Dynasties and the Political Budget Cycle

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Abstract

In this paper, we provide a wide set of results which point toward a better understanding of the role of political dynasties in a representative democracy. Our empirical analysis focuses on local politics in Italy, using a large sample of mayors and mayoral candidates in the period 1998-2012. We highlight the relevance of dynasties in the political arena, in terms of electoral performances and self-perpetuation. However, our main contribution is a test of whether dynastic politicians enforce different policies. Based on three different specifications (panel fixed-effects, standard regression discontinuity design on close elections and propensity-score matching), we find no effect of dynastic mayors on average spending, revenues and transfers. Conversely, we show that dynastic mayors increase spending and obtain higher transfers during the year prior to election, especially when they can run for re-election. We then discuss the mechanisms which might explain such strategic behavior.

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1 Introduction

Even though political positions in democratic societies are generally awarded via elections, families often continue to play a significant role in politics. In fact, political dynasties are common in as diverse settings as, for instance, Argentina (Rossi (2011)), Japan (Fukai and Fukui (1992); Asako et al. (2015)), the Philippines (Querubin (2010)) and the United States (Dal Bó et al. (2009); Feinstein (2010)). The emerging academic literature on such political dynasties has thus far predominantly focused on whether and how political dynasties arise and/or persist, rather than investigating the effects of political dynasties. Specifically, a first group of studies focuses on the causal impact of politicians' tenure length on the probability of having a family-member in politics in the future (Dal Bó et al. (2009); Rossi (2011); Querubin (2010)). Rossi (2011), for instance, exploits an exogenous change in the Argentinian electoral law to show that longer tenure in the Argentinian congress increases the probability of having a relative in future congresses. Dal Bó et al. (2009) and Querubin (2010) find very similar results for the United States and the Philippines using a regression discontinuity design. A second group of studies more directly investigates the electoral performance of dynastic versus non-dynastic politicians (Feinstein (2010); Asako et al. (2015), Van Coppenolle (2014)). Controlling for other individual characteristics, dynastic politicians have higher probabilities of success in national elections in the United States and Japan.

Conversely, in this paper we focus on whether dynastic elected leaders behave differently from other politicians, when in office. Two sets of hypotheses suggest that this might be the case. Firstly, being electorally more competitive compared to other politicians, dynastic leaders might also be more capable to enforce their preferred policy agenda, when in office. Specifically, such ability might be due to inherited political skills, which might include, for example, their ability to hold political power, mobilize local networks, negotiate with local elites and exploit their family reputation (Rossi (2011); Querubin (2010); Dal Bó et al. (2009); Feinstein (2010)). In this light, Cruz et al. (2014)) show that selection into politics in the Philippines is shaped by the centrality - in terms of networks - of the candidate's family within the municipality: because political power is persistent, it is likely that dynastic mayors come from more central families than non dynastic mayors. However, such reasoning also leads to the idea that dynastic leaders might behave less strategically when setting their agenda, as they benefit of

an electoral advantage which makes them less needy of signaling their competence. This is in line with Besley and Reynal-Querol (2015), who argue that dynastic politicians perform particularly well when constraints on the executive are weak, and dynastic transmission of power is easier (i.e. when dynastic politicians have greater intertemporal constraints).

A second main hypothesis leads to the prediction that dynastic politicians might face different electoral incentives compared to other politicians, leading to different sets of policies. In fact, theories of power transmission within elites support the idea of power self-perpetuation. In this light, dynasties, similarly to other elite groups, aim at guaranteeing their power and its perpetuation to future generations (Michels (1915); Mosca (1939); Pareto (1901)). Such idea is not restricted to autocracies but can extend to democracies as well (Michels (1915)). Robinson and Acemoglu (2008) provide a model of endogenous political persistence, where "the elite, by virtue of their smaller numbers and their greater expected returns from controlling politics, have a comparative advantage in investing in de facto power" (Robinson and Acemoglu (2008)). Such higher expected gains might come from the above mentioned inherited political skills. For instance, political connections have indeed been shown to increase firm performances (Faccio (2006),Amore and Bennedsen (2013) Gagliarducci and Manacorda (2014)), politicians' revenues and revenues of their relatives (Folke et al. (2015), Labonne and Fafchamps (2015), Fisman et al. (2012), Querubin and Snyder Jr (2011), Eggers and Hainmueller (2009)). In a similar light, they might profit more from rent seeking activities, thus shaping incentives for policies enabling to remain in office. Such hypothesis is more widely related to the literature on "strong" family ties, which have been linked to worse firms performance (Bertrand and Schoar (2006); Bennedsen et al. (2006)), labour market participation (Alesina and Giuliano (2010)) and academic recruitment (Durante et al. (2011))[1].

Overall, dynastic politicians might behave differently when in office both because they *can* - thanks to inherited political skills - and they *want* - because of higher return from politics. In this paper, we test such hypotheses by providing causal evidence based on Italian local politicians in the period 1985-2012. We provide a wide set of results which point toward a better

[1]Moreover, dynastic politicians might prefer a political career for reputational reasons, i.e. they might perceive a higher utility from holding a political office (this motivation is related to the political science literature on public-service motivations (e.g. Houston (2000)). Finally, an alternative explanation is that dynastic politicians might suffer from a "Carnegie effect" (Durante et al. (2011)) if the advantage granted by their elected ancestors lead them to underinvest in human capital: in this case, a worse outside option might incentivize dynastic mayors to shape policies maximizing their own chance of reelection.

understanding of the role of political dynasties in representative democracies. We firstly show that dynastic politicians have different careers and different electoral performances. In fact, they are more likely to win local elections and to get elected in regional parliaments than non-dynastic politicians; indeed, political power is persistent in Italian municipalities: an elected individual is twice more likely to have a relative in office within ten years than a non-elected individual. However, our main finding is related to the behavior of dynastic politicians while in office: we show that dynastic politicians are more likely to strategically increase public spending in the pre-electoral year. Dynastic mayors increase spending, especially in terms of capital expenditure, which compared to current expenditure can be more easily handled. The increase is substantial, i.e. about 100 euros per capita. Interestingly, we find evidence of a political budget cycle (PBC) only for dynastic mayors at the end of their first term, i.e. those who can run for re-election (Italian mayors have a two-term limit, see next section). Higher expenditures are financed both by debts and by transfers from higher levels of government. These findings are in line with both above suggested channels, as dynastic politicians appear more powerful and more strategic. To reinforce such interpretation, we provide additional evidence. Firstly, we show that according to electoral incentives, dynastic mayors are more likely to have higher pre-electoral spending in more contested elections. Secondly, we show that this effect might be channeled by a worse outside option, as more educated dynastic mayors are less likely to run political budget cycles. Thirdly, in line with the idea of inherited political skills, we find that i) within dynastic mayors, PBC is higher for mayors in more powerful dynasties; ii) dynastic mayors employed in liberal professions - which are based on local networks - have substantially higher PBC; however, we do not find that dynastic mayors enforce higher PBC in regions in Southern Italy, characterized by higher level of clientelism and organized crime. Overall, our results point at a clear strategic behavior of dynastic leaders, which is confirmed by several empirical tests. Interestingly, it appears that the electoral advantage enjoyed by dynastic mayors does not prevent them from behaving more strategically, in terms of higher spending before the elections[2].

[2]This might be due to a relative low cost of enforcing PBC compared to the high cost of losing elections under electoral uncertainty. In fact, our findings show that although statistically significant, the electoral advantage enjoyed by dynastic candidates is relatively small

As in our analysis we take into account all Italian local elected politicians in the period 1985-2012 (N=571,824), we have to rely on a systematic method to identify family ties among politicians. In line with previous studies on academic and political dynasties (Allesina (2011); Durante et al. (2011); Querubin (2013)) as well as inter-generational social mobility (Clark (2014); Clark and Cummins (2015)), we look at politicians sharing the same surname to identify family ties. Clearly, this identification reduces the precision of our estimates. We show that our results are confirmed, or reinforced, when excluding politicians with common surnames, as in such cases, the identification of family ties is more likely to be problematic. Our empirical strategy is robust to three different approaches: i) a panel fixed-effects approach; ii) Regression-Discontinuity-Design (RDD) on local elections won by a close margin; iii) matching estimation on close elections.

Overall, our findings, firstly, contribute to a better understanding of the strategic behaviours that elites might embrace in order to sustain their power (Mosca (1939); Pareto (1901)) in modern democracies (Michels (1915); Robinson and Acemoglu (2008)); secondly, we provide additional evidence for the relevance of dynastic politics in the context of an affluent European country (see Folke et al. (2015) for different results on political dynasties in another affluent European country, i.e. Sweden); conversely, our results are in line with Bragança et al. (2015) and Geys (2015) about the potential rent-seeking behaviours of dynastic politicians; we also contribute to the literature on the political budget cycle (Rogoff (1987); Blais and Nadeau (1992)), showing that the incentives to manipulate expenditure can vary across political groups (Persson et al. (2003)).

The remainder of the paper is structured as follows: in Section 2, we present the institutional background and the data we use; in Section 3 we assess the importance of dynastic mayors in the political arena and describe their main characteristics; in Section 4 we estimate the impact of dynasties on municipal budgets, before discussing the potential channels driving our results in Section 5, and performing robustness tests in Section 6. Section 7 concludes.

2 Institutional background and data

2.1 Local politics in Italy

The Italian political system is characterized by three levels of governance, where municipalities (about 8,000 across the country) represent the lowest level, followed by regions (20) and the national level. Until 2014, provinces (110) represented another level of government between cities and regions. Nonetheless, as in most other European countries, municipal governments have important responsibilities with respect to education, social welfare, culture and recreation, city planning, transport, economic development, waste management and local police. They also have important fiscal powers, whereby setting the local property tax rate is the central annual financial decision (Bordignon et al. (2003)). This is true also in the light of the fact that the share of national transfers has gradually decreased over time, increasing the relevance of local revenues to finance the municipal budget. However, their spending capacity is limited by the so called "Internal Stability and Growth Pact", which limits the ability of municipalities to incur in debts. Morever, such debts - as established by the Italian Constitution - can only cover capital expenditures.

Local elections are held every five years (every four years before 2000) to elect council members and the (directly elected) mayor. The exact electoral system depends on the size of the municipality. In cities of fewer than 15,000 inhabitants, voters effectively have only one vote, which they cast for a candidate mayor and her supportive list of candidates for the municipal council (though additional 'preference votes' for candidates within this list of council candidates are possible). Elections take place in a single round, in which the mayoral candidate obtaining most votes is selected and her supportive list is allocated at least 66% of the council seats. The remaining seats are allocated proportionally to the vote share of all remaining mayoral candidates' supportive lists. In municipalities with more than 15,000 inhabitants, voters are faced with parties (or coalitions thereof) which present a list of candidates for the municipal council and support a candidate mayor. They cast one vote for a candidate mayor and one vote for a list of candidates for the municipal council (which can, but need not be, the list supporting a voter's preferred mayoral candidate). Elections for mayor here follow a run-off system, whereby the two top candidates run in a second round whenever no candidate obtained an

outright majority in round one. The list(s) supporting the winning mayor are allocated at least 60% of the council seats, and now there is a 3% threshold for the proportional allocation of the remaining seats (see Bordignon et al. (2013), for more details).

2.2 Identifying Political Dynasties in Italy

In this paper, we gather a wide set of data concerning local Italian municipalities in order to identify political dynasties and measure fiscal outcomes at the municipality level. Specifically, we base our estimates on three different datasets: i) individual data about all local elected politicians in the period 1985-2012 - which includes some biographical information (e.g. gender, education, date and place of birth, job); ii) local elections outcomes in the period 1993-2012. This dataset also includes data on all candidates running for the mayoral position; iii) a dataset about city fiscal outcomes (revenues and expenditures) in the period 1998-2012. All data are publicly available and provided by the Italian Ministry of Interior for the above mentioned periods.

Political dynasties are a common phenomenon at the municipal level in Italy. To identify dynastic politicians, we rely on the above mentioned datasets about local politicians. Our data, however, do not allow us to identify directly family ties between elected representatives in Italy. Similar to recent studies on academic and political dynasties (Allesina (2011); Durante et al. (2011); Querubin (2013)) as well as inter-generational social mobility (Clark (2014); Clark and Cummins (2015)), we look at individuals with the same surname to identify (presumed) family ties. Specifically, we consider dynastic all mayors having at least one politician elected in the past (since 1985) in the same municipality with their same surname.

Using surnames to operationalize political dynasties is a valid approximation in our Italian setting since children receive the surname of their father. Even so, one can wonder about the precision of a dynastic variable based on surnames, since people can have the same surname without having any kinship ties (i.e. surname homonymy). Moreover, this operationalization only catches ties between family members when they have the same surname. While these reflect the closest family ties that are likely to generate the strongest effects (e.g. children,

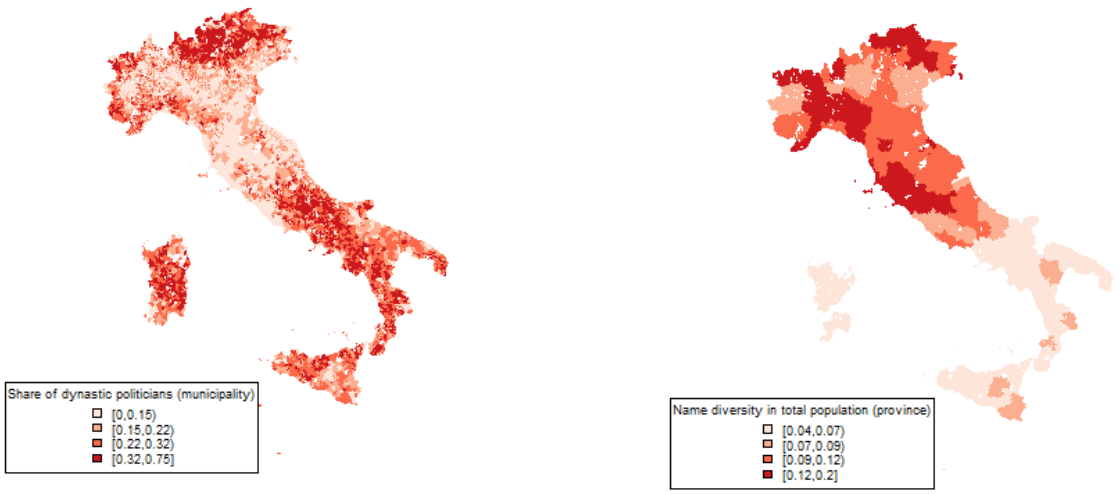
grandchildren), it may overlook more distant kinship ties (e.g. cousins, nephews, son-in-law) as well as ties among spouses and daughters that have changed their name upon marriage. Although data availability prevents us from directly addressing both issues, it is important to observe that they bias our estimates towards zero. Both issues indeed imply that we fail to define a certain number of dynastic politicians as part of a dynasty, such that these remain in the control group. Since dynastic politicians are expected to have different spending patterns compared to non-dynastic politicians, this mis-allocation pushes the average spending in the 'control' group (i.e. non-dynastic politicians) closer to the average in the 'treatment' group (i.e. dynastic politicians) – inducing a bias in our estimates towards zero. This not only stacks the deck against us, but also implies that our findings below reflect a lower bound of the true effect of political dynasties. Nonetheless, we further address this concern through several tests, among the others, by excluding the most common surnames from the estimation sample and by controlling for the relative frequency of each surname at the provincial level in the overall Italian population.

3 Importance and characteristics of dynastic politicians

3.1 Share of dynastic politicians: heterogeneity across time and space

Dynastic local politicians are likely to represent an important share of politicians[3]. As shown in Figure 1a, the share of dynastic politicians by municipality over the period 1998-2012 is heterogeneously spread across the country: it seems to be particularly high in the South and the North of the country (with more than one politician over three having at least the same name as a previous member of the city council), and lower in the Center of the country (with shares closer to 10%). However, such distribution in the shares of dynastic politicians over this period might reflect some underlying characteristics of the municipalities. First of all, surname concentration is not even across the country. Using tax files data (in 2005) recording the occurrence of every surname in the Italian population at the province level [4], we can compute surname

[3] "Politicians" hereafter refer to members of municipality councils.
[4] We are grateful to Giovanna Labartino for providing these data.



(a) Share of dynastic politicians at the city level (between 1998 and 2012)

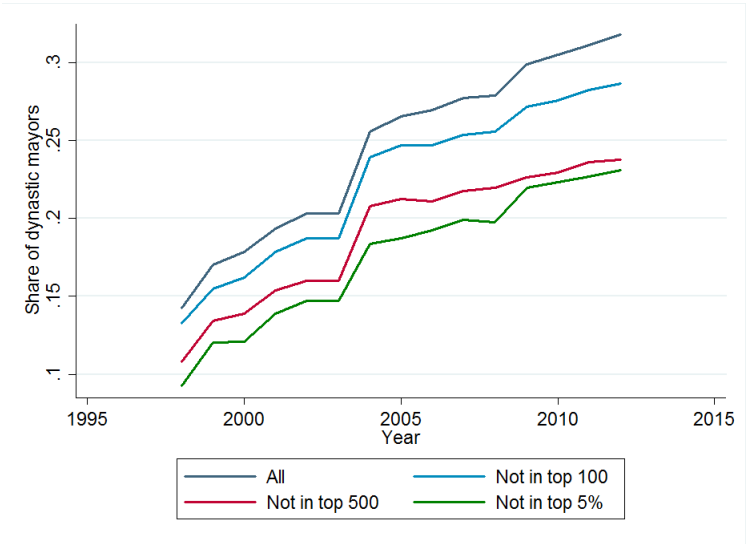
(b) Name concentration by province

Figure 1: Dynastic mayors and name concentration

concentration at the province level: Figure 1b. Name diversity of the total population is very heterogeneous across Italian provinces, and higher in the North than in the South. In the North, the number of surname corresponds to about 10-15% of the number of individuals, while in the South the number of surname corresponds to about 5-10% of the number of individuals.

A second source of heterogeneity stems from the fact that the number of presumably dynastic individuals is not constant over time. Figure 2 highlights this heterogeneity across time, representing the share of dynastic mayors for different categories of mayors, depending on the frequency of their surnames in total population. Over our period of interest (1998-2012), the share of all dynastic mayors doubled between 1998 and 2012, from 15% to more than 30%. If we restrict the sample to individuals whose surname is not among the 100 most common surnames at the province level, the share of assumed dynastic individuals evolves from 13% in 1998 to 28% in 2012. For individuals whose surname is not among the 500 most common names at the province level, this share ranges from 11% In 1998 to 24% in 2012. Finally, for individuals whose surname is not among the 5% most common names within the province, this share ranges from 9% in 1998 to about 23% in 2012. Let us also notice that all of these specifica-

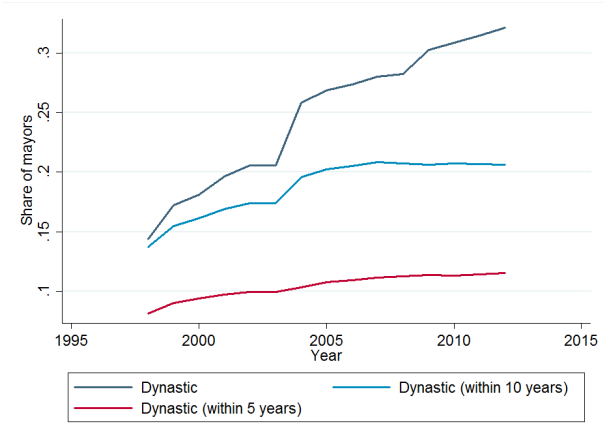
Figure 2: Evolution of dynastic mayors and name concentration



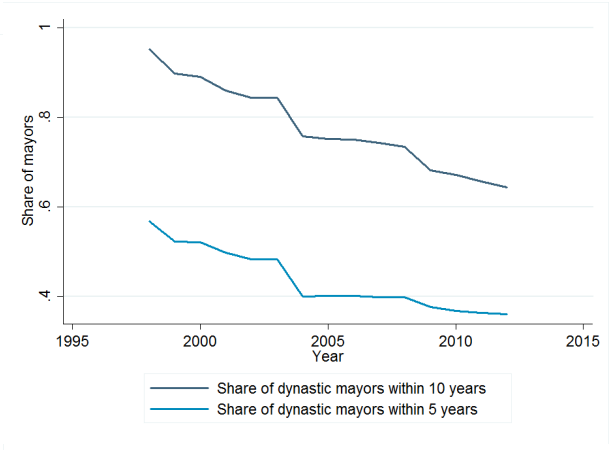
tions are very restrictive, since they drop a large number of observations: about 15% of elected mayors have a surname among the 100 most frequent in their province; about 20-25% have a surname among the 500 most common names; and about 50% have a surname among the 5% most frequent.

However, the huge increase in the number of dynastic candidates reflects the fact that for politicians elected in later years of our dataset, a larger time window is available (i.e. all previous years in our dataset) to determine whether a they are dynastic or not. This can be problematic because the number of individuals not identified as dynastic - while they indeed are - is likely to decrease over time, which can induce a time-varying bias.[5].

[5]Moreover, in Appendix A, we show that the average age difference between first generations and assumed dynastic individuals is increasing over time (from about 8 years in 1998 to about 20 years in 2012). In fact, the distribution of age differences between the first individual of a dynasty (hereafter refered to as "first generations") and his potential successors during the period 1998-2012 is bimodal. The first mode is around 0 and the second one is around 30: this means that on average, between a dynastic individual and the individual who started the dynasty, the two most common age differences are either 0, or 30. This evidence is compatible with the hypothesis that the kind of linkages that we capture the most are either brotherhoods (where the first and second generations of dynastic individuals have almost the same age on average) or father-and-son (where the two generations of dynastic individuals have on average 30 years of difference). Finally, even though our analysis starts in 1998, because of dataset limitations, we can assume that we are relatively more likely to catch brotherhood linkages at the beginning of the period, and relatively more likely to catch father-and-son linkages at the end of the period. See Appendix for further details.



(a) Varying time-windows



(b) Share of all dynastic mayors represented by dynastic mayors identified within 5 and 10 years windows

Figure 3: Dynastic mayors and time windows

In order to address this issue, we provide alternative definitions of dynastic individuals, defining a mayor as dynastic if the first observed individual holding the same name as him entered the municipal council within 10 years or within 5 years before his first appearance in a municipal council. As emphasized in Figures 3a and 3b, dynastic mayors identified within 10 years still correspond to more than 60% of the assumed dynastic individuals in 2012, while those identified within 5 years account for only 35% on that same year.

In the estimation part of our analysis, we use these alternative specifications in terms of name frequency and time-window to control for the robustness of our results.

3.2 Characteristics of dynastic politicians

In this section we provide a comprenshive set of results about how dynastic politicians differ from other politicians. Using a regression discontinuity strategy, we show that dynasties self-perpetuate in the political arena. Moreover, persistence is observable also oustide politics, ad they are likely to have the same job of their ancestors. We also find that dynastic candidates have longer political careers, and they are electorally successful, as they are more likely to win local elections and to get elected in regional parliaments.

3.2.1 Descriptive statistics

Tables 1 and 2 represent the characteristics of dynastic mayors in cities between 1998 and 2012. Overall, dynastic mayors appear to be younger, more educated, more likely to be born in the city they run. The proportion of female is also higher among dynastic mayors, as well as the proportion of mayors representing a civic party (i.e. a party not represented at the national level). Dynastic mayors are less likely to come from a right-wing party, they are proportionally much more represented in the south of the country, and cities ran by them are smaller than cities ran by non-dynastic mayors. Cities run by dynastic mayors also have a higher unemployment rate and lower level of trust. Finally, these descriptive statistics suggest that the per-capita budgets of cities ran by dynastic mayors are much higher than those of non-dynastic cities, and that they increase more between the beginning and the end of the term than in non-dynastic cities (though not along every dimension). We also find that the average length of term for a dynastic mayor is slightly longer than the one of a non-dynastic mayor. Finally, looking at some measures of performances, as the share of actual revenues over expected revenues and the share of due expenditures paid during the year, we find slightly lower performances for dynastic mayors However, as we will see in the next parts of the analysis, some of these facts are driven by structural effects that need to be taken care of.

3.2.2 Members of political dynasties enter younger into politics

Not only do we observe differences across dynastic and non-dynastic mayors, but we also observe these differences across candidates. Our databases allow us to uncover information about dynastic candidates for the mayoral office. Indeed, even though we do not observe directly the characteristics of all candidates between 1993 and 2012, we know the characteristics of those of them who ended up being members of the municipal council[6]. In Table 3, we predict the probability of being a dynastic candidate on several observable characteristics of the candidates, using a linear probability model. In the two first columns, we consider the sample of all candidates for which we have information. In the two last columns, we restrict the sample to elections where information about at least the two best candidates are known. In columns

[6]Specifically, we can observe the best candidates in terms of electoral performance, such as, all mayoral candidates who, even if not elected as mayor, received enough votes to enter in the council as councillor.

Table 1: Characteristics of dynastic mayors

	Non-Dynastic	Obs.	Dynastic	Obs.	Diff.	T-Stat
Mayor characteristics						
Reelected	0.551	8949	0.581	2707	-0.029	-2.687
	(0.497)		(.494)			
Age	51.123	81113	47.915	26141	3.208	47.135
	(9.422)		(10.016)			
Male	0.913	81113	0.898	26141	0.0148	7.262
	(0.282)		(.303)			
Education	14.480	79651	14.704	24790	-0.224	-8.999
	(3.457)		(3.336)			
Born in city	0.498	81113	0.522	26141	-0.025	-6.958
	(0.500)		(.500)			
Experience	11.948	81113	7.180	26141	4.768	106.003
	(6.662)		(5.137)			
Civic	0.590	81113	0.695	26141	-0.104	-30.332
	(0.492)		(.461)			
City characteristics						
South	0.273	81113	.368	26141	-0.095	-29.281
	(0.446)		(.482)			
Population	8088.6	79371	4696.5	25625	3392.1	11.147
	(47716.83)		(17258.763)			
Unemployment	9.322	80780	10.7340	26049	-1.418	-23.429
	(8.287)		(9.115)			
Trust	0.317	70906	.312	22752	.004	3.795
	(0.140)		(.145)			

The considered variables are: reelection rate, age, gender, level of education (as measured by the minimum number of years to obtain a certain degree), the place of birth of the mayor, number of years since the first election in the city council, being a candidate of a civic party (where the base category is being a candidate of a national political party), region, population, unemployment rate and trust
Standard deviations between brackets

Table 2: Characteristics of dynastic mayors

	Non-Dynastic	Obs.	Dynastic	Obs.	Diff.	T-Stat
Average budget						
Total exp.	1582.137	78140	1889.501	25224	-307.364	-7.116
	(2445.329)		(11281.407)			
Current exp.	776.592	78140	886.648	25224	-110.056	-5.931
	(1221.810)		(4720.419)			
Capital exp.	587.691	78143	756.56855	25223	-168.878	-6.920
	(1336.632)		(6403.512)			
Tax rev.	346.746	78146	382.70377	25247	-35.958	-2.581
	(587.284)		(3754.786)			
Loans	138.074	78044	160.904	25204	-22.830	-2.667
	(397.479)		(2287.443)	.	.	.
Capital Transfers	449.084	78037	588.179	25218	-139.095	-8.344
	(1480.282)		(3860.675)			
PBC						
Δ Total exp.	79.741	15344	80.485	5041	-0.744	-0.034
	(1350.649)		(1269.627)			
Δ Current exp.	37.992	15344	44.385	5041	-6.392	-2.325
	(151.959)		(213.781)			
Δ Capital exp.	25.715	15344	24.680	5041	1.036	.050
	(1278.435)		(1240.9415)			
Δ Tax rev.	44.701	15357	58.969	5052	-14.268	-6.505
	(110.467)		(191.77898)			
Δ Loans	3.3721173	15357	16.109071	5047	-12.737	-2.303
	(313.718)		(412.598)			
Δ Capital Transfers	22.426	15357	7.920	5047	14.506	0.758
	(1191.555)		(1140.787)			
Competence						
Term duration	3.632	22337	3.806	6859	-0.174	-7.963
	(1.616)		(1.485)			
Speed of payment	77.805	76622	77.573	24500	0.232	2.985
	(11.270)		(8.155)			
Ability of revenue collection	61.545	76854	60.768	24580	0.777	6.587
	(15.919)		(16.614)			
Growth of private tax base	0.022	36586	0.014	12531	0.008	1.719
	(0.524)		(0.142)			

The considered variables are level and variations of total, current and capital expenditures, tax revenues, contracted loans and received capital transfers (all expressed in euros per capita), duration of the term, speed of payment, ability of revenue collection and yearly growth of the private tax base.
Standard deviations between brackets

Table 3: Characteristics of dynastic candidates

Dynasty	Unrestricted	Unrestricted+FE	Restricted	Restricted+FE
Years of education	-0.003	-0.000	-0.003	-0.000
	(5.22)***	(0.49)	(5.07)***	(0.49)
Years of experience in council	-0.018	-0.018	-0.018	-0.018
	(59.62)***	(52.27)***	(56.73)***	(48.53)***
Male	0.010	0.007	0.011	0.006
	(1.39)	(0.91)	(1.44)	(0.77)
Age	-0.004	-0.003	-0.004	-0.003
	(18.42)***	(13.58)***	(17.06)***	(12.59)***
Born in City	0.099	0.089	0.101	0.091
	(22.23)***	(17.33)***	(21.23)***	(16.21)***
Civic	0.037	0.015	0.037	0.015
	(8.67)***	(2.88)***	(8.11)***	(2.75)***
Name frequency in province	0.285	0.274	0.300	0.288
	(17.96)***	(14.95)***	(17.70)***	(13.96)***
Constant	0.220	0.229	0.213	0.223
	(10.35)***	(9.95)***	(9.42)***	(8.75)***
R^2	0.15	0.34	0.15	0.35
N	61,748	61,748	51,211	51,211

The table reports estimates from a linear regression using dummy variable indicating whether a candidate is dynastic as a dependent variable. Columns 1 and 2 consider all candidates of municipal elections between 1993 and 2014 for whom information was available. Columns 3 and 4 restrict the set of candidates to elections where information about at least the two best candidates were known. Columns 2 and 4 control for city fixed-effects. All specifications include year fixed effects and pooled regressions include region dummies. Standard errors are clustered at the city level.T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

2 and 4, we control for city fixed effects. All specifications include year fixed effects. In every specification, we observe that the younger and less experienced candidates are more likely to be dynastic. Candidates with a lower level of education are also more likely to be dynastic candidates (contrarily to what is suggested in the descriptive statistics, where dynastic mayors are more educated). Finally, dynastic candidates are more present among candidates who run a civic list, and among individuals born in the city.[7]

[7]Moreover, when interacting *Age* and *Experience*, we find that not only dynastic candidates are more likely to be young and inexperienced, but they are also more likely to be young and experienced than young and inexperienced. In other terms, it seems that dynastic individuals enter younger into politics. Such results (reported in a Web Appendix) hold also when restricting the sample to the last years of our data (where the stock of potential first generations of individuals is higher).

3.2.3 Dynastic candidates are more likely to be elected

Data about candidates also enable us determining to what extent being dynastic helps winning elections. Tables 4 and 5 provide information on this point, predicting the probability of being elected using a linear probability model. Specifically, columns 1 and 2 consider all candidates of municipal elections between 1993 and 2014 for whom information was available. Columns 3 and 4 restrict the set of candidates to elections where information about at least the two best candidates were known. In columns 2 and 4, we include city fixed-effects (in this case, we add to the previous set of control variables, also the number of candidates running for the mayoral seat). Across all specifications, in Table 4, it appears that being a dynastic candidate has a positive impact on the probability of being elected (of about 3.5 percentage points). Moreover, this effect does not change when considering incumbent dynastic politicians. The same statement does not hold concerning the interaction with experience (Table 5): while amongst candidates with at least one term of experience within a municipal council dynastic candidates are not more likely to be elected, amongst non-experienced candidates, they are 3-4 points more likely to be elected. Therefore, the dynastic advantage seems to somehow decrease depending on the level of political experience. This might be due to the fact that some of the skills "inherited" are over time acquired also by non-dynastic politicians. Finally, in these two tables the probability of being elected does not depend on the frequency of the name of the candidate at the province level.

3.3 Persistence of political dynasties

Another important feature of political dynasties is that they seem to persist over time. In this section, we evidence two main channels of perpetuation. First of all, power seems to self-perpetuate, as elected individuals are more likely to have a relative in office during the subsequent years than non-elected individuals. Secondly, dynastic politicians seem to have longer political careers at the municipal level.

Table 4: Dynastic advantage and incumbency

Elected	Unrestricted	Unrestricted+FE	Restricted	Restricted+FE
Dynasty	0.032	0.038	0.031	0.037
	(6.28)***	(5.89)***	(5.45)***	(4.97)***
Incumbent	0.388	0.377	0.390	0.401
	(65.23)***	(56.88)***	(56.17)***	(50.16)***
Dynasty*Incumbent	-0.004	-0.008	-0.002	-0.004
	(0.36)	(0.62)	(0.12)	(0.28)
Number of candidates	-0.055	-0.054	-0.045	-0.041
	(38.40)***	(30.81)***	(36.53)***	(25.97)***
Years of experience in council	0.009	0.010	0.009	0.010
	(27.33)***	(25.52)***	(24.65)***	(22.56)***
Years of education	0.007	0.009	0.006	0.009
	(13.51)***	(13.70)***	(11.17)***	(11.36)***
Born in city	0.016	0.027	0.018	0.029
	(4.18)***	(5.01)***	(4.25)***	(4.70)***
Male	0.049	0.054	0.051	0.062
	(7.96)***	(7.21)***	(7.58)***	(7.14)***
Age	-0.003	-0.003	-0.003	-0.003
	(14.81)***	(13.77)***	(13.46)***	(12.23)***
Civic	0.023	0.018	0.022	0.019
	(6.05)***	(3.47)***	(5.28)***	(3.15)***
Name frequency in province	-0.005	0.001	0.004	0.004
	(0.47)	(0.06)	(0.31)	(0.19)
Constant	0.448	0.435	0.431	0.389
	(22.79)***	(18.36)***	(21.10)***	(14.90)***
R^2	0.16	0.20	0.14	0.16
N	63,355	63,355	51,217	51,217

The table reports estimates from a linear regression using dummy variable indicating whether a candidate is elected as a dependent variable.Columns 1 and 2 consider all candidates of municipal elections between 1993 and 2014 for whom information was available. Columns 3 and 4 restrict the set of candidates to elections where information about at least the two best candidates were known. Columns 2 and 4 control for city fixed-effects. All specifications include year fixed effects and pooled regressions include region dummies. Standard errors are clustered at the city level.T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 5: Dynastic advantage and experience

Elected	Unrestricted	Unrestricted+FE	Restricted	Restricted+FE
Dynasty	0.004	0.001	0.002	0.002
	(0.50)	(0.10)	(0.30)	(0.19)
No Experience	-0.127	-0.146	-0.127	-0.148
	(27.37)***	(25.84)***	(24.85)***	(22.93)***
Dynasty*No Exp	0.034	0.042	0.035	0.040
	(3.68)***	(3.82)***	(3.34)***	(3.14)***
Incumbent	0.390	0.379	0.392	0.404
	(73.98)***	(64.80)***	(63.31)***	(57.13)***
Number of candidates	-0.055	-0.054	-0.046	-0.041
	(39.12)***	(30.80)***	(37.30)***	(25.98)***
Years of education	0.007	0.010	0.006	0.009
	(13.76)***	(14.10)***	(11.40)***	(11.71)***
Born in city	0.019	0.030	0.021	0.032
	(4.91)***	(5.71)***	(4.78)***	(5.19)***
Male	0.052	0.060	0.055	0.067
	(8.61)***	(7.95)***	(8.20)***	(7.79)***
Age	-0.002	-0.003	-0.002	-0.003
	(13.18)***	(12.11)***	(12.10)***	(10.89)***
Civic	0.021	0.015	0.020	0.017
	(5.58)***	(2.89)***	(4.93)***	(2.71)***
Name frequency in province	-0.003	0.004	0.006	0.007
	(0.28)	(0.26)	(0.45)	(0.34)
Constant	0.549	0.548	0.532	0.507
	(26.83)***	(22.21)***	(25.07)***	(18.64)***
R^2	0.16	0.20	0.14	0.17
N	63,355	63,355	51,217	51,217

The table reports estimates from a linear regression using dummy variable indicating whether a candidate is elected as a dependent variable. Unexperienced candidates are defined as individuals who spent strictly less than a term in municipal council before the election. Columns 1 and 2 consider all candidates of municipal elections between 1993 and 2014 for whom information was available. Columns 3 and 4 restrict the set of candidates to elections where information about at least the two best candidates were known. Columns 2 and 4 control for city fixed-effects. All specifications include year fixed effects and pooled regressions include region dummies. Standard errors are clustered at the city level. T-Statistic between brackets. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

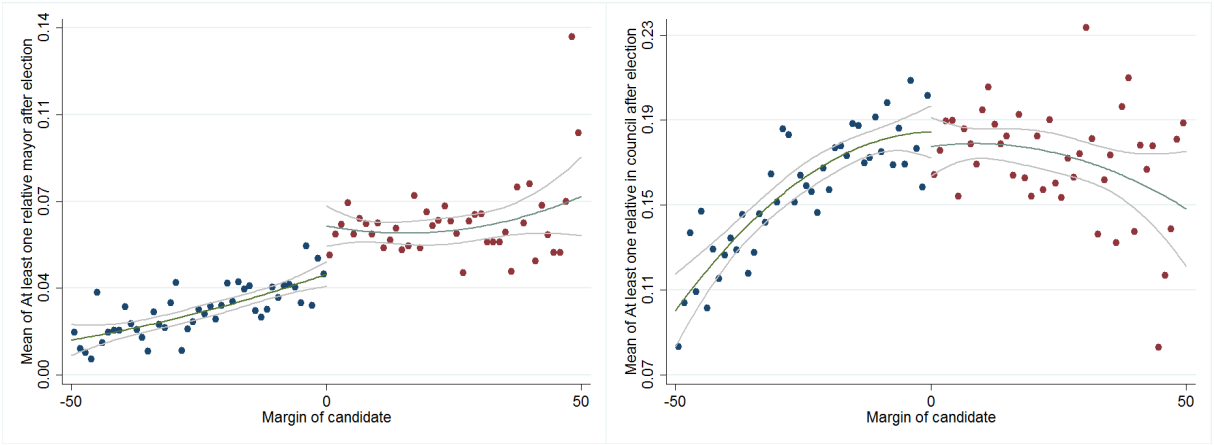
3.3.1 Intergenerational persistence

Consistently with evidence from the literature (Dal Bó et al. (2009); Rossi (2011); Querubin (2010)), we find that power is persistent over time in Italian municipalities. To test for such a persistence, we need to compare the probabilities that an elected candidate and a non-elected candidate have a relative in office in the next years.

To do so, the Regression Discontinuity Design seems particularly interesting, as it allows isolating pure persistence effect from potentially unobserved factors determining both the probability that an individual is elected and the probability that one of his relatives becomes politician (such as charisma, competence, or beauty, for example).[8] In this framework, we compute for each candidate of each election, his margin of votes. For the winner of the election, the margin corresponds to the difference between the share of votes he received and the share of votes received by his best challenger. For all the candidates that lost the election, the margin corresponds to the difference between the share of votes he received and the share of votes received by the winner. This variable takes values between -1 and 1: a positive value indicates that the individual was elected as a mayor, while a negative value indicates that the individual was not elected. Furthermore, for each individual we indicate whether, within the 10 years following the election he ran for, an individual holding the same name as him was elected as mayor (or as a municipal councellor). Figure 4a shows that the average probability that an individual has a relative mayor within 10 years is increasing in his margin of vote and discontinuous around 0: in other terms, elected individuals are more likely to subsequently have a relative in office than non-elected candidates. As the probability of having a relative is discontinuously higher when the margin of votes becomes positive, and to the extent that the margin of votes is continuous around zero [9], a causal interpretation can be inferred. Figure 4b shows that non-elected individuals are much less likely to have a relative in municipal councils (i.e. elected as councilor) during the 10 subsequent years (and much less so when they lost the election by a large margin), but that there is no discontinuity of this variable around the zero-threshold. In other terms, closely elected mayors are not much more likely to have a member of their family in municipal councils during the following years than closely non-elected candidates. In Appendix

[8] We describe this empirical methodology in the next sections of our analysis.

[9] And that other covariates driving electoral performances of dynastic individuals are also continuous around this threshold



(a) Relatives elected mayor (b) Relatives elected as council members

Figure 4: Perpetuation of power - Regression Discontinuity

C, we present detailed results from a regression discontinuity design between closely elected and closely non-elected individuals which confirm that closely elected candidates are significantly more likely to have a relative who becomes mayor during the subsequent 10 years, but not more likely to have a relative that becomes member of the municipal council.

3.3.2 Longevity of dynastic politicians

Not only is political power persistent over generations, but elected individuals who come from political dynasties are also likely to stay longer in municipal councils. In Table 6, we regress the average length of stay (in years) of each observed member of municipal councils on some characteristics (i.e. in this case, our analysis is not restricted to mayors). We define the length of stay as the number of years an individual spent in a municipal council since he first entered it. In columns 1 and 3, we take into account all individuals, while in columns 2 and 4, we only consider individuals who first entered in politics in 1995 or after (in order to be sure to observe their first election in a council). Finally, columns 3 and 4 control for the fact that some term length might be higher in some cities than in others by controlling for city fixed effects. The Table shows that the average number of years an individual spent in a municipal council since his entrance is higher for dynastic individuals than for non-dynastic individuals (between a tenth and a quarter of year depending on the specifications).

Table 6: Longevity of politicians

Length of stay	Full	Restricted	Full+City FE	Restricted+City FE
Dynasty	0.274	0.149	0.165	0.091
	(24.75)***	(14.29)***	(14.67)***	(8.58)***
Age	-0.031	-0.014	-0.029	-0.013
	(74.27)***	(33.14)***	(68.25)***	(29.93)***
Years of education	0.045	0.014	0.077	0.030
	(35.84)***	(10.26)***	(57.75)***	(20.55)***
Male	0.736	0.453	0.795	0.499
	(65.00)***	(40.10)***	(69.93)***	(44.33)***
Born in city	0.238	0.111	0.377	0.224
	(21.80)***	(9.71)***	(33.22)***	(18.58)***
Civic	0.200	0.164	-0.064	0.042
	(19.07)***	(16.19)***	(5.40)***	(3.36)***
South	-0.653	-0.467		
	(58.32)***	(41.10)***		
Constant	8.343	5.557	7.788	4.918
	(83.90)***	(181.76)***	(74.36)***	(154.86)***
R^2	0.20	0.25	0.25	0.32
N	551,067	291,917	551,067	291,917

The table represents estimates from linear regressions, using the length of stay of politicians (in years) in local municipal councils between 1985 and 2012 as dependent variable. Columns 1 and 3 consider all members of city councils from 1985 to 2012. Columns 2 and 4 restrict the sample to members of city councils who were appointed or elected after 1995. Columns 3 and 4 control for city fixed effects. All specifications include year of election fixed effects. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Dynastic politicians have more succesful careers not only in terms of duration but also in terms of climbing the political ladder. In fact, we also find that dynastic politicians are more likely to be elected in provincial or regional parliaments after they entered a municipal council. The results are reported in Table 7. In this case, we consider the entire sample of elected local politcians in the period 1985-2012 and compute our dependent variable as a dummy equal to one if a municipal politician is elected in the provincial or regional parliament (within the same region) at some point in time after his first election in the city council. Even though only 0.5% of all observed members of municipal councils were subsequently part of such higher levels of government, dynastic politicians are significantly more likely to be part of province or region parliaments.

Table 7: Probability of entering provincial or regional administration

	Province		Region	
Member of administration	Full+City FE	Restricted+City FE	Full+City FE	Restricted+City FE
Dynasty	0.001	0.001	0.001	0.000
	(2.27)**	(2.04)**	(3.61)***	(1.79)*
Age	-0.000	-0.000	-0.000	-0.000
	(27.89)***	(12.34)***	(20.99)***	(8.84)***
Years of education	0.002	0.001	0.000	0.000
	(37.08)***	(22.43)***	(20.28)***	(11.37)***
Male	0.007	0.006	0.002	0.001
	(20.00)***	(14.05)***	(7.59)***	(3.86)***
Born in city	0.002	0.001	0.000	-0.000
	(5.23)***	(1.67)*	(1.07)	(0.11)
Civic	-0.003	-0.002	-0.001	-0.000
	(8.36)***	(4.76)***	(3.37)***	(1.37)
Constant	0.005	0.003	0.007	0.003
	(1.43)	(2.63)***	(3.66)***	(4.44)***
R^2	0.03	0.04	0.03	0.04
N	551,285	291,985	551,285	291,985

The table represents estimates from linear regressions, using as a dependent variable a dummy variable indicating whether a local politician was elected to be a member of provincial or regional administration after his entrance in a municipal council. Columns 1 and 3 consider all members of city councils from 1985 to 2012. Columns 2 and 4 restrict the samples to members of city councils who were appointed or elected after 1995. All specifications control for city and year of election fixed-effects. Robust standard errors. T-Statistic between brackets.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

3.3.3 Occupational persistence

Not only is power persistent over time, but the occupations declared by mayors of a same assumed family are also remarkably persistent. Figure 5 represents the transition matrix of occupations between the first observed generation of a family (in rows) and the first subsequent observed politician from this family (in column). Occupations are coded according to the socioprofessional categories of the Italian Census (where 1 indicates occupations requiring higher skills and 9 indicates occupations requiring lower skills, 0 indicates inactive people and 10 indicates retired). Blue (respectively red) bars indicate a significantly higher (respectively lower) proportion of the column category within the row category. From the graph, we can conclude that there is a strong intergenerational persistence of occupations, as most of the blue bars are on the diagonal. Furthermore, offsprings of higher-occupations individuals are themselves more likely to be higher-skilled, while offsprings of lower-occupations individuals are more likely to be lower skilled. Therefore, while this graph cannot help us concluding on whether intergenerational occupational persistence is higher among dynastic politicians than among non-dynastic politicians, it still emphasizes a strong persistence of occupations amongst members of political dynasties.

4 Consequences of political dynasties on local budgets

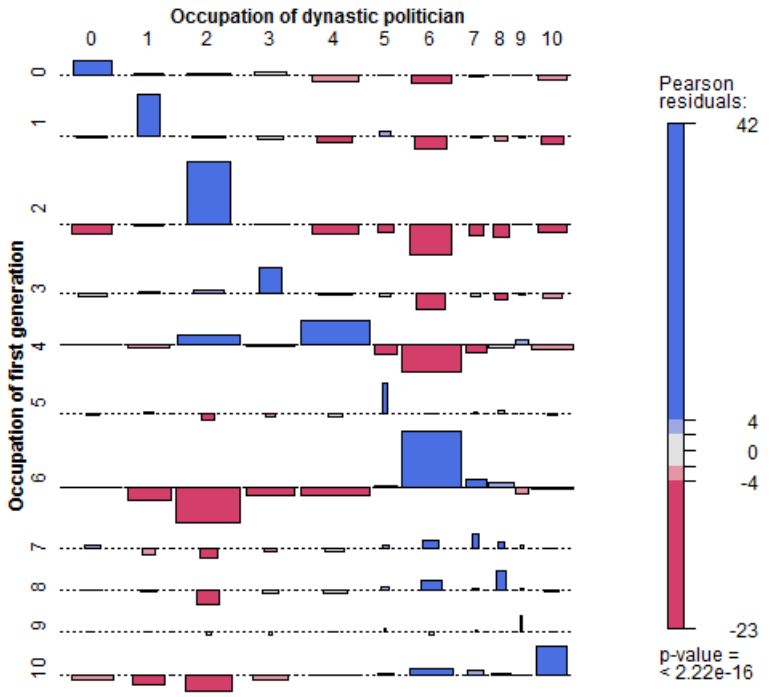
The evidence presented in the last sections show that dynastic politicians differ upon several dimensions from non-dynastic politicians. Importantly, such differences might be linked to different behaviors while in office as elected mayors. In the next sections, we test such hypothesis.

4.1 Panel regressions

4.1.1 Identification strategy

To explore the effects of dynastic mayors on the municipal budgets, we first use a fixed-effects approach. We are interested in two specific features: first of all, we test to what extent the size of the components of municipal budgets varies across dynastic and non-dynastic mayors. Secondly, we test for the presence of a political budget cycles at the municipality level and

Figure 5: Persistence of occupations between generations



Note: The categories refer to the following occupations: 0: Inactive; 1: Legislators and entrepreneurs; 2: Intellectual and scientific professions; 3: Technical professions; 4: Executive positions in office work; 5: Qualified professions in business and services; 6: Artisans, specialized workers and farmers; 7: Site manager and machine operator; 8: Unskilled jobs; 9: Armed forces; 10: Retired

compare their magnitude between dynastic and non-dynastic mayors. In each case, we use the fact that mayors cannot be in office more than twice in a row to isolate the potential effects of reelection incentives. Practically, this amounts to interacting the variable of interest with a dummy indicating whether the mayor is in her first or second term. In our first strategy, we run fixed-effects panel regressions on the full sample of observations between 1998 and 2012.

In a first specification, we test for the effect of dynastic mayors on average revenues and expenditures using the following specification:

$$Y_{it} = \alpha + \beta TL_{it} + \delta D_{it} + \kappa(D_{it} * TL_{it}) + \nu X_{it} + \gamma_t + \epsilon_i + u_{it} \tag{1}$$

where $Y_{i,t}$ is an outcome variable for city i in year t, TL_{it} is a dummy equal to 1 if the mayor is term-limited, D_{it} is a dummy equal to 1 if the mayor of city i in year t is presumably dynastic, X_{it} is a set of city characteristics for city i in year$t$, γ_t is a year-fixed effect, ϵ_i is a city fixed-effect and u_{it} is a time-varying error term.The parameter δ indicates, for mayors who are not term limited, the difference of outcome variable between dynastic and non-dynastic mayors. The parameter κ indicates the difference of effect of dynastic mayors between non-term-limited and term-limited mayors.

In a second specification, we test for presence of stronger political budget cycles for dynastic mayors by estimating the following equation:

$$\begin{aligned} Y_{it} =& \alpha + \beta TL_{it} + \delta D_{it} + \phi^0 LY_{it} \\ &+ \phi^1(D_{it} * TL_{it}) + \phi^2(LY_{it} * D_{it}) + \phi^3(LY_{it} * TL_{it}) \\ &+ \phi^4(LY_{it} * TL_{it} * D_{it}) \\ &+ \gamma X_{it} + \gamma_t + \epsilon_i + u_{it} \end{aligned} \tag{2}$$

where LY_{it} is a dummy equal to 1 if the next election in city i at time t occurs during the following year and 0 otherwise. As this equation includes a triple interaction, we are interested in several parameters. First of all, the parameter ϕ^0 indicates, for mayors who are non-term-limited and non-dynastic, the difference of outcome between the last year of the term and the three first years. The parameter ϕ^2 indicates, for non-term-limited mayors, to what extent this difference is higher for dynastic mayors than for non-dynastic mayors. The parameter ϕ^3 indicates, for mayors who are non-dynastic, to what extent the variation in the last year of the term is higher for term-limited than for non-term-limited mayors. Finally, the parameter ϕ^4 indicates to what extent the difference of political budget cycle between term-limited and non-term-limited mayors is higher for dynastic politicians.

Table 8: Year of election of mayors in the sample (between 1998 and 2012)

	Panel specification		**RDD specification**	
Year	All obs.	Restricted	All obs.	Restricted
1999	4,312	3,855	992	933
2000	841	605	129	104
2001	1,182	880	307	262
2002	891	586	264	217
2003	437	260	133	117
2004	4,202	3,660	1,228	1,102
2005	811	607	133	106
2006	1,178	902	395	345
2007	870	655	297	255
2008	500	358	171	142

The restricted samples are the samples actually used for the estimation of the political budget cycles. In the panel specification, it includes all cities in which we observe two full terms after 1999. In the RDD specification, we include all elections where information on the two best candidates are known, where at least one of them is dynastic, and where the subsequent term is complete (i.e. 5 years long).

Note that we are able to identify political budget cycles thanks to the fact that for each city, the electoral calendar is exogenously defined *ex ante*, and that municipal elections do not occur during the same year for each city. We are therefore able to separate year fixed-effects and the effect of distance to the next election. Furthermore, to make sure that we properly estimate political budget cycles, we only keep cities meeting a certain number of criteria. In the fixed-effects estimation, we only keep cities where two full terms of five years are observed (i.e. for elections occurring after 1999). This ensures that we avoid cases of early termination and that

Table 9: Effect of political dynasties and term limits on average budget

	Total exp	Current exp	Cap. exp	Tax rev	Loans	Cap. transfers
Dynasty	29.117	0.318	22.866	3.053	14.208	11.516
	(1.22)	(0.06)	(1.15)	(1.28)	(2.40)**	(0.63)
TL	-6.233	4.536	-13.383	-0.939	-1.659	-7.193
	(0.54)	(1.74)*	(1.36)	(0.71)	(0.54)	(0.80)
Dynasty*TL	1.095	12.797	-0.312	1.134	-17.097	4.473
	(0.04)	(2.07)**	(0.01)	(0.37)	(2.16)**	(0.22)
Constant	1,414.837	715.865	498.142	273.307	98.899	363.518
	(19.16)***	(29.73)***	(8.52)***	(23.93)***	(5.12)***	(6.62)***
R^2	0.02	0.24	0.02	0.42	0.01	0.01
N	45,908	45,908	45,908	45,908	45,906	45,906

The table presents estimates from fixed-effects panel regressions, using categories of public expenditures and income as dependent variables (all are expressed in euros per capita, and winsorized at the 1% level). The main explanatory variables are a dummy for the mayor being dynastic, and a dummy indicating whether the mayor is term-limited. The sample is made of all cities where two full terms of five years were observed between 1999 and 2012. Years of election are excluded from the estimation. All specifications control for city and year fixed-effects, and control for sex, age, experience, years of educations and place of birth of the mayor, as well as population size. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

we have enough intra-city variation in terms of explanatory variables to identify separately all the effects mentioned above.[10] Overall, this amounts to keeping a sample of 6,184 cities for the fixed-effects analysis (see Table 8 for the full list of elections by year).[11] Finally, in order to avoid potential outliers, we winsorize the outcome data at the 1% level.

4.1.2 Estimation

Table 9 reports the estimation results for average budget components. The reported variables of interest are total expenditures, current expenditures, capital expenditures, taxes, loans and capital transfers from the regional and national government (expressed in Euros per capita). Each regression controls for age, experience, and years of education of the mayor. Covariates also include dummies indicating whether the mayor is born in the city, and whether she is from a civic party. Finally, we also control for the population of the city.

[10] In the Regression Discontinuity Design framework implemented in the next sections, because the inference lies upon inter-city variation (as opposed to intra-city variation in the fixed-effect framework) we impose a slightly less stringent constraint and keep cities where at least one full term is observed between 1999 and 2012.

[11] And of 2938 cities in the Regression-Discontinuity Design specification: the number of cities present in this specification is smaller as additional constraints to identify closely-elected dynastic candidates are necessary (see next section).

In Table 9, we observe no effect of political dynasties on average current and capital expenditures. We do not find any effect on tax revenues and capital transfers from upper layers of the government. However, it appears that dynastic mayors in their first term contract more loans (14 euros per capita on average) than non-dynastic mayors in their first term. We also observe that dynastic term-limited mayors contract much less loans than their non-dynastic counterparts (about 17 euros less).

But while few effects are noticeable in terms of average budget, we find much more variation in terms of political budget cycles. Several conclusions can be drawn from the analysis of Table 10. First, in cities ran by non-dynastic mayors in their first term, expenditures are much higher at the end of the term than at the beginning: in particular, total expenditures per capita are higher by about 48 euros in the last year of the term. This is mainly due to an increase in capital expenditures (which are 39 euros higher). This increase in expenditures seems to be especially financed by an increase in capital transfers from the government and the region (with a difference of about 23 euros per capita between the last year and the three first years of the term), and in contracted loans (with a difference of about 13 euros per capita). However, tax revenues seem to decrease during the last year of the term, the difference being of about 10 euros. Put differently, we observe a strong political budget cycle in our sample: before the elections, non-dynastic and non-term-limited mayors increase capital expenditures and reduce taxes, while increasing loans and transfers from upper layers of the governement.

Second, among mayors who are not term limited, political budget cycles are much higher for dynastic mayors. Indeed, between the last year and the previous years of the term the variation in total expenditures per capita of non-term-limited mayor is 72 euros higher for dynastic than for non-dynastic mayors. This higher political budget cycle comes mostly from a substantial additional increase in capital expenditures during the last year of the term (68 additional euros compared to the three first years), and from an additional increase in current expenditures per capita (about 9 euros). This additional increase in expenditures during the last year of the term is mostly financed by capital transfers from the state and the region (with an additional difference of 55 euros between the last year and the three first years of the term), and contracted loans

(with an additional difference of 20 euros), while taxes increase by an additional 7 euros during the last year (meaning that in absolute term, non-term-limited dynastic mayors do not increase taxes during the last year of their term). Therefore, the political budget cycle of dynastic mayors in their first term is much more pronounced than the one of non-dynastic mayors in their first term: they spend relatively much more at the end of the term than non-dynastic individuals, and finance this additional increase in expenditures mostly through capital transfers from the state and the region. The magnitude of the effect is sizable, as the increase in total expenditures of dynastic mayors is 50% higher than the increase in total expenditures of non-dynastic mayors, while it is 75% higher for capital expenditures, 53% higher for loans and 138% higher for capital transfers). The relative increase of expenditures and revenues of dynastic mayors prior to the election represents an increase of 6% of a standard deviation in total expenditures, of 8% of a standard deviation in capital expenditures, of 9% of a standard deviation in loans, and of 7% of a standard deviation in capital transfers.

A third conclusion from this table is that among non-dynastic mayors, the political budget cycle is not markedly different between term-limited and non-term-limited individual. Except for taxes, which are relatively higher at the end of the term for term-limited individuals, we find no significant difference in the evolution of other variables.

However, we observe a difference of political budget cycle between non-term limited and term-limited dynastic mayors: indeed, compared to non-dynastic term-limited mayors (who do not spend significantly less at the end of the term than non-dynastic reeligible mayors) dynastic term-limited mayors seem to reduce expenditures, loans and capital transfers at the end of the term relatively to their counterparts who are reeligible. Though we observe significant coefficients (at the 10% level) only for loans and capital transfers, the magnitude of these negative coefficients (*Dynasty*LY*TL*) is similar to the magnitude of the positive coefficients indicating the relative PBC of non-term-limited dynastic mayors (*Dynasty*LY*), and especially so on the revenues side. This tends to indicate that term-limited dynastic mayors do not have more political budget cycles that non-dynastic term-limited mayors: put differently, the difference of

Table 10: Effect of political dynasties and term limits on PBC

	Total exp	Current exp	Capital exp	Tax rev	Loans	Cap. transfers
Dynasty	9.846	-1.909	4.786	1.219	8.941	-2.932
	(0.40)	(0.37)	(0.23)	(0.51)	(1.46)	(0.15)
LY	47.681	-1.870	39.376	-8.615	12.939	23.346
	(4.08)***	(1.10)	(3.70)***	(8.02)***	(3.72)***	(2.45)**
TL	-3.537	3.831	-10.887	-3.083	-0.774	-4.617
	(0.28)	(1.37)	(1.02)	(2.26)**	(0.24)	(0.47)
Dynasty*LY	72.094	8.887	68.281	7.762	19.784	55.590
	(3.11)***	(2.97)***	(3.18)***	(3.63)***	(2.87)***	(2.84)***
LY*TL	-8.052	2.897	-7.732	8.367	-2.851	-8.881
	(0.50)	(1.26)	(0.53)	(5.52)***	(0.61)	(0.69)
Dynasty*LY*TL	-43.565	-2.188	-56.813	-5.263	-19.582	-57.615
	(1.11)	(0.45)	(1.60)	(1.50)	(1.79)*	(1.84)*
Constant	1,433.995	717.003	513.598	273.611	103.562	371.909
	(19.39)***	(29.77)***	(8.76)***	(23.91)***	(5.35)***	(6.74)***
R^2	0.02	0.24	0.02	0.42	0.01	0.01
N	45,908	45,908	45,908	45,908	45,906	45,906

The table presents estimates from fixed-effects panel regressions, using categories of public expenditures and income as dependent variables (all are expressed in euros per capita, and winsorized at the 1% level). The main explanatory variables are a dummy for the mayor being dynastic, a dummy indicating whether the mayor is term-limited, and a dummy indicating pre-electoral years. All outcome variables are expressed in euros per capita. The sample is made of all cities where two full terms of five years were observed between 1999 and 2012. Years of election are excluded from the estimation. All specifications control for city and year fixed-effects, and control for sex, age, experience, years of educations and place of birth of the mayor, as well as population size. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

political budget cycles observed between dynastic and non-dynastic mayors during their first term partially disappears for mayors in their second term.

4.2 Matching on Discontinuity

4.2.1 The Regression Discontinuity Design setting (RDD)

Even though the electoral schedule is exogenous, the effects identified in the panel regressions might be biased if the unobserved mayor and city characteristics are correlated with both dynasty and the outcome. This could happen for example if the voters chose their candidate depending on criteria that affect both the probability of having a dynastic mayor and the policies implemented. Also if the probability of election of dynastic candidates is affected by policy

outcomes or by the political budget cycle, then our estimated effect might be biased.

In order to address these issues, a strategy could be to use a regression discontinuity design, focusing on close elections between dynastic and non-dynastic candidates. We define the forcing variable as the difference of vote shares between the best dynastic candidate and the best non-dynastic candidate. This variable can take any value between -1 and 1, and it takes a positive value if a dynastic mayor is elected. The intuition behind this methodology is that the assignment of dynastic or non-dynastic mayors in elections won by a narrow margin is as good as random. Our setting involves a sharp regression discontinuity design. Indeed, let us call D_i the dummy variable indicating whether a dynastic mayor is elected and X_i the margin of the best dynastic candidate. In this case, we have:

$$D_i = \mathbf{1}[X_i > 0]$$

Assuming that the threshold cannot be manipulated (i.e. that the forcing variable is not discontinuous around the forcing threshold of 0), and that there exists no discontinuity in other potential confounding factors around the threshold, we can estimate the effect of dynasty as a Local Average Treatment Effect (LATE), corresponding to the discontinuity of the observed variable at the threshold. Denoting $Y_i(0)$ the outcome variable of a city not ran by a dynastic mayor and $Y_i(1)$, the outcome variable of a city ran by a dynastic mayor, we seek to estimate the following local average treatment effect at the threshold $X_i = 0$:

$$\beta = \mathbf{E}[Y_i(1) - Y_i(0)|X_i = 0]$$

Such an estimate can be found by running the following regression:

$$Y_{it} = \alpha + \beta D_{it} + \delta P(X_{it}) + \gamma P(X_{it})D_{it} + \epsilon_{it}$$

where Y_{it} is the outcome of interest in city i over the *term* t, D_{it} is a dummy equal to 1 if the elected mayor is dynastic and $P(X_{it})$ is a polynomial function of the margin of victory of the mayor. The estimated effect of dynastic mayors is therefore the coefficient $\hat{\beta}$.

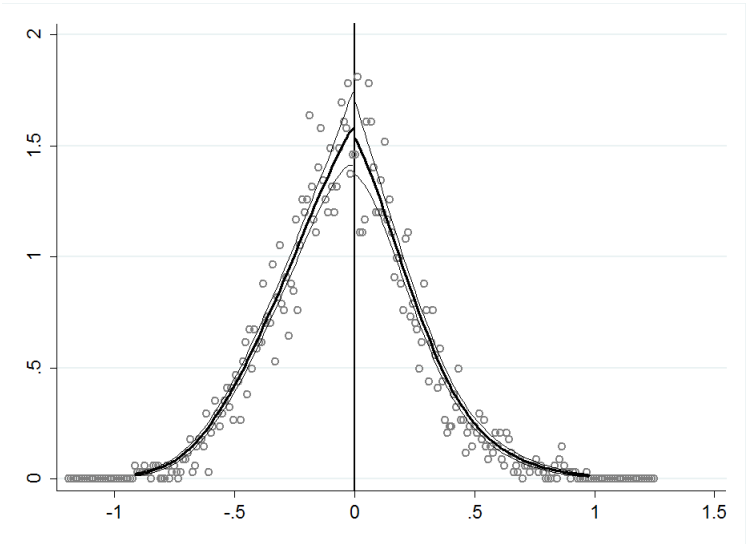
However, as pointed by Hahn et al. (2001) and as summarized by Lee and Lemieux (2010), for the observations below the threshold to be a good counterfactual of individuals on the right of the threshold, and the estimate $\hat{\beta}$ to be unbiased, the potential outcomes $\mathbf{E}[Y_i(1)|X]$ and $\mathbf{E}[Y_i(0)|X]$ must be continuous around the threshold. This implies that if some control variables correlated with the outcome variable are also discontinuous around the threshold, the estimated local treatment effect is likely to be biased. In the following paragraphs, we show that this is precisely what is happening in our setting.

4.2.2 Why Regression Discontinuity is not feasible in our setting

As explained above, the Regression Discontinuity provides unbiased estimates of the treatment if the threshold of the forcing variable cannot be manipulated. This amounts to testing whether the running variable is continuous around the threshold. To check the validity of this hypothesis in our framework, we run a McCrary test (McCrary (2008)), whose results are presented in Figure 6. To identify the margin of dynastic and non-dynastic candidates, we only kept elections where information on at least the two best candidates were known, and at least one dynastic candidate was identified. As previously explained, we only present results for cities where full terms of five years are observed (the number of elections meeting these criteria are presented in Table 8 above). The test suggests that the margin of dynastic candidates on the panel of elections we consider does not seem to be discontinuous around zero.

Another key hypothesis of the Regression Discontinuity Design is that around the threshold, the allocation of the treatment (namely, having a dynastic mayor or not), should be as good as random. Put differently, we should not observe any significant discontinuity around the threshold for other covariates. However, as emphasized in Figure 7, age and experience are markedly lower for dynastic mayors. As shown in Figure 8, the main other control variables are overall balanced around the threshold (except for the place of birth of the mayor, as dynas-

Figure 6: McCrary test on the RDD sample of municipal elections



tic mayors are more likely to be born in the city). Table 11 confirms this intuition: it gathers results from the estimation of a regression discontinuity where we estimate a local polynomial regression with polynoms of order 1, using an optimal bandwidth selected according to the methodology developed by Calonico et al. (2014) and a uniform kernel. Errors are clustered at the municipal level. Overall, around the threshold, dynastic mayors are 2.5 years younger, have spent 6 years less in the municipal council (which corresponds to more than a term of difference) and are 10 points more likely to be born in the city than non-dynastic mayors.

We also show, as an indicative benchmark, to what extent outcome variables are discontinuous around the threshold. For ease of presentation, we report only graphs and results concerning the political budget cycle (we show the outcomes on average spending - which are similar to the fixed effects estimations - in Appendix D). Figures 9 and 10 respectively report variation of expenditures and revenues between the last year of the term and the previous years, as a function of the margin of the best dynastic candidate, for mayors in their first and second term. The results suggest that the variation of total expenditures, capital expenditures and capital transfers is clearly discontinuous at the threshold, and markedly higher for dynastic mayors. However, this appears true only for mayors in their first term. Table 12, which reports estimates of the discontinuity of these different variables at the threshold (following the same

methodology as the one used for the covariates) confirms the graphical representation: total expenditures, capital expenditures and capital transfers per capita increase much more during the last year of the term for dynastic mayors who can run for re-election. Specifically, the difference of variation between dynastic and non-dynastic mayors is of about 190 euros per capita for total and capital expenditures, and of about 169 euros per capita for capital transfers. While these coefficients are much higher than in the fixed-effects specification, we cannot compare them directly, as the selected samples are different.

Despite the clear discontinuities of outcome variables around the threshold, the fact that dynastic mayors are significantly younger and less experienced around the threshold is a major concern. Moreover, as emphasized by Alesina et al. (2015), young mayors in Italian municipalities have higher political budget cycles. Therefore, the estimates provided in Table 12 cannot have a causal interpretation, as estimating a Regression Discontinuity Design cannot guarantee that we isolate the pure effect of being a dynastic mayor[12][13].

[12]Note that the concern that our dynastic measure mostly captures politicians sharing the same surname instead of real family ties is dismissed by the strong discontinuities of age and experience which confirm the fact that we are identifying two groups of individuals (dynastic and non-dynastic) with different characteristics.

[13]More generally while the Regression Discontinuity Design framework is useful for causal inference, it might not be suitable to draw general conclusions: indeed, the nature of the design imposes the estimation to be made on observations with a high level of political competition, and does not allow us generalizing to a less competitive framework.

Figure 7: Discontinuity of age and experience (full sample)

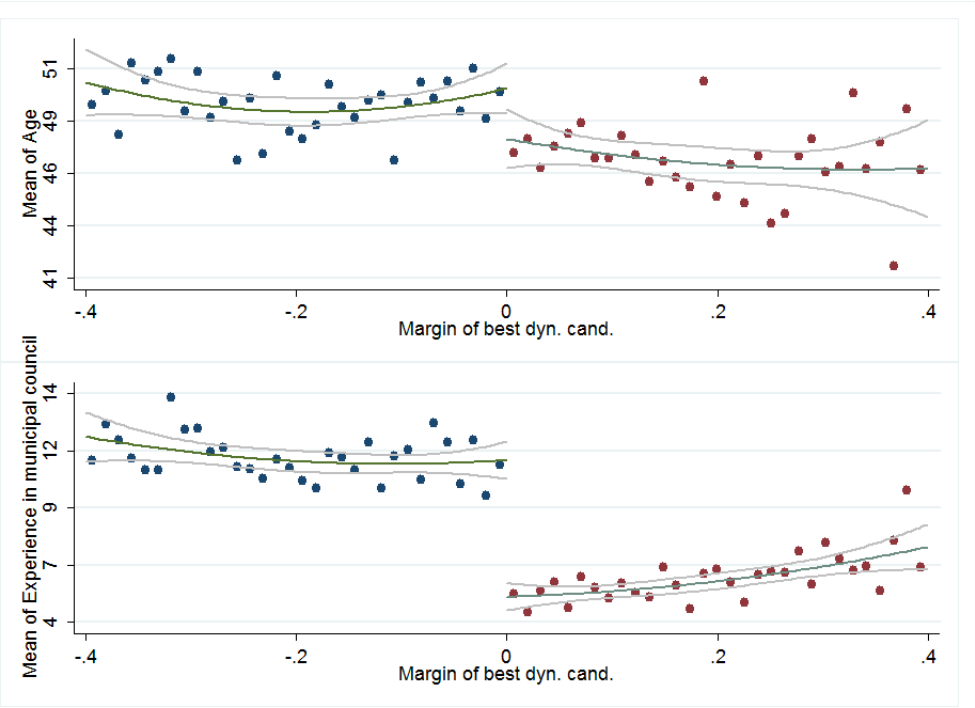


Figure 8: Discontinuity of other variables (full sample)

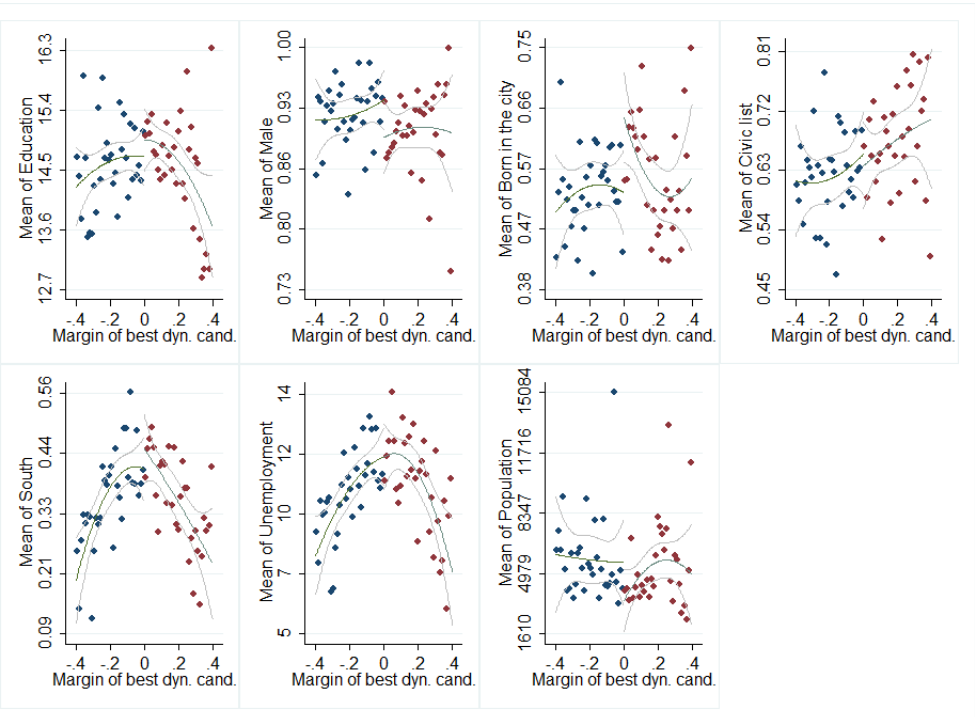


Figure 9: Variation in expenditures and revenues (first term)

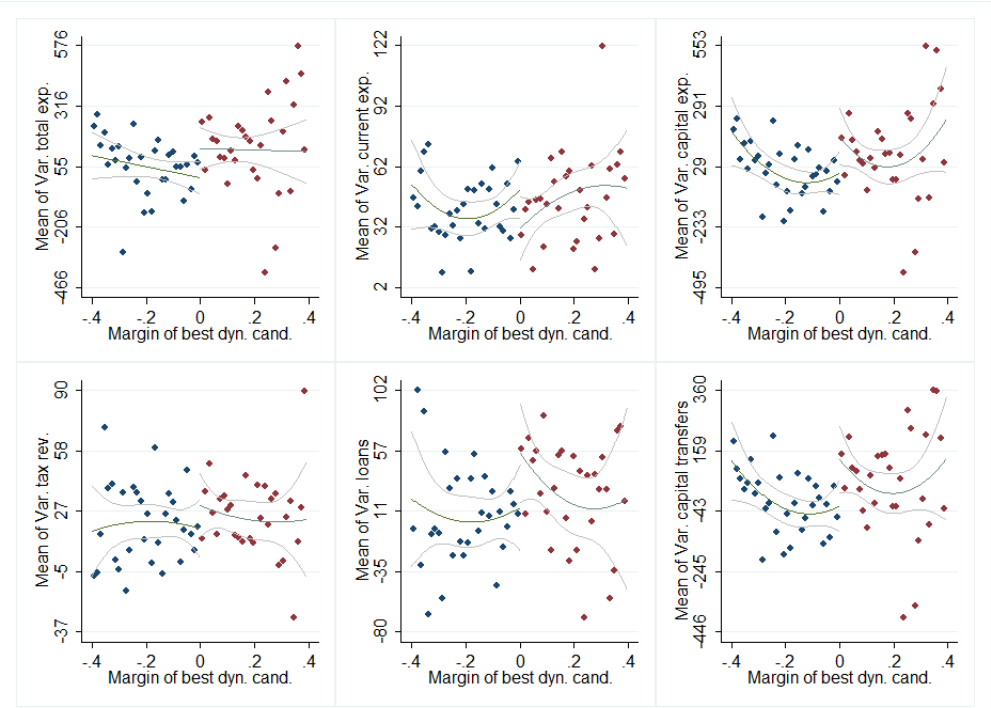


Figure 10: Variation in expenditures and revenues (second term)

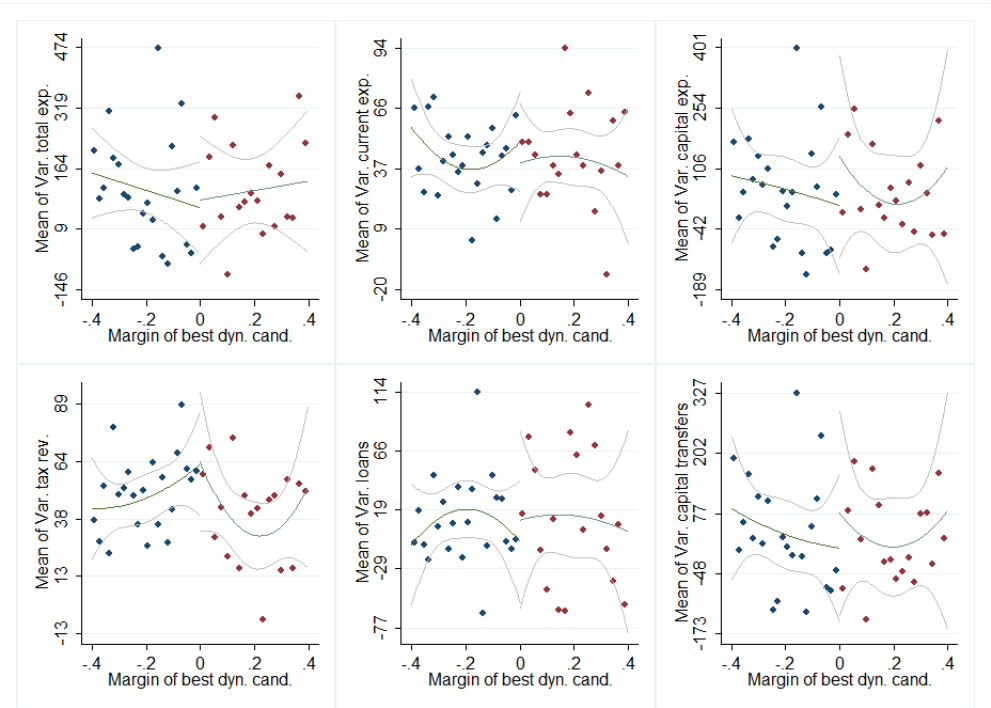


Table 11: Discontinuity of covariates around the threshold

	Age	Exp	Born same city	Sex	Education
Dynasty	-2.490	-5.943	0.103	-0.036	0.231
	(2.78)***	(11.21)***	(2.10)**	(1.34)	(0.78)
Bandwidth	0.21	0.17	0.16	0.15	0.21
N	1,959	1,687	1,620	1,518	1,940
	Civic	Reg South	Pop	Unemployment	Trust
Dynasty	0.005	0.026	-1,185.719	0.207	-0.016
	(0.09)	(0.56)	(0.75)	(0.29)	(1.05)
Bandwidth	0.15	0.18	0.15	0.22	0.13
N	1,519	1,711	1,526	2,098	1,201

The table presents results from a regression discontinuity estimation using an optimal bandwidth calculated thanks to the Calonico et al. (2014) method, using uniform bandwidth and controlling for an order-one polynom of the margin of victory of the best dynastic candidate. Dependent variables are characteristics of mayors and of their city. The sample consists in all full mayoral terms of five years, for years of election between 1999 and 2012. Age corresponds to the age of the mayor during the middle of its term, and experience corresponds to the experience of the mayor at the beginning of the term. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 12: Discontinuity of political budget cycles

No Term Limit	Δ Total exp	Δ Current exp	Δ Capital exp	Δ Tax rev	Δ Loans	Δ Transfers
Dynasty	191.170	-17.826	196.863	10.234	51.182	169.504
	(2.25)**	(1.40)	(2.47)**	(0.87)	(1.75)*	(2.57)**
Bandwidth	0.12	0.12	0.13	0.15	0.13	0.16
N	898	851	910	1,046	940	1,099
Term Limit	Δ Total exp	Δ Current exp	Δ Capital exp	Δ Tax rev	Δ Loans	Δ Transfers
Dynasty	31.017	-2.869	124.021	-4.463	74.036	83.421
	(0.19)	(0.17)	(0.75)	(0.23)	(1.50)	(0.68)
Bandwidth	0.21	0.16	0.20	0.18	0.18	0.24
N	482	359	444	406	396	549

The table presents results from a regression discontinuity estimation using an optimal bandwidth calculated thanks to the Calonico et al. (2014) method, using uniform bandwidth and controlling for an order-one polynom of the margin of victory of the best dynastic candidate. Dependent variables are the difference of categories of expenditures and revenues between the last year and the average of the three first years, winsorized at the 1% level. The sample consists in all full mayoral terms of five years, for years of election between 1999 and 2012. Age corresponds to the age of the mayor during the middle of its term, and experience corresponds to the experience of the mayor at the beginning of the term. Regressions are ran separately on the sample of term-limited and non-term-limited elected mayors. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

4.2.3 Matching on Discontinuity: Estimation strategy

Because some covariates are unbalanced around the threshold, we use a method of matching on discontinuity to control for the impact of potential confounding factors, while still exploiting the strict treatment assignment provided by the threshold of the forcing variable. In the spirit of Alesina et al. (2015), we assume that for observations with values of X_i located in $]-b;b[$, if the following two conditions hold, then we can provide an unbiased estimate of the treatment (namely, the fact of having a dynastic mayor):

$$Y_i(0), Y_i(1) \perp D_i | Z_i \tag{3}$$

$$0 < P(D_i = 1 | Z_i) < 1 \tag{4}$$

Where $Y_i(0)$ and $Y_i(1)$ are respectively the potential outcomes of non-dynastic and dynastic mayors, D_i is the dummy variable indicating whether mayor i is dynastic or not, and Z_i represents the set of observed covariates we control for.

The first condition states that the fact of electing a dynastic mayor is independent of the potential outcomes of the election, conditional on other covariates. If this hypothesis is satisfied, this controls for the potential biases induced by the discontinuity of confounding factors around the threshold. The second condition simply states that for any set of observed characteristics, there exists a common support so that we can observe both treated and untreated individuals.These hypotheses are strong, as this methodology enables estimating a causal effect of political dynasty only to the extent that observable characteristics account for all of the selection bias. Put differently, our results can have a causal interpretation only if we consider that comparing dynastic and non-dynastic mayors with the same observable characteristics is enough to control for selection effects.

In our estimation, we provide results for different bandwidths around the forcing threshold. As in the previous paragraph, we focus on the effects on PBC and we report the estimates on average spending in the Appendix. Namely, we restrict the samples to observations where the margin of the candidate is inferior to 3%, 6% and 9% in absolute value.[14] We use propensity score matching on all the observable variables studied above to control for confounding factors. In order to insure balance between the matched treated and untreated observations, we allow for multiple matching where we associate to each dynastic mayor its 3 closest counterparts among non-dynastic mayors, discarding potential matches located outside of 0.2 standard deviation caliper. For each of the defined subsamples, we provide evidence that our methodology successfully reduces imbalance in terms of age and experience, while not significantly increasing imbalances on other covariates.[15]

4.2.4 Estimation

We first begin by showing that our estimation strategy actually reduces imbalances between the matched dynastic and non-dynastic mayors. For ease of exposition, we only consider mayors in their first term.[16] As in the regression discontinuity design, we consider only full terms after 1998. Tables 13, 14 and 15 respectively present results for restricted samples of observations between a 3%, 6% and 9% bandwidths around the threshold. As suggested by the tables, our matching methodology reduces the imbalances on age and experience, while not significantly affecting most of the other covariates (except for the probability of being born in the city in the sample where the margin of election is below 9%).

The results of our estimations for mayors in their first term are presented in Table 16.[17] In line with previous results, we find that reeligible dynastic mayors increase significantly more their

[14] Contrarily to Regression Discontinuity Design, we do not apply optimal bandwidth in this estimation, as these bandwidths depend on the outcomes variables, while it is generally acknowledged that matching procedures and reduction in imbalances should not depend on the outcome variable. As a consequence, the balance checks presented below hold for all the considered outcomes variables.

[15] The choice of the number of neighbours comes from a tradeoff between bias and variance: indeed, increasing the number of matched pairs increases the amount of treated information (thus potentially increasing the accuracy of the estimated treatment effect), but increases the average distance between the compared treated and untreated units (thus potentially increasing the bias). In the next paragraphs, we show that this methodology is successful in reducing imbalances between compared dynastic and non-dynastic mayors.

[16] Balance checks for mayors in their second term are presented in a Web Appendix.

[17] Note that in the case of term-limited mayors, we only report results for the 9% bandwidth as when considering smaller bandwidths the number of observations is too small, i.e. less than 100.

Table 13: Reduction of bias in propensity score matching (3% bandwidth)

Variable	Unmatched Matched	Dynastic	Non-Dynastic	P-value
Experience	U	4.63	11.20	0.00
	M	6.27	6.23	0.90
Age	U	45.82	50.15	0.00
	M	46.61	46.77	0.86
Male	U	0.91	0.92	0.60
	M	0.93	0.91	0.63
Born in city	U	0.50	0.54	0.59
	M	0.47	0.47	0.90
Education	U	15.08	14.73	0.43
	M	14.95	15.28	0.32
South	U	0.41	0.35	0.35
	M	0.38	0.37	0.81
Civic	U	0.61	0.64	0.61
	M	0.63	0.65	0.59
Unemployment	U	11.03	10.31	0.52
	M	10.48	10.91	0.62
Population	U	4208	4424	0.81
	M	4336	4060	0.71

This table reports the average of the considered variables among dynastic and non-dynastic elected mayors, in unmatched and matched samples, and reports P-values of tests of differences between means across dynastic and non-dynastic individuals.

Table 14: Reduction of bias in propensity score matching (6% bandwidth)

Variable	Unmatched Matched	Dynastic	Non-Dynastic	P-value
Experience	U	4.74	10.73	0.00
	M	6.36	6.56	0.24
Age	U	46.22	49.37	0.00
	M	46.58	46.83	0.70
Male	U	0.90	0.93	0.20
	M	0.92	0.93	0.52
Born in city	U	0.56	0.52	0.36
	M	0.48	0.52	0.27
Education	U	15.04	14.57	0.15
	M	14.82	14.88	0.80
South	U	0.45	0.36	0.06
	M	0.41	0.42	0.73
Civic	U	0.63	0.65	0.62
	M	0.62	0.63	0.72
Unemployment	U	11.79	10.58	0.13
	M	10.87	11.55	0.24
Population	U	4600	6896	0.38
	M	4905	4579	0.50

This table reports the average of the considered variables among dynastic and non-dynastic elected mayors, in unmatched and matched samples, and reports P-values of tests of differences between means across dynastic and non-dynastic individuals.

Table 15: Reduction of bias in propensity score matching (9% bandwidth)

Variable	Unmatched Matched	Dynastic	Non-Dynastic	P-value
Experience	U	4.73	11.17	0.00
	M	6.81	7.21	0.00
Age	U	46.54	49.75	0.00
	M	47.58	47.43	0.77
Male	U	0.91	0.94	0.12
	M	0.93	0.92	0.82
Born in city	U	0.57	0.54	0.33
	M	0.47	0.54	0.02
Education	U	14.90	14.56	0.20
	M	14.84	14.90	0.76
South	U	0.39	0.38	0.86
	M	0.39	0.40	0.67
Civic	U	0.64	0.64	0.96
	M	0.62	0.63	0.70
Unemployment	U	10.99	11.03	0.95
	M	10.62	11.32	0.14
Population	U	4577	6250	0.37
	M	4945	5287	0.74

This table reports the average of the considered variables among dynastic and non-dynastic elected mayors, in unmatched and matched samples, and reports P-values of tests of differences between means across dynastic and non-dynastic individuals.

capital (and therefore total) expenditures during the last year of the term: estimates range between 174 euros per capita and 191 euros per capita for total expenditures, and between 144 and 178 euros per capita for capital expenditures. This increase in expenditures is mostly financed through an increase in capital transfers (between 145 and 208 euros per capita depending on the specification). Overall, the estimates seem to be lower than those found in the Regression Discontinuity Design analysis, which suggests that the latter were likely to be biased upward. However, no clear pattern is found concerning variation in tax revenues and in loans: while we find strong positive effects (of 30 euros and 50 euros per capita respectively) on the 9% bandwidth, the effects decrease with for 6% and 3% until becoming small and non-significant. Furthermore, the political budget cycle seems to be higher for smaller bandwidths. As we show in the next section, this is compatible with the idea that higher political competition triggers strategic behaviors from incumbent mayors seeking reelection. Finally, for mayors who are term-limited, we find no significant effect on the variation of total expenditures, capital expenditures and capital transfers - even though we find an increase in loans per capita.[18]

5 Channels

As we argued in the previous sections, while dynastic mayors in their first term spend relatively more and receive more transfers during the last year in office than their non-dynastic counterparts, they do not do so during their second term. Furthermore, as the results of the matching estimation suggest, it seems that dynastic mayors that were closely elected are more likely to have higher PBC than their largely elected counterparts. Figure 11 confirms this hypothesis: it shows the magnitude of the political budget cycle (in terms of capital expenditures per capita) of dynastic mayors in their first term relatively to the political budget cycle of non-dynastic mayors in their first term, for different equal-sized bins of margin of elections, using the fixed-effects specification. Confidence intervals are at the 10% level. It appears that dynas-

[18] While this procedure of matching on discontinuity reduces the imbalances between treated and control groups, it does not yield more general results than the Regression Discontinuity Design, as it is also based upon the fact that the assignment of the treatment is as good as random around the threshold. However, as we show in the next paragraph, the difference between dynastic and non-dynastic mayors in terms of PBC is likely to be higher in a more competitive environment (i.e. for observations close to the threshold). In a Web Appendix, we show that if we extend the matching to the full sample of observations, we still observe that dynastic mayors have a higher PBC than non-dynastic mayors, though the difference is smaller.

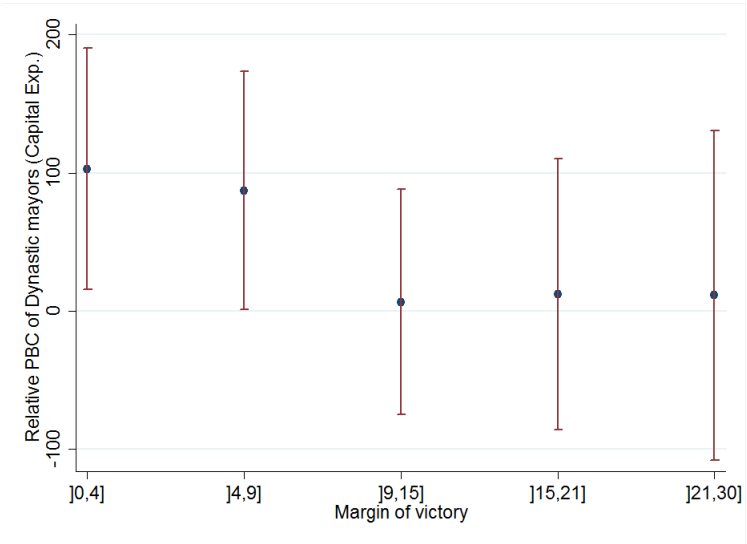
Table 16: Matching estimates: non term-limited mayors

Non-Term-limited						
9% Bandwidth	ΔTot. exp	Δ Curr. exp	Δ Cap. exp	Δ Tax rev	Δ Loans	Δ Cap. transfers
ATE	173.57	6.71	144.09	29.80	50.27	145.21
	(2.60)***	(0.70)	(2.27)**	(2.93)***	(2.38)**	(2.55)**
N	660	660	660	660	660	660
Non-Term-limited						
6% Bandwidth	ΔTot. exp	Δ Curr. exp	Δ Cap. exp	Δ Tax rev	Δ Loans	Δ Cap. transfers
ATE	168.57	-3.85	153.81	16.83	44.55	173.68
	(2.31)**	(0.71)	(2.28)**	(0.12)	(1.81)*	(2.94)***
N	447	447	447	447	447	447
Non-Term-limited						
3% Bandwidth	ΔTot. exp	ΔCurr. exp	Δ Cap. exp	Δ Tax rev	Δ Loans	Δ Cap. transfers
ATE	191.16	-6.81	178.15	16.55	22.17	207.67
	(1.80)*	(-0.46)	(1.83)*	(1.21)	(0.71)	(2.47)**
N	218	218	218	218	218	218
Term-limited						
9% Bandwidth	ΔTot. exp	Δ Curr. exp	Δ Cap. exp	Δ Tax rev	Δ Loans	Δ Cap. transfers
ATE	20.92	-13.44	33.74	-11.48	61.85	20.18
	(0.19)	(-1.16)	(0.30)	(-0.88)	(2.04)**	(0.20)
N	181	181	181	181	181	181

The table represents results from matching estimations, within different bandwidths around the threshold, for term-limited and non-term-limited mayors. Dependent variables are the variation of categories of expenditures and income between the last year and the average of the three first years of the term (winsorized at the 1% level). We use propensity-score matching (where propensity scores are computed using Logit estimation) on the three-nearest-neighbours within a 0.2 standard deviations caliper. Matching is done on the following covariates: political experience, age, sex, place of birth, years of education, list of the candidate (civic list or not), region (north or south), unemployment rate of the city and population. Standard deviations are computed according to the Abadie and Imbens (2006) methodology. T-stats between brackets.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Figure 11: Relative PBC of dynastic mayors and margin of election



tic mayors have relatively higher political budget cycles than non-dynastic mayors when their margin of election is small.

These differences of behavior (as well as the descriptive evidence shown above) suggest that dynastic mayors might want to keep the power both because they (or their family) gain more from it than other types of mayors and/or because they are better at keeping political power (due to the potential experience gathered by their predecessors)[19]. However distinct, these two hypotheses are difficult to disentangle empirically - as the reasons why dynastic politicians are better at keeping power (for example, greater networks of acquaintances) might be the same as the ones granting them more gains from political office. However, additional empirical evidence tend to show that these mechanisms are at stake[20] .

More specifically, our findings tend to show that the dynastic mayors having liberal occupations also have higher PBC: Table 17 shows that when we restrict our sample to mayors who report themselves as being lawyers, doctors or notaries (i.e. professions based on local networks and reputation), the relative political budget cycles of dynastic mayors are much higher than

[19] A further question is whether such higher PBC is useful to actually win the elections. We address this point in Appendix B

[20] Note that the following tests are all based on fixed effect estimations focusing on capital expenditures (the results are similar for total expenditures). Indeed, similar results hold when using the matching RDD estimation shown in Section 4.2.3. Such results are available in the Web Appendix.

Table 17: Liberal occupations and level of education

	Liberal occupations		**Highly Educated**	
Capital Expenditures	Yes	No	Yes	No
Dynasty	-140.750	-1.956	40.584	7.194
	(1.87)*	(0.09)	(1.15)	(0.23)
LY	-22.092	45.818	38.403	39.111
	(0.76)	(3.97)***	(2.78)***	(2.48)**
TL	-53.605	-5.688	-20.045	3.099
	(1.34)	(0.52)	(1.25)	(0.20)
Dynasty*LY	162.896	56.741	57.407	75.325
	(2.54)**	(2.48)**	(1.83)*	(2.53)**
LY*TL	21.312	-11.077	-19.454	0.805
	(0.55)	(0.70)	(0.93)	(0.04)
Dynasty*LY*TL	-133.584	-47.309	-46.550	-64.182
	(1.03)	(1.29)	(0.82)	(1.40)
Constant	891.528	476.753	312.035	692.513
	(1.83)*	(8.87)***	(3.47)***	(8.82)***
R^2	0.02	0.02	0.02	0.02
N	4,510	41,398	18,674	27,234

The table presents estimates from fixed-effects panel regressions, using capital expenditures per capita as a dependent variable (winsorized at the 1% level). Liberal occupations are defined as doctors, lawyers or notaries. Highly educated mayors are those having done at least 18 years of study (i.e. a university degree). The main explanatory variables are a dummy for the mayor being dynastic, a dummy indicating whether the mayor is term-limited, and a dummy indicating pre-electoral years. The sample is made of all cities where two full terms of five years were observed between 1999 and 2012. Years of election are excluded from the estimation. All specifications control for city and year fixed-effects, and control for sex, age, experience, years of educations and place of birth of the mayor, as well as population size. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

for mayors reporting other occupations. Such a finding are in line with the idea of local elites holding power through reputation and local networks which can be transmitted over generations. However, the reputation of dynastic mayors might not have positive consequences only for them: their family could benefit from it too, as there exists a strong persistence of occupations across generations of a political dynasty (making likely that dynastic mayors with high level jobs are not the only ones of their family, see Figure 5). Nevertheless, another channel related to education and occupations might be at stake, namely the "Carnegie effect" (Durante et al. (2011)). Indeed, we have seen that on average, dynastic mayors are slightly less educated than non-dynastic mayors (as 10 additional years of education decrease by about 12% the probability that a candidate is dynastic, as seen in Table 3). Therefore, one could expect that dynastic mayors have more incentives to stay longer in office as, everything else equal, they are likely to earn less on the labour market. This hypothesis seems to be confirmed by columns 3 and 4 of Table 17, which analyze separately low-educated and highly-educated mayors (i.e. with/without a university degree): we do find that the difference of political budget cycle is higher for low-educated dynastic mayors than for high-educated dynastic mayors.

Furthermore, we also observe that the relative political budget cycle of dynastic mayors is substantially higher in smaller municipalities. *A priori*, the effect of population size on our analysis is not clear-cut. Indeed, as shown in the descriptive section, assumed dynastic individuals are more likely to be identified in smaller cities. An explanation could be that surnames diversity is higher in bigger cities: therefore, because municipal council sizes do not increase linearly with the size of the city, we can expect to identify more assumed dynastic individuals in smaller cities. Furthermore, greater family diversity in bigger cities makes more likely that we identify only homonyms rather than individuals of a same family. If we identify better dynasties in smaller municipalities, it is therefore likely that we estimate stronger effects in the latter. However, the size of the effects are unlikely to depend only on measurement issues, as incentives might be different between small and big municipalities. Indeed, one could assume that in smaller municipalities, family ties are stronger and reputation effect are more salient, therefore strengthening competition between families: in such a case, one would also expect to identify more opportunistic behaviors in smaller cities. To the contrary, one could also hypothesize that

in very small villages, being a mayor does not bring as much rents and benefits as in bigger municipalities, so that there are lower incentives to remain in office: in this case, one would expect lower opportunistic behaviors.

Table 18 helps addressing these questions. It emphasizes that the effect is clearly stronger for small cities, but that an effect is still detected for medium-size cities. Nevertheless, no effect is noticeable when the sample is restricted to big cities. Even though such restrictions impose a dramatic drop of the sample size, it is unlikely that this absence of result is only due to a loss of power. Indeed, political budget cycles do not disappear when we restrict our analysis to bigger cities: in fact, its magnitude is similar. The main difference is that assumed dynastic politicians do not have higher political budget cycles than other politicians in big cities. This could be due either to the fact that the dynasty variable captures more homonyms in bigger municipalities, or to the fact that the advantages and incentives inherent to dynasties are smaller in bigger municipalities.

Another way of testing whether mayors from more powerful families have more incentives or power to remain in office is to consider the place of birth of the mayor, and the number of members of his family who were presumably in office before him. We have shown above that dynastic mayors are more likely to be born in the city in which they hold office. However, here again, the effects of the city of birth are not clear-cut. If the fact of being born in the city reflects both a greater power from the family and an increased probability of genuinely identifying dynastic mayors, we would expect political budget cycles of dynastic mayors to be higher when the mayor is born in the city. However, the place of birth of a mayor might not necessarily be a good proxy for the power of his family within the city: in particular, the probability that a mayor whose family has been durably living in the city is born in this same city is decreasing with the population size as smaller municipalities are likely to have less doctors or hospitals for mothers to give birth. This might explain why we observe in Table 19 that dynastic mayors who were born in the city have a lower relative political business cycle that mayors who were not. However, such a result does not invalidate the hypothesis that dynastic mayors with a stronger local implantation have more power and more incentives to remain in office. Indeed, as shown in the columns 3 and 4 of the table, we do not find any

Table 18: PBC of capital expenditures for different city sizes

Capital Expenditures	Pop<=1500	1500<Pop<=15000	Pop>15000
Dynasty	41.511	-8.539	-89.038
	(1.04)	(0.48)	(2.85)***
LY	51.654	32.188	41.166
	(2.18)**	(3.21)***	(2.90)***
TL	-4.520	-16.162	-7.636
	(0.19)	(1.67)*	(0.64)
Dynasty*LY	84.168	38.830	16.835
	(2.15)**	(1.87)*	(0.54)
LY*TL	-33.719	11.174	-17.534
	(1.06)	(0.82)	(0.95)
Dynasty*LY*TL	-29.321	-77.795	-65.057
	(0.46)	(2.24)**	(1.53)
Constant	776.705	582.160	341.799
	(6.21)***	(6.77)***	(3.29)***
R^2	0.02	0.03	0.12
N	19,406	23,799	2,703

The table presents estimates from fixed-effects panel regressions, using capital expenditures per capita as a dependent variable (winsorized at the 1% level). Regressions are ran on different subsamples of cities, defined according to their population in 2001. The main explanatory variables are a dummy indicating whether the mayor is dynastic, a dummy indicating whether the mayor is term-limited, and a dummy indicating pre-electoral years. Each subsample is made of cities where two full terms of five years were observed between 1999 and 2012. Years of election are excluded from the estimation. All specifications control for city and year fixed-effects, and control for sex, age, experience, years of educations and place of birth of the mayor, as well as population size. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

additional political budget cycle for dynastic mayors who were not born in the province of the city they run, while for mayors born in the province, the estimated additional political budget cycle is even higher than in the baseline specification. Therefore, our baseline results are not driven by atypical cases of mayors having a weak implantation at the local level. Our interpretation is further substantiated by the fact that mayors who had two predecessors or more in office before them have higher political business cycles than mayors who are only the second individual of the dynasty in office[21]. Furthermore, if we impose mayors to be dynastic only if their assumed predecessor and themselves were born in the city or in the province (84% of all dynastic mayors), the effect of dynasty if even stronger than in the baseline specification[22]. Again, this appears in line with self-perpertuation theories whereby PBC is easier to enforce by more powerful dynasties - as measured by the number of dynastic politicians.

Another explanation for the different policies implemented by dynastic mayors might be the economic and social environment in which they live. Therefore, one could expect that dynastic mayors have more opportunistic policies in the South than in the North (which, according to the seminal case study of Banfield (1967), is characterized by a form of "amoral familism") and in regions with high mafia prevalence (Campania, Calabria and Sicilia, see Geys and Daniele (2015) and literature there in). However, as emphasized in Table 20, this is not what we find: if anything, dynastic mayors seem to be more opportunistic in center and northern regions, and while their political budget cycle is relatively higher in mafia regions, the estimated effect is not significant. [23]

Finally, one could also expect that if dynastic politicians inherit from the political experience of their predecessors, their city should show better performances during their term. We have seen that on average, dynastic politicians do not seem to spend more than other mayors. However, this result hardly predicts differences in city performance. Table 21 tests directly whether dynastic mayors are indeed better while in office by analyzing their impact on several out-

[21] This estimation strategy is however imperfect in this setup as we might give more weight to individuals with frequent names, or to homonyms with no family ties.

[22] Results of these estimations are presented in a Web Appendix.

[23] Another explanation could come from social capital. Indeed, as emphasized by Nannicini et al. (2013), clientelistic behaviors are less likely to be punished in low social capital areas. We show in a Web Appendix that, in line with the heterogeneity analysis between northern and southern regions, we do not find that dynastic mayors are more opportunistic in low social capital areas (defined by cities with high unemployment and a low level of blood donations).

Table 19: Place of birth

	Born in city		**Born in province**	
Capital Expenditures	Yes	No	Yes	No
Dynasty	-14.315	-12.749	-4.428	-64.047
	(0.35)	(0.39)	(0.20)	(0.76)
LY	38.059	35.933	30.696	98.133
	(2.46)**	(2.41)**	(2.68)***	(3.40)***
TL	-11.763	-21.700	-5.744	-47.467
	(0.68)	(1.51)	(0.51)	(1.73)*
Dynasty*LY	46.384	92.401	76.063	8.859
	(1.59)	(2.87)***	(3.32)***	(0.15)
LY*TL	-1.448	-12.382	1.364	-54.177
	(0.07)	(0.61)	(0.08)	(1.60)
Dynasty*LY*TL	-29.041	-92.171	-65.386	-42.123
	(0.58)	(1.85)*	(1.74)*	(0.38)
Constant	572.793	507.441	592.346	597.772
	(5.38)***	(6.55)***	(8.79)***	(3.41)***
R^2	0.02	0.02	0.02	0.02
N	22,918	22,990	39,624	6,284

The table presents estimates from fixed-effects panel regressions, using capital expenditures per capita as a dependent variable (winsorized at the 1% level). Columns 1 and 2 present results of the baseline specification separately on the subsamples of mayors born and not-born in the city. Columns 3 and 4 present results of the baseline specification separately on the subsamples of mayors born and not-born in the province. Other covariates of interest include a dummy indicating whether the mayor is term-limited, and dummy indicating whether pre-electoral years. The sample is made of all cities where two full terms of five years were observed between 1999 and 2012. Years of election are excluded from the estimation. All specifications control for city and year fixed-effects, and control for sex, age, experience, years of educations and place of birth of the mayor, as well as population size. Robust standard errors. T-Statistic between brackets.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 20: Mafia infiltration and Southern regions

Capital Expenditures	Non-South	South	Non-Mafia Region	Mafia Region
Dynasty	-25.757	39.015	-10.775	14.331
	(1.19)	(0.91)	(0.51)	(0.28)
LY	31.266	33.088	35.411	8.079
	(2.55)**	(1.41)	(3.09)***	(0.28)
TL	0.395	-53.769	-1.851	-43.332
	(0.04)	(1.97)**	(0.18)	(1.24)
Dynasty*LY	72.529	54.071	67.159	78.829
	(2.95)***	(1.26)	(2.86)***	(1.46)
LY*TL	-13.274	32.958	-14.140	27.011
	(0.87)	(0.85)	(0.93)	(0.56)
Dynasty*LY*TL	-41.396	-112.426	-41.730	-127.194
	(1.08)	(1.44)	(1.12)	(1.27)
Constant	471.258	565.827	500.623	346.146
	(9.15)***	(3.37)***	(9.81)***	(1.47)
R^2	0.02	0.03	0.02	0.02
N	34,865	11,043	39,071	6,837

The table presents estimates from fixed-effects panel regressions, using capital expenditures per capita as a dependent variable (winsorized at the 1% level). Southern regions are Abruzzo, Basilicata, Calabria, Campania, Molise, Puglia, Sardegna and Sicilia. Mafia regions include Calabria, Campania and Sicilia. The main explanatory variables are a dummy for the mayor being dynastic, a dummy indicating whether the mayor is term-limited, and a dummy indicating pre-electoral years. The sample is made of all cities where two full terms of five years were observed between 1999 and 2012. Years of election are excluded from the estimation. All specifications control for city and year fixed-effects, and control for sex, age, experience, years of educations and place of birth of the mayor, as well as population size. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 21: Competence of dynastic mayors

	Average length of term	Ability rev. collection	Speed payment	Growth tax base
Dynasty	-0.041	-0.176	0.178	0.002
	(1.85)*	(0.50)	(0.86)	(0.83)
TL	0.026	0.071	0.214	-0.000
	(1.90)*	(0.39)	(1.99)**	(0.24)
Dynasty*TL	0.084	0.043	-0.444	-0.002
	(2.96)***	(0.10)	(1.86)*	(0.82)
Constant	2.156	54.065	77.603	0.016
	(20.96)***	(45.73)***	(109.00)***	(1.35)
R^2	0.77	0.13	0.03	0.46
N	26,447	44,056	44,052	16,437

The table presents estimates from fixed-effects panel regressions. Dependent variables are the average term length of mayors (measured in years), the ability to collect revenue (measured as a ratio of collected revenue over expected revenue), the speed of payment (measured as the share of due expenditures paid during the term) and the yearly growth rate of private tax base (measured in percent). The main explanatory variables are a dummy for the mayor being dynastic, and a dummy indicating whether the mayor is term-limited. In column 1, observations are aggregated at the term level, and include all observed terms between 1998 and 2012. Estimations of column 2,3 and 4 are at the yearly level. Columns 2 and 3 include all cities for which two full terms were observed between 1999 and 2012, while column 4 include all cities for which at least one full term was observed between 2001 and 2011. Years of election are excluded from the estimation. All specifications control for city and year fixed-effects, and control for sex, age, experience, years of educations and place of birth of the mayor, as well as population size. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

come variables indicating the quality of their governance, namely the length of their term (with shorter terms indicating a higher probability of early termination, i.e. political instability), their ability to collect revenue and to reimburse their debt in time, and the growth of private tax base over the term (a proxy for the city GDP)[24]. Overall, we find no significant effect of political dynasty (whether in first or second term) on these variables, showing that dynastic mayors are unlikely to be more competent.

6 Robustness tests

In this section, we present different robustness tests based on the alternative definitions of dynastic mayors presented above. We present the results of the fixed effects specifications, while the ones on the matching RDD are reported in the Web Appendix.

First, as shown in Tables 22 and 23 the results are robust to excluding mayors with common names. As argued above, dropping individuals with the 100 most common surnames at the

[24] In Column 1 our period of observation is an entire electoral term instead of yearly observations.

Table 22: Fixed effects: PBC for mayors whose name is not among the 100 most common names in the province

	Total exp	Current exp	Capital exp	Tax rev	Loans	Cap. transfers
Dynasty	12.656	4.648	5.310	3.966	-1.683	5.411
	(0.41)	(0.74)	(0.21)	(1.32)	(0.21)	(0.22)
LY	43.763	-0.991	33.169	-8.270	9.285	18.123
	(3.10)***	(0.52)	(2.61)***	(6.63)***	(2.25)**	(1.57)
TL	-3.683	2.871	-11.203	-2.637	-4.357	0.662
	(0.25)	(0.89)	(0.90)	(1.66)*	(1.14)	(0.06)
Dynasty*LY	106.305	7.739	105.337	7.809	28.945	81.511
	(3.47)***	(2.11)**	(3.72)***	(3.15)***	(3.19)***	(3.13)***
LY*TL	-10.068	2.256	-7.443	7.048	5.232	-15.032
	(0.53)	(0.90)	(0.43)	(4.01)***	(0.96)	(0.98)
Dynasty*LY*TL	-46.642	0.138	-62.954	-1.809	-30.946	-50.339
	(0.93)	(0.02)	(1.38)	(0.43)	(2.20)**	(1.22)
Constant	1,496.669	732.086	525.909	274.189	109.979	379.001
	(17.82)***	(29.63)***	(7.54)***	(23.22)***	(4.74)***	(5.90)***
R^2	0.02	0.25	0.01	0.41	0.01	0.01
N	33,085	33,085	33,085	33,094	33,092	33,092

The table presents estimates from fixed-effects panel regressions, using categories of public expenditures and revenues as dependent variables (all are expressed in euros per capita, and winsorized at the 1% level). The main explanatory variables are a dummy for the mayor being dynastic, a dummy indicating whether the mayor is term-limited, and a dummy indicating pre-electoral years. The sample is made of all cities whose mayor did not have a name among the 100 most common at the province level, and where two full terms of five years were observed between 1999 and 2012. Years of election are excluded from the estimation. All specifications control for city and year fixed-effects, and control for sex, age, experience, years of educations and place of birth of the mayor, as well as population size. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

province level amounts to dropping about 20% of the initial sample, while dropping those with the 500 most common surnames amounts to dropping about 50% of the sample. Yet, we find robust significant effects of dynasty on political budget cycles in the first term, with much higher estimated coefficients than in the baseline specification (for example, while the estimated impact of dynasty on the increase of capital expenditures is of about 68 euros per capita in the baseline specifications, it is equal to 105 when we exclude the 100 most common surnames, and to 125 when we exclude the 500 most common surnames). These results seem to confirm that our initial results were biased downwards because of homonymy.

A second robustness check consists in testing whether the results hold when we define as dynastic only mayors who had a relative in office during the 10 previous years. This definition imposes a common constraint to all identified dynastic mayors, and overcomes the potential

Table 23: Fixed effects: PBC for mayors whose name is not among the 500 most common

	Total exp	Current exp	Capital exp	Tax rev	Loans	Cap. transfers
Dynasty	-5.339	0.365	-24.107	1.367	7.190	-16.557
	(0.12)	(0.04)	(0.68)	(0.30)	(0.61)	(0.51)
LY	34.727	-0.291	20.318	-7.331	17.758	-1.601
	(1.89)*	(0.12)	(1.23)	(4.62)***	(3.19)***	(0.11)
TL	10.124	1.068	-6.398	-0.440	0.952	6.318
	(0.52)	(0.26)	(0.39)	(0.21)	(0.19)	(0.43)
Dynasty*LY	112.254	6.173	125.032	6.146	-0.991	117.436
	(2.45)**	(1.15)	(2.92)***	(1.80)*	(0.08)	(3.01)***
LY*TL	-13.535	1.303	-5.912	5.462	1.957	-12.297
	(0.56)	(0.39)	(0.27)	(2.37)**	(0.27)	(0.63)
Dynasty*LY*TL	-62.479	-3.958	-82.370	5.698	0.962	-86.603
	(0.87)	(0.48)	(1.23)	(0.94)	(0.05)	(1.48)
Constant	1,406.507	719.075	450.576	289.001	74.159	345.878
	(12.32)***	(24.84)***	(4.59)***	(21.18)***	(2.32)**	(3.86)***
R^2	0.02	0.27	0.02	0.41	0.01	0.01
N	18,846	18,846	18,846	18,852	18,851	18,851

The table presents estimates from fixed-effects panel regressions, using categories of public expenditures and revenues as dependent variables (all are expressed in euros per capita, and winsorized at the 1% level). The main explanatory variables are a dummy for the mayor being dynastic, a dummy indicating whether the mayor is term-limited, and a dummy indicating pre-electoral years. The sample is made of all cities whose mayor did not have a name among the 500 most common at the province level, and where two full terms of five years were observed between 1999 and 2012. Years of election are excluded from the estimation. All specifications control for city and year fixed-effects, and control for sex, age, experience, years of educations and place of birth of the mayor, as well as population size. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 24: Fixed effects: PBC for dynastic mayors with 10 year window

	Total exp	Current exp	Capital exp	Tax rev	Loans	Cap. transfers
Dynasty	5.243	-1.009	0.857	1.679	8.552	-7.311
	(0.21)	(0.19)	(0.04)	(0.71)	(1.41)	(0.38)
LY	47.558	-1.938	39.458	-8.624	12.846	23.508
	(4.07)***	(1.14)	(3.71)***	(8.03)***	(3.70)***	(2.47)**
TL	-3.664	3.699	-11.017	-3.080	-0.790	-4.644
	(0.30)	(1.33)	(1.04)	(2.26)**	(0.24)	(0.48)
Dynasty*LY	69.137	9.356	64.782	7.827	19.728	52.286
	(3.01)***	(3.13)***	(3.04)***	(3.68)***	(2.88)***	(2.69)***
LY*TL	-7.128	2.912	-6.822	8.367	-2.365	-8.240
	(0.44)	(1.26)	(0.46)	(5.52)***	(0.50)	(0.64)
Dynasty*LY*TL	-40.614	-2.359	-53.467	-4.908	-19.205	-55.347
	(1.05)	(0.49)	(1.52)	(1.41)	(1.77)*	(1.79)*
Constant	1,436.904	714.858	517.768	274.242	105.909	375.285
	(19.55)***	(29.71)***	(8.89)***	(24.05)***	(5.50)***	(6.85)***
R^2	0.02	0.24	0.02	0.42	0.01	0.01
N	46,124	46,124	46,124	46,125	46,123	46,123

The table presents estimates from fixed-effects panel regressions, using categories of public expenditures and revenues as dependent variables (all are expressed in euros per capita, and winsorized at the 1% level). The dummy variable indicating whether a mayor is dynastic is equal to 1 if an individual had a relative in the municipal council during the 10 years previous to his entrance in the municipal council. Other explanatory variables are a dummy indicating whether the mayor is term-limited, and a dummy indicating pre-electoral years. The sample is made of all cities where two full terms of five years were observed between 1999 and 2012. Years of election are excluded from the estimation. All specifications control for city and year fixed-effects, and control for sex, age, experience, years of educations and place of birth of the mayor, as well as population size. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

bias induced by the fact that dynastic mayors at the beginning of the period are structurally likely to be different from dynastic mayors identified at the end of the period. Tables 24 shows that the results are similar in terms of magnitude as the baseline specifications.

Finally, another way of testing the robustness of our result (and the likelihood of our potential mechanisms) is to estimate whether political budget cycles are detected for the first members of a dynasty in office (who did not have any relative in office before). Indeed, if the gains from being in office and the political skills of dynastic individuals - and therefore their incentives to remain in office - are higher precisely because of the legacy of his predecessors, we expect to find no significant difference between the "founders" of political dynasties and the other non-dynastic mayors. Table 25 confirms this prediction. Column 1 uses the full sample of observations, and tests whether first generation of dynasties have higher capital expenditures

than the other mayors during the pre-electoral year: the results suggest non-term-limited mayors do not. However, we find a weakly significant increase for term-limited ones. While this is not compatible with individual electoral incentives, it is in line with family electoral incentives, whereby founders of dynasty having a higher PBC in their second term as in the next one, their relatives might enter in the political arena and take benefit of it. Column 2 tests whether there is a difference of political budget cycle between dynastic mayors and founders of dynasties: formally, we estimate the same equation as in column 1, but keeping only these two types of mayors. The results are in line with our prediction, as first generations of dynasties are found to spend much less than dynastic mayors during the pre-electoral year of their first term (about 78 euros per capita). Overall, these results confirm the robustness of our estimation.

7 Conclusion

In this paper, we provide several insights about the relevance of dynasties in the political arena. Our test is based on data from Italian municipalities in the period 1985-2012 (and on mayoral elections in the period 1998-2012). Our main contribution is a test of whether dynastic mayors perform differently. This might be due to higher ability thanks to inherited political skills and/or higher gains from being in office. In line with such hypothesis, we find that they spend more (on capital expenditure) - and receive more transfers - in pre-electoral years, only when they can run for re-election. We provide several tests in line with such interpretation, among the others: i) dynastic mayors have longer careers, and they are more likely to win elections and be elected in higher levels of government; ii) we provide causal evidence of dynastic self-perpetuation among mayors; iii) we show that higher spending is more likely when dynastic leaders face contested elections. Our results are robust to several robustness tests and different estimation strategies (fixed effects, regression discountinuity and matching regression discountinuity).

The results of this paper contribute to a better understanding of the role played by families in contemporary democracies. In fact, they continue to have a significant role in politics across very different countries. In this light, this study relates to the debate about inequality and transmission of wealth and power across generations (Piketty (2013)). We highlight that dynamics

Table 25: Political budget cycles of first generations

Capital expenditures	(1)	(2)
First Generation	14.074	22.972
	(0.74)	(0.70)
LY	63.679	102.487
	(5.94)***	(4.77)***
TL	4.356	-2.667
	(0.43)	(0.11)
First Generation*LY	-30.620	-78.742
	(1.43)	(2.87)***
LY*TL	-35.715	-69.005
	(2.38)**	(2.11)**
First Generation*LY*TL	60.383	86.478
	(1.86)*	(1.98)**
Constant	488.366	838.671
	(9.71)***	(6.88)***
R^2	0.02	0.02
N	45,908	20,334

The table presents estimates from fixed-effects panel regressions, using capital expenditures per capita as a dependent variable (winsorized at the 1% level). The main explanatory variables include a dummy indicating whether the mayor is the first of his family to be in the municipal council, a dummy indicating whether the mayor is term-limited, and a dummy indicating pre-electoral years. In column 1, the estimation is ran on all types of mayors, while in column 2, the estimation is ran on mayors who are part of a political family (either as first generation or as dynasty). Each subsample is made of cities where two full terms of five years were observed between 1999 and 2012. Years of election are excluded from the estimation. All specifications control for city and year fixed-effects, and control for sex, age, experience, years of educations and place of birth of the mayor, as well as population size. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

of power transmission across generations have important consequences in the political arena, as dynastic politicians behave very differently both in terms of their electoral performance and their policy making. Our results point towards a key role of networks to explain the success of political dynasties. In fact, dynastic leaders behave more strategically when they work in liberal professions, typically based on local networks and reputation mechanisms; as well as in small towns (where again, politics is based on personal ties), and when their family has roots in the local community.

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8 Appendix

Appendix A: Some more descriptive evidence about dynastic mayors

In this section, we present additional descriptive statistics on the evolution of assumed dynasties in Italy. Figure 12 shows how the characteristics of dynastic mayors evolved over time. Overall, the geographical location of dynastic mayors does not seem to evolve a lot accross time, and the cities they run always remained smaller than the cities ran by other mayors. Overall, the level of education and the mean age of mayors increased over time, while the share of male mayors and of mayors born in the city decreased over time. Except for a convergence between the share of mayors born in the city they run, we don't observe salient differences of evolution of these characteristics between dynastic and non-dynastic mayors.

Figure 12: Evolution of characteristics of dynastic mayors

However, as explained before and as emphasized in Figures 13 and 14, we observe that the age difference between presumed dynastic mayors and their first observed predecessor has a bimodal distribution (with a mode at 0 and a mode at around 30), and that the average age difference between them is increasing over time in our sample. This suggests that we are more likely to capture brotherhood linkages at the beginning of the sample, and father-and-son linkages at the end of the sample.

Figure 13: Histogram of age differences between dynastic mayors and their oldest predecessor

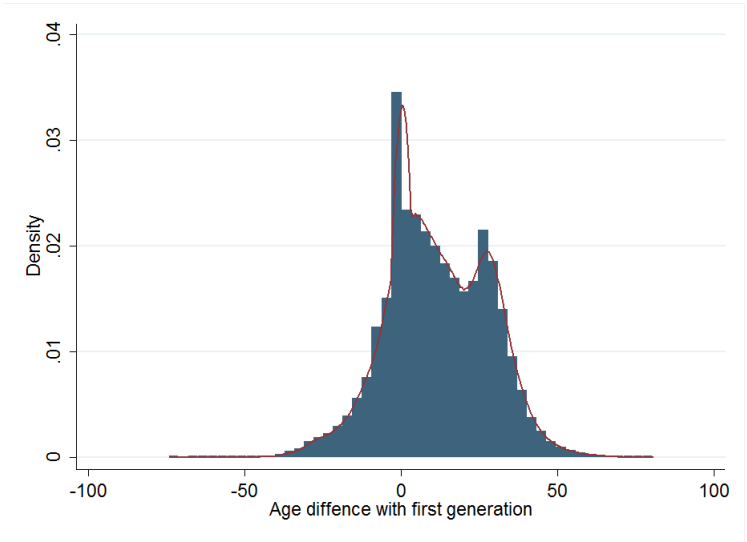
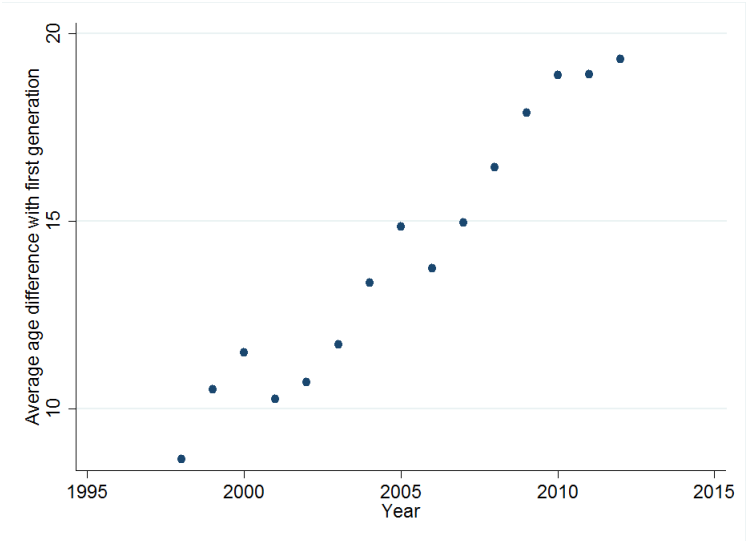


Figure 14: Evolution of age difference between dynastic mayors and their oldest predecessor



Appendix B: Are political business cycles useful for reelection ?

Testing whether rising expenditures during the pre-electoral is useful for reelection has a causal impact on reelection is difficult, as the reasons leading mayors to increase expenditures might be correlated to unobserved factors correlated with their probability of reelection. Nevertheless, we can explore whether, at first glance, the probability of reelection depends on political budget cycles. Table 26 shows how the probability of reelection of mayors in office depend on several observable characteristics, their dynastic nature and the increase in capital expenditures they made in pre-electoral year. We also examine whether the probability of incumbency varies along these dimensions. Columns 1 and 3 include the full sample of mayors who are reeligible, while columns 2 and 4 consider only reeligible dynastic mayors. It appears that dynastic mayors are more likely to be both incumbent and reelected. However, whether we consider all mayors or only the dynastic ones, we do not find any significant effect of the variation of capital expenditures on the probability of reelection and the probability of being incumbent.

Table 26: Political budget cycle and reelection

	Reelection		**Incumbency**	
	All mayors	Dynastic only	All mayors	Dynastic only
Dynasty	0.034		0.034	
	(1.98)**		(2.01)**	
PBC Capital Exp.	0.006	-0.003	0.002	-0.005
	(0.46)	(0.14)	(0.12)	(0.27)
Dynasty*PBC Total Exp.	-0.008		-0.005	
	(0.34)		(0.24)	
Margin	0.002	0.003	0.000	0.001
	(5.46)***	(3.46)***	(0.56)	(1.57)
Years of experience in council	0.004	0.003	0.004	0.001
	(3.49)***	(0.92)	(3.38)***	(0.46)
Male	0.183	0.102	0.160	0.098
	(7.81)***	(2.11)**	(6.74)***	(2.10)**
Age	-0.012	-0.011	-0.010	-0.009
	(16.77)***	(7.39)***	(13.07)***	(5.78)***
Born in city	-0.008	-0.041	-0.020	-0.066
	(0.56)	(1.30)	(1.34)	(2.08)**
Population	0.000	-0.000	0.000	-0.000
	(1.01)	(0.37)	(0.34)	(0.80)
Years of education	-0.004	-0.006	-0.004	-0.004
	(1.86)*	(1.35)	(1.77)*	(0.76)
Civic	-0.024	-0.051	-0.024	-0.073
	(1.64)	(1.63)	(1.62)	(2.40)**
South	-0.063	0.048	-0.037	0.024
	(2.52)**	(1.08)	(1.50)	(0.56)
Unemployment	-0.002	-0.004	-0.001	-0.002
	(1.06)	(1.57)	(0.55)	(0.70)
Constant	0.931	1.121	0.936	1.085
	(13.26)***	(7.60)***	(13.13)***	(7.43)***
R^2	0.09	0.10	0.06	0.08
N	4,807	1,161	4,807	1,161

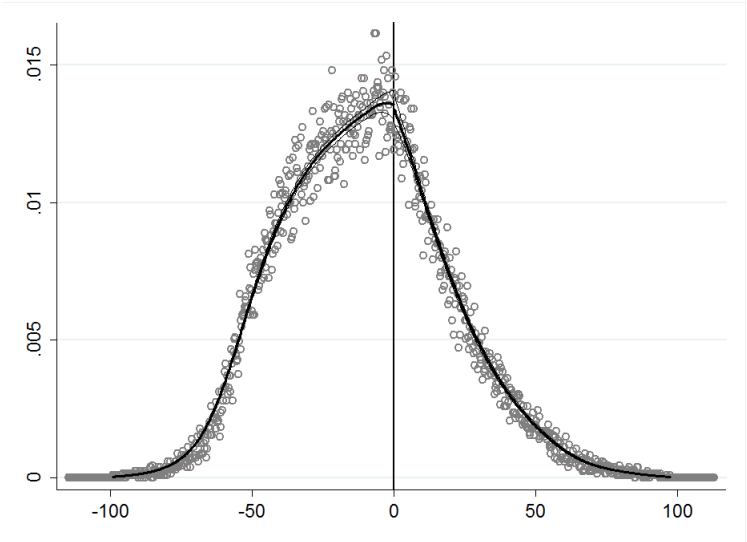
The table presents estimates from linear regressions. In columns 1 and 2, the dependent variable is dummy indicating whether the mayor in office was reelected. In columns 3 and 4, the dependent variable is a dummy indicating whether the mayor ran again for office. In all specifications, the samples are restricted to non-term-limited mayors, among cities where we observed two full terms of five years between 1999 and 2012. The variation of capital expenditures is expressed in thousands of euros per capita. The margin variable indicates the difference in share of votes between the mayor and his best challenger at the previous election. All specifications control year of all election fixed-effects. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Appendix C: Estimating the persistence of power

The regression discontinuity methodology presented in this article design is a suitable setting for estimating the persistence of power. As in Dal Bó et al. (2009), Querubin (2010) and Bragança et al. (2015), we exploit exogenous variation in electoral margins to identify whether closely elected candidates are more likely to have a relative in office (whether mayor or simply in municipal council) during the following years.

First of all, the margin of candidates does not seem to be discontinuous around the zero threshold, as is indicated in Figure 15

Figure 15: McCrary test for the margin of victory of candidates



As for the estimation of political budget cycles, we estimate the discontinuity around the threshold using an optimal uniform bandwidth computed using the Calonico et al. (2014) method, controlling for linear and quadratic polynoms of the forcing variable. Estimations are clustered at the city-candidate level. The results are presented in Table 27, and also include estimation on the full sample.

The results are coherent with the graphical evidence presented above: we find a strong positive discontinuity for the probability that a relative becomes mayor (with an estimated jump between 1.5 and 2 percentage points). However, consistently with the graphical evidence, we

Table 27: Probability that a relative enters in office

Relative mayor	(1)	(2)	(3)	(4)
Elected	0.019	0.014	0.021	0.014
	(3.50)***	(1.94)*	(6.74)***	(3.30)***
Bandwidth	0.17	0.21	1	1
Polynom	1	2	1	2
N	23,076	26,838	55,511	55,511
Relative in council	(1)	(2)	(3)	(4)
Elected	-0.007	-0.009	-0.009	-0.015
	(0.69)	(0.67)	(1.42)	(1.88)*
Bandwidth	0.20	0.24	1	1
Polynom	1	2	1	2
N	23,285	26,438	45,605	45,605

The table presents results from regression discontinuity estimations. In columns 1 and 2, estimations are ran using an optimal bandwidth calculated thanks to the Calonico et al. (2014) method, using uniform bandwidth and controlling for polynoms of order 1 and 2 of the margin of victory of the best dynastic candidate. In columns 3 and 4, all observations are included. In the first panel, the dependent variable is a dummy indicating whether an individual had a relative mayor within 10 years after his candidacy. In the second panel, the dependent variable is a dummy indicating whether an individual had a relative in municipal councils within 10 years after his candidacy. The sample consists in all candidates to elections between 1993 and 2004. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

find no significant discontinuity around the forcing threshold for the probability that a relative enters the municipal council.

Appendix D: Effect of dynasties on average expenditures and revenues (RDD and Matching estimations)

In this section, we present the results from Regression Discontinuity and Matching approach on average expenditures and revenues, for mayors in their first and second term, following the methodologies described in Section 4. Figures 16 and 17 show that no clear discontinuities of average revenues or expenditures are noticeable, either for mayors in their first term or mayors in their second term, suggesting that dynastic mayors do not spend or receive more money than other mayors. The results from the Regression Discontinuity Design and from Matching, respectively presented in Tables 28 and 29, confirm this intuition: except for a significant increase in tax revenues during the first term (only present in the Matching estimation and not in the Regression Discontinuity Design), we find no clear difference between dynastic and non-dynastic mayors concerning average budgets, either in first or second term.

Figure 16: Average expenditures and revenues (first term)

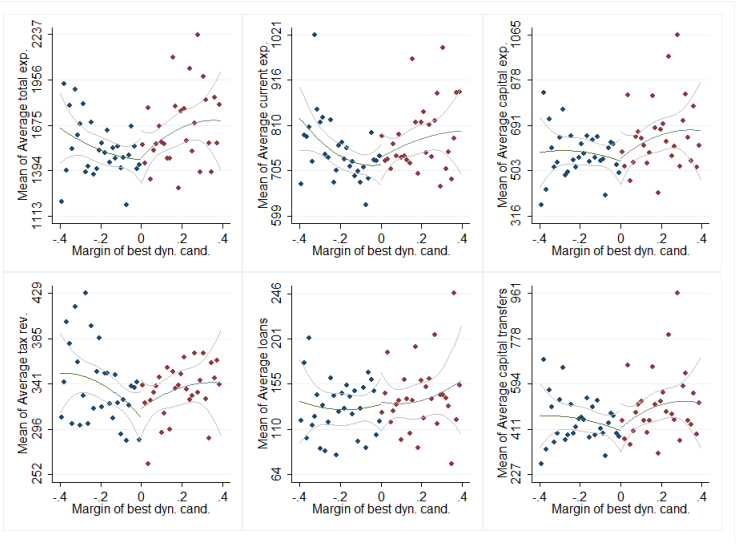


Figure 17: Average expenditures and revenues (second term)

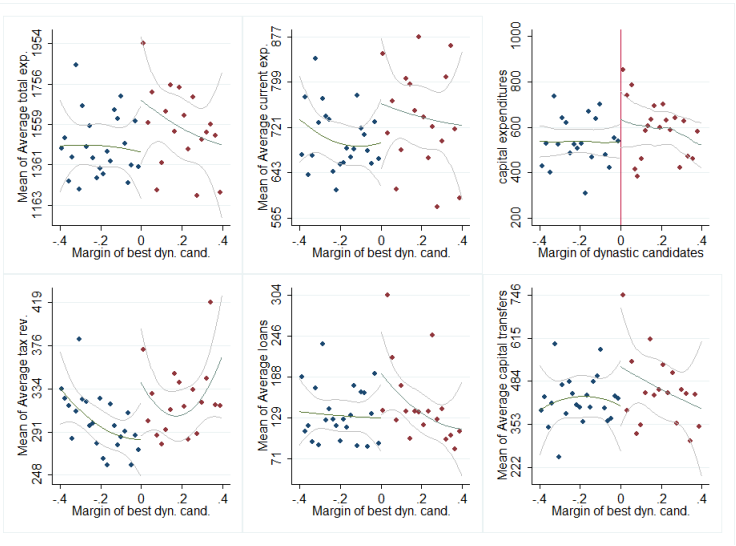


Table 28: Discontinuity of average expenditures and revenues

No Term Limit	Total exp	Current exp	Capital exp	Tax rev	Loans	Transfers
Dynasty	-9.829	-7.904	-13.093	-0.408	5.948	15.585
	(0.09)	(0.19)	(0.18)	(0.02)	(0.28)	(0.25)
Bandwidth	0.12	0.12	0.13	0.13	0.14	0.13
N	914	909	926	957	1,008	963
Term Limit	Total exp	Current exp	Capital exp	Tax rev	Loans	Transfers
Dynasty	173.820	55.547	98.978	38.386	62.815	93.593
	(0.92)	(0.99)	(0.76)	(1.19)	(1.55)	(0.80)
Bandwidth	0.23	0.26	0.23	0.22	0.21	0.23
N	527	606	528	501	499	544

The table presents results from a regression discontinuity estimation using an optimal bandwidth calculated thanks to the Calonico et al. (2014) method, using uniform bandwidth and controlling for an order-one polynom of the margin of victory of the best dynastic candidate. Dependent variables are the average of categories of expenditures and revenues over the term, winsorized at the 1% level. The sample consists in all full mayoral terms of five years, for years of election between 1999 and 2012. Regressions are ran separately on the sample of term-limited and non-term-limited elected mayors. Robust standard errors. T-Statistic between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 29: Matching estimates: average expenditures and revenues

No Term Limit						
9% Bandwidth	Tot. exp	Curr. exp	Cap. exp	Tax rev	Loans	Cap. transfers
ATE	60.72	68.26	-3.89	51.75	-13.56	-2.73
	(0.72)	(2.12)**	(0.07)	(2.75)***	(-0.79)	(0.95)
N	660	660	660	660	660	660
No Term Limit						
6% Bandwidth	Tot. exp	Curr. exp	Cap. exp	Tax rev	Loans	Cap. transfers
ATE	-12.84	47.25	-74.49	45.85	-9.61	-57.31
	(-0.15)	(1.45)	(-1.32)	(2.31)**	(-0.52)	(-1.13)
N	447	447	447	447	447	447
No Term Limit						
3% Bandwidth	Tot. exp	Curr. exp	Cap. exp	Tax rev	Loans	Cap. transfers
ATE	-113.83	-9.24	-114.65	55.93	18.33	-113.06
	(-1.08)	(-0.19)	(1.75)*	(2.42)**	(0.94)	(-1.81)*
N	218	218	218	218	218	218
Term Limit						
9% Bandwidth	Tot. exp	Curr. exp	Cap. exp	Tax rev	Loans	Cap. transfers
ATE	196.82	36.87	91.93	36.51	58.45	85.99
	(1.35)	(0.85)	(0.87)	(1.54)	(1.58)	(0.92)
N	181	181	181	181	181	181

The table represents results from matching estimations, within different bandwidths around the threshold, for term-limited and non-term-limited mayors. Dependent variables are the average of categories of expenditures and income over the term (winsorized at the 1% level). We use propensity-score matching (where propensity scores are computed using Logit estimation) on the three-nearest-neighbours within a 0.2 standard deviations caliper. Matching is done on the following covariates: political experience, age, sex, place of birth, years of education, list of the candidate (civic list or not), region (north or south), unemployment rate of the city and population. Standard deviations are computed according to the Abadie and Imbens (2006) methodology. T-stats between brackets.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Appendix E: Variables

Table 30: Variables used in the analysis

Variable name	Definition	Time span	Source
Dynasty	Whether the politician had a relative in office before his first entrance in council	1985-2014	Ministry of Interior
First Generation	Whether the politician had a relative in office after his first entrance in coucil	1985-2014	Ministry of Interior
Sex	Sex of the politician	1985-2014	Ministry of Interior
Age	Age of the politician in years	1985-2014	Ministry of Interior
Years of education	Minimum number of years to complete the highest degree obtained	1985-2014	Ministry of Interior
Occupation	Classification of mayors' occupations*	1985-2014	Ministry of Interior
Experience	Number of years since first entrance in the council	1985-2014	Ministry of Interior
Place of birth	Place of birth in the format *Name of the city (Province abbreviation)*	1985-2014	Ministry of Interior
Civic	Whether the politician is from a civic list	1985-2014	Ministry of Interior
South	Dummy for southern regions	1985-2014	Ministry of Interior
Population	Population size	1998-2012	Ministry of Interior
Unemployment	Unemployment rate, in percent	2001	Ministry of Interior
Name frequency	Name frequency at the province level (in thousands of individuals per name)	2001	Ministry of Interior
Trust	Level of trust as measured by the "Trust" question in the World Value Survey	1990s	Nannicini et al. (2013)
Total expenditures	Total expenditures per capita	1998-2012	Ministry of Interior
Current expenditures	Current expenditures per capita	1998-2012	Ministry of Interior
Capital expenditures	Capital expenditures per capita	1998-2012	Ministry of Interior
Tax revenues	Collected taxes per capita	1998-2012	Ministry of Interior
Collected taxes	Collected taxes per capita	1998-2012	Ministry of Interior
Contracted loans	Contracted loans per capita	1998-2012	Ministry of Interior
Capital transfers	Capital transfers by the government or the region, per capita	1998-2012	Ministry of Interior
Margin (Dynasty)	Margin of the best dynastic candidate	1993-2014	Ministry of Interior
Margin (Candidate)	Difference of vote shares between the candidate and his best challenger	1993-2014	Ministry of Interior
Number of candidates	Number of candidates in the election	1993-2014	Ministry of Interior
Incumbent	Whether the candidate was already elected as mayor during the previous term	1993-2014	Ministry of Interior
Term Limit	Whether the mayor is reeligible or not	1993-2014	Ministry of Interior
Reelection	Whether the mayor is reelected	1993-2014	Ministry of Interior
Term duration	Number of years the mayors remained in office during the term after his election	1993-2014	Ministry of Interior
Ability of revenue collection	Ratio between actual and expected revenues	1998-2012	Ministry of Interior
Speed of payment	Share of due expenditures paid during the term	1998-2012	Ministry of Interior
Growth of private tax base	Yearly growth of private tax base, expressed in percent	2000-2011	Ministry of Interior

* Done by the authors and based on the name of the job, using the official socioprofessional categories of the Italian government