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Does intensive coaching reduce school dropout? Evidence from a randomized experiment

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Evidence from a randomized experiment[1]

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Abstract

This paper investigates the effect of coaching in vocational education on school dropout using data from a randomized experiment. We find that one year of coaching reduces school dropout by more than 40 percent from 17 to 10 percentage points. The reduction in school dropout results from two equally important channels: a reduction of dropout from the study and a reduction of dropout from the education system once students dropped out of their studies. This suggests that coaching interventions before as well as after study dropout have contributed to less school dropout. The effectiveness of coaching is largest for students with a high ex ante probability of dropout, such as older students, males and students with an adverse socioeconomic background. A cost-benefit analysis suggests that one year of coaching is likely to yield a net social gain.

JEL Codes: I2, H43
Keywords: school dropout, randomized experiment, coaching, impact evaluation

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1. Introduction

School dropout is a serious problem which carries large costs to society and to individuals. A large literature documents the negative effects of school dropout (or lower educational attainment), for instance on life time wage income (Card, 1999; Harmon et al., 2003; Heckman et al., 2006), labor-market participation (Van Elk et al. 2012), health (Oreopoulos, 2007; Lleras-Muney, 2005), crime (Lochner & Moretti, 2004; Machin et al., 2011; Webbink et al., 2012), and educational attainment of the children of dropouts (Oreopoulos et al., 2006). Despite these large negative consequences, evidence on the effectiveness of policies or programs to reduce school dropout is limited.

This paper investigates the effect of an intensive coaching program aimed at reducing school dropout in the Netherlands.[3] Students in intermediate vocational education were randomly assigned to classes that were subject to the coaching program or to classes that received usual care. The coaching program contained a range of mostly preventive activities and was implemented such that two coaches were assigned to a class of on average twenty students. Examples of coaching are working on study skills (e.g., planning and organizing), counseling in case of personal problems and contact with parents. The target population was first- and second year students in intermediate vocational education in the Netherlands. These students are aged mainly between 16 and 20.

The coaching program shares several elements with mentoring programs studied in the literature, such as assignment of a coach/mentor with a strong personal and supportive approach, a large intensity of student-coach interactions and activities, and a focus on students still enrolled in school. However, the context, timing and target group of this program is clearly different. Whereas previous studies mainly studied interventions at middle or high school level, this program focused on students starting in (intermediate) vocational education. This brings in the quality of study choice as an additional factor related to school dropout. For many students the choice for the field of study appears to be a difficult decision. A particular element of the coaching program was to provide timely and intensive counseling when it turned out the choice for the field (or level) of study had not been the right one. The target group of students was the general population of students starting in intermediate vocational education, whereas previous studies mostly focused on students with disadvantaged or lower socioeconomic backgrounds.[4] The target group of students in the Dutch program includes students both under and above the statutory school-leaving age. This enables a comparison of program effects by compulsory schooling status.

Examples of mentoring or coaching programs that were found to generate positive results are Big Brothers/Big Sisters (Tierney et al., 1995), Sponsor-A-Scholar (Johnson, 1999), and the Quantum Opportunities Program

[3] The Netherlands is a country with rather average school dropout rates among Northern European countries. The percentage of 18-24 year olds not in education with at most having attained lower secondary education is in the same range as countries like Sweden, Finland, Denmark, Ireland, Austria, Belgium and Germany (source: http://europa.eu/rapid/pressReleasesAction.do?reference=IP/12/577&format=HTML&aged=0&language=EN&guiLanguage=en).

[4] This means average school dropout rates are lower in this experiment than in previous mentoring studies in the 'care-as-usual' situation. For instance, school dropout rate in the US Quantum Opportunities Program amounted to 50 percent and in the Education Maintenance Allowance control areas to 36 percent, whereas this rate in the Dutch coaching experiment was less than 20 percent on average.

(Hahn et al., 1994; Taggart, 1995; Maxfield et al., 2003). These programs were all carried out in the United States and particularly targeted disadvantaged youth, still enrolled in school. Both Sponsor-a-Scholar and the Quantum Opportunity program contained financial incentives for students.[5] Suggestive evidence for the promise of a strong personal and intensive approach comes from the work of Dynarski et al. (1998) as well. They evaluated twenty dropout prevention programs carried out at middle and high school level in the United States under the *School Dropout Demonstration Assistance Program*.[6] They conclude that whereas most programs did not (or just very little) reduce school dropout or improve school performance, the most promising programs were programs at the middle-school level that were characterized by an intensive and personal approach in smaller groups. Carneiro & Heckman (2003) review a number of evaluations of dropout prevention programs in the United States. They conclude that these studies suggest that sustained interventions targeted at adolescents still enrolled in school can positively impact learning and subsequent employment and earnings, but that interventions targeted at dropouts seem much less successful.

Other evidence on school dropout policies is in the area of financial incentives for students and legal measures. Regarding the first type, means-tested conditional grants have been found effective in a number of countries. The large-scale Education Maintenance Allowance (EMA) in the United Kingdom is an example of such a program, subsidizing children to remain in school for up to two years beyond the official school-leaving age of 16. Dearden et al. (2009) find that EMA raised enrollment rates by about 5 percentage points among 16 years old and 7 percentage points among 17 years old.[7] Legal measures point to increases in statutory minimum school leaving ages that have been carried out in several developed countries throughout the 20th century. Its impact on educational attainment and other outcomes such as wages has been studied for several countries with mixed findings (e.g. Harmon & Walker (1995) for the United Kingdom; Angrist and Krueger (1991), Goldin (1999) and Goldin & Katz (2011) for the United States; and Meghir & Palme (2005) for Sweden).

Our main finding is that the intensive coaching program has a substantial effect on school dropout.[8] One year of coaching reduces school dropout with 40 percent. In terms of dropout levels this equals a fall from 17 to 10 percent. Effects after two years of coaching are only slightly larger in magnitude. This suggests that the largest gains are made in the first year of coaching. Effects are largest for students with a larger ex-ante probability of school dropout, among which are men, students not living with both parents, students above the statutory school-leaving age[9] and students choosing very late to do the particular field of study. Targeting the program towards these groups and on first-year students will probably improve the cost-effectiveness of the program.

[5] In QOP, students could earn two dollars for every hour spent on improvement of personal development or social skills. It was a four-year program with 250 hours of activities and services offered per year. SAS provided financial assistance of 6,000 dollar for those choosing to enroll in post-secondary education (college).
[6] Sixteen of them had an experimental setup with treatment being assigned randomly.
[7] It remains unclear whether the impact of the policy is due to credit constraints or due to an unconstrained price effect. Conditonal cash transfer programs have been evaluated in developing countries as well, such as Mexico and Columbia. Schultz (2004) & Attanasio et al. (2010) find positive effects for a means-tested conditional grants program named PROGRESA in Mexico. Attenasio et al. (2010) find positive effects on enrollment rates of 14-17 year olds of about 5-7 percentage points of a program in rural Columbia. Both studies were based on randomized trials.
[8] School dropout is defined as leaving education without a so-called "startkwalificatie" (start qualification), which is equal to ISCED level 3 (upper secondary education or intermediate vocational education).
[9] The statutory school-leaving age in the Netherlands is 18 for all students not yet having reached the so-called start qualification level or "startkwalificatieniveau" (i.e. equal to ISCED level 3).

Beyond estimating the effect of coaching on school dropout, it is also important to get insight into the channels through which the program reduces dropout from education. We find that the coaching program reduced dropout from the particular field of study in which the student started (from 38 to 30 percent) as well as the probability of complete dropout from education among the ones that decided not to continue with their particular study (from 44 to 26 percent). This suggests that both preventive (before study dropout) and curative actions (among study dropouts) of the coaches have been effective and together resulted in a substantial reduction of school dropout. We find that both channels were equally important in reducing school dropout.

Tentative cost-benefit calculations suggest that one year of intensive coaching yields a net social gain whereas two years of subsequent coaching probably does not, at least not in its current form. The internal rate of return of one year of coaching is calculated at 6.9 percent, whereas that of two years of coaching is calculated at 3.7 percent.

The remainder of this paper is organized as follows. Section 2 discusses the setup of the experiment, gives a program description and discusses the empirical strategy for the effect estimations. Section 3 describes the data. The main estimation results follow in section 4. Section 5 discusses the plausible channels behind the effects on school dropout. Section 6 reports effects of two year of coaching on school dropout and degree completion. Section 7 shows the effect of the coaching for various subgroups, such as males versus females. Section 8 provides a back-of-the-envelope cost-benefit analysis. Finally, section 9 concludes and discusses the implications of our findings.

2. The coaching experiment and empirical strategy

2.1 Setup of the coaching experiment

The setup of the coaching experiment is summarized in figure 1. The experiment was carried out in two subsequent cohorts of first year students in a school for intermediate vocational education in a medium-sized city in the Netherlands. This school had average school dropout rates.[10] The experiment started in school year 2009-2010 with a first cohort of students receiving two years of coaching, which is equal to the nominal study duration at the level of implementation.[11] This was followed by a second cohort receiving one year of coaching. The total number of participating students in both cohorts was 450.

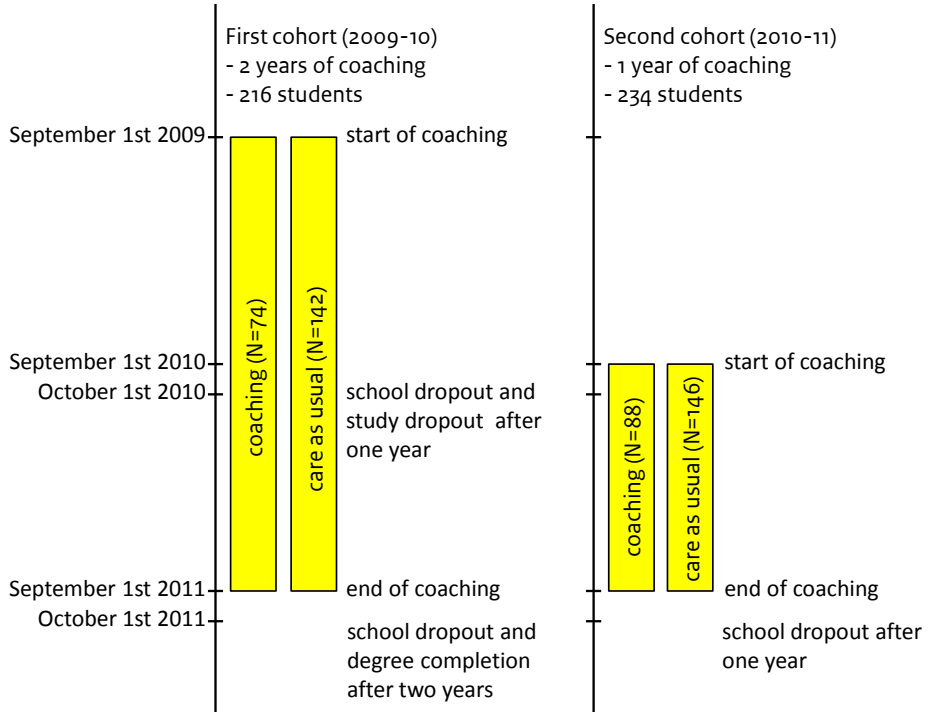
Students were randomly assigned to an experimental and a control condition. The experimental groups were offered the intensive coaching program provided by a full-time equivalent of coaching per class (in most cases two part-time coaches per class), whereas the control groups received 'care as usual'. The most notable part of this 'care as usual' consists of curative actions by a dropout desk that comes into action after it turns out that a student has dropped out from school.

[10] The participating school had the 17th highest school dropout rate among the 42 schools offering intermediate vocational education in the Netherlands.

[11] The experiment was implemented at level 2 of intermediate vocational education, of which completion yields a so-called 'start qualification'. This is equal to ISCED level 3. Leaving education without a start qualification is labeled 'voortijdig schoolverlaten' or early school leaving and government targets are set to reduce the number of early school leavers. We refer to Appendix B for more information on school dropout in the Netherlands.

Four different fields of studies were involved in the experiment within each cohort.[12] The main criterion for participation in the experiment was that these fields of education had enough applications to split the sample into at least two classes of students, such that an experimental group and at least one control group could be constructed. Average degree completion rates of the participating fields of education were very close to average degree completion rates of all studies offered at this level. This indicates that a representative subset of studies was involved in the experiment.

Figure 1 setup of coaching experiment, outcome measures and measurement moments



Random assignment of individual students to classes took place among the complete list of applicants within each field of study. The Ministry of Education provided budget for one experimental class within each of four different studies per cohort. This implied that for each involved study one or more control classes were constructed depending on the total amount of applicants per study. The total number of classes participating in the experiment was 23, of which 8 were experimental classes and 15 were control classes. Groups of similar size were constructed. Since randomization was carried out just before the start of the school year we were able to include the (rather) late applicants in our randomization sample as well. The program was announced to students

[12] These studies are "Helpende Zorg en Welzijn" (health care), "Uiterlijke Verzorging" (hairdressing), "Horeca" (cook) and " Verkoper" (sales). In the second cohort, "Verkoper" was replaced by "Beveiliger" (security) because the former study did not have enough applications to split the sample into two groups of different treatment status. Average degree completion rates of the participating studies are very close to average degree completion rates of all studies offered at this level at Rijnijssel, indicating that a representative subset of studies participated in the experiment.

and parents just after the start of the study year. The timing of the announcement implies that the coaching program cannot have affected study choice.[13]

Table 1 shows the number of students involved in the experiment by treatment status for the pooled sample and separately by cohort. It shows that a little over 500 students were in the pooled randomization sample of all applicants, divided over the two cohorts of roughly equal size. Around 10 percent of all applicants in the randomization sample did not start in the study they applied for. Most of these 'non-starters' chose a different study in the same school or at another school.[14] The percentage of starters in the participating studies is somewhat larger in the treated groups (four percentage point difference with the non-treated groups). This difference is not statistically significant at a 10-percent significance level (p-value of 0.15).

The coaching experiment was funded by the Ministry of Education at a total intervention cost of 720k euro. These costs consist of 60 k euro per full-time equivalent of coaching per class per year, or 3k euro per student per year.[15] We refer to Appendix A for some more context on school dropout in the Netherlands as well as on the background of the experiment.

Table 1 Applicants and starters in participating studies by assignment status.

	Controls (=care as usual)	Treated (=coaching program)	Total
A) Pooled sample (2 cohorts)			
Applicants	327	176	503
Starters in participating studies	288	162	450
(% of applicants)	(88%)	(92%)	(89%)
B) First cohort (2009-10 cohort)			
Applicants	166	81	247
Starters	142	74	216
(% of applicants)	(86%)	(91%)	(87%)
C) Second cohort (2010-11 cohort)			
Applicants	161	95	256
Starters	146	88	234
(% of applicants)	(91%)	(93%)	(91%)

[13] If the program would have been announced earlier, we would have faced the risk of selective applications for the participating studies in the experiment. This could have had negative implications for at least external validity of the effect estimates.

[14] Of all non-starters in the participating studies 94 percent was still in education in the beginning of the school year. Within this group 83 percent started in intermediate vocational education, of which70 percent at the same level and 20 percent at a higher level. Three out of four non-starters chose a different study at the same school, whereas the remaining part went to another school.

[15] The four groups in the first cohort received two years of coaching at a total cost of 480 k euro and the four groups in the second year received one year of coaching at a total cost of 240 k euro.

2.2 The coaching program

The coaching program consisted of various types of interventions, both preventive as well as curative (i.e. among students dropping out from their particular study). The following preventive interventions were part of the coaching program:

- One or more intake sessions with all students to get to know each other, detect possible problems and to make arrangements for various tracks. Different guidance tracks were initiated, for example in the field of dyslexia, fear of failure, social skills, self-confidence or study skills. Coaches also gave guidance in case of financial problems or problems with housing;
- A home visit takes place in the first month of the study with all the students in order to get to know the parents or caretakers and reduce the distance between home and school. Later contacts with parents or caretakers were possible if needed;
- The coach gives instruction on and helps with study planning and organizing with a focus on stimulating self-reliance;
- The coach regularly attends lessons of the treated classes to observe the students and to give them study support if needed after the lessons. The coach informs the study teams regularly in formal team meetings to inform them and gear guidance initiatives to one another.
- The coach visits the students at their internship/apprenticeship. This was not targeted to formal evaluation, but towards observing problems with work or social skills, and if needed initiate extra training for improving these skills. The coach has an active role in obtaining a good match between the student and the company at which the internship takes place;
- In case of school absence the coach immediately contacts the student and/or parents to discuss the reasons. If needed, the coach sets up actions to prevent further study absence.

In case a student is on the verge of dropping out the coach tries to guide the student to another study by setting up an intensive study choice track. This track consists of talks, testing, guidance to another study and checking whether the student is accepted and started at his or her new study.

All the above interventions were carried out by two part-time coaches per class, adding up to a full-time equivalent of capacity available per class.[16] The coaches had 18 years of experience in education on average, of which 8 years at the school were the experiment took place. All coaches, except one, possessed a higher education degree. Almost 60 percent of the coaches were teachers before they started their job as a coach in the experiment.

A local project coordinator had the task to communicate randomization outcomes and ‘rules’ of the experiment to the involved studies, to organize data collection and delivery, and to monitor the experiment. This coordinator also organized regular meetings among the coaches in which coaches could discuss cases with each other, and in which particular themes were addressed aimed at improving expertise of the coaches. These meetings ensured that the coaches worked with the same vision and methodology across the different studies, though the particular interventions could differ along with differences in the situation across the studies and their student populations.

[16] Only one experimental group had a full-time coach instead of two part-time coaches. The average reported weekly number of hours devoted to coaching was 23 per coach per group.

2.3 Empirical strategy

The random assignment of students to the treatment and control group enables us to estimate the effect of the program with the following regression equation using OLS[17]:

$$(1) \qquad DROPOUT_{ics} = \beta_o + \beta_1 Coaching_{ics} + \beta_2 X_{ics} + \beta_3 Cohort_{cs} + \alpha_s + \varepsilon_{ics} ,$$

where $DROPOUT_{ics}$ is a dummy variable that states whether a student *i* from class *c* of study *s* is a school dropout, $Coaching_{ics}$ is a dummy variable indicating whether a student in class *c* and study *s* is assigned to receive the coaching program, X_{ics} is a vector of observable characteristics of the student, Cohort is a dummy indicating whether a student is part of the first or the second cohort, and α_s is a separate dummy for each study.

The random assignment of students ensures that selection bias will not be a concern. Inspection of the covariates of the treatment and control groups shows that the groups are similar in terms of observable characteristics (see next section). Not all applicants that were assigned to a treatment or control group actually started in the field of education they applied for. This could bias our estimates if the decision to actually start could be affected by the treatment status. However, due to the design of the experiment this is not possible. Students were informed about the coaching program only after the start of the school year. The decision not to start in the study they applied for but not started in was made before the announcement of the coaching program. In the analysis we excluded those applicants that applied but never showed up in the participating studies (that is, 11 percent of all applicants).[18] The difference in the incidence of non-starting by treatment status is small and not significant (see table 1). As a sensitivity check we repeat the main analysis on the sample of all applicants and instrument actual treatment (i.e. receiving coaching) with random assignment among all applicants.[19]

Unlike non-starting of applicants, switching of students between experimental and control groups during the experiment would have been an issue to address if it would have occurred, but did not take place in this experiment. It was made very clear that students were not allowed to switch between the control and experimental groups during the experiment, because this could distort the random setup and thereby the credibility of the effect estimates.[20] Regular status updates enabled us to keep track of the position of students in the participating studies and groups during the experiment. These updates did not indicate any switching across treatment status.

Another issue which could potentially bias results in field experiments is spillover effects from experimental groups to control groups. We argue these spillovers are not very likely to have occurred in this experiment due to the very nature of the intervention. The students in the experimental group received personalized coaching

[18] This non-starting is a rather normal phenomenon since students may apply for more than one study at the same school or at different schools.

[19] It should be noted that including non-starters in the participating studies into the analysis may introduce some noise or bias in the estimations because of the possibility that the quality or quantity of care or difficulty of the other studies (and schools) 'non-starters' went to may differ from the participating studies. These differences may have affected dropout rates of non-starters. In the sample of starters on which all main analyses in the paper are based this noise is absent since all other conditions except coaching are expected to be equal across those assigned to coaching and those not.

[20] In two cases permission was asked by a study coordinator to move a particular student from the control to the experimental group because of extra care needs, but this request has not been granted.

and counseling which was not in any way made available for students in the control group. Moreover, control groups and experimental groups had their own schedule and interactions among students took place mostly within their class. Nonetheless, if any spillovers would have occurred, we would expect them to be positive, which would indicate that the effects we find are a lower bound.

3. Data description

The data come from various sources. With regard to outcome variables, data on school dropout and graduation are collected from a national database called BRON, in which information on school careers of all students has been collected. Data on whether or not a student dropped out from the particular study he or she started with are based on monitoring by the school at agreed points in time.[21]

We employ a rich set of student background characteristics and personal situation from various data sources: BRON (highest previously attained education), the school's central administration (country of birth, birth date, sex), intake tests taken before the start of the school year (providing information on cognitive skill levels), and a student survey (degree of personal problems, living situation) carried out among all participating students (both treated and controls) at the very beginning of the experiment.

Table 2 presents sample means by treatment status for the various participating studies for several variables. The sample contains students that actually started in the studies they applied for, which will be the group on which we carry out all analyses in the rest of this paper. The table shows that for all studies treatment and control groups are well comparable on a broad range of characteristics. Only 2 out of 55 within-study differences are statistically significant.[22] Hence, the randomization produced relatively similar groups within each study and cohort. This conclusion is supported by the comparison of sample means for the pooled sample of all studies and both cohorts (see last three columns of panel C), which shows that none of the observed differences are significant after having controlled for field of study and cohort.[23] We will control for field of study and cohort in our later effect estimations.

Table 3 gives a first impression of differences in school dropout and study dropout (between coached and non-coached students, one year after the start of the experiment. The share of school dropouts is more than twice as large in the classes that were not offered intensive coaching (see row 1). This difference is statistically significant tested at a 1%-percent level, even after controlling for cohort and study. Study dropout also occurs more often in the non-coached classes, the difference being 12 percentage points which is statistically significant from zero at a 5%-significance level. Study dropout occurs more often than school dropout. For instance, nearly four out of ten control students have dropped out from their study one year after the start

[21] These study dropout figures have been double-checked by the BRON data which were collected at the end of the project.
[22] These are the fraction of men in Uiterlijke Verzorging (Hairdressing) and highest previous attained degree in Verkoper (Sales).
[23] The most notable difference is in the fraction of males which is higher in the treated groups on average. This difference is due to a larger number of control students in the two studies with relatively high shares of females (i.e. Uiterlijke Verzorging and Helpende Zorg en Welzijn). These two studies had a relatively large number of applicants and therefore had two or more control groups against the standard one experimental group per study.

whereas less than half of these study dropouts have completely dropped out from education. The remaining study dropouts stayed in education, but switched to another study.

Table 2 Comparison of mean characteristics of treatment and control groups, sample of starters of cohorts 2009-10 and 2010-11, by field of study and all studies pooled.

	1) Health care (Helpende Zorg en Welzijn)			2) Hairdressing (Uiterlijke verzorging)		
	Control	Treated	P-value[a]	Control	Treated	P-value[a]
1. Male	0.09	0.08	0.72	0.13	0.02	0.05
2. Age (in years)	18.7	18.8	0.88	17.9	17.7	0.64
3. Under school leaving age or start qualification duty[b]	0.33	0.37	0.70	0.50	0.61	0.25
4. Born in the Netherlands	0.90	0.90	0.88	0.92	0.89	0.72
5. Living with both parents	0.50	0.42	0.43	0.50	0.64	0.15
6. Having problems to some degree in at least one of the following areas: finance, police and justice, family and friends, or living/housing situation	0.45	0.30	0.13	0.37	0.41	0.92
7. Score on verbal skills at intake test (1-5, 5=highest)	3.2	3.2	0.98	3.2	3.5	0.12
8. Score on numeric skills at intake test (1-5, 5=highest)	2.8	2.7	0.44	3.1	2.9	0.26
9. Highest previous attained degree (1-6, 6=highest)	2.4	2.4	1.00	2.4	2.4	0.84
10. Already having obtained a start qualification at start of experiment	0.02	0.03	0.68	0.08	0.04	0.35
11. Late choice for this particular study (July or later)	0.28	0.29	0.91	0.18	0.27	0.21
Number of observations	115	38	153	99	46	145

(Continued)	3) Cook and catering (Horeca)			4) Security (Beveiliger)[c]		
	Control	Treated	P-value[a]	Control	Treated	P-value
1. Male	0.86	0.87	0.95	0.81	0.79	0.88
2. Age (in years)	17.8	17.6	0.55	18.6	18.0	0.23
3. Under school leaving age or start qualification duty[b]	0.57	0.50	0.49	0.33	0.46	0.40
4. Born in the Netherlands	1.00	0.95	0.17	0.95	0.96	0.93
5. Living with both parents	0.83	0.71	0.17	0.38	0.54	0.30
6. Having problems to some degree in at least one of the following areas: finance, police and justice, family and friends, or living/housing situation	0.31	0.50	0.12	0.67	0.48	0.22
7. Score on verbal skills at intake test (1-5, 5=highest)	3.4	3.8	0.14	4.0	3.8	0.21
8. Score on numeric skills at intake test (1-5, 5=highest)	3.9	3.9	0.68	3.6	3.6	0.97
9. Highest previous attained degree (1-6, 6=highest)	2.5	2.3	0.49	2.6	2.6	0.96
10. Already having obtained a start qualification at start of experiment	0.08	0.03	0.33	0.05	0.00	0.33
11. Late choice for this particular study (July or later)	0.25	0.18	0.52	0.24	0.26	0.87
Number of observations	37	38	75	21	24	45

Table 2 (continued)

	5) Verkoper (Sales)[c]			All studies pooled		
	Control	Treated	P-value	Control	Treated	P-value[d]
1. Male	0.75	0.75	1.00	0.29	0.42	0.23
2. Age (in years)	17.7	17.5	0.54	18.3	18.0	0.39
3. Under school leaving age or start qualification duty[b]	0.56	0.81	0.14	0.43	0.52	0.17
4. Born in the Netherlands	0.73	0.94	0.14	0.91	0.92	0.91
5. Living with both parents	0.75	0.50	0.15	0.55	0.58	0.90
6. Having problems to some degree in at least one of the following areas: finance, police and justice, family and friends, or living/housing situation	0.27	0.19	0.61	0.41	0.39	0.63
7. Score on verbal skills at intake test (1-5, 5=highest)	2.7	2.8	0.83	3.3	3.5	0.19
8. Score on numeric skills at intake test (1-5, 5=highest)	3.1	2.9	0.42	3.1	3.2	0.39
9. Highest previous attained degree (1-6, 6= highest)	1.7	2.4	0.05	2.4	2.4	0.54
10. Already having obtained a start qualification at start of experiment	0.00	0.07	0.33	0.05	0.03	0.31
11. Late choice for this particular study (July or later)	0.40	0.50	0.60	0.25	0.28	0.51
Number of observations	16	16	32	288	162	450

Notes:

A missing value on the background characteristics is limited to maximum six percent of the pooled sample.

(a) P-value is adjusted for cohort.

(b) All students under 16 are obliged to go to school. Students of 16 and 17 are obliged to stay in education if they have not completed a degree that counts as a 'start qualification' (i.e. ISCED level 3 or higher).

(c) Beveiliger (second cohort) and Verkoper (first cohort) have both been involved in one cohort instead of two in the other fields of studies.

(d) P-value is adjusted for cohort and study.

Table 3 Comparison of sample means of school dropout and study dropout one year after start, pooled sample of cohorts 2009-10 and 2010-11.

	Control group	Experimental group	P-value[a]	Adjusted P-value[a]
School dropout[b]	0.17	0.07	0.00	0.00
	(N=48)	(N=11)		
Study dropout[c]	0.38	0.26	0.01	0.02
	(N=110)	(N=42)		
Total number of observations	288	162		

Notes:

(a) The adjusted P-value is for an F-test on the null hypothesis that assignment to a coaching group has no effect on school dropout or study dropout, conditional on cohort and study.

(b) School dropout is defined as not being in education one year after the start of the experiment while not having obtained a 'start qualification' (ISCED level 3).

(c) Study dropout is defined as not being in the study anymore in which the student started one year after the start.

4. Main estimation results: effects on school dropout after one year

Regression results of the OLS estimates of coaching on the sample of starters are presented in table 4. Controlling for socioeconomic and personal characteristics reduces the effect estimate by about a quarter (see column 2 versus column 1). One of the factors contributing to this reduction is a difference in the share of students not under start qualification duty[24] anymore, which is somewhat lower in the coached groups (48 versus 57%). Not being under start qualification duty increases the probability of school dropout by 7 percentage points (conditional on all other covariates).[25]

Adding highest previous attained education and cognitive skills hardly change the effect estimate (see column 3 versus column 2). This is because the association of these two factors with the probability of school dropout is relatively small and differences between experimental and control group are limited here (see table 3).

The specification with all covariates shows that assignment to an intensive coaching class is associated with a 7 percentage point reduction in the probability of school dropout after one year (see column 3). This estimate corresponds to a substantial reduction of more than 40 percent in the school dropout rate caused by one year of intensive coaching, from 17 to 10 percent.

To put this effect estimate into perspective, Dearden e.a. (2009) find that a cash transfer in the UK (EMA) to 16-18 year olds reduced dropout rates (or increased enrollment rates) by 4.5 percentage points after one year of receiving EMA. Coming from a much higher baseline dropout rate of 35 percent, this is relatively seen a much smaller effect (i.e. 13 percent less dropout versus a little over 40 percent less in our coaching experiment). Hahn et al. (1994) find that the Quantum Opportunities Program in the US offering intensive counseling and financial incentives services to disadvantaged high school students halved dropout rates, also coming from a much higher base (50 percent). This program had larger effects (in absolute as well as in relative terms) than the effects we find here. At the same time, these effects were reached by a longer intervention period and at a higher cost: nearly 11k dollar per participant (1989 prices) for a four-year intervention period, as compared to a one-year intervention period at an average cost of 3k euro per student (2010 prices).

A sensitivity check in which we instrument actual treatment (i.e. being offered the coaching program) with random assignment among all applicants (including non-starting applicants in the participating studies) yields a slightly smaller effect estimate than the OLS estimate on the sample of starters in the participating studies. Controlling for cohort and field of study this instrumental variable (IV) approach yields a significant effect estimate of minus 8.2 percentage points (p-value of 0.01) relative to minus 9.6 percentage points in the main OLS analysis with the same two controls (cf. column 1 of table 4). [26] One explanation for this limited difference between the OLS and IV estimate seems that the school dropout rate among non-starting applicants that were

[24] A student is under start qualification duty if he or she is under 18 and did not reach a start qualification level yet, which is equal to ISCED level 3.

[25] Another factor contributing somewhat to the observed reduction in the effect estimate after being controlled for is whether a student reports personal problems (on at least one out of five areas) or not. The share reporting problems is a little bit smaller in the coached groups (39 versus 41 percent), whereas reporting problems is positively associated with the probability of school dropout (five percentage points higher probability).

[26] We did not perform two-stage-least-square analyses with larger sets of covariates (as in columns 2 and 3 of table 4) since the majority of added covariates in these columns were missing for the group of 'non-starters' as these are based on surveys that have not been undertaken among non-starting students.

assigned to a control group (but did never start there) was lower than that of starting controls (15 versus 17 percent).

In the remainder of this paper all effect estimations are OLS estimations carried out on the sample of starters in the participating studies.

Table 4 OLS effect estimates of coaching on school dropout after one year, pooled sample of cohorts 2009-10 and 2010-11.

	OLS on sample of starters in participating studies		
Explanatory variable			
	(1)	(2)	(3)
Coaching	-0.096***	-0.073***	-0.071***
	(0.023)	(0.021)	(0.018)
Covariates			
Cohort and field of study	yes	yes	yes
Socioeconomic and personal characteristics (a)	no	yes	yes
Previous education and cognitive skills (b)	no	no	yes
Number of observations	450	450	450

All models include constants. Robust standard errors that correct for clustering of students in classes are in parentheses. Missing data on covariates are imputed by using means by cohort and a dummy for missing is included in the regression. Data on school dropout are complete.

* / ** / *** denotes effect estimate is significant tested at a 1 / 5 / 10 % significance level.

(a) Socioeconomic and personal characteristics include a dummy for male, a dummy for born in the Netherlands, a dummy indicating compulsory education status, a dummy indicating living status (with or without both parents), a dummy indicating timing of choice for the particular study, and a dummy variable indicating whether a student has problems to some degree in at least one of the following areas: housing/living situation, family and friends, police and justice, financial problems.

(b) Previous education includes a vector indicating the highest attained education level at the start of the experiment (with 6 categories). Cognitive skills are proxied by two variables providing a score on a 1-5 point scale on an intake test of verbal and numeric skills.

5. Channels

Beyond estimating the effects of coaching on school dropout, another key issue concerns understanding the channels through which coaches have been effective. We analyze the importance of two possible channels. The first channel is an effect on study dropout, that is, dropout from the particular study in which the student started. This can be interpreted as a preventive channel. The second channel may be that coaching succeeds in reducing dropout from education among study dropouts. We label this as a curative channel. We have seen earlier that coaches employed several activities to prevent study dropout as well as to prevent complete dropout from education once students have decided not to continue their particular study. Figure 2 shows these two subsequent decisions schematically and shows how these differed among coached and non-coached students. It shows study dropout happens less in the groups that received coaching (26 versus 38%). Among those students

that decided not to continue their study, the share deciding to completely leave education is lower as well among coached students (26 versus 44%). This implies coached study dropouts have started another study more often.[27]

Figure 2 Study dropout and school dropout among study dropouts, one year after start of experiment

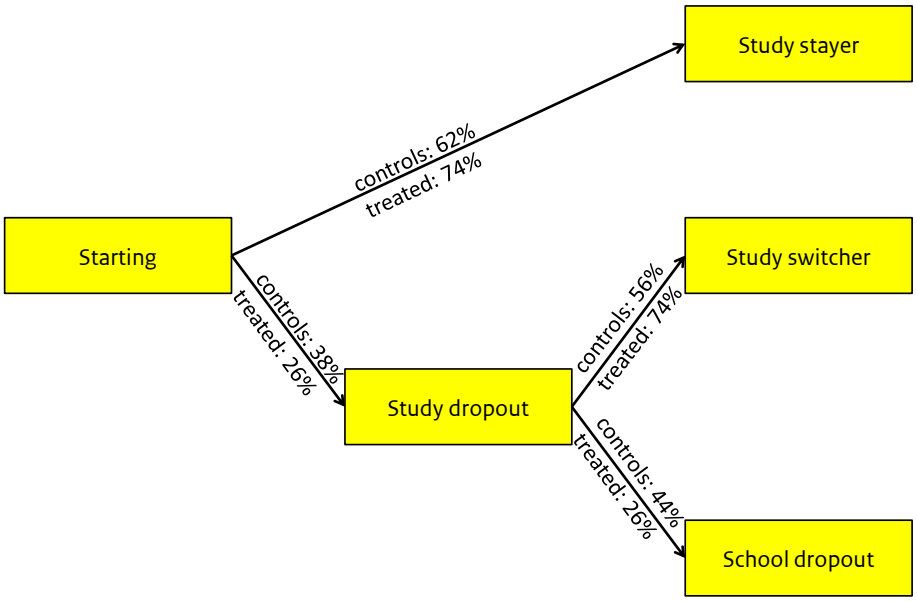


Table 5 shows the corresponding effect estimates. The estimates suggest that both channels played a role in reducing the probability of school dropout. The point estimate on study dropout (see row A) suggests that intensive coaching reduced the average study dropout rate by 8 percentage points from 38 percent to 30 percent. This corresponds to a relative reduction of a little over 20 percent. The point estimate of the effect on school dropout, conditional on study dropout, suggests a reduction in the school dropout rate among study dropouts by 16 percentage points from 44 to 28 percent (see row B).[28] This corresponds to a relative reduction of more than one third. It seems the coaches have been successful in preventing study dropout as well as in motivating and helping study dropouts to find another suitable study and stay in education. Together, these two effects contributed to lower school dropout among all starting students which was the ultimate goal of the coaching program. A simple decomposition calculation suggests the contribution of the two effects to the reduction in school dropout is roughly of equal size.[29]

[27] About three quarters of all study switchers within one year start another study at the same school whereas one quarter moved to another school.
[28] We are aware this effect estimate may not be interpreted as the exact causal effect of coaching on school dropout conditional of study dropout. This is because the degree of study dropout differs among coached and non-coached groups. Stated otherwise, the characteristics of coached and non-coached study dropouts may differ.
[29] This can be seen as follows. If there would have been no effect on school dropout among study dropouts (a 'curative' effect), but only on study dropout (a 'preventive' effect), the aggregate school dropout rate among all starting coached students would have been 30 percentage points times 0.44 = 13.2 percentage points. This would imply a reduction in the aggregate school leaving rate by 3.5 percentage points (down from 16.7 percent among the non-coached students), whereas

Table 5 OLS effect estimates after one year of coaching on (i) study dropout and (ii) school dropout conditional on study dropout, pooled sample of cohorts 2009-10 and 2010-11.

	OLS on sample of starters in participating studies		
Effect of coaching on			
	(1)	(2)	(3)
A. Study dropout	-0.108**	-0.086*	-0.085*
	(0.052)	(0.048)	(0.047)
B. School dropout *among study dropouts*	-0.161***	-0.164**	-0.160**
	(0.054)	(0.064)	(0.067)
Covariates			
Cohort and field of study	yes	yes	yes
Socioeconomic and personal characteristics (a)	no	yes	yes
Previous education and cognitive skills (b)	no	no	yes
Number of observations	450 (A) / 152 (B)	450 (A) / 152 (B)	450 (A) / 152 (B)

Notes:

Both models include covariates for socio-economic and personal characteristics, as well as for previous highest education and cognitive skills. Covariates are the same as in column 3 of table 4. Cluster-robust standard errors are reported in parentheses.

* / ** / *** denotes effect estimate is significant at 1 / 5 / 10 % significance level.

The effect of coaching on study dropout appears to be almost completely concentrated among students choosing very late for the particular study (i.e. in June or later, with studies starting in the beginning of September). The point estimate of the effect of coaching for late deciders suggests a significant reduction of 23 percentage points on the probability of study dropout, whereas the effect estimate on the sample of non-late deciders is just minus two percentage points and insignificant. We find that the probability of study dropout is also substantially larger among late deciders (57 versus 29 percent among earlier deciders), which suggests there was ex ante more room for reducing study dropout among this group. We find late deciders also had significantly stronger doubts about their study choice at the start of their study.

6. Heterogeneous effects of one year of coaching on school dropout

The effect of coaching may differ among student with different characteristics. Table 6 provides estimates of the effects of coaching on school dropout for a selection of subgroups. Intensive coaching seems to have a larger effect on school dropout for male[30] students (see row 1a), for students not under start qualification duty anymore

the total estimated effect is minus 7.1 percentage points (see column 3 of table 4). This shows the contribution of the 'curative' effect is 48 percent of the total effect on school dropout.

[30] Bertrand (2011) finds that broken families are associated with worse parental inputs and that boys' non-cognitive development, unlike girls', appears very much responsive to such inputs. This shows up in more disruptive behavior among boys. We find indications for the same pattern in our experiment as well. We observe that school dropout occurs much more often among men than among women in the group not living with both parents (i.e. 43 versus 17 percent). Men in this group report twice more often having problems with police and drugs as compared to women, and one third more often financial problems and problems with housing. Reporting (one of) these problems is associated with a higher probability of school dropout. Moreover we find that coaching seems much more effective for men than for women in the group not living with

(see row 2a), for students not living with both parents (see row 3a), and for students choosing late for their particular study (see row 4a). Effect estimates for all these groups are around or sometimes more than twice as large as compared with estimates for their counterparts. It follows these subgroups are all groups with a relatively large probability of school dropout.[31] This suggests that coaching is more effective (in absolute terms) for various groups with a relatively large inherent probability of dropout.

We further investigate this suggestion by estimating a probit regression that predicts the probability of school dropout as a function of individual covariates and (field of) study and cohort.[32] This regression is estimated on the control sample only and is used to generate school dropout probabilities for both treated and non-treated students. The fitted probabilities are used to split the sample in two subgroups of roughly equal size, one with a relatively low probability of school dropout, and one with a relatively high probability of school dropout (top half). Students in the top half have an average ex ante school dropout probability of 28 percent, whereas that of students in the bottom half is just 3 percent. The next step is to estimate equation (1) separately for the bottom and top half of the school dropout predicted probability distribution. Rows 5a and 5b of table 6 show the effect estimate for the top half is more than four times as large as for the bottom half. This confirms the observation that effects of coaching seem larger for subgroups that have an ex ante high probability of dropout. This suggests that the efficiency of the coaching program may be improved by targeting the coaching interventions more or earlier on the most vulnerable groups to school dropout, as can be detected by ex ante student characteristics that are relatively easy to measure.[33]

Differentiating by whether or not a student reports personal problems yields less clear differences in effect estimates (see rows 5a and 5b).

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both parents. Effect estimates on school dropout are minus 28 percentage points for men (significant at a 5-percent significance level) compared to minus 4 percentage points for women. The former estimate suggests that coaching succeeded in reducing school dropout among men not living with both parents by two-thirds from 43 to 14 percent. Coaches reported that, among other interventions, they put quite some efforts in helping students to resolve personal problems such as financial problems and problems with housing (or in guiding them to the appropriate organizations that could help them). This may have positively impacted the decision to stay in school, particularly among the group of boys not living with both parents, of which a relatively large share reported these problems.

[31] Control group average school dropout rates are 24% for men, 23% for students not under start qualification duty anymore, and 24% among students not living with both parents. The larger school dropout probability for the last subgroup has been confirmed in analyses with national school dropout data (Jol e.a., 2012). Larger probabilities for men and for students above start qualification duty age are visible in national figures as well (see www.aanvalopschooluitval.nl). Among students choosing late this rate is closer to the average control group rate with 16 percent.

[32] The list of included covariates in this probit regression is the same as used in previous reported regressions.

[33] For instance, the decile with the largest estimated ex ante probability of school dropout (larger than 40 percent) is strongly overrepresented by male students (71 percent), students reporting problems (67 percent), students not under qualification duty anymore (89 percent), students not living with both parents (80 percent) and late deciders (38 percent).

Table 6 OLS Effect estimates of coaching on school dropout after one year for various subgroups, pooled sample of cohorts 2009-10 and 2010-11

Subgroup	School dropout among controls	Effect estimate	Estimation sample
1a) Male	0.238	-0.101**	152
		(0.040)	
1b) Female	0.137	-0.056***	298
		(0.014)	
2a) Not under start qualification duty anymore	0.227	-0.109***	241
		(0.028)	
2b) Under start qualification duty or below statutory school leaving age	0.089	-0.045**	209
		(0.019)	
3a) Not living with both parents	0.236	-0.113**	191
		(0.042)	
3b) Living with both parents	0.067	-0.040*	242
		(0.020)	
4a) Late study choice (after first of June)	0.164	-0.119*	111
		(0.068)	
4b) Non-late study choice (before first of June)	0.137	-0.060**	320
		(0.023)	
5a) Having problems in at least one of the following areas: finance, police and justice, family and friends, or living/housing situation	0.200	-0.078	172
		(0.061)	
5b) Not having any problems in any of the following areas: finance, police and justice, family and friends, or living/housing situation	0.096	-0.056**	253
		(0.026)	
6a) High predicted probability of school dropout (top half)	0.278	-0.119***	225
		(0.035)	
6b) Low predicted probability of school dropout (bottom half)	0.032	-0.028*	225
		(0.016)	

Notes: *** / ** / * denotes coefficient is statistically significant from zero at a 10 / 5/ 1 % significance level.
The estimated model and included covariates are equal to that in column 3 of table 4. Cluster-robust standard errors are in parentheses. The numbers do not add up to 450 in rows 3 (living situation), 4 (time of study choice) and 5 (problems) because of (limited) missing data.

7. Effects after two years of intensive coaching

The first cohort received two years of coaching. This allows us to analyze effects after two years of coaching and compare these with effects after one year of coaching. We investigate effects measured two years after the start of the experiment on two main outcomes: (A) school dropout, and (B) having obtained a start qualification after (the theoretical study duration of) two years. Descriptive statistics on these outcome variables are presented

in table 7.[34] This table shows that the share of cumulative school dropout is still higher in the coached groups, and that the share having obtained a start qualification is lower as compared to the non-coached groups two years after the start. The differences in the two shares among treated and non-treated are of roughly equal size (10 and 11 percentage points respectively).

Table 7 Descriptive statistics of school dropout and degree completion after two years of coaching, first cohort (2009-10)

	Control group	Experimental group	P-value[a]	Adjusted P-value[a]
A. School dropout[b]	0.22	0.12	0.06	0.20
B. Obtained 'start qualification' two years after start[c]	0.49	0.60	0.11	0.23
Total number of observations	142	74	216	

Notes:

(a) The adjusted P-value is for an F-test on the null hypothesis that assignment to a coaching group has no effect on school dropout or having obtained a start qualification, conditional on cohort and study.

(b) School dropout is defined as not being in education one year after the start of the experiment while not having obtained a 'start qualification'. A start qualification level is equal to ISCED level 3.

(c) The start qualification level is equal to ISCED level 3 which is upper secondary education or shorter-term intermediate vocational education.

Effect estimates are presented in table 8. The first row shows that the effect on school dropout after two years of coaching is estimated at minus 7.3 percentage points. This effect estimate suggests a relative decline of school dropout by 34 percent from 22 to 14 percent. The effect after two years of coaching seems only slightly larger than the effect after one year of coaching (see row 1b): minus 7.3 versus minus 6.5 percentage points. This finding suggests that the largest absolute gains of coaching in terms of reducing school dropout are made in the first study-year. An explanation for this small improvement may be that different factors play a role in determining whether a student drops out in the second year as compared to the first year. For instance, the numerical cognitive level is a highly significant predictor of school dropout in the second year[35] (but not in the first year) which suggests that students with low cognitive numerical levels manage to keep up until the second year but then start to run into problems and dropout relatively more often. On the other hand living with both parents is a very strong predictor of school dropout in the first year but does not seem to play a role in the second year. The coaches may have been more successful in (and more busy with) addressing adverse home situations than in addressing low cognitive levels of students (which was not among the primary tasks of the coach obviously).

[34] A comparison of descriptive statistics on student characteristics of the first cohort among coached and non-coached students is presented in Appendix B. This Appendix shows (again) that none of the differences are statistically significant at a 10 percent significance level after having controlled for the particular studies involved, the level at which randomization took place.

[35] A one standard deviation lower score on the numerical intake test corresponds with a 6 percentage points higher school dropout probability. This corresponds to a reduction of more than half of the average probability of school dropout in the second year.

The effect estimate of coaching on having obtained a start qualification two years after the start points at an effect which is in the same order of absolute magnitude as the effect on school dropout. This estimate is marginally significant (p-value of 0.14).[36] This estimate seems to provide some support for positive effects of the coaching program on educational attainment. The point estimate would imply an increase in start qualification attainment share from 49 to 56 percent due to two years of being offered intensive coaching.

Investigating the possibility of heterogeneous effects of coaching on the probability of having obtained a start qualification within two years, we find start qualification duty status to matter most. The effect estimate for the group not under start qualification duty anymore (N=107) is 0.163 and significant at the 5-percent significance level (standard deviation 0.063). This finding suggests that coaching has increased the probability of obtaining a start qualification among this group from 42 to 58 percentage points. The effect estimate for the group under start qualification duty is exactly zero. The share of students reaching a start qualification within two years is around 60 percent in both coached and non-coached groups.

It should be stressed that it is somewhat early to evaluate the definitive effects of coaching on educational attainment since only half of the students in our population manages to obtain a start qualification within two years. Two years is the theoretical study duration of studies at level 2 of intermediate vocational education, of which completion yields a start qualification. This group of relatively fast graduates has probably not been the primary focus of the coaches, and may be overrepresented by students that would have obtained a start qualification anyway, irrespective of coaching.[37] Measurement of attainment three or even four years after the start of the experiment would be needed to obtain a more definitive picture of the effects of intensive coaching on educational attainment.

Table 8 Effects after two years of coaching, first cohort (2009-10)

	OLS
Outcome variable	
1a) School dropout as measured two years after the start, after two years of coaching	-0.073**
	(0.022)
1b) School dropout as measured one year after start, after one year of coaching	-0.065**
	(0.027)
2) Obtained start qualification two years after start	0.063
	(0.040)
Number of observations	216

Notes: the estimated model and included covariates are the same as column 3 of table 4. Cluster-robust standard errors are in parentheses.

** denotes estimated coefficient is statistically significant from zero at a 5 percent significance level

[36] The precision of the effect estimates after two years of coaching is lower compared with earlier estimates on the pooled sample after one year of coaching because the former effects can only be estimated on the first cohort.

[37] The group of fast graduates has favorable characteristics. For instance, comparing the group having obtained a start qualification within two years with those that did not shows that the former group has a significantly lower share of students with (self-reported) problems (36 versus 59 percent).

8. 'Back-of-the-envelope' cost-benefit calculations

How do the benefits of the coaching program compare to its costs? Since we do not know the long-term effects on educational attainment, it is not possible to give a definitive answer yet. By making some assumptions however, we may get an idea of the possible social returns to one or two years of intensive coaching. These tentative back-of-the-envelope calculations suggest one year of coaching is likely to generate a net social gain. Two years of coaching may not be effective enough though to generate a net social gain in its current form.

We start with the calculation for the rate of return of a program of one year of coaching. The costs of one year of coaching amount to 3,000 euro per treated student (i.e. 60 k euro for a FTE equivalent of coaching per group divided by 20 students per group). The returns per year are calculated making the following assumptions: we use average annual earnings of workers without a start qualification as a base (25,265 euro)[38]; we use the effect estimate of (minus) 7.1 percentage points of the effect on school dropout after one year as a proxy for the definitive effect on school dropout; we assume this seven percent of the treated population not becoming an school dropout due to coaching receives two extra years of schooling[39], each yielding a social rate of return of 10 percent[40]; public as well as private costs of these two extra years of schooling for seven percent of the treated population are taken into consideration.[41] The yearly return can then be calculated as follows: 25265*0,10*2*0.071 = 353 euro per year. These returns are assumed to start occurring in the fifth year after the coaching started (to take into account extra study duration and the time to labor market entry) and are assumed to be maintained for 42 years.[42] Bringing these costs and benefits all together yields an internal rate of return of one year of coaching of 6.9 percent. At the advised discount rate of 5.5 percent[43] one year of coaching would then yield a positive net present value of 18 k euro per coached group (at an initial investment of 60 k euro per group). To put it differently, we would need a sustained effect of at least 5.5 percentage point less school dropout in order for one year of intensive coaching to break even at a discount rate of 5.5 percent.

[38] This is a weighted average of wage income of three different subgroups varying by their distance to a start qualification (and thus by their completed years of education), the weights corresponding to relative occurrence of these subgroups in our sample. Wage figures are taken from Arbeidsmarktpanel 2009. We used average yearly wage income of workers aged 20-64.
[39] The reasoning for using two years is as follows. The distance to a start qualification of the group without a start qualification in terms of years of completed education is one year for the group with MBO level 1, two years for the subgroups with completed secondary vocational education, and six years for the subgroup with just primary education. The shares of these subgroups in the sample without a start qualification at the start are 10, 77 and 13 percent respectively. This would imply the average distance to a start qualification in terms of completed years of education is 2.4 years in our sample. Furthermore, a start qualification gives access to higher levels of intermediate vocational education (whereas this access is not granted without a start qualification), such that the definitive difference in years of completed education among students managing to obtain a start qualification and those that do not probably be even larger than this. Nevertheless we use a conservative assumption of two years of additional education linked to those students not becoming a school dropout due to coaching.
[40] OECD (2012) shows that people (aged 25-64) having attained less than upper secondary education earn 19 percent less than people having attained upper secondary education in the Netherlands. The average earnings difference in OECD is 24 percent between these two groups. This earnings difference increased somewhat in the last decade (up from 20 percent in 2000), despite a rather strong decline in the share of people with below upper secondary education (from 36 to 26 percent). This suggests that relative demand for people with below upper secondary education (relative to those with upper secondary education) has fallen. These earnings differences need not represent the causal effects of obtaining an upper secondary level, but come close to other studies which have used credible designs to detect the returns to education (see Card, 1999 and Heckman et al., 2006 for reviews of this literature).
[41] These costs consist of around 5k euro public contribution and 1k euro private contribution per study year per student in senior vocational education.
[42] Average age at start of the experiment is 18. Pension age is 65 at the moment but is agreed to go up to 67 by 2025. We assume benefits of higher educational attainment will continue up to the age of 65.
[43] This is composed of a 2.5 % risk-free real discount rate, and a 3 % risk premium (Ministry of Finance, 2009).

Introducing a one-year coaching program to all first year students at level 2 of intermediate vocational education in the Netherlands and extrapolating the costs and benefits is expected to lead to 2,100 school dropouts less at a total cost of around 90 million euro and a net present value of 27 million euro (under the same assumptions made above). This estimated reduction of such a scaling up would equal fifteen percent of the total national reduction target of the government (to 25 thousand school dropouts by 2014-15 down from 39 thousand in 2010-11).

The estimated internal rate of return of two subsequent years of coaching is 3.7 percent, which implies a net social loss at the advised discount rate of 5.5 percent.[44] This estimate is based on (i) the effect estimate of two years of coaching of -7.3 percentage point on school dropout (based on the first cohort sample), (ii) 6,000 euro of initial investments in coaching per treated student (i.e. two years of 3,000 euro), and (iii) for the rest on the same assumptions as above. To put it differently, we would need an effect of 10 percentage points less school dropout in order to break even with the two year coaching program at its current costs (and at the advised discount rate of 5.5 percent).

To put these estimates into perspective, Angrist & Lavy (2009) estimated an internal rate of return of 8.6 percent of a program offering financial incentives for high school students in Israel upon passing high school matriculation exams, which is found to have increased educational attainment. This return is obtained by substantially lower costs compared with the coaching program studied here (385 US $ per treated student in 2,000 dollars) but at the same time with smaller effects. Cost-benefit calculations of the Education Maintenance Allowance program in the UK point at a net social gain as well (see Dearden et al., 2009).[45]

9. Conclusions and discussion

This paper evaluated effects of a randomized experiment with intensive coaching in vocational education. Intensive coaching turns out to have a substantial impact on school dropout with an estimated reduction of more than 40 percent after one year of coaching.

Intensive coaching reduces the probability of study dropout as well as it reduces the chance of dropping out from education once students drop out of their study. These two channels are of roughly equal importance and together lead to substantially lower school dropout rates and suggest that both preventive and curative coaching interventions have been effective. Nonetheless it is difficult to pinpoint exactly which interventions had the highest contribution. The coordinators of the coaching program stress that it was the combination of the personal approach, the various types of interventions (both before and after study dropout), their intensity, their timing,

[44] Assuming a 3% discount rate, this corresponds to a net present value of 10 k euro at a total initial investment of 120 k euro per coached group.
[45] One should be careful with drawing too fast conclusions from a comparison of cost-benefit calculations of different programs since assumptions being made in those cost-benefit analyses often differ. An example of a difference in assumptions is that the EMA study uses a discount rate of three percent, whereas we use a discount rate of 5.5 percent. The latter rate obviously reduces the chance of finding a positive net rate of return.

and the expertise and network of the coaches that all together contributed to lower school dropout.[46] The potential for coaching to make an impact follows from indications that regular care is generally relatively little in intermediate vocational education as compared to for instance junior secondary vocational education. Allen & Meng (2010) show that less than a quarter of school dropouts in the lower levels of intermediate vocational education claims to have received help with finding another study.[47] Furthermore, a quarter of them never spoke to anyone about their decision to stop, a share which is highest (44 percent) among those indicating personal problems as the main reason of dropping out. This indicates a relatively large vacuum in intermediate vocational education in which the coaches could prove their value added by having intensive and personal interactions with (potential) dropouts in order to help and stimulate them to continue in education. Replications of this approach would be useful to find out to what extent the effects found in this experiment also hold in other settings.

Our findings suggest that a more targeted coaching approach on those students being relatively vulnerable to school dropout may improve cost effectiveness of the coaching approach. Effects of coaching vary among subgroups. Estimated effects are larger for men, students not under start qualification duty anymore, students not living with both parents, and students deciding late to do a particular study. These are all groups with a relatively large ex ante probability of school dropout. These predictive characteristics can be gathered at the intake process relatively easily and may help to identify and target the vulnerable students better and quicker.[48]

It seems important to note that the coaching program operated in a context of two (nationwide) policies that may have had an independent effect on school dropout. The first one is the start qualification duty which is an obligation to students to stay in education as long as they are under the age of 18 and as far as they did not obtain a start qualification (i.e. ISCED level 3) yet. The second factor is financial incentives for schools to reduce school dropout and raise graduation rates. Schools could receive 2,000 euro per dropout less at the time of the experiment. Furthermore, schools in senior vocational education receive a certain amount of money per graduate (apart from contributions based on the number of students). Both students and schools thus faced incentives to (make them) stay in education and (have them) obtain a start qualification. These two types of incentives may have reduced the potential somewhat for intensive coaching to have an (additional) impact on school dropout. A final factor which may have affected the effectiveness of the coaching program is the relative high unemployment at the time the experiment took place. This may have created additional incentives for all students (also controls) to stay in school (i.e. pull factors from the labour market were weaker).[49]

[46] Interventions that were reported to be particularly effective by the coordinators and coaches were working on study skills (study planning and organizing), counseling in case of personal problems (socio-emotional, guide students to internal and external social workers), contact with parents and initiating study choice tracks.

[47] Care in junior secondary vocational education seems more intensive. The percentage of school dropouts claiming to have received help to find another study is 39 percent among school dropouts from this level. The percentage of school dropouts that claims to have received help with obtaining their diploma from school (57 versus 33 percent) or from other bodies (34 versus 18 percent) is also higher in junior secondary vocational education.

[48] School dropout occurs more among: men; students with lower numeric skills; students not living with both parents; students not under start qualification duty anymore; students with (self-reported) problems in at least one of the following areas: finance, police and justice, family and friends, or living/housing situation; and students with low previous attained education levels. In addition, an important predictor of study dropout is a late study choice for the particular study, that is, study choice in the last two months before the study year starts.

[49] Several papers find a positive relationship between (youth) unemployment and enrollment in school (e.g. Rivkin, 1995; Card & Lemieux, 2000; Clark, 2011). This correlation seems even stronger among people with lower educational attainment or cognitive levels (see Rice, 1999; Messchi et al., 2010). Unemployment among youth aged 15-25 rose from 8.4 to 11.7 percent between 2008 and 2010 in the Netherlands (CBS Statline). Unemployment among youth without a start qualification

The high rates of study dropout found in this experiment under regular care (nearly four out of ten after one year) also provide a case to study the potential for earlier interventions to improve study choice and thereby to reduce study dropout. The professionals involved in the experiment mentioned wrong/bad study choice as the most important reason for study dropout.[50] Study dropout is a problem because it is often a prelude to school dropout. As far as study dropouts stay in education, social costs involved with study switching are not insignificant. These costs involve direct study costs but more importantly also costs due to longer time in school and later labor market entrance (e.g. less wage income for the individual and less income taxes for the government). For instance total costs involved with inefficient study choices in Dutch intermediate and higher education have been estimated at around 6 billion euro per year (ROA, 2003). Effective policies to improve study choice and to reduce study switching may therefore yield large returns.

even rose to 21 percent in the third quarter of 2009 (when our experiment started), which is ten percentage points higher compared with the same quarter one year earlier (CBS Webmagazine, December 17, 2009).

[50] Earlier research shows that around 20 percent of all Dutch school leavers mention they regret their study choice (Borghans et al., 2008). Allen & Meng (2010) show that 20 percent of all dropouts in the lower levels of intermediate vocational education report wrong study choice as the most important reason for dropout.

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Appendix A School dropout in the Netherlands and context of experiment

The Netherlands had almost 40 thousand new school dropouts (or early school leavers) in the school year 2010-11 (Ministry of Education, 2012, TK brief). The official definition of an early school leaver in a certain school year is a student aged 12-22 that is (i) in education on the first of October (start of the school year), (ii) not in education one year later, and (iii) has not obtained a so-called 'start qualification'in the meanwhile. A start qualification is equal to a degree of upper secondary education or of intermediate vocational education of at least level 2.[51] The large majority (around 75%) of school dropouts drops out from intermediate vocational education (MBO). More than 40 percent of school dropouts in MBO dropout from MBO level 2 which is the level at which the coaching experiment took place. This means that 30 percent of all new school dropouts in the Netherlands drop out from level 2. Official study duration at level 2 is two years and completing this level yields a start qualification. The national average school dropout rate at level 2 is 13 percent, that is, one out of every seven students leaves education without a start qualification every year (source: www.aanvalopschooluitval.nl).

The target of the current national action program against dropout " Aanval op de Uitval" is to reduce the yearly number of school dropouts to 25,000 by 2014-2015.[52] Total (yearly) costs linked to Dutch dropout policy have been estimated to be around 400 mln euro in 2011 (Ecorys, 2009). An important part of this money is invested though regional covenants with a contact municipality and schools within each region. The covenants describe targets for the subsequent years for each of the 39 regions which add up to the national dropout reduction target. Part of the provided funds are provided unconditionally, another part of the budget is paid to the schools conditional on reaching preset targets for dropout reduction. This implies there is in part a financial incentive to the schools to reduce school dropout. Schools and regions have full autonomy over their choice of anti-dropout measures.

The coaching experiment took place at ROC Rijnijssel. This school is located in Arnhem, a medium-sized city belonging to the 30 largest cities in the Netherlands. The school had around ten thousand participants in school year 2009-10 of which 8.5 percent dropped out of education without a start qualification. This is a rather average dropout rate out rate, as seen over the schools that offer similar type of intermediate vocational education in the Netherlands (source: www.aanvalopschooluitval.nl).[53]

In 2009 the Ministry of Education was actively looking for opportunities to gain more (convincing) evidence on promising dropout interventions and invited institutions that offered senior secondary vocational education to see whether they would be willing to participate in a randomized dropout prevention experiment. ROC Rijnijssel in Arnhem turned out very much interested in an experiment. They were thinking about expanding an intensive coaching setting from MBO level 1 to level 2. Experiences with this intensive coaching setting at level

[51] In terms of the international standard classification of education, this corresponds to ISCED level 3. Intermediate vocational education in the Netherlands has four levels.
[52] The target in European perspective is based on another measure of school dropout. This is the share of students aged 18-24 with only lower secondary education at best and not in education or training. The EU 2020 target for the Netherlands to which the Dutch government has committed itself is 8 percent. The 2010 rate was 9.1 percent, down from 15.1 percent in 2000. The EU-27 average rate was 13.5 percent in 2010, down from 17.6 percent in 2000. (source: http://europa.eu/rapid/pressReleasesAction.do?reference=IP/12/577&format=HTML&aged=0&language=EN&guiLanguage=en)
[53] Rijnijssel had the 17th highest dropout rate out of 42 schools for intermediate vocational education in the Netherlands.

1 had been satisfying and it was felt that this approach contributed to dropout prevention, though convincing evidence was lacking. The school agreed to participate in a randomized experiment at level 2.

Appendix B Descriptive statistics of first cohort, starting in school year 2009-10, receiving two years of coaching.

Characteristic	**All studies pooled**		
	Control	Treated	Adjusted P-value[a]
1. Male	0.23	0.38	0.51
2. Age (in years)	18.2	17.9	0.76
3. Under school leaving age or start qualification duty[c]	0.45	0.61	0.29
4. Born in the Netherlands	0.88	0.93	0.19
5. Living with both parents	0.57	0.62	0.76
6. Having problems to some degree in at least one of the following areas: finance, police and justice, family and friends, or living/housing situation	0.48	0.35	0.11
7. Score on verbal skills at intake test (1-5, 5=highest)	3.2	3.3	0.37
8. Score on numeric skills at intake test (1-5, 5=highest)	3.1	3.2	0.19
9. Highest previous attained degree (1-6, 6=highest)	2.3	2.2	0.96
10. Already having obtained a start qualification at start of experiment	0.05	0.13	0.14
11. Late choice for this particular study (July or later)	0.27	0.28	0.82
Number of observations	142	74	216

Notes:

A missing value on the background characteristics is limited to maximum six percent of the pooled sample.

(a) P-value is adjusted for cohort and field of study.

(b) All students under 16 are obliged to go to school. Students of 16 and 17 are obliged to stay in education if they have not completed a degree that counts as a start qualification (i.e. ISCED level 3 or higher).

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