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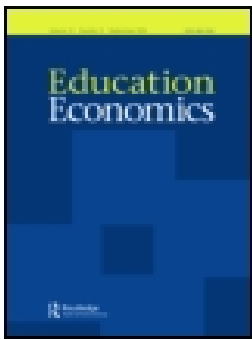
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# The effects of an education-leave program on educational attainment and labor-market outcomes

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## ABSTRACT

I study the effect of an education-leave subsidy for the employed on labor-market outcomes and educational attainment using Finnish administrative linked employer-employee panel data and matching methods. The adult education allowance is available to employees with at least eight years of work experience and allows them to take a leave for 2–18 months to participate in an education program. I find large positive treatment effects on educational attainment and changing occupation. The treatment effects on earnings and employment are negative during the lock-in period and close to zero afterward. The effects are more positive for the less educated.

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## Introduction

Lifelong learning and adult education are important policy objectives in many countries (OECD 2005, 2013). The form of lifelong learning can vary from informal learning at work to formal education (OECD 2005, 22). The rationale is that lifelong learning is thought to bring benefits both for the individual undertaking the education and society at large. It is argued that individuals gain in terms of higher wages, better employment opportunities, and increased well-being, while the societal benefits derive from social outcomes, such as improved civic participation.

Adult education is also potentially a way to counteract the structural changes taking place in the labor market. It has been widely documented not only that occupational structures are changing rapidly (Acemoglu and Autor 2011; Goos and Manning 2007; Goos, Manning, and Salomons 2014) but also that the skills needed in the labor market are changing within and between occupations (Spitz-Oener 2006). These developments mean that many individuals may need to update their skills over their lifetime.

Education-leave programs<sup>1</sup> are one policy tool that encourages adults to upgrade their skills. In such programs, employees have the right to take a prolonged study leave and are partly compensated for their lost earnings, which are an important determinant of the private rate of return on education for adults (OECD 2003). Such programs exist in Austria, Finland, France, Norway, Spain, and Sweden, for example (Bassanini 2004, Table 5). The leave periods vary among countries from a couple of months to about one year, and the amount of subsidy varies from a small fixed daily allowance to full coverage of forgone wages (Bassanini 2004, Table 5).

Despite the policy interest, little is known about the impact of programs aimed at increasing participation in adult education or training for employed workers. Most analyses of government-

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sponsored adult education focus on the unemployed or those under the threat of unemployment (McCall, Smith, and Wunsch 2016), and to my knowledge, education-leave programs have not been evaluated rigorously, even though they are quite common. McCall, Smith, and Wunsch (2016) recently suggested that more evidence is needed regarding the effectiveness of training programs for the employed and that more welfare-relevant outcomes, such as wages or hours of work, should be studied in addition to employment. Moreover, despite changing occupational structures, the role of adult education in individuals' decisions to change occupation has not been studied.

To fill this gap in the literature, I analyze the impact of a Finnish policy instrument called the *adult education allowance* on educational attainment, employment, wages, and changing occupation using rich administrative data and matching methods. The adult education allowance is subject to eligibility criteria, of which the main criterion is the requirement of eight years of work history. The purpose of the adult education allowance is to support employees' voluntary education, and the allowance is intended for full-time education programs at the secondary or tertiary level. The allowance period may vary from 2 to 18 months. Employees are able to utilize this allowance because they are allowed by law to take a study leave for up to two years and then be reinstated at their previous workplace at the previous terms of employment after the education leave. The amount of the allowance is earnings-related and roughly similar in level to unemployment benefits. All education is free in Finland (except for material costs), so the allowance is intended to diminish the opportunity costs of education.

The adult education allowance is a major program. In 2011, the year that I study, the total amount of subsidies was about €70 million; since then, the amount has increased to about €200 million. These are significant numbers compared to the training programs for the unemployed, for which the budget was about €250 million in 2011.<sup>2</sup>

In the analysis, I focus on 2011 because it is the first year after a major reform concerning the adult education allowance in late 2010.<sup>3</sup> Focusing on this year allows me to analyze the longest time period possible under the new system. The treatment group consists of the 6,302 individuals who received the allowance in 2011 and for whom the relevant pre-program data were available from the Finnish Linked Employer-Employee Data (FLEED), which contains labor-market and earnings data on the whole working-age population from 1990 to 2017. Thus, the treatment here is the receipt of the adult education allowance. The comparison group is formed from the population of Finnish employees in 2011 for whom the relevant pre-program data are available (1,685,443 observations). I use propensity score matching to estimate the treatment effect on the treated.

Identification of the causal effect of the adult education allowance is based on the conditional independence assumption (CIA), which states that, conditional on observed covariates, the treatment is independent of potential outcomes. In practice, this means that I need to observe a rich set of variables that affect both selection to treatment and the outcomes. I argue, based on economic theory and prior empirical research, that the data I use are rich enough for a credible analysis. In particular, the data include wage and employment histories, detailed demographic information and lagged outcomes, which have been shown to be important for credible analysis. I also empirically assess the plausibility of the CIA following the methods in Imbens and Rubin (2015) and analyze the sensitivity of the results to violations of the CIA following the method of Rosenbaum (2002).

I study the effects of the adult education allowance up to 2017. I find that earnings dip as employees start an education program. In 2013, the earnings start to converge to the earnings of the comparison group and reach the average earnings of the comparison group by 2015. The results for employment show a similar dip, but the employment rate of the treatment group remains below the comparison group until 2017. This difference in employment rates is driven by some treated individuals continuing an education program until 2017. Concerning the other outcomes, I show that the treated attain more new educational degrees and that they change their occupations much more frequently than the comparison group. Given the long lock-in period, a longer evaluation period could show better economic outcomes for the treated.

The treatment effects are likely to be heterogeneous and thus it may be possible to improve the impact of the subsidy by targeting the groups that benefit the most. To study the heterogeneity of the treatment effects, I conduct subsample analyses and show that employees in the private sector and less educated employees use the subsidy to make bigger changes: they change their field of education and occupation more often than others.

Although I argue that CIA likely holds in this setting, I conduct the following analyses to assess the credibility of the estimates. First, I show that the treatment effects on pseudo-outcomes are small and statistically insignificant, which supports the plausibility of the CIA. Second, I show that the results are robust to substantial violations of the CIA using the method of Rosenbaum (2002). As a final robustness check, I show that the results are similar when using fixed-effects panel data methods that have recently been used in the literature to evaluate the impact of adult education on earnings (Carruthers and Sanford 2018; Stevens, Kurlaender, and Grosz 2019).

## Related literature

This paper is related to three strands of literature. The first is the literature on government-sponsored training for the unemployed, which has been reviewed by Card, Kluve, and Weber (2010) and McCall, Smith, and Wunsch (2016). Much of this literature uses nonexperimental data, and I follow the methods used in this strand of literature. This literature shows that the effects of training programs take years to materialize and depend to a large extent on the business cycle and program details. My focus is different from this literature in that I consider employed workers instead of unemployed.

The second strand is the much smaller literature on training subsidies for employed workers (Dauth and Toomet 2016; Gorlitz and Tamm 2016; Hidalgo, Oosterbeek, and Webbink 2014; Schwerdt et al. 2012). As a whole this literature shows non-existent or small effects on employment and wages.

Schwerdt et al. (2012) conducted a field experiment in which Swiss employees received training vouchers ranging from 200 to 1,500 Swiss francs. The authors did not find any effects on employment, educational attainment, or wages one year after the experiment. Hidalgo, Oosterbeek, and Webbink (2014) conducted a randomized experiment in which low-skilled Dutch workers received training vouchers worth €1,000. They found that the vouchers increased training participation but did not affect job mobility or wages. Gorlitz and Tamm (2016) investigated a German training voucher program and compared participants to employees who received the voucher and intended to attend training but did not do so for random reasons. The voucher covered 50% of training costs, up to a maximum subsidy of €500. They found that the vouchers did not affect employment or wages but had an impact on the job tasks of the voucher recipients. Dauth and Toomet (2016) analyzed a German training subsidy program for older workers. The training can take a variety of forms and lasts 115 days, on average. They found that the program had a positive impact on employment.

The adult education allowance that I study differs from these studies in the nature of the education and the sums of money involved: the subsidy period lasts 2–18 months (the actual education may take even longer), and the education subsidy is, on average, more than €1,300 per month. Thus, compared to the previous studies, this program offers longer periods of education and considerably higher levels of compensation.

The third strand is the literature on the impact of adult education on labor-market outcomes. This literature has mixed findings concerning the impact on wages and employment and shows that the impacts are heterogeneous.

Some of the papers in this strand of literature consider completed degrees. Blanden et al. (2012) used panel-data techniques and found that in the United Kingdom (UK), women gain in terms of wages from attaining certified qualifications as adults, but men do not. Hällsten (2012) showed that earning a tertiary degree in adulthood strongly increases the employment rate but has only small effects on earnings in Sweden. Jepsen, Troske, and Coomes (2014) studied the labor-market returns to community college degrees, certificates, and diplomas. Their results showed that

returns were generally positive, but heterogenous. My focus differs from these studies, as I consider not only those who complete a degree but also those who start a program or complete only part of their degree program. Thus, I focus on the impact of the adult education allowance and not on the completion of a degree.

There are several papers in this strand of literature with a similar focus on enrollment in formal education instead of completing a degree. Stenberg (2011) studied Swedish low-skilled employees and found that adults who complete formal education experience wage gains. Jacobson, LaLonde, and Sullivan (2005b) and Jacobson, Lalonde, and Sullivan (2005a) studied the returns to community college schooling for displaced workers. They found that the earnings impact turned positive about one year after leaving school<sup>4</sup> and that the returns were substantial. Stenberg and Westerlund (2016) studied how enrollment in tertiary education affected the wages of adults. They found a positive impact, but only approximately ten years after enrollment. Stenberg, de Luna, and Westerlund (2014) studied 42–55-year-old workers who enrolled in formal education. They found positive wage impacts for women about seven years after enrollment. The results for men were insignificant. I add to this literature by considering other outcomes in addition to earnings and employment. The literature has not considered how adult education is linked to individuals' decisions on changing occupation, although the recent literature on job polarization has shown that occupational structures are evolving rapidly.

### Adult education allowance

The adult education allowance is granted by the Education Fund, which is administered jointly by the employers' organizations and trade unions. The Education Fund receives its financing from the Unemployment Insurance Fund, which, in turn, is financed by investment income and compulsory unemployment insurance contributions collected from employers and employees.

The allowance is an education-leave program, which means that employees have the right to take a prolonged study leave and are compensated partly for the forgone earnings. Employees are entitled to a study leave of a maximum of two years if their employment relationship has lasted for at least one year.<sup>5</sup> The purpose of the adult education allowance is to support employees' voluntary education. The allowance period may vary from 2 to 18 months, and an individual may receive the allowance only once. The allowance is intended for full-time studies at the secondary or tertiary level.

All employees who fulfill the following criteria are eligible for the adult education allowance. The applicant must

- have at least eight years of work experience;
- have an ongoing employment relationship or pension-insured entrepreneurship that has lasted for at least one year;
- be on unpaid education leave for at least two months;
- be enrolled at an educational institution; and
- not receive other public funding for the education program.<sup>6</sup>

Eligible education programs include programs that lead to a degree and vocational training programs that are organized by a Finnish educational institution under government supervision. The education programs do not need to relate to the work carried out at the current employer. In practice, the studies take a variety of forms. Some employees finish their degrees, and some start completely new degrees, which means that the actual education program may be as short as several months or as long as four to five years.

The adult education allowance is designed to partly reimburse wages lost while the employee attends the education program. This opportunity cost is the main cost component, as all publicly

provided education in Finland is free. Students pay only for study materials and some other minor fees. The allowance consists of a fixed monthly allowance and an earnings-related component. In 2011, the monthly allowance was €553.41 per month, and the earnings-related component was 45% of the difference between an employee's monthly earnings and the monthly allowance. If the monthly wage exceeded €2,702.70, the earnings-related component was 20% of the amount exceeding this threshold. Finally, the allowance was capped at 90% of the employee's monthly earnings.<sup>7</sup> The average allowance was €1,304 per month, and the maximum allowance was €4,138 per month in 2011 (Koulutusrahasto 2011).

These institutional features mean that the employee is the main decision maker concerning the adult education allowance. The employer cannot deny the employee an education leave, does not bear any direct costs of the education, and cannot alter the employment contract in any way due to the employee's decision to attend an education program.

The employee's decision to apply for the allowance can be thought to occur in two stages. First, the person decides whether to study or not. To do this, she compares the costs and benefits. The costs depend on whether she is eligible to apply for the allowance and whether she is aware of the allowance. Heckman and Smith (2004) show that awareness of the program is a major determinant of training program participation. If she is eligible and aware, she applies for the allowance and enrolls in an educational institution if the benefits exceed the costs. If she is not eligible (due to a too-short work history or tenure) or not aware of the program, she will not apply but may still study.

## Data

I use Statistics Finland's FLEED, which contains the whole working-age population between the ages of 15 and 70.<sup>8</sup> The FLEED is based on administrative register data and, thus, is detailed and reliable. To these data, I match the Education Fund's client register. This data set is used to identify the individuals who receive the adult education allowance. The FLEED contains the outcomes (annual earnings, employment, occupation, and educational attainment) and variables needed for credible matching (personal characteristics as well as labor-market and education histories). These data are rich, and I can follow individuals over time from 1988 to 2017. I use the data from 2005 onward due to changes in the content of some variables.

The treatment group consists of the individuals who first received the adult education allowance in 2011. The control group is formed from the working-age population. I drop from the control group anyone who later applies for the adult education allowance.<sup>9</sup> The original data from the Education Fund comprise 6,904 individuals whose allowance period begins in 2011. I find the necessary pre-program data and outcomes for 6,302 individuals.<sup>10</sup> The FLEED contains 1,685,443 employed individuals with all necessary pre-program data and outcomes, and these individuals form the potential control group. I drop individuals with values of variables that predict participation or non-participation perfectly. For example, there are no participants in the treatment group with only a primary education, so anyone with only a primary education is dropped from the potential control group. This also means that the potential control group consists of individuals whose work history and current tenure matches treatment groups work history and current tenure, which are the key eligibility criteria.

The outcomes I consider are annual earnings, employment, educational attainment, and changing occupation. The annual earnings include earnings from employment and taxable social benefits, such as the adult education allowance. Employment status is measured at the last week of the year. I use this measure because it matches the definition of employment used by Statistics Finland. Educational attainment is measured by the change in the level of education or the field of education. The level and field of education are measured with two-digit educational codes, which are based on the International Standard Classification of Education (ISCED). The information on educational attainment come from the Degree Register. The information in this register comes

directly from the providers of education and is highly reliable. Change in occupation is defined as a change in the two-digit occupational code. The occupational classification follows the International Standard Classification of Occupations (ISCO) and is coded by Statistics Finland for each individual. This information is also highly reliable and occupational coding is not dependent on employer. There is a change in the classification in 2010, which means that the occupational codes before and after 2010 are not comparable.

Table 1 shows selected summary statistics comparing the pre-treatment variables of the potential control groups and the treatment group. On average, the treatment group is a bit younger and contains a much larger proportion of women. The annual earnings of the treatment group are, on average, lower and much less dispersed than in the group of potential controls. The educational attainment between the groups also differs: the treatment group has a higher level of education, and the group's field of education is more often Health and Welfare and less often Technology. A notable difference between the groups is that the treated are much more often employed in the public sector and less often in the private sector. Manufacturing workers are underrepresented among the treated, whereas professionals and service and sales workers are overrepresented among the treated.

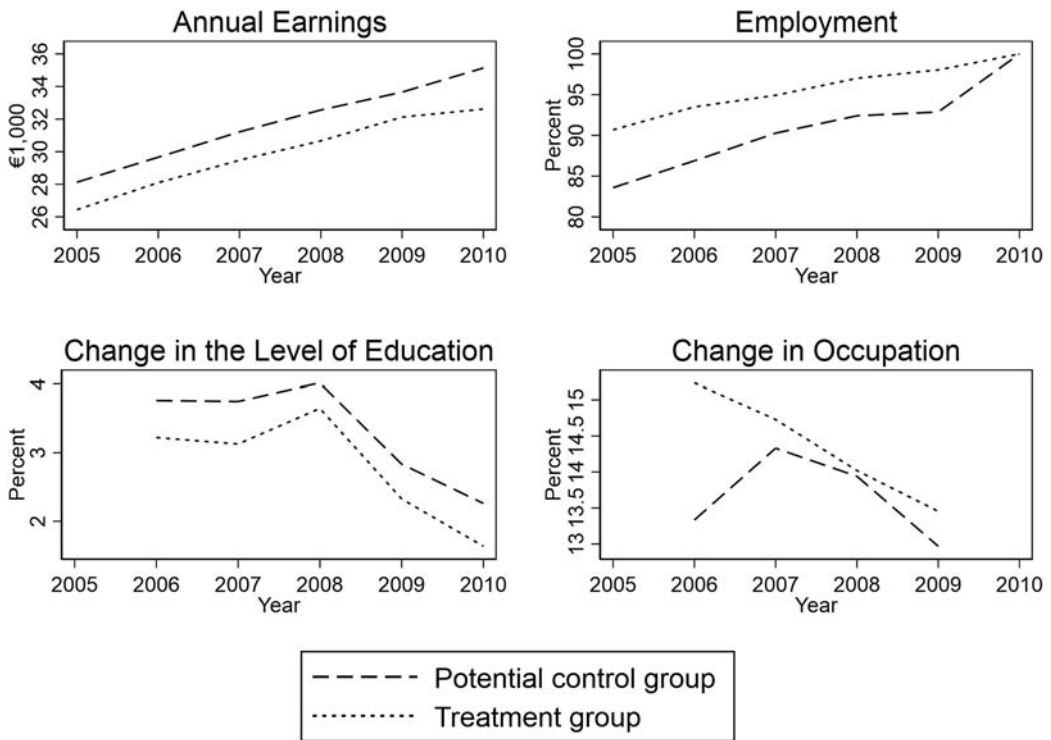
Figure 1 shows the pre-treatment development of annual earnings, employment, the probability of changing the level of education, and the probability of changing occupation for the treatment group and the potential comparison group. As is seen in the upper-left corner, although the level

**Table 1.** Selected summary statistics.

	Potential controls		Treatment group	
	Mean	Std Dev	Mean	Std Dev
Age	41.62	11.15	38.69	7.90
Female	0.52		0.79	
Annual earnings (€1000)	35.21	19.60	32.62	12.77
<i>Level of Education</i>				
Upper secondary	0.51		0.41	
Post-secondary non-tertiary	0.01		0.01	
Short-cycle tertiary	0.17		0.16	
Bachelor's or equivalent	0.16		0.25	
Master's or equivalent	0.14		0.16	
Doctoral or equivalent	0.01		0.01	
<i>Field of Education</i>				
General	0.08		0.07	
Educational science	0.03		0.06	
Humanities and arts	0.05		0.08	
Social sciences and business	0.20		0.21	
Natural sciences	0.03		0.02	
Technology	0.29		0.10	
Agriculture and forestry	0.04		0.02	
Health and welfare	0.16		0.32	
Services	0.12		0.13	
<i>Employer Type</i>				
Private	0.55		0.40	
State	0.10		0.10	
Municipality	0.25		0.42	
Foreign-owned	0.10		0.08	
<i>Occupation</i>				
Managers	0.04		0.02	
Professionals	0.22		0.27	
Technicians and associate professionals	0.21		0.25	
Clerical support workers	0.07		0.07	
Service and sales workers	0.19		0.26	
Skilled agricultural workers	0.03		0.01	
Craft and related trades workers	0.10		0.03	
Plant and machine operators	0.08		0.04	
Elementary occupations	0.06		0.04	

Note. The number of observations is 1,685,443 for the potential controls and 6,302 for the treatment group.





**Figure 1.** Descriptive graphs of the key outcome variables before treatment.

of annual earnings is lower for the treatment group, the trends are similar until 2009. The level of employment in the treatment group is higher throughout the pre-treatment period, but the groups converge by 2010 (by construction). In the lower-left corner, it is evident that the treatment group has a lower probability of changing level of education than the comparison group, but the trends are quite similar. The treatment group also has a higher probability of changing occupation throughout the pre-treatment period. The information for the year 2010 is missing due to the change in the occupational classification. These descriptive graphs show that there are permanent differences between the treatment and potential comparison groups. Matching with respect to these variables is, thus, important for credible analysis. Later, Figures 2–5 show that matching successfully eliminates the differences seen in Figure 1.

## Methods

To evaluate the effect of the allowance on labor market outcomes, I need to identify the potential outcome of not receiving the allowance, which can be estimated using various methods. I rely on the rich data and adopt a selection on observables approach. I assume that there are no general equilibrium effects, that is, that the treatment of an individual does not affect the outcomes of other individuals. This assumption likely holds, as the number of employees receiving the adult education allowance is small compared to the eligible population. The validity of the selection on observables approach relies on the CIA, which is a strong assumption. In practice, the CIA requires that I observe all variables that affect both participation and the outcomes in the absence of participation.

To evaluate the plausibility of the CIA, I first discuss the relevant economic theory regarding the demand for education and then discuss which variables have been found to be important for credible non-experimental estimates of the impacts of training programs.

The standard human capital model suggests that individuals may make different educational choices because of variations in the discounted benefits, opportunity costs, and direct costs of education. The theory produces the following implications (see for instance Checchi 2006). Individual ability, age, and subjective intertemporal discount rate generate variation in the discounted benefits and thus in the demand for training. Variation in the benefits may also be due to resources invested in education (e.g. quantity and quality of teachers) in the region of residence. The opportunity costs, in turn, depend heavily on the current wage and employment prospects. The opportunity costs also depend on, for example, the number of children. If credit markets are imperfect, family income affects training decisions.

The initial human capital stock also affects the demand for training. In the standard model, the benefits are larger and opportunity costs smaller for employees with lower levels of human capital, so they should demand more training. However, Cunha et al. (2006) argue that skill attainment at one stage of the life cycle raises skill attainment at later stages of the life cycle (self-productivity) and that earlier investments increase the productivity of later investments (complementarity). These considerations suggest that individuals with higher human capital stock may want to invest more in training.

The standard model cannot explain why some individuals choose to invest heavily in training mid-career (McCall, Smith, and Wunsch 2016). However, by introducing a stochastic depreciation rate, the model can explain this. For example, a technological or health shock may make an individual's human capital stock obsolete and lead to high investments in training.

These considerations suggest that the conditioning variables should include age, number of children, region of residence, family income, and initial levels of human capital, as well as proxies for ability (e.g. wage, employment, and education histories), subjective intertemporal discount rate (e.g. educational history), and health.

There are also empirical evaluations of data requirements for credible analysis of training programs. Heckman and Smith (1999) study a training program available to both employed and unemployed individuals to compare nonexperimental evaluation strategies to experimental evaluations. Their results show that in addition to controlling for wage history, it is important to control for recent labor force status dynamics and individual characteristics such as age, schooling, marital status, and family income to reduce selection bias. Mueser, Troske, and Gorislawsky (2007) study the same program and reach similar conclusions. Their key finding is that program impacts can be estimated using typically available administrative data (which includes earnings and employment history in addition to individual characteristics). Caliendo, Mahlstedt, and Mitnik (2017) studied the impact of usually unobserved factors, such as psychological attributes and social networks, on matching evaluations of active labor-market programs. They find that accounting for these attributes does not change the estimated treatment effects when using rich administrative data that contain labor-market history data. Recently, Lechner and Wunsch (2013) considered the variables that are needed in matching analyses of training programs for the unemployed to remove selection bias. They find that the key variables are basic individual characteristics, pre-treatment outcomes, and short-term labor-market history. Thus, their conclusion is similar to the ones reached for training programs that are also available for the employed.

The data I use contains the information needed for credible analysis, as identified above. As the estimation method, I use propensity score matching (Rosenbaum and Rubin 1983) because the data set is quite large, and other methods, such as covariate matching and coarsened exact matching estimators, are computationally infeasible even with the resources available from Statistics Finland. I use a logit model to estimate the propensity score and perform single-nearest-neighbor matching with replacement. I calculate standard errors that account for the estimation of the propensity score (Abadie and Imbens 2016). The individuals start to receive the adult education allowance in 2011, and I measure all outcomes in the 2011–2017 period (2016 in the case of changing occupation).<sup>11</sup> I also study the cumulative outcomes from 2011 to 2017.

I measure all covariates from 2010 or earlier. The basic characteristics that I consider are gender, age, nationality, native language, marital status, number of children younger than 3, 7, and 18 years

old, level of education, field of education, region of residence (20 categories), occupation (two-digit ISCO), household disposable income, and amount of debt.

I use pre-treatment outcomes from the previous six years (all models include the other pre-treatment outcomes). This means that I match on the history of educational attainment in all analyses. This is important because the history of educational attainment works as a proxy for tastes and motivation for education. I also match on the information of whether the person applied to an educational institution in the 2005–2009 period. I do not use the information from the year 2010 because the choice to apply then might be made simultaneously with the decision to apply for the adult education allowance.

Short-term labor-market history is captured by employment status (employed, unemployed, and out of the labor force), annual working days during the previous six years, changing employer from 2005 to 2010, and tenure in current employment. These variables act as proxies for individual ability, motivation, and other such determinants of participation and outcomes.

I measure employer characteristics by the number of employees, the two-digit industry code, and the employer's legal form (private, state, municipality, or foreign-owned). The receipt of sickness allowance during the previous six years captures health considerations.

I take the cube roots of all monetary variables to reduce the scale of the variables. In the case of pre-program earnings, I drop observations from the potential comparison group whose earnings fall outside the support of the treatment group. I also include some quadratic terms and interactions to improve the balance of the covariates. The complete specification of the model for the propensity score is given in Online Appendix A.

## Results

### *Quality of matching*

I assess the quality of the matching by calculating the standardized differences of the covariates. These standardized differences are more useful than *t*-tests in assessing the covariate balance because in large samples, even small differences in the covariates would be statistically significant, although the practical difference in the means is small (Imbens and Rubin 2015, 311).

Table B1 in Online Appendix B shows the results of the balancing tests, which reveal that the covariates balance very well. For example, the median standardized difference is 1.0%, and the largest standardized difference in the absolute value is 3.7%.<sup>12</sup> The variance ratios are also quite close to 1 for most variables. The variance ratio is outside the 0.95–1.05 interval for 14% of the continuous variables. Thus, the means and the variances are very similar in the treatment and comparison groups, indicating a good balance of the covariates. Figure B1 shows that the distribution of the linearized propensity score is very similar for the treatment and comparison groups. Moreover, all observations are on common support.

### *Plausibility of the CIA*

To address the plausibility of the CIA, I estimate the treatment effects on pseudo-outcomes, using the lagged values of the dependent variables as the pseudo-outcomes as suggested by (Imbens 2015; Imbens and Rubin 2015). Finding a treatment effect of zero increases the plausibility of the CIA, whereas finding a nonzero treatment effect decreases the plausibility of the assumption. I estimate the 'treatment effect' of the adult education allowance on outcomes in 2010, while conditioning on outcomes in 2009 and earlier.

Naturally, the dependent variables are omitted from the propensity score in this analysis. For change in occupation and employment, I use two-year lags, whereas for other variables I use one-year lags. I use two-year lags for employment because all individuals in the treatment group (and, thus, in the comparison group) are employed in 2010. For changes in occupation, I use a two-year lag because a change in the occupational classification makes it impossible to calculate

**Table 2.** Assessing the CIA: average treatment effects for pseudo-outcomes.

	(1) Earnings t-1	(2) Employment t-2	(3) Change in Occupation t-2	(4) Change in the Level of Education t-1	(5) Change in the Field of Education t-1
ATT	-0.364 (0.223)	0.000 (0.003)	0.009 (0.006)	-0.003 (0.002)	0.002 (0.002)

*Note.* The table reports the average treatment effect on the treated and standard errors, \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . Standard errors take into account the estimation of the propensity score.

The specification of the model for propensity score is given in Online Appendix A1.

In each column, the model for the propensity score omits the lagged value of the dependent variable.

The number of observations in the potential comparison group is 1,685,443, and in the treatment group, 6,302.

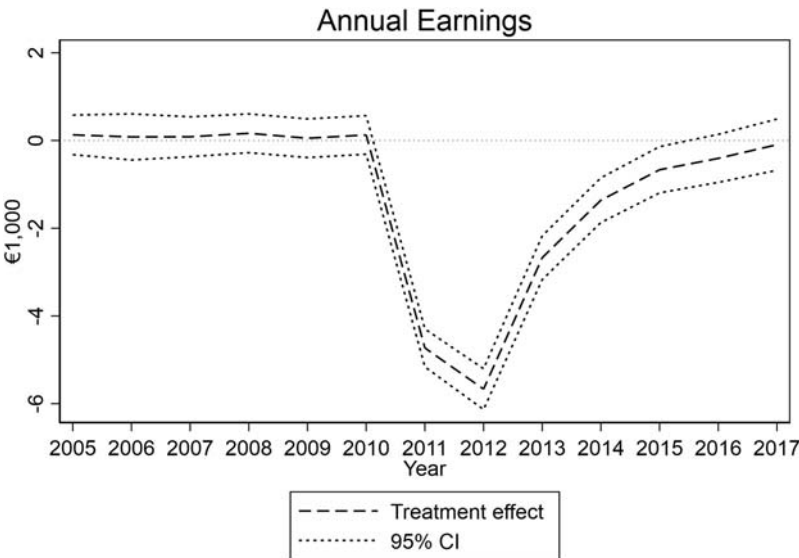
changes in occupation between 2009 and 2010. In Table 2, all five treatment effects are statistically insignificant, which increases the plausibility of the CIA.

### Treatment effects

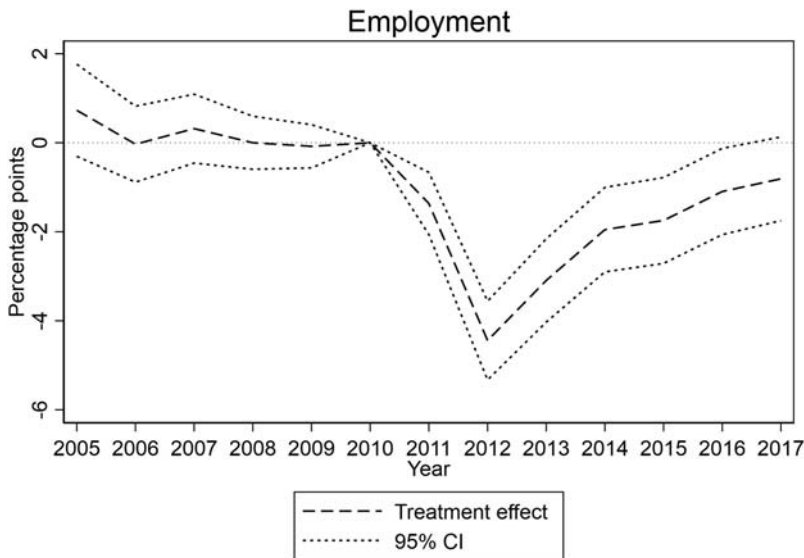
Figure 2 plots the estimated treatment effects against annual earnings from 2005 to 2017.<sup>13</sup> The groups are matched to be similar until 2010; after that, the earnings of the subsidy recipients start to dip as they take leave from their jobs. The earnings difference is largest in 2012, when it is about €5,700. After that, the earnings of the treatment group start to increase, and they reach the level of the comparison group in 2016, where the point estimate is about -€400 but is not statistically significant.

The results are robust to alternative wage measures. Figures D1 through D3 in Online Appendix D show that the results concerning annual earnings are robust to using the cube root of the earnings (this serves a similar purpose as taking logs but allows for zeros), trimming 1% from each tail of the earnings distribution, or focusing on those who are employed in 2016–2017.

Figure 3 shows the results for employment. In 2010, everyone is employed due to the institutional features of the adult education allowance. In 2011, the treatment group is less likely than the comparison group to be employed, as the treatment group is enrolled in an education program; in 2012, this difference is the largest. Then, the employment rates of the two groups start to converge, and the treatment group's rate reaches the comparison group's in 2017.

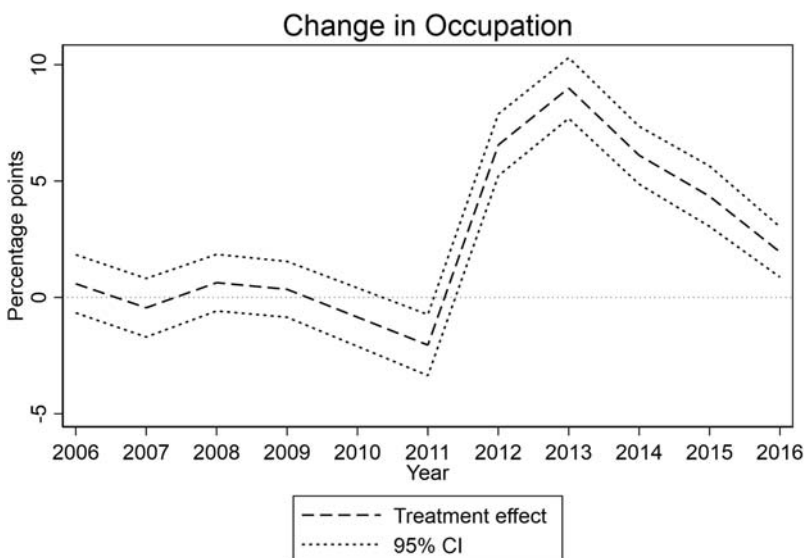


**Figure 2.** Estimated treatment effects on annual earnings.

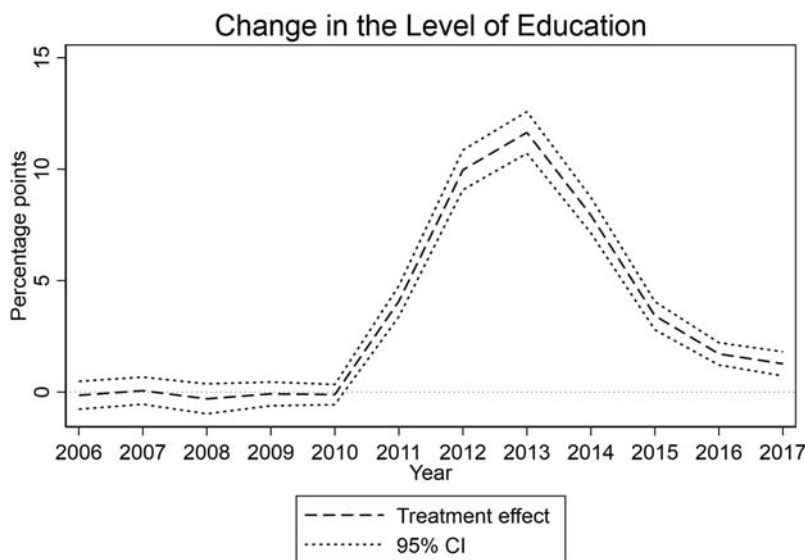


**Figure 3.** Estimated treatment effects on employment.

Although the adult education allowance has insignificant treatment effects on annual earnings and employment by 2017, the allowance has a positive effect on changing occupation. [Figure 4](#) shows that the treatment group switches occupations more often than the comparison group after 2011. The positive effect persists up to 2016, and the point estimates are about two to nine percentage points each year. After 2013, the effect declines, which is natural, as many of the treated have finished their degrees and have already changed occupations. These differences are quite large, given that, on average, about 14% of the employees in the sample change occupations during a given year. The year 2010 is extrapolated in the graph because of a change in the occupational classification, which makes it difficult to accurately define changes in occupation.



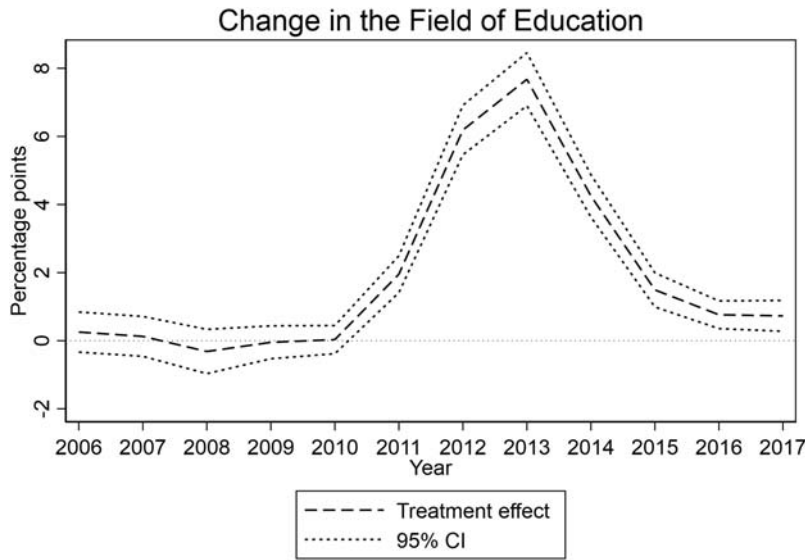
**Figure 4.** Estimated treatment effects on changing occupation.



**Figure 5.** Estimated treatment effects on changing the level of education.

The last two sets of results concern educational attainment. [Figure 5](#) shows the results for changes in the level of education. The figure reveals that the treatment group attains new degrees more often than the comparison group in the 2011–2017 period, but the difference decreases after 2013. The estimated treatment effects are sizable, as they range from about 1.3 percentage points to more than 10 percentage points. In this sample, on average, about 1.3% of employees attain a new degree each year.

[Figure 6](#) shows the results for changes in the field of education. The results are similar to the results for changes in the level of education; the only difference is that the treatment effects are somewhat smaller. The adult education allowance, thus, is used more often to attain a higher degree in the same field of education than to change fields of education.



**Figure 6.** Estimated treatment effects on changing the field of education.

**Table 3.** Cumulative treatment effects.

	(1) Annual earnings (€1,000)	(2) Employment	(3) Change in occupation	(4) Change in the level of education	(5) Change in the field of education
ATT	−15.598*** (1.609)	−0.145*** (0.021)	0.722*** (0.047)	0.400*** (0.009)	0.231*** (0.008)

Note. The table reports the average treatment effect on the treated and standard errors, \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . Standard errors take into account the estimation of the propensity score.

The specification of the model for propensity score is given in Online Appendix A1.

The number of observations in the potential comparison group is 1,685,443, and in the treatment group, 6,302.

One way to summarize the results is to look at the cumulative treatment effects, which are shown in Table 3. Here, I sum all the outcome variables from 2011 to 2017 (2016 for the change in occupation). I use these cumulative results to assess the sensitivity of the results to violations of the CIA and to conduct subsample analyses.

### Sensitivity analysis

To estimate the sensitivity of the results to violations of the CIA, I estimate Rosenbaum bounds for the cumulative outcomes. The key identifying assumption is that there are no unobserved variables that affect selection to treatment and the outcomes. The sensitivity of the results to the failure of this assumption can be analyzed. Suppose that the probability of the treatment depends not only on  $X_i$  but also on some unobservable covariate  $U_i$ ,  $P_i = \Pr(W_i = 1|X_i, U_i) = F(\beta X_i + \gamma U_i)$ , where  $F$  is the logistic function. Rosenbaum (2002, chapter 4) shows how to assess how big  $\gamma$  should be to change the inference concerning the treatment effects. This analysis does not show whether hidden bias is present or not, nor does it suggest relevant magnitudes of hidden bias; however, it does show how strong the selection bias must be to change the inference concerning the treatment effects.

The results are given in Table 4. The table reports the significance level of the treatment effect corresponding to different values of  $\Gamma = e^\gamma$ . The larger  $\Gamma$  is, the stronger the hidden bias; that is, the more the probability of participation deviates from 50% in each matched pair. Positive selection bias arises from treated individuals being more likely to experience positive outcomes (negative bias is defined analogously).

Table 4 shows that for cumulative annual earnings, employment, and changing occupation,  $\Gamma$  larger than 1.25, 1.45, and 1.65, respectively, render the treatment effect statistically insignificant at the 5% level. To put this result into perspective, these magnitudes are similar to changing employers in the previous year. The coefficient on employer change in  $t-1$  in the propensity score logit is  $-0.38$ , which translates to an odds ratio of 1.47. This effect has to be considered quite large, because to be able to take study leave, the employment relationship has had to continue for at least one year. This effect is also larger than the odds ratio of 1.19 between those with a bachelor's or equivalent level of education (the educational group with the highest coefficient in the propensity score logit) compared to those with secondary education (the reference group). For educational attainment  $\Gamma$  should exceed 6 in the case of level of education and 4 in the case of field of education, in order to change the results.

### Heterogeneity of the treatment effects

I report two types of subsample analyses to analyze the heterogeneity of the treatment effects.<sup>14</sup> The first subsample analysis is motivated by occupational restructuring and the resulting need for some employees to update their skills or change occupations. In this analysis, I compare those who are employed in the private sector and those who are employed in the public sector. The occupations more typically found in the public sector (e.g. healthcare and education) are less threatened by automation or globalization compared to the private sector (see e.g. Table 1 in Goos, Manning, and



**Table 4.** Rosenbaum bounds for the cumulative outcomes.

Annual earnings				
$\Gamma$	1.15	1.20	1.25	1.30
Critical P-value	0.000	0.001	0.043	0.352
Employment				
$\Gamma$	1.35	1.40	1.45	1.50
Critical P-value	0.001	0.008	0.059	0.227
Changing occupation				
$\Gamma$	1.55	1.60	1.65	1.70
Critical P-value	0.000	0.001	0.014	0.106
Changing the level of education				
$\Gamma$	5.5	6.0	6.5	7
Critical P-value	0.000	0.000	0.006	0.114
Changing the field of education				
$\Gamma$	3.5	4.0	4.5	5
Critical P-value	0.000	0.005	0.316	0.917

Note. The table reports critical p-values for different values of  $\Gamma$  for the cumulative outcomes. Critical p-values represent the bound on the statistical significance level of the estimated treatment effect. In the case of Annual Earnings, the critical p-values are for the case of negative self-selection, and for the other variables, for the case of positive self-selection.

Salomons 2014). Thus, it is likely that the training needs in these sectors may differ. Moreover, public-sector employees are overrepresented among the recipients of the adult education allowance, as 65% of the recipients work in the public sector, whereas in the whole data set, public-sector employees account for 49% of all employees.

The results in Table 5 show that employees in the private sector experience large wage losses, have worse employment outcomes, change their field of education and occupation more often, and improve their level of education less often than employees in the public sector. Notably, for the public-sector workers, the treatment effect on earnings in 2017 is about €1,000, whereas for the private-sector workers, the point estimate is about –€1,000. Thus, it seems that the nature of the education is different: in the private sector, education is more often about redirecting one's career, and in the public sector, education is more often about advancing along the same career path. These results suggest that to adapt to occupational restructuring, the Education Fund could target private-sector employees in publicity efforts.

The second subsample analysis is motivated by the fact that public programs are frequently aimed at less-educated employees (e.g. Hidalgo, Oosterbeek, and Webbink 2014; Stenberg 2011)

**Table 5.** Cumulative treatment effects by sector.

	(1) Annual earnings	(2) Employment	(3) Change in occupation	(4) Change in the level of education	(5) Change in the field of education	(6) Annual earnings 2017
Public Sector						
ATT	–5.853** (2.006)	–0.008 (0.025)	0.423*** (0.054)	0.432*** (0.012)	0.177*** (0.009)	1.046** (0.349)
Private Sector						
ATT	–24.235*** (2.565)	–0.261*** (0.034)	1.008*** (0.075)	0.346*** (0.013)	0.266*** (0.012)	–1.192* (0.539)
T-statistic for the equality of ATTs	–5.645	–5.995	6.330	–4.861	5.933	–3.485

Note. The table reports the average treatment effect on the treated and standard errors, \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . Standard errors take into account the estimation of the propensity score. Given the independent samples, I ignore the covariance of the ATTs in calculating the  $t$ -statistics, i.e.  $t = \frac{ATT_1 - ATT_2}{\sqrt{SE_{ATT_1}^2 + SE_{ATT_2}^2}}$ . In the public-sector sample, the number of observations in the

potential comparison group is 587,011, and in the treatment group, 3,244. In the private-sector sample, the number of observations in the potential comparison group is 1,098,166, and in the treatment group, 3,058.



and the findings in the prior literature that the returns to adult learning may depend on the level of education. Schwerdt et al. (2012) find that less-educated employees benefit more from training, whereas the Swedish studies tend to show the opposite (e.g. Stenberg 2011; Stenberg and Westerlund 2016).

In Table 6, I split the sample into a subsample of employees with upper-secondary education and employees with higher levels of education. The results in Table 6 show that for the upper-secondary-education subsample, the treatment effects on changing the field of education and occupation are larger than in the high-education group, while the effect on changing the level of education is similar. The treatment effects on annual earnings in 2017 is about €1,000 for the upper-secondary-education subsample, whereas for the higher-education subsample, the treatment effect is slightly negative. The cumulative annual earnings are more negative for the higher-education group, which partly reflects their higher earnings before the subsidy period. The cumulative effects on employment are similar for these two subsamples. Taken together, these results support the earlier results that less-educated adults may benefit more from training than adults with higher levels of education.

### Additional outcomes

In Table E1 in the Online Appendix, I consider some additional outcomes to shed more light on the effects of the adult education allowance. I consider first outcomes related to employment. The three outcomes are being unemployed, being a student, or having 'other' labor market status. These are all measured at the end of the year, similarly to employment. The category of 'other' means that one is not employed, unemployed, studying, or on a pension. It thus measures mostly being outside the labor force. It is seen from the table that the treatment group is more likely to be unemployed after 2012, less likely to be outside the labor force before 2015, and more likely to be studying throughout the period of observation. It is also seen from the table that they are more likely to change employers and experience promotions more often. Thus, it seems that the effects are very heterogeneous. Some are able to progress in their careers, while others are not employed.

### Robustness checks

I show in the appendixes that the results are similar using alternative estimation methods. Online Appendix D, Table D1, shows that the main results are similar when using inverse probability weighting or regression adjustment. Difference-in-differences or a combination of matching and

**Table 6.** Cumulative treatment effects by level of education.

	(1) Annual earnings	(2) Employment	(3) Change in occupation	(4) Change in the level of education	(5) Change in the field of education	(6) Annual earnings 2017
At Least Short-Cycle Tertiary Education						
ATT	−17.497*** (2.219)	−0.139*** (0.025)	0.594*** (0.058)	0.382*** (0.011)	0.153*** (0.008)	−0.367 (0.416)
Upper Secondary Education						
ATT	−9.594*** (1.853)	−0.193*** (0.034)	0.917*** (0.077)	0.397*** (0.014)	0.338*** (0.014)	0.959** (0.356)
T-statistic for the equality of ATTs	2.734	−1.280	3.351	0.842	11.473	2.422

*Note.* The table reports the average treatment effect on the treated and standard errors, \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . Standard errors take into account the estimation of the propensity score. Given the independent samples, I ignore the covariance of the ATTs in calculating the  $t$ -statistics, i.e.  $t = \frac{ATT_1 - ATT_2}{\sqrt{SE_{ATT_1}^2 + SE_{ATT_2}^2}}$ . In the at least short-cycle tertiary sample, the number of observations in the potential comparison group is 823,469, and in the treatment group, 3,740. In the upper secondary sample, the number of observations in the potential comparison group is 861,974, and in the treatment group, 2,562.

differences-in-differences would be alternative estimation strategies based on different identifying assumptions. Imbens and Wooldridge (2009, 70) argue that with panel data, matching is preferred to difference-in-differences. Also, Chabé-Ferret (2017) argues that when multiple pre-treatment outcomes are available, matching is preferred to difference-in-differences. However, Online Appendix D, Tables D2–D4, show that the main results are similar when estimating regressions with individual fixed effects on the matched sample or individual-specific slopes using the full sample.

## Costs and benefits of the allowance

Full cost-benefit analysis is not feasible with the available information, but rough calculations suggest that the allowance is expensive and the benefits are unlikely to cover the costs. The direct costs of the program consist of the amount of the allowance and the costs of providing the education it induces. The indirect costs can be measured by the forgone earnings. The amount of allowance per participant is about €10,000,<sup>15</sup> and the cost of one year of education is also about €10,000.<sup>16</sup> The forgone earnings are, on average, about €16,000 (Table 3). The lock-in effects are large and partly due to the design of the allowance: it is intended for employees with good future employment prospects, and the subsidy period is long (e.g. Wunsch 2016). A conservative estimate of the costs is €36,000 (assuming very conservatively that the allowance induces one year of extra schooling and not taking into account, e.g. the marginal cost of public funds).

The benefits of the subsidy can be calculated from the impact on earnings. The estimates show that, on average, the treatment effect on earnings is close to zero in 2017. For the average member of the treatment group, the remaining career length is about 18 years (they are, on average, 45 in 2017 and likely to retire at around 63). Thus, after 2017, the net present value of the benefits should exceed €36,000 for the benefits to exceed the costs. With a 2% discount rate, this would require a €2,400 constant earnings benefit for the 18-year period. Given that there is no evidence of an impact on earnings six years after starting to receive the subsidy, it is unlikely that the future benefits would be very large. There may be other individual benefits, such as a more satisfying job, but these are difficult to value.

For the lower-educated employees, the costs of the allowance are smaller because the forgone earnings are smaller (Table 6). They also show some earnings benefits at the end of the evaluation period. However, even for them, it is not clear that the benefits exceed the costs.

## Conclusion

In this paper, I evaluate an adult education allowance program using rich administrative panel data and matching methods. I estimate the average treatment effects on the treated individuals on annual earnings, employment, changing occupations, and educational attainment.

I find that the program substantially improves the educational attainment of participants and that the participants change occupations more often than the comparison group. The earnings of the treated drop as they start their education program and reach the level of the comparison group four years after the subsidy period starts. The effect on employment is small and negative until six years after the subsidy period starts.

The goal of the allowance is to support employees' voluntary studies. The results here show that the economic impact of the allowance is negative during the time period studied, and rough calculations suggest that the costs of the allowance greatly exceed the benefits. The large lock-in effects are partly due to the design of the allowance: it is intended for employees with good future employment prospects, and the subsidy period is long. Better targeting and a shorter duration would likely reduce the indirect costs of the allowance.

The adult education allowance that I study differs from that examined in previous studies on adult education subsidies in the nature of the training and education and the sums of money involved: the education periods are longer and the subsidies much larger. Despite the longer education periods

and substantial subsidies, the key results are similar: the subsidies improve education participation, have some impact on the type of work in which the employees engage, and have small or nonexistent effects on employment and earnings after the lock-in period.

Prior literature has argued that education and training subsidies for employed individuals should be targeted at distinct groups in order to have positive impacts on labor-market outcomes (Schwerdt et al. 2012). In Finland, the adult education allowance is untargeted, and subsample analyses show that the treatment effects vary among employees in different sectors and according to the employees' level of education. Employees with only upper-secondary education change their occupation and field of education more often than more highly educated employees, and the impact of the subsidy on their earnings is higher in 2017.

From the perspective of occupational restructuring, the allowance is somewhat misallocated: the occupational groups most often utilizing the allowance are not threatened by automation or outsourcing. If manufacturing workers used the allowance more often, they might be better able to cope with occupational restructuring.

## Notes

1. These are also called training-leave programs in the literature. This study uses *education leave* because many employees use the leave to obtain a new educational degree.
2. <http://budjetti.vm.fi/indox/sisalto.jsp?year=2011&lang=fi&maindoc=/2011/TAE/he/he.xml&opennode=0:1:359:663:1449:1497:1513>.
3. The reform increased the allowance, on average, by about 40%, and the regulation concerning the length of the allowance period was simplified. Previously, the length depended on work history, and the maximum duration was 18 months. The reform does not generate clearly identifiable treatment and control groups, which is why I do not use the reform to estimate the effects.
4. See page 286.
5. The employer may postpone the study leave by six months if it would lead to considerable harm for the employer. Employers who employ more than five employees may postpone the study leave at most twice.
6. Other possible sources of funding are study grants from the Social Insurance Institution KELA (means tested, approximately €80–€250 per month, for a fixed period) or grants from private foundations.
7. Formally, the allowance is determined by  $\text{Min}\{0.9 * E, I(E \leq 2707) * 0.45 * (E - M) + I(E > 2707) * (0.45 * (2707 - M) + 0.2 * (E - 2707))\}$ , where  $E$  is earnings,  $M$  is the fixed monthly allowance, and  $I(\cdot)$  is an indicator function.
8. A description of the data is available at [https://taika.stat.fi/fi/aineistokuvaus.html#!?dataid=YA244\\_19882014\\_jua\\_henkilot\\_000.xml](https://taika.stat.fi/fi/aineistokuvaus.html#!?dataid=YA244_19882014_jua_henkilot_000.xml). The outcomes for 2016 actually come from Statistics Finland's FOLK database, which will replace FLEED starting in 2019.
9. In principle, this group could be used to estimate the impact of earlier vs. later treatment, but because I make rough cost-benefit calculations, later applicants cannot be included in the comparison group.
10. Even though the data covers the whole working-age population, some individuals have missing values for some important variables, such as occupation. For example, the work histories of individuals working in the smallest firms may be incomplete.
11. The data on occupation are currently only available up to 2016.
12. T-tests of the differences in means between the treatment and control groups show t-statistics above 1.96 only for one variable.
13. Tables of the treatment effects are provided in Online Appendix C.
14. The model for the propensity score is slightly different from the full specification because finely grained categorical variables would lead to singleton dummy variables for some subsamples. In these analyses, I omit three occupational dummies due to low cell counts; the industry is a one-digit level; and employment status is "employed" or "other" instead of "employed", "unemployed", or "out-of-labor force".
15. This is a rough calculation based on the average subsidy in 2011 (€1,300/month) multiplied by the average subsidy duration in 2011 (7.7 months). This information is available from Koulutusrahasto (2011). The average duration is calculated by dividing the number of days compensated in 2011 by the number of recipients in that year. It does not thus correspond to the average duration of subsidy spells that started in 2011. The information would be preferable, but is not available.
16. The information on the costs of providing education is available from [https://www.stat.fi/til/kotal/2014/kotal\\_2014\\_2016-05-11\\_tau\\_003\\_fi.html](https://www.stat.fi/til/kotal/2014/kotal_2014_2016-05-11_tau_003_fi.html).

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