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# Labor Market Effects of Adult Education Vouchers: Evidence from a Randomized Field Experiment

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Preliminary Version

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## Abstract

Lifelong learning becomes increasingly important in times of population ageing, but little is known about the returns to adult education or the effectiveness of government attempts to promote adult learning. This paper evaluates the effects of a large-scale randomized field experiment with vouchers for adult education. We find no significant average labor market effects of voucher-induced self-financed adult education for compliers, but also find evidence for substantial effect heterogeneity. Overall, these findings cast doubts on the effectiveness of existing, unrestricted voucher programs to promote adult education.

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# 1 Introduction

Although public funding of life-long learning is politically undisputed in virtually all countries, evidence and economics-based arguments for this kind of intervention is thin on the ground. Government intervention in the adult education market can be justified by the existence of market failures leading to suboptimal private investment into adult education. Private under-investment in adult education could be caused for example by liquidity constraints or imperfect information about the private returns to adult education. If such market imperfections matter, bringing individuals into adult education that otherwise would have not participated should materialize in terms of individual labor market success.

This paper investigates potential labor market effects of lowering the private costs for adult education through the provision of vouchers for fee-based adult education courses. In particular, we evaluate a large-scale randomized field experiment with vouchers for adult education. We exploit the exogenous variation generated by the random assignment of the voucher for the identification of causal effects. The overall voucher effect crucially depends on the effect of voucher-induced adult education on labor market success. This effect might differ from OLS-based estimates of the effects of adult education, because OLS estimates are biased if selection into adult education is driven by non-ignorable unobserved characteristics or because individuals who comply with the voucher assignment have non-average returns to adult education. We study this by comparing OLS estimates of the returns to adult education with IV estimates using voucher receipt as an instrument. Moreover, we investigate potential heterogeneous effects of adult education and voucher receipt on labor market success for selected sub-groups of the population. We characterize the complier population in terms of these selected characteristics. This characterization is particularly interesting from a policy perspective as it describes the population actually affected by a voucher policy.

We find no statistically significant average effects of voucher-induced adult education on earnings and employment probabilities in the short-run. While we cannot statistically reject the existence of small positive effects, our IV point-estimates suggest substantially lower returns to voucher-induced adult education as compared to effect sizes suggested by OLS estimates. Analyzing the potential effect of the voucher on subsequent adult education, we can exclude the possibility that the large persistence in adult education implied by simple OLS estimations is causal. Small positive effects of voucher-induced adult education on continuing human capital investments cannot be excluded and are in fact suggested by point-estimates of the IV estimation.

The results highlight the importance of the selection problem when estimating returns to adult education. As shown previously in the literature, the incidence of adult education and the returns to adult education vary significantly between sub-populations (See Vignoles et al., 2004; Bassanini et al., 2005). Our estimates of the returns to voucher-induced adult education are likely to identify a very particular local average treatment effect (LATE). By comparing first-stage coefficients for specific subgroups to the overall first-stage coefficient, we can characterize the population of compliers. The results indicate that individuals with higher levels of education are more likely to comply with the voucher assignment, while individuals with low levels of education are less likely to comply. A subgroup analysis of labor effects suggests, however, that for example individuals with vocational training have a high potential to benefit from participation in adult education. Thus, an adult education voucher might entail significant labor market effects when targeted at specific subgroups. Our findings cast doubts, however, on the effectiveness of unrestricted voucher programs to promote adult education.

This analysis adds to the literature on designing public policies to promote adult education. Most countries are intervening heavily in the adult education market (See OECD, 2004), but whether this kind of intervention is justified on the basis of scientific evidence is often questionable. Moreover, while the importance of human capital is undisputed, government investment in early education might be more effective and more efficient in every respect (See Cunha and Heckman, 2007; Silles, 2007). Despite these concerns, adult education vouchers are in actual use in some countries. Current education voucher models are provided by the regional chambers of trade in Austria, the Training Cheque in the canton of Geneva (Switzerland), the Training Cheque in Belgium, adult education vouchers in some Italian regions and the North Rhine-Westphalian Training Check.

The remainder of the paper is structured as follows: the following section presents the large-scale randomized field experiment with vouchers for adult education. Section 3 presents the descriptive evidence. Section 4 describes the estimation strategy. Estimation results are presented in Section 5. Section 6 concludes.

## 2 The Experiment

In 2005, the Swiss government mandated a randomized field experiment with vouchers for adult education. The key study objectives were to establish whether the issuance of adult education vouchers would cause a causal increase in uptake of adult education, and if so, among whom and if possible to calculate the deadweight loss of such vouchers. Because

the experiment wanted also to test, whether there is any price sensitivity to the face value of the voucher and if it matters whether vouchers are issued on their own or whether they come with a free offer of advice and support, the experiment issued vouchers with three different face-values (worth 200, 750 and 1200 CHF respectively) and half of the experimental group got an offer for free counseling.

The experiment benefited from the fact that the sample population of the Swiss Labour Force Survey (SLFS) was raised significantly at the beginning of the century prior to being reduced more recently because of financial constraints. The SLFS is structured according to a rotating panel principle in which the respondents are interviewed five years in succession. As a result, about one-fifth of the respondent population is replaced every year. The reduction in the sample population in 2006 yielded the opportunity to select a random sample of 2,437 people for the experiment from the approximately 3,000 individuals who would otherwise have been scheduled to continue participating in the next round of interviews. All these individuals had already been interviewed in 2005, and most of them had also been interviewed in former years, and had been removed from the interview schedule in 2006 because of the sample size reduction. The remaining sample served as the control group.

The experimental group is matched by a control group of about 17,000 individuals who were interviewed by the SLFS as scheduled in 2006 and met the criterion of having been interviewed in 2005 (see also Table 1). The experimental design enables the use of longitudinal data as well as cross-sectional information, which is important with respect to the issue of interest here. For example, it is well known that prior adult education participation is the best predictor for participation in adult education at a given point in time. Although this information could also be obtained on the basis of retrospective surveys, the opportunity to observe the control and experimental group pre-experiment provides a much more reliable method.

With respect to the control and experimental group, age was the only limitation. Only subjects aged 20 to 60 were entitled to receive vouchers. Under-20s would be likely to be still undergoing education or training, and over-60s would be likely to be retired pensioners. There were no limitations as to employment status, as increasing the skills of non-employed individuals may be a goal of the State.

The 2,437 randomly selected individuals received a letter from the Swiss Federal Statistical Office during the first days of January 2006 containing the adult education voucher. The letter stated that the voucher was part of a project to promote life-long learning by the

Federal Office for Professional Education and Training and that participants of the SLFS were particularly well suited to receive this gift. The letter was signed by the General Director of the Swiss Federal Statistical Office to eliminate any doubts as to the legitimacy of the voucher. No public-domain information was generated at any point during the experimental period, to ensure that voucher recipients were unaware that the dispensing of the voucher was part of an observational study.

Recipients were entitled to use the voucher for an adult education module of their choice. There were no restrictions on the content of the adult education module because the intention was to use the choices as a basis for an efficiently designed adult education voucher system later on.

Though problems might have occurred at the beginning and end of the redemption period, the temporal pattern of redemptions showed a significantly lower number of redemptions in the first and last month of the experiment and a fairly stable redemption pattern in the February to June period, peaking in mid-experiment (April). The time pattern for voucher redemptions does not suggest any unintentional special effects. To redeem the voucher, it was necessary to send the voucher with the course organizer's invoice to the Federal Office for Professional Education and Technology, which in turn paid out the amount to participants. The federal agency conducted spot checks to verify that the adult education uptake and invoices were bona-fide. Toward the end of June, the experimental subjects were surveyed for the SLFS as in previous years. To enable later linkage of the latter survey data with the experimental data, the participants were required at the end of the survey to say whether they consented to data linkage. This ensured both that the replies to the SLFS's standard questions were not skewed because of linkage with the adult education voucher, and also ensured that data protection concerns were addressed.

Out of the 2,437 individuals receiving adult education vouchers, 1,888 (77.5%) took part in the SLFS at the end of June 2006. Fortunately, none of the respondents objected to linkage of the SLFS data with the experiment. 449 of the total 2,437 voucher recipients redeemed their vouchers, representing a redemption rate of 18.4 percent.

The evaluation concerns adult education participation in courses that cost money. Like other adult education statistics approaches, the SLFS differentiates between different forms of formal and informal adult education, and the participation rates naturally vary accordingly. The crucial aspect in government-funded adult education is that it is intended to increase opportunity for attending fee-based adult education offerings. Therefore, the statistical module selected from the adult education modules in the SLFS available was

the one in respect of which SLFS participants are subsequently asked whether they had attended fee-based courses within the past 12 months.

### 3 Descriptive Evidence

This section provides descriptive statistics on several background characteristics and outcome variables. In 2007 the experimental and the control group are surveyed in the SLFS for the last time. All individuals of the original sample, who were successfully interviewed in 2006 and 2007 constitute the estimation sample used in this analysis. Selected average characteristics are reported separately for the experimental and the control group and for the years 2005, 2006 and 2007 in Tables 1 to 3.

The reported number of observations show significant panel attrition with respect to the full sample surveyed in 2005 (see Table 1). Only 52 percent of the original control group and 56 percent of the original experimental group are observed in 2007. Selective panel attrition based on unobservables with respect to voucher receipt may have damaged the exogeneity of the voucher instrument. While we cannot formally test whether this is the case, we argue that, unless panel mortality differs substantially between the experimental and the control group, the original random assignment still makes voucher receipt a pretty convincing instrument.

Table 1 shows that the experimental group drawn randomly from the entire SLFS sample differs from the control group in respect of the criteria Swiss, vocational education and university education. The education variables are linked to the nationality variable and the experimental and the control group do not differ statistically anymore, if one controls for nationality. We have, however, no information and the Federal Statistical Office could not provide additional information, why the groups differed in respect to the nationality of the survey respondents. What is important for the experiment is the fact that the randomization was based on observables only and that there was no possibility of survey respondents to influence the assignment to the control or to the experimental group.

Due to differences in response patterns, the control and the experimental group also differ statistically in the 2006 and 2007 sample in respect to the characteristic of having already participated in adult education prior to the experiment (see Tables 2 and 3). As it seems, people active in adult education have also slightly higher response rates in the SLFS and because the participation rate in adult education was higher in the experimental group, the significant difference appear in the 2006 and 2007 samples.

Additional to the educational characteristics that differ already in the full sample, the

2006 sample has also slightly less survey respondents with compulsory education as their highest educational degree. Differences in response patterns between 2006 and 2007 cause the statistical significance of the difference in compulsory and vocational education to disappear, but make the difference in survey respondents with non-academic tertiary degrees between the control and experimental group in 2007 significant. These observable differences caused by panel attrition and the initial over-assignment of vouchers to individuals with Swiss nationality, emphasize the importance of controlling for these characteristics in the empirical analysis.

Nevertheless, we provide mean comparisons between the control and the experimental group for three outcome variables of interest without controlling for any confounding factors in Table 4. The three outcome variables of interest are: gross hourly earnings, employment status and participation in adult education. All variables are measured in 2007.

The first two rows of Table 4 reveal no significant mean differences in gross hourly earnings and employment probabilities in 2007. Figure 1 also provides kernel-density estimates of the distribution of gross hourly earnings for the control and the experimental group measured in 2005, 2006 and 2007. As the reported p-values from two-sample Kolmogorov-Smirnov test of the equality of distributions indicate, we can reject the hypothesis that the wage distributions are different at any point.

Table 4 reports, however, significant differences in the participation in adult education in 2007. Voucher recipients in 2006 appear to be four percentage points more likely to participate in adult education in 2007. This could indicate a positive effect of voucher-induced participation in 2006 on subsequent private investments into adult education. However, Table 3 already indicated significant differences in participation in adult education between the control and the experimental group before the experiment. This observation calls for a more rigorous analysis of the effects of voucher-induced adult education.

## 4 Empirical Strategy

We are interested in the effect of the adult education voucher on future labor market success. The overall effect of the voucher is determined by the pick-up rate and by the effect of adult education on future labor market success. Messer and Wolter (2009) already showed that voucher receipt positively affects immediate participation probabilities in adult education courses. Pick-up rates are, however, relatively small. Whether the adult education voucher positively affects labor market outcomes, thus, crucially depends on the returns to adult education for those individuals who complied with the voucher assignment.



We therefore start by analyzing the effect of self-financed adult education on future labor market success. The following cross-sectional regression model is used to link labor market success to participation in adult education and a set of exogenous covariates, including an indicator for firm-financed adult education:

$$y_{i,t+1} = \alpha'_0 X_{i,t} + \alpha_1 \text{train}_{i,t}^{ff} + \beta \text{train}_{i,t}^{sf} + \epsilon_{i,t}, \quad (1)$$

where  $y_{i,t+1}$  is the outcome of interest measured at  $t + 1$ ,  $X_{i,t}$  is a vector of background characteristics measured at  $t$ ,  $\text{train}_{i,t}^{ff}$  is an indicator for firm-financed adult education in  $t$ ,  $\text{train}_{i,t}^{sf}$  is an indicator for self-financed adult education in  $t$  and  $\epsilon_{i,t}$  is an error term uncorrelated with all right-hand side variables.  $\beta$  is our key parameter of interest. It measures the association between participation in self-financed adult education courses and labor market success in the subsequent period. However,  $\beta$  is likely to be a biased estimate for the effect of interest as returns to adult education are heterogeneous and participation in adult education is selective.

Longitudinal data potentially solves this selectivity bias. We can exploit the panel structure of our data and include past outcomes in equation 1. The simplest way to do so would be to rewrite equation 1 in terms of a value-added model with first-differences in the outcome of interest as dependent variable. An alternative and less restrictive specification would be to include past outcomes on the right-hand side of the equation. In this case, the equation of interest becomes:

$$y_{i,t+1} = \alpha'_0 X_{i,t} + \alpha_1 \text{train}_{i,t}^{ff} + \alpha'_2 X_{i,t-1} + \alpha_3 y_{i,t-1} + \beta \text{train}_{i,t}^{sf} + \epsilon_{i,t}, \quad (2)$$

where  $X_{i,t-1}$  is a vector of labor market controls measured at  $t - 1$  and  $y_{i,t-1}$  is the outcome of interest measured at  $t - 1$ . The inclusion of past outcomes in the estimation should eliminate the confounding effect of time invariant individual characteristics that affect labor market outcomes. Yet, selection into adult education might not only be based on levels of observed or unobserved variables. For instance, if selection into self-financed adult education is driven by trends in the outcome variable, the estimate for  $\beta$  in equation 2 provides biased estimates for the effect of interest.

The exogenous variation in adult education generated by the random assignment of the voucher can be exploited to solve the endogeneity of self-financed adult education. As vouchers are assigned randomly, we can estimate a two-stage least-squares (2SLS) model with vouchers receipt as instrument. In the just-identified model where  $\text{voucher}_{i,t}$  is the

only instrument for the endogenous variable  $train_{i,t}^{sf}$ , the first-stage relating voucher receipt to participation in self-financed adult education is

$$train_{i,t}^{sf} = \pi_0'X_{i,t} + \pi_1 train_{i,t}^{ff} + \pi_2'X_{i,t-1} + \pi_3 y_{i,t-1} + \gamma voucher_{i,t} + \eta_{i,t}, \quad (3)$$

where  $\gamma$  is the first-stage effect of the instrument. The parameter corresponds to the pick-up rate of the voucher. The resulting IV estimate,  $\widehat{\beta}^{IV}$ , is an unbiased estimate for the average effect of self-financed adult education on labor market success for compliers. It identifies the local average treatment effect (LATE) for individuals who participate in adult education when receiving a voucher, but would have not participated if they had not received a voucher (See Imbens and Angrist, 1994).

From a policy perspective, the effect of providing adult education vouchers on labor market outcomes is effectively the parameter of interest. Because of random assignment of the vouchers in the field-experiment, we can obtain an unbiased estimate of this effect by regressing indicators of labor market success on a voucher dummy indicating voucher receipt. The reduced-form regression model is given by:

$$y_{i,t+1} = \lambda_0'X_{i,t} + \lambda_1 train_{i,t}^{ff} + \lambda_2'X_{i,t-1} + \lambda_3 y_{i,t-1} + \delta voucher_{i,t} + \mu_{i,t}. \quad (4)$$

The parameter  $\delta$  is the parameter of interest. It measures the effect of receiving an adult education voucher on the outcome of interest. Not every voucher recipient uses the voucher. The parameter therefore identifies an intention-to-treat (ITT) effect. Algebraically it is linked to the LATE estimate of the effect of self-financed adult education on labor market success estimated by  $\widehat{\beta}^{IV}$ . The estimated ITT effect corresponds to  $\widehat{\beta}^{IV}$  scaled down by the pick-up rate estimated by the first-stage coefficient  $\widehat{\gamma}$ .

## 5 Results

The discussion of the empirical results builds on the insight that returns to adult education are not constant in the population. We start by analyzing average labor market effects in the population. We then continue by investigating heterogeneous effects for sub-populations of interest. Finally, we characterize the sub-population that complies with the voucher assignment based on several background characteristics.

## 5.1 Average Labor Market Effects

We investigate the effects defined in Section 4 based on three different labor market outcomes: hourly earnings, employment probabilities and subsequent probabilities to participate in adult education. All outcomes are measured in 2007. The results should, therefore, be interpreted as estimates for short-run effects.

We start by investigating effects on earnings. Earnings conditional on employment is the most obvious measure of labor market success. Inspired by the seminal work of Mincer (1974), a vast literature on the estimation of causal effects of education on earnings emerged (Card, 1999). Yet, the majority of studies focus on returns to schooling. The existing evidence on the returns to adult education is still thin. In particular, little evidence for the causal effect of self-financed adult education exists.

Table 5 displays estimation results with log hourly gross earnings in 2007 as dependent variable. The first four columns show OLS estimation results based on equation 1 with different sets of covariates. In the first column earnings are regressed on a dummy indicating participation in self-financed adult education in 2006, a dummy indicating participation in firm-financed adult education in 2006, a gender dummy, a dummy for having the Swiss nationality and a linear and quadratic term in age. The results indicate positive associations between, both, self-financed and firm-financed adult education and earnings. The association between earnings and adult education is, however, stronger in the case of firm-financed adult education. This is in line with previous results in the literature suggesting significant positive effects of work-related training on earnings (Barrett and O’Connell, 2001; Booth, 1991). Both adult education coefficients, however, decrease when we additionally control for regional variation, family characteristics and differences in language of the interview (column 2) as well as educational controls (column 3). In particular, the inclusion of indicators for the highest level of educational attainment reduces the coefficient on self-financed adult education significantly. Overall, the results reveal the typically found association between adult education and earnings in the cross-section, but also indicate selective participation in adult education based on observable characteristics.

Column 4 of Table 5 shows estimated coefficients based on equation 2. In this specification, we additionally control for log hourly gross earnings in 2005 and other job characteristics measured in 2005. The results reveal that conditioning on past outcomes further reduces the estimate for the effect of self-financed adult education. Compared to the estimated coefficient in column one (.07) the coefficient decreased by .055 percentage points to .15. This suggests that participation in self-financed adult education increases future

earnings by 1.5 percent.

Does this estimate identify a causal effect? The OLS coefficient provides an unbiased estimate of the effect of adult education on earnings only if the selection-on-observables assumption holds. That is, the control variables included in the model perfectly determine the participation in self-financed adult education. On the one hand, this seems reasonable as the specification estimated in column 4 of Table 5 includes a large set of covariates relevant for the training decision. In particular, the specification also includes past outcomes thus controlling for heterogeneity among individuals typically unobserved in the cross-section. Moreover, our data allows distinguishing self-financed from firm-financed adult education. The results in column 4 show that this distinction matters. The effect of adult education financed by the employer is twice as high as the effect of adult education paid by the individual. This is likely to reflect differences in the selection into these two types of adult education. On the other hand, selection into adult education might still be influenced by other unobserved factors. In particular, participation in self-financed adult education might be partly driven by expected earnings growth (Pischke, 2001). In this case, the estimate would be biased.

The random-assignment of the voucher offers a source of exogenous variation that can be exploited to identify a causal effect of self-financed adult education. Voucher receipt is a valid instrument for self-financed adult education. It satisfies the exogeneity assumption by construction and is also relevant as shown in Messer and Wolter (2009).

The estimated first-stage of the IV approach specified in equation 3 is reported in column 5 of Table 5.<sup>1</sup> The estimated coefficient of a dummy variable indicating voucher receipt identifies the causal effect of the voucher on participation in adult education in 2006. The coefficient of .12 corresponds to the pick-up rate of the voucher. It is lower than the redemption rate, because of the existence of always-takers. The estimate shows that receiving a voucher increases the probability to participate in an adult education course in 2006 by 12 percentage points. This effect is highly significant.

The last row of column 5 reports the Cragg and Donald (1993) minimum eigenvalue statistic as a test of weak instruments. Using weak instruments not only produces estimates with large standard errors, but can also lead to large inconsistencies of the IV estimates (See Bound et al., 1995). The minimum eigenvalue statistic is identical to the F-statistic on excluded instruments because the model contains only one endogenous regressor. Stock

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<sup>1</sup>We model the first-stage as linear despite the binary nature of the outcome variable. Using a linear regression for the first-stage estimates generates consistent second-stage estimates even with a dummy endogenous variable (Angrist and Krueger, 2001).

et al. (2002) suggest that the F-statistic on excluded instruments must be large, typically exceeding 10, for 2SLS inference to be reliable when there is one endogenous regressor. A value of 67.04 clearly exceeds this threshold. However, Monte Carlo simulations show that simply looking at the F-statistic might not be sufficient (See Hall et al., 1996). A more formal test for weak instruments is provided in Stock and Yogo (2005), where critical values for worst-case rejection rates of 5, 10, 20, and 25 percent for nominal 5 percent Wald tests of the endogenous regressors are provided. In our case, the minimum eigenvalue statistic exceeds the critical value at all levels of significance. We can, thus, conclude that voucher receipt is not a weak instrument.

Column 6 of Table 5 shows the corresponding IV estimate of the effect of self-financed adult education on future earnings in the first row. The point-estimate is .006 and insignificant. This estimate is well below the estimate of the OLS specification. However, the standard error of the IV estimate is large indicating that our IV approach lacks precision. In particular, we cannot statistically reject the existence of effect sizes in the magnitude of the OLS coefficients. The lack of precision of the IV estimate results from the fact that we have many always-taker. However, the point-estimate of .004 might still be economically important. It suggests a further reduction of returns to adult education compared to the OLS results. This can be due to two reasons: First, OLS results are upward biased because individuals with favorable earnings growth predominantly select themselves into self-financed adult education and this selection is not perfectly determined by the control variables included in equation 2. Second, considering the LATE interpretation of our IV approach, the effect presented in column 6 might measure the effect for a particular group of individuals with low returns to adult education in terms of future earnings.

Ultimately, we are interested in the causal effect of the voucher on future outcomes. Column 7 of Table 5 shows the estimation based on equation 4. The ITT effect is well identified because of the random assignment of the voucher. The point-estimate is close to zero and insignificant. However, the standard errors are again somewhat large. We can only reject voucher effects larger than a 1.3 percent increase in earnings with 90 percent confidence.

Earnings might not be the only meaningful indicator for labor market success. In a rigid labor market, effects of adult education might materialize more in terms of higher employment probabilities than in terms of increased earnings. Moreover, the earnings results are limited to continuously employed individuals. Participation in adult education might be particularly beneficial for the non-employed (Stenberg and Westerlund, 2008).

Table 6 therefore reports estimation results with an indicator for employment in 2007 as dependent variable.<sup>2</sup> The structure of Table 6 resembles the structure of Table 5 and follows the empirical strategy laid out in Section 4. As we do not condition on continuous employment, all estimations are based on the entire estimation sample. Therefore, we also do not include job characteristics in 2005 as a covariate.

In the first three columns of Table 6 we estimate employment regressions based on Equation 1 with different sets of controls. In column 4 we additionally control for employment status in 2005. The OLS estimates reveal the same picture as in the case of earnings. Self-financed adult education is positively associated with future employment probabilities. However, selection into adult education matters. As we increase the number of controls, the estimated returns to, both self- and firm-financed, adult education decrease. Again, the effects for firm-financed adult education appear to be larger. Results displayed in column 4 suggest that, if the selection-on-observable assumption holds, adult education causes employment probabilities in 2007 to increase by 2 percentage points in the case of self-financed adult education and by 3 percentage points in the case of firm-financed adult education.

Columns 5 and 6 present results of estimating an IV approach with voucher receipt as instrument using 2SLS.<sup>3</sup> As in the first-stage estimation presented in Table 5, which is based on the reduced sample with continuously employed individuals, we find a causal effect of the voucher on self-financed adult education in 2006 of 12 percentage points, which can be seen in column 5 of Table 6. The IV estimate of the effect of self-financed adult education on employment probabilities in 2007 is reported in the first row of column 6 of Table 6. The estimated effect is close to zero and insignificant. The point-estimate is even negative. Standard errors are, however, again large and do not allow to reject the positive OLS estimates. Similar to the interpretation of the earnings results, the decrease in the estimated effect with respect to the OLS coefficients might nevertheless indicate that individuals complying with the voucher assignment do not experience any effect of adult education on future employment probabilities.

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<sup>2</sup>All estimations are based on linear models despite the binary nature of the outcome variable. We regard the OLS results as a linear approximation of the conditional expectation function of interest as suggested in Angrist and Pischke (2009). Moreover, Probit-Estimations produce almost identical results for the effects of interest.

<sup>3</sup>Nonlinear second-stage estimates with continuous or multi-valued regressors require a correctly specified functional form in order to interpret the estimates easily. We, therefore, apply a 2SLS procedure although the dependent variable is binary. Even if the underlying second-stage relationship is nonlinear, 2SLS typically capture an average effect of economic interest analogous to the LATE parameter for dummy endogenous regressors (Angrist and Imbens, 1995).

The estimated ITT effect of voucher receipt on future employment probabilities can be seen in column 7. The point-estimate is  $-.0011$  and not statistically different from zero. The effect is, however, estimated with relatively low precision. The upper 90 percent confidence bound lies at  $.12$ , which implies that we can reject positive effects exceeding a 1.2 percentage point increase in future employment probabilities.

So far the focus is on short-run effects of adult education. More precisely, we investigated the effects of participating in adult education in 2006 on labor market success in 2007. Naturally, human capital investments during adulthood might also entail long-term effects. In particular, long-term effects might exceed the returns in the short-run if participation in adult education is continuous rather than nonrecurring. Clearly, one hope of policy makers is that “adult education begets adult education”. In other words, the voucher might bring individuals into adult education, who will subsequently continue to invest in their human capital. This could be the case, if imperfect information about the returns to adult education leads to a suboptimal private investment in adult education. If individuals learn about the true returns by participating once, subsequent investment decisions might be positively affected by voucher-induced participation in 2006. In this case, long-term effects of voucher-induced adult education might well exceed effects measured in the short-run.

While we have no information on labor market success after 2007, we can analyze the participation in adult education in 2007, which is not directly affected by the voucher, because the voucher was only valid in 2006. Hence, any effect of the voucher on future adult education probabilities must be caused by voucher-induced adult education in 2006.

Table 7 reveals that participation in adult education is indeed highly persistent. Estimates in column show that 40 percent of the individuals, who participated in adult education in 2006, continue to participate in 2007 (after controlling for gender, age and nationality). Adding further covariates to the model slightly reduces the coefficient to  $.36$  (column 3).

Part of the observed correlation between adult education in 2006 and 2007 might simply reflect habit formation or might be caused by adult education modules lasting for more than one year. To control for such confounding factors, we add participation in adult education in 2005 to the model. The results presented in column 4 of Table 7 show that the coefficient on adult education in 2006 is further reduced to  $.29$ . The effect remains, however, large and highly significant.

Does this imply that we can indeed expect voucher-induced adult education to lead to

subsequent private human capital investments? To answer this question we instrument self-financed adult education in 2006 by voucher receipt. Column 6 of Table 7 shows the results of this IV estimation. The estimated effect of adult education in 2006 on adult education in 2007 is .13 and insignificant. The standard errors are, however, again large. That means that the effect, albeit not statistically significant, might still be economically significant. The point-estimate suggests that the probability of subsequent private investment in adult education is raised by 13 percentage points through voucher-induced adult education in 2006. This is, however, well below the estimated association between adult education in 2006 and subsequent adult education in columns one to four. Despite the low precision of the IV estimate, we can even statistically reject the large effects found in the OLS estimations.

The corresponding reduced-form estimate of the voucher in 2006 on the adult education probabilities in 2007 is reported in column 7 of Table 7. The effect is insignificant with a point-estimate of .016, which corresponds to the IV estimate multiplied by the first-stage coefficient. Interpreting these results carefully allows the following conclusion: While small positive effects of the voucher on subsequent private investments in adult education can not be statistically rejected, the effects are certainly not as high as suggested by estimated effects based on models controlling for large sets of background characteristics.

Summing up, the results suggest a more pessimistic view on the effectiveness of adult education vouchers. OLS estimates indicate highly heterogeneous effects of adult education on labor market success in the population and highly selective participation in adult education. The IV estimates foster the concern that returns to voucher-induced adult education are close to zero in the short-run. Voucher-induced adult education might entail small positive effects on subsequent private investments in human capital, but these effects are not of the magnitude suggested by cross-sectional OLS estimates.

## 5.2 Heterogeneous Effects

The variation in the estimates of the effect of adult education on labor market success based on models with different sets of controls points to the existence of very heterogeneous effect of adult education in the population. We begin the analysis of heterogeneous effects by investigating associations between adult education and labor market success for selected subsets of the entire population as suggested by OLS models. We additionally analyze potential variation in the estimated ITT effects for these different sub-populations.

Table 8 reports estimated associations between our three labor market outcomes of



interest and participation in adult education in 2006. The estimates are based on estimating equation 2 by OLS. The results reveal that OLS based estimates for the effects of adult education on future earnings and employment probabilities are stronger for women than for men. Moreover, returns to adult education in terms of future earnings are higher for individuals with low education than for individuals with high levels of educational attainments. The result is in line with theoretical models assuming decreasing private returns to human capital investments.

Table 9 reports estimated ITT effects by subgroups. The first column of Table 9 visualizes differences in first-stage coefficients. Pick-up rates are higher for women than for men. The first-stage also appears to be increasing in the highest level of educational attainment. Estimated returns to voucher receipt in terms of earnings reveal a substantial variation between educational groups. While the point-estimate is negative for individuals holding a non-academic tertiary or a university degree, the estimated returns for individuals with vocational training is positive and even significant at the 10 percent level.

In sum, the sub-sample analysis suggests that significant effect heterogeneity is hidden behind the average effects analyzed in subsection 5.1. In particular, individuals with tertiary degrees are associated with below-average returns to adult education, but above-average pick-up rates. On the other hand, individuals with vocational training as highest educational degree appear to benefit highly from participation in adult education.

### 5.3 Characterizing Compliers

From a policy perspective the population of interest is the group of individuals who change their participation decision in response to the adult education vouchers. In the LATE framework these individuals are called compliers. It is a known result that compliers cannot be individually identified. However, albeit we cannot name or list compliers, we can describe the distribution of characteristics for compliers. This characterization is important for policy makers as it shows who will be ultimately affected by adult education vouchers. It is important to note that this characterization might differ from the descriptive statistics presented in Section 3 as the groups-averages presented there refer to the entire experimental group, which includes also the always-takers. The latter population is, however, not interesting from a policy perspective.

To characterize compliers based on the subset of Bernoulli-distributed characteristics, everything we need to know can be learned from variation in the first stage across covariate groups as shown in Angrist and Pischke (2009). Let  $x_{1i}$  be a characteristic analyzed in the

previous subsection as for example a dummy indicating university graduates. The relative likelihood of a voucher-complier to be a university graduate is given by the following calculation:

$$\frac{P[x_{1i} = 1 | D_{1i} > D_{0i}]}{P[x_{1i} = 1]} = \frac{P[D_{1i} > D_{0i} | x_{1i} = 1]}{P[D_{1i} > D_{0i}]} = \frac{E[D_i | z_i = 1, x_{1i} = 1] - E[D_i | z_i = 0, x_{1i} = 1]}{E[D_i | z_i = 1] - E[D_i | z_i = 0]}$$

where  $z_i = 1$  indicates voucher receipt,  $D_{1i}$  denotes participation in adult education in case of voucher receipt and  $D_{0i}$  denotes the counterfactual outcome of participation in adult education in case individual  $i$  do not receive a voucher. As pointed out in Angrist and Pischke (2009), this equation simply says that the relative likelihood a complier has  $x_{1i} = 1$  is given by the ratio of the first stage for individuals with  $x_{1i} = 1$  to the overall first stage.

Average characteristics for compliers are reported in Table 10. Column three shows the relative likelihood compliers have the characteristic indicated in each row. It becomes apparent that women are more likely to comply with the voucher assignment than men. Moreover, individuals with higher educational degrees are also more likely to participate in adult education in response to receiving a voucher.

These findings- together with the evidence on heterogeneous effects - might explain the low estimates of average labor market returns of voucher-induced adult education as it points to a highly selective use of the voucher. On the one hand, individuals with higher levels of education are more likely to comply with the voucher assignment. Highly educated individuals are typically well informed about the returns to education and potentially have easier access to firm-financed adult education. Thus, highly educated compliers with the voucher assignment might be a highly selected subpopulation of all individuals with tertiary education that face low labor market returns to adult education. Moreover, if private returns to human capital investments are decreasing in the level of human capital, individuals with tertiary education might more generally face low labor market returns to adult education. Highly educated individuals might, however, draw direct utility from adult education, which does not necessarily materialize in terms of labor market outcomes. This could explain the rather high pick-up rates among individuals with tertiary education. On the hand, individuals with less schooling are also less likely to comply with the voucher assignment. The evidence on heterogeneous effects suggests, however, that individuals with lower levels of educational attainment gain the most from adult education. In particular, individuals with vocational training appear to largely benefit from voucher-induced adult education, but this group is unrepresented in the group of compliers.

## 6 Conclusion

In this paper we evaluated the effects of a large-scale randomized field experiment with vouchers for adult education. We find no significant average labor market effects of the voucher. In particular, we find no significant average labor market effects of voucher-induced participation in adult education in 2006 on future labor market success based on an IV approach with voucher receipt as an instrument. Unfortunately, the IV estimates lack precision, so that we cannot reject the existence of small effects. However, all IV point-estimates are close to zero and well below corresponding OLS estimates for the effects of self-financed adult education.

Investigating potential effects of the voucher on subsequent adult education, we can exclude the possibility that the large persistence in adult education implied by simple OLS estimations is causal. Small positive effects of voucher-induced adult education on subsequent human capital investments cannot be excluded and are in fact suggested by point-estimates of the IV estimation.

Estimated OLS effects of self-financed adult education decrease significantly, when adding more covariates to the model, which indicates a highly selective participation in adult education and also highly heterogeneous returns to adult education. A subgroup analysis reveals that especially individuals with low levels of educational attainment might benefit from participating in adult education. A characterization of the complier population shows, however, that predominately individuals with higher levels of educational attainment are affected by the adult education vouchers.

These findings are relevant for guiding policies that try to promote adult education by voucher programs. The results cast doubts on the effectiveness of unrestricted voucher programs to promote adult education. In particular, the potential of voucher programs to generate large positive average effects on labor market outcomes is questionable. The characterization of the group of compliers is extremely policy-relevant as it describes the population of interest from a policy perspective. Only these individuals will change their participation decision in adult education in response to a voucher program. In the case of an unrestricted voucher program, the voucher will mainly affect the behavior of highly educated individuals. This population might, however, not be the primary target for government interventions in the adult education market. Our results suggest that individuals with lower levels of educational attainment might benefit more from voucher-induced adult education. A specifically designed and restricted voucher program might, therefore, potentially generate larger average effects on subsequent labor market success.

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Table 1: Comparison of means between the control and experimental group for selected socio-demographic characteristics (full sample), year 2005

Variable	Control group	Experimental group	Difference
Continuous education in 2005	.427	.448	-.021
Woman	.542	.555	-.013
Swiss	.837	.877	-.040***
Age	41.6	41.3	.258
Single parent	.063	.068	-.004
Children of the age < 5	.162	.155	.007
Compulsory education	.156	.143	.012
Vocational education	.470	.504	-.034***
University entrance diploma	.092	.089	.003
Non-academic tertiary	.150	.162	-.011
University	.132	.102	.030***
Not gainfully employed	.171	.167	.005
Number of observations	17,234	2,437	

\* p<0.10, \*\* p<0.05, \*\*\* p<0.001

*Note:* Age is measured in 2005. Based on all observations in 2005.

Table 2: Comparison of means between the control and experimental group for selected socio-demographic characteristics, year 2006

Variable	Control group	Experimental group	Difference
Continuous education in 2005	.435	.465	-.029**
Woman	.550	.559	-.008
Swiss	.856	.898	-.042***
Age	43.0	42.7	.307
Single parent	.065	.062	.003
Children of the age < 5	.159	.154	.005
Compulsory education	.146	.124	.022**
Vocational education	.469	.494	-.025**
University entrance diploma	.086	.097	-.011
Non-academic tertiary	.163	.177	-.014
University	.135	.108	.028***
Not gainfully employed	.170	.164	.006
Number of observations	14'193	1'888	

\* p<0.10, \*\* p<0.05, \*\*\* p<0.001

*Note:* Age is measured in 2006. Based on all observations in 2006.

Table 3: Comparison of means between the control and experimental group for selected socio-demographic characteristics (estimation sample), year 2007

Variable	Control group	Experimental group	Difference
Continuous education in 2005	.438	.462	-.0239 *
Woman	.56	.559	.000573
Swiss	.863	.913	-.0495 ***
Age	44.2	44.1	.046
Married	.606	.613	-.00707
Children of age < 5	.156	.146	.00915
Compulsory education	.116	.105	.0111
Vocational education	.468	.477	-.00934
University entrance diploma	.0826	.0937	-.0111
Non-academic tertiary	.163	.203	-.0396 ***
University	.147	.116	.0312 ***
Number of observations	9,423	1,441	

\* p<0.10, \*\* p<0.05, \*\*\* p<0.001

*Note:* Age is measured in 2007. Based on 2007 estimation sample.

Table 4: Comparison of means between the control and experimental group for outcome variables of interest (estimation sample), year 2007

Variable	Control group	Experimental group	Difference
Gross hourly earnings	39.6 (16.4 )	39.6 (16.6 )	-.0211
Employment probability	.833 (.373 )	.85 (.357 )	-.0168
Adult education	.425 (.494 )	.456 (.498 )	-.0306 **

\* p<0.10, \*\* p<0.05, \*\*\* p<0.001

*Note:* Based on 2007 estimation sample. Earnings are measured in CHF. Standard deviations in parenthesis.

Table 5: Earnings Estimates

	OLS				IV		Reduced
					1st Stage	2nd Stage	Form
Training 2006	.0705*** (.0098)	.0663*** (.0096)	.0261** (.0088)	.0153** (.0070)		.0056 (.0627)	
Voucher					.1191*** (.0145)		.0005 (.0075)
Firm Training	.126*** (.010)	.115*** (.010)	.087*** (.009)	.030*** (.007)	-.240*** (.013)	.028* (.017)	.027*** (.007)
Wage 2005				.538*** (.011)	.062** (.021)	.538*** (.011)	.538*** (.011)
Woman	-.160*** (.007)	-.167*** (.007)	-.132*** (.007)	-.045*** (.007)	.067*** (.013)	-.044*** (.008)	-.043*** (.007)
Swiss	.092*** (.011)	.078*** (.012)	.056*** (.011)	.023** (.008)	.031* (.016)	.024** (.009)	.024** (.008)
Age	.045*** (.003)	.047*** (.003)	.040*** (.003)	.017*** (.002)	-.004 (.004)	.017*** (.002)	.017*** (.002)
Age <sup>2</sup> /10 <sup>3</sup>	-.461*** (.035)	-.481*** (.036)	-.395*** (.033)	-.173*** (.026)	.017 (.051)	-.173*** (.026)	-.170*** (.026)
Education	No	No	Yes	Yes	Yes	Yes	Yes
Region	No	Yes	Yes	Yes	Yes	Yes	Yes
Language	No	Yes	Yes	Yes	Yes	Yes	Yes
Family	No	Yes	Yes	Yes	Yes	Yes	Yes
Job Char. 2005	No	No	No	Yes	Yes	Yes	Yes
Number of obs	5494	5494	5494	5494	5494	5494	5494
R-sq	.167	.217	.350	.608	.149	.608	.608
MES	-	-	-	-	67.04	-	-

\* p&lt;0.10, \*\* p&lt;0.05, \*\*\* p&lt;0.001

*Note:* Dependent variable: log hourly gross earnings in 2007. Standard errors in parentheses. Education controls include indicators for non-academic tertiary degree, vocational training, university entrance diploma, compulsory education and university degree. Regional controls include dummies for state of residence. Family controls include household size and indicators for being married, being divorced and the number of kids under 5. Job characteristics in 2005 include tenure as well as dummy variables for self-employment, part-time work, having a side-job, 3 firm size categories and industry based on 12 NOGA categories.



Table 6: Employment Estimates

	OLS				IV		Reduced
					1st Stage	2nd Stage	Form
Training 2006	.0810*** (.0090)	.0652*** (.0090)	.0505*** (.0091)	.0199** (.0072)		-.0109 (.0654)	
Voucher					.1243*** (.0111)		-.0014 (.0081)
Firm Training	.148*** (.010)	.135*** (.010)	.122*** (.010)	.030*** (.008)	-.262*** (.011)	.022 (.019)	.025** (.008)
Employed 2005				.594*** (.008)	.057*** (.010)	.596*** (.009)	.595*** (.008)
Woman	-.142*** (.007)	-.141*** (.007)	-.124*** (.007)	-.036*** (.006)	.107*** (.008)	-.033*** (.009)	-.034*** (.006)
Swiss	.051*** (.010)	.013 (.011)	-.001 (.011)	-.005 (.009)	.051*** (.012)	-.004 (.010)	-.004 (.009)
Age	.032*** (.003)	.038*** (.003)	.035*** (.003)	.015*** (.002)	.007** (.003)	.015*** (.002)	.015*** (.002)
Age <sup>2</sup> /10 <sup>3</sup>	-.404*** (.032)	-.489*** (.033)	-.456*** (.033)	-.215*** (.027)	-.098** (.036)	-.218*** (.027)	-.217*** (.027)
Education	No	No	Yes	Yes	Yes	Yes	Yes
Region	No	Yes	Yes	Yes	Yes	Yes	Yes
Language	No	Yes	Yes	Yes	Yes	Yes	Yes
Family	No	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs	10303	10303	10303	10303	10303	10303	10303
R-sq	.088	.114	.125	.448	.124	.447	.447
MES	-	-	-	-	125.76	-	-

\* p&lt;0.10, \*\* p&lt;0.05, \*\*\* p&lt;0.001

*Note:* Dependent variable: employment dummy in 2007. Standard errors in parentheses. Education controls include indicators for non-academic tertiary degree, vocational training, university entrance diploma, compulsory education and university degree. Regional controls include dummies for state of residence. Family controls include household size and indicators for being married, being divorced and the number of kids under 5.

Table 7: Training Estimates

	OLS				IV		Reduced
					1st Stage	2nd Stage	Form
Training 2006	.400*** (.011)	.385*** (.011)	.356*** (.012)	.286*** (.012)		.130 (.106)	
Voucher					.121*** (.011)		.016 (.013)
Firm Training	.408*** (.013)	.396*** (.013)	.372*** (.013)	.293*** (.013)	-.310*** (.011)	.244*** (.036)	.204*** (.013)
Training 2005				.227*** (.009)	.207*** (.008)	.259*** (.024)	.286*** (.009)
Woman	.001 (.009)	.005 (.009)	.028** (.009)	.023** (.009)	.088*** (.008)	.037** (.013)	.049*** (.009)
Swiss	.099*** (.013)	.067*** (.014)	.049*** (.015)	.039** (.014)	.040*** (.012)	.046** (.015)	.051*** (.015)
Age	.019*** (.004)	.023*** (.004)	.021*** (.004)	.016*** (.004)	.004 (.003)	.016*** (.004)	.017*** (.004)
Age <sup>2</sup> /10 <sup>3</sup>	-.249*** (.041)	-.292*** (.043)	-.264*** (.042)	-.203*** (.041)	-.058 (.035)	-.212*** (.042)	-.220*** (.043)
Education	No	No	Yes	Yes	Yes	Yes	Yes
Region	No	Yes	Yes	Yes	Yes	Yes	Yes
Language	No	Yes	Yes	Yes	Yes	Yes	Yes
Family	No	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs	10298	10298	10298	10298	10298	10298	10298
R-sq	.170	.182	.198	.241	.178	.227	.195
MES	-	-	-	-	126.31	-	-

\* p&lt;0.10, \*\* p&lt;0.05, \*\*\* p&lt;0.001

*Note:* Dependent variable: training dummy in 2007. Standard errors in parentheses. Education controls include indicators for non-academic tertiary degree, vocational training, University entrance diploma, compulsory education and university degree. Regional controls include dummies for state of residence. Family controls include household size and indicators for being married, being divorced and the number of kids under 5.

Table 8: OLS Estimates of Adult Education Effects by Subgroups

	Earnings	Employed	Course 2007
Men	.0119 (.010)	.0150 (.010)	.278*** (.019)
Women	.0188* (.010)	.0211** (.010)	.292*** (.015)
Compulsory education	.0697** (.028)	.0395 (.029)	.237*** (.034)
Vocational training	.0262** (.009)	.0121 (.011)	.323*** (.017)
University entrance diploma	.0278 (.033)	.0215 (.028)	.235*** (.038)
Non-academic tertiary	-.00319 (.015)	.0177 (.012)	.256*** (.027)
University	-.0152 (.021)	.0267* (.016)	.259*** (.031)

\* p<0.10, \*\* p<0.05, \*\*\* p<0.001

*Note:* Results report estimates for the effect of participating in adult education in 2006. The OLS estimates are reported for subgroups of the population defined by the characteristic reported in the first column. The first row indicates the dependent variable. All estimates are based on specifications reported in the 4th columns of Tables 5, 6 and 7.

Table 9: ITT Estimates by Subgroups

	Course 2005	Earnings	Employed	Course 2007
Men	.109*** (.015)	-.00306 (.010)	.00307 (.010)	.0135 (.020)
Women	.158*** (.016)	.00679 (.011)	-.00743 (.012)	.0159 (.017)
Compulsory education	.0717** (.026)	-.0199 (.027)	-.00169 (.028)	.0235 (.033)
Vocational training	.119*** (.016)	.0157* (.009)	-.000360 (.012)	.00540 (.019)
University entrance diploma	.157*** (.040)	.00678 (.032)	-.0147 (.032)	.0136 (.044)
Non-academic tertiary	.202*** (.029)	-.0238 (.018)	-.00266 (.014)	.0140 (.031)
University	.158*** (.036)	-.0199 (.029)	-.00435 (.021)	.0123 (.041)

\* p<0.10, \*\* p<0.05, \*\*\* p<0.001

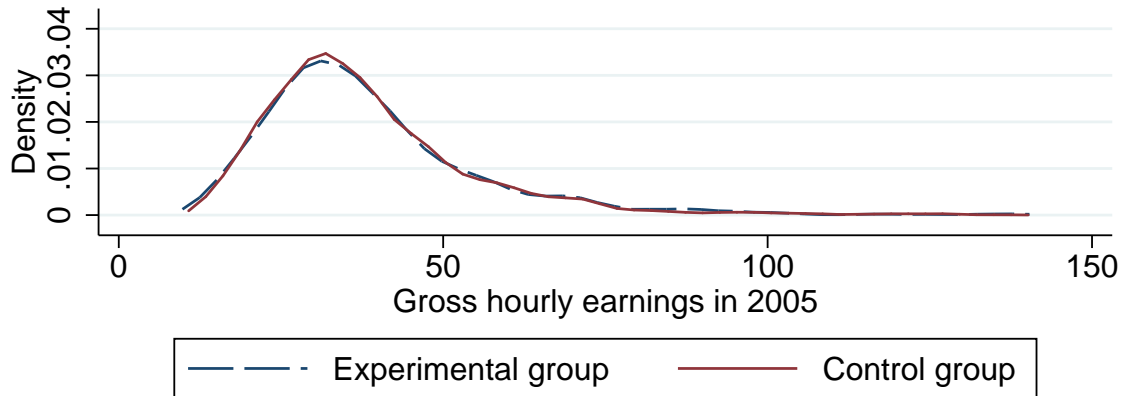
*Note:* ITT estimates for subgroups of the population defined by the characteristic reported in the first column. The first row indicates the dependent variable. The second column reports first-stage results by subgroups based on the first-stage specification used in Table 7. The last three columns report reduced-form estimates by subgroups as specified in the last columns of Tables 5, 6 and 7.

Table 10: Characterizing Compliers

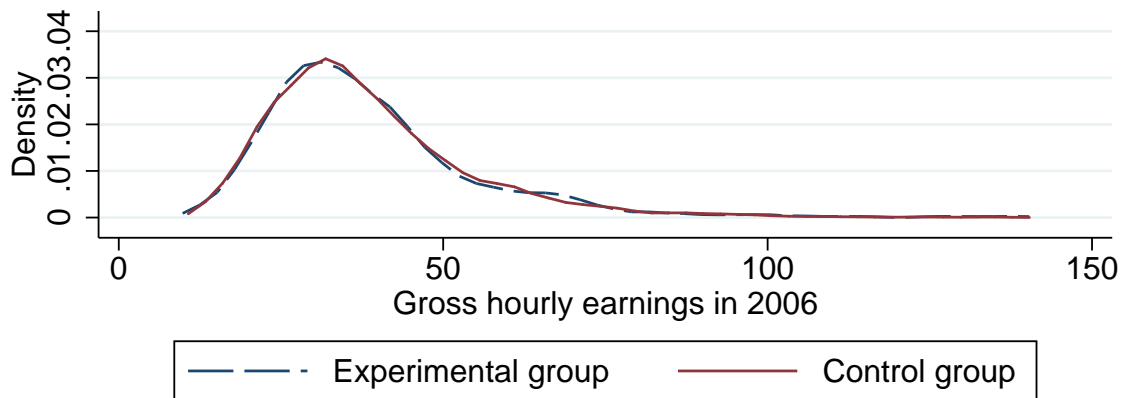
Variable	$P[x_{1i} = 1]$	$P[x_{1i} = 1 D_{1i} > D_{0i}]$	$\frac{P[x_{1i} = 1 D_{1i} > D_{0i}]}{P[x_{1i} = 1]}$
Woman	.56	.64	1.1
Compulsory education	.14	.072	.52
Vocational education	.47	.41	.86
University entrance diploma	.084	.095	1.1
Non-academic tertiary	.16	.24	1.5
University	.14	.16	1.1

*Note:* The table reports an analysis of complier characteristics. The ratio in column 3 gives the relative likelihood compliers have the characteristic indicated in each row.

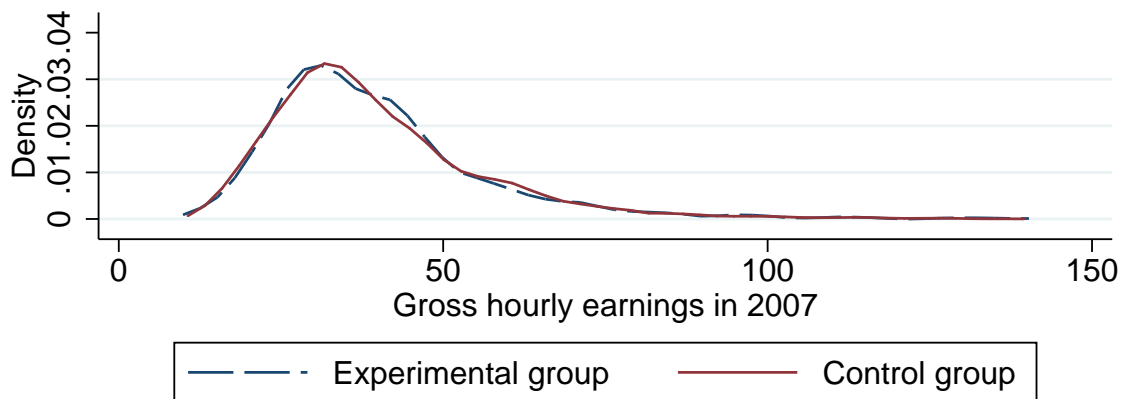
Figure 1: Wage Distributions by Year and Voucher Status



P-value: .94



P-value: .56



P-value: .57

*Note:* Kernel density estimates based on 2007 estimation sample. P-values stem from two-sample Kolmogorov-Smirnov test of the equality of distributions.