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Replacing student grants with loans

Evidence from a Swedish policy reform

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Replacing student grants with loans

Evidence from a Swedish policy reform[a]

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Abstract

The Recruitment grant was a student aid policy that replaced the loans in the student aid system with grants for unemployed adults with low education. In this paper, I estimate the causal effects of repealing this policy on the educational attainment and subsequent labor market outcomes for the target population in a difference-in-differences framework. I find that the repeal reduced enrollment in adult education by 10 percent relative to the pre-repeal enrollment rate, and that the number of passed credits decreased by 29 percent. I also find that repeal increased unemployment by an average of 76 days over the eight-year follow-up, and decreased earnings by about 12,000 SEK ($1,300). Although there are substantial differences in these outcomes across subgroups, the repeal had significant adverse effects for the target population as a whole.

Keywords: Student aid, Adult education, Difference-in-differences, Propensity scores.

JEL-codes: D31, I24, J62, R0.

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1 Introduction

The educational attainment of the Swedish population has increased drastically in the post-war period. Between 1940 and 2016, the number of students enrolled in tertiary education increased from 11,000 to 402,000 (Andrén, 2013; UKÄ, 2018). However, not all students are able to cope with the demands of the modern educational system. For example, only 72 percent of the students that enrolled in upper secondary education in 2012 graduated with a degree within 5 years (Skolverket, 2018). Hence, a significant number of students leave school without a degree, facing limited prospects on the labor market.[1]

To address this problem, Sweden has implemented a comprehensive system of adult education known as Komvux. The purpose of Komvux is to reduce unemployment among the low educated by adjusting the skills of the workforce to the structural changes of the economy, and to bridge the educational gap between younger and older cohorts that has emerged in the wake of the expansion of the educational system. Komvux has been subject to numerous changes since its introduction in 1968, but the primary objectives have remained the same. It is in this context that the Recruitment grant was introduced in 2003 (Prop. 2001/2002:161). It was a student aid reform that replaced the loans in the student aid system with grants for up to one year for unemployed adults with incomplete upper secondary education. The explicit aim of the reform was to increase Komvux enrollment in the target population. The grant amounted to about $1,700 or $3,700 per year depending on whether a student was eligible for supplemental aid or not (Prop. 1999/2000:10), which meant that recipients did not have to accumulate debt while studying. [2] [3]

The grant was abruptly repealed at the end of 2006 (Prop. 2006/2007:17). In this paper, I estimate the causal effect of the repeal on the educational attainment and subsequent labor market outcomes of the target population. The difficulty in identifying the effects is to separate the effect of the repeal from unobserved differences between those who received the grant and those who did not. I address this by exploiting the repeal of the grant in combination with an eligibility requirement in a difference-in-differences (DD) framework.

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[1] This is partly due to the polarized job growth in recent decades, where the demand for middle-skilled jobs that can be automated have decreased relative to low- and high-skilled jobs (Adermon and Gustavsson, 2015).

[2] Dollar values in have been adjusted to 2014 prices.

[3] There are no tuition fees in the Swedish school system for Swedish citizens. The purpose of the student aid system is to cover living expenses to enable everyone to study regardless of their financial situation.

To preview the results, I find that the repeal of the Recruitment grant reduced enrollment in Komvux by 10 percent in the target population relative to the pre-repeal enrollment rate, and that the number of passed credits decreased by 29 percent. I also find that the repeal increased unemployment by an average of 76 days over the eight-year follow-up. However, when it comes to earnings the effect for the target population as a whole is quite small and varies substantially across subgroups. Men and those with low parental education saw their earnings decrease by 28,000 SEK ($3,123) and 13,000 SEK ($1,400) respectively, while women's earnings increased by about 13,000 SEK over the eight-year follow-up period. In sum, the repeal of the Recruitment grant had significant adverse effects on the educational attainment for the target population, and somewhat adverse effects on their labor market outcomes.

2 Previous literature and value added

There is a large empirical literature on the relationship between student aid and educational attainment. Previous studies have generally found that student aid has a positive impact on college completion and reduces dropout and retention (Van der Klaauw, 2002; Dynarski, 2003; Dynarski, 2008; Bettinger, 2004; Goodman, 2008; Angrist et al., 2009; Glocker, 2011). This paper contributes to this literature in two ways.

First, by studying the effect of student aid on a subset of the population that rarely features in the literature - adults with incomplete schooling. Typically, eligibility for student aid selects on academic merit and/or financial need. Hence, the effect of student aid is identified locally at the upper end of the skill distribution, among students who recently graduated from high school. It is not obvious that results from that subset of a population generalize to the rest of the population. This study addresses that limitation in the literature by estimating the effect of student aid for adults between 25–50 years of age located at the lower end of the skill distribution.

Second, almost all the empirical literature on student aid is focused on enrollment decisions at the college level, whereas the Recruitment grant was offered for studies at the compulsory and upper secondary level.[4] Hence, this study will be informative about whether the effects of student aid differ across educational levels.

[4] An exception is Angrist et al. (2002) who study the effect of a voucher lottery in Colombia on students in secondary school.

This study also relates to the behavioral economic literature on financial decisions. Standard economic theory on the effect of student aid on school enrollment is derived from human capital theory, where school enrollment is seen as an investment decision whose soundness depends on the (utility) returns to education and the opportunity cost of enrollment (Becker, 1975). Individuals are assumed to behave rationally in the sense that they are fully informed and form unbiased expectations about outcomes and make decisions that will maximize utility over their lifespan. In the basic model, non-pecuniary costs and returns are ignored so that the enrollment decision only depends on whether the present discounted value of the returns exceed the present discounted value of the costs. In contrast, behavioral economic theory offers several insights as to why standard economic theory might be too simplistic to describe the relationship between student aid and school enrollment.

First, informational asymmetries seem to have an adverse effect on the enrollment decision. Bettinger et al. (2012) found that providing application assistance to low-income individuals increased aid application rates and receipt, as well as college attendance. Second, students may hesitate to take up loans to finance their studies because they exaggerate the risk of default. Such risk averse behavior is explained by prospect theory, which postulates that people tend to overestimate extreme events with a very low probability of occurring (Tversky and Kahneman, 1992). In a study that elicited risk aversion based on a set of survey questions, Hryshko et al. (2011) found that sex and age are strong predictors of risk aversion, with women and older individuals being more risk averse. Third, prospect theory also supports so called framing and labeling effects that lead to risk averse behavior, by suggesting that people make decisions based on a reference point and arrive at different decisions depending on the frame or label of the reference point (Tversky and Kahneman, 1992). For example, Caetano et al. (2011) found that labeling a contract as a "loan" reduced the probability of the contract being accepted by 8 percent compared to a financially equivalent contract.

However, recipients of the Recruitment grant did not have to accumulate debt to finance their studies. They were also actively recruited, informed, and assisted by officials at the municipal level. Hence, this study contributes to the literature on financial decision making by analyzing the impact of a student aid reform that not only had a financial component but also entailed efforts to overcome known obstacles to rational decision making.

The paper proceeds as follows: the next section describes the institutional background of the reform in terms of the Swedish school system, and section 4 presents the data. Estimation and identification is discussed in section 5, and section 6 presents the results. Section 7 concludes.

3 Institutional background

Public education at all levels in Sweden is free of tuition for Swedish citizens. The current school system entails 9 years of compulsory school, comprising the primary and lower secondary level. Compulsory school is followed by 3 years of upper secondary school that upon completion grants admission to tertiary education. However, until 1994 students had the option to enroll in 2-year tracks in upper secondary school that were mostly vocational and did not grant admission to tertiary education (Prop. 1990/1991:85).

The roll out of the current school system was completed in 1972. It was preceded by a school structure with early tracking, in which most people attended 7-8 years of primary school and some attended 1-3 years of secondary school. Very few enrolled in tertiary education. Upper secondary education expanded rapidly in the wake of a comprehensive school reform in 1962, and this expansion generated a significant educational gap between younger and older cohorts. To bridge the gap and meet the increasing demand for educated workers, an educational system for adults called Komvux was introduced in 1968.

Komvux is literally an abbreviation of "municipal adult education" in Swedish, and is precisely that – education for adults, organized by the municipalities but financed by the state. Today, Komvux offers an extensive range of courses at the compulsory and upper secondary level, as well as a few courses at the vocational level. All Swedish residents above 20 years of age may enroll, and anyone that does has a legal right to take a leave of absence from their job and will receive student aid from the state.

3.1 The Swedish student aid system

The current student aid scheme was established by a comprehensive reform in 2001 (Prop. 1999/2000:10). This reform consolidated the different types of student aid that existed at the time into the two-tiered student aid system that is currently in place. In this scheme, all Swedish citizens that study at the tertiary level are eligible for student aid at the first tier, while the second tier is reserved for students that are at least 25 years old and study at the compulsory or upper secondary level.[5] The student aid is comprised of both loans and grants that add

[5] Student aid is awarded for a maximum of 240 weeks at the first tier, and for 80–120 weeks at the second tier depending on prior educational attainment.

up to an amount that is equal across tiers. The difference between the tiers is that the loans in the first tier amount to 65.5 percent of the total student aid, whereas they only amount to 20 percent in the second tier.

Another important feature of the student aid system is that it is tested towards the student's annual income. If the income is above a certain limit, the aid will be reduced by 50 percent of the exceeding amount. In 2006, the limit was about $6,800 per semester (SCB, 2016). There is also an achievement requirement, which stipulates that the student must pass a certain number of credits each semester for the aid to continue.[6] Finally, students can qualify for supplemental aid if their taxable income in the past twelve months exceeded a certain amount. In 2006, the threshold for supplemental aid was about $21,500 (SCB, 2016). The supplemental aid consists entirely of loans and increases the total student aid by 22 percent. For a student studying full-time in 2006, receiving the supplemental student aid would have implied an increase of about $2,000 per year (SCB, 2016).

3.2 The Recruitment grant

The Recruitment grant was introduced in 2003 (Prop. 2001/2002:161) and repealed at the end of 2006 (Prop. 2006/2007:17). The purpose of the Recruitment grant was to encourage unemployed adults with incomplete education to enroll in adult education. In particular, the grant was meant to encourage individuals who in the absence of the reform would not have enrolled in adult education. To achieve that goal, officials at the municipal level – social workers, job counselors, and even librarians – were engaged with recruiting recipients. Typically, a potential recipient would be informed of the grant by a job counselor who would then also assist in the application procedure (Hirasawa and Sundelin, 2006).

The Recruitment grant replaced the loans in the student aid system with grants. It was restricted to studies at the compulsory or upper secondary level of education, and for a maximum of one year. There were three eligibility criteria:

i. Recipients had to be unemployed or at risk of becoming unemployed.
ii. Recipients had to be between 25-50 years of age.
iii. Recipients were not allowed to have received any other form of student aid in the past five years.

Figure 1 shows how the student aid has changed over time. The aid level (vertical axis) applies to full time studies and are reported in thousands of SEK adjusted

[6] The exact number of credits that must be passed varies between educational levels, but typically amounts to 75 percent of what is considered full-time studies.

to 2014 prices. The solid line and the two dashed lines show the grant and loan amounts at the basic and supplemental level respectively. When the Recruitment grant was introduced in 2003 (blue line), the loans in the student aid were replaced by the grant for those eligible for a maximum of 50 weeks at full time studies.

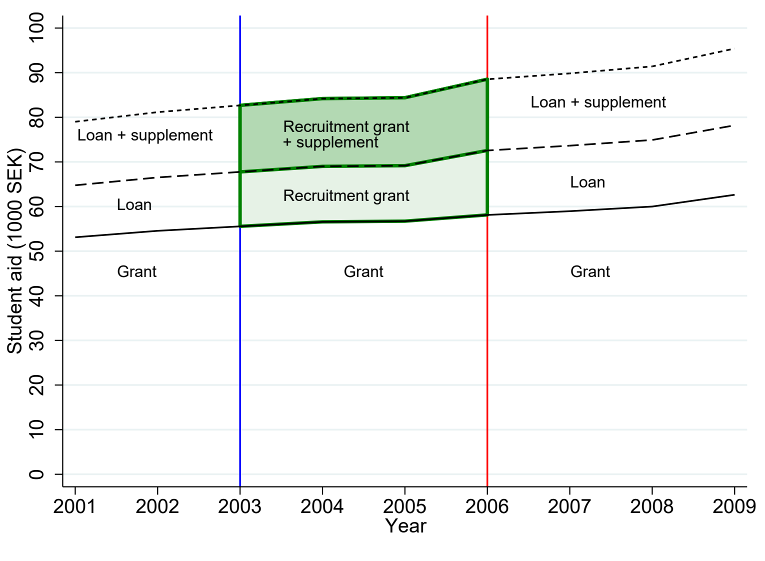

Figure 1. Student aid and the Recruitment grant

Notes. The solid line and the two dashed lines show the grant and loan amounts at the basic and supplemental level respectively. When the Recruitment grant was introduced in 2003 (blue line), the loans in the student aid were replaced by the grant for those eligible for a maximum of 50 weeks at full time studies. The aid level on the vertical axis apply to full time studies and are reported in thousands of SEK adjusted to 2014 price level.

When the Recruitment grant was repealed at the end of 2006 (red line), the grants in the student aid amounted to 58,000 SEK ($7,000) per year, and the loans to 30,500 SEK ($3,500) for those that were eligible for the supplemental aid and to 14,500 SEK ($2,000) for the rest. Hence, the total value of the Recruitment grant was about $2,000 per year for full time students, and $3,500 for those eligible for supplemental aid.

4 Data

I combine several administrative data registers maintained by Statistics Sweden. These include national census data (FoB); the multi-generation register with parent-child links; the longitudinal database about education, income, and employment (LOUISE); the education register; the graduation register for 9:th grade; the pupil register for adult education; and the unemployment register (Händel). In this section, I will elaborate on the sample construction and describe the data.

4.1 Sample selection

I first select the universe of the Swedish population between 21 and 58 years of age from 2003 until 2014, and add the following: parental country of birth, parental educational attainment, year and country of birth, gender, employment, unemployment, grades, educational attainment, Komvux enrollment, passed credits at Komvux, enrollment in tertiary education, labor market income, and student aid. From that dataset, I select all individuals between 25–50 years of age with less than 12 years of education who were unemployed at least one day, separately for 2006 and 2007, to create my main analysis sample.[7]

I then adjust the sample to address two potential problems with the sample design. First, a person is more likely to enroll in Komvux in a given year if they were enrolled in the preceding year. To ensure that this does not confound the results, I exclude individuals in 2007 who received student aid in 2006, and individuals in 2006 who received student aid in 2005. Second, notice that it is possible for an individual to have received the grant at some point between 2003 and 2005 and still enter the sample in 2006 or 2007. I therefore remove all observations that were eligible for the Recruitment grant and received student aid between 2003 and 2005. This reduces the sample by about 1 percent, leaving a final sample of 492,590 observations. [8]

[7] The extended age and time span in the first step is necessary for the study design and enables me to study outcomes in the long run.

[8] The sample design also implies that individuals can appear in the data in both 2006 and 2007. For example, suppose that an individual is 25 years old, unemployed and eligible for the Recruitment Grant in 2006. If this individual remains unemployed in 2007 and does not enroll in adult education in 2006, she will appear in the data both in 2006 and 2007. I do not exploit this longitudinal aspect of the data, and instead treat all observations as if from a repeated cross-section. Serial correlation due to repeated observations of the same individual is handled by clustering the standard errors at the individual level.

4.2 Variable definitions and descriptive statistics

Educational attainment is reported in levels in the registers but has been converted into years of education.[9] Grade point averages are observed at the 9th grade (the final year of compulsory school) and has been percentile ranked within year of graduation to address a discontinuity in the grade system (SFS 1994:1194) as well as grade inflation over time (see Wikström and Wikström, 2005; Vlachos, 2010).

Information on unemployment spells is gathered from the unemployment register, which contains entry and exit dates. I define unemployment on the intensive margin as the number of days in unemployment, and on the extensive margin as a dummy variable equal to 1 if the individual was registered as unemployed at any point during the year. In contrast, information on employment is gathered from LOUISE and only measured at the extensive margin as a dummy variable equal to 1 if the individual was employed at least one hour a week in the month of November (SCB, 2016). Data on enrollment and passed credits in adult education comes from the student registry for Komvux. A course at Komvux has the same number of credits as an equivalent course in the rest of the public school system, and one week of full-time studies corresponds to 20 credits. Finally, labor market incomes have been adjusted to 2014 prices and are based on pre-tax observations of wage earnings, business income, taxable benefits, sick pay and certain parental benefits. Unfortunately, the income data does not include pensions, capital income or income from parental leave.

Descriptive statistics are presented in Table 1 where the sample is compared to individuals aged 25–50 in 2006 and 2007 in the population. As expected, the average educational attainment in the sample is much lower than in the population, as is the grade rank average. Looking at previous levels of unemployment, the difference is striking. The individuals in the sample have an average of 527 days of unemployment in the past three years compared to 132 for the population, underscoring the weak labor market attachment of the individuals targeted by the Recruitment grant. In terms of ethnic background, only 70 percent of the sample were born in Sweden compared to 83 percent in

[9] The conversion into years of education is done as follows: old compulsory school = 7 years; current compulsory school = 9 years; short secondary school = 10 years; old vocational secondary school = 11 years; old theoretical/new secondary school = 12 years; vocational tertiary education = 13 years; short tertiary education = 14 years; baccalaureate degree = 15 years; degree of master of one year = 16 years; degree of master of two years = 17 years; Ph. Licentiate = 20 years; Ph. Doctorate = 22 years.

the population implying a rather large overrepresentation of immigrants in the sample.

Table 1 Descriptive statistics of the sample compared to the population

	(1) Sample		(2) Population	
	Mean	Sd	Mean	Sd
Age	38.4	7.15	37.8	7.32
Woman	0.48	0.50	0.49	0.50
Born in Sweden	0.70	0.46	0.83	0.37
Years of education	9.90	1.24	12.4	2.39
Mother's years of education	9.46	2.37	10.6	2.95
Father's years of education	9.30	2.50	10.5	3.18
Previous unemployment	527.3	400.9	132.2	269.9
Grade rank	19.7	19.2	49.6	28.8
Komvux	0.09	0.29	0.04	0.20
Compulsory level	0.24	0.43	0.15	0.35
Upper secondary level	0.75	0.43	0.84	0.37
Supplemental level	0.009	0.093	0.015	0.120
Observations	492,590		6,174,388	

Note. Column (1) reports descriptive statistics in the sample, and column (2) reports descriptive statistics in the population. The grades are observed at the 9th grade (the final year of compulsory school) and have been percentile ranked within graduation year. The course levels are reported conditional on attending Komvux. Previous unemployment is measured as the number of days of unemployment in the past 3 years. Means and standard deviations.

Focusing on Komvux enrollment, about 9 percent of the sample enrolled in 2006 or 2007 whereas 4 percent of the population did. In terms of study levels, Komvux students in the sample studied more frequently at the compulsory level as opposed to the upper secondary level compared to the population.

Figure 2 plots the number of students enrolled in Komvux from 2003 until 2009. The solid line plots the total number of enrolled students, while the dashed and dotted lines plot the number of enrolled women and men respectively. Throughout the period, women make up about two-thirds of the enrolled students. Focusing on the general trend, enrollment was quite constant until the repeal of the Recruitment grant at the end of 2006 when a rather steep decline ensues that continues until 2008. Thus, Komvux enrollment has been decreasing over the period, which underscores the importance of controlling for this trend when analyzing the impact of the reform.

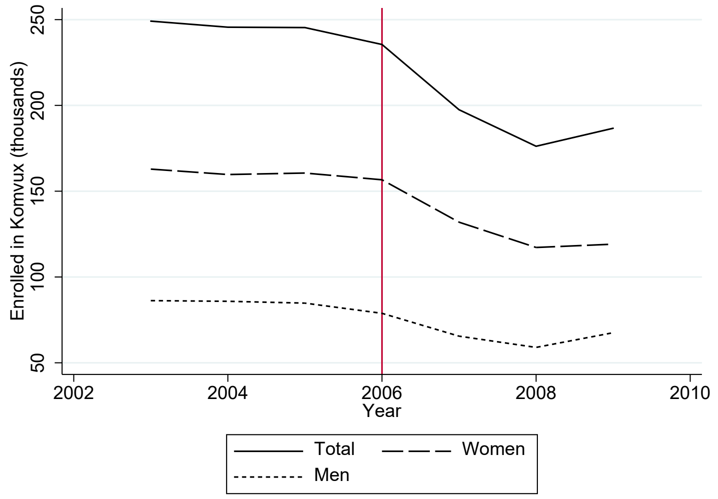

Figure 2. Enrolled in Komvux

Note. The y-axis shows the number of students (thousands) enrolled in Komvux. The dotted line plots men and the dashed line plots women, and the solid line plots the total.

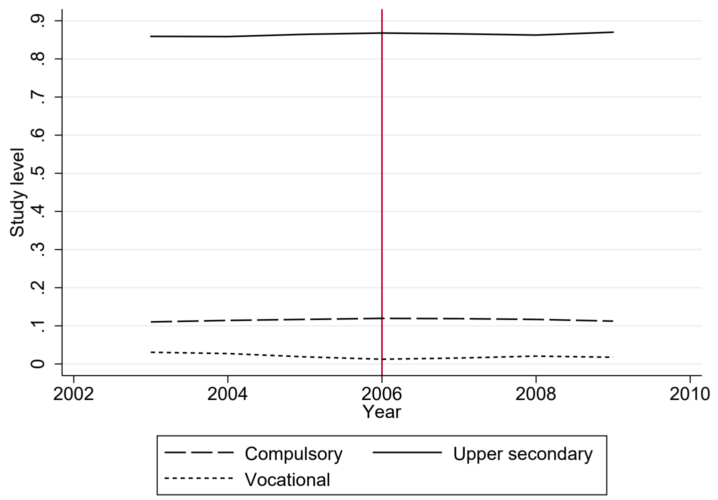

Figure 3. Study levels at Komvux

Note. The y-axis shows the share of students enrolled at different levels in Komvux. The solid line plots the upper secondary level, the dashed line plots the compulsory level, and the dotted line plots the vocational level.

Figure 3 plots the enrollment shares at different levels. The solid line plots the share enrolled at the upper secondary level, which is about 86 percent throughout the period. The dashed line plots the share enrolled at the compulsory level and the dotted line plots the share enrolled at the vocational level. As we can see, the repeal of the Recruitment grant does not seem to have affected the relative demand for courses at different levels.

Figure 4 plots the average number of semesters students enroll in at Komvux, conditional on enrolling at least one semester (six months). The solid line plots the total number of semesters and varies between 4.8 and 5.3 over the period. That may seem like a lot, but the dashed line that plots the number of semesters with passed credits is roughly constant at 2.5 semesters.

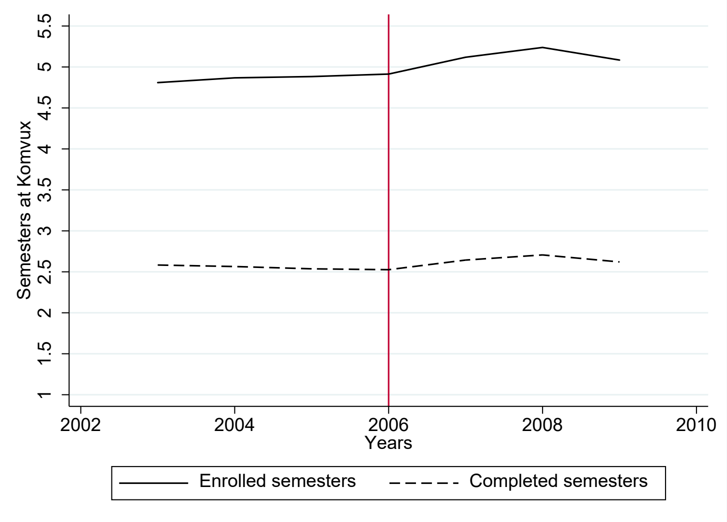

Figure 4. Semesters at Komvux

Note. The y-axis shows the average number of semesters a student stays at Komvux conditional on enrollment. The solid line plots the average including all semesters, and the dashed line plots the average number of semesters with passed credits.

This means that a large number of students enroll without actually attending their courses. Hence, there seems to be a substantial amount of uncertainty associated with the enrollment decision. To the extent that those who attend Komvux due to receiving the grant otherwise would have worked, this also suggests that a potential positive effect of the repeal on employment and earnings due to substitution from education to work might last up to two years if those who attended Komvux re-entered the labor market at that point.

5 Estimation

Student aid typically selects on factors that are related to the future academic achievement of the recipient, making it difficult to evaluate. Nation-wide schemes often select on socioeconomic factors that are negatively related to future academic achievement, such as having an economically disadvantaged family background (see Seftor and Turner, 2002; Kane, 2003; Bettinger, 2004; Alon, 2007; Glocker, 2011). On the other hand, grants and scholarships at the regional and school level often select on academic merit, which of course is positively related to future academic achievement (see Dynarski, 2000; Van der Klaauw, 2002; Dynarski, 2008; Goodman, 2008; Scott-Clayton, 2011). In both of these cases, a simple correlation between student aid and educational achievement will confound the effect of the student aid with the effect of the eligibility requirements.

In this study, I address this by estimating the effect of repealing the Recruitment grant in a difference-in-differences (DD) framework that eliminates the confounding effects of time-invariant differences between the treatment and control group, as well as time-varying factors that are group invariant. I assign treatment status based on the student aid margin for eligibility. Specifically, I assign as treated those who have not received student aid at any point in the past 5 years and thus are eligible for the Recruitment grant. Conversely, I assign those who have received student aid in the past 5 years to the control group. Before I elaborate on the estimation model and its identifying assumptions, I will briefly discuss the interpretation of the estimand identified by the model when the treatment and controls are defined this way.

5.1 The estimand

In an experimental setting, individuals who do not comply with their assigned treatment status are called non-compliers.[10] There are several ways to estimate a treatment effect when non-compliers are present. If actual treatment is observed, one way to do it is to estimate the treatment effect by comparing those who actually were treated with those that were not.[11] However, this estimate will be

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[10] In this particular study, non-compliers can be thought of as those individuals who are offered the Recruitment Grant but do not take it.

[11] The average treatment effect for the compliers is called the local average treatment effect (LATE) because it estimates the treatment effect “locally” at the margin that separates the treatment group from the control group. A LATE thusly obtained is an instrumental variable estimate where the compliance rate is the first stage, and the reduced form effect is equal to the ITTE (Angrist and Pischke, 2008).

biased if those that did not comply with their assigned treatment status are systematically different from those that did along dimensions that are correlated with the outcome.

If actual treatment is not observed, as in this case, one can proceed to estimate the treatment effect by comparing those *assigned* to the treatment group with those assigned to the control group. This amounts to estimating the so called the intention-to-treat-effect (ITTE), which is what is done in this study. Notice that it does not estimate the average treatment effect (ATE) but rather the effect of *offering* treatment (Imbens and Angrist, 1994; Angrist and Pischke, 2008).[12]

Hence, the correct interpretation of the estimand obtained by the DD framework in this study is *the effect of discontinuing to offer the Recruitment grant* on some outcome for the target population.

5.2 The difference-in-differences model

I estimate the DD model using OLS regression with standard errors clustered at the individual level. The estimation model is defined as:

$$Y_{iat} = \alpha + \varphi_a + \tau_t + \delta D_{at} + \varepsilon_{iat}$$

Where Y_{iat} denotes an outcome for observation i in group a observed in year t, φ_a is a time-invariant group effect, τ_t is a year fixed-effect that is constant across eligibility status, and D_{at} is a dummy for treated observations in the period after the repeal. The parameter of interest is δ, which estimates the difference in outcome Y_{iat} for the treatment and control groups before and after the repeal. By differencing twice – first between the treatment and control group and then before and after the repeal – the model controls for unobserved differences between the groups that are time-invariant, as well as time-varying differences that are group invariant. Hence the difference-in-differences epithet.

Since one might expect that the repeal had long run effects on the labor market outcomes for the target population, I also analyze the effect on outcomes observed up to seven years after the initial year of observation. To see how this works, consider the following model:

[12] When the non-compliance only occurs in the treatment group, it is possible to estimate the ATE for the compliers if one is willing to assume that the outcome of the non-compliers is the same regardless of their assigned treatment status. In the context of this study, this assumption amounts to assuming that those who were eligible for the Recruitment grant in 2006 but didn't take it, did so for reasons that are unrelated to the fact that it's been at least five years since they last received student aid. Since I have no way of knowing if that is indeed the case, I focus on estimating the ITTE rather than the ATE.

$$Y_{iat+p} = \alpha + \varphi_a + \tau_t + \delta D_{at} + \varepsilon_{iat} \quad : \quad p = \{0, 1, \dots, 7\}$$

The only difference compared to the previous model is that the year when the outcome is observed varies with p. So, when $p = 2$ the outcome is observed in 2008 for the 2006 treatment and control groups and in 2009 for the 2007 treatment and control groups, and so on. Hence, this analysis will be informative of how the repeal affected the 2007 cohort in the long run. [13]

In contrast, analyzing the persistence of the effects of the repeal across cohorts amounts to estimating the DD model for the 2006 treatment and control groups and treatment and control groups observed further and further into the post-repeal period using repeated cross-sectional data. To do this, I estimate leads and lags of the repeal in a Granger-type DD model. As before, let Y_{iat} denote an outcome for observation *i* in group *a* observed in year *t*. The model is then defined as:

$$Y_{iat} = \alpha + \varphi_a + \tau_t + \delta D_{at} + \sum_{m=1}^{M} D_{a,t-m}\lambda_m + \sum_{s=1}^{S} D_{a,t+s}\gamma_s + u_{iat}$$

Here, δ captures the effect of the repeal for the 2007 cohort, while λ_m estimates the effect of the repeal on a cohort observed *m* years before the repeal, and γ_s estimates the effect of the repeal on a cohort observed *s* years after the repeal. Since there is no reason to expect any anticipatory effect of the repeal, the leads in this model are essentially placebo reforms that should not have an effect on the outcome. The lags will be informative about the persistence of the effect of the repeal across cohorts.

5.3 Identifying assumptions

The key identifying assumption in a DD model is the assumption of parallel trends. It stipulates that in the absence of treatment, the outcome trend for the treatment group would have been parallel with trend for the control group. To assess this assumption, Figure 5 plots outcome trends for the treated (red circles) and control (blue triangles) group over time. Ideally, the trends should be parallel (and stable) in the pre-repeal period.

Each data point in Figure 5 correspond to an outcome for the treatment and control group observed in the year indicated on the x-axis. For example, the first two data points of Figure 5a correspond to the enrollment share in 2003 of the treatment and control group observed in 2003, and the next two data points to

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[13] I use the term “cohort” in reference to the individuals included in the sample frame in a particular year, i.e. those aged 25–50 that also fulfill the rest of the sample restrictions.

the enrollment share in 2004 of the treatment and control group observed in 2004, and so on. Hence, changes in the horizontal distance between the graphs in each figure are indicative of the effect of the repeal. Figure 5a plots Komvux enrollment shares, 5b plots passed credits, 5c plots earnings and 5d plots employment shares. In all figures, the trends are pretty much aligned in the pre-repeal period and there is not much volatility. Focusing first on Figure 5a and 5b, enrollment and passed credits decrease in the treatment group relative to the control group after the repeal in 2006, indicating that the repeal did have an immediate effect on these outcomes. We can also see that the differences between the groups are larger in 2008 and 2009 than in 2007, indicating that the effects of the repeal persisted across cohorts. Focusing on Figure 5c and 5d, it looks like the repeal did not have a sizeable effect on earnings or employment, at least not in 2007. In any case, the main takeaway from this figure is that the treatment and control group does not appear to have been on separate outcome trajectories prior to the repeal.

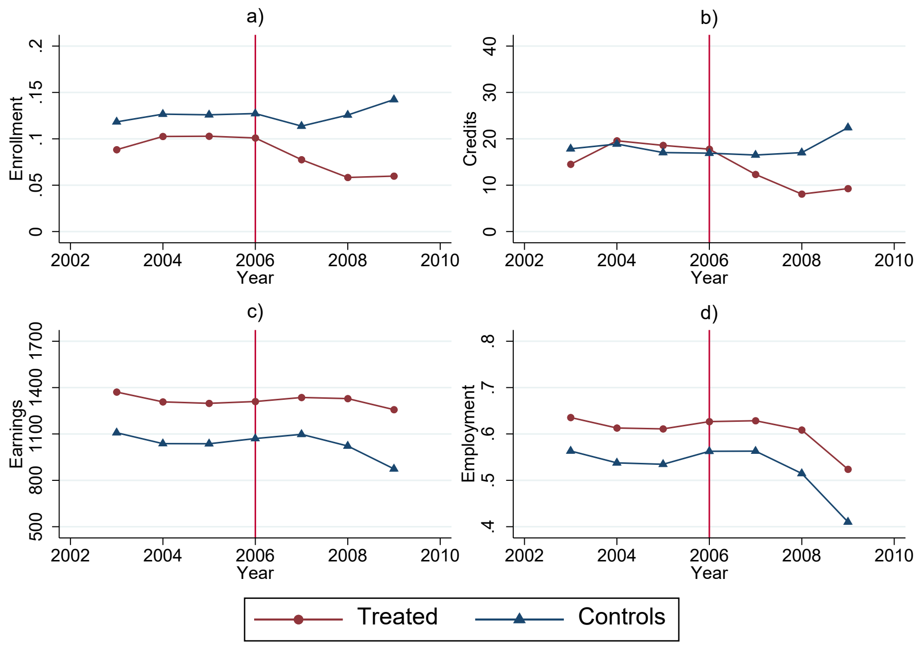

Figure 5. Outcome trends across cohorts for the treatment and control groups

Note. The red line (circles) plots outcomes for the treatment group and the blue line (triangles) plots outcomes for the control group. Figure 5a shows Komvux enrollment shares; 5b shows passed credits at Komvux; 5c shows earnings; and 5d shows employment shares. Each data point corresponds to an outcome for the groups observed in that particular year.

Another way to assess the parallel trends assumption is to look at the similarity of the treatment and control group prior to the repeal in terms of variables that can potentially explain selection behavior. The idea is that if the groups are similar to each other before the repeal takes place, it is less likely that selection bias is differentially affecting the outcome trends. Table 2 reports descriptive statistics for the 2006 treatment and control group on selected covariates observed in 2006. With the exception of gender composition and previous unemployment, the groups are very similar. Women make up 62 percent of the control group but only 46 percent of the treatment group, which means that the student aid margin binds harder for women than for men. Previous unemployment is also higher in the control group at an average of 564 days in the past three years, compared to 498 in the treatment group.

Table 2 Pre-repeal descriptive statistics

	(1) Treated		(2) Control	
	Mean	Sd	Mean	Sd
Age	38.8	6.94	35.4	7.49
Woman	0.46	0.50	0.62	0.49
Born in Sweden	0.72	0.45	0.68	0.47
Years of education	9.95	1.23	9.97	1.09
Mother's years of education	9.37	2.34	9.87	2.48
Father's years of education	9.20	2.48	9.74	2.60
Previous unemployment	498.4	402.0	563.2	363.8
Grade rank	20.3	19.3	19.7	19.7
Komvux enrollment	0.10	0.30	0.13	0.33
Credits	17.8	100.0	16.7	90.1
Observations	227,563		32,900	

Note. Column (1) reports descriptive statistics for the 2006 treated group and column (2) reports descriptive statistics for the 2006 control group. The grades are observed at 9th grade (the final year of compulsory school) and have been percentile ranked within graduation year. Previous unemployment is measured as the number of days in unemployment in the past 3 years. Means and standard deviations.

To assess whether these differences are problematic for the identification strategy, I will do a separate sensitivity analysis by combining the DD framework with a weighting strategy proposed by Stuart et al. (2014). To see how this works, let G_i denote group membership for observation i so that:

$$G_i = \begin{cases} 1 & if\ in\ treatment\ group\ in\ 2006 \\ 2 & if\ in\ control\ group\ in\ 2006 \\ 3 & if\ in\ treatment\ group\ in\ 2007 \\ 4 & if\ in\ control\ group\ in\ 2007 \end{cases}$$

Propensity scores that correspond to the probability of belonging to group j can then be estimated in a multinomial logit model where group affiliation is regressed onto a vector of covariates. The response probabilities for belonging to group 2–4 and 1 respectively, are given by:

$$P(g_i = j|\boldsymbol{x}_i) = \frac{e^{\boldsymbol{x}_i\boldsymbol{\beta}_j}}{1 + \sum_{h=2}^{4} e^{\boldsymbol{x}_i\boldsymbol{\beta}_h}}$$

$$P(g_i = j|\boldsymbol{x}_i) = \frac{1}{1 + \sum_{h=2}^{4} e^{\boldsymbol{x}_i\boldsymbol{\beta}_h}}$$

Where $j = \{1, \dots h, 4\}$ and $\boldsymbol{x}_i$ is a vector of covariates and $\boldsymbol{\beta}$ its associated vector of slope coefficients. In this specification, $\boldsymbol{x}_i$ contains a gender dummy, a dummy for being born in Sweden, a dummy for the father being born in Sweden, a dummy for the mother being born in Sweden, a second degree polynomial in age, and a second degree polynomial in the number of days of unemployment in the past three years. Following Stuart et al. (2014), each observation is then assigned a weight equal to the probability of being in the pre-repeal treatment group relative to the probability of being in the group that it is actually in. The weight for observation i that belongs to group j is thus defined as:

$$w_i = \frac{P(g_i = 1|\boldsymbol{x}_i)}{P(g_i = j|\boldsymbol{x}_i)} = \frac{1}{e^{\boldsymbol{x}_i\boldsymbol{\beta}_j}}$$

Hence, the weight w_i is equal to the inverse of the probability that observation i is in the pre-repeal treatment group relative to it's own group. Intuitively, the weight will be large for observations that are similar to those in the 2006 treatment group and dissimilar to their own group. The strategy can therefore be thought of as weighting the covariate distributions of observations in groups 2-4 to reflect the covariate distribution in the pre-repeal treatment group. Thus, by fitting a weighted DD regression model the idea is to obtain a consistent estimate of the treatment effect even in the presence of bias associated with the covariates in $\boldsymbol{x}_i$ (Stuart et al., 2014). Ideally, these estimates should be very close to those obtained in the main analysis.

6 Results

Table 3 presents DD estimates of the effect of repealing the Recruitment grant on Komvux enrollment, passed credits, earnings and employment. The first column shows the effect on the target population as a whole, while columns 2–7 show estimates for different subgroups of the sample. Also, notice that these are estimates of *immediate* effects – i.e. based on the differences in outcomes observed in 2006 and 2007. In the next section, I will focus on long run effects by looking at outcomes observed up to seven years after the repeal.

Focusing first on the overall effects in the first column, we can see that the repeal reduced the Komvux enrollment rate in 2007 by 1 percentage point, which amounts to a 10 percent decrease compared to the pre-repeal enrollment rate in the treatment group. To put the size of this effect into perspective, a back-of-the-envelope calculation suggests that a $1,000 offer of annual student aid will increase enrollment by about 0.5 percentage points.[14] By comparison, Dynarski (2003) finds that a $1,000 offer of annual student aid increases college enrollment by 3.6 percentage points in the U.S., while Nielsen et al. (2010) estimate that the corresponding effect is 1.35 percentage points in Denmark.

Table 3 DD estimates of repealing the Recruitment grant

	(1) All	(2) Women	(3) Men	(4) High par. ed.	(5) Low par. ed.	(6) Swedish background	(7) Non-swedish background
Enrollment	-0.01***	-0.027***	0.002	-0.017***	-0.009***	-0.013***	-0.013***
	(0.003)	(0.004)	(0.004)	(0.006)	(0.003)	(0.003)	(0.005)
Credits	-5.2***	-9.5***	-1.9*	-8.4***	-4.2***	-5.7***	-4.3***
	(0.82)	(1.19)	(1.02)	(1.73)	(0.92)	(0.97)	(1.44)
Earnings	834	2,762***	-1,436	3,820**	540	4,693***	-1,092
	(744)	(898)	(1,287)	(1,574)	(845)	(947)	(1,188)
Employment	0.005	0.013***	-0.008	0.009	0.007	0.018***	0.000
	(0.004)	(0.005)	(0.006)	(0.008)	(0.004)	(0.005)	(0.006)
Observations	492,590	237,394	255,196	95,051	397,539	322,516	170,074

Difference-in-differences estimates of repealing the Recruitment grant at the end of 2006. The sample is restricted to unemployed individuals in 2006 and 2007 with at most 11 years of education. The treatment group was eligible for the Recruitment grant, but the control group was not. Outcomes are observed in 2006 and 2007. Standard errors are clustered at the individual level.

However, assessing effect sizes in percentage points can be misleading if there are large differences in baseline enrollment rates across the samples. Indeed, the college enrollment rate in Nielsen et al. (2010) is 39 percent, and the college enrollment rate in Dynarski (2003) is 50 percent – much higher than the 9 percent enrollment rate in my sample. Relative to the enrollment rate in the sample, the effect of offering $1,000 of student aid is about 3.5 percent in Nielsen et al. (2010) and about 7.2 percent in Dynarski (2003), while I find that it is about 5 percent. Hence, relative to the enrollment rate, the effect of offering student aid at the compulsory and upper secondary level in Sweden is quite similar in size to the effect at the college level in the U.S. and Denmark.

[14] To arrive at 0.5 percent, I assume (based on previous levels of unemployment) that 10 percent of the sample was eligible for the supplemental student aid.

Though the repeal of the Recruitment grant decreased enrollment, it is possible that those who chose not to enroll because of the repeal would have failed and dropped out anyway. It is also conceivable that talented students are less affected by the prospect of accumulating debt to study, and therefore were unaffected by the repeal. To investigate whether this is the case, I also look at the effect of the repeal on the number passed credits at Komvux. As we can see, the repeal decreased the number of passed credits by about 5.2, which amounts to a 29 percent decrease relative to the pre-repeal treatment group average (Table 2). Since enrollment only decreased by 10 percent, the repeal either had a larger effect on relatively high ability students or decreased the passing rate at given levels of ability. Lastly, looking at earnings and employment we can see that the repeal did not have an immediate effect on these outcomes for the target population as a whole.

Focusing next on columns 2–7, I have split the sample across gender, parental education and ethnic background. Parental education is defined as "high" if at least one parent has at least 3 years of upper secondary education, and as low otherwise. Ethnic background is defined as "Swedish" if a person was born in Sweden and has at least one parent who was also born in Sweden, and as "non-Swedish" otherwise. Looking first at the separate estimates for women and men in columns 2 and 3, we can see that the overall effects on enrollment and credits are driven by the effect of the repeal on women in the target population. The gender difference is striking and difficult to discern. Stenberg et al. (2014) also find a gender gap when estimating the returns to Komvux enrollment, albeit among older workers, and suggest that it stems from differences in the underlying reasons for enrollment. They observe that male participation in their sample is associated with increased levels of sick-leave benefits prior to enrollment, whereas female enrollment appears to be motivated by a "latent demand" for education. Looking next at earnings and employment, the repeal increased earnings and employment for women but not for men, suggesting that women to a larger extent than men substituted studies for work.

When the sample is split across parental education (columns 4 and 5), we can see that the repeal had a larger effect on enrollment, credits and earnings for students with high parental education. The effect on earnings is in line with what one would expect if students with high parental education have better prospects on the labor market (this could be because they have access to social networks of relatively higher quality, or that they simply have higher ability). If that is the case, this would also increase the opportunity cost of enrollment and therefore increase the responsiveness to changes in the cost of education, which explains the relatively large drop in enrollment and passed credits. When the sample is split across ethnic background (columns 6 and 7), we can see that the repeal had a similar effect on enrollment and credits for both groups. We can also see that the repeal had a positive effect on income and employment for those with a Swedish background but not for those with a non-Swedish background.

Hence, even though the repeal decreased enrollment and passed credits at Komvux in 2007, it also increased earnings and employment for some subgroups of the population in the short run. In the next section, I investigate the effects of the repeal on labor market outcomes in the long run.

6.1 Effect dynamics and long-run outcomes

I have plotted annual estimates of the effect of the repeal based on outcomes observed up to seven years after the initial year of observation in Figures 6, 7 and 8. That means that I observe outcomes between 2006–2013 for the 2006 treatment and control groups, and between 2007–2014 for the 2007 treatment and control groups. Hence, the years on the x-axis run from 0 to 7.

As we can see in Figure 6a, the effect of the repeal on employment is very small for the target population as a whole. The estimates indicate a very slight increase for the first three years that peaks at about 1 percentage point. This effect is very small relative to the 56 percent employment rate in the pre-repeal treatment group. Focusing on different subgroups, the effect is slightly larger and more persistent for women, for those with high parental education, and for those with a Swedish background. There is no discernable effect on employment for any of the other subgroups.

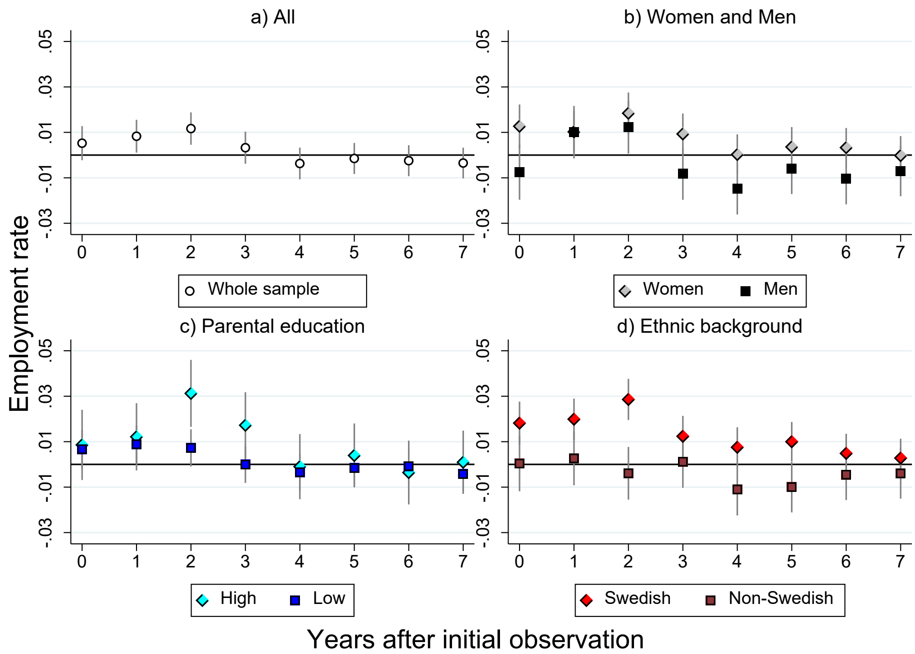

Figure 6. Effect dynamics: employment

Notes. The figure shows DD estimates of the effect of repealing the Recruitment grant on employment rates up to seven years after the initial year of observation. Figure 6a shows estimates of the overall effect on the sample; 6b shows estimates for women (grey diamonds) and men (black squares); 6c shows estimates for those with high (cyan diamonds) and low (dark blue squares) parental education; and 6d shows estimates for those with Swedish (red diamonds) and non-Swedish (maroon squares) background. The grey spikes correspond to two-sided confidence intervals at the 95 percent level.

Figure 7 shows the effect of the repeal on days of unemployment. Looking first at the effect on the target population as a whole, we can see that the repeal increased unemployment by 10 days in the first year and by 14 days in the second year. The effect on unemployment then decrease to about 9 days per year for the rest of the period. Although the increased unemployment persist over the years (unlike the effect on employment), the effect is quite small relative to the average number of days of previous unemployment in the sample (176 days).

Focusing on figures 7b, 7c, and 7d, we can see the repeal increased the days of unemployment more for men, for those with high parental education, and for those with a Swedish background. In particular, individuals with high parental education seems to be the most adversely affected group with an average increase of about 14 days of unemployment per year.

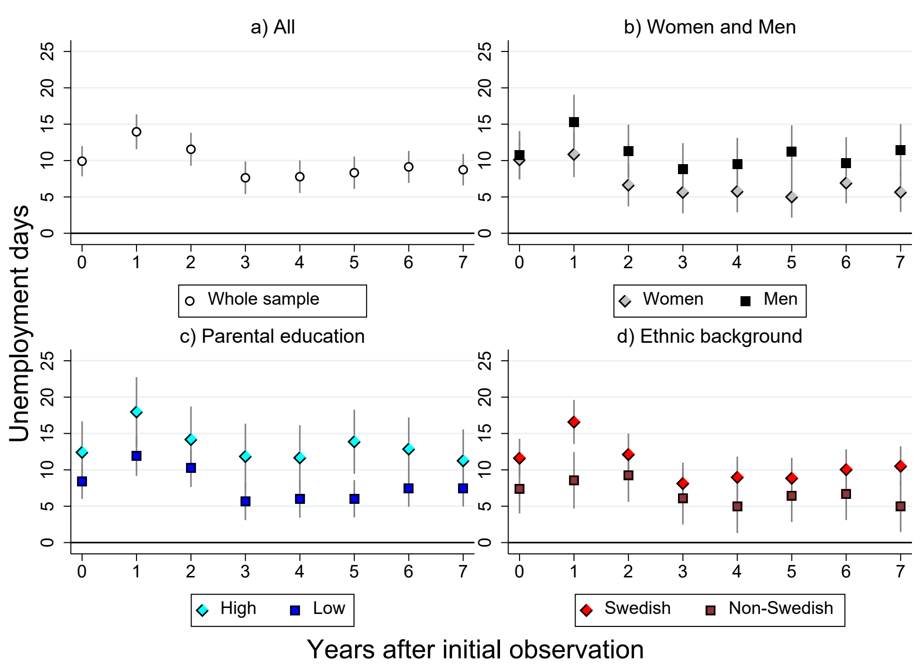

Figure 7. Effect dynamics: days of unemployment

Notes. The figure shows DD estimates of the effect of repealing the Recruitment grant on days of unemployment up to seven years after the initial year of observation. Figure 7a shows estimates of the overall effect on the sample; 7b shows estimates for women (grey diamonds) and men (black squares); 7c shows estimates for those with high (cyan diamonds) and low (dark blue squares) parental education; and 7d shows estimates for those with Swedish (red diamonds) and non-Swedish (maroon squares) background. The grey spikes correspond to two-sided confidence intervals at the 95 percent level.

Hence, the estimated effect of the repeal on employment and on days of unemployment are somewhat at odds with one another. This is surprising since one would expect that effects of the repeal would go in opposite directions for employment and days of unemployment. Given that the results for employment are very small if at all significant, a possible explanation is that the employment indicator does a poor job of quantifying the labor market attachment of the target population, and those results should therefore be interpreted with caution.[15]

Figure 8a shows the estimated effects of the repeal on annual earnings for the target population as a whole. As we could see in Table 3, there is no effect in

[15] Recall that the employment variable only indicates whether an individual was employed at least one hour a week in the month of November.

2007, but a negative trend is discernable in the figure, that stabilizes around 2,500 SEK (about $280) after four years.

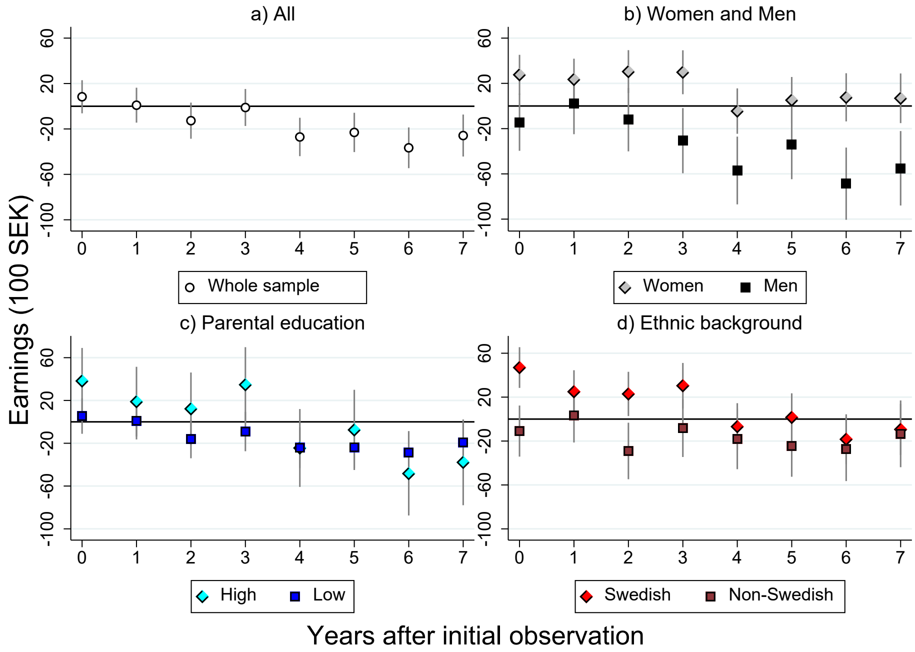

Figure 8. Effect dynamics: earnings

Notes. The figure shows DD estimates of the effect of repealing the Recruitment grant on earnings up to seven years after the initial year of observation. Figure 8a shows estimates of the overall effect on the sample; 8b shows estimates for women (grey diamonds) and men (black squares); 8c shows estimates for those with high (cyan diamonds) and low (dark blue squares) parental education; and 8d shows estimates for those with Swedish (red diamonds) and non-Swedish (maroon squares) background. The grey spikes correspond to two-sided confidence intervals at the 95 percent level.

Looking at Figure 8b, we can see a rather striking difference between men and women. For women, the repeal increased earnings by about 2,500 SEK for the first four years, but then had no further effect. In contrast, the repeal had no discernable effect on men's earnings the first two to three years, but then reduced it by about 5,500 SEK ($610). One plausible explanation for both the lack of an initial earnings effect for men and the lack of a later earnings effect for women is that women more often than men substitute Komvux studies for work, and then end up in relatively low wage occupations after completing their studies.

Focusing on differences across parental education, we can see that there is a positive earnings effect for those with high parental education that eventually fades out and becomes negative. In contrast, the repeal does not appear to have had much of an effect at all on earnings for those with low parental education.

Differences across ethnic background follows roughly the same pattern, where earnings for those with a Swedish background initially increases and then decreases while the earnings effect is either zero or negative for those with a non-Swedish background.

Table 4 presents estimates of the effect of the repeal over the whole observation period on the following outcomes: total number of passed credits at Komvux, enrollment rate in tertiary education, total number of days of unemployment, and total earnings (i.e. the sum of earnings, measured here in hundreds of SEK). As in figures 6, 7 and 8, the outcomes are observed from 2006–2013 for the 2006 treatment and control groups, and from 2007–2014 for the 2007 treatment and control groups.

For educational outcomes, we can see that the effect of the repeal on the total number of passed credits for the whole period is about twice as large as the effect in 2007 estimated in Table 3, which is what we would expect given that a student studies for about 2.5 semesters on average (see Figure 4).

Table 4 DD estimates of effects on long-run outcomes

	(1) All	(2) Women	(3) Men	(4) High par. ed.	(5) Low par. ed.	(6) Swedish backgroun d	(7) Non-Swedish
Total credits	-9.9***	-16.6***	-9.7***	-18.9***	-6.9***	-14.0***	-5.6
	(2.12)	(3.07)	(2.60)	(4.45)	(2.40)	(2.55)	(3.72)
Tertiary ed.	-0.004***	-0.004*	-0.006***	-0.010***	-0.001	-0.004**	-0.004**
	(0.0014)	(0.0019)	(0.0021)	(0.0037)	(0.0014)	(0.0019)	(0.0021)
Unemployment	75.5***	56.2***	85.9***	106.9***	61.2***	86.5***	51.9***
	(5.97)	(7.58)	(9.80)	(11.70)	(6.91)	(7.68)	(9.48)
Total earnings	-12,191**	12,578**	-27,854***	988	-12,653**	9,341	-13,298
	(5,207)	(6,223)	(9,144)	(11,097)	(5,897)	(6,621)	(8,340)
Observations	492,590	237,394	255,196	95,051	397,539	322,516	170,074

Note. Difference-in-differences estimates of effects on long-run outcomes. The sample is restricted to unemployed individuals observed in 2006 and 2007 with at most 11 years of education, who did not receive student aid in the previous year. The outcomes are observed between 2006–2013 for the 2006 treatment and control groups, and between 2007–2014 for the 2007 treatment and control groups. Standard errors are clustered at the individual level.

We can also see that passed credits decreased more for women than for men, more for those with high parental education than those with low parental education, and more for those with a Swedish background than for those with a non-Swedish background. When it comes to enrollment into tertiary education, the repeal caused a 0.4 percentage point decrease over the entire period for the target population. The decrease was particularly large for those with high parental education at 1 percentage point, while those with low parental education were the only ones whose enrollment into tertiary education was unaffected.

When it comes to the labor market outcomes, the results vary substantially across subgroups and not everyone was adversely affected by the repeal in terms of income. For example, the repeal increased unemployment by 107 days for those with high parental education but had no discernable effect on their long-run earnings. In contrast, the repeal actually increased earnings for women by about 13,000 SEK, but decreased them for men by 28,000 SEK. To better understand this, we can look at the dynamics of the earnings effect in Figure 8. There we can see that the gender difference is explained by an initial earnings increase for women that was not completely offset later on when, presumably, the women who enrolled in adult education re-entered the labor market. In contrast, the men never experienced an initial increase in earnings following the repeal, but did experience a drop in earnings later on similar to the women. In any case, the repeal both decreased the earnings (by about 12,000 SEK ($1.300) on average over the period) and increased the unemployment (by 75,5 days on average) for the target population as a whole.

6.2 Robustness checks and effect persistence

As I discussed in section 5.3, the key identifying assumption in the DD framework is that of parallel outcome trends for the treatment and control group had the treatment not occurred. To assess whether the differences between the groups observed in Table 2 are likely to violate this assumption, I estimate a weighted DD regression model as suggested by Stuart et al. (2014). In this model, the weights correspond to propensity scores for the probability of each observation to belong to the 2006 treatment group relative to the group the observation actually belong. Hence, this weighting strategy can be understood as a strategy that makes the covariate distributions more similar across groups (where similarity gauged with respect to the pre-treatment treatment group).

Table 5 reports the same pre-treatment descriptive statistics as Table 2 using the propensity score weights described in section 5.3. As we can see, the differences in gender composition and previous unemployment between the groups are much smaller. The weighted 2006 control group is comprised of 48 percent women and has an average of 519 days of unemployment in the past three years, compared to 62 percent women and 563 days of unemployment when the observations are not weighted. To see how this affects the overall results for the target population, I present weighted DD estimates in Table 6.

Table 5 Weighted pre-treatment descriptive statistics

	(1) Treated		(2) Control	
	Mean	Sd	Mean	Sd
Age	38.8	6.94	38.6	6.89
Woman	0.46	0.50	0.48	0.50
Born in Sweden	0.72	0.45	0.70	0.46
Years of education	9.95	1.23	9.91	1.18
Mother's years of education	9.37	2.34	9.56	2.46
Father's years of education	9.20	2.48	9.46	2.61
Previous unemployment	498.4	402.0	519.4	403.2
Grade rank	20.3	19.3	19.3	19.6
Observations	227,563		32,900	

Note. All observations are weighted by propensity scores that reflect the probability of being in the pre-treatment treated group relative to the group that the observation is actually in. Column (1) reports descriptive statistics for the 2006 treated group and column (2) reports descriptive statistics for the 2006 control group. The grades are observed at 9th grade (the final year of compulsory school) and have been percentile ranked within graduation year. Previous unemployment is measured as the number of days in unemployment in the past 3 years. Means and standard deviations.

The precision is roughly the same as the precision for the unweighted estimates presented in the first column of Table 3. However, the point estimates are slightly less negative for enrollment and credits, and more positive for earnings and employment. This is expected given that the weights are larger for observations with low previous unemployment, making them more likely to be employed if they are not attending Komvux.

Table 6 Weighted DD estimates of repealing the Recruitment grant

	(1) All
Enrollment	-0.007**
	(0.003)
Credits	-3.4***
	(0.78)
Earnings	3,320***
	(1,102)
Employment	0.011**
	(0.005)
Observations	492,590

Note. Difference-in-differences estimates of repealing the Recruitment grant at the end of 2006. The sample is restricted to unemployed individuals in 2006 and 2007 with at most 11 years of education. The treatment group was eligible for the Recruitment grant, but the control group was not. Standard errors are clustered at the individual level.

Table 7 reports the estimated leads and lags from the Granger-type DD model defined in section 5.2. The outcome is Komvux enrollment, and as expected the placebo treatments estimated by the leads in 2005 and 2006 are zero. In contrast, the lags indicate that the adverse effects of the repeal on Komvux enrollment increased in 2008 and 2009. This is not particularly surprising given the state of the Swedish economy during this period. Sweden, along with most of the world, was struggling to cope with the financial crises that originated in the U.S. subprime mortgage market in 2007.

Table 7 Effect dynamics and placebo treatments

	(1) All
Enrollment: repeal $_{t-2}$	0.001
	(0.002)
Enrollment: repeal $_{t-1}$	-0.002
	(0.002)
Enrollment: repeal $_{t}$	-0.012***
	(0.003)
Enrollment: repeal $_{t+1}$	-0.043***
	(0.003)
Enrollment: repeal $_{t+2}$	-0.058***
	(0.003)
Observations	1,502,786

Note. Difference-in-differences estimates of repealing the Recruitment grant on Komvux enrollment. *t* denotes 2007– the first year after the Recruitment grant was repealed. The sample is restricted to unemployed individuals observed in 2006 and 2007. The treatment group was eligible for the Recruitment grant, but the control group was not. Standard errors are clustered at the individual level.

7 Conclusion

In 2003, the Recruitment grant was introduced in order to incentivize enrollment in adult education among unemployed adults with low education. It replaced the loans in the regular student aid scheme with grants for up to one year for the students that were eligible, amounting to a total value of about $2,000 ($3,500 for those that were eligible for supplemental student aid). The grant was offered for studies at the compulsory or upper secondary level to individuals aged 25–50 who were unemployed or at the risk of becoming unemployed. The grant was repealed at the end of 2006, following an election that saw a social democratic government ousted by a conservative alliance. In this paper, I estimate the causal effect of repealing the Recruitment grant on the educational attainment and labor market outcomes for the target population, whose outcomes I observe up to eight years after the repeal. To identify effect of the repeal, I estimate the treatment effect in a difference-in-differences framework that combines the sudden repeal of the grant with an eligibility requirement that required the recipient to not have received any form of student aid in the past five years. This allowed me to use those who had received student aid in the past five years (but was otherwise qualified for the grant) as a control group whose outcomes I compare with the treated group (those who were qualified) before and after the repeal.

I find that the repeal decreased the educational attainment and increased the number of days of unemployment for the target population as a whole over the observation period. The enrollment rate in Komvux decreased by 10 percent relative to the pre-repeal treatment group average, and the number of passed credits decreased by 29 percent. This implies that the repeal had a negative effect on the passing rate, which suggests that the take up of the grant was larger among relatively high ability students or possibly that the grant had a direct and positive effect on the probability of obtaining a passing grade. The former explanation is somewhat supported by the effect of the repeal on enrollment in tertiary education, which decreased by about 20 percent relative to the pre-repeal treatment group average.

The effect of the repeal on labor market earnings is a bit more ambiguous. Although the repeal decreased earnings over the entire observation period by about 12,000 SEK ($1,300) for the target population as a whole, for some subgroups of the population the effect on earnings was initially positive and later faded out and became negative. This dynamic is consistent with what one would expect from an educational investment – an initial cost that is overcome by returns in the long run. However, the long-run returns did not materialize for women in this study, and as a result, the repeal actually increased their overall earnings over the period. In contrast, the repeal had no initial effect on earnings for men, but a negative effect later on. A plausible explanation for these differences is that women to a larger extent than men end up in low wage occupations after completing their studies.

Focusing on unemployment, I find that the effect of the repeal varies considerably across different subgroups of the target population. Over the entire observation period, the repeal increased unemployment by a total of 107 days for those with high parental education compared to 61 days for those with low parental education. The corresponding difference between those with a Swedish and a non-Swedish background is 87 days vs 52 days, and 86 days vs 56 days for men and women respectively. There are many possible explanations for these results. One that immediately springs to mind, that is also consistent with the results on educational attainment, is that the less adversely affected groups are more positively selected into adult education in terms of ability and baseline attachment to the labor market, and therefore are more likely to be employed if they do not study. This implies that the propensity to substitute work for Komvux studies is not uniform across the target population, and that attachment to the labor market following Komvux studies improve more for some subgroups than others. It is of course also possible that these differences to some extent are generated by social structures that affect the function of the labor market, such as access to career paths.

To assess the validity of the DD framework, I implemented a propensity score weighting strategy suggested by Stuart et al. (2014) that weights the covariate distributions in the control groups and the post-repeal treated group to reflect the covariate distribution in the pre-repeal treated group. The resulting estimates confirmed the results from the unweighted DD models for Komvux enrollment and credits, but also generated a positive effect on earnings and employment. The difference between the estimates reflect the fact that the weighting scheme puts more weight on observations with relatively low previous unemployment, making them more likely to be employed if they are not attending Komvux. I also estimated a Granger-type DD model with leads and lags between 2005 and 2009. Reassuringly, the placebo treatments in 2005 and 2006 were both zero while the treatment lags in 2008 and 2009 were both significant, indicating that the adverse effects of the repeal on Komvux enrollment persisted across cohorts.

In conclusion, the implication of these results is that it is indeed possible to increase enrollment in adult education and improve subsequent labor market prospects for unemployed adults with incomplete upper secondary education by offering students grants instead of loans. The repeal significantly decreased enrollment in adult education as well as the number of passed credits. The repeal also increased unemployment for the target population, but the effect on earnings were small and varied across subgroups. In general, the effect of repealing the Recruitment grant varied considerably across subgroups. Going forward, an important task will therefore be to identify the mechanisms that drive the differences across subgroups to ensure increase the efficiency of future educational policy.

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