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of Life-Cycle Returns to Political Office**

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ABSTRACT

Reward or Punishment? The Distribution of Life-Cycle Returns to Political Office*

How political office is remunerated will affect who decides to engage in politics. Even if average returns to office are positive, as unilaterally found in the literature, some office holders' returns are likely zero or negative. The timing of returns to office are crucial too, as politicians often have lucrative pensions and other types of delayed compensation. Utilizing data for all parliament candidates in Denmark from 1994 to 2015 linked to administrative data, we causally estimate the returns to office for first-time runners to parliament. We find large short-term average returns to office, corresponding to a 112% income increase. Quantile Difference-in-Difference estimates reveal considerable heterogeneity, but, strikingly, all MPs experience an economic gain during their first term. The distribution of life-cycle returns, computed as the net present value, reveals that candidates from the top quarter of the pre-office income distribution have no long-term economic gain from winning.

JEL Classification: J33, J4

Keywords: returns to office, political careers, proportional representation

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1 Introduction

Are political careers economically attractive? Theoretically, two competing views exist on the appropriate level of salaries for politicians. Firstly, higher wages could reduce the quality of candidates (Besley, 2004; Poutvaara and Takalo, 2007; Mattozzi and Merlo, 2008). According to this perspective, good candidates are intrinsically motivated to do good for the public and they will run even if wage returns are low. In this case, if politicians' wages increase, good candidates could be crowded out by candidates who are not interested in the public good, but instead run for the prospect of own economic gains. Following the second view, high quality political candidates are expected to have better outside options. With better outside options also comes higher opportunity costs of running for office (Caselli and Morelli, 2004). According to this perspective, the average quality of politicians increases with higher remuneration as the share of qualified candidates who decide to run increases with the level of compensation.

Regardless of which perspective one holds, an important question is what winning a political seat is worth economically for different candidates. This question will be the focus of our paper. We make two empirical contributions to the existing literature on returns to office. First, we consider how returns to office may be highly heterogeneous and maybe even negative for a certain proportion of the candidates. Second, both the timing and the length of the period in which returns are received are likely to be important for the estimated returns to office. Consequently, we need a better understanding of the total life-cycle returns to office.

As our first key contribution, we take a deeper look at the heterogeneity of returns to office. Heterogeneity may be rooted in observable characteristics such as previous occupation, gender, or political affiliation. Eggers and Hainmueller (2009) compare

members of Parliament with candidates who narrowly lost, and find that becoming an MP almost doubled the wealth of Conservative MPs while it had little or no impact on the wealth of MPs from the Labour party. Both [Berg \(2020\)](#) and [Kotakorpi, Poutvaara and Terviö \(2017\)](#) consider effect heterogeneity over several observable characteristics and find some differences in Sweden and Finland.

However, effect heterogeneity may also be manifest over unobservable characteristics that will impact future income trajectories. When prospective candidates weigh the economic cost and benefits of running for office, they should not only consider where they are currently positioned in the income distribution; they should also consider the counterfactual income trajectory that they would follow in the absence of holding office. Some candidates who run for office may forego excellent career opportunities if they win a seat, while others will have lower outside options and therefore gain more from a seat in Parliament. To capture this kind of effect heterogeneity, we apply a newly developed estimator for unconditional quantile difference-in-differences to estimate the distribution of unconditional quantile treatment effects of the treated ([Callaway and Li, 2019](#)).

As our second key contribution, we also consider the timing of returns to office. On the one hand, politicians are, in addition to having a relatively high income while in office, generally rewarded with unusually high pensions vis-à-vis average pensions found in the labor market, and these should evidently be included in an analysis of returns to office. In addition, deferred opportunities for ex-politicians may arise through revolving doors, whereby they become board members or high ranked employees in private organizations ([Weschle, 2019](#); [Egerod, 2020](#)). On the other hand, some politicians may experience an earnings penalty after their parliamentary career, either because they miss out on nonpolitical work experience that they otherwise

would have accumulated in their regular career or due to an unwillingness of employers to hire certain former politicians.

In this paper, we rely on rich data from 1990 to 2017 to provide a long-term view of annual returns to office. Kotakorpi, Poutvaara and Terviö (2017), Berg (2020), and Cirone, Cox and Fiva (2020) have already estimated long-term effects in Finland, Sweden, and Norway.¹ In addition to just looking at annual returns to office over time, our long-term perspective also allows for a more comprehensive way of measuring the true financial returns to office that is in line with Diermeier, Keane and Merlo (2005), who compute returns to congressional careers in the US.

Specifically, we estimate the net present value (NPV) of cumulative returns to office at different periods in a life-cycle. The net present value computations, by which the individual discount rate makes income today comparable to future income streams, addresses timing problems in comparing income streams occurring at different points in time. Essentially, it discounts future income streams, because it assumes that additional income now is more valuable than future income streams. As the financial incentives to run for office may differ depending on where a candidate is in their career, we compute the NPV of winning office for all politicians as well as for different cohorts of politicians, based on their age when first elected and for different pre-office income levels.

We rely on rich administrative data for the entire Danish population going back to 1985, and we link these registry data to information about all candidates that ran in parliamentary elections from 1990 to 2019. For identification of the returns to holding office, we compare first time running candidates who win a seat to first time

¹In an unpublished paper, Willumsen (2011) finds that serving in office for at least one period cause income to rise by 10-15% among Norwegian MPs, even decades after being elected the first time. While the studies discussed above, focus on income, other studies also consider wealth later in life or wealth accumulation while in office (Eggers and Hainmueller, 2009; Querubin and Snyder Jr, 2013; Fisman, Schulz and Vig, 2014).

running candidates who lose. In the spirit of [Eggers and Hainmueller \(2009\)](#) and other previous papers, we limit our sample of candidates to close winners and losers to rule out undue influence from extremely successful first-time candidates. For the full set of close winners and losers, we observe income both before and after the election year, which means that we can use the income paths of candidates who *almost* made it for office as a credible measure of the unobserved counterfactual income stream of newly elected politicians. By including first-time runners only, we ensure that candidates are untreated prior to the close election, and we can utilize the panel dimension of our data to compare close winners and losers in a difference-in-differences design.

In line with previous research, we find substantial short-term returns to holding office. In the first two years after winning office, returns to office of marginally elected candidates are found to be €69,500 annually, equivalent to a 112% increase compared to their average pre-office income. These returns are very high compared to similar estimates for comparable countries. [Berg \(2020\)](#) estimate returns to office of about 20% while still in office for Swedish MPs; [Kotakorpi, Poutvaara and Terviö \(2017\)](#) estimate the private returns to being elected to the Finnish parliament to approximately 25% annually for first-time elected; and, based on Norwegian data, [\(Cirone, Cox and Fiva, 2020\)](#) find that winning a seat in Parliament gives an annual income increase of NOK 150,000 (\sim €15,000).

In addition to the large short-term returns, this paper has two key findings. The first key finding is that, when we adapt a newly developed unconditional quantile treatment effect approach based on quantile difference-in-difference estimates, we obtain positive estimates of the returns to office in the first term across the entire distribution. Since this short-term gain is the most foreseeable for the running candidates, it means that no group of candidates running for office have to settle for a lower income if they win.

The second key finding arises after having computed the net present value of all future returns or losses that accrue from being elected when a first-time runner. When we split these returns by quartiles of the pre-income distribution, we find that for winners from the the fourth quartile, the NPV of winning a seat in Parliament is zero after seven years. This indicates that MPs in the high end of the pre-election income distribution in the longer run are missing out on outside opportunities, which is in stark contrast to the initial finding of very high average returns in the short run.

Do our results apply for other countries? The main takeaway from this paper is that the distribution and timing of returns to office may have a fundamental impact on our view and understanding of MP's remuneration. This is most likely relevant in most countries. Arguably, the importance of analyzing returns beyond short-term averages may depend on the country in question. The income distribution in Denmark is narrow, and measured by the Gini-coefficient, it shares a sixth lowest place among EU countries in 2019 (Kolluru and Semenenko, 2021); Denmark has high trust in politicians compared to other European countries (Van Erkel and Van Der Meer, 2016); and Denmark has low perceived corruption (Barfort et al., 2019). All of this puts our context on par with those of other Nordic countries where returns to office are modest (Kotakorpi, Poutvaara and Terviö, 2017; Berg, 2020; Cirone, Cox and Fiva, 2020) and where distributional effects could be lower than for less economically equal societies. Yet, even in a context of low corruption and high economic equality the returns to office are highly heterogeneous, and our contributions therefore add fundamentally to an improved understanding of the returns to office.

2 Data and context

2.1 Administrative and election data

We rely on administrative data from Statistics Denmark from 1990 to 2018. The administrative data have a very high quality and contain information on a broad range of variables including detailed income measures at the individual and household level and link to family members. We have annual observations for all Danes born between December, 1912 and June, 1997. All Danes have a unique civil registration number, which allows us to link the administrative data to information on all candidates that ran in any parliamentary election from 1990 to 2019. In addition, we have information on the year, district, and political party that candidates ran for, whether they ran for a party with an open or semi-open list, whether they were elected, and a variable measuring their win or lose margin. In Section 3.1, we describe in detail how we construct the win margin and use it to define close winners and losers.

2.2 Danish politicians' salaries and pensions

For our analyses, all measures of income are standardized to 2015 prices and converted from Danish Kroner to Euro.² All Danish MPs receive at least two sorts of remuneration while in office:³ 1) a base pay of around €85,000 per year and 2) a tax-exempt cost allowance of around €8,000 per year, which is equivalent to a taxable income of around €18,000 per year. Originally the cost allowance was intended to cover extra work costs of being a politician (like telephone bills and post delivery),

²The Danish currency is fixed to follow the price of the euro within a bound of $\pm 2.25\%$ around 7.46038 DKK per €. We therefore use a fixed conversion rate of 7.46038 DKK pr €.

³During the time period we study there have been changes made to their compensations, mostly making them less attractive. We review only the current compensation scheme here. This means that we may slightly underestimate the value of a political seat in the earliest of our elections.

but with modern technology it is essentially an additional source of income, and we therefore follow a recent official commission on politicians' salaries and define this as income (Vederlagskommissionen, 2016). In addition to their remunerations while in office, MPs are entitled to a pension paid out annually from retirement age⁴ and until their death. The size of the pension increases with each year of service, until 20 years of service, where it reaches the maximum of 53% of an MP's base salary.⁵ Should the MP pass away before their spouse, their spouse will be entitled to 71% of the full pension of the MP until their own death. The commission on politicians' salaries estimated the annual value of the pension scheme to be approximately €28,000.⁶ While in office, the MPs are free to hold other remunerated jobs without a reduction in their remuneration, except for the MPs who are appointed as ministers. When MPs leave office because of an election or illness, they are entitled to a post-office remuneration equivalent to their salary in office for up to two years. Any income earned during this period is deducted, except for a base rate in the first year.⁷

Our main outcome is gross annual income, which we measure as the total of labor market income (including net profit from own businesses, regular wages, and remunerations), welfare benefits, the value of the tax exempt supplement, and the value of annual payments to employer administered retirement benefits. Both total labor market income and welfare benefits are recorded annually in the administrative data and measured as pre-tax incomes for all candidates. For politicians serving in

⁴For candidates elected before 2007, the retirement age is 60, for those elected after 2007 it follows the early retirement age in Denmark, which varies from 64 to 67 years of age depending on cohort. Since 2017, after the last of the elections we consider, it has followed the general retirement age, which in 2021 is 66.5 years of age and is set to increase for coming cohorts.

⁵<https://www.ft.dk/da/medlemmer/medlemmernes-vilkaar> Accessed on 10 June 2021

⁶The Parliament also provides tax-exempt supplements for housing for MPs whose permanent residence is distant from the Parliament (for keeping a "double-household"), but we do not consider those to be income as they are tied to specific expenses.

⁷Some former politicians circumvent the deduction by establishing themselves as self-employed consultants or similar, which allows them to keep their income in their business and only extract their surplus after the two year period.

parliament, we add the pre-tax value of the tax exempt supplement since this is the value that it would have for ordinary workers. For all candidates, the payments for employer-administered pensions are recorded in the registers. However, for those who win a seat the value of the annual pension increment is not recorded in the registers. Accordingly, we add this to the income of the politicians.⁸

We label our income measure *total income*. It covers all formal office-related incomes of politicians, but it does *not* include income from assets or shareholding as this income is potentially affected by personal choices rather than being an officeholder.⁹ We use gross income because Denmark has progressive taxation and various rules for allowances and tax deduction, which might apply for politicians depending on their personal choices. Winning office could affect such personal choices and accordingly post-tax income becomes endogenous. In Appendix E, we measure returns by an alternative measure that we label *disposable income*. We construct this measure as the gross annual income (*total income*) minus total annual taxes.

2.3 Danish national elections

The Danish national parliament has one chamber, *Folketinget*, with 175 members elected in the Danish principal country while two are elected in Greenland and two in the Faroe Islands. We have election data on candidates in Denmark only and administrative data for people residing in Denmark only, and accordingly we include only these candidates in our analyses. Election terms are not fixed. Instead the Prime Minister decides when to call an election within four years of the most recent election.

⁸Some losing candidates will serve part of a term as substitutes for a winning officeholder. As we cannot identify which losing candidates do this without compromising the anonymity of the candidates, we cannot add the tax exempt supplement and pensions to this group of initially losing candidates while they serve.

⁹Previous research has studied returns to portfolios of elected officials with mixed findings (Zio-browski et al., 2004, 2011; Eggers and Hainmueller, 2013).

Our data covers the following national elections: 1990, 1994, 1998, 2001, 2005, 2007, 2011, 2015, and 2019.

For our purposes, a simplified description of the seat allocation will do, and we refer the interested reader to Appendix A [drop Appendix A?] and [Elklit, Pade and Nyholm Miller \(2011\)](#) for a more elaborate description. The Danish electoral system is a proportional system. There is an overall, proportional allocation of seats within 10 multimember constituencies (*storkredse*), with nationally allocated compensatory seats to assure proportional representation.^{10,11} In each constituency, the seats are distributed to the parties and subsequently allocated to individual candidates. Voters can either vote for a party list or for a personal candidate. A vote for a candidate is also a vote for the party of the candidate.¹² Each party can either run using an open list or a semi-open list.

We include only politicians from parties that ran on open lists.¹³ For parties that run on the open lists, candidates are allocated some share of the votes for the party following one of two general rules. Once these votes are assigned, the candidates are ranked by their personal votes and the m highest ranking candidates are awarded a seat in parliament, where m is the number of seats assigned to the party in the constituencies.

¹⁰Before 2007 the number of constituencies was 16.

¹¹All parties that win at least 2% of the votes get a proportional share of seats according to the Hare's quota. For parties that fail to reach the 2% threshold, there are two other mechanisms that can ensure their representation, but empirically it is the 2% threshold that has been important.

¹²Independent candidates are rare. Since 1945 only one candidate has ever been elected as an independent. Independent candidates are mechanically dropped from our analyses due to the way we operationalize the win margin.

¹³In semi-open lists, parties prioritize their candidates and assign party votes to the top candidates, which reduces internal competition and discounts the value of the personal votes. The open-list structure has been prevalent for decades, and since the election in 2011 a semi-open list has been used only by the Red-Green Alliance, a left-wing party. Another left-wing party, the Socialist People's Party, used semi-open lists in some voting districts and open-list structures in others up to and including the election of 2007, and the right-wing nationalist party the Danish People's Party used semi-open lists in the election of 1998, which was the party's first election on the ballot.

3 Research design

When identifying returns to office, there are two selection mechanisms to consider: *who runs* and *who wins*. Regarding the first mechanism, the decision to run for office is not random. Political candidates and other citizens differ in many ways and the decision to run for office is correlated with various personal attributes, which in turn are likely to be correlated with other personal outcomes such as economic success or ability (Dal Bó et al., 2017; Dahlgaard and Pedersen, 2019). Therefore, simply comparing politicians to nonpoliticians is not a valid approach. In line with most of the established literature, we address our first selection issue, who runs, by considering only candidates running for office (Eggers and Hainmueller, 2009; Querubin and Snyder Jr, 2013; Kotakorpi, Poutvaara and Terviö, 2017; Berg, 2020).

Regarding the second selection mechanism, who wins conditional on running, comparing political winners to political losers will in all likelihood capture the effect of holding political office as well as some additional candidate qualities since winning candidates have higher general ability than losing candidates (Dal Bó et al., 2017). To overcome this problem, the standard in the literature is to compare marginal winners and marginal losers around the threshold for election success as these candidates should be highly comparable prior to running for office. We follow this standard and define a set of close winners and losers. We take this standard a step further by exploiting the panel structure of our data, which allows us to follow all candidates before and after they run for office. This provides comprehensible insights about the income trajectories of the candidates. Specifically, we can use a difference-in-differences (DiD) model on our set of marginal winners and losers and thereby let the income trajectory of losing candidates serve as a counterfactual to the income tra-

jectory of winning candidates to derive credible causal estimates about how winning office affect the income development of winning candidates.

3.1 Defining marginal winners and losers

As we described above, we consider only parties running on open lists. The open list proportional system means that candidates compete both with candidates from other lists and with candidates on their own list, which makes it harder to define a threshold of winning based on the vote distribution than in a first-past-the-post system (Folke, 2014). It is ambiguous whether an additional vote for politician A increases or decreases fellow party member B’s chances of winning. The additional vote for A will increase the party’s probability of winning an extra seat, and thus increase B’s chances of winning a seat, but simultaneously it makes B worse off in the intra-party competition to A, which in turn decreases B’s chances of winning a seat. A variable expressing electoral closeness needs to account for this ambiguity.¹⁴

Some previous papers rely on a simulation based method to identify a forcing variable, which can then be applied in a regression discontinuity design (Dahlggaard, 2016; Kotakorpi, Poutvaara and Terviö, 2017). Unfortunately, Statistics Denmark would not allow us to link such a fine grained measure of individual level electoral success to the administrative data as it would compromise the anonymity of the politicians in our data. Instead, we were allowed to recode personal votes to electoral closeness at the individual level before linking to the data.

Our strategy is to define marginal winners and losers in each party by district and election year. Thus, we take the number of votes and seats for the party as given and then consider how seats are assigned within the party. For every election, district,

¹⁴Previous studies have worked around this challenge in a variety of creative ways. See, for example, Willumsen (2011); Folke (2014); Dahlggaard (2016); Folke, Persson and Rickne (2017); Kotakorpi, Poutvaara and Terviö (2017); Berg (2020); Cirone, Cox and Fiva (2020)

and party, we define a threshold as the midpoint between the elected candidate with the fewest votes and the nonelected candidate with the most votes, i.e. the marginal winner and loser in the list in a district and election year. Such a threshold is highly context-specific and accounts for the ambiguity of between and within party competition as it fixes competition between parties and compares the candidates for each combination of party, district, and year that just made it or marginally lost. One limitation is that the threshold is only defined for parties that won at least one seat in a district in a given year. Based on this year, district, and party specific threshold, we compute for each candidate a score expressing how many votes a candidate received relative to his/her threshold:

$$\text{Relative score}_i = \text{abs}\left(\frac{\text{personal votes}_i}{\text{threshold}_{pdy}} - 1\right)$$

where i is a subset for the individual candidates and pdy are subsets for party, district, and year. The relative score takes the value '0', if a candidate receive exactly the number of votes needed for a marginal seat. As we take the absolute value, the relative score is nonnegative. In Appendix B we explain our measure of electoral closeness further and give a numerical example of how to estimate it.

Upon creating the relative scores, Statistics Denmark required that we aggregated the relative score into 25 fractiles to ensure the candidates' anonymity. The smaller the fractile in which a candidate is placed, the more narrowly did the candidate win or lose. Because our measure of electoral closeness is relatively crude, it does not lend itself well to a regression discontinuity. As we still want to focus only on candidates in close elections, we restrict the sample of candidates to those in the first five fractiles, i.e., the candidates with the 20% smallest values on the relative score

metric. Combined with the panel structure of our data, this gives good identification in a DiD.

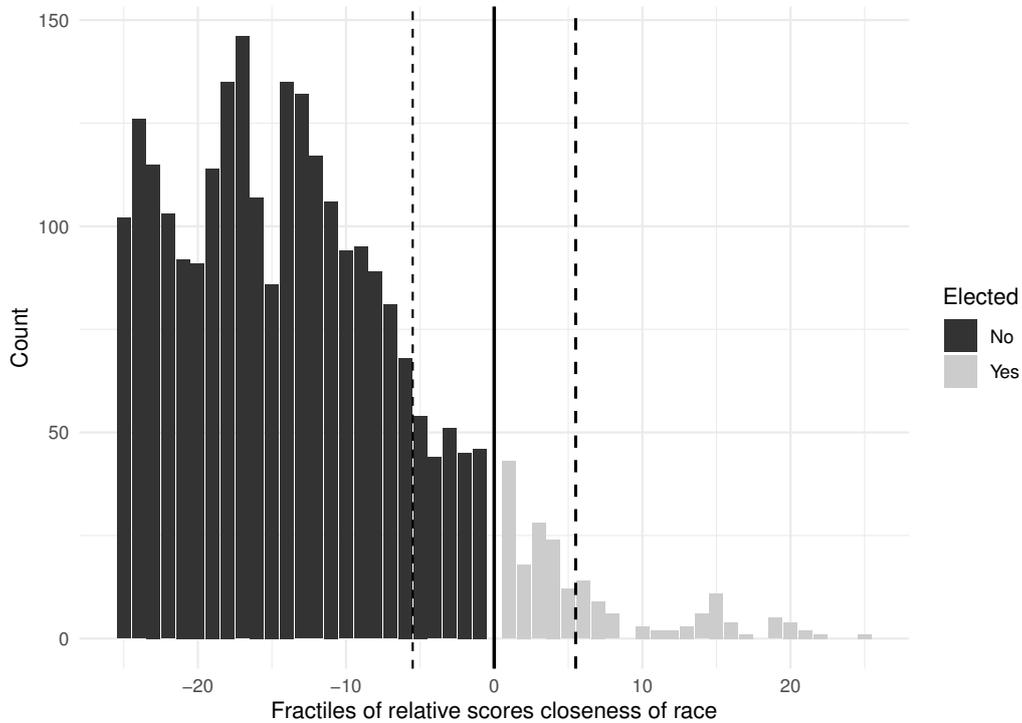
After defining close winners and losers, we limit our sample to candidates who have not previously been running for office in the Danish Parliament. Using first-time runners only ensures that the candidates have not been treated before, and therefore their pre-election income will be untreated¹⁵. The first election for which we have data is 1990. Because we cannot link administrative data to candidacy in elections prior to 1990, we cannot identify first time runners in that election, so we do not estimate returns to office for candidates running before 1994. For every election, from 1994 to 2015, we remove all candidates with a prior candidacy for the parliament.¹⁶ We also have data on candidates in the 2019 election, but we do not have post-election income data on them, which means that we cannot estimate their returns to office.

In Figure 1, we show the distribution of our variable for electoral closeness for our sample across all elections. In the figure, we have flipped the value for the losing candidates around zero, which means that losing candidates are assigned the negative value of the electoral closeness measure. Evidently, the distribution is right skewed because most candidates are far from competitive while few first-time candidates do extremely well. For our main results, we include only candidates between the two

¹⁵See (Querubin and Snyder Jr, 2013) for a thorough discussion of the advantages of including first-time runners only.

¹⁶We will probably misclassify some candidates who ran in 1987 and 1994, but not in 1990, as first-time runners in 1994. To gauge the size of this problem, we identified how many candidates in our data ran in 1998 and 1990, but not in 1994. We found that 4.2% of the first-time candidates in 1998 had also run in 1990, but not in 1994. This indicates that misclassification should only induce a small error. Especially because misclassified first-time runners in 1994 by definition did not serve in office in the years leading up to the election of 1994. Thus, the small proportion of misclassified first-time runners still approaches the treatment (winning office) from an untreated state.

Figure 1: The distribution of fractiles of relative scores (closeness of race) for first time running candidates from parties with open-lists.



Note: The relative score is partitioned into 25 fractiles, indicating what twenty-fifth of electoral closeness a candidate’s race was. For visual purposes, in this plot we have assigned negative values to losing candidates, i.e., their fractile as a negative value. The solid line expresses the threshold of winning and losing (zero), and the closer the value is to zero, the more competitive was the candidate’s race. Candidates between the two dashed lines are the candidates included in our sample.

dashed lines. Table 1 shows the number of marginally winning and losing first-time runners that we have in our sample for each election year.

3.2 Difference-in-differences model

As we have already described above, our measure of electoral closeness does not lend itself well to a regression discontinuity design. Instead, we leverage the panel structure of our data and the close elections to identify the immediate returns to office

Table 1: Number of marginally winning and losing first-time running candidates from open-list parties in each year

Election year	Winners (treatment)	Losers (control)	Total candidates
1994	16	34	50
1998	19	33	52
2001	27	47	74
2005	20	34	54
2007	14	35	49
2011	16	25	41
2015	13	32	45
Total	125	240	365

with a DiD estimator. For our pretreatment measure, we average income over the four years preceding an election. We identify close winners and losers in each election before we stack all the data for our estimations. In our sample, every candidate is a first-time runner, and therefore the four years prior to their first election will always express the candidates' pre-office outcomes. We specify the post-treatment period to be the two years following the election year.¹⁷ We omit the election year, as it will include a mix of the candidates' pre- or post-treatment income. To estimate returns to office, we estimate the following model:

$$Y_{i,t} = \alpha + \beta_1 \text{Elected}_i + \beta_2 \text{PostElection}_t + \beta_3 \text{Elected}_i \times \text{PostElection}_t + \epsilon_{i,t}$$

where $Y_{i,t}$ expresses the income of a first time runner, i , averaged over period t . Elected_i is a dummy, which equals 1 if the first-time runner, i , won a seat, PostElection_t is a dummy, which equals 1 if the period, t , is after the election. The coefficient on the

¹⁷This means that the only post-treatment period that interferes with the subsequent election is for the elections of 2005 and 2007, where a marginal candidate in the 2005 election might rerun in 2007. This can muddle the estimates. However, as the election in 2007 was held in the middle of November this will have a negligible impact.

interaction between these two variables, $Elected_i \times PostElection_t$, expresses the DiD, and hence β_3 is our parameter of interest; how income changes differently between winners and losers following an election. The error term, $\epsilon_{i,t}$, is clustered at party-constituency-year level (231 clusters), as this is the level of treatment assignment¹⁸.

3.3 Estimating quantile treatment effects

Next, we want to investigate the *distribution* of returns to office of winners relative to what it would have been had they not been elected. To study this we follow recent advances in the literature and estimate the unconditional quantile treatment effects on the treated (QTT) (Callaway and Li, 2019). Previous papers have looked at heterogeneous returns to office by various covariates including pre-office income (Kotakorpi, Poutvaara and Terviö, 2017; Berg, 2020). Estimating the QTT goes beyond this approach by not only estimating average returns to office at different pre-office incomes but also estimating how becoming elected changes the income trajectory. We estimate the QTT based on pairwise two-year averages of the candidates' income in the four years leading up to their first election and the two years after, and label these $t-2$, $t-1$, and $t+1$, respectively.

When estimating the *average* treatment effect on the treated, the DiD approach hinges on the assumption of parallel trends. To estimate the QTTs, Callaway and Li (2019) impose a slightly stronger assumption of distributional DiD: $\Delta Y_{0,t} \perp D$, where $\Delta Y_{0,t} = Y_{0,t} - Y_{0,t-1}$ denotes the difference in untreated potential outcomes over time and D is the treatment. This distributional DiD assumption states that the

¹⁸A recent literature discusses inferential issues in DiD designs with different treatment timing or staggered uptake (Goodman-Bacon, 2018; De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2020; Athey and Imbens, 2021). Because everyone is treated or not in each election year, which is simultaneous with the first time they appear in our data, we do not have staggered uptake, we just have new observations appearing in each election.

distribution of changes in untreated potential outcomes does not depend on whether the individual belongs to the treated group or the untreated group.

An additional assumption is invoked concerning dependency, namely the Copula Stability Assumption, which states that both the change in the untreated potential outcomes of the treated group and the initial level of the untreated potential outcomes for the treated group are stable over time (Callaway and Li, 2019). Note that the assumption does not imply anything about the qualitative properties of this dependency; it merely states that the dependency would have been sustained in a counterfactual world in which the treated had not been treated¹⁹.

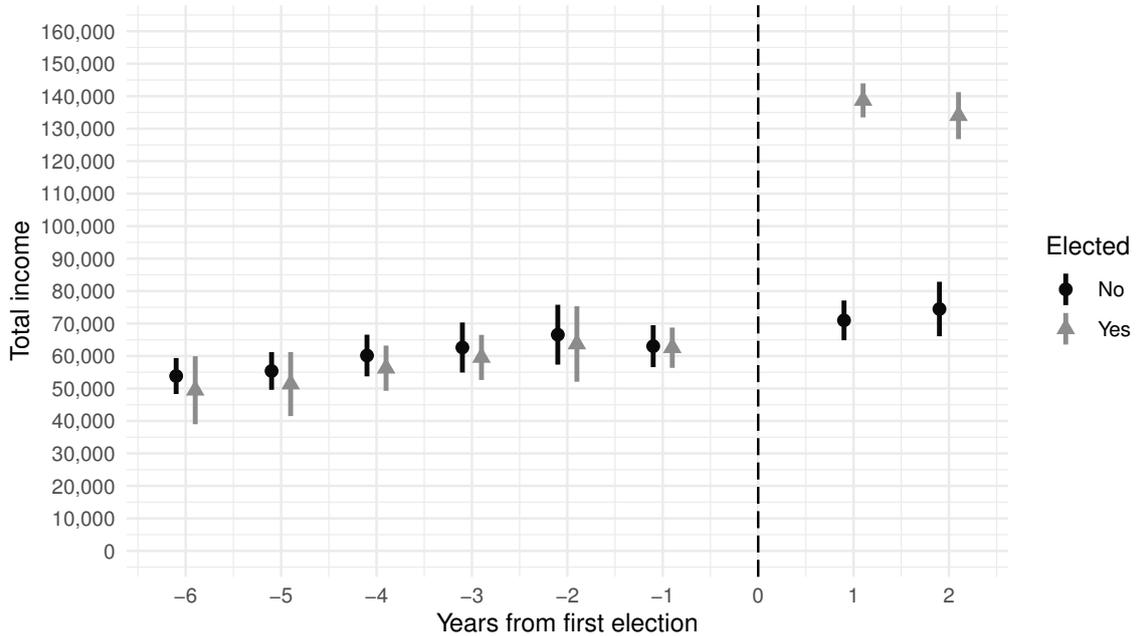
The QTT estimator derives the dependency from the distributions of income in $t-2$ and $t-1$, i.e., prior to the election, and projects this as the counterfactual dependency between the income distributions of elected and nonelected. While this assumption is impossible to test directly, we show in Appendix C that the rank correlation between lagged income and the first difference in income is indistinguishable between the treatment and the control group in the five years leading up to the election, which constitutes strong evidence in favor of the validity of this assumption.

4 Results

Figure 2 shows the average income for winning and losing first-time runners in the six years preceding their first run for office and the average income in the three years after first running for office. The figure provides visual proof both that the income trends of the two groups were as good as parallel before the election and that in every year preceding the election the incomes of the two groups were indistinguishable.

¹⁹As an example related to our analysis, the assumption implies that, if initially the largest income increases accrued to those in the bottom of the income distribution, the largest income increases in the present should continue to be accrued by those initially placed at the bottom.

Figure 2: Average income in the six years preceding and two years following a candidate's first run for office.



Note: Total income in € in 2015 prices. Standard errors clustered at party-constituency-year level. 95% pointwise confidence intervals.

In Appendix D, we show that the full distribution of pre-office income is also balanced over all years, on average, as well as in any given election year, and that the winning and losing candidates are balanced on other covariates. In Appendix D, we also conduct a placebo test to test formally the parallel trends assumption, where we estimate the DiD estimate as if the elections took place between any two years within six years before the actual election. This is a strong test of the assumption as it does not assume any functional form for the trends. The test shows that the DiD between winning and losing first-time candidates in any two years before the actual election is negligible and far from statistically significant.

In Figure 2, we can see a distinct income jump among winning candidates in the years succeeding the election. As described above, we average over the four years preceding the election to find the pre-election income and the two years after the

election to find the post-election income. In Table 2, we present our estimates for the average returns in the two years immediately following the election year. The estimate shows how much, on average, marginally winning candidates' income, including their employer-paid pension, contribution changes after the election compared to marginally losing candidates.

We estimate an average, yearly return of €69,505 over the first two years. The income returns are precisely estimated with a 95% confidence interval of [60,921; 78,089], and it is substantially large and meaningful. It corresponds to 112% of the winning candidates' pre-office income. In the second column of Table 2, we show that our estimates are similar. Here the 95% confidence interval is [60,077; 76,866], when we include fixed effects for party-constituency-year. Compared to results from other Nordic and highly comparable countries, our estimate of the short-term average returns to office is considerably larger (Kotakorpi, Poutvaara and Terviö, 2017; Berg, 2020; Cirone, Cox and Fiva, 2020). In the discussion, we return to why we might find returns that are so much larger in Denmark.

It is worth mentioning that losing candidates also see their income increase, as evidenced by the both statistically and substantially significant coefficient on *Post Election* in Table 2. We point to four potential mechanisms for this decrease: Firstly, as we can see from Figure 2, candidates are generally on an upwards income trajectory around the time they run for office for the first time. Secondly, Figure 2 also reveals that this upwards trajectory sees a drop in the year before the election, probably because candidates forego income to campaign. Such a drop should be temporary. Thirdly, the party might reward losing candidates who campaigned well with jobs in the party organization. Fourthly, losing candidates might be partially treated if they serve in Parliament for winning candidates who (temporarily) leave Parliament due to illness, leave, or pursuit of other career options.

Table 2: Short-Term Average Returns to Office

	Total income (2015 €)	
	(1)	(2)
Elected × Post-election (DiD)	69,505*** (4,377)	68,471*** (4,280)
Post-election	7,421** (2,995)	9,964*** (2,799)
Elected	-1,769 (5,047)	3,076 (6,970)
Constant	64,002*** (3,549)	31,037*** (6,577)
Fixed effects for cluster?	NO	YES
Candidate-years	2,176	2,176
Candidates	365	365

Note: The DiD estimate expresses the immediate returns to office measured by the average returns to office including pension contributions over the first two years after the election year. Estimated without (1) and with (2) fixed effects for clusters of party-constituency-year ($k=231$). Standard errors are clustered at the party-constituency-year level in both models. *, **, and *** indicate $p < 0.1$, < 0.05 , and < 0.01 , respectively.

The increase in losing candidates' income does not bias our estimate of the benefit from winning in a close election. The first three mechanisms would apply to the winning candidates too had they lost, while the last mechanism, the partial treatments of losers, has the reverse implication for the winners, meaning that some of them might not be fully treated. Consequently, the correct way to think of our estimate is to consider it as an intent-to-treat effect, where there is some non-compliance among both losing and winning candidates.

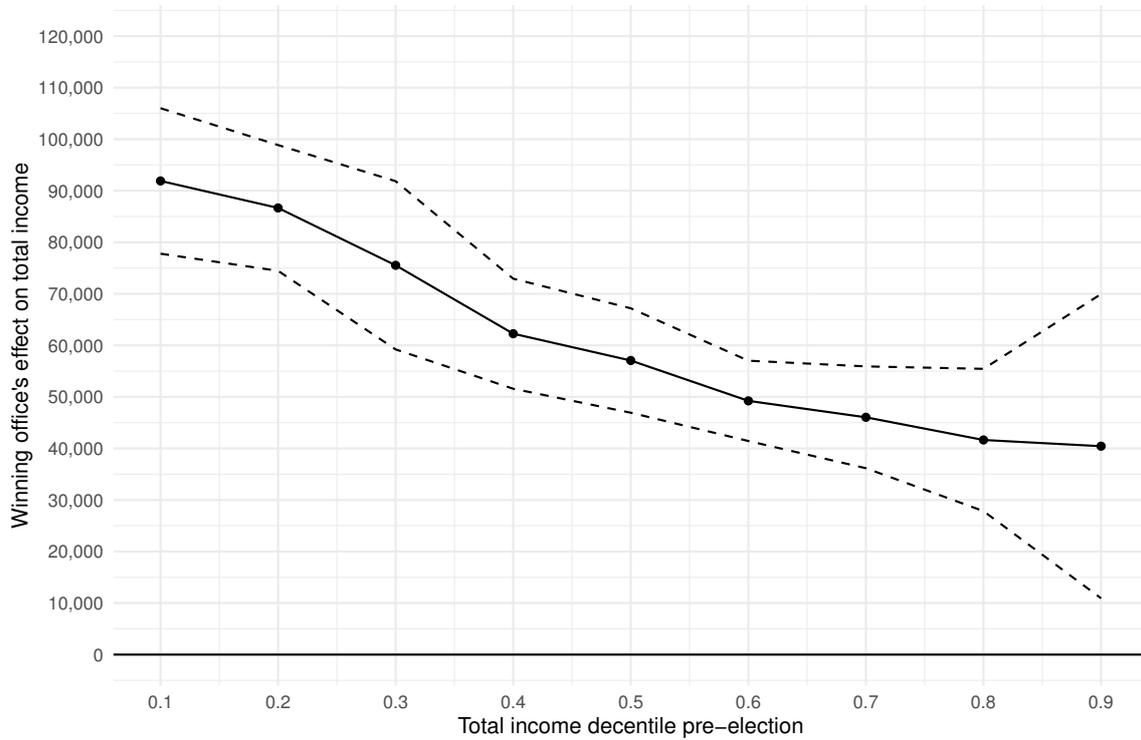
In Appendix G, we show that our results are robust to changing the 20% bandwidth around the election threshold. Furthermore, in Appendix E, we estimate returns to office without pension contributions to get a clearer estimate of the immediate compensation pay, and we estimate the returns to disposable income. Since the pension scheme for MPs is very lucrative, the returns are considerably lower without pensions, with an increase of 74% relative to the baseline, and due to progressive taxation they are even lower, yet still substantial, at 69% relative to the baseline when measured as disposable income. We also show that politicians neither lose nor gain other perks or nontaxable income.

4.1 The distribution of short-term returns to office

Does everyone running for office experience positive returns or is there a set of candidates for whom the returns are null or even negative? To answer this question, we turn to the unconditional quantile treatment effect estimates. Figure 3 shows the quantile treatment effects on the treated for every decile in the income distribution of the first time running candidates in our sample.²⁰ The outcome variable is our comprehensive income measure that we also use in Figure 2.

²⁰We apply the `qte`-package in R, [Callaway \(2016\)](#).

Figure 3: Short Term Quantile Treatment Effects on the Treated



Note: The plot illustrates the estimated returns to office for every decile in the pre-election distribution of income trajectories based on pairwise annual averages of $t-4$ to $t+2$. 95% pointwise confidence intervals computed using the empirical bootstrap with 1000 iterations.

Figure 3 reveals two important results: *i)* across the entire distribution, *all* deciles gain economically from being elected and *ii)* heterogeneity is substantial. Candidates with low pre-election incomes experience returns to office that are close to €92,000 in the first year after election, which is substantially higher than the average of approximately €70,000. Yet, even those in the highest decile, those expected to have the highest counterfactual incomes, still receive substantial economic returns to office of around €40,000.

The unconditional quantile treatment effects leave no doubt that there are some candidates for whom winning office in the short term is substantially better economically than it is for other candidates. Since there is little variation in income while

in office, this largely reflects differences in the opportunity cost of holding political office instead of some other occupation. However, even at the 90th percentile, losing candidates would have been better off economically, if they had won a seat in parliament. This means that across the entire pre-election income distribution all winners gain in terms of their private economic situation. We discuss the implication of this finding in Section 5.

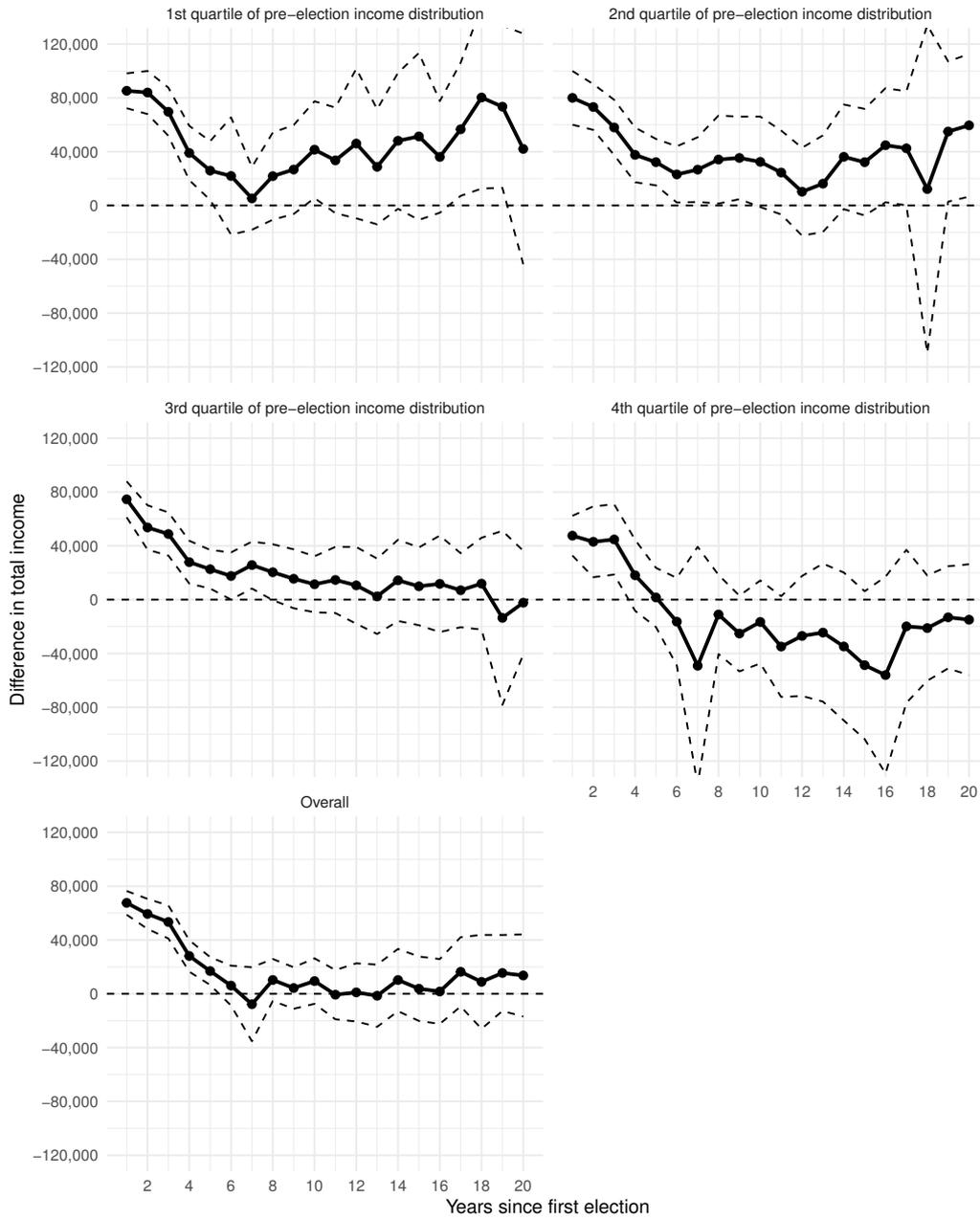
4.2 The distribution of long-term returns to office

In order to study the returns to office over time, we plot the average income difference between winners and losers for up to 20 years after their first run for office. Figure 4 shows the annual returns to office across the pre-office income distribution. To analyze whether long-term returns are different conditional on pre-office income, we also plot the returns over time for each quartile of the income distribution.²¹

Figure 4 shows how returns to office are strong on average in the first years after the election, but drop steadily until they become insignificantly different from zero after six years. Several explanations may apply. Firstly, some of the losing candidates might rerun and become elected later, which will give them a future short-term income increase. Secondly, some of the winning candidates will either not rerun or not get reelected, which might bring their income back on level with the initially losing candidates. For our sample, the average political career length is 2.6 terms for winners and 0.4 for losers, and in Appendix H Figure H.1 shows the number of incumbents in future elections between these losers and winners. Thirdly, while losing candidates forego the chance to serve in parliament, they might excel in other careers

²¹Because we have elections from 1994 to 2015, but only income up until 2018, when we study returns to office in the future we progressively drop observations from more recent elections.

Figure 4: Long-term returns to office



Note: Difference in total income between winners (=1) and losers since their first race across quartiles of pre-election income distribution. Standard errors clustered at party-constituency-year level. 95% pointwise confidence intervals.

with a steeper long-term income gradient than parliamentary careers, meaning that they could catch up.

Figure 4 also shows that the distributional returns to office are very different over time. For candidates from the first, second, and third quarters of the pre-income distribution, the returns to office seem to persist up to about six years after they first stand for election. This means that marginal winners from the lowest three quarters of the pre-income distribution have substantially larger cumulative returns to office than revealed by the estimates of short-term returns. For candidates from the fourth pre-income quartile the long-term effects are smaller in the first years and already in the fourth year they are indistinguishable from zero; probably because of good outside options. Note that the point estimate for the fourth quarter even becomes negative (albeit insignificant) in year 6 after first election and onwards up to year 20.

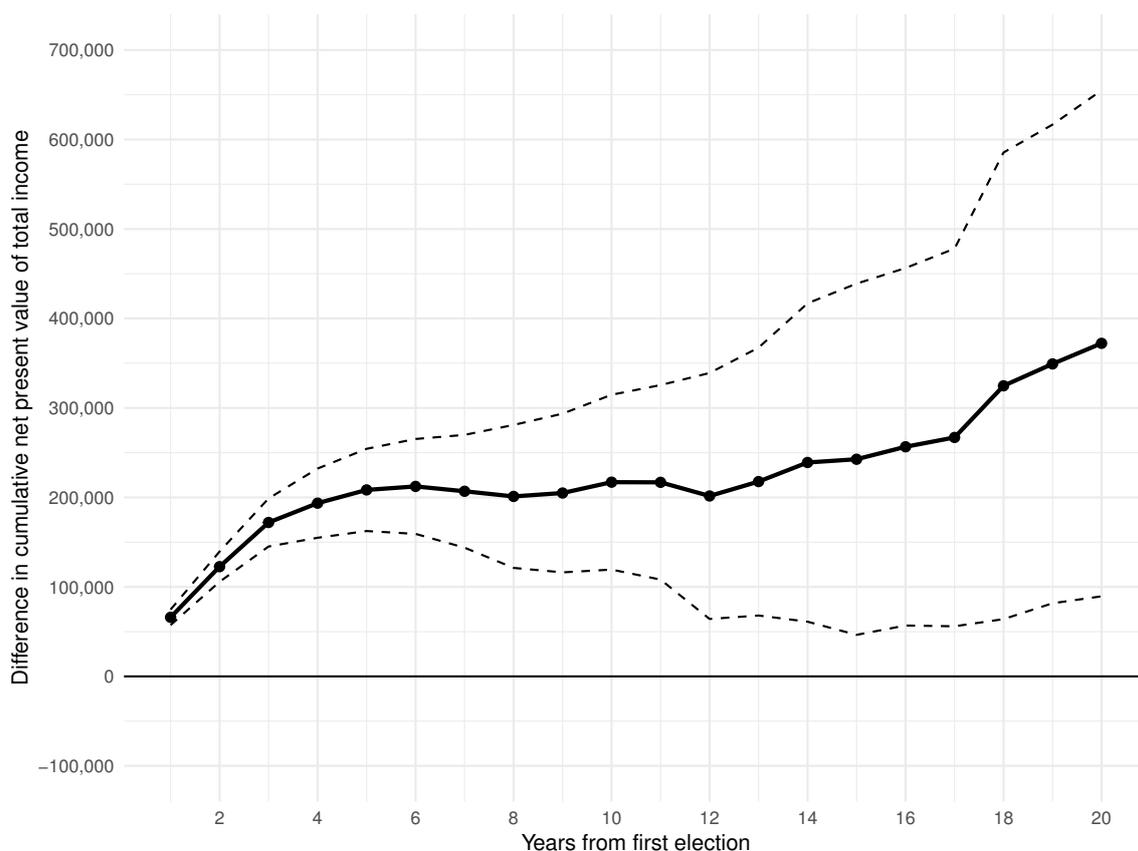
4.3 Towards life-cycle returns to office

Next, we compute the net present value of future returns to winning office. As we do not have data spanning, say, 40 years, we cannot fully claim to estimate life-cycle returns to office. Still, we come quite close. The Net Present Value (NPV) expresses the cumulative returns to office for candidates since the first year they ran for office ($t = 0$). Essentially, we sum over the income differences between winners and losers for all years following the election. Because future earnings are considered less valuable than immediate returns, future earnings are discounted. The NPV gives the value of winning office expressed as the total value of future gains or losses due to being elected. We calculate the NPV for each first-time running candidate, i , from year $t \in [1; 20]$ up until the income most recently measured, T :

$$NPV_{it} = \sum_{t=1}^T \frac{Y_{it}}{(1+r)^t}$$

Since we can track some candidates' income for 20 years and others for only four years after they become elected, T varies individually. We calculate the NPV for the total income, Y , including pensions, and we discount the value of future earnings by an interest rate, r , of 2.58% as this rate was used by the aforementioned commission on politicians' salaries in Denmark (Vederlagskommissionen, 2016).²²

Figure 5: The estimated average NPV of returns to office.



Note: This is the cumulative difference in income over time between winners (=1) and losers since their first race. Future earnings are discounted by 2.58% to account for future earnings being less valuable than current earnings. 95% pointwise confidence intervals. Standard errors are clustered at party-constituency-year level.

²²To check the sensitivity of the estimates, we also used a lower interest rate of 1.24% in Appendix I. This alternative interest rate was also used by the recent commission. As the interest rate is lower, future returns have a higher present value, i.e., a higher level of cumulative returns to office.

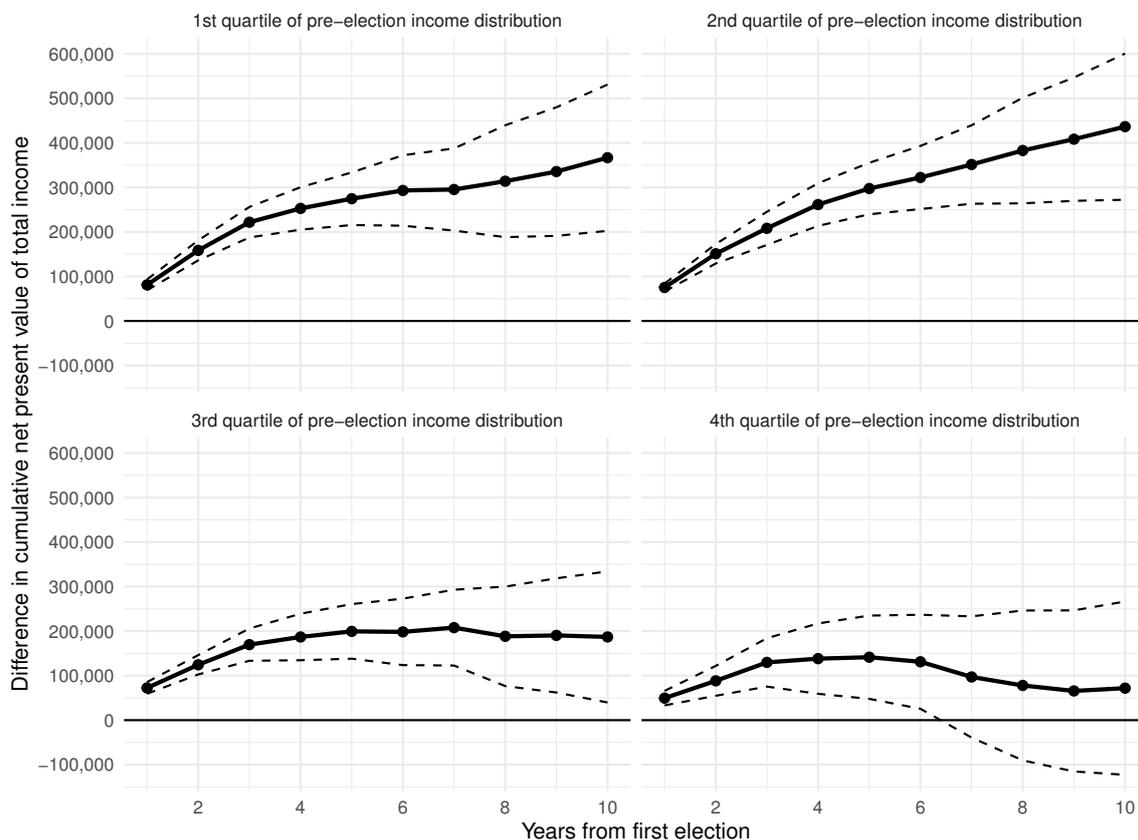
Figure 5 shows the average NPV computed for each year since election. Generally, the estimated life-cycle returns, as we may call them, increase over years since the first election, meaning that, on average, winners tend to experience higher accumulated income each year after their first election as a direct causal outcome of being elected. The cumulative returns to office increase mostly in the first years following election; the years where candidates were shown to have clear returns to office according to Figure 4. Holding office is cumulatively beneficial throughout all 20 post-election years, but the variation around the average estimate increases. The decreasing precision over time is to a large extent driven by the fact that we observe fewer observations, the longer the time span we consider. Given the chosen discount rate, the cumulative returns from holding office 20 years after running for the first time is around €372,000 (95% CI around [89,500; 655,000]) when pensions are included.²³

Next, we investigate the distribution of life-cycle effects and look at the NPV for each quarter of the pre-election income distribution. Figure 6 shows the difference in NPV between winners and losers of the pre-office income quarters. This plot essentially shows the NPV of returns to office conditional on earnings before entering politics. For candidates placed in the first and second quartile of the pre-office income distribution, the NPV of returns remains increasing, although by a falling rate. For candidates placed in the third quartile of the pre-office income distribution, the NPV becomes flat and even decreases a little after approximately four years. The confidence intervals become wider with years after election, both manifesting some divergence between MPs' post-election income streams and showing that the number of observations are smaller after 10 years than when considering the NPV after fewer years. For the 4th quartile, i.e., the 25% highest earners pre-office, the NPV of returns becomes insignificant after seven years and the point estimate goes towards zero. In

²³The difference, in NPV, estimated for income without pension can be seen in Appendix I

Appendix I, we show the NPV of long-term returns conditional on how old MPs were when they first run for office.

Figure 6: The estimated life-cycle returns to office across quartiles of the pre-election income distribution.



Note: This is the cumulative difference in income over time between winners (=1) and losers since their first race conditioning on their pre-election quarter of the income distribution. Future earnings are discounted by 2.58% to account for future earnings being less valuable than current earnings. 95% pointwise confidence intervals. Standard errors are clustered at party-constituency-year level.

5 Discussion and Conclusion

Are political careers economically attractive? This was the question we set out to answer in this paper. Intuitively, one would imagine that while most might receive positive returns some unknown share of MPs could have earned more had they not

entered politics. In this paper, we first demonstrate substantial and comparatively large returns to winning office among Danish MPs. We then take important steps beyond estimating short-term average returns to office. We compute unconditional quantile returns over the entire distribution as well as the net present value of returns since first election. These extensions bring about a more sophisticated understanding of MPs' total returns to office and they contribute fundamentally to a more refined and precise answer to the opening question.

We find positive average returns during the first term in office corresponding to an income increase of 112%. The unconditional quantile difference-in-differences estimates reveal that across the entire distribution of pre-office income trajectories, the returns to office are positive in the short-term. Compared to previous studies from countries that are in many respects similar to Denmark, we find substantially larger returns to office (Kotakorpi, Poutvaara and Terviö, 2017; Berg, 2020; Cirone, Cox and Fiva, 2020). The differences are partly explained by variations in how we measure income.

Berg (2020) rely on disposable income and consider pensions as payouts later in life instead of immediate income. As described above, she finds short-term returns of approximately 20%. In Appendix I, we also consider short-term effects on disposable income only and find a 69% increase over the baseline, i.e. considerably lower than the 112% although still roughly three times the size of Berg (2020). Kotakorpi, Poutvaara and Terviö (2017) find returns to office among Finnish MPs around €20,000. While they do include pensions, they exclude a tax exempt supplement, which is worth at least €11,000 and is similar to the one Danish MPs receive. This amounts to around 22% of the difference between their estimate and ours. Cirone, Cox and Fiva (2020) find returns of around €15,000 for private income only, which does not include pensions.

A large part of the difference between our short-run estimates of returns to office vis-à-vis those found in the recent literature therefore arise due to our choice of adapting a broad income definition. Yet, a considerable difference remain, which suggest that short-run average returns are larger in Denmark than in the other Nordic countries. Voters may be less skeptical or less knowledgeable towards more subtle income streams such as pensions, and politicians may therefore have a preference for other types of income. This highlights the importance of including all income streams, as we do in our paper.

For prospective candidates, their expected income when losing compared to winning determines their expectation of net returns to winning office. Arguably, these individual forecasts of future income streams are more accurate for their first term in office than the estimates of income streams further into the future. Looking at long-term annual returns to office, we find that the point estimates of annual returns become insignificant already after six years. They come close to zero after four years for MPs who belonged to the third quarter of the pre-office income distribution and also insignificant for MPs who belonged in the lowest quarter of the pre-office income distribution already after six years. For MPs who belonged to the top quartile of the pre-office income distribution, the annual returns to office become insignificant in year four, and the point estimates become negative in year six.

The annual returns manifest themselves in accumulated form in the “life-cycle” returns to office. For each year after the candidates’ first run and until 20 years into the future, we compute their cumulative returns based on the net present value of all income from year of first election and until the last year in the computation. Even when discounting future returns to office, we find considerable cumulative returns to winning office. On average, these accumulate to €372,000 over 20 years. However, when we estimate the cumulative NPV for the first 10 years over the prior income

distribution, we again find considerable heterogeneity. The NPV is largest for the lowest pre-office quartiles of the income distribution, and for the top quartile of the pre-office income distribution they become insignificant after seven years. Given the pattern of the annual returns, it comes as no surprise that the top quartile of the pre-office income distribution becomes insignificant.

In general, a large majority of the population thinks that politicians are paid too much and they are against increasing their salary even more (Pedersen and Pedersen, 2020; Pedersen, Hansen and Pedersen, 2022). However, the combination of high short-term returns to office across the income distribution and the fact that, over time, the cumulative returns to office are not universally positive underscores the importance of taking a nuanced and detailed view on the returns to office. The answer to our opening question, whether political careers are economically attractive, depends on whether one looks at the long or short run and whether one considers the average of the full income distribution. The NPV remains positive or insignificantly different from zero even for those who gain the least, but for the top quarter of elected MPs there is actually no economic reward or punishment when measured in terms of life-cycle returns to office.

Our approach to identification of causal returns is yet another adaptation of the approach first suggested by (Eggers and Hainmueller, 2009), where identification of returns to office hinges on the comparability of narrowly winning and losing parliamentary candidates. While this identification strategy is ingenious, and also in our setting appears useful and strong, it does not identify the average return or distribution of returns to office for any person. This approach to identification relies on the persons who run for office and there is not necessarily common support on the entire wage distribution between candidates and the general population from which the candidates are ideally selected from (Imbens and Wooldridge, 2009).

Notably, we may lack common support in the very top end of the wage distribution among those who never run for office. Taken at face value, the fact that all groups of candidates gain economically during their first term may suggest that income is of at least some importance to those who run. And maybe more so for those who chose *not* to run. An implication of this may also be that we have missed some high earning candidates who simply never run, because economically it is unattractive. This might limit the type of candidates among which voters can choose and in turn the politicians elected for Parliament. In future research, we plan to address this important question.

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A Danish national elections

The Danish national parliament has one chamber, Folketinget. It has 179 members, of which 175 are elected in the Danish principal country, while two are elected in Greenland and two on the Faroe Islands. We only have election data on candidates in Denmark and administrative data for people residing in Denmark, so we study only those who run for a seat in the Danish principal country.

The electoral system is a two-tier system, as seat allocation takes place at two levels. The initial allocation takes place in lower-tier of the 10 multi-member constituencies, *storkredse*, where 135 seats are allocated among the competing parties as constituency seats, *kredsmandater*, based on the party votes using the D'Hondt method. The remaining 40 seats are allocated in the upper-tier as compensatory seats (*tillægsmandater*) based on the party votes in the three electoral provinces, *landsdele*, each of which consist of numerous electoral districts (Elklit, Pade and Nyholm Miller, 2011). Before an election, it is determined how many of the 135 constituency seats and 40 compensatory seats each electoral district will have.^{A.1}

As a voter you can vote either for a party or personally for a single political candidate, where the latter implies voting for the party that the candidate represent when seats are allocated in the process described above. A personal vote will then – in parties with an open party list – help the candidate in the intra-party competition in winning one of the seats allocated to the party. In closed-list structures, parties prioritize their candidates, which pre-empts internal competition and essentially makes the personal votes worthless in terms of what candidates end up serving. However,

^{A.1}Every fifth year, the 135 constituency seats are distributed proportionally to the 10 multi-member electoral districts based on the sum of three separate numbers: first, population, second, number of registered voters in the latest general election, and, third, area in square kilometres multiplied by 20 (as a measure of population density). The multi-member constituency of the island of Bornholm in the Baltic Sea, however, is guaranteed at least two seats.

in Denmark the open-list structure has been prevalent in most of the parties for decades, and since the election in 2011 a closed-list structure has been used only by the Red-Green Alliance, a left-wing party that has used the structure since the party's establishment in 1989.^{A.2}

The Prime Minister in office decides when to call an election. However it must be held no later than four years after the previous election. As such, the timing of elections is not exogenously determined, and therefore the electoral periods vary in length. Our data cover the following national elections: 1990, 1994, 1998, 2001, 2005, 2007, 2011, 2015 and 2019.

^{A.2}Another left-wing party, the Socialists People's Party, used closed-list structures in some voting districts and open-list structures in others up to and including the election of 2007, and the right-wing nationalist party the Danish People's Party used closed-list structure in the election of 1998, which was the party's first election on the ballot.

B Measuring electoral closeness

An example: Party A runs three candidates in constituency X in election year Y:

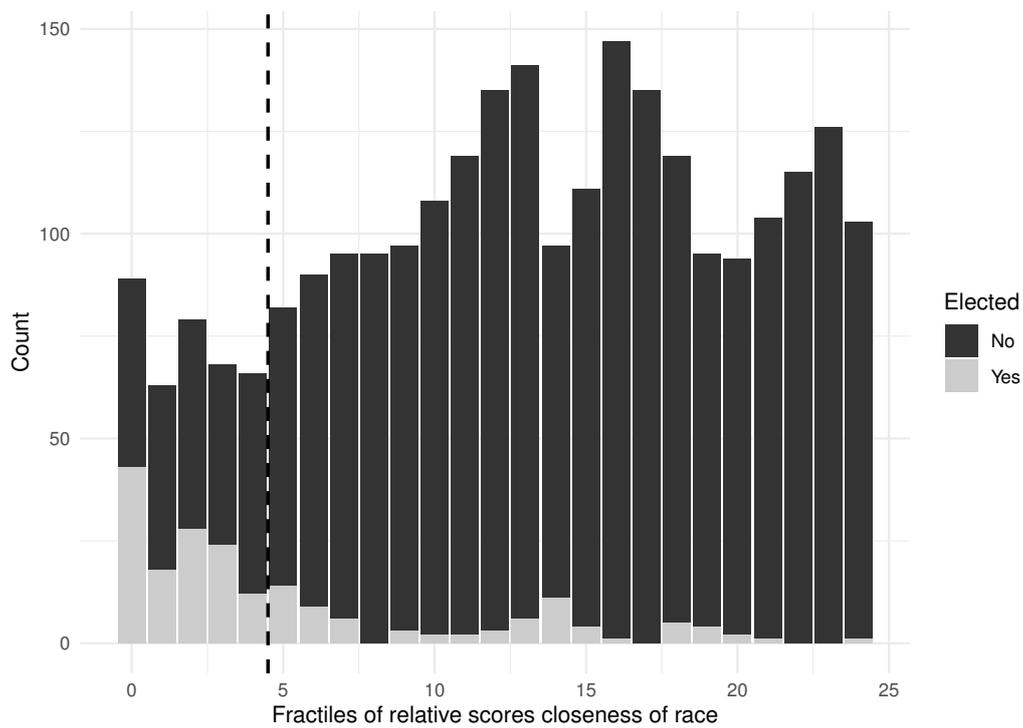
- Candidate 1 wins 1,000 votes
- Candidate 2 wins 550 votes
- Candidate 3 wins 450 votes

Assume that candidates 1 and 2 end up winning a seat. Then the party threshold is $\frac{550+450}{2} = 500$. In absolute votes the candidates distances to the threshold is 500 (candidate 1), 50 (candidate 2) and 50 (candidate 3). Their distances relative to the threshold is then 1, 0.1 and 0.1 respectively.

The distribution of relative scores in the candidate pool is a distribution of electoral closeness. The lower a candidate is placed in the distribution, the closer is the candidate to his/her party-constituency-year threshold. Thus, the lower the fractile in which a candidate is placed, in this distribution, the more narrowly did the candidate lose or win their seat.

Figure B.1 shows the distribution of electoral closeness for first time running candidates. The candidates to the left of the dashed line are first time running candidates who competed in the 20% most competitive elections and hence the candidates who comprise our sample. Note that the reason why the distribution contains so few "clear winners" is precisely because we look at first time runners who rarely win by a landslide.

Figure B.1: The distribution of fractiles of relative scores (closeness of race) for first-time running candidates from parties with open-lists.



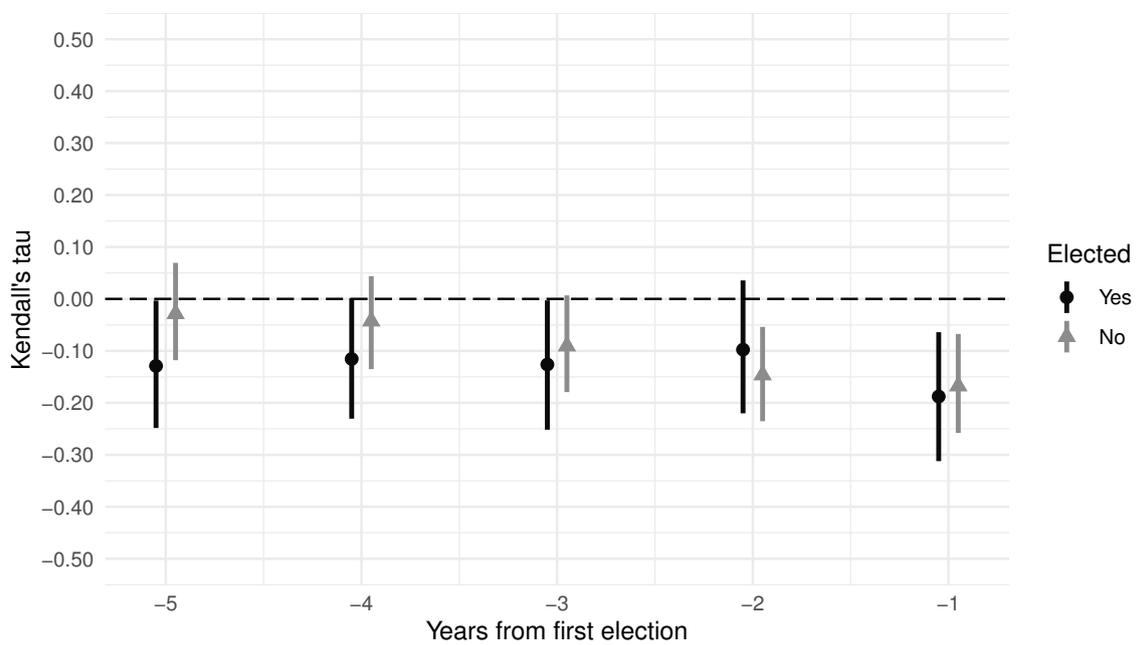
Note: The relative score is partitioned into 25 fractiles, indicating what twenty-fifth of electoral closeness a candidates' race was. The smaller the value, the more competitive was a candidate's election. Candidates to the left of the dashed line are included in our sample.

C Copula dtability assumption

To assess the copula stability assumption we use Kendall's τ to estimate the association between two variables of the candidates, 1) their lagged income, i.e., income in the previous year, $income_{i,t-1}$ and 2) their first difference income, i.e., income change from the previous year to the present year, $\Delta income_{i,t} = income_{i,t} - income_{i,t-1}$. This will express the association between candidates' placement in the income distribution and their income development. As Kendall's τ is used to compare ordinal variables, this measurement of association is not vulnerable to extreme observations. Because the two variables by definition require a past year to be created, we only perform estimates from year t-5 to t-1.

Figure C.1 plots the estimates of Kendall's τ for the change in income and initial placement in the income distribution five years preceding the election for the winning and losing candidates. The 95% pointwise confidence intervals are bootstrapped with 1000 iterations. Kendall's τ varies very little in the five years leading up to the election and is thus stable over the time period. This indicates evidence in favor of the QTT estimator's assumption of copula stability (Callaway and Li, 2019).

Figure C.1: Kendall's τ estimates for the change in income and initial placement in the income distribution for treated and untreated candidates by year before election.



Note: 95% pointwise confidence intervals are computed using the empirical bootstrap with 1000 iterations.

D Pre-election income

We show that our sample is balanced on a number of pre-treatment covariates in Table D.1. We find no significant pre-treatment imbalances.

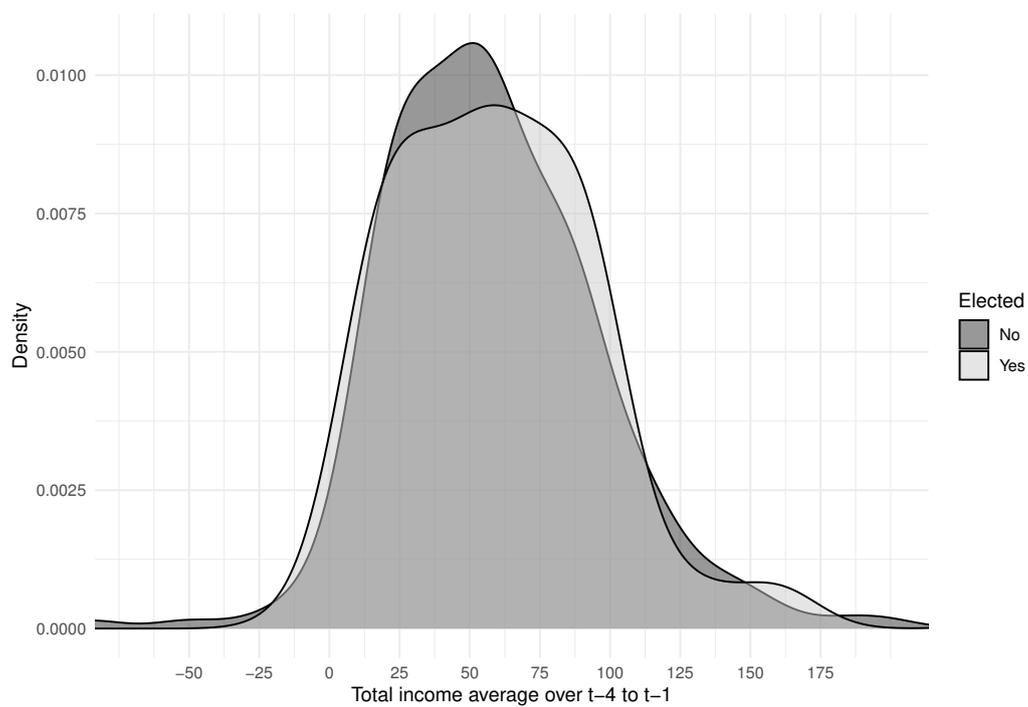
Table D.1: Balance test of pre-treatment variables

Variable	Losers	Winners	Difference	P(Diff \neq 0)
Female	0.392	0.400	0.008	0.878
Age	40.246	40.992	0.674	0.622
Age ²	1734	1846	112	0.330
Married _{$t-1$}	0.558	0.532	-0.026	0.638
Total income + pensions _{$t-1$}	63,028	62,550	-528	0.917
Average total income _{$t-4:t-1$}	62,840	60,579	-2,261	0.642
Mother's family's income %-tile _{$t-1$}	58.05	67.09	9.04	0.098
Right wing dummy	0.608	0.573	-0.035	0.514
Majority coalition dummy	0.500	0.524	0.024	0.663
Education dummies:				
Primary school _{$t-1$}	0,125	0,121	-0.004	0.912
High school _{$t-1$}	0.213	0.161	-0.052	0.229
Vocational education _{$t-1$}	0.188	0.185	-0.003	0.963
Medium-long education _{$t-1$}	0.258	0.306	0.048	0.340
Long education _{$t-1$}	0,217	0,226	0,009	0.843

Note: Difference between pre-treatment variables for our sample of marginally elected first-time runners from open-list parties from 1994-2015. Prices in € in 2015-prices.

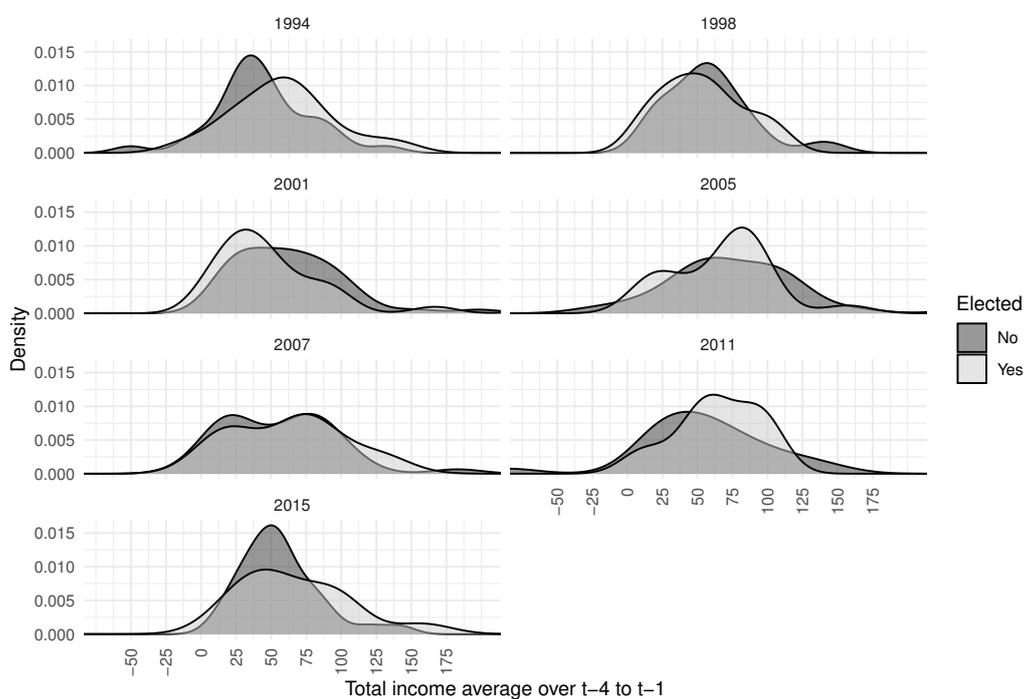
Figure D.1 and D.2 shows the average yearly total income for the candidates comprising our sample, i.e., the first time running candidates who marginally win or lose. The plots shows that in the four years before entering politics marginal winners and losers have similar incomes, on average.

Figure D.1: Distribution of total annual income averaged over the four years preceding the marginal candidates' first run.



Note: Based on our sample of first-time runners from open-list parties who marginally won or lost. X-axis in 1000 € in 2015-prices. Dashed lines indicate group means.

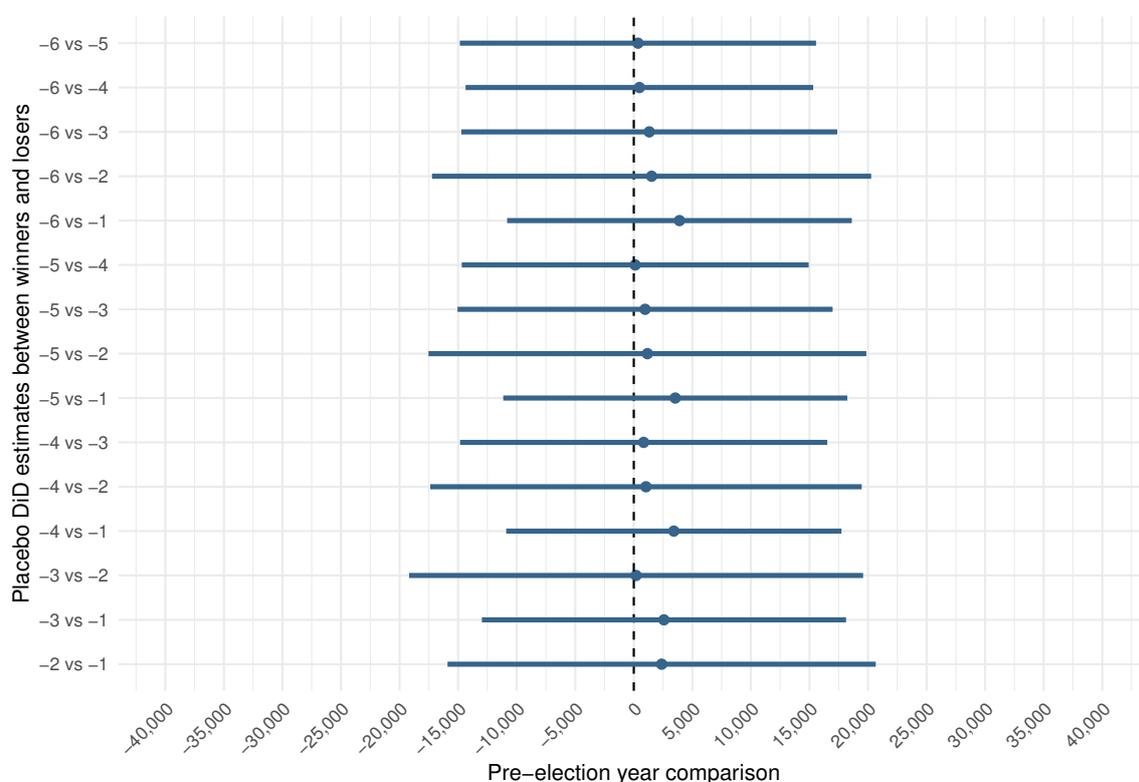
Figure D.2: Election-wise distribution of total annual income averaged over the for years preceding the marginal candidates' first run.



Note: Based on our sample of first-time runners from open-list parties who marginally won or lost. X-axis in 1000 € in 2015-prices.

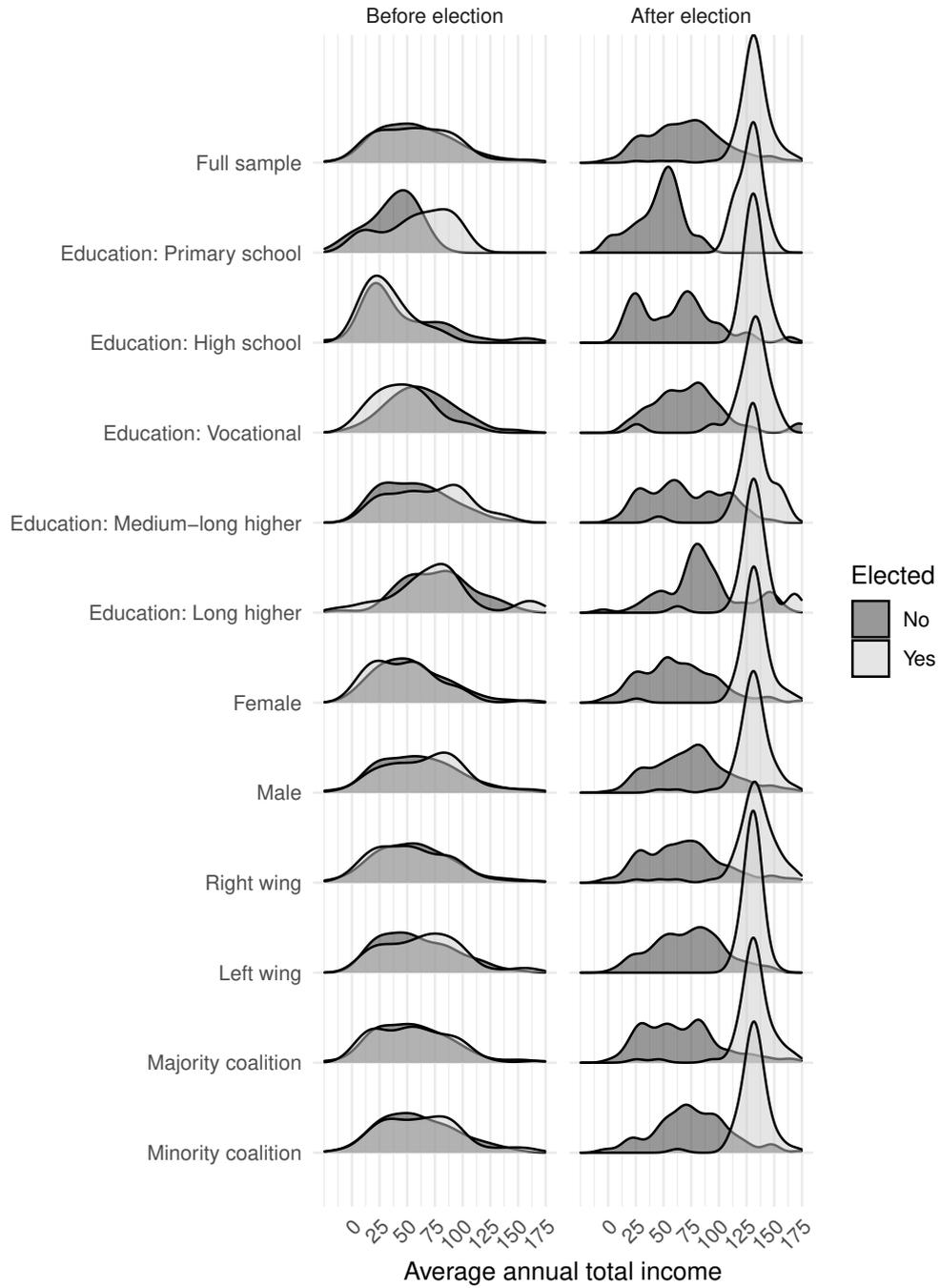
In Figure D.3, we test differences in income for future winners and losers between any two given years in the six years leading up to the election. This essentially mimics the situation if the election of winners had taken place in any pre-election year and compares it to any prior pre-election year. The changes between winners and losers are inseparable for all years, which is strong evidence in favour of the parallel trends assumption, even without imposing restrictions on the functional form of the trends.

Figure D.3: Placebo test of pre-treatment income.



Note: Year-wise comparison of losers and winners total annual income in six years preceding first run. This test parallel trends in income of winners and losers in pre-election years with no assumptions of the trends' functional form. Income in € in 2015-prices. 95% confidence intervals. Standard errors clustered at party-constituency-year level.

Figure D.4: Income distributions for winning and losing candidates before and after the first election for different subsets of candidates.



Note: Income distributions for our sample of marginally winning and losing first-time runners from open-list parties. Distributions made from candidates average annual total income in four years preceding their first run (before election) and two years following their first run (after election). X-axis in 1000 € in 2015-prices.

E The short-term returns to office

Alternative outcome: Income excluding pension

Some might argue that we should estimate returns to office without pensions because they are future windfall. They also come with some uncertainty since one is not guaranteed a long retirement to enjoy the returns from a beneficial pension scheme. Therefore, we also estimate the returns to office without employers' pension contributions in Table E.1. The DiD estimates in Table E.1 express the immediate returns to office measured by the average returns to office *excluding* pension contributions over the first two years after the election year. Standard errors are clustered at party-constituency-year level.

When we estimate returns to office this way, they are €43,771 with a 95% CI of [35,718; 51,823], which corresponds to 74% of the winning candidates' pre-office income excluding their pension contributions. We also estimate these returns with and without fixed effects for party-constituency-year, and the results are very similar regardless of specification.

Alternative outcome: Disposable income

We also estimate the short-term returns in disposable income, that is after taxes are subtracted and without pension. We present these results in Table E.2, where the DiD estimates express the immediate returns to office measured by disposable income, that is after total taxes are subtracted and *excluding* pension contributions over the first two years after the election year. Standard errors are clustered at party-constituency-year level.

Table E.1: Short-term Average Returns to Office: Excluding Pensions

	Total Income (2015 €)	
	(1)	(2)
Elected × Post-election (DiD)	43,771*** (4,106)	42,968*** (4,070)
Post Election	7,664*** (2,762)	9,821*** (2,677)
Elected	−643 (4,687)	2,997 (6,714)
Constant	60,098*** (3,199)	30,549*** (6,366)
Fixed effects for cluster?	NO	YES
Candidate-years	2,176	2,176
Candidates	365	365

Note: The DiD estimate expresses the immediate returns to office measured as annual total income *excluding* pension contributions in the four years prior to and two years after the candidates' first run. Estimated without (1) and with (2) fixed effects for clusters of party-constituency-year ($k=231$). Standard errors are clustered at the party-constituency-year level in both models. *, **, and *** indicate $p < 0.1$, < 0.05 , and < 0.01 , respectively.

Table E.2: Short-term Average Returns to Office: Disposable Income

	Disposable Income (2015 euro)	
	(1)	(2)
Elected \times Post Election (DiD)	21,140*** (3,182)	20,985*** (3,418)
Post Election	5,791*** (1,678)	6,805*** (1,721)
Elected	-3,144 (3,235)	-232 (3,780)
Constant	33,691*** (1,704)	21,432*** (3,512)
Fixed effects for cluster?	NO	YES
Candidate-years	2,176	2,176
Candidates	365	365

Note: The DiD estimate expresses the immediate returns to office measured as annual total income minus total taxes *excluding* pension contributions in the four years prior to and two years after the candidates' first run. Estimated without (1) and with (2) fixed effects for clusters of party-constituency-year ($k=231$). Standard errors are clustered at the party-constituency-year level in both models. *, **, and *** indicate $p < 0.1$, < 0.05 , and < 0.01 , respectively.

When measured this way, the returns to office are €21,140 with a 95% CI of [14,900; 27,379]. These results too are robust to using fixed effects. On the face of it, the effect on the disposable income seems small compared to the effects on gross income with and without pension. However, the baseline is also considerably smaller, and the increase corresponds to a 69% increase in disposable income.

Perks and tax free income

In this section, we want to check whether politicians receive or lose others sources of perks and non-taxable incomes. If they do, we should include this in our estimation to get a full picture of the returns to office. In Table E.3, we estimate the taxation value of perks and tax exempt income. We have data on perks going back to 1993 only, which means that we have only one pre-election year for candidates running in the election of 1994 compared to four pre-election years for all later candidates. We use the same strategy to estimate the DiD with four pre-election years and two post-election years, excluding the year of election and clustering standard errors at party-constituency-year level. Again, outcomes are estimated with and without fixed effects for party, constituency, and year. The DiD estimates reveal no substantial effects of winning office on perks or tax-exempted income. This confirms that politicians are not rewarded with other perks or non-taxable incomes.

Table E.3: Tax-exempt allowances not covered by administrative measurements of perks and tax-free income

	<i>Dependent variable:</i>			
	Perks (2015 €)		Tax-free Income (2015 €)	
	(1)	(2)	(3)	(4)
Elected × Post Election (DiD)	−129 (290)	−203 (298)	105 (91)	43 (96)
Post Election	−74 (179)	51 (177)	−174** (68)	−156** (68)
Elected	−125 (436)	−232 (1,036)	−382** (176)	−287 (277)
Constant	979*** (313)	569 (1,000)	841*** (129)	325 (275)
Fixed effects for cluster?	NO	YES	NO	YES
Candidate-years	2,026	2,026	2,176	2,176
Candidates	365	365	365	365

Note: The DiD estimates show that winning office does not change the administrative measurements of candidates' perks or tax-free income, when comparing the winners and losers four years prior to and two years after their first run. This holds with and without fixed effects for clusters of party-constituency-year ($k=231$). For every model, standard errors are clustered at the party-constituency-year level. *, **, and *** indicate $p < 0.1$, < 0.05 , and < 0.01 , respectively.

F Effect heterogeneity by covariates

In this section, we will look at heterogeneous returns to office by age, gender, education level, and political party. Figure F.1 presents DiD estimates of various subsets of our data. The red dashed line indicates our overall DiD estimate from Table 2 of €69,277.

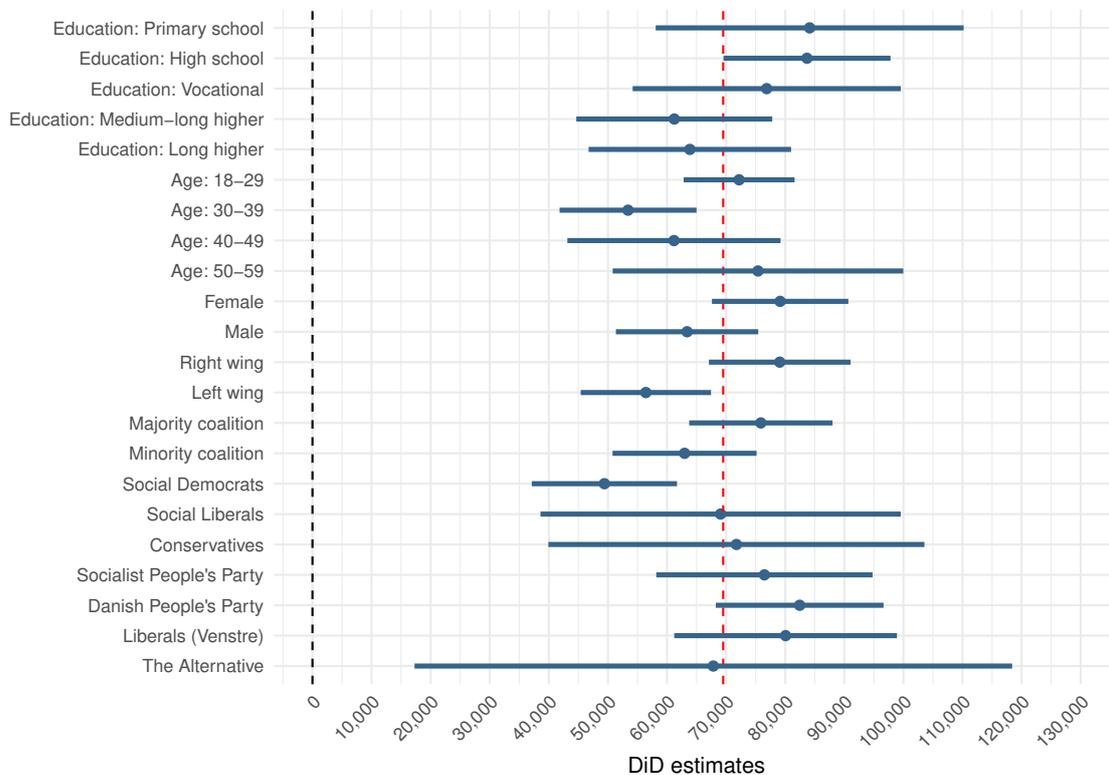
The figure shows clear differences in effect size across subgroups, although all estimates are positive and both substantially and statistically significant. The returns to office are smaller for those with more education, but the estimates for the different groups are not statistically different from each other. For those elected at a very young age, the immediate returns are largest, while they are smallest for those in their thirties.

Looking at the political variables, the average returns to office tend to be higher for right wing politicians compared to left wing politicians. The right wing parties are: Conservatives, Liberal Alliance, Danish People's Party, Liberals (Venstre), and the Progress Party. Majority/minority coalition expresses whether a candidate ran for a party that was part of the political majority coalition in the election period following the election. Denmark has negative parliamentarism with a tradition of minority governments. We define the majority coalition as all parties in government and the parties supporting the government. There is no clear difference over this dimension.

When we break down the returns to office by political party, the party that stands out the most is the Social Democrats, whose candidates experience returns that are smaller than the average, although with estimated returns above €45,000 per year they are still substantial and larger than previous studies of returns to office. Returns

to office for the Liberals and the Danish People’s Party also seem to be slightly larger than the average returns to office.

Figure F.1: DiD estimates across sample covariates

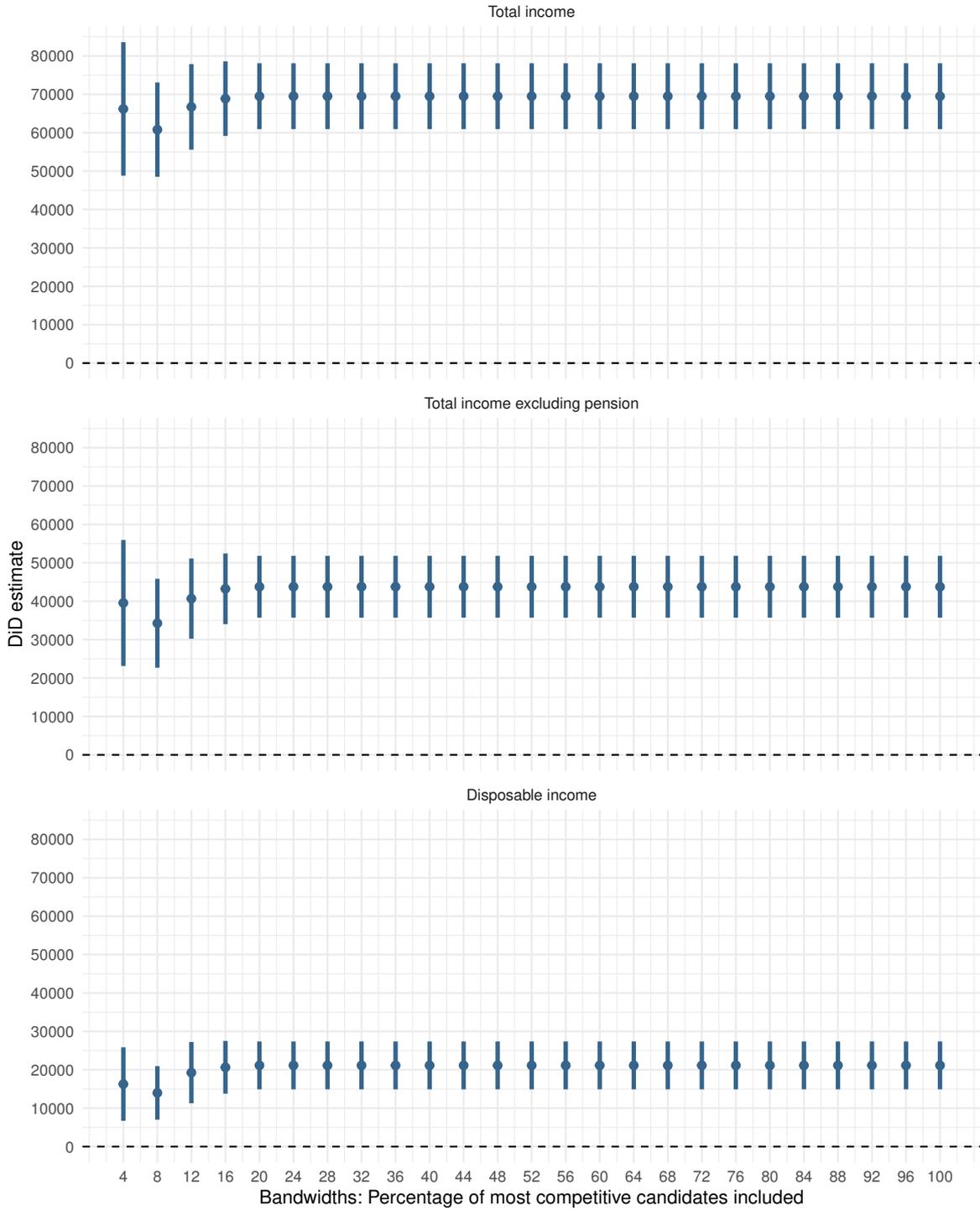


Note: Standard errors clustered at the electorate-party-election year level. The red dashed line indicates the average returns to office of 69,505.

G Robustness to bandwidth selection

To test for the sensitivity of our results, we show two plots with different bandwidth value restrictions. As described in the research design, we subset to the candidates in the 20% closest elections. In Figure G.1, we assess the robustness of our choice of close election, by estimating the average returns in every bandwidth available to us. We start in a 4% bandwidth and progress in 4% increments. The standard errors are clustered at the level of treatment assignment in clusters of electorate-party-election year. The results show that our results are completely robust to other choices of bandwidth. This robustness also hold when adding fixed effects at the level of treatment assignment which is constituency-party-election year.

Figure G.1: DiD estimates over increasing bandwidths.

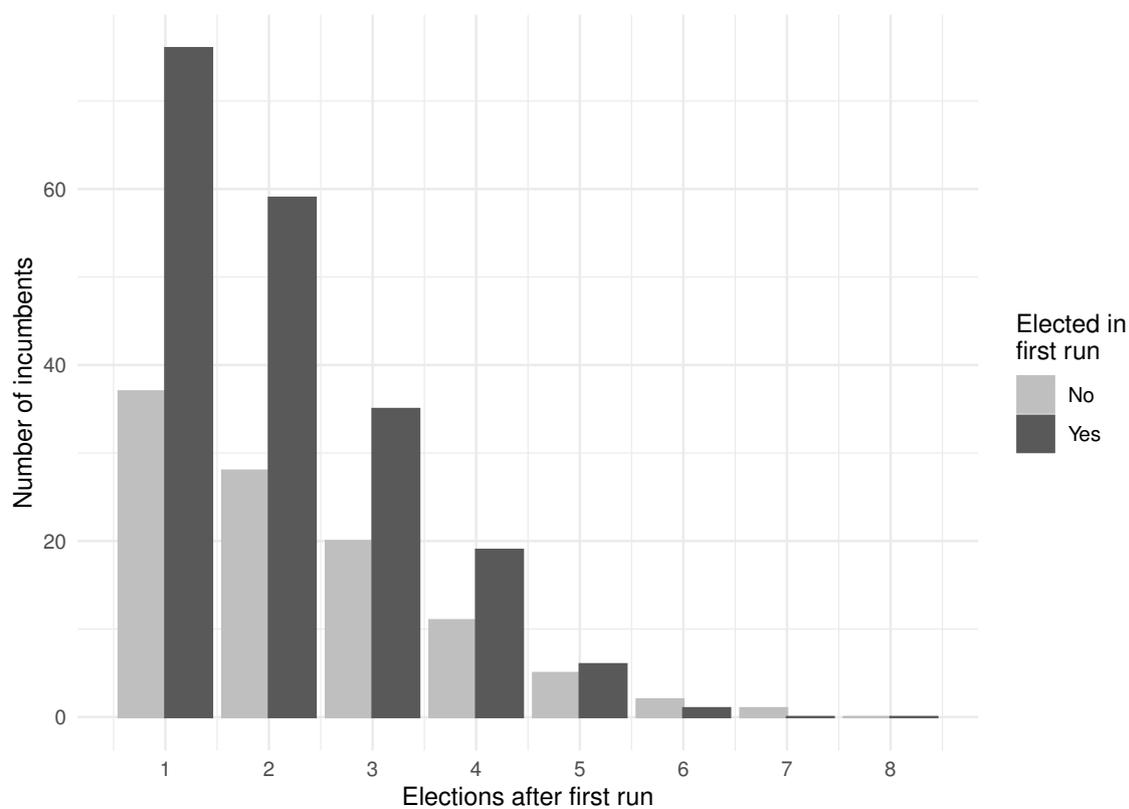


Note: Standard errors clustered at the electorate-party-election year level.

H Future incumbency of winners and losers

In Figure H.1, we follow winning and losing candidates to see whether they are elected in future elections. We see a lasting effect of marginally winning a seat. In the next four elections, there is a higher share of initially winning MPs that win a seat again compared to initially losing candidates.

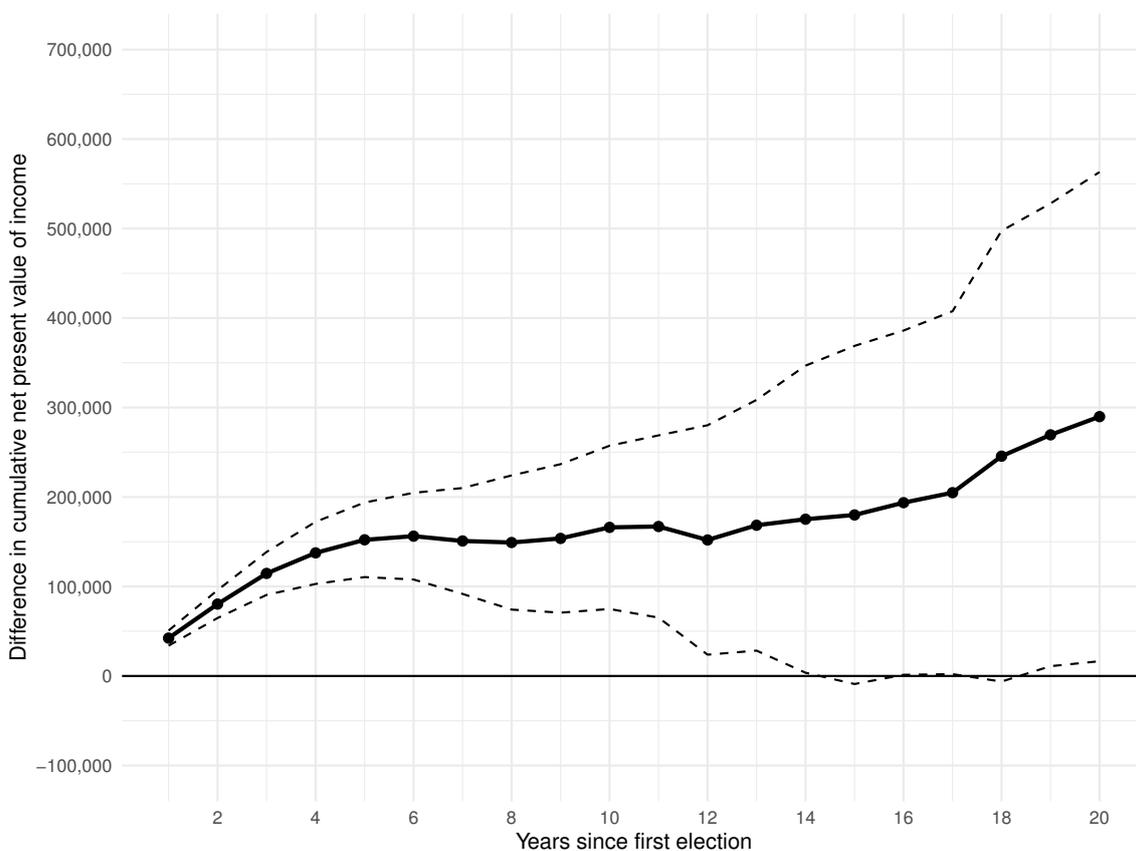
Figure H.1: Number of incumbents in future elections among winners and losers in our sample.



I Net Present Value

In the paper, we plot the net present value (NPV) of future earnings including pensions and with a 2.58% discount rate. Here we show what the NPV of future earnings are under different assumptions. First, Figure I.1 plots the estimated differences in NPV for winners' and losers' income, excluding pension and accounted by an interest rate of 2.58% and standard errors clustered at constituency-party-year level.

Figure I.1: The estimated life-cycle returns to office when excluding pension from income. NPV discounted by 2.58%.



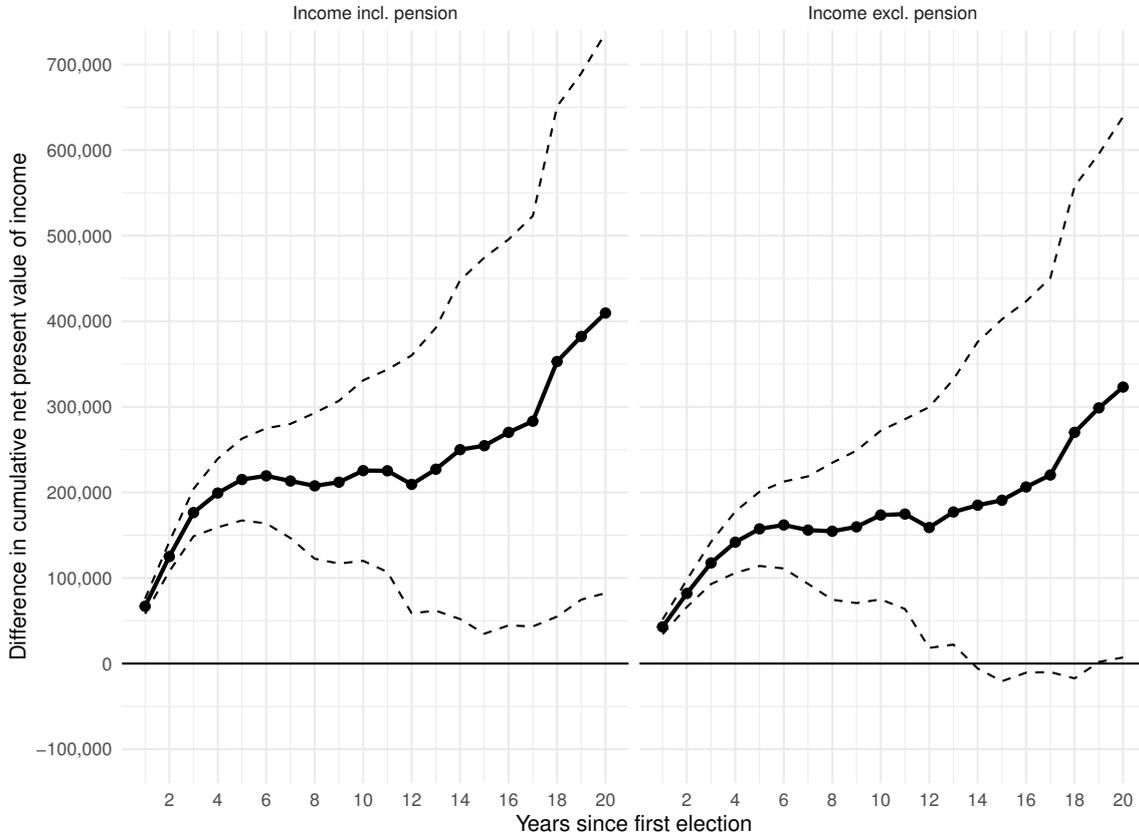
Note: The estimated life-cycle returns to office. This is the cumulative difference in income over time between winners and losers since their first race. Future earnings are explicitly discounted by 2.58% to account for future earnings being less valuable than current earnings. Income measured as total income excluding pension. Standard errors are clustered at party-constituency-year level.

We picked the 2.58% discount rate because it was proposed by the expert commission, *Vederlagskommissionen*. In their report, they also proposed a less conservative discount rate of 1.24%. Figure I.2 plots the estimated differences in NPV for winners' and losers' income with and without including pension accounted by an interest rate of 1.24%. Once again, standard errors are clustered at party-constituency-year level. When discounted by an interest rate of 1.24%, the cumulative returns from holding office 20 years after running for the first time is around €410,000 (95% CI around [82,000; 737,000]) when pensions are included, and €323,000 (95% CI around [7,000; 639,000]) when pensions are not included. These estimates are in line with the estimates of the interest rate of 2.58, but this alternative (and more liberal) interest rate of 1.24 provides estimated NPVs that are larger and slightly more volatile. Altogether, estimates of both interest rates suggest substantial life-cycle returns to office.

Finally, we look at the NPV across different age cohorts, concretely which third (tertile) of the age distribution of the candidate pool the candidate was part of when running for office. The first tertile consists of candidates aged 18-33, the second tertile consists of 33-46 year-olds, and the third consists of candidates aged 46 or older when they ran for office for the first time. We might see different long-term returns to office conditional on where candidates are in their career. Take the youngest candidates. On the one hand, they may be most likely to use a parliamentary career as a stepping stone to become a cabinet minister or obtain a lucrative outside occupation. On the other hand, they are at a point in their career where progression is steep so they might forego outside opportunities by serving in office.

Figure I.3 shows the estimated cumulative effect for each age tertile discounted by our interest rate of 2.58 among winners and losers up to 10 years after they competed for office for the first time. Over ten years, winning office is on average

Figure I.2: The estimated life-cycle returns to office when NPV is discounted by 1.24%.

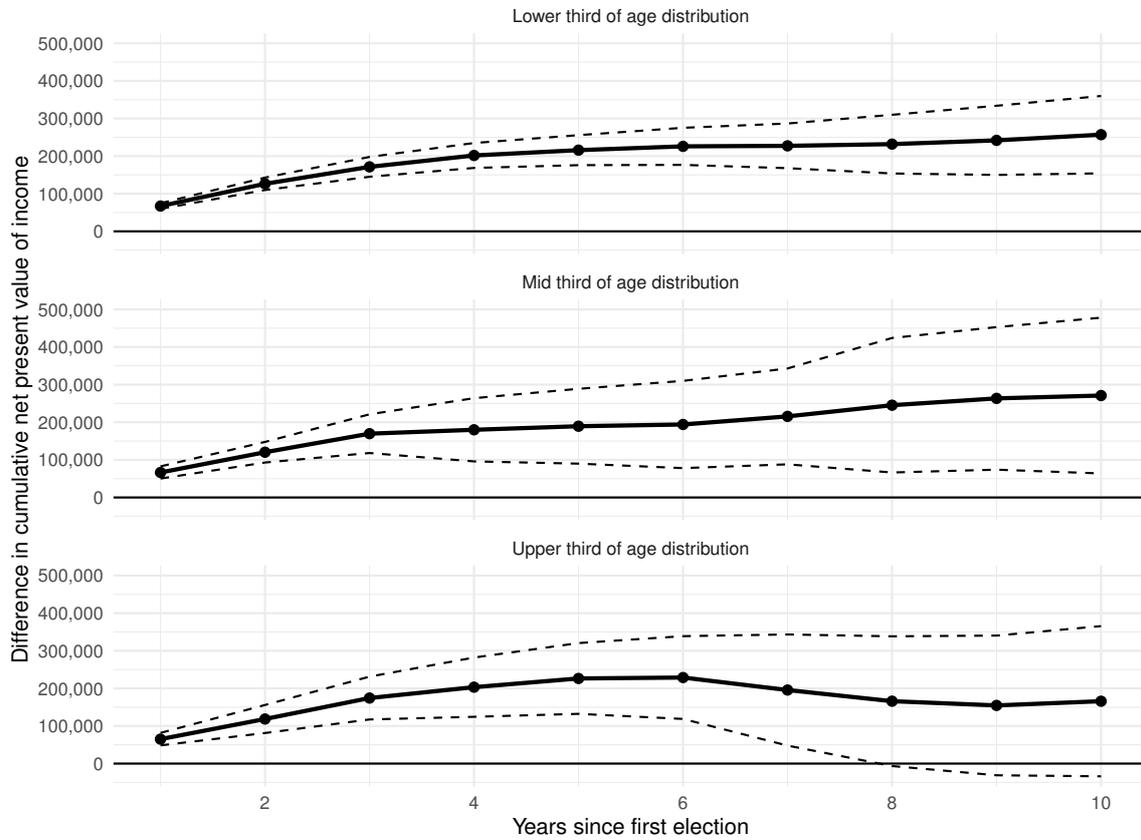


Note: The estimated life-cycle returns to office. This is the cumulative difference in income over time between winners and losers since their first race. Future earnings are explicitly discounted by 1.24% to account for future earnings being less valuable than current earnings. Income measured as total income including or excluding pension. Standard errors are clustered at party-constituency-year level.

about equally attractive for the first and second age tertile, with point estimates around €260,000. For the oldest candidates, the point estimate is around €165,000 and the 95% confidence interval includes zero. The returns to office, however, are not statistically significantly smaller than for the two other age groups^{I.1}. Across all age groups, winning office is never economically unattractive for the candidates. And for the two younger age groups, for whom winning office must be weighted against their

^{I.1}The estimates for the three age tertiles in year 10 are: Tertile 1: 257,015 (95%-CI [154,183; 359,847]), Tertile 2: 270,953 (95%-CI [638,74; 478,031]), and Tertile 3: 165,840 (95%-CI [-33,847; 365,526])

Figure I.3: The estimated life-cycle returns to office across age tertiles.



Note: The cumulative difference in net present value of total income over time between winners and losers in each third of the age distribution since their first race. Future earnings are discounted by 2.58% to account for future earnings being less valuable than current earnings. Standard errors are clustered at party-constituency-year level.

alternative careers to a higher extent than the oldest candidates, the returns seem most secure.