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Does replacing grants by income-contingent loans harm enrolment?

This paper evaluates the impact of a policy reform which replaced universal basic grants by income-contingent loans in the Netherlands.
We study the effect of the reform on the decision to enroll in higher education using administrative data of ten complete cohorts of students.

Our estimates of differences between cohorts show no negative effect of the policy on enrolment. Moreover, difference-in-differences estimates exploiting variation in eligibility for supplementary grants show no negative effect on enrolment.

CPB Discussion Paper

Jonneke Bolhaar, Sonny Kuijpers,
Dinand Webbink, Maria Zumbuehl

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New evidence from a reform in Dutch higher education

Jonneke Bolhaar, Sonny Kuijpers, Dinand Webbink & Maria Zumbuehl

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Abstract

This paper evaluates the impact of a reform which replaced universal basic grants by income-contingent loans on enrolment in Dutch higher education using administrative data of ten complete student cohorts. Estimates of differences between cohorts show no negative effect of the policy on enrolment. Moreover, difference-in-differences estimates exploiting variation in eligibility for supplementary grants show no negative effect on enrolment. These findings suggest that a system of income-contingent loans can facilitate an increase of private contributions without harming access to higher education.

Corresponding author: webbink@ese.eur.nl.

1. Introduction

Private contributions to higher education are a highly controversial policy topic. Although countries like the UK, Australia, New Zealand and the USA ask students to pay substantial fees to attend universities, many others countries (e.g. France, Germany, Italy, Spain, Sweden, and the Netherlands) are more reluctant in asking private contributions, even in the context of increasing student numbers and strong pressures on public resources. Tuition fees may create a financial barrier to access higher education particularly for young people from disadvantaged backgrounds. Drawing upon experiences in Australia, New Zealand and the UK in the last decades economists have advocated the use of income-contingent loans by governments as they can mitigate credit constraints and provide insurance against future income risk (e.g. Barr 2004; Chapman 2006, 2014; Jacobs and Van der Ploeg 2006; Dynarski 2014; Stiglitz 2014; Barr et al. 2017; Murphy et al. 2019; Van Long 2019). Income contingent loans may offer a way out of the dilemma between limited public resources and access to higher education. However, the evidence on the impact of income-contingent loans is still very limited. In particular, it is not clear how students will react to replacing grants by loans in a context where they are used to receiving public grants as, for instance, in countries in continental Europe with a long tradition of '(nearly) free higher education' and a strong public resistance to private contributions.[1]

A recent major reform in Dutch higher education provides a rare opportunity for getting insight in the impact of replacing grants by income-contingent loans in a context of 'nearly free higher education'. After a period of intensive debate in parliament and in the media the Dutch government decided to introduce a so-called social loan system for newly enrolling students in September 2015. In the new system universal basic grants were replaced by income-contingent loans and the already existing supplementary grant for low income students was increased.[2] The system is labeled as social because the repayment scheme of the loan depends on the income level during the professional career, with an exemption for those whose earnings fall below a certain threshold. Even after the introduction the reform remained controversial. In fact, the new Dutch government recently planned to abolish the social loan system in September 2023 and partly compensate students who were exposed to the system.

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[1] Murphy et al. (2019) provide recent examples of resistance against private contributions also within the UK and the USA. See also Lergetporer & Woessmann (2022) investigating income contingency of loans and political support.

[2] Section 2 provides details about the old and new student financing system.

This study focuses on the key question in the policy debate. We evaluate the impact of the reform on the decision to enroll in higher education. The reform was introduced nationwide which poses challenges for the evaluation. To address these challenges we use empirical approaches which exploit respectively variation in access to public grants between and within cohorts of students. We use administrative micro-data of ten complete cohorts of students in Dutch secondary education.

The first part of our empirical analysis focuses on variation in access to grants between cohorts. We compare enrolment decisions of students in cohorts in secondary education before and after the introduction of the social loan system. The two main concerns for identification are potential differences through anticipating behavior and unobserved time factors correlated with enrolment. We address these concerns in the following way.

A comparison of enrolment decisions of students in the last cohort before the reform and the first cohort after the reform might be biased by anticipating behavior. For instance, the announcement of the introduction of the social loan system might have an impact on students who consider taking a gap year after secondary school. These students might decide not to postpone their enrolment in higher education in order to avoid the higher private contribution.[3] As a result, the enrolment rate before the reform could be much higher than after the reform. In addition, students might decide to take other routes through Dutch secondary education. We address this issue by constructing cohorts of students before the anticipating behavior might occur (three years before the nominal graduation date) and following these students untill four years after they could have enrolled in higher education. As such, we compare cohorts which are unlikely to differ because of anticipating behavior and we can observe whether a drop in enrolment is caused by changes in the timing of enrolment.

Unobserved time factors correlated with enrolment in higher education are an obvious concern for this empirical strategy. If the rate of enrolment is strongly volatile between cohorts it is difficult to isolate the effect of the reform from the effect of unobserved time factors. However, this doesn't apply to our setting. Figure 2 shows the rate of enrolment untill four years after the nominal graduation date for students of secondary education tracks which provide access to higher education. We observe that the enrolment rate is quite constant or even increasing for the ten cohorts of our data, and, without prior information, it is difficult to point out the first cohort treated by the reform. In our empirical analysis we address this

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[3] In our data we actually observe substantial decreases in the proportion of students taking a gap year in the last two cohorts not treated by the reform (see Figure 1).

concern about unobserved time factors by using different windows of cohorts before and after the introduction of the reform as our estimation sample and by including polynomial time trends.

Moreover, we investigate unobserved time effects by performing 'in-time placebos', as suggested in Abadie (2015) (see also Heckman & Hotz 1989; Athey & Imbens 2017, Abadie 2021). In particular, we compare enrolment in higher education between cohorts which don't experience changes in the grant or loan system. If the introduction of the new policy has a negative impact on the enrolment of students we expect the estimated treatment effect to be clearly different from the estimates produced by the 'in-time placebos'. If the estimates are quite similar it seems not very likely that the treatment effect can be attributed to the new policy.

The second part of our empirical analysis aims to further mitigate concerns about unobserved time factors by using variation in grant eligibility within cohorts. The reform not only replaced the universal basic grant by income-contingent loans but also increased the supplementary grant for low income students. This increase creates variation in the intensity of the treatment. In particular, after the reform the (total) grant eligibility for students who were fully eligible for the supplementary grant (students with low-income parents) was very similar to their grant eligibility before the reform. For these students the loss of the basic grant was compensated by a higher supplementary grant and, as a result, they were hardly treated by the reform. The reform had a larger financial impact on students who were partially eligible for the supplementary grant and had the largest financial impact on students who were not eligible for the supplementary grants. We exploit this specific implementation of the policy by applying a difference-in-differences strategy to study the impact of the 'uncompensated' loss of the basic grant.

We find that the introduction of the social loan system had, contrary to societal beliefs, no statistically significant negative effect on enrolment in higher education. For students in the academic track of secondary education we find no reduction in enrolment with both empirical approaches. For students in the higher general track of secondary education we also don't find a negative impact of the reform on enrolment. In fact, enrollment increased after the reform. In addition, the 'in-time placebos' produce very similar estimates. This means that the estimated treatment effect is not statistically different from changes in enrolment from unobserved factors not related to the reform.

In the discussion about the introduction of the reform concerns were raised that students might shy away from more challenging levels or types of higher education, or that students might choose educational institutions closer to the parental home, in order to avoid the higher costs of enrolment. Our analysis of these

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educational decisions doesn't provide evidence consistent with these concerns. However, we find evidence for anticipating behavior of students. In the two cohorts prior to the reform substantial proportions of students don't take gap years. Finally, students take more loans and work a few hours more after the reform. These findings show that some decisions and behavior of students are sensitive to the reform but the main decisions about the level, type and institution of higher education appear not be affected by the reform.

In sum, both parts of our empirical analysis suggest that the impact of the introduction of the social loan system on enrolment is likely to be small in general. This also holds for low income students who could obtain a higher supplementary grant due to the reform. Even if the size of the unobserved time effect in the year of the introduction of the new policy would differ strongly from the time-effects in previous and later years the reducing effect would be small. For instance, if the size would be twice the value of the largest time change in the range of ten cohorts in our sample, the estimated reducing effect for students in the academic track would not be larger than 0.8 %-points from a baseline enrolment of 96 %. This finding has important policy implications. It suggests that a carefully designed income-contingent loan system, which includes a long-term repayment scheme with a repayment exception for low earners, can be used to replace public grants without deterring students from enrolment in higher education. Apparently, students expect that the returns to higher education will more than outweigh the short-term increase in costs due to the reform.

We contribute to the economic literature on the financing of higher education and to the literature that studies the impact and design of student loans (e.g. Barr et al. 2019; Britton et al. 2019; Chapman 2006, 2014; Dearden et al. 2008; Dynarski et al 2013). Our study provides new evidence on the effects of a nationwide introduction of a student loan system which can be implemented in many other countries. We find that the introduction of income contingent loans doesn't harm enrolment in higher education. This finding is consistent with earlier findings in Anglo-American contexts. The analysis shows that this type of loan schemes can also be effective in the context of a country with a long tradition of nearly free higher education and substantial public resistance against higher private contributions. Our study differs from previous studies by using rich administrative micro-data of all students from ten cohorts observed over a time range of seven years. In addition, we evaluate a reform that is nationwide implemented and address various identification concerns by using different empirical strategies.

This paper is organized as follows. The context of the Dutch education system, the details of the old and new student financing system and the data are

explained in Section 2. Section 3 investigates the impact of the reform on enrolment decisions by comparing cohorts treated and not treated by the reform. Section 4 applies the difference-in-differences analysis based on the variation in the supplementary grant. Section 5 explores how students deal with losing the basic grant. Finally, Section 6 concludes.

2. Context and data

Structure of Dutch secondary education and higher education
Dutch secondary education is designed as a tracked education system. At the end of primary education at the age of 12 students are sorted into three main tracks (vocational, higher general and academic) based on their abilty. The two highest tracks provide direct access to higher education. The academic track (VWO) with a duration of six years provides access to university. The higher general track (HAVO) with a duration of five years provides access to the lower level of higher education (higher professional education). Students from the vocational track (VMBO) can access higher professional education after completing the four-year track in senior secondary vocational education (MBO 4). However, the structure of the vocational tracks prevents us from constructing cohorts three years before the nominal graduation date which gives access to higher education. Therefore, we focus our analysis on students in the track levels with direct access to higher education. Dutch higher education is mostly publicly financed. There are approximately 40 schools for higher professional education offering four-year programs towards Bachelor of Applied Science degrees. In addition, there are 14 universities (including the Open University) offering three-year bachelor programs, and master programs of one year or longer.

Student financing before and after 2015
All students graduating from the general higher track or from the academic track can enroll in higher education and are eligible for student finance. The Dutch student finance system consists of a combination of public grants, loans and in kind support. The reform of 2015 replaced a universal basic grant by income contingent loans and increased the maximum amount of the supplementary grant. Table 1 summarizes the main components of the old and new system of student finance.

Before 2015

Since 1986 the Student Finance Act regulates the allocation of public grants to students, which take the form of monthly financial transfers. Until 2015 there were four categories of support: the universal basic grant, the supplementary grant, the loan and the "in-kind' support. All students received the basic grant and the size of this grant depended on the living situation of the student (i.e. students living with their parents or independently). This grant was paid for the nominal duration of the specific study in higher education. The supplementary grant (which remained in place also after 2015) depended on parental income and characteristics of the family. Both grants were conditional on educational performance and could be changed into a loan in case the conditions were not met. The third form of support was an additional loan. Both the debt from the additional loan and debt from grants-turned-into-loans due to not meeting the performance requirements had the same pay back requirements. They must be paid back within 15 years, conditional on sufficient income. The last category of support was a travel pass, entitling students to free public transport (during weekends or weekdays). Next to living expenses and direct costs (books, etc.), students pay a fixed tuition fee at the beginning of each academic year. These fees are uniform across all subjects. All elements of the student finances in the old system were available for all students under 30 years of age with a Dutch nationality, or a non-temporary residence permit (with certain exceptions for temporary permits). All elements of the old system were either direct income contingent loans or conditional gifts that were turned into loans if the diploma was not obtained within 10 years.

After 2015: The social loan system

The social loan system introduced in 2015 no longer includes a basic grant and consists of three elements: a supplementary grant, student loans and 'in kind support'. The three components in the new system are comparable to the components in the old system but some features have changed (see Table 1). The amount of the supplementary grant has been increased. The maximum amount of the loan component has also been increased. Moreover, the period for repayment of the loan has been increased to 35 years. In the new system students can still receive a public transport card.

The timeline of the reform

In October 2012 the new Dutch government announced the introduction of a new student finance system for all students starting in September 2014. However, in the

summer of 2013 it was announced that the introduction of the new system would be delayed by one year, and in May 2014 an agreement was made about the introduction date of September 2015. All newly enrolling students in September 2015 were treated with the social loan system. The political discussion about the reform still continues and a majority of the new parliament from early 2021 supports the idea of abolishing the social loan system. Recently, the Dutch government decided to abolish the social loan system in September and to re-introduce basic grants for students. Moreover, the government is planning to compensate students who were exposed to the social loan system.

Data

For this project we compile data from several large administrative datasets from Statistics Netherlands.4 The register data provide us with a sample of all students in secondary and tertiary education. For each student we have a Random Identification Number (RIN), which is the coded Dutch equivalent of the U.S. Social Security number. This allows us to track individuals over time and to link students with their parents. We use data of ten cohorts of students and track them from three years before the nominally expected date of their final exam in secondary education until four years after. We observe enrollment in higher education as well as the type and level of higher education. The data don't provide information about enrollment of Dutch students in higher education abroad. However, very few Dutch students enroll abroad directly after their high school exam.5 In addition, we can use parental information on household income, parental education and migration background to construct the socioeconomic background of students.

For our analysis we constructed cohorts of students based on their educational position three years before the nominally expected date of their final exam. This implies that our sample focuses on students who are following higher general or academic tracks in secondary education. The new system was announced

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4 The data are accessible via a remote-access computer after a confidentiality statement has been signed. In addition, data on student debt were obtained from the Dutch organization responsible for the implementation of student financing (DUO).

5 Statistics on the total % of Dutch students studying abroad, the % in bachelor programs at the higher professional level or in master programs at the university level show no changes in the years before and after the reform (2014 and 2015) . See: https://www.ocwincijfers.nl/sectoren/onderwijs-algemeen/leerlingen-en-studenten/internationale-mobiliteit-van-leerlingen-en-studenten. In addition, 96 % of students from the academic track are observed in higher education four years after the final exam which leaves little margin for emigrating students.

in October 2012, and finally introduced in the fall semester 2015. By the time the change was announced, the first affected cohort had just entered their third last year of secondary education (3rd year higher general track or 4th year academic track). Hence, we consider students who started their third last year of secondary education in September 2012 as the first cohort treated with the loan system. Students who started their third last year in September 2011 are considered as the last cohort treated with old system. We can construct an educational trajectory for each student based on the enrolment data. In particular, we can observe for each student whether they enrolled in higher education within these seven years. For instance, for students who graduate within the nominal duration of secondary education we observe whether they enroll in higher education in the next four years. We also observe whether students directly enroll in higher education or indirectly, for instance after taking one or more gap years, or whether students are delayed in secondary education.

Summary statistics

Table 2 shows summary statistics of the main variables of the two cohorts of students in secondary education before and after the reform. Columns (1) and (2) show statistics for students in the academic track, columns (3) and (4) show statistics for students in the higher general track. We observe no major differences in the composition of the cohorts before and after the reform. The proportions of students with a migrant background, low income parents (income in lowest quarter of household income) or single parents is slightly larger in the cohort after the reform. Summary statistics for all cohorts are shown in Table A.1 in the appendix.

The middle panel of Table 2 shows statistics on the educational route of students through secondary education and enrollment in higher education after the construction of the cohorts. We observe that approximately 8 percent of students in the academic track switch downward to the higher general track. In the higher general track approximately 5 percent of the students move to the upper track in secondary education The middle panel also shows that more than 81 percent of students in the higher general track enrolls in higher education, for the academic tracks this is appproximately 96 percent. A simple comparison of the cohorts before and after suggests that the reform did not affect enrolment. Interestingly, the reform had a clear impact on the timing of enrolment. The cohort before the reform was more likely to enroll directly and less likely to take a gap year or experience delay in the last years before the final exam. These differences are quite substantial; respectively 2 %-points and 4 %-points more direct enrolment of students in the higher general and academic track. If we compare the pre-treatment cohort with

earlier cohorts the differences are even larger (see Figure 1). However, these differences in the timing of enrolment don't translate into differences in the cumulative enrolment after four years. It should also be noted that there is no difference in early transitions in secondary education, for instance by moving to other tracks in secondary education, in the years before the final exam.

3. Estimates of the effect of the reform based on variation between cohorts

In the first part of our empirical analysis we compare enrolment decisions of cohorts of secondary students before and after the introduction of the social loan system. In the first set of analyses we assume local randomization between cohorts. This assumption means that potential outcomes of treated and untreated cohorts are assumed to be equal as in a randomized experiment or in a regression discontinuity model with local randomization (Cattaneo et al. 2018). The social loan system was introduced for new students in Dutch higher education starting in September 2015. All first-time enrolling students after this date will be treated by the new policy and first-time students enrolling before this date will not be treated by the new policy (if they chose to delay their studies they will however be exposed to the new policy).

For estimating the impact of the new policy we focus on estimating linear probability models as specified in Equation (1):

$$Y_{ic} = \alpha_0 + \alpha_1 P_{ic} + \alpha_2 X_{ic} + \mu_c + \varepsilon_{ic} \qquad (1)$$

with Y_{ic} is a dummy variable for enrolment within four years after nominal graduation in secondary education for student i from cohort c, P is a dummy variable for exposure to the new policy, X_{ic} is a vector of observed individual covariates, μ_c are unobserved time factors at the cohort level and ε_{ic} are unobserved individual factors.

      Local randomization means that the potential outcomes of students are uncorrelated with their secondary school cohort. This implies that unobserved time and unobserved individual factors are uncorrelated with the treatment ($cov(P, \mu_c + \varepsilon_{ic}) = 0$). Both unobserved components may confound identification. The composition of cohorts might be different as students might try to avoid the new system and unobserved time factors might have changed after the introduction of the new policy. We address these potential confounders in the following way.

      The composition of cohorts might be different due to anticipating behavior of students through taking other routes through Dutch secondary education. In addition, students might decide not to take a gap year for travelling or working in order to avoid the higher costs imposed by the loan system. We address this issue by constructing cohorts before the anticipating behavior occurs. In particular, we construct cohorts based on their educational position in secondary education three years before the nominally expected date of the final exam after which students can enroll in higher education. Moreover, we observe students until four years after their final exam. This allows us to observe the anticipating behaviour and the

postponement of decisions. Importantly, the composition of the two cohorts in the smallest observation window cannot be affected by the announcement of the new policy. This announcement took place after these students were already enrolled in their cohorts in secondary education. It should be noted that students from the last secondary school cohort will only be treated by the old system if they graduate from secondary education within three years and directly enroll in higher education. If they experience some delay in this trajectory, for instance due to repeating a grade, they will be treated with the new system. Our data allows us to observe the full cohorts of students and to control for a set of covariates (gender, parental income, migration background, single parent family and number of siblings).

Unobserved time factors between cohorts are also an important concern for our identification. If recent cohorts are more likely to enroll in higher education than previous cohorts we might underestimate the potential negative effect of the loan system on higher education enrolment. It should be noted that Figure 2 indicates that enrolment in the years before the policy reform was fairly constant without a clear upward time trend. To address this potential concern we use different windows of cohorts as our estimation sample and include polynomial time trends. In particular, we estimate equation (1) using all cohorts, and using windows of three, two or one cohort before and after the introduction of the reform.

The main estimation results based on the model specified in Equation (1) are shown in Table 3; the top panel shows the results for students from the academic track, the bottom panel shows the results for students from the higher general track. The estimates in columns (1) and (2) are based on all ten cohorts; seven cohorts before the reform and three cohorts after the reform. The next columns use windows of three, two or one cohort across the introduction date of the reform. The odd columns don't include controls, the even columns controls for gender, parental income, migration background, having a single parent, number of siblings. Columns (2), (4) and (6) also include a (quadratic) time trend. Each column shows the results of a separate regression.

The estimates in the top panel of Table 3 for students in the academic track suggest that the reform did not reduce enrolment. The estimates tend to be negative and close to zero. Moreover, the estimates become statistically insignficant when covariates are added to the model. The estimates in the bottom panel suggest that the introduction of the social loan system did not decrease but increase enrolment for students from the general higher track. All point estimates are positive and most of them are statistically significant. The estimates are robust to the window of cohorts used in the analysis and also to other specifications (see Table A.2 in the appendix).

'In-time placebo tests' on the importance of yearly changes

To further address the concern about unobserved time factors between cohorts we compare the estimates from the smallest window with estimates from 'in-time placebos' based on a comparison of enrolment between cohorts without changes in the grant or loan system, as suggested by Abadie (2015) (see also Heckman & Hotz 1989; Athey & Imbens 2017). This placebo analysis is not only important for assessing the potential bias of the estimate of the reform due to unobserved time factors but is also revelant with respect to the estimation of the standard errors in Equation (1). If the unobserved factors are not correlated with the regressors they may still create correlation between the error terms within cohorts, and yield standard errors which are insufficiently conservative. In our application we cannot use the standard solution of adjusting the standard errors for clustering at the cohort level because we only have ten cohorts in our data. The placebo analysis is an inferential exercise which examines whether or not the estimated effect of the actual treatment is large relative to the distribution of the effects estimated for the cohorts not exposed to the reform. If the introduction of the new policy has a negative impact on the enrolment of students we expect the estimated treatment effect, based on a comparison of enrolment in the cohort before and after the introduction of the new policy, to be clearly different from the estimates produced by the 'in-time placebos'. If the estimates are quite similar it seems not very likely that the treatment effect can be attributed to the new policy.

Table 4 shows the the results of the placebo analyses based on a comparison of cohorts that did not experience policy changes in the grant or loan system. Each column in Table 4 shows the effect of a dummy for the post-cohort on enrolment based on a sample of two cohorts only. All models include the full set of controls. The main finding of these placebo analyses is that the yearly changes in enrolment between cohorts in the absence of policy changes in student financing are quite modest. This is consistent with the flat enrolment pattern observed in Figure 2. For students from the academic track the yearly changes vary between -0.1 % and +0.4 %. For students from the higher tracks the yearly changes are also small and vary between -0.4 %-points and +0.7 %-points. If the new policy would have a negative impact on enrolment we would expect the estimate for 2012 (column (6)) to be clearly different from the 'in-time placebos' in the other columns, and also to be more negative. However, this is not what the data show. The estimate for the academic track is hardly different from the 'in-time placebo' estimates and the estimate for the higher general track is even more positive. This suggests that the effect of the policy, if there is any, is small.

The 'in-time placebos' also provide us with a range of estimates of the unobserved time effect (μ_c) for previous and later cohorts. We use this range of estimates for assessing the potential effects of the policy if time effects are not absent. If we assume that the unobserved time effect for the year of the introduction of the new policy will not be very different from the time effects in previous or later years we can obtain an upper bound of the negative effect of the introduction of the social loan system. By using the largest estimate from the range of estimates of the time effect we can bound the negative effect of the introduction of the social loan system. For students from the academic track the adjusted estimate would be -0.00 – 0.42 = -0.42 %, which implies a small reduction of enrolment. For students from the higher general track the largest positive change is 0.68 %-points, which yields an upper bound of 1.07 – 0.68 = +0.39 %, hence, no reduction of enrolment.

The impact of the reform on vulnerable groups of students
The impact of the reform on 'vulnerable students', such as low-income students or students with a migration background, played a prominent role in the policy discussions about the reform. These concerns also motivated the increase of the maximum supplementary grant for low income students (see Table 1). As such, 'vulnerable students' were treated differently by the reform. In the next section we will exploit this differential treatment in our difference-in-differences strategy.

Table 5 shows the estimates for specific samples of students who might be more vulnerable to changes in the private costs of higher education. The first two columns show the impact of the reform on students with and without a migration background. Students with a migration background are not more adversely affected by the reform than other students. Furthermore, we find similar results in columns (3) and (4) for students with low parental income. For students in the higher general track we even observe an increase in enrolment for students with low parental income. Finally, the estimates in columns (5) and (6) show no major differences in enrolment patterns after the reform for female and male students. These estimates suggests that the reform, which included specific adjustments for potentially vulnerable students, did not harm their enrolment.

4. Estimates of the effect of the reform based on variation within cohorts

In the second part of our analysis we exploit variation in the intensity of the treatment within cohorts and implement a difference-in-differences strategy. This analysis aims to further reduce concerns about the importance of unobserved time factors. The reform not only replaced the universal grant by income-contingent loans but also increased the size of the supplementary grant for students from low-income families. This increase creates variation in the intensity of the treatment by the reform between students who were eligible and students who were not eligible for the supplementary grant.

The eligibility for the supplementary grant in Dutch higher education mainly depends on parental income but also on some other factors like the number of siblings in higher education (see appendix B). Figure 3 shows the (total) grant eligibility, which is the sum of the basic grant plus the supplementary grant, by parental income for two cohorts before (2013-2014) and three cohorts after the reform (2015-2017). The top figure shows grant eligibility for students living with their parents, the bottom figure shows grant eligibility for students living independently. In both figures we can distinguish three groups of students who differ in the intensity of the treatment. First, the flat part at the left side of the figures shows the change for students who are fully eligible for the supplementary grant. Second, the declining part in the middle of the figures shows the change for students who are partially eligible for the supplementary grant. Third, the flat part at the right side of the figures shows the changes for students who are not eligible for the supplementary grant. The figures show that students who were eligible for the maximum of the supplementary grants, those with parental income of 31,200 or less, experience the smallest reduction in grant eligibility. For these students the loss of the basic grant was compensated by a higher supplementary grant. Students who were not eligible for the supplementary grant, those with parental income larger than 46,600 Euro, experience the largest reduction in grant eiligibility. Moreover, students who were partially eligible for the supplementary grant, those with parental income between 31,200 and 46,600, experience a reduction in grant eligibility in between the fully eligible and non-eligible students.

The (potential) difference in treatment by the reform for these groups is shown in Table 6. The top panel shows the maximum grant a student may obtain before and after the reform while living with parents or independently. The bottom panel uses data from the last cohort before the reform to show the actually received grant in the first year after enrolment and the predicted grant in the new system for the three groups of students. The actual amounts are the average of the grants of students living with parents and students living independently. We show the

predicted amount in the new system and not the observed amount to illustrate the potential difference in treatment by the new system[6]. The bottom panel shows that the reform hardly changed the (total) grant eligibility for students who were fully eligible for the supplementary grant. Students in the general track even obtained slightly more grants after the reform than before. Therefore, this group of students seems very suitable as control group in our difference-in-differences analysis. The impact of the reform was largest for students who were not eligible for the grant. Their grants would reduce with 120 to 170 Euro per month depending on the secondary education track. For students in the academic track we find the largest financial impact. This can be explained by the fact that these students are more likely to live independently in the first year after enrolment.

We exploit this variation within cohorts by applying a difference-in-difference approach with these three groups of students: students who are fully eligible for the supplementary grants, students who are partially eligible and students who are not eligible. This analysis studies the impact of the reduction of the basic grant without compensation through a higher supplementary grant. For estimating the impact of the new policy we estimate models as specified in Equation (2):

$$Y_{ic} = \beta_0 + \beta_1 P_{ic} + \beta_2 A_{ic} + \beta_3 B_{ic} + \beta_4 P_{ic}.A_{ic} + \beta_5 P_{ic}.B_{ic} + \beta_7 X_{ic} + \varepsilon_{ic} \quad (2)$$

with A_{ic} and B_{ic} are dummy variables for the specific group of students ($A_{ic} = 1$; partially eligible students (parental income between 31,200 and 46,600); $B_{ic} = 1$; non-eligible students (parental income larger than 46,600)). X_{ic} is a vector of observed individual covariates including parental income which determines the group assignment. The difference-in-differences estimates of β_4 and β_5 yield the impact of the policy for a specific group of students while taking into account the trend in enrolment in the group of students who were fully eligible for the supplementary grant (students with parental income of 31,200 or less). These estimates can be interpreted as the causal impact of the policy if the common trend assumption holds. It is assumed that, in the absence of the policy, the trend in enrolment in the two most affected groups whould have been equal to the trend in enrolment in the least affected group.

The pre-trends for the three groups of students provide insight in the plausibility of this assumption. Figure 4 shows the enrolment patterns for the three groups of students by secondary education track. The dots represent estimates of

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[6] We don't use the actually received amount after the reform as the reform might influence the decision to live with parents or independently.

event studies which compare the probability of enrolment of a specific cohort with the enrolment probability of the cohort of 2011, which is the last cohort before the reform. We observe that nearly all estimates for the cohorts before the reform are statistically insignificant. In particular, for cohorts closer to the introduction of the reform we find that the estimates are very close to zero and statistically insignficant. Hence, the probability of enrolment in these cohorts does not differ from the enrolment probability of the last cohort before the reform. Estimates of the differences in the pretrends confirm this pattern (see Table A.3 in the appendix). The estimates show that the difference in the trends of the two most affected groups compared to the least affected group is small and becomes statistically insignificant for the sample of cohorts closer to the introduction of the reform. This implies that the flat pre-reform trend observed in Figure 2 doesn't hide diverging trends in the three groups used in the difference-in-difference analysis. The similarity of the pre-reform pattern for the three groups mitigates concerns about the common trend assumption.

Figure 4 also shows what happened after the reform for the three groups of students. We don't observe a decrease in enrolment for a specific group of students. For the academic track all estimates are statistically insignificant and for the general higher track we even observe an increase in enrolment. This before-after comparison suggests that the reform had no major effect on the probability of enrolment.

Table 7 shows the difference-in-differences estimates using the same samples of cohorts and specifications as in Table 3[7]. Students who were fully eligible for the supplementary grant, which is the group of students hardly affected by the reform, are used as control group. For students in the academic track, which is the track that is financially most affected by the reform, we find no negative effect on enrolment. This holds both for partially eligible students as for non-eligible students. For students in the higher general track we also find that all estimates are statistically insignificant. Despite the fact that the enrolment in the control group of fully eligible students increased relatively strongly after the reform we find no statistically signficant estimates as the enrolment also increased in the two treatment groups after the reform.

It could be argued that the reform might have a larger impact on non-eligible students with relatively low parental income. Hence, students with parental income just above 46,600 Euro. To investigate whether these students are more sensitive to

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[7] We have also estimated difference-in-differences models which combine partially eligible and non-eligible students in one treatment group. The results are quantitively similar to the results with the main specification (see Table A.4 in the appendix).

the reform than students with richer parents we have split the group of non-eligible students by parental income quartile. The estimates are shown in Table A.5 in the appendix. We don't find a negative impact of the reform for students in the lowest quartiles of the parental income distribution of the non-eligible students. This holds for students in both secondary education tracks. As a further sensitivity test we estimated difference-in-differences models for students who might be considered as vulnerable groups, like in Table 5. We don't find a negative effect of the reform for students with a migration background nor for boys or girls (see Table 8).[8]

5. How did students cope with the reform?

The empirical analysis in the previous sections indicates that the reform did not affect enrolment decisions of students. From this we might ask how did students deal with the (potential) loss of public grants. Students may use various strategies to cope with the reform ranging from not taking gap years, switching tracks in secondary education, choosing for different levels, types or institutions of higher education or by working more hours or taking higher loans. Below we investigate the importance of these strategies.

Taking a gap year
Subtantial proportions of students don't enroll directly after their final exam but take one or more gap years before enrolment in higher education; approximately one out of three students in the higher general track and one out of four students in the academic track. Figure 1 shows that students in the two cohorts before the reform were less likely to take one or more gap years which is consistent with anticipating behavior. The drop in gap year taking in the 2010 cohort is likely to be related with the uncertainty in the political decision making which resulted in postponing the initial introduction date by one year (see Section 2). In particular, students in the academic track seem to avoid the social loan system by not taking a gap year.
We have also investigated whether students more often used specific routes through secondary education in order to avoid the social loans system. We find that there routes were used slightly more frequently before the reform (see Appendix C).

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[8] We did not estimate differences-in-difference models for low income students (as in Table 5) because the group compensation is already based on parental income.

Decisions on the level, type or institution in higher education

In the discussions about the introduction of the social loan system the concern was raised that students might shy away from more challenging levels or types of higher education. In order to reduce the expected costs of their study students might be more likely to choose for the lower level of higher education (higher professional instead of academic education) or might be less likely to choose for STEM education (Science, Technology, Engineering, Mathematics), which is often considered to be important for economic growth. It could also be argued that the introduction of the social loan system would induce students to more often choose for studies with higher expected returns on the labor market.

We have investigated whether the reform had an impact on these educational decisions by replicating the main models from Tables 3 and 7, and using as dependent variable dummies for the level of higher education or the type of higher education (STEM or non-STEM). For this analysis we use data of students from the academic track as they can choose between the two levels of higher education. The results are shown in Tables 9 and 10.

The estimates in Tables 9 and 10 suggest that the reform did not have a negative effect on the choice between academic and higher professional education. After the reform we observe a higher rate of enrolment in academic education. We also find no negative effect of the reform on choosing for STEM education. These results suggest that students did not shy away from more challenging levels or types of education. If anything, they seem to enroll more often in studies with higher expected returns on the labor market.

Students might also reduce the cost of studying by choosing an educational institution closer to their parental home which enables them to stay living with their parents. Figure 5 shows the distance between the parental home and the educational institution for students from the two cohorts before and after the reform. We observe no change in the distance to the higher education institution. In addition, we have investigated whether students were more likely to live with their parents after the reform as this might reduce the costs of studying. The data in Table A.1 and Figure A.1 in the appendix show a large increase in the proportion of students registered as living with their parents. However, it is likely that the data suffer from measurement error due to registration issues. In the old system students had a clear incentive to registrate their address in the municipality system because the basic grant for living independently was 180 Euro's per month higher than for living with the parents. The reform eliminated this incentive to registrate which might induce a measurement error. Students might no longer take the effort to registrate their new

home address when living independently. In addition, if the reform would have induced students to continue living with their parents we would expect this effect to be stronger for students with a smaller distance to their institution. A larger distance makes it more difficult to continue living in the parental home because of the time needed for traveling to the educational institution. In Figure A.1 we observe the opposite pattern. As such, it seems not likely that the increase in the proportion of students registered as living with their parents is really caused by the financial consequences of the reform.

Substituting grants with loans or income
Students might also cope with the reform by taking additional loans or increasing their working hours. Additional administrative data provides information about the take-up of student loans and working behaviour of students. We compared the behaviour of the last cohort of students before the reform and the first cohort after the reform. The results are shown in Table A.6 in the appendix.

After the reform students are more likely to take loans. We observe an increase of 16 %-points for students from higher general education and an increase of 25 %-points for students from the academic track. Students are also more likely to take the maximum amount of the loan. Students from the general higher track also work more often and more hours. For students from the academic track we observe a reduction of the share that is working but an increase in the number of hours worked for those who work. The changes in being employed and in the number of working hours are quite modest. This suggests that students mainly adjust their finances by taking more loans. It should be noted that the students in our analysis might be exposed to different labour market conditions which might also affect their working behavior. In addition, interest rates were decreasing during our period of study which might also affect students' willingness to take loans.

6. Conclusions

This paper investigates the impact of introducing income-contingent loans on enrolment in Dutch higher education. The reform replaced universal basic grants by income-contingent loans and increased supplementary grants for low-income students. Moreover, the income-contingent loan system offered a repayment scheme with a duration of 35 years and a repayment exception for low earners. In the Dutch context students were used to receiving public grants and there was a strong public resistance against private contribution to higher education. In

addition, enrolment levels after completion of secondary school tracks giving access to higher education were high.

The first part of the analysis compares enrolment decisions between cohorts of students in secondary education. This analysis shows no negative effect of the reform on enrolment. The estimated differences in enrolment before and after the reform are similar to the estimates from 'in-time placebos' based on a comparison of cohorts which don't experience a policy change. These findings suggest that the impact of the policy reform on enrolment is small. Even under more extreme assumptions about unobserved time effects the estimated impact is small and not larger than a one percentage point reduction in enrolment from a baseline of 96 percent.

The second part of our empirical analysis applies a difference-in-differences strategy based on the specific implementation of the new policy. The reform not only replaced the universal basic grant by income-contingent loans but also increased the supplementary grant for low income students. This increase creates variation in the intensity of the treatment. The reform hardly changed the (total) grant eligibility for students who were fully eligible for the supplementary grant. The reform had the largest financial impact for students who were not eligible for the supplementary grant. The estimates from the difference-in-differences models suggest that the reform did not reduce the probability of enrolment. For all specific groups in this analysis we find that the probability of enrolment is quite constant over time and doesn't change after the reform.

In addition, we have also investigated how students deal with the reform in terms of other educational decisions and the financing of their studies. We find evidence for anticipating behavior of students through not taking gap years. We don't find that students shy away from more challenging levels or types of education (STEM education). In addition, we find no change in the distance between the parental home and the higher education institution. Finally, students take more loans and work a few hours more after the reform. These findings show that some decisions and behavior of students are sensitive to the reform but the main educational decisions in higher education, about the level, type and institution, appear not be affected by the reform.

These empirical findings have important implications for a highly controversial policy topic. Our findings suggest that income-contingent loans can be used to replace public grants without deterring secondary education students from enrolment in higher education. The results are obtained in a context with a large public resistance against higher private contributions and a long tradition of nearly free higher education. We conclude that a carefully designed income-

contingent loan system, which includes a long-term repayment scheme with a repayment exception for low earners, is a promising policy option for increasing private contributions without harming access to higher education.

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Figures and Tables

Figure 1. Anticipation of the reform: Indirect enrolment in higher education by cohort of secondary education (%)
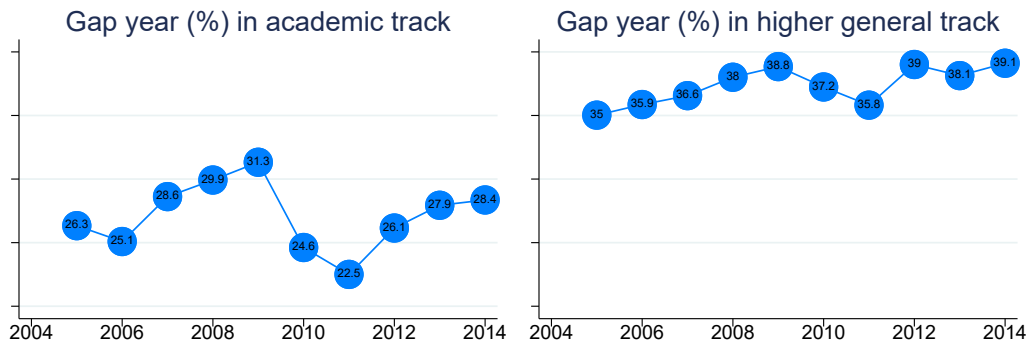
Notes: The figures show the proportion of students taking one or more gap years before enrolment. Cohorts are constructed when students were in secondary education three years before the expected nominal date of their final exam.

Figure 2. Enrolment in higher education (%) until four years after the nominal graduation date by cohort of secondary education
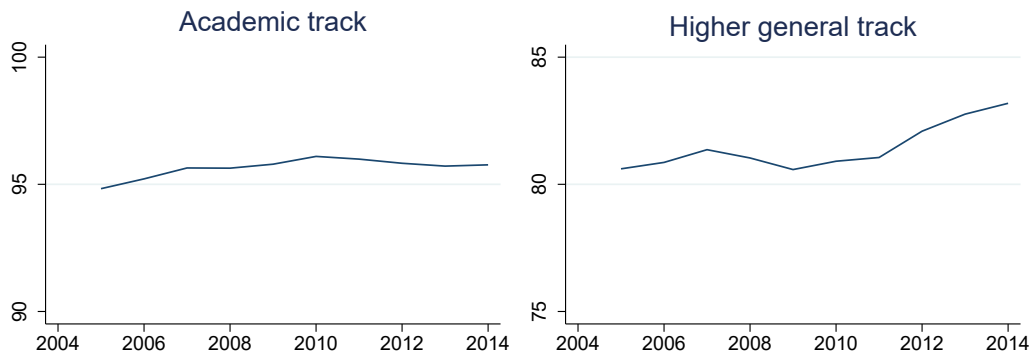
Notes: The figures show the proportion of students enrolling in higher education within four years after the nominal graduation date in secondary education. Cohorts are constructed when students were in secondary education three years before the expected nominal date of their final exam.

Figure 3. Grant eligiblity by parental income 2013-2017
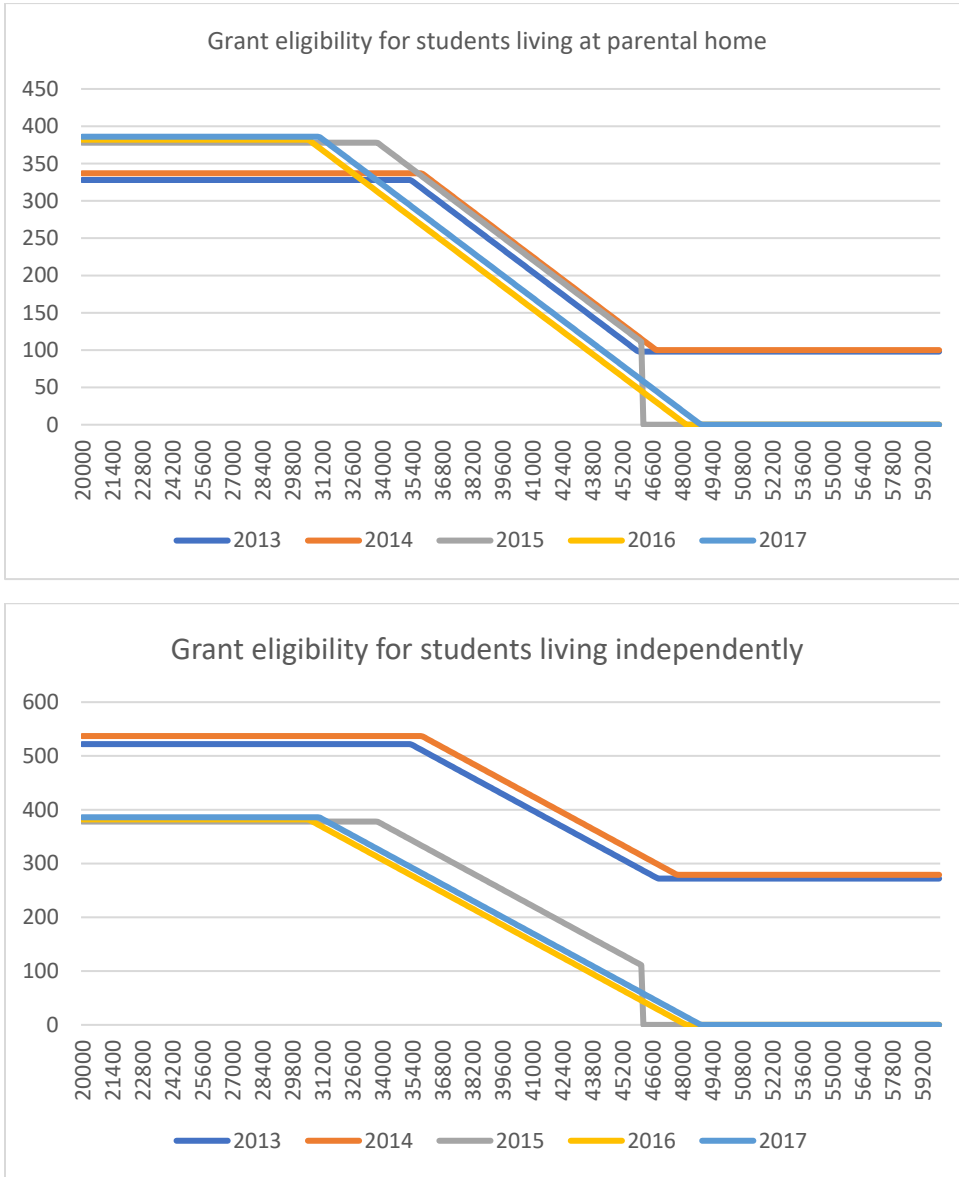
Notes: The figure show grant eligibility for students living at their parental home (top figure) or living independently (bottom figure). The Y-axis shows grant eligibility which is the sum of the basic grant plus the supplementary grant, the X-axis shows parental income. Before the reform students received a universal basic grant and could be eligible for the supplementary grant. The reform changed the universal basic grant into a loan and increased the supplementary grant. The years 2013 and 2014 show grant eligibility before the reform. The reform was introduced in 2015. Parental income is the main determinant of eligilibility for the supplementary grant.

Figure 4 Event study estimates of the probability of enrolment by eligibility for
the supplementary grant for the cohorts 2005-2014.
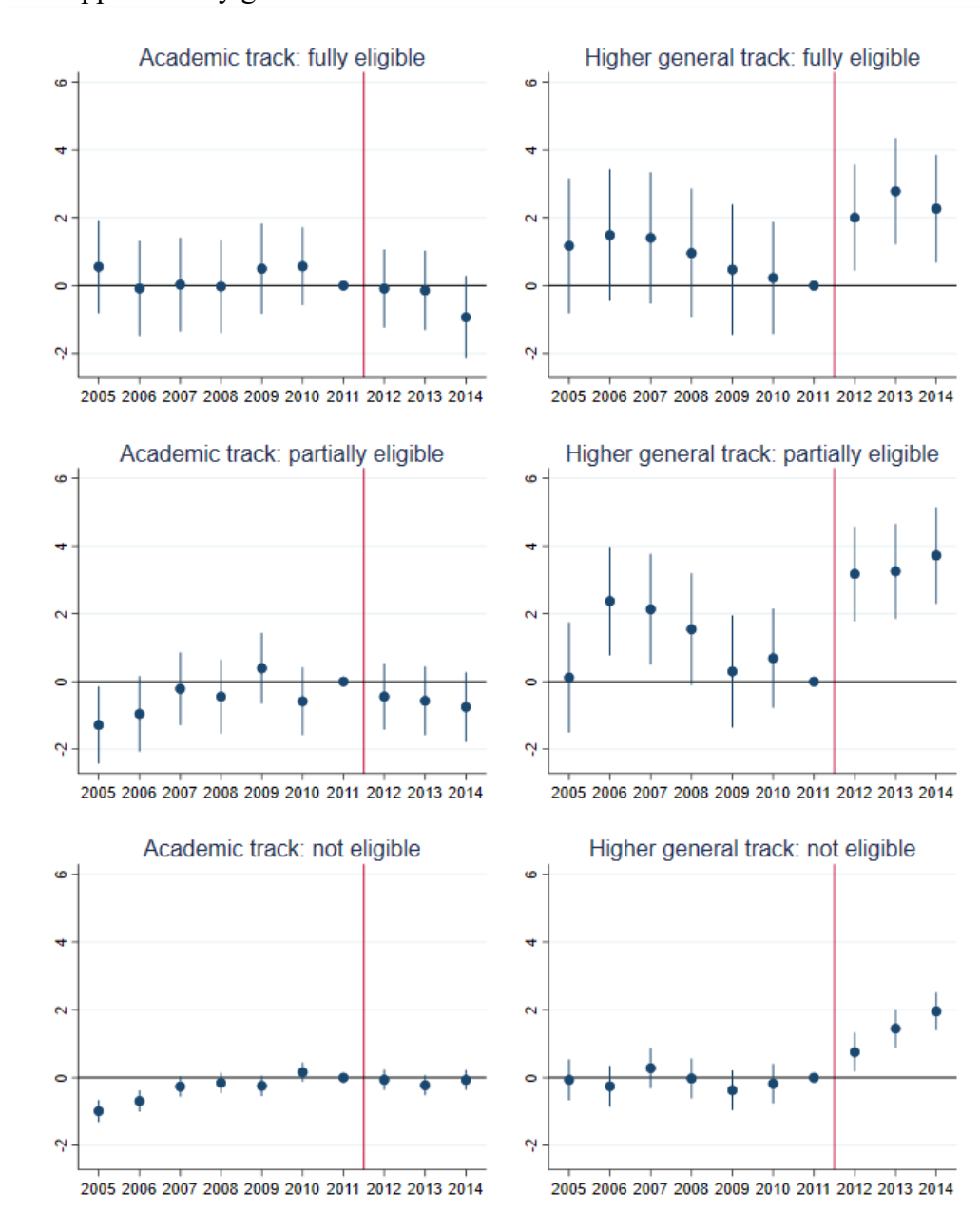
Notes: The figures shows changes in enrolment compared to the baseline cohort 2011,
which is the last cohort before the reform. The event study estimates control for gender,
parental income, migrant background, single parent and number of siblings.

Figure 5 Distance between parental home and educational institute before and after the reform

Distribution of distance home - HE institute

density (left panel, y-axis: 0, .005, .01, .015; x-axis: 0, 100, 200, 300)
distance between highschool home and HE institute in km

— vwo students before change (cohort 2011)
— vwo students after change (cohort 2012)

Distribution of distance home - HE institute

density (right panel, y-axis: 0, .005, .01, .015, .02, .025; x-axis: 0, 100, 200, 300)
distance between highschool home and HE institute in km

— havo students before change (cohort 2011)
— havo students after change (cohort 2012)

Notes: The figures show the distance between the parental home and the educational institute for the cohort before and the cohort after the reform. The left figure is about students in the academic track, the left figure is about students in the higher general track.

Table 1. The finance system for students in Dutch higher education before and after 2015

	Old System (1)	Social Loan System (2)
Universal basic grant (Euro/month)		
Students living with their parents	100	0
Students living independently	280	0
Supplementary grant (Euro/month)		
Maximum	260	380
Student loan		
Maximum	300	480
Repayment period (years)	15	35
In kind support		
Public transport card	Yes	Yes
Opt-in components		
Tuition fee credit	160	160

Note: Based on information from the Dutch government, see https://duo.nl/particulier/studiefinanciering

Table 2: Summary statistics for secondary school cohorts 2011 and 2012

	Academic track		Higher general track	
	(1)	(2)	(3)	(4)
Cohort year secondary education	2011	2012	2011	2012
# students in cohort	38766	38831	45243	46799
Female (%)	53.3	53.3	51.6	51.7
Migrant (%)	16.3	16.4	17.4	17.6
Low income parents (%)	10.1	10.4	15.1	15.8
Parental income percentile	65.3	65.4	57.0	56.7
	(26.3)	(26.6)	(26.6)	(26.9)
Students with single parents (%)	11.9	12.5	14.3	14.3
Number of siblings	1.31	1.29	1.37	1.36
	(0.90)	(0.90)	(0.97)	(0.95)
Secondary education				
Early transition (%)	2.2	2.3	0.0	0.0
Switch to Havo (%)	8.9	8.2		
Switch to Vwo (%)			3.8	5.2
Enrollment in higher education				
Total enrollment after four years (%)	96.0	95.8	81.1	82.1
Direct enrollment (%)	71.3	67.3	45.2	43.1
One or more gap years (%)	22.5	26.1	35.8	39.0
Distance parental home to educational institution (km)	48.9	49.4		
			33.1	33.1
Registered as living with parents (%)	64.1	74.3	72.7	75.5
Supplementary grant analysis (# students)	37845	37856	43772	45220
Eligible for max grant (%)	12.4	13.0	19.2	19.5
Eligible for suppl. grant below max grant (%)	10.6	12.7	14.2	17.1
Not eligible for suppl. grant (%)	76.9	74.2	66.6	63.4

Notes: Cohorts are constructed in secondary education three years before the nominal date of the final exam. The cohort of 2012 was the first cohort treated by the reform if students would graduate in the nominal time. Early transition measures whether a student enrolls in higher education before the nominal duration to the final exam. This is possible if students in the academic tracks switch to higher general track and graduate one year earlier. For students in the higher general track this is only possible is students skip a year.

Table 3. Estimates of the effect of the introduction of the loan system on enrolment in higher education within four years after nominal graduation

Dependent variable: Enrolment in higher education within four years after nominal graduation								
Estimation window	10 cohorts		+/- 3 cohorts		+/- 2 cohorts		+/- 1 cohort	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Academic Track								
Loan system	0.16**	-0.26	-0.19**	-0.29*	-0.27***	-0.04	-0.16	-0.16
	(0.07)	(0.16)	(0.08)	(0.17)	(0.10)	(0.22)	(0.14)	(0.14)
Observations	391,558	391,558	236,063	236,063	156,201	156,201	77,597	77,597
Baseline enrolment (%)	95.6	95.6	96.0	96.0	96.0	96.0	96.0	96.0
Higher General Track								
Loan system	1.77***	0.90***	1.83***	0.74**	1.44***	0.68*	1.03***	1.07***
	(0.12)	(0.29)	(0.15)	(0.31)	(0.18)	(0.40)	(0.26)	(0.26)
Observations	444,415	444,415	275,601	275,601	183,592	183,592	92,042	92,042
Baseline enrolment (%)	80.9	80.0	80.9	80.9	81.0	81.0	81.1	81.1
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is a dummy for enrolling in higher education within four academic years after the nominal graduation date. The main independent variable 'loan system' is a dummy which has value 1 for all students from the cohort year 2012 or more recent cohorts. Cohorts of students were constructed three years before the nominal graduation year. The estimates in columns (1) and (2) are based on 10 cohorts (7 before and 3 after the introduction of the loan system). Columns (3) and (4) use three cohorts before and three cohorts after the policy reform. Columns (7) and (8) compare the enrolment of the cohort of the schoolyear 2011-12 with the enrolment of the cohort of the schoolyear 2012-13. These were the nominal pre-cohort and the first nominal post-cohort. Controls include gender, parental income, migration background, single parent, number of siblings. Columns (2), (4) and (6) also include a quadratic time trend. The top panel uses the sample of students in the academic track of secondary education, the bottom panel uses the sample of students in the higher general track. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4. Placebo estimates of the effect of the loan system on enrolment in higher education within four years after the nominal graduation date.

	Dependent variable: Enrolment in higher education within four years after nominal graduation							
	2006/07	2007/08	2008/09	2009/10	2010/11	**2011/12**	2012/13	2013/14
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Academic Track								
Post-cohort	0.42***	-0.02	0.18	0.30**	-0.11	-0.16	-0.12	0.04
	(0.15)	(0.15)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)
Observations	78,523	79,627	80,123	79,962	78,406	77,597	77,795	78,504
Higher General Track								
Post-cohort	0.40	-0.37	-0.24	0.25	0.09	1.07***	0.68***	0.39
	(0.27)	(0.27)	(0.27)	(0.27)	(0.26)	(0.26)	(0.25)	(0.24)
Observations	84,427	85,174	86,853	88,011	89,252	92,042	94,340	95,548

Notes: The dependent variable is a dummy for enrolling in higher education within four academic years after the nominal graduation date. Cohorts of students were constructed three years before the nominal graduation year. Each column compares the enrolment in the post-cohort with the enrolment in the pre-cohort. The post-cohort in column (1) is 2007 and the pre-cohort is 2006. The 2012-cohort is the first nominal cohort treated with the loan system. All columns include controls for gender, parental income, migration background, single parent and number of siblings. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5. Estimates of the effect of the loan system on enrolment in higher education by migration background, parental income and gender using all cohorts as estimation sample.

	Dependent variable: Enrolment in higher education within four years after nominal graduation					
	Migration background		Low income		Female	
	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Academic Track						
Loan system	0.20	-0.35**	0.05	-0.30*	-0.40*	-0.11
	(0.48)	(0.17)	(0.62)	(0.16)	(0.22)	(0.24)
Observations	63,346	328,212	39,979	351,579	211,402	180,156
Higher General Track						
Loan system	0.79	0.93***	2.31***	0.64**	0.56	1.26***
	(0.72)	(0.31)	(0.80)	(0.30)	(0.39)	(0.42)
Observations	76,727	367,688	68,185	376,230	228,958	215,457

Notes: The dependent variable is a dummy for enrolling in higher education within four academic years after the nominal graduation date. The estimation sample consists of 10 cohorts (7 before and 3 after the introduction of the loan system). All estimates controls for gender, parental income, migration background, single parent, number of siblings and a quadratic timetrend. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6. Grant eligibility before and after the reform (Euro/month)

Students lives	Universal basic grant system		Social Loan System	
	With parents	Independently	With parents	Independently
	(1)	(2)	(3)	(4)
Maximum				
Fully eligible for supplementary grant	360	540	380	380
Not eligible for supplementary grant	100	280	0	0
Observed/ predicted grants	Observed grants in old system		Predicted grants in social loan system	
Secondary education track	Academic	Higher general	Academic	Higher general
Fully eligible for supplementary grant	410	370	380	380
Partially eligible for supplementary grant	340	310	290	290
Not eligible for supplementary grant	170	120	0	0

Notes: For the amounts in the top panel, see Table 1. The grant eligibility shown in columns (1) and (2) in the bottom panel is based on the observed grants of students in the cohort of 2011, the last cohort before the reform, during their first year in higher education. This is the average of obtained grants for students living with their parents or students living independently. The grant eligibility shown in columns (3) and (4) in the bottom panel is the predicted grants for students in the cohort 2011 in the social loan system.

Table 7. Difference-in-differences estimates of the effect of the introduction of the loan system on enrolment in higher education within four years after nominal graduation.

	Dependent variable: Enrolment in higher education within four years after nominal graduation							
Estimation window	10 cohorts		+/- 3 cohorts		+/- 2 cohorts		+/- 1 cohort	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Academic Track								
Partially eligible	0.36	0.42	0.25	0.25	0.24	0.21	-0.34	-0.35
	(0.41)	(0.41)	(0.47)	(0.47)	(0.55)	(0.55)	(0.77)	(0.77)
Non-eligible	0.77**	0.74**	0.65*	0.62*	0.24	0.19	0.04	0.03
	(0.32)	(0.32)	(0.36)	(0.36)	(0.43)	(0.43)	(0.61)	(0.61)
Observations	357,102	357,102	225,768	225,768	152,316	152,316	75,690	75,690
Higher General Track								
Partially eligible	0.71	0.78	0.98	0.88	0.80	0.59	1.40	1.17
	(0.55)	(0.55)	(0.65)	(0.65)	(0.76)	(0.76)	(1.07)	(1.07)
Non-eligible	0.02	-0.19	-0.36	-0.61	-0.81	-1.08*	-0.93	-1.25
	(0.44)	(0.44)	(0.51)	(0.52)	(0.60)	(0.60)	(0.85)	(0.85)
Observations	402,374	402,374	261,379	261,379	177,455	177,455	88,943	88,943
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is a dummy for enrolling in higher education within four academic years after the nominal graduation date. Each column shows a difference-in-difference estimate based on the specification of equation (2). Partially eligibile are students with parental income between 31,200 and 46,600 Euro; Non-eligible are students with parental income larger than 46,600 Euro. Cohorts of students were constructed three years before the nominal graduation year. The estimates in columns (1) and (2) are based on 10 cohorts (7 before and 3 after the introduction of the loan system). Columns (3) and (4) use three cohorts before and three cohorts after the policy reform. Columns (7) and (8) use one cohort before and one cohort after the reform. Controls include gender, parental income, migration background, single parent, number of siblings. Columns (2), (4) and (6) also include a quadratic time trend. The top panel uses the sample of students in the academic track of secondary education, the bottom panel uses the sample of students in the higher general track. The sample size differs from Table 3 because of missing values on the specific parental income variable which determines eligibility for the supplementary grant. *** p<0.01, ** p<0.05, * p<0.1

Table 8. Difference in differences estimates of the effect of the loan system on enrolment by migration background and gender.

	Dependent variable: Enrolment within four years after nominal graduation							
	Migration background				Female			
	Yes	No	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Academic Track								
Partially eligible	0.98	0.49	1.00	0.46	0.20	0.56	0.26	0.62
	(0.79)	(0.48)	(0.79)	(0.48)	(0.53)	(0.63)	(0.53)	(0.63)
Non-eligible	-0.36	1.27***	-0.36	1.17***	0.57	0.97**	0.55	0.90*
	(0.58)	(0.39)	(0.58)	(0.39)	(0.42)	(0.49)	(0.42)	(0.49)
Observations	52,695	304,407	52,695	304,407	192,577	164,525	192,577	164,525
Higher General Track								
Partially eligible	1.32	0.53	1.36	0.56	1.42*	-0.11	1.47**	-0.02
	(1.02)	(0.67)	(1.02)	(0.66)	(0.74)	(0.81)	(0.74)	(0.81)
Non-eligible	-0.53	0.10	-0.69	-0.11	0.47	-0.47	0.30	-0.68
	(0.80)	(0.55)	(0.80)	(0.55)	(0.59)	(0.65)	(0.59)	(0.65)
Observations	62,125	340,249	62,125	340,249	206,746	195,628	206,746	195,628
Controls	No	No	Yes	Yes	No	No	Yes	Yes

Notes: The dependent variable is a dummy for enrolling in higher education within four academic years after the nominal graduation date. The estimation samples consists of 10 cohorts (7 before and 3 after the introduction of the loan system). The left panel, columns (1) to (4), distinguishes students with a migration backgroud from native students. The righ panel, columns (5) to (8), focuses on samples of female versus male students. Controls include gender, parental income, migration background, single parent, number of siblings and a quadratic time trend. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 9. Estimates of the effect of the introduction of the loan system on the choice of the level and the type of higher education of students in the academic track

	Dependent variable: Enrolment in higher education within four years after nominal graduation							
Estimation window	10 cohorts		+/- 3 cohorts		+/- 2 cohorts		+/- 1 cohort	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Level Choice								
Loan system	3.41***	1.88***	4.31***	1.58***	3.44***	1.10**	2.25***	2.20***
	(0.15)	(0.35)	(0.18)	(0.38)	(0.22)	(0.50)	(0.32)	(0.32)
Observations	374,551	374,551	226,302	226,302	149,811	149,811	74,423	74,423
Baseline enrolment (%)			72.7	72.7	73.1	73.1	73.7	73.7
STEM Choice								
Loan system	4.43***	0.89***	2.97***	0.73**	2.34***	-0.23	1.03***	1.05***
	(0.14)	(0.33)	(0.17)	(0.36)	(0.22)	(0.47)	(0.31)	(0.30)
Observations	374,551	374,551	226,302	226,302	149,811	149,811	74,423	74,423
Baseline enrolment (%)			20.7	20.7	21.2	21.2	22.2	22.2
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The estimation sample consists of student in the academic track of secondary education. In the top panel the dependent variable is a dummy for enrolling in academic education versus higher professional education. In the bottom panel the dependent variable is enrolment in a STEM type of education. Controls include gender, parental income, migration background, single parent and number of siblings and time trends (if possible). *** p<0.01, ** p<0.05, * p<0.1

Table 10. Difference-in-differences estimates of the effect of the introduction of the loan system on the choice of the level and the type of higher education of students in the academic track

| Choice | Dependent variable: Enrolment within four years after nominal graduation | | | |
| | Academic level versus higher professional | | STEM | |
	(1)	(2)	(1)	(2)
Partially eligible	1.37*	1.42*	-0.44	-0.46
	(0.81)	(0.81)	(0.72)	(0.69)
Non-eligible	1.55*	0.96	1.16**	1.00*
	(0.62)	(0.61)	(0.56)	(0.54)
Observations	342,366	342,366	342,366	342,366
Controls	No	Yes	No	Yes

Notes: The estimation sample consists of student in the academic track of secondary education. In the left panel the dependent variable is a dummy for enrolling in academic education versus higher professional education. In the right panel the dependent variable is enrolment in a STEM type of education. Controls include gender, parental income, migration background, single parent and number of siblings and time trends. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix

Figure A.1 The probability of living with parents by distance between parental home and educational institution
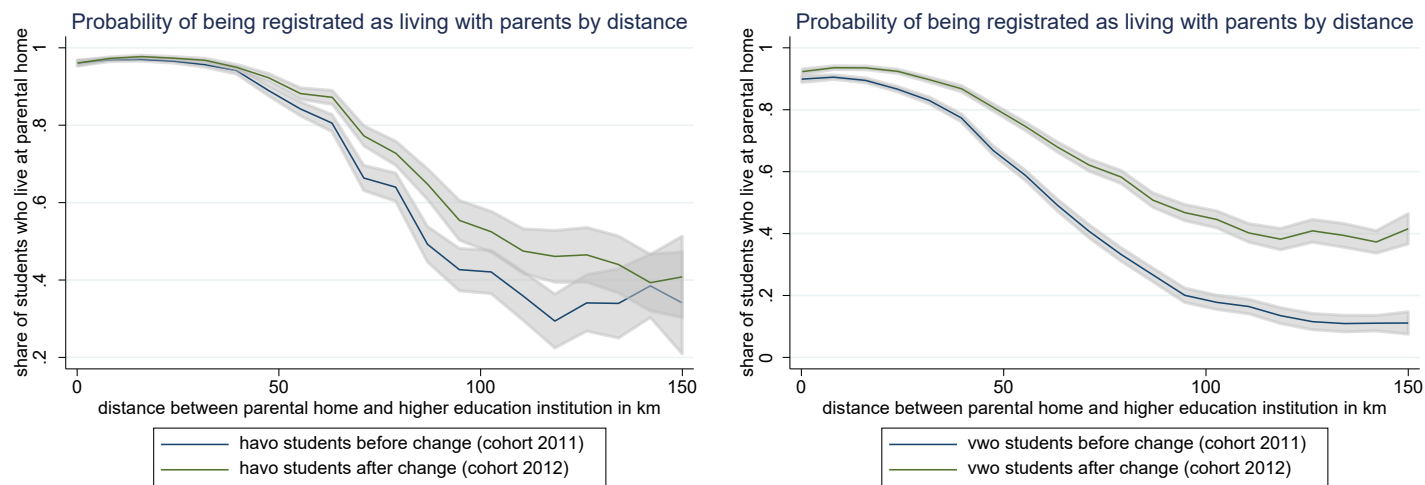
Notes: The information is based on whether students are registered as living with their parents. The top line in the figures refers to the registration after the reform. A concern with this registration is that, before the reform, students had a financial incentive to register not living with their parents as there was was a large difference in the basic grant for students living with their parents and students living independently. The reform eliminated the basic grant and also the incentive to register.

Table A.1: Academic track cohorts

Cohort year secondary education	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014
# students in cohort	37171	38697	39826	39801	40322	39640	38766	38831	38964	39540
Female (%)	54	55	55	55	55	54	53	53	54	53
Migrant (%)	16	16	16	16	16	16	16	16	17	17
Low income parents (%)	11	10	9	9	11	11	10	10	10	10
Parental income percentile	63	64	65	65	64	65	65	65	66	66
Students with single parents (%)	11	11	12	12	12	12	12	13	12	13
Number of siblings	1.38	1.36	1.34	1.34	1.32	1.32	1.31	1.29	1.28	1.27
Secondary education										
Early transition (%)	1.2	1.3	1.6	1.7	1.9	1.9	2.2	2.3	2.1	1.8
Switch to Havo (%)	6.6	6.6	6.8	7.0	8.0	8.6	8.9	8.2	7.6	6.5
Enrollment in higher education										
Total enrollment after 4 years (%)	94.8	95.2	95.6	95.6	95.8	96.1	96.0	95.8	95.7	95.8
Direct enrollment (%)	67.3	68.8	65.5	64.0	62.6	69.6	71.3	67.3	65.7	65.6
One or more gap years (%)	26.3	25.1	28.6	30	31.3	24.6	22.5	26.1	27.8	28.3
Distance parental home to educational institution	45.9	49.7	49.7	49.1	49.2	48.9	48.9	49.4	48.5	49.1
Registrated as living with parents (%)		60.7	61.6	61.9	60.2	61.0	64.1	74.3	76.3	77.2
Supplementary grant analysis (# students)						38619	37845	37856	38002	38595
Eligible for max grant (%)						13	13	13	11	10
Eligible for suppl. grant below max grant (%)						11	11	13	16	16
Not eligible for suppl. grant (%)						77	77	74	73	74

Notes: Cohorts are constructed in secondary education three years before the first option to enroll in higher education.

Table A.1: Summary statistics: Higher general track cohorts

Cohort year secondary education	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014
# students in cohort	41536	42104	42323	42851	44002	44009	45243	46799	47541	48007
Female (%)	52	51	51	51	52	52	52	52	52	51
Migrant (%)	16	17	17	17	17	17	17	18	18	18
Low income parents (%)	16	15	14	14	17	16	15	16	16	16
Parental income percentile	55	56	57	57	56	56	57	57	57	57
Students with single parents (%)	13	13	13	13	14	14	14	14	15	15
Number of siblings	1.45	1.43	1.42	1.41	1.39	1.39	1.37	1.36	1.34	1.32
Secondary education										
Early transition (%)	0	0	0	0	0	0	0	0	0	0
Switch to Vwo (%)	4.5	4.3	4.4	4.1	4.2	3.7	3.8	5.2	5.4	5.5
Enrollment in higher education										
Total enrollment after 4 years (%)	80.6	80.9	81.4	81.0	80.6	80.9	81.1	82.1	82.8	83.2
Direct enrollment (%)	45.6	45.0	44.8	43.0	41.7	43.7	45.2	43.1	44.6	44.1
One or more gap years (%)	34.9	35.8	36.6	38	38.9	37.2	35.8	39	38.1	39.2
Distance parental home to educational institution (km)	39.6	35.7	32.7	33.2	33.5	33.1	33.2	33.1	32.8	32.4
Registrated as living with parents (%)		69.6	70.3	69.7	69.3	70.9	72.7	75.5	76.9	77.7
Supplementary grant analysis (# students)						42571	43772	45220	45958	46307
Eligible for max grant (%)						19	19	19	16	15
Eligible for suppl. grant below max grant (%)						15	14	17	23	22
Not eligible for suppl. grant (%)						66	66	63	61	63

Notes: Cohorts are constructed in secondary education three years before the first option to enroll in higher education.

Table A.2 Estimates of the effect of the introduction of the loan system on enrolment in higher education using ten cohorts

| | Dependent variable: Enrolment in higher education within four years after nominal graduation | | | |
| Secondary education | Academic Track | | Higher General track | |
	(1)	(2)	(3)	(4)
Loan system	0.15**	-0.71***	1.75***	1.52***
	(0.07)	(0.12)	(0.12)	(0.21)
Observations	391,558	391,558	444,415	444,415
Baseline enrolment (%)	95.6		80.9	
Controls	Yes	Yes	Yes	Yes
Time trend				
Linear	No	Yes	No	Yes
Quadratic	No	No	No	No

Notes: The dependent variable is a dummy for enrolling in higher education within four academic years after the nominal graduation date. Cohorts of students were constructed three years before the nominal graduation year. The estimation sample consists of all ten cohorts. The left panel uses the sample of students in the academic track of secondary education, the right panel uses the sample of students in the academic track. Controls include gender, parental income, migration background, single parent, number of siblings. *** p<0.01, ** p<0.05, * p<0.1

Table A.3 Estimates of difference in pre-trends: academic track.

# cohorts before reform	Dependent variable: Enrolment in higher education				
	7	6	5	4	3
	(1)	(2)	(3)	(4)	(5)
timetrend	0.004	0.061	0.036	-0.014	-0.275
	(0.093)	(0.118)	(0.155)	(0.218)	(0.336)
Partially eligible	0.544	0.382	0.303	0.327	0.377
	(0.395)	(0.415)	(0.439)	(0.471)	(0.511)
Non-eligible	1.241***	1.117***	1.124***	1.131***	1.393***
	(0.320)	(0.337)	(0.358)	(0.384)	(0.418)
Partially*timetrend	0.167	0.056	-0.015	0.046	0.121
	(0.120)	(0.152)	(0.198)	(0.280)	(0.429)
Non-eligible*timetrend	0.161*	0.070	0.048	0.101	0.397
	(0.095)	(0.121)	(0.159)	(0.223)	(0.345)
Observations	242,692	211,968	179,375	145,370	111,358

Notes: The dependent variable is a dummy for enrolling in higher education within four academic years after the nominal graduation date. The estimation samples only include cohorts before the reform. The number of cohorts used is indicated in the column head. Alle models control for gender, parental income, migration background, single parent, number of siblings and a quadratic time trend.*** p<0.01, ** p<0.05, * p<0.1

Table A.3 Estimates of difference in pre-trends: higher general track.

# cohorts before reform	Dependent variable: Enrolment in higher education				
	7	6	5	4	3
	(1)	(2)	(3)	(4)	(5)
timetrend	-0.264**	-0.329**	-0.346	-0.299	-0.232
	(0.133)	(0.166)	(0.218)	(0.305)	(0.482)
Partially eligible	-0.338	-0.656	-0.650	-0.653	-0.522
	(0.575)	(0.602)	(0.636)	(0.680)	(0.737)
Non-eligible	3.821***	3.908***	3.862***	3.808***	3.948***
	(0.474)	(0.496)	(0.525)	(0.561)	(0.608)
Partially*timetrend	0.061	-0.153	-0.144	-0.107	0.041
	(0.173)	(0.216)	(0.286)	(0.404)	(0.639)
Non-eligible*timetrend	0.252*	0.316*	0.278	0.327	0.414
	(0.139)	(0.174)	(0.228)	(0.320)	(0.505)
Observations	264,955	230,955	195,996	160,317	123,960

Notes: The dependent variable is a dummy for enrolling in higher education within four academic years after the nominal graduation date. The estimation samples only include cohorts before the reform. The number of cohorts used is indicated in the column head. Alle models control for gender, parental income, migration background, single parent, number of siblings and a quadratic time trend.*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.4. Difference-in-differences estimates of the effect of the introduction of the loan system on enrolment in higher education within four years after nominal graduation.

	10 cohorts		+/- 3 cohorts		+/- 2 cohorts		+/- 1 cohort	
Estimation window								
Dependent variable: Enrolment in higher education within four years after nominal graduation								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Academic Track								
Partially or Non-eligible	0.698**	0.685**	0.586	0.565	0.223	0.178	-0.026	-0.029
	(0.317)	(0.317)	(0.364)	(0.364)	(0.425)	(0.424)	(0.606)	(0.606)
Observations	357,102	357,102	225,768	225,768	152,316	152,316	75,690	75,690
Higher General Track								
Partially or Non-eligible	-0.078	-0.181	-0.285	-0.482	-0.632	-0.894	-0.691	-0.966
	(0.435)	(0.436)	(0.511)	(0.511)	(0.600)	(0.599)	(0.842)	(0.842)
Observations	402,374	402,374	261,379	261,379	177,455	177,455	88,943	88,943
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is a dummy for enrolling in higher education within four academic years after the nominal graduation date. Each column shows a difference-in-difference estimate based on the specification of equation (2). Partially eligible are students with parental income between 31,200 and 46,600 Euro; Non-eligible are students with parental income larger than 46,600 Euro. The difference with table 7 is that these students are now included in one group (dummy). Cohorts of students were constructed three years before the nominal graduation year. The estimates in columns (1) and (2) are based on 10 cohorts (7 before and 3 after the introduction of the loan system). Columns (3) and (4) use three cohorts before and three cohorts after the policy reform. Columns (7) and (8) use one cohort before and one cohort after the reform. Controls include gender, parental income, migration background, single parent, number of siblings. Columns (2), (4) and (6) also include a quadratic time trend. The top panel uses the sample of students in the academic track of secondary education, the bottom panel uses the sample of students in the higher general track. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.5 Difference-in-differences estimates of the effect of the introduction of the loan system on enrolment in higher education within four years after nominal graduation **for non-eligible students in the two lowest income quartiles**

	Dependent variable: Enrolment in higher education within four years after nominal graduation							
Estimation window	10 cohorts		+/- 3 cohorts		+/- 2 cohorts		+/- 1 cohort	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Academic Track								
Lowest quartile non-eligible	0.668*	0.751**	0.598	0.609	0.507	0.466	0.752	0.748
	(0.355)	(0.355)	(0.409)	(0.409)	(0.480)	(0.480)	(0.685)	(0.684)
Second lowest quartile non-eligible	0.775**	0.791**	0.451	0.443	-0.042	-0.086	-0.251	-0.272
	(0.345)	(0.345)	(0.395)	(0.394)	(0.462)	(0.462)	(0.661)	(0.661)
Observations	357,102	357,102	225,768	225,768	152,316	152,316	75,690	75,690
Higher General Track								
Lowest quartile non-eligible	-0.151	-0.024	-0.066	-0.167	-0.107	-0.333	0.253	-0.081
	(0.493)	(0.492)	(0.580)	(0.580)	(0.683)	(0.682)	(0.962)	(0.962)
Second lowest quartile non-eligible	0.240	0.128	-0.126	-0.292	-0.875	-1.039	-0.956	-1.224
	(0.495)	(0.495)	(0.582)	(0.582)	(0.687)	(0.686)	(0.968)	(0.968)
Observations	402,374	402,374	261,379	261,379	177,455	177,455	88,943	88,943
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: This table shows estimates of difference-in-differences models as specified in Equation (2). The group of non-eligible students has been divided in four income quartiles. Hence, the model includes 4 dummy variables for the income quartile of non-eligible students and also four interaction of treatment and income quartile. The model also include a dummy for the partially eligible students and the interaction with treatment. This table shows the difference-in-differences estimates for non-eligible students with parental income in the relatively lowest income quartiles (just above 46,600 Euro). The specifications used are the same as in Table 7. *** p<0.01, ** p<0.05, * p<0.1

Table A.6 Estimates of the introduction of the loan system on taking a loan, working and number of working hours

| | Higher General Track | | | | Academic Track | | | |
| | Loan | Max Loan | Working | Hours working | Loan | Max Loan | Working | Hours working |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Loan system	15.72***	1.96***	1.05***	1.55***	24.92***	6.09***	-0.99***	0.61***
	(0.4)	(0.3)	(0.4)	(0.2)	(0.36)	(0.4)	(0.4)	(0.2)
Observations	69,294	57,432	69,294	69,294	70,777	55,212	70,777	70,777
Baseline	40.4	24.8	59.8	22.2	31.8	32.9	45.0	15.3

Notes: Each column shows the estimated effect of a dummy for the first cohort after the reform on a specific outcome; loan, maximum loan, working and monthly hours of work. The outcomes are measured in the first year of enrolment. Columns (1) to (4) focus on students from the higher general track, columns (5) to (8) focus on students from the academic track. All columns include controls for gender, parental income, migration background, single parent and number of siblings. The estimation sample consists of two cohorts. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix B. Eligibility for the supplementary grant

The eligibility for the supplementary grant in Dutch higher education depends on several factors such a parental income and number of siblings. In determining the size of the supplementary grant the government calculates the so-called expected parental contribution (p) for each student based on the eligibility criteria. The amount of supplementary grant (S) is then calculated using the following formula:

$$S = max\{0, \text{max\_}grant - p\} \text{ with } p = 0 \text{ if } p < 0 .$$

This formula creates variation in the amount of the additional monthly grant a student may receive between zero and the maximum value, which was 380 Euro in 2015.

Students with low-income parents are eligible for the maximum amount of the grant. For these parents the expected contribution is zero. The eligible amount starts to decrease when parents earn more than approximately 33,800 Euro per year (this is based on the year 2015, the precise amount of deductible income is adjusted yearly) and reduces to zero when the expected parental contribution increases.

Appendix C. Switching tracks to avoid the social loan system

Switching tracks

Students in the higher general track are allowed to enroll in the fifth year of the academic track after their final exam. This route gives students the additional option to enroll in university. By taking this route students need seven years (five years in general higher education plus two years in the academic track) instead of the nominal six years for graduating in the academic track. To avoid the social loan system students might decide not to take this longer route towards higher education and to enroll directly after graduating in the higher general track. This is relevant for students in the 2010-cohort as two additional years in the academic track would mean that they would be treated by the social loan system. As such, we would expect that a smaller proportion of students from the 2010-cohort would take this indirect route compared to earlier cohorts. In Table A.1 in the appendix we observe that the proportion choosing for the academic track reduces from 4.2 % to 3.7 % which is consistent with this expectation.

Students in the academic track may also try to avoid the social loan system by switching tracks. For instance, students who need more time than the nominal duration due to retention or the risk of not passing the final exam might decide to switch to the general higher track. This is most relevant for the 2011 cohort as an additional year in the academic track would lead to treatment by the social loan system. Consistent with this expectation we observe an increase in the proportion of students switching to the higher general track in the cohorts before the reform; 8.0 % in 2009, 8.6 % in 2010, 8.9 % in 2011 and 8.2 % in 2012. Another option is that students in the academic track switch to the higher general track and graduate a year earlier than their old classmates. However, this 'early transition' does hardly occur (see Table A.1).