Political Dynasties and the Quality of Government*

Arthur Bragança† UFMG
Claudio Ferraz‡ PUC-Rio
Juan Rios§ Stanford University

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Abstract

This paper examines whether dynastic politicians – politicians that had relatives in office in the past– affect the quality of government. We use a regression discontinuity design with electoral data for mayors in Brazil and examine whether dynastic politicians implement different policies compared to non-dynastic politicians. We find that dynastic politicians spend more resources, specially in investment in urban infrastructure, health and sanitation. However, we do not find improvements in economic growth and changes in the quality of public services.

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†Department of Economics, Universidade Federal de Minas Gerais (UFMG) and Climate Policy Initiative (CPI); Avenida Presidente Antônio Carlos 6627, 31270-901, Belo Horizonte, MG, Brazil. E-mail: *arthurbraganca* at *cedeplar.ufmg.br*

‡Department of Economics, Pontifícia Universidade Católica do Rio de Janeiro (PUC-Rio) and BREAD; Rua Marquês de São Vicente 225, Rio de Janeiro, RJ, 22453-900, Brazil. E-mail:*cferraz* at *econ.puc-rio.br*

§Economics Department, Stanford University; 579 Serra Mall, Stanford, CA 94305-6072. E-mail:*juanfrr* at *stanford.edu*

1 Introduction

The persistence of political power within families is a widespread phenomenon across the world. Many people argue that political dynasties affect the legitimacy of democracy and the quality of government policies as dynastic politicians have incentives to implement policies that will increase their advantage and guarantee the perpetuation of power.[1] In countries with weak institutions, this can lead to patronage and corruption. The perpetuation of dynasties in power might also generate negative consequences through selection. If countries limit the pool of candidates that can enter politics, the quality of government can decrease as leaders are chosen from a restricted set of individuals.

Despite these potential criticisms, dynastic management can also induce positive effects. As argued by Olson (2002) and Besley and Reynal-Queirol (2015) hereditary transmission of power can play a role in improving economic performance when it increases the time horizon that politicians face and improves inter-temporal incentives. The argument is that the establishment of family reputation in politics can be used to control moral hazard.[2] Political dynasties can also serve as a way to allow woman to enter politics using the political capital of the family as suggested by Labonne and Querubin (2015).

While there is growing evidence that political dynasties self-perpetuate in power, there is limited evidence on the consequences of having dynastic politicians in government.[3] This paper examines whether dynastic politicians choose different policies and induce different economic outcomes compared to non dynastic politicians. An important challenge to establish a causal link between dynastic politicians and economic outcomes is the fact that political selection is not random and we expect municipalities governed by dynastic politicians to be different from municipalities governed by non-dynastic politicians in many dimensions. Our paper takes advantage of the rich electoral data available in Brazilian municipalities and use a Regression Discontinuity Design where we compare government policies and local development outcomes in municipalities in which a dynastic candidate won a close election with municipalities in which a dynastic candidate lost a close election. This empirical design enables us to control for unobservable municipal characteristics that might drive both the presence of dynasties and development outcomes.

[1] See Michels (1915) and Pareto (1968) for arguments on this and Acemoglu and Robinson (2008) for a model of endogenous political persistence. For a policy discussion see Economist (2015).

[2] This argument is based on the original agency model of Barro (1973). Evidence that increasing the term-length of politicians improve their performance is provided by Dal Bó and Rossi (2011).

[3] For evidence on the perpetuation of political power see Dal Bó et al. (2009) and Querubin (2013).

We build measures of political dynasties for mayor office in Brazilian municipalities by matching the surnames of candidates from 1996 to 2012. This algorithm enables identification of candidates who had a relative in the office in the past and in the future. The candidates with a relative in office in the future are the ones that succeed in making their dynasty persist while the candidates with a relative in office in the past are the ones who benefit from the electoral advantages dynasties confer to them. We then use data on the presence of relatives in the future to investigate self-perpetuation in Brazil's local governments. We provide evidence that winning a close election for mayor increases the likelihood of having a relative in office in the future in almost 60 percent. These results confirm the existence of self-perpetuation in Brazilian municipalities and are in line with the results found by Dal Bó et al. (2009) and Querubin (2013).

After establishing that persistence of political power is also important for local governments in Brazil, we estimate the consequences of political dynasties for the quality of government. We focus our attention in the sample of electoral races between dynastic versus non-dynastic candidates to assess the effects of political dynasties on government quality. Our estimates indicate that municipal governments spend, on average, 8 percent more in municipalities in which a dynastic candidate won a close election compared to municipalities in which a dynastic candidate lost a close election. The increase in spending is concentrated in capital expenditures which are 16 percent higher for dynastic mayors and in areas related to education, health, sanitation and housing and urban development. We then test whether the increase in expenditures affect local economic performance and the quality of urban infrastructure and public services in education and health. Despite the larger expenditures, we find no significant differences in economic growth, improvements in urban infrastructure, student learning or health indicators between municipalities governed by a dynastic mayor compared to localities with no dynastic mayor.

We interpret our results as evidence that political dynasties deteriorate government performance, leading to larger governments with no significant gains in economic performance or public goods provision. Government expansion seems to be a mechanism that dynastic politicians use to increase rents and transfer resources to supporters (either through government contracts or patronage). Thus, our findings are related to Caselli and Michaels (2013) and Monteiro and Ferraz (2010) who find that spending driven by oil revenues in Brazil do not improve public goods and services. Another potential interpretation of the lack of improvements in public service delivery is that dynastic mayors face electoral advantage but have lower quality. We test for this alternative interpretation of our results by examining whether dynastic politicians are different in observable characteristics from

non-dynastic politicians in their levels of schooling, occupation, age, and gender. Using a Regression Discontinuity Design, we find that dynastic politician that win a close election have similar years of schooling and occupation patterns compared to non-dynastic politicians. They are, however, much more likely to be a woman.

Our results complement recent work that examine whether dynastic politicians perform differently in office. Differently from Labonne and Querubin (2015) we find that dynastic politicians spend more and differently when they get to power. But unlike Besley and Reynal-Queirol (2015), we do not find evidence that dynastic leaders increase economic growth in a context where there are little constraints on the executive as it is the case of Brazil's local governments. Our results are in line with Rossi (2015) who finds that dynastic legislators in Argentina perform worse in congress. Our interpretation of rent extraction are in line with recent studies that examine whether politicians generate rents for their relatives (see Folke et al. (2015) and Gagliarducci and Manacorda (2014)). Finally, our estimates on political selection complement the work of Geys (2015) and Labonne and Querubin (2015) on whether dynastic politicians have different observable characteristics.

Our results are robust to several robustness tests. First, we show that there are no meaningful differences in baseline characteristics in municipalities in which dynastic politicians win close elections and municipalities in which they lose close elections to non-dynastic candidates. There is also no evidence of sorting around the threshold of the treatment variable. These results mitigate concerns that these close elections are not random as discussed elsewhere in the literature (see Vogl (2014) and Eggers et al. (2015)). Another concern about our estimates is that politicians considered dynastic by our algorithm are not relatives but share the same last name. To address this issue, we drop from our sample the 7 most common last names and report all results using this more conservative measure of political dynasties.

The remaining of the paper is organized as follows. Section 2 discusses the institutional background and explains the construction of the datasets used in our analysis. Section 3 presents the results on self-perpetuation and Section 4 shows the results on the effects of political dynasties on economic outcomes. Finally, Section 5 presents our concluding remarks.

2 Institutional Background and Data

As in many other developing countries, family politics is common in Brazil. Ferraz and Finan (2009b) document how political dynasties have persisted over many decades in Brazil and show how the historical presence of political dynasties are negatively correlated with local development outcomes. One of the best illustration of powerful families in Brazil is the Sarney family. They have ruled the state of Maranhão for many decades. José Sarney was a governor of the state of Maranhão and later, the president of Brazil in the transition to democracy. His daughter Roseana Sarney was elected as a governor and a senator and his son José Sarney Filho was elected as a congressman and served as a minister. They own TV and radio stations in the state and have significant control over local politics.

A report produced by *Transparencia Brasil* – a Brazilian NGO– found that among legislators elected to congress in 2010, 44 percent had some family member in politics (228 out of 513). Out of those 53 percent obtained power through hereditary rule being the son, daughter, nephew or grandson of a politician. For elected senators, 66 percent have a family member in politics. As evidence of the importance of power transmission within the family, *Transparencia Brasil* show that many of these politicians use the name of their family members in the ballot-box even tough they were registered under different surnames. An example is Zeca Dirceu, the son of Jose Dirceu an ex-legislator and ex-minister, whose original name is Jose Carlos Becker de Oliveira e Silva. Another example is Andre Moura, the son of two ex-legislators Reinaldo Moura and Lila Moura, whose original name is Andre Luis Dantas Ferreira.

Brazil's legislation limits the inheritance of political power from family ties by prohibiting immediate relatives of incumbent politicians (mayors or governors) to succeed them in power. Nevertheless, there is a loophole in the legislation that allows relatives to run for office if the incumbent politician resigns six months before the election. Thus, many relatives of incumbent mayors run for office even when the mayor faces a term-limit.

2.1 Identifying Local Political Dynasties

Political dynasties are also widespread in the municipal level, although detecting them is more complicated as less data is available and many politicians that are related do not share the same last name. Because we do not have information on political families, we

exploit the structure of Brazilian surnames to build a proxy for political dynasties. The name of a Brazilian citizen is composed of:

First Name Mother's Last Name Father's Last Name,

In most cases, Mother and Father's Last Name are the last name inherited from their respective fathers. But married women can choose to maintain their name or add to their original name their husband's last name, in which case their name will take the form of:

First Name Mother's Last Name Father's Last Name Husband's Last Name.

We assume that politicians that have a common last name belong to the same family. We then match each candidate's surname with the surnames of the mayors both in previous and in future elections. This matching procedure enables us to build two measures used in our analysis. The first is an indicator for dynastic persistence which equals one for politicians whose relatives are in office in the same municipality in the future (P). The second is an indicator for a dynastic candidate which equals one if the candidate had a relative in office in the past (D). The former variable is used to assess the effect of incumbency on dynastic persistence, while the latter is used to investigate the consequences of political dynasties. Both analysis are based on the empirical strategies used previously by Dal Bó et al. (2009) and Querubin (2013).

The variables P and D are constructed matching surnames of all candidates for mayor in the 1996, 2000, 2004, 2008, and 2012 municipal elections with the surnames of elected mayors for the period 1988 to 2012. The variable P can be constructed for 1996 to 2008 as its construction requires information of at least one subsequent election. On the other hand D is constructed for 1996 to 2012 elections, as 1996 is the first year where we have information on the last name of all candidates for office.[4]

To illustrate our matching procedure we display an example in Figure 1 for the municipality of *Felício do Santos* in the state of *Minas Gerais*. Our algorithm identifies *Joaquim Veloso Pinto*, the elected mayor in 1992 and 2004 and the runner-up in 2000, as dynastic because he shares a surname name with *Miguel Arcanjo Veloso* who was in office between 1988 and 1992. *Miguel Arcanjo Veloso* is elected for office again in 1996 and our algorithm identifies him as dynastic because he shares a surname with *Joaquim Veloso Pinto*.

Figure 2 illustrates the construction of our persistence measure P. We consider *Miguel Arcanjo Veloso*, elected candidate in the 1996 elections, a politician that is able to make

[4]The 1988 election was the first municipal election to take place after Brazil transitioned from dictatorship to democracy. For the 1988 and 1992 elections, we only have the name of the elected mayor.

his dynasty persist since he shares his last name with *Joaquim Veloso Pinto*, who was the mayor between 2004 and 2008. However, *Jose Raimundo Rocha*, the runner up in the same electoral race, is not considered to form part of a dynasty, as he does not share a surname with candidates elected in the subsequent electoral races.

The previous figures also display some of the challenges in constructing the measures of political dynasties. First, composite names are common in Brazil and it is important not to mistake them for surnames. In *Joaquim Luiz Oliveira*, *Joaquim Luiz* is the name and *Oliveira* the surname, whereas, in *Joaquim Veloso Pinto*, *Joaquim* is the name and *Veloso* and *Pinto* are two different surnames. We adapted the algorithm to consider these cases and then checked its accuracy manually for more than 10,000 observations. Also, our algorithm does not code candidates as having a relative in office in the future or in the past if the candidate shares the surname with himself. Thus, our measure of dynasties does not include the same candidate running for office in the future.

While we are able to detect a large number of dynastic candidates with our matching procedure, it is important to note that measurement error still remains for two reasons. The first is that individuals who share the same last name might not be relatives. The fact that dynasties are restricted to the municipal-level reduces this concern since the median municipal population is around 10,000 inhabitants, and only a small share of the population is involved in local politics. Nevertheless, it is still possible that some of the matches do not identify relatives. To address this issue, we drop matchings based on the seven most common surnames in our data and show that our results are robust to this more strict measure. A second concern with the matching procedure is that politicians with different last names might actually be relatives. This could be the case of the wife of a politician, her cousin and nephews who might have different names even tough they are related. If this is common, we might be underestimating the incidence of local political dynasties.

In Figure 3 we show the distribution of dynastic candidates and those elected for mayor office from 1996 to 2012. The share of dynastic candidates and those elected increase over time because we have a short-panel to measure this type of persistence. In 2012 more than 20 percent of candidates for mayor are dynastic and among the candidates, almost half of them get elected as mayors.

2.2 Municipal Governments in Brazil

Our study focuses on political dynasties at the municipal level. Municipalities are the smallest administrative division in Brazil and they are responsible for the provision of a broad set of public goods and services such as elementary schools, health clinics, and most urban infrastructure projects such as road building and sanitation. Municipal governments finance the provision of public services through taxes collected at the local level and transfers from the state and federal governments. Transfers represent the largest share of government revenues, especially in small municipalities with limited bureaucratic structure and tax capabilities. Some of these transfers are defined in constitutional rules, whereas others are discretionary and result from a bargaining process between local officials and state and federal officials.[5]

Brazilian municipalities are governed by a mayor (*Prefeito*) and local legislature (*Camara de Vereadores*) elected for a four-year term. While local legislators can get reelected indefinitely, mayors face a two-term limit.[6] Mayors are responsible for proposing a budget, negotiating partnerships with state and federal governments and managing the provision of public services. They are monitored by local legislators (*Vereadores*) who vote on the municipal budget and legislate on local affairs concerning taxation, public policies and urban organization. Therefore, their support is essential to enable mayors to implement public policies.[7]

Corruption and the mismanagement of public resources are one of the main challenges faced by Brazil's local governments. There is widespread embezzlement of resources that should go to education, health, and urban infrastructure. Electoral incentives and the potential for punishment of corrupt mayors in the polls play an important role to generate accountability. However, political power induced by political dynasties might generate incumbency effects that allow family politicians to mismanage resources and stay in power.[8]

[5]See Arretche (1999) and Afonso and Araújo (2000) for an overview of municipalities' responsibilities and financial structure.

[6]Mayors were allowed to run for reelection starting in the 2000 election.

[7]See Ferraz and Finan (2009a) for more details on the performance of local legislators in Brazil.

[8]See Ferraz and Finan (2011); Ferraz and Finan (2008); and Brollo et al. (2013) for evidence on how electoral incentives interact with corrupt practices in Brazil.

2.3 Data Sources

We use electoral data for 5 municipal elections, from 1996 to 2012, obtained from the Federal Electoral Commission (*Tribunal Superior Eleitoral*). The data includes information on all candidates running for office in municipal elections, the votes they obtained and their characteristics such as gender, schooling, age, and previous occupation. Starting in 2004, we also have information on campaign spending for each candidate. Our data includes more than 15,000 candidates for mayor in each municipal election in more than 5,300 municipalities. The specific number of observations varies across elections since the number of candidates and the number of municipalities change over time. Prior to 1996, detailed electoral data is not available from the Federal Electoral Commission. Thus we gathered information from the State Electoral Commissions (*Tribunais Regionais Eleitorais*) on the name of mayors elected in 3,800 municipalities in 1988 and more than 4,800 municipalities in 1992. Our data covers 21 out of the 25 states in 1988 and 25 out of the 26 in 1992.

Our measures of government quality includes information on a variety of policies and outcomes. We start by gathering administrative data on government expenditures and revenues reported by municipalities to the Ministry of Finance (*Secretaria do Tesouro Nacional*). This dataset named *FINBRA* includes information on all revenues and spending for more than 5,000 municipalities. We distinguish between capital expenditures (i.e. investment in buildings and infrastructure) versus current expenditures (i.e. salaries of public servants). We also gather information on revenues classified as either arising from local taxes (i.e. taxes on property and services) or from transfers from the federal and state government (i.e. block grants). Information on different categories of expenditures, such as education, health, urban infrastructure, is obtained from the same source.

Because it is difficult to assess the quality of government from spending patterns due to the possibility of corruption and mismanagement, we also put together information on outcomes affected by government policies in the spirit of Caselli and Michaels (2013) and Ferraz and Finan (2009a). First, we use municipal GDP estimates from Brazil's statistical office *Instituto Brasileiro de Geografia e Estatastica* to examine how dynastic politicians affect local economic growth. Second, we gather information on the number of firms, number of employees, and salaries in the formal labor market both in the private and in the public sector from the *RAIS*, a matched employer-employee dataset administered by the Brazilian Ministry of Labour.

In order to map public spending into public goods provision, we gather data on education and health outcomes as well as on local infrastructure. Information on education on class

size, age-grade-distortion, and *Prova Brasil*– the national standardized test score– come from Brazil's Education Statistics Institute (INEP). From the *Prova Brasil* data, we compute the average test scores in language and mathematics and standardize based on an yearly basis. From the educational census (*Censo Escolar*) administered by INEP, we compute the average class size and the age-grade distortion. All educational outcomes are calculated for fifth grade students enrolled in municipal schools. We use data from *Prova Brasil* for the periods of 2007 and 2011 and data from the educational census for the periods 2007-2008 and 2011-2012. Thus measures of schooling outcomes are taken in the end of mayor's electoral terms. The health outcomes are obtained from the Brazil's public health system, the *Datasus*. We build measures of the share of pregnant women that had frequent prenatal visits during pregnancy, the share of low weight births, and infant mortality. For each measure we compute the average for each electoral term (i.e. 2005 to 2008 and 2009 to 2012).

Data on local infrastructure comes from the Brazilian census. We construct measures of infrastructure such as the percentage of households with paved roads, open sewage, and garbage collection. This data is just available for the 2010 census. We also use data from the Brazilian census to gather information on basic demographics at the baseline such as population, urbanization, income per capita, and schooling. We compute the baseline information using data from the 2000 census.

In most of our estimations we use per capita measures based on estimates of the local population provided by Brazil's statistical office (*Instituto Brasileiro de Geografia e Estatastica*). We average all the variables throughout the electoral term whenever there is information for more than one period of each term. The appendix provides additional information on the sources and the construction of all variables used throughout the paper.

3 Political Power Persistence

We begin our analysis by testing whether political power increases the likelihood of having a relative in office in the future. Because politicians that win and lose elections are different in many dimensions such as their talent, wealth, and political capital, a simple OLS regression will yield a biased coefficient on the probability of posterior political power. Also, to the extent that personal characteristics persist within families, these differences can create a positive association between winning an election and having a relative in office even in the absence of dynastic persistence (Dal Bó et al., 2009).

Hence, we follow Dal Bó et al. (2009) and Querubin (2013) and use a Regression Discontinuity Design to estimate the persistence in political power. We examine whether winners in close elections have a higher probability of having a relative in office in the future compared to runner-ups using the following linear model:

$$P_{imt} = \alpha + \beta W_{im} + f(v_{im}) + \varepsilon_{imt}, \tag{1}$$

where P_{imt} is a indicator variable for whether candidate i in municipality m has a relative in office in year t, W_{im} is an indicator for whether candidate i won the election in municipality m in 1996, and $f(v_{im})$ is a function of the running variable v_{im} that represents the margin of victory of candidate i in municipality m for the 1996 election.

Following Imbens and Lemieux (2008) and Lee and Lemieux (2010), our preferred specification is based on a local linear regression using Imbens and Kalyanaraman (2012) to select the optimal bandwidth. We estimate the model for observations within the bandwidth using a linear spline. As an alternative specification, we approximate the control function using a fourth order polynomial with splines and restrict the analysis to observations with winning margins between -0.5 and 0.5.

Because there is a legislation in Brazil that prohibits mayors' direct relatives from succeeding him in office, winners should have a mechanical lower probability of having a relative in office compared to their runner-ups in the following election. Moreover, mayors can run for reelection hence the effect of power persistence might only appear in the following election. Thus it is important to estimate persistence over several subsequent elections.

Figure 4 presents a graphical description of the results on dynastic persistence for four subsequent election: 2000, 2004, 2008 and 2012. Each figure depicts the proportion of relatives in office in the future against the vote margin of the candidate in the 1996 municipal election using bins of 5 percent vote shares using a quartic polynomial fit of the relationship between dynastic persistence and vote margin.

Panel (A) reveals a negative relationship between the candidate winning the 1996 election and a relative succeeding him in the 2000 election. This result is due to the fact that winners themselves can run for reelection in 2000 and even if mayors decide not to run for reelection, relatives cannot run. Panels (B) and (C) depict the same relationship for the 2004 and 2008 elections. While there is no effect for the 2004 election (Panel B), the effect seems positive but small for the 2008 election (Panel C). But these discontinuities are small and there is no evident dynastic persistence either after two or three electoral races.

In Panel (D), on the other hand, we observe a positive and significant dynastic persistence after four electoral races. Candidates who won a close election in 1996 are more likely to have a relative elected for office in 2012 compared to candidates who lost a close election in 1996. These results indicate that political power of families persist over time, confirming that the results from other settings such as the United States and the Philippines are also present in the context of Brazil (see Dal Bó et al. (2009) and Querubin (2013)).

Table 1 reports the RD estimates of political persistence. Columns 1 and 2 present the mean of the persistence variable in each subsequent election among the candidates who won and lost the 1996 municipal election, respectively. The probability of a relative of the winner to be in office increases over time from 3% in 2000 to 7.2% in 2012, while the probability of a relative of the runner-up to be in office remains constant around 4.5% across all periods.

Column 3 depicts the difference in the means reported in the previous columns. The coefficient is negative in 1.8 percentage points for 2000, consistent with the fact that relatives of the winners in 1996 are not allowed to run in 2000. The estimate becomes positive in 2000 and remains positive in 2004 and 2008. The magnitudes increase from 0.8 percentage points in 2004 to 2.5 percentage points in 2004 and 2.4 percentage points in 2008. These statistics alone suggest that there are substantial differences in persistence over time between winning and losing candidates. However, as we discussed before, these differences can either reflect incumbency advantage or innate differences in political capital across families.

Columns 4 and 5 present RD estimates comparing candidates who won or lost close elections. To the extent that close elections are random, these candidates are similar in both observable an non-observable aspects and the comparison between these groups reflects dynastic persistence. Column 4 reports local linear regression estimates using the Imbens and Kalyanaraman (2012) optimal bandwidth and column 5 reports polynomial spline estimates using a fourth order polynomial and a 0.50 bandwidth.

The estimates for persistence in 2000 confirm the intuition of the mean comparisons. Relatives of the winning candidates in 1996 are less prone to be in office in 2000 than relatives of losing candidates in 1996. However, the estimates for persistence in 2004 and 2008 are quite different from the ones from column 3. These findings confirm the graphical intuition of no persistence in these periods and indicate that the mean differences reflect differences in talent, drive and political capital across families and not dynastic persistence. The RD estimates for the last period confirm the intuition of persistence after some

periods. The likelihood of a relative of the winning candidate in 1996 to be elected in 2012 is 2.8 percentage points. The magnitude of the coefficient is substantial and suggests that being in office increases the likelihood of having a relative in office in the future in almost 60 percent.

The results presented above suggest that dynastic persistence is relevant in our data, as it creates electoral advantages to members of certain families. This raises concerns about electoral competition and political incentives which justifies investigating the effects of these families on government policies.

4 The Effects of Dynastic Politicians on Policies

In the previous section we showed that politicians that are elected by a small margin are more likely to have a family member in office in the future. While this type of political persistence might reduce political competition and compromise the functioning of democracies, it is unclear whether it has negative consequences for the way local governments are run. In this section we assess whether dynastic politicians behave differently in power when compared to non-dynastic politicians and whether this affects economic outcomes such as GDP and employment.

We start by showing in Table 2 the characteristics of localities where political dynasties are present compared to those without political dynasties. All characteristics are drawn from the 2000 population census. Municipalities that have dynastic mayor are smaller, more urban, have a less educated population, and are poorer in terms of GDP per capita. They are less likely to have local institutions that can foster accountability such as local media (i.e. radio) and judiciary courts. All these differences are statistically significant and some of the differences are sizable. Thus a simple comparison of policies and economic outcomes between municipalities with and without dynastic politicians will yield biased estimates of the effects of dynasties. Weak local institutions might drive both the existence of political dynasties and the incentives to adopt bad policies.

We circumvent this potential bias using a Regression Discontinuity Design where we compare outcomes in municipalities where a dynastic candidate won by a small to those where a dynastic lost by a small margin. Our empirical strategy is based on the assumption that close elections provide variation that is as good as random allowing us to control for observable and unobservable municipal characteristics that might differ between localities

with and without a dynastic politician.[9] Specifically, we estimate the following linear regression:

$$y_{mt} = \alpha + \gamma D_{mt} + f(v_{mt}) + \delta X_m + \lambda_t + \varepsilon_{mt} \quad (2)$$

where y_{mt} is the policy or economic outcome of municipality m during the electoral term t, D_{mt} is an indicator for whether the municipality is governed by a dynastic mayor (i.e. someone who belongs to the same family of a politician that has been in power previously), X_m is a set of pre-determined municipal characteristics such as population and income per capita, λ_t is a time dummy and ε_{mt} is an error term. The variable v_{mt} is the difference in vote share between the dynastic and the non-dynastic candidate and it is used as the running variable in all specifications. The function $f(.)$ is a smooth function of the margin of victory and includes an interaction between the dynastic indicator D_{mt} and the margin of victory v_{mt}. The parameter of interest γ captures the difference in policy y in municipalities with and without a dynastic mayor.

We estimate our model with two different econometric specifications: our main results are based on a local linear regression using observations for an optimal bandwidth based on Imbens and Kalyanaraman (2012). In this specification, we allow $f(v_{mt})$ to vary in each side of the discontinuity. We also report results from estimating a fourth order polynomial with a spline in each side of the discontinuity. We include election fixed effects and a set of pre-determined municipal characteristics as controls to gain precision, but all results are robust to models where we do not control for municipal characteristics. In all specifications standard-errors are clustered at the level of the municipality.

The model described above is estimated using data for the 2004 and 2008 municipal elections and for each election we identify dynastic candidates as the ones with relatives in office in previous mandates. We restrict our sample to outcomes for the 2005-2008 and 2009-2012 mandates because these are the periods for which we can measure dynasties using four previous elections and we have data on economic outcomes for the whole electoral term. We exclude from our sample municipalities in which dynastic candidates were neither the winner nor the runner-up and municipalities in which both the winner and the runner-up are dynastic.

Under this application of the Regression Discontinuity Design, it is important to notice two things. First, the estimated effects represent local estimates of dynastic politicians on economic outcomes for municipalities that are highly competitive. These effects might be quite different in localities where dynasties win by a large vote margin. From a theoretical

[9] See Lee (2008) for the original application of Regression Discontinuity to elections.

perspective, it is unclear whether the RD estimates are larger or smaller than average effects. On the one hand, political dynasties facing less electoral competition can be less accountable and therefore perform worse than dynasties facing a lot of competition. On the other hand, if voters select the best families into power, political dynasties facing less electoral competition could be the ones with better performance.[10] In the appendix we present estimates on the effects of political dynasties using an alternative econometric specification that does not rely on close-elections. We estimate difference-in-difference models exploiting changes in policies in municipalities that elect a dynastic politician compared to those municipalities that do not elect a dynastic politician.

A second concern is that, while the RD design controls for unobserved characteristics at the municipal level, it cannot control for unobserved characteristics of politicians, parties, or efforts of mobilization. Because dynastic politicians have a natural incumbency advantage (see Dal Bó et al. (2009) and Querubin (2013)), a dynastic candidate that barely wins an election might be, on average, worse than an average candidate. Thus, we might be comparing candidates with different quality.[11] In order to shed light on this selection mechanism, we analyze in section 4.3 whether elected dynastic candidates are different in observable characteristics.

Before showing the results, we present evidence that indeed our RD design can be considered as good as random in terms of municipal characteristics. We use pre-determined municipal characteristics such as population, proportion of rural households, and income per capita for either 1999 or 2000– at least 4 years before the dynastic election occurs– and test whether they exhibit a discontinuity. Figure 5 provides evidence that municipal characteristics are smooth in the discontinuity that determine whether the municipality is governed by a dynastic politician. Each panel plots an outcome averaged in 5 percent bins against the vote margin of the dynastic candidate. The panels also report a quartic polynomial fit of the relationship between the outcome and vote margin. In Table A1 in the Appendix we report estimates of these discontinuities using both a local linear regression using the Imbens and Kalyanaraman (2012) bandwidth (Panel A) and a quartic polynomial spline for a 0.50 bandwidth. The results confirm the graphical intuition that pre-determined variables are balanced around the winning margin threshold. We complement the analysis on pre-determined characteristics with a test of sorting in the discontinuity. In localities where institutions are weak, dynastic politicians might be more likely to commit some type of electoral fraud and affect electoral outcomes in very

[10] See Besley and Reynal-Queirol (2015).

[11] See Vogl (2014) for a related point made for black mayors in the US.

close elections. This would invalidate the comparison of close-elections between dynastic and non-dynastic candidates as a quasi-randomized empirical strategy. We test for the potential manipulation of elections around the discontinuities using the test proposed by McCrary (2008). In Figure 6, we plot the densities of the vote margin and find no evidence of manipulation of the winning margin by dynastic candidates.

4.1 Political Dynasties and Government Expenditures

We start our analysis by examining whether there are differences in public spending between dynastic and non-dynastic mayors. Figure 7 presents graphical evidence on the effect of political dynasties on the overall pattern of government expenditures and aggregated categories such as capital versus current expenditures. As we can observe in the figures, dynastic politicians do not seem to chose different levels of government spending compared to non-dynastic politicians in similar municipalities. But these politicians do seem to spend more on capital rather than current expenditures.

The regression results for these variables using different Regression Discontinuity specifications are presented in Table 3. Panel (A) reports local linear regression estimates using the Imbens and Kalyanaraman (2012) optimal bandwidth and Panel (B) presents fourth order polynomial spline estimates using a 0.50 bandwidth. All estimates include time fixed-effects and municipal controls to improve precision. Standard-errors are clustered at the municipality level. The coefficient in column (1) suggests that dynastic mayors spend, on average, 8 percent more than non-dynastic mayors. This corresponds to an increase of R$124 to R$149 in expenditures per capita with respect to an average of R$1700.[12] Columns (2) and (3) provide evidence that the increase in expenditures is higher in capital than in current expenditures. Dynastic mayors spend, on average, 16 percent more on capital expenditures compared to non-dynastic mayors. These mayors also spend more on current spending, but the difference is much smaller (4.7 percent) and the effect is non-significant in the polynomial specification.

Given that dynastic mayors spend more resources, we examine the source for these additional expenditures. Brazilian municipalities have two main sources of financing: local taxes and transfers from the central government.[13] Columns (4) and (5) test whether dynastic mayors collect more taxes and receive more transfers from the central government. We find that dynastic mayors receive, on average, 6 to 7 percent more transfers from the

[12] All values are deflated to December 2012. The average R$ to US$ exchange rate was 2.08.

[13] The possibility of running a deficit is limited under Brazil's fiscal responsibility law.

central government compared to non-dynastic mayors. The effects on taxes are similar in magnitude but not significant. Because approximately 65 to 70 percent of municipal revenues come from transfers, we conclude that capital expenditures are mainly financed through more transfers from the central government.

Next, we examine in which areas local governments increase expenditures. Figure 8 provides a graphical illustration of the effect of political dynasties on the four most important categories of spending: administration and planning, education and culture, healthcare and sanitation and housing and urban development. Dynastic politicians seem to spend more on health and sanitation and housing and urban development, expenditures that are intensive in public works. Table 3 provides regression results for the four categories presented in the figure as well as for social assistance and transportation. Together these six categories comprise more than 90 percent of total expenditures. Dynastic mayors spend more resources in education, health, urban development, and public transportation. The effects range from a 6 percent increase in education to a 57 percent increase in transportation. While the effect for education loses significance in the polynomial splines, all the other coefficients are robust to the alternative specifications. In terms of magnitudes, the largest differences are in urban development and health. Combined these categories correspond to a large share of government spending. These categories also include significant spending in public works such as building housing for poor families, urban infrastructure, health clinics, and sanitation.[14]

4.2 Political Dynasties, Economic Performance, and Public Goods

Our previous findings suggest that dynastic politicians increase the size of local governments, mostly through increases in infrastructure spending. We next examine whether municipalities that elect a dynastic politician perform better in terms of economic performance and whether the increase in government spending affects the quality of public services. We first use measures of municipal GDP growth and measures of growth in employment and the number of firms to examine economic performance. We then assess the quality of public services using data on education, health and urban infrastructure. We chose these three sectors because they correspond to most of the spending decided by local mayors and because these are the sectors in which we have shown that dynastic mayors spend more resources.

[14] In the appendix we test for the robustness of our findings for alternative bandwidths and functional forms.

The effects of a dynastic politician on economic performance using the regression Discontinuity Design from equation (2) are shown in Table 5. We present results from estimating a local linear regression with an optimal bandwidth (Panel A) and a quartic polynomial (Panel B). In columns (1) to (4) we show the effects on municipal GDP growth over the electoral term (four years). Municipalities grow on average 24 percent over a four year period. But we do not find significant differences in growth rate in localities where a dynastic politician won by a small margin compared to places where he/she lost by a small margin. These localities, despite higher spending in infrastructure, do not grow more. We then split growth into different sectors and estimate separate regressions for growth in agriculture, manufacturing, and services. We do not find any differences in performance across these localities. Finally, in columns (5) and (6) we show that localities with a dynastic politician do not seem to have a different performance in the labor market either. In column (5) we find that municipalities with dynastic mayors do not grow more in terms of employment (column (5)) or in terms of number of firms (column (6)). The lack of effects in economic performance are robust to different specifications, as those shown in Panel (B) where we estimate the RD model with a quartic polynomial. Alternative specifications in the appendix support this conclusion. Overall we find not evidence of a differential economic performance of dynastic mayors.

We next examine whether dynastic mayors affect the quality of public services. The results shown in Table 6 focus on outcomes for education, health and urban infrastructure. The first three columns show the results for education. We start by examining whether dynastic mayors use resources in education to reduce class size. Spending more on education could reduce class size through the construction of new classrooms and schools and through the hiring of additional teachers. We find that, despite a negative point estimate, municipalities with a dynastic mayor do not have significant smaller average class sizes for primary school children compared to municipalities without a dynastic mayor. The point estimate is very small suggesting no difference between municipalities with and without dynastic mayors. We next examine two different measures of educational quality. In column (2) we look at the age-grade distortion measured by the percentage of students that are lagging behind when comparing their grade to their age. In column (3) we look at test scores in a standardized national exam for 5th grade in Math and Portuguese.[15] We find that despite spending more resources in education, dynastic mayors do not improve educational outcomes in terms of both age-grade distortion and test scores.

Columns (4) to (6) investigate outcomes related to health. We follow Fujiwara (2015) and

[15]We use as outcomes the average of both grades standardized.

use information on the share of pregnant women with more than 7 prenatal visits (column 4) and the share of babies born with low birth weights (column 5). Fujiwara (2015) provides evidence that these health outcomes are responsive to short-term changes in policies and thus we would expect the changes in expenditures to improve these outcomes. Finally in column (6) we test whether infant mortality differs in municipalities that elect a dynastic mayor. Overall we find small point estimate and no significant differences in health outcomes between municipalities with and without a dynastic mayor despite that larger expenditures in health care.

The three last columns in Table 6 look at urban infrastructure. Because we only have indicators for the 2010 census, we test for different levels of urban infrastructure for mayors elected in the 2008 election. We look at the percentage of households that live in a locality with paved roads, open sewage, and garbage in the streets. These variables can be directly affected by mayors as they are responsible for these types of public goods. We do not find any difference between these three measures of urban infrastructure in municipalities that elected a dynastic mayor in close elections and those that did not despite the larger sums of capital expenditures targeted at urban infrastructure in these localities. Overall, we conclude that despite spending more resources on capital expenditures, and in particular health and urban infrastructure, dynastic mayors do not change the quality of these public services.

In sum, dynastic mayors spend significantly more resources in infrastructure compared to non-dynastic mayors with no effects on economic growth, employment growth, or improvements in the quality of a large number of public services. There are two direct interpretations of these findings. One potential interpretation is that dynastic mayors have significant political capital that allows them to divert resources and still remain politically competitive. This explanation would be consistent with the results of Brollo et al. (2013) who find that Brazilian mayors that receive an exogenous increase in their budget can get reelected and extract rents from corruption. The pattern of increasing spending with null effects on public services is consistent with previous work by Caselli and Michaels (2013) and Monteiro and Ferraz (2010) who show that increasing revenues based on oil revenues increased municipal government spending but did not translate into improvements in the quality of public services.

An alternative explanation for the increase in spending with no improvements in economic performance and public services is that dynastic mayors are drawn from a worse pool of candidates. Under this explanation, dynastic mayors spend more with no effects because of negative political selection. In the next section we examine this hypothesis

looking at whether elected dynastic mayors differ from non-dynastic mayors in observable characteristics such as years of schooling and occupation.

4.3 Political Dynasties and Selection

In this section we test whether dynastic mayors elected by a small margin differ from non-dynastic mayors in four characteristics that have been shown to affect policy outcomes: education, occupation, age, and gender. We focus on close elections where a dynastic mayor competes against a non-dynastic mayor and estimate a series of regressions using both a local linear regression and a polynomial spline specification similar to the ones used in the previous section. The results are presented in Table 7.

We start our analysis looking at whether dynastic politicians are less qualified than non-dynastic politicians. We use schooling and an indicator for whether the politician had a high skill occupation before the election as a proxy for political quality.[16] We find that dynastic mayors elected in close-elections do not have different levels of schooling or high skill occupation compared to non-dynastic mayors. The point estimate for schooling in column (1) is 0.63 (standard-error=0.59) in the local linear regression specification and 0.47 (standard-error=0.59) in the polynomial specification. Both coefficients are not statistically significant and represent less than a 5 percent difference in years of schooling with respect to the mean education level of 12.4 years. The point estimates for high skill occupation displayed in column (2) are also small and not statistically significant for both the local linear specification displayed in Panel (A) and the quartic polynomial displayed in Panel (B). Hence, at least in terms of observable skills, dynastic politicians are not different than non-dynastic politicians elected in very competitive elections. This result is related to Geys (2015) who find no evidence of lower education levels among directly elected dynastic mayors in Italian localities.

We next turn to two other characteristics that might differ between dynastic and non-dynastic politicians: age and gender. Because politicians in Brazil face a term-limit after two terms in office, we might expect incumbents to try to maintain political power by encouraging their wives and children to run for office. In Table 7, column (3), we show the results for age. We find no statistically significant difference in age between dynastic and non-dynastic politicians. The point estimates vary from -0.76 to -1.39 and both are quite small when compared to 47, the average age of mayors. Finally, in column (4) we test whether dynastic politicians are more likely to be a female. We find a large and robust

[16] See Ferraz and Finan (2009a), Brollo et al. (2013), and Besley et al. (2011) on use of education of leaders.

increase in probability of the mayor being female when a dynastic politician is elected in a close-election. The point estimates range from 14 to 16 percentage points (standard errors are 0.04 and 0.047 respectively) which represent a 100 percent increase in probability of electing a female mayors. This result is similar to the findings of Labonne and Querubin (2015) who show that in the Philippines a relatively high numbers of women elected as mayors are the result of dynastic women replacing mayors who face a term-limit.

5 Concluding Remarks

This paper uses data from Brazil's local governments to examine whether dynastic mayors implement different policies compared to non-dynastic mayors. We circumvent empirical concerns that municipalities where dynastic mayors get elected are different from those localities that do not elect dynastic mayors using a regression Discontinuity Design. We compare policies and performance of dynastic and non-dynastic mayors in similar municipalities and find that mayors that come from a family that had politicians in power in previous periods spend more resources, specially in infrastructure projects. We find, however, that this additional spending does not induce more economic and employment growth and does not improve the quality of urban infrastructure nor the quality of public services in education and health. We interpret our results as suggestive evidence that dynastic mayors, despite not implementing worse policies as many observers would argue, are likely to reduce the welfare of citizens by spending more resources without positive consequences.

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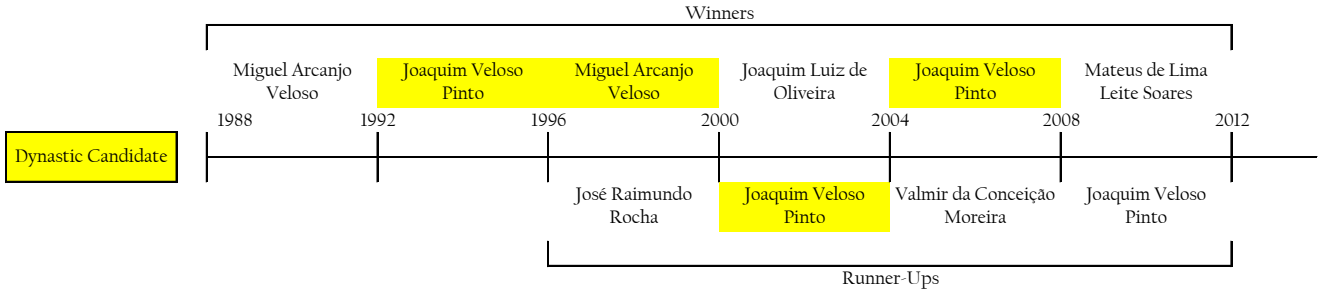

Figure 1: Example of dynastic Candidate *D*

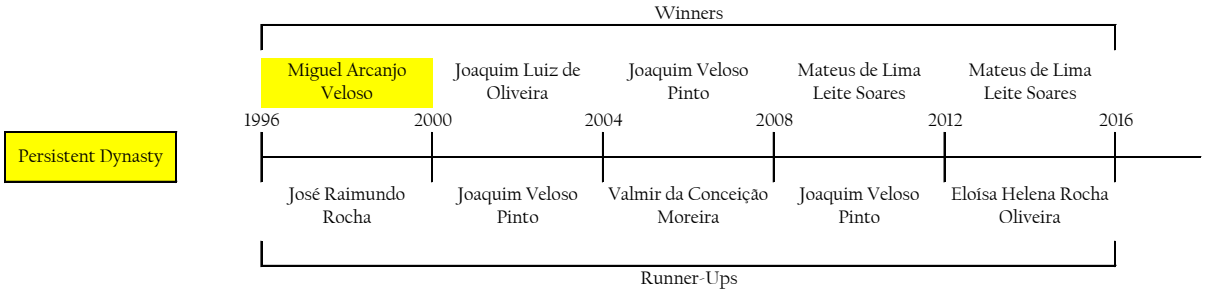

Figure 2: Example of dynastic persistence *P*

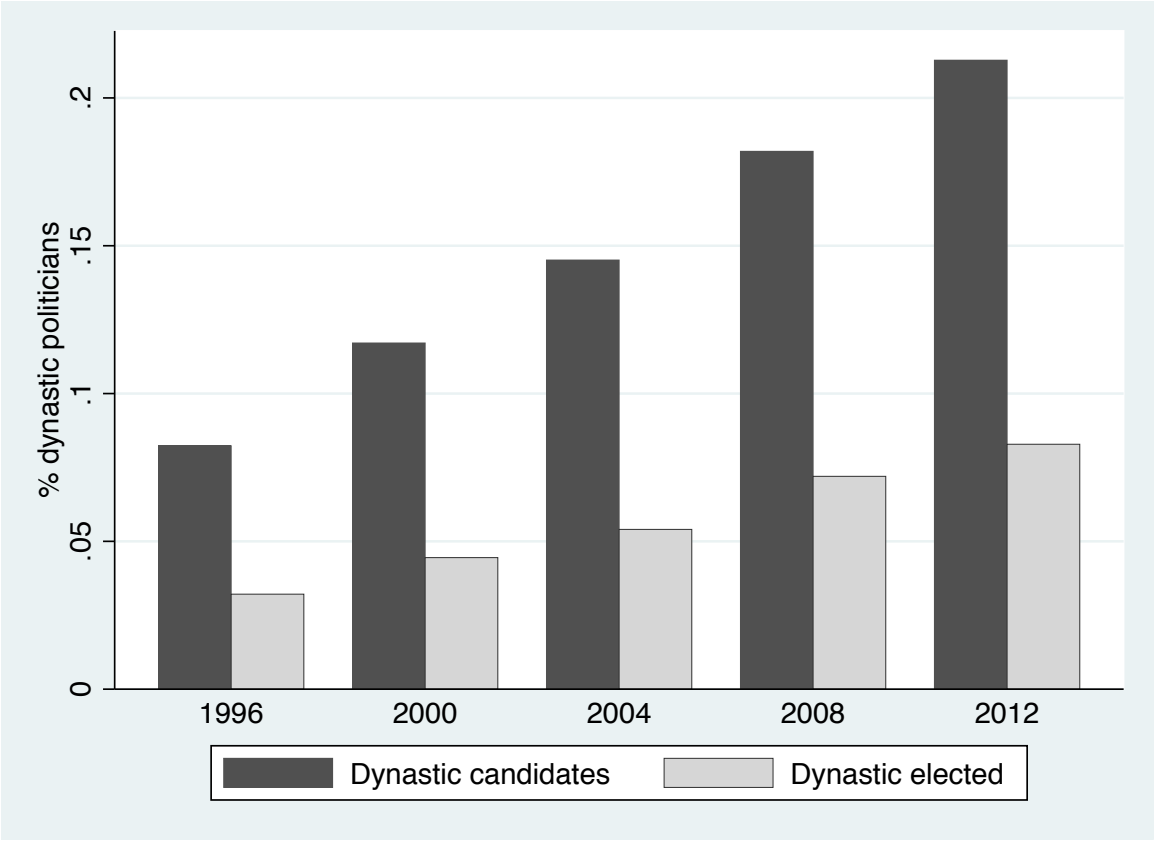

Figure 3: Dynastic candidates and elected mayors

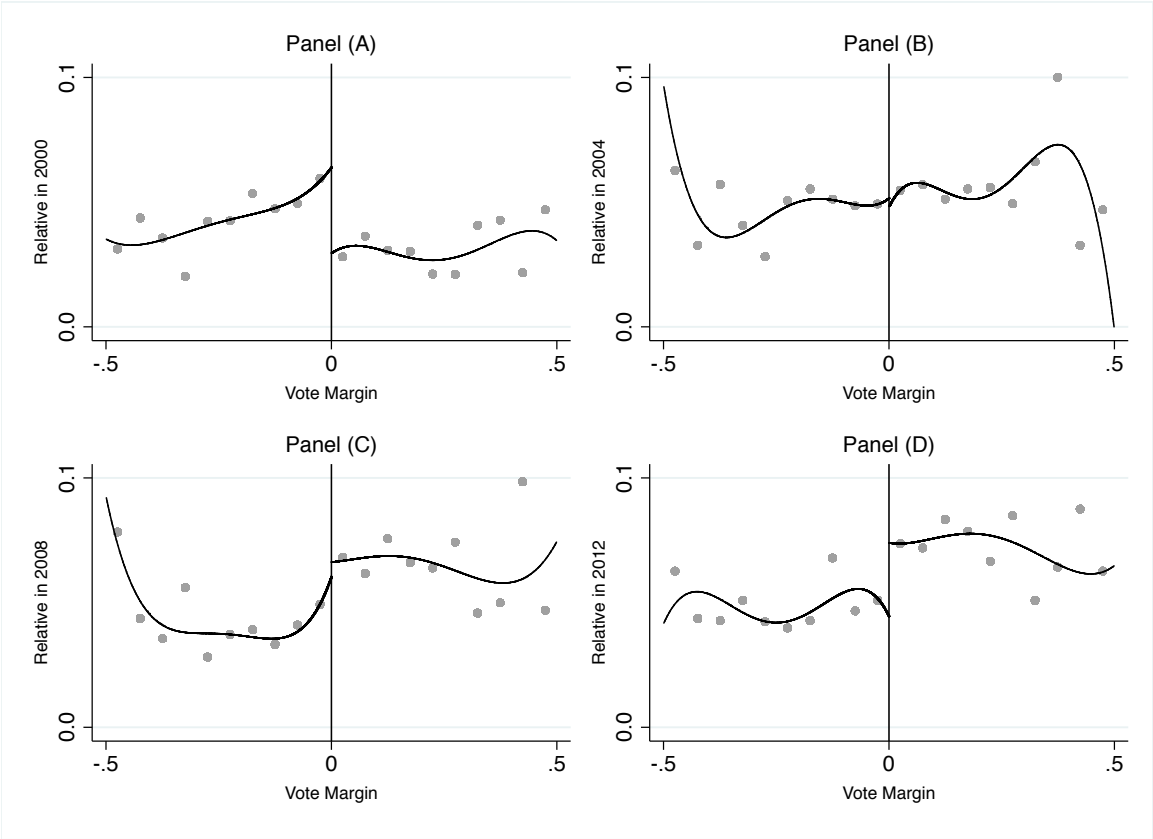

Figure 4: Dynastic Persistence - RD Graphical Illustration

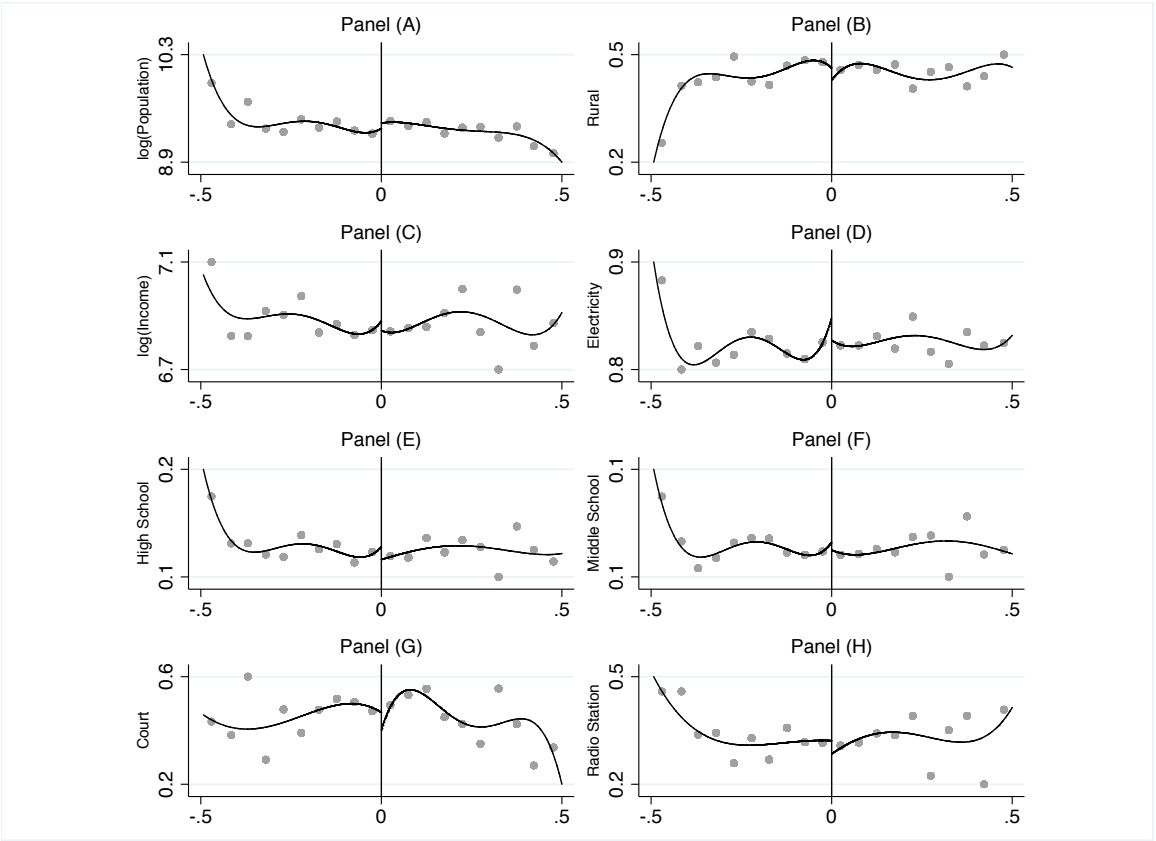

Figure 5: Balance in Pre-determined Characteristics

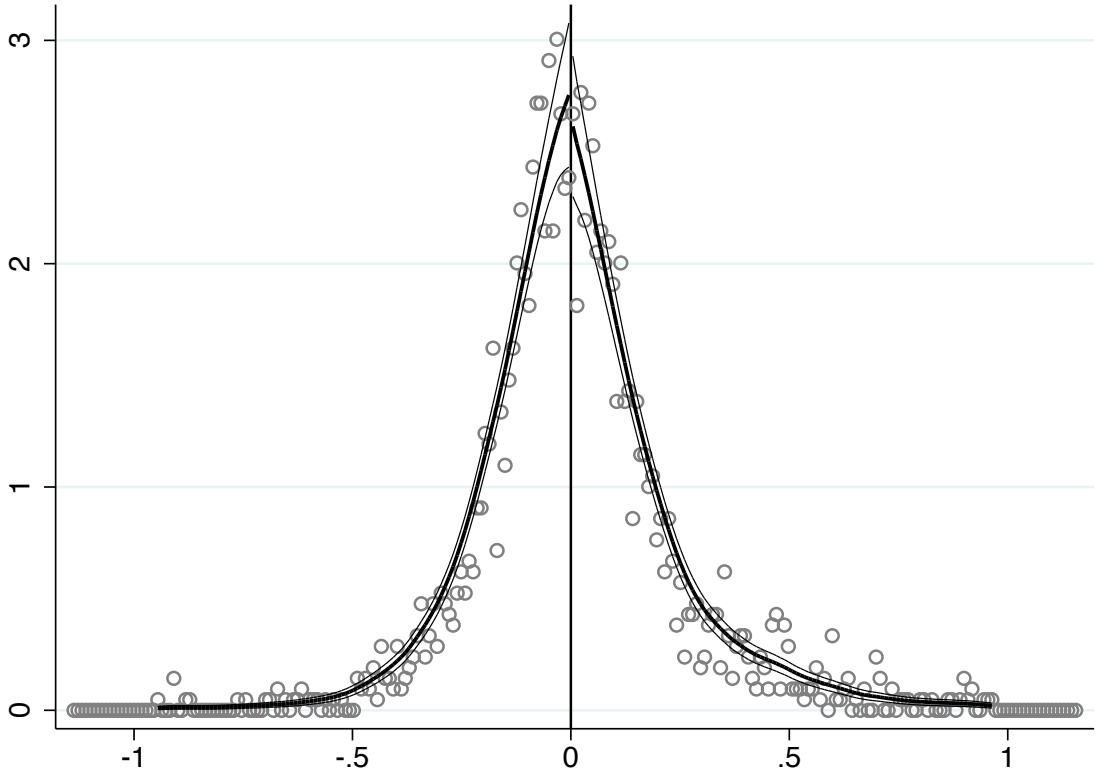

Figure 6: Testing for Sorting in the Discontinuity

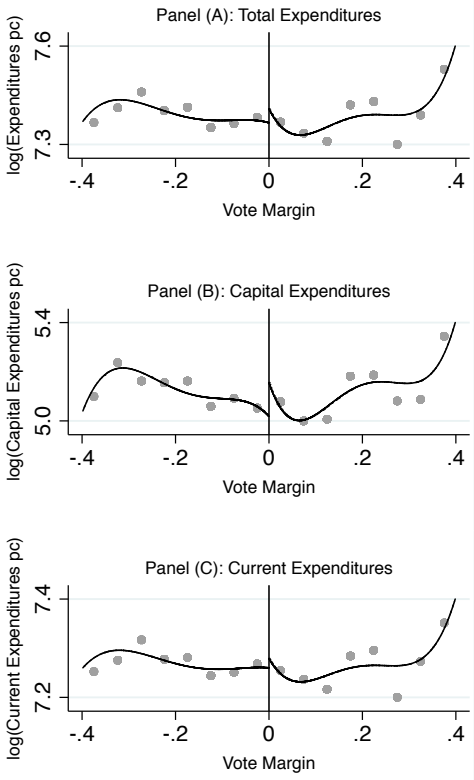

Figure 7: Effect of Dynasties on Expenditures - RD Graphical Illustration

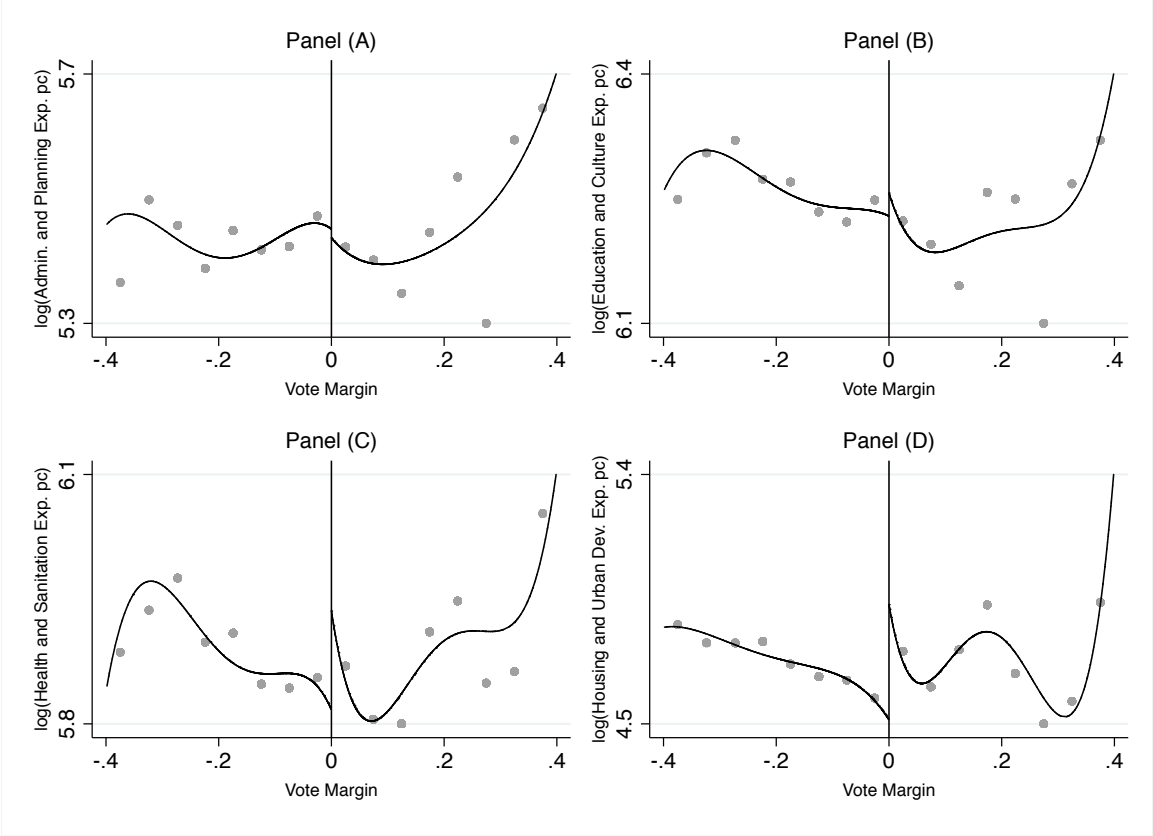

Figure 8: Effect of Dynasties on Expenditures Categories - RD Graphical Illustration

Table 1: The Effect of Winning on the Presence of a Relative in Office in the Future

	Mean			Effect of winning	
	Winner	Runner-Up	Difference		
	(1)	(2)	(3)	(4)	(5)
Relative Elected in 2000	0.030	0.047	-0.018***	-0.033***	-0.033**
	[0.002]	[0.003]	(0.004)	(0.011)	(0.014)
Bandwidth				0.101	0.50
Observations	5,207	5,207	10,414	4,974	10,154
Relative Elected in 2004	0.054	0.046	0.008*	0.002	-0.003
	[0.003]	[0.003]	(0.004)	(0.013)	(0.015)
Bandwidth				0.0885	0.50
Observations	5,207	5,207	10,414	4,482	10,154
Relative Elected in 2008	0.065	0.040	0.025***	0.010	0.005
	[0.003]	[0.003]	(0.004)	(0.012)	(0.015)
Bandwidth				0.112	0.50
Observations	5,207	5,207	10,414	5,398	10,154
Relative Elected in 2012	0.072	0.047	0.024***	0.028**	0.028*
	[0.004]	[0.003]	(0.005)	(0.012)	(0.016)
Bandwidth				0.131	0.50
Observations	5,207	5,207	10,414	6,012	10,154
Specification	-	-	-	IK Bandwidth, Linear	50% bandwidth, Quartic Polynomial

Notes: The unit of observation is a candidate in the 1996 municipal election. Sample is restricted to the winners and runner-ups of the election. The dependent variable indicates whether a relative of the candidate was elected for mayor in each subsequent period. Each cell in columns (1) and (2) reports the sample mean of the specified variable. Each cell in column (3) reports the difference of columns (1) and (2). Each cell in column (4) reports the coefficient from a local linear regression using the Imbens and Kalyanaraman (2012) optimal bandwidth. Each cell in columns (5) reports the coefficient from a regression using a quartic spline. Details on the construction of all variables are presented in the appendix. Standard errors are displayed in parentheses and standard deviations in brackets. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 2: Municipal Characteristics

	Average	Wo. Dyn. Mayor	With Dyn. Mayor	Difference
	(1)	(2)	(3)	(4)
Population in 1000s	30.837	33.443	18.667	-14.776**
	(186.767)	(206.096)	(30.492)	(6.220)
GDP per capita	8.982	9.485	6.951	-2.534***
	(10.310)	(10.786)	(7.752)	(0.345)
FM Radio Station Existence	0.339	0.338	0.341	0.002
	(0.473)	(0.473)	(0.474)	(0.016)
Local Courts	0.444	0.437	0.470	0.033*
	(0.497)	(0.496)	(0.499)	(0.017)
Literacy Rate	0.773	0.785	0.725	-0.060***
	(0.133)	(0.130)	(0.133)	(0.004)
Share of Urb. Pop.	0.588	0.598	0.549	-0.049***
	(0.233)	(0.236)	(0.217)	(0.008)
Theil Index	0.521	0.520	0.524	0.004
	(0.109)	(0.110)	(0.106)	(0.004)
Number of Obs.	5501	4397	1104	-

Notes: The first column presents the average across all municipalities. Columns (2) to (4) present the average among municipalities without and with mayors with relatives in previous offices in the same municipality and the difference, respectively. The radio station and the local courts presence are as of 1999. All the other variables are measured in 2000 before the beginning of our analysis. For details on the construction of all variables see the Appendix. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 3: Political Dynasties and Government Expenditures and Revenues

	log(Expenditures pc)	log(Capital Exp. pc)	log(Current Exp. pc)	log(Taxes pc)	log(Transfers pc)
	(1)	(2)	(3)	(4)	(5)
Panel A: Optimal Bandwidth					
Dynastic	0.082***	0.164**	0.047*	0.016	0.051*
	(0.031)	(0.072)	(0.028)	(0.054)	(0.028)
Observations	1,096	814	1,332	1,698	1,205
Bandwidth	0.102	0.073	0.131	0.198	0.116
Mean of Dependent Variable	1758.0	223.2	1527.0	100.2	1758.0
Panel B: Polynomial Spline					
Dynastic	0.068*	0.154**	0.056	0.030	0.066*
	(0.038)	(0.076)	(0.038)	(0.088)	(0.036)
Observations	2,188	2,188	2,188	2,188	2,188
Bandwidth	0.50	0.50	0.50	0.50	0.50
Mean of Dependent Variable	1772.0	228.6	1544.0	101.1	1781.0

Notes: The unit of observation is a municipality-term. Sample is restricted to municipalities in which either the winner or the runner-up is a dynastic candidate. Panel A reports the coefficients from local linear regressions using the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel B reports the coefficients from regressions using a quartic spline. The dependent variable in regression column is reported in the top of the column. All regressions include year fixed effects and the following controls: log of initial population, log of average household income, share of rural households, share of households with access to electric power, share of adults with 8 to 10 years of schooling, share of adults with 11 or more years of schooling, FM radio station existence and local court existence. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 4: Political Dynasties and Government Expenditures by Category

	log(Admin. and Planning Exp. pc)	log(Education and Culture Exp. pc)	log(Health and Sanitation Exp. pc)	log(Social Assistance Exp. pc)	log(Housing and Urban Dev. Exp. pc)	log(Transp. Exp. pc)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Optimal Bandwidth						
Dynastic	0.038	0.061*	0.095**	0.051	0.272**	0.567**
	(0.047)	(0.036)	(0.044)	(0.070)	(0.117)	(0.240)
Observations	1,336	1,035	957	1,266	820	598
Bandwidth	0.132	0.095	0.087	0.122	0.074	0.057
Mean of Dependent Variable	296.1	566.4	407.2	102.5	160.2	75.9
Panel B: Polynomial Spline						
Dynastic	0.034	0.043	0.126**	0.056	0.318**	0.502**
	(0.064)	(0.043)	(0.050)	(0.094)	(0.123)	(0.219)
Observations	2,188	2,188	2,188	2,188	2,182	2,004
Bandwidth	0.50	0.50	0.50	0.50	0.50	0.50
Mean of Dependent Variable	296.9	563.7	410.0	105.8	162.6	74.6

Notes: The unit of observation is a municipality-term. Sample is restricted to municipalities in which either the winner or the runner-up is a dynastic candidate. Panel A reports the coefficients from local linear regressions using the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel B reports the coefficients from regressions using a quartic spline. The dependent variable in regression column is reported in the top of the column. All regressions include year fixed effects and the following controls: log of initial population, log of average household income, share of rural households, share of households with access to electric power, share of adults with 8 to 10 years of schooling, share of adults with 11 or more years of schooling, FM radio station existence and local court existence. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 5: Political Dynasties and Economic Performance

	GDP Growth	Agricultural GDP Growth	Manufacturing GDP Growth	Services GDP Growth	No. of Employees Growth	No. of Establishments Growth
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Optimal Bandwidth						
Dynastic	0.014	0.059	-0.020	-0.001	-0.133	-0.053
	(0.036)	(0.062)	(0.080)	(0.018)	(0.222)	(0.044)
Observations	1,003	967	670	1,556	1,643	1,144
Bandwidth	0.091	0.088	0.059	0.171	0.193	0.109
Mean of Dependent Variable	0.24	0.05	0.41	0.29	0.76	0.37
Panel B: Polynomial Spline						
Dynastic	0.018	0.098	0.154	-0.013	-0.266	-0.046
	(0.037)	(0.071)	(0.182)	(0.025)	(0.295)	(0.053)
Observations	2,188	2,186	2,188	2,188	2,142	2,186
Bandwidth	0.50	0.50	0.50	0.50	0.50	0.50
Mean of Dependent Variable	0.24	0.05	0.47	0.29	0.74	0.35

Notes: The unit of observation is a municipality-term. Sample is restricted to municipalities in which either the winner or the runner-up is a dynastic candidate. Panel A reports the coefficients from local linear regressions using the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel B reports the coefficients from regressions using a quartic spline. The dependent variable in regression column is reported in the top of the column. All regressions include year fixed effects and the following controls: log of initial population, log of average household income, share of rural households, share of households with access to electric power, share of adults with 8 to 10 years of schooling, share of adults with 11 or more years of schooling, FM radio station existence and local court existence. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 6: Political Dynasties and the Quality of Public Goods

	Education			Health			Infrastructure		
	Class Size	Inadequate Age	Test Score	7+ Prenatal Visits	Low Birth Weight	Infant Mortality	Paved Roads	Open Sewage	Garbage in the Street
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Optimal Bandwidth									
Dynastic	-0.169	-0.013	0.032	0.012	0.002	-1.599	0.012	0.021	0.011
	(0.489)	(0.014)	(0.105)	(0.017)	(0.002)	(1.238)	(0.034)	(0.033)	(0.010)
Observations	1,051	872	844	1,110	899	948	526	788	709
Bandwidth	0.113	0.089	0.086	0.108	0.084	0.088	0.093	0.169	0.138
Mean of Dependent Variable	21.72	0.24	-0.07	0.52	0.07	21.69	0.71	0.19	0.04
Panel B: Polynomial Spline									
Dynastic	-0.605	-0.009	0.144	0.014	0.002	-2.116	-0.005	-0.002	0.019
	(0.609)	(0.016)	(0.121)	(0.022)	(0.003)	(1.364)	(0.041)	(0.049)	(0.014)
Observations	1,965	1,965	1,965	2,126	2,126	2,126	1,138	1,138	1,138
Bandwidth	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50
Mean of Dependent Variable	21.99	0.23	-0.01	0.53	0.07	21.66	0.72	0.18	0.04

Notes: The unit of observation is a municipality-term. Sample is restricted to municipalities in which either the winner or the runner-up is a dynastic candidate. Panel A reports the coefficients from local linear regressions using the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel B reports the coefficients from regressions using a quartic spline. The dependent variable in regression column is reported in the top of the column. All regressions include year fixed effects and the following controls: log of initial population, log of average household income, share of rural households, share of households with access to electric power, share of adults with 8 to 10 years of schooling, share of adults with 11 or more years of schooling, FM radio station existence and local court existence. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 7: Political Dynasties and Political Selection

	Years of schooling	White Collar	Age	Female
	(1)	(2)	(3)	(4)
Panel A: Optimal Bandwidth				
Dynastic	0.627	-0.028	-1.388	0.141***
	(0.586)	(0.057)	(1.188)	(0.040)
Observations	760	1,017	1,168	1,029
Bandwidth	0.068	0.093	0.112	0.094
Mean of Dependent Variable	12.44	0.32	47.30	0.13
Panel B: Polynomial Spline				
Dynastic	0.471	0.007	-0.762	0.160***
	(0.594)	(0.068)	(1.519)	(0.047)
Observations	2,188	2,188	2,188	2,188
Bandwidth	0.50	0.50	0.50	0.50
Mean of Dependent Variable	12.31	0.33	46.93	0.12

Notes: The unit of observation is a municipality-term. Sample is restricted to municipalities in which either the winner or the runner-up is a dynastic candidate. Panel A reports the coefficients from local linear regressions using the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel B reports the coefficients from regressions using a quartic spline. The dependent variable in regression column is reported in the top of the column. All regressions include year fixed effects and the following controls: log of initial population, log of average household income, share of rural households, share of households with access to electric power, share of adults with 8 to 10 years of schooling, share of adults with 11 or more years of schooling, FM radio station existence and local court existence. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

A Appendices

A.1 Data Sources

The data used in the paper comes from a variety of sources. The data is at the level of the municipality-municipal mandate, the lowest government unit below a state in Brazil. This means that growth rates are computed as the ratio between the increase from the beginning to the end of the following term and the value at the beginning of the term. Next, we describe the source of each variable used in the analysis.

Politician's Characteristics: the Tribunal Superior Eleitoral (TSE) provides basic demographic information on each candidate that ran in municipal elections since the 1996 elections at www.tse.gov.br. We use this information to create the following municipal-term indicators: *Relative in Office in the future* - Indicator of whether a candidate in 1996 has a relative (details in the main text) in office in the future in the same municipality won the election, *Dynastic* - Indicator of whether a candidate in the 2004 or 2008 elections has a relative in office in the past in the same municipality won the election, *Age* - Mayor's Age, *Female* - Indicator of a female mayor, *High Occupation* - Indicator of mayor with a white collar job , *Schooling* - Mayor's years of schooling, *Council Support* - Councilors in the government coalition/Number of Councilors, *Campaign Contributions* - Mayor's campaign contribution.

Municipal demographic characteristics: Demographic characteristics of the municipality come from the 1992, 2000 and 2010 population census and the Contas Nacionais database, available at IBGE (www.ibge. gov.br). The estimate for the population in year of the sample was obtained from the IBGE inter-census population estimates. The variables used in the analysis are: *Literacy Rate* - Proportion of the municipal population that is literate, *Log of average household income* - log(Average Household Income), *Share of rural households* - Proportion of the Municipal Population living in rural areas, *Share of households with access to eletric power* - Proportion of the Municipal Population with Access to Eletric Power, *Share of adults with 8 to 10 years of scholing* - Proportion of the Municipal Population that completed Middle School, *Share of adults with 11 years or more of scholing* - Proportion of the Municipal Population that completed High School, *Paved Roads (%)* - Share of households located in streets with paved roads, *Open Sewage (%)* - Share of households located in sreeets with open sewage, *Garbage in the Street (%)* - Share of households located in sreeets with garbage disposed.

Municipal institutional and public management characteristics: the 1999, 2002, 2005,

2006, 2008, 2009, 2011 and 2012 surveys of the *Perfil dos Municípios Brasileiros: Gestão Pública* provide information on various aspects of the public administration and structural municipal features. We constract the following variables from this source: *AM Radio Station Existence* - Existence of an AM Radio Station in the municipality, *FM Radio Station Existence* - Existence of an FM Radio Station in the municipality, *Local Courts* - Existence of a Local Court in the Municiality, *Public Emp. per 1000 inhabitants* - 1000 * Employment in Municipal Government / Population, *Public Emp. per 1000 inhabitants (Permanent Staff)* - 1000 * Employment in Municipal Government (Permanent Staff) / Population, *Public Emp. per 1000 inhabitants (Temporary Staff)* - 1000 * Employment in Municipal Government (Temporary Staff) / Population.

Municipal public finance information: the National Treasury (Secretaria do Tesouro) provides information of spending and revenues through the FINBRA dataset. It contains municipal spending by categories. See www.tesouro.com.br. From this source, we construct all the outcomes in logs per capita so that *log(Category Name pc)* refers to log (Municipal Government Expenditures in this Category / Municipal Population). We consider the following categories cateegories *Expenditures* - total expenditures, *Administration and Planning*, *Education and Culture*, *Health and Sanitation*, *Social Assistance*, *Housing and Urban Development*, *Transportation* - these 7 categories correspond to the break down of expenditure by function, *Capital*, *Investment*, *Current* - these last 3 categories correspond to the break down of expenditure by the nature of the spending.

Private sector wages: the RAIS provides information of public and private sector workers and establishments for all (formal) firms in Brazil. We use this data to construct:*Number of workers* and *Number of Establishments*.

School data: information on the schools characteristics and enrollment came from yearly school census undertaken by INEP, while students' performance came from Prova Brasil. We used here: *Class Size* - Average Class Size for 5th Graders Municipal Schools, *Inadequate Age* - (Enrollment in Inadequate Grade for the Age) / (Total Enrollment) (5th Grade - Municipal Schools), *Test Score* - Average Test Score for 5th Graders in Municipal Schools (Math and Portuguese).

Health data: information on health outcomes was obtained from the Informações de Saúde (TABNET) available at www.datasus.gov.br. These include: *7+ Prenatal Visits* - Share of women who had at least 7 prenatal visits, *Share LBW* - Share of low weight births, *Infant Mortality* - Number of Infant Deaths / Number of Births.

A.2 Additional Results

Table A1: Balance Tests

	log(Population)	% Rural Households	log(Avg. Income)	% Electric Power	% High School	% Middle School	FM Radio Station	Local Court
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Optimal Bandwidth								
Dynastic	0.143	-0.033	-0.033	-0.013	-0.001	0.001	-0.035	-0.001
	(0.103)	(0.029)	(0.061)	(0.017)	(0.008)	(0.005)	(0.057)	(0.067)
Observations	1,242	890	983	1,433	885	810	1,084	916
Bandwidth	0.119	0.081	0.090	0.148	0.080	0.073	0.101	0.083
Mean of Dependent Variable	9.31	0.43	6.81	0.83	0.13	0.10	0.33	0.48
Panel B: Polynomial Spline								
Dynastic	0.068	-0.026	-0.036	-0.027	-0.006	-0.003	-0.046	-0.051
	(0.136)	(0.032)	(0.073)	(0.024)	(0.009)	(0.006)	(0.071)	(0.075)
Observations	2,188	2,188	2,188	2,188	2,188	2,188	2,188	2,188
Bandwidth	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50
Mean of Dependent Variable	9.31	0.43	6.84	0.83	0.13	0.10	0.34	0.47

Notes: The unit of observation is a municipality-term. Sample is restricted to municipalities in which either the winner or the runner-up is a dynastic candidate. Panel A reports the coefficients from local linear regressions using the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel B reports the coefficients from regressions using a quartic spline. All regressions include year fixed effects. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table A2: Government Expenditures and Revenues - DD Estimates

	log(Expenditures pc)	log(Capital Exp. pc)	log(Current Exp. pc)	log(Taxes pc)	log(Transfers pc)
	(1)	(2)	(3)	(4)	(5)
Panel A: Baseline Controls					
Dynastic	-0.002	0.001	-0.005	0.048**	-0.004
	(0.009)	(0.032)	(0.008)	(0.023)	(0.007)
Observations	10,794	10,794	10,794	10,794	10,794
R-Squared	0.975	0.858	0.977	0.954	0.98
Panel B: Matching Histories					
Dynastic	0.007	0.019	0.003	0.049*	0.002
	(0.011)	(0.039)	(0.010)	(0.030)	(0.009)
Observations	10,794	10,794	10,794	10,794	10,794
Bandwidth	0.973	0.856	0.975	0.954	0.979

Notes: The unit of observation is a municipality-term. Panel A reports the impact of dynasties on the dependent variables obtained from a panel regression including year and municipality fixed effects and a set of municipal characteristics interacted with time dummies as controls. The municipal characteristics are log of log of initial population, log of average household income, share of rural households, share of households with access to electric power, share of adults with 8 to 10 years of schooling, share of adults with 11 or more years of schooling, FM radio station existence and local courts. Panel B reports the impact of dynasties on the dependent variables obtained from a panel regression including year and municipality fixed effects as controls. A different coefficient is estimated for each possible political histories in our sample. Coefficients are aggregated to obtain the average impact of political dynasties. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table A3: Government Expenditures by Category - DD Estimates

	log(Admin. and Planning Exp. pc)	log(Education and Culture Exp. pc)	log(Health and Sanitation Exp. pc)	log(Social Assistance Exp. pc)	log(Housing and Urban Dev. Exp. pc)	log(Transp. Exp. pc)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline Controls						
Dynastic	-0.012	-0.008	0.012	0.014	0.043	-0.002
	(0.017)	(0.011)	(0.016)	(0.028)	(0.039)	(0.066)
Observations	10,794	10,794	10,794	10,791	10,758	9,970
R-Squared	0.94	0.94	0.92	0.91	0.87	0.91
Panel B: Matching Histories						
Dynastic	0.006	0.007	0.013	0.028	0.071	-0.038
	(0.021)	(0.015)	(0.019)	(0.032)	(0.050)	(0.078)
Observations	10,794	10,794	10,794	10,791	10,758	9,970
Bandwidth	0.94	0.93	0.92	0.91	0.87	0.91

Notes: The unit of observation is a municipality-term. Panel A reports the impact of dynasties on the dependent variables obtained from a panel regression including year and municipality fixed effects and a set of municipal characteristics interacted with time dummies as controls. The municipal characteristics are log of log of initial population, log of average household income, share of rural households, share of households with access to electric power, share of adults with 8 to 10 years of schooling, share of adults with 11 or more years of schooling, FM radio station existence and local courts. Panel B reports the impact of dynasties on the dependent variables obtained from a panel regression including year and municipality fixed effects as controls. A different coefficient is estimated for each possible political histories in our sample. Coefficients are aggregated to obtain the average impact of political dynasties. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table A4: Economic Performance - DD Estimates

	GDP Growth	Agricultural GDP Growth	Manufacturing GDP Growth	Services GDP Growth	No. of Employees Growth	No. of Establishments Growth
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline Controls						
Dynastic	0.029	0.022	0.060	0.014	0.102	-0.012
	(0.020)	(0.040)	(0.076)	(0.015)	(0.179)	(0.028)
Observations	10,794	10,780	10,794	10,794	10,621	10,788
R-Squared	0.481	0.476	0.495	0.46	0.566	0.596
Panel B: Matching Histories						
Dynastic	0.041	-0.036	0.176	0.024	0.272	0.019
	(0.028)	(0.058)	(0.126)	(0.024)	(0.272)	(0.019)
Observations	10,794	10,780	10,794	10,794	10,621	10,788
R-Squared	0.470	0.422	0.495	0.454	0.572	0.593

Notes: The unit of observation is a municipality-term. Panel A reports the impact of dynasties on the dependent variables obtained from a panel regression including year and municipality fixed effects and a set of municipal characteristics interacted with time dummies as controls. The municipal characteristics are log of log of initial population, log of average household income, share of rural households, share of households with access to electric power, share of adults with 8 to 10 years of schooling, share of adults with 11 or more years of schooling, FM radio station existence and local courts. Panel B reports the impact of dynasties on the dependent variables obtained from a panel regression including year and municipality fixed effects as controls. A different coefficient is estimated for each possible political histories in our sample. Coefficients are aggregated to obtain the average impact of political dynasties. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table A5: Quality of Public Goods - DD Estimates

	Education			Health		
	Class Size	Inadequate Age	Test Score	7+ Prenatal Visits	Low Birth Weight	Infant Mortality
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline Controls						
Dynastic	0.165	0.001	-0.033	0.003	0.001	0.163
	(0.270)	(0.006)	(0.047)	(0.007)	(0.001)	(0.595)
Observations	9,547	9,547	9,547	10,336	10,336	10,336
R-Squared	0.851	0.908	0.874	0.94	0.76	0.82
Panel B: Matching Histories						
Dynastic	0.388	-0.014	-0.046	0.009	0.002	0.108
	(0.369)	(0.009)	(0.054)	(0.009)	(0.002)	(0.777)
Observations	9,547	9,547	9,547	10,336	10,336	10,336
R-Squared	0.85	0.88	0.872	0.94	0.76	0.83

Notes: The unit of observation is a municipality-term. Panel A reports the impact of dynasties on the dependent variables obtained from a panel regression including year and municipality fixed effects and a set of municipal characteristics interacted with time dummies as controls. The municipal characteristics are log of log of initial population, log of average household income, share of rural households, share of households with access to electric power, share of adults with 8 to 10 years of schooling, share of adults with 11 or more years of schooling, FM radio station existence and local courts. Panel B reports the impact of dynasties on the dependent variables obtained from a panel regression including year and municipality fixed effects as controls. A different coefficient is estimated for each possible political histories in our sample. Coefficients are aggregated to obtain the average impact of political dynasties. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table A6: Political Selection - DD Estimates

	Years of schooling	White Collar	Age	Female
	(1)	(2)	(3)	(4)
Panel A: Baseline Controls				
Dynastic	0.571*	0.010	-1.553**	0.112***
	(0.292)	(0.034)	(0.748)	(0.025)
Observations	10,794	10,794	10,790	10,794
R-Squared	0.72	0.57	0.70	0.70
Panel B: Matching Histories				
Dynastic	0.684*	0.060	-3.620***	0.092***
	(0.360)	(0.045)	(0.942)	(0.030)
Observations	10,794	10,794	10,790	10,794
R-Squared	0.72	0.57	0.70	0.70

Notes: The unit of observation is a municipality-term. Panel A reports the impact of dynasties on the dependent variables obtained from a panel regression including year and municipality fixed effects and a set of municipal characteristics interacted with time dummies as controls. The municipal characteristics are log of log of initial population, log of average household income, share of rural households, share of households with access to electric power, share of adults with 8 to 10 years of schooling, share of adults with 11 or more years of schooling, FM radio station existence and local courts. Panel B reports the impact of dynasties on the dependent variables obtained from a panel regression including year and municipality fixed effects as controls. A different coefficient is estimated for each possible political histories in our sample. Coefficients are aggregated to obtain the average impact of political dynasties. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table A7: Government Expenditures and Revenues - Alternative Specifications

	log(Expenditures pc)	log(Capital Exp. pc)	log(Current Exp. pc)	log(Taxes pc)	log(Transfers pc)
	(1)	(2)	(3)	(4)	(5)
Panel A: 75% IKBW					
Dynastic	0.049	0.085	0.072**	0.037	0.069**
	(0.035)	(0.082)	(0.032)	(0.062)	(0.031)
Observations	841	626	1,062	1,438	956
Bandwidth	0.076	0.0547	0.098	0.149	0.0866
Panel B: 125% IKBW					
Dynastic	0.059**	0.154**	0.027	0.007	0.038
	(0.028)	(0.065)	(0.026)	(0.049)	(0.026)
Observations	1,308	1,005	1,520	1,859	1,415
Bandwidth	0.127	0.091	0.163	0.248	0.144
Panel C: Cubic Spline					
Dynastic	0.055*	0.154**	0.056	0.052	0.047
	(0.031)	(0.076)	(0.038)	(0.073)	(0.030)
Observations	2,188	2,188	2,188	2,188	2,188
Bandwidth	0.50	0.50	0.50	0.50	0.50

Notes: The unit of observation is a municipality-term. Sample is restricted to municipalities in which either the winner or the runner-up is a dynastic candidate. Panel A reports the coefficients from local linear regressions using 75% of the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel B reports the coefficients from local linear regressions using 125% of the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel C reports the coefficients from regressions using a cubic spline. All regressions include year fixed effects and the following controls: log of initial population, log of average household income, share of rural households, share of households with access to electric power, share of adults with 8 to 10 years of schooling, share of adults with 11 or more years of schooling, FM radio station existence and local court existence. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table A8: Expenditures by Category - Alternative Specifications

	log(Admin. and Planning Exp. pc)	log(Education and Culture Exp. pc)	log(Health and Sanitation Exp. pc)	log(Social Assistance Exp. pc)	log(Housing and Urban Dev. Exp. pc)	log(Transp. Exp. pc)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 75% IKBW						
Dynastic	0.043	0.005	0.079	0.108	0.293**	0.527*
	(0.053)	(0.041)	(0.050)	(0.080)	(0.133)	(0.286)
Observations	1,068	794	727	1,009	628	440
Bandwidth	0.099	0.071	0.065	0.092	0.055	0.043
Panel B: 125% IKBW						
Dynastic	0.014	0.032	0.115***	0.008	0.364***	0.637***
	(0.043)	(0.033)	(0.040)	(0.063)	(0.106)	(0.213)
Observations	1,531	1,240	1,141	1,465	1,012	735
Bandwidth	0.165	0.119	0.108	0.153	0.092	0.071
Panel C: Cubic Spline						
Dynastic	0.062	0.033	0.081**	0.052	0.196*	0.347*
	(0.053)	(0.036)	(0.041)	(0.076)	(0.100)	(0.178)
Observations	2,188	2,188	2,188	2,188	2,182	2,004
Bandwidth	0.500	0.500	0.500	0.500	0.500	0.500

Notes: The unit of observation is a municipality-term. Sample is restricted to municipalities in which either the winner or the runner-up is a dynastic candidate. Panel A reports the coefficients from local linear regressions using 75% of the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel B reports the coefficients from local linear regressions using 125% of the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel C reports the coefficients from regressions using a cubic spline. All regressions include year fixed effects and the following controls: log of initial population, log of average household income, share of rural households, share of households with access to electric power, share of adults with 8 to 10 years of schooling, share of adults with 11 or more years of schooling, FM radio station existence and local court existence. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table A9: Political Dynasties and Economic Performance - Alternative Specifications

Dependent variable:	GDP growth	Agricultural GDP growth	Manufacturing GDP growth	Services GDP growth	Number of Employees growth	Number of Establishments growth
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 75% IKBW						
Dynastic	0.022	0.122*	-0.013	0.006	-0.104	-0.041
	(0.035)	(0.074)	(0.091)	(0.021)	(0.256)	(0.048)
Observations	760	734	498	1,314	1,388	908
Bandwidth	0.068	0.066	0.044	0.128	0.145	0.082
Panel B: 125% IKBW						
Dynastic	0.011	0.069	-0.056	-0.006	-0.117	-0.051
	(0.028)	(0.054)	(0.083)	(0.017)	(0.203)	(0.038)
Observations	1,189	1,149	821	1,762	1,810	1,367
Bandwidth	0.114	0.109	0.0737	0.214	0.241	0.136
Panel C: Cubic Spline						
Dynastic	0.013	0.070	0.022	-0.002	-0.117	-0.028
	(0.030)	(0.058)	(0.092)	(0.022)	(0.272)	(0.043)
Observations	2,188	2,186	2,188	2,188	2,142	2,186
Bandwidth	0.500	0.500	0.500	0.500	0.500	0.500

Notes: The unit of observation is a municipality-term. Sample is restricted to municipalities in which either the winner or the runner-up is a dynastic candidate. Panel A reports the coefficients from local linear regressions using 75% of the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel B reports the coefficients from local linear regressions using 125% of the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel C reports the coefficients from regressions using a cubic spline. All regressions include year fixed effects and the following controls: log of initial population, log of average household income, share of rural households, share of households with access to electric power, share of adults with 8 to 10 years of schooling, share of adults with 11 or more years of schooling, FM radio station existence and local court existence. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table A10: Public Goods - Alternative Specifications

	Education			Health			Infrastructure		
	Class Size	Inadequate Age	Test Score	7+ Prenatal Visits	Low Birth Weight	Infant Mortality	Paved Roads	Open Sewage	Garbage in the Street
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: 75% IKBW									
Dynastic	0.009	-0.011	0.050	0.003	0.003	-0.636	-0.009	0.009	0.020*
	(0.552)	(0.016)	(0.126)	(0.020)	(0.003)	(1.282)	(0.040)	(0.037)	(0.011)
Observations	825	667	643	873	684	719	404	666	568
Bandwidth	0.084	0.067	0.065	0.081	0.063	0.066	0.070	0.127	0.103
Panel B: 125% IKBW									
Dynastic	0.031	-0.010	0.031	0.014	0.001	-1.531	0.014	0.024	0.013
	(0.447)	(0.013)	(0.095)	(0.016)	(0.002)	(1.076)	(0.031)	(0.031)	(0.009)
Observations	1,248	1,042	1,014	1,326	1,088	1,130	623	898	794
Bandwidth	0.141	0.112	0.108	0.135	0.105	0.111	0.117	0.212	0.172
Panel C: Cubic Spline									
Dynastic	0.286	-0.008	0.001	0.008	-0.001	-0.914	0.016	0.023	0.012
	(0.525)	(0.013)	(0.096)	(0.018)	(0.002)	(1.113)	(0.033)	(0.041)	(0.011)
Observations	1,965	1,965	1,965	2,126	2,126	2,126	1,138	1,138	1,138
Bandwidth	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50

Notes: The unit of observation is a municipality-term. Sample is restricted to municipalities in which either the winner or the runner-up is a dynastic candidate. Panel A reports the coefficients from local linear regressions using 75% of the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel B reports the coefficients from local linear regressions using 125% of the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel C reports the coefficients from regressions using a cubic spline. All regressions include year fixed effects and the following controls: log of initial population, log of average household income, share of rural households, share of households with access to electric power, share of adults with 8 to 10 years of schooling, share of adults with 11 or more years of schooling, FM radio station existence and local court existence. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table A11: Political Dynasties and Political Selection - Alternative Specifications

Dependent Variable:	Years of schooling	White Collar	Age	Female
	(1)	(2)	(3)	(4)
Panel A: 75% IKBW				
Dynastic	0.001	0.525	-1.565	0.120**
	(0.067)	(0.679)	(1.365)	(0.046)
Observations	773	575	924	791
Bandwidth	0.069	0.051	0.084	0.071
Panel B: 125% IKBW				
Dynastic	-0.005	0.465	-2.408**	0.128***
	(0.052)	(0.530)	(1.063)	(0.037)
Observations	1,208	935	1,386	1,229
Bandwidth	0.116	0.085	0.140	0.118
Panel C: Cubic Spline				
Dynastic	0.020	0.697	-2.196*	0.148***
	(0.056)	(0.477)	(1.227)	(0.039)
Observations	2,188	2,188	2,188	2,188
Bandwidth	0.50	0.50	0.50	0.50

Notes: The unit of observation is a municipality-term. Sample is restricted to municipalities in which either the winner or the runner-up is a dynastic candidate. Panel A reports the coefficients from local linear regressions using 75% of the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel B reports the coefficients from local linear regressions using 125% of the Imbens and Kalyanaraman (2012) optimal bandwidth. Panel C reports the coefficients from regressions using a cubic spline. All regressions include year fixed effects and the following controls: log of initial population, log of average household income, share of rural households, share of households with access to electric power, share of adults with 8 to 10 years of schooling, share of adults with 11 or more years of schooling, FM radio station existence and local court existence. Details on the construction of all variables are presented in the appendix. Standard errors clustered at the municipality-level are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.