ERASMUS UNIVERSITY ROTTERDAM

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Master Thesis

The effects of a Numerus Fixus on enrollment and first-year success in higher vocational education

Abstract

Higher education institutions (HEIs) in the Netherlands have introduced selection policies at an increasing level since the early 1970s to select students into their study programs. A cap on the number of enrollments – also known as a 'Numerus Fixus' – enables HEIs to reject those students that do not fulfil the admission criteria as stated by the HEI. This study combines student-level data of first-year students in higher vocational education ('HBO') and program-level data regarding selection policies of HEIs to examine what the introduction of a Numerus Fixus implies for 1) enrollment levels, 2) first-year success – as measured by switch- and dropout rates – and 3) the composition of enrollments. It builds on a standard difference-in-differences design by following the 'synthetic control method' as formulated by Abadie and Gardeazabal (2003). This study confirms that the introduction of a Numerus Fixus *mechanically* leads to a lower level of enrollments, as fewer students are allowed to enroll into the study program. Furthermore, the introduction of a Numerus Fixus leads to lower dropout- and switch rates and has a positive effect on the share of enrolled female-, native- and MBO students. This primarily goes at the expense of the share of non-Western- and HAVO students.

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Date final version:	02-02-2018

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1. Introduction

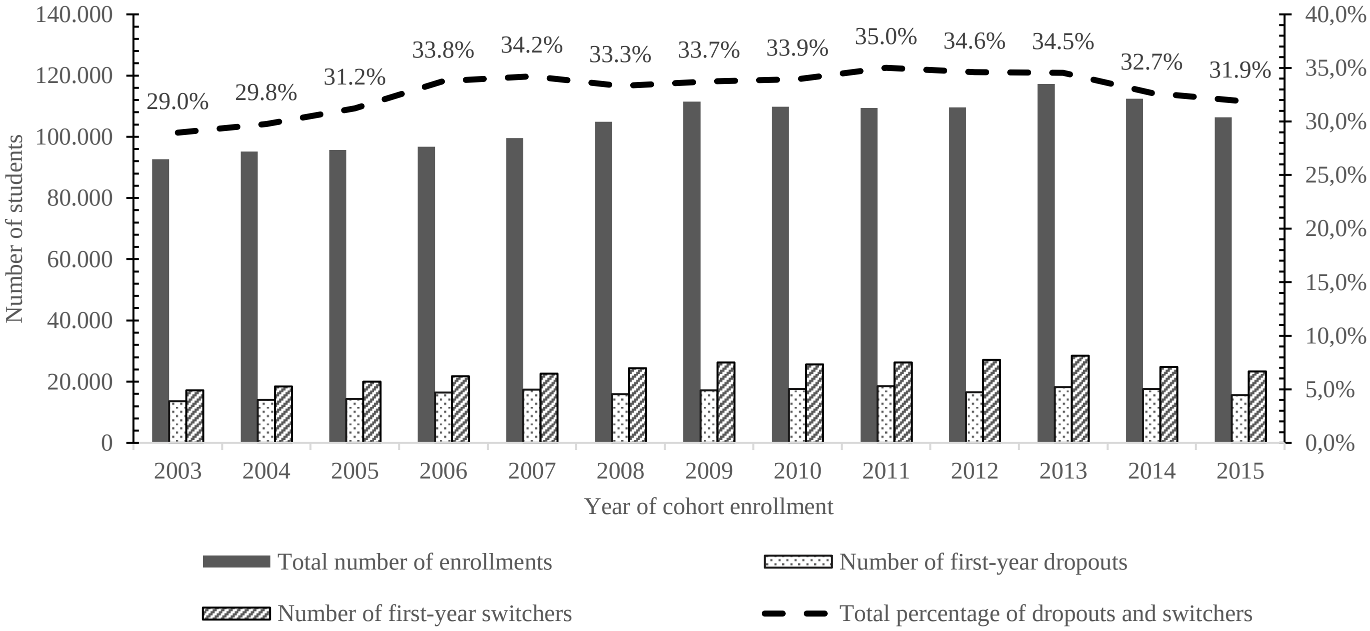
Since several decades, study programs and Higher Education Institutions (HEIs) in the Netherlands have introduced selection policies substantiated by a lack of capacity. In the early 1970s, medical schools decided that they could no longer facilitate the incoming flows of secondary school graduates and therefore introduced admission criteria. Since then, many programs – regardless of their sector or level of education – have followed this example.

Besides a lack of capacity, HEIs mention a variety of arguments to introduce a cap on the number of annual enrollments, also known as a 'Numerus Fixus'. For example, government legislation withholds HEIs to raise tuition fees to market-clearing levels due to accessibility concerns and might – in some cases – even impose a quota on the maximum number of enrollments into a study program. But more importantly, the use of a Numerus Fixus enables HEIs to select the upper layer of students that apply for the program (Winston, 1999). In theory, selecting better students on average improves academic performance and is therefore beneficial for the HEI. The next section elaborates on these mechanisms.

Dutch higher education has been characterized by relatively high switch- and dropout rates. Figure 1 shows the evolution of switch- and dropout rates in Dutch higher education during the period 2003-2015. This poses a problem not only for study programs and HEIs, but for students and society as well. Recent policy changes have increased the level of pressure on students to graduate within a nominal timespan. For example, the adoption of new funding legislation – known as the student loan system ('sociaal leenstelsel') – implies that students are no longer eligible for grants as in the previous system and now have to acquire loans to fund their education. Although students already had the possibility to acquire a student loan in the previous system, they no longer receive the monthly grant which was designated as a 'gift' under the condition that students finish their studies within a ten year timespan. Graduating one or more years after the nominal timespan of approximately four years – for example caused by switching or dropping out of a program – therefore has more substantial consequences than before, as it implies a 'loss' of funds invested in education. Similarly, every student is – in part – funded publicly and thus a lost year of education entails a waste of government funding.

If the use of a Numerus Fixus by HEIs leads to better academic performance by students – accompanied by lower switch- and dropout rates – this would imply a more efficient use of government funds. However, the introduction of a Numerus Fixus is aimed at attaining lower levels of enrollment and therefore the question arises 'what happens to those students that are not selected?' If the introduction of a Numerus Fixus merely shifts the problem from one HEI to another, is it desirable from a welfare perspective? Similarly, do selection policies affect all

Figure 1: Evolution of first year switch- and dropout rates in Dutch higher education



Notes: This figure uses data from the '1cyferho'-database from the Dutch executive education agency (DUO), which is also used for the analysis in this paper. The figure displays the evolution of the total number of enrollments, first-year switchers, first-year dropouts and the total percentage of dropouts and switchers between 2003-2015. Dropout denotes students that enroll in tertiary education but do not enroll into the second year of that program, nor enroll into another program within higher education. Switch denotes students that enroll in tertiary education but switch to another program and/or HEI during or after their first year. The total number of enrollments, number of first-year switchers and number of first-year dropouts are depicted on the vertical axis on the left. The total percentage of dropouts and switchers is depicted on the vertical axis on the right. The year of cohort enrollment is depicted on the horizontal axis.

socioeconomic groups equally or are some put in a disadvantage? These questions highlight the delicate balance between improving efficiency in Dutch higher education whilst preserving its accessibility regardless of students' income or socioeconomic background.

Students differ in their individual characteristics and might be affected differently by the introduction of a Numerus Fixus. Dutch government policy is aimed at preserving accessibility of higher education based on academic aptitude. When the introduction of a Numerus Fixus affects the composition of enrollments through mechanisms which are not related to academic aptitude such as socioeconomic background, this would indicate a failure in the current policy. Therefore, this paper also examines composition effects of the introduction of a Numerus Fixus. An example of a non-aptitude related mechanism is the acquisition of extracurricular training during secondary education to prepare for the admission criteria of HEIs, which is dependent on financial resources of students and parents. The next section elaborates on this mechanism and discusses potential composition effects – and the relevant mechanisms – of the introduction of a Numerus Fixus. Little is known about the consequences of the introduction of selection policy for HEIs, study programs, students and society. Thus, this paper attempts to shed light on several of these issues.

This paper examines the effects of the introduction of a Numerus Fixus on 1) the level of enrollments, 2) first-year success – as measured by retention-, switch-, and dropout rates respectively – and 3) the composition of enrollment in Dutch higher vocational education. For the latter, the effects on the share of female-, native-, non-native Western-, non-Western-, VWO- HAVO- and MBO-students are discussed respectively. The retention rate is defined as the share of students who continue the study program at the same institution after the first year. The retention rate therefore equals 100 percent minus the sum of the switch- and dropout rate after year 1. This paper focuses exclusively on higher vocational education ('HBO') rather than higher education in general, due to the available data. This is further clarified in section 4.2.

The study combines data of the Dutch education executive agency DUO and the 'Nationale Studentenenquête (NSE)' consisting of individual student-level and study program-level data respectively, creating a dataset of all first-year student cohorts from 2003-2015. It applies the 'synthetic control method' as formulated by Abadie and Gardeazabal (2003), building on the difference-in-differences method by assigning weights to control study programs in the 'donor pool' of control units in order to create a 'synthetic' control study program. This method allocates weights depending on the resemblance between treatment- and control study programs, which is based on the evolution of outcomes variables as well as individual- and study characteristics of the treatment unit. This provides a suitable counterfactual for regression analyses. Generally speaking, the synthetic control method does not use a single control unit or an average of control units, but instead uses a weighted average of the set of controls. A further illustration of this method is provided in section 4.2.

This study finds that the introduction of a Numerus Fixus leads to a direct 14 percentage point decrease of enrollments. Furthermore, retention increases by 4.9 percentage point after one year when controlling for individual- and study covariates. This effect is accompanied by a decline in dropout rates by 3 percentage point. Examining the composition of enrollments, the study suggests an increase in the share of female students by 4.2 percentage point after one year, an increase in the share of native students by 4.7 percentage point after one year, a decrease in the share of non-Western students by 4.1 percentage point after one year, and a direct 4.2 percentage point increase in the share of MBO-students. Other effects are not proven to be statistically significant at the 10 percent level.

This paper builds on existing literature regarding the effects of selection policy in higher education. Although several studies examine the effect of different admission criteria on later academic performance, little is known about the effects of selection policy on the retention rate of students, nor on the composition of enrollments. However, a recent study by the Dutch

Education Inspectorate (Inspectie van het Onderwijs, 2017) examines the effect of the introduction of a Numerus Fixus on the composition of enrollment in Dutch higher education. The study entails a before-after analysis and finds a slight decline in the relative number of non-Western- and male students, and in the relative number of students from a low socioeconomic background. The authors conclude that the current use of selection policy in the Netherlands does not indicate any substantial effects on the broad accessibility of higher education. However, the paper does advice a level of vigilance to prevent such developments in the future. This paper complements the current literature by exploiting rich student-level data, including a comprehensive set of individual and study- characteristics.

The remainder of this paper is organized as follows. The next section discusses relevant academic literature on the educational decision-making process of students, arguments for the introduction of a Numerus Fixus, predictors of academic success, and current literature regarding the use of selection policy in higher education. Then, relevant background information is provided regarding the Dutch education system and the evolution of selection policy in the Netherlands. Also, the data used for the analysis are presented. The next section discusses the empirical strategy used in this paper including its validity and assumptions. It also presents the descriptive statistics of the treatment- and control group. Then, the main estimates are presented which consist of a visual representation and statistical inference regarding the outcome variables as mentioned. The next section presents a robustness analysis. The final section concludes and discusses.

2. Literature

This section discusses important considerations regarding the investment in education by providing relevant economic theory. First, this section discusses human capital theory which forms the fundament of modelling education as an investment. It provides insight into the educational decision-making process of students. Additionally, the section discusses how a Numerus Fixus affects the optimal decision of students regarding their investment in education. It also discusses how students might be affected differently by the introduction of a Numerus Fixus due to heterogeneity in individual characteristics, and therefore suggests several mechanisms how such policy can affect the composition of enrollment. The second part discusses the most considerable arguments by Higher Education Institutions (HEIs) to introduce admission criteria. The third part discusses relevant factors at the individual level which can predict academic success in higher education. The final part discusses current literature regarding the effects of selection policies on academic performance.

2.1. Human capital theory

Economists have attempted to model educational decision-making since the mid-20th century. An important field of research known as human capital theory started with the work of several economists in the 1950s and 1960s, upon which many authors have built ever since. However already two centuries earlier, Adam Smith (1776) – the founder of classical free market economic theory – made an analogy between education and the investment in capital:

> *"A man educated at the expense of much labour and time to any of those employments which require extraordinary dexterity and skill, may be compared to [an] expensive machin[e]. the work which he learns to perform, it must be expected, over and above the usual wages of common labour, will replace to him the whole expense of his education, with at least the ordinary profits of an equal valuable capital." (p. 118).*

The fundamental framework of human capital theory was provided by authors such as Mincer (1958; 1962), Becker (1962; 1964) and Schultz (1963). Similar to the analogy by Smith, this field of theory suggests that education should be considered an investment that builds 'human capital' of an individual. In that sense, individuals should invest in education up until the point where the private marginal gains of an additional year of education equal the private marginal costs. In the view of Becker (1964), an investment in human capital is similar to an investment in physical capital, where the invested means include education and training whilst the output depends on the rate of return and is therefore related to individual characteristics such as talent and ability. Mincer (1974) states that the percental increase of labor income due to an additional year of schooling may be interpreted as the financial returns to education. According to the earnings function formulated by Mincer (1958, 1962), labor income forms the dependent variable in a regression including variables related to schooling and experience. A variety of authors has attempted to establish the returns to schooling in the Netherlands, leading to estimates ranging from 7 percent (Hartog et al., 1993) to 8-9 percent for an additional year of schooling (Leuven and Oosterbeek, 1999). Those who have attained higher levels of education receive significantly higher labor income compared to those who have not. Completing higher vocational education leads to a 30 percent higher wage on average compared to someone who only finishes primary education, whereas a university degree implies an 80 percent higher wage on average (Jacobs & Webbink, 2006).

The optimal decision of students regarding their investment in human capital can be attributed to (non-)monetary benefits and costs. The following subsections consider the most relevant benefits and costs. In addition, each subsection discusses how that aspect of the students' optimal decision is affected by the introduction of a Numerus Fixus and how students might be affected differently due to heterogeneity in individual characteristics. Consequently, the subsections suggest potential mechanisms of the introduction of a Numerus Fixus affecting the composition of enrollments.

2.1.1. Monetary benefits and rents

According to human capital theory, the most significant monetary benefit of an investment in human capital is the increase in future labor income due to higher productivity. By attaining education, an individual builds additional human capital which increases their productivity leading to a higher rate of return in the form of wage.[1]

The introduction of a Numerus Fixus affects the returns to education as it reduces the supply of students within a program, and subsequently reduces the inflow of workers within a profession. Consequently, this raises the price – i.e. wage – in the given profession leading to higher returns upon graduating from the program. The additional wage increase which is due to the introduction of a Numerus Fixus can be classified as 'rents' due to the monopolization of the supply of workers in that field. Friedman and Kuznets (1954) quantified the rents for US doctors in the 1950s. They compared their earnings to those of dentists, which had less strict entry requirements. The authors concluded that 16.5 percent of earnings could be attributed to "barriers to entry".[2]

2.1.2. Non-monetary benefits and consumption motives

Non-monetary private benefits of education consist of the potential 'joy' of studying, attaining a higher social status, better health and more exciting job opportunities. The CPB Netherlands Bureau for Economic Policy Analysis (CPB, 2009) shows that additional investments in education are correlated with improved health and longer average lifespan.[3] Moreover, Layard (1980) and Lommerud (1989) state that higher levels of education lead to better job opportunities and consequently higher social status. Investments in education can also be

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[1] Additional monetary benefits due to higher levels of education include lower unemployment (Ashenfelter & Ham, 1979).

[2] A more recent study by Ketel et al. (2016) estimates the returns to medical school by exploiting the lottery system in the Netherlands. The study finds that doctors earn at least 20 percent more than individuals ending up in their next-best occupation and confirms that this is largely due to the existence of profession-related rents.

[3] Clark and Royer (2013) show that an additional year of education decreases the probability of frequent smoking by 13 percent. Similarly, De Walque (2007) states that finishing a form of education after secondary school decreases the probability of frequent smoking by 40 percent. Studies by Grossman (2000; 2006) suggest that the effects of education on health outcomes are primarily driven by individuals being better informed about health-related risks. Furthermore, education raises income on average and therefore allows for larger investments in preventive measures in order to mitigate health-related risks.

attributed to consumption motives. Individuals might receive utility from additional investments in education, for example due to their ‘passion’ for the study program.

The introduction of a Numerus Fixus raises relative income or “status” as stipulated in a paper by Lommerud (1989). Assuming status materializes positively in the individual’s utility function, the introduction of a Numerus Fixus increases the attractiveness of a study program *ceteris paribus*. The introduction of a Numerus Fixus has an ambiguous effect on consumption motives for education. An increase in required effort to enroll into the program might negatively affect consumption motives whilst a higher quality of the program due to peer- and feedback effects might positively affect them. According to Winston (1999), peer effects imply that students’ academic achievements are higher on average in an environment of better students, including the overall quality of the program. Studies by Sacerdote (2001) and Zimmerman (2003) use quasi-experiments and do indeed find small but significant peer effects in higher education. Feedback effects can be interpreted as a ‘pull-factor’ where HEIs with an exceptional student population attract better students. Jacobs and Van der Ploeg (2006) suggest that the opportunity to teach outstanding students appeals to good faculty staff and highly distinguished professors consequently attract better students. Peer- and feedback effects therefore reinforce the effect of the introduction of a Numerus Fixus on consumption motives.

2.1.3. Monetary costs

The most significant monetary cost of an investment in human capital is the opportunity cost of not working during the period when the individual is attaining education. Every year an individual attends a form of schooling implies a lost year of potential labor income. Other monetary costs related to an investment in human capital comprise of direct costs such as tuition fees and the costs of books, teaching materials and other goods complementary to education. Direct costs in higher education add up to approximately €8500,- per student per year in the Netherlands, of which the student pays around 15 percent (Lanser, 2012). Jacobs (2012) states that, from the working age onwards, 70 percent of educational costs in the Netherlands consists of opportunity costs whilst 30 percent consists of direct monetary costs. It must be noted that direct costs are even lower for an individual due to governmental subsidies on tuition fees.

The introduction of a Numerus Fixus does not directly affect the monetary costs of students. However, it is important to consider the effects of a Numerus Fixus on the budget constraint of students. As tuition fees are identical for all study programs in higher vocational education and students have the same opportunities to obtain student loans, the student’s budget constraint is unaffected. However, Fixus programs require stricter admission criteria and therefore demand more effort of secondary school students to meet these criteria. This can prompt students – or their parents – to invest in extracurricular resources such as trainings to prepare for admission criteria. Through this mechanism, the budget constraint of students is potentially affected by the introduction of a Numerus Fixus. When a stricter budget constraint is related to individual characteristics of students – for example due to a lower socioeconomic background – the introduction of a Numerus Fixus might lead to a different composition of enrollments. A report by the Dutch Education Inspectorate (Inspectie van het Onderwijs, 2016) suggests that the attainment of so-called 'shadow education' is indeed increasingly common and appears to have adverse effects on equal opportunities in education.

2.1.4. Non-monetary costs: effort

An investment in human capital also involves non-monetary costs. Such costs for example include the perceived effort of studying. Both Carneiro and Heckman (2003) and Palacios-Huerta (2006) show that non-monetary costs are relatively high, especially for students coming from a lower socioeconomic background. These students are likely to receive less stimulus and financial means from their family, leading to relatively lower participation rates. Carneiro and Heckman (2003) confirm that non-monetary factors such as effort, motivation, social stimuli and social cultural background are important for the investment decision of students.

As discussed before, the introduction of a Numerus Fixus requires students to invest more effort in secondary school in order to meet stricter admission criteria. When the invested effort is on average related to individual characteristics – as suggested by studies from Carneiro and Heckman (2003) and Palacios-Huerta (2006) – the introduction of a Numerus Fixus might lead to a different composition of enrollments.

2.1.5. Non-monetary costs: risk

An important factor explaining differences in educational investments is related to the notion of risk. Students are unable to know upfront whether education will lead to a beneficial outcome. For example, students risk becoming unemployed or having to accept jobs below their skill level (Palacios-Huerta, 2003; 2006). In conjunction, risk-aversion implies the behavior of individuals when exposed to uncertainty. Heterogeneity in risk-aversion might therefore explain differences in educational decision-making across individuals.

Dohmen et al. (2011) examine risk attitudes of people complemented by a behavioral experiment using paid lottery choices to show that levels of risk aversion do indeed differ across individuals. Buonanno and Pozzoli (2007) show that Italian students take the chance of failure into account when choosing a study program and those with a lower socioeconomic background tend to be more risk averse. Hryshko et al. (2011) study the determinants of individual attitudes towards risk and find that policy-induced increases in secondary school graduation rates lead to significantly fewer risk averse individuals in the next generation. Furthermore, they establish that parents' attitude towards risk is a significant determinant of risk aversion.[4]

Several authors have attempted to establish the relationship between risk aversion and educational attainment. For example, Chen (2003) finds that individuals with higher risk aversion have a 4 percent smaller chance to enroll into university. Brown et al. (2006) also suggest that an individual's degree of risk aversion is inversely correlated with their educational attainment. Similarly, they state that the parents' degree of risk aversion is negatively related to the academic performance of their children.[5] A higher level of risk aversion might affect the investment in education through lower willingness to attain student debt, as shown by Brown et al. (2011).

Another determinant of risk aversion might be related to gender. Studies by Hartog et al. (2002) and Dohmen et al. (2011) suggest that women tend to be more risk averse on average than men. It should be noted however, that higher levels of patience might compensate in their educational attainment. Notably, women are less prone to hyperbolic discounting in their educational attainment (Kirby & Marakovic, 1996). Besides gender, Dohmen et al. (2011) show a negative relationship between age and risk tolerance, as well as a positive relationship between parents' education and risk tolerance.

The introduction of a Numerus Fixus might affect risk through several mechanisms. Stricter admission criteria increase the risk of being rejected for a study program, therefore implying that the invested effort – and potentially funds through shadow education – for application might not pay off. This provides an incentive for students to prefer non-Fixus studies as the 'safer' option. When the level of risk-aversion is related to individual characteristics – as suggested by the papers stated before – the introduction of a Numerus Fixus might lead to a different composition of enrollments.

[4] Using data from the German Socio-Economic Panel (SOEP), Leuermann and Necker (2011) confirm that the willingness to take income risks is transmitted from parents to their children. In addition, both studies state that individuals from relatively low socioeconomic backgrounds are more risk averse on average.

[5] Belzil and Leonardi (2013) use Italian panel data – including individual differences in attitudes towards risk, based on lottery pricing experiments – to investigate the effect of risk aversion on the probability of entering higher education. They suggest that, conditional on being eligible to enroll into university, a risk tolerant individual has a 3-percentage point greater probability to attain a university degree compared to someone risk averse. One should however be cautious in interpreting the causal directions of the results in these studies, as higher levels of education might also affect risk preferences.

2.2. Arguments for the introduction of a Numerus Fixus

There are several incentives for Higher Education Institutions (HEIs) to introduce a Numerus Fixus. Primarily, the introduction of a Fixus forms a strategic restriction of educational supply to create excess demand by students and therefore allows selectivity. This enables HEIs to select the best students. Winston (1999) provides an analogy between profit-making firms and HEIs to highlight particular incentives to restrict the enrollment of students.

> *"A school's student-customer population defines and restricts the sources of an input important to its product. Because different customers bring different measures of those inputs—quite apart from their demand for the product, some students will supply high quality inputs while others will not—institutions have strong incentives to care about the identity of those to whom they will sell, and to try to control or influence who their customers will be. Schools are able to do this through excess demand queues that allow them to select those to whom they will sell." (p. 18).*

As discussed in the previous section, the selection of better students consequently leads to peer- and feedback effects. Besides having a positive effect on applications, peer-effects also lead to higher academic success as students perform better in an environment of motivated students. This is reinforced by feedback effects, as HEIs with exceptional students attract better students and staff, whilst a highly distinguished staff attracts motivated students (Winston, 1999).

Another argument for the introduction of a Numerus Fixus by HEIs is a lack of capacity. Jacobs and Van der Ploeg (2006) suggest that admission standards in Europe are generally set due to a lack of capacity and because regulations forbid HEIs to raise tuition fees when facing excess demand. The limited presence of facilities forms an issue for medical- and technical study programs in particular. Notably, the maximum number of enrollments into Medicine studies in the Netherlands is set by the Dutch Minister of Education through a quota. This number is mainly based on the available places in internships and postgraduate specialization tracks (Ketel et al., 2016).

An excess demand for education in a situation of fixed supply could theoretically be solved by increasing the 'price' of education, i.e. tuition fees. If capital markets would function optimally, individuals would be able to attain loans to fully fund their education. However, students are unable to provide their 'human capital' as collateral to attract funding. These capital market imperfections therefore raise liquidity restrictions for those with little wealth. As educational policy in the Netherlands is aimed at providing equal opportunities regardless of socioeconomic background and wealth, HEIs are not allowed to raise tuition fees at will. If HEIs could raise tuition fees at liberty, this would make higher education merely accessible for those with financial means thus potentially placing low performing students in expensive programs (Jacobs & Van der Ploeg, 2006). Selection through academic aptitude rather than financial means might consequently lead to better academic performance on average.

Other arguments for the introduction of a Numerus Fixus are related to the profession in which students from a particular study program generally commence their career. As discussed before, the introduction of a Fixus potentially leads to 'rents' due to the monopolization of the supply of workers in that field. This forms an incentive for representatives to appeal for a restriction of enrollments into relevant study programs.

2.3. Predictors of academic success

Section 2.1. provides insight in how the optimal decision of students is affected distinctively by the introduction of a Numerus Fixus due to heterogeneity in individual characteristics. HEIs use admission criteria in order to select better students. This section extends this line of reasoning by examining how heterogeneity in individual characteristics might be correlated with the success-rates of study programs. It therefore suggests how changes in the composition of enrollment – as a result of the introduction of a Numerus Fixus – might be related to changes in academic success rates of HEIs.

Academic literature suggests that academic success can be attributed to a variety of individual characteristics. First, several studies suggest that gender is an important predictor of academic success. Johnes and Taylor (1989) use a regression framework to estimate differences in undergraduate non-completion rates in the UK. They find that non-completion rates are 40-50 percent higher for males than for females in late 1970s cohorts.[6] A more recent study by Scott et al. (2006) uses regression analysis to evaluate public colleges in the US and finds that schools with a larger share of women show higher graduation rates. Jacob (2002) uses longitudinal data of a nationally representative cohort of eight grade students in the US to examine differential college attendance rates of men and women. He finds that non-cognitive skills and college premiums among women account for nearly 90 percent of the gender gap in higher education.

Another individual characteristic related to academic performance is age. St. John et al. (2001) study heterogeneity in academic persistence and find that age is negatively correlated

[6] Studies by Mortenson (1997; 1998) and by Astin and Oseguera (2002) also find relatively higher retention and graduation rates for females.

with persistence. Timmermans et al. (2011) state as a possible explanation that students who are older when they enroll are more likely to have repeated a class in primary or secondary school due to lower performance. This suggests a selection effect of older students on later academic outcomes. Conversely, Metzner and Bean (1987) suggest that older students have lower 'intent to leave' college which consequently leads to lower dropout rates.

Students' ethnicity can be related to academic outcomes as well. A recent paper by Rienties et al. (2012) makes a cross-institutional comparison of business schools in the Netherlands and shows that non-native students with a western ethnic background perform better academically compared to domestic students. Students from a non-western background perform similar to natives.[7] Rendon (1995) suggests that academic performance of ethnic minorities is lower on average through several mechanisms. These students are often the first in their families to attend higher education and therefore lack realistic expectations. Furthermore, their academic preparation falls behind that of their native peers.

2.4. Current literature related to selection policy

This section discusses existing literature on the effects of admission criteria by HEIs on academic performance outcomes. Using a Numerus Fixus to select students into a program is essentially a mechanism by HEIs to ration the supply of education over students. The assignment of available study slots forms the relevant mechanism of rationing supply and this can be accomplished in a variety of ways. This section presents several mechanisms how HEIs can select students into their programs and how they are related to academic success. It highlights how little is known about the effects of admission criteria on academic success, and therefore emphasizes the relevance of this paper. Additional scope for future research is discussed in the concluding paragraphs of this paper.

Several studies examine different admission criteria and test the predictive validity of widely used measures such as GPA and standardized test scores. Arrow (1993) suggests that the use of either measure is disconcerting as they are unable to provide a consistent and valid prediction of academic achievement beyond the first year of education. A meta-analysis of thirty published studies by Morrison and Morrison (1995) reviews the validity of the widely applicated Graduate Record Examination (GRE) to predict academic outcomes. The authors do not find significant correlation between GRE scores and GPA levels of students. Similarly, Arrow (1993) shows that first-year GPA is unrelated to graduate school grades and later career

[7] A study by Salamonson and Andrew (2006) aimed at assessing the academic performance of nursing students in Australia finds that non-natives show lower academic performance on average.

outcomes. Furthermore, Girves and Wemmerus (1988) show that first-year grades do not have a significant effect on persistence or degree completion rates. In contrast, several studies find positive correlations between selection mechanisms and academic performance. For example, Noble and Sawyer (2002) use logistic regression models to estimate the predictive levels of ACT Composite test scores and secondary school GPA on first-year college GPA. They find that both selection criteria were effective in predicting academic success during the first year of university.[8]

The use of standardized tests as an admission criterium may have several adverse impacts. For example, Brazziel (1992) suggests that standardized tests lead to an underprediction of academic aptitude of older students due to their relative unfamiliarity with tests.[9] Furthermore, a study by Powers (1986) shows that the performance of students on standardized tests might be negatively affected by anxiety and stress. An evaluation of 40 studies by Kulik et al. (1984) furthermore shows that students can raise their scores on aptitude tests by practicing and purchasing training. The use of tests as admission criteria might therefore adversely lead to better outcomes for those who can afford training, rather than merely selecting on academic aptitude.

Over the years, more HEIs have started to introduce admission interviews to select prospective students. A study by Elam and Johnson (1992) examines a medical school in the US and suggests that admission interviews are predictive of third- and fourth-year performance indicators. However, a recent study by Wouters (2017) suggests that the introduction of admission interviews by medical schools in the Netherlands has not led to higher academic performance compared to the previous lottery system. A study by Sandow et al. (2002) uses multivariate regression models to examine the relationship between a variety of admission criteria and dental school performance for classes at an American university. The results indicate that GPA, standardized test scores and interview scores were consistent determinants of academic results.

[8] Similarly, McKenzie and Schweitzer (2001) study predictors of academic performance for first year Australian University students and suggest previous academic performance as the most significant predictor of university performance.
[9] It must be noted that this argument could have lost some of its relevance. More recent research should suggest whether older students are still relatively unfamiliar with tests.

3. Background and data description

3.1. The Dutch education system

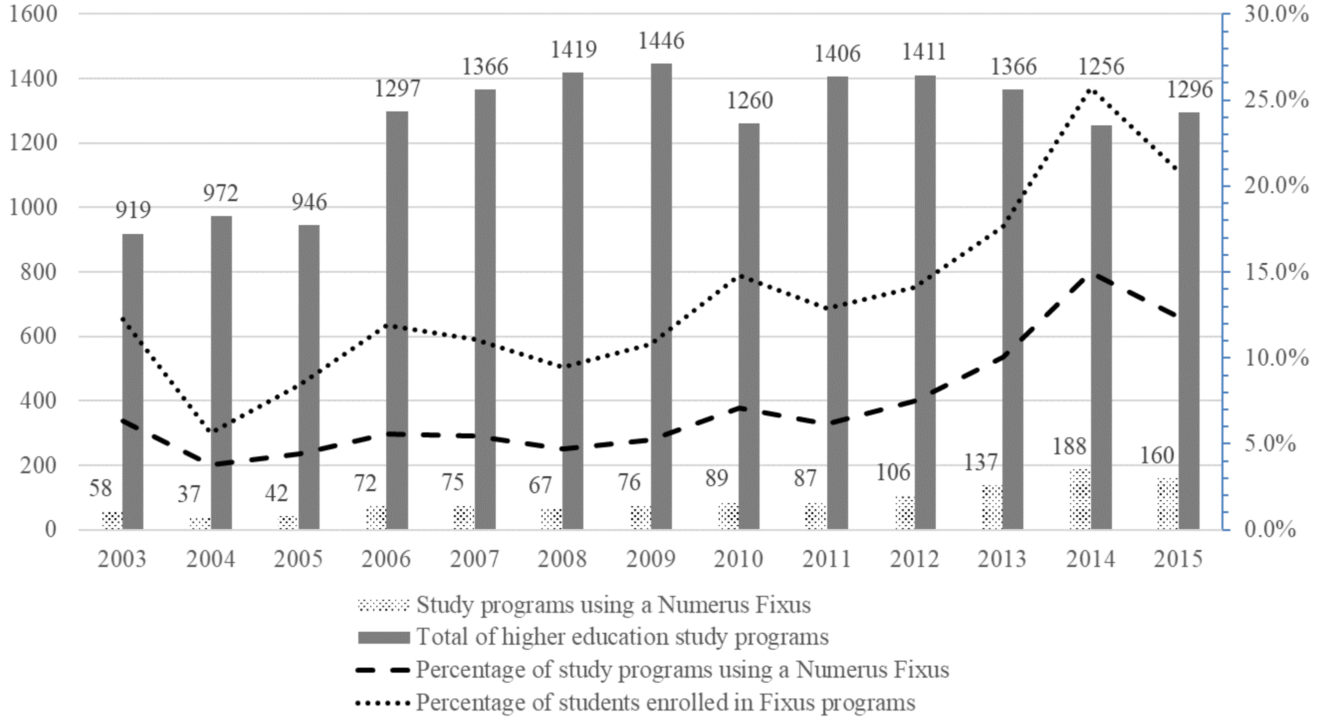
The Dutch secondary school system is divided into three main levels of education. First, pre-vocational education – 'Voorbereidend Middelbaar Beroepsonderwijs (VMBO)' – lasts four years. This level of education prepares students who are then around the age of 16, to continue their education in the 'Middelbaar Beroepsonderwijs (MBO)' which lasts another one to three years. Second, the 'Hoger Algemeen Voortgezet Onderwijs (HAVO)' lasts five years and prepares students for vocational university, also known as 'university of applied sciences' or 'Hoger Beroepsonderwijs (HBO)'. This lasts another four to five years. Third, pre-university education – 'Voortgezet Wetenschappelijk Onderwijs (VWO)' – lasts six years and prepares students for university. Furthermore, students graduating from the highest level of MBO ('MBO-4') may enroll into HBO education. Around 40 percent of eligible MBO graduates do indeed enroll into HBO programs (Woudstra, 2017).

3.2. The evolution of selection policy

Higher education institutions (HEIs) in the Netherlands have implemented selection procedures since the beginning of the 1970s. The implementation of selection policy mainly started with medical schools due to a large inflow of students. Study programs such as dentistry, medical- and veterinary studies introduced a system of weighted lotteries from the 1970s until the late 1990s to cope with large numbers of prospective enrollments. After a central registration procedure, students were placed into one of five brackets depending on their secondary school GPA. The higher a student's GPA, the higher the odds to be admitted into the program.

In the late 1990s, several cases were reported about students with an extraordinary high GPA being rejected for study programs. Consequently the system was adjusted in 1999, enabling students with a GPA of 8 or higher to be automatically accepted into the program (Walsum, 1998). Simultaneously, study programs applying a weighted lottery were allowed to use a decentral selection procedure in addition. These programs were initially permitted to select a maximum of 50 percent of their enrollments through a decentral selection procedure. An important condition entailed that the decentral selection procedure was aimed at enrolling students based on personal qualities relevant to the program which were not related to their GPA. Figure 2 illustrates the evolution of study programs in Dutch higher education applying admission criteria through a Numerus Fixus from 2003-2015.

Figure 2: Evolution of the use of a Numerus Fixus



Notes: This figure uses data from the 'Nationale Studenten Enquête (NSE)' provided by Studiekeuze123, which is also used for the analysis in this paper. The figure displays the evolution of the use of a Numerus Fixus by study program-HEI combinations in the Netherlands from 2003-2015, relative to the total number of study program-HEI combinations. The number of study program-HEI combinations is depicted on the vertical axis on the left. The percentage of study program-HEI combinations using a Numerus Fixus and the percentage of students enrolled in Fixus programs are depicted on the vertical axis on the right. The year of cohort enrollment is depicted on the horizontal axis.

The decentral selection procedure involves several important distinctions compared to the central lottery procedure. When a study program uses a central lottery procedure, it communicates the number of available study slots to 'Studielink' – the registration and enrollment platform for all public institutions of higher education in the Netherlands – which then takes over the enrollment procedure. Studielink places students with a GPA of 8 or higher automatically on the list of enrollments and assigns the remaining slots using a weighted lottery. As discussed before, students are placed into one of five brackets depending on their GPA. A higher GPA leads to a higher weight in the lottery and therefore entails better odds to be admitted into the program. Conversely, when a study program uses a decentral selection procedure, the HEI handles the application procedure itself. The study program is obligated to state the number of available study slots and selection procedure before the application procedure commences. The study program then applies the selection procedure to all students that completed a correct application and constructs a ranking based on their admission criteria. Those students that are high enough on the ranking are enrolled into the program.

From 2011 onwards, study programs could select 100 percent of their enrollments through a decentral selection procedure. At the same time, this year marked the first technical study programs introducing a Numerus Fixus. Recently, a law has been passed stating the abolishment of lotteries in the selection procedures of higher education institutions (HEIs) from the academic year 2017-2018 onwards. Study programs using a Numerus Fixus are from now on obliged to select students through a decentral selection procedure. At least two qualitative criteria must be applied in this selection procedure. Examples of such criteria are interviews, motivation letters and non-cognitive tests. Furthermore, a shift of registration deadlines towards the beginning of the senior secondary school year implies that study programs rely on GPAs based on the second last year in their selection procedure, entailing a much smaller role for the final exams.

3.3. Data description

This study uses a combination of two educational databases. First, it uses individual student-level data of all enrolled first-year students in higher vocational education in the Netherlands from 2003-2015. This information is obtained from the '1cyferho'-database provided by the Dutch educational executive agency (DUO). This database contains information about the age, gender, ethnicity, migration background, enrollment and previous education of each first-year student in a given year. Furthermore, it includes information on whether a student dropped out of higher education or switched to a different program at the end of- or during that year. This information is complemented by study program level data of all HEI-programs in the Netherlands from 2003-2015. This information is obtained from the 'Nationale Studenten Enquête' (NSE) provided by 'Studiekeuze123' and includes data on the introduction of a Numerus Fixus by HEIs. The combination of these databases enables the distinction between students who enroll into studies with or without a Numerus Fixus and therefore forms the fundament of this study.

This study focuses on several main outcome variables. First, the retention of students after their first year. This outcome is defined as 1 if a first-year student continues his current study program at the same HEI and as 0 if he does not *regardless of the underlying motivation*. The latter could either imply that the student switches to the same program at a different institution, to another program at the same institution or that the student drops out of higher education completely. Therefore, the second dependent variable is whether the student switches to another program at the end of the first year. This outcome is defined as 1 if a student switches to another program at the same institution or to the same program at another institution after his first year. The outcome is defined as 0 otherwise. The third dependent variable is whether a student drops out of higher education during the first year. This outcome is defined as 1 if a student drops out of higher education completely during his first year. The outcome is defined as 0 otherwise. These outcome variables are measured at the individual student-level, as this enables the analysis to control for individual characteristics. The final main outcome variable is the annual enrollment of students into study programs. In contrast to the previous outcome variables, this outcome is measured at the program-level and is defined as the logarithm of the number of students enrolling into a study program in each year. The logarithm is applied as it simplifies the interpretation of enrollment effects, where (log) enrollment states the percentage point change due to the treatment. The empirical strategy section elaborates on this.

This paper also examines compositional effects on enrollment due to the introduction of a Numerus Fixus by HEIs. The analysis attempts to establish how the relative number of female-, native-, non-native Western-, non-Western-, VWO-, HAVO- and MBO students are affected by the treatment respectively. All compositional outcome variables measure the percentage point change in the share of the given dependent variable due to the introduction of admission criteria by HEIs. These effects are examined using data at the individual student-level.

This study attempts to measure the effect of the introduction of a Numerus Fixus on the outcome variables as discussed. The treatment implies being enrolled as a first-year student in a study program that introduced a Numerus Fixus. In the context of examining the effects on (log) enrollments, the treatment implies introducing a Numerus Fixus as a study program-HEI combination. This information is available on the study program-HEI level. Control variables related to individual characteristics are added to the equation. These include age, gender, previous education, ethnicity, whether a student has graduated from higher education before and whether a student is a direct entrant. Age is measured at the 31st of December in the year of enrollment. A student's previous education can entail either VWO, HAVO, MBO or 'other', where the latter could – for example – imply that a student did not attend secondary school in the Netherlands. Ethnicity entails a student having a native-, non-native Western- or non-Western background. Being a higher education graduate means that a student has graduated from a study program in either HBO or WO before enrolling into the current study program. Being a direct entrant implies that a student enrolls into tertiary education in the same year as they graduate from their previous education, rather than taking one or more gap-years. Furthermore, year fixed-effects and study program-HEI fixed effects are included. The next section discusses the interpretation of these mechanisms more extensively.

4. Empirical strategy

The HEIs introducing a Numerus Fixus might differ from those who do not. This non-randomness of introduction might be related to characteristics both on an individual student-level as on a HEI-program level. Therefore student- and/or HEI characteristics may be correlated with the inclination of HEIs to introduce a Numerus Fixus. Similarly, this may be correlated with non-observable characteristics. To commence the analysis, the following OLS framework controls for a set of individual characteristics X_{ijt} and HEI characteristics Z_{jt} :

$$Y_{ijt} = \alpha_0 + \alpha_1 T_{jt} + \beta X_{ijt} + \gamma Z_{jt} + \varepsilon_{ijt} \qquad (1)$$

In this framework, Y_{ijt} is the outcome variable of student i from HEI-program j at time t. T_{jt} is the treatment variable, which equals 1 if a Numerus Fixus is introduced by HEI-program j at time t, and equals 0 otherwise. Consequently, α_1 is the parameter of interest. Furthermore, X_{ijt} is a vector of individual student characteristics and Z_{jt} is a vector of HEI characteristics. Additionally, the framework includes an error term ε_{ijt}.

In this framework, the error term ε_{ijt} might include unobserved individual student and/or HEI characteristics. Whenever those characteristics are correlated with the treatment variable T_{jt} and the outcome variable Y_{ijt}, this results in omitted variable bias. Consequently, the parameter of interest α_1 is a biased estimator of the effect of the introduction of a Numerus Fixus on the outcome variable. In order to account for potential biases, this study proposes the following identification strategies.

4.1. Difference-in-differences

In an attempt to obtain an unbiased estimator of the effect of the introduction of a Numerus Fixus on the outcome variables, this paper first attempts to use a difference-in-differences approach. This method compares students attending HEIs that introduce a Numerus Fixus, the treatment group, to students attending HEIs that do not, the control group:

$$Y_{ijt} = \alpha_0 + \alpha_1 T_{jt} + \beta X_{ijt} + \gamma Z_{jt} + \varphi_j + \omega_{\mathrm{t}} + \varepsilon_{ijt} \qquad (2)$$

This equation is similar to equation (1). However, it includes study program-HEI fixed effects φ_j and year fixed effects ω_t. Introducing these fixed effects enables the model to account for unobserved time-invariant heterogeneity between study program-HEI combinations and for annual shocks respectively. Furthermore, standard errors are clustered at the study program-

HEI-year level as student outcomes within a cohort of the same study program and HEI are not independent. The treatment T_{jt} indicates whether a student is enrolled at a HEI-program which has introduced a Numerus Fixus. In the analysis assessing the (log) number of enrollments, the model is estimated at the program-level rather than the student-level, as discussed before. This method compares study program-HEI combinations that introduce a Numerus Fixus, the treatment group, to study program-HEI combinations that do not, the control group:

$$Y_{jt} = \alpha_0 + \alpha_1 T_{jt} + \varphi_j + \omega_t + \varepsilon_{jt} \tag{3}$$

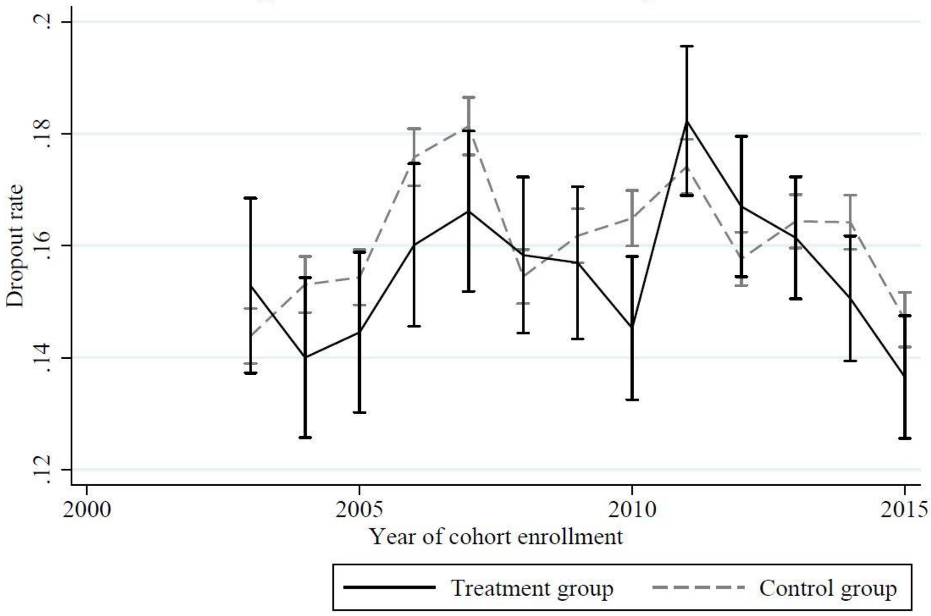
In this framework, Y_{jt} is the (log) enrollment of study program-HEI j at time t. T_{jt} is the treatment variable, which equals 1 if a Numerus Fixus is introduced by study program-HEI j at time t, and equals 0 otherwise. Consequently, α_1 is the parameter of interest. φ_j are study program-HEI fixed effects and ω_t are year fixed effects. Furthermore, standard errors are clustered at the study program-HEI-year level.

In order to obtain an unbiased estimator of the average treatment effect, a set of assumptions has to be made. The key assumption of a difference-in-differences approach is the common trend assumption, implying that the pre-treatment evolution of the outcome variable of the treatment group should follow a similar trend as that of the control group. To test for this assumption, separate graphs are plotted for all outcome variables for both the treatment and control group. As an example, figure 3 shows the evolution of dropout rates for both the treatment and the control group. This figure shows that the dropout rates for the treatment- and control group do not follow the same trend along the pre-treatment period 2003-2014 and therefore indicates a violation of the common trend assumption. Common trend plots for the other outcome variables are presented in Appendix A.

A second assumption of the difference-in-differences approach requires that HEIs in the treatment group do not introduce other policies – that affect selection – in the years where the Numerus Fixus is introduced, when the HEIs in the control group do not and vice versa. When this assumption is violated, the parameter of interest reflects both the effect of the introduction of the Numerus Fixus, as well as the effect of the other policy implying a biased estimator.

An example of a potential threat would be the introduction of academic dismissal policies, better known as a 'binding study advice' (BSA). A binding study advice implies that students must attain a given amount of academic credits ('ECTS') during their first year, and are obligated to leave the program otherwise. The implementation of such policy is likely to affect the enrollment and first-year academic success of students through similar mechanisms

Figure 3: Common trend - dropout rate



Notes: This figure uses data from the '1cyferho'-database from the Dutch executive education agency (DUO), which is also used for the analysis in this paper. The figure displays the evolution of the dropout rate for both the treatment group and the control group over the period 2003-2015, as to visualize the lack of a common trend needed for a difference-in-differences analysis. The solid black line depicts the treatment group. The dashed gray line depicts the control group. The figure includes visualizations of the 95 percent confidence intervals. The retention rate is depicted on the vertical axis. The year of cohort enrollment is depicted on the horizontal axis.

as discussed in section 2 of this paper. It would form a threat to the validity of our results as we do not only measure the effects of the introduction of a Numerus Fixus, but also the effects of the binding study advice.

Unfortunately, this study cannot account for the potential introduction of academic dismissal policies due to data limitations. However, a study by the Dutch Education Inspectorate (Inspectie van het Onderwijs, 2010) suggests that 98 percent of HBO studies used a BSA in 2010. In addition, no announcements are found suggesting the repeal of such policy. Overall, this provides reasonable confidence that the introduction or repeal of such policy does not bias the results of this study.

Furthermore, the introduction of a Numerus Fixus should not affect individuals outside the treatment group. This is known as the 'no interference assumption'. A potential threat to this method is the occurrence of spillover effects. For example, the introduction of a Numerus Fixus can lead to fewer or additional students switching from treatment studies to study programs in the control group. This in turn would affect the academic success rate in the control group and cause the parameter of interest to under- or overestimate the effect of a Numerus Fixus on the outcome variables in question. In other words, the parameter of interest would capture the 'combined effect' of 1) the change of the outcome variable for the treatment group and 2) the inverse change of the outcome variable for the control group.

The presence of spillover effects is quite plausible, as students who do not enroll into Fixus programs due to the admission criteria are likely to enroll into other study programs and therefore affect the rate of academic success of the non-Fixus program. Similarly, students might anticipate the introduction of a Numerus Fixus and therefore not apply for treatment HEIs at all as they fear being rejected. When these students enroll into study programs in the control group, this again leads to an over- or underestimation of the treatment effect.

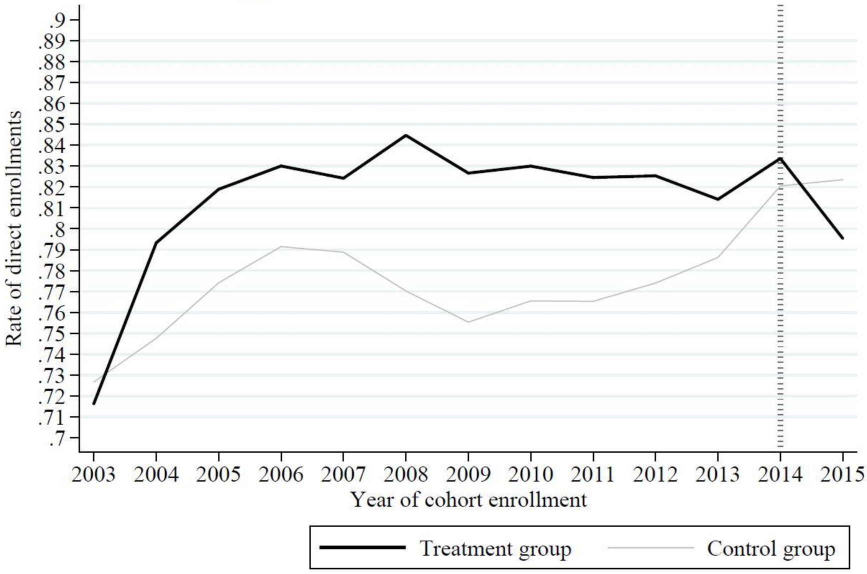
In addition, if the use of admission criteria enables HEIs to select 'better' students, then the non-selected students might enroll into non-Fixus programs and adversely affect their academic success rate. Therefore, the parameter of interest would measure both the increase in academic success of the Fixus program and the decrease of the non-Fixus programs. It must be noted however, that this depends on whether the student performs equally well in both programs. If the introduction of a Numerus Fixus leads to higher academic success for both Fixus and non-Fixus studies – for example due to a more effective allocation mechanism – this would lead to an underestimation of the effect.

This study attempts to account for spillovers by removing study program-HEI combinations from the donor pool that are likely substitutes for treatment programs. Using historical data regarding the study programs that students switch to from treatment programs during the period 2003-2015, a list of eligible substitute programs is constructed and subsequently removed from the donor pool. This method assumes that historical switching behavior forms a reliable proxy for both future switchers and anticipation effects as a result of the treatment.

Another form of anticipation is through time. Students that were previously planning to take a gap-year between secondary school and higher education might decide to enroll directly after graduating to avoid the stricter admission criteria related to the introduction of a Numerus Fixus. However, this would require them to anticipate such policy approximately a year and a half before it is introduced when accounting for admission deadlines. As most study programs announce the introduction of admission criteria during the academic year before, such anticipation effects seem unlikely. Furthermore, the presence of such effects would likely show a peak in the relative number of students that directly enroll into university after graduating from secondary school. This does not seem to be the case as shown in figure 4.

Although the common trend assumption is not satisfied for all outcome variables – therefore affecting the plausibility the results – this paper conducts a difference-in-differences analysis. The results of this analysis are presented in Appendix B and provide a first estimation of the effects of the introduction of a Numerus Fixus. Section 5 of this paper elaborates on these findings and provides a comparison to the findings of the synthetic control method.

Figure 4: Number of direct enrollments



Notes: This figure uses data from the '1cyferho'-database from the Dutch executive education agency (DUO), which is also used for the analysis in this paper. The figure displays the evolution of the share of direct enrollments for both the treatment group and the control group over the period 2003-2015, as to visualize the lack of anticipation effects in 2013 for the introduction of a Numerus Fixus in 2014. However, anticipation effects appear to be present in 2014, which is likely due to the introduction of new funding legislation in 2015 known as the 'sociaal leenstelsel'. The solid black line depicts the treatment group. The solid gray line depicts the control group. The rate of direct enrollments is depicted on the vertical axis. The year of cohort enrollment is depicted on the horizontal axis.

4.2. Synthetic control method

The violation of the common trend assumption prevents a reliable application of the difference-in-differences method. In an attempt to obtain an unbiased estimate of the average treatment effect, the synthetic control method for comparative case studies is applied. This method forms a synthetic control group out of a 'donor pool' of control units by assigning weights to those units in the donor pool that best resemble the dependent variable of the treatment unit during the pre-treatment period. This method dismisses the need for the common trend assumption as it constructs a synthetic control group including a pre-trend that finds an optimal fit with that of the treatment group. The method is developed by Abadie and Gardeazabal (2003) – and further improved by Abadie et al. (2010) – and builds on the standard difference-in-differences methodology by offering an alternative to the selection of control units. A pre-treatment trend of the outcome variable is provided by using a weighted combination of control units. Besides using the outcome variable, the method additionally uses pre-treatment characteristics of the HEIs and their enrolled students to find the optimal fit between treatment- and control pre-trends. This method forms a great improvement to the difference-in-differences method because a weighted average of control HEIs provides a more reliable counterfactual than a conventional group of control HEIs. In a recent paper, Athey and Imbens (2017) evaluate the development of econometric methods in the last decades. They state that the synthetic control method is *"arguably the most important innovation in the policy evaluation literature in the last 15 years". (p. 9)*

An exemplary application of the synthetic control method provides the best illustration of its functioning. Building on the synthetic control method, Abadie, Diamond and Hainmueller (2010) study the effects of Proposition 99, a large-scale tobacco control program which was implemented by the American state of California in 1988 to fight tobacco consumption. Rather than comparing tobacco consumption in California with the consumption in other American states, the authors use the synthetic control method to form a comparable synthetic control region based on a set of characteristics including GDP per capita, percentage of young inhabitants, retail prices, per capita beer consumption and cigarette sales in a set of years preceding the policy. In other words, the authors utilize similarities between California and all other states to create a 'synthetic' California by giving weights to states which resemble California the most. Hereby they deviate from comparing California with existing control states as would be the case in a difference-in-differences approach.

Formally, the method works as follows. Suppose J + 1 HEI-programs – indexed by j – are observed over time t = 1, …, T and only HEI-program $j = 1$ introduces a Numerus Fixus. HEI-programs $j = 2$ to $j = J + 1$ are study programs that together form the 'donor pool' of potential control units. Also, all studies J + 1 are present in both the pre-treatment period T_0 as well as the post-treatment period T_1, where $T = T_0 + T_1$. Thus, the sample forms a balanced panel. Also, $T_1 \geq T_0$ and $1 \leq T_0 \leq T$. Only HEI-program $j = 1$ introduces a Numerus Fixus in T_1.

From the donor pool of potential control units, the synthetic control group can be established using a weighted average of HEI-programs. The synthetic control group consists of a Jx1 vector of weights $W = (w_2, \dots, w_{J+1})$ with each HEI-program in the donor pool being assigned a weight, where the sum of weights equals 1. Let X_1 define a k*1 vector of characteristics of the treatment HEI-program during the pre-treatment period, which this method attempts to match as closely as possible. Let X_0 define a k*J matrix of the same pre-treatment characteristics of the HEI-programs forming the donor pool. The weights are assigned in a way that the characteristics of the HEI-program introducing a Numerus Fixus X_1 are resembled by the synthetic control characteristics X_0. This includes pre-treatment values of the outcome variable. The synthetic control W^* is formed in such a way that the vector $X_1 - X_0W$ (i.e. the differences between the pre-treatment characteristics of the treatment- and the synthetic control HEI-program) is minimized. This method also accounts for differences in relative significance of characteristics as a predictor of the outcome variable.

Let m represent 1, … , k characteristics. Let X_{1m} reflect the value of the m-th variable for the treatment HEI-program and let X_{0m} reflect the value of the m-th variable for the HEI-program in the donor pool (where the latter is a 1xJ vector). If the relative significance of the m-th variable is represented by υ_m, then W^* is chosen in such a way that it minimizes

$$\sum_{m=1}^{k} \upsilon_m (X_{1m} - X_{0m}W)^2 ,$$

implying that variables with large predictive power on the dependent variable are assigned relatively large υ_m weights. This term is known as the 'mean squared prediction error (MSPE)' of the synthetic control estimator. (Abadie and Gardeazabal, 2003)

In the context of the introduction of a Numerus Fixus, the treatment unit is not a single HEI-program but all HEI-programs that introduce a Numerus Fixus in that given year. To adjust the method to this context, a treatment group is constructed as the weighted average of the HEI-programs that introduce a Fixus using the number of students enrolled in each program as a weight. The weights assigned to each student i in the treatment HEI-programs sum up to 1, similar to how the HEI-program weights in the synthetic control group sum up to 1.

As discussed before, this method assumes the absence of anticipation effects and any interference between units. These effects are identical to those in the difference-in-differences method and are controlled for in the same fashion as stated before. Furthermore, several potential threats might affect the validity of the results in this paper. First, Abadie et al. (2015) state that the donor pool should be restricted to units with characteristics that are somewhat similar to the treated unit. Therefore, study program-HEI combinations that differ significantly from the treatment unit with respect to the outcome variable or one of the covariates are labelled as outliers and removed from the donor pool. To illustrate, table 1 presents descriptive statistics of the treatment group and the donor pool of control studies. For example, the study programs in the treatment group have a relatively large share of female students. Accordingly, study programs with an unusual low share of female students are dropped from the donor pool.

Another threat as stated by Abadie et al. (2015) is due to overfitting, which arises when the characteristics of the treatment unit are artificially matched by combining highly peculiar variations in a sample of control units. Therefore, study program-HEI combinations that display significant turbulence of either outcome variables or covariates are removed from the donor pool. Both of these procedures follow the same line as stated by Abadie et al. (2015).

Ideally, this study would compare the introduction of admission criteria by study program-HEI combinations in different years to enlarge the external validity of the results. Unfortunately, the limited number of programs that introduce a Numerus Fixus each year prevents such an analysis. Examining the number of study programs that introduce a Numerus Fixus each year during the period 2003-2015, it shows that most study programs introduce a Fixus in the academic year 2014/2015. This entails 24 study program-HEI combinations that introduce a Numerus Fixus in – cohort enrollment year – 2014.

As all 24 programs turn out to be study programs at universities of applied science (HBO), this study examines the effects of the introduction of a Numerus Fixus in the academic year 2014/2015 on outcome variables related to HBO programs. To maintain a representative donor pool, all study programs affiliated with universities (WO) are omitted from the data. Furthermore, this study only examines study program-HEI combinations that are included during the entire period 2003-2015. Therefore, all study programs that are not observed during all years in this period are omitted from the data. Similarly, study programs that introduce a Fixus in a year different from 2014 and studies that introduce a Fixus in 2014 but directly drop it in 2015 are omitted from the data.

This enables the synthetic control method to utilize an extensive pre-treatment period of eleven years to find the best fit between the evolution of outcome variables of the treatment group and the synthetic control group. Utilizing the entire pre-treatment period allows for the synthetic control method to form the most credible counterfactual possible. In addition, this allows for effect evaluations in two subsequent years, 2014 and 2015. Retention, switch- and dropout rates are measured at the end of the academic year. Therefore, the introduction of a Fixus in 2014 directly has an effect for the cohort of 2014. Similarly, the treatment has a direct effect on (log) enrollment and on the composition of enrollments of cohort 2014 as an introduction of a Fixus in 2014 influences the selection for that academic year.

This method – including the restriction of the donor pool to prevent biases – leads to a sample size including 1,547 study program-HEI combinations in the control group and 312 in the treatment group. This entails 24 treatment units and 119 control units in the donor pool that are tracked from 2003-2015. Appendix D presents the 24 treatment study program-HEI combinations including their student- and study characteristics.

The synthetic control procedure creates a distinct synthetic control group for each outcome variable. Therefore, the number of observed students and the number of study program-HEI combinations at which they are enrolled differ between the outcome variables. This depends on the weight that is given to programs in the donor pool, as discussed before. The same empirical analysis is repeated for the unrestricted sample as presented in the robustness analysis. This illustrates the importance of reducing the donor pool to relevant control units.

Table 1 shows the descriptive statistics for both the treatment group as presented in columns (1-3) and the donor pool as presented in columns (4-6). The table presents the means and standard errors (in parentheses) of the individual- and study characteristics of both groups during pre-treatment and post-treatment periods as presented in columns (1) and (4), and (2) and (5) respectively. Columns (3) and (6) present the differences between the pre-mean and post-mean for the treatment group and the donor pool respectively. The treatment group differs remarkably in a number of areas from the study programs in the donor pool, both during the pre-treatment period as shown in column (7) and the post-treatment period as shown in column (8). The pre-treatment period consists of the eleven years before the introduction of the treatment. The post-treatment period consists of the two years after the introduction of the treatment. This table compares the treatment group to the donor pool of control studies rather than the synthetic control group, because the synthetic control group differs for each separate analysis as discussed before.

The most striking differences between the treatment group and the donor pool of control study programs are the share of female students and the share of HAVO students relative to MBO and VWO students. These differences can be attributed to the relatively large number of female students in nursing studies, which comprise a significant part of the study programs in the treatment group as shown by the table in Appendix D. Notable differences in student- and study characteristics between the treatment group and the donor pool underlines the necessity of using the synthetic control procedure rather than a standard difference-in-differences approach. As discussed before, the synthetic control procedure allows a relevant comparison with a synthetically composed control program by minimizing the pre-mean differences in characteristics of the treatment group and synthetic control group.

Table 1: Descriptive statistics

	Treatment Group			Donor Pool			Difference	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pre-mean	Post-mean	Estimated difference	Pre-mean	Post-mean	Estimated difference	Pre-mean difference	Post-mean difference
Age	20.00	20.41	0.410	20.84	19.85	-0.988***	-0.840	0.558
	(0.161)	(0.517)	(0.426)	(0.05934)	(0.07856)	(0.143)	(0.648)	(0.859)
Female students	0.744	0.759	0.0159	0.580	0.591	0.0106	0.163*	0.168
	(0.00322)	(0.0171)	(0.00963)	(0.00712)	(0.0156)	(0.0180)	(0.0777)	(0.171)
VWO students	0.0888	0.0585	-0.0303***	0.0667	0.0592	-0.00747*	0.0221	-0.000755
	(0.00258)	(0.00255)	(0.00632)	(0.00128)	(0.00383)	(0.00341)	(0.0139)	(0.0419)
HAVO students	0.605	0.614	0.00864	0.476	0.501	0.0251**	0.129***	0.113
	(0.00802)	(0.0228)	(0.0209)	(0.00342)	(0.00818)	(0.00876)	(0.0374)	(0.0895)
MBO students	0.249	0.299	0.0503**	0.356	0.376	0.0195*	-0.107**	-0.0761
	(0.00392)	(0.0266)	(0.0129)	(0.00322)	(0.00835)	(0.00835)	(0.0351)	(0.0913)
Native students	0.797	0.812	0.0151	0.812	0.810	-0.00141	-0.0152	0.00123
	(0.00568)	(0.0143)	(0.0146)	(0.00351)	(0.00848)	(0.00898)	(0.0383)	(0.0927)
Non-native Western students	0.0678	0.0593	-0.00849	0.0892	0.0807	-0.00857	-0.0214	-0.0213
	(0.00159)	(0.00006)	(0.00387)	(0.0024)	(0.00547)	(0.00610)	(0.0262)	(0.0598)
Nonwestern students	0.129	0.129	0.000268	0.0973	0.109	0.0117	0.0316	0.0201
	(0.00253)	(0.0143)	(0.00774)	(0.00255)	(0.00582)	(0.00647)	(0.0278)	(0.0637)
Direct entrants	0.813	0.815	0.00103	0.786	0.840	0.0540***	0.0271	-0.0258
	(0.0104)	(0.0192)	(0.0261)	(0.00339)	(0.00657)	(0.00844)	(0.0371)	(0.0719)
Higher education graduates	0.00751	0.0125	0.00503*	0.0289	0.0224	-0.00649*	-0.0214	-0.00989
	(0.00053)	(0.0039)	(0.00182)	(0.00117)	(0.00249)	(0.00294)	(0.0128)	(0.0272)
Retention rate	0.681	0.723	0.0415	0.65	0.661	0.0119	0.0318	0.0614
	(0.0112)	(0.0279)	(0.0286)	(0.00303)	(0.00654)	(0.00764)	(0.0331)	(0.0715)
Switch rate	0.161	0.134	-0.0273	0.188	0.184	-0.00350	-0.0270	-0.0508
	(0.00875)	(0.0209)	(0.0223)	(0.00241)	(0.00549)	(0.00612)	(0.0263)	(0.0600)
Dropout rate	0.158	0.144	-0.0142	0.163	0.154	-0.00836*	-0.00486	-0.0107
	(0.00363)	(0.00701)	(0.00912)	(0.00165)	(.000335)	(0.00412)	(0.0180)	(0.0366)
Number of students enrolling	154.43	209.31	54.88	133.03	124.13	-8.905	21.40	85.18
	(11.72)	(11.91)	(28.76)	(2.511)	(5.243)	(6.298)	(27.42)	(57.32)
Observations								
Students	30942	7704	38646	174137	29542	203679	205079	37246
Study program-HEI combinations	264	48	312	1309	238	1547	1573	286

Notes: This table presents means and standard errors (in parentheses) of individual student- and study characteristics of the treatment group (columns 1, 2 and 3) and the 'donor pool' of control study programs (columns 4, 5 and 6) during pre-treatment (columns 1 and 4) and post-treatment (columns 2 and 5) periods. Columns (3) and (6) present the differences between the pre-mean and post-mean for the treatment group and donor pool respectively. Columns (7) and (8) present the differences between the treatment group and the donor pool for the pre-mean and post-mean respectively. The pre-treatment period consists of the eleven years before the introduction of the treatment. The post-treatment period consists of the two years after the introduction of the treatment. Standard errors are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

5. Main results

This section discusses the main results of this study. Figures are presented to provide an overview of the development of all relevant outcome variables and the effects related to the introduction of a Numerus Fixus by treatment studies. First, the effects on (log) enrollment and first-year success are presented. Second, the effects on the compositional outcome variables are presented. Finally, the main findings of the regression analysis are presented. This entails the estimation of average treatment effects through statistical inference. Moreover, this section includes an interpretation of the findings and discusses the underlying mechanisms. This section also briefly discusses the findings of the difference-in-differences analysis and how these relate to the findings of the synthetic control method.

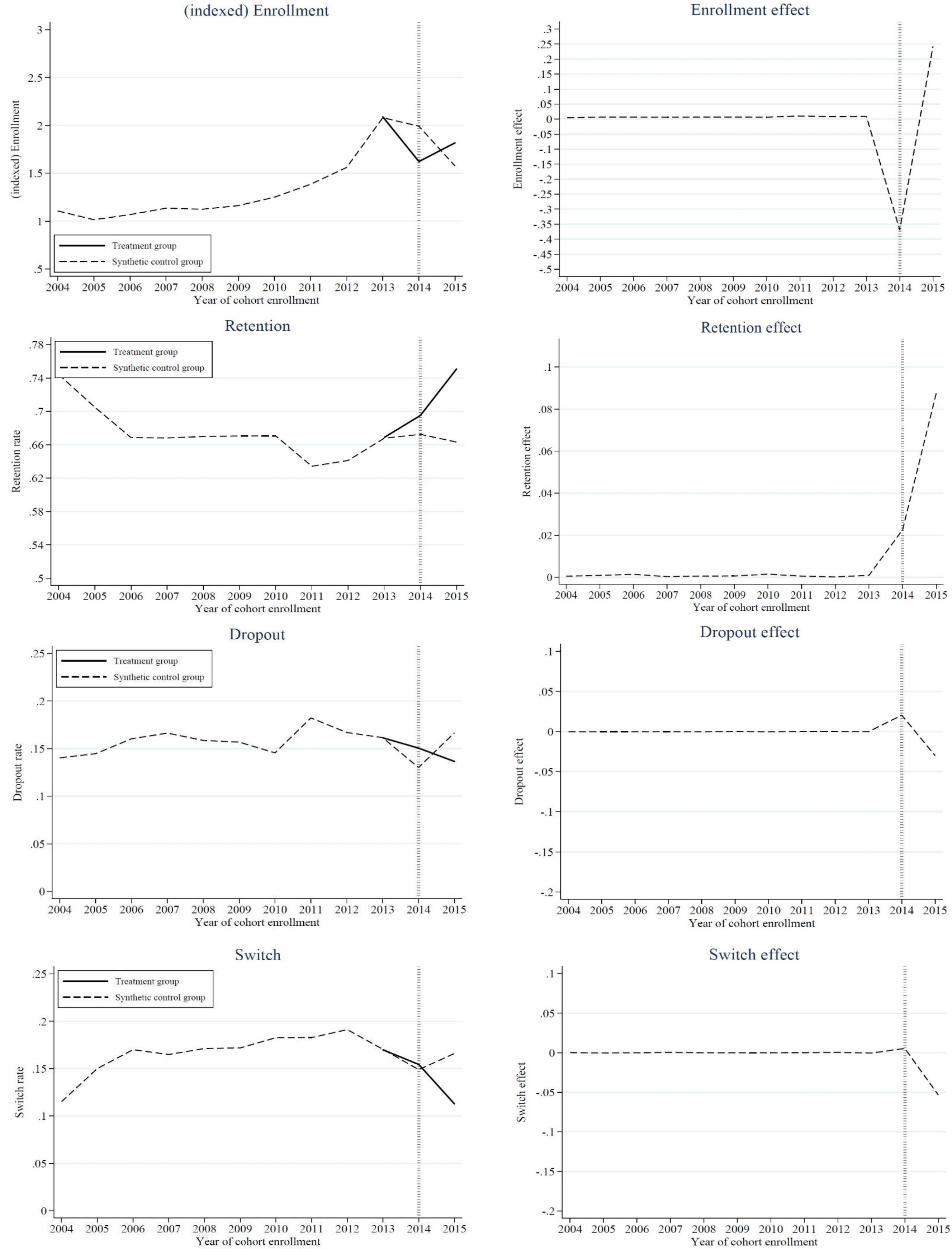
<u>5.1. Effects on enrollment and retention</u>

Figure 5 shows the development of the main outcome variables. The left-hand side embodies plots of the synthetic control procedure on the evolution of (log) enrollment, retention-, dropout- and switch rates respectively. The dashed line from 2003-2013 represents the development of both treatment- and synthetic control studies during pre-treatment years. This is based on a near-perfect replication of the evolution of the treatment group by that of the synthetic control group. The treatment group is constructed through a weighted average of all 24 study program-HEI combinations that introduce a selection procedure in 2014, where the weight is based on the annual number of students that commence with a program.

The solid black line represents the actual evolution of the outcome variable of the treatment group during the (post-)treatment period. The solid black line is only plotted in the years 2013-2015 as the synthetic control method ensures a near-perfect replication of the treatment group, implying that the treatment group and the synthetic control group follow the exact same trend from 2003-2013. Ergo due to visual considerations, the dashed line represents both the treatment group and the synthetic control group from 2003-2013. The extended dashed line during the post-treatment period represents the counterfactual with respect to the evolution of the outcome variable. This illustrates the fundamental premise of what would have occurred with the outcome variable if the study programs in the treatment group would not have introduced a selection procedure in 2014.

Subsequently, the difference between the solid- and the dashed line indicates the effect of the treatment without controlling for confounding variables. The size of these effects is presented in the gap plots on the right-hand side of figure 5. This effect is equal to the vertical 'gap' between the treatment group and the synthetic control group as shown in the graphs on

Figure 5: The effects of the introduction of a Numerus Fixus – main outcome variables



Notes: The left-hand side of this figure displays graphs of the evolution of the main outcome variables for both the treatment group and synthetic control group over the period 2003-2015. The treatment group is depicted by the solid black line. The synthetic control group is depicted by the dashed black line. The mean squared prediction error (MSPE) as formulated by Abadie and Gardeazabal (2003) is minimized over the period 2003-2013. Therefore, both groups follow the same development over this period as shown by the dashed black line. The right-hand side of this figure displays graphs of the accompanying effect sizes. This effect is equal to the vertical 'gap' between the treatment group and synthetic control group as shown in the graphs on the left-hand side of the figure. The size of the outcome variables and effects are depicted on the vertical axis, which represents percentage points within a 0 – 1 range. The year of cohort enrollment is depicted on the horizontal axis.

the left-hand side of the figure. The year of introduction is represented by the vertical dashed line in both sides of the figures.

The left-hand side of figure 5 presents indexed enrollment levels using the number of enrollments in 2003 as an index. The graph shows a gradual increase of enrollment levels in study programs of both the treatment group and the synthetic control group during the pre-treatment period 2003-2013. This period appears to be characterized by a compelling growth, featuring a doubling of enrollment levels in 2013 compared to 2003. The introduction of admission criteria by study programs in the treatment group leads to a clear drop of enrollments directly after the introduction in 2014 back to 2012 enrollment levels. Enrollments do not change much for the synthetic control group, implying an effect of the treatment on the enrollment of study programs in the treatment group entailing more than 35 percentage points in 2014. Enrollments in treatment group study programs bounce back somewhat in 2015, whilst enrollments experience a clear drop for the synthetic control group. This results in a 20 percentage point increase of enrollments for the study programs in the treatment group compared to those in the synthetic control group. The size of the effects in both years is represented in the gap plot on the right-hand side of figure 5.

The left-hand side of figure 5 shows a decrease in retention rates from 2003-2006 for both the treatment group and the synthetic control group. This is followed-up by relatively constant retention rates around 67 percent, interrupted by a slight decrease in 2011 and 2012. Notably, the introduction of admission criteria leads to a continuation of the increase in retention rates for treatment studies in 2014 and 2015, up until a retention rate around 75 percent in 2015. Retention rates for the synthetic control group stay constant around 66-67 percent. This implies an increase in retention of approximately 2 percentage points in 2014 and 8-9 percentage points in 2015 for treatment studies compared to their synthetically generated counterfactual. The size of these effects is once more represented by the gap plot on the right-hand side of figure 5.

An increase in retention rates is *de facto* accompanied by a decrease in either dropout rates, switch rates or both. First, the left-hand side of figure 5 shows the development of dropout rates over the relevant time-period. Dropout rates are relatively constant around 16 percent during the entire time-period, besides a small peak in 2011. The introduction of admission criteria by treatment studies leads to slightly lower dropout rates both in 2014 and 2015. However, the larger decrease in dropout for synthetic control studies in 2014 implies a relative increase in dropout for the treatment group compared to its counterfactual by approximately 2 percentage points in 2014. This is then followed by an approximate decrease of 3 percentage points in 2015, which is again depicted in the gap plot on the right-hand side of figure 5.

Similarly, the left-hand side of figure 5 shows the development of switch rates of both treatment- and synthetic control studies over the period 2003-2015. Switch rates show a sharp increase in the first few years and then remain relatively constant around 17-18 percent with a peak in 2012 around 19 percent. Switch rates then show a similar pattern as dropout rates, declining from 2012 onwards leading to lower switch rates for treatment studies in 2014 and 2015. Similarly, switch rates decline slightly more rapidly for synthetic control studies in 2014 and bounce back in 2015.

5.2. Composition effects

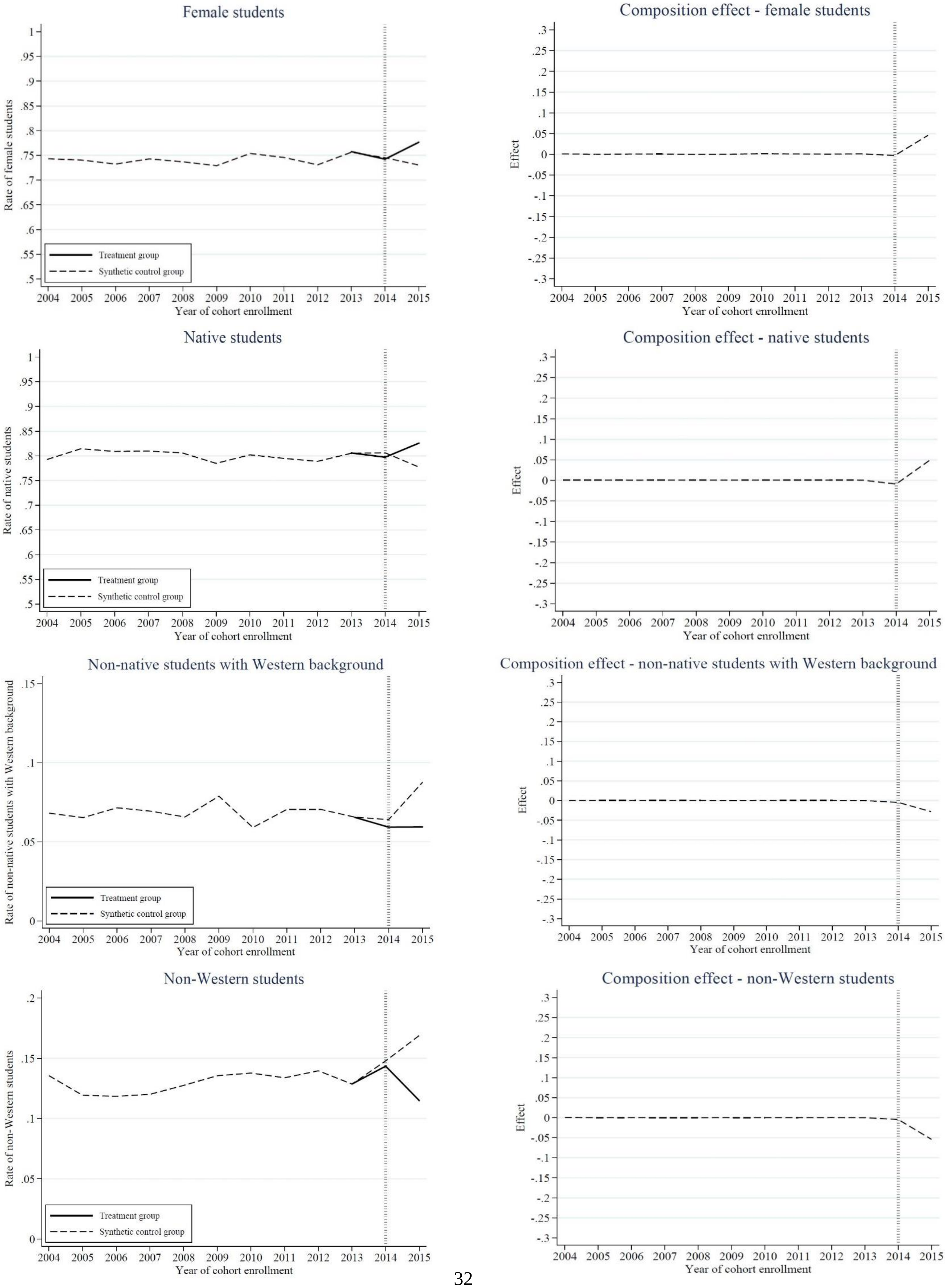
In a similar fashion, figure 6 shows the development of the outcome variables related to the composition of enrollment. The left-hand side comprises plots of the synthetic control procedure on the evolution of the composition of female-, native-, non-native Western-, non-Western-, VWO-, HAVO- and MBO-students respectively. The right-hand side consists of gap plots to visualize the size of the effects due to the introduction of admission criteria in 2014.

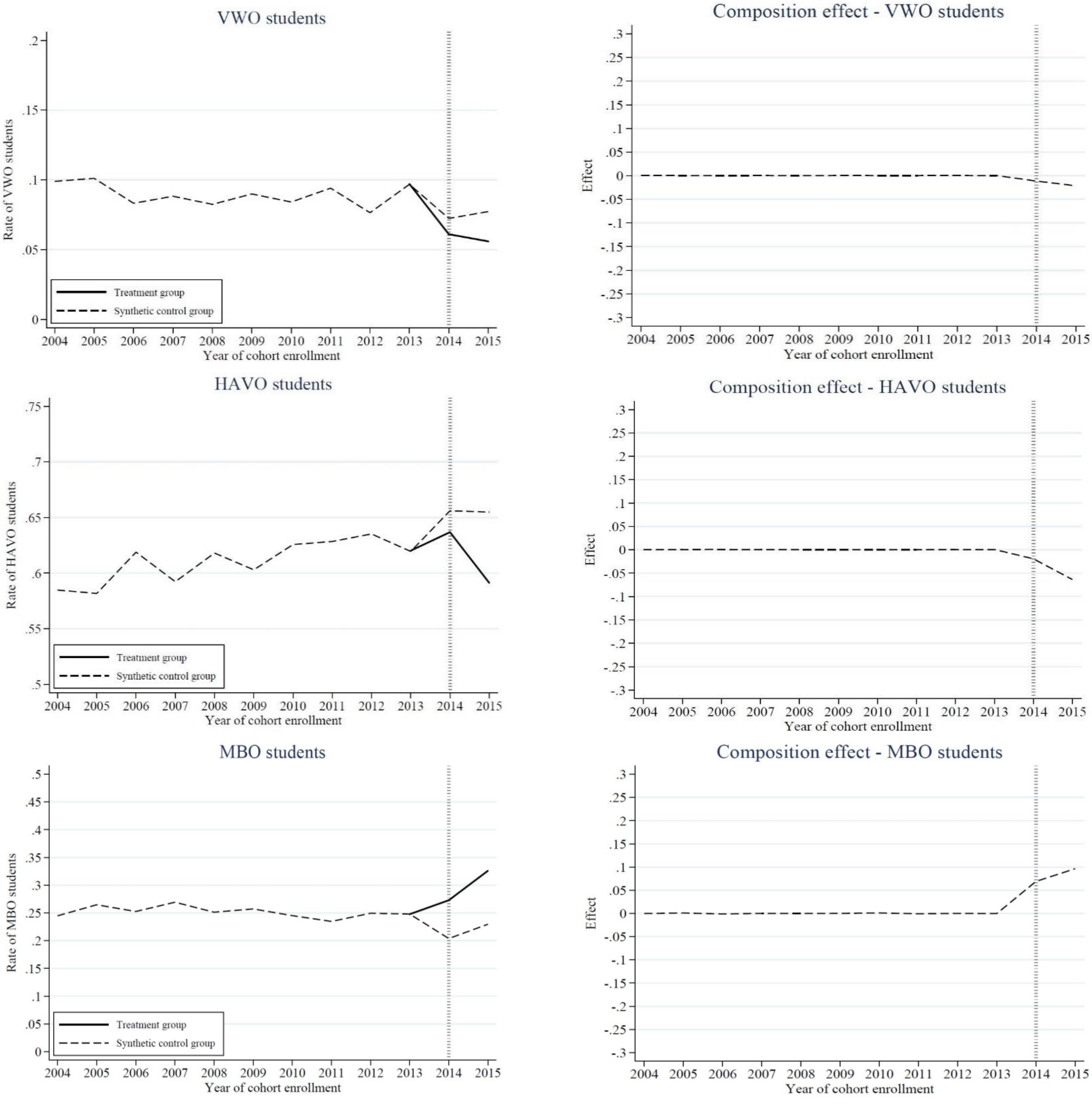
First, the left-hand side of figure 6 shows the evolution of the relative number of female students in both treatment and synthetic control studies during the period 2003-2015. The share of female students remains constant around 74 percent during the pre-treatment period. The introduction of admission criteria appears to have no distinct effect on the relative number of female students in 2014 and remains relatively constant for both treatment and control studies. However, this rate increases by almost 5 percentage points for treatment studies compared to their counterfactual in 2015 as depicted in the gap plot on the right-hand side of figure 6.

The left-hand side of figure 6 shows the evolution of the share of native students for both treatment and synthetic control studies during the period 2003-2015. The share of native students remains relatively constant around 80 percent. The introduction of admission criteria in 2014 has a negative but modest effect on the share of native students in treatment studies compared to their counterfactual. Subsequently, the relative number of native students increases slightly for treatment programs to around 83 percent in 2015 whilst it decreases to approximately 78 percent for the synthetic control program. The size of these effects is represented in the gap plot on the right-hand side of figure 6 by a slight but imperceptible decrease in 2014 and a 5 percentage point increase in 2015.

The left-hand side of figure 6 shows the evolution of the share of non-native Western students for both treatment and synthetic control studies during the period 2003-2015. This rate

Figure 6: The effects of the introduction of a Numerus Fixus – compositional outcome variable





Notes: The left-hand side of this figure displays graphs of the evolution of the compositional outcome variables for both the treatment group and synthetic control group over the period 2003-2015. The treatment group is depicted by the solid black line. The synthetic control group is depicted by the dashed black line. The mean squared prediction error (MSPE) as formulated by Abadie and Gardeazabal (2003) is minimized over the period 2003-2013. Therefore, both groups follow the same development over this period as shown by the dashed black line. The right-hand side of this figure displays graphs of the accompanying effect sizes. This effect is equal to the vertical 'gap' between the treatment group and synthetic control group as shown in the graphs on the left-hand side of the figure. The size of the outcome variables and effects are depicted on the vertical axis, which represents percentage points within a 0 – 1 range. The year of cohort enrollment is depicted on the horizontal axis.

remains relatively constant around 6-7 percent during the pre-treatment period. The relative number of non-native Western students decreases slightly more for the treatment studies compared to the synthetic control studies from 2013 to 2014. The synthetic control group then shows a clear increase from 2014 to 2015 to approximately 9 percent, whilst the treatment group remains constant. As shown in the gap plot on the right-hand side of figure 6, the effect of the introduction of admission criteria is negligible in 2014 and slightly negative around 3-4 percentage points in 2015.

The left-hand side of figure 6 shows the evolution of the share of non-Western students for both treatment and synthetic control studies during the relevant period. This rate remains relatively constant around 13-14 percent during the pre-treatment period. The relative number of non-Western students is depicted by a slightly larger increase from 2013 to 2014 for the synthetic control studies compared to the treatment studies. The treatment group then shows a clear decrease in the relative number of non-Western students compared to a further increasing rate for the synthetic control group. As shown in the gap plot on the right-hand side of figure 6, the effect of the introduction of admission criteria is modest in 2014 but negative around 5 percentage points for 2015.

The left-hand side of figure 6 shows the evolution of the relative number of VWO-students for both treatment and synthetic control studies during the period 2003-2015. This rate remains relatively constant around 8-9 percent during the pre-treatment period. The introduction of admission criteria in 2014 leads to a slightly larger decrease of the relative number of VWO-students for treatment studies in both 2014 and 2015. The relative size of the effect is around 1 percentage point in 2014 and 2 percentage points in 2015, as can be seen in the gap plot on the right-hand side of figure 6.

The left-hand side of figure 6 shows the evolution of the share of HAVO-students for both treatment and synthetic control studies during the period 2003-2015. This share increases slightly over the years from 58-59 percent to 63-64 percent during the pre-treatment period. The introduction of admission criteria in 2014 leads to a larger increase of the relative number of HAVO-students for synthetic control studies in 2014, which remains constant in 2015. The share of HAVO-students increases slightly for treatment studies in 2014 and then decreases suddenly to approximately 59 percent in 2015. The relative size of the effect is around 2 percentage points in 2014 and 6 percentage points in 2015, as can be seen in the gap plot on the right-hand side of figure 6.

Finally, the left-hand side of figure 6 shows the evolution of the relative number of MBO-students for both treatment and synthetic control studies during the period 2003-2015. This rate remains relatively constant around 25 percent during the pre-treatment period. The introduction of admission criteria in 2014 leads to an increase of the relative number of MBO-students to approximately 27-28 percent in 2014 and 33 percent in 2015 for treatment studies. In contrast, this rate decreases to approximately 20-21 percent in 2014 and 23-24 percent in 2015 for the synthetic control study. This implies a positive effect around 7 percentage points in 2014 and around 10 percentage points in 2015 as can be seen in the gap plot on the right-hand side of figure 6.

5.3. Statistical inference

This section extends the use of the synthetic control method to estimate average treatment effects through statistical inference. In contrast to the previous section, the estimations presented here control for relevant covariates. In order to estimate average treatment effects, the weights which are designated to study programs in the 'donor pool' – therefore creating the synthetic control group – are now reassigned to the individual level to enable regression analyses. The weights are reassigned based on the number of enrollments in each study program. For example, if the synthetic control method assigns weight .002 to study *x* which has 100 enrollments in year *t*, then each of these students will be assigned weight 0.002/100 = 0.00002 in year *t*. The weights assigned to all students in the synthetic control studies again sum up to 1. Appendix C provides additional estimation results for each outcome variable, including the coefficients of relevant covariates.

5.3.1. Effects on (log) enrollment and retention rates

Table 2 presents the average treatment effects for the main outcome variables, related to the introduction of admission criteria in 2014. These outcome variables are (log) enrollment, retention-, stay- and switch rates respectively. Four different specifications are estimated for the outcome variables. Column (1) presents the average treatment effect of the introduction of admission criteria including year- and study fixed effects but excluding controls. Column (2) presents the average treatment effect including similar conditions as column (1), but additionally stating the average treatment effect separately for the years 2014 and 2015 following the introduction of admission criteria. Column (3) builds on column (1) by adding individual- and study controls as stated before. Similarly, column (4) builds on column (2) by adding individual- and study controls. The table also indicates the number of observed students and the number of study program / HEI-combinations in which they are enrolled. Standard errors are clustered at the study program-HEI-year level.

First, the average treatment effect in 2014 due to the introduction of admission criteria on (log) enrollment as presented in column (2) on the upper left-hand side of the table shows a decrease in (log) enrollment by 14 percentage points, which is statistically significant at the 10 percent level. The initial drop of enrollments directly after the introduction of admission criteria

Table 2: The average treatment effects – main outcome variables

	(log) Enrollment				*Retention*			
	(1)	(2)			(1)	(2)	(3)	(4)
Average Treatment Effect	-0.044				0.042		0.026	
	(0.071)				(0.026)		(0.027)	
ATE 2014		-0.14*				0.016		0.0047
		(0.076)				(0.045)		(0.043)
ATE 2015		0.047				0.068***		0.049*
		(0.11)				(0.024)		(0.028)
Observations								
Students	N/A	N/A			50481	50481	50481	50481
Study program-HEI combinations	40	40			38	38	38	38
Year- and Study Fixed Effects	Y	Y			Y	Y	Y	Y
Individual- and Study Controls	N	N			N	N	Y	Y
	Switch				*Dropout*			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Average Treatment Effect	-0.016		-0.016		-0.0035		-0.0025	
	(0.015)		(0.015)		(0.015)		(0.016)	
ATE 2014		0.0069		0.0042		0.022		0.024
		(0.017)		(0.016)		(0.022)		(0.024)
ATE 2015		-0.039*		-0.036		-0.029**		-0.030**
		(0.021)		(0.023)		(0.014)		(0.015)
Observations								
Students	54810	54810	54810	54810	56169	56169	56169	56169
Study program-HEI combinations	38	38	38	38	38	38	38	38
Year- and Study Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Individual- and Study Controls	N	N	Y	Y	N	N	Y	Y

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for the main outcome variables. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years seperately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

seems as expected due to the *mechanistic* negative effect of a Numerus Fixus on enrollments. It must be noted however, that this mechanism relies on the Fixus being ‘binding’. In other words, the number of slots must be lower than the number of applicants. Other mechanisms that reinforce this effect include a relative increase of the required risk and effort in students’ optimal decision regarding their investment in education, as discussed in sections 2.1.4. and 2.1.5. of this paper.

More surprising is the backlash of enrollments in 2015 – though not statistically significant – compared to the drop in the synthetic control group. Several mechanisms are at play here. If study programs decide to raise the maximum number of enrollments a year after introduction – assuming there are still more applicants than slots available – this *mechanically* leads to a higher level of enrollments. This would be the case when the initial limitation of enrollment led to overcapacity, implying that the increase in 2015 serves as a correction.

Other mechanisms that potentially explain the increase in enrollments in 2015 are an increase in returns due to profession-related 'rents' and an increase in relative income or 'status', as discussed in sections 2.1.1. and 2.1.2. of this paper. Similarly, feedback- and peer effects might increase the quality of the study programs and positively affect consumption motives of students to enroll, as discussed in section 2.1.2. of this paper.

The drop of enrollments in 2014 can only be attributed with certainty to the direct mechanism of the introduction of a Numerus Fixus, when it is known that the Fixus is 'binding'. Better insights in the number of enrollments compared to the capacity of the relevant study programs would provide information on whether a Numerus Fixus is binding and to what extent. Unfortunately, this study cannot account for this due to data limitations. However, a report by the Dutch Education Inspectorate (Inspectie van het Onderwijs, 2015) states that many HEIs justify their introduction of admission criteria not by a current abundance of registrations. These HEIs aim to prevent attracting the surplus of similar institutions and study programs in the future. Failing to introduce a Numerus Fixus in response to policy implementation by other institutions would lead to a sudden inflow of those students which did not make the selection elsewhere and thus presumably possess lower ability on average. Overall, this suggests that study programs might have introduced a non-binding Numerus Fixus which could explain the peculiarity of these results.

The average treatment effect due to the introduction of admission criteria on retention rates shows no effects in 2014 in either column (2) or column (4). However, column (2) indicates a 6.8 percentage point increase in retention in 2015 due to the treatment which is statistically significant at the 1 percent level. Column (4) shows that this effect is mitigated to a 4.9 percentage point increase which is significant at the 10 percent level when the model controls for individual- and study effects.

Several mechanisms are relevant in explaining the positive effect of the introduction of a Numerus Fixus on retention rates. Most importantly as discussed in section 2.2., HEIs introduce a Numerus Fixus in order to select the top layer of students for their study programs. The intention to increase academic success on average therefore appears to be validated by the increase in retention rates as a result of the introduction of a Numerus Fixus. This is reinforced by peer- and feedback effects, as discussed in section 2.1.4. of this paper. Furthermore, a study by the Dutch Education Inspectorate (Inspectie van het Onderwijs, 2017) suggests that an increase in the required effort due to stricter admission criteria leads to better informed students and therefore reduces the risk of students dropping out of the study program. This is accompanied by a stronger ‘bond’ of the students with their study program due to the introduction of a Numerus Fixus.

The average treatment effect on dropout rates in 2015 as presented in column (2) in the bottom-right corner of table 2 indicates a decrease in dropout rate by 2.9 percentage point which is statistically significant at the 5 percent level. The magnitude of this effect increases slightly to a decrease by 3 percentage point when the model controls for individual- and study controls and remains statistically significant at the 5 percent level. Similarly, the average treatment effect on switch rates in 2015 as presented in column (2) in the bottom-left corner of the table indicates a decrease in switch rate by 3.9 percentage point which is statistically significant at the 10 percent level. This effect loses its statistical significance when the model controls for individual- and study controls. Thus, the increase of retention rates in treatment programs due to the introduction of admission criteria appears to be driven by both a decrease in dropout rates and in switch rates.

Students can drop out or switch to another study program after their first year for a variety of reasons. Due to academic underperformance, a student might not satisfy relevant criteria related to potentially existing academic dismissal policy. A clear example of such policy in the Netherlands is the Binding Study Advice (BSA), implying that a student must acquire an *ex ante* stated amount of academic credits (ECTS) to continue the program. Unfortunately, this study cannot measure this effect as it does not possess reliable data on the implementation of AD policy (BSA) by study programs.

Appendix B of this paper presents the effects of the introduction of a Numerus Fixus on the main outcome variables, based on a standard difference-in-difference analysis. When comparing these findings with the results from the synthetic control method, the following results stand out. Interestingly, the difference-in-differences method shows similar findings compared to the synthetic control method. The size of the coefficients is slightly smaller, although the statistical significance appears to be somewhat larger. The apparent similarity in results provides additional confidence for the direction of the coefficients and therefore reinforces the validity of our findings. The figures in appendix A indicate that several of the outcome variables show a trend that is (almost) parallel. This would imply a valid application of the difference-in-differences method and might explain the similarity in results. Only the effect on enrollments appears to be relatively high, which could be caused by the clear lack of a common trend as shown in the figure.

5.3.2. Effects on compositional outcome variables

In a similar fashion, table 3 presents the average treatment effects for the compositional outcome variables, related to the introduction of admission criteria in 2014. These outcome variables are the relative number of female-, native-, non-native Western-, non-Western-, VWO-, HAVO- and MBO-students respectively. The layout of the table and the four specifications are similar to those of table 2.

First, the average treatment effect due to the introduction of admission criteria on the share of female students as presented in column (1) shows an increase by 3 percentage points which is statistically significant at the 5 percent level. This effect diminishes towards an increase of 2.9 percentage points when the model controls for individual- and study controls, but remains statistically significant at the 5 percent level as shown in column (3). This effect is driven by the increase in 2015 as can be seen in columns (2) and (4). These show positive effects of 3.9 and 4.2 percentage points respectively, both statistically significant at the 5 percent level.

Second, the average treatment effect on the share of native students shows an increase in the share of native students by 4.7 percentage points in 2015 – after controlling for individual- and study controls – which is significant at the 10 percent level.

Third, the average treatment effect on the share of non-native Western students shows no significant effects due to the introduction of admission criteria.

Fourth, the average treatment effect on the share of non-Western students as presented in column (1) shows a decrease of 3 percentage points due to the introduction of admission criteria. The size of this effect diminishes to a decrease of 2.4 percentage points when controlling for individual- and study controls but remains statistically significant at the 10 percent level. This effect appears to be driven by the decrease in 2015 as shown in columns (2) and (4). A decrease in the relative number of non-Western students by 4.1 percentage points in 2015 as presented in column (4) remains statistically significant at the 10 percent level.

Fifth, the average treatment effect on the share of VWO-students shows no significant effects due to the introduction of admission criteria. As can be seen in table 1, the share of VWO students in the study programs that constitute the sample is relatively small. This can be explained by the fact that all studies in both treatment and control groups are higher vocational education (HBO), whilst VWO-students are eligible to enroll into university programs.

Sixth, the average treatment effect on the share of HAVO-students shows no significant effects due to the introduction of admission criteria.

Table 3: The average treatment effects – compositional outcome variables

	Female Students				*Native Students*			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Average Treatment Effect	0.030**		0.029**		0.023		0.027	
	(0.012)		(0.012)		(0.020)		(0.017)	
ATE 2014		0.020		0.017		0.0049		0.0088
		(0.015)		(0.015)		(0.021)		(0.018)
ATE 2015		0.039**		0.042**		0.042		0.047*
		(0.018)		(0.018)		(0.032)		(0.027)
Observations								
Students	66485	66485	66485	66485	51147	51147	51147	51147
Study program-HEI combinations	39	39	39	39	39	39	39	39
Year- and Study Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Individual- and Study Controls	N	N	Y	Y	N	N	Y	Y

	Non-native Western Students				*Non-Western Students*			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Average Treatment Effect	-0.0097		-0.0044		-0.030**		-0.024*	
	(0.0088)		(0.0097)		(0.014)		(0.013)	
ATE 2014		-0.0020		0.0019		-0.014		-0.0071
		(0.0090)		(0.010)		(0.014)		(0.013)
ATE 2015		-0.017		-0.011		-0.046**		-0.041*
		(0.014)		(0.014)		(0.021)		(0.021)
Observations								
Students	54905	54905	54905	54905	50203	50203	50203	50203
Study program-HEI combinations	40	40	40	40	38	38	38	38
Year- and Study Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Individual- and Study Controls	N	N	Y	Y	N	N	Y	Y

	VWO Students				*HAVO Students*			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Average Treatment Effect	-0.010		-0.0010		-0.015		0.026	
	(0.011)		(0.0096)		(0.026)		(0.020)	
ATE 2014		-0.0032		0.0014		0.0068		0.026
		(0.011)		(0.011)		(0.030)		(0.023)
ATE 2015		-0.017		-0.0036		-0.036		0.025
		(0.017)		(0.015)		(0.039)		(0.030)
Observations								
Students	79454	79454	79454	79454	50377	50377	50377	50377
Study program-HEI combinations	46	46	46	46	37	37	37	37
Year- and Study Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Individual- and Study Controls	N	N	Y	Y	N	N	Y	Y

	MBO Students			
	(1)	(2)	(3)	(4)
Average Treatment Effect	0.079***		0.042**	
	(0.023)		(0.020)	
ATE 2014		0.066**		0.042*
		(0.028)		(0.025)
ATE 2015		0.091**		0.042
		(0.036)		(0.029)
Observations				
Students	55928	55928	55928	55928
Study program-HEI combinations	36	36	36	36
Year- and Study Fixed Effects	Y	Y	Y	Y
Individual- and Study Controls	N	N	Y	Y

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for the compositional outcome variables. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years seperately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

Finally, the average treatment effect on the relative number of MBO-students as presented in column (1) shows an increase of 7.9 percentage points due to the introduction of admission criteria. When controlling for individual- and study controls, this effect diminishes to an increase of 4.2 percentage points which is significant at the 5 percent level. When controlling for these covariates, this effect appears to be driven by the increase by 4.2 percentage points in 2014 which is significant at the 10 percent level.

An increase of the relative number of MBO-students due to the introduction of admission criteria can be caused by a variety of reasons. As discussed in sections 2.1.4. and 2.1.5. respectively, the introduction of a Numerus Fixus leads to an increase in the required effort and risk when applying for a study program. The required preparation for students to obtain a high grade-point average in their last year of secondary school involves additional effort and risk, as it is not certain that the invested effort – and possibly financial means – will pay off. This mechanism works differently for students who have recently finished an MBO study. These study programs generally do not have the same grading procedures as those of secondary schools. Therefore, admission criteria also differ for MBO-students.

The Dutch MBO Council – 'MBO Raad (2010)' – states that there are four options for HEIs to assess the aptitude of MBO-students applying for a HBO Fixus study program: 1) The student obtained grades during his MBO study, of which the five best grades form the grade-point average. The admission criteria are equivalent to those of HAVO/VWO-students. 2) The student obtained a rating using terms such as 'good', 'satisfactory' and 'unsatisfactory'. These ratings are translated into grades. Again, the five best grades form the grade-point average and the admission criteria are equivalent to those of HAVO/VWO-students. 3) The student obtained a rating using 'pass' versus 'fail' and 4) The student obtained no ratings to be included with the MBO diploma. Options (3) and (4) have no legal arrangement to translate results into a grade-point average and these students were placed in the third bracket in the central lottery procedure, which is equivalent to a 7–7.5 GPA for HAVO/VWO-students (MBO Raad, 2010).

As admission criteria impose different requirements for MBO-students compared to HAVO/VWO-students, this might explain why the introduction of a Numerus Fixus leads to an increase in the share of MBO-students. It must be noted however, that many study programs in the treatment group use a decentral selection procedure that does not (only) give weight to students' grade-point average. This is shown in Appendix E. Therefore, the increase in the share of MBO-students might also be due to their performance in the non-GPA related admission criteria.

Overall, the introduction of a Numerus Fixus by treatment studies results in the following compositional changes regarding enrollment: 1) an increase in the share of female students, 2) an increase in the share of native students, which mainly goes at the expense of the share of non-Western students, and 3) an increase in the share of MBO students, which mainly goes at the expense of the share of HAVO students. Several mechanisms could explain why the introduction of a Numerus Fixus leads to these compositional effects. Female-, native- and MBO- students appear less susceptible to the adverse effects of the introduction of a Numerus Fixus on levels of risk and perceived effort compared to male-, non-Western- and HAVO students as discussed in sections 2.1.4. and 2.1.5. In addition, MBO-students are affected differently by the introduction of admission criteria as discussed before. In order to verify which mechanisms lead to these compositional effects, it is necessary to examine how each compositional subgroup scores on the relevant admission criteria. Unfortunately, this paper is unable to examine the exact outcomes of students with respect to admission criteria by Fixus studies, due to data limitations.

An important criterium that is often used in the selection procedure of HEIs – and is known as a reliable indicator of cognitive ability – is the grade-point average (GPA) of students. It would be interesting to examine the average GPA of each compositional subgroup separately in order to assess whether compositional effects are driven by levels of GPA rather than the mechanisms discussed before. Unfortunately, data limitations prevent this paper from utilizing such information. It must be noted however, that many of the treatment studies do not seem to (only) use GPA as an admission criterium in their decentral selection procedures. Therefore, changes in the composition of enrollment due to the introduction of a Numerus Fixus are likely driven – at least partly – by non-GPA related criteria.

Appendix B of this paper shows the effects of the introduction of a Numerus Fixus on the compositional outcome variables, based on a standard difference-in-difference analysis. When comparing these findings with the results from the synthetic control method, the difference-in-differences method shows similar findings compared to the synthetic control method. The size of the coefficients is smaller, whereas the statistical significance appears to differ somewhat depending on the outcome variable. The apparent similarity in results provides additional confidence for the direction of the coefficients and therefore reinforces the validity of our findings. The figures in appendix A indicate that several of the outcome variables show a trend that is (almost) parallel. This would imply a valid application of the difference-in-differences method and might explain the similarity in results.

6. Robustness analysis

This section repeats the main analysis without dropping control units in the 'donor pool' of control studies. In the main analysis, control studies were dropped to avoid the occurrence of spillover effects and overfitting, which arises when the characteristics of the treatment unit are artificially matched by combining highly peculiar variations in a sample of control units (Abadie et al., 2015). However, all university study programs (WO) are again dropped from the donor pool as all of the treatment studies are university of applied science (HBO) programs. By repeating the main analysis without dropping control units, this section highlights how the occurrence of spillovers and overfitting can lead to an over- or underestimation of the results when using the synthetic control method.

By preserving more study program-HEI combinations in the data, the sample size now entails 2,873 study program-HEI combinations in the control group and 312 in the treatment group. This implies 24 treatment units and 221 control units in the donor pool that are tracked from 2003-2015. The synthetic control procedure creates a distinct synthetic control group for each outcome variable. Therefore, the number of observed students and the number of program-HEI combinations at which they are enrolled differ between the outcome variables. This depends on the weight that is given to programs in the donor pool, as discussed before.

6.1. Statistical inference

Similar to the main analysis, this section extends the use of the synthetic control method to estimate average treatment effects through statistical inference. In order to estimate average treatment effects, the weights which are designated to study programs in the 'donor pool' – therefore creating the synthetic control group – are now reassigned to the individual level to enable regression analyses. The weights are reassigned based on the number of enrollments in each study program.

6.1.1. Effects on (log) enrollment and retention

Table 4 presents the average treatment effects for the main outcome variables, related to the introduction of admission criteria in 2014. These outcome variables are (log) enrollment, retention-, stay- and switch rates respectively. Four different specifications are estimated for the outcome variables. Column (1) presents the average treatment effect of the introduction of admission criteria including year- and study fixed effects but excluding controls. Column (2) presents the average treatment effect including similar conditions as column (1),

Table 4: The average treatment effects – main outcome variables (Rob. analysis)

	(log) Enrollment				*Retention*			
	(1)	(2)			(1)	(2)	(3)	(4)
Average Treatment Effect	0.046				0.059**		0.047	
	(0.096)				(0.028)		(0.030)	
ATE 2014		0.025				0.032		0.023
		(0.13)				(0.045)		(0.044)
ATE 2015		0.067				0.085***		0.072**
		(0.14)				(0.029)		(0.033)
Observations								
Students	N/A	N/A			54128	54128	54128	54128
Study program-HEI combinations	243	243			41	41	41	41
Year- and Study Fixed Effects	Y	Y			Y	Y	Y	Y
Individual- and Study Controls	N	N			N	N	Y	Y
	Switch				*Dropout*			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Average Treatment Effect	-0.0021		-0.0059		0.0058		0.0067	
	(0.016)		(0.017)		(0.015)		(0.015)	
ATE 2014		0.013		0.0046		0.034		0.037*
		(0.018)		(0.018)		(0.022)		(0.022)
ATE 2015		-0.017		-0.017		-0.022		-0.025
		(0.024)		(0.026)		(0.016)		(0.015)
Observations								
Students	50352	50352	50352	50352	64159	64159	64159	64159
Study program-HEI combinations	39	39	39	39	42	42	42	42
Year- and Study Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Individual- and Study Controls	N	N	Y	Y	N	N	Y	Y

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for the main outcome variables. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years seperately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

but additionally stating the average treatment effect separately for the years 2014 and 2015 following the introduction of admission criteria. Column (3) builds on column (1) by adding individual- and study controls as stated before. Similarly, column (4) builds on column (2) by adding individual- and study controls. The table also indicates the number of observed students and the number of study program / HEI-combinations in which they are enrolled. Standard errors are clustered at the study program-HEI-year level.

Surprisingly, the table shows no significant effects on (log) enrollment and suggests positive effects rather than the decline as shown in the main analysis. The average treatment effect due to the introduction of admission criteria on retention rates shows no effects in 2014 in either column (2) or column (4). However, column (2) indicates an 8.5 percentage point increase in retention in 2015 due to the treatment which is statistically significant at the 1 percent level. Column (4) shows that this effect is mitigated to a 7.2 percentage point increase which is significant at the 5 percent level when the model controls for individual- and study effects. Column (4) in the bottom-right corner of the table indicates a decrease in dropout rate by 3.7 percentage points which is statistically significant at the 10 percent level after controlling for individual- and study controls. The results suggest no statistically significant effects on switch rates. The direction and magnitude of these effects are somewhat similar to those in the main analysis. However, these results suggest a larger increase in retention rates due to the introduction of a Numerus Fixus. Failing to remove those study programs from the donor pool – which potentially receive spillovers due to the treatment – would therefore lead to an overestimation of the effects.

6.1.2. Effects on compositional outcome variables

In a similar fashion, table 5 presents the average treatment effects for the compositional outcome variables, related to the introduction of admission criteria in 2014. These outcome variables are the relative number of female-, native-, non-native Western-, non-Western-, VWO-, HAVO- and MBO-students respectively. The layout of the table and the four specifications are similar to those of table 4.

First, the average treatment effect due to the introduction of admission criteria on the share of female students as presented in column (1) shows an increase by 2.9 percentage points which is statistically significant at the 10 percent level. This effect diminishes towards an increase of 3.3 percentage points when the model controls for individual- and study controls, but remains statistically significant at the 5 percent level as shown in column (3). This effect appears to be driven by an increase of 3.1 percentage points in 2015, which is statistically significant at the 10 percent level. These results are somewhat similar to those of the main analysis.

Second, the average treatment effect on the share of native students as presented in column (1) shows an increase by 3.7 percentage points which is mainly driven by an increase by 6.1 percentage points in 2015. These effects diminish to an increase by 3.1 and 5.3 percentage points for the average treatment effect and the effect in 2015 respectively, when controlling for individual- and study controls. These effects are statistically significant at the 10 percent level.

Third, the average treatment effect on the share of non-native Western students as presented in column (1) shows a decrease by 1.6 percentage points which is mainly driven by

Table 5: The average treatment effects – compositional variables (Rob. analysis)

	Female Students				*Native Students*			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Average Treatment Effect	0.029*		0.033**		0.037*		0.031*	
	(0.016)		(0.016)		(0.019)		(0.017)	
ATE 2014		0.035		0.035		0.013		0.0090
		(0.025)		(0.025)		(0.019)		(0.015)
ATE 2015		0.022		0.031*		0.061**		0.053*
		(0.017)		(0.016)		(0.030)		(0.028)
Observations								
Students	55418	55418	55418	55418	53004	53004	53004	53004
Study program-HEI combinations	39	39	39	39	44	44	44	44
Year- and Study Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Individual- and Study Controls	N	N	Y	Y	N	N	Y	Y
	Non-native Western Students				*Non-Western Students*			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Average Treatment Effect	-0.016*		-0.0073		-0.0088		0.0020	
	(0.0092)		(0.0096)		(0.016)		(0.016)	
ATE 2014		-0.0056		0.00042		-0.00014		0.0099
		(0.0091)		(0.010)		(0.015)		(0.013)
ATE 2015		-0.026*		-0.016		-0.017		-0.0062
		(0.015)		(0.014)		(0.027)		(0.027)
Observations								
Students	55756	55756	55756	55756	54106	54106	54106	54106
Study program-HEI combinations	41	41	41	41	41	41	41	41
Year- and Study Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Individual- and Study Controls	N	N	Y	Y	N	N	Y	Y
	VWO Students				*HAVO Students*			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Average Treatment Effect	-0.016		-0.0042		-0.039		0.0022	
	(0.012)		(0.010)		(0.029)		(0.023)	
ATE 2014		-0.0074		0.00047		-0.019		0.0026
		(0.011)		(0.011)		(0.035)		(0.029)
ATE 2015		-0.026		-0.0090		-0.060		0.0017
		(0.020)		(0.017)		(0.042)		(0.033)
Observations								
Students	262136	262136	262136	262136	52013	52013	52013	52013
Study program-HEI combinations	166	166	166	166	39	39	39	39
Year- and Study Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Individual- and Study Controls	N	N	Y	Y	N	N	Y	Y
	MBO Students							
	(1)	(2)	(3)	(4)				
Average Treatment Effect	0.057**		0.015					
	(0.028)		(0.024)					
ATE 2014		0.025		0.000067				
		(0.036)		(0.034)				
ATE 2015		0.090**		0.030				
		(0.038)		(0.031)				
Observations								
Students	56524	56524	56524	56524				
Study program-HEI combinations	40	40	40	40				
Year- and Study Fixed Effects	Y	Y	Y	Y				
Individual- and Study Controls	N	N	Y	Y				

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for the compositional outcome variables. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years seperately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01) a decrease by 2.6 percentage points in 2015. These effects are no longer significant when controlling for individual- and study controls.

Fourth, the average treatment effect on the relative number of MBO-students as presented in column (1) shows an increase of 5.7 percentage points due to the introduction of admission criteria. This is mainly driven by the increase of 9 percentage points in 2015 which is statistically significant at the 5 percent level. When controlling for individual- and study controls, both effects are no longer statistically significant.

Finally, the average treatment effects on the share of non-Western-, VWO- and HAVO-students show no statistically significant effects.

Overall, the direction of these results is similar to those of the main analysis. However, the magnitude and statistical significance of the results differ. This shows that the analysis would give an underestimation of the composition effects if one would not control for potential spillover effects and overfitting. One might conclude that the overestimation of the effects on the first-year success outcomes – by failing to account for potential spillovers – could indicate that the introduction of a Numerus Fixus has two separate effects on treatment- and control studies: 1) First-year success increases for treatment studies and 2) decreases for control studies. This might be caused by spillover students that enroll into control studies rather than treatment studies, therefore adversely affecting academic success for the control studies. However, this would be too conclusive as we do not know whether spillovers actually occur, due to data limitations. The next section elaborates on this.

7. Conclusion

Over several decades starting in the early 1970s, Higher Education Institutions (HEIs) in the Netherlands have introduced – and repealed – selection procedures using a variety of admission criteria. Putting a cap on the number of annual enrollments – known as a 'Numerus Fixus' – has become more common over the years and has shown a marked development from weighted lotteries to elaborately constructed decentral selection procedures.

Arguments for the introduction of a Numerus Fixus vary widely. Consciously reducing the number of students that enroll into a study program enables a HEI to select the top layer of students through admission criteria, which should on average increase the program's quality. In turn, this provides an incentive for competing HEIs to introduce a Numerus Fixus in order to prevent an incoming flow of 'leftover' enrollments. Furthermore, HEIs deal with limited capacity and are judicially withheld in raising tuition fees to market-clearing levels due to accessibility concerns by the government. Similarly, the government can impose quota for specific programs to prevent a surplus of expensive students – partly funded by tax-payers – ending up in nonrelated professions. For example, the maximum number of enrollments in medical schools depends on the available slots for interns in hospitals and other medical facilities. This effect might be strengthened by related professions monopolizing the line of work to generate 'rents', reducing the need for freshly graduated students.

Overall, many HEIs have implemented admission criteria although little seems to be known about the consequences for students, HEIs and public welfare. Students differ in their individual characteristics and therefore might be affected differently by the introduction of a Numerus Fixus. Dutch government policy is aimed at preserving accessibility of higher education based on academic aptitude. However, it is unknown whether the introduction of a Numerus Fixus might affect the composition of enrollments through mechanisms which are unrelated to aptitude. An example of a non-aptitude related mechanism is the acquisition of extracurricular training during secondary education to prepare for the admission criteria of HEIs, which is dependent on financial resources of students and parents. In an attempt to shed light on several of these factors, this study examines the effects of the introduction of a Numerus Fixus on 1) the level of enrollments 2) first-year success – as measured by retention, switch- and dropout rates – and 3) the composition of enrollment in Dutch higher vocational education.

Failing to (reliably) employ a difference-in-differences method due to implausibility of the common trends assumption, this study builds on existing work by Abadie and Gardeazabal (2003) and utilizes their 'synthetic control method'. Resembling the difference-in-differences method, a control unit is constructed by assigning weights to all control studies in the 'donor pool' of control units and thus creating a synthetic control program. The allocation of weights is based on how well the study programs in the donor pool of control units resemble the treatment unit during the pre-treatment period. This is derived by the evolution of relevant outcome variables, as well as individual- and study characteristics of the treatment unit and therefore provides a suitable counterfactual for regression analyses. Optimally utilizing the available data, this study examines the effects of the introduction of a Numerus Fixus by 24 treatment (HBO) study program-HEI combinations in 2014. Using the entire pre-treatment period of 2003-2013, the synthetic control period employs eleven years to find an optimal fit with the evolution of the outcome variables. Moreover, this enables the study to examine effects directly after the introduction of a Numerus Fixus in 2014, and one year later in 2015.

This study finds that the introduction of a Numerus Fixus directly leads to a 14 percentage point decrease of enrollments for cohort 2014/2015, which is statistically significant at the 10 percent level. This effect constitutes the intention of the policy. It must be noted however, that the introduction of a Numerus Fixus only *mechanically* reduces enrollments when the Fixus is 'binding'. In other words, the number of available places in the study program must be lower than the number of applicants. Other mechanisms that reinforce the decline in enrollments include a relative increase of the required risk and perceived effort for secondary school students when applying for a Fixus program.

Surprisingly, the treatment has a positive – although not statistically significant – effect on enrollments in 2015. Two scenarios could be at play. Either the Fixus is binding, but treatment studies decide to raise the number of available study slots which *mechanically* leads to an increase of enrollments. Or the Fixus is not binding, leaving room for an increase of enrollments in 2015 through the following mechanisms. The introduction of a Numerus Fixus raises profession-related 'rents', relative income and status of those that graduate from these studies. Similarly, feedback- and peer effects increase the quality of the study programs and therefore positively affect consumption motives of students to enroll. Further research should focus on whether admission criteria by a Numerus Fixus are 'binding' in order to assess the relevance of these mechanisms.

The introduction of a Numerus Fixus increases the rate of retention by 6.8 percentage points after one year, which is statistically significant at the 1 percent level. This is in line with the arguments for HEIs to introduce admission criteria as stated by Winston (1999). The selection of better students should overall lead to higher academic achievement and fewer dropouts. The latter is in indeed confirmed as the increase in retention is represented by a decrease in dropout rates by 2.9 percentage points in 2015 and a decrease in switch rates by 3.9 percentage points. These effects are statistically significant at the 5 percent and 10 percent level respectively. An improvement of academic success rates as measured by lower dropout- and switch rates can be explained by several mechanisms. The intention of HEIs to increase academic success by selecting 'better' students appears to be validated by the increase in retention rates. This is reinforced by peer- and feedback effects. Students positively influence the academic achievements of students in their cohort, whilst good students and staff attract more of their kind. Also, an increase in the required effort to enroll leads to better informed students, reducing the risk of students dropping out and creates a stronger 'bond' between them and the study program, as suggested by a study by the Dutch Education Inspectorate (Inspectie van het Onderwijs, 2017).

The introduction of a Numerus Fixus has a positive effect on the share of female-, native- and MBO students. The increase in the share of native students mainly goes at the expense of non-Western students, whilst the increase in the share of MBO students primarily affects the share of HAVO students. These students seem to be less susceptible to the adverse effects of the introduction of a Numerus Fixus on levels of risk and perceived effort compared to male-, non-Western- and HAVO students. Also, admission criteria impose different – and potentially less demanding – requirements for MBO-students and might therefore explain why the introduction of a Numerus Fixus leads to an increase in the share of MBO students at the expense of HAVO students.

These findings are in line with preliminary results from a study by the Dutch Education Inspectorate (Inspectie van het Onderwijs, 2017). This study suggests that the introduction of admission criteria leads to a slight decline in the relative number of non-Western- and male students. Furthermore, the study suggests a decline in the relative number of students with a low secondary school GPA and those coming from low SES backgrounds.

7.1 Limitations

This paper finds multiple significant effects as a result of the introduction of a Numerus Fixus. However, the results presented in this study should be interpreted with caution. There are several limitations to this study that must be considered. First, this paper only looks at the effects with respect to the introduction of a Numerus Fixus by 24 (HBO) treatment studies in 2014. Therefore, several concerns related to the external validity of the results materialize. The 24 study program-HEI combinations consist of a limited number of different study programs and HEIs, as shown in appendix D. It can be argued that these studies are not a proper representation of all studies affiliated to universities of applied science. Also within the treatment group, heterogeneity in study program- and institutional characteristics might imply that effects differ across studies. Moreover, the introduction of a Numerus Fixus in a different year than 2014 might have contrasting effects.

Overall, one must be careful in interpreting these results as being representative for all vocational study programs over a wide timespan. Ideally, further research would utilize a larger sample – consisting of a diverse set of HBO and WO program-HEI combinations – and examine the effects of the introduction of admission criteria in several different years. Furthermore, it would be interesting to examine long-run effects. Besides the outcome variables as presented in this paper, further research could examine the effects of selection policy on study duration by exploiting a longer time-period.

Another limitation of this study is the relevant motive for students to drop out of higher education or switch to another program. As discussed in this paper, students have several reasons to discontinue their current program including a lack of motivation or a dismissal through academic dismissal policy such as a Binding Study Advice (BSA). Due to a lack of proper data on the implementation of BSA policy in Dutch higher education, this study fails to account for dismissals and therefore cannot make this distinction. Ideally, further research could utilize data regarding the use of BSA policy including the number of ECTS required to continue a program. Similarly, other – more elaborate – covariates related to labor market conditions, individual- and institutional characteristics could improve the credibility of these results. Important covariates include socioeconomic scores (SES), the level of education of students' parents, and the number of students relative to the number of staff. Also, the secondary school grade-point average (GPA) – or other variable that functions as a reliable indicator of cognitive abilities – would form a relevant control variable in future analyses.

The implementation of a Numerus Fixus can be given shape in a variety of ways. It would be interesting to examine whether the use of a (weighted) lottery has a distinct effect from the use of a decentral selection policy. Similarly, it would be interesting to study separate compositions of a decentral selection procedure by looking at different effects due to interviews, motivation letters, tests and other criteria. All treatment units in this study implement a decentral selection procedure. Unfortunately, due to data limitations it is unknown whether the programs also (partly) utilize the weighted lottery. However, a paper by the Dutch Education Inspectorate (Inspectie van het Onderwijs, 2017) suggests that the 'Wet Kwaliteit in Verscheidenheid' – the legislation stating the repeal of weighted lotteries – was already announced in 2013. Thus, it is expected that study programs introducing a Numerus Fixus in 2014 anticipated the new legislation by fully implementing decentral selection procedures.

Finally, the effects as presented in this paper are driven by a combination of selection- and treatment effects. For example, an increase in retention might be due to the selection of better students through admission criteria, implying selection effects. However, academic performance of students might also improve due to direct effects as a result of the treatment. By adding relevant individual- and study characteristics, this study partly controls for selection effects. However, the remaining size of the coefficient still partly consists of non-observed selection effects and as a result the treatment effect remains biased. Future research should aim at further distinguishing selection- and treatment effects.

7.2 Discussion

The results from this paper are presented shortly after several noteworthy policies were introduced in Dutch higher education. Primarily, the former grant system was replaced by a system known as the 'sociaal leenstelsel' in September 2015. This entailed that students no longer received a monthly grant but were required to borrow additional funding if necessary. Although students already had the possibility to acquire a student loan in the previous system, they no longer receive the monthly grant which was designated as a ‘gift’ under the condition that students finish their studies within a ten year timespan.

The introduction of the ‘sociaal leenstelsel’ in 2015 seems to be accompanied by an anticipation effect of students in 2014, as can be seen in figure 4. Rather than taking a gap year between secondary school and higher education, many students decided to enroll directly into higher education. Thus, they avoided the new system which would deprive them from a monthly grant. This effect has been labelled as the ‘bow wave effect’ – ‘boeggolfeffect’ – caused by the introduction of the new system (Rijksoverheid, 2017). This represents the sudden increase in enrollments in the cohort before the implementation of the new policy, and the corresponding decline in the first cohort under the new system. Similarly, due to the introduction of the ‘sociaal leenstelsel’ the consequences of dropping out of higher education are significantly larger for students enrolling in 2014. These students have a much larger incentive not to drop out, as that would imply that the new – less favorable – regulations apply to them in case they decide to enroll in a subsequent year. This effect has implications for the interpretation of our findings. The relative increase in dropout rates of study programs in the treatment group in 2014 is driven by a predominant decline in dropout rates of the synthetic control group – as shown on the left-hand side of figure 5 – which might be due to this anticipation effect. Also, the relative increase in enrollments for the treatment group in 2015 is partly driven by a decrease in enrollments for the synthetic control group. This might illustrate the dip that corresponds to the anticipation effect. Unfortunately, this paper cannot account for these effects due to data limitations.

New legislation requires HEIs to use a decentral selection policy instead of the weighted lottery from the academic year 2017-2018 onwards, if they decide to implement a Numerus Fixus. This raises the question whether a decentral selection procedure works more effectively than a weighted lottery in finding the right fit between students and study programs. A recent paper by Wouters (2017) suggests that the use of decentral selection policies by medical schools in the Netherlands does not lead to better academic outcomes compared to the use of a lottery. Knowing that a decentral selection procedure requires a lot more time, effort and resources invested by HEIs compared to the weighted lottery – which was enforced by the Dutch education executive agency DUO – it remains questionable why such legislation has been introduced. It must be noted however, that HEIs might care more about other factors related to the fit between a student and the study program than retention alone.

7.3. Implications

The findings in this paper have several implications for students, HEIs and public welfare. The introduction of a Numerus Fixus leads to a decline in enrollments and an increase in first-year academic success for HEIs introducing the policy. Similarly, higher retention rates involve positive implications for the students enrolled in these programs. However, this study cannot distinguish whether the policy increases the academic success of those students, or whether HEIs merely select 'better' students.

Although the policy involves positive effects for HEIs and the selected students, it is unclear what happens to those students that drop out or do not enroll into study programs using a Fixus. Two main scenarios materialize. First, students that do not pass the admission criteria enroll into other study programs and due to their – on average – lower academic aptitude this leads to a decline in academic performance for those programs. This would suggest a 'waterbed effect', where the increase in academic outcomes for Fixus studies goes at the expense of other study programs that receive the 'unwanted' students. Another scenario assumes that admission criteria function as an optimal 'allocation mechanism' and entail better academic outcomes for both selected and unselected students due to better matching. The robustness analysis in this paper indicates that failing to account for potential spillovers would lead to an overestimation of the effects on the main outcome variables. One might conclude that this indicates that the introduction of Numerus Fixus leads to a waterbed effect. However, this would be too conclusive as we do not know whether spillovers actually occur, due to data limitations.

At the backdrop of this discussion are high dropout- and switch rates in Dutch higher education – as shown in figure 1 – implying large costs for students, HEIs and society (CBS, 2017). As a significant proportion of higher education is publicly funded, it is important to consider the welfare implications of the introduction of admission criteria by HEIs. It must be noted that the results in this paper measure the effects of the introduction of a Numerus Fixus for study programs and students, but do not measure the aggregate welfare effects. In order to obtain relevant coefficients measuring welfare effects, one must refrain from dropping study programs due to potential spillover effects. Retaining these study programs in the donor pool of control studies ensures that the parameters of interest measure the combined effect of 1) the change of the outcome variable for the treatment studies and 2) the subsequent change of the outcome variable for the control studies. For example, if one would want to measure the welfare effect of the introduction of a Numerus Fixus on dropout rates in higher education, one must not only measure the change in dropout rate of the treatment studies but also that of the study programs receiving additional students due to spillovers.

If the use of admission criteria leads to lower switch- and dropout rates – as suggested by the results in this paper – this would imply that students do not 'lose' a year of higher education. As the education of students is (partly) funded by the government, this has positive consequences for public welfare. Also, stricter admission criteria in higher education can lead to higher levels of competition in secondary schools which raises the overall quality of education, as suggested by Jacobs and Van der Ploeg (2006).

However, a further increase in the number of HEIs using a Numerus Fixus could lead to larger flows of students being forced into programs that do not use such policy. This would provide an additional incentive for those HEIs to introduce a Numerus Fixus as well. Indeed, a report by the Dutch Education Inspectorate (Inspectie van het Onderwijs, 2015) suggests that many HEIs justify the introduction of a Numerus Fixus as to prevent attracting the surplus of similar institutions and study programs. Eventually, this might further affect accessibility of higher education. Furthermore, the introduction of a Numerus Fixus can lead to negative welfare effects when it involves a decline of graduates in sectors that contribute to public welfare. For example, several technical study programs have introduced a Numerus Fixus in the last decade due to a lack of facilities. Meanwhile, employers in technical sectors indicate large shortages of adequately trained personnel (Huygen, 2016). Overall, arguments for HEIs to introduce a Numerus Fixus can contradict public interests.

Finally, when assessing the implications of the compositional effects, an important consideration is whether the accessibility of higher education is affected by the introduction of a Numerus Fixus. Compositional effects of such policy might not necessarily pose a problem for accessibility concerns, as long as the effects are driven by criteria testing academic aptitude. However, when current criteria can be prepared for by attaining training, this can impose a disadvantage for those students with less financial means. Indeed, a report by the Dutch Education Inspectorate (Inspectie van het Onderwijs, 2016) suggests that the attainment of so-called 'shadow education' is increasingly common and appears to have adverse effects on equal opportunities in education. Several mechanisms as discussed in this paper – notably the effects of the introduction of a Numerus Fixus on levels of effort and risk – suggest potential consequences for the accessibility of higher education due to heterogeneity in socioeconomic background. Future research should examine whether this indeed plays a significant role in the run-up to higher education. Overall, further research should study long run effects of the introduction of admission criteria and examine what happens to those who are not considered grain, but chaff.

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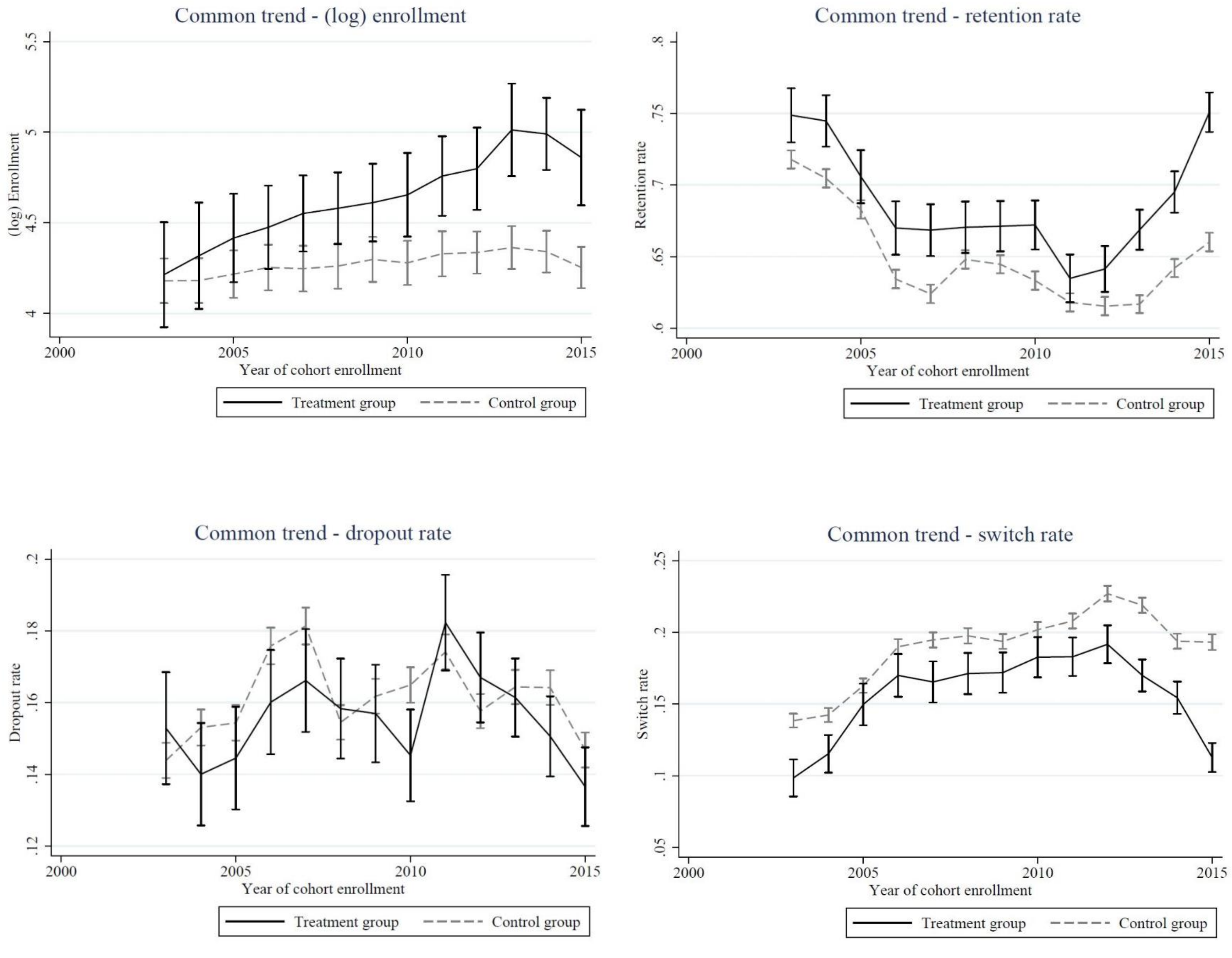
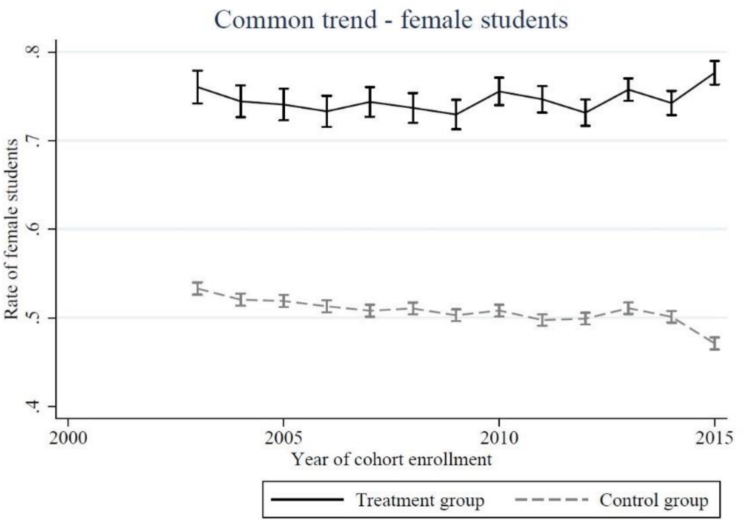
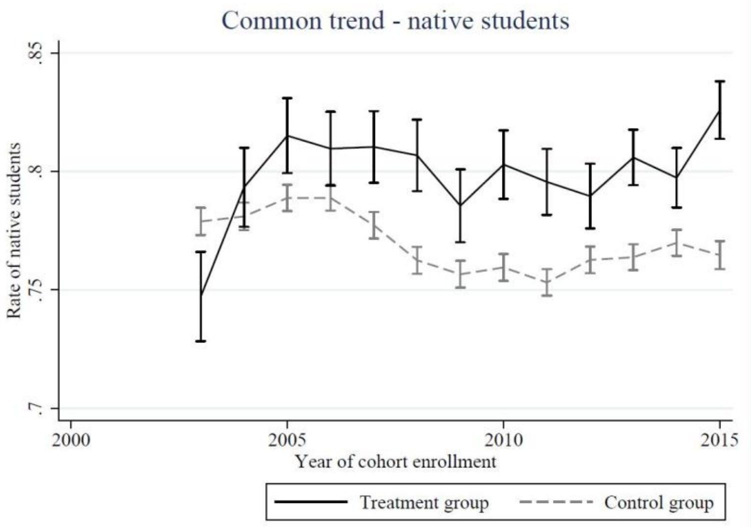
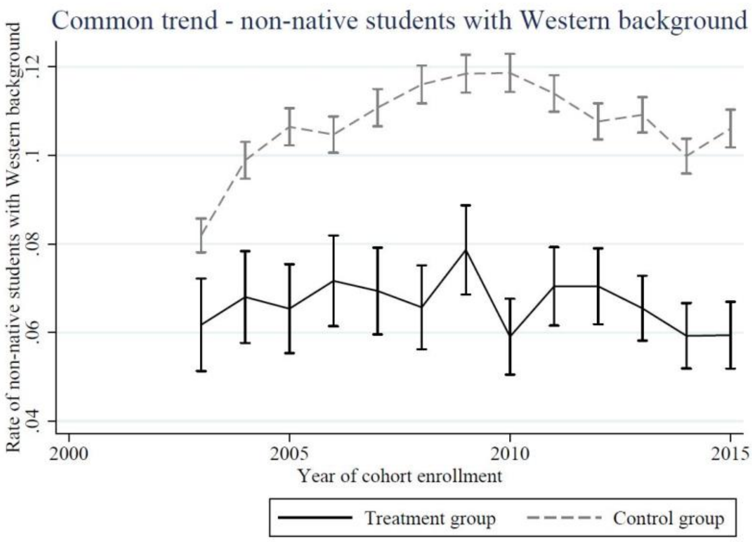
Appendix A: Common trend figures

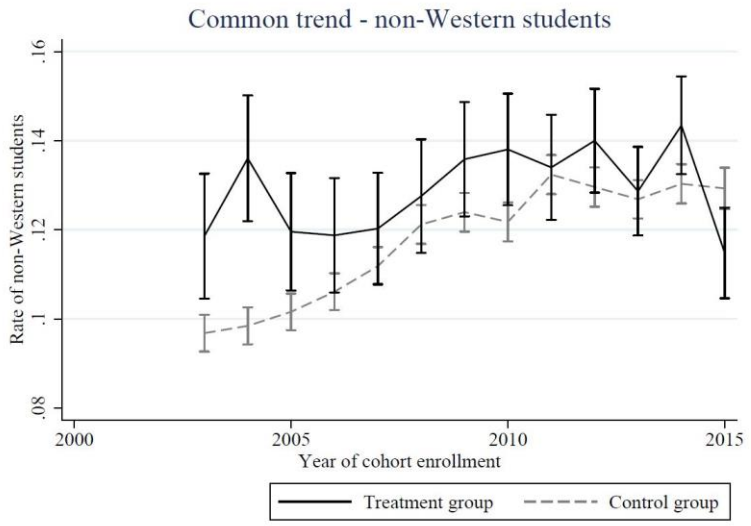


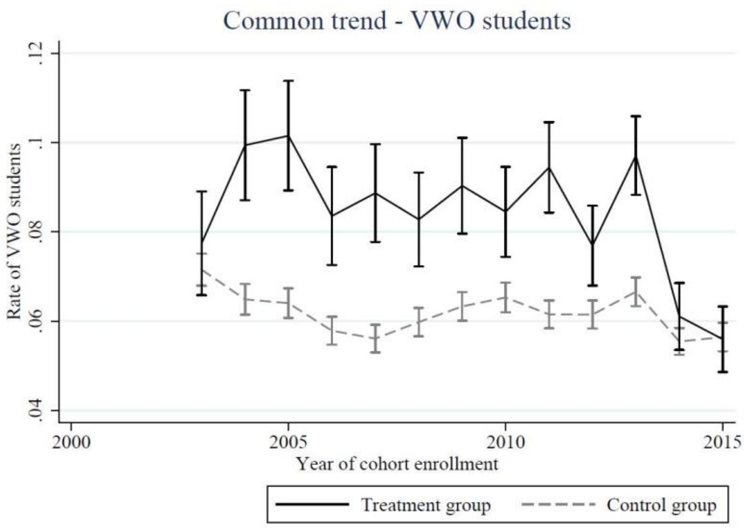
Notes: This figure displays the evolution of the main outcome variables for both the treatment group and the control group over the period 2003-2015, as to visualize that several variables lack the common trend needed for a difference-in-differences analysis. The treatment group is depicted by the solid black line. The control group is depicted by the dashed gray line. The figure includes visualizations of the 95 percent confidence intervals. The outcome variable is depicted on the vertical axis. The year of cohort enrollment is depicted on the horizontal axis.

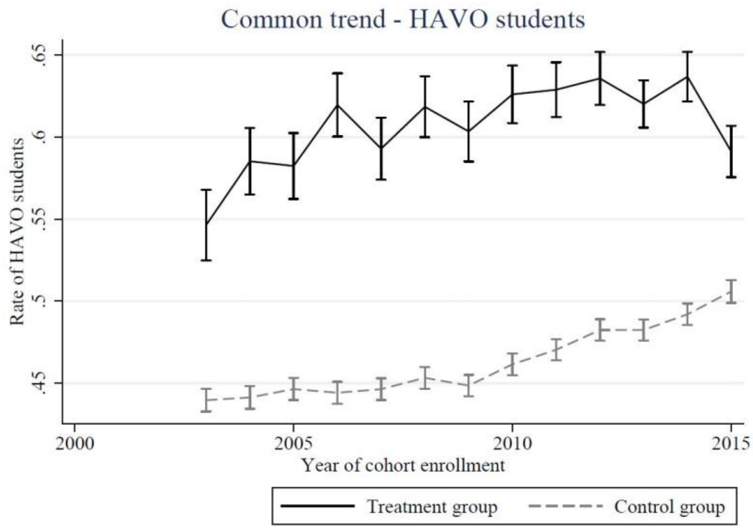

Common trend - female students
Rate of female students
.8
.7
.6
.5
.4
2000
2005
2010
2015
Year of cohort enrollment
Treatment group
Control group

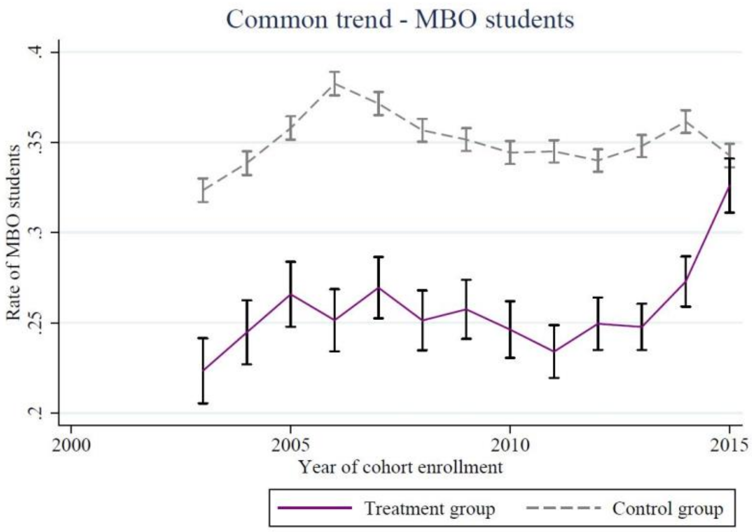

Common trend - native students
Rate of native students
.85
.8
.75
.7
2000
2005
2010
2015
Year of cohort enrollment
Treatment group
Control group


Common trend - non-native students with Western background
Rate of non-native students with Western background
.12
.1
.08
.06
.04
2000
2005
2010
2015
Year of cohort enrollment
Treatment group
Control group


Common trend - non-Western students
Rate of non-Western students
.16
.14
.12
.1
.08
2000
2005
2010
2015
Year of cohort enrollment
Treatment group
Control group


Common trend - VWO students
Rate of VWO students
.12
.1
.08
.06
.04
2000
2005
2010
2015
Year of cohort enrollment
Treatment group
Control group


Common trend - HAVO students
Rate of HAVO students
.65
.6
.55
.5
.45
2000
2005
2010
2015
Year of cohort enrollment
Treatment group
Control group



Notes: This figure displays the evolution of the compositional outcome variables for both the treatment group and the control group over the period 2003-2015, as to visualize that several variables lack the common trend needed for a difference-in-differences analysis. The treatment group is depicted by the solid black line. The control group is depicted by the dashed gray line. The figure includes visualizations of the 95 percent confidence intervals. The outcome variable is depicted on the vertical axis. The year of cohort enrollment is depicted on the horizontal axis.

Appendix B: The average treatment effects – difference-in-differences analysis

	(log) Enrollment		*Retention*			
	(1)	(2)	(1)	(2)	(3)	(4)
Average Treatment Effect	0.32***		0.029**		0.028**	
	(0.046)		(0.013)		(0.013)	
ATE 2014		0.34***		0.017		0.016
		(0.055)		(0.017)		(0.017)
ATE 2015		0.29***		0.041**		0.040**
		(0.069)		(0.017)		(0.018)
Observations						
Students	N/A	N/A	318083	318083	318083	318083
Study program-HEI combinations	3042	3042	3042	3042	3042	3042
Year- and Study Fixed Effects	Y	Y	Y	Y	Y	Y
Individual- and Study Controls	N	N	N	N	Y	Y

	Switch				*Dropout*			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Average Treatment Effect	-0.022***		-0.010		-0.0071		-0.018**	
	(0.0082)		(0.0082)		(0.0073)		(0.0078)	
ATE 2014		-0.0093		-0.0027		-0.0081		-0.014
		(0.011)		(0.011)		(0.0091)		(0.0097)
ATE 2015		-0.035***		-0.018		-0.0059		-0.022**
		(0.010)		(0.011)		(0.011)		(0.011)
Observations								
Students	318083	318083	318083	318083	318083	318083	318083	318083
Study program-HEI combinations	3042	3042	3042	3042	3042	3042	3042	3042
Year- and Study Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Individual- and Study Controls	N	N	Y	Y	N	N	Y	Y

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for the main outcome variables, based on a standard Difference-in-Differences analysis. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years separately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

	Female Students				*Native Students*			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Average Treatment Effect	0.016**		0.019***		0.0084		0.018**	
	(0.0072)		(0.0072)		(0.0085)		(0.0069)	
ATE 2014		0.015		0.016		0.0048		0.013
		(0.010)		(0.0097)		(0.010)		(0.0082)
ATE 2015		0.018*		0.023**		0.012		0.023**
		(0.0095)		(0.0096)		(0.012)		(0.0098)
Observations								
Students	318083	318083	318083	318083	318083	318083	318083	318083
Study program-HEI combinations	3042	3042	3042	3042	3042	3042	3042	3042
Year- and Study Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Individual- and Study Controls	N	N	Y	Y	N	N	Y	Y

	Non-native Western Students				*Non-Western Students*			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Average Treatment Effect	-0.00098		0.0053		-0.0063		-0.014*	
	(0.0043)		(0.0051)		(0.0073)		(0.0073)	
ATE 2014		0.00011		0.0036		-0.0041		-0.0092
		(0.0045)		(0.0052)		(0.0092)		(0.0091)
ATE 2015		-0.0021		0.0071		-0.0087		-0.018*
		(0.0068)		(0.0073)		(0.011)		(0.011)
Observations								
Students	318083	318083	318083	318083	318083	318083	318083	318083
Study program-HEI combinations	3042	3042	3042	3042	3042	3042	3042	3042
Year- and Study Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Individual- and Study Controls	N	N	Y	Y	N	N	Y	Y

	VWO Students				*HAVO Students*			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Average Treatment Effect	-0.022***		-0.016***		-0.030*		0.018*	
	(0.0057)		(0.0052)		(0.017)		(0.0100)	
ATE 2014		-0.017**		-0.013*		-0.0042		0.025*
		(0.0070)		(0.0069)		(0.019)		(0.014)
ATE 2015		-0.028***		-0.019***		-0.058**		0.0095
		(0.0083)		(0.0072)		(0.025)		(0.012)
Observations								
Students	318083	318083	318083	318083	318083	318083	318083	318083
Study program-HEI combinations	3042	3042	3042	3042	3042	3042	3042	3042
Year- and Study Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Individual- and Study Controls	N	N	Y	Y	N	N	Y	Y

	MBO Students			
	(1)	(2)	(3)	(4)
Average Treatment Effect	0.044**		0.016	
	(0.019)		(0.014)	
ATE 2014		0.011		-0.0057
		(0.022)		(0.018)
ATE 2015		0.078***		0.039*
		(0.028)		(0.020)
Observations				
Students	318083	318083	318083	318083
Study program-HEI combinations	3042	3042	3042	3042
Year- and Study Fixed Effects	Y	Y	Y	Y
Individual- and Study Controls	N	N	Y	Y

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for the compositional outcome variables, based on a standard Difference-in-Differences analysis. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years separately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

Appendix C: Additional estimation results

	Retention			
	(1)	(2)	(3)	(4)
Average Treatment Effect	0.042		0.026	
	(0.026)		(0.027)	
ATE 2014		0.016		0.0047
		(0.045)		(0.043)
ATE 2015		0.068***		0.049*
		(0.024)		(0.028)
Share of female students			0.13***	0.13***
			(0.0083)	(0.0083)
Age			0.0018	0.0018
			(0.0011)	(0.0011)
Share of native students			0.060***	0.060***
			(0.014)	(0.014)
Share of nonwestern students			-0.068***	-0.068***
			(0.015)	(0.015)
Higher education graduates			0.067**	0.068**
			(0.026)	(0.026)
Direct entrants			0.0022	0.0023
			(0.0094)	(0.0094)
Share of HAVO students			-0.039**	-0.039**
			(0.017)	(0.017)
Share of VWO students			0.058***	0.058***
			(0.019)	(0.019)
Share of MBO students			-0.017	-0.017
			(0.017)	(0.017)
Constant	0.58***	0.58***	0.45***	0.45***
	(0.038)	(0.038)	(0.051)	(0.051)
Observations				
Students	50481	50481	50481	50481
Study program-HEI combinations	38	38	38	38
Year- and study fixed effects	Y	Y	Y	Y
Individual- and study controls	N	N	Y	Y

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for retention. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years seperately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

	Switch			
	(1)	(2)	(3)	(4)
Average Treatment Effect	-0.016		-0.016	
	(0.015)		(0.015)	
ATE 2014		0.0069		0.0042
		(0.017)		(0.016)
ATE 2015		-0.039*		-0.036
		(0.021)		(0.023)
Share of female students			-0.073***	-0.073***
			(0.0059)	(0.0060)
Age			-0.0035***	-0.0035***
			(0.00050)	(0.00050)
Share of native students			-0.013	-0.013
			(0.0084)	(0.0084)
Share of nonwestern students			0.091***	0.091***
			(0.010)	(0.010)
Higher education graduates			-0.068***	-0.068***
			(0.016)	(0.016)
Direct entrants			0.028***	0.028***
			(0.0075)	(0.0075)
Share of HAVO students			0.062***	0.062***
			(0.012)	(0.012)
Share of VWO students			0.049***	0.049***
			(0.014)	(0.014)
Share of MBO students			0.0030	0.0033
			(0.012)	(0.012)
Constant	0.055***	0.055***	0.081***	0.081***
	(0.015)	(0.014)	(0.024)	(0.024)
Observations				
Students	54810	54810	54810	54810
Study program-HEI combinations	38	38	38	38
Year- and study fixed effects	Y	Y	Y	Y
Individual- and study controls	N	N	Y	Y

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for switch rates. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years seperately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

	Dropout			
	(1)	(2)	(3)	(4)
Average Treatment Effect	-0.0035		-0.0025	
	(0.015)		(0.016)	
ATE 2014		0.022		0.024
		(0.022)		(0.024)
ATE 2015		-0.029**		-0.030**
		(0.014)		(0.015)
Share of female students			-0.040***	-0.040***
			(0.0066)	(0.0066)
Age			0.00082	0.00084
			(0.00073)	(0.00073)
Share of native students			-0.013	-0.013
			(0.011)	(0.011)
Share of nonwestern students			-0.019*	-0.019
			(0.011)	(0.011)
Higher education graduates			0.024	0.024
			(0.024)	(0.024)
Direct entrants			-0.040***	-0.040***
			(0.0084)	(0.0084)
Share of HAVO students			-0.063***	-0.063***
			(0.016)	(0.016)
Share of VWO students			-0.14***	-0.14***
			(0.017)	(0.017)
Share of MBO students			0.0026	0.0028
			(0.015)	(0.015)
Constant	0.16***	0.16***	0.26***	0.26***
	(0.016)	(0.016)	(0.031)	(0.031)
Observations				
Students	56169	56169	56169	56169
Study program-HEI combinations	38	38	38	38
Year- and study fixed effects	Y	Y	Y	Y
Individual- and study controls	N	N	Y	Y

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for dropout rates. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years seperately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

	Share of female students			
	(1)	(2)	(3)	(4)
Average Treatment Effect	0.030**		0.029**	
	(0.012)		(0.012)	
ATE 2014		0.020		0.017
		(0.015)		(0.015)
ATE 2015		0.039**		0.042**
		(0.018)		(0.018)
Age			-0.0039***	-0.0039***
			(0.00080)	(0.00080)
Share of native students			-0.012	-0.012
			(0.0097)	(0.0097)
Share of nonwestern students			0.032***	0.032***
			(0.011)	(0.011)
Higher education graduates			-0.0066	-0.0064
			(0.019)	(0.019)
Direct entrants			0.023***	0.023***
			(0.0078)	(0.0078)
Share of HAVO students			-0.0080	-0.0082
			(0.014)	(0.014)
Share of VWO students			0.062***	0.062***
			(0.015)	(0.015)
Share of MBO students			-0.0075	-0.0077
			(0.013)	(0.013)
Constant	0.72***	0.72***	0.79***	0.79***
	(0.020)	(0.020)	(0.033)	(0.033)
Observations				
Students	66485	66485	66485	66485
Study program-HEI combinations	39	39	39	39
Year- and study fixed effects	Y	Y	Y	Y
Individual- and study controls	N	N	Y	Y

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for the share of female students. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years seperately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

	Share of native students			
	(1)	(2)	(3)	(4)
Average Treatment Effect	0.023		0.027	
	(0.020)		(0.017)	
ATE 2014		0.0049		0.0088
		(0.021)		(0.018)
ATE 2015		0.042		0.047*
		(0.032)		(0.027)
Share of female students			-0.0079	-0.0079
			(0.0071)	(0.0070)
Age			0.0030***	0.0030***
			(0.0010)	(0.0010)
Higher education graduates			-0.035	-0.035
			(0.030)	(0.030)
Direct entrants			0.046***	0.046***
			(0.0083)	(0.0083)
Share of HAVO students			0.56***	0.56***
			(0.028)	(0.028)
Share of VWO students			0.60***	0.60***
			(0.028)	(0.028)
Share of MBO students			0.50***	0.50***
			(0.029)	(0.028)
Constant	0.82***	0.82***	0.22***	0.22***
	(0.027)	(0.027)	(0.042)	(0.042)
Observations				
Students	51147	51147	51147	51147
Study program-HEI combinations	39	39	39	39
Year- and study fixed effects	Y	Y	Y	Y
Individual- and study controls	N	N	Y	Y

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for the share of native students. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years seperately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

	Share of non-native Western students			
	(1)	(2)	(3)	(4)
Average Treatment Effect	-0.0097		-0.0044	
	(0.0088)		(0.0097)	
ATE 2014		-0.0020		0.0019
		(0.0090)		(0.010)
ATE 2015		-0.017		-0.011
		(0.014)		(0.014)
Share of female students			0.00068	0.00069
			(0.0046)	(0.0046)
Age			-0.0011*	-0.0011*
			(0.00065)	(0.00065)
Higher education graduates			-0.037**	-0.037**
			(0.016)	(0.016)
Direct entrants			-0.024***	-0.024***
			(0.0063)	(0.0063)
Share of HAVO students			-0.22***	-0.22***
			(0.021)	(0.021)
Share of VWO students			-0.22***	-0.22***
			(0.021)	(0.021)
Share of MBO students			-0.22***	-0.22***
			(0.021)	(0.021)
Constant	0.057***	0.057***	0.30***	0.30***
	(0.014)	(0.014)	(0.032)	(0.032)
Observations				
Students	54905	54905	54905	54905
Study program-HEI combinations	40	40	40	40
Year- and study fixed effects	Y	Y	Y	Y
Individual- and study controls	N	N	Y	Y

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for the share of non-native Western students. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years seperately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

Share of nonwestern students				
	(1)	(2)	(3)	(4)
Average Treatment Effect	-0.030**		-0.024*	
	(0.014)		(0.013)	
ATE 2014		-0.014		-0.0071
		(0.014)		(0.013)
ATE 2015		-0.046**		-0.041*
		(0.021)		(0.021)
Share of female students			0.021***	0.021***
			(0.0076)	(0.0076)
Age			0.00033	0.00034
			(0.00078)	(0.00078)
Higher education graduates			0.11***	0.11***
			(0.034)	(0.035)
Direct entrants			0.00056	0.00045
			(0.0074)	(0.0074)
Share of HAVO students			-0.17***	-0.17***
			(0.017)	(0.017)
Share of VWO students			-0.21***	-0.21***
			(0.018)	(0.018)
Share of MBO students			-0.10***	-0.10***
			(0.017)	(0.017)
Constant	0.13***	0.13***	0.26***	0.26***
	(0.013)	(0.013)	(0.028)	(0.028)
Observations				
Students	50203	50203	50203	50203
Study program-HEI combinations	38	38	38	38
Year- and study fixed effects	Y	Y	Y	Y
Individual- and study controls	N	N	Y	Y

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for the share of nonwestern students. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years seperately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

Share of VWO students				
	(1)	(2)	(3)	(4)
Average Treatment Effect	-0.010		-0.0010	
	(0.011)		(0.0096)	
ATE 2014		-0.0032		0.0014
		(0.011)		(0.011)
ATE 2015		-0.017		-0.0036
		(0.017)		(0.015)
Share of female students			0.028***	0.028***
			(0.0040)	(0.0040)
Age			-0.0024***	-0.0024***
			(0.00030)	(0.00030)
Share of native students			0.032***	0.032***
			(0.0057)	(0.0057)
Share of nonwestern students			-0.025***	-0.025***
			(0.0063)	(0.0063)
Higher education graduates			0.0035	0.0035
			(0.014)	(0.014)
Direct entrants			0.0015	0.0015
			(0.0043)	(0.0043)
Constant	0.073***	0.073***	0.092***	0.092***
	(0.016)	(0.016)	(0.019)	(0.019)
Observations				
Students	79454	79454	79454	79454
Study program-HEI combinations	46	46	46	46
Year- and study fixed effects	Y	Y	Y	Y
Individual- and study controls	N	N	Y	Y

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for the share of VWO students. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years seperately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

	Share of HAVO students			
	(1)	(2)	(3)	(4)
Average Treatment Effect	-0.015		0.026	
	(0.026)		(0.020)	
ATE 2014		0.0068		0.026
		(0.030)		(0.023)
ATE 2015		-0.036		0.025
		(0.039)		(0.030)
Share of female students			-0.020**	-0.020**
			(0.010)	(0.010)
Age			-0.038***	-0.038***
			(0.0020)	(0.0020)
Share of native students			0.060***	0.060***
			(0.018)	(0.018)
Share of nonwestern students			-0.034*	-0.034*
			(0.019)	(0.019)
Higher education graduates			-0.11	-0.11
			(0.074)	(0.074)
Direct entrants			0.038**	0.038**
			(0.017)	(0.017)
Constant	0.65***	0.65***	1.35***	1.35***
	(0.050)	(0.050)	(0.061)	(0.061)
Observations				
Students	50377	50377	50377	50377
Study program-HEI combinations	37	37	37	37
Year- and study fixed effects	Y	Y	Y	Y
Individual- and study controls	N	N	Y	Y

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for the share of HAVO students. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years seperately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

	Share of MBO students			
	(1)	(2)	(3)	(4)
Average Treatment Effect	0.079***		0.042**	
	(0.023)		(0.020)	
ATE 2014		0.066**		0.042*
		(0.028)		(0.025)
ATE 2015		0.091**		0.042
		(0.036)		(0.029)
Share of female students			-0.0088	-0.0088
			(0.0072)	(0.0072)
Age			0.028***	0.028***
			(0.0026)	(0.0026)
Share of native students			0.11***	0.11***
			(0.013)	(0.013)
Share of nonwestern students			0.13***	0.13***
			(0.014)	(0.014)
Higher education graduates			0.18***	0.18***
			(0.046)	(0.046)
Direct entrants			0.069***	0.069***
			(0.013)	(0.013)
Constant	0.19***	0.19***	-0.52***	-0.52***
	(0.025)	(0.025)	(0.061)	(0.061)
Observations				
Students	55928	55928	55928	55928
Study program-HEI combinations	36	36	36	36
Year- and study fixed effects	Y	Y	Y	Y
Individual- and study controls	N	N	Y	Y

Notes: This table presents the estimation results of the average treatment effects related to the introduction of a Numerus Fixus for the share of MBO students. The coefficients reflect the average marginal change of the outcome variable with respect to the treatment. Column (1) presents the average treatment effect over both post-treatment years, without controlling for individual- and study controls. Column (2) presents the average treatment effect for both post-treatment years seperately, without controlling for individual- and study controls. Columns (3) and (4) build on columns (1) and (2) respectively by controlling for individual- and study controls. Standard errors are presented in parentheses and are clustered at the study program-HEI-year level. Stars indicate the level of significance of the estimates (* p<0.10, ** p<0.05, *** p<0.01)

Appendix D: Treatment study program-HEI combinations and their characteristics

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)
ISATcode	*BRINcode*	*Name of study program*	*Name of HEI*	*Sum of annual enrollments 2003 - 2015*	*Annual enrollments*	*Age*	*Female students*	*VWO students*	*HAVO students*	*MBO students*	*Native students*	*Non-native Western students*	*Non-Western students*	*Direct entrants*	*Higher education graduates*	*Retention rate*	*Switch rate*	*Dropout rate*
34396	07GR	Chemie	Avans Hogeschool	760	78.16	19.63	0.339	0.0686	0.63	0.267	0.826	0.0647	0.104	0.861	0.0023	0.801	0.0937	0.105
			Tilburg		(1.265)	(0.3)	(0.0184)	(0.0184)	(0.0531)	(0.0454)	(0.0198)	(0.0176)	(0.0134)	(0.0268)	(0.00161)	(0.0193)	(0.015)	(0.0102)
34397	07GR	Biologie en Medisch	Avans Hogeschool	1433	121.35	19.44	0.579	0.101	0.661	0.182	0.788	0.076	0.127	0.869	0.00546	0.783	0.122	0.0952
		Laboratoriumonderzoek	Tilburg		(0.768)	(0.339)	(0.0234)	(0.00841)	(0.0235)	(0.0187)	(0.0246)	(0.00677)	(0.0154)	(0.0224)	(0.00163)	(0.0169)	(0.0108)	(0.0139)
34560	07GR	Opleiding tot	Avans Hogeschool	3140	294.59	21.19	0.898	0.0704	0.579	0.328	0.886	0.0465	0.0664	0.796	0.0213	0.766	0.0829	0.152
		Verpleegkundige	Tilburg		(1.958)	(0.44)	(0.00659)	(0.00652)	(0.0198)	(0.0215)	(0.0078)	(0.00339)	(0.00613)	(0.0196)	(0.00891)	(0.0138)	(0.00934)	(0.00922)
34560	21MI	Opleiding tot	Hogeschool Zeeland	1375	117.13	20.65	0.918	0.0979	0.582	0.245	0.834	0.0657	0.0455	0.832	0.00272	0.717	0.103	0.18
		Verpleegkundige	Vlissingen		(1.0652)	(0.748)	(0.00818)	(0.00802)	(0.039)	(0.0218)	(0.048)	(0.00649)	(0.00827)	(0.0397)	(0.00145)	(0.0345)	(0.0146)	(0.0294)
34735	21QA	Cultureel Erfgoed	Amsterdam Hogeschool	1210	97.23	20.52	0.81	0.0391	0.723	0.197	0.835	0.099	0.0644	0.624	0.00154	0.633	0.19	0.177
			van de Kunsten		(0.559)	(0.512)	(0.0091)	(0.00726)	(0.0153)	(0.011)	(0.0119)	(0.00804)	(0.00832)	(0.0219)	(0.00104)	(0.0172)	(0.0118)	(0.0109)
34396	21RI	Chemie	Hogeschool Leiden	862	70.35	19.23	0.292	0.0369	0.663	0.255	0.688	0.0611	0.251	0.896	0.00329	0.628	0.234	0.138
					(0.573)	(0.111)	(0.0233)	(0.00623)	(0.0345)	(0.0257)	(0.0158)	(0.00951)	(0.0153)	(0.0107)	(0.00229)	(0.0222)	(0.0175)	(0.0174)
34397	21RI	Biologie en Medisch	Hogeschool Leiden	1728	145.42	19.28	0.604	0.0585	0.68	0.216	0.612	0.0695	0.319	0.869	0.00972	0.662	0.225	0.113
		Laboratoriumonderzoek			(0.984)	(0.111)	(0.0149)	(0.00417)	(0.025)	(0.016)	(0.0217)	(0.00784)	(0.0249)	(0.00867)	(0.00349)	(0.0332)	(0.0245)	(0.0119)
34560	21RI	Opleiding tot	Hogeschool Leiden	2211	175.60	20.10	0.89	0.1	0.661	0.208	0.872	0.0517	0.0768	0.809	0.00276	0.746	0.110	0.144
		Verpleegkundige			(0.675)	(0.268)	(0.00618)	(0.00804)	(0.0168)	(0.0161)	(0.00963)	(0.00405)	(0.00914)	(0.0132)	(0.000891)	(0.0232)	(0.014)	(0.0127)
34560	22HH	Opleiding tot	Viaa-Gereformeerde	1126	95.08	20.07	0.902	0.0886	0.643	0.243	0.972	0.014	0.0139	0.856	0.0157	0.767	0.0657	0.167
		Verpleegkundige	Hogeschool Zwolle		(0.855)	(0.322)	(0.00965)	(0.0114)	(0.0231)	(0.0212)	(0.00539)	(0.00417)	(0.0037)	(0.0154)	(0.00385)	(0.0114)	(0.00781)	(0.0123)
34396	22OJ	Chemie	Hogeschool Rotterdam	651	53.41	19.61	0.326	0.0457	0.697	0.202	0.643	0.0578	0.299	0.837	0.00208	0.606	0.256	0.138
					(0.534)	(0.295)	(0.0264)	(0.0112)	(0.0425)	(0.0289)	(0.0229)	(0.00895)	(0.0214)	(0.0271)	(0.00208)	(0.0328)	(0.0274)	(0.0147)
34397	22OJ	Biologie en Medisch	Hogeschool Rotterdam	1392	112.91	19.55	0.609	0.0634	0.687	0.173	0.537	0.0548	0.408	0.821	0.00442	0.569	0.277	0.154
		Laboratoriumonderzoek			(0.749)	(0.17)	(0.0129)	(0.00522)	(0.0188)	(0.0135)	(0.0143)	(0.00717)	(0.0164)	(0.017)	(0.00169)	(0.0164)	(0.0147)	(0.013)
34560	23AH	Opleiding tot	Saxion Hogeschool	2805	262.88	20.44	0.883	0.0646	0.575	0.316	0.89	0.0648	0.0448	0.834	0.00183	0.769	0.0884	0.142
		Verpleegkundige	Enschede		(2.0339)	(0.398)	(0.00479)	(0.00531)	(0.0188)	(0.0268)	(0.00913)	(0.00889)	(0.00363)	(0.0183)	(0.00111)	(0.00909)	(0.00995)	(0.00487)
34560	25BA	Opleiding tot	Christelijke Hogeschool	1979	164.045	20.14	0.91	0.18	0.592	0.198	0.962	0.0228	0.0149	0.792	0.00489	0.767	0.067	0.166
		Verpleegkundige	Ede		(1.0351)	(0.415)	(0.00534)	(0.0146)	(0.0143)	(0.0233)	(0.00308)	(0.00279)	(0.00225)	(0.0197)	(0.00123)	(0.0153)	(0.0054)	(0.013)
34579	25BE	Voeding en Dietetiek	Hanzehogeschool	1158	94.44	19.40	0.919	0.103	0.549	0.323	0.913	0.0421	0.0453	0.815	0.00696	0.675	0.188	0.137
			Groningen		(0.628)	(0.0824)	(0.00691)	(0.00976)	(0.0211)	(0.0141)	(0.00836)	(0.0064)	(0.00591)	(0.013)	(0.00368)	(0.0145)	(0.00896)	(0.0125)
34396	25DW	Chemie	Hogeschool Utrecht	982	77.21	20.07	0.293	0.0235	0.653	0.297	0.726	0.0682	0.206	0.829	0.0154	0.604	0.275	0.121
					(0.367)	(0.197)	(0.0118)	(0.00545)	(0.0268)	(0.0259)	(0.0136)	(0.00605)	(0.0115)	(0.0172)	(0.00523)	(0.019)	(0.0205)	(0.00707)
34397	25DW	Biologie en Medisch	Hogeschool Utrecht	2035	163.17	19.64	0.548	0.0457	0.684	0.245	0.715	0.0807	0.204	0.823	0.021	0.627	0.246	0.127
		Laboratoriumonderzoek			(0.779)	(0.13)	(0.0114)	(0.00424)	(0.0173)	(0.0171)	(0.0144)	(0.00622)	(0.0146)	(0.0146)	(0.00457)	(0.0164)	(0.0101)	(0.00989)
34549	25DW	Optometrie	Hogeschool Utrecht	1125	92.63	21.22	0.674	0.101	0.44	0.4	0.625	0.0821	0.293	0.809	0.00513	0.695	0.132	0.172
					(0.664)	(0.346)	(0.0164)	(0.00809)	(0.0239)	(0.0182)	(0.0294)	(0.00945)	(0.0288)	(0.0184)	(0.00192)	(0.0264)	(0.0179)	(0.0131)
34560	25DW	Opleiding tot	Hogeschool Utrecht	3083	248.24	19.96	0.872	0.134	0.571	0.27	0.860	0.0526	0.0867	0.799	0.00515	0.728	0.136	0.136
		Verpleegkundige			(1.0425)	(0.213)	(0.00816)	(0.0127)	(0.0192)	(0.0274)	(0.0101)	(0.0046)	(0.0092)	(0.0132)	(0.00193)	(0.0192)	(0.0122)	(0.01)
34577	25DW	Orthoptie	Hogeschool Utrecht	423	35.94	19.06	0.956	0.172	0.586	0.216	0.609	0.0326	0.359	0.888	0.00188	0.536	0.302	0.162
					(0.452)	(0.123)	(0.0116)	(0.0162)	(0.0176)	(0.0226)	(0.0384)	(0.0101)	(0.0346)	(0.0181)	(0.00188)	(0.0265)	(0.0234)	(0.0137)
34456	25JX	Orientaalse Talen en	Zuyd Hogeschool	1295	110.67	19.43	0.638	0.0821	0.664	0.192	0.655	0.138	0.207	0.773	0.00818	0.527	0.262	0.211
		Communicatie	Heerlen		(0.753)	(0.0657)	(0.198)	(0.0164)	(0.00986)	(0.021)	(0.0126)	(0.0112)	(0.00845)	(0.0113)	(0.00256)	(0.0282)	(0.0182)	(0.0136)
34560	25JX	Opleiding tot	Zuyd Hogeschool	2076	169.37	20.42	0.85	0.074	0.55	0.332	0.863	0.0888	0.0477	0.821	0.00692	0.692	0.106	0.202
		Verpleegkundige	Heerlen		(0.865)	(0.202)	(0.00927)	(0.00623)	(0.012)	(0.0162)	(0.00601)	(0.0049)	(0.00433)	(0.0145)	(0.00308)	(0.0198)	(0.00896)	(0.0148)
34396	25KB	Chemie	Hogeschool van Arnhem	604	50.95	20.43	0.246	0.0348	0.629	0.289	0.795	0.0722	0.132	0.836	0.0299	0.524	0.262	0.214
			en Nijmegen (HAN)		(0.652)	(0.296)	(0.0172)	(0.00602)	(0.0375)	(0.0347)	(0.0166)	(0.0094)	(0.0145)	(0.0161)	(0.00681)	(0.032)	(0.0258)	(0.0155)
34397	25KB	Biologie en Medisch	Hogeschool van Arnhem	1686	139.71	19.51	0.535	0.0324	0.492	0.204	0.571	0.167	0.262	0.755	0.00796	0.635	0.207	0.158
		Laboratoriumonderzoek	en Nijmegen (HAN)		(0.939)	(0.0985)	(0.0159)	(0.00395)	(0.0202)	(0.0142)	(0.0135)	(0.0107)	(0.012)	(0.0144)	(0.00322)	(0.0223)	(0.0159)	(0.0106)
34560	25KB	Opleiding tot	Hogeschool van Arnhem	3507	278.54	19.40	0.881	0.118	0.603	0.246	0.881	0.0551	0.0627	0.854	0.00251	0.656	0.151	0.192
		Verpleegkundige	en Nijmegen (HAN)		(0.944)	(0.167)	(0.00481)	(0.00557)	(0.0152)	(0.0108)	(0.00478)	(0.00333)	(0.00343)	(0.0144)	(0.00108)	(0.0307)	(0.0178)	(0.0163)
				38646	**162.87**	**20.06**	**0.746**	**0.0841**	**0.607**	**0.257**	**0.799**	**0.0665**	**0.129**	**0.814**	**0.00829**	**0.688**	**0.157**	**0.156**
					(11.46)	(0.153)	(0.00371)	(0.00384)	(0.00727)	(0.00688)	(0.00528)	(0.0016)	(0.00267)	(0.00902)	(0.00082)	(0.0108)	(0.00822)	(0.00348)

Notes: This table presents the 24 treatment study program-HEI combinations that are used for the analysis in this paper. Column (1) presents the official ISAT-code which is unique for each study program. Column (2) presents the official BRIN-code which is unique for each HEI. Columns (3) and (4) present the corresponding names of the study program and HEI respectively. Column (5) presents the sum of annual enrollments over the years 2003-2015. Columns (6) - (19) present the (mean) coefficients over the years 2003-2015 of individual- and study characteristics used in the analyses for each study program - HEI combination. The standard errors are presented in parentheses.

Appendix E: Selection procedures and admission criteria

(1)	(2)	(3)	(4)	(5)
ISATcode	*BRINcode*	*Name of study program*	*Name of HEI*	*Selection procedure and admission criteria*
34396	07GR	Chemie	Avans Hogeschool Tilburg	Decentral selection procedure that constructs a ranking based on students' GPA and relevant courses. The procedure also includes a digital assessment, an interview, a motivational assignment, a group discussion with other students and an English language test. An additional assignment can be mandatory for MBO-students with a non-related background. Study programs can also include specific criteria such as mandatory trial-lectures and practical assessments for prospective nurses applying for the program 'Opleiding tot Verpleegkundige'.
34397	07GR	Biologie en Medisch Laboratoriumonderzoek	Avans Hogeschool Tilburg	
34560	07GR	Opleiding tot Verpleegkundige	Avans Hogeschool Tilburg	
34560	21MI	Opleiding tot Verpleegkundige	Hogeschool Zeeland Vlissingen	Decentral selection procedure.* MBO-students can only apply when they have a relevant profile.
34735	21QA	Cultureel Erfgoed	Amsterdam Hogeschool van de Kunsten	Decentral selection procedure.*
34396	21RI	Chemie	Hogeschool Leiden	Decentral selection procedure that constructs a ranking based on 1) two tests that are made by prospective students during a 'selection day' and 2) a digital motivational questionnaire. Test 1 includes questions regarding the recollection and understanding of material taught during the day. Test 2 includes questions about a lecture given during the day.
34397	21RI	Biologie en Medisch Laboratoriumonderzoek	Hogeschool Leiden	
34560	21RI	Opleiding tot Verpleegkundige	Hogeschool Leiden	
34560	22HH	Opleiding tot Verpleegkundige	Viaa-Gereformeerde Hogeschool Zwolle	Decentral selection procedure that constructs a ranking based on 1) a motivation letter, 2) a test regarding the students' relevant knowledge and 3) a case-test during a selection day.
34396	22OJ	Chemie	Hogeschool Rotterdam	Decentral selection procedure that constructs a ranking based on 1) a cognitive test regarding basic calculus and logical reasoning and 2) a personal interview testing motivation.
34397	22OJ	Biologie en Medisch Laboratoriumonderzoek	Hogeschool Rotterdam	
34560	23AH	Opleiding tot Verpleegkundige	Saxion Hogeschool Enschede	Decentral selection procedure that constructs a ranking based on 1) a cognitive test regarding math and biology, 2) a case-test and 3) a motivation letter.
34560	25BA	Opleiding tot Verpleegkundige	Christelijke Hogeschool Ede	Decentral selection procedure.*
34579	25BE	Voeding en Dietetiek	Hanzehogeschool Groningen	Decentral selection procedure.*
34396	25DW	Chemie	Hogeschool Utrecht	Decentral selection procedure that constructs a ranking based on 1) a Curriculum Vitae including GPA, 2) a motivation letter and 3) an assessment testing ambition, orientation and analytical skills.
34397	25DW	Biologie en Medisch Laboratoriumonderzoek	Hogeschool Utrecht	
34549	25DW	Optometrie	Hogeschool Utrecht	
34560	25DW	Opleiding tot Verpleegkundige	Hogeschool Utrecht	Decentral selection procedure that constructs a ranking based on 1) a cognitive test, 2) a personal interview and 3) a test including a simulation patient.
34577	25DW	Orthoptie	Hogeschool Utrecht	Decentral selection procedure that constructs a ranking based on 1) a Curriculum Vitae including GPA, 2) a motivation letter and 3) an assessment testing ambition, orientation and analytical skills.
34456	25JX	Orientaalse Talen en Communicatie	Zuyd Hogeschool Heerlen	Decentral selection procedure.*
34560	25JX	Opleiding tot Verpleegkundige	Zuyd Hogeschool Heerlen	
34396	25KB	Chemie	Hogeschool van Arnhem en Nijmegen (HAN)	Decentral selection procedure that constructs a ranking based on 1) an orientation assignment and 2) a selection day testing a set of skills including teamwork.
34397	25KB	Biologie en Medisch Laboratoriumonderzoek	Hogeschool van Arnhem en Nijmegen (HAN)	
34560	25KB	Opleiding tot Verpleegkundige	Hogeschool van Arnhem en	

Notes: This table presents the 24 treatment study program-HEI combinations that are used for the analysis in this paper, including their admission criteria for the years 2014-2015. Column (1) presents the official ISAT-code which is unique for each study program. Column 2 presents the official BRIN-code which is unique for each HEI. Columns (3) and (4) present the corresponding names of the study program and HEI respectively. Column (5) presents the admission criteria used by the study program - HEI in the years 2014-2015. A star (*) indicates that the details of the selection procedure in 2014 are unknown.