Experimenting with dropout prevention policies

Three bottom-up interventions to prevent school dropout at vocational colleges are empirically evaluated with an RCT. The interventions focus on different phases in the dropout process: the initial match between student and discipline, parental involvement and illicit non-attendance. None of the three interventions is found to significantly reduce early school leaving.

This project shows that piloting or testing new interventions using an RCT set-up is possible and that these small scale trials can prevent interventions that turn out to be ineffective to be rolled-out on a large scale.

Experimenting with dropout prevention policies[1]

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Abstract

This paper concludes that school dropout rates did not decrease significantly in three randomized controlled trials (RCTs) in Dutch vocational training colleges. Colleges could apply for a subsidy on interventions that aim to reduce dropout rates amongst young students that still lack a sufficient degree. All interventions were bottom-up approaches, so initiated by the colleges themselves. The interventions were relatively light interventions for a tough problem. The interventions focused on different elements of the dropout process: one aimed to improve the initial match between student and discipline, one aimed to improve parental involvement and another aimed to decrease illicit non-attendance. Simple analyses at the school level show that participation in the subsidy program did not decrease average dropout rates. The individual level results – making use of the RCT nature of the data – are very similar. The setup of the subsidy and our results show that testing new interventions using an RCT is possible and that these small scale trials can prevent ineffective interventions to be rolled-out on a large scale.

Keywords: RCTs, education policy, dropout prevention

JEL-codes: I21, I28, C93

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1. Introduction

Preventing school dropout is an important policy goal as young people leaving school without a diploma are worse off in virtually every aspect, later in life. The numbers are surprisingly unanimous around the developed world: school dropouts earn lower wages, are more often unemployed, engage more in criminal activities and experience worse health outcomes. Moreover, the evidence now convincingly shows that obtaining a diploma protects against negative outcomes in life and is not just associated to having better experiences. It increases labor market chances (Oreopoulos 2006a, Oreopoulos 2006b, Oreopoulos 2009), increases income (e.g. Bhuller et al. 2014, Devereux and Hart 2010, Leigh and Ryan 2008), increases healthy behaviours (e.g. Brunello et al. 2015, Grimard and Parent 2007, Jürges e.a. 2011) and lowers the probability of illness (e.g. Kemptner et al. 2011, Oreopoulos 2006a, Oreopoulos 2007). In addition, it leads to fewer teenage pregnancies (e.g. Clark et al. 2014, Cygan–Rehm and Maeder 2013, Silles 2011) and less criminal behaviour (e.g. Cullen et al. 2006, Deming 2011, Amin et al. 2016).

Unnecessary school dropout should thus be prevented. To help young individuals at risk several actors are at play; not only teachers and schools, but also local and national governments and even employers can contribute. To design effective and efficient anti-dropout policies at each of these levels requires knowledge on what works. To acquire causal evidence on what anti-dropout policies work, running a randomized controlled trial (RCT) is the golden standard.

Recognizing the need for more causal evidence, the Ministry of Education in The Netherlands started a subsidy program where vocational colleges could apply/compete for financial support to evaluate their self-developed school dropout prevention program with an experiment. The approach was bottom-up: the ideas for the school dropout policy to be evaluated came entirely from the vocational colleges. The subsidy was targeted at vocational colleges as students in these colleges have the highest risk of dropping out. In total four colleges received financial support for the evaluation of a new dropout prevention policy. This paper analyses the results of these four experiments.

The existing literature on early school leaving distinguishes *curative* interventions to get a student that has dropped out back to school, from *preventive* interventions when the student is still in school. Within preventive interventions two main types can be distinguished: interventions *with* a financial incentive for students and interventions *without* a financial incentive for students.

Curative interventions that guide drop-outs back to school generally do not use financial incentives, but, for example, intensive counseling (Schochet et al. 2008), the possibility to obtain partial certificates (Heckman et al. 2008), or group-based programs for problem youth (Van der Steeg et al. 2008). The results of these interventions are mixed. Schochet et al. (2008) found positive effects of the *JobCorps* program in the short run (more degrees, less criminal behavior and a higher income), but these effects faded in the longer run. Heckman e.a. (2008) found that GED certificates in the US led to *worse* labor market outcomes. Such adverse effects are also found by Van der Steeg et al. (2008), who show that the group-based program for problem youth had negative spillover-effects as criminal behavior increased among students (they 'taught' each other in criminal behavior).

Preventive interventions with a financial reward (usually upon completion of the degree) are found to have a positive effect in the short run. More students obtain a degree. In the longer run the evidence is, however, mixed. Some studies find that there is no effect in the long run as the financial incentive accelerates obtaining a degree but does not lead to students obtaining an additional degree or more students obtaining any degree (Rodriguez-Planas 2012). Other studies do find a positive and lasting effect of a financial incentive (Oreopoulos et al. 2014).

Financial rewards are not necessary for preventive interventions to have a positive effect; also preventive interventions without a financial are found to have an effect. An intensive coaching program where students received two hours of individual coaching per week reduced early school leaving by 7%points, from 17% to 10% (Van der Steeg et al. 2015). Group sessions for low-achieving students to help them formulate realistic objectives for their educational career are also found to help in preventing early school dropout (Goux et al. 2017).

The three experiments that are evaluated in this paper all comprise interventions of a preventive nature and without financial incentives. The interventions were designed by professionals at the vocational college and involved improving the match between student and discipline with a renewed intake procedure at two vocational colleges, stricter absenteeism rules and the use of absenteeism counselors at one vocational college and enlarging parental involvement by providing e-coaching for parents at one vocational college.

The contribution of this paper is twofold. First, the paper tries to gain insights into successful field experiments using professional expertise for bottom-up ideas by evaluating a subsidy program to stimulate RCTs at vocational colleges. Second, this paper tries to gain more knowledge on effective interventions to prevent school dropout at vocational colleges. Key finding of this paper is that none of the three experiments that actually took place succeeded in reducing early school leaving. However, useful knowledge was found on how to perform field experiments.

This paper proceeds as follows. Section 2 describes the institutional setting in The Netherlands in which the experiments took place. Subsequently section 3 gives a detailed description of the different interventions. Section 4 presents analyses on the level of the vocational college. The next two sections use the individual (student) level: section 5 provides the empirical strategy and some first analyses, where section 6 shows the main results of the paper. Finally, section 7 concludes.

2. Institutional setting

2.1 School dropout in The Netherlands

In recent years school dropout rates in The Netherlands have gone down, as in many other countries. This means that fewer young people leave high school or a vocational training college without an appropriate degree. However, there is still need to lower dropout rates further.

In The Netherlands someone is classified as *early school leaver* or *school dropout* when he/she is under 23 and leaves the educational system without at least a diploma from senior general secondary education (*havo*), pre-university secondary education (*vwo*) or secondary vocational

education (*mbo*) level 2 or higher. Most dropout takes place at the vocational colleges; in the academic year 2015/2016, 0.4% of high school students dropped out of school while 4.6% of vocational college students left without an appropriate diploma.[2]

In the past decade, the Dutch government has initiated a set of policy initiatives to prevent early school leaving (*aanval op schooluitval,* attacking school dropout). An important element of this broad intervention was the collection and dissemination of dropout rates per vocational training college (and high school). Improved accessibility of administrative data records made it possible to track students through their educational career and identify those that left the education system without a sufficient degree. Another important element was the introduction of financial incentives for vocational training colleges to lower dropout rates. If the college manages to lower its dropout rates below the preset benchmark, it receives a financial reward.[3] A third element of the set of policy initiatives includes regional cooperation between high schools, vocational colleges and municipalities in a Regional Contact and Coordination Office for early school leaving (*Regionaal Meld- en Coördinatiepunt,* or RMC). The RMCs aim to track students that have dropped out and guide them back to school (or, if not possible, to employment). They also track students at risk of dropping out and try to prevent early school leaving. The RMCs can refer students to (health) care providers and (social) assistance when needed to keep them in or guide them back to school. The fourth element of the set of policy initiatives comprised of a subsidy program where vocational colleges could apply/compete for financial support to evaluate their self-developed school dropout prevention program with an experiment, The results of the fourth element are in this paper.

Despite the clear decrease in dropout rates it is unclear how much of the decrease can be causally attributed to the set of policy initiatives and how much the different elements of the set of policy initiatives have contributed. This is important to disentangle considering the need for evidence-based policymaking in the future and calculating the cost-effectiveness of interventions. Also from the literature little is known about the causal effect of the different components. One exception is an evaluation of an early stage of the RMCs in 2006, which at the time showed no improvement in dropout rates (Van der Steeg et al. 2008). It is however not certain that this conclusion has external validity to later initiatives in this domain.

Evidence from an RCT study by Van der Steeg et al. (2015) suggests there is still room to improve. They find a decrease of 40% in dropout from an intensive mentoring program. It is unclear, however, what the effective mechanism has been in this study, as the intervention contained many different elements (mentoring, parental involvement, study choice advice).

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[2] Numbers refer to official Dutch *VSV* statistics compiled by DUO Netherlands.

[3] In 2018 the Dutch government has 57 million euros available to reward vocational training colleges (and high schools) if they succeed to lower dropout rates below the benchmark rate. There is a separate benchmark for each level of secondary vocational education. For the academic year 2017/2017 the benchmarks for secondary vocational education are 27.5% for level 1, 9.4% for level 2, 3.5% for level 3 and 2.75% for level 4.For senior general secondary education and pre-university secondary education the benchmark is 0.5%, for pre-vocational secondary education 2.0%. The benchmark rate is lowered every year. (https://www.rijksoverheid.nl/onderwerpen/vsv/financiele-ondersteuning-vsv).

2.2 Subsidy program for bottom-up experiments

Recognizing the need for more causal evidence, the Ministry of Education in The Netherlands started a subsidy program in 2012 for new, bottom-up approaches in the prevention of early school leaving.[4] Vocational colleges (where dropout rates are traditionally high) could apply for financial support to set up an experiment with a new self-developed dropout prevention program. Financial support was up to a maximum of 50,000 euros per year for three years. The conditions for application were, *inter alia*, that students would be randomly assigned to either a treatment or control group and that the size of the experiment would be sufficient to find statistically significant effects, in order to evaluate the experiment properly and gain new causal evidence on dropout prevention policies. This paper analyses the results of these experiments.

In the autumn of 2012 all vocational training colleges in The Netherlands were invited to apply before November 1, 2012 for the subsidy program for experiments with anti-dropout policies. In total 13 of the 66 vocational colleges applied (see Table 1). Most of the proposed experiments required additional funds from the colleges themselves on top of the maximum subsidy of 150,000 euro the colleges could apply for. Schools both had an intrinsic and a financial incentive to invest additional funds in the experiment: an intrinsic incentive because colleges want their students to strive in school and society, and a financial incentive because colleges receive a premium from the government when the dropout rate is below the benchmark rate.

A committee scored the plans on expected effectiveness, possibility to scale up/disperse knowledge, and feasibility. Although there were funds available to subsidize five experiments, only four applications were approved by the committee. In December 2012 the vocational colleges that applied for the subsidy were informed whether the subsidy was granted to them. The selected interventions focused on different elements of the dropout process, i.e. better matching between student and discipline, stricter absenteeism rules and parental involvement. These interventions will be explained in more detail in the next section.

Table 1 shows that the vocational colleges that applied for the subsidy were on average larger than the colleges that did not apply. Both groups had on average similar dropout rates prior to the experiments. In the group of vocational colleges that applied, the colleges that were granted a subsidy also were larger and had lower dropout rates on average than the colleges that were denied the subsidy, although the difference is not statistically significant (see Table 1). Given that all applications were reviewed by a committee and the highest scoring plan were granted the subsidy, it is not surprising to see that the colleges that were granted a subsidy were scored significantly higher than the colleges that were denied a subsidy.

For each of the experiments at a vocational college, CPB Netherlands Bureau for Economic Policy Analysis provided guidelines, performed the randomization of students over the treatment and control group, collected data, and analyzed the results.

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[4] *Regeling van de Minister van Onderwijs, Cultuur en Wetenschap van 24 september 2012, nr. BVE/433067, houdende wijziging van de Regeling regionale aanpak voortijdig schoolverlaten en prestatiesubsidie voor het voortgezet onderwijs in verband met toevoeging subsidiegrondslag voor experimenteel onderzoek vsv..*

Table 1 Comparing vocational colleges that applied with those did not apply, and colleges that were granted and denied the subsidy for a dropout prevention RCT

	Applied	Did not apply	p-value
Number of colleges	13	53	
Average number of students in 2011/2012	8,335	5,049	0.018
Average dropout rate in 2011/2012	7.8%	7.6%	0.912

	Granted	Denied	p-value
Number of colleges	4	9	
Average number of students in 2011/2012	10,066	7,566	0.151
Average dropout rates in 2011/2012	7.3%	8.0%	0.400
Average reviewer score	6.9	5.0	0.036

Note: Institutes with fewer than 10 students are dropped for this table. Data are from DUO.

Experiments were supposed to start in the summer of 2013. Three colleges managed, but for one of the selected vocational training colleges it proved difficult to implement their original plan. The college was given the opportunity to set up an experiment with an alternative intervention the next year (start summer 2014). This was the largest of the four selected schools, with a relatively low dropout rate. The alternative intervention ran for two years, just like the other experiments.

Of the three vocational colleges that started with their experiment in summer 2013, one college dropped out of the experiment during the first year of the intervention (2013/2014). This was the smallest of the four colleges that was granted a subsidy. The experiment at this college focused on improving the intake procedure for new students by, *inter alia*, organizing introductory meetings for students and for their parents (separately) before the start of the academic year. Due to a change in staff composition between subsidy application and start of the experiment, there was no longer enough support from higher management to continue the program; the management was not convinced anymore of the added value of the experiment. The intervention was therefore only partially implemented, and poorly administered and monitored in this college during the first year, and not implemented at all in the second year. In the results section we do analyze college level dropout data for this college. Individual level data, however, have not been provided to us by the college. This paper will therefore only discuss the student-level results from the other three experiments.

3. Detailed description of selected interventions

3.1 Vocational training college A: new intake procedure

The experiment at college A aimed to combat early school leaving by trying to improve the match between student and study choice with a new in-depth intake procedure. Prospective students applying for a study at school A were randomly assigned to either the new in-depth intake procedure or the existing (shorter) intake procedure. The experiment with the new intake procedure ran in 2013 (for students applying for the academic year 2013/2014) and in 2014 (for students applying for the academic year 2014/2015).

The intervention

The new in-depth intake procedure aimed at preventing students from dropping out of school because of a poor field of study choice. It should help to discover and formulate the educational and counseling needs of aspiring students and find out whether the field of study they apply for fits the student's expectations and qualities/competences. If it does not, and another field of study suits his/her capacities better, the intake can guide him to this alternative field of study.

Although the goal of the new intake procedure might not differ (much) from the old one, the new in-depth intake procedure differs in four important aspects from the old intake procedure. First, in the renewed intake procedure the student receives individual counseling with a career counselor, whereas in the old intake a student often received counseling with a group of students (this depended on the field of study the student applied for). Second, the counselors involved in the new intake procedure receive two training sessions of three hours each on recognizing students at risk of dropping out. These training sessions were held before the start of the experiment and were led by professionals in communication skills. Third, students and their parents need to provide more information to the counselor in the new intake procedure. Especially the provision of information by parents is an important difference with the old procedure. The student brings the forms he and his parents completed to the meeting with the counselor, and at the end of the meeting the counselor advises positively or negatively for the study of choice. The mentor of the student also receives the advice that is given and can undertake the necessary actions in case of a negative advice, like strongly discourage the student to start with the study. Finally, the new intake procedure differs in duration from the old intake. The new intake lasts 60 minutes; the old intake varied in length. Table 2 summarizes the differences between the intake procedures.

The experiment

All students who applied for a field of study within one of the six participating disciplines at college A between March 28, 2013 and December 10, 2014 were assigned by lottery to either the new intake procedure (treatment group) or the old intake procedure (control group). Of the 743 students that applied during this period, 367 were assigned to the treatment group and 376 to the control group.

Table 2 Overview of the treatment and control condition in vocational training college A

existing intake procedure (control)	new intake procedure (treatment)
• mostly group counseling	• individual counseling
• no training sessions for counselors on recognizing students at risk of dropping out	• training sessions for counselors on recognizing students at risk of dropping out
• no provision of information by parents	• parents need to provide information to counselor
• no standardized forms, intake forms differ over field of study	• three standardized forms used by all fields of study
• duration varies	• duration 60 minutes

As there was no fixed time frame within which students could apply for a study in the next academic year, college A received applications throughout the year. Therefore at the end of every week college A sent a list to the researchers at CPB with all students that applied during that week. The researchers at CPB randomized the students on the list over the treatment and control group by

lottery and returned the resulting list to college A the same day. This catered to the wish of college A to know quickly which intake procedure they had to send a prospective student to. In many cases the batch consisted of uneven number of students and in some exceptional cases the batch of students consisted of just one student. As a consequence, the number of observations in the control and treatment group could not be fully controlled (equalized), which explains the small imbalance in size of the groups (367 vs. 376).

Students were not informed that they took part in an experiment. In total 39 counselors performed intake meetings: 15 counselors only used the new intake procedure and the other 24 counselors only performed intakes according to the old intake procedure.[5] To prevent 'pollution' of the experiment there were no counselors who did both type of intakes. In addition, it was agreed upon with the counselors of the different groups not to exchange nor discuss extensively the working/intake procedures. This was to prevent counselors in the control group copying (elements of) the new intake procedure.

3.2 Vocational training college B: absenteeism counselors

At college B a new anti-absenteeism intervention was introduced to battle early school leaving. Unauthorized absenteeism often preludes dropping out of school entirely. In the new anti-absenteeism policy the college intervenes earlier (at a lower number of hours of absenteeism) and more firmly by having the student meet a counselor who also contacts the students' parents. In the experiment the new anti-absenteeism policy is evaluated against the existing anti-absenteeism policy. The experiment ran for two years, in 2013/2014 and in 2014/2015.

The intervention

The existing anti-absenteeism policy (in college B and nationwide) targets students between 18 and 23 years old with more than sixteen hours of unauthorized absence within four consecutive weeks. When a student has more than sixteen hours of unauthorized absence within four weeks, he is registered at the office for absenteeism of college B. This office registers the student and contacts the Regional Contact and Coordination Office (RMC), the regional office for the prevention of early school leaving.[6] Social workers from the office guide the (former) student back to college or work. In the meantime the student's mentor from the college stays in contact with the student and the RMC and offers guidance such that a return to college B is possible.

The new anti-absenteeism policy differs from the existing policy in a number of ways. Most important, it aims to intervene earlier and more firmly in case of unauthorized absence. In contrast to the old policy, a student who has more than *eight* hours of unauthorized absence within the last four weeks (instead of sixteen hours) is registered at the office for absenteeism of college B. In this case, the office for absenteeism does not yet contact the RMC. First, the participant is called for a consultation at the office during which the student is informed and warned about the consequences

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[5] The 15 (out of 39) counselors that use the new intake procedure were selected by the school. There are no differences in level of education, years of experience in education, or in how they perceive the effectiveness of the intake to reduce school dropout.

[6] There are 39 RMCs in the Netherlands. Each RMC covers a geographical area in which a number of high schools and vocational college reside. See for a map: http://www.rmcnet.nl/.

of long lasting absenteeism and early school leaving. In addition, parents are informed about the absence. If, however, the student continues to be absent and reaches the threshold of sixteen hours of unauthorized absence, the office for absenteeism of college B will contact the RMC and the old policy enters into force.

The purpose of the appointment with the college office for absenteeism is twofold. On the one hand, it aims to raise awareness on the side of the student by letting him or her know (s)he is now being scrutinized. On the other hand, it aims to inform the student about possible consequences of frequent unauthorized absence, such that they can adjust their behavior. During the meeting, the counselor tries to motivate the student to take responsibility for his/her study career by emphasizing the importance of having good qualifications (better job opportunities, higher salary, etc.). The counselor also contacts the parents of the student to remind or point them at the consequences of unauthorized absenteeism and discuss mutual expectations and responsibilities regarding attendance and absenteeism. When personal or behavioral problems or learning difficulties play a role in the absenteeism of the student, the counselor can refer the student to assistance programs.

Table 3 Overview of the treatment and control condition in vocational training college B

existing anti-absenteeism policy (control)	new anti-absenteeism policy (treatment)

• if 8-15 hours of absenteeism within 4 weeks: *first time*
     o   no intervention
 	
 after first time
     o   no intervention
 • if 16+ hours of absenteeism within 4 weeks:
     o   college registers student and contacts regional office for the prevention of early school leaving
	• if 8-15 hours of absenteeism within 4 weeks: *first time*
	     o consultation in college office for absenteeism
	     o parents contacted about absenteeism
	 after first time
	     o no intervention
	 • if 16+ hours of absenteeism within 4 weeks:
	     o college registers student and contacts regional office for the prevention of early school leaving

Students are only called for a consultation at the office for absenteeism the first time (during the experiment) they have more than eight hours of unauthorized absence within four consecutive weeks.[7] Formally, students are not obliged to go to the meeting with the counselor and there is no sanction if they do not show up.

The experiment
The experiment focuses on two cohorts of students: a first cohort that consists of all students in school B in the school year 2013/2014 and a second, new cohort that consists of all students that newly entered school B in the school year 2014/2015. The experiment ran until the summer of 2015. Hence, the first cohort was in the experiment for two years and the second cohort only for one year. The first cohort consists of 4,017 students of whom 2,009 students were randomly assigned to the treatment regime and 2,008 to the control regime. The second, additional cohort consists of 2,296

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[7] There are some other cases in which the student is not called in for a consultation at the office for absenteeism. Most importantly this is the case if the student is already seeing the school's social worker, if the student is already registered at the RMC or if the student's mentor indicates that the student has grounded reasons for the absenteeism.

students of whom 1,149 students were assigned to the treatment regime and 1,147 in the control regime. Hence, in total, CPB randomly assigned 6,313 students aged 18-23 year old to either the treatment regime (3,158 students) or the control regime (3,155 students).

By comparing the control and treatment group, the impact of the new anti-absenteeism policy can be estimated (as compared to the existing policy). However, a large group of students will not be impacted by the new-anti-absenteeism policy because they are never absent for at least eight hours (within four consecutive weeks). To estimate the effect of the new anti-absenteeism policy on students that are impacted by the new policy, IV techniques can be used.

Students were not informed about the regime they were assigned to. As students and their parents were only informed via a newsletter that an experiment would take place to test a new anti-absenteeism policy, without giving more details, most students may not have been fully aware that an experiment was taking place.

3.3 Vocational training college C: E-coaches

School C introduced e-coaching for parents of first year students to battle early school leaving. The intervention aimed at improving parental involvement at home with the school career of their child. The impact of e-coaching is evaluated in an experiment, where the parents of a random selection of first-year students receive e-coaching. The experiment in college C ran in school years 2014/2015 and 2015/2016, one year later compared to the other colleges. Duration of the experiment was one year in both cases.

The experiment started with a one-year delay because college C originally wanted to evaluate a different intervention. The original intervention combined sports and career workshops. The intervention aimed at improving self-discipline, self-determination and self-control through especially for this purpose designed sports classes. The experiment to evaluate this intervention started at the start of school year 2013/2014. Students were randomized into the treatment or control group. Unfortunately, it turned out to be very difficult in practice to maintain a control group and a treatment group. As a result, college C decided to give the treatment to all students and opt for a new experiment in the following academic year. This new experiment was e-coaching for parents.

The intervention
The intervention at college C focused on stimulating parents' involvement at home with the studies of their son/daughter to reduce the drop out of (first year) students. The idea for using e-coaching originates from the health care field, where numerous studies show that e-interventions are an effective way of counseling. Its main strengths are that it is easy accessible, easy to fit into one's own schedule and that it lowers the sense of shame.
The e-coach intervention was designed for parents of first year students under 18 at college C. The e-coach intervention was a combination of different elements. First, the e-coach regularly (once every six weeks) contacts parents via e-mail. The email that is sent has been created centrally and contains information on how parents can best support their child. E-coaches are free to add some

personal lines to the email, but in general they did not do so. Second, the e-coach helps parents by answering questions they have on non-attendance, involvement and career choices and also more general pedagogical questions. Third, parents receive a digital newsletter four times per year, specially designed for the experiment and again containing information on supporting their child, involvement, etc. Fourth, parents are invited to specially organized information meetings around a certain theme, where they also can get into contact with the college and other parents. Fifth, a website (only accessible with a password) is set up with information. After a year the intervention ends. The effects of the intervention can however last beyond this if parents have gained insights, skills, etc. that they keep on using.

The appointed e-coaches are teachers from the college that receive a special training on how they can support parents in supporting their child. E-coaches support on average nine parents, although the caseload varied quite a bit between fields of study, from 2 to 20 parents per e-coach.[8] Some fields of study appointed the tutor of the student as his/her e-coach, other fields of study choose to appoint another teacher as the e-coach of the student's parents.

Table 4 Overview of the treatment and control condition in vocational training college C

no e-coaching (control)	e-coaching (treatment)
• no e-coach	• e-coach contacts parents by email every six weeks, email contains info how to support child
• no e-coach	• e-coach answers questions from parents
• no newsletter	• digital newsletter 4 times per year
• no invitation for thematic meeting	• thematic information meeting 2 times per year.
• no access to special website	• access to special website with information

The experiment

Effectiveness of the intervention was tested by randomly assigning an e-coach to the parents of 50% of the first year students and comparing the outcomes of students with and without an e-coach.

All first-year students under 18 from the participating fields of study were randomly assigned (by CPB) to either the control or the treatment group. Older students were not selected as parental involvement with children 18 years or over is subject to different legal rules. In the first (academic) year the experiment ran (2014/2015), random assignment was only stratified by field of study. In the second year, stratification on the class level was used. This is one level lower than field of study: most fields have sufficient students to fill more than one class. Stratification on the lower (class-) level in the second year was performed to balance the workload of e-coaches, who usually were appointed as e-coach for all students in the treatment group within a certain class. The lottery to randomly assign students to the treatment or control group was performed at the start of the academic year by CPB. In the first year, 547 students participated, 271 of whom were assigned to the treatment group and 276 to the control group. In the second year, 684 students participated, 341 of whom were assigned to the treatment group and 343 to the control group.

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[8] In the first year the experiment ran, we performed a survey among e-coaches. They survey was held early March, so more than halfway the academic year.

Parents from students in the treatment group were informed that they took part in a research project approved by the ministry of Education to analyze the effect of e-coaching as a tool to improve the support system for students. Parents from students in the control group were not informed about the experiment.

4. College level analysis

First, we analyze developments in student dropout rates at the college level. This allows us to distinguish between general developments at all vocational training colleges in The Netherlands, and specific developments at the colleges that applied for and/or were selected for the RCT subsidy. One disclaimer is in order here: we are underestimating the impact of the RCT interventions on dropout rates by looking at college dropout rates. This is because by analyzing average dropout numbers on the college level we ignore the fact that a substantial percentage of students in the selected institutes were not exposed to the intervention. At college B the intervention applied to a quarter of the students , while at college A and C it applied to a maximum of 5% of students. The individual level results in the next section are not hampered by this problem.

Figure 1: Average dropout rates per (group of) vocational training college(s)
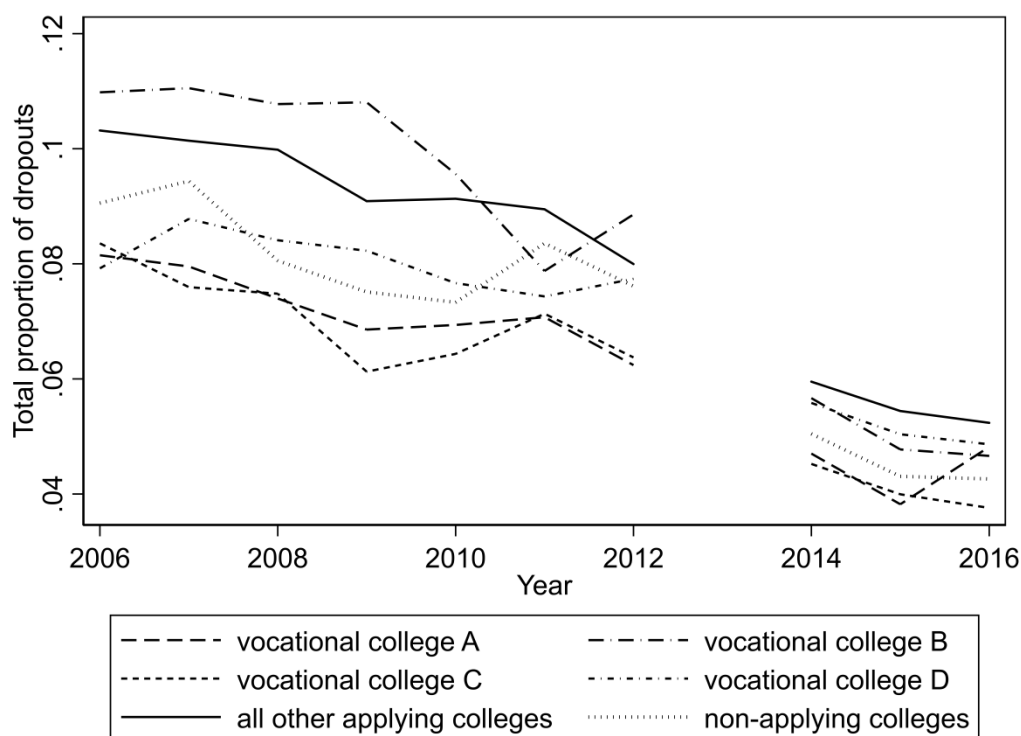
Data: DUO, own calculations, institutes with fewer than 10 students are dropped here.

We use college level dropout data from the Dutch authority that deals with student registers and school funding (DUO) for the academic years 2005/2006 up until 2015/2016. There is a large break in the way dropout is calculated in academic year 2012/2013, therefore we do not report any data for this year. Interestingly, this is the schoolyear in which colleges could apply for the subsidy. So we have dropout data for each vocational training college in the years prior to the subsidy call, and we

have data for the years in which the experiments were executed. Note that the experiment only started in 2014/2015 in vocational training college C and never truly started in college D.

At the college level we can compare dropout rates of schools that have never applied for the subsidy to schools that applied but did not get the subsidy and to the selected schools. We can compare these numbers before and after the start of the experiment (2013/2014 initially). The results are shown graphically above. Eyeballing the figure, there does not seem to be a diverging trend between the participating and non-participating schools. Indeed, dropout rates go down, but this is the case for all other schools too and it is also partly due to a different way of calculating the dropout rates.

To formally test the differences between the several subgroups of vocational training colleges, we have added Table 5. The upper panel compares colleges that did and did not apply for the subsidy. Dropout numbers in the year before application (2011/2012) are slightly higher in colleges that do apply (7.8% vs 7.7%), but the difference is small and not significant. In the years that followed, dropout rates decreased for all colleges. The dropout rates decreased, however, much more among colleges that did not apply than among colleges that applied. In 2015/2016, when the experiments have finished, the difference is 2.2%points and highly significant.

The lower panel of Table 5 compares colleges that were granted a subsidy and colleges that were not granted a subsidy, within the group of colleges that applied. Before application for the subsidy, in 2011/2012, the dropout rate among colleges that were eventually granted the subsidy was 0.7%point lower (not significant because of small N) than among colleges that were denied the subsidy. After the experiment, in 2015/2016, the dropout rate had decreased for both groups, but the 0.7%point difference in dropout rate remained.

Table 5 Differences in average dropout rates between selected, applied and other colleges over time

	Applied	Did not apply	p-value
Dropout rates in 2011/2012 (before application)	7.8%	7.7%	0.912
Dropout rates in 2013/2014 (start experiment)	5.7%	5.0%	0.241
Dropout rates in 2015/2016 (after experiment)	5.0%	4.3%	0.118

	Granted	Denied	p-value
Dropout rates in 2011/2012 (before application)	7.3%	8.0%	0.400
Dropout rates in 2013/2014 (start experiment)	5.1%	6.0%	0.206
Dropout rates in 2015/2016 (after experiment)	4.5%	5.2%	0.253

Data: DUO, own calculations, institutes with fewer than 10 students are dropped here.

The college level analyses shows that the institutes that performed RCTs do not do significantly better after the experiments. This is confirmed in a simple difference-in-difference estimator on the institute level. This analysis (available upon request) shows that time is the only explanatory factor in explaining dropout rates (N=67).

5. Individual level analysis

5.1 Data

For all students involved in the experiment, the vocational training colleges provided information on background characteristics (age, gender, field of study, country of birth) and on several intermediate outcomes, such as enrollment, unauthorized absenteeism, and graduation. For school B we also know which students actually had a consultation with the college office for absenteeism.

To identify school dropout, the college-level data are supplemented with national-level administrative data from Statistics Netherlands on enrollment in education to check whether a student that has dropped out of one of the participating colleges dropped out of the education system completely or enrolled in another school.[9][10] In the combined dataset, with data from colleges and nationwide administrative data, the educational careers of students in the experiment can be tracked until the start of school year 2016/2017. More specifically, we observe all diplomas obtained after (and before) the start of the experiment and all enrollments registered until October 1, 2016. With this information, we can hence identify the dropouts.

5.2 Sample selection

In total, our dataset contains 8,238 students. However, not all of these 8,238 students are relevant for the dropout prevention policies investigated in this paper. Recall that the official definition of *school dropout* is 'leaving the educational system without a basic qualification before the age of 23'. Some of the students that participated in the experiment had already obtained a diploma that qualifies as a basic qualification before the start of the experiment. They are therefore – by definition – not 'at risk' of dropping out of school, and we exclude them from our estimation sample. Furthermore, some students participating in the experiment did not meet the age criterion, as they were already 22 years or over on the first of October in the year the experiment started.[11] These students are also excluded from the estimation sample.

This leaves us with an estimation sample containing 526 students for vocational college A, 6,061 students for vocational college B and 1,193 students for vocational college C. For this sample, school dropout is measured one, two and three years after the start of the experiment.

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[9] Note that the definition of school dropout in The Netherlands is 'leaving *the education system* without an appropriate diploma (*startkwalificatie*)'.

[10] This could be done using the personal identification numbers (BSN) of each student. Out of a total of 8,361 observations, only 30 students could not be found by Statistics Netherlands due to missing or wrong personal identification numbers. Most of these were in school A. Another 7 students were excluded from the analysis as their records were included twice in the data. Furthermore, we excluded potential students who could not be assigned to either of the two school years involved in our experiment (School A), students who never started and/or only have missing absence data (School B), and students who never started and/or were enrolled into a study program that eventually did not participate in our experiment (School C). This leaves us with a total number of 8,238 observations.

[11] According to the official definition, only students who are aged 21 or below on October 1 in year *t* are included in the dropout numbers for year *t*+1. See https://www.rijksoverheid.nl/onderwerpen/vsv/cijfers-schooluitval-meten (in Dutch) for a detailed description of how dropout rates are measured in the Netherlands.

Table 6 provides descriptive statistics on dropout rates for the three participating vocational training colleges and their (potential) students involved in our experiment.

	Vocational college A	Vocational college B	Vocational college C
number of observations in full sample	710	6,310	1,218
number of observations in estimation sample	526	6,061	1,193
College level dropout rate before experiment (2011/2012)	6.2%	8.9%	6.4%
For estimation sample only			
% in treatment group (for estimation sample)	50.0%	50.2%	49.6%
Dropout rate in control group 1 year after start experiment	14.4%	11.1%	2.5%
Dropout rate in control group 2 yrs after start experiment	16.7%	12.8%	7.2%
Dropout rate in control group 3 yrs after start experiment	26.5%	14.2%	

There are some notable differences between the three schools participating in our experiment. School B has a much higher dropout rate (8.9%) at the college level before the experiment started compared to college A (6.2%) and C (6.4%). In the estimation sample, however, the dropout rate is highest at vocational college A and lowest at vocational college C. These large differences arise because of the different target group of the intervention at the colleges. At vocational college C, the e-coach intervention was specifically aimed at first year students under 18. In the Netherlands, most students at vocational colleges have an implicit compulsory scholing age of 18 (this is the so-called *kwalificatieplicht*), effectively preventing them from leaving school before that age. As a result, Table 7 shows a significantly lower average age at the start of the experiment for students in college C. Furthermore, there are differences in educational level, field of study, gender and migration background between the students in the experiment at the different colleges (see below).

5.2 Randomization

Randomizing students over a treatment and control group was performed by the authors of this paper for all vocational colleges. Table 7 contains balancing tables for the three colleges. It shows that the randomization worked out well in all schools for both the full sample (the original sample of students that were randomized over treatment and control group) and the estimation sample. The treatment and control groups do not differ in any of the observed characteristics, except for the variable '(parents with) migrant background' at college A.

We conclude that both in the full and the estimation sample the experiment was balanced.

Table 7 Balancedness of the full sample and estimation sample

	FULL SAMPLE			ESTIMATION SAMPLE		
Vocational college A: intake procedure						
	treatm	control	p-value	treatm	control	p-value
Female	47.9%	49.3%	(0.698)	49.0%	47.1%	(0.663)
Age at start of experiment	18.9	19.0	(0.692)	17.7	17.6	(0.421)
(Parents with) migrant background	36.7%	27.1%	(0.006)	34.6%	26.2%	(0.037)
Level 2	51.3%	49.9%	(0.704)	52.1%	53.6%	(0.727)
Level 3	14.3%	17.2%	(0.299)	10.3%	10.6%	(0.887)
Level 4	34.4%	33.0%	(0.689)	37.6%	35.7%	(0.652)
Field: commerce & business	23.5%	20.2%	(0.292)	24.7%	23.2%	(0.683)
Field: media & design	14.6%	15.2%	(0.816)	13.3%	15.2%	(0.534)
Field: transport & logistics	14.3%	13.3%	(0.691)	12.2%	12.9%	(0.793)
Field: health care	47.6%	51.2%	(0.327)	49.8%	48.7%	(0.794)
N	349	361		263	263	

	FULL SAMPLE			ESTIMATION SAMPLE		
Vocational college B: absenteeism counselors						
	treatm	control	p-value	treatm	control	p-value
Female	48.8%	48.7%	(0.939)	49.3%	49.3%	(0.965)
Age at start of experiment	18.6	18.6	(0.412)	18.6	18.6	(0.619)
(Parents with) migrant background	21.3%	22.1%	(0.455)	21.8%	22.5%	(0.516)
Level 1	4.0%	4.2%	(0.746)	4.1%	4.3%	(0.695)
Level 2	31.7%	31.6%	(0.970)	31.3%	31.7%	(0.730)
Level 3	20.3%	20.4%	(0.965)	20.0%	19.3%	(0.483)
Level 4	44.0%	43.9%	(0.897)	44.5%	44.6%	(0.937)
Field: interior/furniture	1.0%	1.1%	(0.537)	1.0%	1.2%	(0.436)
Field: construction	4.4%	4.4%	(0.996)	4.3%	4.4%	(0.842)
Field: economics & administration	14.6%	14.6%	(0.923)	14.6%	14.5%	(0.911)
Field: commerce & business	6.9%	6.9%	(0.955)	7.0%	6.8%	(0.794)
Field: food services	7.8%	7.9%	(0.920)	7.8%	7.8%	(0.966)
Field: ICT	4.1%	4.1%	(0.953)	4.1%	3.9%	(0.700)
Field: media & design	2.7%	2.5%	(0.636)	2.8%	2.6%	(0.674)
Field: mobility & vehicles	3.1%	3.3%	(0.717)	2.9%	2.9%	(0.986)
Field: technology & engineering	5.5%	5.3%	(0.743)	5.3%	5.2%	(0.928)
Field: transport & logistics	1.7%	1.8%	(0.773)	1.6%	1.8%	(0.523)
Field: hair & beauty	1.5%	1.6%	(0.916)	1.5%	1.6%	(0.800)
Field: safety & sports	7.2%	7.1%	(0.928)	7.2%	7.1%	(0.879)
Field: health care	35.5%	35.3%	(0.889)	35.8%	35.8%	(0.986)
N	3,156	3,154		3.044	3.017	

	FULL SAMPLE			ESTIMATION SAMPLE		
Vocational college C: e-coaching						
	treatm	control	p-value	treatm	control	p-value
Female	29.0%	28.8%	(0.913)	28.5%	28.8%	(0.928)
Age at start of experiment	16.5	16.5	(0.555)	16.5	16.5	(0.601)
(Parents with) migrant background	20.3%	22.2%	(0.412)	20.6%	22.3%	(0.478)
Level 2	39.8%	41.3%	(0.577)	40.5%	41.9%	(0.626)
Level 3	31.7%	30.9%	(0.763)	31.9%	31.3%	(0.811)
Level 4	28.5%	27.8%	(0.765)	27.5%	26.8%	(0.773)
Field: economics & administration	9.6%	9.3%	(0.878)	9.6%	9.5%	(0.933)
Field: commerce & business	12.2%	12.1%	(0.949)	11.0%	10.8%	(0.927)
Field: food services	3.8%	3.9%	(0.909)	3.9%	4.0%	(0.924)
Field: ICT	16.8%	16.8%	(0.999)	17.1%	17.1%	(0.972)
Field: hair & beauty	3.1%	2.6%	(0.587)	3.2%	2.7%	(0.576)
Field: safety & sports	38.3%	38.4%	(0.967)	38.7%	38.8%	(0.976)

Field: health care	15.8%	16.5%	(0.754)	16.2%	16.8%	(0.784)
N	606	612		592	601	

5.3 Compliance

Table 8 shows compliance to the treatment assignment for all three vocational colleges. In vocational college A, where a new intake procedure was introduced, all prospective students that were assigned to the treatment group indeed received the treatment. Virtually all prospective students that were assigned to the control group correctly received the existing intake procedure. As students did not know they were part of an experiment and were assigned to a treatment or control group, the no-show rate at the intake procedure should not differ between treatment and control group. This is the case; the differences in no-show rates are all insignificant.

For vocational college B, where a stricter absenteeism policy was introduced, there is information available on who had a meeting with a counselor, but not on hours of unauthorized absenteeism in the past four weeks (the criterion for the treatment group to be sent to a meeting with a counselor). Furthermore, not for every ground of exemption from the treatment information is available. In the first year, all students that met with a counselor were assigned to the treatment group. Hence, in the first year none of the students in the control group received the treatment. However, we do not know how many students assigned to the treatment group should have had a meeting with a counselor. Hence, we do not know whether there was full compliance from the students assigned to the treatment group. In the second year, some students assigned to the control group *did* meet with a counselor due to some technical error. Again, it is unknown whether all students from the treatment group that had more than eight hours of unauthorized absenteeism had a meeting with a counselor.

Table 8 Compliance with treatment assignment

	YEAR 1		YEAR 2	
Vocational college A: intake procedure				
	treatm	**control**	**treatm**	**control**
invited for / received existing intake procedure	0%	100%	0%	99%
invited for / received new intake procedure	100%	0%	100%	1%
no-show at intake procedure	16%	19%	15%	11%
Vocational college B: absenteeism counselors				
	treatm	**control**	**treatm**	**control**
meeting with counselor because 8+ hours of unauthorized absenteeism	20%	0%	26%	4%
Vocational college C: e-coaching				
	treatm	**control**	**treatm**	**control**
received e-coaching	94%	17%	‡	‡

‡ no exact information available

In vocational college C, which experimented with e-coaching, there were some problems in the execution of the randomization at the start. In the first year (2014-2015), 547 students participated in the experiment: 276 students were in the control group and 271 in the treatment group. 47 students in the control group erroneously received e-coaching (17%) and 17 students in the treatment group erroneously did not receive e-coaching (6%). This non-compliance was due to errors made by the e-coaches in contacting parents the first time. E-coaches filled in a log on every contact they had with parents so that we know exactly which parents were contacted. In the second year (2015-2016), 684 students participated in the experiment. 343 of them were assigned to the control group and 341 in the treatment group. The second year e-coaches did not fill in a log on the contacts they had with parents, and detailed information on compliance is therefore not available. As most of the e-coaches of the first year also were e-coach in the second year of the experiment, the probability of mistakes in contacting the right parents is likely to be (much) smaller than in the first year.

6. Individual level results

6.1 Empirical strategy

When individuals are randomly assigned to the treatment and control group, a comparison of the outcomes between the two groups yield the so-called intention-to-treat effect. This is the causal effect of being assigned to the treatment group. For policy this often is the most relevant measure as it resembles most closely what will happen if the policy is implemented: there will always be individuals in the treatment group that are not treated; for example, because the treatment is not relevant for them, or because some individuals leave the experiment before the treatment actually starts. The intention-to-treat effect is estimated for each experiment separately.

Alternatively, the local average treatment effect (LATE) can be estimated to measure the effect of the treatment on those that really received treatment. The local average treatment effect is especially relevant for the experiment in school B, as being assigned to the treatment or control group only makes a difference for students with more than eight hours of absenteeism in the past four weeks.

To obtain the intention-to-treat effect we estimate

$$Y_{ist} = \alpha_0 + \alpha_1 D_{is} + \alpha_2 X_{is} + \varepsilon_{ist} \tag{1}$$

where Y_{ist} is the outcome of student *i* in school *s* in year *t*, D_{is} a dummy variable indicating whether student *i* in school *s* is assigned to the treatment ($D_{is} = 1$) or the control group ($D_{is} = 0$), X_{is} a set of controls including students characteristics (age, gender, etc.), and ε_{ist} the error term which captures unobservable determinants of the outcome. Randomization of the treatment assignment ensures ε_{ist} and X_{is} are uncorrelated with D_{is}. The OLS-estimator of α_1 therefore represents the ITT-effect, i.e. the effect of being assigned to the treatment regime. The main outcome used for Y_{ist} is a dummy variable indicating whether a student has dropped out of school when he is observed at *t*. October 1 is used as the reference date to measure the outcome variable in year *t*. Students from cohort 2013/2014 can be followed for three years (1 October 2014, 1 October 2015, and 1 October 2016), students from cohort 2014/2015 can only be followed for two years (1 October 2015 and 1 October 2016).

To obtain the LATE-effect for school B, an Instrumental Variable (IV) approach is used where the treatment (early consultation (EC)) is instrumented with assignment to the treatment group. The first stage equals

$$EC_{it} \quad EC_{it} = \beta_0 + \beta_1 D_i + \beta_2 X_i + \epsilon_{it} \tag{2}$$

where EC_{it} represents a dummy variable indicating whether student i has attended at least one consult during the two school years (1=attended consult; 0=not attended consult) and β_1 reflects the difference in compliance rate between treatment and control group.[12] The second stage equals

$$Y_{it} = \gamma_0 + \gamma_1 \widehat{Consult}_i + \gamma_2 X_{is} + \theta_{ist}, \tag{3}$$

where γ_1 is the parameter of interest (LATE). The LATE estimate $\widehat{\gamma_1}$ is calculated as the intention-to-treat estimate ($\widehat{\alpha_1}$) divided by the difference in compliance rates from the first stage ($\widehat{\beta_1}$).In estimation we will use conventional standard errors that are robust to heteroscedasticity.[13]

6.2 Results on early signals for school dropout

Before a student drops out of school, there may already be signals observable that prelude dropping out. A signal that may indicate an increased risk for dropping out is unauthorized absenteeism. In all three experiments, no significant effect of the treatment on unauthorized absenteeism is found. The new intake procedure that was introduced at vocational college A may induce students that otherwise would have dropped out during the first year to not start the study they enrolled for. No significant effect of the renewed intake procedure on probability to start the study enrolled for is found.

To estimate the effect of the experiment on unauthorized absenteeism in the year the student started participating in the experiment, unauthorized absenteeism is used as outcome variable in equation (1) above. Results are shown in Table 9. The first panel gives the estimates for the pooled sample of cohorts used in the experiment. In this panel we use all our observations. In the other panels we split up our sample by cohort. Columns (1), (3) and (5) show the results when no control variables are included. If an experiment is executed correctly with random assignment to treatment and control group and full compliance, this suffices to estimate the intention-to-treat effect of the experiment (and resembles a simple difference-in-means test between the control and treatment group). Columns (2), (4) and (6) in Table 9 show results when a set of control variables is included in the regression. Including control variables is mainly important when there is some unbalancedness between the treatment and control group. As expected, the differences between the two specifications are small.

For all colleges we find no effect of the interventions in the experiments, as the estimates are not significantly different from zero in the pooled sample. However, if we split up our sample by cohort, we observe significant estimates for the first cohort in the experiment at college A. For this cohort the renewed intake procedure led to 5.8%points lower unauthorized absenteeism (as share of total teaching time). For the second cohort at college A the estimated coefficient is close to zero and not

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[12] In our case we have two-sided noncompliance in the second cohort of students (school year 2014/2015), i.e. a number of students have attended a consult in the control group. This was not the case for the first cohort of students. For this cohort we have only one-sided noncompliance.

[13] We do not have to cluster our standard errors because randomization has been done at the individual level.

significant. Unfortunately, the data on absenteeism at college A contain many missing values (about one-third of the observations is missing). Eyeballing these observations, especially the absenteeism data for students that dropped out seem to be missing relatively often.[14] This implies that the estimate for college A is based on a smaller subsample of students (only those for whom absenteeism is not missing) and that the results for college A will be biased. The direction of the bias is a priori not clear. It is therefore also not clear whether the effect found for the first cohort can be interpreted as the causal effect of the new intake procedure.

Table 9 also shows the results of estimating the effect of the treatment on the probability of having started in the field of study the student initially enrolled for. This only is a relevant outcome variable for the experiment at college A (renewed intake procedure). Also for this outcome variable, insignificant effects that are very close to zero are found.

Table 9 Estimated effect of the treatment condition on switch in field of study and unauthorized absenteeism

	Vocational College A: intake procedure		Vocational College B: absenteeism counselors		Vocational College C: e-coaching parents	
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort 1+2 (pooled)						
unauthorized absenteeism[a]	-0.002	-0.019	-0.065	-0.062	-0.005	-0.005
	(0.023)	(0.020)	(0.175)	(0.168)	(0.004)	(0.004)
N	334	334	6,061	6,061	1,191	1,191
Started in initial field of study	0.000	0.012				
	(0.035)	(0.034)				
N	526	526				
Cohort 1						
unauthorized absenteeism[a]	-0.058*	-0.058*	-0.054	-0.054	-0.006	-0.007
	(0.034)	(0.032)	(0.196)	(0.185)	(0.006)	(0.006)
N	136	136	3,836	3,836	520	520
Started in initial field of study	0.047	0.049				
	(0.057)	(0.054)				
N	208	208				
Cohort 2						
unauthorized absenteeism[a]	0.027	0.004	-0.085	-0.051	-0.004	-0.002
	(0.030)	(0.027)	(0.333)	(0.326)	(0.006)	(0.005)
N	198	198	2,225	2,225	671	671
Started in initial field of study	-0.030	-0.015				
	(0.044)	(0.044)				
N	318	318				
Controls		✓		✓		✓

Full set of controls: Gender, age at start of experiment, immigrant, and field of study, level, and year fixed effects.
[a]*: defined as percentage unauthorized absence during first year (weighted by total teaching time) (School A and C) or monthly average hours of unauthorized absence in the period October-June (School B).*

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[14] For students that drop out, in 49% of the cases data on absenteeism is missing. For students that do not drop out, this is only 35%.

6.3 Results on school dropout

Our main object of interest, and the ultimate goal of the Ministry's subsidy program for experiments, is to see whether the experiments lowered early school dropout. Table 10 shows the results of the experiments for this outcome. The upper panel gives the estimates when we pool the two cohorts of the experiment. Again, we use all observations in this panel. The results from this sample are our preferred estimates as they give the average treatment effect over all cohorts. In the other panels we split up our sample by cohort. Both the results of the regressions without control variables (columns (1), (3) and (5) of Table 10) and the results of the regressions with control variables (columns (2), (4) and (6)) are shown.

The main finding is that for all three experiments the estimated effects of the interventions are not significantly different from zero after 1, 2 and 3 years from the start of the intervention. For college B, the estimated effects are close to zero and relatively precise because of the size of this sample.

However, a closer look at each of the two cohorts separately reveals a disparity between the two cohorts in the effect of the intervention. The disparity encompasses sign, size and significance of the estimated coefficients.

The estimated effect of the new intake procedure at vocational college A after one year is negative (it lowers the dropout rate) and sizeable (-8.2%) for the first cohort, but not significantly different from zero. The size of the effect diminishes slowly if time proceeds. For the second cohort however, the estimated effect after one year is positive (it increases the dropout rate) and much smaller (+1.4%) and again not significantly different from zero.

For vocational college C the estimated effect of the e-coaching intervention after one year is very close to zero for both the first and second cohort. For the first cohort also the effect on dropout rates two years after the experiment can be estimated. After two years the effect is larger (-3.0 %points), but not significantly different from zero.

The new anti-absenteeism policy at college B had a significant effect on the dropout rate after one year for the first cohort. Students assigned to the treatment group had a 2.3% point lower probability to drop out. The effect for the first cohort after two and three years is smaller and no longer significantly different from zero. For the second cohort, the effect after one year, however, is positive, but not significant. For the second cohort after two years a positive effect of 2.3%point that is significant at the 10% level is found.

	Vocational College A: intake procedure		Vocational College B: absenteeism counselors		Vocational College C: e-coaching parents	
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort 1+2 (pooled)						
Dropout without degree	-0.015	-0.026	-0.011	-0.009	0.005	0.005
1 yr after start experiment	(0.030)	(0.029)	(0.008)	(0.007)	(0.010)	(0.009)
N	526	526	6,061	6,061	1,193	1,193
Dropout without degree	-0.015	-0.024	-0.002	-0.000	-0.030	-0.030
2 yrs after start experiment	(0.032)	(0.031)	(0.009)	(0.008)	(0.020)	(0.021)
N	526	526	6,061	6,061	521	521
Cohort 1						
Dropout without degree	-0.084	-0.082	-0.025**	-0.023**	0.0004	-0.0004
1 yr after start experiment	(0.054)	(0.051)	(0.010)	(0.010)	(0.013)	(0.013)
N	208	208	3,836	3,836	521	521
Dropout without degree	-0.085	-0.070	-0.016	-0.014	-0.030	-0.030
2 yrs after start experiment	(0.057)	(0.055)	(0.011)	(0.010)	(0.020)	(0.021)
N	208	208	3,836	3,836	521	521
Dropout without degree	-0.076	-0.066	-0.007	-0.005		
3 yrs after start experiment	(0.058)	(0.056)	(0.011)	(0.010)		
N	208	208	3,836	3,836		
Cohort 2						
Dropout without degree	0.028	0.014	0.014	0.014	0.009	0.008
1 yr after start experiment	(0.035)	(0.033)	(0.012)	(0.011)	(0.013)	(0.013)
N	318	318	2,225	2,225	672	672
Dropout without degree	0.028	0.011	0.021	0.023*		
2 yrs after start experiment	(0.037)	(0.036)	(0.014)	(0.013)		
N	318	318	2,225	2,225		
Controls		✓		✓		✓

Full set of controls: Gender, age at start of experiment, immigrant, and school, field of study, level, and year fixed effects.
*Note: * indicates significance at 10% level, ** at 5% level and *** at 1% level.*

As explained in section 6.1, for school B it is also relevant to estimate the local average treatment effect (LATE) to measure the effect of the treatment on those that really received treatment. Table 11 shows the results of the IV procedure. Again the upper panel shows results for the pooled sample of the two cohorts and the other panels for the cohorts separately.

The first stage (results in second row), where early consultation (*EC*) is explained by assignment to the treatment group is (as expected) very strong. Assignment to the treatment group strongly predicts early consultation. In the second stage, where the effect of early consultation is estimated for those who received early consultation, the estimated effects are found to be insignificant in all 3

experiments when using the pooled sample. The estimated effects are close to zero after 2 years from the start of the experiment.

However, if we split up our sample by cohort we find that in the first cohort a significant negative effect is found in the first year after the experiment. This effect fades out after two and three years: the coefficient becomes smaller and is no longer significantly different from zero. For the second cohort no significant effect is found after one year, but after two years the estimated effect is significant at the 10% level after two. These effects are, in contrast with the effects for the first cohort, all positive. And where the effect for the first cohort faded out after one year, the effect for the second cohort grows larger.

Table 11 Regression results for Local Average Treatment Effect for Vocational college B

	Dropout without degree 1 yr after start experiment		Dropout without degree 2 yrs after start experiment		Dropout without degree 3 yrs after start experiment	
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort 1+2 (pooled)						
IV	-0.051	-0.043	-0.010	-0.001	-0.026	-0.017
	(0.036)	(0.034)	(0.039)	(0.037)	(0.043)	(0.040)
First stage	0.218***	0.217***	0.218***	0.217***	0.261***	0.261***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)
N	6,061	6,061	6,061	6,061	3,836	3,836
Cohort 1						
IV	-0.098**	-0.089**	-0.060	-0.052	-0.026	-0.017
	(0.040)	(0.037)	(0.042)	(0.039)	(0.043)	(0.040)
First stage	0.261***	0.261***	0.261***	0.261***	0.261***	0.261***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
N	3,836	3,836	3,836	3,836	3,836	3,836
Cohort 2						
IV	0.095	0.101	0.149	0.163*		
	(0.084)	(0.079)	(0.096)	(0.091)		
First stage	0.143***	0.142***	0.142***	0.142***		
	(0.016)	(0.016)	(0.016)	(0.016)		
N	2,225	2,225	2,225	2,225		
Controls		✓		✓		✓

How can the remarkable finding of negative effects for the first cohort and positive effects for the second cohort be explained? First, this could arise when the first and second cohort in the experiment are very different in how they react to the treatment. There are however no major differences in the composition of the first and second cohort and we would therefore not expect the reaction to the treatment to be very different. Second, different execution of the experiment could also result in different results for the treatment effect. To our knowledge, the experiment was performed in the same way for both cohorts. The remarkable difference between the first and second cohort therefore remains an unsolved puzzle.

7. Conclusion and discussion

This paper evaluates three RCTs on interventions to prevent early school dropout at vocational colleges. The interventions were designed by the vocational colleges themselves and selected out of in total thirteen applications for a subsidy program, initiated and financed by the Dutch Ministry of Education. Four applications were rewarded, of which three were successfully executed at three different vocational colleges. The interventions differed in nature and ranged from a renewed intake procedure (college A) to absenteeism counselors (college B) to e-coaching for parents (college C). There was great enthusiasm for the experiments at the vocational colleges which resulted in high compliance with the randomization of students over a treatment and control group.

The effect of the intervention is analyzed not only for school early drop-out, but also for unauthorized absenteeism, an early signal for dropping out of school. At all three vocational colleges, the experiment had a decreasing effect on unauthorized absenteeism, but this effect is generally not significant.

Unfortunately for the participating vocational colleges, none of the interventions seems to have had a significant overall impact on school dropout rates. Only if we split up our sample by cohort a significant estimated effect arises of the intervention for the first cohort at vocational college B, where students were invited for an appointment with college office for absenteeism at a lower number of hours absence than before. Given that this effect fades away after one year, and that the set-up of the intervention did not change between the first and second cohort, we choose to regard this significance as a statistical artifact of splitting the sample in many subgroups. All in all, this paper thus concludes that school dropout rates did not decrease significantly in three randomized controlled trials (RCTs) in Dutch vocational training colleges. Simple analyses at the school level also show that participation in the subsidy program did not decrease average dropout rates.

There can be multiple explanations for this finding. First, the number of observations could be too small. Although the experiments could gain precision from including more students, the estimated coefficients are consistently very close to zero. Second, the participating colleges could be colleges that already had very low school dropout rates before the start of the experiments, which made it more difficult to lower these rates even further. Among all vocational colleges, college A and college C indeed had relatively low dropout rates. However, (almost) all vocational colleges managed to lower school dropout rates, both before and after the period in which the experiment took place (see Figure 1 in section 4). Third, it could be that the interventions have no *additional* effect on school dropout rates, given the efforts already in place to prevent early school leaving. At all vocational colleges, the intervention under evaluation was not the only effort the vocational college

undertook to prevent early school dropout. Fourth, the control group can be indirectly influenced by the treatment. This could be the case, for example, in vocational college C, where teachers received a training to be an e-coach. Teachers might have used their new skills also in the regular contact they had with students in the control group. If the new skills are important in the effectiveness of the intervention, the control group may have benefited from the treatment group, resulting in smaller differences in outcomes between treatment and control group.

Moreover, our findings should not be interpreted as a failure of the entire subsidy program. It shows that piloting or testing new interventions using an RCT set-up is possible and that these small scale trials can prevent interventions that turn out to be ineffective to be rolled-out on a large scale.

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Appendix

Table A.1 Regression results for Local Average Treatment Effect for Vocational college B, pooling cohort 1 and 2

	Dropout without degree 1 yr after start experiment		Dropout without degree 2 yrs after start experiment		Dropout without degree 3 yrs after start experiment	
IV	-0.051	-0.043	-0.010	-0.001	-0.026	-0.017
	(0.036)	(0.034)	(0.039)	(0.037)	(0.043)	(0.040)
First stage	0.218***	0.217***	0.218***	0.217***	0.261***	0.261***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)
N	6,061	6,061	6,061	6,061	3,836	3,836
Controls		✓		✓		✓