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## Intergenerational Effects of Türkiye's Education Reform on Children's Academic and Health Outcomes

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# Intergenerational Effects of Türkiye's Education Reform on Children's Academic and Health Outcomes\*

## Abstract

This paper examines the intergenerational effects of Türkiye's 1997 compulsory schooling reform, which increased mandatory schooling from five to eight years. Drawing on a novel dataset from the nationally representative 2022 Turkish Child Survey, we use a regression discontinuity design (RDD) to identify the causal effect of mothers' exposure to the reform. We find that the reform significantly increased maternal educational attainment and, in turn, improved children's performance in Turkish, while having no statistically significant effect on mathematics achievement. Maternal exposure to the reform also had positive effects on children's mental health, increasing happiness and reducing anxiety, but did not affect their general health status. These effects are heterogeneous by child gender, with boys benefiting more than girls. Analysis of potential channels shows that increased parental investment may be one relevant channel explaining the observed effects, while reduced child work appears to play a limited role.

## JEL classification

I21, I24, J13, J24

## Keywords

mother's education, compulsory schooling, child outcomes, mental health, physical health

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## 1. Introduction

Parents with higher education levels tend to have children who also reach higher levels of education and better health outcomes. One possible explanation is selection: parents who attain more education and earn higher incomes may naturally have children who are inclined to do the same, due to shared characteristics or values. Another explanation is causation: obtaining more education can change parents' perspectives and behaviors, leading them to create environments that improve child's outcomes.

A growing body of research has estimated the causal effect of parental education on children's educational attainment (see reviews by Björklund and Salvanes, 2011; Black and Devereux, 2011; and Holmlund, Lindahl, and Plug, 2011). To identify these causal effects, some studies have used a twin design (Behrman, Rosenzweig, and Taubman, 1994; Behrman and Rosenzweig, 2002; Pronzato, 2012; Amin, Lundborg, and Rooth, 2015) and an adoptive design (Sacerdote, 2002; Plug, 2004; Björklund et al., 2006; Chen & Li, 2009). Another branch of the literature implements an instrumental variables design, where extensions in compulsory schooling laws (Black, Devereux, Salvanes, 2005; Oreopoulos, Page, and Stevens, 2006; Holmlund et al., 2011; Andrabi, Das, and Khwaja, 2012; Lundborg, Nilsson, and Rooth 2014; Cui, Liu, and Zhao, 2019), school construction (Mazumder, Rosales-Rueda, Triyana, 2023) and elimination of apartheid-style policies (Agüero and Ramachandran, 2020) serve as instruments for schooling. In many cases, this literature finds that maternal education matters most for children's schooling<sup>1</sup>. By contrast, evidence on maternal education and child health is mixed: while several studies associate maternal schooling with better health outcomes (Desai and Alva, 1998; Breierova and Duflo, 2004; Chou, Liu, Grossman, and Joyce, 2010; Grépin & Bharadwaj, 2015; Güneş, 2015; Makate and Makate, 2016; Cui et al., 2019; Dursun et al. 2022; Wu, 2022; Mazumder et al., 2023), other works finds little or inconclusive effects on health, health-related behaviors, and mortality (Chen & Li, 2009; Chou et al., 2010; Carneiro, Meghir and Parey, 2013; Dinçer, Kaushal, and Grossman et al., 2014; Alderman and Headey, 2017; Keats, 2018; Baltagi et al., 2019; Arendt et al., 2021), underscoring important context and research-design dependencies.

Our paper contributes to these two strands of the literature by examining how maternal exposure to Turkey's compulsory schooling law, affects children's educational and health outcomes during pre-adolescence (ages 6-12), and by investigating the mechanisms through which these effects operate.<sup>2</sup>

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<sup>1</sup> Most twin and adoptive studies emphasize the importance of fathers' schooling (e.g., Behrman and Rosenzweig, 2002 for twin studies; Plug, 2004 for adoptive studies), whereas IV studies often highlight the significance of mothers' education. However, Amin et al. (2015) provide the first twin-based evidence of possible role model effects suggesting that mothers' schooling matters more than fathers' schooling for daughters' educational attainment.

<sup>2</sup> Our study focuses on the impact of the mother's education reform on children's academic success rather than on the father's education. According to the Turkish Health and Demographic Survey 2018 Main Report, the average age difference between spouses is 4.2 years; 43% of currently married women in Turkey have spouses who are at least five years older than themselves and 10% have spouses who are 10 years or older. This implies that men typically become fathers later than women become mothers, leading to a significant sample size difference between

Implemented in 1997, the policy extended mandatory schooling from 5 to 8 years. Students who had completed fifth grade by summer 1997 were exempt from the new requirement, while younger cohorts were mandated to complete 8 years. This discontinuity provides a unique opportunity to examine how the reform-induced increase in mothers' education affected their children's educational and health outcomes.

Our paper makes several important contributions. First, the literature on the effects of maternal education on school age children comes predominantly from highly developed countries, with few exceptions (Andrabi, Das & Khwaja, 2012; Mazumder et al. 2023, Cui et al., 2019; Agüero and Ramachandran, 2020). Our analysis provides important evidence from a middle-income country context where educational systems, labor markets, and household dynamics differ substantially from those in advanced economies. Second, existing work on low- or middle-income countries examining the effects of maternal education focuses primarily on early childhood outcomes<sup>3</sup>. We extend this evidence by analyzing school-age children during pre-adolescence (ages 6-12), a critical developmental period that has received less attention in the literature. Third, unlike the limited previous work from less developed countries on school-age children—which uses geographic differences in school supply (Andrabi et al., 2012; Hasan, Nakajima, and Rangel, 2020; Mazumder et al., 2023) or temporal and exogenous variations in enforcement as the exogenous driver (Cui et al., 2019)—our identification strategy leverages mothers' birth cohorts, which are more plausibly unrelated to potential child-development outcomes. Finally, this paper contributes new evidence on previously underexplored dimensions of child development. Most notably, only a few studies have examined the impact of maternal education on children's mental health beyond early childhood in developing-country contexts (Cui et al., 2019)<sup>4</sup>. We provide some of the first evidence on the estimated effect of an increase in maternal education on pre-adolescent mental health. We further advance the literature by examining novel mechanisms—specifically, children's time allocation and parental engagement in their activities.

Using a novel data set from the nationally representative, 2022 Turkish Child Survey, we estimate the impact of mothers' exposure to the education policy on children's academic performance and health

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those exposed to the education reform and those who were not. In addition, as in many developing countries, mothers are the primary caregivers of children in Turkey.

<sup>3</sup> Similar to the broader literature on the causal impact of maternal education on child health, studies providing evidence for Türkiye also produce mixed and inconclusive results. Specifically, while Güneş et al. (2015) and Dursun et al. (2022) show a positive impact of maternal education on infant health and on the health of children under the age of 5, Baltagi et al. (2019) find no causal evidence for infant health, and Dincer et al. (2014) provide weak evidence that maternal schooling leads to a decline in child mortality. On educational outcomes, Akgündüz et al. (2024) find that reform-induced increases in maternal education improved school readiness among children aged 36-59 months old, whereas Akar et al. (2025) find no evidence that the reform increased mothers' time spent on cognitively stimulating activities with young children aged 0-5, although they do find that maternal education increased mothers' total time spent with children, particularly through housework and social activities involving children.

<sup>4</sup> As evidence from a developed country, Lindeboom et al. (2009) find little evidence that a mother's education has a causal effect on children's mental health at ages 7, 11, and 16 in the United Kingdom.

outcomes using a Regression Discontinuity Design (RDD).<sup>5</sup> The survey provides rich, detailed measures of children's outcomes including academic performance in Turkish language and mathematics, as well as indicators of physical and mental health, parental material and time investments, and children's daily activities and social interactions. This unique dataset allows us to examine not only the direct effects of maternal education on children's academic performance and health but also the mechanisms through which these effects operate. Türkiye provides a particularly compelling context for this analysis, as educational attainment prior to the reform was markedly lower than in the developed countries where most intergenerational studies have been conducted, highlighting the substantial scope for policy-driven improvements in female education<sup>6</sup>.

Our findings indicate that the compulsory schooling policy significantly increased the likelihood of mothers completing middle school with suggestive evidence of spillover effects at the high school level. Our main results demonstrate that an increase in maternal education significantly enhances children's performance in Turkish language, but has no significant impact on mathematics. We further observe distinct gender differences in these effects with sons experiencing more substantial academic benefits than daughters.<sup>7</sup> Additionally, exposure to the reform lowers the likelihood that children frequently feel sad/unhappy and anxious/nervous, but does not affect their general health status. To our knowledge, this is the first study to examine the effect of maternal education on mental health outcomes in pre-adolescent school-aged children in a developing country context<sup>8</sup>.

The literature on the relationship between maternal education and parental inputs identifies several important pathways. Higher maternal education often leads to increased income, enabling families to invest more resources in their children's educational and physical health outcomes (Lundborg et al., 2014; Carneiro et al., 2013; Mazumder et al., 2023). They also tend to place a higher value on education, fostering a supportive atmosphere that prioritizes academic achievement (Piopiunik, 2014). Additionally, more educated women are likelier to partner with similarly educated individuals, which can enhance the availability of both financial and nonfinancial resources for their children (Carneiro et al., 2021; Macmillan and Tominey, 2023) although evidence for assortative mating as a mechanism is mixed (Mazumder et al., 2023). Educated mothers also exhibit healthier behaviors during pregnancy

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<sup>5</sup> In our main analysis, we do not use the education reform as an instrument for mothers' education because it may fail to satisfy the exclusion restrictions assumptions. In particular, better-educated women maybe likely to marry equally educated men with higher income, and the reform may affect outcomes through additional channels, making it an invalid instrument (see also Akyol & Kirdar, 2022; Akyol & Mocan, 2023; Kirdar et al., 2018; Akar, Akyol and Okten, 2025).

<sup>6</sup> For instance, in our sample, only 54 percent of mothers unexposed to the reform had completed at least middle school.

<sup>7</sup> Evidence from several developed countries also reports similar findings. For instance, Black et al. (2005) show that there is a significant causal relationship between a mother's education and her son's education in Norway, but no significant relationship between a mother's education and her daughter's education. Similarly, Lundborg et al. (2014) find that in Sweden, higher maternal education enhances sons' cognitive and noncognitive skills as well as their health status.

<sup>8</sup> Our findings are consistent with Cui et al. (2019), who find improved mental health outcomes for adolescents (aged 10-19) in China.

and possess greater knowledge about nutrition and health practices (Glewwe, 1999; Currie and Moretti, 2003; Liu and Eriksson, 2022), potentially leading to better child health. Moreover, higher-educated parents are more likely to make greater time investments (Guryan, Hurst, & Kearney, 2009; Akar, Akyol, and Okten, 2025); these investments, in turn, improve children's academic outcomes (Andrabi et al., 2012). Maternal education may further influence achievement by improving parenting practices (Burton, Phipps & Curtis, 2002; Dooley and Stewart, 2007; Hasan et al., 2020).

Our paper contributes to the existing literature on intergenerational educational effects by identifying the following specific mechanisms driving these relationships. First, through the material investment channel, our findings suggest that educated mothers enhance their children's outcomes by reallocating household resources. Unlike other studies (Lundborg et al., 2014; Carneiro et al., 2013; Mazumder et. al., 2023) that find maternal education increases overall household resources, we do not observe such effects. Instead, educated mothers provide more physical resources such as books and computers that support learning, demonstrating a reallocation toward children's educational needs rather than an increase in total resources. This creates an enriched home environment conducive to educational success. Additionally, educated mothers appear to invest their own time in their children's development by engaging them in meaningful conversations about national issues. These interactions may not only strengthen cognitive development but also demonstrate the value of learning and critical thinking. Second, educated mothers also appear to influence their children's time allocation and engagement. We provide suggestive evidence that they reduce their children's involvement in unpaid family work, thereby allowing children to devote more time and attention to their studies and broader educational development. To our knowledge, this is the first study to document how maternal education reshapes children's time use, whereas prior work has primarily examined policies targeting children directly (Dayioglu and Kirdar, 2022; Ozmen, 2022). Furthermore, educated mothers tend to encourage and facilitate their children's social interactions with peers, possibly recognizing that healthy friendships contribute to both emotional development and academic motivation. Together, these findings suggest that the combination of material resources, quality time, social opportunities, and protection from premature work responsibilities may create conditions that are conducive to both scholastic achievement and positive mental health outcomes across the child's development.

We find no statistically significant effect of mothers' exposure to the reform on husbands' middle-school completion. However, because the reform also directly affected men, paternal education remains a relevant channel when husbands' own exposure to the reform is considered, making it difficult to fully separate maternal education effects from assortative mating and broader household human-capital effects. In addition, we find weak suggestive evidence that mothers' exposure increased women's relative educational standing within marriage, which is consistent with a potential improvement in women's intra-household bargaining position and may, in turn, facilitate greater paternal involvement in children's academic performance and mental health. Thus, while the results do not point strongly to

spousal human capital or assortative mating as the primary mechanism, they remain consistent with broader household-level channels.<sup>9</sup>

Our findings remain robust across a range of sensitivity analyses, including placebo tests, alternative model specifications, alternative outcome variables, and alternative sample restrictions. Nonparametric reduced-form estimates support our parametric RDD results. To address potential measurement error in mothers self-reported assessments, we conduct supplementary analysis using academic self-evaluations from children's siblings (aged 13–17), confirming that these self-evaluations align consistently with maternal assessments. We additionally address concerns related to multiple hypothesis testing by applying the Benjamini-Hochberg (BH) procedure and by constructing standardized indices for related outcome variables.

The rest of the paper is organized as follows: Section 2 provides the necessary background information on the Turkish Compulsory Schooling Reform and details the empirical strategy used in our analysis. Section 3 describes the data, measures of variables, descriptive statistics, and the interpretation of the RDD graphs. In Section 4, we present the main results. Section 5 investigates the mediating possible channels. Section 6 show the robustness of results, and Section 7 concludes.

## **2. Method**

### **2.1. 1997 Compulsory Schooling Reform in Turkey**

Since 1923, Turkey's formal education system has been centrally managed by the Ministry of National Education (MONE), which oversees all educational reforms and policies. Prior to 1997, the Turkish education system consisted of three distinct tiers: five years of compulsory primary education, followed by three years of voluntary lower secondary education (middle school), and three years of voluntary upper secondary education. Upon completing primary school, students could choose to discontinue their education or continue at general, vocational, or religious middle schools.

The compulsory education law came into force in August 1997, just before the 1997-1998 school year. Students who have not completed their 5-years of education on this date and have not been awarded the primary school diploma are required to complete 8-years of schooling to earn a primary education diploma. Thus, students who started primary school in September 1993 or afterward were bound by the policy, whereas students who started in September 1992 or earlier were exempt from it. The official school starting age in Turkey is six but in practice, many individuals start primary education at age seven. Hence individuals born in January 1987 or later are exposed and those born before January 1987 are not exposed to the compulsory schooling reform. However, some students may start either earlier or

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<sup>9</sup> Utilizing the same education reform in Turkey, Akgündüz, Akyol, Aydemir, Demirci, and Kırdar (2024) investigate the impact intergenerational effects of compulsory schooling reform on early childhood development, and report that for mothers with at least one child aged 2–5, exposure to the reform increases women's bargaining power in marriage. In contrast, Andrabi et al. (2012) do not find evidence for supporting channels through which education affects women's household bargaining power in Pakistan.

later than their designated year, implying imperfect compliance in the treatment status of the 1986 and 1987 cohorts<sup>10</sup>. The 8-years primary school diploma, being the first official educational credential available in Turkey, created a powerful incentive for compliance among students and their families. Parents who did not comply with the new compulsory schooling law faced monetary penalties and even possible incarceration in case of repeated noncompliance (Kırdar et al. 2016).

The reform was implemented nationwide and applied uniformly. To support compliance, the Ministry of National Education expanded school infrastructure, increased teacher hiring, and introduced transportation and boarding facilities, particularly in underserved rural areas. Public education expenditures rose substantially during 1998–2000 (Dulger 2004; Kırdar et al. 2016). Importantly, these complementary investments accompanied the national rollout of the reform and were not assigned differentially across adjacent birth cohorts. Accordingly, identification relies on the sharp cohort-based discontinuity in compulsory schooling exposure.

The impact of the reform was both immediate and substantial. The enrollment rate for eight-year primary education increased dramatically from 53% in the 1996-1997 academic year to 85% in 1997-1998, the initial year of implementation. By the 2000-2001 academic year, enrollment had further increased to 95%. The reform also generated positive spillover effects in secondary education, where enrollment rates rose from 38% in 1997-1998 to 44% in 2000-2001 (MONE Education Statistics, 2006-2007).

We further present the change in school attainment with the nationally representative micro level data from Turkish Child Survey, 2022. [Figure 1.a.](#) and [1.b.](#) displays mean middle school (at least eight years of schooling) completion rates for all females and males in the survey sample by birth year, respectively. Separate linear polynomials are fitted on both sides of the cut off. We observe a clear discontinuity at the cutoff, suggesting a significant increase in middle school completion rates for those born after January 1987.

## 2.2. Empirical Strategy

In this paper, we employ a sharp regression discontinuity design with the month-year of birth as the running variable to examine the impact of a mother’s exposure to the education reform on child outcomes. Our specification is as follows:

$$y_i = \beta_0 + \beta_1 Policy_i + I(Policy_i = 0)f(x_i) + I(Policy_i = 1)g(x_i) + X_i\Gamma + \epsilon_i \quad (1)$$

where  $y_i$  denotes the outcome variable for individual  $i$ , i.e. outcome variables for mother and child. The treatment variable,  $Policy_i$ , takes the value of 1 if the month-year of birth is after January 1987 and 0

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<sup>10</sup> We will discuss this issue in detail in the Robustness Check section.

otherwise. In this sharp regression discontinuity design, the coefficient of interest identifies the local reduced-form effect of exposure to the post-reform schooling regime for cohorts near the cutoff.

The indicator function,  $I(\cdot)$ , takes the value of 1 if the condition in parentheses is true and 0 otherwise. The functions  $f(\cdot)$  and  $g(\cdot)$  capture differential time trends in the outcome variable on the left- and right-hand-side of the cutoff, respectively. The running variable  $x_i$  is the month-year of birth, which is normalized at the cutoff value, 1987 January.

In Eq. (1), where the dependent variable is defined at the child level,  $X_i$  is a vector of control variables including mother's birth-month dummies, the total number of children, child age, the ages of the first- and last-born children, birth-order dummies, child sex dummies, and interactions between birth order and child sex<sup>11</sup>. Mother's birth-month dummies control for seasonality in birth timing and cohort-specific early-life conditions<sup>12</sup>. Family size is captured by the number of children, reflecting potential resource dilution, while child age accounts for mechanical differences arising from age-related development. The ages of the first- and last-born children proxy for fertility timing, parental experience, and family life-cycle effects. Birth-order dummies capture systematic differences across siblings<sup>13</sup>, such as differential parental investments and learning-by-doing effects, while child sex dummies account for gender differences in outcomes and parental responses. Interactions between birth order and child sex allow birth-order effects to vary by gender, reflecting heterogeneity in parental behavior and social norms. It is important to note that we examine whether the education reform has any impact on these variables before incorporating them as control variables in our regression analysis<sup>14</sup>.

$\epsilon_i$  represents the error term.  $\beta_1$  reports the reduced form effects of the education reform on the outcome variables of interest. The regressions are weighted using sample weights, and standard errors are clustered at the month-year of birth level, following Lee and Card (2008). Because our analysis considers several related child outcomes and potential channels, we report Benjamini–Hochberg (1995) adjusted p-values for families of related outcomes in robustness checks section to account for multiple hypothesis testing.

In the estimation, we employ both local polynomial parametric and nonparametric approaches. For the parametric approach, we use several alternative bandwidths with differential linear time trends on each side of the cutoff, presenting estimates for five bandwidths ranging from 7 years to 3 years on each side of the cutoff. Additionally, rather than using differential time trends around the cutoff, we estimate

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<sup>11</sup> In the regression where the dependent variable is maternal outcomes,  $X_i$  is simply a vector of control variables, including mother's birth-month dummies.

<sup>12</sup> We should also note that Turkish Child Survey 2022 does not include variables on mother characteristics such as residence type (e.g., rural/urban), current place of residence, or the region where the mother grew up.

<sup>13</sup> We additionally re-estimate the model by grouping birth order into broader categories to address concerns about sparse support at higher birth orders. The number of firstborn children is 1,665; second-born, 1,612; third-born, 476; fourth-born, 84; fifth-born, 19; and sixth-born, 4. We classify children as firstborn (reference category), second-born, third-born, and fourth-born or higher. The results are qualitatively unchanged.

<sup>14</sup> See [Table A.1.](#) in Appendix for the detailed results.

the model by including both linear and quadratic terms for the difference between individuals' month and year of birth and the cutoff value, January 1987.

We also conduct nonparametric RDD estimation using the Calonico, Cattaneo, Farrell, and Titiunik (2017, CCFT)<sup>15</sup> optimal bandwidth selection method. As Lee and Lemieux (2010) note, nonparametric estimation does not provide a solution to all functional-form concerns in RD designs. We therefore view the local polynomial estimates as complementary to, rather than a substitute for, our parametric specifications. In our analysis, these estimates provide supplementary evidence.

We do not use the education reform as an instrument for mothers' education. Our objective is to estimate the reduced-form effect of exposure to the reform on child outcomes, not the causal effect of maternal education itself. In our sharp RDD framework, the estimates should therefore be interpreted as local effects of reform exposure for mothers near the eligibility cutoff. An IV interpretation would require the exclusion restriction that reform exposure affects child outcomes only through maternal education. This restriction is unlikely to hold in this setting, since the reform may affect outcomes through multiple channels, including marriage-market sorting, household income, paternal education, fertility timing, and other family-level decisions (see also Akyol and Kirdar, 2022; Akyol and Mocan, 2023; Kirdar et al., 2018; Akar, Akyol, and Okten, 2025, for similar arguments).

### 2.3. Identification

The RDD design relies on the standard continuity (smoothness) assumption: in the absence of the reform, potential outcomes would evolve smoothly with the running variable at the cutoff, so any discontinuity at the threshold can be attributed to treatment. This identifying assumption is fundamentally untestable; therefore, we provide indirect evidence on its plausibility using three standard diagnostic tests: (i) examining whether the density of the running variable is continuous at the cutoff, (ii) testing for balance in predetermined (pre-treatment) covariates around the threshold, and (iii) implementing placebo cutoffs.

First, we examine the continuity of the running-variable density around the cutoff. A discontinuity in density could indicate manipulation or sorting around the threshold. In our context, such manipulation is unlikely because the running variable—month-year of birth—is determined before the policy was enacted. Nonetheless, we formally test for a density discontinuity using the procedure proposed by Cattaneo et al. (2018). [Figure A.1](#) in the Appendix shows no evidence of manipulation ( $T = -0.5543$ ,  $p = 0.5794$ ).

Second, we assess balance in predetermined covariates by testing for discontinuities at the cutoff in characteristics measured prior to treatment. [Table A.1](#) in the Appendix shows no statistically significant

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<sup>15</sup> We use the “`rdrobust`” Stata command (Calonico, Cattaneo, Farrell, and Titiunik 2017) for the estimation with CCFT bandwidths.

jumps in pre-treatment covariates, consistent with local comparability of observations on either side of the threshold.

Finally, we conduct placebo tests to examine whether discontinuities arise at cutoffs where no policy change occurred. We first restrict the sample to the control group and assign a placebo cutoff of January 1983, estimating placebo effects for children whose mothers were not exposed to the education policy. We then define several additional placebo cutoffs and estimate corresponding effects for mothers born within nine years before and after January 1987. In both exercises, we find no evidence of discontinuities at placebo thresholds ([Tables A.15.a](#) and [A.15.b](#) in the Appendix). Additional details are provided in the Robustness Checks section. Although these diagnostics cannot validate the identifying assumption, they provide evidence consistent with its plausibility in our setting and support the credibility of our RDD design.

### **3. DATA**

#### **3.1. Data Description**

We use the nationally representative Turkish Child Survey 2022, which was conducted in collaboration with the Turkish Statistical Institute, the Ministry of Family and Social Services General Directorate of Child Services, and UNICEF Turkey. The purpose of this survey was to generate reliable child welfare indicators that facilitate a child-oriented and holistic assessment of welfare in Türkiye.

The survey was conducted between October 10 and December 16, 2022, covering 9,010 households with at least one child aged 0–17 and collecting information on 14,705 children in this age group. Data were gathered through face-to-face household interviews using four instruments: (i) a household roster (“Main Characteristics of Household Members”), (ii) a household-level questionnaire completed by the respondent, and (iii) an individual questionnaire for the 0–17 age group, iv) the “13–17 Age Group Individual Questionnaire” was administered directly to individuals aged 13–17. Information for each child was reported primarily by mothers (the main caregivers in Türkiye) or, if unavailable, by another primary caregiver (e.g., father, grandmother, aunt, or another household member). The survey collected detailed information on children’s education, living conditions, early childhood development, health and disability/functionality, quality of school life, parental involvement, breastfeeding and nutrition, and social engagement.

We restrict our sample to mothers with at least one school-aged child between 6 and 12 years old — the middle childhood period. In Türkiye, this age range falls under compulsory education, which works in our favor: since school attendance is legally required, our sample is unlikely to suffer from selection into schooling, making it better suited for studying academic outcomes.

In our sample if a mother has more than one child aged 6–12, we select the youngest eligible child and keep one observation per mother. This choice improves compatibility with the assumption of independent observations in cross-sectional analysis and avoids giving disproportionate weight to larger

families. We also choose the youngest child because they are most consistently observed in the household at the time of interview and their outcomes align most closely with contemporaneously measured household and maternal characteristics. As robustness checks, we re-estimate our models using all eligible children and verify that results are not sensitive to alternative child-selection rules. Additionally, to minimize potential cohort effects on the outcome variables, we construct five different samples, including mothers born within three to seven years before and after the January 1987 cutoff.

### 3.2. Measures of Variables

The Turkish Child Survey provides comprehensive information on parents' and children's demographic characteristics, educational attainment, and labor market outcomes. Using these data, we construct several maternal and child outcome variables to examine the impact of maternal exposure to the reform. Definitions of these variables are discussed below and summarized in [Table A.2](#) in the Appendix.

The identification of individuals who were exposed to the education reform is determined by the month and year of birth. To measure exposure to the compulsory schooling reform, we therefore create a binary variable, *Policy*, which is set to 1 for individuals born after January 1987 and 0 otherwise.

Since the survey does not provide information on years of schooling<sup>16</sup>, we construct a binary variable, *Middle School*, coded as 1 if the mother completed at least middle school, corresponding to eight years of schooling, and 0 otherwise.<sup>17</sup> Using these responses, *Middle School* is coded as 1 if the mother completed middle school or reported a highest-attended education level above middle school, including high school, tertiary education, master's, or PhD. The variable is coded as 0 if the highest level attended is below middle school or if the mother attended middle school but did not complete it.

Regarding labor market outcomes, the survey collects detailed information on parents' employment status, household income, and the satisfaction of basic needs, providing a comprehensive view of their socio-economic conditions (see Table A.2. for the definition of variables).

To assess the impact of a mother's exposure to the Turkish compulsory schooling reform on her child's academic performance, we measure academic performance separately for Turkish language and mathematics. For each subject, the variable is coded as 1 if the mother evaluates the child's performance as "very successful" or "successful," and 0 if she reports "fair," "unsuccessful," or "very unsuccessful."

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<sup>16</sup> As a supplementary check, we also constructed an approximate years-of-schooling measure using reported education level and completion status. Respondents who completed a reported education level were assigned the nominal years required to complete that level, while respondents who did not complete the reported level were assigned the number of years corresponding to the immediately preceding completed level. The estimated reduced-form effect of reform exposure on this constructed years-of-schooling measure is positive but statistically insignificant. Because the survey lacks information on exact years of schooling attended, the interpretation of this result is necessarily limited. Detailed results are available upon request.

<sup>17</sup> Specifically, the survey records (i) the highest education level ever attended and (ii) whether that level was completed. The highest-attended level is reported in the following categories: preschool (kindergarten/crèche), primary school, middle school (general/vocational/technical), high school (general/vocational/technical), 2–3-year tertiary (college), 4-year tertiary (college/faculty), 5–6-year faculty, master's (excluding integrated 5–6-year faculties), PhD, and unknown. Completion status is coded as completed vs. not completed.

As mentioned earlier, the survey gathers comprehensive data on children’s education, living conditions, and mental and physical health. To measure the impact of maternal education on children’s overall health, we use information from the health module. Good health is coded as 1 if the mother reports the child’s general health as “excellent” or “good” (0 = “fair,” “bad,” or “very bad”). For mental health, we construct two binary variables: anxious/nervous and sad/unhappy. The variable *anxious/nervous* is coded as 1 if the mother reports that the child appears very anxious or nervous “every day” or “once a week” (0 = “once a month,” “a few times a year,” or “never”). Similarly, *sad/unhappy* is coded as 1 if the mother reports that the child appears very sad or unhappy “every day” or “once a week” (0 = “once a month,” “a few times a year,” or “never”).

The survey also provides information on parental time investment. To measure the impact of the policy on parental time investment, we construct a binary variable, *parental involvement*, based on whether the mother or the other parent in the household engages in activities such as talking about books, movies, or TV shows; going to the library or bookstore with the child; discussing the child’s school situation; or talking about national issues. The variable is coded as 1 if the reported frequency is “every day,” “almost every day,” “once or twice a week,” or “once or twice a month,” and 0 if the response is “rarely or never.”

The survey also collects information on children’s time use, including whether the child engaged in specific work-related activities during the previous week. Based on these responses, we construct four binary variables capturing unpaid family work, agricultural work, sales-related work, and other paid employment. Each variable is coded as 1 if the child engaged in the corresponding activity during the previous week, and 0 otherwise. We also construct a binary measure of social interaction based on how often the child communicates or spends time with others through phone calls, messages, the internet, or in person. This variable is coded as 1 if the child reports any regular social interaction, including daily, several times a week, once a week, several times a month, or once a month, and 0 otherwise.

### 3.3. Sample Statistics

[Table 1](#) presents the descriptive statistics for our main outcome variables by treatment and control groups, which include mothers with at least one child aged 6-12 years, born between 1981 and 1992.<sup>18</sup> Panel A shows the descriptive statistics for maternal outcomes, while Panel B reports for child outcomes. If a mother has more than one child in the 6-12 age group, we consider outcomes only for the youngest child. On average, mothers in the treatment group are younger (32.78 years) compared to those in the control group (38.41 years). Additionally, mothers in the treatment group have a higher proportion of at least middle school completion (75.0%) compared to the control group (53.9%), and a lower employment rate (24.8% versus 35.6%). The descriptive statistics indicate that household income is higher among mothers in the control group compared to those in the treatment group. Additionally,

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<sup>18</sup> See [Table A.3](#) in Appendix for the descriptive statistics of other outcome variables.

mothers exposed to the education policy report having slightly more children overall (2.48 vs. 2.36) and a higher number of children aged 6–12 (1.485 vs. 1.366).

As reported in Panel B, the average age of child is lower for mothers in the treatment group (8.08 years) compared to those in the control group (8.40 years). Regarding children’s academic performance, we document that mothers in the treatment group have children who are more likely to achieve higher performance in subjects such as Turkish language (78.6%), compared to mothers in the control group (72.7%). Moreover, on average, we do not observe any significant performance differences between the treatment and control groups in children’s mathematics. Additionally, among mothers exposed to the reform, 30% report that their child is often sad/unhappy and 33% that the child is anxious/nervous, compared with 32% and 35%, respectively, among control-group mothers. Moreover, 88% of mothers in the treatment group and 90% in the control group report that the child is in good or excellent health.

For descriptive purposes, we also investigate the association between mothers’ education (at least middle school completion) and children’s education and health outcomes. [Table A.4](#) reports the association between mothers’ completion of at least middle school and children’s outcomes across alternative bandwidths. The estimates show a positive and robust relationship with children’s academic achievement: maternal middle school completion is associated with a 11.5–15.2 percentage point increase in success in Turkish and a 13.6–16.3 percentage point increase in success in mathematics. For health outcomes, maternal schooling is positively associated with the probability that the child is reported to be in good health, with effects ranging from 4.1 to 7.2 percentage points. However, the coefficients for sadness/unhappiness and anxiety/nervousness are small and statistically insignificant throughout.

### 3.4. Interpretation of RDD graphs

We provide graphical evidence from the regression discontinuity design to illustrate the relationship between the outcomes of interest and the running variable, defined as mothers’ month-year of birth. The figures plot raw outcome means against the running variable, with separate linear trends fitted on either side of the cutoff.

[Figure A.2](#) presents the corresponding graph for mothers’ middle school completion among mothers with at least one child aged 6–12 years. The figure is consistent with an increase in middle school completion around the cutoff for mothers born after January 1987. [Figures A.3.a–A.3.e](#) present RDD graphs for child outcomes: Turkish language, mathematics, sadness/unhappiness, anxiety/nervousness, and general health status. These figures are intended to provide a descriptive visualization of outcome patterns around the cutoff. The graphical evidence is suggestive of changes at the threshold for some outcomes, particularly Turkish language and selected mental health measures. By contrast, visual evidence of a discontinuity is less pronounced for mathematics and general health status. We therefore

interpret the graphical patterns descriptively and rely on the formal RDD estimates reported in the tables for statistical inference in the next section.

## 4. RESULTS

### 4.1. Potential Sample Selection

In the main analysis, we focus on mothers with at least one child aged 6–12. A potential concern is that the education reform may affect the composition of this group through changes in marriage, fertility timing, or children’s age distribution. We therefore conduct supplementary checks for possible selection into the analysis sample.

Using the sample of all women observed in the Turkish Child Survey, we first estimate whether policy exposure affects the probability that an observed woman in a survey-eligible household has at least one child aged 6–12 and therefore enters the main estimation sample<sup>19</sup>. [Table 2](#) reports results for bandwidths ranging from three to seven years around the January 1987 cutoff. Across specifications, the estimated coefficients are small, statistically insignificant, and sensitive in sign, providing no robust evidence that the reform affected inclusion in the main analysis sample.

We also examine related family-formation characteristics within the relevant sample. As shown in [Table A.1](#), maternal policy exposure is not significantly associated with the probability of being married, age at first birth, number of children, or the age of the first or last child. These results do not suggest meaningful changes in the composition of the estimation sample of mothers with children aged 6–12.

These findings should nevertheless be interpreted cautiously. Because the Turkish Child Survey covers only households with at least one individual aged 0–17, the estimates in [Table 2](#) should be viewed as a within-sample check for selection into the main estimation sample, not as population-wide evidence on marriage, fertility, or parenthood responses to the reform.

This interpretation is broadly consistent with prior studies using the *Turkiye Demographic and Health Survey*, including Gulesci and Meyersson (2013), Kırdar et al. (2018), Usta (2020), and Akgündüz et al. (2024), which find limited or no effects of the same compulsory schooling reform on women’s marriage and fertility outcomes in representative samples of women population.<sup>20</sup>

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<sup>19</sup> The Turkish Child Survey samples households with at least one individual aged 0–17. Thus, the survey is not mechanically restricted to mothers: women without children of their own may also be observed if they live in an eligible household, for example as adult daughters, sisters, or other female relatives. However, women living in households without any individual aged 0–17 are not observed in the survey sample.

<sup>20</sup> In particular, Kırdar et al. (2018) found that while the reform reduced the likelihood of giving birth by age 17, it had no significant effect on the probability of giving birth at any age beyond 17. These findings align with our results in [Table 2](#), as our sample consists of mothers aged 30–40 years.

## 4.2. First Stage: Mothers' Schooling

We first examine the impact of mothers' exposure to the education reform on their own educational attainment. Column (1)-(5) of [Table 3](#) presents the results at bandwidths 3 through 7, respectively. The first row of Table 3 shows that the education reform significantly increased the likelihood of mothers completing middle school across all bandwidths. Specifically, the estimated coefficients range from 0.092 percentage point to 0.113 percentage point, all of which are statistically significant and consistent in magnitude<sup>21</sup>. These findings demonstrate that the policy had a robust and strong impact on secondary-level educational attainment for mothers<sup>22</sup>.

Prior studies also show that the reform increased educational attainment beyond the compulsory level, particularly by raising high school completion among women (Kirdar et al., 2016; Aydemir and Kirdar, 2017; Torun, 2018; Akyol and Mocan, 2022). We therefore examine whether the compulsory schooling reform generated spillovers to high school and college attainment. As reported in the second row of Table 3, the effect on high school completion is statistically significant only under the narrower 3-year bandwidth; for 4–7-year bandwidths, the point estimates are sizable but not statistically significant. These results indicate that any spillovers beyond the mandated grade level are limited in scope and concentrated in specifications closest to the cutoff. Moreover, although the coefficients for college completion are mostly positive across bandwidths, they are small and statistically insignificant, suggesting that the policy had little to no effect on mothers' likelihood of completing tertiary education.

As discussed in the methodology section, we treat results from local polynomial approaches as supplementary evidence. Here, we present nonparametric RDD estimates based on the CCFT bandwidths for our education outcomes. Panel A of [Table A.7](#) reports the nonparametric reduced-form estimates of the impact of mothers' exposure to the education policy on their educational attainment. We find that the reform significantly increases the likelihood of completing at least middle school, with an RD estimate of 0.92 ( $p = 0.011$ ), consistent with our parametric estimates in Table 3. Maternal exposure to the reform also has a positive, though smaller, effect on the probability of completing high school (0.066,  $p = 0.076$ ). However, we find no significant impact at the college level (0.023,  $p = 0.515$ ). These nonparametric findings corroborate our main results in Table 3.

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<sup>21</sup> These results are consistent with recent studies that investigate the impact of mothers' exposure to the same education reform on maternal outcomes. For instance, Akgündüz et al. (2024) show that the reform increases the probability of completing middle school by 14 to 20 percentage points among mothers with young children (aged 2–5) in the TDHS survey. A more recent study by Akar et al. (2025) reports that the reform's impact on middle school completion rates ranges from 8.6 to 13.6 percentage points for all mothers with at least one child aged 0–5, using the time use survey data.

<sup>22</sup> To generate results that are more comparable with the existing literature, we also examine the impact of the reform on the education levels of all women and men in the survey sample, reported separately in Panels A and B of [Table A.5](#). Our findings indicate that the reform significantly increases the likelihood of completing middle school for both women and men; additionally, we find evidence of spillover effects for men only.

### 4.3. Main Results

This section presents the main results of our analysis: the reduced form RDD estimates for child’s educational and health outcomes. Columns 1-5 of [Table 4](#) present these results using bandwidths from three to seven years around the cutoff.

[Panel A](#) of Table 4 indicates that maternal exposure to the reform increases a child’s likelihood of being rated “very successful” or “successful” in Turkish language. The largest estimates are in Turkish, with coefficients between 0.105 and 0.140. For mathematics, the narrowest bandwidth yields an 8.5 percentage-point increase in the probability that the mother rates the child as “successful or very successful,” but estimates decline in magnitude and lose significance in wider bandwidths. Overall, we do not detect robust effects on mathematics; the isolated positive estimate near the cutoff does not persist across specifications. This pattern is consistent with the possibility that positive associations in simple OLS ([Table A.4](#)) reflect unobserved confounding (e.g., shared familial endowments), and it accords with evidence that mathematical ability is partly heritable and linked to brain-expressed genes (Skeide et al., 2020).

In [Panel B](#), estimates for “good or excellent health” are close to zero and statistically indistinguishable from zero across bandwidths. By contrast, the probability that a child is “often sad or unhappy” decreases across all bandwidths, ranging from  $-0.068$  to  $-0.116$ , and remains statistically significant at conventional levels, though only marginally so at narrower bandwidth of 3. Maternal exposure to the reform reduces the likelihood that a child is “often anxious or nervous,” with significant effects at the 6- and 7-year bandwidths. In conclusion, the reform has no detectable effect on general health status but yields measurable improvements in mental health.

We further examine heterogeneity by child gender, motivated by persistent son preference in parts of Turkish society (Altindag, 2016; Robitaille and Aydede, 2022). [Table A.6](#) shows that intergenerational gains are concentrated among boys — maternal exposure raises Turkish language achievement and improves mental health consistently across all bandwidths — while estimates for girls, though consistent in sign, are statistically insignificant. This pattern aligns with evidence from more developed countries documenting similar gains in education and skills for sons (Black et al., 2005; Lundborg et al., 2014), and suggests that the overall effects of maternal exposure on academic achievement and mental health are driven primarily by improvements among boys.

We also provide non-parametric RDD results based on the CCFT bandwidths for our main outcome variables. Panel B of [Table A.7](#) reports nonparametric reduced-form estimates of the impact of maternal exposure to the education reform on children’s outcomes. Consistent with the parametric results in Table 4, we find a sizable, statistically significant effect on Turkish-language performance (0.120,  $p < 0.0001$ ). For mathematics, the parametric approach yields no robust effect, whereas the nonparametric estimate suggests a 0.049 percentage-point increase in the probability of being rated “successful or very successful” ( $p = 0.148$ ), which is not statistically significant at conventional levels. Finally, in line with

Table 4, the nonparametric results indicate improvements in mental health: maternal exposure reduces the probability that a child is often anxious/nervous by 0.062 percentage points ( $p=0.059$ ) and often sad/unhappy by 0.080 percentage points ( $p=0.005$ ).

## 5. Identifying the Possible Channels

This section examines mechanisms through which mothers' exposure to the reform may influence children's educational and mental health outcomes. Increases in maternal schooling can operate along several margins. First, exposure to the reform may affect children through changes in mothers' labor-market outcomes and marriage, which alter resources and decision-making relevant to child development. Second, increases in schooling may strengthen key inputs to human capital—parental time, material investments, and the structure of home routines—and raise the productivity of those inputs. Even holding input levels constant, higher maternal education can improve how effectively those inputs are used.

### 5.1. Parental Characteristics

[Table 5](#) presents the effects of maternal exposure to the education policy on key parental and household characteristics across bandwidths ranging from three to seven years. [Panel A](#) reports estimate for mothers' current employment, monthly household income, and the household's ability to meet basic needs. The policy does not have a statistically significant effect on mothers' employment status in any bandwidth<sup>23</sup>. Although wage data are unavailable, the coefficients on log monthly household income are positive, ranging from 0.035 to 0.087, but not statistically significant. Consistently, the reform has no statistically significant impact on whether the household reports being able to meet its basic needs, with coefficients close to zero across most specifications. These results suggest that maternal employment, current household income, and perceived economic sufficiency are unlikely to be the main channels through which the reform affected children's outcomes.

Maternal exposure to the schooling reform could potentially affect children's outcomes through changes in paternal characteristics. [Panel B](#) of [Table 5](#) examines this possibility by estimating the effects of maternal exposure on husbands' middle-school completion and current employment status<sup>24</sup>. The estimates are not statistically significant across bandwidths, suggesting limited evidence that reform-exposed mothers were systematically matched with husbands who had higher educational attainment or different employment status. This finding should be distinguished from the positive effect of the reform

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<sup>23</sup> This result is also in line with Dincer et al. (2014), Güneş, (2015), and Usta (2020), Akgündüz et al. (2024), which use data from TDHS to investigate the impact of education reform on mothers' employment.

<sup>24</sup> The analysis sample includes all mothers regardless of marital status or co-residence with a partner. Husbands (defined as the child's father) are observed only when they are present in the same household and can be linked in the data. Consequently, the husband sample is smaller because some mothers are unmarried, divorced, separated, widowed, or not co-residing with the child's father due to long-distance work arrangements, which are common in Turkey.

on men's own educational attainment in the overall survey population<sup>25</sup>: while the policy directly increased educational attainment of all men<sup>26</sup>, Table 5 examines whether maternal exposure changed the characteristics of the spouses of treated women. The evidence therefore does not indicate that changes in husbands' education or employment are a primary channel through which maternal exposure affected children's outcomes.

Maternal exposure may also affect children's outcomes by changing women's relative position within the household. To examine this mechanism, [Table 5](#) reports estimates for women's educational standing relative to their husbands and for spousal age differences. We find limited evidence that maternal exposure increases the probability that a woman's middle-school completion status is at least equal to her husband's; the estimates are statistically significant only within the 7-year bandwidth. This pattern is consistent with a potential improvement in women's intra-household bargaining position<sup>27</sup>. However, because this measure partly reflects the direct increase in women's own schooling, it should not be interpreted as conclusive evidence of changes in marital sorting or bargaining power. Additionally, the estimated effects on spousal age differences are not statistically significant across bandwidths. Hence, Table 5 provides limited evidence that changes in paternal characteristics or spousal age gaps explain the estimated effects, while suggesting weak evidence of an improvement in mothers' relative educational standing within marriage.

## 5.2. Parental Investment

We examine whether the observed effects of the reform can be partly explained by changes in parental investment induced by mothers' exposure to the education policy. Panel A of [Table 6](#) reports the estimated effects on physical investments in children. The results indicate that maternal exposure to the reform is associated with higher education-related investments. In particular, children of exposed mothers are more likely to own literature books and computers, with estimates that are positive and statistically significant across all bandwidths<sup>28</sup>. The estimates for test/workbook ownership are also positive across specifications, although statistically significant only in the narrowest bandwidth, suggesting weaker evidence for this outcome.

The reform is also associated with increases in children's use of computers for homework. The estimated effects are positive across all bandwidths and statistically significant in most specifications, suggesting that maternal exposure to the reform may have improved children's access to digital learning

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<sup>25</sup> See [Table A.5](#) in Appendix for this result.

<sup>26</sup> We should also note that the education reform itself increased the educational attainment of husbands in our analysis sample.

<sup>27</sup> This finding is consistent with Akgündüz et al. (2024), who also show that the same education reform influences mothers' bargaining power, particularly for those with a child in early childhood period.

<sup>28</sup> These finding aligns with existing studies (Brown, 2006; Carneiro et al. 2013; Cui et al. 2019). For instance, Carneiro et al. (2013) show that more educated mothers are more likely to invest in their children through books, providing musical instruments, special lessons, or availability of a computer using data from US.

resources. There is also some evidence of increased participation in supplementary education, as reflected in positive estimates for both weekday and weekend course hours at bandwidth 4 and 5. However, these effects are statistically significant only in some bandwidths and should therefore be interpreted as suggestive rather than robust evidence.

By contrast, the estimates for private school enrollment are generally negative and mostly statistically insignificant, with significance appearing only in the widest bandwidth. Similarly, the estimates for the child nutrition index are small, statistically insignificant, and unstable in sign. These findings suggest that the reform primarily affected education-related investments rather than broader household investments such as nutrition or private schooling.

Although we find some increase in household income, this effect is not statistically significant. Therefore, the rise in material educational investments is unlikely to be explained solely by an income channel. Instead, the results are consistent with the interpretation that more educated mothers may reallocate household resources toward learning-related inputs, such as books, computers, and supplementary educational materials. However, because the evidence on household income is not statistically significant, and because the table does not directly observe household budgeting decisions, this interpretation should be viewed as suggestive rather than conclusive.

Panel B of [Table 6](#) examines parental time investments and children's time use. The most robust result concerns parents' discussions of national issues with their children. The estimated effects are positive and statistically significant across all bandwidths, indicating that maternal exposure to the reform increased cognitively oriented parental engagement. The estimates for discussing school progress and visiting libraries or bookstores are positive, but statistically significant only in the narrowest bandwidth. Thus, these outcomes provide only weak suggestive evidence of increased parental involvement. The estimates for discussing books, movies, or television shows are statistically insignificant and do not reveal a clear pattern.

The results also provide evidence of changes in children's time allocation. Maternal exposure to the reform is associated with a higher likelihood that children frequently socialize with friends, although the statistical significance of this effect varies across bandwidths. In addition, the reform is associated with a reduction in children's unpaid family labor of approximately 2.4–3.3 percentage points across most specifications<sup>29</sup>. However, the estimates for work in agriculture, sales, other jobs, and overall employment status are generally small and statistically insignificant. These findings suggest that the reform did not substantially reduce children's overall employment, but it may have reduced their involvement in unpaid family work.

Overall, the results in [Table 6](#) suggest that maternal exposure to the education reform increased education-oriented parental investments. The strongest evidence is found for children's ownership of

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<sup>29</sup> Excluding mothers whose child receive conditional cash transfers for education or social and economic support from our analysis sample does not alter our results.

literature books and computers and parents' discussions of national issues with children. Evidence for supplementary course hours, test/workbook ownership, library or bookstore visits, and children's time use outcomes---more socialization and less unpaid family work is suggestive rather than conclusive as significance of these coefficients vary across bandwidths. Thus, these findings are consistent with the view that maternal education may improve child outcomes not only through increased schooling of mothers themselves, but also through changes in parental investment, household resource allocation, and children's time use.

## 6. Robustness Checks

### 6.1. Alternative Sample Specifications

We construct alternative sample specifications to assess the robustness of our main analytical sample. In the baseline analysis, if a mother has more than one child in the target age group, we consider only the outcomes of the youngest eligible child. As a robustness check, we re-estimate the models using all eligible children aged 6–12, without restricting the sample to the youngest child, and cluster standard errors at the mother and the mother's month–year of birth. [Table A.8](#) reports these results. We find that as in our main analysis, maternal exposure to the policy significantly increases the likelihood of a child's success in the Turkish language and improves mental health.

We restrict our analysis sample to mothers whose youngest child is between 6 and 8 years to address concerns that our results may be driven by grade-related increases in coursework difficulty. Focusing on mothers with children aged 6–8 ([Table A.9](#)), we find stronger and more consistent effects of maternal exposure to the education reform on children's academic performance relative to our main results. Specifically, the coefficients for Turkish are larger, while the effects for mathematics are significant at wider bandwidths but not at narrower ones, consistent with our baseline findings. For health outcomes, the results broadly mirror the main analysis: we find insignificant effects on general health status and a reduction in the likelihood of being sad, while the negative effect on anxiety loses significance overall but is marginally significant at some bandwidths.

Some studies investigating the impact of Turkish education policy on individual-level outcomes suggest that exposure to the policy for those born in 1986 and 1987 uncertain (Aydemir and Kirdar, 2017; Kirdar et al., 2018; Akgündüz et al. 2024). Following this literature, we exclude the entire 1986 and 1987 birth cohort from the sample. The estimates in [Table A.10](#) in the Appendix confirm our main results.

Finally, we exclude mothers whose children receive cash transfers for education or social and economic<sup>30</sup> from the analysis sample to ensure that our results are not influenced by the effects of these

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<sup>30</sup> “Cash transfers” refer to two child-related government programs captured in our survey: Conditional Education Assistance and Social and Economic Support. Conditional Education Assistance is available to households without social security coverage that are deemed in need under Law No. 3294, conditional on the child's continued enrollment in formal education and no more than four days of absence in a month during months when school is

programs. [Table A.11](#) in the Appendix shows that our findings remain robust to this sample modification.

## 6.2. Alternative Outcome Variables

We assess robustness using alternative ordered versions of our outcomes in [Table A.12](#) in Appendix. Academic performance is coded as an ordinal variable from 5 to 1 based on maternal reports: “very successful” (5), “successful” (4), “fair” (3), “unsuccessful” (2), and “very unsuccessful” (1). Mother-rated general health is coded from 5 to 1 for “excellent,” “good,” “fair,” “bad,” and “very bad,” respectively. The two mental health measures—sad/unhappy and anxious/nervous—are coded from 5 to 1 based on reported frequency: every day (5), once a week (4), once a month (3), once a year (2), and never (1), so higher values initially indicate more frequent symptoms. We then construct standardized indices, including an academic success index and a mental health index. We standardize each component using the control-group mean and standard deviation and average the standardized components within each outcome family<sup>31</sup>.

Using these ordered outcomes, maternal exposure to the reform raises Turkish achievement, with significance at 6–7-year bandwidths, while mathematics effects are significant only at 3–4 years. Mother’s exposure to the reform improves the academic success index at 3, 4, 6, and 7 years. For health, there is no effect of the reform on mother-rated general health; sadness significantly declines at 6–7 years; anxiety is generally insignificant; and the mental health index is negative but not statistically significant across bandwidths. We conclude that ordered outcomes largely validate the main results.

## 6.3. Alternative Specifications

Instead of incorporating differential time trends by education policy exposure, we include linear and quadratic terms for the difference between individuals’ month and year of birth and the cutoff value (January 1987). The results presented in [Table A.13](#) in the Appendix demonstrate that our main findings are robust to these alternative trend specifications.

We also estimate the model excluding child related control variables—such as child age, first child age, last child age, the number of children, child order, child sex, and the interaction between child order

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in session. Social and Economic Support assistance is provided to families who are unable to meet their children’s basic needs due to economic deprivation and aims to support children being cared for within their families rather than in institutional care. We measure receipt using caregiver self-reports: (Q1) “Did the child receive conditional education assistance in the previous school year (2021/2022)?” and (Q2) “In the last 12 months, did you receive social and economic support for the child from the Ministry of Family and Social Services?” We define any transfer receipt as an indicator equal to 1 if Q1 = 1 or Q2 = 1 (and also report each component separately). In the estimation sample, nearly 3 percent of the sample receives conditional education assistance, nearly 2 percent receives social and economic support.

<sup>31</sup> We construct the indices once using the control group within the largest bandwidth, 84 months, and then use the same index across all bandwidth specifications. This approach keeps the scale of the index fixed across regressions and makes the estimates comparable across bandwidths.

and child sex. [Table A.14](#) in the Appendix shows that maternal exposure to the education reform significantly increases children’s achievement in Turkish and reduces sadness and anxiety, consistent with the main findings in Table 4.

#### **6.4. Placebo Education Policy**

We conduct two placebo tests to confirm the absence of treatment effects at placebo cutoffs. First, we restrict our sample to individuals born between 1979 and 1986, who belong to the control group and were not exposed to the education policy. Specifically, we assess the impact of a placebo policy on our outcome variables by creating an indicator variable, *placebo policy*, set to 1 for individuals born in or after January 1983, and 0 otherwise. As shown in [Table A.15.a.](#) in the Appendix, the placebo policy has no significant impact on children’s education and mental outcomes. These findings confirm that our main results are driven by the change in the Turkish compulsory schooling policy.

To further validate our findings, we create seven placebo policies with cutoff birth years of 1982, 1983, 1984, 1985, 1986, 1988 and 1989. First, we estimate the impact of these placebo policies on children outcomes for mothers who were born within nine years before and after January 1987. The results, presented in [Table A.15.b.](#) in the Appendix, show that the placebo policies eliminate the observed effects of the true education policy on our outcome variables.

#### **6.5. Assessing the reliability of mothers’ self-reported education measures**

Our analysis relies on mothers’ assessments of their children’s academic performance, which raises the possibility of measurement error in these reports. More-educated mothers may overstate performance due to social desirability, potentially biasing estimates upward; alternatively, they may hold higher standards and rate their children more critically than less-educated mothers. In our main results, maternal exposure to the reform increases the likelihood that a child is rated “successful” or “very successful” in Turkish language performance, with no corresponding effect in mathematics—patterns that are not consistent with a uniform reporting bias.

To probe this concern further, we use self-assessments from siblings aged 13–17. As described in the Data section, the survey administers the “13–17 Age Group Individual Questionnaire” directly to adolescents allowing us to compare mothers’ reports with adolescents’ own evaluations. We restrict our analysis sample (mothers who have at least one child aged 6–12) to mothers who also have at least one child aged 13–17, so that the comparison is made within the same set of mothers. We then examine whether reform exposure affects the discrepancy between the mother’s rating and the older child’s self-assessment. [Table A.16](#) in Appendix shows no reform effect on these mother–child assessment differences in any subject.

Additionally, we examine whether the observed effects are concentrated at the extreme upper or lower categories ([Table A.17](#)). Our findings show that the reform effect is not driven by changes at the tails of the academic performance distribution. Specifically, in Panel A the coefficients for being reported as “very successful” are small and statistically insignificant for both Turkish language and mathematics, indicating no effect at the upper tail. In Panel B, the coefficients for being reported as “very unsuccessful” are also small and mostly insignificant. Although a few estimates are weakly significant at the 10% level, these effects are very small in magnitude and should be interpreted cautiously because “very unsuccessful” is a relatively rare outcome. Moreover, the significance is not robust across bandwidths. Our results suggest that the reform did not meaningfully affect either the upper or lower tail of academic performance. As shown in the previous section, effects are also absent in placebo tests. Thus, these results indicate that our main findings are not driven by measurement error arising from maternal reporting bias.

## 6.6. Multiple Hypothesis Testing and Outcome Aggregation

Since our analysis examines several related outcomes and potential channels, we address multiple hypothesis concerns in two complementary ways. First, we report Benjamini–Hochberg adjusted p-values for families of related outcomes---child educational outcomes (Turkish language and mathematics performance); mental health outcomes (feeling sad/unhappy, feeling anxious); parental characteristics; parental physical investment outcomes; parental time investment outcomes and children’s time allocation outcomes in Appendix [Table A.18](#).<sup>32</sup> Second, we construct standardized indices for each outcome family and re-estimate the main specification using these indices as dependent variables, with results reported in Appendix [Table A.19](#).

The Benjamini–Hochberg adjusted p-values confirm the robustness of main findings for Turkish language achievement, which remains significant across all bandwidths after adjustment, while the absence of a robust effect in mathematics is equally confirmed. For mental health outcomes, the finding that maternal exposure to the reform reduces the children being reported as sad or unhappy at 5, 6, 7 year bandwidths is consistent with our main results though there is loss of significance at narrower bandwidths. Similarly, the probability of children being anxious or nervous decreases at 6- and 7-year bandwidths consistent with our main results in Table 4.

In our examination of potential channels, the most robust result is children’s ownership of a computer, which remains statistically significant after adjustment across all but one bandwidth. The evidence for child ownership of literature books remains significant at the 5, 6, and 7-year bandwidths. Course hours and computer time for homework remain significant mainly at the 4- and 5-year bandwidths, indicating that these effects are not uniformly robust across specifications. While Table 6

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<sup>32</sup> We adjust p-values using the Benjamini-Hochberg false discovery rate procedure (Benjamini and Hochberg, 1995), implemented in Stata using the `qqvalue` command (Newson, 2010).

reports positive effects on several time-investment measures, the BH-adjusted p-values show that only parental discussion of national issues remains significant in narrower bandwidths. In contrast, results for children's time allocation outcomes are substantially weakened after adjustment, with only weak evidence for unpaid family labor in one bandwidth. The evidence for children frequently socializing with friends also weakens after adjustment. Specifically, the BH-adjusted p-value remains statistically significant at the 3-year bandwidth, while the estimates at the 5, 6, and 7-year bandwidths provide only weakly suggestive evidence and are not statistically significant at conventional levels. Additionally, after adjusting for multiple hypothesis testing, the positive association between women's middle school completion being at least equal to the husband's and the outcome is not statistically significant at the conventional 5 percent level in any specification, though it remains significant at the 10 percent in the final specification. Overall, the channel results point most strongly toward increased parental physical investment.

The index<sup>33</sup> results in Appendix [Table A.19](#) support this interpretation. Mothers' exposure to the reform has a positive and statistically significant effect on the education index across all bandwidths. The mental health index also provides evidence of improvements at larger bandwidths. For potential channels, the parental physical investment index is positive and statistically significant across all bandwidths, while the parental time investment index is significant mainly at narrower bandwidths and marginally significant at wider bandwidths. The child labor index is not statistically significant across bandwidths, suggesting that reduced child labor is unlikely to be a primary channel.

## 7. Conclusion

This paper examines the intergenerational effects of Türkiye's 1997 compulsory schooling reform, which increased mandatory education from five to eight years. Using a regression discontinuity design, we study whether mothers' exposure to the reform affected the academic performance and health of their children. We also examine potential channels through which maternal education may affect child outcomes.

The findings indicate that mothers' exposure to the reform improved children's academic performance, with effects concentrated in Turkish language achievement and no evidence of comparable gains in mathematics. Beyond academic outcomes, we find evidence that the maternal exposure to the reform improved children's emotional well-being. Children of exposed mothers are less likely to be

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<sup>33</sup> Standardized summary measures constructed by averaging related outcomes within each outcome family. For each index, component variables are first oriented so that higher values indicate better outcomes or greater investment. Each component is then standardized using the mean and standard deviation of the control group within the largest bandwidth of 84 months. The standardized components are averaged to form the index, and the resulting index is used consistently across all bandwidth specifications.

reported as sad/unhappy or anxious while general health status remains largely unchanged. We also document heterogeneity by child gender, with larger effects for sons than for daughters.

The analysis of potential channels suggests that these effects are unlikely to be explained by broad changes in household economic conditions, parental employment, or fathers' characteristics. Instead, the evidence points more strongly toward changes in child-focused investments. Mothers exposed to the reform appear more likely to provide educational resources, such as books, computers, and other learning-related inputs, and to engage in activities that may support children's learning and socio-emotional development. We also find suggestive evidence of reduced involvement in unpaid family work. Additionally, we provide weak and non-robust evidence that mothers' educational position relative to their husbands may have improved, which could have facilitated greater child-focused investment. Thus, our findings underscore the far-reaching implications of maternal education, suggesting that educational reforms targeting mothers may generate substantial intergenerational benefits through both academic and socio-emotional pathways.

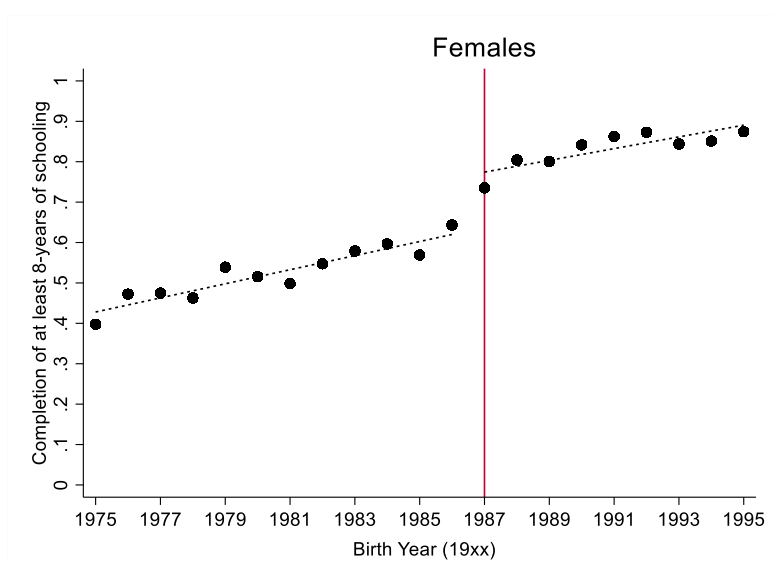
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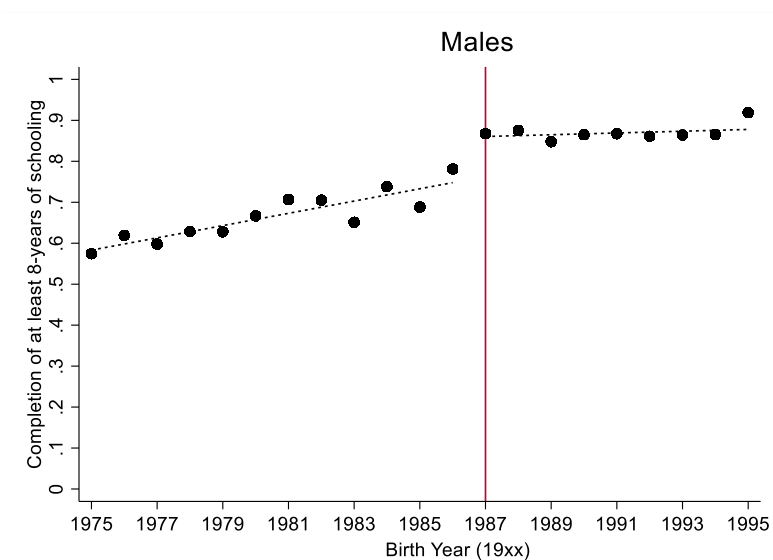
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## TABLES AND FIGURES



*Figure 1.a.* Mean of at least middle school completion rates of all females in the survey sample by the birth year



*Figure 1.b.* Mean of at least middle school completion rates of all males in the survey sample by the birth year

Table 1. Descriptive Statistics

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	ALL		TREATMENT		CONTROL	
	N	mean	N	mean	N	mean
PANEL A: Maternal Outcomes						
Age	2,492	35.87 (3.284)	1,165	32.78 (1.709)	1,327	38.41 (1.714)
At least middle school completion	2,492	0.634 (0.482)	1,165	0.750 (0.433)	1,327	0.539 (0.499)
Current Employment	2,489	0.307 (0.461)	1,163	0.248 (0.432)	1,326	0.356 (0.479)
Log of Monthly Household Income	2,492	9.024 (0.634)	1,165	8.962 (0.582)	1,327	9.075 (0.670)
Household Meets Basic Needs	2,492	0.098 (0.298)	1,165	0.091 (0.288)	1,327	0.104 (0.305)
Age of First-Born Child	2,492	11.66 (3.298)	1,165	10.48 (2.879)	1,327	12.64 (3.303)
Age of Youngest Child	2,492	6.103 (3.424)	1,165	5.038 (3.203)	1,327	6.983 (3.351)
Number of Children	2,492	2.417 (1.001)	1,165	2.481 (1.009)	1,327	2.363 (0.991)
Number of Children in Household Aged 6–12	2,492	1.403 (0.605)	1,165	1.485 (0.661)	1,327	1.336 (0.546)
PANEL B: Child Outcomes						
Child age	2,492	8.438 (1.919)	1,165	8.075 (1.803)	1,327	8.739 (1.961)
Turkish Language	2,380	0.754 (0.430)	1,004	0.786 (0.416)	1,060	0.727 (0.441)
Mathematics	2,361	0.697 (0.460)	1,105	0.700 (0.458)	1,256	0.694 (0.461)
Child often anxious or nervous	2,477	0.345 (0.475)	1,159	0.333 (0.472)	1,318	0.354 (0.478)
Child often sad or unhappy	2,478	0.310 (0.463)	1,158	0.302 (0.459)	1,320	0.317 (0.466)
Child's general health status: Good Health	2,491	0.888 (0.315)	1,165	0.876 (0.330)	1,326	0.899 (0.301)

*Notes:* The sample includes mothers with at least one child aged 6-12 years, born between 1981 and 1992. The treatment group consists of those born after January 1987, while the control group includes those born before January 1987. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. The statistics report the number of observations, means, and standard deviations in parentheses for the specified outcomes.

*Source:* 2022 Turkish Child Survey, TURKSTAT

Table 2: Potential Sample Selection

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	<b>3</b>	<b>4</b>	<b>5</b>	<b>6</b>	<b>7</b>
<i>Dependent Variables</i>					
Reform	-0.033 (0.039)	-0.009 (0.038)	-0.007 (0.035)	0.009 (0.033)	0.004 (0.032)
Num. Obs.	2,105	2,745	3,412	4,047	4,659

*Notes:* Reform is a binary variable that equals 1 if the individual was born after January 1987 and 0 otherwise. The dependent variable, *sample selected*, is also a binary variable that takes the value 1 if a woman has at least one child aged 6-12, and 0 otherwise. The sample includes all women in the survey population. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth and including birth-month dummies. The regressions are weighted using sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

*Source:* 2022 Turkish Child Survey, TURKSTAT

Table 3. Impact of the Education Policy on Educational Attainment

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	<b>3</b>	<b>4</b>	<b>5</b>	<b>6</b>	<b>7</b>
<i>Dependent Variables</i>					
At least middle school completion	0.092* (0.050)	0.078* (0.044)	0.067 (0.042)	0.074* (0.040)	0.113*** (0.038)
At least high school completion	0.106* (0.058)	0.070 (0.053)	0.050 (0.047)	0.043 (0.045)	0.062 (0.042)
At least college completion	0.037 (0.049)	0.030 (0.043)	0.018 (0.040)	0.016 (0.039)	0.016 (0.035)
Num. Obs.	1,357	1,752	2,160	2,492	2,803

*Notes:* Reform is a binary variable that equals 1 if the individual was born after January 1987 and 0 otherwise. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth and including birth-month dummies. The regressions are weighted using sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

*Source:* 2022 Turkish Child Survey, TURKSTAT

Table 4: Impact of Mother's Exposure to the Education Policy on Children's Outcomes-  
Reduced Form Regression Results

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	<i>3</i>	<i>4</i>	<i>5</i>	<i>6</i>	<i>7</i>
<i>Dependent Variables</i>					
PANEL A: Educational Outcomes					
Turkish Language	0.140*** (0.049)	0.105** (0.043)	0.116*** (0.038)	0.121*** (0.035)	0.118*** (0.033)
Num. Obs.	1,298	1,672	2,064	2,380	2,678
Mathematics	0.085** (0.041)	0.061 (0.041)	0.031 (0.036)	0.036 (0.034)	0.031 (0.033)
Num. Obs.	1,292	1,661	2,050	2,361	2,656
PANEL B: Health Outcomes					
Good Health	0.016 (0.040)	-0.007 (0.035)	-0.018 (0.031)	-0.020 (0.028)	-0.011 (0.026)
Sad/Unhappy	-0.068 (0.045)	-0.068* (0.040)	-0.080** (0.038)	-0.116*** (0.035)	-0.095*** (0.033)
Anxious/Nervous	-0.046 (0.049)	-0.049 (0.044)	-0.062 (0.041)	-0.086** (0.039)	-0.072* (0.038)
Num. Obs.	1,357	1,751	2,159	2,491	2,802

Notes: Reform is a binary variable equal to 1 if the individual was born after January 1987, and 0 otherwise. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth, birth-month dummies, the number of children, child age, first child age, last child age, dummies for child order and child sex, and child order and child sex interaction. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Sources: 2022 Turkish Child Survey, TURKSTAT

Table 5. Impact of Mother's Exposure to the Education Policy on Parental Characteristics

	(1)	(2)	(3)	(4)	(5)
<b>Bandwidth (years)</b>	<b>3</b>	<b>4</b>	<b>5</b>	<b>6</b>	<b>7</b>
<i>Dependent Variables</i>					
PANEL A: Mother's outcomes					
Mother's Current Employment	-0.037 (0.041)	-0.028 (0.039)	-0.025 (0.034)	-0.055 (0.034)	-0.044 (0.033)
Log of Monthly Household Income	0.087 (0.071)	0.044 (0.063)	0.035 (0.058)	0.063 (0.055)	0.038 (0.052)
Household Meets Basic Needs	0.019 (0.031)	0.030 (0.026)	0.013 (0.026)	-0.003 (0.026)	0.003 (0.023)
Num. Obs.	1,357	1,752	2,160	2,492	2,803
PANEL B: Husband's (Father's) outcomes					
Husband's Middle School Completion	0.074 (0.057)	0.038 (0.053)	-0.001 (0.047)	-0.000 (0.045)	0.010 (0.041)
Husband's Current Employment	-0.001 (0.031)	-0.014 (0.027)	-0.029 (0.026)	-0.011 (0.024)	-0.012 (0.023)
Women's middle school completion at least equal to husband's	0.052 (0.043)	0.055 (0.037)	0.025 (0.034)	0.045 (0.032)	0.070** (0.029)
Age Difference Between Spouses	0.074 (0.486)	0.093 (0.448)	0.066 (0.411)	-0.124 (0.371)	-0.185 (0.358)
Num. Obs.	1,211	1,556	1,924	2,220	2,499

Notes: Reform is a binary variable equal to 1 if the individual was born after January 1987, and 0 otherwise. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth and including birth-month dummies. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Source: 2022 Turkish Child Survey, TURKSTAT

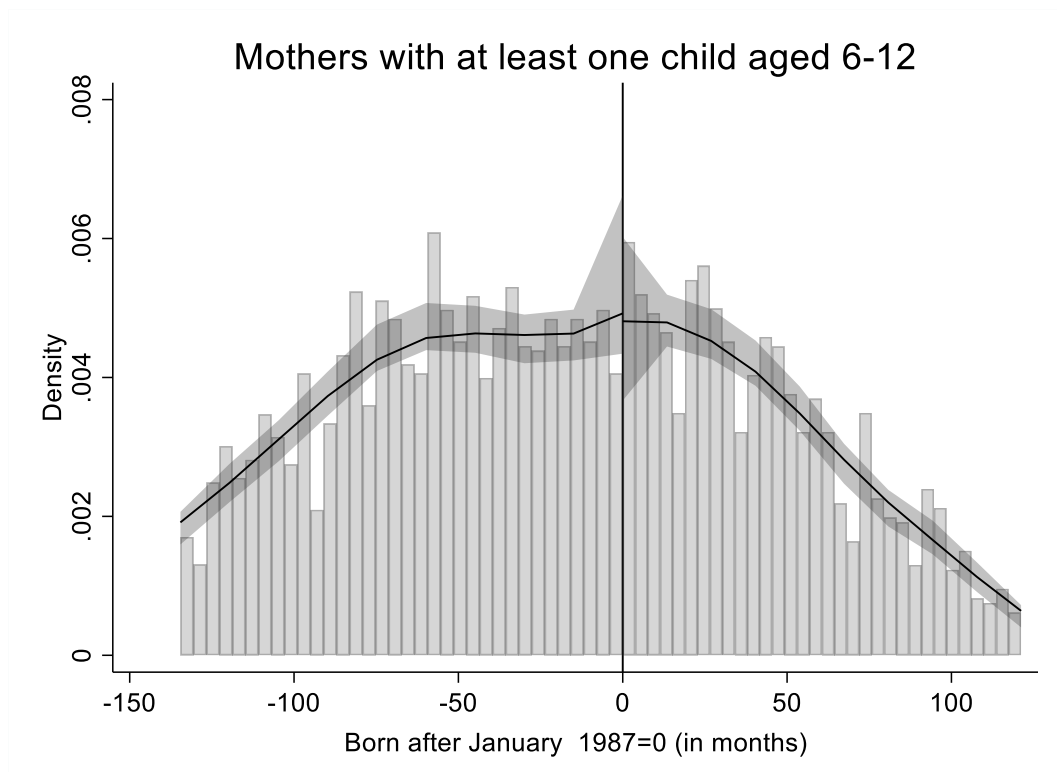
Table 6. Impact of Mother's Exposure to the Education Policy on Parental Investment

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	3	4	5	6	7
<i>Dependent Variables</i>					
PANEL A: Physical Investment					
Child Owns Literature Books	0.100** (0.040)	0.080** (0.036)	0.080** (0.031)	0.057* (0.030)	0.051* (0.029)
Child Owns Test/Workbooks	0.140** (0.066)	0.082 (0.063)	0.091 (0.056)	0.060 (0.050)	0.071 (0.045)
Child Owns a Computer	0.201*** (0.042)	0.088** (0.042)	0.099*** (0.036)	0.093*** (0.033)	0.074** (0.032)
Private School Enrollment	-0.023 (0.033)	-0.033 (0.028)	-0.035 (0.025)	-0.028 (0.023)	-0.036* (0.021)
Child's Weekend Course Hours	0.255 (0.288)	0.670** (0.271)	0.602** (0.253)	0.307 (0.238)	0.239 (0.228)
Child's Weekday Course Hours	0.120 (0.639)	1.214** (0.581)	1.409** (0.557)	0.816 (0.521)	0.765 (0.478)
Weekly Computer Time for Homework	0.692 (0.472)	1.038** (0.434)	0.945** (0.382)	0.736** (0.355)	0.661* (0.342)
Child Nutrition Index	0.036 (0.049)	0.024 (0.045)	0.030 (0.040)	-0.001 (0.036)	-0.011 (0.034)
PANEL B: Time Investment					
Parents Discuss National Issues with Child	0.125*** (0.041)	0.103** (0.041)	0.079** (0.038)	0.063* (0.036)	0.068** (0.034)
Parents Discuss School Progress with Child	0.053** (0.024)	0.030 (0.022)	0.023 (0.020)	0.026 (0.019)	0.027 (0.018)
Parents Visit Library or Bookstore with Child	0.103** (0.049)	0.072 (0.047)	0.050 (0.043)	0.045 (0.043)	0.050 (0.039)
Parents Discuss Books, Movies, or TV Shows with Child	0.050 (0.050)	0.005 (0.049)	-0.030 (0.044)	-0.026 (0.042)	-0.015 (0.039)
Child Frequently Socializes with Friends	0.121** (0.048)	0.061 (0.042)	0.076* (0.039)	0.070* (0.036)	0.069** (0.033)
Child Works as Unpaid Family Labor	-0.011 (0.016)	-0.029* (0.015)	-0.033** (0.014)	-0.031** (0.014)	-0.024* (0.013)
Child Works in Agriculture	0.007 (0.040)	-0.002 (0.035)	-0.019 (0.031)	-0.013 (0.029)	0.005 (0.028)
Child Works in Sales	0.014** (0.007)	0.008 (0.008)	0.006 (0.006)	0.005 (0.006)	0.005 (0.006)
Child Works in Other Jobs	-0.001 (0.003)	-0.005 (0.005)	-0.002 (0.004)	-0.005 (0.004)	-0.005 (0.004)
Child's Employment Status	-0.005 (0.039)	-0.020 (0.034)	-0.036 (0.031)	-0.031 (0.029)	-0.013 (0.028)
Num. Obs.	1,357	1,751	2,159	2,491	2,802

Notes: Reform is a binary variable equal to 1 if the individual was born after January 1987, and 0 otherwise. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth, birth-month dummies, the number of children, child age, first child age, last child age, dummies for child order and child sex, and child order and child sex interaction. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

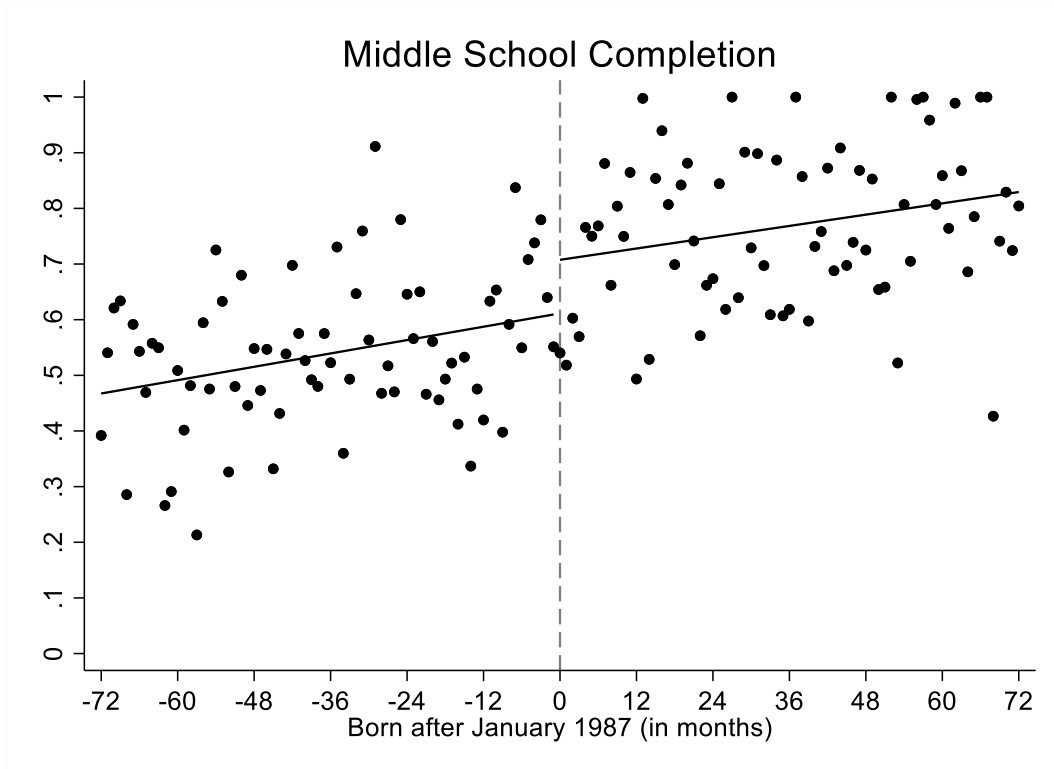
Sources: 2022 Turkish Child Survey, TURKSTAT

**ONLINE APPENDIX FOR “INTERGENERATIONAL EFFECTS OF TURKIYE'S EDUCATION REFORM ON CHILDREN'S ACADEMIC AND HEALTH OUTCOMES”**



**Figure A.1.** Estimated Density of the Running Variable and the Cattaneo-Jansson-Ma Tests

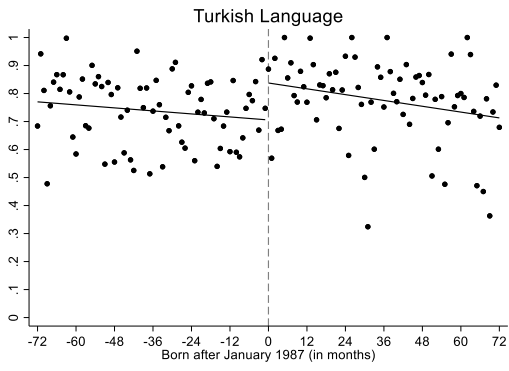
*Notes: Test Result,  $T=-0.5543$ ,  $p=0.5794$*



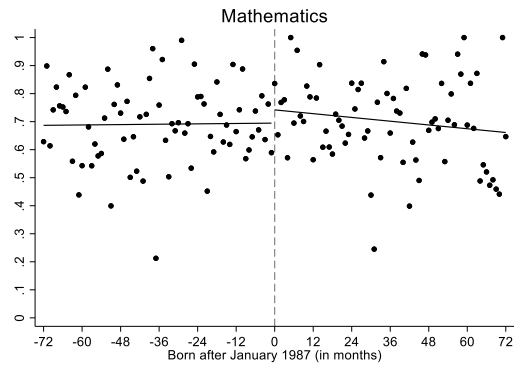
**Figures A.2.** RDD graph for Middle School Completion Rate Across Cohorts

*Notes: The sample includes mothers with at least one child aged 6–12 years old. The cutoff point is January 1987, and the running variable is the mother's birth month relative to January 1987. Each dot represents the survey-weighted raw mean of middle school completion for a birth-month cohort. The graph uses observations within 72 months on each side of the cutoff, and separate linear trends are fitted on either side. The data come from the 2022 Turkish Child Survey.*





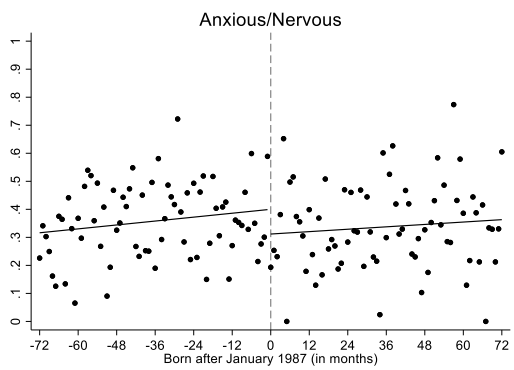
a) Turkish Language



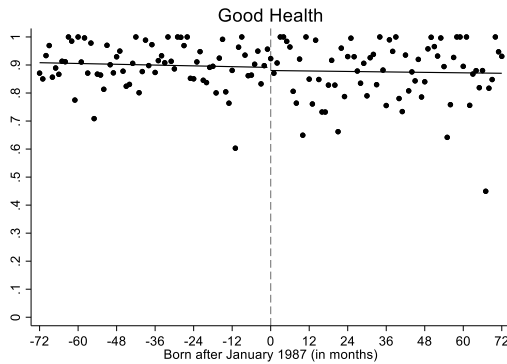
b) Mathematics



c) Sad/unhappy



d) Anxious/Nervous



e) General Health Status

**Figures A.3.** RDD graphs for Child’s Outcomes

*Notes: The sample includes mothers with at least one child aged 6–12 years old. The cutoff point is January 1987, and the running variable is the mother’s birth month relative to January 1987. Each dot represents the survey-weighted raw mean of middle school completion for a birth-month cohort. The graph uses observations within 72 months on each side of the cutoff, and separate linear trends are fitted on either side. The data come from the 2022 Turkish Child Survey.*

Table A.1. Impact of Mother's Exposure to the Reform on Covariates

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	<i>3</i>	<i>4</i>	<i>5</i>	<i>6</i>	<i>7</i>
Marital Status of mom: currently married	0.027 (0.035)	0.033 (0.034)	0.005 (0.031)	0.012 (0.028)	0.001 (0.026)
Birth age of mom	-0.124 (0.495)	-0.180 (0.409)	0.018 (0.360)	0.031 (0.334)	0.155 (0.301)
Number of Children	-0.003 (0.119)	0.053 (0.108)	0.097 (0.100)	0.086 (0.093)	0.065 (0.085)
Child Age	0.367 (0.256)	0.416* (0.219)	0.354* (0.193)	0.193 (0.177)	0.215 (0.159)
First Child Age	0.053 (0.463)	0.143 (0.387)	-0.032 (0.346)	-0.033 (0.320)	-0.160 (0.288)
Last Age	0.093 (0.436)	0.228 (0.388)	-0.068 (0.347)	-0.186 (0.304)	-0.253 (0.282)
Child sex	0.088 (0.061)	0.077 (0.057)	0.015 (0.051)	0.026 (0.046)	0.032 (0.042)
Order	-0.071 (0.126)	-0.007 (0.110)	0.006 (0.099)	0.001 (0.092)	-0.034 (0.085)
Girl, First Child	-0.006 (0.043)	-0.030 (0.040)	0.013 (0.036)	0.012 (0.032)	0.007 (0.032)
Girl, Second Child	0.002 (0.046)	-0.006 (0.042)	-0.002 (0.037)	-0.001 (0.034)	-0.006 (0.032)
Girl, Third Child	-0.071 (0.044)	-0.032 (0.037)	-0.023 (0.034)	-0.032 (0.030)	-0.028 (0.028)
Girl, Fourth Child	-0.012 (0.014)	-0.008 (0.012)	-0.004 (0.011)	-0.006 (0.010)	-0.005 (0.010)
Girl, Fifth Child	-0.002 (0.005)	-0.000 (0.005)	0.000 (0.005)	0.002 (0.004)	0.001 (0.004)
Boy, First Child	0.019 (0.048)	0.020 (0.041)	-0.010 (0.039)	-0.014 (0.038)	0.017 (0.033)
Boy, Second Child	0.020 (0.057)	0.013 (0.050)	-0.016 (0.045)	-0.004 (0.041)	-0.015 (0.037)
Boy, Third Child	0.047 (0.036)	0.047 (0.032)	0.038 (0.028)	0.045* (0.026)	0.030 (0.024)
Boy, Fourth Child	0.014 (0.019)	0.004 (0.018)	0.008 (0.016)	0.003 (0.014)	0.005 (0.013)
Boy, Fifth Child	-0.012 (0.008)	-0.009 (0.006)	-0.006 (0.006)	-0.005 (0.005)	-0.006 (0.004)
Boy, Sixth Child	0.001 (0.001)	0.001 (0.003)	0.001 (0.004)	0.001 (0.004)	0.001 (0.004)
Mother Birth Month: December	-0.130 (0.110)	-0.122 (0.093)	-0.089 (0.079)	-0.083 (0.072)	-0.069 (0.066)
Mother Birth Month: November	-0.081 (0.098)	-0.073 (0.090)	-0.046 (0.078)	-0.042 (0.071)	-0.031 (0.066)
Mother Birth Month: October	-0.080	-0.044	-0.040	-0.040	-0.032

	(0.144)	(0.125)	(0.112)	(0.102)	(0.095)
Mother Birth Month: September	-0.091	-0.079	-0.055	-0.046	-0.038
	(0.132)	(0.115)	(0.103)	(0.094)	(0.088)
Mother Birth Month: August	-0.054	-0.043	-0.026	-0.008	-0.008
	(0.156)	(0.137)	(0.122)	(0.109)	(0.099)
Mother Birth Month: July	0.041	0.033	0.027	0.031	0.028
	(0.135)	(0.116)	(0.106)	(0.098)	(0.092)
Mother Birth Month: June	-0.030	-0.016	-0.018	-0.010	-0.015
	(0.097)	(0.088)	(0.080)	(0.075)	(0.070)
Mother Birth Month: May	-0.023	-0.023	-0.021	-0.029	-0.016
	(0.120)	(0.109)	(0.102)	(0.092)	(0.088)
Mother Birth Month: April	0.071	0.057	0.052	0.025	0.010
	(0.097)	(0.088)	(0.079)	(0.072)	(0.069)
Mother Birth Month: March	0.119	0.093	0.042	0.034	0.003
	(0.144)	(0.130)	(0.112)	(0.107)	(0.099)
Mother Birth Month: February	0.129	0.096	0.084	0.067	0.069
	(0.134)	(0.110)	(0.103)	(0.092)	(0.087)
Mother Birth Month: January	0.129	0.120	0.090	0.099	0.100
	(0.194)	(0.165)	(0.152)	(0.145)	(0.132)
No. Obs.	1,357	1,752	2,160	2,492	2,803

*Notes:* Reform is a binary variable equal to 1 if the individual was born after January 1987, and 0 otherwise. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth and including birth-month dummies. The regressions are weighted using sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Source: 2022 Turkish Child Survey, TURKSTAT

TABLE A.2. Definition of Variables

Variables	How observed in the survey	Response categories	Dummy construction
Policy	Mother's month and year of birth information is given	birth month and birth year	Policy = 1 if born after January 1987; 0 otherwise.
Middle School	educational attainment constructed from (i) highest education level ever attended and (ii) whether that level was completed	Highest-attended level: preschool (kindergarten/crèche), primary school, middle school (general/vocational/technical), high school (general/vocational/technical), 2–3-year tertiary (college), 4-year tertiary (college/faculty), 5–6-year faculty, master's, PhD, unknown. Completion: completed / not completed.	Middle School = 1 if the mother completed middle school (highest-attended level is middle school and completed) or if she reports a level above middle school (high school, 2–3-year tertiary, 4-year tertiary, 5–6-year faculty, master's, PhD). Middle School = 0 if highest-attended level is preschool or primary school, or if she attended middle school but did not complete it. Education coded as unknown is treated as missing
Household income	Respondents report the household's average monthly net income over the last 12 months.	Open-ended response in Turkish Lira (TL)	Log of monthly household income
Satisfaction of basic needs	Parent's response to: "To what extent does the household income coming in meet your household's basic needs?"	Very easy; easy; difficult; very difficult	Satisfaction of basic needs = 1 if the response is very easy or easy; 0 otherwise.
Employment Status	Respondents are asked: "What is your current employment status?"	1 = Employed; 21 = Seeking a job; 22 = Currently in education/training; 23 = Retired or left working life due to age; 24 = Unable to work due to disability or chronic illness; 25 =	Employed = 1 if currently employed; 0 otherwise

Homemaker/caring for children, elderly, sick, etc.; 27 = Wealthy/not needing to work

Women's middle school completion at least equal to husband's (Mother's education  $\geq$  Father's education)

=1 if the woman's middle-school-completion status is at least equal to her husband's, and 0 otherwise. It equals 1 if both spouses completed at least middle school, if neither spouse completed at least middle school, or if the woman completed at least middle school while her husband did not. It equals 0 only if the husband completed at least middle school but the woman did not.

Academic performance	Mother's response to: "How do you evaluate your child's performance in any subjects?"	Very successful; successful; fair; unsuccessful; very unsuccessful	Academic performance = 1 for very successful or successful; 0 for fair, unsuccessful, or very unsuccessful.
Good health	Mother's response to: "How is your child's health in general?"	Excellent; good; fair; bad; very bad	Good health = 1 for excellent or good; 0 for fair, bad, or very bad.
Anxious /Nervous	Mother's response to: "How often does the child appear very anxious or tense?"	Every day; once a week; once a month; a few times a year; never	Anxious = 1 for every day or once a week; 0 otherwise.
Sad/Unhappy	Mother's response to: "How often does the child appear very sad or unhappy?"	Every day; once a week; once a month; a few times a year; never	Sad = 1 for every day or once a week; 0 otherwise.

Parental involvement in books / movies / TV	Response to: “How often do you or the other parent in your household (if any) talk with your child about books, movies, or TV shows?”	Every day; almost every day; once or twice a week; once or twice a month; rarely or never	Parental involvement = 1 for all responses except rarely or never; 0 for rarely or never.
Parental involvement: library / bookstore	Response to: “How often do you or the other parent in your household (if any) go to the library or bookstore with your child?”	Every day; almost every day; once or twice a week; once or twice a month; rarely or never	Parental involvement = 1 for all responses except rarely or never; 0 for rarely or never.
Parental involvement: school situation	Response to: “How often do you or the other parent in your household (if any) talk about your child’s school situation?”	Every day; almost every day; once or twice a week; once or twice a month; rarely or never	Parental involvement = 1 for all responses except rarely or never; 0 for rarely or never.
Parental involvement: national issues	Response to: “How often do you or the other parent in your household (if any) talk about national issues?”	Every day; almost every day; once or twice a week; once or twice a month; rarely or never	Parental involvement = 1 for all responses except rarely or never; 0 for rarely or never.
Unpaid family work	Child time-use module: whether the child assisted in the family’s or a relative’s workplace during the previous week (paid or unpaid)	Participation in the activity during the previous week	Unpaid family work = 1 if the child engaged in the activity during the past week; 0 otherwise.
Agricultural work	Child time-use module: whether the child worked on the household’s land or with animals during the previous week	Participation in the activity during the previous week	Agricultural work = 1 if the child engaged in the activity during the past week; 0 otherwise.
Sales-related work	Child time-use module: whether the child produced or sold goods during the previous week	Participation in the activity during the previous week	Sales-related work = 1 if the child engaged in the activity during the past week; 0 otherwise.

Child Frequently Socializes with Friends	Frequency of child socializing by call, message, internet, or in person	Daily; several times a week; once a week; several times a month; once a month	Socializing = 1 if the response falls in any listed frequency category; 0 otherwise.
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Table A.3. Descriptive Statistics (cont'd)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	ALL		TREATMENT		CONTROL	
	N	mean	N	mean	N	mean
PANEL A: Parental Characteristics						
Husband's middle school completion	2,220	0.714 (0.452)	1,048	0.740 (0.439)	1,172	0.691 (0.462)
Husband's Employment Status	2,230	0.919 (0.272)	1,055	0.923 (0.266)	1,175	0.916 (0.277)
Age differences btw spouses	2,233	-4.327 (3.954)	1,055	-4.934 (3.799)	1,178	-3.809 (4.011)
Mother's Education >= Father's Education	2,220	0.812 (0.391)	1,048	0.881 (0.324)	1,172	0.754 (0.431)
PANEL B: Parental Investment						
Parents Discuss National Issues with Child	2,491	0.260 (0.438)	1,165	0.226 (0.418)	1,326	0.287 (0.453)
Parents Discuss School Progress with Child	2,491	0.940 (0.238)	1,165	0.939 (0.239)	1,326	0.941 (0.237)
Parents Visit Library or Bookstore with Child	2,491	0.366 (0.482)	1,165	0.362 (0.481)	1,326	0.369 (0.483)
Parents Discuss Books, Movies, or TV Shows with Child	2,491	0.725 (0.447)	1,165	0.708 (0.455)	1,326	0.738 (0.440)
Child Frequently Socializes with Friends	2,487	0.440 (0.496)	1,162	0.397 (0.490)	1,325	0.475 (0.500)
Child Works as Unpaid Family Labor	2,491	0.033 (0.179)	1,165	0.025 (0.156)	1,326	0.040 (0.195)
Child Works in Agriculture	2,491	0.110 (0.313)	1,165	0.118 (0.323)	1,326	0.103 (0.305)
Child Works in Sales	2,491	0.006 (0.078)	1,165	0.007 (0.082)	1,326	0.005 (0.073)
Child Works in Other Jobs	2,491	0.004 (0.062)	1,165	0.004 (0.062)	1,326	0.004 (0.061)
Child Nutrition Index	2,491	0.123 (0.411)	1,165	0.107 (0.423)	1,326	0.136 (0.401)
Child's Weekday Course Hours	2,441	4.431 (5.659)	1,144	4.498 (5.952)	1,297	4.375 (5.405)
Child's Weekend Course Hours	2,441	2.086	1,144	2.012	1,297	2.148

		(2.753)		(2.735)		(2.769)
Weekly Travel Time to School (Hours)	2,441	13.49 (9.937)	1,144	13.04 (9.649)	1,297	13.86 (10.16)
Weekly Computer Time for Homework	2,491	2.775 (4.355)	1,165	2.550 (4.135)	1,326	2.961 (4.522)
Child Owns a Computer	2,441	0.317 (0.465)	1,144	0.274 (0.446)	1,297	0.352 (0.478)
Child Owns Test/Workbooks	2,441	0.679 (0.467)	1,144	0.666 (0.472)	1,297	0.689 (0.463)
Child Owns Literature Books	2,441	0.727 (0.446)	1,144	0.697 (0.460)	1,297	0.751 (0.433)

*Notes:* The sample includes mothers with at least one child aged 6-12 years, born between 1981 and 1992. The treatment group consists of those born after January 1987, while the control group includes those born before January 1987. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. The statistics report the number of observations, means, and standard deviations in parentheses for the specified outcomes.

*Source:* 2022 Turkish Child Survey, TURKSTAT

Table A.4. Association between Mothers' Middle School Completion and Children's Outcomes

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	<i>3</i>	<i>4</i>	<i>5</i>	<i>6</i>	<i>7</i>
<i>Dependent Variables</i>					
PANEL A: Educational Outcomes					
Turkish Language	0.140*** (0.034)	0.149*** (0.031)	0.135*** (0.028)	0.117*** (0.027)	0.114*** (0.025)
Num. Obs.	1,298	1,672	2,064	2,380	2,678
Mathematics	0.159*** (0.031)	0.164*** (0.030)	0.148*** (0.027)	0.137*** (0.025)	0.137*** (0.023)
Num. Obs.	1,292	1,661	2,050	2,361	2,656
PANEL B: Health Outcomes					
Good Health	0.071*** (0.024)	0.065*** (0.021)	0.056*** (0.021)	0.048** (0.019)	0.041** (0.017)
Sad/Unhappy	-0.048 (0.042)	-0.041 (0.035)	-0.021 (0.032)	-0.030 (0.030)	-0.022 (0.027)
Anxious/Nervous	-0.018 (0.034)	-0.001 (0.030)	0.013 (0.028)	0.004 (0.026)	0.004 (0.024)
Num. Obs.	1,357	1,751	2,159	2,491	2,802

Notes: *Middle School* is a binary variable equal to 1 if a mother completes at least middle school or above (at least eight years of schooling), and 0 otherwise. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth, birth-month dummies, the number of children, child age, first child age, last child age, dummies for child order and child sex, and child order and child sex interaction. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Sources: 2022 Turkish Child Survey, TURKSTAT

Table A.5. Impact of the Education Policy on Educational Attainment for the sample of all women and all men

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	3	4	5	6	7
<i>Dependent Variables</i>					
PANEL A: All Women					
Middle School	0.059 (0.036)	0.064** (0.031)	0.065** (0.029)	0.073*** (0.027)	0.104*** (0.028)
High School	0.062 (0.042)	0.063 (0.039)	0.052 (0.035)	0.040 (0.033)	0.053* (0.032)
College	0.031 (0.042)	0.019 (0.036)	0.012 (0.035)	0.018 (0.032)	0.023 (0.031)
Num. Obs.	1,909	2,494	3,089	3,658	4,206
PANEL B: ALL Men					
Middle School	0.145*** (0.048)	0.109*** (0.040)	0.140*** (0.040)	0.152*** (0.036)	0.152*** (0.034)
High School	0.157** (0.060)	0.097* (0.051)	0.081* (0.046)	0.093** (0.042)	0.077** (0.039)
College	0.094** (0.037)	0.044 (0.037)	0.007 (0.034)	0.001 (0.033)	0.002 (0.030)
Num. Obs.	1,699	2,184	2,705	3,203	3,688

Notes: Reform is a binary variable equal to 1 if the individual was born after January 1987, and 0 otherwise. The sample in Panel A (B) includes all women (men) in the survey population. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth and including birth-month dummies. The regressions are weighted using sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Source: 2022 Turkish Child Survey, TURKSTAT

Table A.6. The impact of mother's exposure to the education reform on child outcomes by child gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>Bandwidth (years)</i>	3	4	5	6	7	3	4	5	6	7
Dependent Variables	<b>BOYS</b>					<b>GIRLS</b>				
PANEL A: Educational Outcomes										
Turkish Language	0.224*** (0.080)	0.143** (0.067)	0.157** (0.061)	0.156*** (0.055)	0.164*** (0.051)	0.036 (0.068)	0.065 (0.059)	0.064 (0.051)	0.075 (0.048)	0.061 (0.045)
Num. Obs.	695	894	1,101	1,271	1,418	603	778	963	1,109	1,260
Mathematics	0.114* (0.058)	0.057 (0.059)	0.033 (0.053)	0.036 (0.049)	0.025 (0.046)	0.036 (0.068)	0.063 (0.062)	0.025 (0.056)	0.032 (0.053)	0.036 (0.052)
Num. Obs.	691	888	1,095	1,262	1,408	601	773	955	1,099	1,248
PANEL B: Health Outcomes										
Good Health	0.031 (0.051)	0.029 (0.046)	0.015 (0.040)	0.008 (0.037)	0.015 (0.036)	-0.017 (0.046)	-0.055 (0.044)	-0.058 (0.039)	-0.054 (0.037)	-0.039 (0.035)
Sad/Unhappy	-0.254*** (0.062)	-0.198*** (0.060)	-0.174*** (0.051)	-0.229*** (0.048)	-0.191*** (0.045)	0.134* (0.068)	0.074 (0.060)	0.018 (0.055)	0.014 (0.051)	0.015 (0.049)
Anxious/Nervous	-0.191** (0.076)	-0.154** (0.071)	-0.122** (0.061)	-0.181*** (0.058)	-0.153*** (0.055)	0.117* (0.070)	0.063 (0.062)	0.007 (0.056)	0.028 (0.052)	0.021 (0.050)
Num. Obs.	729	939	1,159	1,332	1,487	628	812	1,000	1,159	1,315

Notes: Reform is a binary variable equal to 1 if the individual was born after January 1987, and 0 otherwise. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. Columns (1)–(5) show the results for boys, while columns (6)–(10) present the results for girls. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth, birth-month dummies, the number of children, child age, first child age, last child age, dummies for child order and child sex, and child order and child sex interaction. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Sources: 2022 Turkish Child Survey, TURKSTAT

Table A.7. Non-Parametric Reduced Form Estimation

	RD Estimate	Standard Error	p-value	BW Loc. Poly.Left (h)	BW Loc. Poly.Right (h)	BW Bias Corr.Left (b)	BW Bias Corr. Right (b)	Observations
PANEL A: Maternal Educational Outcomes								
Middle School	0.092	0.036	0.011	131.183	71.511	153.296	112.758	3,860
High School	0.066	0.037	0.076	132.658	114.301	389.109	170.000	3,860
College	0.023	0.036	0.515	65.600	60.918	110.679	69.641	3,860
PANEL B: Child Outcomes								
Turkish Language	0.120	0.034	0.000	101.460	80.714	175.058	170.000	3,698
Mathematics	0.049	0.034	0.148	132.498	63.023	199.836	89.416	3,666
Anxious/Nervous	-0.062	0.033	0.059	107.054	170.000	187.735	148.500	3,837
Sad/Unhappy	-0.080	0.029	0.005	97.180	170.000	182.742	87.126	3,842
Good Health	-0.018	0.024	0.468	89.588	168.098	146.449	170.000	3,859

*Notes:* Nonparametric estimations are carried out using the procedures developed by Calonico, Cattaneo, and Titiunik (2014). The estimations are provided for a polynomial order of zero, and optimal bandwidths (h), which differ on both sides of the cutoff, are used. Different bandwidths are also chosen for bias estimation (b), and a triangular kernel is applied. These optimal bandwidths are calculated conditional on covariates and sampling weights. Covariates include birth-month dummies, the number of children, child age, first child age, last child age, child sex, dummies for child order, and child order and sex interaction. In the optimal bandwidth selection for maternal outcomes reported in Panel A, only birth-month dummies are used as covariates. If a mother has more than one child in the 6–12-year age group, only the youngest child is considered. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

*Source:* 2022 Turkish Child Survey, TURKSTAT

Table A.8. Estimation without a Restriction to Last Born Children

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	<b>3</b>	<b>4</b>	<b>5</b>	<b>6</b>	<b>7</b>
<i>Dependent Variables</i>					
PANEL A: Educational Outcomes					
Turkish Language	0.120** (0.055)	0.091* (0.047)	0.104** (0.041)	0.107*** (0.038)	0.109*** (0.036)
Num. Obs.	1,801	2,334	2,883	3,299	3,710
Mathematics	0.092 (0.056)	0.054 (0.048)	0.031 (0.043)	0.029 (0.040)	0.035 (0.037)
Num. Obs.	1,794	2,321	2,867	3,276	3,684
PANEL B: Health Outcomes					
Good Health	0.061 (0.038)	0.031 (0.033)	0.010 (0.029)	0.005 (0.027)	0.009 (0.025)
Sad/Unhappy	-0.092* (0.056)	-0.095** (0.048)	-0.113*** (0.043)	-0.139*** (0.040)	-0.115*** (0.037)
Anxious/Nervous	-0.082 (0.057)	-0.080 (0.049)	-0.100** (0.044)	-0.108*** (0.040)	-0.098*** (0.038)
Num. Obs.	1,873	2,429	2,998	3,432	3,857

Notes: Reform is a binary variable equal to 1 if the individual was born after January 1987, and 0 otherwise. *The sample includes all eligible children aged 6–12, without restriction to the last-born child.* The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth, birth-month dummies, the number of children, child age, first child age, last child age, dummies for child order and child sex, and child order and child sex interaction. The regressions are weighted using the sample weights. Standard errors are clustered *at the mother level and mother's month-year of birth*. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Sources: 2022 Turkish Child Survey, TURKSTAT

Table A.9. Restricting the sample to mothers with child aged between 6 and 8

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	3	4	5	6	7
<i>Dependent Variables</i>					
PANEL A: Educational Outcomes					
Turkish Language	0.140*	0.131**	0.142**	0.168***	0.150***
	(0.076)	(0.063)	(0.055)	(0.051)	(0.048)
Num. Obs.	739	959	1,186	1,358	1,518
Mathematics	0.092	0.080	0.049	0.085*	0.088*
	(0.065)	(0.058)	(0.051)	(0.048)	(0.046)
Num. Obs.	732	946	1,170	1,337	1,494
PANEL B: Health Outcomes					
Good Health	-0.003	-0.017	-0.010	-0.004	0.025
	(0.057)	(0.051)	(0.046)	(0.043)	(0.040)
Sad/Unhappy	-0.074	-0.091	-0.093	-0.114**	-0.084*
	(0.068)	(0.063)	(0.056)	(0.053)	(0.050)
Anxious/Nervous	-0.044	-0.059	-0.083	-0.072	-0.047
	(0.066)	(0.061)	(0.055)	(0.053)	(0.052)
Num. Obs.	795	1,033	1,274	1,462	1,635

Notes: Reform is a binary variable equal to 1 if the individual was born after January 1987, and 0 otherwise. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. *The sample includes mothers with at least one child aged 6-12 and whose youngest child is between 6 and 8 years old.* The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth, birth-month dummies, the number of children, child age, first child age, last child age, dummies for order and sex, and order and sex interaction. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Sources: 2022 Turkish Child Survey, TURKSTAT

Table A.10. DONUT HOLE APPROACH

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	<b>3</b>	<b>4</b>	<b>5</b>	<b>6</b>	<b>7</b>
<i>Dependent Variables</i>					
PANEL A: Educational Outcomes					
Turkish Language	0.245** (0.115)	0.105 (0.074)	0.135** (0.061)	0.144*** (0.054)	0.132*** (0.047)
Num. Obs.	850	1,224	1,616	1,932	2,230
Mathematics	-0.030 (0.107)	-0.035 (0.085)	-0.063 (0.060)	-0.031 (0.053)	-0.027 (0.047)
Num. Obs.	844	1,213	1,602	1,913	2,208
PANEL B: Health Outcomes					
Good Health	0.014 (0.082)	-0.043 (0.052)	-0.055 (0.043)	-0.051 (0.036)	-0.032 (0.034)
Sad/Unhappy	-0.115 (0.108)	-0.087 (0.074)	-0.113* (0.064)	-0.176*** (0.052)	-0.124*** (0.047)
Anxious/Nervous	-0.107 (0.121)	-0.097 (0.081)	-0.114 (0.072)	-0.145** (0.060)	-0.109** (0.053)
Num. Obs.	888	1,282	1,690	2,022	2,333

Notes: Reform is a binary variable equal to 1 if the individual was born after January 1987, and 0 otherwise. **The 1986 and 1987 birth cohort are excluded.** The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth, birth-month dummies, the number of children, child age, first child age, last child age, dummies for child order and child sex, and child order and child sex interaction. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Sources: 2022 Turkish Child Survey, TURKSTAT

Table A.11. Estimation Excluding Mothers Whose Child Receive Conditional Cash Transfers for Education or Social and Economic Support

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	<b>3</b>	<b>4</b>	<b>5</b>	<b>6</b>	<b>7</b>
<i>Dependent Variables</i>					
PANEL A: Educational Outcomes					
Turkish Language	0.139*** (0.049)	0.104** (0.044)	0.109*** (0.038)	0.114*** (0.035)	0.104*** (0.033)
Num. Obs.	1,245	1,604	1,977	2,283	2,565
Mathematics	0.077* (0.039)	0.047 (0.040)	0.013 (0.037)	0.022 (0.034)	0.018 (0.033)
Num. Obs.	1,239	1,593	1,963	2,264	2,543
PANEL B: Health Outcomes					
Good Health	0.010 (0.040)	-0.005 (0.034)	-0.016 (0.032)	-0.016 (0.029)	-0.004 (0.026)
Sad/Unhappy	-0.060 (0.044)	-0.054 (0.040)	-0.076** (0.037)	-0.113*** (0.035)	-0.092*** (0.033)
Anxious/Nervous	-0.032 (0.051)	-0.029 (0.047)	-0.052 (0.043)	-0.081** (0.040)	-0.068* (0.039)
Num. Obs.	1,300	1,677	2,065	2,387	2,682

Notes: Reform is a binary variable equal to 1 if the individual was born after January 1987, and 0 otherwise. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth, birth-month dummies, the number of children, child age, first child age, last child age, dummies for child order and child sex, and child order and child sex interaction. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Sources: 2022 Turkish Child Survey, TURKSTAT

Table A.12. Alternative Outcome Measures: Ordered Outcomes

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	<b>3</b>	<b>4</b>	<b>5</b>	<b>6</b>	<b>7</b>
<i>Dependent Variables</i>					
PANEL A: Educational Outcomes					
Turkish Language	0.122 (0.090)	0.091 (0.076)	0.104 (0.069)	0.128** (0.064)	0.151** (0.060)
Num. Obs.	1,298	1,672	2,064	2,380	2,678
Mathematics	0.131* (0.075)	0.137* (0.070)	0.038 (0.064)	0.057 (0.062)	0.057 (0.061)
Num. Obs.	1,292	1,661	2,050	2,361	2,656
Success Index	0.181* (0.099)	0.156* (0.085)	0.103 (0.079)	0.135* (0.075)	0.148** (0.072)
Num. Obs.	1,303	1,678	2,071	2,388	2,687
PANEL B: Health Outcomes					
Mother-rated health status	-0.023 (0.072)	-0.030 (0.058)	-0.052 (0.055)	-0.024 (0.051)	0.025 (0.051)
Sad/Unhappy	-0.108 (0.100)	-0.079 (0.092)	-0.120 (0.089)	-0.208** (0.082)	-0.145* (0.077)
Anxious/Nervous	0.025 (0.119)	0.026 (0.113)	-0.006 (0.107)	-0.050 (0.099)	-0.007 (0.093)
Mental Health Index	-0.030 (0.072)	-0.021 (0.068)	-0.052 (0.066)	-0.102 (0.062)	-0.060 (0.058)
Num. Obs.	1,357	1,751	2,159	2,491	2,802

Notes: Reform is a binary variable equal to 1 if the individual was born after January 1987, and 0 otherwise. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. *Educational outcome* (Turkish language/mathematics) is defined as an ordinal variable that assumes the values 5, 4, 3, 2, or 1, corresponding respectively to the mother's assessment of her child's performance in a given subject as "very successful," "successful," "fair," "unsuccessful," or "very unsuccessful." Success Index is defined as the averaging z-scores of two educational outcomes. *Mother-rated health status* is defined as an ordinal variable that takes the values 5, 4, 3, 2, or 1, corresponding respectively to the mother's assessment of the child's general health as 'excellent,' 'good,' 'fair,' 'bad,' or 'very bad. The two mental health measures—sad/unhappy and anxious/nervous—are coded from 5 to 1 based on reported frequency: every day (5), once a week (4), once a month (3), once a year (2), and never (1), so higher values initially indicate more frequent symptoms. The dependent variable *index* is constructed by first standardizing each component using the mean and standard deviation of the control group and then averaging the standardized components within each outcome family. The indices are constructed once using the control group within the largest bandwidth (84 months) and are subsequently applied across all bandwidth specifications to keep the scale fixed and ensure comparability of estimates across regressions. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth, birth-month dummies, the number of children, child age, first child age, last child age, dummies for child order and child sex, and child order and child sex interaction. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Sources: 2022 Turkish Child Survey, TURKSTAT

Table A.13. Alternative formulation of time trends

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	<b>3</b>	<b>4</b>	<b>5</b>	<b>6</b>	<b>7</b>
<i>Dependent Variables</i>					
PANEL A: Educational Outcomes					
Turkish Language	0.141*** (0.050)	0.106** (0.043)	0.116*** (0.038)	0.122*** (0.035)	0.119*** (0.033)
Num. Obs.	1,298	1,672	2,064	2,380	2,678
Mathematics	0.086** (0.041)	0.063 (0.041)	0.032 (0.036)	0.038 (0.035)	0.034 (0.033)
Num. Obs.	1,292	1,661	2,050	2,361	2,656
PANEL B: Health Outcomes					
Good Health	0.016 (0.040)	-0.007 (0.035)	-0.018 (0.031)	-0.020 (0.028)	-0.011 (0.027)
Sad/Unhappy	-0.069 (0.046)	-0.067* (0.040)	-0.081** (0.038)	-0.116*** (0.035)	-0.094*** (0.033)
Anxious/Nervous	-0.046 (0.049)	-0.049 (0.045)	-0.063 (0.041)	-0.085** (0.039)	-0.072* (0.038)
Num. Obs.	1,357	1,751	2,159	2,491	2,802

Notes: Reform is a binary variable equal to 1 if the individual was born after January 1987, and 0 otherwise. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. The regressions control for split linear and quadratic terms for the difference between the month and year of birth of individuals and the cutoff value, January 1987, with the running variable being the month-year of birth, birth-month dummies, the number of children, child age, first child age, last child age, dummies for child order and child sex, and child order and sex interaction. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Source: 2022 Turkish Child Survey, TURKSTAT



Table A.14. Estimation without child related control variables

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	3	4	5	6	7
<i>Dependent Variables</i>					
PANEL A: Educational Outcomes					
Turkish Language	0.124** (0.049)	0.085** (0.042)	0.108*** (0.038)	0.118*** (0.036)	0.115*** (0.033)
Num. Obs.	1,298	1,672	2,064	2,380	2,678
Mathematics	0.072 (0.046)	0.040 (0.043)	0.013 (0.038)	0.028 (0.036)	0.024 (0.033)
Num. Obs.	1,292	1,661	2,050	2,361	2,656
PANEL B: Health Outcomes					
Good Health	0.014 (0.040)	-0.006 (0.035)	-0.018 (0.031)	-0.020 (0.028)	-0.011 (0.026)
Sad/Unhappy	-0.056 (0.046)	-0.059 (0.041)	-0.075* (0.038)	-0.110*** (0.036)	-0.090*** (0.034)
Anxious/Nervous	-0.026 (0.050)	-0.031 (0.045)	-0.050 (0.042)	-0.079* (0.040)	-0.067* (0.038)
Num. Obs.	1,357	1,751	2,159	2,491	2,802

Notes: Reform is a binary variable equal to 1 if the individual was born after January 1987, and 0 otherwise. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Sources: 2022 Turkish Child Survey, TURKSTAT

Table A.15.a. Estimation with Placebo Education Policy-1

	(1)	(2)	(3)	(4)	(5)
<i>Dependent variables</i>	Turkish Language	Mathematics	Good Health	Anxious/Nervous	Sad/Unhappy
Placebo Reform	0.009 (0.087)	0.049 (0.092)	-0.101 (0.064)	-0.025 (0.109)	-0.018 (0.110)
No Obs.	1,633	1,647	1,719	1,709	1,712

Notes: Placebo Reform is a binary variable equal to 1 if the individual was born after January 1983, and 0 otherwise. The sample includes mothers born between 1979 and 1986, who belong to the control group and were not exposed to the education policy. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Sources: 2022 Turkish Child Survey, TURKSTAT

Table A.15.b. Estimation with Placebo Education Policy-2

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Placebo policy cutoff</i>	<i>1982</i>	<i>1983</i>	<i>1984</i>	<i>1985</i>	<i>1988</i>	<i>1989</i>
Turkish Language	-0.164 (0.105)	-0.083 (0.071)	0.003 (0.052)	0.013 (0.038)	0.043 (0.035)	-0.011 (0.051)
Num. Obs.	3,112					
Mathematics	-0.053 (0.127)	-0.022 (0.081)	-0.016 (0.068)	-0.024 (0.043)	-0.036 (0.036)	-0.0001 (0.049)
Num. Obs.	3,084					
Good Health	-0.047 (0.062)	-0.050 (0.046)	-0.035 (0.033)	-0.065** (0.025)	-0.042 (0.027)	0.017 (0.032)
Num. Obs.	3,253					
Anxious/Nervous	0.138 (0.117)	-0.034 (0.086)	-0.061 (0.056)	-0.056 (0.044)	-0.050 (0.036)	-0.006 (0.045)
Num. Obs.	3,234					
Sad/Unhappy	0.182* (0.093)	-0.053 (0.084)	-0.056 (0.055)	-0.052 (0.039)	-0.037 (0.035)	0.003 (0.042)
Num. Obs.	3,236					

Notes: Placebo policy is a binary variable equal to 1 if the individual was born after January 1982/83/84/85/88/89, and 0 otherwise. The sample includes mothers who were born within 9 years before and after January 1987. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth, birth-month dummies, child age, first child age, last child age, dummies for child order and child sex, and child order and child sex interaction. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Source: 2022 Turkish Child Survey, TURKSTAT

Table A.16: Assessing the reliability of mothers' self-reported education measures

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	<i>3</i>	<i>4</i>	<i>5</i>	<i>6</i>	<i>7</i>
<i>Dependent Variables</i>					
Turkish Language	-0.082 (1.646)	0.455 (1.700)	0.950 (1.712)	1.201 (1.574)	1.504 (1.496)
Num. Obs.	482	613	739	858	954
Mathematics	-0.246 (1.646)	0.277 (1.692)	1.041 (1.701)	1.275 (1.566)	1.609 (1.490)
Num. Obs.	482	613	739	858	954

Notes: This table examines whether exposure to the 1997 education reform affects reporting differences between mothers and adolescents. The sample includes mothers from the main analysis sample who also have at least one child aged 13–17. If a mother has more than one child in the age group of 13-17 years, only the youngest child is considered. Reform is an indicator equal to 1 if the mother was born after January 1987 and 0 otherwise; the running variable is the mother's month-year of birth. The dependent variable is the mother-adolescent gap in reported academic performance for the adolescent (aged 13–17), defined as the mother's rating minus the adolescent's self-rating for each subject, so positive values indicate that the mother reports higher performance than the adolescent. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth, birth-month dummies, the number of children, child age, first child age, last child age, dummies for child order and child sex, and child order and child sex interaction. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Sources: 2022 Turkish Child Survey, TURKSTAT

Table A.17: Impact of Mother's Exposure to the Education Policy on child's probability of being very successful and very unsuccessful in subjects

	(1)	(2)	(3)	(4)	(5)
<b>Bandwidth (years)</b>	<b>3</b>	<b>4</b>	<b>5</b>	<b>6</b>	<b>7</b>
<b>Dependent Variables</b>					
PANEL A: Being very <b>successful</b> in subjects					
Turkish Language	-0.029 (0.047)	-0.038 (0.039)	-0.054 (0.039)	-0.035 (0.035)	-0.003 (0.033)
Num. Obs.	-0.029	-0.038	-0.054	-0.035	-0.003
Mathematics	0.043 (0.048)	0.061 (0.041)	-0.005 (0.039)	-0.003 (0.036)	0.008 (0.034)
Num. Obs.	1,292	1,661	2,050	2,361	2,656
PANEL B: Being very <b>unsuccessful</b> in subjects					
Turkish Language	0.004 (0.004)	-0.002 (0.004)	-0.004* (0.003)	-0.006* (0.003)	-0.003 (0.002)
Num. Obs.	1,298	1,672	2,064	2,380	2,678
Mathematics	0.009* (0.005)	0.004 (0.005)	0.002 (0.004)	0.001 (0.004)	0.004 (0.004)
Num. Obs.	1,298	1,672	2,064	2,380	2,678

Notes: Reform is a binary variable equal to 1 if the individual was born after January 1987, and 0 otherwise. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. The dependent variable, academic performance in a specific subject, in Panel A (B) is coded as 1 if the mother evaluates her child's performance in a specific subject as "very successful (very unsuccessful)" 0 otherwise. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth, mother's birth-month dummies, the number of children, child age, first child age, last child age, dummies for child order and child sex, and child order and child sex interaction. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Sources: 2022 Turkish Child Survey, TURKSTAT

Table A.18. Results of The Benjamini-Hochberg (BH) (adjusted p-values) for all bandwidths

<i>Bandwidths (years)</i>	<i>3</i>	<i>4</i>	<i>5</i>	<i>6</i>	<i>7</i>
<b>Table 4: Child Educational and Health Outcomes</b>					
Turkish language	0,012	0,033	0,006	0,002	0,001
Mathematics	0,043	0,135	0,390	0,290	0,338
Sad or Unhappy	0,281	0,187	0,073	0,002	0,009
Anxious or Nervous	0,349	0,272	0,134	0,030	0,057
<b>Table 5: Parental Characteristics</b>					
Mother's Current Employment	0,411	0,665	0,698	0,358	0,590
Log of Monthly Household Income	0,411	0,665	0,698	0,358	0,799
Household Meets Basic Needs	0,618	0,496	0,698	0,964	0,933
Husband's Middle School Completion	0,411	0,665	0,956	0,964	0,933
Husband's Current Employment	0,714	0,665	0,698	0,964	0,897
Women's middle school completion at least equal to husband's	0,411	0,496	0,698	0,358	0,075
Age Difference Between Spouses	0,714	0,665	0,956	0,964	0,799
<b>Table 6. Parental Investment</b>					
Child Owns Literature Books	0,058	0,063	0,030	0,160	0,158
Child Owns Test/Workbooks	0,099	0,264	0,145	0,273	0,158
Child Owns a Computer	0,0001	0,063	0,030	0,047	0,158
Child Nutrition Index	0,548	0,600	0,450	0,986	0,747
Private School Enrollment	0,548	0,267	0,192	0,270	0,158
Child's Weekday Course Hours	0,852	0,063	0,030	0,239	0,158
Child's Weekend Course Hours	0,548	0,063	0,030	0,273	0,339
Weekly Computer Time for Homework	0,293	0,063	0,030	0,160	0,158
Parents Discuss National Issues with Child	0,015	0,061	0,142	0,197	0,116
Parents Discuss School Progress with Child	0,049	0,213	0,316	0,263	0,219
Parents Discuss Books, Movies, or TV Shows with Child	0,049	0,213	0,316	0,359	0,257
Parents Visit Library or Bookstore with Child	0,324	0,912	0,497	0,539	0,694
Child Frequently Socializes with Friends	0,034	0,213	0,142	0,197	0,116
Child Works as Unpaid Family Labor	0,864	0,218	0,083	0,101	0,241
Child Works in Agriculture	0,864	0,950	0,551	0,660	0,869
Child Works in Sales	0,165	0,449	0,551	0,557	0,556
Child Works in Other Jobs	0,864	0,449	0,551	0,359	0,520

Notes. This table reports Benjamin-Hochberg adjusted p-values, which account for multiple hypothesis testing within each outcome family. Outcome families include child educational outcomes (Turkish language and mathematics); mental health outcomes (feeling sad/unhappy, feeling anxious); parental characteristics; parental physical investment outcomes; parental time investment outcomes and children's time allocation outcomes measures. Smaller adjusted p-values indicate stronger evidence against the null hypothesis after controlling the false discovery rate within the relevant family.

Sources: 2022 Turkish Child Survey, TURKSTAT

Table A.19. Impact of Mother's Exposure to the Education Policy on Outcome Index

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth (years)</i>	3	4	5	6	7
<i>Dependent Variables</i>					
Education Index	0.275*** (0.088)	0.199** (0.081)	0.176** (0.073)	0.189*** (0.070)	0.176*** (0.066)
Num. Obs.	1,303	1,678	2,071	2,388	2,687
Mental Health Index	-0.096 (0.102)	-0.103 (0.093)	-0.130 (0.086)	-0.180** (0.082)	-0.152* (0.079)
Num. Obs.	1,351	1,743	2,146	2,477	2,785
Parental Physical Investment	0.142** (0.058)	0.162*** (0.053)	0.165*** (0.046)	0.115** (0.044)	0.100** (0.042)
Num. Obs.	1,357	1,751	2,159	2,491	2,802
Parental Time Investment Index	0.191*** (0.057)	0.116** (0.057)	0.077 (0.052)	0.071 (0.052)	0.080* (0.048)
Num. Obs.	1,357	1,751	2,159	2,491	2,802
Child Labor Index	0.030 (0.058)	-0.035 (0.066)	-0.050 (0.052)	-0.055 (0.046)	-0.032 (0.048)
Num. Obs.	1,357	1,751	2,159	2,491	2,802

Notes: Reform is a binary variable equal to 1 if the individual was born after January 1987, and 0 otherwise. If a mother has more than one child in the age group of 6-12 years, only the youngest child is considered. The dependent variable *index* is constructed by first standardizing each component using the mean and standard deviation of the control group and then averaging the standardized components within each outcome family. The indices are constructed once using the control group within the largest bandwidth (84 months) and are subsequently applied across all bandwidth specifications to keep the scale fixed and ensure comparability of estimates across regressions. The regressions control for split linear time trends on either side of the cutoff, with the running variable being the month-year of birth, birth-month dummies, the number of children, child age, first child age, last child age, dummies for child order and child sex, and child order and child sex interaction. The regressions are weighted using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance \*\*\* at the 1% level, \*\* at the 5% level, \* at the 10% level.

Sources: 2022 Turkish Child Survey, TURKSTAT