An Early Assessment of Extension of Compulsory School Attendance in Turkey: Evidence from a Natural Experiment

Mehmet Alper Dinçer*

Bilge Erten†

October 7, 2015

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 and idleness. 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 3.8 percent increase in high school attendance on average, which reduced the likelihood of teenagers to work for a wage, particularly in the non-agricultural sector and in jobs that are seasonal or temporary in duration. We find that these effects work through different channels depending on the degree of tightness in child labor markets and differences in program choice. 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 idleness. For girls, due to the presence of underemployment, the reform resulted in them reducing their idleness significantly, yielding a smaller tradeoff between school attendance and child labor in services. The latter was also a result of their higher enrollment into distance education, relaxing their time constraints, which was absent for males. Lastly, using administrative data, we show a substantial increase in the number of religious schools, and document that more than half of the increase in vocational high school attendance was due to the increase in religious school enrollment. We interpret these findings as evidence that education reforms may not have secularizing effects and their empowering effects may depend on tightness of labor markets and gender biases in schooling decisions.

JEL Classification: I25, J16, O15, O22

*Thomas J. Alexander Fellow, OECD and Education Reform Initiative, Sabancı University, Istanbul, Turkey. alperdincer@sabanciuniv.edu.

†Assistant Professor, Economics Department, Northeastern University, Boston, USA. b.erten@neu.edu.

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 168 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 and idleness. We use two nationally representative surveys, the 2013 and 2014 Household Labor Force Surveys, which contain 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 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;

[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.

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 3.2 percentage points (ppt) on high school attendance, which corresponds to a 3.8 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 13 percent (5 ppt), which includes religious high schools, we do not find any evidence that it had a significant effect on academic or distance high school attendance in our full sample.[8] 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 (4.6 ppt) and that of boys by 14 percent (5.5 ppt) respectively whereas we find no evidence of a significant effect on academic high school attendance.[9] We also find that the reform had a significant and positive effect of 25 percent (1.5 ppt) on female distance high school attendance in

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

[8]After the first year of the reform using 2013 HLFS data, we estimate the reform to have a smaller positive impact on vocational high school attendance of 10 percent (3.3 ppt), and a small negative effect on academic high school attendance by 2.7 percent (1.1 ppt).

[9]After the first year of the reform using 2013 HLFS data, we estimate slightly smaller effects on vocational high school attendance by gender, and a small decline in female academic high school attendance.

its first year of implementation, without evidence of a significant impact on male distance high school attendance.

Second, our RD estimates reveal that this increase in high school attendance had significant negative effects on various forms of child labor. Two years after the reform was implemented, and relative to the sample means, teenagers were 18 percent (2 ppt) less likely to work for a wage in the non-agricultural sector, 20 percent (2.4 ppt) less likely to work for a wage regardless of the sector, and 58 percent (2.3 ppt) less likely to work in a seasonal or temporary job for a wage. We find that the effects of the reform are larger and more precisely estimated for wage work than total work due to the presence of unpaid family work in total. We find no evidence that the reform had a significant effect on child labor in agricultural sector, which consists of primarily unpaid family work. We find that the reform had a negative effect of 18 percent (8.4 ppt) decline in hours of wage work performed.

Third, we find that the reform had also a negative effect of 20 percent (1.6 ppt) on idleness 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 could be the presence of heterogenous treatment effects depending on the gender of the respondent. In particular, the differences in the degree of the tightness of child labor markets and sectoral segregation of these markets by gender could 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 (2.8 ppt) on male child labor in industrial sector, whereas we find no evidence of a significant effect on female child labor in industrial sector, where only 2 percent of girls are employed as opposed to 9 percent of boys. In contrast, we find that the reform had a negative effect of 48 percent (1.9 ppt) on female child labor in services, whereas there is no evidence of a significant effect on male child labor in services, despite the fact that 10 percent of boys and 4 percent of girls are employed in services. The fuzzy RD estimates show that a one ppt increase in high school attendance induced by the reform reduces male wage work in non-agricultural sector by 0.98 ppt, hence an almost one-to-one tradeoff between high school attendance and male child labor for a wage in this sector. This substitution effect declines to 0.83 ppt for male child labor in industrial sector, and it is 0.55 ppt for female child labor in services. Our results also show that the reform had a negative effect of 25 percent (2.8 ppt) on female idleness, and we find no evidence of a significant impact on male idleness. The fuzzy RD estimates show that a one ppt increase in high school attendance induced by the reform reduces female idleness by 0.82 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 heterogenous 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 propose an additional channel aside from the relative tightness of child labor markets to explain these heterogenous effects of the reform by gender. Within the context of the education reform's implementation in Turkey, it was permitted that students could comply with the compulsory schooling law by enrolling in distance high school. This involves online classes and studying from textbooks to pass completion exams. During the legislative process, the option of allowing students to enroll in distance high school was largely criticized due to the fears that parents from socially conservative backgrounds would register their daughters to distance high school to keep them away from co-educational schools. In fact, our findings show that after one year of the reform, the female distance high school enrollment increased by 25 percent (1.5 ppt) relative to the sample mean. We find no evidence that the reform had a significant impact on male distance high school enrollment. Since teenage girls who registered these programs to comply with the law do not have to be physically present at school during the day, this relaxes their time constraints imposed from the reform. We interpret these differential effects of the reform on high school program choice as an additional channel through which we observe smaller effects on female child labor compared to males.

We examine whether the reform had heterogeneous effects depending on the regional high school enrollment rates before the reform was implemented. Our findings suggest that the reform led to a larger increase in high school attendance in regions where school enrollment rates prior to the reform were lower than the country median. The reform had a larger impact on male school attendance in these previously lagging regions, while it also had a significant impact on female school attendance in regions with high pre-reform enrollment rates. The reform-induced increase in male school attendance in low pre-reform enrollment regions generated large declines in male child labor, particularly in wage work in non-agricultural sectors, and seasonal or temporary wage work in these sectors. We find no evidence that this led to a decline in male idleness. In contrast, the reform-induced increase in female school attendance in both low and high pre-reform enrollment regions, albeit imprecisely estimated in low pre-reform enrollment regions, led to a decline in female idleness in both regions.

Finally, using administrative data, we show a substantial increase in the number of religious schools, and document that more than half of the increase in vocational high school attendance was due to the increase in religious school enrollment. The combination of evidence from administrative data in Section 2.2 on the increase in the relative supply of religious high schools and relative enrollment into these schools during the reform period together with our findings on the significant and large impact of the reform on vocational high school attendance implies that the reform led to a substantial increase in religious high school attendance.[10] This suggests that the ruling pro-Islamist Justice and Development Party (JDP) government was largely successful in increasing religious education among the youth, which was their key motivation in passing the new compulsory schooling law in 2012. Using additional administrative and survey data, we show that non-academic programs are of lower quality compared to academic programs. Therefore, it is likely that the reform-induced increase in education will prove to be of lower quality as well.

Our study contributes to the literature on the effects of education on child labor. Several studies

[10] Religious and vocational high schools are both non-academic programs. Therefore, it is safe to assume that vocational high school students in HLFS data includes religious high school students as well.

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.[11] 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 intensive margin of child labour (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.[12] 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 and secularizing 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 and idleness outcomes. Our RD results show a dramatic increase in vocational high school attendance, which is consistent with the concurrent increase in the number and enrollment to religious high schools shown from administrative data. Thus, our findings do not provide any evidence for secularizing effects of education reform in the presence of large investments in religious schools. However, our results on reduction of child labor among boys and girls and idleness 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

[11] 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.

[12] 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.

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, 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 – increase in compulsory schooling – that could affect the incidence of child labor and idleness and its heterogeneity by gender in a majority-Muslim, 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, and background information on high school enrollment trends and quality; Section 3 presents the data sources and empirical strategy. Section 4 presents the results, and Section 5 concludes.

2 Context

2.1 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.[13] 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.[14] 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

[13] In Turkey, all distance education programs at the secondary level is provided under an institution called Open High School. Open High School grants the same diploma as formal high school institutions.

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

change.[15] Therefore many interpreted the introduction of the option of distance education after completing junior high school as a step which is likely to ease the excessive burden on formal high school institutions [16]

The combination of government's ambitious expansion targets for non-academic programs,[17], its preference to recognize distance education as a valid option to comply with the new law, 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 and lower quality programs.[18] The Republican People's Party (RPP), the main opposition party, also argued that girls would be disproportionately affected by the inclusion of distance education because socially conservative parents who are dissatisfied with unisex education in formal high schools are more likely to prefer sending their daughters to distance education.[19]

Yet, the main focus of tension throughout the legislative process between the ruling Islamic party of JDP and the main opposition party of RPP was the reopening of religious junior high schools, instead of resource shortages or option of distance education. 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 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, or the post-modern coup, as they paved the way for the resignation of the leader of the Islamic 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. [20]

During the 1996-1997 school year, prior to 1997 reform, religious junior high schools attracted more than 12 percent of junior high school students.[21] In 2002, when the JDP came to power, only 3 percent of eligible children attended religious high schools.[22] 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.[23]

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

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

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

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

[19] http://www.chp.org.tr/Public/1/Folder//bykp_444un1yili.pdf

[20] A large empirical literature examines the effects of the 1997 compulsory schooling law 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).

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

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

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

2.2 Trends in High School Enrollments by Gender and Program Type, and Quality of High School Education in Turkey

In this section, we will compare trends in high school enrollments by gender and program type by using administrative and survey data. The upper left 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 new compulsory schooling law began to be implemented.[24] 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 middle graph in Figure 1 shows that the transition rates – the ratio of the number of new 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 an increase in access to high school education, although compliance with the law remained imperfect.

However, a closer look at the administrative data reveals a dramatic change in enrollment patterns by program types. The lower left graph in Figure 1 shows that the share of new registrations to non-academic programs has risen from 45% to 61% for boys and 40% to 54% for girls three years after the policy change between 2011 and 2014. Consequently, during the post-intervention period the share of academic programs in total student population has dropped by 10 percentage points. The lower middle graph in Figure 1 shows that this reduction in academic programs has been accompanied by an increase of student population of 4 percentage points in vocational (non-religious) and 6 percentage points in religious programs.

We document a striking change in the composition of schools over this period. The upper right graph in Figure 1 shows that the annual change in the number of religious high schools was 32 percent in 2012 and another 21 percent in 2013, while the annual change in the number of all high schools was 8 percent in 2012 and another 5 percent in 2013.[25] From the academic year 2010-11 to 2013-14, the Ministry of National Education transformed a total of 1,477 academic high schools, some of which were converted into religious high schools (ERI, 2014). The lower right graph in Figure 1 shows that the number of Anatolian religious high schools and religious high schools increased by 73 percent, Anatolian high schools increased by 57 percent, and vocational high schools increased by 23 percent from 2011 to 2014.[26] These figures illustrate that there has been a substantial increase in the supply of religious schools in total, raising its share in the total composition of high schools.

Moreover, in the absence of administrative data, the Household Labor Force Surveys of 2004-2013 unravel a significant jump in enrollment to distance education. The upper left graph in Figure 2 shows that prior to the policy change, the annual growth of enrollment in distance education was around 8,000 for boys and girls, which increased to roughly 24,000 and 12,000 after the policy change in 2012, and increased further to 52,000 and 36,000 in 2013 for boys and girls, respectively.

Quality of education varies by program type in Turkey (Alkan et al., 2008). This implies that the

[24] 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.

[25] The decline in the number of all high schools in 2014 is due to the consolidation of schools into single units, and closing of certain schools.

[26] The Anatolian religious high schools are religious high schools that admit students with an entrance exam. The Anatolian high schools are academic high schools that admit students with an entrance exam.

changes in the distribution of student popularion by program types that took place after the implementation of the new education reform has implications for the quality of education that students receive. According to the Programme of International Student Assessment (PISA) 2012, vocational high school students perform 23 points lower than their peers in general high schools in mathematics literacy. The upper right graph in Figure 2 shows that the difference between selective Anatolian vocational and academic high schools is 83 points, which roughly corresponds to two school years of learning in academic high schools. However, it should be noted that these differences do not only reflect differences in program quality but also in socio-economic status. Alkan et al. (2008) addresses this problem by estimating the impact of program type on learning growth during high school. Their results reproduced in the lower left graph of Figure 2 show that vocational and Anatolian vocational schools perform approximately 0.15 percentiles lower than general high schools. Lastly, the lower right graph in Figure 2 shows that in the verbal section of 2005 university entrance exam, religious programs perform on a par with general schools and distance education programs perform on a par with vocational programs(Alkan et al., 2008).

3 Data and Empirical Methodology

3.1 Data

We use data from Turkey's Household Labor Force Surveys (HLFS) of 2013 and 2014, which are nationally representative household surveys including detailed information on education and labor market outcomes. The surveys, which were conducted with 146,055 households in 2013 and 140,718 households in 2014, cover data on socioeconomic indicators of households regarding demographics, education, and employment. In particular, the surveys include information on school attendance, type of school attended, grade, NEET and employment status and number of hours worked. Throughout our analysis, we will primarily use data from the 2014 HLFS because it includes the most recent cohort that complied with the reform and has information on the month of birth required for RD design. The reason for why we also use the 2013 HLFS is because the 2014 HLFS does not include information on whether the respondent attends distance high school whereas the 2013 HLFS has this information.[27]

In section 2.2, administrative data comes from the official statistical yearbooks of Ministry of National Education, including National Education Statistics Formal Education 2008-2009, 2009-2010, 2010-2011, 2011-2012, 2012-2013, 2013-2014 and 2014-2015 and survey data comes from HLFS 2004-2013. Other descriptive data used in Figures 1 and 2 comes from Alkan et al. (2008).

Table 1 presents the summary statistics on major indicators for teenagers from the 2014 HLFS of Turkey.[28] 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, allowing us to 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. Based on the 2013 data, 80 percent of them attended high

[27]In 2014, the Turkish Statistical Institute changed its methodology on the types of school attendance asked in the survey, and decided not to collect information on whether the respondent attends distance high school.

[28]The data for the distance high school attendance comes from the 2013 HLFS while the rest of the data comes from the 2014 HLFS.

school, among which 32 percent attended vocational high school, 41 percent attended academic high school, and 7 percent attended distance high school. On average based on the 2014 data, 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). In contrast, only 6 percent of girls and 8 percent of boys attended distance high school according to the 2013 data. 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 and idleness (i.e. NEET status). Based on the 2014 data, roughly 13 percent of 15-18 year-old teenagers were employed in non-agricultural sector and 5 percent of them in the agricultural sector, corresponding to an 18 percent total employment rate. On average 10 percent of boys worked in services and 9 percent of them worked in industry, while 4 percent of girls worked in services and only 2 percent of them worked in industry. When combined with agricultural work, the average total employment rate for boys (25 percent) was more than twice the average for girls (10 percent). In contrast, the average idleness for girls (11 percent) was almost twice the average for boys (6 percent). This implies that 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 and the more conservative attititudes towards young girls' employment.

On average 12 percent of teenagers in our sample worked for a wage, and the remaining 6 percent were unpaid family workers. 17 percent of boys and 6 percent of girls worked for a wage, which was predominantly in non-agricultural sector. Among these, 6 percent of boys and 3 percent of girls worked in a seasonal or temporary job for a wage. 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 it was 39.3 hours for girls. The corresponding hours of work for wage 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 68 percent of the teenagers' mothers and 80 percent of their fathers had completed primary school, while 17 percent of their mothers and 31 percent of their fathers had completed junior high school. Gender differences in parental education indicators are close to zero and most of the differences are not statistically significant. The average household size is 4.2. While households of females are slightly larger than that of males, the difference is not statistically significant.

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 employment and idleness. 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. For outcome variables for which the estimated optimal bandwidth is larger than the maximum bandwidth that can be symmetrically applied, which is 23 by our design[29], we use the maximum bandwidth in the estimations. Furthermore, 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 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.[30] We include the following control variables in our specifications: dummy variables for the level of schooling completed by respondent's mother and father respectively, month-of-birth fixed effects, and region fixed effects.[31]

3.3 Preliminary Checks

We present a standard validity check for the RD design (Imbens and Lemiuex, 2008). In Figure 3, we examine whether the predetermined characteristics that we control in later regressions are continuous 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 dummy variables equal to one if the respondent's mother and father completed primary school, and junior high school respectively, respondent's household size, and regional dummy variables for whether the respondent lives in Istanbul, West Marmara, Aegean, East Marmara, West Anatolia, Mediterranean, Central Anatolia, West Black Sea, East Black Sea, Northeast Anatolia, Central East Anatolia, or Southeast Anatolia of Turkey. 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, which validates our RD design.

[29] For the post-reform cohorts of 1998 and 1999, we have 24 month-year-of-birth cohorts. Given that we assign 0 for the initial post-reform cohort born in January 1998, we have 23 post-reform cohorts that are on the right side of the cutoff.

[30] In estimations with annual data, we cluster standard errors at the year of birth level.

[31] 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.

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 4, where local averages of education outcomes (upper three graphs) and child labor and idleness (i.e. NEET) 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 4 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 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 increase in academic high school attendance.

While Figure 4 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 attending high school (in total), and attending different types 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 for the type of education respondent's father and mother has completed, household size, 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 3.2 percentage points treatment effect on high school attendance, and is statistically significant at 1 percent level. In terms of magnitude, a 3.2 percentage point increase in high school attendance corresponds to a 3.8 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 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 3.7 percentage points; and with the maximum symmetric bandwidth, it is 3.8 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 3.2 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 and idleness 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 4.4 percentage points, corresponding to a 12 percent increase relative to the sample mean. In alternative specifications listed in the remaining columns, the RD estimates remain largely significant, ranging from 4.0 percentage points to 6.0 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 -1.9 percentage points. In alternative specifications, the point estimates are insignificant and smaller ranging from -0.5 to 0.9 percentage points, with the exception of 1.5 bandwidth with a significant 2.4 percentage point decline. The latter is likely the result of using an artificially large bandwidth that includes much older individuals in the comparison group. To summarize, we find that the reform had a large impact on attending vocational high schools (12 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 5 examines graphically whether the reform had heterogenous 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 relavant 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 3.5 and 3.4 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 4.6 percentage points (13 percent relative to the sample mean) and 5.5 percentage points (14 percent relative to the sample mean) on the vocational high school attendance of girls and boys, respectively, and an average effect of 5.0 percentage points for the full sample (13 percent relative to the sample mean). Even though the estimates are slightly higher for boys, column 4 indicates that the differences 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 any of the samples. Although the imprecisely estimated negative effect for boys is larger than that for girls, the difference between them is not statistically significant.

Since we do not observe attendance to distance high school in the 2014 HLFS data, in Panel B we use the 2013 HLFS data, which has information on distance high school attendance, to re-estimate the RD treatment effects on educational outcomes by gender. This will capture the effect of the reform after the first year of its implementation. We use a bandwidth of 2 years around the discontinuity of being born after or before 1998 in all regressions given the absence of the month of birth information in the 2013 HLFS data. The RD estimate in column (1) shows a positive effect of 3.0 percentage points on high school attendance, which is nearly identical to the estimate in Panel A of 3.2 percentage points. In general, we expect to estimate smaller effects in Panel B since the 2013 data covers a shorter period of time after the reform's implementation compared to the 2014 data. Columns 2 and 3 indicate that the reform had the same positive effect of 3.2 percentage points for female and male high school attendance, without any statistically significant difference between them.

Interestingly, the RD results in Panel B of Table 3 reveal that the effects of the reform on the likelihood of attending different program types vary significantly by gender. The RD estimates in columns 2 - 4 show that the reform had a significant positive impact of 3.9 and 3.2 percentage points on the likelihood of vocational high school attendance for girls and boys, respectively, with no statistically significant difference between them. However, in comparison to the subsample means, the effects correspond to a larger increase for girls (13 percent) compared to boys (9 percent). On the other hand, the RD estimates in the third row show that the reform had a significant and negative effect of 2.7 percentage points (6 percent decline) on the probability of girls to attend an academic high school whereas there is no evidence of a significant effect on the boys' probability of attending academic high school. The SUR test results in column 4 indicates that the difference in estimates between the two subsamples is significant at 1 percent level. Finally, based on the specifications with full set of controls, the RD estimates in the bottom row show a positive and significant effect of 1.5 percentage points (25 percent relative to the sample mean) on the girls' likelihood of attending distance high school, while there is no evidence of a significant impact on the likelihood of boys to attend distance high school. The difference in estimates between the female and male samples is statistically significant at 1 percent level. The increased precision of estimates in Panel B for the outcome of academic high school attendance could be due to a more detailed classification of high schools in the 2013 HLFS, and the use of annual bins in RD estimation, both of which are likely to reduce noise and increase precision in estimates.[32]

Overall, the heterogeneous RD results imply that the reform led to a higher female and male vocational high school attendance, which includes religious schools as explained in Section 2.2. The reform also had a positive impact on female distance high school attendance within its first year of implementation, while we find no evidence that it had an effect on male distance high school attendance. We also find no evidence after its two years of implementation that it had a significant effect on academic high school attendance. This implies that the increase in total high school attendance is primarily due to the rise in vocational high school attendance.

[32]It could also be a result of covering one less treatment cohort in the 2013 data compared to the 2014 data.

4.2 Child Labor and Idleness Outcomes

4.2.1 Child Labor and Idleness Outcomes: Full Sample

We illustrate the RD treatment effects first graphically in the bottom graphs of Figure 4, where the child labor and idleness 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 4 show negative jumps in non-agricultural employment in wage work and log hours of wage work performed by 15–18 year-old teenagers at the discontinuity of less than 5 percentage points. The lower right graph shows a negative jump in being not in education, employment, or training (NEET), or being idle at the cutoff point. While these lower graphs in Figure 4 illustrate negative RD treatment effects for child labor and idleness outcomes, we will 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 and idleness 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 school attendance is the independent variable, in order to make the regression results comparable across outcomes and relavant samples. Column 1 reports OLS regression results with high school attendance. The correlations show that high school attendance is negatively associated with being employed in all sectors examined, including agriculture and non-agricultural sectors, working for a wage, working at a seasonal or temporary job for a wage, the hours of work performed, and being idle (not in education, employment or training).

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 percentage points on working for a wage in the non-agricultural sector, corresponding to an 18 percent decline relative to the sample mean. The quadratic RD estimate in column 3 is a precisely estimated 7.1 percentage points. The other linear RD estimates with varying bandwidths in columns 4-6 remain significant, ranging from 1.7 to 3.5 percentage points. The significant linear RD-2SLS estimate in column 7 indicates that the 3.2 percentage point increase in high school attendance induced by the reform had a negative effect of 2 (0.032 x -0.619) percentage point decline in working for a wage in non-agricultural sector. When these RD results are compared to those in the second row for being employed in non-agricultural sector in total, it is clear that the effects are larger and more precisely estimated for the wage work compared to the total, which includes unpaid family work. The same pattern is observed for the results in all employment categories and log hours of work. The implication is that the teenagers that complied with the education reform could quit wage work relatively more easily compared to their unpaid family work.

Next, we examine whether the reform had an effect on working for a wage in the industrial sector. While the linear RD estimate in column 2 is an imprecisely estimated treatment effect of -0.7 percentage points, the quadratic RD estimate in column 3 is a precisely estimated effect of 4.2 percentage points. Alternative specifications vary also in magnitude and significance. We find similar RD treatment effects for working for a wage in the service sector in the third row of Table 4. One of the reasons for the sensitivity of the results to control functions and bandwidths used could be that the presence of heterogenous treatment effects by gender due to the gendered segregation of teenage labor markets in Turkey. We will analyze these effects in detail in Section 4.2.2.

In contrast to the non-agricultural sector, the RD treatment effects on being employed in the agricultural sector are generally smaller and less precisely estimated. Again, we find more precise estimates for wage work in agricultural sector in some specifications; however, the estimates are not robust to alternative control functions or bandwidths used. For the total of being employed in agricultural sector, in which more than 80 percent is unpaid family work, we find no evidence of a significant effect of the reform.

Next, we examine whether the reform had an effect on working for a wage regardless of the sector of employment. The linear RD estimate in column 1 shows a significant and negative treatment effect of 2.4 percentage points, which is a 20 percent decline relative to the sample mean. In the alternative specifications, the RD estimates remain highly significant, and the significant fuzzy RD estimate in column 7 implies that the 3.2 percentage point increase in high school attendance induced by the reform led to a 2.4 percentage point decline in working for a wage. This implies a tradeoff that is rather high, but less than a one-to-one effect, where a one percentage point increase in school attendance generates 0.75 percengate point decline on average.

In particular, we test for whether the reform had an effect on working for a wage in a seasonal or temporary job. The RD estimates, which are very stable and significant across all specifications, imply a treatment effect of 2.3 percentage points, which corresponds to a 58 percent decline relative to the sample mean. This further suggests that teenagers who complied with the education reform quit temporary or seasonal jobs substantially in response to the time constraints that they faced.

When we examine the effects of the reform on log hours of work, we find the similar pattern that only for wage work the RD results are robust to differences in control functions and bandwidths used. The marginally significant linear RD estimate in column 2 indicates that the reform led to a 8.3 percentage points decline in hours of wage work performed. The RD estimates in alternative specifications remain significant. The precisely estimated fuzzy RD estimate in column 7 shows that the reform-induced 3.2 percentage point increase in high school attendance generated an 8.4 percentage point decline in hours of wage work, corresponding to an 18 percent decline relative to the mean. The magnitude of the treatment effect for hours of wage work is slightly less than that for the propensity to work for a wage, which is 20 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 the propensity to work for a wage relative to hours of wage work. Instead, we find evidence for a greater decline in the propensity to work for a wage than hours of wage work, which implies that most of the decline in hours came from more teenagers leaving the labor market, suggesting that the core channel of adjustment was the extensive margin.

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 is significant at 1 percent level, some of the alternative specifications are imprecisely estimated. One reason for the sensitivity of the results to different specifications is the presence of heterogenous effects by gender, which we will focus on in the next section.

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, child idleness is a more commonly observed situation for teenage girls given the constraints facing their human capital accumulation and their market work (mostly in retail services) in socially conservative areas. Hence, to the extent that the compulsory schooling law changes the allocation of time among teenagers between school, work, and idleness, we expect that it generates heterogeneous outcomes by gender.

4.2.2 Child Labor and Idleness Outcomes: Heterogeneous Effects by Gender

Figure 6 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 child labor in services while the right graph indicates no evidence of a significant jump in male child labor in services. In contrast, in Panel B, the left graph does not show any evidence of a significant jump for female child labor in industry while the right graph indicates a negative jump for male child labor in industry. In Panel C, the left graph does not show any evidence of a significant jump for female child labor in agriculture, and the right graph shows some evidence of a minor effect for male child labor in agriculture. Finally, in Panel D, the left graph shows a significant negative jump in female idleness whereas the right graph indicates no evidence of a significant jump in male idleness at the cutoff. 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 and idleness 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 child labor in non-agricultural sector, particularly in wage work, while the corresponding effects in columns 3 and 4 are also negative for females, they are imprecisely estimated. The magnitude of the precisely estimated fuzzy RD estimates in column 6 is of particular interest. These imply that a one percentage point increase in high school attendance caused by the reform reduces male wage work in non-agricultural sector by 0.98 percentage point, hence an almost one-to-one tradeoff between high school attendance and male child labor for a wage in this sector. The estimated tradeoff is slightly smaller, 0.95, in total work in non-agricultural sector.

A breakdown of non-agricultural employment into industrial and service sector employment allows us to clearly examine the sectoral effects of the reform on child labor. The RD results in columns 5 and 6 show that the reform had a negative effect of 2.5 (2.8) percentage points on male child labor in wage work (in total work) in industrial sector, corresponding to a 31 (31) percent decline relative to the sample mean. The magnitude of the fuzzy RD estimates imply that a one percentage point increase in high school attendance induced by the reform leads to a 0.73 (0.83) percentage point decline in wage work (total work) in industrial sector. In contrast, we find no evidence that the reform had a significant effect on female child labor in industrial sector, where only 2 percent of teenage girls in our sample work. Column 7 shows that the difference between estimates for the two samples is statistically significant both for the total of industrial child labor, and for only wage work within this sector.

Next, we examine whether the reform had heterogenous effects on service sector employment by gender. The RD results in columns 3 and 4 show the reform had a significant and negative effect of 1.9 percentage points on female child labor in services, which entails predominantly wage work. This corresponds to a 48 percent decline relative to the mean. The magnitude of the fuzzy RD estimate in column 4 suggests that a one percentage point reform-induced increase in female high school attendance results in a 0.55 percentage point decline in female wage work in services. In contrast, the RD estimates for boys are highly imprecisely estimated and rather small in relative magnitude, despite the fact that 8 (10) percent of boys in our sample are employed in services in wage work (in total), which is comparable or larger than the percentage of boys in industrial jobs (8 percent in wage work, and 9 percent in total). However, the differences in estimates between the two samples is not statistically significant. 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.

In contrast to non-agricultural forms of child labor, we find no evidence that the reform had a significant effect on reducing female or male child labor in agriculture in total or in wage work. One of the explanations for these results in total agricultural child labor could be that the 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.

Next, we examine whether the reform's effects on working for a wage regardless of the sector varied by gender. The RD estimates in columns 5 and 6 show that the reform had a negative effect of 3.7 percentage points on male wage work, corresponding to a 22 percent decline relative to the sample mean. For the female sample, while the RD treatment effects in columns 3 and 4 are negative, they are imprecisely estimated. However, when we restrict the outcome variable to working for a wage in a seasonal or temporary job, we find significant and large effects in both samples. For girls, the RD results show a treatment effect of 2.7 percentage points, yielding a 90 percent decline relative to the mean. For boys, the RD results indicate a treatment effect of 2.1 percentage points, a 35 percent decline relative to the mean.

In order to examine the intensive margin, we test whether the reform had heterogenous effects by gender on log hours of work. Our RD results show precisely estimated treatment effects for only the male sample, without evidence of a significant effect for females despite negative and large coefficients. The magnitude of the RD estimates show a significant treatment effect of 14.2 percentage point (21 percent relative to the mean) decline in male hours of wage work, and a marginally significant treatment effect of 12.8 percentage point (14 percent relative to the mean) decline in male hours of total work. However, the differences in estimates between males and females are insignificant for the outcome variables of working for a wage, working for a wage in a seasonal or temporary job, and log hours of wage and total work.

Finally, we test whether the reform had heterogeneous effects on idleness (or, neet) by gender. The RD estimates in columns 3 and 4 show that the reform had a significant and negative effect of 2.8 percentage points on female idleness, corresponding to 25 percent relative to the mean. The precisely estimated fuzzy RD estimate implies a large tradeoff between female high school attendance and female idleness: a one percentage point increase in high school attendance induced by the reform results in a 0.82 percentage point decline in female idleness. This fuzzy RD estimate is larger than the fuzzy RD estimates for female child labor, which is what we would expect since the female labor market is much less tight compared to the male labor market. As described in Section 3.1, the share of idle females (11 percent) is almost twice that of idle males (6 percent) while the share of working females (10 percent) is less than half of the share of working males (25 percent). 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, the relatively less tight female child labor market allows part of the adjustment to take place through a reduction of excess idle females.

4.3 Heterogenous Effects by Pre-Reform Enrollment Rates across Regions

In order to gain more insight for the channels through which education has an effect on child labor and idleness outcomes, we test for whether the reform produced heterogeneous effects by pre-reform enrollment rates across regions. We merge the gross high school enrollment rates at the 26-region level in 2011 to the 2014 HLFS data. We anticipate the reform to generate larger effects in regions that were lagging behind in high school enrollment rates prior to the implementation of the reform in 2012. We examine whether the effects of the reform differed by pre-reform region based on enrollment rates by splitting the sample into two: the low pre-reform enrollment sample, which is below the country median, and the high pre-reform enrollment sample, which is above the country median. Table 6 presents the results for sharp RD estimates, and Table 7 presents them for the fuzzy RD estimates. Columns 1 - 3 present the linear RD treatment effects for the entire sample; columns 4 - 6 present them for the low pre-reform enrollment sample; and columns 8 - 10 present them for the high pre-reform enrollment sample. Column 7 presents a SUR test result of whether the difference in estimates between females and males in the low enrollment sample is significant, column 11 does the same for the high enrollment sample, and column 12 reports a SUR test result for whether the difference in estimates between the overall low and overall high enrollment samples is significant.

In the first row of Table 9, column 4 reports a linear RD treatment effect of 4.9 percentage points on high school attendance in low pre-reform enrollment regions. The corresponding effect in column 8 for high pre-reform enrollment regions is an imprecisely estimated effect of 1.6 percentage points. Column 12 shows that the difference in estimates between low and high pre-reform enrollment regions is not statistically significant. Among the low pre-reform enrollment regions, column 6 reports a linear RD estimate of 5.9 percentage points for male high school attendance, while column 5 reports an imprecisely estimated effect of 3.6 percentage points for female high school attendance. However, the difference in estimates between the two subsamples is not statistically significant. Among the high pre-reform enrollment regions, column 9 reports a linear RD estimate of 3.6 percentage points for female high school attendance, column 10 reports an imprecisely estimated effect of 0.9 percentage points for male high school attendance, with no statitically significant difference between the two samples.

In the second row of Table 9, the linear RD estimates for vocational high school attendance have the same significance levels, but are larger in magnitude. The overall RD treatment effect for the low enrollment regions is 6.6 percentage points, which appears to be driven by the male sample with a significant effect of 10.8 percentage points, whereas that for the high enrollment regions is 3.7 percentage points, which appears to be driven by the female sample with a significant effect of 7 percentage points. The third row presents the RD treatment effects on academic high school attendance, which are for the most specifications imprecisely estimated, except for a significant negative impact on male academic high school attendance in low enrollment regions.

To summarize, the RD treatment effects on education outcomes in the regions with low pre-reform enrollment rates point out that the main compliers with the reform were males in low pre-reform enrollment regions and females in high pre-reform enrollment regions. The reform led to an increase in the high school attendance of both of these groups by greatly increasing their vocational high school attendance while decreasing their academic high school attendance to lesser extent. In addition, although the estimates are imprecise, the large point estimates for female high school attendance in low pre-reform enrollment regions suggest that the reform likely had a positive impact on these outcomes.

Next, we examine whether these heterogenous RD treatment effects in high school attendance induced different effects on child labor and idleness. We find that in the low pre-reform enrollment regions where the reform had the largest effects on school attendance, particularly on males, the reform had the largest and significant RD treatment effects on all child labor outcomes. The effects are more precisely estimated in Table 7, where the fuzzy RD estimates show that the reform had a negative effect of 4.1 percentage point on male wage work in non-agricultural sector, 2.8 percentage point on male total work in non-agricultural sector, 1.2 percentage point on male wage work in industry, 5.1 percentage point on male wage work regardless of the sector, 2.9 percentage point on male seasonal or temporary work for a wage, and 19.76 percentage point decline in male log hours of wage work in low pre-reform enrollment regions. The corresponding fuzzy RD estimates for female sample in these regions are imprecisely estimated. However, the sharp RD estimates show negative and significant treatment effects on several child labor indicators, including employment in non-agricultural sector, particularly services, wage work in general, wage work in seasonal and temporary jobs, and log hours of wage work.

The only form of child labor for which we find an overall effect (2.1 percentage points) in the high pre-reform enrollment sample is seasonal or temporary work for a wage, and this effect is primarily due to the significant treatment effect (3 percentage points) on the female sample in these regions. The latter is likely a result of the significant and positive effect of the reform on female high school attendance, particularly vocational, in high pre-reform enrollment regions. This is supported by the fuzzy RD estimates in Table 7 that show a marginally significant treatment effect on seasonal or temporary wage work in this subsample. Interestingly, we also find evidence of a significant negative RD treatment effect on female idleness in high pre-reform enrollment sample. The precisely estimated fuzzy RD estimate in Table 7 implies that the reform-induced increased in female high school attendance in these regions resulted in a decline in female idleness. Although the sharp RD estimate of the treatment effects on female idleness in low pre-reform treatment regions is imprecisely estimated in Table 6, the fuzzy RD estimates in Table 7 show a highly significant treatment effect of 3.3 percentage points on female idleness in these regions.

To summarize, our findings suggest that the reform led to a larger increase in high school attendance in regions where school enrollment rates prior to the reform were lower than the country median. The reform had a somewhat larger impact on male school attendance in these regions, while it also had a significant impact on female school attendance in regions with high pre-reform enrollment rates. The reform-induced increase in male school attendance in low pre-reform enrollment regions generated large declines in male child labor, particularly in wage work in non-agricultural sectors, and seasonal or temporary wage work in these sectors. We find no evidence that this led to a decline in male idleness. In contrast, the reform-induced increase in female school attendance in both low and high pre-reform enrollment regions, albeit imprecisely estimated in low pre-reform enrollment regions, led to a decline in female idleness in both regions. We also find some evidence that it reduced female child labor mostly in low pre-reform enrollment regions.

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 and idleness. 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 majority-Muslim, developing-country context with high prevalence of child labor and idleness.

We find that the reform led to an increase of 3.8 percent (3.2 ppt) of high school attendance, and this had significant negative effects on various forms of child labor. Teenagers in the treated cohorts are less likely to work for a wage, particularly in the non-agricultural sector and in jobs that are seasonal or 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 as well as the different constraints facing program choice by gender. 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 idle females from inactivity to 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 child labor in agricultural sector, 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 agricultural child labor.

Another channel in addition to the structure of child labor markets is the difference in constraints facing program choice by gender. While we find no evidence that the reform had an effect on distance high school enrollment of boys, we find that it had a significant positive effect on distance high school enrollment of girls as families from socially conservative backgrounds preferred to keep their daughters away from co-educational schools. Since distance high school does not not require physical presence at school, it relaxes the binding time constraints that applied to girls complying with the reform. The absence of a similar effect for boys makes their time constraints more binding.

Our findings suggest that the reform led to a larger increase in high school attendance in regions where school enrollment rates prior to the reform were lower than the country median. The reform had a larger impact on male school attendance in these previously lagging regions, which led to large declines in male child labor in non-agricultural sector without an evidence of a significant impact on male idleness. It also led to an increase in female high school attendance, but the effects were more evenly distributed between low and high enrollment regions. This led to a strong decline in female idleness in both regions, and had some effect on child labor in low enrollment regions.

Our results on the effects of the reform on composition of high school programs show a dramatic increase in vocational high school enrollment without any evidence of an increase in academic programs, which could have two explanations. Firstly, resource contraints might have restricted the expansion of academic programs. Secondly and more plausibly, the expansion of vocational programs are likely to be related to the main political drive behind the policy change because per capita student expenditures are not necessarily higher in academic programs compared to non-academic ones. Given that the key political motivation of the policy change was to undo the secularizing effects of the 1997 reform, an expansion in non-academic programs including religious programs was expected. This change has been clearly documented with administrative data and it is highly likely that the RD estimates on vocational program attendance maps the same change since students of religious programs are classified under vocational rather than academic programs our HLFS data.

Combining evidence from administrative data, we show a substantial increase in the number of religious schools, and document that more than half of the increase in vocational high school attendance was due to the increase in religious school enrollment. The combination of evidence from administrative data on the increase in the relative supply of religious high schools and relative enrollment into these schools together with our findings on the significant and large impact of the education reform on vocational high school attendance implies that the reform led to a substantial increase in religious high school attendance. A key implication of this result is that the ruling JDP government was largely successful in increasing religious education among the youth, which was their key motivation in passing the compulsory schooling law in 2012. Another implication is that given the lower quality of these religious high schools, the reform is likely to create the side effect of producing a new generation with increased education that is of lower quality on average.

Our findings suggest that education reforms may not necessarily have secularizing effects if the government has an active policy of converting several academic high schools into religious ones, disproportionaly increasing their relative supply. This also implies that the future empowering effects from increasing returns to education will be limited since the returns to religious education tends to be lower than academic education. Moreover, the non-pecuniary benefits from education will be relatively less given that religious schools tend to promote more conservative attitudes than academic ones in general. However, in the short run, our findings provide support for empowering effects of the education reform on teenagers through a reduction on child labor and idleness.

References

Acemoglu, Daron and Joshua Angrist, "How Large Are Human-Capital Externalities? Evidence from Compulsory Schooling Laws," *NBER Macroeconomics Annual*, 2000, *15*, 9–59.

Alkan, Ahmet, Ali Çarkoğlu, Alpay Filiztekin, and Fırat İnceoğlu, "Türkiye ortaöğretim sektöründe katma değer oluşumu: üniversiteye giriş yarışı," 2008.

Angrist, Joshua D. and Alan B. Krueger, "Does Compulsory School Attendance Affect Schooling and Earnings," *The Quarterly Journal of Economics*, November 1991, *106* (4), 979–1014.

Arends-Keunning, M. and S. Amin, "School incentive programs and children's activities: The case of Bangladesh," *Comparative Education Review*, 2004, *48*, 295–317.

Assaad, Ragui, Deborah Levison, and Nadia Zibani, "The Effect of Child Work on Schooling in Egypt," *Unpublished paper (University of Minnesota)*, 2005.

Aydemir, Abdurrahman and Murat G. Kirdar, "Low Wage Returns to Schooling in a Developing Country: Evidence from a Major Policy Reform in Turkey," Technical Report, IZA Discussion Papers 2015.

Beegle, Kathleen, Rajeev Dehejia, and Roberta Gatti, "Why Should We Care about Child Labor? The Education, Labor Market, and Health Consequences of Child Labor," *Working Paper no 3479, World Bank Policy Research*, 2005.

Bhalotra, Sonia and Zafiris Tzannatos, "Child Labor: What Have We Learnt?," Discussion Paper 0317, Social Protection Unit. Washington, D.C: World Bank 2003.

Black, Sandra, Paul Devereux, and Kjell G. Salvanes, "Staying in the Classroom and out of the Maternity Ward? The Effect of Compulsory Schooling Laws on Teenage Births," *Economic Journal*, 2008, *118* (530), 1025–54.

Boozer, M. and T. Suri, "Child labor and schooling decisions in Ghana," Unpublished paper, Yale University 2001.

Brown, Drusilla K., Alan V. Deardorff, and Robert M. Stern, "Child labor: Theory, evidence, and policy," Working paper, Tufts University 2001.

Cardoso, Eliana and Andre Portela Souza, "The Impact of Cash Transfers on Child Labor and School Attendance in Brazil," Working paper 2003.

Carpio, Ximena V. Del and Karen Macours, "Leveling the Intra-household Playing Field: Compensation and Specialization in Child Labour Allocation," *Research in Labour Economics*, 2010, *31*, 259–296.

Cesur, Resul and Naci Mocan, "Does Secular Education Impact Religiosity, Electoral Participation and the Propensity to Vote for Islamic Parties?," in "Koç University-TUSIAD Economic Research Forum Working Papers" number 1422 Koc University-TUSIAD Economic Research Forum 2014.

— , **Bahadir Dursun, and Naci Mocan**, "The Impact of Education on Health and Health Behavior in a Middle-Income, Low-Education Country," Technical Report 20764, National Bureau of Economic Research 2014.

Cigno, Alessandro and Furio Camillo Rosati, *The Economics of Child Labour*, Cambridge, Massachusetts: Oxford University Press, 2005.

Clark, Damon and Heather Royer, "The Effect of Education on Adult Mortality and Health: Evidence from Britain," *American Economic Review*, 2013, *103* (6), 20872120.

Dayioglu, Meltem, "Patterns of Change in Child Labor and Schooling in Turkey: The Impact of Compulsory Schooling," *Oxford Development Studies*, 2005, *33*, 231–246.

de Hoop, Jacobus and Furio C. Rosati, "Cash Transfers and Child Labour," *IZA Discussion Paper No. 7496*, 2013.

Dincer, Mehmet Alper, Neeraj Kaushal, and Michael Grossman, "Women's Education: Harbinger of Another Spring? Evidence from a Natural Experiment in Turkey," *World Development*, 2014, *64* (C), 243–258.

Edmonds, Eric V., "Child labour," NBER Working Paper No. 12926 2007.

— **and Maheshwor Shrestha**, "The Impact of Minimum Age of Employment Regulation on Child Labor and Schooling: Evidence from UNICEF MICS Countries," NBER Working Paper No. 18623 2012.

— **and Nina Pavcnik**, "The Effect of Trade Liberalization on Child Labor," *Journal of International Economics*, 2005, *65* (C), 401 419.

— , — , **and Petia Topalova**, "Trade Adjustment and Human Capital Investments: Evidence from Indian Tariff Reform," *American Economic Journal: Applied Economics*, 2010, *2* (4), 42–75.

ERI, "Education Monitoring Report 2013," Technical Report, Education Reform Initiative, Sabancı University 2014.

Gertler, Paul J., Sebastian W. Martinez, and Marta Rubio-Codina, "Investing Cash Transfers to Raise Long-term Living Standards," *American Economic Journal: Applied Economics*, 2012, *4* (1), 164–192.

Goksel, Idil, "Determinants of School Attainment in Turkey and the Impact of the Extension of Compulsory Education," *Unpublished paper*, 2008.

Grootaert, Christiaan and Harry Anthony Patrinos, "Policies to Reduce Child Labor," in "The Policy Analysis of Child Labor," New York: Palgrave Macmillan, 1999.

Guarcello, Lorenzo, Fabrizia Mealli, and Furio. C. Rosati, "Household Vulnerability and Child Labor: The Effect of Shocks, Credit Rationing, and Insurance," *Journal of Population Economics*, 2010, *23* (1), 169–198.

Gulesci, Selim and Erik Meyersson, "'For the Love of the Republic' Education, Religion and Empowerment," Unpublished paper 2015.

Güneş, Pınar Mine, "The role of maternal education in child health: Evidence from a compulsory schooling law," *Economics of Education Review*, 2015, *47*, 1–16.

Gunnarsson, V., P. Orazem, and M. Sanchez, "Child labor and school achievement in Latin America," *World Bank Economic Review*, 2006, *20*, 31–54.

ILO, "Marking progress against child labour - Global estimates and trends 2000-2012," Technical Report, International Labour Office, International Programme on the Elimination of Child Labour (IPEC) - Geneva: ILO 2013.

Imbens, Guido and Karthik Kalyanaraman, "Optimal bandwidth choice for the regression discontinuity estimator," Unpublished paper 2009.

_ **and Thomas Lemiuex**, "Regression Discontinuity Designs: A Guide to Practice," *Journal of Econometrics*, 2008, *142* (2), 615–635.

Kirdar, Murat G, Meltem Dayıoğlu, and İsmet Koç, "The impact of schooling on the timing of marriage and fertility: Evidence from a change in compulsory schooling law," 2009.

Kirdar, Murat, Meltem Dayioğlu, and Ismet Koc, "Does longer compulsory education equalize schooling by gender and rural/urban residence?," 2014.

_ **, Meltem Dayioglu Tayfur, and İsmet Koç**, "The effect of compulsory schooling laws on teenage marriage and births in Turkey," 2011.

Kondylis, Florence and Marco Manacorda, "School Proximity and Child Labor: Evidence from Rural Tanzania," *Journal of Human Resources*, 2012, *47* (1), 32–63.

Levison, Deborah, Karine Moe, and Felicia Marie Knaul, "Youth Education and Work in Mexico," *World Development*, 2001, *29*, 167–188.

_ **, Richard Anker, Shahid Ashraf, and Sandya Barge**, "Is Child Labor Really Necessary in India's Carpet Industry," in "Economics of Child Labor in Hazardous Industries of India" Hindustan Publishing, New Delhi, India 1998, pp. 95–133.

Lleras-Muney, Adriana, "The Relationship between Education and Adult Mortality in the United States," *Review of Economic Studies*, 2005, *21* (1), 189–221.

Lochner, Lance, "Non-production benefits of education: Crime, health, and good citizenship," Technical Report, NBER Working Paper No. 16722 2011.

McCrary, Justin and Heather Royer, "The Effect of Female Education on Fertility and Infant Health: Evidence from School Entry Policies Using Exact Date of Birth," *American Economic Review*, 2011, *101* (1), 158–195.

Mocan, Leyla, "The Impact of Education on Wages: Analysis of an Education Reform in Turkey," in "Koç University-TUSIAD Economic Research Forum Working Papers" number 1424 Koc University-TUSIAD Economic Research Forum 2014.

OECD, "Education at a Glance 2014: Highlights," Technical Report, OECD Publishing 2014.

Orazem, P. and L. Gunnarsson, "Child labour, school attendance, and performance: A review," Working Paper no. 11177, Department of Economics, Iowa State University 2004.

Oreopolous, Phillip, "Estimating Average and Local Average Treatment Effects of Education when Compulsory Schooling Laws Really Matter," *American Economic Review*, 2006, *96* (1), 152–175.

_ **and K. G. Salvanes**, "Priceless: The Nonpecuniary Benefits of Schooling," *Journal of Economic Perspectives*, 2011, *25* (1), 159–184.

Ravallion, Martin and Quentin Wodon, "Does Child Labour Displace Schooling? Evidence on Behavioural Responses to an Enrollment Subsidy," *The Economic Journal*, 2000, *110* (462), C158–C175.

Ray, R. and G. Lancaster, "Does child labour affect school attendance and school performance? Multi country evidence on SIMPOC data," Unpublished paper, University of Tasmania 2003.

Rosati, F. and M. Rossi, "Children's working hours and school enrollment: Evidence from Pakistan and Nicaragua," *World Bank Economic Review*, 2003, *17*, 283–295.

Siddiqui, Faraaz and Harry A. Patrinos, "Child Labor Issues, Causes and Interventions," World Bank Human Capital and Operations Policy Working Paper 56, Washington, D.C.: The World Bank 1995.

Appendix A List of Variables

Outcome Variables:

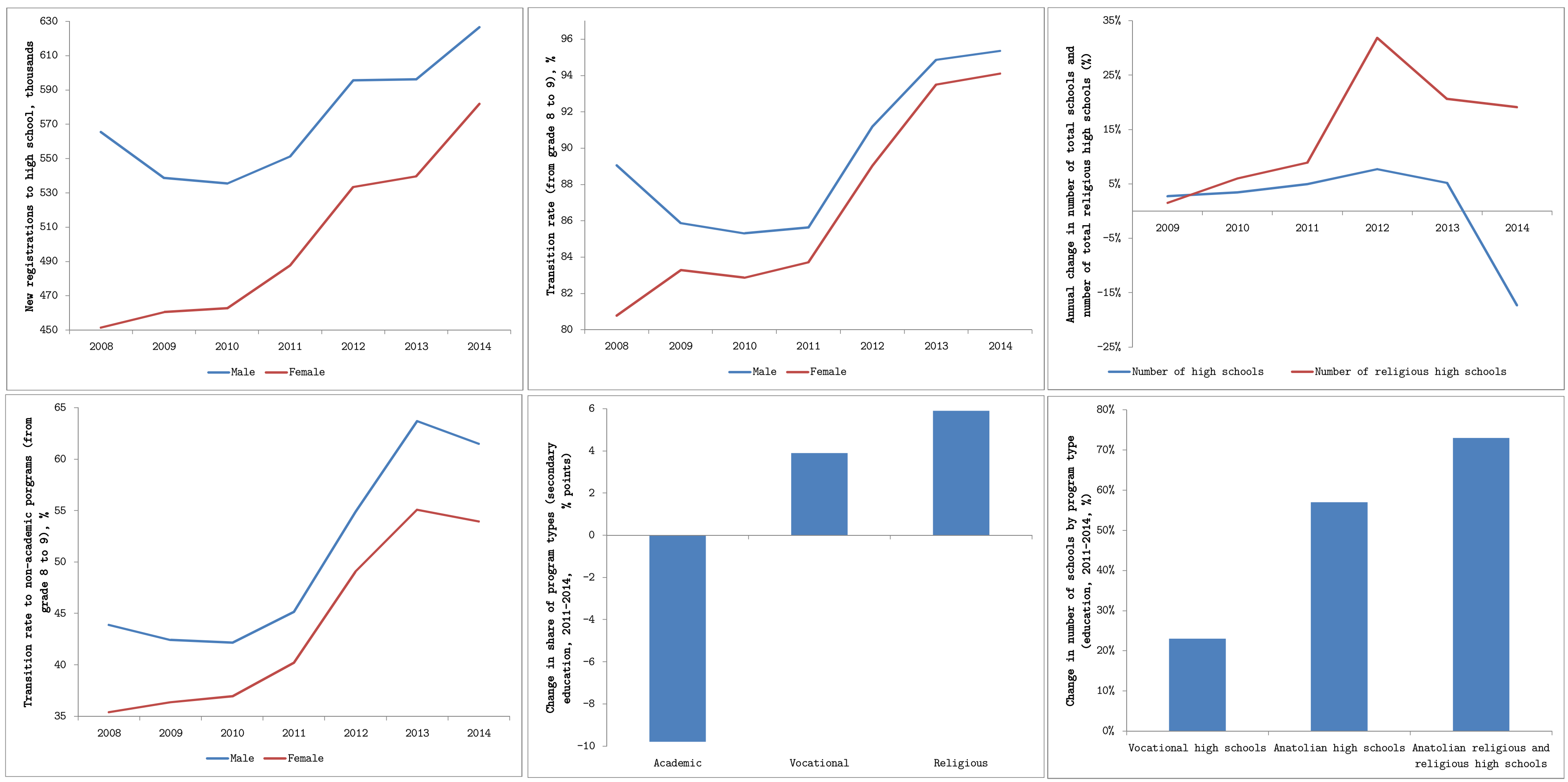
- High School Attendance: A dummy variable equal to one if the respondent attends high school.
- Vocational High School Attendance: A dummy variable equal to one if the respondent currently attends vocational high school.
- Academic High School Attendance: A dummy variable equal to one if the respondent currently attends academic high school.
- Distance High School Attendance: A dummy variable equal to one if the respondent currently attends distance high school.
- Employed in Non-agriculture - Wage Work: A dummy variable equal to one if the respondent is employed in non-agricultural sector and works for a wage.
- Employed in Non-agriculture - Total: A dummy variable equal to one if the respondent is employed in non-agricultural sector.
- Employed in Industry - Wage Work: A dummy variable equal to one if the respondent is employed in industrial sector and works for a wage.
- Employed in Industry: A dummy variable equal to one if the respondent is employed in industrial sector.
- Employed in Services - Wage Work: A dummy variable equal to one if the respondent is employed in service sector and works for a wage.
- Employed in Services: A dummy variable equal to one if the respondent is employed in service sector.
- Employed in Agriculture - Wage Work: A dummy variable equal to one if the respondent is employed in agricultural sector and works for a wage.
- Employed in Agriculture: A dummy variable equal to one if the respondent is employed in agricultural sector.
- Works for a Wage: A dummy variable equal to one if the respondent works for a wage.
- Seasonal or Temporary Work for a Wage: A dummy variable equal to one if the respondent works for a wage in a seasonal or temporary job.
- Log Hours of Wage Work (per week): The log of hours per week for working for a wage, which takes the value of zero if the respondent does not work.
- Log Hours of Work (per Week): The log of hours of work per week, 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:

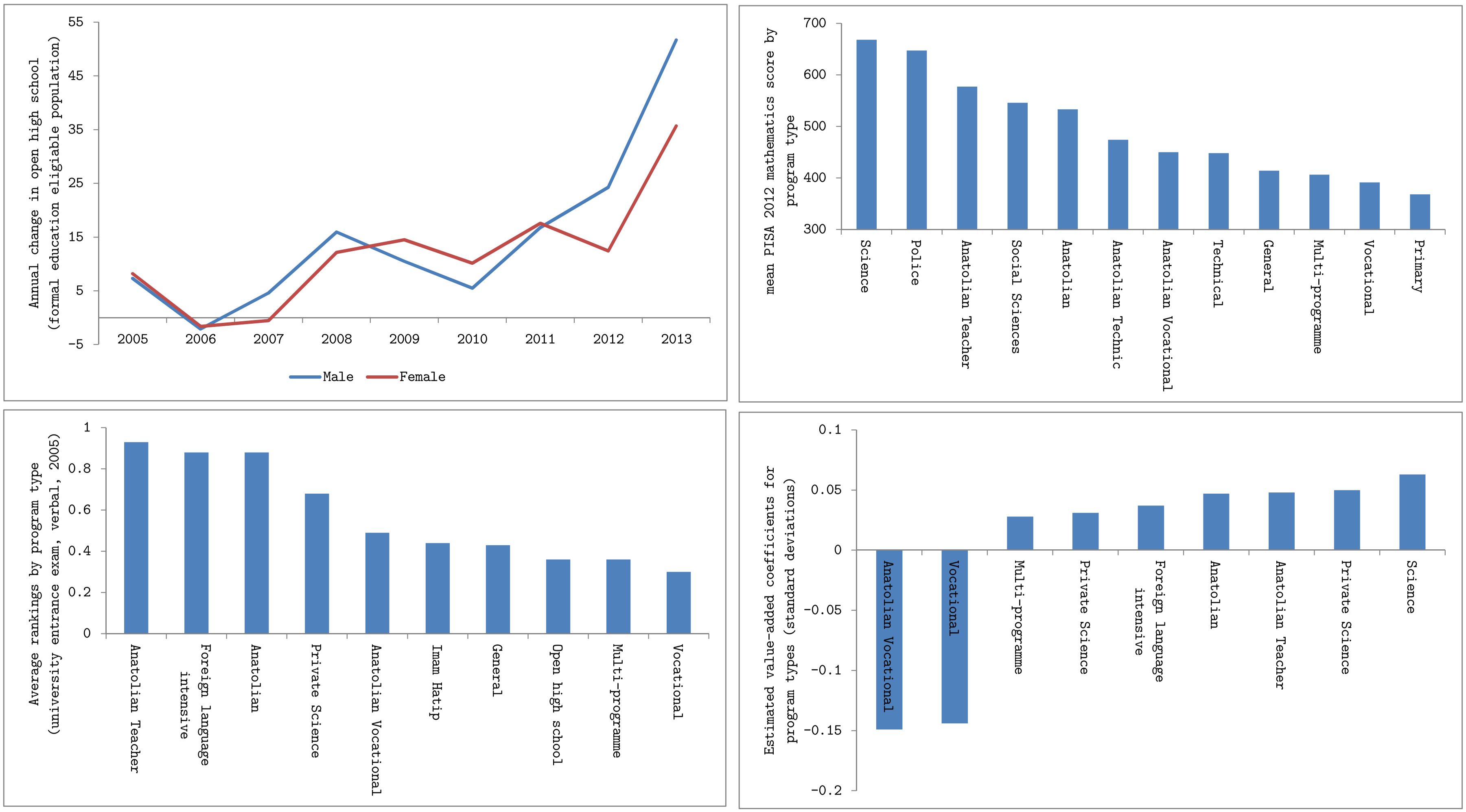
- Mother completed primary school: A dummy variable equal to one if the respondent's mother completed primary school (or above).
- Father completed primary school: A dummy variable equal to one if the respondent's father completed primary school (or above).
- Mother completed junior high school: A dummy variable equal to one if the respondent's mother completed junior high school (or above).
- Father completed junior high school: A dummy variable equal to one if the respondent's father completed junior high school (or above).
- 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 and Number of High Schools



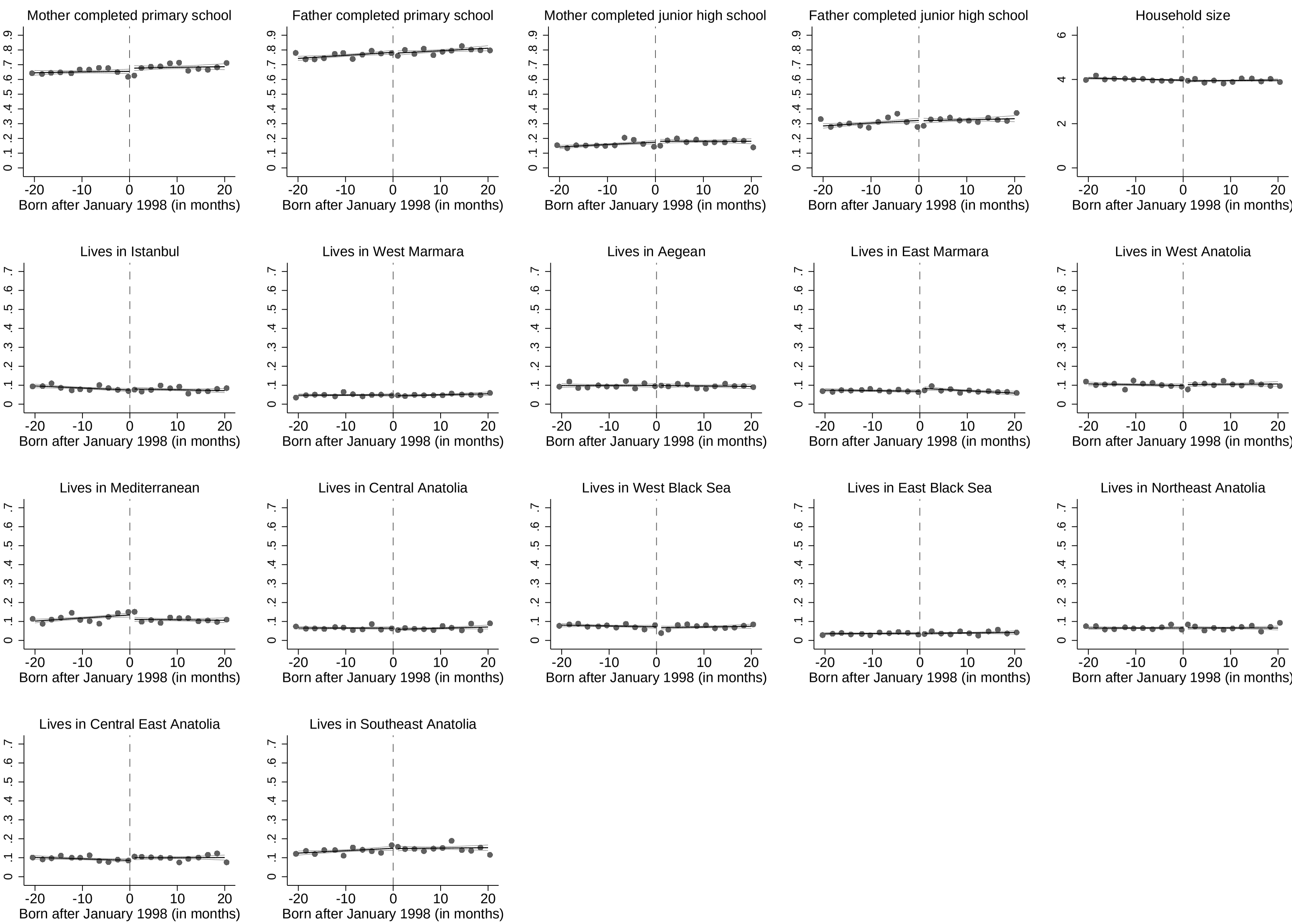
Note: Administrative data is from National Education Statistics Formal Education 2008-2009, 2009-2010, 2010-2011, 2011-2012, 2012-2013, 2013-2014 and 2014-2015.

Figure 2: Trends in Distance High School Enrollment and Quality of High Schools



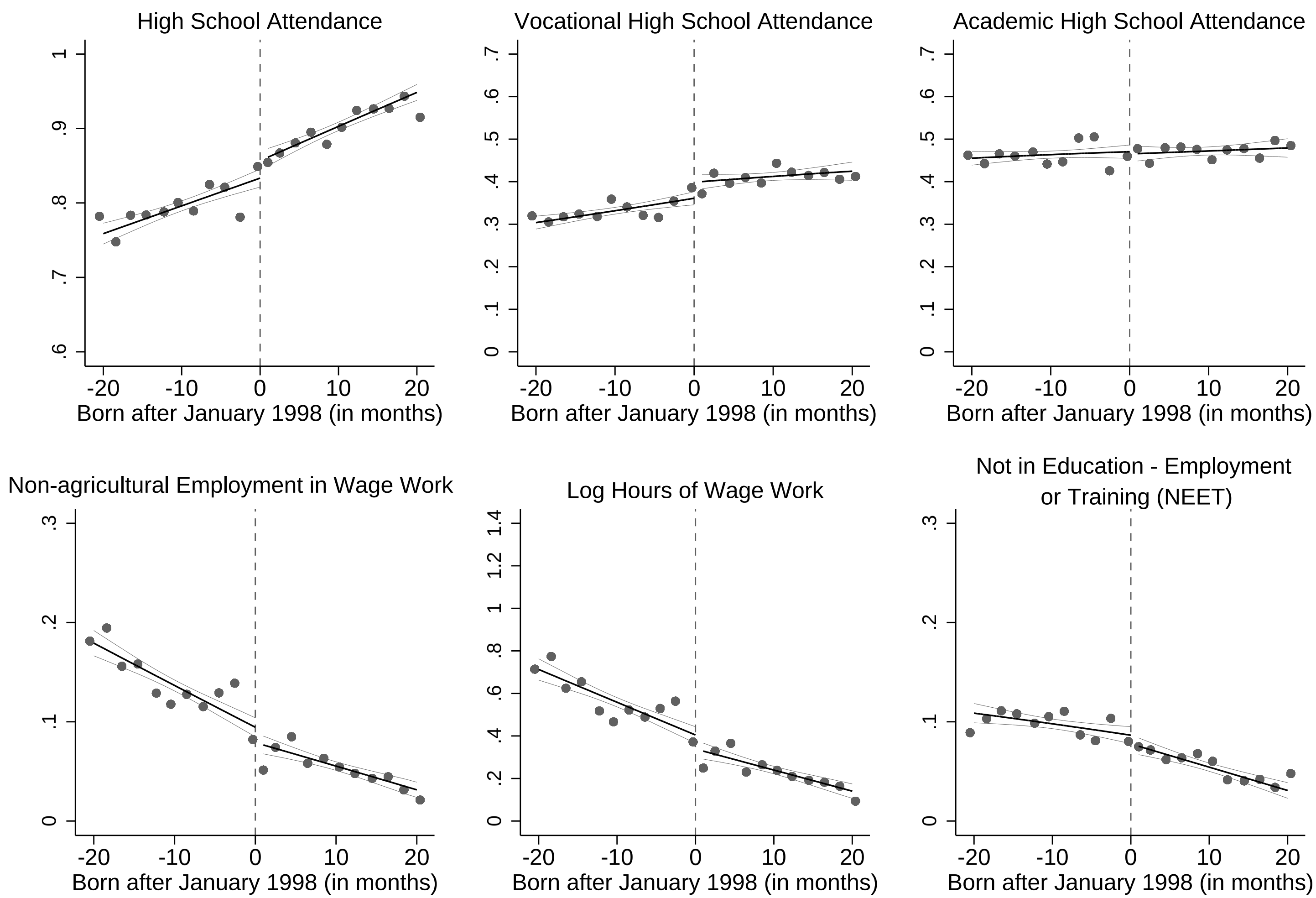
Note: Authors calculations from PISA 2012 Turkey micro data; 2004-2013 HLFS; Alkan et al. (2008).

Figure 3: Balanced Covariates



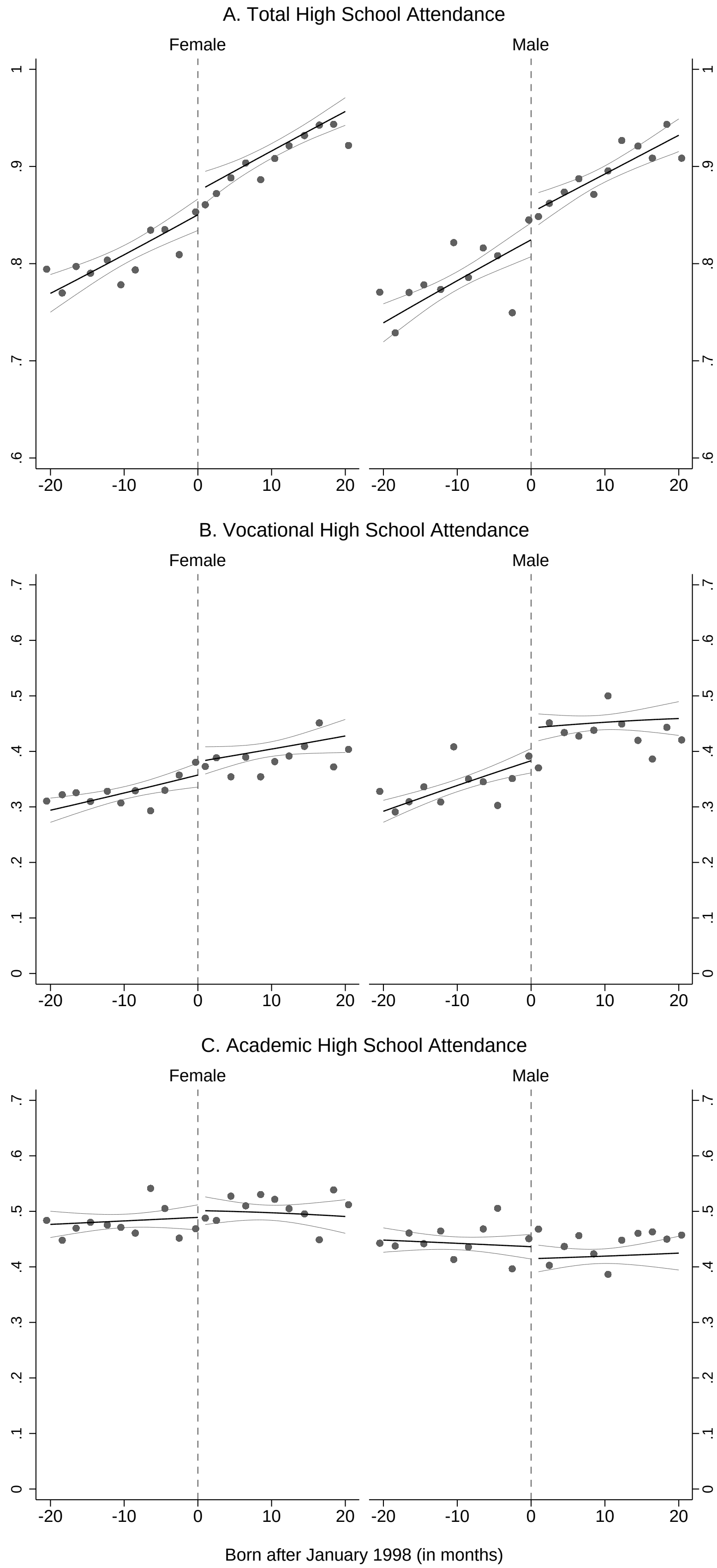
Note: Data is from the 2014 Household Labor Force Survey of Turkey. Figures plot pre-determined covariates in monthly bins against the month-year-of-birth of being born in January 1998. The vertical line in each graph represents the cut-off point, January 1998. Gray lines show 95 percent confidence intervals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

Figure 4: RD Treatment Effects: Education, Child Labor, and Idleness Outcomes



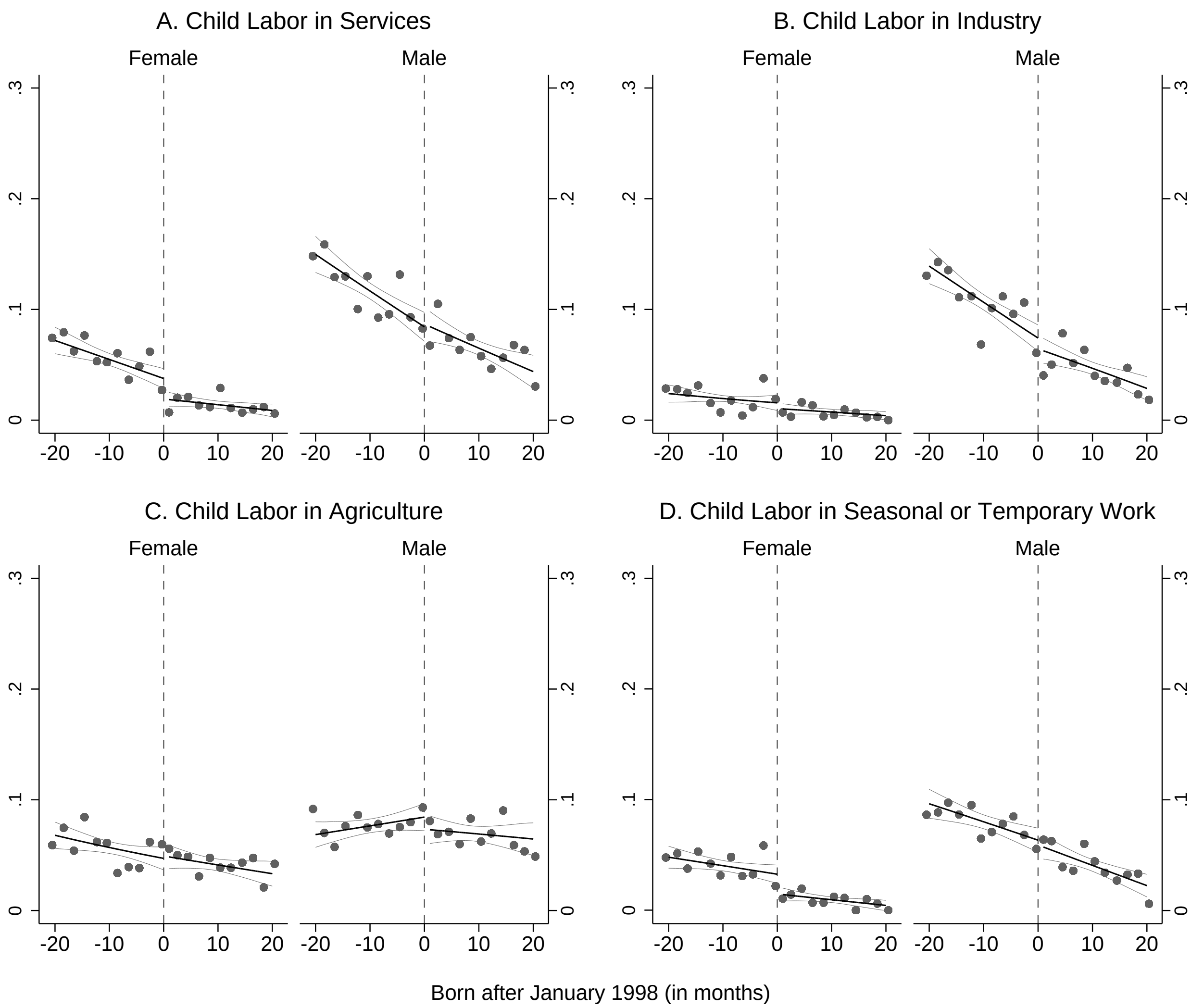
Note: Data is from the 2014 Household Labor Force Survey of Turkey. Figures plot high school attendance rates, agricultural employment in wage work, log hours of wage work per week, and idleness (NEET) rates in monthly bins against the month-year-of-birth of being born in January 1998. The vertical line 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: Education Outcomes by Gender



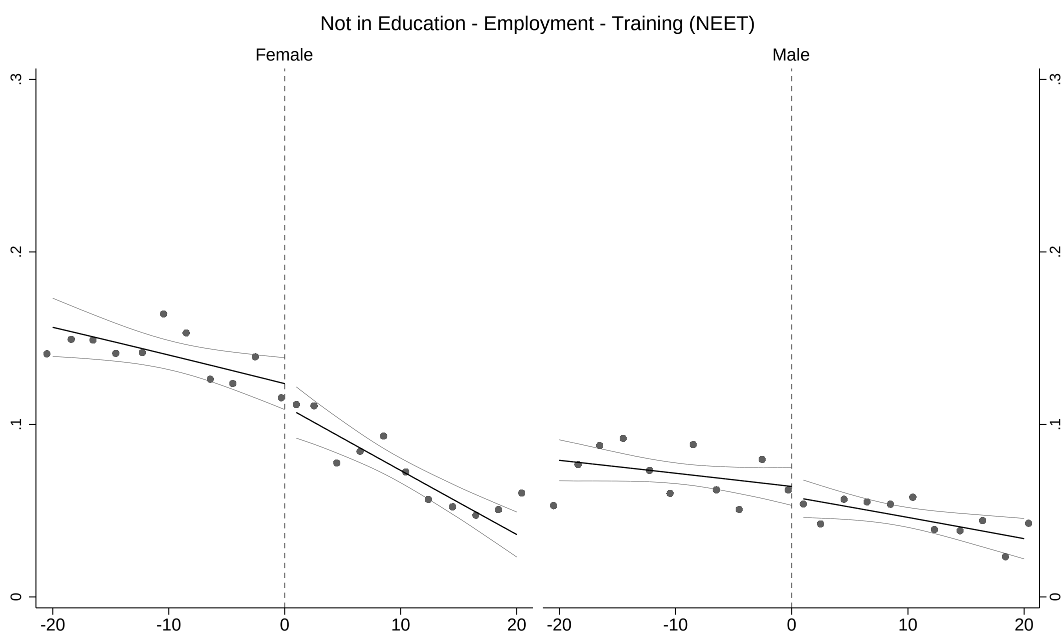
Note: Data is from the 2014 Household Labor Force Survey of Turkey. Figures plot high school attendance rates for total, vocational, and academic high school attendance in monthly bins against the month-year-of-birth of being born in January 1998. The vertical line in each graph represents the cut-off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

Figure 6: RD Treatment Effects: Child Labor Outcomes by Gender



Note: Data is from the 2014 Household Labor Force Survey of Turkey. Figures plot child labor in services, industry, agriculture, and seasonal or temporary jobs for a wage 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 7: RD Treatment Effects: Propensity of Idleness by Gender



Note: Data is from the 2014 Household Labor Force Survey of Turkey. Figures plot propensity to be idle, or not be 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 TEENAGERS

Panel A: Education

	Overall sample			Female sample			Male sample			Female - Male
	Mean	SD	Obs	Mean	SD	Obs	Mean	SD	Obs	Difference
High School Attendance	0.84	0.36	23,809	0.86	0.35	11,521	0.83	0.38	12,288	0.03 (0.01)***
Vocational High School Attendance	0.38	0.48	23,809	0.36	0.48	11,521	0.39	0.49	12,288	-0.03 (0.01)***
Academic High School Attendance	0.46	0.50	23,809	0.49	0.50	11,521	0.43	0.50	12,288	0.06 (0.01)***
High School Attendance[1]	0.80	0.40	33,426	0.80	0.40	16,196	0.79	0.41	17,230	0.01 (0.00)**
Vocational High School Attendance[1]	0.32	0.47	33,426	0.30	0.46	16,196	0.34	0.47	17,230	-0.03 (0.01)***
Academic High School Attendance[1]	0.41	0.49	33,426	0.44	0.50	16,196	0.38	0.49	17,230	0.06 (0.01)***
Distance High School Attendance[1]	0.07	0.25	33,426	0.06	0.24	16,196	0.08	0.26	17,230	-0.01 (0.01)***

Panel B: Child Labor and Idleness Outcomes

	Overall sample			Female sample			Male sample			Female - Male
	Mean	SD	Obs	Mean	SD	Obs	Mean	SD	Obs	Difference
Employed in Non-agriculture - Wage Work	0.11	0.31	23,809	0.05	0.23	11,521	0.16	0.37	12,288	-0.11 (0.00)***
Employed in Non-agriculture - Total	0.13	0.33	23,809	0.06	0.24	11,521	0.19	0.39	12,288	-0.13 (0.01)***
Employed in Industry - Wage Work	0.05	0.22	23,809	0.02	0.13	11,521	0.08	0.28	12,288	-0.06 (0.00)***
Employed in Industry	0.06	0.23	23,809	0.02	0.14	11,521	0.09	0.29	12,288	-0.07 (0.00)***
Employed in Services - Wage Work	0.06	0.24	23,809	0.04	0.19	11,521	0.08	0.27	12,288	-0.04 (0.00)***
Employed in Services	0.07	0.26	23,809	0.04	0.20	11,521	0.10	0.30	12,288	-0.06 (0.00)***
Employed in Agriculture - Wage Work	0.01	0.09	23,809	0.01	0.08	11,521	0.01	0.10	12,288	0.00 (0.00)***
Employed in Agriculture	0.05	0.22	23,809	0.04	0.20	11,521	0.06	0.24	12,288	-0.02 (0.00)***
Works for a Wage	0.12	0.32	23,809	0.06	0.24	11,521	0.17	0.38	12,288	-0.11 (0.01)***
Seasonal or Temporary Work for a Wage	0.04	0.21	23,809	0.03	0.16	11,521	0.06	0.24	12,288	-0.03 (0.00)***
Log Hours of Wage Work (per week)	0.46	1.25	23,809	0.23	0.90	11,521	0.67	1.48	12,288	-0.45 (0.02)***
Log Hours of Work (per Week)	0.65	1.43	23,809	0.35	1.08	11,521	0.93	1.64	12,288	-0.58 (0.02)***
Not in Education, Employment, or Training (NEET)	0.08	0.28	23,809	0.11	0.31	11,521	0.06	0.24	12,288	0.04 (0.00)***

Panel C: Covariates

	Overall sample			Female sample			Male sample			Female - Male
	Mean	SD	Obs	Mean	SD	Obs	Mean	SD	Obs	Difference
Mother completed primary school	0.68	0.47	23,809	0.69	0.46	11,521	0.67	0.47	12,288	0.02 (0.01)**
Father completed primary school	0.80	0.40	23,809	0.80	0.40	11,521	0.80	0.40	12,288	0.00 (0.01)
Mother completed junior high school	0.17	0.37	23,809	0.17	0.38	11,521	0.17	0.37	12,288	0.00 (0.01)
Father completed junior high school	0.31	0.46	23,809	0.32	0.47	11,521	0.31	0.46	12,288	0.01 (0.01)
Household size	4.22	1.43	23,809	4.23	1.44	11,521	4.21	1.41	12,288	0.01 (0.03)

Notes: The table presents the mean, standard deviation, and number of observations from the the 2014 Household Labor Force Survey of Turkey. The statistics for the variables reported with [1] come from the 2013 Household Labor Force Survey. The distance high school attendance is available in the 2013 Household Labor Force Survey, but not in the 2014 Household Labor Force Survey. 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 / statistics	(1) Linear RD ĥ bandwidth	(2) Quadratic RD ĥ bandwidth	(3) Linear RD 0.75 ĥ bandwidth	(4) Linear RD 1.5 ĥ bandwidth	(5) Linear RD 23 bandwidth	(6) Mean	(7) Bandwidth (ĥ)	(8) Observations
High school attendance	0.032***	0.051**	0.024**	0.037***	0.038***	0.84	20	23,809
	(0.011)	(0.023)	(0.010)	(0.009)	(0.010)			
Vocational high school attendance	0.044***	0.040**	0.048***	0.060***	0.051***	0.37	26	27,737
	(0.013)	(0.017)	(0.013)	(0.011)	(0.013)			
Academic high school attendance	-0.019	0.009	-0.005	-0.024**	-0.006	0.46	28	29,171
	(0.011)	(0.019)	(0.013)	(0.012)	(0.013)			

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 ĥ, respectively. Columns (3) and (4) report local RD regressions with a linear control function using optimal bandwidth 0.75 ĥ and 1.5 ĥ, 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 for the type of education respondent's father and mother has completed (no schooling, completed primary, junior-high or a higher level of school), household size, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level.

TABLE 3: RD TREATMENT EFFECTS ON EDUCATION BY GENDER

Panel A: 2014 HLFS Survey							
	Overall sample	**Female**	**Male**	**Difference**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Linear RD	Linear RD	Linear RD	(2)-(3)	Mean	Bandwidth (ĥ)	Observations
Outcome	ĥ bandwidth	ĥ bandwidth	ĥ bandwidth	p-value	(Overall/Female/Male)		(Overall/Female/Male)
High school attendance	0.032***	0.035**	0.034**	0.96	0.84/0.86/0.83	20	23,809/11,521/12,288
	(0.011)	(0.016)	(0.013)				
Vocational high school attendance	0.050***	0.046**	0.055**	0.79	0.38/0.36/0.39	20	23,809/11,521/12,288
	(0.013)	(0.019)	(0.023)				
Academic high school attendance	-0.010	-0.004	-0.013	0.79	0.46/0.49/0.43	20	23,809/11,521/12,288
	(0.013)	(0.023)	(0.021)				
Panel A: 2013 HLFS Survey							
High school attendance	0.030***	0.032**	0.032***	0.94	0.80/0.80/0.79	2	33,426/16,196/17,230
	(0.002)	(0.006)	(0.002)				
Vocational high school attendance	0.033***	0.039***	0.032***	0.23	0.32/0.30/0.34	2	33,426/16,196/17,230
	(0.002)	(0.004)	(0.001)				
Academic high school attendance	-0.011***	-0.027***	0.007	0.00	0.41/0.44/0.38	2	33,426/16,196/17,230
	(0.002)	(0.002)	(0.004)				
Distance high school attendance	0.002	0.015***	-0.011	0.00	0.07/0.06/0.08	2	33,426/16,196/17,230
	(0.002)	(0.001)	(0.005)				

Notes: Data is from the 2014 Household Labor Force Survey in Panel A and the 2013 Household Labor Force Survey in Panel B. In Panel A, 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. Since the month of birth variable is not available in the 2013 survey, in Panel B each column reports a reduced-form RD treatment effect of being born after 1998 with a linear control function in year of birth on each side of the discontinuity. In Panel A, the bandwidth is 20 months in all regressions, which is the optimal bandwidth ĥ estimated by the Imbens and Kalyanaraman (2009) algorithm when high school attendance is the dependent variable. In Panel B, the bandwidth is 2 years in all regressions, which is the closest number of years that correspond to the optimal monthly bandwidth 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 for the type of education respondent's father and mother has completed (no schooling, completed primary, junior-high or a higher level of school), household size, 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.

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

Outcome / statistics	(1) OLS ĥ bandwidth	(2) Linear RD ĥ bandwidth	(3) Quadratic RD ĥ bandwidth	(4) Linear RD 0.75 ĥ bandwidth	(5) Linear RD 1.5 ĥ bandwidth	(6) Linear RD 23 bandwidth	(7) Linear RD-2SLS ĥ bandwidth	(8) Mean	(9) Bandwidth	(10) Observations
Employed in Non-agriculture:										
Wage Work	-0.235***	-0.020*	-0.071***	-0.035***	-0.020**	-0.017*	-0.619**	0.11	20	23,809
	-0.011	-0.011	-0.016	-0.008	-0.009	-0.01	-0.295			
Total	-0.250***	-0.017	-0.055***	-0.030***	-0.018**	-0.017	-0.546*	0.13	20	23,809
	-0.013	-0.011	-0.015	-0.009	-0.009	-0.01	-0.31			
Employed in Industry:										
Wage Work	-0.149***	-0.007	-0.042***	-0.018**	-0.007	-0.008	-0.227	0.05	20	23,809
	(0.011)	(0.008)	(0.013)	(0.008)	(0.008)	(0.008)	(0.236)			
Total	-0.150***	-0.007	-0.037***	-0.019**	-0.008	-0.009	-0.230	0.06	20	23,809
	(0.011)	(0.008)	(0.012)	(0.008)	(0.007)	(0.007)	(0.231)			
Employed in Services:										
Wage Work	-0.086***	-0.012	-0.029**	-0.017**	-0.013**	-0.009	-0.391	0.06	20	23,809
	-0.009	-0.008	-0.012	-0.007	-0.006	-0.008	-0.264			
Total	-0.100***	-0.01	-0.019	-0.010	-0.010	-0.007	-0.316	0.07	20	23,809
	-0.01	-0.008	-0.011	-0.007	-0.006	-0.008	-0.247			
Employed in Agriculture:										
Wage Work	-0.019***	-0.004	-0.005	-0.009***	-0.004*	-0.004*	-0.134	0.01	20	23,809
	(0.004)	(0.003)	(0.005)	(0.002)	(0.002)	(0.002)	(0.107)			
Total	-0.079***	-0.001	-0.014	0.003	0.001	-0.000	-0.024	0.05	20	23,809
	(0.007)	(0.005)	(0.010)	(0.006)	(0.005)	(0.006)	(0.162)			
Works for a Wage	-0.254***	-0.024**	-0.076***	-0.044***	-0.023**	-0.021**	-0.752**	0.12	20	23,809
	(0.010)	(0.011)	(0.015)	(0.008)	(0.010)	(0.010)	(0.339)			
Seasonal or Temporary Work	-0.026***	-0.023***	-0.023***	-0.021***	-0.022***	-0.020***	-0.738***	0.04	20	23,809
for a Wage	(0.007)	(0.005)	(0.008)	(0.006)	(0.005)	(0.005)	(0.284)			
Log Hours of Work:										
Wage Work	-1.040***	-0.083*	-0.298***	-0.167***	-0.078**	-0.076*	-2.632**	0.46	20	23,809
	(0.041)	(0.043)	(0.057)	(0.031)	(0.038)	(0.041)	(1.313)			
Total	-1.317***	-0.064	-0.267***	-0.107**	-0.051	-0.058	-2.021	0.65	20	23,809
	(0.043)	(0.050)	(0.073)	(0.040)	(0.040)	(0.047)	(1.388)			
Not in Education, Employment,	-0.529***	-0.016**	-0.004	-0.004	-0.016**	-0.019**	-0.508***	0.08	20	23,809
or Training (NEET)	-0.013	-0.007	-0.012	-0.007	-0.008	-0.008	-0.179			

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 ĥ 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 ĥ and 1.5 ĥ 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 are a dummy variable equal to one if the respondent is employed in the non-agricultural sector working for a wage, a dummy variable equal to one if the respondent is employed in the non-agricultural sector (working for a wage or unpaid), a dummy variable equal to one if the respondent is employed in the industrial sector working for a wage, a dummy variable equal to one if the respondent is employed in the industrial sector (working for a wage or unpaid), a dummy variable equal to one if the respondent is employed in the service sector working for a wage, a dummy variable equal to one if the respondent is employed in the service sector (working for a wage or unpaid), a dummy variable equal to one if the respondent is employed in the agricultural sector working for a wage, a dummy variable equal to one if the respondent is employed in the agricultural sector (working for a wage or unpaid), a dummy variable equal to one if the respondent works for a wage, a dummy variable equal to one if the respondent works for a wage in a seasonal or temporary job, the log hours of work per week that the respondent works for a wage (taking the value of zero if the respondent does not work), the log hours of work per week that the respondent works in total (taking the value of zero if the respondent does not work), and a dummy variable equal to one if the respondent is idle (not in education, employment, or training, i.e. neet). All specifications control for a set of dummy variables for the type of education respondent's father and mother has completed (no schooling, completed primary, junior-high or a higher level of school), household size, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level.

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

	Overall sample		**Female**		**Male**		**Difference**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Linear RD	Linear RD-2SLS	Linear RD	Linear RD-2SLS	Linear RD	Linear RD-2SLS	(3)-(5)	Mean	Bandwidth	Observations
Outcome / statistics	ĥ bandwidth	ĥ bandwidth	ĥ bandwidth	ĥ bandwidth	ĥ bandwidth	ĥ bandwidth	p-value	(Overall/Female/Male)		(Overall/Female/Male)
Employed in Non-agriculture:										
Wage Work	-0.020*	-0.619**	-0.014	-0.413	-0.033**	-0.983***	0.27	0.11/0.05/0.16	20	23,809/11,521/12,288
	(0.011)	(0.295)	(0.013)	(0.390)	(0.015)	(0.341)				
Total	-0.017	-0.546*	-0.013	-0.363	-0.032**	-0.946***	0.29	0.13/0.06/0.19	20	23,809/11,521/12,288
	(0.011)	(0.310)	(0.014)	(0.412)	(0.015)	(0.354)				
Employed in Industry:										
Wage Work	-0.007	-0.227	0.005	0.134	-0.025*	-0.732**	0.03	0.05/0.02/0.08	20	23,809/11,521/12,288
	(0.008)	(0.236)	(0.008)	(0.263)	(0.013)	(0.312)				
Total	-0.007	-0.230	0.007	0.200	-0.028**	-0.827**	0.01	0.06/0.02/0.09	20	23,809/11,521/12,288
	(0.008)	(0.231)	(0.008)	(0.275)	(0.012)	(0.337)				
Employed in Services:										
Wage Work	-0.012	-0.391	-0.019**	-0.548*	-0.008	-0.251	0.37	0.06/0.04/0.08	20	23,809/11,521/12,288
	(0.008)	(0.264)	(0.007)	(0.329)	(0.012)	(0.356)				
Total	-0.010	-0.316	-0.019**	-0.562	-0.004	-0.119	0.22	0.07/0.04/0.10	20	23,809/11,521/12,288
	(0.008)	(0.247)	(0.008)	(0.365)	(0.012)	(0.345)				
Employed in Agriculture:										
Wage Work	-0.004	-0.134	-0.005	-0.144	-0.004	-0.119	0.87	0.01/0.01/0.01	20	23,809/11,521/12,288
	(0.003)	(0.107)	(0.003)	(0.108)	(0.004)	(0.139)				
Total	-0.001	-0.024	0.002	0.062	-0.003	-0.076	0.63	0.05/0.04/0.06	20	23,809/11,521/12,288
	(0.005)	(0.162)	(0.007)	(0.208)	(0.007)	(0.211)				
Works for a Wage	-0.024**	-0.752**	-0.019	-0.557	-0.037**	-1.102***	0.33	0.12/0.06/0.17	20	23,809/11,521/12,288
	(0.011)	(0.339)	(0.014)	(0.425)	(0.015)	(0.382)				
Seasonal or Temporary Work	-0.023***	-0.738***	-0.027***	-0.772**	-0.021**	-0.627*	0.62	0.04/0.03/0.06	20	23,809/11,521/12,288
for a Wage	(0.005)	(0.284)	(0.006)	(0.362)	(0.008)	(0.320)				
	0.04	0.04	0.03	0.03	0.06	0.06				
Log Hours of Work:										
Wage Work	-0.083*	-2.632**	-0.059	-1.721	-0.142**	-4.222***	0.26	0.46/0.23/0.67	20	23,809/11,521/12,288
	(0.043)	(1.313)	(0.052)	(1.568)	(0.062)	(1.486)				
Total	-0.064	-2.021	-0.035	-1.011	-0.128*	-3.824**	0.20	0.65/0.35/0.93	20	23,809/11,521/12,288
	(0.050)	(1.388)	(0.050)	(1.430)	(0.072)	(1.564)				
Not in Education, Employment,	-0.016**	-0.508***	-0.028**	-0.819***	-0.002	-0.066	0.02	0.08/0.11/0.06	20	23,809/11,521/12,288
or Training (NEET)	(0.007)	(0.179)	(0.011)	(0.167)	(0.007)	(0.191)				

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 ĥ 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 are a dummy variable equal to one if the respondent is employed in the non-agricultural sector working for a wage, a dummy variable equal to one if the respondent is employed in the non-agricultural sector (working for a wage or unpaid), a dummy variable equal to one if the respondent is employed in the industrial sector working for a wage, a dummy variable equal to one if the respondent is employed in the industrial sector (working for a wage or unpaid), a dummy variable equal to one if the respondent is employed in the service sector working for a wage, a dummy variable equal to one if the respondent is employed in the service sector (working for a wage or unpaid), a dummy variable equal to one if the respondent is employed in the agricultural sector working for a wage, a dummy variable equal to one if the respondent is employed in the agricultural sector (working for a wage or unpaid), a dummy variable equal to one if the respondent works for a wage, a dummy variable equal to one if the respondent works for a wage in a seasonal or temporary job, the log hours of work per week that the respondent works for a wage (taking the value of zero if the respondent does not work), the log hours of work per week that the respondent works in total (taking the value of zero if the respondent does not work), and a dummy variable equal to one if the respondent is idle (not in education, employment, or training, i.e. neet). All specifications control for a set of dummy variables for the type of education respondent's father and mother has completed (no schooling, completed primary, junior-high or a higher level of school), household size, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level.

TABLE 6: RD TREATMENT EFFECTS ON EDUCATION AND CHILD LABOR BY PRE-REFORM ENROLLMENT RATE (SHARP RD ESTIMATES)

	Full Sample			**Low Pre-Reform Enrollment Sample**				**High Pre-Reform Enrollment Sample**				**Low-High**
	Overall	**Female**	**Male**	**Overall**	**Female**	**Male**	**Female-Male**	**Overall**	**Female**	**Male**	**Female-Male**	**Enrollment**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Linear RD	Linear RD	Linear RD	Linear RD	Linear RD	Linear RD	Difference	Linear RD	Linear RD	Linear RD	Difference	Difference
Outcome / statistics	h bandwidth	h bandwidth	h bandwidth	h bandwidth	h bandwidth	h bandwidth	(5)-(6), p-value	h bandwidth	h bandwidth	h bandwidth	(9)-(10), p-value	(4)-(8), p-value
High school attendance	0.032***	0.035**	0.034**	0.049**	0.036	0.059**	0.51	0.016	0.036**	0.009	0.30	0.17
	(0.011)	(0.016)	(0.013)	(0.020)	(0.030)	(0.023)		(0.014)	(0.015)	(0.022)		
Mean	0.84	0.86	0.83	0.80	0.81	0.79		0.88	0.89	0.86		
Vocational high school attendance	0.050***	0.046**	0.055**	0.066***	0.023	0.108***	0.03	0.037	0.070**	0.004	0.14	0.38
	(0.013)	(0.019)	(0.023)	(0.017)	(0.023)	(0.028)		(0.022)	(0.033)	(0.032)		
Mean	0.38	0.36	0.39	0.33	0.33	0.33		0.42	0.39	0.44		
Academic high school attendance	-0.010	-0.004	-0.013	-0.009	0.023	-0.045**	0.04	-0.012	-0.029	0.017	0.33	0.91
	(0.013)	(0.023)	(0.021)	(0.019)	(0.031)	(0.020)		(0.018)	(0.033)	(0.028)		
Mean	0.46	0.49	0.43	0.46	0.48	0.45		0.46	0.50	0.42		
Employed in Non-agriculture: Wage Work	-0.020*	-0.014	-0.033**	-0.033***	-0.021**	-0.042**	0.21	-0.011	-0.012	-0.025	0.70	0.22
	(0.011)	(0.013)	(0.015)	(0.012)	(0.008)	(0.018)		(0.015)	(0.021)	(0.024)		
Mean	0.11	0.05	0.16	0.09	0.03	0.15		0.13	0.08	0.17		
Employed in Non-agriculture: Total	-0.017	-0.013	-0.032**	-0.025**	-0.018**	-0.028	0.57	-0.014	-0.011	-0.035	0.48	0.56
	(0.011)	(0.014)	(0.015)	(0.012)	(0.009)	(0.018)		(0.016)	(0.023)	(0.025)		
Mean	0.13	0.06	0.19	0.11	0.03	0.18		0.14	0.08	0.20		
Employed in Industry: Wage Work	-0.007	0.005	-0.025*	-0.009	0.008	-0.020	0.10	-0.007	-0.000	-0.027	0.18	0.93
	(0.008)	(0.008)	(0.013)	(0.008)	(0.006)	(0.014)		(0.014)	(0.014)	(0.019)		
Mean	0.05	0.02	0.08	0.04	0.01	0.07		0.06	0.03	0.09		
Employed in Industry: Total	-0.007	0.007	-0.028**	-0.006	0.011*	-0.018	0.08	-0.009	0.002	-0.035*	0.09	0.86
	(0.008)	(0.008)	(0.012)	(0.007)	(0.007)	(0.014)		(0.013)	(0.014)	(0.020)		
Mean	0.06	0.02	0.09	0.05	0.01	0.08		0.07	0.03	0.10		
Employed in Services: Wage Work	-0.012	-0.019**	-0.008	-0.025**	-0.029***	-0.022	0.69	-0.003	-0.012	0.003	0.47	0.15
	(0.008)	(0.007)	(0.012)	(0.009)	(0.007)	(0.016)		(0.012)	(0.012)	(0.018)		
Mean	0.06	0.04	0.08	0.05	0.02	0.08		0.07	0.05	0.08		
Employed in Services: Total	-0.010	-0.019**	-0.004	-0.019*	-0.029***	-0.010	0.27	-0.005	-0.013	-0.000	0.59	0.38
	(0.008)	(0.008)	(0.012)	(0.010)	(0.008)	(0.017)		(0.012)	(0.015)	(0.019)		
Mean	0.07	0.04	0.10	0.06	0.03	0.10		0.08	0.05	0.10		
Employed in Agriculture: Wage Work	-0.004	-0.005	-0.004	-0.006	-0.004	-0.010	0.61	-0.002	-0.006**	0.002	0.01	0.37
	(0.003)	(0.003)	(0.004)	(0.005)	(0.007)	(0.008)		(0.002)	(0.003)	(0.002)		
Mean	0.01	0.01	0.01	0.01	0.01	0.02		0.00	0.00	0.00		
Employed in Agriculture: Total	-0.001	0.002	-0.003	0.005	0.005	0.008	0.85	-0.006	-0.000	-0.011	0.23	0.26
	(0.005)	(0.007)	(0.007)	(0.009)	(0.010)	(0.014)		(0.005)	(0.007)	(0.007)		
Mean	0.05	0.04	0.06	0.08	0.06	0.09		0.02	0.02	0.03		
Works for a Wage	-0.024**	-0.019	-0.037**	-0.040***	-0.025**	-0.051***	0.12	-0.013	-0.018	-0.023	0.89	0.12
	(0.011)	(0.014)	(0.015)	(0.012)	(0.010)	(0.017)		(0.015)	(0.022)	(0.024)		
Mean	0.12	0.06	0.17	0.11	0.04	0.17		0.13	0.08	0.18		
Seasonal or Temporary Work for Wage	-0.023***	-0.027***	-0.021**	-0.026***	-0.024***	-0.028**	0.75	-0.021***	-0.030***	-0.014	0.35	0.68
	(0.005)	(0.006)	(0.008)	(0.008)	(0.007)	(0.012)		(0.006)	(0.009)	(0.011)		
Mean	0.04	0.03	0.06	0.05	0.02	0.07		0.04	0.03	0.05		
Log Hours of Work: Wage Work	-0.083*	-0.059	-0.142**	-0.147***	-0.084**	-0.198***	0.09	-0.037	-0.054	-0.082	0.82	0.11
	(0.043)	(0.052)	(0.062)	(0.047)	(0.039)	(0.067)		(0.059)	(0.083)	(0.094)		
Mean	0.46	0.23	0.67	0.43	0.15	0.68		0.48	0.29	0.67		
Log Hours of Work: Total	-0.064	-0.035	-0.128*	-0.081	-0.048	-0.086	0.64	-0.059	-0.037	-0.157	0.38	0.76
	(0.050)	(0.050)	(0.072)	(0.058)	(0.037)	(0.083)		(0.063)	(0.088)	(0.102)		
Mean	0.65	0.35	0.93	0.70	0.34	1.03		0.61	0.36	0.84		
Not in Education, Employment, or Training	-0.016**	-0.028**	-0.002	-0.016	-0.033	-0.002	0.22	-0.016*	-0.026**	-0.004	0.12	0.98
(NEET)	(0.007)	(0.011)	(0.007)	(0.017)	(0.026)	(0.013)		(0.008)	(0.012)	(0.010)		
Mean	0.08	0.11	0.06	0.11	0.14	0.08		0.06	0.07	0.05		
Bandwidth	20	20	20	20	20	20		20	20	20		
Observations	23,809	11,521	12,288	13,274	6,419	6,855		10,535	5,102	5,433		

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 ĥ estimated by the Imbens and Kalyanaraman (2009) algorithm when high school attendance is the dependent variable. Columns (1) - (3) report results for the full sample, columns (4) - (7) report results for the low pre-reform enrollment sample, and columns (8) - (11) report results for the low pre-reform enrollment sample. Within each sample, the first column reports results for the overall sample, the second one reports them for females, and the third one reports them for males. All regressions use a sharp RD specification, reporting 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. Column (7) reports the p-value for the SUR test of equality between treatment effects for subsamples of males and females reported in columns (5) and (6), and column (11) does the same for results reported in columns (9) and (10). Column (12) reports the p-value for the SUR test of equality between treatment effects for subsamples of low and high pre-reform enrollment samples reported in columns (4) and (8). The dependent variables are a dummy variable equal to one if the respondent is employed in the non-agricultural sector working for a wage, a dummy variable equal to one if the respondent is employed in the non-agricultural sector (working for a wage or unpaid), a dummy variable equal to one if the respondent is employed in the industrial sector working for a wage, a dummy variable equal to one if the respondent is employed in the industrial sector (working for a wage or unpaid), a dummy variable equal to one if the respondent is employed in the service sector working for a wage, a dummy variable equal to one if the respondent is employed in the service sector (working for a wage or unpaid), a dummy variable equal to one if the respondent is employed in the agricultural sector working for a wage, a dummy variable equal to one if the respondent is employed in the agricultural sector (working for a wage or unpaid), a dummy variable equal to one if the respondent works for a wage, a dummy variable equal to one if the respondent works for a wage in a seasonal or temporary job, the log hours of work per week that the respondent works for a wage (taking the value of zero if the respondent does not work), the log hours of work per week that the respondent works in total (taking the value of zero if the respondent does not work), and a dummy variable equal to one if the respondent is idle (not in education, employment, or training, i.e. neet). All specifications control for a set of dummy variables for the type of education respondent's father and mother has completed (no schooling, completed primary, junior-high or a higher level of school), household size, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level.

TABLE 7: RD TREATMENT EFFECTS OF EDUCATION ON CHILD LABOR BY PRE-REFORM ENROLLMENT RATE (FUZZY RD ESTIMATES)

	Full Sample			**Low Pre-Reform Enrollment Sample**			**High Pre-Reform Enrollment Sample**		
	Overall	**Female**	**Male**	**Overall**	**Female**	**Male**	**Overall**	**Female**	**Male**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Linear RD-2SLS	Linear RD-2SLS	Linear RD-2SLS	Linear RD-2SLS	Linear RD-2SLS	Linear RD-2SLS	Linear RD-2SLS	Linear RD-2SLS	Linear RD-2SLS
Outcome / statistics	h bandwidth	h bandwidth	h bandwidth	h bandwidth	h bandwidth	h bandwidth	h bandwidth	h bandwidth	h bandwidth
Employed in Non-agriculture: Wage Work	-0.619**	-0.413	-0.983***	-0.674**	-0.575	-0.703***	-0.648	-0.345	-2.769
	(0.295)	(0.390)	(0.341)	(0.296)	(0.594)	(0.258)	(0.771)	(0.582)	(5.364)
Mean	0.11	0.05	0.16	0.09	0.03	0.15	0.13	0.08	0.17
Employed in Non-agriculture: Total	-0.546*	-0.363	-0.946***	-0.512**	-0.503	-0.472**	-0.842	-0.312	-3.987
	(0.310)	(0.412)	(0.354)	(0.260)	(0.567)	(0.232)	(0.831)	(0.634)	(8.307)
Mean	0.13	0.06	0.19	0.11	0.03	0.18	0.14	0.08	0.20
Employed in Industry: Wage Work	-0.227	0.134	-0.732**	-0.174	0.215	-0.334*	-0.435	-0.006	-3.088
	(0.236)	(0.263)	(0.312)	(0.158)	(0.205)	(0.193)	(0.786)	(0.387)	(6.917)
Mean	0.05	0.02	0.08	0.04	0.01	0.07	0.06	0.03	0.09
Employed in Industry: Total	-0.230	0.200	-0.827**	-0.126	0.304	-0.299	-0.556	0.044	-3.947
	(0.231)	(0.275)	(0.337)	(0.150)	(0.245)	(0.187)	(0.795)	(0.388)	(9.140)
Mean	0.06	0.02	0.09	0.05	0.01	0.08	0.07	0.03	0.10
Employed in Services: Wage Work	-0.391	-0.548*	-0.251	-0.499**	-0.791	-0.370	-0.213	-0.339	0.319
	(0.264)	(0.329)	(0.356)	(0.233)	(0.707)	(0.288)	(0.661)	(0.351)	(2.432)
Mean	0.06	0.04	0.08	0.05	0.02	0.08	0.07	0.05	0.08
Employed in Services: Total	-0.316	-0.562	-0.119	-0.386*	-0.807	-0.173	-0.286	-0.356	-0.040
	(0.247)	(0.365)	(0.345)	(0.212)	(0.739)	(0.273)	(0.656)	(0.418)	(2.011)
Mean	0.07	0.04	0.10	0.06	0.03	0.10	0.08	0.05	0.10
Employed in Agriculture: Wage Work	-0.134	-0.144	-0.119	-0.131	-0.111	-0.162	-0.140	-0.172**	0.184
	(0.107)	(0.108)	(0.139)	(0.130)	(0.208)	(0.153)	(0.184)	(0.085)	(0.447)
Mean	0.01	0.01	0.01	0.01	0.01	0.02	0.00	0.00	0.00
Employed in Agriculture: Total	-0.024	0.062	-0.076	0.103	0.140	0.143	-0.340	-0.006	-1.237
	(0.162)	(0.208)	(0.211)	(0.174)	(0.344)	(0.249)	(0.451)	(0.206)	(3.159)
Mean	0.05	0.04	0.06	0.08	0.06	0.09	0.02	0.02	0.03
Works for a Wage	-0.752**	-0.557	-1.102***	-0.804**	-0.686	-0.865***	-0.788	-0.518	-2.585
	(0.339)	(0.425)	(0.382)	(0.375)	(0.701)	(0.299)	(0.785)	(0.602)	(5.041)
Mean	0.12	0.06	0.17	0.11	0.04	0.17	0.13	0.08	0.18
Seasonal or Temporary Work for Wage	-0.738***	-0.772**	-0.627*	-0.521**	-0.655	-0.469*	-1.309	-0.840*	-1.599
	(0.284)	(0.362)	(0.320)	(0.263)	(0.583)	(0.251)	(1.144)	(0.448)	(4.142)
Mean	0.04	0.03	0.06	0.05	0.02	0.07	0.04	0.03	0.05
Log Hours of Work: Wage Work	-2.632**	-1.721	-4.222***	-2.989**	-2.313	-3.349***	-2.251	-1.501	-9.257
	(1.313)	(1.568)	(1.486)	(1.453)	(2.478)	(1.207)	(3.021)	(2.260)	(17.483)
Mean	0.46	0.23	0.67	0.43	0.15	0.68	0.48	0.29	0.67
Log Hours of Work: Total	-2.021	-1.011	-3.824**	-1.646	-1.317	-1.448	-3.608	-1.045	-17.650
	(1.388)	(1.430)	(1.564)	(1.181)	(1.534)	(1.151)	(3.345)	(2.418)	(37.037)
Mean	0.65	0.35	0.93	0.70	0.34	1.03	0.61	0.36	0.84
Not in Education, Employment, or Training	-0.508***	-0.819***	-0.066	-0.332	-0.912***	-0.027	-0.983	-0.737***	-0.421
(NEET)	(0.179)	(0.167)	(0.191)	(0.226)	(0.225)	(0.216)	(0.684)	(0.260)	(1.050)
Mean	0.08	0.11	0.06	0.11	0.14	0.08	0.06	0.07	0.05
Bandwidth	20	20	20	20	20	20	20	20	20
Observations	23,809	11,521	12,288	13,274	6,419	6,855	10,535	5,102	5,433

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 ĥ estimated by the Imbens and Kalyanaraman (2009) algorithm when high school attendance is the dependent variable. Columns (1) - (3) report results for the full sample, columns (4) - (6) report results for the low pre-reform enrollment sample, and columns (7) - (9) report results for the low pre-reform enrollment sample. Within each sample, the first column reports results for the overall sample, the second one reports them for females, and the third one reports them for males. All regressions use a fuzzy RD specification, reporting 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 are a dummy variable equal to one if the respondent is employed in the non-agricultural sector working for a wage, a dummy variable equal to one if the respondent is employed in the non-agricultural sector (working for a wage or unpaid), a dummy variable equal to one if the respondent is employed in the industrial sector working for a wage, a dummy variable equal to one if the respondent is employed in the industrial sector (working for a wage or unpaid), a dummy variable equal to one if the respondent is employed in the service sector working for a wage, a dummy variable equal to one if the respondent is employed in the service sector (working for a wage or unpaid), a dummy variable equal to one if the respondent is employed in the agricultural sector working for a wage, a dummy variable equal to one if the respondent is employed in the agricultural sector (working for a wage or unpaid), a dummy variable equal to one if the respondent works for a wage, a dummy variable equal to one if the respondent works for a wage in a seasonal or temporary job, the log hours of work per week that the respondent works for a wage (taking the value of zero if the respondent does not work), the log hours of work per week that the respondent works in total (taking the value of zero if the respondent does not work), and a dummy variable equal to one if the respondent is idle (not in education, employment, or training, i.e. neet). All specifications control for a set of dummy variables for the type of education respondent's father and mother has completed (no schooling, completed primary, junior-high or a higher level of school), household size, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level.