Adjustments to Reduced Cash Transfers: Religious Safety Nets and Children's Long-Term Outcomes*

Naomi Gershoni † Rania Gihleb ‡ Assaf Kott §
Hani Mansour ¶Yannay Shanan ‖

March 20, 2025

Abstract

This paper examines how access to informal insurance shapes family responses to reductions in social welfare benefits, and how these adjustments affect children's development. In 2003, Israel reformed its child allowance program, significantly reducing unconditional cash benefits for large families. Using a sharp date-of-birth cutoff introduced by the reform, we show that Jewish families substituted for the loss in government benefits by enrolling their school-aged children in ultra-Orthodox religious schools. These schools provide valuable services unavailable in mainstream public schools but focus primarily on religious studies with minimal secular education. In the long run, this shift resulted in lower educational attainment among Jewish students and steered them toward a more religious lifestyle. The safety net provided by religious schools prevented Jewish parents from having to reduce their completed fertility or to increase their labor supply. In contrast, Arab families—who lacked access to comparable informal insurance—responded by reducing completed fertility and increasing paternal employment. Consequently, we find little evidence that the decline in transfers negatively affected the education or labor outcomes of Arab children.

JEL codes: Z12, J13, J22, H41, I38

Keywords: Child allowance, Human capital, Religion, Cash benefits, Fertility, Labor supply

*Previously circulated as "Religious Safety Nets and their Effects on Human Capital." The authors gratefully acknowledge research support by the following sources. Gershoni, Gihleb, Mansour and Shanan: US-Israel Binational Science Foundation (BSF grant No. 2020162). Kott: Policy Impacts Early Career Scholars Grant.

†Department of Economics, Ben Gurion University of the Negev and IZA, naomige@bgu.ac.il
‡Department of Economics, University of Pittsburgh and IZA, gihleb@pitt.edu
§Department of Economics, Ben Gurion University of the Negev, kotta@bgu.ac.il
¶Department of Economics, University of Colorado Denver, hani.mansour@ucdenver.edu
‖Department of Economics, Bar Ilan University, yannay.shanan@biu.ac.il

1 Introduction

The behavioral adjustments of families to reduced transfers from social welfare programs can have significant and long-lasting effects on their children's well-being and development. These adjustments, whether by reallocating household resources or changing income sources, are shaped by the specific constraints families face and the availability of alternative support options (Moffitt, 1992; Hanna and Olken, 2018; Aizer et al., 2022, 2025). In many countries, transfers provided by informal networks, such as religious organizations, and family or community members, are an important source of insurance against income shocks, often substituting for government transfers (Hungerman, 2005; Chen, 2010; Iyer, 2016; Auriol et al., 2020; Ruffini et al., 2025). However, the implications of informal insurance availability on family adjustments and children's long-term outcomes remain unclear.

In this article, we study how families with and without informal insurance options adapted to a significant reduction in unconditional welfare transfers and how their children were subsequently impacted in the long run. We use a reform in Israel's child allowances program, which provides families with a nontaxable monthly allowance for each child from birth until they turn 18. Prior to 2003, the allowance schedule increased at an increasing rate with birth order. The 2003 reform linearized the benefits schedule, substantially reducing the monthly allowance provided for third- or higher-order children born after June 1, 2003 compared to children of the same birth order born just before this date. The differential reduction in transfers for treated families was sizable, on average amounting to approximately $9,200 over more than 10 years. This is equivalent to about 6 months of full-time minimum-wage employment. Importantly, transfers for subsequent children born after 2003 did not vary across this cutoff, ensuring that families on either side faced the same marginal cost of having additional children.

Although the reform was applied equally to all Israeli citizens, affected families may have had access to different alternative support options. In particular, Jewish families can choose to enroll their children in ultra-Orthodox (UO) religious schools which provide in-kind

amenities not available in traditional public schools, such as a longer school day, free lunch, and subsidized after-school care (Schiffman, 2005).[1] Although publicly funded, UO schools do not follow the mandatory curriculum set by the Ministry of Education (MoE), prioritizing religious studies over secular subjects (State Comptroller and Ombudsman, 2020). Despite their religious emphasis, many of these schools welcome and actively recruit non-UO Jewish students, effectively serving as a religious safety net for families willing to compromise on the quality of their children's secular education. However, the informal safety net provided by UO schools is not accessible to Arab citizens of Israel for whom traditional public schools are the only option.[2]

We use the sharp date-of-birth cutoff in a regression discontinuity design (RDD) to isolate the income effect of the reform on the fertility, labor market, and school choice adjustments that Jewish and Arab families made. We also use the same approach to estimate the consequences of the decline in transfers for children's long-term outcomes, including education, employment, marriage, and fertility. The timeline of the legislation made it difficult for families to manipulate the timing of births in response to the reform. Nonetheless, to validate the research design, we provide evidence that families on either side of the cutoff are similar across a wide range of demographic and economic characteristics, and we verify that there is no discontinuous jump in births around the cutoff. In addition, we show that the results are robust to varying bandwidth sizes, alternative model choices, and donut-hole specifications, further mitigating concerns about endogenous birth timing.

We use administrative data covering the universe of Jewish and Arab families whose child was born in 2003, and retrieve the child's exact date of birth from the population registry. We also obtain information on the birth years of all older and younger siblings to identify

[1] Ultra-Orthodox Jews describe communities within Judaism that strictly adhere to Jewish laws and traditions and reject modern values (Berman, 2000). In 2003, about 6% percent of the Israeli population can be classified as UO. In 2022, their share in the population increased to 13%.

[2] Arab citizens of Israel, many of whom (but not all) identify as Palestinian, comprise about 20% of Israel's population. At the time of the reform, about 81% of Israel's Arab population were Muslim, 11% were Christian, and the remainder belonged to the Druze religious minority (Central Bureau of Statistics, Israel, 2022).

the family size at the time of the reform, the birth order of children, and to estimate fertility responses. We combine these data with restricted administrative records from the Israel Tax Authority, enabling us to estimate short- and long-term effects on parental labor supply. We use data from the Ministry of Education to determine the type of schools children are enrolled in and to observe long-term outcomes, such as the likelihood of high school completion, the probability of matriculating, and the chances of attending postsecondary education, including religious seminaries. Lastly, we use data from the population registry to estimate effects on children's age at marriage and age at first birth.

The results indicate that reduced child allowances had no effect on the short-term or completed fertility of Jewish families. In fact, the estimated effect on completed fertility is positive, though not statistically significant. Moreover, there is little evidence that Jewish mothers or fathers responded to the reform by increasing the number of months they were employed, either soon after the implementation of the reform or in the long run.

How did Jewish families adjust to reduced transfers? Consistent with the role of religious schools as informal safety nets, we find that following the reduction in transfers, treated Jewish families were about 4 percent more likely to enroll their elementary school-aged children (ages 5-12 in 2003) in UO schools. This effect is roughly twice as large for boys compared to girls, likely due to the higher returns to religious education among boys as it enables them to attend postsecondary religious seminaries (Yeshiva), thereby deferring military service and securing generous government funding. Interestingly, we find a much smaller insignificant change in the probability that younger children aged 0-4 in 2003 are ultimately enrolled in UO schools. These findings suggest that when parents experience a decline in transfers, their investment decisions in children's human capital vary depending on the child's age and gender (Shah and Steinberg, 2017).

The decline in transfers and the subsequent differential investments carry significant long-term consequences on children's outcomes. We find that affected boys exhibit a higher enrollment rate in UO high schools, along with substantially higher dropout rates and a lower

likelihood of obtaining a high school diploma. Notably, these boys are 4.6 percentage points less likely to pursue postsecondary education, with no corresponding (statistically significant) increase in their likelihood of attending a Yeshiva or entering the labor market. They are also more likely to marry by age 20 and to have their first birth before age 22, suggesting a more religious lifestyle. Overall, these findings may indicate that affected children were more likely to rely on the publicly funded safety net as adults.[3]

In contrast, Arab families, who do not have access to UO schools, adjusted to the decline in benefits by reducing their fertility and increasing their labor supply. Fertility declined soon after the reform was implemented and remained lower in the long term, resulting in a 10 percent decrease in completed fertility. While we observe no change in the labor supply of Arab mothers, Arab fathers increased the number of months they reported being employed, with this effect persisting even 10-12 years after the reform. These adjustments appear to mitigate the potential negative effects of reduced child allowances on children's outcomes. In fact, we find some evidence that the decline in transfers reduced the likelihood of high school dropout among Arab girls and increased the labor supply of boys in early adulthood.[4]

The paper makes two main contributions to the literature. First, we contribute to the literature on unconditional cash transfers by examining how families with different adjustment options respond to reduced transfers and how their choices map onto children's long-term outcomes. A large body of literature has studied the effects of social safety nets on children's outcomes in the U.S. and other countries, while also analyzing how they affect parental fertility and labor supply choices (Moffitt, 1992; Aizer et al., 2022; Shah and Gennetian, 2024; Dustmann et al., 2024). Examples include studies of the U.S. Food Stamps Program (Hoynes and Schanzenbach, 2012; Hoynes et al., 2016; Bailey et al., 2023), the EITC (Dahl and Lochner, 2012; Hoynes et al., 2015; Manoli and Turner, 2016-2017; Bastian and Michelmore,

[3]In addition to child allowances, Israel offers different types of safety net programs, such as guaranteed minimum income and disability insurance. Unfortunately, we do not observe these transfers in our data.

[4]Consistent with our findings, Mari (2024) shows that a reduction in child-related payments to large families in the Netherlands had little effect on fertility but negatively impacted the educational outcomes of children in poorer families.

2018), and cash transfers from welfare-to-work experiments (Hill et al., 2001; Gennetian and Miller, 2002; Clark-Kauffman et al., 2003; Morris and Gennetian, 2003). However, these programs are means-tested or rely on other forms of conditional transfers and cannot be directly compared to the effects of unconditional cash transfers. Studies of universal child-related benefits mostly focused on the effect of *expected* transfers on fertility, addressing a combination of income and price effects (Milligan, 2005; Cohen et al., 2013; González, 2013). Importantly, these previous studies have not been able to systematically analyze how different parental adjustments to income shocks relate to children's adult outcomes.

Meanwhile, evidence on the long-term effects of unconditional transfers to families is scarce, and whether they are an effective tool for promoting child development remains uncertain (Heckman and Mosso, 2014; Hendren and Sprung-Keyser, 2020).[5] While Barr et al. (2022) and Aizer et al. (2016) reported substantial positive effects on children's human capital accumulation, adult earnings, and health in the U.S., Borra et al. (forthcoming) found little evidence that Spain's introduction of a "baby bonus" improved children's human capital or health outcomes. Evidence from changes in other sources of non-work income, such as lottery winning or casino profits, indicate relatively small effects on both labor supply (Akee et al., 2010; Cesarini et al., 2017; Jones and Marinescu, 2022) and child development (Cesarini et al., 2016).

By comparing two groups subject to the same policy but with different adjustment options, we show that parental responses are essential for understanding the effects of reduced transfers on children's development. Importantly, the decision to change human capital investments for some children but not for others is consistent with evidence that families make heterogeneous adjustments to their children's education based on their age at exposure, and that families discount the future consequences of their investment decisions in response to

[5]Evidence from developing countries mostly focus on short-term effects on children's cognition, health, and education with little evidence on parent's behavioral responses and on the persistence of these effects in the long run (Baird et al., 2011; Macours et al., 2012; Amarante et al., 2016; Haushofer and Shapiro, 2016; Baird et al., 2019). A recent experimental study of unconditional cash transfers to poor families of newborns in the U.S. found no short-term effects on children's development and health (Hart et al., 2024; Sperber et al., 2023).

immediate income shocks (Shah and Steinberg, 2017; Carrillo, 2020; Dustmann et al., 2024). In contrast, consistent with the quantity-quality tradeoff, reducing completed fertility and increasing parental labor supply can improve children's human capital and may mitigate the potential harm of reduced cash transfers during childhood (Angrist et al., 2010; Bagger et al., 2020).

Second, we contribute to the literature on religious institutions as ex-post insurers by examining how access to religious safety nets affects intra-household behavioral responses to income shocks and children's long-term outcomes. In his seminal work, Berman (2000) argues that the low labor supply and high fertility rates among UO men serve as a commitment device to their community which provides mutual insurance to its members. Building on this idea, Dehejia et al. (2007), Chen (2010), and Auriol et al. (2020) provide evidence that insurance is an important determinant of religiosity, while Hungerman (2005), Scheve et al. (2006), Gruber and Hungerman (2007), Dills and Hernández-Julián (2014) show that government and church activities function as substitutes.[6]

To our knowledge, this is the first study to show that religious schools provide insurance to non-community members at the cost of compromising their children's secular education, while crowding out alternative adjustment mechanisms, such as fertility and labor supply. In addition, we are the first to document how reliance on religious safety nets impacts children's development, potentially leading to lasting intergenerational economic and cultural effects. These findings have significant implications for the design and effectiveness of public policies in contexts where religious institutions receive public funding and when state and religious governance structures coexist (Acemoglu et al., 2020; Basurto et al., 2020; Bazzi et al., 2023).

The remainder of the paper is organized as follows. Section 2 describes the institutional background of Israel's child allowance program and the 2003 reform. Section 3 describes the data sources and the sample used in the analysis. In section 4 we detail the empirical

[6]The finding that economic distress increases religiosity can also be consistent with a mechanism through which religious organizations provide spiritual or coping insurance against future risk (Auriol et al., 2020). See, for example, the work of Ager and Ciccone (2017) and Sinding Bentzen (2019).

strategy and investigate the validity of the research design. We discuss the results in section 5 and conclude in section 6.

2 Institutional Background

2.1 The 2003 Child Allowances Reform

Israel's child allowances program, established in 1959, is a nontaxable monthly income transfer that all mothers receive for every child, from birth until that child turns 18. Importantly, eligibility for the allowance does not depend on the mother's marital status, employment status, her household's income, or the overall number of children in the household, and can be thought of as a monthly unconditional cash transfer lasting for 18 years per child (Frish, 2004; Cohen et al., 2013).[7]

Although eligibility for the child allowance program is unconditional, the magnitude of the payment was historically designed to increase with parity at an increasing rate from the second to the fifth child, remaining constant for sixth or higher parity children.[8] As can be seen in Table 1, before 2003 payments almost doubled for third- compared to second-born children and for fourth- compared to third-born children. This convex schedule was a key interest of UO communities, where fertility rates are extremely high, and was therefore a critical demand of the UO parties that represent them.[9] In February 2003, a new government coalition, which did not include these parties, announced its intention to linearize the payment schedule for newly born children.[10] According to this so-called Netanyahu reform,

[7]Moreover, eligibility for other welfare programs is not determined based on the income from child allowances.

[8]Households assigned a veteran status received a larger allowance from 1975-1993. However, starting in 1994, the transfer's generosity was no longer tied to military service (Frish, 2008).

[9]At the time of the reform, the average total fertility rate for UO women exceeded 7, compared to roughly 3 for the entire population (Hleihel, 2017; Israel Central Bureau of Statistics, 2024).

[10]Clearly, the linearization of the schedule disproportionately impacts populations with high fertility rates. As mentioned above, this includes UO Jews, but also religious Jewish families and Muslim families, for whom total fertility rates in 2003 were approximately 4 and 4.5, respectively (Israel Central Bureau of Statistics, 2024).

payments for children born after June 1, 2003, would be fixed regardless of their birth order, while children born before that date would continue to benefit from a convex payment schedule, albeit with lower amounts. This significant decline in allowance generosity could not be expected by the public as the details of the reform were first revealed on April 29, 2003, and it was voted into law on May 29, 2003.

Despite frequent changes to the allowance schedule in subsequent years, the sharp date-of-birth cutoff set by the reform remained significant, resulting in substantial variation in payments to families with a third- or higher-parity child born in 2003, depending on whether the birth occurred before or after this cutoff (see Table 1). For example, in 2004, a mother with four children under the age of 18 whose fourth child was born in June 2003 received NIS 616 ($191) per month in child allowances, compared to NIS 1,048 ($324) per month received by a mother whose fourth child happened to be born in May 2003.[11]

To further illustrate the significance of the difference in child allowances generated by the reform's cutoff, Figure 1 plots the yearly child allowance payments for households with the same number and age distribution of children, except that the fourth child was born either just before or just after the June 2003 cutoff (depicted in black solid line and a dashed gray line, respectively). In this simulation, we assume a three-year spacing between siblings, which only affects the years in which older siblings age out of the program. The difference between the payments received by these two types of households changed over the years but remained substantial (20 to 50 percent) until 2012 when the oldest sibling turned 18, and the fourth child became third for the purpose of the allowance calculation. A smaller difference remained for three additional years until another sibling aged out of eligibility and starting in 2015, the benefit amounts leveled out for the two households. Over the period 2003-2015, the difference in non-work income between the households amounted to NIS 27,300 ($8,800), roughly one-third of the average yearly income for similar families in our data in 2000. As this simulation highlights, the age structure of older siblings is an additional source

[11] We use 2021 NIS throughout the paper, and the average exchange rate for that year which was 1 USD = 3.23 NIS.

of variation in allowance amounts and will thus be controlled for in the empirical analysis.

In Appendix Figure A.1, we present additional simulations. Panel A compares families with the same age structure as in Figure 1, except that a fifth child was born in 2006, after the reform. Because the monthly allowances for this additional child are equal between the two families, regardless of the exact timing of the fourth birth, the difference between the payments they received remains identical to the difference in Figure 1. This demonstrates that the 2003 reform did not generate variation in *expected* benefits for future children who were not yet conceived at the time of its enactment. Panels B, C, and D present this comparison for families with 2, 3, and 5 children, respectively, in which the last child was born in 2003. These figures show that the reform did not impact the amount of child allowances received by families with a second birth in 2003, and that starting with the third child, the difference in benefits across the cutoff increases with the parity of the child born in 2003. In our analysis, we focus on families with a fourth or higher-order birth in 2003 since the difference in benefits for a third birth is relatively small.

2.2 Ultra-orthodox Schools in Israel

Israel's schooling system is predominantly public, especially at the elementary education level. The system was designed to accommodate the country's significant ethnic and religious diversity and can be broadly categorized into four types of schools: Arab, Jewish-secular, Jewish-religious, and Jewish-UO. While the first three types differ somewhat in curriculum and funding, they all adhere to the same regulations set by the Ministry of Education (MoE), including mandatory standards for core subjects such as Math, English, and Science. The MoE also regulates teaching hours per student, class sizes, and the scope of extracurricular activities schools may offer. Accordingly, the MoE directly manages most school budgets, employs teachers, and pays their salaries based on a collective agreement.

In contrast, UO schools operate as autonomous entities. They are run by nonprofit organizations that receive direct state funding but face minimal oversight regarding their

curriculum, teaching hours, or teachers' salaries. This autonomy allows UO schools to prioritize religious studies at the expense of secular education (Kingsbury, 2020; Barak-Corren and Perry-Hazan, 2022).[12] In practice, even in UO schools that officially declare teaching the subjects required for the state-administered high school diploma exams ("Bagrut"), only 7% of boys and 20% of girls are eligible for a diploma, compared to over 70% in non-UO Jewish schools (State Comptroller and Ombudsman, 2020).

Meanwhile, the flexible administration of their budgets, combined with additional funding from unique government transfers and private donations, enables UO schools to offer amenities not available in other types of schools (Schiffer, 1999).[13] These amenities include longer school days, hot meals, and transportation (Kamil, 2001; Weissbrod, 2003; Blass and Bleikh, 2016; Lipiner and Zussman, 2021). Many UO elementary schools also subsidize afterschool care, while UO high schools commonly provide subsidies for boys attending boarding schools (Schiffer, 1999). Despite the insular nature of UO communities, many UO schools leverage these amenities to actively recruit non-UO students, thereby gaining the political support of their families (Schiffman, 2005; Lipiner and Zussman, 2021). At the same time, many non-UO and even non-religious families would consider enrolling their children in these schools due to the amenities that they provide. In fact, the share of Jewish students enrolled in a UO school was 20% in 2004 and 24% in 2010, well above the share of UO children in the population (Blass and Bleikh, 2016).

[12]In principle, up to 45% of their funding could be withheld if the MoE's standards for core subjects are not met. However, in practice, their funding is rarely suspended even when there is clear evidence of noncompliance, and UO schools that claim to teach these subjects often fail to do so adequately (State Comptroller and Ombudsman, 2020).

[13]The organizations that manage UO schools are often affiliated with political parties that secure these transfers during coalition negotiations. These payments are typically routed through the Ministry of Religious Services to bypass MoE supervision, which officially prohibits such funding. UO boarding schools also receive support from the Ministry of Labor (Schiffer, 1999).

3 Data

We utilize rich administrative data on the universe of Israeli households who had a child in 2003 to study the effects of the reduction in allowances. The population registry provides exact dates of birth for children born in 2003 and year of birth for all their siblings. Birth timing of children born up to 2003 is critical for implementing our identification strategy, whereas birth timing of younger siblings serves as an outcome in our analysis of fertility choices following the reform. Basic demographics of parents and children such as sex, ethnicity (Jews/Arabs), locality of residence in 2003, birth and immigration year, and country of origin come from the population registry. We proxy households' socioeconomic status (SES) using their residential locality classification, following the ten-cluster system of the Israeli Central Bureau of Statistics. In the registry data, we also observe the marital status of the children and their fertility outcomes as they age, with records updated through 2021.

We combine these data with children's educational records, which we obtain from the Ministry of Education. For each child, we identify the type of school that they attend each year (secular, religious, or UO) and their outcomes in state-administered matriculation exams between 2000-2020.[14] Students must pass a series of these exams that add up to at least 20 credits in order to receive a high school diploma known as "Bagrut". Each exam is on a specific subject and associated with a number of credits between 1 to 5, indicating its level of difficulty. Our data provides information both on Bagrut eligibility and on the total number of credits awarded, which is a measure of the quality of the diploma. This diploma is an important prerequisite for college enrollment and is required for many entry-level positions in the labor market. For children who are old enough to attain postsecondary education, we use annual records of schooling years to infer whether and when they were enrolled in postsecondary studies. These records are available up to 2021.

[14] We build on the official classification of school type recorded by the MoE but further refine it using data from the Haredi Institute for Public Affairs, a research institute that specializes in collecting and analyzing data related to the UO ("Haredi") society in Israel. Based on their data, we redefined the classification of 3.5% of the schools in our sample.

Lastly, data on labor market outcomes come from the Israel Tax Authority records, which include information on salary workers as reported by their employers and on self-employed individuals. For parents, we use data for the years 2000-2015, allowing us to follow parents' outcomes for 12 years after the reform. For the children, we look at early labor market outcomes using data for the years 2015-2020. During these periods, we observe the annual number of months worked and annual labor earnings.

Table 2 presents descriptive statistics for families with births in 2003. We restrict our sample to Jewish and Arab families where both the mother and the father were present in the household at the time of birth.[15] Column (1) presents characteristics of all families with births that year, while Column (2) focuses on families whose fourth or higher parity birth occurred in 2003, the main group analyzed in our study. The high parity sample has parents with lower labor force participation and substantially lower household income compared to the overall sample. This is notable given that mothers and fathers in this sample are, on average, more than 3.5 years older. Arab and UO Jewish families are overrepresented in the high parity sample, consistent with the higher fertility rates in these population groups. This is also in line with mothers in these families having their first child at a younger age. Column (3) restricts the sample to families with a high parity birth within a 70-day bandwidth around the cutoff (our main sample for the analysis). Despite potential seasonality in birth timing, the characteristics of this restricted sample remain virtually identical to the entire sample of high parity births.

In Columns (4) and (5) of Table 2, we further divide this sample by Arabs and Jews, respectively. We run our analysis separately for these two distinct population groups, which differ on several important background characteristics. Jewish fathers have higher labor earnings than Arab fathers despite the fact that, on average, they work fewer months per

[15] Approximately 2% of the families in Israel are classified as neither Jewish nor Arab and belong to various population groups, such as immigrants from the former Soviet Union who have Jewish relatives but are not Jewish themselves. In addition, we exclude families whose 2003 births occurred abroad (about 7% of the population), and families with missing data on other children's birth timing or on other covariates (about 6% of the population). Approximately 1.5% of the remaining families are single-parent households at the time of birth.

year. This anomaly reflects the fact that the Jewish families sample includes two types of households that differ in terms of male labor force participation: UO (54.3%) and non-UO.[16] While non-UO Jewish men mostly work full-time and earn more than their Arab counterparts, UO Jewish men are known to have extremely low labor market participation rates, as they are expected to attend Yeshiva and devote their time to religious studies. Turning to the mothers, both Jewish and Arab mothers earn less than their husbands. However, Arab mothers' only work an average of 1.4 months of employment per year with average annual earnings of NIS 6,436, roughly equivalent to full-month earnings at the minimum wage.

4 Methodology

4.1 Empirical Strategy

We estimate the causal effect of reduced child allowances using a regression discontinuity design based on the precise birth date of the child born in 2003 around the reform's cutoff (the pivotal child). As discussed above, children born after June 1, 2003, in families with at least two children under the age of 18 received a lower monthly allowance than those in similarly sized families born just before June 1. However, because the difference in allowances for third-born children is relatively small, our analysis focuses on families whose fourth or higher birth order child was born in 2003. We conduct placebo tests using families whose first or second child was born in 2003, as the reform should not have produced any difference in the allowance for children born on either side of the cutoff.

An important feature of the reform is that the differential allowance only applied to the specific child born in 2003 and did not impact the payment schedule that families across the threshold received for their older children or for additional children born after 2003. Thus, the cost of having an additional child is similar for families on either side of the cutoff.

[16] UO families prior to 2003 are identified based on children enrolled in UO schools. Because we cannot assess religiosity for families whose oldest children have not yet enrolled in school, our measure is likely to underestimate the share of UO families in the population.

Moreover, any other policy change or event is expected to have the same effect on families on both sides of the cutoff. Therefore, any difference in outcomes between households with births before and after the cutoff can be attributed to the income effect of reducing cash transfers.

Although the change in the allowance is tied to the pivotal child, we assume that families pool resources across children. Thus, we consider the treatment to vary at the household level while estimating the effects of reduced child allowances on both household- and individual-level outcomes. First, we estimate the effects on household-level fertility, and mothers' and fathers' employment. Then, we investigate the potential adjustment through school stream choice at the individual child level, including only children born up to 2003. This sample restriction is crucial since the conception of children born after 2003 may have been affected by the treatment. In addition, we use the same sample to study the long-term outcomes of children, including high school dropout, high school diploma eligibility and quality, postsecondary attainment, and age at marriage and parenthood. To investigate long-term effects, the sample is further restricted to children who are sufficiently old to be observed in the data.

Formally, we estimate the following specification:

$$Y_i = \alpha + \beta D_{h(i)} + f(days_{h(i)}) + \theta' X_i + \epsilon_i, \tag{1}$$

where Y_i is the outcome of child i in household h. $D_{h(i)}$ is an indicator equal to one if the pivotal child in household h was born on or after June 1st, 2003 (the reform cutoff). The running variable $days_{h(i)}$ is defined as the number of days between the exact date of birth in 2003 and the cutoff date. In our main specification, $f(days_{h(i)})$ is a linear trend in this variable which is interacted with $D_{h(i)}$ to allow it to vary across the cutoff. X_i is a vector of individual level characteristics that are time-invariant or measured prior to 2003. These characteristics include sex, birth-cohort indicators, parents' age and its quadratic,

mother's age at first birth, months of employment and labor earnings during 2000 for each of the parents, an indicator for being identified as UO before 2003, indicators for SES cluster based on the locality of residence, and county fixed effects. Because the amount of child allowances and their duration depend on the age distribution of existing children, X_i also includes indicators for the age composition of siblings in 2003, and an indicator for twins birth in 2003.[17] ϵ_i is an idiosyncratic error term. We cluster standard errors at the household level. We estimate a similar specification for parental labor supply and fertility outcomes, with the exception that the vector X includes only covariates at the household level.

Equation 1 is estimated separately for the Arab and Jewish populations. For each of these groups, we use a fixed bandwidth of 70 days around the cutoff and check the robustness of the results to alternative and optimally selected bandwidths (Calonico et al., 2019, 2020). The advantage of using a fixed bandwidth is that the treatment effects for all outcomes and time horizons are estimated using the same sample. In addition, we test the robustness of our results to different weighting methods.

The main assumption underlying our strategy is that families across the reform cutoff date are, on average, comparable on both observed and unobserved characteristics. Stated differently, if it were not for the differential impact of the reform across the birth date cutoff, these outcomes would have evolved smoothly for families across the threshold.

4.2 Investigating the Validity of the Research Design

To investigate the validity of this assumption, we first test whether families responded to the reform by changing their date of birth. Since the policy change was only publicly announced one month before the June 1st cutoff, it could not have altered households' realized fertility at least until the end of November 2003 (nine months after the announcement). Thus, households could not have manipulated their treatment status by changing the timing of

[17] Siblings' age composition is controlled for using a set of indicators for having any siblings at each of the following age categories in 2003: 1-4, 5-9, 10-13, 14-17. Siblings that reached the age of 18 are not taken into account when calculating allowances. We also note that very few families in our sample had children aged 18 or over 18 in 2003.

conception.

Nonetheless, families already carrying a pregnancy would have had an incentive to give birth before the cutoff to maximize the allowance for their newborn child.[18] As Gans and Leigh (2009) and LaLumia et al. (2015) show, there is evidence that expecting parents change their timing of birth in response to policy shocks. These changes are achieved through inductions and cesarean sections.[19] We test for manipulation of the running variable following McCrary (2008) and Cattaneo et al. (2018) by plotting the distributional density of fourth or higher parity births around the reform cutoff. As can be seen from Figure 2, there is no significant discontinuity in the number of births at the cutoff when we include all population groups in the analysis. Panels (a) and (b) of Figure 3 show that the same is true for the Arab and Jewish sub-populations.

To further assess the plausibility of the identifying assumption, we test for balance in observed household characteristics across the reform cutoff. For this purpose, we estimate a specification similar to Equation 1 using each of the observed household characteristics as an outcome. The results in Table 3 provide evidence that most of the differences between treatment and comparison households are small and statistically insignificant, with only 3 exceptions out of 49 estimates. This is true for the entire population as well as separately for the Arab and Jewish populations. Nonetheless, to improve precision, all these characteristics will be included as controls in the regressions. Additionally, we check the robustness of our findings to donut hole RD specifications which omit observations close to the cutoff to avoid potential bias due to manipulation in the timing of births.

[18] The difference in lifetime allowances was not expected to be as large as the realized one since, initially, the plan was for the payment schedules to converge after several years.

[19] These procedures are less prevalent in Israel compared to other OECD countries. For instance, in 2002, data from the OECD shows that the rate of cesarean sections in Israel was 153 out of 1,000 live births compared to 212 in the UK, 234 in Canada, and 366 in Italy. Late-term abortions in Israel are not easily accessed and women from religious backgrounds are unlikely to use them as a contraceptive method.

5 Results

5.1 The Magnitude of the Change in Transfers

We begin by estimating the size of the reduction in child allowances experienced by families giving birth around the cutoff date. While child allowance receipt is not directly observed, we can accurately impute it since monthly payments are determined by three factors we observe: the number of children under 18 in each household, their birth order, and their birth dates. Based on these factors, we calculate the total yearly allowance received by each household until the pivotal child turns 18 and divide this amount by the number of children below age 18 in the household. Subsequently, we define a measure of total exposure to child allowance per child by aggregating these annual amounts for each child over the years.

Figures 4 (a) and (b) illustrate the sharp decline in this measure for Jewish and Arab families with a fourth or higher-parity birth after the cutoff, respectively. Based on equation 1, we estimate that Jewish families with a post-cutoff birth experienced a reduction of NIS 6,132 in transfers per child relative to similarly-sized households with pre-cutoff birth. This amounts to an average total loss of approximately NIS 32,017 at the household level. Since, on average, families received differential benefits for their pivotal child for 10.5 years, this translates to a reduction of NIS 250 per month. The estimated changes in allowances are almost identical for Arab families.

5.2 Parental Adjustments

5.2.1 Fertility and Employment

Standard economic theory assumes that children are a normal good and, thus, predicts that a decline in non-labor income would lead to a reduction in fertility (Becker, 1960; Lindo, 2010). In addition, parents are expected to adjust their labor supply and increase their employment in response to a negative income shock (see e.g., Keane, 2011). In this section, we test these predictions separately for Jewish and Arab families.

In Table 4, we estimate the effect of the reduction in allowances on the number of subsequent children born shortly after the reduction in transfers (up to 2006) and over a longer period (up to 2015), which can be considered as completed fertility. This measure counts the number of additional children born starting from 2004, the year after the reduction in child allowances. The results in Panel A for Jewish households show small and insignificant effects on both short- and long-term fertility (Columns (1) and (2), respectively). Moreover, in contrast to theoretical predictions, the point estimates are positive. These findings imply that Jewish families did not adjust to the change in allowances by decreasing fertility.

Panel B presents the same estimates for Arab families. The results in Column 1 indicate that Arab families reduced their fertility by 0.046 children born within three years of the reduction in child allowances. Although this effect is not statistically significant at conventional levels, it persists in the long term. The results suggest a decline of 0.102 children in completed fertility, which is statistically significant at the 10% level (Column 2). Notably, both the short and the long-term estimates correspond to a 10% reduction in fertility relative to the control mean. The overall reduction in completed fertility amounts to 1.6% from an average of 6.4 children.

By presenting the same estimates year-by-year after 2003, Figure 5 further confirms that the decline in fertility for Arab families appeared almost immediately after the reduction in transfers and kept increasing over time proportionally to the increase in the control mean (presented in the lower panel of Figure 5). At the same time, the estimated effects for Jewish families are close to zero during the first seven years and then become positive though insignificant.

Finally, Figure 6 presents graphical evidence for the RD estimates in Table 4, showing a clear discontinuous drop at the cutoff in both short- and long-term fertility for Arab parents, and no comparable discontinuity for Jewish parents.

The second margin of parental adjustment we consider is employment. In Columns (3)-(4) of Table 4, we report the effects of the reduction in child allowances on the cumulative

number of months worked during the three years following the reduction in transfers (2004-2006) by fathers and mothers, respectively. Columns (5)-(6) present the estimated effects on employment in the long run, 10-12 years after the reduction in transfers (2013-2015). On average, Arab men worked about 23 months during the 3-year period following the reduction in transfers, just over 7.5 months per year. Jewish men worked slightly less during the same period, approximately 6 months per year. Average employment slightly increases for Jewish men in the long run and remains stable for Arab men. The low levels of employment among Jewish men can be explained by the high share of UO men in the sample who typically attend religious seminaries and have low attachment to the labor market (Berman, 2000). The underemployment of Arab men can be explained through more traditional factors, such as limited access to labor markets, language and discriminatory barriers, and lower levels of skills (Yashiv and Kasir, 2011, 2015). Jewish mothers in our sample worked slightly less than their partners in the short run but surpassed them in the long run. Their employment was also substantially higher than that of Arab mothers who, on average, worked about 1.5 months per year in the short run and slightly more than 3 months per year in the long run. The employment rates of Arab women in Israel, especially those with a large number of children, have been historically very low. In contrast, many UO Jewish women who typically have large families work outside the home and provide for their families while their husbands attend religious seminaries (Cahaner and Malach, 2023).

The results in Panel A of Table 4 show little evidence that the decline in benefits affected either short- or long-term employment of Jewish parents. In contrast, Panel B reports positive effects for Arab fathers. Specifically, the estimated increase of 0.9 months in the short run corresponds to a non-negligible 3.9% increase relative to the mean but the coefficient is not statistically significant. This effect increases to almost 8% in the long run and becomes significant at the 5% level. As we report in Appendix Table A.1, we find that the earnings of Arab fathers increase both in the short and in the long term but these results are not precisely estimated.

Overall, we find that Arab families adjusted to the reduction in cash transfers through decreased maternal fertility and increased paternal labor supply. In contrast to the lack of response for Jewish families, these adjustments align with theoretical predictions and empirical evidence that children and leisure are normal goods. These findings are robust to varying the size of the bandwidth (Appendix Figure A.2).[20] In addition, the estimated effects remain consistent, with minor changes to their precision, when we assign higher weights to observations near the cutoff using a triangular kernel function or when we remove observations in an eight-day donut hole around the cutoff (Appendix Table A.2).

For Arab mothers, the magnitude of the effect on fertility is comparable, albeit somewhat lower, to other findings in the literature on the fertility effects of child-related policies. When converted and standardized using USD purchasing power parity, our results imply that each additional $1,000 increases fertility by approximately 1.1%.[21] The modest labor supply response to the reduction in transfers aligns with existing literature (Cesarini et al., 2017; Jones and Marinescu, 2022).

5.2.2 Children's Schooling Streams

The lack of fertility and labor force responses among Jewish families suggests they may have adjusted to the decline in cash transfers by reducing their investments in child quality (Angrist et al., 2010; Bagger et al., 2020). As discussed in Section 2, Jewish families in Israel can choose to enroll their children in UO schools, which provide additional amenities and services at the expense of secular education quality. Thus, in this section, we examine the possibility that treated Jewish families substituted the unconditional transfers provided by the government with benefits that condition on enrolling children in UO schools.

[20] Panels (a)-(b) in Appendix Figure A.2 plot estimates for short- and long-term fertility adjustments for Arab families. Panels (c)-(d) plot short- and long-term term employment adjustments for Arab fathers. We vary the bandwidths between 21 and 105 days, depicted on the horizontal axis. Our preferred coefficient based on the 70-day window is reported in yellow, while the Calonico et al. (2020) automatic bandwidth is plotted in green.

[21] Milligan (2005) found a 2.6% increase per CAD 1,000, and Raute (2019) report a 2.1% increase per EUR 1,000.

In Table 5, we estimate the effect of the reduction in child allowances on the likelihood that families enroll their children in an UO school after 2003. Columns (1)-(2) present the results for siblings of the pivotal child who were 5-12 years old in 2003. These children were about to enter or already enrolled in elementary school at the time of the reduction in transfers, and switching them to an UO school would have enabled families to gain access to important amenities that could substitute for the immediate loss in cash transfers. The results in Column (1) Panel A confirm that the reduction in transfers increased the likelihood that elementary school-aged children in 2003 ever enrolled in an UO school at the elementary level (grades 1-8) by 2.7 percentage points, or by about 4 percent relative to the mean among families in the control group. The results in Column (2) Panel A indicate that these effects persisted in high school (grades 9-12), with a similar 3.2 percentage points increase in enrollment probability (5% increase relative to the control mean). Both effects are statistically significant at the 1% level. These effects are stronger for boys (Panel B) than for girls (Panel C), consistent with the larger return to religious schooling for boys. Specifically, the results indicate that the reduction in transfers increased the probability that elementary school-aged Jewish boys ever enrolled in an UO elementary school by 4.9 percentage points, an effect that is statistically significant at the 1 percent level (Column 1, Panel B), while having a much smaller and statistically insignificant effect on girls (Column 1, Panel C). The estimated effect on enrolling in an UO high school is also larger for boys than for girls, but this difference is not statistically significant. Figures 7 (a) and (b) present the same findings graphically demonstrating a clear discontinuous increase in UO enrollment after the reform.[22]

Columns (3) and (4) of Table 5 present the results for children who were 0-4 years old in 2003. These children were too young to be enrolled in school at the time of the reduction in child allowances, and thus, their families could not have changed their schooling track at

[22] We find a similar corresponding decrease in the likelihood that elementary school-aged boys ever enrolled in a religious non-UO schools. This suggests that the children whose enrollment was affected come from religious or traditional families.

the time of the reform to gain access to the amenities provided by UO schools. The results indicate that the reduced transfers had little impact on the likelihood that Jewish boys or girls in this age group ever attended an UO school at any grade. We also find no evidence that the reduced transfers impacted the probability of ever enrolling in an UO high school among children who were 13-17 years old in 2003 (See Columns 5-6 in Appendix Table A.4).[23] This is expected since it would have been substantially more difficult to change schooling tracks at this stage. As we show in Appendix Figure A.3 and Appendix Table A.3, the results for children aged 5-12 in 2003 are robust to alternative bandwidth selections and to using a triangular kernel or an eight-day donut hole around the cutoff.[24]

Overall, these findings imply that Jewish families adjusted to the decline in transfers by changing their investments in the human capital of elementary school-aged boys. The differential adjustment by children's age and gender suggests that this was a compromise for the parents rather than a change in their preferences. The differential response of Jewish and Arab families is in line with the differential access to the alternative safety net provided by UO schools. It is unlikely that this results from differences in other welfare programs provided by the government, as during this period Jewish and Arab families were equally eligible for these programs.

5.3 Children's Long-Term Outcomes

Building on the different adjustments made by Jewish and Arab parents in response to the reduction in child allowances, we next examine the effects on children's long-term outcomes, including earning a Bagrut diploma, dropping out of high school, pursuing postsecondary education, employment, marriage, and fertility. Specifically, for Jewish families, the decision

[23] In addition, Appendix Table A.4 presents two placebo tests: applying the reform cutoff day (June 1) to a sample of 2002 births and examining parity-one and parity-two births in 2003. In both tests, the results show no evidence of a change in the likelihood of enrolling children in UO schools.

[24] We do not find any significant effects on UO enrollment for children in families with parity-three birth in 2003. This could be because the decline in transfers is much smaller for these families or because they are less likely to be on the margin of sending their kids to religious schools. As mentioned above, for these families, we estimate an insignificant negative effect on fertility which may indicate that their choice of adjustment is more similar to that of Arab families.

to enroll children in UO schools may have had persistent effects on academic trajectories by altering the quality of early childhood education. For Arab families, adjustments in fertility and parental employment could have shaped children's long-term educational and economic prospects. We interpret these patterns as consequences of the distinct short-term strategies employed by Jewish and Arab parents, though we acknowledge that other channels may have contributed to the observed effects of the reduction in transfers on children's long-term outcomes. The analysis in this section focuses on siblings of the pivotal child, born at or before 2002, and aged at least 19 in 2021, the last year available in our data for these outcomes.

5.3.1 School Quality and the Outcomes of Jewish Children

Enrolling children in UO schools served as an alternative safety net for Jewish families. As explained above, these schools provide lower-quality secular education, and their students' attainment in the Bagrut matriculation exams is extremely low. Thus, we first focus on the effects of the reduction in child allowances on the likelihood of receiving the Bagrut matriculation diploma and on the quality of the diploma obtained (measured by the total number of credits earned). Earning this diploma is essential for academic higher education and many non-academic job opportunities, though it is possible to graduate high school by completing 12 years of schooling without obtaining the diploma. Therefore, we also separately estimate the effect of the reduction in transfers on the likelihood of dropping out.

In Columns (1)-(3) of Table 6, we focus on Jewish children aged 5-12 in 2003, which is the group whose enrollment decisions were affected by the reduction in transfers. Panel A reports the results for this group, while Panels B and C present the results for boys and girls, respectively. The results in Column (1) indicate that the reduction in child allowances decreased the likelihood of obtaining a Bagrut diploma by about 3 percentage points (10% relative to the control mean), and by 3.7 percentage points for boys (17% relative to the control mean). In line with our findings that only boys were more likely to be transferred to

UO schools following the shock, the estimated effect for girls is much smaller and insignificant. The magnitude of the negative effects on earning a Bagrut diploma mirror those estimated for UO high school enrollment in Column (2) of Table 5. Furthermore, the results in Columns (2)-(3) of Table 6 for boys show that the decrease in child allowances led to 36% relative increase in the likelihood of dropping out and 24% relative decrease in Bagrut credits.

Meanwhile, we do not detect any significant changes in these outcomes for Jewish boys and girls who were 1-4 years old at the time of the reduction in transfers.[25] While the point estimates are mostly close to zero, the 1.9 percentage points insignificant decrease in the likelihood of obtaining a diploma for boys aged 1-4 in 2003 (Column 4) may indicate that the reduction in child allowances led to some negative effects on high school outcomes that were not mediated through UO high school enrollment. Another potential explanation for this decline could be related to spillovers from older siblings who were transferred to UO schools. Taken together, these results indicate that the reduction in transfers influenced the educational outcomes of Jewish boys who switched schooling tracks, while younger and female siblings were largely shielded from long-term negative effects. These findings align with prior research showing that parental human capital investments vary by age at exposure and gender, and with evidence that parents tend to discount the future consequences of their short-term adjustments (Shah and Steinberg, 2017; Carrillo, 2020; Dustmann et al., 2024).

Long-Term Effects on Education and Employment. The changes observed in high school matriculation may persist, resulting in long-term effects on postsecondary educational attainment and employment in early adulthood. Table 7 examines the long-term effects of the reduction in child allowances when the children were aged 19-26. The outcomes we examine are measured based on the years observed for each individual within this age range and either indicate participation in a specific activity at any point during this period or present the average over these years. Column (1) evaluates the likelihood of attending postsecondary

[25] We exclude children born in 2003, who are included in the analysis of schooling streams, because they are too young to observe their matriculation outcomes in the last available year of data.

education, Column (2) focuses on the likelihood of enrollment in Yeshiva religious seminaries, and Columns (3) and (4) assess employment outcomes, including annual months employed and the likelihood of ever being employed.

Among Jewish boys, the reduction in transfers led to a 4.2 percentage point decline in postsecondary enrollment, corresponding to a 20% decrease relative to the mean (Column 1). This decrease is similar in magnitude to the effect on enrollment in UO schools. If boys avoid postsecondary education because they commit to the UO lifestyle, we would expect to see a corresponding increase in Yeshiva attendance. In fact, the results in Column (2), suggest a 2.5 percentage point increase in the likelihood of attending a religious seminary (about a 4% increase), but this effect is not statistically significant. Thus, we conclude that although some may have attended a Yeshiva, the decline in the likelihood of pursuing secular postsecondary education was not entirely offset by an increase in religious seminary attendance.

While the decline in educational attainment may negatively impact labor market prospects, it could also increase the time available for work. This effect may be particularly pronounced within the age range we observe, as these years typically correspond to postsecondary education in Israel. An additional negative effect on employment may be expected if some men adopt the UO lifestyle, which emphasizes full-time religious studies and discourages labor market participation. The results on employment for boys show little evidence of a change in the number of annual months employed or the likelihood of ever being employed (Columns 3 and 4 in Panel A), suggesting these opposing factors may offset each other.

Turning to Jewish girls, we find no statistically significant effects on either postsecondary education or employment. However, the point estimate for postsecondary educational attainment is sizable, suggesting a decline of 3.4 percentage points, or 9% relative to the control mean. This may indicate that some girls experienced negative educational impacts from the decline in transfers, either due to fewer financial resources, their parents' choice of high school stream (where we also observe an insignificant estimate), or spillover effects from male

siblings.

Long-Term Effects on Marriage and Fertility. Lastly, we analyze data from the population registry to estimate effects on children's age at marriage and age at first birth. Figures 8 and 9 present the estimated effects on a set of indicators for ever being married and for having a first child by a certain age between 20-26, respectively. The lower panel in each of the figures displays the share of the sample that were married or had their first child by each age. The results indicate that Jewish boys were about 4 percentage points more likely to marry by age 20 and to have their first child before age 22. We interpret this pattern as indicative of a shift towards a more religious and traditional lifestyle among impacted boys. In contrast, we find no significant or meaningful effects among Jewish girls.

UO Families. A natural question is whether the educational outcomes of UO children were affected, given that their families would have enrolled them in UO schools regardless of the transfer amounts. Indeed, Appendix Table A.6 shows no evidence that these families were more likely to enroll their children in UO schools.[26] We also do not find evidence that UO families adjusted to the reduction in child allowances by reducing fertility or increasing employment (Appendix Table A.5). While we observe no adjustments along these margins, Appendix Table A.7, shows some negative effects on children's educational outcomes. Specifically, both the likelihood of obtaining a Bagrut diploma and the number of Bagrut credits decrease substantially for boys. Although the estimates for children aged 1-4 are imprecise due to the limited sample size, their magnitudes are comparable to those of elementary school-aged boys. We do not find similar effects for girls, which may suggest that even UO families prioritize preserving secular education for girls over boys when facing a reduction in child allowances. These findings are in line with the large body of literature showing that resources during childhood resources shape adult outcomes.

[26]Ideally, we would focus our main analysis on non-UO families. However, our identification of UO families pre-2003 is noisy because we identify religiosity based on school type, and in some families, the children are too young to be in school before 2003.

5.3.2 Parental Adjustments and the Outcomes of Arab Children

Arab families responded to the decline in transfers by reducing fertility and increasing paternal employment. These parental adjustments may have mitigated the negative impact of the reduction in transfers on children's long-term outcomes and could even have led to overall improvements in their educational attainment and labor market prospects.

Indeed, the results in Columns (1)-(3) of Table 8 indicate that for children aged 5-12 at the time of the reduction in child allowances, the decline in transfers had little effect on the likelihood of earning a Bagrut diploma, the likelihood of dropping out of high school, and the number of Bagrut credits. Interestingly, among children aged 1-4 in 2003, we find that the decline in transfers led to a decrease in the likelihood of dropping out of high school by a statistically significant 5.7 percentage points, or about 29% relative to the average in the control group (Panel A, Column 5). This decline is particularly pronounced among girls who experienced a 7.8 percentage point decline in the probability of dropping out of high school, an effect that is statistically significant at the 1 percent level. These results are consistent with a quantity-quality tradeoff in which a decline in fertility increases human capital, and further indicate that such benefits may be heterogenous across children within the family (Angrist et al., 2010; Bagger et al., 2020).

Long-Term Effects on Education and Employment. We report long-term effects on Arab children's postsecondary educational attainment and employment outcomes in Table 9. We find little evidence that the decline in transfers during childhood affected the likelihood of attending postsecondary education. This is consistent with the null effects on earning a Bagrut diploma, which is a prerequisite for postsecondary studies. While we do not find additional long-term benefits for girls, the results indicate that boys work 0.46 additional months per year at the ages 19-26, an increase of about 5.7% relative to the average in the control group (Column (2)). This increase in labor supply is not due to an increase in labor force participation, as we see no effect on the likelihood of ever being employed (Column

(3)).[27]

Long-Term Effects on Marriage and Fertility. We conclude the analysis of long-term outcomes for Arab children by examining the effects on their likelihood of ever being married and of having a first child by a certain age between 20-26. The results in Figure 10 for boys indicate a decline in the likelihood of ever being married starting at age 22, but the effects are not precisely estimated. The results for girls are small and are not statistically significant. Similarly, we find little evidence that the reduction in transfers led to a change in the likelihood of having a child at young ages for both boys and girls (Figure 11). These results align with the null effects on human capital accumulation.

6 Conclusion

In this paper, we examine the effects of reducing cash benefits received by large families generated because of a reform to the Israeli child allowance program. The decline in benefits was unexpected and primarily impacted families whose fourth or higher birth order child was born after June 1, 2003. Based on this sharp and arbitrary cutoff, we use a regression discontinuity design to compare the short-term adjustments of otherwise similar families around the reform's cutoff and to study the long-term effects of the reduction in child allowance transfers on families and children. We separately analyze the choices and outcomes of Jewish and Arab families, as the two populations had different options for adjusting to the decline in transfers. In particular, Jewish families could adjust by sending their children to UO schools that provide valuable amenities, thus acting as a type of alternative, informal religious safety net.

For Jewish families, we find that they substituted for the immediate decline in welfare transfers by enrolling their school-aged children, particularly boys, in religious schools run by UO communities. These schools provide little secular education but offer other important

[27] Children ages 1-4 are too young to enable us to observe their long-term outcomes.

services such as a longer school day and free meals. Consequently, we find that this short-term adjustment led to a decline in high school matriculation rates and an increase in high school dropout rates. The change in schooling decisions was driven by families who had not previously enrolled any children in UO schools, indicating that they relied on religious schools as a social safety net. This trade-off prevented Jewish families from having to alter their fertility or labor market decisions.

Meanwhile, Arab families, who did not have access to this alternative insurance mechanism, adjusted to the decline in cash transfers by reducing their completed fertility and increasing fathers' labor supply. For children in Arab families, we find that lower exposure to child allowances did not lead to negative effects on educational outcomes. In fact, treated families exhibit improvements in dropout rates for girls and in employment duration for boys during early adulthood. These findings suggest that parental responses to the cut in allowances mitigated the adverse consequences of reduced household resources.

Overall, our results show that considering parents' available adjustment options when facing budget cuts is critical, as different options can lead to different adjustments and to significantly different consequences for children. Our findings demonstrate that reducing the generosity of the state's social safety-net—with the aim of reducing fertility rates—had an unintended effect of increasing reliance on the religious safety-net system. This prevented a decline in parental fertility and potentially increased ultra-religious lifestyle adoption among affected children, who ended up marrying and having children earlier. As a result, the 2003 reform may have increased the future economic burden on the state and potentially heightened social and political tensions.

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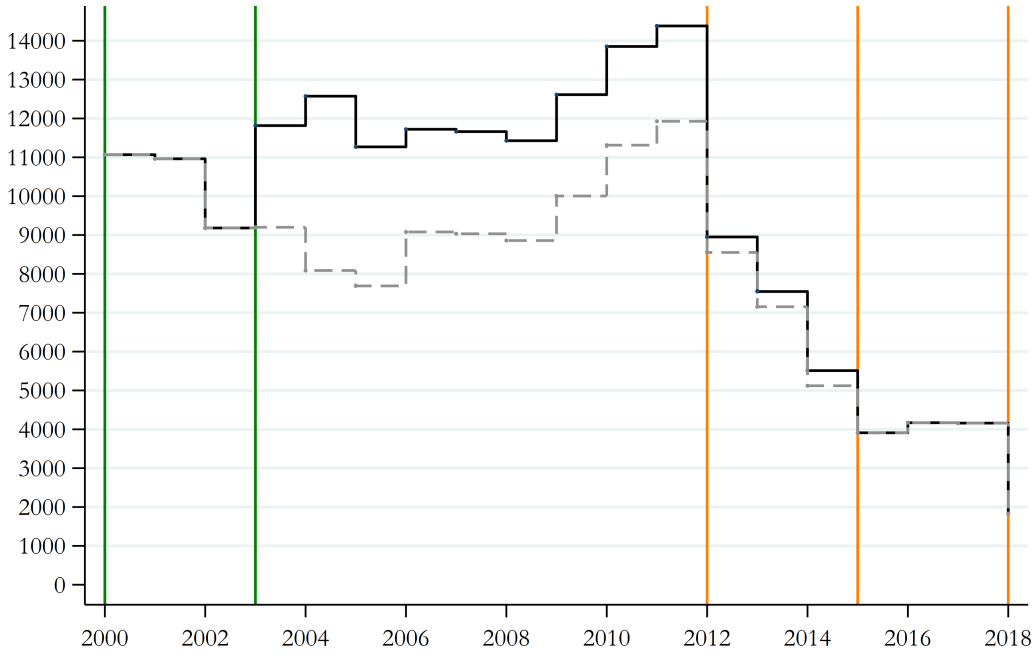
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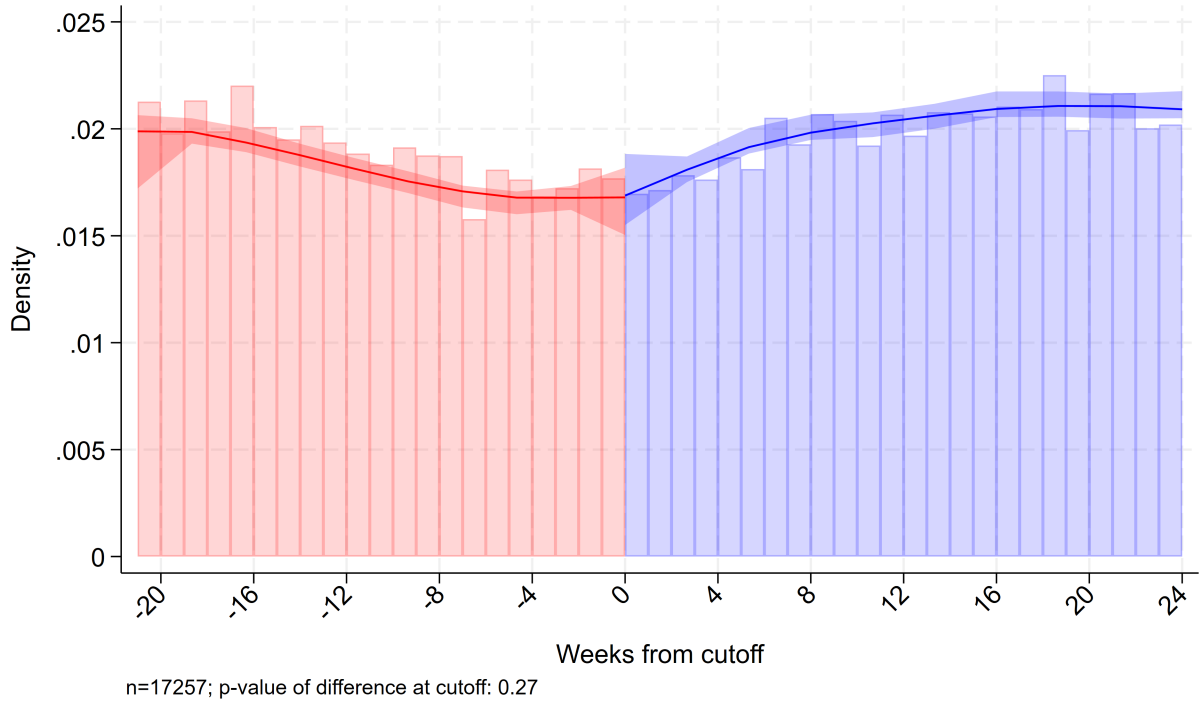
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Figure 1: Household Child Allowance Stream 2000-2018
4th Child Born in 2003



Notes: Y-axis shows the yearly child allowance to be received in NIS by the simulated sibling composition. All amounts are in 2021 NIS. Solid and dashed line indicates the amount when the birth in 2003 occurred pre-June (untreated) and post-June (treated), respectively. The vertical orange line indicates that a child reached the age of 18 and the mother no longer receives an allowance for that child. The green vertical line indicates a birth. A three years spacing between births is assumed. The overall gap in allowances between treated and untreated families in this simulation is NIS 27,300.

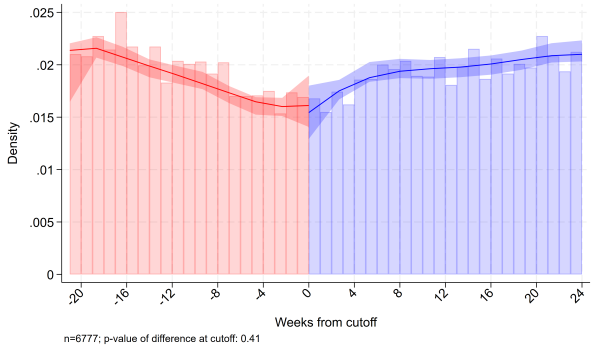
Figure 2: Manipulation Test



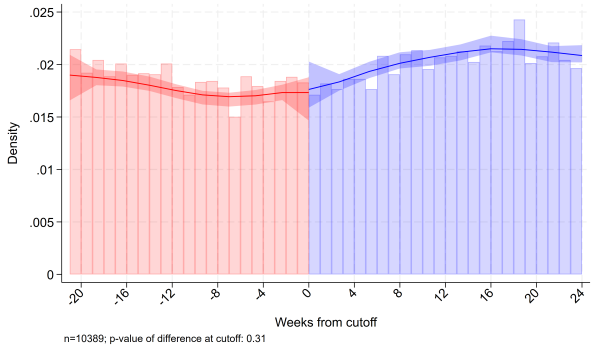
Notes - The figure presents the distributional density of four or higher parity births around the reform cutoff (McCrary, 2008; Cattaneo et al., 2018). The number of observations and the p-value for the significance of the discontinuity at 0 is listed just below the graph.

Figure 3: Manipulation Tests by Population Group

(a) Arab



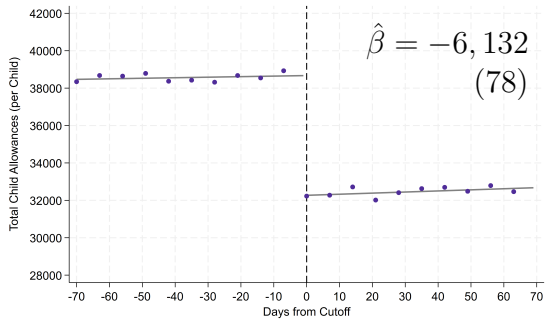
(b) Jewish



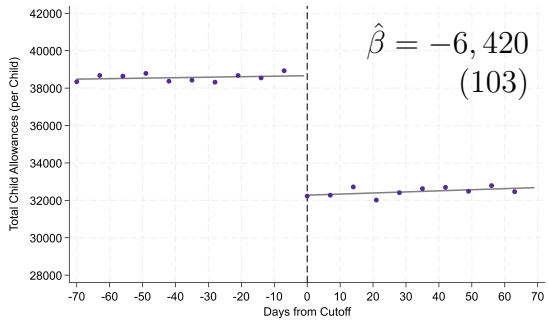
Notes - Panel (a) and (b) presents the distributional density of four or higher parity births around the reform cutoff for the Arab and Jewish populations (McCrary, 2008; Cattaneo et al., 2018). The number of observations and the p-value for the significance of the discontinuity at 0 is listed just below the graph.

Figure 4: The Effect of the 2003 Cutoff Rule by Ethnicity

(a) Jewish Families

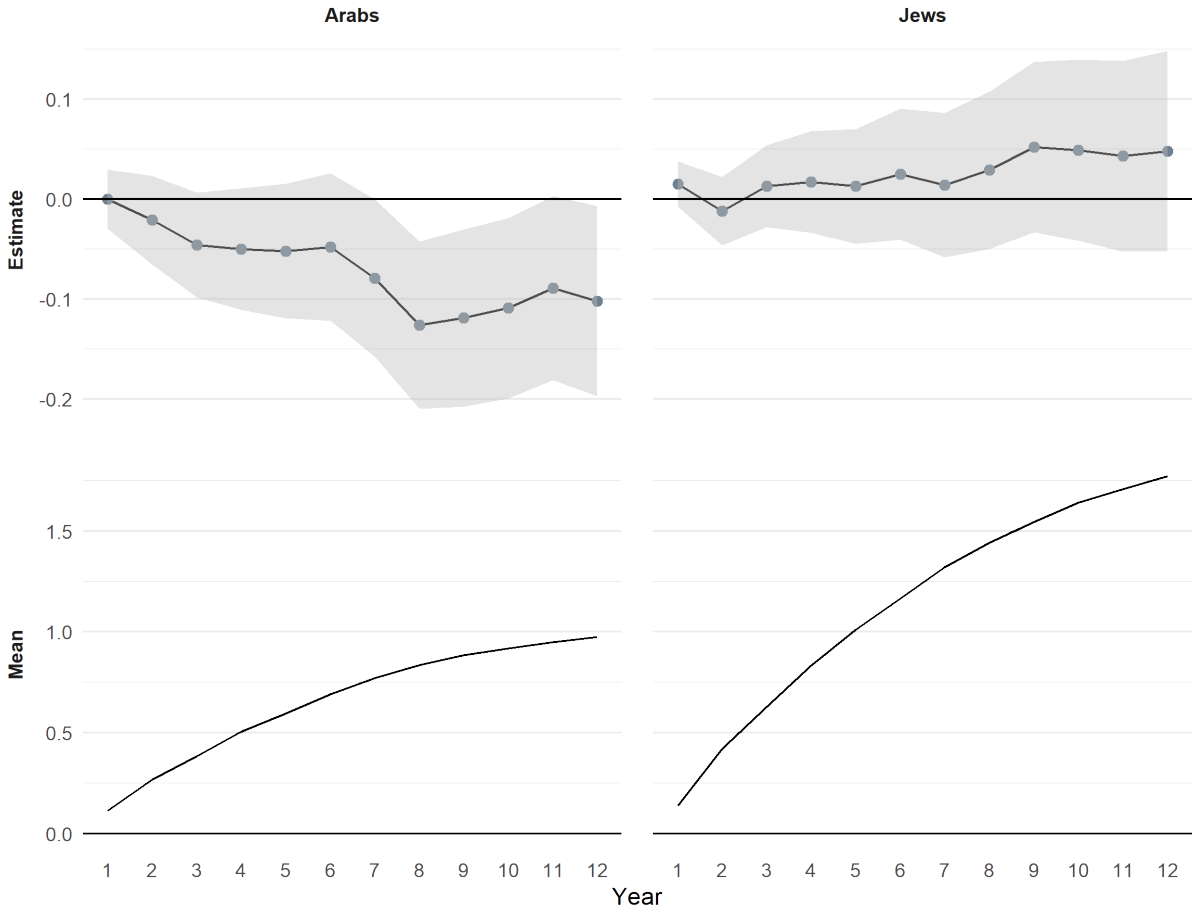


(b) Arab Families



Notes - The figures display the mean child allowance per child against the birthdate of each family's fourth or higher parity child who was born in a 10 weeks bandwidth around the June 1, 2003 cutoff, with the x-axis centered at the cutoff. Panel (a) focuses on Jewish families while Panel (b) shows only Arab families. The sample for each ethnicity contains both the child born around the cutoff and their older siblings. 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. Child allowance per child is calculated as follows: For each household-year we calculate child allowances, which are determined by the number and birthdates of children in the household that year. This annual household total is divided by the number of children under 18 in that year. For each child, these annual per-child amounts are then summed across all years until the child reaches age 18.

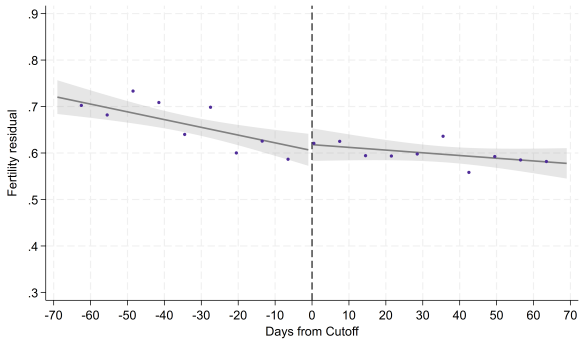
Figure 5: The Dynamic Effect of Reduction in Transfers on Fertility



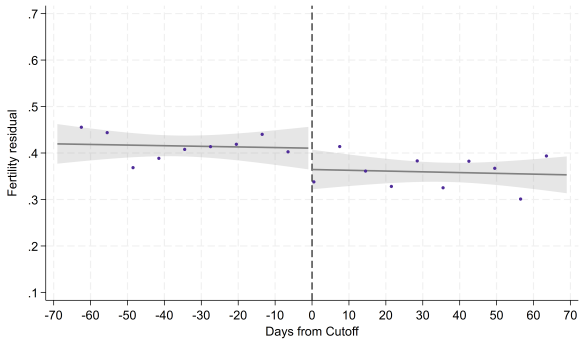
Notes - The figure presents estimates of the effect of reduced child allowances on the number of subsequent children born after 2003 for each year following 2003, separately for Jews and Arabs. The estimates are obtained using the RD model in equation 1. The corresponding means are presented below the estimates, separately for each group. The gray areas indicate 90% confidence intervals.

Figure 6: Effect of Reduction in Transfers on Short Term and Completed Fertility

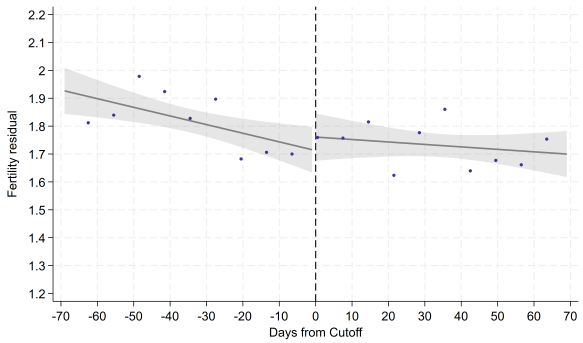
(a) Short Term: Jewish



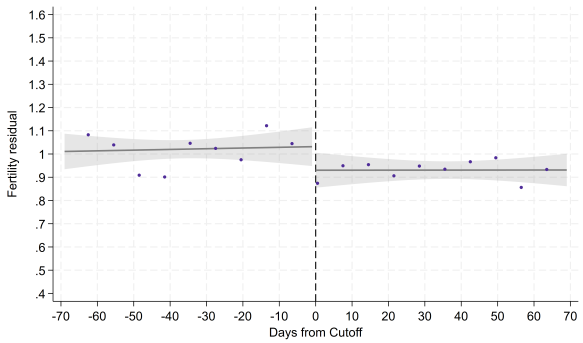
(b) Short Term: Arab



(c) Long Term: Jewish



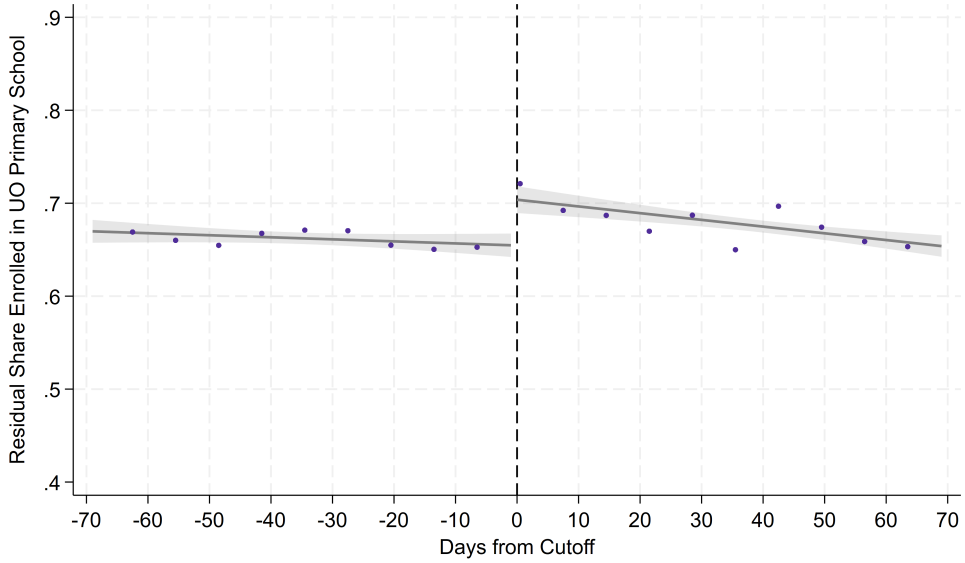
(d) Long Term: Arab



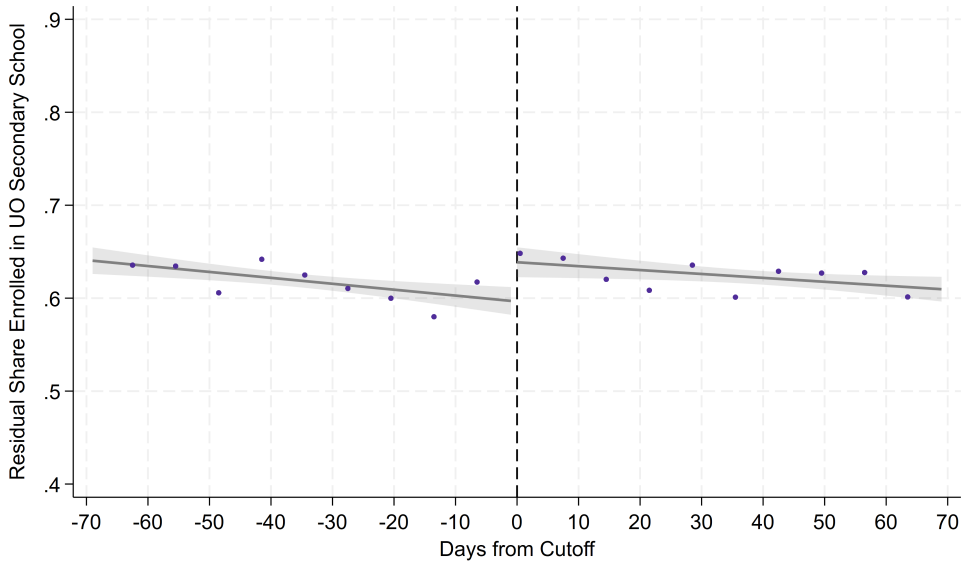
Notes - The figure displays residual fertility against the birthdate of each mother's fourth or higher parity child who was born in a 10 weeks bandwidth around the June 1, 2003 cutoff, with the x-axis centered at the cutoff. Panels (a) and (b) show short-term fertility, defined as the number of subsequent children born up to 2005. Panels (c) and (d) show long-term fertility, defined as the number of subsequent children born up to 2015. The samples in Panels (a) and (c) contain Jewish mothers while the samples in Panels (b) and (d) contain Arab mothers. Residuals are obtained by regressing the fertility outcome on the set of controls in equation 1, where the sample mean is added to the residual.

Figure 7: Effect of Reduction in Transfers on UO Enrollment

(a) Grades 1-8

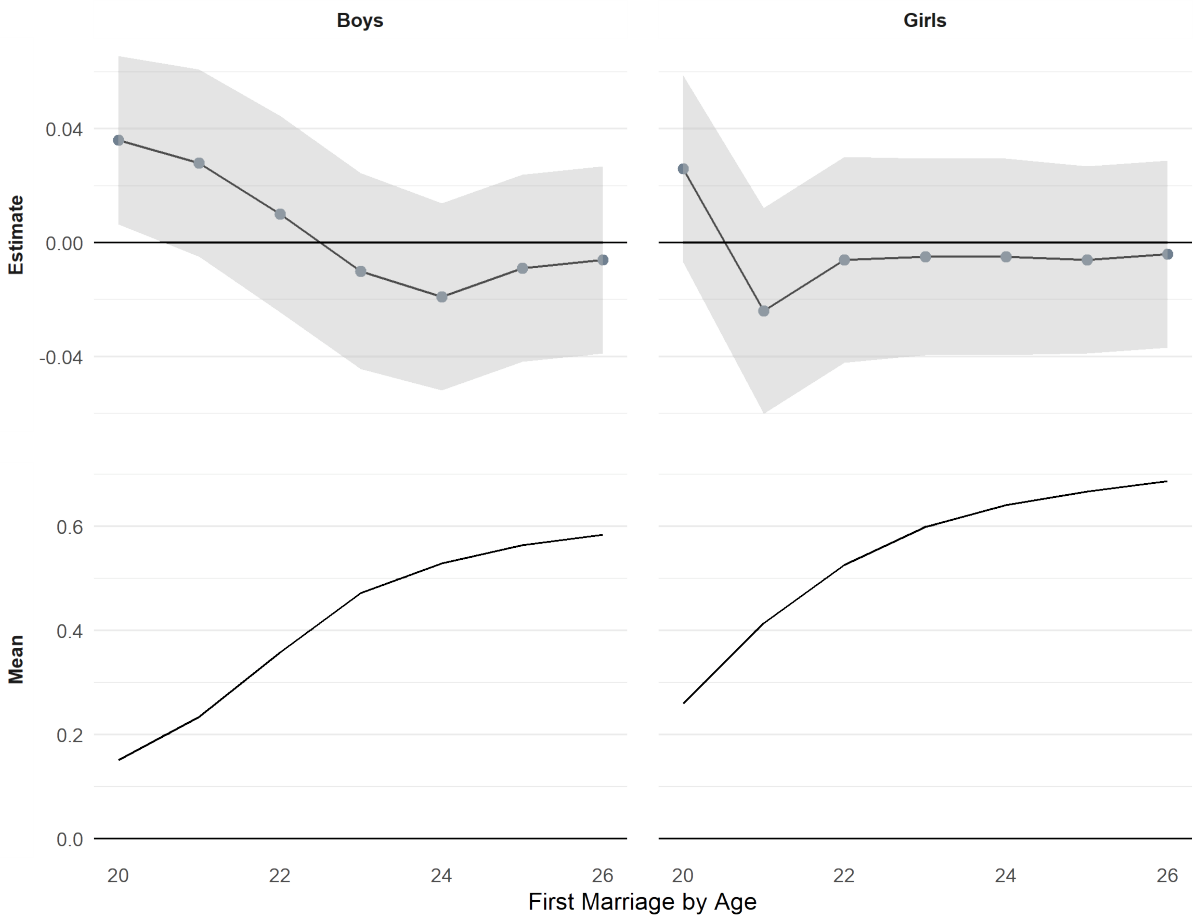


(b) Grades 9-12



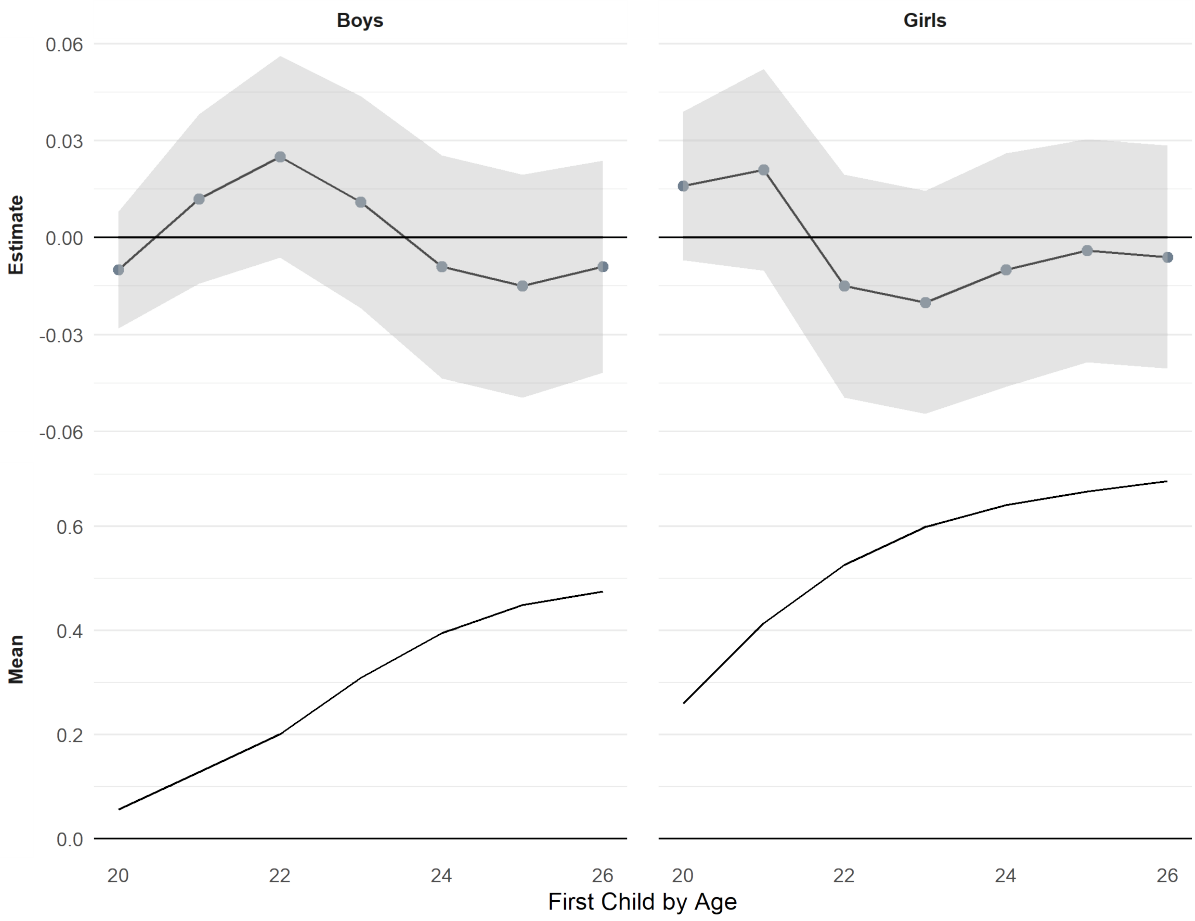
Notes - The figure displays residual enrollment in UO schools against the birthdate of each family's fourth or higher parity child who was born in a 10 weeks bandwidth around the June 1, 2003 cutoff, with the x-axis centered at the cutoff. The sample includes elementary-school-aged (5-12) older siblings of the pivotal child (born in 2003). Panels (a) presents ever enrollment in UO school during grades 1 to 8 while Panel (b) shows ever enrollment in grades 1 to 9. Residuals are obtained by regressing the enrollment outcomes on the set of controls in equation 1, where the sample mean is added to the residual.

Figure 8: Effects of Reduction in Transfers on Marrying in Early Adulthood (Jewish children age 5-12 in 2003)



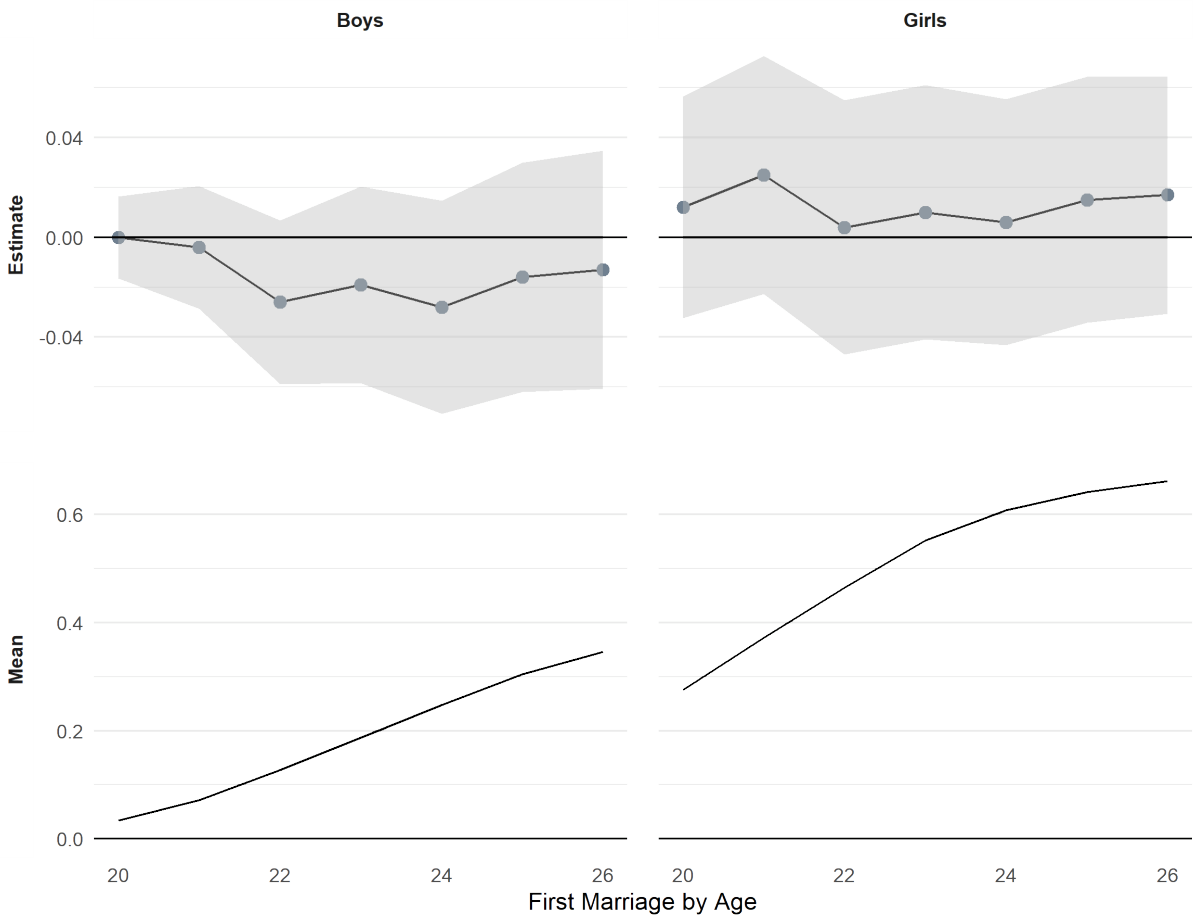
Notes: The figure presents estimates of the effect of reduced child allowances on an indicator for being married between the ages of 20-26 for Jewish individuals who were 5-12 years old in 2003. The estimates are obtained using the RD model in equation 1. The corresponding means are presented below the estimates, separately for each group. The gray areas indicate 90% confidence intervals.

Figure 9: Effects of Reduction in Transfers on Having a First Child in Early Adulthood (Jewish children age 5-12 in 2003)



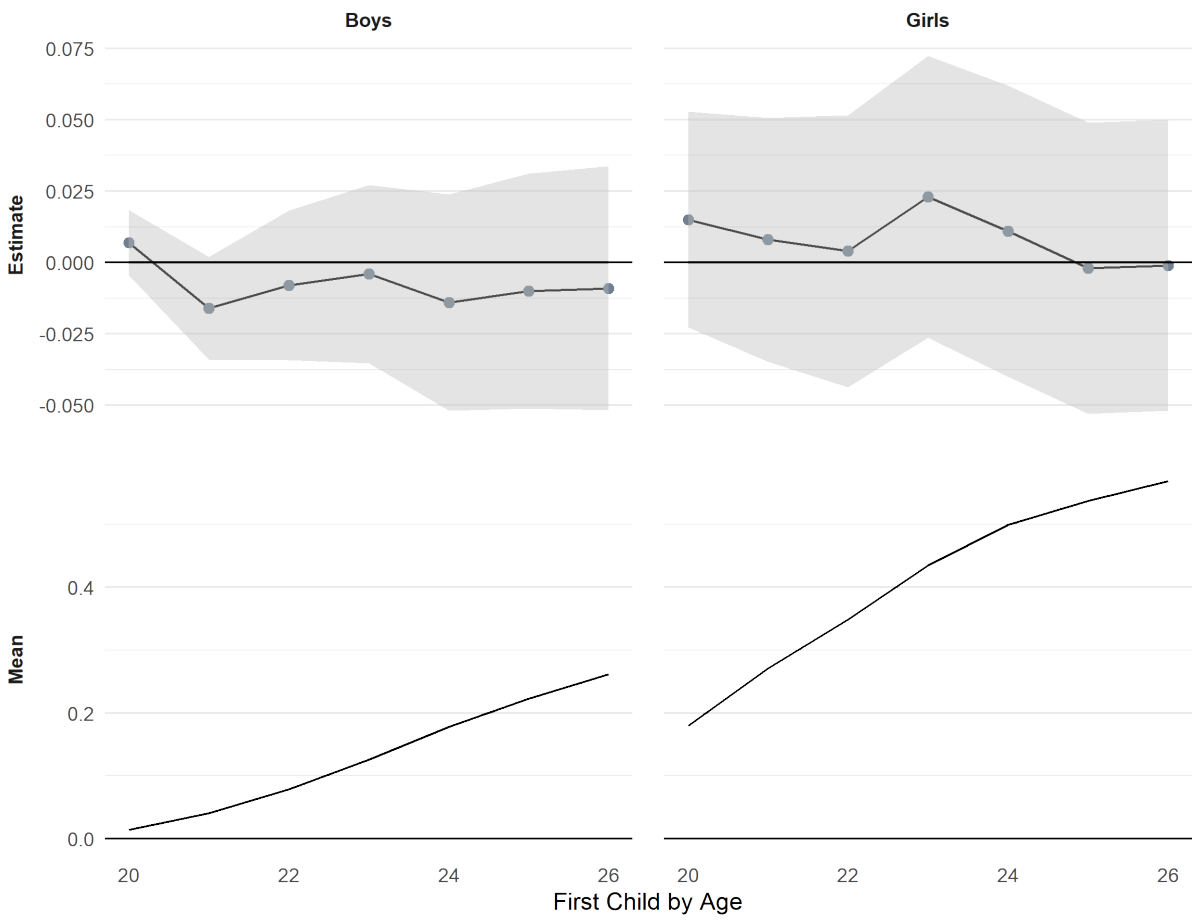
Notes: The figure presents estimates of the effect of reduced child allowances on an indicator for having a first child between the ages of 20-26 for Jewish individuals who were 5-12 years old in 2003. The estimates are obtained using the RD model in equation 1. The corresponding means are presented below the estimates, separately for each group. The gray areas indicate 90% confidence intervals.

Figure 10: Effects of Reduction in Transfers on Marrying in Early Adulthood (Arab children age 5-12 in 2003)



Notes: The figure presents estimates of the effect of reduced child allowances on an indicator for being married between the ages of 20-26 for Arab individuals who were 5-12 years old in 2003. The estimates are obtained using the RD model in equation 1. The corresponding means are presented below the estimates, separately for each group. The gray areas indicate 90% confidence intervals.

Figure 11: Effects of Reduction in Transfers on Having a First Child in Early Adulthood (Arabs children age 5-12 in 2003)



Notes: The figure presents estimates of the effect of reduced child allowances on an indicator for having a first child between the ages of 20-26 for Arab individuals who were 5-12 years old in 2003. The estimates are obtained using the RD model in equation 1. The corresponding means are presented below the estimates, separately for each group. The gray areas indicate 90% confidence intervals.

Table 1: Allowance Schedule (2021 NIS)

Year	2001	2002	2003		2004		2005		2009		2012	
Birth Order among Children under Age 18			Children Born before Jun-03	Children Born after Jun-03	Children Born before Jun-03	Children Born after Jun-03	Children Born before Jun-03	Children Born after Jun-03	Children Born before Jun-03	Children Born after Jun-03	Children Born before Jun-03	Children Born after Jun-03
1	228	192	182	182	154	154	149	149	178	178	179	179
2	228	192	182	182	154	154	149	149	178	178	267	267
3	457	381	313	182	213	154	194	149	248	212	300	267
4	926	774	666	182	527	154	447	149	447	230	468	267
5+	1142	956	802	182	605	154	498	149	395	178	398	179

Notes - The table reports allowance schedule for selected years by birth order. The allowance amounts are in 2021 NIS.

Table 2: Descriptive Statistics, Families with Births in 2003

		4th and higher parity births			
			Birth +/- 70 Days from June 1, 2003		
	(1)	(2)	(3)	(4)	(5)
Jews	0.724	0.628	0.630		
	(0.447)	(0.483)	(0.483)		
Arabs	0.276	0.372	0.370		
	(0.447)	(0.483)	(0.483)		
Jewish UO before 2003	0.163	0.341	0.344	0.547	
	(0.369)	(0.474)	(0.475)	(0.498)	
Father's age in 2003	32.692	36.427	36.480	36.580	36.311
	(6.260)	(5.902)	(5.844)	(5.864)	(5.807)
Mother's age in 2003	29.257	32.880	32.890	33.487	31.874
	(5.492)	(4.836)	(4.814)	(4.764)	(4.728)
Father's months of employment in 2000	7.480	6.292	6.264	5.817	7.024
	(5.319)	(5.637)	(5.648)	(5.748)	(5.390)
Mother's months of employment in 2000	5.809	4.035	4.022	5.545	1.432
	(5.293)	(5.198)	(5.198)	(5.407)	(3.550)
Father's labor earnings in 2000	84,995	65,868	66,683	74,534	53,325
	(198,979)	(108,999)	(109,375)	(128,020)	(64,420)
Mother's labor earnings in 2000	38,412	24,408	25,174	36,187	6,436
	(97,552)	(87,876)	(125,193)	(155,947)	(20,144)
Mother's age at first birth	25.306	22.266	22.258	22.676	21.545
	(4.889)	(3.179)	(3.212)	(3.050)	(3.354)
Number of siblings under age 5 in 2003	0.757	1.328	1.297	1.364	1.182
	(0.801)	(0.924)	(0.906)	(0.902)	(0.900)
Number of siblings age 5–9 in 2003	0.631	1.801	1.820	1.829	1.804
	(0.952)	(0.987)	(0.978)	(0.996)	(0.948)
Number of siblings age 10–13 in 2003	0.253	0.868	0.873	0.905	0.818
	(0.638)	(0.980)	(0.983)	(0.994)	(0.962)
Number of siblings age 14–17 in 2003	0.115	0.409	0.407	0.433	0.363
	(0.448)	(0.795)	(0.795)	(0.817)	(0.755)
Number of children	2.739	5.342	5.328	5.442	5.135
	(1.827)	(1.550)	(1.551)	(1.644)	(1.359)
Twin birth	0.023	0.029	0.031	0.036	0.021
	(0.149)	(0.168)	(0.172)	(0.187)	(0.142)
Male birth	0.512	0.514	0.511	0.521	0.495
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Locality SES	4.521	3.330	3.318	3.910	2.310
	(2.439)	(2.085)	(2.083)	(2.285)	(1.108)
Observations	130,401	32,708	11,606	7,310	4,296

Notes: The table reports summary statistics for families with a birth in 2003. Column 2 reports statistics for the subsample of families with a fourth or higher parity birth. In Column 3 the sample is further restricted to families with a birth in a 70-day bandwidth around the cutoff. Columns 4 and 5 separate the sample to Jewish and Arab populations, respectively. The values of all variables refer to 2003 unless indicated otherwise. Standard deviations are reported in parentheses.

Table 3: Balancing Tests, Families with 4+ Children (in 2003)

	All (1)	Jews (2)	Arabs (3)
Jews	-0.021		
	(0.018)		
Arabs	0.021		
	(0.018)		
Jewish Orthodox before 2003	-0.002	0.014	
	(0.018)	(0.023)	
Father's age in 2003	0.217	0.213	0.236
	(0.224)	(0.280)	(0.375)
Mother's age in 2003	0.214	0.154	0.418
	(0.180)	(0.223)	(0.297)
Father's months of employment in 2000	0.042	0.097	-0.122
	(0.213)	(0.270)	(0.338)
Mother's months of employment in 2000	0.102	0.157	0.244
	(0.196)	(0.254)	(0.218)
Father's labor earnings in 2000	522	1,318	294
	(3,982)	(5,770)	(3,948)
Mother's labor earnings in 2000	7,243	11,614	1,055
	(7,157)	(11,073)	(1,269)
Mother's age at first birth	0.068	0.131	0.022
	(0.121)	(0.143)	(0.212)
Number of siblings under age 5 in 2003	0.026	0.077*	-0.055
	(0.034)	(0.043)	(0.055)
Number of siblings age 5–9 in 2003	-0.040	-0.047	-0.025
	(0.037)	(0.047)	(0.059)
Number of siblings age 10–13 in 2003	0.054	0.040	0.084
	(0.037)	(0.046)	(0.061)
Number of siblings age 14–17 in 2003	0.048	0.024	0.093**
	(0.029)	(0.038)	(0.047)
Number of children	0.064	0.064	0.085
	(0.058)	(0.076)	(0.086)
Twin Birth	0.003	0.002	0.005
	(0.006)	(0.008)	(0.009)
Male birth	0.011	0.022	-0.007
	(0.019)	(0.024)	(0.032)
Locality SES	-0.118	-0.066	-0.118*
	(0.079)	(0.107)	(0.068)
Observations	11,606	7,310	4,296

Notes: The table reports results from regressions testing for balance of observable characteristics within the 70-day bandwidth. Column 1 reports the results for the entire population. Columns 2 and 3 report results for the Jewish and Arab sub-populations, respectively. The values of all variables refer to 2003 unless indicated otherwise. Standard errors are in parentheses. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 4: Short and Long Term Adjustments to Reduction in Transfers

	Subsequent Children		Months Employed 2004-2006		Months Employed 2013-2015	
	2004-2006 (1)	Up to 2015 (2)	Fathers (3)	Mothers (4)	Fathers (5)	Mothers (6)
Panel A: Jewish						
Treatment	0.013	0.048	-0.730	-0.643	0.043	-0.607
	(0.025)	(0.061)	(0.609)	(0.535)	(0.653)	(0.621)
Control mean	0.629	1.773	20.615	16.999	22.878	23.722
Observations	7,310	7,310	7,310	7,310	7,310	7,310
Panel B: Arab						
Treatment	-0.046	-0.102*	0.891	0.132	1.809**	-0.147
	(0.032)	(0.058)	(0.768)	(0.483)	(0.833)	(0.769)
Control mean	0.386	0.975	23.079	4.708	23.011	9.459
Observations	4,296	4,296	4,296	4,296	4,296	4,296

Notes: The table reports the effects of reducing child allowances on the number of subsequent children in the household and on the cumulative number of months employed by each of the parents. The sample includes families whose 4th (or higher parity) child was born in 2003. Panel A reports the effects for Jews, while Panel B reports the effects for Arabs. Columns 1, 3, and 4 report the effect within 3 years of the reduction in transfers, whereas columns 2, 5, and 6 report the effect 10 to 12 years after the reduction in transfers. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4th child prior to June 1, 2003. Robust standard errors are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 5: Effect of Reduction in Transfers on UO School Enrollment

	Ages 5-12		Ages 0-4	
	Grades 1-8	Grades 9-12	Grades 1-8	Grades 9-12
	(1)	(2)	(3)	(4)
Panel A: All				
Treatment	0.027***	0.032***	0.013	0.005
	(0.010)	(0.012)	(0.014)	(0.015)
Control mean	0.670	0.616	0.684	0.622
Observations	18,079	18,079	16,864	16,864
Panel B: Boys				
Treatment	0.049***	0.042***	0.008	0.013
	(0.013)	(0.014)	(0.016)	(0.017)
Control mean	0.670	0.621	0.686	0.621
Observations	9,261	9,261	8,676	8,676
Panel C: Girls				
Treatment	0.006	0.023	0.020	-0.003
	(0.013)	(0.015)	(0.017)	(0.019)
Control mean	0.669	0.611	0.683	0.623
Observations	8,818	8,818	8,188	8,188

Notes: The table reports the effects of reducing child allowances on the probability of enrolling Jewish children in ultra-Orthodox (UO) schools at grades 1 to 8 (columns 1 and 3) and at grades 9 to 12 (columns 2 and 4). The sample includes Jewish families whose 4th (or higher parity) child was born in 2003. Panel A reports the effects for both boys and girls, while Panels B and C report the effects by gender. Columns 1 and 2 report the effects for children ages 5-12 in 2003, whereas columns 3 and 4 report the effects for children ages 0-4. All regressions include the controls listed in Equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4th child before June 1, 2003. Standard errors clustered at the household level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 6: Effect of Reduction in Transfers on Educational Attainment of Jewish Children

	Ages 5-12			Ages 1-4		
	Bagrut Diploma (1)	HS Dropout (2)	Bagrut Credits (3)	Bagrut Diploma (4)	HS Dropout (5)	Bagrut Credits (6)
Panel A: All						
Treatment	-0.030**	0.019**	-1.259***	-0.014	-0.006	-0.484
	(0.013)	(0.009)	(0.360)	(0.018)	(0.011)	(0.466)
Control mean	0.287	0.057	10.147	0.304	0.067	9.319
Observations	18,079	18,079	18,079	9,411	9,411	9,411
Panel B: Boys						
Treatment	-0.037**	0.025**	-1.890***	-0.019	-0.004	-0.659
	(0.016)	(0.012)	(0.454)	(0.020)	(0.017)	(0.567)
Control mean	0.215	0.069	7.983	0.195	0.080	6.344
Observations	9,261	9,261	9,261	4,813	4,813	4,813
Panel C: Girls						
Treatment	-0.023	0.011	-0.615	-0.013	-0.007	-0.461
	(0.018)	(0.010)	(0.482)	(0.027)	(0.014)	(0.666)
Control mean	0.364	0.045	12.422	0.421	0.052	12.509
Observations	8,818	8,818	8,818	4,598	4,598	4,598

Notes: The table reports the effects of reducing child allowances on the educational attainment of Jewish children. The sample consists of families whose 4th (or higher parity) child was born in 2003. Panel A reports the effects for both boys and girls, while Panels B and C report the effects by gender. Columns 1 to 3 report the effects for children ages 5-12 in 2003, whereas columns 4 to 6 report the effects for children ages 1-4. Children born in 2003 (i.e., those who were age 0 in 2003) are excluded, as they reached grade 12 in 2021, a year for which we have no data. Columns 1 and 4 report results on matriculating high school by obtaining a Bagrut diploma. Columns 2 and 5 report results on dropping out of high school before the completion of twelve years of schooling. Columns 3 and 6 report results on the quality of the diploma by examining the unconditional overall number of Bagrut units. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4th child prior to June 1, 2003. Standard errors clustered at the household level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 7: Effect of Reduction in Transfers on Jewish Children's Long-Term Outcomes

	Any Post-Secondary (1)	Any Yeshiva (2)	Months Employed (Annual) (3)	Ever Employed (4)
Panel A: Boys				
Treatment	-0.042**	0.025	0.037	0.004
	(0.017)	(0.020)	(0.152)	(0.020)
Control mean	0.213	0.656	3.442	0.730
Observations	9,261	9,261	9,261	9,261
Panel B: Girls				
Treatment	-0.034		0.114	-0.008
	(0.021)		(0.134)	(0.007)
Control mean	0.382		7.698	0.979
Observations	8,818		8,818	8,818

Notes: The table reports the effects of reducing child allowances on long term outcomes of Jewish children aged five to twelve at the time of the reduction in transfers (2003). The sample includes families whose fourth (or higher parity) child was born in 2003. Panel A presents results for boys, while Panel B presents results for girls. Columns 1, 2, and 4 report effects on ever attending postsecondary schooling, ever attending Yeshiva, or ever being employed from age 19 to 26 (or last available year), respectively. Column 3 reports the effect on average annual months employed between age 19 to 26 (or last available year). All regressions include controls as specified in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4th child prior to June 1, 2003. Standard errors clustered at the household level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 8: Effect of Reduction in Transfers on Educational Attainment of Arab Children

	Ages 5-12			Ages 1-4		
	Bagrut Diploma (1)	HS Dropout (2)	Bagrut Credits (3)	Bagrut Diploma (4)	HS Dropout (5)	Bagrut Credits (6)
Panel A: All						
Treatment	-0.013	-0.015	0.237	0.001	-0.057**	0.786
	(0.021)	(0.020)	(0.565)	(0.029)	(0.026)	(0.730)
Control mean	0.357	0.206	13.190	0.406	0.195	13.679
Observations	9,736	9,736	9,736	4,682	4,682	4,682
Panel B: Boys						
Treatment	0.000	-0.013	0.361	-0.025	-0.024	-0.138
	(0.026)	(0.030)	(0.709)	(0.039)	(0.041)	(0.989)
Control mean	0.238	0.287	9.512	0.280	0.274	9.589
Observations	4,791	4,791	4,791	2,166	2,166	2,166
Panel C: Girls						
Treatment	-0.024	-0.011	-0.003	0.012	-0.078***	1.213
	(0.030)	(0.023)	(0.717)	(0.039)	(0.029)	(0.953)
Control mean	0.468	0.131	16.643	0.513	0.128	17.167
Observations	4,945	4,945	4,945	2,516	2,516	2,516

Notes: The table reports the effects of reducing child allowances on the educational attainment of Arab children. The sample consists of families whose 4th (or higher parity) child was born in 2003. Panel A reports the effects for both boys and girls, while Panels B and C report the effects by gender. Columns 1 to 3 report the effects for children ages 5-12 in 2003, whereas columns 4 to 6 report the effects for children ages 1-4. Children born in 2003 (i.e., those who were age 0 in 2003) are excluded, as they reached grade 12 in 2021, a year for which we have no data. Columns 1 and 4 report results on matriculating high school by obtaining a Bagrut diploma. Columns 2 and 5 report results on dropping out of high school before the completion of twelve years of schooling. Columns 3 and 6 report results on the quality of the diploma by examining the unconditional overall number of Bagrut units. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4th child prior to June 1, 2003. Standard errors clustered at the household level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

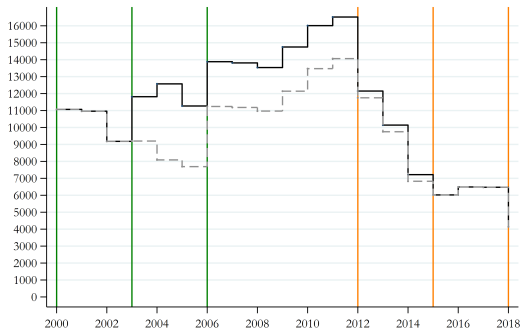
Table 9: Effect of Reduction in Transfers on Arab Children's Long-Term Outcomes

	Any Post-Secondary (1)	Months Employed (Annual) (2)	Ever Employed (3)
Panel A: Boys			
Treatment	-0.006	0.457**	0.000
	(0.024)	(0.230)	(0.011)
Control mean	0.196	7.988	0.971
Observations	4,791	4,791	4,791
Panel B: Girls			
Treatment	0.010	0.238	0.004
	(0.031)	(0.236)	(0.022)
Control mean	0.336	4.828	0.819
Observations	4,945	4,945	4,945

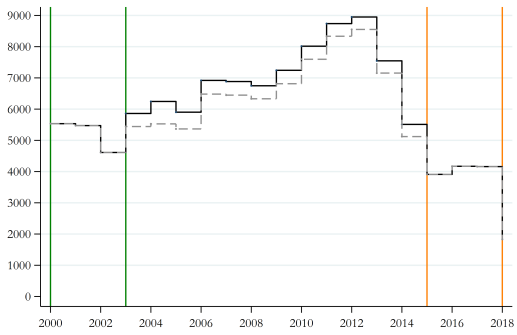
Notes: The table reports the effects of reducing child allowances on long term outcomes of Arab children aged five to twelve at the time of the reduction in transfers (2003). The sample includes families whose fourth (or higher parity) child was born in 2003. Panel A presents results for boys, while Panel B presents results for girls. Columns 1 and 3 report effects on ever attending postsecondary schooling and employment status from age 19 to 26 (or last available year), respectively. Column 2 reports the effect on average annual months employed from age 19 to 26 (or last available year). All regressions include controls as specified in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4th child prior to June 1, 2003. Standard errors clustered at the household level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Figure A.1: Household Child Allowance Stream 2000-2018

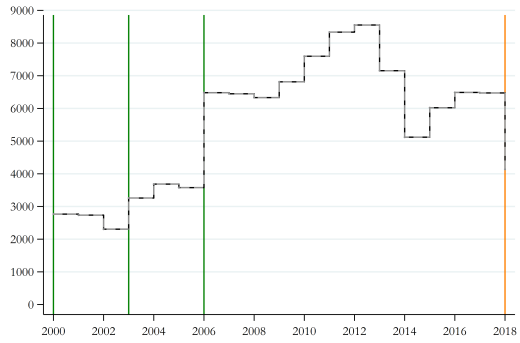
(a) 4th in 2003 and 5th in 2006, gap NIS 27,300



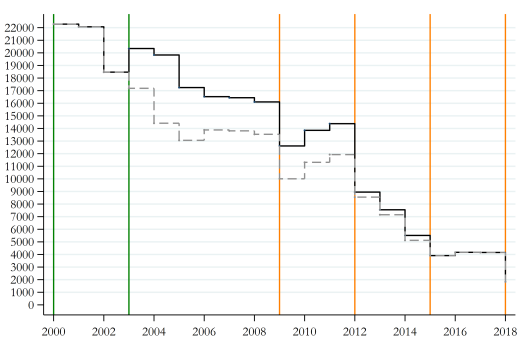
(c) 3rd in 2003, gap NIS 5,397



(b) 2nd in 2003, gap NIS 0

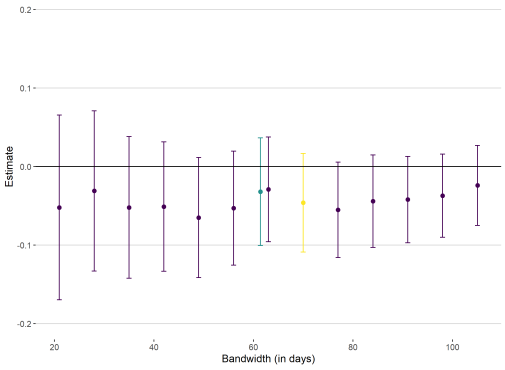


(d) 5th in 2003, gap NIS 29,382

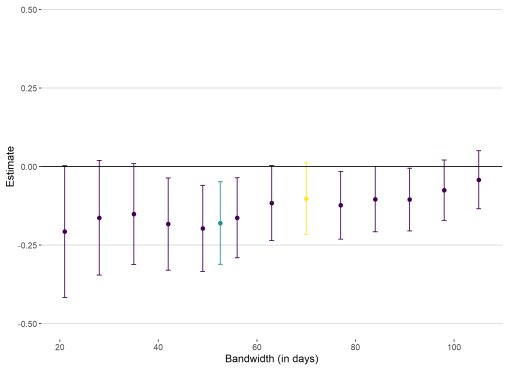


Notes: Y-axis shows the yearly child allowance to be received in NIS by the simulated sibling composition. All amounts are in 2021 NIS. Solid and dashed line indicates the amount when the birth in 2003 occurred pre-June (untreated) and post-June (treated), respectively. The vertical orange line indicates that a child reached the age of 18 and the mother no longer receives an allowance for that child. The green vertical line indicates a birth. A three years spacing between births is assumed.

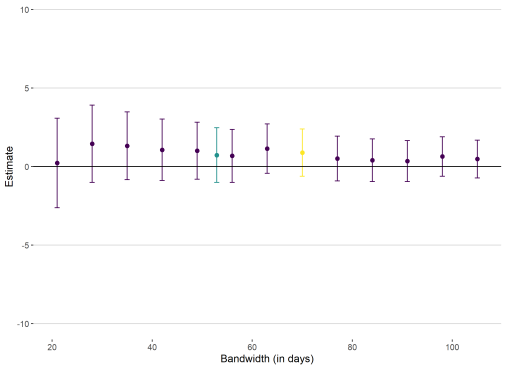
Figure A.2: Bandwidth Robustness of Fertility and Labor Adjustments Among Arab Families



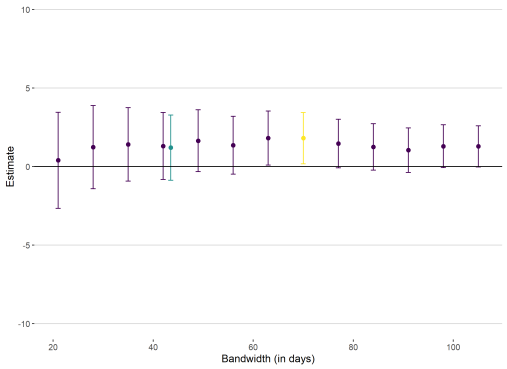
(a) Fertility 2004-2006



(b) Fertility Up to 2015



(c) Months Employed 2004-2006 (Fathers)

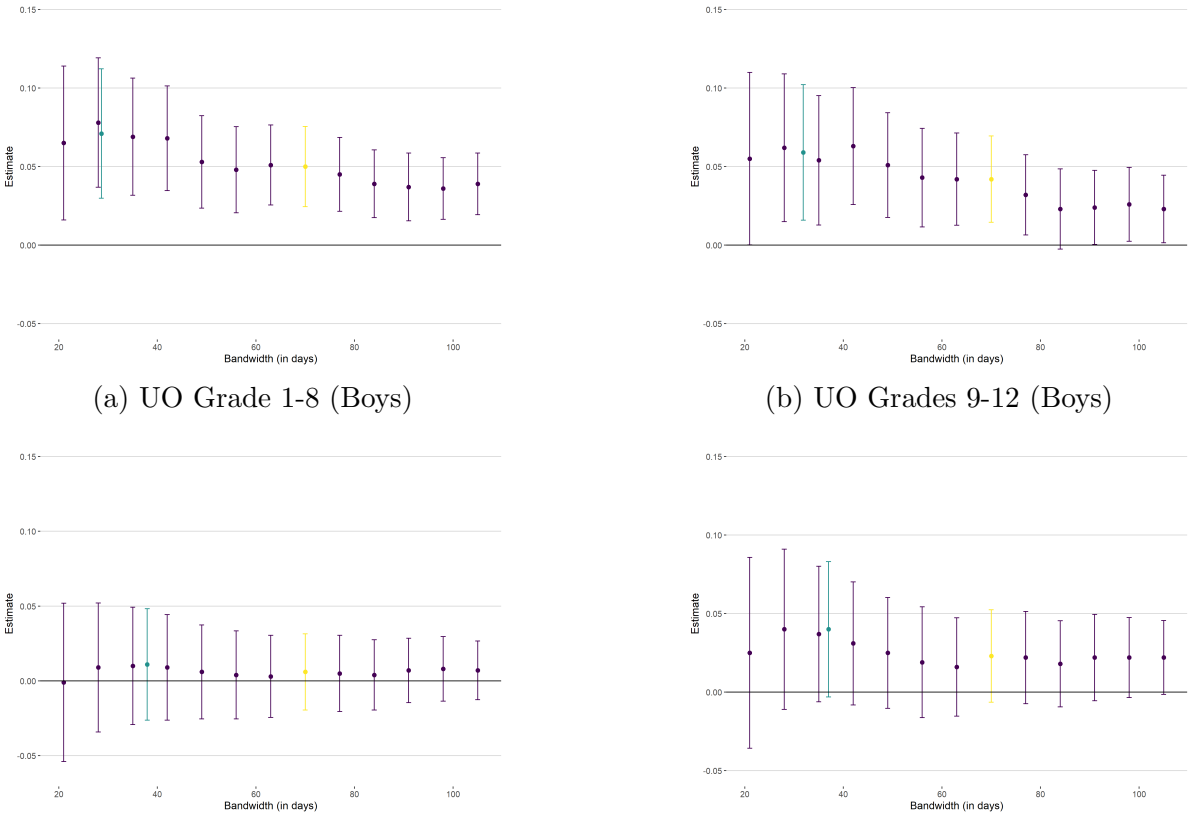


(d) Months Employed 2013-2015 (Fathers)

● Automatic bandwidth ● Fixed bandwidth

Notes: The figure reports robustness checks for Arab parents of reducing child allowances on several outcomes. Bandwidths vary from 21 to 105 days, as depicted on the horizontal axis. Panels (a) and (b) report short- and long-term fertility adjustments, while Panels (c) and (d) report short- and long-term employment adjustments for Arab fathers. The preferred coefficient based on the 70-day bandwidth is reported in yellow, and the Calonico et al. (2020) automatic bandwidth is plotted in green.

Figure A.3: Bandwidth Robustness of Schooling Stream Adjustments Among Jewish Children Ages 5-12



(a) UO Grade 1-8 (Boys)

(b) UO Grades 9-12 (Boys)

(c) UO Grade 1-8 (Girls)

(d) UO Grades 9-12 (Girls)

• Automatic bandwidth • Fixed bandwidth

Notes: The figure reports robustness checks for Jewish families' schooling stream adjustments from reducing child allowances. Bandwidths vary from 21 to 105 days, as depicted on the horizontal axis. Panels (a) and (b) report schooling stream adjustments for boys, while Panels (c) and (d) report schooling stream adjustments for girls. The preferred coefficient based on the 70-day bandwidth is reported in yellow, and the Calonico et al. (2020) automatic bandwidth is plotted in green.

Table A.1: Short and Long Term Labor Earnings Effects

	Labor Earnings 2004-2006		Labor Earnings 2013-2015	
	Fathers (1)	Mothers (2)	Fathers (3)	Mothers (4)
Panel A: Jewish				
Treatment	-585	-238	-10,672	-5,458
	(8,228)	(4,407)	(11,648)	(7,065)
Control mean	206,621	105,571	285,277	189,107
Observations	7,310	7,310	7,310	7,310
Panel B: Arab				
Treatment	4,871	-986	12,120	-2,804
	(7,044)	(1,908)	(10,514)	(3,969)
Control mean	157,557	17,926	200,050	44,655
Observations	4,296	4,296	4,296	4,296

Notes: The table reports the effects of reducing child allowances on labor earnings of each of the parents. The sample includes families whose 4th (or higher parity) child was born in 2003. Panel A reports the effects for Jews, while Panel B reports the effects for Arabs. Columns 1 and 2 report the effect within 3 years of the reduction in transfers, whereas columns 3 and 4 report the effect 10 to 12 years after the reduction in transfers. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4th child prior to June 1, 2003. Robust standard errors are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.2: Robustness Tests for Short Term Adjustments Among Arab Parents on Fertility and Employment

	Subsequent Children		Months Employed 2004-2006		Months Employed 2013-2015	
	2004-2006	Up to 2015	Fathers	Mothers	Fathers	Mothers
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Kernel						
Treatment	-0.047*	-0.148**	0.931	-0.033	1.562*	-0.259
	(0.027)	(0.063)	(0.828)	(0.524)	(0.902)	(0.844)
Control mean	0.386	0.975	23.079	4.708	23.011	9.459
Observations	4,296	4,296	4,296	4,296	4,296	4,296
Panel B: Donut						
Treatment	-0.046	-0.101	1.006	0.228	2.313**	-0.520
	(0.037)	(0.067)	(0.869)	(0.540)	(0.937)	(0.865)
Control mean	0.386	0.976	23.047	4.623	22.961	9.373
Observations	4,066	4,066	4,066	4,066	4,066	4,066

Notes: The table reports robustness checks for Arab parents of reducing child allowances on the number of subsequent children in the household and on the cumulative number of months employed by each of the parents. The sample includes families whose 4th (or higher parity) child was born in 2003. Panel A reports estimates using a triangular kernel function, while Panel B reports estimates obtained after removing observations in an eight-day donut hole around the cutoff (four days on each side). Columns 1, 3, and 4 report the effect within 3 years of the reduction in transfers, whereas columns 2, 5, and 6 report the effect 10 to 12 years after the reduction in transfers. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. Robust standard errors are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.3: Robustness Tests for Short Term Adjustments on UO enrollment

	Triangular Kernel		Donut	
	Grades 1-8	Grades 9-12	Grades 1-8	Grades 9-12
	(1)	(2)	(3)	(4)
Panel A: All				
Treatment	0.031***	0.036***	0.030**	0.037***
	(0.012)	(0.013)	(0.012)	(0.013)
Control mean	0.670	0.616	0.671	0.618
Observations	18,079	18,079	17,083	17,083
Panel B: Boys				
Treatment	0.054***	0.047***	0.052***	0.049***
	(0.014)	(0.016)	(0.014)	(0.016)
Control mean	0.670	0.621	0.672	0.624
Observations	9,261	9,261	8,761	8,761
Panel C: Girls				
Treatment	0.007	0.024	0.008	0.028
	(0.015)	(0.017)	(0.015)	(0.017)
Control mean	0.669	0.611	0.669	0.611
Observations	8,818	8,818	8,322	8,322

Notes: The table reports results from a series of robustness checks for the results in Table 5, which reports the effects of the reduction in child allowances on the probability of enrolling Jewish children in Ultraorthodox (UO) schools at grades 1 to 8 (Columns 1 and 3) and at grades 9 to 12 (Columns 2 and 4). The sample includes families whose 4th (or higher parity) child was born in 2003. Panel A reports the effects for both boys and girls, while Panels B and C report the effects by gender. Columns 1 and 2 report the results using triangular kernel weighting, while columns 3 and 4 report the results using an eight-day donut hole (four days on each side). Standard errors clustered at the household level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.4: Placebo Tests for Short Term Adjustments on UO enrollment

	2002 Births		Parity 2		Ages 13+	
	Grades 1-8	Grades 9-12	Grades 1-8	Grades 9-12	Grades 1-8	Grades 9-12
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All						
Treatment	-0.004	0.002	-0.005	-0.005	0.018	0.014
	(0.010)	(0.012)	(0.017)	(0.013)	(0.034)	(0.020)
Control mean	0.674	0.624	0.062	0.034	0.571	0.535
Observations	17,529	17,529	1,684	1,684	2,196	2,196
Panel B: Boys						
Treatment	-0.001	0.013	-0.033	0.011	-0.011	0.020
	(0.012)	(0.014)	(0.025)	(0.014)	(0.046)	(0.026)
Control mean	0.681	0.627	0.070	0.029	0.541	0.507
Observations	8,904	8,904	836	836	997	997
Panel C: Girls						
Treatment	-0.006	-0.010	0.020	-0.019	0.028	0.007
	(0.012)	(0.015)	(0.024)	(0.023)	(0.044)	(0.030)
Control mean	0.668	0.620	0.053	0.039	0.597	0.560
Observations	8,625	8,625	848	848	1,199	1,199

Notes: The table reports a series of placebo tests for the results in Table 5 which reports the effects of the reduction in child allowances on the probability of enrolling Jewish children in Ultraorthodox (UO) schools at grades 1 to 8 (Columns 1, 3 and 5) and at grades 9 to 12 (Columns 2, 4 and 6). Panel A reports the effects for both boys and girls, while Panel B and C report the effects by gender. The sample for Columns 1 and 2 report the results of a placebo exercise using the arbitrary cutoff date of June 2, 2002—one year before the actual cutoff and adjusted by one day to align with the same day of the week. Columns 3 and 4 report the results of placebo exercise using families whose 2nd child was born in 2003 (1st born children are too young for this analysis). Column 5 and 6 report the effect among children age 13 and older (in 2003) from families with a fourth or higher parity birth in 2003. All regressions include the controls listed in equation 1. Standard errors clustered at the household level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.5: Short and Long Term Adjustments to the Reduction in Transfers on UO Families

	Subsequent Children 2004-2006		Months Employed 2004-2006		Months Employed 2013-2015	
	2004-2006	Up to 2015	Fathers	Mothers	Fathers	Mothers
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.020	-0.014	-0.870	-0.388	0.282	0.225
	(0.037)	(0.089)	(0.858)	(0.672)	(0.963)	(0.865)
Control mean	0.864	2.462	14.485	14.216	18.502	21.076
Observations	3,998	3,998	3,998	3,998	3,998	3,998

Notes: The table reports the effects of reducing child allowances on the number of subsequent children in the household and on the cumulative number of months employed by each of the parents among Ultra-Orthodox Jewish households. The sample includes Ultra-Orthodox families whose 4th (or higher parity) child was born in 2003. Columns 1, 3, and 4 report the effect within 3 years of the reduction in transfers, whereas Columns 2, 5, and 6 report the effect 10 to 12 years after the reduction in transfers. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4th child prior to June 1, 2003. Robust standard errors are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.6: Effect of Reduction in Transfers on UO School Enrollment for UO Children

	Ages 5-12		Ages 0-4	
	Grades 1-8	Grades 9-12	Grades 1-8	Grades 9-12
	(1)	(2)	(3)	(4)
Panel A: All				
Treatment	0.002	0.018	0.003	0.008
	(0.006)	(0.014)	(0.010)	(0.016)
Control mean	0.988	0.927	0.962	0.897
Observations	11,274	11,274	10,273	10,273
Panel B: Boys				
Treatment	0.004	0.021	-0.003	0.028
	(0.008)	(0.017)	(0.012)	(0.020)
Control mean	0.986	0.931	0.961	0.898
Observations	5,794	5,794	5,277	5,277
Panel C: Girls				
Treatment	0.001	0.017	0.010	-0.013
	(0.007)	(0.018)	(0.013)	(0.021)
Control mean	0.990	0.922	0.964	0.896
Observations	5,480	5,480	4,996	4,996

Notes: The table reports the effects of reducing child allowances on the probability of enrolling Jewish children in Ultraorthodox (UO) schools at grades 1 to 8 (Columns 1 and 3) and at grades 9 to 12 (Columns 2 and 4) among Ultra-Orthodox Jewish households. The sample includes Ultra-Orthodox families whose 4th (or higher parity) child was born in 2003. Panel A reports the effects for both boys and girls, while Panels B and C report the effects by gender. Columns 1 and 2 report the effects for children ages 5-12 in 2003, whereas Columns 3 and 4 report the effects for children ages 0-4. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4th child prior to June 1, 2003. Standard errors clustered at the household level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.7: Effect of Reduction in Transfers on Educational Attainment of UO Children

	Ages 5-12			Ages 1-4		
	Bagrut Diploma (1)	HS Dropout (2)	Bagrut Units (3)	Bagrut Diploma (4)	HS Dropout (5)	Bagrut Units (6)
Panel A: All						
Treatment	-0.019	0.013	-0.646*	-0.025	-0.015	-0.366
	(0.013)	(0.011)	(0.368)	(0.019)	(0.013)	(0.493)
Control mean	0.086	0.056	3.442	0.162	0.069	4.744
Observations	11,274	11,274	11,274	6,280	6,280	6,280
Panel B: Boys						
Treatment	-0.024**	0.015	-0.649*	-0.027	-0.018	-0.590
	(0.011)	(0.015)	(0.379)	(0.017)	(0.020)	(0.490)
Control mean	0.032	0.059	1.376	0.049	0.079	1.657
Observations	5,794	5,794	5,794	3,231	3,231	3,231
Panel C: Girls						
Treatment	-0.018	0.009	-0.818	-0.020	-0.014	0.023
	(0.021)	(0.015)	(0.583)	(0.032)	(0.017)	(0.780)
Control mean	0.144	0.053	5.678	0.285	0.057	8.123
Observations	5,480	5,480	5,480	3,049	3,049	3,049

Notes: This table reports the effects of reducing child allowances on the educational attainment of Jewish children in Ultra-Orthodox Jewish households. The sample consists of Ultra-Orthodox families whose 4th (or higher parity) child was born in 2003. Panel A reports the effects for both boys and girls, while Panels B and C report the effects by gender. Columns 1 to 3 report the effects for children ages 5-12 in 2003, whereas columns 4 to 6 report the effects for children ages 1-4. Children born in 2003 (i.e., those who were age 0 in 2003) are excluded, as they reached grade 12 in 2021, a year for which we have no data. Columns 1 and 4 report results on matriculating high school by obtaining a Bagrut diploma. Columns 2 and 5 report results on dropping out of high school before the completion of twelve years of schooling. Columns 3 and 6 report results on the quality of the diploma by examining the unconditional overall number of Bagrut units. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4th child prior to June 1, 2003. Standard errors clustered at the household level are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.