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Leadership Experiences, Labor Market Entry, and Early Career Trajectories

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DISCUSSION PAPER SERIES

IZA DP No. 11434

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MARCH 2018

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ABSTRACT

Leadership Experiences, Labor Market Entry, and Early Career Trajectories

Matching archive data on election discontinuities to register data on labor market trajectories we estimate the causal effects of being elected into Swedish student union councils on subsequent labor market careers. Marginally elected students are much more likely to have a rapid transition into employment. Effects are not confined to establishments, organizations or industries where previous candidates are employed, suggesting that the benefits are general in nature. Elected representatives have higher labor earnings within three years, but not thereafter. Overall, leadership experiences before labor market entry boost individuals' early careers, whereas mid-term outcomes are unaffected.

JEL Classification: I26, I23, J24

Keywords: extracurricular activities, earnings, higher education

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1. Introduction

It is a popular and widespread perception that young individuals can improve their early labor market prospects by holding a leadership position before entering the labor market. Examples of such activities include editing school newspapers, coaching children in sports, or serving on the board of youth organizations. Previous studies lend some support to this idea; most notably, Kuhn and Weinberger (2005) show, without making strong causal claims, that high school team captains and club presidents on average have higher future earnings and are more likely to become managers than other high school students. In this paper, we contribute to the literature by presenting what we believe to be the first study of the causal effect of civilian leadership activities during college years on the school-to-work transition phase and later labor market outcomes.

Our paper makes use of unique data on student union (SU) elections at three major universities in Sweden. We use regression discontinuity (RD) techniques (with a discrete running variable) to isolate differences between closely ranked winners and losers in elections to these SU councils. In contrast to those who just missed being elected to the SU councils, students who acquired a position got high-profile and well-recognized leadership experiences.¹ Our outcomes are measured by matching SU election data to data from population-wide registers on labor market outcomes over time.

Our research strategy has several benefits: The use of register data allows us to move beyond snapshots and assess the dynamic impact of leadership experiences over time. Moreover, we can use various measures (based on employment indicators

¹ A simple Google search on “student representative” or similar terms returns numerous university websites in various countries 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). In addition, previous research suggests that students who participate in extracurricular activities do so partly to improve their labor market prospects (see e.g. Roulin and Bangerter 2013).

and earnings measures) to characterize the impact on the career trajectories of the students. Finally, and most importantly, the election discontinuities allow us to handle self-selection problems and isolate the causal component of the association between leadership activities and labor market outcomes.

Leadership experiences during school years may affect subsequent labor market outcomes through several channels (and, as with most quasi-experimental studies, we will not be able to fully distinguish between them): Experiences acquired as a student representative may endow the individual with useful human capital such as leadership skills (see e.g. Kuhn and Weinberger 2005). The experience may also serve as a signal of pre-existing (non-cognitive) skills or traits to future potential employers. Previous research within the “employer learning” literature (building on Spence’s signaling theory) has, e.g., highlighted the role of uncertainty about worker skills at the time of labor market entry (e.g. Altonji and Pierret 2001; Lange 2007; Spence 1973).² This uncertainty could be reduced if students have documented experiences of leadership activities during their college years.³ Finally, being a student representative may provide useful social ties to high-ability students or university officials; prior research has shown that social networks play an important role in the school-to-work transition phase (e.g. Shue 2013; Kramarz and Skans 2014). It should also be noted that these three potential benefits—increased human capital, signaling, and networks—could potentially be counteracted by the crowding out of other useful activities (e.g. studying). A close parallel in terms of possible advantages and disadvantages exists in the literature on working while enrolled in

² An important finding in this literature is that the value of signals should deteriorate as the market acquires information that 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 and Skans 2013). Note also that Arcidiacono and colleagues find that higher education reduces the role of employer learning though market experience (Arcidiacono et al. 2010), but this conclusion has been contested (Light and McGee 2012).

³ As we will rely on RD identification between marginal winners and losers it is useful to note that signaling makes particular sense if employers are unable to separate out the marginal winners (who have no incentive to highlight that they were in fact marginal) from other winners, and marginal losers (who may be unable to provide a credible signal of being marginal) from other losers.

high school or at college.⁴ All of this is centered on the demand side, but it is also conceivable that participation in leadership activities affects the supply decision through a shift in preferences, confidence or as a response to the attractiveness of available job offers.

Our study of leadership experiences prior to labor market entry is clearly related to other research on extracurricular activities (ECA). This literature has mainly been developed within other disciplines than economics (e.g. sociology, psychology, and education).⁵ Within economics, the study of ECA has primarily focused on the impact on educational attainment (e.g. Barron et al. 2000; Eide and Ronan 2001; Lipscomb 2007; Lozano 2008; Rees and Sabia 2010; Rouse 2012). Studies covering labor market outcomes include Henderson et al. (2006) using nonparametric regression techniques to assess the impact of being a college athlete on subsequent labor market success, finding a positive wage premium.⁶ A particularly compelling analysis is provided by Stevenson (2010), who examines the role of sports in spurring a 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.

The focus of this paper is on the impact of having a leadership position within ECA. To this end, we use data from a companion (political science) paper of ours (Lundin et al. 2016) where we showed that participation in SU councils increases the probability of running for office within regular politics later in life. For that paper, we

⁴ The literature includes findings of negative effects on educational achievement (e.g. Stinebrickner and Stinebrickner 2003; Häkkinen 2006) and indications of positive labor market effects, at least in the short run (e.g. Light 2001; Hotz et al. 2002; Häkkinen 2006; Geel and Backes-Gellner 2012).

⁵ The analyses 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 and Matjasko (2012).

⁶ Also related to our study is Grönqvist and Lindqvist (2016) who find positive long-run effects on labor earnings from officer training during compulsory military service after high school.

collected data on SU candidates and their election outcomes from three major universities (Lund, Stockholm, and Uppsala) between the years 1982 and 2005.⁷ These data are, for the current paper, matched to information from national registers on employment and annual earnings between 1985 and 2010.

The council elections follow a closed-ballot-list election system where groups of students (“student parties”) provide lists of pre-ranked candidates before the elections. The number of students elected as representatives from each list depends on the votes given to each list (all students are eligible to vote). Thus, candidates are admitted in accordance with their rank within each list, and closely ranked winners and losers will be separated by an election threshold. We use these within-list election discontinuities to identify the causal impacts of interest. Importantly, the lists are compiled with little knowledge regarding the competition from other parties, and without any election polls, suggesting that students will be unable to sort themselves relative to the discontinuity. Furthermore, we show that the pre-election uncertainty regarding the number of seats allocated to each list is substantial: only 13 percent of the lists receive the same number of seats as in the election the year before. In line with the presumption that students are unable to sort themselves exactly around the thresholds, we find no systematic differences between closely ranked winners and losers in terms of important observed and predetermined characteristics such as in-school employment and duration of studies.⁸

Our results show, first, that students who become student representatives in the councils have a markedly increased (20 percentage points) probability of being employed in the year after the end of the election term. The effect is robust to a wide set of possible specifications of the empirical model. However, the effect on the

⁷ 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.

⁸ Our empirical approach resembles recent studies on how participation in “regular politics” affects employment and earnings. Lundqvist (2013) uses Swedish data, finding no effects on subsequent earnings, and Kotakorpi et al. (2013) use Finnish data, finding a short-run positive effect on earnings. The focus of these studies is, however, very different from ours: they document the relationship between participation in national or sub-national parliamentary elections and labor market performance for adults, often approaching the end of their careers.

probability of being employed is short-lived as other students catch up within two years. We find no impact on the probability of being employed at workplaces, organizations, or industries where previous representatives are employed, suggesting that the benefits are valid across large segments of the labor market.

Second, we find that elected student representatives have higher annual earnings and are more likely to hold a well-paid job (defined as a job with a pay exceeding the median earnings among 30-year-old university graduates) within three years after the SU election than those who marginally failed to be elected. However, similar to the employment responses discussed above, these effects, although large in the short-run, vanishes over time.

Given the scarce (almost non-existing) previous evidence on the labor market effects of ECA in general, and leadership activities in particular, we believe that our causal analysis provides a major contribution to the literature. Our results suggest that becoming a student representative causally increases the probability of a rapid transition into the labor market without providing long-term benefits. Although admittedly not a sharp test, the fact that the benefits are transitory (of a duration similar to the learning curve estimated by Lange, 2007), together with the observation that the effects appear not to be confined to the specific segments of the labor market where previous candidates are employed (as they should be, if networks were the main driving force), suggests that the key benefit of holding a leadership position may be that it provides a credible signal of important pre-existing non-cognitive skills. But as we will discuss in the concluding section, we cannot rule out the possibility that there is a human capital mechanism linking leadership experiences in student unions to labor market entry and early career trajectories.

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 and also provides standard RD falsification tests. Section 4 presents the results regarding labor market

entry and career trajectories as well as results related to alternative outcomes such as academic achievement. Section 5 concludes the paper.

2. Institutional setting

2.1 Higher education in Sweden

In Sweden, public universities and university colleges 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 are usually between three and five years long, or combine single-subject courses into degrees. As a result, students (including those initially admitted to programs) tend to adjust the duration of studies, e.g., in response to the results of job-search activities. Therefore, the time it takes to complete a degree varies considerably between students. And so does the timing of completion of a degree: whereas most students complete their studies at the end of a semester, some students take out their degree in the middle of the academic year, or much later after finding a job. Information on enrollment and formal graduation dates are therefore much poorer proxies for where students are in the school-to-work process than one would expect from, e.g., a US perspective.

Almost all students support themselves through generous government grants (loans and allowances) that are available for up to six years for all students irrespective of parental income. As shown below, a large proportion of students add to their income by working while enrolled.

2.2 Student unions

Until July 1, 2010, it was mandatory for every student at a university or university college in Sweden to be a student union (*studentkår*) member. Accordingly, these organizations represent a sizeable population. While student unions exist in many

countries, their features differ across countries and settings (see e.g. Klemenčič 2012). In some countries, they mostly organize cultural and sports activities. In others, such as Sweden and the UK, student unions are more similar to interest organizations; that is, they act as “labor unions.” This means that they aim to influence decisions made by universities, municipalities, and the national government. Swedish student unions have formal representation in every decision-making body within the universities, including at faculty and department levels, having up to one-third of the seats in these bodies. The unions also provide their members with various services, such as judicial counseling, housing, and health counseling. Thus, they have some formal power and a fairly important position within the Swedish system of higher education. Notably, since all college-educated Swedes were mandated to be SU members, these organizations and their councils are well known to most potential future employers. It is also common knowledge that several prominent Swedish politicians (e.g. former Prime Minister Fredrik Reinfeldt), publicists, and other successful former students were active in SU councils during their student days.

2.2.1 The student unions in our sample

In our analysis, we use data from 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*). The reason why we focus on these is that they were the only unions with well-documented formal elections with preserved data on both winning and losing candidates.⁹ All three organizations have more than 15,000 members.¹⁰ Their

⁹ Smaller universities and university colleges often select their representative in less formal direct elections during meetings. The only other school where we could find proper data was Chalmers technical university which, however, has a very different election system (and very few marginal candidates) and we therefore decided not to include them in our analysis.

organizations consist of sub-branches at lower levels (faculty level, departments, etc.) of the university, where students organize to impact local decision-making. In each student union there is a “legislative” council and an executive body.

2.2.2 The elections

Council members are elected by their fellow students for one year at the time. Elections typically take place in the spring and elected members serve for one (academic) year starting July 1st. The electorate votes on “student parties” that present closed proportional representation (PR) lists with a pre-set ranking of council candidates. Candidates are typically screened, selected and ranked in internal (i.e. party specific) elections or by internal nomination committees. The thresholds for entering a list at a low ranked position is likely to be low (some lists have more candidates than there are seats in the councils), but it can often be difficult to acquire a high-ranked position.

Ballots with the predetermined lists 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 claim to represent 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. In some of the elections, it was also possible to alter the predetermined ranking by adding a vote for an individual candidate within the preferred list. If the share of individual votes for a candidate were high enough, the list ranking was altered *ex post*.¹¹ All of these elements closely mirror the Swedish public election system.

¹⁰ In 2010, Uppsala Student Union had about 33,000 members (the largest in Sweden) and Stockholm University Student Union had 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 no longer exists.

¹¹ A lower-ranked candidate (receiving more individual votes than a higher-ranked candidate) was only moved up in the ranking if his or her share of individual votes exceeded a predetermined threshold. As we show below, the initial rank is, however, a very good predictor of the final outcome.

2.2.3 What do council members do?

Within the councils, elected student representatives engage in activities such as debates and negotiations with other parties, and they make budget decisions and decisions about policy proposals as well as determine the allocation of funds to specific student-run projects. Council meetings are held about once a month, but in-between meetings, representatives generally spend a considerable amount of time (see below) initiating policy proposals, writing policy documents, and lobbying towards various organizations and authorities on the behalf of student interests.

2.2.4 What do students say about their motivations and experience?

We are interested in the labor market effects of participating in the SU councils, that is, of taking part in the processes of leading a quite large and significant organization during the college years. To get a sense of the students' own expectations and stated experiences, we performed a small-scale survey among all *candidates* in the SU elections at Uppsala University in 2011 and 2012 (i.e. later cohorts than our main sample) before their results were announced.¹² Many of these candidates believed that experiences from SU politics (if they were to be elected) would be important for their future professional careers. For instance, about one third of the candidates noted that professional career motives were a “fairly” or “very” important reason to run in SU elections, and almost two thirds reported that they believed that becoming a council member would improve skills and networks that are important on the labor market. We made a follow-up survey one year later. When asked about elected council members' (n=27) experiences at the end of their term, around 70 percent 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

¹² The survey was carried out as a web survey. In total, 141 students participated, which implies a response rate of about 67 percent. Background characteristics of survey participants resemble those of the total population. Thus, non-responses are not likely to bias the results to any important extent. Age and sex are the only variables we can use to compare the respondents in the mini-survey and the students included in the dataset used in our empirical analysis. The students are on average 24 years in both datasets. The female share is somewhat larger in the survey (52 percent) than in our data used in the empirical analysis (40 percent). This can most likely be explained by the fact that the mini-survey concerns a more recent time period.

survey also provides some information about what the treatment of being elected to a SU council really means. Council members state that they participated in activities such as debates and negotiations in the council and in various committees within the university. They initiated policy proposals, wrote reports and policy documents, and contacted external actors on behalf of the SU council. These council members also claimed to have devoted a lot of time to political activities: on average 12 hours a week. Students that run for office, but were not elected used, on average, 7 hours per week to political activities.¹³

2.2.5 External validity

As always with case studies such as this one it is difficult to assess to what extent the analysis is generalizable to other settings. Ultimately, this needs to be settled by contrasting our results to future research from other settings since current evidence is scarce. But a few potentially relevant idiosyncrasies, that may help assessing the validity in different types of settings, are worth noting already here: The *candidates*, according to our small survey, perceive themselves to have very good credentials before running for office. (We cannot say if these statements reflect overconfidence or true merits.) They also appear to be unusually interested in politics and administration. Our data do not allow us to dig much further into their characteristics as we do not have comparable data on non-candidates (although see Table A2 for earnings distributions). The *student unions* on the other hand are well-known organizations. Most employers will understand the process and be able to infer what types of tasks a student has performed during a period in one of the councils. Employers are thus likely to understand that it is easy to get a position on a list, but difficult to get elected, and demanding to participate in a council. Thus, taken at face value, these aspects suggest that the analysis is most readily transferable to other

¹³ Elected (non-elected) candidates used 7 (4) hours per week for engagement in other voluntary associations. For additional details and results from the survey, see Lundin et al. (2016) (including the supplementary material available online).

settings where ambitious students take part in well-known and demanding leadership activities during college.

3. Data and method¹⁴

3.1 Statistical model

We derive the causal effects of becoming a student representative by using the logic of a regression discontinuity (RD) design (Thistlethwaite and Campbell 1960) with a discrete running variable. The basic idea is to compare labor market outcomes for closely ranked winners and losers from the same SU council list. 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 whereas other closely placed candidates on the same ballot lists just miss getting into the councils.

Thresholds have a strong, but not deterministic, relationship to the allocation of SU council seats. Discrepancies may arise for two reasons: First, some of our elections permitted preferential votes alongside the votes on the lists. In these elections, students who received enough individual votes above a certain pre-specified threshold were treated as first-ranked (we only have very imperfect information on the number of votes on each individual). Second, 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 initial ranking and threshold as an instrumental variable for actually acquiring a seat; that is, we make use of the “fuzzy RD design.”

Formally, we run the following two-stage regression:

¹⁴ The exposition (and content) here draws heavily on our previous paper, which focuses on the effect of SU-participation on public election candidacies; see Lundin et al. (2016).

$$\begin{aligned}
Elected_{i,l} &= \alpha_l^1 + \delta Above_i + \varphi^1(Rank_{i,l} - T_l) + \mu^1(Rank_{i,l} - T_l)Above_i + X_1\beta^1 + \varepsilon_{i,l}^1 \\
Outcome_{i,l} &= \alpha_l^2 + \gamma Elected_i + \varphi^2(Rank_{i,l} - T_l) + \mu^2(Rank_{i,l} - T_l)Above_i + X_1\beta^2 + \varepsilon_{i,l}^2
\end{aligned} \tag{1}$$

We let *Outcome* denote our various variables measuring labor market outcomes for individual *i*. *Rank* is the list-ranking in the SU council election. *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. The α :s are fixed effects for each list (denoted by *l*) which handles all issues related to selection into specific lists. Since lists are year-specific, this ensures that we only compare candidates in the same election cohort to each other, and thus also ensures that all differences across elections are controlled for. Finally, there is an error term for each stage. The parameter of interest is γ , which captures the effect of being elected on the outcome measures. We also add a set of individual-level covariates (age, sex, immigration status, duration of studies, and previous work experience), denoted by *X*, to increase the precision of the estimates.

Note that our “running variable” (the list rank) is discrete. Hence, our model is explicitly estimated by comparing linear predictions from the two sides towards the threshold, which in practice closely mimics standard procedures in RD-applications with continuous running variables (see e.g. Lee and Lemieux 2010). However, standard tests for optimal bandwidths do not apply.¹⁵

Although most of our outcomes are binary, we estimate linear probability models following standard procedures in the RD literature. Throughout, we rely on standard

¹⁵ Lee and Card argue for clustering on specific values of the running variables due to the fact that the functional form may be misspecified (hence generating error correlations within such clusters) (Lee and Card 2008). As in most applications, clustering on each rank across lists gives smaller estimated standard errors, presumably since the number of clusters becomes too small and we therefore do not cluster the presented standard errors on rank.

errors that are robust to heteroscedasticity and clustered to account for repeated observations at the individual level (referring to about one third of the individuals). We return to the repeated observations in section 4.1.3.

3.2 Data

We have gathered archive data from student unions at Uppsala, Stockholm, and Lund. We include information on elections covering as many years as we could find the necessary data in the archives. These data span the period 1982 to 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. As shown in Table A1 in the online appendix, we do not have information for all year-school combinations because the archives were incomplete.¹⁶ Our raw data cover 30 different SU elections and 5,154 candidates. Using the social security numbers, *Statistics Sweden* matched our SU data to national registers containing basic demographics (sex, age, and immigration status), basic educational information, and labor market outcomes such as employment and earnings during 1985–2010.

We exclude the few lists where none or all of the candidates were elected, because 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, as they have a zero probability of becoming elected. Last, 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 is fairly evenly distributed across universities and years (see Table A1 in the appendix).

¹⁶ The key missing variable that forced us to ignore many year-school combinations is the social security number (*personnummer*) of failing candidates. The variable is crucial for us to be able to retrieve outcomes and characteristics.

Table 1 reports descriptive statistics of SU council candidates above and below the election thresholds. The first two columns include all candidates, and the last two columns focus on our main sample consisting of the five candidates closest to the thresholds on each side (we present extensive robustness tests on the choice of bandwidth below). 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.

[Table 1 about here]

Table 1 shows that there are 30 election cohorts (year×university). On average, 10 lists participated in each election. Candidates above and below the thresholds are similar in terms of mean age (24 years), the proportion of immigrants (0.07), and the proportion of women (0.40). Our used sample 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, there is a very large difference in the probability of entering the council between the five closest below (0.08) and the 5 closest above (0.88).¹⁷

3.2.1 Outcomes

Our register data allow us to track the employment status and annual earnings of the students. *Employment* is measured through an indicator calculated by *Statistics Sweden* capturing the employment status in November.¹⁸ We use annual earnings to track the career progression of the students. We define a dummy for holding a *well-paid job* taking the value one when annual earnings exceed the median of 30-year-old college graduates in the same year. *Annual earnings* are analyzed on logs and in percentiles of the earnings distribution for 30-year-old college graduates.

¹⁷ The jump for the marginal candidates is somewhat smaller, as expected (see Figure 3 below).

¹⁸ The algorithm generates aggregate employment rates close to those in the Labour Force Surveys.

We use data from before ($t < 0$) participating in SU council elections, during the year of the council ($t = 0$) and follow them across time thereafter ($t > 0$). Figure 1 shows descriptive statistics for our outcomes. Almost 60 percent were employed one year after participating in the SU council election. Very few, about 5 percent, earned more than the median 30-year-old with a college education at that initial stage in their career. This is not surprising, given that the students are 24 years old on average when they run in the SU elections. The candidates have very positive career trajectories going forward: five years after the SU elections, more than 80 percent had a job, while around 45 percent had reached the median of 30-year-old graduates. The average candidate starts off earning around the 20th percentile in the earnings distribution of 30-year-old graduates, but rapidly progress towards earning around the 50th percentile after 6 years.¹⁹

[Figure 1 about here]

To tease out the robustness of the results and to provide some more insights into the potential underlying mechanisms, we also use a set of other outcome variables capturing, e.g., network effects and educational performance. These are defined when analyzed to avoid repetition.

3.3 The validity of the RD approach

Our analysis relies on the key assumption of no systematic differences in skills between candidates across the threshold (T in eq. 1), apart from those that are captured by the rank variables. This assumption will be valid if the parties are unaware of exactly how many seats they will acquire when they compile the lists: candidates can only self-sort exactly around the threshold if it is known beforehand.

¹⁹ In the appendix (Table A2), we instead show how the candidates' earnings relate to year-specific quartiles of the earnings-distribution of all college graduates at age 30. This description show that the candidates (elected or not) are over-represented in the top and the bottom quartiles of the overall distribution.

There are good reasons to expect that the exact location of the election thresholds is very difficult to predict: First, party lists are constructed and votes are cast with very little information. When lists are compiled, potential candidates are not able to foresee who they are competing against in terms of the number and nature of competing parties. Since students come and go, there are large year-to-year variations in the pool of competing candidates even within stable parties. Furthermore, there are frequent entries and exits of entire parties. Thus, it is not possible for the parties to exactly monitor changes in their competition, nor changes in students' preferences, and it is an even more demanding task to predict the effect on such changes on their exact number of seats. In contrast to regular elections, there are no opinion polls.

Second, the number of seats per party is volatile between years. Figure 2 displays the distribution of new seats in the SU elections. Since our RD approach identifies the effects at the threshold, it is important to assess the extent to which students can predict the exact number of seats the list will get by observing last year's election results. Reassuringly, only 13 percent of the lists received the same number of seats in two consecutive elections. Thus, only one in eight seats in the councils were allocated to a list that remained at a stable representation since the last election. This reflects both a frequent entry and exit of parties between elections, and volatile voting patterns.

[Figure 2 about here]

Last, we have conducted a survey among recent SU council candidates at one Swedish university (see section 2.2.4). 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 that were very far from the thresholds,

the respondents were wrong almost as often as they were right when predicting the outcome (and many were unable to make a prediction).²⁰

As is standard in the RD literature, we test if closely ranked winners and losers, on average, differ in observed characteristics after controlling for the rank-distance to the threshold. If our assumptions are valid, this should be the case. As prescribed by, e.g., Lee and Lemieux (2010), we estimate equation (1) with observed predetermined characteristics (the X :s) as outcome.²¹

[Table 2 about here]

Table 2 shows that neither sex, immigration status, age, duration of studies, previous work experience, nor experience from previous years' SU councils differ at the threshold.²² The point estimate for duration of studies (1/4 of a year) may appear large, but the estimate is far from statistically significant and should be evaluated relative to a mean of 3.7 years of study. All other point estimates are clearly very small compared to the variable means. An F-test shows that the variables in a regression on the instrument are jointly insignificant. As we show below (e.g. Table 3), all key results are insensitive to whether or not we include these covariates as controls in our main analysis. Note that the standard RD test of the number of observations on the two sides of the threshold holds by construction in our case since the running variable is a rank; however, a figure is nevertheless supplied in the appendix (Figure A1).

²⁰ This analysis is presented in the online appendix to Lundin et al. (2016).

²¹ These estimates are derived without other X :s on the right-hand-side, following standard conventions.

²² These variables constitute a nearly exhaustive list of the predetermined variables that are available in our data. In our companion paper (Lundin et al. 2016), we also show that the probability of having participated in a public election before being a SU candidate is also balanced at the threshold.

3.4 The first stage

In our main analysis we use the IV model outlined in equation (1) above. As expected, the first stage is very strong. The relationship between rankings and thresholds and the probability of being elected is depicted in Figure 3, showing a distinct jump in the election probability exactly at the threshold. The small deviation from the linear predictions of the ranking variable for the closest observations is consistent with preferential votes on individual candidates which may push the last candidate down across the threshold at the same time as forfeited seats are allocated to the last candidate below the threshold.

[Figure 3 about here]

4 Results

We present the main results in two subsections, beginning with the impact of becoming a student representative on labor market entry (employment) and then turning to the impact on subsequent early career trajectories.

4.1 Labor market entry

We use equation (1) to estimate the impact on the probability of employment at different horizons. We study the unconditional direct effects on all horizons. Thus, early-year outcomes may be part of a mediating process for later outcomes. For completeness we show results for years both before and after the SU election. Candidates are elected for one academic year and employment in $t=0$ is for November that year. Thus, employment in $t=1$ is for November in the year when the term expired, i.e. 5 months after the end of the academic term. As expected, the results in Figure 4 indicate no effects during the year before the election ($t=-1$). There is no evidence of any employment responses during the year of participation in

the SU council ($t = 0$). Thus, participation does not change the incidence of employment *during* the council term.

[Figure 4 about here]

However, 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 end of the term in the SU council. Candidates who were marginally elected to a SU council were 20 percentage points more likely to be employed at that time than SU candidates who just missed being elected (starting from a baseline of about 45 percent). This initial effect wears off rapidly, which is consistent with the fast career progressions demonstrated above in Figure 1. The effects on employment probabilities are statistically insignificant two to five years after the election. The point estimates are small for all the remaining years, with the exception of year 2 where the estimate suggests an impact of 8 percentage points.

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 by a common linear term (thus as eq. 1 but without X:s and with $\mu^1 = 0$ and $\mu^2 = 0$). Column 2 adds an interaction between the ranking and the threshold (i.e. with estimated μ^1 and μ^2). Column 3 presents our preferred model (corresponding to Figure 4), which also includes the X:s controlling 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 use a narrower sample window containing only the three closest candidates. Finally, column 6 relies only on the two truly marginal candidates within each list. The estimates of the last column are however less well-identified as the model cannot control for the ranking at all and therefore imposes $\varphi^1 = 0$, $\varphi^2 = 0$, $\mu^1 = 0$ and $\mu^2 = 0$. Figure A2 in the online

appendix provides results for all bandwidths. The key takeaway is that our 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 three candidates, or to focusing on the truly marginal candidates. The most noticeable differences across the columns are that the statistical precision is reduced (to significance at the 10 percent level) when including the quadratic term and that the estimate is somewhat smaller when focusing on the two marginal candidates.

[Table 3 about here]

Figure 5 shows the mean outcomes by each value of the list ranking (after removing list specific means and adding the grand mean). Clearly, the relationship appears linear (apart from at the threshold), reiterating that the functional form used in equation 1 is reasonable. There appears to be some added noise for students who were elected with a broad margin (i.e., with a distance of 3 and 4). However, because of the fewer observations at these points (see the density plot in Figure A1), this is not surprising.

[Figure 5 about here]

In the appendix we provide further results documenting heterogeneity by types of lists. We show that the effect is larger when excluding lists associated with leftist politics but, at the same time, we do not find that being elected on a left-wing list is damaging to the career prospects (Table A3, see note for details). The same table also shows that the effects are more pronounced for candidates on lists that eventually receive at least one seat on the student union executive board. The estimates are unchanged if removing lists which only acquire one seat.

4.1.1 Effects by graduation status, study duration, and previous employment

Table 4 shows results from variations of the model focusing on the short-run employment effect. We first note that the interpretation of our results would be different if winning meant taking up a part-time job *before* graduation instead of affecting employment immediately *after* graduation. The endogenous nature of study durations (see Section 2.1) in Sweden makes the distinction between working while enrolled and post-graduate employment much less precise than it would be in, e.g., a US setting. However, to address the issue head on we re-estimate the model twice using the two different outcomes *Employed Graduate* and *Employed non-Graduate* using the same sample and empirical set-up as in the main analysis. For both outcomes, zeros include all who do not satisfy both criteria (i.e. employed *and* being a graduate in Column 2 and employed *without* being a graduate in Column 3). Hence, the sum of the two corresponds to the original Employment dummy (the baseline in Column 1). The “Graduate” part is defined as holding a college degree at the end of the year when employment is measured (see below for a separate analysis of graduation responses). The point estimates show that the overall effect on employment is an even split between these two parts, although only the effect on “Employed Graduate” is statistically significant at the 5 percent level. The results thus suggest that being elected affects both the incidence of working-while-enrolled and the post-graduation employment rates. In the online appendix (Table A4), we show further results where we use data on part-time vs. full-time work for a subsample where we have information on this. The exercise shows that 2/3 sampled jobs are full time and that the impact on the probability of holding a part-time job is estimated to be small (negative, insignificant) which means that the estimate becomes larger when excluding (sampled) part-time jobs. Overall, these results indicate that

the main employment impact is driven by an increase in the incidence of full-time jobs, and not part-time jobs.²³

[Table 4 about here]

In a related exercise, we split the sample according to *elapsed duration of studies* and estimate the model using the original outcome variable. The employment effect is significant, with similar point estimates, both for those who have a maximum of three years of prior college studies (Column 4 of Table 4) and those who have at least four years of university studies before the election (Column 5). 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.

Finally, we split the sample according to *previous employment*; that is, into samples of those employed (Column 6) and not employed (Column 7), respectively, during any of the past three years. The employment effect is most prominent among candidates who lack previous work experience: the estimate is significant at the 10 percent level and almost twice as large as for the group of students who have worked before (0.24 vs. 0.14). This suggests that serving in the council is a substitute for work experience, e.g., by providing similar types of references and/or signals of preexisting traits or abilities. Taken at face value, the point estimates suggest that a human capital story would require that the human capital acquired through leadership experiences adds very little for those with some basic work experience.

²³ The working time survey starts in 1996 and it covers all public sector jobs and half of private sector jobs. Table A3 shows that in elections from 1995 onwards, 58 percent of candidates are employed a year after elections. 8.6 percent are in sampled part-time jobs, 16.7 percent are in sampled full-time jobs, and 32.7 are in non-sampled jobs. The impact on part-time jobs is negative (-0.028) and far from significant, the impact on sampled full time jobs is positive (0.08) but insignificant (se 0.05) and the impact on non-sampled jobs is positive (0.13) and significant.

4.1.2 Academic performance

The employment effects we capture encompass the net effects of being elected to the SU councils. This could potentially mask an attenuating effect through reduced academic performance, which in turn may affect the labor market outcomes. Furthermore, it is possible that this explains the short-term nature of the effects if the (presumed) negative impacts on academic performance affect individuals' careers later in life. To investigate this issue, we study indicators of academic performance. Unfortunately, our performance indicators are not ideal; in particular, *we lack data on grades*. The reason is that Swedish universities use very idiosyncratic grading metrics (using letters, numbers, or phrases) and grades are, to the best of our knowledge, not collected centrally at all. The grading metrics vary even across fields within the same university.

Instead, we measure average *annual credit-point production* (scaled by the official metric of full-time studies). The sample is smaller since we only could find these data from 1993 to 2009. We estimate the model separately for two-year intervals starting in the year of the relevant election. The results are reported in Table 5, Panel A. Although the point estimates suggest a tendency for reduced study pace during the first few years, the point estimates are modest (5 percent of full-time studies) and statistically insignificant. In the last period (years 4–5), the point estimate turns positive, but is even smaller. In Panel B of the same table, we estimate the impact on degree probabilities, also displaying a tendency for negative effects, but without being statistically significant. The same is true for the impact on the probability of acquiring a PhD. Overall, we conclude that we are unable to document statistically significant effects on academic performance.

[Table 5 about here]

4.1.3 Repeat candidacies

Students in our data sometimes appear multiple times. In the baseline specifications we handle this by clustering the standard errors on the students to ensure that the inference is not biased by repeat candidacies. This assumes that the effect of being elected once is the same as the incremental effect of being elected a second time. In this case, the model is correctly specified if we accept responses through future (endogenous) candidacies as part of the process. However, the short-run nature of the effects could be explained by repeat candidacies *if* the returns to being elected are highly concave (e.g., if what matters is *if* you have been elected, not how many times) so that losers either catch up through subsequent wins *or* are more willing to run (and win) again later. In order to study these concerns, we have performed a set of additional exercises.

Results are presented in Table 6. Column 1 shows the baseline. Column 2 shows that winning an election has a positive causal effect on the probability of winning later. Thus, losing students are not making up for their losses by winning later.²⁴ Instead, the results suggest that there is some additional *divergence* in terms of the total number of wins over time.

[Table 6 about here]

In Column 3, we analyze whether the effects differ between students with and without a previous win. To gain precision, we estimate a pooled model where we interact the variable of interest (and the instrument) with the dummy for previous wins. The point-estimate for the interaction is negative, but insignificant.

Finally, Column 4 analyzes the impact after three years using the baseline sample and Column 5 repeats this analysis for the sample of *candidates without future wins*.

²⁴ We focus on the process of winning later, since we can do this much better than analyzing subsequent candidacies. The reason is that we collected data on winners also in years when data on (losing) candidates were missing. We treat cases where the election outcomes are missing for the next year as missing observations.

The results are very similar. The fact that estimates are very similar supports the notion that repeat wins are not causing the convergence, with the obvious caveat that the second sample is selected on an endogenous outcome.²⁵

Importantly, the overall fraction of losing candidates who become elected in subsequent elections is 18.3 percent, which bounds the repeat candidacy bias to 0.036 percentage points (0.18×0.200) under the extreme assumption that only the first experience as a SU council member matters (and using the estimate of 0.200 from column 1 for the impact). In sum, we do not believe that repeat candidacies is a major explanation for why losing candidates catch up over time, despite the fact that the effect for repeated leadership experiences may be marginally smaller than the effect of the first experience as indicated by Column 3.

4.1.4 Network effects and effects on different segments of the labor market

Next, we study *where* student candidates find employment. In particular, we investigate 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 investigate this hypothesis, we rely on data on the actual working patterns of previous student representatives (again using register data).

The results are displayed in Table 7. In the first column, we use a dummy that takes the value one for students who find employment at an establishment *where a previous elected representative is already 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. Column 3 replicates the

²⁵ In previous versions we showed that this is true at all horizons.

analysis of column 2 at the “organization” level. An organization can, e.g., be a firm, 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 a particular industry, for obvious reasons, is much larger than ending up at a particular establishment. The final column shows the impact on working with non-elected previous candidates and the result gives a similar picture.

[Table 7 about here]

We have also verified that the overall effect is not driven by *jobs within universities or organizations such as the national student association* (not displayed in the table). Redefining the outcome of the baseline regression to be a dummy for holding a job excluding all jobs at universities (2 percent of all jobs) or at any NGO or other civil organization (12 percent of all jobs), as indicated by industry dummies, gives an estimate of 0.152 with standard errors 0.066. Thus $\frac{3}{4}$ of the increase in job finding stems from jobs outside of the University/NGO domain.

Overall, we interpret the results as suggesting that the short-run employment effects of being elected into the council are of a general nature, as we fail to find any systematic sorting at the establishment level, at the organization level, or at the industry level.

4.2 Early career trajectories

The results above suggest that the effects on employment are large but transitory in nature. This is a natural consequence of the fact that employment rates are growing fast for all candidates shortly after the elections. We thus proceed by estimating the impact on the probability of finding a well-paid job and the impact on earnings.

We start by studying well paid jobs as measured by dummies for annual earnings exceeding the median earnings of college-educated 30-year olds. This allows us to comfortably pool across our long data period (1982–2010) during which the earnings distributions changed substantially (see e.g. Skans et al. 2009). As our students are on average 24 years old when they run for the councils, it takes time to reach this target in most cases.²⁶

Figure 6 shows estimates of equation (1) on this outcome using the same time interval as for labor market entry in Figure 4 above. The results show a similar temporary effect as for employment, but two years later. Serving as a student representative has a significant impact on the probability of holding a well-paid job three years after the election. Thus, SU council experience is useful for getting a job one year after the SU election, and also for holding a well-paid job three years thereafter. But as with employment, the effect appears to wear off with time.²⁷ In Table A5, we show that the effect during the third year is stable across the same set of variations as the employment effect discussed in Section 4.1.²⁸

[Figure 6 about here]

We now turn to annual earnings. We start by estimating the impact on log earnings. In order to increase precision, and to reduce the problem with zero-earnings cases, we have performed this analysis for three-year averages. Results are reported in Table 8, Panel A. We find insignificant differences in the periods before the election and a large positive impact during the three years after the election. Consistent with the analysis of well-paid jobs, the effect disappears after that.

²⁶ The ambitious career target also ensures that any effects we find almost by definition appear after the end of studies.

²⁷ Previous versions also explored slightly higher and lower earnings thresholds and the time profiles are consistent with the progression with earlier effects on lower thresholds and later (but less precise) impacts on higher thresholds.

²⁸ Further results are presented in Figures A2 and A3 in the online appendix.

Figure 7 presents shows the mean outcomes by each value of the list ranking (after removing list specific means and adding the grand mean) for earnings in the three years following the election. As with the one-year employment effect described in Figure 5, the relationship appears linear on each side of the threshold.

[Figure 7 about here]

The magnitude of the estimated earnings response (0.34) may seem surprisingly large but much of the estimate is driven by movements in the left-end tail of the log earnings distribution (a reduction in the incidence of very low earnings amongst the elected candidates). In order to provide estimates that are less sensitive to extreme values, we have transformed the earnings into percentiles of the overall earnings distribution of 30-year-old college graduates and re-estimated the models on these ranked data. Estimates are presented in Panel B of Table 8. Reassuringly, the results present a similar picture as the analysis of log earnings. The effects are centered around the first three years after the election, but disappear thereafter. The magnitudes imply that a positive election outcome moves the candidates up 2.7 percentiles in the graduate earnings distribution during the first three years. To get a sense of the relationship between the estimates in Panels A and B, it may be useful to know that climbing one percentile on average corresponds to a 5.9 percent earnings increase (in 2011) which implies that 2.7 percentiles correspond to 15.9 percent higher earnings.

[Table 8 about here]

Unfortunately, we do not have access to data on occupations for the full economy. The data we have only cover the period from 1996 onwards, and the sample only cover 50 percent of workers each year with an full sample of public sector employees

workers in very large firms, but highly incomplete coverage of workers in smaller private firms. Due to partial nature and non-random sampling of these data, they are not particularly well suited for our research design. We have however used the data to study the impact on leadership positions later in life by creating a dummy variable for candidates who at any time during the first 10 years after graduation are observed in a managerial position within the data (measured at the one-digit level according to the ISCO nomenclature). This is a rare and high career target that very few students (7 percent) achieve, in particular early in the careers. The estimates are presented in Column 1, Panel C of Table 8. We repeat the analysis in Column 2 focusing on becoming a “professional”, which is the second highest 1-digit ISCO category and a target that many more students (44 percent) achieve within 10 years. Column 3 combines these two and adds Associate Professional (the third level). Consistent with the results from our main analysis, these estimates are all insignificant and small. The somewhat poorer quality of these data suggests that a more detailed investigation of occupational effects is a topic worthy of future research. But with this caveat in mind, we interpret the results we have as suggesting that the labor market returns to participation in the councils are in terms of a faster labor market entry and early progression, but without any long run impact on the most ambitious career targets.²⁹

5 Conclusions

In this paper we have examined the impact of acquiring leadership experiences during college years on labor market entry and early career trajectories. Relying on an RD approach using combined archive and register data, we add to a very scarce set of papers that have been able to provide credible evidence of how leadership experiences prior to labor market entry affect economic outcomes.

²⁹ In contrast, our companion paper (Lundin et al, 2016) shows that the impact on political careers is very persistent. Importantly, for the purpose of this paper, those effects are much too small to be a relevant explanation for the significant short run labor market effects we document here.

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 by 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. Half of the overall effect is estimated to be driven by earlier labor market entry/post-graduation employment whereas the other half is due to an increased incidence of employment prior to graduation. 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 larger among students who lack prior work experience, which indicates that participating in leadership activities within the councils appear to serve as a substitute for part-time work experience. Further results show that elected representatives are more likely to hold a well-paid job and have higher overall annual earnings within the first 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. Instead, all our estimates suggest that the benefits are transitory.

The research design and our data do not allow us to provide a sharp conclusion on exactly *why* leadership activities during college affect labor market outcomes. Conceptually, participation in the councils may help students to accumulate leadership skills, signal preexisting (non-cognitive) skills, and generate useful labor market networks or provide students with additional intrinsic motivation to enter the market. However, we cautiously suggest that the evidence is 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.³⁰ Our results appear stronger among students who lack previous work experience and the time profile of the effects is well in line with the learning estimates provided by Lange (2007). We also note that the fact that the impact appears to be widely dispersed across different industries and firms, and not confined to segments where previous representatives work or workplaces with institutional ties to the student unions (i.e. universities or non-government organizations), suggests that the signals work beyond the immediate network generated through the councils.³¹ To align our results (taken at face value) with a human capital story it seems necessary to assume that the skills learned in the councils are not fostering future accumulation of human capital. The reason is that the catch-up appears to be so rapid. This could be the case if council participation let the students learn some necessary basic professional conduct, which potentially could explain our results if such skills are easily acquired at any post-graduation job but do not have further benefits once acquired. We cannot rule out this hypothesis.

Regardless of the underlying mechanisms, our main conclusion does, however, convey an important message: Acquiring leadership experiences prior to labor market entry may turn out to be a useful investment since these activities provide the youths in our sample with a kick-start in their careers. This finding highlights that it would be fruitful to expand the school-to-work-transition literature to other potential certifications and determinants of non-cognitive skills. Our sample is drawn from what appears to be a very ambitious group of students that participate in a visible and well-known activity. As a consequence, in addition to expanding the research to other countries, a particularly useful avenue would be to assess if similar patterns also arise in less prestigious leadership activities.

³⁰ Note that the RD analysis 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 an RD design.

³¹ See Hensvik and Skans (2016) for estimates of employer learning through networks.

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Tables and Figures

Table 1. Descriptive statistics

	All (irrespective of ranking)		Closest 5 (main sample)	
	Above threshold	Below threshold	5 above threshold	5 below threshold
<i>Individual data</i>				
Candidate age (years)	24.4	24.7	24.3	24.4
Women (proportion)	0.410	0.383	0.393	0.400
Immigrants (proportion)	0.075	0.067	0.076	0.063
Duration of studies (years)	3.910	3.961	3.773	3.632
Employed the year before (proportion)	0.423	0.447	0.408	0.431
SU experience (proportion)	0.388	0.165	0.340	0.198
Elected to SU council (proportion, main independent variable)	0.891	0.035	0.881	0.084
Number of observations	1,257	3,897	919	1,251
Number of unique individuals	843	2,731	687	1,031
<i>Lists (parties per year and university)</i>				
Number of included candidates per list	4.3	13.4	3.2	4.3
Total number of lists	290	290	289	289
<i>Election cohorts (year and university)</i>				
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. The two last columns focus on the five students on each side who are closest to each threshold.

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

	Female	Immigrant	Age	Years since start of studies	Employed the year before	SU experienced
Estimate (s.e.)	-0.022 (0.064)	0.012 (0.034)	-0.009 (0.394)	0.254 (0.318)	0.008 (0.065)	-0.013 (0.057)
Mean dep. var.	0.397	0.0687	24.36	3.692	0.421	0.260
N	2,170	2,170	2,170	2,167	1,847	1,924

Notes: Estimates based on equation (1) 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 three columns have slightly fewer observations due to missing values on the outcome variables (from truncation of the sample window). 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 3. The impact on employment one year after the SU election: robustness

	1	2	3	4	5	6
Estimate	0.193***	0.201***	0.200***	0.246*	0.213**	0.143*
(s.e.)	(0.063)	(0.069)	(0.066)	(0.136)	(0.098)	(0.079)
First stage	0.634***	0.631***	0.630***	0.546***	0.580***	0.614***
(s.e.)	(0.030)	(0.030)	(0.030)	(0.053)	(0.043)	(0.046)
N	2,106	2,106	2,106	2,106	1,416	522
Sample, closest:	5+5	5+5	5+5	5+5	3+3	1+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 terms	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. Covariates are the ones presented in Table 2. and indicators for missing values of the last three of these. “Sample, closest 5+5” indicates closest 5 on each side. 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 4. The impact on employment one year after the SU election: heterogeneity

	Baseline	Outcomes defined by graduation status:		Pre-election duration of studies (years)		Employed before	
		Employed Graduate	Employed Non-graduate	3 or less	4 or more	No	Yes
Estimate (s.e.)	0.200*** (0.066)	0.104** (0.048)	0.096 (0.064)	0.181* (0.100)	0.201* (0.107)	0.238* (0.139)	0.137 (0.087)
N	2,106	2,106	2,106	1,110	993	676	1,163
Mean dep.var.	0.562	0.202	0.359	0.506	0.624	0.428	0.641
Sample, closest:	5+5	5+5	5+5	5+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 on each side. 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 5. The impact of being elected to a SU council on academic performance

Panel A The impact of being elected to a SU council on credit point production				
	Credit points (as fraction of full time)			
	Year 0–1	Year 2–3	Year 4–5	
Estimate	-0.088	-0.075	0.030	
(s.e.)	(0.067)	(0.049)	(0.040)	
N	1,363	1,363	1,279	
Mean dep var	0.664	0.308	0.159	
Sample, closest:	5+5	5+5	5+5	
Covariates	Yes	Yes	Yes	
Lagged pol. cand.	No	No	No	
Ranking	Yes	Yes	Yes	
Ranking*above threshold	Yes	Yes	Yes	
Quadratic terms	No	No	No	
Panel B The impact of being elected to a SU council on degree probabilities				
	Graduation with a university diploma			With a PhD
	Within 1 year	Within 3 years	Ever	Ever
Estimate	0.013	-0.090	-0.042	-0.022
(s.e.)	(0.053)	(0.066)	(0.054)	(0.028)
N	1,926	1,926	2,168	2,168
Mean dep var	0.224	0.509	0.786	0.059
Sample, closest:	5+5	5+5	5+5	5+5
Covariates	Yes	Yes	Yes	Yes
Lagged pol. cand.	No	No	No	Yes
Ranking	Yes	Yes	Yes	No
Ranking*above threshold	Yes	Yes	Yes	No
Quadratic terms	No	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 on each side. Sample for graduates within 1 and 3 years in Panel B does not include students with previous degrees. 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 6. The role of repeat candidacies

	Outcome: Employment (<i>t</i> +1)	Outcome: Future SU seat	Outcome: Employment (<i>t</i> +1)	Outcome: Employment (<i>t</i> +3)	Outcome: Employment (<i>t</i> +3)
Estimate	0.200***	0.116**	0.193***	0.024	0.029
(s.e.)	(0.066)	(0.058)	(0.067)	(0.058)	(0.074)
Interacted with past-seat dummy			-0.070 (0.072)		
N	2,106	2,042	1,376	2,139	1,495
Sample	Baseline	Baseline	Non-missing dummy for past seat	Baseline	No future seat
Covariates	Yes	Yes	Yes	Yes	Yes
Ranking	Yes	Yes	Yes	Yes	Yes
Ranking*above threshold	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. Covariates are the ones presented in Table 2 and indicators for missing values of the last three of these. Note that these “covariates” include a dummy for past seats. 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 7. Impact on the probability of being employed with former 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 (s.e.)	0.010 (0.029)	0.029 (0.040)	0.072 (0.195)	1.497 (1.401)	6.092 (4.388)
Mean dep. variable	0.057	0.075	0.541	7.547	24.23
Sample, closest:	5+5	5+5	5+5	5+5	5+5
N	2,106	2,106	2,106	2,106	2,106
Covariates	Yes	Yes	Yes	Yes	Yes
Ranking	Yes	Yes	Yes	Yes	Yes
Ranking*above threshold	Yes	Yes	Yes	Yes	Yes

Notes: The outcomes are measured in the year following the election. The dependent variable is calculated using elected candidates during the previous five 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.

Table 8. The earnings impact of being elected to a SU council

Panel A: Earnings effects in ln(Earnings)			
	1–2 years before	1–3 years after	4–6 years after
Estimate	0.061	0.336**	-0.061
(s.e.)	(0.178)	(0.148)	(0.144)
N	1,622	2,045	1,870
Mean dep var	10.87	12.11	12.84
Sd dep var	1.209	1.142	1.091
Sample, closest:	5+5	5+5	5+5
Covariates	Yes	Yes	Yes
Ranking	Yes	Yes	Yes
Ranking*above threshold	Yes	Yes	Yes
Panel B: Earnings effects in percentiles of the distribution of college graduate earnings			
	1–2 years before	1–3 years after	4–6 years after
Estimate	-0.012	2.741*	0.469
(s.e.)	(1.288)	(1.660)	(2.287)
N	1,716	2,082	1,914
Mean dep var	14.80	17.54	29.71
Sd dep var	9.375	11.96	16.91
Sample, closest:	5+5	5+5	5+5
Covariates	Yes	Yes	Yes
Ranking	Yes	Yes	Yes
Ranking*above threshold	Yes	Yes	Yes
Panel C: Effects on ever observed in a high ranked occupation during 10 years after election.			
	Manager (ISCO=1)	Professional (ISCO=2)	Manager, Professional or Associate professional (ISCO=1,2,3)
Estimate	-0.005	0.051	0.045
(s.e.)	(0.052)	(0.078)	(0.076)
N	1,257	1,257	1,257
Mean dep var	0.070	0.449	0.550
Sample, closest:	5+5	5+5	5+5
Covariates	Yes	Yes	Yes
Ranking	Yes	Yes	Yes
Ranking*above threshold	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 on each side. Observations with zero earnings are removed in Panel A (at most 5.5 percent of the sample). Panel C is only estimated for elections from 1995 onwards. 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.

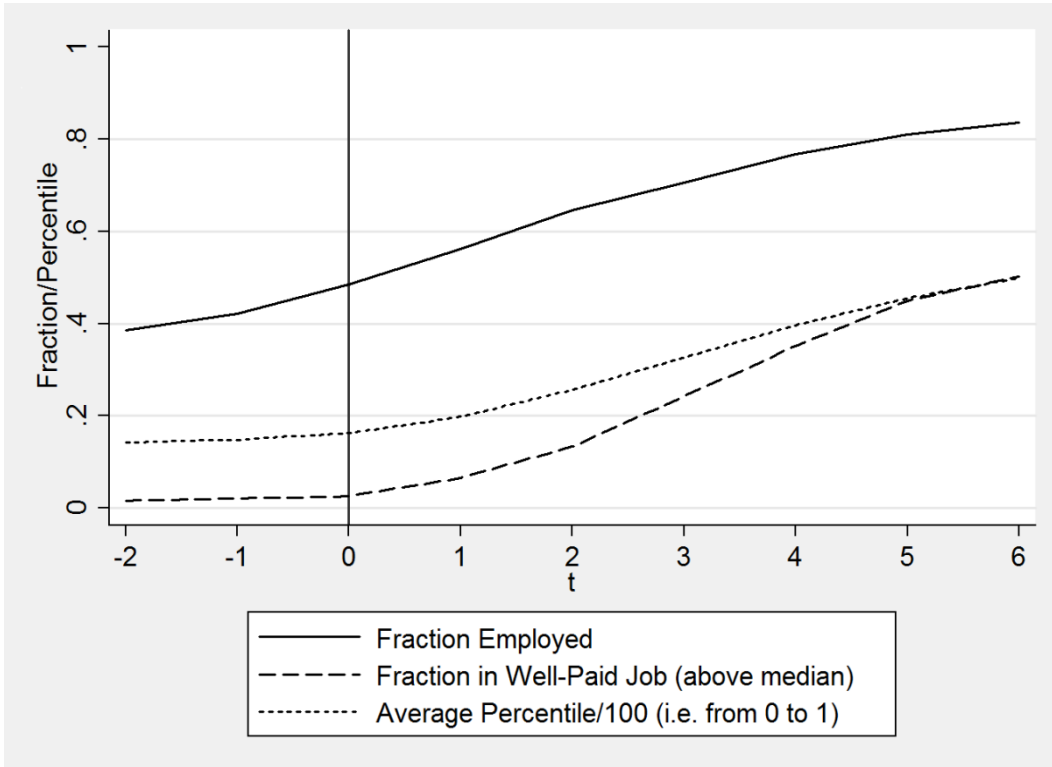


Figure 1. Employment rate, fraction with a well-paid job, and average earnings percentile of candidates in years relative to the year of election

Notes: The employment indicator captures employment in November according to Statistics Sweden’s algorithm. The earnings threshold for a well-paid job is the median within the distribution of annual earnings among all 30-year-olds with a university degree (masters or bachelor). The “average percentile” is calculated relative to the same distribution (30-year old graduates) and divided by 100 to fit the same scale. *X-axis:* years relative to year of election ($t = 0$).

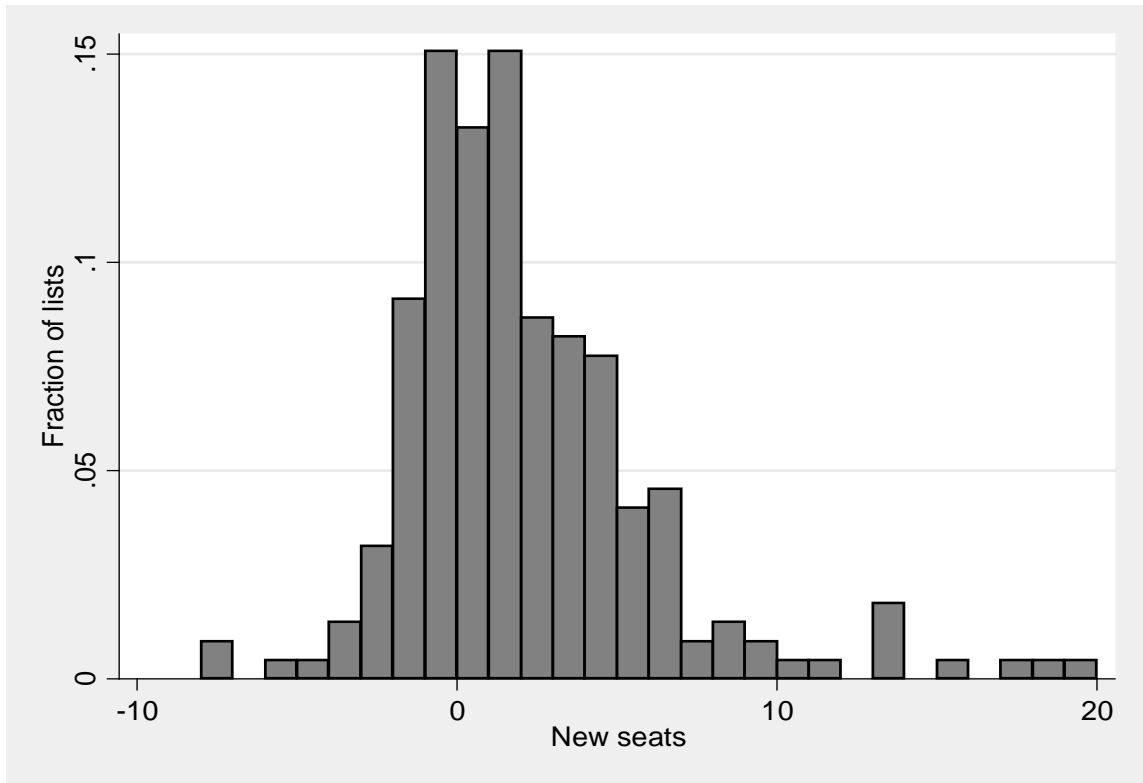


Figure 2. Distribution of new seats among lists receiving seats in SU council elections

Note: Figure is reproduced from Lundin et al. (2016).

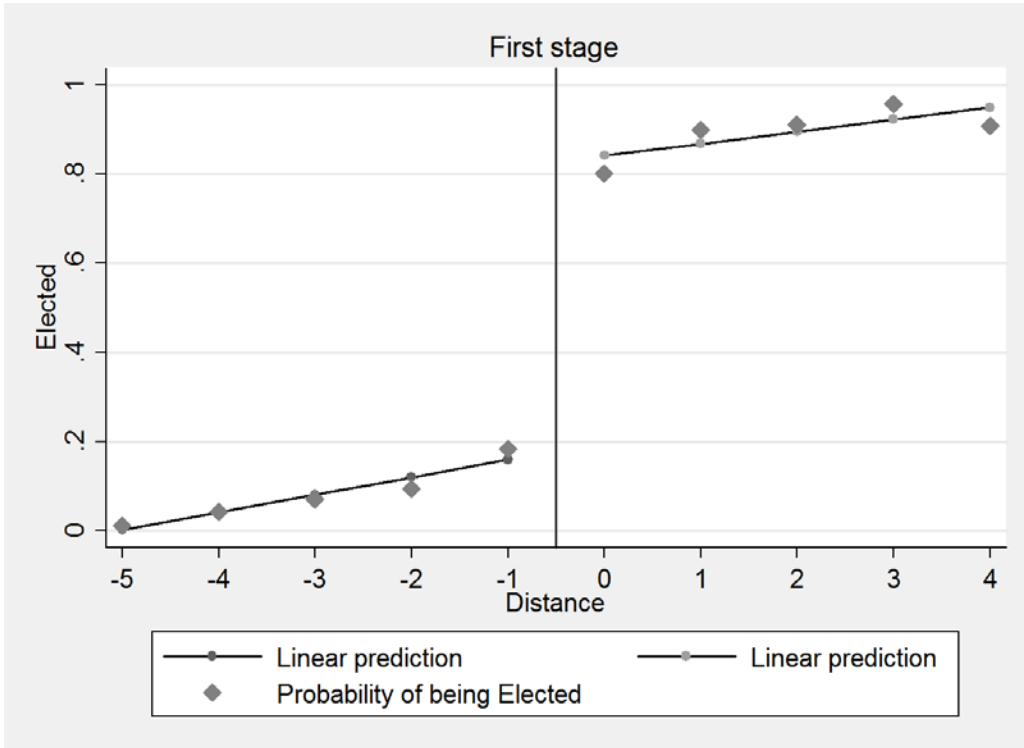


Figure 3. First stage: being elected to a SU council as a function of distance to threshold (ranking)

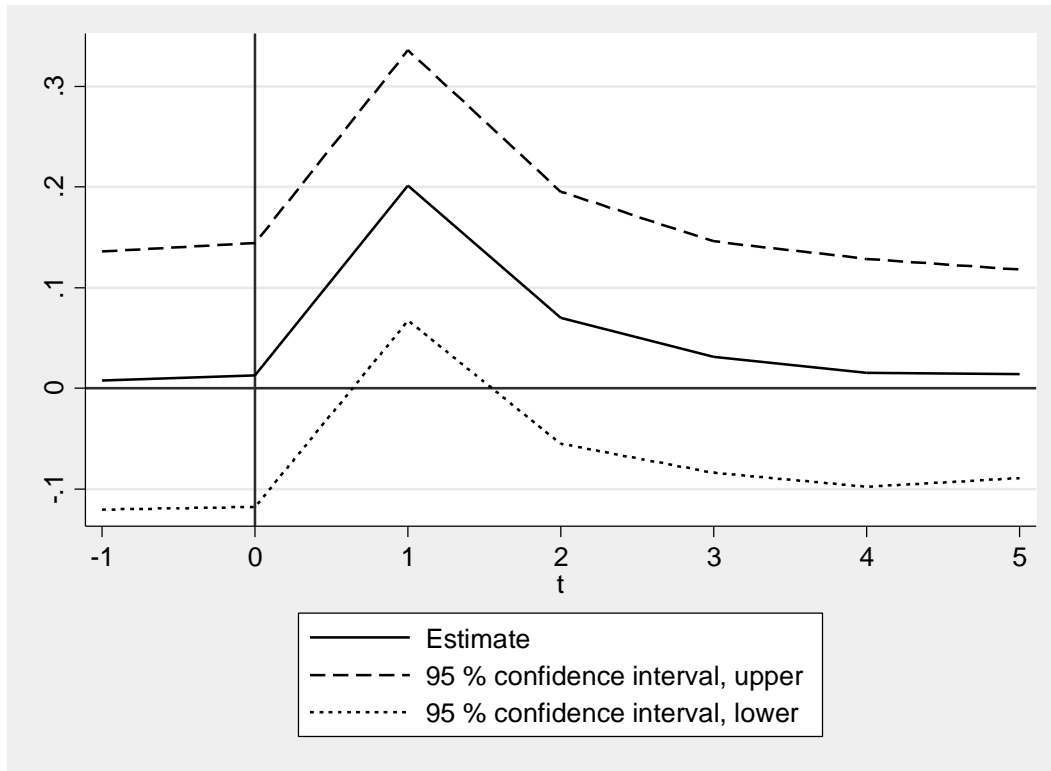


Figure 4. The impact of becoming a student union representative on employment up to five years after SU council elections

Notes: The figure depicts regression coefficients (and 95 % confidence intervals) from instrumental variables models (see equation 1) from $t-1$ to $t+5$, where $t = 0$ 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. X-axis: years relative to year of election ($t = 0$).

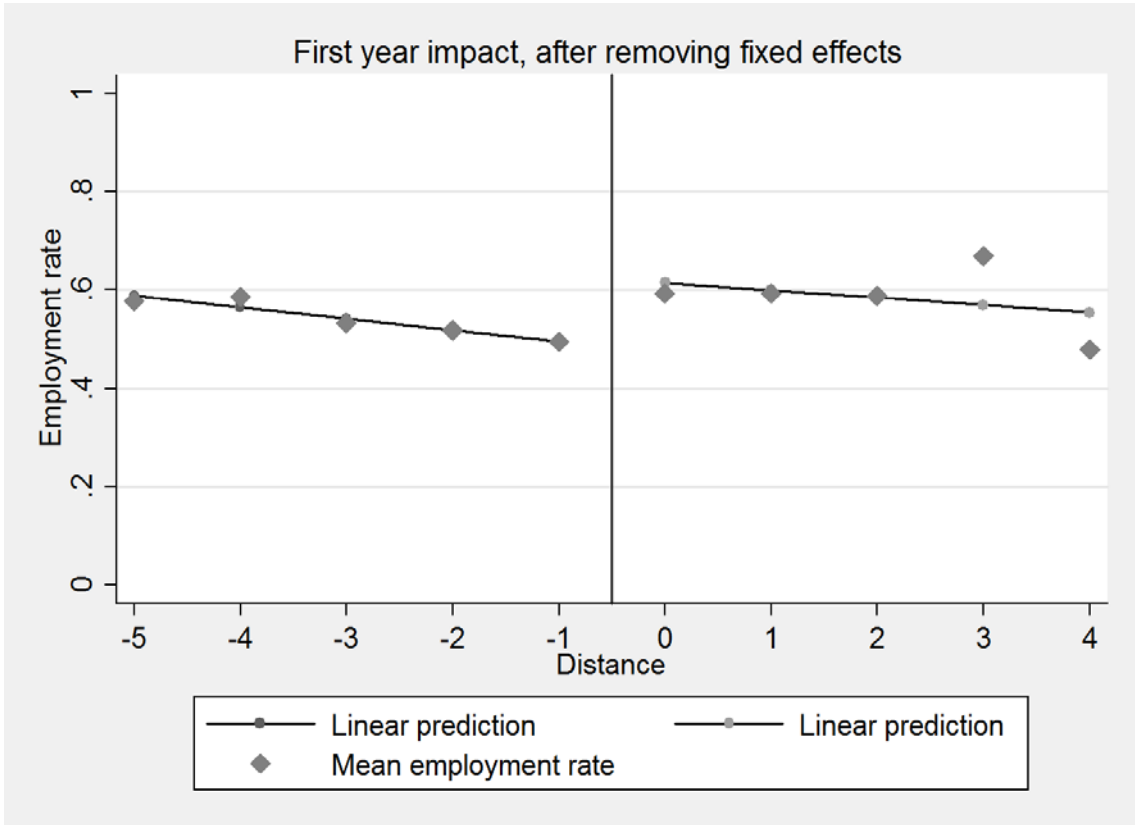


Figure 5. Relationship between list rankings and first-year employment

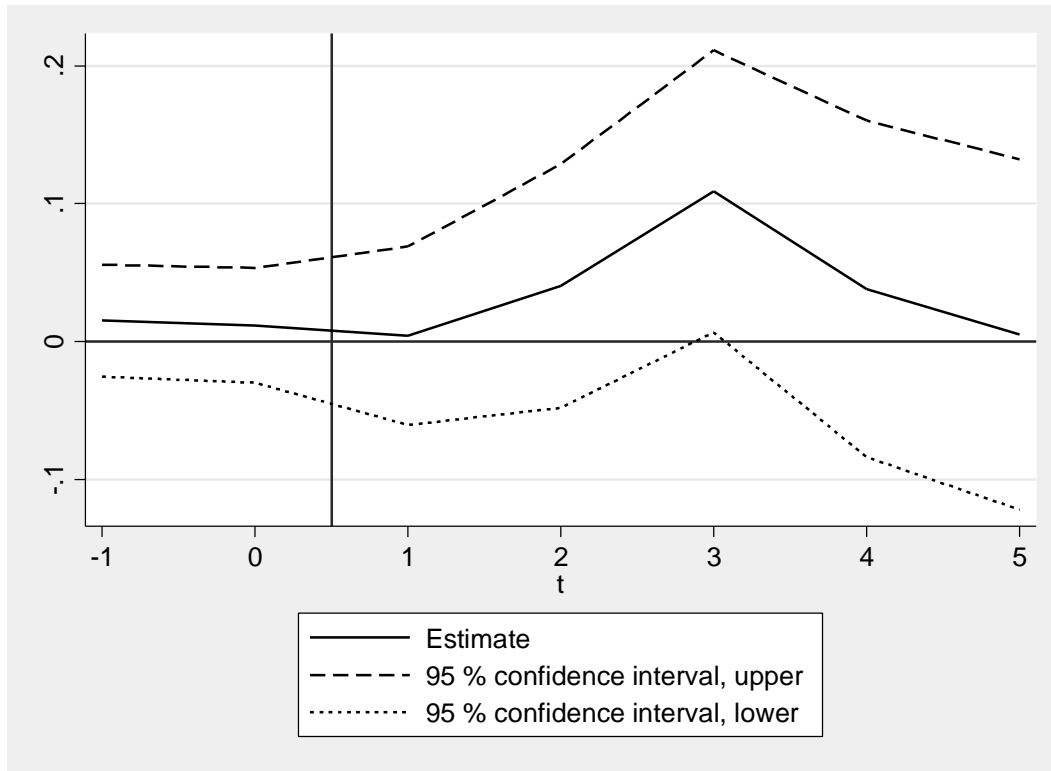


Figure 6. The effect of being elected to a SU council on the probability of holding a well-paid job

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 in a well-paid job (above the 50th percentile of earnings among college-educated 30-year-olds) changes if the student gets elected. X-axis: years relative to year of election ($t = 0$).

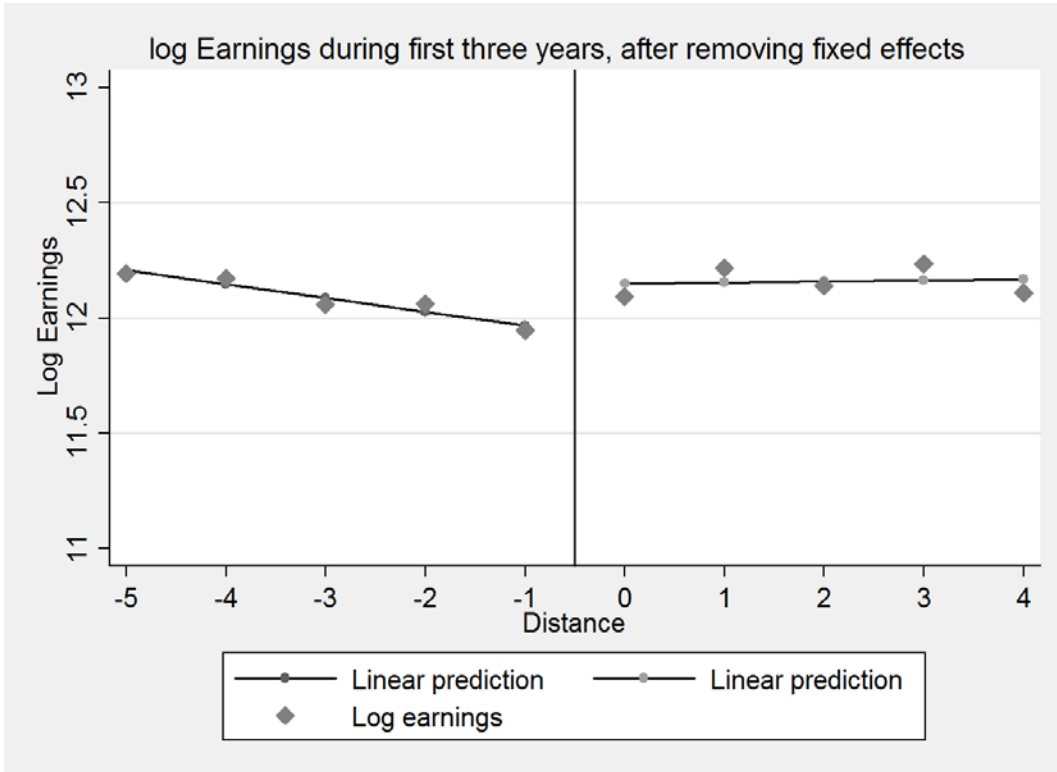


Figure 7. Relationship between list rankings and $\ln(\text{Earnings})$ one to three years after election

Appendix – additional tables and figures

Table A1. Number of observations by year and university

Year	Lund	Stockholm	Uppsala	Total	Average/year
1982	0	0	182	182	182
1983	0	0	0	0	0
1984-1990	1,467	0	0	1,467	210
1991-1993	606	0	0	606	202
1994	0	0	0	0	0
1995-1996	0	417	0	417	209
1997-2004	0	1,277	1,079	2,356	295
2005	0	126	0	126	126
2006-2010	-	-	-	-	-
Total	2,073	1,820	1,261	5,154	215

Note: We group years when data availability was equal during multiple subsequent years.

Table A2. Fraction of students by quartile of labor earnings among student representatives at age 30

	Quartile				All
	Lowest	2 nd	3 ^d	Highest	
Representatives	28.53	23.45	21.61	26.41	100
N	202	166	153	187	708
Failed candidates	26.31	23.52	22.84	27.33	100
N	463	414	402	481	1,760
All candidates	26.94	23.50	22.49	27.07	100
N	665	580	555	668	2,468
All graduates	25	25	25	25	100

Note: The data cover all student representatives (elected at least once) and failed (never elected) candidates that ran and graduated before age 30. Quartile thresholds are calculated from yearly data among all 30-year-old college graduates residing in Sweden.

Table A3. Type of party

	Baseline	Left-wing party lists ^A	Lists that are <u>not</u> left-wing ^A	Lists represented on the SU board ^B	Lists <u>not</u> represented on the SU board ^B	Only lists with more than one seat
Estimate (s.e.)	0.200*** (0.066)	0.010 (0.156)	0.254*** (0.074)	0.243*** (0.088)	0.136 (0.102)	0.208*** (0.075)
N	2,106	511	1,595	999	1,107	1,868
Mean dep.var.	0.562	0.528	0.572	0.500	0.668	0.839
Sample, closest:	5+5	5+5	5+5	5+5	5+5	5+5
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Ranking	Yes	Yes	Yes	Yes	Yes	Yes
Ranking*above threshold	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 on each side. 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.

^A Left-wing parties are parties with names that clearly identifies a political affiliation to the left of the Social democrats. All parties in our “leftist” category (apart from the party “Argus”, a well-known Swedish organization) have names including the terms “Left”, “Radical”, “Communist”, and/or “Socialist”.

^B Lists represented on student union board include all lists where at least one of the elected members where a board member, a chairman of the council or a vice chairman of the council.

Table A4. Effects on Part Time vs. Full Time employment one year after the SU election

	Baseline (all jobs)	Elections from 1995 onwards				Not sampled in working-time survey
		Baseline (all jobs)	Excluding sampled Part Time	Sampled Part Time	Sampled Full Time	
Estimate (s.e.)	0.200*** (0.066)	0.183** (0.072)	0.211*** (0.074)	-0.028 (0.046)	0.080 (0.054)	0.131** (0.067)
N	2,106	1,310	1,310	1,310	1,310	1,310
Mean dep.var.	0.562	0.580	0.493	0.086	0.167	0.327
Sample, closest:	5+5	5+5	5+5	5+5	5+5	5+5
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Ranking	Yes	Yes	Yes	Yes	Yes	Yes
Ranking*above threshold	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Data on working time are available from 1996 onwards. Survey covers all public sector jobs, all jobs in large firms and a sample of smaller firms, on average about half of all private-sector employees are sampled. 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 on each side. 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 A5. The impact of being elected to a SU council on holding a well-paid job after three years and log earnings: robustness

Panel A Well-Paid jobs after 3 years	1	2	3	4	5	6
Estimate	0.109**	0.116**	0.109**	0.200*	0.175**	0.085
(s.e.)	(0.054)	(0.054)	(0.052)	(0.104)	(0.074)	(0.056)
N	2,139	2,139	2,139	2,139	1,441	532
Sample, closest:	5+5	5+5	5+5	5+5	3+3	1+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 terms	No	No	No	Yes	No	No
Panel B log earnings after 1-3 years	1	2	3	4	5	6
Estimate	0.336**	0.367**	0.336**	0.429	0.387*	0.234
(s.e.)	(0.151)	(0.154)	(0.148)	(0.294)	(0.210)	(0.170)
N	2,045	2,045	2,045	2,045	1,376	508
Sample, closest:	5+5	5+5	5+5	5+5	3+3	1+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 terms	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. Observations with zero earnings in years 1-3 are removed in Panel B (less than 5 percent of the sample). Covariates are the ones presented in Table 2 and indicators for missing values of the last three of these. “Sample, closest 5+5” indicates closest 5 on each side. 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.

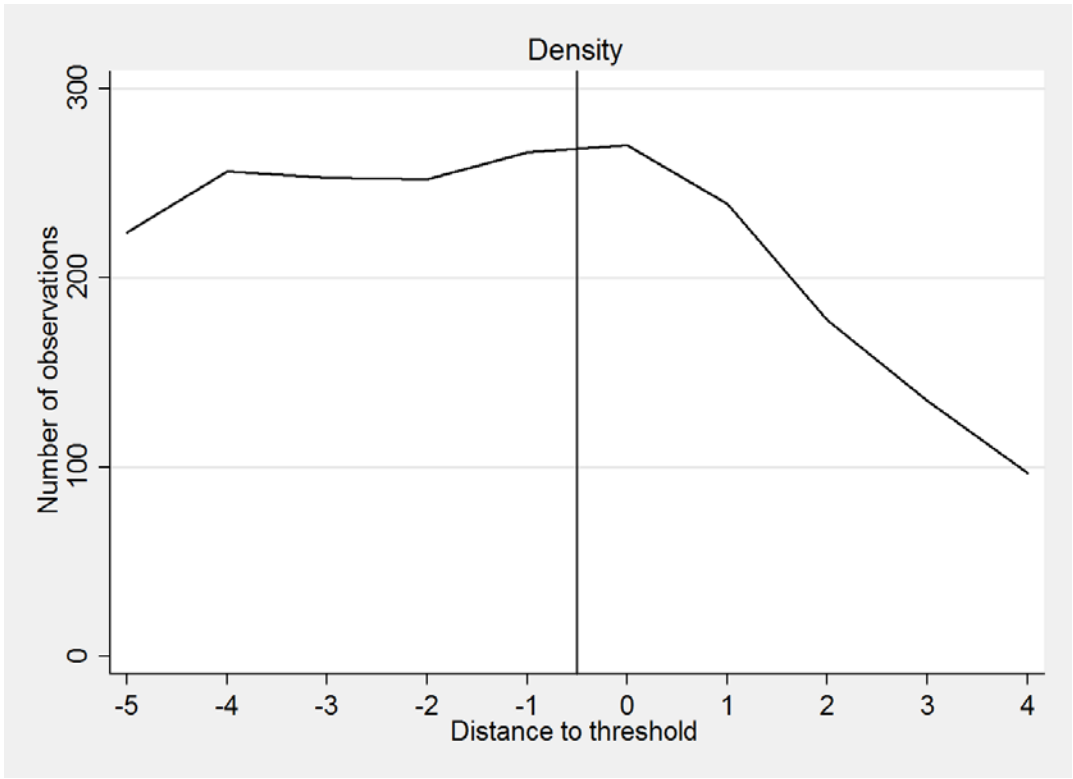


Figure A1. The number of observations by rank relative to the threshold

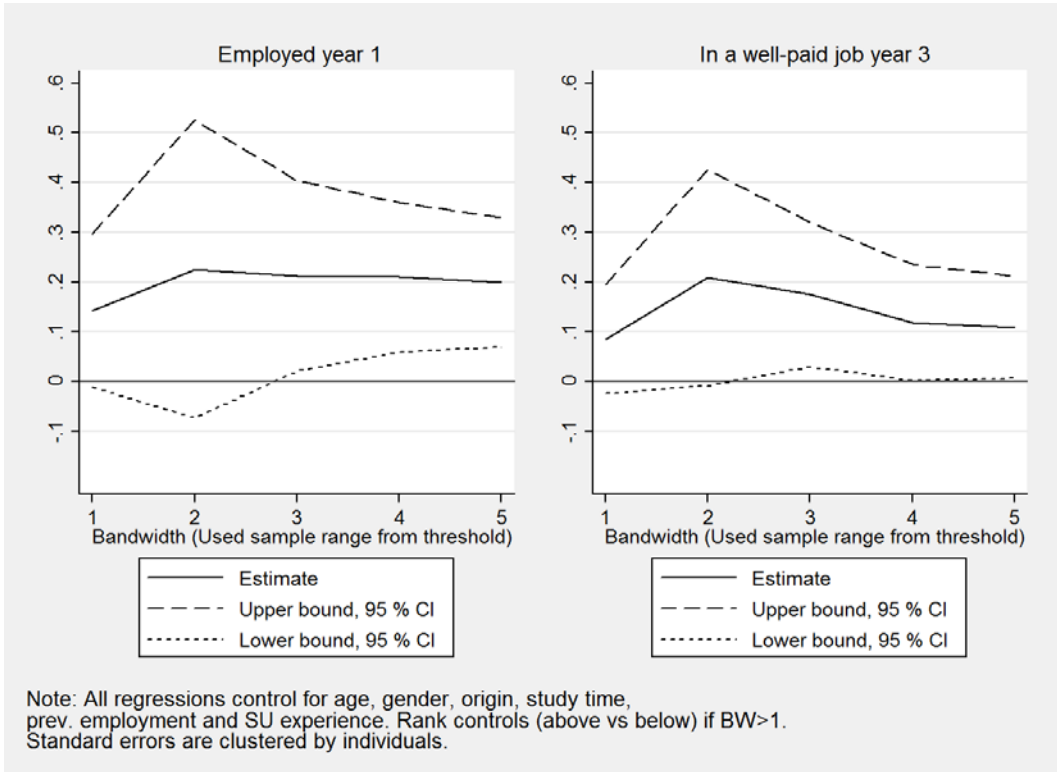


Figure A2. Estimates for different bandwidths for the probability of being employed year 1 and the probability of holding a well-paid job after three years

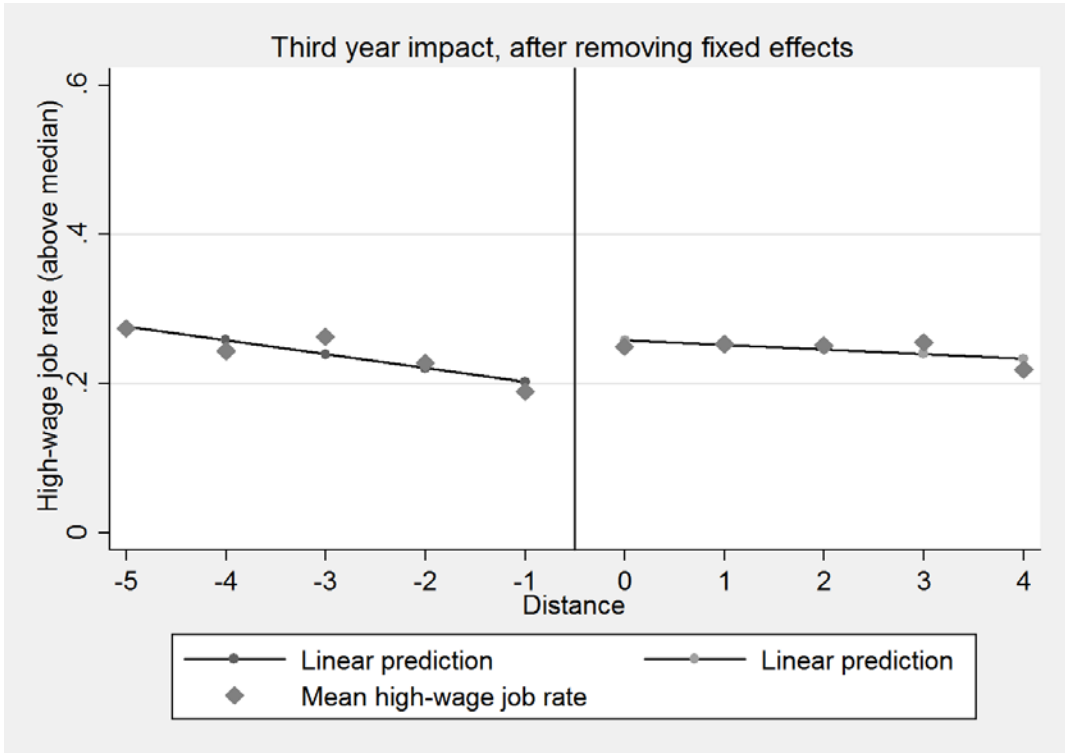


Figure A3. Reduced form relationship between list rankings and the probability of holding a well-paid job (median) after three years from election