Losing future entitlement to unemployment benefits. Impact on educational attainment

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Abstract

Providing income support to unemployed school-leavers reduces the returns to investments in education because it makes the consequences of unemployment less severe. We exploit in a difference-in-differences approach two policy reforms in Belgium to study whether conditioning future entitlement to unemployment benefits for school-leavers on age and educational attainment affects degree completion and dropout in secondary and higher education. The first reform disqualified labor market entrants over the age of 25 from benefits for which they were otherwise eligible if unemployed with little or no employment experience one year after leaving education. The second reform conditioned the eligibility for this unemployment benefit for youth below the age of 21 on the attainment of a high school degree. While we find evidence that the first reform significantly raised degree completion and reduced dropout in higher education, the second reform did not improve the graduation rate in high school.

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1. Introduction

Most developed countries provide some form of social protection for youths who enter the labor market upon leaving school. In most countries this consists in a means-tested welfare benefit, but in some others, unemployed school-leavers are entitled to unemployment insurance (UI) without means-test. In Australia and New Zealand UI is provided immediately upon registration as job seeker, but imposes very strict job search requirements (Langenbucher, 2015). In Belgium, Denmark and Luxembourg entitlement to UI is not only subject to (less strict) job search requirements, but also to a waiting period (OECD 2011).[2] There is an abundant literature that studies the negative effect of cash benefits on work incentives (see e.g. Tatsiramos and Van Ours, 2014 for a survey; Le Barbanchon, 2016 and Kolsrud et al., 2018 for more recent evidence). However, much less is known about the impact of such welfare and UI schemes on the investment in educational attainment. By providing support in case of unemployment these schemes reduce the returns to investment in education and, hence, may lower the long-run earnings capacity of youth (Kesselman, 1976; Moffitt, 2002). In this paper we study the effect on educational attainment of two reforms that restrict in Belgium the access to UI to which youth with little or no employment experience is entitled one year after leaving the education system. The first reform disqualified youths aged 25 or more for this UI. This predominantly affected students in college or university. The second reform conditioned the eligibility for UI on attaining a high school degree, but only for those below the age of 21. Hence, this second reform targeted at low educated youth.

Welfare generosity may have opposite effects on educational investments depending on whether one considers recipients or non-recipients of welfare benefits. For welfare recipients, reducing the level of benefits may reinforce liquidity constraints, which induces job search and acceptance rather than investment in human capital. By contrast, for non-welfare recipients, in particular for those in education, lower generosity incentivizes participation in schooling as, by enhancing earnings capacity, this reduces welfare dependence. To the best of our knowledge, Miller and Saunders (1997) are the first to examine the effect of welfare generosity on educational attainment. They studied this at the state level in the USA, but did not find any significant impact on high school completion. However, one may question whether the between-states-comparison of this early study provides convincing causal evidence. A more recent study of Cammeraat et al. (2017) reports that the introduction of a mandatory activation program for young welfare benefit recipients in the Netherlands did not significantly affect the enrollment in education for youths aged 25 and 26. However, these authors performed the analysis irrespectively of the welfare recipiency status of the youths. Their finding can therefore reflect the net effect of the aforementioned counteracting theoretical forces.[3]

Riddell and Riddell (2014) provide *indirect* evidence for the first mentioned prediction. They studied the impact of an activation program for welfare recipients on educational attainment. In particular, they analyzed the impact of a generous earnings supplement paid out to long-term welfare recipients in case

[2] Based on age and educational attainment, school-leavers in Luxembourg are eligible for unemployment benefits after a waiting period of six months (Luxembourg Employment Agency, 2019) and in Belgium after one year (Cockx and Van Belle, 2019). In Denmark, all school leavers who join an unemployment fund within two weeks after graduation can immediately obtain unemployment benefits. School-leavers who register after this two-week deadline, are paid out unemployment benefits only after one year (A-Kasser, 2019). Until 2007 Swedish high school graduates were entitled to unemployment benefits from age 20 onwards (von Buxhoeveden, 2019).

[3] The authors rather attribute the absence of the effect to the start of the Great Recession right before the reform.

they exited welfare for full-time employment. This study, based on a randomized controlled trial, found firm evidence that the supplement reduced the likelihood of educational upgrading at all levels, from high school completion to enrolment in higher education. This led to the conclusion that 'work-first' policies – which includes the lowering of benefit levels – reduce educational activity and may have adverse consequences on the long-run earnings capacity of welfare recipients. In the current study we cannot test whether this conclusion upholds, because we do not have data on educational activity of UI claimants. We rather focus on the effects of youths who are still in education.

The existing literature provides evidence that students are forward looking and take into account expected earnings when making study decisions (see e.g. Arcidiacono et al., 2012 and Beffy et al. 2012). While Kodde (1988) demonstrates that the effect of economic downturns on educational investments is indeterminate, other research has shown that recessions induce higher enrollment and degree completion in upper secondary education and lower dropout (see e.g. Clark, 2011; Tumino and Taylor, 2015; Reiling and Strom, 2015) while they accelerate time-to-degree in higher education (Messer and Wolter, 2010). Based on this evidence, it can therefore be expected that cutting future entitlement of social benefits also influences educational choices. More related to our context, Hernaes et al. (2017) and Bratsberg et al. (2018) provide evidence that imposing stricter eligibility requirements on social assistance enhances degree completion and decreases dropout in secondary education in Norway.[4] Hernaes et al. (2017) also find that these favorable effects persist by contributing to higher educational attainment, higher labor earnings, and lower transfer dependency at age 25, while Bratsberg et al. (2018) find that stricter conditionality reduces criminal offenses by young men.

While the last two studies evaluated imposing stricter eligibility criteria for welfare benefits immediately after labor market entry, we contribute to this literature by examining whether the loss of future eligibility for non-means-tested UI affects educational outcomes of students. By evaluating two reforms, we not only identify the effects on high school students, but also on those enrolled in higher education. Benefit eligibility does not apply immediately after the end of education, but only after a waiting period of one year. Finding behavioral reactions under such conditions would therefore be strong evidence of forward looking behavior. The behavioral economics literature suggests that higher educated individuals are less present biased than lower educated ones (Becker and Mulligan, 1997; Lavecchia et al. 2014). The fact that we find only significant behavioral reactions for students in higher education is exactly in line with this prediction.

Our analysis exploits two natural experiments induced by two distinct policy reforms in 2015. The first reform disqualified labor market entrants over the age of 25 from benefits - the so called "activation allowance" - for which they were otherwise eligible if unemployed with little or no employment experience one year after leaving education. This reform aimed in the first place at fostering work incentives, but, also, by weakening the safety net in case of long-term unemployment, *indirectly* strengthened incentives to complete education successfully. The second reform conditioned the eligibility for this unemployment benefit for youth below the age of 21 *directly* on the attainment of a high school degree. The stated objective of this reform was to address the extremely high unemployment rate of high

[4] The stricter activation requirements were implemented by local authorities and covered community service, participation in work or training programs, general work counseling, and active job search.

school dropouts not only by work incentives but also by financially incentivizing students to complete high school, an aim, which our study reveals not to be realized. [5]

To evaluate the impact of these policy reforms, we make use of population data of students enrolled in secondary and higher education before and after the reforms. Among the enrolled we can distinguish between those who drop out, those who complete a degree (irrespectively of pursuing education or not), and those pursuing education without degree completion.[6] Our identification strategy relies on a difference-in-differences model. It compares degree completion and dropout rates before and after the policy reform between students who, based on age or educational attainment, were at the margin of being affected or not by the reforms.

Our main findings can be summarized as follows. Losing eligibility to the activation allowance significantly raised the rate of degree completion in higher education on average by 2.0 percentage points, i.e. from 40.2% to 42.2%. Although the second reform made eligibility to the activation allowance conditional upon graduating from secondary education, we do not find an increase in the likelihood of high school graduation. The different findings for secondary and higher education can be explained as follows. First, the reforms had more severe consequences for students enrolled in higher education because their entitlement to the activation allowance was permanently withdrawn, while high school dropouts regain entitlement from age 21 onwards. Second, it has been shown that job seekers strongly underestimate how long they will remain unemployed (Spinnewijn, 2015), and behave according to time-inconsistent (hyperbolic) preferences (Della Vigna and Paserman, 2005). Moreover, as mentioned, lower educated youth has a higher tendency to focus on the present than higher educated (Becker and Mulligan, 1997; Lavecchia et al. 2014). Consequently, even if the risk of unemployment one year after school leaving is higher for the low educated than for those in higher education, their myopia implies that the reform does not affect their behavior.[7]

The remainder of this paper is organized as follows. Section 2 summarizes the institutional context and the UI reforms that may have affected the behavior of students in secondary and higher education. Section 3 discusses the methodology and expected effects of the reforms for the different treatment groups. Section 4 takes a first look at our dataset, describing degree completion and dropout before and after the policy reforms and section 5 presents the results. The last section provides some concluding discussion.

[5] In Flanders, the northern part of Belgium and focus of our analysis, 30% of high school dropouts was still unemployed one year after leaving school in 2017, while this was only 12% and 4% for students graduating from secondary and higher education respectively.

[6] This latter group is indirectly identified by subtracting the former mentioned groups from the total enrolled group of students. Because as a fraction of the number of enrolled students these groups add up to one, we consider only the degree completion and dropout rates as outcome variables in our analysis.

[7] One might argue that higher educated students are better informed about the reform than lower educated ones. However, this is not so obvious, because high schools usually provide specific information to last year students about their social rights, including their eligibility to UI. This is not the case for bachelor or master students. In a survey conducted among 1,000 students Vanneste (2016) reports that the fraction of students that was informed about the activation allowance was not substantially larger in higher than in secondary education: 53% against 43%.

2. Institutional context

Belgium is a federal state in which many competences have been decentralized. Generally, place-based matters are decentralized to the Regions and language-based matters to the Communities. Educational policy is decided upon at the level of the Communities. By contrast, the rules and payment of UI are determined at the federal level. In Belgium administrative data about education are only available for the Flemish Community. The analysis in this paper is therefore restricted to Flanders.[8] In this section we first explain the main institutional features of secondary and higher education in Flanders. Next, we clarify the pre-reform eligibility conditions for the activation allowance for school-leavers within the federal UI. Subsequently, we discuss the policy reforms of 2015 that will be evaluated in this study.

2.1. Secondary and higher education in Flanders

After finishing primary school, students enroll in secondary education, usually at the age of 12. After the second year, students are grouped into four different tracks according to ability and preferences.[9] The academic track provides students with a theoretical background and prepares for academic higher education. Programs in technical secondary education provide pupils with a theoretical and technical background to prepare them for professional higher education or the labor market. Programs in vocational secondary education prepare pupils for the labor market. A small fraction of students is enrolled in artistic secondary education that prepares for artistic programs in professional higher education. Because only few pupils are enrolled in the artistic track, we do not consider these students in our analysis.

Secondary education lasts in principle for six years. It is compulsory until age 18. Students can drop out of secondary education after their 18th birthday or after June 30 in the year they become 18 in case they have their birthday in the second half of the year. In 2015, 10.4% of young adults dropped out of secondary education without obtaining a degree (Vlaams Ministerie van Onderwijs en Vorming, 2018). If students do not perform well during secondary education, they can be required to repeat a grade or downgrade to a lower ranked track. Downgrading and grade repetition occurs frequently during secondary education. Many students start in the academic track and downgrade to the technical or vocational track (Cockx and Picchio, 2019). 24.3% of 15-year-old students have already repeated a grade in primary or secondary education in Flanders (De Witte and Hindriks, 2018).[10] This makes that only 72% of students successfully complete secondary education within 6 years of studying (De Groote and Declercq, 2018).

All high school graduates are entitled to start at almost all programs in higher education, regardless of their specific high school degree.[11] Pupils who graduate from vocational secondary education have to complete an additional year in school (7th year of vocational secondary education) before they are allowed

[8] Flanders is the Dutch-speaking part of Belgium, located in the North. About 60% of the population of 11 million inhabitants lives in Flanders. A small minority of the Dutch-speaking inhabitants live in Brussels. In the South and in Brussels French is the main language.

[9] At the start of the second year, students decide on elective courses that prepare for a particular track.

[10] This share of 15-year-old students that repeated a grade in primary or secondary education is even higher in the French speaking community (46.1%). The OECD average is 12%.

[11] The government imposes entry exams for only a very few programs: medicine at universities and some artistic programs at colleges.

to enroll in programs in higher education. When students make their enrollment decision, they can choose between professional and academic programs. Both professional and academic bachelor programs last for 3 years. Professional bachelors are practically oriented and prepare for the labor market. These programs are offered by colleges. Academic bachelor programs are offered by universities and are more theoretical than the professional ones. They prepare for the corresponding academic master, lasting one or two years depending on the study program. There is very strong ex post selection, especially after the first year of higher education, where many students drop out, switch to other majors or from academic to professional bachelor programs (Declercq and Verboven, 2018). Only 42% of the entrants in higher education in academic year 2005-2006 obtained their bachelor degree within the minimal duration of three years of studying. 29% of entrants did not complete a degree. In total, 72% of the enrolled students eventually obtained a bachelor degree. An academic year in secondary and higher education starts in September and ends officially in June.[12]

2.2. The activation allowance for youth

In Belgium, school-leavers with an employment record of less than one year are eligible for non-means-tested UI based on age and educational qualifications. This unemployment benefit is called the "activation allowance" and aims to support young people who just left education and entered the labor market. School-leavers are entitled to flat rate benefits, which depend on age and family status. In 2019, school-leavers who live on their own without dependents are paid out a monthly benefit of 541 euro under the age of 21 and of 911 euro above the age of 21.[13] In Belgium, most unemployed school-leavers still live with their parents. They claim a monthly allowance of 465 euro.[14] Since January 2012, a time limit was set on claiming the activation allowance. For non-heads of households with household income above a certain threshold the limit was set to three years, independently of the age. For all other school-leavers this time limit was set only from the age of 30 onwards.

School-leavers can claim the activation allowance after completing a waiting period that starts at the moment a school-leaver registers as a job seeker or starts working.[15] This waiting period lasts at least one year. Only periods during which one is available for the labor market count. The waiting period is therefore extended during periods of inactivity, such as sickness or resumed education. The activation allowance gives rise to an increasing profile of unemployment benefits. Such higher coverage for long-term than for short-term unemployed is justified on a principle of need. In order to be eligible for the activation allowance before 2015, school-leavers had to complete, but not necessarily pass the last high school year in the academic track. Students enrolled in other tracks (technical, artistic or vocational) were already eligible as soon as they completed the first three years of high school.

In case one is not entitled to the activation allowance one is still eligible for the regular UI and for welfare benefits. Individuals younger than 36 years are entitled to regular UI if they can prove a period of at least

[12] Secondary school starts on September 1, while higher education starts by the end of September. Students who do not succeed for all courses in secondary or higher education can be required to take retake exams in order to proceed to the next grade. These retake exams take place before the start of the next academic year.

[13] https://www.rva.be/nl/documentatie/baremas/inschakelingsuitkeringen.

[14] 84% of young people registering as a job-seeker between the age of 23-27 in 2008-2010 still lived with their parents (Cockx and Van Belle, 2019). We expect this share to be larger for younger school-leavers.

[15] There is one exception for school-leavers registering as a job seeker in July. These school-leavers can start their waiting period only from August 1.

one year of full-time employment. Welfare benefits are means-tested. Since the majority of the school-leavers still live with their parents, they generally do not qualify for these welfare benefits. Therefore, most school-leavers who lose the eligibility to the activation allowance, lose the full amount of the benefit. Those who do not, are subjected to the stigma of the means-test. Since the 2015 policy reforms suppressed the eligibility for the activation allowance of certain groups of school-leavers, they therefore imposed a non-negligible welfare cost on these groups. We can therefore expect substantial behavioral reactions by those who are immediately affected. However, it is less obvious that the reforms also change the behavior of students for whom the change in the eligibility rules matters only if they do not find stable employment within a year.

2.3. The policy reforms

On December 31 2014, the Belgian government signed an agreement to strengthen the conditions for claiming the activation allowance as from January 1 2015. The main aim of these reforms was to increase young people's work incentives. It is unlikely that these reforms have been anticipated before January 1, 2015. Even if the principle of the reforms was part of the government agreement of October 2014, there had been very little discussion about it in press before its implementation. Moreover, the timing came as a complete surprise at the end of government negotiations on December 31, 2014.

The government agreement involved two major reforms in the eligibility conditions in the activation allowance. Figure 1 provides an overview of the implied changes. First, as of January 1 2015, school-leavers aged 25 or older can no longer start claiming the activation allowance.[16] Before this date, this age threshold was 30. Because school-leavers have to be registered as job seeker for at least one year before they can claim the activation allowance, as of 2015 this registration has to occur before the age of 24 to safeguard benefit eligibility. At this age, many young people are still enrolled in higher education, but not in high school. The evaluation of this first reform only considers therefore the impact on the educational outcomes of students enrolled in higher education. Second, starting from September 2015,[17] high school drop-outs can no longer claim the activation allowance before the age of 21.[18] Before the reform, drop-outs from the academic track had to complete, but not necessarily pass the last high school year. Students enrolled in other tracks (technical, artistic or vocational), were already eligible as soon as they completed the first three years of high school.

[16] Once a claim has started, non-heads of household with an income above a certain threshold are entitled for maximum three years, so maximum until they are 28 years old; other job seekers are entitled maximum until the end of the month of their 33rd anniversary.

[17] We take into account that this reform has been anticipated as from the moment that the reform was decided, i.e. on December 31, 2014.

[18] Formally, after the reform, eligibility for the activation allowance is not conditional upon graduating from high school, but conditional upon successfully passing the sixth year of high school. This makes a difference for students in the vocational track, because, if they drop out after the sixth year, they do not formally have a high school degree, while they are nevertheless eligible. In the text we will ignore this subtle imprecision for simplicity.

Figure 1. The policy reform

Requirement (at the timing of claiming UI)	**Before the reform**	**After the reform**	**Implementation date**
Age	< 30	< 25	1 January 2015
Education	General track: Completed the 6th year of HS Other tracks: Completed the 3rd year of HS	< 21: Successfully completed HS ≥ 21: Same previous weaker requirement	1 September 2015

HS: High school

In order to affect study decisions, students should be informed about the policy reforms. As of January 1 2015, the reforms were systematically covered in the media. A survey by Vanneste (2016) among 1000 students in the final year of secondary or higher education in the Fall of 2016 (more than a year so after the reforms) finds that 47% of students (43% in secondary school and 53% in higher education) were informed about the existence of the activation allowance itself. Even if students were generally even less well informed about the specific eligibility conditions and the recent policy changes, it can be expected that students for whom the allowance is more relevant because of a higher unemployment risk, are better informed. Moreover, the survey was taken in the fall, while most technical and vocational high schools report that they organize information sessions about the activation allowance in the spring term towards the end of the academic year. We therefore can expect that the shares of informed students are higher than the ones measured by Vanneste (2016).

3. Methodology and expected effects

If students are forward looking, well informed and consider the possibility of not being entitled to the activation allowance in the future, the policy reforms may alter their behavior and therefore have influenced degree completion and dropout. To evaluate the impact of both reforms on educational attainment in secondary and higher education, we use a difference-in-differences approach and compare degree completion and dropout before and after the reforms between students who were affected by the reforms and students in a control group. This section first discusses the expected effects of the reforms for the different age groups and how this results in the different treatment and control groups. Subsequently, we discuss expected heterogeneity in the treatment effects and the difference-in-differences model used to evaluate the impact of the reforms.

3.1. Expected effects and treatment and control groups

The first policy reform affected *by surprise* students enrolled in higher education in the academic year 2014-2015. As of January 1 2015, these students were informed that they could no longer claim the activation allowance after the age of 25. Because of the waiting period of one year, this implied that enrolled students older than 24 suddenly lost their entitlement. In the analysis we consider only students

close to the critical age threshold of 24: students between the age of 22 and 24 on December 31 of the academic years retained for the analysis.[19] Since we only have access to population data which are grouped by calendar year of birth, we define the following two treatment groups and control group:

- Treatment group 1: students enrolled in higher education aged *24* on December 31
    - Members of cohorts who attain this age in 2014 or later are no longer entitled to the activation allowance.
- Treatment group 2: students enrolled in higher education aged *23* on December 31
    - Members of cohorts who attain this age in 2014 or later are only under some condition eligible for the activation allowance in the subsequent year. Students born before August 1 are eligible only if they do not pass the exams and register as job seeker before their 24th birthday.[20] Students born after July 31 are still eligible if they finish the academic year, as long as they register as job seeker before their 24th birthday.
- Control group: students enrolled in higher education aged *22* on December 31
    - Members of cohorts who attain this age in 2014 or later are not affected by the policy reform for at least one year.

To the extent that educational attainment reduces the likelihood of unemployment, the reform incentivizes students in the first treatment group to pass their exams and not to stop their studies prematurely. By behaving in this way, they avoid bearing the lost income induced by the reform.

We distinguish between two subgroups in the second treatment group: the older subgroup born before August 1 has similar incentives as the first treatment group, because its members can only remain eligible to the activation allowance if they decide to stop their studies before their 24th anniversary and, hence dropout. Since it is very unlikely that the value of the activation allowance outweighs the value of successfully passing the academic year, we argue that the educational incentives of this subgroup are similar as of those in the first treatment group. For the second, younger subgroup, born after August 1, the incentives to graduate are somewhat weaker, because these individuals remain entitled to the activation allowance if they register as job seeker before their 24th anniversary. However, they still have incentives to graduate, because if they don't, they face the enhanced cost of repeating the academic year, as they will no longer be entitled to the allowance at the end of the subsequent academic year. In summary, the second treatment group might have slightly lower incentives than the first treatment group to graduate, but incentives remain positive.

Finally, students in the control group are only affected by the reform in the subsequent academic year when this group obtains the features of the second treatment group. We therefore retain it as control group.

The second policy reform affected students enrolled in secondary education in academic year 2014-2015. From September 1, 2015 school-leavers who did not successfully complete secondary education are not eligible for the activation allowance before their 21st birthday. This reform therefore differently affected

[19] Including 25-year-old students as an additional treatment group does not affect our main results, but the parallel trends assumption does not always hold for this age group. Therefore, we do not retain it in the analysis reported in this paper. The reader can obtain the results including this age group upon request.

[20] Students who pass the exams cannot start the waiting period for the activation allowance before August 1.

students in the last year of compulsory secondary education according to their age.[21] To make treatment groups as similar as possible to control groups, we consider only students between the age of 18 and 21. Age is measured on December 31 of the academic years retained for the analysis.[22] We define the following treatment and control groups:

- Treatment group 1 (2): Students enrolled in secondary education aged 18 (19 and born after August 31) on December 31.
    - Members of cohorts who attain this age in 2014 or later cannot claim the activation allowance before their 21st birthday if they do not graduate from high school.
- Control group: Students enrolled in secondary education aged 19 on December 31 and born before September 1, or aged 20 on December 31
    - Members of cohorts who attain this age in 2014 or later remain eligible for the activation allowance even if they do not graduate from high school.

Students in the first two age groups are affected by the policy reform, but the intensity of the treatment differs between the two age groups. For the second treatment group, the impact of the reform is smaller than for the first one, because the entitlement to the activation allowance is delayed for at most 4 months, i.e. the maximum time that elapses between August 31 and the end of the calendar year. We therefore expect a smaller impact of the reform for the second treatment group.

3.2. Treatment effect heterogeneity

In the empirical analysis, we consider heterogeneity in the treatment effect along several dimensions. We expect that students enrolled in programs that prepare directly for the labor market are more informed about labor market policies and more likely to consider the consequences of these policies when making their educational choices. In higher education, we expect a larger effect of losing entitlement to UI for students in professional bachelor and master program. These programs prepare for the labor market, while academic bachelor programs have much less value on the labor market as they just prepare students for the subsequent master program. The same reasoning can be applied to the different tracks in secondary education. The academic track prepares for academic higher education. Programs in technical secondary education prepare pupils for professional higher education or the labor market while the aim of the vocational track is the access to the labor market. Therefore, we expect a larger effect of labor market policies on study decisions in vocational secondary education.

We also expect heterogeneity according to student background. First, we expect a more pronounced effect of losing entitlement to the activation allowance for students in low-income families. Most school-leavers are still living with their parents and thus supported by them in case they are unemployed. For low-income families, access to the activation allowance will be more important. Next, we expect a

[21] The policy reform could not only have affected students in the last year of secondary education, but also in previous years. However, we consider only students in the last year of secondary education in our analysis because being at the margin of obtaining a high school degree the incentives induced by the reform are stronger for this group than for the other groups.

[22] This means that we consider only students who had to repeat at least one grade during primary or secondary education. Students who are 17 years old on December 31 and enrolled in the last year of secondary education are also affected by the policy reform. Including 17-year-old students as an additional treatment group does not affect our main results, but the parallel trends assumption does not always hold for this age group. Therefore, we do not retain this treatment group in the analysis reported in this paper. The reader can obtain the results including this age group upon request.

different treatment effect between Belgian and non-Belgian students in higher education, and between native and non-native students in secondary education. Estimating the model on the sample of non-Belgian students in higher education can be seen as a placebo test. Most foreign students will return to their home country after their studies. For foreign students the reforms are not relevant and we therefore do not expect an effect for these students. In secondary education, we distinguish between students speaking Dutch (the official language of the region of Flanders) and another language at home. A priori, it is not clear which group we expect to be most affected by the policy reform. On the one hand, we expect that students who speak Dutch at home are more informed about the policy changes and therefore more likely to consider the loss of entitlement to the activation allowance during their studies. On the other hand, families who speak Dutch at home are on average of higher socio-economic status. Consequently, losing access to the activation allowance has less severe consequences for them.

3.3. Difference-in-differences model

To estimate the causal impact of the reforms on dropout and degree completion in secondary and higher education, we make use of the difference-in-differences approach. We contrast the evolution of degree completion and dropout between students who were affected by one of the reforms, and students who were not affected by this reform. Let Y_{ijt} denote the outcome of interest of student i in treatment group j at the end of period t, with t=2011, 2012, 2013 for the control period and t=2014 for the treatment period (with t referring to the calendar year at the start of the corresponding academic year). The probability that this outcome occurs can be estimated by a linear probability model with the following structure:

$$Y_{ijt} = \alpha + \sum_{t=2011}^{2014} \beta_t T_t + \gamma_j D_{ij} + \delta_j D_{ij} T_{2014} + \theta X_i + \varepsilon_{ijt},$$

with T_t an indicator for the time period in which the outcome is measured, $D_{ij} = 1$ if individual i belongs to treatment group j and X_i a vector of personal characteristics affecting degree completion and dropout in secondary or higher education. δ_j is the difference-in-differences estimator and reveals the causal impact of the reform for treatment group j. δ_j measures how much degree completion or dropout of treated students increased or decreased after the reform in treatment group j relative to students who were not affected by the reform.

The difference-in-differences approach allows for trends in dropout and degree completion over time. In order to identify the causal effect of the reform, the trends in outcomes of students in treatment and control groups should evolve parallel in absence of the reform. This implies that there can be no shocks differently affecting students of different age groups. We assess this parallel trends assumption in two ways. First we perform a graphical assessment of the common trends assumption by plotting the counterfactual outcome of the treatment group in the absence of treatment.[23] The parallel trends

[23] The counterfactual outcome of the treatment group in absence of the policy reform and its 95% confidence interval are predicted as follows. We estimate a difference-in-differences model with interaction effects between the treatment group and time dummies for the pre- and post-reform periods. We predict the counterfactual outcome by setting these interaction effects to zero. The counterfactual outcome is shifted to the level of the observed outcome of the treated in the year before the policy reform. By construction, the confidence interval is zero for this year (the reference one). We take the standard errors of the

assumption is rejected if the observed outcome of the treatment group falls outside the 95% confidence interval of the counterfactual outcome in the pre-reform period. Second, we perform a placebo analysis and estimate a similar regression with interaction effects between the treatment groups and year dummies. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period.

4. Data

To examine the impact of both policy reforms on degree completion and dropout, we make use of administrative grouped population data of secondary and higher education in Flanders before and after the policy reforms.[24] We include three years before the policy reform (academic year 2011-2012 until 2013-2014). As from academic year 2011-2012, all students enrolled in higher education aged 24 or younger started their studies after the introduction of flexible study programs in Flanders in 2005. Including previous years in the analysis can bias the estimates because older students had started their studies before this reform. For secondary education, there were no major policy reforms, but the parallel trends assumption holds in fewer cases if we include more years in the pre-reform period. We include only one year in the post-reform period because the policy reform could have affected school enrollment and the choice of study program. This could lead to a different composition of students in the treatment and control group over time and violate the difference-in-differences identifying assumptions. Enrollment in the first year after the reform cannot have been affected because the reform was announced only after students made their enrollment decisions in 2014.

The data are grouped by academic year, age, gender, study program, nationality, and socio-economic status.[25] Socio-economic status is measured by an indicator equal to one if a student received a study grant in secondary or higher education and zero otherwise. The study grant variable is our proxy for household income because only low-income families are entitled to scholarships. For each specific group, we observe the number of enrolled students, the number of students that obtain a degree at the end of the academic year, and the number of dropouts.[26] The enrolled students who do not obtain a degree and neither dropout pursue education either because they repeat their academic year or because they pass to the next grade as they were not enrolled in the graduation year. This last option applies only to students in higher education. In secondary education, the data contain only students in their graduation year. For the analysis we transform these grouped data into individual data of size equal to the number of enrolled students within each group and by constructing two dichotomous discrete outcome variables, one which is equal to one if a degree is obtained and zero otherwise; the other which is equal to one if the individual

interaction effects to construct a 95% confidence interval around the counterfactual outcome of the treated. The confidence interval is then equal to the confidence interval of the (placebo) treatment effects.

[24] For secondary education, we observe only students enrolled in full-time secondary education. We do not observe students enrolled in part-time vocational education who combine studies with an apprenticeship. For higher education, we observe only students enrolled in full-time programs that lead to a professional bachelor, academic bachelor or master degree. We do not observe students in other types of programs or international students spending part of their program in Belgium.

[25] For students enrolled in secondary education, we do not observe nationality but rather whether or not they speak a foreign language (different from Dutch) at home.

[26] Due to privacy issues, we do not observe enrollment, degree completion and dropout if the number of enrolled students is smaller than five.

drops out and zero otherwise. This allows us to write the evaluation problem as linear probability models (see Section 3.3).

Table 1 presents student characteristics and the outcome variables (degree completion and dropout) for students enrolled in higher education (professional bachelor, academic bachelor and master programs) according to treatment status and period. Treatment status is defined on the basis of age on December 31 of the respective academic year. The treatment group consists of all 23 and 24-year-old students (treatment groups 1 and 2 as defined in the previous section). The control group consists of all students who are 22 years old on December 31. The treatment period refers to academic year 2014/15, while the retained pre-treatment periods are 2011/12 until 2013/14. The first two columns show the means for the student' characteristics in the control and the treatment groups before the policy reform. The last two columns show the difference relative to the pre-reform period. We test for a different change in the composition of the treatment and control groups over time.[27]

Table 1 shows that our sample includes slightly more women than men. Most students have the Belgian nationality, especially in the control group. Students in the treatment group are older than students in the control group and thus more likely to be enrolled in a master program. Master programs attract a larger share of international students because these programs are more likely to be offered in English. Approximately 20% of students in the control and treatment group obtain a study grant during higher education which is our proxy for household income. Differences in the composition of the treatment and control group between the pre- and post-reform period are small and the change over time in the treatment group is not significantly different from the change in the control group. Most students are enrolled in master programs. This is caused by the fact that we consider only 22 to 24-year-old students. At this age, students should already have completed their bachelor program if they did not incur any delay in their educational career. However, as mentioned in Section 2, study delay is common in education in Flanders (Declercq and Verboven, 2018). Some students also complete more than one bachelor program. This explains why a significant number of 24-year-old students are still enrolled in bachelor studies. After the policy reform, the share of students enrolled in master programs in the control group decreased by 2.1 percentage points (from 43.6% to 41.5%), while the share of students enrolled in master programs in the treatment group decreased by 0.9 percentage points (from 48.7% to 47.8%). The decrease is significantly larger (at a level of only 5%) in the control group than in the treatment group.

The following rows of Table 1 show degree completion and dropout for students in the treatment and control group. Before the policy reform, degree completion is slightly lower in the treatment group (41.2% versus 43.3% in the control group). After the policy reform, degree completion in the control group decreased by 1.1 percentage points (from 43.3% to 42.2%), while the share of students obtaining a degree in the treatment group increased by 1.0 percentage points (from 41.2% to 42.2%). After the policy reform, fewer students from the treatment group drop out without a degree (a decrease from 13.4% to 12.6%). Dropout in the control group is similar before and after the policy reform. Both the variation over time in degree completion and dropout in the treatment group is significantly different from the variation over time in the control group. These figures provide a first indication that losing eligibility to the activation allowance has raised degree completion and reduced dropout in higher education.

[27] To test for differences in the composition between the treatment and control groups over time, we estimate simple difference-in-differences models and regress each explanatory variable on a dummy for the treatment group, a dummy for the post-reform period and the interaction effect between both (See Section 3.3).

Table 1. Descriptive statistics: higher education

	Pre-reform 2011/12 – 2013/14		Post-reform 2014/15	
	Control	Treated	Δ Control	Δ Treatment
Explanatory variables				
Male	48.0%	49.5%	-0.5%	-0.1%
Belgian	94.1%	88.3%	-0.4%	-0.7%
Study grant HE	21.0%	20.2%	-0.2%	-0.2%
Prof. bachelor	38.2%	34.5%	+2.9%	+2.2%
Acad. Bachelor	18.2%	16.8%	-0.8%	-1.3%
Master	43.6%	48.7%	-2.1%	-0.9%**
Outcome variables				
Diploma	43.3%	41.2%	-1.1%	+1.0%***
Dropout	9.2%	13.4%	0.1%	-0.8%***
Observations	80213	76989	28754	29222

Note: Student characteristics in treatment and control groups before and after the policy reform. Outcomes in the post-reform period are expressed as percentage point changes relative to the outcome in the pre-reform period. Treatment group = 23 and 24 year-old students. Control group = 22 year-old students. Age is measured on December 31 of the respective academic year. Explanatory variables include gender, indicators for Belgian nationality, whether a student obtained a study grant, and type of program (professional or academic bachelor or master). Outcome variables are degree completion and dropout at the end of the year. * p<0.10, ** p<0.05, *** p<0.01 indicate whether the change in the treatment group significantly differs from the change in the control group.

Table 2 shows descriptive statistics for the treatment and control groups in secondary education. Treatment status is defined on the basis of age on December 31 of the academic year for students enrolled in the last year of secondary education. Students in the treatment group are 18 years old, or 19 and born after August 31 (respectively treatment group 1 and 2 as defined in the previous section). Students in the control group are 20 years old or 19 and born before August 31. If students are on time they graduate from secondary education in the year they become 18. So we consider only students who accumulated at least one year of study delay during primary or secondary education. But as grade retention is common in Flanders, the population retained for the analysis is still large.

Among the older students in the control group, we observe a larger fraction of boys (62.9% of the students in the control group versus 56.9% of the students in the treatment group). The larger share of boys in the control group can be explained by the assignment of treatment status on the basis of age. In secondary education, boys are more likely than girls to repeat a grade. In the treatment group, relatively more students speak Dutch at home than in the control group (the official language of the region of Flanders), and fewer students obtain a study grant (our proxy for socio-economic status). The decrease in the share of boys and native students after the reform is significantly larger (at a level of only 5%) in the control group. Among the control group, fewer students are enrolled in the general track. This can be explained by the fact that students in the last year of technical or vocational secondary education are more likely to have repeated at least 2 years. In the regression model, we control for personal characteristics and choice of study program in order to account for the fact that the populations sampled may differ systematically during the pre- and post-reform periods.

Table 2. Descriptive statistics (6th year of secondary education)

	Pre-reform 2011/12 – 2013/14		Post-reform 2014/15	
	Control	Treated	Δ Control	Δ Treatment
Panel A: Last year of compulsory secondary education				
Explanatory variables				
Male	62.9%	56.9%	-2.8%	-0.4%**
Dutch	71.0%	87.3%	-3.9%	-2.3%**
Study grant SE	39.9%	30.2%	+1.8%	+2.3%
General SE	9.3%	19.4%	-0.9%	-1.3%
Technical SE	43.3%	40.6%	-0.6%	-0.4%
Vocational SE	47.4%	40.0%	+1.5%	+1.8%
Outcome variables				
Diploma	82.9%	90.1%	+1.1%	+0.4%
Dropout	9.6%	4.2%	-1.3%	+0.1%***
Observations	11108	50524	6913	34202

Note: Student characteristics in treatment and control groups before and after the policy reform. Outcomes in the post-reform period are expressed as percentage point changes relative to the outcome in the pre-reform period. Treatment group = 18 year-old students and 19 year old students born after August 31. Control group = 20 year-old students and 19 year-old students born before September 1. Age is measured on December 31 of the respective academic year. Explanatory variables include gender, indicators for speaking Dutch at home, whether a student obtained a study grant, and track choice in the final year of secondary education (general, technical or vocational). Outcome variables are degree completion and dropout at the end of the year. * p<0.10, ** p<0.05, *** p<0.01 indicate whether the change in the treatment group significantly differs from the change in the control group.

Next, we present the outcome variables. Before the policy reform, 82.9% and 90.1% of students in the control and treatment group obtain a degree at the end of the academic year. After the policy reform, these shares slightly increase to respectively 84.0% and 90.5%. After the policy reform, a slightly higher share of students from the treatment group drop out without a degree (an increase from 4.2% to 4.3%) while there is a reverse and significant, trend in the control group. The remaining group of students who do not obtain a degree at the end of the academic year and do not stop their studies re-enroll in the last year of secondary education in the next year.

5. Empirical results

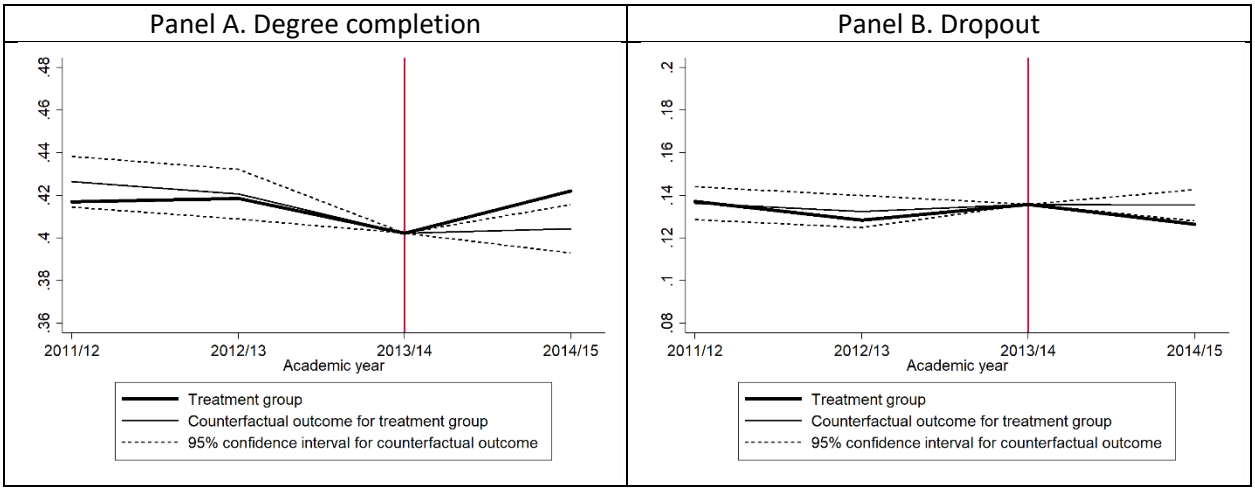
In this section, we discuss the empirical results. We start by evaluating the first policy reform and investigate whether this reform affected degree completion and dropout in higher education. For the benchmark model, we present both a graphical and corresponding econometric analysis of the difference-in-differences model. Next, we evaluate the second policy reform that affected students in secondary education.

5.1. Higher education

The first policy reform could have affected study decisions in higher education because as from January 2015, it was not possible anymore to claim the activation allowance after the age of 25. Figure 3 compares degree completion and dropout for students enrolled in higher education in the treatment and control group. The vertical line is drawn at the last year of the control period. Treated students are 23 or 24 years old (treatment group 1 and 2 as defined in Section 3 for higher education). Given that we expect that the policy reform affected degree completion and dropout of both treatment groups in the same direction, we group these students in one treatment group. In the regression analysis, we distinguish between both treatment groups. Students in the control group are 22 years old. The thick solid line shows degree completion and dropout for students in the treatment groups. Approximately 41% of students in the treatment group obtain a degree in the year before the policy reform and 13% drop out without a degree. The thin solid line presents the counterfactual outcome of the treatment group in the absence of the policy reform surrounded by its 95% confidence interval.

Comparing the observed outcomes of the treated students (thick solid line) with their counterfactual outcome in absence of the treatment (thin solid line) provides a first assessment of the parallel trends assumption. From both graphs, we can see that the observed outcome of the treatment group remains within the 95% confidence interval of the counterfactual outcome of the treatment group before the policy reform. This seems to suggest that the parallel trends assumption is not rejected for both outcome variables. In the estimation of the difference-in-differences models we formally test for parallel trends. After the policy reform, degree completion in the treatment group increases and exceeds the 95% confidence interval of the counterfactual outcome of the treated. This seems to indicate that the policy reform increased degree completion in the treatment group. A reversed pattern can be observed for unsuccessful dropout in higher education in panel B. Before the policy reform, dropout follows a similar trend in the treatment and control group. After the policy reform, dropout is lower in the treatment group.

Figure 3. Degree completion and dropout in higher education



Note: Treatment group = 23 and 24 year-old students. Control group = 22 year-old students. Age is measured on December 31 of the respective academic year. The vertical line is drawn at the last period before the reform. The thick solid line shows the observed outcome of the treatment group. The thin solid line shows the counterfactual outcome of the treatment group in absence of the treatment. The thin dotted lines are the 95% confidence intervals of the counterfactual path.

Figure A1 in appendix shows degree completion and dropout for students enrolled in professional bachelor programs (panel A), academic bachelor programs (panel B), and master programs (panel C). The positive effect on degree completion is found in all graduate programs, while the negative effect on dropout is found only for professional bachelor programs.

Table 3. Degree completion in higher education

VARIABLES	(1) All students	(2) Acad. Bachelor	(3) Prof. Bachelor	(4) Master
Panel A: Baseline model				
Treatment effect	0.020***	0.024**	0.016**	0.022***
	(0.005)	(0.011)	(0.007)	(0.007)
Counterfactual outcome	0.402	0.355	0.330	0.473
Parallel trends: p-value	0.213	0.985	0.454	0.210
Observations	215178	37097	79705	98376
Panel B: Different effects according to age				
Age 23				
Treatment effect	0.016***	0.022*	0.012	0.018**
	(0.005)	(0.013)	(0.009)	(0.008)
Counterfactual outcome	0.400	0.368	0.345	0.456
Age 24				
Treatment effect	0.026***	0.030*	0.024**	0.028***
	(0.006)	(0.016)	(0.010)	(0.009)
Counterfactual outcome	0.406	0.333	0.304	0.501
Similar effects for both groups: p-value	0.165	0.644	0.266	0.318
Parallel trends Age 23: p-value	0.167	0.939	0.493	0.140
Parallel trends Age 24: p-value	0.635	0.942	0.510	0.660
Observations	215178	37097	79705	98376

Note: Treatment group = 23 and 24 year-old students. Control group = 22 year-old students. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 until 2013-2014. Treatment period = 2014-2015. The regressions control for gender, socio-economic status, nationality and include time dummies. The regression in column 1 additionally controls for the type of study program. The counterfactual outcome is the predicted outcome for the treated in absence of the treatment in the post-reform period. To test for parallel trends, we estimate similar regressions with interaction effects between the treatment groups and year dummies. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value of the test for parallel trends is reported for each specification. The p-value of the test for similar effects according to age is reported in panel B. Heteroskedasticity robust standard errors in parentheses.
* p<0.10, ** p<0.05, *** p<0.01.

Table 3 presents the output of the regression model for degree completion. We report only the estimated treatment effects and the counterfactual outcome of the treated in absence of the policy reform as predicted by our model. The p-values of the placebo tests on the parallel trends assumption are stated and show that the parallel trends assumption is never rejected. The first panel reports the results of the baseline model in which we combine both treatment groups. The first column shows the output on the full sample of students enrolled in higher education. Degree completion of the treated students has increased significantly by 2 percentage points after the policy reform relative to degree completion in the control group. In absence of the treatment, 40.2% of students in the treatment group would have obtained a degree at the end of the academic year. The policy reform raises it to 42.2%, which corresponds

to a proportional increase of 5%. The next three regressions show that degree completion has significantly increased for academic bachelor programs, professional bachelor programs and master programs. According to Panel B of Table 3, the effect on degree completion is higher for the oldest age group when considering the whole sample, but the difference between the two age groups is not significant. The incentives to graduate are somewhat weaker for the younger age group, because these individuals remain entitled to the activation allowance if they register as job seeker before their 24th anniversary.

Table 4 shows that the policy reform significantly reduced the dropout rate in higher education by 0.7 percentage points (- 5.3% in relative term). In absence of the policy reform, 13.3% of all treated students would have dropped out at the end of the academic year without a degree. The decrease in dropout is only significant for professional bachelor programs. Finally, panel B shows that dropout decreased relatively more for the oldest treatment group.

Table 4: Dropout in higher education

VARIABLES	(1) All students	(2) Acad. Bachelor	(3) Prof. Bachelor	(4) Master
Panel A: Baseline model				
Treatment effect	-0.007**	-0.007	-0.015***	0.001
	(0.003)	(0.009)	(0.006)	(0.003)
Counterfactual outcome	0.133	0.182	0.199	0.067
Parallel trends: p-value	0.389	0.733	0.460	0.124
Observations	215178	37097	79705	98376
Panel B: Different effects according to age				
Age 23				
Treatment effect	-0.003	-0.002	-0.011*	0.004
	(0.003)	(0.010)	(0.007)	(0.003)
Counterfactual outcome	0.120	0.164	0.184	0.054
Age 24				
Treatment effect	-0.014***	-0.017	-0.023***	-0.006
	(0.004)	(0.013)	(0.009)	(0.005)
Counterfactual outcome	0.156	0.215	0.226	0.088
Similar effects for both groups: p-value	0.017	0.304	0.192	0.058
Parallel trends Age 23: p-value	0.301	0.685	0.111	0.140
Parallel trends Age 24: p-value	0.702	0.868	0.869	0.660
Observations	215178	37097	79705	98376

Note: Treatment group = 23 and 24 year-old students. Control group = 22 year-old students. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 until 2013-2014. Treatment period = 2014-2015 until 2016-2017. The regressions control for gender, socio-economic status, nationality and include time dummies. The regression in column 1 additionally controls for the type of study program. The counterfactual outcome is the predicted outcome for the treated in absence of the treatment in the post-reform period. To test for parallel trends, we estimate similar regressions with interaction effects between the treatment groups and year dummies. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value of the test for parallel trends is reported for each specification. The p-value of the test for similar effects according to age is reported in panel B. Heteroskedasticity robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A1 and A2 in appendix analyses the heterogeneity in the treatment effects according to gender, household income and nationality. The parallel trends assumption is rejected for degree completion for

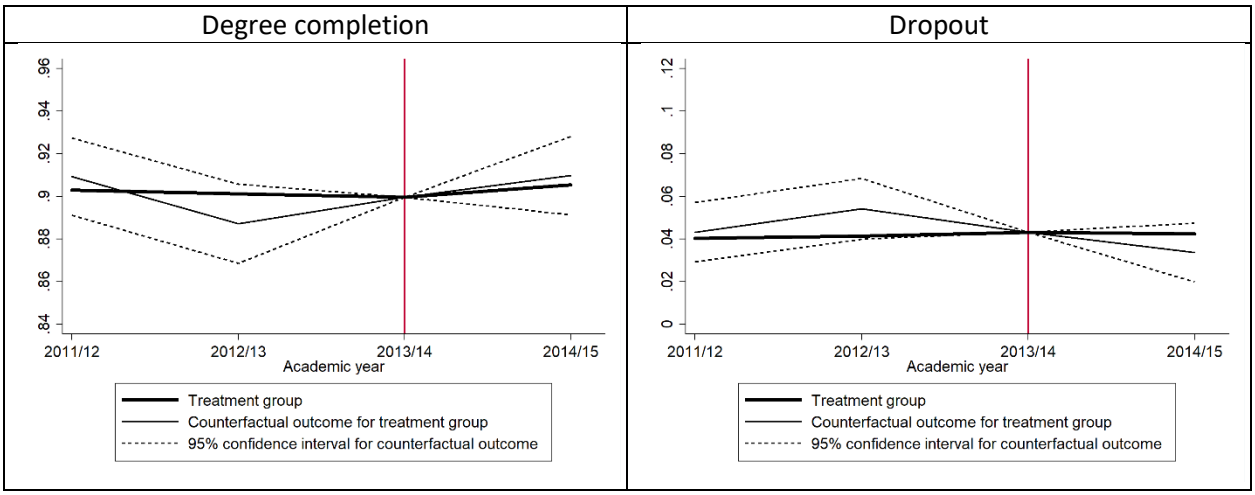
women and low-income students (Table A1). For men and high-income students, we find that after the reform, degree completion in the treatment group has significantly increased relative to the control group. The parallel trends assumption for dropout is never rejected (Table A2). The policy reform had a similar negative effect on dropout of men and women (significant at the 10% level). Dropout significantly decreased for high income students while there is no effect for low income students. The last two columns estimate the difference-in-differences model for Belgian and non-Belgian students. While the reform had a clear effect on degree completion and dropout for Belgian students, we do not find a significant effect for students with a different nationality. The magnitude of the treatment effects is also smaller for non-Belgian students indicating that the non-significance is not driven by the smaller sample size of non-Belgian students. After their studies, most foreign students will return to their home country and the reforms are therefore not relevant for these students. This makes them less likely to be affected by local labor market policies. Estimating the difference-in-differences model on the sample of non-Belgian students could therefore also be seen as a kind of placebo analysis because we expect a smaller or no treatment effect for this group of students.

5.2. Secondary education

The second policy reform could have affected study outcomes in secondary education because as from September 2015, school-leavers under the age of 21 could claim the activation allowance only after successfully completing the last year of secondary education. Figure 4 compares degree completion and dropout in the final year of secondary education for students in the treatment and control group. Treated students are 18 years old or 19 years old and born after August 31 (treatment group 1 and 2 as defined in Section 3 for secondary education). Students in the control group are 19 years old and born before September 1 or 20 years old. Approximately 90% of the students in the treatment group obtains a degree at the end of the year and 4% drops out without a degree (thick solid line). Observed degree completion and dropout remain within the 95% confidence interval of the counterfactual outcome of the treated in absence of the policy reform (the thin solid lines). This seems to suggest that the parallel trends assumption is not rejected for both outcome variables and that there is no impact of the reform on dropout and degree completion in secondary education.

Figure A2 in appendix shows degree completion and dropout by the track in which students were enrolled in the last year of secondary education. Most students who graduate from the academic track continue their studies in higher education. The fraction of students enrolling in higher education is much lower for technical and vocational secondary education. It is therefore more likely that labor market policies affect degree completion and dropout in these programs. Panel A of Figure A2 considers the students enrolled in academic secondary education. Degree completion and dropout for students enrolled in technical secondary education and vocational secondary education are shown in respectively panel B and C. From these graphs, there seems to be no effect of the policy reform on degree completion or dropout for students enrolled in the different tracks in secondary education. Note that the treatment and control groups do not always seem to follow a parallel trend.

Figure 4. Degree completion and dropout in the last year of secondary education



Note: Treatment group = 18 year-old and 19 year-old students born after September 1. Control group = 19 year-old students born before September 1 and 20 year-old students. Age is measured on December 31 of the respective academic year. The vertical line is drawn at the last period before the reform. The thick solid line shows the observed outcome of the treatment group. The fine solid line shows the counterfactual outcome of the treatment group in absence of the treatment. The thin dotted lines are the 95% confidence intervals of the counterfactual path.

Table 5 presents the output of the difference-in-differences regressions for degree completion and follows a similar structure as the corresponding Table 3 for higher education. Panel A reports the treatment effects of the baseline model. The parallel trends assumption is not rejected at the 5% level. We do not find that conditioning entitlement to unemployment benefits on educational attainment significantly affected degree completion in secondary education.

The final panel of Table 5 allows for a different treatment effect for the two treatment groups (18 year-old and 19 year-old students born after August 31). Remember that the policy reform had a less severe impact on the 19-year-old students because the policy reform implied an increase in their waiting period of at most four months. We therefore expect that the incentive to complete a degree is stronger for the 18 year-old students. For this specification, we reject the parallel trends assumption for 18 year-old students enrolled in vocational secondary education. The estimated effects for 18 year-old students in vocational secondary education are therefore likely to be biased. There is no significant change in degree completion for 18 year-old students enrolled in the academic or technical track. For 19 year-old students, we find that degree completion even decreased after the policy reform.

Table 6 presents the output of similar difference-in-differences regressions for the other outcome variable: dropping out of secondary education without a degree. The first regression in panel A shows a significant increase in dropout for treated students after the reform. This negative effect can be attributed to the students in vocational secondary education in the last column of the Table. Notice that the parallel trends assumption is rejected at the 5% level for this group. This negative effect can therefore be biased. The parallel trends assumption is not rejected for students in the academic and technical track, but we do not find a significant change in dropout in these programs. The next panel shows an increase in dropout for both 18 and 19 year-old students. For 18 year-old students, this effect is again driven by students in vocational secondary education for which we reject the parallel trends assumption. For 19 year-old

students, we find an increase in dropout after the reform which do not seem to be driven by a specific study track.

Table 5. Degree completion in the final year of secondary education

VARIABLES	(1) All students	(2) Academic SE	(3) Technical SE	(4) Vocational SE
Panel A: Baseline model				
Treatment effect	-0.006	0.024	-0.008	-0.011
	(0.008)	(0.024)	(0.011)	(0.012)
Counterfactual outcome	0.912	0.899	0.924	0.898
Parallel trends: p-value	0.076	0.054	0.527	0.105
Observations	82121	14210	33641	34270
Panel C: Different effects according to age				
Age 18				
Treatment effect	-0.004	0.026	-0.006	-0.008
	(0.008)	(0.024)	(0.011)	(0.012)
Counterfactual outcome	0.917	0.900	0.928	0.903
Age 19 (born after 1/9)				
Treatment effect	-0.026**	-0.013	-0.024	-0.029
	(0.012)	(0.037)	(0.018)	(0.018)
Counterfactual outcome	0.876	0.876	0.893	0.860
Similar effects for both groups: p-value	0.029	0.176	0.212	0.181
Parallel trends Age 18: p-value	0.075	0.063	0.483	0.041
Parallel trends Age 19: p-value	0.342	0.099	0.181	0.486
Observations	82121	14210	33641	34270

Note: Treatment group = 18 year-old students and 19 year-old students born after September 1. Control group = 19 year-old and born before September 1 and 20 year-old students. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 until 2013-2014. Treatment period = 2014-2015. The regressions control for gender, socio-economic status, language spoken at home and include time dummies. The regression in column 1 additionally controls for the type of study program. The counterfactual outcome is the predicted outcome for the treated in absence of the treatment in the post-reform period. To test for parallel trends, we estimate similar regressions with interaction effects between the treatment groups and year dummies. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value of the test for parallel trends is reported for each specification. The p-value of the test for similar effects according to age is reported in panel B. Heteroskedasticity robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

A possible explanation for the increase in dropout and decrease in degree completion of 19 year-old students -on which we find reliable (unbiased) results- could be that the policy reform incentivized students to drop out of regular full-time education and enroll in part-time vocational secondary education. In these study programs, students combine their studies with working to prepare them better for the labor market. Students in part-time vocational education can already earn a wage. Losing entitlement to the activation allowance makes it therefore more attractive to enroll in these programs. However, this hypothesis cannot be tested with our data since we do not observe students enrolled in part-time vocational education.

Table A3 and A4 in appendix assess whether the treatment effects differ according to gender, household income and language spoken at home. We do not find that the reform increased degree completion or

reduced unsuccessful dropout for specific subgroups. In contrast, our results show that after the reform, degree completion even decreased and dropout increased for particular groups of students. For example, among 19 year-old students, degree completion is lower for boys and from those living in low income families. Within the same group of students, dropout is significantly higher for boys. Related to the previous explanation, it may be the case that part-time vocational education that combines studies with an apprenticeship is more accessible to boys and attracts students from lower socio-economic background.

Table 6. Dropout in the final year of secondary education

VARIABLES	(1) All students	(2) Academic SE	(3) Technical SE	(4) Vocational SE
Panel A: Baseline model				
Treatment effect	0.014**	-0.001	0.010	0.020**
	(0.006)	(0.020)	(0.008)	(0.009)
Counterfactual outcome	0.028	0.045	0.023	0.031
Parallel trends: p-value	0.173	0.523	0.336	0.015
Observations	82121	14210	33641	34270
Panel C: Different effects according to age				
Age 18				
Treatment effect	0.014**	-0.003	0.010	0.020**
	(0.006)	(0.020)	(0.008)	(0.009)
Counterfactual outcome	0.025	0.044	0.019	0.027
Age 19 (born after 1/9)				
Treatment effect	0.019**	0.034	0.014	0.019
	(0.009)	(0.031)	(0.013)	(0.013)
Counterfactual outcome	0.057	0.058	0.052	0.061
Similar effects over time: p-value	0.499	0.119	0.682	0.882
Parallel trends Age 18: p-value	0.142	0.568	0.261	0.005
Parallel trends Age 19: p-value	0.797	0.283	0.802	0.935
Observations	82121	14210	33641	34270

Note: Treatment group = 18 year-old and 19 year-old students born after September 1. Control group = 19 year-old and born before September 1 and 20 year-old students. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 until 2013-2014. Treatment period = 2014-2015. The regressions control for gender, socio-economic status, language spoken at home and include time dummies. The regression in column 1 additionally controls for the type of study program. The counterfactual outcome is the predicted outcome for the treated in absence of the treatment in the post-reform period. To test for parallel trends, we estimate similar regressions with interaction effects between the treatment groups and year dummies. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value of the test for parallel trends is reported for each specification. The p-value of the test for similar effects according to age is reported in panel B. Heteroskedasticity robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

6. Conclusion

We studied whether providing financial support to unemployed school-leavers reduces the incentives to invest in education. More specifically, we evaluated how two policy reforms that restricted access to

unemployment benefits for school-leavers (the "activation allowance") affected dropout and degree completion in secondary and higher education in Flanders, the region in the North of Belgium. The first reform scrapped the entitlement to this allowance for those older than 25. The second reform conditioned eligibility for the activation allowance on obtaining a high school degree, but only for those below the age of 21. After the reforms, many school-leavers were not anymore eligible for the activation allowance and the policy reforms created additional incentives for students to complete secondary or higher education.

Using a difference-in-differences approach, we find that the first reform significantly increased, although not in a great extent, degree completion in higher education of the affected students by on average 2 percentage points, i.e. from 40.2% to 42.2%. In addition, the reform also significantly lowered dropout in higher education by 0.7 percentage points, from 13.3% to 12.6%. As expected, foreign students are not affected by the reform. For these students the reforms are not relevant, because most of them will return to their home country after their studies.

Although the second reform made eligibility to the activation allowance conditional upon graduating from secondary education, we do not find evidence for any significant increase in degree completion or decrease in dropout in the last year of secondary education. We even found a reverse result for 19 year-old students, especially for boys and those in low-income households. An explanation of this result could be that the policy reform incentivized some students to drop out of regular full-time education and enroll in part-time vocational secondary education. In these study programs, students are engaged in an apprenticeship employment contract and can already earn a wage. Losing entitlement to the activation allowance makes it therefore more attractive to enroll in these programs. This hypothesis cannot however be tested with our data since we do not observe students enrolled in such programs.

According to our results there is no clear-cut evidence to prove that future entitlement to UI affects educational attainment. On the one hand, we find evidence that students in higher education are forward looking and that lower generosity of future UI makes them more likely to graduate from higher education. By investing in higher education, students increase their future earnings capacity in order to reduce social benefits dependency. On the other hand, conditioning entitlement to UI on high school graduation does not foster degree completion in high school, although this was an explicit aim of the policy reforms.

It is hard to blame a lack of information for driving the different findings between secondary and higher education students. As a matter of fact, according to a recent survey of 1,000 students in Flanders (Vanneste, 2016), the fraction of respondents that was informed about the activation allowance was not substantially larger in higher than in secondary education. One explanation is rather the potential lack of forward looking behavior (myopia) among lower educated youth as compared to their higher educated counterparts, as suggested by some studies in the behavioral economics literature (Becker and Mulligan, 1997; Lavecchia et al. 2014). We cannot however rule out that the removal of the diploma requirement at the age of 21 makes ineffective the reform conditioning entitlement to UI on high school graduation. Students enrolled in higher education face a permanent loss of the activation allowance from age 25 onwards, while youth without a high school degree regain entitlement from age 21 onwards.

The analysis in this paper is limited to evaluating the impact of the reforms on degree completion and dropout at the end of the academic year. Losing future entitlement to the activation allowance could also have discouraged enrollment in master programs for bachelor graduates or the decision to start a second master program. These effects can only be detected as from the second year after the policy reform because the reform was announced after students made their enrollment decisions in 2014.

Consequently, this reform could have decreased human capital investments in higher education. And, as stated above, the second policy reform could have encouraged enrollment in part-time vocational secondary education. In further research it would be interesting to follow students during several years in full-time and part-time secondary and higher education to get a more complete picture of the consequences of the reforms on educational attainment.

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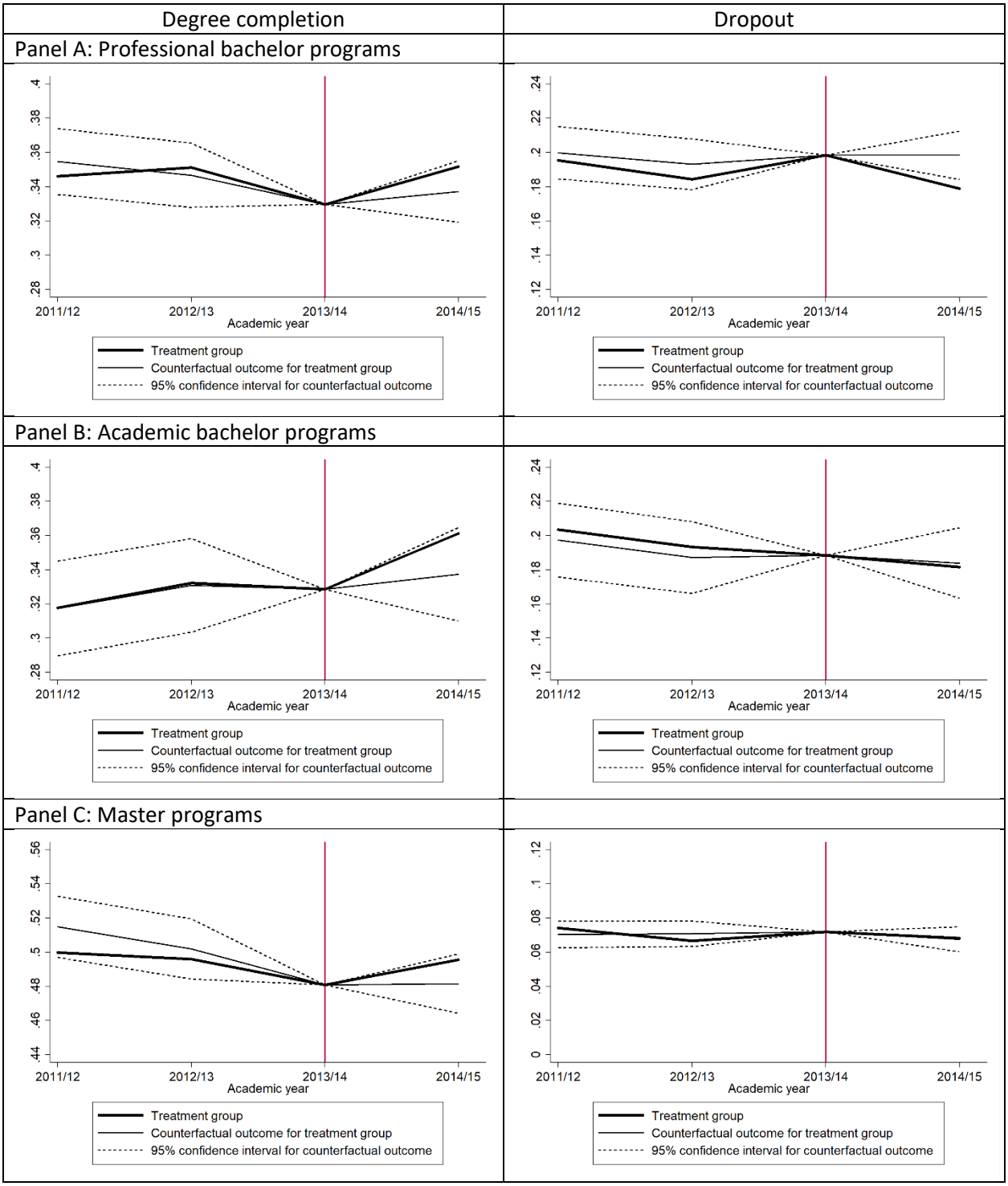
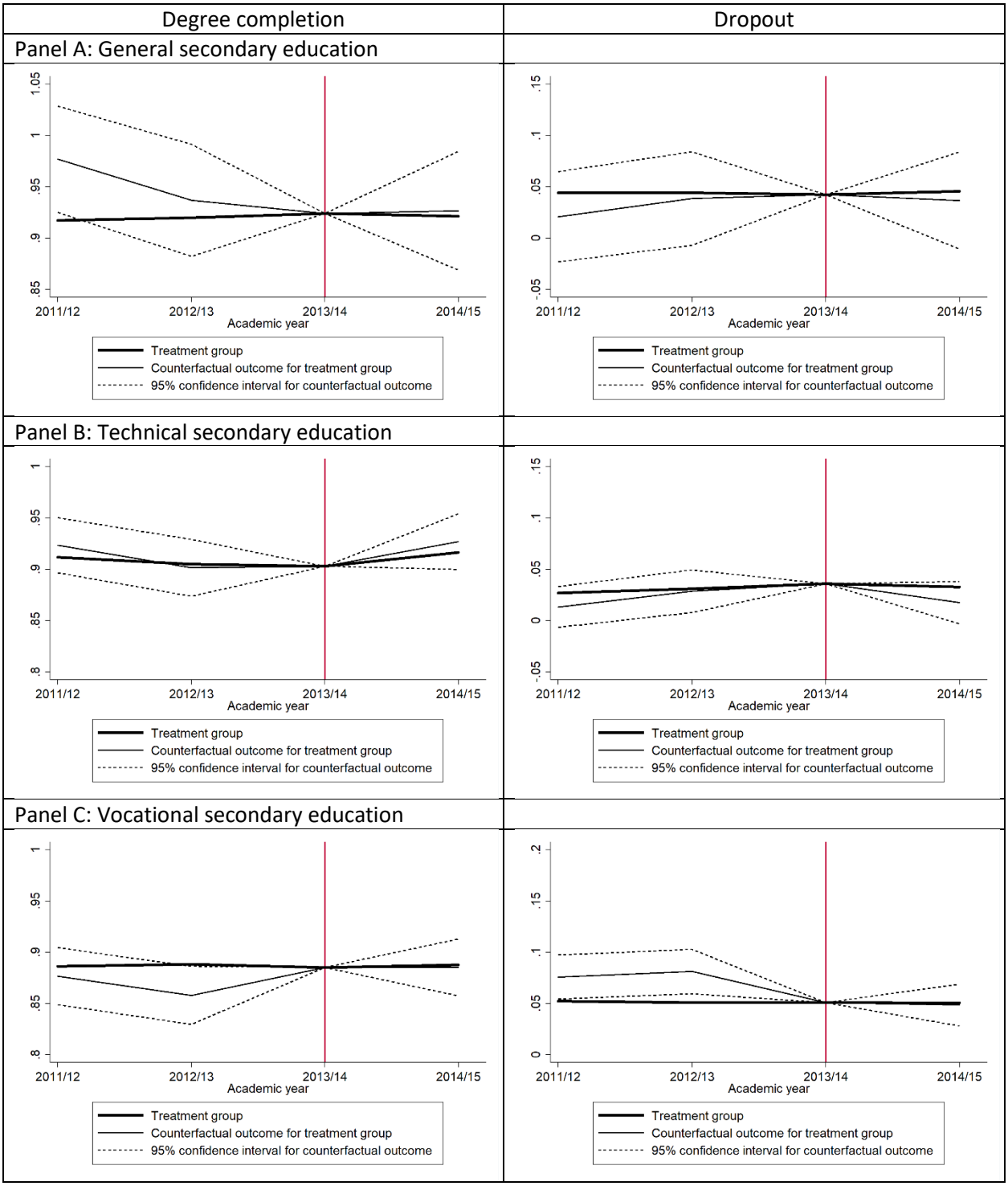
Appendix: Additional tables and figures

Figure A1. Degree completion and dropout in higher education



Note: Treatment group = 23 and 24-year old students. Control group = 22 year-old students. Age is measured on December 31 of the respective academic year. The vertical line is drawn at the last period before the reform. The thick solid line shows the observed outcome of the treatment group. The fine solid line shows the counterfactual outcome of the treatment group in absence of the treatment. The thin dotted lines are the 95% confidence intervals of the counterfactual path.

Figure A2. Degree completion and dropout in the 6th year of secondary education



Note: Treatment group = 18 year-old students and 19 year-old students born after September 1. Control group = 19 year-old students born before September 1 and 20 year-old students. Age is measured on December 31 of the respective academic year. The vertical line is drawn at the last period before the reform. The thick solid line shows the observed outcome of the treatment group. The fine solid line shows the counterfactual outcome of the treatment group in absence of the treatment. The thin dotted lines are the 95% confidence intervals of the counterfactual path.

Table A1. Degree completion in higher education (heterogeneous effects)

VARIABLES	(1) Men	(2) Women	(3) High income	(4) Low income	(5) Belgian nationality	(6) Other nationality
Panel A: Baseline model						
Treatment effect	0.023***	0.017**	0.021***	0.016	0.020***	0.008
	(0.007)	(0.007)	(0.005)	(0.010)	(0.005)	(0.014)
Counterfactual outcome	0.382	0.422	0.416	0.351	0.423	0.270
Parallel trends: p-value	0.292	0.001	0.641	0.044	0.218	0.980
Observations	104653	110525	170923	44255	195981	19197
Panel B Different effects according to age						
Age 23						
Treatment effect	0.019**	0.013*	0.019***	0.006	0.016***	0.011
	(0.008)	(0.008)	(0.006)	(0.011)	(0.006)	(0.016)
Counterfactual outcome	0.379	0.421	0.413	0.353	0.418	0.250
Age 24						
Treatment effect	0.028***	0.023***	0.023***	0.034**	0.028***	0.004
	(0.009)	(0.009)	(0.007)	(0.014)	(0.007)	(0.017)
Counterfactual outcome	0.387	0.425	0.422	0.347	0.431	0.292
Similar effects for both groups: p-value	0.364	0.287	0.557	0.058	0.004	0.682
Parallel trends Age 23: p-value	0.641	0.002	0.492	0.080	0.187	0.607
Parallel trends Age 25: p-value	0.197	0.061	0.817	0.165	0.720	0.688
Observations	104653	110525	170923	44255	195981	19197

Note: Treatment group = 23 and 24 year-old students. Control group = 22 year-old students. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 until 2013-2014. Treatment period = 2014-2015. The regressions control for gender, socio-economic status, nationality, type of study program and include time dummies. The counterfactual outcome is the predicted outcome for the treated in absence of the treatment in the post-reform period. To test for parallel trends, we estimate similar regressions with interaction effects between the treatment groups and year dummies. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value of the test for parallel trends is reported for each specification. The p-value of the test for similar effects according to age is reported in panel B. Heteroskedasticity robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A2: Dropout in higher education (heterogeneous effects)

VARIABLES	(1) Men	(2) Women	(3) High income	(4) Low income	(5) Belgian nationality	(6) Other nationality
Panel A: Baseline model						
Treatment effect	-0.009*	-0.007*	-0.010***	-0.001	-0.010***	0.014
	(0.005)	(0.004)	(0.003)	(0.007)	(0.003)	(0.013)
Counterfactual outcome	0.148	0.122	0.135	0.136	0.132	0.145
Parallel trends: p-value	0.642	0.351	0.641	0.702	0.284	0.466
Observations	104653	110525	170923	44255	195981	19197
Panel B: Different effects according to age						
Age 23						
Treatment effect	-0.003	-0.005	-0.005	0.002	-0.006*	0.021
	(0.005)	(0.005)	(0.004)	(0.008)	(0.004)	(0.014)
Counterfactual outcome	0.133	0.110	0.120	0.129	0.118	0.142
Age 24						
Treatment effect	-0.019***	-0.012**	-0.018***	-0.006	-0.017***	0.007
	(0.007)	(0.006)	(0.005)	(0.010)	(0.005)	(0.014)
Counterfactual outcome	0.172	0.143	0.160	0.150	0.157	0.147
Similar effects for both groups: p-value	0.026	0.269	0.025	0.459	0.031	0.299
Parallel trends Age 23: p-value	0.383	0.372	0.539	0.664	0.366	0.537
Parallel trends Age 24: p-value	0.739	0.385	0.949	0.672	0.510	0.398
Observations	104653	110525	170923	44255	195981	19197

Note: Treatment group = 23 and 24 year-old students. Control group = 22 year-old students. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 until 2013-2014. Treatment period = 2014-2015. The regressions control for gender, socio-economic status, nationality, type of study program and include time dummies. The counterfactual outcome is the predicted outcome for the treated in absence of the treatment in the post-reform period. To test for parallel trends, we estimate similar regressions with interaction effects between the treatment groups and year dummies. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value of the test for parallel trends is reported for each specification. The p-value of the test for similar effects according to age is reported in panel B. Heteroskedasticity robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A3: Degree completion in the final year of secondary education (heterogeneous effects)

VARIABLES	(1) Men	(2) Women	(3) High income	(4) Low income	(5) Dutch	(6) Other language
Panel A: Baseline model						
Treatment effect	-0.007	-0.006	0.004	-0.023*	0.003	-0.026*
	(0.010)	(0.011)	(0.010)	(0.012)	(0.009)	(0.015)
Counterfactual outcome	0.897	0.931	0.900	0.925	0.908	0.890
Parallel trends: p-value	0.202	0.348	0.349	0.030	0.072	0.837
Observations	47429	34692	55438	26683	68796	13325
Panel B: Different effects according to age						
Age 18						
Treatment effect	-0.002	-0.007	0.006	-0.020*	0.005	-0.024
	(0.010)	(0.011)	(0.010)	(0.012)	(0.009)	(0.015)
Counterfactual outcome	0.901	0.935	0.905	0.929	0.912	0.893
Age 19 (born after 1/9)						
Treatment effect	-0.042***	-0.001	-0.013	-0.044**	-0.020	-0.034
	(0.016)	(0.017)	(0.016)	(0.019)	(0.014)	(0.023)
Counterfactual outcome	0.865	0.890	0.858	0.896	0.870	0.875
Similar effects for both groups: p-value	0.003	0.710	0.142	0.119	0.026	0.650
Parallel trends Age 18: p-value	0.186	0.366	0.312	0.041	0.073	0.881
Parallel trends Age 19: p-value	0.653	0.213	0.892	0.110	0.337	0.814
Observations	47429	34692	55438	26683	68796	13325

Note: Treatment group = 18 year-old students and 19 year-old students born after September 1. Control group = 19 year-old and born before September 1 and 20 year-old students. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 until 2013-2014. Treatment period = 2014-2015. The regressions control for gender, socio-economic status, language spoken at home, the type of study program and include time dummies. The counterfactual outcome is the predicted outcome for the treated in absence of the treatment in the post-reform period. To test for parallel trends, we estimate similar regressions with interaction effects between the treatment groups and year dummies. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value of the test for parallel trends is reported for each specification. The p-value of the test for similar effects according to age are reported in panel B. Heteroskedasticity robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A4: Dropout in the final year of secondary education (heterogeneous effects)

VARIABLES	(1) Men	(2) Women	(3) High income	(4) Low income	(5) Dutch	(6) Other language
Panel A: Baseline model						
Treatment effect	0.011	0.018**	0.013*	0.016*	0.012*	0.022*
	(0.008)	(0.008)	(0.008)	(0.009)	(0.007)	(0.011)
Counterfactual outcome	0.035	0.020	0.034	0.022	0.027	0.049
Parallel trends: p-value	0.233	0.518	0.089	0.112	0.333	0.672
Observations	47429	34692	55438	26683	68796	13325
Panel B: Different effects according to age						
Age 18						
Treatment effect	0.009	0.020**	0.012	0.016*	0.012*	0.020*
	(0.008)	(0.008)	(0.008)	(0.009)	(0.007)	(0.011)
Counterfactual outcome	0.031	0.017	0.030	0.019	0.023	0.047
Age 19 (born after 1/9)						
Treatment effect	0.028**	0.005	0.017	0.020	0.014	0.032*
	(0.012)	(0.013)	(0.012)	(0.013)	(0.011)	(0.017)
Counterfactual outcome	0.065	0.046	0.069	0.044	0.060	0.054
Similar effects for both groups: p-value	0.074	0.149	0.612	0.677	0.853	0.423
Parallel trends Age 18: p-value	0.270	0.451	0.077	0.149	0.281	0.568
Parallel trends Age 19: p-value	0.312	0.135	0.533	0.135	0.937	0.338
Observations	47429	34692	55438	26683	68796	13325

Note: Treatment group = 18 year-old students and 19 year-old students born after September 1. Control group = 19 year-old and born before September 1 and 20 year-old students. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 until 2013-2014. Treatment period = 2014-2015. The regressions control for gender, socio-economic status, language spoken at home, type of study program and include time dummies. The counterfactual outcome is the predicted outcome for the treated in absence of the treatment in the post-reform period. To test for parallel trends, we estimate similar regressions with interaction effects between the treatment groups and year dummies. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value of the test for parallel trends is reported for each specification. The p-value of the test for similar effects according to age is reported in panel B. Heteroskedasticity robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.