Political dynasties, development, and corruption: A study on the island of Java

Marco Colleoni, Giacomo De Luca, Jahen Rezki

Abstract

Dynastic politicians are relatives of previously elected politicians, who enter the public domain in their turn. We study this phenomenon in Indonesia, where it is central in the public debate over democratic legitimisation, because the dynasties tend to persist in the districts where they manage to emerge. To our knowledge, the question of their effect on economic development and public good allocation has not been analysed yet by the scientific literature. We do so through data collection in the island of Java, and we implement two empirical strategies - RDD and DID - and compare their outcomes. First, the two of them outline a generally positive correlation between dynastic politicians and economic outcomes. However, it inverts in the RD specification, if we only focus on "vertical" dynasties, i.e. those whose active members in politics are not limited to husbands and wives. Second, neither RDD nor DID highlight that corruption and transparency records worsen when dynastic politicians are in power. Yet, this might be due to the ambiguity embedded in the outcomes: less corruption may be recorded because of more honesty, or because of a more accommodating juridical system. Third, DID finds positive correlations between dynastic politicians and measures of the revenue and expenditure of the local governments, while RDD implies the opposite. Such inconsistency may be due to the shortcomings of RDD in this setting, because there is evidence that the cutoff point might be manipulated by the politicians, thereby not representing an exogenous threshold. Furthermore, the robustness checks highlight that some of the effects isolated by RDD might be due to pre-existing characteristics of the districts. Overall, the DID strategy seems more reliable, and we fail to conclude that dynastic politicians adversely impact on the economic environment of their districts. Yet, the effects of a change in rule typically become manifest in the long run, as they pass through a change of the administrative environment and of the bureaucratic culture.

1 Introduction

In all democracies, self-interested politicians are confronted with incentives in decision-making to remain in power, but they face the binding constraint of term limits. Across time and space, there is diverse evidence of politicians' relatives entering the public domain with the endorsement of the incumbents, perhaps in with the hope of circumventing such constraint. In consequence, we wonder how politicians' relatives who successfully secure a place in politics differ from non-dynastic politicians when it comes to the management of public affairs. Are they in quest for personal enrichment and power grab through public policy, so as to perpetrate the family's position of centrality in

politics? Or are they more competent in the field, hence capable of delivering a welfare-improving allocation of the public goods? If both cases are true at the same time, then what is the dominant component? With these questions in mind, we focus on the understudied case of Indonesia, a young democracy where the presence of more members of the same families in local politics is becoming an increasingly widespread phenomenon. To this purpose, we have taken advantage of a very granular and detailed data collection implemented on our behalf by the Institute for Economic and Social Research of the University of Indonesia. Although it is currently only available in the island of Java, this is by far the most important component of the Indonesian archipelago. Among others, it hosts the capital Jakarta and produces more than half of the country's GDP[1].

Indonesia is divided into provinces, which are further split into regencies[2]. Since these latter second-level administrative divisions are endowed with more policy-making capacity and administrative power than the former first-level divisions, our data are computed at regency level. Local elections to directly appoint the executive branches of these entities have been organised starting from 2005. We have retrieved the candidates list and vote shares in the regency elections since that year, and we have built an exhaustive dataset of social and demographic outcomes at the regency level, up to the year 2020. The outcomes can be grouped into four categories. First, broad economic and development indicators such as - but not limited to - GDP, employment shares, and the Gini index are reported. Second, local public expenditure and its share allocated to specific sectors in which regencies have some discretion (i.e. health, education, and infrastructure) is recorded, as well as the number of public goods specific to those sectors (e.g. hospitals and schools). Third, the revenue in fields whose taxation depends on the discretion of the local government (restaurants, hotels, and general entertainment) is registered, together with the revenue collected from the taxes defined by the central government; in parallel, we have computed the money sums accruing to the regencies from the transfers from the central government - which either come as discretionary allocations or result from a centralised computer-based implementation. Fourth, information is available concerning indicators of public (mis)management, such as the recorded corruption cases, audit data relating to the transparency of the administration, or the total pay of the regency employees. The broad spectrum of the variables under study is intended to cope with the research question. If political dynasties correlate with a more efficient public good allocation, we should observe positive effects on the variables of the first group - which in fact we do. Instead, if the rapacity channel dominates, we should observe a positive effect of dynastic politicians on the variables of the fourth group, or a surge in revenues but not in expenditures.

To the purpose of identifying dynastic politicians, our database lists their names as well as those of their relatives who are or have been active in politics. Each individual row includes information on the presence of the spouse, children, siblings, parents, or parents-in-law in the public domain. Politicians who do not have their own entry, but are only included as relatives of someone else, can be considered as the founders of a dynasty. As such, they may not be dynastic agents by themselves; rather, the dynastic politicians should be seen as their continuation in the political sphere. The aim of this paper is to compare the public policy outcomes of the rule of dynastic and non-dynastic politicians. With the purpose of coping with the likely endogeneity concerns, two empirical strategies can be conceived. The first is a regression discontinuity design that uses the difference in the

[1]With a population of 150 million - roughly equivalent to 55% of the country - Java alone would score 8th in the world in 2023, if it were an independent state.

[2]It should be noted that big cities may form a regency alone.

vote shares of the dynastic and non-dynastic candidates as the running variable. The cutoff being set at zero, we define the treated group as composed by the yearly observations accruing from the legislatures where a dynastic candidate has won the elections against a non-dynastic candidate. Conversely, the control group is composed by the yearly observations stemming from legislatures where a non-dynastic candidate is in power after having won the contest against a dynastic candidate. The fundamental idea is that, in the neighbourhood of the cutoff point, the politician who gets elected is as good as random. Hence, by construction, RDD gives more attention to the outcomes of the legislatures where a dynastic candidate has barely won, and to those of the legislatures where he or she has barely lost. This is done through the non-parametric selection of the optimal bandwidth, and through the triangular weighting scheme of the observations falling between the cutoff point and the bandwidth limits. However, RDD comes with a series of limitations. First, in order to check whether the effects that emerge from this specification are attributable to pre-existing district characteristics rather than the candidate, we run a separate analysis where we consider the outcomes of the two years before the start of a given legislature, as if they belonged to the treatment group determined by that legislature. Overall, we find that dynastic rulers are associated with higher development outcomes, less revenue, less public expenditure, but not higher corruption outcomes than non-dynastic politicians. Yet, the results from the falsification tests above described imply that the effects may sometimes be pre-existing in the same districts. Second, to justify the as-good-as-random claim, the RD threshold must be exogenous and not subject to manipulation. Instead, the McCrary density tests performed on the running variable show a high concentration of observations immediately at the right of the threshold. In this setting, it can exist because dynastic candidates might be able to manipulate the electoral outcomes in their favour, so much as to be over-represented on the winning side.

For this reason, we take advantage of a second empirical strategy by means of two difference-in-difference estimations. To this purpose, we generate a balanced panel of legislatures, comprising the five-year length as well as the two years before the start of each one. We consider those legislatures where a dynastic ruler is in power as the treatment group, and those where a non-dynastic ruler is in power as the control group. Alternatively, we also set up the analysis at regency level, considering the treatment to start from the moment when the first dynastic politician is elected. Both specifications - as opposed to RDD - make use of the full database, thereby improving on sample size, but DID relies crucially on the parallel-trends assumption. In short, the DID findings are consistent with RDD when it comes to the positive correlation between treatment and development outcomes, and the negative correlation between treatment and corruption outcomes. Instead, DID hints mostly to a positive effect of the dynastic politicians on revenues and expenditures, while RDD suggests the opposite. The reason for such discrepancy may be the following. Because RDD puts more weight on politicians that barely win or lose, and because the density tests imply that dynastic politicians are favoured when they are about to lose, it follows that in the treatment group there are many individuals who barely win despite being favoured. Hence, the observations in the treatment group of the RD specification might be endowed with lower ability on average. This in turn can influence their willingness to raise or spend public money. Consequently, the RD specification might encounter a further empirical issue: the lack of a relevant unobservable characteristic (ability). Therefore, we tend so see DID findings - albeit strongly dependent on the parallel trends assumption - as more reliable. Since they mostly fail to reject the null hypothesis, we conclude that there is currently no evidence of adverse welfare effect from the rule of dynastic politicians.

Despite the current empirical shortcomings, the role of dynastic politicians in Indonesian policy-making deserves careful consideration. Its relevance has often placed the question at the centre of the public debate of the fourth most populated country in the world. In order to better understand the issue, we present an historical overview of the main facts. Indonesia started its democratic transition in 1998, with the resignation of then-president Suharto. Since that moment, a system of institutional checks and balances has been gradually implemented, whereby the power of the executive has been constrained in the spirit of a modern democracy. Although the country has formally held elections since 1971, it is only after 1998 that the national parliament and local councils have gained more centrality in policy-making. Starting from 2004, citizens were also allowed to vote directly for the president and the local regents. In particular, the process of devolving more powers away from the central institutions has been met favourably in the country, as a way to limit potential future tendencies to concentrate all power in one person again. This has become particularly evident in 2015, when political pressures made president Joko Widodo retract his proposition to reintroduce indirect elections in the regencies, and the Supreme Court ruled against a law that was meant to hinder the chances of the relatives of local politicians to run for the position of regents. Yet, it is all the same evident that the process of power devolution strengthened the position of the local elites. In some cases - such as in the province of Banten - one sole family has come to be particularly pervasive in the whole public domain. The literature provides some explanation of how similar dynasties may have come to exist. Kenawas (2022) puts the emphasis on the conflict within the central state. While the liberal elites have carefully designed tools to prevent the rise of a new dictator at the central level, power devolution has given more discretion and freedom of action to politicians who locally aspire to a hegemonic position. Aspinall and As'ad (2016) underline the weak party structure and high entry costs in local politics. Because the candidates need to be nominated by a party in order to run in the regency elections, those with higher endowments achieve the goal more easily, and may even receive nominations by more than one party, thus further restricting the candidates' pool. Yet, these people are not necessarily part of the economic elite; rather, they exploit their proximity to the elite - especially in the oil sector - and a position of network centrality at the local level. This explanation is in line with the evidence produced by Labonne and Querubin (2017), who study the emergence of political dynasties in the Philippines, by exploiting the direct links provided by the naming structure in that country. As the Philippines used to be a Spanish colony, the surnames of both parents are registered as the children's. Labonne and Querubin find that network centrality is indeed the main driver of political success.

Instead of the causes behind the emergence of political dynasties, our work aims to enquire on their effects. Ferraz, Finan, and Martinez-Bravo (2022) approach the issue in the context of Brazil. They are concerned with the effects of hindering the existence of local political elites. Similarly to the Indonesian case, the military dictatorship that ruled the South American country from 1964 to 1985 put obstacles against the political participation of families that might constitute a point of reference potentially able to threat the central power, albeit only at the local level. It did so by means of an ad-hoc electoral law, which favoured the candidates nominated by the central state and set incentives against the creation of alternative parties. Ferraz et al. find that development outcomes and public good provision in the municipalities that used to record a high concentration of power before the dictatorship significantly improved in the early 2000s with respect to the rest of the country. This result indirectly implies a negative effect of local elites, as getting rid of these becomes the key driver of such reversal of fortune. However, estimating a direct effect of the local elites - in the shape of families of politicians - has not been, to our knowledge, an

attempt yet undertaken in the economic literature. Our contribution is to propose a first analysis of the question, thanks to a unique dataset covering all politicians in Java from 2005 to 2020. We acknowledge that the effects of a change in the institutional environment, such as those entailed by the emergence of dynasties of politicians, can mostly become manifest in the long run. Thus, this may be a reason why the empirical setting lacks strongly significant results, and it can prove to be a motivation to replicate our work in the future, when more data are disposable. Furthermore, we focus on an area - such as that of South-East Asia, where Indonesia is located - in which the issue of the emergence of dynastic politicians is pressing in the current debate, and our research may have potentially policy-relevant implications at the national and international level. Indeed, the discussion on the pros and cons of local autonomy lies in the framework of understanding the best conditions to preserve democratic legitimisation. Our work contributes to assessing if - and in which degree - local autonomy in the context of rising local dynasties poses a risk to the democratisation path in countries that have recently emancipated from autocratic governments. With the purpose of describing our analysis in more detail, this paper is organised as follows. Section 2 deals with data collection and the coding process to define treatment status. Section 3 proposes some relevant descriptive statistics. Section 4 indulges on the two main empirical strategies, the main results, and the robustness checks. Section 5 concludes.

2 Data

Our database has three components: the list of dynastic candidates, the electoral information, and the socio-economic outcomes. The geographic unit is the regency, i.e. the second-level administrative division of the country after the province. Regencies are sampled on a yearly base from 2005 to 2020. In total, there are 1792 observations, spread across 112 spatial and 16 temporal units. In quite rare cases, there are missing values before the year 2008, because some current regency-level cities (such as Serang and Tangerang Selatan) have been carved out of their former regency to become distinct administrative units. Furthermore, it should be underlined that not all outcome variables are available in each regency-year combination. Because these vary by outcome of interest, the precise available number of observations is reported in each inference table at section 4. In what follows, I describe the characteristics of the three data groups.

To begin with, the list of dynastic candidates reports the names of 350 politicians: 155 of these have their own row entry in the list, while the remaining 195 are only included as relatives. They can be can be fathers, mothers, siblings, spouses, children, or parents-in-law of the politicians with their own row entry. The data at our disposal on the dynastic candidates has been directly collected in Indonesia, and is a novel set of information in the literature. Despite the care that has possibly been adopted in filling out this database, an issue stands out and must be highlighted. Each dynasty has a founder, i.e. a politician that was not dynastic but started his own political lineage. One might expect the founders of the dynasties to be of a smaller or equal number than the proper dynastic candidates. If we interpret these latter as the individuals having their own row entry in the dynastic list, then those without such entry must be the founders. Yet, they are reported in higher amounts than the proper dynastic candidates, which is a contradiction to the first hypothesis. Thus, should one think that at least some of the dynastic candidates have been neglected while filling the row entries of the list? If yes, then who? Facing the impossibility of a correct answer, we have produced two scenarios covering the extreme cases: in the first, all individuals mentioned

in the dynastic list are considered as dynastic candidates; in the second, only those with their own row entry are imputed as such. In order to build the full dataset, we follow a sequential procedure involving the other two components of the main database.

First, we merge the names of the individual politicians recorded in the dynastic list with the names of the politicians included in the electoral data. These latter contain information about the outcomes of the political competition at the regency level from 2005 to 2020, including the winning candidates and vice-candidates for the position of regent, as well as the runner-up candidates and vice-candidates to the same position. It should be noted that, due to the structure of these data, an alternative to the name merger is not viable. Indeed, any other merger would not have made clear if - in the legislatures where at least a dynastic candidate showed up - this was the winner or runner-up of the electoral contest. We need this precise information to define the treatment groups. Furthermore, the politicians who are not listed with their own row entry in the dynastic data are not reported the district where they run, which makes the name-based merger all the more necessary in this case[3]. Clearly, we have scrutinised the spelling of the politician names before venturing with the merger. In fact, some relevant differences are also observed, both within the electoral data themselves, as well as across these and the dynastic list. However, they are promptly corrected, as summarised in the Appendix at Table 1A and Table 1B. In addition, the electoral data record turnout and the total votes accruing to each candidate. Importantly, we also have information on the vote-shares of the first- and second-ranked candidate-teams for every election. We use their difference to build the running variable. The indication of whether a candidate or vice-candidate regent is a dynastic politician - which we backed up from the merged list - allows us to build treatment as a binary variable. In particular, treatment can defined based on the running team, or the single candidate. In the former case, we say that treatment equals 1, if at least one person among the winning candidate-vicecandidate team is dynastic and none is among the losing candidate-vicecandidate team. We tend to prefer this definition because vice-regents can influence the policy-making of the regents, especially if they come from an advantaged background.

Second, the electoral and dynastic data are merged with the main outcome data, allowing to incorporate relevant socio-economic information on a regency-year basis. As anticipated, the outcome variables can be grouped in four distinct areas.

1. The economic indicators include the total real GDP, GDP growth, unemployment rate, employment rate, population, literacy rate, population density, urban share, poverty share, number of people below the poverty line, morbidity rate, immunisation rate, human development index, households total expenditure, households education expenditure, households health expenditure, Gini index, and the partition of GDP by sector (accommodation, agriculture, construction, education, electricity, finance, manufacture, mining, oil, trade and retail, wholesale).

2. The local government outflows include the total expenditure (distinguished by capital and labour), the partition of such expenditure by the sectors in which the regencies can implement autonomous policies[4], the number of schools and school enrollment by school level, as well as the number of hospitals and clinics.

[3]The addition of the politicians without their own row entry makes the number of merged politicians surge by around 40%.

[4]They are education, health, and infrastructure.

3. The local government income is summarised by the measure of total revenue, which is composed of the transfers from the government, the collected taxes, and the money accruing from the companies participated by the regencies. In turn, the total transfers from the government include the development funds sent at discretion of the central administration (dak), the development funds based on objective criteria imputed in a computerised social welfare function (dau), and the general transfers for purposes different from development. The total tax collected includes the sum mandated by central policy[5], and the tax accruing from the sectors in which the regencies have legislative autonomy[6].

4. The data on the quality of management include recorded corruption amounts,[7], the audit score based on criteria of administration transparency, and the salaries paid to the employees of the regency.

The aim of this paper is to isolate the effect of the rule of dynastic politicians at the local level. As anticipated, if a surge in the economic indicators stems out of the presence of dynastic politicians, then we might infer a positive effect. Instead, if an increase in the corruption outcomes or a fall in the expenditures without a proportionate fall in the revenues follows from the presence of dynastic politicians, then we might infer a negative effect.

3 Descriptive statistics

Building from the previous section, we have identified 350 individuals, who belong to or have built a political dynasty. In what follows, we highlight two relevant characteristics of these families of politicians. First, it should be noted that they are almost evenly split between "horizontal" dynasties (whose core component is the husband-wife relation) and "vertical" dynasties (whose core component is the husband-son relation); second, they are locally concentrated, as some regencies are characterised by a strong presence of dynastic candidates while the same phenomenon is almost unknown to others.

For every dynastic politician listed in the data, Table 2 reports the percentage of relatives that are also listed as dynastic. It emerges that the spouses of 34% of all dynastic politicians are also active in politics. This is by far the most important category, hinting at one common tendency in the process of dynasty formation. Most of the times, the founder of the dynasty is a man who tries to overcome term limits by endorsing the wife as successor. On a smaller but still relevant scale, the children and siblings of active politicians are also in politics, in respectively 20% and 27% of the cases. It should be noted that the share of politicians with a father in politics, summed to the share of those with a mother in politics (18.6%+5.1%=23.7%), is higher than the share of those with a child in politics. Even if this result may sound counter-intuitive at first, it is explained by the fact that almost everybody, whose mother is an active politician, also have the father as politician. In other words, it is very rare that a dynastic politician is only tied to the mother, when she is a politician. Thus, the presence of women in political dynasties in Indonesia is deeply related to their role of wives. Yet, this role is not at all negligible. As Chart 1 shows, roughly 38% of the

[5]The regencies send this back to the government, but for the part that remains available locally - which is called pad.

[6]They are entertainment, restaurant, billboards, and hotels

[7]Both as a total and as partitioned by the category of corrupted local official.

350 dynastic politicians is reported to be part of a political dynasty of at maximum two elements[8]. In short, not only the presence of spouses is abundant, but also the politicians whose dynasty is made of at maximum two individuals make up almost half of the sample. Combining these two pieces of evidence, we conceive that the sample must include a significant portion of "horizontal" dynasties - i.e. those made of husband and wife. To test this guess, we have produced Chart 2, where we distinguish the cumulative distribution of the extension of the dynasty when a spouse is politician, and when a father is politician. The figure graphs a second-order stochastic dominance of the former[9]. In more than 60% of the cases in which a spouse is politician, the political dynasty is composed of 2 members. Instead, less than 30% of the cases in which a father is politician have a dynastic extension of 2. Typically, these "vertical" dynasties include more than one family member.

The second distinctive tract of political dynasties in Java is that they appear to be quite locally concentrated. As anticipated, the island is divided in 112 regencies. Chart 3A underlines that 33 of these have always had at least one candidate regent coming from a political dynasty, in all the elections held in the period between 2005 and 2020. As it is visible in Chart 3B, the dynastic candidates have always won in 24 of the 88 regencies where they participated at least once in the elections. Instead, they never participated in the elections from 2005 to 2020 in another 24 regencies.[10]. Such result speaks of a polarisation of the presence of dynastic politicians, with 33 regencies having dynastic candidates who always win when they run, as well as 24 of the same administrative units in which the phenomenon is still unknown. The polarisation is particularly evident at the aggregate geographical level depicted in Chart 4. The island of Java is composed of five provinces (in addition to the capital Jakarta). In one of these provinces - Banten - the dynastic candidates always win in as many as 62.5% of the regencies[11]. The second in the list is Eastern Java with 23.7%. This geographical aspect is quite worrisome, because the risk is to have regencies completely infiltrated by the same families. Indeed, once a dynastic power succession happens, it is more likely that it strengthens. Chart 5A illustrates that, while 34% of non-dynastic politicians get reelected in the same regency, this ratio grows to 54 for the dynastic politicians. Furthermore, such number increases by more than 3 percentage points for the latter category, if we include the possibility to be elected for a new mandate elsewhere. In contrast, it increases by 2 percentage points for the non-dynastic politicians, as it can be observed by comparing the two columns of Chart 5B. Furthermore, a positive - albeit weak - correlation exists between the fact of being a dynastic candidate and the vote share margin of the winner of the electoral contest, and it is documented at Chart 6. In sum, it seems that political dynasties become endemic quite easily, as their members get elected with stronger majorities, and tend to stay longer in power, but they emerge only in a limited number of regencies. This generates a polarisation outcome, whereby some local authorities have become strongholds of the dynastic politicians. Consequently, the current debate is split between two points of view associating these local dynastic politicians either to authoritarian threats that could extend their power grab to the rest of the country, or to a much

[8]There are 77 politicians in a dynasty of at maximum 3 members. This should not be interpreted as the fact that somebody is left alone - which would contradict the definition of dynasty. Rather, a wife and a husband may have a different maximal family extension, if one of the two is also related to somebody with whom the other has no tie. In other words, we need not expect that the number of people in a group presented in Chart 1 is divisible for the number measuring their dynasty extension.

[9]The outlier at 13 in the spouses cumulative distribution is the very widespread Chasanah family.

[10]Chart 3B is depicted conditionally on the participation of the dynastic candidates. Thus, it disregards the regencies where they never participated in the elections. Indeed, the total number of districts is 112 (Total regencies of Chart 3A) - 24 (Regencies where dynastic candidates never participate) = 88 (Total regencies of Chart 3B)

[11]Banten is the place where the biggest dynastic family of Java - the Chasanah - is from.

needed element of check and balance at the local level preventing possibly despotic approaches from the central government. In light of this, we provide a tentative empirical approach to the question in the next section.

4 Empirical strategies and estimation

The attempt to measure the average causal effect of the dynastic politicians on public policy and development outcomes rests fundamentally upon the removal of the treatment selection bias. If the political dynasties are tied with an economic elite, then more developed regencies pose a better ground for their emergence. If these places have also a more unequal distribution of wealth, then education and human capital might be concentrated too. This hinders the availability of alternative candidates to those endorsed by local elites, who might in turn discourage redistribution to maintain the status quo. Alternatively, richer regencies may have higher education levels, and a wider support for progressive taxation schemes, which can in turn be accepted by the elites for fear of losing influence. In both cases, it is evident that the emergence of dynastic politicians is not at all exogenous to public policy choices. A tentative causal interpretation requires to deal with this endogeneity. Consequently, we propose two empirical strategies, which are going to be described in more detail in further separate subsections. First, we follow a RD approach using the vote share as running variable. The underlying idea is that the candidate who gets elected in the neighbourhood of a zero difference in the vote share with the runner up is as good as random. Yet, for RDD to be unbiased, it is also required that the threshold must be unanticipated by the agents. If this was not the case, there could be some unaccounted factors that hinder treatment compliance, thus biasing the estimates, as we show in more detail subsection 4.1. Alternatively, the second approach is to follow a DID strategy, whereby we compare the difference between treatment groups before and after the moment when treatment kicks in. In this case, the crucial assumption is that the outcomes have parallel trends across the two groups before the cutoff, so that the change in their difference can be attributable to the treatment itself.

4.1 Regression discontinuity

The initial empirical specification we use to have a preliminary idea of the effect of dynastic politicians on public policy and development outcomes is a regression discontinuity design with triangular Kernel-weighting function, MSE-optimal bandwidth selection, and polynomial order 1, as in specification [1] below.

$$[1] \quad Y_{it} = \beta_0 + \beta_1 D_{it} + \beta_2 C_{it} + \beta_3 D_{it} * C_{it} + \beta_4 X_{it} + \epsilon_{it}$$

The unit of observation is a regency (i) in year (t).
Y_{it} is the outcome variable of interest.
D_{it} is a binary variable that accounts for assignment to treatment. It takes value of 1 if regency (i) at time (t) has a dynastic governor or vice-governor, and they have won the latest election against a team of non-dynastic candidates. Conversely, the variable takes value of 0 if a team of non-dynastic governor and vice-governor has won the latest elections against a pair of candidate and vice-candidate, of whom at least one is dynastic. In other words, those legislatures where voters did

not have the alternative between a dynastic and a non-dynastic team of candidates are excluded. This is done to avoid having in the treatment group the legislatures where dynastic candidates have barely won against other dynastic candidates, as well as to avoid having in the control group the legislatures where non-dynastic candidates have barely won against other non-dynastic candidates. Otherwise, as mentioned at the beginning of this section 4, some unobserved characteristics of the regencies where the supply of candidates is of only one type can be correlated with the outcomes we wish to measure[12]

C_{it} is the running variable: it measures the difference in the vote-share between the winner and the runner-up in regency (i) in the latest election. To be as aligned as possible with regards to the econometric literature, we fit a polynomial of order 1. Hence, this variable only enters linearly in specification [1].

The interaction $D_{it} * C_{it}$ opens the possibility that the linear trends in the outcome variable have a different slope before and after the threshold.

X_{it} is a vector of covariates. Table 3 shows that all the major variables are balanced across treatment groups. We always control for the electoral cycle. Unless the outcome of interest is determined in per-capita terms, we also control for population.

Standard errors ϵ_{it} are clustered by regency (i).

Unless when the variables are set per-capita terms or as percentage, the unit of measure is one billion Indonesian Rupees, roughly equivalent to 60.000 US Dollars.

As anticipated in section 2, we run the analysis summarised by equation [1] on three different treatment specifications. In the first instance, we consider all the politicians mentioned in the dynastic sheet to compose the treatment group. They include those with a specific row entry, as well as those that are only mentioned as relatives of the former - who might be the founders of the dynasties. In the interest of clarity, the outcomes are grouped in the four categories mentioned at section 2. Hence, the RD estimates from this scenario are reported at Tables 4A, 4B, 4C, and 4D. It should be noted that there are three different RD point estimates for each outcome of interest. Following Cattaneo et al. (2019), we focus on the "Robust" coefficient[13]. Table 4A implies the following: a positive effect of the dynastic candidates on human development index, literacy rates, immunisation, number of people employed, and GDP; a negative effect on unemployment and the number of people living below the poverty line. However, of all the variables above, only the effect on HDI is statistically different from zero. Table 4B shows that dynastic politicians seem to spend more in education and health, which results in greater public good provision, as underlined by

[12] We certainly concede that it would also be possible to include all observations into either treatment group and generate a covariate to control for three types of candidates supply across the elections. However, a fundamental problem with the density test on the running variable - which is described in the continuation of this section - persists even in this latest scenario.

[13] "Conventional" is the ATT as estimated through the undershooting procedure. Given the kernel function and the polynomial order that we choose to approximate the unknown true regression function, the algorithm runs the task for producing the optimal bandwidth of the point estimate. The standard errors are estimated from a smaller bandwidth, but this process involves discretion, which is why Cattaneo et al. (2019) suggest to disregard it for the purpose of inference. The "Bias-corrected" approach runs first a higher-order polynominal - typically one degree more - in order to approximate a further degree of the curvature of the true unknown population regression function. With that information, the bias that the confidence interval would suffer if it was estimated using the same bandwidth as the point estimate is computed and corrected for. The point estimate is then also corrected for this bias, so that it is centred in the middle of the confidence interval. The "Robust" approach produces sounder standard errors by including the variation introduced by the estimation of the bias coefficient, which is then also supposed to be a random variable. Therefore, Cattaneo et al. (2019) recommend to use this latter approach for the purpose of inference.

the number of primary school enrollment. Table 4C underlines that legislatures where dynastic politicians are in power tend to gain less revenues, either from the central government, or by means of the taxes locally raised, or as development funds[14]. Finally, Table 4D shows lower expenditures in personnel, but higher recorded corruption amounts and a worsening transparency record[15] when the dynastic politicians rule. However, most of these findings are not statistically significant. Furthermore, since RDD is a static model, we wish to understand if the effects we measure were already present in the same regencies before the dynastic candidates start their office. For this purpose, we observe the same outcome variables two years before the start of each legislature, we assign these to the treatment they would receive in the new legislature, and we run model [1] again. If we find a point estimate of a comparable size and sign, it means that the results we attribute to dynastic politicians were already there. Tables 5A, 5B, 5C, and 5D report the outcomes of interest. In the vast majority of cases, we fail to reject the null hypothesis of a pre-existing effect. Yet, with the exception of the revenue outcomes in Table 5C, the point estimates have the same sign and comparable magnitudes to those in Tables 4A to 4D, thereby hinting to an already existing trend in the districts where regencies candidates emerge. This confirms the idea, anticipated at the beginning of this section, that the RD method might not be a good solution for solving the endogeneity concerns. In sum, although no evidence of a bad management of the public resources emerges from this first analysis, we cannot say that dynastic politicians sorted good outcomes on socio-economic indicators, because these seem to be at least partially regency-specific.

As anticipated in section 2, we now run a consistency check by defining an alternative treatment. In specific, we include only those candidates having their own row entry in the dynastic sheet, since they are dynastic politicians stricto sensu. In other words, they must not be the founders of a dynasty, since they are the relatives of politicians who have been running for office. In this specification, we lose some observations - passing from 158 to 131 unique legislatures in the treated group, and from 40 to 24 unique legislatures in the control group. Table 6A shows that the findings stemming from development variables are mostly consistent with the previous definition of treatment, as they emerge in Table 4A. This is also the case of the revenue and corruption outcomes at Tables 6C and 6D[16]. However, there is a discrepancy across the expenditure outcomes with the two treatment definitions: in this case, having a dynastic politician in power is associated with less expenditures across all sectors but health, while the sign of the same coefficient is positive if the former treatment definition is used. The negative effects on revenue might be suggestive that dynastic politicians refrain from implementing significant redistribution programs. Yet, both in Table 4B and in Table 6B we fail to reject the null hypothesis.

Finally, we restrict the treatment definition even further, by excluding from the numbers of dynastic politicians not only the individuals without their own row entry in the dynastic list, but also those belonging to purely "horizontal" dynasties, i.e. political families only composed of husband and wife. The rationale for this restriction rests in the descriptive statistics presented at Section 3, which depict a quite heterogeneous composition. Political dynasties are either big families with many individuals involved in politics, or duos where the wife succeeds the husband in the local administration. This latter situation can be viewed as a camouflage that former incumbents implement to remain in power despite the term limit. Yet, those horizontal dynasties are way less

[14] Dak is the discretionary amount devoted to development funds by the central government; Dau has more stringent criteria.

[15] The audit score increases in coulmn (4)

[16] With the exclusion of the audit variable, on which a negative effect is estimated at Table 6D.

connected and spread around the local environment, which fact makes it sensible to test what happens by not considering them in the treatment group. When the sample is refined by means of their exclusion, we lose some further amounts of observations, passing from 131 to 103 unique legislatures in the treatment group, and from 24 to 19 unique legislatures in the control group. In Table 7A, the presence of vertical political dynasties is associated with a deterioration of the outcomes for GDP, unemployment, and the number of people living below the poverty line. This is an interesting difference with respect to Table 6A and Table 4A, where all types of dynasties are included in the treated group. It suggests that not all dynasties, but perhaps those analysed here, may be harmful to local development. More akin to the previous case is the fact that the overall expenditure and revenue outcomes in Table 7B and Table 7C are negative. Yet, the transfers from the government to regencies where vertical families of dynastic politicians are in power are higher. This is likely associated with the deterioration of the development outcomes in the same treated group. In contrast, the vertical dynasties do not seem to produce more corruption or a deterioration of the transparency records, as shown in Table 7D.

In sum, RD results do not allow to disentangle a definite effect of the rule of dynastic politicians. The generally positive development outcomes (Table 4A) seem to be pre-existing to the dynastic rule (Table 5A), and suggest that the relatives of politicians might more easily emerge in richer regencies. However, when we specifically focus on bigger families, we observe a seizable deterioration of the same outcomes (Table 7A), which may hint that these are "bad" dynasties. In contrast, corruption outcomes seem to improve under the rule of these dynasties (Table 8D), but this involves an ambiguous interpretation. Indeed, it can be the result of more honesty, or of the fact that the local juridical system is more accommodating. Taxation and revenues are overall negatively associated to the presence of dynastic politicians (Tables 4C, 6C and 7C), implying that they correlate with less redistribution. In general, however, we almost always fail to reject the null hypothesis that the estimates are statistically equal to zero.

Furthermore, it is worth stressing a problem likely to hinder the causal interpretation of the findings from the RD model. As anticipated at the beginning of this section, one fundamental assumption for RDD to causally estimate the average effect on the treated is that the cutoff be exogenous. In other words, it must not be manipulated by the individuals under study to self-select into one treatment group. Another way to phrase this assumption in our setting is the continuity of potential outcomes. Suppose that an individual assigned to the treatment group reports an outcome with value X immediately after the cutoff. Then, it should report a value of the same outcome in the neighbourhood of X, if it was assigned to the control group immediately before the cutoff. This assumption cannot be verified by construction, because we do not dispose of a counterfactual where the same observation is assigned to both treatment groups. However, we can test the distribution of the running variable to check if there is a relevant concentration on one side of the threshold. If this is the case, then cutoff manipulation may be happening. Chart 7 reports the evidence from a similar test for the sample where all people listed in the dynastic database are included as dynastic politicians[17]. In fact, there is a significant density in the treated group (i.e. the winning side of the elections) right after the 50%-vote-share cutoff, which implies that dynastic candidates are favoured when competing against non-dynastic politicians. This could be because they manage to directly manipulate the voting outcome, so as to be persistently on the winning side when they are close to lose. However, if this was the case, the observed discontinuity should cease to exist at other placebo cutoffs. Since it does only partially, this explanation of the observed

[17]But results are unaltered by the other sample restrictions.

bunching is not robust. Alternatively, the dynastic candidates might be favoured by means of a propaganda machine that they more easily have access to. In this case, however, we would have a relevant unobserved characteristic that is prone to bias the causal interpretation of the empirical results, i.e. ability. If dynastic candidates get favoured by the propaganda, then they start with a margin of votes on top of the adversary. Knowing that RDD compares candidates belonging to the two treatment groups by putting more weight on those who win closely, a dynastic candidate that gets close to lose despite the propaganda advantage is likely to be a lesser able politician than his non-dynastic counterpart. Ability has an impact on public expenditure, because less able politicians might refrain from spending, or have access to less resources, or be less able to manage their funds. For these reasons, the RD method does might not be a good application to tackle the selection bias entailed by our research question.

However, by virtue of their characteristics, RD findings can also be suggestive of the following interpretation. Although dynastic candidates might be favoured when they are close to lose the electoral contest, there is not a strong evidence that they engage in policies harming the social welfare. This can be because dynastic politicians are not more self-interested than others on average, or because in our RD estimation we have a higher weight on politicians with a relatively low margin of victory. As before, we concede that these might be less able - hence we should expect worse outcomes - but the following case can also be true. Assuming that strong dynasties win by high margins, the candidates in the treatment group of the RD specification cannot belong to those dynasties. To check if high margins of victory are responsible for effects that go beyond the dynastic element, we implement a *horse-race* through the standard OLS model, whereby we compare the relevance of the coefficients associated with the political dynasties, the vote-share differences, and the interaction of the two. The idea is that if this latter coefficient is significant, then there is an effect limited to the strong political dynasties; instead, if the second turns out to be more important, then there is a channel depending more on the political competition in the regency, rather than with political dynasties. As underlined at Table 8A, we fail to reject the hypothesis that the electoral competition, the presence of political dynasties, and the interaction of the two are irrelevant in affecting the development outcomes. Furthermore, by means of a Wald test, we fail to reject the hypothesis that the coefficients associated with the treatment (dynastic politicians) and the margin of victory are equal (and hence equal to zero). Instead, Table 8B and Table 8C suggest that, while there is not an effect of treatment per se, revenues and expenditures are positively associated to politicians who win by a high margin, as the interaction coefficient is generally positive and sometimes statistically significant. The presence of an effect in these outcomes and its absence from the development variables may reinforces the interpretation that we have in fact an issue with ability in RD. Indeed, expenditures and revenues require competence to be managed and collected, but RD only include dynastic candidates who are arguably less able than others, winning as they do by a small margin - despite the advantage of being dynastic. A similar finding is confirmed by a further specification of the model - DID - which is going to be the object of the next section. Finally, at Table 9, we evidence that certain type of regencies are associated with a deterioration in the transparency of the local administration, as measured by the increasing effect in the audit records. These are the regencies where mining and agriculture represent a relevant share of the local GDP. Because the latter industries are typically concentrated and have strong barriers to entry - due to latifundia and the concessions required to exploit the mines - we conclude that it is possbile that public mismanagement might be related to resource course, rather than to political dynasties per se.

4.2 Difference in difference

In the RD approach, we implemented two separate sets of regressions: the main specification (such as in tables 4A to 4D, 6A to 6D, and 7A to 7D), and the robustness checks prior to the start of any legislature (such as in tables 5A to 5D). As a further but conceptually akin empirical approach, we have built a balanced panel. Every legislature is imputed the outcome variables running from two years before their start until their end, which is mandated by law to be in the fifth year after the election[18]. Hence, we estimate the following DID model:

$$[2] \quad Y_{ict} = \beta_0 + \beta_1 P_{ic} + \beta_2 C_t + \beta_3 P_{ic} * C_t + \beta_4 X_{ict} + \epsilon_{ict}$$

Y_{ict} is the outcome variable of interest in legislature i, regency c, and year t.
P_{ic} is a binary variable taking value of 1 if legislature i in regency c belongs to the treated group. This is defined as an incumbent team of governor and vice-governor, of whom at least one is a dynastic politician.
C_t is a binary variable taking value of 1 if the observation is reported after the start of the concerned legislature. In other words, it takes value of zero in the two years before the election, which have been appended to every legislature.
X_{ict} is a the population covariate recorded in legislature i, regency c, and year t, unless the variables of interest are expressed in per-capita terms.
Standard errors ϵ_{ict} are clustered by regency c.

The DID strategy has the advantage of exploiting the whole sample as opposed to RD, which ignores by construction the legislatures with a high margin of victory. Furthermore, we excluded from the RD treatment the legislatures where candidates of the same type have been running for office. For example, contests between two dynastic or two non-dynastic teams have been dropped. Instead, we include these in the DID analysis, because the type of politicians against whom the dynastic candidates have won is legislature-specific. Thus, the fixed effect accounts for this[19]. Despite the increased number of clusters obtained by the inclusion of all winning dynastic candidates in the treated group, specification [2] seems to suggest overall a null result, even if it is the dynastic politicians who score better in the circumstances where significance is achieved. In particular, there are two variables in Table 10A, for which the DID estimation doesn't fail to reject the null hypothesis with a 90% confidence interval. They are the total GDP - which shows a positive significant impact of the dynastic politicians - and the Gini index, which hints at lower inequality. However, the average Gini index across the sample is 0.32, which suggests that the -0.01 coefficient might be a precisely estimated zero effect. Indeed, it vanishes if we remove the legislatures that are preceded by treated legislatures[20]. Also, a higher number of people employed and a lower unemployment rate are associated with dynastic politicians, but the magnitude of the effects and the amplitude of the standard errors make us fail to reject the null hypothesis, which fact is along the lines of the results from the RD specification. Instead, Table 10Buggests that dynastic politicians are related to more public expenditures, especially in the domains of health and education, that are dependent on the local government. As previously anticipated, this suggests that RD might indeed suffer

[18] However, some have ended before the legal term.

[19] In RDD, if we want to use the whole set of dynastic politicians who have won, we should build a variable accounting for who they have won against. Even in that case, however, the density test seems to suggest it is not a viable empirical strategy for our purposes, as specified at Chart 6B.

[20] The rationale is that there is in fact not a dynastic change in the election when a dynastic politician is elected, if he or she or another dynastic politician was also in power before.

from failing to take into account the unobserved ability of the politicians, which is arguably lower for dynasty members who barely pass the threshold. Lower ability would make them refrain from public expenditures, which require competence both in management and on the subject of interest. When the possible selection on lower ability levels is overcome, such as by the DID specification - which does not allocate weights depending on the distance from the electoral cutoff - then we observe that the negative effect is overcome and inverted. Table 10C and Table 10D mostly depict negative coefficients, but fail to reject the null hypothesis, which fact may be interpreted in two ways: either as evidence of a null result, or as a limitation related to this specific empirical approach. By construction, the DID estimation implies that the effect should kick in right when treatment begins. However, the effect of political dynasties may take definitely more time to become manifest.

For this reason, we enlarge the reference spatial unit from legislatures to regencies. Table 11 reports the development outcomes of a DID specification where districts are treated once the first dynastic politician gets in power. While the there is a strong consistency in the sign of the effects with Table 10A, we fail to reject the null also in the cases of the Gini index and total GDP[21]. The other outcomes are omitted for brevity, but we fail to reject the null for almost them all. However, the results are consistent with those estimated in Tables 10B to 10D, including the positive expenditure outcomes, which - as previously explained - seem to question the validity of the RD findings at Tables 4B, 6B, and 7B. In sum, the results from the DID method are consistent within their two specifications and across the OLS specification, highlighting mild positive effects of the dynastic politicians on development, expenditure, and revenue variables; instead, mostly negative coefficients are returned from corruption outcomes. As previously seen, this could be due to both of the following: dynastic canidates are more honest, or the juridical system is more accommodating to them. Overall, our findings do not seem to sustain the hypothesis that dynastic politicians harm local welfare per se. Yet, a longer database than the one currently available - which spans 15 years at maximum - would probably be more apt to confirm this conclusion, because the effects of institutional changes or political culture shifts (as the one entailed by the growth of families of politicians) tend to become manifest in the long-run, as it is argued by a long literature stream in political economy.

5 Conclusion

Studies about the emergence of local political dynasties in young democracies represent a fairly recent research stream within the literature of political economy. Much has yet to be said, especially on what concerns the effects that dynastic politicians have on the development and the allocation of public goods in their districts. Do they serve an exclusive elite and are more self-interested than others? Or are they more competent and expert due to a longer history of dealing with politics and the bureaucracy? The following results stem from our combined use of different empirical strategies. First, the development outcomes do not seem to be negatively associated with dynastic politicians; if anything, the opposite is true in the instances where statistically significant coefficients emerge - such as those on the human development index in RDD and those on GDP in difference-in-difference model. Second, development outcomes shrink in RDD, if treatment is restricted to widespread dynasties - which fact could lead to the implication that bigger families of politicians, much enshrined

[21]It should be noted that the baseline year changes across the specifications in Table 11. The criterion is that the baseline is the fist year in which the outcome is measured. Hence, it is 2005 in 7 cases, and 2007 in the remaining 2.

in the local political and economic landscape, have worse effects than smaller dynasties that are only composed of husband and wife; however, there is not a similar shrinking evidence when DID is implemented. Third, expenditure and revenues are negatively associated with treatment in RDD and positively correlate with the same variable in DID; this opens the door to the issue that the former specification might fall short of considering unobserved ability, which is likely lower in the dynastic candidates who win by a small margin albeit being favoured. Fourth, corruption-related outcomes generally correlate negatively with treatment, and this has an ambivalent interpretation: either dynastic politicians are more honest, or the juridical system is more skewed in their favour. Overall, it is evident that the phenomenon of political dynasties is spreading and is quite polarised, in that it concerns mostly some regencies: for example, in 24 of these, dynastic politicians always win the elections when they participate. This is particularly relevant, since constitutional reforms in Indonesia have decentralised some powers to local administrative units with the specific intention of strengthening the democratisation process. Hence, a clear answer to what happens because of the rule of dynastic politicians is compelling to this purpose: avoiding any potential unintended consequence of the decentralisation, such as perhaps the establishment of a corrupted network creating local power bases that keep the members of the same family steadily in power. We have tried to remove the selection bias ingrained in our research question by means of two empirical strategies mentioned before. In particular, RDD focuses on comparing dynastic politicians who win by a small amount against non-dynastic politicians, and non-dynastic politicians who win by a small amount against dynastic politicians. However, the possibility that the voting threshold be not exogenous, as well as the presence of potentially relevant unobservables, undermine the reliability of its findings. DID exploits the whole sample, taking advantage of the spatial and temporal fixed effects. Yet, even supposing that the crucial common trends assumption holds, this empirical strategy implies that the treatment we wish to measure impacts on the outcomes of interest precisely from the moment when it kicks in. The mechanism by which dynastic politicians can influence development, expenditure or revenue outcomes is expected to pass through a change in the quality of the administration and in the bureaucratic environment induced by their presence. Such change may take time to realise. If this is the case, then the effect we wish to isolate is more likely to become manifest in the long run. Because the first free local elections in Indonesia took place in 2005 and our data are available until 2020, the true effects of a change in the environment induced by the advent of families of politicians might not have become manifest yet. As of now, however, we are generally unable to affirm that this phenomenon is harming social welfare in the island of Java.

References

[1] Aspinall E., M.U. As'ad, "Understanding family politics: Successes and failures of political dynasties in regional Indonesia", South-East Asia Research, 2016

[2] Cattaneo M., N. Idrobo, R. Titiunik, "A Practical Introduction to Regression Discontinuity Designs", published online by Cambridge University Press, 2019

[3] Fafchamps M., J. Labonne, "Family Networks and Distributive Politics", Journal of the European Economic Association, 2020

[4] Ferraz C., F. Finan, M. Martinez-Bravo, "Political Power, Elite Control, and Long-Run Development: Evidence from Brazil", Journal of the European Economic Association, 2024

[5] Kenawas Y., "The irony of Indonesia's democracy: The rise of dynastic politics in the post-Suharto era", Asian Journal of Comparative Politics, 2023

[6] Cruz C., J. Labonne, P. Querubin, "Politician Family Networks and Electoral Outcomes: Evidence from the Philippines", American Economic Review, 2017

[7] Marshall J., "Can Close Election Regression Discontinuity Designs Identify Effects of Winning Politician Characteristics?", American Journal of Political Science, 2022

Appendix

Table 1A

New name in the electoral data	Old name in the electoral data
Winning candidates	
Dadang M Naser	Dadang M. Naser
Obar Sobarna	Obar obarna
Abubakar	Abu Bakar
Hariyanti Sutrisno	Haryanti
Abdul Kholiq Arif	Abdul KholiQ Arif
Anna Sophanah	Anna Sophana
Dedi Mulyadi	Dedy Mulyadi
Mohammad Basyr Achmad	Moh. Basyir Achmad
Rachmat Yasin	Rahmat Yasin
Rudi Gunawan	Rudy Gunawan
Saiful Ilah	Saiful Illah
Seno Samudro	Seno Samodro
Uu Ruzanul Ulum	UU RUZANUL ULUM
Losing candidates	
Abu Almafakhir	Abu Almafachir
Badrul Kamal	Badrul kamal
Winning vice-candidates	
As'ad	As'at
Budiyono	Budiono
Sugeng Rismianto	Sugeng Rismiyanto
Sumarno	Sumarmo
Losing vice-candidates	
Suhardono	Suhartono
Sumardi	Sunardi
Suprapto	Suprapto HS

The list of individuals who are recorded with a different spelling within the electoral data.

Table 1B

Name in the electoral data	Name in the dynastic list
Winning candidates	
Abdullah Azwar Anas	Azwar Anas
Airin Rachmi Diany	Airin Rachmi Diani
Atty Suharti	Atty Suharti
Bambang Priyanto	Bambang Riyanto
Buchori	H. M. Buchori
Dadang M Naser	Dadang M Nasser
Hariyanti Sutrisno	Haryanti Sutrisno
Idza Priyanti	Idza Priyanto
Irianto MS Syafiuddin	Irianto Mahfudz Sidik Syaifudin
Irna Narulita	Irna Nuralita
Irvan Rivano Muchtar	Irvan Riano Muchtar
Iti Oktavia Jayabaya	Iti Octavia Jayabaya
Mohammad Makmun Ibnu Fuad	Makmun Ibnu Fuad
Muh. Samanhudi Anwar	Muhammad Samanhudi Anwar
Ratu Tatu Chasanah	Ratu Atut Chasanah
Tubagus Iman Ariadi	Tubagus Iman Ariyadi
Uche CH. Suganda	Utje Choeriyah Hamid Suganda
Yulianto	Yuliyanto
Losing candidates	
Abuya B. Kasrim	Abuya Busyro Karim
Raden Sri Hevyiana	Sri Heviyana Suparti
RM. Yudhi Sancoyo	RM Yadhi Sancoyo
Winning vice-candidates	
Achmad Alf Arslan Djunaid	Achmad Afzan Arslan Djunaid
Heryani	Heriyani
Muh. Haris	Muh Haris
Mukti Agung Widodo	Mukti Agung Wibowo
Nopes Nurayana	Noves Nurayana
Ratu Tatu Chasanah	Ratu Atut Chasanah
Tubagus Haeru Jaman	Tubagus Haerul Jaman
Losing vice-candidates	
Ach. Fadil Muzakki Syah	Ach Fadhil Muzakki Syah
Airin Rachmi Diany	Airin Rachmi Diani
Mohamad Hilmi	Mohammad Hilmi
Siti Sukesi	Siti Suksesi
Listed as fathers, without their own entry	
Aag Hamid Suganda	Aang Hamid Suganda
Fuad Amin Imran	Fuad Amin Imron
Tjep Tjep Muhtar Soleh	Tjetjecp Muhtar Soleh
Listed as spouses, without their own entry	
Aag Hamid Suganda	Aang Hamid Suganda
Achmad Dimyati Natakusumah	Achmad Dimyati Natakusuma
Amat Antono	Amat Hartono
Hendy Boedoro	Handy Boedoro
Sunarna	Sunama
Moch Itoc Tochija	Itoc Tochija
Listed as children, without their own entry	
Ady Setiawan	Aji Setiawan
Listed as siblings, without their own entry	
Fuad Amin Imran	Fuad Amin Imron
Sjahfazad Masdar	Sjahrazad Masdar
Listed as other relatives, witout their own entry	
Haryadi Suyuti	Haryadi Sayuti
Abd. Hamid Wahid	Abdurrahman Wahid

Some of the individuals who are recorded with a different spelling across the electoral data and the dynastic data.

Table 2

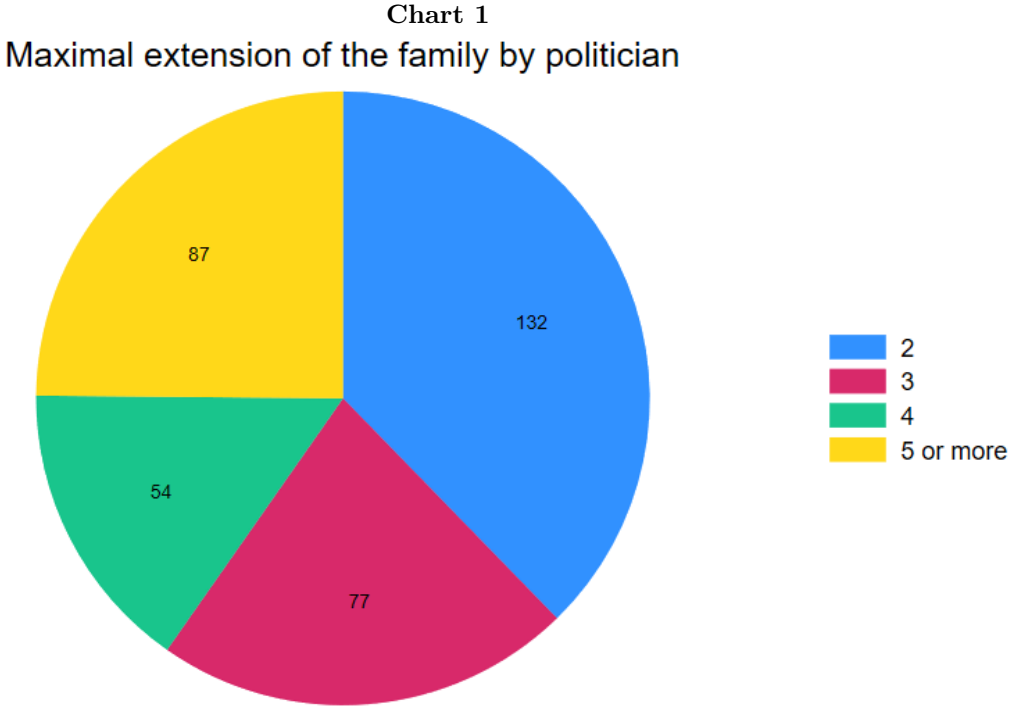
	(1)
	Mean
Father	0.186***
	(8.92)
Mother	0.0514***
	(4.35)
Spouse	0.346***
	(13.58)
Children	0.206***
	(9.51)
Siblings	0.271***
	(11.40)
N	350

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

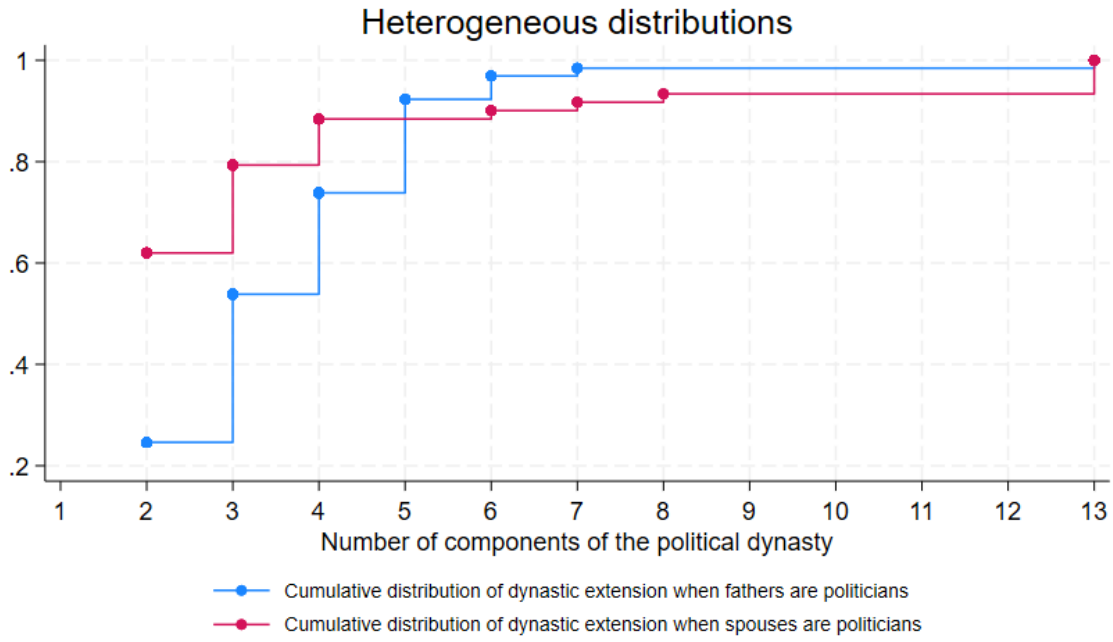
The percentage of individuals recorded as dynastic politicians, for whom the relative reported in the first column of the table is a politician.

Chart 1



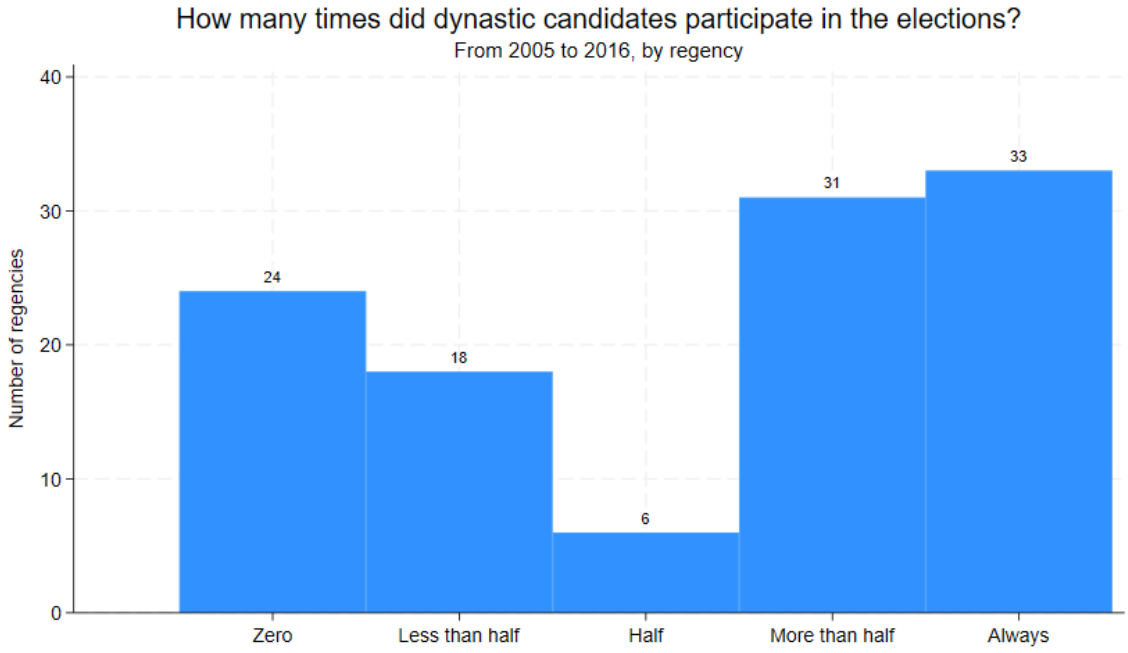
The distribution of the maximal extension of the dynasties.

Chart 2



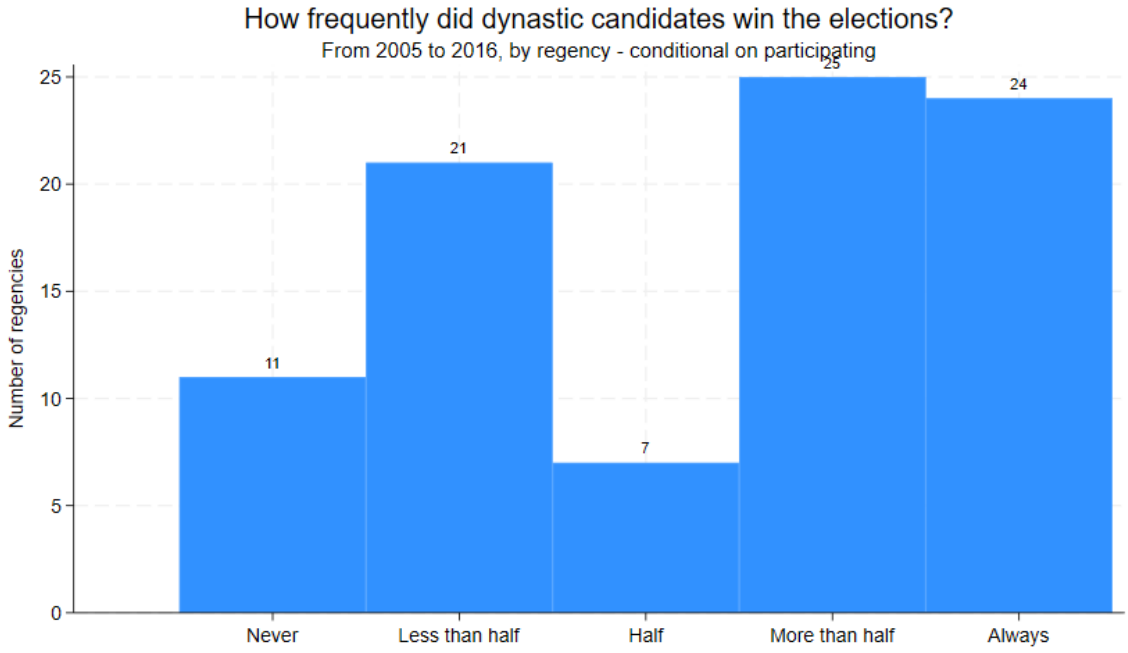
The cumulative distribution of dynastic extension, by relative in politics. Blue: the father is a politician. Red: the spouse is a politician.

Chart 3A



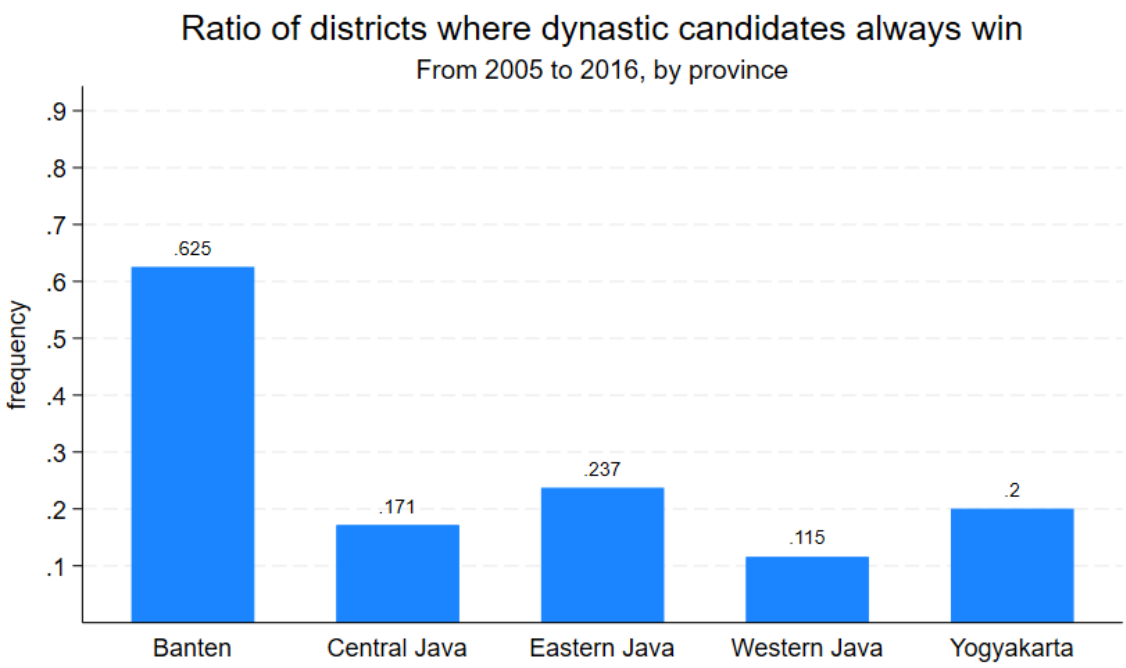
The distribution of regencies, by the number of times that a dynastic candidate participates in the local elections from 2005 to 2016.

Chart 3B



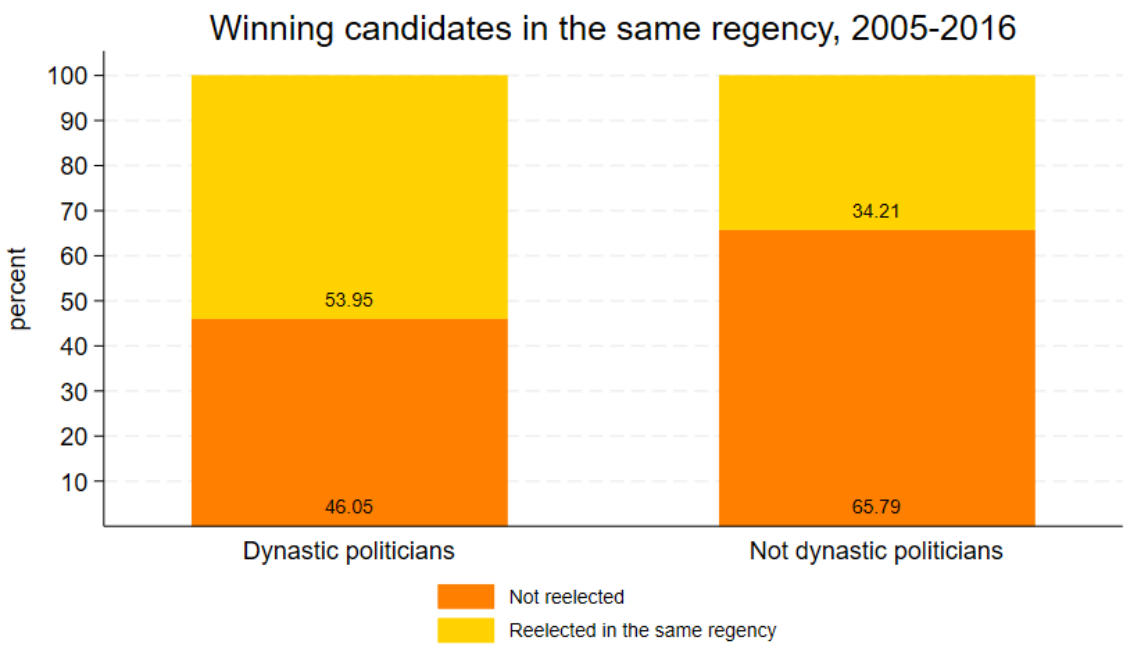
The distribution of regencies, by the number of times that a dynastic candidate wins in the local elections from 2005 to 2016, conditioning on participation in at least one local election.

Chart 4



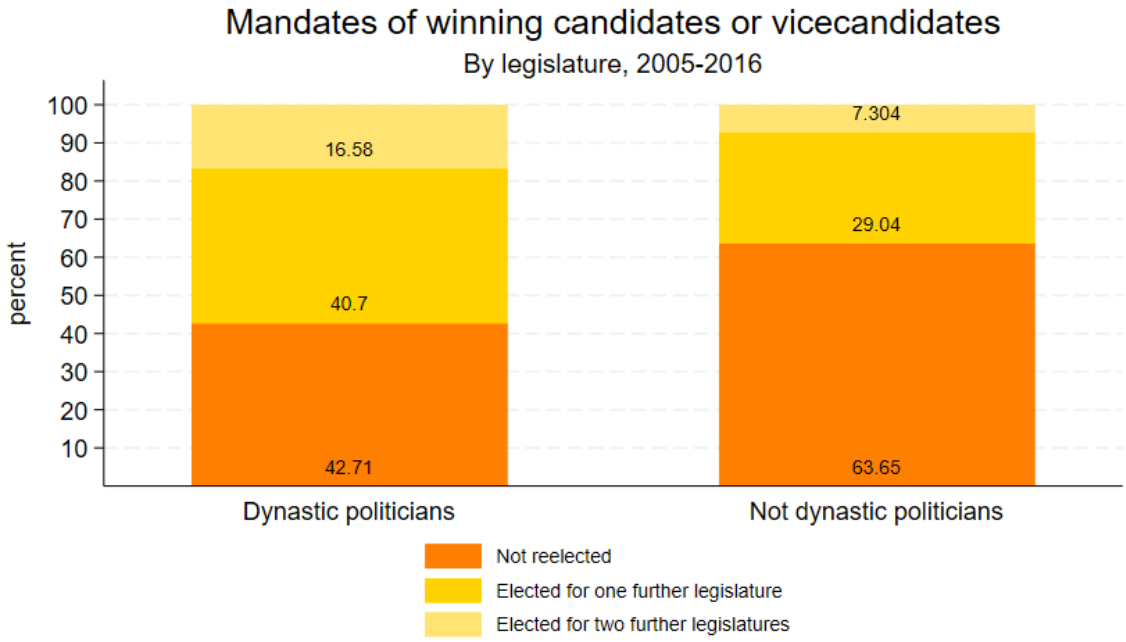
The distribution of regencies where dynastic candidates always win, by province in Java.

Chart 5A



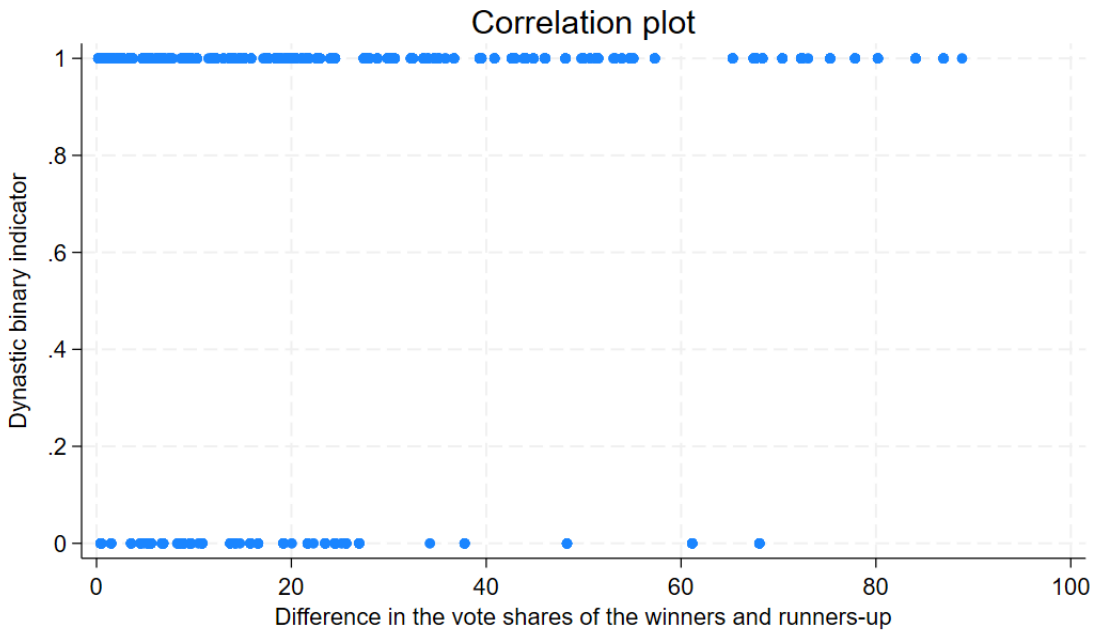
The shares of reelection in the same regency, for dynastic and non-dynastic politicians

Chart 5B



The shares of election in the same regency for a second term, or in another regency for a new term, for dynastic and non-dynastic politicians.

Chart 6



The positive correlation of the margin of victory and the dynastic indicator.

Table 3

Variable	(1) Control group	(2) treatment group	(3) Difference (billion IDR)	(4) Observations
total_exp	1,306.894	1,424.376	117.482	465
	(727.869)	(1,011.593)	(193.051)	
log_exp_cap	13.974	13.942	-0.032	418
	(0.603)	(0.563)	(0.156)	
educ_exp	414.714	465.324	50.610	341
	(241.745)	(313.488)	(67.702)	
log_educ_cap	12.762	12.882	0.120	328
	(0.521)	(0.570)	(0.152)	
ner_primary	96.161	95.369	-0.792*	482
	(2.590)	(2.790)	(0.385)	
ner_high_school	48.245	51.773	3.528	482
	(14.701)	(13.628)	(3.789)	
health_exp	133.576	125.456	-8.120	341
	(111.656)	(110.434)	(26.839)	
log_health_cap	11.512	11.503	-0.009	329
	(0.737)	(0.750)	(0.176)	
infra_exp	100.253	111.538	11.286	278
	(55.325)	(99.194)	(18.921)	
total_revenue	1,305.818	1,434.554	128.736	484
	(729.532)	(1,035.093)	(196.951)	
log_rev_cap	13.978	13.963	-0.015	437
	(0.615)	(0.539)	(0.160)	
gov_transfer	823.406	820.369	-3.037	476
	(391.731)	(495.676)	(97.475)	
log_gov_transfer_cap	13.575	13.471	-0.105	429
	(0.510)	(0.501)	(0.131)	
general_transfer	712.436	735.130	22.694	484
	(316.335)	(445.740)	(82.261)	
dau	712.439	735.360	22.921	484
	(316.352)	(446.115)	(82.267)	
log_dau_cap	13.450	13.379	-0.071	437
	(0.535)	(0.516)	(0.139)	
dak	99.601	79.397	-20.204	476
	(105.763)	(89.298)	(17.560)	
log_dak_cap	10.951	10.702	-0.248	427
	(0.896)	(0.991)	(0.230)	
total_tax	166.514	237.777	71.263	481
	(130.878)	(356.766)	(49.174)	
log_tax_cap	11.658	11.748	0.090	434
	(0.977)	(0.909)	(0.261)	
total_real_gdp	6,143.513	8,158.233	2,014.721	339
	(4,710.584)	(11,385.409)	(1,994.367)	
gdp_oil_gas	6,235.121	8,223.127	1,988.006	339
	(4,871.392)	(11,355.688)	(2,040.939)	
GDP_Agri	1,401.194	1,154.638	-246.556	339
	(765.823)	(971.697)	(260.344)	
GDP_mining	366.025	220.535	-145.490	336
	(500.533)	(781.884)	(187.907)	
lit_rate	89.206	93.066	3.860*	482
	(6.865)	(5.172)	(1.891)	
urban_rate	31.880	59.761	27.881***	105
	(14.548)	(27.968)	(6.712)	
pop_density	1,425.860	2,294.082	868.222	431
	(1,543.865)	(3,225.895)	(564.364)	
poverty_rate	16.571	13.041	-3.529	482
	(6.957)	(6.011)	(1.944)	
morbidity_rate	30.942	30.235	-0.707	482
	(6.847)	(7.117)	(1.468)	
hdi	69.588	71.215	1.627	484
	(5.115)	(4.204)	(1.335)	
gini	0.315	0.325	0.010	439
	(0.059)	(0.058)	(0.014)	
Corruption Amount in (Rp)	1.836	20.638	18.802*	484
	(6.884)	(150.185)	(8.047)	
Observations	66	418	484	

The balance table of relevant outcomes across treatment and control groups.

Table 4A

	(1) unemployment	(2) no_people_employed	(3) total_real_gdp	(4) hdi_new	(5) gini	(6) immunization	(7) lit_rate	(8) no_people_living_below
Conventional	-0.00749	12872.7	5369.3	10.22***	0.0230	4.938	5.691	-20946.5
	(0.0229)	(28422.9)	(8932.8)	(3.873)	(0.0190)	(5.044)	(4.887)	(33612.7)
Bias-corrected	-0.00911	15079.6	6082.5	11.77***	0.0219	6.113	6.072	-28609.4
	(0.0229)	(28422.9)	(8932.8)	(3.873)	(0.0190)	(5.044)	(4.887)	(33612.7)
Robust	-0.00911	15079.6	6082.5	11.77***	0.0219	6.113	6.072	-28609.4
	(0.0281)	(33925.6)	(10594.0)	(4.086)	(0.0214)	(5.893)	(6.092)	(40264.9)
N	709	709	828	566	770	679	803	827
cutoff	0	0	0	0	0	0	0	0
N_left	79	74	111	63	100	79	96	106
N_right	274	261	396	189	332	258	336	370
bwidth_left	16.05	15.08	21.98	13.93	19.27	16.25	17.75	20.20
bwidth_right	16.05	15.08	21.98	13.93	19.27	16.25	17.75	20.20
order	1	1	1	1	1	1	1	1
bwselect	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

RD on development outcomes. The treatment groups include all listed dynastic politicians who win against non-dynastic politicians.

Table 4B

	(1) total_exp	(2) log_exp_cap	(3) educ_exp	(4) log_educ_cap	(5) ner_primary	(6) health_exp
Conventional	-4.690	-0.0522	30.58	0.111	0.911	34.51
	(216.2)	(0.227)	(63.02)	(0.283)	(0.939)	(46.55)
Bias-corrected	50.30	-0.0683	19.93	0.0913	1.159	49.52
	(216.2)	(0.227)	(63.02)	(0.283)	(0.939)	(46.55)
Robust	50.30	-0.0683	19.93	0.0913	1.159	49.52
	(249.2)	(0.265)	(74.85)	(0.346)	(1.090)	(52.53)
N	779	771	723	721	824	723
cutoff	0	0	0	0	0	0
N_left	99	99	81	97	100	96
N_right	349	349	266	355	342	329
bwidth_left	19.32	19.29	14.40	22.92	18.47	19.61
bwidth_right	19.32	19.29	14.40	22.92	18.47	19.61
order	1	1	1	1	1	1
bwselect	mserd	mserd	mserd	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

RD on expenditure outcomes. The treatment groups include all listed dynastic politicians who win against non-dynastic politicians.

Table 4C

	(1) total_revenue	(2) log_rev_cap	(3) gov_trans	(4) log_gov_transfer_cap	(5) dak	(6) log_dak_cap	(7) dau	(8) log_dau_cap	(9) total_tax	(10) log_tax_cap
Conventional	-4.988	-0.0708	-16.16	-0.162	-8.546	-0.331	-15.51	-0.163	52.77	0.122
	(216.3)	(0.231)	(137.1)	(0.229)	(33.89)	(0.340)	(109.0)	(0.232)	(106.6)	(0.373)
Bias-corrected	57.31	-0.0915	-3.044	-0.178	-2.311	-0.316	-5.426	-0.192	31.51	0.184
	(216.3)	(0.231)	(137.1)	(0.229)	(33.89)	(0.340)	(109.0)	(0.232)	(106.6)	(0.373)
Robust	57.31	-0.0915	-3.044	-0.178	-2.311	-0.316	-5.426	-0.192	31.51	0.184
	(247.2)	(0.271)	(157.2)	(0.271)	(39.35)	(0.408)	(124.7)	(0.273)	(124.7)	(0.427)
N	803	795	792	783	792	781	803	794	800	792
cutoff	0	0	0	0	0	0	0	0	0	0
N_left	96	101	89	99	94	84	91	96	101	106
N_right	355	360	314	349	330	307	319	354	360	398
bwidth_left	19.09	19.54	16.01	19.36	17.95	15.65	16.26	19.08	19.47	23.42
bwidth_right	19.09	19.54	16.01	19.36	17.95	15.65	16.26	19.08	19.47	23.42
order	1	1	1	1	1	1	1	1	1	1
bwselect	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

RD on revenue outcomes. The treatment groups include all listed dynastic politicians who win against non-dynastic politicians.

Table 4D

	(1) adm_exp	(2) log_adm_exp	(3) corruption_amount	(4) audit
Conventional	-1.52905e+10	-0.331*	33.51	0.293
	(4.20750e+10)	(0.176)	(29.39)	(0.195)
Bias-corrected	-1.60858e+10	-0.444**	29.73	0.239
	(4.20750e+10)	(0.176)	(29.39)	(0.195)
Robust	-1.60858e+10	-0.444**	29.73	0.239
	(5.50929e+10)	(0.199)	(36.66)	(0.228)
N	281	281	483	483
cutoff	0	0	0	0
N_left	11	11	56	33
N_right	139	142	270	160
bwidth_left	18.75	19.68	26.41	10.11
bwidth_right	18.75	19.68	26.41	10.11
order	1	1	1	1
bwselect	mserd	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

RD on corruption outcomes. The treatment groups include all listed dynastic politicians who win against non-dynastic politicians.

Table 5A

	(1) unemployment	(2) no_people_employed	(3) total_real_gdp	(4) hdi_new	(5) gini	(6) immunization	(7) lit_rate	(8) no_people_living_below
Conventional	0.000880	27180.5	11036.8		0.0587**	8.029	15.42**	-91109.0*
	(0.0155)	(17613.9)	(12579.3)		(0.0262)	(6.019)	(7.007)	(53009.0)
Bias-corrected	0.00312	31347.6*	14207.4		0.0657**	9.584	17.55**	-107955.0**
	(0.0155)	(17613.9)	(12579.3)		(0.0262)	(6.019)	(7.007)	(53009.0)
Robust	0.00312	31347.6	14207.4		0.0657**	9.584	17.55**	-107955.0*
	(0.0161)	(20667.6)	(14107.3)		(0.0302)	(6.593)	(7.692)	(58184.3)
RD_Estimate				23.63***				
				(4.668)				
N	275	275	328	219	303	270	321	328
cutoff	0	0	0	0	0	0	0	0
N_left	29	29	43	9	33	27	28	31
N_right	89	89	142	40	107	90	108	106
bwidth_left	12.76	12.77	19.65	5.589	14.37	13.89	13.22	12.90
bwidth_right	12.76	12.77	19.65	5.589	14.37	13.89	13.22	12.90
order	1	1	1	1	1	1	1	1
bwselect	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Robustness check: RD on development outcomes. The treatment groups include all listed dynastic politicians who win against non-dynastic politicians. We use the outcomes of the two years before the start of a given legislature.

Table 5B

	(1) total_exp	(2) log_exp_cap	(3) educ_exp	(4) log_educ_cap	(5) ner_primary	(6) health_exp
Conventional	755.1***	0.650***	118.7	0.280	1.132	190.8***
	(243.7)	(0.229)	(98.51)	(0.210)	(1.163)	(62.09)
Bias-corrected	837.8***	0.721***	141.6	0.316	1.416	215.8***
	(243.7)	(0.229)	(98.51)	(0.210)	(1.163)	(62.09)
Robust	837.8***	0.721***	141.6	0.316	1.416	215.8***
	(272.4)	(0.277)	(114.2)	(0.252)	(1.287)	(69.84)
N	310	307	280	280	328	280
cutoff	0	0	0	0	0	0
N_left	29	34	24	22	49	24
N_right	110	120	94	82	158	94
bwidth_left	14.17	16.05	13.44	10.78	22.92	13.54
bwidth_right	14.17	16.05	13.44	10.78	22.92	13.54
order	1	1	1	1	1	1
bwselect	mserd	mserd	mserd	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Robustness check: RD on expenditure outcomes. The treatment groups include all listed dynastic politicians who win against non-dynastic politicians. We use the outcomes of the two years before the start of a given legislature.

Table 5C

	(1) total_revenue	(2) log_rev_cap	(3) gov_trans	(4) log_gov_transfer_cap	(5) dak	(6) log_dak_cap	(7) dau	(8) log_dau_cap	(9) total_tax	(10) log_tax_cap
Conventional	739.7***	0.662***	269.6**	0.548***	96.19***	0.388	130.0	0.411**	347.0***	1.501***
	(233.8)	(0.232)	(105.3)	(0.189)	(32.26)	(0.393)	(81.84)	(0.196)	(119.0)	(0.415)
Bias-corrected	824.4***	0.763***	284.5***	0.627***	99.27***	0.444	133.5	0.492**	375.5***	1.675***
	(233.8)	(0.232)	(105.3)	(0.189)	(32.26)	(0.393)	(81.84)	(0.196)	(119.0)	(0.415)
Robust	824.4***	0.763***	284.5**	0.627**	99.27***	0.444	133.5	0.492**	375.5***	1.675***
	(263.3)	(0.287)	(119.0)	(0.244)	(38.36)	(0.477)	(92.66)	(0.250)	(137.2)	(0.466)
N	320	316	315	311	315	309	320	316	319	315
cutoff	0	0	0	0	0	0	0	0	0	0
N_left	34	31	30	31	28	30	34	33	32	35
N_right	121	117	109	113	105	114	117	117	115	121
bwidth_left	15.38	14.67	13.96	14.32	13.63	14.70	15.07	14.90	14.55	15.84
bwidth_right	15.38	14.67	13.96	14.32	13.63	14.70	15.07	14.90	14.55	15.84
order	1	1	1	1	1	1	1	1	1	1
bwselect	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Robustness check: RD on revenue outcomes. The treatment groups include all listed dynastic politicians who win against non-dynastic politicians. We use the outcomes of the two years before the start of a given legislature.

Table 5D

	(1) adm_exp	(2) log_adm_exp	(3) corruption_amount	(4) audit
Conventional	4.99105e+10	0.529	1.471	0.525
	(3.24190e+10)	(0.417)	(1.138)	(0.338)
Bias-corrected	7.47404e+10**	0.710*	4.017***	0.636*
	(3.24190e+10)	(0.417)	(1.138)	(0.338)
Robust	7.47404e+10**	0.710	4.017	0.636
	(3.65693e+10)	(0.434)	(2.655)	(0.417)
N	127	127	177	176
cutoff	0	0	0	0
N_left	10	11	11	11
N_right	68	70	39	38
bwidth_left	23.58	25.08	7.809	7.990
bwidth_right	23.58	25.08	7.809	7.990
order	1	1	1	1
bwselect	mserd	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Robustness check: RD on corruption outcomes. The treatment groups include all listed dynastic politicians who win against non-dynastic politicians. We use the outcomes of the two years before the start of a given legislature.

Table 6A

	(1) unemployment	(2) no_people_employed	(3) total_real_gdp	(4) hdi_new	(5) gini	(6) immunization	(7) lit_rate	(8) no_people_living_below
Conventional	0.0502***	-43426.7*	8772.3	15.24***	0.0172	18.49***	17.06***	-81385.1**
	(0.0104)	(24336.6)	(10099.8)	(2.878)	(0.0232)	(3.693)	(5.121)	(39724.1)
Bias-corrected	0.0602***	-56462.9**	7783.3	17.68***	0.0127	22.38***	20.51***	-92343.3**
	(0.0104)	(24336.6)	(10099.8)	(2.878)	(0.0232)	(3.693)	(5.121)	(39724.1)
Robust	0.0602***	-56462.9*	7783.3	17.68***	0.0127	22.38***	20.51***	-92343.3***
	(0.0136)	(31818.2)	(9788.6)	(3.483)	(0.0225)	(1.804)	(3.221)	(33572.0)
N	557	557	642	468	607	511	619	641
cutoff	0	0	0	0	0	0	0	0
N_left	32	32	40	32	50	23	41	34
N_right	166	170	208	136	247	125	208	188
bwidth_left	9.764	10.16	10.69	10.01	15.98	8.717	11.36	9.672
bwidth_right	9.764	10.16	10.69	10.01	15.98	8.717	11.36	9.672
order	1	1	1	1	1	1	1	1
bwselect	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

RD on development outcomes, having only the politicians listed with their own row entry in the dynastic list.

Table 6B

	(1) total_exp	(2) log_exp_cap	(3) educ_exp	(4) log_educ_cap	(5) ner_primary	(6) health_exp
Conventional	-284.3	-0.255	-56.10	0.00714	0.0837	10.55
	(280.5)	(0.256)	(70.67)	(0.169)	(1.620)	(61.98)
Bias-corrected	-373.1	-0.348	-78.11	-0.0778	-0.284	4.544
	(280.5)	(0.256)	(70.67)	(0.169)	(1.620)	(61.98)
Robust	-373.1	-0.348	-78.11	-0.0778	-0.284	4.544
	(346.5)	(0.299)	(87.00)	(0.205)	(1.462)	(72.65)
N	608	601	550	548	639	550
cutoff	0	0	0	0	0	0
N_left	41	25	39	23	45	39
N_right	204	170	192	160	219	188
bwidth_left	10.90	8.972	11.82	9.024	12.20	11.77
bwidth_right	10.90	8.972	11.82	9.024	12.20	11.77
order	1	1	1	1	1	1
bwselect	mserd	mserd	mserd	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

RD on expenditure outcomes, having only the politicians listed with their own row entry in the dynastic list.

Table 6C

	(1) total_revenue	(2) log_rev_cap	(3) gov_trans	(4) log_gov_transfer_cap	(5) dak	(6) log_dak_cap	(7) dau	(8) log_dau_cap	(9) total_tax	(10) log_tax_cap
Conventional	-300.9	-0.250	-159.4	-0.244	-48.59	-0.812**	-116.0	-0.220	23.91	0.111
	(269.1)	(0.246)	(112.5)	(0.209)	(45.23)	(0.411)	(73.13)	(0.205)	(132.2)	(0.410)
Bias-corrected	-410.9	-0.353	-149.5	-0.335	-47.27	-0.969**	-118.4	-0.310	12.24	0.0478
	(269.1)	(0.246)	(112.5)	(0.209)	(45.23)	(0.411)	(73.13)	(0.205)	(132.2)	(0.410)
Robust	-410.9	-0.353	-149.5	-0.335	-47.27	-0.969**	-118.4	-0.310	12.24	0.0478
	(332.5)	(0.288)	(135.7)	(0.245)	(50.95)	(0.487)	(87.05)	(0.229)	(142.2)	(0.475)
N	618	611	611	603	611	601	618	610	618	611
cutoff	0	0	0	0	0	0	0	0	0	0
N_left	41	25	40	24	40	24	41	25	41	36
N_right	207	168	217	166	207	171	227	172	207	207
bwidth_left	11.32	8.763	12.26	8.764	11.71	8.955	13.21	9.075	10.95	10.55
bwidth_right	11.32	8.763	12.26	8.764	11.71	8.955	13.21	9.075	10.95	10.55
order	1	1	1	1	1	1	1	1	1	1
bwselect	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

RD on revenue outcomes, having only the politicians listed with their own row entry in the dynastic list.

Table 6D

	(1) log_adm_exp	(2) corruption_amount	(3) audit
Conventional	0.251	90.76	0.0207
	(0.363)	(85.05)	(0.314)
Bias-corrected	-0.283	98.52	-0.00619
	(0.363)	(85.05)	(0.314)
Robust	-0.283	98.52	-0.00619
	(0.179)	(101.7)	(0.325)
N	152	307	307
cutoff	0	0	0
N_left	11	24	18
N_right	98	125	78
bwidth_left	28.97	16.59	8.884
bwidth_right	28.97	16.59	8.884
order	1	1	1
bwselect	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

RD on corruption outcomes, having only the politicians listed with their own row entry in the dynastic list.

Table 7A

	(1) unemployment	(2) no_people_employed	(3) total_real_gdp	(4) hdi_new	(5) gini	(6) immunization	(7) lit_rate	(8) no_people_living_below
Conventional	-0.00375	36898.8	-6692.8	16.30***	0.0191	-3.213	1.157	65746.2**
	(0.0212)	(51256.1)	(19502.9)	(3.217)	(0.0209)	(2.120)	(2.670)	(28017.1)
Bias-corrected	-0.00219	46136.4	-28169.0	24.04***	0.00942	-6.015***	0.351	110705.6***
	(0.0212)	(51256.1)	(19502.9)	(3.217)	(0.0209)	(2.120)	(2.670)	(28017.1)
Robust	-0.00219	46136.4	-28169.0	24.04***	0.00942	-6.015**	0.351	110705.6***
	(0.0298)	(69035.2)	(25708.5)	(7.693)	(0.0332)	(2.976)	(3.717)	(39960.9)
N	443	443	506	370	478	401	483	505
cutoff	0	0	0	0	0	0	0	0
N_left	31	40	40	7	29	26	30	29
N_right	131	179	197	67	142	131	171	153
bwidth_left	11.08	17.07	14.78	6.835	10.78	11.61	12.95	10.63
bwidth_right	11.08	17.07	14.78	6.835	10.78	11.61	12.95	10.63
order	1	1	1	1	1	1	1	1
bwselect	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

RD on development outcomes, having only the dynastic politicians listed with their own row entry, who do not have the spouse as the only relative in politics.

Table 7B

	(1) total_exp	(2) log_exp_cap	(3) educ_exp	(4) log_educ_cap	(5) ner_primary	(6) health_exp
Conventional	-243.7	-0.244	28.75	0.490	3.417*	14.03
	(374.6)	(0.265)	(71.51)	(0.366)	(1.931)	(79.53)
Bias-corrected	-332.0	-0.188	131.4*	0.585	3.727*	-15.49
	(374.6)	(0.265)	(71.51)	(0.366)	(1.931)	(79.53)
Robust	-332.0	-0.188	131.4	0.585	3.727	-15.49
	(499.7)	(0.437)	(100.2)	(0.461)	(2.542)	(109.8)
N	473	466	417	415	503	417
cutoff	0	0	0	0	0	0
N_left	36	10	17	35	34	31
N_right	192	126	135	205	171	167
bwidth_left	15.03	9.283	10.38	19.50	12.91	14.50
bwidth_right	15.03	9.283	10.38	19.50	12.91	14.50
order	1	1	1	1	1	1
bwselect	mserd	mserd	mserd	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

RD on expenditure outcomes, having only the dynastic politicians listed with their own row entry, who do not have the spouse as the only relative in politics.

Table 7C

	(1) total_revenue	(2) log_rev_cap	(3) gov_trans	(4) log_gov_transfer_cap	(5) dak	(6) log_dak_cap	(7) dau	(8) log_dau_cap	(9) total_tax	(10) log_tax_cap
Conventional	-255.4	-0.241	2.980	-0.0562	41.29	0.296	-52.17	-0.144	-40.04	0.0696
	(399.9)	(0.257)	(204.9)	(0.261)	(63.34)	(0.490)	(155.4)	(0.238)	(163.7)	(0.441)
Bias-corrected	-343.4	-0.217	129.9	-0.250	62.91	0.253	-1.435	-0.383	-252.1	0.275
	(399.9)	(0.257)	(204.9)	(0.261)	(63.34)	(0.490)	(155.4)	(0.238)	(163.7)	(0.441)
Robust	-343.4	-0.217	129.9	-0.250	62.91	0.253	-1.435	-0.383	-252.1	0.275
	(551.0)	(0.416)	(309.5)	(0.429)	(98.88)	(0.696)	(214.3)	(0.374)	(230.9)	(0.704)
N	482	475	478	471	478	469	482	475	482	475
cutoff	0	0	0	0	0	0	0	0	0	0
N_left	30	15	30	25	30	30	30	30	36	10
N_right	175	139	169	152	169	151	170	153	201	119
bwidth_left	13.65	9.642	12.33	10.73	13.12	10.87	12.81	11.06	15.29	8.744
bwidth_right	13.65	9.642	12.33	10.73	13.12	10.87	12.81	11.06	15.29	8.744
order	1	1	1	1	1	1	1	1	1	1
bwselect	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

RD on revenue outcomes, having only the dynastic politicians listed with their own row entry, who do not have the spouse as the only relative in politics.

Table 7D

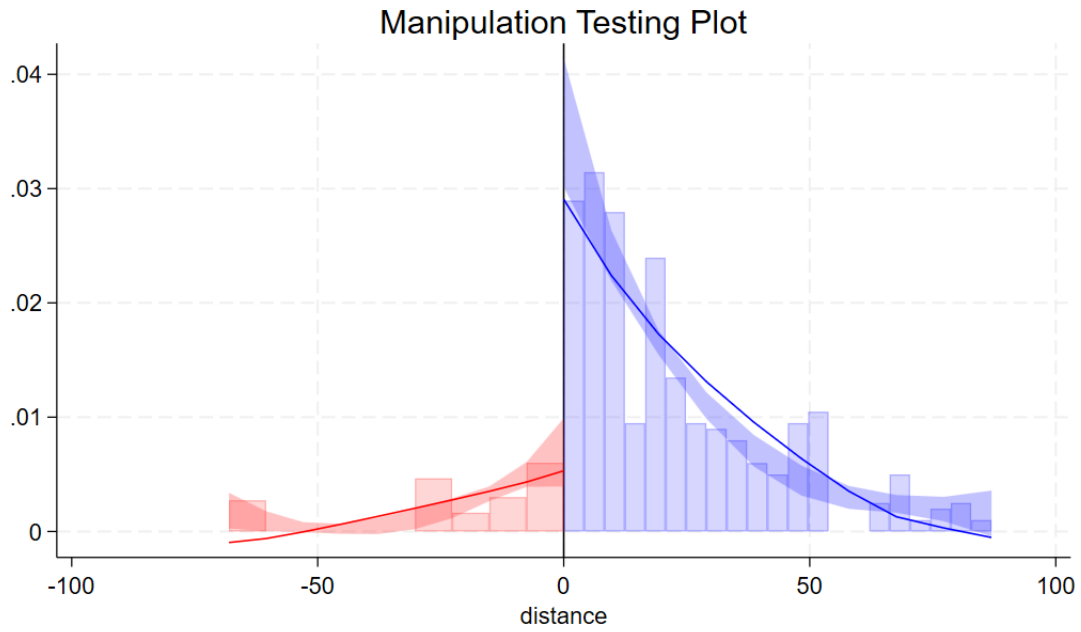
	(1) adm_exp	(2) log_adm_exp	(3) audit	(4) corruption_amount
Conventional	33.13	0.0543	-0.689**	-38.91**
	(30.45)	(0.188)	(0.311)	(16.67)
Bias-corrected	65.44**	-0.0119	-1.057***	-54.12***
	(30.45)	(0.188)	(0.311)	(16.67)
Robust	65.44**	-0.0119	-1.057*	-54.12*
	(28.39)	(0.228)	(0.569)	(27.63)
N	202	202	469	367
cutoff	0	0	0	0
N_left	15	15	12	9
N_right	128	128	120	93
bwidth_left	28.94	28.56	9.189	8.523
bwidth_right	28.94	28.56	9.189	8.523
order	1	1	1	1
bwselect	mserd	mserd	mserd	mserd
kernel	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

RD on corruption outcomes, having only the dynastic politicians listed with their own row entry, who do not have the spouse as the only relative in politics.

Chart 7



The density test on the running variable. Treatment group: dynastic candidates (all those listed) that win against non-dynastic. Control group: vice versa.

Table 8A

	(1) unemployment_rate	(2) no_people_employed	(3) total_real_gdp	(4) hdi_new	(5) gini	(6) immunization	(7) lit_rate	(8) no_people_living_below_poverty
treat	0.00269	8832.8	-0.726	1.211	-0.00174	1.526	3.107*	-6322.4
	(0.00911)	(15220.7)	(5.130)	(2.005)	(0.0119)	(1.636)	(1.683)	(14768.5)
vote_diff	-0.000260	886.4	-0.242*	-0.00411	0.0000794	0.0438	0.0442	-19.38
	(0.000390)	(594.3)	(0.144)	(0.0474)	(0.000422)	(0.0419)	(0.0511)	(476.8)
interaction	0.000158	-442.1	0.372	0.00205	-0.000145	-0.0869	-0.0749	56.08
	(0.000402)	(649.3)	(0.237)	(0.0598)	(0.000470)	(0.0573)	(0.0564)	(547.5)
tot_pop	9.95e-09***	0.408***	0.0000342***	-0.000000754	1.27e-09	-0.00000115***	0.000000819	0.0740***
	(2.55e-09)	(0.0107)	(0.00000822)	(0.000000683)	(5.12e-09)	(0.000000394)	(0.000000500)	(0.00804)
_cons	0.0575***	46735.3***	-7.202	69.43***	0.331***	96.80***	90.51***	51264.1***
	(0.00898)	(16294.9)	(8.996)	(1.786)	(0.0116)	(1.564)	(1.805)	(15708.6)
N	196	196	197	140	197	154	192	197

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

OLS on development outcomes. The treatment group is composed of all listed dynastic politicians who win against non-dynastic politicians.

Table 8B

	(1) total_exp	(2) log_exp_cap	(3) educ_exp	(4) log_educ_cap	(5) ner_primary	(6) health_exp
treat	-127.9	-0.239	35.68	0.0496	-0.781	-18.57
	(171.5)	(0.150)	(46.83)	(0.152)	(0.701)	(41.71)
vote_diff	-6.702	-0.00723*	0.569	0.00319	-0.0193	-1.148
	(4.983)	(0.00372)	(1.540)	(0.00405)	(0.0253)	(1.374)
interaction	9.257	0.00834*	-0.573	-0.00365	0.0243	0.843
	(5.824)	(0.00461)	(1.649)	(0.00487)	(0.0276)	(1.352)
tot_pop	0.00108***		0.000345***		8.20e-08	0.000158***
	(0.0000798)		(0.0000211)		(0.000000202)	(0.0000160)
_cons	561.7***	14.44***	118.1***	12.95***	96.71***	86.17**
	(152.7)	(0.134)	(39.75)	(0.141)	(0.698)	(38.98)
N	192	192	185	185	197	185

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

OLS on expenditure outcomes. The treatment group is composed of all listed dynastic politicians who win against non-dynastic politicians.

Table 8C

	(1) total_revenue	(2) log_rev_cap	(3) gov_transfer	(4) log_gov_transfer_cap	(5) dak	(6) log_dak_cap	(7) dau	(8) log_dau_cap	(9) total_tax	(10) log_tax_cap
treat	-111.7	-0.235	-104.8	-0.293**	-34.59	-0.531**	-68.33	-0.271**	-26.52	-0.252
	(170.5)	(0.151)	(84.93)	(0.140)	(27.71)	(0.266)	(61.75)	(0.136)	(73.26)	(0.256)
vote_diff	-7.554	-0.00787**	-3.501	-0.00847**	-1.047	-0.0156*	-2.408	-0.00771**	-2.482	-0.0103
	(5.004)	(0.00376)	(2.398)	(0.00337)	(0.849)	(0.00907)	(1.685)	(0.00309)	(1.816)	(0.00672)
interaction	10.09*	0.00888*	4.675*	0.00939**	1.404	0.0170*	3.224*	0.00857**	4.274	0.0111
	(5.830)	(0.00464)	(2.675)	(0.00429)	(0.865)	(0.00987)	(1.931)	(0.00402)	(3.112)	(0.00787)
tot_pop	0.00107***		0.000367***		0.0000681***		0.000298***		0.000380***	
	(0.0000839)		(0.0000402)		(0.00000969)		(0.0000338)		(0.0000787)	
_cons	580.3***	14.46***	578.7***	13.95***	91.38***	11.84***	485.2***	13.78***	-94.32	12.49***
	(151.7)	(0.137)	(77.67)	(0.128)	(23.73)	(0.228)	(59.38)	(0.125)	(92.14)	(0.224)
N	192	192	192	191	192	191	192	191	192	192

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

OLS on revenue outcomes. The treatment group is composed of all listed dynastic politicians who win against non-dynastic politicians.

Table 8D

	(1) adm_exp	(2) log_adm_exp	(3) corruption_amount	(4) audit_report
treat	2.03750e+10	-0.0208	18.61	0.287*
	(2.01722e+10)	(0.176)	(12.21)	(0.168)
vote_diff	522265029.8	-0.00106	0.0661	0.00548
	(446081809.5)	(0.00379)	(0.179)	(0.00343)
interaction	-784146953.8	0.00114	-0.315	-0.0123***
	(604710918.5)	(0.00475)	(0.275)	(0.00389)
tot_pop	144891.2***		0.0000175**	-4.37e-08
	(14966.4)		(0.00000768)	(5.17e-08)
_cons	7.40487e+10***	12.42***	-12.65	1.766***
	(2.09529e+10)	(0.149)	(8.006)	(0.148)
N	97	97	149	168

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

OLS on mismanagement outcomes. The treatment group is composed of all listed dynastic politicians who win against non-dynastic politicians.

Table 9

	(1) audit_report	(2) audit_report
treat	0.0618	0.195*
	(0.129)	(0.101)
vote_diff	-0.00654***	-0.00560**
	(0.00242)	(0.00261)
max_share_GDP_agri_sna	0.0938	
	(0.333)	
max_share_GDP_mining		2.687*
		(1.486)
_cons	1.840***	2.106***
	(0.140)	(0.0975)
N	140	118

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

OLS on the audit outcome. The treatment group is composed of all listed dynastic politicians who win against non-dynastic politicians.

Table 10A

	(1) unemployment	(2) no_people_employed	(3) total_real_gdp	(4) gini	(5) immunization	(6) lit_rate	(7) hdi_new	(8) no_people_living_below
ATET								
r1vs0.did_treat	-0.00414	4147.8	3.343**	-0.0133***	-0.224	-0.343	-0.0750	763.1
	(0.00258)	(4643.4)	(1.513)	(0.00488)	(0.406)	(0.294)	(0.0990)	(4116.5)
Controls								
tot_pop	-2.41e-08*	0.425***	0.0000239**	2.99e-08	-0.00000193	0.000000130	-0.000000329	0.0566*
	(1.41e-08)	(0.0306)	(0.0000101)	(1.86e-08)	(0.00000127)	(0.000000585)	(0.000000723)	(0.0324)
1.time_var	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
2.time_var	-0.00166	2751.4	1.672***	-0.00175	0.0307	0.414***	0.621***	-8892.7***
	(0.00160)	(3243.3)	(0.319)	(0.00377)	(0.262)	(0.138)	(0.0319)	(2033.1)
3.time_var	0.000902	-1436.9	0.366	0.0215***	-0.220	1.012***	1.306***	-10339.8***
	(0.00222)	(4598.8)	(0.695)	(0.00502)	(0.351)	(0.261)	(0.0775)	(3109.2)
4.time_var	-0.00533*	9869.1*	2.346***	0.0217***	0.109	1.359***	1.920***	-14001.1***
	(0.00276)	(5521.2)	(0.583)	(0.00476)	(0.291)	(0.301)	(0.0817)	(3593.4)
5.time_var	-0.00725***	8464.6*	3.541***	0.0219***	-0.643	1.767***	2.492***	-20005.6***
	(0.00251)	(5087.7)	(0.649)	(0.00438)	(0.399)	(0.294)	(0.0865)	(3258.0)
6.time_var	-0.0121***	6801.1	5.359***	0.0342***	-0.582	2.118***	3.118***	-29104.6***
	(0.00274)	(4789.4)	(0.984)	(0.00482)	(0.446)	(0.307)	(0.0936)	(3764.5)
7.time_var	-0.0162***	13946.3***	7.197***	0.0365***	-0.211	2.423***	3.719***	-37973.4***
	(0.00280)	(5289.5)	(1.379)	(0.00557)	(0.441)	(0.296)	(0.104)	(4435.9)
_cons	0.107***	33746.1	0.934	0.279***	98.64***	91.92***	68.17***	88202.1**
	(0.0169)	(35185.7)	(12.04)	(0.0218)	(1.464)	(0.695)	(0.863)	(38454.4)
N	984	984	1156	1073	949	1124	785	1155

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

DID on development outcomes. Treatment: legislatures where dynastic politicians get elected.

Table 10B

	(1) total_exp	(2) log_exp_cap	(3) educ_exp	(4) log_educ_cap	(5) ner_primary	(6) health_exp
ATET						
r1vs0.did_treat	18.71	-0.0816**	81.31*	0.0970	0.290	38.51**
	(62.77)	(0.0365)	(41.01)	(0.185)	(0.498)	(16.59)
Controls						
tot_pop	0.00147***		0.000589***		-0.00000121	0.000246***
	(0.000350)		(0.000176)		(0.00000139)	(0.0000822)
1.time_var	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)
2.time_var	160.2***	0.0684***	70.56***	0.174**	0.163	43.95***
	(36.93)	(0.0232)	(19.71)	(0.0775)	(0.207)	(8.382)
3.time_var	226.7***	0.212***	-51.93	-0.119	0.511	12.54
	(36.82)	(0.0308)	(31.61)	(0.150)	(0.447)	(10.75)
4.time_var	406.5***	0.360***	106.2***	0.416***	0.658	71.03***
	(37.25)	(0.0279)	(25.29)	(0.135)	(0.470)	(9.817)
5.time_var	534.0***	0.452***	108.5**	0.266	1.335***	93.46***
	(42.41)	(0.0274)	(44.94)	(0.169)	(0.475)	(15.20)
6.time_var	732.6***	0.619***	153.1***	0.435***	1.376***	115.2***
	(49.88)	(0.0297)	(37.79)	(0.163)	(0.445)	(15.87)
7.time_var	906.6***	0.695***	248.5***	0.716***	1.742***	164.3***
	(76.63)	(0.0351)	(35.19)	(0.154)	(0.463)	(17.61)
_cons	-522.7	13.87***	-302.5	12.61***	96.31***	-169.6*
	(405.5)	(0.0122)	(210.8)	(0.0479)	(1.620)	(96.09)
N	1089	1078	1003	1001	1152	1003

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

DID on expenditure outcomes. Treatment: legislatures where dynastic politicians get elected.

Table 10C

	(1) total_revenue	(2) log_rev_cap	(3) gov_trans	(4) log_gov_transfer_cap	(5) dak	(6) log_dak_cap	(7) dau	(8) log_dau_cap	(9) total_tax	(10) log_tax_cap
ATET										
r1vs0.did_treat	60.72	-0.0338	-9.215	-0.0311	13.25	0.0959	-20.37	-0.0349	52.77	-0.0525
	(61.71)	(0.0222)	(25.80)	(0.0199)	(15.37)	(0.106)	(17.88)	(0.0234)	(40.50)	(0.0610)
Controls										
tot_pop	0.00135***		0.000273***		0.0000896***		0.000183***		0.000672**	
	(0.000365)		(0.0000584)		(0.0000210)		(0.0000615)		(0.000275)	
1.time_var	0	0	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
2.time_var	189.6***	0.113***	64.35***	0.0825***	11.25**	0.113*	51.67***	0.0726***	54.14***	0.210***
	(33.17)	(0.0115)	(6.250)	(0.00947)	(4.287)	(0.0599)	(5.004)	(0.00950)	(9.501)	(0.0234)
3.time_var	206.1***	0.182***	71.36***	0.107***	11.06	0.147**	57.68***	0.0850***	37.75*	0.333***
	(33.68)	(0.0191)	(21.49)	(0.0211)	(10.62)	(0.0682)	(16.40)	(0.0225)	(21.03)	(0.0448)
4.time_var	416.0***	0.348***	207.2***	0.261***	63.33***	0.588***	143.2***	0.211***	86.94***	0.566***
	(35.79)	(0.0174)	(19.13)	(0.0171)	(11.44)	(0.0859)	(15.71)	(0.0235)	(21.58)	(0.0507)
5.time_var	548.8***	0.450***	256.8***	0.339***	70.32***	0.681***	187.2***	0.287***	130.0***	0.740***
	(42.26)	(0.0193)	(20.55)	(0.0171)	(12.13)	(0.0962)	(15.47)	(0.0191)	(23.02)	(0.0511)
6.time_var	697.8***	0.562***	342.1***	0.431***	102.2***	0.883***	239.3***	0.360***	157.1***	0.900***
	(46.35)	(0.0183)	(23.69)	(0.0178)	(13.39)	(0.0870)	(16.84)	(0.0200)	(26.19)	(0.0487)
7.time_var	923.7***	0.679***	445.5***	0.518***	114.0***	1.024***	331.0***	0.439***	209.2***	1.092***
	(79.69)	(0.0260)	(43.73)	(0.0234)	(13.99)	(0.103)	(39.96)	(0.0257)	(31.90)	(0.0483)
_cons	-404.2	13.86***	381.9***	13.42***	-40.32*	10.68***	418.2***	13.32***	-598.9*	11.60***
	(422.4)	(0.00834)	(63.94)	(0.00995)	(24.00)	(0.0580)	(69.83)	(0.0108)	(327.2)	(0.0137)
N	1123	1111	1107	1094	1107	1090	1123	1110	1119	1107

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

DID on revenue outcomes. Treatment: legislatures where dynastic politicians get elected.

Table 10D

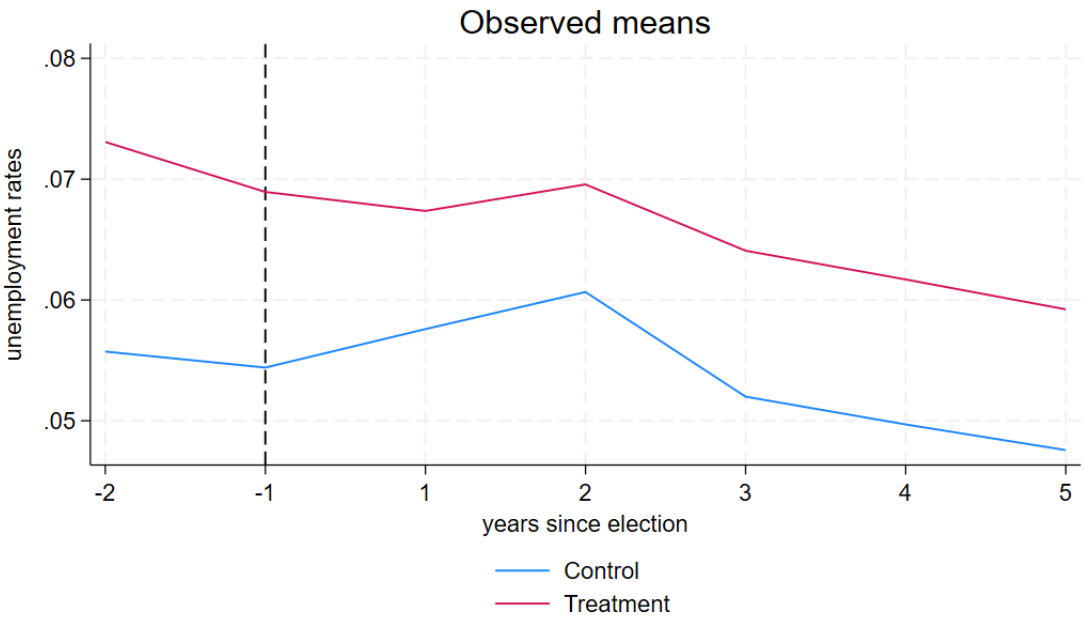
	(1) adm_exp	(2) log_adm_exp	(3) corruption_amount	(4) audit
ATET				
r1vs0.did_treat	-8.78692e+09	-0.162	11.44	-0.0449
	(1.69504e+10)	(0.117)	(11.38)	(0.202)
Controls				
tot_pop	154339.4*		0.0000514	5.73e-08
	(84835.1)		(0.0000434)	(0.000000196)
1.time_var	0	0	0	0
	(.)	(.)	(.)	(.)
2.time_var	3.07714e+10*	0.0470	-2.591	0.0308
	(1.79172e+10)	(0.0781)	(15.86)	(0.0665)
3.time_var	3.59950e+10**	0.209**	-7.020	-0.362**
	(1.54698e+10)	(0.0889)	(14.36)	(0.166)
4.time_var	6.16630e+10***	0.311***	-15.09	-0.249
	(1.72470e+10)	(0.0939)	(13.39)	(0.183)
5.time_var	7.53950e+10***	0.403***	-4.523	-0.405***
	(1.63652e+10)	(0.0904)	(10.31)	(0.149)
6.time_var	1.03346e+11***	0.546***	25.80	-0.468***
	(1.69160e+10)	(0.0869)	(19.46)	(0.145)
7.time_var	1.22724e+11***	0.595***	-0.204	-0.540***
	(2.09342e+10)	(0.104)	(15.25)	(0.154)
_cons	1.55879e+10	12.16***	-48.81	2.138***
	(9.80225e+10)	(0.0381)	(46.43)	(0.231)
N	531	531	881	1103

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

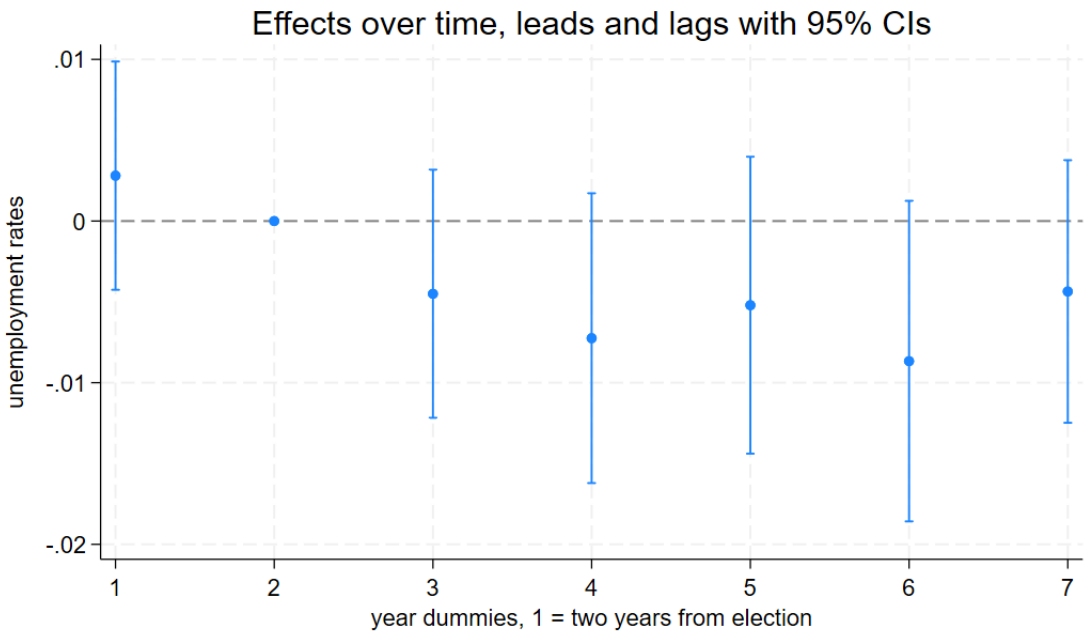
DID on corruption outcomes. Treatment: legislatures where dynastic politicians get elected.

Chart 8A



The trend chart of unemployment variable across treatment groups.

Chart 8B



The time-specific effects on unemployment along the legislature. Baseline is the year before the election.

Table 11

	(1) unemployment	(2) no_people_employed	(3) total_real_gdp	(4) gini	(5) immunization	(6) lit_rate	(7) hdi_new	(8) no_people_living_below
ATET								
r1vs0.did_treat	0.00178	4196.8	2.694	0.00119	-0.00618	-0.429	0.0940	8920.9
	(0.00298)	(5544.4)	(2.797)	(0.00346)	(0.314)	(0.289)	(0.146)	(5800.5)
Controls								
tot_pop	-1.78e-08*	0.454***	0.0000405***	-9.65e-09	-0.00000161***	-0.000000404	-0.000000567	0.0405***
	(1.04e-08)	(0.0248)	(0.00000835)	(6.71e-09)	(0.000000550)	(0.000000736)	(0.000000517)	(0.0145)
2007.year	0	0	1.968***	-0.0416***	0.975**	1.137***		890.0
	(.)	(.)	(0.685)	(0.00523)	(0.444)	(0.239)		(1985.9)
2008.year	-0.00655***	-7514.6	2.808**		0.659	1.572***		-12936.2***
	(0.00138)	(4860.0)	(1.109)		(0.446)	(0.251)		(3859.2)
2009.year	-0.0129***	-782.5	3.658***	-0.0178***	1.625***	1.784***		-30829.1***
	(0.00193)	(4663.0)	(1.226)	(0.00462)	(0.434)	(0.252)		(4937.6)
2010.year	-0.0208***	-7536.6	4.699***	-0.0124**	1.714***	2.318***	0	-38965.2***
	(0.00203)	(6483.5)	(1.353)	(0.00474)	(0.467)	(0.267)	(.)	(4482.6)
2011.year	-0.0271***	-6564.6	5.635***	0.0368***	1.325**	2.476***	0.728***	-42484.8***
	(0.00290)	(7044.5)	(1.509)	(0.00530)	(0.509)	(0.312)	(0.0266)	(4616.2)
2012.year	-0.0308***	8684.9	6.618***	0.0506***	1.961***	2.829***	1.448***	-51531.9***
	(0.00278)	(5995.5)	(1.667)	(0.00567)	(0.549)	(0.299)	(0.0489)	(4912.7)
2013.year	-0.0298***	4892.6	7.815***	0.0442***	2.106***	3.861***	2.182***	-53716.4***
	(0.00321)	(5886.8)	(1.874)	(0.00547)	(0.538)	(0.338)	(0.0654)	(5171.2)
2014.year	-0.0324***	3944.1	8.978***	0.0495***	2.285***	4.784***	2.658***	-59324.7***
	(0.00294)	(5522.4)	(2.027)	(0.00500)	(0.559)	(0.427)	(0.0677)	(5330.5)
2015.year	-0.0327***	-9079.1	10.09***	0.0720***	-0.0673	4.950***	3.370***	-57657.4***
	(0.00308)	(5956.5)	(2.239)	(0.00518)	(0.526)	(0.376)	(0.0738)	(5418.5)
2017.year	-0.0380***	11721.4**	12.94***	0.0572***	0.0697	5.198***	4.532***	-64745.5***
	(0.00308)	(5526.3)	(2.594)	(0.00549)	(0.513)	(0.394)	(0.0893)	(5702.4)
2018.year	-0.0384***	14546.7**	14.56***	0.0576***		5.543***	5.153***	-78559.5***
	(0.00331)	(5703.0)	(2.880)	(0.00583)		(0.432)	(0.0986)	(6208.4)
2019.year	-0.0458***	25026.6***	16.25***	0.0457***		5.630***	5.799***	-84522.3***
	(0.00349)	(6338.0)	(3.105)	(0.00581)		(0.416)	(0.108)	(6517.7)
2020.year	-0.0291***	20767.7***	14.38***	0.0503***		6.700***		-74748.9***
	(0.00313)	(6509.7)	(3.039)	(0.00542)		(0.438)		(6382.5)
2005.year			0	0	0	0		0
			(.)	(.)	(.)	(.)		(.)
2006.year			1.377**	-0.0232***	1.886***	0.762***		15520.6***
			(0.574)	(0.00434)	(0.477)	(0.202)		(2061.6)
2016.year			11.49***	0.0605***	0.280	5.030***	3.963***	-62326.9***
			(2.407)	(0.00514)	(0.534)	(0.395)	(0.0800)	(5589.8)
_cons	0.113***	7901.0	-25.84***	0.308***	97.04***	89.87***	67.10***	140176.9***
	(0.0119)	(30127.9)	(9.811)	(0.00841)	(0.681)	(0.850)	(0.592)	(16794.3)
N	1450	1450	1781	1670	1443	1752	1119	1776

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

DID on development outcomes. Treatment starts at the moment when dynastic politicians are first elected, in the districts where this happens.