Compulsory Schooling for Whom? The Role of Gender, Poverty, and Religiosity*

Bilge Erten† Pinar Keskin‡

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Abstract

We exploit an extension of compulsory schooling in Turkey to identify which population segments that otherwise would not have attended high school complied with the law and which type of schools they chose to attend. By adopting a regression discontinuity design, we find that the reform increased high school attendance for both boys and girls. The main compliers with the reform among boys were those who would have participated in paid employment prior to the change in law. Conversely, female compliers would have likely not been in education, employment, or training (NEET), or they could have been employed in unpaid work. Although regional poverty rates do not affect the compliance rates for boys or girls, we find that the reform had a positive impact on girls' high school attendance only in more religiously conservative regions, and that the NEET status of girls in these regions declined. Finally, we find that the marginal students chose to attend vocational high schools, as opposed to academic high schools. We provide some suggestive evidence showing that the increase in vocational school attendance was not driven by an increase in the supply of these schools.

JEL Classification: I25, J16, J22

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†Department of Economics, 43 Leon Street, 312A Lake Hall, Northeastern University, Boston, MA 02115. Phone (office): (617) 373 6275. b.erten@northeastern.edu.

‡Address for Correspondence: Department of Economics, Pendleton East, Wellesley College, 106 Central Street, Wellesley, MA 02481. Phone (office): (781) 283-2438. pinar.keskin@wellesley.edu.

1 Introduction

Early school dropout remains an important problem that has adverse lifetime economic and health consequences in both advanced and developing countries. In the United States, 6.5% of 16-to-24 year olds have dropped out of school without receiving a high school diploma, with particularly high rates of dropout for Hispanic (10.6%) and black youth (7.4%) (U.S. Department of Education, 2015). In the European Union, while the average dropout rate was 11% in 2015, several Southern European countries have alarming rates: Spain 20%, Italy 14%, and Portugal 13%, for example. Early school dropout rates are much higher in developing countries, particularly in low-income countries with a high prevalence of child and adolescent labor (UNESCO, 2010).

The costs of high school dropout are strikingly high. In the U.S. alone, the average cost of an individual high school dropout is $250,000 over his or her lifetime in terms of lower tax contributions, greater reliance on Medicaid and Medicare, and higher rates of criminal activity (Levin and Belfield, 2007). High school dropouts are more likely to have lower lifetime wealth and report being in poor health, unemployed, and unhappy (Oreopoulos, 2007). They are also more likely to face incarceration (Lochner and Moretti, 2004). The estimated savings from reduced crime associated with a one-percent increase in high school graduation rates is $1.6 billion annually (Brunello and De Paola, 2013). Therefore, the goal of reducing dropout rates has been a priority area, as demonstrated by the "No Child Left Behind Act" (2001) in the United States and the "Lisbon Summit" (2000) and the "Europe 2020" strategic plans in the European Union.[1]

In most countries, compulsory schooling laws are a commonly employed policy tool to prevent early school dropout. However, the impact of compulsory schooling on individual students is likely to display substantial heterogeneity. In developing countries, for instance, low

[1]The "No Child Left Behind Act" (2001) targeted an average high school graduation rate of 90 percent, while the "Lisbon Summit" (2000) declared a commitment to achieve a minimum of 85% completion of upper secondary education, and the "Europe 2020" plan targeted a maximum 10% high school dropout rate by 2012 (European Commission, 2008).

returns to education and tight credit constraints often lead to significant gender differences in adolescents' access to schooling. In particular, the lack of paid employment opportunities and the presence of cultural norms against girls' participation in the labor force typically lock girls into the household domain, with no possibilities for education, employment, or training.[2] Biased social norms and religious practices may impose additional constraints on the education of girls. Conversely, the availability of job market opportunities for boys in their adolescent years may allow them to gain early work experience with a tangible return and potential lifelong benefits. In other words, any intervention that extends compulsory schooling can potentially take boys out of employment and girls out of the house. From a policy design perspective, evaluating the the costs and benefits associated with compulsory schooling laws requires first understanding who are the main compliers with an education reform.

In this paper, we analyze the heterogeneous effects of a compulsory schooling law in Turkey that made completing high school education mandatory. In April 2012, the Turkish parliament passed a new law to increase compulsory schooling from 8 to 12 years.[3] Since the school starting age overlaps with the calendar year, we expect individuals born after January 1998 to be more likely to register for high school because they were more likely to be 8^{th} graders in the 2011-12 school year. This implies 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 exposure to the compulsory schooling law. In our fuzzy regression discontinuity (RD) design, we assign treatment according to an individual's month and year of birth, with those who were born after January 1998 being assigned to the treated status. We use a nationally representative survey, the 2015 Household Labor Force Survey, which contains detailed information on

[2]Gender gaps in school enrollment generally increase with age (Adukia, 2017; Burde and Linden, 2013).

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

education and labor market outcomes.

Our findings reveal that the reform was successful in increasing the education of both boys and girls. The main compliers with the reform among boys were those who would have participated in paid employment prior to the change in law, whereas female compliers would have likely not been in education, employment, or training (NEET), or they could have been engaged in unpaid work. Our results show that these gender differences remain unchanged regardless of differences in poverty rates across regions. However, we find that the reform had a positive impact on girls' high school attendance only in more religiously conservative regions. We also find that the reform led to a significant decline in the NEET status of girls in more religiously conservative regions, with no evidence of a significant impact on those in other regions. The differences in regional levels of religiosity do not affect the results for boys.

The fact that compulsory schooling dramatically increased female enrollment in more religiously conservative communities highlights the importance of compulsory schooling laws in overcoming cultural barriers to female education by providing necessary incentives to parents. This is particularly salient in contexts where the main compliers are inactive adolescent girls who were previously neither investing in human capital accumulation nor gaining work experience in religiously conservative regions of Turkey.

After identifying the main compliers with the compulsory schooling law, we examine what types of schools the marginal students chose to attend in response to the law. In particular, we examine whether the reform had a differential impact on attending vocational versus academic high schools. Our findings indicate that the main compliers chose to attend vocational high schools as opposed to academic ones. In particular, in poorer and more religiously conservative regions, the vocational high school attendance of girls increased significantly, whereas we find no evidence of a significant impact for academic high schools. For boys, regardless of regional characteristics, the reform led to an increase in vocational high school attendance, while we observe no evidence of a significant impact for attendance

at academic ones. Finally, we have some suggestive evidence that these effects are not operating through regional differences in vocational school supply. Since we assess the effects of compulsory schooling on a large number of outcomes, we adjust the standard errors for multiple hypothesis testing following Simes (1986). Most of our main findings are robust to this adjustment for multiple hypothesis testing.

Our study relates to the extended literature on the causal effects of compulsory schooling laws on returns to education in the labor market (Angrist and Krueger, 1991; Oreopolous, 2006), health outcomes (Lleras-Muney, 2005; Clark and Royer, 2013), and fertility behavior (Black et al., 2008; McCrary and Royer, 2011), among other dimensions. We contribute to this growing literature by offering the first study to examine the immediate effects of compulsory schooling on adolescent labor market outcomes by focusing on identifying the main compliers with the reform. This contrasts with previous studies that evaluate the medium- to long-run effects of compulsory schooling on adult labor market and health outcomes for the treated cohorts.[4]

Second, a growing body of empirical work focuses on policy interventions against cultural barriers preventing girls' access to education. Such barriers may include the parents being less willing to allow their daughters to travel to distant schools, an absence of sex-specific latrines, or a lack of female teachers. In Afghanistan, where the gender gap in primary school enrollment was 17 percentage points in 2007, Burde and Linden (2013) find that placing a school in a village significantly improves academic participation and achievement, particularly for girls. Their findings show that the gains for girls are large enough to remove the gender enrollment gap in treatment villages. Adukia (2017) finds that the construction of sex-specific latrines substantially increases enrollment of adolescent girls in India, and the

[4]Our work also relates to previous studies on the effects of the previous compulsory schooling reform implemented in Turkey in 1997. These studies include, but are not limited to, Cesur and Mocan (2014) and Gulesci and Meyersson (2015), who find a negative effect on women's religiosity, Dincer et al. (2014) and Güneş (2015), who find a negative effect on fertility and child mortality, and Erten and Keskin (2018), who find no significant impact on physical or sexual violence and a positive impact on psychological violence and financial control behavior.[5] Although our findings complement these studies, our paper differs significantly in its focus on the effects of the 2012 compulsory schooling reform in Turkey on high school continuation and adolescent labor market outcomes and its heterogeneous effects by gender, poverty status, and religiosity.

author highlights that privacy and safety are particularly relevant for the schooling decisions of girls. Kazianga et al. (2013) assess a "girl-friendly" primary school program in Burkina Faso, which included gender-segregated latrines, access to clean water, an increased presence of female teachers, and the provision of take-home rations for girls conditional on 90% attendance. Their findings also show a larger increase in girls' enrollment compared to boys and that the unique characteristics of the schools accounted for the entire difference in treatment effects by gender. Meyersson (2014) finds that having a municipal mayor affiliated with the Islamic party increased female schooling in Turkey by increasing education facilities provided by religious foundations and relaxing the implementation of the headscarf ban in classrooms. Our study contributes to this growing literature by analyzing the heterogeneous effects of compulsory schooling using the pre-reform Islamic vote shares as a proxy for religious preferences, and showing that incentives provided by compulsory schooling laws help to alleviate the cultural barriers facing girls' access to education.

Finally, our work joins a large literature that examines the causal effects of school attendance on child labor using exogenous variation from unconditional cash transfers (Edmonds and Schady, 2012; de Carvalho Filho, 2012), conditional cash transfers (Benedetti et al., 2016; Del Carpio et al., 2016), or enrollment subsidies and scholarships (Ravallion and Wodon, 2000; Edmonds and Shrestha, 2014). A common finding in these studies is that cash transfers generate greater reductions in paid economic activities among boys than among girls, while they generate a significant decline in the performance of household chores by girls (Edmonds, 2006; de Hoop and Rosati, 2014). Moreover, the unconditional cash transfers result in a smaller increase in school enrollment than do conditional cash transfers (Baird et al., 2011), and parents generally prefer to send their sons rather than their daughters to school in the absence of conditionality (Akresh et al., 2013). Our main contribution to this literature is to examine the effects of high school attendance on an older group of children, adolescents, using exogenous variation from a compulsory schooling law. One advantage of our setup is the absence of a positive income effect for the treatment group, which is present

in cash-transfer programs and might make it more difficult to disentangle the underlying mechanisms of income versus time constraints facing child labor allocation.

2 The 12-Year Compulsory Schooling Policy in Turkey

In April 2012, the Turkish parliament 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 and replaced with one for successfully completing twelfth grade. The option to attend religious junior high schools, which had been removed by the 1997 compulsory schooling law that required eight years of secular education, was reinstated. An additional option to attend distance education programs after eighth grade was included.[6]

The law on school starting age in Turkey implies that a child begins mandatory schooling in September of the year when he/she turns 6 years old. The 2012 compulsory schooling law, which became effective in the 2012-2013 school year, made twelve years of education mandatory. Thus, if a student had completed eighth grade in 2012, he/she could drop out. In contrast, if a student had completed seventh grade in 2012, he/she was obliged to continue school through eighth grade. The combination of the school starting age law and the 2012 compulsory schooling law implied that children born before January 1998 could drop out after eight years, whereas those born after January 1998 had to complete

[6]The legislative process of the 2012 compulsory schooling reform brought many challenges and faced substantive criticism. Several think tanks and NGOs in Turkey raised concerns regarding the shortage of resources allocated to investments in education, which could possibly hinder the effective implementation of the proposed change. The media widely covered criticism from the opposition party and independent observers. "The 4+4+4 Tension," *Milliyet*, Feb 24, 2012, available at `http://www.milliyet.com.tr/4-4-4-gerilimi/siyaset/siyasetdetay/24.02.2012/1507022/default.htm`. The Economic Research Foundation of Turkey (TEPAV), an active NGO in Turkey, published a report that projected that the Education Ministry would have to raise spending by 54% to successfully implement the reform. "The cost of 4+4+4 is 20.7 billion Turkish lira," TEPAV, Mar 27, 2012, available at `http://www.tepav.org.tr/tr/haberler/s/2765`. Another NGO that specializes in education policy in Turkey, the Education Reform Initiative associated with Sabanci University, highlighted similar issues regarding the lack of resources and unintended consequences of the reform. "Transition to 4+4+4: Notes on September 2012 Policy Changes," available at `http://www.egitimreformugirisimi.org/tr/node/77`.

twelve years of education. Although some cases might not have completely fit this rule because of imperfect compliance with the school starting age or grade repetition, the official requirements were such that students born after January 1998 were more likely to comply with the new compulsory schooling law than the older cohorts were.[7]

Overall, the 2012 Compulsory Schooling Law was successful in substantially raising new enrollments of students in high school. Panel A of Figure 1 shows a significant increase in the total number of new high school registrations after the 2011-2012 school year when the compulsory schooling law began to be implemented. Three years after the policy change, new high school registrations 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). Panel B of Figure 1 shows that the transition rates – the ratio of the number of new high school registrations to the number of graduates from primary education in a given year – rose steadily between 2011 and 2014: from 84% to 94% for girls and from 86% to 95% for boys. These trends highlight that the expansion of compulsory schooling has been accompanied by a substantial increase in enrollments in high school education.

Finally, Figure 2 illustrates the differences in student enrollments and transition rates by type of high school. In Panel A, we observe that the new registrations to vocational high school has increased for both boys and girls during the implementation of the reform starting in 2011-2012 school year, whereas there was not much of an increase for enrollments to academic high school. The transition rates from grade 8 to 9 shown in Panel B also confirm these trends, indicating that much of the increase in transition rates was driven by transitions to vocational high schools. In subsequent analysis, we provide evidence in Table 6 using micro data that confirm these aggregate trends from administrative data.

[7]Notably, as Cesur and Mocan (2014) explain in greater detail, Turkish students who are 72 months old by the end of a calendar year can start school in September of that year (Resmi Gazete, Number 21308).

3 Data and Empirical Methodology

3.1 Data

We use data from Turkey's Household Labor Force Survey (HLFS) of 2015, which is a nationally representative household survey conducted with 149,615 households. The survey includes information on school attendance, type of school attended, NEET status, employment status, and number of hours worked. The dataset includes information on the type of high schools students attend, including vocational high schools that train students for a particular trade or profession and academic high schools that provide students with a foundation in general academic skills, such as math, Turkish, and English.[8] Although the survey includes detailed information on market work, including paid and unpaid employment, it does not have information on the type of domestic work, including household chores.

Table 1 presents summary statistics for adolescents from the 2015 HLFS. We provide summary statistics for individuals between the ages of 15 and 20 because the estimated optimal bandwidths in our local regression analyses fall into this range.[9] Panel A indicates that roughly 83 percent attended high school, of whom 37 percent attended vocational high schools and 45 percent attended academic high schools. On average, the high school attendance rate for girls (84 percent) was slightly higher than that of boys (82 percent), and girls were more likely to attend academic high schools than vocational high schools relative to boys.

In Panel B of Table 1, we report descriptive statistics for the labor market outcomes of 15-to-20 year-olds in our sample. Roughly 17 percent engaged in paid employment and 6 percent participated in unpaid employment, corresponding to a 23 percent total employment rate. Male participation in paid employment (23 percent) was more than twice the female participation in paid employment (10 percent). Among these, temporary paid employment

[8] In this classification, religious high schools are included under vocational high schools since these schools train students to work at a religious institution as an imam or in other related occupations.

[9] We chose the average of optimal bandwidth (30 months) calculated from the first-stage results for high school attendance for boys and girls, which are 27 and 33 months around the cutoff point respectively.

was performed by 8 percent of boys and 4 percent of girls. A higher proportion of boys also participated in unpaid employment (8 percent) compared to girls (5 percent). However, the proportion of girls not in education, employment, or training (12 percent) was twice as high as that of boys (6 percent).[10] These results are consistent with the overall pattern in Turkey, where female labor force participation remains rather low.[11]

The summary statistics for log hours worked reflect similar patterns as do those for participation statistics. In both paid and unpaid employment, girls work fewer hours compared to boys. In turn, they also earn less in terms of monthly earnings from paid employment. In paid employment, the average hours worked per week by boys (11.7 hours) was approximately 2.5 times that of girls (4.6 hours). If we restrict the sample to those participating in paid employment, the average hours worked was 48.8 for boys and 43.5 for girls.

Panel C reports summary statistics on predetermined household characteristics. Roughly 94 percent of mothers and 88 percent of fathers were present in the household. The average household size is 5.5. Females are more likely to live in slightly larger households and less likely to have their parents present in the household, compared to males.

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 drop out after 8 years, as explained in Section 2. We use this break point in a regression discontinuity (RD) design to estimate the causal effect of high school attendance on labor market outcomes. Due to imperfect compliance with the law, our

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

[11]In our entire dataset, average female labor force participation in paid employment is 30%.

empirical strategy is a fuzzy RD design. Our identifying assumption is that these two cohorts born one month apart do not exhibit any systematic differences other than exposure to the compulsory schooling law. Provided that this assumption holds, this setting provides an assignment of treatment that is as good as random. In our RD design, we assign treatment according to the individual's month and year of birth of, with those who were born after January 1998 being assigned to the treated status.

We use a fuzzy 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; Erten and Keskin, 2018; Oreopolous, 2006). Since we are employing a fuzzy RD rather than a sharp RD, our specification is an intent-to-treat (ITT) specification with the following 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 function of the forcing variable on each side of the cutoff point. We allow the slope to vary on each side of the cutoff. We use local linear regressions in our RD estimations (Imbens and Lemiuex, 2008) and adopt the optimal bandwidth selection using the Imbens and Kalyanaraman (2012) routine.[12] This implies the selection of an optimal bandwidth for each outcome variable examined. In addition, we use specifications that adopt the optimal bandwidth from the first-stage results for high school attendance, which is estimated as 54 months around the discontinuity, for the second-stage results. This static bandwidth approach complements the former results, for which we use the optimal bandwidth. We cluster standard errors at the month-year of birth level to accommodate for specification error in the

[12]The key intuition for using an optimal bandwidth approach is that there is a tradeoff between bias and precision: the closer one gets to the cutoff point and narrows down the bandwidth, the smaller the bias will be as the control and treatment groups become more similar; however, the narrower the bandwidth, the lower the precision of estimates due to the smaller sample size (Lee and Lemieux, 2010). The optimal bandwidth algorithms are designed to minimize bias while maximizing precision.

forcing variable, following (Lee and Card, 2008). Since we evaluate the effects of compulsory schooling on a large number of outcomes, we adjust standard errors for multiple hypothesis testing following Simes (1986). Thus, for each outcome variable, we report results based on both standard p-values and p-values adjusted for multiple-hypotheses testing. We include the following control variables in our specifications: a set of dummy variables indicating whether the father is present in the household and whether the mother is present in the household, counts of the number of household members by age and gender present in the household within different age categories, household size fixed effects, month-of-birth fixed effects, and region fixed effects.[13]

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 for 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 a set of dummy variables indicating whether the father is present in the household and whether the mother is present in the household and counts of the number of household members by age and gender present in the household within the age categories of 15-44, 45-64, and 65 and above. The graphs do not indicate any significant jumps at the cutoff point. Overall, we conclude that the predetermined covariates appear to be balanced around the threshold.

[13]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. We also note that the HLFS does not report rural/urban status of the respondents for all years we study in this paper. Hence, we do not control for the type of residency in our regressions.

4 Effects of the Compulsory Schooling Law

4.1 Schooling Outcomes

We begin by testing the effect of the compulsory schooling reform on high school attendance. Figure 4 provides a graphical illustration of the RD design by comparing treatment and placebo effects using the 2015 HLFS and the 2007 HLFS. In Panel A, the left-hand graph plots the average high school attendance rates in monthly bins against the month and year of birth, with a cutoff of January 1998 using the 2015 HLFS data. As described in Section 2, the education reform required those born after this date to attend high school, whereas older cohorts had the option of dropping out after completing junior high school. Local linear smoothers on each side of the cutoff are overlaid on the graph, which shows a significant jump at the discontinuity with an approximately 4-5 percentage-point (ppt) increase in the probability of attending high school. We use data from the 2007 HLFS to conduct a placebo test to examine the validity of the RD design. The right-hand graph in Panel A of Figure 4 shows the same relationship using the 2007 HLFS, where the age cutoff is the same age, comparing 17- and 18-year-old individuals. The same age cutoff corresponds to being born before or after January 1990. The right-hand graph indicates no evidence of a jump in high school attendance for teenagers of the same age in the 2007 HLFS. Hence, the jump observed around the discontinuity of the reform implementation in the 2015 HLFS data is not likely to be driven by some underlying relationship between age and high school attendance but is rather an outcome of the reform. Finally, Figure 4 provides a graphical illustration of RD treatment effects by gender using the 2015 HLFS. In Panel A, we observe that there is evidence of a jump around the cutoff point in high school attendance for both groups. However, the magnitude of the upward shift appears larger for males than for females.

While these graphs reveal a positive RD treatment effect of being exposed to the compulsory schooling reform, the results could be further refined with regression analysis. Using the 2015 HLFS, Table 2 reports the RD treatment effects on high school attendance for all

individuals in the first row and separately for females and males in the second and third rows, respectively. In each row, column 5 displays the optimal bandwidth estimated by the Imbens and Kalyanaraman algorithm in months on each side of the cutoff, and column 6 reports outcome means for the relevant sample. All columns include controls for a dummy variable indicating whether the father is present in the household, a dummy variable indicating whether the mother is present in the household, counts of the number of household members by age and gender present in the household with the age categories of 15-44, 45-64, and 65 and above, household size fixed effects, month-of-birth fixed effects, and region fixed effects.

The first row of Table 2 presents estimates of RD treatment effects on high school attendance for all individuals. The optimal bandwidth, calculated using the Imbens and Kalyanaraman (2012) algorithm, is 54 months around the discontinuity. Using a local linear specification, column 1 presents an RD estimate of 5.4 ppt for the treatment effect on high school attendance, which is statistically significant at the 1 percent level. In terms of magnitude, a 5.4 ppt increase in high school attendance corresponds to a 7 percent increase relative to the mean. For robustness, we include alternative specifications by allowing the bandwidth to vary by restricting it to one-half of, one and one-half times, and twice the optimal bandwidth in columns 2-4. The estimated effects remain significant within the approximate range of 3.9 to 9.2 ppt. The subsequent rows report the RD treatment effects on females and males, respectively. The RD estimates with the optimal bandwidth in column 1 show that the reform led to a 2.9 ppt increase in high school attendance for females and a 5 ppt increase for males. The magnitudes of these effects correspond to 3 and 6 percent increases relative to the respective sample means. In alternative specifications, the RD estimates for these subsamples remain highly significant and positive.[14] In short, the compulsory schooling law had a positive impact on high school attendance of approximately 5 ppt for the whole sam-

[14]The only exception is the RD estimate with half of the optimal bandwidth for males, which is likely driven by the loss of power in the much smaller sample size. The corresponding optimal bandwidth for females is significantly larger, allowing for a more precise estimate.

ple, with moderately larger effects for males than females. An implication is that the fuzzy RD estimates in the two-stage least squares specification will be larger than the sharp RD estimates. In our results, however, we report both of these estimates for comparison.

As a robustness check, Table A2 in Appendix B reports the RD estimates using a static bandwidth of 54 months around the cutoff, which is the optimal bandwidth estimated for the entire sample's high school attendance. The findings in this table are similar to those in Table 2. For both girls and boys, we find that the reform led to a significant increase in high school attendance, and the estimates are robust to alternative specifications with different bandwidths.

4.2 High School Attendance and Labor Market Outcomes

In this section, we test whether the reform had a significant impact on labor market outcomes. Panel B of Figure 4 provides a graphical illustration of the RD treatment effects on the probability of total employment (including both paid and unpaid). The left-hand graph plots the average total employment in monthly bins against the month and year of birth, with a cutoff of January 1998 using the 2015 HLFS data. There is evidence of a clear downward shift at the discontinuity in the probability of total employment. Using data from the 2007 HLFS, we conduct a placebo test to test the validity of the RD design. The right-hand graph in Panel B of Figure 4 shows the same relationship comparing 17- and 18-year-olds using the 2007 HLFS data, for which the same age cutoff corresponds to being born before or after January 1990. The right-hand graph shows no evidence of a jump in being employed for the same age group in the 2007 HLFS. Thus, this is reassuring evidence that the downward shift that we observe around the discontinuity of the reform implementation in the 2015 HLFS data is not resulting from some underlying relationship between age and employment but is rather an consequence of the reform.

Figure 5 provides an illustration of RD treatment effects on labor market outcomes by gender. Panel B indicates some decline in paid employment for both groups at the disconti-

nuity, while the downward shift appears much larger for males. In Panel C, there is evidence of a significant downward shift in unpaid employment for females, while there is no evidence of a significant change for males. Finally, in Panel D, we observe a significant downward shift in the NEET status for females, with no corresponding evidence of a significant change for males.

To examine the effects of the reform on adolescent labor market outcomes in a more refined analysis, we provide the results of RD regressions in Table 3. Columns 1 and 3 present the reduced-form (RF) RD estimates and columns 2 and 4 present the instrumental variables (IV) RD estimates using the cutoff point of January 1998 as an instrument for high school attendance. The comparison of the RD estimates indicate that the reform had heterogeneous effects by gender. First, we find that the reform led to a large decline in boys' participation in paid employment of 4.1 ppt, corresponding to a 17% decline relative to the mean. The IV estimate in column 4 indicates that a 1 ppt increase in high school attendance results in a 0.8 ppt decline in male paid employment. In contrast, we find no evidence of a robust change in female paid employment given the significant RF estimate and insignificant IV estimate reported in the first row. However, we find evidence of a significant negative effect of the reform on paid temporary employment for both boys and girls. The third row RD estimates show that the reform had a significant impact in reducing unpaid employment on family farms or in small family-owned enterprises but only for girls, with no evidence of a significant effect for boys. In terms of magnitude, the reform led to a 1.6 ppt decline in unpaid female employment, corresponding to a 32% decline relative to the sample mean. The RD estimates on hours worked parallel those on participation, implying that the labor market adjustment took place primarily at the extensive margin. Finally, the RD estimates in the last row show that the reform had a significant and negative impact of 2.8 ppt on the NEET status of girls, corresponding to a 22% decline relative to the mean. In contrast, we find no evidence of a significant impact on male NEET status, and the estimates are close to null.

As a robustness check, Table A3 in Appendix B reports the RD estimates using a static bandwidth of 54 months around the cutoff, which is the optimal bandwidth estimated for the entire sample's high school attendance. The findings in this table are similar to those in Table 3. While the RD treatment effects on paid employment for boys are large and statistically significant, those for girls are insignificant and much smaller. In contrast, the RD treatment effects on NEET status and unpaid employment for girls are significant and large, while those for boys are insignificant and small.

Overall, our results indicate that the main compliers with the compulsory schooling reform varied significantly by gender. Among boys, the main compliers were predominantly those who were previously participating in paid employment, whereas among girls, the main compliers were previously either participating in unpaid employment or had NEET status. Hence, by complying with the compulsory schooling law, boys might have lost some early work experience in return for accumulating additional human capital. In contrast, given the absence of returns to unpaid work or being NEET, the net benefits of the reform for main compliers among girls are likely to be positive. In the following subsections, we will further examine this heterogeneity by focusing on the regional disparities in poverty levels and religious preferences that could shed light on the different effects of the reform by gender.

4.3 Heterogeneous Effects by Pre-Reform Regional Poverty Rates

Previous studies have emphasized several reasons why children might attend school less and work more in poorer settings (Edmonds and Schady, 2012; Beegle et al., 2009). At lower levels of income, parents have a lower preference for schooling. As income increases, parents demand a higher level of schooling, which reduces paid employment due to the rigidity of time required in paid work, holding wages constant. As children allocate more time to schooling and away from paid work, unpaid work within the household may increase to the extent that it is more flexible. In models of child labor, it is also common to assume that children work in households in which subsistence income is otherwise not attained (Basu and Van, 1998).

Liquidity constraints may also force poor parents to choose a lower level of schooling than is optimal for their children given the returns to schooling and its opportunity costs (Baland and Robinson, 2000; Edmonds, 2006).

We consider whether the effect of the compulsory schooling law in Turkey varies with pre-reform regional poverty rates. We identify the pre-reform levels of regional poverty using the regional poverty rates in 2011 provided by the Turkish Statistical Institute. Our findings are reported in Table 4. The RD estimates in the first row indicate that the reform had similar effects of approximately 4 to 6 ppt increases in high school attendance in regions that are above and below the median poverty rate. However, it should be noted that in wealthier regions the reform had only a marginally significant effect on female high school attendance. We fail to reject the hypothesis that the RD treatment effects across regions on schooling are statistically different from one another. Nevertheless, the characteristics of main compliers with the reform significantly differ by gender. Regardless of pre-reform differences in regional poverty, the main compliers among girls were those who engaged in unpaid work or were NEET, whereas the main compliers among boys were those who performed paid work.

As a robustness check, Table A4 in Appendix B reports the RD estimates using a static bandwidth of 54 months around the cutoff, which is the optimal bandwidth estimated for the entire sample's high school attendance. The findings in this table are similar to those in Table 4. The RD estimates in the first row show that the reform led to a similar increase in high school attendance in regions with above-median and below-median poverty rates for both boys and girls. While this led to a significant decline in the NEET status of girls and a significant decline in paid employment for boys, the results do not differ significantly by regional poverty rate.

4.4 Heterogeneous Effects by Pre-Reform Regional Islamic Vote Shares

Another possible regional disparity through which the reform may produce different effects by gender is the significant difference in religious preferences across regions. In religiously conservative regions, the barriers to girls' high school education may be particularly severe due to the characteristics of a secular education system, including restrictions on wearing a headscarf, the absence of gender segregation in classes, and the presence of a secular curriculum.[15] Following puberty, parents with strong religious beliefs may prefer their daughters to stay at home and perform household chores instead of continuing into high school in a secular environment. Moreover, more religiously conservative communities also tend to have more conservative gender attitudes, with a view of the world in which women do not perform market work or participate in political activities. These gender attitudes also reduce the perceived returns on educating girls at upper levels, providing additional barriers to entry for girls into high school education.

To examine the heterogeneous effects of the reform with respect to religious preferences, we merge data on Islamic vote shares by region in the 2011 national elections as a measure of Islamic preferences. The Islamic vote shares are calculated as the share of votes received by the pro-Islamist Justice and Development Party (JDP) in each region. We split the sample by the average national Islamic vote share and compare regions that received an above-median share to those that received a below-median share.

Table 5 reports the results. The RD treatment effects in column 1 and 2 show that the reform had a significant positive effect on female high school attendance in more religiously conservative areas, whereas we find no evidence of a significant impact in less religiously conservative regions. The difference between the two groups of regions is also statistically significant at the 5 percent level. In contrast, columns 5 and 6 indicate that the reform

[15]The restrictions on the use of headscarves at schools were removed on September 22, 2014, two years after the implementation of the 2012 Compulsory Schooling Law.

had a significant positive impact on male high school attendance in both groups of regions, regardless of their levels of religious conservatism. The difference between the two groups of regions for boys is not statistically significant. Moreover, we find evidence that the reform led to a significant decline in NEET status for girls in more religiously conservative regions, implying that the main compliers with the reform among girls were those who were staying at home and not participating in education, employment, or training. We find no evidence of a change in the NEET status of girls in less religiously conservative regions. However, in more religiously conservative regions, the reform led to a decline in male paid employment and, to a lesser degree, male unpaid employment, and in less religiously conservative areas, the reform led to a decline only in male paid employment.

As a robustness check, Table A5 in Appendix B reports the RD estimates using a static bandwidth of 54 months around the cutoff, which is the optimal bandwidth estimated for the entire sample's high school attendance. The findings in this table are similar to those in Table 5. The RD estimates in the first row show that the reform's effect on female high school attendance was driven primarily by the increase in more religiously conservative regions. However, there is no evidence of a difference in male high school attendance based on regional levels of religiosity. The results also show that this led to a significant decline in the NEET status of girls in more religiously conservative regions.

5 Did the Reform Affect Type of High School Attended?

Compulsory schooling may also affect the distribution of types of schools attended. First, the marginal student who would have dropped out early in the absence of compulsory schooling reform may prefer vocational high schools over academic ones. Completing vocational high school provides a graduate with a certificate for a specific vocation, such electrician or hairdresser, which allows the graduate to start his or her own business or join someone

else's business as a certified specialist. Second, as suggested by Lang and Kropp (1986), students who would have attended high school even in the absence of such laws might want to differentiate themselves from the marginal students and gain expertise in new trades by attending vocational high schools, which can increase their potential earnings in the labor markets. In fact, Tansel and Bodur (2012) find that the return to completing vocational high school is generally higher than that of completing academic high schools in Turkey.[16]

We examine whether the reform had differential effects based on the type of high school attended. Figure 6 provides an illustration of RD treatment effects on types of high school attendance by gender. Panel B shows a significant upward shift in both female and male vocational high school attendance, with the shift for males appearing to be larger than that for females. However, Panel C indicates no evidence of a significant jump in either female or male academic high school attendance.

For a more precise analysis, we turn to the regression results reported in Table 6. The RD estimates in columns 1 and 2 indicate that the reform increased girls' attendance of vocational high schools in poorer and more religiously conservative regions. We find no evidence of a significant change in female attendance of vocational high schools in less poor or religious areas. This is consistent with the fact that these are also the regions for which we have not found a statistically significant change in female high school attendance after the reform. However, the RD estimates in columns 3 and 4 show that the reform had a similar positive impact on male vocational high school attendance regardless of regional levels of poverty or religiosity. We find no evidence of a significant change in female or male academic high school attendance in any region.[17]

[16]We note upfront that the 2012 compulsory schooling law also eliminated the disadvantage that vocational high school graduates had in the university entrance system relative to academic high school graduates, thereby potentially increasing the incentives to enroll at vocational schools. Prior to the 2012 law, during college admissions, the placement scores of academic high school graduates were multiplied by a higher coefficient relative to those of vocational high school graduates in applications to science-focused university programs such as medicine. The 2012 law made the coefficients uniform for the graduates from different types of high schools (Dogan and Yuret, 2015). However, this may not be a serious concern for the purpose of our study since the transition rate from vocational high schools to post-secondary education still remains low.

[17]As a robustness check, Table A6 in Appendix B reports the RD estimates using a static bandwidth of

A potential alternative mechanism driving the effects that we observe, particularly for girls, is that in poorer or more conservative regions, the reform had a differential impact on vocational high school supply. If the authorities increased the supply of vocational high schools much more than academic ones in such regions, we would expect that the differential supply of schools was what generated the large increase in vocational school attendance instead of greater demand for vocational high school attendance.

To examine whether the supply side effects are a significant mechanism driving our results, we present the difference-in-difference estimates of the effect of the reform on vocational high school supply in Table 7. In particular, in Panel A, we test for whether there is a positive and significant impact of being located in a poorer region in the post-reform period on the number of vocational high schools. The results show that poorer regions have a higher number of vocational high schools and the supply of vocational high schools increased nationwide after the reform. However, we find no evidence of a differential increase in the number of vocational high schools in poorer regions of Turkey. In Panel B, we find that more religiously conservative regions have a higher supply of vocational high schools, and the supply of these schools increased nationwide following the reform. However, we find no evidence of a differential increase in the number of vocational schools in more religiously conservative regions. Thus, we find no empirical support for the school-supply channel, which implies that greater demand among the marginal students for vocational high schools in response to the reform seems to play a larger role in explaining the types of high schools that students attended after the reform.

54 months around the cutoff. The findings are in line with those in Table 6. The RD treatment effects for girls are driven primarily by an increase in vocational high school attendance in poorer and more religiously conservative regions, whereas the reform led to a significant increase in male vocational high school attendance regardless of regional differences in poverty and religiosity.

6 Concluding Remarks

In order to design and implement compulsory schooling laws in a more cost-effective way, it is essential to understand which segments of the population are likely to comply with them, and which ones are likely to drop out nevertheless. This paper identifies the main compliers with an education reform that made high school attendance mandatory in Turkey.

We find that the reform led to an increase in continuing high school education for both boys and girls. While the main male compliers were those who would have participated in paid employment prior to the reform, female compliers would have likely not been in education, employment, or training (NEET), or they could have been employed in unpaid work. Our results show that these gender differences remain unchanged regardless of differences in poverty rates across regions. However, we find that the reform had a positive impact on girls' high school attendance only in more religiously conservative regions. We also find that the reform led to a significant decline in the NEET status of girls in more religiously conservative regions, with no evidence of a significant impact on others. The differences in regional levels of religiosity do not alter the results for boys. We also show that the main compliers chose to attend vocational high schools as opposed to academic ones. We present some suggestive evidence that these effects are not operating through regional differences in vocational school supply.

One limitation of our study is that we document the short-term impacts of the reform. The long-term effects may significantly differ from the immediate impacts, as general equilibrium effects will change the returns to education and labor market outcomes. Another caveat to the interpretation of the results is that, in addition to extending the duration of compulsory schooling, the reform brought additional changes to the university entrance exam, which makes it difficult to fully disentangle the effects of various changes.

Our results indicate that the relative tradeoffs facing the main compliers with the reform differ significantly by gender, regional poverty, and religious conservatism. As the main female compliers were previously confined to performing household chores or unpaid work, with

no potential for gaining early work experience or training, it is clear that the need to comply with the law resulted in a net benefit for them in terms of both pecuniary and non-pecuniary (e.g., health-related or intra-household bargaining power related) benefits. However, as the main male compliers were previously participating in paid employment, whether the compulsory schooling reform resulted in a net benefit for them requires a comparison of the tradeoff between gaining early work experience and investing in further human capital accumulation. Based on the reported estimates in previous studies, the return to completing vocational high school is higher than the return to additional work experience, resulting in a lifetime reward for receiving a vocational school degree that allows one to work as a certified specialist (Tansel and Bodur, 2012). Hence, this evidence suggests that male compliers too gained on average from continuing to high school education.

Even in poorer regions, where families are more resource-constrained, the main compliers among girls were those that were previously NEET. The fact that regional poverty levels do not make a substantial difference in terms of who the main compliers are is indicative of barriers in social norms that go well beyond resource and credit constraints. Moreover, the evidence shows that the reform had the largest impact on girls' schooling in religiously conservative areas where these girls were NEET previously. Although there is some overlap between poverty and religious conservatism across regions, this evidence suggests that the unique barriers that girls face in religiously conservative regions within a secular education system need to be taken into account when designing education policies.

Our study has important implications for many developing countries in which large gender gaps remain in the completion of secondary school. This is particularly important given that the vast majority of countries have achieved universal completion of primary school education, while girls' schooling lags behind boys in a number of poor and developing countries (United Nations, 2018). The presence of biased social norms can be an important barrier against the completion of high school by girls, and this can be particularly severe in countries with higher religious and/or conservative preferences. In such contexts, our analysis implies

that compulsory schooling laws can generate positive social change by reducing cultural barriers against girls' educational attainment. Addressing such gender biases in educational attainment is key to eliminating gender inequality in the long run.

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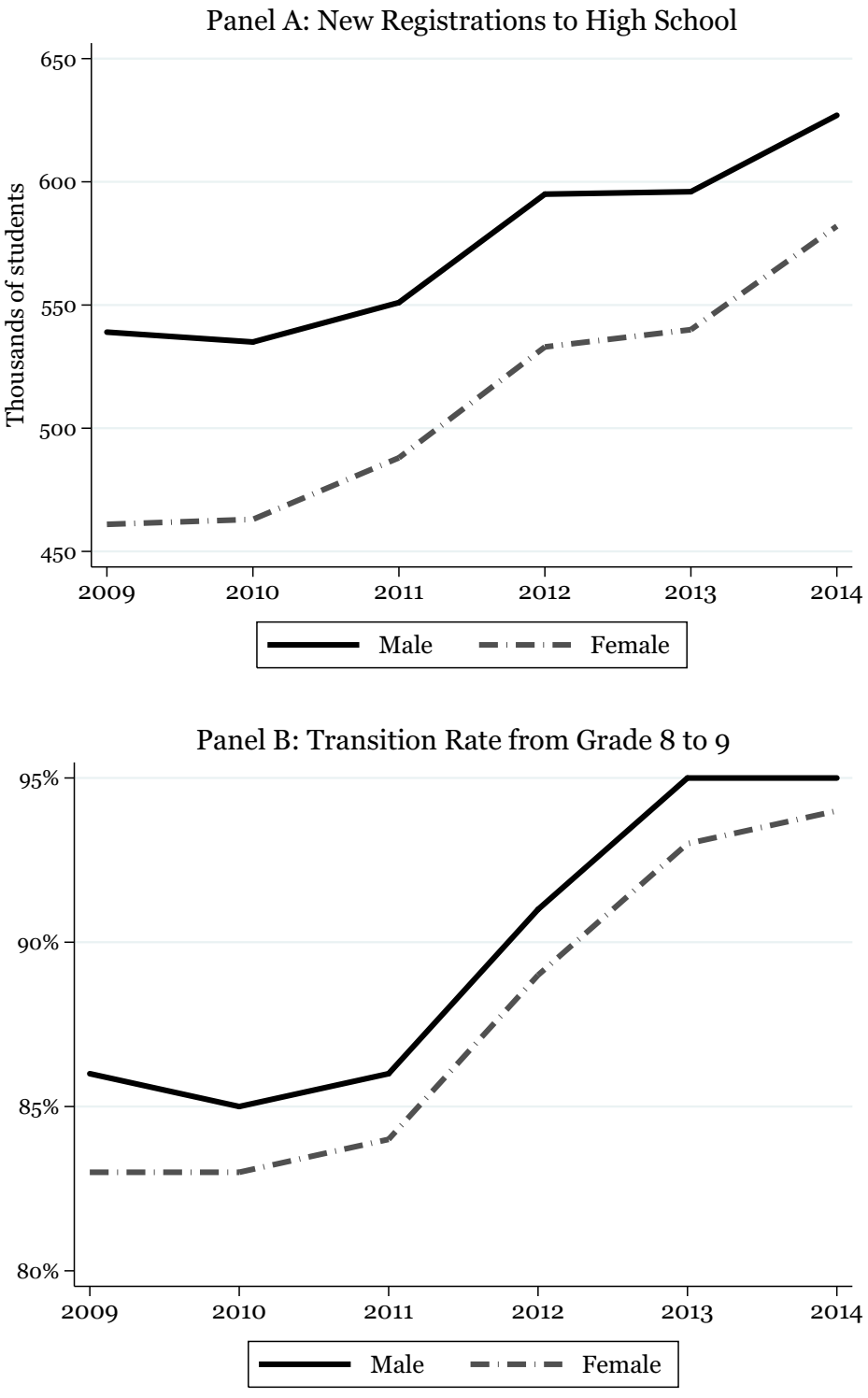
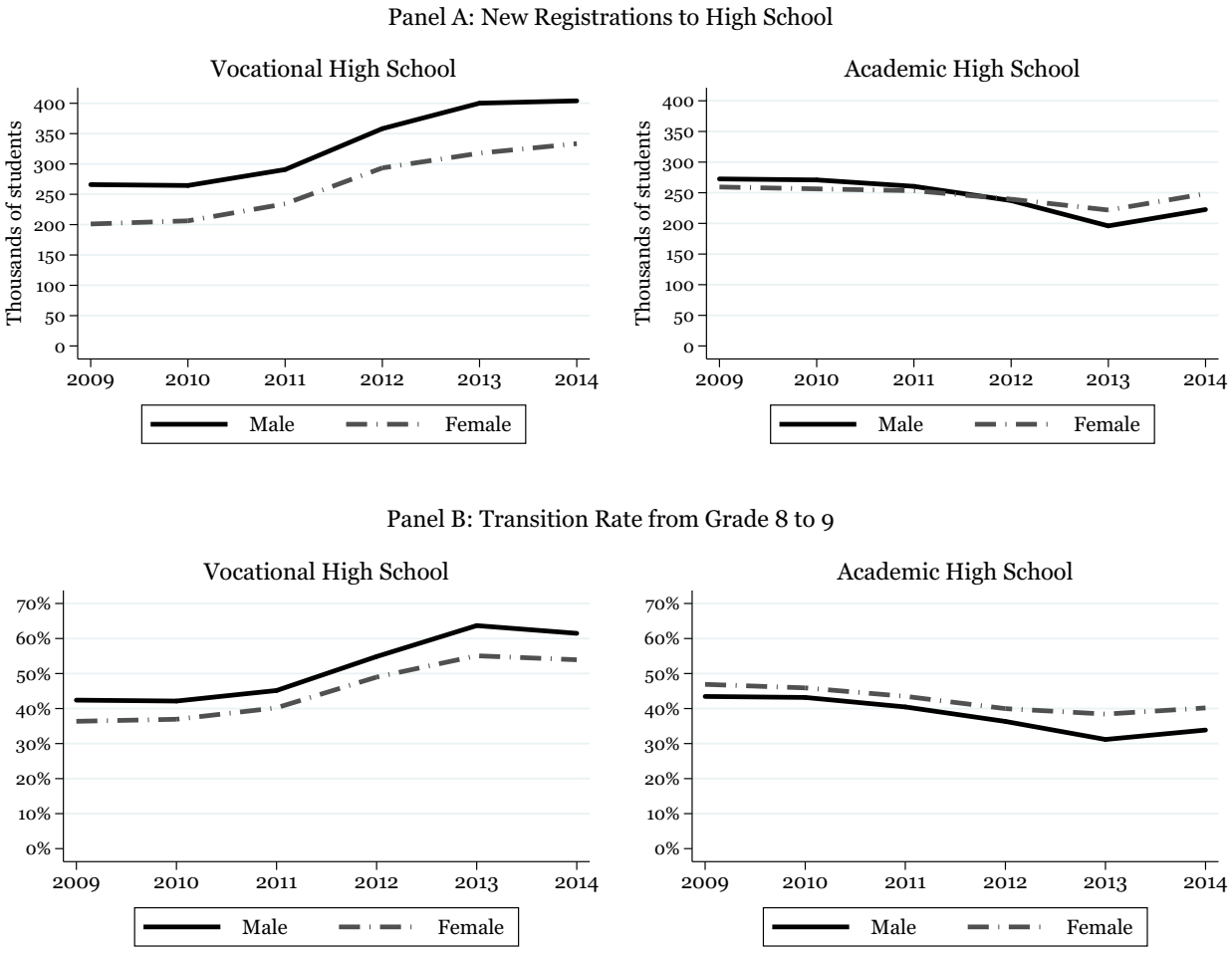
Figure 1: Trends in High School Enrollment in Turkey



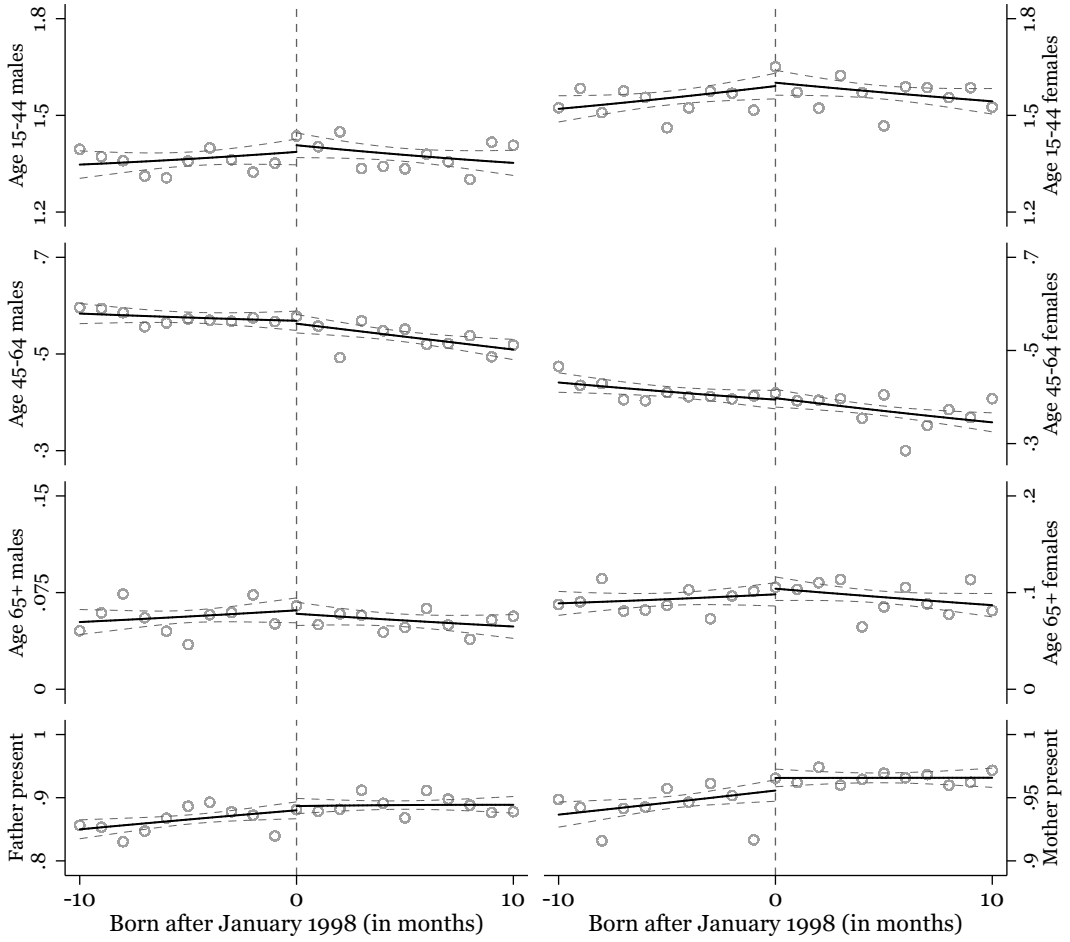
Note: Data is from the National Education Statistics Formal Education Yearbooks 2008-2009, 2009-2010, 2010-2011, 2011-2012, 2012-2013, 2013-2014 and 2014-2015. Panel A plots the number of new students registered in high school education, and Panel B plots the percentage of students who transitioned from 8th grade to 9th grade by completing 8th grade and registering as a new student to 9th grade.

FIGURE 2: ENROLLMENT AND TRANSITION RATES TO VOCATIONAL VS. ACADEMIC HIGH SCHOOLS IN TURKEY



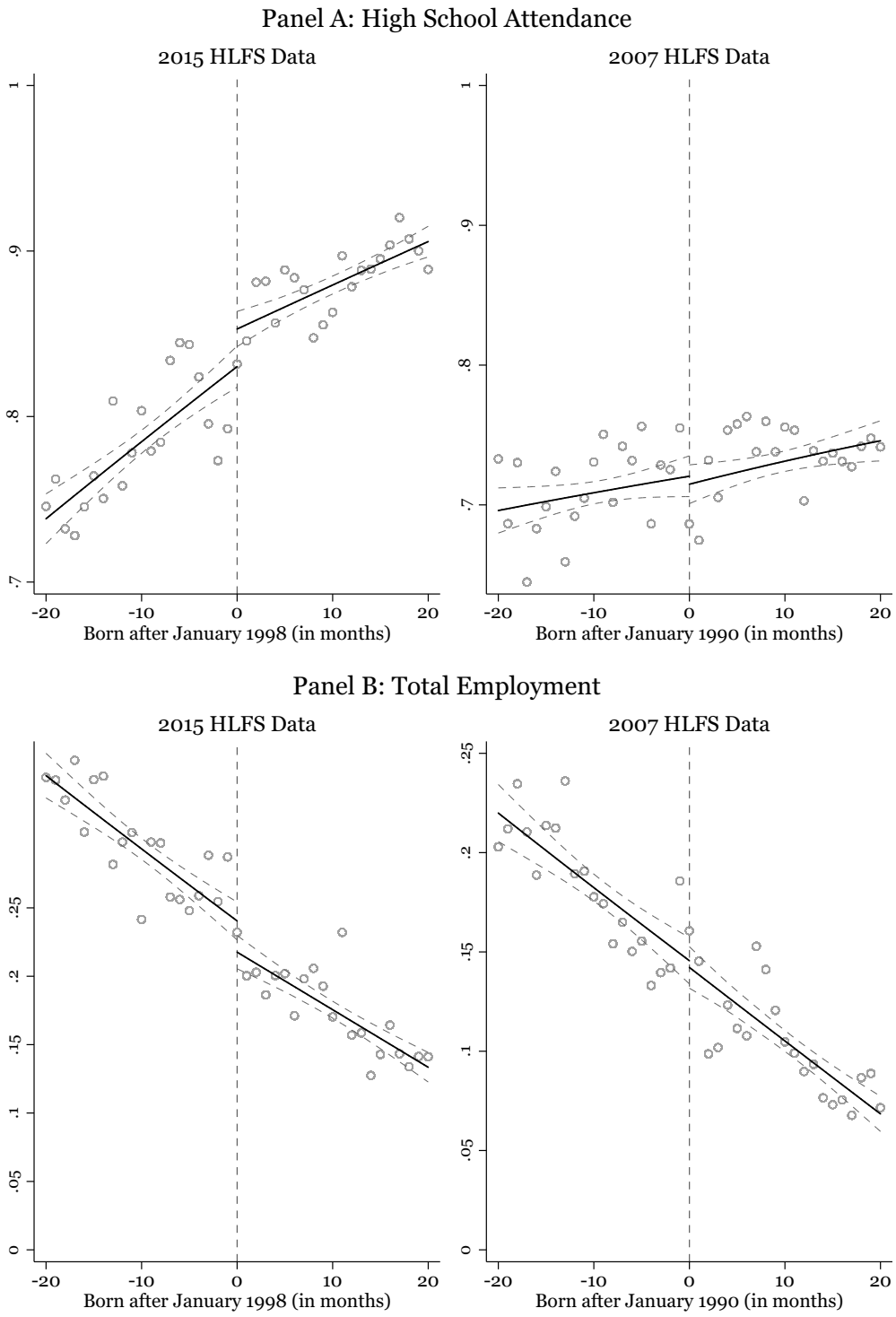
Note: Data is from the National Education Statistics Formal Education Yearbooks 2008-2009, 2009-2010, 2010-2011, 2011-2012, 2012-2013, 2013-2014 and 2014-2015. Panel A plots the number of new students registered in vocational vs. academic high schools, and Panel B plots the percentage of students who transitioned from 8th grade to 9th grade in vocational vs. academic high schools.

Figure 3: Balanced Covariates



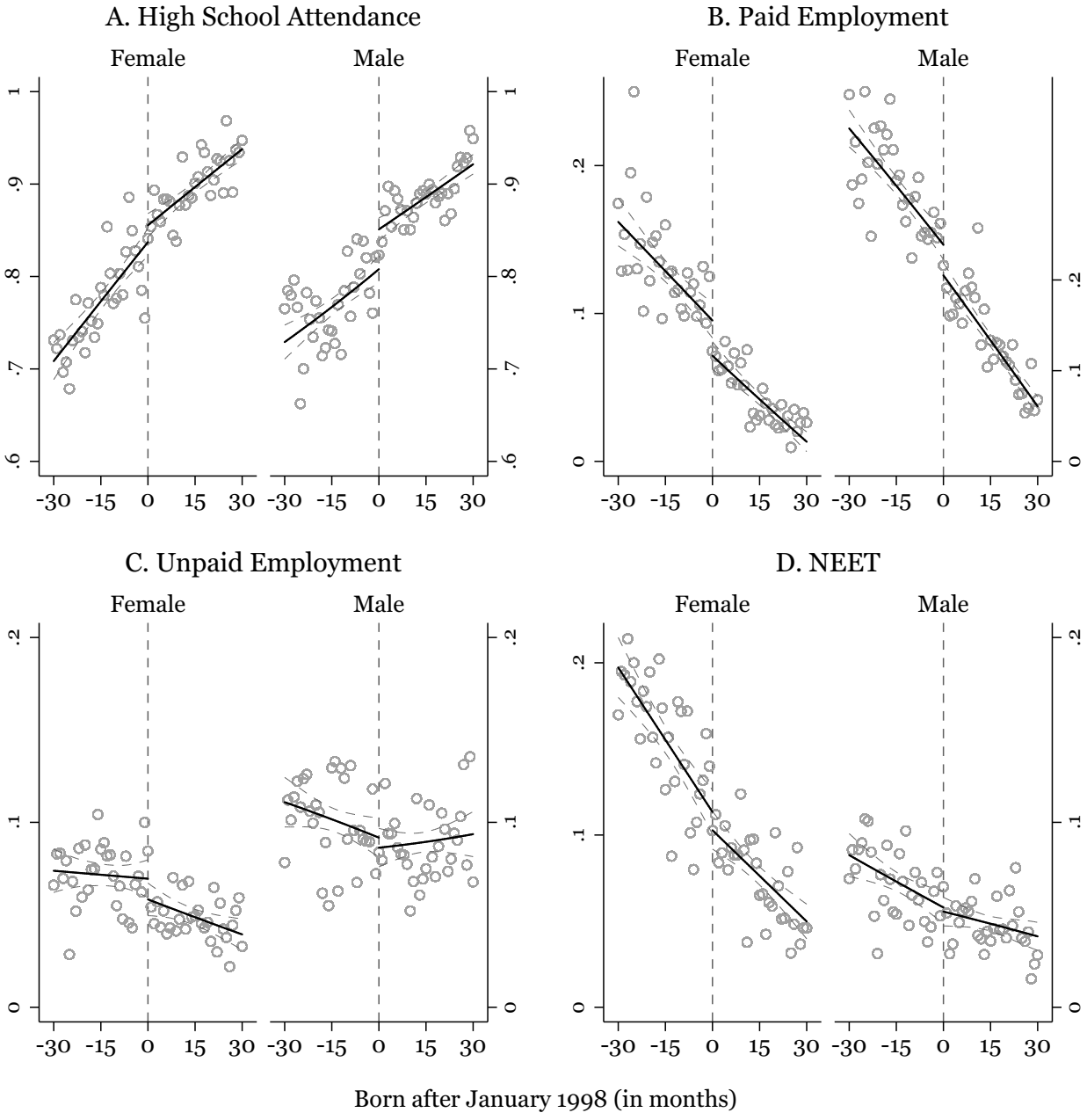
Note: Data is from the 2015 Household Labor Force Survey of Turkey. Figures plot pre-determined covariates in monthly bins against the month-year-of-birth of being born in January 1998. The vertical line in each graph represents the cut-off point, January 1998. Gray lines show 95 percent confidence intervals around the mean level. Variable definitions are listed in Appendix A.

Figure 4: Treatment and Placebo



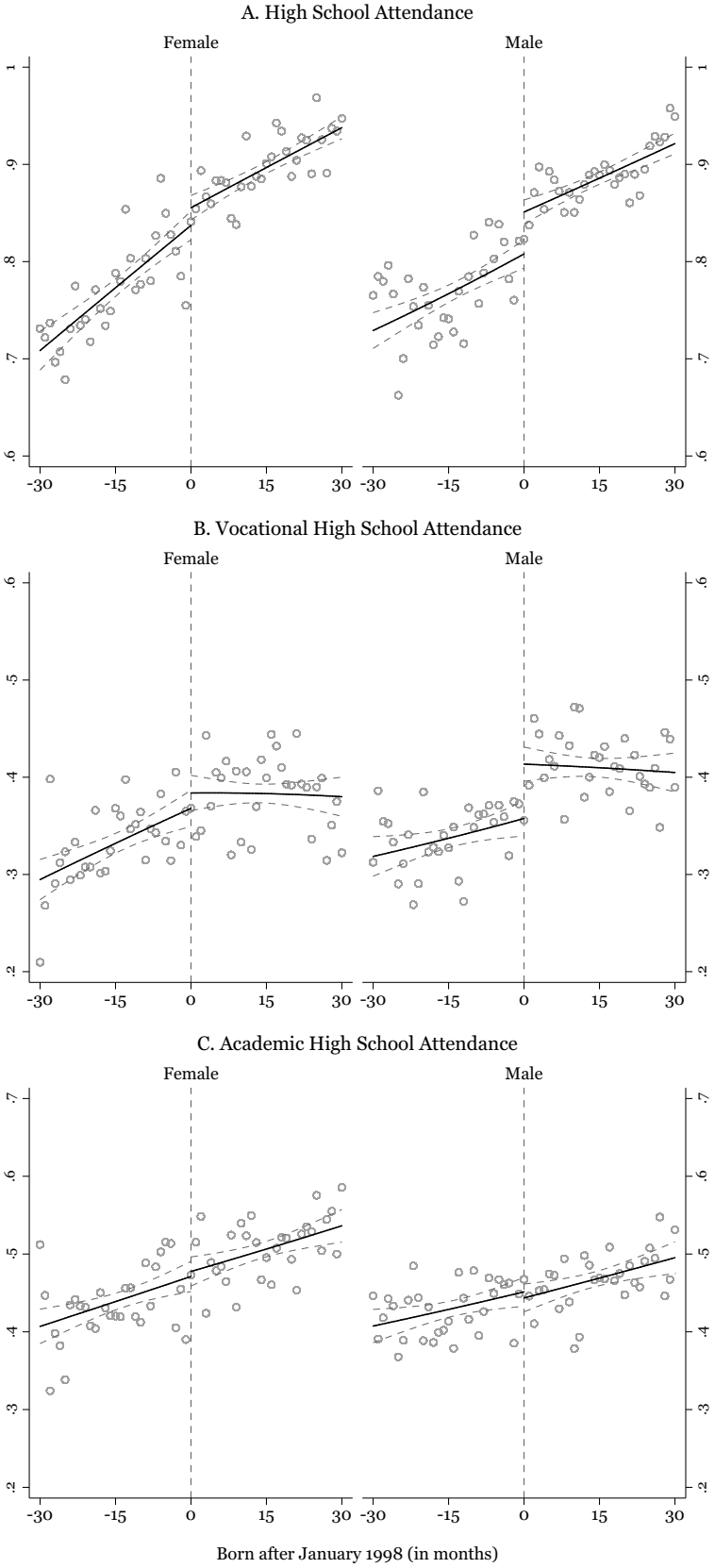
Note: Data are from the 2015 and 2007 Household Labor Force Surveys of Turkey. The figures plot a dummy variable equal to one if the respondent attends high school and a dummy variable equal to one of the respondent participates in paid employment in monthly bins. Gray dashed lines show 95 percent confidence intervals around the mean level.

FIGURE 5: RD TREATMENT EFFECTS BY GENDER



Note: Data is from the 2015 Household Labor Force Survey of Turkey. Figures plot high school attendance, participation in paid employment, participation in unpaid employment, and being NEET (neither in education, employment, or training) 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 ON TYPES OF SCHOOLING BY GENDER



Note: Data is from the 2015 Household Labor Force Survey of Turkey. Figures plot high school attendance in total, vocational high school attendance, academic high school attendance in monthly bins against the month-year-of-birth of being born in January 1998. The vertical line in each graph represents the cut-off point, January 1998. Gray lines show 95 percent confidence intervals around the mean level.

Table 1: Summary Statistics of 15-20 Year-Old Individuals

	Overall	Female	Male	Difference	Observations
	(1)	(2)	(3)	(4)	(5)
	Mean	Mean	Mean	Est.	
	(S.D.)	(S.D.)	(S.D.)	(S.E.)	(All/Female/Male)
Panel A: High School Attendance					
Total	0.83	0.84	0.82	0.02***	(36,295/17,553/18,742)
	(0.38)	(0.37)	(0.38)	(0.00)	
Vocational	0.37	0.37	0.38	-0.01*	(36,231/17,508/18,723)
	(0.48)	(0.48)	(0.48)	(0.01)	
Academic	0.45	0.47	0.44	0.03***	(36,231/17,508/18,723)
	(0.50)	(0.50)	(0.50)	(0.01)	
Panel B: Labor Market Outcomes					
Participation in the last week:					
Paid employment	0.17	0.10	0.23	-0.14***	(36,295/17,553/18,742)
	(0.37)	(0.30)	(0.42)	(0.00)	
Paid temporary employment	0.06	0.04	0.08	-0.04***	(36,295/17,553/18,742)
	(0.24)	(0.19)	(0.27)	(0.00)	
Unpaid employment	0.06	0.05	0.08	-0.03***	(36,295/17,553/18,742)
	(0.24)	(0.21)	(0.27)	(0.00)	
Total employment	0.23	0.14	0.31	-0.17***	(36,295/17,553/18,742)
	(0.42)	(0.35)	(0.46)	(0.01)	
Log hours worked in the last week (including 0s):					
Paid employment	0.63	0.36	0.89	-0.53***	(36,295/17,553/18,742)
	(1.43)	(1.10)	(1.63)	(0.02)	
Paid temporary employment	0.21	0.13	0.28	-0.15***	(36,295/17,553/18,742)
	(0.86)	(0.68)	(0.99)	(0.01)	
Unpaid employment	0.21	0.15	0.27	-0.12***	(36,295/17,553/18,742)
	(0.83)	(0.69)	(0.94)	(0.01)	
Total employment	0.84	0.51	1.16	-0.65***	(36,295/17,553/18,742)
	(1.57)	(1.26)	(1.76)	(0.02)	
Log monthly earnings from paid employment (including 0s)	0.92	0.52	1.29	-0.77***	(36,295/17,553/18,742)
	(2.27)	(1.75)	(2.61)	(0.03)	
Not in Education, Employment, or Training (NEET)	0.09	0.12	0.06	0.06***	(36,295/17,553/18,742)
	(0.28)	(0.32)	(0.24)	(0.00)	
Panel C: Household Characteristics					
Mother present	0.94	0.92	0.96	-0.04***	(39,044/19,213/19,831)
	(0.23)	(0.27)	(0.19)	(0.00)	
Father present	0.88	0.86	0.90	-0.05***	(39,044/19,213/19,831)
	(0.32)	(0.35)	(0.29)	(0.00)	
Household size	5.48	5.58	5.38	0.21***	(39,044/19,213/19,831)
	(2.27)	(2.31)	(2.24)	(0.03)	

Notes: The table presents the mean, standard deviation, and number of observations from the 2015 Household Labor Force Survey of Turkey. The sample includes teenagers who are born within 30 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. The column 4 reports differences in the group means between females and males with standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

Table 2: RD Treatment Effects on Schooling

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bandwidth	RD ($\hat{h}$)	RD (0.5 $\hat{h}$)	RD (1.5 $\hat{h}$)	RD (2 $\hat{h}$)	$\hat{h}$	Mean	N
High school attendance:							
All	0.054***	0.039***	0.063***	0.092***	54	0.80	47,903
	(0.009)	(0.012)	(0.009)	(0.011)			
Female	0.029**	0.037**	0.056***	0.076***	33	0.83	18,801
	(0.014)	(0.015)	(0.014)	(0.015)			
Male	0.050***	0.023	0.070***	0.059***	27	0.82	17,451
	(0.016)	(0.016)	(0.014)	(0.013)			

Notes: Data is from the 2015 Household Labor Force Survey of Turkey. 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. Column (1) reports local RD regressions with a linear control function using optimal bandwidth $\hat{h}$. Columns (2), (3) and (4) report local RD regressions with a linear control function using optimal bandwidth 0.5 $\hat{h}$, 1.5 $\hat{h}$ and 2 $\hat{h}$, respectively. Column (5) reports the optimal bandwidth estimated by the Imbens and Kalyanaraman (2009) algorithm. Column (6) reports the outcome mean within the optimal bandwidth, and column (7) reports the number of observations used in the estimations. The dependent variable is a dummy variable equal to one if the respondent attends high school. The first row presents the RD estimates for all individuals, the second row presents them for females, and the third row presents them for males. All specifications control for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age categories and gender present in the household, household size fixed effects, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

Table 3: RD Treatment Effects on Labor Market Outcomes by Gender

	Female		**Male**	
	(1) RF	(2) IV	(3) RF	(4) IV
Participation in the last week:				
Paid employment	-0.018**	-0.578	-0.041**	-0.813***
	(0.007)	(0.384)	(0.017)	(0.260)
	[0.02]	[0.15]	[0.04]	[0.00]
Mean	0.10	0.10	0.24	0.24
Bandwidth	30	30	26	26
Observations	17,553	17,553	17,451	17,451
Paid temporary employment	-0.028***	-0.926*	-0.034***	-0.477***
	(0.007)	(0.555)	(0.009)	(0.121)
	[0.00]	[0.12]	[0.00]	[0.00]
Mean	0.04	0.04	0.08	0.08
Bandwidth	31	31	45	45
Observations	17,917	17,917	22,462	22,462
Unpaid employment	-0.016**	-0.210**	-0.003	-0.043
	(0.008)	(0.090)	(0.008)	(0.114)
	[0.04]	[0.07]	[0.79]	[0.78]
Mean	0.05	0.05	0.08	0.08
Bandwidth	69	69	37	37
Observations	26,539	26,539	20,814	20,814
Total employment	-0.032***	-1.058**	-0.050***	-0.746***
	(0.009)	(0.522)	(0.015)	(0.165)
	[0.00]	[0.10]	[0.01]	[0.00]
Mean	0.14	0.14	0.31	0.31
Bandwidth	32	32	33	33
Observations	18,253	18,253	20,034	20,034
Log hours worked in the last week: (including 0s)				
Paid employment	-0.049*	-1.577	-0.146**	-2.910***
	(0.027)	(1.170)	(0.068)	(1.042)
	[0.07]	[0.18]	[0.05]	[0.01]
Mean	0.36	0.36	0.90	0.90
Bandwidth	31	31	26	26
Observations	17,917	17,917	17,451	17,451
Paid temporary employment	-0.091***	-2.921*	-0.091**	-1.450***
	(0.026)	(1.727)	(0.035)	(0.559)
	[0.00]	[0.12]	[0.02]	[0.01]
Mean	0.13	0.13	0.28	0.28
Bandwidth	32	32	38	38
Observations	18,568	18,568	20,922	20,922

Table 3: RD Treatment Effects on Labor Market Outcomes by Gender, cont'd.

	Female		Male	
	(1) RF	(2) IV	(3) RF	(4) IV
Unpaid employment	-0.053**	-1.024**	0.006	0.093
	(0.026)	(0.446)	(0.027)	(0.421)
	[0.05]	[0.07]	[0.83]	[0.83]
Mean	0.15	0.15	0.26	0.26
Bandwidth	45	45	31	31
Observations	21,287	21,287	19,149	19,149
Total employment	-0.106***	-3.472*	-0.157***	-2.367***
	(0.034)	(1.788)	(0.053)	(0.608)
	[0.01]	[0.10]	[0.01]	[0.00]
Mean	0.51	0.51	1.19	1.19
Bandwidth	29	29	37	37
Observations	17,553	17,553	20,814	20,814
Log monthly earnings	-0.124***	-4.244*	-0.359***	-5.700***
(including 0s)	(0.041)	(2.573)	(0.109)	(1.452)
	[0.01]	[0.12]	[0.01]	[0.00]
Mean	0.55	0.55	1.30	1.30
Bandwidth	33	33	31	31
Observations	18,801	18,801	19,486	19,486
Not in Education, Employment,	-0.028**	-0.663***	-0.008	-0.153
or Training (NEET)	(0.011)	(0.145)	(0.006)	(0.099)
	[0.02]	[0.00]	[0.24]	[0.15]
Mean	0.13	0.13	0.07	0.07
Bandwidth	39	39	56	56
Observations	20,095	20,095	24,889	24,889

Notes: Data is from the 2015 Household Labor Force Survey of Turkey. The optimal bandwidth is estimated by the Imbens and Kalyanaraman (2009) algorithm. Columns 1 and 3 report reduced-form (RF) RD treatment effects, and columns 2 and 4 report two-stage least-squares (IV) RD treatment effects (by using treatment as an instrument for high school attendance) of being born after January 1998 with a linear control function in the month-year of birth on each side of the discontinuity. The variable definitions are listed in Appendix A. All specifications control for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age categories and gender present in the household, household size fixed effects, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively. The figures in square brackets show p-values corrected for multiple-hypothesis testing using Simes adjustment.

Table 4: Heterogeneous RD Treatment Effects by Pre-Reform Regional Poverty Rates

	Female				Male			
	RF		IV		RF		IV	
	Above Median (1)	Below Median (2)	Above Median (3)	Below Median (4)	Above Median (5)	Below Median (6)	Above Median (7)	Below Median (8)
Participation in the last week:								
High school attendance	0.054***	0.040*			0.054**	0.060***		
	(0.018)	(0.023)			(0.021)	(0.022)		
	[0.02]	[0.16]			[0.02]	[0.05]		
Mean	0.79	0.86			0.80	0.83		
Bandwidth	49	32			25	52		
Observations	11,900	7,329			9,066	9,604		
Paid Employment	0.001	-0.026	0.030	-0.841	-0.067***	-0.036*	-1.310***	-0.791*
	(0.013)	(0.017)	(0.257)	(0.742)	(0.022)	(0.020)	(0.448)	(0.440)
	[0.91]	[0.19]	[0.91]	[0.43]	[0.01]	[0.14]	[0.01]	[0.18]
Mean	0.11	0.11	0.11	0.11	0.22	0.33	0.22	0.33
Bandwidth	47	27	47	27	25	66	25	66
Observations	11,659	6,688	11,659	6,688	9,306	10,916	9,306	10,916
Unpaid Employment	-0.020*	-0.000	-0.847	-0.013	-0.001	-0.008	-0.013	-0.115
	(0.011)	(0.012)	(0.698)	(0.591)	(0.012)	(0.009)	(0.167)	(0.136)
	[0.16]	[0.98]	[0.44]	[0.98]	[0.94]	[0.49]	[0.94]	[0.49]
Mean	0.05	0.04	0.05	0.04	0.09	0.05	0.09	0.05
Bandwidth	32	26	32	26	43	33	43	33
Observations	9,754	6,332	9,754	6,332	11,819	7,947	11,819	7,947
Total Employment	-0.016	-0.002	-0.306	-0.088	-0.072***	-0.033	-1.321***	-0.465
	(0.018)	(0.022)	(0.333)	(1.065)	(0.026)	(0.025)	(0.417)	(0.297)
	[0.45]	[0.98]	[0.45]	[0.98]	[0.02]	[0.28]	[0.00]	[0.20]
Mean	0.15	0.15	0.15	0.15	0.31	0.32	0.31	0.32
Bandwidth	47	23	47	23	25	36	25	36
Observations	11,566	5,807	11,566	5,807	9,066	8,244	9,066	8,244
Log monthly earnings	-0.083	-0.241**	-1.529	-5.649*	-0.424***	-0.284**	-6.345***	-4.696*
	(0.069)	(0.099)	(1.367)	(3.334)	(0.123)	(0.141)	(1.721)	(2.409)
	[0.35]	[0.05]	[0.44]	[0.23]	[0.01]	[0.14]	[0.00]	[0.18]
Mean	0.69	0.64	0.69	0.64	1.19	1.72	1.19	1.72
Bandwidth	61	30	61	30	29	52	29	52
Observations	13,247	7,045	13,247	7,045	9,991	9,499	9,991	9,499
NEET	-0.030*	-0.046***	-0.614***	-0.747***	-0.002	-0.007	-0.030	-0.096
	(0.016)	(0.016)	(0.178)	(0.162)	(0.011)	(0.012)	(0.144)	(0.156)
	[0.16]	[0.02]	[0.00]	[0.00]	[0.94]	[0.58]	[0.94]	[0.54]
Mean	0.15	0.12	0.15	0.12	0.07	0.05	0.07	0.05
Bandwidth	43	53	43	53	45	33	45	33
Observations	11,117	9,293	11,117	9,293	12,104	7,947	12,104	7,947

Notes: Data is from the 2015 Household Labor Force Survey of Turkey. The optimal bandwidth is estimated by the Imbens and Kalyanaraman (2009) algorithm. All columns report reduced-form (RF) RD treatment effects of being born after January 1998 with a linear control function in the month-year of birth on each side of the discontinuity. The columns with "above median" ("below median") report estimates for the sample of regions which had poverty levels above (below) the median in 2011. The variable definitions are listed in Appendix A. All specifications control for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age categories and gender present in the household, household size fixed effects, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively. The figures in square brackets show p-values corrected for multiple-hypothesis testing using Simes adjustment.

Table 5: Heterogeneous RD Treatment Effects by Pre-Reform Regional Islamic Vote Shares

	Female				Male			
	RF		IV		RF		IV	
	Above Median (1)	Below Median (2)	Above Median (3)	Below Median (4)	Above Median (5)	Below Median (6)	Above Median (7)	Below Median (8)
Participation in the last week:								
High school attendance	0.041**	-0.012			0.070***	0.060***		
	(0.019)	(0.020)			(0.020)	(0.020)		
	[0.10]	[0.81]			[0.00]	[0.02]		
Mean	0.86	0.83			0.83	0.82		
Bandwidth	29	34			31	37		
Observations	8,669	8,735			9,441	9,930		
Paid Employment	-0.007	0.000	-0.181	0.017	-0.046**	-0.038*	-0.681**	-0.556**
	(0.010)	(0.015)	(0.266)	(0.785)	(0.023)	(0.020)	(0.272)	(0.252)
	[0.48]	[0.98]	[0.49]	[0.98]	[0.07]	[0.12]	[0.03]	[0.07]
Mean	0.11	0.11	0.11	0.11	0.24	0.23	0.24	0.23
Bandwidth	34	42	34	42	31	35	31	35
Observations	9,533	9,494	9,533	9,494	9,259	9,749	9,259	9,749
Unpaid Employment	-0.012	-0.009	-0.142	-0.294	-0.024**	0.016	-0.315**	0.233
	(0.009)	(0.010)	(0.090)	(0.425)	(0.011)	(0.012)	(0.156)	(0.187)
	[0.24]	[0.79]	[0.25]	[0.98]	[0.06]	[0.28]	[0.05]	[0.27]
Mean	0.04	0.06	0.04	0.06	0.06	0.09	0.06	0.09
Bandwidth	69	50	69	50	48	33	48	33
Observations	13,881	10,175	13,881	10,175	11,303	9,557	11,303	9,557
Total Employment	-0.017	-0.026	-0.466	-8.401	-0.076***	-0.022	-1.114***	-0.432
	(0.013)	(0.018)	(0.363)	(57.562)	(0.026)	(0.019)	(0.306)	(0.316)
	[0.25]	[0.79]	[0.25]	[0.98]	[0.01]	[0.28]	[0.00]	[0.27]
Mean	0.15	0.15	0.15	0.15	0.30	0.41	0.30	0.41
Bandwidth	35	39	35	39	30	74	30	74
Observations	9,812	9,129	9,812	9,129	9,259	13,683	9,259	13,683
Log monthly earnings	-0.102	-0.002	-1.737	-0.076	-0.320*	-0.335***	-4.608**	-5.431***
	(0.070)	(0.084)	(1.210)	(2.976)	(0.169)	(0.126)	(2.001)	(2.036)
	[0.24]	[0.98]	[0.25]	[0.98]	[0.08]	[0.03]	[0.04]	[0.04]
Mean	0.75	0.62	0.75	0.62	1.39	1.50	1.39	1.50
Bandwidth	47	47	47	47	31	57	31	57
Observations	11,266	9,966	11,266	9,966	9,441	11,799	9,441	11,799
NEET	-0.031**	-0.018	-0.687***	-1.409	-0.008	-0.008	-0.106	-0.180
	(0.012)	(0.019)	(0.185)	(1.218)	(0.008)	(0.010)	(0.104)	(0.177)
	[0.08]	[0.79]	[0.00]	[0.98]	[0.36]	[0.39]	[0.31]	[0.31]
Mean	0.10	0.13	0.10	0.13	0.06	0.07	0.06	0.07
Bandwidth	31	40	31	40	47	69	47	69
Observations	9,242	9,299	9,242	9,299	11,163	13,098	11,163	13,098

Notes: Data is from the 2015 Household Labor Force Survey of Turkey. The optimal bandwidth is estimated by the Imbens and Kalyanaraman (2009) algorithm. All columns report reduced-form (RF) RD treatment effects of being born after January 1998 with a linear control function in the month-year of birth on each side of the discontinuity. The columns with "above median" ("below median") report estimates for the sample of regions which had Islamic party vote shares above (below) the median in 2011. The variable definitions are listed in Appendix A. All specifications control for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age categories and gender present in the household, household size fixed effects, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively. The figures in square brackets show p-values corrected for multiple-hypothesis testing using Simes adjustment.

Table 6: Heterogeneous RD Treatment Effects on Type of School Attended

	Female		Male	
	(1)	(2)	(3)	(4)
Panel A: Heterogeneous Effects by Poverty				
	Above Median	Below Median	Above Median	Below Median
Vocational high school attendance	0.055**	0.014	0.070***	0.073***
	(0.025)	(0.026)	(0.019)	(0.026)
	[0.07]	[0.60]	[0.00]	[0.01]
Mean	0.33	0.40	0.34	0.40
Bandwidth	30	38	40	60
Observations	9,546	7,859	11,486	10,025
Academic high school attendance	-0.014	0.033	0.004	-0.004
	(0.024)	(0.028)	(0.023)	(0.024)
	[0.58]	[0.48]	[0.85]	[0.87]
Mean	0.48	0.44	0.46	0.42
Bandwidth	38	50	27	38
Observations	10,542	8,875	9,754	8,255
Panel B: Heterogeneous Effects by Islamic Vote Share				
	Above Median	Below Median	Above Median	Below Median
Vocational high school attendance	0.061***	-0.002	0.084***	0.084***
	(0.020)	(0.027)	(0.025)	(0.017)
	[0.01]	[0.95]	[0.00]	[0.00]
Mean	0.38	0.34	0.37	0.39
Bandwidth	26	62	47	36
Observations	7,986	10,849	11,033	9,855
Academic high school attendance	-0.005	0.027	-0.007	-0.000
	(0.024)	(0.028)	(0.023)	(0.024)
	[0.85]	[0.67]	[0.77]	[0.99]
Mean	0.47	0.46	0.45	0.43
Bandwidth	54	40	44	41
Observations	11,616	9,113	10,695	10,194

Notes: Data is from the 2015 Household Labor Force Survey of Turkey. The optimal bandwidth is estimated by the Imbens and Kalyanaraman (2009) algorithm. All columns report reduced-form (RF) RD treatment effects of being born after January 1998 with a linear control function in the month-year of birth on each side of the discontinuity. In Panel A, the columns with "above median" ("below median") report estimates for the sample of regions which had poverty levels above (below) the median in 2011. In Panel B, the columns with "above median" ("below median") report estimates for the sample of regions which had Islamic party vote shares above (below) the median in 2011. The variable definitions are listed in Appendix A. All specifications control for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age categories and gender present in the household, household size fixed effects, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively. The figures in square brackets show p-values corrected for multiple-hypothesis testing using Simes adjustment.

Table 7: Difference-in-Difference Estimates of the Effect of the Reform on Vocational High School Supply

	(1)	(2)
Panel A: Difference-in-Difference Estimate of the Effect of Reform by Poverty		
Poverty above median	246.128***	
	(16.802)	
Post	9.392**	
	(4.399)	
Poverty above median * Post	7.212	
	(5.055)	
Panel B: Difference-in-Difference Estimate of the Effect of Reform by Islamic Vote Share		
Islamic vote above median		14.677*
		(8.140)
Post		9.340***
		(1.731)
Islamic vote above median * Post		6.596
		(5.432)
Observations	729	729

Notes: Data is from the National Education Statistics Formal Education Yearbooks 2007-2008, 2008-2009, 2009-2010, 2010-2011, 2011-2012, 2012-2013, 2013-2014, 2014-2015, and 2015-2016. The table shows difference-in-difference estimates of the effect of the reform on vocational high school supply by regional poverty rate and Islamic vote share. The dependent variables in Panel A are a dummy variable equal to one if the vocational high school is located in a region which has a poverty rate above the median in 2011, a dummy variable equal to one if the year is after the reform of 2012, and an interaction of the two variables. The dependent variables in Panel B are a dummy variable equal to one if the vocational high school is located in a region which has an Islamic vote share above the median in 2011, a dummy variable equal to one if the year is after the reform of 2012, and an interaction of the two variables. All specifications include 26 region fixed effects. Standard errors are clustered at the province level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

FOR ONLINE PUBLICATION

Appendix A List of Variables

Outcome Variables:

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

Covariates:

- Mother present: A dummy variable equal to one if the respondent's mother is present in the household.
- Father present: A dummy variable equal to one if the respondent's father is present in the household.

- Number of different age-groups of males or females present: These are the counts of the number of individuals, male or female, who fall into a particular age category within the household. The age categories include 15-44, 45-64, 65-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.

Appendix B Additional Tables

Table A1: Additional Summary Statistics for 15- to 17-Year-Old Adolescents

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

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

Table A2: RD Treatment Effects on Schooling Using a Static Bandwidth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bandwidth	RD ($\hat{h}$)	RD (0.5 $\hat{h}$)	RD (1.5 $\hat{h}$)	RD (2 $\hat{h}$)	$\hat{h}$	Mean	N
High school attendance:							
All	0.054***	0.039***	0.063***	0.092***	54	0.80	47,903
	(0.009)	(0.012)	(0.009)	(0.011)			
Female	0.058***	0.022	0.085***	0.106***	54	0.80	23,467
	(0.014)	(0.016)	(0.015)	(0.015)			
Male	0.059***	0.057***	0.057***	0.090***	54	0.80	24,436
	(0.013)	(0.016)	(0.013)	(0.014)			

Notes: Data is from the 2015 Household Labor Force Survey of Turkey. Each column reports a reduced-form RD treatment effect of being born after January 1998 with a linear control function in month-year-of-birth on each side of the discontinuity. The bandwidth is 54 months in column 1, which is the optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman (2009) algorithm when high school attendance is the independent variable. Columns 2, 3 and 4 report local RD regressions with a linear control function using bandwidth 0.5 $\hat{h}$, 1.5 $\hat{h}$ and 2 $\hat{h}$, where $\hat{h}$ is 54 months estimated by the Imbens and Kalyanaraman (2009) algorithm when high school attendance is the independent variable. Column 6 reports the outcome mean, and column 7 reports the number of observations used in the estimations. The dependent variable is a dummy variable equal to one if the respondent attends high school. The first row presents the RD estimates for all individuals, the second row presents them for females, and the third row presents them for males. All specifications control for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age categories and gender present in the household, household size fixed effects, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

Table A3: RD Treatment Effects on Labor Market Outcomes by Gender Using a Static Bandwidth

	Female		Male	
	(1) RF	(2) IV	(3) RF	(4) IV
Participation in the last week:				
Paid employment	-0.004	-0.072	-0.041***	-0.693***
	(0.008)	(0.136)	(0.014)	(0.193)
Mean	0.14	0.14	0.28	0.28
Paid temporary employment	-0.019***	-0.332**	-0.030***	-0.504***
	(0.007)	(0.150)	(0.008)	(0.156)
Mean	0.04	0.04	0.08	0.08
Unpaid employment	-0.016**	-0.267**	-0.006	-0.102
	(0.007)	(0.117)	(0.007)	(0.124)
Mean	0.05	0.05	0.08	0.08
Total employment	-0.020*	-0.339*	-0.047***	-0.795***
	(0.010)	(0.181)	(0.014)	(0.192)
Mean	0.18	0.18	0.35	0.35
Log hours worked in the last week: (including 0s)				
Paid employment	-0.009	-0.159	-0.147***	-2.510***
	(0.031)	(0.538)	(0.055)	(0.768)
Mean	0.50	0.50	1.06	1.06
Paid temporary employment	-0.064***	-1.091**	-0.101***	-1.726***
	(0.022)	(0.507)	(0.031)	(0.566)
Mean	0.12	0.12	0.29	0.29
Unpaid employment	-0.053**	-0.909**	-0.015	-0.251
	(0.025)	(0.402)	(0.025)	(0.429)
Mean	0.15	0.15	0.26	0.26
Total employment	-0.062	-1.069	-0.162***	-2.761***
	(0.040)	(0.677)	(0.053)	(0.734)
Mean	0.66	0.66	1.33	1.33
Log monthly earnings (including 0s)	-0.055	-0.938	-0.296***	-5.057***
	(0.051)	(0.919)	(0.100)	(1.407)
Mean	0.77	0.77	1.57	1.57
Not in Education, Employment, or Training (NEET)	-0.042***	-0.713***	-0.007	-0.115
	(0.010)	(0.102)	(0.006)	(0.092)
Mean	0.14	0.14	0.07	0.07
Bandwidth	54	54	54	54
Observations	23,467	23,467	24,436	24,436

Notes: Data is from the 2015 Household Labor Force Survey of Turkey. The bandwidth is 54 months in all regressions, which is the optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman (2009) algorithm when high school attendance is the independent variable. Columns 1 and 3 report reduced-form (RF) RD treatment effects, and columns 2 and 4 report two-stage least-squares (IV) RD treatment effects (by using treatment as an instrument for high school attendance) of being born after January 1998 with a linear control function in the month-year of birth on each side of the discontinuity. The variable definitions are listed in Appendix A. All specifications control for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age categories and gender present in the household, household size fixed effects, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

TABLE A4: HETEROGENEOUS RD TREATMENT EFFECTS BY PRE-REFORM REGIONAL POVERTY RATES USING A STATIC BANDWIDTH

	Female				**Male**			
	RF		**IV**		**RF**		**IV**	
	Above Median (1)	Below Median (2)	Above Median (3)	Below Median (4)	Above Median (5)	Below Median (6)	Above Median (7)	Below Median (8)
Participation in the last week:								
High school attendance	0.049***	0.067***			0.059***	0.055**		
	(0.017)	(0.020)			(0.019)	(0.022)		
Mean	0.79	0.83		0.79	0.83		0.79	0.83
Paid Employment	-0.010	-0.002	-0.209	-0.033	-0.051***	-0.030	-0.860***	-0.540*
	(0.012)	(0.019)	(0.256)	(0.273)	(0.018)	(0.019)	(0.261)	(0.327)
Mean	0.12	0.16	0.12	0.16	0.26	0.30	0.26	0.30
Unpaid Employment	-0.012	-0.016	-0.238	-0.243	-0.004	-0.009	-0.074	-0.163
	(0.010)	(0.011)	(0.202)	(0.164)	(0.012)	(0.009)	(0.190)	(0.174)
Mean	0.05	0.04	0.05	0.04	0.08	0.06	0.08	0.06
Total Employment	-0.022	-0.019	-0.447	-0.276	-0.055***	-0.038*	-0.934***	-0.703*
	(0.018)	(0.021)	(0.353)	(0.306)	(0.020)	(0.023)	(0.254)	(0.393)
Mean	0.16	0.20	0.16	0.20	0.34	0.36	0.34	0.36
Log monthly earnings	-0.088	-0.048	-1.800	-0.711	-0.317***	-0.279**	-5.352***	-5.103**
	(0.073)	(0.109)	(1.583)	(1.599)	(0.114)	(0.138)	(1.785)	(2.572)
Mean	0.63	0.97	0.63	0.97	1.41	1.78	1.41	1.78
NEET	-0.033**	-0.053***	-0.677***	-0.791***	-0.005	-0.006	-0.092	-0.101
	(0.015)	(0.016)	(0.178)	(0.160)	(0.011)	(0.011)	(0.166)	(0.180)
Mean	0.16	0.12	0.16	0.12	0.08	0.05	0.08	0.05
Bandwidth	54	54		54	54		54	54
Observations	25,570	19,342		12,458	9,506		13,112	9,836

Notes: Data is from the 2015 Household Labor Force Survey of Turkey. The bandwidth is 54 months in all regressions, which is the optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman (2009) algorithm when high school attendance is the independent variable. All columns report reduced-form (RF) RD treatment effects of being born after January 1998 with a linear control function in the month-year of birth on each side of the discontinuity. The columns with "above median" ("below median") report estimates for the sample of regions which had poverty levels above (below) the median in 2011. The variable definitions are listed in Appendix A. All specifications control for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age categories and gender present in the household, household size fixed effects, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

Table A5: Heterogeneous RD Treatment Effects by Pre-Reform Regional Islamic Vote Shares Using a Static Bandwidth

	Female				Male			
	RF		IV		RF		IV	
	Above Median (1)	Below Median (2)	Above Median (3)	Below Median (4)	Above Median (5)	Below Median (6)	Above Median (7)	Below Median (8)
Participation in the last week:								
High school attendance	0.064***	0.034			0.062***	0.063***		
	(0.017)	(0.021)			(0.019)	(0.018)		
Mean	0.82	0.80			0.81	0.80		
Paid Employment	-0.004	-0.003	-0.060	-0.097	-0.034*	-0.034	-0.551**	-0.539**
	(0.013)	(0.014)	(0.194)	(0.402)	(0.020)	(0.020)	(0.274)	(0.274)
Mean	0.14	0.13	0.14	0.13	0.29	0.27	0.29	0.27
Unpaid Employment	-0.010	-0.014	-0.149	-0.418	-0.024**	0.006	-0.397*	0.092
	(0.008)	(0.010)	(0.119)	(0.421)	(0.010)	(0.011)	(0.203)	(0.180)
Mean	0.04	0.06	0.04	0.06	0.06	0.09	0.06	0.09
Total Employment	-0.013	-0.018	-0.209	-0.515	-0.058***	-0.028	-0.948***	-0.447
	(0.015)	(0.017)	(0.221)	(0.572)	(0.021)	(0.020)	(0.313)	(0.281)
Mean	0.18	0.18	0.18	0.18	0.35	0.36	0.35	0.36
Log monthy earnings	-0.075	-0.010	-1.180	-0.308	-0.228	-0.333**	-3.701*	-5.329***
	(0.078)	(0.080)	(1.215)	(2.319)	(0.152)	(0.128)	(2.058)	(1.972)
Mean	0.84	0.70	0.84	0.70	1.67	1.47	1.67	1.47
NEET	-0.045***	-0.032*	-0.698***	-0.928***	-0.008	-0.008	-0.135	-0.123
	(0.013)	(0.018)	(0.138)	(0.247)	(0.009)	(0.010)	(0.122)	(0.140)
Mean	0.13	0.15	0.13	0.15	0.06	0.07	0.06	0.07
Bandwidth	54	54		54	54		54	54
Observations	24,065	22,206		12,104	10,626		11,961	11,580

Notes: Data is from the 2015 Household Labor Force Survey of Turkey. The bandwidth is 54 months in all regressions, which is the optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman (2009) algorithm when high school attendance is the independent variable. All columns report reduced-form (RF) RD treatment effects of being born after January 1998 with a linear control function in the month-year of birth on each side of the discontinuity. The columns with "above median" ("below median") report estimates for the sample of regions which had Islamic party vote shares above (below) the median in 2011. The variable definitions are listed in Appendix A. All specifications control for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age categories and gender present in the household, household size fixed effects, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

Table A6: Heterogeneous RD Treatment Effects on Type of School Attended Using a Static Bandwidth

	Female		Male	
	(1)	(2)	(3)	(4)
Panel A: Heterogeneous Effects by Poverty				
	Above Median	Below Median	Above Median	Below Median
Vocational high school attendance	0.052**	0.023	0.051***	0.077***
	(0.021)	(0.027)	(0.017)	(0.026)
Mean	0.31	0.37	0.33	0.41
Observations	12,148	9,156	12,881	9,607
Academic high school attendance	-0.004	0.037	0.004	-0.024
	(0.022)	(0.028)	(0.021)	(0.021)
Mean	0.46	0.44	0.45	0.42
Observations	12,148	9,156	12,881	9,607
Panel A: Heterogeneous Effects by Islamic Vote Share				
	Above Median	Below Median	Above Median	Below Median
Vocational high school attendance	0.066***	-0.007	0.069***	0.067***
	(0.022)	(0.027)	(0.025)	(0.017)
Mean	0.34	0.35	0.36	0.38
Observations	11,729	10,321	11,716	11,349
Academic high school attendance	-0.006	0.033	-0.012	-0.007
	(0.024)	(0.027)	(0.022)	(0.020)
Mean	0.46	0.44	0.44	0.42
Bandwidth	54	54	54	54
Observations	11,729	10,321	11,716	11,349

Notes: Data is from the 2015 Household Labor Force Survey of Turkey. The bandwidth is 54 months in all regressions, which is the optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman (2009) algorithm when high school attendance is the independent variable. All columns report reduced-form (RF) RD treatment effects of being born after January 1998 with a linear control function in the month-year of birth on each side of the discontinuity. In Panel A, the columns with "above median" ("below median") report estimates for the sample of regions which had poverty levels above (below) the median in 2011. In Panel B, the columns with "above median" ("below median") report estimates for the sample of regions which had Islamic party vote shares above (below) the median in 2011. The variable definitions are listed in Appendix A. All specifications control for a set of dummy variables indicating whether the father is present in the household, whether the mother is present in the household, counts of the number of household members by age categories and gender present in the household, household size fixed effects, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.