Student loan choice architecture matters: dynamic effects of a default loan amount change and of a change in the online loan application screen. *

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Abstract

The effect of two changes in the student loan choice architecture (nudges) on borrowing behavior and study success are studied, using detailed registry data including all students in higher education between 2004 and 2015 in the Netherlands. DUO, the Dutch executive governmental institution on education which is responsible for providing the student financing, (1) changed the amount a student borrows by default (automatically) when the eligibility for the performance-related grants has expired and (2) removed the 'maximum borrowing tick box' from the loan application screen. A quasi-experimental design is employed to estimate the (dynamic) impact of the default option change and the removal of the tick box on borrowing behavior, by means of differences-in-differences estimations. The default loan amount change resulted in an average reduction of the requested loan amount of approximately €330 (72%) in the first month and €1200 (46%) in the first year for students who did not borrow when they were still eligible to receive grants. The removal of the tick box reduced the chance first-time borrowers requested the maximum loan amount with 30 percentage points. However, the impact on borrowing behavior was more modest: on average the requested loan amount was reduced by approximately 8.8%. The impact of student borrowing behavior on study success is estimated by employing an instrumental variable analysis, exploiting the exogenous variation in borrowing behavior created by the default loan amount change. Though there are certain limitations attached to this approach, it can be concluded that the reduction in the requested loan amounts at least did not lead to a reduction in average study success. The results indicate the design of the choice architecture has a substantial impact on decision-making in the context of student loans, and that default options are strong tools which have a non-neutral impact on behavior, not only with respect to one-time decisions, but also in a context where individuals can adjust their decision at frequent intervals facing negligible transaction cost.

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Contents

List of Tables

List of Figures

1 Introduction

In the Netherlands, the government provides substantial financial aid to students in higher education in the form of grants and loans, which together comprise the student finance system. Student financing (borrowing) is an important topic from a social and policy perspective, since the student finance system enables financially constrained young adults to enroll in higher education (Wet Studiefinanciering 2000, 1999). However, research has shown the decision-making, including borrowing behavior, of students is substantially influenced by behavioral biases such as debt aversion (Field, 2009; Caetano et al., 2011; Boatman et al., 2017), self-control issues (Cadena and Keys, 2013) and framing effects (Pallais, 2015). Consequently, the design of the choice architecture with respect to student borrowing decisions can reinforce or weaken the behavioral biases of students, and subsequently influence economic outcomes.[1] This paper studies the impact of two recent changes in the choice architecture (nudges) in the student finance/loan system in the Netherlands on borrowing behavior and study success.

The first nudge came into effect as of the academic year 2009/2010 and it affected students whose entitlement to the performance-related education grants has expired (i.e., entered the loan phase). This nudge is referred to as a change in the default option since DUO, the Dutch governmental institution responsible for providing the student grants and loans, changed the amount a student entering the loan phase borrowed by default. A default option is defined as a pre-set course of action that takes effect if nothing is specified by the decision maker (Thaler and Sunstein, 2008).[2] The second nudge was the removal of the 'maximum borrowing tick box' from the student loan application screen. This nudge came into effect in March 2014. Previously, the maximum borrowing tick-box had a prominent place on the web page when applying for a grant/loan. If students wanted to borrow less than the maximum amount, students had to tick another box and manually fill in their loan amount. After March 2014, students could only manually fill in the amount they wanted to borrow; the tick box was removed.

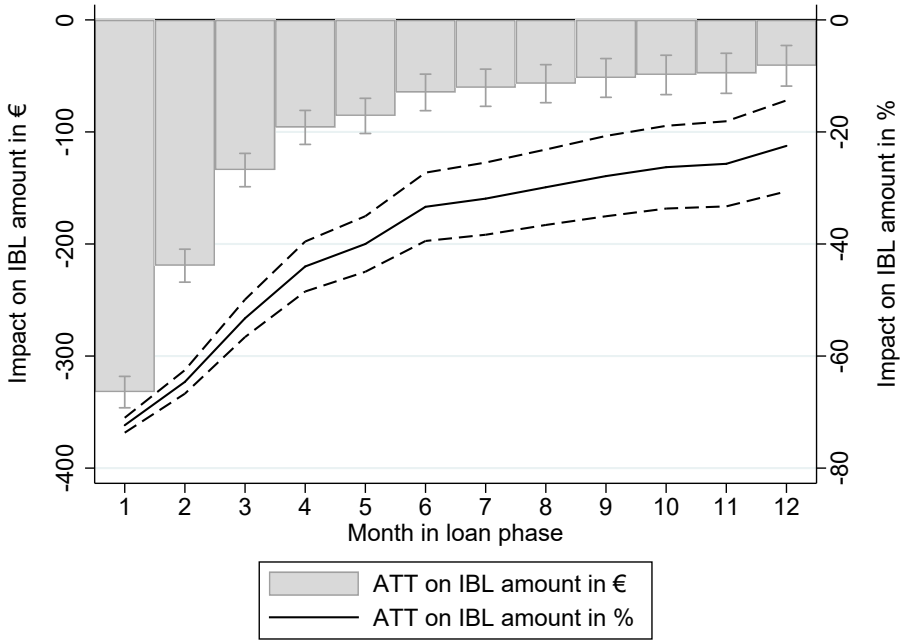
Van der Steeg and Waterreus (2015) did a preliminary analysis on the effect of the two nudges, by means of aggregated data and a relatively simple comparison of student borrowing behavior of cohorts before and after the default changes. They find that the removal of the maximum borrowing tick reduced the average requested loan amount for first year students who borrow with 8%. Furthermore, the chance these students request the maximum loan amount is reduced with 30 percentage points.[3] The analysis by Van der Steeg and Waterreus (2015) of the impact of the default loan amount change is based on imperfect information with respect to the timing and the heterogeneous impact of the nudge for different subgroups, and consequently

[1] Choice architecture is a term coined by Thaler and Sunstein (2008), and refers to the fact that there are multiple ways to present a choice to a decision-maker and that the ultimate decision is influenced by the manner in which the choice is presented.

[2] Note that a default option is defined (in this context) as a pre-set course of action that takes effect if nothing is specified by the decision maker. Default does not refer to the failure of paying the interest or principal on a loan in this paper.

[3] These results have been featured in the 2018 Budget Memorandum of the Netherlands (Rijksoverheid, 2017).

Figure 1. Impact of the default loan amount change in the first 12 months after entering the loan phase for students who did not borrow in the grant phase.



Note: This figure plots the results from the differences-in-differences specification without control variables. The left y-axis reports the impact of the treatment on the requested IBL in €. The right y-axis presents the percentage change (relative to the estimated counterfactual of the treatment group) of the in the requested interest-bearing loan amount. The vertical lines and the dashed lines represent the 95% confidence interval.

results in inaccurate estimates. First, this paper improves on the brief analysis by Van der Steeg and Waterreus (2015), by taking into account the differences in timing and impact of the default loan amount change for different subgroups and thereby providing the causal impact of this nudge on borrowing behavior. In addition, this paper improves on Van der Steeg and Waterreus (2015), by employing more advanced (quasi-experimental) empirical methods enabled by the detailed micro registry data, including student finance data and extensive background characteristics of all students in higher education between September 2004 and August 2015.

By exploiting the difference in timing of the default loan amount change for different subgroups in a differences-in-differences (DD) design, this study finds the default loan amount change resulted in an average reduction of the requested loan amount of approximately €330 (72%) in the first month and €1200 (46%) in the first year for students who did not borrow when they were still eligible to receive grants. The impact of the default loan amount change is visualized in Figure 1. The impact of the removal of the tick box is estimated by considering the borrowing behavior of first-time borrowers in the months around the removal of the the tick box, also using a DD design. On average, the loan amount of first-time borrowers is reduced by just over €30 (8.8%) and the chance first-time borrowers request the maximum loan amount is reduced with 30 percentage points, in the first month.

The (maximum) loan amounts or eligibility for student finance was not influenced by both

nudges, in other words, the budget constraint of the student was not affected by both changes. Consequently, (standard) human capital theory, as introduced by Mincer (1958; 1962), Schultz (1963) and Becker (1994), is insufficient to explain this substantial impact on borrowing behavior. Insights from behavioral economics do provide (non-mutually exclusive) mechanisms through which economic outcomes can be affected by both nudges, but default option changes in particular: (1) the default (and the tick box) can be regarded as an implicit recommendation by the default setter (McKenzie et al., 2006; Tannenbaum and Ditto, 2011; Altmann et al., 2017), (2) status quo bias, inertia and procrastination of decision making can cause individuals to stick to the default option (see, for example, O'Donoghue and Rabin 2001; Tversky and Shafir 1992; Madrian and Shea 2001), and (3) the default option and the tick box can be regarded as an anchor or reference point from which insufficient adjustment is made (Jetter and Walker, 2016; Tversky and Kahneman, 1975; Marx and Turner, 2017).

In addition, both nudges essentially create an exogenous shock to student borrowing behavior, which can be exploited in an instrumental variable estimation to estimate the relation between borrowing behavior and study success. However, due to a weak first stage and data limitations associated with using the removal of the tick box as the instrument, only the default loan amount change can be credibly used as an instrument in a 2SLS estimation of the effect of borrowing behavior on study success. Study success is measured by the speed in which a student earns a bachelor or master diploma after entering the loan phase. The main conclusion that can be drawn with respect to the consequences for study success is that the substantial reduction in the requested loan amounts caused by the nudges at least did not have a negative impact on study success.

In theory, borrowing behavior can influence study success if students face trade-offs between spending their time working or studying, nudging students to borrow less (more) could increase (decrease) the time spend on work on consequently decrease (increase) study success.[4] On the other hand, it is also possible that nudging students to borrow less (more) only causes students to take on less (additional) debt, without having an effect on the time spend on studying and/or study success (Lochner and Monge-Naranjo, 2012; Avery and Turner, 2012).

Even though student debt is reduced and study success is not negatively affected, it cannot be concluded, based on the results of this paper, that reducing student debt is a goal policy makers should pursue in general. First, there are significant limitations attached to the estimation of the impact on study success: (1) this paper only looks at the chance to obtain a bachelor or master diploma (within a certain amount of months) after entering the loan phase as a measure of study success (grades and credits earned are not considered due to data limitations), and (2) the 2SLS estimates are only informative about a relatively small group of students (a local average treatment effect is estimated). Second, this study estimates the impact of study success

[4]Over 60% of the Dutch students (16-29 years old) combine working and studying, and just under 50% of the working students work in a different field than their study field Quintini (2015). At the same time, 34% of the respondents of a survey among Dutch students say that they borrow money from DUO in order to work less while studying (Nibud, 2017).

for students who are already enrolled in higher education, and have exceeded their nominal study duration. Consequently, the results might not be informative about students in earlier stages of their study and/or potential higher education students. Third, estimated returns to college completion imply that borrowing to finance college is generally optimal from a lifetime perspective for a young adult (Avery and Turner, 2012). Jacobs and Webbink (2006) show that the higher education wage premium in the Netherlands is substantial and has been increasing between 1989 and 2002.

On the other hand, a significant number of students do face problems regarding repayment of their student debt. In 2015, 13% (approximately 90,000) of the repaying students were unable to repay their monthly obligatory repayments due to insufficient income, this amount has quadrupled since 2009 (Trouw, 2017). Furthermore, in 2015, 20% of the students were in arrears with repayments (NOS, 2015). Approximately 10% of the total student debt is not repaid (CPB, 2014).

The main lesson that can be drawn from the results of this study, with respect to both nudges, is that the design of the choice architecture has a substantial impact on borrowing behavior through influencing behavioral biases of students. With respect to the default option change, this study fits in the growing body of evidence that default options are strong tools that have a non-neutral impact on behavior (see, for example, Johnson and Goldstein, 2003; Madrian and Shea, 2001; Beshears et al., 2009; Johnson et al., 2002). However, most studies on the behavioral impact of default options are in a context where a decision is made only once. An important distinguishing feature of this study is that it provides evidence for the dynamic effects of default option changes: the impact on behavior remains economically and statistically significant in a context where individuals can adjust their decision at frequent intervals facing negligible transaction cost. Furthermore, the pattern of these dynamic effects suggest that procrastination seems to be a significant driver of the impact of the default option change on decision-making/behavior, which is in line with previous work (Beshears et al., 2009; Van Rooij et al., 2008; Brown and Previtero, 2014). Consequently, this suggests that addressing procrastination directly and stimulating active choice, for example, by: forcing choices, changing the incentives around deadlines, or increasing the salience of future payoffs (Brown and Previtero, 2014), could lead to more deliberate and well-thought-out borrowing decisions.

The remainder of the paper is organized as follows. Section 2 describes the student finance system in the Netherlands, the two changes to the student loan choice architecture and the rationale behind these two changes. Human capital theory and insights from behavioral economics are discussed in Section 3, providing the potential mechanisms through which both nudges can influence borrowing behavior. Section 4 outlines the empirical strategies employed to estimate the effect of both nudges on student borrowing behavior, and of student borrowing behavior on study success. The data used in this quasi-experimental study is described in Section 5. Section 6 presents the results. Section 7 discusses the relation between the results and the theoretical mechanisms, and the position of this paper in the literature. Section 8 concludes.

2 Description of Treatments & Political Background

This section first describes the student financing system in the Netherlands, in place during the time span of interest (September 2004 to August 2015), in order to provide the relevant institutional context of the two treatments. Subsequently, the two treatments, changes in the design of the student loan choice architecture, are described in detail. Finally, the rationale and the political context of the two treatments is discussed.

2.1 Student finance system in the Netherlands.

The government of the Netherlands provides grants and loans to full-time or dual higher education students over the age of 18 and under the age of 30. This system of loans and grants is referred to as student finance. In order to be eligible for student finance, the student must have the rights of a Dutch citizen (DUO, 2017).[5] Higher education in the Netherlands includes higher vocational education and universities.[6] Dienst Uitvoering Onderwijs (hereafter: DUO) is the Dutch executive governmental institution on education which is responsible for providing the student financing. DUO is a government agency of the Ministry of Education, Culture and Science of the Netherlands. The goal of the Dutch government is to financially support students who are in need of additional funds on top of their (potential) income from work and/or (expected) contribution from their parents (Wet Studiefinanciering 2000, 1999).

The student finance system consists of five components (DUO, 2009):[7]

1. The basic grant ('basisbeurs'). All students that meet the requirements related to the performance-related grant (discussed below) receive the (monthly) basic grant. The size of this grant only depends on the living situation of the student: students who live with their parents receive a lower basic grant compared to students who do not live with their parents.

2. The supplementary grant ('aanvullende beurs'). This (monthly) grant is also part of the performance-related grant, however, not all students are eligible to receive the supplementary grant. The eligibility and size of this grant depends on (1) the income of the parents of the student, (2) the amount of siblings eligible for the supplementary grant and (3) the amount of other children under the care of the parents of the student.

[5] The rights of non-Dutch nationals depends on the residence permit or nationality.

[6] It is also possible to receive student finance when enrolled in certain study programs of intermediate vocational education (MBO). However, due to data limitations only students enrolled in higher educations are included in this study.

[7] The student financing system was reformed as of the 1st of September, 2015: the basic grant was abolished (among other reforms). Even though this reform had a substantial impact on the student loan system, it will not be discussed in detail since it is outside the time period studied in this paper. The system described in this section is the system in place prior to this reform. During the time span of the data set used in this paper (September 2004 - August 2015), there were no significant changes in the maximum amounts of the different components of student finance, besides the introduction of the tuition fee credit in September 2007 and the yearly upward adjustments based on the inflation rate.

3. The student travel product ('studentenreisproduct'). The travel product is part of the performance-related grant and the student is eligible to receive the student travel product as long as the student is eligible for student finance. Students can choose between a week product (free public transport from Monday to Friday and a 40% discount during the weekend) and weekend product (free public transport during the weekend and a 40% discount from Monday to Friday). The student travel product can be used for the large majority of public transport facilities within the Netherlands.

4. The interest-bearing loan ('rentedragende lening'). On top of the basic and supplementary grant, students can borrow money at a reduced interest rate from the government (DUO). Students are allowed to borrow for as long as they are eligible for student finance. The requested borrowed amount can be adjusted monthly. The maximum amount that students are allowed to borrow depends on (1) whether the student is still eligible to receive the basic and (potentially) supplementary grant, and (2) the amount of supplementary grant received.[8] Students have to start paying back this loan including interest one year after they have completed their studies.[9]

5. The tuition fee credit ('collegegeldkrediet'). The tuition fee credit, introduced in September 2007, is an additional interest-bearing (monthly) loan on top of the other student finance components meant for assisting the financing of the tuition fee. The maximum amount students can request only depends on whether the student pays the statutory or institutional tuition fee.[10] The repayment regulation of this loan is identical to to the (standard) interest-bearing loan (component 4).

The (1) basic grant, (2) supplementary grant and the (3) student travel product make up the performance-related education grant ('prestatiebeurs'). It is a performance-related grant since the grant technically remains a loan until the student receives a diploma within 10 years. If the student does not manage to complete the study successfully within 10 years, the student must repay the received education grant. Students in higher education are eligible for the performance-related education grant during the nominal period of the study, this phase is referred to as the grant phase. If the student exceeds the nominal study period, the student is only eligible to receive components 3-5 of student finance for an additional three years on top of the nominal duration of the study, this period is referred to as the loan phase. Most bachelor

[8]Students are always able to borrow (by means of the interest-bearing loan) the difference between the maximum amount and the amount of the supplementary grant they received. This ensures that all students (with the same living-situation) have the same 'budget' (the potential total amount received from DUO), regardless of the income of the parents.

[9]Students are allowed to voluntarily start repaying the loan before they are required to do so.

[10]The statutory tuition fee is the tuition fee set by the government at a yearly basis, this tuition fee is paid by most students and is identical across studies and higher education institutions. The institutional tuition fee is paid by students who do not meet the conditions relating the the statutory tuition fees, this is because universities do not receive government funding for this population of students. The institutional tuition fee is not allowed to be lower than the statutory tuition fee and depends on the study program and university (VSNU, 2016).

Table 1. Maximum amounts for the interest-bearing loan, in euro.

	Grant phase				Loan phase
	Living with parents		Not living with parents		
Year	Max. SG	Max. loan	Max. SG	Max. loan	Max. loan
2008	209	488 - SG	228	507 - SG	819
2009	214	496 - SG	233	516 - SG	832
2010	221	509 - SG	241	528 - SG	853
2011	223	509 - SG	243	528 - SG	853
2012	225	509 - SG	245	528 - SG	853
2013	232	520 - SG	252	541 - SG	873
2014	239	533 - SG	260	554 - SG	895
2015	245	547 - SG	267	568 - SG	917

Note: Max. is an abbreviation for maximum, SG is an abbreviation for supplementary grant. Year is the calendar year. The amounts are rounded off to zero decimal places. Source: DUO.

programs in higher vocational education and university have a nominal duration of respectively 4 and 3 years, most master programs have a nominal duration of 1 or 2 years. To illustrate: a student of study with a nominal duration of 4 years, is eligible to receive student finance for 7 years. However, the student only receives the basic and (potentially) the supplementary grant during the first 4 years (the grant phase). After 4 years, the student enters the loan phase and is still eligible for the student travel product, the interest-bearing loan and the tuition fee credit for a period of 3 years. It must be noted that the amount of years a student is eligible to receive the travel product after the nominal study duration was reduced from 3 years to 1 year as of September 2012 (Wijziging Wet Studiefinanciering 2000, 2012). However, this change is mostly irrelevant for this paper since it happened outside of the period of analysis of the two treatments.

The fourth component, the interest bearing loan (IBL), is the main focus of this study, since both treatments involve this component. Note that this component is completely independent from the tuition fee credit (component 5). Table 1 presents the maximum possible loan amounts (for a selected number of years), which differ by (1) whether or not the student is in the grant phase or loan phase and (2) the amount of supplementary grant received. Students can apply for the grants and/or request/adjust the interest-bearing loan at any time via a personalized online web page, this is a relatively simple and quick procedure (with negligible transaction/effort costs). If a student requests or adjusts the requested student finance amounts, the amounts are automatically applied to the subsequent months, unless the student adjusts the requested amounts or the eligibility changes in later months. For example, if a student requests a monthly interest-bearing loan amount of €100 in September, this amount will be automatically paid out in the subsequent months as well, unless the student adjusts the requested loan amount or loses eligibility for student finance.

The repayment of the interest-bearing loan and the tuition fee credit (including interest)

must be done within 15 years after the completion of the study, in the form of monthly repayments. The interest rates are low compared to the market interest rates and monthly repayments cannot be higher than 12% of earned income. If a student earned less than 84% of the statutory minimum wage, no repayments are required. The residual debt is ultimately forgiven after 15 years if the income of the student was insufficient during the repayment period.[11]

2.2 Description of the treatments.

Section 2.2 describes the two treatments studied in this paper in detail.

2.2.1 Treatment 1: Changes in the standard (default) loan amount when entering the loan phase. The first change to the student loan choice architecture came into effect as of the academic year 2009/2010 and it affected students whose entitlement to the performance-related education grant has expired (i.e., entered the loan phase). This nudge is referred to as a change in the default option since DUO changed the amount a student entering the loan phase borrowed by default. A default option is defined as a pre-set course of action that takes effect if nothing is specified by the decision maker (Thaler and Sunstein, 2008).[12] Even though all students entering the loan phase were affected, the default change had a different impact on different subgroups of students, depending on their requested loan amount in the last month of the grant phase.[13] Table 2 illustrates the impact of the treatment on the different groups.

First, consider the standard loan amount for students entering the loan phase before the treatment. Students with no loan (group 1) in the last month of the grant phase, by default received the maximum monthly loan amount in the loan phase (€819 in 2008, see the last column of Table 1). Students with a requested loan amount lower than the maximum loan amount in the last month of the grant phase, continued to receive this requested loan amount in the grant phase by default (group 2). The same applies to students students with a requested loan amount of €0 (group 4). Important to note is the difference between students with no loan (group 1) and a loan of €0 (group 4). Students with a loan of €0 actively requested a loan of €0, whereas students with no loan did not apply for a loan. Even though both groups receive the same amount as an interest-bearing loan (€0), the students in group 4 were considered similarly to students with a positive loan amount lower than the maximum loan amount (group 2), and thus received €0 by default when entering the loan phase. Furthermore, due to the design of the application procedure, it is by construction not possible to be in group 1 while receiving

[11] Note that the repayment regulations have been changed as of September 2015, together with the reforms of the student finance system, see footnote 7. These new repayment regulations are not relevant for this study, since they were introduced after the time period studied in this paper.

[12] Note that a change in the default loan amount refers to the change in the standard loan amount which the student borrows if no action is taken (the definition used in the behavioral economics literature). Default does not refer to the failure of paying the interest or principal on a loan (the definition used in the finance literature).

[13] The interest-bearing loan only refers to component four of the study finance system described in Section 2.1. The tuition fee credit (component five) is an optional additional (interest-bearing) loan which does not influence the treatment effects described in this section.

Table 2. Change in the default (interest-bearing) loan amount when entering the loan phase.

	Grant phase	Loan phase		
		Pre-treatment	Time of treatment	Post-treatment
Group 1	No loan	Max. loan	September 2009	Sum of the grants and the interest-bearing loan received in the grant phase
Group 2	Loan of € x < max	Loan of € x	March 2010	
Group 3	Max. loan	Max. loan	March 2010	
Group 4	loan of € 0	Loan of € 0	March 2010	

Note: The amounts displayed in this table are the interest-bearing loan amounts (component three of the student finance system described in Section 2.1). The requested loan amount in the last month of the grant phase determines the default loan amount in the loan phase. Max. = maximum, see Table 1 for the maximum loan amounts.

a supplementary grant. Consequently, students who are eligible to and actually receive the supplementary grant, but do not wish to have an interest-bearing loan from DUO, will be in group 4. Lastly, students who borrow the the maximum amount they are allowed to borrow (which depends on their living situation and the amount of supplementary grant received) in the last month of the grant phase (group 3), received the maximum loan amount that is applicable in the loan phase.[14]

The system described above was changed and this change is the first treatment which is studied in this paper. The system after the treatment was similar for all groups: all students received the sum of the basic grant, (if applicable) the supplementary grant and the requested interest-bearing loan amount received in the last month of the grant phase as the default/standard interest-bearing loan in the loan phase. It is important to note the difference in the time of the treatment: group 1 was treated as of September 2009, whereas groups 2-4 were treated 6 months later, as of March 2010. This difference in the time of the treatment is essential, since it enables the use of a differences-in-differences identification strategy, disucced in Section 4.1.

Table 3 provides the range of the changes in the default loan amounts for the different groups, which differ by living situation and the amount of supplementary grant received. This difference is caused by the fact that these factors influence the maximum allowed loan amount and/or the amount of basic and supplementary grant, which in turn affects the standard loan amount received when entering the loan phase Appendix A provides a more detailed numerical description of the treatment effects. Note that only the cases for no or maximum amount of supplementary grant are presented in Table 3, in reality the student can receive any amount from €0 up to the maximum amount of supplementary grant (see Table 1). Table 3 therefore provides upper and lower bounds of impact of the treatment for the different groups. The treatment had the biggest impact on students in group 1, who went from receiving a default loan amount equal to the maximum loan amount to the amount of basic grant received in the last month of the

[14]Note that this is the same default loan amount of group 1.

Table 3. Summary of change in the default loan amounts for different subgroups, upper and lower bounds.

	Living with parents		Not living with parents	
	No SG	Max SG	No SG	Max SG
Group 1	-727	NA	-564	NA
Group 2	93	307	260	493
Group 3	-243	-243	-57	-57
Group 4	93	307	260	493

Note: Max = maximum, SG = supplementary grant, NA= Not applicable. The amounts are in euro and rounded off to zero decimal places. Source: DUO.

grant phase (which only depends on the living situation of the student).[15] Students in group 3 also received a lower loan amount by default, since the maximum loan amount in the loan phase is higher than the sum of the (1) basic grant, (2, if applicable) supplementary grant and (3) maximum loan amount received in the last month of the grant phase (see Table 1). Groups 2 and 4 receive a higher loan amount by default, since after the treatment, the basic and (if applicable) supplementary grant is added to the loan amount these students received by default when entering the loan phase.

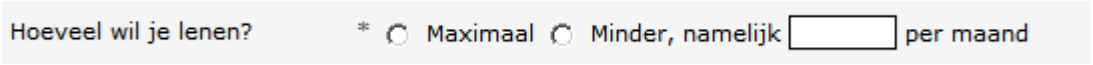
Students were informed 1 month in advance by DUO about the fact they were entering the loan phase, accompanied by information about the standard/default loan amount they will receive when entering the loan phase. The content of this letter did not change significantly as a result of the treatment.

2.2.2 Treatment 2: Removal of the 'maximum borrowing tick box' from the application screen. The second change to the design of the student loan choice architecture was the removal of the 'maximum borrowing tick box' from the student loan application screen. This nudge came into effect in March 2014. Previously, the maximum borrowing tick box had a prominent place on the web page when applying for a grant/loan. If students wanted to borrow less than the maximum amount, students had to tick a second box and manually adjust their loan amount. After March 2014, students could only manually fill in the amount they wanted to borrow; the tick box was removed from the application screen. See Figure 2 for screenshots of the student finance application screen before and after the treatment. The removal of the tick box did not influence the maximum amount students could borrow: students could still request the same maximum loan amount.

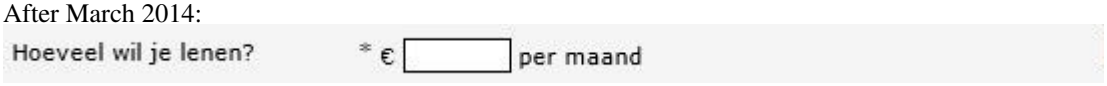
[15] Recall that after the treatment students in this group only receive (by default) the amount of basic grant received in the grant phase, since they do not have an interest-bearing loan or supplementary grant.

Figure 2. Screenshots of the loan application screen before and after the removal of the maximum borrowing tick box.

Before March 2014:



After March 2014:



Note: Translation: "Hoeveel wil je lenen" = How much do you want to borrow?; "Maximaal" = Maximum; "Minder, namelijk ... per maand" = Less, namely ... per month.

2.3 Rationale behind the treatments and political context.

2.3.1 Treatment 1: Changes in the standard loan amount when entering the loan phase. In April 2008, the Socialist Party (SP) in the Netherlands published a report (Leijten et al., 2008) about the student finance system and DUO (at the time called IB-Groep). In this report, the left-wing (socialist) party publishes quotes from people that sent mail to the SP concerning the IB-Groep and the student finance system. The SP claims the (private) costs of studying were increasing while the government at the same time is "normalizing student debt". The report draws attention to the fact that students who enter the loan phase automatically receive the maximum loan amount in the loan phase. However, the report (falsely) claims all students receive the maximum loan amount by default when entering the loan phase. The government responded to this report by means of a letter addressed to the members of parliament (the Second Chamber) on the April 24, 2008 (Plasterk, 2008a). In this letter, the government (also falsely) claims that all students automatically receive the sum of the basic grant, supplementary grant and interest-bearing loan when entering the loan phase.[16] On June 24, 2008, this letter (among other subjects) was discussed in the Second Chamber. The Minister of Education confirmed his earlier statement was false and now provided the Second Chamber with the correct information about the automatic loan amounts. Furthermore, the government claims the reason for providing students who did not specify a loan amount (group 1) automatically with a maximum loan upon entering the loan phase was to ensure the students would not accidentally end up with an income of €0. Nevertheless, the Minister of Education told the members of parliament the issue would be discussed with DUO (IB-Groep) and student organizations (Tweede Kamer, 2008). On December 4 2008, in a final letter of the government to the members of parliament on this issue, the government announced that the old default loan amount would be changed to the new situation described in Section 2.2.1. The government argued the new system will be more in line with the preferences of the students (Plasterk, 2008b). The government also stated the technical implication of changing the default loan amount is complicated, this is likely to be the reason group 1 was treated 6 months earlier than the other three groups.

[16] This statement would have been correct if the letter was written after March 2010.

2.3.2 Treatment 2: Removal of the 'maximum borrowing tick box' from the application screen. The aim of the removal of the maximum borrowing tick box was to stimulate students to think about the amount they should borrow. Before the removal of the tick box, students could request the highest possible loan amount without knowing what amount they were actually requesting. DUO wanted to stimulate students to make a more thought-out decision with respect to their requested interest-bearing loan amount.

3 Theoretical Background

This section provides a theoretical background, discussing the mechanisms through which the two treatments/nudges influence borrowing behavior of students. Section 3.1 discusses human capital theory and Section 3.2 discusses insights from behavioral economics. The connection between the theoretical mechanisms and the results of this study will be discussed in more detail in Section 7.

3.1 Human capital theory.

Human capital theory provides valuable insights into relevant factors that influence investments in education and borrowing behavior. Mincer (1958; 1962), Schultz (1963) and Becker (1994) laid the foundation of human capital theory, where education is regarded as an investment that increases the amount of human capital of the individual. The optimal investment choice equalizes the marginal cost of an additional year of education to the marginal gains of education. As long as the private benefits of an additional year of education exceed the private costs, the student will continue to invest. The (private) cost of education mainly consists of forgone wages for as long as individuals do not work. Each additional year of being enrolled in education (full time) implies one year of forgone labor income. In addition, there are indirect (private) costs associated with education, for example: the tuition fee and books. Furthermore, there are also substantial non-monetary costs of studying, since studying (successfully) requires effort and perseverance. Carneiro and Heckman (2003), Cunha et al. (2006) and Cunha and Heckman (2008) find that these non-cognitive skills have a large impact the formation of cognitive skills, adult outcomes and are an important determinant of the lower investments in human capital by students from groups with a lower social economic status. The most important monetary gain of investing in education is the increase in future wages. In addition, a higher level of educational attainment can also bring non-monetary gains, for example: intrinsic enjoyment of studying, better health and a higher social status (Vila, 2000; Dziechciarz-Duda and Król, 2013).

Note that in the case of perfect capital markets, students only base their investment decision on the expected marginal costs and expected marginal gains. However, in reality, capital market imperfections originate because human capital cannot serve as collateral due to its illiquid nature (non-slavery) which leads to under investment in human capital and thus provides an argument to supply government loans in order to resolve capital market imperfections and ensure

accessibility (Jacobs, 2002). If students are credit-constrained due to capital market imperfections, students who receive a lower contribution from their parents have less opportunities to study. Recall from Section 2.1 that ensuring accessibility is an important motive of the Dutch government to provide student finance (Wet Studiefinanciering 2000, 1999).

However, from the perspective of human capital theory, both changes in the student loan choice architecture should have no effect on student borrowing behavior and the acquisition of human capital, since the budget constraint of the individual and all the factors listed above, are not affected. Section 3.2 will explore insights from behavioral economics that provide mechanisms through which borrowing behavior may be affected by the changes in the choice architecture.

3.2 Insights from behavioral economics.

Standard economic theory predicts that, if transaction costs are small, default options should have little impact on economic outcomes. Rational individuals with well-defined preferences will opt out of any default option that does not maximize their utility, regardless of the nature of the default option. The same holds for the removal of the tick box. However, there is a growing body of evidence showing the impact of the design of the choice architecture on the decision making of individuals. Especially with respect to default options, where evidence shows that default options have a significant impact on human behavior in a variety of contexts. Beshears et al. (2009), Cribb et al. (2016) and Madrian and Shea (2001) find that defaults exert a substantial influence on retirement savings outcomes. Johnson and Goldstein (2003) and Abadie and Gay (2006) show that presumed consent legislation (often referred to as an opt-out system) substantially increases the amount of organ donors, compared to an opt-in system. Johnson et al. (2002) find that the default option has a significant impact on the consent to receive e-mail marketing. The impact of default options in the context of student borrowing is studied by Marx and Turner (2017). The authors experimentally tested the impact of student loan nudges on borrowing decisions and find that students are biased towards borrowing the (random) amount that is offered to them. Furthermore, students offered a nonzero loan amount were 40 percent more likely to borrow compared to students who received a $0 loan offer.

To meaningfully interpret the results presented in Section 6, it is important to explore why and how the change of the default option and the removal of the tick box can influence borrowing behavior. Standard economic theory is insufficient to explain the impact of defaults, however, insights from behavioral economics provides mechanisms through which these nudges can influence economic outcomes.

Default (and the tick box) as an implicit recommendation. First, default options can convey information and can arise from a strategic interaction between a default setter and a decision maker; an individual can stick to the default option to follow the (implicit) recommendation by the default setter. This might also be applicable to the tick box, since it potentially can

be perceived as the popular or recommended option due to the prominent place in the loan application procedure/screen.

McKenzie et al. (2006) show that defaults communicate an implicit recommendation, which influences the decision-maker. In addition, the authors find that people who are uncertain about their preferences are more likely than others to be influenced by this implicit recommendation. In a field study by Brown et al. (2012), where employees provided explanations for their retirement savings decision, approximately one third indicate they regarded the default as an endorsement by the default setter. Tannenbaum and Ditto (2011) illustrate that default options convey information about the reasons as to why a default-setter sets a specific default option. The authors argue individuals assess what these reasons might be and use these assessments in the decision-making process.

Altmann et al. (2017) provide a theoretical framework to analyze which defaults are more likely to affect behavior, and test the main predictions of the model experimentally. The authors find that the relative level of information of default setters and decision makers, and their alignment are the most important drivers. The authors show that the behavioral effects of a default are larger when the interests of both parties are aligned. Furthermore, individuals are more likely to opt for the default option the less they are privately informed about the relevant decision environment.

Status quo bias, inertia and procrastination of decision making. Second, the status quo bias, inertia and procrastination of decision making can cause people to stick to the default option (Madrian and Shea, 2001; Carroll et al., 2009; Sunstein, 2013).[17] Status quo bias, a term introduced by Samuelson and Zeckhauser (1988), refers to the tendency of individuals to stick to their original decisions or to the current situation; individuals disproportionately stick to the status quo. The status quo bias is driven by loss aversion, which refers to the observation that individuals weigh losses stronger than gains (Kahneman and Tversky, 1979). Since the status quo (the default loan amount) serves as the reference point for the loss evaluation, and individuals weigh the potential losses from switching higher than the potential gains, individuals are biased towards the status quo.

Inertia refers to the observation individuals seem to regret an unfortunate situation more if it was a consequence of an active decision, compared to if no active choice was made (Kahneman and Tversky, 1982; Landman, 1987). Similarly Ritov and Baron (1992), show that individuals react more strongly to adverse outcomes caused by action, irrespective of whether or not the status quo was maintained, and individuals prefer inaction over action even when inaction was associated with change. Consequently, inertia can cause people to stick to the default option.

Similarly, individuals can stick to the default option due to the human tendency to procrastinate decisions. The model by O'Donoghue and Rabin (2001) illustrates that providing individuals with additional options can induce procrastination. If individuals are provided with

[17] This mechanism does not apply to the removal of the tick box.

multiple options, the individual will have the tendency to not complete the task because of the persistent but unfulfilled aspirations to improve on the decision. The second main finding by O'Donoghue and Rabin (2001) is that people can procrastinate more when pursuing important goals compared to unimportant goals. Important goals come along with a higher level of intended effort, which in turn can increase the likeliness the individual will procrastinate the task. One could argue a student has a large range of options when considering to request an interest-bearing loan amount, and that borrowing money and investing in education are important decisions. Therefore, the predictions of the model by O'Donoghue and Rabin (2001) suggest that the decision in the context of this study could be prone to procrastination behavior.

Furthermore, the predictions by O'Donoghue and Rabin (2001) suggest the complexity of the decision potentially increases the chance the default option is chosen. Tversky and Shafir (1992) argue that individuals do not always precisely know how to trade off costs against benefits, risk against value, and immediate satisfaction against future discomfort, which in combination with uncertainty about the outcome (as in the case of student loans) complicates the decision and can cause people to defer choice and stick to the default. Similarly, in the context of this study, a low level of financial sophistication further increases the complexity associated with the decision, which implies that financially illiterate individuals should be more likely to defer choice and thus stick to the default option. Agnew and Szykman (2005) indeed find, by means of an experimental study, that individuals with weak financial knowledge are substantially more likely to follow the default option. In addition, Van Rooij et al. (2008) attempt to identify potential explanations for default choices (in several contexts), using survey data, and conclude that procrastination and financial illiteracy are the most prevailing explanations for default choices.

Anchor and reference point. Third, the maximum loan tick and the default loan offer when entering the loan phase can serve as an anchor or reference point, from which insufficient adjustment is made by the decision maker. This implies that different starting points (default loan amounts) result in different outcomes, which are biased towards the initial values (Tversky and Kahneman, 1975). Jetter and Walker (2016) analyze the wagering decisions in the US gameshow *Jeopardy!*, and provide evidence that anchoring plays a substantial role in financial-decision making. Raising the anchoring amount by 10% results in a 3.1% increase in the wager. No anchoring effect is found for children under the age of 13, but the authors do find evidence for the existence of the anchoring effect for teenagers and even more so for college students. Furthermore, the abovementioned study by Marx and Turner (2017), who test the impact of student loan offers on borrowing behavior, finds that students who received a nonzero offer were substantially more likely to borrow exactly the amount they were offered. In addition, students who received a nonzero loan offer were more likely to borrow than students who received a loan offer of $0. These findings suggest that the offered loan amount served as an anchor or reference point for at least a portion of the students.

4 Empirical Strategy

Section 3 outlined the theoretical mechanisms through which both changes to the student loan choice architecture, described in Section 2, can influence the borrowing behavior of student. This section will present the empirical strategies employed in order to estimate the impact of both nudges/treatments on the borrowing behavior of students. Multiple empirical strategies are employed. First, as a starting point, the borrowing behavior of cohorts before and after the treatment is compared. Second, a quasi-experimental design, in the form of a differences-in-differences (DD) estimation, is employed for both treatments. The DD estimation is the preferred empirical strategy for both treatments, since this empirical strategy enables the estimation of the causal impact of the nudges/treatments on borrowing behavior. Lastly, the nudges essentially create an exogenous shock to the student borrowing behavior. This implies that the estimates from the DD model can be used as the first stage in a 2SLS estimation of the effect of student borrowing on study success, where study success is measured by the speed in which a student earns a bachelor or master diploma after entering the loan phase.

4.1 Treatment 1: Changes in the standard (default) loan amount when entering the loan phase.

As described above, this section will outline three different empirical strategies. First, the simple before-after comparison serves as a starting point. Second, a quasi-experimental design is created due to the fact 1 group of students was confronted with the nudge before the other students were treated. This difference in the time of the treatment is exploited in a differences-in-differences strategy, enabling the estimation of the causal impact of the nudge on borrowing behavior. Third, since the nudge creates an exogenous shock to borrowing behavior, the DD specification also serves as the first stage in a 2SLS model, estimating the causal relation between student borrowing behavior and study success.

4.1.1 Before-after comparison of cohorts. In order to estimate the impact of the treatment on borrowing behavior, the most straight-forward empirical strategy is to compare the amounts students borrow, the main outcome variable of interest, of the cohorts entering the loan phase before and after the treatment. In order to estimate the effect of change in the default loan amount on the borrowing behavior (for one of the four groups of) students, by means of a before-after comparison of (two) cohorts, the following regression model is used:

$$Y_{icm} = \alpha + \mu_m + T_c + \sum_{\tau=1}^{12} \gamma_\tau (T_c \times M_\tau) + X_{icm}^{'} \beta + \varepsilon_{icm}, \tag{1}$$

where Y_{icm} is an indicator of student borrowing behavior, for example the loan amount of the interest-bearing loan, for student i, in cohort c, in month m, μ_m is a set of month fixed effects. T_c is a dummy which equals 1 for the treated cohort and 0 for the cohort before the treated cohort (control cohort). M_τ is an indicator for the months after the student enters the loan phase, the

variable ranges from 1 to 12, where $M_t = 1$ is the first month of the loan phase.

The coefficient of interest is γ, which results in the treatment effect per month, for the first 12 months after entering the loan phase (γ_1, γ_2, ... , γ_{12}). This specification estimates the dynamic treatment effect of the default option change. Recall that students are able to adjust their loan amount at monthly intervals. This specification therefore provides valuable insights, since it captures differences in treatment effects per month, caused by the adjustments to the borrowing behavior (interest-bearing loan amounts) made by students. In other words, this specification includes heterogeneous treatment effects for each of the first 12 months in the loan phase.

X'_{icm} is a vector of individual-level controls: sum of the requested loan amount in the last 12 months of the grant phase, migration background, gender, living situation, age, study type (higher vocational education or university), study program (9 categories), prior education (6 categories), average grade of prior education, tuition fee credit dummy, supplementary grant received in the last month of the grant phase and the social status of the district.[18]

This regression should be executed separately for each group of students (groups 1 to 4), where for group 1, the students entering the loan phase in September 2008 (before) and September 2009 (after) are included, and for groups 2 to 4 the students entering the loan phase in September 2009 (before) and September 2010 (after) are included.[19] Important to note is that only students that enter the loan phase in September are included in the sample, in order to be consistent with the differences-in-differences method discussed below. In addition, only including students that enter the loan phase in September, ensures the same months are included for all students, meaning that the second month of the loan phase is October for all students in the sample. More than 50% of the students enter the loan phase in September, which is due to the fact most students start receiving student finance in September.[20] Furthermore, note that including all students that enter the loan phase in a given academic year is problematic, since the time of treatment for groups 2 to 4 was March 2010, which means part of the control group/cohort would be confronted with the treatment.

4.1.2 Quasi-experimental design: Differences-in-differences. Even though, as mentioned in Section 2.1, there were no changes in the student finance system that could confound the empirical analysis of the treatment, there are many potential reasons as to why cohorts entering the loan phase could differ over time in ways that may affect their borrowing behavior. The control variables included in the before-after comparison are not likely to perfectly control for these

[18] See Appendix B and/or Table 5 for a more detailed description of these variables.

[19] Recall that the time of the treatment was September 2009 for group 1, and March 2010 for groups 2 to 4, as described in Section 2.2.1.

[20] Given that most study programs start in September, this is the moment most student start receiving student finance. From the students that enter the loan phase in the academic year 2009/2010, 52.89% entered in September (this percentage is relatively stable for different years). As explained in Section 2.1, students enter the loan phase after they have received student finance for as long as their nominal study duration. Even though the nominal study duration varies, the large majority of study programs do not stop in the middle of the academic year. Consequently, most students that start studying in September, also will enter the loan phase in September if they do not manage to complete the study program within the nominal duration.

differences and some examples of confounding factors could be unobserved heterogeneity or the effects of the Great Recession (which began in the late 2000s). For example, cohorts close to the Great Recession might have less opportunities to enter the labor market and therefore have lower (potential) forgone earnings, which may increase their investment in human capital (see Section 3.1 for a discussion of human capital theory).[21]

To account for these potential confounding factors, the simple before-after comparison of cohorts can be improved by exploiting differences in the timing of the default changes: group 1 was affected by the change in the default loan amount six months before the other groups of students were affected. This difference in the timing of the treatment creates a quasi-experimental design, which allows to estimate the impact of the default loan amount change for group 1 more accurately, since the students in other groups can be used as a control group.

In the preferred model, group 4 without a supplementary grant is used as the control group. In this analysis, only the months in the loan phase are included, and, similar to the before-after comparison discussed above, only students that enter the loan phase in September are included in the analysis. The main DD model, which estimates the average (monthly) effect of the default option change on the borrowing behavior in the first 12 months of the loan phase, can be represented as follows:

$$Y_{igcm} = \alpha + \mu_m + \gamma G_g + \lambda T_c + \delta(G_g \times T_c) + X_{igcm}^{'}\beta + \varepsilon_{igcm}, \tag{2}$$

where Y_{icm} is an indicator of student borrowing behavior, for example the loan amount of the interest-bearing loan, for student i, in group g, in cohort c, in month m. μ_m is a set of month fixed effects, where m now ranges from 1 to 12, where $m = 1$ is the first month of the loan phase. T_c is a dummy which equals 1 for the treated cohort, and G_g is a dummy which is equal to 1 for group 1 (treated group) and 0 for students in group 4 without a supplementary grant in the grant phase (control group). The interaction term $(G_g \times T_c)$ is the regressor of interest: the DD estimator, which indicates students in group 1 who enter the loan phase after the default loan amount was changed. $X_{igcm}^{'}$ is a vector of (individual-level) controls, accounting for relevant student characteristics (similar to the set of controls listed in Section 4.1.1).

In addition, analogous to the specification of regression model 1, the following model results in the treatment effects per month, for the first 12 months of the loan phase:

$$Y_{igcm} = \alpha + \mu_m + \sum_{\tau=1}^{12} \theta_\tau(G_g \times M_\tau) + \sum_{\tau=1}^{12} \vartheta_\tau(T_c \times M_\tau) + \sum_{\tau=1}^{12} \delta_\tau(G_g \times T_c \times M_\tau) + X_{igcm}^{'}\beta + \varepsilon_{igcm}, \tag{3}$$

where the coefficient of interest is δ, which is the estimated treatment effect per month, for the first 12 months after entering the loan phase (δ_1, δ_2, ... , δ_{12}). This specification allows

[21] Another potential consequence of the impact of the Great Recession is that the parental contribution was lower on average, which potentially results in students being more dependent on the student loans.

to estimate the dynamic treatment effect of the default option change. Note the regression model 3 essentially provides similar estimates compared to estimating the regression model of 2 separately for each month during the first year of the loan phase.

Selection of the control group and the common trend assumption underlying the DD model. Selecting the control group and the validity of the common trend assumption are important factors determining the internal validity of the DD strategy. Since the treatment effect on the treated is determined by taking into account the trend in the control group. If there is a common trend in the control and the treatment group, we can interpret the deviation from this trend in the treatment group, relative to the control group, as the causal effect of the treatment on the treated. First the selection of the control is discussed, followed by some remarks on the common trend assumption.

Selecting the control group. Recall from Section 2.2.1 that, by construction, it is not possible to receive a supplementary grant and be in group 1. Therefore, intuitively, the most suitable control group seems to be students in group 4 who did not receive a supplementary grant in the last month of the grant phase. In theory these students should be relatively similar to each other, since (1) both students in practice receive €0 as an interest-bearing loan in the last month of the grant phase and (2) their social economic status is comparable since both do not receive a supplementary grant. Nevertheless, it should be noted that in a DD model, both groups are not required to be similar in order to estimate a causal treatment effect. Table 4 indeed shows that there are significant differences on some background characteristics between the two groups of students.[22]

Common trend assumption. The main identifying assumption of a DD model is the common trend assumption: in absence of the default option change, the borrowing behavior is determined by the sum of a time-invariant group effect and a time effect which is common across both groups. If this assumption holds, the estimated treatment effect in the DD model can be interpreted as the causal impact of the treatment. The validity of the common trend assumption is approximated in Section 6.2.4, where it is concluded the assumption is likely to be valid.[23] Other potential concerns to the internal validity of this empirical strategy, related to sample size and sample selection bias, are also addressed in Section 6.2.4. It is concluded that these factors are unlikely to bias the results.

4.1.3 Estimating the effect of borrowing behavior on study success (2SLS). Regression models 2 and 3 aim to estimate the effect of the treatment on borrowing behavior. However, these

[22]When comparing background characteristics of group 1 to all students in groups 2, 3 and 4 we find that the differences in background characteristics are even larger (see Table C.2in Appendix C.2)

[23]Note that the validity of the common trend assumption can only be approximated, due to the lack of the counterfactual.

Table 4. Comparison of background characteristics of group 1 and group 4 without SG.

	Group 1 average	Group 4 average	Estimated difference
Study program	5.950	5.891	0.0590
			(0.0508)
Study type	0.672	0.668	0.00385
			(0.0116)
Prior education	5.750	5.770	-0.0202
			(0.0167)
Grade prior education	68.06	67.84	0.215
			(0.126)
Age	22.64	22.77	-0.133***
			(0.0229)
Male	0.516	0.483	0.0323**
			(0.0123)
Living with parents	0.342	0.299	0.0431***
			(0.0115)
Migration background	0.118	0.119	-0.000874
			(0.00799)
Social status of district	-0.208	-0.335	0.126***
			(0.0281)

Note: Sample consists of students in groups 1 and 4 (excluding students who receive a supplementary grant), who enter the loan phase in September 2008 (pre-treatment cohort). Only 1 month per student is included. The total amount of students in this sample are 8,765 (4,955 in group 1 and 3,810 in group 4). Columns 1 and 2 present the means of relevant background characteristics, column 3 provides the estimated difference between the means of group 1 and group 4. Standard errors reported in the parentheses, */**/*** denote significance at a 10/5/1 percent confidence level. Study program refers is a categorical variable of the 9 study directions (CROHO). Study type is a dummy equal to 1 if the student is in university (wo) and 0 if the student is in higher vocational education (hbo). Prior education is a categorical variable with 6 categories. Male, living with parents and migration background are dummies equal to 1 (0) for (fe)males, students (not) living with their parents and students with(out) a migration background. See Appendix B.2 for a detailed description of all variables.

regression models also represent the first stage of a two-stage least squares (2SLS) estimation of the impact of borrowing behavior on study success.[24] Study success is an important outcome variable, since the goal of the student finance system is to enable people to study regardless of their financial situation, essentially aiming to increase study success and educational attainment. Study success is measured by two indicators: the amount of months it takes before the student obtains (1) a bachelor or (2) a master diploma after entering the loan phase.

Borrowing behavior can influence study success if students face trade-offs between spending their time working or studying, nudging students to borrow less (more) could increase (decrease) the time spend on work on consequently decrease (increase) study success. This mechanism might be especially relevant in the Dutch context, since over 60% of students (16-29 years old) combine working and studying, which is the highest among the countries participating in

[24]This empirical strategy is similar to the strategy used by Duflo (2001), who uses the differences-in-differences estimate of the effect of school construction programs on educational attainment as a first stage in a 2SLS estimation of the impact of education on wages.

the 2012 Survey of Adult Skills (PIAAC). Furthermore, approximately 48% of the working students in the Netherlands, work in a different field from the study field Quintini (2015). At the same time, 34% of the respondents of a survey among Dutch students say that they borrow money from DUO in order to work less while studying (Nibud, 2017).

On the other hand, it is also possible that nudging students to borrow less (more) only causes students to take on less (additional) debt, without having an effect on the time spend on studying and/or study success (Lochner and Monge-Naranjo, 2012; Avery and Turner, 2012).

Consider the following equation, which specifies the causal effect of borrowing behavior (requested interest-bearing loan amount in the loan phase) on study success:

$$E_{igcm} = \alpha + \gamma G_g + \lambda T_c + \theta L_{igcm} + X'_{igcm}\beta + \varepsilon_{igcm}, \tag{4}$$

where E_{igcm} is the study success of student i, in group g, entering the loan phase in the academic year/cohort c, in month m, and G_g and T_c are essentially group and cohort fixed effects respectively. Ordinary least-squares estimation (OLS) of equation 4 leads to biased estimates if the error term (ε_{igcm}) is correlated with L_{igcm}, the requested interest-bearing loan amount (the regressor of interest).[25]

However, under the assumptions that (1) in absence of the default option change, the borrowing behavior is determined by the sum of a time-invariant group effect and a time effect which is common across both groups (common trend assumption), and (2) that the default option change had no direct impact on study success (only potentially via the impact on borrowing behavior), the interaction term from equation 2 is available as an instrument for equation 4. The validity of the common trend assumption is confirmed in Section 6.1.4. With respect to the second assumption, it seems unlikely that a change in the default loan amount when entering the loan phase has a direct effect on study success, other than through the indirect effect via the impact on borrowing behavior (the requested interest-bearing loan amount).

Under these two assumptions, the default option change essentially generates an exogenous shock to the loan amounts of students entering the loan phase, this exogenous shock can be exploited to study the effect of changes in borrowing behavior on study success in an instrumental variable (IV) design.

Consequently, the first stage is given by:

$$L_{igcm} = \alpha + \gamma G_g + \lambda T_c + \delta(G_g \times T_c) + X'_{igcm}\beta + v_{igcm}, \tag{5}$$

where L_{igcm} is the endogenous regressor, the requested interest-bearing loan amount, for student i, in group g, in cohort c, in month m. X'_{igcm} is a vector of student characteristics, similar to the set of controls listed above but also including one of two dummy variables equal 1 when the

[25]For example, if students with a relatively low social economic status have a lower study success, see for example APA (2017) and NCES (2015), and borrow more on average, see for example Houle (2014), the OLS estimate will be biased downwards.

student is enrolled in (1) a bachelor or (2) a master program when entering the loan phase.[26]

Important to consider is the relation between study success and total/cumulative loan amounts (in the loan phase). The longer the student takes to successfully complete the study, the total borrowed amount will, on average, be higher. Suppose that the change in the default loan amount increases the requested loan amount of students (the first stage), which would in turn cause students to, on average, successfully complete their study program quicker. The higher monthly loan amount has reduced the time a student spent on work, increased the time spent on study related tasks, and therefore reduced the total study duration of the student. However, in this situation, the total (cumulative) loan amount might still be lower, since the student is now eligible to receive student finance for a shorter period of time. To account for this issue, only the effect of the treatment in the first month of the loan phase used as the exogenous shock to the borrowing behavior.[27]

The second stage can be represented by:

$$E_{igcm} = \alpha + \gamma G_g + \lambda T_c + \theta \hat{L}_{igcm} + X'_{igcm}\beta + \varepsilon_{igcm}, \tag{6}$$

where E_{igcm} is an indicator of study success, for student i, in group g, in cohort c, in month m. $\hat{L}_{igcm}$ is the predicted value of the endogenous regressor, obtained in the first stage regression, used to estimate the effect of borrowing behavior on study success.

Local average treatment effect. An important characteristic of the instrumental variable framework is that the estimated coefficient is a local average treatment effect (LATE), see Angrist and Pischke (2008) for an extensive discussion on this issue. The IV (the change in the default loan amount) essentially creates two groups of students (analogous to randomly assigned treatment and control groups): (1) students who borrow 'less' (group 1 after the nudge, the treatment group) and (2) students who borrow 'more' (the other students, the control group).[28] Subsequently, four subpopulations can be distinguished. Complier students are students who would borrow 'less' as a result of the default option change, and borrow 'more' in absence of the default option change. The never-takers are students that never borrow 'more', irrespective of the treatment. Always-takers are students who always borrow 'less', irrespective of the treatment. Defiers are students who borrow 'more' when influenced by the treatment, and borrow 'less' when not influenced by the treatment. In order to estimate the LATE, defiers are assumed to not exist. This is also referred to as the monotonicity assumption: the instrument may have no effect on some students, but all of the students that are affected must be affected in the same

[26] Which dummy is added to the set of controls depends on which outcome variable (obtaining a bachelor, or obtaining a master) is used. Adding this dummy controls for a potential difference in study progress prior to the treatment, which is correlated to indicators of study success in the loan phase.

[27] The downside of this method is that the effect size (in the first stage) is much smaller when only considering the effect of the treatment on borrowing behavior in the first month. However, a robustness check is discussed in Section 6.1, where the cumulative effect of the default in the first 3, 6 or 12 months in the loan phase is used.

[28] Note that the treatment is the default option change for group 1 (as of September 2009), which significantly reduced the default borrowing amount, see table 3.

way/direction (Angrist and Pischke, 2008).[29]

The IV estimate of the effect of borrowing behavior on study success is only informative for the subpopulation of compliers, since this is the only subpopulation that changes its behavior as a result of the instrument. This feature of the IV design is important to keep in mind when interpreting the results. With respect to the context of this study, one can argue the IV estimate is only informative about students who display irrational behavior by letting themselves be influenced by the treatment (the complier students). Appendix C.3.3 explores the characteristics of compliers.

4.2 Treatment 2: Removal of the 'maximum borrowing tick box' from the application screen.

The empirical strategy to estimate the impact of the second treatment/nudge on student borrowing behavior is similar to the empirical strategies used for the first treatment/nudge. A simple before-after comparison of cohorts serves as starting point. Subsequently, a differences-in-differences strategy is employed, which is the preferred specification since this empirical strategy enables estimating a causal effect of the nudge on borrowing behavior.

4.2.1 Before-after comparison of cohorts. Also with respect to the second nudge/treatment (removal of the 'maximum borrowing tick box'), a simple before-after comparison of cohorts serves as a good starting point, especially since the empirical strategies in this case are limited since all students who visit the application screen (a personalized web page) were affected equally and simultaneously by the nudge. In order to estimate the effect of the removal of the 'maximum borrowing tick box' on the borrowing behavior of students, by means of a before-after comparison of (two) cohorts, the following regression model is used:

$$Y_{icm} = \alpha + \mu_m + \lambda T_c + \sum_{\tau=1}^{6} \gamma_\tau (T_c \times M_\tau) + X_{icm}^{'} \beta + \varepsilon_{icm}, \tag{7}$$

where Y_{icm} is an indicator of student borrowing behavior, for example the loan amount of the interest-bearing loan conditional borrowing a positive amount, for student i, in cohort c, in month m, μ_m is a set of month fixed effects. T_c is a dummy which equals 1 for the treated cohort (student that started studying in September 2014) and 0 for the cohort before the treated cohort (control cohort: students that started studying in September 2013). M_τ is an indicator for the month of the academic year, where $M_t = 1$ represents September. $X_{icm}^{'}$ is a vector of (individual-level) controls, accounting for relevant student characteristics, including: migration background, gender, living situation, age, study type (higher vocational education or university), study program (9 categories), prior education (6 categories), average grade of prior education, tuition fee credit dummy, supplementary grant received (4 categories).[30]

[29] In the context of this study, it seems counter intuitive that students would borrow 'more' as a result of a significant decrease in the default loan amount (the IV). Therefore, it is likely the monotonicity assumption holds, though it is not testable.

[30] See Appendix B and/or Table 5 for a more detailed description of these variables.

This specification accounts for the fact not all students receiving student finance are by definition influenced by the treatment. Only students who visit the student finance application screen will be confronted with the nudge. After applying for student finance, it is not necessary to visit the website (application screen) every month, since the student finance continues automatically, until the student loses eligibility. Therefore, in order to provide a meaningful before-after comparison of different cohorts and to ensure the students in the sample are confronted with the treatment, only the first-year students who start studying as of September are included in the sample. In addition, only the first 6 months of the academic year are included, to avoid the fact that the control cohort (2013/2014) is confronted with the treatment, which will confound the empirical strategy.[31]

It is important to stress that this empirical strategy is not likely to identify a causal relation, since it is impossible to separate the cohort fixed effect from the treatment effect. The next empirical strategy provides a solution for this issue.

4.2.2 Differences-in-differences, improved before-after comparison of cohorts. The empirical strategy can be improved by constructing a sample of students who apply for an interest-bearing loan for the first time, in the months around the removal of the tick box. Subsequently, a differences-in-differences strategy can be employed, by comparing borrowing behavior of new applicants in February (control group) and April (treatment group) in the years before (2013) and after (2014) the treatment.[32] This empirical strategy can be represented by the following equation:

$$Y_{igc} = \alpha + \gamma G_g + \lambda T_c + \delta(G_g \times T_c) + X_{igc}^{'}\beta + \varepsilon_{igc}, \tag{8}$$

where Y_{igc} is an indicator of student borrowing behavior, for example the loan amount of the interest-bearing loan, for student i, in group g, in cohort c, T_c is a dummy which equals 1 for the treated cohort. G_g is a dummy which equals 1 for students who applied for a loan for the first time in April (treatment group), and 0 for students who applied for a loan for the first time in February. The interaction term $(G_g \times T_c)$ is the regressor of interest, the DD estimator.

The main identifying assumption of this DD model is the common trend assumption: in absence of the treatment, the borrowing behavior is determined by the sum of a time-invariant group effect and a time effect which is common across both groups. The validity of the common trend assumption is approximated in Section 6.2.4, where it is concluded the assumption is likely to be valid.

Even though this DD model provides the causal impact of the nudge on the requested loan

[31] Recall that the tick box was removed in the seventh month of the 2013/2014 cohort/academic year (March 2014).

[32] Additionally, students in the loan phase are omitted from the sample, since the DUO, by default, provides student with a positive interest-bearing loan when entering the loan phase, see Table 2. Consequently, it is possible these students start receiving a positive interest-bearing loan amount without having been confronted with the loan application screen and the nudge.

amount for first-time borrowers in the first month, a significant downside of this empirical strategy is that estimating dynamic treatment effects is problematic. This is because students in the control group who visit their personal loan application screen at least two months after they have first applied for an interest-bearing loan (in February), will be confronted with the treatment.

4.2.3 Estimating the effect of borrowing behavior on study success (2SLS). In addition, analogous to the IV design described with respect the first treatment, the nudge can in theory be regarded as an exogenous shock in the loan amount of students, which can be exploited to estimate the causal relation between borrowing behavior and study success by means of a 2SLS estimation. However, due the small amount of periods after the treatment included in the data set, it is not possible to construct a credible and informative indicator of study success. Furthermore, the exogenous shock generated by the removal of the tick box is substantially smaller compared to the shock caused by the change of the default loan amount (treatment 1), which results in a weak first stage. Consequently, due to the data limitations and the weak first stage it is not possible to exploit the variation in borrowing behavior created by the removal of the tick box in order to estimate the effect of borrowing behavior (or of the nudge) on study success.

5 Data

The main data sets used in this study are two micro registry data sets from DUO. The first data set contains monthly registry data on all student finance components. More specifically, it includes data on: the basic grant, supplementary grant, the interest-bearing loan, the tuition fee credit, living situation of the student (whether or not the student lives with his parents), the student travel product and the type of higher education the student is enrolled in. The data spans from September 2004 to August 2015 and includes all students in the Netherlands that received student finance.

The second data set is also collected by DUO and includes yearly data on all registrations, background characteristics and results from all students enrolled in higher education. This data set is referred to as the 1 Cijfer HO (1cHO) data set. This data set also spans from the cohort 2004 to the cohort of 2014. A selection of the included variables is used in this paper, the most important variables (mainly used as covariates in the regression analyses) are: average grade of the highest completed education prior to entering higher education, age, gender, nationality, immigration background, study type, study program, type and date of diploma, and postal code of the student. Both data sets can be linked and are anonymised.

The third data set used is collected by The Netherlands Institute for Social Research (SCP) and contains data on the social status of postal code areas (neighborhoods/districts), which can be linked to the postal code data of the student (SCP, 2017). The social status of an area is determined by the level of education, income and the position on the labor market of the people

Table 5. Summary statistics.

	Grant phase		Loan phase	
	Mean	SD	Mean	SD
Student finance components				
Basic grant (€)	179.85	86.35	0.00	0.00
Supplementary grant (€)	57.32	93.85	0.00	0.00
Tuition fee credit (€)	11.65	39.48	14.29	43.54
Receives student travel product (=1)	0.97	0.18	0.84	0.37
Interest-bearing loan amount (€)	101.90	182.70	385.13	365.10
Interest-bearing loan amount > 0 (=1)	0.28	0.45	0.62	0.49
Borrows maximum loan amount (=1)	0.18	0.38	0.27	0.44
Living situation				
Living with parents (=1)	0.48	0.50	0.30	0.46
Not living with parents (=1)	0.52	0.50	0.70	0.46
Type of higher education				
hbo (=1)	0.65	0.48	0.44	0.50
wo (=1)	0.35	0.48	0.56	0.50
Study program				
Education (=1)	0.08	0.28	0.05	0.23
Agriculture (=1)	0.02	0.16	0.02	0.14
Science (=1)	0.03	0.18	0.03	0.18
Engineering (=1)	0.17	0.37	0.15	0.36
Health (=1)	0.12	0.33	0.08	0.27
Economics (=1)	0.29	0.45	0.30	0.46
Law (=1)	0.03	0.18	0.08	0.28
Behavioural and social science (=1)	0.17	0.37	0.18	0.39
Language and culture (=1)	0.07	0.26	0.10	0.30
Other (=1)	0.01	0.07	0.00	0.03
Highest prior education				
< havo (=1)	0.00	0.03	0.00	0.01
mbo (=1)	0.11	0.31	0.01	0.11
havo (=1)	0.20	0.40	0.03	0.16
vwo (=1)	0.23	0.42	0.06	0.24
Other (=1)	0.02	0.15	0.01	0.10
Higher education (=1)	0.44	0.50	0.89	0.31
Grade prior education (0-100)	67.20	5.23	66.80	4.95
Demographic characteristics				
Age	20.42	2.13	23.86	1.88
Woman (=1)	0.52	0.50	0.45	0.50
Man (=1)	0.48	0.50	0.55	0.50
Migration background (=1)	0.21	0.41	0.25	0.43
Social status of district (-8.19-2.93)	-0.25	1.24	-0.39	1.28
Amount of observations	44,515,036		10,635,806	
Amount of students	1,273,664		581,670	

Note: Outliers are excluded, see Appendix B.3 for a definition of outliers. All observations from the merged data sets are included (September 2004 to August 2015). havo and vwo are levels of secondary education, which grant access to higher vocational education (hbo) and university (wo) respectively. mbo is intermediate vocational education, which grants access to higher vocational education. Note that the tuition fee credit was introduced in September 2007.

that live in that district. Data on the social status of districts is available for the years 1998, 2002, 2006, 2010 2014 and 2016. The average score in the Netherlands, over all years, is 0. The status score variable ranges between -8.19 and 2.93 for the included years in this study (2004-2014), where a higher status score refers to an area with a better social status.

Appendix B provides more detail on the merge of the data sets and includes a list of all the variables used in this study. Table 5 presents summary statistics of selected variables separate for students in the grant and loan phase. Only full-time students enrolled in higher education are included in this study. A small number of observations in the student finance data set have strange values which should not be possible given the maximum possible amounts students are eligible to receive, these outliers are excluded. See Appendix B.3 for the method used to flag outliers.

6 Results

Section 6 presents the main results. First the results of the the change in the standard loan amount when entering the loan phase, are presented and discussed, followed by the results of the removal of the maximum loan tick box.

6.1 Treatment 1: Changes in the standard loan amount when entering the loan phase.

Section 6.1 presents the results of the changes in the standard loan amount (in September 2009 and March 2010) when entering the loan phase. First the baseline model: the before-after comparison of cohorts is discussed. Subsequently, the results of the differences-in-differences strategy, the preferred specification are presented. Lastly, the estimates of the effect of borrowing behavior on study success are presented.

6.1.1 Results of the before-after comparison of cohorts. The results from the estimation of equation 1, the before-after comparison of the borrowing behavior of cohorts, are presented in Table 6. This table includes the estimated treatment effects, which corresponds to γ in equation 1, of 12 different regressions (columns 1 to 12). This specification allows us to consider the dynamic effects of the default change, where the first row (September) is the first month in the loan phase, and the last row (August) is the 12th month of the loan phase.

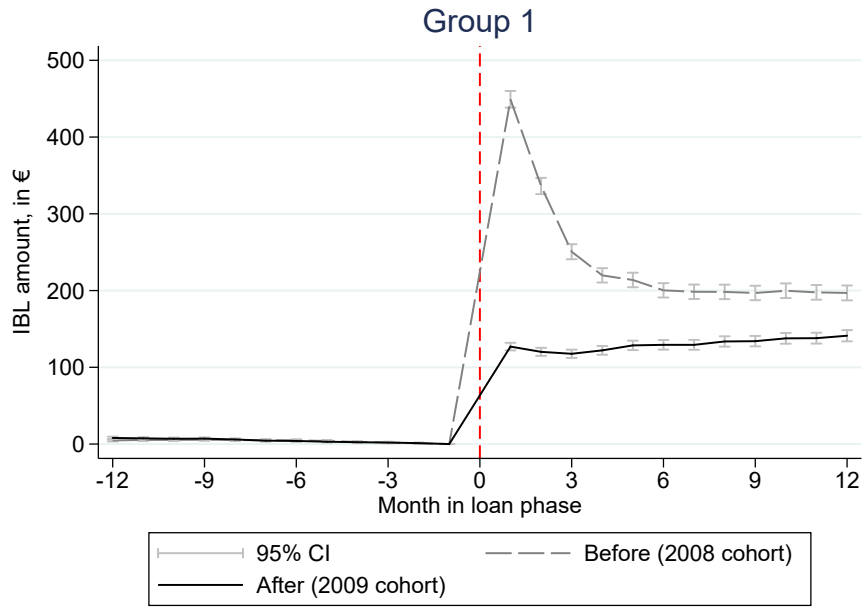
The table consist of 4 parts, groups 1 to 4, where three specifications are reported per group. The only difference between the three specifications is the outcome/dependent variable. The first outcome variable is the main outcome variable of interest: the requested interest-bearing loan amount (columns 1, 4, 7 and 10). The second outcome variable is a dummy indicating whether or not the student borrows the maximum amount (columns 2, 5, 8 and 11). The third outcome variable is a dummy indicating whether or not the student borrows a positive amount (columns 3, 6, 9 and 12).

Table 6. Before-after comparison of cohorts, treatment effect of changing in the standard loan amount when entering the loan phase, by group.

	Group 1			Group 2			Group 3			Group 4		
	(1) IBL	(2) Max IBL	(3) IBL > 0	(4) IBL	(5) Max IBL	(6) IBL > 0	(7) IBL	(8) Max IBL	(9) IBL > 0	(10) IBL	(11) Max IBL	(12) IBL > 0
Sep	-321.46***	-0.48***	-0.12***	126.17***	-0.01**	-0.02***	-51.62***	-0.76***	-0.01***	93.10***	-0.00	0.33***
	(5.96)	(0.01)	(0.01)	(5.15)	(0.01)	(0.01)	(3.25)	(0.01)	(0.00)	(2.89)	(0.00)	(0.01)
Oct	-215.25***	-0.32***	-0.07***	66.01***	-0.04***	-0.04***	-48.38***	-0.70***	-0.01***	59.68***	-0.00*	0.23***
	(5.90)	(0.01)	(0.01)	(5.69)	(0.01)	(0.01)	(3.57)	(0.01)	(0.00)	(3.16)	(0.00)	(0.01)
Nov	-132.13***	-0.21***	-0.01	34.52***	-0.06***	-0.03***	-46.81***	-0.66***	-0.01***	40.68***	-0.01**	0.18***
	(5.55)	(0.01)	(0.01)	(5.99)	(0.01)	(0.01)	(3.71)	(0.01)	(0.00)	(3.37)	(0.00)	(0.01)
Dec	-96.57***	-0.16***	0.01	24.70***	-0.06***	-0.04***	-42.98***	-0.63***	-0.01***	33.38***	-0.01***	0.15***
	(5.43)	(0.01)	(0.01)	(6.30)	(0.01)	(0.01)	(3.83)	(0.01)	(0.00)	(3.54)	(0.00)	(0.01)
Jan	-84.04***	-0.14***	0.02**	14.40**	-0.06***	-0.04***	-60.58***	-0.62***	-0.02***	31.64***	-0.01**	0.15***
	(5.57)	(0.01)	(0.01)	(6.54)	(0.01)	(0.01)	(3.86)	(0.01)	(0.00)	(3.74)	(0.00)	(0.01)
Feb	-70.00***	-0.12***	0.03***	15.49**	-0.05***	-0.03***	-53.57***	-0.58***	-0.01***	28.79***	-0.00	0.13***
	(5.56)	(0.01)	(0.01)	(6.80)	(0.01)	(0.01)	(3.99)	(0.01)	(0.00)	(3.93)	(0.00)	(0.01)
Mar	-66.78***	-0.12***	0.03***	16.85**	-0.03***	-0.04***	-46.94***	-0.56***	-0.01**	31.68***	-0.00	0.13***
	(5.63)	(0.01)	(0.01)	(7.04)	(0.01)	(0.01)	(4.11)	(0.01)	(0.00)	(4.07)	(0.00)	(0.01)
Apr	-61.38***	-0.10***	0.03***	18.23**	-0.03***	-0.03***	-43.87***	-0.54***	-0.01**	30.64***	0.00	0.13***
	(5.72)	(0.01)	(0.01)	(7.28)	(0.01)	(0.01)	(4.19)	(0.01)	(0.00)	(4.20)	(0.00)	(0.01)
May	-59.97***	-0.10***	0.03***	13.61*	-0.03***	-0.03***	-44.97***	-0.53***	-0.01**	27.66***	0.00	0.12***
	(5.74)	(0.01)	(0.01)	(7.40)	(0.01)	(0.01)	(4.24)	(0.01)	(0.00)	(4.29)	(0.00)	(0.01)
Jun	-60.17***	-0.10***	0.03**	11.38	-0.03***	-0.03***	-36.87***	-0.52***	-0.00	30.99***	0.00	0.12***
	(5.85)	(0.01)	(0.01)	(7.59)	(0.01)	(0.01)	(4.30)	(0.01)	(0.00)	(4.40)	(0.00)	(0.01)
Jul	-57.72***	-0.10***	0.03**	13.48*	-0.03***	-0.03***	-35.64***	-0.51***	-0.00	30.94***	0.01	0.12***
	(5.89)	(0.01)	(0.01)	(7.85)	(0.01)	(0.01)	(4.37)	(0.01)	(0.00)	(4.52)	(0.00)	(0.01)
Aug	-53.77***	-0.09***	0.03***	12.87	-0.03***	-0.03***	-38.08***	-0.50***	-0.01	35.60***	0.01**	0.12***
	(6.02)	(0.01)	(0.01)	(8.10)	(0.01)	(0.01)	(4.48)	(0.01)	(0.00)	(4.61)	(0.00)	(0.01)
$\bar{Y}$	183.33	0.14	0.40	416.89	0.15	0.89	697.76	0.48	0.92	152.20	0.07	0.35
sd(Y)	292.45	0.35	0.49	274.97	0.36	0.32	259.60	0.50	0.27	255.21	0.25	0.48
Adj. R^2	0.12	0.14	0.07	0.13	0.05	0.03	0.10	0.37	0.04	0.11	0.06	0.09
N	112,920	112,920	112,920	69,433	69,433	69,433	184,510	184,510	184,510	188,922	188,922	188,922

Note: Each column represents a different OLS regression of equation 1, for the four groups and three dependent variables. Dependent variables: (1) "IBL" = Interest-bearing loan amount, in € ; (2) "Max IBL" = dummy equal to 1 if student borrows the maximum IBL; (3) "IBL > 0" = dummy equal to 1 if IBL > 0. All controls listed in Section 4.1 are included. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

Figure 3. Effect of changing the default loan amount: before-after comparison of cohorts of students in group 1 on the IBL amount.



Note: X-axis represents the month in the loan phase, where negative values are last months of the grant phase. Only students entering the loan phase in September 2008 (before the treatment) and September 2009 (after the treatment) are included. The vertical lines represent the 95% confidence interval. No control variables are included.

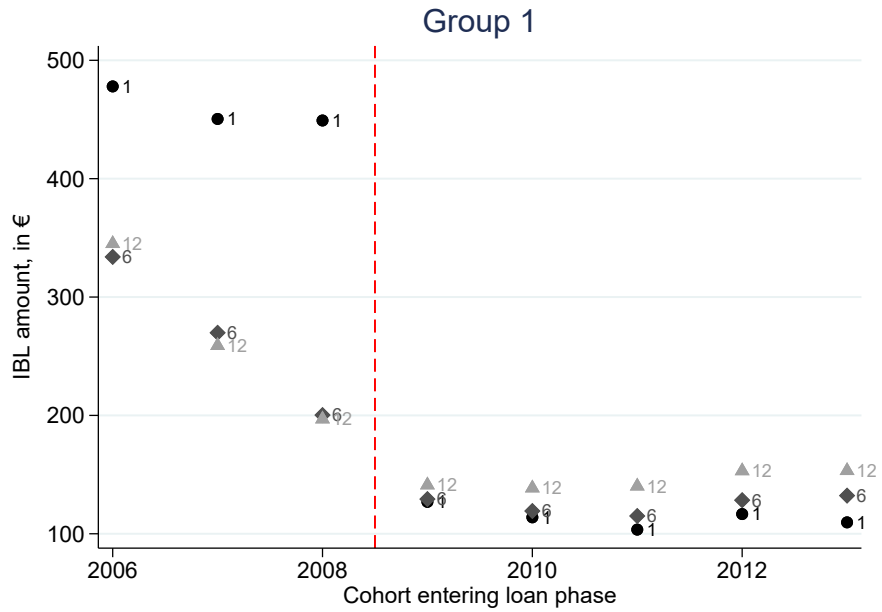
Note that the impact of the treatment did not only differ by these four groups, but also within these groups: by the living situation and amount of supplementary grant received (see Section 2.2.1 and Table 3). Consequently, the estimates presented in Table 6 are average treatment effects per group. The remainder of this section discusses the estimates presented in Table 6 in more detail, per group.

Group 1. First consider group 1, where students are by default borrowing €564 or €727 less (depending on the living situation, see Table 3) when entering the loan phase as a result of the treatment. On average, students borrow €321 less in the first month of the loan phase (September), which is a sizable decrease (column 1).[33] However, students actively adjust their loan amounts in the next months, since the treatment effect in the fourth month of the loan phase (December) has reduced to €97. This is also illustrated in Figure 3, where the pattern is much smoother over the first 12 months in the loan phase for the 2009 cohort compared to the 2008 cohort. Figure 4 includes multiple pre- and post-treatment cohorts, it is clear that the effect of the treatment is sizable in the first month, but becomes smaller when comparing the borrowing behavior in the 6th and 12th month of the loan phase. See Appendix C.1 for additional figures.

Furthermore, the chance that students in group 1 borrow the maximum interest-bearing loan amount in the first month of the loan phase is reduced with 48 percentage points (column 2). Even though the size of the treatment effects diminishes for both of these main outcome

[33] Recall that only students are included in the sample who enter the loan phase in September, see Section 4.1.

Figure 4. IBL amount in the 1st, 6th and 12th month of the loan phase for the cohorts of students in group 1, entering the loan phase from 2006-2013.



Note: X-axis represents the cohort of students entering the loan phase, where only students entering the loan phase in September are included. The dots represent the average loan amount for students in Group 1 in the 1st, 6th and 12th month of the loan phase, the red striped line represents the time of the treatment. No control variables are included.

variables of interest, the treatment effect remains sizable and statistically significant for at least the first 12 months of the loan phase.

The third column presents the effect of the treatment on the chance a student has a requested interest-bearing loan amount (of larger than €0). One might expect this variable to be unaffected by the treatment, since both before and after the treatment the students received a positive loan amount by default. However, the chance a student borrows a positive amount in the first month of the loan phase is reduced with 12 percentage points.

A possible explanation for this observation, is the anchoring effect, discussed in Section 3.2. Prior to the treatment, students in group 1 received approximately €819, whereas they received €93 or €260 (depending on the living situation) after the treatment (see Appendix A). Students receiving €819 might be more inclined to borrow a positive amount compared to students receiving €93 or €260, since the step from the anchor (the default loan amount) to €0 (no loan) is significantly larger. A potential explanation for the fact this effect is most prevalent in the first 2 months is that the majority of students students who deviate from the default, do so in the first month(s).[34]

Groups 2 and 3. With respect to group 2, the estimates in column 4 indicate that the effect of the treatment on the requested interest-bearing loan amount, though sizable in month 1 of

[34]This is confirmed by Figure 8, which reports the average amount of students opting for the default option by group.

the loan phase, becomes insignificant as of month 9 (May). Students in group 3, who before the treatment received the maximum allowed loan amount (€819), on average borrow €52 less in the first month of the loan phase because of the treatment (column 7). Furthermore, the chance students in this group borrow the maximum amount is reduced with 76 percentage points (column 8). Both effects are relatively persistent. This could be explained by the fact students were not aware of the fact the maximum borrowing amount in the loan phase exceeds the sum of the grants and the maximum (interest-bearing) loan amount of the grant phase.

The decrease in the treatment effect in January can be explained by the fact the maximum loan amount in the loan phase was increased from €832,43 to €853 in January 2010, which only influenced the control group. Since, before the treatment, students in group 3 following the default option were borrowing the maximum possible amount, and the requested (maximum) loan amount was automatically adjusted in case the maximum possible amount was adjusted. After the treatment, the loan amount was no longer adjusted automatically when students opted for the default loan option.

Group 4. With respect to group 4, who received no interest-bearing loan amount by default prior to the treatment, the effects are once again sizable, with the exception of the effect on the chance a student borrows the maximum amount. The requested loan amount is increased with €93 in the first month of the loan phase (column 10). The treatment effect remains statistically significant, but does diminish over time: the requested loan amount is increased with €36 in the twelfth month of the loan phase (August).

Recall that before the treatment students in group 4, by default, received an IBL amount of €0 when entering the loan phase. After the treatment, this group by default received an IBL amount of in between €260 and €493 (depending on the living situation and supplementary grant received). Since students in this group borrowed €0 in the grant phase, the treatment essentially 'pushed' students into borrowing a positive amount. Column 12 reports that, on average, the chance that students borrow a positive amount increases with 33 percentage points (column 12) in the first month, and with 12 percentage points in the twelfth month (August). Concluding, it seems that providing a non-zero default loan amount compared to a zero default loan amount has a strong impact on the decision of whether or not to borrow. This observation is line with the findings of Marx and Turner (2017), who find that students receiving a nonzero loan offer (either $3,500 or $4,500) were 40 percent more likely to borrow than students who received a $0 loan offer.

6.1.2 Differences-in-differences estimation results, for group 1. As discussed in Section 4.1, the difference in the time of the treatment for the different groups of students can be exploited in a DD design. This section presents and discusses the results of the DD estimations of the effect of the change in the default loan amount for group 1.

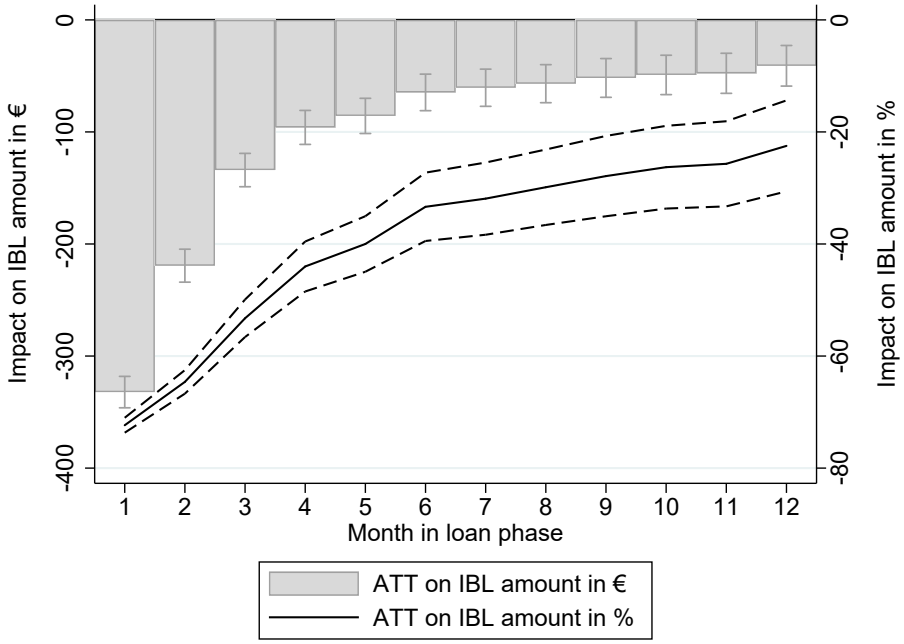
Table 7 reports the estimates of a (OLS/DD) regression analysis of equation 3, for different

Table 7. DD estimates of the change in the standard loan amount in the loan phase, preferred DD specification with all controls.

	(1) IBL	(2) Max IBL	(3) IBL > 0
Sep	-332.53***	-0.48***	-0.16***
	(7.10)	(0.01)	(0.01)
Oct	-219.12***	-0.32***	-0.09***
	(7.41)	(0.01)	(0.01)
Nov	-133.77***	-0.21***	-0.02
	(7.46)	(0.01)	(0.01)
Dec	-95.14***	-0.15***	0.00
	(7.55)	(0.01)	(0.01)
Jan	-85.19***	-0.14***	0.02
	(7.81)	(0.01)	(0.01)
Feb	-63.76***	-0.11***	0.03**
	(8.09)	(0.01)	(0.01)
Mar	-57.92***	-0.10***	0.04***
	(8.23)	(0.01)	(0.01)
Apr	-53.94***	-0.09***	0.04**
	(8.39)	(0.01)	(0.01)
May	-49.35***	-0.08***	0.04**
	(8.55)	(0.01)	(0.01)
Jun	-46.68***	-0.08***	0.03**
	(8.70)	(0.01)	(0.01)
Jul	-45.12***	-0.08***	0.03**
	(8.85)	(0.01)	(0.01)
Aug	-38.92***	-0.07***	0.04**
	(8.97)	(0.01)	(0.01)
Controls	Yes	Yes	Yes
$\bar{Y}$	157.46	0.11	0.34
sd(Y)	275.48	0.31	0.47
Adj. R^2	0.11	0.11	0.08
N	202,076	202,076	202,076

Note: The table reports estimates from a DD regression (of equation 3) of the effect of the treatment (for group 1) on three different outcome variables: (1) "IBL" = Interest-bearing loan amount, in € ; (2) "Max IBL" = dummy equal to 1 if student borrows the maximum IBL; (3) "IBL > 0" = dummy equal to 1 if IBL > 0. Students in group 4 who did not receive supplementary grant (in the grant phase) are used as the control group. Students entering the loan phase in September 2008 and 2009 are included in the sample, the included controls are listed in Section 4.1. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

Figure 5. Impact of the default loan amount change in the first 12 months after entering the loan phase for students who did not borrow in the grant phase.



Note: This figure plots the results from the differences-in-differences specification (equation 3) without control variables. The left y-axis reports the impact of the treatment on the requested IBL in €. The right y-axis presents the percentage change (relative to the estimated counterfactual of the treatment group) of the in the requested interest-bearing loan amount. The vertical lines and the dashed lines represent the 95% confidence interval.

outcome variables, including all (individual-level) control variables listed in Section 4.1. In the first month after entering the loan phase (September) students in group 1 on average borrowed €333 less because of the change in the default loan amount. Additionally, the chance students borrowed the maximum loan amount is on average reduced with 48 percentage points. Both effects are decreasing in size over time, since students actively adjust their loan amounts in the next months: the treatment effect in the fourth month of the loan phase (December) has reduced to €95.

In order to be able to present the percentage reduction of the requested loan amount, the counterfactual (would the treated group not have been treated) is estimated for the model with no controls. This counterfactual is used to calculate the relative change in the interest-bearing loan amount are reported in Figure 5 (right y-axis). These percentage changes correspond to the changes in euro reported in Figure 5 (left y-axis) and in column 1 of Table C.3 (DD model with no controls), which are very similar to the estimates in column 1 of Table 7 (DD model with all controls). Consequently, the reduction in the IBL amount of approximately €330 in September, corresponds to a reduction of 72.34%, and the reduction of €95 in December corresponds to a reduction of 44.01%.

Summing up all the monthly treatment effects, amounts to a total reduction of the requested loan amount of approximately €1221 in the first year of the loan phase. Alternatively, a basic

DD estimation (equation 2 without controls) is preformed on data collapsed by academic year, implying only 1 observation per student is included, and this results in an estimated treatment effect of - €1207. This amounts to a total average reduction in the requested interest-bearing loan amount in the first year of the loan phase of approximately 46%, relative to the estimated counterfactual of the treated group. Concluding, the change of the default loan amount had an economically and statistically significant impact on the borrowing behavior of students in the loan phase.

6.1.3 Heterogeneous treatment impact and effects. The results of the previous section presented the average treatment effects for all students in group 1, which essentially is the average treatment effect for two subgroups of students within group 1: students living with their parents and students not living with their parents. Recall from Section 2.2.1, the impact of the treatment was either a reduction in the standard loan amount of €727 for students living with their parents and or a reduction of €564 for students not living with their parents (a difference of €163).[35] This section presents the impact of the treatment separately for these two subgroups. Subsequently, the second part of this section explores if there is any variation in the treatment effect across different coviarate groups (gender, type of higher education and migration background). The estimation of heterogeneous treatment effects across different covariate groups provides valuable insights into whether or not certain subgroups are more (less) influenced by a default option.

Heterogeneous treatment impact, by living situation. Table 8 report the estimates of the regressing equation 3 separately for students living with their parents and students not living with their parents. Columns 1 and 2 of Table 8 report the estimates using the complete sample, which correspond to the estimates reported in columns 1 and 2 of Table 7. The difference in the estimated treatment effect in the first month of the loan phase (September) is substantial and comes close to the difference in the size of the treatment. On average students living with their parents borrow €424 less (in the first month of the loan phase) due to the reduction of the default loan amount of €727, whereas students not living with their parents on average borrow €283 less (in the first month of the loan phase) due to the reduction of the default loan amount of €564. The chance that students borrow the maximum amount, is on average reduced with 56 (44) percentage points for students (not) living with their parents.

Nevertheless, note that the difference in the treatment effect between the two subgroups can either be because (1) students living with their parents respond differently to a default amount change than students not living with their parents, or (2) because the difference in the default loan amount change (size of the treatment). However, it is unfortunately not possible to quantify

[35] For groups 2, 3 and 4 the impact of the treatment is also influenced by the amount of supplementary grant received. However, students in group 1 cannot receive a supplementary grant by construction. Hence, there are only two possible values of the impact of the treatment for group 1.

Table 8. Effect of the default loan amount change for group 1 on borrowing behavior, by living situation.

	All		Living with parents		Not living with parents	
	(1) IBL	(2) Max IBL	(3) IBL	(4) Max IBL	(5) IBL	(6) Max IBL
Sep	-332.53***	-0.48***	-423.51***	-0.56***	-282.88***	-0.44***
	(7.10)	(0.01)	(10.74)	(0.01)	(9.09)	(0.01)
Oct	-219.12***	-0.32***	-315.20***	-0.42***	-167.49***	-0.27***
	(7.41)	(0.01)	(11.25)	(0.01)	(9.48)	(0.01)
Nov	-133.77***	-0.21***	-202.26***	-0.27***	-97.39***	-0.17***
	(7.46)	(0.01)	(11.11)	(0.01)	(9.64)	(0.01)
Dec	-95.14***	-0.15***	-157.12***	-0.21***	-62.58***	-0.12***
	(7.55)	(0.01)	(11.05)	(0.01)	(9.84)	(0.01)
Jan	-85.19***	-0.14***	-136.58***	-0.19***	-58.80***	-0.11***
	(7.81)	(0.01)	(11.46)	(0.01)	(10.16)	(0.01)
Feb	-63.76***	-0.11***	-119.52***	-0.16***	-35.99***	-0.08***
	(8.09)	(0.01)	(11.81)	(0.01)	(10.52)	(0.01)
Mar	-57.92***	-0.10***	-109.21***	-0.15***	-32.54***	-0.08***
	(8.23)	(0.01)	(11.91)	(0.01)	(10.69)	(0.01)
Apr	-53.94***	-0.09***	-110.19***	-0.15***	-26.70**	-0.06***
	(8.39)	(0.01)	(12.03)	(0.01)	(10.92)	(0.01)
May	-49.35***	-0.08***	-103.79***	-0.14***	-23.62**	-0.06***
	(8.55)	(0.01)	(12.35)	(0.01)	(11.11)	(0.01)
Jun	-46.68***	-0.08***	-97.30***	-0.14***	-22.77**	-0.05***
	(8.70)	(0.01)	(12.56)	(0.01)	(11.30)	(0.01)
Jul	-45.12***	-0.08***	-92.54***	-0.13***	-22.99**	-0.05***
	(8.85)	(0.01)	(12.72)	(0.01)	(11.50)	(0.01)
Aug	-38.92***	-0.07***	-77.55***	-0.11***	-21.23*	-0.05***
	(8.97)	(0.01)	(12.95)	(0.01)	(11.61)	(0.01)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
$\bar{Y}$	157.46	0.11	106.08	0.09	183.45	0.12
sd(Y)	275.48	0.31	250.66	0.29	283.70	0.32
Adj. R^2	0.11	0.11	0.17	0.20	0.07	0.08
N	202,076	202,076	67,894	67,894	134,182	134,182

Note: The table reports estimates from a DD regression, of equation 3, of the effect of the treatment (for group 1) on two different outcome variables: (1) "IBL" = Interest-bearing loan amount, in € ; (2) "Max IBL" = dummy equal to 1 if student borrows the maximum IBL. Students in group 4 who did not receive supplementary grant (in the grant phase) are used as the control group. Students entering the loan phase in September 2008 and September 2009 are included in the sample. All controls listed in Section 4.1 are included. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

how much of the difference in the treatment effect is caused by the difference in the magnitude of the treatment.

Heterogeneous treatment effect across different covariate groups. In order to accurately measure the difference in the treatment effect across different covariate groups, it is important to control for the difference in the impact of the treatment discussed above. This is because the average living situation is different across different covariate groups. For example, the proportion of males and higher vocational educational students that are living with their parents (42.6% and 51.7% respectively) is much higher than females and university students (27.4% and 25.5% respectively).[36] Therefore, in order to correctly measure the heterogeneous treatment effect (holding the impact of the treatment constant), Table 9 and Table 10 report the treatment effect for different subgroups separately for students living with their parents and students not living with parents, respectively.

Table 9 reports larger treatment effects (in absolute terms) for males and students of higher vocational education (hbo), compared to females and university students. There is no significant difference in the treatment effect between students with or without a migration background, for students not living with their parents. Table 10 reports larger treatment effects (in absolute terms) for higher vocational students and students with a migration background. The difference in the treatment effect between males and females is not significant for students not living with their parents.

These two tables bring forward a number of interesting observations. First, only the difference between higher vocational education and university students is robust across the two tables. It can be concluded that higher vocational education students are substantially more influenced by the treatment (column 4), both compared to university students (column 5) or all students (column 1). Second, males living with their parents were also more influenced by treatment, both compared to females and all students living with their parents. Third, students with a migration background not living with their parents were also more heavily influenced by the treatment, both compared to students without a migration background and all students not living with their parents. Fourth, more generally, it can be concluded that there is considerable variation in the size of impact on the borrowing behavior, caused by the default loan amount in September 2009 for group 1, across different subgroups.

These findings imply that higher vocational education students (hbo) and, to a lesser extent, males and students with a migration background are more affected by the default loan amount change. This suggests that the mechanisms driving the impact of the default loan amount change, are more prevalent for these sub groups. Recall the three theoretical mechanisms discussed in Section 3.2 are: (1) implicit recommendation, (2) status quo bias, inertia and procrastination of decision making, (3) anchoring bias or reference point. Nevertheless, there is limited evidence on how these mechanisms differ among these specific sub groups. There

[36] These percentages are calculated by using the sample of students entering the loan phase in September 2009.

Table 9. Heterogeneous effects of the default loan amount change for students living with their parents.

		Gender		Study type		Mig. background	
	(1) All	(2) Female	(3) Male	(4) hbo	(5) wo	(6) No Mig.	(7) Mig.
Treatment	-169.52***	-114.70***	-197.79***	-238.65***	-81.50***	-169.67***	-168.11***
	(9.47)	(15.00)	(11.92)	(12.98)	(13.35)	(9.92)	(29.20)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\bar{Y}$	106.08	84.85	117.38	123.52	84.83	101.67	132.70
sd(Y)	250.66	222.67	263.66	269.50	223.77	245.49	278.39
Adj. R^2	0.13	0.11	0.16	0.18	0.08	0.14	0.13
N	67,894	23,580	44,314	37,287	30,607	58,246	9,648

Note: The table reports estimates from a DD regression, of equation 2, of the effect of the treatment (for group 1) on the requested IBL amount (in €) for different samples, for students living with their parents. wo (hbo) refers to students enrolled in university (higher vocational education). Students in group 4 who did not receive supplementary grant (in the grant phase) are used as the control group. Students entering the loan phase in September 2008 and September 2009 are included in the sample. The heterogeneous effects are significantly different from one another at the 1% confidence level for gender and study type, the difference in the treatment effect for migration background is insignificant. The statistical significance of the difference in the treatment effect is tested by estimating a regression model including interaction terms (not included in the table). All controls listed in Section 4.1 are included. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

Table 10. Heterogeneous effects of the default loan amount change for students not living with their parents.

		Gender		Study type		Mig. background	
	(1) All	(2) Female	(3) Male	(4) hbo	(5) wo	(6) No Mig.	(7) Mig.
Treatment	-74.86***	-70.47***	-78.09***	-116.33***	-54.73***	-66.84***	-132.87***
	(8.61)	(11.42)	(12.96)	(15.58)	(10.32)	(9.11)	(25.75)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\bar{Y}$	183.45	168.42	200.02	184.70	182.93	179.41	211.09
sd(Y)	283.70	272.10	295.07	290.19	280.91	280.85	300.98
Adj. R^2	0.05	0.04	0.06	0.08	0.05	0.05	0.08
N	134,182	70,343	63,839	39,918	94,264	117,039	17,143

Note: The table reports estimates from a DD regression, of equation 2, of the effect of the treatment (for group 1) on the requested IBL amount (in €) for different samples, for students not living with their parents. wo (hbo) refers to students enrolled in university (higher vocational education). Students in group 4 who did not receive supplementary grant (in the grant phase) are used as the control group. Students entering the loan phase in September 2008 and September 2009 are included in the sample. The heterogeneous effects are significantly different from one another at the 1% confidence level for study type, 5% confidence level for migration background. The treatment effect is insignificantly different between females and males. The statistical significance of the difference in the treatment effect is tested by estimating a regression model including interaction terms (not included in the table). All controls listed in Section 4.1 are included. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

is some evidence with respect to systematic gender differences.[37] Even though it is unclear how the first mechanism will differ by gender, Van Rooij et al. (2008) find that procrastination is more prevalent under men. At the same time, studies show there is no systematic gender differences in anchoring behavior (Jetter and Walker, 2016; Beblo et al., 2017). This suggests that differences in procrastination behavior might cause the gender differences in the impact of default option change. It remains unclear what is exactly driving the heterogeneity of the treatment effect with respect to higher vocational education students and students with a migration background.

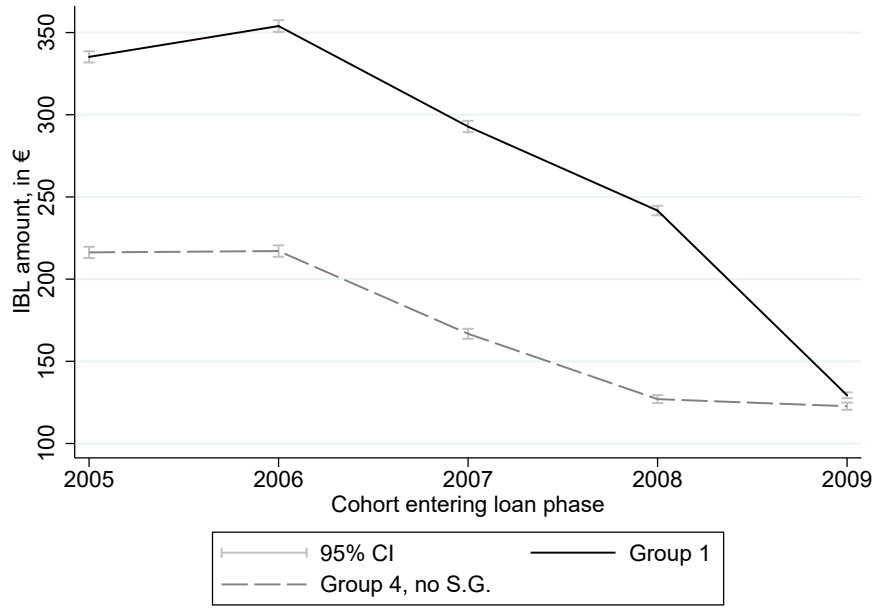
6.1.4 Examining the internal validity of the DD design. This section examines the internal validity of the DD estimations. First, several robustness checks of the preferred specification reported in Section 6.1.2 are discussed. Second, the validity of main identifying assumption of the DD empirical strategy: the common trend assumption, is approximated. Third, the potential existence of sample selection bias is examined. From this section it can be concluded that the degree of internal validity of the DD analysis is high.

Robustness of the specification A number of checks are preformed to confirm the robustness of the specification used in Table 7. First, note that the treatment effects in Table 7 are remarkably similar to the effects reported in Table 6: the before-after comparison of cohorts. The fact that these estimates are similar provides confidence in the accuracy of the treatment effect. Second, Appendix C.2.2 shows that the estimations from the preferred specification presented in Table 7 are robust to using different specifications: (1) excluding the (individual-level) control variables and (2) including multiple pre-treatment periods and the inclusion of group specific time trends (as proposed by Angrist and Pischke, 2008). Third, Appendix C.2.3 shows the estimates are unchanged when only using the student finance data set, thereby keeping the observations that are dropped due to the merge of the different data sets used to generate the control variables (see Appendix B.1). Fourth, Appendix C.2.4 explores the whether the DD results are robust to using different control groups, where it is concluded group 4 indeed is the most suitable control group. Lastly, Appendix C.2.5 confirms that the reduction in the regular loan amount due to the treatment, is not compensated by an increase in an alternative loan: the tuition fee credit (TFC).

Validity of the common trend assumption. The main identifying assumption of the DD design is the common trend assumption. The validity of the common trend assumption can be approximated at best, due to the lack of the counterfactual. Figure 6 plots the average interest-bearing loan amounts for the treated group (group 1) and the control group (group 4 without

[37] There is also evidence that anchoring effect Beblo et al. (2017) and intertia/status quo bias (Van Rooij et al., 2008) is less prevalent under people with higher levels of education. However, higher vocational education and university education are both regarded as higher education and therefore fall into the same level of education in the majority of the the literature.

Figure 6. Treatment 1, common trend of the IBL amount.



Note: The dependent variable is IBL: the interest-bearing loan amount. The average values by group and cohort (of entering the loan phase) are plotted in order to approximate the validity of the common trend assumption. The sample consist of students in group 1 (treatment group) and group 4 (excluding the students who received a supplementary grant in the grant phase), who enter the loan phase in September in different years (2005-2009). The vertical lines represent the 95% confidence interval.

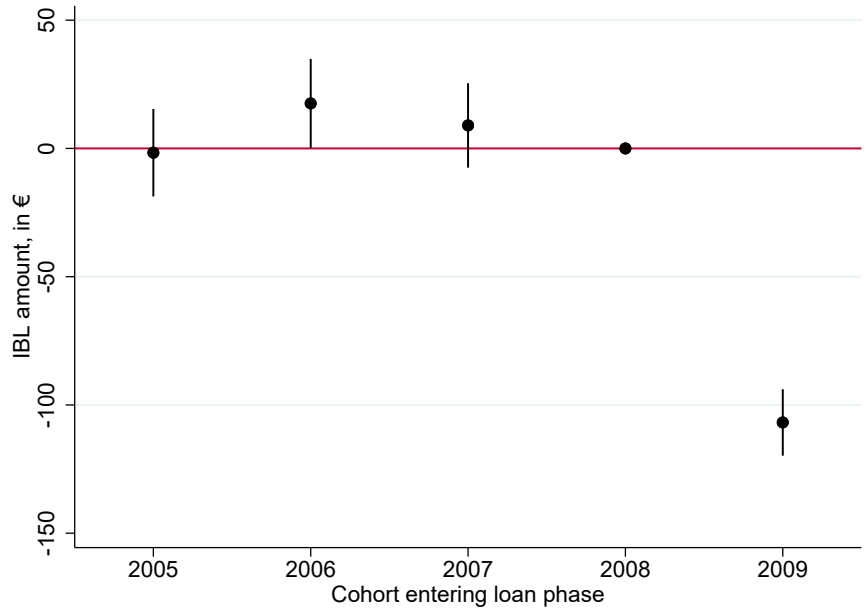
supplementary grant in the grant phase). Judging from visual inspection, the common trend assumption seems to be valid.

Nevertheless, Figure 7 presents a precise examination/approximation of the validity of the common trend assumption. Figure 7 plots the estimates from a placebo treatment regression, including all control variables, with the requested loan amount as the dependent variable.[38] Multiple pre-treatment periods are included, and the last pre-treatment period is the reference category. The validity of the common trend assumption is likely if the 'placebo treatments' in the pre-treatment periods (2005, 2006 and 2007 in this case) are insignificant. Note that the coefficient for 2009 presents the treatment effect, as estimated by regressing equation 2.

Even though the coefficient for 2006 is significantly different from 0, with a confidence interval of 95%, by a small amount, it seems reasonable to assume the common trend assumption is not violated. Appendix C.2.1 includes figures for other outcome variables, and the overall picture of the validity of the common trend assumption is similar. Since it is reasonable to assume the common trend assumption is valid, the regression results present unbiased estimates of the effect of the change in the default loan amount on the borrowing behavior of students in group 1.

[38]This involves estimating the difference-in-differences model over the pre-treatment period, but with the assumption that the treatment took effect at an earlier date. Since this treatment precedes the actual default loan amount change investigated, the difference-in-difference estimator should be statistically insignificant and close to zero.

Figure 7. Treatment 1, placebo treatment plot to examine common trend of the IBL amount.



Note: The dependent variable is IBL: the interest-bearing loan amount. The figure includes plots of the coefficients of a placebo treatment regression. Sample consist of students in group 1 (treatment group) and group 4 (excluding the students who received a supplementary grant in the grant phase), who enter the loan phase in September in different years (2005-2009). All controls are included in this placebo treatment regression. The vertical lines represent the 95% confidence interval.

Sample selection bias. First of all, it is important all groups, but especially the main groups of interest (1 and 4), have sufficient observations in order to have enough power in the regression analysis. Table 11 presents the sizes of the four different groups of students in years of interest. It can be concluded all groups have a sizable amount of students in the years of interest (2008-2010). In addition, it is important to note that the relative size of each group remains approximately stable over the years. Changes in the relative size of each group would be worrying since this could indicate anticipation effects.

Second, another potential threat to the internal validity is that the composition of the treatment and control groups might change as a result of the treatment, causing sample selection bias. As discussed in Section 2.2.1, the default loan amount is determined by DUO based on student finance data in the last month of the grant phase. Recall that students receive a letter from DUO one month prior to entering the loan phase, in which information is provided about the standard/default loan amount they will receive when entering the loan phase. It could potentially have been the case that students adjusted their loan amounts in advance differently (in the last month of the grant phase already), anticipating the treatment. Note that this is very unlikely for two reasons. First, the content of this letter did not change significantly as a result of the treatment. Second, when students want to adjust their requested student finance amounts, students can indicate in which month their requested adjustment should be made. Therefore, students can indicate a couple of months in advance what amount they want to receive when

Table 11. Sizes of different groups, for cohorts of students entering the loan phase from 2008-2010.

	Group 1	Group 2	Group 3	Group 4	Total
2008	4,955	3,119	8,451	7,883	24,408
	(20.3)	(12.8)	(34.6)	(32.3)	(100.0)
2009	5,411	3,252	8,536	8,678	25,877
	(20.9)	(12.6)	(33.0)	(33.5)	(100.0)
2010	5,648	3,091	7,901	8,940	25,580
	(22.1)	(12.1)	(30.9)	(34.9)	(100.0)
Total	16,014	9,462	24,888	25,501	75,865
	(21.1)	(12.5)	(32.8)	(33.6)	(100.0)

Row Percentages in Parentheses

Note: Sample consist of students who enter the loan phase in September of the academic year listed on the left side of the table. Only September (the first month of the loan phase) is included, so that each student is included only once in the sample.

entering the loan phase. In order to check if the composition indeed did not change due to the treatment, Table 11 is replicated but now defining the groups based on the borrowing behavior three months prior to entering the loan phase in Table C.1 in Appendix C.2.1. It can be concluded the sizes of the groups do not differ substantially as measured three months or one month before entering the loan phase. Consequently, it is unlikely that the treatment caused a change in the composition of the groups.

Lastly, sample selection bias could potentially be caused by dynamic sample selection, since data for multiple periods (months) after the time of treatment (entrance to the loan phase) is used in the DD model. From the students entering the loan phase in September 2009, 82.8% is still in the data set after 12 months in the loan phase and only 63% is still in the data after 13 months. This is due to the fact the 13th month is the start of a new academic year, and this is the main reason why only the first 12 months are included in the DD model. Note that there could be multiple reasons as to why a student is not in the student finance data set anymore, for example, the student could have: dropped out, successfully completed the study or stopped requesting/receiving student finance components. Appendix C.2.6 checks for the existence of dynamic selection, in other words: did the treatment influence the amount of students that are no longer in the data set as of some point during the first year of the loan phase. It is concluded that it is not likely dynamic selection has a large impact on the results.

6.1.5 March 2010 default loan amount change for groups 2, 3 & 4. The difference in the timing of the treatment provided an opportunity to accurately estimate the effect of the default loan amount change for group 1 by means of a DD design, as discussed in Sections 6.1.2 - 6.1.4. However, a DD strategy, in theory, could also be employed to estimate the impact of the nudge on groups 2, 3 and 4, using group 1 as the control group.

Considering the default loan amount did not change for group 1 between September 2009 and September 2010, it can serve as a control group to estimate the impact of the treatment on the other 3 groups. Equations 2 and 3 can be estimated (separately by group), with group 2, 3 or 4 being the treated group. However, instead of using students entering the loan phase in September 2008 and 2009, now students entering the loan phase in September 2009 and 2010 are included in the sample. However, an important limitation of the DD approach in this case is that it is not possible to test for validity of the common trend assumption. This is due to the fact group 1 (now the control group), was treated on September 2009 and thus had different default loan amounts in the years prior to 2009.

Nevertheless, the DD analysis serves as an additional estimation of the treatment effect for groups 2, 3 and 4 on top of the before-after comparison of cohorts presented in Table 6. Even though there are differences between the effects reported in columns 4 and 5 (group 2), 7 and 8 (group 3) and 10 and 12 (group 4) of Table 6 (discussed in Section 6.1.1) and the estimates reported in Table 12, the reported effects are in the same directions, follow the same patterns and are within the same order of magnitude. The most notable difference between the two tables, is that in Table 12 the estimates for group 2 are larger in magnitude and more significant in more months.

As discussed, both models have downsides, however, the fact that both specifications provide relatively similar results is a good sign. At the minimum, it indicates these groups of students were also influenced by the default loan amount in a significant matter.

6.1.6 How many students opted for the default option? The results presented in the previous sections indicate the default loan amounts significantly influences the borrowing behavior of students, in all 4 groups. Furthermore, as discussed in Section 2.3, the default loan amount was changed in order for it to be more aligned with the expected preferences of the students (especially with respect to group 1). This section explores how many students comply with and stick to the default option, and provides suggestive evidence of whether or not this was influenced by the treatment.

Figure 8 presents the percentage of students opting for and sticking with the default loan amount (option) in the first month, first 3 months, first 6 months and first 12 months of the loan phase, for all 4 groups.[39] From visual inspection it can be concluded that for groups 1, 3 and 4 the treatment led to less people following the default option in the first month. However, for group 1 it is striking to see that there seems to be no increase in the amount of people sticking to the default, indicating that a large amount of students in group 1 quickly adjusted their requested loan amount, deviating from the default option, prior to the treatment. With respect to group 3, close to 90% of the students follow the default option in the first month of the loan phase,

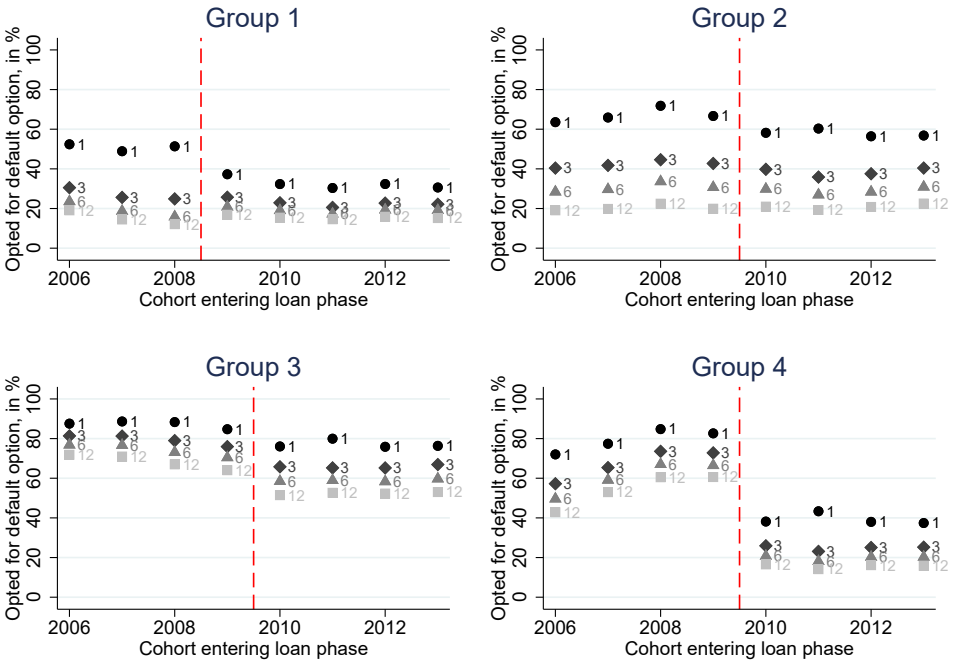
[39]These dummy variables are equal to 1 in case the student follows and sticks to the default option, and equal to 0 if the student deviates from the default option in any way. If a student drops out of the data set, the dummy variable is set as missing.

Table 12. March 2010 default loan amount change for groups 2-4, DD estimation results.

	Group 2		Group 3		Group 4	
	(1) IBL	(2) Max IBL	(3) IBL	(4) Max IBL	(5) IBL	(6) IBL > 0
Sep	142.57***	-0.01	-38.16***	-0.76***	104.67***	0.40***
	(6.38)	(0.01)	(4.69)	(0.01)	(4.43)	(0.01)
Oct	84.33***	-0.03***	-33.11***	-0.69***	73.24***	0.29***
	(6.92)	(0.01)	(5.03)	(0.01)	(4.73)	(0.01)
Nov	50.22***	-0.05***	-33.96***	-0.65***	51.61***	0.22***
	(7.23)	(0.01)	(5.23)	(0.01)	(4.97)	(0.01)
Dec	41.81***	-0.05***	-28.62***	-0.62***	45.45***	0.19***
	(7.59)	(0.01)	(5.46)	(0.01)	(5.24)	(0.01)
Jan	30.25***	-0.05***	-47.35***	-0.61***	42.86***	0.18***
	(7.92)	(0.01)	(5.70)	(0.01)	(5.60)	(0.01)
Feb	30.05***	-0.04***	-41.79***	-0.58***	38.50***	0.17***
	(8.20)	(0.01)	(5.89)	(0.01)	(5.83)	(0.01)
Mar	28.04***	-0.03**	-38.51***	-0.55***	37.81***	0.16***
	(8.45)	(0.01)	(6.04)	(0.01)	(6.00)	(0.01)
Apr	31.10***	-0.02	-34.01***	-0.53***	38.26***	0.16***
	(8.76)	(0.01)	(6.24)	(0.01)	(6.23)	(0.01)
May	22.56**	-0.03**	-38.63***	-0.53***	31.47***	0.15***
	(8.89)	(0.01)	(6.34)	(0.01)	(6.36)	(0.01)
Jun	19.60**	-0.03**	-30.97***	-0.51***	34.14***	0.14***
	(9.14)	(0.01)	(6.50)	(0.01)	(6.55)	(0.01)
Jul	21.79**	-0.03***	-29.65***	-0.51***	34.44***	0.14***
	(9.37)	(0.01)	(6.60)	(0.01)	(6.69)	(0.01)
Aug	19.80**	-0.03**	-33.95***	-0.50***	37.41***	0.14***
	(9.70)	(0.01)	(6.84)	(0.01)	(6.90)	(0.01)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
$\bar{Y}$	232.56	0.09	473.51	0.31	141.58	0.36
sd(Y)	279.12	0.29	371.38	0.46	242.06	0.48
Adj. R^2	0.31	0.07	0.60	0.47	0.09	0.07
N	188,031	188,031	303,108	303,108	307,520	307,520

Note: The table reports estimates from a DD regression (each column is a different estimation of equation 3) of the effect of the default loan amount change in March 2010 that affected groups 2, 3 and 4 on three different outcome variables: (1) "IBL" = Interest-bearing loan amount, in € (columns 1, 3 and 5); (2) "Max IBL" = dummy equal to 1 if student borrows the maximum IBL (columns 2 and 4); (3) "IBL > 0" = dummy equal to 1 if IBL > 0 (column 6). Students entering the loan phase in September 2009 and September 2010 are included in the sample. Group 1 is used as the control group. All controls listed in Section 4.1 are included. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

Figure 8. Opting for the default loan amount when entering the loan phase, for 1, 3, 6 and 12 months in the loan phase.

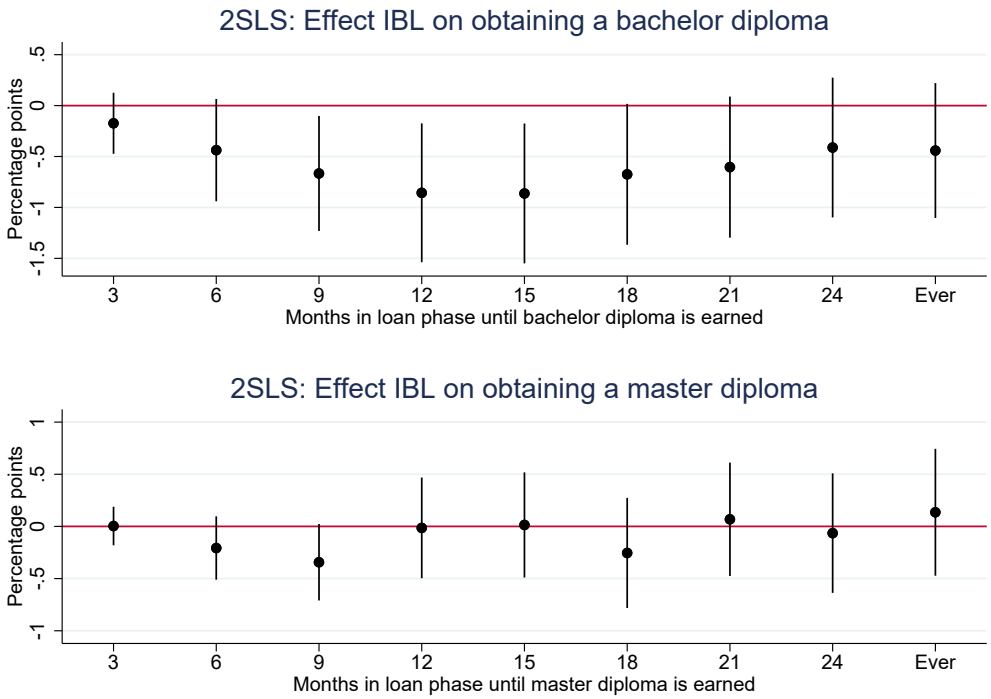


Note: This figure presents the average amount of students, per group, opting for the default loan amount after 1, 3, 6 and 12 month(s) of the loan phase. This is a dummy variable which is equal to 1 in case the students follow the default option, note that for the 3/6/12 month dummy to be equal to 1 the student must request the default loan amounts in all months up to and including month 3/6/12. The vertical red line indicates the time of the treatment.

which is reduced to around 80% after the treatment. For this group the default option prior to the treatment seems to be more popular. A potential explanation for this pattern is that students in this group borrow the maximum loan amount in the grant phase and therefore are likely to want to borrow the maximum possible amount in the loan phase as well. A similar explanation can be put forward with respect to group 4, where after the treatment a substantially lower amount of students opt for the default option. Recall that students in group 4 actively requested to borrow €0 in the (last month of the) grant phase. Consequently, the default amount before the treatment (€0) seems to be more in line with the preferences of students in this group, since after the treatment, students in this group are by default assigned a positive interest-bearing loan amount (see Table 2).

6.1.7 Effect of borrowing behavior on study success (2SLS). The previous sections were concerned with the effect of the default loan amount change on the borrowing behavior of students. However, it is also valuable to know more about what the consequences of borrowing more or less are, with respect to educational attainment or study success. There could be multiple mechanisms through which borrowing behavior can influence study success. First, nudging students to borrow more could increase study success if students are debt averse, and consequently under invest in education from a life-time perspective. Second, if students face trade-offs between

Figure 9. Estimated coefficients of the second stage of multiple 2SLS analyses, estimating the effect of borrowing €100 more on the chance a student earns a diploma within x months after entering the loan phase, in %-points.



Note: This figure presents the estimated coefficients of the second stage of 9 different 2SLS estimations of equation 6, where in the first 8 models a dummy equal to 1 if the student has earned a bachelor (top panel) or a master (bottom panel) diploma within 3, 6, 9, 15, 18, 21 or 24 months after entering the loan phase is used. The dependent variable in the last model is a dummy equal to 1 if the student ever earns a bachelor or master diploma after entering the loan phase. The vertical line represents the 95% confidence interval. All controls listed in Section 4.1 are included. Additionally, whether or not the student is enrolled in a bachelor (top panel) or master (bottom panel) program when entering the loan phase is added as a control variable.

spending their time working or studying, nudging students to borrow less (more) could increase (decrease) the time spend on work on consequently decrease (increase) study success. 34% of the respondents of a survey among Dutch student indicate that they borrow money from DUO in order to work less while studying (Nibud, 2017). On the other hand, it is also possible that nudging students to borrow less (more) only causes students to take on less (additional) debt, without having an effect on the time spend on studying and/or study success (Lochner and Monge-Naranjo, 2012; Avery and Turner, 2012).

Figure 9 reports the coefficients of interest from the estimation of equation 6. These are the coefficients of 2SLS estimations where the the effect of the treatment in the first month of the loan phase for group 1 is used as the exogenous shock to the borrowing behavior (first stage), in order to estimate the effect of borrowing €100 more on the chance a student obtains a bachelor or master diploma.[40] The top panel of Figure 9 indicates that borrowing €100 more reduces the

[40] As discussed in Section 4.1, only the effect of the treatment in the first month of the loan phase used as the exogenous shock to the borrowing behavior (the first stage). This implies that there is only 1 observation used per

chance to obtain a bachelor diploma within 12 months after entering the loan phase with 0.86 percentage points. The effect also is statistically significant, at the 95% confidence level, for the chance to obtain a bachelor diploma within 9 and 15 months. There is no significant effect on the probability to ever obtain a master diploma after entering the loan phase. Furthermore, there is no significant effect of borrowing behavior on the chance to obtain a master diploma; all of the estimated coefficients are insignificant in the bottom panel of Figure 9.[41]

Recall from Section 4.1.3 that the 2SLS results are only informative about the students who change their borrowing behavior due the nudge: the complier students. Therefore, it is useful to know more about who complier students are. Appendix C.3.3 explores the characteristics of complier students. As discussed in Angrist and Pischke (2008), in the case of characteristics that can be described by dummy variables, we can learn more about complier characteristics from the variation in the first stage across covariate groups. From Table C.8 in Appendix C.3.3, it can be concluded compliers are more likely to be males, higher vocational education students (hbo) and students with a migration background. Compliers are less likely to be females, university students and students without a migration background.[42]

The estimates of the reduced form, which is a (OLS/DD) regression of the dependent variable of interest (study success) on the instrument (the nudge) and exogenous variables are presented in Figure 10.[43] The reduced form estimates the effect of the default loan amount change on study success. For example, the reduction in the default loan amount for group 1 resulted in an increase of 2.8 percentage points in the chance students earn a bachelor diploma within 12 months. Figure 10 reports the same pattern with respect to statistical significance compared to the 2SLS estimates. Angrist and Krueger (2001) note that an insignificant coefficient in the reduced form regression usually implicates that the causal effect of interest either absent, or the instrument is too weak to detect it. Appendix C.3.2 discusses the common trend assumption underlying the reduced form estimates, where it is concluded the common trend assumption is likely to be valid.

In conclusion, the estimates of some 2SLS estimations suggest that borrowing less actually increases the study success of students in the loan phase.[44] This finding is counter intuitive,

student, which is the first month of the loan phase. Consequently, the first stage corresponds with the first row of column 1 of Table 7.

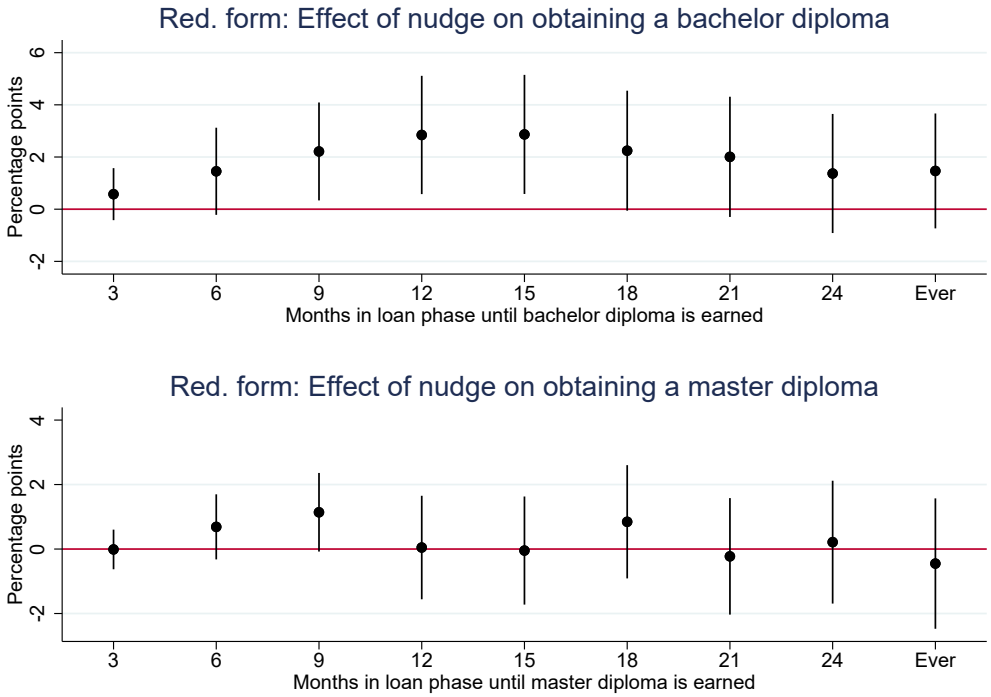
[41] Note that whether or not the student is enrolled in a bachelor (top panel) or master (bottom panel) program when entering the loan phase is added as a control variable, in order to control for the study progress at the time the student enters the loan phase. The estimated results are similar when performing the 2SLS estimation separately on subsamples of students enrolled in a bachelor or master program. Furtermore, of the students in group 1 entering the loan phase in September 2009, approximately 65% is enrolled in a bachelor program and 31% is enrolled in a master program.

[42] Note that these conclusions correspond with the findings presented in Section 6.1.3, where heterogeneity in the effect of the default loan amount change across different subgroups is estimated. The difference between the estimates presented in Section 6.1.3 and the estimates presented in Appendix C.3.3, is that Appendix C.3.3 presents the impact of the treatment in the first month, and Section 6.1.3 is concerned with the average treatment effect over the academic year (12 months).

[43] This essentially means equation 3 is estimated, with study success as the dependent variable.

[44] Appendix C.3.1 provides additional estimations (first and second stage, reduced form and OLS), using the chance a student obtains a bachelor and master program within 12 months after entering the loan phase.

Figure 10. Estimated coefficients of the reduced form, estimating the effect of the treatment (default loan amount change) on the chance a student earns a bachelor diploma within x months after entering the loan phase in %-points.



Note: This figure presents the estimated coefficients of the reduced form of 9 different OLS/DD regressions, where in the first 8 models a dummy equal to 1 if the student has earned a bachelor (top panel) or a master (bottom panel) diploma within 3, 6, 9, 15, 18, 21 or 24 months after entering the loan phase is used. The dependent variable in the last model is a dummy equal to 1 if the student ever earns a bachelor or master diploma after entering the loan phase. The vertical line represents the 95% confidence interval. All controls listed in Section 4.1 are included. Additionally, whether or not the student is enrolled in a bachelor(top panel) or master (bottom panel) program when entering the loan phase is added as a control variable.

given the mechanisms through which borrowing behavior is expected to influence study success. However, it can already be concluded on the basis of Figure 9, that these findings are not robust across all measures of study success. Therefore, we can at least conclude that the (exogenous) reduction in the requested interest-bearing loan amount, did not have a negative impact on study success of students who exceeded the nominal study duration and entered the loan phase. This finding is in contrast to the results of Marx and Turner (2017), who find that students who were induced to borrow $4,000, earned 3.7 additional credits and improved their GPAs by 0.6 points. A more detailed comparison between the results of this study and the findings by Marx and Turner (2017) is made in Section 7.

Even though this finding might be counter intuitive, there are a number of potential (suggestive) explanations for the slightly positive effect (for bachelor diploma), or the lack of a negative effect (for master diploma), of a reduction of the requested loan amount on study success. First, it is possible that the change in the default option triggered a decision-making process, where students re-evaluate their time allocation, study effort and/or the prevalence of their different

sources of income. Perhaps, students were less incentivized to deliberately re-evaluate these time allocation and/or budgetary decisions prior to the default option change, since they automatically received the maximum possible loan amount. Second, it may be that the substantial requested amount borrowed induced by the default loan amount when entering the loan phase (prior to the default loan amount change), was primarily saved and/or used for consumption instead of study-related purposes.[45] In this case, one would expect that the default loan amount change would indeed have no negative effect on study success.[46]

Robustness and limitations of the 2SLS results. First, recall from Section 4.1.3 that only the effect of the default loan amount change in the first month of the loan phase is used as the instrument or first stage. However, it can be concluded that the results are robust to selecting the amount of months included in the sample, since using the (cumulative) effect of the treatment on borrowing behavior in the first 3, 6 or 12 months gives similar result in the second stage. See Appendix C.3.4 for the 2SLS estimates using the cumulative effect of the treatment on the borrowing behavior in the first 3 months as the first stage.

Second, there are various indicators to measure study success. The indicator used in this paper is a dummy equal to 1 if the student obtained a bachelor or master diploma in the first 12 months of the loan phase. Unfortunately, data on a potentially more detailed measures of study success: study credits earned or GPA, is not available. Therefore, a complete picture with respect to study success can not be provided due to data limitations.

Lastly, it is important to note that (1) the sample only consists of students who have exceeded the nominal study duration, and (2) the estimated local average treatment effect is only informative for the population of compliers.[47] Therefore, the estimates presented in this section might not be informative about the relation between borrowing behavior and study success for other subgroups of students.

6.2 Treatment 2: Removal of the 'maximum borrowing tick box' from the application screen.

Section 6.2 presents the effects of the removal of the 'maximum borrowing tick box' from the loan application screen in March 2014 on the borrowing behavior of students. First the baseline model: the before-after comparison of cohorts is discussed. Subsequently, the results of the differences-in-differences strategy, the preferred specification are presented.

[45] In 2011/2012, 13% of the students indicated that they save their loan from DUO in order to have funds after their study is completed. Furthermore, 12% of the students report that they borrow money from DUO, because of the favorable conditions and not out of necessity Nibud (2012).

[46] In theory, this mechanisms could also explain a positive effect of a reduction in the requested loan amounts on study success, if the consumptive expenses led to a reduction in the time spend on study-related activities.

[47] This also means that it is only possible to estimate the study success of students already enrolled, students that are not enrolled are not included in the data.

Table 13. Before-after comparison of cohorts of first year students, treatment effect of removal of the tick box.

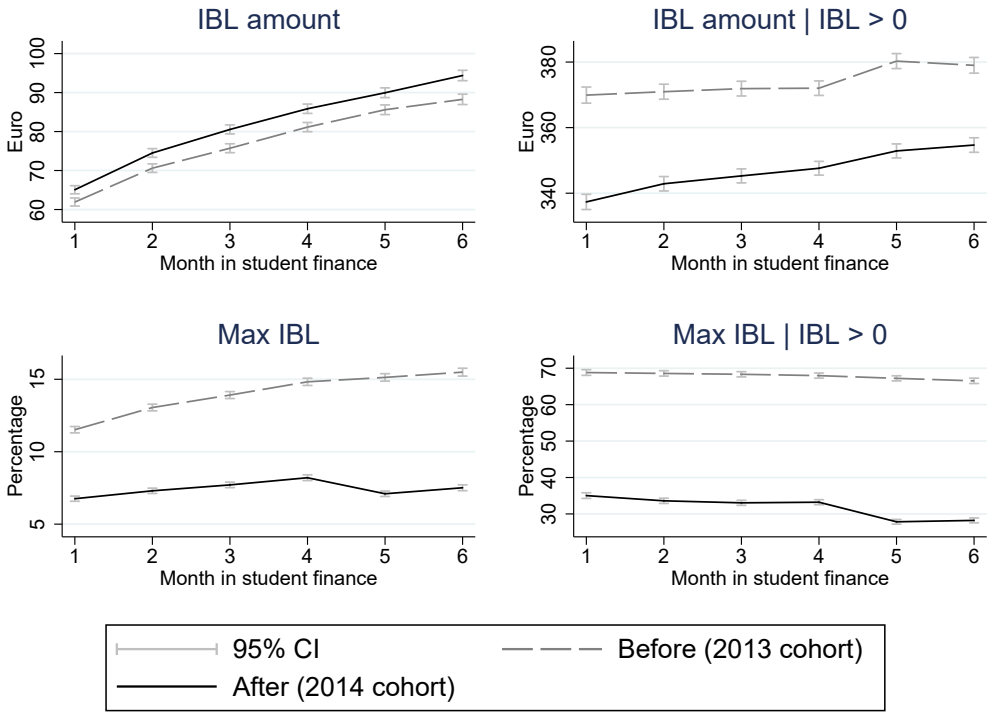
	(1) IBL > 0	(2) IBL	(3) IBL | IBL > 0	(4) Max IBL	(5) Max IBL | IBL > 0	(6) ≥ 99% Max IBL | IBL > 0
Sep	0.020***	1.208*	-32.890***	-0.051***	-0.336***	-0.245***
	(0.002)	(0.655)	(1.549)	(0.001)	(0.005)	(0.005)
Oct	0.020***	1.198*	-29.576***	-0.062***	-0.347***	-0.245***
	(0.002)	(0.687)	(1.444)	(0.001)	(0.005)	(0.005)
Nov	0.022***	2.128***	-27.938***	-0.067***	-0.352***	-0.246***
	(0.002)	(0.711)	(1.408)	(0.001)	(0.005)	(0.005)
Dec	0.021***	1.923***	-25.261***	-0.072***	-0.347***	-0.239***
	(0.002)	(0.737)	(1.380)	(0.001)	(0.005)	(0.005)
Jan	0.022***	1.171	-29.672***	-0.085***	-0.391***	-0.368***
	(0.002)	(0.767)	(1.422)	(0.001)	(0.005)	(0.005)
Feb	0.024***	2.224***	-26.348***	-0.086***	-0.383***	-0.348***
	(0.002)	(0.816)	(1.480)	(0.002)	(0.005)	(0.005)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
$\bar{Y}$	0.22	78.65	359.81	0.11	0.49	0.53
sd(Y)	0.41	164.02	148.03	0.31	0.50	0.50
Adj. R^2	0.27	0.27	0.22	0.23	0.27	0.24
N	898,982	898,982	196,509	898,982	196,509	196,509

Note: Each column represents a different OLS regression of equation 7 on a sample of first-year students who started receiving student finance as of September 2013 (control) or September 2014 (treated). Dependent variables: (1, 2) "IBL (| IBL > 0)" = Interest-bearing loan amount (conditional on IBL > 0), in € ; (3, 4, 5) "(≥ 99%) Max IBL (| IBL > 0)" = dummy equal to 1 if student borrows (≥ 99%) the maximum IBL (conditional on IBL > 0); (6) "IBL > 0" = dummy equal to 1 if IBL > 0. All controls listed in Section 4.2 are included. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

6.2.1 Results of the before-after comparison of cohorts. The results from the estimation of equation 7, the before-after comparison of cohorts of first-year students, are presented in Table 13. This table only includes the estimated treatment effects, which corresponds to γ in equation 7. Figure 11 provides a comparison of means between the control and treatment group for the most important outcome variables, visualizing the before-after comparison presented in Table 13.

The first column of Table 13 indicates that about 2 percentage points more students borrow money from DUO in the form of the interest-bearing loan in 2014 (compared to 2013). The estimates reported in column 2 indicate that first-year students borrow a very small amount more in 2014 compared to 2013 (also illustrated in the top left panel of Figure 11). However, when considering the average IBL amount conditional on having a loan (IBL > 0), we see the expected/intuitive reduction in the requested interest-bearing loan amount (column 3). On average, first-year students with an interest-bearing loan, borrow €33 less in September 2014 (after the treatment) compared to first-year students in September 2013 (also see the top right

Figure 11. Effect of the removal of the tick box: before after comparison of cohorts.



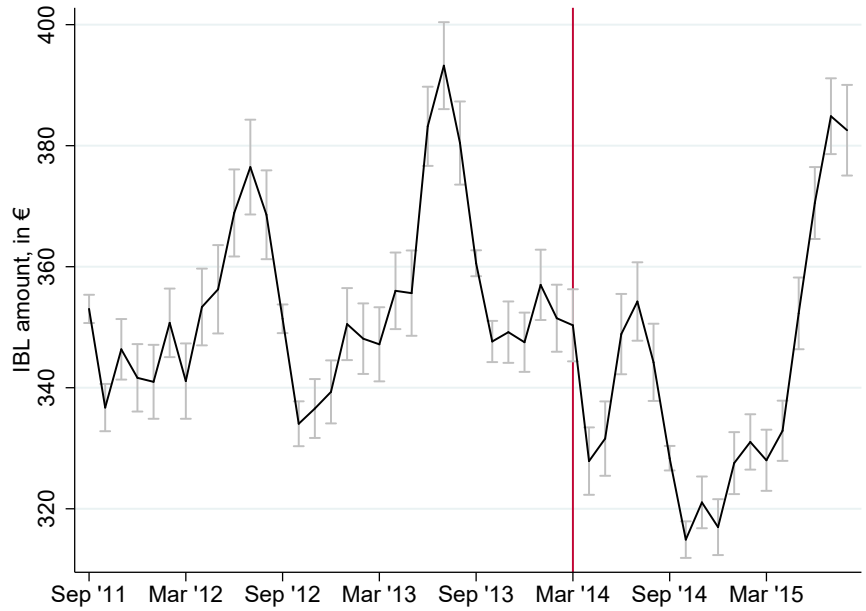
Note: The average values of the following outcome variables are plotted: interest-bearing loan amount (top left), interest-bearing loan amount conditional on IBL>0 (top right), percentage of students borrowing the maximum loan amount (bottom left), and the percentage of students borrowing the maximum loan amount conditional on IBL>0 (bottom right). The x-axis represents the month in the grant/nominal phase, which corresponds with the month in student finance. Only first-year students who started receiving student finance as of September 2013 (control) or 2014 (treated) are included in the sample. The vertical lines represent the 95% confidence interval. No control variables are included.

panel of Figure 11).

The last three columns are concerned with the proportion of students that borrow (close to) the maximum eligible interest-bearing loan amount. Column 4 indicates that on average, the chance that students borrow the maximum loan amount is reduced with 5.1 percentage points in the first month (also see the bottom left panel of Figure 11). Column 5 shows that of the first-year students that borrow, the chance these students borrow the maximum loan amount is reduced with 33.6 percentage points in September (also illustrated in the bottom right panel of Figure 11).

However, recall that after the treatment students could only manually fill in their requested loan amount. Most people request rounded amounts when manually filling in the requested loan amount, whereas the maximum loan amount usually is not rounded to to the nearest euro. To account for this, column 5 reports estimates of equation 7 with the proportion of students that borrow at least 99% of the maximum loan amount (conditional on having an interest-bearing loan) as the dependent variable. The chance that first year students borrow at least 99% of the maximum loan amount is reduced with 24.5 percentage points. Note, this is a sizable difference compared to the reduction of 33.6 percentage points reported in column 5. See Appendix D.1 for

Figure 12. Average monthly loan amount of first-time borrowers plotted over time.



Note: The figure presents the average requested interest-bearing loan amount, for first-time borrowers from September 2011 up to and including August 2015. The vertical lines represent the 95% confidence interval. The vertical red line indicates the time of the removal of the maximum borrowing tick box. No control variables are included.

additional figures comparing relevant outcome variables between cohorts of first-year students.

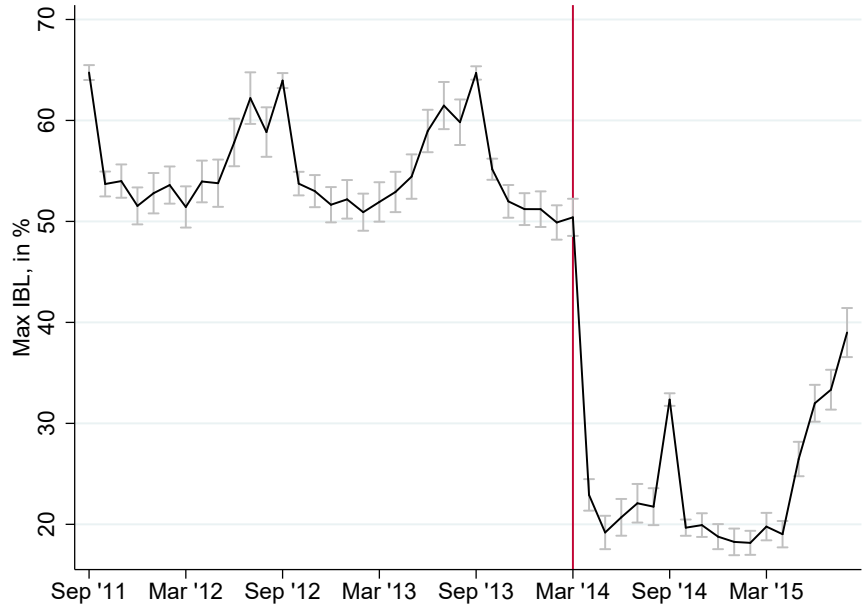
The outcome variables that are conditional on having a interest-bearing loan are the most informative, since it seems unlikely the nudge influenced the decision of students whether or not to borrow (extensive margin). However, if students decide to borrow from DUO, and visit the loan application screen on their personal web page, they are confronted with the 'maximum loan tick box'. If the treatment has an effect on the borrowing behavior of students, it is most likely to be in the intensive margin: if a student decides to borrow, the nudge might influence the amount the student borrows. Consequently, it is informative to only consider students that borrow for the first time, since it ensures that everyone in the sample has been confronted with the loan-application screen.

Figures 12, 13 and 14 presents the borrowing behavior, measured by three different indicators, of first-time borrowers in the grant phase.[48] Figure 12 visualizes the drop in the average requested interest-bearing loan amount for first-time borrowers after the removal of the tick box in March 2014. The average requested IBL drops in March 2014, which is not the case in March in other years. Additionally, Figure 13 (14) shows that the average proportion of students that borrow (at least 99% of) the maximum loan amount is greatly reduced after March 2014.[49]

[48]This implies each student is only included in the sample once: in the month the student borrows for the first time. Recall that first-time borrowers in the grant phase are excluded since DUO automatically assigns students with a certain loan amount when entering the loan phase, which means it cannot be determined if they have visited the student finance application screen.

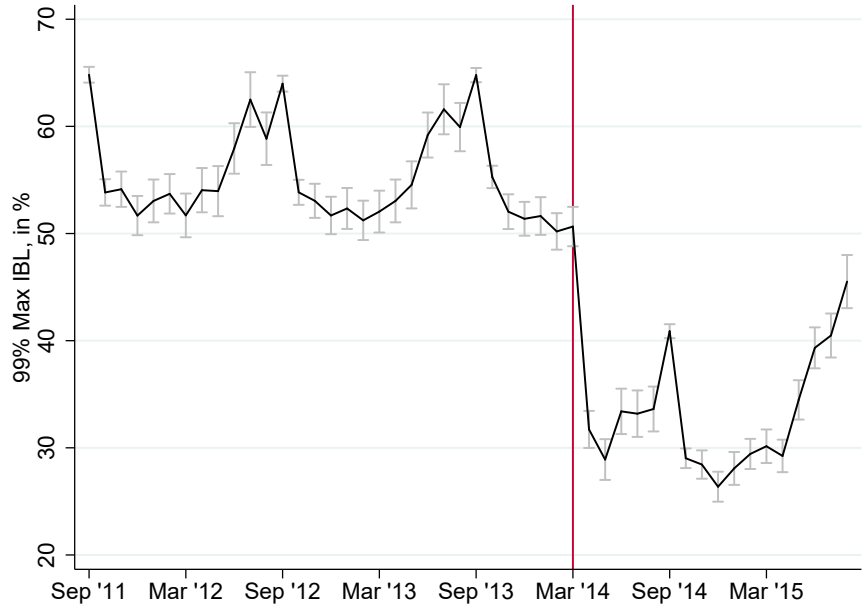
[49]Important to note is that the tick box was removed in March 2014, which means students that visit the loan application screen in March 2014 are confronted with the treatment. Since students requesting an IBL starting in

Figure 13. Dummy indicating if first-time borrowers borrow the maximum IBL amount.



Note: The figure presents the average of a dummy equal to 1 if a student borrows the maximum IBL amount, for first-time borrowers from September 2011 up to and including August 2015. The vertical lines represent the 95% confidence interval. The vertical red line indicates the time of the removal of the maximum borrowing tick box. No control variables are included.

Figure 14. Dummy indicating if first-time borrowers borrow at least 99% of the maximum IBL amount.



Note: The figure presents the average of a dummy equal to 1 if a student borrows $\geq$ 99% of the maximum IBL amount, for first-time borrowers from September 2011 up to and including August 2015.. The vertical lines represent the 95% confidence interval. The vertical red line indicates the time of the removal of the maximum borrowing tick box. No control variables are included.

Table 14. Impact of removing the tick box on the borrowing behavior, DD estimation results.

	(1) IBL	(2) Max IBL	(3) $\geq$ 99% Max IBL
Treatment	-30.502***	-0.300***	-0.214***
	(5.635)	(0.018)	(0.018)
Controls	Yes	Yes	Yes
$\bar{Y}$	345.88	0.44	0.46
sd(Y)	158.06	0.50	0.50
Adj. R^2	0.11	0.12	0.10
N	11,308	11,308	11,308

Note: Each column represents a different OLS regression of equation 8, on the sample of first-time borrowers in the grant phase in February (control group) and April (treated group). Columns 1-3 uses the sample of fist-time borrowers in 2013 and 2014. Dependent variables: "IBL = Interest-bearing loan amount, in € & "($\geq$ 99%) Max IBL" = dummy equal to 1 if student borrows ($\geq$ 99%) the maximum IBL. The included controls are listed in Section 4.2. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

Even though the results of the before-after comparison are intuitive, the estimates discussed above do not describe a causal relation between the removal of the tick box and borrowing behavior: it is impossible to separate the cohort fixed effect from the treatment effect (as discussed in Section 4.2).The next section discusses the results of the DD model, using data on first-time borrowers in the months surrounding the removal of the tick box.

6.2.2 Differences-in-differences estimation results. Table 14 presents the estimates of the coefficient of interest (δ) from different DD regressions of equation 8, including all control variables listed in Section 4.2. This DD model considers the borrowing behavior of new loan applicants closely around the default change, by comparing borrowing behavior of first-time borrowers in February (nontreated) and April (treated), in 2013 (pre-treatment) and 2014 (post-treatment).[50] The estimate in column 1 means that first-time borrowers, on average, borrow €31 (in the first month) less because of the removal of the tick box. Furthermore, the chance first-time borrowers request the maximum loan amount is on average reduced with 30 percentage points in the first month (column 2). However, the chance that students borrow at least 99% of the maximum loan amount is 'only' reduced with 21.4 percentage points on average. This suggest a significant amount of students still desire to borrow the maximum amount, but do not bother to fill in exactly the maximum possible amount (specific to the euro cent). Nevertheless, the reduction in the chance students borrow (close to) the maximum amount is sizable.

Important to note is that, analogous to the analysis of the default loan amount change, the estimates of the simple before-after comparison of cohorts results are remarkably similar to the DD estimates. Note that in the DD model only students that borrow are included in the sam-

the next month, the treatment effect is observed as of April 2014.

[50] Appendix D.2.4 presents a model utilizing data on 2 months before and after the treatment, which serves as a robustness check.

ple, therefore, only the outcome variables that are conditional on the student borrowing can be meaningfully compared. The difference in the effect on the interest-bearing loan amount (conditional on borrowing) is small: €30.50 (DD) compared to €32.89 (before-after). The effect on the chance a student borrows (at least 99% of) the maximum loan amount is also relatively similar: 30 (21.4) percentage points in the DD model, compared to 33.6 (24.5) percentage points in the before-after model. The advantage of the before-after comparison over the DD design, is that the before-after comparison of cohorts enables us to consider the impact of the nudge beyond the first month. Table 13 indicates that the effect of the nudge remains relatively stable over the first six months. Even though there are significant limitations attached to the before-after design, the fact that the estimates for the immediate (first-month) effects are remarkably similar to the DD estimates provides confidence about the economic and statistical significance of the nudge beyond the first month (as estimated by before-after comparison).

Lastly, in order to be able to present the percentage reduction of the requested loan amount, the counterfactual (would the treated group not have been treated) is estimated in the model with no controls. This counterfactual can be used to calculate the relative change in the interest-bearing loan amount. The percentage reduction in the requested loan amount for first-time borrowers (corresponding to the reduction of €31.52 reported in column 1 of Table D.1) is 8.8%.

6.2.3 Heterogeneous treatment effects. Table 15 explores if the treatment had a different impact on different subgroups, analogous to the estimation of the heterogeneous treatment effects for the default loan amount change in Section 6.1.3. In this case, only the difference in the treatment effect between students with and without a supplementary grant is statistically significant.[51] The effect of the treatment on borrowing behavior is larger (in absolute terms) for students without a supplementary grant. There could be two potential (non-mutually exclusive) explanations for the difference in the treatment effect. First, provided the amount of supplementary grant received is a proxy for social economic status (SES), it could be that students with a lower SES responded differently to the nudge compared to students with a higher SES. Second, the maximum possible loan amount in the grant phase is related to the amount of supplementary grant received (see Table 1). Students without a supplementary grant could borrow €541 in 2013, whereas students who received the maximum amount of supplementary grant (€252) could only borrow €289 (€541 - €252). Consequently, it could be the case that this difference in the maximum loan amount causes the difference in the treatment effect. However, it is not possible to quantify the relative influence of each of the two potential explanations.

[51] This is confirmed by a regression including interaction terms, testing if the treatment effect is significantly different between the subgroups. For both dependent variables holds that is the only case for which significant heterogeneous effects are found. The difference between the treatment coefficient is significant at the 95% confidence level if IBL is the dependent variable, and at the 90% confidence level if the dummy equal 1 if the student borrows the maximum IBL amount is the dependent variable.

Table 15. Heterogeneous treatment effects of the removal of the tick box.

		Gender		Study type		Mig. background		Suppl. grant	
	(1) All	(2) Female	(3) Male	(4) hbo	(5) wo	(6) No Mig.	(7) Mig.	(8) S.G.	(9) No S.G.
Y: IBL									
Treatment	-30.50***	-29.68***	-31.28***	-30.05***	-32.07***	-31.30***	-26.85***	-13.55**	-37.54***
	(5.63)	(7.93)	(8.02)	(6.87)	(9.75)	(6.67)	(9.80)	(6.51)	(7.65)
$\bar{Y}$	345.88	334.35	358.08	339.03	357.68	347.50	340.16	283.24	373.79
sd(Y)	158.06	158.44	156.75	153.59	164.81	163.33	137.69	95.93	171.72
Adj. R^2	0.11	0.10	0.11	0.11	0.10	0.09	0.22	0.01	0.04
N	11,308	5,813	5,495	7,153	4,155	8,813	2,495	3,485	7,823
Y: Max IBL									
Treatment	-0.30***	-0.29***	-0.31***	-0.29***	-0.32***	-0.30***	-0.30***	-0.25***	-0.32***
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)	(0.02)	(0.03)	(0.02)
$\bar{Y}$	0.44	0.41	0.47	0.45	0.43	0.56	0.41	0.58	0.38
sd(Y)	0.50	0.49	0.50	0.50	0.50	0.50	0.49	0.49	0.49
Adj. R^2	0.12	0.11	0.13	0.13	0.12	0.13	0.10	0.07	0.09
N	11,308	5,813	5,495	7,153	4,155	2,495	8,813	3,485	7,823

Note: The IBL amount is dependent variable in the top panel, and the dummy indicating if the student borrows the maximum IBL amount is the dependent variable in the bottom panel. Each column represents a different OLS regression of equation 8, where a dummy equal to 1 if the student is a first-time borrower is the dependent variable, on the sample students in the grant phase in February (control group) and April (treated group) in the years 2013 (pre-treatment) and 2014 (year of the treatment). Each column includes estimates for specific subgroups. Only the difference in the treatment effect between students with and without a supplementary grant is significant (5% confidence level for IBL & 10 % confidence level for Max. IBL), this is tested by estimating a regression model including interaction terms. The included controls are listed in Section 4.2. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

6.2.4 Examining the internal validity of the DD design. This section examines the internal validity of the DD estimations. First, several robustness checks of the preferred specification reported in Section 6.1.2 are discussed. Second, the validity of main identifying assumption of the DD empirical strategy: the common trend assumption, is approximated. Third, the potential existence of sample selection bias is examined.

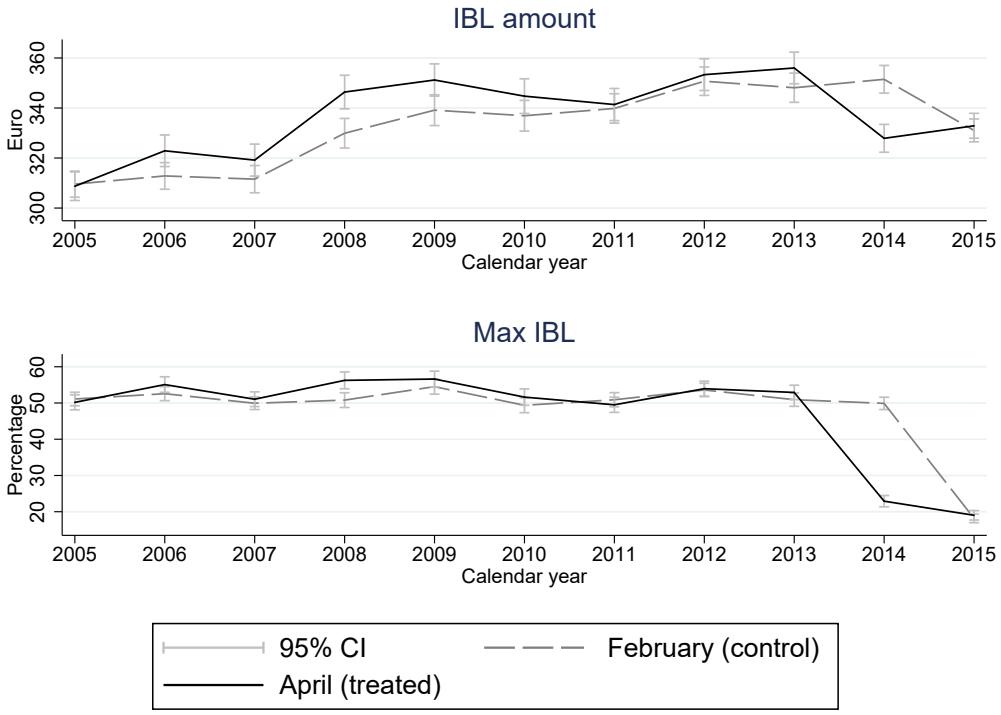
Robustness checks First, Appendix D.2.2 shows that the estimations by the preferred specification presented in Table 14 are robust to using different specifications: (1) excluding the (individual-level) control variables and (2) including multiple pre-treatment periods and the inclusion of group specific time trends. Second, Appendix D.2.3 shows the estimates are unchanged when only using the student finance data set, thereby keeping the observations that are dropped due to the merge of the different data sets used to generate the control variables (see Appendix B.1). Third, Appendix D.2.4 presents a model utilizing data on 2 months before and after the treatment. In other words, instead of only using Febraury (nontreated) and April (treated), January (nontreated) and May (treated) are also included in the sample. It is concluded the results of Table 14 are robust to using wider range of months around the time of the treatment (March). Lastly, Appendix D.2.5 confirms that the reduction in the regular loan amount due to the treatment, is not compensated by an increase in an alternative loan: the tuition fee credit (TFC).

Validity of the common trend assumption. The main identifying assumption of the DD design is the common trend assumption. The validity of the common trend assumption can be approximated at best, due to the lack of the counterfactual. Figure 15 plots average values for interest-bearing loan amounts (top panel) and the dummy indicating whether or not the student borrows the maximum IBL amount (bottom panel), for the treated group (first-time borrowers in April) and the control/nontreated group (first-time borrowers in February). Judging from visual inspection, the common trend assumption seems to be valid. For both variables the two lines are following a similar trend, with the exception of 2014: the time of the treatment.

Nevertheless, Figure 16 presents a more formal examination/approximation of the validity of the common trend assumption. Figure 16 plots the estimates from a placebo treatment regression, including all control variables, for both variables. Multiple pre-treatment periods are included, and the last pre-treatment period is the reference category (2013). The common trend assumption is likely to be valid if the other pre-treatment periods are insignificant. Note that the coefficient for 2014 presents the treatment effect, as estimated by regressing equation 8. No coefficient, apart from 2014 (the calendar year of the treatment), is significantly different from 0, which ensures it is reasonable to assume the common trend assumption is not violated.

Appendix D.2.1 includes figures for the dummy indicating whether or not the student borrows at least 99% of the maximum IBL amount, the overall picture of the validity of the common trend assumption is similar. Since it is reasonable to assume the common trend assumption is

Figure 15. Treatment 2, common trend of the IBL amount and the dummy indicating if a student borrows the maximum IBL amount.

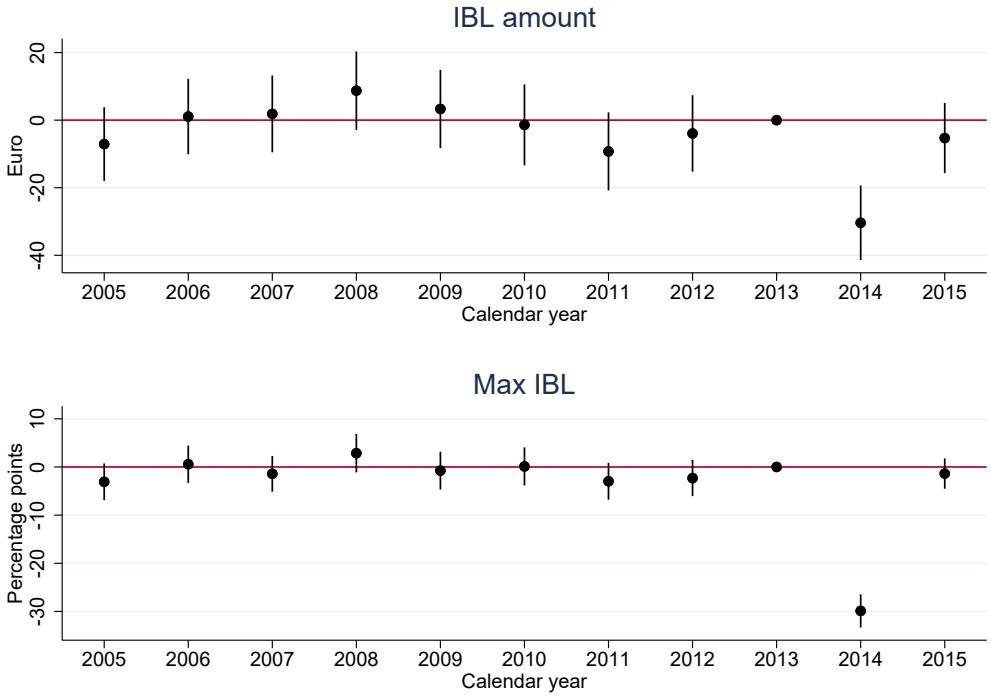


Note: The variable plotted on the y-axis is the IBL amount (top panel) and a dummy equal to 1 if the student borrows the maximum IBL amount (bottom panel). The average values for first-time borrowers in February (control) and April (treated) by (calendar) year are plotted in order to approximate the validity of the common trend assumption. The vertical lines represent the 95% confidence interval.

valid, the regression results present unbiased estimates of the effect of the removal of the tick box on the borrowing behavior of first-time borrowers.

Examining potential sample selection bias. Since only first-time borrowers are included in the sample, a potential concern related to the internal validity of this particular DD design is sample selection bias. If the treatment influenced the amount of first-time borrowers, this potentially creates sample selection bias which would confound the DD estimates. Table 16 estimates equation 8, where a dummy indicating if the student is a first-time borrower is the dependent variable, on the sample of students in the grant phase in February and April. When considering the main model, using data of 1 pre-treatment period and including all controls, it can be concluded that there are no concerns with respect to sample selection bias, since the estimate in column 2 does not significantly deviate from 0. However, when including all available pre-treatment cohorts of students in the sample, as is done in column 3, the coefficient is significantly different from 0 (at a 5% significance level). Nevertheless, using this specification, the nudge only decreased the chance a student borrows for the first time with 0.06 percentage points. It is unlikely this very small decrease has a significant influence on the DD estimates. Concluding, it seems unlikely sample selection bias will confound the DD estimates.

Figure 16. Treatment 2, placebo treatment plot to examine common trend of the IBL amount and of the dummy indicating if student borrows maximum IBL amount.



Note: The figure includes plots of the coefficients of a placebo treatment regression, where the dependent variable plotted on the y-axis is the IBL amount (top panel) and a dummy equal to 1 if the student borrows the maximum IBL amount (bottom panel). Frst-time borrowers in February (control) and April (treated) by (calendar) year, are included. All controls listed in Section 4.2 are included. The vertical lines represent the 95% confidence interval.

Table 16. Effect of the removal of the tick box on the chance a student borrows for the first time, examining potential sample selection bias.

	Dep. variable: first-time borrower in the grant phase		
	(1)	(2)	(3)
	No controls	All controls	Multiple pre-periods
Treatment	-0.00015	-0.00016	-0.00058**
	(0.00027)	(0.00027)	(0.00025)
Controls	No	Yes	Yes
Group-specific trend	No	No	Yes
$\bar{Y}$	0.01	0.01	0.01
sd(Y)	0.08	0.08	0.09
Adj. R^2	0.00	0.00	0.00
N	1,566,272	1,561,323	6,515,946

Note: The dependent variable is a dummy equal to 1 if the student is a first-time borrower. The sample consists of students in the grant phase in February (control group) and April (treated group). Columns 1 and 2 use the sample of fist-time borrowers in 2013 and 2014, where column 3 uses the sample of first-time borrowers in 2005-2014 and includes group-specific time trends. All controls listed in Section 4.2 are included. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

7 Discussion

The results discussed in Section 6 provide convincing evidence that the two nudges influenced the borrowing behavior of students. In summary, the removal of the tick box reduced the chance first-time borrowers requested the maximum loan amount with 30 percentage points, and the requested interest-bearing loan amount of first-time borrowers was reduced by just over €30 in the first month, which corresponds to a reduction of approximately 8.8%. The change in the default loan amount for students entering the loan phase had a substantial impact: on average, the requested interest-bearing loan amount was reduced with approximately €330 (72%) in the first month, and with approximately €1200 (46%) in the first 12 months of the loan phase, for students who did not borrow in the grant phase.

This section first explores which mechanisms are likely to drive the impact of both nudges on borrowing behavior. Second, the results with respect to study success are discussed, together with a comparison between the results of this paper and a closely related study by Marx and Turner (2017). Lastly, distinguishing features and limitations of this study are considered.

7.1 Which mechanisms drive the impact of the nudges on borrowing behavior?

As discussed in Section 3.2, insights from behavioral economics provide three potential mechanisms through which both nudges can influence behavior: (1) the default (and the tick box) can be regarded as an implicit recommendation by the default setter, (2) status quo bias, inertia and procrastination of decision making can cause individuals to stick to the default option, and (3) the default option and the tick box can be regarded as an anchor or reference point from which insufficient adjustment is made.[52] This section aims to link these theoretical mechanisms to the results reported in Section 6. Subsequently, some policy implications are briefly discussed.

7.1.1 Mechanisms driving the impact of the default loan amount change. With respect to the default loan amount change a clear pattern, consisting of three phases, arises: (1) the impact of the nudge on borrowing behavior is the largest in the first month, (2) reduces in size (in absolute terms) in the subsequent months and (3) eventually reaches a plateau (where impact of the treatment remains significant after 12 months). Even though the speed in which this pattern occurs differs per group, this pattern is visible for all four subgroups of students, see for instance Tables 6 and 12.

This consistent pattern is informative about the mechanisms driving the impact of the default loan amount change on borrowing behavior. The immediate effect of the nudge, in the first month, is largest in size, since all three of the mechanisms are in effect and work in the same direction. Students that wish to deviate from the default option might do so, but could

[52] Since there was a default loan amount both before and after the treatment, these mechanisms are in place both before and after the treatment. However, these mechanisms explain why the borrowing behavior of students changes when the default loan amount changes.

procrastinate this choice. However, due to inertia (and procrastination) and the fact some students can regard the default option as the status quo, an implicit recommendation, an anchor or reference point, some students will (1) not deviate from the default option or (2) will opt for a new interest-bearing loan amount which is likely to be biased towards the default loan amount. The latter mechanisms can explain why the impact of the default eventually reaches a plateau while remaining economically and statistically significant (for all groups of students).

It is not possible to quantify the impact of each of these mechanism with the data available in this study. The mechanisms work in the same directions, are not mutually exclusive and are likely to occur simultaneously. Nevertheless, it seems that procrastination behavior plays an important role, especially in the first months. This observation is in line with the hypotheses put forth in previous work (e.g. Brown and Previtero, 2014; Beshears et al., 2009; Van Rooij et al., 2008), that procrastination is a significant driver behind the impact of default options on behavior. In addition, due to the differences in the borrowing behavior in the grant phase and the variation in the default options, it is possible to provide some suggestions on when procrastination is most prevalent.

It is important to realize that procrastination only plays a role in the case a students wishes to deviate from the default option. As previously discussed, Altmann et al. (2017) provide evidence that the alignment of the preferences of the decision maker with the default option is an important determinant of compliance with the default option. To illustrate, consider students in group 3, for who the default loan amount (both before and after the treatment) is intuitively likely to be aligned with the preferences of students in this group. Recall that students in group 3 borrow the maximum possible loan amount in the grant phase and prior to (after) the treatment receive (close to) the maximum possible loan amount in the loan phase. Student in group 3 on average have a high rate of compliance with the default option (see Figure 8). Consequently, the effect of the treatment on this group is relatively stable over the first 12 months of the loan phase (see Tables 6 and 12), which implies procrastination is not very prevalent.

In contrast, consider group 1, who also received the maximum possible loan amount by default before the treatment upon entering the loan phase (before September 2009). However, students in this group did not borrow in the grant phase, which makes it unlikely they suddenly want to borrow the maximum loan amount when entering the loan phase. From Figure 3 and Figure 4, it becomes clear that a large proportion students of the 2008 cohort (pre-treatment) adjusted their requested loan amount, but did not do so immediately. Interestingly, the pattern in Figure 3 for the 2009 cohort (post-treatment) is much more stable, implying that fewer adjustments to the requested loan amounts were made. The default loan amount after the treatment intuitively is more in line with the preferences of the students in group 1, meaning that less students are expected to deviate from the default and/or adjust their requested loan amounts. Consequently, procrastination plays a smaller role for students in group 1 after the treatment compared to before the treatment.

7.1.2 Mechanisms driving the impact of the removal of the 'maximum borrowing tick box'. Now consider the removal of the maximum borrowing tick box. As discussed in Section 6.1.1, this nudge influences the intensive margin and not the extensive margin of borrowing behavior. In other words, whether or not the tick box is present on the loan application screen is not relevant for when or whether or not the student decides to take action (extensive margin), merely the choice once action is taken is influenced (the intensive margin). Consequently, in the context of the pattern described in the previous paragraph, the first two phases do not apply since procrastination behavior is not likely to be influenced by this nudge. This predicts that the impact of the nudge will be relatively stable, i.e. there are no large effects in the first months which eventually decrease in size. From Tables 13 and 14, we can indeed conclude that the effect in the first months remains relatively stable over the first 6 months; the effect size does not decrease (in absolute terms) as it does in the case of the default loan amount change.[53]

The 'maximum loan tick box' can be regarded by the student as an implicit recommendation by the designers of the loan application screen (the government), or as as a reference point or an anchor. However, these mechanisms are likely to be less strong compared to a default loan amount, since a tick box on a loan application screen is merely a suggestion on the application screen whereas the default loan amount is the amount a student borrows if no action is taken. In addition, inertia or procrastination are not likely to be a determining factor for behavior in this case. In conclusion, the finding that the removal of the 'maximum borrowing tick box' had a smaller impact on borrowing behavior, compared to the default loan amount change, can be explained by the fact that less and weaker mechanisms are are at work.

7.1.3 Policy implications. In general, one of the main lessons of this paper is that the choice architecture strongly influences student borrowing behavior, especially in the case of the default loan amount change. More specifically, as discussed above, it seems that procrastination behavior is an important mechanism driving the impact of the default loan amount change. Consequently, it might be beneficial for policy makers to address procrastination directly. Brown and Previtero (2014) provide some suggestions: forcing choices, changing the incentives around deadlines, or increasing the salience of future payoffs.

In the context of this study, the moment the student enters the loan phase (and is confronted with the default loan amount), is likely to be a point in time where such interventions are fruitful. At the least, effective communication methods need to in place in order to communicate the implications of the default option, and the implications of procrastinating or not making a decision. This is especially important, since it is impossible for the government/DUO to determine the optimal loan amount for a given individual, since this depends on a large amount of (interrelated) factors, which implies there is a significant chance the (personalized) default loan amount will not exactly match the preferences of the student. Consequently, stimulating active

[53] Though it must be noted that precisely estimating the (dynamic) medium term effects (treatment effect beyond the first month) is problematic for this nudge, due to reasons discussed in Section 4.2.

choice potentially reduces procrastination behavior and could increase the amount of students that make a deliberate (well-thought-out) decision with respect to their requested loan amount.

Furthermore, there is substantial heterogeneity in the impact of the default loan amount change across subgroups: the impact was larger for male students, higher vocational education students and students with a migration background. It is valuable to know that the abovementioned subgroups are more strongly influenced by a default option, especially when constructing policies targeting specific subgroups.

7.2 Impact on study success and a comparison with a closely related paper by Marx and Turner (2017).

Let us now compare and contrast the findings of this study with respect to the change in the default option, to arguably the most closely related study: the (working) paper by Marx and Turner (2017), who also estimate the impact of student loan nudges on borrowing behavior and study success. An important difference between the two studies is that Marx and Turner (2017) employ a field experiment at a large community college rather than the quasi-experimental design in combination with the large registry data set used in this study. The authors randomly assigned students to receive a loan offer of either zero or an amount equal to $3,500 for freshmen (students with less than 30 credits) and $4,500 for sophomores, where it is important to note a loan offer is not a true default since all students still have to opt into borrowing.

Marx and Turner (2017) find that a nonzero loan offer (either $3,500 or $4,500) increases the requested amount borrowed by $348 (32% increase relative to the control group mean). Furthermore, the authors find that offering students a nonzero loan amount increases the chance a student decides to borrow with 9 percentage points, a 39% increase relative to control group mean of 23%. This result is comparable to the results of this study with respect to group 4. Students in group 4 borrowed €0 in the grant phase and received a loan of €0 by default prior to the treatment (entrants to the loan phase before March 2010). After the default loan amount change, students in group 4 received a nonzero loan by default (entrants to the loan phase after March 2010). As discussed in Section 6.1.1, the results from the before-after comparison of cohorts suggest a nonzero loan default increased the probability students decide to borrow with 33 percentage points in the first month: from 16.5% to 49.6% (see Table 6).[54]

Interesting is the difference between the estimated impact on study success in Marx and Turner (2017) and this study. Even though the default loan amount change on average caused a substantial decrease in the requested loan amounts (the first stage), no negative effect on study success is found, which is not in line with results of Marx and Turner (2017). Even though Marx and Turner (2017) find no effect on fall or spring enrollment, students receiving a nonzero amount did earn significantly more study credits and higher GPAs. More specifically, they find that on average students earned 3.7 more credits and increased GPA by 0.6 when induced to

[54]The effect size for group 4 is even larger when employing the DD strategy, see Table 12.

borrow by the nudge, representing increases of approximately 30% relative to the control group means.

The differences between the results of this paper and the study by Marx and Turner (2017) could be due to multiple, non-mutually exclusive, factors. First, the measurement of study success is different: Marx and Turner (2017) have data on GPA and study credits earned which unfortunately is not included in the data used for this study. Second, the sample of students is different: Marx and Turner (2017) considers freshmen and sophomores, whereas this study considers students who have studied for at least 4 years and have exceeded their nominal study duration. Third, the differences in the estimates could (partially) originate from the institutional differences in the student finance, and the educational system between the United States and the Netherlands. Fourth, since both studies estimate the local average treatment effect of nudge induced borrowing on study success (or educational attainment), the estimated results are only informative for the compliant subpopuplation. It is possible that differences in the compliant subpopulations contribute to the difference between the treatment effects.[55]

Nevertheless, it remains remarkable that even though this study finds relatively large effects of the default loan amount change on borrowing behavior no negative effects on study success are found, whereas Marx and Turner (2017) do find significant effects on study success in combination with a relatively smaller impact on borrowing behavior.

7.3 Distinguishing features and limitations.

The results of this study with respect to impact of the nudges on borrowing behavior, fit into the growing body of evidence of the effect of default options in several contexts, discussed in Section 3.2. However, this study has three main distinguishing features. First, the student finance registry data is detailed and includes all students in higher education in the Netherlands who received student finance. This data is linked to a second data set including an extensive set of background characteristics. Second, this rich data set in combination with the difference in the time of the treatment allows for the use the DD estimates of the impact of the default loan amount change for group 1, as the first stage in a 2SLS analysis estimating the causal relation between borrowing behavior and educational attainment. To my knowledge, there is currently hardly any evidence on this relation, besides the recent study by Marx and Turner (2017) discussed above.

Third, the panel data set in combination with the fact students can adjust their borrowing behavior at monthly intervals, brings the opportunity to provide insights into the dynamic impact of a default option (change). Most studies on the impact of default options on behavior are concerned with situations where a individual makes a decision once. For example, this is the case for previous studies in the contexts of student loan offers (Marx and Turner, 2017), organ donation (Johnson and Goldstein, 2003; Abadie and Gay, 2006), consent to receive e-mail

[55]The characteristics of the subpopulation complying with a default loan amount could be different relative to a subpopulation complying to a loan offer.

marketing (Johnson et al., 2002). There are, to my knowledge, currently hardly any studies that consider the dynamic effects of a default option. A paper by Venema et al. (2017), who study the effectiveness of a default nudge encouraging stand-up working, is the exception and finds that the default change continued to have a significant effect on behavior 2 months after the intervention, though the effect did reduce in size compared to the immediate impact. This study provides further evidence for the persistence of default effects, arriving at a similar conclusion with respect to the dynamic effects as Venema et al. (2017). Even though the impact of the default loan amount when entering the loan phase is largest in the first month and decreases in the later months, the effect size in the subsequent months remains economically and statistically significant. These results suggest that default options are not only substantially influencing behavior when a one-time decision is made, but have a lasting impact on behavior even if individuals can adjust their initial decision facing negligible/low transaction costs.[56]

This study also has some limitations which are important to keep in mind when interpreting the results. First, this study do not reveal whether or not the default option change was optimal or beneficial (welfare improving) for the students. Even though the results do show a clear influence of the nudges on borrowing behavior, more (individual-level) information is required to be able to draw conclusions on whether or not the nudges contributed to the alignment of the actual borrowing behavior and the optimal borrowing amounts from a lifetime perspective.

Results of the 2SLS estimation of the effect of student borrowing behavior on study success suggest that at least there is no negative effect of a reduction in the requested interest-bearing loan amount on study success. Besides the fact there are several limitations related to this 2SLS estimation (limited measurement of study success and results are only informative about compliers), there are many factors influencing the optimal investment in human capital, which in turn influences the optimal student-loan amount (see Section 3.1). For example, no information on other income sources (from parents or work), time allocation, spending/consumption behavior and repayment behavior of students is included in the data. Consequently, it is unclear how the allocation of time changed for the treated students, or at what rate the change in the requested interest-bearing loan amount (from DUO) was substituted/offset by changes in other loans, contributions from parents or income from work.

Especially, more information on repayment behavior would be valuable, since students can, in theory, repay their borrowed money at any time. It could be the case students who enter the loan phase and start borrowing (more) by default, immediately repaid the loan after checking the balance of their bank account and realizing the amount they were borrowing from DUO. This might especially be relevant for students in group 1. Figure 8 shows that approximately 50% of students in group 1 before the treatment follow the default (the maximum loan amount) in the first month, whereas only 25% still follow the default after 3 months. In this case, this potentially results in an overestimation of the impact of the default change.

[56]Transaction costs are negligible in the context of this study, changing the requested interest-bearing loan amount can be done online, is free of charge and requires minimal effort and time.

8 Conclusion

This paper studies the effect of two changes in the student loan architecture (nudges) on borrowing behavior and study success, using detailed registry data including all students in higher education between 2004 and 2015 in the Netherlands. The first nudge was a change in the amount students borrow by default when entering the loan phase. A quasi-experimental experimental design, enabled by the variation in the time of the treatment, is employed to estimate the impact of this nudge on the borrowing behavior of students, by means of a differences-in-differences estimation. The default loan amount change resulted in an average reduction of the interest-bearing loan amount of approximately €330 (72%) in the first month and €1200 (46%) in the first year for students who did not borrow when they were still eligible to receive grants. In addition, there is significant variation in the treatment effect across several covariate groups. Male students, higher vocational education students and students with a migration background are more influenced by the default loan amount change, compared to all female students, university students and students without a migration background, respectively. Furthermore, the exogenous variation in the borrowing behavior created by the default loan amount change, is exploited in a 2SLS estimation of the effect of borrowing behavior on study success (measured by the time it takes before a student obtains a bachelor or master diploma after entering the loan phase). It is concluded that the substantial reduction in the requested loan amounts caused by the nudge at least did not have a negative impact on study success, which is in contrast to the results of Marx and Turner (2017).

The second nudge was the removal of the 'maximum borrowing tick box' from the loan application screen in March 2014. A differences-in-differences strategy is again employed to estimate the impact of this nudge on student borrowing behavior. The removal of the tick box on average reduced the chance first-time borrowers requested the maximum loan amount with 30 percentage points. The requested loan amount in the first month was on average reduced by 8.8%.

The main lesson that can be drawn from the results of this study, with respect to both nudges, is that the design of the choice architecture has a substantial impact on economic outcomes through influencing behavioral biases of students. With respect to the default option change, this study confirms that default options are strong tools that have a non-neutral impact on behavior, not only with respect to one-time decisions, but also in a context where individuals can adjust their decision at frequent intervals facing negligible transaction cost. Consequently, the main policy implication of this study is that it is important to consider the consequences of the design of the choice architecture, regardless of the policy objective. Furthermore, the default-setter has to be aware of the non-neutral impact of default options on decision-making. More specifically, the results suggest that procrastination seems to be a significant driver of the impact of the default option change on decision-making/behavior, which is in line with previous studies (Beshears et al., 2009; Van Rooij et al., 2008; Brown and Previtero, 2014). Consequently,

this suggests that addressing procrastination directly and stimulating active choice, for example, by: forcing choices, changing the incentives around deadlines, or increasing the salience of future payoffs (Brown and Previtero, 2014), could lead to more deliberate and well-thought-out borrowing decisions.

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Appendix A Detailed numerical description of the change in the default loan amount

The tables in this Appendix describe in detail how the treatments from Table 3 in Section 2.2.1 originate.

- Table A.1 presents the numerical impact of the treatment on students who live with their parents and receive the maximum amount of supplementary grant.
- Table A.2 presents the numerical impact of the treatment on students who do not live with their parents and receive the maximum amount of supplementary grant.
- Table A.3 presents the numerical impact of the treatment on students who live with their parents and receive no supplementary grant.
- Table A.4 presents the numerical impact of the treatment on students who do not live with their parents and receive no supplementary grant.

Note that as described in Section 2.2.1, it is not possible to be in group 1 and receive a supplementary grant (SG), by construction. The small differences in the tables between the amounts of basic grant (BG) between group 1 and groups 2-4 is caused by the fact group 1 was treated 6 months before groups 2-4. Furthermore, here only the cases for no or maximum amount of supplementary grant are tabulated, in reality the student can receive any amount from €0 up to the maximum amount of supplementary grant (see Table 1), the tables in this section therefore provide upper and lower bounds of the treatment. The requested loan amount for students in group 2 in the grant phase is set at €50, but the amount the student in group 2 borrows does not matter for the size of treatment.

Table A.1. Impact of the treatment for students who receive the maximum supplementary grant and live with their parents.

	Before the treatment				After the treatment				Treatment
	BG	SG	IBL	Tot.	BG	SG	IBL	Tot.	
Group 1									
Grant phase	92	-		92	93	-		93	
Loan phase	-	-	819	819	-	-	93	93	
Difference				727				0	**-727**
Group 2									
Grant phase	93	214	50	357	96	221	50	367	
Loan phase	-	-	50	50	-	-	367	367	
Difference				-307				0	**307**
Group 3									
Grant phase	93	214	282	589	96	221	288	604	
Loan phase	-	-	832	832	-	-	604	604	
Difference				243				0	**-243**
Group 4									
Grant phase	93	214	0	307	96	221	0	317	
Loan phase	-	-	0	0	-	-	317	317	
Difference				-307				0	**307**

Note: Difference refers to the difference between the total amount (Tot.) received in the grant and loan phase. BG = basic grant, SG = supplementary grant, "-" = Not applicable. The amounts are in euro and rounded to integers.

Table A.2. Impact of the treatment for students who receive the maximum supplementary grant and do not live with their parents.

	Before the treatment				After the treatment				Treatment
	BG	SG	IBL	Tot.	BG	SG	IBL	Tot.	
Group 1									
Grant phase	256	-		256	260	-		260	
Loan phase	-	-	819	819	-	-	260	260	
Difference				564				0	**-564**
Group 2									
Grant phase	260	233	50	543	266	241	50	557	
Loan phase	-	-	50	50	-	-	557	557	
Difference				-493				0	**493**
Group 3									
Grant phase	260	233	282	775	266	241	288	795	
Loan phase	-	-	832	832	-	-	795	795	
Difference				57				0	**-57**
Group 4									
Grant phase	260	233	0	493	266	241	0	507	
Loan phase	-	-	0	0	-	-	507	507	
Difference				-493				0	**493**

Note: Difference refers to the difference between the total amount (Tot.) received in the grant and loan phase. BG = basic grant, SG = supplementary grant, "-" = Not applicable. The amounts are in euro and rounded to integers.

Table A.3. Impact of the treatment for students who do not receive the supplementary grant and live with their parents.

	Before the treatment				After the treatment				Treatment
	BG	SG	IBL	Tot.	BG	SG	IBL	Tot.	
Group 1									
Grant phase	92	-		92	93	-		93	
Loan phase	-	-	819	819	-	-	93	93	
Difference				727				0	**-727**
Group 2									
Grant phase	93	0	50	143	96	0	50	146	
Loan phase	-	-	50	50	-	-	146	146	
Difference				-93				0	**93**
Group 3									
Grant phase	93	0	496	589	96	0	509	604	
Loan phase	-	-	832	832	-	-	604	604	
Difference				243				0	**-243**
Group 4									
Grant phase	93	0	0	93	96	0	0	96	
Loan phase	-	-	0	0	-	-	96	96	
Difference				-93				0	**93**

Note: Difference refers to the difference between the total amount (Tot.) received in the grant and loan phase. BG = basic grant, SG = supplementary grant, "-" = Not applicable. The amounts are in euro and rounded to integers

Table A.4. Impact of the treatment for students who do not receive the supplementary grant and do not live with their parents.

	Before the treatment				After the treatment				Treatment
	BG	SG	IBL	Tot.	BG	SG	IBL	Tot.	
Group 1									
Grant phase	256	-		256	260	-		260	
Loan phase	-	-	819	819	-	-	260	260	
Difference				564				0	**-564**
Group 2									
Grant phase	260	0	50	310	266	0	50	316	
Loan phase	-	-	50	50	-	-	316	316	
Difference				-260				0	**260**
Group 3									
Grant phase	260	0	516	775	266	0	528	795	
Loan phase	-	-	832	832	-	-	795	795	
Difference				57				0	**-57**
Group 4									
Grant phase	260	0	0	260	266	0	0	266	
Loan phase	-	-	0	0	-	-	266	266	
Difference				-260				0	**260**

Note: Difference refers to the difference between the total amount (Tot.) received in the grant and loan phase. BG = basic grant, SG = supplementary grant, "-" = Not applicable. The amounts are in euro and rounded to integers.

Appendix B Merging the data sets, list of variables, missing values and outliers

Appendix B.1 Merging the three data sets.

The data sets used in this study are two data sets from DUO (student finance data and 1cHO) and the status score data set from the SCP. The student finance data is a monthly data set which spans from September 2004 to August 2015 and includes all students in the Netherlands that received student finance within the time span. The 1cHO data set is also collected by DUO and includes yearly data on all registrations, background characteristics and results from all students enrolled in higher education. This data set includes cohorts 2004 up to and including 2014. The third data set used is collected by The Netherlands Institute for Social Research (SCP) and contains data on the social status of districts (SCP, 2017). Data on the social status of districts is available for the years 1998, 2002, 2006, 2010 2014 and 2016. See Table B.2 for a list of the relevant variables included in these different data sets.

In order to be able to merge these three data sets, the 1cHO and SCP data set first has to be converted into monthly data. With respect to the 1cHO data, yearly observations are simply multiplied by 12 for a given individual (in order to create monthly records). The SCP data is linearly interpolated to created yearly observations, and subsequently multiplied by 12 to create monthly observations. The student finance data is merged to the 1cHO data by means of the (anonymised) personal number. Only observations which were both in the student finance data set and in the 1cHO are retained. Table B.1 presents the percentage of the total amount of observations that was perfectly matched (66.44%). Only 3.77% of the (monthly) loan data is omitted as a result of the the merge. Less important: 29.80% of the 1cHO data was not merged correctly, which is mainly due to the fact this is yearly data (multiplied by 12) as opposed to the monthly student finance data. This means that students that, for example, stop studying in October (and thus stops receiving student finance), will still have observations for the months November-August for that academic year/cohort in the 1cHO data. Moreover, Appendix C.2.3 and Appendix D.2.3 show that only using the student finance data gives similar results in the regression analysis compared to using the merged data set with no controls (from 1cHO) included in the regression model. Subsequently, the SCP statusscore data can be matched using data on the postal code of the student (included in 1cHO).

Table B.1. Merging student finance data & 1cHO

	Frequency	Percent
Student finance data only	3,173,947	3.77%
1cHO only	25,104,984	29.80%
Matched	55,978,032	66.44%
Total	84,256,963	100.00%

Table B.2. Variables used from all data sets.

Student finance data (DUO)	1cHO (DUO)	SCP status score
Anonymised personal number	Anonymised personal number	Postal code (district)
Assigned basic grant	Mode of study (full/part time)	Status score of a district
Assigned supplementary grant	First-year student at current institution	
Assigned interest-bearing loan	First-year student in higher education	
Tuition fee credit received	Type of higher education	
Living situation	Study program (CROHO)	
Type of student travel product	Years in higher education	
Requested basic grant	Year of diploma	
Requested interest-bearing loan	Month of diploma	
Type of education	Type of diploma	
	Average grade of highest completed education prior to higher education	
	Gender	
	Age	
	Ethnicity	
	Nationality	
	Migration background	
	Highest completed prior education prior to higher education	
	Highest completed prior education within higher education	
	Highest completed prior education	
	Diploma year of prior education	
	Start month	
	End month	

Appendix B.2 Variables & missing values.

Table B.2 contains the variables used from the (1) monthly registry student finance data set by DUO (monthly data from September 2004 to August 2015), (2) yearly 1cHO data set collected by DUO (cohort 2004 to cohort 2014) and (3) SCP statusscore data set containing the years 1998, 2002, 2006, 2010 2014 and 2016.

With respect to the student finance data, it is important to note the difference between the assigned and requested amounts. It is possible students request a higher amount than they are allowed to borrow (the assigned amount). These requested amounts are adjusted by DUO, to match the assigned amount. Additionally, the supplementary grant received is included as a covariate in the regression models, as a proxy for social economic status of the student. A categorical variable is constructed, 4 four categories: $\leq 25\%$, $\leq 50\%$, $\leq 75\%$ and $\leq 100\%$ of the maximum amount of supplementary grant. The maximum amount of supplementary grant is in turn dependent on the year, month, and the living situation of the student, as explained

in Section 2.1. A minor limitation of this data set is the fact that no data on the interest rates students pay over their interest-bearing loan is included. The main reason it is not included is that it is complicated, and error prone, to deduct the interest rate the student will pay on the basis of this registry data. This is mainly due to the fact it depends on when the student stops receiving student finance. The interest rate is revised each calendar year, if the student is still receiving student finance, and every 5 years afterwards. This is only a minor limitation, since there will only be a small amount of variation in the interest rate between students in the main regression models. Furthermore, the DD models do not require control variables in order to estimate unbiased coefficients.

With respect to the 1cHO data set, first consider the mode of study variable. This variable is used to only include full-time students. Second, the two variables on the prior education are used to generate one variable which contains the highest prior education of the student. This variable includes 6 categories: < havo, havo, mbo, vwo, higher education and other. havo and vwo are levels of secondary education, which grant access to higher vocational education (hbo) and university (wo) respectively. mbo is intermediate vocational education, which grants access to higher vocational education. Third, the variable "Average grade of highest completed education prior to higher education" has a significant number of missing observations (30.25%).[57] Additionally, there are no grades available for students of the intermediate vocational education (mbo) which means the missing values are nonrandom. However, since this variable can be used as a proxy for ability (as a covariate in the regression analyses), it is theoretically important to include in the regression model. A mean substitution method is chosen, which implies the missing values are replaced by the sample mean. Additionally a dummy variable is included, which is equal to 1 when the missing value is replaced with the sample mean. Fourth, the migration background dummy is equal to 1 is a first- or second-generation immigrant.

Lastly, concerning the SCP statusscore data set, the social status of a district is determined by the level of education, income and the position on the labor market of the people that live in that district. Data on the social status of districts is available for the years 1998, 2002, 2006, 2010, 2014 and 2016. The average score in the Netherlands, over all years, is 0. The statusscore variable ranges between -8.19 and 2.93 for the included years in this study (2004-2014), where a higher statusscore refers to a district with a better social status. It serves as an additional proxy for social economic status, on top of the amount of supplementary grant received. The amount of supplementary grant received is a more precise proxy, since it is directly determined by the income of the parents (among some other criteria) and because the social status of a district is more revealing for students who live with their parents compared to students who moved out. However, it is included in the regression models for the loan phase nudge, since students in the loan phase do not receive a supplementary grant.[58] Important to note that this is the second (and

[57]Together with statusscore, this is the only relevant variable with a significant amount unintentional missing values. The only other variables relevant variables with missing values are: living situation (0.13%), highest completed prior education (0,32%).

[58]It must be noted that a variable is constructed for students in the loan phase, which equals the amount of

final) variable that includes a significant amount of missing values (10.27% for students in the loan phase during the period of analysis). The same procedure as described above is applied (mean substitution). Lastly, note that the addition of individual level control variables is not essential in order to estimate a causal relation in a differences-in-differences regression.[59]

Appendix B.3 Dealing with outliers

A small number of observations in the student finance data set have strange values, which should not be possible according to the maximum possible amounts students are eligible to receive. This means observations are flagged as an outlier when:

1. The received basic grant is higher than €300.
2. The supplementary grant received is higher than €280.
3. The received interest-bearing loan amount is higher than €1000.
4. The received tuition fee credit is higher than €200.

Using this method, 0.48% of the observations are flagged as an outlier. However, all observations for a given individual are excluded when at least 1 outlier is present, this is done in order to not frustrate the construction of other variables which rely on the fact no months are excluded for an individual. This means that 1.41% of the observations are excluded.

supplementary grant received in the last month of the grant phase, which also serves as a proxy for social economic status.

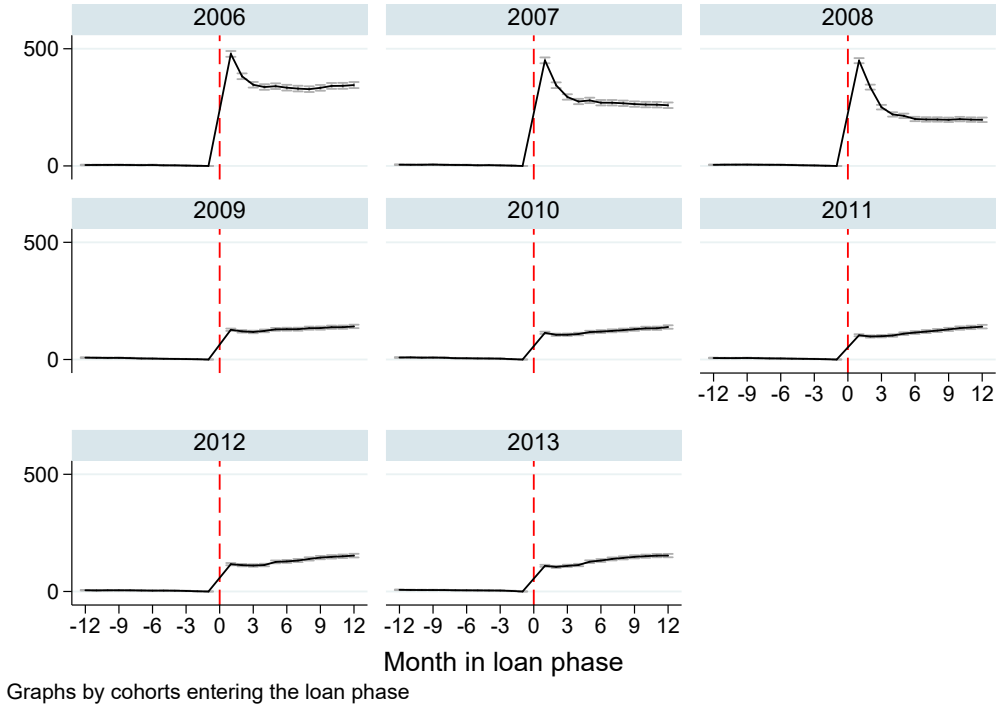
[59] This statement is confirmed by the fact the estimated coefficients reported in Section 6 are not influenced significantly when including all the controls listed in the regressions.

Appendix C Treatment 1: Robustness of the results

Appendix C.1 Additional figures of the before-after comparison.

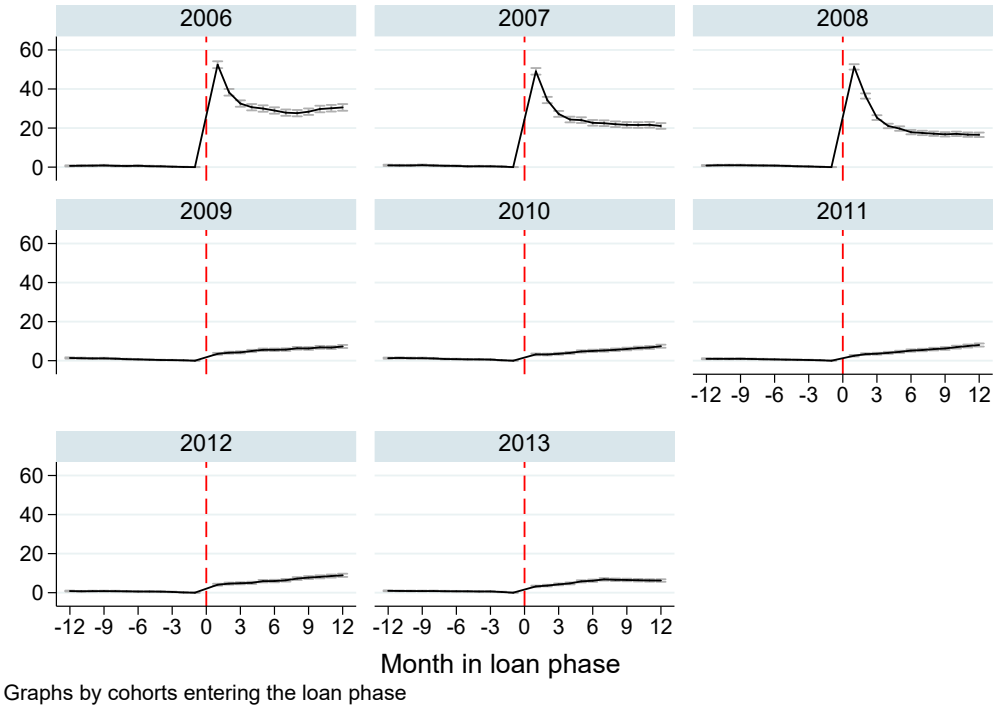
Figures C.1 and C.2 present the means of the IBL and the dummy indicating if the student borrows the maximum IBL, respectively, for all included cohorts (available in the data set) of students entering the loan phase in September. Note that the negative values on the y-axis represent the last 12 months in the grant phase.

Figure C.1. Interest-bearing loan amounts by cohorts entering the loan phase, group 1, in euro.



Note: Average requested interest-bearing loan amounts are on the y-axes. Only students entering the loan phase in September are included. The vertical lines represent the 95% confidence interval.

Figure C.2. Average percentage of students that borrow the maximum loan amount, by cohorts entering the loan phase, group 1, in percentages.



Note: Average requested interest-bearing loan amounts are on the y-axes Only students entering the loan phase in September are included. The vertical lines represent the 95% confidence interval.

Appendix C.2 Additional figures and robustness of the differences-in-differences analysis.

Appendix C.2.1 Examining the main identifying assumptions and potential internal validity concerns of the main DD analysis. Table C.1 present the sizes of the groups, defining the groups based on student finance data of the student three months prior to entering the loan phase. Recall that DUO determines the default loan amount in the loan phase based on student finance data of the last month in the grant phase. However, as discussed in Section 4.1, Table C.1 is constructed in order to ensure the sizes/composition of the control and treatment group did not change as a result of the treatment.

As discussed in Section 4.1, group 4 without supplementary grant is the preferred control group in the DD analysis due to the fact their borrowing behavior is relatively similar. Even though the DD strategy does not require the two groups to be similar with respect to relevant background characteristics, it is insightful to explore and compare the background characteristics of the different groups of students in the loan phase. Table 4 in the main text presents a comparison of background characteristics between students in group 1 and students in group 4 (excluding the students who received a supplementary grant in the grant phase). Table C.2 in this section/Appendix presents a comparison of group 1 versus all other groups (2, 3 and 4). From comparing the between Table 4 in the main text and Table C.2 it can indeed be concluded that students in group 4 (excluding students who received a supplementary grant) are more sim-

Table C.1. Sizes of the different groups (determined 3 months prior to entering the loan phase), for cohorts of students entering the loan phase in 2008-2010.

	Group 1	Group 2	Group 3	Group 4	Total
2008	5,084	3,144	8,258	7,922	24,408
	(20.8)	(12.9)	(33.8)	(32.5)	(100.0)
2009	5,580	3,202	8,301	8,794	25,877
	(21.6)	(12.4)	(32.1)	(34.0)	(100.0)
2010	5,770	3,134	7,729	8,947	25,580
	(22.6)	(12.3)	(30.2)	(35.0)	(100.0)
Total	16,434	9,480	24,288	25,663	75,865
	(21.7)	(12.5)	(32.0)	(33.8)	(100.0)

Row Percentages in Parentheses

Note: Sample consist of students who enter the loan phase in September of the year listed on the left side of the table. Only September (the first month of the loan phase) is included, so that each student is included only once in the sample.

ilar to students in group 1, with respect to relevant background characteristics, than the students in all other groups.

However, the main identifying assumption of the DD strategy is the common trend assumption. Even though the most important figures with respect to the common trend assumption are included in the main text, this Appendix provides a number of additional figures. The figures in are using the sample of students in groups 1 and 4 (excluding students receiving a supplementary grant), who enter the loan phase in September. Data for the first 12 months in the loan phase is included, which implies the estimates here present the average monthly treatment effect over the first year. The aim of the figures in this section is to examine the common trend assumption of the DD estimates resulting from equation 2.

- Figure C.3 tests the common trend assumption by plotting the average values for the IBL amount and a 'loan dummy' (dummy equal to 1 if the student has an interest-bearing loan). From visual inspection can be concluded the common trend assumption seems to be valid. Nevertheless, the following figure presents a more precise examination of the common trend assumption.

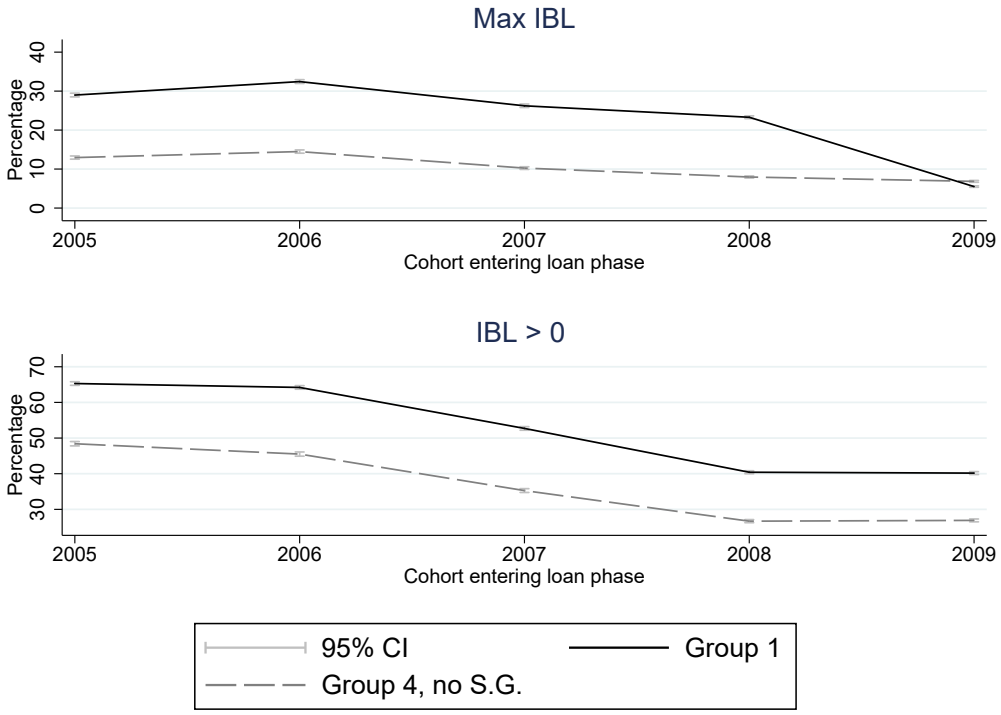
- Figure C.4 presents the estimates from a placebo treatment regression, including all control variables, using the 'maximum IBL dummy' and the 'loan dummy' as the dependent variable. The last pre-treatment period is taken as the reference category. Ideally all coefficient of the pre-treatment periods would be on the horizontal line, meaning that there was no treatment effect in the pre-treatment periods (implying the common trend assumption is likely to be valid). Note, the treatment effect of the 'loan dummy' is insignificant when considering the average monthly effect over the first 12 months in the loan phase, consequently, the common trend pictures are also showing insignificant results.

Table C.2. Comparison of background characteristics of group 1 versus group 2-4.

	Group 1 average	Groups 2-4 average	Estimated difference
Study program	5.950	5.926	0.0241
			(0.0382)
Study type	0.672	0.661	0.0109
			(0.00870)
Prior education	5.750	5.692	0.0579***
			(0.0137)
Grade prior education	68.06	67.50	0.556***
			(0.0921)
Age	22.64	22.93	-0.293***
			(0.0191)
Male	0.516	0.499	0.0164
			(0.00921)
Living with parents	0.342	0.222	0.120***
			(0.00791)
Migration background	0.118	0.191	-0.0730***
			(0.00697)
Social status of district	-0.208	-0.432	0.224***
			(0.0212)

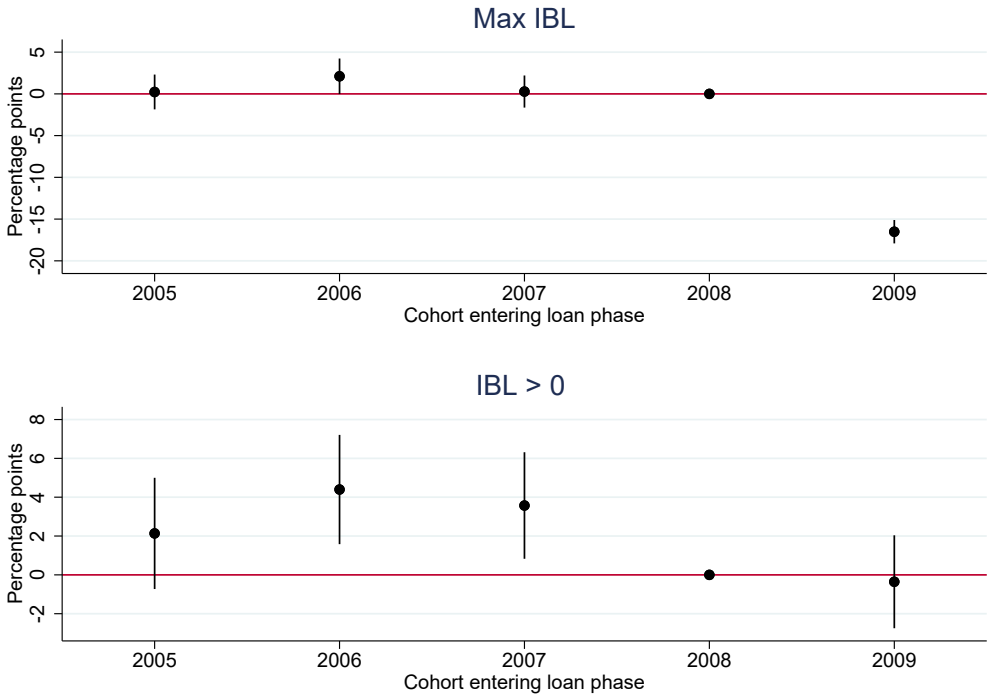
Note: Sample consists of all groups of students, who enter the loan phase in September 2008 (pre-treatment cohort). Only 1 month per student is included. The total amount of students in this sample are 24,408 (4,955 in group 1 and 19,453 in groups 2-4). Columns 1 and 2 give the means of relevant background characteristics, column 3 provides the estimated difference between the means of group 1 and groups 2-4. Standard errors reported in the parentheses, */**/*** denote significance at a 10/5/1 percent confidence level. Study program refers is a categorical variable of the 9 study directions (CROHO). Study type is a dummy equal to 1 if the student is in university (wo) and 0 if the student is in higher vocational education (hbo). Prior education is a categorical variable with 6 categories. Male, living with parents and migration background are dummies equal to 1 (0) for (fe)males, students (not) living with their parents and students with(out) a migration background. See Appendix B.2 for a detailed description of all variables.

Figure C.3. Treatment 1, common trend of dummies indicating if the student borrows the maximum IBL amount (top), and if IBL amount > 0 (bottom).



Note: The variables plotted on the y-axis is a dummy equal to 1 if the student borrows the maximum interest-bearing loan amount (top panel) and a dummy equal to 1 if the student has an interest-bearing loan (bottom panel). The average values by group and cohort (of entering the loan phase) are plotted in order to approximate the validity of the common trend assumption. The vertical lines represent the 95% confidence interval. The vertical lines represent the 95% confidence interval.

Figure C.4. Treatment 1, placebo treatment plot to examine the common trend of the maximum IBL amount dummy (top) and of the loan dummy (bottom).



Note: The dependent variables are a dummy equal to 1 if the student borrows the maximum interest-bearing loan amount (top panel) and a dummy equal to 1 if the student has an interest-bearing loan (bottom panel). Sample consist of students in group 1 (treatment group) and group 4 (excluding the students who received a supplementary grant in the grant phase), who enter the loan phase in September in different years. The vertical lines represent the 95% confidence interval. All controls are included in this placebo treatment regression.

Appendix C.2.2 Robustness check: DD analysis with no controls and including multiple pre-treatment cohorts. Table C.3 presents two robustness checks of the specification reported in Table 7. Columns 1, 2 and 3 report the estimated coefficient of interest of equation 3, excluding control variables. Note that control variables at the individual level are not required in a DD regression in order to get unbiased estimations, but they can increase the precision of the estimates (Angrist and Pischke, 2008). Columns 4, 5 and 6 extends the sample by including all pre-treatment periods available in the data set (2006-2009), this model includes year fixed effects and a group (month) specific time trend, as suggested by Angrist and Pischke (2008). This only requires a small adjustment to equation 3: (1) the T_c is replaced by year fixed effects (which is simply a more general specification), and (2) a group specific time trend is added. The addition of a group specific time trend assures that the treatment effect is not due to the fact the interest-bearing loan amount was decreasing in group 1 regardless of the treatment.

The estimates reported in Table C.3 are very close to the reported estimates of the preferred model (Table 7). This means the preferred specification discussed in the main text is robust to (1) excluding the control variables and to (2) including multiple pre-treatment periods and the inclusion of group specific time trends.

Table C.3. Robustness check of the DD estimates of the change in the standard loan amount in the loan phase, two different specifications.

	No controls			Multiple pre-periods		
	(1) IBL	(2) Max IBL	(3) IBL > 0	(4) IBL	(5) Max IBL	(6) IBL > 0
Sep	-332.16***	-0.48***	-0.15***	-299.31***	-0.44***	-0.11***
	(7.15)	(0.01)	(0.01)	(9.61)	(0.01)	(0.02)
Oct	-219.35***	-0.32***	-0.09***	-186.28***	-0.28***	-0.04***
	(7.50)	(0.01)	(0.01)	(9.74)	(0.01)	(0.02)
Nov	-134.02***	-0.21***	-0.02	-121.45***	-0.19***	0.00
	(7.59)	(0.01)	(0.01)	(9.83)	(0.01)	(0.02)
Dec	-95.97***	-0.15***	0.00	-88.49***	-0.14***	0.02
	(7.74)	(0.01)	(0.01)	(9.91)	(0.01)	(0.02)
Jan	-85.68***	-0.14***	0.02	-78.09***	-0.12***	0.03*
	(8.01)	(0.01)	(0.01)	(10.06)	(0.01)	(0.02)
Feb	-64.72***	-0.11***	0.03**	-63.98***	-0.10***	0.04**
	(8.32)	(0.01)	(0.01)	(10.21)	(0.01)	(0.02)
Mar	-60.56***	-0.10***	0.03**	-56.51***	-0.09***	0.04**
	(8.46)	(0.01)	(0.01)	(10.29)	(0.01)	(0.02)
Apr	-56.92***	-0.09***	0.03**	-49.60***	-0.08***	0.05***
	(8.66)	(0.01)	(0.01)	(10.39)	(0.01)	(0.02)
May	-51.82***	-0.09***	0.03**	-49.81***	-0.08***	0.05***
	(8.82)	(0.01)	(0.02)	(10.46)	(0.01)	(0.02)
Jun	-49.12***	-0.08***	0.03*	-44.13***	-0.07***	0.05**
	(8.97)	(0.01)	(0.02)	(10.53)	(0.01)	(0.02)
Jul	-47.69***	-0.08***	0.03*	-44.72***	-0.08***	0.05***
	(9.12)	(0.01)	(0.02)	(10.62)	(0.01)	(0.02)
Aug	-40.96***	-0.07***	0.04**	-37.48***	-0.07***	0.05***
	(9.24)	(0.01)	(0.02)	(10.69)	(0.01)	(0.02)
Controls	No	No	No	Yes	Yes	Yes
Group-specific trends	No	No	No	Yes	Yes	Yes
$\bar{Y}$	157.47	0.11	0.34	199.61	0.15	0.41
sd(Y)	275.48	0.31	0.47	301.15	0.36	0.49
Adj. R^2	0.06	0.09	0.03	0.13	0.11	0.11
N	202,200	202,200	202,200	334,166	334,166	334,166

Note: The table reports estimates from a DD regression (of equation 3) of the effect of the treatment (for group 1) on three different outcome variables, described under Table 6. These estimates serve as robustness checks for the DD estimation results presented in 7. Students in group 4 who did not receive supplementary grant (in the grant phase) are used as the control group. Columns 1-3 use the sample of the 2008 & 2009 cohorts, where columns 4-6 uses the sample of students entering the loan phase in 2006-2009 and includes group-specific time trends, as suggested by Angrist and Pischke (2008). The included controls are listed in Section 4.1. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

Appendix C.2.3 Robustness check: Only using the student finance data set. Table C.4 compares estimates from the preferred specification (only excluding controls) of the merged data sets (columns 4-6) with the estimates only using the student finance data (columns 1-3). This

robustness check ensures that the part of the student finance data lost by merging the different data sets does not significantly impact the DD estimations of the treatment effect. From comparing the columns 1-3 with columns 4-6, in both tables, it can be concluded the portion of student finance data lost by merging the different data sets has no sizable impact on the treatment effect.

Table C.4. Impact of changing the default loan amount upon entering the loan phase on borrowing behavior, DD estimation results, only using student finance data set.

	Only student finance data			Merged data sets		
	(1) IBL	(2) Max IBL	(3) IBL > 0	(4) IBL	(5) Max IBL	(6) IBL > 0
Treatment	-111.054***	-0.170***	-0.008	-108.033***	-0.167***	-0.004
	(6.693)	(0.007)	(0.012)	(6.838)	(0.007)	(0.013)
Controls	No	No	No	No	No	No
Group-specific trend	No	No	No	No	No	No
$\bar{Y}$	157.17	0.11	0.34	157.47	0.11	0.34
sd(Y)	276.08	0.31	0.47	275.48	0.31	0.47
Adj. R^2	0.04	0.06	0.02	0.04	0.06	0.02
N	211,443	211,443	211,443	202,200	202,200	202,200

Note: Each column represents a different OLS regression of equation 2. Students in group 4 who did not receive supplementary grant (in the grant phase) are used as the control group. Data on the first 12 months in the loan phase of students entering the loan phase in September of 2008 or 2009 is used. Dependent variables: "IBL = Interest-bearing loan amount, in €; "Max IBL" = dummy equal to 1 if student borrows the maximum IBL; "IBL > 0" = dummy equal to 1 if IBL > 0. Only the student finance data set is used in columns 1-3, this means no control variables are included but some data lost by merging the different data sets is retained in this estimation. Columns 4-6 presents estimates using the merged data set, excluding control variables in order to be able to compare the estimated coefficients. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

Appendix C.2.4 Robustness: Results and examining the common trend assumption of the DD estimation with the alternative control groups.

Results. In the main DD analysis, group 1 is the treatment group and group 4 without a supplementary grant is the control group. Table C.5 reports the estimates from equation 2, exploring if using a different group of students as the control group has an impact on the estimated treatment effect. Note that the treatment effect, in this case, is the average monthly effect over the first 12 months of the loan phase. Columns 1 and 2 present the estimates using the preferred control group. In columns 3 and 4 all students in group 4 are used as the control group, whereas in columns 5 and 6 all students from groups 2, 3 and 4 are used as the control group. It can be concluded the difference between columns 1 (2) and 3 (4) the estimates is small. On the other hand, the estimated treatment effect is smaller in size when using all students in groups 2, 3 and 4 as the control group.

Table C.5. DD estimates of the change in the standard loan amount in the loan phase, alternative control groups.

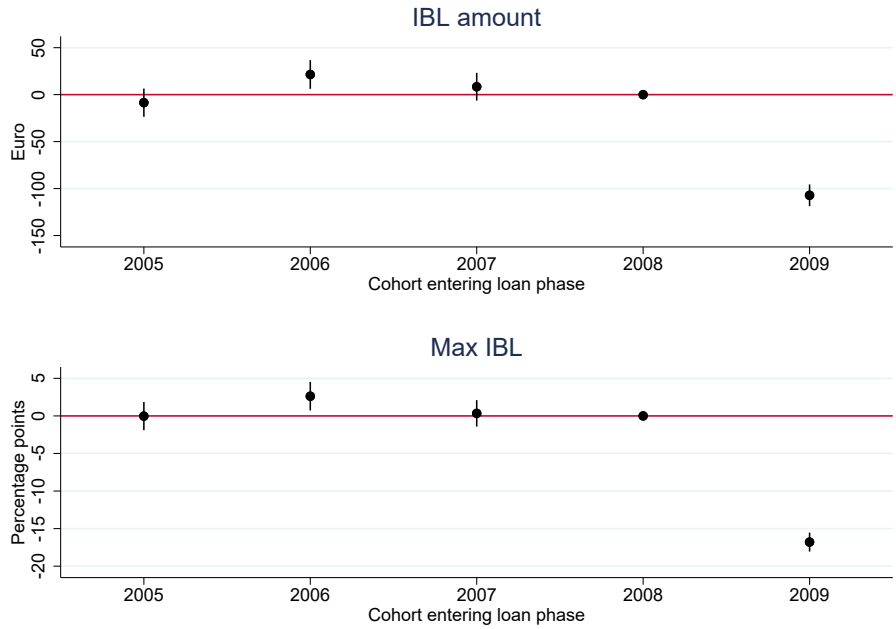
	Group 4, no SG		Group 4, all students		Groups 2-4, all students	
	(1) IBL	(2) Max IBL	(3) IBL	(4) Max IBL	(5) IBL	(6) Max IBL
Treatment	-106.66***	-0.17***	-107.42***	-0.17***	-97.26***	-0.14***
	(6.60)	(0.01)	(5.89)	(0.01)	(5.86)	(0.01)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
$\bar{Y}$	157.46	0.11	153.18	0.10	378.63	0.34
sd(Y)	275.48	0.31	272.29	0.30	370.99	0.47
Adj. R^2	0.08	0.08	0.07	0.07	0.21	0.18
N	202,076	202,076	290,769	290,769	552,267	552,267

Note: The table reports estimates from a DD regression (of equation 2) of the effect of the treatment (for group 1) on the two main outcome variables: "IBL": requested loan amount, and "Max IBL": dummy equal to 1 if the student borrows the maximum amount. These coefficients represent the average monthly effect over the first 12 months in the loan phase. Students in group 4 who did not receive supplementary grant (in the grant phase) are used as the control group in columns 1 and 2. All students in group 4 is the control group in columns 3 and 4. All students in all other groups (2, 3 and 4) is the control group in columns 5 and 6. All models use the sample of students entering the loan phase in September 2008 (pre-treatment) or 2009 (post-treatment). The included controls are listed in Section 4.1. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

However, below, the common trend assumption for the different control groups is examined and especially with respect to the maximum loan dummy the assumption seems to be violated, which most likely explains (at least part of) the difference in the estimated treatment effects.

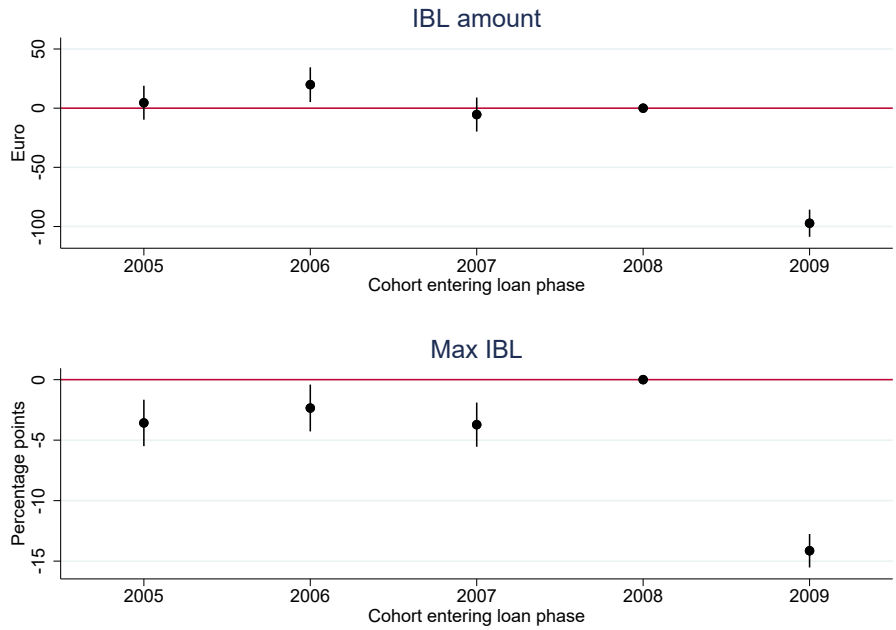
Examining the common trend assumption. Figures C.5 and C.6 examine the common trend assumption underlying the DD estimations presented in Table C.5, by means of placebo treatment regressions. Figure C.5 presents the coefficient of interest of a placebo treatment regression, examining the common trend assumption underlying the DD estimation presented in column 3 (top panel) and column 4 (bottom panel) of Table C.5. Figure C.6 presents the coefficient of interest of a placebo treatment regression, examining the common trend assumption underlying the DD estimation presented in column 5 (top panel) and column 6 (bottom panel) of Table C.5.

Figure C.5. Treatment 1, placebo treatment plot with all students in group 4 as control group to examine the common trend of the IBL amount and of the max. IBL dummy .



Note: The dependent variables are the IBL amount (top panel) and a dummy equal to 1 if the student borrows the maximum IBL amount (bottom panel). Sample consist of students in group 1 (treated) and group 4, who enter the loan phase in September in different years. The vertical lines represent the 95% confidence interval.

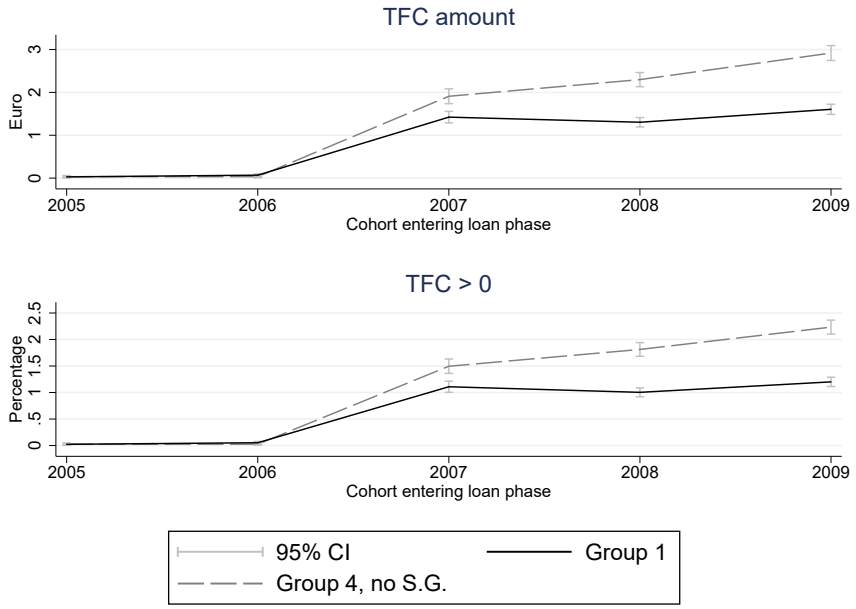
Figure C.6. Treatment 1, placebo treatment plot with all students in groups 2-4 as control group to examine the common trend of the IBL amount and of the max. IBL dummy .



Note: The dependent variables are the IBL amount (top panel) and a dummy equal to 1 if the student borrows the maximum IBL amount (bottom panel). Sample consist of students in group 1 (treated) and groups 2 - 4, who enter the loan phase in September in different years. The vertical lines represent the 95% confidence interval.

Appendix C.2.5 Robustness check: Substitution between the regular loan and the TFC. Recall from Section 2.1that the tuition fee credit (TFC), introduced in September 2007, is an additional interest-bearing (monthly) loan meant for assisting the finance of the tuition fee. This loan can be requested on top of the regular/standard interest-bearing loan (IBL), and is subject to the same repayment regulations. Consequently, in theory it could be the case that the reduction in the requested IBL amount due to the treatment (the change in the default loan amount), is compensated by an increase in the requested TFC. However, Figure C.7 suggests that the treatment did not lead to a substitution between the requested IBL amount and the requested TFC, since the means for both groups follows the same pattern over time. This is also confirmed by estimating the main DD specification, using the requested TFC as the dependent variable.

Figure C.7. Mean of a dummy equal to 1 if TFC > 0, and of the requested TFC amounts for group 1 and group 4.



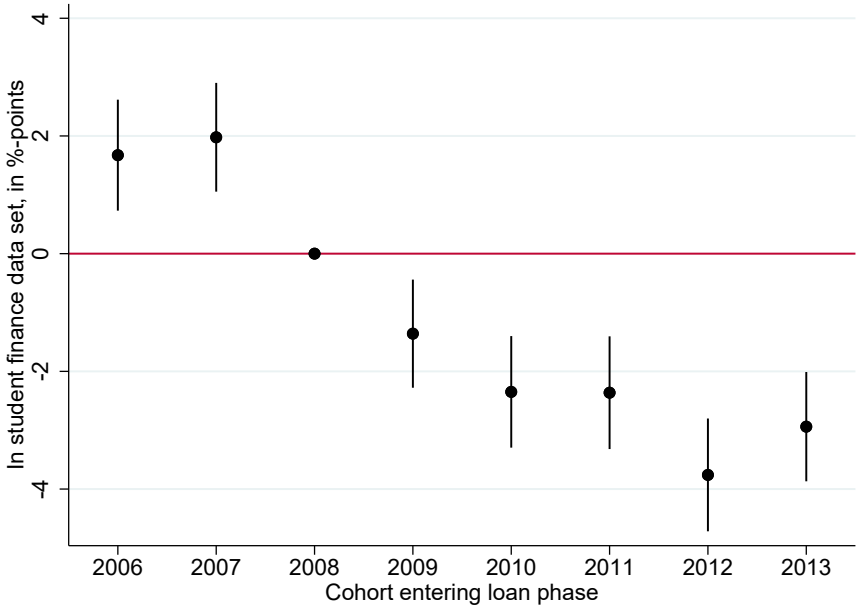
Note: The average values of the requested tuition fee credit (TFC, top panel) and of a dummy equal to 1 if the student has requested a positive TFC (bottom panel), are plotted on the y-axes. The sample consist of students in group 1 (treatment group) and group 4 (excluding the students who received a supplementary grant in the grant phase), who enter the loan phase in September in different years (2005-2009). Note that the TFC was introduced in 2007. The vertical lines represent the 95% confidence interval.

Appendix C.2.6 Robustness check: Dynamic selection. The three most important reasons that a student is no longer in the student finance data are that the student (1) dropped out, (2) successfully completed the study program or (3) or stopped requesting/receiving student finance components (potentially due to ineligibility). From all the students that are still in the student finance data set in the first month of the loan phase, 82.8% is still in the data set after 12 months in the loan phase and only 63% is still in the data after 13 months. This is most likely due to the fact the 13th month is the start of a new academic year. However, it is important to consider if the treatment influenced the amount of students that are no longer in the data set as of some

point during the first year of the loan phase, since this could cause sample selection bias.

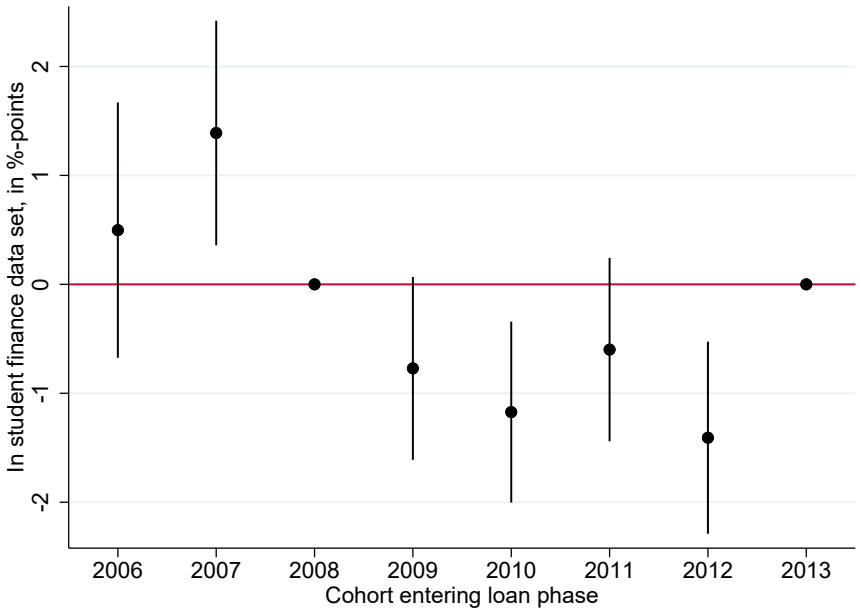
Figure C.8 plots the coefficients of a placebo treatment regression for group 1, with groups 2, 3 and 4 used as the reference category. The dependent variable is a dummy equal to 1 if the student is still in the data set. Data for the first 12 months in the loan phase is included. The last pre-treatment period is used as the reference category, and thus is equal to 0 by definition. The coefficient for 2009, is statistically different from 0, which could mean the treatment has an effect on the amount of students that drop out of the data somewhere during the first year of the loan phase. However, all pre-treatment period coefficients are also statistically different from 0, which means the common trend assumption is violated. A downward trend of group 1, relative to groups 2-4, is visible. Figure C.9 plots the coefficients of the same regression as plotted in Figure C.8, but now also including a group specific time trend, similar to for example columns 7, 8 and 9 of Table 7. Angrist and Pischke (2008) suggest including a group specific time trend as a robustness check, to ensure the treatment effect is not due to the fact the dependent variable was decreasing in group 1 (the treated) regardless of the treatment. When comparing the two figures, it can be concluded that this group specific time trend significantly impacts the estimated coefficients. The coefficient of interest: 2009, is no longer significantly different from 0. In conclusion, it is unlikely the that dynamic selection causes the DD to be confounded by sample selection bias, since the increase in the amount of students that drop out of the data in 2009 is not due to the treatment, but part of a trend.

Figure C.8. Treatment 1, placebo treatment plot to examine potential dynamic selection, not including a group specific time trend.



Note: The dependent variable is a dummy equal to 1 (0) if the student is still (no longer) in the data set. Sample consist of students in group 1 (treated in 2009) and groups 2, 3 and 4 (reference category, treated in 2010), who enter the loan phase in September in different years. The vertical lines represent the 95% confidence interval. No controls are included in this placebo treatment regression.

Figure C.9. Treatment 1, placebo treatment plot to examine potential dynamic selection, including a group specific time trend.



Note: The dependent variable is a dummy equal to 1 (0) if the student is still (no longer) in the data set. Sample consist of students in group 1 (treated in 2009) and groups 2, 3 and 4 (reference category, treated in 2010), who enter the loan phase in September in different years. The vertical lines represent the 95% confidence interval. No controls are included in this placebo treatment regression. A group specific time trend is included.

Appendix C.3 2SLS estimation: Additional tables and figures, and characterizing the compliers.

Appendix C.3.1 Tables including the first stage, second stage, reduced form and OLS estimations. Table C.6 and Table C.7 provide more details of the 2SLS estimation with respect to the chance of receiving a bachelor or master diploma within 12 months after entering the loan phase. The reported coefficient from the first stage (column 1) is the exogenous shock to student borrowing behavior created by the default loan amount change for group 1 in the first month of the loan phase (September). There are no concerns with respect to the weak instrument problem, since the value of the F-test of the excluded instruments is 2210.34.

The second stage (column 2) and reduced form (column 3) coefficients correspond to the coefficients reported in Figure 9 and Figure 10. The coefficient from the OLS regression is reported in column 4. However, as discussed in Section 4.1 the independent variable (borrowing behavior) is likely to be endogenous. For example, if students with a relatively low social economic status have a lower study success, see for example APA (2017) and NCES (2015), and borrow more on average, see for example Houle (2014), the estimates of column 4 of Table C.6 and Table C.7 are biased downwards.

Table C.6. 2SLS estimation of effect of borrowing behavior on obtaining a bachelor diploma within 12 months after entering the loan phase.

	2SLS			
	(1)	(2)	(3)	(4)
	1st Stage	2nd Stage	Red. Form	OLS
Treatment	-332.268149***		0.028455**	
	(7.070386)		(0.011566)	
IBL		-0.000086**		-0.000054***
		(0.000035)		(0.000011)
Dep. variable	IBL	Bachelor in 1 yr	Bachelor in 1 yr	Bachelor in 1 yr
Controls	Yes	Yes	Yes	Yes
$\bar{Y}$	183.25	0.27	0.27	0.27
sd(Y)	299.76	0.44	0.44	0.44
Adj. R^2	0.32	0.23	0.23	0.23
N	18,592	18,592	18,592	18,592

Note: Columns 1, 2 and 4 presents estimates from regression analysis of equations 5, 6 and 4 respectively. Column 3 presents the results of the reduced form estimation. Study success in this case is a dummy equal to 1 if the student received a master diploma in the first 12 months of the loan phase. All controls listed in Section 4.1 are included. Additionally, whether or not the student is enrolled in a bachelor program is added as a control variable. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

Table C.7. 2SLS estimation of effect of borrowing behavior on obtaining a master diploma within 12 months after entering the loan phase.

	2SLS			
	(1)	(2)	(3)	(4)
	1st Stage	2nd Stage	Red. Form	OLS
Treatment	-332.348335***		0.000471	
	(7.069094)		(0.008188)	
IBL		-0.000001		-0.000029***
		(0.000025)		(0.000007)
Dep. variable	IBL	Master in 1 yr	Master in 1 yr	Master in 1 yr
Controls	Yes	Yes	Yes	Yes
$\bar{Y}$	183.25	0.12	0.12	0.12
sd(Y)	299.76	0.33	0.33	0.33
Adj. R^2	0.32	0.29	0.29	0.29
N	18,592	18,592	18,592	18,592

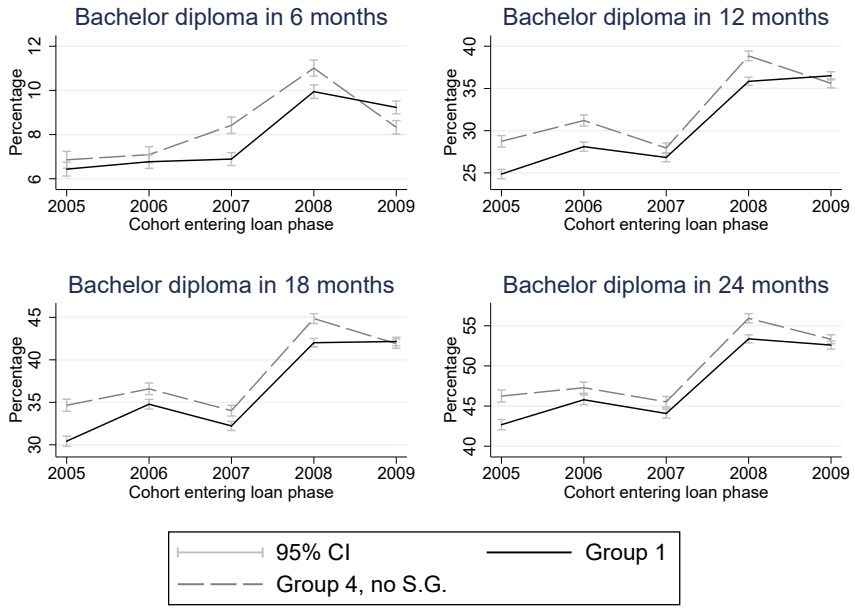
Note: Columns 1, 2 and 4 presents estimates from regression analysis of equations 5, 6 and 4 respectively. Column 3 presents the results of the reduced form estimation. Study success in this case is a dummy equal to 1 if the student received a master diploma in the first 12 months of the loan phase. All controls listed in Section 4.1 are included. Additionally, whether or not the student is enrolled in a master program when entering the loan phase is added as a control variable. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

Appendix C.3.2 Testing the common trend assumption underlying the reduced form estimations. The common trend assumption underlying the reduced form can be examined analo-

gously to the common trend assumption underlying the DD estimations of the impact of the nudge on borrowing behavior. Consequently, Figure C.10 (Figure C.11) plots the mean of dummy variables equal to 1 if a student earns a bachelor (master) diploma within 6, 12, 18 or 24 months after entering the loan phase, for different cohorts of students entering the loan phase in September. Figure C.12 (Figure C.13) plots the coefficients of a placebo treatment regression, where the common trend assumption is likely to be valid if the coefficients in the pre-treatment years (2005-2007) are not significantly different from 0, where the last pre-treatment period is the omitted/reference category.

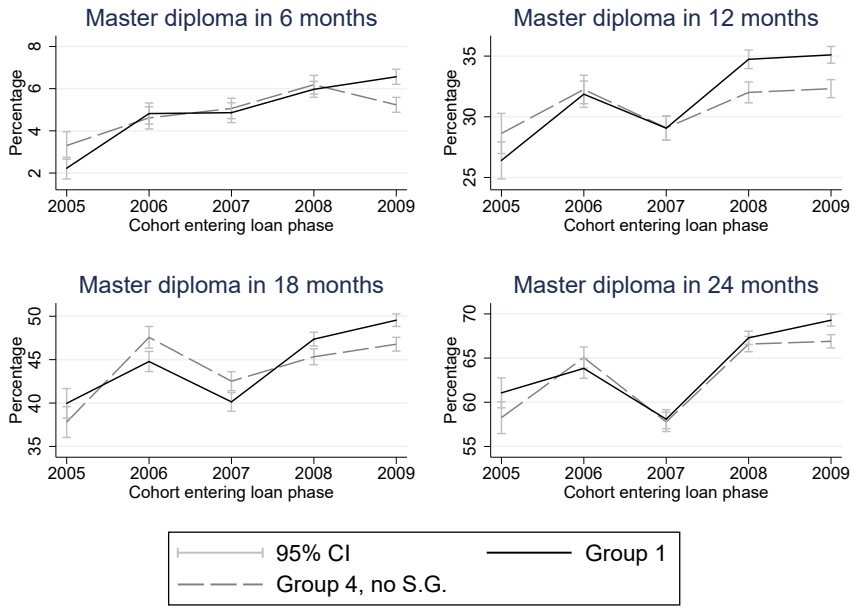
It can be concluded that the common trend assumption is likely to be valid for all dummy variables. However, only 1 reports a statistically significant treatment effect (the coefficient for the 2009 cohort), the dummy equal 1 if the student obtains a bachelor diploma within 12 months after entering the loan phase.

Figure C.10. Examining common trend of the chance of obtaining a bachelor diploma within 6, 12, 18 and 24 months.



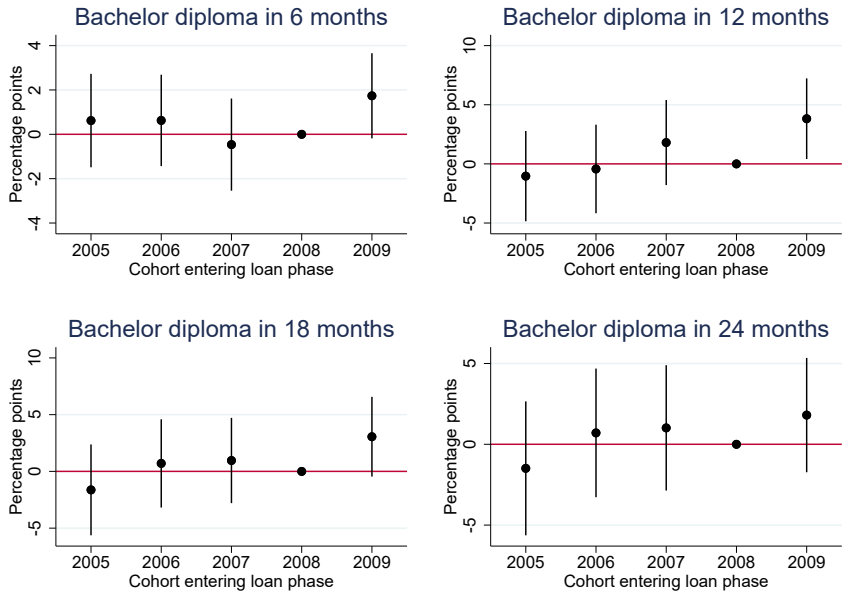
Note: The average value of a dummy equal to 1 if the student has obtained a bachelor diploma within 6 (top left), 12 (top right), 18 (bottom left) or 24 (bottom right) months is plotted for different cohorts of students entering the loan phase. Only students enrolled in a bachelor program when entering the loan phase are included in the sample. The vertical line represents the 95% confidence interval.

Figure C.11. Examining common trend of the chance of obtaining a master diploma within 6, 12, 18 and 24 months.



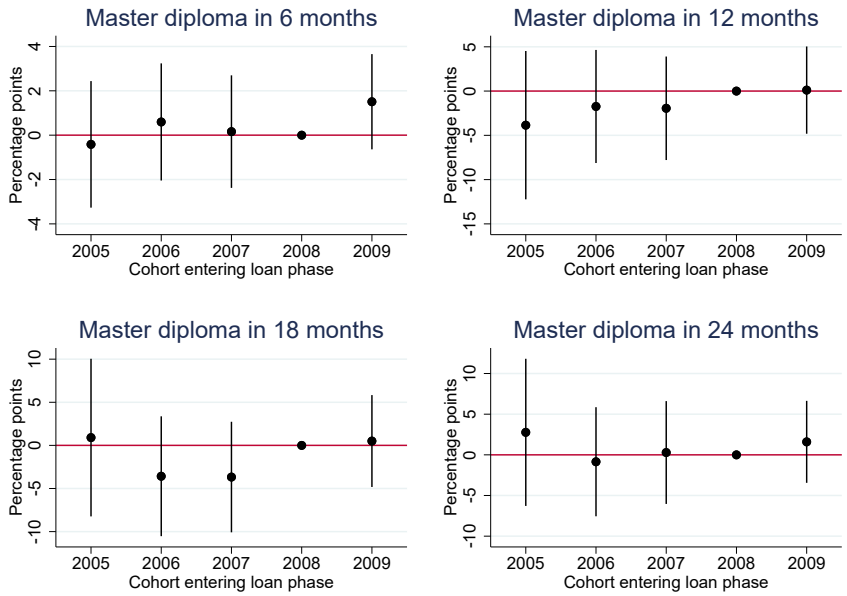
Note: The average value of a dummy equal to 1 if the student has obtained a master diploma within 6 (top left), 12 (top right), 18 (bottom left) or 24 (bottom right) months is plotted for different cohorts of students entering the loan phase. Only students enrolled in a master program when entering the loan phase are included in the sample. The vertical line represents the 95% confidence interval.

Figure C.12. Examining common trend of the chance of obtaining a bachelor diploma within 6, 12, 18 and 24 months, placebo treatment plot.



Note: Placebo treatment plots of a dummy equal to 1 if the student has obtained a bachelor diploma within 6 (top left), 12 (top right), 18 (bottom left) or 24 (bottom right) months is plotted for different cohorts of students entering the loan phase. Only students enrolled in a bachelor program when entering the loan phase are included in the sample. The vertical line represents the 95% confidence interval.

Figure C.13. Examining common trend of the chance of obtaining a master diploma within 6, 12, 18 and 24 months, placebo treatment plot.



Note: Placebo treatment plots of a dummy equal to 1 if the student has obtained a master diploma within 6 (top left), 12 (top right), 18 (bottom left) or 24 (bottom right) months is plotted for different cohorts of students entering the loan phase. Only students enrolled in a master program when entering the loan phase are included in the sample. The vertical line represents the 95% confidence interval.

Appendix C.3.3 Characterizing compliers. Recall that the 2SLS model estimates a local average treatment effect (LATE), which implies the results are only informative about the subpopulation of compliers (students who change their borrowing behavior because of the treatment). Hence, it is informative to know the characteristics of the compliers. In other words, are complier students more or less likely to be female or university students for example? The answer to this question is given by the ratio of the first stage of a specific subgroup to the overall first stage (Angrist and Pischke, 2008). This means the relative likelihood that a complier student is a female is given by the ratio of the first stage for females to the overall first stage.

However, recall that the size of the treatment (the change to the default loan amount) differs by the living situation of the student in the last month of the grant phase. The 2SLS estimation performed in the main text uses the average treatment effect as the exogenous shock to borrowing behavior, which is an average of the treatment effect of students not living with their parents and living with their parents (see Section 6.1.3). Consequently, it is important to take this into account when trying to characterize the complier students. Therefore, Table C.8 reports the ratio of the first stage for a specific subgroup to the overall first stage on (1) all students, (2) only students who are living with their parents and (3) only students who are not living with their parents.

Table C.8. Complier characteristics ratios.

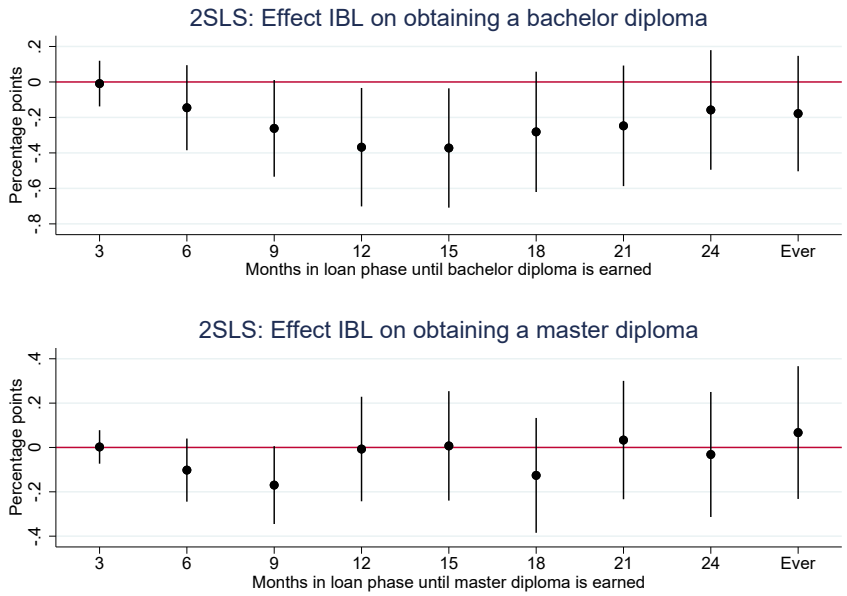
	All students		Living with parents		Not living with parents	
	(1) First Stage (in €)	(2) Ratio	(3) First Stage (in €)	(4) Ratio	(5) First Stage (in €)	(6) Ratio
All students	-332.348		-422.549		-283.384	
Males	-386.883	1.164	-480.654	1.138	-316.477	1.117
Females	-269.621	0.697	-315.034	0.655	-252.169	0.797
hbo	-428.819	1.590	-516.086	1.638	-335.596	1.331
wo	-264.922	0.618	-290.088	0.562	-256.094	0.763
Mig. background	-394.604	1.490	-499.803	1.723	-336.385	1.314
No mig. background	-323.286	0.819	-410.377	0.821	-276.067	0.821

Note: The exogenous shock to student borrowing behavior in the first month of the loan phase caused by the change to the default loan amount for group 1 is the instrument. Columns 1, 3 and 5 report the first stage of the 2SLS estimation for different subgroups for all students, students living with their parents and students not living with their parents respectively. Columns 2, 4 and 6 report the ratio of the first stage of a specific subgroup (indicated at the left side of the table) to the overall first stage (displayed in the first row). wo refers to university education and hbo refers to higher vocational education.

Though the ratios do differ by the sample on which the first stage is estimated (all students, living with parents or not living with parents) a consistent pattern emerges. Compliers are more likely to be males, higher vocational education students (hbo) and students with a migration background. Compliers are less likely to be females, university students and students without a migration background.

Appendix C.3.4 Robustness check: exploiting the shock to the cumulative requested IBL amount in the first 3 months after entering the loan phase as the instrument. In the main 2SLS estimation (Figure 9), the effect of the default loan amount change in the first month of the loan phase is used as the instrument creating an exogenous shock to borrowing behavior. As a robustness check, Figure C.14 reports the 2SLS estimates using the effect of the treatment on the requested interest-bearing loan amounts in the first 3 months of the loan phase. When compared to Figure 9, the size of the coefficients is smaller but the overall pattern of the effects remains the same.

Figure C.14. 2SLS, using the effect of the nudge on the cumulative requested IBL amounts of the first 3 months of the loan phase as the instrument.



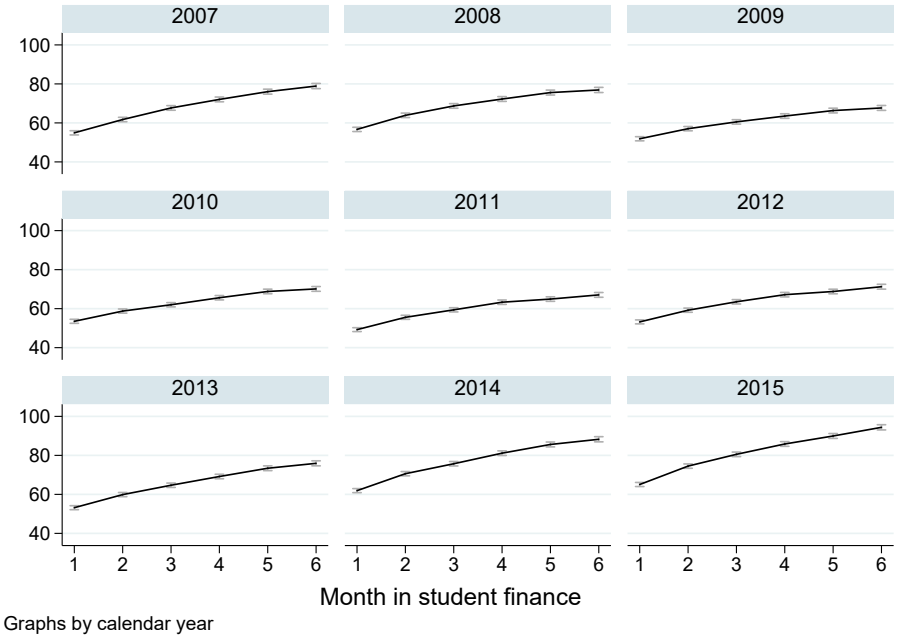
Note: The reported coefficients are the estimates from the second stage of the 2SLS estimation. The effect of the nudge on the cumulative requested IBL of the first 3 months of the loan phase is used as the instrument. The coefficients are rescaled so that they represent the effect of borrowing €100 more on chance of obtaining a bachelor (top panel) or master (bottom panel) diploma in percentage points. The vertical line represents the 95% confidence interval. All controls listed in Section 4.1 are included. Additionally, whether or not the student is enrolled in a bachelor program (top panel) or master program (bottom panel) when entering the loan phase is added as a control variable.

Appendix D Treatment 2: Robustness of the results.

Appendix D.1 Before-after comparison.

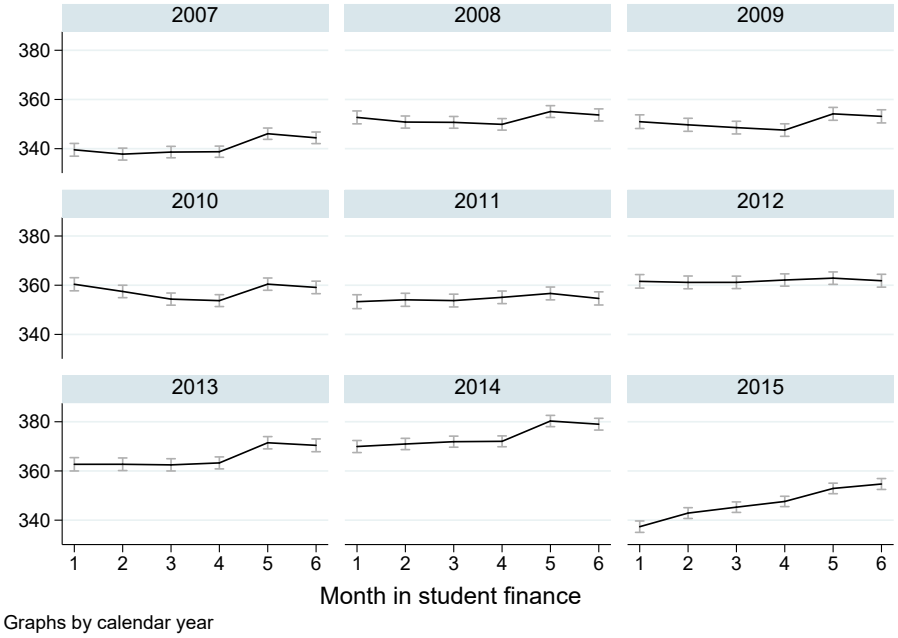
Figures D.1, D.2 and D.3 present the means of the IBL amount, the dummy indicating if the student borrows the maximum IBL amount and the dummy indicating if the student borrows at least 99% of the maximum IBL amount, respectively, for all included cohorts (available in the data set) of first-year students who start receiving student finance in September. Note that the tick box was removed in March 2014, implying that the cohort of students who start studying in September 2015 is the first cohort after the 'treatment'.

Figure D.1. Treatment 2, average requested IBL amount, by cohorts of first-year students, in euro.



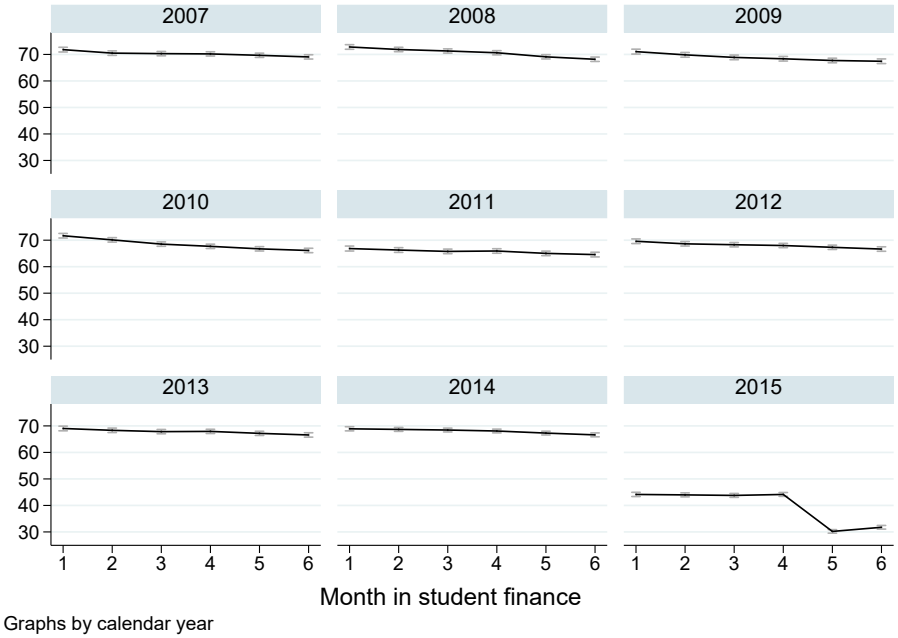
Note: Average requested interest-bearing loan amounts are on the y-axes (in euro). Only students entering the loan phase in September are included. The vertical lines represent the 95% confidence interval.

Figure D.2. Treatment 2, average requested IBL amount, conditional on IBL > 0 , by cohorts of first-year students, in euro.



Note: Average requested interest-bearing loan amounts are on the y-axes (in euro). Only students entering the loan phase in September are included. The vertical lines represent the 95% confidence interval.

Figure D.3. Treatment 2, average proportion of students borrowing $\geq$ 99% of the maximum IBL amount, conditional on IBL > 0, by cohorts of first-year students, in percentages.



Note: Average proportions of students borrowing $\geq$ 99% of the maximum interest-bearing loan amount are on the y-axes (in euro). Only students entering the loan phase in September are included. The vertical lines represent the 95% confidence interval.

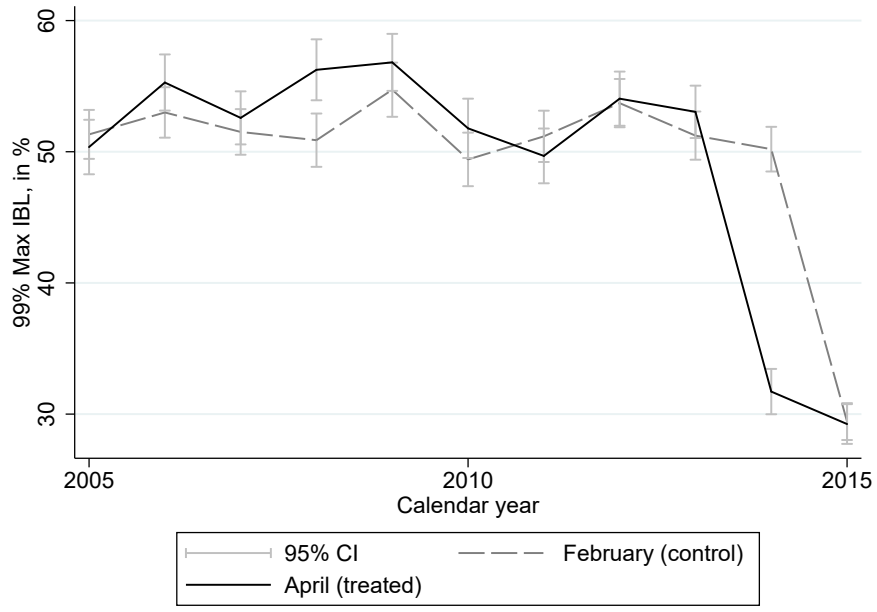
Appendix D.2 Differences-in-differences & common trend assumption.

Appendix D.2.1 Additional figures examining the common trend assumption of the main DD analysis. This section provides some additional figures to approximate the validity of the common trend assumption underlying the main DD results presented in Section 6.2.2.

- Figure D.4 tests the common trend assumption by plotting the average values for the '$\geq$ 99% maximum IBL amount dummy' (dummy equal to 1 if the student borrows $\geq$ 99% of the maximum IBL amount). From visual inspection can be concluded the common trend assumption seems to be valid. Nevertheless, the following figures are a more precise examination of the common trend assumption.

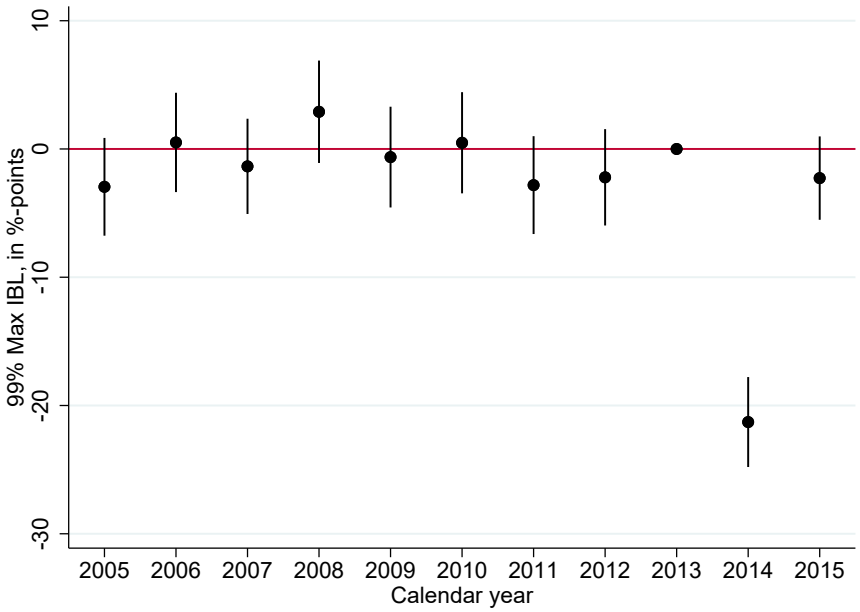
- Figure D.5 presents the estimates from a placebo treatment regression, including all control variables, using the '$\geq$ 99% maximum IBL amount dummy' as the dependent variable. The last pre-treatment period is taken as the reference category. Ideally all coefficient of the pre-treatment periods would be on the red horizontal line, meaning that there was no treatment effect in the pre-treatment periods (implying the common trend assumption is likely to be valid), which also is the case. The 2015 coefficient is also not significantly different from 0, which is correct since both the students who borrow for the first time in February and April are confronted with the treatment in the (calendar) year 2015.

Figure D.4. Treatment 2, common trend of the $\geq$ 99% maximum IBL amount dummy.



Note: The variable plotted on the y-axis is a dummy equal to 1 if the student borrows at least 99% of the maximum interest-bearing loan amount. Sample consist of first-time borrowers in February (control) and April (treated) in (calendar) years 2005-2015. The vertical lines represent the 95% confidence interval.

Figure D.5. Treatment 2, placebo treatment plot to examine the common trend of the $\geq$ 99% maximum IBL amount dummy, including all controls.



Note: All controls are included in this placebo treatment regression. The dependent variable is $\geq$ 99% maximum IBL: a dummy equal to 1 if the student borrows at least 99% of the maximum interest-bearing loan amount. Sample consist of first-time borrowers in February (control) and April (treated) in (calendar) years 2005-2015. The vertical lines represent the 95% confidence interval.

Appendix D.2.2 Robustness check: DD analysis with no controls and including multiple pre-treatment cohorts. Table D.1 presents two robustness checks of the specification reported in Table 14. Columns 1, 2 and 3 report the estimated coefficient of interest of equation 8, excluding control variables. Note that control variables at the individual level are not required in a DD regression in order to get unbiased estimations, but they can increase the precision of the estimates (Angrist and Pischke, 2008). Columns 4, 5 and 6 extends the sample by including all pre-treatment periods available in the data set (2005-2013), this model includes year fixed effects and a group (month) specific time trend, as suggested by Angrist and Pischke (2008). This only requires a small adjustment to equation 8: (1) the T_c is replaced by year fixed effects (which is simply a more general specification), and (2) a group specific time trend is added. The addition of a group specific time trend assures that the treatment effect is not due to the fact the interest-bearing loan amount was decreasing in April regardless of the treatment.

The estimates reported in Table D.1 are very close to the reported estimates of the preferred model (Table 14). This means the preferred specification discussed in the main text is robust to (1) excluding the control variables and to (2) including multiple pre-treatment periods and the inclusion of group specific time trends.

Table D.1. Impact of removing the tick box on the borrowing behavior, robustness check of preferred DD specification.

	No controls			Multiple pre-periods		
	(1)	(2)	(3)	(4)	(5)	(6)
	IBL	Max IBL	$\geq$ 99% Max IBL	IBL	Max IBL	$\geq$ 99% Max IBL
Treatment	-31.515***	-0.290***	-0.203***	-28.041***	-0.290***	-0.204***
	(5.944)	(0.018)	(0.019)	(4.850)	(0.015)	(0.016)
Controls	No	No	No	Yes	Yes	Yes
Group-specific trend	No	No	No	Yes	Yes	Yes
$\bar{Y}$	345.80	0.44	0.47	334.64	0.50	0.51
sd(Y)	157.98	0.50	0.50	151.81	0.50	0.50
Adj. R^2	0.00	0.06	0.03	0.13	0.08	0.07
N	11,343	11,343	11,343	48,552	48,552	48,552

Note: Each column represents a different OLS regression of equation 8, on the sample of first-time borrowers in the grant phase in February (control group) and April (treated group). Columns 1-3 uses the sample of fist-time borrowers in 2013 and 2014, where columns 4-6 uses the sample of first-time borrowers in 2005-2014 and includes group-specific time trends, as suggested by Angrist and Pischke (2008). Dependent variables: "IBL = Interest-bearing loan amount, in € & "($\geq$ 99%) Max IBL" = dummy equal to 1 if student borrows ($\geq$ 99%) the maximum IBL. The included controls are listed in Section 4.2. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

Appendix D.2.3 Robustness check: Only using the student finance data set. Table D.2 compares estimates from the preferred specification (excluding controls) of the merged data sets (columns 4-6) with the estimates only using the student finance data (columns 1-3). This robustness check ensures that the part of the student finance data lost by merging the different data sets does not significantly impact the DD estimations of the treatment effect. From comparing the columns 1-3 with columns 4-6, in both tables, it can be concluded the portion of student finance data lost by merging the different data sets has no sizable impact on the treatment effect.

Table D.2. Impact of removing the tick box on the borrowing behavior, DD estimation results, only using student finance data set.

	Only student finance data			Merged data sets		
	(1) IBL	(2) Max IBL	(3) $\geq$ 99% Max IBL	(4) IBL	(5) Max IBL	(6) $\geq$ 99% Max IBL
Treatment	-31.14***	-0.28***	-0.19***	-31.52***	-0.29***	-0.20***
	(5.40)	(0.02)	(0.02)	(5.94)	(0.02)	(0.02)
Controls	No	No	No	No	No	No
Group-specific trend	No	No	No	No	No	No
$\bar{Y}$	352.31	0.49	0.51	345.80	0.44	0.47
sd(Y)	154.13	0.50	0.50	157.98	0.50	0.50
Adj. R^2	0.01	0.07	0.04	0.00	0.06	0.03
N	13,269	13,269	13,269	11,343	11,343	11,343

Note: Each column represents a different OLS regression of equation 8, on the sample of first-time borrowers in the grant phase in February (control group), and April (treated group) in the years 2013 (pre-treatment) and 2014 (year of the treatment). Dependent variables: "IBL = Interest-bearing loan amount, in € & "($\geq$ 99%) Max IBL" = dummy equal to 1 if student borrows ($\geq$ 99%) the maximum IBL. In the columns 1-3, only the student finance data set is used. Columns 4-6 uses the merged data set, excluding control variables. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

Appendix D.2.4 Robustness check: DD analysis using 4 months around the treatment. In the main text the samples used in the DD analyses include first-time borrowers 1 month before (February) and 1 month after (April) the removal of the tick box. Table D.3 presents estimates similar to the estimates in Table 14 and Table D.1, but using a sample of 2 months before and 2 months after the treatment. The tradeoff is that using more months increases the amount of observations, but the common trend assumption and the precision of the estimates might be compromised. The estimated equation is similar to equation 8, where now month fixed effects are used instead of the group dummy (both models are identical if only 2 months are included in the sample):

$$Y_{imc} = \alpha + \mu_m + \lambda T_c + \delta D_{mc} + X'_{imc}\beta + \varepsilon_{imc}, \tag{9}$$

where Y_{imc} is the outcome variable, for example the requested interest-bearing loan amount, for student i, in month m, in cohort c, T_c is a dummy which equals 1 for the treated cohort, μ_m are month fixed effects. D_{mc} , the regressor of interest (the DD estimator), is a dummy which equals 1 students who applied for a loan for the first time in April or May 2014 (treated after the treatment). In the columns 7-9 of Table D.3, the T_c (year dummy) is replaced with year fixed effects, and a group specific time trend is added. This model serves as a robustness check, as suggested by Angrist and Pischke (2008). From comparing the reported estimates in Table D.3 and Table 14 and Table D.1, it can be concluded the main estimates (reported in the main text) are robust to using a sample of 4 months before and after the treatment.

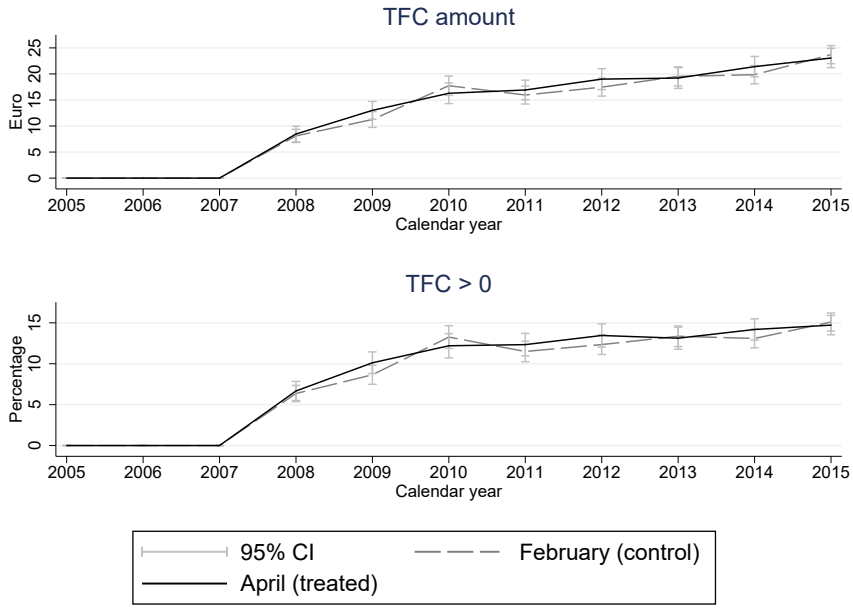
Table D.3. Impact of removing the tick box on borrowing behavior, DD estimation results 4 months around the time of treatment.

	No controls			All controls			Multiple pre-periods		
	(1) IBL	(2) Max IBL	(3) $\geq$ 99% Max IBL	(4) IBL	(5) Max IBL	(6) $\geq$ 99% Max IBL	(7) IBL	(8) Max IBL	(9) $\geq$ 99% Max IBL
Treatment	-31.184***	-0.313***	-0.224***	-31.511***	-0.320***	-0.231***	-32.001***	-0.317***	-0.229***
	(4.348)	(0.013)	(0.014)	(4.116)	(0.013)	(0.013)	(3.564)	(0.011)	(0.011)
Controls	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Group-specific trend	No	No	No	No	No	No	Yes	Yes	Yes
$\bar{Y}$	347.47	0.45	0.47	347.46	0.45	0.47	335.71	0.50	0.51
sd(Y)	157.87	0.50	0.50	157.95	0.50	0.50	151.86	0.50	0.50
Adj. R^2	0.00	0.07	0.03	0.11	0.14	0.11	0.13	0.08	0.07
N	21,219	21,219	21,219	21,150	21,150	21,150	88,825	88,825	88,825

Note: Each column represents a different OLS regression of equation 8on the sample of first-time borrowers in the grant phase in January and February (control group), and April and May (treated group). Columns 1-6 uses the sample of fist-time borrowers in 2013 and 2014, where columns 7-9 uses the sample of first-time borrowers in 2005-2014 and includes group-specific time trends, as suggested by Angrist and Pischke (2008). Dependent variables: "IBL = Interest-bearing loan amount, in € & "($\geq$ 99%) Max IBL" = dummy equal to 1 if student borrows ($\geq$ 99%) the maximum IBL. The included controls are listed in Section 4.2. Student clustered standard errors in parenthesis. */**/*** denote significance at a 10/5/1 percent confidence level.

Appendix D.2.5 Robustness check: Substitution between the regular loan and the TFC. Recall from Section 2.1that the tuition fee credit (TFC), introduced in September 2007, is an additional interest-bearing (monthly) loan meant for assisting the finance of the tuition fee. This loan can be requested on top of the regular/standard interest-bearing loan (IBL), and is subject to the same repayment regulations. Consequently, in theory it could be the case that the reduction in the requested IBL amount due to the treatment (the removal of the tick box), is compensated by an increase in the requested TFC. However, Figure D.6 suggests that the treatment did not lead to a substitution between the requested IBL amount and the requested TFC, since the means for both groups follows the same pattern over time. This is also confirmed by estimating the main DD specification, using the requested TFC as the dependent variable.

Figure D.6. Mean of a dummy equal to 1 if TFC > 0, and of the requested TFC amounts for first-time borrowers in February and April.



Note: The average values of the requested tuition fee credit (TFC, top panel) and of a dummy equal to 1 if the student has requested a positive TFC (bottom panel), are plotted on the y-axes. The sample consist of first-time borrowers in February (control) and April (treated) in (calendar) years 2005-2015, note that the TFC was introduced in 2007. The vertical lines represent the 95% confidence interval.