Tightening Eligibility Requirements for Unemployment Benefits. Impact on Educational Attainment

Bart Cockx, Koen Declercq and Muriel Dejemeppe[1]

April 2023

Abstract

Imposing stricter eligibility conditions on unemployment insurance (UI) may increase the returns to education investment because these make the consequences of unemployment more severe. In most countries, entitlement to regular UI hinges on completing a qualifying period of work and social contributions. In Belgium, this requirement also exists but is relaxed for education-leavers in that they can substitute time actively searching for a job for employment during the qualifying period. We evaluate the impact on degree completion and dropout of a 2015 reform that withdrew this weaker requirement from graduates aged 25 or over and from high school dropouts younger than 21. We find that the reform significantly increased degree completion and reduced dropout for students in higher education but not for those in high school. We argue that the higher prevalence of behavioral biases among the lower-educated could explain these contrasting findings.

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

Keywords: unemployment insurance, conditionality, degree completion, school dropout, behavioral biases

---

[1] Bart Cockx: Department of Economics, Ghent University, UCLouvain (IRES/LIDAM), IZA, CESifo, and ROA, Maastricht (bart.cockx@ugent.be), Koen Declercq: CEREC, UCLouvain – Saint-Louis Bruxelles (koen.declercq@usaintlouis.be), Muriel Dejemeppe: IRES/LIDAM, UCLouvain (muriel.dejemeppe@uclouvain.be). We are grateful to the editor and two anonymous reviewers for their constructive comments. We also thank Patrick Arni, Pierre Cahuc, Gerard van den Berg, and Bruno Van der Linden, an anonymous reviewer from the National Bank of Belgium, and participants at seminars at Maastricht University, KU Leuven, LISER, ULBrussels, the LEER conference in Leuven, the Belgian Day for Labor Economists in Maastricht, the Workshop on Labour and Family Economics in York, the EALE/SOLE conference in Berlin, and the COMPIE conference in Mannheim for fruitful discussions and comments. We acknowledge the National Bank of Belgium for funding this research, as well as the Ministries of Education of the Dutch- and French-speaking Communities of Belgium and ARES and the CRef for providing the data.

1. Introduction

In most countries, entitlement to regular unemployment insurance (UI) hinges on having completed a qualifying period of work and having made social contributions before the claim.[1] A consequence is that youths who do not acquire sufficient work experience after leaving education cannot claim unemployment benefits (UB). They then either remain financially dependent on their parents or are sustained by means-tested welfare benefits. This financial hardship during the qualifying period before UI eligibility incentivizes youths to search harder for a job and to accept job offers more quickly. An abundant literature has studied these effects of UI generosity on work incentives.[2] However, the anticipation of financial constraints could also affect human capital investment decisions. On the one hand, it could incentivize youths to invest in education because this shields them from the risk of unemployment during the qualifying period (Kesselman, 1976; Moffitt, 2002). On the other hand, there is substantial evidence that adolescents are present-biased and have difficulty taking future awards into account when making decisions (Lavecchia et al., 2014). Thus, there remains an empirical question of whether the prospect of tighter eligibility conditions for UI effectively changes the educational choices of youths. There has been remarkably little research on this question. This is surprising because the answer may have important consequences for the optimal design of UI. We provide empirical evidence based on a reform of a specific UI scheme for education-leavers in Belgium.[3]

In Belgium, to be eligible for regular UI individuals must have worked full time for 12 months within the last 21 months. However, education-leavers can claim UI under weaker conditions. They may substitute an active job search or part-time employment for full-time employment during the 12-month qualifying period. This is the so-called "activation allowance" financed by social security contributions from salaried employment, as are the regular UB. Periods of inactivity extend the qualifying period. Similar schemes exist in Denmark and Luxembourg, and in Sweden until 2007, but such schemes are the exception rather than the rule (OECD, 2011).[4]

In 2015, the Belgian government tightened the eligibility requirements for this more lenient UI for education-leavers. Youths aged 25 or over when submitting a benefit claim, as well as high-school dropouts younger than 21, were no longer entitled to the activation allowance. They could therefore only claim UI if they complied with the stricter qualifying conditions of the regular UI scheme. The 2015 reform induced exogenous variation in eligibility requirements by age and educational attainment. We exploit these in a difference-in-differences (DiD) approach to study the impact of tightening the eligibility conditions for UI on degree completion and dropout by students still in

[1] Australia and New Zealand are exceptions to this rule. In these countries, UI is provided immediately upon registration as a job seeker, but strict job-search requirements are imposed (Langenbucher, 2015).

[2] See, for example, Tatsiramos and Van Ours (2014) or Schmieder and von Wachter (2016) for reviews, and Le Barbanchon (2016) and Kolsrud et al. (2018) for more recent evidence. This literature reports on not only the effects of generosity on work incentives but also the consequences for job match quality. Some recent studies also show how the length of the UI qualifying period has an impact on separation decisions (Albanese et al., 2020; Martins, 2021) and on the *ex-ante* design of work contracts, i.e., the duration of fixed-term contracts (Brébion et al., 2022).

[3] Individuals leaving secondary or higher education with or without a degree are referred to as "education-leavers".

[4] Depending on age and educational attainment, education-leavers in Luxembourg are eligible for unemployment benefits after a qualifying period of six months (Luxembourg Employment Agency, 2019). In Denmark, all education-leavers who join an unemployment fund within two weeks of 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).

education. By being targeted at two different age groups, those 25 or older and those younger than 21, the reform allows us to estimate the behavioral reactions for students at two different levels of education, i.e., college/university and high school.

Our main findings can be summarized as follows. Tightening the eligibility requirements for UI significantly increased the rate of degree completion of Belgian students in higher education by 2.8 percentage points (pp) and reduced dropout 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) manages education with full autonomy, and we cannot reject the hypothesis that the reported effects of the 2015 reform are similar between these communities.

The existing empirical literature has mainly investigated whether programs that make work more rewarding hurt educational investment and not whether stricter eligibility criteria for social benefits can enhance educational attainment. Keane and Wolpin (2000), Riddell and Riddell (2014), and Blundell et al. (2016) provide causal evidence that financially rewarding work lowers educational attainment. Keane and Wolpin (2000) estimate a structural dynamic model of schooling, work, and occupational choice decisions over the life cycle. They show that introducing wage subsidies in the U.S. 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 of tax credits for lone-mother welfare recipients on labor supply and human capital accumulation over the life cycle in the UK. They find that tax credits increase labor supply and reduce the post-compulsory educational attainment of young women. Riddell and Riddell (2014) studied the impact of an activation program for welfare recipients on educational attainment. The program provided a generous earnings supplement paid out to long-term welfare recipients in case they returned to full-time employment. Based on a randomized controlled trial, this study 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 regarding the prediction that tightening the eligibility conditions for UI increases the returns to education. Most closely related to our research are the studies of Hernaes et al. (2017) and Cammeraat et al. (2022), which evaluate how stricter eligibility requirements for means-tested welfare benefits affect schooling outcomes.[5] Hernaes et al. (2017) find that imposing stricter eligibility criteria in the form of activation and work requirements for recipients of social assistance enhances degree completion and decreases dropout in secondary education in Norway.[6] They also show 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 enrollment in higher education for youths

[5] To the best of our knowledge, Miller and Saunders (1997) were 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-state 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 by Hernaes et al. (2017) and 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).

aged 25 and 26. They argue that the fact that this policy reform was introduced at the start of the Great Recession explains this null effect.

Imposing stricter eligibility conditions on UI is expected to have a different impact on educational attainment than similar initiatives for social assistance because of the distinct (i) nature of these schemes, (ii) timing of the qualifying activity, and because (iii) it induces a behavioral reaction in a different subpopulation. First, after the 2015 reform in Belgium, targeted education-leavers could no longer substitute job-search efforts for full-time employment to qualify for UI. This imposes a very different type of activity requirement than the social assistance schemes studied by Hernaes et al. (2017) and Cammeraat et al. (2022) in that employment is an activity that an individual cannot perfectly control. Finding a job and keeping it does not only depend on individual effort. It also partly hinges on the chances of being offered a job and of being retained in it. This distinction resembles the difference between assigning financial incentives (rewards or losses) to inputs (effort for an activity) or outputs (such as employment). There is evidence from behavioral education economics that financial incentives for educational inputs, such as reading a book or good behavior, are more effective than incentives for educational outputs (Fryer, 2011; Gneezy et al., 2011).

A second reason why we expect a different reaction to the stricter eligibility conditions in this UI scheme than for social assistance is that the financial reward of complying, i.e., the entitlement to UI, does not materialize immediately after leaving school, but rather only after the qualifying period of one year. Numerous studies in behavioral economics have found that adolescents tend to be present-biased and have difficulty taking future awards into account when making decisions (see Lavecchia et al., 2014; Koch et al. 2015; Levitt et al., 2016, for example). The delay in the financial reward may therefore decrease its power to trigger behavioral reactions. Moreover, this 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; Golsteyn et al., 2014; Lavecchia et al. 2014). We therefore expect that the reduced behavioral response to postponing this reward matters less for highly educated youths compared to low-educated youths. This prediction is supported by the studies of Leuven et al. (2010) and Bettinger (2011), who find that the provision of delayed financial incentives leads to higher academic achievements for higher-ability students only.

A third reason why we expect a different reaction from the Belgian UI reform is that the target populations are different. The reform particularly affects youths who expect to lose entitlement to the UI benefit for education-leavers. This excludes youths who expect to be eligible for means-tested social assistance, the target groups studied by Hernaes et al. (2017) in Norway and Cammeraat et al. (2022) in the Netherlands. To the extent that lower-educated youths are more likely eligible for means-tested welfare benefits, this, together with the arguments discussed in (i) and (ii), could explain the strong behavioral reaction uncovered for high school students in Norway and the absence of an effect on enrollment in higher education in the Netherlands. This is also consistent with our finding that the 2015 UI reform in Belgium only induced a behavioral reaction in students in higher education and not those in high school. An additional explanation for the stronger reaction of higher-educated youths in response to the UI reform is that job seekers with a high underlying job-finding rate tend to be overly pessimistic about their employment chances, whereas job seekers with a low job-finding rate are over-optimistic (Mueller et al. 2021). The higher-educated would therefore overreact relative to their risk of being unemployed after the qualifying period of one year.

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 remarks.

2. Institutional Context

Belgium is a federal state in which many competencies have been decentralized. Place-based matters are decentralized to the regions (Flanders, Wallonia, and Brussels), and language-based matters to the communities (Flemish and French).[7] 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.[8] 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 system. Subsequently, we discuss the policy reform of 2015 that is evaluated in this study.

2.1. Secondary and Higher Education

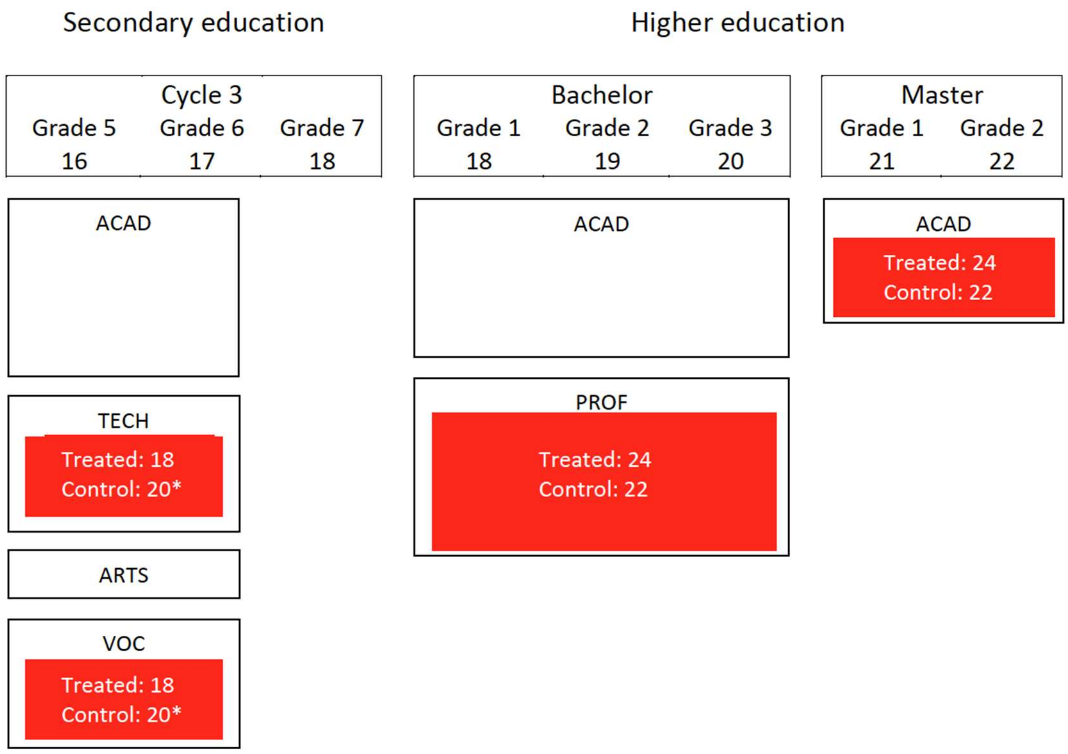
After finishing primary school, students enroll in secondary education, usually at the age of 12. Secondary education is divided into six grades (seven grades in the vocational track) and three cycles, each comprising two grades. Each grade starts on September 1st and ends on June 30, but it can take more time to complete these as grade repetition – more on which below – is quite common. Education is compulsory until the age of 18. Students can drop out of secondary education after their 18th birthday or after June 30 of the year they turn 18 if their birthday is in the second half of the year.

During secondary education, students are grouped into four different tracks according to ability and preference. 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 directly prepare pupils for the labor market. A small fraction of students is enrolled in artistic secondary education, which prepares them 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 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 in secondary education. Many students start in the academic track and downgrade to the technical or vocational track (Cockx et al.,

[7] 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).
[8] Students enrolled in Dutch-speaking secondary or higher education institutions in Flanders or 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.

2019). By age 15, 24.3% and 46.1% of students in the Flemish and French Communities, respectively, have already repeated a grade in primary or secondary education (De Witte and Hindriks, 2018).

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



Note: "Grade number" refers to the grade numbering in the third cycle of secondary education and in bachelor or master programs in higher education; the numbers underneath 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 distinct 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.

There is no general central admission exam to higher education. High school graduates (including those graduating from the seventh grade in the vocational track) are admitted to almost all programs in higher education.[9] The right panel of Figure 1 visualizes the structure of higher education in Belgium. Students can choose between a professional or an academic bachelor program. Professional bachelor programs are more practically oriented and directly prepare students for the labor market. These programs lead to a bachelor degree after three years. Professional bachelor programs were traditionally offered by colleges, but recently an increasing share of colleges has been integrated into universities. Academic programs are more theoretical. The first three years lead to a bachelor degree, but without direct preparation for the labor market. Rather, these are a prerequisite for the corresponding academic master, which last 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.

[9] The regional governments impose entry exams for very few programs, namely medicine at universities and some artistic programs at colleges. The French Community also organizes an entry exam for engineering programs at universities.

2.2. The Activation Allowance for Youth

To be eligible for regular UI in Belgium, individuals below the age of 36 must prove one year of full-time employment in the last 21 months.[10] However, education-leavers are eligible for the so-called "activation allowance". This is a specific UI scheme with weaker qualifying conditions. In contrast to regular UI, for which only full-time employment counts to qualify for UI, part-time employment[11] or an active job search may substitute full-time employment. As for regular UI, periods of inactivity such as sickness or resumed education extend the qualifying period of 12 months, but an active job search or employment need not be realized within 21 months, as in the regular scheme.

The qualifying period starts upon entry to the labor market, i.e., either at first registration as a job seeker or at the start of the first employment spell.[12] The regional public employment services (PES) monitor the job search. During the qualifying period, youths must respond to invitations from counselors to remain eligible. In addition, in the 7th and 11th month of the qualifying period they must provide proof of their past job search efforts in face-to-face meetings with counselors. Job seekers are only eligible for the activation allowance if they can present two positive evaluations of their job search effort.

Before 2015, a high school degree was not required to qualify for the activation allowance. High school dropouts in technical, artistic, or vocational education were already eligible as soon as they completed the first three years of high school; those in the general track had to complete the last year of high school but did not necessarily have to pass it.

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 under the age of 21 (and above 18) and living on their own without dependents were entitled to a monthly benefit of 494 euros, and those above the age of 21 were entitled to 811 euros.[13] However, in Belgium most unemployed education-leavers still live in their parents' home, in which case the monthly allowance amounted to only 425 euros if older than 18.[14] The average gross monthly wage of young labor market entrants was 1,830 euros for high school dropouts and 2,426 euros for university graduates. This corresponds to an average replacement rate of 23% and 17.5%, respectively.[15] This is much lower than the replacement rate of 65% at the start of the regular UI, but the latter is subject to a floor and a cap and declines with unemployment duration.[16] From January 2012, a time limit was set on claiming the activation allowance. For non-heads of household (such as education-leavers still living with their parents) with a household income above a certain threshold, the limit was set to three years, regardless of age. For all other education-leavers, this time limit was set only from the age of 30 onwards.

---

[10] This required contribution period for regular UI is stricter in Belgium than in neighboring countries. In France, entitlement to UI requires only 130 employment days in the last 24 months; in the Netherlands, the qualifying period is 26 weeks of employment in the last 36 months; in Germany, it is 12 months in the last 30 months (source: https://ec.europa.eu/social/). This may partly explain the existence of a specific, less restrictive scheme for youths in Belgium.

[11] Part-time workers only qualify to the extent that one is still searching for a full-time job. Otherwise, the time that they neither work nor search for a job extends the qualifying period.

[12] For school-leavers registering in July, the qualifying period starts only on August 1.

[13] https://www.rva.be/burgers/volledige-werkloosheid/hoeveel-bedraagt-uw-uitkering/hoeveel-bedraagt-uw-uitkering-na-studies#wat-is-het-bedrag-van-de-inschakelingsuitkeringen

[14] At the start of the qualifying period in 2014, i.e., the year before the reform, the share of unemployed education-leavers living with their parents was 82% (calculated on confidential data that we acquired for another research project).

[15] Average wage for 2016: see https://www.jobat.be/nl/art/wat-is-een-realistisch-eerste-loon.

[16] For most education-leavers, the regular UI is also a flat rate because the benefit is bounded by a relatively high floor that is binding for most youths, and in particular for the low-educated.

2.3. The Policy Reform

On December 31, 2014, the Belgian government signed an agreement to strengthen the conditions for claiming the activation allowance, to be enforced from January 1, 2015. The main aim of this reform was to enhance young people's work incentives. It is unlikely that this reform had been anticipated before January 1, 2015. Even if the principle of the reform was part of a government agreement dated from October 2014, there had been little discussion about it in the press prior to 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, and it came into force the day after.

The government agreement involved two major changes in the eligibility conditions for the activation allowance. Figure 2 provides a schematic overview of these changes. First, as of January 1, 2015, education-leavers aged 25 or older are no longer entitled to the activation allowance.[17] Before this date, this age threshold was 30. Because education-leavers must be registered as job seekers for at least one year before they can claim the activation allowance, as of 2015 this registration must 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 therefore only considers the impact on the educational outcomes of students enrolled in higher education. Second, starting from September 2015[18] high school dropouts can no longer claim the activation allowance before the age of 21.[19]

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

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

Note: HS = high school

Youths who are no longer entitled to the activation allowance are, in principle, still eligible for means-tested welfare benefits. However, since most (around 80%) of education-leavers live with their parents, they generally do not qualify for these welfare benefits because they must legally be supported by their parents. Even though direct statistics regarding the fraction of youths who transit to social assistance are not publicly available, we can indirectly deduce that this fraction is small. Indeed, within the group of cohabitants (comprising youths living with their parents), the shares of

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

[18] We consider that this reform was anticipated only from the moment that the reform was decided upon, i.e., on December 31, 2014.

[19] Formally, since the reform eligibility for the activation allowance is not conditional on graduating from high school but conditional on successfully passing the sixth year of high school. This matters for students in the vocational track because if they drop out after the sixth year, they do not formally have a high school degree, although they are nevertheless eligible. Here, we ignore this subtle imprecision for simplicity.

individuals who entered welfare between 2015 and 2017 among those who lost the activation allowance for some reason (not necessarily related to the reform) ranged between 5.5% and 7.4% only.[20] This strongly suggests that the target population for the 2015 double reform in Belgium was very different from the population affected by the welfare reforms in Norway and the Netherlands analyzed by Hernaes et al. (2017) and Cammeraat et al (2022).

3. Methodology

If students are forward-looking, well-informed, and consider the possibility that they will not be 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 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 in the academic year 2014-2015. We then 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 Entitlement above Age 25

The first part of the policy reform *unexpectedly* affected students enrolled in higher education in the academic year 2014-2015. As of January 1, 2015, these students could no longer start claiming the activation allowance after the age of 25.[21] Because of the qualifying period of one year, this implied that enrolled students aged 24 or over on December 31, 2014, suddenly lost their entitlement. In addition, students enrolled at the age of 24 in subsequent academic years were no longer eligible 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 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 birthday and, hence, by dropping out before completing their degree. Students born after July 31 remain eligible if they finish the academic year, if they register as job seekers, or if they find a job before their 24th birthday. Therefore, this last group has an additional incentive to complete their degree in the current academic year: retaining eligibility for the activation allowance. Because these contrasting incentives depend 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 gains the characteristics of the 23-year-old students. In sum, we consider only Belgian students aged 22 and 24 on December 31 of the academic years used in the main analysis.

[20] These statistics are not available by educational attainment, but we can expect these shares to be larger for the low-educated. Source: RVA (2019, Table 22).

[21] 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 therefore did not apply to this group.

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 countries. We use the sample of non-Belgian students for a placebo analysis, however: 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 Entitlement for High School Dropouts

The second policy reform affected students enrolled in secondary education from the 2014-2015 academic year. From September 1, 2015, education-leavers who do not complete secondary education are not eligible for the activation allowance before their 21st birthday. This reform therefore affected students in the last year of compulsory secondary education differently depending on their age.[22]

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 qualifying 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 to the previous group because the end of their qualifying period is postponed until their 21st birthday. However, the impact of the reform is smaller than for the first group because 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 qualifying 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 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 over on December 31 are not affected by the policy reform and can start claiming the activation allowance after a one-year qualifying 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 of the French Community, whereas we also consider the group of students aged 19 and born before September for the analysis of the Flemish Community. For students aged 18, the qualifying period is extended to their 21st birthday.

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 aspect of the reform and students who were not affected. Let Y_{it} denote the outcome of interest (degree

[22] The policy reform could have not only 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.

completion or dropout) of student i at the end of year t, with t=2011, 2012, 2013 being the control period and t=2014, 2015 being 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:

$$Y_{it} = \alpha + \sum_{2013 \neq s = 201}^{2015} (\beta_s T_{st} + \delta_s D_i T_{st}) + \gamma D_i + \varepsilon_{it},$$

with T_{st} being 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 corresponds to time-fixed effects that capture the temporal 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. 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 by 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 we implement the analysis separately for the Flemish and French Communities 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 students, family income (for the Flemish Community only), and whether Dutch is spoken at home or not (for secondary school only).

Because not all students are at risk of long-term unemployment and, hence, of losing the activation allowance, estimated treatment effects should be interpreted as intent-to-treat (ITT) effects. To get a better sense of the effect size, we follow Hernaes et al. (2017) in dividing δ_s by the exposure risk of losing eligibility for the activation allowance one year after ending full-time education.[23] This allows us to scale the treatment effect to the size it would have if the exposure risk was 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 the placebo treatment effect in the pre-treatment period, i.e., $\sum_{s=201}^{2012} \hat{\delta}_s T_{st}$ for $s \leq 2013$. We also construct the 95% confidence interval around this placebo treatment effect. The parallel trend assumption is rejected in a given year when the horizontal axis, corresponding to a zero-placebo treatment effect, does not fall within this confidence interval. For $s > 2013$, we plot the average treatment effects in each post-treatment year. A statistically significant effect of the reform is detected when the horizontal axis falls outside the 95% confidence interval.

Tables A1 and A2 in the Appendix show that some of the background characteristics of the 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

[23] 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 exposure risk for graduates in higher education; we do not do this for high school graduates because the causal impact of the reform is found to be not statistically different from zero.

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 the policy was announced at the end of the year, after enrollment decisions were already made. We fix this by applying a conditional difference-in-differences estimator and making 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, for example, by Albanese and Cockx (2019). More precisely, we weight the observations in the treatment and control groups in the years before and after the reform such 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 Tables A3 and A7 in the 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 on secondary and higher education provided for this study by the Ministries of Education of both the Flemish and French Communities 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 that 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 or master 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 program. Enrollments of international students spending part of their program in Belgium (for example, through Erasmus exchange) are not included in these statistics. In contrast to the data for high school, these registration data do not permit us to isolate bachelor or master students in their final year; enrollments in higher education, therefore, refer to the global bachelor or master program enrollment.

We include three years before the policy reform (academic years 2011-2012 to 2013-2014) and two years in the post-reform period. To our knowledge, except for the policy reform studied in this paper, during this period there were no other reforms that could have had a differential impact on students in the control and treatment groups.

The data are collected from four different administrative sources (higher education and secondary education in both 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 or master in higher education), and depending on the data source, also by nationality (for higher education in the Flemish and French Communities), socio-economic status[24] (observed in secondary and higher education in the Flemish

[24] Socio-economic status is measured by an indicator equal to one if a student received a study grant in secondary school or higher education and zero otherwise. Such a grant is only awarded to low-income families.

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 who obtain a degree at the end of the academic year, and the number of dropouts.[25] 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 rates. Because for higher education only global program enrollments are measured – three years for the bachelor and, depending on the discipline, one or two years for the master – 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 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 retain only students in their graduation year. For the analysis, we transform these grouped data into individual data of a 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 that is equal to one if a degree is obtained and zero otherwise, the other that is equal to one if the individual drops out and zero otherwise.[26]

Figures A1 and A2 show the evolution of degree completion and dropout in higher and secondary education over time. These outcomes are not only presented for the different age groups retained in the analysis but also for the age groups not considered in the analysis.

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 for the activation allowance from age 25 onwards in higher education. Next, we consider the effects of imposing the qualifying requirement for high school students. We present both a graphical and corresponding econometric analysis of the difference-in-differences (DiD) models. For both educational levels, we first present the global effects by study program (professional bachelor and master 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 we allow for different 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

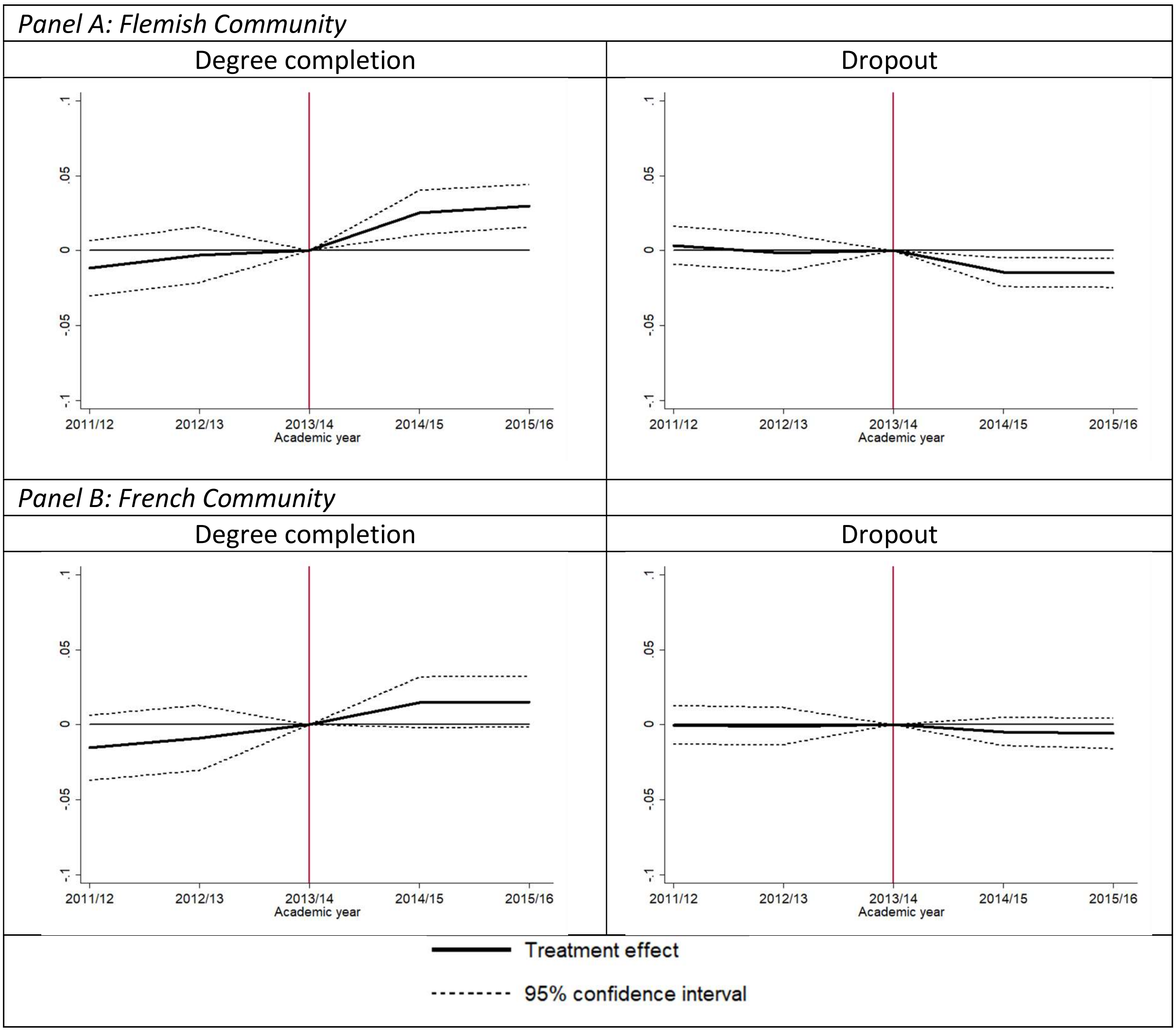
The thick solid lines in Figure 3 display treatment and placebo treatment effects in the post- and pre-treatment periods on degree completion and dropout for 24-year-old (treated) students enrolled in

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

[26] Note that this expansion to the individual level is only possible because of the binary nature of the outcome variables.

higher education in the Flemish Community (Panel A) and the French Community (Panel B). The surrounding dashed lines confine the 95% confidence intervals. The vertical line indicates the last year of the control period. This is the reference period for which the treatment effect is normalized to zero. Twenty-two year-old students enrolled in higher education are in the control group.

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 indicates the last year before the reform. The thick solid line shows the estimated treatment effect in the post-reform period and the placebo treatment effect in the pre-reform period as predicted by a saturated weighted difference-in-differences regression using 500 bootstrap replications. The thin dotted lines are the 95% confidence intervals of the (placebo) treatment effects.

Looking at the pre-reform period in the four graphs, we can see that the placebo treatment effects are close to zero and statistically not significantly different from zero. This suggests that the parallel trend assumption is not rejected for either outcome variable. This is confirmed in the formal tests reported in Table 1. After the policy reform, degree completion in the treatment group increased and the 95% confidence interval leads to the rejection of a zero-treatment effect in the Flemish Community. Although we also observe a positive treatment effect in the French Community, this effect is not different from zero at the 5% significance level. A reversed pattern can be observed for dropout from higher education. Before the policy reform, dropout follows similar trends in treatment and control groups. After the policy reform, dropout fell both in the Flemish and French Communities, but the decrease in dropout is statistically significantly different from zero 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 for 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 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 of significance) that treatment effects are similar in the Flemish and French Communities (first test of panel D), we restrict the treatment effects to being the same in both regions in panel A. The p-values for the placebo tests of the parallel trend assumption are shown in panel D and indicate that the parallel trend assumption is never rejected in either region.

The first panel reports that degree completion for the treated students 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 raised this 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 pre-final years. Based on aggregate statistics from the 2013-2014 pre-treatment year, available for the Flemish Community only, we estimate the fraction of final-year enrollments in higher education to be 57.6%[27] and the corresponding counterfactual degree completion rate for final-year students to be 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 calculate the exposure risk of the cohort of 23-year-old master 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 qualifying period over the course of 2013. These data do not allow us to calculate the exposure risk of graduates from a professional bachelor. However, based on data that we have for the Flemish Community only, there is evidence that this exposure risk is similar for this group.[28] Since this exposure risk is estimated for graduates, we should apply it to the

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

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

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). For comparison, Hernaes et al. (2017) 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 for various tests		
Equality of the effects in the Flemish and French Communities	0.173	0.094
Parallel trend: Flemish Community	0.448	0.732
Parallel trend: 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 to 2013-2014. Treatment period = 2014-2015 and 2015-2016. Results are based on weighted difference-in-differences regressions. The regressions allow for different trends in the two 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 trend assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value for the test of parallel trends is reported for each specification and region. We also report the p-value for the test of similar effects in the Flemish and French Communities. 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 ATET 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 risk, and not dropouts, for whom we expect a larger risk of still being unemployed one year after leaving higher education. Second, individuals base their 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 of being long-term unemployed and, hence, their exposure risk (Mueller et al. 2021).

The second regression shows that the policy reform significantly reduced the dropout rate from higher education by 1.1 pp. In absence of the policy reform, 13.1% of all treated students would have dropped out without a degree at the end of the academic year. 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 panels 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 French Community, the effect on dropout, although negative, turns out to be not significantly different from zero. Table A3 in the Appendix shows the corresponding unweighted difference-in-differences estimates and yields similar conclusions.

Placebo Analysis

Next, we conduct a placebo analysis by limiting the analysis to non-Belgian students. After finishing their studies, most foreign students return to their home country.[29] This means that the reform is not relevant for these students. Figure 4 shows the rates of 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 any significant effects for students of other nationalities. The magnitude of the treatment effects is also closer to zero for non-Belgian students, suggesting that this non-significance is not driven by the smaller sample size of this group.

Robustness analysis

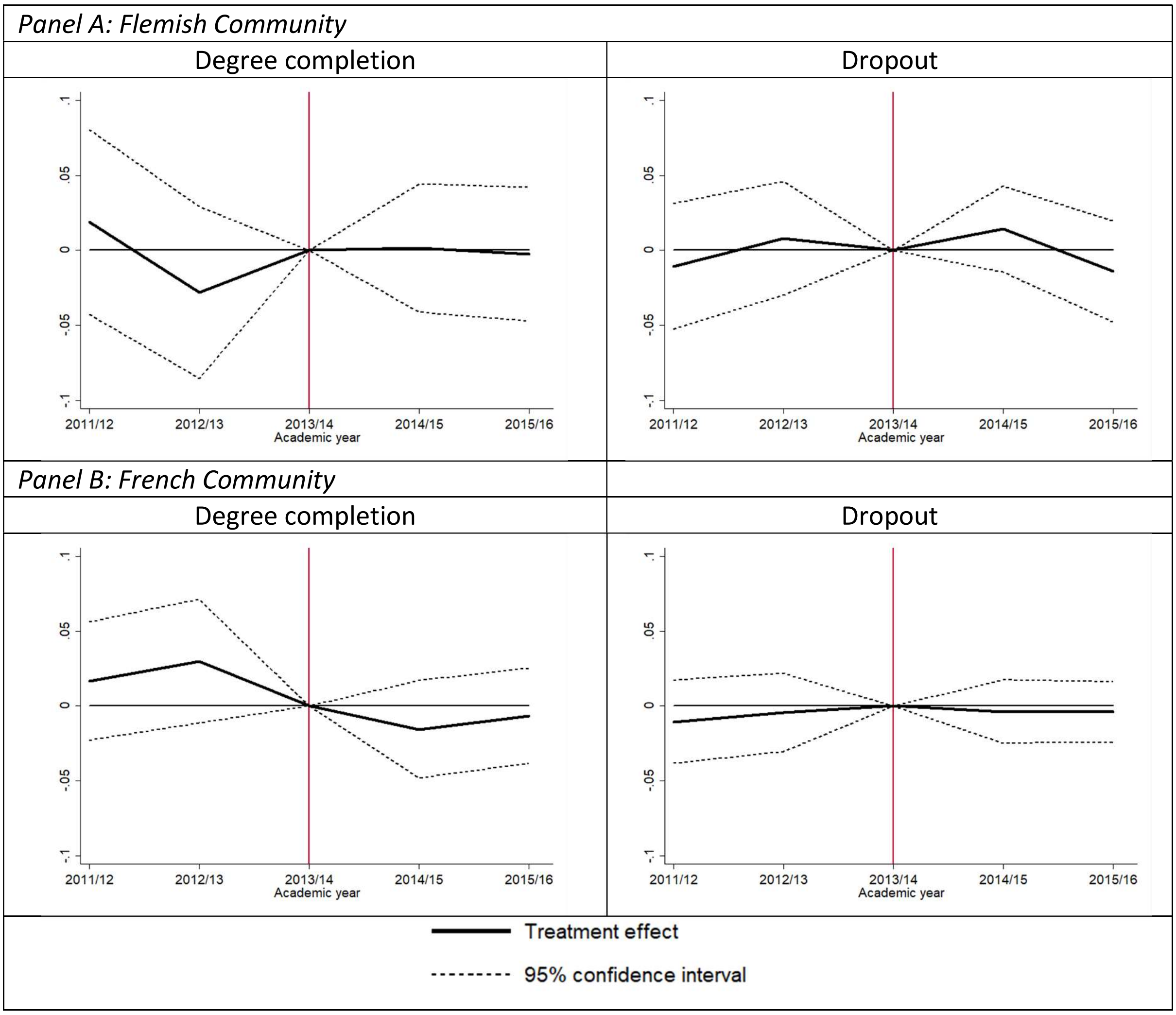
Given that we find no effect for non-Belgian students, we can use them as an additional control group and implement a triple difference (DDD) analysis (Gruber, 1994; Cortes and Pan, 2013; Olden and Møen, 2022). In this model, we compare degree completion and dropout before and after the reform between 22- and 24-year-old Belgian students and students of other nationalities in the same age group. This requires a less restrictive identification assumption in that it allows for a differential trend for the different age groups, with the assumption that this differential trend is the same for Belgian and foreign students. The results of this triple difference model are shown in Table A5. The findings are very similar to those of the benchmark analysis. For the Flemish Community, the point estimates for both outcomes (degree completion and dropout) are virtually the same as in the benchmark analysis. For the French Community, the point estimates increase in absolute value and approach the values found for the Flemish Community. Even though these values are larger in absolute value, the estimates of the benchmark model are comprised by the 95% confidence interval estimated in the triple difference approach, which leads to less precise estimates than in the standard difference-in-differences approach because it relies on less restrictive assumptions.

Finally, we assess whether our estimates are robust to using an alternative control group. Although students in the control group are not directly affected by the policy reform, they can eventually be affected if they are still enrolled in higher education after their 24th birthday. Consequently, students in the control group could anticipate the potential loss of future entitlement to the activation allowance by increasing their study effort or dropping out of higher education. We argue that this is

[29] There are no publicly available statistics regarding the fraction of foreign students who stay in Belgium after completing higher education. However, based on informal contacts with an employee at the administration of higher education of the French Community, this fraction is of the order of 20% only.

less likely for 21-year-old students than for 22-year-old students because the latter group is closer to the age at which the reform could have an impact.

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 indicates the last period before the reform. The thick solid line displays the estimated treatment effects in the post-reform period and the placebo treatment effects in the pre-reform period as predicted by a saturated weighted difference-in-differences regression using 500 bootstrap replications. The thin dashed lines confine the 95% confidence intervals of the (placebo) treatment effects.

Table A6 and Figure A3 in the Appendix show that when using the 21-year-old students as a control group instead of the 22-year-old students, the results for dropout hardly differ from the benchmark analysis. For degree completion, the overall estimate for the two language Communities is also very close to that reported in the benchmark analysis, but this average conceals significantly different effects in opposite directions for the two communities. In the Flemish Community, the effect of the reform on degree completion is now estimated to be 50% higher than that reported in the benchmark results: It increases from 2.8 pp to 4.2 pp, and this new value exceeds the upper bound of the 95% confidence interval of the benchmark estimate. By contrast, the effect in the French Community is now estimated to decrease (statistically insignificantly) degree completion by 0.8 pp instead of the statistically significant increase of 1.5 pp that emerged in the benchmark results.

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 reference to 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 reference to master)*	-0.017**	-0.009
	(0.007)	(0.005)
Panel C: Gender		
Male	0.024***	-0.018***
	(0.006)	(0.004)
Female *(in reference to 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 reference to 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 groups. 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 to 2013-2014. Treatment period = 2014-2015 and 2015-2016. Results are based on weighted difference-in-difference regressions. The regressions allow for different trends in each region and group. 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, based on whether the student received a study grant.

The degree of correspondence of these results with the benchmark analysis is reflected in the parallel trend analysis before the reform. For the dropout outcome, the results of the parallel trend analysis are very convincing. By contrast, even though the formal tests do not reject the assumption of parallel trends for degree completion, the graphical analysis in Figure A3 is not very convincing. This may relate to the fact that 21-year-old students are very different from 24-year-old students (i.e., they have accumulated much less grade repetition than the 24-year-olds) and not all of them can complete their degree at that age: Because an academic master degree requires a minimum of four years of study after obtaining a high school degree, for which the minimum age is, in turn, 18 years, no master student can complete a two-year master program at the age of 21; this is only possible for students in the professional bachelor and students in one-year master programs if they did not repeat any grade during their school career. Therefore, our main conclusions are based on the analysis using the 22-year-old students as the control group.

Treatment Heterogeneity

Table 2 analyses heterogeneity in the treatment effects over time according to 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 programs are more likely to obtain their degrees than students in bachelor programs.

5.2. Secondary Education

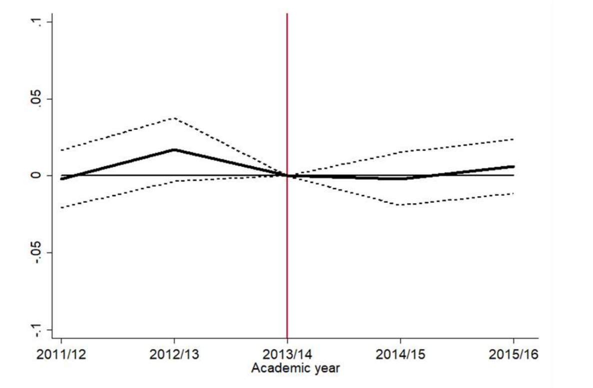
Global Treatment Effects in Secondary Education

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 completing the last year of secondary education. Figure 5 shows treatment and placebo treatment effects on degree completion and dropout in the final year of secondary education for students in the treatment group in the Flemish and French Communities. Note 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, placebo treatment effects are not statistically significantly different from zero. This is evidence that the parallel trend assumption cannot be rejected. In 2014/15, the first post-reform schoolyear, dropout is significantly *higher* for the treated students in the Flemish Community, and degree completion is significantly *lower* in the French Community. Everywhere else, the treatment effects are remarkably close to zero. These observations are very counterintuitive. We cannot think of a mechanism that could explain these findings. Moreover, these outcomes fluctuate much more for the control group than for the treatment group, also in the pre-reform period (see Figure A2). 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 in the outcomes of the control group may also be related to the relatively small sample size: The sample size for the control group is only about 40% of that of the treated group (Table A2). These arguments suggest 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 that the treatment effects be equal in the two post-treatment years. Reported treatment effects are close to zero, and none are significant at the 5% level.

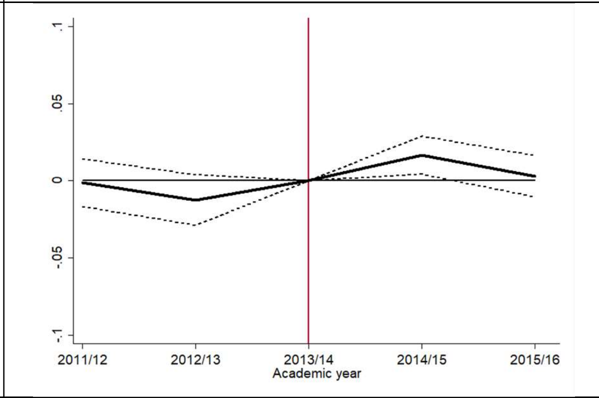
Figure 5. Degree Completion and Dropout in the Final Year of Secondary Education

Panel A: Flemish Community

Degree completion

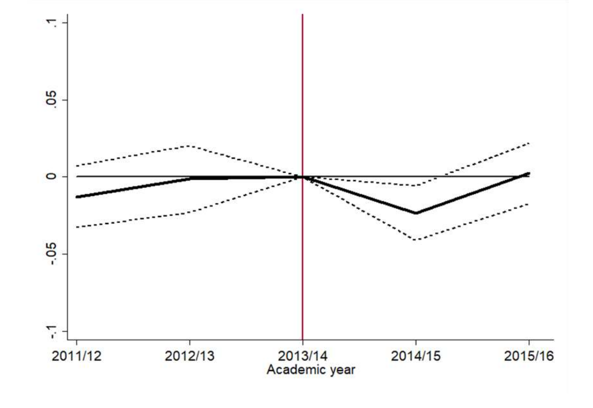


Dropout



Panel B: French Community

Degree completion



Dropout

Treatment effect

--------- 95% confidence interval

Note: Treatment group = 18-year-old students. Control group = For the Flemish Community, 19-year-old students born before September 1 and 20-year-old students, and 20-year-old students for the French Community. Age is measured on December 31 of the respective academic year. The vertical line indicates the last period before the reform. The thick solid line shows the estimated treatment effect in the post-reform period and the placebo treatment effect in the pre-reform period as predicted by a saturated weighted difference-in-differences regression using 500 bootstrap replications. The thin dashed lines confine the 95% confidence intervals of the (placebo) treatment effects.

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 Communities	0.239	-
Parallel trend: Flemish Community	0.144	0.262
Parallel trend: French Community	0.386	-
Observations	135,507	79,929

Note: Treatment group = 18-year-old students. Control group = For the Flemish Community, 19-year-old students born before September 1 and 20-year-old students, and 20-year-old students for the French Community. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 to 2013-2014. Treatment period = 2014-2015 and 2015-2016. Results are based on weighted difference-in-difference regressions. The regressions allow for different trends in each region. 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 trend 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 and 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 the results of the analysis of treatment heterogeneity across various dimensions. It confirms that the counterintuitive findings that the reform decreases degree completion and increases 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 educational outcomes in secondary education, and if anything, it was 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 reference to 2014-2015)*	0.016**	-0.014*
	(0.007)	(0.007)
Panel B: Study programs		
Technical track	-0.010	0.013*
	(0.006)	(0.007)
Vocational track *(in reference to technical track)*	0.013	-0.008
	(0.008)	(0.011)
Panel C: Gender		
Male	-0.000	0.005
	(0.007)	(0.007)
Female *(in reference to male)*	-0.007	0.011
	(0.007)	(0.008)
Panel D: Language spoken at home (Flemish Community)		
Dutch	0.005	0.010
	(0.007)	(0.006)
Other languages *(in reference to 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 reference to 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 groups. Treatment group = 18-year-old students. Control group = For the Flemish Community, 19-year-old students born before September 1 and 20-year-old students, and 20-year-old students for the French Community. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 to 2013-2014. Treatment period = 2014-2015 and 2015-2016. Results are based on weighted difference-in-difference regressions. The regressions allow for different trends in each region and group. 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, based on whether the 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 enhanced degree completion and reduced dropout in higher education to a considerable extent. First, the stakes of the reform were lower for high school students as the loss of entitlement was only temporary and lasted until 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; Golsteyn et al., 2014;

Lavecchia et al. 2014) and more (over-)optimistic about finding a job (Mueller et al. 2021), both of which imply 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 substantially enhances 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 in the first year after leaving. 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 requirement of being employed full-time for a full year before becoming eligible for UI is a much too distal objective that might be perceived as unrealistic. It may therefore fail to trigger a behavioral reaction in the direction of this goal, i.e., self-regulatory behavior (van Hooft et al., 2013; van Hooft, 2016). By contrast, the risk of being forced to engage in specific activation programs, as in Norway, is much more proximal and concrete. There is indeed some evidence that tying incentives to more concrete and easier-to-understand objectives than a financial loss works better in job-search (ibid.) and educational environments (Fryer, 2011; Gneezy et al., 2011). Finally, those affected by the UI reform in Belgium differ from those who were affected by the social assistance reform in Norway. High school students in Belgium may not be significantly affected by the reform because they are more likely to be eligible for welfare benefits and are therefore less at risk of the income loss induced by the reform. Thus, the reform was more relevant for students in higher education. The reverse holds for the reform in Norway.

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 taking future awards into account when making decisions (see, for example, 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 context and that of Hernaes et al. (2017), make a crucial difference. Currently, evidence regarding high-stakes incentive schemes is scant. The study by Enke et al. (2021) is an exception in that it explicitly studies the impact of high stakes on behavioral biases. In a lab experiment, they find that very high stakes are never sufficient to de-bias participants, but their 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 Hossain and List, 2012, for example). Even though Levitt et al. (2016) do not find evidence for loss aversion in a field experiment in an educational context, their findings are 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 needed to get a better understanding of the mechanisms that drive these different findings.

6. Conclusion

We studied whether tightening eligibility criteria for UI can have an impact on investment in human capital. In 2015, a double reform in Belgium abolished more favorable qualifying conditions for unemployment insurance for two groups of education-leavers: high school dropouts and older graduates. We investigated whether this reform enhanced educational attainment for these two groups. While for high school students we did not find any evidence of this, we did find that the reform significantly enhanced degree completion rates and decreased dropout rates in higher education. When dividing our estimates by 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 stakes of the reform for the former and the fact that the higher educated may be less affected by present-bias and be more pessimistic about 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 only other available evidence. Hernaes et al. (2017) find that imposing activity requirements on means-tested welfare benefits significantly increased the high school completion rate in Norway. We propose 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 proximal and easier-to-understand objectives, such as concrete activity requirements, than to a distal and unrealistic goal, such as that implied by the eligibility requirement for UI after disqualification for the activation allowance (i.e., full-time employment for a year) (Fryer, 2011; Gneezy et al., 2011; van Hooft et al., 2013; van Hooft, 2016). However, these explanations remain speculative and call for future research to confirm whether and which of these design features are crucial.

From a policy perspective, this research and other related studies suggest that imposing eligibility restrictions has different implications on educational achievement depending on the form of these restrictions and whether these are imposed in the context of UI or social assistance. In our study, we have considered the effects of such restrictions in a specific form of UI for education-leavers. Such schemes only exist in a couple of countries, and the reader may therefore question how relevant the findings of this research are for countries in which such specific schemes do not exist. We argue that our findings can also be relevant for those countries in that the reform we study resembles an extension of the qualifying period in a regular UI system during which workers must deliver proof of employment. Our findings therefore suggest that such extensions may have positive effects on educational achievement, but only for students in higher education and not for those who aim to enter the labor market right after leaving (possibly prematurely) high school. These positive effects must be added to the potential positive incentive effects such restrictions may have on employment. However, Cockx et al. (2020) studied the impact of the same UI reform on work incentives and found that the reform only stimulated job-finding for very short-lived jobs, if any. Moreover, these benefits must be traded off against the welfare losses that the non-receipt of UI could generate. The size of these losses remains an open question as we lack the data to evaluate them. Nevertheless, our analysis clearly demonstrates that not only work incentives should be taken into account when designing an optimal UI scheme. An avenue for future research is a design that takes the implied incentives for human capital accumulation into account.

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 programs for bachelor graduates, or the decision to start a second master program. Consequently, this reform could also have decreased human capital investment in higher education. In future research, it would be interesting to follow students in specific study programs in secondary and higher education across several years to get a more complete picture of the consequences of the reform on educational attainment.

7. References

A-Kasser (2019). Apply for membership of an unemployment fund – special rule for new graduates, https://www.a-kasser.dk/graduates. Accessed March 12, 2019

Albanese, A. & Cockx B. (2019). Permanent Wage Cost Subsidies for Older Workers. An Effective Tool for Employment Retention and Postponing Early Retirement? *Labour Economics,* 58, 145–166.

Albanese, A., Picchio, P., & Ghirelli, C. (2020). Timed to say goodbye: does unemployment benefit eligibility affect worker layoffs? *Labour Economics 65*, 101846.

Angrist, J. & Pischke, J. (2013). Mostly harmless econometrics: an empiricists companion. Princeton University Press.

Becker, G. S. & Mulligan, C. B. (1997). The Endogenous Determination of Time Preferences. *Quarterly Journal of Economics* 112(3), 729–758.

Bettinger, E. P. (2011). Paying to Learn: The Effect of Financial Incentives on Elementary School Test Scores. *Review of Economics and Statistics* 94(3), 686–698.

Blundell, R., Costa Dias, M., Meghir, C., & Shaw, J. (2016). Female Labor Supply, Human Capital, and Welfare Reform. *Econometrica* 84(5), 1705–1573.

Bratsberg, B., Hernaes, O., Markussen, S., Raaum, O., & Roed, K. (2019). Welfare Activation and Youth Crime. *The Review of Economics and Statistics* 101(4), 561–574.

Brébion C., Biole, S., & Khoury, L. (2022). Unemployment Insurance Eligibility and Employment Duration. Mimeo University of Paris Dauphine-PSL.

Cammeraat, E., Jongen, E., & Koning, P. (2022). Preventing NEETs during the Great Recession: The effects of a mandatory activation program for young welfare recipients. *Empirical Economics* 62, 749-777.

Cockx, B., Declercq, K., Dejemeppe, M., Inga, L., & Van der Linden, B. (2020). Switching from an inclining to a zero-level unemployment benefit profile: Good for work incentives. *Labour Economics* 64, 101816.

Cockx, B., Picchio, M., & Baert, S. (2019). Modeling the Effects of Grade Retention in High School. *Journal of Applied Econometrics* 34(3), 403–424.

Cockx, B. & Van Belle, E. (2019). Waiting longer before claiming, and activating youth: no point? *International Journal of Manpower*, 40(4), 658–687.

Cortes, P. & J. Pan (2013). Outsourcing Household Production: Foreign Domestic Workers and Native Labor Supply in Hong Kong. *Journal of Labor Economics*, 31(2), 327–371.

Enke, B., Gneezy, U., Hall, B., Martin, D. C., Nelidov, V., Offerman, T., & van de Ven, J. (2021). Cognitive Biases: Mistakes or Missing Stakes? NBER Working Paper 28650.

Fryer, R. (2011). Financial Incentives and Student Achievement: Evidence from Randomized Trials. *Quarterly Journal of Economics* 126(4), 1755–1798.

Gneezy, U., Meir, S., & Rey-Biel, P. (2011). When and Why Incentives (Don't) Work to Modify Behavior. *Journal of Economic Perspectives* 25(4), 191–210.

Golsteyn, B. H. H., Grönqvist, H., & Lindahl, L. (2014). Adolescent Time Preferences Predict Lifetime Outcomes. *The Economic Journal* 214(580), 739–761.

Gruber, J. (1994). The Incidence of Mandated Maternity Benefits. *American Economic Review* 1, 622–641.

Gunnes, T., Kirkeboen, L., & Ronning M. (2013) Financial incentives and study duration in higher education. *Labour Economics* 25, 1–11.

Hernaes, O., Markussen, S., & Roed, K. (2017). Can welfare conditionality combat high school dropout? *Labour Economics* 48, 144–156.

Hirano, K., Imbens, G., & Ridder, G. (2003). Efficient estimation of average treatment effects using the estimated propensity score. *Econometrica* 71(4), 1161–1189.

Hossain, T. & List, J. (2012). The Behavioralist Visits the Factory: Increasing Productivity Using Simple Framing Manipulations. *Management Science* 58(12), 2151–2167.

Kahneman, D. & Tversky, A. (1979). Prospect Theory: An Analysis of Decision under Risk. *Econometrica* 47(2), 263–292.

Keane, M. & Wolpin, K. (2000). Eliminating Race Differences in School Attainment and Labor Market Success. *Journal of Labor Economics* 18(4), 614–652.

Kesselman, J. (1976). Tax effects on job search, training, and work effort. *Journal of Public Economics* 6, 255–272.

Koch, A., Nafziger, J., & Skyt Nielsen, H. (2015). Behavioral Economics of Education. *Journal of Economic Behavior & Organization* 115, 3–17.

Kolsrud, J., Landais, C., Nilsson, P., & Spinnewijn, J. (2018). The optimal timing of unemployment benefits: Theory and evidence from Sweden. *American Economic Review* 108(4), 985–1033.

Langenbucher, K. (2015). How demanding are eligibility criteria for unemployment benefits, quantitative indicators for OECD and EU countries. OECD Social, Employment and Migration Working Papers No. 166, OECD Publishing.

Lavecchia A. M., Liu, H., & Oreopoulos, P. (2014). Behavioral Economics of Education: Progress and Possibilities. NBER Working Papers 20609, National Bureau of Economic Research.

Le Barbanchon, T. (2016). The effect of the potential duration of unemployment benefits on unemployment exits to work and match quality in France. *Labour Economics* 42, 16–29.

Leuven, E., Oosterbeek, H., & van der Klaauw, B. (2010). The Effect of Financial Rewards on Students' Achievement: Evidence from a Randomized Experiment. *Journal of the European Economic Association* 8(6), 1243–1265.

Luxembourg Employment Agency (2019) Applying for unemployment benefits as a young school leaver. Retrieved from http://adem.public.lu/en/demandeurs-demploi/demander-indemnites-chomage/residents/jeunes-sortant-de-lecole/index.html. Accessed March 12, 2019

OECD (2011). *OECD Economic Surveys: Belgium*. OECD Publishing.

Martins, P. S. (2021). Working to get fired? Unemployment benefits and employment duration. *Journal of Policy Modeling* 43(5), 1016–1030.

Miller, Robert A., & Saunders, S. G. (1997). Human capital development and welfare participation. *Carnegie-Rochester Conference Series on Public Policy* 46, 1–43.

Moffitt, R. (2002). Welfare programs and labor supply, In A. Auerbach & M. Feldstein (Eds.), *Handbook of Public Economics, vol. 4* (pp. 2393–2430). Elsevier.

Mueller, A. I., Spinnewijn, J., & Topa, G. (2021). Job Seekers' Perceptions and Employment Prospects: Heterogeneity, Duration Dependence, and Bias. *American Economic Review* 111(1), 324–363.

Olden, A. & J. Moen (2022). The Triple Difference Estimator. *The Econometrics Journal* 25, 531–553.

Riddell, C. & Riddell, W. C. (2014). The pitfalls of work requirements in welfare-to-work policies: Experimental evidence on human capital accumulation in the Self-Sufficiency Project. *Journal of Public Economics* 117, 39–49.

RVA (2019). De beperking van het recht op inschakelingsuitkeringen: aard van de uitstroom in 2017. RVA.

Schmieder, J. F.. & T. von Wachter (2016). The Effects of Unemployment Insurance Benefits: New Evidence and Interpretation. *Annual Review of Economics* 8, 547–581.

Sutter, M., Kocher, M. G., Rützler, D., & Trautmann, S. (2013). Impatience and Uncertainty: Experimental Decisions Predict Adolescents' Field Behavior. *American Economic Review* 103(1), 510–531.

Tatsiramos, K. & Van Ours, J. (2014). Labor market effects of unemployment insurance design. *Journal of Economic Surveys* 28(2), 284–311.

van Hooft, E. A. J. (2016). Self-Regulatory Perspectives in the Theory of Planned Job Search Behavior: Deliberate and Automatic Self-Regulation Strategies to Facilitate Job Seeking. In U.-C. Klehe & E. A. J. van Hooft (Eds.), *The Oxford Handbook of Job Loss and Job Search* (pp. 205–222). Oxford University Press.

Van Hooft, E. A. J., Wanberg, C. R., & van Hoye, G. (2013). Moving beyond job search quantity: Towards a conceptualization and self-regulatory framework of job search quality. *Organizational Psychology Review* 3(1), 3–40.

VDAB (2015). *Schoolverlatersrapport,* Editie 2015. Retrieved from https://www.vdab.be/trends/archief. Accessed August 17, 2021.

Von Buxhoeveden, M. (2019). Unemployment insurance and youth labor market entry. IFAU working paper 2019:12.

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	20.2	18.0	-0.3	+0.1
Master	52.1	58.3	-3.0	-1.7*
Observations	67,186	24,355	24,323	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	51,367	22,424	17,991	8631

Note: Student characteristics for the 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 in 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, and *** p<0.01 in the Δ Treatment column indicate whether the change in the treatment group significantly differs from the change in the control group.

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
Technical track	49.3	52.6	-1.3	-1.2
Observations	10,375	37,364	3284	12,781
Panel D: French Community				
Male	56.8	47.4	+0.5	+1.1
Technical track	55.9	67.9	-2.3	+0.8***
Observations	9563	23,359	3253	7961

Note: Student characteristics for the 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 in the pre-reform period. Treatment group = 18-year-old students. Control group = For the Flemish Community, 19-year-old students born before September 1 and 20-year-old students, and 20-year-old students for the French Community. Age is measured on December 31 of the respective academic year. * p<0.10, ** p<0.05, and *** p<0.01 in the Δ Treatment column indicate whether the change in the treatment group significantly differs from the change in the control group.

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 for various tests		
Equality of the effects in the Flemish and French Communities	0.077	0.026
Parallel trend: Flemish Community	0.533	0.661
Parallel trend: French Community	0.198	0.989
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 to 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 trend assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value for the test of parallel trends is reported for each specification and region. The p-value for the test of 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 for various tests		
Equality of the effects in the Flemish and French Communities	0.640	0.802
Parallel trend: Flemish Community	0.327	0.669
Parallel trend: 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 to 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 each region. 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 trend assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value for the test of parallel trends is reported for each specification and region. The p-value for the test of 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 of Belgian Students in Higher Education (DDD)

	Degree	Dropout
Panel A: All students		
Treatment effect	0.027**	-0.006
	(0.012)	(0.008)
Counterfactual outcome	0.441	0.126
Panel B: Flemish Community		
Treatment effect	0.028	-0.014
	(0.019)	(0.013)
Counterfactual outcome	0.550	0.154
Panel C: French Community		
Treatment effect	0.026*	-0.010
	(0.014)	(0.009)
Counterfactual outcome	0.437	0.109
Panel D: P-values for various tests		
Equality of the effects in the Flemish and French Communities	0.956	0.438
Parallel trend: Flemish Community	0.386	0.822
Parallel trend: French Community	0.841	0.888
Observations	287,928	287,928

Note: Treatment group = 24-year-old students with Belgian nationality. Control groups = 22-year-old students with Belgian nationality and 22- and 24-year-old students with other nationalities. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 to 2013-2014. Treatment period = 2014-2015 and 2015-2016. Results are based on weighted triple difference-in-differences regressions. The regressions allow for different trends in each region. 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 for the test of parallel trends is reported for each specification and region. The p-value for the test of 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 A6. Degree Completion and Dropout of Belgian Students in Higher Education (alternative control group)

	Degree	Dropout
Panel A: All students		
Treatment effect	0.020***	-0.011***
	(0.004)	(0.003)
Counterfactual outcome	0.448	0.131
Panel B: Flemish Community		
Treatment effect	0.042***	-0.014***
	(0.006)	(0.004)
Counterfactual outcome	0.430	0.144
Panel C: French Community		
Treatment effect	-0.008	-0.007*
	(0.007)	(0.004)
Counterfactual outcome	0.472	0.115
Panel D: P-values for various tests		
Equality of the effects in the Flemish and French Communities	0.000	0.181
Parallel trend: Flemish Community	0.068	0.699
Parallel trend: French Community	0.125	0.478
Observations	278,356	278,356

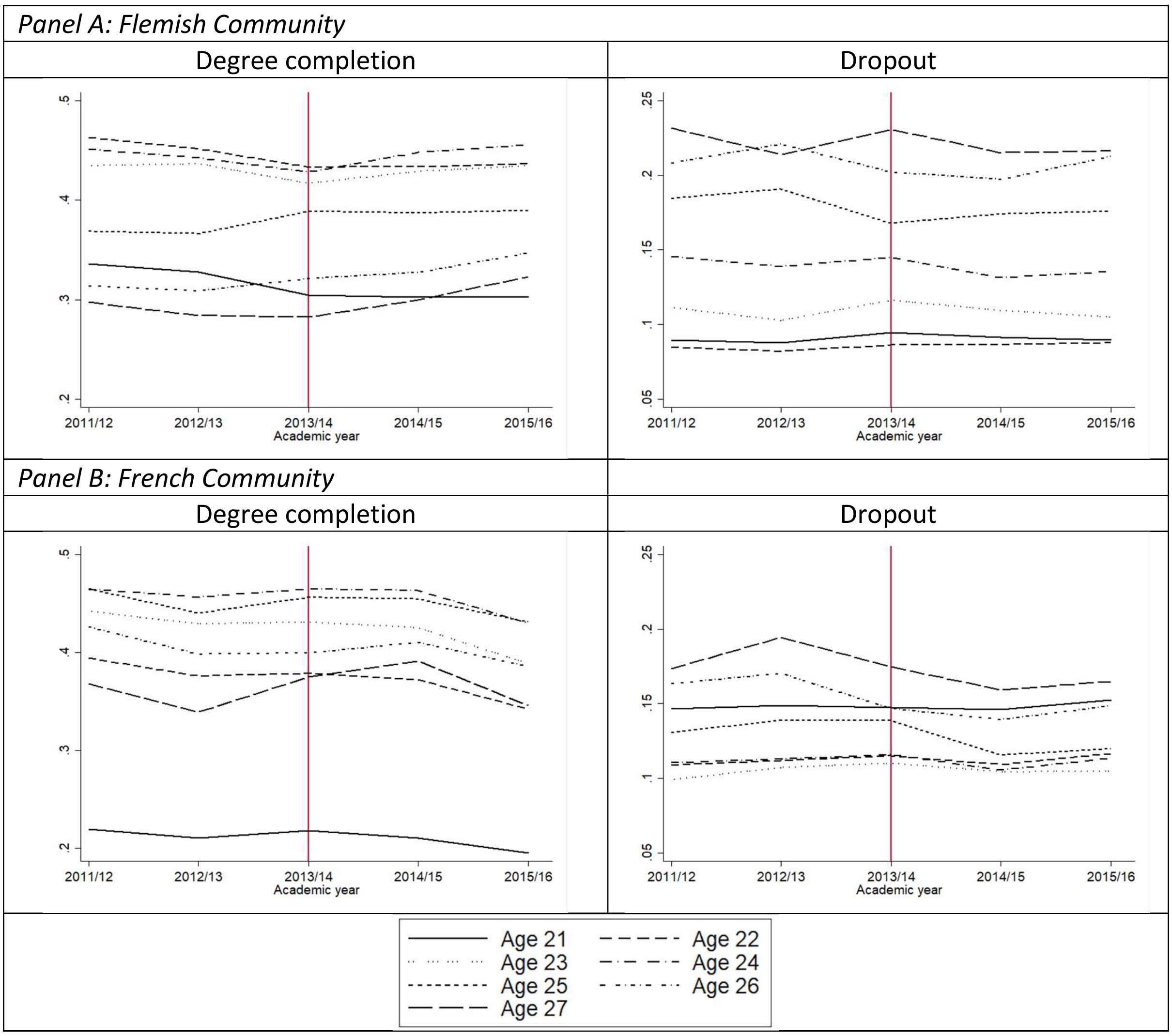
Note: Treatment group = 24-year-old students. Control group = 21-year-old students. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 to 2013-2014. Treatment period = 2014-2015 and 2015-2016. Results are based on weighted difference-in-differences regressions. The regressions allow for different trends in each region. 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 trend assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value for the test of parallel trends is reported for each specification and region. The p-value for the test of 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 A7. 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 for various tests		-
Equality of the effects in the Flemish and French Communities	0.731	
Parallel trend: Flemish Community	0.010	0.068
Parallel trend: French Community	0.445	-
Observations	135,507	79,929

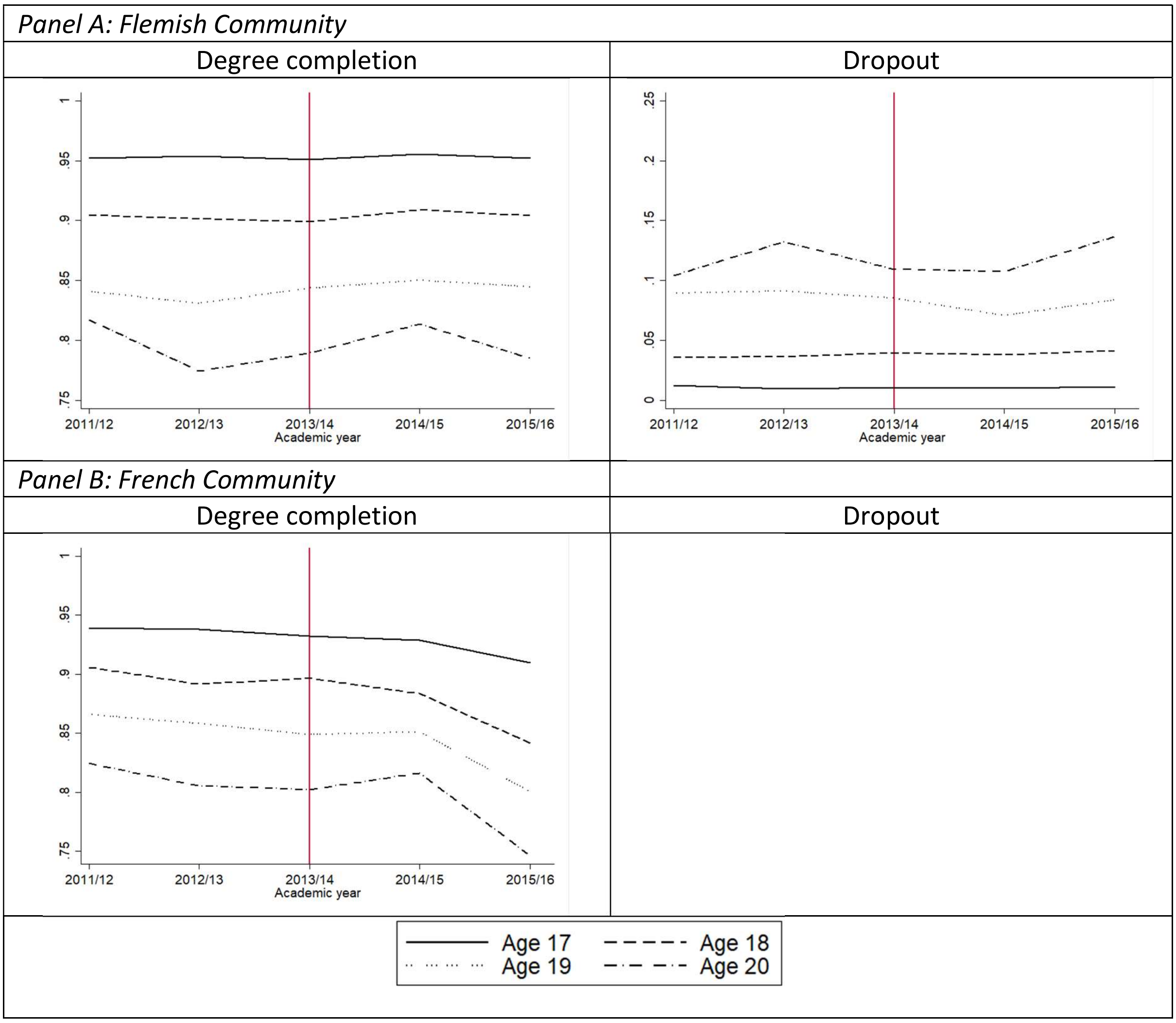
Note: Treatment group = 18-year-old students. Control group = For Flanders, 19-year-old students born before September 1 and 20-year-old students, and 20-year-old students for the French Community. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 to 2013-2014. 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 for the test of parallel trends is reported for each specification and region. The p-value for the test of 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.

Figure A1. Degree Completion and Dropout from Higher Education



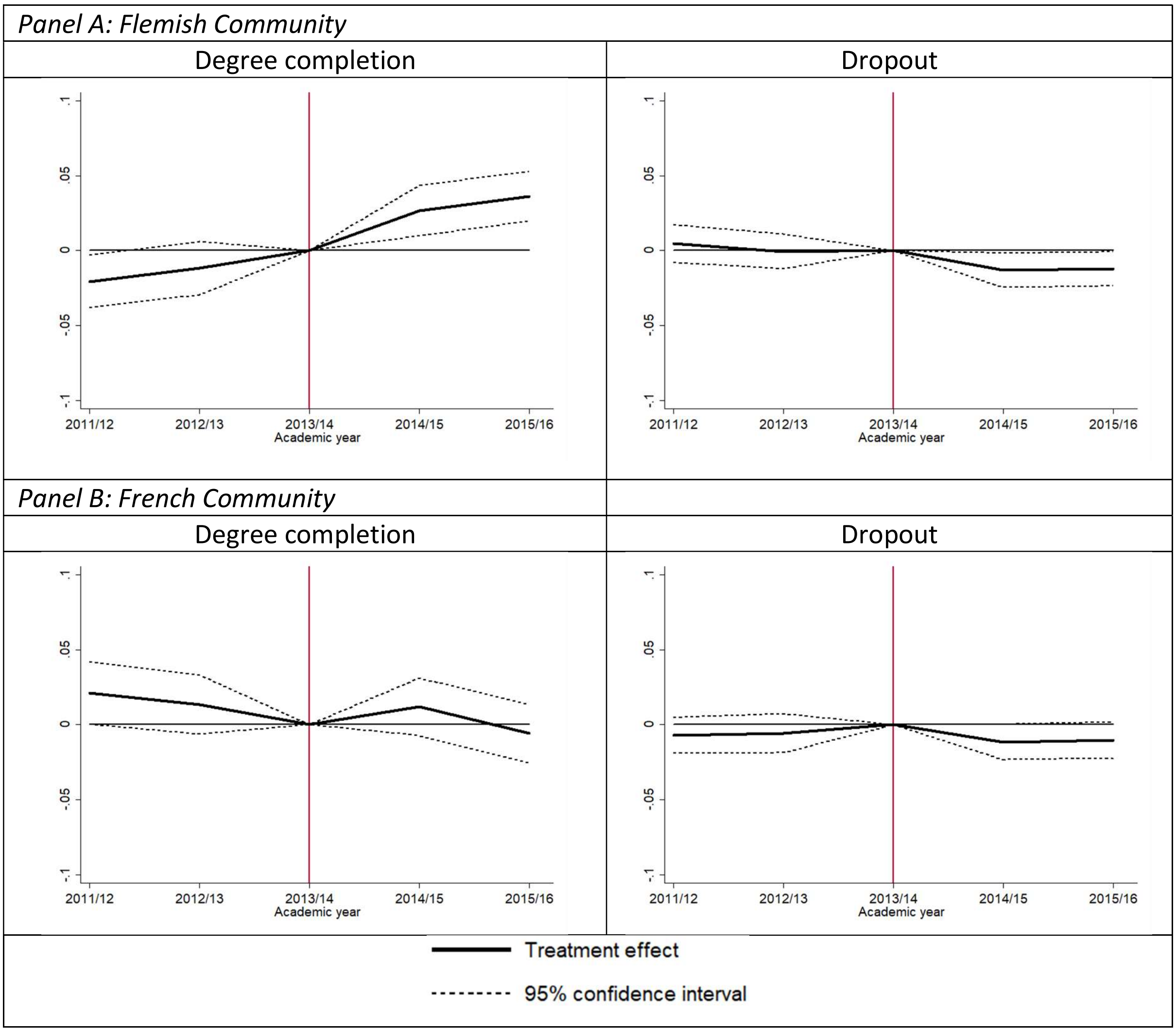
Note: Observed degree completion and dropout at the end of the academic year in master and professional bachelor programs. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 to 2013-2014. Treatment period = 2014-2015 and 2015-2016.

Figure A2. Degree Completion and Dropout in the Final Year of Secondary Education



Note: Observed degree completion and dropout at the end of the academic year in technical and vocational secondary education. Age is measured on December 31 of the respective academic year. Control period = 2011-2012 to 2013-2014. Treatment period = 2014-2015 and 2015-2016.

Figure A3. Degree Completion and Dropout of Belgian Students in Higher Education (alternative control group)



Note: Treatment group = 24-year-old students. Control group = 21-year-old students. Age is measured on December 31 of the respective academic year. The vertical line indicates the last year before the reform. The thick solid line shows the estimated treatment effect in the post-reform period and the placebo treatment effect in the pre-reform period as predicted by a saturated weighted difference-in-differences regression using 500 bootstrap replications. The thin dotted lines are the 95% confidence intervals of the (placebo) treatment effects.