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Send correspondence to:
Meltem Dayioglu Tayfur
Middle East Technical University
dmeltem@metu.edu.tr

[1] Middle East Technical University, Department of Economics. Universiteler Mah., Dumlupınar Bul., No.1, 06800 Cankaya, Ankara.
[2] Central Bank of the Republic of Turkey, Structural Economic Research Department. Istiklal Caddesi No.10, 06050 Ulus, Ankara, Turkey. muserref.kucukbayrak@tcmb.gov.tr
[3] TED University, Department of Economics. Ziya Gokalp Cad., No.48, 06420 Kolej, Ankara, Turkey. IZA and ERF. semih.tumen@tedu.edu.tr

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Abstract

We exploit the age-specific minimum wage rule—which sets a lower minimum wage for workers of age 15 than that for workers of age 16 and above—to estimate its effects on youth employment and education in Turkey. Using a regression discontinuity approach, we find that youth minimum wage policy substantially reduced employment probabilities of young males. In terms of magnitudes, the employment probability declined by 2.5-3.1 percentage points at 16-year-old age cut-off. Due to the policy, probability of unemployment increased around 2 percentage points. Our findings also suggest that the policy change increased high school enrollment among young males. We conjecture that the effects of the policy have mostly been driven by the demand-side forces rather than the supply side.

Keywords: Age-specific minimum wages; youth employment; education; regression discontinuity design.

JEL Classifications: J21; J24; J31; J38.

1 Introduction

Youth are disproportionally represented among the unemployed. According to the latest estimates of the ILO (2017), there are 70.9 million unemployed youth (ages 15-24) worldwide, constituting over 35 percent of the total unemployed. What explains their over-representation is the higher unemployment risk they face, which the ILO estimates it to be three times that of adults, and not their population size. Lam (2014), argues that there is only a weak relationship between the proportion of youth and the unemployment rate, so that a decline in the youth ratio is not likely to offer a natural solution to their high unemployment rate. Furthermore, empirical evidence shows youth to be more vulnerable to economic shocks and downturns. At the height of the global financial crisis, while the unemployment rate among youth in OECD counties averaged at 16.8 percent, this figure was 7.1 percent among adults.[1] High and repeated spells of unemployment is a concern not only because it affects the current livelihoods of young people but also because it affects their future labor market outcomes in the form of lower employment and wages (Gregg and Tominey, 2005). If high unemployment rates among youth are a cause for concern, particularly as economies around the world head towards what is likely to be one of the worst recessions in recent times, another concern relates to the group of young people who are neither in employment nor in education or training (NEET). ILO (2017) estimates the size of this group to be over a fifth of the young people. Non-employment for out-of-school youth means loss of labor market experience and wages and poorer job prospects, but also higher likelihood of adverse behavior that may include involvement in crime, drugs, and other antisocial behavior (Henderson et al., 2017). Facilitating a smooth transition from school-to-work is, therefore, an important policy objective of governments.

Among the measures used in facilitating school-to-work transition are sub-minimum wages or what is referred to as youth minimum wages. The age-based determination of minimum wages is quite common in countries that have minimum wages, though its implementation shows variation by age threshold used to demarcate youth and adult rates, the number of age thresholds used and therefore, the number of youth minimum wages instituted, and the youth-adult minimum wage ratio (Grimshaw, 2014). Sub-minimum

[1] OECD Statistics data base, https://stats.oecd.org.

wages for youth are justified on the grounds that young workers lack job experience, and therefore, they are of lower productivity as compared to adults and that they receive on-the-job training, be it formal or informal, for which employers need to be compensated. Furthermore, statutory minimum wages may compress the wage distribution and therefore, increase the wages received by young workers at the lower end of the distribution (Acemoglu, 2001), which may in turn increase the opportunity cost of schooling, thereby discourage further education (Belman and Wolfson, 2014). Arguments against sub-minimum wages include age-based discrimination and the principle that wages should be determined not on the basis of age but on the basis of productivity, which may not differ between adults and youth in low productivity jobs. The assumption that youth benefit from on-the-job training may not prove correct either. Although setting a high minimum wage may in theory increase the opportunity cost of schooling, higher wages may also reduce the job finding rate among youth so that forgone wages may fall instead, increasing school participation (Pacheco and Cruickshank, 2007). Furthermore, youth may drop out of school for reasons other than their current labor market prospects such as low benefits of schooling due, for instance, to poor quality of schooling, limited access to school or poor school performance.

In Turkey, teenagers 15 years of age and above can legally work. Until 2014, 15-year-olds were entitled to receive 85 percent of adult minimum wages for which the age threshold was set at 16 years. On January 1, 2014, the youth minimum wage was abolished and the coverage of the adult minimum wage was extended to include 15-year-olds. The policy change was unexpected since there was no discussion or deliberation on it prior to the annual December meeting of the tripartite Minimum Wage Commission that sets the countrywide minimum wage. We exploit both the age-based rule as well as its abolition in understanding how the education and labor market outcomes of 15-year-old boys change due to the policy. We employ a Regression Discontinuity Design (RDD) and monthly data that allow us to assess the impact of the minimum wage as 15-year-olds turn 16 and get entitled to receive a higher wage. Furthermore, we look at how the elimination of the age-based rule change the outcomes of interest within a Difference-in-Discontinuities design. We also carry out a battery of robustness checks that include falsification tests that have become common in RD designs. In our analyses, we use various rounds of a representative micro panel dataset, the Turkey Survey of Income and Living Standards

(SILC). We restrict our analysis to boys since they are more likely to enter the labor market and respond to market incentives as compared to girls, whose decisions are also affected by social, cultural and religious factors in Turkey (SPO and the World Bank, 2009).

The empirical literature on the effect of sub-minimum wages on youth employment is concentrated on developed countries, with very little work done on developing countries. Furthermore, available evidence is mixed. We contribute to this literature by offering a case study from Turkey, where the prevalence of work among teenagers is high, informality is widespread and enforcement of labor regulations in the formal sector including the minimum wage is strict. Although the dual nature of the labor market may suggest that the youth minimum wage would be relevant for the formally employed youth and will not be binding for others in informal employment, we argue and demonstrate that informal sector wages are closely tied to the minimum wage in Turkey.

We find the youth minimum wage policy to reduce the employment probability of 15-year-old boys by 2.5-3.1 percentage points. Given that prior to the policy change the average employment rate of 15-year-olds boys was 12 percent, this would correspond to a 21-26 percent drop in their employment probability. We also find that unemployment in this age group increases by about 2 percentage points, which is also substantial given that 7 percent of 15-year-olds were unemployed in 2013. There is some evidence that school participation of boys also increases by 1.4 percentage points but this is a much smaller change particularly when judged against 78 percent average school enrollment of this age group. The probability of being in NEET also increases by 1.8 percentage points. Our main results come from a standard RDD. However, our findings do not change appreciably when we use the Difference-in-Discontinuities design where we exploit the abolition of sub-minimum wages. Our findings are consistent with a demand side explanation: as the coverage of adult minimum wages expands to 15-year-olds, this group of young boys lose their cost advantage over older boys. Some of those who lose their jobs return to school, others queue for higher paying jobs, yet some leave the labor market.

The plan of the paper is as follows. Section 2 briefly summarizes the relevant empirical literature. Section 3 presents background information on the youth minimum wage policy in Turkey. Section 4 describes the data and the identification strategy. Section 5 presents the findings in three sub-sections. First, we start by presenting with the regression discontinuity estimation results followed by the results for Difference-in-Discontinuities estimations. The final part in Section 5 is devoted to robustness checks. Section 6 concludes.

2 Related literature

How minimum wages affect the labor market is one of the most extensively studied topics in labor economics. Particular attention has been paid to the employment and unemployment effects of minimum wages on low-skill workers and youth, the two groups most likely to be affected by such policies. Such studies mostly consider the impact of uniformly applied minimum wages on youth rather than the impact of age-specific policies.[2] In what follows, we mainly summarize the findings of studies that consider age-specific minimum wage policies.

Using firm-level micro data, Pereira (2003) studies the effect of the abolition of youth minimum wages for 18-19-year-olds in Portugal in 1987 and finds adverse employment effects for this age group as firms substitute away from youth towards 20-25-year olds. Portugal and Cardoso (2006), using matched employer-employee panel data and the same policy change in Portugal but for 17-19-year-olds, which resulted in a substantial wage increase but not abolition of youth wages for 17-year-olds, find that firms reduce the share of youth among the newly hired workers. However, the authors also find a reduction in job separation rates for youth in existing firms. Yannelis (2014) finds evidence for Greece that the introduction of an age-specific minimum wage for workers under 25 years of age has favorable effects on 20-24-year-olds as compared to 25-29-year-olds. Hyslop and Stillman (2007) study the policy reform that changed the age structure of youth minimum wage and its rate in New Zealand. With the policy, the age group exposed to the youth rate is lowered from 18-19-year-olds to 16-17-year-olds. They find adverse effects on youth employment two years after the reform, despite failing to find an effect in the shorter run. Utilizing the abolition of youth minimum wages in six provinces of Canada, Shannon (2011) finds weak evidence for a reduction in employment rate and hours worked for 15-16-year-olds following the reform. Marimpi and Koning (2018), employ cross-national

[2]Among others see Allegretto et al. (2011), Sen and Waal (2011), Gorry (2013), Neumark and Wascher (2014), Liu and Regmi (2016).

data from 30 OECD countries for the 2000–2014 period and find that in countries where youth minimum wages apply employment and labor force participation rates of persons younger than 25 are relatively higher.

The majority of the aforementioned studies employ a difference-in-differences (DID) methodology in assessing the minimum wage policy impact. Another common empirical approach used in the literature is RDD. Exploiting the discontinuities of a stepwise minimum wage structure in the Netherlands applicable to 15-23-year-old workers, Kabatek (2015) finds a significant increase in job separation rates around the discontinuity points. Similarly, Olssen (2011) examining how a 10 percent increase in minimum wage for each year until age 21 affects the employment of 15-21-year-old workers in Australia, arrive at the conclusion that increases in minimum wage does not significantly affect youth employment hours. Kreiner et al. (2017) find for Denmark that as workers turn 18 and get entitled to a 40 percent increase in minimum wages, their employment drops by 15 percentage points. Fidrmuc and Tena (2018) also find negative employment and labor force participation effects of youth minimum wages applicable to workers younger than 22 in the UK. Dickens et al. (2014), however, find the youth minimum wage policy to increase employment and activity rates of low-skilled youth in the UK.

In regards to education outcomes, Neumark and Wascher (1995a,b,c, 2003), studying the relation between minimum wage, employment and school enrollment in the US, find that minimum pay policies lead students to leave school to queue up for minimum wage jobs. Neumark and Wascher (1995a,b) also find that the minimum wage policy increases the proportion of teens who are neither in school nor employment. They argue that employers substitute more skilled youth for less skilled youth, which leads the latter to be out of work and out of school. Pacheco and Cruickshank (2007) find that increases in minimum pay reduce the enrollment rates of 16-19-year-olds in New Zealand. However, using Canadian data from 1993 to 1999, Campolieti et al. (2005) show that minimum wages do not significantly affect school enrollment of young people. Similarly, Crofton et al. (2009) find minimum wages not to be significantly associated with dropout rates except for Hispanic students.

There are only a handful of studies that examine the extent at which the Turkish labor market is affected by minimum wage policies and mostly, they focus on aggregate effects and employ methodologies that cannot establish causality [see, e.g., Ozturk (2012)]. Exceptions do however exist that use various strategies for identification given that minimum wages are set at the national level. Gurcihan-Yunculer and Yunculer (2016), for instance, use the 2004 minimum wage hike and variation in the proportion affected by industry and occupation groups within a DID framework, but fail to find a significant negative employment effect, neither overall nor for 15-24-year-old workers. However, they do find a compression in wages at the lower end. Favorable economic conditions at the time, which may have facilitated the unusually high increase in minimum wages, may explain the lack of an adverse employment effect. Using the same policy change and a similar methodology, but regional variation in the percentage of workers earning wages equal to or lower than the minimum wage for identification, Bakis et al. (2015) find that the minimum wage increase reduces the labor supply of teenagers (ages 15-19) and increases their school enrollment. Using the regional variation in minimum wage to median wage ratio (the Kaitz index), Pelek (2015) also finds negative employment effects of minimum wages for 15-29-year-old workers for the period covering 2004-2014. Different from the aforementioned studies that employ cross-sectional data, Papps (2012) make use of the rotating panel feature of Household Labor Force Survey (HLFS)[3] and the variation in the ratio of labor costs to gross wages over time among low-wage workers within a DID framework and conclude that increases in minimum wages reduce the probability that a worker remains employed a quarter later with a larger impact on those under 30.

We add to this literature by explicitly focusing on youth minimum wages and teens and a rich set of outcomes that include employment, unemployment, labor force participation and NEET, thereby depicting a fuller picture on youth minimum wage effects. Unlike the change in minimum wages in general, the change in youth minimum wages does not create an income effect via changes in household income contributed by other household members and therefore, is more appropriate in understanding its unique impact on youth.

3 Institutional setting: Age-specific minimum wages in Turkey

A significant fraction of workers in Turkey earn just the minimum wage. Totally, 35.8 percent of private formal sector workers, i.e., those with social security registration, and

[3] HLFS in Turkey has a specific rotating scheme where households are in the sample 4 times over an 18-month period. However, the panel structure of HLFS is not publicly available.

20.7 percent of public sector workers in 2017 were reported as minimum-wage earners to the Social Security Institution. The tripartite Minimum Wage Determination Commission sets the minimum wage at least every two years since 1951.[4] Due to high inflation rates, from 1997 to 2015 the Commission determined the minimum wage twice a year, but annually since 2016.

From 1989 to 2013, workers younger than 16 were subject to a lower minimum wage called the youth minimum wage. Age-specific minimum wage policy aimed to facilitate school-to-work transition of young individuals. Between 1994 and 2013, the gap between the youth and adult rates were more or less stable with workers younger than 16 (essentially 15-year-olds) receiving nearly 15 percent less than those 16 and above.

On December 31, 2013, the Minimum Wage Determination Commission abolished the age-specific minimum wage policy and declared a single (adult) minimum wage to be applied to all minimum-wage workers from January 1, 2014. This change was not anticipated. The issue of setting a single minimum wage for all workers irrespective of age was raised during the meetings, beginning on December 6, 2013 and ending on December 31, 2013. However, it did not receive any media attention or coverage prior to its announcement at year end. The employer representative sitting on the tripartite committee voted against the abolition of the youth minimum wage and reflected employers' views in writing saying that they were in support of the youth minimum wage and its extension to age 21. As a consequence of this policy change, the nominal minimum wage applied to 15-year-old workers increased by 20.7 percent from December 2013 to January 2014. Taking inflation into account, the real minimum wage for workers under age 16 increased by 14.3 percent in the first half of 2014, while that for workers age 16 and above hardly changed.

This policy change potentially increased employers' labor cost.[5] As shown in Figure (1) until 2014, the real cost of minimum-wage workers under age 16 was substantially lower than that of older workers. Between the first half of 2007 and second half of 2013, the real cost of minimum-wage workers under age 16 was, on average, 12.2 percent lower than

[4] The Labor Act no. #4857 and the Minimum Wage Regulation constitute the legal premise of minimum wage.

[5] The average tax wedge for a single worker in Turkey is about 39 percent. It has fluctuated between 37-39 percent during in the last decade with only 0.7 percentage point increase in 2014, the year the minimum wage policy changed (OECD, 2020). This constancy generates a parallel trend in the amount of pay received by 15-16-year-old workers and what it costs employers to employ them.

it was for older workers. Following the policy change, in the first half of 2014, the real cost of 15-year-old minimum wage workers to employers increased by 14.1 percent.

An important concern for our study is whether youth minimum wages bind given the high incidence of informality, particularly among teens in Turkey. In the standard two-sector neo-classical model, an increase in minimum wages decreases employment in the formal sector and depresses wages in the informal sector (Mincer, 1976). Contrary to the predictions of the classical model, empirical work have shown that minimum wage policy can create spillover effects so that it increases wage rates even in segments where minimum wages do not apply (Maloney and Mendez, 2003, Lemos, 2009, Del Carpio and Pabon, 2017). Acting as a reference price for wage setting processes, minimum wages can create what is referred to as a 'lighthouse effect'.[6] Indeed, the minimum wage in Turkey is an important reference point for collective bargaining both in the public and private sector, its level is intensely debated and is used by the government as a reference point for various social transfers. Therefore, we would expect the minimum wage to have economy wide effects. We look for empirical evidence for the relevance of youth minimum wage for teen wages by comparing the two. Since monthly wage data is not available in SILC, we turn to HLFS of Turkey.[7] In particular, we examine the teen wage distribution using the Kernel estimator and look for spikes in and around the youth minimum wage. Kernel density estimates are commonly used in empirical literature because they depict unconditional wage distributions, thereby showing spikes if there exists any, which, if occur around the minimum wage are taken as an indication that minimum wages bind [see, for example, Pereira (2003); Portugal and Cardoso (2006); Rani et al. (2013)].[8]

According to HLFS, 90.5 percent of 15-year-old and 84.2 percent of 16-year-old males were working without social security coverage in 2013. Using the same data source, in Figure (2) and Figure (3), we plot Kernel density estimator for the distribution of log real wages for 15 and 16-year-old boys in 2013 and 2014, respectively. The dashed lines in these figures correspond to log real minimum wages in each year.[9] Note again the

[6] There are also other explanations why minimum wages may increase average wages in the informal sector. These include sorting and compositional changes in the formal and informal sectors (Boeri et al., 2011) and demand factors (Fiszbein, 1992).

[7] In HLFS, date of birth information is not available, we observe respondents' age in years.

[8] Rani et al. (2013) point out that there can be other reasons creating spikes in the wage distribution such as the presence of wages specific to some occupations. Since we are looking at teens, who are concentrated in low-skill occupations, it is unlikely that occupation specific factors would generate spikes.

[9] Since the minimum wage is set biannually in Turkey, we take the averages of minimum wages for each group in each year to avoid complication in the figures.

substantial improvement in minimum wages for 15-year-olds as the youth minimum wage is replaced by adult minimum wage in 2014. In real terms, no improvement is observed in adult minimum wages from 2013 to 2014 so that there is only one dashed line for 16-year-olds in Figure (3). A visual inspection of these figures suggests that young male workers are concentrated at or around the real minimum wage in both years. Furthermore, an improvement in the wage distribution of 15 and 16-year-olds is observed following the policy change: both distributions shift to the right and the concentration around the minimum wage increases in 2014. No other spikes other than the ones observed around the minimum wage are observed, which suggest that the minimum wage has relevance for 15-year-old and 16-year-old male workers.

4 Data and empirical approach

4.1 Data description and summary statistics

We employ the 2012-2015 and 2014-2017 waves of SILC of Turkey. SILC is a micro-level longitudinal household survey, which has been annually conducted by the Turkish Statistical Institute (TurkStat) since 2006. It has a rotating panel design, where respondents are retained in the sample for four years. Even though SILC is compiled annually, it includes retrospective monthly information on the general activity status of individuals aged 15 years and above. We make use of this monthly data in tracking changes in education and labor force status of youth over a 12-month period each year. A caveat with monthly data is that it does not provide detailed information on labor market activity such as hours of work, occupation held or wages earned. Neither do we observe whether the respondent is employed formally or informally. Therefore, the outcome variables we analyze, which are self-reported, include whether the respondent is employed, unemployed, in education (or in training) or in NEET. We determine respondent's labor force status using the information given on employment and unemployment status. Following the definition of OECD, we define NEET to include the unemployed and inactive. Another advantage of SILC over other data sources is that it contains month of birth and year of birth information so that we are able to generate age in months that allows us to observe changes in status as youth turn 16.

Since our focus is on the change in the activity status of 15-year-olds as they become eligible for a higher minimum wage, we use the 2014-2015 waves of the 2012-2015 panel that provide monthly information over a 24-month period from January 2013 to December 2014. We restrict our model estimations to 15-16-year-old males.[10] This corresponds to a 24-month bandwidth around the cut-off value of 16 years (and 0 months). We cannot include individuals younger than 15 since they cannot legally work. To keep the comparison groups as similar as possible we do not include boys older than 16 either.[11] Numerous employment subsidies exists for youth as they turn 18, which may alter employers' demand in their favor. The behavior of 17-year-olds may also differ due to the change in schooling options; students typically graduate from high school at age 17, upon which they may choose to pursue some form of higher education. For 15 and 16-year olds, the choice of schooling is whether or not to attend high school.

The first two panels of Table (1) presents several characteristics of 15-year-old and 16-year-old males before the policy change in 2013. The last two panels repeat the same exercise for 2014. In terms of individual and household characteristics, 15 and 16-year-olds are very similar. Both groups of children have, on average, completed 7.6 years of schooling, which correspond to a little less than lower-secondary schooling. The overwhelming majority self-report to be in good health. They live in households with 4.2 persons and have a household head (typically the father) that has, on average, about 6 years of schooling.

In terms of the outcomes of interest, in 2013, 78 percent of 15-year-olds were in school, as compared to 70 percent of 16-year-olds. In contrast, a larger percentage of 16-year-olds (27 percent) were in the labor force as compared to 15-year-olds (at 19 percent). The proportion in employment and unemployment (as a percent of their population) were higher among the older group, so was the percent in NEET. Note, however, that the majority of NEET for both age groups are made up of unemployed as opposed to 'idle' youth. Going from 2013 to 2014, the proportion of 15 year-olds enrolled in school increased, those in employment fell, while the proportion unemployed went up. Parallel changes did not occur for 16-year-olds, among whom the proportion in unemployment and NEET fell.

[10] This corresponds to young males who are aged between 15 years-0 month-old to 16 years-11 months old.

[11] Kreiner et al. (2017) point out similar threats to RD setting in their study. They discuss that in Denmark, teenagers not only are able to receive higher minimum wages but also become eligible to certain types of welfare benefits as they cross the age threshold. To eliminate the potential bias, they remove welfare benefit recipients from their analysis.

When we turn to mean hours of work and monthly wages, which we compute based on information provided for the reference week in the month the teens were interviewed, we observe lower hours of work for 15-year-olds in 2013. The gap in work hours decreased in 2014, as the mean hours of work increased for 15-year-olds but slightly decreased for 16-year-olds. The mean log monthly wages were higher among 16-year-olds in 2013, and this gap increased as the mean wages of the older group increased.

4.2 Empirical Methodology and Identification Strategy

We use two different identification strategies in understanding the effect of youth minimum wages on the labor market and education outcomes of 15-year-olds. The first strategy relies on the fact that 15-year-olds get entitled to receive a higher minimum wage as they turn 16. Using 2013 monthly data, we track the outcomes of 15-year-olds just before and after they celebrate their 16th birthday. The second identification strategy relies on the abolishment of youth minimum wages on January 1, 2014, for which we use monthly data from 2013 and 2014.

The data structure for the first identification strategy fits well to a RDD, which is typically used in program evaluations when assignment to the program (or "treatment") is determined by a known continuous variable or the rating variable. In our case, the rating variable is age; teens who are younger than 16 receive a sub-minim wage but those 16 and above the adult wage.

Let $D_i = D(z_i) = \mathbb{1}(z_i \geq z_0)$, where the rating variable z is age and z_0 is the age cut-off (16 years) when treatment changes. The outcome variable, y_i, can take two values based on z: y_{1i} if a young worker is able to get the adult minimum wage, i.e., $D_i = 1$, or y_{0i} if he is not. The difference between these two, $y_{1i} - y_{0i}$, gives the impact of the youth minimum wage policy (Angrist and Pischke, 2008). However, a person can be either 16 years old and over, or under, but never both. Therefore, we cannot observe y_{1i} and y_{0i} at the same time to derive the impact of the policy (Imbens and Lemieux, 2008). Yet, the RD design enables us to evaluate the policy effect by comparing average outcomes of the persons who are just below and just above the age threshold

Under continuity and certain smoothness conditions in the close vicinity of the cut-off (16 years), the average effect of the youth minimum wage policy can be obtained by differing left and right limits of the conditional expectation function (CEF). More formally, Equation (1) gives the effect of this policy (Hahn et al., 2001):

$$\lim_{z \searrow z_0} \mathbb{E}[y_i|z] - \lim_{z \nearrow z_0} \mathbb{E}[y_i|z] = \mathbb{E}[y_{1i} - y_{0i}|z = z_0] = \mathbb{E}[\beta_i|z = z_0]. \tag{1}$$

Based on continuity, $\mathbb{E}[y_{1i}|z = z_0 - \varepsilon]$ can be regarded as a counterfactual for $\mathbb{E}[y_{1i}|z = z_0]$, for arbitrarily small $\varepsilon > 0$. However, our rating variable age is available in months which might violate the continuity condition on potential outcomes (Calonico et al., 2014). In fact, we might not compare local averages at $z = z_0$ and $z = z_0 - \varepsilon$, because we do not observe outcomes for all small $\varepsilon > 0$. However, in their influential work, Lee and Card (2008) argue that RD inference can still be possible even with a discrete rating.[12] In the case of a discrete rating, we can identify $E[\beta_i|z = z_0]$ by Equation (2):

$$y_i = \alpha + \beta D_i + f(z_i) + \eta_i \tag{2}$$

where $u_i = f(z_i) + \eta_i$ and $f(\cdot)$ is a continuous link function such that $f(0) = \mathbb{E}[y_0|z = z_0]$. By approximating this function with a first order polynomial[13], Equation (2) becomes

$$y_i = \alpha + \beta D_i + \gamma(z_i - z_0) + a_i + \eta_i. \tag{3}$$

Here, $a_i \equiv f(z_i) - \gamma(z_i - z_0)$ is the specification bias which measures the deviation of $f(\cdot)$ from true CEF. It is also assumed to be random with $\mathbb{E}[a_i|z = z_i] = 0$.[14] Since the specification bias is viewed as a random error, there exist a within-group correlation in η. To account for this correlation, error terms should be adjusted to have consistent estimates for β. Indeed, if we assume the equality of random errors in each side of the cut off, clustered standard errors will be valid for inference (Lee and Card, 2008).

Letting different trends in both sides of the cut-off, the model we estimate becomes:

$$y_i = \beta_1 D_i + \beta_2(z_i - z_0) + \beta_3 D_i \cdot (z_i - z_0) + u_i, \tag{4}$$

where y_i is a binary outcome variable -employment,unemployment, labor force participation, school enrollment and NEET- D_i is the treatment dummy taking the value of 1 for individuals 16 and older, and $z_i - z_0$ is age in months relative to 16th birthday. Following

[12] They propose a parametric approach because local linear regression cannot assign any weight to the observations on $z_0 - \varepsilon$ for very small ε due to lack of continuous data. However, later research reveals that non-parametric approach can be also used [e.g. Calonico et al. (2014)]. We follow both approaches in the estimation of our models.

[13] Higher order polynomials are also possible. The idea, however, remains the same.

[14] Lee and Card (2008) point out that orthogonality of a_i and z_i might not be always easy to satisfy. However, the classical approach requires no specification error, which is a condition that is more restrictive.

Gelman and Imbens (2018), we allow for a first order polynomial link between outcome and rating variables. We also include quarterly calendar time and month of birth dummies as controls. Additional covariates are not used because as argued by Angrist and Pischke (2008) they are not necessary to identify unbiased or consistent estimates in the RD design.

The abolition of youth minimum wage in 2014 offers a unique opportunity to assess its impact on 15-year-olds. The proposed analysis is in the spirit of DID, where we compare the discontinuity before and after the policy change. Borrowing from Grembi et al. (2016), we call it as "difference-in-discontinuities" (or "diff-in-disc") approach.

Within the diff-in-disc framework, y_i can take four values: y_{1i}, post (when $T_i = 1$, and Post $= 1$), y_{0i}, post (when $T_i = 0$, and Post $= 1$), y_{1i}, pre (when $T_i = 1$, and Post $= 0$) or y_{0i}, pre (when $T_i = 0$, and Post $= 0$). T is dummy 1 for individuals under 16 and Post is 1 for year 2014 and 0 for 2013. Letting $\mu^-_{\text{pre}} = \mathbb{E}[y_{0i}|z_i = z_0, t \leq t_0]$, $\mu^+_{\text{pre}} = \mathbb{E}[y_{1i}|z_i = z_0, t \leq t_0]$, $\mu^-_{\text{post}} = \mathbb{E}[y_{0i}|z_i = z_0, t \geq t_0]$, and $\mu^+_{\text{post}} = \mathbb{E}[y_{1i}|z_i = z_0, t \geq t_0]$, Grembi et al. (2016) show that $\hat{\tau}_{DD}$ (Equation (5)) is the diff-in-disc estimator for the treatment effect:

$$\hat{\tau}_{DD} = (\mu^+_{\text{post}} - \mu^-_{\text{post}}) - (\mu^+_{\text{pre}} - \mu^-_{\text{pre}}). \tag{5}$$

Based on this, we estimate the following equation under diff-in-disc framework:

$$y_i = \beta_1 D_i + \beta_2(z_i - z_0) + \beta_3 T_i \cdot (z_i - z_0) + \alpha_1 \cdot \text{Post} + \alpha_2 T_i \cdot \text{Post} + u_i. \tag{6}$$

where, the coefficient of interest is α_2. As in (4) we allow different trends on both sides of the cut-off.

4.3 Visual evidence

We start by presenting suggestive visual evidence on how the outcome variables evolve with age and observe the size and direction of a jump – if any - at the cut-off value. In Figure (4), Panels A through E, we plot the mean values for each outcome variable (i.e. employment, unemployment, labor force participation, NEET) by month of age for 15-16-year-old males in 2013. Before the policy change, teens become entitled to a higher minimum wage as they turn 16. Therefore, we center age in months at age 16 and show the distance in months from this cut-off value, extending 12 months in either direction.

We also plot linear trends, which we allow to differ on either side of the cut-off, along with 95 percent confidence intervals.

The employment probability of teens, which is given in Panel A of Figure (4), increases with age but registers a sharp drop at the cut-off. The magnitude of the decline is about 4 percentage points, suggesting that as 15-year-olds turn 16 and get entitled to receiving a higher minimum wage their employment probability drops. When we turn to the probability of unemployment, which is given in Panel B of Figure (4), we also observe a jump at the cut-off but in the opposite direction, suggesting an increase in the probability of unemployment. The magnitude of the change is about 2 percentage points. The probability of labor force participation (Panel C), which also increases with age, reduces at the cut-off but the drop is not statistically significant at the 5 percent significance level (p value=0.08). When we turn to school enrollment (Panel D), which falls with age, we observe a jump in the trend line showing an increase in enrollment, though the persistent decline resumes beyond the cut-off, and the jump itself is not statistically significant at the 5 percent level (with p value=0.12). Finally, we consider the change in NEET (Panel E), which also registers a sharp increase on the order of 3 percentage points that is statistically significant (p value=0.012), mimicking the changes observed for unemployment in Panel C. Hence, the visual analysis suggests worsening labor market outcomes for 15-year-olds, as their likelihood of employment decreases and unemployment and NEET increases as they turn 16.

5 Results

We first present the results of the RD model followed by the Difference-in-Discontinuities model.[15] The results of the former model are given in Table (3), with the outcome variables shown in columns 1 through 5. The results of the latter model are presented in Table (4), with the same format. For the RD model, the estimations include 14,256 observations of 15-16-year-old males in 2013. The diff-in-disc model include observations from 2014 as well and therefore, the number of observations increases to 28,823.

[15]The RD model rests on the assumption that assignment to 'treatment' is as good as random. Since the assignment variable depends on date of birth, it is unlikely to be manipulated. Nonetheless, we examine the discontinuity of the rating density at the cut-off value as a way of testing manipulation. Based on the density test developed by Cattaneo et al. (2015), we do not reject the null hypothesis of no manipulation (p value=0.50 under robust standard errors).

5.1 Results of the RD Model

The results of the RD model are presented in Panels A through C in Table (2). Panels differ in the estimation methods and bandwidths used. In Panel A, we present the estimation results of (nonparametric) local linear regressions, where we use optimally computed bandwidths. Following the algorithm developed by Calonico et al. (2017), we use mean-squared error (MSE) and coverage error-rate (CER) optimal bandwidths. In Panel B and C, we present logistic and OLS regression results, where we zoom in around the cut-off by taking a narrower bandwidth of 6-months.

The results for employment, given in Column 1, show a statistically significant negative effect in all estimations, providing strong evidence that as 15-year-olds turn 16 and get entitled to receive a higher minimum wage, their probability of employment drops. The estimated effect is on the order of 2.5 to 3.1 percentage points, which is quite sizable given that the average employment rate of 15-year-olds in 2013 was 12 percent. We are no able to show whether the employment loss originates from quits or lay-offs. Both are likely to play a role. The demand-side explanation would be that employers hire 15-year-olds due to their cost advantage, however, as this advantage is lost, they are replaced by more experienced older workers. As we have demonstrated earlier, only a small fraction of 15-year-olds actually receive a wage higher than the youth minimum wage. However, the observation that many are clustered around the minimum wage suggests that minimum wage is taken as a reference point in wage setting so that as boys turn 16 and become 'adults', there is the expectation that their wages would increase. Quits, probably, reflect this unmet expectation. We can, however, rule out any labor supply adjustment due to an income effect, since we are solely looking at youth and not adult minimum wages.

Our results suggest a drop in the employment of 15-year-olds by a fifth to a quarter due to the policy, which is within the range of estimates we encounter in the scant literature. Kreiner et al. (2017) finds that as Danish workers turns 18, their employment decreases by a third. Based on the employment elasticity of minimum wage, results of Pereira (2003) imply about a 15 percent employment decline for 18-19-year-olds. Similarly, elasticity estimates of Yannelis (2014) suggest approximately 10 percent employment impact for workers under 25.

The probability of unemployment, on the other hand, increases as 15-year-olds turn 16 (Column 2, Table (2)). The positive effect of the policy on unemployment is observed in all specifications, with an effect size of 1.9 to 2.2 percentage points. Considering that 7 percent of the 15-year-olds were unemployed in 2013, the estimated magnitude correspond to about a 30 percent increase. Within the framework of a two-sector model, this increase might be interpreted as youth queuing for higher paying jobs in the formal sector. However, in the Turkish context with a large informal sector, it is more likely that displaced youth look for other informal sector jobs. As noted earlier only a small proportion of 16-year-olds are employed formally and HLFS data show that nearly 80 percent of this age group look for jobs as elementary workers or sales and service workers. These are occupations where informal employment is pervasive.

The effect of the higher minimum-wage on labor force participation is negative in all estimations but it is not statistically significant (Column 3, Table (2)). Neither is the effect on school enrollment. Although the policy effect is positive in all estimations, in only one of the estimations, where a wider bandwidth is used, do we observe a statistically significant effect of 1.4 percentage points at the 10 percent level (Column 4, Table (2)). Given that 78 percent of 15-year-olds are in school, this would increase the school enrollment of boys by 2 percent, which is substantially smaller than the change observed for employment and unemployment. The substantially smaller effect suggests that the minimum wage plays only a small role, if at all, in keeping teens at school. In Child Labor Surveys conducted by TurkStat when school drop-outs are asked the reasons why they have left school, the overwhelming majority cite being disinterested in school. Hence, factors other than wages must act as pull factors in drawing children out of school along with push factors that perhaps include the school environment.

The effect of the minimum wage policy on NEET (which includes the unemployed) is positive and statistically significant in all estimations suggesting that the probability that youth are neither in employment nor enrolled in school or vocational training increases as they become entitled to receiving higher wages. This result is consistent with the fall in employment, increase in unemployment and practically no change, or at best, a minor increase, in school enrollment. Consistent with the explanation of Neumark and Wascher (1995a,b) as employing teens become more expensive, they are replaced by equally costly but more experienced workers. Hence, lower wages of 15-year-olds induce employers to hire them but this cost advantage lasts for a year, which is likely to be too short for learning a trade but too long to go back to school, having dropped out for at least a whole year.

5.2 Results of Difference-in-Discontinuities Model

We now turn to the results of the Diff-in-Disc model, which are presented in Table (3). We carry out parametric estimations using logistic and OLS regression by taking 6-month and 12-month bandwidths, whose results are given in Panels A and B of Table 4, respectively. The results for most of the outcomes studied are very similar to what is obtained from the RD regression estimates.

The results for employment and unemployment outcomes given in Table (3), Columns 1 and 2 respectively, suggest that the probability of employment for 15-year-old boys become less likely, while unemployment more likely due to the abolishment of the youth minimum wage in 2014. The estimated effect sizes are 3.2-5.9 percentage points for employment and 1.9-3.2 percentage points for unemployment, which are slightly higher than what is estimated using the RD model. The policy impact on labor force participation is negative and statistically significant in all but one estimation, suggesting that the labor force participation of 15-year-olds become less likely by 1.6-3.2 percentage points due to the abolishment of youth minimum wage. Although the RD model also shows a negative program effect for labor force participation, the size of the effect is higher under the Diff-in-Disc model and more likely that it is statistically significant. Due to the policy change, the probability that 15-year-olds will enroll in school increases by 1.7-2.7 percentage points. Although this is a slightly higher effect than the one estimated under the RD model, it is still relatively small when compared to the effects observed for employment and unemployment. Turning to NEET, the probability that 15-year-olds are neither in employment nor in education or training increases with the policy by 2.0-3.6 percentage points. Overall, similar to the case of the RDD model, the results of the Diff-in-Disc model suggests that the main adjustment to the policy occurs at employment, unemployment and NEET margins.

5.3 Robustness checks

We carry out three sets of robustness checks. The first concerns a placebo test, where we re-estimate equation (4) in the post-policy period. In the second robustness check, we consider the potential effects of the 2012 school reform and finally, in the third robustness check we re-estimate standard errors to see if cluster size matters for the significance levels of our estimates.

Placebo Test. In the placebo test we consider the possibility that the effects we find for youth minimum wages might be a data artifact or caused by factors other than the minimum wage policy. Hence, we re-estimate Equation (5) for 15 and 16-year-old boys using the 2014-2017 SILC panel for years 2015 and 2016 that correspond to the post-reform period. Since the same minimum wage applies to all workers regardless of age, we do not expect any discontinuity in the post-policy period. We are not able to repeat a similar robustness check for the pre-reform period, simply because there is no period during which youth minimum wages are not implemented.

The results presented in Table (4), which follows the same format as our main results given in Table (2), show no policy effect. Out of a set of six estimations for five outcomes, in only one case do we find a statistically significant coefficient for the policy, which we interpret as chance finding. Furthermore, the estimated effects are all close to zero.

2012 Education Reform. In 2012, compulsory education was increased to 12 years to cover high school education. Prior to the change, compulsory schooling consisted of 8 years of basic education. The new schooling system was put into effect beginning in September 2012 and affected children who would be graduating from 8-year-basic education but not those who were already enrolled in high school or dropped-out earlier. Assuming that children start school at age 6, which is the statutory school-start age, children born in 1998 or later are expected to be affected by the school policy. The change in mandatory schooling may contaminate our results if, independent of youth minimum wage, it creates a significant jump in high school enrollment of 15 and 16-year-old boys in years under study.

As discussed earlier, we do not find strong effects on the school enrollment of teens for the years under study. If the extension of compulsory schooling were effective, we would have seen a strong effect on school enrollment, which would then, perhaps, affect the labor market outcomes of teens. That this effect is absent in the first stage suggests that what we are finding as a policy effect is indeed to do with the minimum wage policy. Nonetheless, we re-estimate our RD model using data from 2011, a year before the change occurred in the school policy. The results shown in Table (**??**) suggests that in 2011, as 15-year-old boys turned 16 and qualified for higher minimum wage, their employment probability reduced by 3.3-3.6 percentage points, which is similar policy effect to the one noted earlier. We also observe a higher likelihood of school enrollment, with an effect size of 2.1-3.5 percentage points. Unlike the estimations results for 2013 and 2014, we do not observe a statistically significant policy effect on unemployment; the estimated coefficients are positive in all estimations with magnitudes similar to those obtained earlier but lack statistical significance. (Note that observation numbers are smaller in 2011 as compared to 2013.) That we do not find a policy effect on unemployment carries over to NEET, the majority of whom are made up of unemployed youth.

A potential reason that the extension of compulsory schooling does not bring about a significant change in high school enrollment include lack of enforcement. Tumen (2018) shows that weaknesses in enforcement hinders the implementation of the 2012 school reform. He finds that even though the law forces individuals to enroll in school, it does not create a jump in high school enrollment ratios, neither for boys nor girls. Another plausible reason is the introduction of distance-learning as part of compulsory school, which would allow youth to work full-time. Yet another explanation is the early school-drop out of youth. The descriptive statistics presented earlier suggested that youth completed fewer years than would correspond to 8-year compulsory basic education.

Clustering standard errors. Independent of research design, clustering standard errors is a routine procedure when observations within the same cluster are believed to have unobserved characteristics that are correlated. In our case, boys born in the same month of a given year might share common, but unobserved, characteristics. This is the reason why in all our estimations we cluster for birth-month in a given year. However, a potential problem with this correction is the use of too few clusters. Asymptotic approximations for clustered standard errors require large numbers of clusters (Angrist and Pischke, 2008). We re-run our parametric regressions with standard errors corrected by the Moulton (1986) factor as suggested by Cameron and Miller (2015). In addition, we perform wild cluster bootstrap to test significance of our treatment coefficients. The results (not reported) hardly change from those presented in Table (2) and are available upon request.

6 Conclusion

In this paper, we studied the causal impact of the youth minimum wage policy on labor market and education outcomes of teens in Turkey by making use of its age-based structure and abolition. Before 2014, 15-year-olds were entitled to receive 85 percent of adult minimum wages. This cost advantage was lost abruptly in January 2014 with the uniform application minimum wages irrespective of age. Using a Regression Discontinuity Design, which relies on the increase in minimum wage as 15-year-olds turn 16 before the policy change, and a Difference-in-Discontinuities design, which uses the abolishment of youth minimum wage in 2014 for identification, and micro data sets that cover periods before and after the policy change, we examined whether employment, unemployment, labor force, education and NEET participation of 15-year-old boys change due to the policy.

We argue and demonstrate that despite high informality, minimum wages are relevant and important for teens, which is consistent with the lighthouse effect discussed in the literature. We find that higher minimum wages increase the wages received by 15 and 16-year-old employed teens but it also the case that the employment probability of 15-year-old boys decline with the increase in minimum wages. The estimated effect is quite sizeable; a conservative estimate would suggest a reduction of boys' employment by a fifth. In contrast, the unemployment probability of boys increases – by 30 percent - but a significant change in their labor force participation is not observed. We do not observe an appreciable increase in boys' school enrollment either, however and in parallel with their heightened unemployment probability, they become more likely to be in NEET.

Our findings suggest that youth minimum wages are useful in easing the transition of youth from school to employment, which has been demonstrated by studies from developed countries. We add to this literature by showing that youth minimum wage policies can also work in developing country contexts despite the existence of informal working arrangements. However, we also note that restricting the eligible youth to a single age group may not be desirable since a single year is likely to be too short for the acquisition of job-specific skills and development of job attachment, but too long of an absence from school to make it a viable option. Stepwise structure of youth minimum wages, as used in some OECD countries, may offer a better alternative.

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Figure 1: Gross Statutory (Real) Minimum Wages, by Age (From the first half of 2007 to the end of the second half of 2015.) *Source:* Ministry of Family, Labor, and Social Services. *Notes:* Nominal monthly wages are deflated by producer price index (2007=100).

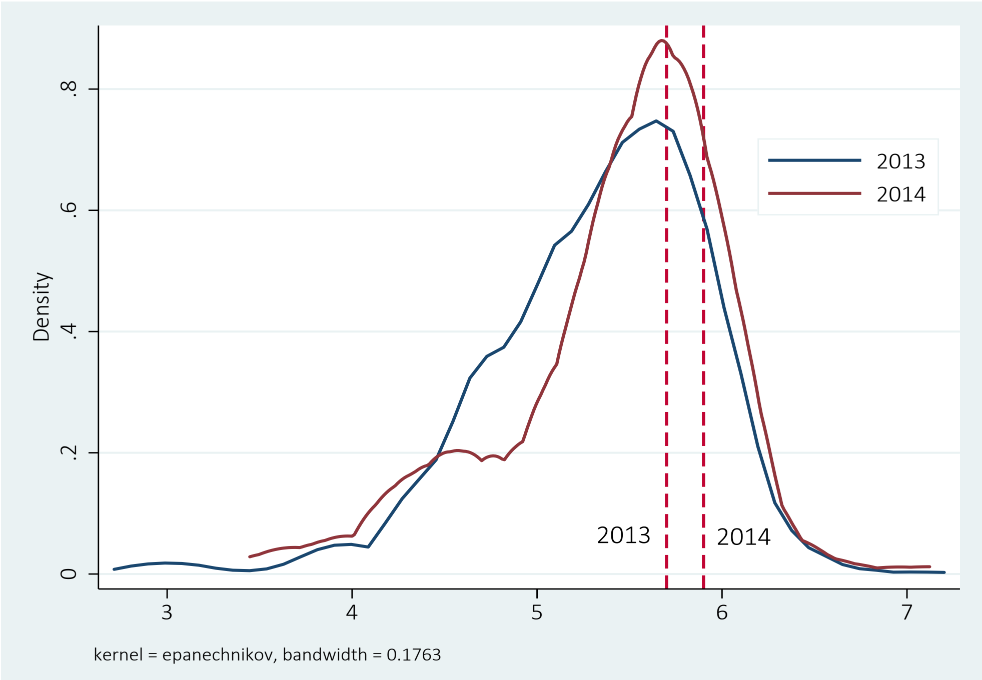

Figure 2: Kernel Density Plots of the Log of Real Monthly Wages, 15-Year-Old Boys
Source: Own calculations using 2013-2014 HLFS. *Notes:* Includes boys in employment only. Appropriate sampling weights are used. Dashed lines refer to log (real) minimum wages.

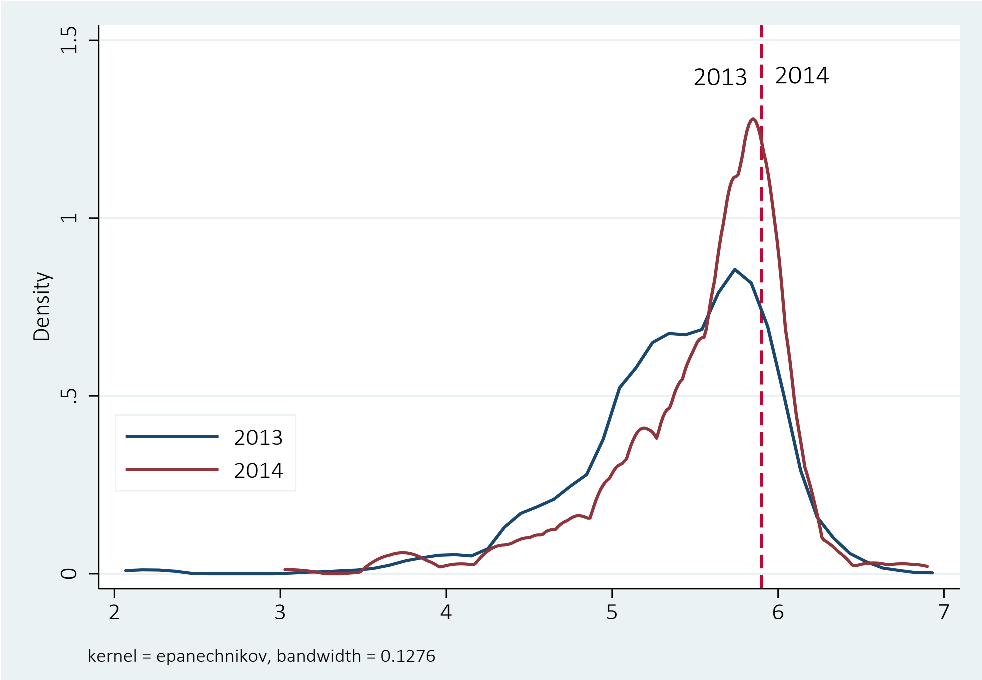

Figure 3: Kernel Density Plots of the Log of Real Monthly Wages, 15-Year-Old Boys. *Source:* Own calculations using 2013-2014 HLFS. *Notes:* Workers do not attend school while working. Appropriate weights are used. Dashed lines refer to the log of average minimum wage in a year in real terms.

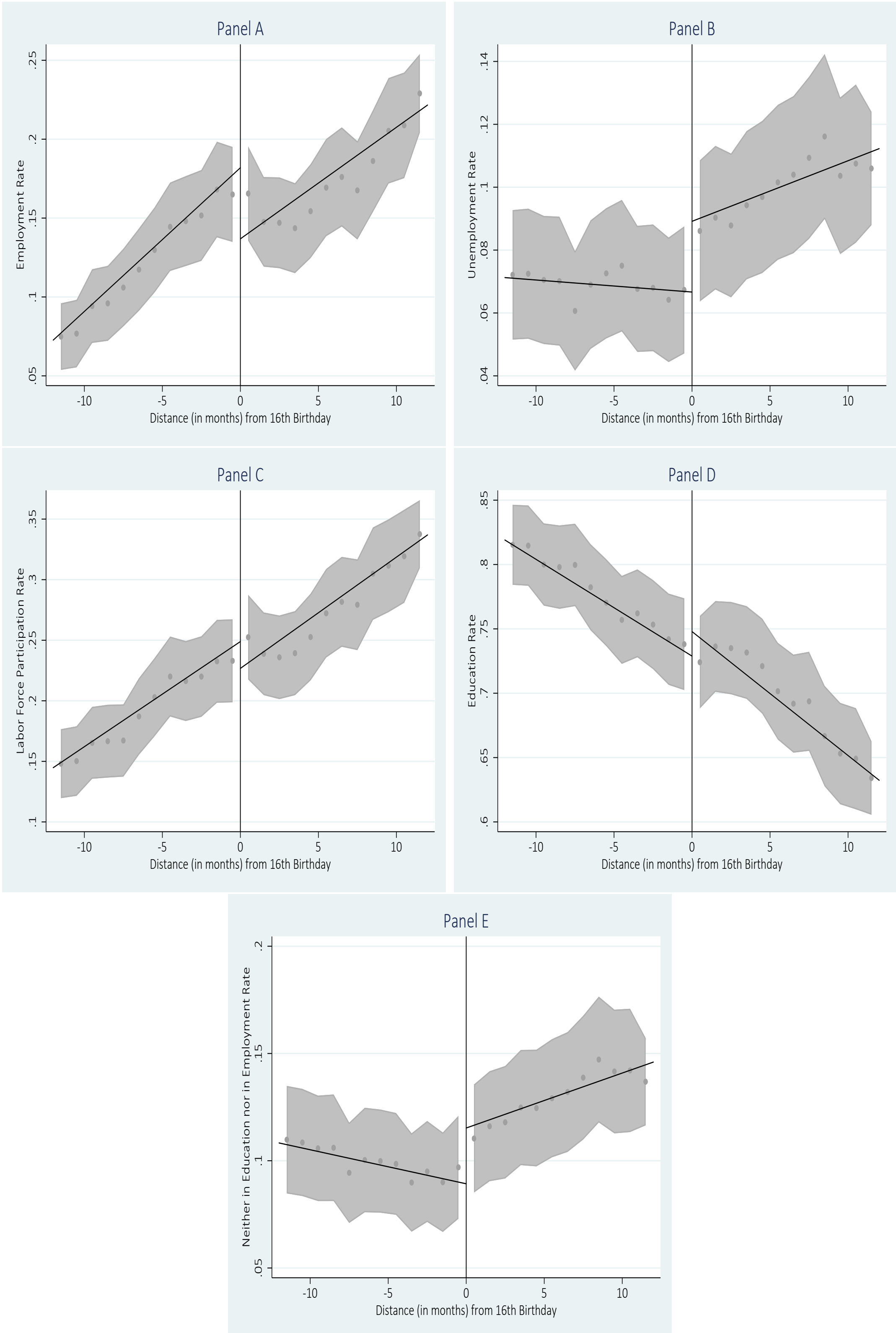

Figure 4: Labor Market and Schooling Outcomes of Young Males in 2013 *Source:* Own calculations using 2012-2015 rounds of SILC. *Notes:* Age in months is centered at 16 years. Points to the left and right of the cut-off represents the distance to age 16 in months.

Summary Statistics

	2013		2014		2014-2013 (p value for mean difference)	
	Age 16	Age 15	Age 16	Age 15	Age 16	Age 15
Years of educ.	7.6 (1.4)	7.6 (1.4)	7.6 (1.5)	7.6 (1.4)	0.86	0.99
In good health	0.92	0.91	0.92	0.93	0.12	0.00
Household size	4.1 (1.4)	4.2 (1.4)	4.1 (1.4)	4.2 (1.5)	0.14	0.97
Household head's years of educ.	6.4 (4.1)	6.0 (3.7)	6.2 (3.8)	6.3 (4.0)	0.01	0.00
In education	0.7	0.78	0.71	0.81	0.01	0.00
Employed	0.17	0.12	0.18	0.09	0.28	0.00
Unemployed	0.1	0.07	0.09	0.08	0.05	0.02
In labor force	0.27	0.19	0.27	0.17	0.38	0.00
Neither in emp. nor in ed.	0.13	0.10	0.10	0.10	0.00	0.42
Hours of work§	50.4 (19.2)	43.8 (19.7)	48.5 (18.5)	49.6 (19.3)	0.02	0.00
Log real monthly wage§	5.2 (0.73)	5.0 (0.73)	5.4 (0.84)	5.4 (0.84)	0.01	0.00
# of observations	7,012	7,244	7,260	7,303		

Table 1: **Summary statistics for 15 and 16-year-olds boys, by year** *Source:* Own calculations using 2012-2015 rounds of SILC data. *Notes:* Standard deviations are given in parenthesis for continuous variables. § Include employed teens in the reference week in the month the survey was conducted.

Estimation Results for RDD model (2013)

	Employed (1)	Unemployed (2)	In labor force (3)	In education (4)	Neither in em. nor ed. (5)
Panel A: Local Linear Regression					
Estimated coefficient	-0.031***	0.019***	-0.013	0.014*	0.018***
	(0.01)	(0.003)	(0.01)	(0.008)	(0.004)
MSE optimal bandwidth	12.67	19.19	13.01	12.45	19.98
# of Observations	7,179 (left)	7,244 (left)	7,179 (left)	7,179 (left)	7,179 (left)
	7,432 (right)	11,574 (right)	7,972 (right)	7,432 (right)	11,261 (right)
Estimated coefficient	-0.025**	0.019***	-0.01	0.01	0.018***
	(0.011)	(0.003)	(0.011)	(0.009)	(0.004)
CER Optimal Bandwidth	8.98	13.59	9.21	8.82	14.15
# of Observations	4,780 (left)	7,244 (left)	5,374 (left)	4,780 (left)	7,179 (left)
	5,201 (right)	8,123 (right)	5,766 (right)	5,201 (right)	8,523 (right)
Panel B: Logistic Regression					
Estimated coefficient	-0.026***	0.022***	-0.01	0.013	0.018***
	(0.007)	(0.003)	(0.01)	(0.008)	(0.004)
Bandwidth	6	6	6	6	6
# of Observations	7,670	7,749	7,670	7,670	7,670
Panel C: OLS					
Estimated coefficient	-0.030**	0.021***	-0.009	0.012	0.018***
	(0.011)	(0.002)	(0.01)	(0.008)	(0.004)
Bandwidth	6	6	6	6	6
# of Observations	7,670	7,749	7,670	7,670	7,670

Table 2: Source: Own calculations using 2012-2015 rounds of SILC. *Notes:* ***, ** and * refer to 1%, 5% and 10% significance levels, respectively. Standard errors, clustered at age (in months), are reported in parentheses. Marginal effects in logit estimates correspond to a discrete change in the probability. Quarterly calendar time dummies (Last quarter is the reference) and month of birth dummies (December is the reference) are used.

Estimation Results for Diff-in-Disc model

	Employed (1)	Unemployed (2)	In labor force (3)	In education (4)	Neither in em. nor ed. (5)
Panel A: Logistic Regression					
Estimated coefficient	-0.057***	0.032***	-0.031***	0.027***	0.036***
	(0.005)	(0.007)	(0.008)	(0.007)	(0.006)
Bandwidth	6	6	6	6	6
# of Observations	15,348	15,348	15,348	15,348	15,348
Estimated coefficient	-0.036***	0.021***	-0.016**	0.017**	0.020***
	(0.006)	(0.005)	(0.007)	(0.008)	(0.006)
Bandwidth	12	12	12	12	12
# of Observations	28,186	28,186	28,186	28,186	28,186
Panel B: OLS					
Estimated coefficient	-0.059**	0.028***	-0.032***	0.025***	0.034***
	(0.006)	(0.006)	(0.008)	(0.008)	(0.006)
Bandwidth	6	6	6	6	6
# of Observations	15,348	15,348	15,348	15,348	15,348
Estimated coefficient	-0.032***	0.019***	-0.014	0.01	0.021***
	(0.008)	(0.005)	(0.008)	(0.008)	(0.006)
Bandwidth	12	12	12	12	12
# of Observations	28,186	28,186	28,186	28,186	28,186

Table 3: **Source:* Own calculations using 2012-2015 rounds of SILC. *Notes:* ***, ** and * refer to 1%, 5% and 10% significance levels, respectively. Standard errors, clustered at age (in months), are reported in parentheses. Marginal effects in logit estimates correspond to a discrete change in the probability. Quarterly calendar time dummies (Last quarter is the reference) and month of birth dummies (December is the reference) are used.

Estimation Results for RDD model (2015-2016)

	Employed (1)	Unemployed (2)	In labor force (3)	In education (4)	Neither in em. nor ed. (5)
Panel A: Local Linear Regression					
Estimated coefficient	0.001	-0.002	-0.001	0.004	-0.005
	-0.003	-0.003	-0.004	-0.004	-0.003
MSE optimal bandwidth	17.28	14.53	15.82	15.97	14.71
# of Observations	12,913 (left)	12,913 (left)	12,913 (left)	12,913 (left)	12,913 (left)
	19,198 (right)	16,219 (right)	17,228 (right)	17,228 (right)	16,219 (right)
Estimated coefficient	0.0004	-0.002	-0.003	0.006	-0.005
	-0.003	-0.003	-0.004	-0.004	-0.003
CER optimal bandwidth	12.2	10.26	11.17	11.28	10.39
# of Observations	12,913 (left)	10,776 (left)	11,842 (left)	11,842 (left)	10,776 (left)
	14,146 (right)	12,024 (right)	13,100 (right)	13,100 (right)	12,024 (right)
Panel B: Logistic Regression					
Estimated coefficient	0.002*	0.001	0.003*	-0.003	0
	-0.001	-0.002	-0.002	-0.003	-0.003
Bandwidth	6	6	6	6	6
# of Observations	28,168	28,168	28,168	28,168	28,168
Panel C: OLS					
Estimated coefficient	0.002	0.0003	0.002	-0.002	-0.0001
	-0.001	-0.001	-0.002	-0.003	-0.003
Bandwidth	6	6	6	6	6
# of Observations	28,168	28,168	28,168	28,168	28,168

Table 4: Source: Own calculations using 2014-2017 rounds of SILC. *Notes:* ***, ** and * refer to 1%, 5% and 10% significance levels, respectively. Standard errors, clustered at age (in months), are reported in parentheses. Quarterly calendar time dummies (Last quarter is the reference), month of birth dummies (December is the reference) and a year dummy for 2016 are used. Effective number of observations are reported.

Estimation Results for RDD model (2011)

	Employed (1)	Unemployed (2)	In labor force (3)	In education (4)	Neither in em. nor ed. (5)
Panel A: Local Linear Regression					
Estimated coefficient	-0.035*	0.01	-0.033***	0.035***	0.002
	-0.019	-0.021	-0.01	-0.009	-0.015
MSE optimal bandwidth	17.09	24.73	19.35	15.48	26.08
# of Observations	2,375 (left)	2,375 (left)	2,375 (left)	2,375 (left)	2,375 (left)
	3,556 (right)	4,837 (right)	3,931 (right)	3,187 (right)	5,214 (right)
Estimated coefficient	-0.033*	0.009	-0.028**	0.035***	0.002
	-0.019	-0.021	-0.012	-0.01	-0.015
CER optimal bandwidth	12.15	17.57	13.75	11	18.53
# of Observations	2,375 (left)	2,375 (left)	2,375 (left)	1,986 (left)	2,375 (left)
	2,614 (right)	3,556 (right)	2,804 (right)	2,216 (right)	3,744 (right)
Panel B: Logistic Regression					
Estimated coefficient	-0.036**	0.024	-0.012	0.023**	0.012
	-0.014	-0.02	-0.013	-0.01	-0.016
Bandwidth	6	6	6	6	6
# of Observations	2,611	2,611	2,611	2,611	2,611
Panel C: OLS					
Estimated coefficient	-0.034**	0.023	-0.01	0.021*	0.012
	-0.013	-0.02	-0.013	-0.01	-0.016
Bandwidth	6	6	6	6	6
# of Observations	2,611	2,611	2,611	2,611	2,611

Table 5: Source: Own calculations using 2012-2015 rounds of SILC. *Notes:* ***, ** and * refer to 1%, 5% and 10% significance levels, respectively. Standard errors, clustered at age (in months), are reported in parentheses. Marginal effects in logit estimates correspond to a discrete change in the probability. Quarterly calendar time dummies (Last quarter is the reference) and month of birth dummies (December is the reference) are used.