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The impact of training vouchers on low-skilled workers☆

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H I G H L I G H T S

- Evaluates effects of training vouchers for low-skilled using randomized experiment
- Vouchers increase training participation by 0.2, relative to a 0.45 base
- Increased participation comes at deadweight loss of 60%
- No significant impact on monthly wages or on job mobility
- New trainees differ in observed characteristics from always-takers and never-takers

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A B S T R A C T

This paper reports about a randomized experiment in which training vouchers of €1000 were given to low-skilled workers. The vouchers increase training participation by almost 20 percentage points in two years, relative to a base rate of 0.45. This increased participation comes at a substantial deadweight loss of almost 60%. Consistent with predictions from human capital theory, we find that vouchers cause a shift towards more general forms of training. We do not find any significant impact of the program on monthly wages or on job mobility. The program does, however, have a significant impact on future training plans. Compared to always-takers, new trainees are more often male, more risk averse, work shorter hours and are less likely to have participated in training prior to treatment. Compared to never-takers, they are more often female, work longer hours and have a somewhat lower formal education level.

1. Introduction

This paper reports about a randomized experiment designed to evaluate the impact of training vouchers on training participation and labor market outcomes of low-skilled workers.[1] The experiment was initiated by the Dutch government which, like governments elsewhere, is concerned with the human capital acquisition of its low-skilled citizens. Governments of European countries have even set explicit and

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[1] To avoid confusion, this paper looks at training participation of people who are employed at the moment that the vouchers are assigned. A different literature studies public-sector training programs which are almost exclusively targeted at the unemployed. See Doerr et al. (2013) for a recent analysis of training vouchers in the context of active labor market policies.

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ambitious targets regarding the participation of adults in further education (Messer and Wolter, 2009).

Training vouchers are one of the possible instruments that governments use, or are considering to use, to stimulate adult learning. Training vouchers give recipients an earmarked budget that they can spend on training courses. A key element of this instrument is that it gives workers the freedom to choose in which course to enroll. It also allows all workers to participate, including those that might not be "cherry picked" by their employers.[2] In that sense it is very different from for example tax facilities that allow employers to deduct training expenditures from their tax (as evaluated in Leuven and Oosterbeek, 2004).

In the experiment more than 600 (out of 1266) low-skilled workers in The Netherlands were given training vouchers of €1000 each. We examine the impact of these vouchers on: i) the training participation of the recipients; ii) the type of training attended; iii) earnings; iv) job mobility; and v) future training plans. Comparing training participation rates between treated and controls is informative about voucher take-up, about the importance of liquidity constraints in training participation, and about the deadweight loss due to vouchers (how many vouchers are used for training that otherwise also would have taken place). Comparing the type of training attended between treated and controls tells us something about the constraints that workers face when their training is funded by their employers. Comparing earnings and job mobility between treated and controls is informative about the impact of the extra training or the different types of training induced by vouchers on labor market outcomes. Finally, comparing future training plans between treated and controls informs us about possible dynamic spillovers of voucher-induced training participation. If "training begets training" this should be taken into account when judging the effectiveness of vouchers.

While there is an extensive economics literature on work-related training,[3] evidence on the impact of training vouchers is very limited. Two studies are, however, closely related to ours. The first is Schwerdt et al. (2012) who analyze an adult education voucher program in Switzerland (see also Messer and Wolter, 2009). This paper is based on a field experiment in which 2437 Swiss adults were given education vouchers which they could use for any form of training during the first six months of 2006. The sizes of the vouchers were 200, 750 or 1500 Swiss francs. 18.4% of these vouchers were redeemed. The study finds no significant average effects of the program on earnings, employment, and subsequent education one year after treatment. Effects are, however, heterogeneous: low-educated individuals are the most likely to benefit from adult education. But they are at the same time the least likely to use the voucher.

Although the Swiss experiment and the experiment that we analyze share some relevant features, there are some noteworthy differences: i) while the Swiss study covered people from all skill levels, the Dutch experiment focussed on low-skilled employees and was conducted in four industrial sectors in which the majority of the workers is low-skilled; ii) in the Swiss experiment the redemption period was 6 months, in the Dutch experiment this period is two years; iii) in the Swiss experiment vouchers were equal to 200, 750 or 1500 Swiss francs, in the Dutch experiment to €1000, making the Dutch voucher slightly larger than the largest voucher in Switzerland.[4]

The second study closely related to ours, is the report of Doets and Huisman (2009) who evaluated the same experiment as we do here.[5] Our reevaluation of the data from the experiment is justified on two grounds. First, we repair an important flaw in their analysis. Based on respondents' answers about their perceived treatment status, Doets and Huisman (2009) reassign 21% of the treated to the control group, and 6% of the control group to the treatment group. This is likely to invalidate the interpretation of their results as causal effects since (as we will show) being misinformed about the actual treatment status is not random.[6] Second, Doets and Huisman (2009) only look at the impact of the vouchers on training participation. We also analyze the impact of the program on wages, the probability of changing jobs and on future plans to enroll in a course. In addition we assess the impact of vouchers on the type of training and we characterize the workers who respond to the voucher program. In short, although we evaluate the same experiment, we address different questions and use a different methodology than Doets and Huisman (2009).

Relative to a two-year base rate of 0.45, receiving a voucher increases training participation by almost 20 percentage points. This increase in participation comes with a deadweight loss of 59%.[7] This means that more than half of training funded by the voucher program would have been financed by private funds in the absence of the program. Compared to people who also participate in training without a voucher, new trainees are more often male, more risk averse, work shorter hours and are less likely to have participated in training prior to treatment. Compared to people who even with vouchers do not participate in training, they are more often female, work longer hours and have a somewhat lower formal education level. We do not find a significant impact of the program on monthly wages nor on the probability of changing jobs. The program does, however, have a significant impact on future training plans.

The rest of this paper is organized as follows. The next section briefly summarizes the economics literature on work-related training. The purpose of this is to put the contribution of the current paper into perspective. Section 3 describes the experiment and the data. Section 4 describes the empirical approach. Section 5 presents and discusses the results. Finally, Section 6 summarizes and concludes.

2. Related literature

This section briefly summarizes contributions from the economics literature on work-related training that are relevant for the results of the current paper. The first subsection reviews the theoretical training literature, and more specifically the distinction between general and specific training. This is important for our paper in order to see why training vouchers may cause substitution towards general training and away from specific training. This distinction is also useful to understand the possible impact of vouchers and voucher-induced training on labor market outcomes. The second subsection reviews the empirical training literature. This contains two parts: the determinants of training receipt and the wage-returns to training.

2.1. Theory

One of the main purposes of introducing vouchers is that they give workers the choice of courses they wish to take. This is likely to reduce the influence that employers have on training decisions. It is therefore important to understand the different incentives for each party to invest on training. Human capital theory, as formalized by Becker (1962), provides a theoretical framework to analyze this (see Leuven, 2005 for a review).

Human capital investments are embodied in individual workers, so once a worker is trained, firms will benefit from this knowledge or

[2] See Groot and Maassen van den Brink (2009) for an overview of the forms of training vouchers and related instruments that are in place in different countries.

[3] We briefly summarize this literature in Section 2 to put our study and its results in perspective.

[4] The two experiments were designed independently. Otherwise there would perhaps have been scope for alignment in order to improve comparability, for example of the phrasing of questions in the respective surveys.

[5] One of the authors of the current paper (Oosterbeek) was together with Doets involved in the design of the experiment.

[6] In Appendix A of this paper we discuss this issue in more detail and report estimation results that show that being misinformed about the actual treatment status is significantly correlated with gender, level of formal education, training participation prior to treatment, risk tolerance and firm size.

[7] When we reproduce the results reported in this paper using the wrong assignment of Doets and Huisman (2009), we find a much larger effect of vouchers on training participation and a much smaller deadweight loss of the voucher program.

ability only if the worker stays with the firm. If the worker can move easily to another firm, then firms lose their incentives to invest in training. It is in this context that economists differentiate between specific and general training.[8] General training refers to skills that the worker can use in many other firms, so if s/he changes jobs, those skills can be used in the new job as well. Specific training refers to skills that are not portable to other firms and are useful only to the current firm (Becker, 1962).

In case of general training, firms will try to poach trained workers from other firms. Anticipating this, firms will refrain from training their workers (Leuven, 2005). If this is the case then since vouchers represent a contribution to the workers, they should use it for general courses. This training will result in a wage increase in their current job or in another firm if they are poached.

Indeed, theory suggests that workers will finance general training themselves. Workers are willing to take a wage cut during training since the training will pay off later on. However, if the worker has a liquidity constraint or if there exist minimum wage regulations which prevent a wage cut then workers will underinvest in general training (Leuven, 2005). It makes sense then, that if governments are going to finance some type of training it should be general, targeting especially those who face liquidity constraints.

In an imperfectly competitive labor market it is possible that some skills are more productive in the worker's current firm than in other firms, i.e. specific training. If the firm can capture the entire gains from training it will be willing to invest efficiently, unless the worker inefficiently leaves the firm. This will cause the firm to lose the whole investment. In this scenario firms will have an incentive to pay higher wages to avoid turnover. One solution for an efficient provision of training is long-term contracting. Here both parties will incur the costs of specific training and both will receive the benefits. This is dependent on the two parties staying together. However since one party knows that the other will suffer if they separate, it has an incentive to try to get more returns by threatening the other party to end the contract. Under this setting, known as “hold-up”, there will be underinvestment since the investor will not receive the full marginal return on the investment (Leuven, 2005). Uncertainty about the gains of specific training should result in workers using their voucher for general training instead.

2.2. Empirics

While the distinction between general and specific training is conceptually straightforward, it is hard to operationalize empirically. Consequently, the empirical training literature is — in most cases — only loosely connected to the theoretical training literature. The empirical training literature has dealt with two main issues: the determinants of training participation, and the wage returns to training.

2.2.1. Determinants of training

Most studies that examine the determinants of training participation regress an indicator of training participation (often during the past 12 months) on explanatory variables. Recent examples of this approach are Albert et al. (2010), Watanabe (2010) and Thangavelu et al. (2011); older studies include Booth (1993), Barron et al. (1993), Greenhalgh and Stewart (1987) and Pischke (2001). While there are differences across countries and datasets, it is common to find that (1) men are more likely to participate in training than women; (2) training participation is higher among more highly educated people; (3) training participation increases with firm size; and (4) training participation decreases with age.

Whether these relationships reflect the preferences of workers or of firms is unclear. Using survey information on workers who report that they were restricted in their training choices, Leuven and Oosterbeek (1999) attempt to disentangle workers' and firms' preferences. They find that different training levels by level of education can be attributed to workers' preferences. The same holds for the age effect on training, while they attribute the gender training gap to firm preferences.

2.2.2. Returns to training

A challenge in the estimation of the returns to private sector training is to address the endogeneity of training participation. Participants and non-participants are likely to not only differ in terms of their observed characteristics but also in terms of their unobserved characteristics. This does not only entail unobserved characteristics of the worker (such as motivation and ability) but possibly also unobserved characteristics of the job or the employer (new equipment, entering a new market). Brunello et al. (2007) provide an extensive summary of relevant studies (see also Haelermans and Borghans, 2012 for a review).

There are two main approaches to tackle the endogeneity of training participation. The first approach is to augment the wage equation with a Heckman-type selection correction term based on a training participation equation (Lynch, 1992 and Veum, 1995 are examples). The main problem with this approach is that it is hard to find a variable which affects training participation and has arguably no direct effect on wages. The same problem makes an instrumental variable approach unattractive. The second approach is to estimate fixed-effects regressions. This corrects the estimates for permanent unobserved individual effects (examples are Barron et al., 1993; Booth, 1993; Frazis and Loewenstein, 2005; Greenhalgh and Stewart, 1987; Parent, 1999). This method fails if selection into training is also based on unobservables that are time-variant, for example when the training is part of a package including the purchase of new machines.

The fixed-effect estimates of wage returns to training are typically smaller than standard OLS estimates, suggesting that fixed-effect estimates at least partially eliminate selection bias. Estimates using both approaches are nevertheless typically rather high. Some estimates even suggest that one week of training has the same return as a full year of formal education (see Bartel, 1995; Frazis and Loewenstein, 2005; Barron et al., 1993; Loewenstein and Spletzer, 1999 for the US; Blundell et al., 1996 for the UK; Fougére et al., 2001 for France; Kuckulenz and Zwick, 2003 for Germany; Schøne, 2004 for Norway). Such high returns are implausible and may be viewed as failed specification tests.

More recently, a third approach to deal with the endogeneity problem when estimating the return to training has been proposed by Leuven and Oosterbeek (2008). They use information from survey questions to construct a comparison group of workers who wanted to participate in training and didn't do so because of some random event. They show that this comparison group is more similar to the group that received training than the entire group of non-trainees in terms of observable characteristics. Also the characteristics of the training events attended by the trainees and the characteristics of the training events missed by the comparison group are quite similar. The main finding is that while a naive OLS estimate suggests a return to training of 9.5%, the estimate based on the newly created comparison group is close to 1% and not statistically significant. Two recent studies applied this approach using German data and find strikingly similar results as the original study (Görlitz, 2011; Fahr and Simons, 2008).

3. The experiment and the data

The voucher experiment analyzed here, was conducted by CINOP Centre of Expertise and was initiated and partially funded by the Ministry of Education of The Netherlands. CINOP recruited and worked together with four sectoral training funds that were willing to cooperate in this training program. These funds cover the following four sectors: (1) Animal husbandry and greenhouse horticulture; (2) potatoes, vegetables and fruit; (3) food industry; and (4) natural stone. The four funds

[8] This literature considers private sector training (or on-the-job training) which does not include formal education or training for the unemployed.

Table 1
Numbers of firms, employees and participants by fund.

Funds	Total		In experiment	
	Firms	Employees	Firms	Employees
Animal husbandry; horticulture	53,000	105,000	150	380
Potatoes, vegetables and fruit	5,700	33,000	89	210
Food industry	900	120,000	21	238
Natural stone	900	3500	50	438
Total			310	1266

Source: Doets and Huisman (2009).

Table 2
Numbers of observations by treatment and wave.

Wave(s)	Treatment	Control	Total
2006	639	627	1266
2006 + 2007	465	468	933
2006 + 2008	457	434	891
2006 + (2007 and/or 2008)	521	522	1043

Note: 2006 corresponds to the baseline, 2007 to the first follow-up and 2008 to the second follow-up. The numbers of observations in, for example, row "2006 + 2007" equal the number of observations (in treatment, control and total) that responded to the baseline survey and the first follow-up.

belong to sectors of the economy with relatively large shares of low-skilled male workers in The Netherlands.[9] Table 1 shows per fund the number of firms and employees covered, in total and in the experiment. The funds vary clearly in number of firms and average firm size. "Animal husbandry and greenhouse horticulture" cover a large number (over 50,000) of firms with, on average, only two employees. The food industry at the other extreme covers only 900 firms, with an average size of 130 employees.

The treatment consisted of giving each individual in the treatment group a voucher of €1000 for a training, €500 came from the government and the other €500 from the sector funds. The funds were in charge of administering the vouchers and they informed the individuals of their treatment status. If the workers did not use the entire amount a receipt of the remaining balance was issued on their name such that they can use it in the future on some other courses. Also if they changed jobs or changed their employment status they maintained their vouchers or their remaining balance. Voucher recipients could use their balance during the two years of the experiment. Workers could use the vouchers for a course of study or training session of their choice, including the learning materials pertaining to the course. The restriction is that the education or training has to contribute to the worker's labor market position. There were no restrictions regarding the provider of the education or training or its duration, other than the amount of the voucher and the redemption period of two years.[10]

The sample consists of 1266 individuals from various companies within the four sectors. Data were collected in three rounds: at baseline prior to treatment assignment (2006); in a first follow-up exactly one year after the baseline (2007); and in a second follow-up two years after the baseline (2008). To increase response rates, participants were paid to respond to the surveys: €50 for the baseline survey, €25 for the first follow-up and €50 for the second follow-up. There was no attempt to hide that the issuing of vouchers and the collection of data were part of an experiment to study the effects of vouchers. The reason is that since participants are working in the same narrowly defined sectors or in the same companies, they may get informed about the issuing (or not) of the vouchers. This openness implies that Hawthorne effects cannot be excluded. If there were such effects they would probably bias the estimates upwards. This would be the case if participants want to show the usefulness of the intervention. Our understanding of the literature is, however, that Hawthorne effects are more of a theoretical possibility that an actual threat (cf. Levitt and List, 2011).

Table 2 shows the numbers of observations by wave and combinations of waves, and by treatment status. The surveys contain personal information (such as age, gender and education), information regarding wages and working hours and information regarding training.

Assignment to treatment and control groups was done through lotteries which took place in six rounds between August 31 and December 1 of 2006. The lotteries were stratified by sectoral fund. This means that the random assignment is conditional on sectoral fund. Table 3 reports the means and standard deviations of the main variables by assigned treatment status. The table shows that the random assignment indeed produced groups with similar characteristics on average. The only variable that is significantly different between the two groups is a dummy variable that is equal to one if the respondent has savings of €1000 for an emergency situation. The difference is, however, modest in size. The last two columns of the table show results from regressions of training participation and wages in 2007 (two relevant outcome variables) on these pretreatment variables. This shows that age, training participation at baseline and firm size are relevant predictors of training participation in 2007. Gender, immigrant status, education level, age, risk tolerance, earnings at baseline and working hours are relevant predictors of earnings in 2007.[11]

The experiment was targeted at low-skilled workers. The Dutch government regards everyone with a formal education level below secondary vocational as lacking the skills to enter the labor market. For people with a secondary vocational degree it depends on the exact level of that degree whether someone is considered as low-skilled. To reach a large group of people with low education levels, the experiment was targeted to sectors in the economy where a vast majority of the workers perform low-skilled work. Indeed we find that in our dataset between 73% and 92% of the participants meet the criterion of the Dutch government (19% has secondary vocational, where we do not know the exact level). 6.6% of the participants in the experiment have higher education levels. We looked at the job descriptions of these people, only 16 out of 82 describe their job as "head", "manager", "director" or comparable. The others all report titles of low-skilled jobs.

Table 2 shows that there is substantial attrition in the two follow-up waves. From the original 1266 observations in 2006, it goes down to 933 in 2007, implying an attrition rate of 0.26. By 2008, 891 individuals filled out the questionnaire (an attrition rate of 0.30). To assess whether sample attrition is systematically related to assigned treatment we regressed binary indicators for attrition in 2007, 2008, and 2007 and 2008 on the assigned treatment status. We did this without and with controlling for the predetermined variables included in the regressions in the last two columns of Table 3. Results are reported in Table 4. This shows that attrition is not systematically related with treatment status. The full regression results are reported in Table A3 in Appendix A. Out of the 60 coefficients reported in this table, only four are significant at the 10%-level and one at the 5%-level. There thus seems to be no systematic relationship between attrition and participants' characteristics. We may therefore be confident that sample attrition will not bias the results presented in Section 5.

[9] The sample is not representative of the low-skilled workers in The Netherlands.

[10] Officially the vouchers in this program were known as Individual Learning Accounts (ILAs). Since the accounts did not accrue interest and since individuals could not put additional money in their account, the program was essentially a voucher program with the administration executed by the institution administering the accounts. Individuals were informed that they had been given an ILA and then after paying for the course they had to send the invoice to get the money back in the same way as vouchers. For this reason we refer to the program as a voucher program.

[11] In Section 4 we discuss the partial non-response on the earning question in 2007 (and 2008). More elaborate specifications of the training participation and log wage equation including dummies for the various education categories confirm the basic message from these regressions: treatment status is balanced on variables that are relevant predictors of two key outcome variables.

Table 3
Descriptive statistics by assigned treatment status, and association between background variables and training participation and wages in 2007.

Variable	Without voucher		With voucher		p-Value	Training participation		Log wages	
	Mean	SD	Mean	SD		Coeff	s.e.	Coeff	s.e.
Male (dummy)	0.709	(0.454)	0.735	(0.441)	[0.306]	0.002	(0.047)	0.138***	(0.045)
Married (dummy)	0.711	(0.452)	0.669	(0.470)	[0.105]	0.020	(0.043)	0.025	(0.026)
Children (dummy)	0.491	(0.500)	0.471	(0.497)	[0.469]	0.014	(0.037)	0.000	(0.021)
Immigrants (dummy)	0.069	(0.253)	0.067	(0.251)	[0.928]	0.002	(0.088)	−0.071	(0.044)
Age (in years)	38.212	(11.068)	37.618	(11.194)	[0.343]	0.007	(0.011)	0.007	(0.009)
Age squared/100						−0.016	(0.013)	−0.005	(0.011)
Education									
– Uncompleted education	0.048	(0.214)	0.051	(0.218)	[0.834]	0.009	(0.082)	−0.066	(0.065)
– Primary education	0.067	(0.250)	0.058	(0.234)	[0.523]	−0.033	(0.057)	−0.044	(0.038)
– Lower secondary	0.419	(0.493)	0.436	(0.494)	[0.547]				
– Intermediate secondary	0.182	(0.386)	0.204	(0.401)	[0.337]	−0.063	(0.043)	0.015	(0.024)
– Secondary vocational	0.198	(0.398)	0.188	(0.389)	[0.641]	−0.033	(0.037)	0.051**	(0.020)
– Upper secondary	0.054	(0.227)	0.041	(0.198)	[0.268]	0.023	(0.066)	0.034	(0.046)
– Higher education	0.021	(0.143)	0.017	(0.130)	[0.658]	−0.301***	(0.109)	0.202*	(0.107)
– Other/unknown	0.013	(0.112)	0.014	(0.118)	[0.838]	−0.047	(0.130)	−0.194*	(0.100)
Risk tolerance (1–10)	6.344	(1.998)	6.155	(2.164)	[0.106]	0.010	(0.007)	0.007*	(0.004)
Savings of €1000 (dummy)	0.435	(0.496)	0.380	(0.486)	[0.046]	0.017	(0.034)	−0.008	(0.020)
Training 2006 (dummy)	0.411	(0.492)	0.369	(0.481)	[0.132]	0.216***	(0.043)	0.010	(0.017)
Log monthly earnings	7.410	(0.527)	7.383	(0.629)	[0.404]	0.007	(0.042)	0.663***	(0.065)
Working hours per week	34.496	(8.757)	33.952	(8.893)	[0.273]	−0.001	(0.003)	0.009**	(0.004)
Firm size (employees)/100	1.952	(3.526)	1.652	(3.269)	[0.117]	0.005	(0.004)	0.001	(0.003)
N	639		627			933		409	

Note: Columns "With voucher" and "Without voucher" report means and standard deviations for background variables separately for treatment and control observations. Risk tolerance is based on respondents' answer to the question "How do you see yourself: Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?" The answer is on a scale from 1 ("unwilling to take risks") to 10 ("fully prepared to take risk"). Training 2006 equals one if respondent participated in any work or career related training activity during the 12 months prior to the interview. The column "p-Value" reports the p-value from a test of the difference of the means of the treated and controls. The columns "Training participation" and "Log wages" report the coefficients of regressions of training participation and wages in 2007 on background characteristics. These regressions also include dummies for sector funds and missing values. Robust standard errors clustered at firm level are in parentheses. ***, **, and * indicate significances at 1%, 5% and 10% levels.

4. Empirical approach

To examine the impact of the voucher program on outcomes we estimate OLS regressions of the following form:

$$y_i = \alpha_y + \delta_y D_i + X_i\beta_y + \varepsilon_{y,i} \tag{1}$$

where Y_i indicates the outcome variable of interest for observation *i*. We will consider various outcomes: training participation, indicators of the type of training as well as labor market outcomes such as wages and job/sector mobility, and future plans regarding training. D_i is a dummy that takes the value of 1 if individual *i* was assigned to the treatment group and 0 otherwise. Since assignment to treatment is only random conditional on the sector fund in which someone is working, we will in all analyses include dummies for sector funds as randomization controls. X_i is a vector of characteristics of the worker, firm size and dummies for missing values for some variables. To account for the fact that workers in the same firm may experience common shocks, we cluster standard errors at the firm level. The coefficient δ_y can be interpreted as the causal impact of the program on outcome *y* because the treatment was randomly assigned among the participants.[12]

If exposure to the program affects training participation, then assignment to treatment can potentially be used as an instrumental variable for training participation in a model to estimate the causal effect of training on earnings. In that model the regression of training participation on the treatment dummy is the first stage equation, and the regression of earnings on the treatment dummy is the reduced form equation. The ratio of the reduced form coefficient and the first stage coefficient is then the causal effect of training on earnings for people whose training status is determined by their treatment status (compliers). In Section 5.4, we will present results from this approach. Provided that the first stage effect is sufficiently strong, this will give a LATE estimate of the effect of training participation on earnings, if the program only affects earnings through its effect on training participation. This excludes for example that the program influences earnings through a change of the type of training.

5. Results

5.1. Training participation

Table 5 reports estimates of the impact of the voucher program on training participation. The first column shows that voucher receipt increases training participation in the first year by 6.2 percentage points if we ignore control variables. Including control variables, this estimate increases to 8.6 percentage points. The results in the last two columns show the cumulative impact after two years, the period within which recipients could spend their voucher. The result in the final column (which includes control variables) shows that training participation during the two year period is 19.6 percentage points higher in the treatment group than in the control group. This effect is significantly different from zero at the 1%-level. The increase should be compared to a two-year participation rate of 0.45 among the controls; this implies a 44% increase.

A key concern with this type of interventions is whether the public investment is replacing private investment. To calculate the deadweight loss we have to take the voucher utilization rate into account. While the two-year participation rate in the treatment group is 0.62, the voucher utilization rate is 0.41.[13] This means that a share of 0.21 of the voucher recipients participated in training without using the voucher. In the control group the two-year participation rate equals 0.45, all of it privately paid. The difference between 0.45 and 0.21 (0.24) is then the share of voucher recipients who would have participated in privately-paid training in the absence of the voucher and stopped doing that with the voucher. This is the privately-paid training that is crowded out through the vouchers. The 0.41 voucher utilization rate thus comes at a cost of 0.24 crowding out. This implies a deadweight loss of 59% ($\frac{0.24}{0.41} \times 100\%$).

[12] We add subscript *y* to the intercept, coefficients and error term to signify that these are different across different outcomes.

[13] Both training participation and voucher utilization are self-reported.

Table 4
Impact of treatment status on attrition from the sample, by wave(s).

Wave(s)	Coeff	s.e.	Controls	*N*
2007	−0.019	(0.027)	Funds	1266
	0.003	(0.011)	All	1266
2008	0.022	(0.025)	Funds	1266
	0.031	(0.022)	All	1266
2007 and/or 2008	−0.018	(0.021)	Funds	1266
	−0.005	(0.015)	All	1266

Note: Each coefficient comes from a separate linear probability model in which an attrition indicator is regressed on assigned treatment status, dummies for sector funds (and covariates). Covariates are the variables included in the last two columns of Table 3, plus dummies for missing values. Robust standard errors clustered at firm level are in parentheses. ***, **, and * indicate significances at 1%, 5% and 10% levels.

These figures are summarized in Table 6 which also presents results for 2007. Our estimate of the deadweight loss is remarkably similar to that found by Messer and Wolter (2009) in the Swiss experiment where in spite of a much lower take up rate of the vouchers of 0.18, they find a deadweight loss of 60%.

In Section 1 we mentioned that a substantial share of the voucher recipients answered in the second follow-up survey that they did not receive a voucher. We checked whether this group includes a large share of the 21% of voucher recipients who participated in training without using the voucher. It turns out that this is not the case. The training participation rate in this group is 0.35. To assess the impact of "perceived voucher eligibility" on training participation we conducted instrumental variable analyses where the potentially endogenous variable perceived voucher eligibility is instrumented by the assigned treatment status. Results are reported in Table A2 in the Appendix A. This shows that the impact of perceived voucher eligibility on training participation (among the compliers) is larger than the impact of voucher eligibility on training participation. Compliers in this analysis are participants who report that they have a voucher when assigned to the treatment group and who report that they don't have a voucher when assigned to the control group. If reporting the actual status only reflects information (and not paying attention or justifying not using the voucher), these estimates can be seen as the effects in case of a more successful information campaign.

5.2. Characterizing compliers

Table 7 reports the numbers of observations by treatment status and training participation, where the sample is restricted to observations that responded to the second follow-up survey. This leaves us with 891 observations, of whom 434 did not receive a voucher and 457 received a voucher, and of whom 412 did not participate in training and 479 participated in training. Following the terminology from the impact evaluation literature (e.g. Imbens and Angrist, 1994), we can distinguish three types of observations: never-takers, always-takers and compliers. Never-takers are observations who would never participate in training independent of their treatment status. Always-takers are observations who would always participate in training independent of their treatment status. Compliers are observations whose training participation depends on their treatment status: with a voucher they participate, without a voucher they don't. It is assumed that the opposite case (people who participate without a voucher and don't participate with a voucher) does not exist.

Table 5
Effect of the voucher program on training participation.

	2007		2007–2008	
	(1)	(2)	(3)	(4)
Voucher recipient	0.062	0.086***	0.165***	0.196***
	(0.039)	(0.037)	(0.043)	(0.038)
F-value	2.53	5.40	14.72	26.60
Controls	Funds	All	Funds	All
N	933	933	891	891

Note: Each coefficient comes from a separate linear probability model in which training participation is regressed on assigned treatment status, dummies for sector funds (and covariates). Covariates are the variables included in the last two columns of Table 3, plus dummies for missing values. Robust standard errors clustered at firm level are in parentheses. ***, **, and * indicate significances at 1%, 5% and 10% levels.

Table 6
Crowding out and deadweight-loss of vouchers.

		2007	2007–2008
(1)	Training participation controls	37%	45%
(2)	Training participation treated	42%	62%
(3)	Voucher utilization among treated	20%	41%
(4)	Crowding-out [(1)-((2)-(3))]	15%	24%
(5)	Deadweight loss $[\frac{(4)}{(3)} \times 100]$	75%	59%

The 175 observations that received a voucher but did not participate in training can be categorized as never-takers. Likewise, the 197 observations that did not receive a voucher but participated in training can be categorized as always-takers. The 237 observations who didn't receive a voucher and who didn't participate in training consist of never-takers and compliers. While it is impossible to distinguish these two types at an individual level, we know that because the vouchers were randomly assigned, the number of never-takers in this group is equal to 175 (the number of never-takers in the top-right cell) times the ratio of non-voucher recipients to voucher recipients (434/457), which is 166. This implies that 71 observations in the top-left cell are compliers. Likewise we can calculate that the number of always-takers in the bottom-right cell equals: 207 $(=\frac{457}{434} \times 197)$. This leaves 75 compliers in the bottom-right cell. The total number of compliers is thus 146, and their share in the sample is 0.163; this coincides with the estimate in the third column of Table 5. The shares of never-takers and always-takers are 0.383 and 0.453, respectively.

Abadie (2003) has proposed a method to infer how the characteristics of compliers differ from characteristics of always-takers and never-takers (see also Kling, 2001). This method comes down to restricting the sample to successive subsamples based on binary indicators (for example the subsample of women) and regress training participation on treatment status within that subsample. The estimate gives the share of compliers in that subsample, which can be compared to the share of compliers in the entire sample (0.163). If the estimate in the subsample exceeds the estimate in the entire sample, we can conclude that the group of compliers contains more observations with that characteristic than the full sample.

To see how the compliers differ from always-takers and never-takers, we apply an alternative approach. [14] This approach allows us to: i) contrast compliers separately to always-takers and to never-takers; ii) to look at differences between compliers and others in a multivariate framework; and iii) to include continuous variables.

We first restrict the sample to individuals that took a course during the two years of program. This restricted sample consists of all the always-takers in the full sample and of the compliers who were assigned to treatment. Since all the always-takers are included in the restricted sample and since assignment to treatment is random, the always-takers should be randomly assigned to treatment and control. There should therefore not be any systematic relation between the individual characteristics of always-takers and their treatment status. Hence, if we regress treatment status on characteristics in the restricted sample, then any characteristic that is significantly related to treatment status is associated with the compliers who received a voucher. The first column of Table 8 presents the results from a linear probability model. This shows that compared to always-takers compliers are on average more likely to be male, less likely to be married, have lower risk tolerance, are less likely to have participated in training in the baseline year and work fewer hours per week, than always-takers.

[14] We thank Edwin Leuven for suggesting this.

Table 7
Number of observations by voucher receipt and training participation.

		Voucher		Sum
		No	Yes	
Training	No	237 [never-takers + compliers]	175 [never-takers]	412
	Yes	197 [always-takers]	282 [always-takers + compliers]	479
Sum		434	457	891

The above results tell us how the people who are triggered by the vouchers to participate in training compare to the people who would also have participated in training in the absence of the voucher program. It is also possible to compare the people who respond to the vouchers to the people who even when given vouchers do not participate in training. To that end we next restrict the sample to the 412 people who did not participate in training. This restricted sample consists of all the never-takers in the full sample and of the compliers who were assigned to control (cf. Table 7). Since all the never-takers are included in the restricted sample and since assignment to treatment is random, the never-takers should be randomly assigned to treatment and control. There should therefore not be any systematic relation between the individual characteristics of never-takers and their treatment status. Hence, if we regress treatment status on characteristics in the restricted sample, then any characteristic that is positively (negatively) related to treatment status is negatively (positively) associated with the compliers who did not receive a voucher. The second column of Table 8 presents the results from a linear probability model. This shows that compared to never-takers compliers are on average less likely to be male, work more hours per week, have more savings, and are less likely to have attended intermediate secondary education instead of lower secondary education.[15] Hence, of the people who would otherwise not participate in training, the voucher program triggers women, people who work long hours and people who did not attend intermediate secondary education, to participate.[16]

Table 8
Characterizing compliers.

Variable	Training participants		Non-participants	
	(1)		(2)	
	Coeff	s.e.	Coeff	s.e.
Male	0.145*	(0.077)	0.223***	(0.070)
Married	−0.127**	(0.062)	0.013	(0.061)
Children	0.027	(0.059)	−0.073	(0.058)
Immigrant	−0.098	(0.080)	−0.082	(0.115)
Age	0.017	(0.017)	−0.006	(0.019)
Age squared	−0.020	(0.021)	0.007	(0.022)
Education (relative to lower secondary)				
– Uncompleted	−0.084	(0.129)	−0.048	(0.136)
– Primary	0.041	(0.106)	−0.066	(0.111)
– Intermediate secondary	−0.074	(0.063)	0.180***	(0.062)
– Secondary vocational	0.091	(0.056)	−0.065	(0.056)
– Upper secondary	−0.089	(0.097)	−0.003	(0.113)
– Higher	0.052	(0.155)	−0.428***	(0.084)
– Other/unknown	0.001	(0.176)	0.253	(0.170)
Risk tolerance	−0.022*	(0.012)	−0.010	(0.011)
Savings of €1000	−0.026	(0.045)	−0.093**	(0.044)
Previous training	−0.196***	(0.042)	−0.049	(0.066)
Log monthly earnings	0.057	(0.070)	0.086	(0.087)
Working hours per week	−0.009**	(0.004)	−0.013**	(0.005)
Firm size (employees)/100	−0.005	(0.006)	−0.010	(0.010)
N	479		412	

Note: Results are from a linear probability model where treatment status is regressed on characteristics conditional on training participation being equal to 1 (column (1)) or 0 (column (2)). Regressions also include dummies for sector funds and for missing values. Robust standard errors clustered at the firm level are in parentheses. ***, **, and * indicate significant differences at 1%, 5% and 10% levels.

5.3. Type of training

In this subsection we compare the type of training obtained by workers in the treatment group to the type of training obtained by workers in the control group. We, thus, condition on having received training and examine effects on the intensive margin. A limitation here is that conditional on training receipt, workers in treatment and control groups are no longer comparable. Those in the control group that took training consist of always-takers, while those in the treatment group that took training consist of always-takers and of compliers. The type of training obtained by treated and controls can therefore be different i) because always-takers change the type of training they attend in response to the treatment, and ii) because compliers attend different types of training than always-takers. Since we cannot identify at an individual level compliers and always-takers in the bottom-right cell of Table 7, we cannot directly disentangle these two factors. Our approach to this is to present estimates of treatment on training characteristics from specifications without control variables and with control variables. Estimates from the specification without control variables are more likely to capture the two factors together. The results with control variables are corrected for observed differences between treated and controls, and since the control group consists only of always-takers, these results are therefore informative about the effect of vouchers on the training characteristics of always-takers.[17] We acknowledge that this approach depends on a selection-on-observables assumption and is therefore more tentative in nature than the other results presented in this paper. Yet, we believe it is an interesting descriptive analysis.

Table 9 shows the effect of the voucher-receipt on various training characteristics, motivations to train and purposes of taking a course. The results are shown separately for the two follow-up surveys (2007 and 2008). This is because the surveys are not identical, some questions appear only in one and not in the other. The results for 2008 are the more interesting results since they include the entire treatment period.

We find that the individuals in the treatment group are substantially more likely to take the initiative themselves to enroll in a course, this is the case in 2007 but even more so in 2008. These results are at least as strong when control variables are included than when these are omitted. This suggests that always-takers are taking the initiative themselves more often when they are awarded a voucher. The estimates also show that the workers in the treatment group are significantly less likely to take a course that combines well with work. The effect is a bit larger when controls are included, suggesting that this effect can be attributed to always-takers. The same is true for the finding that trainees that have a voucher are significantly less likely to take the course during working hours than trainees without a voucher. There is no difference whether the courses handed out a diploma at the end or not, neither in the cost of the course (the cost is only asked in the first follow-up survey in 2007). The number of hours dedicated to the course is more or less the same for both groups (in 2007 less for the treated and in 2008 more).

The second panel shows results from the purpose of taking a course. The treated group is significantly less likely to take a course to improve their current job tasks. They are more likely to take a course to improve their conditions in the labor market and to change sectors. These results hold when control variables are included suggesting that they also apply to always-takers.

[15] There is also a significant coefficient for higher education, but this concerns just 7 of the 412 observations in this regression.

[16] We have also used Abadie's approach to characterize compliers. According to these results compliers differ from non-compliers in terms of intermediate secondary education (lower among compliers), secondary vocational education (higher among compliers), higher education (higher among compliers), previous training (lower among compliers). These findings are consistent with the pattern of results in Table 8.

[17] We also applied propensity score matching to estimate the impact of vouchers on training characteristics of always-takers. This gives results that are very similar to those from the OLS-regressions with controls.

Table 9
Impact of voucher receipt on types of training courses.

Dependent variables	Survey	Controls for funds		All controls		p-Value	N
Worker took initiative	2007	0.192***	(0.054)	0.205***	(0.057)	0.479	367
Worker took initiative	2008	0.211***	(0.041)	0.231***	(0.044)	0.242	479
Cost of the course	2007	0.159	(0.175)	0.181	(0.192)	0.818	154
Diploma	2008	0.004	(0.048)	−0.014	(0.053)	0.243	409
Training hours	2007	−1.237	(0.973)	−1.334	(1.019)	0.799	333
Training hours	2008	1.474*	(0.885)	1.032	(0.908)	0.177	388
Training combined with work	2007	0.003	(0.043)	0.006	(0.040)	0.844	358
Training combined with work	2008	−0.072**	(0.034)	−0.084**	(0.038)	0.379	420
Course during working hours	2008	−0.114**	(0.045)	−0.123***	(0.043)	0.555	479
Training purpose							
Improve current job conditions	2007	−0.049	(0.056)	−0.057	(0.059)	0.679	367
Improve current job tasks	2008	−0.259**	(0.108)	−0.190*	(0.114)	0.117	405
Improve current job (promotion)	2008	−0.050	(0.120)	−0.046	(0.122)	0.930	449
Improve condition labor market	2007	0.029	(0.045)	0.067	(0.052)	0.050**	367
Improve condition labor market	2008	0.238***	(0.089)	0.261***	(0.100)	0.582	459
To change jobs	2007	−0.009	(0.033)	−0.002	(0.035)	0.626	367
To change jobs	2008	0.141	(0.136)	0.153	(0.132)	0.824	381
To change sectors	2007	0.001	(0.032)	0.011	(0.034)	0.487	367
To change sectors	2008	0.303*	(0.156)	0.257*	(0.146)	0.366	382
Attitude towards training							
Positive attitude of your employer	2007	−0.001	(0.088)	0.008	(0.108)	0.822	339
Positive attitude of your employer	2008	−0.210**	(0.086)	−0.206**	(0.090)	0.880	469
Positive attitude of your family	2007	0.051	(0.069)	0.085	(0.073)	0.366	343
Positive attitude of your family	2008	0.068	(0.061)	0.127**	(0.061)	0.047**	470
Positive attitude of your partner	2007	0.187**	(0.091)	0.185*	(0.094)	0.955	335
Positive attitude of your partner	2008	0.072	(0.066)	0.174**	(0.068)	0.001***	451

Note: Each coefficient comes from a separate regression in which the dependent variable is regressed on assigned treatment status, sector dummies (and covariates) conditional on training participation being equal to one. Covariates are the variables included in the last two columns in Table 3, plus dummies for missing values. Robust standard errors clustered at the firm level are in parentheses. ***, **, and * indicate significant differences at 1%, 5% and 10% levels. The column "p-Value" reports the p-value from a test of the difference of the coefficients from the specifications with and without control variables.

Finally the third panel shows the attitudes of employers, family and partners towards the workers taking a course. Employers are significantly more negative about their workers enrolling in a course in 2008 when the training participant received a voucher. The opposite is true for family and partners. Notice that the differences in attitudes of family is only significant when control variables are included, suggesting that these effects are attributable to the attitudes of the families of always-takers becoming more positive.[18]

The findings in Table 9 suggest that the courses that the treated individuals are taking are not specific to their current jobs, and are therefore more general. To clarify, a general type of training refers to skills or knowledge that can be applicable in many other sectors or companies. This contrasts to specific training which is considered to be specific to one job or one company, it includes skills which cannot be used when the individual changes jobs (Brunello et al., 2007). This supports the theory that suggests that since firms will not capture the benefits from general training they will not invest in it. This makes general training a better investment for workers since they will capture the returns from it and avoid being held up (cf. Section 2).

5.4. Earnings

In this subsection we estimate the effect of the voucher program on workers' earnings. A potential concern here is that the response rate of earnings is not so high. In 2007 out of 933 individuals that completed the questionnaire, only 44% provides information on their earnings. In 2008 this percentage goes up to 77% out of 891 observations. Non-response on the earning question can bias our results especially if it is correlated with the treatment variable. We regressed dummies for non-response on the treatment indicator and other observables. Table 10 shows the results. For 2008 the only significant coefficients are for the dummy for primary education, baseline earnings and firm size (all at the 10%-level). In 2007 only the coefficient for higher education is significant (also at the 10%-level). The lack of a systematic relation between reporting earnings and the treatment indicator suggests that the impact estimates are not biased due to partial non-response.

[18] We also checked for differences in characteristics of the training such as difficulty, usefulness, how interesting they were and so on. We do not show the results but none of these variables are different between the treated and controls.

Table 10
Probability of observing earnings.

Independent variables	2007		2008	
	Coeff	s.e.	Coeff	s.e.
Voucher	−0.027	(0.029)	−0.005	(0.029)
Male	−0.052	(0.041)	−0.032	(0.042)
Married	0.025	(0.030)	0.009	(0.038)
Children	0.002	(0.031)	0.002	(0.034)
Immigrant	0.009	(0.054)	−0.013	(0.067)
Age	0.005	(0.009)	−0.002	(0.012)
Age squared	−0.007	(0.011)	−0.001	(0.014)
Education (relative to lower secondary)				
– Uncompleted	0.050	(0.066)	0.055	(0.079)
– Primary	−0.007	(0.064)	−0.125*	(0.073)
– Intermediate secondary	−0.023	(0.039)	0.045	(0.035)
– Secondary vocational	0.005	(0.042)	0.054	(0.037)
– Upper secondary	−0.017	(0.062)	0.037	(0.061)
– Higher	0.108*	(0.060)	0.077	(0.099)
– Other/unknown	−0.015	(0.111)	0.081	(0.107)
Risk tolerance	−0.000	(0.006)	0.008	(0.008)
Savings of €1000	0.037	(0.026)	0.024	(0.031)
Previous training	0.006	(0.026)	0.005	(0.034)
Log monthly earnings	0.004	(0.038)	0.080*	(0.047)
Working hours per week	0.002	(0.003)	−0.004	(0.003)
Firm size (employees)/100	0.000	(0.003)	0.005*	(0.003)
N	933		891	

Note: Dependent variable equals 1 if earnings are observed for the year indicated in the top row, and 0 if the participant responded in that year but did not report earnings. Regressions also include dummies for sector funds and for missing values. Robust standard errors clustered at the firm level are in parentheses. ***, **, and * indicate significant differences at 1%, 5% and 10% levels.

Table 11
Estimates of the impact of vouchers and training on (log) monthly earnings.

	2007		2008	
Reduced form				
Voucher	−0.030	−0.019	−0.020	−0.004
	(0.054)	(0.021)	(0.040)	(0.025)
OLS				
Actual training	0.030	0.017	0.109***	0.037
	(0.035)	(0.015)	(0.037)	(0.025)
IV				
Predicted training	n.a.	n.a.	−0.116	−0.022
			(0.242)	(0.124)
Controls	Funds	All	Funds	All
Mean dep. var. in control group	7.620	7.620	7.558	7.558
SD dep. var. in control group	(0.355)	(0.355)	(0.546)	(0.546)
N	409	409	687	687

Note: Each coefficient comes from a separate regression in which log monthly earnings are the dependent variable. Reduced form estimates are based on regressions of log monthly earnings on assigned treatment status, sector dummies (and covariates). OLS-estimates are based on regressions of log monthly earnings on actual training participation, sector dummies (and covariates). IV-estimates are based on regressions of log monthly earnings on training participation, sector dummies (and covariates), where training participation is instrumented by assigned treatment status. Covariates are the variables included in the last two columns in Table 3, plus dummies for missing values. IV-estimates for 2007 are not reported because the first stage results point to a weak instrument problem. Robust standard errors clustered at the firm level are in parentheses. ***, **, and * indicate significant differences at 1%, 5% and 10% levels.

We start with estimating Eq. (1) with the logarithm of monthly earnings as the dependent variable. The coefficient $\delta_{ln(earnings)}$ gives the average intention-to-treat effect on those individuals that were assigned a voucher. The results are shown in the first row (Reduced form) of Table 11. In both 2007 and 2008 the estimates are not significantly different from zero. Especially for 2008, the point estimates are very close to zero. The estimates are, however, not very precise, so that we cannot reject small positive impacts.

Table 11 also reports OLS estimates and IV estimates of the impact of training on (log) monthly earnings. For the IV estimates, treatment status is used as instrumental variable for actual training participation. For 2008, the OLS estimate from the specification without controls is sizable at 10.9%, but reduces to an insignificant 3.7% when control variables are included. The IV estimates are negative. The standard errors are, however, very large, so that these estimates are not very informative.

5.5. Job mobility

In this subsection we assess whether the program has an impact on job mobility. We make a distinction between job changes to another company in the same sector and job changes to another company in another sector. Information about both types of job mobility is available in both follow-up surveys which means that we have four dependent variables. Table 12 shows the results for specifications without and with control variables. The results indicate that receipt of a voucher does not have a significant impact on the probability that individuals change jobs (either to another company or another sector) after one or two years of the program. All point estimates are close to zero.

5.6. Training plans

Finally, we look at the impact of voucher-receipt on workers' motivation to continue updating their skills after the program has ended. In the 2008 survey, individuals were asked whether they had concrete plans to follow a course, whether they will do so in the next six months and if the decision of taking a course was influenced by their employers. Table 13 shows the results of the impact of the program on these variables. The results show that individuals who received a voucher are more likely to have the plan to enroll in a course in the next six months, and also their employers are less likely to influence their future training decision. This is consistent with our findings in Section 5.3 regarding employers' support of training activities.

Table 12
Impact of vouchers on job mobility.

	2007		2008	
Other company				
Voucher	0.014	0.018	0.014	0.011
	(0.017)	(0.017)	(0.019)	(0.019)
Mean dep. var. in control group	0.089	0.089	0.105	0.105
N	919	919	878	878
Other sector				
Voucher	−0.003	−0.004	0.019	0.019
	(0.015)	(0.015)	(0.017)	(0.017)
Mean dep. var. in control group	0.061	0.061	0.061	0.061
N	883	883	840	840
Controls	Funds	All	Funds	All

Note: Each coefficient comes from a linear probability model in which job mobility is regressed on assigned treatment status, sector dummies (and covariates). Covariates are the variables included in the last two columns in Table 3, plus dummies for missing values. Robust standard errors clustered at the firm level are in parentheses. ***, **, and * indicate significant differences at 1%, 5% and 10% levels.

6. Conclusions

This paper analyzes the impact of training vouchers of €1000 on the training participation and related outcomes of low-skilled workers in The Netherlands. To this end we exploit data from a randomized experiment that was conducted in four sectors with a majority of low-skilled workers. Relative to a base two-year training participation rate of 45%, receiving a voucher increases training participation by almost 20 percentage points. Together with information about the number of vouchers redeemed, this implies a deadweight loss close to 60%. This means that more than half of the vouchers that were used would otherwise have been financed by private parties.

The deadweight loss of 60% is remarkably close to the deadweight loss reported for the very similar voucher experiment in Switzerland. An important difference between the Swiss experiment and ours is that the redemption period in Switzerland was only six months whereas in The Netherlands it was two years. It thus seems that the high deadweight loss in Switzerland cannot be attributed to the short redemption period.

We analyzed the characteristics of the workers whose training participation is triggered by the receipt of a voucher. It turns out that these compliers differ from the people that would also have participated in training without a voucher (always-takers) by being on average more likely to be male, being less likely to be married, having lower risk tolerance, being less likely to have participated in training in the baseline year and working fewer hours per week. We also compared compliers to never-takers and find that compliers are on average less likely to be

Table 13
Impact of vouchers on future plans to follow a course — 2008.

	Plans		Plans next 6 months		Influence of employer	
Voucher	0.064	0.132*	0.129	0.200**	−0.210**	−0.211**
	(0.073)	(0.073)	(0.088)	(0.092)	(0.084)	(0.086)
Controls	Funds	All	Funds	All	Funds	All
Mean dep. var. in control group	3.44	3.44	2.69	2.69	3.07	3.07
SD dep. var. in control group	(1.20)	(1.20)	(1.31)	(1.31)	(1.27)	(1.27)
N	875	875	876	876	870	870

Note: Each coefficient comes from an OLS regression in which an indicator of future training plans is regressed on assigned treatment status, sector dummies (and covariates). Covariates are the variables included in the last two columns in Table 3, plus dummies for missing values. All indicators are measured on a 5-point scale from low to high. Robust standard errors clustered at the firm level are in parentheses. ***, **, and * indicate significant differences at 1%, 5% and 10% levels.

male, more likely to have savings, work more hours per week, and are less likely to have attended intermediate secondary education instead of secondary vocational education.

Examining the types of training attended by voucher-recipients and non-recipients we find that recipients more often participate in training that is general instead of specific. This difference is not only due to the types of training chosen by compliers but also to the different types of training chosen by always-takers in response to getting a voucher. This result is consistent with human capital theory which predicts that workers can reap the benefits from general training while they have to share the benefits from specific training with their employers. It is therefore natural that workers will invest their vouchers mainly in general training.

We also analyzed the impact of vouchers on workers' wages, their job mobility and their future training plans. Consistent with other recent studies, we find no significant (short-run) impact from vouchers (or training participation) on wages. This may be due to the short time spell elapsed between training participation and the moment wages are measured. It may also be due to the relatively small size of the investment. While €1000 is not an ignorable amount, it is too small to realistically translate into a wage increase of more than some tenths of a percent (recall that the return to one entire year of full-time education is often not above 7 or 8%). With the numbers of workers included in this experiment, we lack the power to pick up the sorts of effects of training on wages that one might expect based on simply scaling down the usual effects of a year of formal schooling. However, and this is important, we can rule out some of the large estimates in the published literature. We also find no impact on job mobility, neither within or across sectors, but we do find a significant and substantial impact on future training plans.

This collection of results paints a nuanced picture of the effects of the program. Increased participation, a shift towards general training and more future training plans can be seen as the bright colors. The substantial deadweight loss and the absence of visible labor market effects are the darker shades.

Appendix A

A.1. Actual treatment status and perceived treatment status

The second follow-up survey asked respondents about their awareness of their treatment status. 21% of the treated answered that they were not entitled to the voucher, while 6% of the controls answered that they received a voucher. In a report describing the voucher experiment, Doets and Huisman (2009) reassign the 21% of misinformed treated to the control group, and the 6% of misinformed controls to the treatment group. While this seems an intuitive thing to do, it is likely to invalidate the interpretation of estimates based on the reassignment as causal effects.

There are various reasons why people may report a different treatment status than their actual treatment status. The first is that they were never informed about it. The second is that they were informed but didn't pay attention or forgot. The third is that they were informed, paid attention and did not forget, but claim unawareness to justify that they haven't used the voucher. The second and third reasons clearly cause that the newly created treatment and control groups are no longer comparable. But also if unawareness is caused by the first reason, the reassignment may cause trouble unless the reason for not being informed is due to randomness.

The first and second columns in Table A1 report estimates from a regression of an indicator of being misinformed of the actual treatment status on pretreatment variables for people who were given a voucher. This shows that being misinformed is not random. Gender, immigrant status, training participation at baseline, risk tolerance and firm size are all significant predictors of being misinformed. We ran a similar regression for being misinformed among people who were originally assigned to the control group. Results are reported in the third and fourth columns of Table A1. Intermediate secondary education, unknown education and firm size are significant predictors of being misinformed in the control group. These results invalidate the reassignment of misinformed people.

Table A1
Determinants of being misinformed about treatment status by actual treatment status.

	Treated				Controls			
	Coeff	s.e.	Coeff	s.e.	Coeff	s.e.	Coeff	s.e.
	(1)		(2)		(3)		(4)	
Male	−0.172***	(0.041)	−0.233***	(0.068)	−0.002	(0.024)	0.002	(0.036)
Married	−0.031	(0.036)	−0.015	(0.055)	0.027	(0.028)	0.036	(0.040)
Children	0.014	(0.036)	0.039	(0.049)	−0.012	(0.032)	−0.029	(0.044)
Immigrant	−0.060	(0.063)	−0.195*	(0.112)	0.046	(0.044)	0.091	(0.068)
Age	0.009	(0.013)	0.011	(0.016)	0.004	(0.006)	0.005	(0.010)
Age squared/100	−0.018	(0.016)	−0.018	(0.020)	−0.005	(0.008)	−0.007	(0.012)
Education (relative to lower secondary)								
–Uncompleted	0.033	(0.075)	−0.074	(0.126)	0.003	(0.048)	−0.016	(0.073)
– Primary	−0.057	(0.073)	−0.084	(0.099)	0.008	(0.039)	0.027	(0.062)
– Intermediate secondary	0.030	(0.044)	0.036	(0.053)	−0.041**	(0.019)	−0.065**	(0.029)
– Secondary vocational	0.028	(0.049)	0.061	(0.056)	0.007	(0.030)	0.005	(0.042)
– Upper secondary	−0.077	(0.087)	−0.073	(0.103)	−0.013	(0.045)	−0.015	(0.063)
– Higher	0.097	(0.090)	0.131	(0.125)	−0.001	(0.072)	−0.013	(0.093)
– Other/unknown	−0.009	(0.137)	0.006	(0.171)	−0.057**	(0.024)	−0.077*	(0.040)
Risk tolerance	0.012*	(0.007)	0.009	(0.009)	−0.004	(0.006)	−0.003	(0.008)
Savings of 1000	−0.002	(0.033)	0.026	(0.044)	0.009	(0.019)	0.007	(0.027)
Training 2006	0.110***	(0.039)	0.127**	(0.055)	0.022	(0.021)	0.021	(0.033)
Log monthly earnings	−0.041	(0.068)	−0.036	(0.085)	0.019	(0.033)	0.025	(0.047)
Working hours per week	0.004	(0.004)	0.004	(0.005)	0.002	(0.002)	0.003	(0.003)
Firm size (employees)/100	0.011**	(0.005)	0.010*	(0.005)	−0.004*	(0.002)	−0.005*	(0.002)
N	639		457		627		434	
Adj. R^2	0.066		0.063		−0.000		−0.001	

Note: Dependent variable equals 1 if respondent answered in the second follow-up survey that s/he did not receive a voucher while s/he did, 0 otherwise, and vice versa. Results are from linear probability models. Results in columns (2) and (4) are based on observations that actually responded to the second follow-up survey. Results in columns (1) and (3) are based on the entire sample, where for observations that did not respond to the second follow-up survey it is assumed that they are not misinformed. Regressions also include dummies for sector funds and for missing values. Robust standard errors clustered at the firm level are in parentheses. ***, **, and * indicate significant differences at 1%, 5% and 10% levels.

From the information about the informedness about the actual treatment status, we can construct the variable "perceived voucher eligibility". The effect of perceived voucher eligibility on training participation can be estimated using an instrumental variable approach, where the endogenous variable perceived voucher eligibility is instrumented by the assigned treatment status. Table A2 reports the results. The first two columns show first stage results; the last two columns show 2SLS results. The impact of perceived voucher eligibility on training participation among the compliers in Table A2 is larger than the impact of voucher eligibility on training participation in Table 5. Compliers in this analysis are participants who report that they have a voucher when assigned to the treatment group and who report that they don't have a voucher when assigned to the control group, hence these are people who report their actual treatment status accurately.

Table A2
Effect of perceived voucher eligibility on training participation.

	Perceived eligibility		Training participation	
	(1)	(2)	(3)	(4)
Assigned treatment status	0.623***	0.653***		
	(0.036)	(0.034)		
Perceived voucher eligibility			0.264***	0.300***
			(0.065)	(0.056)
F-value	299.48	368.87		
Controls	Funds	All	Funds	All
N	891	891	891	891

Note: Columns (1) and (2) report estimates of regressions of perceived eligibility on assigned treatment status. These are the first stage results. Columns (3) and (4) report the 2SLS estimates of regressions of training participation on perceived voucher eligibility, where perceived voucher eligibility is instrumented by assigned treatment status. Regressions in columns (1) and (3) also include dummies for sector funds. Regressions in columns (2) and (4) also include dummies for sector funds and the variables included in the last two columns of Table 3, plus dummies for missing values. Regressions are restricted to observations who responded to the second follow-up survey, which included the question about perceived voucher eligibility. Robust standard errors clustered at firm level are in parentheses. ***, **, and * indicate significances at 1%, 5% and 10% levels.

A.2. Response to follow-up surveys

To assess whether attrition is significantly related to treatment status and other covariates, we regressed indicators for responding in 2007, in 2008, and in 2007 and/or 2008 on treatment status and other covariates. Table 4 in the main text reports only the coefficients of treatment status in these regressions. Table A3 presents the full regression results. This shows that response to the follow-up surveys is not systematically related to pretreatment variables. Only the coefficients for working hours are statistically significant in all three regressions, all at the 10%-level.

Table A3
Full regression results for response to follow-up surveys.

	2007		2008		2007 and/or 2008	
	Coeff	s.e.	Coeff	s.e.	Coeff	s.e.
Voucher	0.003	(0.011)	0.031	(0.022)	−0.005	(0.015)
Male	−0.015	(0.016)	0.032	(0.041)	0.015	(0.025)
Married	0.010	(0.012)	0.020	(0.030)	0.014	(0.020)
Children	0.006	(0.014)	0.035	(0.029)	−0.005	(0.019)
Immigrant	−0.032	(0.031)	−0.079	(0.060)	−0.002	(0.048)
Age	0.005	(0.005)	0.017*	(0.010)	0.008	(0.007)
Age squared	−0.005	(0.006)	−0.015	(0.012)	−0.008	(0.008)
Education (relative to lower secondary)						
– Uncompleted	−0.013	(0.023)	−0.013	(0.041)	−0.044	(0.037)
– Primary	0.040	(0.035)	−0.060	(0.046)	0.028	(0.033)
– Intermediate secondary	−0.012	(0.013)	0.027	(0.029)	0.017	(0.021)
– Secondary vocational	0.006	(0.013)	0.050	(0.031)	0.017	(0.020)
– Upper secondary	0.022	(0.025)	0.041	(0.050)	0.032	(0.034)
– Higher	0.027	(0.056)	−0.000	(0.107)	0.017	(0.062)
– Other/unknown	−0.084**	(0.032)	0.095	(0.084)	−0.033	(0.087)
Risk tolerance	−0.001	(0.003)	−0.008	(0.005)	−0.002	(0.004)
Savings of ¤1000	0.004	(0.014)	0.030	(0.025)	0.010	(0.017)
Previous training	0.006	(0.013)	0.005	(0.022)	0.002	(0.016)
Log monthly earnings	−0.009	(0.020)	−0.010	(0.037)	−0.010	(0.024)
Working hours per week	−0.003*	(0.001)	−0.005*	(0.003)	−0.004*	(0.002)
Firm size (employees)/100	−0.002	(0.002)	0.002	(0.004)	−0.001	(0.002)
N	1266		1266		1266	

Note: Dependent variable equals 1 if participant responded to the follow-up survey(s) indicated in top row, 0 otherwise. Results are from linear probability models. Regressions also include dummies for sector funds and for missing values. Robust standard errors clustered at the firm level are in parentheses. ***, **, and * indicate significant differences at 1%, 5% and 10% levels.

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