Losing Prospective Entitlement to Unemployment Benefits. Impact on Educational Attainment

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Abstract

Providing income support to unemployed education-leavers reduces the returns to investments in education because it makes the consequences of unemployment less severe. We evaluate a two-part policy reform in Belgium to study whether conditioning the prospective entitlement to unemployment benefits for education-leavers on age or schooling attainment can affect educational achievements. The results show that the prospect of financial loss in case of unemployment can significantly raise degree completion and reduce dropout in higher education, but not in high school. We argue that the higher prevalence of behavioral biases among lower educated and younger students could explain these contrasting findings.

JEL codes: H52, I21, I26, I28, J08, J18, J24, J65, J68

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1. Introduction

Most developed countries provide some form of social protection for youths who enter the labor market upon leaving education. In most countries, this consists of a means-tested welfare benefit, but in some others, unemployed education-leavers[2] are entitled to unemployment insurance (UI) without means-test. In Australia and New Zealand UI is provided immediately upon registration as a 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).[3]

There is abundant literature that studies the negative effect of UI and welfare benefits on work incentives.[4] What is much less known is the impact of such welfare and UI schemes on investments in educational attainment. By providing support in case of unemployment these schemes reduce the returns to investments in education and, hence, lower the long-run earnings capacity of youth (Kesselman, 1976; Moffitt, 2002). To the extent that investments in human capital decrease the risk of unemployment, withdrawing UI should increase this return. However, there is substantial evidence that adolescents are present biased and have difficulty in taking future awards into account when making decisions (Lavecchia et al., 2014). This questions whether youths effectively change their educational choices in reaction to the prospect of losing the entitlement to UI in case they would not find employment after labor market entry. We study this question based on a two-part reform in Belgium that restricts the access to UI to which youth with little or no employment experience is entitled one year after leaving the education system. The first part of this reform disqualified youths aged 25 or more for this UI. This predominantly affected students in college or university. The second part conditioned the eligibility for UI on earning a high school diploma, but only for those below the age of 21. Hence, this second reform targeted low-educated youth.

The existing empirical literature has mainly studied whether programs that make work more rewarding have a negative impact on educational investments, and not whether the withdrawal of benefits for those who are not working can enhance educational attainment. Keane and Wolpin (2000), Riddell and Riddell (2014), and Blundell et al. (2016) provide causal evidence that financially rewarding work reduces educational attainment. Keane and Wolpin (2000) estimate a structural dynamic model of schooling, work, and occupational choice decisions over the life cycle. They show for the U.S. that introducing wage subsidies would reduce the returns to education and therefore completed schooling levels. Blundell et al. (2016) build on this model to study the consequences of the introduction in the UK of tax credits for lone-mother welfare recipients on labor supply and human capital accumulation over the life cycle. They find that tax credits increase labor supply and reduce post-compulsory educational attainment of young women in the UK. Riddell and Riddell (2014) 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

[2] Individuals leaving secondary or higher education, with or without a degree are referred to as "education-leavers".
[3] Based on age and educational attainment, education-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 education-leavers who join an unemployment fund within two weeks after graduation can immediately obtain unemployment benefits. Education-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).
[4] See e.g. Tatsiramos and Van Ours, 2014 or Schmieder and von Wachter, 2016 for surveys; Le Barbanchon, 2016 and Kolsrud et al., 2018 for more recent evidence.

recipients in case they left 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 enrollment in higher education. This led to the conclusion that 'work-first' policies reduce educational activity and may have adverse consequences on the long-run earnings capacity of welfare recipients.

To the best of our knowledge, there is no research on the prediction that a lower UI generosity reduces the return to education and affects schooling outcomes. Most closely to our research are the studies of Hernaes et al. (2017) and Cammeraat et al. (2022) that evaluate how stricter eligibility requirements for means-tested welfare benefits affect schooling outcomes.[5] Hernaes et al. (2017) find that imposing on social assistance stricter eligibility requirements in the form of mandatory activities enhances degree completion and decreases dropout in secondary education in Norway.[6] They also find that these favorable effects persist by contributing to higher educational attainment, higher labor earnings, and lower transfer dependency at age 25. By contrast, Cammeraat et al. (2022) report that the introduction of a mandatory activation program for young welfare benefit recipients in the Netherlands did not significantly affect the enrollment in higher education for youths aged 25 and 26. They argue that the fact that this policy reform was introduced at the start of the Great Recession explains its null effect.

This research provides evidence on this question based on the 2015 reform of UI for youths in Belgium. In particular, we investigate whether the nature and timing of eligibility restrictions, as well as the level of education of the targeted individuals matter. First, in this study, the stricter eligibility condition consists in losing the full entitlement to non-means-tested UI rather than, as in Hernaes et al. (2017) and Cammeraat et al. (2022), imposing activity requirements on the receipt of means-tested welfare benefits. On the one hand, one may expect a stronger behavioral reaction in the former than in the latter, because losing the full benefit entitlement is a higher stake than being forced to engage in particular activities. On the other hand, the latter may be more effective than the former to the extent that avoiding activity requirements is more concrete and easier to understand than avoiding financial loss. Similar reasoning explains why financial incentives for educational inputs, such as reading a book or good behavior, may be more effective than incentives for educational outputs (Fryer, 2011; Gneezy et al., 2011). In addition, a growing literature suggests that non-financial incentives are more powerful than financial ones (see e.g. Cassar and Meier, 2018), although there seems to be less evidence for this in an educational context (Levitt et al., 2016). Second, (in contrast to Norway) in Belgium, the eligibility restrictions are not imposed immediately after leaving school, but one year later. Given the numerous studies in behavioral economics that find that adolescents tend to be present biased and have difficulty in taking future awards into account when making decisions (see e.g. Lavecchia et al., 2014; Koch et al. 2015; Levitt et al., 2016), this further delay of the financial incentive may decrease its power to trigger behavioral reactions. Finally, while the Norwegian study focuses on high school, this study considers behavioral reactions in both high school and university. This is of interest, as the

[5] To the best of our knowledge, Miller and Saunders (1997) are the first to examine the effect of welfare generosity on educational attainment. They did not find any significant impact on high school completion, but one may question whether the between-states comparison of this early study provides convincing causal evidence.

[6] 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. Bratsberg et al. (2019) build on the paper of Hernaes et al. (2017) to demonstrate that these stricter activation requirements also reduced crime among teenage boys from disadvantaged families, partly arising "from the simple fact that when youth are kept occupied by activation or in school, there is less time and opportunity left for committing crime" (p. 561).

behavioral economics literature suggests that more able and higher educated individuals are less present biased than lower educated ones (Becker and Mulligan, 1997; Sutter et al., 2013; Lavecchia et al. 2014; Golsteyn et al., 2014). The higher educated are therefore expected to react more strongly to the Belgian policy reform than the lower educated. The studies of Leuven et al. (2010) and Bettinger (2011) indeed find that the provision of incentives only leads to higher academic achievements for higher ability students.

Our analysis exploits two natural experiments induced by a two-part policy reform in 2015. The first part of this 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 part aimed in the first place at fostering work incentives,[7] but, also, by weakening the safety net in case of long-term unemployment, *indirectly* strengthened incentives to complete education successfully. The second part 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 school dropouts not only by work incentives but also by financially incentivizing students to complete high school.

To evaluate the impact of this two-part policy reform, we make use of register files of the full population of students enrolled in secondary and higher education before and after the reform in both the Flemish and French Communities of Belgium. Based on these register data, we can identify the effects of the reform on two outcomes: the degree completion and dropout rate at the end of the considered year of enrollment.[8] The identification relies on a difference-in-differences approach that compares aforementioned outcomes before and after the policy reform between students who, based on age or educational attainment, were being affected, or not by the reform.

Our main findings can be summarized as follows. The loss of entitlement to UI significantly raised the rate of degree completion of Belgian students in higher education in the graduation year by 2.8 percentage points (pp), and reduced dropout (including dropout in non-final years) by 1.1 pp. By contrast, we did not find any evidence that the stricter eligibility conditions for UI enhanced high school graduation or reduced high school dropout. In Belgium, each language Community (Flemish or French) has the competence education autonomously. Yet, we cannot reject the hypothesis that the reported effects of the reforms differ between these Communities. This reinforces the external validity of our findings and makes it more likely they can be extrapolated to other countries.

To be able to compare the magnitude of the effect on degree completion with the one reported by Hernaes et al. (2017), we follow these authors by dividing the effect by the exposure risk. This results loosely in the "average treatment effect on the treated" (ATET). The ATET of losing the activation allowance on degree completion for final year students in higher education is then estimated to be 28 pp, which is higher than the 17pp reported by Hernaes et al. (2017) for the welfare reform in

[7] Cockx et al. (2020) study the impact of this reform on the transition to work and find that losing eligibility to UI has increased the transition to very short-lived jobs only.

[8] A student who does not complete a degree does not necessarily drop out: she may continue education, either by repeating the grade, or, in case she is not in her graduation year, by continuing education in the next grade.

Norway. However, the latter ATET refers to high school graduation for which we find a zero effect in Belgium.

There are several potential explanations for this contrasting finding according to education level. First, the reform 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, as already mentioned above, lower educated youths have a higher tendency to be present biased, which implies that they are less responsive to this uncertain future income loss. Third, even if the higher educated are less at risk of income loss induced by the reform because they are less likely unemployed one year after entering the labor market, there is evidence that job seekers with a high underlying job finding rate tend to be over-pessimistic concerning their employment chances, whereas job seekers with a low job-finding rate are over-optimistic (Mueller et al. 2021). This suggests that the higher educated might overreact (and the lower educated underreact) to the reform relative to the objective risk to be affected by it. Finally, as mentioned above, the absence of a behavioral reaction of high school students may also be related to the fact that the risk of a financial loss may be too abstract and therefore less comprehensible than the risk of being forced to engage in specific activities as in the Norwegian welfare reform studied by Hernaes et al. (2017).

The remainder of this paper is organized as follows. Section 2 summarizes the institutional context and the UI reform that may have affected the behavior of students in secondary and higher education. Section 3 discusses the methodology and expected effects of the reform for the different treatment groups. Section 4 describes the data and Section 5 presents the results. The last section provides some concluding discussion.

2. Institutional Context

Belgium is a federal state in which many competencies have been decentralized. Generally, place-based matters are decentralized to the Regions (Flanders, Wallonia, and Brussels) and language-based matters to the Communities (Flemish and French Community).[9] 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 available for both communities.[10] In this section we first explain the main institutional features of secondary and higher education in Belgium. Next, we clarify the pre-reform eligibility conditions for the activation allowance for education-leavers within the federal UI. Subsequently, we discuss the policy reform of 2015 that will be evaluated in this study.

2.1. Secondary and Higher Education

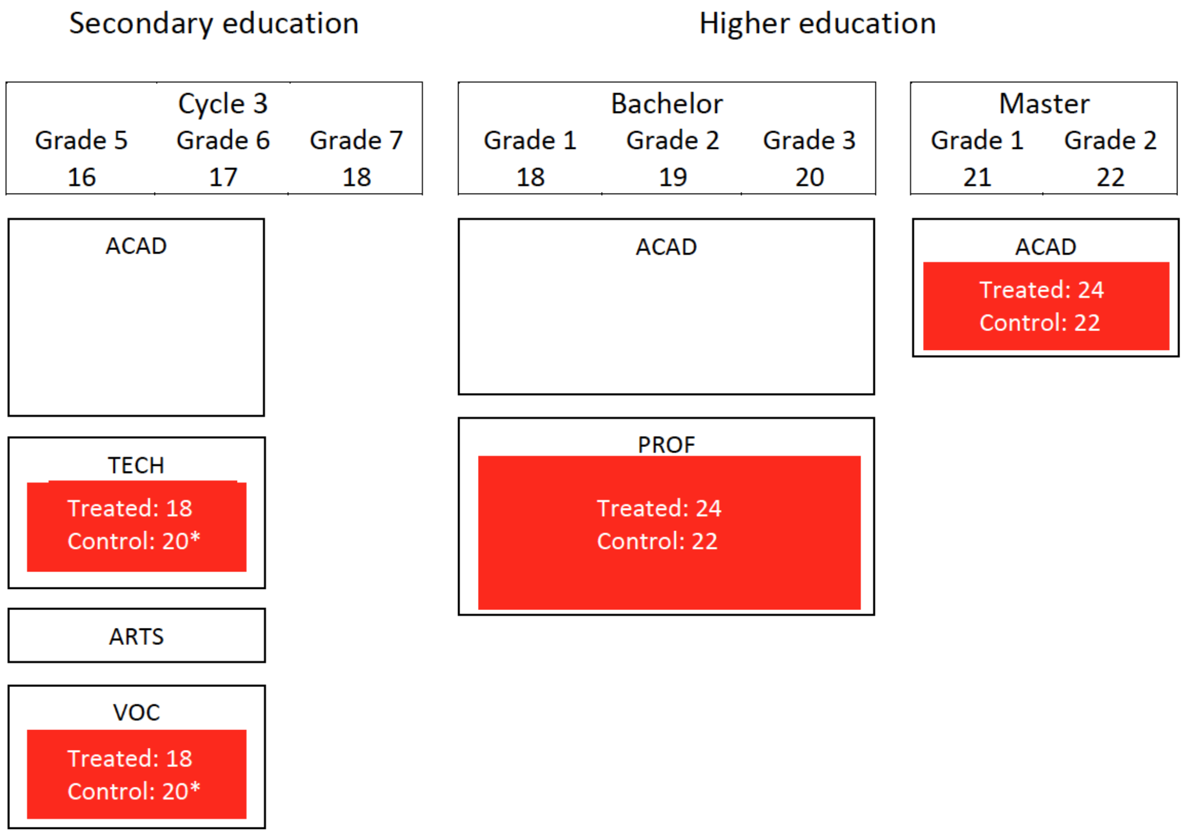
After finishing primary school, students enroll in secondary education, usually at the age of 12. Secondary education is subdivided into six grades (seven grades in the vocational track) and three cycles, each comprising two grades. Each grade starts on September 1st and ends on June 30, but it can take more time to complete it as grade repetition – on which more below – is quite common.

[9] About 56% of the population of 11.5 million inhabitants lives in Flanders (the Dutch-speaking region in the north of Belgium), 31% lives in Wallonia (the French-speaking region in the south of Belgium), and 10% lives in Brussels (the bilingual, but predominantly French-speaking region of Belgium).

[10] Students enrolled in Dutch-speaking secondary or higher education institutions in Flanders or in Brussels are included in the data of the Flemish Community, while students enrolled in French-speaking secondary or higher education institutions in Wallonia or Brussels are included in the data of the French Community.

Education 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.

Figure 1: Third Cycle of Secondary Education and Higher Education in Belgium



Note: "Grade number" refers to the grade numbering respectively in secondary education, and in the bachelor's or master's in higher education; the numbers below indicate the theoretical age of entry in the corresponding grade in the absence of grade repetition. ACAD = academic track; TECH = technical track; ARTS = artistic track; VOC = vocational track; PROF = professional track in higher education. The different groups we retain in our analysis as treatment and control groups are indicated in red.
*In Flanders, students aged 19 and born before September are also part of the control group.

During their secondary education, students are grouped into four different tracks according to ability and preferences. The academic track provides students with a theoretical background and prepares them 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 directly for the labor market. A small fraction of students are enrolled in artistic secondary education that prepares for artistic programs in professional higher education. The ordering (excluding the artistic track as a special case) also corresponds to an implicit hierarchy attached to these tracks. Students previously enrolled in a high track may always decide to downgrade to a lower track, but once enrolled in a lower track it is virtually impossible to move up to a higher track later on. The left panel of Figure 1 shows the structure of the third cycle of secondary education in Belgium. At the end of each school year, the teachers in the class council gather to evaluate the overall performance of students in each grade. If students performed below the norm, the council may impose that students have to repeat a grade or downgrade to a lower-ranked track. Downgrading and grade repetition occur frequently during secondary education. Many students start in the academic track and downgrade to the technical or vocational track (Cockx and Picchio, 2019). By age 15, 24.3% and 46.1% of students have already repeated a grade in primary or secondary education, respectively in the Flemish and French Community (De Witte and Hindriks, 2018).

There is no general central admission exam to higher education. Therefore graduation in secondary education (including the seventh grade in the vocational track) is sufficient for admission to almost all programs.[11] The right panel of Figure 1 visualizes the structure of higher education in Belgium. When students make their enrollment decision, they can choose between a professional and an academic bachelor's program. Professional bachelor programs are more practically oriented and directly prepare for the labor market. This program leads to a bachelor's degree after three years. Professional bachelor programs were traditionally offered by colleges, but recently an increasing share of colleges is integrated into universities. Academic programs are more theoretical. The first three years lead to a bachelor's degree without directly preparing for the labor market. They rather prepare for the corresponding academic master, lasting one or two years, depending on the study program. The academic year starts in September and usually ends by the end of June. Students who fail exams are offered a retake opportunity in Summer.

2.2. The Activation Allowance for Youth

In Belgium, education-leavers are eligible for non-means-tested unemployment insurance if they satisfy the age and educational requirements. This "activation allowance" can be claimed not earlier than one year after their first registration as job seeker or after the start of their first job. The activation allowance is a flat-rate benefit, the level of which depends on age, and household composition. In 2015, the year of the policy reform, education-leavers living on their own without dependents were entitled to a monthly benefit of 494 euro under the age of 21 and 811 euro above the age of 21.[12] However, in Belgium most unemployed education-leavers still live at their parents' in which case monthly allowance amounted to only 425 euro.[13] The average gross monthly wage of young labor market entrants was 1,830 euro for high school dropouts and 2,426 euro for university graduates. This corresponds to an average replacement rate of 23% and 17.5%, respectively.[14] Since January 2012, a time limit was set on claiming the activation allowance. For non-heads of households (such as education-leavers still living at their parents') with household income above a certain threshold, the limit was set to three years, independently of the age. For all other education-leavers, this time limit was set only from the age of 30 onwards.

Education-leavers can claim the activation allowance after completing a waiting period that starts when they register as job seekers or start 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. To be eligible for the activation

[11] The regional governments impose entry exams for only very few programs: medicine at university and some artistic programs at college. The French Community also organizes an entry exam for engineering programs at universities.
[12] https://www.rva.be/nl/documentatie/baremas/inschakelingsuitkeringen
[13] 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.
[14] Average wage for 2016: see https://www.jobat.be/nl/art/wat-is-een-realistisch-eerste-loon
[15] For these school-leavers registering in July the waiting period starts only on August 1.

allowance before 2015, education-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 means-tested welfare benefits. Since the majority of the education-leavers live with their parents, they generally do not qualify for these welfare benefits. Therefore, most education-leavers who lose the eligibility for the activation allowance, lose the full amount of the benefit. Those who do not, are subjected to the stigma of the means test. We lack statistics on the fraction of education-leavers entitled to means-tested welfare when denied the activation allowance. However, given the positive correlation between family income and the educational attainment of children, one can expect that high school students in the vocational and technical track are more likely entitled than bachelor's or master's students.

2.3. The Policy Reform

On December 31, 2014, the Belgian government signed an agreement to strengthen the conditions for claiming the activation allowance from January 1, 2015. The main aim of this reform was to enhance young people's work incentives. It is unlikely that this reform has 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 the press before its implementation. Moreover, the timing came as a complete surprise as the reform was not on the agenda of the Council of Ministers on December 31, 2014, while it came into force the day after.

Figure 2. A Schematic Overview of the Two-Part Policy Reform

Requirement (at the timing of claiming UI)	**Before the reform**	**After the reform**	**Implementation date**
1. Age	< 30	< 25	1 January 2015
2. Education	General track: Completed the 6th year of HS Other tracks: Completed the 3rd year of HS	< 21: Successful completion of HS ≥ 21 and < 25: Same weaker requirements as before the reform	1 September 2015

Note: HS = High school

The government agreement involved two major changes in the eligibility conditions for the activation allowance. Figure 2 provides a schematic overview of the implied changes. First, as of January 1, 2015, education-leavers aged 25 or older are no longer entitled to the activation allowance.[16] Before this date, this age threshold was 30. Because education-leavers have to be registered as a 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,

[16] The entitlement to a claim expires after three years for claimants who are not heads of the household, but only if the household income exceeds a certain threshold; other claimants remain entitled until their 33rd anniversary.

starting from September 2015,[17] high school dropouts can no longer claim the activation allowance before the age of 21.[18] Before the reform, dropouts 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. Although the pre-reform conditions for claiming the activation allowance differed between tracks, for high school dropouts the same age threshold at 21 was implemented after the reform irrespectively of the track.

3. Methodology

If students are forward-looking, well-informed, and consider the possibility of not being entitled to the activation allowance in the future, the policy reform may alter their behavior and therefore influence degree completion and dropout. To evaluate the impact of the two-part reform on educational attainment, we use a difference-in-differences approach and compare degree completion and dropout before and after the reform between students who were affected by the reform and students in a control group. This section first discusses how the treatment and control groups are defined and the different incentives students have according to their age on December 31 from the academic year 2014-2015. Next, we present the difference-in-differences model used to evaluate the impact of the reform.

3.1. Determination of Treatment and Control Groups

The First Part of the Policy Reform: Suppressing the Entitlement above Age 25

The first part of the 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 start claiming the activation allowance after the age of 25.[19] Because of the waiting period of one year, this implied that enrolled students aged 24 or more on December 31, 2014, suddenly lost their entitlement. Also, students enrolled at the age of 24 in subsequent academic years were not eligible anymore for the activation allowance. To the extent that educational attainment reduces the likelihood of unemployment, the reform incentivizes these students to pass their exams and not to stop their studies prematurely. In this way, they avoid the risk of income loss induced by the reform.

Students aged 23 on December 31 face different incentives induced by the policy reform depending on the timing of birth within the year. Those born before August 1 can only avoid the eligibility loss induced by the reform by ending their studies before their 24th anniversary and, hence by dropping out before completing their degree. Students born after July 31 remain eligible if they finish the academic year, as long as they register as a job seeker or find a job before their 24th birthday. Therefore, this last group has an additional incentive to complete their degree in the current academic year to retain eligibility for the activation allowance. Because of these contrasting incentives

[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.

[19] The age threshold of 25 refers to the age when job seekers start claiming the activation allowance. Job seekers who start claiming the activation allowance before their 25th birthday, and, hence, must have found a job or registered as a job seeker before their 24th birthday, remain eligible for at least 3 years. The reform did therefore not apply to this group.

depending on the month of birth and data are only available by birth year, we do not consider this age group in the analysis.

Finally, students aged 22 on December 31 are only affected by the reform in the subsequent academic year when this group obtains the features of the 23-year-old students. In sum, we consider only Belgian students aged 22 and 24 on December 31 of the academic years for the analysis.

We limit our main analysis to the sample of students with Belgian nationality because foreign students are less informed about the Belgian entitlement rules, notably because these rules are not relevant when they return to their home country. We use the sample of non-Belgian students for a placebo analysis: Finding a small and not statistically significant treatment effect for this group corroborates the validity of our identification strategy.

The Second Part of the Policy Reform: Suppressing the Entitlement to High School Dropouts

The second policy reform affected students enrolled in secondary education from the academic year 2014-2015. From September 1, 2015 education-leavers who did not successfully complete secondary education are not eligible for the activation allowance before their 21st birthday. This reform therefore differently affected students in the last year of compulsory secondary education according to their age.[20]

First, students aged 18 or younger on December 31 cannot claim the activation allowance before their 21st birthday if they do not graduate from high school. For this group of high school dropouts, the reform implied an extension of the waiting period until their 21st birthday.

Second, students aged 19 on December 31 face different incentives according to their month of birth. Students born after August 31 face similar incentives as the previous group because the end of their waiting period is postponed until their 21st birthday. However, the impact of the reform is smaller than for the first group, 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. Therefore, we do not consider these students as part of the treatment group. Next, students born before September 1 are not affected by the policy reform because they start their waiting period after their 20th birthday. We consider the group of students aged 19 on December 31 and born before September 1 as part of the control group for the Flemish Community. However, because the data are only available by birth year for the French Community, we cannot consider the group of 19-year-old students in the analysis for the French Community.

Finally, students aged 20 or more on December 31 are not affected by the policy reform and can start claiming the activation allowance after a one-year waiting period even if they did not graduate from high school. In sum, we consider only students aged 18 and 20 on December 31 of the academic years retained for the analysis for the French Community while we also consider the group of students aged 19 and born before September for the analysis for the Flemish Community. For students aged 18, the waiting period is extended until their 21st birthday.

[20] 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 for this group at the margin of graduation the incentives induced by the reform are stronger than for the other groups.

Figure 1 visualizes the selected treatment and control groups for the evaluation of the two-part reform.

3.2. Difference-in-Differences Model

To estimate the causal impact of the reform on dropout and degree completion in secondary and higher education, we make use of a difference-in-differences approach. We contrast the evolution of degree completion and dropout between students who were affected, separately by each part of the reform, and students who were not affected. Let Y_{it} denote the outcome of interest (degree completion or dropout) of student i at the end of year t, with t=2011, 2012, 2013 for the control period and t=2014, 2015 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:

$$Y_{it} = \alpha + \sum_{2013 \neq s=2011}^{2015} (\beta_s T_{st} + \delta_s D_i T_{st}) + \gamma D_i + \varepsilon_{it},$$

With T_{st} an indicator for the year in which the outcome is measured ($T_{st} = 1$ for $s = t$ and $T_{st} = 0$ for $s \neq t$), $D_i = 1$ if individual i belongs to the treatment group and $D_i = 0$ otherwise. α is a constant term measuring the average outcome of the reference individual (member of the control group for $s = 2013$). β_s correspond to time fixed effects that capture the time evolution of the outcome in the control group, and γ measures the effect of belonging to the treatment group (i.e. the age cohort affected by the reform) on the outcome. δ_s is the difference-in-differences estimator. For $s > 2013$ δ_s measures the average causal impact of the reform on the treatment group in year s, i.e. δ_s measures how much degree completion or dropout of treated students increased or decreased after the reform in the treatment group relative to the counterfactual of no reform. In the benchmark model, we set $\delta_{2014} = \delta_{2015}$, yet implement the analysis separately for the Flemish and French Community so as to take differences in the institutional setting and data sources into account. In a heterogeneity analysis we subsequently allow the treatment effect to differ between periods, i.e. $\delta_{2014} \neq \delta_{2015}$, and between other dimensions, such as the study program, the gender of the students, and, for the Flemish Community only, the family income and, for secondary school only, whether Dutch is spoken at home or not.

Because not all students are at risk of long-term unemployment, and, hence, of losing the activation allowance, estimated treatment effects have no clear interpretation. To get a better sense of the effect size, we follow Hernaes et al. (2017) by dividing δ_s by the exposure risk of losing eligibility for the activation allowance one year after ending full-time education.[21] This allows to scale the treatment effect to the size it would have in case the exposure risk would be 100%, which is therefore close to an "average treatment effect on the treated" (ATET).

For $s \leq 2013$, δ_s measures the placebo impact of the reform in the pre-treatment period. The parallel trend assumption is tested by the following joint null hypothesis: $\forall s \leq 2013$: $\delta_s = 0$. We report in each table the p-value of this joint test, but also assess the parallel trend assumption graphically by plotting both the predicted outcome of the treated group in the pre-treatment period, i.e. $\hat{\alpha} + \hat{\gamma} +$

[21] Hernaes et al. (2017) estimate *individual* exposure risks based on pre-treatment data not used in their causal analysis. This is not possible with our data. We therefore proxy this approach by using an aggregate estimate of this exposure risk for graduates in higher education; not for high school graduates, because the causal impact of the reform is found to be not statistically different from zero.

$\sum_{s=2011}^{2012}(\hat{\beta}_s + \hat{\delta}_s)T_{st}$ for $s \leq 2013$, relative to its prediction in the counterfactual of no treatment, i.e. the same prediction after setting $\hat{\delta}_s = 0$: $\hat{\alpha} + \hat{\gamma} + \sum_{s=2011}^{2012} \hat{\beta}_s T_{st}$ for $s \leq 2013$. We also construct the 95% confidence interval around this predicted counterfactual, so that (the absence of) a rejection of the parallel trend assumption in a particular year is visualized in case the predicted outcome falls (inside) outside this interval. The differences between the corresponding predictions in the post-treatment period, i.e. for $s > 2013$, visualize the significance of the average treatment effects in each post-treatment year, and its significance by the location of the prediction relative to the confidence interval of the predicted counterfactual outcome.

Table A1 and A2 in Appendix show that some of the background characteristics of the enrolled treatment and control student populations evolve differently over time between the pre-treatment period (2010-2013) and the first post-treatment academic year starting in 2014, especially in the high school populations and in the population enrolled in higher education in the French Community. The temporal variation until 2014 must have been caused by group-specific shocks or trends that are exogenous to the policy reform, because, as mentioned in Section 2.3, the policy was announced at the end of the year after the enrollment decisions. We fix this by applying a conditional difference-in-differences estimator and make the control units comparable to the treated by inverse probability weighting as proposed by Horvitz and Thompson (1952) and Hirano et al. (2003), and as implemented e.g. by Albanese and Cockx (2019). More precisely, we weigh the observations in the treatment and control groups in the years before and after the reform so that they exactly match the observations in the treatment group in the first year after the reform. Standard errors are calculated using 500 bootstrap replications. Nevertheless, as illustrated for the benchmark model in Table A3 and A5 in Appendix, not taking these compositional shocks into account does not yield very different results, so these group-specific compositional shocks do not seem to be important.

4. Data

To examine the impact of the two-part policy reform on degree completion and dropout, we make use of administrative grouped population data of secondary and higher education provided for this study by the Ministry of Education of both the Flemish and French Community in Belgium. For secondary education, we include only students enrolled in the last grade of full-time technical and vocational secondary education because they are least likely to enroll in higher education. Students enrolled in part-time vocational education who combine studies with an apprenticeship are excluded because for this group there are special eligibility requirements for the activation allowance which cannot be verified with the available data. For higher education, we only retain students enrolled in full-time programs that lead to a professional bachelor's or master's degree, including one-year bachelor after bachelor programs in the Flemish Community, and one-year specialization programs in the French Community. We do not consider students in academic bachelor programs because almost all of these students subsequently enroll in the corresponding master's program. Enrollments of international students spending part of their program in Belgium (for example Erasmus exchange) are not included in these statistics. In contrast to the data for high school, these register data do not permit to isolate bachelor’s or master’s students in their final year; enrollments in higher education, therefore, refer to the global bachelor's or master's program enrollment.

We include three years before the policy reform (the academic year 2011-2012 until 2013-2014) and two years in the post-reform period. To our knowledge, there were during this period, except for the policy reforms studied in this paper, no other policy reforms.

The data are collected from four different administrative sources (higher education and secondary education in respectively the Flemish and French Communities) and therefore contain different explanatory variables depending on the source. Data are grouped by year, age, gender, and study program (vocational or technical in high school, and professional bachelor's or master's in higher education), and, depending on the data source, also by nationality (higher education in the Flemish and French Community), socio-economic status[22] (observed in secondary and higher education in the Flemish Community), and an indicator equal to one when students speak a foreign language at home (secondary education in the Flemish Community).

For each specific group, we observe the number of enrolled students (in the final grade for high school, but, as mentioned above, globally for those in higher education), the number of students that obtain a degree at the end of the academic year, and the number of dropouts.[23] Taking the ratios of the two latter numbers to the first one defines the two outcomes of interest, i.e. the degree completion and dropout rate. Because for higher education only global program enrollments are measured – over all three years for the bachelor's and, depending on the discipline, one or two years for the master's – the degree completion rates are driven downwards as the denominators also include students enrolled in the pre-final years for whom degree completion is theoretically impossible. We further discuss this point when discussing the results in Section 5.1. Finally, note that the treatment effect on the dropout rate is not necessarily the same. This only happens if the impact on the residual category is zero. This residual category consists of enrolled students who 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, as for high school we retained 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 (Angrist and Pischke, 2013, p. 40), 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 drops out and zero otherwise.[24]

5. Empirical Results

In this section, we report and discuss the impacts of the two-part policy reform on degree completion and dropout. We first consider the effects of imposing the age eligibility requirement of the activation allowance from age 25 onwards in higher education. Next, we consider the effects of imposing the qualification requirement on high school students. We present both a graphical and corresponding econometric analysis of the difference-in-differences models. For both educational levels, we first present the global effects over study programs (professional bachelor's and master's for higher education, and vocational and technical education for high school), the two post-treatment years (2014 and 2015), and the available background characteristics of the students, but allow for different

[22] 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. Such a grant is only awarded to low-income families.
[23] Due to privacy issues, we do not observe enrollment, degree completion and dropout if the number of enrolled students is smaller than five.
[24] Note that this expansion to the individual level is only possible because of the binary nature of the outcome variables.

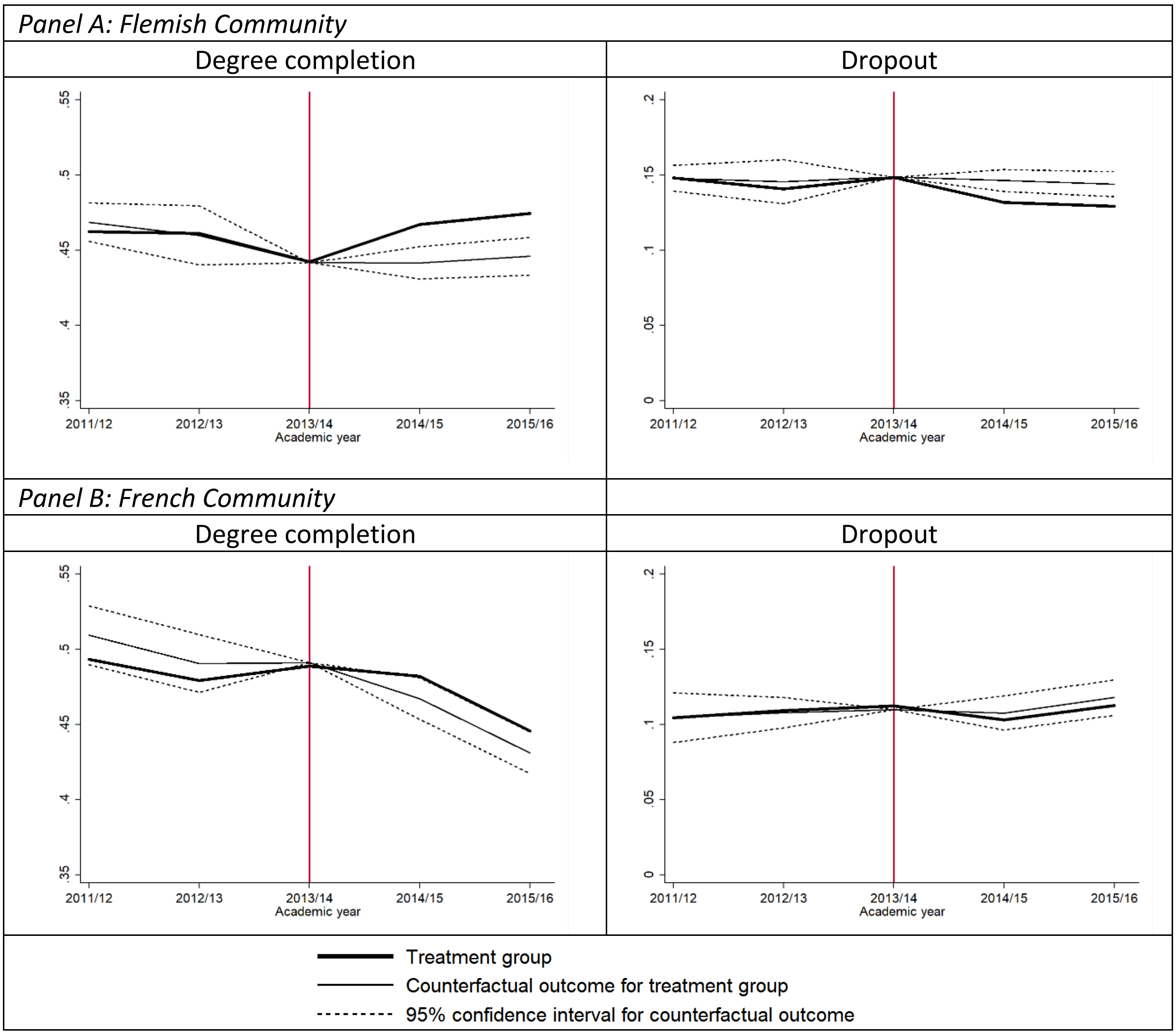
effects of the reform according to the language community. As we find no significantly different effects between the Flemish and French Communities, we subsequently impose equality of the effect in this dimension and study the extent of effect heterogeneity in the other mentioned dimensions.

5.1. Higher Education

Global Treatment Effects of the Age Eligibility Requirement in Higher Education

Figure 3 compares degree completion and dropout for students enrolled in higher education in the treatment and control groups in the Flemish Community (Panel A) and the French Community (Panel B). The vertical line is drawn in the last year of the control period. Treated students are 24 years old and students in the control group are 22 years old. The thick solid line shows the model predictions for degree completion and dropout for students in the treatment group. 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.

Figure 3. Degree Completion and Dropout of Belgian Students in Higher Education



Note: Treatment group = 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 as predicted by a weighted difference-in-differences regression using 500 bootstrap replications. The thin dotted lines are the 95% confidence intervals of the counterfactual path.

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 the four graphs, we can see that the observed outcome of the treatment group remains within the 95% confidence interval of the predicted counterfactual outcome of the treatment group before the policy reform. This suggests that the parallel trends assumption is not rejected for either outcome variable. 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 both in the Flemish and French Communities. This indicates that the policy reform increased degree completion in the treatment group. A reversed pattern can be observed for unsuccessful dropout in higher education. Before the policy reform, dropout follows a similar trend in the treatment and control groups. After the policy reform, dropout falls below the predicted counterfactual both in the Flemish and French Communities, but it decreases below the 95% confidence interval of the predicted counterfactual in the Flemish Community only.

Table 1 presents the output of the conditional difference-in-differences models for degree completion and dropout for Belgian students. 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. Because of the different sets of control variables that are used to estimate the propensity scores in the weighted difference-in-differences models, we estimate separate regressions for both regions. The treatment effects for the Flemish and French Communities are reported in panels B and C. As we cannot reject at the 5% level that treatment effects are similar in the Flemish and French Communities (First test of panel D), we restrict the treatment effects to be the same in both regions in panel A. The p-values of the placebo tests on the parallel trends assumption are stated in panel D and show that the parallel trends assumption is never rejected in either region.

The first panel reports that degree completion of the treated students has increased significantly by 2.2 percentage points after the policy reform relative to degree completion in the control group. In absence of the treatment, 44.6% of students in the treatment group would have obtained a degree at the end of the academic year. The policy reform raises it to 46.8%, a proportional increase of 4.9%. However, as mentioned above, this fraction underestimates the share relative to the number of enrollments in the final year, because enrollments also include students in the prefinal years. Based on aggregate statistics in the pre-treatment year 2013-2014 available for the Flemish Community only, we estimate the fraction of final year enrollments in higher education to be 57.6%,[25] and the corresponding counterfactual degree completion rates for final year students 77.4% (=44.6%/0.576). This means that the reform increased the degree completion rate of final year enrollments by 2.2pp/0.774 = 2.8 pp.

As explained in Section 3.2, to get a better sense of the effect size, we divide the estimated treatment effect by the exposure risk of losing eligibility for the activation allowance one year after ending full-time education. Based on administrative data from the Cross Roads Bank of Social Security to which we have access for another research project, we could calculate the exposure risk of the cohort of 23-

[25] Based on aggregate statistics obtained from the Ministry of Education in the Flemish Community the fraction of final year enrollments of 24-year-old students the academic year 2013-2014 is 44.3% in professional bachelor's and 69% in master's. Since in our data the share of students in bachelor and master programs is respectively 46% and 54%, the fraction of final year enrolments in higher education is 44.3%*0.46+69.0%*0.54 = 57.6%.

year-old master's students who graduated in 2012 to be about 10%: 586 out of 5907 graduates claimed the activation allowance at the end of the one-year waiting period in the course of 2013. These data do not allow to calculate the exposure risk of the graduates of a professional bachelor's. However, based on data that we have for the Flemish Community only, there is evidence that this exposure risk is very similar for this group.[26] Since this exposure risk is estimated for graduates, we should apply it to the final year students who obtained the degree only. This yields an estimate for the ATET on the degree completion rate in higher education of about 28 pp (= 2.8pp/0.10). To compare, Hernaes et al. estimate the ATET of the activity requirements in the Norwegian welfare system on high school completion to be 17.0 pp.

Table 1. Degree Completion and Dropout of Belgian Students in Higher Education

	Degree	Dropout
Panel A: All students		
Treatment effect	0.022***	-0.011***
	(0.004)	(0.003)
Counterfactual outcome	0.446	0.131
Panel B: Flemish Community		
Treatment effect	0.028***	-0.015***
	(0.006)	(0.004)
Counterfactual outcome	0.444	0.145
Panel C: French Community		
Treatment effect	0.015**	-0.005
	(0.007)	(0.004)
Counterfactual outcome	0.448	0.113
Panel D: P-values of various test		
Equality of the effects in the Flemish and French Community	0.173	0.094
Parallel trends: Flemish Community	0.448	0.732
Parallel trends: French Community	0.347	0.986
Observations	252 009	252 009

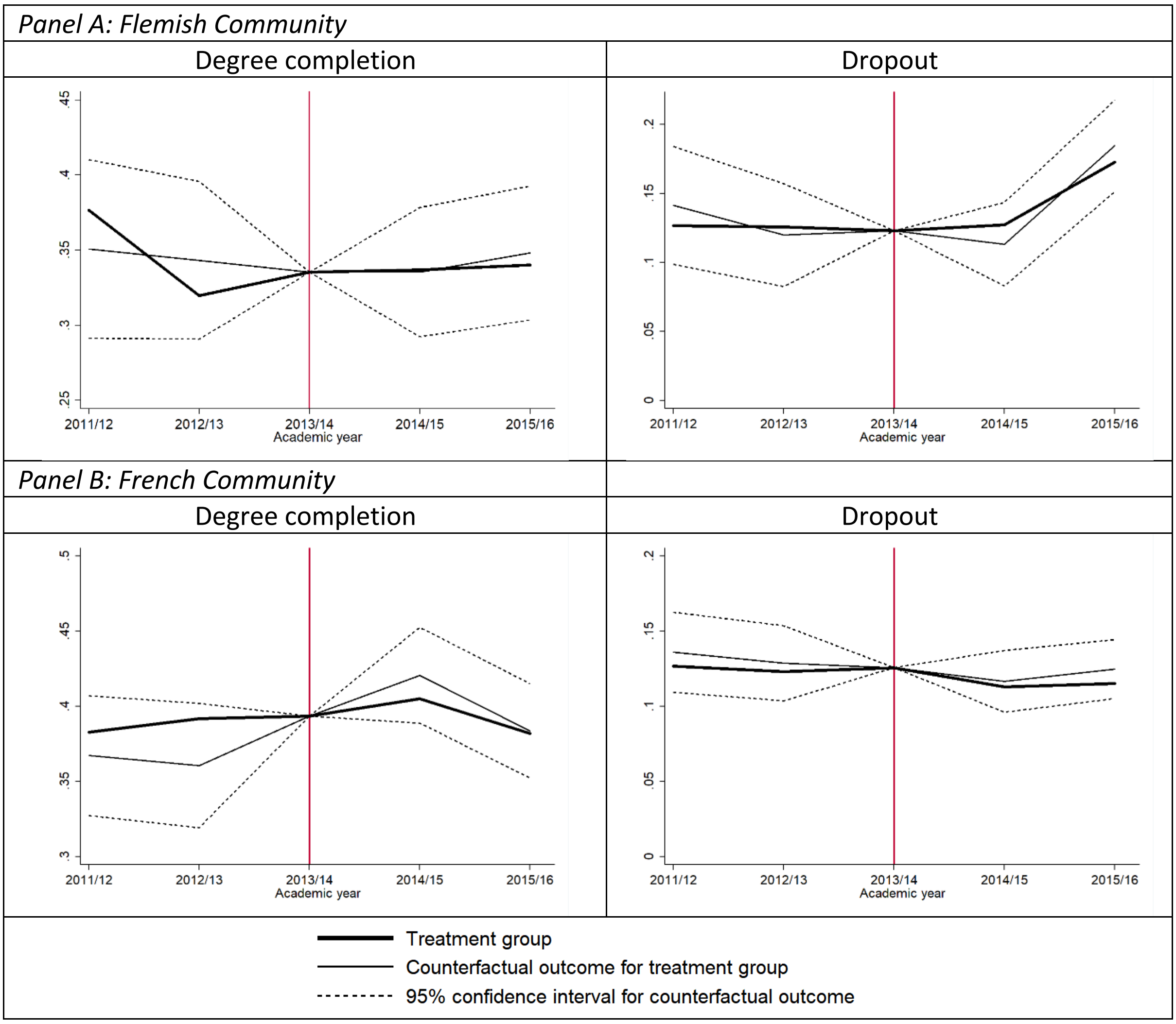
Note: Treatment group = 24-year-old students. Control group = 22-year-old students. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 until 2013-2014. Treatment period = 2014-2015 and 2015-2016. Results are based on weighted difference-in-differences regressions. The regressions allow for a different trend in both regions. The counterfactual outcome is the predicted outcome for the treated in the absence of the treatment in the post-reform periods. To test for parallel trends, we estimate separate regressions for both regions 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 for each region. The p-value of the test for similar effects in the Flemish and French Communities is reported. Standard errors are computed by 500 bootstrap replications. * p<0.10, ** p<0.05, *** p<0.01.

These are large effects. Yet, they are arguably an upper bound for the aforementioned effect on those in higher education because of the following two reasons. First, the objective exposure risk is a lower bound since only graduates from master programs are considered in the calculation of the exposure

[26] The Flemish public employment service (VDAB) reports each year by educational attainment the fraction of education-leavers who on June 30 of the year after they left are registered as unemployed job seekers. Because this registration is approximately one year after education is left, this moment coincides approximately with the moment at which they should become eligible for the activation allowance. Based on these data it can be found that the fraction of education leavers in 2013 that is registered as unemployed job seekers on June 30, 2014, is 6.7% and 7.2%, respectively for those with at most a professional bachelor's or a master's degree (VDAB, 2015).

risk and not dropouts for whom we expect a larger risk of still being unemployed one year after leaving higher education. Second, individuals base reactions on the *perceived* exposure risk rather than on the *objective* exposure risk. As mentioned in the Introduction, recent evidence suggests that the highly educated overestimate their chances to be long-term unemployed, and, hence, their exposure risk (Mueller et al. 2021).

Figure 4. Placebo: Degree Completion and Dropout of Non-Belgian Students in Higher Education



Note: Treatment group = 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 as predicted by a weighted difference-in-difference regression using 500 bootstrap replications. The thin dotted lines are the 95% confidence intervals of the counterfactual path.

The second regression shows that the policy reform significantly reduced the dropout rate in higher education by 1.1 pp. In absence of the policy reform, 13.1% of all treated students would have dropped out at the end of the academic year without a degree. When dividing this effect by the aforementioned exposure risk, we obtain an ATET of 11 pp: If all enrolled students (and not only those in the final year) are affected by the policy reform, dropout decreases by 11 pp. The second and third panel of Table 1 show that treatment effects are smaller in the French Community than in the Flemish Community. While we still find a significant increase in degree completion in the Flemish Community, the effect on dropout, although negative, turns out to be not significantly different from zero. Table A3 in Appendix shows the corresponding unweighted difference-in-differences estimates and yields similar conclusions.

Table 2. Degree Completion and Dropout of Belgian Students in Higher Education (Heterogeneous Effects)

	Degree	Dropout
Panel A: Different effects over time		
Academic year 2014-2015	0.021***	-0.010***
	(0.006)	(0.004)
Academic year 2015-2016 *(in deviation from 2014-2015)*	0.002	-0.001
	(0.006)	(0.004)
Panel B: Study programs		
Master	0.030***	-0.007**
	(0.006)	(0.003)
Professional bachelor *(in deviation from Master)*	-0.017**	-0.009
	(0.007)	(0.005)
Panel C: Gender		
Male	0.024***	-0.018***
	(0.006)	(0.004)
Female *(in deviation from Male)*	-0.004	0.014**
	(0.006)	(0.004)
Panel D: Household income (Flemish Community)		
Low income	0.025**	-0.012
	(0.013)	(0.009)
High income *(in deviation from low income)*	0.004	-0.004
	(0.007)	(0.005)

Note: Treatment effects are reported for the first group followed by the difference in treatment effects between the second and first group. Treatment group = 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 and 2015-2016. Results are based on weighted difference-in-difference regressions. The regressions allow for a different trend in both regions and both groups. Standard errors for the treatment effects are computed by 500 bootstrap replications. * p<0.10, ** p<0.05, *** p<0.01. Household income is proxied by an indicator variable equal to one for low-income families if a student received a study grant.

Finally, we conducted a placebo analysis by limiting the analysis to non-Belgian students. After their studies, most foreign students will return to their home country and the reform is therefore not relevant for these students. Figure 4 shows the corresponding graphs for degree completion and dropout of non-Belgian students and Table A4 in the Appendix shows the treatment effects. While the reform has a clear effect on degree completion and dropout for Belgian students, we do not find a significant effect for students of another nationality. The magnitude of the treatment effects is also closer to zero for non-Belgian students suggesting that the non-significance is not driven by the smaller sample size of this group.

Treatment Heterogeneity

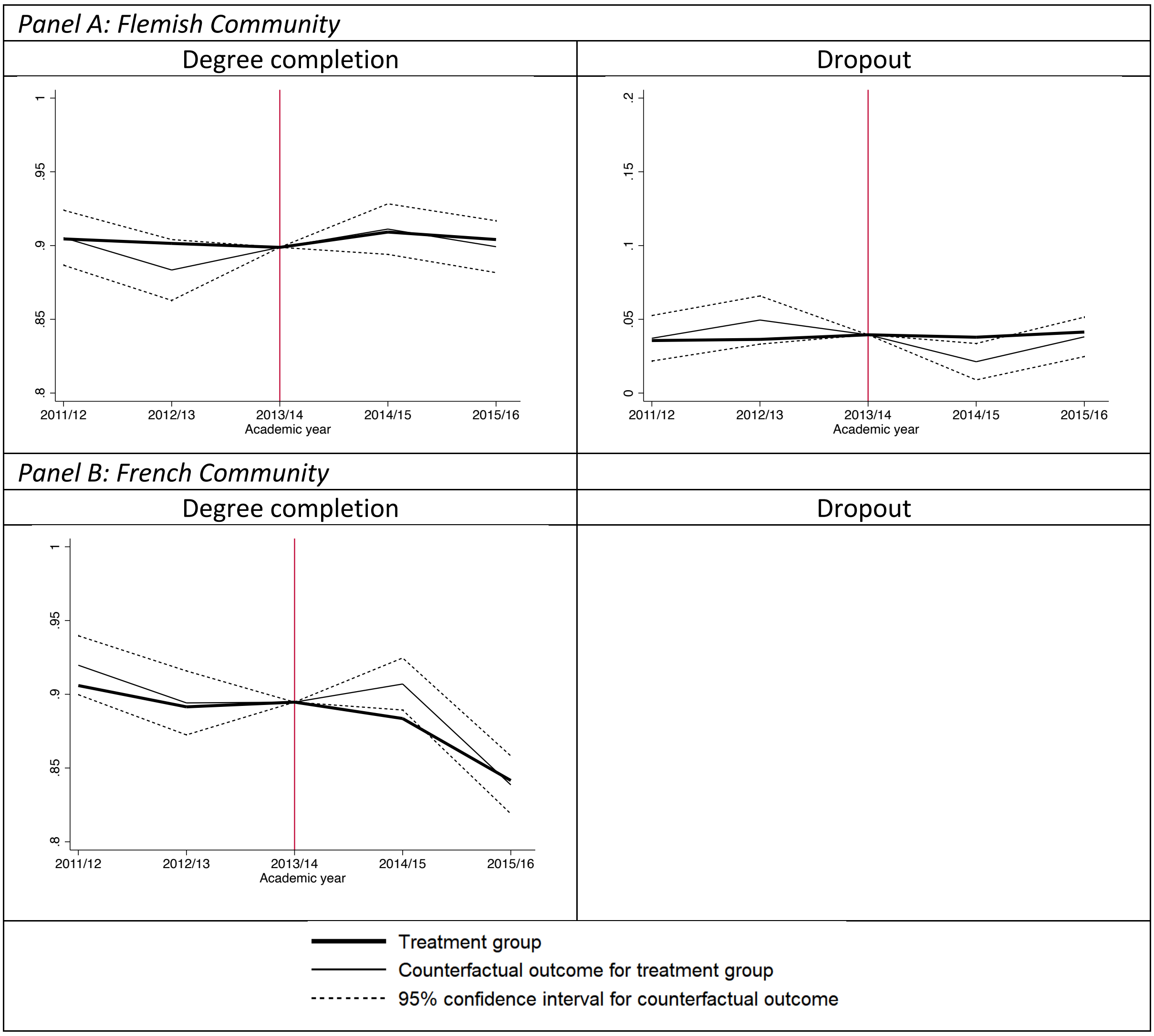
Table 2 analyses the heterogeneity in the treatment effects over time, according to the study program, gender, and household income. This last variable is observed in the Flemish Community only. We find that male students turn out to be significantly less likely to drop out of higher education after the policy reform than female students, but they are not significantly more likely to obtain a degree. Our results also suggest that students in master's programs are more likely to obtain their degrees than students in bachelor's programs.

5.2. Secondary Education

Global Treatment Effects in Secondary Education

The second policy reform could have affected study outcomes in secondary education because as of September 2015, education-leavers under the age of 21 could claim the activation allowance only after successfully completing the last year of secondary education. Figure 5 compares degree completion and dropout in the final year of secondary education for students in the treatment and control group in the Flemish and French Communities. Notice that we do not have data on dropout in the French Community. Treated students are 18 years old and students in the control group are 20 years old. In addition, the control group for the Flemish Community also comprises 19-year-old students born before September. Throughout the pre-treatment period, the 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 is evidence that the parallel trends assumption is not rejected. In 2014/15, the first post-reform schoolyear, the dropout rate exceeds the 95% confidence interval in the Flemish Community, and the degree completion rate falls below this interval, but only in the French Community. Everywhere else the observed and counterfactual outcomes lie very close to each other. The former observations seem to suggest that the reform temporarily – only in the first post-reform schoolyear – *decreased* degree completion, but only in the French Community, and that it also temporarily *increased* the dropout rate in the Flemish Community, while we ignore what happened with the dropout in the French Community. This is very counterintuitive, and we cannot think of a mechanism that could explain these findings. Moreover, as these outcomes fluctuate much more for the control group than for the treatment group, also in the pre-reform period, this suggests that these findings are caused by temporary region and control group-specific shocks rather than by behavioral reactions of the treated group. This larger variability of the outcomes of the control group may also be related to the relatively small sample size: The sample size of the control group is only about 40% of that of the treated group (Table A2). All these arguments point to concluding that the significant findings are spurious. This interpretation is reinforced in the evidence from the formal regressions reported in Table 3 in which we impose the treatment effects to be equal in the two post-treatment years. Reported treatment effects are close to zero, and none of them are significant at the 5% level.

Figure 5. Degree Completion and Dropout in the Final Year in Secondary Education



Note: Treatment group = 18-year-old students. Control group = 20-year-old students and 19-year-old students born before September 1 in the Flemish Community and 20-year-old students in the French Community. 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 as predicted by a weighted difference-in-difference regression using 500 bootstrap replications. The thin dotted lines are the 95% confidence intervals of the counterfactual path.

Table 3. Degree Completion and Dropout in the Final Year of Secondary Education

	Degree	Dropout
All students		
Treatment effect	-0.004	-
	(0.005)	-
Counterfactual outcome	0.894	-
Flemish Community		
Treatment effect	0.002	0.010*
	(0.007)	(0.005)
Counterfactual outcome	0.905	0.030
French Community		
Treatment effect	-0.010	-
	(0.007)	-
Counterfactual outcome	0.872	-
P-values of various test		
Equality of the effects in the Flemish and French Community	0.239	-
Parallel trends: Flemish Community	0.144	0.262
Parallel trends: French Community	0.386	-
Observations	135507	79929

Note: Treatment group = 18-year-old students. Control group = 20-year-old students and 19-year-old students born before September 1 in the Flemish Community and 20-year-old students in the French Community. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 until 2013-2014. Treatment period = 2014-2015 and 2015-2016. Results are based on weighted difference-in-difference regressions. The regressions allow for a different trend in both regions. The counterfactual outcome is the predicted outcome for the treated in the 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 for each region. The p-value of the test for similar effects in the Flemish and French Communities is reported. Standard errors are computed by 500 bootstrap replications. * p<0.10, ** p<0.05, *** p<0.01.

Treatment Heterogeneity

Table 4 reports again the treatment heterogeneity in various dimensions. It confirms that the counterintuitive findings that the reform decreases the degree completion and increases the dropout are found in the first post-reform year only. In the second year after the reform, these counterintuitive effects disappear. The remaining panels of Table 4 show that there is no evidence that the reform would have affected the educational outcomes of any specific group. We therefore conclude that the reform did not have any significant impact on the educational outcomes in secondary education, and if any, only temporarily and in the opposite direction of what was intended.

Table 4. Degree Completion and Dropout in the Final Year of Secondary Education (Heterogeneous Effects, Weighted DiD)

	Degree	Dropout (Flemish Community)
Panel A: Different effects over time		
Academic year 2014-2015	-0.012**	0.017***
	(0.006)	(0.006)
Academic year 2015-2016 *(in deviation from 2014-2015)*	0.016**	-0.014*
	(0.007)	(0.007)
Panel B: Study programs		
Technical SE	-0.010	0.013*
	(0.006)	(0.007)
Vocational SE *(in deviation from technical SE)*	0.013	-0.008
	(0.008)	(0.011)
Panel C: Gender		
Male	-0.000	0.005
	(0.007)	(0.007)
Female *(in deviation from male)*	-0.007	0.011
	(0.007)	(0.008)
Panel D: Language at home (Flemish Community)		
Dutch	0.005	0.010
	(0.007)	(0.006)
Other language *(in deviation from Dutch)*	-0.021	-0.005
	(0.013)	(0.010)
Panel E: Household income (Flemish Community)		
Low income	-0.010	0.008
	(0.011)	(0.008)
High income *(in deviation from low income)*	0.018	0.002
	(0.008)	(0.006)

Note: Treatment effects are reported for the first group followed by the difference in treatment effects between the second and first group. Treatment group = 18-year-old students. Control group = 20-year-old students and 19-year-old students born before September 1 in the Flemish Community and 20-year-old students in the French Community. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 until 2013-2014. Treatment period = 2014-2015 and 2015-2016. Results are based on weighted difference-in-difference regressions. The regressions allow for a different trend in both regions and both groups. Standard errors for the treatment effects are computed by 500 bootstrap replications. * p<0.10, ** p<0.05, *** p<0.01. Household income is proxied by an indicator variable equal to one for low-income families if a student received a study grant.

5.3. Interpretation of Results

There are a couple of factors that may explain the fact that the reform did not positively affect the educational attainment of high school students, while it did enhance degree completion and reduce dropout in higher education to a large extent. First, the stake of the reform was lower for the high school students as the loss of entitlement was only temporary and lasted to the age of 21 only, while it was permanent for the older students in higher education. Second, the lower educated are more likely to be present biased (Becker and Mulligan, 1997; Sutter et al., 2013; Lavecchia et al. 2014;

Golsteyn et al., 2014), and third, more (over-)optimistic with respect to job finding (Mueller et al. 2021), which both implies lower responsiveness to a prospective income loss.

In contrast to our findings, Hernaes et al. (2017) find that the imposition of activity requirements in welfare does substantially enhance high school completion in Norway. One explanation is that the timing of the exposure risk is not as delayed in Norway as it is in Belgium, because in Norway the welfare benefit can be claimed immediately after leaving school, while the activation allowance in Belgium cannot be claimed earlier than one year later. The impact of present biased preferences is, therefore, stronger in Belgium than it is in Norway. Another explanation is that, in contrast to students in higher education, for high school students the risk of losing income is too abstract to comprehend, and therefore fails to trigger behavioral reactions. By contrast, the risk of being forced to engage in specific activities, as in Norway, is much more concrete. There is indeed some evidence that tying incentives to more concrete and easier-to-understand objectives than a financial loss works better in an educational environment (Fryer, 2011; Gneezy et al., 2011), but further research is required to confirm whether this is the crucial design feature that explains this differential effectiveness of prospective work incentives on the educational achievement of high school students.

The finding that prospective incentives can induce important behavioral reactions within an educational environment seems contradictory to numerous studies in behavioral economics that find that adolescents tend to be present biased and have difficulty in taking future awards into account when making decisions (see e.g. Lavecchia et al., 2014; Koch et al. 2015; Levitt et al., 2016). A potential explanation is that incentive schemes with very high stakes, as in our study and in that of Hernaes et al. (2017), make a crucial difference. Currently, evidence about high stake incentive schemes is scant. The study of Enke et al. (2021) is an exception in that it explicitly studies the impact of high stakes on behavioral biases. They find in a lab experiment that very high stakes are never sufficient to de-bias participants, but the context is different. For instance, they consider the effect of rewards framed as gains rather than as losses, as in this study. There is indeed a large literature that has found that individuals have reference-dependent preferences that exhibit loss aversion in line with Kahneman's and Tversky's (1979) prospect theory, which means that they respond more strongly to losses than to gains (see e.g. Hossain and List, 2012). Even if Levitt et al. (2016) do not find evidence for loss aversion in a field experiment in an educational context, their findings were based on relatively small stakes. Moreover, as discussed in this study, behavioral reactions may depend on cognitive abilities, and on whether incentives are monetary or not. More research is necessary to get a better understanding of the mechanisms that drive these different findings.

6. Conclusion

We studied whether prospective work incentives can have an impact on investments in human capital. In particular, we investigated whether scrapping the eligibility for UI one year after ending full-time education could enhance educational attainment. While for high school students we did not find any evidence for this, we did find that such a reform can significantly enhance degree completion rates and decrease dropout rates in higher education. When dividing our estimates by the exposure risk, we estimate an ATET of 28 pp on degree completion and -11 pp on dropout.

We explained that the stronger impact for students in higher education than for high school students could be related to the higher stake of the reform and to the fact that the higher educated may be affected by present bias, and be more pessimistic with respect to the chances of finding a job.

However, our finding that high school students do not react to such prospective work incentives, seems to contrast with the, to the best of our knowledge, only other available evidence. Hernaes et al. (2017) do find that imposing activity requirements on means-tested welfare benefits does significantly increase the high school completion rate in Norway. We proposed two explanations for this contrasting finding. One is that the present bias of preferences matters more in Belgium than in Norway, because in Norway the eligibility restrictions apply immediately after ending full-time education, while in Belgium they cannot be imposed earlier than one year later. Another explanation is that incentives work better for low-skilled youths if they are tied to more concrete and easier-to-understand objectives, such as activity requirements, than to a financial loss (Fryer, 2011; Gneezy et al., 2011). However, these explanations remain speculative and call for future research to confirm whether and which of these design features are crucial.

Finally, the analysis in this paper is limited to evaluating the impact of the reform 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's programs for bachelor graduates or the decision to start a second master's program. Consequently, this reform could also have decreased human capital investments in higher education. In further research, it would be interesting to follow students during several years in secondary and higher education in more specific study programs to get a more complete picture of the consequences of the reform on educational attainment.

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Appendix: Additional Tables and Figures

Table A1. Descriptive Statistics: Higher Education

	Pre-reform 2011/12 – 2013/14		Post-reform 2014/15	
	Control	Treated	Δ Control	Δ Treatment
Panel A: Flemish Community				
Male	45.7	47.4	-0.2	-0.3
Belgian	95.7	86.5	-0.7	-1.2
Study grant HE	20.2	18.0	-0.3	+0.1
Master	52.1	58.3	-3.0	-1.7*
Observations	67186	24355	24323	9606
Panel B: French Community				
Male	41.5	45.4	-0.3	-0.9
Belgian	84.3	74.1	-1.1	+1.3***
Master	41.7	58.7	-1.5	+0.8***
Observations	51367	22424	17991	8631

Note: Student characteristics in treatment and control groups before and after the policy reform. Outcomes in the first year of the post-reform period are expressed as percentage point changes relative to the average outcome of the pre-reform period. Treatment group = 24-year-old students. Control group = 22-year-old students. Age is measured on December 31 of the respective academic 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 in the column Δ Treatment.

Table A2. Descriptive Statistics: Final Year of Secondary Education

	Pre-reform 2011/12 – 2013/14		Post-reform 2014/15	
	Control	Treated	Δ Control	Δ Treatment
Panel A: Flemish Community				
Male	63.0	56.2	-1.9	-0.3
Dutch at home	73.1	89.9	-4.4	-2.3**
Study grant	39.3	30.3	+1.9	+2.3
TSO	49.3	52.6	-1.3	-1.2
Observations	10375	37364	3284	12781
Panel D: French Community				
Male	56.8	47.4	+0.5	+1.1
TSO	55.9	67.9	-2.3	+0.8***
Observations	9563	23359	3253	7961

Note: Student characteristics in treatment and control groups before and after the policy reform. Outcomes in the first year of the post-reform period are expressed as percentage point changes relative to the average outcome of the pre-reform period. Treatment group = 18-year-old students. Control group = 20-year-old students and 19-year-old students born before September 1 in the Flemish Community and 20-year-old students in the French Community. Age is measured on December 31 of the respective academic 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 in the column Δ Treatment.

Table A3. Degree Completion and Dropout of Belgian Students in Higher Education (Unweighted DiD)

	Degree	Dropout
All students		
Treatment effect	0.021***	-0.011***
	(0.004)	(0.003)
Counterfactual outcome	0.446	0.132
Flemish Community		
Treatment effect	0.028***	-0.017***
	(0.006)	(0.004)
Counterfactual outcome	0.443	0.147
French Community		
Treatment effect	0.012*	-0.004
	(0.007)	(0.004)
Counterfactual outcome	0.451	0.112
P-values of various test		
Equality of the effects in the Flemish and French Community	0.077	0.026
Parallel trends: Flemish Community	0.533	0.661
Parallel trends: French Community	0.198	0.989
Observations	252009	252009

Note: Treatment group = 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 and 2015-2016. Results are based on unweighted difference-in-difference regressions. The regressions include time dummies and region-specific constants. The counterfactual outcome is the predicted outcome for the treated in the absence of the treatment in the post-reform periods. To test for parallel trends, we estimate separate regressions for both regions 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 for each region. The p-value of the test for similar effects in the Flemish and French Communities is reported. Standard errors are computed by 500 bootstrap replications. * p<0.10, ** p<0.05, *** p<0.01.

Table A4. Placebo: Degree Completion and Dropout of Non-Belgian Students in Higher Education

	Degree	Dropout
Panel A: All students		
Treatment effect	-0.008	-0.003
	(0.011)	(0.007)
Counterfactual outcome	0.379	0.132
Panel B: Flemish Community		
Treatment effect	-0.001	-0.000
	(0.018)	(0.013)
Counterfactual outcome	0.342	0.149
Panel C: French Community		
Treatment effect	-0.011	-0.004
	(0.013)	(0.009)
Counterfactual outcome	0.401	0.121
Panel D: P-values of various test		
Equality of the effects in the Flemish and French Community	0.640	0.802
Parallel trends: Flemish Community	0.327	0.669
Parallel trends: French Community	0.341	0.736
Observations	35 909	35 909

Note: Treatment group = 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 and 2015-2016. Results are based on weighted difference-in-differences regressions. The regressions allow for a different trend in both regions. The counterfactual outcome is the predicted outcome for the treated in the absence of the treatment in the post-reform periods. To test for parallel trends, we estimate separate regressions for both regions 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 for each region. The p-value of the test for similar effects in the Flemish and French Communities is reported. Standard errors are computed by 500 bootstrap replications. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A5. Degree Completion and Dropout in the Final Year of Secondary Education (Unweighted DiD)

	Degree	Dropout
All students		
Treatment effect	-0.004	-
	(0.004)	-
Counterfactual outcome	0.894	-
Flemish Community		
Treatment effect	-0.003	0.008**
	(0.006)	(0.004)
Counterfactual outcome	0.909	0.031
French Community		
Treatment effect	-0.006	-
	(0.006)	-
Counterfactual outcome	0.868	-
P-values of various test		-
Equality of the effects in the Flemish and French Community	0.731	
Parallel trends: Flemish Community	0.010	0.068
Parallel trends: French Community	0.445	-
Observations	135507	79929

Note: Treatment group = 18-year-old students. Control group = 19-year-old and born before September 1 and 20-year-old students in Flanders, 20-year-old students in French Community. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 until 2013-2014. Treatment period = 2014-2015 and 2015-2016. Results are based on unweighted difference-in-difference regressions. The regressions include time dummies and dummies for regions. The counterfactual outcome is the predicted outcome for the treated in the 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 for each region. The p-value of the test for similar effects in the Flemish and French Communities is reported. Standard errors are computed by 500 bootstrap replications. * p<0.10, ** p<0.05, *** p<0.01.