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ABSTRACT

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

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 over secular subjects. 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 Classification: Z12, J13, J22, H41, I38
Keywords: child allowance, cash transfers, religion, fertility, labor supply

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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 charitable and 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 longer school days, 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

[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% of the Israeli population could be classified as UO, and by 2022, their share increased to 13% (retrieved from the Israeli Central Bureau of Statistics Social Survey Table Generator).

education. However, this informal safety net is not accessible to Arab citizens of Israel, for whom traditional public schools are the only option.[2]

To estimate the fertility, labor market, and school choice adjustments in response to the negative income shock, we use the sharp date-of-birth cutoff introduced by the reform in a regression discontinuity design (RDD). Our analysis focuses on the adjustments of Jewish and Arab families whose fourth or higher birth order child was born in 2003, for whom the difference in allowances across the reform cutoff was substantial. We then apply 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. Nevertheless, 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 utilize administrative data covering the universe of Jewish and Arab families with a child 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 family size at the time of the reform, determine birth order, and estimate fertility responses. These data are linked to restricted administrative records from the Israel Tax Authority, which allow us to estimate short- and long-term effects on parental labor supply. In addition, we use data from the Ministry of Education to determine the type of schools children are enrolled in and to track long-term outcomes such as high school completion, matriculation, and postsecondary education, including religious seminaries. Finally, we rely on population registry data 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

[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 was Muslim, 11% Christian, and the remainder belonged to the Druze religious minority (Central Bureau of Statistics, Israel, 2022).

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, presumably 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 Arab mothers did not change their labor supply, we observe an increase in the number of months that Arab fathers were employed in the long-term. 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.

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). 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

[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.

et al., 2015; Manoli and Turner, 2016-2017; Bastian and Michelmore, 2018), welfare-to-work experiments (Gennetian and Miller, 2002; Clark-Kauffman et al., 2003; Morris and Gennetian, 2003), and various large-scale welfare reforms in the U.S. and other developed countries (Løken et al., 2018; Hartley et al., 2022; Kalil et al., 2023; Shanan, 2024; Dustmann et al., 2024). 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). Other studies of unconditional cash transfers to newborns in poor families found no short-term effects on children's development and health (Hart et al., 2024; Sperber et al., 2023). Importantly, these previous studies have not systematically examined how different parental adjustments to income shocks influence children's long-term outcomes or the role of informal safety nets in shaping family responses.

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).[4] 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) and Mari (2024) found little evidence that child-related payments affected children's human capital or health outcomes. Relatedly, evidence from changes in other sources of non-work income, such as lottery winnings 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 immediate income shocks (Shah and Steinberg, 2017; Carrillo, 2020; Dustmann et al., 2024). In contrast, consistent with the quantity-quality tradeoff, reducing completed fertility 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

[4] 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).

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.[5]

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 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).[6]

Although eligibility for the child allowance program is unconditional, the magnitude of the payment was

[5] 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).

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

historically designed to increase with parity at an increasing rate from the third to the fifth child, remaining constant for sixth or higher parity children.[7] 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.[8]

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.[9] According to this so-called Netanyahu reform, 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.[10]

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

[7]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).

[8]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).

[9]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).

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

households. Over the period 2003-2015, the difference in transfers between the households amounted to NIS 27,300 ($8,800), roughly one-third of the average yearly earnings for similar families in our data in 2000. As this simulation highlights, the age structure of older siblings is an additional source 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 across the June 1 threshold 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 June 1st cutoff had no impact on 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).[11] In practice, even in UO schools that officially

[11] In principle, up to 45% of UO schools' 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).

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).[12] 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 after-school 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).

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 also use the socioeconomic rank of their residential locality, 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.[13] Students must pass a

[12]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).

[13]We build on the official classification of school type recorded by the MoE but further refine it using data

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.

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 2000-2013, allowing us to follow parents' outcomes for 10 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.[14] 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,

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.

[14]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.

they work fewer months per 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.[15] 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 "pivotal" 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. 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

[15] We define families as UO in 2003 if all school-aged children are enrolled in UO schools or if the father attends a Yeshiva.

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 in 2000 for each of the parents, an indicator for being identified as UO before 2003, indicators for socioeconomic rank based on the locality of residence, and district 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.[16] ϵ_i is an idiosyncratic error term. Standard errors are clustered 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. 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.

[16]Siblings' age composition is controlled for using a set of variables for the number of 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. Only a few families in our sample had children aged 18 or over in 2003.

4.2 Investigating the Validity of the Research Design

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.

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, households could not have manipulated their treatment status by changing the timing of 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. 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.[17] Therefore, 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 separately.

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 the differences between treatment and comparison households for the entire population, as well as separately for the Arab and Jewish populations, are small and statistically insignificant. The only marginally significant difference is in the locality-level socioeconomic rank for Arab families. Nonetheless, to improve precision, all these characteristics are 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 birth timing.

[17] 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 based on the three observed factors that determine the monthly payments: 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 2013), which can be considered as completed fertility for most families.[18] 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 statistically insignificant effects on both short- and long-term fertility (Columns

[18] Our results remain practically identical when we restrict the sample to mothers over age 30, for whom this measure more accurately reflects completed fertility.

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, as evidenced by the significant decline of 0.109 children reported in Column 2. Notably, both the short and the long-term estimates indicate an approximately 12% reduction in fertility relative to the control mean. The overall reduction in completed fertility amounts to 1.8% from an average of 6.1 children.

Figure 6 presents graphical evidence for these RD estimates, 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. In addition, Figure 5 shows the estimated effects on fertility year-by-year after 2003. The results confirm 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.

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, 8-10 years after the reduction in transfers (2011-2013). 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 modestly 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 less than their partners in the short run but matched 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 less 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 although it 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).[19] 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). Lastly, in Appendix Table A.3, we conduct two placebo tests that confirm the absence of any observed responses when applying the reform cutoff day (June 1) to a sample of births during 2002 or when we examine parity-one and parity-two births in 2003 for which the cutoff was irrelevant.[20]

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 1.4%.[21] The modest labor supply response to the reduction in transfers aligns with existing literature (Cesarini et al., 2017; Jones and Marinescu, 2022; Sauval et al., 2024).

5.2.2 Children's Schooling Streams

The lack of fertility and labor supply responses among Jewish families suggests they may have adjusted to the decline in cash transfers by relying on informal safety nets and reducing their investments in children's human capital. As discussed in Section 2, Jewish families in Israel can choose to enroll their children in UO

[19]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.

[20]We do not find significant effects on fertility or labor supply for either Jewish or Arab families with a third birth in 2003. This is likely because the decline in transfers is much smaller for these families.

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

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.

In Table 5, we estimate the effect of the reduction in child allowances on the likelihood that families enroll their children in a 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 were already enrolled in elementary school at the time of the reduction in transfers, and switching them to a 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 a UO school at the elementary level (grades 1-8) by 2.7 percentage points, or by about 4% 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. Figures 7 (a) and (b) present the same findings graphically demonstrating a clear discontinuous increase in UO enrollment after the reform cutoff. [22]

Consistent with the larger return to religious schooling for boys, we find larger effects for boys (Panel B) than for girls (Panel C). Specifically, the results indicate that the reduction in transfers increased the probability that elementary school-aged Jewish boys ever enrolled in a UO elementary school by 4.9 percentage points, an effect that is statistically significant at the 1% 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 a UO high school is also larger for boys than for girls.[23]

We also find substantial heterogeneity by children's age. 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 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 a UO school at any grade. We also find no evidence that the reduced transfers impacted the probability of ever enrolling in a UO high school among children who were 13-17 years old in 2003 (See Columns 5-6 in Appendix Table A.5). This is expected since it would have been substantially more difficult

[22]The effects are larger when we restrict the sample to families that did not enroll any of their children in UO schools prior to 2003. Ideally, we would focus our analysis on non-UO families. However, we identify religiosity based on school type, and in some families, the children are too young to be in school before 2003.

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

to change schooling tracks at this stage.

Two additional placebo tests are presented in Appendix Table A.5 confirming the lack of an effect when we apply the June 1st cutoff date to a sample of 2002 births (Columns 1-2) and when we examine parity-one and parity-two births in 2003 (Columns 3-4). Concurrently, the results for boys and girls aged 5-12 in 2003 are robust to alternative bandwidth selections (Appendix Figure A.3) and to using a triangular kernel or an eight-day donut hole around the cutoff (Appendix Table A.4).[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 to gain access to amenities provided by UO schools. The fact that these adjustments varied by children's age and gender indicates this was a compromise for the parents rather than a change in their preferences. Importantly, the contrast between the responses of Jewish and Arab families is consistent with their differential access to the alternative safety net provided by UO schools.[25]

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 to enroll some 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 offset potential negative effects on 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.

[24] We do not find significant effects on UO enrollment for children in families with a third 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.

[25] This differential response is unlikely to be driven by pre-reform earnings differences between Arab and Jewish families. Jewish families with below-median income in 2003—who are more comparable to the average Arab family—also responded to the decline by increasing enrollment of school-aged boys in UO schools.

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 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 statistically significant changes in these outcomes for Jewish boys and girls who were 1-4 years old at the time of the reduction in transfers.[26]

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 lead to long-term effects on postsecondary educational attainment and employment in early adulthood. Table 7 examines these long-term effects when the affected children were aged 19-26.

[26] 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.

Column 1 evaluates the likelihood of attending postsecondary 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. The outcomes 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.

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 a negative but 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.

Ultra-Orthodox 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.7 shows no evidence that these families were more likely to enroll their children in UO schools. 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.6). While we observe no adjustments along these margins, Appendix Table A.8, 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 suggests that UO families prioritize their high school matriculation when facing a reduction in child allowances. This interpretation is consistent with the higher returns to matriculation for UO girls in both the marriage and labor markets.

5.3.2 Parental Adjustments and the Outcomes of Arab Children

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

Accordingly, the results in Table 8 show that the reduction in transfers had no negative effects on the high school outcomes of Arab children. In fact, there is some evidence of positive effects among children aged 1-4 in 2003. In particular, we find a substantial and significant decrease in the likelihood of dropping out of high school driven mainly by girls (Panel A, Column 5). These results are consistent with a quantity-quality tradeoff in which a decline in fertility increases human capital (Angrist et al., 2010; Bagger et al., 2020).

Long-Term Effects on Education and Employment. We find additional support for improvement in the outcomes of Arab children when we examine long-term effects on postsecondary educational attainment and employment outcomes. The results in Table 9 suggest that boys have increased their labor supply, working 0.46 additional months per year at the ages 19-26 (Column 2), with no impact on the likelihood of ever being employed (Column 3). Meanwhile, consistent with the null effects on earning a Bagrut diploma, we do not detect any effects on postsecondary educational attainment for either boys or girls (Column 1).

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

This paper examines how access to informal insurance through religious institutions impacts family responses to cuts in social welfare programs, and how those responses translate into long-term outcomes for children. Exploiting a sharp discontinuity in Israel's 2003 child allowance reform which reduced unconditional cash transfers to large families, we show that Jewish and Arab families adjusted along markedly different margins, reflecting unequal access to alternative forms of support.

Among Jewish families, the reduction in unconditional cash transfers led to increased enrollment of school-aged boys in UO schools, especially in households not previously affiliated with UO communities. These schools offer valuable short-term support—such as extended school hours and subsidized services but prioritize religious education. As a result, affected boys were less likely to complete high school or pursue postsecondary education, and more likely to adopt a traditional religious lifestyle in early adulthood. Jewish parents, however, did not choose to enroll school-aged girls or their younger children in UO schools, indicating that UO schools served as an adjustment to reduced transfers rather than a change in their preferences.

In contrast, Arab families, who lacked access to comparable religious safety nets, responded to the income shock through more standard economic adjustments, reducing fertility and increasing paternal labor supply. These changes not only helped buffer the effects of reduced transfers, but may have even led to an improvement in children's educational and labor market outcomes.

These findings highlight the importance of alternative safety-nets in the design and reform of social safety-net programs. In this case, support from publicly funded religious institutions led to unintended consequences by allowing some Jewish families to maintain fertility at the cost of reducing children's human capital and potentially increasing long-term dependence on formal safety nets. Arab families without access to such institutions, on the other hand, were compelled to make economically efficient and potentially development-enhancing adjustments in response to reduced income transfers.

The differential adjustments carry starkly different welfare implications. Using the Marginal Value of Public Funds (MVPF) framework proposed by Hendren and Sprung-Keyser (2020), we estimate an MVPF below one for Arab families while the MVPF for Jewish families approached infinity, even under conservative assumptions.[27] This implies that increasing child allowances to large Arab families is an inefficient public investment because it reduces future tax revenues by lowering labor supply and reducing human capital, making the cost of child allowances greater than families' willingness to pay for them. However, in the presence of alternative conditional transfers, such as the amenities provided by religious schools, the cost of these unconditional transfers is offset by the distortionary effects of the informal safety nets.

More broadly, this study underscores the need for policy evaluations that consider both the behavioral margins available to families and the broader institutional environment in which they operate. Effective welfare reform requires a careful accounting of how policy changes interact with existing support structures to shape long-term economic and social outcomes, particularly in contexts where religious and public welfare systems coexist and compete.

[27]For Arab families, the net cost of increasing child allowances for high parity children includes a *negative* fiscal externality because of the change in fathers' labor supply. In contrast, for Jewish families, the net cost in the presence of religious schools includes *positive* fiscal externalities due to the expected life-cycle increase in children's earnings and thus, in tax revenues. These positive externalities are calculated based on the estimated change in postsecondary attainment (Table 7) and estimates on the returns to education in the Israeli context (Lavy, 2021). The social welfare implications of changes in completed fertility following the cut in child allowances are more complex and beyond the scope of this paper.

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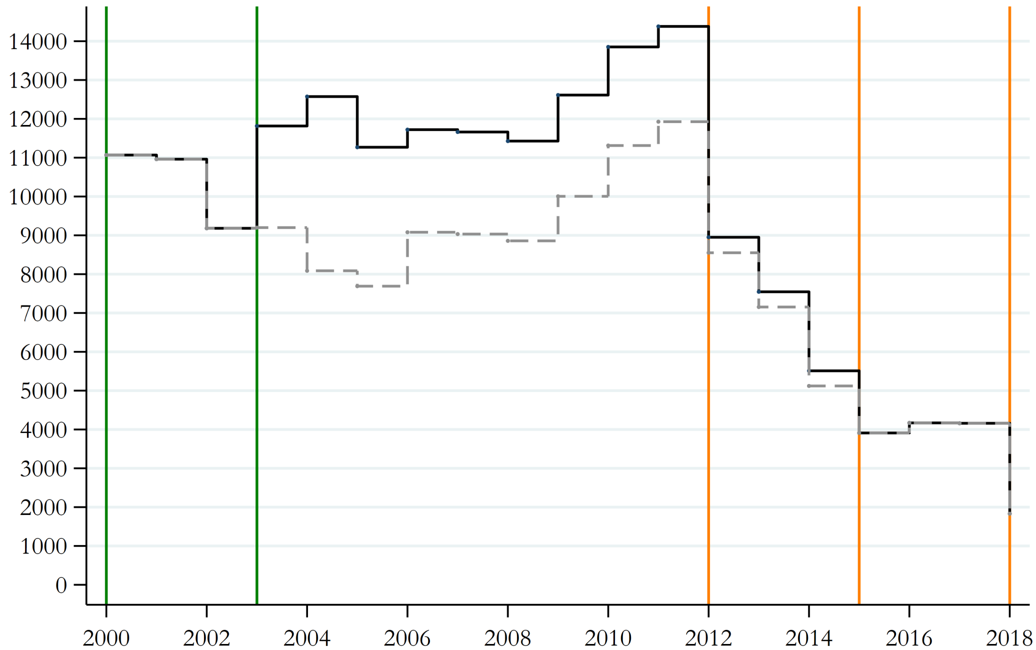
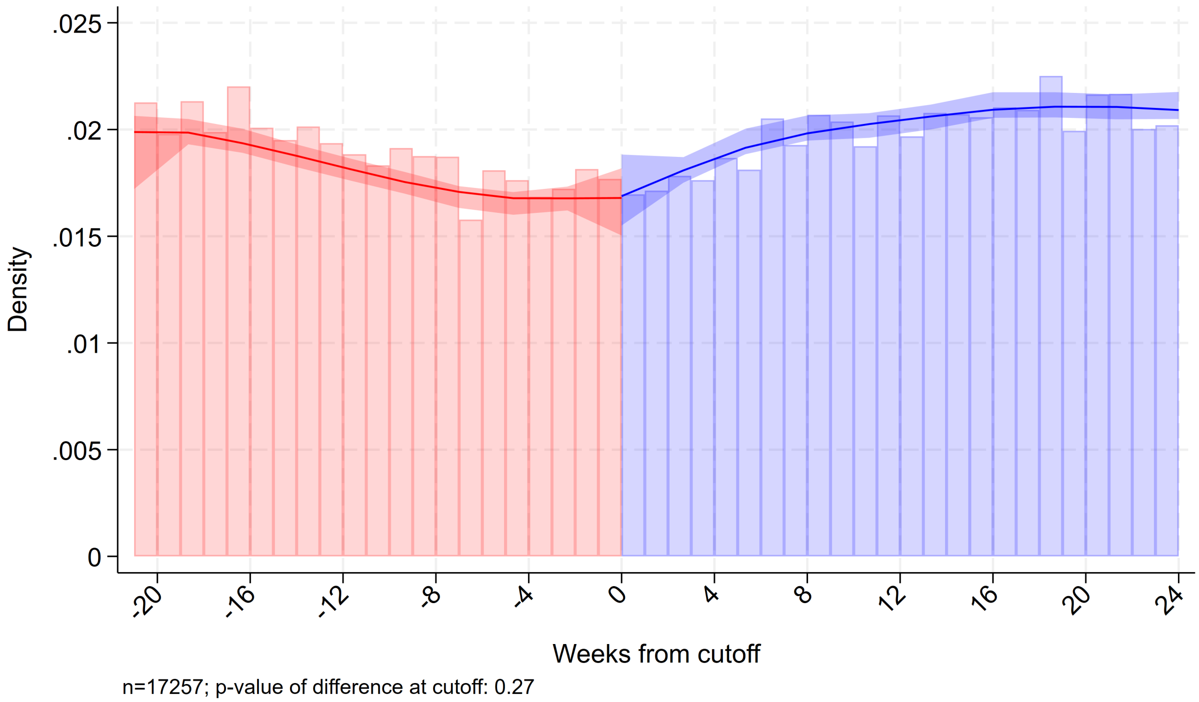
Figure 1: Total Simulated Annual Child Allowances by Treatment Status 2000-2018, 4th Child Born in 2003



Notes - This Figure displays simulated yearly child allowance to be received in NIS by a family whose 4th child was born in 2003. All amounts are in 2021 NIS. Solid and dashed lines indicate 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. We assume a three years spacing between births. The overall gap in allowances between treated and untreated families in this simulation is NIS 27,300.

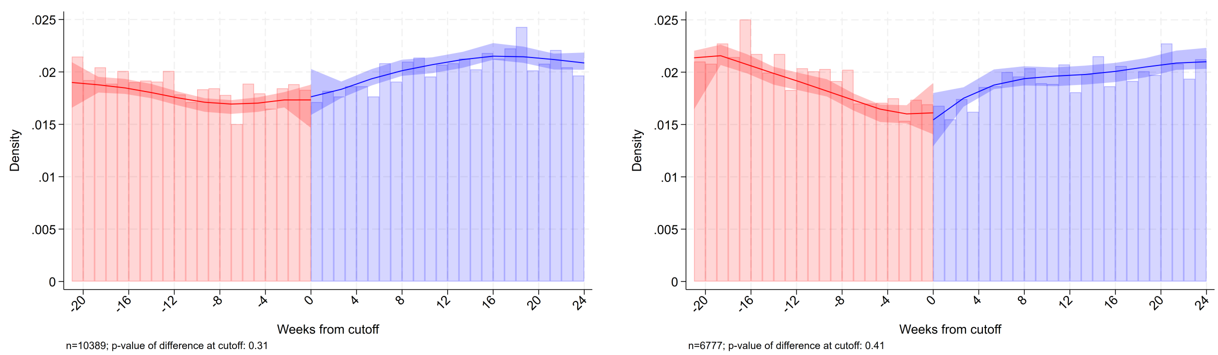
Figure 2: Manipulation Test



Notes - The figure displays the distributional density of four or higher parity births around the reform cutoff of June 1, 2003 (McCrary, 2008; Cattaneo et al., 2018). The number of observations and the p-value for the significance of the discontinuity at zero is listed just below the graph. Histograms denote the distribution of births.

Figure 3: Manipulation Tests for Jewish and Arab Families

(a) Jewish (b) Arab

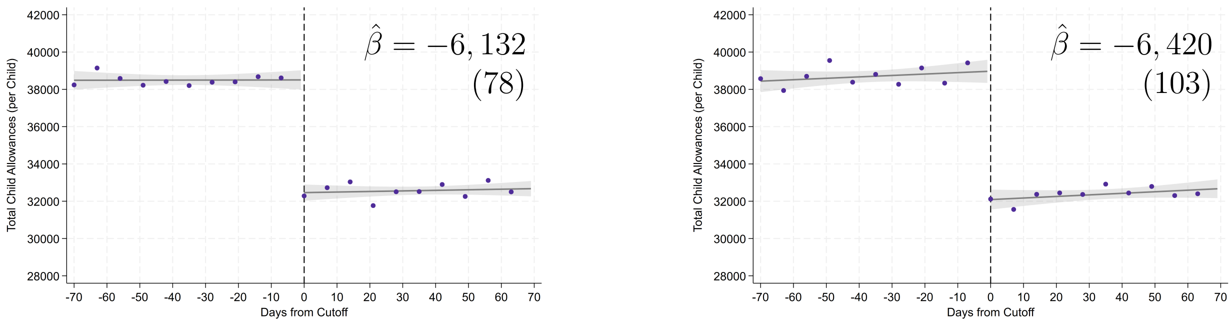


Notes - Panel (a) and (b) present the distributional density of four or higher parity births around the reform cutoff of June 1, 2003 for Jewish and Arab families (McCrary, 2008; Cattaneo et al., 2018). The number of observations and the p-value for the significance of the discontinuity at zero are listed just below the graph. Histograms denote the distribution of births.

Figure 4: The Change in Transfers per Child for Jewish and Arab Families

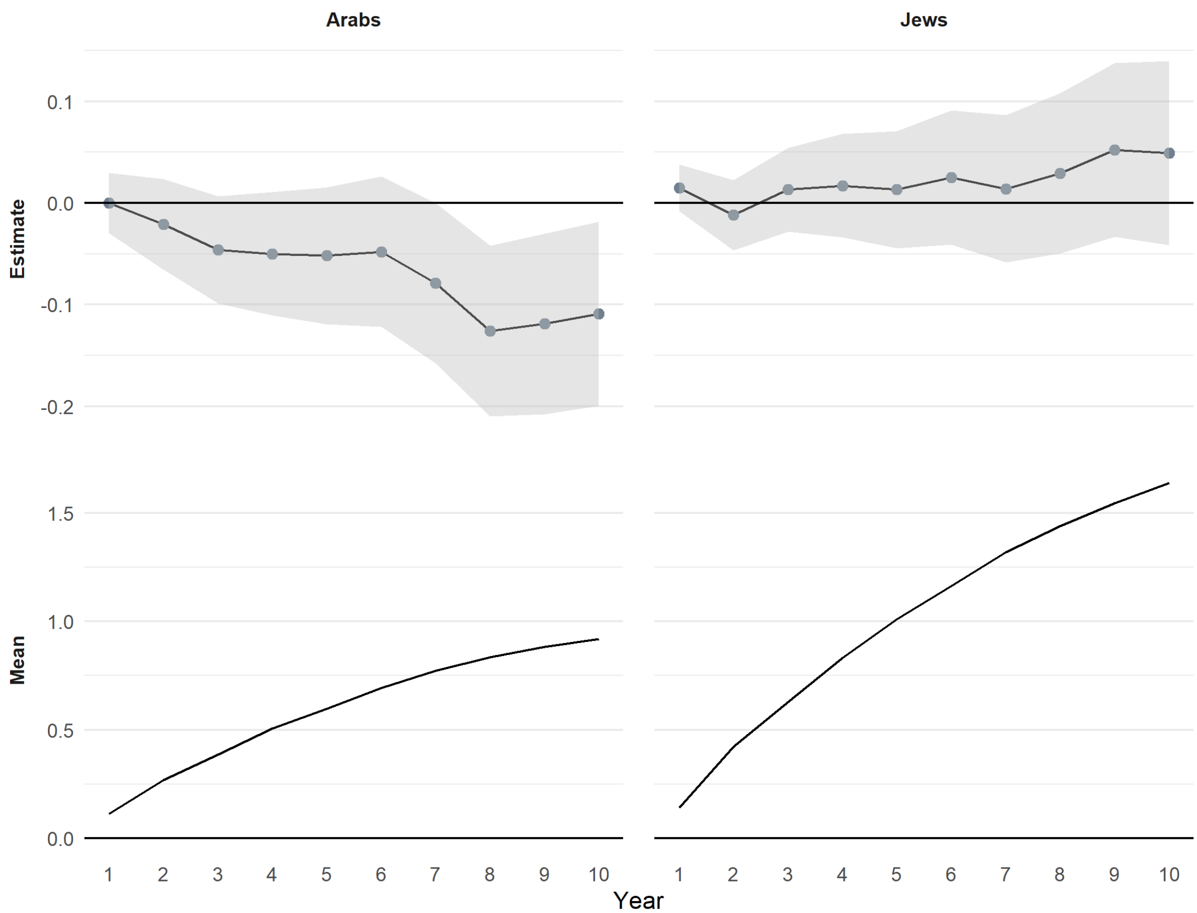
(a) Jewish Families

(b) Arab Families



Notes - This figure displays the mean child allowance per child against the birthdate of each family's fourth or higher parity 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 group contains both the child born around the cutoff and their older siblings. Each point represents an average over a seven-days bin. Fitted linear lines of the underlying data are allowed to vary across the cutoff date. The 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.

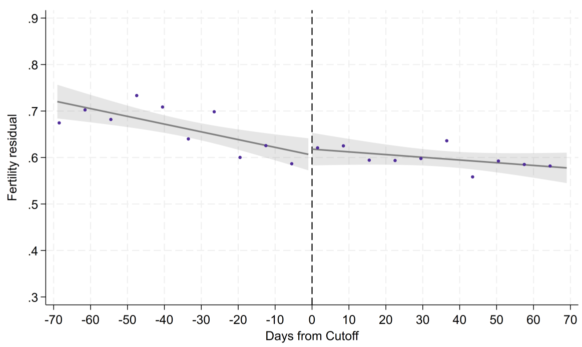
Figure 5: The Dynamic Effect of Reducing Transfers on Fertility



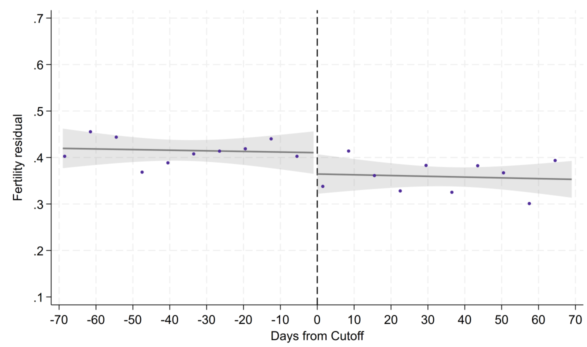
Notes - This figure displays the effects of reducing child allowances on the number of subsequent children in the household for each year following 2003, separately for Jews and Arabs. The sample includes families whose 4th (or higher parity) child was born in 2003. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The corresponding means are presented below the estimates, separately for each group. The gray areas indicate 90% confidence intervals.

Figure 6: The Effects of Reducing Transfers on Short-term and Completed Fertility

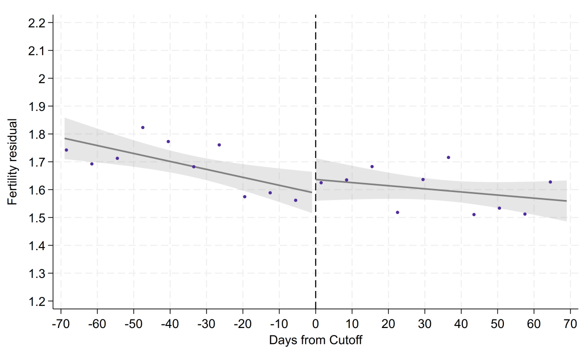
(a) Short-term: Jewish



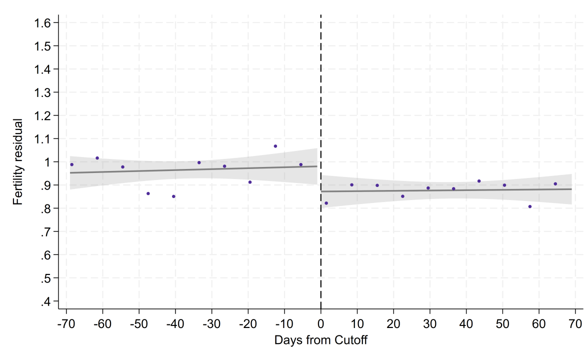
(b) Short-term: Arab



(c) Completed: Jewish



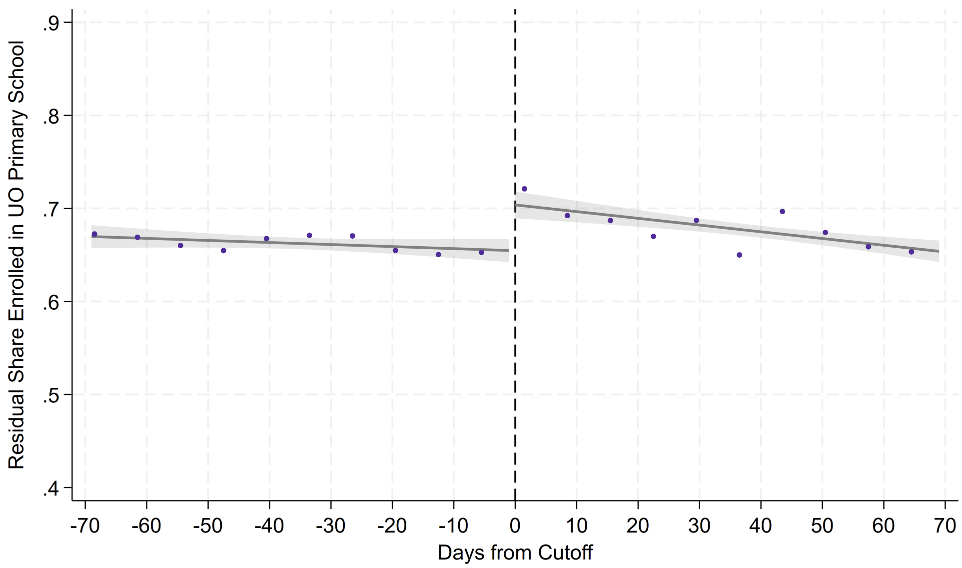
(d) Completed: Arab



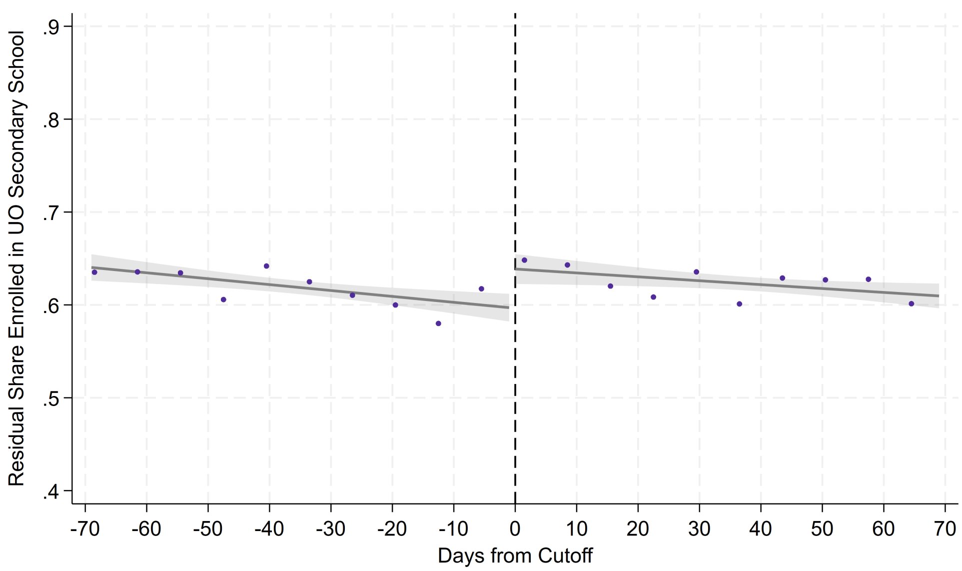
Notes - This figure displays residualized fertility against the birthdate of each mother's fourth or higher parity child who was born in a 10 weeks bandwidth around the June 1, 2003 cutoff, with the x-axis centered at the cutoff. Panels (a) and (b) show short-term fertility, defined as the number of subsequent children born up to 2006. Panels (c) and (d) show completed fertility, defined as the number of subsequent children born up to 2013. The samples in Panels (a) and (c) include Jewish mothers while the samples in Panels (b) and (d) include 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: The Effects of Reducing Transfers on Enrollment in Ultra-Orthodox Schools, Jewish children age 5-12 in 2003

(a) Grades 1-8

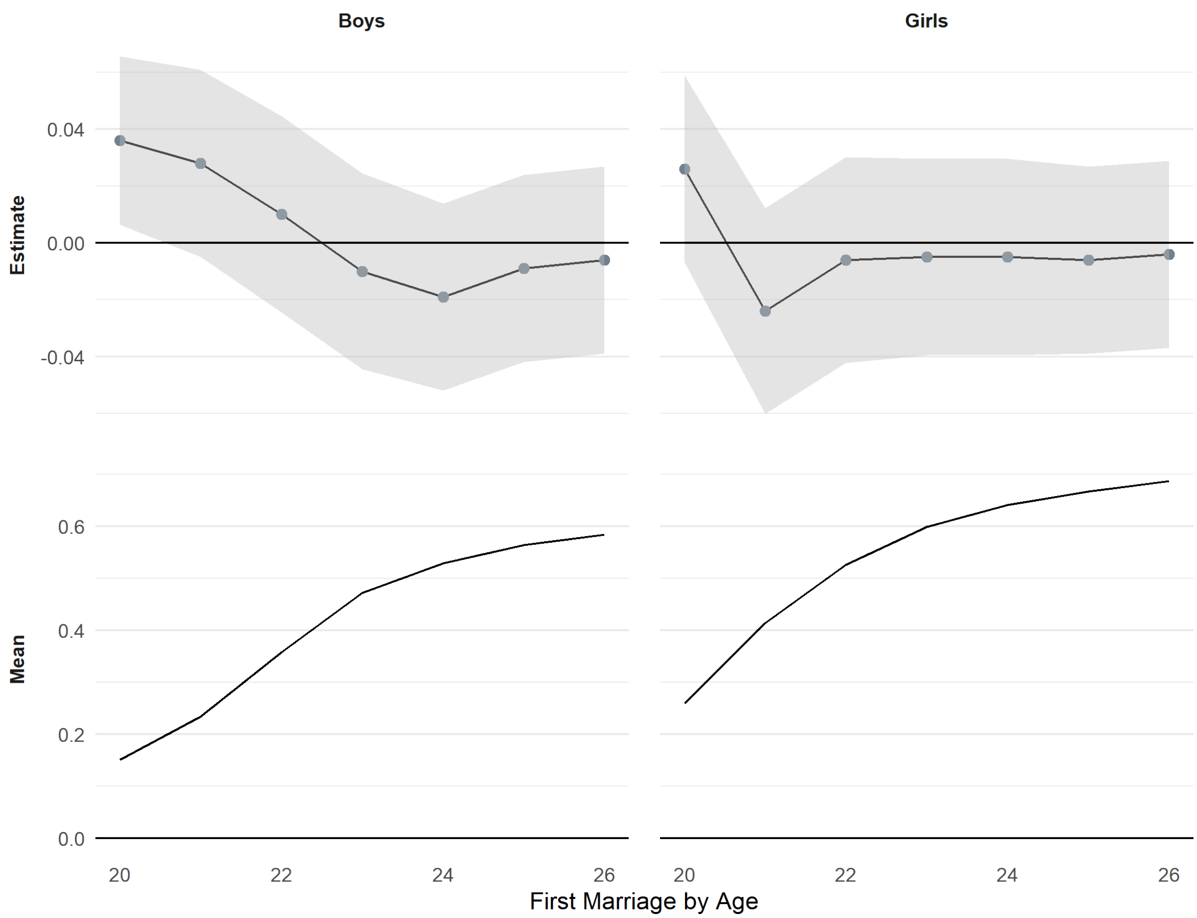


(b) Grades 9-12



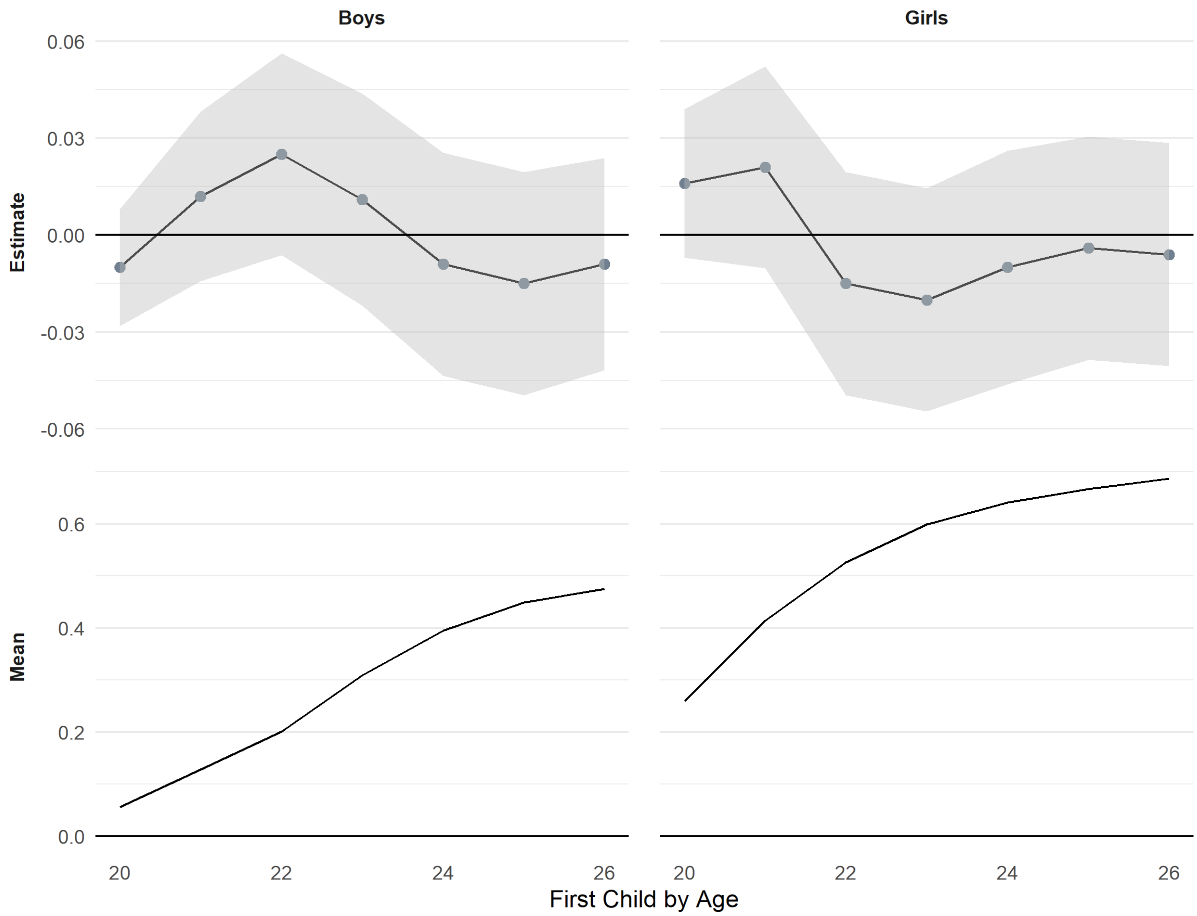
Notes - This figure displays residualized enrollment in ultra-Orthodox (UO) schools against the birthdate of each family's fourth or higher parity child who was born in a 10 weeks bandwidth around the June 1, 2003 cutoff, with the x-axis centered at the cutoff. The sample includes elementary-school-aged (5-12) older siblings of the pivotal child (born in 2003). Panel (a) presents the probability of being ever enrolled in a UO school during grades 1 to 8 while Panel (b) shows the same probability for grades 9 to 12. 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: The Effects of Reducing Transfers on Age at First Marriage, Jewish children age 5-12 in 2003



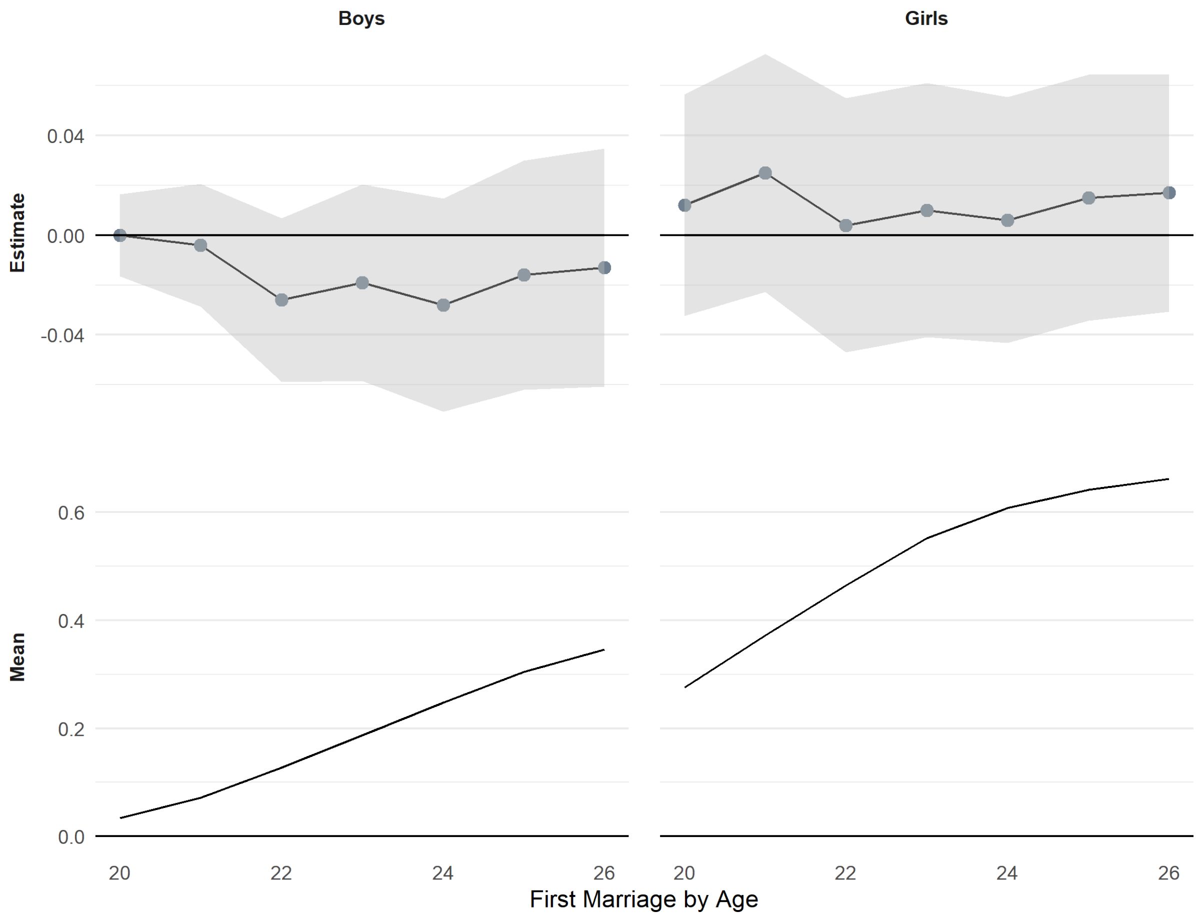
Notes - This figure displays the effects of reducing child allowances on the likelihood of being married between the ages of 20-26 for Jewish individuals who were 5-12 years old whose fourth sibling was born in 2003. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The corresponding means are presented below the estimates, separately for each group. The gray areas indicate 90% confidence intervals.

Figure 9: The Effects of Reducing Transfers on Age at First Birth,
Jewish children age 5-12 in 2003.



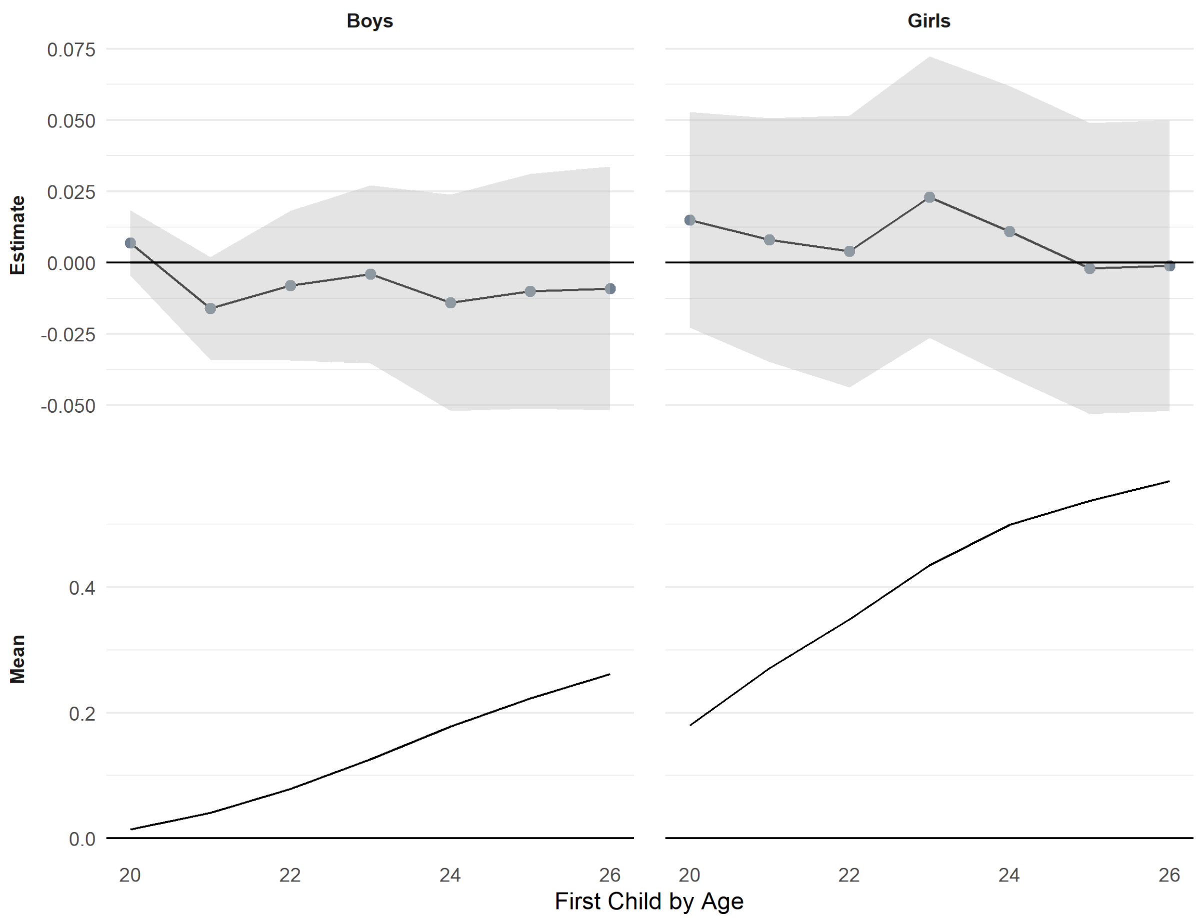
Notes - This figure displays the effects of reducing child allowances on the likelihood of having a first child between the ages of 20-26 for Jewish individuals who were 5-12 years old whose fourth sibling was born in 2003. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The corresponding means are presented below the estimates, separately for each group. The gray areas indicate 90% confidence intervals.

Figure 10: The Effects of Reducing Transfers on Age at First Marriage, Arab children age 5-12 in 2003



Notes - This figure displays the effects of reducing child allowances on the likelihood of being married between the ages of 20-26 for Arab individuals who were 5-12 years old whose fourth sibling was born in 2003. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The corresponding means are presented below the estimates, separately for each group. The gray areas indicate 90% confidence intervals.

Figure 11: The Effects of Reducing Transfers on Age at First Birth,
Arab children age 5-12 in 2003



Notes - This figure displays the effects of reducing child allowances on the likelihood of having a first child between the ages of 20-26 for Arab individuals who were 5-12 years old whose fourth sibling was born in 2003. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. 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 - This table reports the 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		
	All	All	All	Jewish	Arab
	(1)	(2)	(3)	(4)	(5)
Jewish	0.724	0.628	0.630		
	(0.447)	(0.483)	(0.483)		
Arab	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 children	2.755	5.406	5.397	5.532	5.167
	(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 - This table reports summary statistics for families with a birth in 2003. Column 1 reports statistics for all families, while 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 June 1, 2003 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)	Jewish (2)	Arab (3)
Jewish	-0.021		
	(0.018)		
Arab	0.021		
	(0.018)		
Jewish UO	-0.002	0.014	
	(0.018)	(0.023)	
Father's age	0.217	0.213	0.236
	(0.224)	(0.280)	(0.375)
Mother's	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 children	0.088	0.095	0.099
	(0.065)	(0.087)	(0.092)
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 - This table reports results from regressions testing for balance of observable characteristics. The bandwidth is 70 days in all regressions and the running variable is allowed to vary on either side of the cutoff. The outcomes are listed in each row and each estimate represents a separate regression. 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. Robust standard errors are reported in parentheses. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 4: The Effects of Reducing Transfers on Fertility and Employment

	Subsequent Children		Months Employed 2004-2006		Months Employed 2011-2013	
	2004-2006	Up to 2013	Fathers	Mothers	Fathers	Mothers
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Jewish						
Treatment	0.013	0.059	-0.730	-0.643	0.254	-0.951
	(0.025)	(0.055)	(0.609)	(0.535)	(0.650)	(0.615)
Control mean	0.629	1.640	20.615	16.999	22.255	22.250
Observations	7,310	7,310	7,310	7,310	7,310	7,310
Panel B: Arab						
Treatment	-0.046	-0.109**	0.891	0.132	1.786**	-0.570
	(0.032)	(0.055)	(0.768)	(0.483)	(0.827)	(0.728)
Control mean	0.386	0.920	23.079	4.708	23.214	8.551
Observations	4,296	4,296	4,296	4,296	4,296	4,296

Notes - This 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 Jewish families, while Panel B reports the effects for Arab families. 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 8 to 10 years after the reduction in transfers. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions and the running variable is allowed to vary on either side of the cutoff. 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: The Effects of Reducing Transfers on Enrollment in Ultra-Orthodox Schools

	Ages 5-12		Ages 0-4	
	Grades 1-8 (1)	Grades 9-12 (2)	Grades 1-8 (3)	Grades 9-12 (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 - This table reports the effects of reducing child allowances on 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 children in 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 and the running variable is allowed to vary on either side of the cutoff. 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 6: The Effects of Reducing Transfers on the 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 - This table reports the effects of reducing child allowances on the educational attainment of Jewish children. The sample includes children in 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 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 and the running variable is allowed to vary on either side of the cutoff. 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: The Effects of Reducing Transfers on the Long-Term Outcomes of Jewish Children

	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 - This table reports the effects of reducing child allowances on long term outcomes of Jewish children aged 5-12 at the time of the reduction in transfers. The sample includes children in Jewish 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 and the running variable is allowed to vary on either side of the cutoff. 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: The Effects of Reducing Transfers on the 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 - This table reports the effects of reducing child allowances on the educational attainment of Arab children. The sample includes children in Arab 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 and the running variable is allowed to vary on either side of the cutoff. 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.

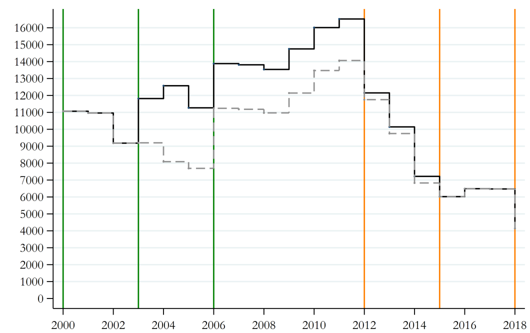
Table 9: The Effects of Reducing Transfers on the Long-Term Outcomes of Arab Children

	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

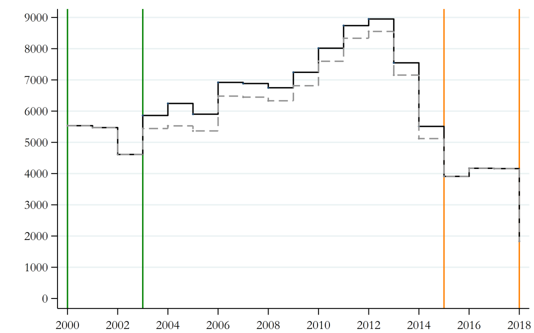
Notes - This table reports the effects of reducing child allowances on long term outcomes of Arab children aged 5-12 at the time of the reduction in transfers. The sample includes children in Arab families whose fourth (or higher parity) child was born in 2003. Panel A presents results for boys, while 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 and the running variable is allowed to vary on either side of the cutoff. 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: Total Simulated Annual Child Allowances by Treatment Status 2000-2018

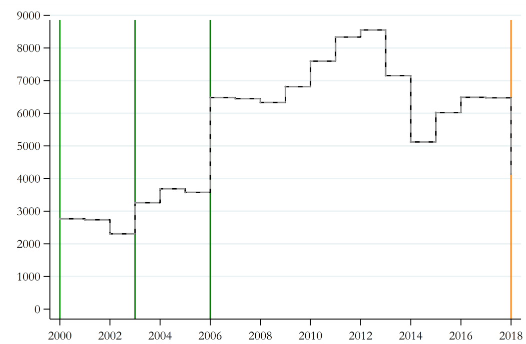
(a) 4th in 2003 and 5th in 2006, gap NIS 27,300



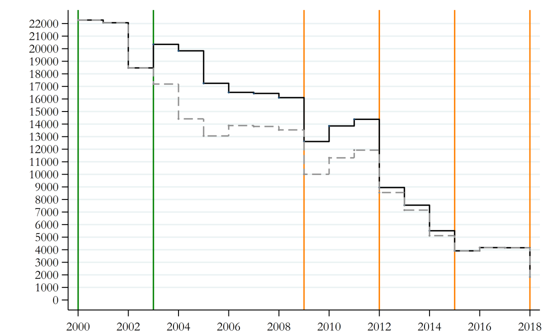
(c) 3rd in 2003, gap NIS 5,397



(b) 2nd in 2003, gap NIS 0

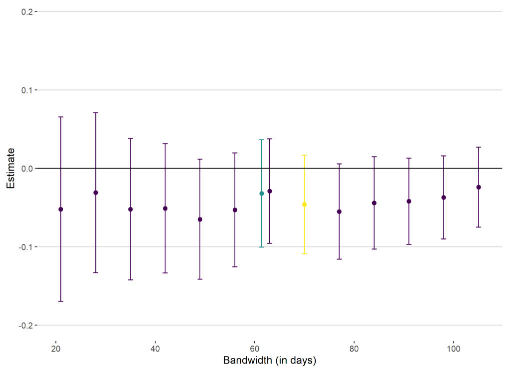


(d) 5th in 2003, gap NIS 29,382

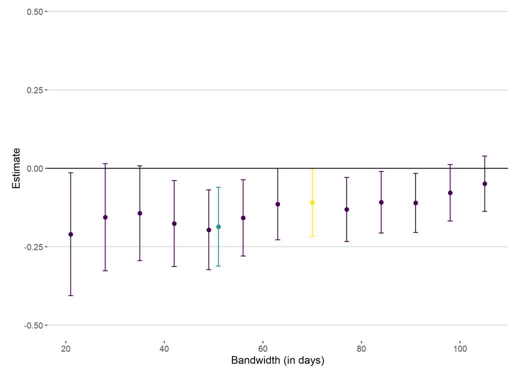


Notes - This Figure displays simulated yearly child allowance to be received in NIS by a family whose 4th child was born in 2003. All amounts are in 2021 NIS. Solid and dashed lines indicate the amount when the birth in 2003 occurred pre-June (untreated) and post-June (treated), respectively. The vertical 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. We assume a three years spacing between births.

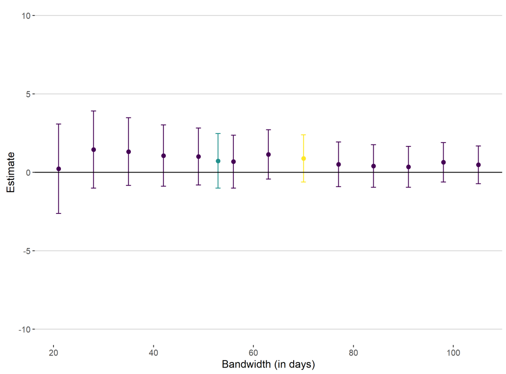
Figure A.2: Robustness to Alternative Bandwidths: Effects on Fertility and Employment of Arab Families



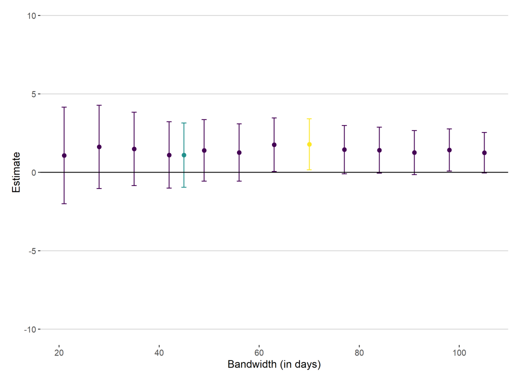
(a) Fertility 2004-2006



(b) Fertility Up to 2013



(c) Months Employed 2004-2006 (Fathers)

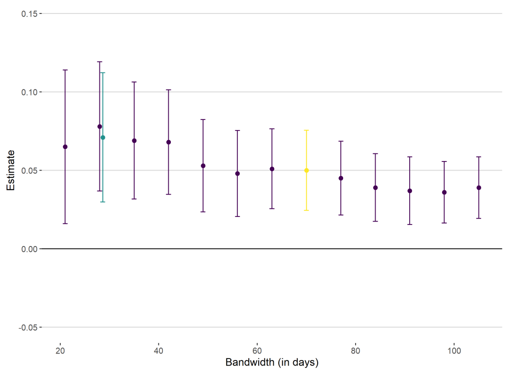


(d) Months Employed 2011-2013 (Fathers)

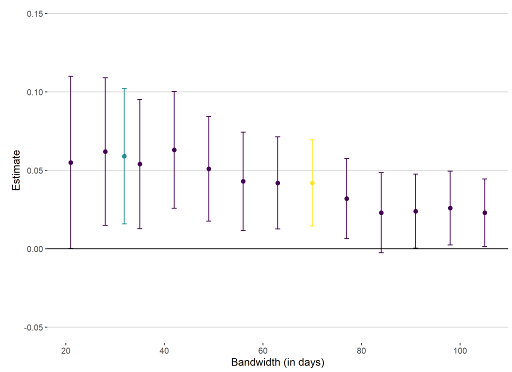
● Automatic bandwidth ● Fixed bandwidth

Notes - This figure displays 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 effects on short-term and completed fertility adjustments. Panels (c) and (d) report effects on short- and long-term employment adjustments for Arab fathers. The coefficient based on the 70-day bandwidth is reported in yellow, and the Calonico et al. (2020) automatic bandwidth is plotted in green.

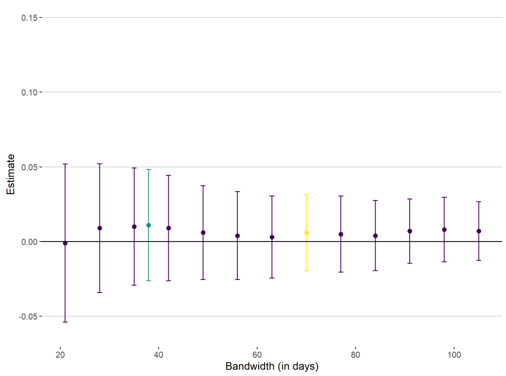
Figure A.3: Robustness to Alternative Bandwidths: Effects on Enrollment in Ultra-Orthodox Schools



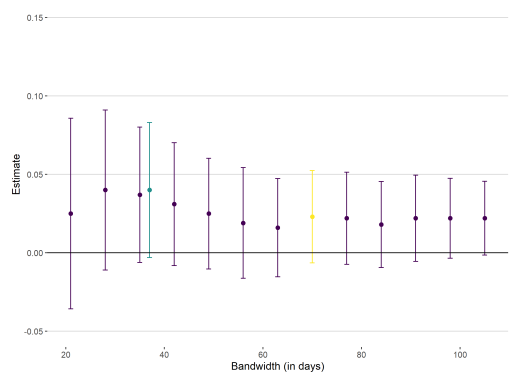
(a) UO Grade 1-8 (Boys)



(b) UO Grades 9-12 (Boys)



(c) UO Grade 1-8 (Girls)



(d) UO Grades 9-12 (Girls)

● Automatic bandwidth ● Fixed bandwidth

Notes - This figure displays robustness checks for Jewish 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 effects on the probability of enrolling children in UO schools for boys. Panels (c) and (d) report effects on the probability of enrolling children in UO schools for girls. The 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: The Effects of Reducing Transfers on Labor Earnings

	Labor Earnings 2004-2006		Labor Earnings 2011-2013	
	Fathers (1)	Mothers (2)	Fathers (3)	Mothers (4)
Panel A: Jewish				
Treatment	-585	-238	-10,838	-3,483
	(8,228)	(4,407)	(10,909)	(6,350)
Control mean	206,621	105,571	266,627	164,869
Observations	7,310	7,310	7,310	7,310
Panel B: Arab				
Treatment	4,871	-986	8,742	-4,337
	(7,044)	(1,908)	(9,868)	(3,408)
Control mean	157,557	17,926	200,050	44,655
Observations	4,296	4,296	4,296	4,296

Notes - This 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 8 to 10 years after the reduction in transfers. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions and the running variable is allowed to vary on either side of the cutoff. 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: Fertility and Employment of Arab Families

	Subsequent Children		Months Employed 2004-2006		Months Employed 2011-2013	
	2004-2006	Up to 2013	Fathers	Mothers	Fathers	Mothers
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Kernel						
Treatment	-0.047*	-0.147**	0.931	-0.033	1.535*	-0.523
	(0.027)	(0.063)	(0.828)	(0.524)	(0.902)	(0.800)
Control mean	0.386	0.920	23.079	4.708	23.214	8.551
Observations	4,296	4,296	4,296	4,296	4,296	4,296
Panel B: Donut						
Treatment	-0.046	-0.111*	1.006	0.228	1.966**	-0.993
	(0.037)	(0.063)	(0.869)	(0.540)	(0.933)	(0.823)
Control mean	0.386	0.921	23.047	4.623	23.178	8.456
Observations	4,066	4,066	4,066	4,066	4,066	4,066

Notes - This 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 8 to 10 years after the reduction in transfers. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions and the running variable is allowed to vary on either side of the cutoff. 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: Placebo Tests: Fertility and Employment of Arab Families

	Subsequent Children		Months Employed 2004-2006		Months Employed 2011-2013	
	2004-2006	Up to 2013	Fathers	Mothers	Fathers	Mothers
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2002 Placebo Cutoff						
Treatment	0.034	0.060	0.649	-0.115	-0.769	-0.915
	(0.033)	(0.054)	(0.776)	(0.450)	(0.830)	(0.682)
Control mean	0.400	0.842	23.113	4.466	23.219	7.574
Observations	4,368	4,368	4,368	4,368	4,368	4,368
Panel B: Parity 1 and 2						
Treatment	-0.008	-0.060	0.224	-0.224	-0.581	0.349
	(0.027)	(0.046)	(0.542)	(0.562)	(0.559)	(0.646)
Control mean	0.841	1.992	26.829	10.307	27.408	13.921
Observations	6,938	6,938	6,938	6,938	6,938	6,938

Notes - This 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 from applying the reform cutoff day of June 1 to a sample of births during 2002, while Panel B reports estimates examining 1- and 2-parity births 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 8 to 10 years after the reduction in transfers. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions and the running variable is allowed to vary on either side of the cutoff. Robust standard errors are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.4: Robustness Tests: Enrollment in Ultra-Orthodox Schools

	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 - This table reports robustness checks for the effects of reducing transfers 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 children in 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). All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions and the running variable is allowed to vary on either side of the cutoff. 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: Placebo Tests: Enrollment in Ultra-Orthodox Schools

	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 - This table reports robustness checks for the effects of reducing transfers on the probability of enrolling Jewish children in ultra-Orthodox (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. The bandwidth is 70 days in all regressions and the running variable is allowed to vary on either side of the cutoff. 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.6: The Effects of Reducing Transfers on the Fertility and Employment of pre-2003 Ultra-Orthodox Families

	Subsequent Children		Months Employed 2004-2006		Months Employed 2011-2013	
	2004-2006 (1)	Up to 2013 (2)	Fathers (3)	Mothers (4)	Fathers (5)	Mothers (6)
Treatment	-0.020	-0.022	-0.870	-0.388	0.685	-0.393
	(0.037)	(0.080)	(0.858)	(0.672)	(0.952)	(0.847)
Control mean	0.864	2.275	14.485	14.216	17.592	19.398
Observations	3,998	3,998	3,998	3,998	3,998	3,998

Notes - This 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 pre-2003 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 8 to 10 years after the reduction in transfers. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions and the running variable is allowed to vary on either side of the cutoff. 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.7: The Effects of Reducing Transfers on Enrollment in ultra-Orthodox Schools for pre-2003 ultra-Orthodox Families

	Ages 5-12		Ages 0-4	
	Grades 1-8 (1)	Grades 9-12 (2)	Grades 1-8 (3)	Grades 9-12 (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 - This table reports the effects of reducing child allowances on 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 children in pre-2003 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 and the running variable is allowed to vary on either side of the cutoff. 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.8: The Effects of Reducing Transfers on the Educational Attainment of pre-2003 ultra-Orthodox 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. The sample includes children in pre-2003 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 and the running variable is allowed to vary on either side of the cutoff. 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.