Students' behavioural response to the introduction of the 'langstudeerdersmaatregel'

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Abstract

In 2011 the 'langstudeerdersmaatregel' was introduced as a way to cut budgets on education and improve student performance. Students studying longer than the nominal duration plus a one year buffer were required to pay a fine of €3,063 per year. This paper evaluates the behavioural (anticipatory) effects of the measure as all financial effects were compensated at its abolishment in 2012. A quasi-natural experiment is evaluated, making use of a different policy measure ('Bachelor-before-Master' act) as source of exogenous variation. For sake of comparison between universities only bachelor's students are considered. A differences-in-differences set-up is employed in order to estimate the policy's effects on graduation rates. Significant effects were found for fifth year students only.

Keywords: Higher education, Policy change, Evaluation, Behavioural effects

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1. Introduction

As a result of the worldwide financial crisis that started in 2008 the Netherlands was forced to cut budgets. Whereas the field of education is generally protected from substantial cuts, this time around there was no way to prevent them. Several measures such as a student loan scheme were discussed, but

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at the time politicians were not yet ready to agree to such a major reform of student financial support. Instead the so-called 'langstudeerdersmaatregel' was announced in 2010. It targeted students who studied longer than the legally specified nominal duration of their studies. In short, it implied that all students studying longer than the nominal term (n) + 1 years in higher education were obliged to pay a 'fine' of a little over €3,000 per additional year of their studies. Note that legal tuition fees in the Netherlands for EEA citizens in 2012/2013 were €1,771 and for 2013/2014 €1,835. Hence, an increase of €3,000 is substantial: 169%. Clearly, this was to provide students with a negative financial incentive to graduate earlier (and possibly within the legally specified term). While framed as an austerity measure, it was also seen as a potential solution to a low graduation rate. For example, considering the cohort that started in 2007, only 25% of the students obtained a bachelor's degree within three years[1], the specified nominal term.

The aim of this paper is to evaluate the behavioural effects that this policy change induced at the students' level. The focus is specifically on behavioural effects, because political developments in relation to the policy measure are a complicating factor. First, the measure was announced in 2010, but after protests from students implementation was postponed to September 2012. Second, after a political crisis new elections took place shortly after the policy measure came into force. The newly formed government decided to abolish the measure in October 2012 (VVD-PvdA, 2012b). Moreover, they decided to reimburse payments that had already been settled. Thus, not only did regulation not last more than four months, all direct financial consequences for students were compensated. This leaves the behavioural component of the effects, as they could not be reversed as easily. In particular the anticipation effect could be substantial, as discussion about the fine started already in 2010 (VVD-CDA, 2010). Students are likely to have adjusted their behaviour to avoid the fine, for example by increasing their effort in order to graduate before payments were due, or by dropping out.

For sake of comparison between universities the analysis will focus on bachelor's programs within academic education. Since a downward trend in average time to graduation is present over time effects of the measure cannot be established simply through monitoring. In order to accommodate for this trend a quasi-natural experiment is evaluated. A different educational policy measure that was introduced at the same time, the Bachelor-before-Master

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[1]CBS StatLine - "Wetenschappelijk Onderwijs; Studievoortgang Vanaf Startjaar 1995".

act, is identified as exogenous source of variation. This act implies that students need to finish their bachelor's program before they are allowed to start a master's program. Several universities implemented this act before they were legally obliged to do so, and several others were granted a temporary postponement. This provides variation between universities that can be exploited.

This is relevant for the evaluation of the 'langstudeerdersmaatregel' because both measures exert their pressure within the same time frame: the final months of a study year. The hypothesis of this paper is that students studying at a university that has implemented the Bachelor-before-Master act on its own initiative already expect extra pressure to graduate in the final months of their bachelor's program, whereas students from the universities that were granted an exception are not. Hence, a positive effect of treatment is expected for both groups of students, but it is expected to be stronger for students that did not expect extra pressure to graduate at all. In order to do estimate this effect, progress of students from four different universities is compared, making up a control and treatment group. Furthermore, the analysis will distinguish between fourth year students and students that have been enrolled five or six years, since the cut-off point for paying the fine is at five years.

This paper will start by providing a concise overview of the set-up of financial support for students in Dutch higher education and go into the specifics of the 'langstudeerdersmaatregel'. Next, section 3 will provide an overview of the relevant literature, discussing both internationally oriented literature and research that is directly applicable to the Netherlands. Section 4 will discuss the empirical strategy and section 5 presents the employed dataset. Section 6 presents the results, and section 7 concludes with a discussion of the results and their implications.

2. Background information

2.1. Financial support Dutch higher education

The Dutch financial support system for students is divided into five different components, which are shown in table 1 below. The basic scholarship is available to all students, as are the student loan, public transport card and tuition fees credit. The additional scholarship is the exception to the rule, as it is only available to students who cannot count on financial support from

their parents, for a variety of reasons.

The maximum amount of monthly financial support in January to August 2014 was either €786.36 or €986.14, depending on whether the student was living with his or her parents or not. This difference is primarily driven by a difference of almost €180 in the basic scholarship. This can for example be used to pay for rent or groceries. The most interesting feature of the system is not the amount of support that is provided though. The fourth column of the table distinguishes two categories of support: performance-based and a loan, and in principle all five components are issued as a loan facility. However, the performance-based components are generally considered to be the 'real' financial support to students, as they are converted to an endowment if the student graduates within ten years. Note also that for the public transport card no fees are due, students themselves can decide if they want to use the opportunity to travel for free during either the week or weekend days. However, if they do not graduate within ten years they will have to repay a sum of €83.97 plus interest for every month the card was in use.

Table 1: Financial support for students in the Netherlands (monthly)
Applies to January - August 2014

	Living at home (with parents)	Living away from home	Category
Basic scholarship	€100.25	€279.14	Performance
Additional scholarship	€237.46	€258.35	Performance
Student loan	€295.73	€295.73	Loan
Public transport card	€83.97	€83.97	Performance
Tuition fees credit	€152.92	€152.92	Loan
Total	€786.36	€986.14	

Source: *DUO (2011); website DUO* [2]

Tuition fees in the Netherlands amounted to €1,835 for higher education in 2014, the same period as specified in table 1. Internationally these costs are relatively low, as can for example be illustrated by a sharp increase in British students enrolling in Dutch higher education after tuition fees in the

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[2]http://duo.nl/particulieren/student-hbo-of-universiteit/studiefinanciering/bedragen.asp

United Kingdom were raised[3]. An important detail is that tuition fees are paid directly to the educational institution. This is relevant because the discussion that follows will explain how the 'langstudeerdersmaatregel' was set up. It will be shown that universities also incurred a budget cut as a consequence of this policy measure.

2.2. 'Langstudeerdersmaatregel'

So far this section has concisely presented the financial context of the educational system in which the 'langstudeerdersmaatregel' will be evaluated. This paragraph will describe the policy measure itself in more detail. The original text of the bill can be found in the references as OCW (2011).

The policy measure was first mentioned in the coalition agreement of 2010 (VVD-CDA, 2010), which stated that 'in order to improve performance tuition fees will be increased for students studying longer than the nominal term'. The bill was sent to parliament on February 1st, 2011. This paragraph will first present a detailed description of the policy measure. Second, several key events in the public debate with respect to reactions to the policy measure will be discussed.

2.2.1. Description of the policy measure

The 'langstudeerdersmaatregel' was a policy measure that provided a legal possibility to demand higher tuition fees from students studying longer than the nominal term of a study program, plus a one year buffer. It applied to both bachelor's and master's programs, and amounted to an increase of €3,063 in study year 2012/2013. Thus, students taking five or more years to finish their bachelor's program had to pay the basic tuition fees plus the increase. The reason for this is that the nominal duration of a bachelor's program is equal to three years, adding a buffer of one year to this amounts to four. Therefore, students were now allowed to be enrolled for four years in their bachelor's program while paying the basic amount of tuition fees. The bill also explains that there were two main reasons for the introduction: boosting students' performance and cutting budgets. It was argued that, given the large increase of enrolments in higher education, the costs of students taking too long to graduate were no longer proportional.

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[3]See for example http://www.theguardian.com/money/2012/aug/17/save-25000-university-tuition-fee-refugees

The nominal term of a study program was defined based on the number of ECTS credits. 60 ECTS credits were considered to be the normal amount, which is indeed equal to the number of ECTS credits attainable per year in the majority of study programs. Note that in case of programs that required deviating amounts of ECTS credits the nominal term was generally rounded up. A crucial factor is that the policy measure defined a student studying longer than the nominal term plus one year based on years of enrolment (from here onwards 'students studying longer than n+1'). Furthermore, all years of enrolment since study year 1991/1992 were taken into account. Lastly, the policy measure included an exception for students with a disability, for whom had already been established that they were eligible for an additional year of financial support. For those students a year was added to the nominal term, implying that they only started paying increased tuition fees for a bachelor's program from year six onwards.

Financially, the policy measure was to structurally improve budgets by €370 million. This sum was calculated based on an estimate of €6,000 costs per student that remained in education over n+1. Now, note that only half of that had to be paid by the students. The other half had to be covered by the educational institution, thereby providing them with an incentive to push students to either finish their studies or to drop out. Furthermore, educational institutions received a generic budget cut as a result of this policy measure as well.

2.2.2. Criticism, protests and adjustments to the policy measure

In the months after the announcement of the increase in tuition fees it was picked up by several student organisations and educational institutions. On December 1st 2010 the Dutch newspaper NRC featured an article in which the president of the board of the Radboud University Nijmegen argued that the policy measure was unlawful. He argued this based on the principle of legal certainty: students who had already started their studies could not lawfully be subjected to this measure, as they had been unable to take it into account while planning their studies. Quickly, the policy measure had been coined as a 'fine for studying too long', a phrasing that would later also be used by the Ministry itself. A number of important events occurred in the two years that followed before the measure actually came into force. The most important events will be discussed below. For a more detailed overview see table 15 in the appendix.

In January 2011 15,000 students went to The Hague and protested against the introduction of the fine. Several political parties from the opposition expressed their support for the students, stating that they themselves would have had to pay the fine if the policy measure had been introduced during their studies. The State Secretary of Education responded by telling the students he was only trying to prevent them having to repay a high public debt later in their lives. Eventually though, in April, it became clear that the policy measure would be postponed for a year. The government needed the support of SGP in order to be able to count on a majority voting in favour, but SGP stated it would not vote in favour if it the policy measure came into force in 2011. The negotiated solution was to ensure the bill passed quickly, but to set the increase in tuition fees equal to €0 for 2011/2012. Keeping this in mind, the bill was passed on the 5th of July 2011.

For a while the debate on the topic calmed down, but in November the debate was stirred up by student organisations LSVb, ISO and LKvV when they announced they were taking the government to court over this matter. They argued along the same lines as the president of the board of the Radboud University Nijmegen did. Three main objections to the policy measure were posited:

1. The right of education: The Netherlands had committed itself to the implementation of free higher education, whilst this policy measure lead to the opposite.
2. The principle of legal certainty: students who had already started their studies had been unable to take this policy measure into account.
3. Generalization: the policy measure did not distinguish between different groups or different studies.

The last argument of these three was at the center of the public debate. This was the main point of critique because the Secretary of State of Education opposed the introduction of additional exceptions. Many students argued that the fine would be a strong disincentive for participating in a board, or for expanding their horizon abroad during their studies. Moreover, many older students raised objections against counting years of enrolment from 1991 onwards. However, a far more convincing point of critique was the way in which the policy measure treated part-time students and students with disabilities. As part-time students were treated similar to full-time students, they would most likely have had to pay the fine for half of their studies. Part-time students follow the same program as full-time students, but often have a job on the side. They aim to finish a program in 6-8 years this way.

A similar argument was applicable to students with chronic diseases. In particular students who would get ill during their studies would be in trouble, as the standard rules would still apply to them. Eventually, on the 17th of February 2012 the State Secretary of Education promised the Senate these groups of students would be compensated. In the public opinion though, there was a very negative image to the policy measure already, which could not be constrained by these adjustments any more.

Then the third phase of events around the policy measure started, as the First Rutte Cabinet collapsed in April 2012. A month later the trial against the government started, but on the 11th of July the judge ruled that the policy measure was not unlawful, and that its implementation could proceed. In those months however, politics were shifting rapidly. During the campaigns for the new elections it became clear that several parties were no longer convinced about their support for the policy measure. The crucial factor was the 180-degree turn the political party CDA made. In August they decided to retract their support for the fine and ask the Secretary of State of Education for an alternative. At the time it looked like this was the end of the policy measure, but the Secretary of State of Education claimed there were no alternatives. Given the financial position at the time it was very important that all budget cuts were covered, and thus the policy measure was not abolished yet. Political party GroenLinks did attempt to block the implementation of the policy measure by rushing a law through to do so, but failed to get enough support. As a result of this, the measure was not cancelled and came into force in September 2012. Every student that had taken more than the nominal duration plus one year and re-enrolled was obliged to pay the fine of €3,063, in addition to the basic tuition fees of €1,771. Finally, on the first of October 2012 the new coalition of VVD and PvdA announced they would abolishing the policy measure. In addition, all payments that had already been settled would be reimbursed.

3. Literature review

Now that the 'langstudeerdersmaatregel' has been introduced, the next step is to provide a broader scientific perspective. It is important to recognize the relevant field of literature and to determine where this study fits in. Since the 'langstudeerdersmaatregel' is in principle an increase in tuition fees for students, there are two branches of literature that are of specific importance for this paper. Firstly, price elasticity of higher education will be discussed. Many studies with respect to the effects on the decision whether

to enrol in higher education can be found; the effects on an increase in tuition fees *during* one's studies are less prominent. Secondly, less obvious, literature regarding debt aversion is of interest. This can be explained in a straightforward manner. The 'langstudeerdersmaatregel' was introduced while simultaneously emphasizing the possibility to borrow the sum of €3,000 through the tuition fees credit. This was stressed in order to warrant access to the system. Hence, all students could continue their studies: if they were unable to pay €3,000 with private funds, they could take out a loan of €3,000. This is where the principle of debt aversion comes in, as there is a wide field of literature claiming that especially students from low-income families can be debt averse. This implies that they are less inclined to take out a loan as a result of their background and possibly because they are more cautious in their estimates of future returns to education.

After discussing the topics of price elasticity of higher education and debt aversion, this section will go into studies that apply a similar empirical strategy as the paper in hand. Specifically section 3.3 will discuss both theoretical and empirical aspects of the decision to continue one's studies and pick up the pace. It increasingly focuses on the methodological approach and results in order to relate them to the study at hand.

3.1. Price elasticity

Price elasticity of higher education is a difficult topic, as mentioned before. The main issue is that a major part of the literature focuses on the enrolment decision in higher education of high-school students. This is not a problem in itself, but one can easily see why price elasticity of enrolment is likely to differ from the price elasticity that one perceives during their studies. As students have already decided to invest both time and money into their studies, they are likely to have a lower price elasticity. That implies that a change in price has a smaller effect on whether or not to continue their studies, than it would have had before they started studying, and thus on the decision whether to enrol or not. This decision of continuing one's studies will henceforth be referred to as within-year persistence or the persistence decision.

Well known literature reviews on this subject were written by Leslie and Brinkman (1987, 1988), which were updated about a decade later by

Heller ([1997])[4]. A large part of more recent information with respect to price elasticity of higher education however, is based on evaluations of policy changes. In particular two policies come to mind, firstly the introduction of the Higher Education Contribution Scheme (HECS) in Australia and, secondly, the Pell Grant in the United States.

3.1.1. Higher Education Contribution Scheme (HECS) - Australia

The HECS was introduced in Australia in 1989 and reintroduced tuition fees in higher education. Under this system, students have to contribute approximately 20-30% of the average cost of the program (Chapman and Ryan, [2005])[5]. Students are given the option to either pay the tuition fees up front, or to defer payment and take out a loan. They are then required to pay back this loan via a future increase in income taxes. According to Vossensteyn and Canton ([2001]), the latter option was chosen by 71% of HECS-liable students in 1997. Note that, depending on the level of income, the percentage added to the income tax differs. Originally, the reason for these different repayment rates was to provide a safeguard for students for whom private benefits of studying (in terms of increased salary) would be relatively low. However, further alterations of the HECS implemented a division of disciplines into three bands, with a new feature of varying levels of private contributions[6]. An interesting aspect of this division is that it takes into account both the costs and the expected future benefits of the discipline. This implies that a positive relationship exists between expected future benefits and the level of private contributions. Hence, while in essence HECS is viewed upon and intended as a cost recovery system, it is not fully consistent with such a set-up.

The HECS has been evaluated extensively, and the general consensus is that it had no significant negative impact on student enrolment. Chapman ([1997]) summarizes several studies that use attitudinal surveys in order to determine whether HECS was, or was not, a contributing factor in the enrolment decision of high school students. The results showed that on one

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[4]See for an older review Jackson and Weathersby ([1975]).

[5]See for an overview of the required private contributions Jackson ([2003]). Between 1989 and 2003 these increased (as a result of price indexing) from A$1,800 to about A$2,800 dollars. Converted to US$ (historical rates) this was between US$2278 and US$4,315.

[6]See for an overview of these changes and the repayments rates per income category Jackson ([2003]).

occasion, 70% of respondents attached more weight to other factors than HECS as influence on their decision. In a second survey, performed by Ernst & Young in 1991, HECS was ranked 13th of 17 factors that might contribute to a negative enrolment decision. In addition, no relationship was found with socio-economic status, thus invalidating the argument that HECS would especially impact enrolment from students with a low socio-economic status. The latter point is also supported by a comparison of enrolment in 1988 and 1993, divided into high, medium and low family wealth by the Australian Council of Educational Research. Amongst others based on these results Chapman (1997) concludes that:

> 'The bottom line is that there is no evidence of HECS diminishing access to higher education of the disadvantaged, nor indeed, in an aggregate sense' (pp. 749).

The introduction of HECS in Australia did not go unnoticed in the Netherlands either, which resulted in the aforementioned study by Vossensteyn and Canton (2001), as part of a larger study by the CPB. Apart from a description of the HECS they also summarize several evaluation studies that were not covered by Chapman (1997). These studies (Andrews, 1999; Encel, 2000; Ramsay et al., 1998) provide further support for the previously drawn conclusions. In short, they look at the potential influence of debt averse behaviour by low socio-economic status students and in particular Encel (2000) looks at the effects of several government programs on participation of indigenous Australians. Lastly, a more recent study by Chapman and Ryan (2005) uses a quantitative methodological approach in which they consider participation data from three cohorts of Australians: 1988, 1993 and 1999. Their main focus is whether the relationship between socio-economic status and university participation changed over time. They estimate an expected value of university participation, conditional on socio-economic status. Based on these conditional mean functions and some additional analyses (e.g. Lorenz curves and Gini-coefficients), the conclusion is that, conditional on socio-economic status, a relative increase in participation in the middle of the wealth distribution occurred. Moreover, participation of females increased significantly, though this development was already visible in the 1990s (UNESCO, 2010).

3.1.2. Pell Grant - United States

Studies looking to determine the price elasticity of higher education through programs in the United States, such as the Pell Grant, show similar results. Hansen (1983) evaluates the effects of student aid on both greater access and greater persistence, as presumably both should improve. The sources that are used originate from the census bureau of the US, the annual and Current Population Survey from 1971-1972 and 1978-1980. The aim is to evaluate the introduction of the 'Basic Educational Opportunity Grant (BEOG) program' in 1972, which was later renamed the Pell Grant program[7].

A comparison of enrolment rates, split into races and socio-economic status, shows that 'greater availability of student financial aid [..] did little, if anything to increase access' (pp. 93). This is however solely based on the comparison of basic enrolment statistics. Thus, these conclusions should be interpreted with caution, as they by no means show a causal relationship. Kane (1995) corrects two weaknesses in the approach of Hansen (1983), as identified by McPherson and Schapiro (1991). First, the dataset is extended by increasing the number of years before and after the policy change. Secondly, males and females are pooled in the approach of Hansen (1983), which contaminates the results, as during those years the Vietnam war especially affected male enrolment. Besides, the methodological approach also differs in the sense that now a linear regression framework is employed. However, even after correcting for these weaknesses the results hold. Hence, again the results support the view that the introduction of the Pell Grant program did not significantly increase college enrolment for students with a low socio-economic status. Both Hansen (1983) and Kane (1995) provide a discussion of these results including possible explanations as to why their findings are unable to support theoretical expectations. It might for instance be the case that student aid was not sufficiently targeted to affect access. Moreover, both authors argue that it could be the case that the marginal student was simply not aware of the (process of applying for) the Pell Grant program. This would imply that the larger part of the aid went to students who would have enrolled in college without the support of a Pell Grant as well. A final possibility, which cannot be (in)validated, is that in absence of the Pell Grant program enrolment rates of students with a low socio-economic status would have decreased or increased at a lower rate. In other words, the

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[7]This program was established as a result of 1972 amendments to the Higher Education Act .

counterfactual is unknown.

There are some other signals based on evidence from the United States as well. Kane (2003) exploits several discontinuities in the eligibility formula for the Cal Grant program in California. These discontinuities are based on both a GPA threshold and an income threshold. The results suggest a positive relationship between student aid and enrolment, the latter increasing about 3 to 4%-points when a student is awarded the Cal Grant. Dynarski (2003, 2008) presents similar estimates. In the 2003 study, the identifying assumption is related to a student having a deceased parent. In 1981 the Social Security student benefit program was eliminated which provided a source of exogenous variation in student financing, as students with a deceased parent starting after 1982 were no longer eligible for additional aid. The estimation uses a dummy for 'deceased father', as in 90% of the cases students were entitled to benefits through their fathers. The identifying assumption implies that any relative shift in the attendance of the children of deceased fathers is attributable to the withdrawal of student benefits. Dynarski (2003) finds a strong negative effect of the elimination of the Social Security student benefit, with a decrease in probability of enrolment around 25%. When taking into account the size[8] of the benefits, this implies a response of about 3,6%-points per $1,000 dollars of student aid offered. Dynarski (2008) exploits the introduction of large scholarship programs in two Southern states (Arkansas and Gerogia) in order to identify the effect of college costs on college completion. In this empirical set-up, states that have not (yet) introduced merit programs constitute the control group. Several analyses and robustness checks show a significant positive effect of the merit programs on enrolment and persistence.

Several other relevant studies which will not be extensively discussed here are summarized by Deming and Dynarski (2009). They provide an overview of experimental and quasi-experimental evidence on the relationship between college costs and educational attainment[9]. In short, the most important conclusion being drawn is that program design matters; in particular, there appears to be an important trade-off between targeting and program effectiveness. The main argument here is that highly-targeted programs impose

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[8]According to Dynarski (2003) 'the average student benefit for the child of a deceased parent was about $6,700 in 1980, more than enough to cover the $1,900 cost of tuition fees at a public university.' (pp. 285)

[9]In particular the overview in Table 1 is recommended (pp. 24-27)

substantial paperwork burdens which decrease their effectiveness.

3.1.3. Further international evidence

Unfortunately, there is little evidence on price elasticity of higher education from other countries. Studies using data from the UK that are often mentioned are Barr (2004) and Dearden et al. (2011). Barr (2004) provides a theoretical discussion in which specific attention is paid to the UK system, but does not go into empirics. In contrast, Dearden et al. (2011) exploit several policy changes in higher education financing in the UK to estimate the causal impact of tuition fees and grants on participation. Robust evidence is found that a £1,000 increase in tuition fees reduces participation by 3.9%-points, while an £1,000 increase in maintenance grants increases participation by 2.6%-points.

Two other studies from respectively Germany and the Netherlands provide relevant evidence as well. First, Hübner (2012) examines whether tuition fees affect enrolment behaviour by means of a differences-in-difference framework, using states with and without tuition fees as treatment and control group. Based on this method he finds that the introduction of tuition fees reduced enrolment probabilities by 2.7%-points for every €1,000. However, it should be noted that there are some caveats to this analysis. For one, the author was unable to correct for a potential selection effect into states without tuition fees. Moreover, the analysis is unable to correct for differences in student benefits at the state level. Disregarding these issues for the time being, the results are in line with previous findings.

Secondly, Canton and de Jong (2005) investigate the role of economic factors in the university enrolment decision in the post-war (WWII) period in the Netherlands: 1950-1999. This study focuses on the demand side of the enrolment decision, motivated by the substantial increase in Dutch higher education participation over time (see figure 1). The focus of this study is the enrolment decision of the marginal student. They argue that it is crucial to understand that the 'marginal student' is a changing phenomenon. Whereas a marginal student might now be a student who is indecisive between enrolling in higher professional education or academic education, in the 1950s and 1960s this would have been a student from a low-income family (pp. 656). An error correction specification is applied, examining both short- and long-run effects and taking into account the stochastic trend problem. Other issues that emerged related to system changes and other policies influencing tuition

Figure 1: Participation in Dutch Higher Education: 1950 - 2013
Percentage of population 18-25
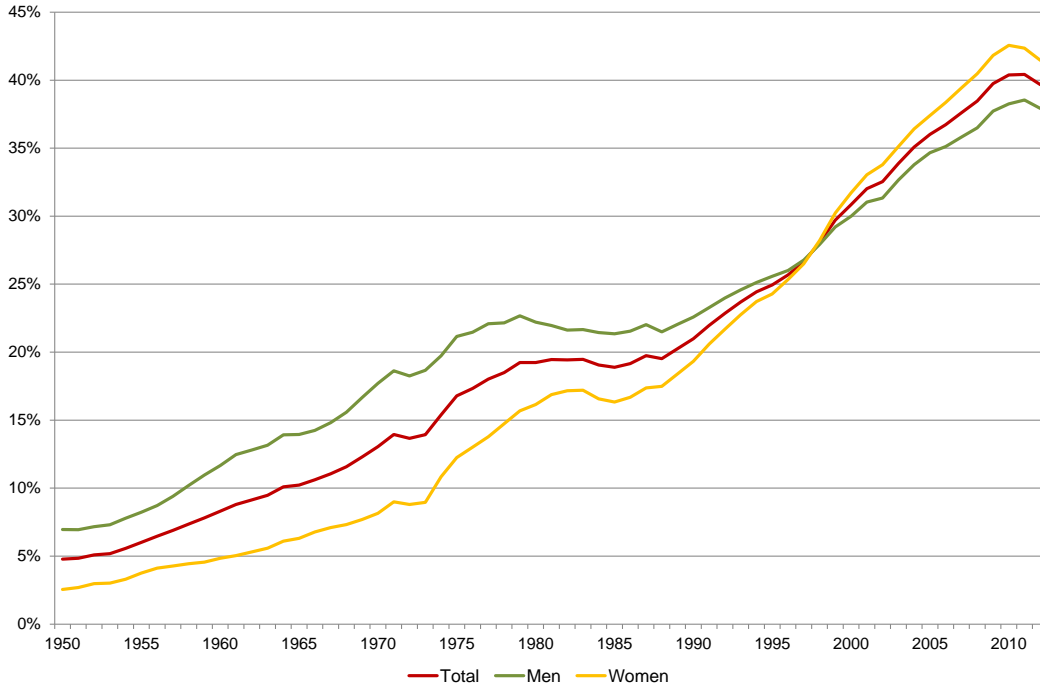
fees. Skipping towards the results, it is shown that the effect of tuition fees on the enrolment decision is weak, while simultaneously the positive effect of financial aid is striking (0.16-0.32). Hence, based on this study one would have no choice but to conclude that a change in tuition fees would not necessarily influence the enrolment decision of high-school students.

Summarizing, many studies have been carried out in order to determine the relationship between prices, subsidies and enrolment and persistence. In general, most reliable evidence is based on (quasi-)experimental research from Australia and the United States. It should be noted though that the majority of studies concerns the enrolment decision, and hence does not directly provide a price elasticity of the persistence decision. These studies find a consistent negative relationship with enrolment of $\pm$ 3 to 4%-points for every €1,000 change in tuition fees. These results are also supported by evidence from other countries, such as the UK and Germany. Studies that simultaneously examine the enrolment and persistence decision such as

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Deming and Dynarski (2009), Dynarski (2008), St. John and Starkey (1995) find a relationship for persistence as well, though often presented without similar specific and comparable estimates. A few studies that go into this effect in more detail will be discussed extensively in section 3.3.

3.2. Debt aversion

Debt aversion is a concept which this paper touched upon only slightly so far. The principle was explained in the introduction of section 3 and can easily be connected to several of the studies discussed above. For instance, van Elk et al. (2011) put forward a critical remark with respect to several of the studies that use programs in the US (such as Dynarski, 2003; Kane, 2003) as they examine increases in costs of higher education that are not accompanied by loan possibilities. Van Elk et al. (2011) argue that the real elasticity with respect to access will be significantly lower if loan possibilities are improved.

Callender and Jackson (2005) present an overview of the evidence on student debt aversion from the UK perspective. The primary consensus in the literature is that financial concerns play a major role in the enrolment decision. Secondly, there is consensus that, theoretically, it is to be expected that students with a low socio-economic status are more likely to be deterred by the costs of higher education than others. However, it is emphasized that the cost of studying is not necessarily the main reason that potential students do not enrol; it is one of many. Evidently, this point also applies to the discussion in this paper.

Focusing specifically on student debt, Callender and Jackson (2005) state that 'most large-scale quantitative studies treat debt in a superficial manner' (pp. 514). This conclusion is drawn mainly based on the simplistic way in which questions with respect to debt are posed to the participants. An example would be *'Are you worried about debt?'*. Since this question can only produce a dichotomous outcome it is not considered highly informative. The authors attempt to bridge this gap by extensively examining the relationship between student attitudes towards debt and the outcome of their enrolment decision. This is done by measuring two features: general debt aversion and specific cost/benefit judgement. Both were evaluated by means of multiple attitude statements. Furthermore, various background variables were included as controls, in order to for instance differentiate between different social groups and nationalities. The results of the analysis show

that debt aversion is a class issue. According to Callender and Jackson (2005) students from poorer backgrounds are more debt averse than those from other social classes. This is for instance clearly visible for those students studying at a lower level of education (mostly vocational). The findings therefore suggest that policies aimed at increasing participation should be targeted specifically at these social groups. In addition, the authors argue that policy makers might reconsider using negative incentives such as tuition fees and loans, as positive incentives such as grants have proven to attain better results for lower socio-economic groups (see for instance Heller, 1997; St. John and Starkey, 1995).

There are several other studies that support this point as well. For instance, Burdman (2005) argues that while a lot of attention has been given to the increasing debt level of students, less is known about students who are averse to borrowing. As student aid is increasingly dependent on loans, this could narrow their options. One of the striking features of her research is that many students do not perceive the availability of loans as student aid. Moreover, many students do not weigh costs and benefits of studying, and if they do they only employ a relatively short-term perspective.

Christie and Munro (2003) provide additional evidence which shows students are poorly informed about potential benefits of higher education. Moreover, it is argued that one should not underestimate the impact of the availability of cultural resources. Students who are unable to benefit from personal experience of family and friends perceive the investment in education as more risky, and they anticipate lower potential benefits compared to their peers. Hence, strong social class differences emerge with respect to the decision of taking out a loan to finance higher education.

More recent research by Callender and Jackson (2008), Cadena and Keys (2012), Avery and Turner (2012) and Rothstein and Rouse (2011) underscores these findings. Callender and Jackson (2008) for example show that students from low-income families are more likely to perceive the costs of higher education as debt than their wealthier peers. Besides, they might be more inclined to pick a university with relatively lower living costs in order to limit these costs.

To conclude, while the previous section has shown that students with a low socio-economic status do not necessarily have a different price elasticity for their enrolment decision, a difference does exist when focusing on perception of debt. Therefore, policy makers cannot simply think of a student as taking

perfectly rational decisions. It is clear that most students do not weigh costs and benefits of higher education, and those who do underestimate the long-term benefits. Moreover, students with a lower socio-economic status anticipate lower future benefits and higher costs compared to their peers, inducing them to be more reluctant in their enrolment decision. Lastly, it has become clear that primarily for students with a low socio-economic status indebtedness is to be avoided and perceived as a major burden. Relating this to the main topic of this paper, we arrive at the conclusion that not only the negative financial incentive might have influenced students' behaviour, but also the potential consequence of having to take out a loan in order to pay the increase in tuition fees.

3.3. Quasi-experimental studies

The previous paragraphs provided a detailed description of both price elasticity of higher education and debt averse behaviour. It was shown that price elasticity of higher education is relatively low, and contrary to many beliefs, there is little evidence that students with a lower socio-economic status respond stronger to price changes than their peers. A critical remark is that many of the reviewed studies deal with price increases without any accompanying improvements in loan possibilities. This does provide a relevant estimate for the price elasticity, but it could very well be that improvements in loan possibilities are able to reduce the significance of the demand response. Naturally, the latter point is of special interest to policy makers. Finally, there is substantial evidence that debt aversion is primarily a problem for students with a low socio-economic status. Thus, price increases that are accompanied by improved loan schemes could prove to induce a stronger demand decrease for students from low-income families, not necessarily because their price elasticity of demand differs from their peers, but because they exhibit debt averse behaviour.

This section will attempt to connect the notions above with the empirical approach that will be presented in section 4. One of the main goals is to examine the effects of policy changes on the persistence decision in a bit more detail. It will become clear why establishing the elasticity of within-year persistence has been so challenging for researchers. In order to do so several studies employing a similar methodological approach as the paper in hand will be reviewed. Specific attention will be paid to the methodological approach and concurrent issues, in combination with the approach chosen by the researchers to deal with them. In short, this section will evaluate

the credibility of several comparable studies and proceed towards the quasi-natural experiment that this study caries out.

Häkkinen and Uusitalo (2003) investigate the replacement of a loan-based student support system by grants in Finland, back in 1992. Accompanying this change was a reduction in the maximum duration of financial aid, effectively decreasing the duration with one year. The rationale behind this policy was to strengthen the incentive to study and to graduate earlier. Moreover, its aim was to motivate students to reduce time spent on their part-time jobs and use this time to increase study effort. This policy came about when a policy debate around median graduation time and age, respectively 27.5 and 6.5 years, sprouted in the early 1990s. These statistics were considered relatively high in an international context, motivating the formal target of the policy that 75% of students should graduate their master's degree within five years.

The authors use individual level panel data from Statistics Finland, ranging from 1987 to 1999. The sample consists of 350,000 individuals, for whom they have individual characteristics and graduation data which was recorded on a monthly basis. Unfortunately, data on enrolment or credits of individual courses was not available. Since the last observation date is August 2000, individuals who did not yet graduate at that point in time are censored[10]. In order to cope with censored observations, a duration model is applied. Section 4 will go into more detail as to why a duration model is more suited in such a situation. The authors show that median graduation time for the post 1992 cohorts was approximately three months shorter. When compared to the substantial decrease in funding, this is considered to be 'a modest effect on [...] graduation times' (pp. 14). Furthermore, since the effects were concentrated in the fields with a high average duration, it is argued that not the switch from loans to grants, but the decrease in aid duration had the largest impact. A critical remark to these findings is that unemployment among graduates increased in the same period as the study. This potentially distorts the picture as it seems plausible that due to increasing unemployment students decided to remain at university longer.

Evidence from Germany is provided by Heineck et al. (2006), who perform

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[10]This implies that the 'event of interest' from the perspective of the researcher, in this case graduation or drop-out, has not (yet) occurred at the end of the data collection.

a similar duration analysis. This paper is of special interest because the focus is on the introduction of tuition fees specifically for long term students. Since 1998 several German states have introduced such tuition fees, implying that students who were not finished within their nominal study duration plus four semesters (equal to 2 years) were obliged to pay approximately €511 per semester. This adds up to about €1,000 per year in tuition fees. Note that this policy bears close resemblance to the 'langstudeerdersmaatregel', the main difference being that the increase in tuition fees in Germany was only a third of the fees required by the 'langstudeerdersmaatregel'. The analysis focuses specifically at students at the University of Kostanz, for which the authors were able to obtain administrative data. These files contain data from 1985 to 2003, covering personal characteristics, enrolment, way of termination and the amount of tuition fees paid. The aim of the duration model that is employed is to analyse in which way the length of study was influenced by tuition fees. Five different possibilities of termination are distinguished: graduating, transferring, switching, dropping out and failing. Again, the choice for duration analysis is related to coping with right-censored data.

The identification strategy that is employed is to compare students who began their studies at different dates. This is crucial, as this implies an inability to fully control for other time-varying factors affecting control and treatment groups. The authors explain that a suitable control group was not available and this is the second-best option. The results of the analysis show that the effect of tuition fees is in accordance with expectations for all five ways of termination. This means that a positive relationship with graduation, transferring, dropping out and failing was found, and a negative relation with switching. Finally, there is one crucial issue, following from the cumulative incidence functions (Heineck et al., 2006, Figure 2, pp. 23). These functions display the long-run probabilities of obtaining a degree, divided into five different disciplines. They show that, in the long-run, the probability of obtaining a degree falls for all disciplines. Moreover, for Chemistry and Economics a significant decline is observed even in the short-run. Thus, while for half of the disciplines the desired effects are obtained, the reduction in average study length for the other half is solely the result of a surge in the number of drop-outs. The researchers are of the opinion that 'this can hardly be called a success' (pp. 29). However, eventually, it is up to politics to decide whether these 'costs' are acceptable. Likewise, the discussion by the authors clearly shows the trade-off of policies that increase tuition fees for students studying longer than the nominal term. It is emphasized that

one should keep in mind that there are two sides of the coin to the expected effects: on the one hand it stimulates students to increase effort, but on the other hand it also has the potential to increase the drop-out rate.

A study from Italy (Garibaldi et al., 2012) shows that a €1,000 increase in 'continuation' tuition fees (i.e. the persistence decision) reduces the probability of late graduation by 5.2%, conditional on a benchmark probability of 80%. A notable addition is that their results show no changes in student drop-out rate or average student performance. These results are based on an ingenious regression discontinuity design, exploiting the administrative structure of the Bocconi University in Milan[11]. This structure consists of twelve tuition levels, to which students are assigned based on their level of family income. A discontinuity design is then able to compare those students that were just below and above the assignment thresholds, the implicit assumption being that these are very similar in unobservable characteristics. Within this framework, students who are in their final year are the point of focus, exploiting the current tuition they are paying as a predictor for the future tuition they would be required to pay if they would continue the program. By now it should be straightforward that from the identification strategy it follows that tuition fees are generally seen as a sunk cost, but that tuition fees for the last year can be avoided by exerting extra effort. Recall that the results showed a significant decrease in probability of late graduation. Combining those results with the notion that, from a social welfare point of view, it is suboptimal for students to study longer than required, it is argued that raising 'continuation' tuition fees might be able to rebalance private incentives. Naturally, the goal of such a policy is to increase social welfare.

Lastly, one study in particular deserves some attention, as it has direct relevance for the Netherlands and it utilizes an interesting identification strategy. Belot et al. (2007) analyse a Dutch reform of the student aid system and its impact on the performance and time allocation of students. The analysed reform is a decrease of one year in the maximum duration of grants[12] from five to four years (60 to 48 months), the latter being equal to

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[11]Bocconi University is a private university, in a university system that consists for the majority of public universities. This is an indication that the students at Bocconi University might not be fully representative for all Italian students. One of the reasons is that regular tuition fees at Bocconi University (between €715 and €6,101) are relatively high when compared to public Italian universities.

[12]If expressed in currency, this reduces the total value of the grant by €684 to €4,385,

the nominal duration of a study program. The authors collected data from first-year students before and after the introduction of the reform by means of a questionnaire. These groups are then used as the control and treatment group. The most interesting aspect of the analysis comes in here, which is the implementation of a second difference. This difference is purely based on the set-up of the Dutch higher educational system, which consists of both higher professional education and academic education. These levels are relatively similar in terms of students, and are both affected by the reform. However, the authors argue that the expected effect of treatment is substantially stronger for students in academic education, as their average study duration is significantly higher[13]. Thus, even though both the treatment and the control group received treatment, it is still possible to obtain an estimate for the treatment effect. It should be noted, on forehand, that this estimate can only be interpreted as a lower-bound of the real treatment effect. As this also holds for the identification strategy that will be employed in the following section, we will return to this point there.

As introduced above, the authors estimate a difference-in-differences (DD) model, for which the final specification is presented below:

$$Y_i = \alpha + \beta X_i + \delta Year_i + \lambda Uni_i + \gamma Year_i \times Uni_i + \epsilon_i \qquad (1)$$

It should be relatively straightforward to see that equation (1) depicts the standard DD specification, with α as the constant, X_i as a vector of individual characteristics (control variables), $Year_i$ and Uni_i respectively as dummies for time and higher education level (recall: higher professional education and academic education), and of course the interaction term between these two plus an error term.

Estimation of the model is performed by the implementation of a multinomial logit-model with three options: continue the program, switch or drop-out. The results show that switching behaviour decreased significantly in academic education after the reform, whilst for dropping out no effect was found. Focusing on grade point average (GPA) in particular, a significant increase was established in academic education, compared to no increase in

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depending on whether a student lived at home and whether he was eligible for the maximum amount of supplementary grant.

[13]In higher professional education the average study duration was equal to 54 months at the time, whereas students in academic education on average needed 66 months to complete their studies.

higher professional education. The second aspect, time allocation, shows a less convincing picture, since none of the DD-estimates are significant. In consequence, one can conclude that students did not invest more time in their studies relative to time spent working. This is a striking conclusion, as we already found that performance did increase. The authors argue that this might be a consequence of self-reporting bias, referring to two other studies (Kremer et al., 2009; Leuven et al., 2010) which arrive at a similar peculiar conclusion.

In conclusion, this paragraph reviewed several studies that employ sophisticated empirical strategies in order to improve our understanding of the persistence decision, focusing either on the positive or negative outcome (drop-out) of this decision. Evidence from Finland, Germany, Italy, the United States and the Netherlands was discussed, and the overall picture is relatively similar. Whereas Häkkinen and Uusitalo (2003) find a modest effect on graduation times, Heineck et al. (2006) conclude that there was a decrease in average time to graduation but that this was driven by a substantial increase in drop-outs. Stronger positive evidence is provided by Garibaldi et al. (2012), who have shown that an increase in 'continuation' tuition fees of €1,000 can reduce probability of late graduation by 5.2%, notably without any changes to drop-out levels. DesJardins et al. (1999) and Chen (2008) show that financial factors can reduce the risk of stop-out, but not necessarily simultaneously the risk of drop-out. Moreover, they establish that evidence on the positive effects of grants is relative convincing, while the effects of loans are less clear. Finally, Belot et al. (2007) show that a decrease of one year in grant duration in the Netherlands positively affected student performance, while at the same time students did not invest more time in their studies. Hence, the overall picture is that a policy change that shifts part of the financial burden of higher education from the government to the students has the ability to positively affect student performance. However, it should be noted that one needs to be cautious while measuring the success of such a measure, as it provides an incentive for increasing effort but can also lead to a surge in drop-outs. Hence, a single statistic of interest such as 'average time to graduation' can present a very distorted picture of reality, or at the least conceal the processes that drive it.

4. Empirical strategy

The final note of the literature review that a single statistic of interest can conceal processes that drive it is an important remark, even more because

it is also applicable to the empirical strategy that this section will discuss: the focus of the empirical strategy that this paper employs is also time to graduation.

The 'langstudeerdersmaatregel' came into force in September 2012. This implies that all students who were in or above the *fifth* year of their bachelor's program in study year 2012/2013 were required to pay the fine. The anticipation effects for students should hence be expected to have occurred in the year before. The hypothesis is therefore that students in or above the *fourth* year of their bachelor's program in study year 2011/2012 show a significant increase of effort and results.

For sake of comparison between universities only bachelor's programs are considered in the analysis of this paper. The fine is also applicable to master's programs, but comparing master's programs is more difficult as there is more variation in their duration (e.g. 1, 1.5 or 2 years) and their curriculum. If students are for example expected to do an internship this can affect their chance to graduate compared to students who are not expected to do so. Moreover, it could imply they are only able to graduate in a specific month of the year based on the constraints introduced by the time schedule of the internship. Therefore, a comparison of students in their bachelor's programs is more reliable which positively affects the external validity of the results.

In order to evaluate the effects of the policy measure simply monitoring graduation rates will not provide correct results, because an upward trend in average time to graduation exists. In order to cope with this trend, an exogenous source of variation is identified so that a Difference-in-Differences (DD) set-up can be employed. This exogenous source of variation is a second policy measure (Bachelor-before-Master act) that was implemented by the Dutch government, which came into force at the exact same time (i.e. September 2012).

4.1. Bachelor-before-Master act

The 'Bachelor-before-Master act (in Dutch 'harde knip') states that students are required to be in possession of a bachelor's degree before enrolling in a master's program. After 2002, when the bachelor-master division was implemented in the Netherlands, most universities had regulations in place which defined when students were allowed to enter a master's program. Generally, this would imply that students who had obtained 80-90% of the

credits in a relevant bachelor's program (at the same university) were allowed to follow courses from the master's program. The prime argument for such policies was that students who would be able to pass the master's courses should not be prohibited from taking them solely because they did not pass a few courses from their bachelor's program. From study year 2012/2013 onwards however, universities were no longer allowed to admit students without a bachelor's degree to a master's program.

The link between the Bachelor-before-Master act and the 'langstudeerdersmaatregel' originates from some exceptions to the Bachelor-before-Master act. Specifically, three universities had implemented the policy before the law passed, and three other universities requested a temporary postponement, which was granted. Table 2 shows the date (year) of introduction per university. These exceptions imply that the Bachelor-before-Master act be used as a source of exogenous variation in order to evaluate the effects of the 'langstudeerdersmaatregel'. Specifically, students enrolled at universities that had implemented the Bachelor-before-Master act before this was required by law can be compared with students studying at one of the three universities that were granted a temporary exemption.

Table 2: Introduction Bachelor-before-Master act per university

Institution	Year of introduction
University of Utrecht	2002
University of Amsterdam	2009
Delft University of Technology	2010
Leiden University	2012
Eindhoven University of Technology	2012
University of Groningen	2012
Maastricht University	2012
Radboud University Nijmegen	2012
University of Twente	2012
Erasmus University Rotterdam	2012
VU University Amsterdam	Postponed (exemption for 1 year)
Tilburg University	Postponed (exemption for 1 year)
Wageningen University	Postponed (exemption for 1 year)

Crucial in this set-up is that, from the perspective of students, the main

point of pressure of *both* policies lies in those last months of their third, or fourth year of their bachelor's program (and so on). The rationale behind it is that students studying at a university that implemented the Bachelor-before-Master act on its own initiative are already accustomed to policies focused on reducing their study duration, whereas students studying at one of the universities that were granted a postponement are not. In their case the 'langstudeerdersmaatregel' is the first policy that exerts substantial pressure to finish one's bachelor's program within the academic year.

The difference between these two groups will be exploited in the analysis, with students studying at universities that introduced the Bachelor-before-Master act on their own initiative as the control group, and students studying at the universities that were granted an exemption as the treatment group. An important note here is that the assumption is not that the control group is *not* affected by the 'langstudeerdersmaatregel'. The assumption is that they exert a common trend upfront, but that the treatment effect is stronger for the treatment group than for the control group.

Figure 2: Fictional DD

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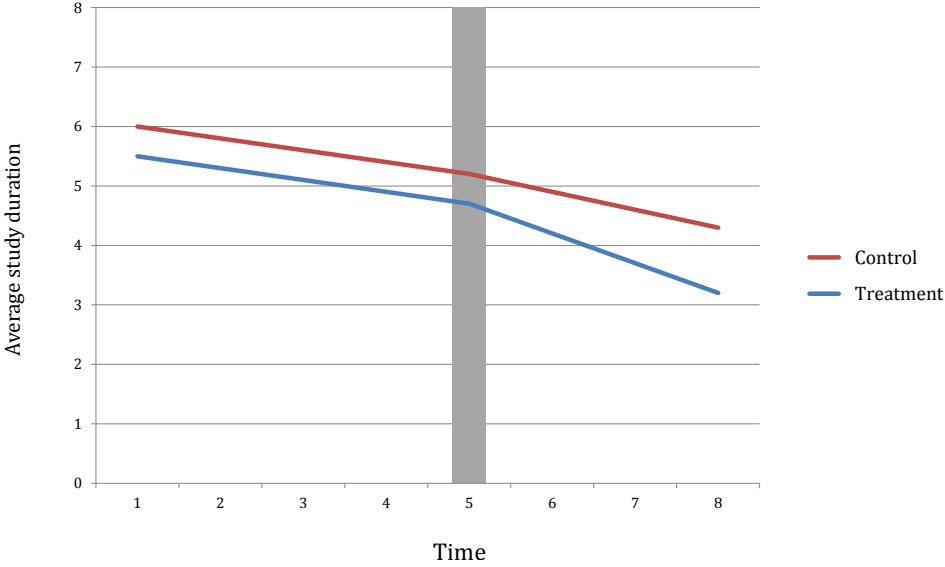
A graphical illustration of this assumption is shown in figure 2 [14]. The existence of a downward trend in average study duration is known, hence a slight downward trend is used for the first years here as well (time 1-4). Then, at time equal to 5, treatment occurs. Next, we observe a kink in both the control and treatment curves, the effect for the treatment curve being more substantial, which is also illustrated by the growing distance between both curves. This is exactly the effect we expect to find for the 'langstudeerdersmaatregel': a downward pressure on all students, but a stronger effect for students studying at a university that has not introduced a variant of the Bachelor-before-Master act on its own initiative.

4.2. Comparability between universities

In order to compare students between universities in a reliable way, the various study programs offered by the six universities that either introduced the Bachelor-before-Master act before or after 2012 were analysed. Based on this analysis the sample was restricted in two ways.

Table 3: Control and treatment universities

Institution	Year of introduction	Control / Treatment
University of Utrecht	2002	Control
University of Amsterdam	2009	Control
VU University Amsterdam	Postponed (exemption for 1 year)	Treatment
Tilburg University	Postponed (exemption for 1 year)	Treatment

First, Delft University and Wageningen University were excluded from the analysis. These universities have a unique profile which limits the comparability. As a result, four universities will be included in the analysis, divided into a control and treatment group as shown by table 3.

Second, analysis is restricted to study programs that all four relevant universities offer. Based on this condition the following four clusters of study programs were selected:

1. Economics

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[14]Note that this graph is only intended as clarification of the strategy, and as such does not depict actual educational data.

2. Law (Dutch)
3. Psychology
4. Biomedical sciences

4.3. DD-setup

The first difference of the DD-setup should now be clear: students from two treatment universities will be compared to students from two control universities. In both cases only bachelor's students from four selected clusters of study programs will be considered.

The second difference is the effect over time. This will be implemented through a comparison of results of two cohorts: a cohort that did experience treatment and a cohort that did not. For example, consider the cohort of fourth year students in study year 2011/2012. The hypothesis is that a higher percentage of students of this cohort will graduate at the end of the year, compared to earlier cohorts. The best way to implement this comparison is to consider the chance of graduating for the previous cohort as well, which consists of students that were at the exact same point of their studies one year earlier. In this case, that implies taking into account the outcomes of the cohort of fourth year students in study year 2010/2011.

The DD-setup is summarized in Table 4, considering the example of fourth year students. It shows four different categories of students which will be compared to one another. First, the rows of the table show the different cohorts in time: before and after the policy change. Second, the columns of the table split the cohorts into treatment and control universities. Thus, four different categories are identified.

Table 4: Example: *fourth* year students DD

	Treatment Universities	Control Universities
After policy change	4th year in 2011/2012 - started 2007/2008	4th year in 2011/2012 - started 2007/2008
Before policy change	4th year in 2010/2011 - started 2006/2007	4th year in 2010/2011 - started 2006/2007

The DD-setup will be estimated by means of a standard DD-specification, as specified in equation (2) below. As the equation shows, the dependent variable is a dummy indicating whether the student graduated in year 4 of his or her studies or not. Since only students already in their fourth year are

considered, a value of 0 for this dummy implies that students either needed longer to graduate their bachelor's, or dropped out in or after their fourth year. The α term is the intercept. βX_i is a vector of control variables, and $\gamma Pre\ Post_i$ and $\delta\ Treatment\ University_i$ respectively depict whether the student is in the pre or post policy change cohort and whether the student is studying at one of the control or treatment universities. The interaction of both these terms, for which η is the estimator, captures the DD effects of interest. ϵ_i is the error term.

$$
\begin{aligned}
Graduated\ after\ 4\ years_i = {} & \alpha + \beta X_i \\
& + \gamma\ Pre\ Post_i + \delta\ Treatment\ University_i \\
& + \eta\ (Pre\ Post_i * Treatment\ University_i) + \epsilon_i \quad (2)
\end{aligned}
$$

The next section will explain the dataset that is used in order to estimate equation (2).

5. Data

5.1. Dataset and data collection DUO

The dataset that is used to estimate equation (2) was provided by DUO (Dutch: Dienst Uitvoering Onderwijs), the organisation responsible for providing information services and payments of student aid to students in the Netherlands. A selection of variables from their national administrative dataset was provided with data spanning from 2002/2003 to 2012/2013.

Data collection relevant for the provided dataset is done on a yearly basis by DUO. Every year on the 1st of October, one month after the start of the academic year, a snapshot is taken of the student population at that moment. This implies that a student's enrolment status at the 1st of October defines his or her enrolment for that academic year. The national administrative dataset of DUO hence consists of yearly records per student. These records show whether a student is still enrolled, at which university the student is enrolled and which study program and whether the student is enrolled as full-time or part-time student. In contrast, graduation data is collected on a continuous basis.

The subset that DUO provided for the analysis of this paper unfortunately does *not* contain yearly records per student. This subset is build up as a micro dataset with data per student, not varying over time. The subset consists of the following records per student:

Enrolment data:

- Year of enrolment: first and last
- University of enrolment: first and last
- Study program at enrolment: first and last
- Full/Part-time at enrolment: first and last

Graduation data:

- Year of first degree: Bachelor's / Higher professional / Master's
- University of first degree: Bachelor's / Higher professional / Master's
- Study program of first degree: Bachelor's / Higher professional / Master's

Background information:

- Gender
- Age 31/12 year of first enrolment
- Ethnicity
- Secondary school curriculum
- Average grade secondary education

Thus, the dataset shows us when a student first enrolled in higher education, and the university and study program at that time. Moreover, it shows the last year of enrolment in higher education for that student, and again the relevant university and study program. From these values total time spent in higher education can be deducted[15]. It is also known when the student graduated his first bachelor's, higher professional and master's degree. From this total time to graduation was deducted. Lastly, based on the values at 'Last enrolment' we know whether a student switched to higher professional education, or a different study program, when he or she did not finish the original study program yet.

An example might clarify this. Let's say student X enrolled in 2006 (= study year 2006/2007) at the University of Utrecht and started studying Economics full-time. The dataset then holds these four values as the first

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[15]It is not known whether a student might have been *un*enrolled in the meantime. However, temporarily unenrolling is very unusual in the context of Dutch higher education, for instance because that implies losing rights to student aid. Therefore, in the analysis students are assumed to remain enrolled from their first year of enrolment to their last year of enrolment

four entries for student X (enrolment data). Secondly, let's assume student X graduates his bachelor's degree in his fourth year, hence in study year 2009/2010 and the university and study program did not change. Those are the following three entries (graduation data).

If we now assume student X enrolled in a master's program afterwards, but dropped out in the first year, the final entries become clear. First, student X's last year of enrolment is now 2010/2011. Second, no data is registered for his 'Year of first master's degree' since he dropped out. The master's program he enrolled in is registered though, as 'Last study program of enrolment', just like the university is registered as 'Last university of enrolment'.

To summarize, we have data on the first moment of enrolment in higher education, data on graduation and data on the last enrolment in higher education. Based on these data points several variables were created containing information on total time to graduation, whether a student switched study programs, switched universities or dropped out. In this fashion the dependent variable, *Graduated after 4 years* was also deducted.

Finally, note that the data only considers students who enrolled in higher education directly after graduating from secondary education; hence, students who enjoyed a gap year have not been included in the analysis[16]. Students who enrolled in higher professional education[17] were also filtered out as the analysis in this study focuses on academic education only. Therefore, the variable depicting data on higher professional degrees attained only shows students who switched from academic education to higher professional education.

5.2. Factors affecting chance to study over n+1

A first valuable insight into the data focuses on the students studying longer than n+1, specifically on their background characteristics. The literature section has mentioned a few characteristics that are known to affect the time it takes one to finish his or her studies. Table 16 in the appendix provides a

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[16]This decision is based on advise of data experts of DUO. Students that do not enrol in higher education directly after secondary education differ significantly from the average student. Given the fact that the final analysis of this study uses a relatively reduced subset this group would have had the potential to influence the results strongly.

[17]This consisted of roughly 50,000 enrolments

Table 5: Logit regression: Background characteristics - Student n+1

	(1) Student over n+1
Gender	-0.587***
	(-41.38)
Age 31/12 year of enrolment	0.120***
	(12.50)
Average grade secondary education	-0.0977***
	(-74.69)
Ethnicity	0.247***
	(14.62)
Constant	4.164***
	(17.70)
Controls	
Year of enrolment	Yes
Secondary education level	Yes
Institution	Yes
Study program	Yes
Pseudo R^2	0.21
Observations	153,810

t statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

more extensive overview on the covariates of relevance here.

Table 5 shows the results of regressing several of these background characteristics in our data on a dummy capturing whether the student takes more than four years, or four years or less to finish their bachelor's. Note that this model uses 153,810 observations; this model is estimated for the whole dataset, thus including all universities and study programs. From the next subsection onwards only the subset for the DD-specification will be used, hence sampling the four selected universities and study programs.

In agreement with the literature we find that male students[18] are more likely to study longer than the nominal duration + 1 and are hence affected more by the introduction of increased tuition fees than female students.

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[18]Gender - Male = 0; Female = 1

Similarly, older students (at start of their studies) and students from foreign descent (ethnicity $= 1$) are more likely to take longer, whilst a high average grade in secondary education affects the probability of studying longer negatively.

5.3. Characteristics DD-dataset

The subset relevant for the DD-setup contains 31,018 students. It spans from 2002/2003 to 2012/2013. Enrolments are divided by year and treatment or control university as shown in table 6. Overall enrolments at University of Utrecht and University of Amsterdam, the control universities, are slightly higher than at treatment universities VU University Amsterdam and University of Tilburg.

Table 6: Enrolment by year

| Year of enrolment | University | |
	Control	Treatment
2002	816	1,004
2003	1,276	1,124
2004	1,621	1,093
2005	1,612	1,133
2006	1,757	1,164
2007	1,733	1,168
2008	1,650	1,379
2009	1,937	1,400
2010	1,803	1,389
2011	1,800	1,355
2012	1,655	1,149
Total	17,660	13,358

Summary statistics for the subset are shown in table 7 below. It becomes clear that 56% of students in the subset are female, average age at 31/12 of first year of enrolment is 18.44 and the average grade with which the students finished secondary education amounts to 67.81 (6.8 in common notation). Moreover, the youngest student in our dataset was 16 when he or she enrolled, while the oldest was 23. Ethnicity is reported by DUO in multiple categories, but compiled into a dummy in this paper. Table 7 shows that 20% of the

Table 7: Summary statistics of relevant subset

Variable	Obs	Mean	Std. Dev.	Min	Max
Gender	31,018	.56	.50	0	1
Age 31/12 year of enrolment	31,018	18.44	.64	16	23
Average grade secondary education	31,000	67.81	5.04	57	91
Ethnicity	31,018	.20	.40	0	1
Year of enrolment	31,018	2007.34	3.03	2002	2012
1st HBO B Degree - Year obtained	1,347	2008.97	1.67	2004	2012
1st WO B Degree - Year obtained	14,374	2008.35	2.06	2004	2012
1st WO M Degree - Year obtained	9,167	2009.04	1.70	2005	2012
Year of last enrolment	31,018	2010.90	1.75	2002	2012

Note: HBO = Higher Professional Education, WO = Academic Education

students in the subset have a non-western background, counting both first and second generation. 1,347 students who originally started a bachelor's program in academic higher education eventually obtained a degree from higher professional education (HBO in Dutch). Out of 31,018 students 14,374 obtained a bachelor's degree, and 9,167 even obtained a master's degree during the time period the data spans. Data on the last year of enrolment is provided for every student as well. The dataset and analysis therefore do not suffer from incomplete information. Finally, not included in the table, the dataset contains information on university and study program, which can both take the four known values. See table 3 for the universities, the study programs are listed just below there.

The main DD-specification uses a single pre and post cohort. Fourth, fifth and sixth year students will be compared in the exact same way. Hence, the DD-specification in equation (2) is exactly similar for fifth and sixth year students. Table 8 shows the summary statistics for this comparison. Note that in total 858 students in year 4 are used, whilst for year 5 and 6 respectively 314 and 171 students are in the sample. The number of observations for sixth year bachelor's students is particularly low, which was to be expected since not many students follow a bachelor's program for 6 years. The analysis will be extended to include extra pre cohorts (for year 4, 5 and 6) as a robustness check in the next section.

Table 8: Summary statistics baseline specification

| | Year 4 | | Year 5 | | Year 6 | |
	Pre	Post	Pre	Post	Pre	Post
Observations	447	411	147	167	111	60
Graduated after year 4	273	279	83	96	43	27
Percentage	61.1	67.9	56.5	57.5	38.7	45.0
University (%)						
University of Utrecht	28.2	23.8	23.8	22.2	17.1	20.0
University of Amsterdam	28.0	24.8	27.2	26.9	28.8	26.7
VU University of Amsterdam	22.1	24.8	25.2	27.5	24.3	26.7
University of Tilburg	21.7	26.5	23.8	23.4	29.7	26.7
Study Program (%)						
Economics	31.3	34.8	28.6	31.7	27.9	30.0
Law (Dutch)	35.8	31.6	33.3	35.9	51.4	28.3
Psychology	25.5	23.4	27.9	24.0	17.1	26.7
Biomedical Sciences	7.4	10.2	10.2	8.4	3.6	15.0
Background information						
Gender (% female)	47.7	43.8	42.9	41.3	44.1	35.0
Age (average in years)	18.5	18.5	18.6	18.6	18.6	18.8
Ethnicity (% non-western)	26.6	29.4	27.2	32.9	42.3	31.7
Average grade secondary education	6.8	6.7	6.6	6.7	6.6	6.5

6. Results

The previous sections presented the empirical strategy and the dataset that will be employed. A DD-setup will be estimated with data provided by DUO. This section will first present the main results from the specification as defined by equation (2). This specification compares a single cohort before the policy change with a single cohort after the policy change, in both cases controlling for the effects of university and study program. Afterwards, two robustness checks will be presented. First, the dataset is extended to include three pre cohorts instead of one. Second, the strict definition of students admissible for the analysis is softened. More specifically, the constraint on staying in the same study program is relaxed. Hence, the second robustness check includes students that are in the fourth (or fifth/sixth) year of their bachelor's program, but no longer in the same study program (or university) as they originally enrolled in.

6.1. Main results

Table 9 shows the results from equation (2) for fourth, fifth and sixth year students. Recall that the dependent variable is a dummy indicating whether a student who is in his or her fourth year graduated that year or not. The variable of interest, the DD-estimator, shows a negative and insignificant coefficient for year 4. Based on the summary statistics in table 12 in the appendix this is not a illogical result: the percentage of students graduated increased stronger for the control group than the treatment group, exactly opposed to our hypothesis. When we consider year 5 however, a positive effect is found, with a coefficient of 0.259 and significant at the 5%-level. This implies that an extra 25.9% of fifth year students of the treatment universities graduated compared to the control universities, controlling for the trend over time. The results for year 6 students however are again insignificant.

Almost all covariates have the expected sign. First, gender has a positive effect. This was expected as female students are known to graduate earlier. Second, age at 31st of December in the year of enrolment has a small negative coefficient, significant for fifth year students. The negative sign shows that students who are older when they enrol for the first time have a lower chance to graduate. The third covariate is interesting. For year 4 it shows the sign we would expect, a positive and significant effect since higher grades signal higher ability and hence better odds to graduate. For year 5 and 6 this covariate takes a small negative sign though, but it is also no longer

Table 9: Results baseline specification (single pre and post cohort)

	(1) Year 4	(2) Year 5	(3) Year 6
Gender	0.070**	0.136**	0.058
	(1.97)	(2.14)	(0.64)
Age 31/12 year of enrolment	-0.024	-0.138***	-0.052
	(-0.99)	(-3.38)	(-0.86)
Average grade secondary education	0.007*	-0.007	-0.007
	(1.88)	(-1.05)	(-0.60)
Ethnicity	-0.072*	-0.112*	0.055
	(-1.90)	(-1.75)	(0.64)
Treatment university	-0.162***	-0.167	-0.173
	(-2.84)	(-1.62)	(-1.29)
Pre/post policy	0.104**	-0.099	0.099
	(2.34)	(-1.27)	(0.82)
Interaction DD	-0.030	0.259**	-0.033
	(-0.47)	(2.35)	(-0.20)
Constant	0.635	3.611***	1.810
	(1.16)	(3.76)	(1.25)
Controls			
University	Yes	Yes	Yes
Study program	Yes	Yes	Yes
Observations	858	314	171
R^2	0.059	0.091	0.069

t statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

significant. One could hypothesize that other factors than ability or previous grades become relatively more important when one already takes 5 or 6 years to finish a bachelor's degree. The fourth and last covariate is a dummy for ethnicity, taking the value of 1 if a student is from a non-western background (first and second generation). Again, for year 4 and 5 the expected result is found: a negative significant effect. For more information on why these are the expected results, see table 16 in the appendix.

The question that remains is how to interpret these results. Is is true that the policy measure induced extra effort for fifth year students but not for fourth and sixth year students? In order to go into that question two

robustness checks are performed to see whether the results hold when two restrictions on our sample are relaxed.

6.2. Robustness checks

Two robustness checks will be presented. First, the data will be extended to include three pre cohorts instead of one. Second, students who started in the same year but switched study programs are included in the analysis, thereby increasing the number of observations in both the pre and post cohort.

Including three pre cohorts implies controlling for the trend over time in a more reliable way, as four years of data are covered. Table 13 in the appendix shows the summary statistics when three pre cohorts are included. Obviously, the statistics on the post cohort do not change. From this table follows that the number of observations from pre cohorts in year 4, 5 and 6 are now respectively 1,339, 594 and 298. In the main analysis with a single pre cohort these numbers were 447, 147 and 111 (see table 8). Aside from these changes the data on the pre cohorts is quite comparable to the data on the single pre cohort. One difference does stand out, which is the percentage of fifth year students obtaining a degree in that year. Considering a single pre cohort this was 56.45%. After including two extra pre cohorts this percentage is reduced to 50.17%.

Table 10 shows the results of the first robustness check. The first three columns, with a gray background, show the baseline results as presented previously in table 9. Columns four, five and six show the results with multiple pre cohorts, hence the titles 'Y4 (single)' and 'Y4 (multi)'. Before we look at the results, notice that an extra control variable was added. Since multiple pre cohorts are considered now, 'Year of enrolment' was added as an extra covariate. In the comparison of a single pre and post cohort this variable would be exactly equal to the pre/post policy dummy and hence be omitted. Notice as well that, indeed, the number of observations in the comparison with multiple pre cohorts is substantially higher than in the first three columns.

The signs and size of the covariates are very similar to one another, hence those will not be discussed separately. The most relevant values are of course the estimates for the DD-estimators. If we look at year 4 first, we see a slightly lower coefficient which remains insignificant. The picture is very similar for years 5 and 6. The coefficients did change, especially for year 6 as that is positive now, but it remains insignificant. The main results are

Table 10: Results robustness check 1 (baseline specification plus 2 pre cohorts)

	(1) Y4 (multi)	(2) Y5 (multi)	(3) Y6 (multi)
Gender	0.084***	0.099**	0.070
	(3.34)	(2.48)	(1.16)
Age 31/12 year of enrolment	-0.030*	-0.093***	-0.051
	(-1.77)	(-3.43)	(-1.31)
Average grade secondary education	0.007***	-0.005	-0.006
	(2.69)	(-1.09)	(-0.94)
Ethnicity	-0.078***	-0.129***	-0.042
	(-2.90)	(-3.19)	(-0.72)
Treatment university	-0.177***	-0.107*	-0.203**
	(-4.85)	(-1.81)	(-2.43)
Pre/post policy	0.107**	-0.047	0.101
	(2.17)		
Interaction DD 4	-0.036		
	(-0.68)		
Interaction DD 5		0.240***	
		(2.78)	
Interaction DD 6			0.017
			(0.12)
Constant	3.327	-11.923	40.186
	(0.11)	(-0.24)	(0.54)
Controls			
University	Yes	Yes	Yes
Study program	Yes	Yes	Yes
Year of enrolment	Yes	Yes	Yes
Observations	1749	761	358
R^2	0.048	0.058	0.056

t statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

thus backed up when two pre cohorts are added. Again a significant and positive effect is found for fifth year students, but no significant effects for fourth and sixth year students.

The second robustness check requires a short explanation on forehand. As described earlier this check includes students that switched study programs. The original analysis only considered students who stayed enrolled in the same study program as they originally started at that university. Since we have data on graduation and on where a student was enrolled last, it is also known whether a student stayed enrolled at the same university and continued studying the same study program or not.

Including students who switched universities or study programs is particularly interesting because they receive exactly the same treatment as students who did not switch. Recall that the 'langstudeerdersmaatregel' is solely based on the number of years a student is enrolled. However, a crucial difference here is that these students are most likely not at the same point in their studies as students who did not switch. After all, switching to another study program almost always causes delays.

Table 14 in the appendix shows the summary statistics when students who switched are taken into consideration as well. Just like with the first robustness check the number of observations changes substantially. This time however, the other values undergo changes as well. The percentage of students that graduated within the relevant year for example drops to values between 25% and 50%. This clearly shows that the sample has been expanded to include students who have not spent four years within the same study program yet, and are hence further away from graduating. Seeing that these students are added to both the pre and post cohort, this should prove to be a useful robustness check.

The results of the second robustness check are presented in table 11. Again, the first three columns show the baseline results as presented previously in table 9. Columns four, five and six show the results of the specification which includes students that switched. Notice that the number of observations more than doubled, with especially strong increases in year 5 and 6. The reason for these strong increases is that students are evaluated at a specific point in time within their studies, measured from their first year of enrolment. Students who switched study programs or university often start a new program, thereby increasing the odds that they take five or six years to finish their first bachelor's degree. Students who did not switch however, are likely

Table 11: Results robustness check 2 (baseline specification plus switching)

	(1) Y4 (switch)	(2) Y5 (switch)	(3) Y6 (switch)
Gender	0.087***	0.100***	0.035
	(3.95)	(3.23)	(0.92)
Age 31/12 year of enrolment	-0.032**	-0.050***	-0.021
	(-2.14)	(-2.58)	(-0.91)
Average grade secondary education	0.016***	0.002	0.004
	(6.73)	(0.58)	(0.90)
Ethnicity	-0.038	-0.093***	0.080**
	(-1.63)	(-2.98)	(2.17)
Treatment university	-0.119***	-0.022	-0.051
	(-3.23)	(-0.42)	(-0.85)
Pre/post policy	0.061**	0.006	-0.007
	(2.38)	(0.18)	(-0.18)
Interaction DD 4	-0.005		
	(-0.13)		
Interaction DD 5		0.041	
		(0.73)	
Interaction DD 6			0.035
			(0.51)
Controls			
University	Yes	Yes	Yes
Study program	Yes	Yes	Yes
Constant	0.021	1.230***	0.381
	(0.06)	(2.68)	(0.70)
Observations	2355	1236	716
R^2	0.059	0.037	0.035

t statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

to graduate earlier in comparison to students who did switch. This explains the substantial increase in observations: many students that switched are simply not yet finished in their fifth or sixth year, but many non-switching students have graduated already[19].

Looking at the estimates for the DD-estimator the picture is again similar to the baseline results, with one important difference: the coefficient for the DD-estimator in year 5 has dropped and is no longer significant. Thus, none of the DD-estimators show a significant effect in this specification.

In conclusion, the first robustness check, extending the dataset, backed up the results provided by the baseline specification. A substantial and positive effect was found for fifth year students, indicating that significantly more fifth year students graduated in 2011/2012 than was to be expected based on the existing trend over time. The second robustness check, including students who switched either study programs, universities or both, was unable to back up those results and showed no significant effects.

The DD-setup controlled for the effects of the introduction of the 'Bachelor-before-Master act' through comparing four selected universities. The effects that were found should therefore be attributable to the introduction of the 'langstudeerdersmaatregel'. No significant effects were found for fourth and sixth year students, not by the baseline specification nor by the robustness checks.

7. Discussion

This study has analysed the effects of the 'langstudeerdersmaatregel', a policy measure that introduced increased tuition fees for students studying longer than the nominal duration plus one year. The policy measure came legally into force in 2011, but after being postponed for one year the increased tuition fees were first due in September 2012. The preliminary coalition agreement that was announced only a month later meant the end of the policy measure. The 'langstudeerdersmaatregel' was abolished and all payments that had already been settled were reimbursed.

Behavioural effects however were not as easily reversed. The hypothesis that this study posited is that, even though the policy measure was only

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[19]Analysis of the dataset shows that in total 38.8% of students switched study programs or universities in their bachelor's program.

into force for a month, it still exerted a significant influence on students. We have analysed whether students have increased their effort in the year before the increased tuition fees were due (2011-2012).

It has long been known that a downward trend in average time to graduation is present in the Dutch higher educational system. This implies that effects of the policy measure could not be established through monitoring graduation rates. In order to cope with this trend, this paper has made use of a quasi-natural experiment and employed a DD-setup building on exogenous variation rendered by a different policy measure that was implemented in the same year: the Bachelor-before-Master act. Based on variation in the implementation dates of this act four universities were assigned to a control and treatment group. Students studying at a university that implemented the Bachelor-before-Master act on its own initiative are assumed to be accustomed to policies focused on reducing their study duration, whereas students studying at one of the universities that were granted a postponement are not. Crucial here is that both measures apply pressure in the final months of the study year.

Based on comparability of study programs only bachelor's programs in academic education were considered. Four clusters of study programs were selected to warrant comparability as they are offered at all four universities. Furthermore, analysis was split in fourth year students and students that had already been studying for five or six years. The rationale behind this is that fourth year students might be more receptive to negative financial incentives as they had only incurred a one year delay so far.

The baseline specification showed a significant positive effect for fifth year students, but no effects were found for fourth and sixth year students. In order to test the robustness of these results two checks were performed. First the sample was extended to include three pre cohorts instead of one. Similar coefficients were found, again showing a significant positive effect for fifth year students and no significant effects for fourth and sixth year students. The second robustness check also extended the sample, but this time by relaxing the constraint that students were omitted if they switched study programs or universities. Doing so increased the number of observations of both the pre and post cohort by almost a factor 3. The results of the second robustness checks showed no significant effects.

Does this mean that the 'langstudeerdersmaatregel' only resulted in positive behavioural effects for fifth year students? That is a difficult question

to answer. Within the boundaries of this study, the answer is of course yes. Outside of those boundaries, one could speculate that not finding an effect for sixth year students can be explained because the majority might no longer have the intention to finish their studies. The main difficulty in generalizing the results is the fact that no positive significant effects were found for fourth year students. The hypothesis of this paper is that they should have been able to graduate when they increased their effort. A critical note would be that total average time to graduation in Dutch higher education in the relevant years for this study was close to four and a half years. That might imply that exerting extra effort in order to graduate within the same year is a more realistic option for fifth year students. The question remains whether that is the correct explanation.

Several other factors that could limit the external validity need to be taken into account. First, the dataset only includes yearly records and data collection halted in October 2012. One can imagine that the policy measure acted like a wake-up call for many students, but graduating in 2010/2011 was not a realistic option for them. If the dataset would span more years after treatment the reliability of the analysis would improve. Moreover, this shows that graduation is a very rough variable to use. A second improvement would therefore be the use of institutional data so that ECTS credits per student can be tracked. Analysis based on ECTS credits has potential to expose behavioural effects that were not observed by this study. Analysis based on ECTS credits could for example show an average increase in the number of ECTS credits obtained by students affected by the measure but a less significant effect on graduation counts. As this study considers a graduation dummy as the outcome variable the existence of such effects cannot be ruled out. Moreover, other outcome variables based on institutional data that signal behavioural changes could be considered. For example, students that were inactive for a year but suddenly started participating in courses or exams could be identified as potentially affected by the policy measure as well, which provides a more subtle way of evaluating the effects. Note though that when using institutional data one would still need to distinguish the impact of the Bachelor-before-Master act from the impact of the 'langstudeerdersmaatregel', for example by comparing institutional data from several institutions.

Second, one needs to consider whether doubts existed about the credibility of treatment upfront. If students for instance would have believed that the judge would rule against the government there would not have been any

anticipatory behavioural consequences. While much uncertainty about the introduction of the policy measure was present, it already passed both the House of Representatives and the Senate in 2011. As media coverage was very clear on this topic we feel the credibility of treatment is not affected.

Thirdly, the employed identification strategy could be debated. Based on variation in the implementation dates of the Bachelor-before-Master act treatment and control groups were defined. This is based on the assumption that students studying at a university that implemented the Bachelor-before-Master act on its own initiative are accustomed to policies focused on reducing their study duration, whereas students studying at one of the universities that were granted a postponement are not, or not as much. One could however argue that this assumption is flawed and that the postponement had other causes.

Now, what does this evaluation imply for current policies concerning higher education? This is a tough question to answer, as this study cannot provide a definite answer as to the impact of the policy measure. In order to confidently answer whether the 'langstudeerdersmaatregel' lead to the expected results the analysis would need to be extended to consider institutional data, in order to account for more subtle behavioural effects. In addition, it should be completely clear that no financial consequences were evaluated as they were compensated after the abolishment. When considering reintroducing this policy measure, or one similar to it, financial consequences should be the point of focus. There appears to be no place for such a measure in the current debate though, as recently a student loan system was announced to improve student performance and reduce financial support for students.

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Appendix

Tables

Table 12: Summary statistics - Baseline specification
Fourth year students only - Split into control and treatment

| | Pre | | Post | |
	Control	Treat	Control	Treat
Observations	251	196	200	211
Graduated after year 4	167	106	154	125
Percentage	66.5	54.1	77.0	59.2
University (%)				
University of Utrecht	50.2	0	49.0	0
University of Amsterdam	49.8	0	51.0	0
VU University of Amsterdam	0	50.5	0	48.3
University of Tilburg	0	49.5	0	51.7
Study Program (%)				
Economics	27.5	36.2	26.0	43.1
Law (Dutch)	39.4	31.1	33.5	29.9
Psychology	27.1	23.5	32.0	15.2
Biomedical Sciences	6.0	9.2	8.5	11.8
Background information				
Gender (% female)	47.4	48.0	49.0	38.9
Age (average in years)	18.5	18.5	18.5	18.5
Ethnicity (% non-western)	24.7	29.1	23.5	35.1
Average grade secondary education	6.8	6.7	6.8	6.7

Table 13: Summary statistics - Robustness check 1 (baseline specification plus 2 pre cohorts)

| | Year 4 | | Year 5 | | Year 6 | |
	Pre	Post	Pre	Post	Pre	Post
Observations	1339	411	594	167	298	60
Graduated after year 4	813	279	298	96	118	27
Percentage	60.7	67.9	50.2	57.5	39.6	45.0
University (%)						
University of Utrecht	27.0	23.8	19.5	22.2	18.5	20.0
University of Amsterdam	30.6	24.8	33.8	26.9	28.5	26.7
VU University of Amsterdam	20.1	24.8	20.0	27.5	22.1	26.7
University of Tilburg	22.3	26.5	26.6	23.4	30.9	26.7
Study Program (%)						
Economics	32.1	34.8	31.3	31.7	32.2	30.0
Law (Dutch)	38.2	31.6	38.9	35.9	42.6	28.3
Psychology	23.5	23.4	24.1	24.0	20.1	26.7
Biomedical Sciences	6.2	10.2	5.7	8.4	5.0	15.0
Background information						
Gender (% female)	47.7	43.8	42.3	41.3	37.6	35.0
Age (average in years)	18.5	18.5	18.6	18.6	18.6	18.8
Ethnicity (% non-western)	26.4	29.4	31.0	32.9	36.6	31.7
Average grade secondary education	6.7	6.7	6.6	6.7	6.6	6.5

Table 14: Summary statistics - Robustness check 2 (baseline specification plus switching)

| | Year 4 | | Year 5 | | Year 6 | |
	Pre	Post	Pre	Post	Pre	Post
Observations	1168	1188	584	654	372	344
Graduated after year 4	484	548	217	256	101	90
Percentage	41.4	46.1	37.2	39.1	27.2	26.2
University (%)						
University of Utrecht	28.2	24.7	26.5	25.4	26.6	26.7
University of Amsterdam	33.5	33.3	32.9	35.5	34.7	33.4
VU University of Amsterdam	19.1	20.3	17.1	20.0	16.7	18.0
University of Tilburg	19.3	21.7	23.5	19.1	22.0	21.8
Study Program (%)						
Economics	33.9	33.0	31.5	32.0	26.6	28.8
Law (Dutch)	38.2	37.3	35.8	38.5	41.4	33.7
Psychology	17.5	16.8	17.5	15.1	17.2	17.7
Biomedical Sciences	12.8	12.9	15.2	14.4	14.8	19.8
Background information						
Gender (% female)	45.2	43.9	40.6	41.3	40.1	38.4
Age (average in years)	18.6	18.5	18.6	18.6	18.6	18.6
Ethnicity (% non-western)	27.1	25.8	26.7	30.0	33.3	29.1
Average grade secondary education	6.7	6.6	6.6	6.7	6.6	6.5

Table 15: Chronology of important events with relation to the policy measure

#	Event	Date
1	Coalition agreement VVD-CDA VVD-CDA (2010)	30-9-2010
2	Article NRC Wijkerslooth http://vorige.nrc.nl/article2645952.ece	1-12-2010
3	Student protests at Malieveld, Den Haag http://www.volkskrant.nl/vk/nl/2844/Archief/archief/article/detail/1827056/2011/01/22/Pechtold-Ich-bin-ein-langstudeerder.dhtml	21-1-2011
4	Claim by law firm Stibbe 'fine is unlawful' http://www.nrc.nl/nieuws/2011/04/04/drie-bezwaren-tegen-de-langstudeerboete/	4-4-2011
5	Implementation is postponed by one year http://www.volkskrant.nl/vk/nl/4884/Bezuinigingen-in-het-hoger-onderwijs/article/detail/1874710/2011/04/13/Omstreden-boete-voor-langstudeerders-uitgesteld.dhtml	13-4-2011
6	House of Representatives votes in favour	26-4-2011
7	Senate votes in favour	5-7-2011
8	Students take Dutch government to court http://www.trouw.nl/tr/nl/5009/Archief/archief/article/detail/3038224/2011/11/17/Studenten-slepen-staat-voor-de-rechter-om-langstudeerboete.dhtml	17-11-2011
9	Publication of first monitoring report Intomart GfK bv (2012)	20-2-2012
10	Promise by State Secretary of Education that part-time students en chronically ill students will be compensated https://www.eerstekamer.nl/toezegging/groep_deeltijdstudenten_die_recht	17-3-2012
11	Cabinet Rutte-I collapsed	21-4-2012
12	Start of trial http://www.volkskrant.nl/vk/nl/5288/Onderwijs/article/detail/3258564/2012/05/21/Rechtszaal-overvol-voor-langstudeerzaak.dhtml	21-5-2012
13	Judge's verdict: fine is not unlawful http://www.nu.nl/politiek/2856285/langstudeerboete-gaat-.html	11-7-2012
14	Debate in House of Representatives	23-8-2012
15	Student organisation LSVB starts gathering complaints w.r.t. the fine http://nos.nl/op3/artikel/415812-meldpunt-klachten-langstudeerboete.html	7-9-2012
16	Elections for House of Representatives	12-9-2012
17	Preliminary coalition agreement VVD-PvdA VVD-PvdA (2012a)	1-10-2012

Table 16: Covariates

#	Hypothesis	Short	Source
1	Study progress for female students is higher than for male students, for instance because male students exert less effort than female students. This implies that there is more room for male students to increase effort, which, ignoring potential gender differences in price elasticity, leads us to the expectation that the fine influenced male students stronger than female students.	Gender	Breen et al. (2010) Buchmann and DiPrete (2006) ResearchNed (2013)
2	Ethnicity of students has been established as an important factor in study progress, in the Netherlands for instance by multiple studies of the 'Inspectie van het Onderwijs'. In 2007 a report specifically aimed at improving participation and study progress of foreign students in higher education was presented. Hence, as foreign students take longer to complete their studies, they are likely to be affected to a higher degree than their Dutch (European) counterparts.	Ethnicity	Inspectie van het Onderwijs (2007) Severiens et al. (2006)
3	The effect of age on educational achievement has been studied extensively over the years, with mixed results (see for a literature review Richardson, 1994). However, recent studies such as Berg and Hofman (2005) with relevance for the Netherlands, have shown that older students are generally less successful. They show that various explanations opposing this conclusion can be presented, such as the 'gap-year' or students with a longer educational route; however, after investigation and correction for most of these explanations the effects remain. Hence, the impact of the 'langstudeerdersmaatregel' is expected to increase with age.	Age	Belloc et al. (2010) Berg and Hofman (2005) Inspectie van het Onderwijs (2009)
4	Large differences in study progress exist between disciplines as well, bèta studies being the upward exception with respect to study progress. Note that bèta studies have a large number of male students, hence there is a strong correlation with the previous hypothesis. However, one can also imagine differences between the field of medicine and business, or philosophy. Medicine since it has a higher nominal study duration, whereas the latter, philosophy, is often chosen as a second field of study. The expectation here is therefore that the effects of the fine are more pronounced in disciplines with a higher expected study duration (relative to the nominal duration).	Discipline	CBS (2010) ResearchNed (2011)
5	There are several paths a student can follow in order to become eligible for enrolment in higher education. It has been shown that the previous level of education significantly affects study progress in subsequent levels of education. For example, students can follow different levels of secondary education, or even work their way up through basic vocational education. These students are likely to be strongly, intrinsically motivated, however, completing higher education often requires more time. Hence, the question is which of both factors contributes more, and is affected strongest by the 'langstudeerdersmaatregel'. The expectation is that these students will be strongly demotivated by the fine and, since the expected costs are relatively high for this group, more students will drop out.	Previous level of education	HBO-raad (2008) ResearchNed (2011)