CPB Netherlands Bureau for Economic Policy Analysis

CPB Discussion Paper | 338

The Effects of Additional Study Choice Activities:

Evidence from a Randomized Controlled Trial

Anja Deelen
Sonny Kuijpers

The Effects of Additional Study Choice Activities: Evidence from a Randomized Controlled Trial

Anja Deelen and Sonny Kuijpers*

CPB Netherlands Bureau for Economic Policy Analysis †

14 December, 2016

Abstract

This paper analyses the effect of additional study choice activities on the probability that subscribers withdraw from the study field of their preference before the study actually commences, as well as on the study performance of students who actually start with the study. Subscribers were randomly assigned to a study choice activity, which consisted of an assignment (representative of the first year curriculum) that provided them with additional information on the content of the study field of their preference. The data contain subscribers at three Dutch faculties in 2013/2014 – 2014/2015. Estimates suggest that the assignments did not cause a significant increase in the probability of withdrawal before the start of the study. We also find no significant effects of the intervention on the study results of enrolled students. Although we cannot exclude that other, more rigorous, interventions may generate significant positive effects on the quality of choice, our paper therefore suggests that small-scale interventions such as a single assignment – provided on top of other study choice activities – are ineffective.

Keywords: Study Choice Activities, Field of Study
JEL Classifications: I20, I21, I23

*Contact: Anja Deelen (A.P.Deelen@cpb.nl), Sonny Kuijpers (S.Kuijpers@cpb.nl), CPB Netherlands Bureau for Economic Policy Analysis, The Hague, The Netherlands (www.cpb.nl).

†The authors thank Ryanne van Dalen and Debby Lanser who worked on this project in an earlier phase, Guy Bendermacher (FHML), Sylvia Haerkens (FASoS), and Linda van der Tuijn (Tilburg School of Economics and Management) for their cooperation and the deliverance of data, and Adam Booij (UvA), Dinand Webbink (EUR), and various colleagues at CPB for their useful comments on earlier versions of this paper.

Abstract in Dutch

Omdat een groot deel van de startende studenten in het Nederlandse hoger onderwijs gedurende of na het eerste jaar stopt met de gekozen studie (hbo: 36%, wo: 24%), is verbetering van het studiekeuzeproces een belangrijke uitdaging. Naast studiekeuzeactiviteiten in het traject voorafgaand aan de inschrijving (open dagen, meeloopdagen, etc.) zijn faculteiten sinds 1 september 2013 verplicht om een studiekeuzecheck uit te voeren nadat de aankomende student zich heeft ingeschreven inschrijving voor een studie. Hoe de studiekeuzecheck eruit ziet, verschilt per hogeschool of universiteit. Het kan daarbij gaan om een gesprek met iemand van de opleiding, proefstuderen of bijvoorbeeld het invullen van een digitale vragenlijst.

Deze studie onderzoekt de effecten van een extra opdracht in het kader van de studiekeuzecheck. We kijken naar het effect op de kans dat ingeschrevenen zich terugtrekken nog voordat de studie van start gaat en op de studieresultaten van hen die daadwerkelijk beginnen met de studie. Ingeschrevenen werden voor dit onderzoek gerandomiseerd toegewezen aan een controle- of behandelgroep. De behandelgroep kreeg een opdracht toegestuurd die representatief was voor de opdrachten die in het eerste studiejaar worden aangeboden. Hiermee beschikte de behandelgroep over additionele informatie over de inhoud van de studie. De achterliggende gedachte is dat deze extra informatie zou kunnen leiden tot betere zelfselectie, waardoor de samenstelling van de groep die daadwerkelijk met de studie start zo wijzigt dat deze gemiddeld een betere match heeft met de studie en daardoor gemiddeld betere studieresultaten behaalt. De data betreffen ingeschrevenen aan drie Nederlandse faculteiten in de periode 2013/2014 – 2014/2015.

Wij vinden geen significant effect van de extra opdracht op de kans dat ingeschrevenen zich terugtrekken nog voordat de studie van start gaat. We vinden – consistent daarmee – ook geen significant effect op de studieresultaten van hen die daadwerkelijk beginnen met de studie. Hoewel we niet kunnen uitsluiten dat omvangrijkere interventies wel effecten zouden kunnen sorteren, suggereert ons onderzoek dat kleinschalige interventies, zoals een enkele opdracht bovenop de al aangeboden studiekeuzeactiviteiten, niet effectief zijn.

1 Introduction

High drop-out rates in post-secondary education are a waste of resources, not only for drop outs themselves and their parents, but also for the tax payer. Since 24% of students at Dutch universities and 36% of students at Dutch universities of applied sciences quit or switch studies during or after their first year of study (Broek et al. 2015), there seems to be room to improve the choice of study field. This is in spite of a broad range of existing activities aimed at providing a good match between the preferences and abilities of potential students and their educational choice. A classic example is 'selection at the gate', where universities use selective admission policies in order to retain only those subscribers who are likely to succeed in their study field of preference.[1] Unlike selection, other activities are directly aimed at affecting the choice behaviour of potential students, thereby improving the *self-selection* of students into different fields of study. The latter kind of activities can be organized both by universities (such as open days or student-for-a-day programmes) and high schools (e.g. study counselling). A common feature of these programmes is the provision of *information* to potential students. The idea is that better informed individuals are more likely to choose a field of study that matches their preferences and abilities and are hence more likely to obtain a diploma eventually. The effects of these 'study choice activities', however, remain largely unknown.

At the same time, there is a growing body of literature that investigates the effects of providing information in the context of (higher) education, yet the evidence is mixed. Bettinger et al. (2012), for example, discuss a randomized experiment in Ohio and North Carolina among low-income individuals receiving tax preparation help. The treatment group received customized information about the amount of (student) financial aid they were estimated to be eligible for, which was then compared with tuition costs for nearby colleges. Subsequently, these individuals were encouraged to complete the Free Application for Federal Student Aid (FAFSA). The authors find that participants who received the customized information were no more likely to file a FAFSA than the control group that did not receive this information.[2]

McGuigan et al. (2012) present results from a randomized experiment among high schools in London. In half of the 54 participating schools, year 10 pupils (i.e. 14/15 year olds) were provided with information about the costs and benefits of staying on in education, employment prospects, university tuition fees, maintenance grants, and loans. Eight to twelve weeks after the intervention, participating pupils had significantly improved their perception of the costs and (employment) benefits associated with staying on in education, as well as their knowledge of student finance. They were also more likely to report planning to stay on in full-time education after age 16, but it is unknown whether these intentions were translated into actual behaviour.

Finally, Kerr et al. (2015) examine the impact of an information intervention offered to 97 randomly chosen high schools in Finland. Graduating pupils in treatment schools were provided with accurate information about the earnings distributions, employment rates, and the most common occupations associated with detailed post-secondary educational degrees. Although survey results suggest that the intervention led to belief updating, there was no average impact on actual applications or enrolment in higher education.

In this paper, we contribute to the literature by presenting the results of a randomized experiment in which potential students (subscribers) at Dutch universities received a home assignment that was representative of the first year curriculum of their study of choice. Hence, a major difference between the information intervention evaluated here and the interventions discussed above is the type of information that was offered to the potential students. The home assignment was meant to give subscribers a more realistic perception of the *content* of the study

[1] See, for example, Reumer and Van der Wende (2010) for a Dutch case study.

[2] Another group received the 'main' treatment, which comprised of the customized information and – on top of that – immediate assistance and a streamlined process to complete the FAFSA. This main treatment did substantially increase FAFSA submissions and ultimately the likelihood of college attendance, persistence, and aid receipt.

field of their preference, rather than of the costs and (employment) benefits. Another difference lies in the timing of the treatment: in our experiment, the intervention took place after high school pupils had already made their initial study choice. At this point in time, it was one of the last opportunities for potential students to change their mind without incurring significant direct financial costs to themselves and the tax payer.

The home assignment was randomly given to a 50% subsample of potential students, who subscribed for one of three study fields at two different Dutch universities. Since the intervention was offered on top of regular matching activities offered by universities, the home assignment should be seen as an *additional* study choice activity. Possible effects of this activity are expected to run through self-selection into the study field. Of interest, therefore, is the causal effect of taking part in the home assignment on the persistence of study field choice and (subsequently) on average student achievement. We first analyse whether the intervention affects the probability of withdrawing the subscription before the study commences. Second, we estimate whether subscribers who decide to enrol – after having been exposed to the intervention – perform better on average than the group that takes this decision based on less information. We apply the Instrumental Variable approach to address non-compliance, since the group that actually decided to take part in the assignment may be a selective group compared with the entire group drawn by the lottery.

We find that the intervention had no significant effect on the probability of withdrawal before the study commences. In line with this, we also find no significant effect of taking part in the assignment on the total number of credits (ECTS) obtained. We therefore conclude that offering a home assignment on top of the regular matching activities offered by Dutch universities does not affect (self-)selection of potential students. Although we cannot exclude that other, more rigorous, interventions may generate positive effects on the quality of choice, our paper suggests that small-scale interventions such as a single assignment – provided on top of other study choice activities – are ineffective. The remainder of this paper is structured as follows. Section 2 describes the experiment and explains the methodology of this study, whereas Section 3 reports on the data and provides descriptive statistics. The results are presented in Section 4, and Section 5 concludes the study.

2 Description of the Experiment and Methodology

2.1 Dutch Study Choice Policies

This study analyses the effect of additional study choice activities within the scope of the Law 'Quality and Diversity'. The study check came on top of activities that were already carried out before the law came into force on 1 September, 2013. High schools and colleges generally supply study choice counselling, while universities and universities of applied sciences typically organize 'open days' and activities like 'trial study days' and 'walk along days'. On 'open days', educational institutions provide information about their study fields and also offer the possibility to meet staff and students. During a 'trial study day', lessons are organized for those interested in a particular study field and there is room to explore the study material and to meet students and staff. On a 'walk along day' (student-for-a-day programme), a potential student is linked to an actual student and joins him or her during a typical study day, including attending classes.

Notwithstanding the availability of these study choice activities, drop-out rates have been high: 24% of students at Dutch universities and 36% of students at Dutch universities of applied sciences quit during or after their first year of study (Broek et al. 2015). The Law 'Quality and Diversity' therefore aimed at improving the timeliness of the study choice as well as the quality of the match between students and their field of choice.

Timeliness of study choice is improved by the obligation to subscribe for a study field

before 1 May of the calendar year in which the study commences (the academic year starts in September). Potential students are allowed to subscribe to a maximum of three studies. If one subscribes before 1 May, the institution is obliged to carry out the 'study choice check', which assesses the quality of the match between the subscriber and the study field of choice. Hence, the study choice check is an activity done by the potential student *after* he or she has subscribed for a study field. Following this study choice check, the subscriber receives (non-binding) advice from the university or university of applied sciences regarding their opinion on the quality of the match. Access to the preferred field of study is guaranteed when subscription is timely (if no *numerus clausus* is used). The design of the study field check is up to the institution.

2.2 Description of the Experiment

Three faculties are involved in our experiment, which assesses the impact of additional study choice checks that take the form of an assignment. From Maastricht University, the Faculty of Arts and Social Sciences (FASoS) participated in the academic years 2013/2014 and 2014/2015, whilst the Faculty of Health, Medicine and Life Sciences (FHML) participated in the academic year 2014/2015. Tilburg School of Economics and Management (Tilburg University) also took part in the academic year 2014/2015.

The implementation of the study choice check and intervention differs over the faculties and the two academic years that we observe (see Table 1). In the academic year 2013/2014, when the law 'Quality and Diversity' had not yet come into force, FASoS carried out a survey among all study-field subscribers and did an intervention in the form of an assignment among a randomized 50% subsample. From the academic year 2014/2015 onward, institutions for higher education were legally obliged to offer a check on the study field choice made by subscribers. A survey (FASoS and FHML) and a matching day (Tilburg University) were held to meet the study check requirements. The survey, which all subscribers were required to fill in, contained questions on personal characteristics as well as questions related to the field of study. In addition, all three faculties gave an assignment to a randomized sample of subscribers, which is the subject of this study.

The assignments were always given to a 50% randomized subsample of the subscribed persons. The content of the assignment offered a 'preview' of the curriculum of the first year of the study and was intended to increase the subscribers' insight in the curriculum of the field of study of their choice. Responses to the assignment were to be submitted directly online. However, the submitted assignments were neither checked nor marked, since the main aim of the assignments was to inform the prospective students, rather than to test them.

Table 1: Overview of interventions by faculty and academic year

		FASoS	FHML	TiSEM
2013/2014	All students	Survey	-	-
	Intervention group only	Assignment	Survey 1)	-
2014/2015	All students	Survey	Survey	Matching day
	Intervention group only	Assignment	Assignment	Assignment

Notes: 1) For 2013/2014, FHML held a survey among a 50% randomized subsample. Our study, however, is confined to the four interventions that took the form of an assignment.
FASoS is the abbreviation of Faculty of Arts and Social Sciences and FHML is the Faculty of Health, Medicine and Life Sciences, both at Maastricht University; TiSEM is used here as an abbreviation for Tilburg School of Economics and Management.

Let us now consider the activities organized by the various faculties in each academic year in more detail. Maastricht University, where FASoS and FHML reside, presented itself to subscribers through open house and experience days, the latter enabling pupils to join current students to get an impression of the study. After online subscription for **FASoS**, all prospective

students had to fill in an online survey. Assessing the survey results, FASoS study advisors categorized the students into three groups: green, orange, and red. The green group consisted of persons for whom no impediments were found to start in their study field of choice, and they received an invitation for the introductory day in August right away. Subscribers in the orange group were also invited for the introductory day, but were asked to think about a comment made by the study advisor. Individuals in the red group were obliged to have a conversation with a study advisor, programme director, or teacher. This conversation led to oral advice on the subject of whether or not to enrol, confirmed by an email that also mentioned the introductory day. For the academic year 2013/2014, this advice was not binding yet.

In 2014/2015, the (by then obligatory) study check also consisted of the survey, categorization of subscribers in groups (green, orange, red), and talks. On top of that, in both academic years an assignment was given to a randomized sample. Per 'colour group', 50% of the individuals were selected by lottery and invited online to take part in an assignment. This is the intervention that we evaluate in this paper. The assignment was taken from a first year course of FASoS and consisted of one page of text on civil disobedience and four questions.[3] In case of the red group, the assignment was given prior to the talk with the study advisor.

The procedure at **FHML** for 2014/2015 is much like that at FASoS. All subscribers of FHML had to fill in an online survey. Based on the survey results, FHML study advisors categorized the students, this time into two groups: green (positive study advice) and red (advice indicating that the applicants' expectations and/or skills did not fully match with the study programme). In case of the latter advice, individuals had to have a conversation with the study advisor. Subsequently, 50% of the individuals in the green group were selected by lottery and invited online to take part in an assignment. This is the intervention that we evaluate in this research. The assignment, which was representative of a first year course at FHML, consisted of one page of text on delivery and infant mortality and was meant as an introduction to problem-based learning, which is the key approach used in FHML courses.[4]

Preceding the intervention, **TiSEM** offered potential students several opportunities to explore their fields of study, such as open days, a student-for-a-day programme, and trial lectures. Subsequently, individuals who subscribed were obliged to attend a matching day during which they had to take part in an online assessment. The assessment report, which was automatically generated, was only intended for self-reflection by the potential students, in order for them to get a clearer idea of whether the respective study programme would be a good match for them. Although there was ample opportunity to communicate with other participants and staff, only occasionally was individual advice given to students during or after the matching day. After the matching day, 50% of the attendants were selected by lottery. Within a couple of weeks after their matching day, individuals drawn by the lottery received an invitation by email to take part in an assignment. This is the intervention that we evaluate in this study. The assignment, which consisted of questions and computations regarding a profit and loss account of a major retail firm, was representative of a first year course assignment.

2.3 Methodology

Our main interest is to determine the causal effect of the intervention on the persistence of the study field choice and on the average student achievement. The intervention could induce

[3] Questions were for example: 'Does the subject of the assignment interest you? Yes, why? / No, why?'; 'Would you like to learn more about the subject? Yes, why? / No, why?'; 'Does the assignment change the way you think about your choice of the programme BA Cultuurwetenschappen/Arts and Culture? No, I am still certain... / Yes, I am more certain... / Yes, but I am less certain...'.

[4] Questions were for example: 'What problem is described in this text? Formulate the main question which defines the essence of the problem.'; 'What do you already know about this topic? Write this down in not more than 100 words.'; 'What do you not know yet? Reflect on at least three learning goals and write these down. Start each learning goal with an interrogative (for example 'how', 'why', 'how come', 'who').'.

self-selection into or out of the study field: subscribers who decide to enrol after having been exposed to the intervention could on average have a better match to the study field than the ones who make this decision based on less information. Although the achievement of individual students is not expected to be affected by the intervention, the average achievement of the group could improve due to improvement of the average match.

We are interested in the effect of the treatment (i.e. taking part in the assignment) on our outcomes (i.e. study choice persistence and average achievement). Because of the random assignment of the intervention, we could directly estimate the following OLS equation:

$$Y_{ift} = \beta_0 + \beta_1 D_{ift} + \beta_2 X_{ift} + \epsilon_{ift} \tag{1}$$

Here, Y_{ift} represents the outcome of individual i at the faculty f in year t, D_{ift} represents a dummy that equals 1 if the individual has been assigned to the intervention, X_{ift} is a set of controls (e.g. student background characteristics and faculty fixed effects), and ϵ_{ist} is the error term, which captures unobservable determinants of the outcome. β_1 is the parameter of interest, representing the causal impact of *being assigned to taking part in* a study choice assignment on the outcome.

β_1 would also represent the causal impact of *taking part in* the study choice assignment if all subscribers who received the assignment would have actually participated.[5] This is not the case, however. Slightly less than half of the subscribers who were assigned to the intervention did not actually take part in the online assignment. Due to this non-compliance, β_1 may understate the 'true' effect of participation. In fact, it measures the intention-to-treat (ITT) effect, i.e. the effect of asking (but not forcing) subscribers to submit a study choice assignment.

In order to estimate the impact of taking part in the assignment, we employ a two-stage least squares (IV) approach, where we use the random assignment of the intervention as an instrument for participation. The first stage in this framework is

$$T_{ift} = \alpha_0 + \alpha_1 D_{ift} + \alpha_2 X_{ift} + \theta_{ift} \tag{2}$$

where T_{ift} represents a dummy that equals 1 if the individual has taken part in the assignment. The parameter of interest is α_{ift}, which represents the effect of being assigned to the intervention (D_{ift}) on the probability of actually participating. The second stage is

$$Y_{ift} = \gamma_0 + \gamma_1 \hat{T}_{ift} + \gamma_2 X_{ift} + \upsilon_{ift} \tag{3}$$

where $\hat{T}_{ift}$ is the predicted probability from Equation 2. γ_1, our ultimate parameter of interest, represents the impact of taking part in the assignment for those who were caused to do so by (randomly) receiving the assignment. In other words, γ_1 represents the average treatment effect on the treated (ATT). Finally, in the IV setup, Equation 1 is the reduced form equation, estimating the direct effect of the lottery on student outcomes (ITT).[6]

We measure the effect of the intervention on the persistence of study field choice and student achievement. Persistence of study field choice is measured by the percentage of subscribers that do not actually enrol but withdraw before the onset of the study. Achievement of enrolled students is measured by the number of European Credit Transfer and Accumulation System (ECTS) credits obtained during the first year of study. Quitting the study during the academic year cannot be observed explicitly in our data, but is taken into account implicitly in the measurement of student achievement, because in case of quitting the number of obtained ECTS credits remains limited.

[5] We register the assignment as 'taken part in' if the subscriber has – at the minimum – opened the assignment link. So where we speak of 'taking part in the assignment' we mean that at least the assignment link has been opened.

[6] The ATT is numerically equal to the quotient of $\hat{\beta}_{ift}$ in Equation 1 and $\hat{\alpha}_{ift}$ in Equation 2.

3 Data

3.1 Data sources

Both the surveys and the assignments were administered online. The results of the surveys provided us with information on the basic background characteristics of the subscribers, such as age, gender, and nationality, as well as characteristics that are more closely related to the choice of study, such as the number of orientation activities undertaken and expectations regarding future performance. We use this information for two purposes. First, we perform a check on the randomization procedure using these background characteristics. Second, we include them as covariates in our most elaborate regression models. For each subscriber who was invited to take part in the assignment, the resulting data tells us whether he or she actually opened (and hence 'took part in') it. This is our main treatment variable; neither the degree to which the subscribers completed the assignments nor the quality of their answers to the questions play a role in this study.

Furthermore, the faculties provided us with the exam results of all enrolled first-year students for the academic years concerned. This allowed us to calculate the total number of credits (ECTS) obtained by each student in his or her first academic year. The subscribers who withdrew before the study commenced are the ones who do not appear in this exam results file.

3.2 Sample selection

Throughout this study, we will focus on the subscribers who were involved in the random assignment of the intervention. This comes down to a total of 1,307 individuals; 666 of them were sent an invitation to take part in an assignment and 641 of them formed the control group. For all of these individuals, it is known whether they withdrew before the beginning of the study or actually enrolled in the programme. Data on background characteristics, however, are not available for this entire group. As the Tilburg faculty did not carry out a survey, there are hardly any characteristics available for this subgroup. Moreover, at the Maastricht faculties, not all subscribers completely filled out the survey, which means that most covariates have some missing observations. We address these issues by imputing missing covariate data with their mean and by including dummy variables for missing values. This way we include as many observations in our regressions as possible.

For the analyses examining the effects of the intervention on average student achievement (number of credits obtained), we only look at the subsample of subscribers that actually enrolled. Here, the sample includes 873 students.

3.3 Randomization check and predictors of outcomes

In Table 2, we analyse the predictive value of the covariates with respect to our instrumental variable (being assigned to the intervention) and outcome variables (withdrawal rate and number of ECTS credits obtained), respectively. The first column can therefore be seen as a randomization check, whereas the second and third columns present potential predictors of student behaviour. The estimates in the first column show that the subscribers in the treatment and control groups are similar with respect to their observed characteristics, as all variables are (jointly) insignificant predictors of the probability of being assigned to the intervention.[7] In the second column, it can be seen that subscribers who have undertaken more orientation activities and for whom the programme concerned is the study of first choice are less likely to withdraw their subscription. Finally, the third column shows that, among the students who

[7]The number of observations (600) is based on the subsample of subscribers for whom we do not have any missing covariate data. We have no reason to believe that the missing elements are unequally or selectively distributed among the treatment and control groups, however, as the random assignment of the intervention took place after the survey.

actually enrol, men obtain on average significantly fewer ECTS credits.[8] In the appendix we present the mean and standard deviation of each covariate (Table 4) as well as separate tables for the two Maastricht faculties (Table 5 and 6).

Table 2: Covariates table

	Assigned to intervention		Withdrawal (control group)		Number of ECTS (control group)	
	(1)		(2)		(3)	
	coeff	se	coeff	se	coeff	se
Male	0.078	(0.051)	0.017	(0.069)	-9.404**	(4.269)
Age	0.001	(0.014)	-0.020	(0.020)	-1.643	(1.190)
Dutch-born	-0.010	(0.071)	-0.077	(0.089)	-3.587	(5.780)
Pre-university high school (*vwo*) graduate	-0.087	(0.125)	0.151	(0.159)	-4.239	(9.327)
High school GPA 6.6-7.5	-0.060	(0.049)	-0.001	(0.061)	-0.240	(3.297)
High school GPA $\geq$ 7.6	-0.122	(0.081)	0.070	(0.097)	-6.117	(7.175)
Number of orientation activities undertaken	-0.010	(0.022)	-0.113***	(0.027)	1.389	(1.620)
Study of first choice	-0.016	(0.052)	-0.395***	(0.071)	-3.385	(3.109)
Plans to move out of parents' house	-0.012	(0.050)	-0.000	(0.062)	3.878	(3.476)
Expects to earn $\geq$ 50 ECTS in first year	0.046	(0.047)	0.034	(0.056)	3.194	(3.456)
Expects to devote 21-30 hours a week to studying	0.031	(0.052)	-0.046	(0.065)	1.698	(3.963)
Expects to devote $>$ 30 hours a week to studying	-0.023	(0.070)	-0.088	(0.082)	1.029	(4.980)
Special circumstances	-0.012	(0.069)	-0.050	(0.081)	-0.008	(5.008)
Number of observations	600		298		175	
P-value (F)	0.782		0.000		0.273	
Fixed effects	yes		yes		yes	
$\overline{y}$	0.510		0.332		45.493	
% TiSEM	0.317					
% FHML	0.362					
% FASoS	0.321					
(% year 1)	0.505					
(% year 2)	0.495					
(% red advice)	0.633					
(% orange advice)	0.110					
(% green advice)	0.257					

Notes: Each column is a regression. Robust standard errors are in parentheses. All models include faculty×year×colour fixed effects. The number of observations of the regressions relates to the subsample of subscribers for whom we do not have any missing covariate data. The regressions in columns 2 and 3 are estimated for the control group only, as the outcomes of the individuals in the treatment group might have been affected by the intervention. Significance levels: *** 1%, ** 5%, * 10%.

4 Results

In the first row of Table 3 we present IV-estimates of the effects of taking part in the assignment on our outcome variables. The table also contains the reduced form and first stage estimates (in the second and third row, respectively). The reduced form estimates represent the (ITT) effect of being invited to take part in the assignment on the outcome variables. The first stage estimates represent the effect of being invited to take part in the assignment on the probability of actually taking part. Columns (1) and (3) contain the results without any student controls, whereas columns (2) and (4) present the results using all controls. All models include faculty×year×colour fixed effects, and robust standard errors are used.

[8] The latter two regressions are only estimated for the control group, as the outcomes of the individuals in the treatment group might have been affected by the intervention.

Columns (1) and (2) contain estimates of the effect of taking part in the assignment on the probability of withdrawing before the study programme commences (i.e. the probability that a subscriber will change his or her mind). Both the IV and reduced form estimates are close to zero and insignificant.[9] They are also robust to the inclusion of control variables.[10] Hence, we find no significant effect of taking part in a study choice assignment on the persistence of the choice of study.

Consequently, effects on average student achievement are not expected, because it is unlikely that the intervention has affected the *composition* of the group of withdrawers without having any effect on their *number*. In theory, however, the (observable and unobservable) characteristics of the group of students that actually enrol might have become more favourable in the treatment group than in the control group as a result of self-selection due to the intervention.

As a check, we therefore present estimates of the effect of taking part in the assignment on the (standardized) total number of credits obtained in columns (3) and (4). The results in the third column (without student controls) can be interpreted as the 'total' selection effect of the intervention, whereas the coefficients in the fourth column (including student controls) can be seen as measures of the effect of selection on unobservable characteristics. Both IV estimates are positive, but insignificant at conventional levels. This also holds for the reduced form estimates. Hence, both the ITT effects (as measured by the RF estimates) and the ATT effects (as measured by the IV estimates) are not statistically significantly different from zero.[11] Moreover, the fact that the point estimates hardly change once we include student controls suggests that the net effect of selection on *observables* is (very close to) zero.[12]

In the appendix we estimate separate regressions for each faculty. Although the point estimates seem to vary across faculties, none of the coefficients are statistically significantly different from zero.

[9] The fact that the point estimates are very close to zero increases the confidence that the insignificance of our results is not due to lack of power.

[10] This is not surprising given the random assignment of the intervention.

[11] Our findings provide no reason to investigate the performance of the withdrawers who have switched to other study fields.

[12] We have also checked this in a different way. Using the coefficients from column (3) of Table 2, we predicted the total number of ECTS obtained for the students in the treatment group for whom we do not have any missing covariate data. Then, we compared the average predicted number of ECTS for all students in the treatment group with that for the subset of students in the treatment group who did eventually enrol. It turns out that the students in the "post-withdrawal" group are predicted to obtain slightly *fewer* credits than the students in the "full" sample on the basis of their background characteristics (44.2 versus 45.8, respectively), thus suggesting that there has been no (positive) selection on observables.

Table 3: Estimates of the effects of study choice activities

	Withdrawal rate		Number of ECTS	
	(1)	(2)	(3)	(4)
Effect of intervention (IV)	-0.002	-0.003	0.099	0.104
	(0.046)	(0.043)	(0.110)	(0.111)
Reduced form	-0.001	-0.002	0.058	0.061
	(0.025)	(0.023)	(0.065)	(0.065)
First stage	0.536***	0.540***	0.584***	0.581***
	(0.018)	(0.018)	(0.022)	(0.022)
Number of observations	1,307	1,307	873	873
R^2 (IV)	0.088	0.215	0.099	0.134
student controls	no	yes	no	yes
fixed effects	yes	yes	yes	yes
$\overline{y}$-control	0.332			
sd(y)	0.471			

Notes: Included student controls are: gender, age, nationality, education level, high school GPA, special circumstances, plans to move out of parents' house, study of first choice, number of orientation activities undertaken, expected number of ECTS, and expected number of hours devoted to studying. The number of ECTS has been standardized to have mean 0 and standard deviation 1. Robust standard errors are in parentheses. All models include faculty×year×colour fixed effects. Significance levels: *** 1%, ** 5%, * 10%.

5 Conclusion

In this paper we determined the causal effects of an intervention aiming to reduce the drop-out rate during the early stages of post-secondary education by improving the match between students and their study field of choice.

Over the course of one or two years, three Dutch university faculties participated in a randomized controlled trial in which half of the subscribers were invited to take part in an assignment. The assignments, which were offered in addition to regular matching activities, were representative of the content of the first year of the study field and were intended to increase insight in the curriculum of the field of study of their choice. Because the intervention provided additional information that could be used in the process of decision making regarding the actual enrolment into the study field, we expected that a possible effect of the intervention would run through self-selection into (or not into) the study field.

In order to estimate the causal effect of the intervention on the persistence of the study field choice and on student achievement, we employed a two-stage least squares (IV) approach where we used the random assignment of the intervention as an instrument for taking part in the assignment. We argue that the intervention had no significant effect on the probability of withdrawal before the study commences. In line with this, we also find no significant effects of taking part in the assignment on the total number of credits (ECTS) obtained. Therefore, we conclude that offering a home assignment on top of the regular matching activities offered by Dutch universities does not affect the (self-)selection of subscribers.

We offer three possible explanations for our findings. First and most importantly, the intervention may have been too small to generate substantial selection effects. The assignments were relatively short and were neither checked nor marked. Second, the intervention was offered in addition to regular study choice activities. Perhaps for some participants the information gathered through the home assignment was not fully complementary to the information they already had collected during earlier study orientation activities. Third, given the nature of non-compliance, our results might be based on a selective subsample of subscribers. Recall that slightly less than half of the people who were assigned to the intervention did not actually take part in the online assignment. If this non-compliance is non-random, the estimated (causal)

effects of the intervention may differ from the effects one would find if *all* subscribers had taken part in the assignment. It is possible, for instance, that only the most motivated people in the treatment group made the effort to participate. If, at the same time, more motivated people are unlikely to withdraw their subscription to begin with, one would not expect to find any (selection) effects of taking part in this assignment. In that case, the estimates are not informative about the effects for subscribers with a higher *ex-ante* probability of withdrawing their subscription.[13]

Although we cannot exclude that other, more rigorous, interventions may generate positive effects on the quality of choice, our paper suggests that small-scale interventions such as a single assignment – provided on top of other study choice activities – are ineffective. Future research is needed to determine how rigorous interventions need to be in order to be effective.

Bibliography

Bettinger, E. P., Long, B. T., Oreopoulos, P., and Sanbonmatsu, L. (2012). The role of application assistance and information in college decisions: Results from the H&R Block FAFSA experiment. *The Quarterly Journal of Economics*, 127(3):1205–1242.

Broek, A. v. d., Wartenbergh, F., Bendig-Jacobs, J., Braam, C., and Nooij, J. (2015). Monitor Beleidsmaatregelen 2015: Studiekeuze, studiegedrag en leengedrag in relatie tot beleidsmaatregelen in het hoger onderwijs, 2006-2015.

Kerr, S. P., Pekkarinen, T., Sarvimaki, M., and Uusitalo, R. (2015). Post-secondary education and information on labor market prospects: A randomized field experiment. IZA Discussion Paper 9372.

McGuigan, M., McNally, S., and Wyness, G. (2012). Student Awareness of Costs and Benefits of Educational Decisions: Effects of an Information Campaign. CEE Discussion Paper 139.

Reumer, C. and Van der Wende, M. (2010). Excellence and Diversity: The Emergence of Selective Admission Policies in Dutch Higher Education - A Case Study on Amsterdam University College. University of Berkeley: Center for Studies in Higher Education. Research and Occasional Papers Series: CSHE.15.10.

[13] Tentative analysis, however, suggests that the (*ex-ante*) withdrawal probabilities do not seem to differ between compliers and non-compliers on average. We performed a similar exercise as in footnote 11; results available upon request.

Appendix

Table 4: Summary statistics of covariates

	Mean	Sd	N
	(1)	(2)	(3)
Male	0.246	0.431	890
Age	19.61	2.451	890
Dutch-born	0.657	0.475	891
Pre-university high school (*vwo*) graduate	0.909	0.288	890
High school GPA 6.6-7.5	0.488	0.500	656
High school GPA $\geq$ 7.6	0.104	0.305	656
Number of orientation activities undertaken	2.094	0.985	891
Study of first choice	0.703	0.457	890
Plans to move out of parents' house	0.817	0.387	832
Expects to earn $\geq$ 50 ECTS in first year	0.676	0.468	888
Expects to devote 21-30 hours a week to studying	0.545	0.498	886
Expects to devote $>$ 30 hours a week to studying	0.219	0.414	886
Special circumstances	0.103	0.304	885

Notes: Column (3) contains the number of non-missing observations for each covariate.

Table 5: Covariates table FASoS

	Assigned to intervention		Withdrawal (control group)		Number of ECTS (control group)	
	(1)		(2)		(3)	
	coeff	se	coeff	se	coeff	se
Male	0.075	(0.071)	0.021	(0.086)	-9.950	-6.100
Age	0.001	(0.016)	-0.019	(0.021)	-1.435	-1.549
Dutch-born	0.003	(0.078)	-0.142	(0.094)	-4.417	-6.627
Pre-university high school (*vwo*) graduate	-0.066	(0.139)	0.152	(0.153)	-3.124	-9.847
High school GPA 6.6-7.5	-0.070	(0.095)	-0.228*	(0.124)	-7.016	-8.479
High school GPA $\geq$ 7.6	-0.128	(0.120)	-0.108	(0.159)	-10.343	-11.146
Number of orientation activities undertaken	0.011	(0.033)	-0.070*	(0.038)	2.145	-2.687
Study of first choice	-0.164	(0.130)	-0.378*	(0.224)	-5.460	-11.426
Plans to move out of parents' house	-0.006	(0.080)	0.073	(0.103)	9.516	-5.911
Expects to earn $\geq$ 50 ECTS in first year	-0.012	(0.070)	0.062	(0.081)	2.866	-5.378
Expects to devote 21-30 hours a week to studying	-0.005	(0.090)	-0.013	(0.115)	4.909	-8.257
Expects to devote $>$ 30 hours a week to studying	0.012	(0.095)	-0.092	(0.119)	0.750	-8.040
Special circumstances	0.085	(0.104)	-0.096	(0.141)	1.078	-9.054
Number of observations	261		126		90	
P-value (F)	0.907		0.008		0.326	
Fixed effects	yes		yes		yes	
$\overline{y}$	0.526		0.307		38.304	

Notes: Each column is a regression. Robust standard errors are in parentheses. All models include year×colour fixed effects. The number of observations of the regressions relates to the subsample of subscribers for whom we do not have any missing covariate data. The regressions in columns 2 and 3 are estimated for the control group only, as the outcomes of the individuals in the treatment group might have been affected by the intervention. Significance levels: *** 1%, ** 5%, * 10%.

Table 6: Covariates table FHML

	Assigned to intervention		Withdrawal (control group)		Number of ECTS (control group)	
	(1)		(2)		(3)	
	coeff	se	coeff	se	coeff	se
Male	0.067	(0.075)	0.039	(0.111)	-7.462	-6.569
Age	-0.007	(0.034)	-0.021	(0.047)	-1.369	-1.753
Dutch-born	-0.231	(0.220)	0.345*	(0.206)	-23.754	-14.484
High school GPA 6.6-7.5	-0.041	(0.059)	0.071	(0.074)	2.760	-3.193
High school GPA $\geq$ 7.6	-0.092	(0.143)	0.084	(0.118)	-14.631	-13.094
Number of orientation activities undertaken	-0.035	(0.032)	-0.145***	(0.038)	0.429	-1.616
Study of first choice	0.025	(0.058)	-0.376***	(0.081)	-4.264	-3.024
Plans to move out of parents' house	0.004	(0.066)	-0.058	(0.078)	-1.529	-4.050
Expects to earn $\geq$ 50 ECTS in first year	0.106	(0.066)	0.027	(0.080)	2.768	-4.642
Expects to devote 21-30 hours a week to studying	0.052	(0.065)	-0.046	(0.086)	1.686	-4.143
Expects to devote $>$ 30 hours a week to studying	-0.196	(0.125)	-0.085	(0.136)	2.066	-6.767
Special circumstances	-0.060	(0.096)	-0.035	(0.106)	-2.470	-5.707
Number of observations	339		172		85	
P-value (F)	0.468		0.000		0.181	
$\overline{y}$	0.501		0.492		53.833	

Notes: Each column is a regression. Robust standard errors are in parentheses. The number of observations of the regressions relates to the subsample of subscribers for whom we do not have any missing covariate data. The regressions in columns 2 and 3 are estimated for the control group only, as the outcomes of the individuals in the treatment group might have been affected by the intervention. Significance levels: *** 1%, ** 5%, * 10%.

Table 7: Estimates of the effects of study choice activities (FASoS)

	Withdrawal rate		Number of ECTS	
	(1)	(2)	(3)	(4)
Effect of intervention (IV)	0.088	0.076	0.189	0.211
	(0.084)	(0.080)	(0.169)	(0.167)
Reduced form	0.048	0.042	0.133	0.150
	(0.045)	(0.044)	(0.120)	(0.119)
First stage	0.543***	0.558***	0.705***	0.712***
	(0.034)	(0.034)	(0.038)	(0.038)
Number of observations	420	420	280	280
R^2 (IV)	0.007	0.104	0.026	0.091
student controls	no	yes	no	yes
fixed effects	yes	yes	yes	yes
$\overline{y}$-control	0.307			
sd(y)	0.472			

Notes: Included student controls are: gender, age, nationality, education level, special circumstances, plans to move out of parents' house, study of first choice, number of orientation activities undertaken, expected number of ECTS, and expected number of hours devoted to studying. The number of ECTS has been standardized to have mean 0 and standard deviation 1. Robust standard errors are in parentheses. All models include year×colour fixed effects. Significance levels: *** 1%, ** 5%, * 10%.

Table 8: Estimates of the effects of study choice activities (FHML)

	Withdrawal rate		Number of ECTS	
	(1)	(2)	(3)	(4)
Effect of intervention (IV)	-0.046	-0.049	0.106	0.074
	(0.059)	(0.051)	(0.144)	(0.140)
Reduced form	-0.036	-0.039	0.094	0.066
	(0.046)	(0.041)	(0.127)	(0.124)
First stage	0.781***	0.787***	0.884***	0.882***
	(0.027)	(0.027)	(0.028)	(0.030)
Number of observations	473	473	249	249
R^2 (IV)	0.011	0.291	0.005	0.113
student controls	no	yes	no	yes
$\overline{y}$-control	0.492			
sd(y)	0.500			

Notes: Included student controls are: gender, age, nationality, education level, special circumstances, plans to move out of parents' house, study of first choice, number of orientation activities undertaken, expected number of ECTS, and expected number of hours devoted to studying. The number of ECTS has been standardized to have mean 0 and standard deviation 1. Robust standard errors are in parentheses. Significance levels: *** 1%, ** 5%, * 10%.

Table 9: Estimates of the effects of study choice activities (TiSEM)

	Withdrawal rate	Number of ECTS
	(1)	(2)
Effect of intervention (IV)	-0.045	-0.088
	(0.147)	(0.401)
Reduced form	-0.011	-0.024
	(0.037)	(0.108)
First stage	0.250***	0.270***
	(0.030)	(0.034)
Number of observations	414	344
R^2 (IV)	0.004	0.000
student controls	no	no
$\overline{y}$-control	0.175	
sd(y)	0.375	

Notes: The number of ECTS has been standardized to have mean 0 and standard deviation 1. Robust standard errors are in parentheses. Significance levels: *** 1%, ** 5%, * 10%.

Publisher:

CPB Netherlands Bureau for Economic Policy Analysis
P.O. Box 80510 | 2508 GM The Hague
T (088) 984 60 00

December 2016 | ISBN 978-90-5833-753-5