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The Labor Market and Fertility Impacts of Decreasing the Compulsory Schooling Age

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ABSTRACT

While an extensive literature investigates the effects of longer schooling, we know very little about what happens when compulsory schooling is shortened. This paper looks at the effects of a reform in Hungary that decreased the school leaving age from 18 to 16. We show that the reform increased the probability of being neither in education nor in employment and being inactive at ages 16-18 substantially while its effects on employment are not significantly different from zero in most specifications. These effects are similar among boys and girls but strongly heterogeneous by social background and ability. The reform had a moderate effect on teenage motherhood on average, but it increased the probability of giving birth substantially among the most disadvantaged girls. We conclude that through its heterogenous effects, the reform is expected to widen social inequalities.

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Keywords: *education, teenage fertility, school leaving age, public works, NEET, inactivity*

JEL: *I2, J13, J20*

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A kötelező iskolalátogatási korhatár csökkentésének munkapiaci és fertilitási hatásai

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ÖSSZEFOGLALÓ

Széles irodalom vizsgálja a kötelező iskolalátogatási korhatár növelésének hatásait. Nagyon keveset tudunk azonban arról, mi történik, ha egy országban csökkentik ezt a korhatárt. Tanulmányunk a kötelező iskolalátogatási korhatár 18 évről 16 évre csökkentésének hatásait vizsgálja Magyarországon. Megmutatjuk, hogy míg a reform megnövelte annak a valószínűségét, hogy a 16-18 éves fiatalok sem iskolába nem jártak, sem nem dolgoztak, addig a foglalkoztatás valószínűségét alig befolyásolta. Ezek a hatások fiúk és lányok között hasonlók, azonban a családi háttér és iskolai teszteredmények mentén eltérnek. A lemorzsolódás és az inaktivitás azok között nőtt meg leginkább, akik a legveszélyeztetettebbek voltak a lemorzsolódásra. Ezek között a fiatalok között drámaian nőtt a lemorzsolódás, miközben a reform a munkavállalás valószínűségét nem befolyásolta. Azaz, a közülük lemorzsolódott fiatalok vagy munkanélküliek, vagy inaktívak lettek. A reform növelte a tinédzserkori anyaság valószínűségét is, főként a leghátrányosabb helyzetű diákok között. A reform hatásai várhatóan tovább növelik a társadalmi egyenlőtlenségeket Magyarországon.

JEL Classification Codes: I2, J12, J20

Keywords: kötelező iskolalátogatási korhatár, lemorzsolódás, tinédzserkori anyaság, közmunka, oktatás, inaktivitás

1. Introduction

Over the last several decades, developed countries have gradually increased the compulsory schooling age (Brunello, Fort, and Weber 2009). In most cases, individuals moved by compulsory schooling policies to complete more years of education become more productive and earn higher wages (Angrist and Krueger 1991; C. Harmon and Walker 1995). In addition to the individual benefits of higher productivity and earnings, longer schooling has broader positive impacts on society in the form of productivity spillovers, but also through the fiscal externalities of improved health and health-related behaviors (Oreopoulos, 2007; Lleras-Muney, 2005; Clark and Royer, 2013; Barcellos, Carvalho and Turley, 2018; Fonseca, Michaud and Zheng, 2019), reduced crime rates (Lochner and Moretti, 2004; Hjalmarsson, Holmlund and Lindquist, 2011; Machin, Marie and Vujić, 2011; Costa and Machin, 2016) and teenage motherhood (Black et al. 2008; McCrary and Royer 2011; Cygan-Rehm and Maeder 2013; DeCicca and Krashinsky 2020; Adamecz-Völgyi and Scharle, 2020).

While its benefits are well-documented, compulsory schooling is a costly instrument and may not be productive for all students (Harmon, 2017). Compulsory schooling may prevent swift labor market entry by forcing some individuals to stay in school longer than what it would be optimal for them. For some parts of the population it may not give useful skills (Pischke and von Wachter, 2008) and in some cases, additional years of schooling may not result in additional qualifications (Grenet, 2013). If compulsory schooling indeed has limited effects on some groups, we would expect that reductions in the compulsory schooling age could have some benefits in the forms of increased employment for those who choose to leave school as a result. However, as governments rarely decrease the compulsory schooling age, we know very little about what happens when secondary schooling is shortened.

In this paper we fill this gap by examining the impacts of a reform that decreased the compulsory schooling age from 18 to 16 in Hungary in 2011. Using detailed administrative data and a difference-in-differences research design we examine education, labor market, and teenage fertility outcomes at ages 16-18.

We find that decreasing the compulsory schooling age had a significant negative impact on schooling. The probability of dropping out of school at ages 16-18 increased by 4 percentage points (78%) on average. We find that contrary to the stated goal of the policy reform of facilitating swift labor market entry, we find no effect on the probability of employment in most specifications. The reform increased the probability of being neither in school nor in employment (NEET) by 4 percentage points (65%), the probability of unemployment by 1 percentage points (74%) and the probability of inactivity by 3 percentage points (60%). Thus, most dropouts ended up either in unemployment or inactivity. On average, the reform had a small effect on the probability of teenage motherhood (0.6 percentage points, 21%) and had no effect on the cumulative number of abortions.

The effects of the policy are heterogeneous by family background and ability. The likelihood of dropping out of school increases by 12 percentage points (96%) among the children of parents with a primary school education, compared with 3 percentage points (65%) for those with a vocational education, 1 percentage points (45%) for those with a high school education, and 0.3 percentage points (30%) for those with at least some college. Children with lower test scores are also much more likely to drop out of school as a result of the policy. The most disadvantaged children see substantial increases in teenage motherhood (3-6 percentage points or 50-84%). Overall, these results suggest that decreasing the compulsory schooling age does not facilitate labor market entry but instead leaves students who drop out in a difficult position. This is particularly true for students from lower-income backgrounds, which means that lowering the compulsory schooling age strongly increases inequality.

Our work most directly contributes to the literature on the effects of compulsory schooling as mentioned above. Most of this literature finds positive effects on earnings (Angrist and Krueger 1991; Oreopoulos 2006; C. Harmon and Walker 1995; Stephens Jr. and Yang 2014), though there are some exceptions (Pischke and von Wachter 2008; Devereux and Hart 2010; Grenet 2013). The literature on compulsory schooling has also found non-pecuniary benefits (Oreopoulos and Salvanes 2011) as well as reduced mortality, better health and health-related behaviors (Oreopoulos, 2007; Lleras-Muney, 2005; Clark and Royer, 2013; Barcellos, Carvalho and Turley, 2018; Fonseca, Michaud and Zheng, 2019), reduced crime rates (Lochner and Moretti, 2004; Hjalmarsson, Holmlund and Lindquist, 2011; Machin, Marie and Vujić, 2011; Costa and Machin, 2016) and teenage motherhood (Black et al. 2008; McCrary and Royer 2011; Cygan-Rehm and Maeder 2013; DeCicca and Krashinsky 2020; Adamecz-Völgyi and Scharle, 2020).

Most of this literature studies increases in the compulsory schooling age. We know about only two instances when countries shortened schooling. Büttner and Thomsen (2015) note that most states in Germany have abolished the final year of academic high schools (secondary schools that constitute the traditional route to university) in the last decade while keeping their curriculum unchanged. Thus, they shortened academic high schools by one grade while they did not alter the length of compulsory schooling in other secondary school types. Büttner and Thomsen (2015) look at the causal effects of this reform in Saxony-Anhalt and find that it reduced math grades and delayed the university enrollment of women. Krashinsky (2014) looks at a similar reform in Canada where the fifth year of academic high schools was also abolished. He estimates that university students with one less year of high school showed significantly lower performance in all subjects than university students before the reform. These reforms, however, are special in that they applied for academic high school students only while the SLA in general is more likely to be binding for low SES, low ability students who would probably be more likely to attend some forms of vocational education. Thus, to the best of our knowledge, the Hungarian reform is a unique example of decreasing the SLA for all students.

The remainder of this paper proceeds as follows. In Section 2 we provide background on the reform we study. In Section 3 we introduce our data and in Section 4, the methods. In Section 5, we report our results. Section 6 concludes.

2. Institutional background, reform and identification strategy

For most students in Hungary, elementary school has eight grades (Grade 1-8, from ages 6/7 to 14/15) and secondary school has four (Grade 9-12, from ages 14/15 to 18/19).[1] Grade retention is possible, so some students are older than their classmates, might reach the SLA in primary school and would not enroll in secondary school. Most students (98%) completed primary school and 70% earned a secondary degree before the reform by age 20 (Source: Admin3).

Before the reform, compulsory schooling lasted until the end of the academic year in which one reached age 18. Starting from September 2012, the reform decreased compulsory schooling until the exact day when one reached age 16. The reform was introduced without a public ex-ante impact assessment, so we must rely on the news and informal government communication to grasp its rationale. First, a previous reform in 1996 that increased the SLA from 16 to 18 was not successful in decreasing the probability of dropping out and increasing the probability of earning a secondary degree (Adamecz-Völgyi 2021). Due to grade retentions, potential dropouts may be 2-4 years older than their peers in class. Hence, even if they did stay in school until age 18, they wouldn't necessarily have completed 12 grades and earned a degree by then. Second, the age 18 SLA put a huge burden on vocational secondary schools (that most potential dropouts attended) that did not have appropriate financial and human resources to support the academic development of unmotivated, low-ability, low-SES students. The reform intended to ease this burden as well as to directly reduce associated financial costs (and potentially, assumed negative peer effects). Third, according to government communication, the goal was "to let 16-18 years old kids who did not want to

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

stay in school to find employment", i.e., they wanted to increase the supply of blue-collar workers. Lastly, policymakers wanted to give a strong signal that redirects students from applying to four-grade academic high schools (that constitute the traditional route to university) towards shorter vocational schools to "ensure the supply of professionals" (as opposed to increasing the share of university graduates).

There have already been some attempts to investigate this reform. Hermann (2020) compares the probability of dropping out of school and the probability of earning a secondary degree between cohorts before and after the introduction of the reform using a similar empirical strategy and the same administrative data that this paper uses. He finds that at age 17-18, the share of dropouts was 5-7 percentage points (or almost two times) larger after the reform than it was before the reform. However, he also shows that these differences fade away at older ages and concludes that the reform probably did not decrease the probability of earning a secondary degree. Köllő and Sebők (2020) look at the yearly evolution of the share of 17-year-olds in employment or public works, and the share of 17-year-olds neither in education nor in employment (NEET) between 1992 and 2019. They find that both measures start to go up in 2012-2013: the share of NEET's almost doubles while the share of those in employment goes up from almost zero to 2% between 2011 and 2016. Lastly, Köllő and Sebők (2021) make a descriptive comparison between the share of 17-year-old Roma dropouts before the reform, in the 2011 Hungarian Census, and after the reform, in the 2016 Hungarian Microcensus. They show that the probability of being a dropout increased substantially among the Roma youth and they raise awareness about increasing ethnic inequalities as a result.

The first affected cohort included those who did not enroll in secondary school by September 2011. Those who enrolled in secondary school in Sept 2011 or before thus stayed under the old CSL age of 18 while those who either enrolled in secondary school in Sept 2012 or later, or did not ever enroll in secondary school, were exposed to the reform. The reform was accepted 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 some students (and parents) might have anticipated the decrease and their decision about secondary school enrollment might have been affected. Still, even though the decrease might have been excepted by some, the method of its enactment (and the first potential treated cohort) was not known by the public.

We define the treated and control cohorts based on when they finished primary school. By finished, we don't necessarily mean that they earned a primary degree, but this is the last year when we see them in primary school in the data. As it will be detailed in the next section, the available data allow us to compare five cohorts: those who finished primary school before the reform, between 2009 and 2011, and those who finished primary school after the reform, in 2012 and 2013[2]. Using these five cohorts and exploiting that the reform affected students at ages 16-18 but not at ages at 15 and 19, we set up a difference in differences (DiD) identification strategy to look at the causal effects of the reform. To do this, we have to assume parallel trends, i.e. that the trends of the outcome variables were the same before the reform. We investigate this assumption in Section 5.

3. Data

3.1. The Admin3 database

We use the Panel of Linked Administrative Data (Admin3) database, provided by the Databank of the Centre for Economic and Regional Studies (KRTK). The anonymized dataset links individual monthly data from the National Insurance Fund Administration, the Hungarian State Treasury, the Educational Authority, the Ministry of Finance, and the National Tax and Customs Administration, thus it covers information on educational outcomes and parental background, employment and healthcare (Sebők 2019) for a random 50% of the population born before 1 Jan 2003 (people with a Social Security Number in 2003). Labour market data are available from 2003 to 2017 while healthcare and education data are available from 2009 to 2017. As argued above, we use the subsample of those who finished primary school in 2009-2013 (referred to as cohorts in the rest of the paper). Table 1 summarizes the coverage of the estimation sample, grey cells indicate those affected by the reform. The data cover the 2009-2012 cohorts between ages 15 and 19 properly. However, as the database ends in 2017, it does not cover over half of students in the 2013 cohort at age 19. Furthermore, those who are 19 years old in 2017 are the relatively older students, as younger students would reach age 19 after 2017. Table A 1 in Appendix A shows that the missing students are among those who finished primary school at age 14, while those who finished at age 15-18 are fully covered by the data. This indicates that those in the data at age 19 in the 2013 cohort are the relatively "worse" students. Due to this selection, our main estimation sample includes the 2009-2012 cohorts at age 15-19 and the 2013 cohort at age 15-18. We provide robustness checks using various alternative subsamples in Section 5.

[2] Note that a further reform was implemented in 2013 that changed the curriculum of vocational training schools. About 20% of secondary school students attend vocational training schools. Hermann, Horn, and Tordai (2020) show that changing the curriculum of vocational training schools by decreasing the number of classes in general education subjects (math, literature and foreign languages) and replacing them by classes directly related to crafts decreased the human capital of students. We provide a robustness check to show that this reform is not affecting our results by excluding the 2013 cohort from the sample in Section 5.

Table 1: Estimation sample: ages covered by the sample by age and cohort

Year of completing primary school	Age of observation				
	15	16	17	18	19
2009	571,772	602,476	611,666	614,274	613,131
2010	605,189	614,908	619,076	618,950	618,097
2011	581,139	584,618	584,227	583,178	581,765
2012	569,737	569,212	568,030	566,599	548,682
2013	558,884	557,760	556,630	540,139	*262,016*
Total	2,886,721	2,928,974	2,939,629	2,923,140	2,623,691

Source: Admin 3. Time of observation: 2009-2017. Grey cells indicate the treated groups. Note that in 2013, the data do not cover more than half of students at age 19 because they would age 19 after 2017. Thus, we exclude them from the main estimation sample.

We restrict the estimation sample to those who were in school at age 15 for at least one month to exclude those (1) who are in the sample but have no school enrollment data and (2) who might have moved abroad before age 15. Furthermore, following REF, we exclude women who had a child by age 14. We assume that (1) the probability of having children by age 14 is not influenced by whether the SLA is 16 or 18 and (2) those having children do not go to school so the actual SLA legislation would not influence them.

From Admin3, we use data on age, sex, employment status, public works program participation, registered unemployment, child-related social transfers, school enrolment by type of school (primary, secondary), and earned degrees until Dec 2017. While the main observation period ends in 2017, there is an extra variable indicating whether one was in school in March 2019. Besides school enrolment, Admin3 also contains the results of national school tests (National Assessment of Basic Competences, NABC) taken in the Spring of grades 6, 8 and 10, as well as related survey data on family background (parental education). For some students, the NABC data is missing. Missingness in the NABC is not random, and it is correlated with social background and ability. 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 NABC: parental education and grade 6 math test scores[3]. Parental education is missing for 6% of the sample while grade 6 test scores are missing for 7.8%, and the share of missing values is not statistically significantly different among those who completed primary school before vs. after the reform (Table A 3 – A 5 in Appendix A). When we look at the heterogeneity of the effects of

[3] We do not use grade 8 or 10 test scores as they might have been affected by the reform. The evaluation of this question is beyond the scope of this paper.

the reform with respect to parental education and math test scores, we handle those with missing NABC data as separate groups keeping in mind that they are likely the most disadvantaged students.

Depending on data availability, we define the year of finishing primary school (referred to as cohort) as

- either the year when the Grade 8 test was completed for the last time, if this is available, or
- the year of completing the grade 10 test for the last time minus two years for those who have data on the grade 10 test but not on the grade 8 test and who did not repeat grades in secondary school, or
- the last year when they were enrolled in a primary school[4].

In terms of fertility outcomes, we observe all inpatient and outpatient care cases in public health institutions, along with International Statistical Classification of Diseases (ICD) codes to 2 numerical digits precision (for public healthcare providers). From the ICD codes, we identify birth-giving (O6-O8, Z37-Z38) and abortions (O04) (Table A 2 in Appendix A).

3.2. Outcome variables

We define the following outcome variables:

Dropping out: not being in school for at least 6 months and not earning a secondary school degree by the end of 2017 and not being back to school in March 2019. The first month after the last spell in school is the first month of the drop-out period. According to the data, most people would drop out at the end of the academic year (either in July or in September) even if, for example, they give birth during the academic year. Thus, it is likely that schools keep students enrolled administratively until the end of the academic year even if they do not go to school physically anymore. Also, at-home studying is possible, and the data do not differentiate between those who study at home and those who physically go to school. Thus, we probably do not observe the exact month of dropping out within an academic year.

[4] We also experimented with alternative definitions of the year of completing primary school including using the grade 8 test data only or the primary school enrollment data only and they all led to very similar results.

Probability of motherhood: whether one had her first child. Binary variable: 0 for those not having a child in a particular month yet. Once an individual has a child, it turns to 1 and stays 1 until the end of the observation period.

Number of abortions: the total number of abortions one had (cumulative).

Employment: whether one works in an open labor market position (private and public sector employment are included but public works is excluded).

Public works: whether one participates in a public works program.

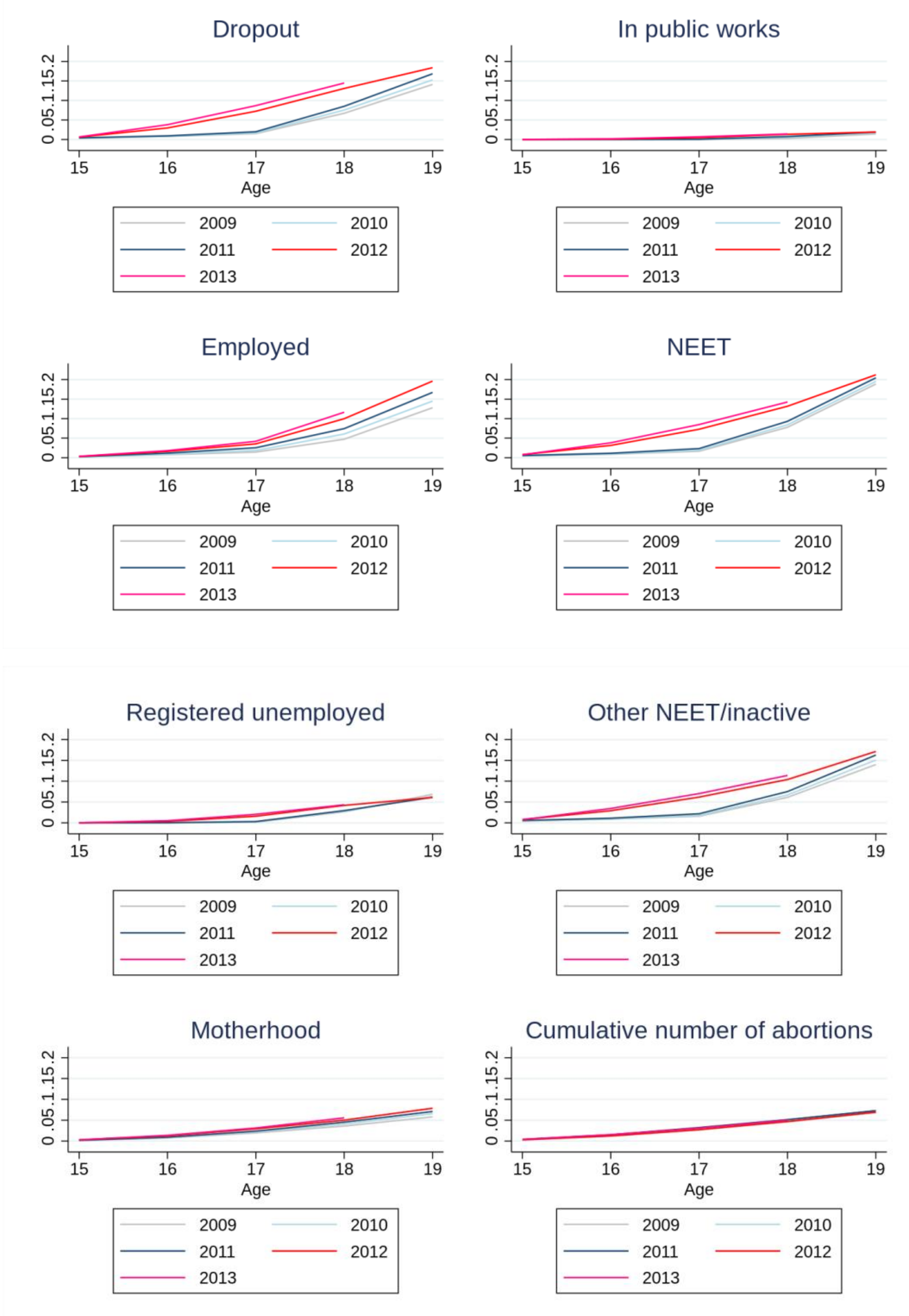
Registered unemployed: whether one is registered as unemployed.

NEET: whether one is neither in school nor in employment.

Other NEET/inactivity: whether one is neither in school/mother/in employment/in public works nor registered unemployed. These young people are considered "invisible" to the system. It is possible that they are inactive (who do not work and don't want to work), work in the shadow economy, are non-registered unemployed looking for work, or they might had migrated (or work) abroad. Theoretically, those living in Hungary are encouraged to at least register as unemployed to have free social security insurance (and thus get free healthcare), although those with no previous employment would not get any unemployment benefits. Non-working people (of all ages) not being visible to unemployment offices is a well-known problem of labor policy (REF). The problem is especially crucial for dropouts who never worked before and thus are not eligible for unemployment benefits (and have low incentives to register).

Descriptive statistics are reported in Table A 3 – A 5 in Appendix A, separately at age 15, 16-18 and 19. Figure 1 plots the average of the outcome variables (along with their 95% confidence intervals) by cohort and age. While there are only small differences between the three pre-reform cohorts (2009-2011), the probability of dropping out increased substantially between the 2011 and 2012 cohorts at ages 16-18. Reassuringly, the gap between the 2011 and 2012 cohorts is zero at age 15 and closing in at age 19. Furthermore, we also see large increases in the probability of being NEET and specifically, being Other NEET/inactive, especially at age 17. The average increase in the probability of employment is modest and registers mostly at age 18. There are no differences in the probability of motherhood and the number of abortions across the pre- and post-reform cohorts on average.

Figure 1: The outcome variables in the pre-reform (2009-2011) and post-reform (2012-2013) cohorts



Source: Admin3. All probabilities are plotted along with their 95% confidence intervals (without multiple testing). The lines indicate cohorts of students who were finishing primary school prior to the reform in 2010 and 2011 and after the reform in 2012 and 2013. The probability of motherhood and the cumulative number of abortions are plotted on the subsample of women (number of observations: 4,262,228) while all other outcome variables are plotted on the total sample (number of observations: 8,858,191).

4. Empirical methods and robustness checks

As mentioned above, we exploit heterogeneity in treatment status across individuals and over time based on whether

- individual i finished primary school in a pre-reform cohort (in 2009-2011) or in a ***post-reform cohort*** (in 2012 or 2013), and
- individual i is **aged between 16 and 18**, or aged either 15 or 19 at the time of observation, i.e., the reform could or could not have an impact on them.

Our DiD models are formalized as

$$outcome_{i,t} = \alpha + \beta_{DiD} * aged\ 16-18_{i,t} * post-refom\ cohort_i + \beta_2 * aged\ 16-18_{i,t} +$$

$$\beta_3 * post-reform\ cohort_i + \beta_4 * \mathrm{X_i} + \beta_5 * month_t + \beta_6 * \mathrm{t} + \mathrm{u_{i,t}},$$

(Equation 1)

where

$outcome_{i,t}$	is one of the outcome variables of individual i in month t;
$aged\ 16-18_{i,t}$	is a binary variables capturing whether one is aged between 16-18;
$post-refom\ cohort_i$	is a binary variable capturing whether completed primary school after the introduction of the reform;
$\mathrm{X_i}$	is a vector of time-independent individual characteristics (female, parental education, grade-6 math test score quintiles, month of birth FE);
$month_t$	is month of observation FE;
t	is a linear time trend; and
$u_{i,t}$	stands for a usual error term, clustered by year and month of birth.

Our parameter of interest is β_{DiD}, the estimated DiD coefficient on the interaction term of $aged\ 16-18_{i,t}$ and $post-refom\ cohort_i$. The DiD models rely on the assumptions of parallel trends, i.e., that the trends of the outcome variables were similar among those aged 16-18 vs 15 or 19 before the reform. Parallel trends are shown in Figure A 1 in Appendix A.

As robustness checks, we estimate several alternative specifications of Equation 1, including

- not having individual controls (Model R1);

- controlling for month of observation FE's instead of the linear time trend (Model R2);

- controlling for the age when young people finished primary school[5] (Model R3);

- controlling for cohort FE's (Model R4).

Furthermore, while for our main results, we use those who finished primary school in 2009-2012 at ages 15-19 and those who finished primary school in 2013 at ages 15-18, we also test alternative samples as

- those who finished primary school in 2009-2013 at ages 15-18 (i.e., using only those at age 15 as control group) (Model R5);
- those who finished primary school in 2009-2012 at ages 15-19 (i.e., dropping cohort 2013) (Model R6);
- those who finished primary school in 2011-2012 (right before and after the introduction of the reform) at ages 15-19 (Model R7); and
- those who finished primary school in 2010-2013 (2 years before and after the introduction of the reform) at ages 15-19 (Model R8).

Heterogenous effects

We re-estimate our main model within subsamples by (1) gender, (2) age of observation, (3) parental education and (4) grade 6 math test score quintiles. In our main specification, we estimate the effects of the reform on eight outcome variables and on several subsamples, testing altogether 134 parallel hypotheses on the treatment variable. Testing several statistical hypotheses together increases the probability of finding significant effects by chance, known as the problem of multiple inference (Anderson

[5] See average ages when finishing primary school by cohort in Figure A 2 in Appendix A.

2008) Thus, we correct all hypotheses tests by using the multiple testing procedure of Benjamini and Hochberg (1995).

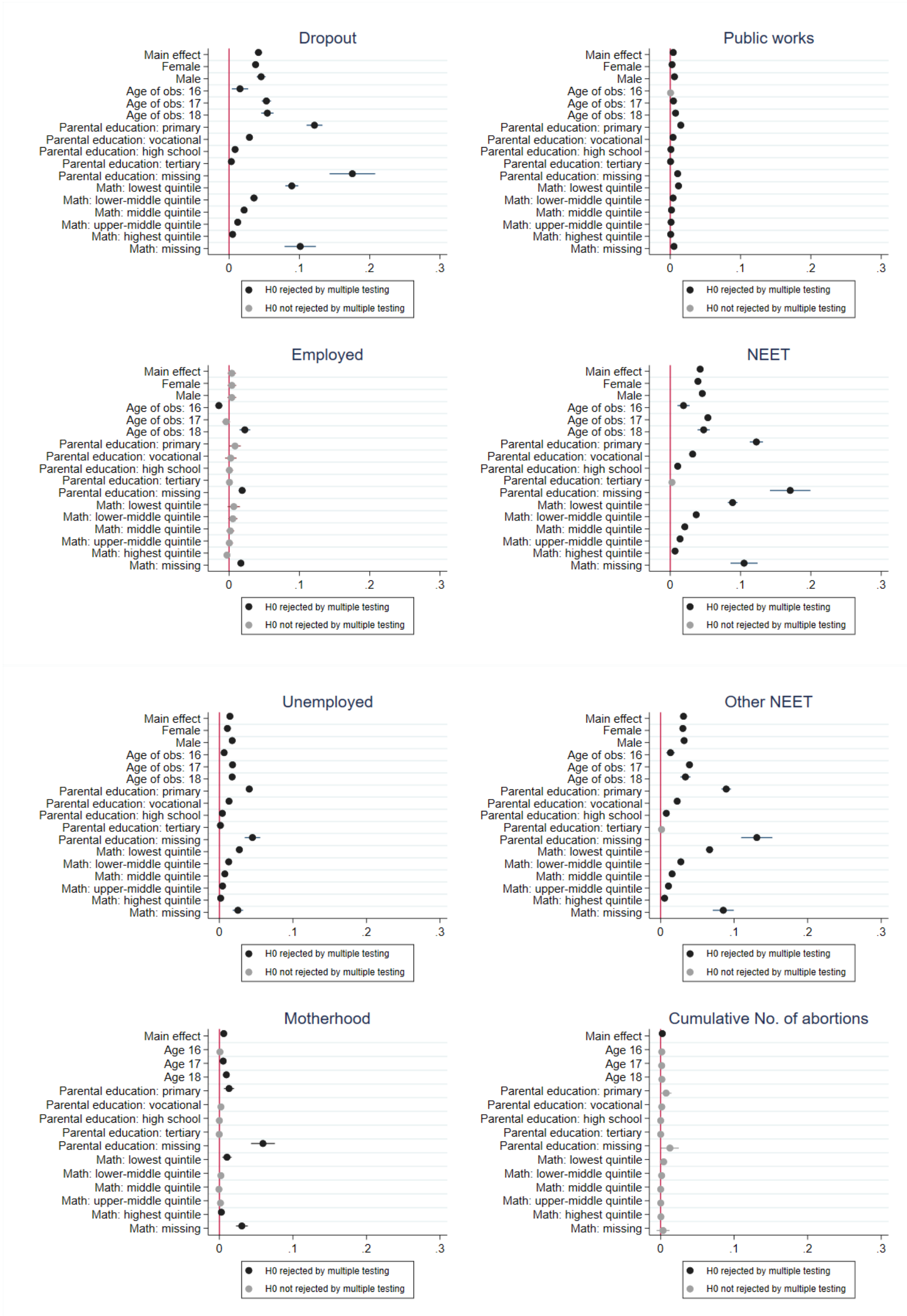
Lastly, we also estimate the effects of the reform by predicted dropout probability deciles. We build a predictive model using the data of the pre-reform cohorts and the same explanatory variables as listed above. Then, we use the estimated coefficients to make out-of-sample predictions on the data of the post-reform cohorts to predict their counterfactual dropout probability (that would have happened in the lack of the reform). We create deciles of these predicted probabilities of the pre- and post-reform cohorts and re-estimate our main model within these deciles. This method offers two advantages. First, it pools individual background characteristics to one index, and the deciles of this index create categories in the order of increasing dropout risk. Second, as we re-estimate the original models within these deciles, where treated and control individuals are matched based on their theoretical probability of dropping out, we practically combine statistical matching with diff-in-diffs.

5. Results

The "Main" models in Figure 2 shows the estimated average effects[6]. The reform significantly increased the probability of dropping out of the school system without a degree at ages 16-18 on average by 4 percentage points (78%), the probability of being NEET by 4 percentage points (65%), and the probability of being inactive by 3 percentage points (60%). The estimated average effect on the probability of employment is insignificant zero. These labor market effects are similar across men and women, and for most outcome variables, they are somewhat larger at age 17-18 than at age 16. At age 18, the reform increase the probability the probability of being NEET by 5 percentage points (181%) and the probability of employment by 2 percentage points (32%). In terms of parental education, there are even larger heterogeneities. The probability of dropping out increased the most in the lowest parental education groups: among those whose parents have at most a primary degree and among those who did not complete the family background survey of the NABC test. Compared to the average effect of 4 percentage points, the impacts are 2-4 times larger in these groups on the probability of being NEET (12-17 percentage points) and inactive (9-13 percentage points) while they are below 2 percentage points on the probability of employment. On the other hand, the effects are close to zero among those with high school or tertiary educated parents. Similarly, the effects are also relatively large among those who did poorly or have not even participated at the grade 6 national math test.

[6] The estimated coefficients are reported in Table A 6 in Appendix A.

Figure 2: Heterogeneous effects by gender, age, parental education and grade 6 math test scores (effect sizes)



Source: Admin3. Estimated coefficients are plotted along with their non-multiple testing corrected 95% confidence intervals. Multiple testing is conducted using the procedure of Benjamini and Hochberg (1995). No. of hypothesis tests taken together: 132. Effects on the probability of motherhood and the cumulative number of abortions are estimated on the subsample of women (number of observations: 4,262,228) while all other effects are estimated on the total sample (number of observations: 8,858,191). The estimated coefficients are reported in Table A 6 in Appendix A.

Among women, we find a 0.6 percentage point (21%) effect on average on the probability of motherhood and no effect on the cumulative number of abortions. However, among the most disadvantaged women, the reform increased the probability of motherhood by 3-6 percentage points (50-85%).

Looking at the heterogeneity of these results by predicted dropout probability deciles reveals that the effects sizes are positively correlated with the theoretical risk of dropping out (Figure B 2 in Appendix B). In the most at-risk group (the highest decile), the probability of dropping out increased by 18 percentage points, 4.5-times the average effect size. The effect on the probability of being NEET is of the same size, 18 percentage points, while the effect on employment is zero. Thus, among the most vulnerable students, all dropouts ended up in either unemployment or inactivity.

6. Discussion

This paper looked at the effects of decreasing the SLA from 18 to 16 on labor market outcomes and fertility at ages 16/18 in Hungary. In terms of the implementation of the reform, similarly to Hermann (2020), we find that the reform increased the probability of dropping out substantially. As opposed to policymakers' expectations, the reform barely had any effects on the probability of employment. Among the most at-risk students, employment effects are zero while the effects on being NEET are of the same size as the effects on dropping out. This result is intuitive, as teenagers with no work experience, without a secondary degree and low human capital are expected to have poor chances on the labor market. Furthermore, being a school dropout probably provides a strong negative signal for employers about abilities, expected productivity and non-cognitive skills that would also hinder labor market success.

Instead of increasing employment substantially, we find that the reform had large effects on the probability of being NEET, and more specifically, on being inactive. We find that these effects are similar among men and women, but they are substantially larger among those whose parents have at most a primary degree and whose grade 6 math test scores belong to the lowest quintile of the test score distribution. As these negative effects are heterogenous by social background, the reform will increase social inequalities and is expected to reduce intergenerational mobility.

We find a 21% effect on the probability of teenage motherhood and no effect on the cumulative number of abortions on average. However, among the most disadvantaged women, we find that the reform increased the probability of teenage motherhood by 50-84%, without any effects on the number of abortions. These results are in line with the earlier evidence that compulsory schooling affects the fertility outcomes of disadvantaged women more (Adamecz-Völgyi and Scharle, 2020). Furthermore, they also suggest that reform probably did not increase the probability of unwanted pregnancies but rather these women chose motherhood as an exit strategy.

Several caveats apply to our results. First, we see young people up until age 19 only in the data. In Hungary, some students would go back to school after dropping out and earn a secondary degree at age 21-22 or later. In our evaluation, we implicitly assume that the share of those who would go back to school after dropping out did not change due to the reform. However, if it did, depending on its direction, we might over- or underestimate the effect of the reform on the probability of dropping out. Second, we can only evaluate the contemporaneous effects of the reform at ages 16-18 but it is expected to have long-run human capital effects as well. The evaluation of these is left to the future.

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Appendix A

Table A 1: Estimation sample: No. of observations by the age and year of completing primary school

	Age of completing primary school				
	14	15	16	17	18
Year of completing primary school					
2009	678,113	2,027,865	238,909	52,731	15,701
2010	636,187	2,059,637	279,120	72,332	28,944
2011	589,938	1,934,877	276,776	76,874	36,462
2012	526,986	1,899,687	268,362	83,599	43,626
2013	387,070	1,682,521	290,725	79,164	35,949
Total	2,818,294	9,604,587	1,353,892	364,700	160,682

Source: Admin 3. Time of observation: 2009-2017. Grey cells indicate the treated groups. Note that in 2013, the data do not cover more than half of students at age 19 because they would age 19 after 2017.

Table A 2: ICD codes of delivery and abortions in Admin3

Delivery	O60	Preterm labour and delivery
Delivery	O61	Failed induction of labour
Delivery	O62	Abnormalities of forces of labour
Delivery	O63	Long labour
Delivery	O64	Obstructed labour due to malposition and malpresentation of fetus
Delivery	O65	Obstructed labour due to maternal pelvic abnormality
Delivery	O66	Other obstructed labour
Delivery	O67	Labour and delivery complicated by intrapartum haemorrhage, not elsewhere classified
Delivery	O68	Labour and delivery complicated by fetal stress [distress]
Delivery	O69	Labour and delivery complicated by umbilical cord complications
Delivery	O70	Perineal laceration during delivery
Delivery	O71	Other obstetric trauma
Delivery	O72	Postpartum haemorrhage
Delivery	O73	Retained placenta and membranes, without haemorrhage

Delivery	O74	Complications of anaesthesia during labour and delivery
Delivery	O75	Other complications of labour and delivery, not elsewhere classified
Delivery	O76	Abnormality in fetal heart rate and rhythm complicating labor and delivery
Delivery	O77	Other fetal stress complicating labor and delivery
Delivery	O80	Encounter for full-term uncomplicated delivery
Delivery	O82	Encounter for cesarean delivery without indication
Delivery	Z37	Outcome of delivery
Delivery	Z38	Liveborn infants according to place of birth and type of delivery
Abortion	O04	Induced termination of pregnancy

Table A 3: Descriptive statistics before and after the reform (age 15)

	Pre-reform (cohorts 2009-2011)	Post-reform (cohorts 2012-2013	Difference	Robust SE clustered by year and month of birth	t-test p-values
Total sample					
Female	0.49	0.484	-0.006	0.009	0.454
Year of birth	1995.138	1997.501	2.363	0.184	0
Month of birth	6.576	6.526	-0.05	0.721	0.944
Calendar month of observation	6.458	6.5	0.042	0.015	0.005
Age when completing primary school	14.893	15.006	0.114	0.085	0.182
Age of observation	15	15	0	0	
Month of observation (t)	97.78	126.04	28.26	2.127	0
Parental education					
Primary	0.14	0.15	0.011	0.007	0.13
Vocational	0.254	0.245	-0.009	0.003	0.004
High school	0.291	0.28	-0.011	0.008	0.153
Tertiary	0.233	0.249	0.015	0.009	0.08
Parental education is missing	0.082	0.076	-0.006	0.011	0.612
Grade 6 math test scores	1492.122	1489.97	-2.152	6.076	0.724
Grade 6 math missing	0.384	0.091	-0.293	0.061	0
Year of completing primary school (CPS)	2010.005	2012.495	2.49	0.13	0
Outcome variables (total)					
Dropout	0.004	0.006	0.003	0	0
Public works	0	0	0	0	0.316
Employment	0.002	0.003	0.001	0	0
NEET	0.005	0.008	0.003	0	0
Registered unemployed	0	0	0	0	0.981
Other NEET	0.005	0.008	0.003	0	0
No. of obs. (total sample)	2886721				
Outcomes of women					
Probability of motherhood	0.001	0.003	0.001	0	0
Cumulative number of abortions	0.003	0.003	0	0	0.283
No. of obs. (sample of women)	1407189				

Pre-reform: sample of those completing primary school in 2009-2011. Post-reform: sample of those completing primary school in 2012-2013. Source: Admin3. Level of observation: monthly at ages 15 (12 observations per individual).

Table A 4: Descriptive statistics before and after the reform (age 16-18)

	Pre-reform (cohorts 2009-2011)	Post-reform (cohorts 2012-2013	Difference	Robust SE clustered by year and month of birth	t-test p-values
Total sample					
Female	0.486	0.483	-0.003	0.009	0.745
Year of birth	1995.039	1997.472	2.433	0.184	0
Month of birth	6.571	6.537	-0.034	0.714	0.962
Calendar month of observation	6.495	6.522	0.027	0.01	0.006
Age when completing primary school	14.958	15.02	0.061	0.088	0.488
Age of observation	17.003	16.994	-0.009	0.002	0
Month of observation (t)	120.574	149.601	29.027	2.13	0
Parental education					
Primary	0.143	0.151	0.008	0.007	0.274
Vocational	0.251	0.246	-0.005	0.003	0.122
High school	0.284	0.28	-0.004	0.008	0.63
Tertiary	0.228	0.249	0.022	0.009	0.02
Parental education is missing	0.094	0.074	-0.02	0.012	0.104
Grade 6 math test scores	1490.91	1489.742	-1.168	6.25	0.852
Grade 6 math missing	0.4	0.089	-0.311	0.06	0
Year of completing primary school (CPS)	2009.986	2012.493	2.507	0.127	0
Outcome variables (total)					
Dropout	0.034	0.083	0.049	0.005	0
Public works	0.002	0.007	0.005	0	0
Employment	0.03	0.054	0.024	0.002	0
NEET	0.038	0.083	0.045	0.005	0
Registered unemployed	0.01	0.022	0.011	0.001	0
Other NEET	0.032	0.069	0.037	0.004	0
No. of obs. (total sample)	8791743				
Outcomes of women					
Probability of motherhood	0.023	0.032	0.008	0.003	0.004
Cumulative number of abortions	0.03	0.03	0	0.002	0.837
No. of obs. (sample of women)	4262318				

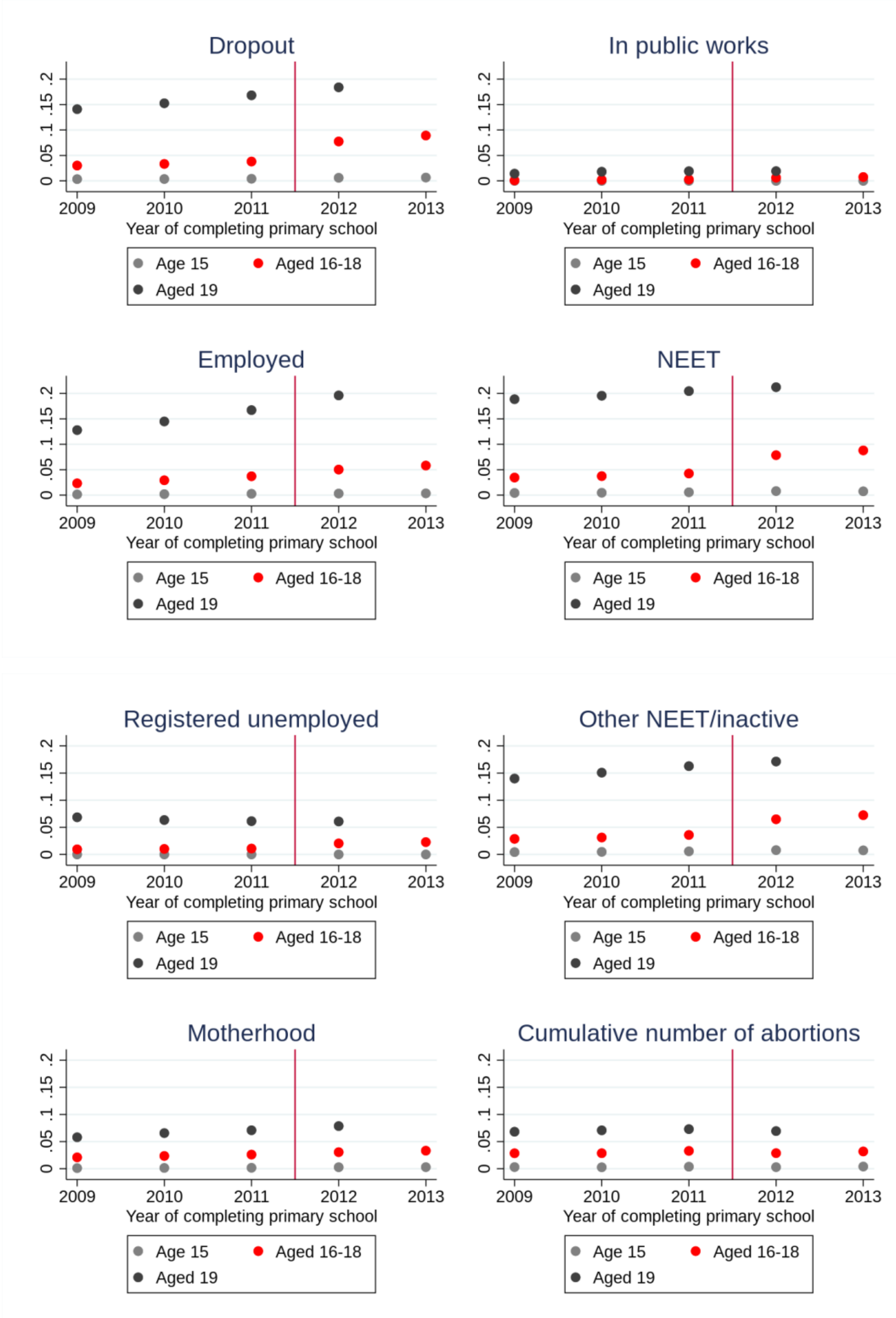
Pre-reform: sample of those completing primary school in 2009-2011. Post-reform: sample of those completing primary school in 2012-2013. Source: Admin3. Level of observation: monthly at ages 16-18 (36 observations per individual).

Table A 5: Descriptive statistics before and after the reform (age 19)

	Pre-reform (cohorts 2009-2011)	Post-reform (cohort 2012)	Difference	Robust SE clustered by year and month of birth	t-test p-values
Total sample					
Female	0.485	0.467	-0.019	0.009	0.042
Year of birth	1995.01	1997.146	2.136	0.178	0
Month of birth	6.568	6.482	-0.086	0.761	0.91
Calendar month of observation	6.502	7.098	0.596	0.181	0.001
Age when completing primary school	14.974	15.169	0.195	0.095	0.044
Age of observation	19	19	0	0	
Month of observation (t)	144.183	169.132	24.948	1.962	0
Parental education					
Primary	0.143	0.166	0.023	0.009	0.01
Vocational	0.251	0.244	-0.007	0.004	0.104
High school	0.284	0.268	-0.016	0.01	0.1
Tertiary	0.227	0.234	0.007	0.01	0.508
Parental education is missing	0.095	0.089	-0.006	0.014	0.653
Grade 6 math test scores	1490.783	1481.858	-8.925	7.459	0.234
Grade 6 math missing	0.401	0.103	-0.298	0.06	0
Year of completing primary school (CPS)	2009.983	2012.323	2.34	0.118	0
Outcome variables (total)					
Dropout	0.154	0.21	0.056	0.017	0.001
Public works	0.017	0.02	0.003	0.002	0.114
Employment	0.146	0.195	0.049	0.006	0
NEET	0.196	0.227	0.031	0.013	0.017
Registered unemployed	0.064	0.065	0.001	0.004	0.889
Other NEET	0.151	0.182	0.031	0.009	0.001
No. of obs. (total sample)	2623691				
Outcomes of women					
Probability of motherhood	0.065	0.093	0.028	0.009	0.002
Cumulative number of abortions	0.071	0.077	0.006	0.005	0.187
No. of obs. (sample of women)	1258401				

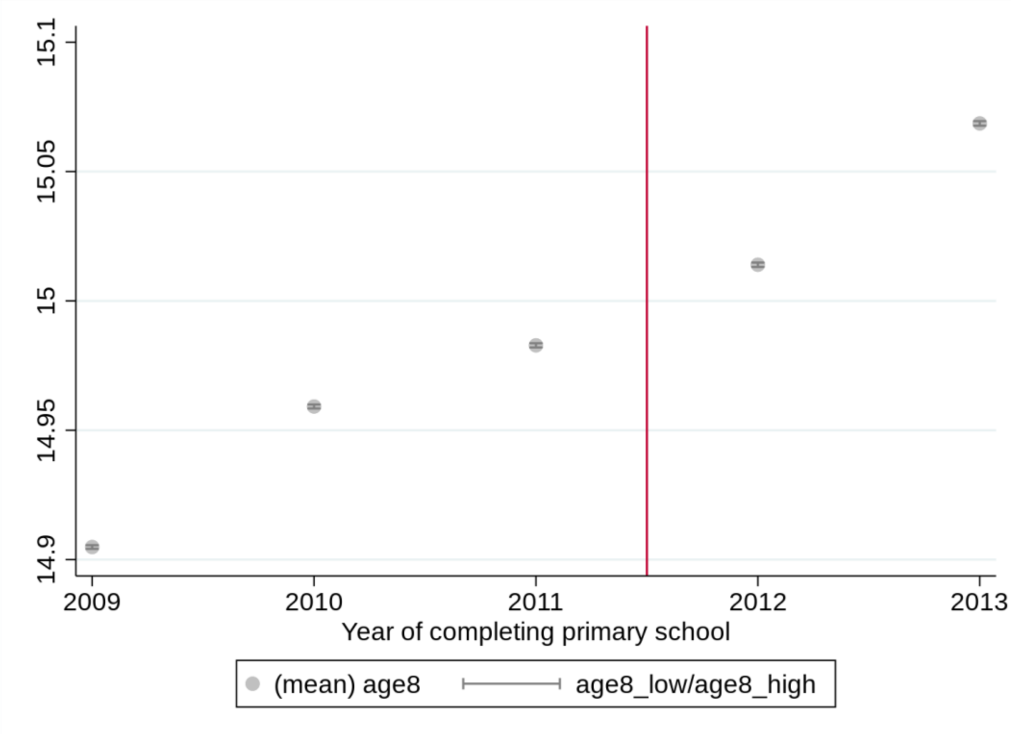
Pre-reform: sample of those completing primary school in 2009-2011. Post-reform: sample of those completing primary school in 2012. Source: Admin3. Level of observation: monthly at age 19 (12 observation per individual).

Figure A 1: Parallel trends: outcome variables before and after the reform



Source: Admin3. The probability of motherhood and the cumulative number of abortions are plotted on the subsample of women (number of observations: 4,262,228) while all other outcome variables are plotted on the total sample (number of observations: 8,858,191).

Figure A 2: Age when completing primary school before and after the reform



Source: Admin3. Number of observations: 8,858,191.

Table A 6: Heterogeneous effects by gender, age, parental education and grade 6 math test scores

Outcome	Sample	Beta	SE	95% CI low	95% CI high	Un-corrected p-values	Whether the multiple testing procedure rejects H0	Beta as % of the control mean
Dropout	Main	0.042	0.003	0.037	0.047	0.000	1	78
Dropout	Female	0.038	0.002	0.033	0.042	0.000	1	73
Dropout	Male	0.046	0.003	0.039	0.052	0.000	1	82
Dropout	Age 16	0.016	0.006	0.004	0.027	0.009	1	27
Dropout	Age 17	0.053	0.003	0.047	0.060	0.000	1	89
Dropout	Age 18	0.054	0.004	0.046	0.063	0.000	1	72
Dropout	PE: primary	0.122	0.006	0.110	0.133	0.000	1	96
Dropout	PE: vocational	0.029	0.002	0.025	0.033	0.000	1	65
Dropout	PE: high school	0.009	0.001	0.007	0.010	0.000	1	45
Dropout	PE: tertiary	0.003	0.001	0.002	0.004	0.000	1	30
Dropout	PE: missing	0.175	0.017	0.143	0.208	0.000	1	94
Dropout	Math: lowest quintile	0.089	0.005	0.080	0.098	0.000	1	83
Dropout	Math: lower-middle quintile	0.035	0.003	0.030	0.041	0.000	1	63
Dropout	Math: middle quintile	0.021	0.002	0.018	0.025	0.000	1	63
Dropout	Math: upper-middle quintile	0.012	0.001	0.010	0.015	0.000	1	62
Dropout	Math: highest quintile	0.005	0.001	0.004	0.007	0.000	1	42
Dropout	Math: missing	0.101	0.011	0.079	0.124	0.000	1	146
Public works	Main	0.004	0.001	0.003	0.005	0.000	1	89
Public works	Female	0.003	0.000	0.002	0.003	0.000	1	79
Public works	Male	0.006	0.001	0.004	0.008	0.000	1	94
Public works	Age 16	0.000	0.001	-0.001	0.002	0.573	0	
Public works	Age 17	0.005	0.001	0.003	0.006	0.000	1	76
Public works	Age 18	0.008	0.001	0.006	0.009	0.000	1	104
Public works	PE: primary	0.015	0.002	0.012	0.018	0.000	1	95
Public works	PE: vocational	0.004	0.000	0.003	0.005	0.000	1	103
Public works	PE: high school	0.001	0.000	0.001	0.001	0.000	1	69
Public works	PE: tertiary	0.000	0.000	0.000	0.001	0.006	1	52
Public works	PE: missing	0.011	0.002	0.006	0.015	0.000	1	91
Public works	Math: lowest quintile	0.012	0.001	0.009	0.014	0.000	1	97
Public works	Math: lower-middle quintile	0.004	0.001	0.003	0.005	0.000	1	77
Public works	Math: middle quintile	0.002	0.000	0.001	0.002	0.000	1	60
Public works	Math: upper-middle quintile	0.001	0.000	0.000	0.002	0.001	1	62
Public works	Math: highest quintile	0.001	0.000	0.000	0.001	0.013	1	56
Public works	Math: missing	0.005	0.002	0.002	0.009	0.003	1	99
Employed	Main	0.004	0.003	-0.002	0.010	0.184	0	
Employed	Female	0.004	0.003	-0.002	0.010	0.190	0	
Employed	Male	0.004	0.003	-0.002	0.010	0.214	0	
Employed	Age 16	-0.015	0.002	-0.019	-0.010	0.000	1	

Employed	Age 17	-0.004	0.003	-0.010	0.001	1.000	0	
Employed	Age 18	0.022	0.004	0.015	0.030	0.000	1	32
Employed	PE: primary	0.008	0.004	0.000	0.017	0.043	0	
Employed	PE: vocational	0.003	0.004	-0.006	0.011	0.556	0	
Employed	PE: high school	0.001	0.003	-0.005	0.006	0.850	0	
Employed	PE: tertiary	0.001	0.002	-0.003	0.005	0.770	0	
Employed	PE: missing	0.019	0.003	0.013	0.024	0.000	1	47
Employed	Math: lowest quintile	0.007	0.004	-0.002	0.015	0.130	0	
Employed	Math: lower-middle quintile	0.005	0.003	-0.001	0.012	0.106	0	
Employed	Math: middle quintile	0.002	0.003	-0.004	0.007	0.556	0	
Employed	Math: upper-middle quintile	0.000	0.003	-0.005	0.006	0.866	0	
Employed	Math: highest quintile	-0.003	0.002	-0.007	0.001	1.000	0	
Employed	Math: missing	0.017	0.002	0.012	0.021	0.000	1	41
NEET	Main	0.043	0.002	0.039	0.046	0.000	1	65
NEET	Female	0.039	0.002	0.036	0.043	0.000	1	59
NEET	Male	0.046	0.002	0.041	0.050	0.000	1	71
NEET	Age 16	0.019	0.004	0.010	0.027	0.000	1	26
NEET	Age 17	0.054	0.003	0.049	0.058	0.000	1	72
NEET	Age 18	0.047	0.004	0.039	0.056	0.000	1	52
NEET	PE: primary	0.122	0.005	0.113	0.132	0.000	1	95
NEET	PE: vocational	0.032	0.002	0.028	0.036	0.000	1	57
NEET	PE: high school	0.011	0.001	0.008	0.013	0.000	1	31
NEET	PE: tertiary	0.002	0.002	-0.001	0.006	0.162	0	
NEET	PE: missing	0.171	0.015	0.142	0.199	0.000	1	93
NEET	Math: lowest quintile	0.089	0.003	0.082	0.096	0.000	1	81
NEET	Math: lower-middle quintile	0.037	0.002	0.033	0.041	0.000	1	55
NEET	Math: middle quintile	0.021	0.002	0.016	0.025	0.000	1	43
NEET	Math: upper-middle quintile	0.014	0.001	0.011	0.016	0.000	1	39
NEET	Math: highest quintile	0.007	0.002	0.003	0.011	0.002	1	24
NEET	Math: missing	0.105	0.010	0.086	0.124	0.000	1	132
Unemployed	Main	0.014	0.001	0.012	0.016	0.000	1	74
Unemployed	Female	0.011	0.001	0.009	0.013	0.000	1	64
Unemployed	Male	0.017	0.001	0.015	0.020	0.000	1	81
Unemployed	Age 16	0.006	0.002	0.003	0.010	0.001	1	30
Unemployed	Age 17	0.018	0.001	0.015	0.021	0.000	1	82
Unemployed	Age 18	0.017	0.001	0.015	0.020	0.000	1	61
Unemployed	PE: primary	0.041	0.002	0.036	0.046	0.000	1	84
Unemployed	PE: vocational	0.013	0.001	0.011	0.015	0.000	1	68
Unemployed	PE: high school	0.004	0.001	0.003	0.005	0.000	1	46
Unemployed	PE: tertiary	0.001	0.000	0.001	0.002	0.000	1	33
Unemployed	PE: missing	0.045	0.005	0.034	0.056	0.000	1	100
Unemployed	Math: lowest quintile	0.027	0.002	0.024	0.031	0.000	1	72

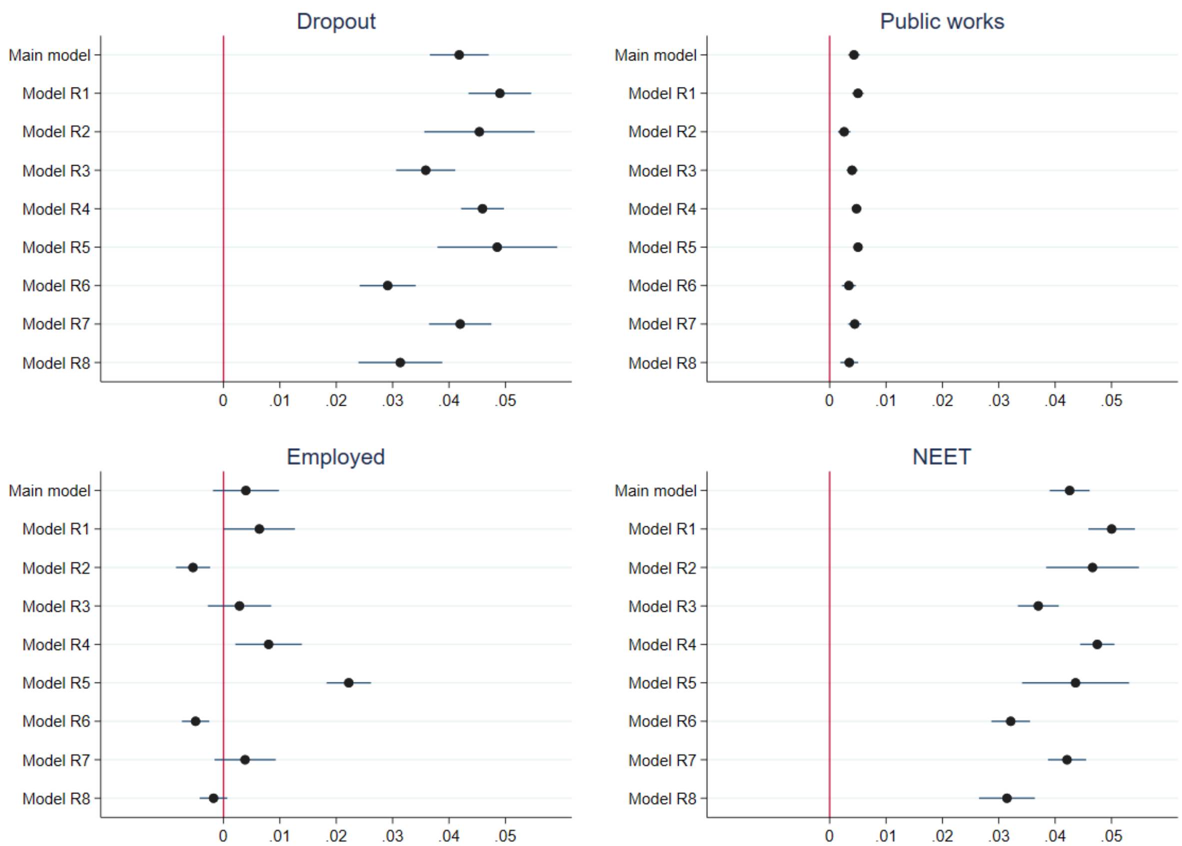
Unemployed	Math: lower-middle quintile	0.013	0.001	0.011	0.015	0.000	1	62
Unemployed	Math: middle quintile	0.007	0.001	0.006	0.009	0.000	1	57
Unemployed	Math: upper-middle quintile	0.004	0.001	0.003	0.006	0.000	1	52
Unemployed	Math: highest quintile	0.002	0.001	0.000	0.003	0.008	1	36
Unemployed	Math: missing	0.025	0.004	0.018	0.032	0.000	1	106
Other NEET	Main	0.031	0.001	0.028	0.034	0.000	1	60
Other NEET	Female	0.030	0.002	0.027	0.033	0.000	1	55
Other NEET	Male	0.032	0.001	0.029	0.035	0.000	1	64
Other NEET	Age 16	0.013	0.003	0.008	0.019	0.000	1	24
Other NEET	Age 17	0.039	0.002	0.035	0.043	0.000	1	66
Other NEET	Age 18	0.034	0.003	0.027	0.041	0.000	1	47
Other NEET	PE: primary	0.089	0.003	0.083	0.096	0.000	1	95
Other NEET	PE: vocational	0.023	0.002	0.020	0.026	0.000	1	52
Other NEET	PE: high school	0.008	0.001	0.005	0.010	0.000	1	27
Other NEET	PE: tertiary	0.001	0.002	-0.002	0.005	0.453	0	
Other NEET	PE: missing	0.131	0.011	0.110	0.152	0.000	1	88
Other NEET	Math: lowest quintile	0.067	0.003	0.062	0.072	0.000	1	79
Other NEET	Math: lower-middle quintile	0.028	0.002	0.024	0.031	0.000	1	51
Other NEET	Math: middle quintile	0.016	0.002	0.012	0.019	0.000	1	38
Other NEET	Math: upper-middle quintile	0.011	0.001	0.009	0.013	0.000	1	35
Other NEET	Math: highest quintile	0.005	0.002	0.002	0.009	0.004	1	21
Other NEET	Math: missing	0.085	0.007	0.071	0.100	0.000	1	137
Motherhood	Female	0.006	0.001	0.004	0.008	0.000	1	21
Motherhood	Age 16	0.001	0.002	-0.004	0.006	0.777	0	
Motherhood	Age 17	0.005	0.001	0.002	0.008	0.000	1	18
Motherhood	Age 18	0.009	0.002	0.006	0.013	0.000	1	28
Motherhood	PE: primary	0.013	0.004	0.006	0.020	0.000	1	15
Motherhood	PE: vocational	0.002	0.001	0.000	0.004	0.039	0	
Motherhood	PE: high school	0.000	0.001	-0.001	0.001	1.000	0	
Motherhood	PE: tertiary	0.000	0.000	-0.001	0.001	1.000	0	
Motherhood	PE: missing	0.059	0.008	0.043	0.075	0.000	1	50
Motherhood	Math: lowest quintile	0.010	0.003	0.004	0.017	0.001	1	16
Motherhood	Math: lower-middle quintile	0.002	0.002	-0.001	0.005	0.194	0	
Motherhood	Math: middle quintile	-0.001	0.001	-0.003	0.002	1.000	0	
Motherhood	Math: upper-middle quintile	0.001	0.001	0.000	0.003	0.128	0	
Motherhood	Math: highest quintile	0.003	0.001	0.001	0.004	0.003	1	89
Motherhood	Math: missing	0.031	0.004	0.023	0.039	0.000	1	84
Cumulative No. of abortions	Female	0.002	0.001	0.000	0.004	0.020	1	7
Cumulative No. of abortions	Age 16	0.002	0.002	-0.001	0.005	0.291	0	

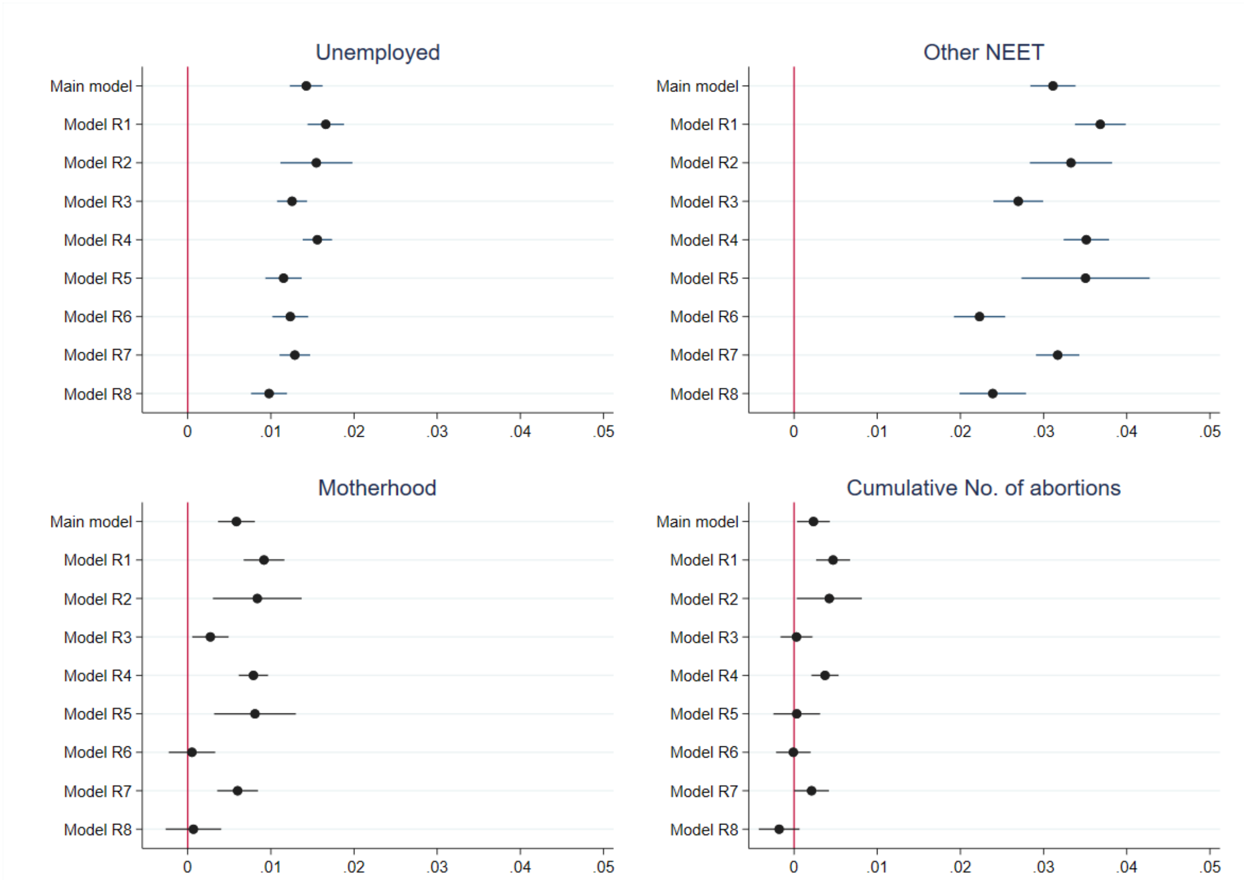
Cumulative No. of abortions	Age 17	0.001	0.001	-0.001	0.004	0.239	0
Cumulative No. of abortions	Age 18	0.002	0.001	-0.001	0.004	0.164	0
Cumulative No. of abortions	PE: primary	0.007	0.004	0.000	0.015	0.044	0
Cumulative No. of abortions	PE: vocational	0.002	0.001	-0.001	0.004	0.209	0
Cumulative No. of abortions	PE: high school	0.000	0.001	-0.002	0.002	0.925	0
Cumulative No. of abortions	PE: tertiary	0.000	0.001	-0.001	0.001	0.862	0
Cumulative No. of abortions	PE: missing	0.013	0.006	0.001	0.025	0.038	0
Cumulative No. of abortions	Math: lowest quintile	0.004	0.002	-0.001	0.009	0.081	0
Cumulative No. of abortions	Math: lower-middle quintile	0.001	0.002	-0.003	0.006	0.501	0
Cumulative No. of abortions	Math: middle quintile	0.000	0.002	-0.003	0.003	0.949	0
Cumulative No. of abortions	Math: upper-middle quintile	0.000	0.001	-0.002	0.003	0.889	0
Cumulative No. of abortions	Math: highest quintile	0.000	0.001	-0.002	0.003	0.664	0
Cumulative No. of abortions	Math: missing	0.003	0.004	-0.005	0.012	0.462	0

Source: Admin3. Multiple testing procedure of XXX. No. of hypothesis tests taken together: 132. In some subsamples, the control means were very low and thus the effect sizes (in absolute values) were extremely high. Thus, we do not report effect sizes in percent that are lower than the lowest decile or higher than the highest decile. The probability of motherhood and the cumulative number of abortions are plotted on the subsample of women (number of observations: 4,262,228) while all other outcome variables are plotted on the total sample (number of observations: 8,858,191).

Appendix B: Robustness checks

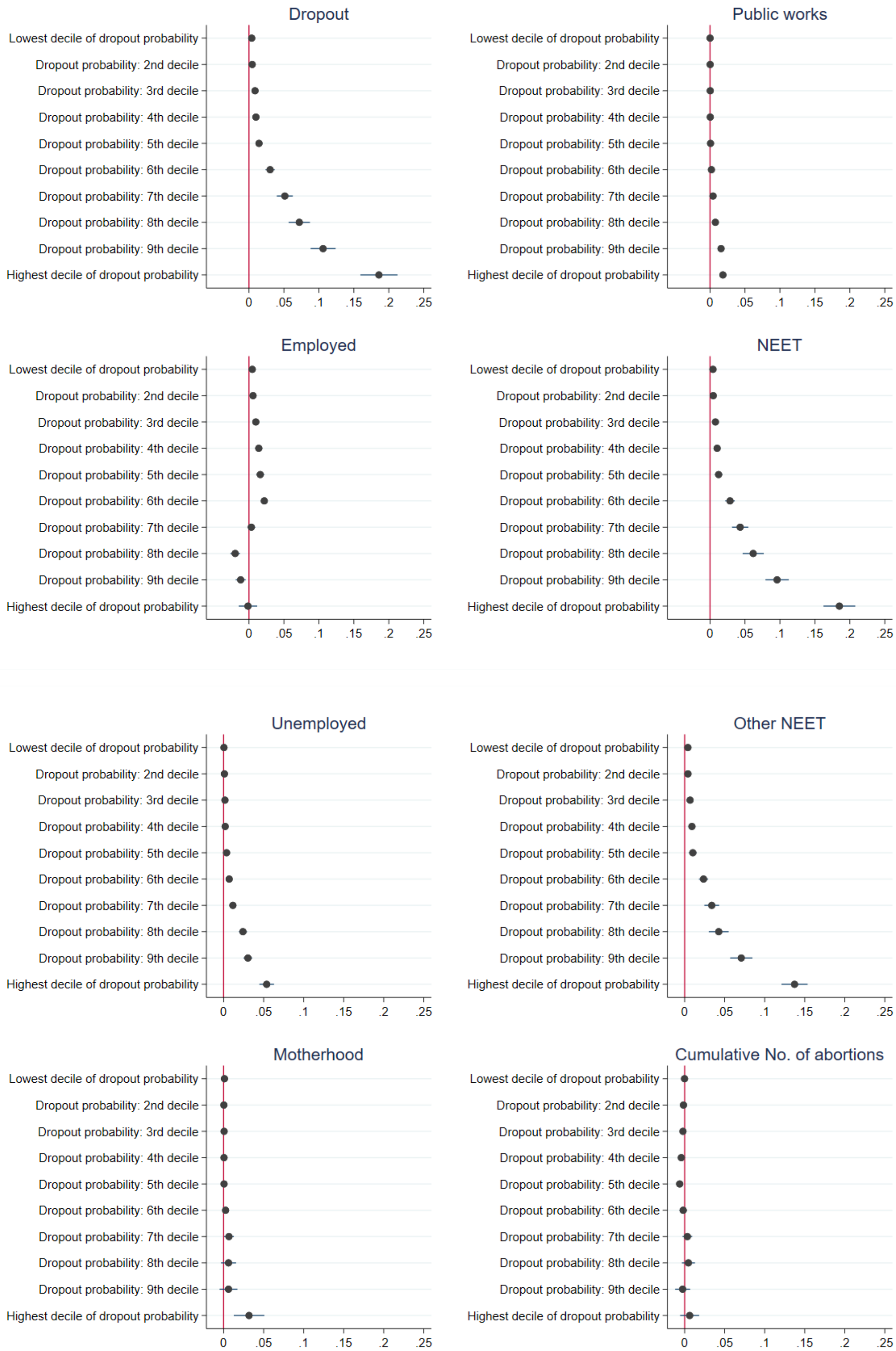
Figure B 1: Robustness checks (effect sizes)





Source: Admin3. Estimated coefficients are plotted along with their non-multiple testing corrected 95% confidence intervals. Robustness checks: not having individual controls (Model R1); controlling for month of observation FE's (Model R2); controlling for the age when young people finished primary school (Model R3); controlling for cohort FE's (Model R4); sample restricted to those who finished primary school in 2009-2013 at ages 15-18 (Model R5); sample restricted to those who finished primary school in 2009-2012 at ages 15-19 (Model R6); sample restricted to those who finished primary school in 2011-2012 at ages 15-19 (Model R7); sample restricted to those who finished primary school in 2010-2013 at ages 15-19 (Model R8).

Figure B 2: Heterogeneous effects by predicted dropout probability deciles (effect sizes)



Source: Admin3. Estimated coefficients are plotted along with their non-multiple testing corrected 95% confidence intervals.