

Berg, Hélène

Working Paper

Politicians' Payments in a Proportional Party System

CESifo Working Paper, No. 7278

Provided in Cooperation with:

Ifo Institute – Leibniz Institute for Economic Research at the University of Munich

Suggested Citation: Berg, Hélène (2018) : Politicians' Payments in a Proportional Party System, CESifo Working Paper, No. 7278, Center for Economic Studies and ifo Institute (CESifo), Munich

This Version is available at:

<https://hdl.handle.net/10419/185476>

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Helène Berg

Impressum:

CESifo Working Papers

ISSN 2364-1428 (electronic version)

Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH

The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute

Poschingerstr. 5, 81679 Munich, Germany

Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de

Editors: Clemens Fuest, Oliver Falck, Jasmin Gröschl

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Politicians' Payments in a Proportional Party System

Abstract

Is politics a lucrative business? The question is approached in this paper, as one of few to quantify the monetary returns to holding political office in a typical developed democracy where parties are the main political actors. By applying 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 be a lucrative business. In the long-run however, the effect is instead compositional in the sense that ex-politicians receive more pension income and work less.

JEL-Codes: C230, D720, J440.

Keywords: returns to politics, difference-in-difference.

Heléne Berg
(previously Lundqvist)
Department of Economics
Stockholm University
Sweden – 10691 Stockholm
helene.berg@ne.su.se

September 24, 2018

I am thankful for comments from Matz Dahlberg, Olle Folke, Eva Mörk and Johanna Rickne. I also thank participants the UCLS conference held in Krusenberg in August 2014, the 4th National Conference of Swedish Economist held in Umea in September 2014 and the UCFS symposium held in Uppsala in October 2014 as well as seminar participants at the University of Lund and SITE/SSE for helpful discussions, suggestions and comments. Financial support from Handelsbanken's Research Foundation is gratefully acknowledged.

1 Introduction

There are plenty of rich politicians, and in particular rich *ex*-politicians who have become wealthy after exiting politics. One of the more extraordinary examples is former US president Bill Clinton who, in the period after he left the oval office, earned an average of \$189,000 per speech—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 Another example of a career profitability that seems to have been boosted by political activities is that of Harvard University president emeritus Larry Summers; in between his service in the Clinton and Obama administrations, aside from his Harvard presidency, he spent several years as a very well-paid financial consultant.²

This suggests that politics is a lucrative business. In other words, the monetary returns to political office seem to be high. But do these examples represent typical returns to office?

In an attempt to answer that, two aspects deserve emphasis. First, it is possible that these extraordinary examples are just that—that is, *extraordinary*—and therefore tend to be the only examples we come across. But more importantly, for the examples above to really be the result of their past political life—that is, that those incomes actually represent returns to political office—we need to distinguish those successes from where they would have been had they not become politicians.

To figure out what the counterfactual to being elected into political office is—and thereby to identify and estimate the causal returns to political office—is the aim of this paper.

Of course, we can never *know* what would have been. But we can use careful empirical strategies to convincingly get “close enough” to the true counterfactual. The strategy taken here 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, and therefore that the pre-post-election-difference in their income 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 argue, the causal effects of

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

²<http://chronicle.com/article/Larry-Summersthe/124790/>

being elected into politics—that is, the returns to political office—can be estimated.

Returns to office play a crucial role for who the politicians are and how they behave. For one thing, the returns are what motivates 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).³ Second, the returns determine what *type* of people that decides to engage in politics—that is, whether voters get the “good” (as in benevolent) or the “bad” (as in extractive) ones representing them. Theoretical models studying how returns to office affect the selection of politicians come to different conclusions, where some highlight the adverse effect stemming from the idea that extrinsic rewards may crowd out intrinsic motivation (Caselli and Morelli, 2004; Messner and Polborn, 2004; Mattozzi and Merlo, 2008; Poutvaara and Takalo, 2007). The available empirical evidence suggests a (net) positive selection effect of higher returns (Ferraz and Finan, 2009; Gagliarducci and Nannicini, 2013; Kotakorpi and Poutvaara, 2010), although Fisman et al. (2015) show opposite results.⁴ Third, once in office, higher returns can work as an incentive for politicians to do a good job—that is, the higher the returns to office, the higher the returns also 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).

Given the relevance of the returns to political office in these selection and incentive aspects, it is important to figure out what, exactly, the returns are. But for a long time, we knew very little about this. Recently however, there has been an increasing interest in these issues, and we now have at least limited knowledge about what they are. For example, in Great Britain, conservative candidates who ran successfully to the Parliament gained £250,000 compared to those who ran but were not elected. This is according to Eggers and Hainmueller (2009), who collected estates of deceased members of the British parliament. They conclude that successful politicians died almost twice as wealthy. For 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, with the conclusion 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, a period when government spending

³Although for some, the possibility of implementing some desired policy can be the main driver (Besley and Coate, 1997; Osborne and Slivinski, 1996).

⁴See also Keane and Merlo (2010) for how various policy changes affect the selection of politicians.

spiked and when a lot of media attention was drawn to the war and little to extractive politicians. 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—at least in the short run, and at least among the most prominent politicians.

Thus, evidence from a limited set of countries—either India, a developing country, or the US or UK, countries with much focus on individual politicians—shows that the returns to political office are large or even huge. In contrast, there is limited, suggestive evidence that returns in typical, developed parliamentary democracies—where parties rather than individual politicians often are the main political actors—are much smaller; Lundqvist (2011) 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 the estimated effects fade out quickly over time. Hence, given the possible selection and incentive effects of the returns to politics as outlined above, differences in political institutions—through differences in returns—could have consequences for who the politicians are and how they behave.⁵

But before thinking more deeply about such far-reaching consequences, note that with the available evidence, it is not clear that the returns to office really are smaller in typical, developed parliamentary democracies. Zero returns to local politics say little about the returns to national politics (Lundqvist, 2011). And interpreting the insignificant long run estimates in Kotakorpi et al. (2017) as lack of long-lasting income effects is probably a bit rash. The reason is that, with the regression discontinuity design they use for identification, the estimated effects 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 really close to being elected in a given election—often run again and indeed are elected in the subsequent election (and/or vice versa, because candidates in the treatment group who were just barely elected fail to be reelected in subsequent elections).

In light of this, the contribution of this paper is to provide new and credible evidence of what the returns to political office can be in a developed democracy with quite different political institutions than in the US and UK. Specifically, (i) I study returns to national rather than local politics; (ii) in a setting where it truly makes sense to consider long run effects since, unlike an RD strategy, the DD strategy employed defines treatment and controls groups that are consistent over time; and (iii) by applying this 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.

⁵See also Peichl et al. (2013), who estimate the so-called politicians’ wage gap by comparing politicians to a individuals in executive positions.

The last point deserves special emphasis; by distinguishing between income from different sources (labor income, pension, capital income, income on-the-side from private firms etc.), it is possible to add to the existing literature interesting evidence on 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 outside income on-the-side. 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 that outside income plays little role partly contrasts the study by Gagliarducci et al. (2010) on “moonlighting politicians” and is, at least indirectly, 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 politicians’ 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. Besides the above mentioned study on *local* monetary returns to politics (Lundqvist, 2011), Folke et al. (2017) show that the future income and level of education among children of closely elected local mayors are positively affected. In a similar close-election framework, Folke and Rickne (2018) find substantially increased divorce rates among women, but not among men, after being elected to a top political position. As to the nomination and selection of political candidates, it is characterized 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).

Proceeding in Section 2 with a description of how Swedish members of the parliament are elected and of the remuneration that they get, Section 3 then 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.⁶ MPs are elected from 29 electoral districts in separate, proportional elections. Parties play a crucial role in the elections as candidates can only run by running for a party, and voters choose a party as opposed to 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, and 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.⁷ Each party typically has a single list per district from which the mean (median) number of elected candidates is a low 2.2 (1), out of a mean (median) of 30 (25) listed candidates.

Since 1994—the earliest post-election year in the analysis—wages of the 349 elected MPs are set annually by a remuneration committee consisting of three people appointed by the Board of the Parliament (*Riksdagsstyrelsen*). The monthly wage has since then more than doubled from 26,500 SEK to 57,000 SEK (approximately from \$3,300 to \$7,100) in 2011—the latest year in the analysis.⁸ Even adjusting for inflation, this increase implies that, in terms of direct remuneration, it has become more lucrative 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 superior 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, and 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 65.⁹

⁶Reelection rates are quite high; around 60 and 30 percent over one and two elections, respectively.

⁷Starting with the 1998 election, voters can mark one preferred candidate on the ballot paper (so-called preference voting), in which case this candidate in effect is ranked first on that particular voter’s ballot paper.

⁸The Annual Report of the Remuneration Committee (*Riksdagens arvodesnämnds verksamhetsredogörelse till Riksdagen 2014*, 2014/15:RAR1).

⁹The compensation scheme has been criticized and is now changed, so that those elected into the parliament for the first time in the 2014 election are eligible for maximum two years after they exit. In addition, rather than calling it guaranteed income, it has been

These types of income sources—along with plenty of other useful information—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

Although it is intrinsically impossible to know with certainty what a counterfactual state is, with a combination of a suitable research design and really good data one can (hopefully) come close enough.

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 (since only one year of post-election data is covered, the 2010 election will however not be included).¹⁰ There are several important features of the data: First, there is very detailed information about the elections. In particular, each candidate’s ranking on the party list is included, which 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.¹¹ 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, as well as on individual characteristics such as age, sex, foreign background, educational attainment and occupation are matched using a unique person identifier. The registers are in annual form and cover the years 1990–2011 for all candidates, which 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 who, according to the description below, 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, the data is used to define a treatment and a control group. These will then be applied in a difference-in-difference (DD) frame-

relabel “transitional aid” (*omställningsstöd*), as the former was thought to send out the wrong signals about its purpose.

¹⁰Data comes partly from Statistics Sweden, partly from the Swedish Election Authority, and has been put together by the former.

¹¹Information on the list placement is missing for the 1994 election, and I discuss below how I deal with this.

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 (1141.3)	2083.4 (1007.0)	8.97
Labor income	3608.7 (1809.7)	2642.6 (1617.5)	10.91
Pension income	102.0 (511.1)	87.4 (436.3)	0.60
Age	44.7 (10.1)	43.4 (11.3)	2.36
Married	0.60 (0.49)	0.60 (0.49)	-0.04
Children under 18	0.66 (0.99)	0.83 (1.13)	-3.06
Less than high school	0.069 (0.25)	0.068 (0.25)	0.05
High school graduate	0.26 (0.44)	0.29 (0.45)	-1.02
< 2 years university	0.12 (0.32)	0.14 (0.35)	-1.48
\geq 2 years university	0.51 (0.50)	0.49 (0.50)	0.93
Graduate studies	0.041 (0.20)	0.015 (0.12)	3.18
Female	0.47 (0.50)	0.49 (0.50)	0.76
Born in Sweden	0.94 (0.24)	0.92 (0.28)	1.80
Born in other Nordic country	0.015 (0.12)	0.017 (0.13)	0.36
Born in non-Nordic Europe	0.022 (0.15)	0.026 (0.16)	-0.50
Born in North America	0.0019 (0.043)	0.0018 (0.043)	0.02
Born elsewhere	0.020 (0.14)	0.039 (0.19)	-1.99
Both parents foreign-born	0.0093 (0.096)	0.012 (0.11)	-0.46
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 (8 SEK \approx 1 USD). 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.

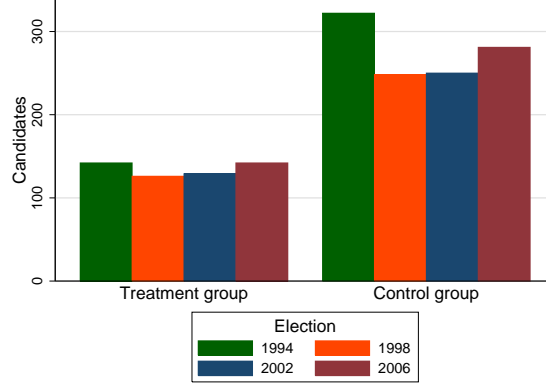
work that estimates the treatment effect of interest—the effect of being elected into the parliament on future income. The treatment group consists of the 539 candidates elected for the first time in any one of the elections in 1994, 1998, 2002 and 2006. The control group instead consists of candidates 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.¹²

A contribution of the analysis is to distinguish between different lengths of the political office, which is 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, and they are therefore not considered for the treatment or the control group. Along the same line of reasoning, because previous experience is limited to only one earlier election for candidates in the 1994 election, the treatment and control groups may in part be misclassified. In other words, those elected in 1994 but not elected in 1991 are defined as being elected for the first time in 1994, even though there is no information on whether or not they were elected in 1988 or earlier. 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 falsely classifying 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. Whereas the former is more or less uniformly distributed, there are slightly more control candidates from the 1994 and the 2006 elections than there are from the two elections in between. This is because only those who failed to be elected in a given election *nor in any of the later elections studied* are part of the control group, which has implications for the first and the last elections: As noted above, the list rankings are missing for the 1994 election. 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. And as for the 2006 candidates, there simply are no later elections that can disqualify them for the control group.

¹²Please refer to Appendix A for further details on how the treatment and control groups are defined.

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



Source: Statistics Sweden & The Swedish Election Authority.

3.3 Identification and estimation

The following equation states how the treatment and control groups, as just defined, are used to identify a DD estimate of the effect τ of being elected into the parliament on income Y in year t for candidate i running in election year j :

$$Y_{ijt} = \tau \text{elected}_{ijt} + \text{election}_j \times \text{cand}_i + \text{election}_j \times \text{year}_t + \beta \text{age}_{ijt} + \Gamma' \mathbf{X}_{ijt-1} + \varepsilon_{ijt} \quad (1)$$

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 candidate and time fixed effects, $\text{election}_j \times \text{cand}_i$ and $\text{election}_j \times \text{year}_t$ respectively, which are allowed to vary depending on elections.¹³ 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, thus failing the identifying assumption of parallel counterfactual trends.

Furthermore, the vector \mathbf{X} in equation (1) contains the candidates' marital status, number of children and indicators for highest completed

¹³The vast majority of candidates are only part of the treatment or the control group in one of the elections and consequently, for them, the candidate fixed effects do not vary. By definition, this is true for everyone in the treatment group (as it is only possible to be elected for the first time once). In contrast, 135 of the 1101 control candidates are individuals who qualify for the control group in several elections (118 for two elections; 16 for three elections; and 1 for all four elections).

education—that is, things that potentially could vary over time (they are measured one year before the outcome variable) and would therefore not be captured by the candidate fixed effects. 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, and 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 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 with regards to 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 this is crucial for a causal interpretation of the results, the likelihood of this assumption will be investigated in several ways. First, the assumption of parallel *counterfactual future* trends is more likely to hold if *pre* income trends run parallel, which can and will be tested directly. Second, 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. Finally, 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, where the control group is dropped altogether so that those elected for the first time later on serve as controls for the treatment group in a given election.

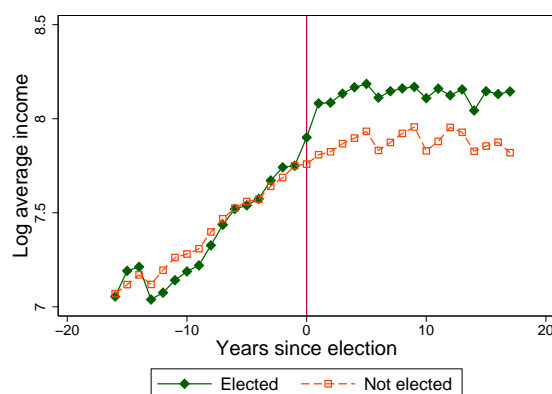
4 Results: The returns to politics

This section presents the results of the analysis of the effect of being elected into parliament on future income. There is an emphasis on graphical presentations, where the corresponding estimation results are mostly referred to the Appendix.

The main result of the paper is given in Figure 2. It shows, separately

for candidates in the treatment and control groups as defined in the previous section, log annual disposable income, with the x-axis centered at the year of the election (1994, 1998, 2002 or 2006). To ease visual interpretation, 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.¹⁴ Aside from adjusting the level of the series in graphs, the reweighting has in practice very little impact on the results (which can be seen by comparing the non-weighted econometric estimates presented throughout the paper with their weighted counterparts in 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 which 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 0.199.¹⁵ That is, the effect of being elected is a 20 percent increase in disposable income. This

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

¹⁵That the weighting in Figure 2 does not matter for identification can be seen by comparing the non-weighted estimates in Table 2 with the weighted counterparts in Table 6 in the Appendix.

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 those who were just on the margin to being elected;¹⁶ column 4 drops the control group altogether and instead relies for identification on the sequential treatment as given by the four different elections. As can be seen, the estimated effect is very robust—even to the rather restrictive specification that drops the control group altogether and thereby only includes 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	0.199*** (0.0195)	0.218*** (0.0184)	0.227*** (0.0240)	0.212*** (0.0201)	0.171*** (0.0322)	0.178*** (0.0318)	0.177*** (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 age². “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.

The estimated income effect of being elected of around 20 percent is equivalent to an annual increase in disposable income of around 66,500 SEK, or \$8,300. This is, in several ways, a very general result. 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). And third, the estimated effect is the average effect for candidates from all four elections in 1994, 1998, 2002 and 2006. The analysis below attends to the first two aspects in more detail (type of income and timing of the effect), while Figure 6 in the Appendix provides graphical results separately by election. Although somewhat less stable due to the smaller sample, the positive income effect of being elected is clearly visible in all four graphs,

¹⁶Note that, despite the close margin-terminology, this is still a difference-in-difference rather than a regression discontinuity design, as the source of identifying variation is between groups, over time (and thus not from discontinuous changes between groups at a given point in time).

with an estimated effect ranging between 15 percent for the 1994 election and around 25 percent for 2002 and 2006.

The three rightmost columns of Table 2 present baseline estimates along with robustness checks for the 1998 election only. Since an analysis of how effects differ depending on length of the political career is facilitated by looking at one particular election, and since the 1998 one is appropriate thanks to sufficiently long pre and post periods, this particular election will be the focus in the remainder of the result section. As is seen in the table, the income effect is somewhat smaller for those elected for the first time in 1998 compared to the average, but there is still a substantial effect of around 17 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).

Next, in order to learn about the mechanisms behind the rather large estimated returns to office, the effect is disentangled both across income types and over time. 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, $n = 54$).¹⁷ The three graphs in panel a of Figure 3 plot the disposable income evolution for these groups, respectively, 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 in the bottom figure, which was reelected at least twice and thus were still in office in 2010, does the positive income effect persist.

Panel b and c of Figure 3 contain the equivalent analysis, but replace total disposable income and instead look specifically at labor income and pension income. As can be seen, the positive income effect as estimated above is clearly driven by labor income, which follows a very similar pattern to disposable income for all three tenure groups.¹⁸ 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 of disposable income as well as labor and pension income are provided in Tables 7–9 in the Appendix,¹⁹ and confirm the graphical evidence. Regarding disposable income, the estimated effect is around 20 percent each year the treatment group spends in parliament, occasionally as high as 30 percent, whereas in years after ex-

¹⁷3 individuals in the 1998 sample were reelected in 2006 but not in 2002.

¹⁸Note that labor income is defined in SEK rather than in logs, as there are several zeros.

¹⁹Additionally, Appendix Tables 10–12 give results from regressions where the treatment group is weighted so as to match the age and the pre-election income level of the control group, as is done in the graphical analysis.

iting parliament, their disposable income is 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, even to a significantly lower level for the group only elected once (column 2 in Appendix Table 8/the top panel in Figure 3). Regarding pension income, it clearly increases for those elected when they exit parliament, but the statistical significance of this result is weaker.

For each outcome with year-by-year regressions, two “placebo effects” prior to the election in 1998 are estimated. These can be regarded as tests 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 placebo effects are much smaller than the treatment effects, which is reassuring. It is however a bit worrisome that they are statistically significant for labor income. This is true for two of the three tenure groups of elected candidates in the unweighted regressions (see columns 2–3 in Table 8), whereas the weighted regressions that deal with the fact that the pre-treatment level of income as well as the age differ between the treatment and control group do not have this problem (see Table 11). But again, irrespectively of weights, the magnitude is much smaller than the estimated effects while still in office, suggesting that there is indeed a quite large, positive effect on labor income of being elected.

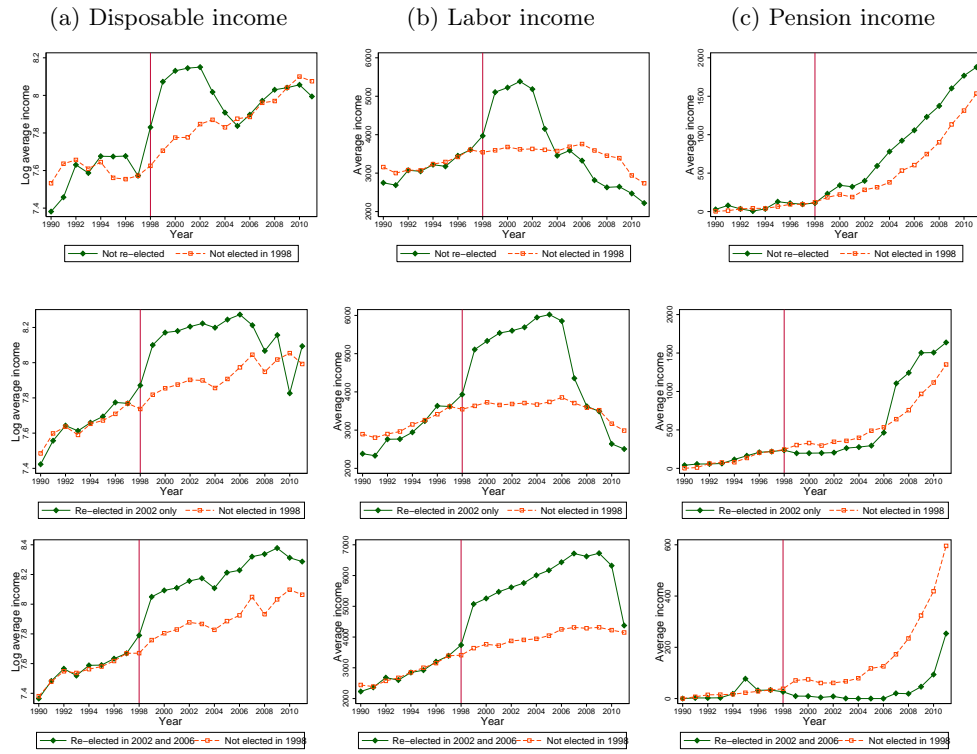
4.1 Remarks on the returns

Concluding the main result section above, 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, and the pattern seen here is highly suggestive of the former being the main mechanism.²⁰ With comprehensive data on different sources of income, the likelihood of this mechanism is investigated further below.

First however, note that although there is no longer any significant difference in the level of disposable income when those elected do not get reelected, there are interesting long-run compositional effects. In particular, there is a clear pattern of increasing pension income, at the expense of sharply dropping labor income. Aside from a word of caution due to significant placebo estimates (for labor income) and weaker statistical significance (for pension income), for this pattern to be given a causal interpretation, it is essential to rule out differences in age composition as a confounding factor. On average, the treatment group is 1.3 years older than the control group; a modest

²⁰Kotakorpi et al. (2017) reach a similar conclusion.

Figure 3: Disposable income, labor income and pension income among treated and control candidates from the 1998 election



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 duration in parliament. 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.

yet statistically significant difference (see Table 1). The question is whether controlling quadratically for age—as in the analysis above—is sufficient to pick up this difference?

Table 3 shows regressions that vary the inclusion of age as controls. The top panel does so for all elections 1994–2006, while the bottom panel is restricted to the 1998 election in focus. Columns 1–3 are estimated using the baseline sample, while columns 4–6 restrict the control group to those who were just on the margin to being elected. For each sample, the first respective columns (1 and 4) reproduce the baseline estimates in Table 2. The regressions in following two columns then remove the quadratic control for age, and then add controls for individual characteristics, respectively. From the results it seems as if controlling for age does matter to some extent; when the age control is removed, the effect is reduced but is still statistically significant. On the other hand, as seen from columns 3 and 6 which are very similar to the baseline estimates, the other controls (marital status, number children and level of education) capture the same variation as age. Thus, excluding controls for age implies some bias, but this bias can quite easily be removed with observables. This suggest that the regressions are not confounded by complex, non-observable characteristics, and thus that the patterns estimated above—where age ineed is controlled for quadratically—can be given a causal interpretation.

As noted above, the returns to office are higher for those elected in later years, which coincides with increases in the parliamentary wage.²¹ What else can the data reveal regarding the “direct” mechanism through generous political remuneration? As a first exercise, consider a comparison between the statutory parliamentarian wage (including reimbursements)²² 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, which are small and not statistically significant; see Table 4.²³

The main mechanism being the direct remuneration effect is also consistent with the pattern of heterogeneous effects across different subgroups of candidates. Figure 4 plots heterogeneous effects across four different dimensions, and 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, as opposed to those who have larger possibilities of (ab)using their time in office for outside earnings opportunities.

²¹As shown in Kotakorpi et al. (2017), this is also the case for Finnish MPs.

²²As listed in the Annual Report of the Remuneration Committee (*Riksdagens arvodesnämnds verksamhetsredogörelse till Riksdagen 2014*, 2014/15:RAR1).

²³I thank Marianne Bertrand for suggesting this.

Table 3: Excluding controls for age

Elections: 1994–2006						
	(1)	(2)	(3)	(4)	(5)	(6)
Elected	0.199*** (0.0195)	0.163*** (0.0259)	0.211*** (0.0210)	0.203*** (0.0252)	0.202*** (0.0319)	0.242*** (0.0268)
Candidates	1640	1640	1640	994	994	994
Observations	35025	35025	35004	21341	21341	21324
Election: 1998						
	(1)	(2)	(3)	(4)	(5)	(6)
Elected	0.171*** (0.0322)	0.129*** (0.0412)	0.167*** (0.0361)	0.174*** (0.0452)	0.144*** (0.0541)	0.172*** (0.0486)
Candidates	374	374	374	220	220	220
Observations	8034	8034	8034	4741	4741	4741
Sample	Full	Full	Full	Restr. ¹	Restr. ¹	Restr. ¹
Age	Yes	No	No	Yes	No	No
Additional X	No	No	Yes	No	No	Yes

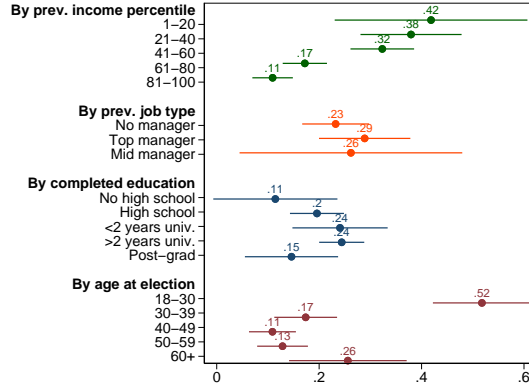
Note: All regressions include election-by-year fixed effects and (election-by-)candidate fixed effects. “Restr.¹” indicates samples that exclude non-elected candidates who were not marginally close to being elected. “Age” indicates quadratic controls for age. “Additional X” are marital status, number 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 4: 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	-4.583 (17.92)	-5.951 (17.96)	0.833 (28.01)	-0.957 (28.55)
Additional X	No	Yes	No	Yes
Candidates	1640	1640	1640	1640
Observations	35046	35025	33513	33493

Note: All regressions are run on candidates from elections 1994–2006 and include election-by-year fixed effects, election-by-candidate fixed effects and controls for age and age². “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 4: Effects of being elected on disposable income for different sub-groups



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.

Previous managerial position does not matter for the size of the returns,²⁴ and 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 is now changing, although available evidence is still scarce and pertains to a limited set of countries.

This paper contributes by quantifying the returns to being elected into the national parliament in a party-centered, mature democracy. A priori, there are reasons to believe that the political returns differ in such a setting compared to in countries with more focus on particular candidates (e.g., the US and the UK) as well as to developing countries (e.g., India), where most available evidence is from.²⁵

Applying a difference-in-difference strategy to rich data on candidates to

²⁴Because information on type of job is only available from 2001, this regression is estimated only on elections 2002 and 2006.

²⁵See for example Diermeier et al. (2005), Eggers and Hainmueller (2009) and Fisman et al. (2014).

the Swedish national parliament, the paper concludes that politics indeed can be a lucrative business also in this setting. The average politician's disposable income increases with around 20% as a result of being elected. Broadly, returns to political office can either stem from direct remuneration or increased outside earnings, and analyses possible thanks to comprehensive income data show the former to be the main mechanism: Rather than opening up possibilities of earnings outside the parliament, the relatively high direct wage implies that those with the lowest default option benefit the most from being elected. At the same time, 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 paper is written concurrently with a set of papers examining various aspects of becoming and being a politician. For example, their children are better off (Folke et al., 2017), while the successful women experience higher divorce rates (Folke and Rickne, 2018). 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. How the pattern of heterogeneous effects across different groups as presented here interacts with selection effects is one such interesting avenue for future research: Is the selection of candidates different from the lower income groups, where the returns are the highest, than in the higher income groups, where the gains from being elected are smaller? And if so, what is the implication for the quality of politicians?

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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 5 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 and 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 and will be part of the 1998 treatment group, and similarly for Daniel. Thus, the 1998 treatment group consists of Sarah and Daniel.

Figure 5: Hypothetical election results for “the Party Party”

PARLIAMENTARY ELECTION 1994		PARLIAMENTARY ELECTION 1998		PARLIAMENTARY ELECTION 2002	
The Party Party		The Party Party		The Party Party	
	<u>Elected</u>		<u>Elected</u>		<u>Elected</u>
1. Lars	x	1. Simon	x	1. Sarah	x
2. Julia	x	2. Sarah	x	2. Peter	x
3. Simon	x	3. Daniel	x	3. Daniel	o
4. Eric	o	4. Alice	o	4. Elisabeth	o
5. Sarah	o	5. Peter	o	5. Bo	o
6. Carl	o	6. Emma	o	6. Sven	o
7. Alice	o	7. Bo	o	7. Emelie	o
8. Hans	o	8. Michael	o	8. Oscar	o

Note: Hypothetical party lists in three consecutive elections.

Turning to the control group of 1998, where 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, since as shown to the right, he is elected in the subsequent 2002 election.

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 dis-

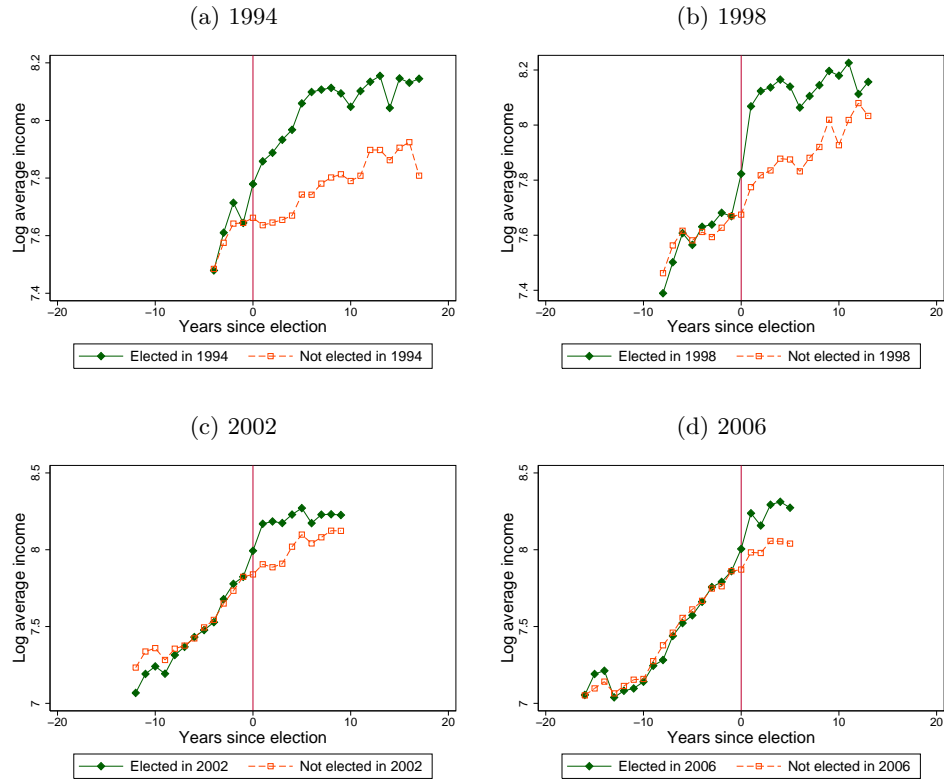
played in Table 5.

Table 5: 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 6: 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 6: Total effects of being elected on disposable income (in logs); weighted regressions

	Elections: 1994–2006				Election: 1998		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Elected	0.226*** (0.0204)	0.232*** (0.0195)	0.235*** (0.0295)	0.212*** (0.0201)	0.206*** (0.0337)	0.203*** (0.0329)	0.214*** (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 age². “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 7: Year-by-year effects of being elected in 1998 on disposable income (in logs)

	All elected in 1998	Re-elected		
		No	In 2002 only	In 2002 and 2006
1996	0.00843 (0.0290)	-0.0276 (0.0478)	0.0647* (0.0383)	-0.00396 (0.0374)
1997	-0.0348 (0.0383)	-0.157* (0.0901)	0.0323 (0.0366)	-0.00502 (0.0463)
1998	0.0963** (0.0381)	0.0837 (0.0639)	0.115*** (0.0379)	0.0866 (0.0547)
1999	0.225*** (0.0453)	0.216*** (0.0706)	0.231*** (0.0489)	0.223*** (0.0638)
2000	0.220*** (0.0420)	0.220*** (0.0702)	0.247*** (0.0477)	0.200*** (0.0592)
2001	0.206*** (0.0411)	0.213*** (0.0658)	0.231*** (0.0470)	0.183*** (0.0598)
2002	0.199*** (0.0417)	0.189*** (0.0659)	0.225*** (0.0465)	0.188*** (0.0617)
2003	0.194*** (0.0418)	0.0780 (0.0585)	0.279*** (0.0486)	0.215*** (0.0610)
2004	0.144*** (0.0535)	0.000295 (0.0861)	0.285*** (0.0502)	0.168** (0.0815)
2005	0.150*** (0.0533)	-0.100 (0.102)	0.300*** (0.0553)	0.231*** (0.0646)
2006	0.138*** (0.0531)	-0.0853 (0.103)	0.279*** (0.0568)	0.190*** (0.0664)
2007	0.0947* (0.0521)	-0.100 (0.0855)	0.128* (0.0654)	0.181*** (0.0689)
2008	0.149** (0.0583)	0.0363 (0.0933)	0.0582 (0.0651)	0.264*** (0.0772)
2009	0.139*** (0.0518)	-0.00351 (0.0736)	0.0950 (0.0717)	0.241*** (0.0721)
2010	-0.00465 (0.0626)	0.00658 (0.0791)	-0.279* (0.143)	0.142** (0.0714)
2011	0.0912 (0.0593)	-0.00751 (0.0935)	0.0606 (0.0652)	0.158** (0.0782)
Additional X	No	No	No	No
Candidates	374	281	284	302
Observations	6551	4896	4948	5289

Note: All regressions include year fixed effects, individual fixed effects and controls for age and age². Standard errors clustered on individual are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 8: Year-by-year effects of being elected in 1998 on labor income (in annual 100 SEK)

	All elected in 1998	Re-elected		
		No	In 2002 only	In 2002 and 2006
1996	237.1*** (76.01)	182.6** (88.45)	452.0*** (146.4)	139.6 (108.7)
1997	275.4*** (89.60)	287.1*** (110.4)	373.1*** (134.7)	213.4 (141.0)
1998	536.7*** (93.30)	595.0*** (117.8)	617.8*** (112.7)	438.5*** (146.5)
1999	1507.2*** (135.9)	1493.3*** (265.7)	1541.8*** (209.6)	1464.0*** (165.2)
2000	1583.0*** (143.5)	1550.0*** (305.2)	1691.4*** (198.9)	1517.6*** (178.3)
2001	1755.4*** (143.7)	1719.2*** (297.6)	1894.4*** (203.8)	1671.2*** (178.6)
2002	1701.8*** (149.0)	1472.3*** (292.3)	1896.4*** (207.6)	1704.5*** (190.9)
2003	1584.3*** (174.9)	543.6 (347.2)	2177.3*** (227.1)	1880.3*** (192.5)
2004	1590.2*** (202.7)	-88.15 (382.8)	2498.8*** (227.9)	2135.5*** (201.0)
2005	1612.7*** (214.6)	-13.42 (420.2)	2503.4*** (258.2)	2173.0*** (204.2)
2006	1515.5*** (221.2)	-362.8 (419.8)	2233.4*** (299.1)	2282.1*** (212.1)
2007	1208.7*** (251.8)	-807.9* (436.9)	770.4* (425.6)	2544.0*** (252.1)
2008	995.7*** (258.4)	-879.4* (459.7)	140.3 (424.2)	2497.0*** (256.8)
2009	1068.7*** (271.8)	-778.4 (500.7)	80.23 (433.6)	2618.9*** (281.2)
2010	703.1** (286.7)	-827.1* (488.9)	-634.1 (449.6)	2287.8*** (334.8)
2011	-190.7 (292.0)	-947.8** (472.2)	-650.1 (440.0)	409.3 (461.1)
Additional X	No	No	No	No
Candidates	374	281	284	302
Observations	6556	4898	4952	5290

Note: All regressions include year fixed effects, individual fixed effects and controls for age and age². Standard errors clustered on individual are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 9: Year-by-year effects of being elected in 1998 on pension income (in annual 100 SEK)

	All elected in 1998	Re-elected		
		No	In 2002 only	In 2002 and 2006
1996	-23.67 (27.31)	-36.39 (65.07)	13.15 (42.42)	-38.14 (30.13)
1997	-45.87 (34.20)	-85.40 (86.56)	-6.845 (48.81)	-45.06 (33.59)
1998	-67.61* (36.67)	-110.0 (89.33)	-25.50 (54.39)	-65.50* (35.79)
1999	-109.4* (57.75)	-56.02 (130.7)	-126.4 (127.6)	-121.7*** (41.16)
2000	-108.4 (66.28)	12.66 (169.9)	-161.1 (132.0)	-135.2*** (44.64)
2001	-115.7* (65.14)	-19.91 (159.6)	-167.8 (135.0)	-126.6*** (45.72)
2002	-139.6* (72.69)	-3.772 (183.3)	-217.6 (139.4)	-155.5*** (50.94)
2003	-99.14 (78.84)	151.9 (197.1)	-202.2 (149.8)	-174.5*** (56.15)
2004	-81.72 (85.84)	281.9 (221.0)	-238.9 (152.9)	-201.3*** (59.89)
2005	-114.8 (92.22)	321.9 (226.4)	-317.9** (157.4)	-275.9*** (64.61)
2006	-65.37 (100.0)	408.9 (250.2)	-195.5 (173.1)	-297.4*** (68.42)
2007	59.94 (119.8)	484.7* (273.5)	365.0 (255.9)	-335.6*** (76.55)
2008	30.30 (122.5)	511.0* (269.0)	389.8 (257.0)	-426.6*** (81.08)
2009	24.21 (133.8)	578.0** (291.8)	493.0* (273.3)	-533.6*** (82.39)
2010	-22.91 (136.6)	630.3** (299.4)	379.2 (274.4)	-588.4*** (86.07)
2011	-49.94 (132.8)	558.8* (284.7)	332.8 (242.9)	-576.3*** (114.3)
Additional X	No	No	No	No
Candidates	374	281	284	302
Observations	6556	4898	4952	5290

Note: All regressions include year fixed effects, individual fixed effects and controls for age and age². Standard errors clustered on individual 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); weighted regressions

	All elected in 1998	Re-elected		
		No	In 2002 only	In 2002 and 2006
1996	0.0128 (0.0295)	0.0276 (0.0790)	0.0503 (0.0369)	-0.00347 (0.0365)
1997	-0.0415 (0.0404)	-0.0953 (0.122)	-0.0147 (0.0372)	-0.0192 (0.0463)
1998	0.107*** (0.0357)	0.110 (0.0739)	0.119*** (0.0400)	0.101* (0.0534)
1999	0.252*** (0.0427)	0.272*** (0.0855)	0.267*** (0.0492)	0.272*** (0.0630)
2000	0.265*** (0.0406)	0.260*** (0.0745)	0.301*** (0.0483)	0.269*** (0.0586)
2001	0.260*** (0.0399)	0.272*** (0.0720)	0.289*** (0.0469)	0.261*** (0.0596)
2002	0.246*** (0.0405)	0.207*** (0.0762)	0.287*** (0.0443)	0.260*** (0.0607)
2003	0.229*** (0.0422)	0.0511 (0.0746)	0.325*** (0.0483)	0.290*** (0.0619)
2004	0.195*** (0.0558)	-0.0187 (0.0998)	0.343*** (0.0513)	0.261*** (0.0841)
2005	0.182*** (0.0518)	-0.142 (0.106)	0.335*** (0.0571)	0.302*** (0.0642)
2006	0.182*** (0.0505)	-0.0924 (0.105)	0.296*** (0.0587)	0.280*** (0.0666)
2007	0.136*** (0.0514)	-0.0945 (0.0914)	0.165** (0.0695)	0.251*** (0.0702)
2008	0.211*** (0.0696)	-0.0456 (0.142)	0.116 (0.0732)	0.384*** (0.0884)
2009	0.166*** (0.0596)	-0.105 (0.133)	0.137* (0.0772)	0.325*** (0.0730)
2010	0.00209 (0.0668)	-0.111 (0.130)	-0.249* (0.144)	0.199*** (0.0714)
2011	0.0962 (0.0646)	-0.154 (0.131)	0.100 (0.0750)	0.206*** (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 age². 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 annual 100 SEK); weighted regressions

	All elected in 1998	Re-elected		
		No	In 2002 only	In 2002 and 2006
1996	134.8 (83.41)	73.18 (105.4)	327.0** (151.2)	82.92 (113.8)
1997	45.32 (96.43)	42.88 (134.0)	109.0 (142.2)	36.28 (139.6)
1998	382.9*** (140.6)	469.5** (196.3)	495.8*** (168.2)	366.4* (193.7)
1999	1507.1*** (184.8)	1556.8*** (321.8)	1583.1*** (264.1)	1469.9*** (235.1)
2000	1579.5*** (169.1)	1598.8*** (333.4)	1721.4*** (233.3)	1534.3*** (212.3)
2001	1802.6*** (185.0)	1866.4*** (340.3)	2002.5*** (254.1)	1796.2*** (236.2)
2002	1757.7*** (179.2)	1654.0*** (323.5)	2037.3*** (250.6)	1791.5*** (229.3)
2003	1577.1*** (202.7)	647.7* (374.1)	2207.5*** (269.5)	1906.3*** (229.5)
2004	1543.8*** (226.9)	-17.23 (407.2)	2507.8*** (270.9)	2120.5*** (236.1)
2005	1569.3*** (236.4)	-2.067 (443.1)	2509.4*** (297.2)	2177.7*** (236.5)
2006	1443.7*** (238.6)	-338.0 (436.6)	2215.1*** (332.3)	2238.2*** (235.2)
2007	1208.0*** (277.4)	-656.7 (482.0)	867.5** (438.4)	2470.8*** (276.8)
2008	990.4*** (283.0)	-708.6 (499.0)	254.1 (437.9)	2404.7*** (281.9)
2009	1052.6*** (301.4)	-608.6 (548.3)	207.1 (456.3)	2482.8*** (312.3)
2010	795.4** (309.7)	-346.8 (519.8)	-280.2 (463.5)	2169.9*** (361.6)
2011	-39.69 (308.5)	-390.5 (487.3)	-225.7 (447.2)	316.6 (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 age². 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 annual 100 SEK); weighted regressions

	All elected in 1998	Re-elected		
		No	In 2002 only	In 2002 and 2006
1996	-21.74 (35.52)	-9.273 (70.60)	-24.86 (74.86)	-24.03 (26.62)
1997	-30.45 (41.04)	-24.52 (92.45)	-30.59 (81.52)	-27.95 (28.12)
1998	-40.59 (44.08)	-33.08 (97.44)	-39.83 (88.39)	-39.67 (28.26)
1999	-75.54 (65.44)	22.79 (136.8)	-137.0 (150.5)	-89.11** (36.61)
2000	-65.37 (73.74)	96.24 (175.6)	-161.6 (157.6)	-93.20** (37.50)
2001	-65.51 (73.42)	84.61 (170.1)	-154.2 (163.1)	-89.37** (35.14)
2002	-78.18 (81.18)	67.15 (193.7)	-199.4 (171.0)	-85.55** (40.85)
2003	-20.53 (86.88)	228.2 (203.1)	-165.5 (180.4)	-99.21** (45.88)
2004	8.246 (93.82)	351.6 (223.2)	-193.5 (185.4)	-111.3** (47.50)
2005	-19.56 (97.90)	338.9 (225.5)	-269.8 (184.5)	-154.3*** (51.52)
2006	32.01 (105.3)	405.3 (249.6)	-144.0 (192.9)	-162.4*** (51.77)
2007	142.4 (125.0)	429.2 (275.4)	396.9 (250.8)	-187.7*** (60.33)
2008	114.2 (127.6)	420.2 (267.1)	416.4* (247.8)	-251.0*** (65.93)
2009	79.64 (139.0)	396.9 (287.5)	458.5* (259.2)	-314.1*** (68.86)
2010	7.222 (139.8)	403.3 (290.6)	317.9 (255.8)	-365.7*** (77.04)
2011	-77.36 (135.9)	292.1 (270.8)	212.2 (221.0)	-381.1*** (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 age². Standard errors clustered on individual are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.