Religious Safety Nets and their Effects on Human Capital Accumulation

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Abstract

This paper studies the effects of substituting between state and religious safety nets on human capital investments, fertility, and labor market outcomes. In 2003, Israel reformed its child allowance program which significantly reduced the unconditional cash benefits received by large families. Using a sharp date-of-birth cutoff in the reform, we show that Jewish families substituted for the short-term loss in government benefits by enrolling their young boys in Ultraorthodox religious schools. These schools provide important services not available in mainstream public schools but do not offer a traditional secular education. In the long-term, we find that the reform led to a decrease in the likelihood that Jewish boys matriculate high school. The substitution between secular and religious schools prevented Jewish families from changing their fertility or labor supply decisions. In contrast, the reform led to a 13 percent decline in the completed fertility of Arab families, who do not have access to comparable religious schooling system, while having little impact on the educational outcomes of Arab children.

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1 Introduction

Religious organizations provide different forms of social insurance in many countries around the globe (Chen, 2010; Iyer, 2016; Auriol et al., 2020). Several papers have documented that religious and state institutions act as substitutes, especially in response to changes in the social safety net and during periods of economic or social distress (Hungerman, 2005; Gruber and Hungerman, 2007, 2008; Dills and Hernández-Julián, 2014). However, the long-term demographic and economic implications of relying on religious safety nets remain unclear, even if they may have significant economic and political repercussions (Berman, 2000; Bazzi et al., 2019).

In this paper, we study the long-term consequences of relying on religious schools in Israel to compensate for a decline in government transfers. In addition to a traditional secular schooling system, Jewish families in Israel can enroll their children in Ultraorthodox schools which receive public funds but are independently managed and face little regulation from the state.[1] These schools focus on providing students with religious education and put little emphasis on traditional secular topics, such as math or English.[2] Unlike secular public schools, Ultraorthodox schools provide a host of important services such as a longer school day, free lunch and transportation, and other in-kind benefits which can substitute for the decline in the state's safety net.[3] To our knowledge, this is the first paper to estimate the long-run economic and demographic effects of relying on religious safety nets instead of state-provided benefits.

Every child in Israel is entitled to receive a nontaxable monthly allowance from birth until they turn 18. Until 2003, the allowance schedule increased at an increasing rate for

[1] Ultraorthodox Jews describe groups within Judaism that strictly adhere to Jewish laws and traditions and reject modern values (Berman, 2000). About 13 percent of the Israeli population can be classified as Ultraorthodox.

[2] The investment in religious literacy is typically considered more valuable for boys who can join a religious seminary after graduation. In contrast, girls who are often the sole breadwinners in Ultraorthodox households, benefit more from a more traditional education.

[3] In addition, Ultraorthodox communities provide other forms of mutual insurance, such as interest-free loans, and in-kind loans (Berman, 2000).

the third, fourth, and fifth child.[4] Our empirical strategy utilizes an unanticipated reform in Israel's child allowance program which linearized the benefits schedule with respect to the child's birth order. Specifically, the reform decreased the amount of monthly allowance provided for a third or higher birth order child born after June 1, 2003 relative to the allowance received by a child with the same birth order who happened to be born right before this date.[5] The gap in cash transfers for similarly sized families across the cutoff was significant, amounting to over 5 percent of the average yearly income of families with four or more children. Transfers for subsequent children born after 2003 were similar for treated and control families, indicating that the reform did not impact the cost of having an additional child for families across either side of the cutoff.

The reform was announced to the Israeli public in February 2003, following the election of a new government in January 2003. The new government coalition did not include any Ultraorthodox Jewish parties for whom the non-linear child subsidy program was critically important. The exact details of the reform were not announced until the end of April, 2003. Ultimately, the reform passed on May 29, 2003 and the June 1 cutoff was publicly announced. Thus, the timeline and announcement of the reform made it difficult to manipulate the timing of birth. In fact, any changes in conceptions could not have changed fertility decisions until at least November 2003. Based on this sharp and arbitrary cutoff date, we use a regression discontinuity design (RDD) and compare otherwise similar families whose higher birth-order child was born on either side of the June 1, 2003 cutoff. To investigate the validity of the design, we provide evidence that families on either side of the cutoff are similar on a wide range of demographic and economic characteristics, and show that there is little evidence of a discontinuous jump in births around the cutoff. To further alleviate concerns about

[4] Sixth and higher birth order children received the same allowance as the fifth child. As siblings age out of the program at age 18, the birth order of the younger siblings changes which impacts the overall cash transfers the family receives.

[5] The total fertility rate among women ages 15-49 in Israel was 3.0 in 2003. The total fertility rate among Ultraorthodox Jewish women in the same age group was closer to 7.5, while the total fertility rate among Muslim women and non-Ultraorthodox religious Jewish women was a little over 4. These data are based on the Statistical Abstract of Israel available from the Israel Central Bureau of Statistics available at: `https://www.cbs.gov.il/en/publications/Pages/2004/Statistical-Abstract-of-Israel-2004-No55.aspx`.

endogenous birth timing, we show that the results are robust to different bandwidth sizes and to donut hole specifications.

Our main sample includes the universe of families whose fourth or higher order child was born in 2003, but we also estimate effects on smaller families for whom the reform had a small or no impact. We use population registry data to obtain the exact date of birth of the child born in 2003 and information on the year of birth of all older and younger siblings. The age structure of older siblings born before 2003 is important as it influences the overall transfers a family receives. Information on the birth of younger siblings enable us to estimate effects on fertility. We merge this data with information obtained from the ministry of education on the schooling track (secular vs religious) in which children are enrolled in, and with information on the children's likelihood of matriculating high school. We use this information to examine whether families adjust to the decline in government transfers by reducing the human capital investment in their children to gain immediate benefits provided by Ultraorthodox schools. In addition, we combine restricted administrative data from the Israel Tax Authority for 2000-2015 which enables us to measure effects on parental labor supply and labor earnings. Although our analyses focus on the adjustments of the Jewish population, we also estimate results for the Arab population who was impacted by the same decline in government transfers but cannot benefit from the amenities offered by Ultraorthodox schools.

Consistent with the hypothesis that Ultraorthodox schools act as religious safety nets, we find that the reform increased the likelihood that Jewish boys ages 5-12 in 2003 enroll in Ultraorthodox schools. The change of the schooling track at young ages persists over time and leads to a decline in the probability of matriculating high school. The effects are smaller for older children for whom it is not optimal to change human capital investments and for younger children ages 0-4 in 2003. The effects on Jewish girls are smaller and statistically insignificant. The change in children's enrollment is concentrated among marginally religious families who did not typically enroll their children in Ultraorthodox schools prior to the reform.

We find little evidence that the income effect from reducing child-related cash benefits changed the fertility outcomes of Jewish families in the short- or long-term. Moreover, we do not find evidence that Jewish fathers or mothers adjusted to the decline in cash benefits by increasing their labor supply, and we do not detect a change in their labor earnings in response to the reform. Thus, the immediate benefit from enrolling children in Ultraorthodox schools enabled Jewish families to maintain their fertility choices without necessitating an increase in parental labor supply.

In contrast, we find that the reform reduced the number of additional children born to Arab families whose fourth or higher order birth occurred after June 1, 2003 by about 10 percent. This effect persists over time, indicating a change in completed fertility, and is consistent with prior evidence that children are a normal good (Lindo, 2010). We find some evidence that the decline in cash benefits led to an increase in the labor supply of Arab fathers, but these effects are not consistently estimated with precision. Lastly, there is little evidence that the reform impacted the educational outcomes of Arab children, which indicates that the decline in fertility prevented families from reducing the investment in their children's schooling.

The paper contributes to the literature in several important ways. First, we contribute to the literature on state capacity, economic conditions, and religiosity by providing evidence that families compensate for the decline in welfare payments by relying on the services and amenities provided by religious schools (Chen, 2010). Importantly, we show that the short-term access to religious safety nets comes at the cost of reducing children's long-term human capital accumulation. This substitution enabled Jewish, but not Arab families, to continue and have high fertility rates which, in the long run, may have increased the economic burden on the state and contributed to religious and social tensions. In contrast, prior literature has primarily focused on estimating the relationship between economic conditions and religious participation, as measured by religious attendance and charitable donations but did not examine the long-term effects of increased religiosity on fertility, labor market participation, or children's long-term educational outcomes (Hungerman, 2005; Gruber and Hungerman, 2007; Dills and Hernández-Julián, 2014; Bazzi et al., 2019).[6]

Second, we contribute to the literature on resource allocations across children in response to non-labor income shocks. Specifically, the change in the schooling decision for some children in the family is consistent with evidence that families make heterogeneous adjustments to their children's education based on their age at exposure, and with evidence that families discount the future consequences of their investment decisions in response to immediate income shocks (Shah and Steinberg, 2017; Carrillo, 2020; Bau, 2021). In comparison to the findings of Shah and Steinberg (2017) and Carrillo (2020), we show that these heterogeneous adjustments occur even when the opportunity cost of attending school does not change.

Third, we provide evidence that child-related monthly unconditional cash transfers have large effects on fertility for families that have no access to informal welfare networks. This is in contrast to the transitory effects of one-time cash benefits provided at birth and the small fertility effects of U.S. welfare programs (González, 2013; Hoynes et al., 2016). Moreover, our identification strategy enables us to isolate the effects of cash transfers on fertility, holding the price of an additional birth constant. In comparison, previous studies on the effects of changes in expected child allowances on fertility were unable to distinguish between the income and price effects (Milligan, 2005; Cohen et al., 2013).[7] In addition, we contribute to the growing literature on the effects of unconditional cash transfers on labor supply (Marinescu, 2018). Consistent with previous studies, we find that reducing unconditional child-related cash benefits have small positive effects on labor supply or labor earnings, indicating that the labor supply of non-work income elasticity is small (Akee et al., 2010; Cesarini et al., 2017; Jones and Marinescu, 2022).

[6]For example, Hungerman (2005) used the 1996 welfare reform in the U.S. to show that church activities substitute for government services.

[7]Cohen et al. (2013) used variation in the generosity of the child allowances program in Israel in a difference-in-differences framework to estimate the impact of financial incentives on fertility. They finds that a decline in child allowances lowers fertility, but their analysis cannot isolate the income effect of the policy.

Lastly, we contribute to the literature on unconditional child-related cash payments by examining how families adjust to a decline in unconditional cash benefits. In contrast, much of the evidence on the effects of income transfers on families and children have relied on policies that increase access to food or impact family resources by incentivizing labor force participation (Hoynes et al., 2016; Bastian, 2020; Aizer et al., 2022).[8] None of these studies, however, examined how families compensate for a decline in child-related cash transfers, and the implications such adjustments have on children's long-term outcomes. This is important to inform the design of safety net programs, because the effects of expansions or contractions in generosity may not be symmetric.

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

Israel's child subsidy program is a nontaxable monthly income transfer which all mothers receive from the birth of each of their children and until each of them 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 (Frish, 2004; Cohen et al., 2013), and can be thought of as a monthly unconditional cash transfer lasting for 18 years per child.

While eligibility is unconditional, the amount of transfers was historically designed to

[8] For instance, transfers from the Food Stamps program and the Earned Income Tax Credit (EITC) have been shown to have small effects on fertility, larger effects on employment and hours of work, and improvements in children's short- and long-run outcomes (Hoynes and Schanzenbach, 2012; Hoynes et al., 2016; Bastian and Michelmore, 2018; Bastian, 2020). In comparison, evidence from programs that provide one-time cash benefits to children indicate that these transfers increase short-term fertility and lengthen mother's non-participation in the labor force, but have little effects on children's long-term outcomes (González, 2013; Borra et al., 2021).

increase at an increasing rate for the third, fourth, and fifth child. Sixth or higher order children received the same allowance as the fifth child. This convex timetable was a key demand of the ultraorthodox Jewish parties, who speak for voters who typically have large families and low labor market attachment. In 2003, a new Israeli government coalition, which was formed without the support of any ultraorthodox parties, unexpectedly voted to reform the payment schedule of child allowances. The so-called Netanyahu reform linearized the payment schedule for children born after June 1, 2003 so that each child, regardless of his/her birth order, receives the same payment (see Table 1). The reform was announced in April 29, 2003 and was voted into law on May 29, 2003. As a result of the sharp June 1 threshold, the allowance for an additional child born in 2003 in families who already had two or more children varied significantly based on whether the birth occurred before or after the reform cutoff. For example, in 2004, a mother with 4 children under the age of 18 whose fourth child was born in July of 2003 received NIS 614 per month in child allowances compared to NIS 1,048 per month that a mother whose fourth child happened to be born in May 2003.[9] These payments changed quite frequently over the years that followed but the sharp distinction between children who were born before and after the June 2003 cutoff was maintained.

To illustrate the significance of the difference in child allowances generated by the reform, Figure 1 plots changes in the yearly child allowance payments in households with the same number and age distribution of children, except that the child in 2003 was born either just before or just after the cutoff. In all the simulations that we present we assume three year spacing between siblings. Panel A shows this comparison for a family whose fourth child was born in 2003. In families whose fourth child was born just before the cutoff, the total allowance payment increased by 35 percent between 2002 and 2004, while families whose fourth child was born just after the cutoff experienced a 12 percent decrease during the same period. The difference between the payments received by these two types of households

[9] We use 2021 NIS throughout. During that year, the average exchange rate was 1 USD = 3.23 NIS.

remains substantial (20 to 50 percent) until 2012, when the oldest sibling turned 18 and the fourth child became the third for the purpose of the allowance calculation. A smaller difference remained for 3 additional years until another sibling aged out of eligibility and starting 2015, the benefit amounts evened-out for the two households. Over the entire period 2000-2018, the difference in non-work income between the households amounted to NIS 27,300 ($8,800 based on the 2021 exchange rate), which, based on our calculation, amounts to about 5 percent of average yearly income for these families. This simulation also highlights that the age structure of older siblings is an additional source of variation in allowance amounts (and will thus be accounted for in the empirical analysis).

In Panel B we present a comparison of families with the same age structure as in panel A except that a fifth child is 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 birth of the fourth child, the difference between the payments that they receive remains identical to the difference in panel A. This demonstrates that the 2003 reform did not generate variation in expected benefits for future children who were not yet conceived at the time of its enactment.

Panels C and D present this comparison for families with 3 and 5 children, respectively, in which the last child was born in 2003. These figures show that the difference across the cutoff increases with the parity of the child born in 2003, starting with the third child. Finally, panels E and F show that the reform cutoff did not make any difference for 1st or 2nd births. The simulations indicate that the effect of the decrease in child allowances was most strongly felt by those families with four or more children whose labor earnings are typically low.

3 Data

Our main sample covers the universe of Israeli households who had a child during 2003. For these children, the data include exact dates of birth which are important for our identification strategy. Because the total amount of allowance varies by the child's birth order, we also draw information on the year of birth of all the siblings. The birth timing of younger siblings will additionally serve as an outcome in our analysis of fertility choices following the reform.

We combine several restricted administrative data sources on parents' and children's characteristics and outcomes to facilitate a comprehensive analysis of the reform's impact. Basic demographics such as sex, ethnicity (Jews/Arabs), locality of residence in 2003, country of origin, birth and immigration year come from the population registry. We define households' socioeconomic status (SES) cluster by their locality of residence using the Israeli Central Bureau of Statistics classification. In the registry data we also observe the marital status of each parent between 2002-2017. In addition, our records include parental education (highest certificate), the type of schools that children attend (secular, religious, or ultraorthodox), and whether children matriculated or dropped out from high school as recorded by the Israeli Ministry of Education.[10] School records are available for all children between 1991-2018 enabling us to follow households pre- and post-reform.

Lastly, information on parents' labor market outcomes are drawn from the Israel Tax Authority records which include information on salary workers as reported by their employers, and on self-employed individuals. These data cover the years 2000-2015, allowing us to follow parents' outcomes for 12 years after the reform. During this period, we observe the annual number of months worked and the annual labor earnings for each parent individually.

Table 2 presents descriptive statistics for families whose fourth or higher parity child was born in 2003 which are the main focus of our study. We first show these statistics for births throughout 2003, and then limit the sample to 70 days before and after the reform cutoff as we do in our main analysis. The average characteristics are practically identical for these two samples. Compared to the full sample of families with children born in 2003 presented in Appendix table A.1, these families have less educated parents, with lower labor force participation and substantially lower income (30% lower at the household level). This

[10]These data include certificates obtained abroad.

is despite the fact that, by definition, the parents in the high parity births sample are on average three years older. The share of Arabs and Jewish ultraorthodox families is also higher in the high parity births sample, as would be expected given the higher fertility rates in these population groups. An additional difference worth noting is the much lower age at first birth for mothers with higher parity births.

In columns (3) and (4) 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. For instance, Jewish parents are more educated, have higher labor earnings, and have a wider choice over the type of school their children attend. These differences are in line with the ones observed for the entire population reported in Appendix table A.1.

4 Methodology

4.1 Empirical Strategy

The causal effect of the reform is estimated using a regression discontinuity design based on the exact date of birth of children born in 2003. As described above, in 2003 the Israeli government set an arbitrary cutoff according to which children born after June 1, 2003 in families with at least two children under the age of 18 received a lower monthly child allowance compared to children in similarly sized families but who happen to be born right before June 1. As the simulations presented above show, the difference in the allowance amount was substantially larger for households whose fourth or higher birth order child, and thus, our analysis focuses on these households. Since the reform had no impact on families whose first or second child was born in 2003, and a smaller impact on families whose third child was born in 2003, we use these families to conduct placebo tests.

Importantly, the differential effect of the reform only applied to the specific child born in 2003 and did not impact the payment schedule that families across the threshold received for their older children or future additional children. 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 and any difference in outcomes between households with a birth before and after the cutoff can be attributed to a pure income effect of the change in cash transfers.

Formally, we estimate the following specification:

$$Outcome_h = \alpha + \beta D_h + \gamma days_h + \delta \cdot days_h \times D_h + \theta' X_h + \epsilon_h, \tag{1}$$

where D_h is an indicator equal to one if the child in household h was born on or after June 1st, 2003 (the reform cutoff). The running variable $days_h$ is defined as the number of days between the exact date of birth in 2003 and the cutoff date. In our main specification, we include a linear trend in this variable which can vary across the threshold. X_h is a vector of household level characteristics that are time invariant or measured prior to 2003. These characteristics include ethnicity (Jewish, Arab, or Other), parents' age and its quadratic, mother's age at first birth, months of employment and labor earnings during 2000 for each of the parents, and locality of residence fixed effects. Because the amount of child allowances and their duration depend on the family size and the age distribution of existing children, X_h also includes the number and the age composition of siblings in 2003. In addition, we control for the sex of the child born in 2003 and for whether it was a twins birth. ϵ_h is an idiosyncratic error term.[11]

Our outcomes of interest include short-term adjustments in the number of children born after the reform, the number of months worked by each parent, household earnings, and the schooling track in which children are enrolled. We estimate the long-term effects of the reform (up to 12 years after the reform) on completed fertility, parental labor market

[11] An alternative approach is to use the reform as an instrumental variable for the change in child allowances. Although we estimate the effect of the reform on the change in government payments, the reform may have impacted our outcomes in channels other than the income change which would violate the exclusion restriction.

outcomes, and children's likelihood of matriculating high school.

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 optimally selected bandwidths (Calonico et al., 2019, 2020). The advantage of using a fixed bandwidth is that the treatment effects for all outcomes and time horizons are estimated using the same sample. In addition, we test the robustness of our results to different weighting methods.

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

4.2 Investigating the Validity of the Research Design

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

Families, however, would have an incentive to give birth before the cutoff to maximize the allowance for their newborn child.[12] 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, cesarean sections.[13] We test for manipulation of the running variable following McCrary (2008) and Cattaneo et al. (2018) by plotting the distributional density of four or higher parity births around the reform cutoff.

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

[13]These procedures are not as prevalent in Israel relative 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. Abortions in Israel are not easily accessed and women from religious backgrounds are unlikely to use them a contraceptive method.

As can be seen from Figure 2, there is no significant discontinuity in the number of births at the cutoff when we include all population groups in the analysis. Panels (a) and (b) of Figure 3 show that the same is true for the Arab and Jewish sub-populations.

To further assess the plausibility of the identifying assumption, we test for balance in observed household characteristics across the reform cutoff. For this purpose, we estimate a specification similar to Equation 1 using each of the observed household characteristics as an outcome. The results in Table 3 provide evidence that most of the differences between treatment and comparison households are small and statistically insignificant. This is shown for the entire population and also separately for the Arab and Jewish populations. Out of the 52 estimates, the only two that are statistically significant or marginally significant are for mother's age and the number of older siblings who are 14 to 17 years old for the Arab population. According to these estimates, post-cutoff Arab mothers might be half a year older that those pre-cutoff, and have 0.05 more teenage children. Although such small differences are expected even when assignment is as good as random, these characteristics as well as the ones that do not show significant differences will be controlled for in our regressions. Additionally we check the robustness of our findings to donut hole RD specifications which omit observations close to the cutoff to avoid any potential bias due to manipulation in the timing of births.

5 Results

5.1 Short-Term Adjustments in Children's Schooling

The K-12 public education system in Israel can be broadly categorized into four groups: Secular Arab schools, Secular Jewish schools, Religious Jewish schools, and Independent Ultraorthodox (also know as "Haredi") schools. Ultraorthodox schools are single-sex and receive public funding. The generosity of the funding to these schools depends on government coalition agreements with Ultraorthodox parties. Historically, these coalition agreements enabled Ultraorthodox schools to manage their budget and determine their educational curricula without significant regulation from the state (Barak-Corren and Perry-Hazan, 2022). Consequently, these schools primarily focus on teaching religion and provide little formal secular education (e.g., math, science or English) (Kingsbury, 2020). Moreover, Ultraorthodox schools are able to use their budgets to provide valuable amenities that are not available in mainstream public schools, such as a longer school day, free meals, and free transportation (Schiffer, 1999; Blass and Bleikh, 2016).

In Table 4, we estimate the effect of the reform on the likelihood that families enroll their children in an Ultraorthodox school after 2003. In Panel A, we focus on the results for children ages 0-4 in 2003. These children would have not been enrolled yet in elementary school at the time of the reform and thus families could have not changed their schooling track to gain access to the amenities provided by Ultraorthodox schools. The results in Panel A indicate that the reform had little impact on the likelihood that Jewish boys or girls in this age group ever attended an Ultraorthodox school between grades 1 and 8 (Columns 2 and 3). The results in Column 4-6 of Panel A also provide little evidence that the reform changed the likelihood that these children ever attended an Ultraorthodox school between grades 9 and 12.

In Panel B of Table 4 we present the results for children who were 5-12 years old in 2003. These children were about to enter elementary school or were already enrolled in school at the time of the reform, and switching them to an Ultraorthodox school would have enabled families to gain access to important amenities that can substitute for the immediate loss in cash transfers. The results indicate that the reform increased the likelihood that school-aged children in 2003 ever enrolled in an Ultraorthodox school between grades 1-8 by 2.6 percentage points, or about a 4 percent increase relative to the mean among control families (Column 1). Similarly, the results in Column 4 indicate that the reform increased the probability that young children in 2003 ever attended an Ultraorthodox high school (grades 9-12) by 3.8 percentage points, about a 6 percent increase relative to the mean. Both effects are statistically significant at conventional levels, and provide strong evidence that enrolling children in an elementary Ultraorthodox school has long-term and persistent effects on the high school track that children attend. These overall effects are stronger for boys than girls, consistent with the larger return to religious schooling for boys and the higher return to secular schooling for girls. Specifically, the results in Column 2 indicate that the reform increased the probability that Jewish boys ever enrolled in an Ultraorthodox elementary school by 4.1 percentage points, an effect that is statistically significant at the 1 percent level, while having a much smaller and statistically insignificant effect on girls (Column 3). The effects of the reform on enrolling in an Ultraorthodox high school is also larger for boys than girls, but the effects by gender are not statistically different.[14]

These results are driven by families who did not enroll any of their children in Ultraorthodox schools prior to the reform and are larger for younger children. Moreover, we find little evidence that the decline in cash benefits increased Yeshiva attendance by fathers (Appendix Table A.2). This is consistent with the fact that attending a Yeshiva requires prior human capital investments in religious education and a high level of commitment to the community (Berman, 2000). The school enrollment and Yeshiva attendance results suggest that the reform led Jewish families on the margins of Ultraorthodoxy to increase their commitment to the community in order to access benefits provided by Ultraorthodox schools (Chen, 2010)

5.2 Children's Long-Term Educational Attainment

We next examine whether the reform, partially through its impact on the schooling choices of parents, affected the long-term educational outcomes of children. We focus on the effects of the reform on the likelihood of receiving a matriculation certificate (Bagrut). Earning a Bagrut diploma is an important prerequisite for college enrollment and is required for many entry-level positions in the labor market. To earn a diploma, students are expected to pass centrally administrated tests worth 20 or more credits, which include minimum credits that

[14] We find no evidence that the reform impacted the probability of ever enrolling in an Ultraorthodox high school among children who were 13-17 years old in 2003.

vary by subject. These minimum requirements to matriculate high school are similar across the different schooling tracks.

The results in Column 1 of Table 5 indicate that the reform reduced the likelihood that Jewish boys ages 5-12 in 2003 obtained a Bagrut diploma by about 4.4 percentage points, an estimate that is statistically significant at the 1 percent level (Column 1). Relative to the average among families who gave birth to their fourth child prior to June 1, this result implies that the reform reduced the likelihood of matriculating high school for Jewish boys by about 18 percent. The estimated effects of the reform on matriculating high school for Jewish boys who were 1-4 or 13-17 years old at the time of the reform are negative but not statistically significant. Similarly, there is little evidence that the reform impacted the probability of matriculating high school for Jewish girls, regardless of age at exposure (Column 1, Table 6. There is also little evidence that the reform impacted the long-term educational outcomes of Arab children (Appendix Table A.3).

We also examine whether the reform affected the quality of the Bagrut diploma, unconditional on matriculation. Specifically, we estimate the effect of the reform on the overall number of credits a student has successfully completed, and the probability of completing 4 or more credits in English and mathematics (the minimum credits required for these subjects are three). The results in Columns 2-4 of Table 5 provide evidence that the reform reduced the quality of the high school diploma for Jewish boys ages 5-12 in 2003. The reform reduced their likelihood of completing four credits of Mathematics and English, and the overall number of completed credits. The results are economically meaningful and statistically significant.[15] The results for boys ages 1-4 or 13-17 and for girls of all ages are negative but are smaller in magnitude and, with two exceptions, cannot be distinguished from zero.

Overall, the results are consistent with prior findings indicating that parents make heterogeneous adjustments to their children's human capital investments in response to income

[15]Specifically, the reform reduced the probability that 5-12 year old children in 2003 complete at least 4 credits of English and Mathematics by 25 percent (columns 2-3, Panel B of Table 5). The reform led to a similar 25 percent decline in the overall number of completed credits.

shocks, and that parents may trade-off the receipt of immediate benefits at the cost of a substantial decline in their children's long-term human capital accumulation (Shah and Steinberg, 2017; Carrillo, 2020). The results by age-at-exposure suggest that the increased likelihood of enrolling children in Ultraorthodox schools to compensate for the decline in cash transfers is an important mechanism through which the reform impacted long-term educational outcomes.

5.3 Fertility and Labor Market Adjustments

Under the assumption that children are a normal good, economic theory predicts that a decline in non-labor income is expected to reduce fertility (Lindo, 2010; González, 2013). Did the reform in Israel's child allowance impact the short- or long-term fertility of Jewish mothers?

In Table 7, we estimate the effect of the reform on the number of subsequent children born two years after the reform (up to 2005). The results in Column 1 of Panel A show no evidence that the decline in cash transfers lowered the short-term fertility of Jewish mothers. There is also no evidence that the reform led to a decline in completed fertility, as measured by the number of additional children born through 2015 (Column 2). In fact, both estimates are positive but statistically insignificant. In contrast, the results in Panel B for the Arab population show that the reform led to 0.05 decline in the number of children born two years after the reform, a decline of about 12 percent relative to the mean among the control group (Column 1). This decline persisted over time, indicating that the reform impacted completed fertility. By 2015, the decline in cash transfers reduced the fertility of Arab mothers by about 10 percent (0.117/1.145). The effects on completed fertility are depicted graphically in Panels A and B of figure 4.[16] Thus, the results indicate that the short-term adjustments that Jewish families on the margin of Ultraorthodoxy were able to make enabled

[16] Each observation in these figures measures the average number of children born 9 years after the reform grouped in one week bins. To estimate the solid lines, we first residualize the fertility outcomes based on the controls included in equation 1, after which we estimate the impact of the reform. The shaded grey areas represent the 95 percent confidence intervals.

them to maintain their fertility at the cost of lowering the educational attainment of some of their older children.

The reform had no impact on the child allowances received by families whose first or second birth was in 2003, and we expect the reform to have no impact on their fertility or labor market decisions. Reassuringly, as we show in Panel A of Table 8, we find no evidence that the reform impacted the completed fertility of Arab or Jewish mothers who had their first or second birth in 2003. In Panel B, we estimate the effects of the reform on families whose 3^{rd} child was born in 2003 for whom the reform led only to a small decline in cash benefits. The estimated effects for both the Arab and Jewish populations are negative and similar in size, but neither is statistically significant.

In Columns 3-4 of Table 7 we report the effects of the reform on the cumulative number of months worked during the three years following the reform (2003-2005) by fathers and mothers, respectively, where non-employment is coded as zero.[17] On average, Arab and Jewish fathers in families whose 4^{th} child was born prior to June 1, 2003 worked about 20 months in the 3-year period following the reform, or just over 6 months per year (Column 3 of Panels A and B). The low levels of employment among Jewish men can be explained by the high share of Ultraorthodox 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.[18] Jewish mothers worked about 17 months during 2003-2005, a substantially higher labor market participation compared to Arab mothers who, on average, worked about 4 months during the same period.[19]

[17]This variable is reported by the Israel Tax Authority at the yearly level. It counts the number of months in which an individual worked non-zero days within each month.

[18]The underemployment of both Arab and Jewish men could also reflect a higher likelihood of tax evasion. It is unlikely, however, that the propensity for tax evasion would vary across treated and control groups.

[19]The employment rates of Arab women with a large number of children in Israel has been historically very low. In contrast, many Ultraorthodox Jewish women who typically have large families work outside the home as their husbands attend the Yeshiva.

The results in Panel A of Table 7 provide little evidence that the reform impacted the short-term labor supply decisions of Jewish fathers or mothers. The estimated effect for Arab fathers in Panel B is larger in magnitude, suggesting that the reform led to an increase of about 0.9 months in their labor supply, or about 4.5 percent relative to the mean. This effect, however, is not statistically significant. In the last two columns of Table 7 we estimate the effect of the reform on the number of months worked during 2013-2015 (7-9 years after the reform). The results continue to suggest that Jewish parents did not adjust their labor supply decisions in response to the decline in cash transfers. In contrast, there is more robust evidence that Arab father increased their labor supply 1.8 months, about 8 percent increase relative to a mean of 21 months in the control group.[20]

We conduct several additional checks to examine the robustness of results. In Appendix Table A.4 we show that, as expected, using triangular kernel weights slightly increases the magnitude of the effects on fertility. We also verify that the results are robust to using an automatic bandwidth selector (Calonico et al., 2019, 2020) which reduces the bandwidth selection from 70 to about 50 or fewer days. The results in Appendix Table A.5 indicate that that the decline in cash transfers reduced the completed fertility of Arab families by about 20 percent relative to the mean. Moreover, the long-run effects for Jewish families suggest that the reform increased their completed fertility by about 7 percent relative to a mean of 1.5 additional children. As an additional placebo test, we verify that the completed fertility of Arab or Jewish families who gave birth after June 1, 2002 does not change compared to families who gave birth right before this fictitious cutoff (see Appendix Table A.6).

Jewish and Arab families are eligible to receive the same benefits from Israel's social safety net programs. Thus, it is unlikely that differences in long-term outcomes between them can be attributed to differential access to alternative welfare programs that may have substituted for the decline in child allowances. Instead, the results indicate that Jewish families adjusted to the decline in government transfers by changing their investments in children's human

[20] As we report in Appendix Table A.7, we find little evidence that the reform impacted households' labor earnings.

capital, while Arab families responded by decreasing their fertility (Berman, 2000; Chen, 2010).

6 Conclusion

Several studies have documented that religious institutions act as an insurance against unanticipated income shocks (Chen, 2010; Auriol et al., 2020). In this paper, we examine the long-term economic and demographic effects of relying on religious safety nets in response to a decline in government transfers.

Specifically, we examine the effects of a reform to the Israeli child allowance program which reduced the cash benefits received by large-sized families. The decline in the benefits was unexpected and primarily impacted families whose 4^{th} or higher birth order child was born after June 1, 2003. Based on this sharp and arbitrary cutoff, we use a regression discontinuity design to compare the short-term adjustments of otherwise similar families around the reform's cutoff, and to study the long-term effects of the reform on families and children. We provide evidence that the reform did not lead to a discontinuous jump in the number of births or selectively changed the timing of birth for families around the cutoff.

We show that Jewish families substituted for the immediate decline in welfare transfers by enrolling their young boys in religious schools run by Ultraorthodox communities. These schools do not provide a typical secular education but provide other important services such as a longer school day and a free meal. Consequently, we find that this short-term adjustment led to a decline in the likelihood of matriculating high school, and an increase that children drop out from high school. The change in schooling decisions is driven by families who did not enroll any of their children in Ultraorthodox schools prior to the reform, indicating that they relied on religious schools as a social safety net. This trade-off prevented Jewish families from having to alter their fertility or labor market decisions. Arab families, who do not have access to this alternative insurance mechanism, adjusted by reducing their completed fertility with some evidence of an increase in fathers' labor supply.

The results of the paper have important policy implications related to the unintended effects of reducing the generosity of the state's social safety net, especially in the presence of alternative insurance mechanisms. In Israel's case, the increased reliance on Ultraorthodox schools in response to a decline in government support has likely resulted in increased future economic burden on the state and a potential increase in social and political tensions.

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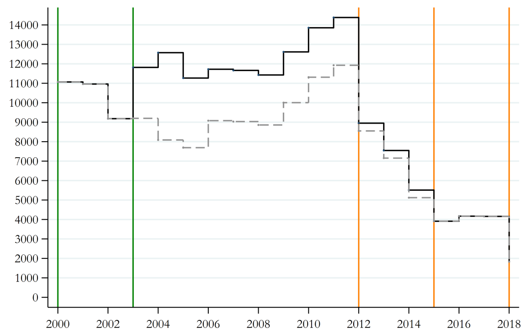
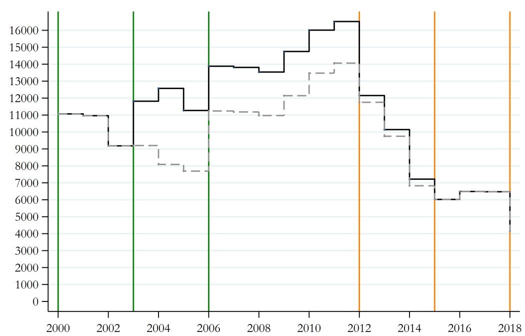
Figure 1: Household Child Allowance Stream 2000-2018

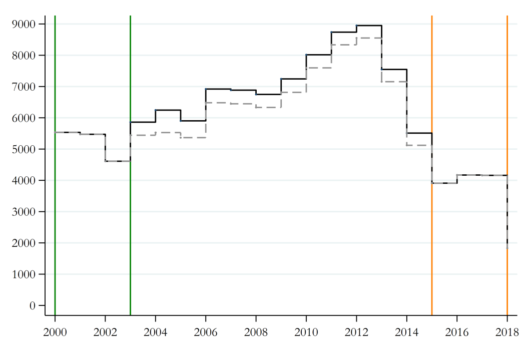
(a) 4th in 2003, gap NIS 27,300



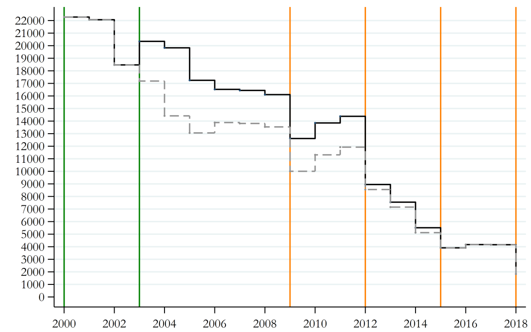
(b) 4th in 2003 and 5th in 2006, gap NIS 27,300



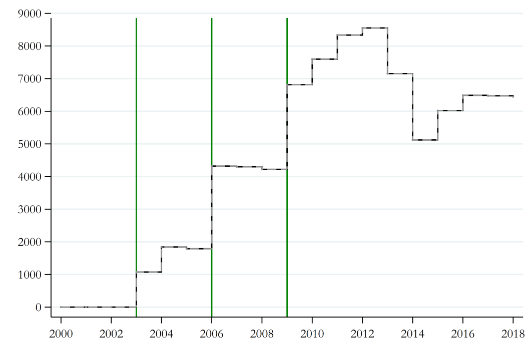
(c) 3rd in 2003, gap NIS 5,397



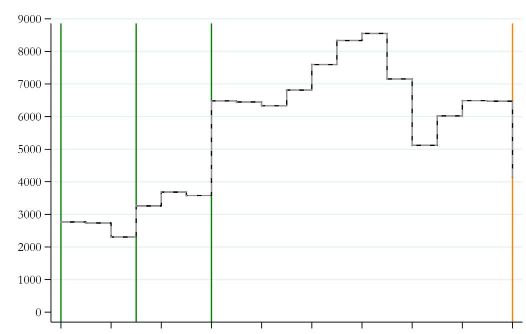
(d) 5th in 2003, gap NIS 29,382



(e) 1st in 2003, gap NIS 0

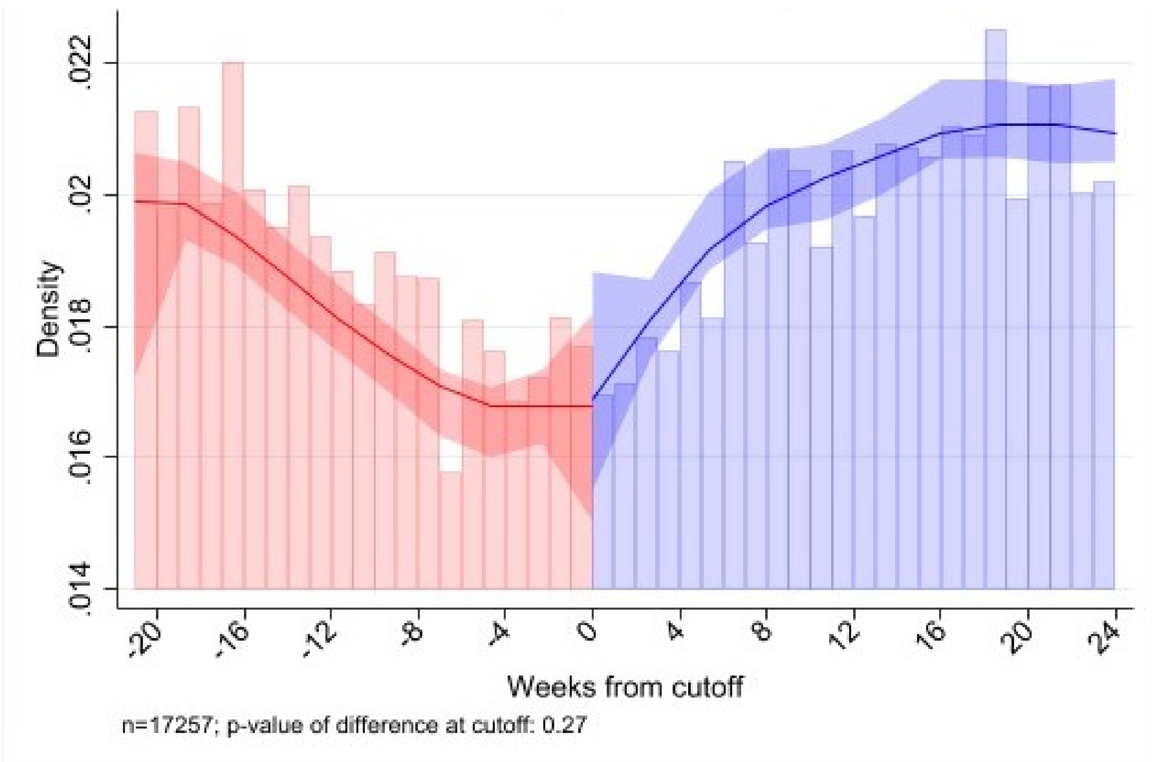


(f) 2nd in 2003, gap NIS 0



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

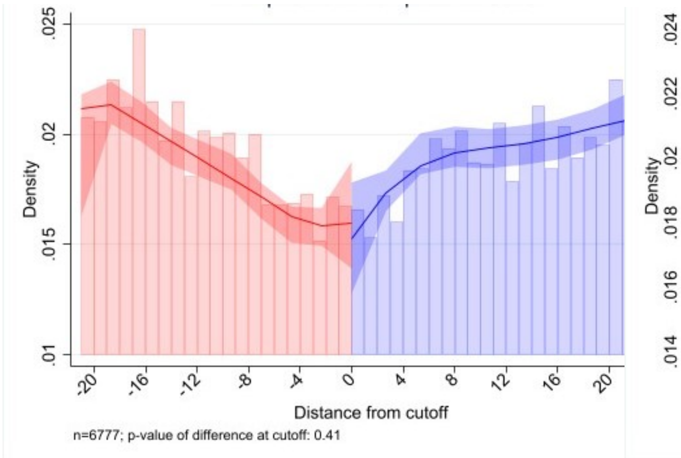
Figure 2: Manipulation Test



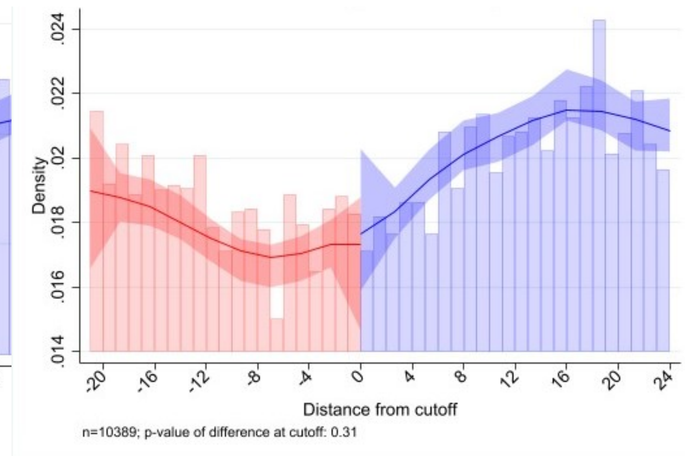
Notes - The figure presents the distributional density of four or higher parity births around the reform cutoff (McCrary, 2008; Cattaneo et al., 2018). The number of observations and the p-value for the significance of the discontinuity at 0 is listed just below the graph.

Figure 3: Manipulation Tests by Population Group

(a) Arab



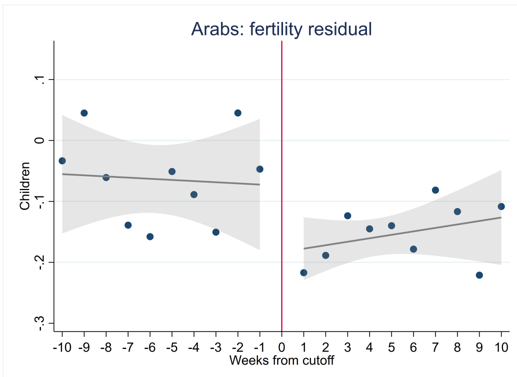
(b) Jewish



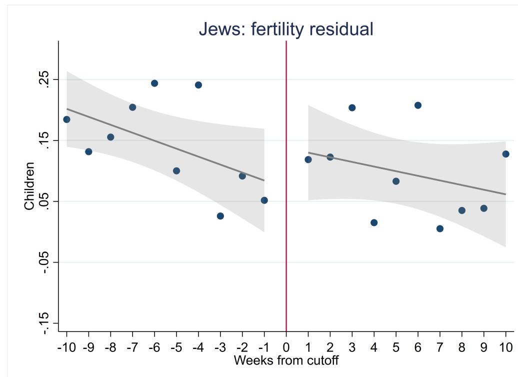
Notes - Panel (a) and (b) presents the distributional density of four or higher parity births around the reform cutoff for the Arab and Jewish populations (McCrary, 2008; Cattaneo et al., 2018). The number of observations and the p-value for the significance of the discontinuity at 0 is listed just below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

Figure 4: Income Effect of Reducing Cash Benefits on Completed Fertility

(a) Arab



(b) Jewish



Notes - Each observation is the average completed fertility, defined as the cumulative number of children born to a mother nine years after the reform, for mothers in a one-day bin based on the birth date of the child. The vertical line denotes the reform cutoff date, which has been normalized to 0. The solid lines are estimated using a polynomial regression based on daily individual-level data. Shaded area indicates 95 percent confidence interval.

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 allowance schedule for selected years by birth order. The allowance amounts are in 2021 NIS.

Table 2: Descriptive Statistics, Families with 4+ Children (in 2003)

		Birth +/- 70 Days from June 1, 2003		
	All	All	Arabs	Jews
Father with more than HS education	0.171	0.174	0.094	0.22
	(0.376)	(0.379)	(0.292)	(0.414)
Mother with more than HS education	0.234	0.236	0.064	0.326
	(0.423)	(0.424)	(0.245)	(0.469)
Jews	0.593	0.597		
	(0.491)	(0.491)		
Arabs	0.403	0.398		
	(0.49)	(0.49)		
Jewish Orthodox before 2003	0.306	0.308		0.517
	(0.461)	(0.462)		(0.5)
Father's age in 2003	36.41	36.44	36.22	36.58
	(6.058)	(5.96)	(6.086)	(5.869)
Mother's age in 2003	32.79	32.81	31.76	33.49
	(4.861)	(4.834)	(4.757)	(4.763)
Father's months of employment in 2003	6.319	6.316	6.504	6.185
	(5.689)	(5.7)	(5.554)	(5.794)
Mother's months of employment in 2003	3.576	3.5	1.257	5.002
	(4.958)	(4.883)	(3.316)	(5.182)
Father's labor earnings in 2003	39,172.0	39,826.7	31,242.0	45,604.2
	(62,404.60)	(64,237.22)	(45,291.55)	(73,840.50)
Mother's labor earnings in 2003	13,380.9	13,347.0	3,605.8	19,859.9
	(26,540.9)	(27,883.5)	(12,161.6)	(32,991.9)
Household labor earnings in 2003	52,552.9	53,173.6	34,847.8	65,464.1
	(73,733.49)	(75,814.22)	(48,821.4)	(87,427.1)
Mother's age at first birth	22.20	22.20	21.47	22.67
	(3.196)	(3.224)	(3.34)	(3.046)
Number of siblings under age 5 in 2003	0.862	0.86	0.848	0.869
	(0.345)	(0.347)	(0.359)	(0.338)
Number of siblings age 5-9 in 2003	0.91	0.917	0.919	0.917
	(0.286)	(0.275)	(0.273)	(0.276)
Number of siblings age 10-13 in 2003	0.514	0.517	0.482	0.54
	(0.5)	(0.5)	(0.5)	(0.498)
Number of siblings age 14-17 in 2003	0.248	0.247	0.221	0.264
	(0.432)	(0.431)	(0.415)	(0.441)
Number of children	5.369	5.357	5.241	5.441
	(1.565)	(1.568)	(1.445)	(1.642)
Twin Birth	0.029	0.031	0.021	0.036
	(0.167)	(0.172)	(0.145)	(0.187)
Male birth	0.513	0.51	0.498	0.518
	(0.5)	(0.5)	(0.5)	(0.5)
Observations	35,410	11,662	4,296	7,310

Notes - This table reports sample means. Column 1 reports sample means for all 4+ families in 2003. Column 2 reports sample mean for the estimation sample using a 70 day bandwidth across the cutoff. Columns 3 an 4 reports sample means for the Arab and Jewish populations, respectively.

Table 3: Balance table of treated and untreated families of parity 4+

	Whole Population	Arab	Jewish
Father with more than HS education	-0.010 (0.015)	-0.026 (0.019)	0.003 (0.020)
Mother with more than HS education	-0.006 (0.017)	-0.015 (0.017)	0.003 (0.023)
Jewish	-0.015 (0.020)		
Arab	0.018 (0.018)		
Jewish Orthodox in 2002	0.000 (0.018)		0.021 (0.024)
Father's age in 2003	0.213 (0.224)	0.236 (0.375)	0.213 (0.280)
Mother's age in 2003	0.214 (0.180)	0.418 (0.297)	0.154 (0.223)
Father's months of employment in 2000	0.052 (0.212)	-0.122 (0.338)	0.097 (0.270)
Mother's months of employment in 2000	0.107 (0.195)	0.244 (0.218)	0.157 (0.254)
Father's labor earnings in 2000	345.386 (2181.042)	161.493 (2171.450)	724.837 (3173.573)
Mother's labor earnings in 2000	3977.865 (3908.207)	580.264 (698.130)	6387.402 (6089.700)
Mother's age at first birth	0.056 (0.121)	0.022 (0.212)	0.131 (0.143)
Number of siblings under age 5 in 2003	0.006 (0.013)	0.001 (0.023)	0.014 (0.016)
Number of siblings age 5-9 in 2003	-0.011 (0.010)	-0.016 (0.017)	-0.009 (0.013)
Number of siblings age 10-13 in 2003	0.022 (0.019)	0.044 (0.032)	0.011 (0.023)
Number of siblings age 14-17 in 2003	0.024 (0.016)	0.060** (0.026)	0.003 (0.021)
Number of Children in the family	0.060 (0.058)	0.085 (0.086)	0.064 (0.076)
Twin Birth	0.004 (0.006)	0.005 (0.009)	0.002 (0.008)
Male	0.008 (0.019)	-0.001 (0.032)	0.014 (0.024)
Observations	11,662	4,296	7,310

Notes - Column 1 reports results from regressions testing for balance of observable characteristics. Columns 2-3 report results from similar regressions for the Arab and Jewish sub-populations, respectively. Standard errors are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 4: Income effects of reducing cash benefits on enrollment in religious schools

	UO Enrollment Grades 1-8			UO Enrollment Grades 9-12		
	All	Boys	Girls	All	Boys	Girls
Panel A: Ages 0-4						
Treatment	0.006	0.003	0.009	0.006	0.019	-0.008
	(0.014)	(0.017)	(0.017)	(0.015)	(0.018)	(0.018)
Observations	16864	8676	8188	16585	8631	7954
Control mean	0.680	0.691	0.667	0.637	0.648	0.624
Panel B: Ages 5-12						
Treatment	0.026**	0.041***	0.012	0.038***	0.047***	0.030*
	(0.012)	(0.015)	(0.015)	(0.012)	(0.015)	(0.016)
Observations	18079	9261	8818	17897	9261	8636
Control mean	0.664	0.681	0.647	0.628	0.646	0.609

Notes - This table reports the effects of the reform on the probability of enrolling Jewish children in Ultraorthodox (UO) schools. The sample includes families whose 4^{th} child was born in 2003. Panel A reports the effects for children ages 0-4 in 2003 and Panel B reports the effects for children ages 5-12 in 2003. Columns 1-3 reports the results grades 1-8 for all children and by gender. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4^{th} child prior to June 1, 2003. Standard errors are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level

Table 5: Income effects of reducing cash benefits on educational attainment of Jewish boys

	Bagrut Diploma	Unconditional English 4 units	Unconditional Math 4 units	Unconditional Bagrut units
Panel A: Ages 1-4				
Treatment	-0.016	-0.030*	-0.020	-0.548
	(0.020)	(0.018)	(0.015)	(0.561)
Observations	4985	4985	4985	4985
Control mean	0.189	0.168	0.084	6.150
Panel B: Ages 5-12				
Treatment	-0.038**	-0.048***	-0.024*	-1.930***
	(0.016)	(0.015)	(0.013)	(0.464)
Observations	9566	9566	9566	9566
Control mean	0.209	0.188	0.094	7.743
Panel C: Ages 13-17				
Treatment	-0.023	-0.010	-0.010	-0.663
	(0.028)	(0.027)	(0.024)	(0.767)
Observations	2225	2225	2225	2225
Control mean	0.172	0.169	0.098	6.887

Notes - This table reports the effects of the reform on the educational attainment of Jewish boys. The sample includes families whose 4^{th} child was born in 2003. Panel A reports the effects for children ages 0-4 in 2003, Panel B reports the effects for children ages 5-12 in 2003, and Panel C reports the effects for children ages 13-17 in 2003. Columns 1 reports results on matriculating high school by obtaining a Bagrut diploma. Columns 2-4 report effects on the quality of the diploma by examining the unconditional probability of completing a 4 or more units of English education, of completing a 4 or more units of mathematics, and the overall number of units completed, respectively. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4^{th} child prior to June 1, 2003. Standard errors are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level

Table 6: Income effects of reducing cash benefits on educational attainment of Jewish girls

	Bagrut Diploma	Unconditional English 4 units	Unconditional Math 4 units	Unconditional bagrut units
Panel A: Ages 1-4				
Treatment	-0.011	-0.041*	-0.025	-0.412
	(0.027)	(0.023)	(0.020)	(0.671)
Observations	4700	4700	4700	4700
Control mean	0.412	0.279	0.147	12.256
Panel B: Ages 5-12				
Treatment	-0.019	-0.023	-0.015	-0.461
	(0.018)	(0.017)	(0.014)	(0.470)
Observations	9109	9109	9109	9109
Control mean	0.351	0.283	0.141	11.993
Panel C: Ages 13-17				
Treatment	-0.025	-0.019	0.011	-0.921
	(0.035)	(0.034)	(0.028)	(0.853)
Observations	2255	2255	2255	2255
Control mean	0.331	0.278	0.159	11.323

Notes - This table reports the effects of the reform on the educational attainment of Jewish girls. The sample includes families whose 4^{th} child was born in 2003. Panel A reports the effects for children ages 0-4 in 2003, Panel B reports the effects for children ages 5-12 in 2003, and Panel C reports the effects for children ages 13-17 in 2003. Columns 1 reports results on matriculating high school by obtaining a Bagrut diploma. Columns 2-4 report effects on the quality of the diploma by examining the unconditional probability of completing a 4 or more units of English education, of completing a 4 or more units of mathematics, and the overall number of units completed, respectively. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4^{th} child prior to June 1, 2003. Standard errors are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level

Table 7: Income effect of reducing cash benefits on fertility and employment

	Subsequent Children 2005	Subsequent Children 2015	Number of Months Employed 2003-2005		Number of Months Employed 2013-2015	
			Fathers	Mothers	Fathers	Mothers
Panel A: Jewish population						
Treatment	0.018	0.058	0.213	-0.674	-0.018	-0.681
	(0.026)	(0.062)	(0.580)	(0.535)	(0.653)	(0.622)
Observations	7310	7310	7310	7310	7310	7310
Control mean	0.629	1.772	18.976	16.973	22.635	23.662
Panel B: Arab population						
Treatment	-0.054*	-0.117**	0.912	0.117	1.812**	-0.213
	(0.032)	(0.058)	(0.768)	(0.483)	(0.833)	(0.767)
Observations	4296	4296	4296	4296	4296	4296
Control mean	0.448	1.145	20.080	4.209	21.481	8.530

Notes - This table reports the effects of the reform on number of employed months by the father. The sample includes families whose 4^{th} child was born in 2003. Panel A reports the effects within 3 years of the reform, Panel B reports the effects within years 7-9, and Panel C reports the effects for years 10-12. Column 1 reports the results for the whole population. Columns 1-2 report the results for the Arab and Jewish populations, respectively. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4^{th} child prior to June 1, 2003. Standard errors are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 8: Income effect of reducing cash benefits on completed fertility for smaller families

	Arab Population	Jewish Population
Panel A: Families with a first or second birth in 2003		
Treatment	-0.028	-0.003
	(0.043)	(0.025)
Observations	6,818	20,045
Control mean	1.996	1.622
Panel B: Families with a third birth in 2003		
Treatment	-0.055	-0.063
	(0.069)	(0.041)
Observations	2,383	6,634
Control mean	1.362	1.020

Notes - This table reports the effects of the reform on the number of subsequent children within 9 years of the reform. Panel A reports the effects for families whose first or second birth occurred in 2003. Panel B reports the effects for families who had a third child in 2003. Columns 1-2 reports the results for the Arab and Jewish populations, respectively. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth prior to June 1, 2003. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.1: Descriptive Statistics, All Families with Children Born in 2003

	Full Sample	Arabs	Jews
Father with more than HS education	0.256	0.133	0.296
	(0.437)	(0.339)	(0.456)
Mother with more than HS education	0.308	0.121	0.371
	(0.462)	(0.327)	(0.483)
Jews	0.679		
	(0.467)		
Arabs	0.295		
	(0.456)		
Jewish Orthodox before 2003	0.157		0.231
	(0.364)		(0.421)
Father's age in 2003	32.66	32.34	32.78
	(6.325)	(6.619)	(6.184)
Mother's age in 2003	29.19	27.54	29.89
	(5.505)	(5.641)	(5.307)
Father's months of employment in 2003	7.543	7.073	7.725
	(5.423)	(5.387)	(5.437)
Mother's months of employment in 2003	5.241	2.035	6.648
	(5.048)	(4.027)	(4.819)
Father's labor earnings in 2003	53,124.5	31,789.3	62,615.1
	(76,020.12)	(41,298.92)	(85,914.46)
Mother's labor earnings in 2003	21,843.9	5,521.2	29,151.9
	(36,641.65)	(13,831.01)	(41,261.61)
Household labor earnings in 2003	74,968.4	37,310.5	91,766.9
	(93,165.15)	(46,369.3)	(103,895.79)
Mother's age at first birth	25.15	22.99	26.03
	(4.872)	(4.09)	(4.855)
Number of siblings under age 5 in 2003	0.891	0.923	0.884
	(0.312)	(0.266)	(0.32)
Number of siblings age 5-9 in 2003	0.378	0.417	0.364
	(0.485)	(0.493)	(0.481)
Number of siblings age 10-13 in 2003	0.166	0.177	0.161
	(0.372)	(0.382)	(0.367)
Number of siblings age 14-17 in 2003	0.075	0.081	0.073
	(0.264)	(0.272)	(0.26)
Number of children	2.767	3.089	2.66
	(1.839)	(1.919)	(1.806)
Twin Birth	0.023	0.017	0.025
	(0.149)	(0.13)	(0.157)
Male birth	0.512	0.51	0.513
	(0.5)	(0.5)	(0.5)
Observations	139,623	41,236	94,840

Notes - This table reports sample means. Column 1 reports sample means for all families in 2003. Columns 2 and 3 report sample means for the Arab and Jewish populations, respectively.

Table A.2: The income effect of reducing child allowance on fathers' Yeshiva Attendance

	Any sibling 1-3 years	Any sibling 4-6 years	Any sibling 7-9 years
Panel A: Uniform Weights			
Treatment	-0.002	0.009	-0.007
	(0.014)	(0.015)	(0.016)
Observations	7,310	7,310	7,310
Control mean	0.381	0.346	0.329
Bandwidth in days	70	70	70
Panel B: Kernel Weights			
Treatment	0.003	0.004	-0.014
	(0.015)	(0.017)	(0.017)
Observations	7,310	7,310	7,310
Control mean	0.381	0.346	0.329
Bandwidth in days	70	70	70
Panel C: Automatic Bandwidth			
Treatment	0.009	0.005	-0.016
	(0.017)	(0.016)	(0.017)
Observations	5,116	6,439	6,654
Control mean	0.381	0.347	0.332
Bandwidth in days	49	62	63

Notes - This table reports the effects of the reform on the probability that the father attends a Yeshiva. The sample includes families whose 4^{th} child was born in 2003. Panel A reports effects from regressions using uniform weights. Panel B reports effects from regressions using triangular kernel weights. Panel C reports effects using the Calonico et al. (2019) bandwidth and uniform weighting. Columns 1 reports effects 1-3 years after the reform. Columns 2 and 3 report effects 4-6 and 7-9 years after the reform, respectively. All regressions include the controls listed in equation 1. The control means are calculated using families who gave birth to their 4^{th} child prior to June 1, 2003. Standard errors are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.3: The income effect of reducing child allowance on educational attainment of Arab children

	Boys Bagrut Diploma	Boys HS Dropout	Girls Bagrut Diploma	Girls HS Dropout
Panel A: Ages 2-6				
Treatment	-0.000	-0.046	-0.036	-0.030
	(0.030)	(0.035)	(0.032)	(0.025)
Observations	3,656	3,492	4,148	3,924
Control mean	0.433	0.329	0.469	0.136
Panel B: Ages 7-12				
Treatment	-0.011	0.013	0.021	-0.015
	(0.029)	(0.036)	(0.031)	(0.028)
Observations	3,515	3,335	3,678	3,358
Control mean	0.217	0.342	0.401	0.173

Notes - This table reports the effects of the reform on educational attainment. The sample includes families whose 4^{th} child was born in 2003. Panel A reports effects for children ages 2-6 in 2003 and Panel B reports effects for children ages 7-12 in 2003. Columns 1 and 3 report effects on the likelihood of earning a Bagrut diploma, for boys and girls, respectively. Columns 2 and 4 report effects on the likelihoof of dropping out from high school, for boys and girls, respectively. All regressions include the controls listed in equation 1. The control means are calculated using families who gave birth to their 4^{th} child prior to June 1, 2003. Standard errors are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.4: The income effect of reducing child allowances on the number of subsequent children using triangular weights

	All Population	Arab Population	Jewish Population
Panel A: Within 3 years			
Treatment	0.011	-0.058*	0.038
	(0.022)	(0.035)	(0.028)
Observations	11,662	4,296	7,310
Control mean	0.555	0.448	0.629
Panel B: Within 9 years			
Treatment	0.022	-0.167***	0.102*
	(0.043)	(0.057)	(0.057)
Observations	11,662	4,296	7,310
Control mean	1.333	1.028	1.545

Notes - This table reports the effects of the reform on the number of subsequent children. The sample includes families whose 4^{th} child was born in 2003. Panel A reports the effects within 3 years of the reform and Panel B reports the effects within 9 years of the reform. Columns 1 reports the results for the whole population, and columns 2-3 report the effects for the Arab and Jewish populations, respectively. All regressions include the controls listed in equation 1 are are weighted using a triangular kernel. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4^{th} child prior to June 1, 2003. Standard errors are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.5: The income effect of reducing child allowances on the number of subsequent children using automatic bandwidth selectors

	All Population	Arab Population	Jewish Population
Panel A: Within 3 years			
Treatment	0.012	-0.079**	0.038
	(0.022)	(0.038)	(0.032)
Observations	10,050	3,008	4,626
Control mean	0.555	0.438	0.609
Bandwidth in days	61	50	44
Panel B: Within 9 years			
Treatment	0.015	-0.214***	0.110*
	(0.044)	(0.063)	(0.065)
Observations	9,116	2,933	4,736
Control mean	1.337	1.021	1.522
Bandwidth in days	56	49	45

Notes - This table reports the effects of the reform on the number of subsequent children. The sample includes families whose 4^{th} child was born in 2003. Panel A reports the effects within 3 years of the reform and Panel B reports the effects within 9 years of the reform. Columns 1 reports the results for the whole population, and columns 2-3 report the effects for the Arab and Jewish populations, respectively. All regressions include the controls listed in equation 1. The bandwidth is selected using Calonico et al. (2019). The control means are calculated using families who gave birth to their 4^{th} child prior to June 1, 2003. Standard errors are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.6: The income effect of reducing child allowances on the number of subsequent children using June 1, 2002 cutoff

	Arab Population	Jewish Population
Panel A: Within 3 years		
Treatment	0.020	-0.007
	(0.031)	(0.027)
Observations	4369	7121
Control mean	0.468	0.649
Panel B: Within 9 years		
Treatment	0.084	0.021
	(0.052)	(0.054)
Observations	4369	7121
Control mean	1.052	1.540

Notes - This table reports the effects of the reform on the number of subsequent children using June 1, 2002 cutoff. The sample includes families whose 4^{th} child was born in 2002. Panel A reports the effects within 3 years of the reform and Panel B reports the effects within 9 years of the reform. Columns 1 reports the results for the whole population, and columns 2-3 report the effects for the Arab and Jewish populations, respectively. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4^{th} child prior to June 1, 2003. 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 income effect of reducing child allowance on household income

	All Population	Arab Population	Jewish Population
Panel A: Within 1-3 years			
Treatment	-6345.689	671.116	-9683.063
	(7856.778)	(6440.355)	(11598.405)
Observations	11,662	4,296	7,310
Control mean	185115.755	117341.551	231946.807
Panel B: Within 7-9 years			
Treatment	-12501.700	-3441.882	-16930.952
	(14345.836)	(12512.469)	(21145.482)
Observations	11,662	4,296	7,310
Control mean	323851.403	202676.901	407659.703
Panel C: Within 10-12 years			
Treatment	-17972.290	1876.657	-28209.945
	(16049.779)	(15300.103)	(23353.965)
Observations	11,662	4,296	7,310
Control mean	394989.036	242895.935	499988.350

Notes - This table reports the effects of the reform on household labor earnings. The sample includes families whose 4^{th} child was born in 2003. Panel A reports the effects within 3 years of the reform, Panel B reports the effects within years 7-9, and Panel C reports the effects for years 10-12. Column 1 reports the results for the whole population. Columns 1-2 report the results for the Arab and Jewish, respectively. All regressions include the controls listed in equation 1. The bandwidth is 70 days in all regressions. The control means are calculated using families who gave birth to their 4^{th} child prior to June 1, 2003. Standard errors are reported in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.