Extracurricular leadership activities at college and their impact on labor market entry and career trajectories[1]

by

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September 4, 2014

Abstract

We study how leadership experiences acquired through extracurricular activities during college years affect subsequent labor market performance, using a regression discontinuity design to isolate the causal effects. The design is applied to elections of representatives at Swedish Student Union (SU) councils. Archive data on failing and succeeding candidates at three major Swedish universities are mapped to register data on their subsequent labor market careers. The results show that elected students are much more likely to have a rapid transition into employment than failing candidates who were on the margin of being elected. The employment effects are not particular to workplaces, organizations or industries where previous candidates are employed, which suggests that the benefits of having been a student representative are more general than specific. Elected representatives also have a statistically increased probability of holding a well-paid (above median) job within three years, but not thereafter. Overall, our estimates suggest that participation in extracurricular leadership activities boost the students' initial career trajectories, whereas long-run outcomes appear unaffected.

Keywords: Extracurricular activities, labor market entry, earnings, higher education

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Work in progress - do not quote

Table of contents

1   Introduction

A smooth and effective transition from higher education into work is beneficial both from the perspective of the individual student and for the society as a whole. A popular perception is that college students can improve their labor market prospects by participating in extracurricular activities (ECA) as student representatives or by holding other leadership positions while enrolled in higher education.[5] Indeed, previous research suggests that students who participate in ECA partly do so in order to increase their labor market prospects (see e.g. Roulin & Bangerter, 2013). However, economists have, in contrast to scholars within other social sciences, devoted surprisingly little attention to the question of how ECA in general affect student outcomes. As a consequence, there exists very limited credible causal evidence on how participation in leadership activities during college affects future labor market outcomes.

In this paper, we make use of unique data on university students in Sweden to examine the impact of acquiring leadership experiences through extracurricular activities during college years on labor market entry and career trajectories. More specifically, we use Regression Discontinuity (RD) designs to isolate differences between bare winners and bare losers in elections to student union (SU) councils at three major universities in Sweden. Our outcomes are measured by matching SU election data to population wide register data on employment and earnings over time. The RD approach allows us to address the non-random assignment that has plagued previous research on ECA, in particular within neighboring social sciences (Farb & Matjasko, 2012).

Our focus is on identifying the overall causal effects of becoming a student representative. This is an event which may affect the labor market outcomes through several mechanisms and we will not be able to present a sharp separation between these. An obvious first mechanism is that experiences acquired as a student representative may endow the students with useful human capital such as leadership skills (Kuhn & Weinberger, 2005). The experience may also serve as a signal of pre-existing skills or traits to future potential employers. Previous research within the "employer learning" literature (building on Spence's signaling theory) has, for example, highlighted the role

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[5] For instance, a simple Google search indicate numerous university web sites, all over the world, where it is claimed that becoming a student representative provides the student with experiences and skills that will enhance the CV. Our own small scale survey of current student representatives in Sweden confirms that the elected students share this perception (see section 2.2 below).

of uncertainty about worker skills at the time of labor market entry (e.g. Altonji & Pierret, 2001; Lange, 2007; see also Spence, 1973).[6] This uncertainty could be decreased if the students have documented experiences of leadership activities during college years. Finally, being a student representative may provide useful social ties to high-ability students or university officials and previous research has shown that social networks play an important role in the school to work transition phase (e.g. Kramarz & Skans, 2014; Shue, 2013). It should also be noted that these three potential benefits—increased human capital, signaling and networks—potentially could be counteracted by the crowding out of other useful activities (e.g. studying). A close parallel in terms of possible mechanisms exists in the literature on working while enrolled in school or at college.[7]

As already alluded to, most previous empirical studies on ECA and their consequences has been published within other social sciences (e.g. sociology, psychology and education). The analyses within this literature have examined a large number of activities (e.g. sports, music, performing arts, student government, and voluntary work) and their relationship to a host of different outcomes such as academic achievement, labor market entry, sexual activity, drug use and delinquency. Most of this work has, however, relied on survey-based approaches in a high school setting. A general shortcoming has also been the inability to demonstrate causal evidence for the identified relationships (for a literature review, see Farb & Matjasko, 2012).

The relatively scarce economic literature on ECA has mostly focused on the impact on educational attainment (e.g. Barron, Ewing, & Waddell, 2000; Eide & Ronan, 2001; Lipscomb, 2007; Lozano, 2008; Rees & Sabia, 2010; Rouse, 2012). A few recent studies also cover labor market outcomes: (Henderson, Olbrecht, & Polachek, 2006) use nonparametric regression techniques to assess the impact of being a college athlete on subsequent labor market success, finding a positive wage premium. A compelling analysis is provided by Stevenson (2010), who examines the role of sports in spurring a

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[6] An important finding in this literature is that the value of signals should deteriorate as the market acquires information which is accumulated through labor market experience. Most of the literature has focused on how the market learns about cognitive skills, but analyses also show similar effects for non-cognitive skills (Hensvik & Skans, 2013). Note also that Arcidiacono and colleagues find that higher education reduces the role of employer learning though market experience (Arcidiacono, Bayer, & Hizmo, 2010), but that this conclusion has been contested (Light & McGee, 2012).

[7] The literature include findings of negative effects on educational achievement (e.g. Häkkinen, 2006; Stinebrickner & Stinebrickner, 2003) and indications of positive labor market effects, at least in the short-run (e.g. Geel & Backes-Gellner, 2012; Hotz, Xu, Tienda, & Ahituv, 2002; Häkkinen, 2006; Light, 2001).

successful job career. She uses the "Title IX" reform in the US (which required high schools to level out gender differences in athletic participation) as a natural experiment to handle self-selection issues. The results show that the ensuing rise in female sports participation significantly increased female labor force participation. Finally, Kuhn and Weinberger (2005) focus specifically at leadership experiences using longitudinal survey data to analyze the labor market effects of having been a team captain or club president at school. They conclude that individuals who have these leadership experiences earn significantly more as adults, and they are more likely to be managers, than those who lack such high school experiences.[8]

Our analysis differs from previous research, and contributes to the literature, by focusing on the labor market effects of a specific leadership experience (being a student representative) in college. In contrast to previous studies we also measure the outcomes using register data rather than survey data, which allow us to move beyond snapshots and assess the dynamic impact of ECA on various labor market outcomes. Most importantly, however, we address the self-selection problems by using election discontinuities to isolate the causal effects of being elected as a student representative on labor market entry and subsequent career trajectories.

We have collected candidate data and election results from elections to Swedish SU councils at three major universities (Lund, Stockholm and Uppsala) between the years 1982 and 2005.[9] These data are matched to information from national registers on employment and annual earnings between 1985 and 2010. We use election discontinuities to identify the causal impact, comparing bare winners to bare losers amongst student who ran as candidates to become student representatives within the councils. SU council elections follow a closed ballot list election system where groups of students ("student parties") provide lists of pre-ranked candidates before the elections, and other students vote on these lists. The number of students elected as representatives depend on the share of votes given to each list in accordance with the rank order of the students, which means that bare winners and losers are separated by an election threshold within each list.

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[8] Note also that **Grönqvist and Lindquist (201X)** examine the impact on labor market outcomes of leadership training within the military and find positive effects. Their empirical set up is similar to ours.

[9] As these data are drawn from the student unions' own archives, which sometimes were incomplete (in particular concerning the identities of candidates who were not elected), data do not cover all years within each university.

An important precondition for proper identification of causal effects in this RD-setting is that the exact locations of the election thresholds were unpredictable at the time when the ballot lists were submitted. This is indeed likely to be the case, since lists are compiled with very little knowledge regarding the competition, and without any election polls. Furthermore, only 13 per cent of the parties received the same number of seats as in the previous election. In line with the presumption that students are unable to sort themselves exactly around the thresholds, we find no systematic differences between bare winners and bare losers in observed (predetermined) characteristics.[10]

The results of our analysis suggest that students who become student representatives have a markedly increased probability of being employed in the year that follows the election. The effect is found to be robust to a wide set of variations of the RD-model. However, the effect on the probability of being employed is short-lived as other students catch up within two years. We find no impact on the probability of entering workplaces, organizations or industries where previous representatives are employed, which suggest that the benefits are valid across larger segments of the labor market.

We also analyze the impact on the probability of holding a well-paid job, defined as a job with a pay exceeding the median among 30 year old university graduates. Elected student representatives are significantly more likely to achieve this level of earnings within 3 years than those who marginally failed to be elected. However, throughout the analysis our results indicate that the benefits of becoming a student representative, although large in the short-run, vanish over time.

Overall, our results thus suggest that becoming a student representative causally increases the probability of a rapid transition into the labor market, in particular amongst students without prior work experience. This indicates that participation in the councils could serve as a substitute for work experience. The representatives also benefit in terms of a faster career trajectory in the first few years thereafter, but the effects do not appear to provide permanent benefits. The short to medium-run nature of the effects, and the fact that the results appear particularly pronounced amongst youths

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[10] It should be noted that our empirical approach mirrors recent studies of how participation in "regular politics" affect employment and earnings; on Swedish data finding no effects on subsequent earnings (Lundqvist, 2011), and on Finnish data finding a short-run positive effect on earnings (Kotakorpi, Poutvaara, & Terviö, 2013). The focus of these studies is, however, quite different from ours, since they document the relationship between participation in national or sub-national parliamentary elections and labor market performance for a general set of adults. Related to this, it should also be noted that our companion paper (Lundin, Nordström-Skans, & Zetterberg, 2013), using the same SU-data and empirical approach as in this paper, shows that participation in SU councils increases the probability of running for office within regular (primarily local) politics later in life.

who lack previous labor market experience, could be indicative of an interpretation in terms of signaling effects.

The rest of the paper is structured as follows. Section 2 describes the institutional setting, explaining the role of student unions in Sweden and how students are elected into the councils. Section 3 explains the data and the method as well as provides standard RD falsification tests. Section 4 presents the results regarding labor market entry and career trajectories. Section 5 concludes.

2 Institutional setting

2.1 Higher education in Sweden

In Sweden, public universities (*Universitet*) and university colleges (*Högskola*) provide tuition-free higher education. Students are admitted to programs or courses based on grade point averages from high school (GPA) and scholastic aptitude test scores (corresponding to SAT). At the undergraduate level, students can participate in programs, which usually are between three and five years long, or combine single-subject courses into degrees. As a result, students (also those initially admitted to programs) tend to adjust the duration of studies, for example, in response to the results of job search activities and the time to degree therefore varies considerably between students. Almost all students support themselves through generous government grants (loans and allowances) that are available for a maximum of six years for all students irrespective of parental income. As shown below, a fairly large proportion of students add to their income by working while enrolled.

2.2 Student unions

Until July 1, 2010 it was mandatory for all students at universities and university colleges in Sweden to be a member of a student union (*Studentkår*). Accordingly, these organizations tend to represent a sizeable student population. Whilst student unions exist in a large number of countries, their features differ somewhat (see e.g. Klemenčič, 2012). In some countries, they mostly organize activities such as culture and sports. In others, such as in Sweden or in the UK, student unions are more similar to interest organizations. That is, they act as "labor unions." This means that they (try to) influence the decisions by, e.g., universities, municipalities and the national government. Swedish student unions represent the students in every decision-making body within universities,

having up to one-third of the seats in these bodies. The unions also provide their members with various kinds of services: judicial counselling, housing and health counselling are some examples. Thus, they have some formal power and a fairly important position within the Swedish system of higher education. In our analysis, we focus on three of the largest student unions in Sweden: Uppsala Student Union (*Uppsala studentkår*); Stockholm University Student Union (*Stockholm universitets studentkår*); and Lund Student Union (*Lunds studentkår*). All these three organizations have more than 15,000 members who are represented by the councils.[11]

In each of the student unions studied in this paper, there is a "legislative" council and an executive body. Council members are elected by their fellow students on a yearly basis. The electorate vote on "student parties" that present closed lists with a preset ranking of council candidates.[12] Ballots are sent to student union members shortly before the elections, and no registration is needed to be able to vote. Some of the parties are ideologically based, whereas others are based on alternative motives such as representing a certain faculty or educational program. Council seats are proportionally allocated to the lists and then distributed according to the (predetermined) candidate ranking within each candidate list.

Within the councils, elected student representatives are involved in activities such as debates and negotiations with other parties. Council meetings are held about once a month, but in between meetings representatives spend a considerable amount of time (see below) initiating policy proposals, craft budgets, write policy documents and conduct lobbying activities by approaching various organizations and authorities on the behalf of the students' interest.

In the present paper, we are interested in the labor market effects of participating in the SU councils; that is, taking part in the processes of leading a quite large and significant organization during the college years. The experiences individuals receive by being a student representative might very well be valued on the labor market.

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[11] Uppsala Student Union has about 33,000 members (the largest in Sweden) and Stockholm University Student Union approximately 20,000 members. In 1990, Lund Student Union had approximately 15,000 members. However, during the second half of the 1990s, Lund Student Union was split up into a large number of faculty-based student unions. Thus, it does no longer exist.

[12] In the few cases (certain years in Uppsala and Lund) where votes on individual candidates also were allowed, our statistical approach takes potential selection biases into account (see details below).

A survey[13] which we carried out among *candidates* in the SU elections at Uppsala University during 2011 and 2012 indicates that many of the current council candidates believe that experiences from SU politics (if they get elected) would be important for their future professional careers. For instance, about one third of the candidates note that professional career motives was a "fairly" or "very" important reason to run in SU elections, and almost two thirds report that they believe that becoming a council member improves skills and networks that are important on the labor market. *Elected* council members (surveyed towards the end of their term) emphasized skills they had learned within the council: around 70 per cent stated that their labor market skills had increased to a "fairly" or "very" large extent as a consequence of being involved in the activities within the SU council. The survey also indicates that council members devote a lot of time to SU politics: on average, 12 hours a week. To summarize, there are reasons to believe that college students can improve their future labor market prospects by holding a leadership position as a student union representative during their college years.

3   Data and method

3.1   Statistical model

We derive the causal effects of becoming a student representative by using the Regression Discontinuity (RD) design (Thistlethwaite & Campbell, 1960). The basic idea is to compare labor market outcomes for bare winners and bare losers in the SU council elections. As the election result allocates a certain number of seats to each list, the key *threshold* is given by the position on each ballot list that coincides with the number of seats allocated to that list. These list-specific thresholds allow some candidates to enter the councils with a small margin whereas other closely placed candidates on the same ballot lists are left outside of the councils. Thresholds have a strong, but not deterministic, relationship to the allocation of SU council seats. There are two reasons for the discrepancies: Firstly, a small number of elections permit preferential votes alongside the votes on the lists. In these elections, students who received enough individual votes were treated as first-ranked (we only have very

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[13] The survey was carried out as a web survey. In total, 141 students participated in the survey, which implies a response rate of about 67 per cent. Background characteristics of survey participants resemble the total population. Thus, non-responses are not likely to bias the results to any important extent.

imperfect information on the number of votes on each individual). Secondly, candidates can forfeit their seat. In this case, the first-ranked of the remaining students is elected instead. Obviously, the frequency of individual votes and the probability of forfeiting may be correlated with important unobserved characteristics. For these reasons, we use the relationship between ranking and threshold as an instrumental variable for actually acquiring a seat; i.e., we make use of the "fuzzy RD design", which is the standard way to handle self-selection on the margin in a set-up such as this (see e.g., Lee and Lemieux 2010).

Our models include list fixed effects throughout, since there may be systematic differences between candidates across ballot list. Lists which gain many council seats may, for instance, have relatively more (or less) motivated candidates than others. By including the list fixed effects we only draw inferences from differences between candidates *within* the same lists.

Formally, we run the following two stage regression:

$$Elected_{i,l} = \alpha_l^1 + \delta Above_i + \varphi^1(Rank_{i,l} - T_l) + \mu^1(Rank_{i,l} - T_l)Above_i + X_i\beta^1 + \varepsilon_i^1$$

$$Outcome_{i,l} = \alpha_l^2 + \gamma Elected_i + \varphi^2(Rank_{i,l} - T_l) + \mu^2(Rank_{i,l} - T_l)Above_i + X_i\beta^2 + \varepsilon_i^2$$

(1)

We let *Outcome* denote our different variables measuring status on the labor market for individual *i*, *Rank* is the list ranking in the SU council election,[14] *T* is the list-specific threshold (defined by the number of seats allocated to the list), *Above* is a dummy variable taking the value one if the individual is placed above the threshold, *Elected* is a dummy variable for being elected into the SU council, α is a fixed effect for each list (denoted by *l*) and, finally, there is an error term for each stage. The parameter of interest is γ which equals the effect of being elected on the outcome measures. We also add a set of individual level covariates (capturing age, sex, immigration status, duration of studies and previous work experience) denoted by *X,* to increase the precision of the estimates.

Most of our outcomes are binary and the models are therefore estimated as linear probability models, following standard procedures in the RD literature. Throughout, we

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[14] In the analysis, variables measuring the direct impact of list rankings using various functional forms are used. *Equation 1* describes the version which allows for a linear term that is different on different sides of the election threshold.

rely on standard errors that are robust to heteroscedasticity and clustered to account for repeated observations at the individual level.[15]

3.2 Data

We have gathered archive data from the three Swedish student unions, including information on SU council elections between 1982 and 2005. We recorded party list names, candidate rankings and social security numbers (*personnummer*) of individual candidates, as well as election results and indicators for taking up the seat in the council and for representation on the SU board. We use information for the somewhat scattered set of years (see the Appendix for details, *Table A1*) for which we could find information. Raw data cover 30 different SU elections (5,154 candidates). Using the social security numbers, *Statistics Sweden* has matched our SU data onto national registers on basic demographics (sex, age and immigration status), some basic educational information and labor market outcomes such as employment and earnings during 1985-2010.

We exclude the few SU election lists where none or all of the candidates were elected, since we only have candidates on one side of the threshold within those lists. Candidates whose rank is higher than the total number of seats available in the SU council are also excluded throughout since they have a zero probability of becoming elected. Lastly, to focus the analysis on a sample of reasonably inexperienced students, we remove candidates older than 40 (removing only 60 observations, without affecting the results). The final data set has a patchy coverage, but it is fairly evenly distributed across universities and years (see *Table A1* in the appendix).

Descriptive statistics of SU council candidates above and below the election thresholds are portrayed in *Table 1*. The first two columns include all candidates, whereas the last two columns focus on our main used sample consisting of the five candidates closest to the thresholds. Candidates are considered to be above the threshold if their list rank is at least as high as the number of seats allocated to their list.

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[15] Our data contains some individuals appearing more than once. Thus, standard errors are clustered at the level of the individual in order for the statistical inference to be correct. But since we have an incomplete account of each candidate's SU council participation record, our model assumes that the impact of being elected is independent of previous experiences (i.e. it is assumed to be additive).

Table 1. Descriptive statistics

	All (irrespective of ranking)		Closest 5 (main sample)	
	Above threshold	Below threshold	Above threshold	Below threshold
Individual data				
Candidate age	24.4	24.7	24.3	24.4
Women	0.410	0.383	0.393	0.400
Immigrants	0.075	0.067	0.076	0.063
Elected to SU council (main independent variable)	0.891	0.035	0.881	0.084
N (observations)	1,257	3,897	919	1,251
Number of unique individuals	843	2,731	687	1,031
Lists (parties per year and university)				
Number of included candidates per list	4.3	13.4	3.2	4.3
Total number of lists	290	290	289	289
Election cohorts (year and university)				
Average number of lists per election	9.7	9.7	9.6	9.6
Number of elections	30	30	30	30

Note: The data on the left-hand side exclude SU council candidates with a ranking above the number of available seats in the SU council.

Table 1 shows that there are 30 election cohorts (year×university). On average, there are approximately ten lists participating in each election. Candidates above and below the thresholds are similar in terms of mean age (24 years) and the proportion of immigrants (0.07) and women (0.40). The sample restricted to students closer to the election thresholds does not diverge from the full sample to any noticeable extent. For reasons discussed above, candidates above the threshold do not always acquire their seat in the council. Yet, the threshold has a huge impact on who enters the council; the proportion increases from 0.08 if falling below the threshold to 0.88 if being placed above it.

Two main outcome variables are examined in the paper: *employment* and whether the individual has as *well-paid job*. Employment is measured each year in November, and we use data from one year (t-1) before participating in SU council elections to five years (t+5) after the election. If the individual earned at least as much as the median 30-year old person with a college education, he or she is considered to have a well-paid job. We also present results for the probability of earning above the 75th percentile.

Figure 1. Fraction of SU council candidates in employment and having a well-paid job relative to the year of the SU council election
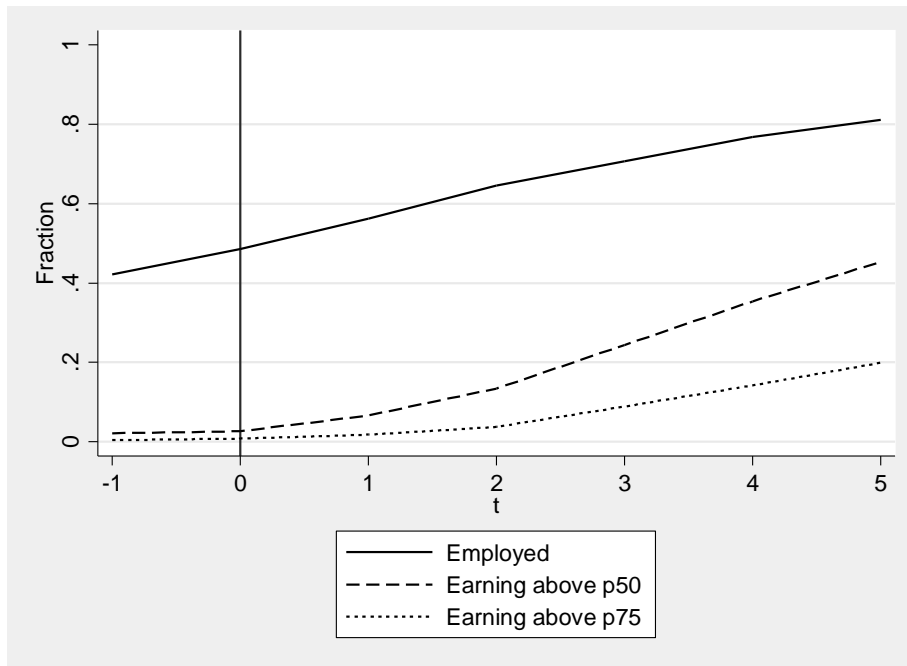

Figure 1 shows that almost 60 per cent were employed one year after participating in the SU council election. Very few, about 5 per cent, earned more than the median 30-year old person with a college education at that initial stage in their career. This is not surprising since the students are 24 years old on average when running in the SU elections. Using a higher earnings threshold (the 75 percentile) instead, we see that very few reached this level until at least 3 years after the election. But, not surprisingly, the candidates have very positive career trajectories. Five years after the SU elections, more than 80 per cent had a job, while around 45 per cent had reached the median and 20 per cent the 75 percentile of 30-year old college graduates.

3.3 The validity of the RD approach

Our analysis relies on the key assumption that there are no systematic differences in abilities or skills between candidates across the threshold, apart from those that are captured by the rank variables. This assumption will be valid if the parties are unaware of how many seats they will acquire when they compile the lists. As noted by Lee and Lemieux (2010), candidates can only self-sort exactly around the threshold if it is known beforehand.

Importantly, there are three very strong reasons for why the election thresholds in our particular setting are very difficult to predict: First, party lists are constructed, and votes are cast, with very little information. When lists are compiled, student are unaware of whom their competitors will be, this concerns both the nature and number of competing parties. Students come and go, which generates large variation in the sets of candidates from year to year within more stable parties, and there are frequent entries and exits of parties. Thus, it is not possible for the parties to exactly monitor changes in their competition, changes in voting students' preferences, and foresee the effect on such changes on their expected number of seats. An absence of opinion polls also makes it very hard for the electorate to predict the voting patterns of other students before voting.

Secondly, the number of seats per party is very volatile between years. *Figure 2* displays the distribution of new seats in the SU elections. Only a low 13 per cent of the lists received the same number of seats in two consecutive elections. This means that only one in eight seats in the councils were allocated to a list that remained at a stable representation since the last election. The pattern reflects both a frequent entry and exit of parties between elections, and very volatile voting patterns.

Figure 2. Distribution of new seats among lists receiving seats in SU council elections
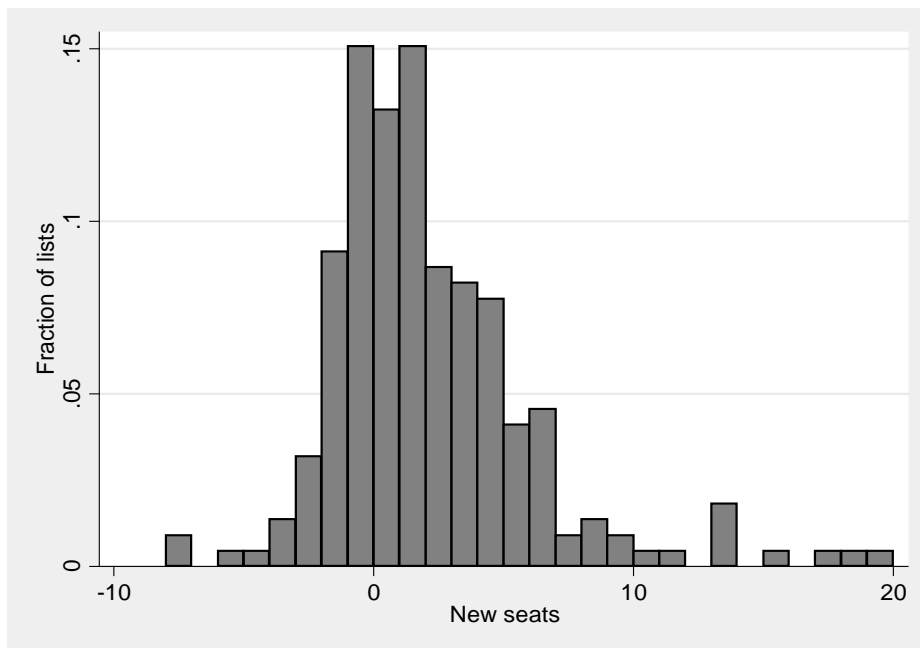
Note: Excludes first years in data sequences, N=219, whereof 82 are new lists.

Lastly, we have conducted a survey among recent SU council candidates at one Swedish university (see section 2.2). This survey demonstrates that SU council candidates themselves are unable to predict whether they will be elected or not, prior to SU elections. Once we removed candidates very far from the thresholds, the respondents were wrong almost as often as they were right when predicting the outcome (and many declared that they could not make a prediction).

As is standard in the RD literature, we have also tested the prediction that bare winners and bare losers, on average, have identical observed characteristics after controlling for the rank-distance to the threshold. If our assumptions are valid, this should be the case. This is examined by estimating equation (1) using observed predetermined characteristics as outcome variables.

Table 2. Validity test: Estimates of equation (1) using predetermined characteristics as outcome variables

	Female	Immigrant	Age	Years since start of studies	Employed the year before
Estimate	-0.009	-0.022	0.012	0.254	0.008
	(0.394)	(0.064)	(0.034)	(0.318)	(0.065)
Mean dep. var.	24.36	0.397	0.0687	3.692	0.421
N	2,170	2,170	2,170	2,167	1,847

Notes: Estimates based on equation using the threshold as an instrument for being elected into the SU council. All models include list fixed effects. The model does not include any additional covariates and corresponds to the first column in *table 3* below. The sample includes the first five candidates on each side of the threshold. The last two columns have slightly fewer observation,s due to missing values on the outcome variables. Standard errors are clustered at the individual level and robust to heteroscedasticity. * = sign. at < 0.10. ** = sign. at < 0.05. *** = sign. at < 0.01.

Table 2 shows that neither sex, immigration status, age, duration of studies nor work experience during the year prior to the SU election changes significantly at the threshold.[16] All point estimates are also small compared to the means of the variables. An F-test for the joint significance of the variables in a regression towards the instrument shows that they also are jointly insignificant with a p-value of 0.76. Furthermore, including all the variables in the main analysis has no impact on the results as shown below. [17]

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[16] These variables constitute a nearly exhaustive list of the predetermined variables that are available in our data. In our companion paper (Lundin et al., 2013), we also show that the final variable, capturing the probability of having participated in a regular election before, also is balanced at the threshold.
[17] Another common test in RD studies is to analyze the number of observations on the two sides of the threshold, but these are equal by definition in our case (a figure is nevertheless supplied in the appendix).

3.4   The first stage

Our main analysis uses the IV model outlined in section 3.1. As already indicated by *Table 1*, the first stage is very strong. The relationships between rankings and thresholds and the probability of being elected are depicted in *Figure 3* below. It shows that there is a major jump in the election probability exactly at the threshold. The fact that there is a small deviation from the linear predictions of the ranking variable for the closest observations is consistent with the fact that preferential votes on individual candidates can push the last candidate across the threshold, while forfeited seats are allocated to the last candidate below the threshold.

Figure 3. The first stage relationship – probability of being elected as a function of distance to threshold (ranking).
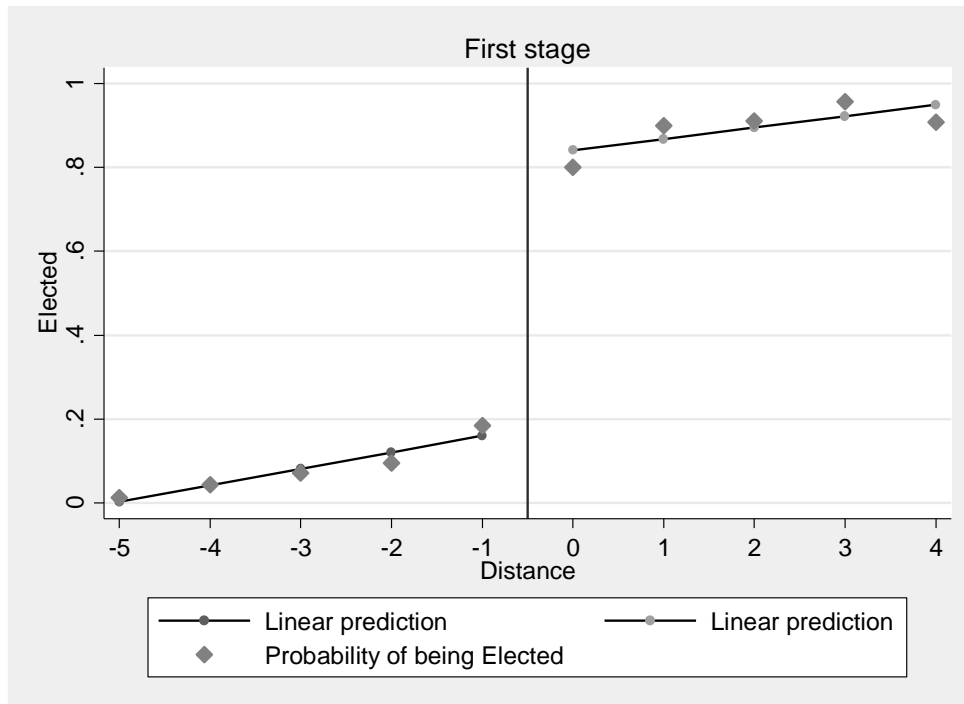

4   Results

We present the results in two subsections. We start with labor market entry and then turn to the subsequent career trajectories.

4.1   Labor market entry

We first use the model of equation (1) to estimate the impact on the probability of employment; for completeness we show results for years before and after the SU-election.

Figure 4. The impact of becoming a student union candidate on employment up to five years after SU council elections
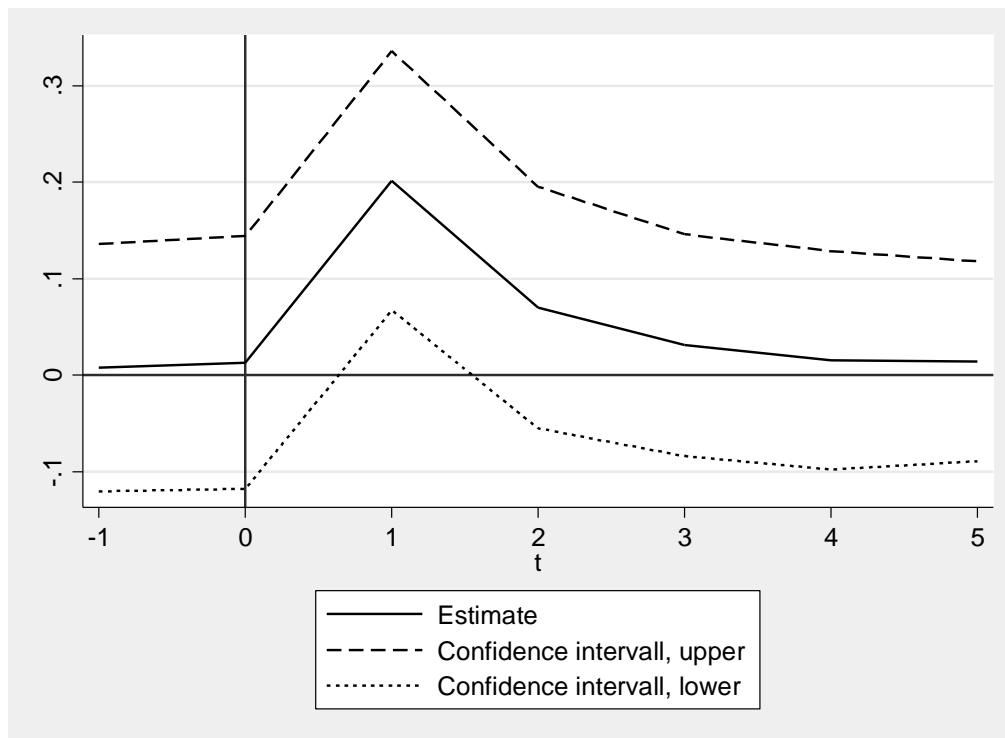
Notes: The figure depicts regression coefficients (and 95 % confidence intervals) from instrumental variables models (see equation 1) from *t*-1 to *t*+5, where *t* is the year when the student participated in the SU council elections. The Y-axis indicates estimates of how the probability of being employed changes if the student gets elected.

As expected, the results in *Figure 4* show no effects during the year before the election or during the year of the actual participation in the council. More importantly, the figure clearly shows that becoming a student representative in a SU council has a positive impact on employment during the first year after the SU election (hence, directly after the SU council term). Candidates who were marginally elected to a SU council were 20 percent more likely to be employed on year after (starting from a baseline of about 45 percent). As demonstrated in *figure 1* above, this is a phase of very fast career progression and the effect therefore wares of rapidly as other students catch up. Hence, the effects on employment probabilities are statistically insignificant during the 2-5 years after the election. With the exception of year 2 (where estimates suggest

an elevated employment probability of around 10 percentage points) the point estimates also remain fairly small after the initial year.

In order to assess the robustness of the estimated first-year impact, *table 3* shows detailed results from various models, consistently using employment during the year after the election as the outcome. Column (1) shows the estimates from a simple model which only captures the impact of rankings with a common linear term. Column (2) adds an interaction between the ranking and the threshold. Column (3) presents our preferred model (corresponding to *figure 4* above) which also includes controls for demographics, duration of studies and pre-election employment. In column (4), we enter the ranking variables with quadratics (separately above and below). In column (5), we revert to the model of equation (1), but only use a narrower sample window of the three closest candidates. Finally, column (6) only relies on the two truly marginal candidates within each list (hence, without controlling for the ranking).

Table 3. The impact on employment one year after the SU election: Robustness

	1	2	3	4	5	6
Estimate	0.193***	0.201***	0.198***	0.257*	0.218**	0.146*
	(0.068)	(0.069)	(0.066)	(0.135)	(0.097)	(0.078)
N	2,106	2,106	2,106	2,106	1,416	522
Sample, closest:	5	5	5	5	3	1
Covariates	No	No	Yes	Yes	Yes	Yes
Ranking	Yes	Yes	Yes	Yes	Yes	No
Ranking*above threshold	No	Yes	Yes	Yes	Yes	No
Quadratic*above	No	No	No	Yes	No	No

Notes: Estimates are from instrumental variables models (see equation 1) using the threshold as an instrument for being elected to the SU council. Standard errors (within parentheses) are clustered for repeated observations at the individual level and robust to heteroscedasticity. * = sign. at < 0.10. ** = sign. at < 0.05. *** = sign. at < 0.01.

The results are robust to the inclusion or exclusion of covariates, to changes in the functional form of the rank-controls (linear, split, quadratic) and to narrowing the sample bandwidth to the closest 3 candidates, or to focusing on the truly marginal candidates. The most noticeable differences across the columns is that the statistical precision is reduced (only significant at the 10 percent level) when including the quadratic term and that the estimate is somewhat smaller when focusing on the two marginal candidates.

Figure 5 shows the reduced form relationship non-parametrically after removing the list fixed effects, but without accounting for covariates. Clearly, this relationship appears to be reasonably linear away from the threshold. There do appear to be some added noise for students who were elected with a broad margin (i.e., with a distance of 3 and 4), but this is not surprising since there are fewer observations at these points (see the density plot in the appendix, *Figure A1*).

Figure 5. Reduced form relationship between list rankings and first-year employment.
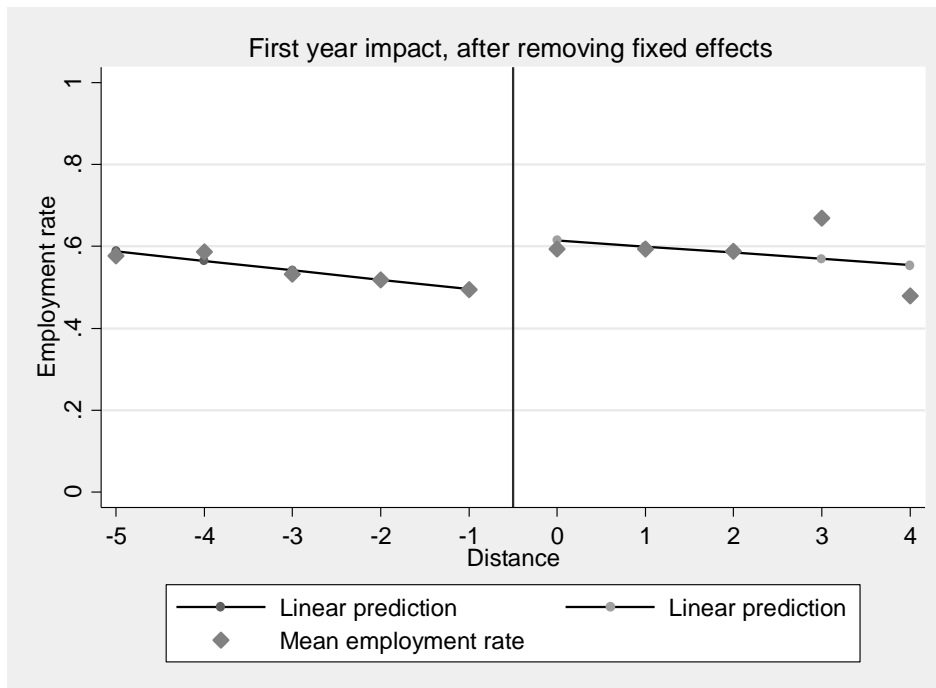
Having showed that the results are insensitive to model specification and to size of the sample window, we perform a second set of analyses. Here we focus on *where* the student candidates find employment. In particular, we are interested in whether the positive impact on employment is driven by access to certain establishments, organizations or industries where the skills or networks of student representatives are likely to be particularly relevant. In order to analyze this hypothesis without relying on speculation regarding which these segments may be, we rely on data on the actual working patterns of previous student representatives.

The results are presented in *table 4*. In the first column, we use a dummy that takes the value 1 for students who find employment at an establishment where a previous

elected representative already is employed. The dummy takes the value zero if this is not true, regardless of whether the student is employed or not. The second column replicates this analysis using the number of previously elected representatives within the establishment as the outcome (again, taking the value zero also if non-employed). None of these exercises indicate that jobs are found at establishments with a specific need for previous student representatives (the means of the dependent variables are displayed in the table). Column (3) replicates the analysis of Column (2) at the firm or organizational level. An organization can, for example, be a government agency, a municipality or a university. Again, the estimates are both small and insignificant. Finally, we re-estimate the model at the (2-digit) industry level with similar results, although the point estimates now are much larger since the mean baseline probability of ending up in the industry is much larger for natural reasons. The final column instead shows the impact on working with non-elected previous candidates and the result give a similar picture.

Table 4. The impact on the probability of following in the tracks of former student representatives

	Previous student representatives within the establishment/firm/industry of entry				
	At least one previously elected within establishment	# of previously elected within establishment	# of previously elected within firm/organization	# of previously elected within 2-digit industry	# of previous candidates, *excluding elected*, within 2-digit industry
Estimate	0.009	0.028	0.068	1.467	5.998
(s.e.)	(0.029)	(0.040)	(0.194)	(1.405)	(4.404)
Mean dep. variable	0.057	0.075	0.541	7.547	24.23
N	2,106	2,106	2,106	2,106	2,106

Notes: The outcomes are measured in the year following the election. The dependent variable is calculated using elected candidates during the previous 5 years, excluding those who also ran during the relevant (election) year. Estimates are regression coefficients from instrumental variables models (see equation 1) using the threshold as an instrument for being elected to the SU council. All models include list fixed effects. The sample consists of the five candidates closest to the threshold. Standard errors (within parentheses) are clustered for repeated observations at the individual level and robust to heteroscedasticity. * = sign. at < 0.10. ** = sign. at < 0.05. *** = sign. at < 0.01.

Overall, we interpret the results from this analysis as suggesting that the benefits of being a student representative are of a general nature. This does, of course, not exclude

the possibility that the benefits arise through labor market networks, but if so, these networks need to be widely dispersed and not pertaining to certain firms or industries.[18]

A final set of analyses on labor market entry deals with issues of heterogeneity. *Table 5* shows the results of analyses on different subsamples. First, we divide the sample according to previous employment, splitting the sample into those employed versus not employed during any of the past three years. *Table 5* shows that the results primarily appear to be driven by the subset of students who lack previous work experience; the estimate is almost twice as large in this group compared to the group of students who have worked before (0.24 vs. 0.13).[19] This suggests that experiences as a student representative may be a substitute for work experience, for example by providing similar types of references or signals of traits or abilities. Taken literary, the result suggests that human capital explanations would require that being a student representative primarily adds human capital to those without work experience, which could be true if the learning curve is heavily concave.

Table 5. The impact on employment one year after the SU election: Heterogeneity

	Baseline	Employed before		Years since start of studies		Sex	
		No	Yes	3 or less	4 or more	Male	Female
Estimate	0.198***	0.248*	0.137	0.171*	0.205*	0.204**	0.236*
	(0.066)	(0.140)	(0.087)	(0.100)	(0.108)	(0.083)	(0.129)
N	2,106	676	1,163	1,110	993	1,268	838
Mean dep.var.	0.562	0.428	0.641	0.506	0.624	0.553	0.575
Sample, closest:	5	5	5	5	5	5	5
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ranking	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ranking*above threshold	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates are from instrumental variables models (see equation 1) using the threshold as an instrument for being elected to the SU council. All models include list fixed effects. The sample consists of the five candidates closest to the threshold. Standard errors (within parentheses) are clustered for repeated observations at the individual level and robust to heteroscedasticity. * = sign. at < 0.10. ** = sign. at < 0.05. *** = sign. at < 0.01.

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[18] To verify our conclusions, we have also estimated whether the students end up in universities or in NGO:s, but again finding no significant effects.
[19] The result does not appear to be driven by labor market conditions. When exploring how the estimates differ depending on aggregate unemployment we find (if anything) larger effects during years of lower unemployment.

The next two columns of *table 5* focus on the elapsed duration of studies. We find that the impact of being a student representative on labor market entry is significant, with similar point estimates, both for those who have a maximum of three years' of prior university studies and those who have at least four years of university studies before the election. This is consistent with the notion of a large variation in the duration of studies at Swedish universities and university colleges discussed in section 2.1. The final two columns show that the impact is very similar for men and women.

4.2   Career trajectories

The results in the previous section suggested that the effects on employment were large, but transitory in nature as other students are catching up within a year or two. In order to study longer run effects it is therefore necessary to focus the attention towards more ambitious career outcomes. In order to analyze whether being elected as a student representative have an impact on the career trajectories of the students, we analyze the impact on the probability of finding a well-paid job, which we proxy by a dummy for earning at least as much as the median 30-year old person with a college education during that particular year. As these students on average are 24 year old when running, it takes time to reach this target in most cases.

In the analysis we thus let the dummy for holding a well-paid job replace employment as the outcome and repeat the analysis of the previous section. *Figure 6* shows the impact on this target using the same time interval. The analysis generates a similar temporary effect as for employment, but at a slightly later moment. There is a significant impact of serving as a student representative on the probability of holding a well-paid job three years after the election. Thus, a student council experience is useful for getting a job one year after the SU election, and on the probability of holding a well-paid job three years thereafter. But as with the employment, the effect appears to wear off as other students catch up.

Figure 6. The impact on the probability of holding a well paid job.
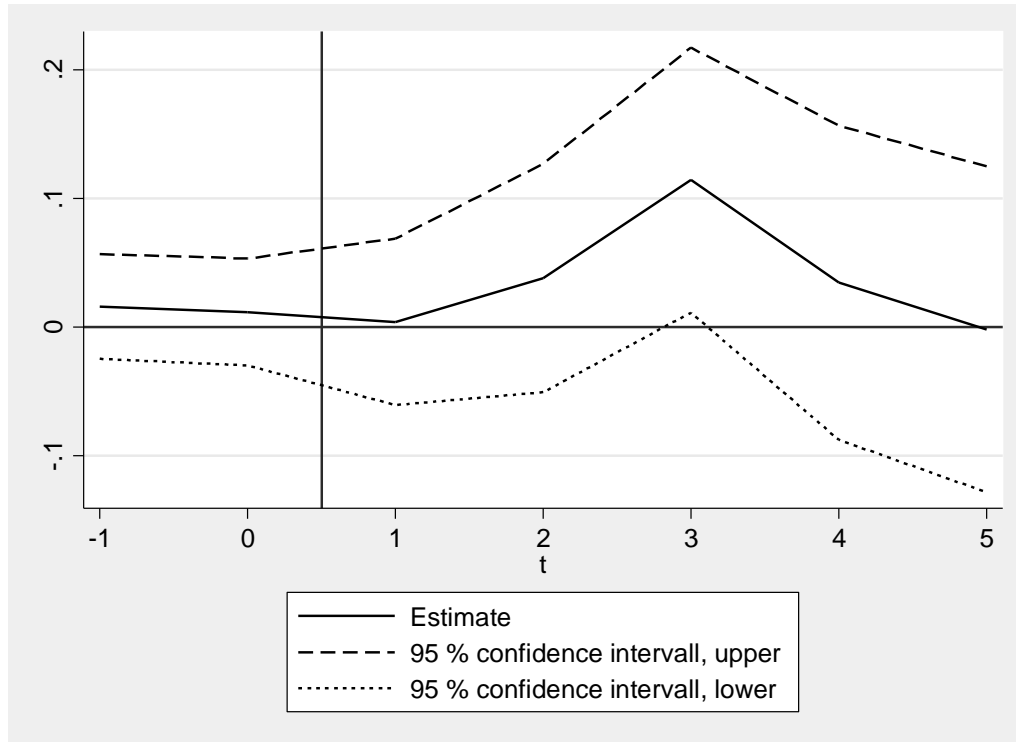
Notes: The figure depicts regression coefficients (and 95 % confidence intervals) from instrumental variables models (see equation 1) from *t*-1 to *t*+5, where *t* is the year when the student participated in the SU council elections. The Y-axis indicates estimates of how the probability of being employed changes if the student gets elected.

In *Table 6* we show that the effect during the third year is stable across the same set of variations as in the previous subsection (the nonparametric reduced form relationship for the third year is shown in the appendix). We have also explored using an even higher earnings threshold (the 75th percentile) in order to see whether the impact continues to be important, but at even higher levels. Since virtually no students achieve this earnings level within less than three years after the election, we then focus on a slightly later time period (3-8 years after, the impact is insignificant in the years before for obvious reasons). The effects are statistically insignificant throughout when using our baseline model, although there are some indications of positive effects during year five, but also these effects appear to decline thereafter.[20] Overall, we interpret the evidence as suggesting that the positive impact lasts beyond the initial employment stage, but our interpretation is also that the impact appears to wear off over time.

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[20] In the appendix we also show that the estimates after five years are statistically significant in some of the tighter specifications and display the nonparametric relationship during the fifth year.

Table 6. The impact on the probability of holding a well-paid job after three years: Robustness

	1	2	3	4	5	6
Estimate	0.115**	0.122**	0.114**	0.207**	0.184**	0.103*
	(0.054)	(0.054)	(0.053)	(0.104)	(0.074)	(0.057)
N	2,139	2,139	2,139	2,139	1,441	532
Sample, closest:	5	5	5	5	3	1
Covariates	No	No	Yes	Yes	Yes	Yes
Ranking	Yes	Yes	Yes	Yes	Yes	No
Ranking*above threshold	No	Yes	Yes	Yes	Yes	No
Quadratic*above	No	No	No	Yes	No	No

Notes: Estimates are from instrumental variables models (see equation 1) using the threshold as an instrument for being elected to the SU council. Standard errors (within parentheses) are clustered for repeated observations at the individual level and robust to heteroscedasticity. * = sign. at < 0.10. ** = sign. at < 0.05. *** = sign. at < 0.01.

As a final analysis, aiming to validate this conclusion, we have also used log income as the dependent variable. Given the large impact we find on employment, we have performed the analysis using the log of total earnings aggregated over multiple years to reduce the importance of observations with zero earnings. The results, which are presented in *Table 7*, show a large impact on log earnings during the three years after the election, but insignificant differences in the periods before and after that.[21]

Table 7. The impact on ln(Earnings)

	Impact on ln(Earnings) before and after		
	1-2 years before	1-3 years after	4-6 years after
Estimate	0.061	0.335**	-0.060
	(0.178)	(0.148)	(0.144)
N	1,622	2,045	1,870
Sample, closest:	5	5	5
Covariates	Yes	Yes	Yes
Ranking	Yes	Yes	Yes
Ranking*above threshold	Yes	Yes	Yes
Quadratic*above	No	No	No

Notes: Estimates are from instrumental variables models (see equation 1) using the threshold as an instrument for being elected to the SU council. All models include list fixed effects. The sample consists of the five candidates closest to the threshold. Standard errors (within parentheses) are clustered for repeated observations at the individual level and robust to heteroscedasticity. * = sign. at < 0.10. ** = sign. at < 0.05. *** = sign. at < 0.01.

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[21] See the appendix for robustness checks of the estimate in the mid column.

5   Conclusions

In this paper, we have examined the impact of participating in extracurricular activities (ECA) as a student representative during college years on key measures of labor market performance and early career outcomes. Relying on an RD design we add to a very scarce set of paper that has been able to provide credible evidence of how participation in ECA affects student outcomes.

The results show that becoming a student representative causally increases the probability of a rapid transition to work. The probability of being employed one year after participating in a SU election increases with about 0.2 from a baseline of 0.45 if a candidate manages to get elected. The impact is found to be robust to a wide set of modifications of the RD-model. This effect is not confined to segments (workplaces, firms or industries) of the labor market where previous representatives are employed, suggesting that the benefits are general in nature. We also show that the effects appear to be somewhat larger amongst students who do not have prior work experience, which indicates that participating in leadership activities within the councils serves as a substitute for working during college years. Furthermore, elected representatives have a statistically increased probability of holding a well-paid job within three years. However, although our findings suggest that the initial career trajectories are enhanced by a positive election outcome, the effects do not appear to be permanent. Students who did not become managed to catch up within a few years.

The research design and data does not allow us to provide any sharp conclusions regarding the underlying mechanisms of why leadership activities during college affect early career trajectories. It is not unlikely that participation in the councils helps students to accumulate leadership skills as well as signal preexisting skills in these dimensions and generate useful labor market networks. However, for various reasons (none of which are uncontestable) we interpret the evidence as being most readily reconciled with an interpretation in terms of signaling. In particular we note that the impact is short-lived and, as argued in the employer learning literature, the value of an initial signal should decrease as workers accumulate labor market experience.[22] Along the same lines, the results appear stronger among students who lack previous work

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[22] Note that the RD analysis explicitly compares students who receive different signals despite being equally skilled ex ante. See Feng and Graetz (2013) for an explicit analysis of signaling effects during labor market entry after college using am RD-design.

experience. We also note that the fact that the impact appears to be widely dispersed across different industries and firms suggest that the signals work beyond the immediate network generated through the councils.

Although we are unable to provide firm evidence on the exact underlying mechanisms, we do however believe that our main conclusion convey an important message: Participating in leadership-oriented extracurricular activities during college years may turn out to be a useful investment.

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Appendix – additional tables and figures

Table A 1. Number of observations by year and university

Year	Lund	Stockholm	Uppsala	Total
1982	0	0	54	54
1983	0	0	0	0
1984	172	0	0	172
1985	93	0	0	93
1986	128	0	0	128
1987	39	0	0	39
1988	50	0	0	50
1989	65	0	0	65
1990	72	0	0	72
1991	70	0	0	70
1992	64	0	0	64
1993	44	0	0	44
1994	0	0	0	0
1995	0	74	0	74
1996	0	74	0	74
1997	0	87	40	127
1998	0	71	60	131
1999	0	85	51	136
2000	0	94	62	156
2001	0	75	55	130
2002	0	92	46	138
2003	0	94	69	163
2004	0	65	41	106
2005	0	84	0	84
Total	797	895	478	2,170

Note: The table concerns the used data.

Table A 2. The effects on high wage employment (above 75th) five years after: Robustness

	1	2	3	4	5	6
Estimate	0.081	0.082	0.076	0.211**	0.145*	0.138**
	(0.054)	(0.054)	(0.053)	(0.102)	(0.076)	(0.061)
N	2,028	2,028	2,028	2,028	1,365	504
Sample, closest:	5	5	5	5	3	1
Covariates	No	No	Yes	Yes	Yes	Yes
Ranking	Yes	Yes	Yes	Yes	Yes	No
Ranking*above threshold	No	Yes	Yes	Yes	Yes	No
Quadratic*above	No	No	No	Yes	No	No

Notes: Estimates are from instrumental variables models (see equation 1) using the threshold as an instrument for being elected to the SU council. All models include list fixed effects. Standard errors (within parentheses) are clustered for repeated observations at the individual level and robust to heteroscedasticity. * = sign. at < 0.10. ** = sign. at < 0.05. *** = sign. at < 0.01.

Table A 3. The effects on log earnings 1-3 years after: Robustness

	1	2	3	4	5	6
Estimate	0.336**	0.367**	0.335**	0.456	0.403*	0.272
	(0.151)	(0.154)	(0.148)	(0.295)	(0.211)	(0.176)
N	2,045	2,045	2,045	2,045	1,376	508
Sample, closest:	5	5	5	5	3	1
Covariates	No	No	Yes	Yes	Yes	Yes
Ranking	Yes	Yes	Yes	Yes	Yes	No
Ranking*above threshold	No	Yes	Yes	Yes	Yes	No
Quadratic*above	No	No	No	Yes	No	No

Notes: Estimates are from instrumental variables models (see equation 1) using the threshold as an instrument for being elected to the SU council. All models include list fixed effects. Standard errors (within parentheses) are clustered for repeated observations at the individual level and robust to heteroscedasticity. * = sign. at < 0.10. ** = sign. at < 0.05. *** = sign. at < 0.01.

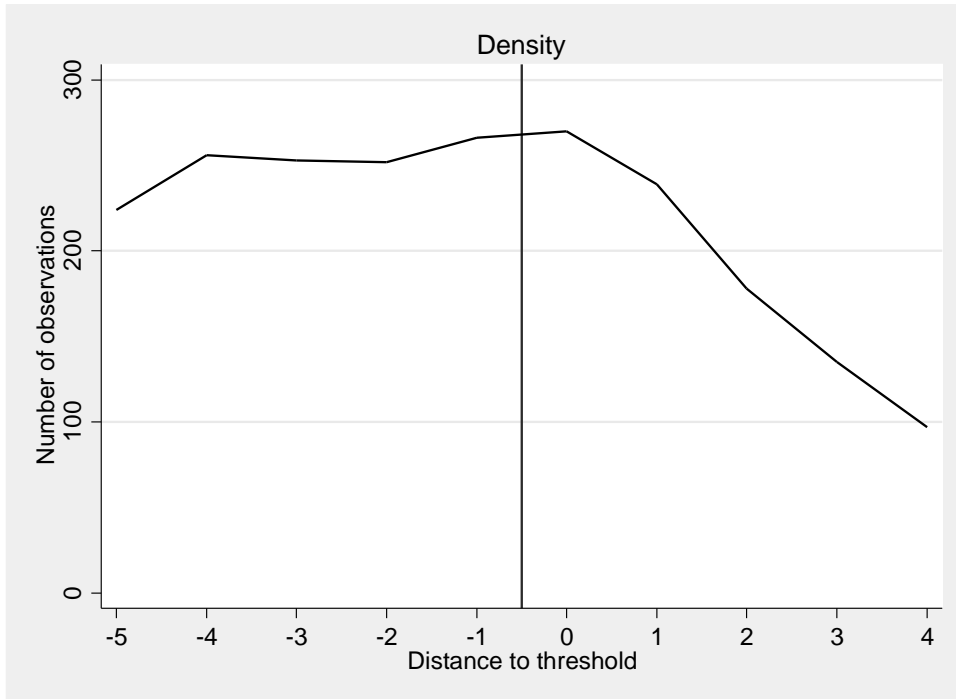Figure A 1. The number of observation by rank relative to the threshold

Figure A 2. Reduced form relationship between list rankings and the probability of holding a well-paid job (main definition) after three years.
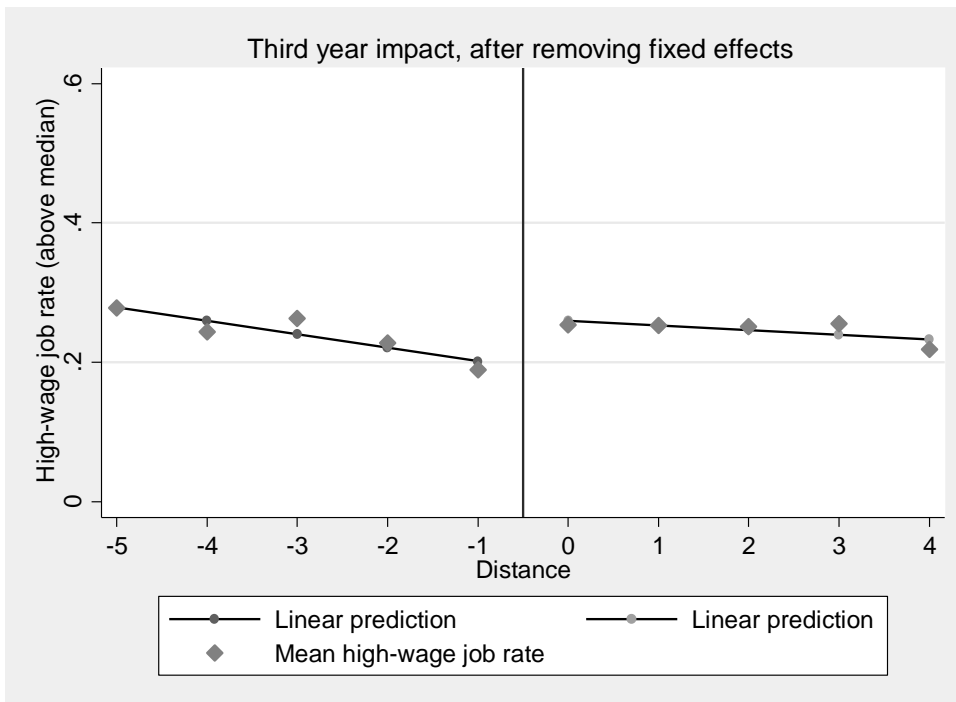
Figure A 3. Reduced form relationship between list rankings and the probability of holding a well-paid job (above 75th percentile) after 5 years
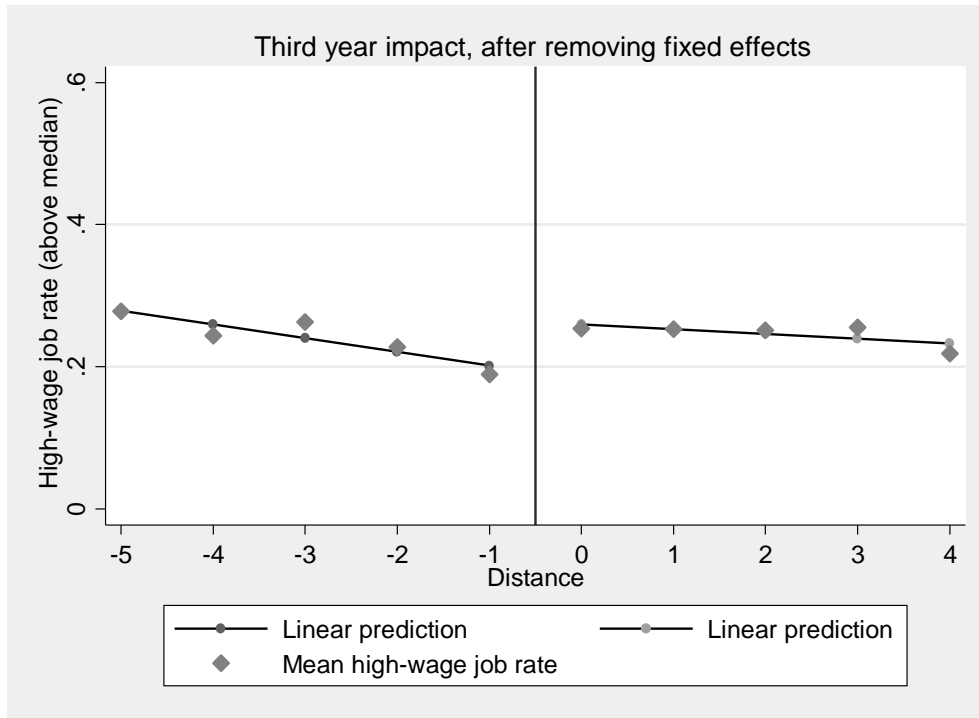

Figure A 4. The impact on the probability of holding a well-paid job (above 75th percentile)