The impact of age-specific minimum wages on youth employment and education: a regression discontinuity analysis

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Meltem Dayioglu
Department of Economics, Middle East Technical University, Ankara, Turkey
Müşerref Küçükbayrak
Central Bank of the Republic of Turkey, Ankara, Turkey, and
Semih Tumen
Department of Economics, TED University, Ankara, Turkey

Abstract

Purpose – Using a regression discontinuity design in tandem with a difference-in-discontinuities analysis, the study finds that increasing the minimum wage reduces the employment probability of young males by 2.5–3.1 percentage points.

Design/methodology/approach – The authors exploit an age-specific minimum wage rule – which sets a lower minimum wage for workers of age 15 than the adult minimum wage paid to workers of age 16 and above – and its abolition to estimate the causal effect of a minimum wage increase on youth employment and education in Turkey.

Findings – The authors also document that, initially, the minimum wage increase does not lead to a major change in high school enrollment, while the likelihood of transitioning into "neither in employment nor in education and training" (NEET) category notably increases. However, in the medium term, the NEET effect is transitory; school enrollment increases over time and absorbs the negative employment effect.

Originality/value – The authors argue that policy effects have mostly been driven by demand-side forces rather than the supply side.

Keywords Age-specific minimum wages, Youth employment, Education, Regression discontinuity design

Paper type Research paper

1. Introduction

The transition from school to work is a challenge for many young people. Lack of labor market experience, limited social networks and imperfect information on availability of jobs matching their skills make their labor market integration harder compared to adults. Indeed, ILO (2017) estimates that over a fifth of the youth population are neither in employment nor in education or training (NEET) and they face an unemployment risk that is three times that of adults. Furthermore, empirical evidence shows that young people are more vulnerable to economic shocks and downturns. Non-employment for out-of-school youth means loss of labor market experience and wages, poorer job prospects and higher likelihood of risky behavior including involvement in crime, drugs and other antisocial behavior (Henderson *et al.*, 2017).

JEL Classification — J21, J24, J31, J38

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Facilitating a smooth transition from school to work is, therefore, a very important policy objective for governments.

Among the measures used in achieving a smoother 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, although its implementation exhibits variation by (1) the age threshold used to demarcate youth and adult rates, (2) the number of age thresholds used (i.e., the number of youth minimum wages instituted) and (3) the youth-adult minimum wage ratio (Grimshaw, 2014). Sub-minimum wages for youth are justified on the grounds that young workers lack job experience and, therefore, they are of lower productivity compared to adults; so, they should receive on-the-job training for which employers need to be compensated. Furthermore, statutory minimum wages may compress the wage distribution and increase wages received by young workers at the lower end of the distribution (Acemoglu, 2001), which may in turn increase the opportunity cost of schooling and, 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 substantially differ between adults and youth in jobs with low skill requirements. 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 rates 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 academic performance.

In Turkey, the legal minimum age of full-time employment is 15. Until 2014, 15-year-olds were entitled to receive 85% 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 and its abolition to understand how employment and education outcomes of 15-year-old males change in response to the policy. We employ a regression discontinuity design (RDD) that allows us to assess the minimum wage impact 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 changes the outcomes of interest within a difference-in-discontinuities design. We also carry out a battery of robustness checks including various falsification tests that have become common in RDD estimations. In our analyses, we use various waves of a nationally representative micro-level longitudinal data set, the Survey of Income and Living Conditions (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 has mostly focused on developed countries, with very little work done on developing economies. Furthermore, the available evidence is mixed. We contribute to this literature by presenting the Turkish case, where the prevalence of work among teenagers [1] 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% points. Given that prior to the policy change the average employment rate of 15-year-olds boys was 12%, this would correspond to a 21–26% drop in their employment probability. Initially, there is only a small increase in high school enrollment and most young males who lost their jobs transition into the NEET category. However, in the medium and long term, the NEET effect is shown to be mostly transitory. School enrollment increases over time and absorbs the negative employment effect. In other words, our examination of medium- and long-term effects of the policy shows larger education effect and almost no NEET effect, suggesting delayed schooling response. Our main results come from a standard RDD implementation. However, our findings do not change appreciably when we use a difference-in-discontinuities analysis, where we exploit the abolition of sub-minimum wages. The results 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 individuals. Majority of those who lost their jobs return to school over time and a much smaller fraction queue for higher paying jobs or leave the labor market.

The plan of the paper is as follows. Section 2 summarizes the related literature. Section 3 presents background information on the institutional details of youth minimum wage policy in Turkey. Section 4 describes our data and identification strategy. Section 5 presents the findings in four sub-sections. We start by presenting 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 and medium-term effects. Section 6 concludes.

2. Related literature

How minimum wages affect the labor market is one of the most extensively studied topics in labor economics. The seminal paper by Card and Krueger (1994) challenged the conventional wisdom that higher minimum wages reduce employment and initiated a lively debate that is yet to be resolved [2]. In this literature, particular attention has been paid to the employment and unemployment effects of minimum wages on low-skilled workers and youth, the two groups most likely to be affected by such policies. Most of the existing studies consider the impact of uniformly implemented minimum wages on youth rather than the impact of age-specific policies [3]. In what follows, we focus our attention to 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- and 19-year-olds in Portugal in 1987, and finds that the increase in minimum wage applied to this age group reduces their employment, while creating a substitution effect toward 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 workers aged 17–19-years among the newly hired. However, the authors also find a reduction in job separation rates for them 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 positive employment 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 to 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-country data from 30 OECD countries for the 2000–2014 period and find that, in countries where youth minimum wages are implemented, employment and labor force participation rates of individuals younger than 25 are relatively higher than those in countries not implementing youth minimum wages.

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 (2021) finds a significant increase in job separation rates around the discontinuity points. Similarly, Olssen (2011) examining how a 10% increase in minimum wage for each year until age 21 affects the employment of 15–21-year-old workers in Australia, arrives at the conclusion that increases in minimum wage do not significantly affect youth hours of work. Kreiner *et al.* (2020) find for Denmark that as workers turn 18 and get entitled to a 40% increase in minimum wages, their employment drops by 15% 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 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 USA, find that minimum pay policies lead students to leave school to queue up for minimum wage jobs. Neumark and Wascher (1995a, b) also report that the minimum wage policy increases the proportion of teens who are neither in education nor in employment. They argue that employers substitute more skilled youth for less skilled ones, which leads the latter to be out of work and out of school. Pacheco and Cruickshank (2007) find that increases in minimum pay reduce enrollment rates of 16–19-year-olds in New Zealand [4]. 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) document 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) makes use of the rotating panel feature of the Household Labor Force Survey (HLFS) [5] and the variation in the ratio of labor costs to gross wages over time among low-wage workers within a DID framework and concludes 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 contribute to this literature by explicitly focusing on youth minimum wages and a rich set of outcomes that include teens' employment, school enrollment 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 a major 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

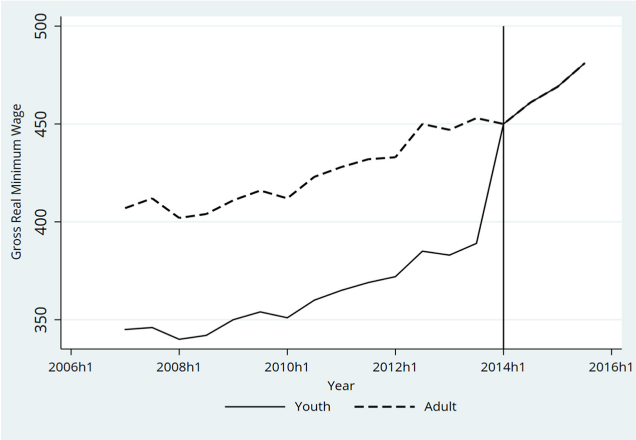
A significant fraction of workers in Turkey earn just the minimum wage. In total, 37.3% of private formal sector workers, i.e., those with social security registration, in 2019 were reported as minimum-wage earners to the Social Security Institution. The tripartite Minimum Wage Determination Commission sets the minimum wage at least every two years since 1951 [6]. 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 was more or less stable with workers younger than 16 (essentially 15-year-olds) receiving nearly 15% 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 public announcement at the end of the year. 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% from December 2013 to January 2014. Taking inflation into account, the real minimum wage for workers under age 16 increased by 14.3% in the first half of 2014, while that for workers of age 16 and above hardly changed.

This policy change potentially increased employers' labor cost [7]. 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% lower than 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 14.3%.

An important concern for our study is whether youth minimum wages are relevant given the high incidence of informality, particularly among teens in Turkey. Of the 15–16-year-old boys who were out of school, 86.3% in 2013 and 86.9% in 2014 held informal sector jobs. In the standard two-sector neoclassical 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 has 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" [8]. 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



Note(s): Nominal monthly wages are deflated by producer price index (2007 = 100)
Source(s): Ministry of Family, Labor, and Social Services

Figure 1. Gross statutory (real) minimum wages, by age (from the first half of 2007 (h1) to the end of the second half of 2015)

wages by comparing age-specific distributions. Since monthly wage data is not available in SILC, we turn to HLFS data set [9]. In particular, we examine the teen wage distribution using the Kernel estimator and look for spikes at 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) and Rani *et al.* (2013)) [10].

In Figures 2 and 3, we plot Kernel density estimates of the log real wage distributions for 15- and 16-year-old boys in 2013 and 2014, respectively. The vertical lines in these figures correspond to log real minimum wages in each year [11]. Note again that due to the substantial improvement in real minimum wage for 15-year-olds following the policy change, the youth minimum wage is replaced by the adult minimum wage in 2014. Because there was no improvement in the adult (real) minimum wage from 2013 to 2014, there is only one vertical line for 16-year-olds in Figure 3. A visual inspection of these figures suggests that young workers are concentrated at or around the real minimum wage in both years. An improvement is observed in the wage distribution of 15- and 16-year-olds following the policy change as implied by the rightward shift in the Kernel density functions and the higher concentration of youth around the minimum wage in 2014 [12]. No other spikes, other than the ones around the minimum wage, are observed suggesting that the minimum wage is relevant for 15-year-old and 16-year-old boys. It is also interesting to note that the mode of the distribution for 15-year-olds after the policy change remains closer to the old lower minimum wage as opposed to the new higher minimum wage suggesting the reluctance on the part of the employers in paying the higher minimum wage to 15-year-olds. Nonetheless, their wages go up.

4. Data and empirical approach

4.1 Data description and summary statistics

We employ the 2012–2015 and 2014–2017 panel waves of SILC of Turkey in our empirical analysis. SILC is a micro-level longitudinal household survey, which has been annually

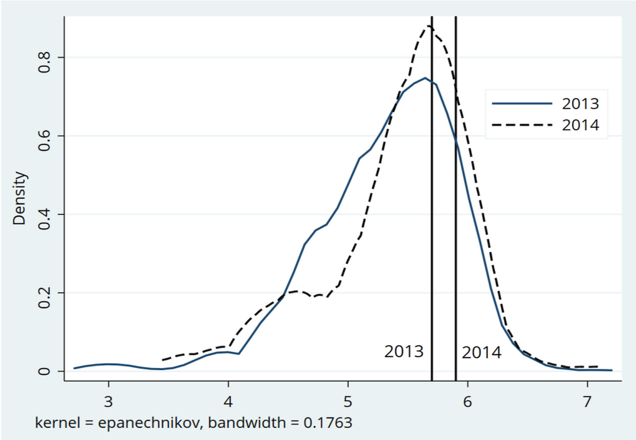


Figure 2. Kernel density estimates of log real monthly wages, 15-year-old boys

Note(s): Includes boys in employment only. Sampling weights are used. Vertical lines refer to log (real) minimum wages

Source(s): Own calculations using the 2013-2014 HLFS

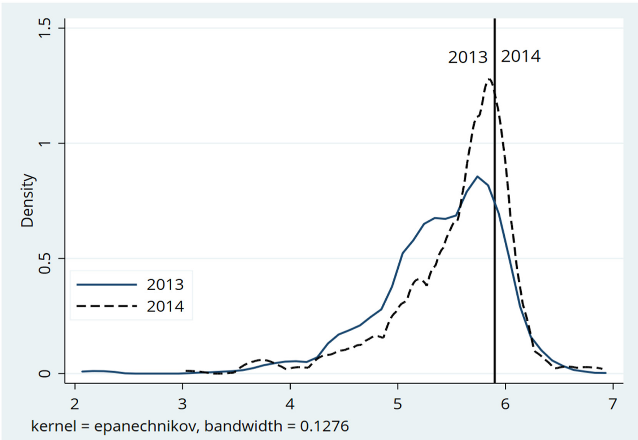


Figure 3. Kernel density estimates of log real monthly wages, 16-year-old boys

Note(s): Workers do not attend school while working. Sampling weights are used. Vertical lines refer to the log of average minimum wage in a year in real terms

Source(s): Own calculations using the 2013-2014 HLFS

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. The main activity of a respondent in each month is inquired via a separate question. Respondents can only report one activity for any given month and are instructed to report the one that has taken the most amount of their time in that month [13]. We make use of this monthly data in tracking the changes in education and employment status of youth over a 12-month period each year. Essentially, we transform each observation into 12 data points, since we have 12-month information (from January to December of each year) for each individual.

A caveat with monthly data is that it does not provide detailed information on labor market activities such as hours of work, occupation held, or wages earned. Neither do we observe whether the respondent is employed formally or informally. Based on the self-reported main activity status of individuals, we generate three outcome variables in the form of dummies showing whether the individual is employed, in education (or in training), or in NEET. 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 soon as youths 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. In particular, in the RDD model where we consider how the employment status of youth changes as they turn 16, we concentrate on monthly data covering 2013 (using the 2014 wave). In the difference-in-discontinuities model, where we exploit the policy change in 2014, we add in monthly data from 2014 (using 2015 wave). We restrict our analysis to 15–16-year-old males [14, 15]. This corresponds to a 24-month bandwidth around the cutoff value of 16 years (and 0 months). We cannot include individuals younger than 15 since employment related questions are asked to persons aged 15 and above. To keep the comparison groups as similar as possible, we exclude boys older than 16 either [16]. 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 present several characteristics of 15-year-old and 16-year-old boys 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 quite similar. Both groups of teens have, on average, completed 7.6 years of schooling, which

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
HH'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
NEET	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 Mon. 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		
# of individuals	1,118	1,134	1,079	1,121		

Note(s): Standard deviations are given in parentheses for continuous variables. Information on the main activity status (in education, employed and NEET) is collected on a monthly basis whereas information on individual and household characteristics is collected annually at the time of the survey. §: Includes employed teens in the reference week in the month the survey was conducted. HH: Household head

Table 1. Summary statistics for 15- and 16-year-olds boys, by year

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, around 6 years of schooling.

In terms of the outcomes of interest, in 2013, 78% of 15-year-olds were in school, as compared to 70% of 16-year-olds. The proportion in employment was higher among the older group, so were the proportion in NEET. Going from 2013 to 2014, the proportion of 15 year-olds enrolled in school increased, while those in employment and NEET fell.

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 to 2014.

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

Let $D_i = D(z_i) = 1(z_i \geq z_0)$, where the rating variable z is age and z_0 is the age cutoff (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 RDD strategy enables us to evaluate the policy effect by comparing average outcomes of the individuals who are just below and just above the age threshold.

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

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

where β_i is the treatment effect of interest. 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 RDD inference can still be feasible even with a discrete rating variable. 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 (see, e.g., Calonico *et al.* (2014)). We follow both approaches in the estimation of our models. In the case of a discrete rating variable, we can identify $E[\beta_i|z = z_0]$ by Equation (2):

$$y_i = \alpha + \beta D_i + u_i, \tag{2}$$

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 [17], 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 $\mathbb{E}[a_i|z = z_i] = 0$. Lee and Card (2008) point out that orthogonality of a_i and z_i might not always be easy to satisfy. However, the classical approach requires no specification error, which is a condition that is more restrictive. Since the specification bias is viewed as a random error, there exists 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 on both sides of the cutoff, the model we estimate takes the form of:

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

where y_i is a binary outcome variable— employment, school enrollment and NEET— D_i is the treatment dummy taking the value of 1 for individuals 16 and older and 0 otherwise and $z_i - z_0$ is age in months relative to the 16th birthday. Following Gelman and Imbens (2019), 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 control variables are not used because monthly data track employment and schooling outcomes only, and as argued by Angrist and Pischke (2008), they are not necessary to identify unbiased or consistent estimates in the RDD framework.

Our second identification strategy relies on the abolition of youth minimum wage in 2014. Within the spirit of DID, we compare the discontinuity before and after the policy change. Borrowing from Grembi *et al.* (2016), we call this second strategy as the difference-in-discontinuities (or diff-in-disc) approach.

Within the diff-in-disc framework, y_i can take four values: y_{1i}-post ($D_i = 1$ and Post $= 1$), y_{0i}-post ($D_i = 0$ and Post $= 1$), y_{1i}-pre ($D_i = 1$ and Post $= 0$), or y_{0i}-pre ($D_i = 0$ and Post $= 0$). D is a dummy variable taking the value of 1 for individuals under 16, and 0 otherwise; while the dummy variable 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 $\widehat{\tau}_{DD}$ (see Equation (5) below) is the diff-in-disc estimator for the treatment effect:

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

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

$$y_{it} = \gamma + \beta_1 D_i + \beta_2(z_i - z_0) + \beta_3 D_i \cdot (z_i - z_0) + \alpha_1 \text{Post}_t + \alpha_2 D_i \cdot \text{Post}_t + \eta_{it}, \tag{6}$$

where the coefficient of interest in this specification is α_2. As in Equation (4), we allow for different trends on both sides of the cutoff.

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 cutoff value. In Figure 4, Panels A through C, we plot the mean values for each outcome variable (i.e., employment, school enrollment and 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 cutoff value, extending 12 months in either direction. We also plot linear trends, which we allow to differ on either side of the cutoff, along with 95% 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 cutoff. The magnitude of the decline is about 4% 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 school enrollment (Panel B), which falls with age, we observe a jump in the trend line showing an increase in enrollment, though the persistent decline resumes beyond the cutoff, and the jump itself is not statistically significant at 5% level (with p-value = 0.12). Finally, we consider the change in NEET (Panel C), which also registers a sharp increase in the order of 3% points that is statistically significant (p-value = 0.012). Hence, the visual analysis suggests worsening employment outcomes and an increase in the proportion in NEET as 15-year-olds turn 16.

5. Results

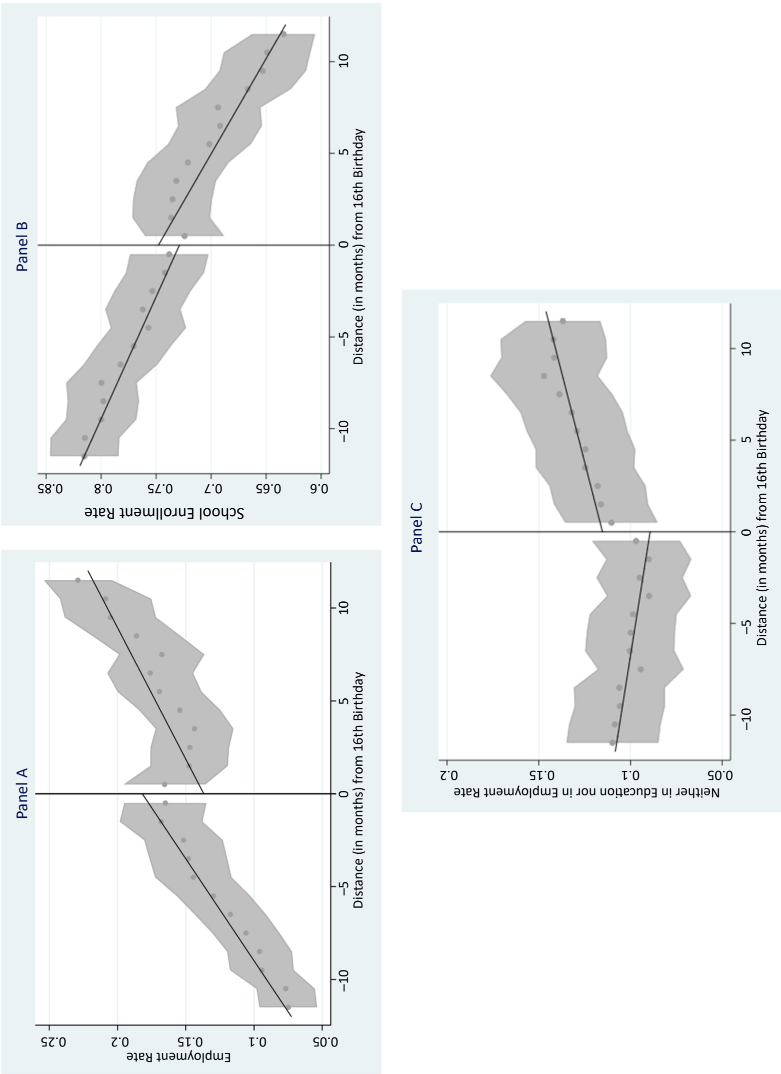
We first present the results of the RDD model followed by the diff-in-disc model. For the RDD model, the estimations include 14,256 observations of 15–16-year-old males in 2013. The diff-in-disc model includes observations from 2014 as well and, therefore, the number of observations increases to 28,823.

5.1 Short-term effects

5.1.1 Results of the RDD model. We present the estimation results of (non-parametric) local linear regressions, where we employ 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 Table 2. We report cluster robust point estimates in the Table.

The results for employment, given in Table 2 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 in the order of 2.5–3.1% points, which is quite sizable given that the average employment rate of 15-year-olds in 2013 was 12%. The implied elasticity of employment with respect to the change in minimum wage is between −1.47 and −1.81. Kreiner *et al.* (2020) find that as Danish workers turn 18, their employment decreases by a third due to the age-specific minimum wage policy, which yields an elasticity of minimum wage around −0.8. Pereira (2003) finds that employment-minimum wage elasticity for 18–19-year-olds lies between −0.2 and −0.4. The elasticity estimates of Yannelis (2014) are also between −0.28 and −0.46 for workers under 25 [18].

The policy-induced employment loss may originate from quits or lay-offs. 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 or 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 the minimum wage is



Note(s): Age in months is centered at 16 years. Points to the left and right of the cutoff represents the distance to age 16 in months

Source(s): Own calculations using the 2012-2015 rounds of SILC

Figure 4. Employment, schooling and NEET outcomes of young males in 2013

Table 2.
RDD estimation

Estimation results for RDD model (2013)	Employed (1)	In education (2)	NEET (3)
Local linear regression			
Estimated coefficient	−0.031*** (0.01)	0.014* (0.008)	0.018*** (0.004)
MSE optimal bandwidth	12.67	12.45	19.98
# of observations	7,179 (left)	7,179 (left)	7,179 (left)
	7,432 (right)	7,432 (right)	11,261 (right)
Estimated coefficient	−0.025** (0.011)	0.010 (0.009)	0.018*** (0.004)
CER optimal bandwidth	8.98	8.82	14.15
# of observations	4,780 (left)	4,780 (left)	7,179 (left)
	5,201 (right)	5,201 (right)	8,523 (right)

Note(s): ***, ** 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 and month of birth dummies are used

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 would reflect this unmet expectation. We can, however, rule out any labor supply adjustment due to an income effect stemming from household income, since we are solely looking at youth and not adult minimum wages.

The short-term effect of a higher minimum-wage on school enrollment is positive but rather small (Column 2, Table 2) and, only the estimate with MSE optimal bandwidth produces statistically significant result. This result implies that boys who lost their jobs do not immediately transition to school. The most optimistic estimate suggests that less than half of those who lost their jobs went back to school in the short term. This can be driven by either mechanical reasons, i.e., timing of job loss not coinciding with the start of the academic year, or choice [19].

The short-term 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 (Column 3, Table 2). 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.

5.1.2 Results of the diff-in-disc model. We now turn to the results of the diff-in-disc model. We carry out parametric estimations using logistic and ordinary least squares (OLS) regressions, whose results are presented in Panels A and B of Table 3, respectively [20]. We choose a 12-month bandwidth in both estimations to be consistent with the optimal bandwidth used in the RDD model. The results obtained from the diff-in-disc model are very similar to what is obtained from the RDD estimates.

The results for employment (Table 3, Column 1) suggest that the probability of employment for 15-year-old boys declines in the short-term with the abolishment of the youth minimum wage in 2014. The estimated effect sizes are 3.2–3.6% points, which are only slightly higher than what is estimated using the RDD model [21].

When we turn to the effect on school enrollment, we find that the probability that 15-year-olds will enroll in school due to the policy change increases by 1–1.7% points. (The estimated effect is not statistically significant at conventional levels in OLS estimation.) For NEET, the probability that 15-year-olds are neither in employment nor in education or training increases with the policy by 2.0–2.1% points. Both effects are statistically significant at 1%. Overall,

Estimation results for diff-in-disc model	Employed (1)	In education (2)	NEET (3)
Panel A: Logistic regression			
Estimated coefficient	−0.036*** (0.006)	0.017** (0.008)	0.020*** (0.006)
Bandwidth	12	12	12
# of observations	28,186	28,186	28,186
Panel B: OLS			
Estimated coefficient	−0.032*** (0.008)	0.010 (0.008)	0.021*** (0.006)
Bandwidth	12	12	12
# of observations	28,186	28,186	28,186

Note(s): ***, ** 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 and month of birth dummies are used

Table 3.
Diff-in-disc estimation

similar to the case of the RDD model, the results of the diff-in-disc model suggest that the main adjustment to the policy occurs at employment and NEET margins in the short term.

5.2 Medium-term effects

As we discuss above, the education effect we report is likely to be a short-run response. Following the abolishment of the youth-minimum wage, 15-year-olds who lose their jobs may decide to go back to school. However, doing so may be difficult given that they were out of school for at least half of the school year. To see the longer-run effects of the minimum-wage policy, we extend our time window to cover 2 whole school years following the policy change. We re-estimate the diff-in-disc model using 2014–2017 rounds of SILC, which correspond to outcomes for 2013–2016. The results are reported in Table 4.

Medium-term results for employment (Column 1) suggest that the probability of employment for 15-year-old boys due to the policy change declines by 2.3–2.8% points. When compared to short-run effects, the coefficient on employment is quantitatively very similar. The estimated effect size for education (Column 2) increases to 3% points and becomes statistically significant, suggesting a delayed school response in medium term. The estimated coefficient for NEET falls and loses its significance in the medium term (Column 3). These results suggest that the education response is much higher in longer term and the policy effect on NEET is short-lived [22].

Estimation results for diff-in-disc model (2013–2016)	Employed (1)	In education (2)	NEET (3)
Panel A: Logistic regression			
Estimated coefficient	−0.028*** (0.005)	0.030*** (0.005)	−0.006 (0.004)
Bandwidth	12	12	12
# of observations	39,050	39,050	39,050
Panel B: OLS			
Estimated coefficient	−0.023*** (0.005)	0.030*** (0.005)	−0.006 (0.004)
Bandwidth	12	12	12
# of observations	39,050	39,050	39,050

Note(s): ***, ** 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 and month of birth dummies are used

Table 4.
Diff-in-disc estimation

5.3 Robustness checks

We carry out a set of robustness checks. First, we test the density of the running variable in our models to provide evidence for non-manipulation. Second, we carry out a set of falsification tests by estimating Equation (4) in the post-policy period and with artificial cutoffs. Third, we estimate Equation (4) by excluding observations closest to the cutoff and with second order polynomials. Fourth, we estimate Equation (4) with parametric regressions. Finally, we re-estimate standard errors to see if cluster size matters for the significance levels of our estimates.

5.3.1 Density test for the running variable. Econometric identification of the policy effect within RDD requires that no other policy coincides with the age-specific minimum wage policy. There are no schooling or labor market policies other than the minimum wage policy that apply to 15-year-olds as they turn 16 [23].

We also analyze the density of our running variable, which is age, by using local polynomial density estimator developed by Cattaneo *et al.* (2019). The estimated difference in the density of age at the 16-year-old cutoff is 0.0001, and the p-value associated with the test of the null hypothesis that this difference is zero is 0.4942. This implies that the density of the age variable does not change abruptly at the 16-year-old cutoff. This is what we would expect since it is rather difficult, if not impossible, to change one's age to qualify for a higher minimum wage, which is the treatment in our design [24].

5.3.2 Placebo tests. In the placebo tests, 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. We carry out two separate placebo tests. The first concerns setting artificial cutoff values and checking the existence of a treatment effect on our outcome variables in an RDD setting using different age groups. The second falsification exercise applies RDD model to the same age groups using the same age cutoff, as our original model, but in the post-policy period. In both cases, there should be no treatment effect at these cutoffs since the treatment status does not change at the chosen cutoffs, i.e., subjects are all treated (Cattaneo *et al.*, 2020) [25].

In the first falsification test, we estimate Equation (4) using 2013 data for different age groups via local linear regressions. Parallel to our main model, we consider pairs of three separate age groups, 16–17, 18–19 and 20–21-year-olds, for whom youth minimum wages do not apply. To construct a group that is similar to our target group of 15–16-years-olds, we consider youth with at most secondary education by excluding university graduates, who comprise only 1% of males under age 21. Again, similar to our main model, we set the age cutoff for 16–17-year-olds at 17 years (i.e., 17 years, 0 months), 18–19-year-olds at 19 years and 20–21-year-olds at 21 years, where again the running variable is age in terms of months. Table 5 presents the estimation results for our three outcome variables. The results do not lend support to a significant discontinuity at different cutoffs. Of the 9 coefficients estimated, only one coefficient is statistically different from zero, but its magnitude is close to zero.

In the second falsification exercise, we estimate Equation (4) for 15- and 16-year-olds 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. The results presented in Table 6 show no policy effect: the estimated coefficients are close to zero and are not statistically significant at conventional levels.

5.3.3 Donut-hole. We re-estimate our RDD model by excluding the observations close to the age cutoff to remove any distortion due to potential approximation errors. Specifically, we estimate Equation (4) with a sample that excludes the observations one month before and after the discontinuity. The results presented in Table 7 show that our estimates are robust to the exclusion of observations adjacent to the age cutoff.

Estimation results for RDD model with alternative cutoffs (2013)

	Employed (1)	In education (2)	NEET (3)
Local linear regression			
16–17-year-olds	0.007 (0.007)	−0.0003 (0.007)	−0.007*** (0.002)
# of observations	6,228 (left)	6,228 (left)	6,228 (left)
	5,180 (right)	5,180 (right)	5,180 (right)
18–19-year-olds	0.007 (0.012)	0.008 (0.009)	−0.016 (0.012)
# Of observations	2,801 (left)	2,801 (left)	2,801 (left)
	1,509 (right)	1,509 (right)	1,509 (right)
20–21-year-olds	−0.0004 (0.022)	−0.010 (0.020)	0.010 (0.013)
# of observations	832 (left)	832 (left)	832 (left)
	1,240 (right)	1,240 (right)	1,240 (right)

Note(s): ***, ** 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 and month of birth dummies used. Effective number of observations is reported

Table 5. RDD estimation with alternative cutoffs

Estimation results for RDD model (2015–2016)

	Employed (1)	In education (2)	NEET (3)
Local linear regression			
Estimated coefficient	0.001 (0.003)	0.004 (0.004)	−0.005 (0.003)
MSE optimal bandwidth	17.28	15.97	14.71
# of observations	12,913 (left)	12,913 (left)	12,913 (left)
	19,198 (right)	17,228 (right)	16,219 (right)
Estimated coefficient	0.0004 (0.003)	0.006 (0.004)	−0.005 (0.003)
CER optimal bandwidth	12.2	11.28	10.39
# of observations	12,913 (left)	11,842 (left)	10,776 (left)
	14,146 (right)	13,100 (right)	12,024 (right)

Note(s): ***, ** 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, month of birth dummies and a year dummy for 2016 are used. Effective number of observations is reported

Table 6. RDD estimation (placebo)

Estimation results for RDD model with restricted sample (2013)

	Employed (1)	In education (2)	NEET (3)
Local linear regression			
Estimated coefficient	−0.039*** (0.01)	0.018* (0.01)	0.027*** (0.005)
MSE optimal bandwidth	11.20	12.61	15.97
# of observations	5,990 (left)	6,591 (left)	6,591 (left)
	6,288 (right)	6,829 (right)	8,464 (right)
Estimated coefficient	−0.031** (0.012)	0.014 (0.011)	0.023*** (0.005)
CER optimal bandwidth	7.93	8.93	11.31
# of observations	3,588 (left)	4,192 (left)	5,990 (left)
	4,034 (right)	4,598 (right)	6,288 (right)

Note(s): ***, ** 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 and month of birth dummies are used

Table 7. RDD estimation (restricted sample)

5.3.4 Second-order polynomials. In our main model, we use local linear regressions mainly because Figure 4 suggests linearity of outcomes. As a robustness check, we re-estimate Equation (4) by assuming a second order polynomial relationship between age and the outcome variables. Our time window does not allow for the use of higher (i.e., third or fourth) order polynomials (Gelman and Imbens, 2019).

The results given in Table 8 are broadly in line with our main findings that the increase in the minimum wage reduces the employment probability of boys and increases their NEET probability in the short-term. The effect sizes are somewhat smaller when we use second order polynomials. As noted earlier, the use of higher order polynomials with a short time window runs the risk of overfitting the relationship between age and the outcome variables. As shown in Table 8, the optimal bandwidths in specifications with quadratic polynomials are larger than those in local linear regressions. Given that we do not have information on boys younger than 15, our time window cannot be extended to the left of the cutoff beyond 12 months to accommodate the wider optimal bandwidth, which may explain the smaller effect sizes [26].

5.3.5 Parametric estimations. Our RDD estimates presented in Table 2 are based on local linear regressions that do not impose any functional form on data. We, nonetheless, estimate Equation (4) with logit and OLS for the sake of completeness.

Parametric estimates for all outcomes, given in Columns 1 through 3 in Table 9, are qualitatively equivalent to our earlier short-term estimates. However, as compared to the non-

Table 8. RDD estimation (quadratic)

Estimation results for RDD model with quadratic polynomials (2013)

	Employed (1)	In education (2)	NEET (3)
Local linear regression			
Estimated coefficient	−0.013** (0.007)	0.002 (0.007)	0.012*** (0.004)
MSE optimal bandwidth	19.3	21.62	22.41
# of observations	7,179 (left)	7,179 (left)	7,179 (left)
	11,261 (right)	12,261 (right)	12,751 (right)
Estimated coefficient	−0.010 (0.008)	0.020 (0.007)	0.008** (0.004)
CER optimal bandwidth	13.01	14.58	15.11
# of observations	7,179 (left)	7,179 (left)	7,179 (left)
	7,972 (right)	8,523 (right)	9,067 (right)

Note(s): ***, ** 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 and month of birth dummies are used

Table 9. RDD estimation (parametric)

Parametric estimation results for RDD model (2013)

	Employed (1)	In education (2)	NEET (3)
Panel A: Logistic regression			
Estimated coefficient	−0.045*** (0.009)	0.022*** (0.007)	0.025*** (0.004)
Bandwidth	12	12	12
# of observations	14,070	14,070	14,070
Panel B: OLS			
Estimated coefficient	−0.043*** (0.009)	0.020** (0.008)	0.023*** (0.003)
Bandwidth	12	12	12
# of observations	14,070	14,070	14,070

Note(s): ***, ** 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 and month of birth dummies used. Marginal effects in logit estimates correspond to discrete change in probability parametric estimates given in Table 2 they are slightly higher. The probability of employment, given in Column 1, declines in the short term by 4.3–4.5% points as boys turn 16 and get entitled to receive a higher minimum wage. Their probability of school enrollment, on the other hand, increases with the policy by 2–2.2% points. The NEET probability also increases with the age-specific minimum wage policy, the effect size being on the order of 2.3–2.5% points. These estimates are more or less in line with the baseline short-term results reported in Table 2.

5.3.6 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 unobserved characteristics. This is the main reason why in all our estimations we cluster for birth-month in a given year. Moreover, the convention in the literature is to perform the clustering at the level that the treatment is provided. In our case, the treatment is provided at birth-month level, which also justifies our choice of clustering. That said, a potential problem with this choice is the use of too few clusters. Asymptotic approximations for clustered standard errors require larger number 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 [27].

6. Concluding remarks

In this paper, we study the causal impact of a youth minimum wage policy on employment 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% of the adult minimum wage. This cost advantage was lost abruptly in January 2014 with the uniform application of adult minimum wages irrespective of age. Using an RDD strategy, which relies on the increase in minimum wage as 15-year-olds turn 16 before the policy change, and a diff-in-disc design, which uses the abolishment of youth minimum wage in 2014 for identification, and micro-level longitudinal data sets that cover periods before and after the policy change, we examine whether employment, education and NEET outcomes of 15-year-old boys are affected due to the policy change.

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 is also the case that the employment probability of 15-year-old boys declines 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 [28].

The disemployment effect of the minimum wage policy on youth is in line with the findings of previous studies. Yet, our estimates imply larger employment elasticities possibly because we consider two highly substitutable age groups. We do not observe an appreciable increase in boys' school enrollment immediately following the abolishment of the youth minimum wage policy. The effect becomes larger and statistically significant in the longer-run. The policy change came in the middle of the school year, which must have delayed the return of some youth back to school. Our estimates on NEET show that boys become more likely to be in NEET in the short-term, while this NEET effect vanishes over time.

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. Stepwise structure of youth minimum wages, as used in some OECD countries, may offer a better alternative.

Notes

1. The sample we use in our empirical analysis covers teenagers aged 15–16 years. They constitute 1.2 million (nearly a fifth) of 15–24-year-old young males in Turkey.
2. See, for example, Katz and Krueger (1992), Fields (1994), Dickens *et al.* (1994), Neumark and Wascher (1995a, b) and Neumark and Wascher (2003).
3. Among others see Van Soest (1994), Benhayoun (1994), Allegretto *et al.* (2011), Sen *et al.* (2011), Gorry (2013), Neumark *et al.* (2014) and Liu *et al.* (2016).
4. See also Pacheco (2009).
5. 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.
6. The Labor Act #4857 and the Minimum Wage Regulation constitute the legal premise of minimum wages.
7. The average tax wedge for a single worker in Turkey is about 39 percent. It has fluctuated between 37 and 39 percent in the last decade with only 0.7 percentage point increase in 2014, the year 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.
8. 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).
9. In HLFS, date of birth information is not available. We only observe respondents' age in years.
10. 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 in this setting.
11. Since the minimum wage is set biannually in Turkey, we take the averages of minimum wages for each group in each year for a clearer exposition.
12. We also examine the cumulative density distribution estimates for 15- and 16-year-old boys in 2013 and 2014, which are given in the Appendix as Figures A1 and A2. For both age groups, we observe substantial improvements in wages in 2014. But there is no visible change in the preceding years—see Figure A4.
13. We cannot analyze the joint-time use of teens because of the way the relevant information is collected. However, work and schooling are incompatible activities in Turkey because average work hours are very long and part-time work is not common. According to HLFS, the proportion of 15–16-year-old boys who combine work and school is 9.6 percent. Among them 32.7% work part-time implying that only about 3% of 15–16-year-old boys work part-time while attending school. These observations suggest that not observing the joint-time use of youth is unlikely to affect our estimates significantly.
14. This corresponds to young males who are aged between 15 years and 0 month-old to 16 years-11 months old.
15. We also replicate our analysis for girls, but we do not find any significant effects of the age-specific minimum wage policy on the three outcome variables. This may have to do with social and cultural factors playing a bigger role in girls' than boys' employment decisions. However, it must also be noted that the optimal bandwidths for the three outcome variables estimated for girls mostly fall outside of our observation windows suggesting data constraints.

16. Kreiner *et al.* (2020) point out similar threats to the RDD 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.
17. Higher order polynomials are also possible. The idea, however, remains the same.
18. Our estimates for the implied elasticity of employment with respect to the change in minimum wage is between −1.47—[−1.47 = (0.025/0.12)/(14.3–0)*100)— and −1.81—[−1.81 = (0.031/0.12)/(14.3–0)*100)— which are higher than the estimates reported in the literature. The magnitude of the elasticity is high since it includes both substitution and scale effects. In order to distinguish these effects, we estimate the diff-in-disc model with an older control group, 17-year-old boys. We do not include boys older than 17 in this group due to the availability of employment subsidies for 18- to 24-year-olds. Employment subsidies are commonly used by employers and may contaminate our results. We also exclude secondary school graduates as they are unlikely to be substitutable with 15-year-olds. The logistic regression estimate for employment is −0.016 (with p-value = 0.02), which is lower than the original diff-in-disc estimate presented in the text. The magnitude of the employment elasticity reduces to −0.93.
19. Within the RDD context, we also try to see how school dropout behavior is affected. We do this by taking a sub-sample of 15-year-olds who are enrolled in school in September 2013 and we follow their dropout behavior until April 2014. The cutoff date is January 2014. The results, which are not reported but available upon request, suggest that the policy of increasing the minimum wage for the youth increases school dropouts immediately after the change in January.
20. Logistic regression estimate of α_2 in Equation (6) is the discrete change in probability, which is calculated as the difference in the predicted probability of the outcome when the interaction term ($T \cdot$Post) is zero and when it is one.
21. The presence of adjustment costs may prevent employers from adjusting their workforce immediately following a policy change. However, a significant majority of 15–16-year-olds are likely employed informally and/or in jobs with low skill requirements. Therefore, neither hiring nor firing are likely to involve much cost. In such a setting, observing immediate employment effects following a policy change is not hard to justify.
22. We thank one of our referees for pointing out to us the possibility of slower adjustment in education over time.
23. In Figure A3 presented in the Appendix, we show on the basis of a selected set of macroeconomic indicators (GDP per capita, GDP per capita growth rate, proportion of early school leavers and infant mortality) that 2013 was not a special year that could possibly affect the employment rate of teens.
24. Another test for selective sorting around the cutoff is a test on covariate balance. As mentioned earlier, we do not use any covariates in our estimations because predetermined variables are not available on a monthly basis.
25. We are not able to carry out placebo tests where subjects are all non-treated for the reason that SILC collects data on ages 15 plus and there is no period during which youth minimum wages are not implemented.
26. We also estimate the RDD model using a donut specification with quadratic polynomials because attenuation bias might also be a reason why the effects corresponding to the model with quadratic time trends are lower than the effects corresponding to the linear model. However, the use of quadratic polynomials under the donut specification still results in smaller effect sizes when compared to the linear model.
27. When we alternatively cluster at the individual level, standard errors are somewhat more conservative than birth-month clustering. However, the qualitative nature of the findings does not change.
28. Another potential adjustment margin is hours of work. Due to data limitations, we were not able to examine how hours of work changes as youth minimum wages change, which we leave for future work.

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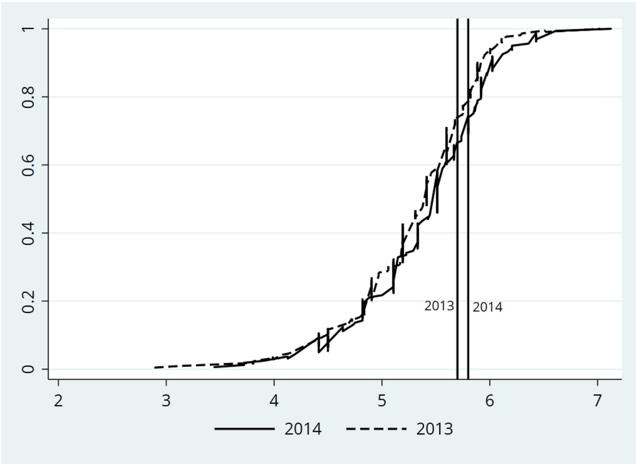
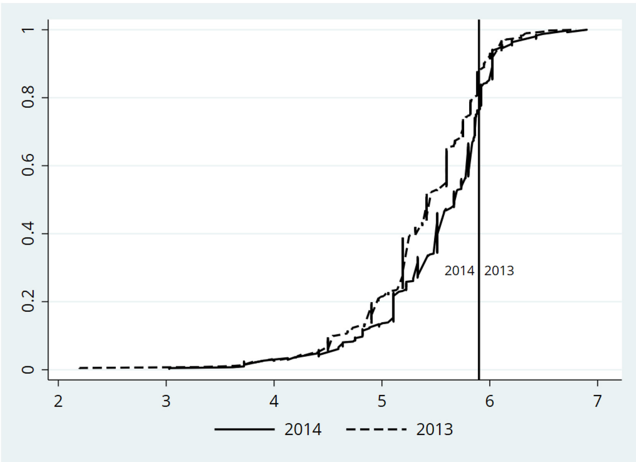
Corresponding author
Semih Tumen can be contacted at: semih.tumen@tedu.edu.tr

Appendix



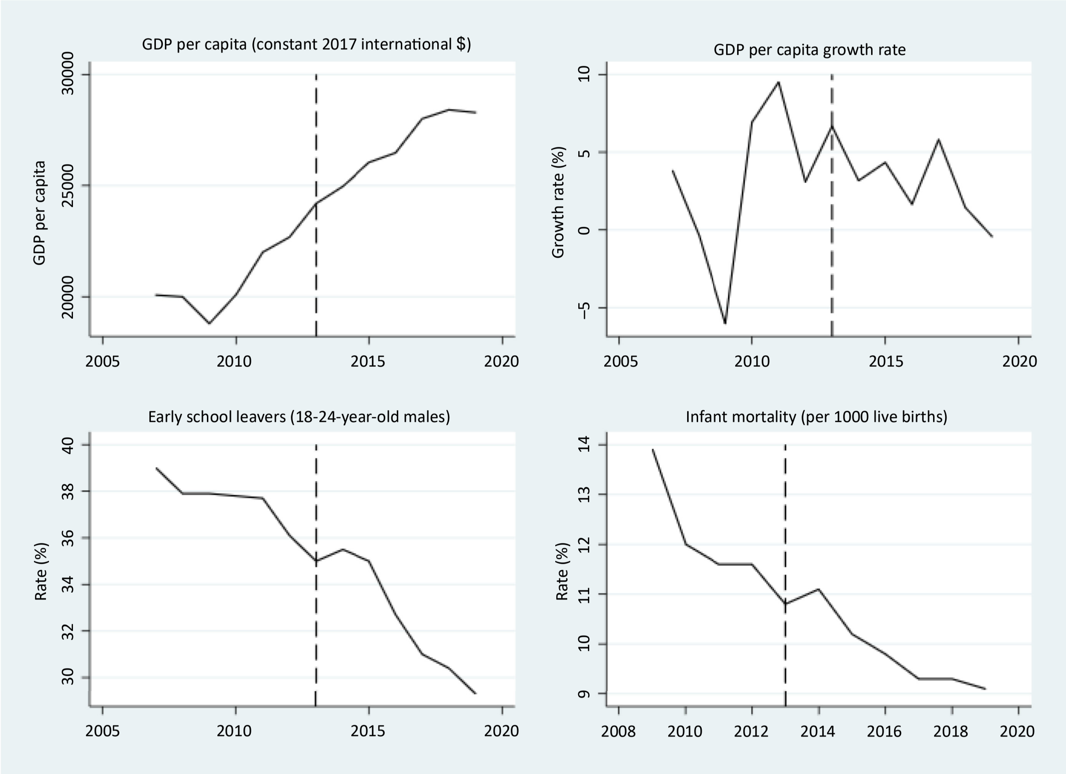
Note(s): Sample includes boys in employment only. Sampling weights are used. Vertical lines refer to log (real) minimum wages

Figure A1. Cumulative density estimates of log real monthly wages, 15-year-old boys



Note(s): Sample includes boys in employment only. Sampling weights are used. Vertical lines refer to log (real) minimum wages

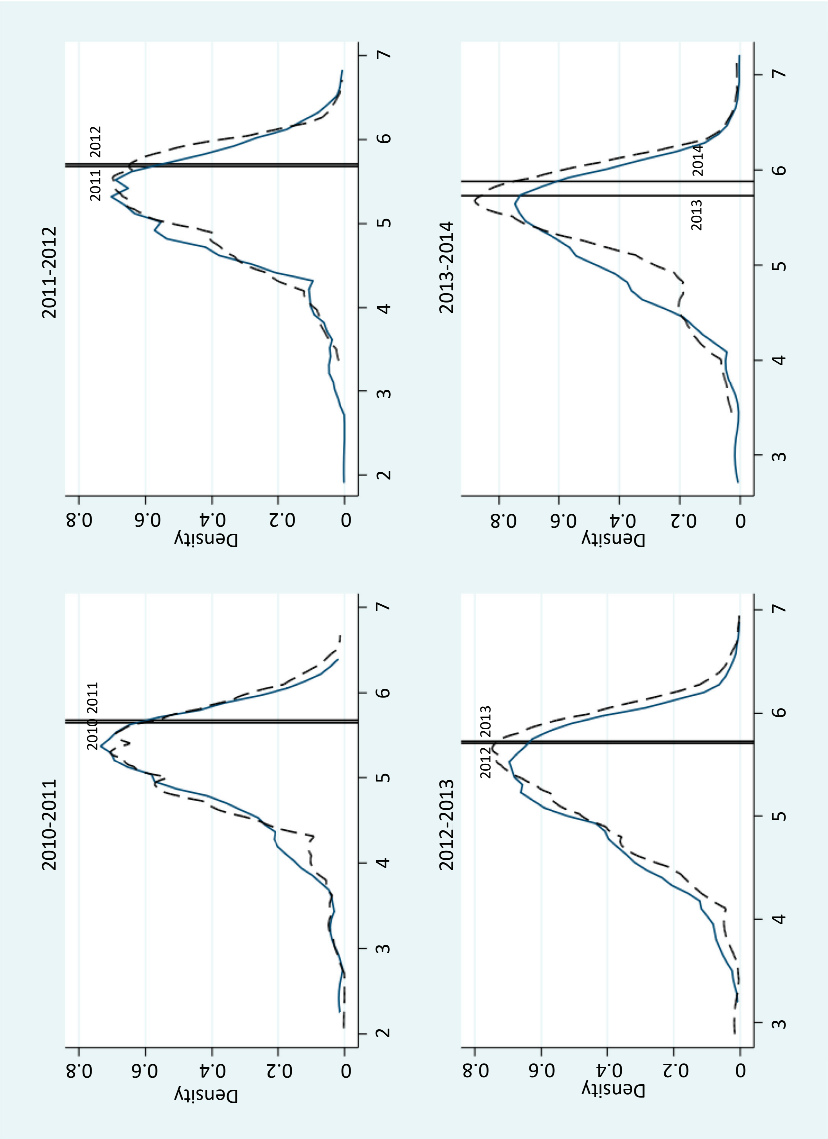
Figure A2. Cumulative density estimates of log real monthly wages, 16-year-old boys



Note(s): Share of early school leavers is defined as the ratio persons aged 18 to 24 who has completed at most lower secondary education and is not involved in further education or training in the total population of the same group. Vertical dashed line shows year 2013

Source(s): World Bank Development Indicators, Eurostat, and Turkish Statistical Institute (Turkstat)

Figure A3.
Selected indicators



Note(s): Sample includes boys in employment only (2010-2014, HLFS). Sampling weights are used. Vertical lines refer to log (real) minimum wages

Figure A4.
Kernel density estimates of log real monthly wages, 15-year-old boys