

## Return to Political Power in a Low Corruption Environment

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First draft: August 1, 2014  
This version: September 22, 2015

We use exogenous changes in the size of local municipalities in Denmark to estimate the effect of political power on the income of politicians and their family members. We exploit two dimensions of political power: heterogeneity in politicians' roles within a given district, and exogenous increases in political power as proxied by population and budget size. Our difference-in-differences results indicate that an increase in political power has: 1) an economically small but statistically significant effect on the income of re-elected politicians; 2) an economically larger effect on income of influential politicians such as coalition party leaders and mayors; and 3) an economically large effect on politicians' offspring. We estimate a positive and significant elasticity of income to political power, which spans from 3 percent for re-elected candidates to 14 percent for mayors. To control for differential changes in electoral competition, we instrument the likelihood of re-election with average party votes in other municipalities and in national elections. We conclude that, even in a low-corruption environment, there is an economically relevant return to political power—beyond the return to office holding—which mostly benefits influential politicians and their offspring.

Keywords: Reward to Politics; Political Power; Family Ties; Local Politics  
JEL: D72; D73

Electronic copy available at: <http://ssrn.com/abstract=2666087>

We are grateful to Yosef Bhatti and Lene Holm Pedersen for directing us to budget data for Danish municipalities, and for comments from seminars participants at INSEAD, Bocconi University, Copenhagen Business School and Hong Kong University of Science and Technology. We also thank the Danish Social Science Research Foundation for financial support. All errors remain our own.

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## 1. Introduction

Many modern political economy models are grounded in the notion that individuals engage in politics if the payoffs received outweigh the costs of holding a political office (see Besley 2006 and references therein). Those benefits are typically monetary forms of higher income, wealth, career opportunities, or the non-pecuniary form of personal rent. Empirically, the literature has focused on measuring the return to holding a political office in different institutional settings (e.g. Di Tella and Fisman 2004; Svaleryd and Vlachos, 2009). In this study, we address the different question of what is the return to power for a heterogeneous group of office-holding politicians within the same legislative structure.

We use a natural experiment that increases the power of politicians running municipality councils in Denmark and estimate the monetary benefits accruing from such increase in political power. We show that exogenous changes in political power—measured as changes in population or budget size—have a heterogeneous impact on politicians' income. Powerful politicians and their offspring experience a statistically and economically significant increase in income that is much larger than the average effect for all politicians.

Recent studies on the return to politics use a regression discontinuity design to estimate the income effect of holding political office.<sup>1</sup> This methodology compares income changes of politicians who were marginally elected to politicians who were marginally defeated. Eggers and Hainmueller (2009) use this approach to estimate a significant lifetime reward for being a member of the British Parliament. Using data from India, Fisman, Schulz, and Vig (2014) find that winning politicians experience a significant increase in asset growth. Kotakorpi, Poutvaara, and Tervio (2015) find a significant effect of political

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<sup>1</sup> See Lee and Lemieux (2010) for a review of the papers that use a regression discontinuity design to estimate the causal impact of election on economic outcomes. Another approach is to compare the income of politicians before and after the election year. Using the latter approach, Cingano and Pinotti (2013) show that politician-employees experience a higher wage growth following the political appointment.

election on the income of Finnish members of parliament, whereas the effect for local politicians is trivial. Adopting an historical perspective, Querubin and Snyder (2013) estimate a positive wealth effect of winning a political race during times of crisis and war, while the effect is insignificant in normal times. Finally, Lundqvist (2011) finds no effect on income of local politicians in Sweden, while Willumsen (2011) finds a significant effect of election on income in Norway.<sup>2,3</sup>

The regression-discontinuity approach is effective in detecting a causal return to office holding.<sup>4</sup> Our focus is on differential return to power across heterogeneous politicians already holding office. This question is important given that return to politics is skewed. Politicians within a legislative structure differ in personal traits such as education, income, networks, and talent and, thus, end up with differential power within the legislature: some become party leaders, coalition leaders, and mayors, while others become back benchers with less influence on political outcomes. We document that the return to politics is increasing in political power using exogenous variation due to an administrative reform.

We exploit the passage of an administrative reform that changed the size of local municipalities in Denmark in 2005. In particular, this reform merged 239 municipalities into 66 larger municipalities while it left 32 municipalities unchanged. This reform provides us with exogenous variations in political power for elected local politicians, which we

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<sup>2</sup> In addition to measuring return to holding political office, an interesting literature has focused on labor supply and productivity of politicians. For instance, Ferraz and Finan (2010) and Hoffman and Lyon (2013) find that the output of politicians (measured as more legislative bills) increase with the salary that politicians receive. Fisman et al. (2015) estimate how politicians' labor supply is affected by the salary they receive.

<sup>3</sup> The regression discontinuity approach can be augmented to make causal statements away from the boundaries (see Angrist and Rokkanen 2015; and Cuñat, Gine, and Guadalupe 2015). This approach increases the range of politicians for whom it is possible to estimate the return to political offices.

<sup>4</sup> The regression discontinuity approach has recently been challenged by evidence suggesting that even close elections are not necessarily random in their outcome. Victories in U.S. House elections have been shown to correlate with lagged variables such as incumbency status and previous vote share. Such sorting challenges the premise of the discontinuity tests: that outcomes in close elections are quasi-random (see Snyder 2005; Caughey and Sekhon 2011; and Grimmer, Hersh, Feinstein, and Carpenter 2012). However, Eggers et al. (2015) document that such correlations are typically not observed.

exploit to study how political power affects incomes of both politicians and their close family members. To circumvent the identification challenge of elected politicians being different from non-elected in unobservable traits that may correlate with income, our main analysis employs re-elected politicians in merging municipalities and uses re-elected politicians in non-merging municipalities as counterfactuals. The advantages of our estimation method are threefold. First, by focusing on re-elected politicians, we separate out the return to power from the return to office, as all our subjects are office-holders before and after the reform. Second, we are able to measure the return to power for any type of politician represented both in the treatment and in the control group; this allows us to examine the heterogeneity in the return to politics across different types of politicians. Third, we can measure the return to power for family members connected to any politician.

Our identification strategy, combined with comprehensive data on incomes of all Danish politicians and their family members from 2002 to 2009, makes us able to derive a number of significant contributions to the existing literature. First, we show that an exogenous increase in political power induces a statistically significant but economically small increase in politicians' income: the average re-elected politician in a merged municipality experiences an increase of 3.8 percent in total personal income around the reform (as compared to a re-elected politician in a non-merging municipality). Second, the positive effect on income is increasing in political power. We find a 5.4 percent increase in income for politicians with above median votes, an 8.5 percent increase for party leaders, a 12.7 percent increase for coalition party leaders, and finally, a 17.2 percent increase for mayors. By contrast, politicians elected with the smallest number of votes in a municipality experience no significant increase in income.<sup>5</sup> Third, using the increase in population or

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<sup>5</sup> Notice that this heterogeneity rules out the concern that our findings on the increase in incomes are mechanically driven by the fact that "all" politicians in merged municipalities are compensated because of a larger municipality size.

budget size as proxy for the increase in political power, we can interpret our results as an elasticity of income to political power. For instance, the elasticity of income to population is 14.3 percent, and the elasticity of income to budget size is 14.0 percent, for mayors. The elasticity implies that if the population of the municipality doubles the mayors' income increases by 14 percent. Fourth, we show that the benefits of political power accrue not only to a politician directly, but also to his/her offspring: the elasticity of income to power is almost twice as high for offspring of the average politician as for the average re-elected politician. This elasticity is even higher when we condition on offspring who live in the same municipality as the parents, and hence have a better ability to benefit from the increase in political power. Finally, we provide weak evidence suggesting that one way in which politicians use their power to gain personal benefits is through corporate board appointments in the private sector.

Collectively, these findings suggest that political power pays off, not just for the politicians directly but also for their family members, and that such effects are mostly accrued by local politicians in key positions. Two important issues emerge when interpreting these results. First, our difference-in-differences method eliminates the return to holding office; thus, the total return to engage in local governments may be significantly higher. Second, according to Transparency International, Denmark consistently ranks as one of the least corrupt countries in the world; thus, our estimates can be seen as a lower bound for return to political power.<sup>6</sup>

In the next section, we discuss the administrative setting of local municipalities in Denmark and our empirical strategy. In Section 3 we provide a data description. Section 4 provides our main results, obtained from differences-in-differences and selection models as well as our estimated elasticity of income to political power. Section 5 shows that the return

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<sup>6</sup> Annual rankings of corruption worldwide from Transparency International are available on their webpage: [www.transparency.org](http://www.transparency.org).

to power is skewed; powerful politicians experience a large return to increases in political power, whereas the marginally elected politicians experience no such effect. Section 6 documents that offspring receive significant benefits from parents' political activities. Section 7 investigates how changes in political power affect the frequency of being appointed to the board of private companies. Section 8 concludes.

## **2. Institutional setting and empirical strategy**

Municipalities in Denmark are governed by local councils, which have the overall responsibility for providing public services in various sectors: primary and secondary education; special education; primary healthcare; elder care; social and business services; collective transportation; roads; and, often, provision of electricity, water, and heating. The vast services provided imply that municipalities account for approximately 48 percent of total public expenditures in Denmark.<sup>7</sup>

Municipalities are governed by local councils that have an odd number of seats, varying from 9 to 31 members (except for Copenhagen, which has 55 members). The election period is four years, and elections are held on the third Tuesday in November. After the election, local councils start working in January of the following year. At the first meeting, the council elects a mayor by simple majority vote.

The electoral system is proportional, and in most municipalities, the parties that run for election are the same as those that run for the national election; however, local parties do exist in some municipalities. The last four local elections took place in November of 2001, 2005, 2009, and 2013.

In this paper we use exogenous changes in political power to estimate the return that different types of politicians receive. Our source of exogenous variation in political power

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<sup>7</sup> Source: Danish Ministry of the Interior and Health (2005).

comes from a municipality reform in 2005 that changed the geographic borders of Danish municipalities.<sup>8</sup> Figure 1 maps the municipalities before and after the administrative reform, and Table 1 (Panel A) details how the reform reduced the number of municipalities.

**[[ INSERT Figure 1 about Here ]]**

**[[ INSERT Table 1 about Here ]]**

The left side of Figure 1 shows the municipality map prior to the 2005 reform: 271 municipalities ranging from fewer than 5,000 to almost 600,000 inhabitants. The aim of the reform was to increase economic and administrative efficiency by creating larger municipalities—that is, with at least 20,000 inhabitants. The selection of which municipalities to merge was based almost entirely on two criteria (Lassen and Serritzlew 2011): geography and population size.<sup>9</sup> The right side of Figure 1 and Panel A of Table 1 show the reform's outcome: 239 municipalities were merged into 66 larger municipalities, while 32 mostly large municipalities were left unchanged. Hence the average municipality increased in size from approximately 159 km<sup>2</sup> to 440 km<sup>2</sup> and, in terms of inhabitants, from about 20,000 to 56,000. Panel B of the table reports the reform's effect on the number of municipalities in terms of population and budget size in million Danish kroner (DKK).<sup>10</sup>

Our aim is to measure how exogenous variations in the size of local municipalities affect the income of re-elected politicians and close family members. We classify

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<sup>8</sup> Counselors in the new municipalities were chosen by the local elections in November 2005. However, to ensure continued operation in the merging municipalities, the tenure of the previous councils was prolonged by one year (i.e., until the end of 2006). In this transitory period, old municipalities transferred administrative entities to the new municipalities and then were fully dissolved on January 1, 2007. The newly elected councils in municipalities *not* involved in a merger commenced their activities on January 2006 (i.e., as if no reform had occurred). Five municipalities on the island of Bornholm merged into an island-wide municipality at the very start of debate over the reform (near the end of 2002); in the empirical analysis, we include Bornholm in the control group and note that our results are robust to excluding Bornholm.

<sup>9</sup> Some additional factors were at play in rare cases. A few municipalities were split into two parts, with each part merging into separate larger municipalities. Two small municipalities remained independent because the ruling coalition in neighboring municipalities was of a different political orientation, and a few poor municipalities had a hard time finding neighbors willing to merge.

<sup>10</sup> One Euro is equivalent to 7.45 Danish kroner.

municipalities into treatment municipalities (those that increased in size) and control municipalities (those that did not). We focus on re-elected politicians for two reasons. First, doing so circumvents potential endogeneity problems arising from elected and non-elected politicians being different in important dimensions that may correlate with personal and family income. Second, it highlights the main theme of this paper, which is how changes in legislative power have a heterogeneous impact on politicians already holding offices. We propose two ways of measuring this thesis: First we estimate a difference-in-differences model that captures the changes in income for politicians and their close family around the reform. Second, we exploit that the political reform gave rise to significant heterogeneity in the increase in political power, measured by population and budget size, to estimate the elasticity of politicians and family members' income to political power. We discuss and extend this approach in the following three remarks:

First, the most important premise of our methodology is that the increase in municipality size results in an increase of political power. Panel B of Table 1 reports the evolution of two measures of political power during the reform period. First, the number of inhabitants per politicians grew on average from 795 to 1,813, equivalent to an increase of more than 225 percent, in treatment municipalities, compared with a decrease on average of 27 inhabitants per politicians (or 0.1 percent) in control municipalities. Second, expenditures per elected politician increased by more than 160 percent in treatment municipalities, while they decreased by 8 percent in control municipalities due to the increasing number of seats in the local councils. Hence, Table 1, Panel B provides solid evidence that, on average, the reform significantly increased political power in merging municipalities while leaving it unaltered in control municipalities.

Second, our identification may deliver biased estimates if the reform affected the quality of politicians by increasing political competition in merging municipalities more



than in non-merging municipalities. We provide supportive evidence for this claim in the following section. If the quality of re-elected politicians is correlated with income, then our estimates on performance may be biased (either downward or upward). We solve this challenge by adopting a selection model based on two exclusion criteria for re-election: the aggregate party votes at the national level in the 2005 municipality election excluding the politician's own municipality; and, the vote distribution from the national parliamentary election closest to the reform date of November 1, 2005. We claim that neither of these measures is affected by the ability of individual politicians in a given municipality and, thus, that either is plausible as an instrument for the likelihood of being re-elected.

Third, our difference-in-differences identification strategy requires that the pre-reform “parallel trend” hypothesis be valid—in other words, that there be no pre-reform differential trends between treatment and control municipalities that correlate with the income of re-elected politicians. Suppose that in the years prior to the reform, a variable likely affecting politicians' rent-extraction, e.g., electoral competition, increased more in non-merging than in merging municipalities. If this differential trend continued during the reform window, then we would be unable to separate the effect of the reform from the effect of the pre-existing diverging trends. Testing the parallel trends hypothesis, we find no statistical difference in income prior to the reform (see Appendix A). In addition, we perform a (unreported) placebo test, which assumes that the reform took place in 2001, which reveals no differences with respect to personal income between treatment and control groups. Furthermore, we show that the key variables describing electoral competition yield no clear evidence disconfirming the parallel trend hypothesis prior to the reform.

### **3. Data and descriptive statistics**

We assemble a dataset of politicians around the political reform in 2005. Our dataset contains economic and personal information about the politicians as well as their close family members, from relevant official registers. The dataset is constructed based on several different administrative registers made available from Statistics Denmark, as explained below.

Election data are from the Ministry of Economic Affairs and the Interior. We have received complete election data for municipality elections in November 1997, 2001, and 2005. This includes a list of all candidates, the number of votes received, and an indicator for whether a candidate has been elected to the local council or not. From this data we can identify if candidates are new candidates, have been candidates before, or were members of the former municipality council.

Individual and family data originate from the official Danish Civil Registration System. These records include the personal identification number (CPR), gender, date of birth, and CPR numbers of family members (spouses and children). The sample contains the universe of the Danish population, and the personal identification number provides unique identification across individuals. Thus, with this data we can identify each politician's family members, which we will use to study whether increasing political power affects the income of politicians' family members.

Income data are from the official records at the Danish Tax Authorities (SKAT). This dataset contains personal income by CPR numbers on the Danish population. SKAT receives this information directly from the relevant sources; employers similarly supply statements of wages paid to their employees. Through Statistics Denmark, we obtain access to personal income and wealth data from 1997 to 2009.

Corporate appointments are from the Central Business Register (CVR) at the Danish Business Authority. All firms with limited liability companies are obliged to report changes

in their executive and board positions within two weeks of the change. We obtain corporate appointments in the period from 1997 to 2009, allowing us to observe individual appointments in the private sector.

The starting point of our analysis is the candidates running for the local elections in 2005. In Table 2, Panel A shows that 11,395 candidates ran in 2005, of which 8,375 ran in treatment (and 3,020, respectively, in control) municipalities. Out of the 11,395 candidates, 2,890 were incumbents. Altogether, 2,460 candidates were elected in the 98 municipalities, with 1,837 of these in the treatment municipalities and 623 in the control municipalities. Among the elected candidates 1,758 were incumbent, distributed with 1,352 in treatment and 406 in control municipalities. Finally, the number of mayors were the same as the post-reform number of municipalities, thus we have 66 mayors in treatment and 32 in control municipalities.

The ratio of candidates seeking re-election to all candidates is larger in treatment municipalities whereas the ratio of elected candidates to all candidates is very similar across treatment and control municipalities. The ratio of re-elected candidates to all candidates is higher in treatment municipalities, whereas the ratio of re-elected candidates to incumbent candidates running for re-election is higher in control municipalities. In our analysis, we focus on how exogenous changes in political power distribute rent to office-holding politicians, that is, we focus on re-elected candidates. The table shows that it is harder for an incumbent candidate to be re-elected in the merging municipalities than in the non-merging municipalities. From these observations, we conclude that the reform did increase electoral competition in the merged municipalities beyond that which it did in the non-merging municipalities. This finding underlines the importance of controlling for selection as part of our empirical strategy.

**[[ INSERT Table 2 about Here ]]**

The Ministry of Interior also provides us with social security numbers of candidates and elected politicians. From Statistic Denmark we receive detailed socio-economic information, including income, education, and other relevant variables.

**[[ INSERT Table 3 about Here ]]**

Table 3, Panel A provides summary statistics for the personal characteristics of all candidates, elected candidates, and reelected candidates. We notice that local politicians are around 50 years old when they stand for election; most of them are men; and their income is from approximately DKK 300,000 (candidates) to DKK 426.500 (re-elected) before the election and, for all groups, increases during the election cycle. This variation is illustrated in Figure 2, which plots the average income for different groups of politicians during the election cycle 2006-2009. It is clear that re-elected politicians are better at generating income than are candidates or the newly elected, although part of this difference can be found before the election. Furthermore, mayors have twice the income of the average re-elected candidate and close to three times the income of the average candidate in the 2005 election. Figure 3 shows that the average income for all candidates during the election cycle 2006–2009 is positively correlated with the size of the municipalities. The average income of re-elected politicians is more than DKK 100,000 (corresponds to 28 percent higher income) larger in municipalities with more than 100,000 inhabitants compared to municipalities with fewer than 10,000 inhabitants. Table 3, Panel B shows the pre-election characteristics depending on how much power the elected politicians have. We also notice that political power is positively correlated with age, being male, pre-election income, and election cycle income.

Figures 2 and 3 and Table 3 indicate that powerful politicians have different income profiles than do non-powerful politicians. Figure 2 and 3 also motivate our empirical

strategy which exploits an exogenous increase in district size to identify the causal effect of political power on personal income.

#### 4. The income effect of changes in district size

In this section, we provide our basic result, showing how the changes in district size lead to an increase in income for re-elected politicians.

##### 4.1 Difference-in-differences and the elasticity of income to district size

The main equation for estimating the impact of changes in district size on income is specified in Equation (1), where the dependent variable is the log income,  $y_{it}$ , of individual  $i$  in year  $t$ :

$$y_{it} = \alpha_i + \gamma_i + \beta'X_i + \delta'Treatment \times Post_{it} + \epsilon_{ijt}, \quad (1)$$

where the parameters  $\alpha_i$  represent individual fixed-effects, and  $\gamma_i$  represents a set of yearly indicator variables.  $X_i$  is a vector of time-varying individual characteristics (age and age squared), and  $treatment \times post$  is the interaction term between an indicator for *treated* municipalities (those that merge) and an indicator for the *post*-election period from 2006–2009. Note that the election took place in November 2005, and we therefore include 2005 in the pre-reform period. Table 4 reports the results.

Column 1 in Panel A provides the results obtained on the full sample of all political candidates. As shown, the coefficient on the interaction term is negative at 2 percent and significant at a 5 percent level. Since most candidates do not expect to have a political career and will spend little time on politics-related issues, we do not want to draw any strong conclusions from this finding.

Column 2 focuses on the 2,460 candidates who were elected to the municipality councils in the November 2005 election. As shown, the treatment effect is zero and statistically insignificant at conventional levels. Again, it is hard to interpret this result. We are comparing the newly elected and re-elected in merging municipalities versus non-merging municipalities. No evidence exists to suggest that changes in income of taking office are higher in merging municipalities than non-merging for newly elected politicians. After all, the non-merging municipalities are larger and often centered around the largest cities in terms of population, and Figure 2 shows that the income of elected politicians is positively correlated with district size.

Column 3 focuses on the changes in income for the 1,758 re-elected candidates. Re-elected candidates were office-holders both before and after the election. Thus, for this group, the treatment effect measures the effect of an average increase in political power on personal income for politicians in merging municipalities, using as counterfactuals a group of re-elected politicians who did not face a change in political power. The coefficient on the interaction term is positive and significant at the 10 percent level. Economically, the coefficient suggests that re-elected politicians in merged municipalities experienced a 3.8 percent increase in income (relative to re-elected politicians in unchanged municipalities).

The economic interpretation of our results so far is challenging: the coefficients in Panel A of Table 4 represent the average increase in income around the reform for the average politician in a merging municipality beyond the increase experienced by a re-elected politician in a non-merging municipality. Rather, we are measuring the return to an exogenous increase in political power for politicians who might not possess significant political power. In the following section, we will therefore re-examine the relationship between political power and income for subgroups of powerful politicians.

To facilitate the economic interpretation of our findings, we estimate the elasticity of personal income to continuous changes in political power. To this end, we apply the two measures of changes in political power that were introduced in Table 1. The first is the logarithm to new population over old population, and the second is the logarithm to the new budget size over old budget size. Intuitively, both these variables capture essential parts of influence and decision-making power.

Panels B and C of Table 4 report results from OLS regressions of how changes in political power impact personal income of re-elected politicians. We interact the change in political power with an indicator for post-reform years and focus on the re-elected politicians. As shown, the effect of greater political power is positive and significant at the 5 percent level for both measures. As we take the logarithm of both dependent and independent variables, we can interpret the coefficients as two measures of the elasticity of personal income to political power: Doubling the district size measured by population yields a 2.7 percent increase in average income for re-elected politicians. Since the population increased by 260 percent on average in merging municipalities (see Table 1), the elasticity approach is consistent with an average impact on re-elected politicians' income in merging municipalities of 7.1 percent, which is higher than our difference-in-differences estimate using discrete treatment. The elasticity of income to changes in budget size is 2.6 percent. Thus, doubling the budget size increases income by 2.6 percent. The average change in budget size in the merging municipalities is 315 percent, implying that income increases by 8.2 percent for the average re-elected politician.

To sum up, Table 4 yields that changes in district size provide a statistically significant but economically small increase in yearly income of the average re-elected politician. The effect is larger when we use continuous treatments such as increase in population and budget size rather than a discrete treatment.

The relatively contained size of these effects reflects that our analysis estimates a lower bound for return to power. First, we exclude the rent to office holding by focusing on re-elected politicians and including individual fixed-effects. Second, we are estimating return to local politics in Denmark, the world's least corrupt country according to the international watchdog Transparency International. Thus, it is interesting that we, given the institutional setting, can document a positive elasticity. We also expect the returns to political power to be larger in most countries around the world. Finally, return to politics may drastically vary across types of politicians. In Table 4 we implicitly assume that rent is equally distributed among all re-elected politicians. Below we will show that rent extraction varies significantly according to the power that individual politicians possess within the legislative structure.

Before discussing the return to political power, we pause at two interpretational caveats, which we will address in the final part of this section and in the following section.

Our first concern is about the presence of diverging trends in incomes between the two groups of re-elected politicians prior to the reform. If such diverging trends exist, it will be difficult to conclude that the ex-post differences in income only come as a result of the reform. To shed light on this issue, Appendix A unpacks the post-reform indicator in a set of dummies before and after the reform year. We notice no statistically significant treatment effect in 2004 (the year before the reform), a weak positive effect in the reform year, and a large significant effect in the year after the reform. Thus, Appendix A indicates that no diverging trends exist before the reform and, thus, supports the notion that the political reform induced the increase in income for re-elected politicians in merging municipalities. In addition, we perform a (unreported) placebo test in which we assume that the administrative reform occurred in 2001. When we replicate our analysis assuming



that the reform took place four years before, we find an insignificant effect of political power on income. This holds for re-elected as well as powerful politicians.

#### **4.2 Controlling for selection**

A second concern that potentially complicates the causal interpretation of our finding is that the reform changed the political competition, making it more difficult to get re-elected in merging municipalities. We document in Table 2 that the political reform increases competition for re-election in merging municipalities more than it does in non-merging municipalities. If this effect obtains, the reform is likely to induce selection of higher quality re-elected politicians in merging municipalities than that of re-elected politicians in non-merging municipalities. Thus, at this stage, we cannot rule out the possibility that part of the treatment effect captures this selection effect.

In Table 5, we control for the potential effect of the reform having affected competition differently in treatment and control municipalities. As identifying conditions, we want to find an instrument that is correlated with the likelihood of being re-elected, but uncorrelated with the individual characteristics of such politician. We use two instruments that satisfy these conditions. The first is the average vote that the candidates' party received in the 2005 election across all municipalities, excluding the municipality in which the candidate is running for election. The second variable is the result of the national election, which took place in January 2005.

We use a Heckman selection model to control for the selection into the pool of re-elected candidates. Panel A of Table 5 shows the first stage of this estimation procedure. The dependent variable is an indicator equal to one if a politician is re-elected. In Column 1 we notice that aggregate municipality votes have a positive impact on the likelihood of an incumbent candidate being re-elected. The effect is statistically significant at the 1 percent

level. Consistently, Column 2 shows that the aggregate party votes from the national election in January 2005 also positively affect the likelihood of an incumbent candidate being re-elected in the local election. This effect is also statistically significant at a 1 percent level.

Panel B of Table 5 reports the income effect of increased political power after we have controlled for selection. In Column 1 we observe that the treatment effect is 3.3 percent using the aggregate votes from the municipality election as identifying variable. The effect is only marginally statistically significant. In Column 2, where we use the party votes from national election as identifying variable, we obtain a treatment effect of 3.2 percent that is significant at the 10 percent level. These estimates appear similar to our baseline results in Table 4. Taken together, these findings suggest that controlling for selection does not induce major revision of our estimates and thereby lend support to a causal interpretation of results in Table 4.

## **5. Powerful politicians**

As discussed in the introduction, one advantage of our methodology is that we can measure the return to power for a broad group of politicians. In principle, we can measure the return to power for any type of politician who is identifiable both in the merging and in the non-merging municipalities. A priori, we have no reason to expect return to power to be equally distributed across politicians. Instead, we expect that the return to power is larger for powerful politicians and smaller for the marginally elected politician.

In Panel A of Table 6 we analyze how the return to changes in district size varies with the power of the politicians within the legislative council. Panel A replicates the difference-in-differences analysis of Table 4, focusing on powerful politicians. Column 1 shows that the treatment effect for politicians with more than the median number of votes is 6.4

percent, which is substantially more than the 3.8 percent average treatment effect for re-elected politicians in Table 4. Furthermore, the treatment effect for party leaders is 8.5 percent in Column 2, and 12.7 percent for party leaders from the coalition behind the mayor in Column 3. In Column 4, we estimate the effect for politicians who were also appointed as municipality mayors. The coefficient of the interaction term is positive, significant at the one percent level, and almost 4 times larger than the treatment effect for the average politician estimated in Table 4. Mayors on average receive 17.2 percent higher income when their district sizes increase.

For all four groups of powerful politicians, we find a significant interaction coefficient, which is also larger than the one for the average politician. In other words, the increase in income induced by the reform was substantially larger for politicians affected by the reform and who were also occupying key political positions. To reinforce the importance of political power, we conduct the opposite test, i.e., we estimate the income effect of the reform for “weak” politicians, that is, re-elected politicians who received the smallest number of votes in a given municipality. In unreported regression we find an economically and statistically insignificant change in the yearly incomes of those politicians.

Panels B and C of Table 6 estimate the elasticity of income to political power for the same group of politicians as are found in Panel A. As in Table 4, we use the changes in our two measures of political power to estimate the elasticity of income to political power. Note that, for both measures, the elasticity of the majors’ income to changes in political power is at least two times higher than for other powerful politicians. Mayors’ income increases by around 14 percent when the district doubles. In comparison, party leaders and coalition party leaders have an elasticity of income to power of around 4 and 5.5 percent, respectively.

This section shows that the return to power is significant for more powerful politicians in general and for mayors in particular. This finding highlights the importance of developing estimation methods that can cater to the heterogeneous distribution of power within a political legislature.

## **6. Family income**

In the previous sections, we document a positive increase in re-elected politicians' personal income from increasing their political power. Their personal income, however, may only constitute a fraction of their total return to power. Another source of benefits may be that close family members improve their labor market outcomes by getting better jobs or not being fired. Strong evidence exists that politically connected firms in general benefit from their family-based relationships with powerful politicians (Fisman 2001; Bunkanwanicha and Wiwattanakantang 2009; Amore and Bennedsen 2013).

In this section we examine whether the close family members of re-elected politicians benefit from our reform-induced increase in political power. Thus, we do not look at corporate connection as such but simply ask if the income of close family members is affected by changes in political power. We consider family members who do business with the local municipality but also, for instance, better employment opportunities and lower likelihood of unemployment spells.

We use as dependent variable the logarithm of yearly income of close family members around re-elected politicians and then use a specification similar to our baseline regression models in Equation 1. The left-hand side of Table 7 shows the results when we estimate the income effect for the spouses of re-elected politicians. Column 1 shows the result for spouses of all re-elected candidates. Notice that the number of observations is lower than in Table 4, attributable to the fact that not all re-elected politicians are married. Column 2

establishes the effect on the spouses of politicians with more than an average number of votes; Column 3, on the spouses of party leaders; Column 4, on the spouses of coalition leaders; and Column 5, on spouses of mayors. Our results indicate that the effects for all five columns are not statistically significant at any conventional level. Even if the coefficient is positive in four out of five columns, we conclude that no evidence exists that the yearly incomes of spouses of re-elected politicians are affected by an increase in political power.

The right-hand side of Table 7 documents the same exercise using as dependent variable the yearly income of the children of re-elected politicians. The coefficient of the key interaction term is positive and significant when we look at the child of an average re-elected politician (Column 1). Interestingly, the coefficient is more than 3 times as large than the coefficient for the re-elected politician him/herself (see tables 4 and 6). The coefficient is even larger for children of re-elected politicians with a high number of votes (Column 7), party leaders (Column 8), coalition leaders (Column 9), and mayors (Column 10).

It is remarkable that the average child appears to have an economic benefit when one of his/her parents obtains more political power. As mentioned previously, Denmark is a low-corruption country, and we are analyzing local politicians who, in general, are not very powerful. Still, the result shows that the average child of re-elected politicians in merging municipalities benefitted significantly from the parent's increased political power.

It is worth noting that the effect is heterogeneous across children. In Appendix B we replicate the analysis of Table 7, focusing on children who are living in the same municipality as the legislature where the parent is elected. For these children, economic effect is even larger for the average child. It is also larger for the children of majors, but this result is not statistically significant.

## 7. Corporate board appointments

In this section we investigate one channel through which political power may trigger greater individual and family income. Specifically, we study how an increase in political power affects the likelihood of being appointed a board member of private companies. In Table 8, the dependent variable is the number of directorships in a given year. The key variable of interest is again the treatment indicator interacted with the dummy for post-reform years. As is similar to our previous regressions, we control for individual heterogeneity through individual fixed effects, age effects, and for time trends using yearly indicators.

Panel A in Table 8 yields a positive and statistically significant treatment effect on board appointments for all re-elected politicians (Column 1). The number of directorships increases by 0.2 for re-elected politicians in the treated municipalities relative to the control group. For politicians with above-median votes (Column 2) and for party leaders (Column 3), we find even larger effects that also are statistically significant. For coalition leaders (Column 4) and majors (Column 5), the interaction terms lose their statistical significance even though the coefficients are economically large. In panels B and C we obtain estimates of similar magnitude, although most of the effects are statistically insignificant.

The results of Table 8 are consistent with corporate appointments being one of the channels through which political power generates higher income. We do not claim that this channel is the only, nor the most important, channel, but we believe the table yields some suggestive evidence on where returns to politics may come from.

## 8. Conclusion

Our study delivers three important contributions that further our understanding of the relationship between political power and return to political office.

First, we propose a new identification methodology that uses an administrative reform to estimate the sensitivity of politicians' income to changes in political power. This methodology has the advantages of enabling us to measure returns to political power for any kind of politician who is present both in the districts affected and unaffected by the reform. Our focus on return to political power has importantly expanded recent research that employs regression discontinuity design to establish the causal effect on income of holding a political office. Interestingly, our methodology is replicable in other countries where there have been reforms of legislative districts.

Second, we estimate an arguably lower bound for return to political power. We focus on one of the least corrupt countries in the world and on local politicians, who in general are less powerful than national politicians. Even in this context, we find a significant return to power in the sense that the income of re-elected politicians in key influential positions increases when the political power rises. We further document that this effect is positive for the average re-elected politician and substantially stronger for the more influential politicians. In contrast, we did not find any return to power for "weak" re-elected politicians. We believe our lower-bound analysis is important because it suggests that one should expect to find a significantly larger return to politics in many other settings such as legislatures with more power or other countries with higher levels of corruption.

Third, exploiting the richness of our data, we document that politicians' offspring, too, receive a significant return from increases in political power. Hence, measuring only politicians' own return from being engaged in politics may significantly underestimate the total return that politicians receive from holding political office.

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**Table 1. Danish municipalities before and after the administrative reform**

This table reports the effect of the administrative reform on the number and size of municipalities in Denmark. Effective January 1, 2007, the reform merged 239 municipalities into 66 municipalities (treatment group), while 32 municipalities were unaffected (control group). Panel A shows the distribution of municipalities by population size before and after the reform for the treatment and control group. Panel B reports the mean size of municipalities measured by population and total budget. Panel C shows the increase in political power measured as the size of municipality per elected politician. In Panel C, we test for differences in means of political power measured by population and budget size per elected politician before and after the reform form. \*, \*\*, and \*\*\* denote significance at (respectively) 10%, 5%, and 1%.

	Treatment			Control		
	Before (1)	After (2)	Difference (2) - (1)	Before (3)	After (4)	Difference (4) - (3)
<i>A. Number of municipalities by population size</i>						
>100,000	1	3	2	3	3	0
50,000-100,000	8	23	15	5	5	0
25,000-50,000	16	34	18	12	12	0
10,000-25,000	88	5	-83	9	9	0
<10,000	126	1	-125	3	3	0
All	239	66	-173	32	32	0
<i>B. Mean size of municipalities measured by population and budget</i>						
Population	14,380	52,386	38,006	62,091	62,171	80
Budget (mio. DKK)	747.1	3,078.4	2331.3	3,960.0	4,050.9	90.9
<i>C. Measure of political power</i>						
Population / Elected politicians	794.8	1813.0	1,015.1***	2318.0	2290.5	-27.0
Budget / Elected politicians	40.8	106.4	65.6***	150.7	138.4	-12.2

**Table 2. Electoral results in 2005 local election**

This table reports the results of the 2005 local elections held on Tuesday, November 19, 2005. Panel A reports the number of candidates, incumbent candidates, elected candidates, re-elected candidates, and mayors based on the election results. Panel B reports election probabilities and tests for differences in mean between the treatment and control group. \*, \*\*, and \*\*\* denote significance at (respectively) 10%, 5%, and 1%.

	All	Treatment	Control	Difference
		(1)	(2)	(1) - (2)
<i>A. Candidates</i>				
All candidates	11,395	8,375 (73%)	3,020 (27%)	-
Incumbent candidates	2,890	2,370 (82%)	520 (18%)	-
Elected candidates	2,460	1,837 (75%)	623 (25%)	-
Re-elected candidates	1,758	1,352 (77%)	406 (23%)	-
Mayors	98	66 (67%)	32 (33%)	-
<i>B. Election probabilities</i>				
Incumbent to all candidates (%)	25.3	28.3	17.2	11.1*** [12.2]
Elected to all candidates (%)	22.1	22.1	22.1	0.0 [0.01]
Re-elected to elected candidates (%)	67.6	73.6	60.6	12.4*** [6.03]
Re-elected to incumbent candidates (%)	60.8	57.0	78.1	-21.9*** [-9.02]
Mayors to incumbent candidates (%)	3.1	2.6	5.4	-2.8*** [-3.39]

**Table 3. Individual characteristics of politicians**

The average characteristics are calculated pre-election (2005), and during the election cycle (2006–2009). We report average age, fraction of male, and total personal income. Panel A shows the individual characteristics for all, elected, and reelected candidates. Panel B shows individual characteristics for powerful politicians: politicians with high personal votes (personal votes above median votes within in the municipality), party leaders, coalition party leaders (winning coalition based on election results), and (post-election) mayors.

	Pre-election			Election cycle	N
	Age (years)	Male (%)	Income (1,000 DKK)	Income (1,000 DKK)	
<i>A. Candidates</i>					
All candidates	49.5	70.4	305.3	324.2	11,395
Elected candidates	50.4	72.7	390.5	434.2	2,522
Re-elected candidates	52.5	75.4	426.5	449.7	1,758
<i>B. Powerful politicians</i>					
High personal votes	52.1	75.7	445.1	494.4	966
Party leaders	53.4	81.8	464.9	528.5	461
Coalition party leaders	53.8	83.1	513.5	614.4	195
Mayors	54.9	91.8	617.6	779.0	98

**Table 4. Income effect, by politician's type**

The table reports results from OLS regressions. The dependent variable is the natural logarithm of total incomes. In Panel A, treatment is an indicator equal to one if the municipality was changed by the administrative reform, and zero otherwise. In Panel B, we use population change as a continuous treatment defined as the logarithm of the population of the post-reform municipality minus the logarithm of the population of the pre-reform municipalities. In Panel C, we use budget change as continuous treatment defined as the logarithm of the budget of the post-reform municipality minus the logarithm of the budget of the pre-reform municipality. Post is an indicator equal to one for the post-reform years, or 2006-2009, and zero for the years from 2002 to 2005. Column (1) reports the results obtained on the full sample of candidates in the 2005 elections. Column (2) reports the results on the subsample of elected candidates. Column (3) reports the results on the subsample of re-elected candidates. Each regression includes individual fixed effects and year fixed effects, as well as the logarithm of politician's age and its squared term (coefficients unreported). Standard errors clustered by new municipality are reported in parentheses. \*, \*\*, and \*\*\* denote significance at (respectively) 10%, 5%, and 1%.

	<b>All candidates (1)</b>	<b>Elected candidates (2)</b>	<b>Re-elected candidates (3)</b>
<i>A. Discrete treatment</i>			
Treatment × Post	-0.020** (0.010)	0.000 (0.017)	0.038* (0.020)
Individual fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
N	88,290	19,596	13,684
<i>B. Continuous treatment using population size</i>			
Population change × Post	-0.014** (0.006)	0.010 (0.011)	0.027** (0.011)
Individual fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
N	88,290	19,596	13,684
<i>B. Continuous treatment using budget size</i>			
Budget change × Post	-0.014** (0.006)	0.010 (0.011)	0.026** (0.011)
Individual fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
N	88,290	19,596	13,684

**Table 5. Controlling for selection**

The table reports results from Heckman selection models to control for the selection into the pool of re-elected politicians. In Panel A, we report results from the first stage selection equation, in which the dependent variable is a dummy equal to one for politicians who were re-elected and zero for politicians who ran for re-elections, but were not re-elected. In Column (1), we use as identifying variable the average of the share of votes obtained by the politician's party outside the politician's municipality in the 2005 election, whereas in Column (2), we use the ratio of votes obtained by the politician's party in the 2005 general election. Panel B reports the results from the second stage of the Heckman selection model, in which the dependent variable of the outcome equation is the natural logarithm of total personal income. Treatment is an indicator equal to one if the municipality merged due to the administrative reform, and zero otherwise. Post is an indicator equal to one for the post-reform years, or 2006-2009, and zero for the years from 2002 to 2005. Column (1) reports the results obtained using the corresponding Column (1) of Panel A, and Column (2), the corresponding Column (2) of Panel A. Each regression includes year fixed effects as well as the logarithm of a politician's age and its squared term (coefficients unreported). Standard errors clustered by new municipality are reported in parentheses. \*, \*\*, and \*\*\* denote significance at (respectively) 10%, 5%, and 1%.

	(1)	(2)
<i>A. First stage</i>		
Party votes in other municipalities	0.665*** (0.151)	
Party votes in national election		1.448*** (0.256)
Year fixed effects	Yes	Yes
N	22,721	22,721
<i>B. Second stage</i>		
Treatment × Post	0.033* (0.018)	
Treatment × Post		0.032* (0.005)
Year fixed effects	Yes	Yes
N	13,684	13,384

**Table 6. Income effect by political power**

The table reports results from OLS regressions. The dependent variable is the natural logarithm of total personal income. Treatment is an indicator equal to one if the municipality merged due to the administrative reform, and zero otherwise. Post is an indicator equal to one for the post-reform years, or 2006–2009, and zero for the years from 2002 to 2005. We measure powerful politicians four ways: Column (1) reports the results on the subsample of re-elected candidates with personal votes above the median in their municipality; Column (2) reports the results on the subsample of re-elected party leaders; Column (3) reports the results on the subsample of re-elected party leaders from the winning coalition; and Column (4) reports the results on the subsample of re-elected politicians who become mayors. Each regression includes individual fixed effects and year fixed effects, as well as the logarithm of a politician’s age and its squared term (coefficients unreported). Standard errors clustered by new municipality are reported in parentheses. \*, \*\*, and \*\*\* denote significance at (respectively) 10%, 5%, and 1%.

	Powerful politicians			
	High personal votes (1)	Party leaders (2)	Coalition leaders (3)	Mayor (4)
<i>A. Discrete treatment</i>				
Treatment × Post	0.064** (0.025)	0.085** (0.033)	0.127*** (0.044)	0.172*** (0.042)
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	6,654	3,593	1,517	707
<i>B. Continuous treatment using change in population size</i>				
Population change × Post	0.035** (0.015)	0.043* (0.022)	0.058** (0.025)	0.143*** (0.027)
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	76,654	3,593	1,517	707
<i>C. Continuous treatment using change in budget size</i>				
Budget change × Post	0.032** (0.015)	0.040* (0.022)	0.053** (0.25)	0.140*** (0.026)
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	6,654	3,593	1,517	707

**Table 7. Political power and family income**

The table reports results from OLS regressions. In Columns 1 to 5, the dependent variable is the natural logarithm of total incomes of a politician’s spouse, whereas in Columns 6 to 10, it is the logarithm of incomes of a politician’s offspring. Treatment is an indicator equal to one if the municipality was changed by the administrative reform, and zero otherwise. Post is an indicator equal to one for the post-reform years, or 2006–2009, and zero for the years from 2002 to 2005. We measure powerful politicians four ways: Column (1) and (6) report the results on the subsample of re-elected candidates with personal votes above the median in their municipality; Columns (2) and (6) report the results on the subsample of re-elected candidates with above median votes, Column (3) and (8) report the results on the subsample of re-elected party leaders; Column (4) and (9) report the results on the subsample of re-elected party leaders from the winning coalition; and Column (5) and (10) report the results on the subsample of re-elected politicians who become mayors. Each regression includes individual fixed effects and year fixed effects, as well as the logarithm of a politician’s age and its squared term (coefficients unreported). Standard errors clustered by new municipality are reported in parentheses. \*, \*\*, and \*\*\* denote significance at (respectively) 10%, 5%, and 1%.

	Spouse’s income					Children income				
	Re-elected candidates	High personal votes	Party leaders	Coalition leaders	Mayor	Re-elected candidates	High personal votes	Party leaders	Coalition leaders	Mayor
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>A. Discrete treatment</i>										
Treatment × Post	0.008 (0.027)	0.040 (0.034)	0.005 (0.042)	0.034 (0.089)	-0.013 (0.127)	0.140*** (0.050)	0.117* (0.068)	0.154* (0.085)	0.190* (0.113)	0.194 (0.090)
<i>B. Continuous treatment using change in population size</i>										
Treatment × Post	-0.004 (0.012)	0.005 (0.018)	-0.018 (0.026)	-0.001 (0.055)	-0.049 (0.090)	0.072*** (0.025)	0.055 (0.038)	0.097* (0.054)	0.109 (0.069)	0.150* (0.090)
<i>C. Continuous treatment using change in budget size</i>										
Treatment × Post	-0.003 (0.012)	0.005 (0.017)	-0.015 (0.025)	0.005 (0.053)	-0.049 (0.090)	0.068*** (0.024)	0.051 (0.037)	0.095* (0.052)	0.107 (0.067)	0.151* (0.090)
N	12,092	6,608	3,128	1,304	636	18,415	8,874	5,137	2,177	1,189



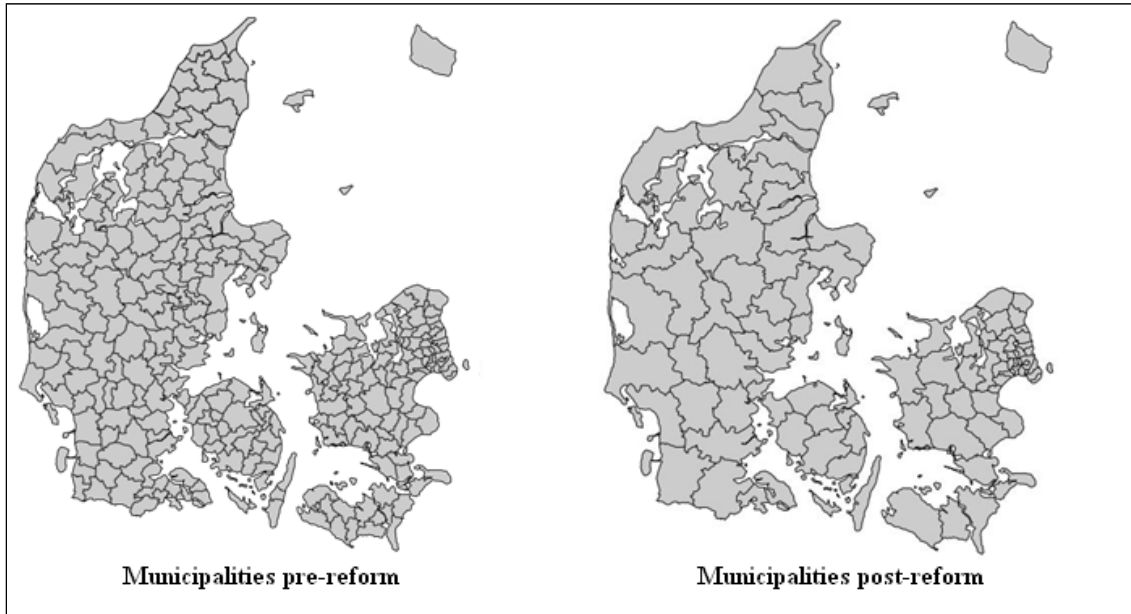
**Table 8. Effect on re-elected politicians' corporate appointments**

The table reports results from OLS regressions. The dependent variable is the number of corporate board appointment in a given year. Treatment is an indicator equal to one if the municipality was changed by the administrative reform, and zero otherwise. Post is an indicator equal to one for the post-reform years, or 2006-2009, and zero for the years from 2002 to 2005. Column (1) reports the results obtained on the sample of all re-elected politicians in the 2005 elections. Column (2) reports the results on the subsample of re-elected candidates with personal votes above the median in their municipality. Column (3) reports the results on the subsample of re-elected party leaders. Column (4) reports the results on the subsample of re-elected party leaders from the winning coalition. Column (5) reports the results on the subsample of re-elected politicians who become mayors. Each regression includes individual fixed effects and year fixed effects, as well as the logarithm of a politician's age and its squared term (coefficients unreported). Standard errors clustered by new municipality are reported in parentheses. \*, \*\*, and \*\*\* denote significance at (respectively) 10%, 5%, and 1%.

	<b>Powerful politicians</b>				
	<b>Re-elected (1)</b>	<b>High personal votes (2)</b>	<b>Party leaders (3)</b>	<b>Coalition leaders (4)</b>	<b>Mayor (5)</b>
<i>A. Discrete treatment</i>					
Treatment × Post	0.213** (0.091)	0.302** (0.117)	0.335* (0.181)	0.442 (0.296)	0.553 (0.477)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
N	13,684	7,537	3,593	1,517	707
<i>B. Continuous treatment using change in population size</i>					
Population change × Post	0.094* (0.056)	0.150* (0.089)	0.146 (0.193)	0.336 (0.350)	0.222 (0.430)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
N	13,684	7,537	3,593	1,517	707
<i>C. Continuous treatment using change in budget size</i>					
Budget change × Post	0.086 (0.055)	0.137 (0.087)	0.130 (0.192)	0.317 (0.346)	0.193 (0.430)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
N	13,684	7,537	3,593	1,517	707

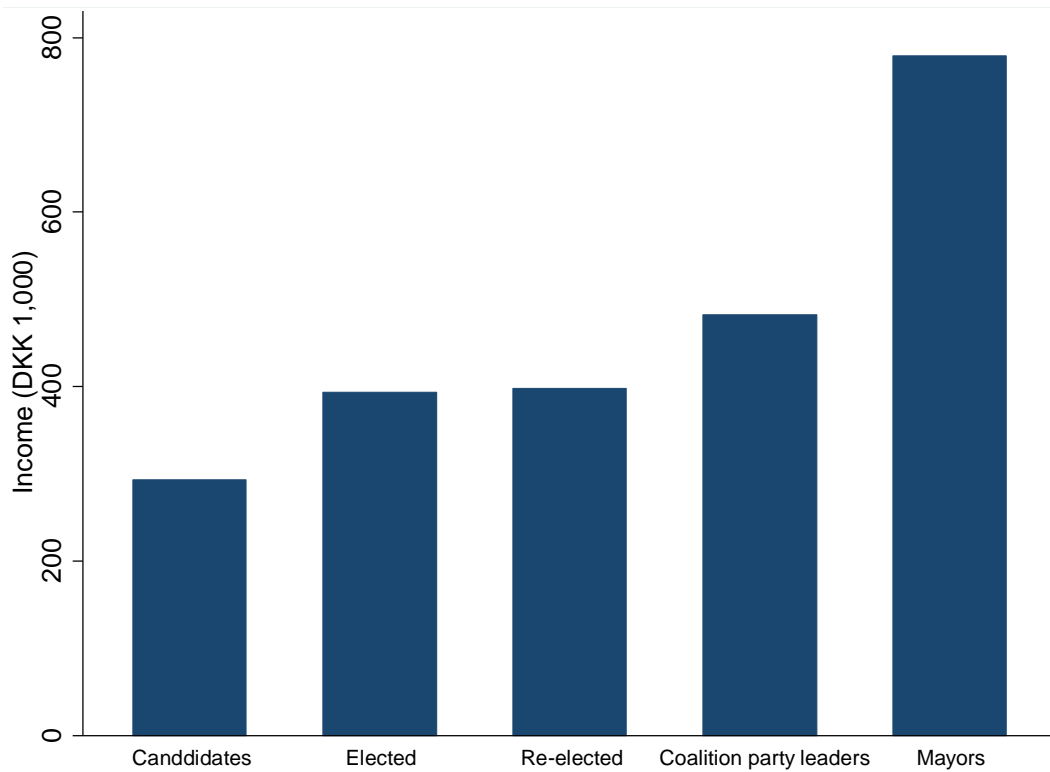
### Figure 1. Danish municipalities before and after the administrative reform

This chart maps Danish municipalities before and after the administrative reform of 2005. The reform merged 239 small- and medium-sized municipalities into 66 larger municipalities, while 32, primary larger, municipalities were unaffected by the reform (see Table 1 for distribution of municipalities on population size pre- and post-reform).



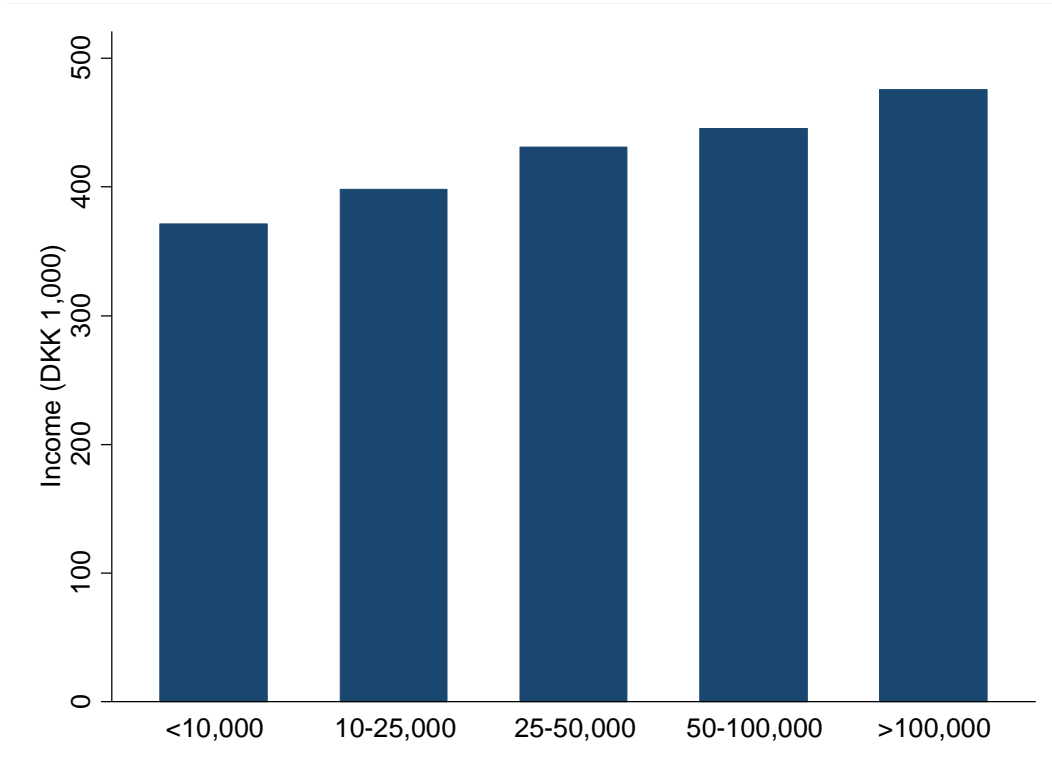
**Figure 2. Average yearly income during election cycle**

This figure plots the average personal income in 1,000 DKK during the election cycle from 2006 to 2009. We report the average income for all *candidates*, *elected candidates*, and *re-elected candidates* in the 2005 election, as well as coalition party leaders (winning coalition based on election results) and politicians who become *mayors* afterwards. One DKK equals 7.45 Euro.



**Figure 3. Income of elected politicians and population size**

This figure plots the average personal income of elected politicians during the election cycle from 2006 to 2009. We report the average income for municipalities with a population less than 10,000; 10,000 to 25,000; 25,000 to 50,000; 50,000 to 100,000; and above 100,000 people.



## Appendix A. Timing of income effect by political power

The table reports results from OLS regressions. The dependent variable is the natural logarithm of total personal income. Treatment is an indicator equal to one if the municipality merged due to the administrative reform, and zero otherwise. The post-reform indicator is replaced with a set of dummies for the years before and after the reform year (reference period is two years or earlier the reform year). We measure powerful politicians five ways: Column (1) reports the results for the subsample of reelected politicians; Column (2) reports the results on the subsample of re-elected candidates with personal votes above the median in their municipality; Column (3) reports the results on the subsample of re-elected party leaders; Column (4) reports the results on the subsample of re-elected party leaders from the winning coalition; and Column (5) reports the results on the subsample of re-elected politicians who become mayors. Each regression includes individual fixed effects and year fixed effects, as well as the logarithm of a politician's age and its squared term (coefficients unreported). Standard errors clustered by new municipality are reported in parentheses. \*, \*\*, and \*\*\* denote significance at (respectively) 10%, 5%, and 1%.

	<b>Powerful politicians</b>				
	<b>Re-elected (1)</b>	<b>High personal votes (2)</b>	<b>Party leaders (3)</b>	<b>Coalition leaders (4)</b>	<b>Mayor (5)</b>
<b>A. Discrete treatment</b>					
Treatment×Post <sub><i>t</i> = -1</sub>	0.028 (0.018)	0.029 (0.020)	0.068** (0.032)	0.066 (0.041)	0.034 (0.037)
Treatment×Post <sub><i>t</i> = 0</sub>	0.043** (0.021)	0.064** (0.025)	0.123*** (0.031)	0.154*** (0.050)	0.299*** (0.059)
Treatment×Post <sub><i>t</i> = +1</sub>	0.089*** (0.250)	0.136*** (0.031)	0.172*** (0.041)	0.160*** (0.048)	0.155*** (0.048)
Treatment×Post <sub><i>t</i> = +2</sub>	0.026 (0.029)	0.044 (0.036)	0.061 (0.038)	0.139** (0.059)	0.156*** (0.050)
Treatment×Post <sub><i>t</i> = +3</sub>	0.022 (0.025)	0.042 (0.037)	0.051 (0.046)	0.126 (0.076)	0.107 (0.066)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
N	13,684	6,554	3,593	1,517	707

## Appendix B. Political power and children income

### – subsample of children living in municipality

The table reports results from OLS regressions. The dependent variable is the natural logarithm of incomes of a politician’s offspring living in the same municipality. Treatment is an indicator equal to one if the municipality merged due to the administrative reform, and zero otherwise. The post-reform indicator is replaced with a set of dummies for the years before and after the reform year (reference period is two years or earlier than the reform year). We measure powerful politicians five ways: Column (1) reports the results for the subsample of reelected politicians; Column (2) reports the results on the subsample of re-elected candidates with personal votes above the median in their municipality; Column (3) reports the results on the subsample of re-elected party leaders; Column (4) reports the results on the subsample of re-elected party leaders from the winning coalition; and Column (5) reports the results on the subsample of re-elected politicians who become mayors. Each regression includes individual fixed effects and year fixed effects, as well as the logarithm of children age and its squared term (coefficients unreported). Standard errors clustered by new municipality are reported in parentheses. \*, \*\*, and \*\*\* denote significance at (respectively) 10%, 5%, and 1%.

	Powerful politicians				
	Re- elected (1)	High personal votes (2)	Party leaders (3)	Coalition leaders (4)	Mayor (5)
<i>A. Discrete treatment</i>					
Treatment × Post	0.210*** (0.060)	0.111 (0.105)	0.133 (0.110)	0.118 (0.178)	0.238 (0.197)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
N	11,512	5,446	2,969	1,207	686
<i>B. Continuous treatment using change in population size</i>					
Population change × Post	0.153*** (0.033)	0.150*** (0.049)	0.146** (0.066)	0.149 (0.094)	0.252** (0.101)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
N	11,512	5,446	2,969	1,207	686
<i>C. Continuous treatment using change in budget size</i>					
Budget change × Post	0.147 (0.033)	0.145** (0.049)	0.143** (0.063)	0.152 (0.093)	0.260 (0.101)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
N	11,512	5,446	2,969	1,207	686