Does Compulsory Schooling Reduce Child Labor? Evidence from Turkey

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Abstract

We use a change in compulsory schooling law in 2012 in Turkey to estimate the causal effects of high school attendance on prevalence of child labor. The change in the law implied that individuals born after January 1998 were obliged to complete twelve years of schooling whereas those born earlier had the option of dropping out after eight years. Using this discontinuity, we adopt a Regression Discontinuity (RD) design to estimate causal estimates of school attendance. We find that the reform resulted in a 3.7 percent increase in high school attendance on average, which reduced the likelihood of teenagers to participate in paid employment, particularly in non-agricultural sectors and temporary jobs. We find that these effects work through different channels depending on the degree of tightness in child labor markets and regional levels of poverty. For boys, due to the tighter male labor market, the reform resulted in them having a large reduction in child labor, particularly in industrial sector, without any evidence of a significant effect on their inactivity. For girls, due to the presence of underemployment, the reform resulted in them reducing their inactivity significantly, yielding a smaller tradeoff between school attendance and child labor in services. For both boys and girls, the reform had a larger impact on their high school attendance and employment decisions in regions that were above the country median prior to the reform's implementation. In these poorer regions, the rise in high school attendance resulted in a reduction in paid employment and, to a lesser extent, an increase in unpaid employment. We interpret these findings as evidence that the empowering effects of education reforms may depend on relative tightness of labor markets and differences in levels of poverty across regions.

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1 Introduction

Over the past decade and a half, the number of children in child labor has fallen by one third and the incidence of child labor has declined from 16% to 10% worldwide (ILO, 2013). Despite the progress made, the fact that there are still 264 million child workers, 85 million of whom work in hazardous activities that harm physical and mental well-being, has made the elimination of child labor a priority among other global public policy concerns.[1] As part of this process, many developing countries have put in place regulations to reduce child labor and passed compulsory schooling laws to ensure that every child completes a minimum years of schooling, motivating a large literature examining the tradeoff between the employment of children and their educational attainment.[2] Several empirical studies examine the effects of child labor on schooling outcomes in developing countries,[3] and document a negative correlation between education and child labor.[4] Although a large body of empirical work examines the effects of conditional cash transfers and enrollment subsidies on child labor,[5] previous studies did not exploit an exogenous increase in schooling that has an isolated effect on child labor due to binding time constraints without a simultaneous income effect to examine the effects of school attendance on child labor in a developing-country context.

We study the consequences of an extension of compulsory schooling in Turkey on child labor outcomes. We use a nationally representative survey, the 2014 Household Labor Force Survey, which contains detailed information on education and labor market outcomes. Child labor is a prevalent aspect of the labor market in Turkey. Among the 15–18 year-old teenagers in our sample, 25 percent of boys and 10 percent of girls are employed, and 6 percent of boys and 11 percent of girls are idle, i.e. not in education, employment or training (NEET).[6] In order to isolate the causal effect of school attendance, we implement a regression discontinuity (RD) design, which allows us to estimate a meaningful causal

[1] See ILO (2013) and the United Nations Convention on the Rights of the Child, which was first ratified in 1989 and entered into force in 1990. Despite the significant decline in past decade, child labor continues to be prevalent across the developing world, with Asia having the largest number of child workers (78 million) and Sub-Saharan Africa having the highest incidence (21%) followed by Asia (9.3%), Latin America (8.8%), and Middle East and North Africa (8.4%) (ILO, 2013). Since 1992, ILO has initiated the International Programme on the Elimination of Child Labour (IPEC) with the gradual objective of elimination of child labor through national capacity building.

[2] See Edmonds (2007) and Brown et al. (2001) for surveys of literature on economics of child labor.

[3] These include Orazem and Gunnarsson (2004), Arends-Keunning and Amin (2004), Boozer and Suri (2001), Rosati and Rossi (2003), Ray and Lancaster (2003), Gunnarsson et al. (2006), Edmonds (2007), Edmonds and Shrestha (2012), among others.

[4] These include Edmonds (2007), Assaad et al. (2005), Dayioglu (2005), Goksel (2008), Boozer and Suri (2001), Levison et al. (1998), Levison et al. (2001), among others.

[5] These include Ravallion and Wodon (2000), Cardoso and Souza (2003), Del Carpio and Macours (2010), Guarcello et al. (2010), Gertler et al. (2012), among others. See de Hoop and Rosati (2013) for a literature review of the effects of conditional cash transfers on child labor.

[6] OECD (2014) defines youth inactivity as the share of young people not in employment, education or training (NEET) as a percentage of the total number of young people in the corresponding age group, and argues that "young people who are NEET are at risk of becoming socially excluded, with income below the poverty-line and without the skills to improve their economic situation." It estimates that more than 20% of 15-29 year-olds in Turkey are neither employed nor in education and training, which is one of the highest rates across countries in the OECD.

treatment effect by comparing school attendance and child labor outcomes for younger individuals who were exposed to the reform and older individuals who were not exposed in the context of Turkey. Our empirical strategy follows previous studies that used compulsory schooling laws in Western countries to estimate the returns to education in the labor market (Angrist and Krueger, 1991; Acemoglu and Angrist, 2000; Oreopolous, 2006), in health outcomes (Clark and Royer, 2013; Lleras-Muney, 2005), in fertility behavior (McCrary and Royer, 2011; Black et al., 2008) and other outcomes. We contribute to this growing literature by providing detailed evidence from a majority-Muslim, developing-country context and focusing on the effects of high school attendance on child labor and idleness.

A large body of theoretical work focuses on how child labor is allocated between school attendance, leisure, and work (Cigno and Rosati, 2005; Edmonds, 2007). The relative return to child time in schooling that depends partly on returns to education, returns to formal child labor, returns to child labor in home production, and the direct costs of schooling is one of the key factors that shift allocation of child time between schooling, leisure, and work. One of the main implications of these models is that an increase in school attendance would have a smaller effect on child labor if there is underemployment in child labor market and the child time allocated to leisure is relatively large. Under these conditions, an increase in school attendance would reduce allocation of time to leisure or idleness, without having a large effect on child labor. However, under tight labor market conditions, where leisure time is already restricted and the pool of idle children is small, an increase in time spent on school attendance is likely to reduce time spent on child labor relatively more. We interpret our empirical findings from a natural experiment setting based on these theoretical predictions.

In April 2012, the parliament of Turkey passed a new law to increase compulsory schooling from 8 to 12 years.[7] This law implied that 8^{th} graders in the 2011-12 school year were obliged to complete 12 years of schooling, while individuals who had completed 8^{th} grade earlier could drop out. Given that the school starting age overlaps with the calendar year, we expect individuals born after January 1998 to be more likely to register to high school because they were more likely to be 8^{th} graders in the 2011-12 school year. Hence, the expansion of compulsory schooling in 2012 implied that the individuals born before January 1998 could drop out after 8 years whereas those born after January 1998 were bound to complete 12 years of education. Our identifying assumption is that these two cohorts born one month apart do not display any systematic differences other than being exposed to the compulsory schooling law or not. In our RD design, we assign treatment according to the month and year of birth of the individual, with those that are born after January 1998 assigned to the treated status.

Our RD estimates show that the reform had a positive effect of 3.1 percentage points (ppt) on high

[7] This follows the previous compulsory schooling law passed in 1997, which increased mandatory schooling from 5 to 8 years in Turkey.

school attendance, which corresponds to a 3.7 percent increase relative to the sample mean. We find that the reform affected the allocation of students across program types. While the reform significantly increased the probability of attending a vocational high school by 11 percent (4 ppt), we do not find any evidence that it had a significant effect on academic or distance high school attendance in our full sample. An examination of the impact of the reform on program choice by gender reveals that the reform led to an increase in vocational high school attendance of girls by 13 percent (4.7 ppt) and that of boys by 15 percent (5.8 ppt), respectively. We find no evidence of a significant effect on academic high school attendance.

Second, our RD estimates reveal that this increase in high school attendance induced a decline in paid employment of children aged 15-18. Two years after the reform was implemented, and relative to the sample means, teenagers were 21 percent (2.5 ppt) less likely to engage in paid employment, particularly in non-agricultural sectors (19 percent) and temporary jobs (58 percent). We find that the reform had a negative effect of 19 percent (8.6 ppt) on hours worked in paid employment. We find no evidence that the reform had a significant effect on unpaid employment in our full sample. Unpaid employment primarily consists of unpaid family work in agricultural sector.

Third, we find that the reform had also a negative effect of 20 percent (1.6 ppt) on not being in education, employment, or training in our full sample; however, the result is sensitive to specifications. We also find sensitive results to specifications for child labor in industrial and service sectors. One of the reasons for the sensitivity to the specification appears to be the presence of heterogeneous treatment effects depending on the gender of the respondent. In particular, the differences in the degree of the underemployment of child labor markets and sectoral segregation of these markets by gender result in differences in how teenagers adjust their time allocation between high school attendance, leisure, and work.

Fourth, our findings show that the reform had a sizable negative impact of 31 percent (2.5 ppt) on male child labor in industrial sector, whereas we find no evidence of a significant effect on female child labor in industrial sector. In contrast, we find that the reform had a negative effect of 45 percent (1.8 ppt) on female child labor in services, whereas there is no evidence of a significant effect on male child labor in services. The fuzzy RD estimates show that a one ppt increase in high school attendance induced by the reform reduces male paid employment in non-agricultural sector by 0.92 ppt, hence an almost one-to-one tradeoff between high school attendance and male child labor in this sector. This substitution effect declines to 0.70 ppt for male child labor in industrial sector, and it is an imprecisely estimated 0.56 ppt for female child labor in services. Our results also show that the reform had a negative effect of 26 percent (2.9 ppt) on the NEET status of girls, and we find no evidence of a significant impact for boys. The fuzzy RD estimates show that a one ppt increase in high school attendance induced by

the reform reduces the NEET status of girls by 0.88 ppt, which is a large substitution effect.

Altogether our results indicate significant negative effects of high school attendance on child labor and idleness outcomes. An examination of heterogeneous effects by gender reveal diverging effects of the reform depending on the tightness of the specific child labor market and sectoral composition of child employment. Since female idleness (11 percent) is almost twice as much as male idleness (6 percent) and female child labor (10 percent) is less than half of male child labor (25 percent), the male child labor market is tighter relative to the female one. We interpret our findings based on the predictions of theoretical work on child labor markets. Our finding of a larger tradeoff between high school attendance and child labor in male child labor markets confirms the theoretical prediction that in tighter markets, the effect of school attendance on child labor would be larger. Similarly, our finding of a large negative impact on female idleness due to higher school attendance and no corresponding effect on male idleness provides empirical support for the theoretical prediction that in relatively underemployed labor markets, the effect of school attendance on idleness would be larger.

We examine whether the reform had heterogeneous effects depending on the regional poverty rates before the reform was implemented. Our results show that the reform had a larger impact on increasing high school attendance in poorer regions. This increase in school attendance induced a substantial decline in paid employment in these regions due to the rigidity of time required in paid employment. At the same time, this led to an increase in unpaid employment, which is more flexible and can be combined with schooling more easily. The overall effect was a decline in total employment in poorer regions in response to higher school attendance induced by the reform.

Our study contributes to the literature on the effects of education on child labor. Several studies report a negative correlation between education and child labor. Edmonds (2007) documents using data from middle-income countries that school attendance rates are lowest among children who perform market work. Assaad et al. (2005) find that Egyptian girls have lower school attendance than boys that is associated with a large burden of domestic work that they perform. Dayioglu (2005) and Goksel (2008) find a negative association between child labor and school attendance in Turkey, and using probit models, they document a negative association between compulsory schooling and child labor.[8] The evidence that relies on such correlations is likely to suffer from omitted variable bias given that unobservables such as socioeconomic status, ability and upbringing might affect both school attendance and child labor. A related body of research uses exogenous variation from conditional cash transfers (CCTs) that parents receive in return for sending their children to school to identify the effects of schooling on child labor. Although several studies document that CCTs reduce the extensive and

[8]Boozer and Suri (2001) document a strong negative correlation between child labor and schooling in Ghana; Levison et al. (1998) show the same for Peru; and Levison et al. (2001) show it for Mexico.

intensive margin of child labor (de Hoop and Rosati, 2013; Del Carpio and Macours, 2010), the evidence is mixed. In Brazil, Cardoso and Souza (2003) find no evidence that the CCTs had a significant effect on child labor since they were too small to reduce the incentives to forgo child labor income. Other studies examined whether changes in the price of schooling might affect child labor. Ravallion and Wodon (2000) find that an enrollment subsidy in Bangladesh increased schooling much more than it reduced child labor, suggesting that increased schooling comes in part from reduced idleness.[9] We contribute to this literature by examining the effect of an exogenous increase on school attendance due to a compulsory schooling reform on child labor and idleness. An advantage of our empirical setting is that the reform does not have an additional income effect that is generally present in exogenous variations coming from CCTs or enrollment subsidies, which allows us to measure the isolated effect of time spent on school attendance on the incidence and time allocated to child labor.

Our work relates to a broader literature studying the empowering effects of education. Gulesci and Meyersson (2015) find that the 1997 compulsory schooling law in Turkey had a secularizing effect by reducing the level of women's religiosity, a positive effect on decision rights over marriage, and a positive effect as on household consumption. Cesur and Mocan (2014) find that the same law had a negative effect on women's propensity to identify themselves as religious, wear a religious head cover, and cast a vote for Islamic parties. This relates to the broader literature on the effects of education on non-pecuniary outcomes, including political participation, health outcomes, and criminal activity (Oreopolous and Salvanes, 2011; Lochner, 2011). Our study contributes to this literature by examining the effects of the 2012 compulsory schooling reform in Turkey on different types of high school attendance, and child labor outcomes. Our results on the reduction of child labor among boys and girls and inactivity among girls provide support for empowering effects of education.

Our work also relates to the large literature on causal channels relating child labor, schooling, and trade policy. Boozer and Suri (2001) use rainfall patterns in Ghana to estimate the effect of child labor on schooling, and find that an hour of child labor reduces school attendance by 0.38 hours. Arends-Keunning and Amin (2004) find that the decline in hours worked among the Food for Education Program (FFE) participants is similar in magnitude to the increase in time spent in school in Bangladesh. Using panel data from Vietnam, Beegle et al. (2005) find that each additional hour of work is associated with a nearly 3 ppt decline in the probability of attending school. A related body of work focuses on the effects of international trade on child labor. Edmonds and Pavcnik (2005) use regional and intertemporal variation in the real price of rice in Vietnam to estimate the effect of international trade on child labor,

[9]Related studies used the availability and accessibility of schools as an indicator of schooling costs, finding mixed evidence on the effects of school accessibility on child labor. Siddiqui and Patrinos (1995) and Bhalotra and Tzannatos (2003) find that distance to school tends to increase child labor, while Grootaert and Patrinos (1999) and Kondylis and Manacorda (2012) find no evidence that school proximity reduces child labor.

and find that higher rice prices lead to lower child labor due to a positive income effect. Edmonds et al. (2010) show that the tariff cuts in India in 1991 had a negative effect on schooling and a positive effect on child labor among communities that relied on employment in protected industries prior to liberalization. Our study contributes to this literature by examining another channel – an increase in compulsory schooling – that could affect the incidence of child labor and its heterogeneity by gender and levels of poverty in a developing-country context.

The rest of the paper is organized as follows: Section 2 provides a description of the context by presenting an overview of the twelve-year compulsory schooling policy. Section 3 presents the data sources and empirical strategy. Section 4 presents the results, and Section 5 concludes.

2 Overview of the Twelve-Year Compulsory Schooling Policy in Turkey

In April 2012, The Grand National Assembly of Turkey passed Law No. 6287, which stipulated an extension of compulsory schooling from eight to twelve years. This law came to be known as 4+4+4 given the three four-year length components: primary school, junior high school, and high school. The diploma that had been awarded at the end of eighth grade was abolished, replacing it with one for completing the twelfth grade successfully. The option to attend religious junior high schools was consequently reinstated. An additional option to attend distance education programs after eighth grade was included. Prior to this law, this option was not offered to those who were still under the coverage of compulsory years of schooling.

The legislative process of Law No. 6287 brought many challenges and faced substantive criticism. The bill was proposed by five members of the parliament from Justice and Development Party (JDP) as a legislative proposal rather than a government bill, which would have required an evaluation from the Department of Finance and the Court of Accounts regarding its cost-benefit assessment and feasibility. Critics argued that one of the reasons for why it was proposed as a legislative proposal was that the Department of Finance was not likely to approve it given the large costs of financing it.[10] Similarly, several think tanks and NGOs in Turkey raised concerns regarding the shortage of resources allocated to investments in education, which could possibly hinder an effective implementation of the proposed change.[11] Therefore, the introduction of the option of distance education after completing junior high school was primarily considered as a way of reducing the excessive burden on formal high school institutions. [12] The combination of government's ambitious expansion targets for non-academic

[10] http://www.milliyet.com.tr/4-4-4-gerilimi/siyaset/siyasetdetay/24.02.2012/1507022/default.htm.

[11] http://www.tepav.org.tr/tr/haberler/s/2765; http://erg.sabanciuniv.edu/tr/node/981.

[12] http://www.gazetevatan.com/zor-tercih--561250-gundem/

programs,[13] and substantial resource shortages in schooling investments raised concerns about the overall quality and access to education, as students from disadvantaged backgrounds were likely to end up in under-resourced learning environments.[14]

Throughout the legislative process the focus of tension between the ruling pro-Islamist party of JDP and the main opposition party of RPP has been the reopening of religious junior high schools. The political conflict on the provision of religious education dates back to Turkey's former education reform. In 1997, the military decided to intervene with a set of decisions to prevent the spread of Islamist movement in Turkey. These decisions were announced by the National Security Council (NSC) on February 28, 1997, and came to be known as the 1997 military memorandum, as they paved the way for the resignation of the leader of the Islamist party at the time and the end of his coalition government. Among these decisions were the extension of compulsory schooling from five to eight years to be provided only in secular schools and the removal of the option of attending a religious junior high schools as these schools were shut down.[15]

During the 1996-1997 school year, prior to 1997 reform, religious junior high schools attracted more than 12 percent of junior high school students.[16] In 2002, when the JDP came to power, only 3 percent of eligible children attended religious high schools.[17] A decade later, when the Law No. 6287 passed in 30 March 2012, the Prime Minister's speech emphasized that the primary objective of the new law was to undo the effects of the 1997 reform by revitalizing religious junior high schools and expanding religious high school program.[18]

Despite the political hurdles, the 2012 Compulsory Schooling Law was successful in substantially raising new enrollments of students to high school. The upper graph in Figure 1 shows a significant increase in total number of new registrations to high schools after the 2011-2012 school year when the compulsory schooling law began to be implemented.[19] Three years after the policy change, new registrations to high schools were 19% higher for girls (increasing from roughly 488,000 in 2011 to 582,000 in 2014) and 13% higher for boys (increasing from roughly 551,000 in 2011 to 627,000 in 2014). The upper right graph in Figure 1 shows that the transition rates – the ratio of the number of new

[13]Ministry of National Education's Strategic Plan 2010-2014 targets the share of vocational programs to reach 50%; http://sgb.meb.gov.tr/Str_yon_planlama_V2/MEBStratejikPlan.pdf.

[14]http://www.tepav.org.tr/upload/files/haber/1333525190-3.Guven_Sak___Yeni_kanun_teklifi_neden_yeterli_degildir.pdf

[15]A large empirical literature examines the effects of the 1997 compulsory schooling law in Turkey on various outcomes(Kirdar et al., 2009, 2011, 2014; Cesur et al., 2014; Cesur and Mocan, 2014; Mocan, 2014; Aydemir and Kirdar, 2015; Dincer et al., 2014; Güneş, 2015; Gulesci and Meyersson, 2015).

[16]See National Education Statistics Formal Education 1996-1997.

[17]See National Education Statistics Formal Education 2001-2002.

[18]http://www.hurriyet.com.tr/gundem/20242073.asp

[19]Data comes from the official statistical yearbooks of Ministry of National Education: National Education Statistics Formal Education 2008-2009, 2009-2010, 2010-2011, 2011-2012, 2012-2013, 2013-2014 and 2014-2015.

registrations to high schools to the number of graduates from primary education in a given year – have risen steadily between 2011 and 2014 for girls from 84% to 94% and for boys from 86% to 95%. These trends highlight that the expansion of compulsory schooling has been accompanied by a substantial increase in access to high school education.

3 Data and Empirical Methodology

3.1 Data

We use data from Turkey's 2014 Household Labor Force Survey (HLFS) 2014, which is a nationally representative household survey including detailed information on education and labor market outcomes. The survey, conducted with 140,718 households in 2014, covers data on socioeconomic indicators of households regarding demographics, education, and employment. In particular, the survey includes information on school attendance, type of school attended, grade, NEET status, employment status, and number of hours worked. The survey distinguishes between vocational high schools, which are designed to train students for a particular trade or profession, and academic high schools, which are designed to prepare students for university education. In this classification, religious high schools are included under vocational high schools since these schools train students to work at a religious institution as an imam or other related occupations. The survey does not have separate information on religious high schools. In addition, the survey does not include data on domestic work, including household chores, while it includes detailed information on market work, including paid and unpaid employment in services, industry, and agricultural sectors.

Table 1 presents the summary statistics on major indicators for teenagers from the 2014 HLFS of Turkey. We provide summary statistics for teenagers of ages born 20 months before and after the cutoff date of January 1998 because the estimated bandwidths in our local regression analyses fall into this range. Hence, we focus on 15-18 year-old teenagers. Panel A indicates that roughly 84 percent of teenagers attended high school, among which 38 percent attended vocational high school while 46 percent attended academic high school. On average, high school attendance rate for girls (86 percent) was higher than that of boys (83 percent). The average rate of vocational high school attendance was lower for girls (36 percent) than boys (39 percent), while the academic high school attendance was higher for girls (49 percent) than boys (43 percent). The last column shows that these differences in estimates for boys and girls are statistically significant.

In Panel B of Table 1, we report descriptive statistics for child labor outcomes. Roughly 12 percent of 15-18 year-old teenagers engaged in paid employment and 6 percent of them participated in unpaid employment, corresponding to an 18 percent total employment rate. On average 4 percent of girls work

in services and 2 percent of them work in industry, while boys participate in each sector with an equal share of 8 percent. 6 percent of boys and 3 percent of girls participated in temporary paid employment, including seasonal jobs. When combined with unpaid work, the average total employment rate for boys (25 percent) was more than twice the average for girls (10 percent). In contrast, the average of not being in education, employment, or training for girls (11 percent) was almost twice the average for boys (6 percent). Hence, male child labor market was relatively tighter than female child labor market, which is not surprising considering that there are ample opportunities of both physical and service-related work for boys whereas young girls employment possibilities are much more restricted in Turkey.

The descriptive statistics on hours of work reveals that boys perform about three times as much labor as girls. In total employment, the average hours of work per week performed by boys (11.6 hours) was almost three times as much as that by girls (3.9 hours). If we restrict the sample to those that are employed, average hours of work was 46.7 hours for boys and 39.3 hours for girls. The corresponding hours of work for paid employment was less: 35.5 hours for boys and 27.2 hours for girls. The last column shows that the difference in estimates for girls and boys are statistically significant.

Panel C of Table 1 reports summary statistics on predetermined characteristics of teenagers aged 15-18 in our sample. Roughly 97 percent of the mothers and 92 percent of the fathers were present in the household. The number of school age boys (girls) present in the household was on average 1.12 (1.10). The number of 0 to 5 year-old children present in the household was 0.16 boys and 0.16 girls on average. The average household size is 4.2. While households of females are slightly larger than that of males, the difference is not statistically significant.

Table A1 provides additional descriptive statistics on 15–17 year-old teenagers based on data from the 2012 Child Labor Force Survey. This survey has information on household chores, but lacks information on month of birth and does not exist for any years after 2012. Roughly 64 percent of girls in this age interval perform household chores while only 14 percent of boys do so. Among children who are not in education, employment or training, 87 percent of girls and 39 percent of boys perform household chores. In terms of hours of work per week, girls work more than three times as much as boys in domestic work. For this reason, the children who fall under the category of not in education, employment, and training should not be considered purely idle. It is clear from these statistics that a large majority of them, particularly girls, perform domestic work.

3.2 Identification

The 2012 Compulsory Schooling Law and the law on school starting age jointly implied that individuals born after January 1998 were obliged to complete 12 years of schooling while those born earlier could stop schooling after 8 years, as explained in Section 2. We use this cutoff point in a Regression Discontinuity

(RD) design to estimate the causal effect of school attendance on child labor outcomes. Our identifying assumption is that these two cohorts born one month apart do not exhibit any systematic differences other than being exposed to the compulsory schooling law or not. Assuming that this assumption holds, this setting provides an as-good-as-random assignment of treatment. In our RD design, we assign treatment according to the month and year of birth of the individual, with those that are born after January 1998 assigned to the treated status.

We use an RD design by exploiting the discontinuity at the birth date and using this discontinuity as an instrument for school attendance by following previous research (Clark and Royer, 2013; Oreopolous, 2006; Gulesci and Meyersson, 2015). We provide both the reduced-form estimates (i.e. sharp RD), and the two-stage least squares estimates (i.e. fuzzy RD) for child labor and idleness (i.e. NEET) outcomes. Our specification follows a basic RD form:

$$y_i = \alpha + \beta t_i + f(x_i) + \epsilon_i \tag{1}$$

$$\forall x_i \in (c-h, c+h)$$

where y_i is the dependent variable,t_i is the treatment status, x_i is the forcing variable, and h is the bandwidth around the cutoff point c. The control function, $f(x_i)$, is a continuous n-order polynomial function of the forcing variable on each side of the cutoff point. We use local linear regressions in our RD estimations (Imbens and Lemiuex, 2008), and adopt the optimal bandwidth selection using the Imbens and Kalyanaraman (2009) routine. This implies the selection of an optimal bandwidth for each outcome variable examined. In addition, we use specifications that adopt the optimal bandwidth from the first stage results for high school attendance, which is estimated at 20 months around the discontinuity for second-stage results and sub-group analysis. This 'static' or 'constant' bandwidth approach complements the former results where we use the optimal bandwidth. We cluster standard errors at the month-year of birth level. We include the following control variables in our specifications: a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age and gender present in the household with the age categories of 0-5, 6-17, 18-44, 45-64, 65-above, month-of-birth fixed effects, and region fixed effects.[20]

3.3 Preliminary Checks

We present a standard validity check for the RD design (Imbens and Lemiuex, 2008). In Figure 2, we examine whether the predetermined characteristics that we control in later regressions are continuous

[20]We use fixed effects for 12 regions, including Istanbul, West Marmara, Aegean, East Marmara, West Anatolia, Mediterranean, Central Anatolia, West Black Sea, East Black Sea, Northeast Anatolia, Central East Anatolia, and Southeast Anatolia.

at the discontinuity. Each graph represents local averages of the outcome in one-month bins, plotted against the forcing variable, with overlaid smoothed linear regression lines using raw data on each side of the cutoff. The gray lines represent 95 percent confidence intervals. The pre-determined characteristics that we plot are a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, and counts of the number of household members by age and gender present in the household with the age categories of 0-5, 6-17, 18-44, 45-64, 65-above. The graphs do not indicate any significant jumps at the cutoff point. Overall, we conclude that the pre-determined covariates appear balanced around the threshold.

4 Effects of the 2012 Compulsory Schooling Law

4.1 Education Outcomes

4.1.1 Education Outcomes: Full Sample

We begin with a graphical illustration of the RD design in Figure 3, where local averages of education outcomes (upper three graphs) and child labor and inactivity outcomes (lower three graphs) in 15–18 age cohort are plotted against the respondent's month-and-year of birth where the cutoff is January 1998 in monthly bins. The education reform required that those born after this date completed twelve years of schooling, while the older cohorts had the option of dropping out after completion of eighth grade. Each graph displays a local linear smoother overlaid using raw data on each side of the cutoff, with gray lines showing 95 percent confidence intervals. The cutoff point is indicated by the vertical dashed line at zero.

The upper left graph of Figure 3 shows a positive jump in high school attendance of 15–18 year-old teenagers at the discontinuity of less than 5 percentage points. The upper middle graph shows a positive jump in vocational high school attendance while the upper right graph shows no evidence of a significant jump in academic high school at the cutoff point. Overall, these graphs point out that the reform had a significant positive impact on high school attendance, which was primarily driven by a substantial increase in vocational high school attendance without any evidence of a significant change in academic high school attendance.

While Figure 3 reveals a positive RD treatment effect of compulsory schooling law on school attendance, the results could further be refined with regression analysis. Table 2 reports the RD treatment effects on high school attendance of high schools for 15–18 year-old teenagers using the 2014 HLFS data. In each row, column 6 reports outcome means for the relevant sample, and column 7 displays the optimal bandwidth estimated by the Imbens and Kalyanaraman algorithm in months. We include

controls for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age and gender present in the household with the age categories of 0-5, 6-17, 18-44, 45-64, 65-above, month-of-birth fixed effects, and region fixed effects.

Row 1 of Table 2 reports the estimates for RD treatment effects on high school attendance. The optimal bandwidth, calculated using the Imbens and Kalyanaraman (2009) algorithm, results in a bandwidth of 20 months around the discontinuity. Using a local linear specification, column 2 presents an RD estimate of 3.1 percentage points treatment effect on high school attendance, and is statistically significant at 1 percent level. In terms of magnitude, a 3.1 percentage point increase in high school attendance corresponds to a 3.7 percent increase relative to the sample mean. For robustness, we include alternative specifications with a quadratic control function in column 2, and allow the bandwidth to vary by reporting linear RD estimates with a three quarters of the optimal bandwidth in column 3, one and a half bandwidth in column 4, and a maximum symmetric bandwidth of 23 months around the cutoff in columns 5, respectively. With the quadratic control function, the RD estimate increase to 5.1 percentage points and remains significant. With the three quarters of the optimal bandwidth, the estimate is 2.4 percentage points; with the one and a half bandwidth, it is 3.6 percentage points; and with the maximum symmetric bandwidth, it is 3.7 percentage points, which are all precisely estimated. To sum up, taking the linear RD estimate with optimal bandwidth as reference, the education reform had a positive effect on high school attendance of roughly 3.1 percentage points. This RD estimate is robust to alternative functional forms and bandwidths used. The point estimate implies that the fuzzy RD estimates in the two-stage least squares specification will be larger than the sharp RD estimates for child labor outcomes. In our results, we will report both of these estimates for comparison.

The remaining rows of Table 2 present the RD treatment effects on attending different program types. The second row displays the estimated RD treatment effects for the outcome variable of vocational high school attendance. Column 1, using the local linear specification, reports a significant RD estimate of 4 percentage points, corresponding to a 11 percent increase relative to the sample mean. In alternative specifications listed in the remaining columns, the RD estimates remain largely significant, ranging from 4.9 percentage points to 5.4 percentage points. The third row of Table 2 presents the RD estimates of the reform's effect on academic high school attendance. Column 1 reports an insignificant RD estimate of -1.5 percentage points. In alternative specifications, the point estimates are insignificant and smaller ranging from -0.1 to -1.1 percentage points. Overall, we find that the reform had a large impact on attending vocational high school (11 percent), and there is no evidence that the reform had a significant impact on attending academic high school based on 2014 HLFS data.

4.1.2 Education Outcomes: Heterogeneous Effects by Gender

Figure 4 examines graphically whether the reform had heterogeneous effects on educational outcomes by gender. In Panel A, the two graphs show a positive jump in high school attendance for both girls and boys at the discontinuity of less than 5 percentage points. The middle graphs in panel B show a positive jump in vocational high school attendance for girls and boys at the discontinuity. We observe that the jump is particularly large for boys. The bottom graphs in panel C illustrate no evidence of a jump in female academic high school attendance and some evidence of a negative jump in male academic high school attendance.

In order to refine the RD results with regression analysis, Table 3 reports the RD treatment effects on educational outcomes by gender. In Panel A, we use the 2014 HLFS data, which includes the month-year-of-birth information and excludes the distance high school information. In Panel B, we use the 2013 HLFS data, which has year of birth information and distance high school information. In Panel A, we use a bandwidth of 20 months around the discontinuity in all regressions, which is the optimal bandwidth estimated by the Imbens and Kalyanaraman (2009) algorithm when school attendance is the independent variable, in order to make the regression results comparable across outcomes and relevant samples within the constant bandwidth. Column 1 reports the linear RD results for the full sample for ease of comparison with subsamples. Columns 2 and 3 report RD estimates for the subsample of girls and boys, respectively. The results in the first row show that the reform had a positive effect of roughly 3.3 and 3.5 percentage points on the likelihood of high school attendance of girls and boys, respectively. Column 4 shows that the difference between the estimates for females and males is not statistically significant. The results in the second row indicate that the reform had a positive impact of 4.7 percentage points (13 percent relative to the sample mean) and 5.8 percentage points (15 percent relative to the sample mean) on the vocational high school attendance of girls and boys, respectively. Even though the estimates are slightly higher for boys, column 4 indicates that the difference between the two subsamples is not statistically significant. Finally, the RD estimates in the third row of Table 3 show no evidence of a significant effect of the reform on academic high school attendance for boys or girls.

4.2 Child Labor Outcomes

4.2.1 Child Labor Outcomes: Full Sample

We illustrate the RD treatment effects first graphically in the bottom graphs of Figure 3, where child labor and inactivity outcomes in 15–18 age cohort are plotted against the respondent's month-and-year of birth where the cutoff is January 1998 in monthly bins. Each graph displays a local linear smoother

overlaid using raw data on each side of the cutoff, with gray lines showing 95 percent confidence intervals. The cutoff point is indicated by the vertical dashed line at zero.

The lower left and lower middle graphs of Figure 3 show negative jumps in participation in paid employment and log hours of work per week in paid employment by 15–18 year-old teenagers at the discontinuity. The lower right graph also shows a negative jump in being not in education, employment, or training (NEET) at the cutoff point. While these lower graphs in Figure 4 illustrate negative RD treatment effects for child labor and inactivity, we explore these effects further with a regression analysis in Table 4.

Table 4 reports the RD treatment effects of the reform for the full sample of 15–18 year-olds on child labor outcomes. We use a bandwidth of 20 months around the discontinuity in all regressions, which is the optimal bandwidth estimated by the Imbens and Kalyanaraman (2009) algorithm when high school attendance is the independent variable in order to make the regression results comparable across outcomes and relevant samples. Column 1 reports OLS regression results with high school attendance. The correlations show that high school attendance is negatively associated with participating in paid employment, unpaid employment, hours worked in any type of employment, not being in education, employment, or training, and monthly earnings from paid employment.

The linear RD estimate in column 2 of the first row in Table 4 shows that the reform had a marginally significant and negative effect of 2.5 percentage points on participating in paid employment, corresponding to an 21 percent decline relative to the sample mean. In alternative specifications of columns 3-5, the estimate remains highly significant. The significant linear RD-2SLS estimate in column 7 indicates that the 3.1 percentage point increase in high school attendance induced by the reform had a negative effect of 2.5 (0.031 x -0.795) percentage point decline in paid employment. This fuzzy RD estimate implies a trade-off that is rather high, but less than a one-to-one effect, where a one percentage point increase in school attendance generates 0.80 percentage point decline in paid employment on average. The subsequent rows of Table 4 show that the reform had a larger impact on paid employment in non-agricultural sectors compared to agricultural sector. In particular, the reform had a negative effect of 2.3 percentage points on paid employment in temporary jobs, which corresponds to a 58 percent decline relative to the mean. This suggests that teenagers who complied with the education reform quit temporary or seasonal jobs substantially in response to the time constraints that they faced.

We find no consistent evidence of a significant impact on participating in unpaid employment. The only significant specification is a linear RD estimate with 0.75 bandwidth, while all of the alternative estimates are insignificant. The coefficients are positive, indicating an imprecisely estimated positive impact on unpaid employment, which takes place primarily as family work in the agricultural sector.

The change in paid employment seems to be mainly driven by changes in the extensive margin

(participation), rather than the intensive margin (hours). The linear RD treatment effect on log hours of paid work is a negative 8.6 percentage points, corresponding to a 19 percent decline relative to the mean. The magnitude of the treatment effect for hours of paid work is slightly less than the participation, which is 21 percent decline relative to the mean. If the adjustment had taken place through the intensive margin where teenagers worked less hours but stayed employed, there would have been a smaller decline in participation relative to hours of paid employment. Instead, we find evidence for a greater decline in participation than hours worked, which implies that most of the decline in hours came from teenagers leaving the labor market, implying that the core channel of adjustment was the extensive margin.

Our results for the effects of the reform on monthly earnings from paid employment indicate an imprecisely estimated and negative effect of 11.3 percentage points, corresponding to a decline of 18 percent relative to the sample mean. Since most of the adjustment in paid employment occurred in the extensive margin, this slightly smaller decline in earnings relative to the decline in participation suggests that the reform had a slightly larger impact for those at the lower part of the earning distribution.

Finally, we examine whether the reform had a significant impact on being idle, or not being in education, employment or training (NEET). The linear RD estimate in column 2 shows a significant treatment effect of 1.6 percentage points, indicating a 20 percent decline relative to the mean. While the fuzzy RD estimate in column 7 is significant at 5 percent level, some of the alternative specifications are imprecisely estimated. This imprecision in estimates can also be observed in some of the sectoral estimates of paid employment including services and industrial sectors. One of the reasons for the sensitivity of the results to control functions and bandwidths used could be the presence of heterogeneous treatment effects by gender due to the gendered segregation of teenage labor markets in Turkey.

In particular, we anticipate the reform to have heterogeneous effects because of differences in constraints facing the use of child labor in Turkey. Since the use of male child labor is more socially acceptable and qualify for non-agricultural jobs that require physical strength, the gender division of child labor is concentrated on boys doing the majority of market work. On the other hand, not being in school or employed is a more commonly observed situation for teenage girls given the constraints facing their human capital accumulation and their market work in socially conservative areas. Hence, to the extent that the compulsory schooling law changes the allocation of time among teenagers between school, work, and leisure, we expect that it generates heterogeneous outcomes by gender.

4.2.2 Child Labor Outcomes: Heterogeneous Effects by Gender

Figure 5 provides a graphical illustration of whether the reform's effects on child labor outcomes differed by gender. In Panel A, the left graph shows a negative jump in female paid employment in services while the right graph indicates no evidence of a significant jump in male paid employment in services.

In contrast, in Panel B, the left graph does not show any evidence of a significant jump for female paid employment in industry while the right graph indicates a negative jump for male paid employment in industry. In Panel C, the left graph does not show any evidence of a significant jump in unpaid employment for females or males. Finally, in Panel D, the left graph shows a significant negative jump in paid temporary employment for both females and males. For more refined analysis, we proceed with the RD regression results in Table 5.

Table 5 tests whether the effects of the reform on child labor outcomes differ by gender. We present the sharp RD estimates (i.e. the reduced-form specification) and the fuzzy RD estimates (i.e. the two-stage least squares specification using treatment as an instrument for high school attendance) for the overall sample and subsamples of girls and boys. In all specifications, we use a bandwidth of 20 months around the cutoff point, which is the optimal bandwidth estimated by the Imbens and Kalyanaraman (2009) algorithm when high school attendance is the dependent variable. In the first row, both the sharp and fuzzy RD specifications in columns 5 and 6 show significant and negative treatment effects on male paid employment. While the corresponding effects in columns 3 and 4 are also negative for females, they are imprecisely estimated. The magnitude of the fuzzy RD estimate in column 6 implies a one-to-one trade-off between school attendance and paid employment for boys. The RD treatment effects for boys are particularly large for non-agricultural employment, implying a one percentage point increase in high school attendance induced by the reform reduces male paid employment by 0.92 percentage points. For girls, the linear RD estimate in column 3 shows a negative treatment effect of 1.8 percentage points on paid employment in services; however, the fuzzy RD estimate in column 4 is imprecisely estimated. The magnitudes of the effects correspond to a 45 percent decline in female paid employment in services and a 31 percent decline in male paid employment in industry. Column 7 shows that the differences between estimates for the two samples is statistically significant only for paid employment in industry.

Although the proportions of industrial and service sector employment for boys are equal (8 percent), we find that the reform had a larger and precisely impact on industrial sector employment. One implication is that boys are much less likely to quit their jobs if they are employed in services in response to the increased time constraints due to school attendance. One of the reasons for the asymmetry between the responses of male child labor in industry and services could be that the working hours might be more flexible in service sector jobs, such as hairdressers, in comparison to industrial sector jobs, such as auto mechanics.

Our results indicate that the reform had a negative effect of 2.6 and 1.8 percentage points on paid employment in temporary jobs for females and males respectively, without any evidence of a statistically significant difference between them. In contrast, we find no evidence of a statistically significant effect on unpaid employment for boys or girls. One of the explanations could be that teenagers do not have

much decision-making power when it comes to unpaid family work. It could also be that such work has more flexible working hours compared to non-agricultural sector jobs.

A comparison of the RD treatment effects for hours of work by gender shows that the overall effects are driven by the reduction in hours of work by boys. The change in male paid employment is largely driven by changes in the extensive margin instead of the intensive margin. While the reform led to a 21 percent decline in their participation, it induced a 20 percent decline in their hours of work. Given the similar magnitudes of decline in estimates, the reduction in hours of work appears to be driven by boys quiting their jobs instead of working for fewer hours. For girls, we only find a statistically significant effect for hours worked in services and temporary jobs.

The reform-induced changes in paid employment had a negative impact on monthly earnings of boys. The RD estimate in column 5 of the last second row shows that the reform had a negative impact of 19.7 percentage points on the log monthly earnings of boys. This corresponds to 22 percent relative to the mean, which is slightly above the reform's effect on participation in male paid employment (21 percent). This suggests that those who quit their jobs were earning slightly above the average.

Finally, we test whether the reform had heterogeneous effects on having NEET status by gender. Figure 6 shows a negative jump for girls while the right graph indicates no evidence of a significant jump for boys. In Table 5, the RD estimates in columns 3 and 4 show that the reform had a significant and negative effect of 2.9 percentage points on girls not being in school or employed, corresponding to 26 percent relative to the mean. The precisely estimated fuzzy RD estimate implies a large trade-off between female high school attendance and not being in school or employed: a one percentage point increase in high school attendance induced by the reform results in a 0.88 percentage point decline in having NEET status. This fuzzy RD estimate is larger than the fuzzy RD estimates for female paid employment (0.52), which is consistent with having more excess supply of children in female labor market compared to the male labor market. The tightness of the male child labor market results in a relatively larger decline in male child labor in response to increased time constraints from high school attendance. In contrast, in female child labor market with excess labor supplies, part of the adjustment takes place through a reduction of excess inactive females.

4.3 Heterogeneous Effects by Regional Poverty Levels

Given that the RD treatment effect identifies only a local treatment effect at the specific cutoff value based on date of birth, the average treatment effect may differ from the RD treatment effect to the extent that the subsample around the cutoff differs from the whole sample. Moreover, it is equally important to consider whether the estimated local effect has economic importance.

Previous studies have emphasized several reasons for why children might go to school less and work

more in poorer settings (Edmonds and Shrestha, 2012; Beegle et al., 2005). At lower levels of income, parents have a lower preference for schooling. As income increases, parents demand a higher level of schooling, which reduces paid employment due to rigidity of time required in paid work, holding wages constant. As children allocate more time into schooling away from paid work, unpaid work within the household may increase to that extent that it is more flexible. In models of child labor, it is also common to assume that children work in households in which subsistence income is otherwise not attained Basu and Van (1998). Liquidity constraints may also force poor parents to choose a lower level of schooling than optimal for their children given the returns to schooling and its opportunity costs (Baland and Robinson, 2000; Edmonds, 2006). At lower levels of income, households may have less access to food, nutrition, school materials, and transportation, which could reduce the relative return to time allocated to schooling. The level of income may also affect productivity within the household and therefore the allocation of children's time to household work (Edmonds and Pavcnik, 2005; Basu and Dutta, 2010).

We consider whether the effect of the compulsory schooling law in Turkey varies with the levels of regional poverty prior to the reform's implementation. To the extent that children in poorer regions might have gone to school less and work more in poorer regions prior to the reform, we would expect the compulsory schooling law to have a larger impact on children from poorer areas. While this may provide limited inference for wealthier and higher-educated regions, it is nonetheless of significant importance for poorer communities where children face highest barriers to access human capital accumulation.

In particular, we would expect that poor regions experience larger treatment effects of the education reform on high school attendance and child labor. We investigate this possible heterogeneity in Table 6. In order to allow heterogeneous RD effects of the reform on school attendance and child labor, we identify the pre-reform levels of regional poverty using the regional poverty rates in 2011 provided by the Turkish Statistical Institute. A comparison of outcome means for above- and below-median averages in the last rows of each panel shows that average high school attendance is lower in regions that are above the median level of poverty. This holds for both girls and boys. We should thus expect to find larger effects for the regions with above-median poverty rates. We again use a bandwidth of 20 months around the cutoff, the baseline bandwidth for heterogeneous effects by gender on school attendance and child labor, in order to make the analysis of median samples more relevant for the RD design.

The results in Table 6 show that the RD treatment effect on high school attendance is larger in poorer regions. In above-median poverty regions, the reform induced a 4.9 percentage point increase in high school attendance, corresponding to a 6 percent increase relative to the mean. In contrast, below-poverty regions experienced an imprecisely estimated 1.5 percentage point increase in high school attendance, implying a mere 1.7 percent increase relative to the mean. This applies to both boys and girls, but it is more precisely estimated for boys. The positive RD treatment effect on vocational

high school attendance is much larger in poorer regions and precisely estimated also for girls. For the academic high school attendance, the RD treatment effects are negative in poorer regions, and in case of boys, precisely estimated. This suggests that the reform led to an increase in demand for vocational high schools while lowering the demand for academic high schools in poorer regions, which could be driven by the desire of these children to acquire job-specific skills rather than training for university exams.

This increase in high school attendance had significant consequences for employment of children. The results in Table 6 show a negative RD treatment effect on paid employment of 4.7 percentage points in poorer regions (39 percent relative to the mean), while the corresponding effect in more affluent regions is null. The difference in estimates between the two subsamples is statistically significant. The negative RD treatment effects in poorer regions on paid employment are 3.3 percentage points for girls and 6.3 percentage points for boys. However, the difference in estimates between above- and below-median poverty regions is significant only for boys.

The decline in paid employment was accompanied by an increase in unpaid employment in poorer regions. The RD estimates in Table 6 show that the reform led to a 2.3 percentage point increase in unpaid employment in poorer regions, corresponding to a 33 percent increase relative to the mean. This effect was particularly strong for boys, whose unpaid employment increased by 3.2 percentage points (36 percent relative to the mean). The difference in estimates between above- and below-median poverty regions is significant for boys and the overall sample. The RD estimates for total employment are all negative for poorer regions, however, they are imprecisely estimated. For the above-median poverty regions, the RD treatment effect is a 2.3 percentage points, or 12 percent relative to the mean, decline in total employment.

In poorer regions, the decline in paid employment had a negative impact on monthly earnings of children. The RD estimates imply a decline of 24 percentage points, or 40 percent relative to the mean, in monthly earnings in poorer areas. The difference in estimates between subsamples is significant. This effect is present for both girls and boys, and even though the point estimates are larger for boys, the relative magnitude is larger for girls. Finally, we find the the reform had a larger impact on not being in school or employed in poorer regions. However, the differences are not as precisely estimated as those for participating in paid or unpaid employment. The effects for girls are larger in both regions compared to boys, consistent with the lower opportunity cost of sending inactive girls to school.

All in all, this serves to show that the reform had more pronounced effects on high school attendance and child labor in regions where arguably the barriers to accessing education were higher. The presence of similar heterogeneous effects for girls and boys suggests that parents from poor households respond similarly to budget and time constraints that they face regardless of the gender of their children.

5 Conclusion

The main purpose of this paper is to provide evidence on the effect of a change in compulsory schooling that exogenously increased high school attendance on the prevalence of child labor and idleness in Turkey. Using an RD design allows us to estimate causal effects of the education reform on various forms of child labor. While previous studies examined basic correlations between schooling decisions and child labor, such correlations are likely to suffer from omitted variable bias as unobservables such as socioeconomic status, ability, and upbringing might affect both schooling and work decisions. Other studies that utilized exogenous variation from conditional cash transfers (CCTs) and enrollment subsidies to identify the effects of schooling on child labor have difficulty in isolating the positive income effect coming from transfers and subsidies from the binding constraints on children's time coming from school attendance. The estimates of the effects of these programs are often sensitive to the relative amount of subsidy/transfer to returns on child labor. An advantage of our empirical setting is that the compulsory schooling reform does not entail a positive income effect. The key contribution of this paper is to assess the isolated effect of time spent on school attendance on the incidence and time allocated to child labor in a developing-country context with high prevalence of child labor.

We find that the reform led to an increase in high school attendance and a decline in participation of teenagers in paid employment. Teenagers in the treated cohorts are less likely to work for a wage, particularly in the non-agricultural sector and in jobs that are temporary in duration. Moreover, high school attendance generates differential effects on child labor by gender depending on the differences in the degree of the tightness of child labor markets and their sectoral segregation. In relatively tighter male child labor market, the effect of school attendance on child labor is larger, generating a bigger tradeoff between school attendance of boys and their child labor, particularly in the industrial sector. In relatively underemployed female child labor market, the effect of school attendance on idleness is larger, pulling inactive females into school, resulting in a smaller tradeoff between school attendance of girls and their child labor, particularly in the service sector. In contrast, we find no evidence that the reform had a significant effect on unpaid employment, where the teenagers mostly perform unpaid family work with little autonomy regarding their labor market decisions. More flexible working hours is likely to be another reason for why the time constraints from school attendance are not binding in case of the unpaid work.

Our findings suggest that the reform led to a larger increase in high school attendance in regions where poverty rates prior to the reform were lower than the country median. The reform had a larger impact on high school attendance in these regions, which led to large declines in paid employment and monthly earnings from such employment. This led to an increase in unpaid employment to a lesser

extent due to the flexibility of unpaid work. The overall effect was a decline in total employment in poorer regions. Our results show that the main compliers with the reform were those teenagers living in above-median poverty regions of Turkey. One of the policy implications of this finding is that any future policy aimed at reducing child labor needs to focus on regional development policies targeted to reducing poverty and enhancing local sustainable development.

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Appendix A List of Variables

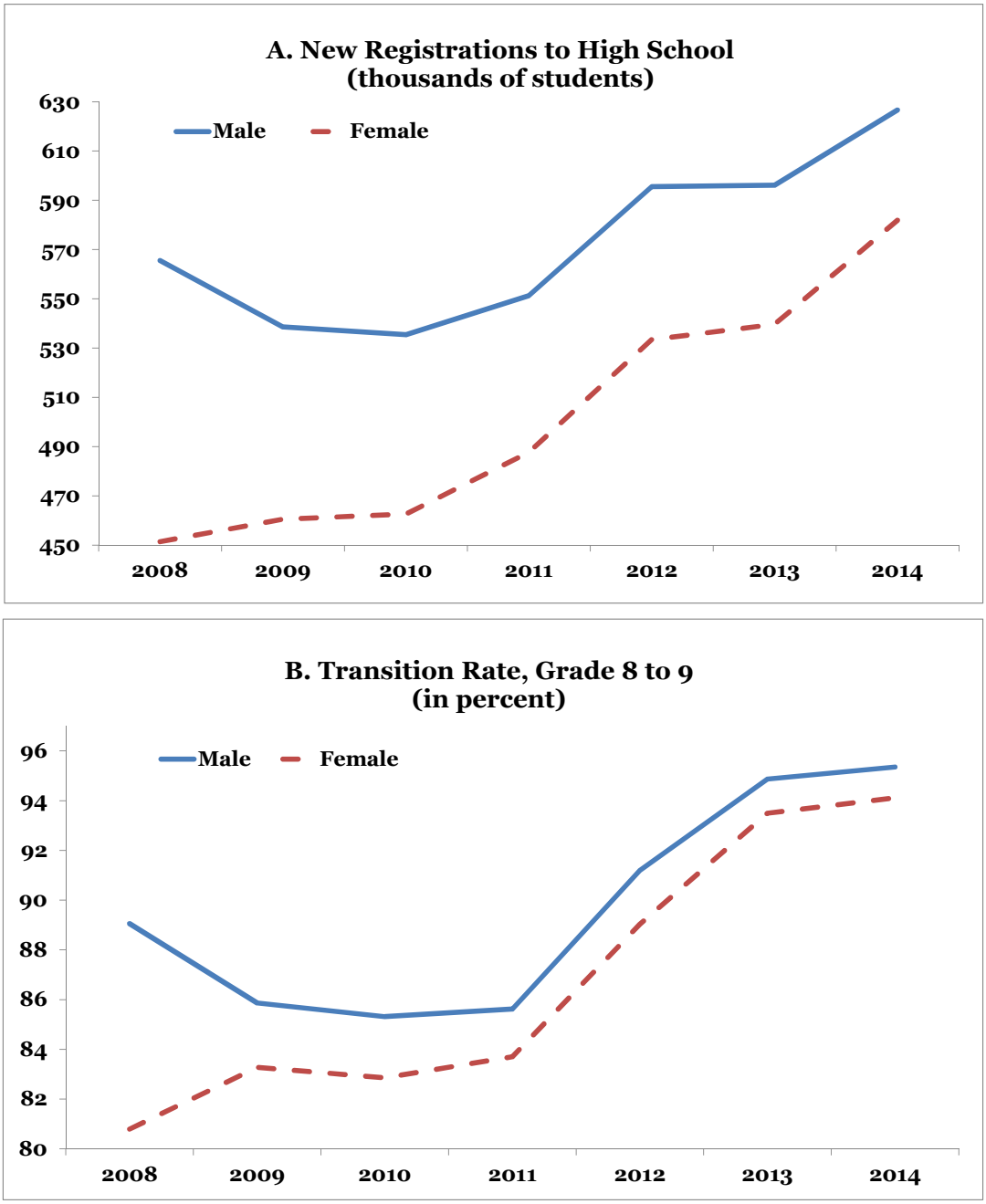
Outcome Variables:

- High School Attendance: A dummy variable equal to one if the respondent attends high school.
- Vocational High School Attendance: A dummy variable equal to one if the respondent attends vocational high school.
- Academic High School Attendance: A dummy variable equal to one if the respondent attends academic high school.
- Paid Employment: A dummy variable equal to one if the respondent participates in paid employment, which includes working for a wage or a salary.
- Paid Employment in Non-agriculture: A dummy variable equal to one if the respondent participates in paid employment in non-agricultural sectors, including industry and services.
- Paid Employment in Industry: A dummy variable equal to one if the respondent participates in paid employment in industrial sector.
- Paid Employment in Services: A dummy variable equal to one if the respondent participates in paid employment in service sector.
- Unpaid Employment: A dummy variable equal to one if the respondent works in unpaid family work.
- Paid Temporary Employment: A dummy variable equal to one if the respondent participates in paid employment in a temporary or seasonal job.
- Total Employment: A dummy variable equal to one if the respondent participates in paid or unpaid employment.
- Log Hours of Work: The log of hours of work performed in the last week, which takes the value of zero if the respondent does not work. We measure this variable for any of the employment categories listed above.
- Log Monthly Earnings from Paid Employment: The log of monthly earnings from paid employment, which takes the value of zero if the respondent does not work.
- Not in Education, Employment, or Training (NEET): A dummy variable equal to one if the respondent is not currently in education, employment, or training.

Covariates:

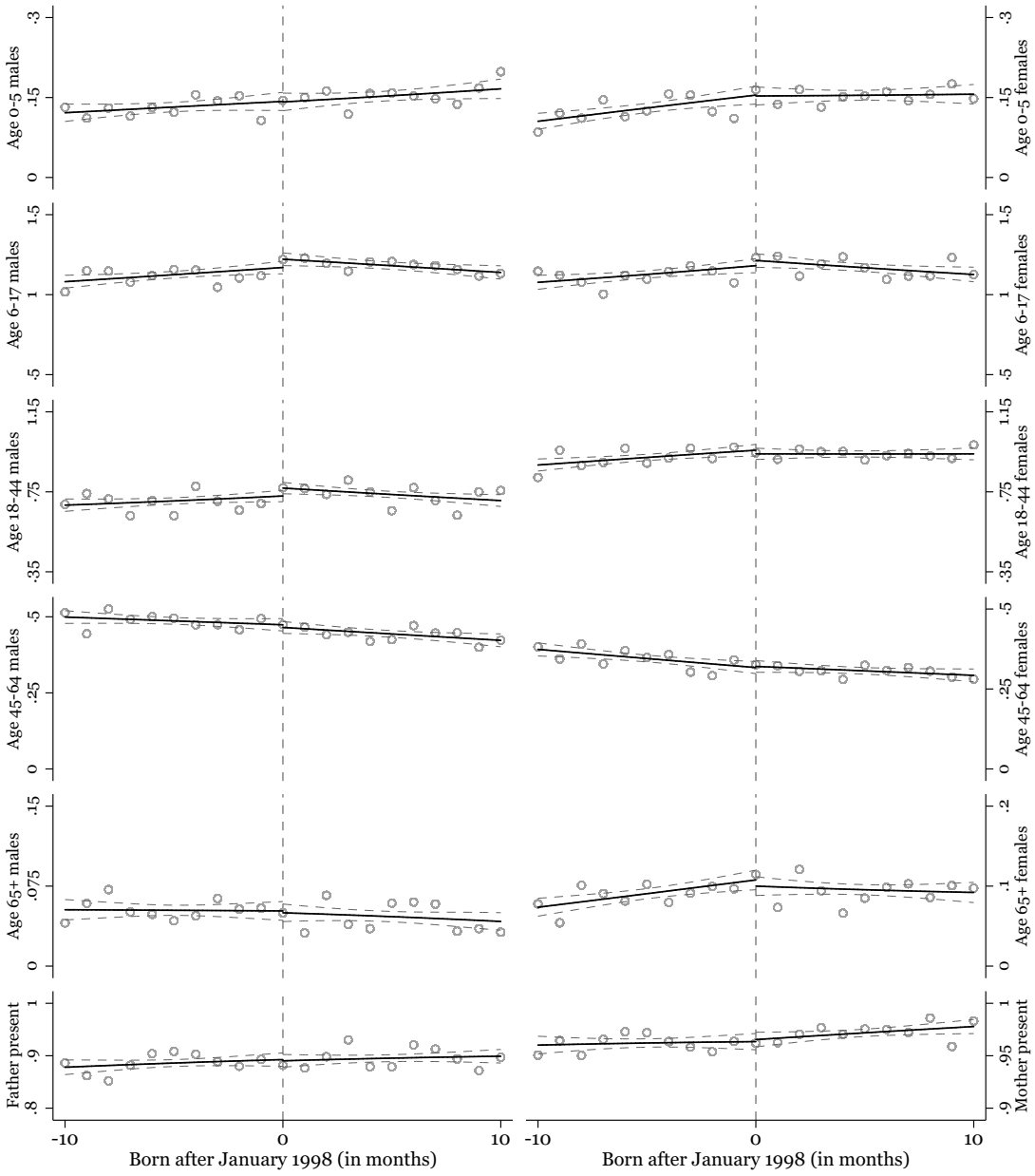
- Mother present: A dummy variable equal to one if the respondent's mother is present in the household.
- Father present: A dummy variable equal to one if the respondent's father is present in the household.
- Number of school age boys (girls) present: The number of school age (6-17 year-old) boys (girls) present in the household.
- Number of boys (girls) 0 to 5 present: The number of boys (girls) aged 0 to 5 present in the household.
- Household size: The number of individuals living in the same household with the respondent.
- Region dummies: Dummy variables for each of the twelve regions where the respondents were located.

Figure 1: Trends in High School Enrollment in Turkey



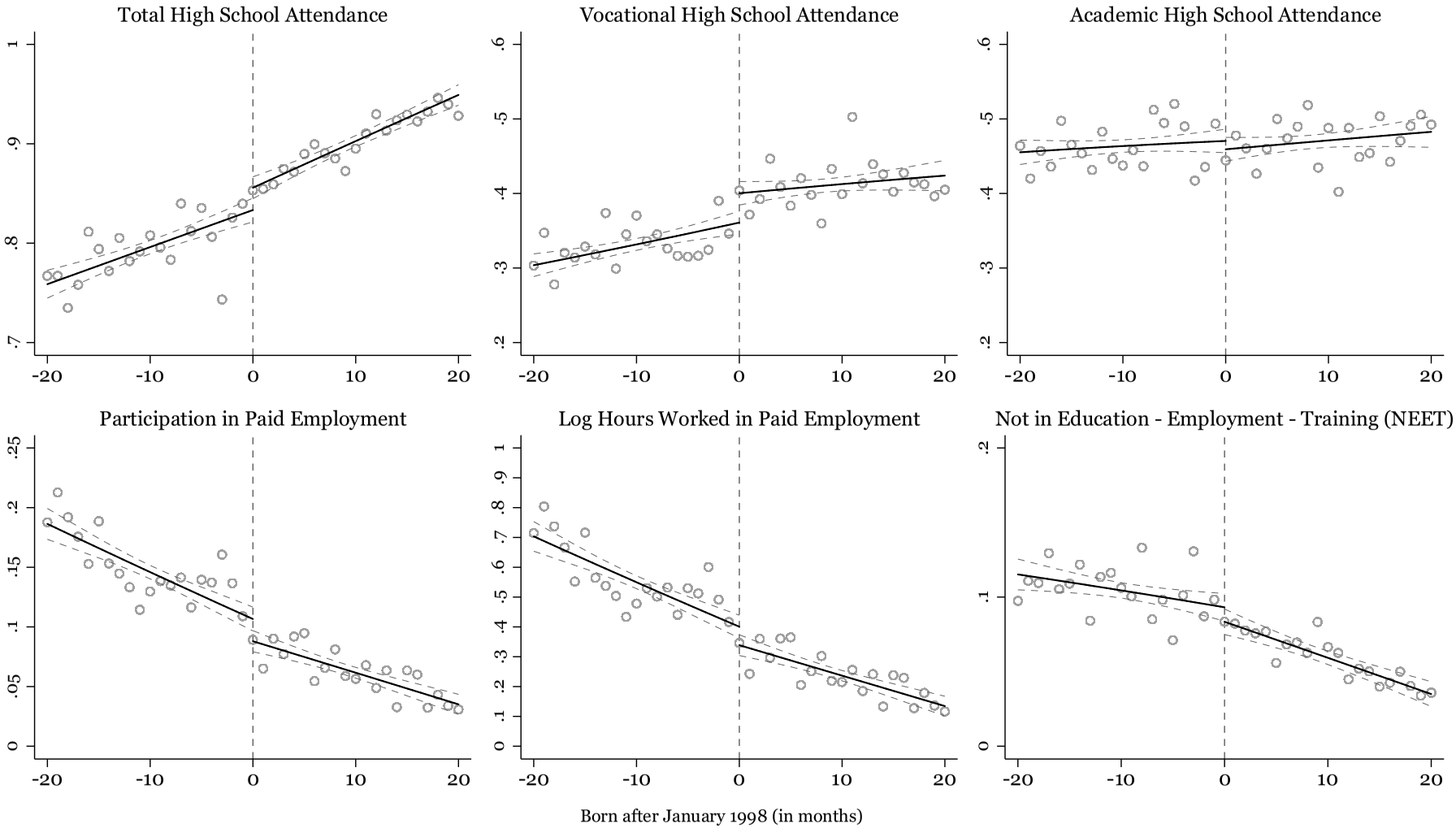
Note: Data is from the National Education Statistics Formal Education 2008-2009, 2009-2010, 2010-2011, 2011-2012, 2012-2013, 2013-2014 and 2014-2015.

Figure 2: Balanced Covariates



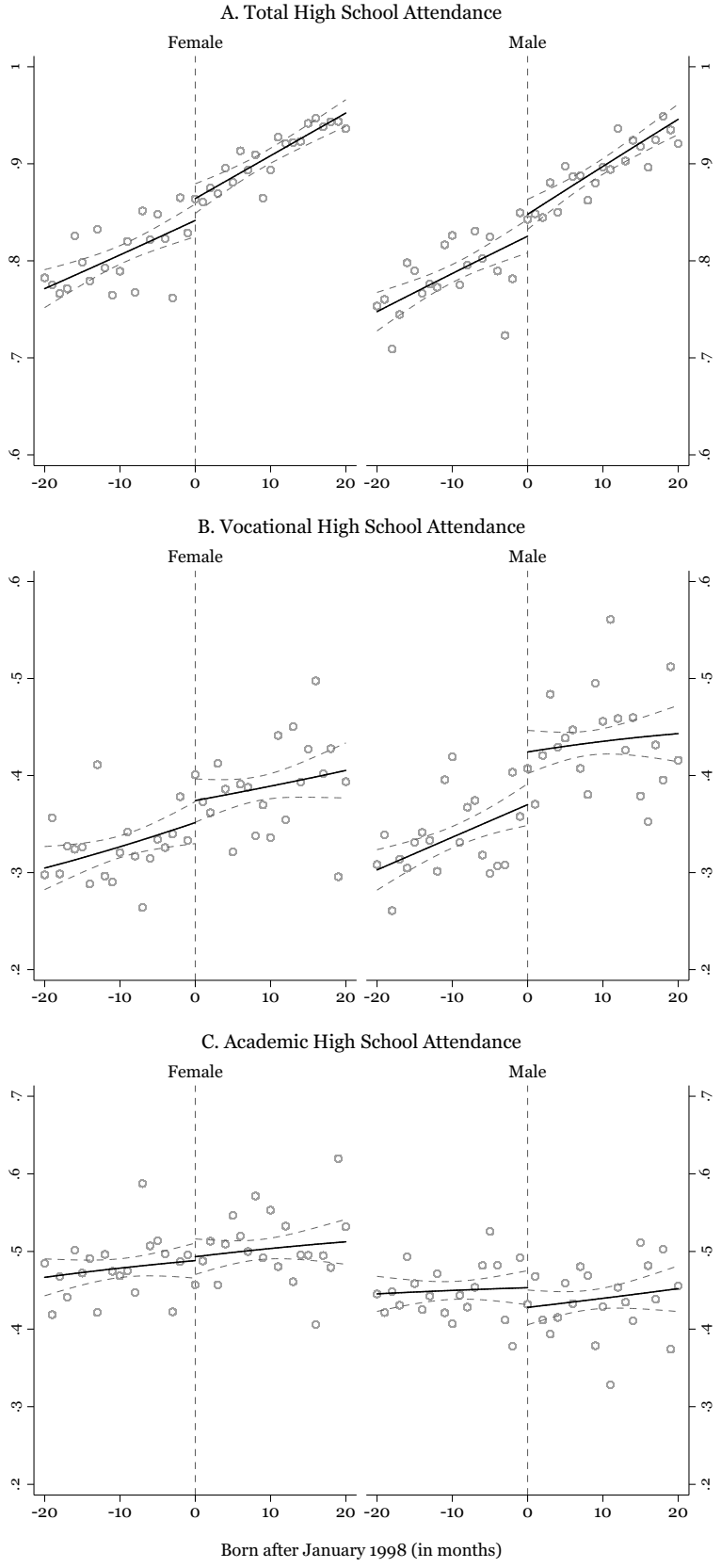
Note: Data is from the 2014 Household Labor Force Survey of Turkey. Figures plot pre-determined covariates in monthly bins against the month-year-of-birth of being born in January 1998. The vertical line in each graph represents the cut-off point, January 1998. Gray lines show 95 percent confidence intervals around the mean level. Variable definitions are listed in Appendix A.

Figure 3: RD Treatment Effects: Education and Child Labor Outcomes



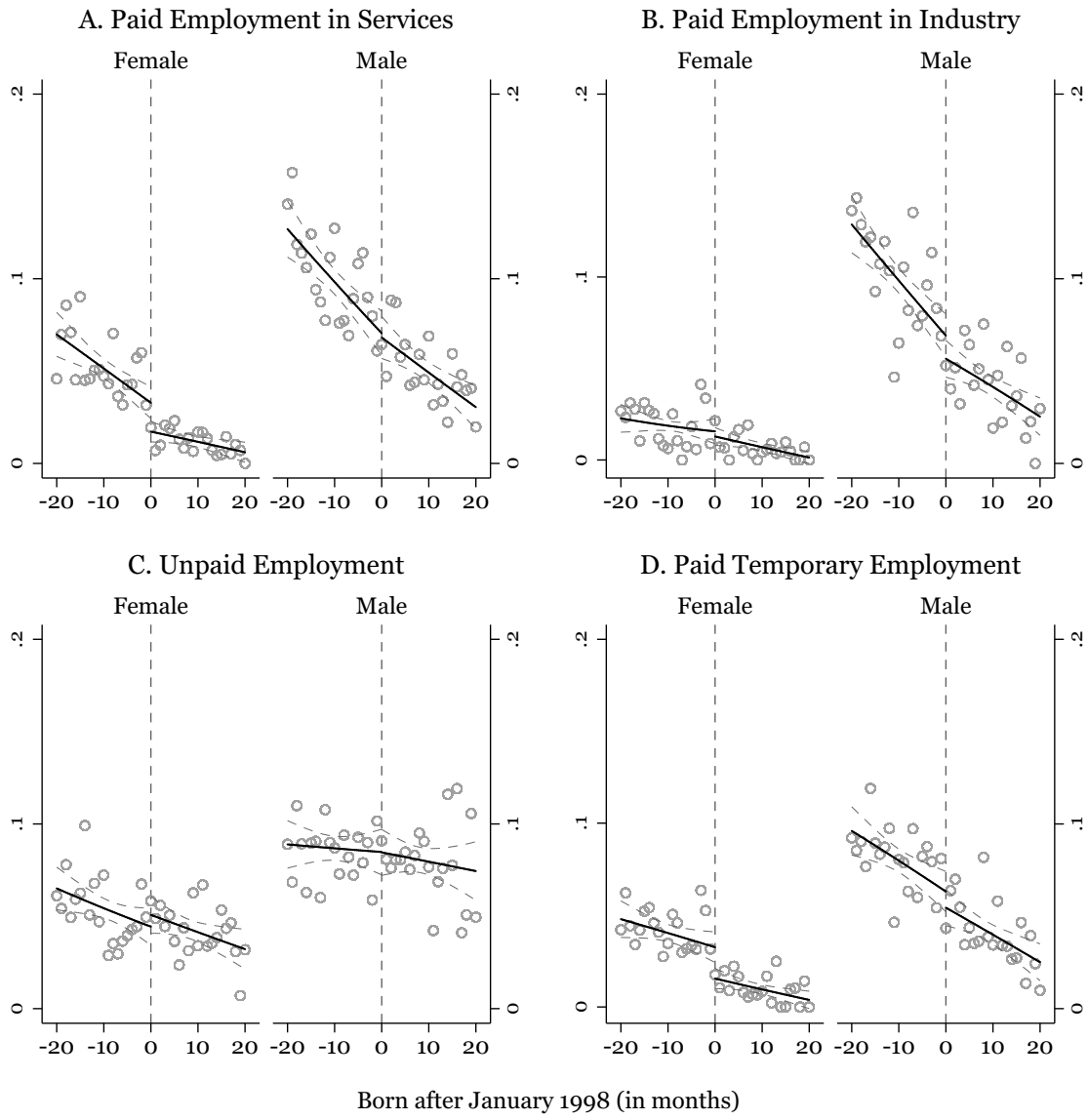
Note: Data is from the 2014 Household Labor Force Survey of Turkey. Figures plot high school attendance in total, vocational high school attendance, academic high school attendance, participation in paid employment, log hours of work per week in paid employment, and not being in education, employment, or training (NEET) in monthly bins against the month-year-of-birth of being born in January 1998. The vertical line in each graph represents the cut-off point, January 1998. Gray lines show 95 percent confidence intervals around the mean level.

FIGURE 4: RD TREATMENT EFFECTS: EDUCATION OUTCOMES BY GENDER



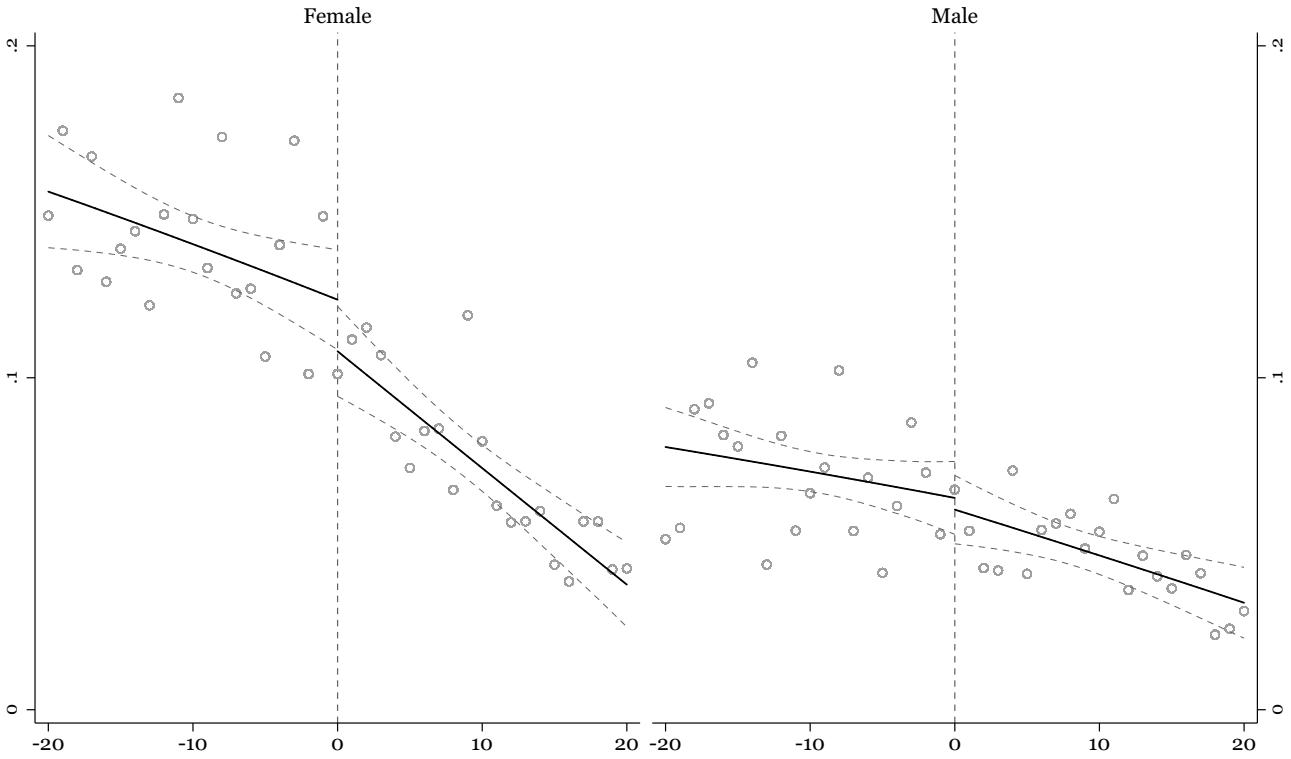
Note: Data is from the 2014 Household Labor Force Survey of Turkey. Figures plot high school attendance in total, vocational high school attendance, academic high school attendance in monthly bins against the month-year-of-birth of being born in January 1998. The vertical line in each graph represents the cut-off point, January 1998. Gray lines show 95 percent confidence intervals around the mean level.

FIGURE 5: RD TREATMENT EFFECTS: CHILD LABOR OUTCOMES BY GENDER



Note: Data is from the 2014 Household Labor Force Survey of Turkey. Figures plot participation in paid employment in services, participation in paid employment in industry, participation in unpaid employment, and participation in paid temporary employment in monthly bins against the month-year-of-birth of being born in January 1998. The vertical line in each graph represents the cut-off point, January 1998. Gray lines show 95 percent confidence intervals around the mean level.

Figure 6: RD Treatment Effects: Not in Education, Employment, or Training (NEET)



Note: Data is from the 2014 Household Labor Force Survey of Turkey. Figures plot propensity of not being in education, employment, or training (NEET) in monthly bins against the month-year-of-birth of being born in January 1998. The vertical line in each graph represents the cut-off point, January 1998. Gray lines show 95 percent confidence intervals around the mean level.

TABLE 1: SUMMARY STATISTICS OF 15-18 YEAR-OLD CHILDREN

	Overall sample		Female sample		Male sample		Difference
Panel A: Education							
	Mean	SD	Mean	SD	Mean	SD	Est. (SE)
High School Attendance:							
Total	0.84	0.36	0.86	0.35	0.83	0.38	0.03 (0.01)***
Vocational	0.38	0.48	0.36	0.48	0.39	0.49	-0.03 (0.01)***
Academic	0.46	0.50	0.49	0.50	0.43	0.50	0.06 (0.01)***
Panel B: Child Labor Outcomes							
	Overall sample		**Female sample**		**Male sample**		**Difference**
	Mean	SD	Mean	SD	Mean	SD	Est. (SE)
Participation in the last week:							
Paid employment	0.12	0.32	0.06	0.24	0.17	0.38	-0.11 (0.01)***
Non-agriculture	0.11	0.31	0.05	0.23	0.16	0.37	-0.11 (0.00)***
Industry	0.05	0.22	0.02	0.13	0.08	0.28	-0.06 (0.00)***
Services	0.06	0.24	0.04	0.19	0.08	0.27	-0.04 (0.00)***
Agriculture	0.01	0.09	0.01	0.08	0.01	0.10	0.00 (0.00)***
Paid temporary employment	0.04	0.21	0.03	0.16	0.06	0.24	-0.03 (0.00)***
Unpaid employment	0.06	0.23	0.04	0.19	0.07	0.26	-0.03 (0.00)***
Total employment	0.18	0.38	0.10	0.30	0.25	0.43	-0.15 (0.01)***
Log hours worked in the last week (including 0s):							
Paid employment	0.45	1.24	0.22	0.88	0.66	1.46	-0.44 (0.02)***
Non-agriculture	0.42	1.20	0.20	0.84	0.62	1.43	-0.42 (0.02)***
Industry	0.20	0.86	0.07	0.51	0.32	1.07	-0.25 (0.01)***
Services	0.22	0.89	0.13	0.68	0.30	1.04	-0.17 (0.01)***
Agriculture	0.03	0.35	0.02	0.29	0.04	0.39	-0.02 (0.01)***
Paid temporary employment	0.16	0.74	0.09	0.56	0.22	0.87	-0.12 (0.01)***
Unpaid employment	0.18	0.78	0.12	0.62	0.24	0.89	-0.12 (0.01)***
Total employment	0.64	1.41	0.34	1.06	0.92	1.63	-0.57 (0.02)***
Log monthly earnings from paid employment (including 0s)	0.62	1.87	0.30	1.33	0.91	2.22	-0.61 (0.03)***
Not in Education, Employment, or Training (NEET)	0.08	0.28	0.11	0.31	0.06	0.24	0.04 (0.00)***
Panel C: Individual and Household Characteristics							
	Overall sample		**Female sample**		**Male sample**		**Difference**
	Mean	SD	Mean	SD	Mean	SD	Est. (SE)
Mother present	0.97	0.18	0.96	0.19	0.98	0.15	-0.01 (0.01)***
Father present	0.92	0.28	0.90	0.30	0.93	0.26	-0.03 (0.00)***
Number of school age boys present	1.12	1.00	0.68	0.89	1.55	0.91	-0.87 (0.01)***
Number of school age girls present	1.10	1.09	1.66	1.03	0.57	0.86	1.09 (0.01)***
Number of boys 0 to 5 present	0.16	0.43	0.17	0.44	0.15	0.43	0.02 (0.01)**
Number of girls 0 to 5 present	0.16	0.45	0.17	0.47	0.15	0.42	0.03 (0.01)***
Household size	4.22	1.43	4.23	1.44	4.21	1.41	0.01 (0.03)
Observations	23,809		11,521		12,288		

Notes: The table presents the mean, standard deviation, and number of observations from the 2014 Household Labor Force Survey of Turkey. The sample includes teenagers who are born within 20 months before or after January 1998. The difference estimates slightly differ from simple differences due to rounding error. The variables are described in Appendix A.

TABLE 2: RD TREATMENT EFFECTS ON EDUCATION

Outcome	(1) Linear RD $\hat{h}$ bandwidth	(2) Quadratic RD $\hat{h}$ bandwidth	(3) Linear RD 0.75 $\hat{h}$ bandwidth	(4) Linear RD 1.5 $\hat{h}$ bandwidth	(5) Linear RD h = 23 months	(6) Mean	(7) Bandwidth ($\hat{h}$)	(8) Observations
High school attendance:								
Total	0.031**	0.052**	0.025**	0.037***	0.036***	0.84	20	23,809
	(0.012)	(0.024)	(0.011)	(0.009)	(0.011)			
Vocational	0.040***	0.049***	0.049***	0.056***	0.050***	0.37	26	27,737
	(0.014)	(0.017)	(0.014)	(0.011)	(0.014)			
Academic	-0.015	-0.000	-0.006	-0.021*	-0.007	0.46	28	29,171
	(0.012)	(0.020)	(0.014)	(0.012)	(0.014)			

Notes: Data is from the 2014 Household Labor Force Survey of Turkey. Each column reports a reduced-form RD treatment effect of being born after January 1998 with a linear or quadratic control function in month-year-of-birth on each side of the discontinuity. Columns (1) and (2) report local RD regressions with a linear and quadratic control function using optimal bandwidth $\hat{h}$, respectively. Columns (3) and (4) report local RD regressions with a linear control function using optimal bandwidth 0.75 $\hat{h}$ and 1.5 $\hat{h}$, respectively. Column (5) reports local linear RD regressions with a bandwidth of 23 months, which is the maximum symmetric bandwidth around the cutoff. Column (7) reports the optimal bandwidth estimated by the Imbens and Kalyanaraman (2009) algorithm. Column (6) reports the outcome mean within the optimal bandwidth, and column (10) reports the number of observations used in the estimations. The dependent variables are a dummy variable equal to one if the respondent attends high school, a dummy variable equal to one if the respondent attends vocational high school, a dummy variable equal to one if the respondent attends academic high school. All specifications control for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age and gender present in the household with the age categories of 0-5, 6-17, 18-44, 45-64, 65-above, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

TABLE 3: RD TREATMENT EFFECTS ON EDUCATION BY GENDER

	Overall	**Female**	**Male**	**Difference**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Linear RD	Linear RD	Linear RD	(2)-(3)	Mean	Bandwidth ($\hat{h}$)	Observations
Outcome	$\hat{h}$ bandwidth	$\hat{h}$ bandwidth	$\hat{h}$ bandwidth	p-value	(Overall/Female/Male)		(Overall/Female/Male)
High school attendance:							
Total	0.031**	0.033**	0.035**	0.90	0.84/0.86/0.83	20	23,809/11,521/12,288
	(0.012)	(0.016)	(0.013)				
Vocational	0.049***	0.047**	0.058**	0.75	0.38/0.36/0.39	20	23,809/11,521/12,288
	(0.014)	(0.019)	(0.022)				
Academic	-0.009	-0.005	-0.014	0.80	0.46/0.49/0.43	20	23,809/11,521/12,288
	(0.015)	(0.024)	(0.021)				

Notes: Data is from the 2014 Household Labor Force Survey. Each column reports a reduced-form RD treatment effect of being born after January 1998 with a linear control function in month-year-of-birth on each side of the discontinuity. The bandwidth is 20 months in all regressions, which is the optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman (2009) algorithm when high school attendance is the dependent variable. Column (1) reports results for the whole sample, column (2) reports them for the subsample of males, and column (3) reports them for the subsample of females. Column (4) reports the p-value for the SUR test of equality between treatment effects for subsamples of males and females reported in columns (2) and (3). The dependent variables are a dummy variable equal to one if the respondent attends high school, a dummy variable equal to one if the respondent attends vocational high school, a dummy variable equal to one if the respondent attends academic high school, and a dummy variable equal to one if the respondent attends distance high school. All specifications control for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age and gender present in the household with the age categories of 0-5, 6-17, 18-44, 45-64, 65-above, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level in Panel A, and year cohort level in Panel B. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

TABLE 4: RD TREATMENT EFFECT OF EDUCATION ON CHILD LABOR OUTCOMES

Outcome / statistics	(1) OLS $\hat{h}$ bandwidth	(2) Linear RD $\hat{h}$ bandwidth	(3) Quadratic RD $\hat{h}$ bandwidth	(4) Linear RD 0.75 $\hat{h}$ bandwidth	(5) Linear RD 1.5 $\hat{h}$ bandwidth	(6) Linear RD h = 23 months	(7) Linear RD-2SLS $\hat{h}$ bandwidth	(8) Mean
Participation in the last week:								
Paid employment	-0.259***	-0.025**	-0.078***	-0.045***	-0.024**	-0.022*	-0.795**	0.12
	(0.010)	(0.011)	(0.015)	(0.008)	(0.010)	(0.011)	(0.357)	
Paid employment in non-agriculture	-0.240***	-0.021*	-0.072***	-0.036***	-0.021**	-0.017	-0.662**	0.11
	(0.011)	(0.011)	(0.016)	(0.008)	(0.009)	(0.011)	(0.306)	
Paid employment in industry	-0.150***	-0.008	-0.043***	-0.018**	-0.007	-0.007	-0.245	0.05
	(0.011)	(0.009)	(0.014)	(0.009)	(0.008)	(0.008)	(0.238)	
Paid employment in services	-0.090***	-0.013	-0.029**	-0.018**	-0.014**	-0.010	-0.418	0.06
	(0.009)	(0.008)	(0.011)	(0.007)	(0.006)	(0.008)	(0.276)	
Paid employment in agriculture	-0.020***	-0.004	-0.006	-0.009***	-0.003	-0.004*	-0.133	0.01
	(0.004)	(0.003)	(0.005)	(0.002)	(0.002)	(0.002)	(0.114)	
Paid temporary employment	-0.029***	-0.023***	-0.023***	-0.021***	-0.020***	-0.019***	-0.723**	0.04
	(0.007)	(0.004)	(0.008)	(0.005)	(0.005)	(0.005)	(0.291)	
Unpaid employment	-0.072***	0.006	0.004	0.015**	0.006	0.006	0.198	0.06
	(0.007)	(0.005)	(0.010)	(0.006)	(0.005)	(0.005)	(0.198)	
Total employment	-0.338***	-0.018	-0.073***	-0.028**	-0.017	-0.016	-0.571	0.18
	(0.011)	(0.013)	(0.019)	(0.011)	(0.011)	(0.013)	(0.366)	
Log hours worked in the last week (including 0s):								
Paid employment	-1.050***	-0.086*	-0.298***	-0.165***	-0.080**	-0.075*	-2.749**	0.45
	(0.042)	(0.046)	(0.058)	(0.032)	(0.039)	(0.044)	(1.375)	
Paid employment in non-agriculture	-0.975***	-0.071	-0.278***	-0.133***	-0.068*	-0.061	-2.290*	0.42
	(0.046)	(0.045)	(0.064)	(0.032)	(0.036)	(0.042)	(1.207)	
Paid employment in industry	-0.601***	-0.030	-0.172***	-0.069**	-0.026	-0.030	-0.950	0.20
	(0.045)	(0.034)	(0.054)	(0.033)	(0.029)	(0.031)	(0.933)	
Paid employment in services	-0.374***	-0.042	-0.106**	-0.064**	-0.042*	-0.031	-1.340	0.22
	(0.034)	(0.031)	(0.042)	(0.028)	(0.025)	(0.031)	(1.012)	
Paid employment in agriculture	-0.075***	-0.014	-0.020	-0.032***	-0.012	-0.015	-0.459	0.03
	(0.015)	(0.011)	(0.021)	(0.010)	(0.009)	(0.010)	(0.441)	
Paid temporary employment	-0.122***	-0.075***	-0.078**	-0.069***	-0.064***	-0.063***	-2.391**	0.16
	(0.026)	(0.017)	(0.031)	(0.021)	(0.018)	(0.018)	(0.990)	
Unpaid employment	-0.258***	0.020	0.018	0.051**	0.024	0.021	0.633	0.18
	(0.023)	(0.019)	(0.038)	(0.022)	(0.017)	(0.018)	(0.719)	
Total employment	-1.333***	-0.064	-0.274***	-0.108**	-0.051	-0.054	-2.052	0.64
	(0.042)	(0.054)	(0.076)	(0.043)	(0.041)	(0.051)	(1.462)	
Log monthly earnings from paid employment (including 0s)	-1.528***	-0.113	-0.395***	-0.212***	-0.105*	-0.098	-3.633**	0.62
	(0.075)	(0.069)	(0.081)	(0.044)	(0.055)	(0.066)	(1.842)	
Not in Education, Employment, or Training (NEET)	-0.529***	-0.016*	-0.003	-0.004	-0.017**	-0.019**	-0.499**	0.08
	(0.013)	(0.008)	(0.012)	(0.007)	(0.008)	(0.008)	(0.194)	
Bandwidth	20	20	20	20	20	20	20	20
Observations	23,809	23,809	23,809	18,793	29,172	25,884	23,809	23,809

Notes: Data is from the 2014 Household Labor Force Survey of Turkey. The bandwidth is 20 months in all regressions, which is the optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman (2009) algorithm when high school attendance is the dependent variable. Column (1) reports OLS results with high school attendance as the independent variable. Columns (2) and (3) report local RD regressions with linear and quadratic control functions. Columns (4) and (5) report local RD regressions with a linear control function using 0.75 $\hat{h}$ and 1.5 $\hat{h}$ bandwidths, respectively. Column (6) reports local linear RD regressions with a bandwidth of 23 months, which is the maximum symmetric bandwidth around the cutoff. Column (7) reports local RD regressions in a two-stage least squares specification, where the treatment is used as an instrument for high school attendance. The dependent variables for participation in the last week are a dummy variable equal to 1 if the respondent participates in paid employment, dummy variables equal to 1 if the respondent participates in paid employment in one of the following sectors: non-agriculture (sum of industry and services), industry, services, and agriculture, a dummy variable equal to 1 if the respondent participates in paid temporary employment, a dummy variable equal to 1 if the respondent participates in unpaid family work, and a dummy variable equal to 1 if the respondent participates in either paid or unpaid employment (total employment). The dependent variables representing log hours worked in the last week are natural logs of hours of work performed under each category of employment by coding missing values for non-employed individuals as zero. The following dependent variables are natural log of monthly earnings reported from paid employment by coding the earnings of non-employed individuals as zero, and a dummy variable equal to 1 if the individual is not in education, employment, or training, i.e. has NEET status. The last two rows report the bandwidth and observations used in the regressions. All specifications control for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age and gender present in the household with the age categories of 0-5, 6-17, 18-44, 45-64, 65-above, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

TABLE 5: RD TREATMENT EFFECT OF EDUCATION ON CHILD LABOR AND IDLENESS OUTCOMES BY GENDER

	Overall		Female		Male		Difference	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Linear RD	Linear RD-2SLS	Linear RD	Linear RD-2SLS	Linear RD	Linear RD-2SLS	(3)-(5)	Mean
Outcome / statistics	$\hat{h}$ bandwidth	$\hat{h}$ bandwidth	$\hat{h}$ bandwidth	$\hat{h}$ bandwidth	$\hat{h}$ bandwidth	$\hat{h}$ bandwidth	p-value	(Overall/Female/Male)
Participation in the last week:								
Paid employment	-0.025**	-0.795**	-0.017	-0.522	-0.036**	-1.025**	0.30	0.12/0.06/0.17
	(0.011)	(0.357)	(0.015)	(0.455)	(0.014)	(0.407)		
Paid employment in non-agriculture	-0.021*	-0.662**	-0.012	-0.366	-0.032**	-0.917**	0.23	0.11/0.05/0.16
	(0.011)	(0.306)	(0.014)	(0.420)	(0.014)	(0.375)		
Paid employment in industry	-0.008	-0.245	0.006	0.191	-0.025*	-0.703**	0.02	0.05/0.02/0.08
	(0.009)	(0.238)	(0.009)	(0.308)	(0.013)	(0.330)		
Paid employment in services	-0.013	-0.418	-0.018**	-0.557	-0.008	-0.214	0.31	0.06/0.04/0.08
	(0.008)	(0.276)	(0.007)	(0.348)	(0.011)	(0.324)		
Paid employment in agriculture	-0.004	-0.133	-0.005	-0.156	-0.004	-0.108	0.82	0.01/0.01/0.01
	(0.003)	(0.114)	(0.004)	(0.119)	(0.004)	(0.132)		
Paid temporary employment	-0.023***	-0.723**	-0.026***	-0.782**	-0.018**	-0.507*	0.47	0.04/0.03/0.06
	(0.004)	(0.291)	(0.006)	(0.382)	(0.008)	(0.284)		
Unpaid employment	0.006	0.198	0.010	0.300	0.006	0.172	0.75	0.06/0.04/0.07
	(0.005)	(0.198)	(0.007)	(0.234)	(0.009)	(0.283)		
Total employment	-0.018	-0.571	-0.006	-0.196	-0.029*	-0.835**	0.22	0.18/0.10/0.25
	(0.013)	(0.366)	(0.015)	(0.433)	(0.017)	(0.395)		
Log hours worked in the last week (including 0s):								
Paid employment	-0.086*	-2.749**	-0.050	-1.501	-0.134**	-3.816**	0.24	0.45/0.22/0.66
	(0.046)	(1.375)	(0.057)	(1.691)	(0.057)	(1.551)		
Paid employment in non-agriculture	-0.071	-2.290*	-0.030	-0.909	-0.122**	-3.470**	0.18	0.42/0.20/0.62
	(0.045)	(1.207)	(0.054)	(1.589)	(0.058)	(1.463)		
Paid employment in industry	-0.030	-0.950	0.031	0.938	-0.102*	-2.881**	0.01	0.20/0.07/0.32
	(0.034)	(0.933)	(0.034)	(1.221)	(0.051)	(1.288)		
Paid employment in services	-0.042	-1.340	-0.061**	-1.847	-0.021	-0.590	0.38	0.22/0.13/0.30
	(0.031)	(1.012)	(0.028)	(1.220)	(0.046)	(1.274)		
Paid employment in agriculture	-0.014	-0.459	-0.020	-0.592	-0.012	-0.346	0.74	0.03/0.02/0.04
	(0.011)	(0.441)	(0.014)	(0.460)	(0.017)	(0.500)		
Paid temporary employment	-0.075***	-2.391**	-0.084***	-2.544**	-0.059*	-1.681	0.54	0.16/0.09/0.22
	(0.017)	(0.990)	(0.021)	(1.257)	(0.031)	(1.027)		
Unpaid employment	0.020	0.633	0.025	0.765	0.024	0.693	0.98	0.06/0.04/0.07
	(0.019)	(0.719)	(0.021)	(0.696)	(0.032)	(1.012)		
Total employment	-0.064	-2.052	-0.021	-0.648	-0.109	-3.104**	0.22	0.18/0.10/0.25
	(0.054)	(1.462)	(0.056)	(1.650)	(0.069)	(1.572)		
Log monthly earnings from paid employment (including 0s	-0.113	-3.633**	-0.054	-1.632	-0.197**	-5.602***	0.17	0.62/0.30/0.91
	(0.069)	(1.842)	(0.086)	(2.549)	(0.084)	(2.084)		
Not in Education, Employment, or Training (NEET)	-0.016*	-0.499**	-0.029**	-0.881***	-0.004	-0.121	0.03	0.08/0.11/0.06
	(0.008)	(0.194)	(0.011)	(0.210)	(0.007)	(0.197)		
Bandwidth	20	20	20	20	20	20		
Observations	23,809	23,809	11,521	11,521	12,288	12,288		

Notes: Data is from the 2014 Household Labor Force Survey of Turkey. The bandwidth is 20 months in all regressions, which is the optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman (2009) algorithm when high school attendance is the dependent variable. Columns (1) and (2) report results for the overall sample, columns (3) and (4) report them for the subsample of females, and columns (5) and (6) report them for the subsample of males. Columns (1), (3), and (5) report local RD regressions with linear control functions, and columns (2), (4), and (5) report local RD regressions in a two-stage least squares specification, where the treatment is used as an instrument for high school attendance. Column (6) reports the p-value for the SUR test of equality between treatment effects for subsamples of males and females reported in columns (3) and (5). The dependent variables for participation in the last week are a dummy variable equal to 1 if the respondent participates in paid employment, dummy variables equal to 1 if the respondent participates in paid employment in one of the following sectors: non-agriculture (sum of industry and services), industry, services, and agriculture, a dummy variable equal to 1 if the respondent participates in paid temporary employment, a dummy variable equal to 1 if the respondent participates in unpaid family work, and a dummy variable equal to 1 if the respondent participates in either paid or unpaid employment (total employment). The dependent variables representing log hours worked in the last week are natural logs of hours of work performed under each category of employment by coding missing values for non-employed individuals as zero. The following dependent variables are natural log of monthly earnings reported from paid employment by coding the earnings of non-employed individuals as zero, and a dummy variable equal to 1 if the individual is not in education, employment, or training, i.e. has NEET status. The last two rows report the bandwidth and observations used in the regressions. All specifications control for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age and gender present in the household with the age categories of 0-5, 6-17, 18-44, 45-64, 65-above, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

TABLE 6: HETEROGENOUS RD TREATMENT EFFECTS BY PRE-REFORM REGIONAL POVERTY RATE

	Overall		**Female**		**Male**	
	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median
Outcome / statistics	(1)	(2)	(3)	(4)	(5)	(6)
Impact on participation in the last week						
High school attendance	0.049**	0.015	0.049	0.024	0.051*	0.014
	(0.020)	(0.015)	(0.029)	(0.019)	(0.026)	(0.021)
p-value	0.19		0.46		0.35	
Outcome mean	0.81	0.87	0.82	0.88	0.79	0.85
Vocational high school attendance	0.084***	0.021	0.059**	0.015	0.114***	0.037
	(0.025)	(0.023)	(0.023)	(0.035)	(0.036)	(0.031)
p-value	0.12		0.37		0.11	
Outcome mean	0.33	0.41	0.32	0.39	0.33	0.43
Academic high school attendance	-0.030	0.004	-0.002	0.019	-0.057**	-0.012
	(0.021)	(0.021)	(0.031)	(0.035)	(0.026)	(0.036)
p-value	0.27		0.65		0.28	
Outcome mean	0.47	0.45	0.49	0.48	0.44	0.42
Paid Employment	-0.047***	0.001	-0.033**	0.000	-0.063***	-0.004
	(0.012)	(0.019)	(0.013)	(0.024)	(0.018)	(0.023)
p-value	0.03		0.16		0.07	
Outcome mean	0.12	0.12	0.05	0.07	0.18	0.17
Unpaid Employment	0.023**	-0.009	0.016	0.001	0.032**	-0.012
	(0.009)	(0.007)	(0.012)	(0.011)	(0.015)	(0.012)
p-value	0.01		0.39		0.03	
Outcome mean	0.07	0.05	0.05	0.03	0.09	0.06
Total Employment	-0.023	-0.007	-0.017	0.003	-0.029	-0.016
	(0.015)	(0.021)	(0.016)	(0.026)	(0.024)	(0.026)
p-value	0.49		0.47		0.71	
Outcome mean	0.19	0.17	0.10	0.10	0.27	0.23
Observations	10,368	10,857	4,939	5,257	5,429	5,600
Log monthy earnings from paid emp.	-0.241***	0.015	-0.121*	0.038	-0.363***	-0.046
	(0.072)	(0.114)	(0.070)	(0.149)	(0.121)	(0.148)
p-value	0.07		0.27		0.16	
Outcome mean	0.60	0.63	0.24	0.35	0.92	0.90
Observations	11,256	11,599	5,473	5,576	5,783	6,023
NEET	-0.025*	-0.010	-0.038	-0.031*	-0.014	0.007
	(0.015)	(0.010)	(0.024)	(0.017)	(0.015)	(0.011)
p-value	0.44		0.85		0.3	
Outcome mean	0.11	0.07	0.14	0.08	0.08	0.05
Observations	11,256	11,599	5,473	5,576	5,783	6,023

Notes: Data is from the 2014 Household Labor Force Survey of Turkey. The table shows heterogenous RD treatment effects by splitting the sample of observations, within a bandwidth of 20 months around the January 1998 cutoff, at the median value of regional poverty rate in 2011 before the 2012 education reform was implemented. Odd columns report above median RD estimates and even columns report below-median RD estimates for the whole sample in columns (1) and (2), females in columns (3) and (4), and males in columns (5) and (6). The rows with p-values report the p-values for the SUR test of equality between treatment effects for above the median and below the median subsamples. The dependent variables are a dummy variable equal to one if the respondent attends high school, a dummy variable equal to one if the respondent attends vocational high school, a dummy variable equal to one if the respondent attends academic high school, a dummy variable equal to one if the respondent participates in paid employment, a dummy variable equal to one if the respondent participates in unpaid employment, a dummy variable equal to one if the respondent is employed, the log of monthly earnings of the respondent from paid employment, and a dummy variable equal to one if the respondent is not in education, employment, or training (NEET). All specifications include a linear control for birth distance to January 1998 in months on each side of the discontinuity and additional controls for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age and gender present in the household with the age categories of 0-5, 6-17, 18-44, 45-64, 65-above, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

TABLE A1: ADDITIONAL SUMMARY STATISTICS OF 15-17 YEAR-OLD TEENAGERS

	Overall		**Female**		**Male**		**Difference**
	Mean	SD	Mean	SD	Mean	SD	Est. (SE)
Household chores	0.37	0.48	0.64	0.48	0.14	0.35	0.50 (0.03)***
Hours of household chores (including 0s)	1.17	1.73	2.22	1.94	0.26	0.73	1.96 (0.10)***
Household chores if child is NEET	0.70	0.46	0.87	0.34	0.39	0.49	0.47 (0.04)***
Hours of household chores if child is NEET (including 0s)	2.20	1.82	3.00	1.63	0.72	1.10	2.27 (0.13)***
Observations	1,445		701		744		

Notes: The table presents the mean, standard deviation, and number of observations from the 2012 Child Labor Force Survey of Turkey. The sample includes 15-17 year-old teenagers since this survey only reports data in two categories: 6-14, and 15-17 year-olds. The difference estimates slightly differ from simple differences due to rounding error. Household chores is a dummy variable that takes the value of 1 if the child performs household chores, and hours of household chores is a categorical variable for whether the child performs hours of work listed within the following categories: 0, 1-2, 3-7, 8-15, 16-30, 31-39, and more than 40 hours per week. The latter is the only measure available in the survey. The third and fourth rows report the summary statistics for the variables conditional on child being NEET, that is, not in education, employment, or training. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.