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ABSTRACT

Out of School and into Trouble? Labor Market Impacts of Decreasing the School Leaving Age

This paper investigates the labor market impacts of a reform that universally lowered the school leaving age from 18 to 16 in Hungary. Using a difference-in-cross-cohort-comparisons approach and linked individual education-employment administrative panel data, we find that the policy led to an increase in the likelihood of dropping out from school and inactivity among individuals aged 16 to 18 but no corresponding increase in employment. Dropouts who were employed predominantly worked in low-skilled occupations. These effects were more pronounced among those from lower socioeconomic status, exacerbating existing inequalities. Our results suggest that the decrease in the school leaving age had adverse effects on school to work transition and did not yield the expected improvements in labor market integration.

JEL Classification: I21, J13, J16, J24

Keywords: education reform, employment, school leaving age

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1. Introduction

Over the past few decades, many developed countries increased the minimum school leaving age (SLA) to improve human capital and economic outcomes (Brunello, Fort and Weber, 2009). While compulsory schooling has been associated with various benefits, it also comes with considerable costs and might not be equally advantageous for all students (Harmon, 2017). Prolonged compulsory schooling could delay entry into the labor market, it might not lead to the acquisition of relevant skills (Pischke and von Wachter, 2008) and, in certain cases, additional years of schooling might not result in earning a secondary school degree (Grenet, 2013). Therefore, if compulsory schooling is less effective for certain groups, reducing the SLA could potentially lead to increased employment prospects for those leaving school earlier. However, as governments rarely opt to decrease the SLA, we have limited knowledge about the consequences of such an education reform.

This paper aims to address the identified gap by examining the impacts of an education reform that decreased the SLA from 18 to 16 in Hungary in 2012. We analyze the effects of this reform on education and labor market outcomes among affected individuals, using linked education-employment administrative data. The reform was introduced in 2012 and the first affected cohort included those who did not enroll in secondary school by September 2011. Those who enrolled in secondary school in September 2011 or before, stayed under the old SLA of 18. Adopting a difference-in-cross-cohort-comparisons research design, we compare the differences in the outcomes of consecutive school cohort pairs before and after the reform to estimate the causal effects of the reform. Relative to a cross-cohort comparison, this method allows us to differentiate between the causal effects of the reform and the time trend of outcome variables that might emerge between consecutive school cohorts irrespective of the reform.

We estimate the effects of the reform separately for ages 15-19 on six outcome variables. Our findings show that decreasing the SLA had a significant negative effect on secondary schooling and labor market outcomes. The point estimates for the increase in the probability of dropping out range from 1.7 percentage points (pp) (120% increase relative to a baseline mean) at age 16 to 5.5 pp (238%) at age 17. However, the average effect on employment is not statistically significant at any age. Estimates for public works participation range between 0.1 pp at age 16 and 0.5 pp (75%) at age 18. The point estimates of the increase in the probability of being a NEET (not in education, employment, or training) range from 1.7 pp at age 16 (112%) to 4.9 pp at age 17 (190%). Point estimates for the increase in the probability of registered unemployment range from 0.3 pp at age 16 to 1.4 pp at age 18 (48%). Point estimates for the increase in inactivity range from 1.3 pp at age 16 (87%) to 3.2 pp at age 17 (150%). Our results suggest that although affected teenagers left school post-reform, they did not find (good) jobs. Consequently, the majority of dropouts found themselves either NEET or inactive.

Further analysis reveals heterogeneous effects of the reform based on parental education, Grade 8 mathematics performance, and pre-reform subregional employment indicators. Relative to the average effect of the reform of 5.5 pp at age 17, the impact on the probability of dropping out is up to three times higher for certain groups—ranging from 14-15 pp for those with low or missing parental background, 11-12 pp for low or missing mathematics performance, and 9 pp for those living in regions with poor pre-reform employment indicators. Comparable results are estimated for the probability of becoming a NEET or inactive. A comparable trend is also seen for unemployment as well, although the differences are less pronounced compared to the other outcomes. Conversely, the effects are close to zero among those with high school or tertiary-educated parents, pointing to a strong intergenerational dependence on past outcomes. Close to zero effects are also seen for upper-middle or highest mathematics or pre-reform subregional employment quintiles.

Finally, the heterogeneity of our results by predicted dropout probability reveal a positive association between predicted dropout risk and the impact of SLA at age 17. In the highest decile of dropout risk, the probability of dropping out increased by 16 pp, 3 times the average effect size. In the same group, probability of becoming a NEET increased by 15 pp, while probability of becoming inactive increase by 10 pp. Probability of becoming unemployed increased by 3.4 pp for the same group. At the same time, we see little or no change in the probability of public works or being employed in this group.

Overall, our results suggest that decreasing the SLA does not promote labor market integration but instead puts early school leavers in a vulnerable position. This outcome is particularly true for students from low socio-economic status (low-SES) backgrounds, indicating that lowering the SLA could lead to increased inequality. While allowing these students to leave school earlier might provide short-term relief for the education system, it could potentially lead to significant negative social consequences in the long run.

Certain aspects of the 2012 reform have been previously studied. Hermann (2020) finds that between ages 17 and 18, the proportion of dropouts increased after the reform but it did not significantly impact the probability of earning a secondary degree. Köllő and Sebők (2020) show descriptively that the number of NEET youth began to rise in 2012-2013 and Köllő and Sebők (2023) find that there was a substantial increase in the number of Roma dropouts between the 2011 and 2016 microcensuses, further exacerbating ethnic inequalities.

We make three main contributions to the literature. First, our study offers a unique evaluation of the labor market impacts of a reform that decreased the SLA for *all* secondary school students, a topic that has been understudied in the existing literature. We identified only two other instances of similar reforms in different countries. According to Büttner and Thomsen (2015), most states in Germany have abolished the final year of academic high schools[1] while keeping their curriculum unchanged, effectively shortening the duration of academic high schools by one grade while maintaining the length of compulsory schooling in other secondary school types. Their study for Saxony-Anhalt region found that this reform resulted in reduced math grades and delayed university enrollment for women. Similarly, Krashinsky (2014) investigated a related reform in Canada, where the fifth year of academic high schools was eliminated, leading to significantly lower academic performance in university subjects compared to pre-reform levels. These reforms were specific to academic high school students, whereas the SLA reforms in most cases disproportionately affect the low-SES and low-ability students who are most likely to drop out and attend vocational education programs (Clark, 2023).

Second, we also build upon and contribute to the literature on the effects of compulsory schooling, highlighting that the negative consequences of reducing the compulsory school-leaving age are most pronounced for those from low-SES backgrounds. Most studies in this field have demonstrated positive earnings outcomes resulting from higher compulsory schooling ages (Angrist and Krueger, 1991; Harmon and Walker, 1995; Oreopoulos, 2006; Stephens Jr. and Yang, 2014), though there are exceptions (Pischke and von Wachter, 2008; Devereux and Hart, 2010; Grenet, 2013; Clark, 2023). Non-pecuniary benefits (Oreopoulos and Salvanes, 2011), reduced mortality and teenage motherhood rates, improved health and health-related behaviors (Lleras-Muney, 2005; Oreopoulos, 2006; Clark and Royer, 2013; Barcellos, Carvalho and Turley, 2018; Fonseca, Michaud and Zheng, 2020; Black, Devereux and Salvanes, 2008; McCrary and Royer, 2011; Cygan-Rehm and Maeder, 2013; Adamecz-Völgyi and Ágota Scharle, 2020; DeCicca and Krashinsky, 2020), as well as lower crime rates (Lochner and Moretti, 2004; Hjalmarsson, Holmlund and Lindquist, 2015; Machin, Marie and Vujić, 2011; Bell, Costa and Machin, 2016) have also been associated with increased schooling. Unlike most of the existing literature, our study examines a rare instance of decreased compulsory schooling, focusing on an Eastern European country in the 2010s rather than earlier historical periods.

Lastly, our research contributes to the broader literature on the returns to education (Card, 1999; Harmon, Oosterbeek and Walker, 2003; Sianesi and Reenen, 2003; Oreopoulos and Salvanes, 2011) and provides policy recommendations for countries contemplating similar reforms in the future. For example, the idea of decreasing compulsory schooling has been

[1] Academic high school is the secondary school that constitutes the traditional route to go to the university.

brought up recently by the Conservative Party in Scotland[2], and from a fiscal point of view, could offer a tempting solution to decrease education spending. Our findings however reveal substantial negative effects of decreasing the SLA age, on both the affected individuals and society as a whole.

The remainder of this paper proceeds as follows. Section 2 provides background information on the reform we study and describes our data. In Section 3, we introduce our empirical approach. We report our results in Section 4. Section 5 concludes.

2. Background and Data

2.1. Background

In Hungary, the education system comprises eight grades of elementary school (Grade 1-8) for students aged 6/7 to 14/15, followed by four grades of secondary school (Grade 9-12) for ages 14/15 to 18/19.[3] It is important to note that grade retention is possible, leading some students to reach the school leaving age (SLA) during primary school, and subsequently, they may not enroll in secondary school. Before the reform, nearly 98% of students completed primary school, and approximately 70% earned a secondary school degree. Secondary education in Hungary offers two primary tracks: the vocational school track and the high school track, with admission being merit-based, resulting in significant sorting of students by ability across these tracks. Around 80% of students opt for the high school track.

This paper studies a reform that shortened the school leaving age from 18 to 16 from September 2012. The government cited three key reasons for introducing the reform (MTI, 2011). First, it was a response to a previous 1996 reform that increased the SLA from 16 to 18. The 1996 reform did not succeed in reducing the probability of dropping out or increasing the probability of students earning a secondary school degree or employment by age 20 (Adamecz, 2023).[4] Due to grade retention, potential dropouts might have been two to four years older than their peers in the same class. Consequently, even if they remained in school until age 18, completing 12 grades and obtaining a degree was not guaranteed. Furthermore, the SLA for 18 years imposed a burden on vocational schools, which were

[2] "Scots Tories propose 'Victorian era' policy to lower school leaving age to 14", The Times, 5 March 2025. https://www.thetimes.com/uk/scotland/article/lower-school-leaving-age-to-14-say-scottish-conservatives-r36z5mnkw?region=global

[3] Some highly selective elite academic secondary schools recruit students already at Grades 5 and 7; however, about 95% of students complete an 8-grade elementary school and start secondary school in Grade 9.

[4] Due to the lack of comparable data for the cohorts between ages 15 and 19, we cannot compare the effects of the two symmetric reforms at this stage.

predominantly attended by potential dropouts from low-SES backgrounds, but these schools lacked adequate financial and human resources to support the academic development of students who had fallen behind. Second, the 2012 reform sought to mitigate presumed negative peer effects by removing the obligation for students to stay in school, which might disrupt the learning experience of their classmates. Third, the government's communication suggested that the reform sought to enable 16- to 18-year-old students who did not wish to continue school to seek employment, thereby increasing the supply of blue-collar workers and supporting their school-to-work transition.

In Hungary, the minimum wage does not depend on age, but various state programs were available in the examined period which targeted youth employment, including direct subsidies and tax refunds for firms that employed young people, low-skilled workers, and those registered as unemployed. Nevertheless, our identification strategy ensures that these do not bias our results.

2.2. Administrative Data

We utilize the Panel of Linked Administrative Data (Admin3), an anonymized dataset that links individual monthly data from various Hungarian administrative sources, including the National Insurance Fund Administration, the Hungarian State Treasury, the Educational Authority, the Ministry of Finance, and the National Tax and Customs Administration.[5] This comprehensive dataset covers a random 50% of the population born before January 1, 2003 (that is, individuals with a Social Security Number in 2003).

Our data contains educational outcomes and employment status from the above-mentioned administrative records. Labor market data are available from 2003 to 2017, while education data are available from 2009 to 2017. We use administrative data on age, gender, employment status, public works program participation, registered unemployment, school enrollment by school type (primary, secondary), and degrees earned until December 2017. The administrative data are further linked to the results of a national mathematics and writing test taken in the spring of grades 6, 8, and 10 called the National Assessment of Basic Competences (NABC) and survey information associated with the NABC on family background, including parental education.[6]

2.3. Sample Composition

For our analysis, we focus on the subsample of individuals who completed primary school between 2009 and 2013, with the majority (93%) being born between 1994 and 1999. Table 1

[5] See Sebők (2019) for more details on the data.

[6] We do not have access to ethnicity in the administrative data used in this paper. These data are only available in the microcensus, used by Köllő and Sebők (2020) in a related context.

provides a summary of the estimation sample, with shaded cells indicating students directly affected by the reform. While the data completely cover cohorts from 2009 to 2012 between ages 15 and 19, some students in the 2013 cohort reached age 19 after 2017, resulting in incomplete data coverage. The missing students are among those who finished primary school at age 14, while the data fully cover those who finished primary school at age 15-18. To avoid bias due to this selection, we exclude from our main analysis these relatively "worse" students who may have repeated grades, enabling us to utilize both age 15 and age 19 outcomes for our difference-in-cross- cohort-comparisons (DiCCC) strategy. In other words, the main estimation sample consists of the 2009-2012 cohorts through ages 15 to 19, and the 2013 cohort through ages 15 to 18. To corroborate our results, we provide several robustness checks using various alternative subsamples in Section 4 (the subsample of the 2009-2013 cohorts at ages 15-18; 2009-2012 cohorts at ages 15-19; 2011-2012 cohorts at age 15-19 and the 2010-2013 cohorts at ages 15-19).

We also exclude individuals born in Hungary between 1991 and 2002 who fall into one of the following three categories, accounting for 3.8% of the sample: (1) those with no school enrollment data (i.e., those who did not attend any school in Hungary between 2009-2017), (2) those who dropped out or moved abroad before 2009, that is, before finishing primary school, and (3) those who completed primary school before 2009. Additionally, we exclude 109 women who had a child by age 14, assuming that they are out of school already and thus would not be affected by the reform. As a result, our main analytical sample comprises 240,677 individuals tracked monthly from ages 15 to 18/19 between 2009 and 2017.

For some students, the NABC data is missing. Missing observations in the NABC are not random, and are correlated with social background and ability. The NABC tests are low-stakes for students but high-stakes for schools; thus, some schools might ask some low-ability students not to participate to increase school-level test scores. We use two variables from the NABC, parental education and Grade 8 math test scores, with approximately 10% of the data missing for both variables (Appendix Tables A1-A5). Note that as these data were collected well before the reform, the reform could not have affected the probability of having missing data. For parental education, the share of missing values is not statistically significantly different among those who completed primary school before versus after the reform. For math test scores, however, the share of missing values is significantly larger in the pre-reform cohorts than in the post-reform cohorts at ages 15 and 19 (but not at ages 16, 17 and 18). Thus, on top of applying missing flags whenever we control for test scores, we provide a robustness check where we do not control for math test scores in Section 4.3. When we examine the heterogeneous effects of the reform with respect to parental education and Grade 8 math test scores, we treat those with missing NABC data as a separate group, keeping in mind that they are likely the most disadvantaged students.

2.4. Outcome Variables

We analyze six educational and labor market outcomes to provide a comprehensive understanding of the reform's impact on the educational attainment and labor market engagement of young people in Hungary.

Dropout. — The first outcome variable we examine is school dropout, defined as a student not being enrolled in school according to the official administrative census and neither returning to school nor obtaining a secondary degree by age 19. The month of dropout is identified as the first month following the individual's last recorded enrollment. However, the data suggests that most students officially drop out at the end of the academic year (typically in July or August), regardless of whether they exit school earlier in the year, for instance, due to childbirth. Schools often maintain administrative enrollment until the end of the academic year, even if the student is no longer attending classes. Therefore, we cannot reliably observe the precise month of dropout within an academic year.[7]

Employment. — Our primary labor market outcome is employment, which is measured using data from the National Tax Authority. This registry records all employment contracts for tax purposes. Employment is treated as a binary variable, taking the value of one if an individual is registered as employed and zero otherwise. We do not differentiate between employment in the private and public sectors, between permanent and fixed-term contracts, and between part-time and full-time employment.

Public works. — We also examine participation in public works schemes, which are primarily low-skilled, subsidized jobs provided by local governments to individuals unable to secure employment in the open labor market. Public works employment is captured by a binary variable indicating whether an individual participates in such a scheme (one) or not (zero). Importantly, we treat public works participation as distinct from formal employment, as individuals in these schemes are not considered employed. Public works participation, particularly for young individuals, is often seen as problematic since it has been shown to impair long-term labor market prospects (Bertrand, Crépon, Marguerie and Premand, 2021; Bhanot et al., 2021).

Unemployment. — Unemployment is another key outcome variable and is defined as registered unemployment. This refers to individuals who are not employed, and registered as unemployed at a local employment office.

[7]Homeschooling is rare but possible. Our data does not distinguish between students who are home-schooled and those attending traditional schools, as home-schooled students remain administratively enrolled in their local schools.

NEET. — A significant labor market outcome for young people is the status of being neither in employment nor in education or training (NEET). [8] We analyze the reform's effect on the likelihood of being NEET, which is a critical measure of disengagement from both education and the labor market.

Inactive. — Our final outcome category, "Inactive," encompasses individuals who do not belong to any of the previously mentioned groups: they are not in school, not employed, not participating in public works, and not registered as unemployed. While these individuals have no administrative records of their activities, they are not recorded as deceased. This group may include those who are inactive, seeking employment but not officially registered as unemployed, working in the informal economy, or temporarily or permanently residing abroad. It is important to note that individuals over 18 are encouraged to register as unemployed to access free healthcare. However, those with no prior employment are ineligible for unemployment benefits, leading many young people, especially dropouts, to remain unregistered and invisible to employment authorities. This invisibility complicates the analysis of labor market outcomes, particularly for school dropouts who lack prior work experience and are not entitled to unemployment benefits. Thus, this is an important outcome to look at.

3. Empirical Framework

This paper examines a reform implemented in September 2012 that reduced the school leaving age from 18 to 16. As mentioned earlier, individuals who enrolled in secondary school in September 2011 or earlier, faced an SLA of 18, while those who enrolled in September 2012 or later faced an SLA of 16 due to the reform. The available data allow us to include five school cohorts in the analysis: 2009, 2010, and 2011 school cohorts that finished primary school before the reform, and 2012 and 2013 school cohorts that finished primary school after the reform. To assess the impacts of the reform, we therefore set up a differences-in-cross-

[8] While our data captures formal education, it excludes informal training programs. However, this is probably not a very important issue in the case of dropouts, as it is unlikely that young people would switch from free public education to expensive private training. To participate in subsidized public training, unemployment registration is needed, and we already capture those in registered unemployment with our previous measure.

cohort-comparisons (DiCCC) empirical strategy.[9]

First, we look at the differences in the outcome variables between consecutive school cohort pairs by age. We estimate the following equations (separately by age between ages 15 and 19):

$$Y_{i,t,c} = \alpha + \beta_{j+1,j} * \mathbb{I}[c = j + 1] + X'_{i,t,c}\gamma + u_{i,t,c}, \tag{1}$$

where j = [2009,2012] and in each estimation we include only cohorts c=j and c=j+1. $\mathbb{I}[c = j + 1]$ is an indicator variable which equals 0 if c=j, the earlier cohort, and 1 if c=j+1, the later cohort. In this equation, i refers to individuals, t refers to calendar months (i.e., the month of observation), and c refers to the school cohort. $\beta_{j+1,j}$ is the coefficient of interest that captures the difference of the outcome variables across the two consecutive school cohorts (j+1 and j); X_i is a vector of individual characteristics (gender, parental education, Grade-8 math test scores, month of observation, month of birth, subregion fixed effects). We employ heteroskedasticity-robust standard errors clustered at the year-and-month-of-birth level.[10]

The difference between the 2012 and 2011 cohorts ($\beta_{2012,2011}$) would represent the causal effect of the reform if no other factors influenced differences in the outcome variables across these two school cohorts. However, if a time trend existed between cohorts, $\beta_{2012,2011}$ would reflect the combined effect of the reform and the time trend. To isolate the reform's impact, we subtract the prior cross-cohort difference (2011 vs. 2010, or $\beta_{2011,2010}$) from $\beta_{2012,2011}$, assuming that a reform that decreased the SLA from 18 to 16 in September 2012 did not affect the cohorts finishing their education before 2012. This approach further assumes that the time trend for the cohorts finishing their education before 2012 can accurately predict what would have happened to the after-reform cohorts had the reform not occurred. Therefore, this adjustment accounts for any time trend unrelated to the reform, forming the basis of our main specification:

$$\text{Main effect} = \beta_{DiCCC} = \beta_{2012,2011} - \beta_{2011,2010}. \tag{2}$$

We construct standard errors for β_{DiCCC} by bootstrapping the original cross-cohort com-

[9]We acknowledge that a regression discontinuity design analysis could be an alternative approach, but due to grade retention and variations in enrollment ages, this is not possible in our setup. Since the reform was implemented for students already in Grade 8, many of whom may have repeated a grade multiple times, there is no longer a clear jump in the probability of being in the treated cohort around the theoretical primary school enrollment cut-off date of birth. Therefore, there is no empirical first stage to set up a regression discontinuity design strategy. Yet another approach would be to use an IV framework, where the endogenous variable would be the number of years spent in school. However, we observe these individuals up to the age of 19, when most of the teenagers are still in school, thus, most observations of this variable would be censored. As a result, these approaches are not feasible in out setup.

[10]While not reported in the paper, estimates using non-clustered standard errors yield very similar results.

parison estimations 50 times, constructing β_{DiCCC} 50 times, and using the empirical distribution of the bootstrapped β_{DiCCC} estimates to estimate the standard error of the mean as the standard deviation of β_{DiCCC}.[11] When bootstrapping, we cluster the observations by year-and-month-of-birth to follow the logic of clustered standard errors in Equation (1).[12]

Our strategy relies on the assumption that, in the absence of the reform, the time trend between school cohorts would remain stable. Further, this approach may face limitations if spillover effects occur, where the reform indirectly influences the cohorts finishing their education before 2012 (due to changing labor market dynamics, such as increased competition or skill reallocation, or changing societal norms in terms of educational expectations, employer preferences or parental expectations), thus violating the Stable Unit Treatment Value Assumption (SUTVA). Although we cannot formally test for this, we conduct a placebo test by calculating the same DiCCC estimates for the school cohorts immediately preceding the reform:

$$\text{Placebo effect} = \beta_{DiCCCplacebo} = \beta_{2011,2010} - \beta_{2010,2009}. \tag{3}$$

Assuming a constant time trend, $\beta_{DiCCCplacebo}$ would be around zero. In our main results, we present β_{DiCCC} alongside $\beta_{DiCCCplacebo}$ for direct comparison. The results of our main analysis is presented in Subsection 4.2.

In addition to the main specification and placebo estimates, we perform several robustness checks. First, we estimate β_{DiCCC} as before but without controlling for any individual characteristics when estimating $\beta_{2012,2011}$ and $\beta_{2011,2010}$ (Rob 1). Second, instead of subtracting $\beta_{2011,2010}$, we subtract the mean of two differences, $\beta_{2011,2010}$ and $\beta_{2010,2009}$ from $\beta_{2012,2011}$, both without control variables (Rob 2) and with control variables (Rob 3). Finally, we subtract the mean of three differences, $\beta_{2011,2010}$, $\beta_{2010.2009}$ and $\beta_{2013,2012}$ from $\beta_{2012,2011}$, again estimating both without control variables (Rob 4) and with control variables (Rob 5). It should be noted that these last two models cannot be estimated for outcomes at age 19. The results of these analyses are presented in detail in Subsection 4.3

To understand how the decrease in the SLA impacted different groups, we conduct two types of heterogeneity analysis. First, re-estimate our main model in Equation (2) by gender, parental education, Grade 8 math test score quintiles, and quintiles of subregional employment rate among males aged 25 to 55 in the years between 2009 and 2011, i.e., before the

[11]A total of $50-200$ replications are generally adequate for estimates of standard errors (Z.Mooney and D.Duval, 1993). As we work with a large sample and estimate FE models, running times are high and increasing the number of rounds would be costly.

[12]Although not shown here, our results are very similar when standard errors are calculated without clustering, though the errors are slightly smaller in this case. Therefore, we adopt a more conservative approach by clustering the standard errors.

reform. Since we estimate the effects of the reform on eight outcome variables for 19 subsamples and five ages, we test all together 1,140 parallel hypotheses. Testing several statistical hypotheses together increases the probability of finding significant effects by chance, known as the problem of multiple inference (Anderson, 2008). Thus, when we investigate the heterogeneous effects of the reform, we correct all hypothesis tests by using the multiple testing procedure of Benjamini and Hochberg (1995).

We also estimate the heterogeneous effects of the reform along the distribution of individuals' baseline expected dropout probability. Following the method of Abadie, Chingos and West (2018), we stratify the sample into deciles of dropout probability, absent of the reform. The method is similar to estimating a probability model to predict dropping out using the individual characteristics of the control group only and then fitting the model to predict the expected dropout probabilities for the treated individuals. However, such endogenous stratification is prone to be biased. Abadie, Chingos and West (2018) suggest a repeated split-sample procedure that has been shown to generate substantially lower bias.[13] In comparison to looking at the heterogeneous effects of the reform along one individual characteristic at a time, this method offers two advantages. First, it pools individual background characteristics together and creates categories of increasing dropout risk. Second, we practically combine statistical matching with difference-in-differences (DiD) since, within each stratum, treated and control individuals are matched based on their predicted probability of dropping out. The methods and results of these two types of heterogeneity analyses are presented in detail in Subsection 4.4.

4. Results

4.1. Descriptive Analysis

We begin our analysis by looking at the outcome variables descriptively. Figure 1 illustrates the number of dropouts in the sample by cohort, separately for women and men. In the pre-reform cohorts, about 1,000 girls and 1,000 boys dropped out of school by age 17. Following the implementation of the reform, the number of female dropouts in the post-reform cohorts doubled to approximately 2,000, while for boys it tripled to nearly 3,000 within our sample. Given that our sample covers a random 50% of the population, these figures suggest that the actual number of dropouts in the entire population was twice as large. This increase in the number of dropouts indicates that the reform might have had a strong negative impact on school attendance by the age of 16 and 17. We now turn to examining

[13] We operationalize this additional robustness check using the `estrat` package in Stata written by Ferwerda (2014).

whether this negative impact on school attendance translated into positive or negative labor market outcomes.

In Figure 2, we plot the average values of the outcome variables by school cohort and age.[14] Several key insights emerge from this figure. First, preceding the reform implementation, there is little change in outcomes for the 2009, 2010, and 2011 cohorts, since the SLA of 18 is binding for these cohorts. This is particularly true for the probability of dropping out of school, participating in public works, unemployment, and NEET. There is some increase in employment at older ages for the 2009, 2010, and 2011 cohorts, most likely reflecting an improvement in economic conditions. Second, there is no difference in outcomes between the cohorts at age 15, when students should not be affected by either the pre-reform higher SLA of 18 or the post-reform lower SLA of 16. Third, for the 2012 and 2013 post-reform cohorts, age 17 students have outcomes that are roughly similar to the outcomes of age 18 students pre-reform, as they are able to leave school already at age 16. For the employment, the pre- and the post-reform cohorts have almost overlapping outcomes at age 17. Fourth, the pre-reform cohorts' outcomes, with the exception of employment, converge at age 19 with the outcomes of post-reform cohorts as both set of cohorts leave school by age 19.

15-year old students are younger than the SLA both before and after the reform, and in line with Figure 1, the first panel of Figure 2 confirms that both before and after the SLA decrease, only about 1% of 15-year-old students leave school. For the pre-reform cohorts that face a SLA of 18, there is little increase in the probability of dropping out at ages 16 and 17. The probability of dropping out increases to around 8% at age 18, and around 16% at age 19. For the post-reform cohorts, the probability of dropping out increases from around 4% at age 16, to around 9% at age 17, to around 15% at age 18, and to 23% at age 19. The probability of dropping out of the pre- and the post-reform cohorts does not close even at age 19, with the statistically significant gap of about 7 percentage points (pp).

The next set of panels in Figure 2 focuses on employment outcomes: public works participation (second panel), employment (third panel), NEET (fourth panel), unemployment (fifth panel), and inactivity (sixth panel). The second panel suggests that public works participation is close to zero in both the pre-reform and post-reform cohorts at age 15. It remains zero in the pre-reform cohorts at ages 16 and 17, but then increases to 0.75% and 2%, for ages 18 and 19, respectively (in the 2011 cohort). In the post-reform cohorts, in line with the pattern of school dropouts, public works participation increases from 1% at age 17, to 2% at age 19 (in the 2012 cohort).

The third panel of Figure 2 suggests that at age 16, a portion of both pre- and post-reform

[14]More detailed descriptive statistics are reported in Appendix Tables A1-A5, separately for ages 15 to 19.

cohorts start finding employment, since it is possible to be employed while in school. For example, in the 2010 pre-reform cohort, employment percentages of 16-, 17-, 18- and 19-year-olds are 1%, 2.5%, 6%, and 15%, respectively. The same percentages for the 2012 post-reform cohort are 2%, 4%, 11%, and 20%, respectively. Although employment rates of the post-reform cohorts in 2012 and 2013 are higher than the pre-reform cohorts, this increase is not larger than the increase between 2009 and 2011, suggesting that overall employment trends explain most of the gap rather than the decrease in the SLA. Further, the observed increase in employment for the post-reform cohorts is not large enough to offset the combined increase in dropping out, documented in the first panel, together with the increase in unemployment and inactivity, displayed in the next three panels. Indeed, as illustrated in the fourth, fifth, and sixth panel of Figure 2, starting at age 16, the gap in NEET, registered unemployed, and inactivity increases between the pre- and post-reform cohorts. By age 18, in the post-reform cohorts, about 15% are NEET (compared to 9% in the pre-reform cohorts of same age, implying a 6 pp increase), 5% are registered unemployed (compared to 3% in the pre-reform cohorts of same age, implying a 2 pp increase), and 10% are inactive (compared to 6% in the pre-reform cohorts of same age, implying a 4 pp increase). For unemployment, this gap closes at age 19, suggesting that it is driven by the decrease in the SLA which led youths with poor labor market prospects to leave school. The gaps for other outcomes do not close at age 19 and remain at 4 pp for NEET and 2 pp for inactivity.

In the following sections, we will probe these descriptive results further using the difference-in-cross-cohort comparisons, as outlined in Section 3.

4.2. Main Results

Motivated by descriptive evidence, presented in Section 4.1, we first estimate simple cross-cohort comparison regressions as outlined in Equation (1). These show how the outcome variables changed between consecutive school cohorts before and after the reform (but do not show the causal effects of the reform yet). These results are presented in Figure 3 (and in more detail in Tables OA3-OA18 in the Online Appendix).

The six panels show the outcome variables of interest: dropout, public works participation, employment, NEET, unemployment, and inactivity. Each dot corresponds to an estimated parameter ($\beta_{j+1,j}$), while the horizontal lines around the dots indicate 95% confidence intervals. Placebo effects (black dots) capture cohort-to-cohort changes in the absence of the reform (cohorts other than 2012 vs 2011). If the pre-reform placebo effects ('Reform-2', 'Reform-1') are close to zero, it suggests that there were no significant trends in outcomes before/after the reform. Post-reform effects (red dots, 'Reform' estimates for cohorts 2012 vs. 2011) represent changes between cohorts potentially affected by the reform. Large deviations between post-reform and placebo effects could indicate a significant impact of the reform on the outcome variables. If the 'Reform+1' (black, 2013 vs 2012) coefficients are insignificant, that would indicate that the reform's impact was immediate and persistent.

The results indicate that in the case of dropout, public works, employment, and unemployment, there were already increasing trends in the pre-reform periods. In the reform year, the estimation results ($\beta_{2012,2011}$) suggest a significant increase in the probability of dropping out, NEET, unemployed and inactivity at ages 17 and 18 compared to the pre-reform changes. For example, for both ages, the change in the probability of dropping out is about 6 pp, in the probability of becoming a NEET is about 5 pp, in the probability of unemployment is about 1.5 pp, while in the probability of being inactive is about 3-4 pp. In most cases, these are much larger changes compared to changes in the years leading up to the reform year. These results suggest that the reform led to dropping out, becoming a NEET, or inactive, rather than having the intended effect of improving the labor market outcomes of the affected cohorts (increasing public works, employment, and reducing unemployment). However, these comparisons alone do not isolate the causal effect of the reform, as other factors may contribute to the estimated cohort differences, as suggested by the significant results of some of the placebo estimates. Therefore, we turn to discuss our preferred difference-in-cross-cohort comparisons (DiCCC) approach, which allows us to provide a causal interpretation of the results.

Our main results for the DiCCC estimates based on Equation (2) and the placebo effects estimates based on Equation (3) are shown in Figure 4. The detailed coefficient estimates are reported in Appendix Tables A6-A11, separately for ages 15 to 19. We find that most of the placebo effects are not significantly different from zero, which suggests that β_{DiCCC} indeed measures the causal effect of the policy change.

Next, we move to the main causal effects, which we will interpret for each outcome in turn. Relative to a baseline mean, the probability of dropping out increased by 1.7 (95% CI: [1.1, 2.2]) pp at age 16 (baseline mean 1.4%), 5.5 [4.3, 6.6] pp at age 17 (baseline mean 2.3%), and 4.3 pp at age 18 (baseline mean 8.6%) in the post-reform period, implying an effect of 120%, 238%, and 50%, respectively (Panel 1 in Figure 4 and Appendix Table A6). This means that the reduction of the SLA impacted a significant number of young people, leading them to drop out of school. The key empirical question is whether they succeeded in the labor market as intended by the logic of the reform.

Relative to a baseline mean, the probability of participating in public works increased by 0.1 [0.1, 0.1] pp at age 16 (baseline mean 0 %), and 0.5 [0.2, 0.8] pp at age 18 (baseline mean 0.7%) in the post-reform period, implying a large effect in percent because beforehand only very few teenagers participated in public works at ages 16 and 17. The effect was 75% at age 18 (Panel 2 in Figure 4 and Appendix Table A7). Although these are large, significant effects, they are not surprising given the very low (almost zero) baseline participation rates prior to the reform. At the same time, the change in employment in the open labor market was economically and statistically insignificant (Panel 3 in Figure 4 and Appendix Table A8).

Relative to a baseline mean, the probability of becoming a NEET increased by 1.7 [1.2, 2.2] pp at age 16 (baseline mean 1.6%), 4.9 [3.7, 6.0] pp at age 17 (baseline mean 2.6%) in the post-reform period, implying an effect of 112% and 190% respectively (Panel 4 in Figure 4 and Appendix Table A9). Similarly, relative to a baseline mean, the probability of becoming unemployed increased by 0.3 [0.3, 0.4] pp at age 16 (baseline mean 0%), and 1.4 [0.7, 2.2] pp at age 18 (baseline mean 3%) in the post-reform period, implying an effect of 467% and 48% at ages 17 and 18 (Panel 5 in Figure 4 and Appendix Table A10). Relative to a baseline mean, the probability of becoming inactive increased by [0.7, 1.8] 1.3 pp at age 16 (baseline mean 1.5%) and 3.2 [2.2, 4.1] pp at age 17 (baseline mean 2.1%) in the post-reform period, implying an effect of 87% and 150%, respectively (Panel 6 in Figure 4 and Appendix Table A11).

For all outcomes, we see a convergence between pre- and post-reform cohorts at age 19, implying that the reform did not have a bite beyond age 18.

Our results suggest that although affected teenagers left school post-reform, they did not find (good) jobs. To corroborate this conclusion further, Appendix Figure A1 shows the outcomes of school dropouts by cohort and suggests that most of them became inactive with only a small share employed. Overall, both before and after the reform, dropouts worked in temporary, low-skilled jobs. Panel (a) of Appendix Table A12 shows that out of the 44,515 students who dropped out, 42% were employed for at least once (at least one month). Panel (b) shows that only 5.7% of person-months were spent in employment. Also, (not shown in the tables) among those who have had at least 1 month of employment, the average employment spell was 3.5 months long, and the median number of months continuously spent in employment was only 2. Panel (b) also shows that those who eventually ended up dropping out from school were already working 2.9% of the time. After dropping out, the employment rate increased to 12%. Panel (c) shows that a large share of employment occurred during the summer which implies temporary employment spells. In line with that, Panel (d) shows that most dropouts were employed in agriculture, manufacturing, trade, or real estate occupations. In more than 50% of the cases, the dropouts were employed in low-skilled jobs, such as cleaners, garbage collectors, or loading workers.

Thus far, our results suggest that while the decrease in the SLA led to a significant increase in the number of students who dropped out of school early, these young people were not in a position to find good jobs. Some of them joined the government's public works scheme, but most of them ended up unemployed or inactive. These average effects of the reform are important, but they might conceal important heterogeneity. In particular, one may expect that marginalized groups, such as lower socio-economic status (SES) students would be more impacted by the decrease in the SLA.

In what follows, in Section 4.3, we will test the robustness of our main results, as outlined in Section 3, and explore the heterogeneous effects by gender, parental education, Grade 8 math test score quintiles, and quintiles of subregional employment rate among males aged 25 to 55 in the years between 2009 and 2011 in Section 4.4.

4.3. Robustness Checks

In this section, we examine the robustness of our main results to various alternative specifications and sample restrictions. Results presented in Figure 5 and Tables OA19-OA24 in the Online Appendix suggest that our findings are broadly robust to alternative specifications and sample definitions. Red dots and lines show our main results (see Figure 4), and black dots and lines show the results of the alternative specifications. The results are consistent in showing that the effects on dropout, public works, NEET, unemployed, and inactivity measured at age 17 are strong and significant across all specifications. The effects observed at age 18 are weaker, and their statistical significance may vary depending on the specification. The employment effects are insignificant at all ages and across all specifications.

The point estimates for the increase in the probability of dropping out range between 0.017 and 0.020 at age 16, between 0.047 and 0.055 at age 17, and between 0.036 and 0.043 at age 18. Estimates for public works participation range between 0.000 and 0.001 at age 16, between 0.003 and 0.004 at age 17 and it is 0.004 in all specifications at age 18. The effect on employment is not statistically significant in most of the specifications. Furthermore, our data shows that dropouts who did get employed stayed in short-term jobs for 3.5 months on average at a time. In more than 50% of these cases, they worked in occupations that did not require professional qualifications, for example as cleaners, garbage collectors, or loading workers. Instead of increasing employment, the reform had large effects on the probability of being NEET, the related point estimates range between 0.016 and 0.019 at age 16, between 0.046 and 0.050 at age 17, and between 0.029 and 0.035 at age 18. Point estimates for the increase in the probability of unemployment equal 0.003 at age 16, range between 0.011 and 0.013 at age 17, and between 0.012 and 0.015 at age 18. Point estimates for the increase in inactivity range between 0.013 and 0.015 at age 16, between 0.030 and 0.035 at age 17, and between 0.017 and 0.020 at age 18.

4.4. Heterogeneous Effects

Figures 6-8, Figure 9 and Tables OA25-OA30 in the Online Appendix present the results of our heterogenous effects estimations at age 17.[15] "Main effect" refers to the estimated β_{DiCCC} coefficients according to Equation (2) and they capture $\beta_{DiCCC} = \beta_{2012,2011} - \beta_{2011,2010}$. These estimates are denoted with red color in the figures. "Placebo" refers to the estimated $\beta_{DiCCCplacebo}$ placebo coefficients as in Equation (3) and capture $\beta_{DiCCCplacebo} = \beta_{2011,2010} - \beta_{2010,2009}$. These estimates are denoted with black color in the figures.

The results in Figures 6- 8 and Tables OA25-OA30 in the Online Appendix suggest that there are meaningful heterogeneities in the effect of the reform across all outcomes except for public works and employment. For example, our results demonstrate a higher probability of dropping out, becoming a NEET or an inactive for individuals whose parents have at most a primary degree, among those who did not complete the family background survey of the NABC test, for those who are in the lowest mathematics quintile or have not participated in the Grade 8 national mathematics test, and for those living in the regions with the lowest quintile of pre-reform subregional employment rate among males aged 25-55.

Relative to the main effect of 5.4 percentage points, the impact on the probability of dropping out is up to three times higher for certain groups: ranging from 13.9 to 15 percentage points for those with low or missing parental background, 10.6 to 11.9 percentage points for low or missing mathematics performance, and 9.1 percentage points for poor pre-reform employment indicators. Similarly, the probability of becoming NEET or inactive increases by 13.1 to 13.7 percentage points for low or missing parental background, 9.8 to 10.4 percentage points for low or missing mathematics, and 8.3 percentage points for poor pre-reform employment indicators. A comparable trend is seen for unemployment, although the differences are less pronounced compared to the other outcomes. Conversely, the effects are close to zero among those with high school or tertiary-educated parents, pointing to a strong intergenerational dependence on past outcomes. Close to zero effects are also estimated for upper-middle or highest mathematics or pre-reform employment quintile.

Finally, to examine the effect of the reform on a unified measure of dropout risk, Figure 9 and Tables OA30-OA36 in the Online Appendix show the heterogeneity of our results by predicted dropout probability. These results reveal a positive association between predicted dropout risk and the impact of SLA. In the highest decile of dropout risk, the probability of dropping out increased by 16 percentage points, 3 times the average effect size. In the same group, the probability of becoming NEET increased by 14.7 percentage points, while

[15] Heterogeneous effects at other ages show similar patterns, but are not reported due to the space limitations and are available upon request.

the probability of becoming inactive increased by 9.8 percentage points. The probability of becoming unemployed increased by 3.4 percentage points for the same group. At the same time, we see little or no change in the probability of public works or being employed in this group.

Put together, these results suggest that the decrease in the SLA harms the most the youths with less-educated parents and with low mathematics skills. Estimation of the results by quintiles of pre-reform subregional employment indicates that the negative effects were also larger in labor markets with lower employment rates. This suggests that when the students from lower-SES backgrounds who were disproportionately impacted entered weaker labor markets, they had a harder time finding employment. This suggests that it is the most vulnerable individuals who drive our results, increasing inequality.

5. Conclusion

This paper has estimated the effects of lowering the SLA from 18 to 16 in Hungary on the labor market outcomes of the affected cohorts. We are the first to analyze such a reform that affected every student in a country, focusing on its direct effects between the ages of 16 and 18. Our results show that the reform substantially increased the probability of dropping out, especially among the most disadvantaged students. Contrary to policymakers' expectations, employment did not increase. Our results show that teenagers with no work experience and without a secondary degree have poor chances in the labor market. Furthermore, being a school dropout provides a strong negative signal to employers about abilities and expected productivity which would hinder labor market success. Dropouts who did get employed stayed in short-term jobs and in occupations that did not require many qualifications. Instead of increasing employment, the reform had large effects on the probability of being NEET, and more specifically, inactive.

We find that these effects are similar among men and women, but heterogeneous by social background and ability; they are substantially greater among those from the most at-risk groups. These include those whose parents have at most a primary degree, and those with low mathematics test scores. These findings suggest that the reform could have increased social inequalities and reduced intergenerational mobility.

The reform analyzed in this paper that decreased the SLA from 18 to 16 in 2012 followed an earlier reform that had increased the SLA from 16 to 18 in 1996. Although we are not able to directly compare the effects of the two symmetric reforms due to data availability issues, we can still make some joint conclusions. First, Adamecz (2023) has shown that increasing the SLA did not affect the probability of employment at age 20, neither did it increase the probability of earning a secondary degree. In this paper, we show evidence that the reverse reform also did not have a meaningful effect on labor market outcomes at age 19. In terms of contemporaneous effects, Adamecz-Völgyi and Ágota Scharle (2020) showed that increasing the SLA decreased the probability of teenage motherhood among Roma ethnic minority women, but this effect faded away by age 20; thus, no effects were found through the human capital channel. Taking these results together provides suggestive evidence that in the Hungarian context, a two-year difference in compulsory schooling might only have a limited short-term impact on employment prospects but has meaningful contemporaneous (incarceration or incapacitation) effects. Since the Hungarian education system allows grade retention, most at-risk students might repeat grades several times (Adamecz, 2023). Only those students who do not repeat a grade can earn a secondary degree by age 18, therefore increasing or decreasing the SLA by two years cannot directly affect the probability of earning a degree for many students, and through that, short-term employment prospects. Since having a secondary degree has a strong effect on employment in the European labor market (Grenet, 2013), the key to a successful intervention could be keeping young people in school until they earn a secondary degree (instead of a certain age).

A few caveats apply to our results. First, the reform was passed in December 2011; thus, theoretically could not have affected the probability of secondary school enrollment in September 2011. However, there was a debate about the reform in the press before its enactment, so its anticipation might have affected some students' and parents' decisions about secondary school enrollment. Still, although some might have expected the reform, the method of its enactment and the first potentially treated cohort were not known by the public ex-ante. Also, when we compare the outcomes of the two pre-reform school cohorts that completed primary school in 2010 and 2011, we find null effects. Second, we see young people in the data up until the age of 19 only. Some students might go back to school after dropping out and earn a secondary degree later. In our evaluation, we implicitly assume that the reform did not influence the share of those who would go back to school after dropping out. However, if it did, we might over- or underestimate the reform's effect depending on the direction of this relationship. Finally, as these cohorts are still young, we can only evaluate the effects of the reform until age 19, but it is expected to have long-run human capital effects as well. The evaluation of these is left to further analysis.

Our results also have significant policy implications for any country that may be planning to lower the SLA. The intended purpose of the reduction may be to relieve the burden on the education budget or to create employment opportunities for young people who do not wish to study. However, our results indicate that merely lowering the SLA cannot direct dropouts to the labor market. Instead, based on our results, various negative consequences can be expected. For example, NEET status and unemployment might result in social isolation and increased mental health burden in the long term. A school environment and a delayed labor market entry would protect young people from these consequences. Furthermore, a flexible model that combines part-time schooling and part-time employment could be more successful in helping young people enter the labor market while mitigating the negative effects of dropping out. Thus, potential dropouts would retain some sense of belonging to the community and not be marginalized, and in the meantime, they would receive institutional support for career planning and entering the workforce, similar to the Danish model (Andersen and Kruse, 2014).

Declarations

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Supplementary information The Online Appendix for this paper is available here.

Declaration of competing interest The authors declare that they have no competing financial interests or personal relationships that could have appeared to influence the work reported in this paper. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not represent the views of the World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent.

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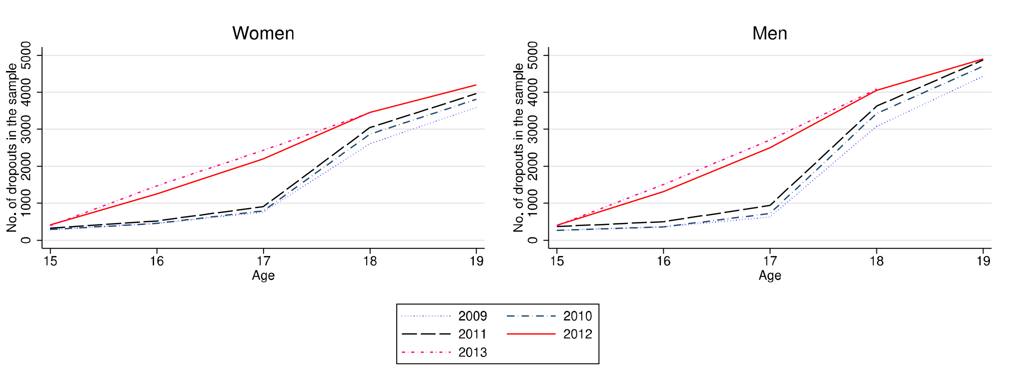
Table 1: Number of Observations in the Sample by Age and School Cohort

	Age					
	15	16	17	18	19	Total
2009	552,992	583,080	591,694	591,227	578,397	2,897,390
2010	587,296	597,787	601,993	590,733	581,629	2,959,438
2011	563,355	568,415	561,374	551,739	533,097	2,777,980
2012	537,835	539,051	537,077	518,635	491,607	2,624,205
2013	519,278	519,570	506,848	481,239	239,443	2,266,378

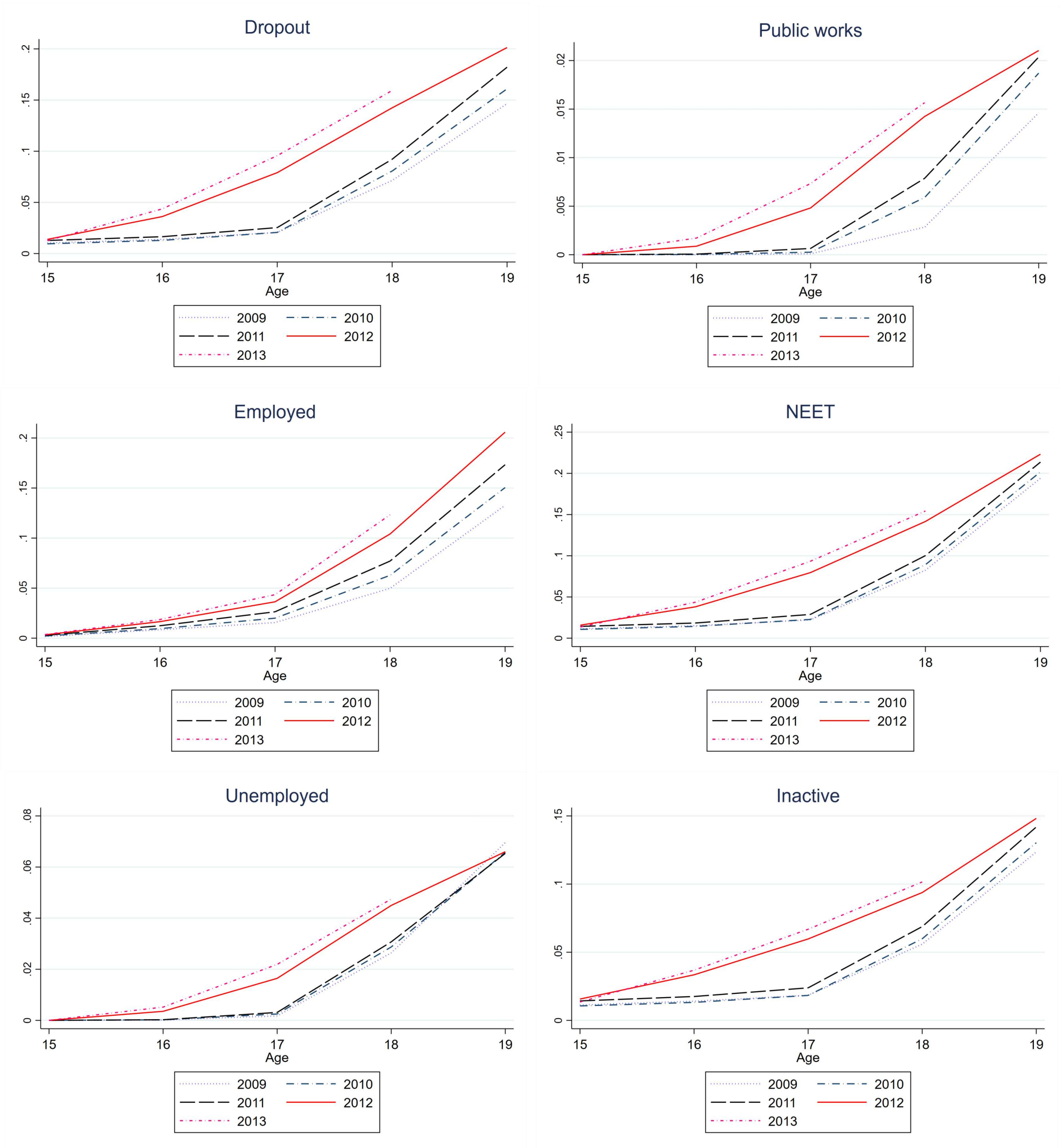
Notes: Table shows the number of observations by cohort, defined as year of finishing primary school (2009, 2010, 2011, 2012, and 2013) and age of observation (15, 16, 17, 18, and 19). Data coverage runs from 2009 to 2017. The gray cells indicate cohorts and ages that were directly affected by the reform: students who finished primary school in 2012 and 2013 were affected when they were 16, 17, and 18. Among students who finished primary school in 2013, more than half of students are not covered at age 19 by the data because they reached age 19 after 2017, the last year of data coverage. Thus, we exclude this cell from sample when we use the age 19 outcomes of the 2013 cohort.

Figure 1: Number of Dropouts in the Sample by School Cohort



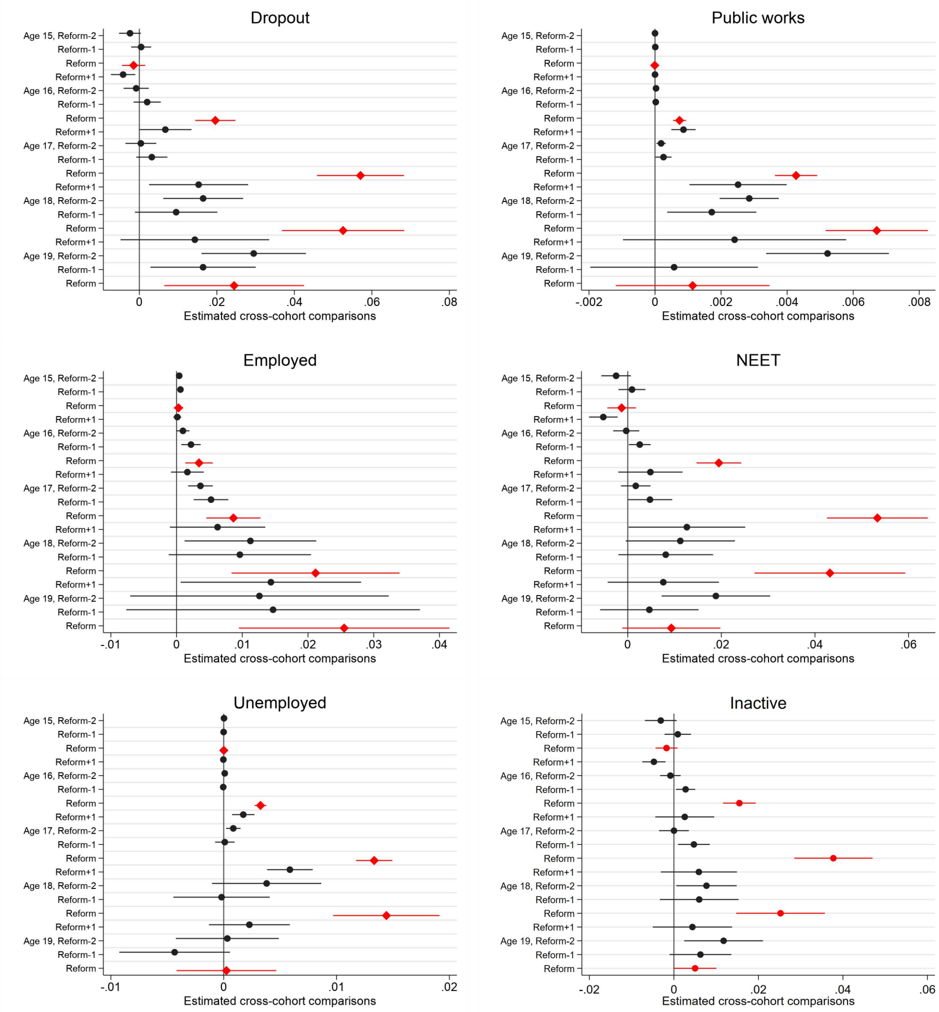
Notes: Figure shows the number of dropouts between ages 15 and 19 by school cohort. The lines indicate school cohorts finishing primary school before (2009, 2010, and 2011) and after (2012 and 2013) the reform. Dropouts are those who left school without earning a secondary degree. Our sample covers a random 50% of the population. Thus, the total number of dropouts in the country was about twice as large as indicated by this figure. ($N = 13,141,574$ person-months)

Figure 2: Outcomes by School Cohorts



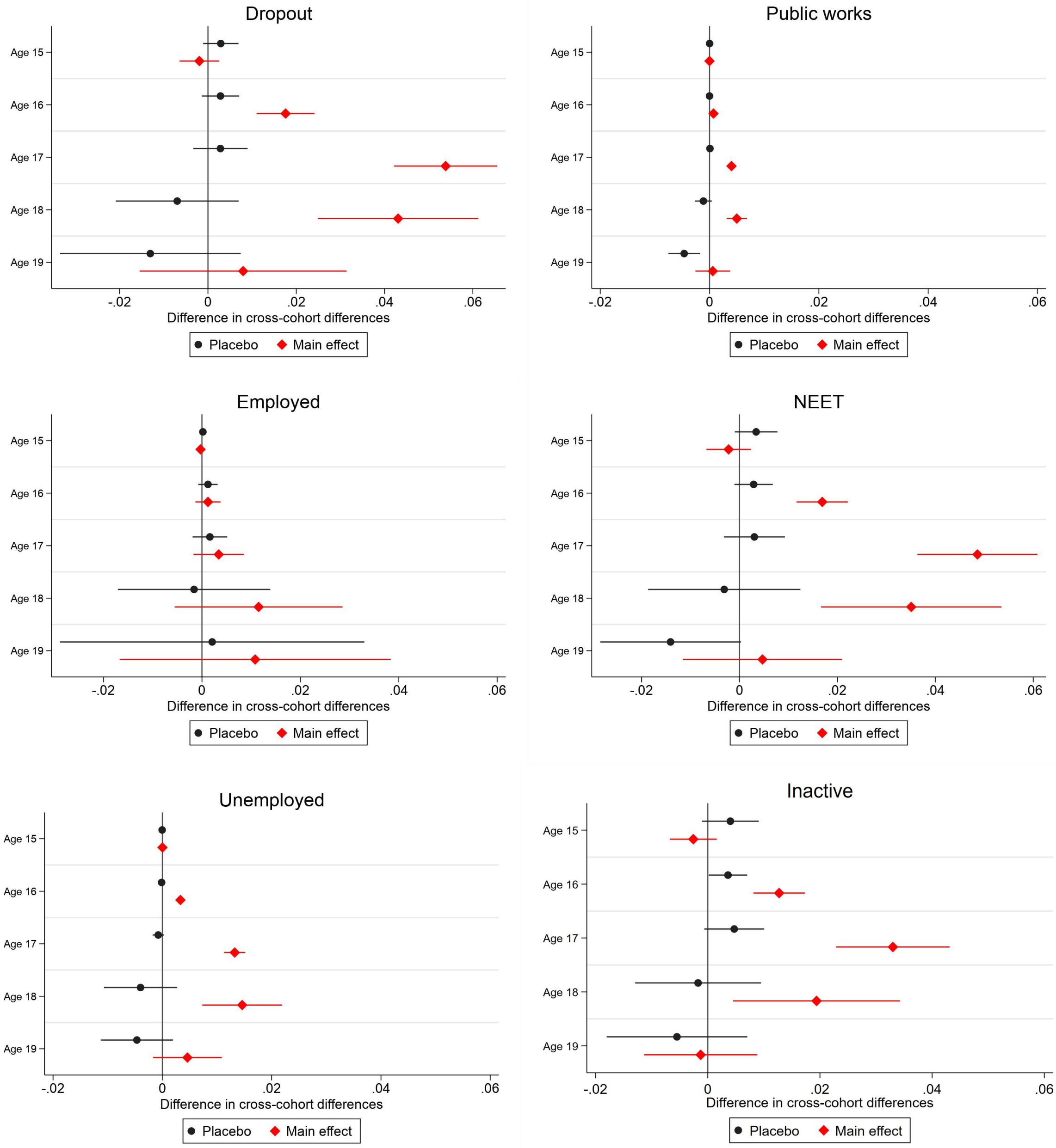
Notes: The figure shows the means of the outcome variables between ages 15 and 19 by school cohorts. The means capture the average probability of being in the indicated state at a specific age. The lines indicate school cohorts finishing primary school before (2009, 2010, and 2011) and after (2012 and 2013) the reform. All outcome variables are plotted on the whole sample ($N = 13,141,574$ person-months).

Figure 3: The Cross-Cohort Comparisons of Outcome Variables



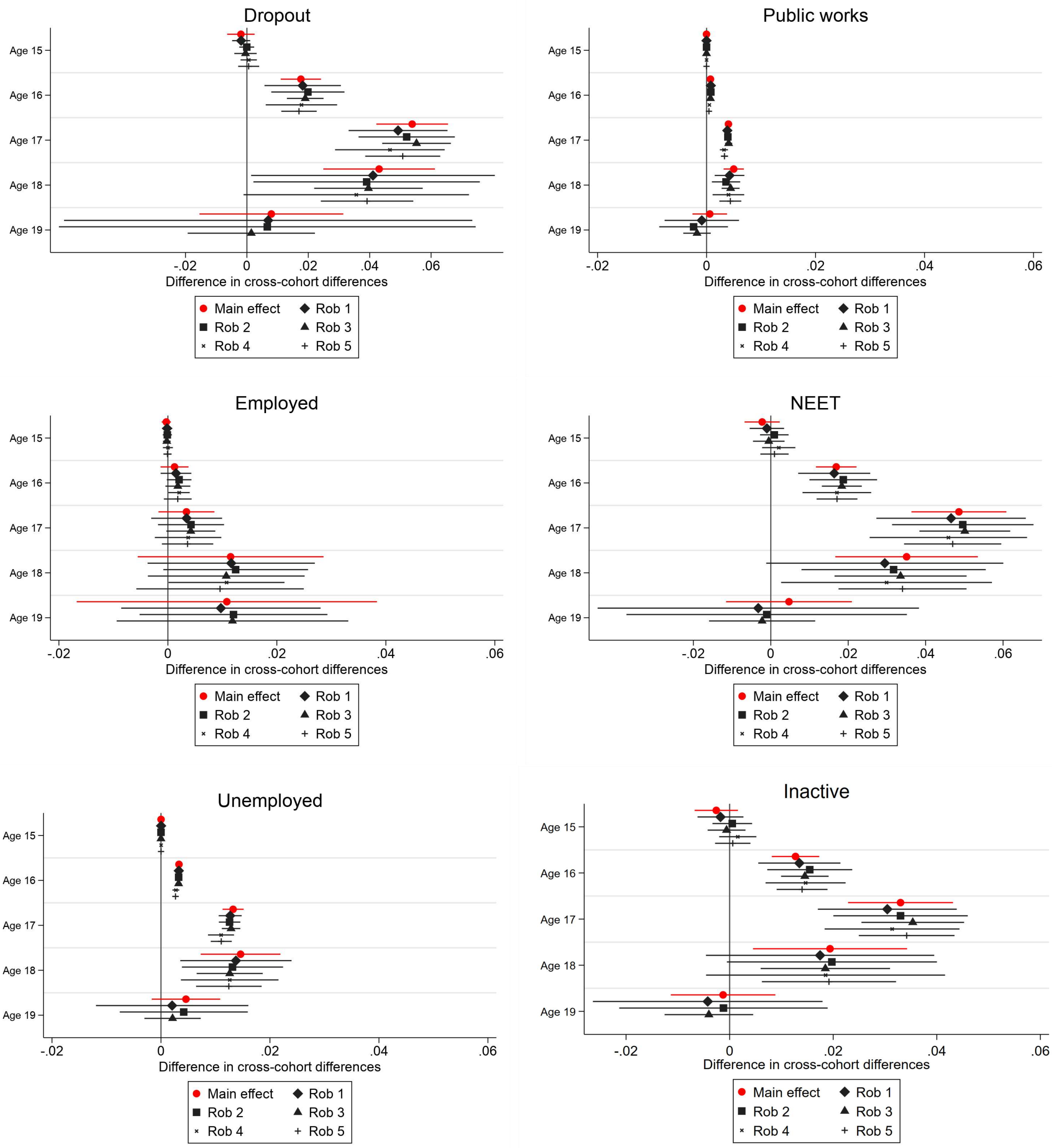
Notes: The figure shows the difference between the outcome variables of two consecutive school cohorts estimated on the subsample of two consecutive school cohorts at a time, separately by age, according to Equation (1), in percentage points. The detailed regression estimates of each model are presented in Tables OA1-OA18 in the Online Appendix. Control variables: gender, parental education, Grade-8 math test scores, month of observation, month of birth, local labor market (subregion) fixed effects. Estimated parameters $\beta_{j+1,j}$ are plotted on the graphs along with their 95% confidence intervals based on bootstrapped standard errors clustered by year-and-month of birth. All plotted parameters are estimated in separate models. "Reform-2" shows the comparison of those who finished primary school in 2010 vs. 2009 ($\beta_{2010,2009}$), "Reform-1" shows the comparison of those who finished primary school in 2011 vs. 2010 ($\beta_{2011,2010}$), "Reform" shows the comparison of those affected by the reform, who finished primary school in 2012 (i.e. in the first treated year) vs. 2011 ($\beta_{2012,2011}$), while "Reform+1" shows the comparison of those who finished primary school in 2013 vs. 2012 ($\beta_{2013,2012}$). $N = 13,141,574$ person-months.

Figure 4: The Effects of the Reform: Differences of Cross-Cohort Comparisons



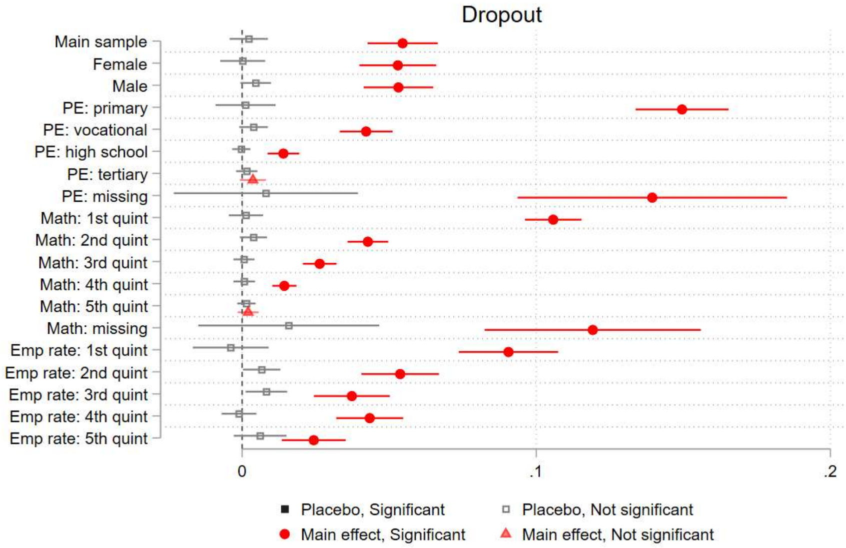
Notes: "Main effect" refers to the estimated β_{DiCCC} coefficients according to Equation (2) and they capture $\beta_{DiCCC} = \beta_{2012,2011} - \beta_{2011,2010}$. "Placebo" refers to the estimated $\beta_{DiCCCplacebo}$ placebo coefficients as in Equation (3) and capture $\beta_{DiCCCplacebo} = \beta_{2011,2010} - \beta_{2010,2009}$. Control variables: gender, parental education, Grade-8 math test scores, month of observation, month of birth, subregion fixed effects. 95% confidence intervals estimated by bootstrapping, clustered by year-and-month of birth. The estimated coefficients, baseline means and the effect sizes in % are reported in Appendix Tables A6-A11. $N = 13,141,574$ person-months.

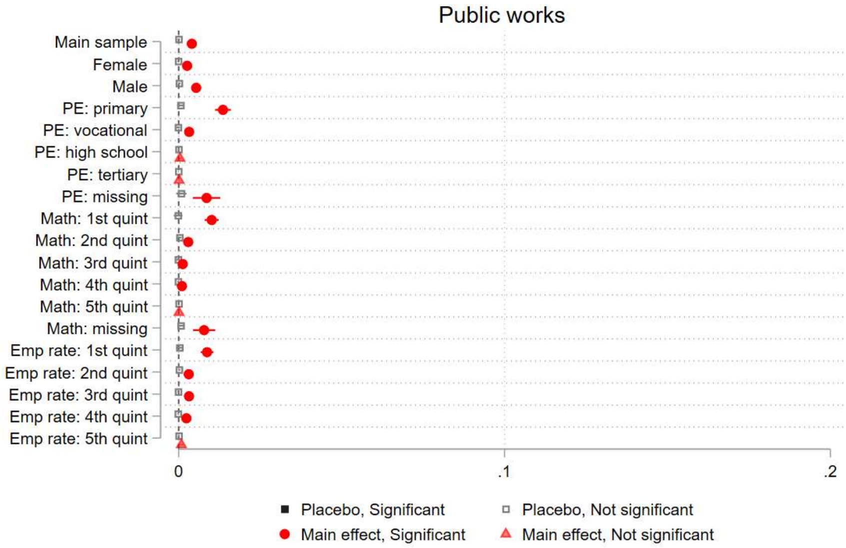
Figure 5: Robustness Tests to the Differences of Cross-Cohort Comparisons Estimates



Notes: "Main effect" refers to the Estimated β_{DiCCC} coefficients according to Equation (2). "Rob 1" refers to estimating β_{DiCCC} the same way as before, but without controlling for any individual characteristics when estimating $\beta_{2012,2011}$ and $\beta_{2011,2010}$. In "Rob 2" and "Rob 3", instead of $\beta_{2011,2010}$, we subtract the mean of $\beta_{2011,2010}$ and $\beta_{2010,2009}$ from $\beta_{2012,2011}$, both without control variables ("Rob 2") and with control variables ("Rob 3"). Lastly, we subtract the mean of $\beta_{2011,2010}$, $\beta_{2010,2009}$ and $\beta_{2013,2012}$ from $\beta_{2012,2011}$, both without control variables ("Rob 4") and with control variables ("Rob 5"). These last two models are not feasible to estimate at age 19. 95% confidence intervals estimated by bootstrapping. $N = 13,141,574$ person-months. The estimated coefficients are reported in Tables OA19-OA24 the Online Appendix.

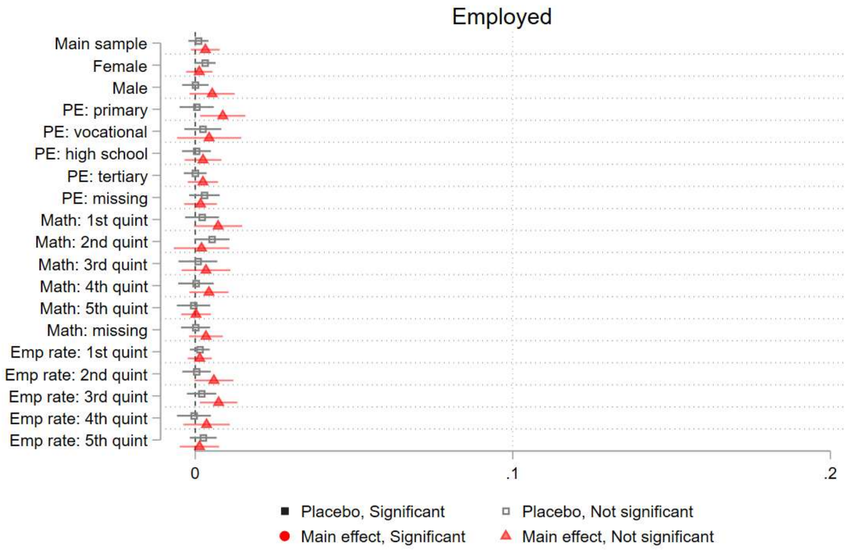
Figure 6: Heterogeneity of the Effects at Age 17 (Part 1)

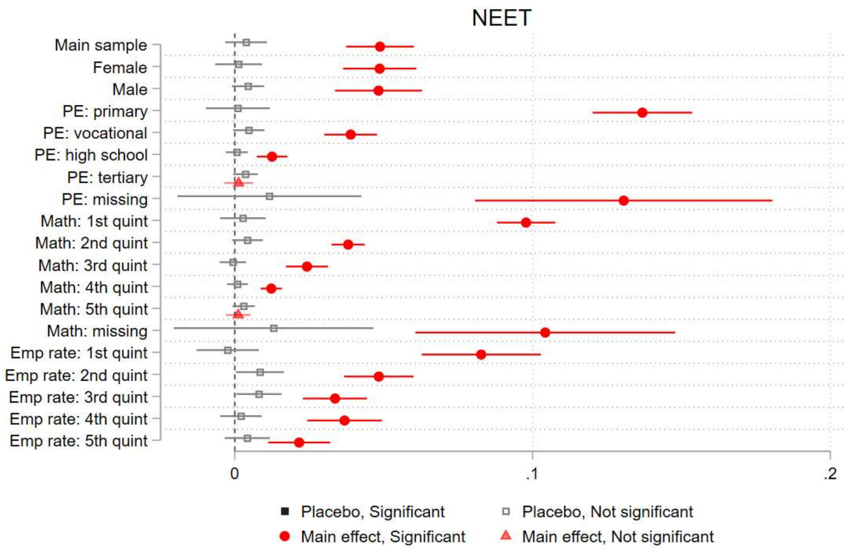




Notes: Estimated β_{DiCCC} coefficients according to Equation (2). "Main effect" refers to the estimated β_{DiCCC} coefficients according to Equation (2) and they capture $\beta_{DiCCC} = \beta_{2012,2011} - \beta_{2011,2010}$. "Placebo" refers to the estimated $\beta_{DiCCCplacebo}$ coefficients as in Equation (3) and capture $\beta_{DiCCCplacebo} = \beta_{2011,2010} - \beta_{2010,2009}$. Control variables (if applicable): gender, parental education, Grade-8 math test scores, month of observation, month of birth, subregion fixed effects. Estimated coefficients are plotted along with their bootstrapped 95% confidence intervals. For the heterogeneity analysis, we tested 1,140 parallel hypotheses together, thus we apply the multiple testing procedure of Benjamini and Hochberg (1995). We indicate whether the procedure rejected ("Significant") or did not reject ("Not significant") the null hypothesis of $\beta = 0$ at the 95% level. $N = 2,798,986$ person-months. The estimated coefficients are reported in Tables OA25–OA30 in the Online Appendix. Further estimates in other age groups are available upon request.

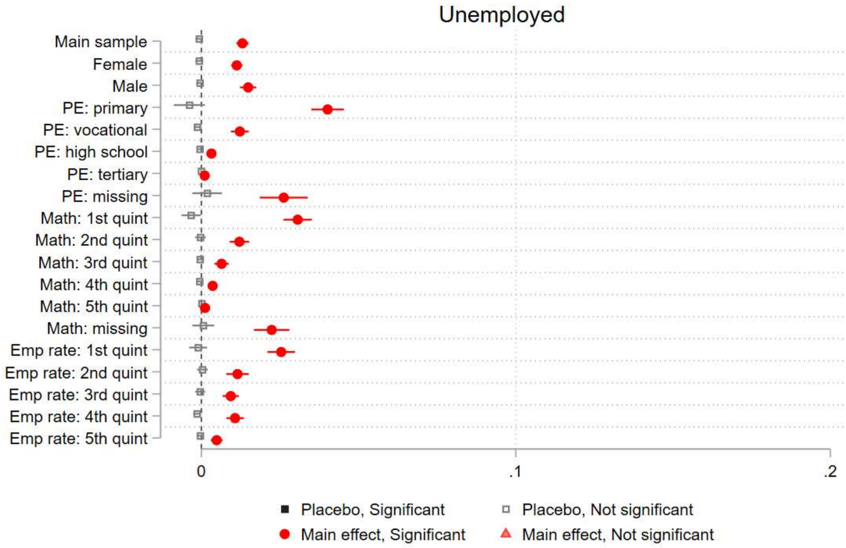
Figure 7: Heterogeneity of the Effects at Age 17 (Part 2)

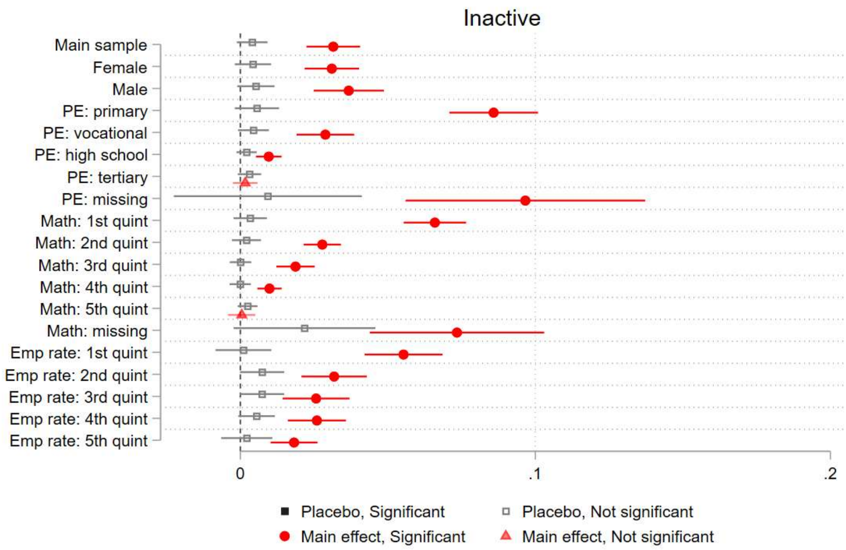




Notes: Estimated β_{DiCCC} coefficients according to Equation (2). "Main effect" refers to the estimated β_{DiCCC} coefficients according to Equation (2) and they capture $\beta_{DiCCC} = \beta_{2012,2011} - \beta_{2011,2010}$. "Placebo" refers to the estimated $\beta_{DiCCCplacebo}$ coefficients as in Equation (3) and capture $\beta_{DiCCCplacebo} = \beta_{2011,2010} - \beta_{2010,2009}$. Control variables (if applicable): gender, parental education, Grade-8 math test scores, month of observation, month of birth, subregion fixed effects. Estimated coefficients are plotted along with their bootstrapped 95% confidence intervals. For the heterogeneity analysis, we tested 1,140 parallel hypotheses together, thus we apply the multiple testing procedure of Benjamini and Hochberg (1995). We indicate whether the procedure rejected ("Significant") or did not reject ("Not significant") the null hypothesis of $\beta = 0$ at the 95% level. $N = 2,798,986$ person-months. The estimated coefficients are reported in Tables OA25–OA30 in the Online Appendix. Further estimates in other age groups are available upon request.

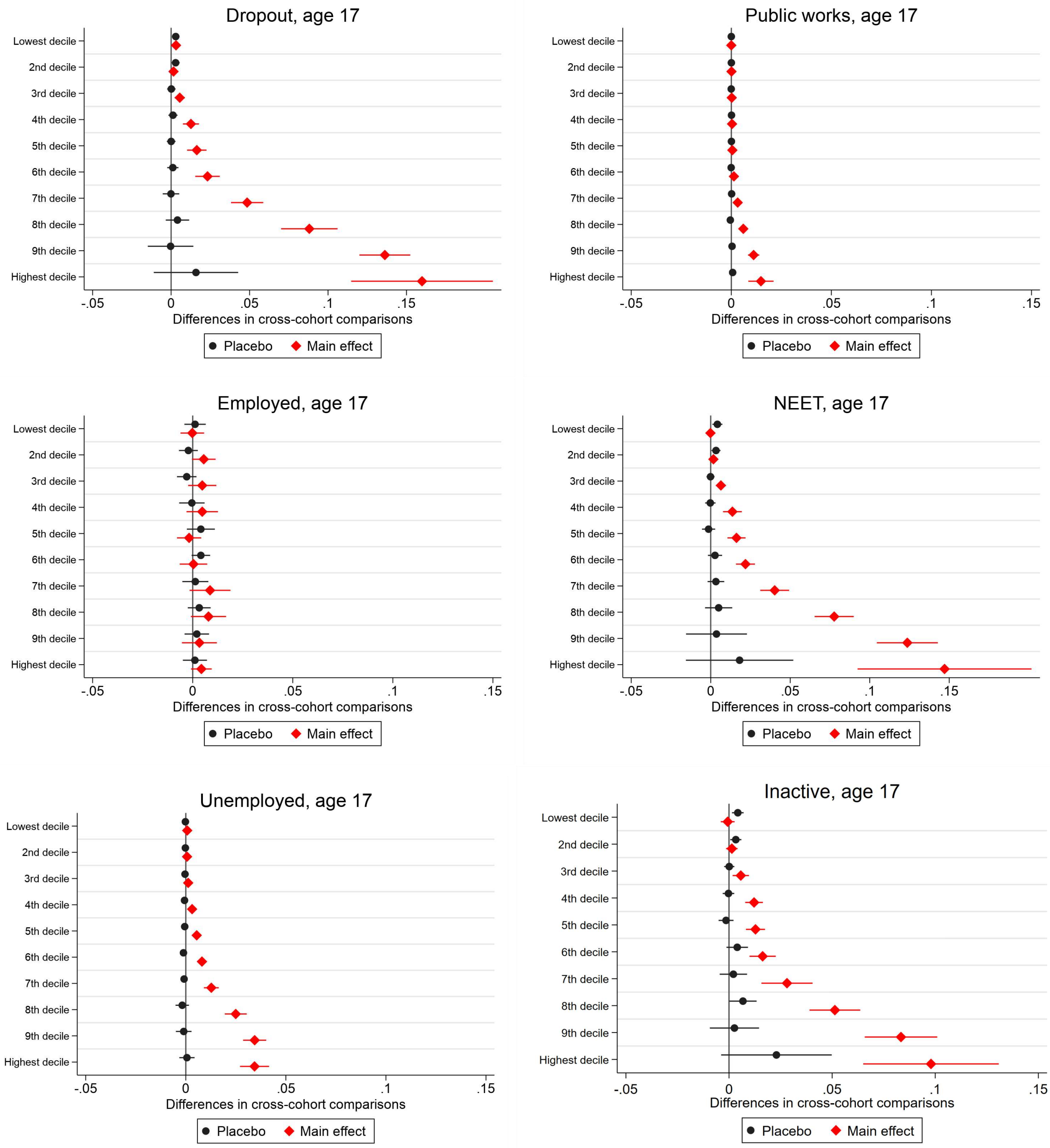
Figure 8: Heterogeneity of the Effects at Age 17 (Part 3)





Notes: Estimated β_{DiCCC} coefficients according to Equation (2). "Main effect" refers to the estimated β_{DiCCC} coefficients according to Equation (2) and they capture $\beta_{DiCCC} = \beta_{2012,2011} - \beta_{2011,2010}$. "Placebo" refers to the estimated $\beta_{DiCCCplacebo}$ coefficients as in Equation (3) and capture $\beta_{DiCCCplacebo} = \beta_{2011,2010} - \beta_{2010,2009}$. Control variables (if applicable): gender, parental education, Grade-8 math test scores, month of observation, month of birth, subregion fixed effects. Estimated coefficients are plotted along with their bootstrapped 95% confidence intervals. For the heterogeneity analysis, we tested 1,140 parallel hypotheses together, thus we apply the multiple testing procedure of Benjamini and Hochberg (1995). We indicate whether the procedure rejected ("Significant") or did not reject ("Not significant") the null hypothesis of $\beta = 0$ at the 95% level. $N = 2,798,986$ person-months. The estimated coefficients are reported in Tables OA25–OA30 in the Online Appendix. Further estimates in other age groups are available upon request.

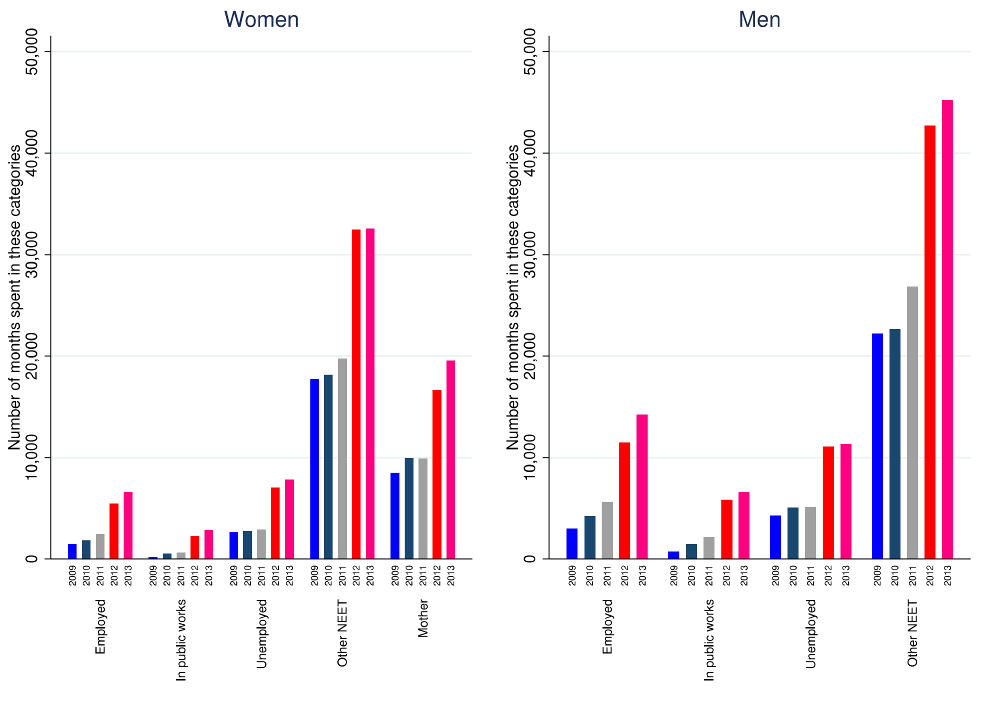
Figure 9: The Heterogeneity of the Effects by Predicted Dropout Probabilities at Age 17



Notes: This figure shows difference-in-differences estimates from Equation (2). "Main effect" refers to the estimated β_{DiCCC} coefficients according to Equation (2) and they capture $\beta_{DiCCC} = \beta_{2012,2011} - \beta_{2011,2010}$. "Placebo" refers to the estimated $\beta_{DiCCCplacebo}$ placebo coefficients as in Equation (3) and capture $\beta_{DiCCCplacebo} = \beta_{2011,2010} - \beta_{2010,2009}$. Control variables (if applicable): gender, parental education, Grade-8 math test scores, month of observation, month of birth, subregion fixed effects. Estimated coefficients are plotted along with their 95% confidence intervals. Predicted dropout probability strata were estimated using the method of Abadie, Chingos and West (2018). No. of observations: $N = 2,798,986$ person-months. The estimated coefficients are reported in Tables OA31-OA36 in the Online Appendix.

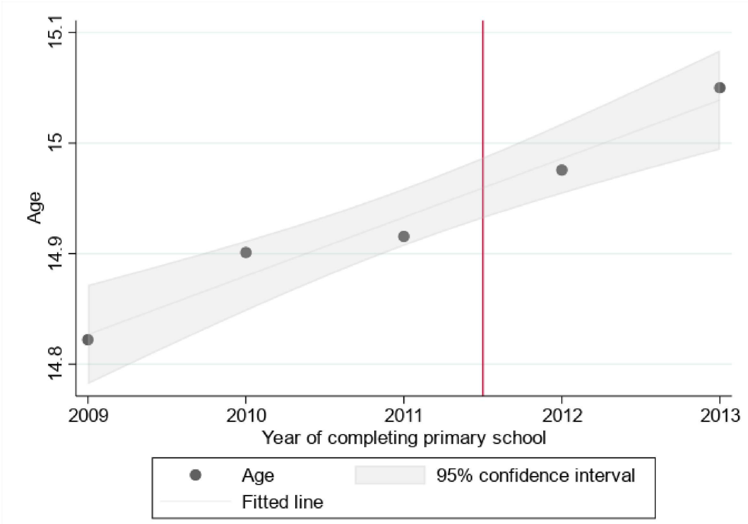
Appendix

Appendix Figure A1: Outcomes of Dropouts Between Ages 16 and 18 by School Cohort



Notes: Figure shows the distribution of outcomes before (2009, 2010, and 2011) and after (2012 and 2013) the reform, between ages 16 and 18. Each bar shows the total number of person-months with the given outcome. The left panel shows women ($N = 233,012$ person-months) and the right panel shows men ($N = 252,505$ person-months).

Appendix Figure A2: Average Age of Finishing Primary School by School Cohort



Notes: Figure shows the average age when students finished primary school before (2009, 2010, and 2011) and after (2012 and 2013) the reform. ($N = 237,044$ individuals.)

Appendix Table A1: Descriptive Statistics, Age 15

	Pre-reform cohorts	Post-reform cohorts	Difference	t-test p-value	Obs
Female	0.49	0.48	-0.01	0.327	2,760,756
Year of birth	1995.36	1997.56	2.20	0.000	2,760,756
Month of birth	6.58	6.55	-0.03	0.964	2,760,756
Calendar month of observation	6.46	6.50	0.04	0.006	2,760,756
Age of finishing primary school	14.83	14.99	0.16	0.043	2,760,756
Age of observation	15.00	15.00	0.00	.	2,760,756
Month of observation (t)	100.42	126.74	26.32	0.000	2,760,756
Parental education					
Primary	0.14	0.16	0.02	0.011	2,760,756
Vocational	0.26	0.25	-0.00	0.509	2,760,756
High school	0.29	0.28	-0.01	0.148	2,760,756
Tertiary	0.23	0.23	0.00	0.972	2,760,756
Missing	0.08	0.08	-0.00	0.918	2,760,756
Grade-8 math test scores	1608.76	1605.33	-3.43	0.498	2,447,979
Grade-8 math test score quintiles					
Lowest quintile	0.18	0.17	-0.00	0.692	2,760,756
Lower-middle quintile	0.18	0.17	-0.01	0.021	2,760,756
Middle quintile	0.18	0.17	-0.01	0.184	2,760,756
Upper-middle quintile	0.18	0.17	-0.01	0.120	2,760,756
Highest quintile	0.18	0.17	-0.01	0.229	2,760,756
Missing	0.10	0.13	0.04	0.004	2,760,756
Subregional employment rate	0.65	0.65	-0.00	0.000	2,747,201
Year of finishing primary school	2010.01	2012.49	2.49	0.000	2,760,756
Outcome variables					
Dropout	0.01	0.01	0.00	0.253	2,760,756
Public works	0.00	0.00	-0.00	0.316	2,760,756
Employment	0.00	0.00	0.00	0.000	2,760,756
NEET	0.01	0.01	0.00	0.150	2,760,756
Registered unemployed	0.00	0.00	-0.00	0.949	2,760,756
Inactive	0.01	0.01	0.00	0.166	2,760,756

Notes: Table shows summary statistics for school cohorts finishing primary school before (2009, 2010, and 2011) and after (2012, 2013) the reform.

Appendix Table A2: Descriptive Statistics, Age 16

	Pre-reform cohorts	Post-reform cohorts	Difference	t-test p-value	Obs
Female	0.48	0.48	-0.00	0.589	2,807,903
Year of birth	1995.29	1997.55	2.26	0.000	2,807,903
Month of birth	6.58	6.55	-0.02	0.969	2,807,903
Calendar month of observation	6.49	6.50	0.01	0.006	2,807,903
Age of finishing primary school	14.88	14.99	0.12	0.151	2,807,903
Age of observation	16.00	16.00	0.00	.	2,807,903
Month of observation (t)	111.56	138.65	27.09	0.000	2,807,903
Parental education					
Primary	0.14	0.16	0.01	0.042	2,807,903
Vocational	0.25	0.25	-0.00	0.792	2,807,903
High school	0.29	0.28	-0.01	0.457	2,807,903
Tertiary	0.23	0.23	0.01	0.540	2,807,903
Missing	0.09	0.08	-0.01	0.338	2,807,903
Grade-8 math test scores	1605.87	1605.28	-0.58	0.911	2,471,535
Grade-8 math test score quintiles					
Lowest quintile	0.18	0.17	-0.01	0.411	2,807,903
Lower-middle quintile	0.18	0.17	-0.01	0.046	2,807,903
Middle quintile	0.18	0.17	-0.00	0.484	2,807,903
Upper-middle quintile	0.18	0.17	-0.00	0.401	2,807,903
Highest quintile	0.18	0.17	-0.00	0.583	2,807,903
Missing	0.11	0.14	0.03	0.051	2,807,903
Subregional employment rate	0.65	0.65	-0.00	0.000	2,793,917
Year of finishing primary school	2009.99	2012.49	2.50	0.000	2,807,903
Outcome variables					
Dropout	0.01	0.04	0.03	0.000	2,807,903
Public works	0.00	0.00	0.00	0.000	2,807,903
Employment	0.01	0.02	0.01	0.000	2,807,903
NEET	0.02	0.04	0.02	0.000	2,807,903
Registered unemployed	0.00	0.00	0.00	0.000	2,807,903
Inactive	0.01	0.03	0.02	0.000	2,807,903

Notes: Table shows summary statistics for school cohorts finishing primary school before (2009, 2010, and 2011) and after (2012, 2013) the reform.

Appendix Table A3: Descriptive Statistics, Age 17

	Pre-reform cohorts	Post-reform cohorts	Difference	t-test p-value	Obs
Female	0.48	0.48	-0.00	0.655	2,798,986
Year of birth	1995.23	1997.51	2.28	0.000	2,798,986
Month of birth	6.57	6.52	-0.04	0.947	2,798,986
Calendar month of observation	6.51	6.53	0.02	0.006	2,798,986
Age of finishing primary school	14.91	15.01	0.10	0.213	2,798,986
Age of observation	17.00	17.00	0.00	.	2,798,986
Month of observation (t)	122.83	150.12	27.29	0.000	2,798,986
Parental education					
Primary	0.15	0.16	0.01	0.031	2,798,986
Vocational	0.25	0.26	0.00	0.814	2,798,986
High school	0.28	0.28	-0.00	0.644	2,798,986
Tertiary	0.22	0.23	0.00	0.668	2,798,986
Missing	0.09	0.08	-0.02	0.191	2,798,986
Grade-8 math test scores	1604.35	1603.02	-1.34	0.799	2,454,305
Grade-8 math test score quintiles					
Lowest quintile	0.18	0.18	-0.00	0.529	2,798,986
Lower-middle quintile	0.18	0.17	-0.01	0.097	2,798,986
Middle quintile	0.18	0.17	-0.00	0.645	2,798,986
Upper-middle quintile	0.18	0.17	-0.00	0.502	2,798,986
Highest quintile	0.17	0.17	-0.00	0.533	2,798,986
Missing	0.12	0.14	0.02	0.122	2,798,986
Subregional employment rate	0.65	0.65	-0.00	0.000	2,785,056
Year of finishing primary school	2009.98	2012.49	2.50	0.000	2,798,986
Outcome variables					
Dropout	0.02	0.09	0.06	0.000	2,798,986
Public works	0.00	0.01	0.01	0.000	2,798,986
Employment	0.02	0.04	0.02	0.000	2,798,986
NEET	0.02	0.09	0.06	0.000	2,798,986
Registered unemployed	0.00	0.02	0.02	0.000	2,798,986
Inactive	0.02	0.06	0.04	0.000	2,798,986

Notes: Table shows summary statistics for school cohorts finishing primary school before (2009, 2010, and 2011) and after (2012, 2013) the reform.

Appendix Table A4: Descriptive Statistics, Age 18

	Pre-reform cohorts	Post-reform cohorts	Difference	t-test p-value	Obs
Female	0.48	0.48	-0.01	0.533	2,733,573
Year of birth	1995.17	1997.43	2.26	0.000	2,733,573
Month of birth	6.57	6.55	-0.02	0.975	2,733,573
Calendar month of observation	6.51	6.58	0.07	0.020	2,733,573
Age of finishing primary school	14.92	15.06	0.13	0.119	2,733,573
Age of observation	18.00	18.00	0.00	.	2,733,573
Month of observation (t)	134.09	161.10	27.01	0.000	2,733,573
Parental education					
Primary	0.15	0.17	0.02	0.011	2,733,573
Vocational	0.26	0.26	0.00	0.453	2,733,573
High school	0.28	0.28	-0.00	0.665	2,733,573
Tertiary	0.22	0.22	-0.00	0.629	2,733,573
Missing	0.09	0.08	-0.01	0.225	2,733,573
Grade-8 math test scores	1602.32	1597.75	-4.57	0.389	2,395,397
Grade-8 math test score quintiles					
Lowest quintile	0.18	0.18	-0.00	0.914	2,733,573
Lower-middle quintile	0.18	0.17	-0.01	0.154	2,733,573
Middle quintile	0.18	0.17	-0.00	0.628	2,733,573
Upper-middle quintile	0.18	0.17	-0.01	0.411	2,733,573
Highest quintile	0.17	0.16	-0.01	0.207	2,733,573
Missing	0.12	0.14	0.02	0.118	2,733,573
Subregional employment rate	0.65	0.65	-0.00	0.000	2,719,639
Year of finishing primary school	2009.98	2012.48	2.50	0.000	2,733,573
Outcome variables					
Dropout	0.08	0.15	0.07	0.000	2,733,573
Public works	0.01	0.01	0.01	0.000	2,733,573
Employment	0.06	0.11	0.05	0.000	2,733,573
NEET	0.09	0.15	0.06	0.000	2,733,573
Registered unemployed	0.03	0.05	0.02	0.000	2,733,573
Inactive	0.06	0.10	0.04	0.000	2,733,573

Notes: Table shows summary statistics for school cohorts finishing primary school before (2009, 2010, and 2011) and after (2012, 2013) the reform.

Appendix Table A5: Descriptive Statistics, Age 19

	Pre-reform cohorts	Post-reform cohorts	Difference	t-test p-value	Obs
Female	0.48	0.46	-0.02	0.030	2,424,173
Year of birth	1995.09	1997.10	2.01	0.000	2,424,173
Month of birth	6.57	6.48	-0.08	0.909	2,424,173
Calendar month of observation	6.53	7.08	0.55	0.001	2,424,173
Age of finishing primary school	14.94	15.21	0.27	0.005	2,424,173
Age of observation	19.00	19.00	0.00	.	2,424,173
Month of observation (t)	145.15	168.64	23.49	0.000	2,424,173
Parental education					
Primary	0.15	0.18	0.03	0.000	2,424,173
Vocational	0.26	0.26	-0.00	0.821	2,424,173
High school	0.29	0.27	-0.02	0.086	2,424,173
Tertiary	0.21	0.20	-0.01	0.106	2,424,173
Missing	0.09	0.10	0.00	0.913	2,424,173
Grade-8 math test scores	1598.48	1584.21	-14.26	0.028	2.111.091
Grade-8 math test score quintiles					
Lowest quintile	0.18	0.19	0.01	0.122	2,424,173
Lower-middle quintile	0.18	0.17	-0.01	0.042	2,424,173
Middle quintile	0.18	0.17	-0.01	0.050	2,424,173
Upper-middle quintile	0.18	0.16	-0.02	0.025	2,424,173
Highest quintile	0.17	0.15	-0.02	0.020	2,424,173
Missing	0.12	0.16	0.05	0.011	2,424,173
Subregional employment rate	0.65	0.65	-0.00	0.000	2,410,705
Year of finishing primary school	2009.97	2012.33	2.35	0.000	2,424,173
Outcome variables					
Dropout	0.16	0.23	0.07	0.000	2,424,173
Public works	0.02	0.02	0.00	0.031	2,424,173
Employment	0.15	0.20	0.05	0.000	2,424,173
NEET	0.20	0.24	0.04	0.008	2,424,173
Registered unemployed	0.07	0.07	0.00	0.411	2,424,173
Inactive	0.13	0.16	0.02	0.001	2,424,173

Notes: Table shows summary statistics for school cohorts finishing primary school before (2009, 2010, and 2011) and after (2012, 2013) the reform.

Appendix Table A6: The estimated DiCCC effects shown in Figure 4. Outcome: Dropout

Outcome	Age	Model	Beta	SE	95% CI Low	95% CI High	Mean	Significant effects in %
Dropout	15	Placebo	0.003	.002	-.001	.007	.009	
Dropout	15	Main effect	–0.002	.002	-.006	.002	.009	
Dropout	16	Placebo	0.003	.002	-.002	.008	.013	
Dropout	16	Main effect	0.017***	.003	.011	.022	.014	120
Dropout	17	Placebo	0.002	.003	-.004	.009	.021	
Dropout	17	Main effect	0.055***	.006	.043	.066	.023	238
Dropout	18	Placebo	–0.007	.008	-.022	.008	.076	
Dropout	18	Main effect	0.043***	.01	.024	.063	.086	50
Dropout	19	Placebo	–0.014	.012	-.037	.01	.154	
Dropout	19	Main effect	0.005	.016	-.026	.036	.171	

Notes: "Main effect" refers to the estimated β_{DiCCC} coefficients according to Equation (2) and they capture $\beta_{DiCCC} = \beta_{2012,2011} - \beta_{2011,2010}$. "Placebo" refers to the estimated $\beta_{DiCCCplacebo}$ placebo coefficients as in Equation (3) and capture $\beta_{DiCCCplacebo} = \beta_{2011,2010} - \beta_{2010,2009}$. Control variables: gender, parental education, Grade-8 math test scores, month of observation, month of birth, subregion fixed effects. 95% confidence intervals estimated by bootstrapping, N=50. * p<0.10 , ** p<0.05, *** p<0.01. P-values are estimated based on the distribution of bootstrapped coefficients. $N = 13,141,574$ person-months. Mean: mean of the control group.

Appendix Table A7: The estimated DiCCC effects shown in Figure 4. Outcome: Public Works

Outcome	Age	Model	Beta	SE	95% CI Low	95% CI High	Mean	Significant effects in %
Public works	15	Placebo	0.000	0	0	0	0	
Public works	15	Main effect	0.000*	0	0	0	0	
Public works	16	Placebo	0.000	0	0	0	0	
Public works	16	Main effect	0.001***	0	.001	.001	0	NR
Public works	17	Placebo	0.000	0	0	0	0	
Public works	17	Main effect	0.004***	0	.003	.005	0	NR
Public works	18	Placebo	–0.001**	.001	-.003	0	.004	
Public works	18	Main effect	0.005***	.002	.002	.008	.007	75
Public works	19	Placebo	–0.004***	.001	-.007	-.002	.017	
Public works	19	Main effect	0.000	.001	-.002	.003	.019	

Notes: "Main effect" refers to the estimated β_{DiCCC} coefficients according to Equation (2) and they capture $\beta_{DiCCC} = \beta_{2012,2011} - \beta_{2011,2010}$. "Placebo" refers to the estimated $\beta_{DiCCCplacebo}$ placebo coefficients as in Equation (3) and capture $\beta_{DiCCCplacebo} = \beta_{2011,2010} - \beta_{2010,2009}$. Control variables: gender, parental education, Grade-8 math test scores, month of observation, month of birth, subregion fixed effects. 95% confidence intervals estimated by bootstrapping, N=50. * p<0.10 , ** p<0.05, *** p<0.01. P-values are estimated based on the distribution of bootstrapped coefficients. While the effects of the reform on public works are significant at ages 16 and 17, we do not report (NR) their percent magnitudes as they are irrealistically large due to the probability of public works participation being close to zero before the reform. $N = 13,141,574$ person-months. Mean: mean of the control group.

Appendix Table A8: The estimated DiCCC effects shown in Figure 4. Outcome: Employed

Outcome	Age	Model	Beta	SE	95% CI Low	95% CI High	Mean	Significant effects in %
Employed	15	Placebo	0.000	0	0	.001	.002	
Employed	15	Main effect	0.000	0	-.001	.001	.002	
Employed	16	Placebo	0.001	.001	0	.003	.009	
Employed	16	Main effect	0.001	.001	-.001	.003	.011	
Employed	17	Placebo	0.001	.002	-.002	.004	.018	
Employed	17	Main effect	0.003	.002	-.001	.008	.023	
Employed	18	Placebo	0.001	.009	-.016	.018	.056	
Employed	18	Main effect	0.012	.011	-.009	.033	.07	
Employed	19	Placebo	0.002	.013	-.023	.027	.142	
Employed	19	Main effect	0.009	.014	-.019	.037	.161	

Notes: "Main effect" refers to the estimated β_{DiCCC} coefficients according to Equation (2) and they capture $\beta_{DiCCC} = \beta_{2012,2011} - \beta_{2011,2010}$. "Placebo" refers to the estimated $\beta_{DiCCCplacebo}$ placebo coefficients as in Equation (3) and capture $\beta_{DiCCCplacebo} = \beta_{2011,2010} - \beta_{2010,2009}$. Control variables: gender, parental education, Grade-8 math test scores, month of observation, month of birth, subregion fixed effects. 95% confidence intervals estimated by bootstrapping, N=50. * p<0.10 , ** p<0.05, *** p<0.01. P-values are estimated based on the distribution of bootstrapped coefficients. $N = 13,141,574$ person-months. Mean: mean of the control group.

Appendix Table A9: The estimated DiCCC effects shown in Figure 4. Outcome: NEET

Outcome	Age	Model	Beta	SE	95% CI Low	95% CI High	Mean	Significant effects in %
NEET	15	Placebo	0.003*	.002	0	.007	.01	
NEET	15	Main effect	–0.002	.002	-.006	.002	.01	
NEET	16	Placebo	0.003	.002	-.001	.007	.014	
NEET	16	Main effect	0.017***	.003	.012	.022	.016	112
NEET	17	Placebo	0.004	.004	-.003	.011	.022	
NEET	17	Main effect	0.049***	.006	.037	.06	.026	190
NEET	18	Placebo	–0.004	.009	-.022	.014	.086	
NEET	18	Main effect	0.035***	.009	.017	.052	.095	37
NEET	19	Placebo	–0.017*	.009	-.035	.001	.198	
NEET	19	Main effect	0.007	.009	-.011	.025	.207	

Notes: "Main effect" refers to the estimated β_{DiCCC} coefficients according to Equation (2) and they capture $\beta_{DiCCC} = \beta_{2012,2011} - \beta_{2011,2010}$. "Placebo" refers to the estimated $\beta_{DiCCCplacebo}$ placebo coefficients as in Equation (3) and capture $\beta_{DiCCCplacebo} = \beta_{2011,2010} - \beta_{2010,2009}$. Control variables: gender, parental education, Grade-8 math test scores, month of observation, month of birth, subregion fixed effects. 95% confidence intervals estimated by bootstrapping, N=50. * p<0.10 , ** p<0.05, *** p<0.01. P-values are estimated based on the distribution of bootstrapped coefficients. $N = 13,141,574$ person-months. Mean: mean of the control group.

Appendix Table A10: The estimated DiCCC effects shown in Figure 4. Outcome: Unemployed

Outcome	Age	Model	Beta	SE	95% CI Low	95% CI High	Mean	Significant effects in %
Unemployed	15	Placebo	0.000	0	0	0	0	
Unemployed	15	Main effect	0.000	0	0	0	0	
Unemployed	16	Placebo	0.000	0	0	0	0	
Unemployed	16	Main effect	0.003***	0	.003	.004	0	NR
Unemployed	17	Placebo	–0.001	.001	-.002	0	.002	
Unemployed	17	Main effect	0.013***	.001	.011	.015	.003	467
Unemployed	18	Placebo	–0.004	.003	-.01	.002	.028	
Unemployed	18	Main effect	0.014***	.004	.007	.021	.03	48
Unemployed	19	Placebo	–0.003	.003	-.009	.002	.068	
Unemployed	19	Main effect	0.004	.004	-.004	.011	.066	

Notes: "Main effect" refers to the estimated β_{DiCCC} coefficients according to Equation (2) and they capture $\beta_{DiCCC} = \beta_{2012,2011} - \beta_{2011,2010}$. "Placebo" refers to the estimated $\beta_{DiCCCplacebo}$ placebo coefficients as in Equation (3) and capture $\beta_{DiCCCplacebo} = \beta_{2011,2010} - \beta_{2010,2009}$. Control variables: gender, parental education, Grade-8 math test scores, month of observation, month of birth, subregion fixed effects. 95% confidence intervals estimated by bootstrapping, , N=50. * p<0.10 , ** p<0.05, *** p<0.01. P-values are estimated based on the distribution of bootstrapped coefficients. While the effects of the reform on registered unemployment are significant at age 16, we do not report (NR) its per cent magnitude as it is irrealistically large due to the probability of unemployment being close to zero before the reform. $N = 13,141,574$ person-months. Mean: mean of the control group.

Appendix Table A11: The estimated DiCCC effects shown in Figure 4. Outcome: Inactive

Outcome	Age	Model	Beta	SE	95% CI Low	95% CI High	Mean	Significant effects in %
Inactive	15	Placebo	0.003*	.002	0	.007	.01	
Inactive	15	Main effect	–0.002	.002	-.007	.002	.01	
Inactive	16	Placebo	0.003*	.002	-.001	.007	.013	
Inactive	16	Main effect	0.013***	.003	.007	.018	.015	87
Inactive	17	Placebo	0.004	.003	-.001	.009	.018	
Inactive	17	Main effect	0.032***	.005	.022	.041	.021	150
Inactive	18	Placebo	0.000	.006	-.012	.012	.058	
Inactive	18	Main effect	0.017**	.007	.004	.031	.064	27
Inactive	19	Placebo	–0.005	.005	-.014	.004	.127	
Inactive	19	Main effect	–0.002	.005	-.013	.008	.136	

Notes: "Main effect" refers to the estimated β_{DiCCC} coefficients according to Equation (2) and they capture $\beta_{DiCCC} = \beta_{2012,2011} - \beta_{2011,2010}$. "Placebo" refers to the estimated $\beta_{DiCCCplacebo}$ placebo coefficients as in Equation (3) and capture $\beta_{DiCCCplacebo} = \beta_{2011,2010} - \beta_{2010,2009}$. Control variables: gender, parental education, Grade-8 math test scores, month of observation, month of birth, subregion fixed effects. 95% confidence intervals estimated by bootstrapping, N=50. * p<0.10 , ** p<0.05, *** p<0.01. P-values are estimated based on the distribution of bootstrapped coefficients. $N = 13,141,574$ person-months. Mean: mean of the control group.

Appendix Table A12: Employment Status of Dropouts Between Ages 16 and 18

(a) Any Employment (Number of Individuals)

	Observations	Percent
Never employed	25,976	58.35%
Employed at least once	18,539	41.65%
Total	44,515	100.00%

(b) Before and After Dropping Out (Number of Person-Months)

	Non-employed	Percent	Employed	Percent	Total
Before dropping out	1,043,395	97.1%	30,697	2.9%	1,074,092
After dropping out	427,326	88.0%	58,191	12.0%	485,517
Total	1,470,721	94.3%	88,888	5.7%	1,559,609

(c) Seasonality of Employment (Number of Person-Months)

Month	Before dropping out	After dropping out	Total
1	1,215	3,529	4,744
2	1,129	3,758	4,887
3	1,457	3,949	5,406
4	1,814	4,179	5,993
5	2,399	4,377	6,776
6	4,542	4,800	9,342
7	5,146	6,272	11,418
8	4,872	5,762	10,634
9	2,900	6,170	9,070
10	2,151	5,887	8,038
11	1,660	5,123	6,783
12	1,412	4,385	5,797
Total	30,697	58,191	88,888

(d) Occupations of Employed Dropouts (Number of Person-Months)

Occupation	Observations	Percent
Simple occupations requiring no qualifications	26 323	55.1%
Commercial and catering occupations	5 383	11.2%
Assembly workers	4 733	9.9%
Operators of processing machinery	3 629	7.6%
Metal and electrical trades	1 129	2.4%
Food industry occupations	973	2.0%
Services	822	1.72%
Construction occupations	673	1.4%
Agricultural occupations	507	1.1%
Other	3 580	7.51%
Total	47,752	100.0%

Notes: Table shows the employment outcomes of school dropouts who finished primary school before (2009, 2010, and 2011) and after (2012, 2013) the reform. Panel (a) shows the number of individuals who had any employment. Being employed at least once is defined as having at least one month of formal employment. Panel (b) shows the number of person-months of employment, both before and after dropping out. Panel (c) shows the distribution of the number of months spent in employment by calendar month. Panel (d) shows the distribution of occupations among those who are employed.