Politicians' Payments in a Proportional Party System*

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Abstract

Are there monetary returns to politics? This question is approached in this paper, as one of few to quantify the monetary returns to holding political office in a country with proportional representation system. I apply a difference-in-difference setting with a carefully chosen control group to rich data on candidates to the Swedish national parliament. Both short and long-run effects of being elected on different types of income are estimated. Results show that, yes, mostly thanks to relatively high remuneration while still in office, politics can yield positive monetary returns. In the long-run however, the effect is instead compositional in the sense that ex-politicians receive more pension income and work less.

Keywords: Returns to politics, difference-in-difference
JEL codes: C23, D72, J44

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1 Introduction

There are plenty of rich politicians, and in particular rich *ex*-politicians who have become wealthy after exiting politics. To take one of the most extraordinary examples, former US president Bill Clinton earned an average of $189,000 per speech in the period after he left the oval office. And this kept him busy; holding close to one speech per week during his first decade as an ex-president, he cashed in an astonishing $90 million.[1] This suggests that politics is a lucrative business. In other words, monetary returns to political office seem to be high. But does it represent typical returns to office?

In an attempt to answer that, two aspects deserve emphasis. First, it is possible that such extraordinary examples are just that—that is, *extraordinary*—and therefore tend to be the only examples we come across. But more importantly, it is necessary to distinguish these successes from where they would have been, had they not been elected. Otherwise, one cannot be sure the successes are the result of a past political life. In other words, one needs to figure out what the counterfactual is.

This paper identifies and estimates the causal monetary returns to politics. The empirical strategy is to apply a difference-in-difference (DD) framework to detailed, comprehensive data on all candidates who ran for the Swedish parliament in the 1990s and 2000s. The idea is that non-winning candidates are comparable to winning candidates on all relevant grounds except for the success of the election. Pre-post-election-differences in their income therefore represent the counterfactual to being elected into parliament. Now, a caveat with this idea is that non-winning and winning candidates might not at all be comparable. Presumably, politicians are elected for a reason. In dealing with this, the details of the data will prove to be truly valuable. In particular, information about how close to being elected each individual candidate was is used to construct a control group that is much more comparable to elected politicians than the average non-winning candidate. With this strategy, I estimate causal effects of being elected into politics on various income measures—that is, on the returns to political office. Several robustness checks as well as an alternative identification strategy indeed support this claim.

Returns to office play a crucial role for who the politicians are and how they behave. The returns are what motivate politicians—that is, they are the reason why some individuals find it worthwhile to forgo time and perhaps money in trying to get elected (Downs, 1957; Merlo, 2006).[2] Once in office, higher returns can also work as an incentive for politicians to do a good job—that is, the higher the returns to office, the higher the returns also

[1] http://edition.cnn.com/2012/07/03/politics/clinton-speaking-fees/

[2] Although for some, the possibility of implementing some desired policy can be the main driver (Besley and Coate, 1997; Osborne and Slivinski, 1996).

to be *re*-elected, and hence the more reason to conduct policy in line with the voters' wishes (Besley, 2004; Ferraz and Finan, 2009; Gagliarducci and Nannicini, 2013).

Despite their relevance, for a long time we knew very little about what, more precisely, the returns are. Recently however, there has been an increasing interest in these issues, and we now have at least limited knowledge about what they are. Most is known about the US. The dynamic structural model estimated by Diermeier et al. (2005) suggests that a seat in the House is worth $600,000 and a seat in the Senate is worth $1,700,000. Their model is able to disentangle the pecuniary value and the non-pecuniary utility from holding office. They conclude that the latter play a significant role. That also the pecuniary returns from US politics *can* be substantial, at least under certain conditions, is shown by Querubin and Snyder (2013). Investigating the wealth accumulation by US congressmen during 1850–80, they find large positive effects in the Civil War era. This is a period characterized by high government spending, and a lot of media attention drawn to the war and little to extractive politicians. There is evidence that also concurrent US Congress members gain financial wealth presumably from (ab)using information and political power (Schweizer, 2011). This is however put into question by Eggers and Hainmueller (2013). They instead find that average benefits from stock trading are absent, which they interpret as a success for accountability mechanisms.

Outside the US, we know from Eggers and Hainmueller (2009) that conservative candidates who ran successfully to the British Parliament gained £250,000 compared to those who ran but were not elected. The authors collected estates of deceased members of the British parliament, and conclude that successful politicians died almost twice as wealthy. And from Fisman et al. (2014) we learn that the conditions for positive wealth effects of politics can be right also in the context of developing countries—in their case India. This is true at least in the short run, and at least among the most prominent Indian politicians.

Thus, evidence from a rather limited set of countries (India, the US and UK) shows that the returns to political office are large or even huge.[3] But there is also suggestive evidence for the opposite from other countries; Berg (2018) estimates that there are no monetary returns at all to being elected into a Swedish local council. Kotakorpi et al. (2017) do find substantive positive income effects of being elected into the Finnish national parliament. But in the regression discontinuity (RD) design they use for identification, the estimated effects fade out quickly over time.

It is no surprise that studies on returns to politics in different countries and different settings yield different results. Although it is hard to say

[3]See also Peichl et al. (2013), who estimate the so-called politicians' wage gap by comparing politicians to individuals in executive positions.

exactly what features make the political returns large or small, the contribution of this paper is to provide new and credible evidence of what the returns can be in a setting like the Swedish.

Specifically, (i) I study returns to national rather than local politics. This is in contrast to Berg (2018) who only looks at the local level. And zero returns to local politics say little about the returns to national politics. Furthermore, (ii) the empirical model is constructed so that it truly makes sense to consider long run effects. Unlike an RD strategy, the DD strategy employed defines treatment and controls groups that are consistent over time. Interpreting the insignificant long run estimates in Kotakorpi et al. (2017) as lack of long-lasting income effects could be a bit rash. The reason is that the RD estimates fading out over time likely reflects that the differences in treatment (being elected) fade out over time. This is because many candidates in the control group—candidates who were close to being elected in a given election—often run again and indeed are elected in the subsequent election.[4] And (iii), by applying the DD method to rich income data in combination with information on the length of the politicians' careers, novel insights into the returns to politics are obtained. Indeed, by distinguishing between income from different sources (labor income, pension, capital income, income on-the-side from private firms etc.), it is possible to learn about possible mechanisms.

I find that the average politician's disposable income increases with around 20% as a result of being elected into the Swedish national parliament. As long as they stay in office, these rather large income effects persist. Further analysis on various types of income suggests that the main mechanism is the relatively high direct remuneration, rather than labor or capital income on-the-side (where there is a slight tendency for negative effects). Following the argument in Eggers and Hainmueller (2013), these results are overall good news from an accountability point of view. For those who leave, there are no long-run effects on the level of disposable income. There are, however, interesting long-run compositional effects; among former MPs, the same level of disposable income is to a larger extent achieved through non-labor income (pensions), as compared to those never elected into parliament.

The finding in the paper that outside income plays little role partly contrasts the study by Gagliarducci et al. (2010) on "moonlighting politicians". At least indirectly, this finding is also related to the literature on revolving doors and the value of political connections for firms (e.g., Faccio, 2006; Fisman, 2001; Goldman et al., 2008; Luechinger and Moser, 2014). The results on positive pension effects connect to a set of papers mostly on US politi-

[4]As a robustness check, I also estimate effects with an RD design. Because of the problem that closely defeated candidates tend to be elected in subsequent elections, these RD estimates are only obtained for the first election period.

cians' retirement decision; see, e.g., Groseclose and Krehbiel (1994) and Hall and Van Houweling (1995).

Based on the same data covering Swedish political candidates, a set of—more or less—related results have been presented in previous papers. The study on *local* monetary returns to politics by (Berg, 2018) has already been mentioned above. Another related example is Folke et al. (2017) who show that the future income and level of education among children of closely elected local mayors are positively affected. Additionally, Folke and Rickne (2020) find substantially increased divorce rates among women after being elected to a top political position, but not among men. The same data has also been used to study selection of political candidates. These studies characterize Swedish politicians as an "inclusive meritocracy" (Dal Bó et al., 2017) that can be affected by gender quotas (Besley et al., 2017) as well as preference votes (Folke et al., 2016).

The next section describes how Swedish members of the parliament are elected and the remuneration that they get. Section 3 introduces the data, and explains how the data is used in the difference-in-difference strategy that estimates the effects on income of being elected into the parliament. The results are presented and discussed in Section 4, followed by concluding remarks.

2 Swedish MPs

The Swedish parliament has 349 members currently representing eight parties (seven during the studied period). Election terms last for four years, and there are no term limits.[5] MPs are elected from 29 electoral districts in separate, proportional elections. Parties play a crucial role in the elections. Candidates can only run by running for a party, and voters choose a party rather than a candidate to vote for. Parties running for elections do so by ranking their nominated candidates on ballot papers. Naturally, overall popularity plays a role in these rankings, but so does representativity in terms of gender, age, experience and political standpoints. Voters then vote by casting these ballot papers. The resulting distribution of votes results in a seat distribution between parties. Given this distribution, the seat distribution within parties (that is, who will fill the seats) is then determined by the candidate rankings.[6] Each party typically has a single list per district from which the mean (median) number of elected candidates is a low 2.2 (1). The mean (median) number of listed candidates on a list is 30 (25).

[5]Reelection rates are quite high; around 60 and 30 percent over one and two elections, respectively.

[6]Starting with the 1998 election, voters can mark one preferred candidate on the ballot paper (so-called preference voting). A candidate who is marked for a preference votes is in effect ranked first on that particular voter's ballot paper.

Since 1994—the earliest post-election year in the analysis—wages of the 349 elected MPs are set annually by a remuneration committee. The committee consists of three people appointed by the Board of the Parliament (*Riksdagsstyrelsen*). The monthly wage has more than doubled since from 26,500 in 1994 to 57,000 SEK (approximately from $2,900 to $6,300) in 2011—the latest year in the analysis.[7] Even adjusting for inflation, this increase implies that, in terms of direct remuneration, it has become more economically rewarding to be elected into the parliament. There are no rules about income from other sources.

In terms of payments directly from the parliament, ex MPs can collect old-age pension after they turn 61 (although it is financially beneficial to wait until 65). The longer they have been in parliament, the higher the pension. Younger ex MPs are instead eligible for a type of compensation that just until recently was termed "guaranteed income" (*inkomstgaranti*). The purpose of the guaranteed income was to ease the transition back into the labor market. It was thus not intended to be permanent. However, it was quite generous; potentially as high as 80 percent of the previous parliamentary wage but reduced with other earning, and *could be* collected until the age of 65.[8]

These types of income sources are all included in the data used to estimate the returns to political office. The following section provides the details of the data and the method.

3 Data and method: Applying party lists to a DD framework

This section presents the data, and how the data is used to identify the effect of being elected into the Swedish national parliament.

3.1 Data

The data for this paper covers all candidates who ran for the Swedish Parliament in any of the six elections held during the period 1991–2010.[9] There are several important features of the data. First, there is very detailed information about the elections. In particular, each candidate's ranking on

[7]The Annual Report of the Remuneration Committee (*Riksdagens arvodesnämnds verksamhetsredogörelse till Riksdagen 2014*, 2014/15:RAR1).

[8]The compensation scheme has been criticized and is now changed. Those elected into the parliament for the first time in the 2014 election are instead eligible for the compensation in at most two years after they exit. In addition, rather than calling it guaranteed income, it has been relabel "transitional aid" (*omställningsstöd*), as the former was thought to send out the wrong signals about its purpose.

[9]Since only one year of post-election data is covered, the 2010 election will not be included. Data comes partly from Statistics Sweden, partly from the Swedish Election Authority, and has been put together by the former.

the party list is included. This makes it possible to separate out candidates who were far down the list and who therefore may not be a very good comparison to those in the top who were elected.[10] Second, it contains the same information on all candidates irrespective of whether they were elected or not. Third, to all the candidates, rich register-based information on various income measures such as disposable income, labor income and pension income are matched using a unique person identifier. Likewise, the registers also cover individual characteristics such as age, sex, foreign background, educational attainment and occupation. The registers are in annual form and cover the years 1990–2011 for all candidates. This thus enables an empirical analysis that follows candidates over a relatively long time period.

Table 1 provides summary statistics of the variables used in the analysis, separately for the elected and non-elected candidates. As described below, these two groups of candidates will be classified into a treatment group and a control group, respectively.

3.2 Defining the treatment and control groups

As displayed in Table 1, a treatment and a control group is defined in the data. The two groups are used in a difference-in-difference (DD) framework that estimates the treatment effect of interest—the effect of being elected into the parliament on future income. The treatment group consists of the 539 candidates elected for the first time in any one of the elections in 1994, 1998, 2002 and 2006. The control group instead consists of candidates who also ran in any of these election, but without ever being elected. As hinted above, however, only candidates who are ranked "sufficiently high" are considered for the control group. Specifically, in most specifications, only as many non-elected candidates as elected candidates off of a given list are potentially defined as controls (or even fewer in a robustness check). Note though, that potential controls in the end only enter the control group if they are not elected in later elections. With these definitions, 1101 candidates qualify for the control group.[11]

A contribution of the analysis is to distinguish between different lengths of the political office. This is the reason why treatment is defined when a candidate is elected for the first time. Because the data starts with the 1991 election, the previous success of candidates in this election is not observed. Candidates in the 1991 election is therefore not part of the sample. Along the same line of reasoning, previous experience is limited to only one earlier election for candidates in the 1994 election. Those elected in 1994 but not elected in 1991 are defined as being elected for the first time in 1994, even

[10] Information on the list placement is missing for the 1994 election. I discuss later in the paper how I deal with this.

[11] Please refer to Appendix A for further details on how the treatment and control groups are defined.

Table 1: Characteristics of candidates in the treatment and control group, measured one year before the election

	Treatment group	Control group	t-stat. of Δ
Disposable income	2580.5	2083.4	8.97
	(1141.3)	(1007.0)	
Labor income	3608.7	2642.6	10.91
	(1809.7)	(1617.5)	
Pension income	102.0	87.4	0.60
	(511.1)	(436.3)	
Age	44.7	43.4	2.36
	(10.1)	(11.3)	
Married	0.60	0.60	-0.04
	(0.49)	(0.49)	
Children under 18	0.66	0.83	-3.06
	(0.99)	(1.13)	
Less than high school	0.069	0.068	0.05
	(0.25)	(0.25)	
High school graduate	0.26	0.29	-1.02
	(0.44)	(0.45)	
$<$ 2 years university	0.12	0.14	-1.48
	(0.32)	(0.35)	
$\geq$ 2 years university	0.51	0.49	0.93
	(0.50)	(0.50)	
Graduate studies	0.041	0.015	3.18
	(0.20)	(0.12)	
Female	0.47	0.49	0.76
	(0.50)	(0.50)	
Born in Sweden	0.94	0.92	1.80
	(0.24)	(0.28)	
Born in other Nordic country	0.015	0.017	0.36
	(0.12)	(0.13)	
Born in non-Nordic Europe	0.022	0.026	-0.50
	(0.15)	(0.16)	
Born in North America	0.0019	0.0018	0.02
	(0.043)	(0.043)	
Born elsewhere	0.020	0.039	-1.99
	(0.14)	(0.19)	
Both parents foreign-born	0.0093	0.012	-0.46
	(0.096)	(0.11)	
Candidates	1101	539	1640

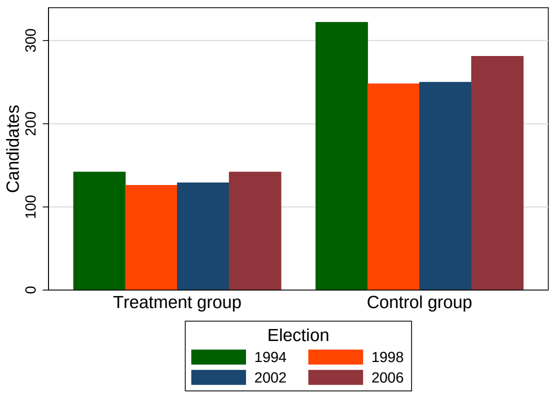
Note: The treatment group consists of candidates elected for the first time in any of the elections in 1994, 1998, 2002 and 2006. The control group instead consists of candidates who also ran in any of these election, but without ever being elected. Columns 1–2 report the mean and standard deviation (in parentheses) of variables measured one year before the relevant election. Column 3 reports the t-statistic of a test of equal group means. Income is measured in 100 SEK deflated to 2000 year values (9 SEK≈1 USD). Disposable income is net of taxes, labor and pension income are gross. The education variables indicate highest completed level. Born elsewhere equals one for individuals born in Africa, Asia, Oceania, Russia or S. America. Both parents foreign-born equals one for individuals born in Sweden but with both parents foreign-born. All variables but the income variables, Age and Children under 18 are binary. There is missing information for at most five individuals on some of the variables.

Source: Statistics Sweden.

though there is no information on whether or not they were elected in 1988 or earlier. This implies that the treatment and control groups from the 1994 election may be partly misclassified. Fortunately, the candidates in the later elections (whose history can be observed) show that such a pattern of moving in and out of the parliament is very rare; among the MPs elected in election t but not elected in $t-1$, only 5–6 percent were elected in $t-2$. This thus suggests that the risk of misclassifying a candidate who was elected in 1994 but not in 1991 as being elected for the first time in 1994 is small.

Figure 1 shows the number of candidates in the treatment and the control group separately across the four elections. The former is more or less uniformly distributed. But there are slightly more control candidates from the 1994 and the 2006 elections. This is because only those who were not elected in a given election *nor in any of the later elections studied* are part of the control group. This has implications for the first and the last elections: As noted above, there are no list rankings in the 1994 election data. To deal with this, instead of considering the top ranked non-elected candidates for the control group, a random sample of all the non-elected candidates were considered. And among this group, fewer were disqualified because they were elected in subsequent elections. Furthermore, as for the 2006 candidates, there simply are no later elections that can disqualify them for the control group.

Figure 1: Number of candidates in the treatment and control groups



Source: Statistics Sweden & The Swedish Election Authority.

3.3 Identification and estimation

The treatment and control groups, as just defined, make up the sample used to identify the effect of interest. In particular, the following equation estimates a DD effect τ of being elected into the parliament on income Y in year t for candidate i running in election year j:

$$
\begin{aligned}
Y_{ijt} = \tau \ elected_{ijt} + election_j \times year_t + election_j \times cand_i \\
+ \beta \ age_{ijt} \ (+\Gamma' \mathbf{X}_{ijt-1}) + \varepsilon_{ijt} \qquad (1)
\end{aligned}
$$

The treatment variable of interest, $elected_{ijt}$, is a dummy variable taking the value 1 for all $t > j$ if candidate i was elected for the first time in election year j, and 0 otherwise. What makes this a DD estimation are the time and candidate fixed effects. Note that these two fixed effects, $election_j \times year_t$ and $election_j \times cand_i$, are allowed to vary with election. The first interaction accounts for the fact that a specific year may have a different impact of the outcome depending on in which election the candidate ran. For example, year 1998 is the year of the election for the 1998 "cohort" but year $t + 4$ for the 1994 "cohort", and this may matter. Furthermore, a given candidate can be part of several cohorts (that is, run in several elections). The second interaction takes into account that the average outcome, conditional on the time-varying X-variables, may differ for different elections for these candidates.[12]

Besides the fixed effects, all regressions control linearly and quadratically for age, age_{ijt}. These controls are important, as otherwise the average 2-year difference between the elected and the non-elected candidates (see Table 1) could imply different counterfactual future income trajectories. This would thus imply that the identifying assumption of parallel counterfactual trends fails to hold. Mostly to ease visual interpretation, the observations are weighted using so-called entropy balancing (Hainmueller, 2012), so that the pre-election income level and age of the treatment group match those of the control group.

Furthermore, the vector $\mathbf{X}$ in equation (1) contains the candidates' marital status, number of children and indicators for highest completed education. That is, things that potentially could vary over time and would therefore not be captured by the candidate fixed effects.[13] Although these variables surely might affect income, it is unlikely that potential *changes* in these variables are correlated with whether or not the candidate is elected. Adding them to the regression should therefore not change the estimate of τ. Therefore, including this vector in some regressions, but not all, serves as a robustness check of the results. Finally, all regressions cluster the error

[12] The vast majority of candidates are only part of the treatment or the control group in one cohort. Consequently for them, a simple candidate fixed effects is sufficient. By definition, as it is only possible to be elected for the first time once, this is true for everyone in the treatment group. In contrast, 135 of the 1101 control candidates qualify for the control group in several elections (118 for two elections; 16 for three elections; and 1 for all four elections). In practice though, the election interactions turn out not not matter for the results (available upon request).

[13] These variables are measured one year before the outcome variable.

term ε_{ijt} on candidates. This deals with potential serial correlation for a given individual over time.

For τ to capture the causal effect of being elected, the identifying assumption of parallel counterfactual trends must be fulfilled. In other words, the income evolution of the control group should represent that of the treatment group, had the latter not been elected. Note that this assumption is stated in terms of changes in income over time. Differences in the *level* of income prior to the relevant election are thus allowed. Indeed, Table 1 shows that disposable income and labor income are higher in the treatment than in the control group one year prior to the election. What the identifying assumption says is that, to the extent that these differences change after the election, this is only due to the fact that the treatment group was elected but the control group was not.

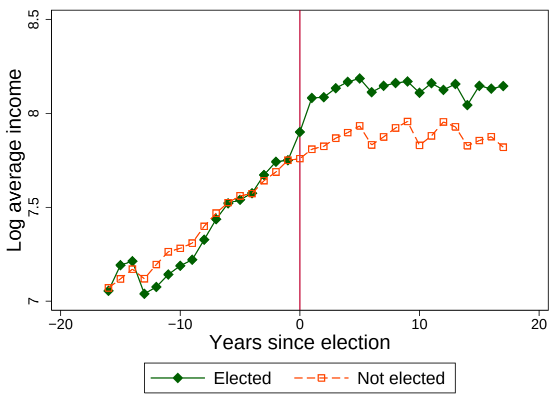
Because it is crucial for a causal interpretation of the results, the likelihood of this assumption will be investigated in several ways. First, the assumption is more likely to hold the more similar the treatment and the control groups are a priori. This is the reason why only sufficiently highly ranked non-elected candidates are part of the control group. For robustness, the control group will be further restricted to only those non-elected who were at the very margin to be elected. Second, as should be clear from the above description, thanks to data from several elections, the treatment of being elected for the first time is sequential. This is exploited in another robustness check of the results. The control group is then defined as those elected in subsequent elections instead of those who are never elected. Third, I estimate two types of "placebo effects"; one between only non-elected candidates and one in years prior to the election. Finally, to provide further evidence of the robustness of the results, as an alternative identification strategy I apply the RD design developed in Berg (2018). To avoid the the problem of non-elected candidates in a given election being elected in subsequent elections, these effects are only estimated for the first election period.

4 Results: The returns to politics

This section presents the results of the analysis of the effect of being elected into parliament on future income. Some of the results are presented graphically. The corresponding estimation results are then mostly referred to the Appendix.

The main result of the paper is given in Figure 2. It shows, log annual disposable income, separately for candidates in the treatment and control groups as defined in the previous section. The x-axis is centered at the year of the election (1994, 1998, 2002 or 2006). As noted above, the observations are weighted so that the pre-election income level and age of the treatment group match those of the control group. This is partly to ease visual interpretation, but is also a way of controlling for pre-election differences between the treatment and control group.[14] In practice, aside from adjusting the level of the series in the graphs, the reweighting has very little impact on the results. This can be seen from comparing the econometric estimates in Table 2 below and the their non-weighted counterparts in Table 7 the Appendix.

Figure 2: Disposable income among treated and control candidates in elections 1994, 1998, 2002 and 2006



Note: The figure plots average disposable income among candidates in the treatment and control groups from the elections in 1994, 1998, 2002 and 2006. Income is measured in logs of 100 SEK deflated to 2000 year values. Observations are weighted so that the pre-election income level and age of the control group match those of the treatment group.

Source: Statistics Sweden & The Swedish Election Authority.

The two income series in Figure 2 follow each other rather closely up until the time of the election. At that point, the income of those elected distinctively jumps, and remains higher throughout the studied period. Under the assumption that the income trajectory of the control group represents the counterfactual evolution for the treatment group, this increase constitutes the causal effect of being elected into parliament. Column 1 of Table 2 estimates this effect to a statistically significant 0.226. That is, the effect of being elected is roughly a 20 percent increase in disposable income. Besides the entropy weights, this regression includes the full control group and controls for age linearly and quadratically. Columns 2–4 provide various robustness tests for this result; column 2 adds controls for marital status, number of children and indicators for highest completed education; column 3 restricts the control group to only include the one candidate who was just on the margin to being elected from a given list; column 4 excludes

[14]Recall from above that there are pre-election income differences (cf. Table 1). Recall also, that the identifying assumption is that in the absence of treatment, these differences remain constant. Controlling for age is therefore potentially important.

candidates who are never elected and instead uses those who are elected in subsequent elections as control group in a given election.[15] As can be seen, the estimated effect is very robust—even to the specification that defines a completely different control group using only the 539 (eventually) elected candidates.

Table 2: Total effects of being elected on disposable income (in logs)

	Elections: 1994–2006				Election: 1998		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Elected	0.226***	0.232***	0.235***	0.212***	0.206***	0.203***	0.214***
	(0.0204)	(0.0195)	(0.0295)	(0.0201)	(0.0337)	(0.0329)	(0.0472)
Sample	Full	Full	Restr.[1]	Restr.[2]	Full	Full	Restr.[1]
Additional X	No	Yes	Yes	Yes	No	Yes	Yes
Candidates	1636	1636	993	539	373	373	220
Observations	34965	34944	21314	11604	8015	8015	4741

Note: All regressions are weighted so that the pre-election income level and age of the control group match those of the treatment group (except for in column 4), and include election-by-year fixed effects, (election-by-)candidate fixed effects and controls for age and age^2. "Restr.[1]" indicates samples that exclude non-elected candidates who were not marginally close to being elected. "Restr.[2]" indicates samples that exclude all non-elected candidates. "Additional X" are marital status, number of children and indicators for highest completed education. Standard errors clustered on candidate are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

In levels, the estimated percentage income effect of being elected is equivalent to an annual increase in disposable income of around 59,000 SEK, or \$6,500. This is about the size of the before-tax monthly remuneration at the end of the study period. This is a very general result, in several ways. First, it holds for the most broad definition of income. Disposable income is the sum of numerous types of after-tax income including labor income, capital income, pensions and unemployment and sickness benefits. Second, it refers to the aggregate effect of being elected, without specifying when the income increase kicks in (the graphical result in Figure 2 is more informative on this). Section 4.2 attends to these two aspects when exploring possible mechanisms.

Third, the estimated effect in columns 1–4 in Table 2 is the average effect for candidates from all four elections in 1994, 1998, 2002 and 2006. Columns 5–7 instead present baseline estimates along with robustness checks for the 1998 election only. Much of the remainder of the result section focus on this particular election. The motivation for this is that an analysis of how effects differ depending on length of the political career is facilitated by looking at one particular election. And the 1998 election is appropriate thanks to sufficiently long pre and post periods. As is seen in the table, the income effect for those elected for the first time in 1998 is very similar to the average.

[15] As each control candidate in column 4 is eventually treated, this regression is unweighted.

The estimated effect is around 20 percent. This effect is also robust to the inclusion of control variables (column 6) as well as to restricting the control group to only the marginal losers (column 7).[16]

4.1 Robustness

For the estimated effect to represent the causal effect of being elected, the assumption of parallel trends needs to hold. This is thoroughly investigated in this section, in particular by estimating "placebo regressions". In addition, to provide a sense of the robustness of the estimated effects, an alternative model that relies on discontinuities is estimated.

Consider, first, the estimation of placebo effects. Here, the control group from above constitutes the placebo treatment group. The new (placebo) control group instead consists of non-elected candidates even further down the list; either as many as the number of placebo elected, or just the ones on the margin of being placebo elected. In other words, the placebo estimations simply disregard those who indeed were elected and pretend as if the list starts with the first non-elected candidate. Table 3 presents the placebo results (the table is structured in the same way as the baseline results in Table 2). The point estimates are remarkably small, and are all non-significant. This means that the income in these two groups of candidates with different list placements evolves parallel after the election. Thus, this is yet an indication that the income of the candidates in the true treatment and control groups would have evolved parallel, had the treatment group not been elected.

We now turn to the estimation of the alternative model. Recall that as a robustness check of the DD model, the control group was restricted to the one candidate who was just on the margin to being elected from a given list (columns 3 and 7 of Table 2). Note that, despite the close margin-terminology, this is still a DD rather than a regression discontinuity (RD) design. This is because the source of identifying variation is between groups *over time*. In an RD, the source of identifying variation is instead *discontinuous changes* between groups *at a given point in time*. Berg (2018) develops a discrete RD design based on candidate rankings on party lists in Swedish local elections. This design is less suitable in the current application. This is partly because of the much smaller sample, and partly because non-elected candidates in a given election tend to be elected in subsequent elections. Yet, it can be applied to check the robustness of some of the results from

[16]Table 8 in the Appendix presents equivalent estimates for the 2002 and the 2006 elections. The results are similar to those for the 1998 election (and the average over all elections), although there is a tendency for somewhat larger effects in the 2006 election. Because the 1994 election lacks detailed data on candidate rankings, this election is not studied in isolation. See however separate graphical results for all four elections in Figure 7 in the Appendix.

Table 3: Placebo estimates of total effects of being elected on disposable income (in logs)

	Elections: 1994–2006				Election: 1998		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Placebo-elected	0.0316	0.0270	0.0305	0.0256	0.0458	0.0272	0.0363
	(0.0198)	(0.0186)	(0.0219)	(0.0188)	(0.0347)	(0.0330)	(0.0427)
Sample	Full	Full	Restr.[1]	Restr.[2]	Full	Full	Restr.[1]
Additional X	No	Yes	Yes	Yes	No	Yes	Yes
Candidates	2116	2116	1595	1596	489	489	355
Observations	45109	45061	33939	34101	10489	10489	7570

Note: All regressions are weighted so that the pre-election income level and age of the control group match those of the treatment group (except for in column 4), and include election-by-year fixed effects, (election-by-)candidate fixed effects and controls for age and age^2. "Restr.[1]" indicates samples that exclude non-elected candidates who were not marginally close to being placebo-elected. "Restr.[2]" indicates samples that exclude all non-placebo-elected candidates. "Additional X" are marital status, number of children and indicators for highest completed education. Standard errors clustered on candidate are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

the DD model.

The idea is to compare the income of a candidate who just barely won a seat to that of other another candidate who was close to winning a seat *for the same party*, but ultimately did not.[17] Because elections result in a fixed final ranking of the parties' candidates, the discontinuity between these candidates—who are referred to as the *borderline elected* and *borderline defeated*—is well-defined. Moreover, other candidates than these two can be used to detect and control for any possible direct effects on income of being more highly ranked.

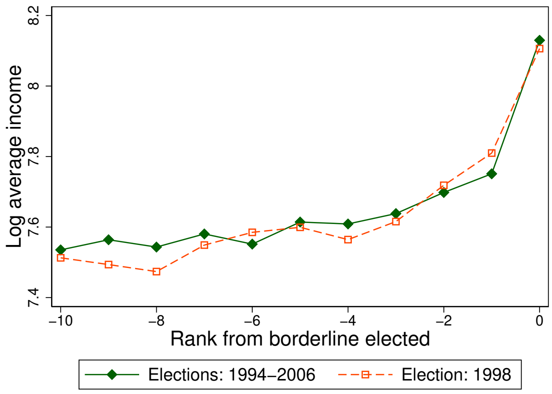
As mentioned, many of the borderline defeated candidates are elected in subsequent elections. This means that estimated income effects from the two models can only be compared in the first election period. Figure 3 shows graphical results from the RD model on disposable income 1–3 years after the election, for all four elections 1994–2006 as well as for the 1998 election only. It plots averages by rank from the borderline elected (who has rank 0). The RD treatment effect is defined as the difference between the borderline elected and the borderline defeated that is beyond the direct effect of being ranked higher. In other words, it is graphically represented by the increase between -1 and 0, taking into account the overall slope starting further down the ranking. Estimating this difference yields a statistically significant point estimate (std. error) of 0.230 (0.014) for all elections. For the 1998 election, the point estimate (std. error) is 0.165 (0.024).[18] Thus,

[17] A special feature of the RD design in Berg (2018) is that it relies on within-party discontinuities rather than between, which is typically the case in RD designs applied to election systems.

[18] The number of observations is 2618 for all elections and 627 for the 1998 election.

the RD model yields very similar results as the DD model. The two models rely on different identifying assumptions and, in theory, identify different treatment effects (for the RD, a very local average treatment effect). The similarity of the results can therefore be taken as evidence that the DD results are robust, in the sense that they seem to represent a quite general effect.

Figure 3: Disposable income 1–3 years after the election



Note: The figure plots average disposable income by rank from borderline elected. Income is measured as averages over 1–3 years after the election, in logs of 100 SEK deflated to 2000 year values.

Source: Statistics Sweden & The Swedish Election Authority.

The placebo regressions and the alternative RD model both support the claim that being elected causes an increase in disposable income of around 20 percent. To learn about the mechanism behind this effect, the next section explores various income measures that disposable income is comprised of, such as labor income and pension income. To maintain the claim that the estimated effects indeed are causal, it is essential to rule out differences in age composition and other individual characteristics as confounding factors. For example, the treatment group is 1.3 years older than the control group; a modest yet statistically significant difference (see Table 1). Is the model above able to pick up this difference?

Table 4 shows results from regressions that sequentially add age and other individual characteristics. The top panel is for all elections 1994–2006, while the bottom panel is restricted to the 1998 election in focus. The results are very robust across the different columns. Table 9 in the Appendix shows equivalent results, but from non-weighted regressions (recall that the estimations above weigh observations based on pre-election income

Only the borderline elected, the borderline defeated and one more non-elected candidates are included in the estimations. The direct effect of rank is assumed to be linear. See Berg (2018) for details.

Table 4: Sensitivity of the results to controlling for individual characteristics

	Elections: 1994–2006			
	(1)	(2)	(3)	(4)
Elected	0.223***	0.226***	0.232***	0.232***
	(0.0258)	(0.0204)	(0.0215)	(0.0195)
Candidates	1636	1636	1636	1636
Observations	34965	34965	34944	34944
	Election: 1998			
	(1)	(2)	(3)	(4)
Elected	0.209***	0.206***	0.204***	0.203***
	(0.0418)	(0.0337)	(0.0373)	(0.0329)
Observations	8015	8015	8015	8015
Candidates	373	373	373	373
Age	No	Yes	No	Yes
Additional X	No	No	Yes	Yes

Note: All regressions are weighted so that the pre-election income level and age of the control group match those of the treatment group, and include election-by-year fixed effects and (election-by-)candidate fixed effects. "Age" indicates quadratic controls for age. "Additional X" are marital status, number of children and indicators for highest completed education. Standard errors clustered on candidate are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

and age). These non-weighted results are somewhat less robust to the inclusion of individual controls. Yet, when the controls are indeed included, the estimates are very close to their weighted counterpart. This is especially true when all elections are included. Overall, this exercise shows that bias from excluding individual controls can quite easily be removed—either with weights or additional variables. This suggest that the main regressions are not confounded by complex, non-observable characteristics. In the estimation of effects of more age-dependent income types below, this is important to keep in mind.

4.2 Mechanisms

Thanks to comprehensive data on different types and sources of income, it is possible to gain insights into possible mechanisms behind the rather large estimated returns to office. This section disentangles the effect both across income types and over time.

The aggregate effect of being elected estimated above does not reveal when the income increase kicks in. This is further explored here by disentangling the effect depending on reelection success. To this aim, the 1998 treatment group is divided into three; one that was elected in 1998 only ($n = 33$), one that was reelected once (in 2002, $n = 36$), and one that was reelected at least twice (in 2002, 2006 and possibly in 2010, $n = 54$).[19] The three graphs in panel a of Figure 4 plot the disposable income evolution for these three groups, as well as for the 1998 control group. A clear pattern emerges; just as in the aggregate figure above, there is a distinct jump for all three treated groups at the time of the election. But a subsequent distinct drop is now also revealed, and this drop coincides with the different times at which they leave office. Only for the treated group that was reelected at least twice and thus was still in office in 2010, does the positive income effect persist (the bottom figure).

Panel b and c of Figure 4 contain the equivalent analysis, but replace total disposable income and instead look specifically at labor income and pension income.[20] As can be seen, the positive income effect as estimated above is clearly driven by labor income. This income measure follows a very similar pattern to disposable income for all three tenure groups.[21] Furthermore, pension income displays the mirror image—upon exiting parliament when labor income decreases, pension income starts increasing relative to the control group.

Econometric estimates of year-to-year effects on disposable income as well as labor and pension income are provided in Tables 10–12 in the Appendix. These estimates confirm the graphical evidence. Regarding disposable income, the estimated effect is around 20–30 percent each year the treatment group spends in parliament. In contrast, in years after exiting parliament, their disposable income is generally not statistically different from the control group. Likewise, the estimates for labor income are all positive as long as the candidate is reelected, and then drop sharply (columns 2–3 in Appendix Table 11/the top panel in Figure 4). Regarding pension income, it increases for those elected when they exit parliament, but the statistical significance of this result is weaker.

In the year-by-year regressions, "pre-election effects" in years 1996 and 1997 are also estimated. These can be regarded as yet another test of the identifying assumption of parallel trends; counterfactual future trends are more likely to be parallel if past income trends run parallel. As shown in the Appendix tables, the pre-effects are all much smaller and, with one exception, not statistically significant. This is thus reassuring for the validity of the model.[22]

[19] 3 individuals in the 1998 sample were reelected in 2006 but not in 2002.

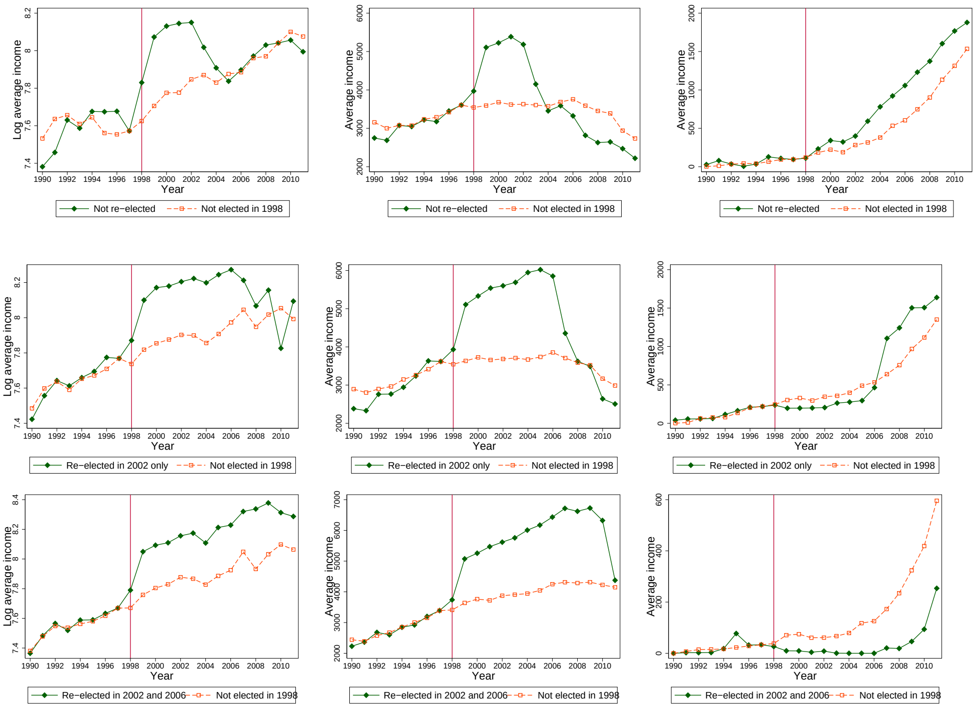
[20] Disposable income is net of taxes while labor income and pension income are gross measures.

[21] Note that labor income is defined in SEK rather than in logs, as there are several zeros.

[22] In these regressions, the weights are needed in order to get non-significant pre-effects on labor income. Equivalent year-by-year regressions for the 2002 and 2006 elections yield similar results as the 1998. Results from non-weighted regressions and for the 2002 and

Figure 4: Disposable income, labor income and pension income among treated and control candidates from the 1998 election

(a) Disposable income (b) Labor income (c) Pension income



Note: The figures plot average disposable income (in log 100 SEK), labor income (in 100 SEK) and pension income (in 100 SEK) among candidates in the treatment and control groups from the 1998 election, with the treatment group separated by reelection success. All income variables are deflated to 2000 year values. Observations are weighted so that the pre-election income level and age of the control group match those of the treatment subgroups.

Source: Statistics Sweden & The Swedish Election Authority.

Concluding the results so far, there is an overall effect on disposable income of around 20 percent of being elected. This large income increase is exclusively driven by the time spent in parliament. Once out of office, the level of disposable income returns to the counterfactual level as captured by the control group. Broadly, returns to political office can either stem from direct remuneration or increased outside earnings. The pattern seen here is highly suggestive of the former being the main mechanism.[23] What else can the data reveal regarding the "direct" mechanism through generous political remuneration? As a simple exercise, consider a comparison between the statutory parliamentarian wage (including reimbursements)[24] and the registered total labor income for those elected while they still are in parliament. This difference amounts to a negligible two percent. On this basis, significant amounts of extra income outside of parliament can be ruled out.

This conclusion is further strengthened by analyzing the effects of being elected on income on-the-side (that is, not from primary source) from private sources and on capital income, respectively.[25] Table 5 shows results from aggregate regressions on all election, while tables 13–14 in the Appendix present year-by-year estimates where the 1998 candidates are divided depending on reelection success. The aggregate effects in Table 5 are small and non-significant. Although the year-by-year estimates are somewhat unstable, broadly, two patterns emerges: First, there is a tendency for positive effects on income on-the-side after exiting parliament, at least for those reelected once (column 3 in Table 13). Note though, that these are quite small amounts stemming from "working extra"—as shown above, the effect on total labor income is negative after exiting. Second, there is a slight tendency for negative effects on capital income. As noted by Eggers and Hainmueller (2013), this is to be expected if effort as a parliamentarian is substituted for effort outside of the parliament (assuming that productivity/ability differences are fully accounted for).

Finally, the main mechanism being the direct remuneration effect is also consistent with the pattern of heterogeneous effects across different subgroups of candidates.[26] Figure 5 plots heterogeneous effects across four different dimensions. The figure clearly shows that the lower the previous income, the larger the effect. That is, the highest return to office accrue to those whose default option is likely relatively low, rather than to those who have larger possibilities of (ab)using their time in office for outside earnings

2006 elections are available upon request.

[23]Kotakorpi et al. (2017) reach a similar conclusion.

[24]As listed in the Annual Report of the Remuneration Committee (*Riksdagens arvodesnämnds versamhetsredogörelse till Riksdagen 2014*, 2014/15:RAR1).

[25]I thank Marianne Bertrand for suggesting this.

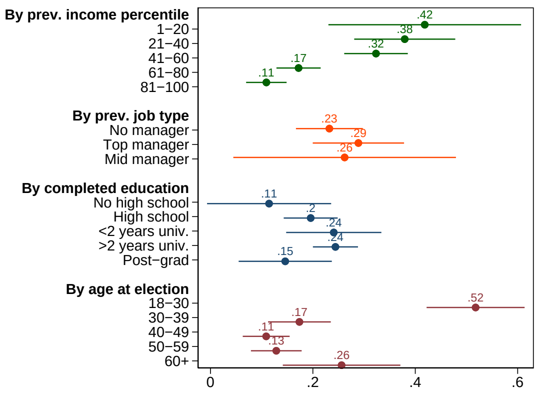
[26]One may also consider heterogeneous effects across different *parties* rather than *candidates*, for example governing vs. opposition parties. Such regressions yield no significant differences (results available upon request).

Table 5: Effects of being elected on on-the-side private labor income and capital income (in 100 SEK)

	Private labor income		Capital income	
	(1)	(2)	(3)	(4)
Elected	-3.230	-2.892	-1.620	0.401
	(18.45)	(18.47)	(27.92)	(28.27)
Additional X	No	Yes	No	Yes
Candidates	1636	1636	1636	1636
Observations	34986	34965	33457	33437

Note: All regressions are run on candidates from elections 1994–2006, are weighted so that the pre-election income level and age of the control group match those of the treatment group, and include election-by-year fixed effects, election-by-candidate fixed effects and controls for age and age^2. "Additional X" are marital status, number of children and indicators for highest completed education. The mean [standard deviation] is 161.7 [339.8] for private labor income and -55.0 [580.2] for capital income. Standard errors clustered on candidate are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Figure 5: Effects of being elected on disposable income for different subgroups



Note: The figure shows point estimates and their 95% confidence intervals of heterogeneous effects along four different dimensions, estimated with four separate regressions, respectively.

Source: Statistics Sweden & The Swedish Election Authority.

opportunities. Previous managerial position does not matter for the size of the returns.[27] Neither does the pattern of effects for candidates with different levels of education suggest that high-ability types are better at accruing outside earnings. Rather, the age profile of the effects again suggests that it is those groups with on average lower income—the young and the old—that benefit the most from being elected.

5 Conclusion

Returns to politics matter greatly for who we get as politicians and how they behave once in office. Despite the important role of these returns, for a long time we knew very little about what they actually are. This paper contributes by adding to the limited and somewhat mixed existing evidence of what the returns to politics are.

The paper applies a difference-in-difference strategy to rich data on candidates to the Swedish national parliament. The conclusion is that politics indeed generates positive monetary returns. The average politician's disposable income increases with around 20% as a result of being elected. This result is supported by various robustness checks as well as by an alternative identification strategy relying on within-party discontinuities.

Broadly, returns to political office can either stem from direct remuneration or increased outside earnings. Analyses possible thanks to comprehensive income data show the former to be the main mechanism. For example, there is no net income gain for MPs once they leave office (although there is a composition effect in the sense that they receive more pension income and work less). The relatively high parliamentary wage implies that those with the lowest default option benefit the most from being elected.

That there are no effects on labor or capital income on-the-side also suggests that the direct remuneration is the mechanism behind the estimated returns. An interpretation of this is that accountability mechanisms work; presumably it is possible to (ab)use the parliamentary seat for such opportunities, but elected politicians do not seem to act on this. Although not entirely new (see Eggers and Hainmueller, 2013), this is an interesting conclusion.

The significant monetary returns stemming from the remuneration in office can be put in perspective of the selection of politicians. In particular, Dal Bó et al. (2017) document that Swedish politicians are positively selected, yet that they are still representative of their voters. Their conclusion is that a combination of strong intrinsic motivation and high wages for full-time positions preserves the incentives of able individuals to enter politics. Notably, this conclusion is well in line with the combination of the absence

[27]Because information on type of job is only available from 2001, this regression is estimated only on elections 2002 and 2006.

of monetary returns for local Swedish politicians as found in Berg (2018), and the presence thereof as found in this paper.

As noted above, previous studies provide mixed evidence of what the returns to politics are. That political returns differ in different countries and different settings is no surprise. It is hard to say exactly what features make the political returns large or small. Whether or not differences in political institutions matter for the size of the returns per se is an intriguing question for future research.

The paper is written concurrently with a set of papers examining various aspects of becoming and being a politician. For example, politicians' children are better off (Folke et al., 2017), while the successful female politicians experience higher divorce rates (Folke and Rickne, 2020). Presumably, this body of literature does not end here; the extensive data at hand enables exploring many more interesting angles of these agents so important for the functioning of democracy.

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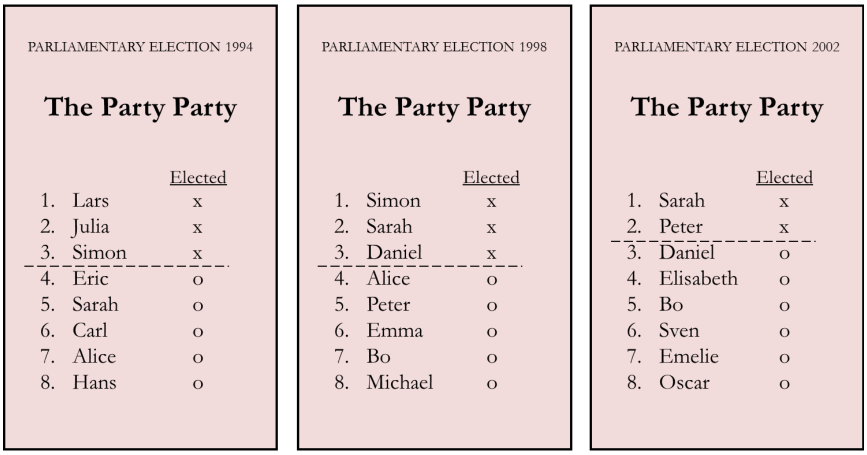
A Defining the treatment and control groups

As explained in Section 3.2, the treatment group consists of the 539 candidates elected for the first time in any one of the elections in 1994, 1998, 2002 and 2006. The control group instead consists of the 1157 candidates who were sufficiently highly ranked in one of these elections, but without being elected in the given election nor in any subsequent election.

To illustrate how this definition plays out, Figure 6 shows the hypothetical voting result for "the Party Party" in the 1994, 1998 and 2002 elections. In this example, only three elections have ever been held. Consider the 1998 election where, as displayed in the middle list, Simon, Sarah and Daniel were elected. However, as shown to the left, Simon were not elected for the first time in 1998. He will therefore not be part of the 1998 treatment group (but since in this example he has not been elected previously, he will be part of the 1994 treatment group). Sarah, on the other hand, was elected for the first time in 1998. She will be part of the 1998 treatment group, and similarly for Daniel. Thus, the 1998 treatment group consists of Sarah and Daniel.

Figure 6: Hypothetical election results for "the Party Party"



Note: Hypothetical party lists in three consecutive elections.

We not turning to the control group of 1998. We first consider as many candidates as there were elected candidates—that is, three. Consequently, Alice will take part of the 1998 control group (it does not matter that she ran in previous elections), as will Emma. In contrast, Peter is disqualified for the 1998 control group. This is because he is elected in the subsequent 2002 election, as shown to the right.

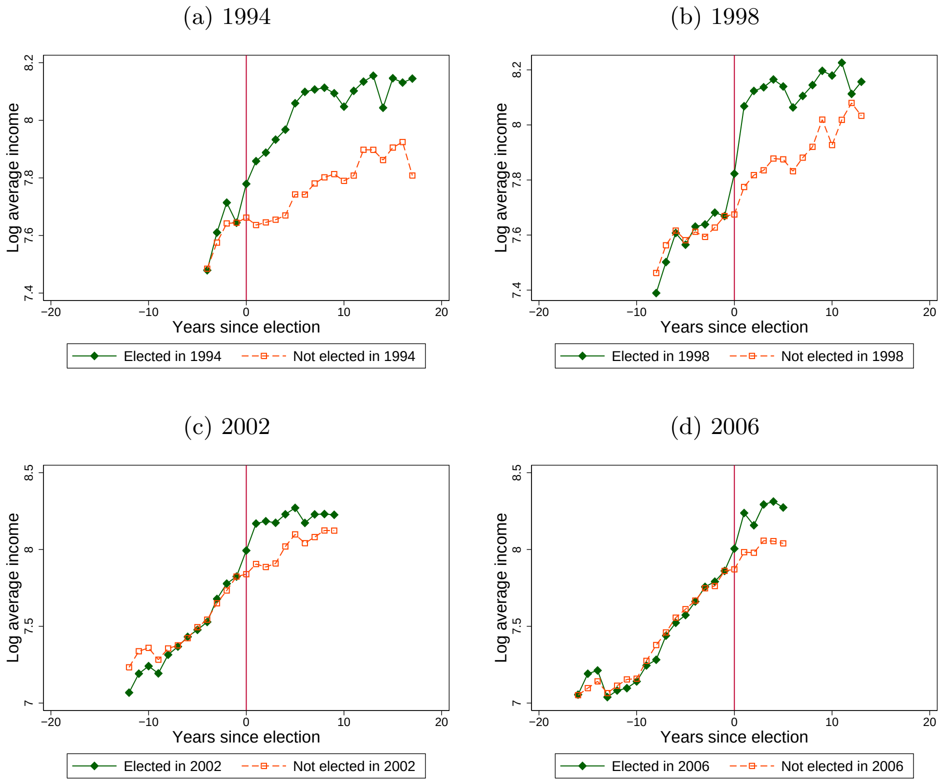
Applying the same line of reasoning also for the 1994 and the 2002 elections (again, assuming that there are no additional elections neither before nor after), the resulting hypothetical treatment and control groups are displayed in Table 6.

Table 6: Resulting hypothetical treatment and control groups

	Election		
	1994	1998	2002
Treatment group	Lars Julia	Sarah Daniel	Peter
Control group	Eric Carl	Alice Emma	Elisabeth

B Additional tables and figures

Figure 7: Disposable income among treated and control candidates, separately by elections



Note: The figures plot average disposable income among candidates in the treatment and control groups from the elections in 1994, 1998, 2002 and 2006. Income is measured in logs of 100 SEK deflated to 2000 year values. Observations are weighted so that the pre-election income level and age of the control group match those of the treatment group.

Source: Statistics Sweden & The Swedish Election Authority.

Table 7: Total effects of being elected on disposable income (in logs); non-weighted regressions

	Elections: 1994–2006				Election: 1998		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Elected	0.199^{***}	0.218^{***}	0.227^{***}	0.212^{***}	0.171^{***}	0.178^{***}	0.177^{***}
	(0.0195)	(0.0184)	(0.0240)	(0.0201)	(0.0322)	(0.0318)	(0.0442)
Sample	Full	Full	Restr.[1]	Restr.[2]	Full	Full	Restr.[1]
Additional X	No	Yes	Yes	Yes	No	Yes	Yes
Candidates	1640	1640	994	539	374	374	220
Observations	35025	35004	21324	11604	8034	8034	4741

Note: All regressions include election-by-year fixed effects, (election-by-)candidate fixed effects and controls for age and age^2. "Restr.[1]" indicates samples that exclude non-elected candidates who were not marginally close to being elected. "Restr.[2]" indicates samples that exclude all non-elected candidates. "Additional X" are marital status, number of children and indicators for highest completed education. Standard errors clustered on candidate are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 8: Total effects of being elected on disposable income (in logs); 2002 and 2006 elections

	Election: 2002			Election: 2006		
	(1)	(2)	(3)	(4)	(5)	(6)
Elected	0.188^{***}	0.214^{***}	0.163^{**}	0.242^{***}	0.235^{***}	0.242^{***}
	(0.0433)	(0.0406)	(0.0653)	(0.0446)	(0.0401)	(0.0539)
Sample	Full	Full	Restr.[1]	Full	Full	Restr.[1]
Additional X	No	Yes	Yes	No	Yes	Yes
Candidates	378	378	234	421	421	254
Observations	8131	8125	5068	8967	8952	5389

Note: All regressions are weighted so that the pre-election income level and age of the control group match those of the treatment group, and include year fixed effects, candidate fixed effects and controls for age and age^2. "Restr.[1]" indicates samples that exclude non-elected candidates who were not marginally close to being elected. "Additional X" are marital status, number of children and indicators for highest completed education. Standard errors clustered on candidate are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 9: Sensitivity of the results to controlling for individual characteristics; non-weighted regressions

	Elections: 1994–2006			
	(1)	(2)	(3)	(4)
Elected	0.163***	0.199***	0.211***	0.218***
	(0.0259)	(0.0195)	(0.0210)	(0.0184)
Candidates	1640	1640	1640	1640
Observations	35025	35025	35004	35004
	Election: 1998			
	(1)	(2)	(3)	(4)
Elected	0.129***	0.171***	0.167***	0.178***
	(0.0412)	(0.0322)	(0.0361)	(0.0318)
Observations	8034	8034	8034	8034
Candidates	374	374	374	374
Age	No	Yes	No	Yes
Additional X	No	No	Yes	Yes

Note: All regressions include election-by-year fixed effects and (election-by-)candidate fixed effects. "Age" indicates quadratic controls for age. "Additional X" are marital status, number of children and indicators for highest completed education. Standard errors clustered on candidate are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 10: Year-by-year effects of being elected in 1998 on disposable income (in logs)

		Re-elected		
	All elected in 1998	No	In 2002 only	In 2002 and 2006
1996	0.0128	0.0276	0.0503	-0.00347
	(0.0295)	(0.0790)	(0.0369)	(0.0365)
1997	-0.0415	-0.0953	-0.0147	-0.0192
	(0.0404)	(0.122)	(0.0372)	(0.0463)
1998	0.107***	0.110	0.119***	0.101*
	(0.0357)	(0.0739)	(0.0400)	(0.0534)
1999	0.252***	0.272***	0.267***	0.272***
	(0.0427)	(0.0855)	(0.0492)	(0.0630)
2000	0.265***	0.260***	0.301***	0.269***
	(0.0406)	(0.0745)	(0.0483)	(0.0586)
2001	0.260***	0.272***	0.289***	0.261***
	(0.0399)	(0.0720)	(0.0469)	(0.0596)
2002	0.246***	0.207***	0.287***	0.260***
	(0.0405)	(0.0762)	(0.0443)	(0.0607)
2003	0.229***	0.0511	0.325***	0.290***
	(0.0422)	(0.0746)	(0.0483)	(0.0619)
2004	0.195***	-0.0187	0.343***	0.261***
	(0.0558)	(0.0998)	(0.0513)	(0.0841)
2005	0.182***	-0.142	0.335***	0.302***
	(0.0518)	(0.106)	(0.0571)	(0.0642)
2006	0.182***	-0.0924	0.296***	0.280***
	(0.0505)	(0.105)	(0.0587)	(0.0666)
2007	0.136***	-0.0945	0.165**	0.251***
	(0.0514)	(0.0914)	(0.0695)	(0.0702)
2008	0.211***	-0.0456	0.116	0.384***
	(0.0696)	(0.142)	(0.0732)	(0.0884)
2009	0.166***	-0.105	0.137*	0.325***
	(0.0596)	(0.133)	(0.0772)	(0.0730)
2010	0.00209	-0.111	-0.249*	0.199***
	(0.0668)	(0.130)	(0.144)	(0.0714)
2011	0.0962	-0.154	0.100	0.206***
	(0.0646)	(0.131)	(0.0750)	(0.0760)
Additional X	No	No	No	No
Candidates	373	280	283	301
Observations	6536	4881	4933	5274

Note: All regressions are weighted so that the pre-election income level and age of the control group match those of the treatment group, and include year fixed effects, individual fixed effects and controls for age and age^2. Standard errors clustered on individual are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 11: Year-by-year effects of being elected in 1998 on labor income (in 100 SEK)

		Re-elected		
	All elected in 1998	No	In 2002 only	In 2002 and 2006
1996	134.8	73.18	327.0**	82.92
	(83.41)	(105.4)	(151.2)	(113.8)
1997	45.32	42.88	109.0	36.28
	(96.43)	(134.0)	(142.2)	(139.6)
1998	382.9***	469.5**	495.8***	366.4*
	(140.6)	(196.3)	(168.2)	(193.7)
1999	1507.1***	1556.8***	1583.1***	1469.9***
	(184.8)	(321.8)	(264.1)	(235.1)
2000	1579.5***	1598.8***	1721.4***	1534.3***
	(169.1)	(333.4)	(233.3)	(212.3)
2001	1802.6***	1866.4***	2002.5***	1796.2***
	(185.0)	(340.3)	(254.1)	(236.2)
2002	1757.7***	1654.0***	2037.3***	1791.5***
	(179.2)	(323.5)	(250.6)	(229.3)
2003	1577.1***	647.7*	2207.5***	1906.3***
	(202.7)	(374.1)	(269.5)	(229.5)
2004	1543.8***	-17.23	2507.8***	2120.5***
	(226.9)	(407.2)	(270.9)	(236.1)
2005	1569.3***	-2.067	2509.4***	2177.7***
	(236.4)	(443.1)	(297.2)	(236.5)
2006	1443.7***	-338.0	2215.1***	2238.2***
	(238.6)	(436.6)	(332.3)	(235.2)
2007	1208.0***	-656.7	867.5**	2470.8***
	(277.4)	(482.0)	(438.4)	(276.8)
2008	990.4***	-708.6	254.1	2404.7***
	(283.0)	(499.0)	(437.9)	(281.9)
2009	1052.6***	-608.6	207.1	2482.8***
	(301.4)	(548.3)	(456.3)	(312.3)
2010	795.4**	-346.8	-280.2	2169.9***
	(309.7)	(519.8)	(463.5)	(361.6)
2011	-39.69	-390.5	-225.7	316.6
	(308.5)	(487.3)	(447.2)	(473.4)
Additional X	No	No	No	No
Candidates	373	280	283	301
Observations	6541	4883	4937	5275

Note: All regressions are weighted so that the pre-election income level and age of the control group match those of the treatment group, and include year fixed effects, individual fixed effects and controls for age and age^2. Standard errors clustered on individual are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 12: Year-by-year effects of being elected in 1998 on pension income (in 100 SEK)

		Re-elected		
	All elected in 1998	No	In 2002 only	In 2002 and 2006
1996	-21.74	-9.273	-24.86	-24.03
	(35.52)	(70.60)	(74.86)	(26.62)
1997	-30.45	-24.52	-30.59	-27.95
	(41.04)	(92.45)	(81.52)	(28.12)
1998	-40.59	-33.08	-39.83	-39.67
	(44.08)	(97.44)	(88.39)	(28.26)
1999	-75.54	22.79	-137.0	-89.11**
	(65.44)	(136.8)	(150.5)	(36.61)
2000	-65.37	96.24	-161.6	-93.20**
	(73.74)	(175.6)	(157.6)	(37.50)
2001	-65.51	84.61	-154.2	-89.37**
	(73.42)	(170.1)	(163.1)	(35.14)
2002	-78.18	67.15	-199.4	-85.55**
	(81.18)	(193.7)	(171.0)	(40.85)
2003	-20.53	228.2	-165.5	-99.21**
	(86.88)	(203.1)	(180.4)	(45.88)
2004	8.246	351.6	-193.5	-111.3**
	(93.82)	(223.2)	(185.4)	(47.50)
2005	-19.56	338.9	-269.8	-154.3***
	(97.90)	(225.5)	(184.5)	(51.52)
2006	32.01	405.3	-144.0	-162.4***
	(105.3)	(249.6)	(192.9)	(51.77)
2007	142.4	429.2	396.9	-187.7***
	(125.0)	(275.4)	(250.8)	(60.33)
2008	114.2	420.2	416.4*	-251.0***
	(127.6)	(267.1)	(247.8)	(65.93)
2009	79.64	396.9	458.5*	-314.1***
	(139.0)	(287.5)	(259.2)	(68.86)
2010	7.222	403.3	317.9	-365.7***
	(139.8)	(290.6)	(255.8)	(77.04)
2011	-77.36	292.1	212.2	-381.1***
	(135.9)	(270.8)	(221.0)	(118.4)
Additional X	No	No	No	No
Candidates	373	280	283	301
Observations	6541	4883	4937	5275

Note: All regressions are weighted so that the pre-election income level and age of the control group match those of the treatment group, and include year fixed effects, individual fixed effects and controls for age and age^2. Standard errors clustered on individual are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 13: Year-by-year effects of being elected in 1998 on on-the-side private labor income (in 100 SEK)

		Re-elected		
	All elected in 1998	No	In 2002 only	In 2002 and 2006
1996	47.06	22.48	58.34	44.07
	(32.22)	(48.83)	(65.57)	(41.83)
1997	-11.86	-76.96	16.06	-13.10
	(31.14)	(65.03)	(50.75)	(41.42)
1998	0.377	-51.78	29.16	-6.759
	(32.97)	(43.31)	(48.74)	(50.61)
1999	-70.77**	-105.4	-9.430	-110.8***
	(33.90)	(67.22)	(47.53)	(40.54)
2000	-50.87	-109.4	14.83	-75.70
	(46.37)	(88.21)	(77.34)	(58.37)
2001	-4.436	-58.84	59.09	-20.92
	(46.23)	(87.07)	(81.55)	(64.66)
2002	64.11	57.54	88.34	45.12
	(55.06)	(120.3)	(90.57)	(75.63)
2003	51.51	20.79	129.5	0.0399
	(53.31)	(110.2)	(97.18)	(67.94)
2004	61.61	95.29	119.5	-6.413
	(52.83)	(127.3)	(91.73)	(56.69)
2005	114.8*	60.94	221.6*	89.76
	(64.87)	(136.0)	(124.5)	(82.14)
2006	87.35	11.47	201.5	51.49
	(66.33)	(135.2)	(123.1)	(83.08)
2007	123.2**	36.09	264.2**	76.65
	(59.06)	(83.33)	(115.6)	(90.45)
2008	61.19	-34.47	231.1**	11.05
	(53.57)	(81.39)	(116.5)	(66.69)
2009	88.49	16.32	220.5*	47.64
	(59.54)	(90.64)	(122.0)	(82.26)
2010	58.64	2.294	125.0	39.08
	(51.92)	(101.7)	(93.67)	(69.80)
2011	31.67	-2.411	48.80	26.34
	(57.43)	(112.6)	(87.17)	(86.93)
Additional X	No	No	No	No
Candidates	373	280	283	301
Observations	6541	4883	4937	5275

Note: All regressions are weighted so that the pre-election income level and age of the control group match those of the treatment group, and include year fixed effects, individual fixed effects and controls for age and age^2. Standard errors clustered on individual are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 14: Year-by-year effects of being elected in 1998 on capital income (in 100 SEK)

		Re-elected		
	All elected in 1998	No	In 2002 only	In 2002 and 2006
1996	55.05	93.12	137.5	-37.12
	(46.64)	(118.8)	(94.34)	(38.21)
1997	-1.263	-31.51	31.88	23.21
	(43.25)	(104.5)	(50.94)	(60.29)
1998	23.60	-104.1	46.47	106.4
	(55.67)	(96.17)	(83.27)	(82.34)
1999	-112.3	-327.2***	-66.89	-4.104
	(70.49)	(105.1)	(82.25)	(109.4)
2000	-33.26	-185.8*	21.12	26.66
	(60.67)	(105.4)	(98.59)	(74.82)
2001	-38.38	-146.6	65.40	-23.46
	(57.42)	(110.8)	(110.5)	(65.68)
2002	-7.984	-20.09	75.87	-24.74
	(58.61)	(138.5)	(98.18)	(65.87)
2003	-14.61	-142.9	138.7	-33.64
	(58.11)	(105.2)	(109.1)	(68.49)
2004	1.908	8.528	123.2	-71.73
	(63.76)	(119.9)	(132.2)	(69.53)
2005	-35.68	-64.17	165.9	-125.0
	(89.13)	(168.9)	(196.6)	(76.67)
2006	-43.25	-58.30	56.92	-107.9*
	(102.3)	(219.9)	(228.6)	(60.44)
2007	-258.8*	-332.0	-12.88	-328.0***
	(147.8)	(326.0)	(229.1)	(117.9)
2008	96.89	312.6	30.41	19.00
	(116.2)	(319.9)	(122.3)	(141.9)
2009	-22.04	-266.0**	399.5	-129.1
	(105.2)	(116.8)	(275.4)	(90.63)
2010	-122.9	50.05	-32.82	-205.8*
	(101.3)	(189.9)	(145.9)	(121.7)
2011	-91.45	143.2	-13.00	-202.5*
	(99.58)	(247.4)	(97.99)	(105.0)
Additional X	No	No	No	No
Candidates	373	280	283	301
Observations	6541	4883	4937	5275

Note: All regressions are weighted so that the pre-election income level and age of the control group match those of the treatment group, and include year fixed effects, individual fixed effects and controls for age and age^2. Standard errors clustered on individual are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.