ERASMUS UNIVERSITY ROTTERDAM
Erasmus School of Economics
Master Thesis Policy Economics

Can Students Be Nudged Towards Making More Conscious Decisions on Higher Education and Borrowing? A Policy Evaluation of the 'Bewuste Studiekeuze' Pilot Program of the Dutch Ministry of Education, Culture and Science.

Griet De Lombaerde
Student ID number: 437472
Supervisor: Prof. Dinand Webbink
Second Assessor: Esmee Zwiers
August 2018

Written at the Ministry of Education, Culture and Science, under the supervision of Dr. Marc van der Steeg.

Abstract

This thesis evaluates the pilot program 'Bewuste Studiekeuze' (Conscious Study Choice) of the Dutch Ministry of Education, Culture and Science. This policy intervention was aimed at increasing the efficiency of information provision by adjusting the timing and form of current communication means and emphasizing the benefits and affordability of higher education. This, to nudge students to take more conscious decisions on higher education enrolment and financial aid. We conclude that, first, there is an important difference between an opt-in and opt-out design in receiving the treatment in the mails. Second, if any, we do not find positive effects on higher education enrolment, and for some subgroups we even find significant negative effects. Third, we find positive effects on the request of a means-tested grant (4% on average), which seems driven by the effect on the opt-in group that opened the mails.

Word of Thank

There are people that deserve a special word of thank. My supervisors and teachers, my family and friends. First Marc van der Steeg and professor Dinand Webbink, who gave me the chance to set my first steps as a policy economist. I will always be thankful for their guidance and belief in me. I want to thank Matthijs Oosterveen, whose feedback during and after the seminar and ETP, has helped many lost students. Also, the teachers that I had, the ones that were enthusiastic and the ones that made us suffer. My education at the Erasmus University of Rotterdam and the University of Ghent gave me the knowledge and ideas about the interesting world of economics. This thesis and my internship at the Ministry of Education, Culture and Science was my first real-life experiment. My kind colleagues of Directie Kennis made this experience definitely less scary. During this period, I learned how economics is about the most human aspects of life. More than ever, I feel the motivation to keep learning and change things.

Without my family, especially my parents, I would not be where I am now. They taught me the most important lessons in life. Their love and encouragement makes me move on. Never will I be able to express the gratitude they are worth, but I hope they can see by my acts.

Finally, I want to thank my friends, who made these years in the wonderful city of Rotterdam the most enjoyable and probably happiest of my life.

Thank you all for letting it happen.

Table of Contents

Acronyms 4
I. INTRODUCTION 5
Research Problem and Relevance 5
Research Question 9
II. LITERATURE 9
Policy Relevance 9
Behavioral Economics/Barriers in Education 10
III. DATA 15
Data Sources and Sample 15
List of Variables Used 16
IV. METHODOLOGY 17
Identification Strategy 17
Empirical Strategy 20
V. RESULTS 23
Main Estimation Results and Heterogeneous Effects 23
VI. CONCLUSION AND DISCUSSION 30
APPENDIX 32
Appendix A: Interventions and Treatment Groups (letters, mails, timing) 32
Appendix B: Variables 38
Appendix C: Empirical Evidence 41
Appendix D: Identification Strategy 45
Appendix E: Analysis Mails 46
Appendix F: Characterizing the Compliers 47
Appendix G: Heterogeneous Effects 48
REFERENCES 49

Acronyms

Abbreviation	Dutch	English translation
VO	Voortgezet Onderwijs	Secondary Education
HAVO	Hoger Algemeen Voortgezet Onderwijs	Higher General Secondary Education
VWO	Voorbereidend Wetenschappelijk Onderwijs	Pre-University Education
VAVO	Voortgezet Algemeen Volwassenen Onderwijs	Secondary General Adult Education
MBO	Middelbaar Beroepsonderwijs	Secondary Vocational Education
HBO	Hoger Beroepsopleiding	Higher Vocational Education
WO	Wetenschappelijk Onderwijs	Scientific Higher Education (University)
MOCW	Ministerie van Onderwijs, Cultuur en Wetenschap	Ministry of Education, Culture and Science
DUO	Dienst Uitvoering Onderwijs	Education Executive Agency of the Dutch Ministry of Education, Culture and Science
SCP	Sociaal en Cultureel Planbureau	The Netherlands Institute for Social Research
CPB	Centraal Planbureau	The Netherlands Bureau for Economic Policy Analysis
BRIN	Basisregistratie Instellingen	*Basic Registration Institutions*
SES	Socio-Economische Status	Socioeconomic status

I. INTRODUCTION

Research Problem and Relevance

Current Situation and Institutional Setting

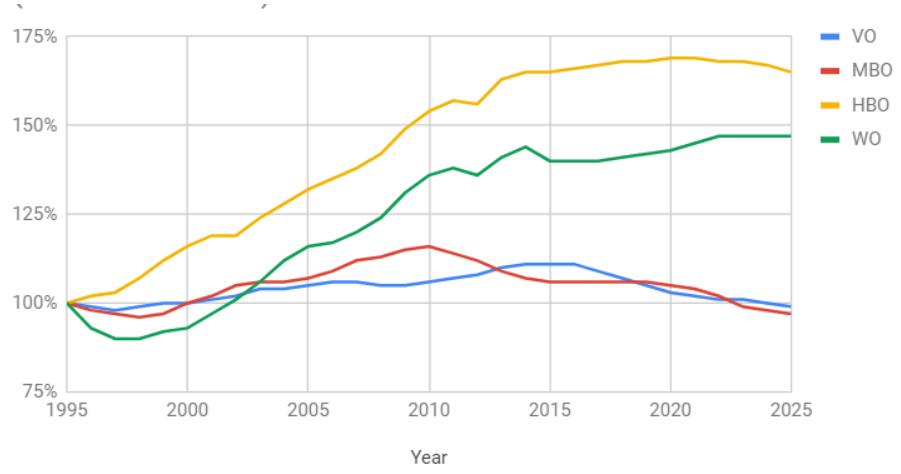
The future is in young people. To succeed in their societal and personal goals, they must prepare today. Are the current choices they make, contributing to these achievements? And if not, what is the cause and what can be done? In this thesis, we investigate the informational and behavioral limitations that induce Dutch students to make suboptimal decisions on higher education, and specifically, the effects of a policy aimed at influencing these.

The Ministry of Education, Culture and Science (*Ministerie van Onderwijs, Cultuur en Wetenschap*, MOCW) is competent to apply and regulate issues on Dutch Education, which is mandatory for children between the ages of 5 and 16. After kindergarten and primary education, around the age of 12, children enter secondary education (*Middelbaar* or *Voortgezet Onderwijs*, VO). Here, they can choose different tracks, namely Secondary Vocational Education (*Middelbaar Beroepsonderwijs*, MBO), Higher General Secondary Education (*Hoger Algemeen Voortgezet Onderwijs*, HAVO) and Preparatory Scientific Education (*Voorbereidend Wetenschappelijk Onderwijs*, VWO). Furthermore, there is Secondary General Adult Education (*Voortgezet Algemeen Volwassenen Onderwijs*, VAVO). In the final years of secondary education, students choose a specialization (profile or domain). At the end of secondary education, students have to take a final test to graduate. To enter higher education (*Hoger Onderwijs*, HO), students must hold a HAVO- or MBO-4 diploma if they want to enrol in Higher Vocational Education (*Hoger BeroepsOnderwijs*, HBO), and a VWO- or first-year HBO diploma, if they want to enrol in Scientific or University Education (*Wetenschappelijk Onderwijs*, WO) (Rijksoverheid, 2018).

Two trends are remarkable. According to an estimation by the MOCW (2017)[1], the current increasing trend in higher education enrolment will continue until 2022, from then on, it will gradually decrease. Moreover, we observe that children of parents with a lower socioeconomic status (SES), are less prone to pursue higher education.

[1] MOCW (2017). Referentieraming 2016. Retrieved July 2018 from: https://www.onderwijsincijfers.nl/kengetallen/onderwijs-algemeen/leerlingen-en-studenten

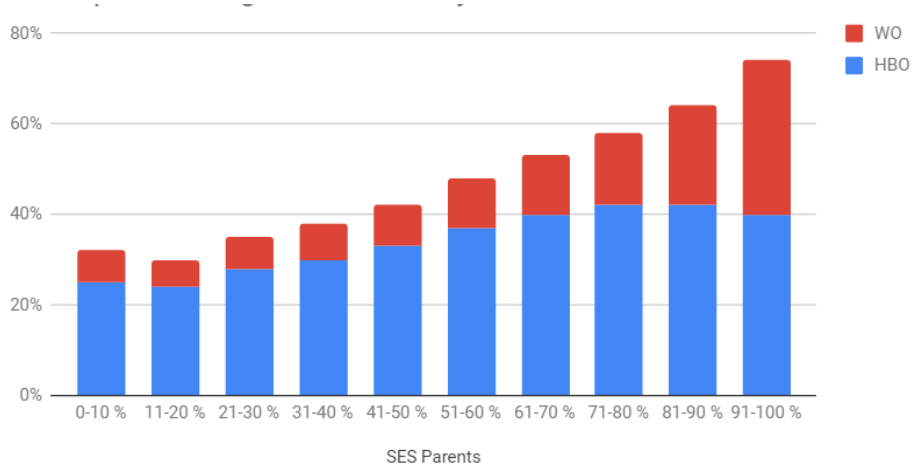
Figure 1: Evolution of Enrolled Students by Type of Higher Education (index 1995 = 100)



Source: OCW, Referentieraming 2016 in: Onderwijs in Cijfers (2017)

From 1995, we see a steady increase in enrolment rates in HBO and VWO, relative to the base year. For MBO, it decreases from 2010. Comparing to the stable evolution of VO enrolment, it is an improvement in the transition to higher education. It is estimated that the enrolment rates will decline from 2022, but this is probably due to the decline in births from 2000.

Figure 2: Participation in Higher Education by Socioeconomic Status Parents

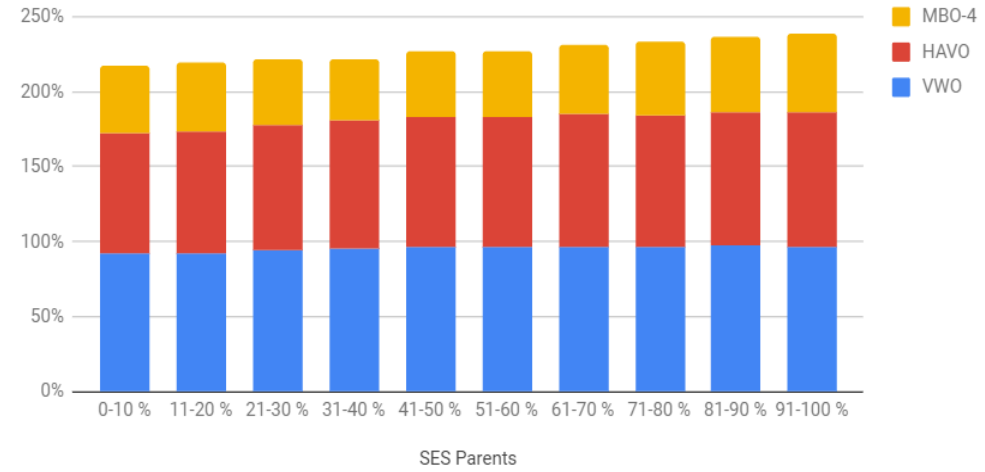


Source: CBS in: Onderwijs in Cijfers (2017)

This graph shows the cohort of students that were 16 years old in October 2009, and that were or had been enrolled in higher education at the age of 22 (October 2015), by the SES percentile of their parents. The main message, is that higher SES families, usually are more likely to obtain higher education. Whereas in the poorest SES groups the enrolment rate is 32%, it is 74% in the richest. Also, the fraction of WO-students is higher at the top. However, these differences probably arise earlier than during the transition of secondary to higher education. This can be seen from Figure 3,

which shows the previous secondary education type of these students. As we see, the difference between SES groups is smaller (4-10%) in the transition from secondary to higher education. The enrolment rates in higher education for MBO students are lower because this is a vocational education.

Figure 3: Fraction in MBO-4, HAVO and VWO at the age of 16, that were/had been enrolled in Higher Education at the age of 22, by SES Parents



Source: CBS in: Onderwijs in Cijfers (2017)

Two takeaways from these figures. First, enrolment rates in higher education in the Netherlands have been increasing and are already quite high. The focus of the research is to find whether an information problem could still be a holdback. Second, there is a clear relationship between socioeconomic status and the pursuit of higher education. If it is transmitted through the information channel, the government may want to mitigate this to fight inequality.

Suboptimal decisions have short term consequences. Not only does it harm the motivation and wellbeing of the youth, it also puts pressure on public resources. Suboptimal decisions have also long-term consequences. They undermine the wellbeing of subsequent generations, and curb economic growth. The importance of human capital for society is undeniable.

An important event was the reform of the student finance system in 2015. Before that, student loans became a gift if students graduated within ten years. After 2015, this gift was cancelled, and now all student loans must be repaid. Many students claim to be afraid of borrowing, and there are concerns about the accessibility of higher education, especially for vulnerable MBO students. To tackle this in the new reform, the government increased the means-tested grant. This pilot program aims at ensuring that eligible students make use of the available services.

Policy Intervention

The pilot program "Bewuste Studiekeuze", is an experiment set up by the Dutch Ministry of Education, Culture and Science to examine students' decisions on higher education and its financial aspect. This randomized control trial (RCT) targets last-year secondary school exam participants, from Higher General Secondary Education (HAVO), Preparatory Scientific Education (VWO) and Secondary Vocational Education fourth level (MBO-4) for the period 2016/2017. Every year in October, the Ministry sends a letter to these students containing important information to make a well-thought decision on higher education. By adjusting the letter (i.e. making it clearer and more concise) and including a mailing service, the experiment tests how much there is to gain in efficiency by nudging different phases of the decision-making process.

To evaluate the effectiveness of the intervention, we are interested in the causal effect of different types of information treatment (adjusted letter, opt-in vs. opt-out email services) on student behavior and decisions. Our working hypothesis is the following: **a modification in the level of clarity and presentation of information, focusing on the benefits vs. the costs of higher education, can improve students' decisions on enrolment and loan applications in The Netherlands**. Therefore, we assess the behavioral responses of students randomly assigned to different treatment groups who received an adjusted letter or were included in a mailing service and compare these to the control group of students that only received the standard letter. The interventions and treatment groups are in Appendix A.

By studying this, the government can make policies that better suit students' needs and, provide higher quality and more accessible information, making them more effective. As such, insufficient transition, drop-out and study switch, and fear of borrowing can be tackled. However, it is important to emphasize that this describes a pilot, and the project has not yet proven to be fully effective. Further research is needed to find which interventions have a positive/negative/no effect and are best suited for the Dutch educational system. Only in a later stadium, policy makers could think of implementing this at the national level. Moreover, it could be interesting to investigate other factors that affect the study choices of the youth and the relations with future outcomes (MOCW, Pilot Bewuste Studiekeuze, 2017).

Research Question

The research question is: **Did the pilot program of the MOCW have an effect on students' decisions on higher education and financial aid, especially among more disadvantaged groups, and could this be interpreted as an improvement in the efficiency of information provision by the Dutch government?**

To answer this question, Section II gives an overview of the literature on behavioral biases related to education and interventions aimed at tackling these. Section III describes the data. Section IV presents the research design for an empirical effect analysis dealing with both average and heterogeneous effects. Section V shows the results. Finally, Section VI concludes and discusses possible explanations and policy recommendations.

II. LITERATURE

Policy Relevance

The relevance of the studied policy stems from the assumption of suboptimal decision making of students with respect to higher education. Observed low enrolment rates, study switch and dropout, as well as inequality, are a first indication. However, higher education may not be the optimal choice for everybody. The key issue is that students make decisions that improve their personal and society's welfare. Education is an important contributor to long-run economic growth, by catalyzing innovation, improving institutions and social-cultural networks, increasing income and reducing inequality Therefore the individual choices on human capital accumulation are primordial (The World Bank, 2018[2]).

A lack of, or incorrect information often causes suboptimal decisions. The role for the government here, is to combat informational limitations, especially among weaker groups. More concretely, the literature points towards a lack of information on the returns and costs of higher education. It has been shown that students and parents tend to underestimate the returns and overestimate the costs, especially among lower socioeconomic strata (Lergetporer et al., 2018; Ross et al., 2013; McGuigan et al., 2012; Bleemer and Zafar, 2017).

This relates to the concept of equality of opportunities as one of the main motivations for government intervention. People with the same ability and effort should have the possibility to attain a similar

[2] The World Bank (2018). Retrieved August 2018 from: http://www.worldbank.org/en/topic/education/overview

socioeconomic position, irrespective of their socioeconomic background. However, these differences in backgrounds do lead to differences in educational attainment and more generally to persistence of inequality over generations (McGuigan et al., 2012). While education should be one of the mechanisms in society that fights inequality, currently it has been a driving force. Children from initially more disadvantaged households also find more difficulties regarding education attainment and future outcomes (Lergetporer et al., 2018).

Behavioral Economics/Barriers in Education

Behavioral economics tells us that people are often confronted with behavioral constraints when making decisions. By integrating insights from psychology, neuroscience and sociology, it can explain suboptimal investment in education (Lavecchia et al., 2016). This, in contrast to the rational human capital model, where individuals weigh costs and benefits to optimally invest in human capital, increasing their productivity and thus their future earnings. Young people, whose brain is not fully developed yet, face specific cognitive limitations which impede conscious decision making. These barriers can have considerable consequences, particularly on issues with a long-run horizon (Ross et al., 2013). The definition and typology of these barriers are subjective. In this thesis, however, we consolidate the insights of Damgaard and Nielsen (2018) and Lavecchia et al. (2016), which are comparable and applicable to other authors' work as well.

Self-control problems or present bias. Young people often have difficulties with the intertemporal trade-off between current vs. future costs and benefits. Because of distractions, they procrastinate homework, (registration) deadlines etc. This can be more pronounced amongst boys (Duckworth and Seligman, 2006) and low-SES students (Mischel et al., 1989; Golsteyn et al., 2014), but training can help to increase self-control or mitigate present bias (Becker and Mulligan, 1997).

Limited attention and cognitive ability. Related to the present-focus, complexity and salience can complicate information processing. Therefore, students decide to focus on known information or routine. The transition to higher education is characterized by large amounts of new information and routines, for example application procedures. Too little and too much information or choice can have negative effects as well. Again, there is evidence that low-SES students suffer more from this bias (Hoxby and Avery, 2004).

Default bias and Framing. Related to the above mentioned, students and parents often stick to the most salient and cognitively easy option. Additionally, loss aversion and low reference points, may lead to underinvestment in education. This can be explained by the reliance on routines, and the social environment (Damgaard and Nielsen, 2018; Lavecchia et al., 2016).

Social norms. To comply with the social image, students are very susceptible to their environment. The projection bias, where people think that the future environment or situation will be similar to the present, can be connected to this phenomenon as well. This is a problem, when social pressure and negative identities lead to self-destructive behavior such as crime, but can also explain why students make unfavorable or shortsighted decisions such as truancy. Social norms are inherent to culture, race, religion, gender etc. (Damgaard and Nielsen, 2018; Lavecchia et al., 2016).

Behavioral Policy Design

For policy interventions to be effective, it is important to understand the behavioral mechanisms that drive and hinder the decisions made by people (Damgaard and Nielsen, 2018). It must be said however, that considering ex-ante observed behavior suboptimal, is often based on behavioral assumptions, and can be assessed by the success of interventions (Lavecchia et al., 2016, p.17). Ross et al. (2013) name three reasons why behavioral policies are attractive in education. First, educational decisions are often characterized by considerable behavioral barriers, inherent to the student population. Second, as these policies often imply minor changes to existing settings, they can be quite cost-effective and therefore, politically more attractive than alternatives. Third, empirical evidence has shown that these policies can have larger effects for certain groups that are most in need, such as low-income households.

Application of Nudges in Education

Thaler and Sunstein (2008, p.6) defined nudging as: *"altering people's behaviors in a predictable way without forbidding any options or significantly changing their economic incentives".* As Sunstein (2014) adds, they are 'liberty-preserving'. The two main reasons for nudging interventions are; (i) their cost-effectiveness, relatively small changes to existing programs can yield large effects; and, (ii) their broad applicability to influence behavior (e.g. economic choices, health, education etc.). Lavecchia et al. (2016) identified different areas where there is to gain by the implementation of behavioral nudges, namely; parental involvement, educational completion, attainment and suitability, and college aid and costs awareness, which can sometimes be correlated with socioeconomic status.

Next, we present a brief overview of different types of nudges that can be used to solve (postsecondary) education problems. This overview is based on the work of Sunstein (2014), Lavecchia et al. (2016), Damgaard and Nielsen (2018), and Ross et al. (2013). It presents the nudging intervention and the behavioral bias it addresses. Table C.1 in Appendix C gives an overview of the empirical evidence on the effectiveness of these interventions.

To assess self-control problems and present bias, Lavecchia et al. (2016) propose to offset immediate costs with immediate benefits. This can be done by precommitment strategies, reminders, deadlines and goal setting, through mailings, text messages etc.; making students think about their future. Furthermore, assistance can be provided to boost the skills that enhance active and conscious decision-making (Damgaard and Nielsen, 2018). As decisions have long-run consequences but must be taken in a short time period, timing is crucial. Too early, and students may postpone or not act; too late, and students may not have time to prepare or act (Ross et al., 2013, p.8; Lavecchia et al., 2016).

Related to this, lowering the cognitive costs of certain actions or decisions, by changing default options or adjusted framing, can make people more likely to do so. Especially if it involves new routines. Simplifying or structuring (administrative) procedures for example, may increase the take-up of services. Underutilization of available resources is an important reason for ineffective policies, and people who need it the most, may be the less prone to use them (e.g. low-income households and financial aid; Ross et al., 2013). This is the main focus of the thesis. Making important information more salient, by reminders, warnings etc., can also help to get people's attention and tackle forgetfulness. Moreover, personal assistance can contribute to efficiency, by offering a more tailored approach. These interventions can help parents as well, whose other responsibilities may ask already considerable mental effort.

Positive social interactions can yield positive outcomes. Social belonging, identity activation and mindset nudges can alter students' beliefs and self-image and motivate responsible decisions. Informing students on the decisions of others, can steer them in the same direction. Empirically, however, these peer group manipulations are not always found to be effective, and social comparison nudges can even have perverse effects (Damgaard and Nielsen, 2018).

To summarize, we could state that these interventions relate to the provision of information. Most of these policy mechanisms work through changing beliefs and perceptions, which change intended actions, and preferably change actual behavior[3]. Better informed individuals are ought to take better thought decisions. Bleemer and Zafar (2017) argue that informational interventions affect (intended) behavior if the information is not known beforehand, or, was not salient enough.

There is empirical evidence for heterogeneous effects of policies based on differences such as age, gender, education (stages), socioeconomic status etc. Damgaard and Nielsen (2018) state that the behavioral interventions are more likely to have positive effects for groups that initially suffer most, because of insufficient or biased information, for example. Therefore, to ensure effectiveness, these

[3] We must notice that in many research, the positive outcome was intended behavior, which may explain why some policies turn out ineffective.

policies should be targeted and pinpoint the barrier that impedes decision-making (Damgaard and Nielsen, 2018).

Empirical Evidence

Although nudge policies have potential in many areas, empirical and experimental evidence enables us to assess whether they achieve the desired effects. Moreover, while some policies may be very successful in one setting, they may be ineffective or aversive in different (institutional) settings (Sunstein, 2014). There is an extensive empirical literature on interventions similar to the one studied in this thesis. we focus on interventions that fight present-bias and self-control problems (e.g. reminders, assistance, defaults and framing), the lack of or biased information, and concern the social environment. The majority of these studies are RCT's using administrative or survey data, and targeting high-school students around the age of 17. In that respect, they are comparable to our study. However, whether we can extrapolate the results from the US is discussed further.

Table C.1 in Appendix C gives an overview of interesting studies to relate to our evaluation. This paragraph summarizes the main findings, and what we can learn from them. First, information provision (e.g. through reminders, text messages, etc.) on the returns and costs to higher education, does have a potential to increase enrolment rates of students (Hoxby and Turner, 2015; Dinkelman and Martinez, 2014). Also, information on the application procedure (e.g. steps, deadlines, available assistance, etc.) could increase the likelihood of enrolment (Castleman and Page, 2015, Bird et al., 2017). However, other studies found information treatment to be ineffective in increasing enrolment (Kerr et al., 2015; Carrell and Sacerdote, 2017). An important explanation to consider for our results as well, is the difference between studies that measure intended versus actual enrolment, as many interventions report positive effects on beliefs and intentions related to higher education (Kerr et al., 2015; McGuigan et al., 2012; Lergetporer et al., 2018; Oreopoulos and Dunn, 2013; Peter and Zambre, 2017).

Second, within nudging interventions, there is a considerable role for the presentation of information, namely defaults and framing. Increasing the salience of important information could be quite effective, especially among weaker groups. Oreopoulos and Ford (2016) found that adding structure to the application procedure increased enrolment in higher education by 19%-points for a treatment group of at-risk students[4] in Canada. However, the effects did not completely persist after two years, probably due to decreased assistance (Oreopoulos and Ford, 2016). In a compelling study, Bergman and Rogers (2017), examine the effect of changing the default opt-in to an opt-out adoption of a mailing service, directed to parents of high-school students in the US. They find that 7.8% of the opt-

[4] Who were not in the university-preparatory track in high school.

in vs. 96.5% of the opt-out group, adopted the service. Moreover, a subsequent survey suggests that the use of the service increases the valuation of it, and that the positive effect thus not only stems from the higher cost of opting-out. The explanation put front by Damgaard and Nielsen (2018) is thought provoking for our study; while the opt-in group is often initially more motivated, there is less to win. Opt-out interventions can reach groups that are harder to reach, and as such generate larger effects. Defaults, are often applied in the context of finance. Marx and Turner (2017) find that going from a default zero to a non-zero loan amount, increased the likelihood of loan take-up by 40%. Benhassine et al. (2015) show that framing an unconditional transfer directed to parents, as financial aid for education, increased enrolment by 30%-points.

Third, information alone may not be sufficient. We see that combined interventions of information and assistance may be more effective (Castleman and Page, 2015, Ross et al., 2013; Oreopoulos and Dunn, 2013; Avery, 2013; Bettinger et al., 2012; Carrell and Sacerdote, 2013, Castleman et al., 2012; Oreopoulos and Ford, 2016). However, Page et al. (2017) state that information on available assistance, can also increase the utilization of these services, and as such have positive effects on enrolment. Oreopoulos and Ford (2016) found that an assistance-only intervention, did not have any effect on enrolment for US high-school students in their final year.

Fourth, to mitigate the influence of the social environment on the information and support students receive, interventions directed to parents, are a potential channel to tackle biased beliefs and socio-economic inequality. Parents play an important role in the decision on educational investment, and thus targeting these, can yield positive results on students' outcomes (Bleemer and Zafar, 2017; Behavioral Insights Team, 2015, Harackiewicz et al., 2012; Benhassine et al., 2015).

Fifth, caution is needed regarding heterogeneous effects. Many interventions had stronger effects for low SES, academically worse performing or initially less informed students (Castleman and Page, 2015; McGuigan et al., 2012; Oreopoulos and Dunn, 2013; Avery, 2003; Bettinger et al., 2012; Oreopoulos and Ford, 2016, Bird et al., 2017). The opposite is also possible however, as Lergetporer et al. (2018) and Harackiewicz et al. (2012) found stronger effects of the information intervention for higher educated households. Furthermore, adverse effects on borrowing of low-SES groups are found by Barr et al. (2017).

Finally, these studies stand out as they took place in The Netherlands. Borghans et al. (2015) investigated the effect of assistance to improve the choice on higher education provided to 4000 high-school students. As found in the survey, the treatment group who received this assistance was 2%-points less likely to regret their educational choice. These effects are even stronger for male and low-SES students (Borghans et al., 2015). Second, Booij et al. (2012) conducted a random experiment

providing information on loan conditions for high-school students, who were assumed to know about the universal eligibility of these. The authors did not find a significant effect on loan take-up, but did find a positive effect of 18% on awareness. The authors put front three explanations for the zero effect on borrowing; (i) the intervention came too late, (ii) the response was too heterogeneous (iii) increased knowledge does not imply that students will borrow more, as they may have been initially too optimistic about the borrowing conditions. This important finding suggests that Dutch students' loan take-up is not remarkably limited by a lack of information.

Finally, it is important to consider the external validity of these studies. Most of them are in the US, where the institutional context differs widely from The Netherlands regarding tuition fees, baseline enrolment rates, educational alternatives, financial credit systems and constraints, income distribution of the population, culture and beliefs, socioeconomic dynamics etc. (Lergetporer et al., 2018). Therefore, we should not only be careful with extrapolating the results to our setting, but also try to understand the mechanisms that drive this behavior. Additionally, Bird et al. (2017) state that apart from the evidence on local nudging interventions, we can insufficiently conclude whether these can be applied at national level as well.

III. DATA

Data Sources and Sample

The main dataset used for this research comes from DUO (Dienst Uitvoering Onderwijs), and consists of secondary education data for 2016/2017 (e.g. type of education, identification of schools, whether students passed and their grades on the final exam); intervention data for 2016/2017 (e.g. type of intervention, intermediary outcome variables such as whether students received and opened the mails and whether they clicked on the links), financial aid data for 2016/2017 (e.g. loan, loan amount, supplementary grant, supplementary grant amount), and higher education data for 2017 (e.g. enrolment, type of education, identification of the institution). Furthermore, data is provided on several background characteristics of the students (e.g. gender, age, postal code of residence). A final background characteristic is provided by the SCP (Sociaal en Cultureel Planbureau), the socioeconomic status scores of the place of residence, which is important to investigate heterogeneity in the behavior of students with different backgrounds.

The sample consists of 17.563 students in the final year of Dutch secondary education who took the final exam during the academic year 2016/2017. It covers the tracks or education types Higher General Secondary Education (HAVO), Pre-University Education (VWO) and Secondary Vocational

Education (MBO). This target group is at the moment of taking important decisions regarding their future, such as enrolling in higher education or not, which study to choose, and how to finance their studies. Figure A.1 in Appendix A gives an overview of the different subsamples in the intervention.

One of the main strengths of the data, is its large sample size, which ensures the power of the results. Additionally, the extended list of variables that can be used as control variables, gives more insight in specific dynamics. Although some interesting background characteristics are missing (e.g. ethnicity, timing of application, motivation etc.), we can argue that this does not cause a serious problem for our methodology, given the randomization of the experiment and the fact that these are likely to be correlated with the included variables (e.g. ethnicity and socioeconomic status).

List of Variables Used

The following variables are used in our analysis;

(i) Background characteristics as from October 2016 and used as control variables[5]: gender, age, type of secondary education, BRIN school code, whether the student's email address was registered at DUO, whether the student passed the final exam of secondary education, the average grade the student obtained for the final exam, socioeconomic status score and - quantile.

(ii) Pre-treatment student finance variables for the academic year 2016/2017: right to supplementary grant, right to student loan, amount of student loan received.

(iii) Intervention/treatment variables for 2016/2017: type of letter/mail, experimental group.

(iv) Intermediate outcome variables: mail 1/2/3/4/5/6 sent/received/opened, link 1/2/3/4 clicked.

(v) Outcome variables of interest as of October 2017: enrolled in higher education (yes/no), student took up a student loan (yes/no), amount of student loan, student took a supplementary grant (yes/no), amount of supplementary grant.

(vi) Additional outcome variables as of October 2017: level of higher education, component of higher education, domain MBO-4.

An overview and explanation of the variables used can be found in Appendix B.

[5] Other background variables available but not used in the analysis are: profile secondary education (HAVO and VWO), domain secondary education (MBO), whether the student had been registered at 'Mijn Studieplan'.

The descriptive statistics of the data serve to gain more insight in the underlying characteristics of the students, and how these differ between groups. They provide a sketch of the population and are a first step to the identification strategy and econometric analysis. We present the descriptive statistics of the groups in the next section.

IV. METHODOLOGY

Identification Strategy

Description of the Experimental Groups

There are three treatment groups which we want to compare to the control group that received the standard letter; (i) students who received the adjusted letter based on behavioral insights, emphasizing that higher education is worth it and financeable; (ii) students who received the adjusted letter and the possibility to register for the mailing service (opt-in); and (iii) students who received the adjusted letter and the possibility to unregister (opt-out). However, the sample should be split in two groups, which we consider different experiments and analyze separately. The reason, is that these groups are not formed randomly, but based on whether the student's mail address was registered at DUO before the start of the intervention. Those who were not previously registered could not opt-out[6]. This implies that the total sample of students is not equally distributed among the four[7] groups. However, because the sample sizes are large enough, we can run separate regressions.

Looking into the background characteristics for both experimental groups, confirms that these differ significantly. Table D.1 in Appendix D shows the mean, standard deviation and sample size of the background variables. This is as expected, as study financing at DUO is only possible from 18 years on. We conclude that these experimental groups are not comparable on background characteristics, and we therefore analyze them separately in the remainder.

Test Random Assignment: Conditional Independence Assumption

The aim of this research is to identify the causal effect of the policy intervention on the student outcomes of interest. The purpose is to find out how adjusting the information provided to students affects their behavior with respect to enrolling in higher education and the use of financial aid, by comparing how similar[8] groups of students respond to different forms of treatment, and compared to

[6] We consider the 24 observations of students belonging to experimental group 0 who received the opt-out treatment as outliers, and drop them from our data set.
[7] One control group and three treatment groups.
[8] Similar means that students would have the same outcomes without treatment.

the control group that did not receive treatment. This difference in potential outcomes is the causal effect of interest.

The assumption that validates the identification, is that the treatment assignment is random, or independent of potential outcomes, conditional on background differences between treated and untreated, known as the Conditional Independence Assumption (CIA). In other words, there are no other unobserved factors that correlate with receiving the treatment, implying that there is no selection bias. All students were equally likely to be treated. By testing this assumption, we exclude this possibility which could invalidate our results. If the CIA holds, then the Ordinary Least Squares (OLS) estimator is unbiased and the estimated coefficients can be interpreted as causal effects. Although the pilot experiment is in fact a Randomized Control Trial (RCT), it should be tested if the randomization was successful, and if these assumptions on the counterfactual are credible. Only then, the identification strategy is valid, and the differences in outcomes can be attributed to the program intervention. There are two main methods to test the randomness assumption. First, by comparing the means of the covariates between treatment and control groups. This gives a first indication of differences across groups. Second, by performing a balancing test to see whether the treatment significantly affects the covariates.

As can be seen from Table 1, within experimental groups, the background variables do not differ significantly across treatment groups. We conclude that the groups are similar and comparable, and that the randomization was successful. Therefore, OLS is a valid estimation method, as there are no other unobserved factors that we did not include and that could bias the results.

Table 1: Covariate Means Analysis Treatment Groups

Experimental Group 0

Background Variables	**All Groups**	**Standard Letter**	**Adjusted Letter**	**Opt-in**	**Opt-out**
Male (%)	47.52 (0.50)	47.77 (0.50)	47.53 (0.50)	47.28 (0.50)	/
Age	17.61 (1.54)	17.61 (1.55)	17.61 (1.54)	17.60 (1.53)	/
SES score	0.14 (1.13)	0.14 (1.13)	0.14 (1.12)	0.14 (1.14)	/
Observations	99,523	33,166	33,167	33,166	24

Standard errors between parentheses.

Education Type (%)	**All Groups**	**Standard Letter**	**Adjusted Letter**	**Opt-in**	**Opt-out**
MBO-4	7.33	7.35	7.43	7.23	/
HAVO	52.80	53.07	52.52	52.83	/
VWO	34.80	34.47	34.90	35.01	/
VAVO	5.07	5.11	5.16	4.93	/

Experimental Group 1

Background Variables	**All Groups**	**Standard Letter**	**Adjusted Letter**	**Opt-in**	**Opt-out**
Male (%)	47.91 (0.50)	47.48 (0.50)	47.28 (0.50)	48.44 (0.50)	48.43 (0.50)
Age	20.40 (2.05)	20.40 (2.05)	20.40 (2.05)	20.40 (2.05)	20.40 (2.04)
SES score	-0.22 (1.24)	-0.22 (1.24)	-0.22 (1.23)	-0.22 (1.25)	-0.22 (1.25)
Observations	72,040	18,011	18,010	18,010	18,009

Standard errors between parentheses.

Education Type (%)	**All Groups**	**Standard Letter**	**Adjusted Letter**	**Opt-in**	**Opt-out**
MBO-4	82.28	82.18	82.25	81.66	83.03
HAVO	3.43	3.62	3.41	3.41	3.26
VWO	5.18	5.16	5.07	5.55	4.92
VAVO	9.12	9.03	9.26	9.38	8.80

The balancing test regresses the covariates or controls on the treatment variable. As such, it tests whether receiving treatment, implies a significant difference in the covariates. If the estimated effect is not significant, then the covariates and treatment are independent, and assignment was random. Table 2 shows us that the diverse forms of treatment (i.e. standard letter, adjusted letter, opt-in mailing, opt-out mailing), do not have significant effects on the background variables (i.e. gender, age, SES score, and education type). Again, this confirms the covariate analysis.

Table 2: Balancing Test

Experimental Group 0

Balancing Test

	(1) Gender	(2) Age	(3) SES	(4) Education Type
Standard letter	0.0000 (.)	0.0000 (.)	0.0000 (.)	0.0000 (.)
Adjusted letter	-0.0024 (0.0039)	-0.0044 (0.0120)	-0.0002 (0.0088)	0.0044 (0.0054)
Opt-in	-0.0049 (0.0039)	-0.0123 (0.0120)	-0.0026 (0.0088)	0.0030 (0.0054)
constant	0.4777*** (0.0027)	17.6132*** (0.0085)	0.1421*** (0.0062)	2.3735*** (0.0038)
R^2	0.000	0.000	0.000	0.000
F	0.8134	0.5400	0.0536	0.3404
N	99499	99499	98964	99499

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Experimental Group 1

Balancing Test

	(1) Gender	(2) Age	(3) SES	(4) Education Type
Standard letter	0.0000	0.0000	0.0000	0.0000
	(.)	(.)	(.)	(.)
Adjusted letter	-0.0020	-0.0109	-0.0014	0.0030
	(0.0053)	(0.0216)	(0.0131)	(0.0100)
Opt-in	0.0096	0.0024	-0.0074	0.0160
	(0.0053)	(0.0216)	(0.0131)	(0.0100)
Opt-out	0.0095	0.0070	-0.0068	-0.0156
	(0.0053)	(0.0216)	(0.0131)	(0.0100)
constant	0.4748***	20.3987***	-0.2174***	1.4105***
	(0.0037)	(0.0153)	(0.0093)	(0.0070)
R^2	0.000	0.000	0.000	0.000
F	2.7096	0.2469	0.1625	3.3945
N	72040	72040	71665	72040

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

There are some concerns related to random experiments. First, the cost side. Nudges are relatively cost-effective as these imply small changes to existing programs. Rewriting the letter and setting up a mailing service is relatively easy and low-cost to implement. Second, ethics. As discussed in the literature on information interventions, one way to cope, is by providing the same information to all, but a slightly different presentation or timing. Third, spillover effects, substitution, and the Hawthorne effect imply that students may get the information through alternative channels (e.g. social media, peers, teachers…), and change their behavior. Because the randomization was made at individual level, it is possible that treated and untreated individuals exchanged information. However, since they received the letters at home and got personalized mails, and as the main difference between the treatments was in the timing and form, rather than in the content, we doubt this is a serious issue. The resulting reactions are ambiguously to predict. Fourth, the previous section confirmed that given the successful randomization, the identification strategy is internally valid. We can trust the results as credible signs of causal effects. Fifth, the external validity to other countries, is arguable. These experiments tend to be quite context-specific given the uniqueness of the Dutch institutional, educational and borrowing system. This is discussed further in the text.

Empirical Strategy

Ordinary Least Squares Estimation

We consider two estimation techniques for the effect analysis of the policy. As the CIA assumption holds, the basic OLS estimator of the treatment effect yields unbiased results. There are no unobserved characteristics in the error term correlated with the treatment and outcomes, and therefore the coefficient of interest will be unbiased. Concretely, we estimate the following equation:

$$Yi = \alpha + \beta Ti + \gamma\, i\, Xi + \varepsilon i \qquad (1)$$

Where Yi are the outcome variables of interest (i.e. enrolment, loan and supplementary grant binary dummies, loan and supplementary grant amount) Ti is the categorical treatment variable (i.e. standard letter, adjusted letter, opt-in, opt-out); Xi is a vector of background control variables (i.e. education type, gender, age, SES score); εi is the error term; α the intercept and β is the causal parameter of interest.

Some remarks are needed here; first, as expected from the covariate analysis, including other controls does not influence our results. Estimating equation (1) with only education type as control does not yield considerably different results than when including the full set. However, for the remainder of the analysis, we include them all. Second, there may be a risk of endogeneity if other unobserved determinants influence the probability that students actually get the treatment. We address this issue in the following paragraph. Third, although the sample is randomized at individual student-level, we cluster standard errors at school level (based on BRIN-code) to account for possible group-effects. This does not affect the results remarkably. Finally, we reiterate that the empirical analysis is done for the two experimental groups separately[9].

Instrumental Variables

The OLS estimator β gives the Intention to Treat (ITT) effect, as it measures the causal effect of the treatment assignment (type of letter or mailing service) on the outcomes of interest. A possible cause of perceived policy ineffectiveness, is partial compliance, which means that not all individuals who get assigned to the treatment, actually receive it. An unfortunate observation is that a large fraction of students who were included in the mailing service, did not open the mails. As can be seen from the graphs in Appendix E, there is not only a difference in the opening rates across mails, but also across treatment groups, students who opted-in being clearly more proactive in opening the received information. Furthermore, the rate of students that clicked the links with important information, is surprisingly low. In other words, within the group that was assigned to the mailing treatment (i.e. intention to treat), only a fraction opened the mails and got treated. Therefore, the treatment groups contain students that did not receive treatment. Partial compliance relates to the selection problem in the sense that students who expect to benefit from the treatment, are more likely to open the mails (Angrist and Pischke, 2009).

[9] Panel A of the tables refers to experimental group 0, whose mail address was not previously registered at DUO, Panel B refers to experimental group 1, whose mail address was previously registered at DUO.

Using the initial treatment assignment (mailing service), as an instrument[10] for the actual treatment (opening the mails), we estimate the effect of Treatment on the Treated (TOT, also known as the Local Average Treatment Effect - LATE). Concretely, we consider mail 1, 3 and 5, as content wise they refer to the outcomes of interest[11]. The method used, is Two Stage Least Squares (2SLS), where in the first stage, the endogenous treatment variable is regressed on the exogenous instrument, and in the second stage, the outcome variable is regressed on the obtained predicted values from the first stage. As such, we find the causal effect of opening the mails on the outcomes, which is the effect for the compliers or LATE. Concretely, we estimate the following equations:

First stage estimation (partial compliance):

$$Mi = \alpha 1 + \beta 1Oi + \gamma 1Xi + ui \qquad (2)$$

Second stage estimation (effect of treatment on the treated):

$$Yi = \alpha 2 + \beta 2\widehat{Mi} + \gamma 2Xi + \varepsilon i * \qquad (3)$$

Where Mi is the endogenous variable standing for the actual assignment to treatment (i.e. opening the mail), Oi are the exogenous instruments standing for the initial assignment to treatment (opt-in vs. standard letter, opt-out vs. standards letter, and opt-in vs. opt-out), β2 is the causal parameter of interest.

An internally valid instrument meets two criteria; it should be correlated with the endogenous variable of interest, and uncorrelated with other (unobserved) factors in the error term that could bias the outcome variable. Formally, we want to test four assumptions.

First, we test for independence. As seen from the CIA, the treatment and control group are similar on background characteristics, and the outcomes are thus independent of these. Second, the exclusion restriction. The only effect of the instrument (initial treatment assignment) on the outcome is through the endogenous treatment variable (actual treatment). This is not straightforward to test, but we can argue that as the treatment concerns the timing and salience of the information, rather than on the content itself, it is unlikely that this treatment could be transmitted through another channel (e.g. treated peers). Third, a strong first stage assures the relevance of the instrument. This is shown in Panel B of Table 3, where we see a significant effect of the instrument on the actual treatment variable. Fourth, monotonicity implies that there are no defiers[12], so we only find the effect on the compliers. This is difficult to test with a scatterplot given the nature of the categorical variables. Even

[10] As this is randomly assigned, there is no selection bias.

[11] I do acknowledge that reading different combinations of mails, may yield different effects.

[12] Defiers are students who, if assigned to treatment, would switch to the control group and thus not get treated, and if assigned to the control group, switch to get treated (Angrist and Pischke, 2009).

though it is possible that students in the control group got the information treatment from peers, and partial compliance implies that some students in the treatment group did not get treated, we assume there are no defiers. The reasoning is that the intervention treatment mainly regarded personalized, detailed and timed information. As such, we can estimate the effect of the information treatment in the mails on the students who got assigned and opened the mails (Angrist and Pischke, 2009).

A brief note on characterizing the compliers. Therefore, we look at what background characteristics of individuals are relatively more likely to comply to the treatment. The ratio of the first stage, indicating the strength of the instrument, for that specific subgroup, over the first stage of the total group. As we see from Table F.1 in Appendix F, there are no considerable differences between boys and girls, but there are large differences between students under/above 18 years, as younger students seem to be more likely to open the mails[13]. Furthermore, VWO and HAVO students are relatively more likely to comply by opening the mails. Finally, students from different SES backgrounds do not differ much in compliance.

Heterogeneous Effects

Finally, these two estimation techniques can be applied in specific settings. As expected and predicted (see literature section), the interventions may induce differential effects across population groups. We expect students in different environments, related to educational track, gender or socioeconomic background, or previous financial history, to make distinct decisions. The reason is that initial knowledge and beliefs, which characterizes certain groups, often interplay with the information treatment. Therefore, it is interesting to analyze the effects separately, as policy makers may be especially interested in tackling suboptimal education decisions for certain groups at risk.

V. RESULTS

Main Estimation Results and Heterogeneous Effects

Main Estimation Results

Panels A, B and C of Table 3 show the estimates of the OLS and IV regression for both experimental groups. We discuss here the main findings.

For experimental group 0, whose mail address was not previously registered at DUO, we find overall negative but insignificant effect estimates on enrolment[14], loan and loan amount, and positive but small estimates for the effects on supplementary grant and supplementary grant amount. Only the

[13] Older students may have more experience with student finance.
[14] Enrolment in higher education (VO or HBO).

effect of the opt-in treatment, increases the likelihood of taking up a supplementary grant significantly by 3.8% on average compared to the baseline estimated effect[15], which are about 185 extra students[16]. Panel B shows a highly significant but small first stage. Although the analysis of the opening rates of the mails in Appendix E show high rates for the opt-in group, the effect is limited as the absolute number of students is modest[17]. The F value of excluded instruments, which is a measure of the instruments' strength, is in all cases well above the proposed cutoff value of 10 (Staiger and Stock, 1997). As expected, the IV estimation yields similar effects, but larger in size. Now however, we find significant positive coefficients for the information in the three mails on the take-up of a supplementary grant of about 23%-points on average compared to the control group. However, due to smaller samples, the standard errors in the IV regressions tend to be larger.

Table 3: Main Estimation Results[18]

Experimental Group 0 (Mail address not previously registered at DUO)

(A) OLS

	(1) Enrolment	(2) Loan	(3) Loan Amount	(4) Supplementary Grant	(5) Supplementary Grant Amount
Standard letter	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)
Adjusted letter	-.0043	-.0044	-4.560	.0038	.999
	(.0036)	(.0036)	(2.981)	(.0027)	(.830)
Opt-in	-.0025	-.0005	-1.473	.0056**	1.279
	(.0034)	(.0035)	(2.772)	(.0027)	(.841)
Baseline mean	.6480	.6321	501.57	.1473	41.25
N	98964	98964	98964	98964	98964

(B) IV First Stage, Opt-In

	(1) Mail1	(2) Mail3	(3) Mail5
Opt-In vs. Standard Letter	.0232***	.0261***	.0248***
	(.0009)	(.001)	(.0009)
F value excluded instrument	600.74	701.72	707.03
N	65963	65963	65963

[15] I calculated this by dividing the estimated coefficient by the baseline mean (.0056/.1473 = .0380).

[16] (.0056*98964/3 = 184.7)

[17] In experimental group 0, 33,166 students were assigned to the opt-in treatment group, of these, only 2451 ever actually opted-in for the mailing service. 1089 students received the first mail, and only 769 opened the first mail. Similar numbers are found for mail 3 and 5.

[18] Enrolment in higher education, loan and supplementary grant take-up are measured in percentages, loan amount and supplementary grant amount are measured in euros.

(C) IV Second Stage, Opt-In

	(1) Enrolment	(2) Loan	(3) Loan Amount	(4) Supplementary Grant	(5) Supplementary Grant Amount
Mail 1	-.106	-.0233	-63.90	.243**	55.51
	(0.148)	(.152)	(119.6)	(.119)	(36.38)
Mail 3	-.0939	-.0208	-56.82	.216**	49.35
	(.131)	(.135)	(106.3)	(.105)	(32.25)
Mail 5	-.0987	-.0218	-59.71	.227**	51.87
	(.138)	(.142)	(111.7)	(.111)	(33.90)
N	65963	65963	65963	65963	65963

Clustered standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

For experimental group 1, whose mail address is previously registered at DUO, we find similar results; in general negative, small and insignificant effect estimates on enrolment, loan and loan amount, and mostly positive effects but insignificant effect estimates on supplementary grant and supplementary grant amount. When we look at the first stage in Panel B, we find again a weak relationship for the opt-in treatment, but strong estimates for the opt-out intervention. This implies that the coefficients in the second stage are smaller for the latter treatments. In this experimental group, we do not find remarkable effects of the intervention.

Experimental Group 1 (Mail address previously registered at DUO)

(A) OLS

	(1) Enrolment	(2) Loan	(3) Loan Amount	(4) Supplementary Grant	(5) Supplementary Grant Amount
Standard letter	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)
Adjusted letter	-.0030	-.0058	-4.640	-.0002	-.891
	(.0049)	(.0053)	(3.781)	(.0039)	(1.080)
Opt-in	-.0019	-.0005	.827	.0004	-.740
	(.0057)	(.0053)	(4.068)	(.0037)	(1.173)
Opt-out	-.0046	-.0024	-3.853	.0018	.670
	(.0047)	(.004)	(3.204)	(.0039)	(1.358)
Baseline mean	.3651	.4348	297.29	.1844	53.89
N	71665	71665	71665	71665	71665

(B) IV First Stage

	(1) Mail1	(2) Mail3	(3) Mail5
Opt-in vs. Standard Letter	.0115***	.0137***	.0126***
	(.001)	(.001)	(.001)
F value excluded instrument	146.65	161.80	173.19
Opt-out vs. Standard Letter	.478***	.633***	.600***
	(.004)	(.004)	(.004)
F value excluded instrument	14599.89	20790.76	22906.82
Opt-in vs. Opt-out	-.467***	-.619***	-.588***
	(.004)	(.004)	(.004)
F value excluded instrument	16327.73	21246.06	21862.58
N	35834	35834	35834

(C) IV Second Stage

	(1) Enrolment	(2) Loan	(3) Loan Amount	(4) Supplementary Grant	(5) Supplementary Grant Amount
Opt-in vs. Standard Letter					
Mail 1	-.164	-.0419	75.18	.0354	.0354
	(.499)	(.463)	(353.4)	(.323)	(.323)
Mail 3	-.138	-.0351	63.01	.0296	.0296
	(.418)	(.388)	(296.2)	(.271)	(.271)
Mail 5	-.151	-.0384	68.86	.0324	.0324
	(.457)	(.424)	(323.7)	(.296)	(.296)
Opt-out vs. Standard Letter					
Mail 1	-.0094	-.0048	-7.923	.0037	.0037
	(.0099)	(.0084)	(6.699)	(.0081)	(.0081)
Mail 3	-.0071	-.0036	-5.989	.0028	.0028
	(.0075)	(.0063)	(5.057)	(.0061)	(.0061)
Mail 5	-.0075	-.0038	-6.317	.0029	.0029
	(.0079)	(.0067)	(5.340)	(.0064)	(.0064)
Opt-in vs. Opt-out					
Mail 1	-.0055	-.0039	-9.945	.0029	.0029
	(.0108)	(.0101)	(8.372)	(.0093)	(.0093)
Mail 3	-.0042	-.0029	-7.502	.0022	.0022
	(.0081)	(.0076)	(6.306)	(.007)	(.007)
Mail 5	-.0044	-.0031	-7.903	.0023	.0023
	(.0086)	(.008)	(6.649)	(.0074)	(.0074)
N	35834	35834	35834	35834	35834

Clustered standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Heterogeneous Effects - Sensitivity Analysis

Table 4 shows the OLS sensitivity analysis for different subsamples based on education type and SES quantile[19]. For experimental group 0, we find the following. The overall effect on enrolment is negative for the adjusted letter and positive for the opt-in intervention. The only significant effects are for HAVO and the poorest (SES1) students, however, these are small in size. For loan and loan amount, we see a reflection of the enrolment effects[20]. We discuss possible explanations in the next section. For supplementary grant and supplementary grant amount, we find overall positive effects, significant for the second richest SES quantile (between 7 and 10% on average compared to the baseline) and for some MBO4 and HAVO students[21]. Additionally, the effects are stronger for students who already had a supplementary grant in the previous year (SG16).

Finally, for experimental group 1, we draw similar conclusions for enrolment, loan and loan amount. What stands out, are the significant negative effects of these interventions for VWO and SES1 students, which at first sight seems counterintuitive. However, this could be due to a smaller sample size in this experimental group, or could indeed point to a more conscious education decision[22]. For supplementary grant and supplementary grant amount, we find significant negative effects of the opt-out intervention on HAVO students, and of all interventions on SES4 students. The coefficients vary between 10 and 19% on average compared to the baseline. As seen from the descriptive statistics, the proportion of VWO and HAVO students in experimental group 1 is relatively small.

[19] Where SES1 stands for the bottom quantile and SES4 the top quantile.

[20] As a robustness analysis, regressing the outcome variables conditional on enrolment in higher education (WO or HBO), mitigates these negative effects to some extent. We can conclude that the negative borrowing effects are partly due to a decrease of enrolment in higher education.

[21] As we can see from Table G.2 in Appendix G, MBO-4 students are more often from lower SES background.

[22] In experimental group 1, only 0.29% of VWO students went to MBO, and 15% did not pass the high school exam. Further research is needed to understand what these students did if they did not continue to higher education. They could have taken a gap year, started working or stayed in high school. However, we emphasize that this is a small sample of students.

Table 4: Heterogeneous Effects

Experimental Group 0 (Mail address not previously registered at DUO)

(A) OLS

	(1) MBO4	(2) HAVO	(3) VWO	(4) VAVO	(5) SES1	(6) SES2	(7) SES3	(8) SES4
Enrolment								
Standard letter	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Adjusted letter	-.0060	-.0044	-.0012	-.0151	-.0083	.0039	-.0029	-.0095
	(.0126)	(.0052)	(.0059)	(.0139)	(.0087)	(.007)	(.0072)	(.0066)
Opt-in	.0067	-.0111**	.0056	.0215	-.0154*	.0103	-.0014	-.0048
	(.0134)	(.0053)	(.0052)	(.0146)	(.0080)	(.0069)	(.0067)	(.0067)
Baseline mean	.2425	.6574	.7545	.4163	.6248	.6494	.6575	.6559
Loan								
Standard letter	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Adjusted letter	-.0017	-.0039	-.0033	-.0148	-.0068	.0065	-.0031	-.0130*
	(.0117)	(.0053)	(.0058)	(.0163)	(.0086)	(.0071)	(.0072)	(.0067)
Opt-in	.0051	-.0092*	.0083	.0229	-.0144*	.0127*	-.0003	-.0018
	(.0140)	(.0053)	(.0055)	(.0171)	(.0085)	(.0072)	(.0069)	(.0066)
Baseline mean	.2729	.6318	.7401	.4233	.6166	.6381	.6436	.6284
Loan Amount								
Standard letter	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Adjusted letter	-3.944	-4.892	-4.192	-.4519	-6.144	4.471	-5.389	-10.11*
	(8.449)	(4.341)	(4.953)	(12.88)	(6.702)	(5.748)	(5.989)	(5.628)
Opt-in	-1.267	-8.691**	5.91	24.14*	-14.52**	10.33*	-3.117	-.4365
	(10.22)	(4.289)	(4.621)	(12.32)	(6.479)	(5.933)	(5.68)	(5.422)
Baseline mean	187.53	496.20	605.13	310.48	460.80	503.45	518.69	514.87
N	7192	52287	34478	5007	20703	23906	25622	28733

Clustered standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

	(1) MBO4	(2) HAVO	(3) VWO	(4) VAVO	(5) SES1	(6) SES2	(7) SES3	(8) SES4	(9) No SG16	(10) SG16
Supplementary Grant										
Standard letter	0	0	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Adjusted letter	.0096	.0067*	.0051	-.0377***	.0048	-.0009	.0096*	.0018	.0032	.0562*
	(.0084)	(.0036)	(.0042)	(.0103)	(.0073)	(.0058)	(.0051)	(.0045)	(.0027)	(.0303)
Opt-in	.0198**	.0048	.0069	-.0132	.0046	.0032	.0123**	.0025	.0048*	.0528*
	(.0095)	(.0039)	(.0046)	(.0126)	(.0072)	(.0058)	(.0050)	(.0043)	(.0028)	(.0311)
Baseline mean	.1116	.1663	.1272	.1386	.2246	.1650	.1293	.0921	.1434	.3923
Supplementary Grant Amount										
Standard letter	0	0	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Adjusted letter	4.048	1.491	1.807	-12.36***	.3427	.3212	3.478**	-.229	.8075	16.27
	(2.573)	(1.123)	(1.315)	(3.755)	(2.411)	(1.778)	(1.545)	(1.307)	(.8435)	(10.84)
Opt-in	7.14***	.5689	1.897	-3.366	1.664	.3116	3.24**	.0571	.9826	19.22*
	(2.704)	(1.224)	(1.342)	(4.282)	(2.316)	(1.698)	(1.546)	(1.231)	(.8487)	(9.852)
Baseline mean	30.89	46.85	34.71	42.15	66.85	44.97	34.43	25.50	40.07	115.34
N	7192	52287	34478	5007	20703	23906	25622	28733	97493	1477

Clustered standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Experimental Group 1 (Mail address previously registered at DUO)

(A) OLS

	(1) MBO4	(2) HAVO	(3) VWO	(4) VAVO	(5) SES1	(6) SES2	(7) SES3	(8) SES4
Enrolment								
Standard letter	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Adjusted letter	.0001	-.0174	-.0522***	.0065	-.0017	-.0048	.0052	-.0117
	(.0048)	(.0266)	(.0202)	(.0244)	(.009)	(.0092)	(.0097)	(.0128)
Opt-in	-.0022	.0123	-.0474**	.0230	-.0125	.0053	.0133	-.0134
	(.0060)	(.0259)	(.0197)	(.0209)	(.0093)	(.0104)	(.0092)	(.0133)
Opt-out	-.0053	-.0169	-.0096	.0106	-.0127	.0026	.0032	-.0111
	(.0052)	(.0258)	(.0204)	(.0165)	(.0081)	(.0089)	(.0096)	(.0119)
Baseline mean	.3140	.6534	.7806	.4763	.3419	.3582	.3616	.4149
Loan								
Standard letter	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Adjusted letter	-.0025	-.0323	-.0479**	.0030	-.0128	-.0061	.0080	-.0125
	(.0057)	(.0247)	(.02)	(.0217)	(.0092)	(.0095)	(.0097)	(.0133)
Opt-in	-.0001	.0133	-.0406**	.0152	-.0129	.0065	.0125	-.0082
	(.0056)	(.0245)	(.0199)	(.0197)	(.0107)	(.0102)	(.0103)	(.0132)
Opt-out	-.0021	-.0289	-.0094	.0010	-.0090	.0024	.0075	-.0117
	(.0045)	(.0254)	(.0199)	(.0131)	(.0101)	(.0089)	(.0097)	(.0107)
Baseline mean	.3949	.6779	.7731	.5071	.4503	.4223	.4133	.4541
Loan Amount								
Standard letter	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Adjusted letter	-3.344	-20.02	-34.55**	9.374	-12.51*	-5.356	5.282	-2.064
	(4.194)	(19.47)	(16.32)	(15.94)	(7.178)	(6.388)	(6.736)	(10.12)
Opt-in	1.229	11.71	-37.52**	17.43	-8.809	4.283	12.03	-2.973
	(4.423)	(19.71)	(16.38)	(13.44)	(7.289)	(7.907)	(7.546)	(10.56)
Opt-out	-4.753	-16.31	-2.104	8.515	-11.38	-1.686	3.224	-3.437
	(3.44)	(19.62)	(16.9)	(10.75)	(7.049)	(6.427)	(7.154)	(8.77)
Baseline mean	263.99	485.13	599.88	351.99	283.87	291.17	295.35	328.98
N	58947	2463	3720	6535	22074	18671	17152	13768

Clustered standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

	(1) MBO4	(2) HAVO	(3) VWO	(4) VAVO	(5) SES1	(6) SES2	(7) SES3	(8) SES4	(9) No SG16	(10) SG16
Supplementary Grant										
Standard letter	0	0	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Adjusted letter	.0008	-.0052	-.0005	-.0089	.0033	.0017	.0075	-.0215***	-.0000	-.0023
	(.0039)	(.0245)	(.0192)	(.0181)	(.0066)	(.007567)	(.0074)	(.0073)	(.0038)	(.0091)
Opt-in	-.0004	.0064	.0102	.0009	-.0029	.0064	.0105	-.0151*	.0024	-.0044
	(.0035)	(.0244)	(.0192)	(.0167)	(.007858)	(.0072)	(.0074)	(.0079)	(.0033)	(.0081)
Opt-out	.0047	-.0485*	-.0044	-.004618	.0000	.0105	.0072	-.0157**	-.0006	-.0061
	(.0041)	(.0255)	(.0183)	(.01356)	(.0081)	(.0079)	(.0071)	(.0073)	(.0026)	(.0104)
Baseline mean	.1752	.2914	.2226	.2034	.2426	.1773	.1445	.1525	.0868	24.27
Supplementary Grant Amount										
Standard letter	0	0	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Adjusted letter	-.0760	-4.313	-6.335	-4.471	.5226	-.7255	1.577	-7.591***	-.8342	-1.572
	(1.079)	(8.099)	(6.042)	(5.482)	(2.171)	(2.272)	(2.307)	(2.381)	(1.075)	(2.692)
Opt-in	-.7337	-5.01	5.34	-2.491	-1.1	2.06	.7916	-5.973***	.1104	-2.867
	(1.13)	(7.695)	(6.376)	(4.766)	(2.459)	(2.204)	(2.301)	(2.228)	(.9563)	(2.608)
Opt-out	1.89	-16.47**	-4.269	-1.737	1.311	2.607	2.791	-6.285**	-.2761	2.438
	(1.421)	(8.354)	(5.813)	(4.293)	(2.744)	(2.556)	(2.642)	(2.552)	(.8606)	(3.644)
Baseline mean	50.56	88.65	66.64	62.92	73.26	50.06	40.82	44.96	.4053	120.92
N	58947	2463	3720	6535	22074	18671	17152	13768	49569	22096

Clustered standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

VI. CONCLUSION AND DISCUSSION

This thesis evaluated the 'Bewuste Studiekeuze' pilot program by the Dutch Ministry of Education, Culture and Science. The aim of this policy was to increase the efficiency of information provision to last-year high school students in The Netherlands, with respect to pursuing higher education, and the financial aspect of this. This is done by adjusting the timing and presentation of the information, and emphasizing the affordability and returns to higher education. We examined different types of treatment (mails, letters), with special interest for their impact on different groups based on previous education and socioeconomic status. The largest effects were found in the take up of a supplementary grant, and this among groups that could be expected to suffer most from biased or limited information. The main findings of this thesis are the following.

First, when we compare the opt-in with the opt-out intervention, we do see a significant difference in the share of students that opened and received the mailing service treatment, which is larger for the opt-out group. This is as suggested by the literature. Automatically targeting students seems to be a more efficient policy measure to provide the treatment, than expecting them to take action. Again, this is linked to the behavioral constraint students already face.

Second, if any, we do not see a significant increase in enrolment. For some small groups of VWO and HAVO students we even see negative significant effects, which at first sight seems counterintuitive. In some cases, we see that the adjusted letter causes (stronger) negative effects, compared to the other interventions. Furthermore, these negative enrolment effects, could be driving the decrease in loan take-up and amount. For the groups at risk, that where of special interest for this intervention (MBO and low-SES students), we see that these are not significantly affected by the intervention. About the reasons for the ineffectiveness of the policy on higher education enrolment, we can only speculate. Whether this is caused by the timing of the intervention, the content or form, is something that should be further investigated. From the literature, we could expect that an information-only treatment is not enough, and more personal assistance could improve the efficiency of the policy intervention. One thing that is important to keep in mind however, is that a conscious decision on higher education does not imply that higher education is the preferred option for everyone. As such, a decrease in enrolment rates could be the outcome of a more well-thought decision process.

Third, in general we do see positive effects on supplementary grant, which are the strongest among some groups of MBO-4, HAVO and SES3 students. MBO-4 and HAVO students tend to be more from lower SES background compared to VWO. For SES3 students, we could expect them to doubt

their eligibility, as they are closer to the median of the income distribution. For these students, the treatment could have provided new information on their eligibility for a supplementary grant, which they did not previously obtained through their peers or family.

We can conclude that although the policy often yielded insignificant and, in some cases, even counterintuitive results, this could be informative about the behavioral and informational biases and limitation Dutch high school students face when making decisions on higher education. More research is needed however, to understand the underlying mechanisms that drive these outcomes, and existing literature could guide when considering policy alternatives. Finally, it is important to acknowledge that students from different backgrounds might have different reasons for behaving the way they do. Targeted policies are thus necessary, when policy makers know which specific problems they want to solve.

APPENDIX

Appendix A: Interventions and Treatment Groups (letters, mails, timing)

Standard Letter

Wetenschap

Hoger Onderwijs en Studiefinanciering
Rijnstraat 50
Den Haag
Postbus 16375
2500 BJ Den Haag
www.rijksoverheid.nl

Onze referentie
10722257

Datum
Betreft Volgend jaar studeren aan een hogeschool of universiteit?

Beste studiekiezer,

Ga je volgend jaar studeren aan een hogeschool of universiteit of denk je daar over na? Lees deze brief dan goed. Er staat belangrijke informatie in over je studiekeuze, aanmelding bij het hoger onderwijs en studiefinanciering.

Kies zorgvuldig
Oriënteer je goed op je studie. Spreek met je vakdocenten, decaan, mentor of studieloopbaanbegeleider en ouders. Bekijk de websites van de hogescholen en universiteiten, bezoek open dagen, ga proefstuderen en stel vragen (bij voorkeur aan studenten).
Objectieve informatie over open dagen, opleidingen, kansen op de arbeidsmarkt, oordelen van studenten in het hoger onderwijs en nog veel meer vind je op www.studiekeuze123.nl. Op www.studiemeteenplan.nl staat praktische informatie over de overstap naar een hogeschool of universiteit. Hier kun je 'mijn studieplan' aanmaken. Dit is een checklist voor wat je moet regelen als je gaat studeren.

Meld je op tijd aan: uiterlijk op 15 januari (selectie) of 1 mei 2017
Keuze gemaakt? Meld je dan aan via www.studielink.nl. Doe dit zo snel mogelijk, maar uiterlijk op 1 mei 2017, zelfs al heb je nog geen diploma. Een hogeschool of universiteit hoeft je niet toe te laten als je je na die datum pas voor het eerst aanmeldt.
Let op: voor opleidingen die studenten selecteren, moet je je vaak al uiterlijk op **15 januari 2017** aanmelden. Kijk daarom nu alvast op de website van de hogeschool of universiteit wat de uiterste aanmelddatum is.

DigiD
Voor de aanmelding bij Studielink heb je een DigiD nodig. Die moet je aanvragen via www.digid.nl. De DigiD heb je ongeveer een week na aanvraag in huis.

Verplichte studiekeuzecheck
Na je tijdige aanmelding op uiterlijk 1 mei heb je recht op een studiekeuzecheck en op een advies door de opleiding. Hoe die check eruit ziet, verschilt per opleiding. Het kan een gesprek zijn, proefcolleges, een digitale vragenlijst of iets anders. Hogescholen en universiteiten kunnen je verplichten om mee te doen aan de studiekeuzecheck. Als je weigert, hoeft de instelling je niet toe te laten tot de opleiding. Vraag daarom of deelname verplicht is en wat de gevolgen zijn als je niet meedoet. De opleiding mag je niet afwijzen als je hebt meegedaan aan de studiekeuzecheck, behalve als je je te laat hebt aangemeld.
Als de opleiding je geen studiekeuzecheck aanbiedt, dan kun je er zelf om vragen. Je hebt er namelijk recht op. Het advies is niet bindend, tenzij je je te laat hebt aangemeld. Alleen opleidingen die selecteren, bieden geen studiekeuzecheck aan. Wil je na de studiekeuzecheck toch een andere opleiding kiezen, dan kan dat. Omdat je je op uiterlijk 1 mei in Studielink hebt aangemeld, behoud je het recht om te worden toegelaten tot andere opleidingen. Je hoeft je dus niet voor de zekerheid voor meerdere opleidingen tegelijk aan te melden.

Extra toelatingseisen
Kijk op de website van de opleiding of op www.studiekeuze123.nl (het tabje "toelating en kosten" van de opleiding) of er extra toelatingseisen gelden, zoals een profiel, bepaalde vakken (havo en vwo) of een opleiding in een bepaald domein (mbo). Zo geeft een hbo-propedeusediploma niet langer meer automatisch recht op toelating tot de universiteit. Sommige opleidingen zoals Dans en Sportkunde, stellen ook extra toelatingseisen. Deze eisen kunnen verschillen en kunnen bijvoorbeeld betrekking hebben op je talent of kunnen bijvoorbeeld een medische keuring betreffen. Ook de pabo stelt extra eisen. Kijk tijdig op www.goedvoorbereidnaardepabo.nl welke eisen worden gesteld en hoe je je daarop kunt voorbereiden.

Geen loting maar decentrale selectie
Voor opleidingen met een maximum aantal plaatsen (numerus fixus) zoals bijvoorbeeld Geneeskunde, wordt niet meer geloot. Deze opleidingen selecteren nu zelf de studenten via decentrale selectie. Om aan de decentrale selectie te kunnen meedoen, moet je je uiterlijk op 15 januari 2017 aanmelden op www.studielink.nl. Kijk voor meer informatie op startstuderen.nl.
Aan de deelname aan de selectie voor een opleiding met een maximum aantal plaatsen zijn geen kosten verbonden, met uitzondering van de opleidingen Geneeskunde, Tandheelkunde, Mondzorgkunde en Fysiotherapie. Het is mogelijk dat je voor deze opleidingen een (kleine) eigen bijdrage moet betalen.

Functiebeperking
Heb je een functiebeperking of chronische ziekte? Neem dan zo snel mogelijk contact op met de opleiding van je keuze en vraag welke voorzieningen er zijn voor jou. Op www.handicap-studie.nl staat een overzicht van regelingen die voor jou nuttig kunnen zijn. Op deze site staan ook de contactpersonen bij de universiteiten en hogescholen die zich bezighouden met studeren met een beperking (onder "informatievoorziening en voorlichting").

Studiefinanciering aanvragen
Ga je volgend jaar studeren aan een hogeschool of universiteit?
Ook als je nog geen 18 bent bij aanvang van de opleiding, heb je vanaf volgend studiejaar (2017/2018) vanaf de eerste maand van je studie al recht op studiefinanciering. Zorg er wel voor dat je minimaal 3 maanden van tevoren studiefinanciering aanvraagt. Kijk voor meer informatie op www.duo.nl.
Je studiefinanciering regel je online via mijn DUO. De studiefinanciering voor opleidingen aan een hogeschool of universiteit bestaat uit een eventuele lening.

Pagina 1 van 3

Onze referentie
10722257

een aanvullende beurs, het studentenreisproduct en het collegegeldkrediet. De lening, het reisproduct en het collegegeldkrediet zijn voor iedereen. De aanvullende beurs is afhankelijk van het inkomen van je ouders.

Een lening bij DUO sluit je af tegen gunstige voorwaarden en een voordelige rente. Je bepaalt zelf of en welk bedrag je wilt lenen en als je later niet zoveel verdient, hoef je ook niet zoveel terug te betalen. Zolang je later minder dan het minimumloon verdient, hoef je zelfs helemaal niet terug te betalen.

Wil je weten hoeveel jij maandelijks nodig hebt als je gaat studeren, en welk bedrag je dan zou lenen? De rekenhulp op www.duo.nl kan je daar bij helpen.

Ga je volgend jaar een mbo-opleiding volgen en is deze opleiding een voltijdse beroeps opleidende leerweg (bol)? Dan kun je in aanmerking komen voor studiefinanciering als je 18 jaar of ouder bent. In het mbo bestaat de studiefinanciering uit een basisbeurs, een aanvullende beurs, een studentenreisproduct (ov-kaart) en eventueel een lening. Ook hier geldt dat de aanvullende beurs afhankelijk is van het inkomen van je ouders.

Rekentoets en centraal examen rekenen
Voordat je gaat studeren, moet je eerst nog je examen halen. De rekentoets is onderdeel van het examen. Iedere leerling in het voortgezet onderwijs en middelbaar beroepsonderwijs maakt deze rekentoets, maar nog niet voor iedereen telt de toets mee voor het behalen van het diploma. Kijk op www.rijksoverheid.nl/rekentoets om te zien wat er precies voor jou geldt.

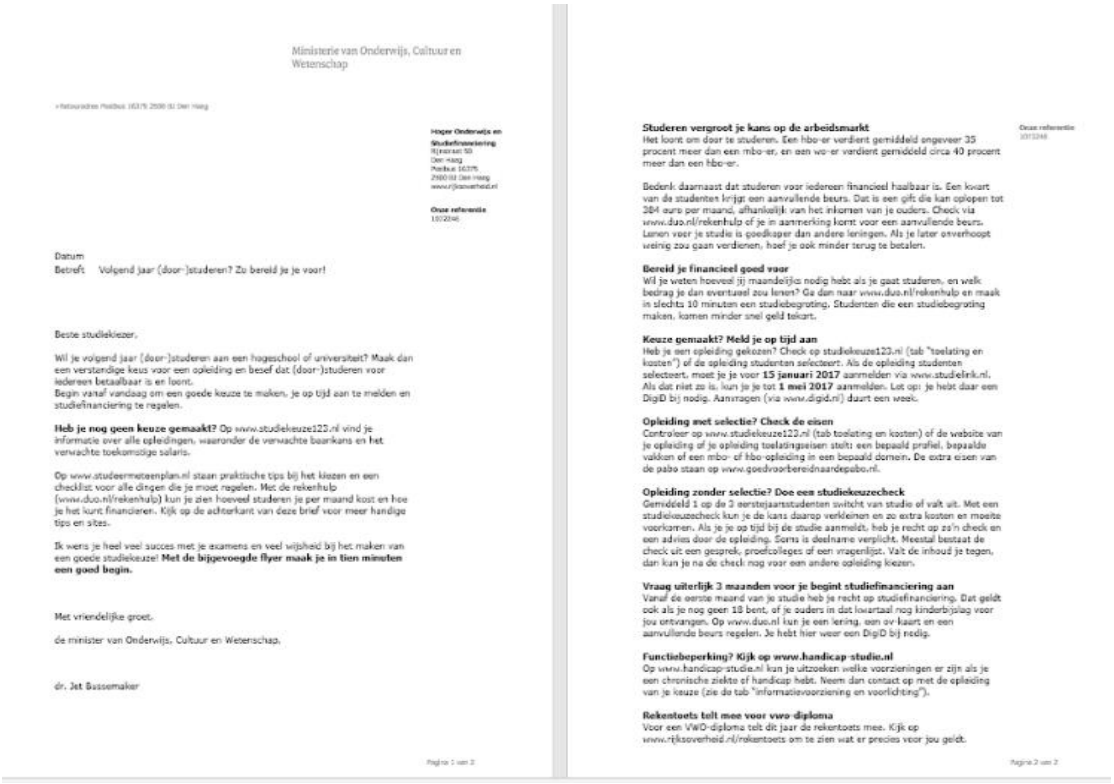
Ik wens je heel veel succes met je eindexamen en veel wijsheid bij het maken van een goede studiekeuze!

Met vriendelijke groet,

de minister van Onderwijs, Cultuur en Wetenschap,

dr. Jet Bussemaker

Treatment 1: Adjusted Letter



Ministerie van Onderwijs, Cultuur en Wetenschap

Hoger Onderwijs en Studiefinanciering
Rijnstraat 50
Den Haag
Postbus 16375
2500 BJ Den Haag
www.rijksoverheid.nl

Onze referentie
10123048

Datum
Betreft Volgend jaar (door-)studeren? Zo bereid je je voor!

Beste studiekiezer,

Wil je volgend jaar (door-)studeren aan een hogeschool of universiteit? Maak dan een verstandige keus voor een opleiding en bezef dat (door-)studeren voor iedereen betaalbaar is en loont.
Begin vanaf vandaag om een goede keuze te maken, je op tijd aan te melden en studiefinanciering te regelen.

Heb je nog geen keuze gemaakt? Op www.studiekeuze123.nl vind je informatie over alle opleidingen, waaronder de verwachte baarkans en het verwachte toekomstige salaris.

Op www.studeermeteenplan.nl staan praktische tips bij het kiezen en een checklist voor alle dingen die je moet regelen. Met de rekenhulp (www.duo.nl/rekenhulp) kun je zien hoeveel studeren je per maand kost en hoe je het kunt financieren. Kijk op de achterkant van deze brief voor meer handige tips en sites.

Ik wens je heel veel succes met je examens en veel wijsheid bij het maken van een goede studiekeuze! **Met de bijgevoegde flyer maak je in tien minuten een goed begin.**

Met vriendelijke groet,

de minister van Onderwijs, Cultuur en Wetenschap,

dr. Jet Bussemaker

Pagina 1 van 2

Studeren vergroot je kans op de arbeidsmarkt
Het loont om door te studeren. Een hbo-er verdient gemiddeld ongeveer 35 procent meer dan een mbo-er, en een wo-er verdient gemiddeld circa 40 procent meer dan een hbo-er.

Bedenk daarnaast dat studeren voor iedereen financieel haalbaar is. Een kwart van de studenten krijgt een aanvullende beurs. Dat is een gift die kan oplopen tot 384 euro per maand, afhankelijk van het inkomen van je ouders. Check via www.duo.nl/rekenhulp of je in aanmerking komt voor een aanvullende beurs. Lenen voor je studie is goedkoper dan andere leningen. Als je later onverhoopt weinig zou gaan verdienen, hoef je ook minder terug te betalen.

Bereid je financieel goed voor
Wil je weten hoeveel jij maandelijks nodig hebt als je gaat studeren, en welk bedrag je dan eventueel zou lenen? Ga dan naar www.duo.nl/rekenhulp en maak in slechts 10 minuten een studiebegroting. Studenten die een studiebegroting maken, komen minder snel geld tekort.

Keuze gemaakt? Meld je op tijd aan
Heb je een opleiding gekozen? Check op studiekeuze123.nl (tab "toelating en kosten") of de opleiding studenten selecteert. Als de opleiding studenten selecteert, moet je je voor **15 januari 2017** aanmelden via www.studielink.nl. Als dat niet zo is, kun je je tot **1 mei 2017** aanmelden. Let op: je hebt daar een DigiD bij nodig. Aanvragen (via www.digid.nl) duurt een week.

Opleiding met selectie? Check de eisen
Controleer op www.studiekeuze123.nl (tab toelating en kosten) of de website van je opleiding of je opleiding toelatingseisen stelt: een bepaald profiel, bepaalde vakken of een mbo- of hbo-opleiding in een bepaald domein. De extra eisen van de pabo staan op www.goedvoorbereidnaardepabo.nl.

Opleiding zonder selectie? Doe een studiekeuzecheck
Gemiddeld 1 op de 3 eerstejaarsstudenten switcht van studie of valt uit. Met een studiekeuzecheck kun je de kans daarop verkleinen en zo extra kosten en moeite voorkomen. Als je je op tijd bij de studie aanmeldt, heb je recht op zo'n check en een advies door de opleiding. Soms is deelname verplicht. Meestal bestaat de check uit een gesprek, proefcolleges of een vragenlijst. Valt de inhoud je tegen, dan kun je na de check nog voor een andere opleiding kiezen.

Vraag uiterlijk 3 maanden voor je begint studiefinanciering aan
Vanaf de eerste maand van je studie heb je recht op studiefinanciering. Dat geldt ook als je nog geen 18 bent, of je ouders in dat kwartaal nog kinderbijslag voor jou ontvangen. Op www.duo.nl kun je een lening, een ov-kaart en een aanvullende beurs regelen. Je hebt hier weer een DigiD bij nodig.

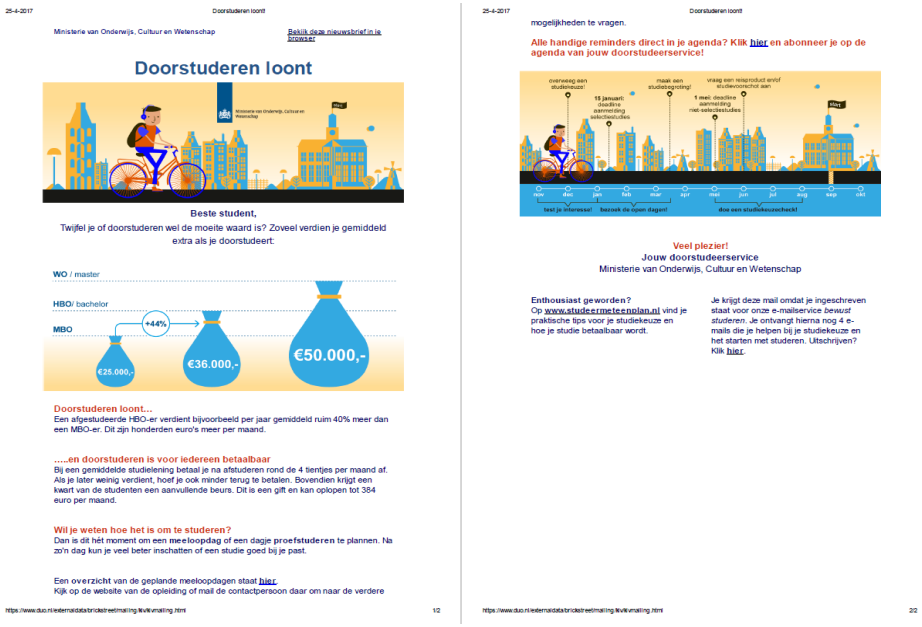
Functiebeperking? Kijk op www.handicap-studie.nl
Op www.handicap-studie.nl kun je uitzoeken welke voorzieningen er zijn als je een chronische ziekte of handicap hebt. Neem dan contact op met de opleiding van je keuze (zie de tab "informatievoorziening en voorlichting").

Rekentoets telt mee voor vwo-diploma
Voor een VWO-diploma telt dit jaar de rekentoets mee. Kijk op www.rijksoverheid.nl/rekentoets om te zien wat er precies voor jou geldt.

Pagina 2 van 2

Treatment 2 and 3: Mailing Service

Mail 1



25-4-2017 Doorstuderen loont

Ministerie van Onderwijs, Cultuur en Wetenschap — Bekijk deze nieuwsbrief in je browser

Doorstuderen loont

Beste student,
Twijfel je of doorstuderen wel de moeite waard is? Zoveel verdien je gemiddeld extra als je doorstudeert:

Doorstuderen loont...
Een afgestudeerde HBO-er verdient bijvoorbeeld per jaar gemiddeld ruim 40% meer dan een MBO-er. Dit zijn honderden euro's meer per maand.

.....en doorstuderen is voor iedereen betaalbaar
Bij een gemiddelde studielening betaal je na afstuderen rond de 4 tientjes per maand af. Als je later weinig verdient, hoef je ook minder terug te betalen. Bovendien krijgt een kwart van de studenten een aanvullende beurs. Dit is een gift en kan oplopen tot 384 euro per maand.

Wil je weten hoe het is om te studeren?
Dan is dit hét moment om een **meeloopdag** of een dagje **proefstuderen** te plannen. Na zo'n dag kun je veel beter inschatten of een studie goed bij je past.

Een overzicht van de geplande meeloopdagen staat hier.
Kijk op de website van de opleiding of mail de contactpersoon daar om naar de verdere

https://www.duo.nl/ederaldatabrickstreetmailing.NvWvmailing.html 1/2

25-4-2017 Doorstuderen loont

mogelijkheden te vragen.

Alle handige reminders direct in je agenda? Klik hier en abonneer je op de agenda van jouw doorstudeerservice!

Veel plezier!
Jouw doorstudeerservice
Ministerie van Onderwijs, Cultuur en Wetenschap

Enthousiast geworden?
Op www.studeermeteenplan.nl vind je praktische tips voor je studiekeuze en hoe je studie betaalbaar wordt.

Je krijgt deze mail omdat je ingeschreven staat voor onze e-mailservice *bewust studeren*. Je ontvangt hierna nog 4 e-mails die je helpen bij je studiekeuze en het starten met studeren. Uitschrijven? Klik hier.

https://www.duo.nl/ederaldatabrickstreetmailing.NvWvmailing.html 2/2

Mail 2

Ministerie van Onderwijs, Cultuur en Wetenschap

Bekijk deze nieuwsbrief in je browser

Beste Student,
Heb jij enig idee hoe jouw leven er volgend jaar uitziet?
Test het met onderstaande quiz! Vraag ook je vrienden of heb het er met je ouders over. **Veel plezier!**

Doe de quiz!

Scroll naar beneden voor de antwoorden

Vraag 1.
Hoeveel contacturen zijn er per week bij jouw studie?

Vraag 2.
Hoe beoordelen de huidige studenten jouw toekomstige studie?

Vraag 3.
Hoeveel studenten doen jouw studie?

Vraag 4.
Hoeveel kroegen zijn er in de stad van je keuze?

Vraag 5.
Wat is het gemiddelde startsalaris als je jouw studie hebt voltooid?

Antwoord vraag 1: Het aantal contacturen varieert tussen de 9 en 24 uur per week. In het HBO is het gemiddelde 16 contacturen, in het WO 14. **Kijk hier voor het aantal contacturen voor jouw studie**.

Antwoord vraag 2: Geen enkele studie scoort, op een schaal van 1 tot en met 5, minder dan een 3.1. Maar er zijn grote verschillen. Zoek hier op hoe **huidige studenten jouw toekomstige studie beoordelen**.

Antwoord vraag 3: Er zijn studies met twee eerstejaars, maar ook met 1.128 eerstejaarsstudenten. Check het precieze **aantal eerstejaars voor jouw toekomstige studie**.

Antwoord vraag 4: Amsterdam heeft 1.050 kroegen, terwijl Dronten het moet doen met 9 dranklokalen. Kijk hier voor **het precieze antwoord voor de stad van jouw keuze** en vind andere nuttige informatie zoals de hoogte van de gemiddelde kamerhuur.

Antwoord vraag 5: Het gemiddelde startsalaris varieert tussen de 1.400 en 3.750 euro bruto per maand in het HBO en tussen de 1.550 en 6.025 euro in het WO. Check in de **Statistische bijlage HBO 2016 / Statistische bijlage WO 2016** Tabel 8 voor het gemiddelde startsalaris van jouw studie, maar ook bijvoorbeeld het verwachte inkomen 5 jaar na afstuderen.

Handige reminders in je agenda? Klik hier en abonneer je op de agenda van jouw doorstudeerservice!

Jouw doorstudeerservice
Ministerie van Onderwijs, Cultuur en Wetenschap

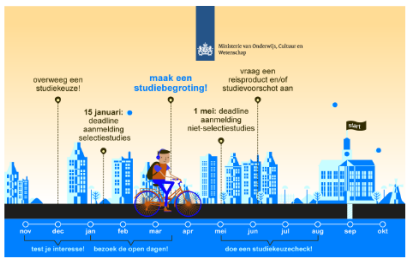
Let op! De deadline voor aanmelding van studies met toelatingseisen ligt op 15 januari. Check op studiekeuze123.nl of jouw studie een selectieve studie is en schrijf je op tijd in bij Studielink.nl!

Je krijgt deze mail omdat je ingeschreven staat voor onze e-mailservice. Je ontvangt hierna nog 2 e-mails die je helpen bij je studiekeuze en het starten met studeren. Uitschrijven? Klik hier.

Mail 3

Ministerie van Onderwijs, Cultuur en Wetenschap

Bekijk deze nieuwsbrief in je browser



Beste Student,

Wat ben je per maand kwijt als je doorstudeert? Studenten die een studiebegroting maken komen minder snel geld te kort. Maak met de rekenhulp van DUO een inschatting van je inkomsten en uitgaven. **Binnen 10 minuten, snel en gemakkelijk!**

Goed om te weten:
- Je krijgt een OV-kaart.
- 1 van de 4 studenten krijgt een aanvullende beurs (max. €388,- per maand).
- 9 van de 10 studenten krijgen zorgtoeslag (max. €83,- per maand).
- Lenen kan tegen gunstige voorwaarden (zeer lage rente en lange aflosperiode).

start de studiebegroting

Al 268.239 studenten gingen je voor!

Dus vergeet niet:
- Doorstuderen is voor iedereen betaalbaar.
- Doorstuderen loont!

Veel succes!

Jouw doorstudeerservice
Ministerie van Onderwijs, Cultuur en Wetenschap

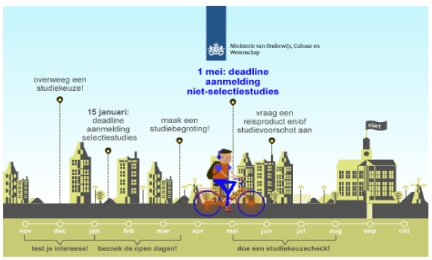
Je krijgt deze mail omdat je ingeschreven staat voor onze e-mailservice. Je ontvangt hierna nog 2 e-mails die je helpen bij je studiekeuze en het starten met studeren. Uitschrijven? Klik hier.

Mail 4

25-4-2017 Je hebt nog één week!

Ministerie van Onderwijs, Cultuur en Wetenschap

Bekijk deze nieuwsbrief in je browser



Beste Student,

Let op: **1 mei** is de deadline voor aanmelding voor een vervolgstudie in het hoger onderwijs.

Meld je hier aan op Studielink!

Het duurt maar 5 minuten!

Twijfel je of doorstuderen voor jou de goede beslissing is?
Bedenk dan:

Doorstuderen loont.

- Een afgestudeerde HBO-er verdient per jaar gemiddeld **ruim 40% meer** dan een MBO-er.
- Een afgestudeerde WO-er verdient per jaar gemiddeld **ruim 35% meer** dan een HBO-er.

https://www.duo.nl/externaldata/brickstreetmailing/NvNvmailing4online.html 1/2

25-4-2017 Je hebt nog één week!

Doorstuderen is betaalbaar voor iedereen.

- 1 op de 4 studenten heeft **recht op een aanvullende beurs**.
- Lenen kan tegen **gunstige voorwaarden** (zeer lage rente, aflostermijn van 35 jaar, geen aflosverplichting zo lang je een laag inkomen hebt).

Doorstuderen – maar welke studie?
Als je nog twijfelt, kun je je voor meerdere studies inschrijven.

Jouw doorstudeerservice
Ministerie van Onderwijs, Cultuur en Wetenschap

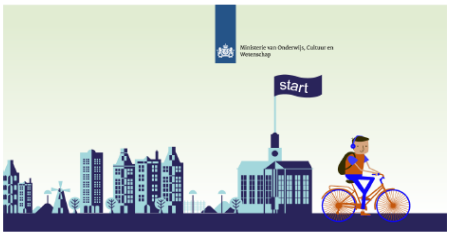
https://www.duo.nl/externaldata/brickstreetmailing/NvNvmailing4online.html 2/2

Mail 5

26-10-2017 Heb jij je financiën al op orde?

Ministerie van Onderwijs, Cultuur en Wetenschap

Bekijk deze nieuwsbrief in je browser



Beste Student,

Eindelijk is het zo ver! Je studie is van start gegaan! Ken je je studiestad en je medestudenten al een beetje? Goede manieren om mensen te leren kennen zijn via een studie-, studenten- of sportvereniging!

Het is belangrijk om je financiën goed op orde te houden. Zet je uitgaven en inkomsten (nog eens) op een rij met de rekenhulp van DUO en bereken je studiefinanciering.

start de studiebegroting

Al 266.239 studenten gingen je voor!

- Leen je te veel? Denk aan wat je later terug moet betalen. En heb je al gecheckt of je recht hebt op een aanvullende beurs? Eén op de vier studenten ontvangt deze!
- Leen je te weinig? Dan ben je misschien veel tijd kwijt aan bijbaantjes wat ten koste kan gaan van je studieprestaties.
- Leen dus bewust!

Jouw doorstudeerservice
Ministerie van Onderwijs, Cultuur en Wetenschap

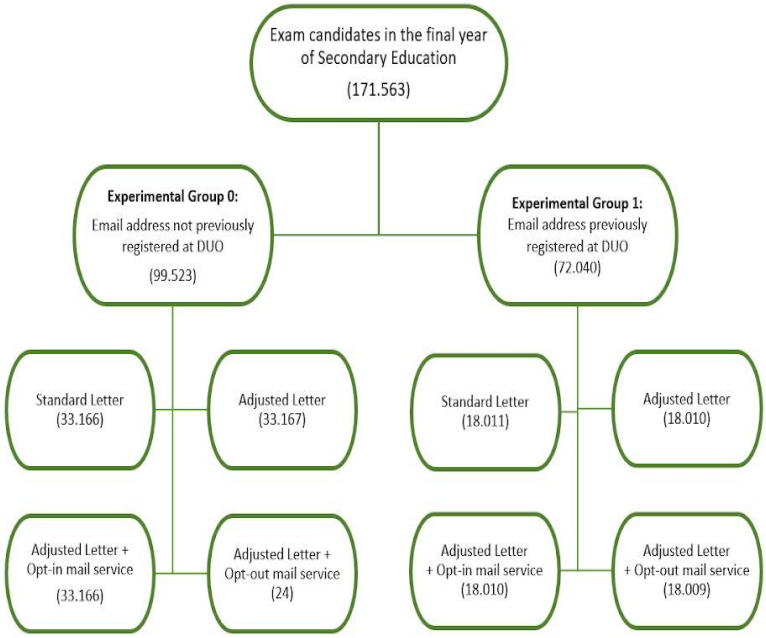
https://www.duo.nl/externaldata/brickstreetmailing/NvNvmailing5online.html

Mail 6

Table A.1: Timing of the Intervention

Mail	Subject	Date
1	Further Education pays off! (*Doorstuderen loont*!)	25 November 2016
2	Take the quiz! (*Doe de quiz!)*	2 January 2017
3	Draw up a study budget in 10 minutes! (*Maak in 10 minuten een studiebegroting!)*	7 March 2017
4	You have one week left! (*Je hebt nog één week!*)	24 April 2017
5	Do you already have your finances in order? (*Heb jij je financiën al op orde?*)	18 September 2017
6	Do you already have an appointment, [name]? (*Heb jij al een afspraak, [naam]?*)	29 November 2017

Figure A.2: Experiment Set-up and Treatment Groups



Sample size between parentheses.

Appendix B: Variables

Table B.1: List of Used Variables

Variable Name	Explanation	Remarks
Background Characteristics (As of October 2016)		
Gender	Male/Female	
Age	in years	
Type of secondary education	MBO-4, HAVO, VWO, VAVO	
Profile HAVO and VWO	Economics and Society, Culture and Society, Nature and Health, Nature and Technique	
Domain MBO-4	None; Finishing Work Wood and Maintenance; Craft, Laboratory and Health Techniques; Construction and Infrastructure, Economics and Administration; Commerce and Entrepreneurship; Catering and Bakery, Information and Communication Technology; Media and Design; Mobility and Vehicles; Technique and Process Industry; Tourism and Recreation; Transport, Shipping and Logistics; Beauty Care Security and Sports; Food, Nature and Living Environment; Care and Wellbeing	
BRIN school code	Two-digit, two-letter basic registration code of the institution	
Student's mail address was already registered at DUO	yes/no	
Student has been registered at 'My Study plan'	yes/no	
Student passed the final exam of secondary education	yes/no	
Average grade on the final exam	On a scale from 0-10	only for HAVO and VWO
Socioeconomic status score and quantile	Score standardized to mean 0, based on income, employment status, and education of the inhabitants of that postal code area.	
Pre-Treatment Student Finance Variables (For the academic year 2016/2017)		

Right to supplementary grant	amount in euros	
Right to student loan	amount in euros	
Amount of loan received	factual amount received	
Intervention/Treatment Variables (For 2016/2017)		
Type of letter/mail	standard letter, adjusted letter with nudges, opt-in mail service, opt-out mail service	
Experimental group	depending on whether the student's mail address was previously known by DUO or not	
Intermediate Outcome Variables		
Mail 1/2/3/4/5/6 sent/received/opened	yes/no	
Link 1/2/3/4 clicked	yes/no	
Outcome Variables of Interest (As of October 2017)		
Enrolled in higher education or MBO	yes/no	
Student took a student loan	yes/no	
Amount of student loan	amount in euros	
Student took a supplementary grant	yes/no	
Amount of supplementary grant	amount in euros	
Additional Outcome Variables (As of October 2017)		
Level of higher education	HBO, WO, MBO	

Component of higher education	Economics, Behavior and Society, Healthcare, Agriculture and Natural Environment, Nature, Education, Law, Cross-sectoral, Languages and Culture, Technique	
Domain MBO-4	None; Finishing Work, Wood and Maintenance; Craft, Laboratory and Health Techniques; Construction and Infrastructure, Economics and Administration; Commerce and Entrepreneurship; Catering and Bakery, Information and Communication Technology; Media and Design; Mobility and Vehicles; Technique and Process Industry; Tourism and Recreation; Transport, Shipping and Logistics; Physical Care; Security and Sports; Food, Nature and Living Environment; Care and Wellbeing.	

Appendix C: Empirical Evidence

Table C.1: Overview Empirical Evidence

Authors	Intervention	Data	Method	Results
Ford et al. (2012)	**Information** and **reminders** on Learning Account (ELA) for college costs. To increase salience and interest.	Administrative data for high school students in Canada	RCT	8%-points increase in **enrolment** higher education for the treatment group.
Castleman and Page (2014)	**Reminders** (text messages) on financial aid application.	Administrative data for students in higher education in the US.	RCT	Treatment group 12%-points more likely to persist in higher education
Castleman and Page (2015)	**Reminders** (text messages) and **assistance** to complete higher education applications and enrol.	Administrative data for last-year high school students (17-18y) and parents, US.	RCT	Between 2 and 7%-points increase in **enrolment** rates in treatment group. Larger effects for students who were initially performing worse or had less access to information.
Bird et al. (2017)	Personal **reminders** on financial aid benefits, applications and available assistance. Combined with positive identity activation.	> 450,000 low-SES and first-generation high-school students (17-18y) in the US, national level.	RCT	Only providing information on costs and benefits did not increase enrolment. Providing planning assistance increased overall **enrolment** by 1.1%-points.
Page et al. (2017)	**Reminders** of assistance for financial aid application.	US	RCT	Positive effect on applications and **enrolment.**
Oreopoulos and Ford (2016)	First wave: financial support (public transport, tuition fees..), and **assistance** on higher education application. This intervention changed defaults and added structure. Second wave: (i) **assistance** and application fee waivers, (ii) only assistance.	Administrative data, >6000 last-year high school students (17-18y) from risk groups in Canada.	RCT, Difference-in-Difference estimation.	First wave: treatment group 19%-points increase in higher education **enrolment**. Second wave: (i) positive effect on **applications and enrolment** (ii) no effect. This intervention shows that assistance alone is not enough.
Dinkelman and Martinez (2014)	**Information** on higher education (video) and financial aid.	Survey and administrative data, >6000	RCT (randomized at school level)	Treatment group 6%-points more likely to **enro**l in college

		students in Chile		preparation courses.
Kerr et al. (2015)	Statistical **information** on earnings and employment differences of higher education graduates.	Survey and administrative data for 3500 last-year high school students, 97 schools in Finland.	RCT (randomized school level)	No significant difference between treatment and control group on **enrolment** and education choice, but positive change in beliefs .
McGuigan et al. (2012)	**Information on costs and returns to higher education** and financial aid (website, cards, video), compared to a contemporaneous information provision by the media. Source of exogenous variation is the timing of the intervention.	Survey, 12000 students from 56 schools in London.	RCT (randomized school level)	Treatment group were 3.9%-points less likely to form negative beliefs, 3.3%-points more likely to form positive beliefs, and 0.6% more likely to **apply** to higher education. Stronger effects for more disadvantaged students
Ross et al. (2013)	Control group only **information**, treatment group received information and **assistance** on financial aid application.		RCT	Treatment group 15.7%-points more likely to **apply for financial aid.**
Lergetporer et al. (2018)	**Information** on costs and benefits of higher education, directed to parents.	Germany	RCT	Positive significant effect on **educational aspirations**, but not enough to close aspiration gap. Stronger response for university graduates.
Bleemer and Zafar (2017)	**Information** on costs and benefits of higher education directed to head of households.	Large survey, US.	RCT	Significant positive effect of benefits information on **intended enrolment**, no effect of cost information. Stronger effects for disadvantaged groups. Persistent effects in the long-run.
Behavioral Insights Team (2015)	**Information** (cards) on returns to higher education directed to parents and students.	Survey data, Somerset (UK)	RCT	Significant positive effect on **intended enrolment.** Lack of role models in low-SES groups could explain incomplete information.

Oreopoulos and Dunn (2013)	**Information** on the costs and benefits of higher education and financial aid (video), and financial aid **assistance**.	Survey, 1600 low-income students in Canada.	RCT	Treatment group 24%-points less likely to form negative **beliefs**, 15%-points increase in higher education (college) aspirations.
Avery (2013)	**Personal assistance** on higher education application directed to low-SES students. To reduce information barriers.	Administrative data, US.	RCT	Treatment group 30%-points more likely to **apply** and 15%-points more likely to **enrol**.
Bettinger et al. (2012)	(i) Personal **information** on costs and benefits of higher education and financial aid; and personal **assistance** financial aid application, or (ii) only information.	Administrative data for low-SES, high-school students in the US.	RCT	Both treatments increase **financial aid applications and enrolment** by 16%- and 11%-points. Only information treatment yielded the same results as the control group.
Carrell and Sacerdote (2013)	**Personal assistance** on financial aid and higher education applications.	Administrative data, last-year high school students in New Hampshire.	RCT	Treatment group overall more likely to **enroll** (different estimates).
Castleman et al. (2012)	**Assistance** on financial aid and applications.	Last-year high school students.	RCT	Treatment group had 15%-points higher **enrolment** rates.
Carrell and Sacerdote (2017)	Statistical **information** (letters) on financial returns to higher education + personalized letters to encourage students to apply.	High-school students (17-18y), US.	RCT	No effect on university **enrolment.**
Peter and Zambre (2017)	Statistical **information** on returns and financing of higher education.	Low-SES students in Germany;	RCT	Positive effect on **intended enrolment.**
Borghans et al. (2015)	**Personal assistance** on higher education.	Survey, 4000 high-school students in the Netherlands.	OLS and IV estimation.	Treatment group 2%-points less likely to regret their higher **education choice**. Stronger effects for male and low-SES students.
Bergman et al. (2017)	**Information** on financial aid directed to higher education applicants.	US	RCT	No effect on higher education **enrolment**.

Booij et al. (2012)	**Information** on loan conditions. Students generally believed to be aware of universal eligibility.	Higher education students in Yhe Netherlands.	RCT	No effect on **borrowing**, positive effect of 18%-points on awareness. These results suggest that student's borrowing behavior in The Netherlands is not limited by a lack of information.
Barr et al. (2017)	**Information** on loan conditions.	Low-SES individuals in the US. Not students.	RCT	Adverse effect: reduced **borrowing**, stronger among low-SES individuals.
Hoxby and Turner (2015)	I**nformation** on application, costs and fee waivers.	US	RCT	Positive effect on **application and enrolment**.
Harackiewicz et al. (2012)	**Information** (mails, brochures, website) on returns to education (STEM courses) , directed to parents.	Administrative and survey data, US.	RCT	Treatment group were 17%-points more likely to **believe in the value of education**. Stronger effects for higher educated parents.
Marx and Turner (2017)	Changing the **default** zero to a non-zero loan amount.	US	RCT	Treatment group 40% more likely to **borrow**, attained higher grades but no effect on **enrolment**.
Bergman and Rogers (2017)	**Opt-in vs. opt-out default** for adoption mailing service directed to parents of high school students.	US	RCT	7.8% of the opt-in group vs. 96.5% of the opt-out group **adopted the service.**
Field (2009)	**Framing/default** financial packages. (tuition waiver vs. loan). To reduce debt aversion.	Law students in the US.	RCT	Treatment group was 36-45%-points more likely to **enrol.**
Benhassine et al. (2015)	Labeled Cash Transfer **framed** as financial aid for education, directed to parents. Not contingent on enrolment.	Administrative and survey data on 4400 households of primary-school children in poor rural areas in Morocco.	RCT	By **improving beliefs** on educational returns, **dropout** rates decreased by 30% and **enrolment** increased by 7.4%-points in the treatment group..

Appendix D: Identification Strategy

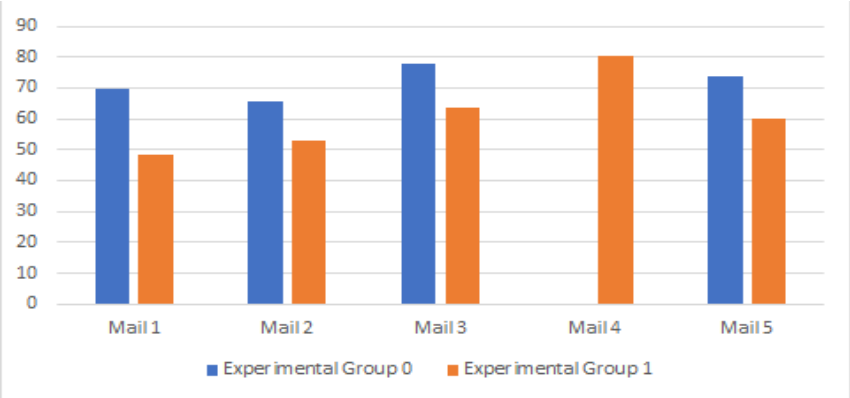
Table D.1: Comparative Statistics Experimental Groups

	Mail address not previously registered at DUO (Experimental Group 0)	Mail address previously registered at DUO (Experimental Group 1)	Significance Difference (t-test)
Male (%)	47.52 (0.50) N = 99,523	47.91 (0.50) N = 72,040	0.39
Age	17.61 (1.54) N = 99,523	20.40 (2.05) N = 72,040	2.79***
Passed the final exam (%)	84.53 (0.36) N = 99,563	77.35 (0.42) N = 72,040	7.18***
Exam grade	6.67 (0.55) N = 78,594	6.50 (0.48) N = 8,255	0.17***
SES score	0.14 (1.13) N = 98,970	-0.22 (1.24) N = 71,665	0.36***
Total Observations	99,522	72,040	

Standard errors between parentheses.
*Significance level (*0.1, **0.05, ***0.01)*

Appendix E: Analysis Mails

Figure E.1: Opening Rates Mails (%)



Opt-in vs. Opt-out

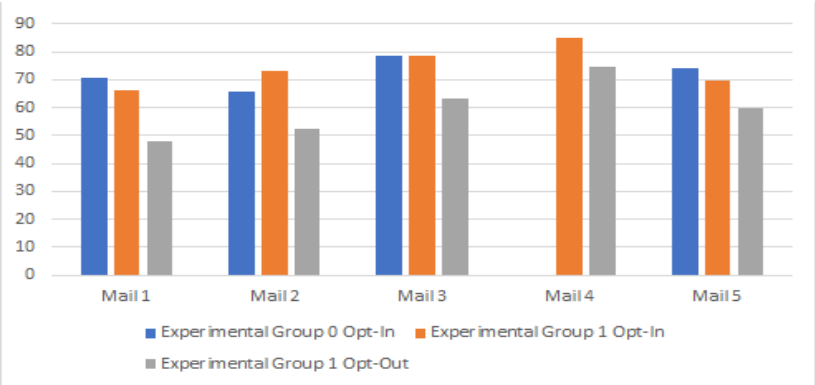
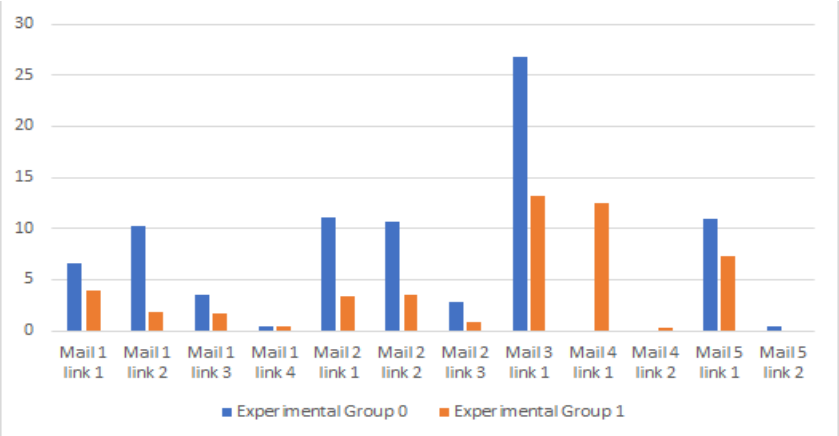


Figure E.2: Links clicked by the students (%)



Appendix F: Characterizing the Compliers

Table F.1: Characterizing the Compliers

Experimental Group 0

Background Characteristic	Mail 1		Mail 3		Mail 5		N
	First stage	Ratio	First stage	Ratio	First stage	Ratio	
All	.0232	1	.0260	1	.0248	1	33,166
Gender							
Male	.0251	.9052	.0235	.9038	.0226	.9113	15,680
Female	.0210	1.081	.0284	1.092	.0268	1.081	17,486
Age							
<18	.0136	.5862	.0289	1.111	.0275	1.109	25,870
≥ 18	.0259	1.116	.0162	.6231	.0154	.6210	7,296
Education Type							
MBO4	.0063	.2716	.0080	.3078	.0076	.3065	2,397
HAVO	.0189	.8147	.0190	.7308	.0188	.0758	17,521
VWO	.0357	1.539	.0430	1.654	.0397	1.601	11,613
VAVO	.0055	.2371	.0092	.3538	.0098	.3952	1,635
SES quantile							
SES1	.0233	1.004	.0262	1.008	.0261	1.052	6,922
SES2	.0214	.9224	.0249	.9577	.0236	.9516	7,911
SES3	.0236	1.017	.0259	.9962	.0240	.9677	8,550
SES4	.0242	1.043	.0271	1.042	.0255	1.028	9,595

Experimental Group 1

Background Characteristic	Mail 1		Mail 3		Mail 5		N
	First stage	Ratio	First stage	Ratio	First stage	Ratio	
All	.0115	1	.0137	1	.0126	1	36,043
Gender							
Male	.0121	1.052	.0135	.9854	.0132	1.048	17,453
Female	.0109	.9478	.0139	1.015	.0120	.9524	18,590
Age							
<18	.0397	3.452	.0416	3.036	.0417	3.310	897
≥ 18	.0108	.9391	.0130	.9489	.0118	.9365	35,146
Education Type							
MBO4	.0095	.8261	.0114	.8321	.0105	.8333	29,680
HAVO	.0219	1.904	.0217	1.584	.0234	1.857	1,202
VWO	.0341	2.965	.0422	3.080	.0372	2.952	1,886
VAVO	.0124	1.078	.0154	1.124	.0130	1.032	3,275
SES quantile							
SES1	.0091	.7913	.0108	.7883	.0099	.7857	11,117
SES2	.0143	1.243	.0158	1.153	.0148	1.175	9,223
SES3	.0117	1.017	.0158	1.153	.0142	1.127	8,608
SES4	.0113	.9826	.0130	.9489	.0118	.9365	6,892

Appendix G: Heterogeneous Effects

Table G.1: Fraction of students from different education types by SES quantile (%, summed over rows)

Experimental Group 0

Education Type	SES1	SES2	SES3	SES4
MBO-4	27.42	29.11	25.52	17.95
HAVO	21.94	24.92	26.34	26.79
VWO	17.52	22.35	25.62	34.51
VAVO	24.27	21.43	23.61	30.70

Experimental Group 1

Education Type	SES1	SES2	SES3	SES4
MBO-4	31.39	26.57	24.10	17.93
HAVO	30.53	24.69	22.13	22.66
VWO	23.41	23.44	24.95	28.20
VAVO	29.76	26.05	23.93	19.21

REFERENCES

Angrist, J. and Pischke, J. (2009). *Mostly harmless econometrics: An empiricist's companion*. Princeton: Princeton University Press.

Avery, C. and Kane, T. (2004). Student Perceptions of College Opportunities: The Boston COACH Program. In Hoxby, C. (Ed.), *College Choices: The Economics of Where to Go, and How to Pay For It*, p. 355-394. Chicago: University of Chicago Press.

Avery, C. and Turner, S. (2012). Student Loans: Do College Students Borrow Too Much – Or Not Enough? *Journal of Economic Perspectives, 26*(1): pp.165-192.

Avery, C. (2013). Evaluation of The College Possible Program: Results from a Randomized Controlled Trial. NBER Working Paper No.19562, National Bureau of Economic Research.

Barr, A., Bird, K. and Castleman, B. (2017). Prompting Active Choice Among High-Risk Borrowers: Evidence from a Student Loan Counseling Experiment. EdPolicyWorks Working Paper Series No. 41.

Becker, G., and Mulligan, C. (1997). The endogenous determination of time preference. *The Quarterly Journal of Economics*, *112*(3), pp. 729-758.

Benhassine, N., Devoto, F., Duflo, E., Dupas, P., and Pouliquen, V. (2015). Turning a shove into a nudge? A" labeled cash transfer" for education. *American Economic Journal: Economic Policy*, *7*(3), pp. 86-125.

Bergman, P. and Rogers, T. (2017). The Impact of Defaults on Technology Adoption, and Its Underappreciation by Policymakers. HKS Faculty Research Paper Working Paper Series RWP17-021.

Bergman, P., Denning, J. T. and Manoli, D. (2017). Broken Tax Breaks? Evidence from a Tax Credit Information Experiment with 1,000,000 Students. IZA Discussion Paper No. 10997

Bettinger, E. P., Long, B. T., Oreopoulos, P. and Sanbonmatsu, L. (2012). The Role of Application Assistance and Information in College Decision: Results from the H&R block FAFSA Experiment. *Quarterly Journal of Economics*, 127, pp. 1205-1242.

Bird, K. A., Castleman, B. L., Goodman, J., and Lamberton, C. (2017). Nudging at a national scale: Experimental evidence from a FAFSA completion campaign. In *Technical Report*. Ed Policy Works Working Paper Series No. 54

Bleemer, Z., and Zafar, B. (2017). Intended college attendance: Evidence from an experiment on college returns and costs. *Journal of Public Economics*.

Booij, A. S., Leuven, E. and Oosterbeek, H. (2012). The Role of Information in the Take-up of Student Loans. *Economics of Education Review*, 31, pp. 33-44.

Borghans, L., Golsteyn, B. H., and Stenberg, A. (2015). Does expert advice improve educational choice? *PloS one*, *10*(12), e0145378.

Carrell, S and Sacerdote, B. (2017). Why Do College Going Interventions Work? *American Economic Journal: Applied Economics*, 9(3), pp. 124-151

Carrell, S. E., Sacerdote, B. I.and West, J. E. (2013). From Natural Variation to Optimal Policy? The Importance of Endogenous Peer Group Formation. *Econometrica*, 81(3), pp. 855-882.

Castleman, B. L., and Page, L. C. (2015). Summer nudging: Can personalized text messages and peer mentor outreach increase college going among low-income high school graduates? *Journal of Economic Behavior & Organization*, *115*, pp. 144-160.

Castleman, B. L., Arnold, K. and Wartman, K. L. (2012). Stemming the Tide of Summer Melt: An Experimental Study of the Effects of Post-High School Summer Intervention on Low-Income Students' College Enrolment. *Journal of Research on Educational Effectiveness*, 5, pp. 1-18.

CBS, Centraal Bureau voor de Statistiek. (2017). in: Onderwijs in Cijfers (2017)

Damgaard, M. T., and Nielsen, H. S. (2018). Nudging in education. *Economics of Education Review*.

Dinkelman, T. and Martinez, C. (2014). Investing in Schooling in Chile: The Role of Information about Financial Aid for Higher Education. *Review of Economics and Statistics*, 96(2), pp. 244-257.

Duckworth, A. L. and Seligman, M. E. (2006). Self-discipline gives girls the edge: Gender in self-discipline, grades, and achievement test scores. *Journal of educational psychology*, *98*(1), 198.

DUO, Dienst Uitvoering Onderwijs (2018). Retrieved May 2018 from: https://duo.nl/open_onderwijsdata/

Golsteyn, B. H., Grönqvist, H., and Lindahl, L. (2014). Adolescent time preferences predict lifetime outcomes. *The Economic Journal*, *124*(580), pp. F739-F761.

Harackiewicz, J. M., Rozek, C. S., Hulleman, C. S., and Hyde, J. S. (2012). Helping parents to motivate adolescents in mathematics and science: An experimental test of a utility-value intervention. *Psychological Science*, *23*(8), pp. 899-906.

Hoxby, C. and Turner, S. (2015). What High-Achieving Low-Income Students Know about College. *American Economic Review: Papers & Proceedings*, 105(5), pp. 514-517.

Kerr, S., Pekkarinen, T., Sarvimäki, M., and Uusitalo, R. (2015). Post-secondary education and information on labor market prospects: A randomized field experiment. IZA Discussion Paper No. 9372

Lavecchia, A. M., Liu, H., and Oreopoulos, P. (2016). Behavioral economics of education: Progress and possibilities. In *Handbook of the Economics of Education* Vol. 5, pp. 1-74. Elsevier.

Lergetporer, P., Werner, K., and Woessmann, L. (2018). Does Ignorance of Economic Returns and Costs Explain the Educational Aspiration Gap? Evidence from Representative Survey Experiments. IZA DP No. 11453.

Marx, B. M., and Turner, L. J. (2017). *Student loan nudges: Experimental evidence on borrowing and educational attainment* (No. w24060). National Bureau of Economic Research.

McGuigan, M., McNally, S., and Wyness, G. (2012). Student Awareness of Costs and Benefits of Educational Decisions: Effects of an Information Campaign. CEE DP 139. *Centre for the Economics of Education*.

Mischel, W., Shoda, Y., and Rodriguez, M. I. (1989). Delay of gratification in children. *Science*, *244*(4907), pp. 933-938.

MOCW (2017). Referentieraming 2016. Retrieved July 2018 from: https://www.onderwijsincijfers.nl/kengetallen/onderwijs-algemeen/leerlingen-en-studenten

MOCW, Ministerie voor Onderwijs, Cultuur en Wetenschap (2017). Pilot Bewuste Studiekeuze.

MOCW, Ministerie voor Onderwijs, Cultuur en Wetenschap (2017). Referentieraming 2016; Retrieved July 2018 from: https://www.onderwijsincijfers.nl/kengetallen/onderwijs-algemeen/leerlingen-en-studenten

Onderwijs in Cijfers (2017). Retrieved July 2018 from: https://www.onderwijsincijfers.nl/kengetallen

Oreopoulos, P. and Dunn, R. (2013). Information and College Access: Evidence from a Randomized Field Experiment. *Scandinavian Journal of Economics*, 115(1), pp. 3-26.

Oreopoulos, P. and Ford, R. (2016). Keeping College Options Open: A Field Experiment to Help All High School Seniors Through the College Application Process. NBER Working Paper No. 22320.

Page, L., Castleman, B. and Meyer, K. (2017). Customized nudging to improve FAFSA completion and income verification. Working Paper.

Peter, F. H., and Zambre, V. (2017). Intended college enrollment and educational inequality: Do students lack information? *Economics of Education Review*, *60*, pp. 125-141.

Rijksoverheid (2018). Retrieved July 2018 from: https://www.rijksoverheid.nl/onderwerpen

Ross, R., White, S., Wright, J., and Knapp, L. (2013). Using behavioral economics for postsecondary success. In Ideas 42 (Vol. 42).

SCP, Sociaal en Cultureel Planbureau (2018). Retrieved May 2018 from: https://www.scp.nl/

Sunstein, C.R. (2014). Nudging: A very short guide. *Journal of Consumer Policy*, 37(4), pp. 583-588.

Staiger, D., and Stock., J. (1997). Instrument Variables with Weak Instruments. *Econometrica*, *65*(3), pp. 557-86.

Thaler, R.H, and Sunstein, C.R. (2008) *Nudge: Improving Decisions about Health, Wealth, and Happiness*. New Haven, CT: Yale University Press.

The Behavioral Insights Team (2015). Behavioral Insights and the Somerset Challenge. A Report from the Behavioral Insights Team. Retrieved June 2018 from: https://www.behaviouralinsights.co.uk/publications/behavioural-insights-and-the-somerset-challenge/

The World Bank (2018). Retrieved August 2018 from: http://www.worldbank.org/en/topic/education/overview.