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The Democratic Challenges of Electoral Representation and Populism: an Empirical Approach

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Abstract

This dissertation aims at improving our understanding of two important phenomena in contemporary democracies: imbalanced electoral representation and the rise of populism. To do so, it explores empirically several natural experiments in different countries and settings.

Elections, which are key to the functioning of contemporary democracies, play a dual role of *selection* and of *accountability*. Nonetheless, despite the generalization of universal suffrage, the representation of different categories of population in the political arena is imbalanced. This comes in part from voters preferences, but also from institutions shaping the incentives of politicians before entering in politics and while in office. In turn, the difficulty of defining what the people is makes populism itself a complex phenomenon, whose causes deserve careful investigation.

The introduction presents the main concepts at the heart of this thesis (namely electoral representation and populism), and summarizes the contributions of political economy on these topics. The first and second chapters of this thesis aim at investigating the topic of electoral representation, through the lens of political dynasties in Italian municipalities and of gender-biases in French local elections, while the third chapter deals with one potential explanation for the rise of populism: prejudices against migrants.

The first chapter explores the consequences of electing a dynastic politician on subsequent public policies. Using a rich dataset of politicians' characteristics in Italy between 1985 and 2012, and identifying dynasties through common surnames of politicians in a single municipality, we document the importance of dynasties in Italian municipalities (which is estimated to represent about 15% of mayors) and show that dynastic candidates are on average younger, less experienced, more likely to be elected and more career-concerned. Most importantly, using panel fixed-effects regressions and a regression discontinuity design on close elections

between dynastic and non-dynastic candidates, we show that while dynastic politicians do not have different expenditures or revenues on average, they run larger Political Budget Cycles. Indeed, they disproportionately increase capital expenditures in pre-electoral years and receive greater transfers from upper layers of government. These deviations are especially pronounced when dynastic mayors face a binding term-limit, and when their margin of election is small. However, using several indicators of municipality performance, we do not find that municipalities ran by dynastic mayors perform differently than others. These findings suggest that dynastic mayors are more likely to be strategic in their use of public budget, and are compatible with two hypotheses: dynastic politicians might indeed have a higher ability of signaling their competence, or greater incentives to remain in office.

The second chapter of this thesis focuses on gender-discrimination from voters in politics. It exploits a natural experiment in the French *départementales* elections of 2015, where for the first time in the history of French elections, candidates had to run by gender-balanced pairs. This arguably confused some voters, who were used to voting for a single candidate and a substitute, and who might have assumed that the first listed candidate was the main one. Using the fact that the order of appearance of the candidates on a ballot is determined by alphabetical order and showing that this rule does not seem to have been used strategically by parties, we argue that the position of female candidates on the ballot is as good-as random. Exploiting this feature, we show that right-wing ballots where the female candidate is listed first receive on average 1.5 percentage points lower shares of vote (a difference of about 4% to 5%), and are 4 percentage points less likely to go to the second round or win the election (a difference of about 5% to 6%). We then use the fact that candidates can report additional information about themselves on the ballot to test for the presence of statistical discrimination. Using a sample of about 12% of the ballots, we show that about 35% of pairs of candidates reported information about themselves on the ballot, and that the gender-discrimination we identified is likely to be statistical: indeed, the effect is driven by ballots on which candidates reported no information at all. We finally show that this discrimination increased the vote shares of political opponents and is correlated with unexplained wage gaps on the labor market.

The third chapter studies the link between migration inflows and the rise of populism. Us-

ing as a natural experiment the dismantling of the Calais migrant camp in France between October 2015 and October 2016, we explore whether municipalities in which temporary centers were opened during this period had different trends of votes in favor of the far-right party *Front National* between the presidential elections of 2012 and 2017. Instrumenting the presence of a temporary migrant center by the number of beds available in holiday villages, which were often used to shelter Calais migrants, we find that the growth of Front National vote shares between 2012 and 2017 was lower by 15.7 percentage points in municipalities which received Calais migrants. This difference suggests that those municipalities had a growth rate of Front National vote corresponding to 25% of the average growth rate between 2012 and 2017 (corresponding to a difference of 4 percentage points in terms of vote shares). It also shows evidence of spillover effects on neighbouring areas, as municipalities located within a 5 km radius around a temporary camp also had a lower growth rate of Front National vote. Overall, this effect is stronger in places with more immigrants and a younger population, and is weaker in municipalities whose mayors publicly volunteered to welcome migrants. This effect is also driven by municipalities which received a small amount of migrants: above a threshold of 39 beds per 1,000 inhabitants, the effect is reversed, and the votes in favor of the Front National increase. Finally, we show that this effect is unlikely to be driven by variation in local economic activity. Hence, while the average effect is negative, and points toward the contact hypothesis, these results are also in line with a vast literature showing that large migration inflows increase far-right votes.

In the conclusion, I explore pathways for a future research agenda aiming at extending and deepening the findings of this thesis.

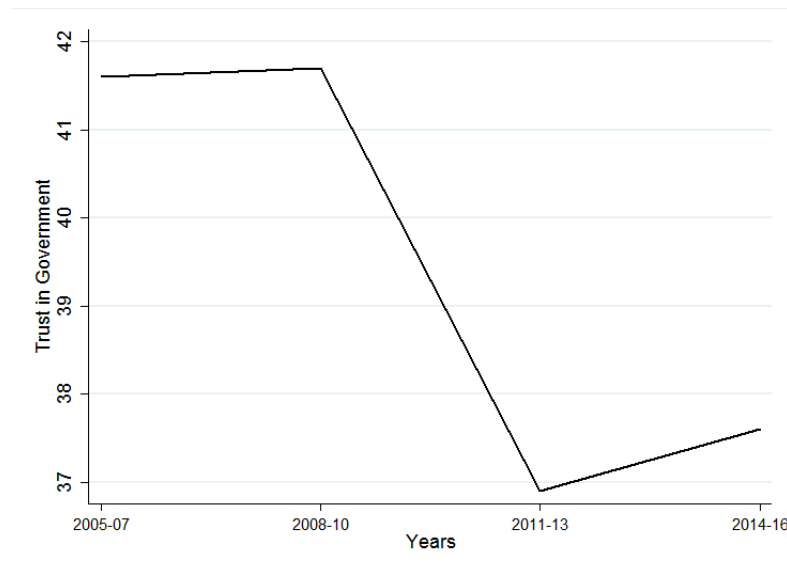
Introduction

Are western democracies tired? Recent years have increasingly seen distrust of politicians and of the democratic process gaining the minds of western citizens. In the OECD, during the last decade, trust in government has been steadily decreasing: while about 42% of OECD voters declared to trust their government in 2005, this share has declined to about 37% in 2016 (Figure 1).

In France, as of the beginning of 2017, 70% of the citizens considered that “democracy is not functioning correctly” and 67% thought that “most of the elected officials only care about the wealthy and the powerful” (CEVIPOF (2017)). At the same time, turnout is decreasing in every type of election and radical political orientations seem to be on the rise in various western democracies. In some cases the consequences are already sizable, with a shift to inward-looking policies illustrated by the election of Donald Trump or the British decision to withdraw from the European Union. In other cases such as France, extreme political orientations are still weakly represented among elected officials, but are increasingly popular in the voting booth. Importantly, the growing discontent of citizens regarding their politicians and their attraction towards populism seems related to low levels of well-being and high levels of pessimism: in the case of France, voters who chose to cast a ballot for Marine Le Pen (the candidate of the *Front National*, the main far-right party) in April 2017 were disproportionately more pessimistic regarding their future perspectives, regardless their socio-economic background (Figure 2)

All in all, contemporary democracies seem to be subject to a form of “intimate adversity” (Gauchet (2007)), characterized by two phenomena often deemed as interwoven: a crisis of representation and a rise of populist movements. This thesis aims at refining our understanding of these phenomena using the tools of political economy, in the European context. These

Figure 1: Trust in Government in OECD countries (2005-2016)

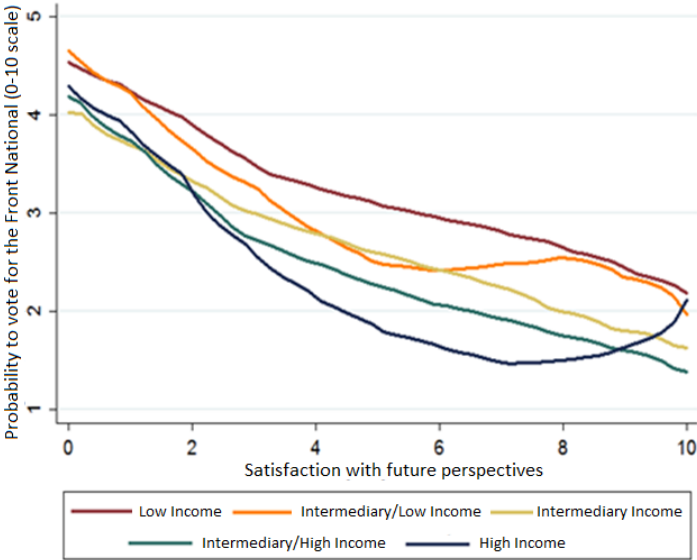


Source: OECD (2017)

topics have been at the heart of philosophical investigations for many centuries. Yet the tools offered by political economy have considerably changed our understanding of politics and the type of questions we can try to answer, both because of the development of powerful methodological and empirical tools, and the emergence of a large quantity of high-quality data.

In this introduction, I first present a brief overview of the way philosophy, history and political economy dealt with the issue of electoral representation. I then analyze the difficulty of defining the concepts of people and of populism, before reviewing the main acknowledged causes of populism. Based on this analysis, I then present the chapters of this thesis, as well as their respective contributions to the literature. Each chapter represents an independent essay. The first two chapters address two important aspects of electoral representation in contemporary representative democracies: the persistence of family dynasties in the political arena, and the lack of representation of women in office. More specifically, the first chapter examines the role of political dynasties in Italian municipalities over the period 1985-2012, while the second chapter studies how gender biases from French voters in local elections in 2015 shaped electoral outcomes. Finally, the third chapter analyzes to what extent the recent migrants crisis can explain the raise of populist votes in France during the 2017 presidential election.

Figure 2: Satisfaction with future perspectives and vote intention for the *Front National* in the 2017 presidential election



Source: Algan, Beasley, et al. (2017)

1 Elections: accountability and selection

1.1 What role do elections play?

The democratic ideal of governance for and by the people has often been seen as more than a simple political system. For Jean-Jacques Rousseau, in democracy, "each of us puts in common her personality and her omnipotence under the supreme direction of the general will; and we receive as a body each member as an indivisible part of the whole" (Rousseau (1762)). Put differently, democracy can be defined as a "a form of society" in which "the place of the power is an empty place" (Lefort (1986)), the archetype of it being the Athenian democracy, where randomly chosen citizens exerted the power directly in a deliberative way. Yet, as acknowledged by Rousseau, such an ideal of direct democracy is hard to implement: "If there were a people of gods, it would govern itself democratically. Such a perfect government is not suited to men". (Rousseau (1762))

Elections have been thought as a way of abiding by these democratic principles in a feasible way, through the concept of representation which entails two dimensions. First, through elections, the people delegates to other individuals the right to make decisions on its behalf. Secondly, elections enable the people to control these decisions, through the possibility of vot-

ing politicians out. As we will argue later, and to put it in the words of contemporary political economy, elections are simultaneously a *selection* device and an *accountability* device.

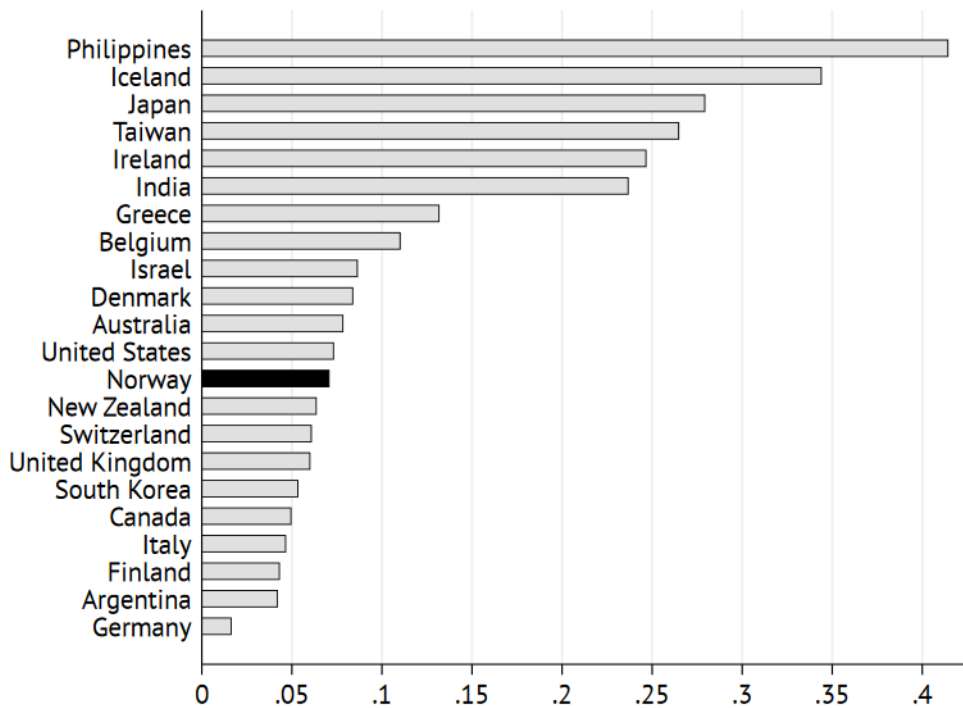
The representative system can therefore be defined as "a procuration given to a certain number of men by the mass of the people, which wants its interests to be defended, and which nonetheless does not always have the time to defend it by itself" (Constant (1819)). In such a framework, the State becomes a "fictitious person" governing in the name of the people and having power on it (Hobbes (1651), Jaume (1983)). Put differently, the governing body is an actor playing the play written by the people (Allonnes (2016)). Democracy is no more the empty circle of the Greek agora, but a pyramid where the top governs the bottom .

But while most modern societies are now governed under the rules of indirect democracy, the debates about the democratic nature of representative systems is as old as this concept. On the one hand, adversaries of representative systems considered elections as features of an aristocratic system, fundamentally opposed to the democratic ideal of governance (Aristotle (n.d.), Rousseau (1762)), and argued that elections fail at preventing the capture of power by experts, making the citizens unable to really exert the power or voice their concerns (Arendt (1995), Castoriadis (1997)). On the other hand, advocates of indirect democracy argued that representatives were more competent to govern than the people (Montesquieu (1867)). Sieyès, one of the founding fathers of the first French constitution, argued that France "must not be a democracy, but a representative regime", since "the vast plurality of our fellow citizens has neither enough education nor enough leisure to get directly involved in making the laws which should govern France; thus, they must limit themselves to naming their representatives" (Sieyès (1789)). At the crossroads of these debates, the convergence of a representative system towards the democratic ideal is likely to depend on its ability to select competent politicians representing the preferences of the people, and to incentivize politicians to implement policies in line with these preferences.

1.2 Who do elections represent ?

While most of western democracies now use the universal suffrage - in which all individuals above a certain age have the right to vote - this has not always been the case. In France, for

Figure 3: Estimated shares of dynastic MPs



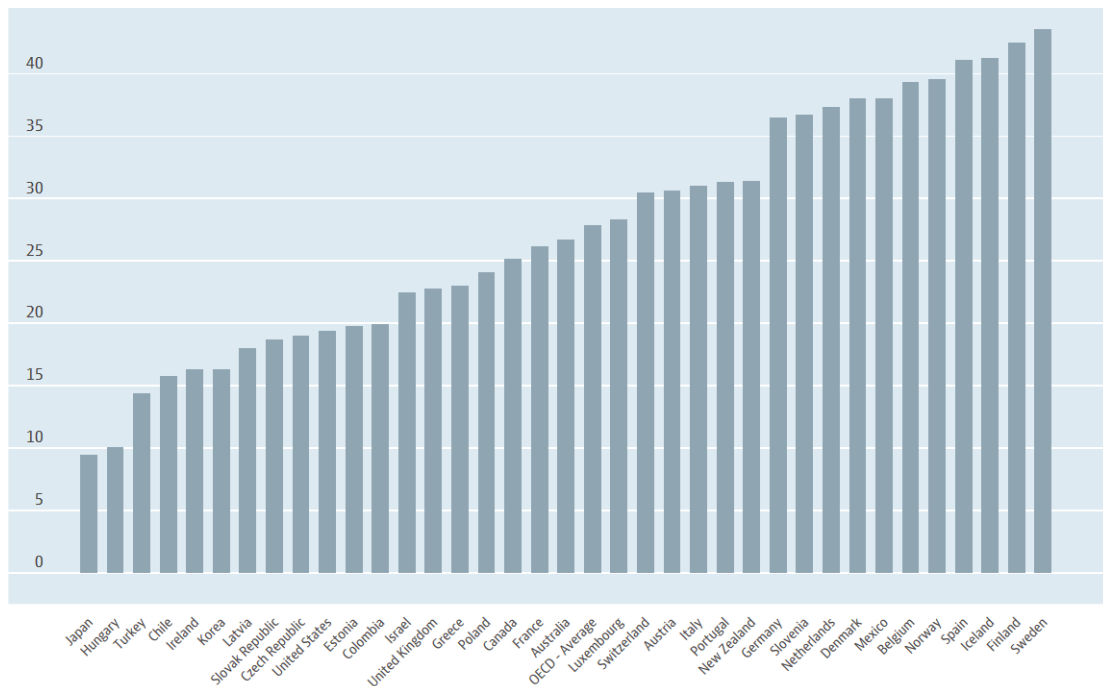
Source: Fiva and Smith (2016)

example, before 1848, only male citizens earning revenues above a certain threshold (between 200 and 300 francs over the period 1815-1848, for example) had the right to vote. After 1848, during the French Second Republic, the income restriction was abolished, but not the gender restriction: every male citizens above 21 had the right to vote. Women had to wait until 1944 to have the right to vote in France.

From this point of view, it is particularly striking that even in well-established democracies, where the transmission of power is no longer hereditary and is based on universal suffrage, the share of dynastic members in parliament is substantial. In a recent study, Fiva and Smith (2016) surveyed the most up-to-date estimations of this phenomenon, which is quantitatively significant in many countries - though with important variations (Figure 3). Conversely, despite the generalization of universal suffrage, women are still far from being equally represented in politics (Figure 4).

These facts show that the imbalanced representation of certain categories of population might not come only from direct laws preventing them from voting. From this point of view, the situation of Afro-Americans in the United States, as recently summarized by Temin (2017),

Figure 4: Share of women parliamentarians in 2014 in OECD countries



Source: OECD (2014))

is enlightening. Despite the end of slavery in 1865 in the United States, Afro-American citizens were systematically less likely to be represented in elections, not only because of laws enforcing segregation (such as the Jim Crow laws between 1876 and 1965), but also because of laws disproportionately increasing their probability of ending up in jail: as a result, a black male has one chance over three to serve jail during his lifetime, and the felony disenfranchisement reduces the relative representation of the Afro-American population in the voting booth.

All in all, understanding the effectiveness of representative democracies implies to understand in which conditions elections are likely to truly represent the preferences of the people. This is precisely what political economy attempts to do.

1.3 The contributions of Political Economy

Early contributions

The first important step towards an understanding of electoral representation was made by Condorcet (1785), who noticed that in a majoritarian ballot, preferences might be non-transitive. Condorcet partially solved this paradox by noticing that an unambiguous winner of an election is a candidate who beats every other competitor in pairwise competition. Yet, while under

specific conditions the aggregation of individual decisions can be thought as giving an unambiguous leader, this is not true in the general case. Indeed, as shown by the "impossibility theorem" of Arrow (1951), there does not exist a single procedure of aggregation which satisfies four basic conditions (universality, absence of dictatorship, unanimity and independence of irrelevant options). Namely, the only way of gathering universality, unanimity and independence of irrelevant options is to be in a dictatorship. But while it is in general impossible to find a unique rule that satisfies all these basic conditions, under specific assumptions, preferences might unambiguously be aggregated in a representative system. Much of the subsequent developments in Political Economy aimed at understanding what these specific conditions are.

Early approaches, coming from the Virginia school and the theory of Public Choice, tried to model to what extent elections were likely to represent people's choice, seeing it mainly as a competition for votes based on policy platforms. In an influential model, Downs (1957) argued that if parties care only about winning and can credibly commit to their electoral promises, under specific conditions, their policy platforms converge to the preferences of the median voter (which, if preferences are single-peaked on a unidimensional voting decision, are also a Condorcet winner (Black (1948))). But while this contribution is seminal, it was hardly verified in the data, and later contributions suggested that, if politicians cannot commit fully to implement their policy platforms (in particular if they have partisan preferences), then we should observe policy divergence (Alesina (1988)).

But this early literature failed at analyzing two fundamental roles of the elections, which were formalized in later contributions: the role of incentives and the role of selection. These roles acknowledge that politics is fundamentally a problem of asymmetry of information, between principals (the citizens) and agents (the politicians) (Besley (2006)).

Elections as an accountability device The important role of elections in providing incentives was first acknowledged by Barro (1973) and Ferejohn (1986), who used economic modeling to understand how elections can control politicians. In particular Barro (1973) argues that politicians and voters are likely to pursue different objectives which the institutional setting might reconcile, while Ferejohn (1986) shows that performance in office of the incumbent (rather than electoral platforms) is likely to affect his reelection prospects. In such models,

elections play the role of incentive mechanisms, preventing the moral hazard which might affect politicians once they are elected. In other words, elections represent an *accountability* device. Importantly, the extent to which elections are likely to discipline politicians in office depends on specific institutional settings: in their seminal book, Persson and Tabellini (2002) show theoretically how political regimes, electoral rules or district size could affect both electoral outcomes and implemented policies.

Elections as a selection mechanism Furthermore, most early models focused on policies rather than politicians. Yet, as acknowledged by Besley (2006), "in a representative democracy, it is politicians who are elected and are charged with making policy". Seminal contributions addressing this topic are the citizen-candidate models by Osborne and Slivinski (1996) and Besley and Coate (1997), where voters select politicians from a pool of candidates who endogenously decide to enter into the political arena. Put differently, and as confirmed empirically in the case of American elections by Lee, Moretti, and Butler (2004), voters "elect" policies rather than they "affect" them. Recent theoretical contributions therefore focused on how elections could help selecting "good" politicians, taking into account the fact that voters might not only value their policy stances but attributes such as competence and honesty (Caselli and Morelli (2004)). Besley (2005) summarized the different ways in which institutions could affect the quality of elected politicians. This includes the relative attractiveness of politics compared to other types of occupations, the relative probability of election of good and bad candidates, their respective outside options and their respective probability of reelection. Other potential channels also include threats that some "nasty" groups can impose on politicians, which lowers the quality of candidates (Dal Bó and Di Tella (2003)).

Apart from the quality of the politician is also the question of how representative of the socio-economic composition of the society a politician should be. Such a class of models indeed predicts that if different categories of individuals value different types of policies, their representation eventually affects the types of policies implemented. However, as acknowledged by Caselli and Morelli (2004), less competent individuals also have greater incentives to become candidates, because they have lower outside options. Therefore, as argued by Dal Bó et al. (2017), if competence is correlated with certain characteristics of individuals, valuing

competence might be done at the expense of representativeness.

1.4 Causes and consequences of differential representation within the population

If, as coined by Latour (2006), researchers do not see the world with the naked eye but with "the dressed eye", with the emergence of high-quality data, the eyes of political economists have become better dressed and equipped to evaluate their theoretical findings.

Politicians' characteristics matter

A first important set of findings has highlighted the fact the identity of elected officials matters. Following the seminal contribution Akerlof and Kranton (2000), identity can be thought of as "a person's sense of self", which is related to her social category and self-image. Since then, identity has been found to play an important role in education (Akerlof and Kranton (2002)) or in forms of organizations (Akerlof and Kranton (2005)). But it also affects crucially the types of public policies implemented by politicians.

In a seminal study, Jones and Olken (2005) showed that economic fluctuations of country GDPs crucially depend not only on the type of political regimes but also on who is in office. Extending their work, Besley, Montalvo, and Reynal-Querol (2011) showed that more educated leaders are more likely to promote growth in their country. At an infra-country level, many studies also showed that the gender of political leaders affected the type of policies implemented: in India, female leaders promote policies which are more salient to female citizens (Chattopadhyay and Duflo (2004)), while in Brazil, women are less likely to be subject to corruption (Brollo and Troiano (2016)). Similarly, larger representations of disadvantaged minorities is likely to increase the amount of transfers they receive (Pande (2003)).

Relatedly, several contributions showed that raising the presence of certain categories of population is unlikely to be done at the expense of competence. For example, gender quotas in Sweden (Besley et al. (2017)), in Italy (Baltrunaite et al. (2014)) have been found to increase the quality of politicians, notably because women are on average more educated than men, and because such quotas helped replacing low-quality male politicians by high-quality female

politicians.¹ Furthermore, evidence from Spain (Casas-Arce and Saiz (2015)) show that parties in which women were the most-underrepresented gained from the introduction of gender-quotas (which helped them solve agency problems).

Evidence on imbalanced electoral representation: perpetuation of power and discrimination

Since the identity of politicians matter, it is therefore important to understand why all categories of population are not equally represented in office. Several mechanisms can explain this phenomenon. Here, I briefly summarize two particular mechanisms which are specifically related to the topics under scrutiny in this thesis: the perpetuation of power among the elite, and discrimination against certain categories of population.

Perpetuation of power The topic of political representation in contemporary democracies cannot be fully understood without exploring the role of elites and families. The first chapter of this thesis aims at shedding new light on these elements, by combining the literature on elite perpetuation with elements from the literatures on rent-seeking behaviors.

Political Dynasties While it has been acknowledged for a long time that elites tend to seek the perpetuation of their power (Pareto (1901), Michels (1915), Mosca (1939)), economic modelling has recently highlighted the mechanisms underlying the capture of power by the elites (Robinson and Acemoglu (2008), Besley and Reynal-Querol (2017)). Similarly, increasing data availability enabled researchers to quantify the magnitude of elite perpetuation in democracies, and to investigate the causes of such a phenomenon. In a seminal contribution Dal Bó, Dal Bó, and Snyder (2009), estimated the share of dynastic congressmen in the US to be around 7%. Since this contribution, several studies quantified the share of dynastic politicians (Figure 3), thus considerably improving our understanding of how families manage to maintain their power. As surveyed by Geys and Smith (2017), dynastic candidates seem to benefit from a large electoral advantage, and candidates to elections are likely to be part of very central families

1. Such findings are in line with more general evidence of an absence of trade-off between representation and competence in Sweden: Dal Bó et al. (2017) show that in this country, while the socioeconomic background of Swedish politicians is representative of the population, they are on average more intelligent and more competent than the average citizen.

in local networks (Cruz, Labonne, and Querubin (2017)). Furthermore, elected candidates are more likely to have a relative in office in subsequent elections (with some exceptions, such as Fiva and Smith (2016)).

However, the extent to which this high prevalence of dynastic politicians is due to their policy-making is still unclear. In fact, little is known about the consequences of dynastic politicians, and contributions on this topic are very recent. In terms of political consequences, Geys (2017) shows that political dynasties induce the selection of less educated politicians. But evidence is still divergent on the policy-making side: while some contributions argue that dynastic politicians have lower performance in office (Asako et al. (2015)), others find that dynastic mayors are likely to have higher expenditures (Bragança, Ferraz, and Rios (2015)). In the first chapter, we investigate whether municipalities ran by dynastic mayors in Italy differ on various indicators of performance, and test whether dynastic mayors are more likely to be strategic in the way they use their budget, for reelection purposes. To do so, we more specifically engage with two streams of literature which explored rent-seeking behaviors: the literature on term-limits and the one on political budget cycles.

Term-limits If elections are a disciplinary device for politicians, term-limited incumbents should not behave in office as those who are not term-limited. On this topic, numerous contributions have emphasized how the incentives of reelection prospects affect the policies implemented by incumbents. The most seminal contribution is by Besley and Case (1995), who showed that term-limited Democrat governors in the United States reduced taxes if they could be reelected. As shown by Alt, Bueno de Mesquita, and Rose (2011) who exploit heterogeneous term-limits in the United States, this effect is a mix of competence and selection. Similarly, mayors with re-election incentives are much less likely to engage in corruption in Brazil (Ferraz and Finan (2011)).²

2. Such findings are part of a broader stream of literature emphasizing how the conditions upon which politicians reach power affect both their selection and the policies they implement. In particular, the relative gains and losses of politicians once in office seem to affect both the pool of candidates running for office and the policies they implement: while higher wages seem to attract more educated politicians and induces them to run their budget more efficiently (Ferraz and Finan (2009), Gagliarducci and Nannicini (2013)), violence against politicians seem to deter educated candidates (Daniele (2015)). At the same time, if politicians can simultaneously have a job and a political mandate, high-quality politicians are also more likely to shirk (Gagliarducci, Nannicini, and Naticchioni (2010)). Coherent with the crucial hypothesis of asymmetry of information between citizens and politicians, the final choice of voters has been found to be largely affected by the information that media provide

Political Budget Cycles A second important stream of research related to rent-seeking behavior is linked to Political Budget Cycles. A seminal theoretical contribution from Rogoff (1990) showed that different types of politicians might have different incentives to signal their (unobserved) competence to citizens right before elections, notably by increasing visible expenditures and decreasing tax rates at the end of a term. Since then, many empirical and theoretical investigations have tried to identify upon which conditions - both in terms of institutions, politicians' characteristics and voters' characteristics - such behaviors are likely to occur. In the original model of Rogoff (1990), it is essentially explained by the fact that the voters cannot perfectly observe the competence of a politician (for example because it is hard for them to be aware of the deficit).

In fact, as summarized by Drazen (2008), at the macroeconomic level, empirical findings suggest that political budget cycles vary substantially depending on electoral systems and forms of government (Persson and Tabellini (2005)). They are also lower in democracies, where the share of informed voters is likely to be higher (Shi and Svensson (2006)). Brender and Drazen (2005) argue that political budget cycles in democracies are a phenomenon which exists essentially in newly-established ones. Relatedly, during the period following the democratic transition in Russia, Akhmedov and Zhuravskaya (2004) found evidence of "opportunistic political cycles" before regional elections, which disappeared over time and were lower in areas where transparency was higher. The magnitude of budget cycles is therefore likely to depend on the salience of budgetary issues in the political debate (Alesina and Paradisi (2017)). However, political budget cycles might not only take the form of different levels of expenditures and revenues. Indeed, the composition of spendings is also likely to be affected, especially if voters punish deficits (Drazen and Eslava (2010)) This hypothesis seems warranted since, even though political budget cycles exist, they do not seem to systematically influence electoral outcomes (Brender and Drazen (2008)). Finally, recent contributions have shown that political budget cycles can be largely affected by the characteristics of the politicians: in particular, young politicians are much more likely to engage into political budget cycles, mostly for career concerns (Alesina, Troiano, and Cassidy (2015)). As we will show in

about the type of the politician (Ferraz and Finan (2008), Snyder Jr and Strömberg (2010)), and about the ability that the electoral system has to represent voters' choices (Fujiwara (2015)).

the first chapter of this thesis, this is also the case of dynastic politicians - especially if they do not face a binding term-limit.

Discrimination mechanisms Finally, among the different mechanisms that can explain why certain categories of population are more represented in office than others, discrimination from voters is a widely discussed hypothesis. However, uncovering this form discrimination and understanding its determinants is challenging. Indeed, numerous selection effects are at stake before voters actually cast their ballot. Let us consider the particular case of women in politics, which is central to the second chapter of this thesis. Numerous studies found that women are less likely to engage in competitive environments. This can be due to differential preferences for cooperation (Kuhn and Villeval (2015)), which are likely to be deeply rooted in the human norms of cooperation and conflict (Seabright (2012), Harari and Perkins (2014)), but also to career discontinuities associated with childbirth (Bertrand, Goldin, and Katz (2010)). Furthermore, parties often tend to field women in hard-to-win districts (Thomas and Bodet (2013), Esteve-Volart and Bagues (2012), Casas-Arce and Saiz (2015)). These selection effects suggest that in the end, female politicians are likely to differ from male politicians both on observable characteristics (as they are often found to be more educated Baltrunaite et al. (2014)), and on unobservable characteristics. Therefore, simply comparing the aggregate scores of male and female candidates from different categories of population is unlikely to yield any causal estimation of discrimination.

Furthermore, while many innovative methods have been used to measure discrimination on the labor market, doing so in the field of politics is much more complicated, since the final decision of voters in the voting booth is not observed. Uncovering discrimination mechanisms in the real world therefore implies to find natural experiments which circumvent such issues. From this point of view, the second chapter of this thesis is related to a recent stream of literature using field experiments in politics to highlight discrimination against certain types of candidates, whether it is statistical³ (as in the case of gender-discrimination in India (Beaman et al. (2009)), or in Italy (De Paola, Scoppa, and Lombardo (2010))) or taste-based⁴ (as recently

3. Stemming from imperfect information and stereotypes (Arrow et al. (1973), Phelps (1972))

4. Coming from preferences (Becker (1957))

found in the case of discrimination against Afro-American candidates in the United States (Broockman and Soltas (2017))).

2 Understanding populism

The third chapter of this thesis aims at understanding the recent rise of populism in France, by exploring whether the relocation of Calais migrants in France had an impact on the votes in favour of the *Front National* (the main far-right party) during the 2017 presidential elections. It is therefore tied to a vast literature trying to define populism and its causes.

2.1 Defining populism

The previous section highlighted the difficulty of defining the concept of people, whose preferences are supposed to be represented by elections. According to Rosanvallon (2000), who studied the case of France in a historical perspective, the very definition of the people is itself problematic. According to him, in France, the concept of people was progressively adopted between the Revolution and the beginning of the Second Republic, in the midst of all the intellectual debates that the Revolution created. An "equilibrium democracy" emerged, which tried to cope with the inherent problem of representation in indirect democracy, through the creation of parties, trade unions and the emergence of public statistics, helping to shape commonly acknowledged categories (Desrosières (2016)). Yet, according to him, since the 1970s, voters' decisions are becoming more volatile and "the people" cannot be considered as a single entity which citizens feel unambiguously members of. It rather seems to be fragmented into different sub-units, whether we refer to the people as an opinion, as an opposition to the elites, or as a sample of emotions visible through the media.

Acknowledging the difficulty of defining what "the people" is can help explaining why populism is so hard to define - both intrinsically and in its relationship with democracy. As acknowledged by Panizza (2005), the definition of populism is intrinsically loose, and can be seen as a "mode of identification available to any political actor in a discursive field", which opposes the "sovereign people" to "the other", where "the 'other' [...] can be presented in political or economic terms or as a combination of both, signifying 'the oligarchy', 'the

politicians', a dominant ethnic or religious group, the 'Washington insiders', 'the plutocracy' or any other group that prevents the people from achieving plenitude".

It is therefore not surprising that this concept gives room to numerous interpretations, notably with respect to how populism relates to democracy. While Panizza (2005) argues that populism is rather a "mirror of democracy" than its enemy, others such as Rosanvallon (2014) define it as a "pathology of electoral-representative democracy, and even more, as a pathology of counter-democracy". Furthermore, while general definitions of populism can apply to numerous parts of the ideological spectrum, it is often associated in Europe to far-right movements, especially since the beginning of the 2000s and the electoral outbreak of the *Front National* in France, or of Pym Fortuyn in The Netherlands.

In fact, in France, the *Front National* can be coined as a "national-populist" party, since the seminal work of Taguieff (1984). As recalled by Winock (1997), such a concept is part of a broader distinction between "protest populism" and "identity populism". The first opposes "those at the top" and "those at the bottom", and is typical of movements such as *Boulangisme* (at the end of the XIXth century), which includes "anti-elitism (those at the top/those at the bottom), trust in the people, defiance against the representative regime (antiparliamentarism), appeal to the people by referendum, hyper-personalization of the movement through the charismatic figure of a "viril" and "honest" leader, interclassist discourse of national unity". The second opposes "those from here" to "those from there". According to Winock (1997), such an antagonism is typical of the antisemitic movement of Edouard Drumont and Jules Guerin at the end of the XIXth century, who claimed to protect the people against the Jews (who were seen as "monopolizing" resources), which especially thrived during episodes such as the Panama scandal or the Dreyfus case. According to Winock (1997), many political movements that emerged in France since the 1930s combined both these aspects of populist ideologies: this includes the French far-right leagues of the 1930s (strongly against the parliamentary system, often openly xenophobic, and inspired by fascist movements in other countries such as Italy, Germany or Portugal), the *poujadiste* movement of the 1950s (which combined strong anti-elite feelings, defence of French Algeria, and latent antisemitism, notably against Pierre Mendès-France). In this light, the *Front National*, created by Jean-Marie Le Pen, who previ-

ously belonged to the *poujadiste* movement, can be seen as an example of this synthesis, which claims to defend the French people against foreigners, immigrants and "the establishment".

2.2 Explaining the rise of populism

If the nature of populism is still widely debated, numerous factors favoring the emergence of such movements have been studied. As summarized by Panizza (2005), these "new relations of representation that become possible because of dislocations of the existing political order" generally emerge in times of social disorders (such as hyperinflation) or when political elites become discredited (in cases of corruption, for example). They might also be favored by economic, cultural and demographic instabilities (including globalization, migration waves or economic fluctuations).

Recent theoretical contributions

The theoretical tools of political economy have also been precious to understand the rise of populism, even though contributions on this specific topic are more recent. Models of populism have interpreted it, for instance, as a way for politicians to signal that they are not colluding with the elite (Acemoglu, Egorov, and Sonin (2013)), a way to dismantle checks and balances (which, as argued by Acemoglu, Robinson, and Torvik (2013), make bribery by the elite cheaper by reducing political rents), or a form of rejection of "disloyal" leaders (Tella and Rotemberg (2016)).

The role of economic factors

In the wake of the 2008 financial crisis, several contributions insisted on the crucial role played by economic conditions in explaining the rise of populism. More specifically, polarization of electoral preferences is likely to be caused by exposure to import shocks (Dorn, Hanson, Majlesi, et al. (2016), Malgouyres (2017), Dippel et al. (2017), Colantone and Stanig (2016))⁵, or by unemployment shocks (Algan, Guriev, et al. (2017)). These findings are more generally linked to a growing literature on the role of the economic environment in shaping beliefs and

5. One important exception is by Becker, Fetzer, and Novy (2017), who argue that trade shocks in the United Kingdom did not affect the Brexit vote

electoral preferences of individuals (see Giuliano and Spilimbergo (2013), Roth and Wohlfart (2016), Alesina, Stantcheva, and Teso (2017), Carreri and Teso (2016)).

The role of culture

Another frequently explored explanation of the rise of political polarization is related to cultural motives. Against the hypothesis of economic insecurity, Inglehart and Norris (2016) argued that "cultural backlash" is the main driver of the rise of populism. According to this hypothesis, populism is mainly driven by members of previously dominant strata of the society, who reject progressive values.

Such findings are in line with those of Algan, Beasley, et al. (2017), where we argue that *Front National* voters in the French elections of 2017 had a lower level of well-being, a higher level of pessimism, and a higher probability of feeling that the quality of their neighbourhood had been decreasing.

They also are in line with a vast literature assessing the impact of migration on electoral outcomes, and which overwhelmingly finds that large migration waves cause a rise in populist votes (Viskanic (2017), Becker and Fetzer (2016), Brunner and Kuhn (2014), Mendez and Cutillas (2014), Otto and Steinhardt (2014), Harmon (forthcoming), Halla, Wagner, and Zweimueller (forthcoming), Barone et al. (2016), Dustmann, Vasiljeva, and Damm (2016), Hangartner et al. (2017)).⁶

3 This dissertation

The next chapters of this thesis contribute in various dimensions to the literature described above.

3.1 Dynasties and Policymaking

The first chapter, called *Dynasties and Policymaking*, is co-authored with Gianmarco Daniele. In this chapter, we evaluate both the causes and consequences of dynastic power in Italy. Using

6. A recent strand of literature also emphasizes the role of exposure to certain types of media contents, including fake news (Barrera et al. (2017)), biased news (DellaVigna and Kaplan (2007), Durante and Knight (2012), Mastroiocco, Minale, et al. (2016)) or entertainment programmes (Durante, Pinotti, and Tesei (2017))).

an exceptionally rich dataset of more than 500,000 Italian politicians at the municipal level in Italy between 1985 and 2012, we document several important findings regarding the existence of political dynasties in Italy. First of all, we estimate the number of politicians who are likely to be dynastic and characterize them. To do so, we rely on common surnames within municipalities and define as dynastic any individual for whom we observe that a politician with the same surname was elected - as a mayor or a simple member of the municipal council - in the same municipality. To dampen concerns of misreporting (either because we wrongly identify dynastic mayors or fail at identifying genuinely dynastic ones), we run robustness checks using several restrictions by excluding individuals with very frequent names at the province level, and identify presumably dynastic ties within a ten-years bandwidth. Our main estimates suggest that between 1985 and 2012, about 15% of Italian mayors were dynastic. Using data indicating the characteristics of local politicians, we find that dynastic mayors are much younger and much less experienced than the others. Dynastic candidates are also more likely to win elections and to be reelected, even when controlling for the frequency of their names. Finally, we also document that dynastic politicians at the municipality level are more career-concerned (as they are more likely to enter upper layers of administration), and that power self-perpetuates within Italian municipalities (as elected candidates are disproportionately more likely to have a relative elected in the same municipality afterwards).

More crucially, we document what happens when such politicians are elected. To do so, we rely on two main specifications. Using the staggered calendar of municipal elections in panel fixed-effects regressions as well as a Regression-Discontinuity design on close elections between dynastic and non-dynastic candidates over the period 1998-2012, we show that on average the expenditures and revenues of cities ran by dynastic mayors are not different than those of cities ran by non-dynastic mayors. However while Italian non-dynastic mayors engage in Political Budget Cycles (notably through higher expenditures and lower tax rates in pre-electoral years), we find that the Political Budget Cycles of dynastic mayors in capital expenditures are twice this magnitude. Importantly, these higher expenditures in pre-electoral years are mostly funded through higher capital transfers from upper layers of government (debt and taxes also increase, but to a lesser extent).

These findings either suggest that dynastic mayors have a higher ability to run such policies (potentially through inherited political skills) or that they have higher incentives from staying in politics (for example because of greater career-concern motives), which we cannot disentangle empirically. Coherent with these hypotheses are the findings that dynastic politicians are much more likely to incur differential Political Budget Cycles when their election margin is low and when they are not facing a binding term-limit. Finally, we do not find any evidence that cities ran by dynastic mayors have different performances than cities ran by non-dynastic mayors - whether we measure it through the probability of early term-termination, the growth of the tax base of private firms, the speed of revenue collection and the ability of payment, or indicators of local corruption.

3.2 Gender Biases: Evidence from a Natural Experiment in French Local Elections

The second chapter of this thesis, which is called *Gender Biases: Evidence from a Natural Experiment in French Local Elections*, is co-authored with Jean-Benoît Eyméoud. In this chapter, we exploit a natural experiment in the French *Départementales* elections of 2015 to causally identify gender-discrimination from voters. In these elections, which aimed at electing 95 local councils, ballots were binominal and had to be gender-balanced (in order to guarantee perfectly gender-balanced local councils) for the first time in the history of French elections. We argue that this reform troubled some voters, who were used to voting for a single person and a substitute, and who might have thought that the name of the first person on the ballot was the name of the main candidate.

Crucially for our study, the order of appearance of the candidates on the ballots was determined by alphabetical order. We show that this feature of the election assigned male and female candidates at the top of the ballot in an as-good-as random manner, since parties did not choose male and female candidates based on their surname, and the characteristics of male and female candidates are similar whatever their position on the ballot.

Comparing more than 9,000 ballots, we show that right-wing ballots with a female candidate listed first lost about 1.5 percentage points of votes (corresponding to a difference of

about 4% to 5%), and were 4 percentage points less likely to go to the second round or to win the election (corresponding to a difference of about 5% to 6%).

Furthermore, given that candidates can report additional information on the ballot (such as their age, political experience, occupation and picture), we collected a sample of ballots representing about 12% of the total population of candidates and estimated that about 35% of them reported some kind of information on the ballot. Those ballots were found to have better electoral performances than the others. We use these additional data to test for the presence of statistical discrimination in the spirit of Altonji and Pierret (2001). In fact, we find that discrimination is coming essentially from ballots on which no information is reported, and that discrimination disappears on ballots where additional information is reported. Such results suggest that the discrimination we measure is likely to be statistical. Finally, we find that our measure of discrimination against right-wing women is correlated with measures of gender discrimination on the labor market, thus suggesting that gender discrimination in politics might be more efficiently tackled if policies addressing it are coordinated with policies on other markets.

3.3 Dismantling the "Jungle": Migrant Relocation and Extreme Voting in France

The third chapter of this thesis, which is called *Dismantling the "Jungle": Migrant Relocation and Extreme Voting in France*, is co-authored with Max Viskanic, and evaluates the impact of the relocation of Calais migrants in France between October 2015 and October 2016 on votes for the *Front National* party during the Presidential election of May 2017.

In this study, we use the dismantling of the Calais camp (which gathered around 7,000 migrants) as a natural experiment to test the effect of local and short-term exposure to migrants. Indeed, the Calais migrants were sent to more than 200 temporary centers (called *Centres d'Accueil et d'Orientation*), where they were offered sanitary and administrative assistance (in order to start a procedure to get the refugee status). These relocations generally involved a small number of migrants (typically a few dozens), who were meant to stay during a maximum period of three months. During this period, the migrants could not work, and once the

procedure to get the refugee status started, they were transferred to permanent centers.

In order to address potential endogeneity biases in the choices of location of migrants, we instrument the presence of a temporary center in a municipality by the number of beds available in holiday villages (*villages vacances*), which were particularly suited to welcoming migrants, since they were likely to be closed during the winter period. The exclusion restriction is likely to be warranted since the stock beds in holiday villages is highly persistent over time and was determined historically (to validate this, we run several tests which confirm that our analysis is unlikely to be affected by differential pre-trends).

Using this instrumental variable strategy, we find that municipalities which received migrants had a growth rate of *Front National* vote between 2012 and 2017 which was lower by about 15.7 percentage points. This effect suggests that in these municipalities, the growth rate of *Front National* vote between 2012 and 2017 was 25% the one of other municipalities (corresponding to an increase lower by 4 percentage points in terms of vote shares). We also show that this effect dissipates spatially: municipalities located within a 5km radius around a CAO had a growth rate of *Front National* vote lower by 1.8 percentage points.

Given the richness of our dataset, we are also able to perform heterogeneity analysis and to investigate the intensive margin of these results. Overall, we find that this negative effect is driven by municipalities which received few migrants: in facts, we find that those that received more than 39 migrants per 1,000 inhabitants saw their *Front National* vote increase more than the average. Furthermore, we find that the decrease in *Front National* vote is larger in municipalities with a bigger share of migrants and of young people in the total population. However, the negative effect was dampened in municipalities whose mayors publicly called to welcome migrants. Overall, these results are among the first to document plausible evidence that small-scale and short-run interactions with migrants are likely to reduce extreme votes (in line with the contact hypothesis of Allport (1954)), while confirming the main findings of the literature - according to which exposure to large migration waves are likely to increase far-right votes.

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Chapter 1

Dynasties and Policymaking

This paper is co-authored with Gianmarco Daniele

1 Introduction

While the effects of families in the transmission of wealth and power have been widely studied (Piketty 2013; Atkinson 2015), there has been considerably less work done on the role of families in politics. This is surprising as, even though political positions in democratic societies are generally awarded via elections, families still continue to play a significant role in the political arena.¹ The emerging academic literature on the role of families in politics has so far focused on how political dynasties arise and persist, and has not investigated their effects. By contrast, this paper focuses on whether dynastic-elected leaders behave differently from other politicians once they are in office.

Our reasoning relies on career concerns models, whereby a politician main objective is to maximize his chances to stay in office, which will depend upon individuals' retrospective voting. Therefore, while in office, politicians will struggle signaling their quality to voters. Dynastic politicians, thanks to their inherited skills, might be in a privileged position to send this signal.

Previous studies show that dynastic politicians are electorally more successful due to inherited political skills, as the ability to mobilize local networks, negotiate with local elites and

1. Dynastic politicians are common in diverse settings, including Argentina (Rossi 2017), Japan (Fukai and Fukui 1992; Asako et al. 2015), the Philippines (Querubin 2013) and the United States (Dal Bó, Dal Bó, and Snyder 2009; Feinstein 2010). For instance, the share of elected dynastic congressmen goes from about 9% in the United States (Dal Bó, Dal Bó, and Snyder 2009) to about 50% in the Philippines (Querubin 2013).

exploit their family's reputation (Dal Bó, Dal Bó, and Snyder 2009; Feinstein 2010; Querubin 2013; Rossi 2017; Cruz, Labonne, and Querubín 2017). While in office, their inherited skills might help dynastic leaders to carry out policies maximizing their chances of re-election. For instance, they might be more able to implement welfare improving policies or strategically enforce them when voters are more receptive, as right before the elections.²

Moreover, their inherited skills might also allow dynastic politicians to achieve greater gains from politics, further increasing their effort to stay in office and implement policies helping their re-election. A plausible example would be to exploit their predecessor's networks to maximize their rent-seeking: indeed, political networks have been shown to significantly increase politicians' revenues, the revenues of their relatives (Folke, Persson, and Rickne 2017, Labonne and Fafchamps 2015, Fisman, Schulz, and Vig 2012, Querubin and Snyder Jr 2011, Eggers and Hainmueller 2009) and the profits of connected firms (Faccio 2006, Amore and Bennedsen 2013, Gagliarducci and Manacorda 2014).³ Consequently, we suggest that dynastic politicians might implement different policies to maximize their re-election both because they *can* - thanks to inherited political skills - and/or because they *want* to - due to higher returns from politics.

In this paper, we test this hypothesis using data on Italian local politicians from the period 1985–2012. We focus on how municipal budgets vary across cities with/without a dynastic mayor. Italian mayors are directly elected, they represent the most visible local elected officer, and they hold a strong power on municipal budgets. This motivates our interest in the revenue (transfers, local taxes and loans) and expenditure (types of spending) pattern of local governments. Moreover, we also consider a set of variables measuring their overall performance while in office, which are not captured by budgetary indicators.

Our estimates, based on a panel fixed-effects estimation and on a regression discontinuity

2. However, such reasoning also suggests that dynastic leaders might behave less strategically when setting their agenda, since they benefit from an electoral advantage that makes it less important for them to signal their competence.

3. Indeed, dynastic politicians might prefer a political career for reputational reasons, i.e., they might perceive a higher utility (than non-dynastic politicians would) from holding political office (this motivation is related to the political science literature on public service motivations (e.g., Houston 2000). Finally, an alternative explanation is that dynastic politicians might suffer from a "Carnegie effect" (Durante, Labartino, and Perotti 2014) if the advantage granted by their elected ancestors led them to underinvest in their own human capital: in this case, a worse outside option might incentivize dynastic politicians to shape policies that maximize their chances of re-election.

design (RDD) on local elections won by a close margin, show that dynastic mayors do not behave differently in terms of average expenditure and average revenue (local taxes, loans and transfers). We also do not find meaningful differences in terms of types of spending. However, dynastic mayors are more likely to increase public spending in the year before an election. They increase spending, especially capital expenditure, and finance such spending mostly through higher transfers. The increase is substantial – between 70 and 190 euros per capita depending on the preferred specification. Indeed, we also find that non-dynastic mayors increase spending in a pre-electoral year, however, less than dynastic leaders. Therefore, dynastic politicians seem more prone to adopt a policy, i.e. higher pre-electoral spending, which is considered optimal also by non-dynastic politicians. This is in line with the idea that dynastic politicians appear to be more strategic because of higher ability and/or higher gains from being in politics. In favour of this interpretation, we also show that they are reactive to electoral incentives, as we find evidence of a political budget cycle (PBC) mostly for i) dynastic mayors at the end of their first term, i.e., those who are eligible to run for re-election (Italian mayors have a two-term limit); ii) dynastic mayors elected in more contested elections.⁴

Finally, when looking more broadly at their performance, we do not find any difference between dynastic and non-dynastic mayors. Specifically, we rely on proxies for political stability, economic growth, governance efficiency and corruption. Overall, as we only find differences in pre-electoral spending, we suggest that dynastic-elected leaders differ concerning policies explicitly linked to their political careers, while their inherited skills do not lead to “better” observable outcomes.

An underlying assumption of our analysis is that dynastic mayors differ from non-dynastic mayors. Indeed, we find that dynastic politicians have more successful careers and better electoral performances: they are more likely to win local elections and get elected in higher levels of government (i.e. provincial parliaments) than non-dynastic politicians⁵. Also, political

4. However, we do not find a stronger PBC for dynastic leaders when a relative will run in the next electoral round. This suggests that pre-electoral spending is used for individual rather than family ambitions.

5. Therefore, it appears that the electoral advantage enjoyed by dynastic mayors does not prevent them from behaving more strategically, in terms of higher spending before elections. This might be due to the relatively low cost of enforcing PBCs compared to the high cost of losing elections under electoral uncertainty. Also we do not find evidence of the fact that PBC affects electoral performance. However, this test relies on a correlation and cannot be causally interpreted (we address this point in the Web Appendix).

power is persistent in Italian municipalities: an elected mayor is twice as likely to have a relative in office as a non-elected mayoral candidate. These striking differences are in line with the relevance of inherited political skills, and justify our interest in the policymaking side.

As our analysis covers all Italian local elected politicians in the period 1985–2012 (N=571,824), we have to rely on a systematic method to identify family ties among them. In line with previous studies on academic and political dynasties (Allesina 2011; Durante, Labartino, and Perotti 2014; Querubin 2013) as well as inter-generational social mobility (Clark 2014; Clark and Cummins 2015), we define family ties as politicians who share the same surname in the same municipality. Clearly, this method of identification reduces the precision of our estimates. We show that our results are confirmed or reinforced when we exclude politicians with frequent surnames, since identifying family ties is more likely to be problematic in these cases.

Our research contributes to four strands of literature. First, it enhances understanding of the strategic behaviors that elites might engage in to sustain their power in modern democracies (Michels 1915; Mosca 1939; Pareto 1901; Robinson and Acemoglu 2008). According to theories of power transmission, dynasties, like other elite groups, strive to guarantee their power and its perpetuation to future generations (Michels 1915; Mosca 1939; Pareto 1901; Besley and Reynal-Querol 2015). In line with our hypothesis of higher gains from politics, Robinson and Acemoglu 2008 provide a model of endogenous political persistence in which “the elite, by virtue of their smaller numbers and their greater expected returns from controlling politics, have a comparative advantage in investing in de facto power” (Robinson and Acemoglu 2008).⁶

Second, we contribute to the emerging literature on families in politics. Previous studies show that dynasties self-perpetuate: they exploit the causal impact of the length of a politician’s tenure on the probability that he will have a family member in politics in the future (Dal Bó, Dal Bó, and Snyder 2009; Rossi 2017; Querubin 2013). A second group of studies directly investigates the electoral dynastic advantage, (Feinstein 2010; Asako et al. 2015, finding that dynastic politicians have a higher probability of success than non-dynastic politicians in national elections in the United States and Japan.⁷ Although our main contribution is to in-

6. Nevertheless, this does not imply that dynastic leaders would have worse policy performance, as recently shown by Besley and Reynal-Querol 2015, who argue that dynastic leaders perform particularly well when constraints on the executive are weak, as dynastic transmission of power is easier.

7. see Folke, Persson, and Rickne 2017 and Fiva and Smith 2016 for different results in other affluent European

investigate the effects of dynastic-elected leaders on policymaking, we also replicate previous findings on the electoral dynastic advantage and on dynasties self-perpetuation within our sample of Italian politicians.

Third, we contribute to the literature on the effects of "strong" family ties, which have been linked to detrimental outcomes in terms of firms' performance (Bertrand and Schoar 2006; Bennedsen et al. 2007), labor market participation (Alesina and Giuliano 2010) and academic recruitment (Durante, Labartino, and Perotti 2014). Finally, we also contribute to the literature on PBCs (Rogoff 1990; Blais and Nadeau 1992), showing that the incentives to manipulate expenditure can vary across political groups (Persson, Tabellini, et al. 2003). In particular, PBCs have been linked to rent seeking: Shi and Svensson 2006 show that the size of the PBC depends on politicians' rents of remaining in power. In this light, the results of this paper complement those of Bragança, Ferraz, and Rios 2015 and Geys 2017 related to the potential rent-seeking behaviors of dynastic politicians.

The remainder of the paper is structured as follows. In Section 2 and 3, we present the institutional background and the data we use. We estimate the impact of dynasties on municipal budgets in Section 4, before discussing the potential channels driving our results in Section 5. In Section 6, we test whether dynastic mayors also differ upon some proxies of their overall performance while in office. In Section 7 we test the underlying assumption that dynastic mayors differ from non-dynastic mayors, and conclude in Section 8.

2 Institutional background and data

2.1 Local politics in Italy

The Italian political system has three levels of governance: municipalities (about 8,000 across the country) represent the lowest level, followed by regions (20) and the national level. Until 2014, provinces (110) represented another level of government between cities and regions. Nonetheless, as in most other European countries, municipal governments have important responsibilities with respect to education, social welfare, culture and recreation, city planning, transport, economic development, waste management and local police. They also have im-

countries, i.e. Norway and Sweden.

portant fiscal powers, and setting the local property tax rate is the central annual financial decision (Bordignon, Cerniglia, and Revelli 2003). As the share of national transfers has gradually decreased over time, local revenues have increasingly financed the municipal budget. However, their spending capacity is constrained by the "Internal Stability and Growth Pact," which limits the ability of municipalities to incur debts. Moreover, according to the Italian Constitution, such debts can only cover capital expenditures.

Local elections are held every five years (every four years before 2000) to elect council members and the (directly elected) mayor. The electoral system depends on the size of the municipality. In cities of fewer than 15,000 inhabitants, voters effectively have only one vote, which they cast for a candidate mayor and her list of supported candidates for the municipal council (though additional 'preference votes' for candidates within this list of candidates are possible). Elections are held in a single round, in which the mayoral candidate who obtains the most votes is selected, and her list of candidates is allocated at least 66% of the council seats. The remaining seats are allocated proportionally to the vote share of the other mayoral candidates and their lists. In municipalities with more than 15,000 inhabitants, voters choose between parties (or coalitions) that present a list of candidates for the municipal council and support a candidate mayor. Voters cast one vote for a candidate mayor and one vote for a list of candidates for the council (which can, but need not be, the list supporting a voter's preferred mayoral candidate). Elections for mayor in these larger municipalities follow a run-off system, whereby the two top candidates run in a second round if no candidate obtains an outright majority in round one. The list(s) supporting the winning mayor are allocated at least 60% of the council seats, and there is a 3% threshold for the proportional allocation of the remaining seats (see Bordignon, Gamalerio, and Turati 2013 for more details).⁸

2.2 Identifying political dynasties in Italy

In this paper, we gather a wide set of data concerning local Italian municipalities in order to identify political dynasties and measure fiscal outcomes at the municipality level. Specifically, we base our estimates on three different datasets: i) individual data about all local elected

8. Even though we do not use the threshold of 15,000 inhabitants as an identifying device in our analysis, in Section 4 we provide evidence that it is unlikely to affect our results.

politicians in the period 1985–2012, which includes some biographical information (e.g., gender, education, date and place of birth, job); ii) local election outcomes in the period 1993–2012;⁹ iii) a dataset about city fiscal outcomes (revenues and expenditures) in the period 1998–2012. All data are publicly available and provided by the Italian Ministry of Interior for the above-mentioned periods.

Political dynasties are common at the municipal level in Italy. To identify dynastic politicians, we rely on the three datasets described above. Our data, however, do not allow us to directly identify family ties between elected representatives in Italy. Similar to recent studies on academic and political dynasties (Allesina 2011; Durante, Labartino, and Perotti 2014; Querubin 2013) as well as inter-generational social mobility (Clark 2014; Clark and Cummins 2015), we search for individuals with the same surname to identify (presumed) family ties. Specifically, we define dynastic mayors as those with at least one politician elected in the past (since 1985) in the same municipality with the same surname.

Using surnames to operationalize political dynasties is a valid approximation in our Italian setting, since children receive the surname of their father. However, such a methodology might suffer from two different types of errors. First, since people can have the same surname without being related, we might wrongly identify individuals from different families as dynastic. Second, this operationalization only identifies ties between family members if they have the same surname. While these reflect the closest family ties that are likely to generate the strongest effects (e.g., children, grandchildren), it may overlook more distant kinship ties (e.g., cousins, nephews, son-in-law) and those among spouses and daughters who have changed their surname upon marriage. Therefore we might wrongly identify as non-dynastic individuals who belong to the same family but have different surnames. Although data availability prevents us from directly addressing both issues, it is important to observe that they bias our estimates towards zero. Both issues indeed imply that we fail to define a certain number of dynastic politicians as part of a dynasty (i.e., these remain in the control, “non-dynastic” group). For instance, since dynastic politicians are expected to have different spending patterns than non-dynastic politicians, this misallocation pushes the average spending in the ‘control’ group

9. This dataset also includes information on candidates who were either elected mayor or who received enough votes to become a councilor.

closer to the average in the ‘treatment’ group (i.e., dynastic politicians) – inducing a bias in our estimates towards zero. This not only stacks the deck against us, but also implies that our findings reflect a lower bound of the true effect of political dynasties. Nonetheless, we further address this concern through several tests, such as excluding the most common surnames from the estimation sample and controlling for the relative frequency of each surname at the provincial level in the overall Italian population.

3 Importance and characteristics of dynastic politicians

3.1 Share of dynastic politicians: heterogeneity across time and space

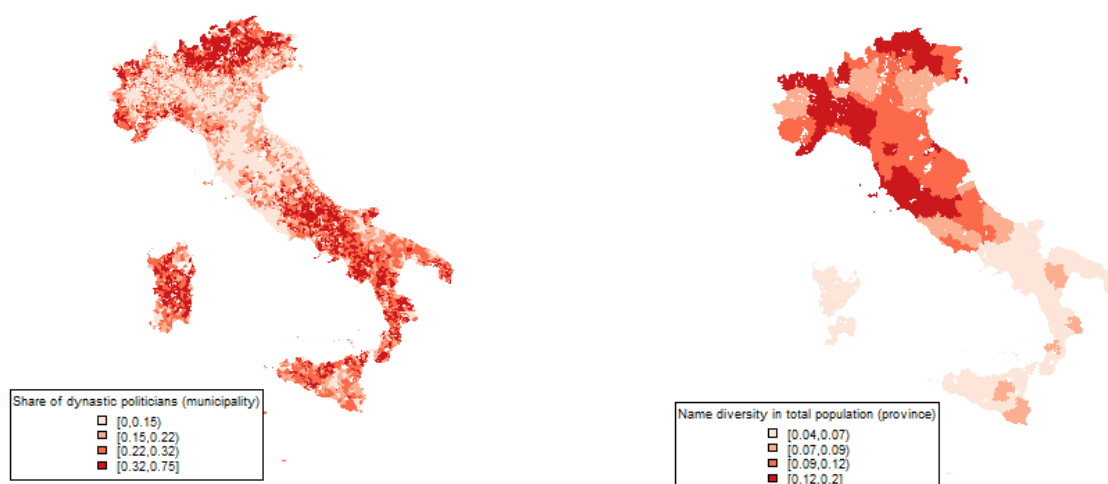
Dynastic local politicians represent an important share of politicians.¹⁰ As shown in Figure 1.1a, the share of dynastic politicians by municipality over the period 1998–2012 is heterogeneously spread across the country: it seems to be particularly high in the south and north of the country (more than one politician in three has at least the same surname as a previous member of the city council), and lower in the center of the country (with shares closer to 10%). However, the distribution in the shares of dynastic politicians during this period might reflect some underlying characteristics of the municipalities. For example, surname concentration is not even across the country. Using tax data (from 2005) that records the occurrence of every surname in the Italian population at the province level¹¹, Figure 1.1b displays surname concentration at the province level. Surname diversity is very heterogeneous across Italian provinces, and higher in the north than the south (i.e., more individuals share the same surname in the south). In the north, the number of surnames corresponds to about 10–15% of the total number of individuals, while in the south this figure is about 5–10%.

A second source of heterogeneity stems from the fact that the number of presumably dynastic individuals is not constant over time. Figure 1.2 highlights this heterogeneity over time, representing the share of dynastic mayors for different categories of mayors, according to the frequency of their surnames in the total population. During our period of interest (1998–2012),

10. We use “politicians” to refer to members of municipality councils. Note that in this section, we mostly focus on dynastic politicians in the period 1998–2012, as our main analysis – on fiscal outcomes – is restricted to this period due to data availability.

11. We are grateful to Giovanna Labartino for providing these data.

Figure 1.1: Dynastic politicians and surname concentration



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

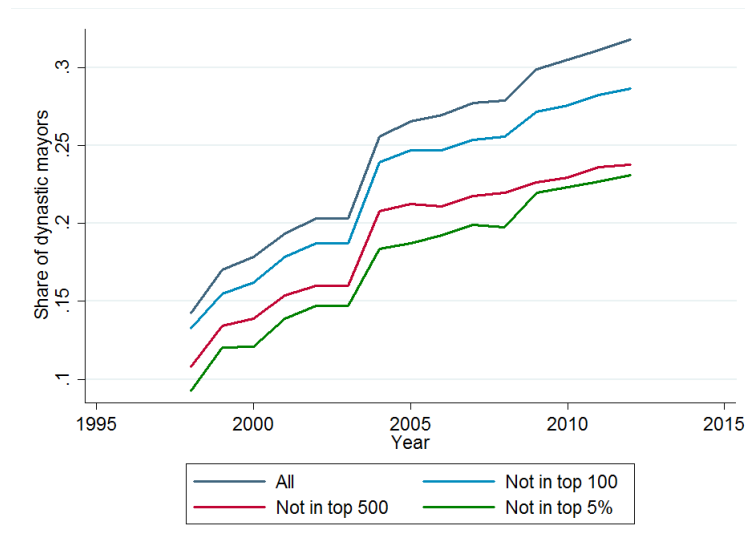
(b) Surname diversity by province

the share of all dynastic mayors doubled, from 15% to more than 30%. If we restrict the sample to individuals whose surname is not among the 100 most common surnames at the province level (which excludes about 15% of elected mayors), the share of assumed dynastic individuals increases from 13% in 1998 to 28% in 2012. For individuals whose surname is not among the 500 most common surnames at the province level (which excludes 20–25%), this share rises from 11% in 1998 to 24% in 2012. Finally, for individuals whose surname is not among the 5% most common surnames in the province (which excludes about 50% of the sample), this share grows from 9% in 1998 to about 23% in 2012.

However, the huge increase in the number of dynastic candidates reflects the fact that for politicians elected in the later years of our dataset, a longer time window is available (i.e., all previous years in our dataset since 1985) to determine whether they are dynastic or not. This can be problematic because the number of dynastic individuals not identified as such is likely to decrease over time, which can induce a time-varying bias.¹²

12. Moreover, in the Web Appendix, we show that the average age difference between first generations and their presumed dynastic successors increases over time (from about 8 years in 1998 to about 20 years in 2012). The distribution of age differences between the first individual of a dynasty (hereafter referred to as “first generation”) and his potential successors during the period 1998–2012 is bimodal. The first mode is around 0 and the second

Figure 1.2: Evolution of dynastic mayors and surname concentration



The figure represents the share of dynastic mayors for different subsamples, based on the frequency of their surname at the province level. The sample "Not in top 100" (resp. 500) includes all mayors whose surname is not among the 100 (resp. 500) most common in the province. The sample "Not in top 5%" includes all mayors whose surname is not among the 5% most common surnames in the province.

In order to address this issue, we provide alternative definitions of dynastic individuals, defining a mayor as dynastic if the first observed individual holding the same surname as him entered the municipal council within 10 years or within 5 years before his first appearance in a municipal council. As emphasized in Figures 1.3a and 1.3b, dynastic mayors identified within 10 years still correspond to more than 60% of the assumed dynastic individuals in 2012, while those identified within 5 years account for only 35% in that year.

Below, our benchmark results impose no restrictions, either in terms of politicians' surname frequency or of time window used to identify dynastic mayors: we use the full sample of politicians and identify dynastic individuals through shared surnames in the same city. However, we use the alternative specifications presented in this section to control for the robustness of the results.

is around 30. This evidence is compatible with the hypothesis that the kinds of linkages that we capture most often are either siblings or fathers and sons. Finally, even though our analysis starts in 1998, because of dataset limitations, we can assume that we are relatively more likely to catch sibling linkages at the beginning of the period, and relatively more likely to catch father-and-son linkages at the end of the period.

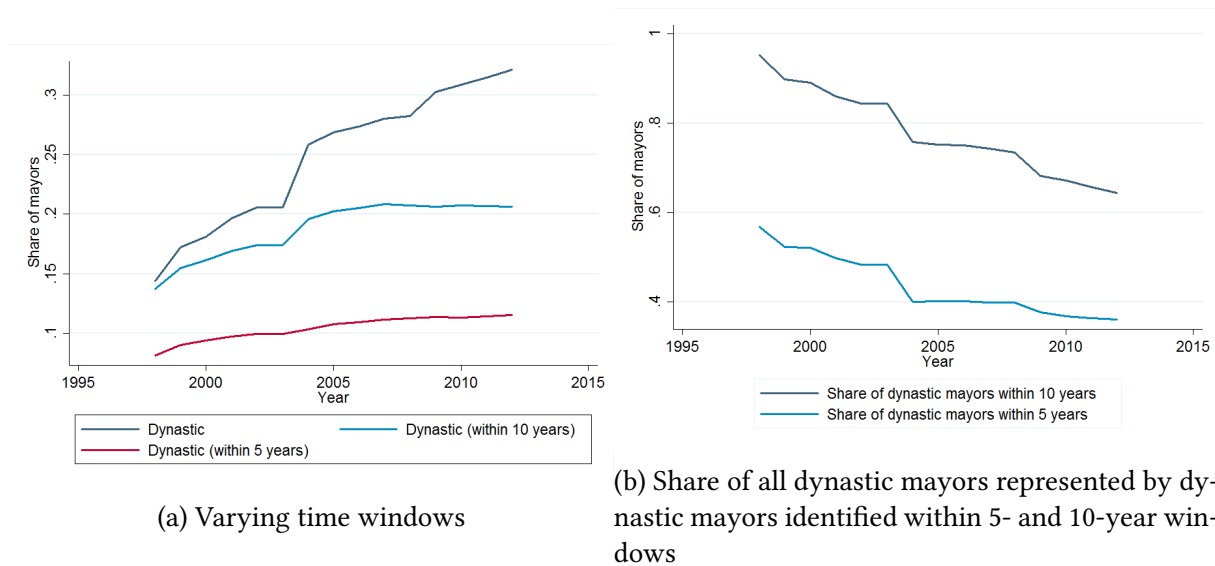


Figure 1.3: Dynastic mayors and time windows

3.2 Characteristics of dynastic politicians

Table 1.1 shows the characteristics of dynastic mayors in cities between 1998 and 2012 (see the Appendix for the exact definition of each variable). Dynastic mayors are much younger (4 years) and have a shorter political tenure (4 years less of previous political experience). This is likely due to the fact that, as they inherit an electoral advantage, they need less political experience to be electorally competitive as mayoral candidates. We will discuss this point later in the paper. Moreover, they are also much more likely to run in civic parties (i.e. parties without a national organization) and in the south of the country.

In terms of cities' characteristics, we observe higher unemployment rates and lower levels of trust in cities run by dynastic leaders. We also find that the average term length for a dynastic mayor is slightly longer than for a non-dynastic mayor. Finally, we find slightly worse performance for dynastic mayors as measured by the share of actual revenues over expected revenues and the share of due expenditures paid during the year. However, as discussed more below, some of these facts are driven by structural effects.

Table 1.1: Characteristics of dynastic mayors

	Non-Dynastic	Obs.	Dynastic	Obs.	Diff.	T-Stat
Mayor characteristics						
Re-elected	0.532 (0.499)	9496	0.536 (0.499)	3222	-0.004	-0.362
Age	51.123 (9.422)	81113	47.921 (10.018)	26119	3.202	47.031
Male	0.913 (0.282)	81113	0.898 (0.303)	26119	0.015	7.228
Education	14.48 (3.457)	79651	14.707 (3.333)	24770	-0.228	-9.126
Born in city	0.498 (0.5)	81113	0.522 (0.5)	26119	-0.024	-6.882
Experience	11.948 (6.662)	81113	7.183 (5.14)	26119	4.765	105.885
Civic	0.59 (0.492)	81113	0.694 (0.461)	26119	-0.104	-30.226
City characteristics						
South	0.273 (0.446)	81113	0.368 (0.482)	26119	-0.095	-29.262
Population	8088.602 (47716.83)	79371	4698.555 (17266.436)	25603	3390.047	11.135
Unemployment	9.322 (8.287)	80780	10.739 (9.114)	26027	-1.418	-23.416
Trust	0.316 (0.14)	70906	0.312 (0.145)	22739	0.004	3.838
Average budget						
Total Exp	1582.137 (2445.329)	78140	1890.757 (11286.531)	25201	-308.621	-7.142
Current Exp	776.592 (1221.81)	78140	887.023 (4722.564)	25201	-110.431	-5.948
Capital Exp	587.691 (1336.632)	78143	757.284 (6406.442)	25200	-169.593	-6.946
Tax rev	346.746 (587.284)	78146	382.897 (3756.499)	25224	-36.151	-2.594
Loans	138.074 (397.479)	78044	160.904 (2287.443)	25204	-22.83	-2.666
Capital transfers	449.084 (1480.282)	78037	588.179 (3860.675)	25218	-139.095	-8.344
Competence						
Term duration	3.632 (1.616)	22337	3.806 (1.485)	6859	-0.174	-7.963
Speed of payment	77.805 (11.269)	76621	77.573 (8.155)	24496	0.231	2.975
Ability of revenue collection	61.544 (15.919)	76854	60.773 (16.613)	24579	0.771	6.536
Growth of private tax base	0.022 (0.524)	36586	0.014 (0.142)	12531	0.008	1.719

The considered variables are: re-election rate, age, gender, level of education (as measured by the minimum number of years to obtain a certain degree), birthplace of the mayor, number of years since the first election to the city council, being a candidate of a civic party (the base category is being a candidate of a national political party), being elected in Southern Italy, city's population and unemployment rate, trust (at the provincial level), levels of total, current and capital expenditures, tax revenues, contracted loans and received capital transfers (all expressed in euros per capita), duration of the term, speed of payment, revenue collection capacity and yearly growth of the private tax base.
Standard deviations in parentheses.

4 Consequences of political dynasties on local budgets

In this section, we present our main results about the behavior of dynastic politicians while in office.

4.1 Identification strategies

Fixed-effects regressions

To explore the effects of dynastic mayors on municipal budgets, we first use a fixed-effects approach on the full sample of observations between 1998 and 2012. We are interested in two specific features: (1) the extent to which the size of the components of municipal budgets varies across dynastic and non-dynastic mayors and (2) the presence of PBCs at the municipality level, and their magnitude for dynastic vs. non-dynastic mayors.

Table 1.2: Year of election of mayors in the sample (1998–2012)

Year	Panel specification		RDD specification	
	All obs.	Restricted	All obs.	Restricted
1999	4,312	3,855	977	919
2000	841	605	126	101
2001	1,182	880	305	261
2002	891	586	269	221
2003	437	260	132	119
2004	4,202	3,660	1,232	1,103
2005	811	607	132	105
2006	1,178	902	396	347
2007	870	655	300	257
2008	500	358	175	145

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

We first test for the effect of dynastic mayors on average revenues and expenditures using the following specification:

$$Y_{it} = \alpha + \beta D_{it} + \nu X_{it} + \gamma_t + \epsilon_i + u_{it},$$

where $Y_{i,t}$ is an outcome variable for city i in year t , D_{it} is a dummy equal to 1 if the mayor of city i in year t is dynastic, X_{it} is a set of city characteristics for city i in year t , γ_t is a year fixed effect, ϵ_i is a city fixed effect and u_{it} is a time-varying error term. The parameter β indicates the difference in outcome variables between dynastic and non-dynastic mayors.

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

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

where LY_{it} is a dummy equal to 1 if the next election in city i at time t occurs during the following year and 0 otherwise.

The parameter β indicates the average value of the outcome variable for dynastic mayors during the three first years of their term. The parameter δ indicates, for mayors who are non-dynastic, the difference in outcomes between the last year of the term and the first three years. The parameter κ indicates the extent to which this difference is higher for dynastic mayors than for non-dynastic mayors.

Note that we are able to identify PBCs because for each city, the electoral calendar is exogenously defined *ex ante*, and because municipal elections do not occur in the same year for each city. We are therefore able to separate year fixed effects from the effect of time until the next election. Furthermore, to make sure that we properly estimate PBCs, we only include cities that meet a certain number of criteria. In the fixed-effects estimation, we only include cities for which two full 5-year terms are observed (i.e., for elections occurring after 1999). This ensures that we avoid cases of early termination and that we have enough intra-city variation in terms of explanatory variables to separately identify all the effects mentioned above.^{13 14}

Overall, this amounts to using a sample of 6,184 cities for the fixed-effects analysis (see Table

13. While such a restriction aims at properly identifying Political Budget Cycles and at obtaining a balanced panel, it might create endogeneity issues if the probability of early termination is correlated with the dynastic nature of a mayor. However, as we show in section 7, dynastic mayors are not more subject to early termination of their term.

14. In the RDD framework implemented in the following sections, because the inference relies upon inter-city variation (as opposed to intra-city variation in the fixed-effect framework), we impose a slightly less stringent constraint and keep cities for which at least one full term is observed between 1999 and 2012.

Table 1.3: Effect of political dynasties on average budget

	Total exp	Current exp	Cap. exp	Tax rev	Loans	Cap. transfers
Dynasty	28.618 (21.019)	3.637 (4.562)	20.914 (17.574)	3.281 (2.096)	9.514 (5.228)*	12.273 (16.323)
R^2	0.02	0.24	0.02	0.41	0.01	0.01
N	47,420	47,420	47,420	47,418	47,416	47,416

The table presents estimates from fixed-effects panel regressions, using categories of public expenditures and income as dependent variables (all are expressed in euros per capita, and winsorized at the 1% level). The main explanatory variable is a dummy indicating whether the mayor is dynastic. The sample is comprised of all cities for which two full 5-year terms were observed between 1999 and 2012. Election years are excluded from the estimation. All specifications control for city and year fixed effects, as well as population size and the mayor's sex, age, experience, years of education, birthplace and term-limit. Standard errors are clustered at the city level. Standard errors in parentheses.

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

1.2 for the full list of elections by year).¹⁵ In order to avoid potential outliers, we winsorize the outcome data at the 1% level. Finally, since the quasi totality of the elections in our sample were held between the months of April and June, in case of a change of mayor, we are not sure of who is deciding of the budget in electoral years: we therefore drop the latter from the estimation.

Estimation

Table 1.3 reports the estimation results for average budget components. The reported variables of interest are total expenditures, current expenditures, capital expenditures, taxes, loans, and capital transfers from the regional and national governments (expressed in euros per capita). Each regression controls for the mayor's age, experience and years of education, as well as for term limits. Covariates also include dummies indicating whether the mayor was born in the city, and whether (s)he is from a civic party (i.e. a party without a national organization). Finally, we also control for the city's population.

In Table 1.3, we observe no effect of political dynasties on average current and capital expenditures. Nor do we find any effect on tax revenues and capital transfers from upper layers of government. However, it appears that dynastic mayors contract slightly more loans (9.5 euros per capita, on average) than non-dynastic mayors.

But while few effects are noticeable in terms of average budget, we find much more varia-

15. There are 2,938 cities in the RDD specification: the number of cities present in this specification is smaller as additional constraints to identify closely elected dynastic candidates are necessary (see next section).

Table 1.4: Effect of political dynasties on PBCs

	Total exp	Current exp	Capital exp	Tax rev	Loans	Cap. transfers
Dynasty	14.595 (21.462)	1.835 (4.617)	8.920 (17.926)	2.133 (2.096)	5.945 (5.336)	4.182 (16.668)
LY	44.550 (10.114)***	-0.615 (1.398)	36.181 (9.143)***	-5.223 (0.840)***	12.898 (3.032)***	19.475 (8.112)**
Dynasty*LY	51.078 (17.137)***	7.183 (2.254)***	43.875 (15.996)***	5.042 (1.570)***	12.852 (5.187)**	30.088 (14.740)**
R^2	0.02	0.24	0.02	0.42	0.01	0.01
N	47,420	47,420	47,420	47,418	47,416	47,416

The table presents estimates from fixed-effects panel regressions, using categories of public expenditures and income as dependent variables (all are expressed in euros per capita, and winsorized at the 1% level). The main explanatory variables are two dummies indicating (1) whether the mayor is dynastic and (2) whether it is the last year in the mayor's term. All outcome variables are expressed in euros per capita. The sample is comprised of all cities for which two full 5-year terms were observed between 1999 and 2012. Election years are excluded from the estimation. All specifications control for city and year fixed effects, as well as population size and the mayor's sex, age, experience, years of education, birthplace and term-limits. Standard errors are clustered at the city level. Standard errors in parentheses.

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

tion in terms of PBCs. Several conclusions can be drawn from the analysis of Table 1.4. First, in cities run by non-dynastic mayors, expenditures are about 45 euros per capita higher in the last year of the term than at the beginning. This is mainly due to an increase in capital expenditures (which are 36 euros per capita higher), which seems to be financed by an increase in capital transfers from the government and the region (with a difference of about 19 euros per capita between the last year and the three first years of the term) and by an increase in contracted loans (with a difference of about 13 euros per capita). However, tax revenues seem to decrease during the last year of the term by about 5 euros. Put differently, we observe a strong PBC in our sample: before the elections, non-dynastic mayors increase capital expenditures and reduce taxes, while increasing loans and transfers from upper levels of government.

Second, PBCs are much higher for dynastic mayors. Indeed, between the last year and the previous three years of the first term, the variation in total per capita expenditures of mayors is 51 euros higher for dynastic than for non-dynastic mayors. This higher PBC comes mostly from a substantial additional increase in capital expenditures during the last year of the term (44 additional euros compared to the previous three 3 years), and from an additional increase in current expenditures per capita (about 7 euros). This increase in expenditures during the last year of the term is mostly financed by capital transfers from the national and regional governments (with a difference of 30 euros) and by an increase in contracted loans (with a

difference of 13 euros), while taxes relatively increase by an additional 5 euros during the last year. Therefore, the PBC of dynastic mayors is much more pronounced than that of non-dynastic mayors: they spend relatively more at the end of the term than non-dynastic mayors, and finance this additional increase in expenditures mostly through capital transfers from the national and regional governments.¹⁶

Importantly, such results are robust to imposing some restrictions on the identification of dynastic mayors. In Table 1.5 we show that if we keep only mayors whose name is not among the 100 most common in the province, the estimated relative PBC of dynastic mayors is higher than in the baseline analysis - suggesting that our main results suffer from attenuation bias. In a Web Appendix, we show that our results are also robust to excluding politicians whose name is among the 500 most frequent in the province, and to considering as dynastic mayors who had a relative in office during the last 10 years.¹⁷

4.2 Regression-Discontinuity Design

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

To address these issues, we use an RDD, focusing on close elections in which the two best candidates are a dynastic and a non-dynastic one. We define the forcing variable as the difference in vote shares between the best dynastic candidate and the best non-dynastic candidate.

16. The ability of receiving higher transfer in pre-electoral years might signal that dynastic politicians are likely to have more connections with upper layers of governments - thus securing transfers more easily, in line with findings of Brollo and Nannicini 2012), who show that, in Brazil, politically aligned mayors are more likely to get transfers in pre-electoral years).

17. Furthermore, we observe several interesting sources of heterogeneity in this analysis. First, the relative political budget cycle of dynastic mayors is much higher in smaller municipalities (although we do not observe any jump at the 15,000 threshold - see Section 2), and for mayors having liberal occupations (such as lawyer, mayor or notaries). We also find that these differences are higher for younger and less experienced mayors. Since family ties and reputational effects are likely to be stronger in smaller municipalities and for liberal occupations, the advantages and incentives inherent to dynasties might be stronger there. Finally, the finding about the age and experience of dynastic politicians is especially compatible with a career concern motive (as proposed by Alesina, Troiano, and Cassidy 2015 about young mayors, and as we emphasize in Section 5).

Table 1.5: Dynasties and PBCs: restriction to mayors whose name is not among the 100 most common in the province

	Total exp	Current exp	Capital exp	Tax rev	Loans	Cap. transfers
Dynasty	12.919 (25.798)	6.587 (5.595)	3.001 (22.091)	3.899 (2.648)	-1.381 (6.562)	4.165 (20.775)
LY	39.736 (12.204) ^{***}	-0.057 (1.577)	30.238 (10.910) ^{***}	-5.480 (0.991) ^{***}	13.084 (3.633) ^{***}	11.226 (9.753)
Dynasty*LY	80.641 (22.208) ^{***}	6.749 (2.750) ^{**}	75.700 (20.583) ^{***}	6.117 (1.834) ^{***}	17.903 (6.710) ^{***}	55.332 (19.125) ^{***}
R^2	0.02	0.26	0.01	0.41	0.01	0.01
N	34,198	34,198	34,198	34,205	34,203	34,203

The table presents estimates from fixed-effects panel regressions, using categories of public expenditures and income as dependent variables (all are expressed in euros per capita, and winsorized at the 1% level). The main explanatory variables are two dummies indicating (1) whether the mayor is dynastic and (2) whether it is the last year in the mayor's term. All outcome variables are expressed in euros per capita. The sample is comprised of all cities for which two full 5-year terms were observed between 1999 and 2012, restricted to mayors whose name is not among the 100 most common at the province level. Election years are excluded from the estimation. All specifications control for city and year fixed effects, as well as population size and the mayor's sex, age, experience, years of education, birthplace and term-limits. Standard errors are clustered at the city level. Standard errors in parentheses.

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

This variable can take any value between -1 and 1 , and it takes a positive value if a dynastic mayor is elected. The intuition behind this methodology is that the assignment of dynastic or non-dynastic mayors in elections won by a narrow margin is as good as random. Our setting involves a sharp RDD. D_i is the dummy variable indicating whether a dynastic mayor is elected, and X_i denotes the margin of the best dynastic candidate. In this case, we have:

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

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

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

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

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

where Y_{it} is the outcome of interest in city i over the *term* t , D_{it} is a dummy equal to 1 if the elected mayor is dynastic and $P(X_{it})$ is a polynomial function of the margin of the best dynastic candidate. The estimated effect of dynastic mayors is therefore the coefficient $\hat{\beta}$.¹⁸

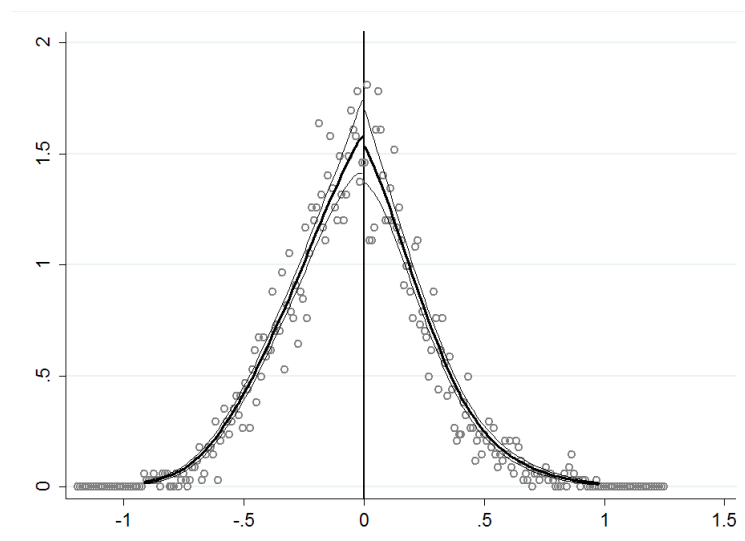
However, as pointed out by Hahn, Todd, and Van der Klaauw 2001 and summarized by Lee and Lemieux 2010, in order for the observations below the threshold to be a good counterfactual of individuals on the right of the threshold, and for the estimate $\hat{\beta}$ to be unbiased, the potential outcomes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

Estimation

As explained above, the RD provides unbiased estimates of the treatment if the threshold of the forcing variable cannot be manipulated. This amounts to testing whether the running variable is continuous around the threshold. To check the validity of this hypothesis in our framework, we run a McCrary test (McCrary 2008), the results of which are presented in Figure 1.C.1. To identify the margin of dynastic and non-dynastic candidates, we only kept elections for which information on at least the two best candidates is available, and in which at least one dynastic candidate was identified. As previously explained, we only present results for cities for which full 5-year terms are observed (the number of elections meeting these criteria is presented

18. As in the fixed-effect estimation, we include only full 5-year terms after 1999, and exclude election years from the estimation.

Figure 1.4: McCrary test on the RDD sample of municipal elections



The figure represents a McCrary test of discontinuity in zero of the density of the margin of the best dynastic candidates for our selected sample of elections between 1999 and 2007

in Table 1.2 above). The test suggests that the margin of dynastic candidates on the panel of elections we consider is not discontinuous around zero.

Another key hypothesis of the RDD is that around the threshold, the allocation of the treatment (i.e., having a dynastic mayor or not) should be as good as random. Put differently, we should not observe any significant discontinuity around the threshold for other covariates. However, as emphasized in Figure 1.5, age and experience are markedly lower for dynastic mayors. As shown in Figure 1.6, the main other control variables are mostly balanced around the threshold. Table 1.6 confirms this intuition: it gathers results from the estimation of an RD in which we estimate a local polynomial regression with polynomials of order 2, using an optimal bandwidth selected according to the methodology developed by Calonico, Cattaneo, and Titiunik 2014 and a triangular kernel.¹⁹ Robust standard errors are clustered at the municipal level.

Overall, around the threshold, dynastic mayors are 2.8 years younger and have spent 6 years less in the municipal council (which corresponds to more than a term of difference). On the one hand, such discontinuities in observed covariates confirm that dynastic leaders differ from other politicians. Their dynastic advantage is likely to determine their mayoral candidacy

19. The results are similar when controlling for a higher or lower order of the polynomials.

Table 1.6: Discontinuity of covariates around the threshold

Order 2 Polynom	Age	Exp	Born in city	Male	Education
Dynasty	-2.877** (1.365)	-6.117*** (0.665)	0.029 (0.077)	-0.038 (0.037)	0.614 (0.454)
Bandwidth	0.278	0.375	0.209	0.250	0.270
N (left)	1291	1563	1067	1217	1247
N (right)	1139	1350	947	1080	1082
	Civic list	South	Population	Unemployment	
Robust	-0.010 (0.067)	0.035 (0.068)	1899.222 (1357.696)	1.048 (1.214)	
Bandwidth	0.259	0.275	0.159	0.241	
N (left)	1244	1285	831	1184	
N (right)	1103	1134	775	1054	

The table presents the results of an RD estimation with an optimal bandwidth calculated using the Calonico, Cattaneo, and Titiunik (2014) method, which employs a triangular kernel and controls for an order-two polynomial of the margin of victory of the best dynastic candidate. Dependent variables are characteristics of mayors and their cities. The sample consists of all full 5-year mayoral terms for election years between 1999 and 2012. Age and experience are measured at the beginning of the term, while population corresponds to the average population during the term. Robust standard errors clustered at the city level in parentheses.

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

at an early age and at an early stage of their political career.

On the other hand, given these discontinuities, which might affect PBCs (Alesina, Troiano, and Cassidy 2015 show, for example, that young mayors in Italian municipalities have higher PBCs), the estimation of the causal effect of dynastic leadership on PBCs might be biased. In fact, our results indicate the type of policies implemented by a certain type of politician, who would be dynastic, young and with little political experience. However, if we assume that dynastic politicians are younger and less experience *because* of their dynastic advantage, this would still provide us an unbiased effect of the policies implemented by dynastic leaders.²⁰

20. We therefore embrace the points of view of Becker et al. 2016, Campa and Serafinelli 2015 and Gagliarducci and Paserman 2016, who discuss RDD results in a similar fashion.

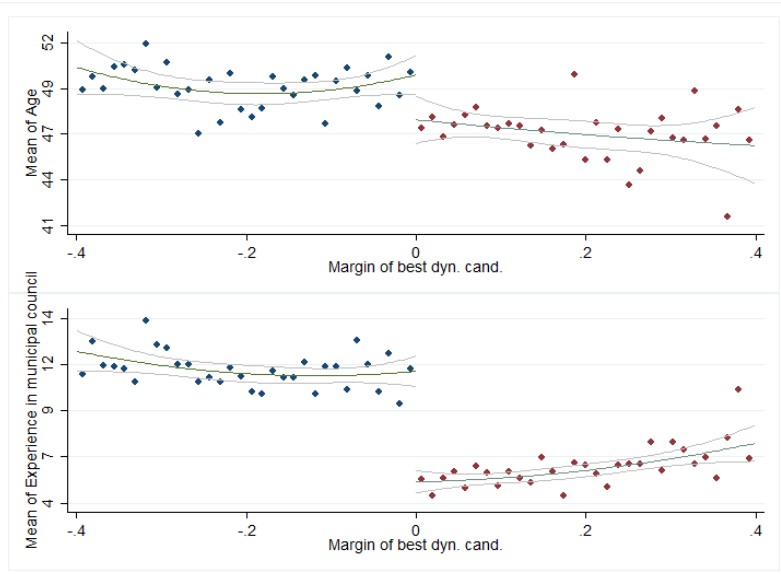


Figure 1.5: Discontinuity of age and experience (full sample)

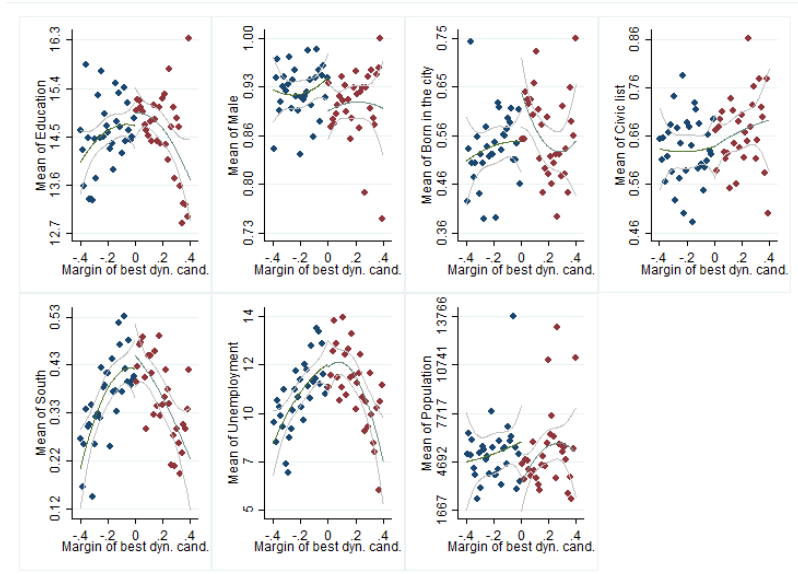


Figure 1.6: Discontinuity of other variables (full sample)

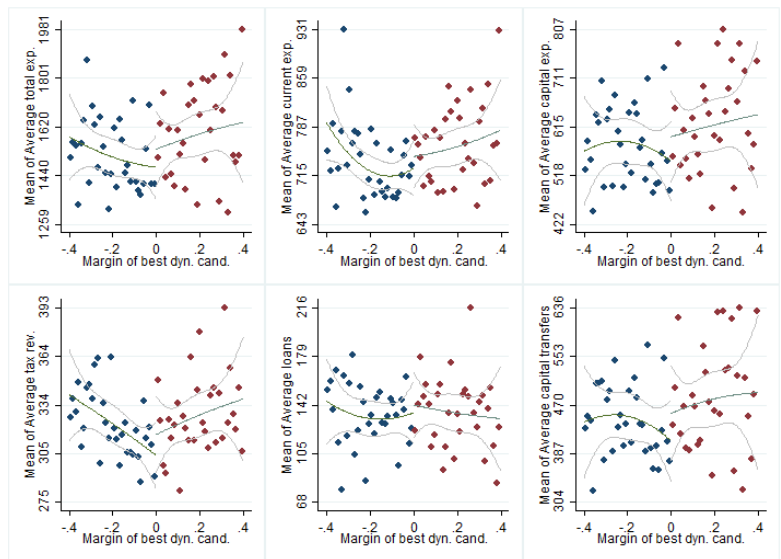


Figure 1.7: Average expenditures and revenues (full sample)

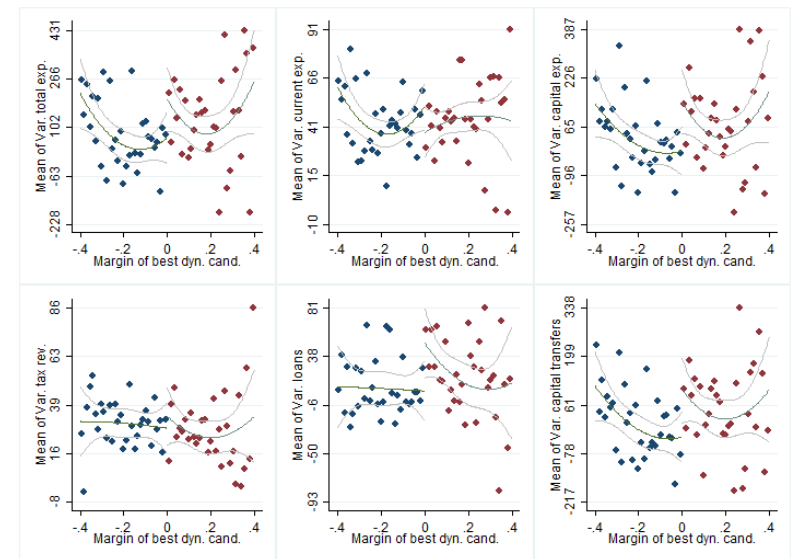


Figure 1.8: Variation in expenditures and revenues (full sample)

Still, even when relaxing this assumption, we partially recover a pure effect of being dynastic, firstly by including age and experience as covariates in our RDD estimates²¹; secondly, in the Web Appendix we complement this RDD with a matching procedure inspired by Alesina, Troiano, and Cassidy 2015, which helps reducing the observed imbalances around the threshold, i.e., it controls for differences in age and experience between dynastic and non-dynastic mayors; thirdly, we test the RDD estimations on the subsample of "young" (below the median age) and "inexperienced" (freshmen candidates) mayors. In this case, the comparison is, for instance, between a young closely elected dynastic candidate and a young closely elected non-dynastic candidate. If the dynastic effect is evident also in these subsamples, this cannot be explained by the observed discontinuities in age and experience.

We report the RDD estimates in Figure 1.7, Figure 1.8 and in Tables 1.7 and 1.8. Specifically, Figure 1.7 reports the differences in average expenditures and average revenues between dynastic and non-dynastic mayors, as a function of the margin of the best dynastic candidate. While Figure 1.8 reports the difference between the last year of the term and the average of the previous years. The results suggest that the variation in capital expenditures and capital transfers is clearly discontinuous at the threshold, and markedly higher for dynastic mayors. Conversely, there are no clear differences for average expenditures and revenues. Tables 1.7 and 1.8, which report estimates of the discontinuity of these different variables at the threshold (following the same methodology as the one used for the covariates, and controlling respectively for order-one and order-two polynomials of the margin of dynastic mayors), confirms the graphical representation: capital expenditures and capital transfers per capita increase much more during the last year of the term for dynastic mayors. Specifically, the difference in variation between dynastic and non-dynastic mayors is about 150 to 190 euros per capita for both capital expenditures and capital transfers. The bottom panels of Table 1.7 and Table 1.8 show that including control variables in the estimation does not seem to affect our estimates. Finally, in Table 1.9 we report the RDD on different subsamples. In the top panel, we compare young

21. However, in this framework, controlling for observed covariates has a limited effectiveness. As emphasized by Calonico et al. 2016, controlling for observed covariates helps improving the consistency of the estimation only if the continuity of the potential outcome is likely to hold. These authors further argue that controlling for interactions between covariates and treatments is likely to improve the consistency of the estimation only in very restrictive situations.

Table 1.7: Discontinuity of Average Budget and PBCs - Order 1 Polynom

Without Covariates						
Order 1 Polynom	Total exp	Current exp	Capital exp.	Tax rev	Loans	Transfers
Dynasty	92.450 (97.246)	4.842 (37.293)	79.400 (67.270)	13.878 (17.612)	18.991 (18.975)	76.948 (61.186)
Bandwidth	0.211	0.186	0.193	0.217	0.219	0.196
N (left)	1066	948	988	1093	1104	1001
N (right)	948	863	888	965	972	900
	Δ Total exp	Δ Current exp	Δ Capital exp	Δ Tax rev	Δ Loans	Δ Transfers
Dynasty	155.417* (85.813)	-12.911 (10.292)	172.010** (80.620)	0.305 (10.947)	52.848* (27.134)	166.523** (67.137)
Bandwidth	0.167	0.196	0.171	0.219	0.188	0.177
N (left)	843	976	857	1079	937	876
N (right)	791	890	803	961	869	821
With Covariates						
Order 1 Polynom	Total exp	Current exp	Capital exp	Tax rev	Loans	Transfers
Dynasty	148.277 (102.634)	29.666 (37.470)	75.332 (69.367)	32.609** (15.755)	16.339 (19.310)	79.356 (63.829)
Bandwidth	0.160	0.178	0.155	0.218	0.213	0.156
N (left)	821	892	804	1079	1048	807
N (right)	748	805	725	932	913	726
	Δ Total exp	Δ Current exp	Δ Capital exp	Δ Tax rev	Δ Loans	Δ Transfers
Dynasty	174.232** (86.426)	-13.341 (10.396)	183.721** (81.041)	4.525 (10.378)	63.433** (27.661)	157.789** (65.842)
Bandwidth	0.164	0.193	0.169	0.238	0.172	0.195
N (left)	812	949	838	1122	849	956
N (right)	751	850	769	989	782	853

The table presents the results of an RD estimation with an optimal bandwidth calculated using the Calonico, Cattaneo, and Titiunik (2014) method, which employs triangular kernel and controls for an order-one polynom of the margin of victory of the best dynastic candidate. Dependent variables are the average of categories of expenditures and revenues over the term (top part of each panel), and the differences of categories of expenditures and revenues between the last year and the average of the first 3 years (bottom part of each panel). All variables are winsorized at the 1% level. The sample consists of all full 5-year mayoral terms, for election years between 1999 and 2012. The first panel includes no covariates, while the second panel controls for term-limit, experience, age, place of birth, sex and years of education of the mayor, mean population and unemployment in the city, as well as dummies indicating whether the mayor is from a civic party and whether the city is in the South of the country. Robust standard errors clustered at the city level in parentheses.

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

Table 1.8: Discontinuity of Average Budget and PBCs - Order 2 Polynom

Without Covariates						
Order 2 Polynom	Total exp	Current exp	Capital exp	Tax rev	Loans	Transfers
Dynasty	148.937 (121.730)	-8.355 (38.965)	91.057 (79.409)	37.122 (23.879)	20.297 (23.856)	91.561 (72.737)
Bandwidth	0.219	0.323	0.231	0.209	0.243	0.239
N (left)	1104	1420	1150	1061	1188	1174
N (right)	972	1242	1019	942	1056	1043
	Δ Total exp	Δ Current exp	Δ Capital exp	Δ Tax rev	Δ Loans	Δ Transfers
Dynasty	167.455* (97.754)	-13.041 (12.147)	172.886* (89.394)	-1.737 (12.693)	50.537 (31.226)	193.545** (79.063)
Bandwidth	0.235	0.279	0.252	0.310	0.271	0.224
N (left)	1135	1260	1187	1347	1239	1101
N (right)	1016	1117	1062	1199	1102	986
With Covariates						
Order 2 Polynom	Total exp	Current exp	Capital exp	Tax rev	Loans	Transfers
Dynasty	162.249 (119.631)	3.910 (38.349)	76.821 (77.668)	54.062** (21.302)	16.190 (23.645)	79.599 (71.763)
Bandwidth	0.209	0.329	0.220	0.218	0.247	0.222
N (left)	1040	1403	1083	1079	1177	1095
N (right)	907	1207	939	932	1025	950
	Δ Total exp	Δ Current exp	Δ Capital exp	Δ Tax rev	Δ Loans	Δ Transfers
Dynasty	189.197* (98.401)	-13.656 (12.328)	193.428** (91.028)	1.218 (13.061)	63.369** (31.305)	202.975** (80.207)
Bandwidth	0.231	0.268	0.241	0.294	0.254	0.222
N (left)	1102	1209	1131	1279	1168	1074
N (right)	971	1056	1001	1113	1029	943

The table presents the results of an RD estimation with an optimal bandwidth calculated using the Calonico, Cattaneo, and Titiunik (2014) method, which employs triangular kernel and controls for an order-two polynomial of the margin of victory of the best dynastic candidate. Dependent variables are the average of categories of expenditures and revenues over the term (top part of each panel), and the differences of categories of expenditures and revenues between the last year and the average of the first 3 years (bottom part of each panel). All variables are winsorized at the 1% level. The sample consists of all full 5-year mayoral terms, for election years between 1999 and 2012. The first panel includes no covariates, while the second panel controls for term-limit, experience, age, place of birth, sex and years of education of the mayor, mean population and unemployment in the city, as well as dummies indicating whether the mayor is from a civic party and whether the city is in the South of the country. Robust standard errors clustered at the city level in parentheses.

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

dynastic and young non-dynastic mayors. In this case, previous findings are not only confirmed, but reinforced, since the estimated coefficients are much higher. Similar findings arise from the second panel, comparing inexperienced mayors. The higher estimated effect suggests that career concerns might play a role: young dynastic mayors might be more interested in pursuing a political career, and in turn, enforcing PBCs. Also, based on Signaling Models, it might be that inexperienced non-dynastic mayors lack the necessary skills to enforce such strategic spending. Nevertheless, these results validate that previous findings are not driven by the fact that dynastic politicians are younger and less experienced. Finally, in the bottom panel, we report the RDD estimates dropping common surnames (the 100 most common in the province). Also in this case, previous findings are confirmed. We report in the Web Appendix similar tests using other surnames' cutoffs.²²

Overall these results therefore suggest that dynastic mayors increase spending in a pre-electoral year, financing it through higher capital transfers. As we observe a political budget cycle also for non-dynastic mayors, it appears that dynastic leaders are more able to enforce this strategic policy, in line with the idea that they might have higher ability or higher gains from politics. In the next section, we provide some suggestive evidence in line with this interpretation.

5 Channels

In this section, we show that the difference in political budget cycles vary depending upon electoral incentives, suggesting that dynastic politicians are either better at or more willing to keep power for themselves. However distinct, these two hypotheses are difficult to disentangle empirically, since the reasons why dynastic politicians are better at holding on to power might be the same as the ones helping them extracting more gains from the political process.

Finally, we do not find any evidence supporting the fact that political budget cycles are higher when there are incentives to transmit power to another member of the family. This finding suggests that political budget cycles are rather used to keep power for oneself rather

22. Finally, in the Web Appendix, we test whether dynastic mayors use their current expenditures on different items (both on average and in pre-electoral years): we find few differences which are, if anything, of small magnitude and not robust to different specifications.

Table 1.9: PBC: Robustness checks

Without Covariates (Second-order polynomial)						
Young	Δ Total exp	Δ Current exp	Δ Capital exp	Δ Tax rev	Δ Loans	Δ Transfers
Robust	331.855** (161.421)	-6.472 (17.793)	319.472** (145.355)	5.154 (15.850)	81.139** (40.734)	323.800*** (125.495)
Bandwidth	0.231	0.234	0.241	0.349	0.303	0.223
N (left)	483	489	498	606	565	473
N (right)	541	544	555	694	643	526
Inexperienced	Δ Total exp	Δ Current exp	Δ Capital exp	Δ Tax rev	Δ Loans	Δ Transfers
Dynasty	260.650* (155.746)	-14.798 (18.569)	318.366** (142.130)	-7.547 (19.174)	73.049* (42.778)	309.291** (126.193)
Bandwidth	0.219	0.246	0.202	0.279	0.293	0.186
N (left)	389	416	362	443	455	326
N (right)	688	748	655	796	821	613
Uncommon name	Δ Total exp	Δ Current exp	Δ Capital exp	Δ Tax rev	Δ Loans	Δ Transfers
Dynasty	159.222 (116.743)	-11.178 (12.909)	185.824* (107.316)	-5.888 (16.477)	49.615 (40.289)	207.217** (93.342)
Bandwidth	0.281	0.322	0.310	0.293	0.264	0.264
N (left)	740	806	783	755	719	720
N (right)	664	717	704	681	645	645

The table presents the results of an RD estimation with an optimal bandwidth calculated using the Calonico, Cattaneo, and Titiunik (2014) method, which employs a triangular kernel and controls for an order-two polynomial of the margin of victory of the best dynastic candidate. Dependent variables are the differences of categories of expenditures and revenues between the last year and the average of the first 3 years, winsorized at the 1% level. No covariates are included. The sample consists of all full 5-year mayoral terms, for election years between 1999 and 2012. The first panel restricts the analysis to mayors who are younger than the median age observed in the sample. The second panel restricts the analysis to mayors who have less experience in city council than the median experience observed in the sample. The third panel restricts the sample to elections where no candidate had a name among the 100 most common at the province level. Robust standard errors clustered at the city level in parentheses.

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

Table 1.10: Term limits and PBCs: Fixed-effects

	Total exp	Current exp	Capital exp	Tax rev	Loans	Cap. transfers
Dynasty	11.147 (22.806)	-0.792 (4.867)	4.653 (19.201)	1.228 (2.223)	8.002 (5.746)	-0.448 (17.785)
LY	48.167 (11.526)***	-1.555 (1.671)	39.270 (10.474)***	-8.552 (1.059)***	13.967 (3.476)***	23.296 (9.383)**
TL	-2.549 (12.175)	4.660 (2.753)*	-10.117 (10.434)	-3.110 (1.333)**	-1.250 (3.228)	-2.551 (9.566)
Dynasty*LY	62.304 (21.871)***	7.873 (2.829)***	60.294 (20.336)***	7.501 (2.014)***	18.991 (6.505)***	45.949 (18.598)**
LY*TL	-9.281 (16.107)	2.530 (2.293)	-8.210 (14.679)	8.161 (1.505)***	-3.205 (4.698)	-9.945 (12.863)
Dynasty*TL	11.320 (26.941)	9.872 (6.123)	13.177 (23.221)	2.377 (2.907)	-9.270 (8.053)	14.829 (21.230)
Dynasty*TL*LY	-35.181 (38.241)	-1.393 (4.746)	-50.090 (34.670)	-5.115 (3.425)	-18.732 (10.697)*	-48.884 (30.600)
R^2	0.02	0.24	0.02	0.42	0.01	0.01
N	47,420	47,420	47,420	47,418	47,416	47,416

The table presents estimates from fixed-effects panel regressions, using categories of public expenditures and income as dependent variables (all are expressed in euros per capita, and winsorized at the 1% level). The main explanatory variables are three dummies indicating (1) whether the mayor is dynastic, (2) whether the mayor is term-limited (3) whether it is the last year in the mayor's term. All outcome variables are expressed in euros per capita. The sample is made of all cities where two full terms of five years were observed between 1999 and 2012. Election years are excluded from the estimation. All specifications control for city and year fixed effects, as well as population size and the mayor's sex, age, experience, years of education, birthplace and term-limits. Standard errors are clustered at the city level. Standard errors between brackets.

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

than for easing the transmission of power to family members.

5.1 Electoral Incentives

Binding term limits

Italian mayors cannot hold the office more than two terms in a row. Therefore, incentives for reelection apply only to mayors in their first term. If political budget cycles are used as a tool for reelection, we should observe higher PBC for mayors in their first term. In Table 1.10, we test the presence of different pre-electoral spending among term-limited and non-term-limited mayors using the panel fixed-effects estimation.²³ The coefficient of interest is the triple interaction ($Dynasty*LY*TL$), where (TL) is a dummy equals to one for mayors

23. Such a comparison might suffer from selection effects in the reelection of incumbents. In particular, re-elected mayors, whether dynastic or not, might be better politicians and might not need to run big PBCs. While we do not find that PBCs have a differential effect on reelection when they are run by dynastic and non-dynastic mayors, we cannot rule out that some unobserved differences between term-limited and non-term limited explain the results below.

Table 1.11: Term limits and PBCs: RDD

Without Covariates (Second-order polynomial)						
No Term Limit	Δ Total exp	Δ Current exp	Δ Capital exp	Δ Tax rev	Δ Loans	Δ Transfers
Dynasty	196.430*	-18.614	204.538**	1.414	37.844	227.041***
	(107.048)	(14.874)	(98.337)	(14.520)	(36.336)	(84.801)
Bandwidth	0.215	0.254	0.217	0.303	0.255	0.206
N (left)	778	861	787	931	862	752
N (right)	734	820	742	895	821	721
Term Limit	Δ Total exp	Δ Current exp	Δ Capital exp	Δ Tax rev	Δ Loans	Δ Transfers
Dynasty	68.990	-1.469	133.952	0.103	91.508	64.113
	(261.928)	(22.410)	(248.339)	(29.229)	(71.510)	(201.588)
Bandwidth	0.343	0.301	0.357	0.297	0.274	0.374
N (left)	453	395	469	391	360	492
N (right)	321	285	327	277	261	345

The table presents the results of an RD estimation with an optimal bandwidth calculated using the Calonico, Cattaneo, and Titiunik (2014) method, which employs a triangular kernel and controls for an order-two polynomial of the margin of victory of the best dynastic candidate. Dependent variables are the differences of categories of expenditures and revenues between the last year and the average of the first 3 years, winsorized at the 1% level. The sample consists of all full 5-year mayoral terms, for election years between 1999 and 2012. Regressions are run separately on the sample of term-limited and non-term-limited elected mayors, and include no covariates. Robust standard errors clustered at the city level in parentheses.

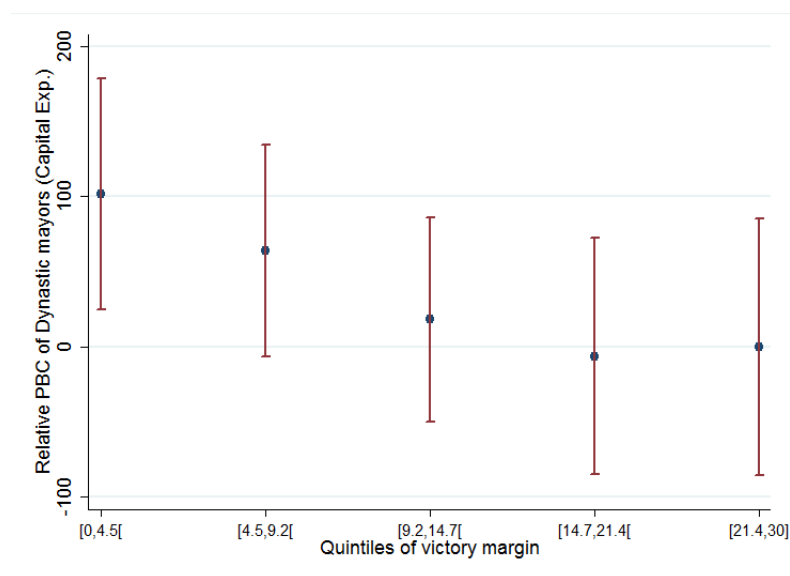
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

in their second term. The results suggest the dynastic mayors have a higher pre-electoral spending especially during their first term when they are eligible for re-running. Conversely, the negative coefficients of the triple interactions - although not significant - suggest that term-limited dynastic mayors do not behave differently from non-dynastic term-limited ones. This interpretation is suggested also by Table 1.11, where we replicate the RDD estimations on the subsample of first term (top panel) and second term (bottom panel) politicians. In this case, we observe statistically significant (and much higher) coefficients only for mayors in their first term, therefore eligible for rerunning.

Electoral competition

The key role of electoral incentives is further substantiated by the heterogeneity of political budget cycles with respect to electoral competition. In Figure 1.9, we plot the relative PBC of dynastic mayors in their first term, for different levels of political competition (as measured by their margin of victory in the previous election, therefore, in this case, we can rely only on the fixed effects estimation). We consider elections with margins of less than 30%, and decompose them into quintiles. The results suggest that PBC (in terms of capital expenditures) are relatively higher for dynastic politicians when political competition is higher - i.e. when

Figure 1.9: Political Budget Cycles and Margin of victory



The figure represents the interaction coefficient $Dynasty*LY$ (with confidence intervals at the 10% level) for fixed-effects regressions run on subsamples corresponding to quintiles of the margin of the winner in the previous election.

the mayor in place won by a narrow margin (for similar results, see Labonne 2016).²⁴ Note that this test might explain why we estimated such higher coefficients in the RDD than in the fixed-effects estimation.

5.2 Intergenerational Transmission of Power and Dynasty Founders

In the previous section, we discussed whether PBCs are used differently depending on the individual electoral incentives. However, political budget cycles might be used as well to for "family" electoral incentives, i.e. to transmit power to another member of a family.

In Table 1.12, we test whether PBCs are higher when a family member is running in the next election, and the mayor himself does not run again for election. The results clearly indicate that the political budget cycles are not relatively higher in this case. Such a finding, combined

24. An alternative interpretation of this result would be that it signals higher rent-seeking from dynastic mayors when their power is challenged: if mayors know they have fewer chances of being reelected, they might engage relatively more in rent-seeking activities. However, it is not clear why such rent-seeking motives should be channeled through higher expenditures and transfers only at the end of the term. Instead, one might think that it should translate into higher expenditures and transfers throughout the whole term, which is not what we observe, even under high political competition. Finally, note also that we do not find different effects when focusing on areas where corruption is more likely to take place, i.e. areas with a strong presence of organized crime (as proxied by two indicators of organized crime at the city level: the number of seized firms/houses to organized crime and the number mafia casualties). These results are available upon request."

Table 1.12: PBC and Member of Family Running Immediately After

	Total exp	Current exp	Capital exp	Tax rev	Loans	Cap. transfers
Fam. Candidate	135.650 (122.098)	-17.083 (17.031)	112.527 (93.897)	-4.440 (7.773)	30.434 (19.829)	79.844 (82.083)
LY	70.366 (16.298)***	7.365 (2.431)***	57.150 (13.551)***	-1.561 (1.379)	10.663 (4.303)**	38.122 (12.345)***
Fam. Candidate*LY	39.427 (163.283)	-14.686 (14.322)	37.746 (130.703)	-14.226 (11.479)	1.484 (29.751)	77.578 (116.397)
R^2	0.03	0.20	0.02	0.51	0.01	0.02
N	23,881	23,881	23,881	23,876	23,875	23,875

The table presents estimates from fixed-effects panel regressions, using categories of public expenditures and income as dependent variables (all are expressed in euros per capita, and winsorized at the 1% level). The main explanatory variables are three dummies indicating (1) whether the mayor presumably has a member of his family running for office after him and (2) whether it is the last year in the mayor's term. All outcome variables are expressed in euros per capita. The sample is made of all cities where two full terms of five years were observed between 1999 and 2012, and restricts to mayors who did not run again for office. Election years are excluded from the estimation. All specifications control for city and year fixed effects, as well as population size and the mayor's sex, age, experience, years of education, birthplace and term-limits. Standard errors are clustered at the city level. Standard errors between brackets.

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

with the absence of differential political budget cycles for term-limited mayors, suggest that PBC are unlikely to be a tool for intergenerational transmission of power²⁵.

Furthermore, it is relevant for our hypothesis to test whether "founders" of political dynasties enforce, as well as their successors, higher pre-electoral spending. In fact, if the gains from being in office and/or the inherited political skills – and therefore their incentives to remain in office – are higher precisely because of the legacy of their predecessors, we expect to find no significant difference between the "founders" of political dynasties and other non-dynastic mayors. The results gathered in Table 1.13 (fixed effects) and Table 1.15 (RDD) confirm this prediction. Using the full-sample of observations, we test simultaneously whether first-generation mayors and dynastic mayors have higher PBC than non-dynastic mayors. The results show that while dynastic mayors indeed have higher political budget cycles than non-dynastic mayors, dynastic "founders" are not significantly different from the latter. Table 1.15 shows similar results in a RDD framework, in this case we compare closely elected "founders" with closely elected non-founders and non-dynastic mayors.²⁶

25. For this test, we report only fixed effects estimates as we do not have enough observations to estimate a RDD.

26. This RDD specification closely resembles previous ones. All RDD assumptions are respected in this case (results available upon request).

Table 1.13: PBC, First Generations and Dynastic mayors

	Total exp	Current exp	Capital exp	Tax rev	Loans	Cap. transfers
Dynasty	17.830 (22.104)	2.658 (4.709)	6.882 (18.632)	1.979 (2.150)	6.763 (5.521)	3.637 (17.264)
First Gen.	12.799 (19.520)	3.266 (4.419)	-7.530 (16.579)	-0.538 (2.140)	3.276 (5.003)	-1.885 (15.419)
LY	37.914 (10.983)***	-2.462 (1.592)	32.296 (9.969)***	-5.991 (0.954)***	10.611 (3.350)***	16.539 (8.730)*
LY*Dynasty	57.572 (17.634)***	8.990 (2.344)***	47.692 (16.453)***	5.796 (1.626)***	15.093 (5.353)***	32.971 (15.093)**
LY*First Gen.	22.913 (17.359)	6.379 (2.528)**	13.540 (15.759)	2.670 (1.443)*	7.899 (5.323)	10.198 (14.300)
R^2	0.02	0.24	0.02	0.42	0.01	0.01
N	47,420	47,420	47,420	47,418	47,416	47,416

The table presents estimates from fixed-effects panel regressions, using categories of public expenditures and income as dependent variables (all are expressed in euros per capita, and winsorized at the 1% level). The main explanatory variables are dummies indicating (1) whether the mayor is dynastic, (2) whether the mayor is a dynasty-founder (meaning that an individual with the same name as the mayor is elected afterwards) (3) whether it is the last year in the mayor's term. All outcome variables are expressed in euros per capita. The sample is made of all cities where two full terms of five years were observed between 1999 and 2012. Election years are excluded from the estimation. All specifications control for city and year fixed effects, as well as population size and the mayor's sex, age, experience, years of education, birthplace and term-limits. Standard errors are clustered at the city level. Standard errors between brackets.

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

6 Performance

The evidence provided so far stresses that dynastic leaders behave differently in office, strategically increasing public spending – and attracting transfers – in pre-electoral years. A final relevant question is whether dynastic politicians, being more able and/or more interested in staying in office, are also “better” in terms of maximizing citizens' welfare. We address this point in Table 1.14 where we test whether dynastic mayors perform better on a set of outcomes, which we believe are indicators of good governance. First, we include the length of their term, in which shorter terms indicate a higher probability of early termination. This is a proxy for political instability as it measures the mayor's ability to hold his office until the end of the electoral term (Daniele, Galletta, Geys, et al. 2017).²⁷ Second, we consider their ability to collect revenue (i.e. the ratio between collected and assessed revenue within the year) and to reimburse public debt on time (i.e. the ratio between paid and committed outlays within the year). They are both considered efficiency indicators for the management of the municipal

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

Table 1.14: Competence of dynastic mayors

Fixed-Effects	Term length	Ability rev.coll.	Speed payment	Tax base growth	Corruption - Mayor
Dynasty	-0.005 (0.033)	-0.170 (0.313)	0.083 (0.185)	-0.009 (0.55)	-0.002 (0.001)
R^2	0.13	0.13	0.03	0.02	0.00
N	14,474	45,494	45,485	17,603	20,835
RDD	Term length	Ability rev.coll.	Speed payment	Tax base growth	Corruption - Mayor
Dynasty	-0.010 (0.123)	-0.657 (1.979)	0.227 (0.921)	0.006 (0.005)	-0.000 (0.002)
Bandwidth	0.250	0.300	0.287	0.154	0.147
N (left)	1389	1301	1270	431	655
N (right)	1234	1162	1128	385	599

The table presents estimates from fixed-effects panel regressions (first panel) and RDD regressions (second panel). Dependent variables are the mayors' average term length (measured in years), ability to collect revenue (measured as a ratio of collected revenue over expected revenue), speed of payment (measured as the share of due expenditures paid during the term), the yearly growth rate of the private tax base (measured in percentage points), the yearly presence of a corruption scandal (attributed to any member of the city council), and the yearly presence of a corruption scandal (attributed to the mayor). The main explanatory variable is a dummy indicating whether the mayor is dynastic. For the panel specification: In Column 1, observations include all observed terms which started between 1999 and 2008. Columns 2,3 include all cities for which two full terms were observed between 1999 and 2012, Column 4 includes all full terms between 2000 and 2011, and column 5 includes all terms (full and and not-full) which started between 2005 and 2011. In Column 1, observations are aggregated at the term level. Estimations of Columns 2 to 6 are at the yearly level. All specifications control for city and year fixed effects as well as population size, and for the mayor's sex, age, experience, years of education, birthplace and term-limit. For the RDD: In Column 1, observations include all observed terms which started between 1999 and 2008, Columns 2,3 include all full terms between 1999 and 2012, Column 4 includes all full terms between 2000 and 2011 and Column 5 includes all terms all terms (full and and not-full) which started between 2005 and 2011. The estimation is made with an optimal bandwidth calculated using the Calonico, Cattaneo, and Titiunik 2014 method, which employs a triangular kernel and controls for an order-two polynomial of the margin of victory of the best dynastic candidate. No controls are included in the RDD specification. In all cases, election years are excluded from the estimation. Robust standard errors clustered at the city level in parentheses.

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

government, barely affected by differences in political ideology across mayors (Gagliarducci and Nannicini 2013). Third, we look at the growth rate of the private-sector tax base, a proxy of the growth rate of the private sector (not considering the shadow economy). Fourth, we include a measure of observed corruption at the city level. Specifically, this is a dummy equal to one if a mayor is charged with criminal charges related to his political office, as reported by local news. This measure is available only for the period 2006-2012.²⁸ Overall, we find no clear effects of political dynasty on these variables, showing that dynastic mayors are unlikely to be more (or less) competent. In a nutshell, being more strategic does not make them “better” or “worse” politicians.²⁹

28. We thank Tommaso Giommoni (Bocconi University) for sharing with us his proprietary data on Italian local politicians criminal charges. The data have been collected systematically analyzing Italian local news (through the platform Factiva) reporting keywords related to criminal charges linked to the name of a local politician. If a mayor is charged after the conclusion of his office for a crime taking place while he was in office, we consider the charge valid for the period in which the mayor was in office. For additional details on this measure, see Giommoni 2017.

29. A related question is whether PBCs are effective in terms of increasing the probability of being re-elected. However, the likelihood of observing a higher PBC is endogenous, for instance, as shown in Section 5, it depends not only on being dynastic but also on other dimensions, as electoral incentives. Therefore, our framework only allows testing the presence of a correlation between PBCs and electoral performances. We deal with this point in the Web Appendix, where we do not find a clear link between PBCs and electoral performance.

Table 1.15: Founders of dynasties: RDD

Without Covariates						
Order 2 Polynom	Δ Total exp	Δ Current exp	Δ Capital exp	Δ Tax rev	Δ Loans	Δ Transfers
First Generation	-137.975 (101.387)	-2.820 (11.431)	-124.519 (96.099)	-17.155 (12.872)	-14.853 (28.091)	-53.243 (79.039)
Bandwidth	0.226	0.301	0.222	0.233	0.249	0.233
N (left)	970	1176	953	985	1039	986
N (right)	884	1066	867	903	941	904

With Covariates						
Order 2 Polynom	Δ Total exp	Δ Current exp	Δ Capital exp	Δ Tax rev	Δ Loans	Δ Transfers
First Generation	-112.947 (101.505)	4.394 (12.161)	-97.844 (96.611)	-8.714 (12.799)	-9.748 (28.199)	-42.411 (80.251)
Bandwidth	0.220	0.260	0.215	0.233	0.242	0.224
N (left)	927	1041	908	963	998	938
N (right)	854	959	842	893	912	866

The table presents the results of an RD estimation with an optimal bandwidth calculated using the Calonico, Cattaneo, and Titiunik (2014) method, which employs triangular kernel and controls for an order-two polynomial of the margin of victory of the best first-generation candidate. Dependent variables are the differences of categories of expenditures and revenues between the last year and the average of the first 3 years, winsorized at the 1% level. The sample consists of all full 5-year mayoral terms, for election years between 1999 and 2012. Covariates include experience, age, place of birth, sex and years of education of the mayor, average population population and unemployment in the city, as well as dummies indicating whether the mayor is from a civic party and whether the city is in the South of the country. Robust standard errors clustered at the city level in parentheses

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

7 Are dynastic politicians really different?

In this section we provide a comprehensive set of results about an important assumption of our analysis. We assumed, based on previous studies, that dynastic politicians are electorally more competitive. In this section, we provide evidence showing that this is the case also among Italian dynastic mayors. Firstly, we find that dynastic candidates have longer political careers and are electorally more successful, as they are more likely to win local elections and to get elected in provincial/regional parliaments. Secondly, we show that, as previously shown in the US, Argentina and the Philippines (Dal Bó, Dal Bó, and Snyder 2009; Rossi 2017; Querubin 2013), also in Italy dynasties self-perpetuate in the political arena.

Overall, this set of results confirms that dynastic leaders substantially differ from other politicians in terms of electoral performance. This provides a strong motivation for our interest in investigating whether their policy making is different as well.

7.1 Electoral Performance

Firstly, we focus on the electoral performance of dynastic candidates. Table 1.16 uses a linear probability model to predict dynastic candidates' probability of being elected. Specifically, Columns 1 and 2 consider the set of candidates to elections for which information about at least the two best candidates is known. All columns include city and year fixed effects; we also add to the previous set of control variables the number of candidates running for mayor. Column 1 suggests that being a dynastic candidate has a positive impact on the probability of being elected (of about 3.5 percentage points). Moreover, this effect does not change when considering incumbent dynastic politicians (see the interaction $\text{Dynasty} \times \text{Incumbent}$). The same does not hold concerning the interaction with experience (Column 2): among candidates with at least one term of experience on the municipal council, dynastic candidates are not more likely to be elected, but among inexperienced candidates, they are 3–4 points more likely to be elected. Therefore, the dynastic advantage seems to somehow decrease depending on a candidate's level of political experience. This might be due to the fact that non-dynastic politicians also acquire some of the "inherited" skills over time. In this light, as shown in the previous section, the PBCs differences between dynastic and non-dynastic mayors are especially high for the subsample of inexperienced politicians.

Not only is political power persistent over generations but elected individuals who come from political dynasties are also likely to serve longer on municipal councils. In Table 1.16, Column 3, we regress their political experience (measured by the number of years as elected local politician) for each observed mayor or municipal council member on various characteristics. Specifically, we include only those who first entered in politics in 1995 or after (in order to observe a longer period to determine their first election to a council). As above, we include city and year fixed effects. From Column 3, we find that the average number of years an individual spent in a municipal council is higher for dynastic individuals than for non-dynastic individuals (about two additional months).

Table 1.16: Electoral performances and longevity of dynastic politicians

	Elected	Elected	Longevity	Provincial Admin.	Regional Admin.
Dynasty	0.035 (0.007)***	0.002 (0.011)	0.09077 (0.01088)***	0.00084 (0.00043)*	0.00038 (0.00024)
Incumbent	0.401 (0.008)***	0.404 (0.007)***			
Dynasty*Incumbent	-0.003 (0.015)				
Number of candidates	-0.041 (0.002)***	-0.041 (0.002)***			
Years of experience in council	0.010 (0.000)***				
Years of education	0.009 (0.001)***	0.009 (0.001)***	0.02956 (0.00160)***	0.00129 (0.00006)***	0.00031 (0.00003)***
Male	0.062 (0.009)***	0.067 (0.009)***	0.49856 (0.01158)***	0.00572 (0.00041)***	0.00085 (0.00026)***
Born in City	0.029 (0.006)***	0.032 (0.006)***	0.22332 (0.01265)***	0.00087 (0.00053)	-0.00003 (0.00031)
Age	-0.003 (0.000)***	-0.003 (0.000)***	-0.01278 (0.00048)***	-0.00022 (0.00002)***	-0.00009 (0.00001)***
Civic	0.019 (0.006)***	0.017 (0.006)***	0.04172 (0.01643)**	-0.00227 (0.00050)***	-0.00031 (0.00024)
Name frequency in province	0.004 (0.019)	0.006 (0.019)			
No Experience		-0.148 (0.006)***			
Dynasty*No Exp		0.041 (0.013)***			
R^2	0.16	0.17	0.32	0.04	0.04
N	51,216	51,216	291,988	291,988	291,988

The table reports estimates from linear regressions. The outcome variable of Columns 1 and 2 is a dummy variable indicating whether a candidate to an election between 1993 and 2012 is dynastic as a dependent variable, restricting the sample to cities where information about at least two candidates was known. In Column 3, the outcome variable is the observed longevity of a politician since in its first election in the municipal council. In Columns 4 and 5, the outcome variables are dummies indicating respectively whether an individual entered provincial and regional administration after its first entrance in a municipal council. In these three columns, we restrict the sample to politicians who were appointed or elected after 1995. All specifications control for city and year fixed effects. Standard errors are clustered at the city level. Standard errors in parentheses.

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

Dynastic politicians have more successful careers not only in terms of duration but also in terms of climbing the political ladder: they are more likely to be elected to provincial or regional parliaments after serving on a municipal council. The results are reported in Columns 4 and 5 of Table 1.16. Using the same specifications as those of Column 3, we define as a dependent variable a dummy equal to 1 if a municipal politician is later elected to the provincial or regional parliament (within the same region).

Among members of municipal council appointed or elected after 1995, only 1% were subsequently elected to a provincial parliament, and about 0.26% of all observed members were part of a regional parliament. However, after controlling for year and city fixed effects and other individual characteristics, a dynastic politician has a higher probability than a non-dynastic politician of entering a provincial administration of about 0.08 percentage points (corresponding to about 8% of the sample average). For regional parliaments, even though the difference in probabilities is not significant, it corresponds to about 0.04 percentage points (which corresponds to approximately 15% of the sample average).³⁰

7.2 Persistence of political dynasties

Another important feature of political dynasties is that they seem to persist over time (Dal Bó, Dal Bó, and Snyder 2009 and Querubin 2013). In this section, we show that power seems to self-perpetuate, as elected individuals are more likely than non-elected individuals to have a relative in office in subsequent years. To test for such persistence, we need to compare the probabilities that an elected candidate and a non-elected candidate will have a relative in office in subsequent years.

As in the previous analyses, we exploit an RDD since it allows us to isolate a pure persistence effect from potentially unobserved factors that can determine both the probability that an individual will be elected and the probability that one of his relatives will be elected. In this framework, we compute the margin of votes for each candidate of each election. For the winner of the election, the margin corresponds to the difference between the share of votes he received and the share of votes received by his best challenger. For all losing candidates of an

30. In the Web Appendix, we also document intergenerational persistence in terms of occupations, and find that the job category of a dynastic politician is very often similar to that of the first generation in the dynasty.

election, the margin corresponds to the difference between their share of votes and the share of votes received by the winner. This variable takes values between -1 and 1: a positive value indicates that the individual was elected mayor, while a negative value indicates that he was not elected. Furthermore, for each individual running for mayor between 1993 and 2002, we indicate whether an individual with the same surname was elected mayor (or as a municipal councilor) within the 10 years after the election he ran in.³¹

Figure 1.10a shows that the average probability that an individual's relative will be elected mayor within 10 years increases with his margin of votes and is discontinuous around 0: in other words, elected officials are more likely to subsequently have a relative in office than losing candidates. As the probability of having a relative elected within 10 years is discontinuously higher when the margin of votes is positive, to the extent that the margin of votes is continuous around zero³², a causal interpretation can be inferred. Figure 1.10b shows that losing candidates are much less likely to have a relative elected to municipal office in the next 10 years (and much less so if they lost the election by a large margin), but that there is no discontinuity of this variable around the zero threshold. In other words, mayors who are elected by a narrow margin are not much more likely to have a member of their family elected to the municipal council within the next 10 years than candidates who narrowly lose.

In the Web Appendix, we present detailed results from an RDD between closely elected and closely non-elected individuals, which confirm that closely elected candidates are significantly more likely to have a relative elected mayor during the subsequent 10 years, but are not more likely to have a relative elected to the municipal council. While raw comparisons of means across elected and non-elected candidates suggest that elected candidates are almost 50% as likely as non-elected candidates to have a relative elected mayor within 10 years (3.1% vs. 2.2%), the jump in the probability of a relative being elected mayor within 10 years at the zero cutoff is of the same magnitude and corresponds to about 1 percentage point: this suggests that being

31. We impose such a restriction because our sample of elections is from 1993 to 2012: therefore, for all elections during this time, a 10-year bandwidth ensures that the number of years considered after an election in the estimation is the same.

32. Note that other covariates predicting the electoral performances of candidates are also continuous around this threshold (results available upon request).

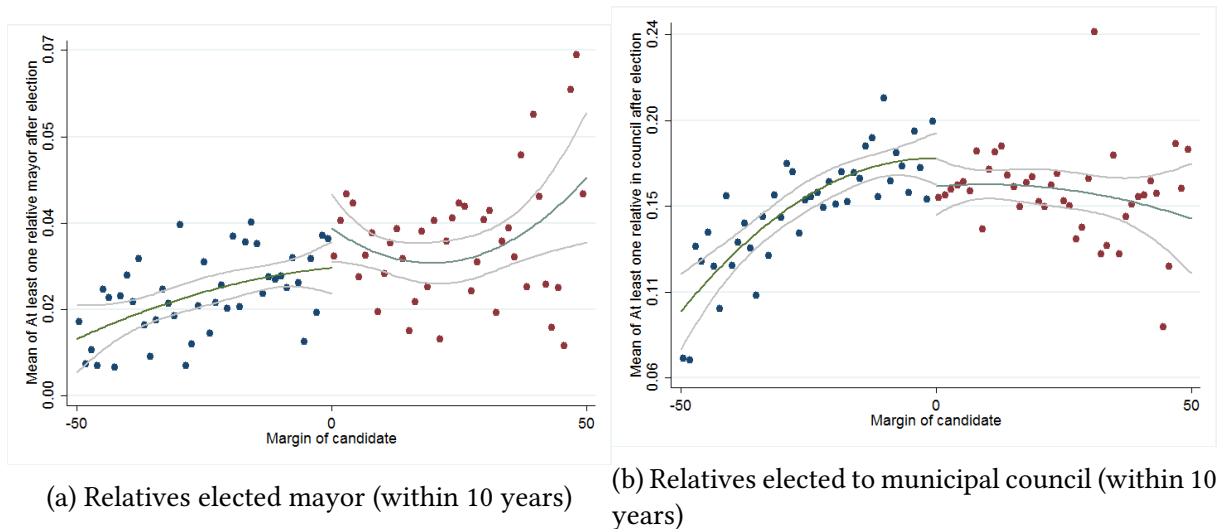


Figure 1.10: Perpetuation of power –RD

elected mayor increases the probability of having a relative elected mayor by about 50%.^{33 34}

8 Conclusion

In this paper, we provide several insights about the relevance of dynasties in the political arena. Our test is based on data from Italian municipalities in the period 1985–2012 (and on mayoral elections in the period 1998–2012). Our main contribution is a test of whether dynastic mayors perform differently than non-dynastic mayors. Such differences might be due to higher ability thanks to inherited political skills and/or higher gains from being in office. In line with such a hypothesis, we find that dynastic mayors spend more (on capital expenditure) – and receive more transfers – in the year before an election. We show several tests in line with such an interpretation and provide evidence about the different electoral performance of dynastic politicians, a crucial assumption of our main test. Conversely, we do not find

33. Note that the results on perpetuation for mayors are likely to be biased downwards: indeed, because mayors usually stay in office for 4 or 8 years, the probability that someone from the same family will be elected within 10 years is estimated for only the last 2 to 6 years of the 10-year window. As we show in the Web Appendix, extending the estimation to the full sample of candidates between 1993 and 2012, and without imposing a 10-year bandwidth, does not change the results.

34. The downward trend on the right-hand side of the graph about perpetuation in municipal councils is somewhat surprising: while individuals who lost elections by a large margin are very unlikely to later have relatives in the city council, we find that individuals who won by a large margin are less likely to have a relative in the city council than those who won by a narrow margin (yet we do observe a slightly upward perpetuation trend for the office of mayor). A potential explanation might be that mayors who have a strong grip on the city, and who want to transmit their power to their heirs, might have greater incentives to do so by helping them become mayor rather than a city councilor.

differences concerning other relevant dimensions as average revenue and expenditure, and a set of outcomes measuring mayors' performance.

The results of this paper enhance our understanding of the role played by families in contemporary democracies, which continue to have a significant role in politics across very different countries. In this light, this study contributes to the debate about inequality and the transmission of wealth and power across generations (Piketty 2013). We highlight that the dynamics of power transmission across generations have important political consequences, since dynastic politicians behave very differently in terms of both their electoral performance and their policy making. Although there are many potential explanations for the opportunistic behavior of dynastic mayors, the political skills and experience they inherit from their predecessors are likely to shape both their incentives to remain in office and the policies they implement to this end.

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Appendix

1.A Additional descriptive evidence about dynastic mayors

Age difference between dynastic politicians and dynastic founders

As explained in the main text and emphasized in Figures 1.A.1 and 1.A.2, we observe that the age difference between dynastic mayors and their first observed predecessor has a bimodal distribution (with a mode at 0 and a mode at around 30), and that the average age difference between them is increasing over time in our sample. This suggests that we are more likely to capture brotherhood linkages at the beginning of the sample, and father-and-son linkages at the end of the sample.

Figure 1.A.1: Histogram of age differences between dynastic mayors and their oldest predecessor

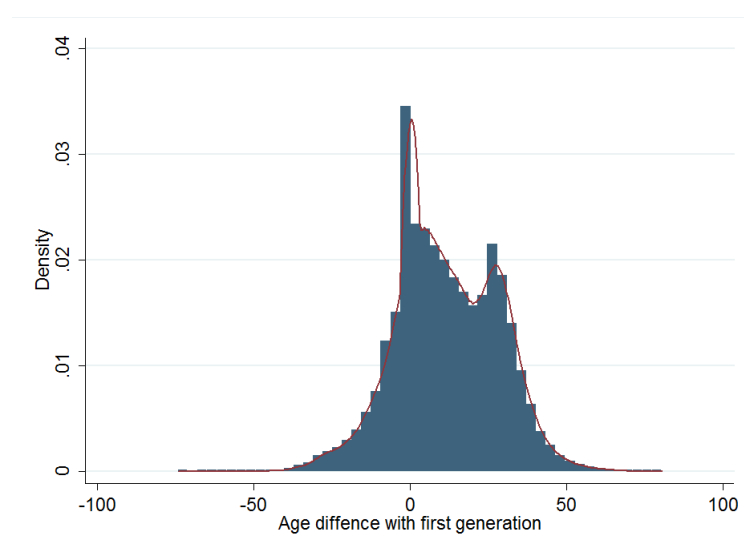
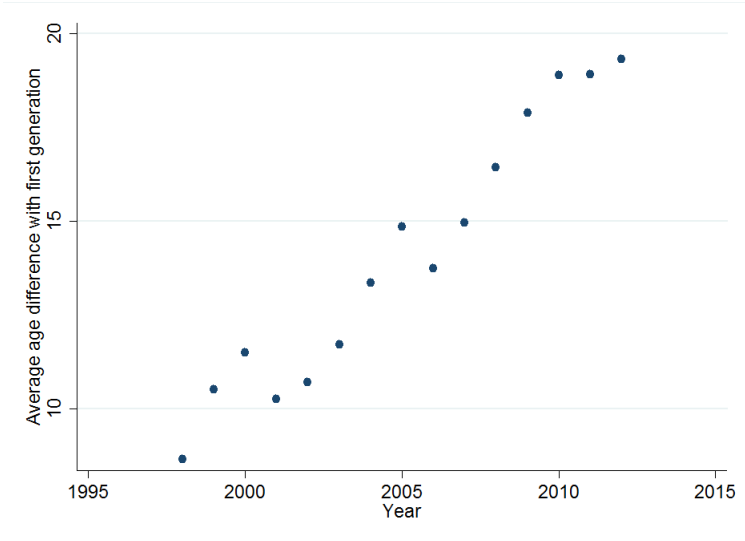


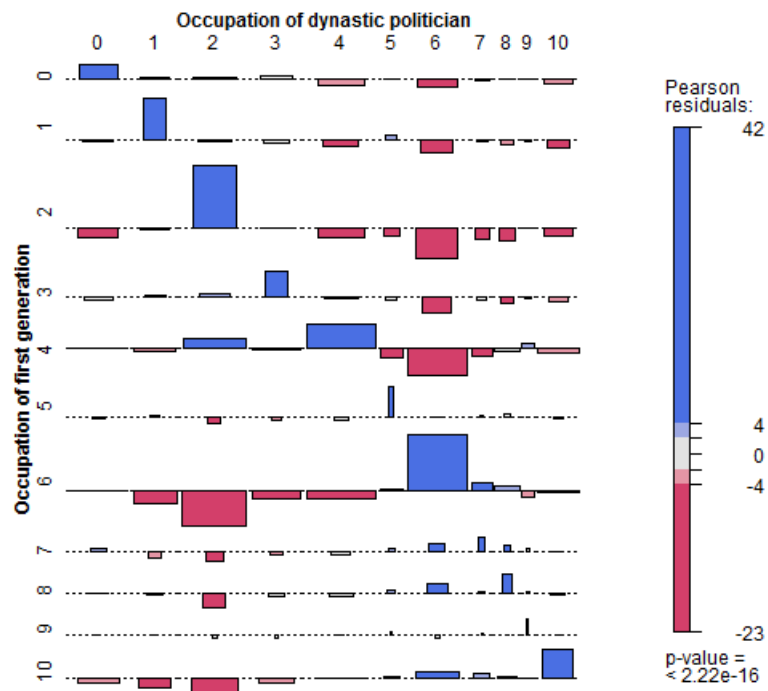
Figure 1.A.2: Evolution of age difference between dynastic mayors and their oldest predecessor



Occupational persistence

Not only is power persistent over time, but the occupations declared by mayors of a same family are also remarkably persistent. Figure 1.A.3 represents the transition matrix of occupations between the first observed generation of a family (in rows) and the first subsequent observed politician from this family (in column). Occupations are coded according to the socioprofessional categories of the Italian Census (where 1 indicates occupations requiring higher skills and 9 indicates occupations requiring lower skills, 0 indicates inactive people and 10 indicates retired). Blue (respectively red) bars indicate a significantly higher (respectively lower) proportion of the column category within the row category. From the graph, we can conclude that there is a strong intergenerational persistence of occupations, as most of the blue bars are on the diagonal. Furthermore, offspring of higher-occupation individuals are themselves more likely to be higher-skilled, while offspring of lower-occupation individuals are more likely to be lower skilled. Therefore, while this graph cannot help us concluding on whether intergenerational occupational persistence is higher among dynastic politicians than among non-dynastic politicians, it still emphasizes a strong persistence of occupations among members of political dynasties.

Figure 1.A.3: Persistence of occupations between generations



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

1.B Are political budget cycles useful for reelection ?

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

Table 1.B.1: Political budget cycle and reelection

	Reelection		Incumbency	
Dynasty	0.030 (0.016)*		0.001 (0.015)	
PBC Capital Exp.	0.011 (0.013)	-0.015 (0.017)	0.004 (0.012)	-0.005 (0.017)
Dynasty*PBC Capital Exp.	-0.025 (0.021)		-0.011 (0.020)	
Margin	0.002 (0.000)***	0.002 (0.001)*	-0.001 (0.000)***	-0.001 (0.001)
Years of experience in council	0.005 (0.001)***	0.005 (0.003)*	0.005 (0.001)***	0.007 (0.003)**
Male	0.168 (0.022)***	0.094 (0.043)**	0.210 (0.021)***	0.151 (0.041)***
Age	-0.012 (0.001)***	-0.012 (0.001)***	-0.010 (0.001)***	-0.010 (0.001)***
Born in city	-0.007 (0.014)	-0.008 (0.029)	0.004 (0.013)	-0.019 (0.027)
Population	0.000 (0.000)**	-0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Years of education	-0.004 (0.002)*	-0.004 (0.004)	-0.002 (0.002)	-0.003 (0.004)
Civic	-0.020 (0.014)	-0.056 (0.029)*	-0.038 (0.013)***	-0.075 (0.026)***
South	-0.040 (0.024)*	0.028 (0.041)	-0.052 (0.021)**	-0.012 (0.037)
Unemployment	-0.001 (0.001)	-0.003 (0.002)	0.000 (0.001)	0.002 (0.002)
Constant	0.923 (0.067)***	1.094 (0.132)***	0.859 (0.064)***	0.963 (0.127)***
R^2	0.08	0.09	0.07	0.09
N	5,324	1,394	5,520	1,453

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

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

1.C Estimating the persistence of power

The regression discontinuity methodology presented in this paper is a suitable setting for estimating the persistence of power. As explained in the text, we exploit exogenous variation in electoral margins to identify whether closely elected candidates are more likely to have a relative in office (whether mayor or simply in municipal council) during the following years. We test two specifications: in the first one, we estimate the probability of having a relative in office during the 10 years following any election between 1993 and 2002. In the second one, we estimate the probability of having a relative in office anytime any observed election between 1993 and 2014.

First of all, the margin of candidates does not seem to be discontinuous around the zero threshold, as is indicated in Figure 1.C.1a (which represents the density of all candidates at the municipal election of 1993-2002) and in Figure 1.C.1b (which represents the density of all candidates at the municipal election of 1993-2012).

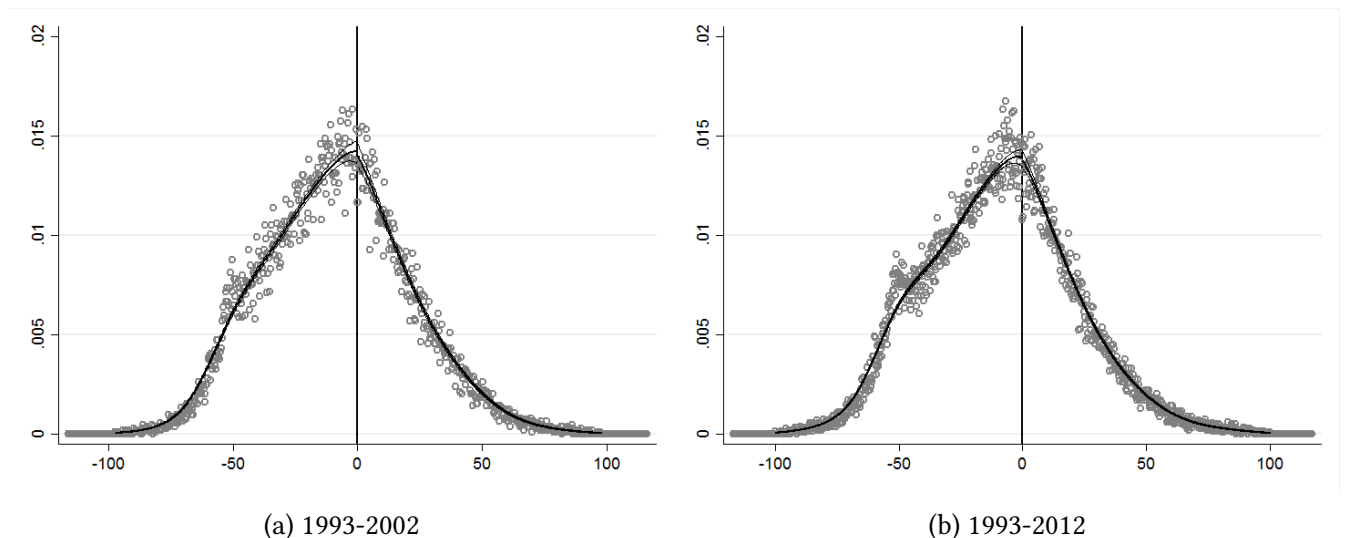


Figure 1.C.1: McCrary tests for the margin of victory of candidates

As for the estimation of political budget cycles, we estimate the discontinuity around the threshold using an optimal uniform bandwidth computed using the Calonico, Cattaneo, and Titiunik (2014) method, controlling for linear and quadratic polynomials of the forcing variable. Estimations are clustered at the city-individual level. The results are presented in Table 1.C.1.

The results are coherent with the graphical evidence presented above: we find a positive

Table 1.C.1: Probability that a relative enters in office

Relative elected	Within 10 years	Within 10 years	Anytime after	Anytime after
As Mayor	0.010 (0.006)	0.010 (0.007)	0.007* (0.004)	0.008* (0.004)
Bandwidth	17.917	29.681	16.936	30.373
Polynom	1	2	1	2
Years considered	1993-2002	1993-2002	1993-2012	1993-2012
N (left)	9696	14,989	17,784	29,234
N (right)	8045	11,197	14,962	21,879
In Council	0.003 (0.014)	0.004 (0.017)	-0.003 (0.009)	-0.002 (0.010)
Observations	39613	39613	77601	77601
Bandwidth	22.001	29.083	20.186	34.317
Polynom	1	2	1	2
Years considered	1993-2002	1993-2002	1993-2012	1993-2012
N (left)	11,638	14,750	20,842	32110
N (right)	9,312	11,082	16,996	23,298

The table presents results from regression discontinuity estimations. Estimations are ran using an optimal bandwidth calculated thanks to the Calonico, Cattaneo, and Titiunik (2014) method, using triangular bandwidth and controlling for polynoms of order 1 and 2 of the margin of victory of the best dynastic candidate. In columns 1 and 2, we estimate the probability that an individual running for an election between 1993 and 2002 has a relative in office within 10 years after the considered election. In columns 3 and 4, we estimate the probability that an individual running for an election between 1993 and 2012 has a relative in office anytime after this election. In the first panel, the dependent variable is a dummy indicating whether an individual had a relative elected mayor after his candidacy. In the second panel, the dependent variable is a dummy indicating whether an individual had a relative in municipal councils after his candidacy. Robust standard errors clustered at the city-individual level between brackets.

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

discontinuity for the probability that a relative becomes mayor (with an estimated jump between 0.7 and 1 percentage points), which is close from significance at the 10% level within 10 years after election, and significant at the 10% level anytime after election. However, consistently with the graphical evidence, we find no significant discontinuity around the forcing threshold for the probability that a relative enters the municipal council.

1.D Matching estimates

In this section, we show the results of a matching-on-discontinuity procedure, in the spirit of Alesina, Troiano, and Cassidy (2015). This methodology combines the strict treatment assignment provided by the threshold of the forcing variable and a propensity-score matching procedure which allows us controlling for the impact of potential confounding factors. In the spirit of Alesina, Troiano, and Cassidy (2015), we assume that for observations with values of X_i located in $] - b, b[$, if the following two conditions hold, then we can provide an unbiased estimate of the treatment (namely, the fact of having a dynastic mayor):

$$Y_i(0), Y_i(1) \perp D_i | Z_i \quad (1.1)$$

$$0 < P(D_i = 1 | Z_i) < 1 \quad (1.2)$$

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

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

In our estimation, we provide results for different bandwidths around the forcing threshold: namely, in order to check that the results are not specific to our choices of bandwidths, we report the results for 10 different bandwidths ranging from 4% to 40% (in absolute value around

the zero threshold). Note that the results for smaller bandwidths, are more likely to reveal causal effects, as they rely on closer elections where unobserved selection into dynastic mayors is less likely to be true. To the contrary the results obtained for wider ones are more general but less likely to be causal.³⁵ We use propensity score matching on all the observable variables studied above to control for confounding factors. In order to ensure balance between the matched treated and untreated observations, we match each dynastic mayor with his closest counterpart among non-dynastic mayors, discarding potential matches located outside of 0.2 standard deviation caliper.³⁶

Tables 1.D.1, 1.D.2 and 1.D.3 report the reduction of bias in control variables between matched and unmatched samples across dynastic and non-dynastic mayors. Overall, we find that our procedure significantly reduces the imbalances in age and experience (which were the most unbalanced covariates in the RD specification), while generally not increasing imbalances on other covariates. Figure 1.D.1 displays ATT estimates of Political Budget Cycles for our different variables of interest (defined as the difference between the last year of the term and the mean of the three first years) between dynastic and non-dynastic mayors, for 10 bandwidths between 4% and 40%. Our results are very similar from the RDD results: we find that dynastic mayors increase significantly more capital expenditures (and thus total expenditures) at the end of the term than non-dynastic mayors, and they finance it mainly through increased capital transfers and increased loans.

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

36. Note that the results also hold when we allow for multiple matching, where, for example, we associate to each dynastic mayor its 3 closest counterparts among non-dynastic mayor. Generally speaking, the choice of the number of neighbours comes from a tradeoff between bias and variance: indeed, increasing the number of matched pairs increases the amount of treated information (thus potentially increasing the accuracy of the estimated treatment effect), but increases the average distance between the compared treated and untreated units (thus potentially increasing the bias).

Table 1.D.1: Reduction of bias in propensity score matching for different bandwidths

Bandwidth	Unmatched Matched	Experience			Age			Male			Number obs.
		Dyn.	Non-Dyn.	P-val	Dyn.	Non-Dyn.	P-val	Dyn.	Non-Dyn.	P-val	
4%	U	5.05	10.98	0.00	46.68	50.14	0.00	0.90	0.94	0.13	392.00
	M	5.05	5.10	0.84	46.68	46.99	0.75	0.90	0.91	0.63	392.00
8%	U	5.30	11.33	0.00	47.12	49.66	0.00	0.90	0.94	0.02	806.00
	M	5.30	5.37	0.65	47.12	45.53	0.02	0.90	0.88	0.56	806.00
12%	U	5.38	11.20	0.00	47.23	49.59	0.00	0.90	0.94	0.05	1161.00
	M	5.38	5.42	0.78	47.23	46.54	0.22	0.90	0.93	0.14	1161.00
16%	U	5.48	11.24	0.00	46.98	49.53	0.00	0.90	0.94	0.01	1521.00
	M	5.48	5.43	0.61	46.98	45.87	0.03	0.90	0.88	0.15	1521.00
20%	U	5.48	11.10	0.00	46.92	49.38	0.00	0.90	0.93	0.02	1817.00
	M	5.49	5.43	0.48	46.95	46.20	0.11	0.90	0.89	0.35	1817.00
24%	U	5.47	11.09	0.00	46.84	49.30	0.00	0.91	0.93	0.10	2099.00
	M	5.47	5.35	0.10	46.84	46.26	0.18	0.91	0.89	0.19	2099.00
28%	U	5.58	11.08	0.00	46.70	49.26	0.00	0.90	0.93	0.04	2294.00
	M	5.58	5.45	0.07	46.70	46.45	0.56	0.90	0.89	0.24	2294.00
32%	U	5.66	11.25	0.00	46.71	49.34	0.00	0.91	0.93	0.03	2492.00
	M	5.66	5.51	0.02	46.71	46.09	0.13	0.91	0.88	0.04	2492.00
36%	U	5.71	11.26	0.00	46.80	49.43	0.00	0.91	0.93	0.04	2669.00
	M	5.71	5.62	0.17	46.80	46.22	0.12	0.91	0.89	0.11	2669.00
40%	U	5.77	11.30	0.00	46.72	49.46	0.00	0.91	0.93	0.04	2809.00
	M	5.77	5.67	0.13	46.72	46.43	0.44	0.91	0.88	0.05	2809.00

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

Table 1.D.2: Reduction of bias in propensity score matching for different bandwidths

Bandwidth	Unmatched Matched	Born in City			Education			South			Number obs.
		Dyn.	Non-Dyn.	P-val	Dyn.	Non-Dyn.	P-val	Dyn.	Non-Dyn.	P-val	
4%	U	0.57	0.53	0.46	15.12	14.69	0.21	0.43	0.38	0.34	392.00
	M	0.57	0.59	0.61	15.12	14.83	0.38	0.43	0.44	0.84	392.00
8%	U	0.59	0.53	0.08	14.99	14.70	0.21	0.43	0.41	0.53	806.00
	M	0.59	0.54	0.10	14.99	15.31	0.16	0.43	0.46	0.33	806.00
12%	U	0.61	0.54	0.01	14.87	14.73	0.47	0.43	0.42	0.99	1161.00
	M	0.61	0.60	0.53	14.87	14.86	0.97	0.43	0.39	0.20	1161.00
16%	U	0.60	0.55	0.07	14.88	14.76	0.48	0.41	0.41	0.90	1521.00
	M	0.60	0.54	0.01	14.88	14.78	0.56	0.41	0.40	0.68	1521.00
20%	U	0.58	0.54	0.08	14.85	14.68	0.28	0.41	0.40	0.70	1817.00
	M	0.58	0.56	0.25	14.85	14.64	0.18	0.40	0.39	0.56	1817.00
24%	U	0.57	0.54	0.14	14.85	14.69	0.28	0.40	0.40	0.83	2099.00
	M	0.57	0.52	0.01	14.85	14.80	0.73	0.40	0.37	0.13	2099.00
28%	U	0.56	0.54	0.22	14.84	14.71	0.33	0.39	0.39	0.97	2294.00
	M	0.56	0.51	0.02	14.84	14.81	0.80	0.39	0.38	0.42	2294.00
32%	U	0.56	0.54	0.24	14.82	14.62	0.14	0.38	0.38	0.95	2492.00
	M	0.56	0.48	0.00	14.82	14.69	0.34	0.38	0.35	0.17	2492.00
36%	U	0.56	0.53	0.20	14.71	14.60	0.38	0.37	0.37	0.82	2669.00
	M	0.56	0.50	0.00	14.71	14.71	0.95	0.37	0.34	0.18	2669.00
40%	U	0.56	0.53	0.22	14.70	14.57	0.31	0.37	0.36	0.72	2809.00
	M	0.56	0.52	0.03	14.70	14.69	0.94	0.37	0.38	0.40	2809.00

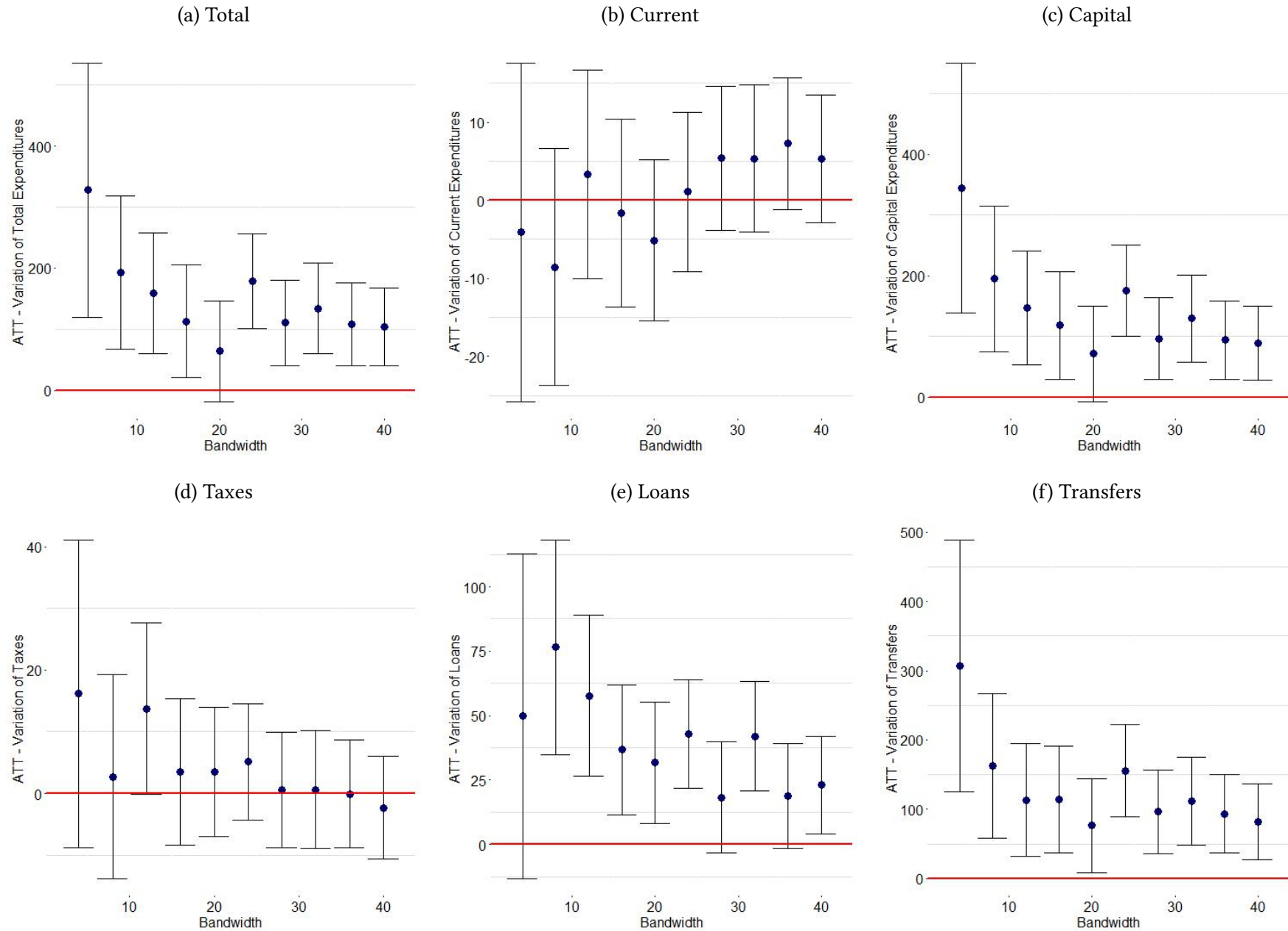
This table reports the average of the considered variables among dynastic and non-dynastic elected mayors, in unmatched and matched samples, as well as the p-values of tests of differences between means across dynastic and non-dynastic individuals.

Table 1.D.3: Reduction of bias in propensity score matching for different bandwidths

Bandwidth	Unmatched Matched	Civic List			Unemployment			Population			Term-limit			Number obs.
		Dyn.	Non-Dyn.	P-val	Dyn.	Non-Dyn.	P-val	Dyn.	Non-Dyn.	P-val	Dyn.	Non-Dyn.	P-val	
4%	U	0.64	0.66	0.77	11.54	10.65	0.31	4275.70	3754.91	0.40	0.18	0.22	0.35	392.00
	M	0.64	0.64	0.89	11.54	11.14	0.62	4275.70	4048.17	0.72	0.18	0.24	0.15	392.00
8%	U	0.65	0.62	0.44	11.85	11.32	0.40	4368.80	6131.84	0.26	0.18	0.22	0.26	806.00
	M	0.65	0.64	0.91	11.85	12.66	0.21	4368.80	4214.50	0.74	0.18	0.20	0.52	806.00
12%	U	0.64	0.62	0.43	11.86	11.59	0.61	4320.44	6134.85	0.10	0.19	0.22	0.27	1161.00
	M	0.64	0.65	0.62	11.86	11.24	0.25	4320.44	3931.25	0.22	0.19	0.20	0.74	1161.00
16%	U	0.65	0.62	0.33	11.75	11.34	0.37	4138.06	5844.24	0.05	0.21	0.25	0.09	1521.00
	M	0.65	0.68	0.14	11.75	11.59	0.73	4138.06	4418.97	0.34	0.21	0.21	0.87	1521.00
20%	U	0.64	0.63	0.56	11.63	11.08	0.18	4386.19	5536.79	0.12	0.21	0.26	0.01	1817.00
	M	0.64	0.62	0.40	11.60	10.69	0.02	4390.43	4257.79	0.66	0.21	0.21	0.98	1817.00
24%	U	0.65	0.63	0.41	11.53	11.07	0.23	4933.17	5614.62	0.34	0.23	0.28	0.01	2099.00
	M	0.65	0.66	0.50	11.53	10.90	0.11	4933.17	5078.47	0.77	0.23	0.22	0.80	2099.00
28%	U	0.66	0.63	0.20	11.37	10.92	0.22	5082.47	5550.88	0.50	0.24	0.29	0.00	2294.00
	M	0.66	0.65	0.66	11.37	10.90	0.20	5082.47	5335.68	0.74	0.24	0.24	0.99	2294.00
32%	U	0.66	0.63	0.14	11.22	10.64	0.10	5173.80	5477.35	0.64	0.25	0.31	0.00	2492.00
	M	0.66	0.64	0.60	11.22	10.82	0.25	5173.80	4949.58	0.68	0.25	0.24	0.60	2492.00
36%	U	0.66	0.63	0.07	11.02	10.54	0.15	5060.16	5377.02	0.60	0.26	0.32	0.00	2669.00
	M	0.66	0.66	0.66	11.02	10.63	0.26	5060.16	4971.03	0.86	0.26	0.27	0.62	2669.00
40%	U	0.66	0.63	0.07	10.96	10.43	0.10	5073.42	5408.24	0.56	0.26	0.33	0.00	2809.00
	M	0.66	0.65	0.54	10.96	10.99	0.91	5073.42	4898.88	0.74	0.26	0.26	0.98	2809.00

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

Figure 1.D.1: Matching estimates of different PBCs between Dynastic and non-Dynastic mayors (ATT)



1.E Robustness tests

In this section, we present different robustness tests based on the alternative definitions of dynastic mayors presented in the main text.

Exclusion of the most common surnames

As shown in Table 1.E.1 (fixed-effects estimation), the results are robust to excluding mayors who have one of the 500 most common surnames in the province (for a similar approach, see Geys (2017)). As argued in the main text, dropping individuals with the 500 most common surnames amounts to dropping about 50% of the sample. Yet we find that dynasty has robust, significant effects on PBCs with higher estimated coefficients than in the baseline specification (for example, while the estimated impact of dynasty on the increase in capital expenditures is about 43 euros per capita in the baseline specifications, it is equal to 75 when we exclude the 100 most common surnames, and 89 when we exclude the 500 most common surnames). These results seem to confirm that our initial results were biased downwards because of homonymy. When applying the same sample restriction on the RDD, we still observe similar coefficients, even though they are not significant anymore due to small sample size (Table 1.E.2).

Table 1.E.1: Dynasties and PBCs: Excluding candidates with the 500 most common names in the province (FE)

	Total exp	Current exp	Capital exp	Tax rev	Loans	Cap. transfers
Dynasty	5.075 (38.818)	5.571 (8.229)	-17.721 (31.312)	3.183 (4.018)	1.762 (9.874)	-8.678 (29.603)
LY	29.033 (16.574)*	-0.027 (2.025)	17.755 (14.786)	-5.460 (1.301)***	20.195 (4.802)***	-7.570 (12.941)
Dynasty*LY	82.919 (31.188)***	3.224 (3.931)	89.251 (29.273)***	7.801 (2.577)***	3.427 (9.309)	76.770 (27.252)***
R^2	0.02	0.27	0.02	0.41	0.01	0.01
N	19,446	19,446	19,446	19,451	19,450	19,450

The table presents estimates from fixed-effects panel regressions, using categories of public expenditures and income as dependent variables (all are expressed in euros per capita, and winsorized at the 1% level). The main explanatory variables are two dummies indicating (1) whether the mayor is dynastic and (2) whether it is the last year in the mayor's term. All outcome variables are expressed in euros per capita. The sample is comprised of all cities for which two full 5-year terms were observed between 1999 and 2012, restricted to mayors whose name is not among the 500 most common at the province level. Election years are excluded from the estimation. All specifications control for city and year fixed effects, as well as population size and the mayor's sex, age, experience, years of education, birthplace and term-limits. Standard errors are clustered at the city level. Standard errors in parentheses.

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

Table 1.E.2: Discontinuity of PBCs - Excluding candidates with the 500 most common names (RDD)

Order 2 Polynom	Δ Total exp	Δ Current exp	Δ Capital exp	Δ Tax rev	Δ Loans	Δ Transfers
Dynasty	178.259 (179.471)	-18.849 (33.772)	227.988 (153.290)	-3.621 (32.744)	66.361 (64.313)	187.309 (122.530)
Bandwidth	0.223	0.298	0.224	0.284	0.312	0.214
N (left)	269	305	270	297	314	258
N (right)	250	303	252	289	305	240

The table presents the results of an RD estimation with an optimal bandwidth calculated using the Calonico, Cattaneo, and Titiunik (2014) method, which employs triangular kernel and controls for an order-two polynomial of the margin of victory of the best dynastic candidate. Dependent variables are the differences of categories of expenditures and revenues between the last year and the average of the first 3 years, winsorized at the 1% level. No covariates are included. The sample consists of all full 5-year mayoral terms, for election years between 1999 and 2012. The sample is restricted to elections where no candidate had a name among the 500 most common at the province level. Robust standard errors clustered at the city level in parentheses.

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

Identification of dynastic mayors in a 10-years bandwidth

A second robustness check tests whether the results hold when we define as dynastic only mayors who had a relative in office during the previous 10 years. This definition imposes a common constraint on all identified dynastic mayors, and overcomes the potential bias induced by the fact that dynastic mayors at the beginning of the period are structurally different from those identified at the end of the period. Table 1.E.3, using the fixed-effects estimation, shows that the results are similar in magnitude to the baseline specifications. Similar results arise from the RDD specifications in Figure 1.E.4.

Table 1.E.3: Fixed effects: PBC for dynastic mayors with 10-year window

	Total exp	Current exp	Capital exp	Tax rev	Loans	Cap. transfers
Dynasty (10Y)	-2.619 (23.288)	-1.552 (4.884)	5.220 (19.077)	0.080 (2.124)	2.068 (5.575)	-0.653 (17.856)
LY	46.578 (9.942)***	-0.150 (1.383)	38.358 (8.980)***	-4.832 (0.837)***	14.179 (2.991)***	21.686 (7.959)***
Dynasty (10Y)*LY	55.146 (18.670)***	7.307 (2.450)***	44.806 (17.449)**	4.874 (1.656)***	10.832 (5.643)*	26.598 (16.266)
<i>N</i>	47,644	47,644	47,644	47,643	47,641	47,641

The table presents estimates from fixed-effects panel regressions, using categories of public expenditures and income as dependent variables (all are expressed in euros per capita, and winsorized at the 1% level). The main explanatory variables are two dummies indicating (1) whether the mayor is dynastic and entered in office no more than 10 years after the year of entry of the first generation (2) whether it is the last year in the mayor's term. All outcome variables are expressed in euros per capita. The sample is comprised of all cities for which two full 5-year terms were observed between 1999 and 2012. All specifications control for city and year fixed effects, as well as population size and the mayor's sex, age, experience, years of education, birthplace and term-limits. Standard errors are clustered at the city level. Standard errors in parentheses.

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

Table 1.E.4: Discontinuity of PBCs - 10-year window (RDD)

Order 2 Polynom	Δ Total exp	Δ Current exp	Δ Capital exp	Δ Tax rev	Δ Loans	Δ Transfers
Robust	162.905 (107.964)	-7.652 (12.485)	183.449* (100.495)	-8.496 (14.125)	49.193 (36.433)	183.332** (90.564)
Bandwidth	0.235	0.323	0.242	0.278	0.243	0.231
N (left)	929	1130	948	1042	952	923
N (right)	865	1052	884	957	886	857

The table presents the results of an RD estimation with an optimal bandwidth calculated using the Calonico, Cattaneo, and Titiunik (2014) method, which employs triangular bandwidth and controls for an order-two polynomial of the margin of victory of the best dynastic candidate entering the political arena no more than 10 years after the entrance of the first generation. Dependent variables are the differences of categories of expenditures and revenues between the last year and the average of the first 3 years, winsorized at the 1% level. No covariates are included. The sample consists of all full 5-year mayoral terms, for election years between 1999 and 2012. Robust standard errors clustered at the city level in parentheses.

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

1.F Composition of expenditures

In this section, we present results on the decomposition of current expenditures into several categories which include administration, justice, local police, education, culture, tourism, transports, environment, social expenditures and economic development. These data are available from 2001 to 2011, and we consider all full terms starting from 2000 (excluding election years). Table 1.F.1 represents the results for the fixed-effects specification, We find that dynastic mayors spend marginally more on environmental items on average (about 13 euros per capita over the term), and slightly more in sports-related items (about 3 euros per capita). We also find that dynastic mayors have a higher PBC of about 3.5 euros in terms of social expenditures. However, we do not find any difference when consider the Regression Discontinuity Design estimates (Table 1.F.2). Overall, these differences are therefore modest in magnitude, and not robust to different specifications.

Table 1.F.1: Decomposition of current expenditures per capita by sector - Fixed Effects specification

	Admin	Justice	Police	Education	Culture	Sport	Tourism	Transports	Environment	Soc. Exp.	Econ. Dev
Dynasty	4.718 (5.073)	-0.177 (0.135)	-0.489 (1.160)	0.040 (1.735)	0.540 (0.674)	3.180 (1.741)*	0.322 (0.800)	-0.200 (1.596)	12.754 (6.432)**	3.181 (3.049)	0.111 (0.226)
Constant	362.385 (20.550)***	1.431 (0.504)***	47.397 (6.864)***	97.471 (6.504)***	19.797 (3.383)***	13.822 (3.162)***	6.882 (3.239)**	96.103 (6.998)***	238.920 (43.277)***	57.089 (15.261)***	7.295 (1.427)***
R^2	0.04	0.00	0.01	0.01	0.01	0.01	0.00	0.03	0.32	0.02	0.00
N	23,469	23,469	23,469	23,469	23,469	23,469	23,469	23,469	23,469	23,469	23,469
	Admin	Justice	Police	Education	Culture	Sport	Tourism	Transports	Environment	Soc. Exp.	Econ. Dev
Dynasty	4.379 (5.089)	-0.179 (0.138)	-0.491 (1.141)	0.055 (1.740)	0.464 (0.677)	3.211 (1.726)*	0.235 (0.807)	-0.434 (1.607)	12.185 (6.489)*	2.349 (3.072)	0.093 (0.229)
LY	2.398 (1.039)**	0.003 (0.031)	0.417 (0.293)	-0.382 (0.322)	0.341 (0.188)*	-0.018 (0.254)	0.172 (0.205)	-0.731 (0.406)*	-1.099 (1.979)	-1.150 (0.787)	0.068 (0.094)
Dynasty*LY	0.611 (1.624)	0.005 (0.044)	-0.108 (0.526)	0.051 (0.453)	0.190 (0.332)	-0.111 (0.411)	0.281 (0.463)	1.075 (0.691)	2.429 (2.596)	3.428 (1.741)**	0.048 (0.185)
Constant	363.926 (20.575)***	1.433 (0.504)***	47.655 (6.823)***	97.231 (6.540)***	20.020 (3.381)***	13.806 (3.187)***	7.003 (3.248)**	95.689 (6.977)***	238.333 (43.037)***	56.514 (15.048)***	7.340 (1.424)***
R^2	0.04	0.00	0.01	0.01	0.01	0.01	0.00	0.04	0.32	0.02	0.00
N	23,469	23,469	23,469	23,469	23,469	23,469	23,469	23,469	23,469	23,469	23,469

The table presents estimates from fixed-effects panel regressions, using items of current expenditures (all are expressed in euros per capita, and winsorized at the 1% level) as outcome variable. In the top panel, the main explanatory variable is a dummy indicating whether the mayor is dynastic. In the bottom panel, this variable is interacted with a variable indicating whether we are in pre-electoral year. The sample is comprised of all full terms starting from 2000 to 2011. Election years are excluded from the estimation. All specifications control for city and year fixed effects, as well as population size and the mayor's sex, age, experience, years of education, birthplace and term-limit. Standard errors are clustered at the city level. Standard errors in parentheses.

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

Table 1.F.2: Decomposition of current expenditures per capita by sector - RDD specification

	Admin	Justice	Police	Education	Culture	Sport	Tourism	Transports	Environment	Soc. Exp.	Econ. Dev
Dynasty	28.496 (28.523)	0.314 (0.458)	-2.479 (5.109)	2.026 (5.846)	1.113 (2.732)	-1.021 (2.240)	-0.034 (3.660)	5.158 (8.508)	25.078 (16.967)	-6.669 (17.327)	1.286 (2.009)
Bandwidth	0.251	0.194	0.335	0.235	0.233	0.181	0.238	0.254	0.203	0.262	0.198
N (Left)	640	522	736	604	602	490	610	641	541	656	530
N (Right)	572	464	691	543	540	438	548	576	481	586	474
	Δ Admin	Δ Justice	Δ Police	Δ Education	Δ Culture	Δ Sport	Δ Tourism	Δ Transports	Δ Environment	Δ Soc. Exp.	Δ Econ. Dev
Dynasty	-8.328 (9.841)	-0.039 (0.085)	-0.866 (2.671)	3.984 (3.084)	-0.762 (1.630)	1.668 (1.283)	-0.310 (2.283)	-1.323 (3.355)	11.003 (12.303)	3.943 (8.112)	-0.463 (0.863)
Bandwidth	0.256	0.201	0.266	0.216	0.203	0.218	0.243	0.248	0.196	0.203	0.231
N (Left)	640	532	654	567	537	575	619	631	523	535	596
N (Right)	573	477	586	504	479	506	555	562	465	478	536

The table presents the results of an RD estimation with an optimal bandwidth calculated using the Calonico, Cattaneo, and Titiunik 2014 method, which employs a triangular kernel and controls for an order-two polynomial of the margin of victory of the best dynastic candidate. Dependent variables are the average of items of current expenditures over the term (top part of each panel), and the differences of items of current expenditures between the last year and the average of the first 3 years (bottom part of each panel). All variables are winsorized at the 1% level. The sample is comprised of all full terms starting from 2000 to 2011. No covariates are included. Robust standard errors clustered at the city level in parentheses.

1.G Variables

Table 1.G.1: Variables used in the analysis

Variables	Definition	Time span	Source
Dynasty	Whether the politician had a relative in office before first being elected to the council	1985–2012	Ministry of Interior
First generation	Whether the politician had a relative in office after being elected to the council	1985–2012	Ministry of Interior
Sex	Sex of the politician	1985–2012	Ministry of Interior
Age	Age of the politician in years	1985–2012	Ministry of Interior
Years of education	Minimum number of years to complete the highest degree obtained	1985–2012	Ministry of Interior
Occupation	Classification of mayors' occupations*	1985–2012	Ministry of Interior
Experience	Number of years since first elected to the council	1985–2012	Ministry of Interior
Place of birth	Place of birth in the format <i>Name of the city (Province abbreviation)</i>	1985–2012	Ministry of Interior
Civic	Whether the politician is from a civic list	1985–2012	Ministry of Interior
South	Dummy for southern regions	1985–2012	Ministry of Interior
Population	Population size	1998–2012	Ministry of Interior
Unemployment	Unemployment rate, in percent	2001	Ministry of Interior
Surname frequency	Surname frequency at the province level (in thousands of individuals per surname)	2001	Ministry of Interior
Trust	Level of trust as measured by the "Trust" question in the World Value Survey	1990s	Nannicini et al. 2013
Total expenditures	Total expenditures per capita	1998–2012	Ministry of Interior
Current expenditures	Current expenditures per capita	1998–2012	Ministry of Interior
Capital expenditures	Capital expenditures per capita	1998–2012	Ministry of Interior
Tax revenues	Collected taxes per capita	1998–2012	Ministry of Interior
Collected taxes	Collected taxes per capita	1998–2012	Ministry of Interior
Contracted loans	Contracted loans per capita	1998–2012	Ministry of Interior
Capital transfers	Capital transfers from the government or the region, per capita	1998–2012	Ministry of Interior
Margin (Dynasty)	Margin of the best dynastic candidate	1993–2012	Ministry of Interior
Margin (Candidate)	Difference in vote shares between the candidate and his best challenger	1993–2012	Ministry of Interior
Number of candidates	Number of candidates in the election	1993–2012	Ministry of Interior
Incumbent	Whether the candidate was elected mayor during the previous term	1993–2012	Ministry of Interior
Term Limit	Whether the mayor is eligible for re-election	1993–2012	Ministry of Interior
Reelection	Whether the mayor is re-elected	1993–2012	Ministry of Interior
Term duration	Number of years the mayor remained in office during the term after his election	1993–2012	Ministry of Interior
Ability of revenue collection	Ratio between actual and expected revenues	1998–2012	Ministry of Interior
Speed of payment	Share of due expenditures paid during the term	1998–2012	Ministry of Interior
Growth of private tax base	Yearly growth of private tax base, in percent	2001–2011	Ministry of Interior
Decomp. of current exp.	Per capita expenditures by item of expenditures	2001–2011	Ministry of Interior
Corruption - Mayor	The mayor	2006–2012	Giommoni 2017

* Calculated by the authors based on the name of the job, using the official socio-professional categories of the Italian government

Chapter 2

Gender-Biases: Evidence from a Natural Experiment in French Local Elections

This paper is co-authored with Jean-Benoît Eyméoud

1 Introduction

Are women discriminated against in politics ? While decades of research have investigated the reasons behind the under-representation of women in politics, uncovering discriminatory behaviors of voters proved being a difficult task, because of the numerous selection effects which affect the observed and unobserved characteristics of women present in the political arena.

In this paper, we provide causal evidence of discrimination against women in politics. To do so, we use a unique feature of the French *Départementales*¹ elections of 2015, which allows us to unambiguously disentangle selection effects from preferences over female candidates in a real-world setting. For the first time in the history of French elections, candidates ran by pairs, which necessarily had to be gender-balanced. Therefore, each pair of candidates included a man and a woman (each with a substitute of the same gender). Upon casting their ballot, voters could only opt for one of the different pairs of candidates, so that for every pair, each male and female candidate received exactly the same number of votes. If a pair was elected, both candidates were appointed to the same seat in the *Conseil Départemental* (the *Département*

1. The *Département* is a French territorial unit gathering numerous competences in terms of schooling, public infrastructures, culture, sports.

assembly where the elected candidates are seating), so that their fates were completely tied.

Crucially, within each pair, the order of appearance of the candidates on the ballot was determined by alphabetical order: this order determined only the place of the candidate's name on the ballot. As we argue, such a setting yields an as-good-as-random allocation of the order of gender on the ballot, and allows us to explore whether pairs where the woman appears first on the ballot have different electoral outcomes than pairs where the man appears first.

The rationale behind this test is that, although the order of appearance of the candidates on the ballot does not have any impact on the subsequent prerogatives attributed to the candidates, some voters may mistakenly have thought that the first candidate would be the "main" candidate. Indeed, since voters were typically used to voting for a single candidate and a substitute, the new rules are unlikely to have been fully understood by everyone. As the French statistical institute IFOP acknowledged some weeks before the election: "These elections were characterized by insufficient information", and "the introduction of pairs of candidates unsettled long-established landmarks" in the mind of voters (IFOP (2015)). Therefore, any observed difference between pairs with a male or female candidate listed on the first position would mean that we observe two phenomena. First, limited attention from some voters, as defined by DellaVigna (2009). Indeed, because the fates of both candidates on a ballot are tied, if all voters knew perfectly the rules of the elections, we would not find any treatment effect. Secondly, a pure gender bias from these voters.

The identification of this bias comes from several particularly interesting features of our setting. First, the number of male and female candidates are exactly identical - in order to enforce strict parity in local councils. Secondly, while the characteristics of male and female candidates are on average different, candidates characteristics do not predict whether the male or the female candidate appears first on the ballot. The effect we measure is therefore unlikely to be affected by selection biases, since it consists in comparing whether identical pairs on average perform differently when the male or the female candidate is first on the ballot. Furthermore, our identification strategy is strengthened by the fact that parties did not seem to strategically match male and female candidates based on their surname in order, for example,

to place the male candidate at the top of the ballot: indeed, the distribution of the first letter of male and female surnames are identical.

Comparing treated and untreated pairs of candidates of identical political affiliations across precincts, we show that right-wing pairs where the female candidate appeared first lost about 1.5 percentage points in shares of vote during the first round, while on average this was not the case of pairs from other parties. These effects substantially affected the outcome of the election: indeed, the affected pairs were 4 points less likely to go to the second round or to win the election.

This setting not only allows to identify pure discrimination, but also to characterize the type of discrimination at stake. Discrimination is often viewed as being either taste-based or statistical. In the first case, voters dislike voting for female candidates whatever their characteristics or the information they have about them. In the second case, voters apply stereotypes on women candidates because of a lack of information about the characteristics of the candidates. We argue that, in our setting, our treatment effect reflects statistical rather than taste-based discrimination against female candidates. To identify it, we follow a methodology similar to the one developed by Altonji and Pierret (2001) and exploit a unique feature of the French electoral law, which states that the candidates can report additional information about themselves on the ballot - such as their political experience, age, occupation, or picture. Comparing treatment effects between ballots with reported information and ballots without any information, we show that, for the right-wing pairs, discrimination disappears when information about the candidates is displayed.

We show that these missing votes do not reflect differential abstention, and did not translate into blank and null votes; instead they translated into higher shares of votes for the competing candidates. However, the competing pairs with a female candidate listed first did not receive more votes than others. Such a result stacks the deck against our interpretation of the results as reflecting statistical rather than taste-based discrimination. Indeed, if our result was driven by taste-based discrimination, we should have observed that pairs of candidates with a male candidate listed first benefited more from the discrimination against right-wing women.

Finally, we explore two different types of heterogeneity. First, we show that discrimination

does not depend directly on the incumbency status of the candidates. Assuming that these characteristics are a proxy for candidates' quality, this alleviates the concern that the results are directly driven by differences of quality between male and female candidates. Secondly, we test whether discrimination depends on the characteristics of the precincts. We show that, while electoral discrimination does not vary with the age, unemployment rate and level of education of the population, it is greater in areas with high gender discrimination on the labor market, as measured by the unexplained component of a Oaxaca-Blinder decomposition of wage gaps at the local level.

These results bear important implications for the public debate around electoral discrimination. First of all, while our results show discrimination against right-wing female candidates, it does not imply that right-wing voters are more prejudiced against women than voters from other parties. Indeed, the presence of limited attention is necessary for the identification of discrimination. Not observing discrimination against the female candidates of other parties can simply indicate that they are less subject to the limited attention bias. Secondly, since the amount of information available about the candidates on the ballot seems to play an important role on the outcome of the election, it calls for a more general reflexion about a potential standardization of the ballots' layout. Finally, since electoral discrimination seems to be higher in places with a greater gender discrimination on the labor market, policies aiming at reducing gender biases in politics are likely to be more effective if coordinated with policies on other markets.

Our contribution to the literature is threefold. First, we contribute to the debate about the reasons why women are underrepresented in politics. Many studies analyzed the selection processes faced by women upon entering in politics. Women might select themselves less into politics because of a lack of self-confidence (Hayes and Lawless (2016)) or differential returns from politics (Júlio and Tavares (2017)). More generally, women face tradeoffs between family balance and competitive professional environments (Bertrand, Goldin, and Katz (2010)). Conditional on entering politics, parties might also fail at promoting women to high positions and at fielding them in winnable races (Sanbonmatsu (2010), Thomas and Bodet (2013), Esteve-Volart and Bagues (2012), Casas-Arce and Saiz (2015)), even though their entrance in politics

often causes an increase in the quality of elected officials (Baltrunaite et al. (2014), T. J. Besley et al. (2017)). Yet, evidence on the last hurdle potentially faced by women in politics (namely, discrimination from voters) are mixed, and if anything, tend to argue that discrimination against women does not exist².

Secondly, our study is among a small group of studies causally identifying statistical discrimination in politics in a real-world setting. Understanding the determinants of gender discrimination is of particular importance since women in office are likely to behave differently than men in office (Chattopadhyay and Duflo (2004), Ferreira and Gyourko (2014), Brollo and Troiano (2016)). The debate over whether discrimination involves discriminatory tastes (Becker (1957)) or imperfect information (Phelps (1972), Arrow et al. (1973)) is a long-standing one. Current evidence on gender-discrimination in politics vastly points towards the existence of statistical discrimination. Numerous survey studies show that different types of individuals have different preferences over female politicians: McDermott (1998) and Burrell (1995) find that women are more likely to prefer female candidates, while K. Dolan (1998) finds that minorities and elderly are more likely to vote for women. McDermott (1997) argues that liberal voters are more likely to prefer female candidates. Such preferences are likely to be driven by gender stereotypes (Koch (2002)). In particular, in a context of low information, the gender of the candidate can be interpreted by the voters as signals about the ideology of the candidates: McDermott (1998) shows that female candidates are typically perceived as more liberal and more dedicated to honest government. Evidence from lab experiments also tend to point the existence of different mechanisms leading to statistical discrimination. Leeper (1991) shows that even when women candidates emit "masculine" message, voters attribute them "feminine" characteristics. Huddy and Terkildsen (1993) show that gender-based expectations over policies were more related to gender-traits stereotypes than to gender-beliefs stereotypes. King

2. Analyses of aggregate votes generally found that male and female candidates have equal success rates in elections, thus arguing that voters do not have gender biases (Darcy and Schramm (1977), Seltzer, Newman, and Leighton (1997), McElroy and Marsh (2009)). Some studies even argue that women might have an electoral advantage compared to men (Black and Erickson (2003), Borisjuk, Rallings, and Thrasher (2007)), and that after their first election, they are at least as likely to be reelected as men (Shair-Rosenfield and Hinojosa (2014)). Milyo and Schosberg (2000) even argue that the barriers to entry faced by women makes female incumbents of higher quality than male incumbents, resulting in an advantage for female incumbents. On the other hand, several studies argue that voter biases are marginal compared to partisan preferences (K. A. Dolan (2004), K. Dolan (2014), Hayes and Lawless (2016)).

and Matland (2003) show that biases against women are likely to depend on partisan preferences, while Mo (2015) shows that both explicit and implicit attitudes against women shape the probability of voting for female candidates.

However, very few studies managed to propose causal identifications of discrimination in politics using natural experiments. Most of the studies on gender in politics rely primarily on aggregate data, surveys or laboratory experiments, which are problematic for several reasons. Raw comparisons of aggregate data are unlikely to fully control for the selection process leading to the observed political competition. This is especially true if male and female candidates are likely to differ in unobserved characteristics which might drive both their probabilities of running as a candidate and of winning the election. Respondents' answers in surveys might be affected by characteristics of the interviewer, such as her gender (Huddy et al. (1997), Flores-Macias and Lawson (2008), Pino (2017), Benstead (2013)), religion (Blaydes and Gillum (2013)) or language (Lee and Pérez (2014)). Finally, while laboratory experiments allow to disentangle more accurately the mechanisms leading to potential gender-biases, they are hardly likely to represent real-world election settings.

By overcoming these issues, natural experiments are particularly appealing. Discrimination on the labor market has been plausibly identified through a vast range of field and natural experiments, involving audit and correspondence studies, and the precise mechanisms behind observed discrimination have been extensively discussed (see, among others, Bertrand and Mullainathan (2004), Bertrand, Chugh, and Mullainathan (2005), Charles and Guryan (2008), and Bertrand and Duflo (2017) for a survey). Recent developments of big data also have allowed to plausibly identify statistical discrimination on the housing market (Laouénan and Rathelot (2017)). However, field experiments are hardly applicable in the political arena - in particular since the secrecy of the vote prevents from fully understanding voters' motives - and natural experiments remain rare. However, recent studies managed to exploit natural experiments and to causally identify discrimination from voters - mostly in a statistical way. Bhavnani (2009), Beaman et al. (2009) and De Paola, Scoppa, and Lombardo (2010) suggest that reserved seats for women in office is an efficient way of reducing gender stereotypes and statistical discrimination. Relatedly Pino (2017) shows that women living in environments emphasizing

traditional gender roles are less likely to vote for women³.

Finally, our analysis provides evidence of limited attention from voters. Since the seminal work of Simon (1955), various pieces of research - coming especially from laboratory experiments - suggested that attention is a scarce resource and that individuals make decisions using only part of the available information (see DellaVigna (2009) for a survey). Voters might themselves be myopic, punishing or rewarding incumbents for what happens shortly before the election (Achen and Bartels (2004)), and replacing information about a whole electoral term (which might be more difficult to access) by easy-to-grasp information about the last year in office (Healy and Lenz (2014)). However, to the best of our knowledge, few studies provided evidence as regard to whether individuals actually know the rules of the election when they cast their ballot. By focusing on a type of discrimination which is possible only because of limited attention of the voters, we therefore show that a non-negligible part of them were subject to limited attention concerning the rules of the election⁴. We therefore also contribute to a recent stream of research showing how ballot layout can influence voters' decision. Recent evidence showed that minor candidates are likely to perform better when their name is located close to the name of a major candidate (Shue and Luttmer (2009)), or when it is listed at the top of the ballot (Ho and Imai (2006), Ho and Imai (2008) among others). Relatedly, the number of decisions to make on a ballot can induce "choice fatigue", which substantially affects abstention (Augenblick and Nicholson (2015)). Because our identification relies upon ballot order effects, it therefore reasserts that limited attention concerning the rules of an election can play a key role on aggregate outcomes. From this standpoint, this paper is to the best of our knowledge among the first to highlight the link between limited attention and discrimination in politics⁵.

The remainder of the paper is structured as follows. Section 2 describes the institutional setting and the data we use. We provide descriptive statistics and various balance-checks

3. To the best of our knowledge, only one study identified taste-based discrimination in an electoral setting (Broockman and Soltas (2017), on racial discrimination in Republican primary elections in the United States. Another field where gender-biases have been explored through the lens of natural experiments is the field of academic recruitment - see Bagues, Sylos-Labini, and Zinovyeva (2016) for example.

4. Whether this limited attention is due to differential costs of acquiring electoral information regarding the electoral rules is left for further research.

5. A recent contribution from Bartoš et al. (2016) shows that in contexts of complete information, discrimination can occur if processing all the available information is costly. In such a case, agents might focus only on a subset of information, thus triggering statistical discrimination. As we argue later, such a setting is unlikely to apply to our context, since the amount of information to process by default in our setting is minimal.

showing that selection into the treatment is unlikely. Section 3 describes our estimation strategy. Section 4 gathers our main empirical results. Section 5 studies potential channels for our results and Section 6 concludes.

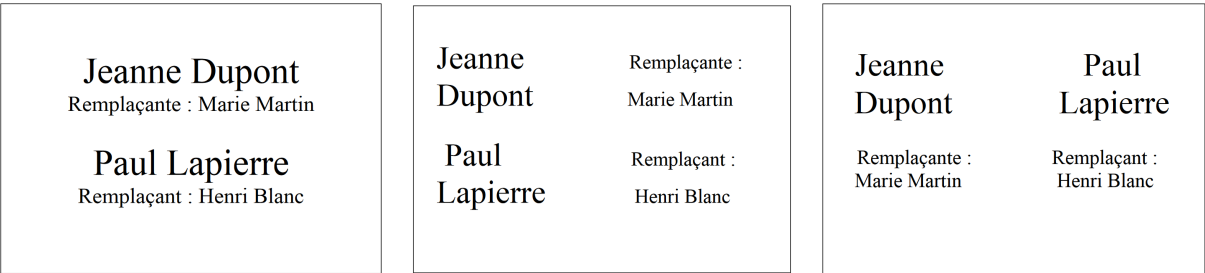
2 Institutional Framework and Data

2.1 Institutional Framework

This study relies on data from the 2015 French departmental elections, which took place on March 22nd and March 29th. Departmental councillors were elected in 2,054 *cantons* (subdivisions of the *départements*). In each of these precincts, lists ran by pairs which necessarily had to be gender-balanced. Each candidate of a pair had to have a substitute of the same sex as her. Overall, 9,097 pairs of candidates ran for office.

Within each list, the order of the candidates on the ballot was determined by alphabetical order. Such a requirement is imposed by the article L.191 of the French electoral legislation. The rules for printing electoral ballots are also stringent: it must be printed in only one color on a blank sheet of format 105x148 mm, weigh between 60 and 80 grams per square meter and be in landscape format. For each candidate, the name of its substitute must be written right after its name, using a smaller font. According to the articles L.66, L.191, R.66-2, R.110 and R.111 of the electoral code, any ballot not respecting these requirement is considered as null. Figure 2.1 shows examples of compliant ballots, as communicated by the Ministry of Interior. The ballots on the day of the election are the only ones to be subject to these requirements, which do not affect campaign advertisement leaflets or electoral posters.

Figure 2.1: Examples of valid ballots



2.2 Data and Descriptive Statistics

For this analysis, we retrieved information about all the pairs of candidates from the Ministry of Interior. Our database includes information on age, gender, incumbency status, political affiliation and socioprofessional categories of each of these candidates. We matched these information with the *Répertoire National des Elus*, to know whether the candidates also had other political experience at the municipal, regional or parliamentary level. Finally, we also matched these information with sociodemographic information at the precinct-level, retrieved from the 2013 Census.

In order to carry on our analysis, we classified candidates into different partisan groups. We classified as extreme-left the lists labeled as *Communists*, *Extreme-Left*, *Front de Gauche* and *Parti de Gauche*. We classified as left-wing the lists labeled as *Parti Socialiste*, *Union de la Gauche*, *Radicaux de Gauche* and *Divers Gauche*. We classified as right-wing the lists labeled as *MoDem*, *Union du Centre*, *Union des Démocrates et des Indépendants*, *Debout La France*, *Divers-Droite*, *Union des Droites*, *UMP*. Finally we classified as extreme-right the lists labeled as *Front National* and *Extreme Droite*⁶.

We first begin by documenting the differences between candidates of different partisan groups in Table 2.1. Overall, 28% of candidates were left-wing, a number which is comparable to the share of right-wing candidates. 14% of candidates were classified as extreme-left, while 22% were classified as extreme-right. Concerning political experience we categorized a candidate as having previous political experience if she was, at the time of election, either an incumbent, a municipal councillor in a municipality belonging to the precinct, a regional councillor, or a member of parliament.

For all parties, the share of male candidates with political experience is greater than the share of female candidates with political experience. Incumbents were slightly more numerous among right-wing candidates (69% of men and 53% of women) than among left-wing candidates (63% of men and 46% of women). Only 29% of men and 19% of women were previously elected among extreme-left candidates. These proportions shrink to respectively 15% and 9%

6. By an abuse of language, we hereafter call "parties" the broad categorizations of *extreme-left*, *left-wing*, *right-wing* and *extreme-right* candidates, described above.

Table 2.1: Characteristics of male and female candidates by partisan affiliation

	All		Extreme Left		Left		Right		Extreme Right	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Previous Political Exp. (W)	0.344	0.475	0.194	0.395	0.462	0.499	0.526	0.499	0.094	0.292
Previous Political Exp. (M)	0.470	0.499	0.293	0.455	0.631	0.483	0.685	0.465	0.153	0.361
Age (W)	51.410	12.061	53.273	11.714	51.651	10.789	51.528	10.878	50.739	14.750
Age (M)	52.533	12.927	53.718	12.774	54.022	11.602	53.226	12.128	49.741	15.260
Farmer (W)	0.019	0.136	0.015	0.122	0.012	0.107	0.032	0.177	0.009	0.096
Intermediary Profession (W)	0.057	0.233	0.016	0.126	0.028	0.164	0.085	0.279	0.086	0.281
Private Sector Employee (W)	0.279	0.449	0.226	0.418	0.253	0.435	0.286	0.452	0.347	0.476
Liberal Occupation (W)	0.068	0.252	0.038	0.192	0.073	0.260	0.091	0.288	0.035	0.183
Education Occupation (W)	0.115	0.319	0.147	0.354	0.154	0.361	0.095	0.294	0.052	0.222
Civil Servant(W)	0.117	0.321	0.162	0.368	0.163	0.370	0.106	0.308	0.047	0.212
Public Firm Worker (W)	0.039	0.194	0.063	0.243	0.045	0.207	0.035	0.183	0.021	0.143
Other Occupation(W)	0.099	0.299	0.050	0.219	0.077	0.266	0.108	0.311	0.152	0.359
Retired (W)	0.206	0.404	0.282	0.450	0.196	0.397	0.161	0.367	0.250	0.433
Farmer (M)	0.034	0.181	0.014	0.116	0.028	0.164	0.059	0.236	0.022	0.146
Intermediary Profession (M)	0.096	0.294	0.017	0.129	0.056	0.229	0.135	0.342	0.143	0.350
Private Sector Employee (M)	0.235	0.424	0.232	0.422	0.188	0.391	0.214	0.410	0.316	0.465
Liberal Occupation (M)	0.079	0.269	0.030	0.170	0.072	0.259	0.127	0.333	0.046	0.209
Education Occupation (M)	0.104	0.306	0.147	0.355	0.133	0.339	0.070	0.255	0.069	0.254
Civil Servant(M)	0.101	0.301	0.118	0.322	0.147	0.354	0.082	0.274	0.056	0.231
Public Firm Worker (M)	0.039	0.194	0.063	0.244	0.052	0.221	0.034	0.181	0.015	0.120
Other Occupation(M)	0.054	0.226	0.044	0.205	0.046	0.209	0.055	0.229	0.061	0.239
Retired (M)	0.259	0.438	0.335	0.472	0.280	0.449	0.224	0.417	0.271	0.445
Woman First	0.506	0.500	0.502	0.500	0.496	0.500	0.524	0.500	0.502	0.500
Observations	9097		1250		2507		2714		1929	

This table presents the mean and standard deviation of the characteristics of the candidates. Columns 1 and 2 report information for the full population of candidates, while the remaining columns reported the mean and standard deviation by party.

among extreme right candidates. Except for extreme-right candidates, the candidates of all parties were on average between 52 and 54 years old, and the male candidates are older than the female candidates. Extreme-right candidates are younger (around 50 years old), and among them, female candidates are older than male candidates. Finally, a majority of male and female candidates came from the private sector or were retired. Civil servants and teachers were over-represented among left-wing and extreme-left candidates, while intermediary professions were over-represented among the right-wing candidates. Finally, we find that within each party, half of the pairs of candidates had the female candidate listed first.

Balance checks

In this section, we test the as-good-as-random nature of the order of appearance of female candidates on the ballots. Namely, we check whether the pairs where the female candidate is listed first differ on observable characteristics compared to pairs where the male candidate is

listed first. For sake of brevity, we focus both on the full population of candidates, and on the subsamples that we will use later in our analysis.

As we argue in the next section, in order to identify causal effects of the treatment, our estimation needs to satisfy the *Stable Unit Treatment Value Assumption* (SUTVA), which states that the potential outcomes of a unit are not affected by the treatment status of another unit. This hypothesis is likely to be violated if we consider altogether several candidates from a same precinct. Indeed, let us assume that the treatment affects negatively a given pair of candidates. One can therefore imagine that the votes they lost positively affected another pair of candidates from the same precinct (especially if the voters reacting to the treatment are non-partisan).

In order to avoid such a scenario, we run an analysis on different samples of pairs of candidates having the same partisan affiliation, and being the sole pair of candidates of their party in their precinct. Such subsamples meet the *SUTVA* assumption: while it is possible that these candidates are affected by the treatment status of candidates of other parties, they cannot be affected by the treatment status of other units in the sample.

In Table 2.2, we systematically test for imbalances, both on the whole population of candidates and on the subsamples of interest. To do so, using a logistic model, we regress the dummy variable indicating whether the female candidate is listed first on the whole set of individual characteristics. Overall, whether we consider the full population of candidates or the restricted subsamples, even though some variables appear to have a significant effect, the characteristics of the candidates explain very few (if any) of the variance of the treatment variable, and they are not jointly significant. No imbalances are found for extreme-left and right-wing candidates candidates. Among left-wing pairs, women are more likely to be listed first on the ballot if they work in intermediary professions or are retired, and if the male candidate is an retired. Finally, among extreme-right candidates, younger female candidates are more likely to be on the top of the ballot. So do female candidates who are retired or working in liberal occupation. Women paired with male candidates working in intermediary professions and in the education sector are less likely to be at the top of the ballot. Yet, overall, the absence of joint significance suggests that if any selection into the treatment based on the characteristics

of the candidates exists, it is of low magnitude.

2.3 Manipulation of the treatment

An important related question is whether parties selected male and female candidates in order to have male candidates at the top of the ballot. In this case, we should observe that the distribution of first letters of surnames are different across gender. In Figure 2.2, we plot the frequency of the first letter of surnames for male and female candidates, both on the total population of candidates and on our subsamples of interest: in all cases, the distributions are strikingly similar. In Table 2.3, we formalize this graphical intuition by performing different tests of equal distributions. Namely, we perform the tests of Kolmogorov-Smirnov, of equality of medians, and of Mann-Whitney-Wilcoxon. Overall, for all the tests and all the samples of interest, we cannot reject the null hypothesis that the distributions are identical. The only exception is for the restricted subsamples of left-wing candidates, where the distributions seem slightly different : the Kolmogorov Test and the Mann-Whitney-Wilcoxon test reject the hypothesis of equal distributions at the 10% level. But as Figure 2.2c shows, this difference seems mainly driven by an over-representation of women with names beginning by the letter B, and is unlikely to represent a more general manipulation of the treatment. This suggests that parties did not strategically chose to match candidates based on their surnames. Finally, as additional checks for the absence of manipulation of the treatment, we report in Annex the share of votes received by candidates in the first round depending on the first letter of the candidates' surnames: we find that, for each first letter of the candidates' surnames, the share of votes is very close to the sample average.

2.4 Data on ballot layout

An important feature of the French electoral law is that it allows candidates to add additional information about themselves on the ballot, so long as it does not confuse the voter about their identity. In order to account for this specificity, we manually collected data on the electoral ballots that were used for these elections. While there does not exist a systematic recording of electoral ballots for the local elections in France, we could access a sample corresponding

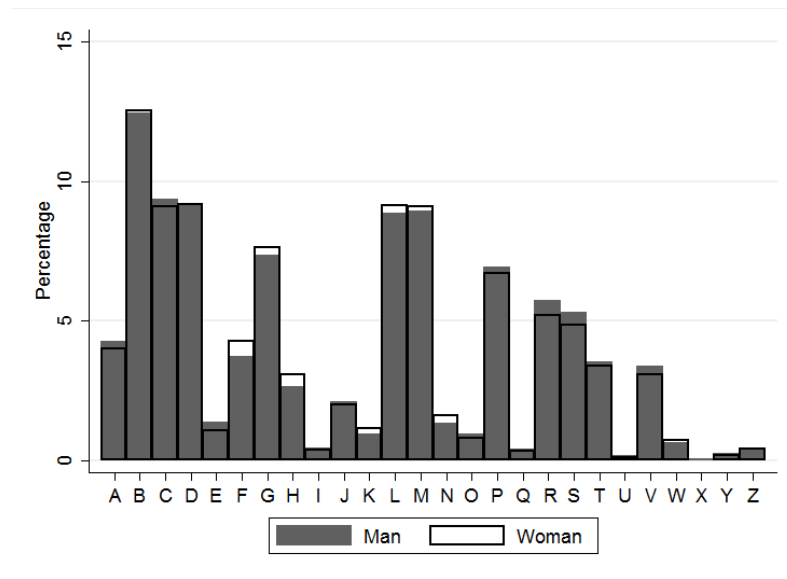
Table 2.2: Determinants of the treatment (Total population of candidates and restricted samples)

Woman First	Restricted Samples				
	All	Extreme Left	Left	Right	Extreme Right
Previous Political Exp. (W)	0.078 (0.050)	0.070 (0.151)	0.019 (0.115)	0.102 (0.117)	0.134 (0.171)
Previous Political Exp. (M)	-0.016 (0.051)	0.120 (0.133)	0.044 (0.130)	-0.228 (0.142)	-0.197 (0.136)
Age (W)	-0.001 (0.002)	-0.004 (0.007)	-0.005 (0.007)	0.005 (0.006)	-0.009 (0.004)**
Age (M)	-0.001 (0.002)	0.007 (0.006)	0.001 (0.006)	-0.008 (0.006)	-0.001 (0.004)
Intermediary Profession (W)	0.260 (0.105)**	-0.040 (0.501)	0.712 (0.403)*	0.381 (0.238)	0.319 (0.198)
Private Sector Employee (W)	0.148 (0.072)**	0.173 (0.257)	0.335 (0.227)	0.155 (0.180)	0.184 (0.141)
Liberal Occupation (W)	0.369 (0.106)***	0.187 (0.373)	0.458 (0.288)	0.370 (0.227)	0.670 (0.286)**
Education Occupation (W)	0.103 (0.088)	0.112 (0.274)	0.377 (0.241)	-0.059 (0.245)	0.100 (0.243)
Civil Servant(W)	0.103 (0.088)	0.058 (0.270)	0.319 (0.240)	-0.030 (0.225)	0.389 (0.244)
Public Firm Worker (W)	0.098 (0.121)	0.200 (0.329)	0.274 (0.339)	0.053 (0.322)	-0.304 (0.349)
Retired (W)	0.076 (0.086)	-0.228 (0.271)	0.486 (0.256)*	-0.064 (0.216)	0.531 (0.177)***
Intermediary Profession (M)	0.019 (0.100)	0.379 (0.553)	0.681 (0.327)**	0.055 (0.226)	-0.390 (0.207)*
Private Sector Employee (M)	0.045 (0.085)	0.209 (0.276)	0.106 (0.241)	0.042 (0.197)	-0.292 (0.186)
Liberal Occupation (M)	0.057 (0.104)	0.136 (0.421)	0.290 (0.290)	-0.036 (0.215)	-0.088 (0.276)
Education Occupation (M)	-0.036 (0.100)	0.046 (0.294)	0.398 (0.251)	-0.157 (0.260)	-0.432 (0.250)*
Civil Servant(M)	-0.077 (0.099)	-0.174 (0.301)	0.259 (0.245)	-0.199 (0.246)	-0.317 (0.256)
Public Firm Worker (M)	-0.043 (0.128)	-0.195 (0.343)	0.202 (0.316)	-0.249 (0.331)	-0.553 (0.430)
Retired (M)	0.028 (0.092)	-0.098 (0.288)	0.207 (0.240)	0.187 (0.207)	-0.264 (0.206)
XLeft	0.089 (0.095)				
Left	0.035 (0.089)				
Right	0.119 (0.089)				
XRight	0.060 (0.090)				
Pseudo R2	0.00	0.01	0.01	0.01	0.01
Chi2	29.35	18.43	11.83	16.72	22.13
N	9,081	1,187	1,341	1,389	1,883

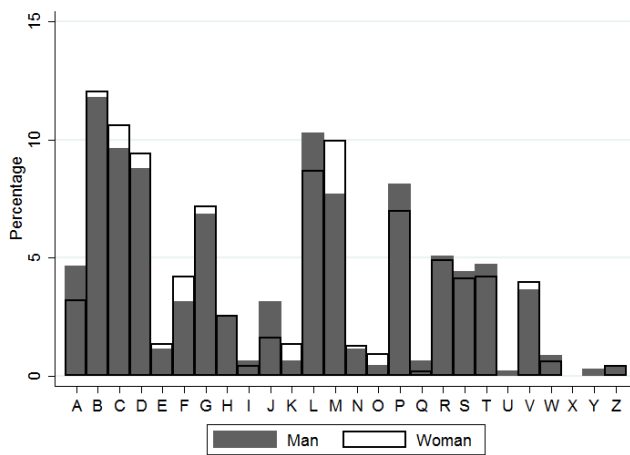
Logistic Regressions. Column 1 considers all candidates. In columns 2 to 5 each subsample considers only the candidates who are the only ones of the considered party in the precinct where they run. The outcome is a variable equal to one if the female candidate is first on the ballot and zero otherwise. The coefficients on male and female candidates' occupations are expressed considering farmers and other occupations as the reference modality. Standard errors in parentheses are clustered at the precinct level in column 1, and robust in columns 2 to 5.

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

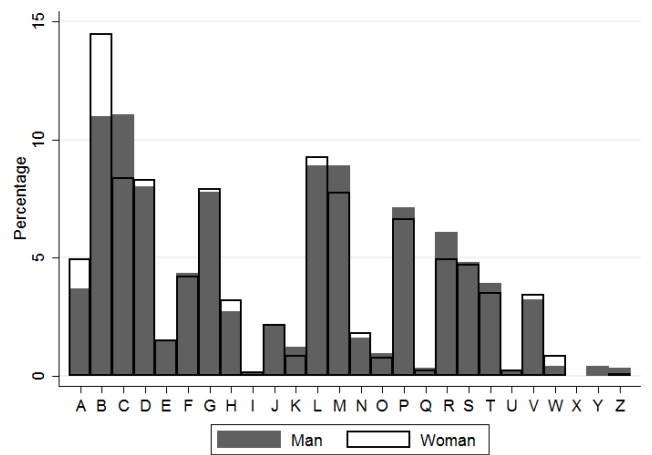
Figure 2.2: Distribution of surname initials across gender and parties



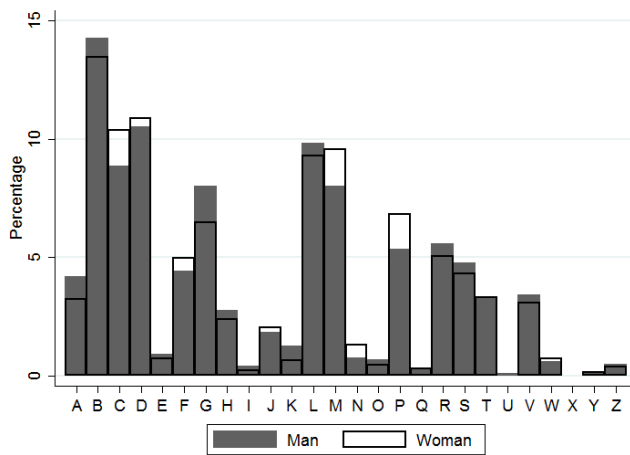
(a) Total population of candidates



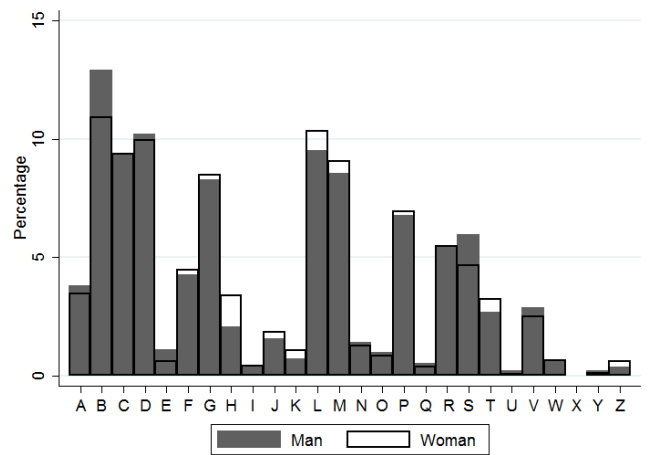
(b) Restricted Sample: Extreme Left



(c) Restricted Sample: Left



(d) Restricted Sample: Right



(e) Restricted Sample: Extreme Right

Table 2.3: Tests of equal distributions of surnames initials

P-Value	Restricted samples				
	All	Extreme-Left	Left	Right	Extreme-Right
KS	0.211	0.782	0.094*	0.855	0.377
Median	0.320	0.774	0.132	0.622	0.474
MWW	0.385	0.652	0.0546*	0.583	0.372

The table presents the P-values of three tests of equal distributions: Kolmogorov-Smirnov (KS), non-parametric test of equality of medians (Median), and Mann-Whitney-Wilcoxon rank-sum test (MWW). The null hypothesis is that the distributions of first letters in the surnames is the same across male and female candidates. Column 1 considers all candidates. In columns 2 to 5, each subsample considers only the candidates who are the only ones of the considered party in the precinct where they run.

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

to about 12% of the electoral ballots of the considered elections. To do so, we used three types of data. First, the Centre for Political Research of SciencesPo (CEVIPOF) provided 780 ballots. Secondly, exploiting the fact that some departments recorded a numeric version of the ballots (namely the departments of Allier, Aude, Ille-et-Villaine, Loire-Atlantique and Savoie), we systematically contacted the administrative centers in charge of the election. We managed to recover 168 ballots from the *Loire-Atlantique* department. Finally, we systematically looked up for pictures of ballots on the Internet, using Google, Twitter and Facebook keywords⁷. Using this methodology, we managed to recover 191 full ballots.

To the best of our knowledge, this represents the first effort to collect and analyze ballot layouts in a systematic way. Yet, because our dataset is not complete, it might be subject to biases. In particular, because the data collected by the Centre for Political Research of Sciences Po are based on voluntary contributions of voters, it tends to over represent precincts located in urban areas. Secondly, online data might over-represent famous candidates, who might be more likely to campaign online. On the other hand, it might also allow candidates without a strong visibility to get a wider audience. In Table 2.4, we regress the availability of the ballot on the main characteristics of the candidates for each of the subsamples of interest, using a logistic regression.

Overall, we find differences in terms of age and socio professional categories. The ballots we analyze are indeed those of slightly younger candidates, especially among left-wing can-

7. Using in particular requests such as "Bulletins de vote élections départementales 2015", or other versions of it.

Table 2.4: Determinants of ballot availability

Ballot Availability	Restricted Samples				
	All	Extreme Left	Left	Right	Extreme Right
Woman First	-0.056 (0.064)	-0.057 (0.190)	-0.085 (0.172)	-0.172 (0.169)	-0.014 (0.145)
Previous Political Exp. (W)	-0.119 (0.076)	0.128 (0.236)	-0.088 (0.175)	-0.104 (0.176)	0.092 (0.259)
Previous Political Exp. (M)	-0.084 (0.075)	-0.114 (0.213)	-0.140 (0.184)	0.067 (0.215)	0.024 (0.209)
Age (W)	-0.004 (0.003)	-0.012 (0.010)	-0.019 (0.010)*	-0.002 (0.010)	0.004 (0.007)
Age (M)	-0.009 (0.003)***	-0.009 (0.010)	-0.029 (0.009)***	-0.006 (0.009)	0.002 (0.006)
Intermediary Profession (W)	-0.186 (0.178)	-1.194 (1.086)	-0.712 (0.804)	-0.169 (0.339)	-0.282 (0.322)
Private Sector Employee (W)	0.150 (0.113)	-0.453 (0.381)	-0.113 (0.366)	-0.549 (0.262)**	0.048 (0.217)
Liberal Occupation (W)	0.312 (0.150)**	0.797 (0.474)*	-0.013 (0.470)	0.010 (0.309)	0.196 (0.413)
Education Occupation (W)	0.289 (0.131)**	-0.219 (0.391)	0.416 (0.372)	0.070 (0.331)	0.299 (0.352)
Civil Servant(W)	0.031 (0.137)	-0.238 (0.388)	0.475 (0.366)	-0.597 (0.352)*	-0.219 (0.397)
Public Firm Worker (W)	0.271 (0.179)	-0.419 (0.519)	0.318 (0.495)	-0.121 (0.462)	0.271 (0.478)
Retired (W)	0.014 (0.136)	-0.501 (0.416)	0.498 (0.400)	-0.690 (0.346)**	-0.144 (0.272)
Intermediary Profession (M)	0.363 (0.149)**	0.922 (0.780)	-0.616 (0.520)	0.084 (0.343)	1.085 (0.411)***
Private Sector Employee (M)	0.254 (0.131)*	0.793 (0.462)*	-0.342 (0.341)	0.010 (0.302)	0.772 (0.388)**
Liberal Occupation (M)	0.163 (0.162)	0.812 (0.634)	-0.066 (0.411)	-0.053 (0.326)	0.866 (0.503)*
Education Occupation (M)	0.178 (0.151)	-0.382 (0.539)	-0.273 (0.358)	-0.087 (0.397)	1.520 (0.437)***
Civil Servant(M)	0.203 (0.152)	0.049 (0.526)	-0.364 (0.355)	0.252 (0.364)	1.342 (0.452)***
Public Firm Worker (M)	0.381 (0.188)**	0.241 (0.576)	-0.094 (0.438)	-0.163 (0.538)	1.074 (0.662)
Retired (M)	0.077 (0.145)	0.285 (0.515)	-0.309 (0.373)	-0.059 (0.309)	0.740 (0.420)*
XLeft	-0.154 (0.144)				
Left	0.081 (0.128)				
Right	-0.010 (0.129)				
XRight	-0.195 (0.134)				
Pseudo R2	0.01	0.04	0.04	0.02	0.02
Chi2	71.04	35.94	39.82	17.28	24.68
N	9,081	1,187	1,341	1,389	1,883

Logistic Regressions. Column 1 considers all candidates. In columns 2 to 5 each subsample considers only the candidates who are the only ones of the considered party in the precinct where they run. The outcome is a variable equal to one if we could observe the ballot and zero otherwise. The coefficients on male and female candidates' occupations are expressed considering farmers and other occupations as the reference modality. Standard errors in parentheses are clustered at the precinct level in column 1, and robust in columns 2 to 5.

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

didates. At the extreme-left, we observe more ballots when the female candidate has a liberal occupation, and when the male candidate is working in the private sector. Among right-wing candidates, ballots are more likely to be observed if the female candidate is working in the private sector or as a civil servant, or is retired. Finally, among extreme-right candidates, imbalances are found on most of male occupations (except for public firm workers). Nevertheless, three important comments need to be made. First and foremost, the position of the female candidate is not predictive of the availability of the ballot. Second, while some differences are significant, they explain a small share of ballot availability, and we cannot reject the null hypothesis of joint nullity of the estimates for the restricted samples of right-wing and extreme-right candidates. Finally, no party seems to be over-represented in the sample.

In Table 2.5, we provide evidence that the treatment status is uncorrelated with the reporting decision and the kind of information reported. We categorized the type of information into three types: declared past or present political experience, age and occupation. Moreover, since it is possible to put the picture of the candidates on the ballot, we identified the pairs of candidates who did so. We observe, that out of 1,139 ballots available, 36% have some kind of information reported for at least one candidate : 35% of the ballots report information related to the male candidate and 33% report information related to the female candidate. 26% of the ballots report information related to the political experience of the male candidate and 22% report information related to the political experience of the female candidate. 5% of male candidates report their occupations, while it is the case of 7% of female candidates. Less than 1% of male and female candidates report their age. Finally, about 9% of the candidates put their picture on the ballot. We also observe that the decision to report any information is very correlated between male and female candidates: out of 412 ballots with at least one information, 88% report information for both candidates. Importantly, none of these reporting decisions are correlated to the treatment.

3 Estimation strategy

Our main estimation strategy aims at analyzing whether candidates lose or gain from having the female candidate first on the ballot.

Table 2.5: Balance check on reported information: all candidates

	Man First	N	Woman First	N	Diff	T-Stat
At least one information	0.362	575	0.362	564	0.000	0.001
At least one information (M)	0.348	575	0.351	564	-0.003	-0.114
At least one information (W)	0.327	575	0.340	564	-0.013	-0.481
Information: Political Experience (M)	0.268	575	0.253	564	0.014	0.548
Information: Political Experience (W)	0.221	575	0.220	564	0.001	0.041
Information: Occupation (M)	0.050	575	0.060	564	-0.010	-0.726
Information: Occupation (W)	0.066	575	0.070	564	-0.005	-0.323
Information: Age (M)	0.009	575	0.005	564	0.003	0.682
Information: Age (W)	0.009	575	0.005	564	0.003	0.682
Photo	0.090	575	0.092	564	-0.002	-0.103

This table presents T-Tests of difference of information reporting across treatment status for the full sample of available ballots

In an initial specification, we test whether, on average, the electoral performances of pairs where the female candidate is first on the ballot are different from those where the male candidate is first. Identification takes place within the potential outcomes framework from the Rubin Causal Model, where we assume two potential outcomes for each unit i - $Y_i(0)$ and $Y_i(1)$ - and the causal effect of the program on the unit i is defined as $\tau_i = Y_i(1) - Y_i(0)$. The actual observed outcome is defined as such:

$$Y_i^{obs} = \begin{cases} Y_i(0) & \text{if } T_i = 0 \\ Y_i(1) & \text{if } T_i = 1 \end{cases}$$

In this framework, the Average Treatment Effect is defined as $ATE = \mathbb{E}[Y_i(1) - Y_i(0)]$. A naive estimate of this quantity is given by $\overline{Y_1^{obs}} - \overline{Y_0^{obs}}$. In general, such a quantity is unbiased under the *Stable Unit Treatment Value Assumption* (SUTVA) and the *complete randomization assumption*.

As explained above, the *SUTVA* is likely to be violated if we do not restrict our analysis to a sample of observations which cannot interact with each other (meaning that the treatment status of one observation will not affect the outcome of any other unit). To do so, we therefore restrict our analysis to candidates who are the only ones to represent their party in the precinct.

The second assumption states that both the potential outcomes and the covariates are in-

dependent from the treatment. Formally, the condition writes as such:

$$T_i \perp (Y_i(0), Y_i(1), X_i)$$

In our setting, the treatment-assignment is based on a procedure which is supposedly as-good-as-random, since the order of the candidates (and hence the place of the female candidate) on the ballot is determined by alphabetical order. However, as we have shown in the last section, while the treatment assignment is hardly affected by candidates' characteristics, the covariates are not systematically perfectly balanced across treatment status. In our setting, it therefore seems more plausible to assume the milder assumption of *unconfoundedness*, which states that the potential outcomes and the treatment are independent after controlling for covariates potentially affecting them. Formally, this assumption writes:

$$T_i \perp (Y_i(0), Y_i(1)) | X_i$$

Our baseline OLS specification is therefore the following:

$$Y_i = \alpha + \beta T_i + \delta X_i + \epsilon_i \tag{2.1}$$

where Y_i is an outcome variable indicating the electoral performance of pair i , T_i is the treatment variable, which is equal to 1 if the female candidate in pair i is first on the ballot and 0 otherwise, X_i is a set of candidates characteristics, and ϵ_i is an error term.

While our main specification does not model how the electoral performance of a pair of candidates depends on the characteristics of the other candidates, in additional specifications we control for the average characteristics of the opponents of the considered pair, and compare the results of the different candidates pairwise.

4 Results

4.1 Main estimation

In this section, we present our main results, by estimating equation (1). In order to do so, we compare the scores received by candidates in the first round of the election in the control and

in the treatment group. Note that in this setting, the number of candidates is not identical in each precinct, and the scores of pairs facing different are therefore not directly comparable. In order to make the electoral performances comparable across different number of candidates, we control in each regression for the number of candidates competing in the precinct.

In Table 2.1, we test whether the order of the candidates affect the electoral performance of the pairs. It summarizes the estimates of such an average treatment effect across several specifications. Panel (A) reports results without any controls except the number of candidates in the precinct. Panel (B) reports results when we also control for individual characteristics. Panel (C) involves the same control variables, but interacts the characteristics of male and female candidates. Panel (D) is similar to the third one, but also controls for precinct characteristics (including the average age of the population, the share of voters in rural areas, the share of voters with at least an undergraduate degree, and the unemployment rate, all as of 2013), and for the first letter of the female's surname.

Overall, the results suggest that the performances of extreme-left, left-wing and extreme-right pairs are not affected by the order of appearance of the candidates. However, right-wing pairs lose a sizable share of votes if the female candidate is first. Estimates of the loss range between 1.4 and 1.9 points, representing between 4 and 5.4 percents of the average vote share. Importantly, the magnitude of the coefficient is very similar across the specifications, and especially stable in all the specifications including covariates, suggesting that the inclusion of covariates hardly affects the general pattern.

This discrimination had a substantial electoral impact. In Table 2.2, we show that gender discrimination prevented some right-wing pairs of candidates from winning the election. More specifically we regress a dummy variable indicating whether the considered pair reached the second round or won the election during the first round. Panel (A) includes no control except the number of competing candidates. Panel (B) includes the broadest set of controls - namely, interacted individual characteristics from the candidates, number of competing candidates, precinct characteristics and the first letter of the woman's surname. We find that right-wing candidates were between 3.9 and 4.9 percentage points less likely to reach the second round or win the election in the first round, corresponding to a lower probability ranging between

Table 2.1: Effect on share of votes in the first round

(A)	XLeft	Left	Right	XRight
Woman First	0.381 (0.396)	-0.291 (0.536)	-1.878 (0.536)***	0.035 (0.348)
R^2	0.15	0.14	0.28	0.09
N	1,188	1,341	1,391	1,893
Indiv. Controls	N	N	N	N
Precinct characteristics	N	N	N	N
First letter of the woman's surname	N	N	N	N
Number of candidates	Y	Y	Y	Y
(B)	XLeft	Left	Right	XRight
Woman First	0.084 (0.350)	-0.206 (0.480)	-1.589 (0.497)***	0.066 (0.327)
R^2	0.35	0.32	0.40	0.22
N	1,187	1,341	1,389	1,883
Indiv. Controls	Y	Y	Y	Y
Precinct characteristics	N	N	N	N
First letter of the woman's surname	N	N	N	N
Number of candidates	Y	Y	Y	Y
(C)	XLeft	Left	Right	XRight
Woman First	0.123 (0.364)	-0.149 (0.492)	-1.583 (0.511)***	0.122 (0.335)
R^2	0.39	0.37	0.43	0.25
N	1,187	1,341	1,389	1,883
Indiv. Controls	Inter.	Inter.	Inter.	Inter.
Precinct characteristics	N	N	N	N
First letter of the woman's surname	N	N	N	N
Number of candidates	Y	Y	Y	Y
(D)	XLeft	Left	Right	XRight
Woman First	-0.085 (0.420)	-0.206 (0.586)	-1.397 (0.581)**	0.429 (0.378)
R^2	0.43	0.41	0.49	0.38
N	1,187	1,334	1,389	1,882
Indiv. Controls	Inter.	Inter.	Inter.	Inter.
Precinct characteristics	Y	Y	Y	Y
First letter of the woman's surname	Y	Y	Y	Y
Number of candidates	Y	Y	Y	Y
Mean of Outcome Variable	10.66	28.44	34.91	25.79

OLS Regressions. Each subsample considers only the candidates who are the only ones of the considered party in the precinct where they run. The outcome variable is the share of votes received by each pair of candidates in the first round of the election. Panel (A) controls only for the number of candidates in the precinct. Panel (B) also controls for age, socioprofessional categories and political experience of male and female candidates. Panel (C) controls for the same variables but interacts the age of man and woman, the socioprofessional categories of man and woman, and the political experience of man and woman. Panel (D) adds to these controls the first letter of the woman's surname, as well as the unemployment rate, the average age of the population, the share of individuals with a graduate degree and the share of voters living in rural areas within the precincts. Robust standard errors between parentheses.

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

Table 2.2: Effect on probability of getting to the second round or of winning the election in the first round

(A)	XLeft	Left	Right	XRight
Woman First	0.015 (0.013)	0.002 (0.026)	-0.049 (0.020)**	-0.004 (0.023)
R^2	0.01	0.01	0.03	0.01
N	1,188	1,341	1,391	1,893
Indiv. Controls	N	N	N	N
Precinct characteristics	N	N	N	N
First letter of the woman's surname	N	N	N	N
Number of candidates	Y	Y	Y	Y
(B)	XLeft	Left	Right	XRight
Woman First	0.010 (0.015)	0.013 (0.030)	-0.039 (0.022)*	0.002 (0.026)
R^2	0.26	0.26	0.25	0.22
N	1,187	1,334	1,389	1,882
Indiv. Controls	Inter.	Inter.	Inter.	Inter.
Precinct characteristics	Y	Y	Y	Y
First letter of the woman's surname	Y	Y	Y	Y
Number of candidates	Y	Y	Y	Y
Mean of the Outcome Variable	0.058	0.64	0.83	0.58

OLS Regressions. Each subsample considers only the candidates who are the only ones of the considered party in the precinct where they run. The outcome variable is a dummy variable indicating whether the pair of candidates went to the second round of the election or was elected in the first round. Panel (A) controls only for the number of candidates in the precinct. Panel (B) also controls for interacted age of man and woman, interacted socioprofessional categories of man and woman, interacted political experience of man and woman, the first letter of the woman's surname, as well as the unemployment rate, the average age of the population, the share of individuals with a graduate degree and the share of voters living in rural areas within the precincts. Robust standard errors between parentheses.

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

Table 2.3: Effect on probability of being elected

(A)	XLeft	Left	Right	XRight
Woman First	0.006 (0.012)	-0.016 (0.026)	-0.045 (0.026)*	-0.001 (0.006)
R^2	0.01	0.00	0.04	0.01
N	1,188	1,341	1,391	1,893
Indiv. Controls	N	N	N	N
Precinct characteristics	N	N	N	N
First letter of the woman's surname	N	N	N	N
Number of candidates	Y	Y	Y	Y
(B)	XLeft	Left	Right	XRight
Woman First	0.002 (0.014)	-0.016 (0.032)	-0.040 (0.031)	0.006 (0.007)
R^2	0.22	0.19	0.24	0.14
N	1,187	1,334	1,389	1,882
Indiv. Controls	Inter.	Inter.	Inter.	Inter.
Precinct characteristics	Y	Y	Y	Y
First letter of the woman's surname	Y	Y	Y	Y
Number of candidates	Y	Y	Y	Y
Mean of the Outcome Variable	0.044	0.35	0.57	0.016

OLS Regressions. Each subsample considers only the candidates who are the only ones of the considered party in the precinct where they run. The outcome variable is a dummy variable indicating whether the pair of candidates eventually won the election. Panel (A) controls only for the number of candidates in the precinct. Panel (B) also controls for interacted age of man and woman, interacted socioprofessional categories of man and woman, interacted political experience of man and woman, the first letter of the woman's surname, as well as the unemployment rate, the average age of the population, the share of individuals with a graduate degree and the share of voters living in rural areas within the precincts. Robust standard errors between parentheses.

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

4.7 and 5.9 percents.

This gender bias seems to have affected the final result of the election. In Table 2.3, we regress a dummy indicating whether the considered pair won the election on the treatment status. Controls are defined in the same way as in Table 2.2. Overall, we find that, because of gender discrimination, right-wing pairs of candidates were between 4 and 4.5 points less likely to win the election. An important point is that the magnitude of the results is exactly the same as the magnitude observed when we considered the probability of going to the second round or winning the election in the first round. It suggests that the overall effect is channeled through the probability of reaching the second. In fact, we find no treatment effect during the second round⁸. This additional noise explains why our results are less significant: the simplest specification only yields significance at the 10% level, and the treatment effect is not significant anymore when we include covariates - even though the point estimates are very stable.

8. These results are available upon request.

4.2 Alternative specifications

Full Sample

In Table 2.4, we run the same baseline model on the full population of candidates. While in such a setting we cannot exclude that the *SUTVA* is violated, it provides consistent evidence that our main estimates are not an artifact of our sample selection. Panel (A) reports average treatment effects on the vote shares during the first round on the population of candidates in each of the four specifications detailed in our main estimation - controlling in each of them for the party of the candidate. We find no evidence of treatment effects whatsoever.

However, when we interact the treatment with a dummy indicating that the pair of candidates is from the right-wing, we find a strongly negative interaction term, of the same magnitude than the one found in the main specification (i.e. between -1.4 and -1.5 percentage points).

Opponents' characteristics and dyadic estimation

In this section, we check that our estimates are not affected by the characteristics of the political opponents faced by a given pair of candidates. In Table 2.5, we run the most stringent regression of the main specification - including interacted individual characteristics, the first letter of the female's surname and the characteristics of the precinct - controlling for the average characteristics of the male and female opponents on the age, political experience and occupation dimensions, as well as for the share of opponents with a female candidate listed first. We still find a statistically significant effect on the restricted sample of right-wing candidates, even though the effect is smaller and drops down to 1 percentage point.

Finally, in order to take into account more thoroughly the structure of the political competition, we compute for each pair of candidates the difference between their score and the score of each of their opponents in the first round of the election. We then regress the relative score between the considered pair and its considered opponent on their respective characteristics and treatment statuses.

Table 2.4: OLS estimation on Full Sample

(A)	(1)	(2)	(3)	(4)
Woman first	-0.117 (0.210)	-0.191 (0.185)	-0.172 (0.186)	-0.187 (0.226)
R^2	0.40	0.54	0.54	0.55
N	9,097	9,081	9,081	9,018
Indiv. Controls	N	Y	Inter.	Inter.
Precinct characteristics	N	N	N	Y
First letter of the woman's surname	N	N	N	Y
Number of candidates	Y	Y	Y	Y
(B)	(1)	(2)	(3)	(4)
Woman First	0.445 (0.492)	0.489 (0.493)	0.443 (0.502)	0.432 (0.506)
Extreme Left	-1.286 (0.447)***	-1.070 (0.440)**	-1.041 (0.444)**	-0.910 (0.438)**
Left	12.081 (0.465)***	8.007 (0.451)***	7.988 (0.456)***	7.972 (0.449)***
Right	15.981 (0.508)***	11.044 (0.497)***	11.064 (0.499)***	11.076 (0.494)***
Extreme Right	12.703 (0.459)***	14.880 (0.461)***	14.845 (0.466)***	14.989 (0.462)***
Woman First*Extreme Left	0.048 (0.628)	-0.383 (0.609)	-0.351 (0.617)	-0.338 (0.607)
Woman First*Left	-0.143 (0.649)	-0.452 (0.618)	-0.388 (0.627)	-0.426 (0.617)
Woman First*Right	-1.510 (0.695)**	-1.464 (0.654)**	-1.389 (0.659)**	-1.418 (0.646)**
Woman First*Extreme Right	-0.378 (0.629)	-0.313 (0.609)	-0.214 (0.619)	-0.163 (0.615)
R^2	0.40	0.54	0.54	0.55
N	9,097	9,081	9,081	9,018
Indiv. Controls	N	Y	Inter.	Inter.
Precinct characteristics	N	N	N	Y
First letter of the woman's surname	N	N	N	Y
Number of candidates	Y	Y	Y	Y

OLS Regressions. All columns consider the full population of candidates. The outcome variable is the share of votes received by each pair of candidates in the first round of the election. Panel (A) presents the treatment effect on the full population. Panel (B) interacts this treatment with the party of the candidates. Column (1) controls only for the number of candidates in the precinct and the party of each candidate. Column (2) also controls for age, socioprofessional categories and political experience of male and female candidates. Column (3) controls for the same variables but interacts the age of man and woman, the socioprofessional categories of man and woman, and the political experience of man and woman. Column (4) adds to these controls the first letter of the woman's surname, as well as the unemployment rate, the average age of the population, the share of individuals with a graduate degree and the share of voters living in rural areas within the precincts. Clustered standard errors at the precinct level in parentheses.

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

Table 2.5: Effect on votes in the first round, controlling for average characteristics of opponents

Share of votes in the first round	XLeft	Left	Right	XRight
Woman First	0.064 (0.401)	0.267 (0.537)	-1.065 (0.512)**	0.427 (0.375)
R^2	0.48	0.52	0.59	0.40
N	1,187	1,333	1,387	1,882
Indiv. Controls	Inter.	Inter.	Inter.	Inter.
Precinct characteristics	Y	Y	Y	Y
First letter of the woman's surname	Y	Y	Y	Y
Number of candidates	Y	Y	Y	Y
Mean of opponents' characteristics	Y	Y	Y	Y

OLS Regressions. Each subsample considers only the candidates who are the only ones of the considered party in the precinct where they run. The outcome variable is the share of votes received by each pair of candidates in the first round of the election. Each regression controls for the number of candidates in the precinct, the interacted age of man and woman, interacted socioprofessional categories of man and woman, interacted political experience of man and woman, the first letter of the woman's surname, and the average of each of these variables among the competing candidates in the precinct. It also controls for the unemployment rate, the average age of the population, the share of individuals with a graduate degree and the share of voters living in rural areas within the precincts. Robust standard errors between parentheses.

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

Formally, we therefore run the following estimation:

$$Y_{ij} = \alpha + \beta T_i + \gamma T'_j + \delta X_i + \nu X'_j + \epsilon_{ij} \quad (2.2)$$

where Y_{ij} is the difference between the score of the pair i and the score of the pair j , T_i is the treatment status of pair i , T'_j is the treatment status of pair j , X_i is a set of characteristics of pair i , X'_j is a set of characteristics of pair j , and ϵ_{ij} is an error term.

We run the specifications in the same fashion as in the main specification. Panel (A) controls only for the number of competing candidates. Panel (B) controls for the characteristics of each dyad of pairs. Panel (C) controls for the same characteristics, but interacting them within each pair of the dyad. Finally, panel (D) adds as controls the first letter of each woman in the dyad, and the sociodemographic characteristics of the precinct.

The results of this estimation are gathered in Table 2.6. The results look very similar to the main estimation: we do not find any treatment effect for extreme-left, left-wing and right-wing candidates, but we do find a negative treatment effect for right-wing candidates, corresponding to between -1.4 and -2 percentage points. To the contrary, we do not find, in any of the specifications, that the treatment status of the considered opponent affects the score of the considered pair.

Table 2.6: Results: Dyadic Specification

(A)	XLeft	Left	Right	XRight
Woman First	0.595 (0.482)	-0.384 (0.675)	-2.058 (0.702)***	-0.020 (0.463)
Woman First (Opponent)	0.193 (0.404)	0.105 (0.504)	-0.391 (0.498)	0.072 (0.377)
R^2	0.09	0.03	0.01	0.07
N	4,450	4,413	4,333	6,603
Indiv. Controls	N	N	N	N
Indiv. Controls (Opponent)	N	N	N	N
Precinct characteristics	N	N	N	N
First letter of the woman's surname	N	N	N	N
Number of candidates	Y	Y	Y	Y
(B)	XLeft	Left	Right	XRight
Woman First	0.163 (0.432)	-0.002 (0.537)	-1.442 (0.567)**	-0.060 (0.13)
Woman First (Opponent)	0.075 (0.325)	0.483 (0.381)	-0.160 (0.374)	0.312 (1.03)
R^2	0.40	0.47	0.44	0.40
N	4,438	4,406	4,316	6,569
Indiv. Controls	Y	Y	Y	Y
Indiv. Controls (Opponent)	Y	Y	Y	Y
Precinct characteristics	N	N	N	N
First letter of the woman's surname	N	N	N	N
Number of candidates	Y	Y	Y	Y
(C)	XLeft	Left	Right	XRight
Woman First	0.110 (0.444)	0.042 (0.543)	-1.603 (0.574)***	0.042 (0.466)
Woman First (Opponent)	0.017 (0.330)	0.470 (0.372)	-0.062 (0.375)	0.217 (0.306)
R^2	0.43	0.50	0.47	0.42
N	4,438	4,406	4,316	6,569
Indiv. Controls	Inter.	Inter.	Inter.	Inter.
Indiv. Controls (Opponent)	Inter.	Inter.	Inter.	Inter.
Precinct characteristics	N	N	N	N
First letter of the woman's surname	N	N	N	N
Number of candidates	Y	Y	Y	Y
(D)	XLeft	Left	Right	XRight
Woman First	-0.049 (0.513)	0.315 (0.660)	-1.460 (0.635)**	0.409 (0.504)
Woman First (Opponent)	0.067 (0.326)	0.530 (0.371)	0.087 (0.363)	0.189 (0.288)
R^2	0.45	0.51	0.51	0.48
N	4,438	4,406	4,316	6,569
Indiv. Controls	Inter.	Inter.	Inter.	Inter.
Indiv. Controls (Opponent)	Inter.	Inter.	Inter.	Inter.
Precinct characteristics	Y	Y	Y	Y
First letter of the woman's surname	Y	Y	Y	Y
Number of candidates	Y	Y	Y	Y

OLS Regressions. Each subsample considers only the candidates who are the only ones of the considered party in the precinct where they run, and compares them to all of their political opponents. The outcome variable is the difference between the share of votes of the considered pair and the share of the considered competing pair. Panel (A) controls only for the number of candidates in the precinct. Panel (B) also controls for age, socioprofessional categories and political experience of man and woman, within the considered pair and the competing pair, as well as for the party of the competing pair. Panel (C) controls for the same variables but interacts the age of man and woman, the socioprofessional categories of man and woman, and the political experience of man and woman within the considered pair and the competing pair. Panel (D) adds to these controls the first letter of the woman's surname in the considered pair, as well as the unemployment rate, the average age of the population, the share of individuals with a graduate degree and the share of voters living in rural areas within the precincts. Standard errors clustered at the precinct level between parentheses.

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

5 Channels

5.1 Taste-based or statistical discrimination ?

How can this observed gender discrimination be explained ? On the one hand, voters may be reluctant to vote for women, regardless of their characteristics or quality. We would then talk, in the spirit of Becker (1957), of taste-based discrimination. On the other hand, if the characteristics and quality of candidates are not perfectly observable by the voters, they might apply potentially negative group stereotypes on the female candidate. In that case, we would then talk, following the seminal contributions of Arrow et al. (1973) and Phelps (1972), of statistical discrimination. In this section, in the spirit of Altonji and Pierret (2001), we show evidence pointing towards the presence of statistical discrimination.

It is worth noticing that, in our particular setting, testing properly for the presence of statistical discrimination needs to cope with an additional element: the limited-attention bias from the voters. As we explained above, according to the electoral law, two elected candidates from a same ballot have exactly the same prerogatives once in office: there is no hierarchy between them. In this light, had voters perfectly known this framework, they should not have been influenced by the relative positions of the two candidates of the ballot.

In such a framework, testing for statistical discrimination requires a shifter of information that affects the knowledge that the voters have of candidates, while keeping the level of information about the electoral rule constant. To do so, we exploit an additional feature of the electoral rule, that allows candidates to report additional information on the ballot. Importantly, this additional information is only about the candidate herself, and is not informative about the rule of the election. Consequently, it is unlikely to affect the understanding that a voter has about the general rules of the election. Using this information we test whether, conditional on characteristics that we can observe thanks to administrative data but which might not be observed by the voters, discrimination is lower when these information are revealed on the ballot.

It is important to notice that, in the theory of statistical discrimination, individuals have imperfect information about the quality of the people they face. Contrarily to some settings where quality is easily observable (such as transaction data on the housing market, for example

Laouénan and Rathelot (2017)) getting a proper measure of the quality of a politician is difficult. Most of the literature on the topic proxied the quality of politicians with their education level (Ferraz and Finan (2009), Besley, Montalvo, and Reynal-Querol (2011), Daniele and Geys (2015) among others), or with the performance of their constituency (Alesina, Troiano, and Cassidy (2015), Daniele and Vertier (2016)). However, recent contributions found new ways of measuring political competence, notably through earnings and IQ score (T. Besley et al. (2017), Dal Bó et al. (2017)). In our study, we do not observe such characteristics, nor the actual performance of previously elected leaders in office: hence the extent to which we can control for the quality of politicians is limited. However, the information we have on candidates embeds part of it, since it includes previous political experience.

In Table 2.1, we show that reporting information matters for electoral results. For sake of brevity, we only present results on the whole sample of ballots that we could manually recover. Here again, we explain the share of votes received in the first round and present different specifications, with an increasing number of controls, and controlling in each of them for the number of candidates in the precinct and the party of the considered pair of candidates. The results presented in this table cannot be interpreted as causal, since the fact of reporting information might be correlated to unobservable characteristics which also matter for electoral success. Nevertheless, it is indicative of the role that information might play in the electoral process.

Overall, we find that, conditional on observed characteristics, the ballots which report at least one type of information for at least one candidate receive between 2.4 and 2.6 points more than their counterparts. This advantage seems to be coming from reported information about political experience: if at least one of the candidates mentions such experience on the ballot, the pair gains between 3 and 3.2 percentage points more. Conversely, if any of the candidates mentions her occupation or prints her picture, they do not seem to have an advantage⁹.

In Table 2.2, we show how reported information affects discrimination against right-wing women in the first round of the election. Namely, we evaluate whether displaying information on the ballot affects the discrimination faced by right-wing female candidates. To do so, we

9. Note that, because of bunching of information reporting, both by gender and by type of information, disentangling the impact of information by gender and by type is hardly feasible in our setting.

Table 2.1: Ballots with reported information gain more votes

(A) - Full Sample of available ballots	(1)	(2)	(3)	(4)
At least one information	2.655 (0.686) ^{***}			
Photo		1.408 (1.000)		
Any information on political experience			3.279 (0.749) ^{***}	
Any information on occupation				-0.801 (1.123)
R^2	0.56	0.55	0.56	0.55
N	1,138	1,138	1,138	1,138
Indiv. Controls	Y	Y	Y	Y
Precinct characteristics	N	N	N	N
Number of candidates	Y	Y	Y	Y
(B) - Full Sample of available ballots	(1)	(2)	(3)	(4)
At least one information	2.438 (0.710) ^{***}			
Photo		1.297 (1.031)		
Any information on political experience			3.049 (0.789) ^{***}	
Any information on occupation				-0.949 (1.166)
R^2	0.58	0.57	0.58	0.57
N	1,138	1,138	1,138	1,138
Indiv. Controls	Inter.	Inter.	Inter.	Inter.
Precinct characteristics	N	N	N	N
Number of candidates	Y	Y	Y	Y
(C) - Full Sample of available ballots	(1)	(2)	(3)	(4)
At least one information	2.509 (0.712) ^{***}			
Photo		1.346 (1.059)		
Any information on political experience			3.151 (0.792) ^{***}	
Any information on occupation				-0.876 (1.157)
R^2	0.58	0.57	0.58	0.57
N	1,137	1,137	1,137	1,137
Indiv. Controls	Inter.	Inter.	Inter.	Inter.
Precinct characteristics	Y	Y	Y	Y
Number of candidates	Y	Y	Y	Y

OLS Regressions. Each column considers the full sample of candidates for which we could observe a ballot. The outcome variable is the share of votes received by the pair of candidates in the first round of the election. Panel (A) controls for the number of candidates in the precinct, as well as the age, socioprofessional categories and political experience of male and female candidates. Panel (B) controls for the same variables but interacts the age of man and woman, the socioprofessional categories of man and woman, and the political experience of man and woman. Panel (C) adds to these controls the unemployment rate, the average age of the population, the share of individuals with a graduate degree and the share of voters living in rural areas within the precincts. Clustered standard errors at the precinct level in parentheses.

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

interact the treatment variable with a dummy indicating whether any type of information is available on the ballot. In this case, we observe that, for right-wing candidates, discrimination disappears when information is displayed on the ballot: while, on ballots with no information, discrimination seems to be particularly high - with about 5 to 5.8 points less of received votes when the female candidate is listed first - this effect is totally cancelled out when at least one information about the candidates is revealed. This result holds for all the specifications even after controlling for individual and locals characteristics. Therefore, it suggests the presence of statistical discrimination.

Such a finding could be explained by the historically low representation of women among right-wing politicians - since, as the literature on the topic as shown (Beaman et al. (2009), De Paola, Scoppa, and Lombardo (2010)), exposure to women in office increases the probability of voting for them in the future. As a matter of fact, the main right-wing party has often preferred to field male candidates in various types of elections - notably during the parliamentary elections of the decade 2000 which were subject to gender quotas - while other parties were more compliant.

In the following paragraphs, we explore whether alternative explanations are likely to explain our results.

5.2 Are candidates with political experience less likely to be discriminated ?

One might worry that the difference of vote shares that we observe when a female is listed first or second only reflect the differences of underlying characteristics existing between them. Let us assume that voters believe that the first candidate is the "main" candidate and that they do not have a preference over the gender of this candidate. If the quality of female candidates is lower than the quality of male candidates and the voters vote based the quality of the presumed "main" candidate, then the observed result might only reflect this underlying difference of quality between male and female candidates.

While we do not observe the quality of the candidates, we do observe a proxy of it: the political experience of the candidate. In Table 2.3, we interact the treatment with the incumbency

Table 2.2: Information affects the level of discrimination among right-wing female candidates

Share of votes in the first round	(1)	(2)	(3)	(4)
Woman First	-4.962 (2.430)**	-5.737 (1.975)***	-5.813 (2.029)***	-5.127 (1.811)***
Any Info. Ballot	-0.064 (2.068)	-2.435 (1.782)	-2.580 (1.899)	-2.846 (1.733)
Woman First*Any Info. Ballot	5.292 (2.931)*	7.521 (2.649)***	7.584 (2.704)***	6.710 (2.545)***
R^2	0.18	0.42	0.42	0.54
N	165	165	165	165
Indiv. Controls	N	Y	Inter.	Inter.
Precinct characteristics	N	N	N	Y
Number of candidates	Y	Y	Y	Y

OLS Regressions. Each column considers the restricted sample of right-wing pairs of candidates for which we could observe the ballot. The outcome variable is the share of votes received by the pair of candidates in the first round of the election. Column (1) controls only for the number of candidates in the precinct. Column (2) also controls for the age, socioprofessional categories and political experience of male and female candidates. Column (3) controls for the same variables but interacts the age of man and woman, the socioprofessional categories of man and woman, and the political experience of man and woman. Column (4) adds to these controls the unemployment rate, the average age of the population, the share of individuals with a graduate degree and the share of voters living in rural areas within the precincts. Robust standard errors in parentheses.

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

status of the candidates and show that discrimination is not responsive to it. In panels (A) and (B), we show that the treatment effect does not vary with respect to past political experience of either the female (Panel (A)) or the male candidate (Panel (B)), whatever the stringency of the set of included controls: in all cases, the interaction term is not statistically significant. Thus, differences of experience between male and female candidates do not drive directly the effect we detect.

5.3 Where Did the Missing Votes Go ?

Right-wing pairs of candidates received less votes when the female candidate was listed first on the ballot. A key question is therefore to understand where these lost votes went. A first hypothesis is that discriminatory voters did not show up on the day of election, leading to a differential abstention. This hypothesis cannot be ruled out, since every voters receive the ballots and electoral programs of all candidates at home. A second hypothesis is that voters who might have voted for the right-wing pair, were the male candidate first, ended up casting no ballot at all or invalid ones: in this case, we would expect an increase in blank and invalid ballots. Finally, discriminatory voters might instead have cast their ballot for another pair

Table 2.3: Absence of treatment heterogeneity with respect to male and female characteristics on the right-wing ballots

(A)	(1)	(2)	(3)	(4)
Woman First	-2.073 (0.858)**	-1.528 (0.805)*	-1.395 (0.804)*	-1.161 (0.762)
Previously Elected (W)	5.334 (0.752)***	4.175 (0.744)***	4.107 (0.748)***	3.716 (0.707)***
Woman First*Previously Elected (W)	0.243 (1.075)	-0.098 (1.019)	-0.309 (1.017)	-0.368 (0.966)
R^2	0.33	0.40	0.43	0.48
N	1,391	1,389	1,389	1,389
Indiv. Controls	N	Y	Inter.	Inter.
Precinct characteristics	N	N	N	Y
Number of candidates	Y	Y	Y	Y
(B)	(1)	(2)	(3)	(4)
Woman First	-2.435 (1.135)**	-2.083 (1.091)*	-1.941 (1.138)*	-1.835 (1.063)*
Previously Elected (M)	6.506 (0.917)***	4.700 (0.905)***	5.036 (0.928)***	4.774 (0.877)***
Woman First*Previously Elected (M)	1.046 (1.275)	0.627 (1.227)	0.451 (1.270)	0.569 (1.199)
R^2	0.34	0.40	0.43	0.48
N	1,391	1,389	1,389	1,389
Indiv. Controls	N	Y	Inter.	Inter.
Precinct characteristics	N	N	N	Y
Number of candidates	Y	Y	Y	Y

OLS Regressions. Each column considers the restricted sample of right-wing pairs. The outcome variable is the share of votes received by the pair of candidates in the first round of the election. Column (1) controls only for the number of candidates in the precinct. Column (2) also controls for the age, socioprofessional categories and political experience of male and female candidates. Column (3) controls for the same variables but interacts the age of man and woman, the socioprofessional categories of man and woman, and the political experience of man and woman. Column (4) adds to these controls the unemployment rate, the average age of the population, the share of individuals with a graduate degree and the share of voters living in rural areas within the precincts. Panel (A) interacts the treatment with the political experience of the female candidate. Panel (B) interacts the treatment with the political experience of the male candidate. Robust standard errors in parentheses.

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

Table 2.4: Abstention, Blank and Null votes do not depend on the treatment status of the right-wing candidate

(A) - Abstention Rate	(1)	(2)	(3)	(4)
Right-Wing Woman First	0.002 (0.003)	0.003 (0.003)	0.003 (0.003)	-0.001 (0.002)
R^2	0.10	0.15	0.21	0.63
N	1,391	1,389	1,389	1,389
Indiv. Controls	N	Y	Inter.	Inter.
Precinct characteristics	N	N	N	Y
Number of candidates	Y	Y	Y	Y
(B) - Blank and Null Votes	(1)	(2)	(3)	(4)
Right-Wing Woman First	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
R^2	0.41	0.45	0.47	0.55
N	1,391	1,389	1,389	1,389
Indiv. Controls	N	Y	Inter.	Inter.
Precinct characteristics	N	N	N	Y
Number of candidates	Y	Y	Y	Y

OLS Regressions. Each column considers the restricted sample of precincts with only one right-wing candidate. In Panel (A), the outcome variable is the abstention rate in the precinct. In Panel (B), the outcome variable is the share of blank and null votes in the precinct. Column (1) controls only for the number of candidates in the precinct. Column (2) also controls for the age, socioprofessional categories and political experience of male and female candidates among the right-wing pair. Column (3) controls for the same variables but interacts the age of man and woman, the socioprofessional categories of man and woman, and the political experience of man and woman within the right-wing pair. Column (4) adds to these controls the unemployment rate, the average age of the population, the share of individuals with a graduate degree and the share of voters living in rural areas within the precincts. Robust standard errors in parentheses.

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

of candidates: in this case, we would expect an increase in the share of votes of the other candidates.

We test these hypotheses in Tables 2.4 and 2.5, focusing on constituencies where only one right-wing candidate ran, and on the treatment status of this candidate. Here again, we present results for different types of specification. The results in Table 2.4 suggest that there exists no differential abstention between the precincts where the female right-wing candidate was listed first and those where she was listed second. This result confirms that the decisions leading to a lower share of votes for female-led right-wing candidates were unlikely to be made before the election day. Similarly, we do not find a higher share of blank or null votes in these constituencies. In both cases, this absence of effect holds whatever the specification.

In Table 2.5, we check whether the opponents of the right-wing candidate in these precincts received a higher share of votes in the first round when the right-wing female candidate was listed first on the ballot. In Panel (A), we regress the score of each competing pair of candidates

on the treatment status of the right-wing pair. Overall, we find that when the right-wing female candidate was listed first on the ballot, the competing pairs had on average between 0.33 and 0.51 points higher shares of votes. This effect is significant at least at the 10% level across all the specifications. In Panel (B), we propose an indirect test of absence of taste-based discrimination. Namely, for all competing pairs of candidate, we check whether the additional vote shares they received when the right-wing woman was listed first differed with their own treatment status - i.e. with the position of the female candidate on their own ballot. Our results suggest that while opponents received more votes when they faced a right-wing pair with a female candidate listed first, this advantage did not depend on the position of the woman on their own ballot. This result therefore leads us to argue that the discrimination we identify is unlikely to be taste-based: had it been so, we would have expected opponents to receive less votes if their own female candidate was listed first. In other terms, we would have expected a negative and significant interaction term in the regressions of Panel (B).

5.4 Variation across precinct characteristics

In this section, we test whether discrimination varies with local characteristics of the precinct. Namely, we test whether the treatment effect that we find varies with respect to the level of education of the population (measured through the share of people above 15 holding a graduate degree), the unemployment rate among the population aged between 15 and 64, and the average age of the population. Finally, we relate our observed treatment effect to discrimination against women on the labor market. To do so, we build on data released by Chamkhi (2015), reporting unexplained wage gaps between men and women in 321 French employment zones in 2010. These unexplained wage gaps are computed from a Oaxaca-Blinder decomposition, controlling for a wide range of explanatory factors. We then interact this measure of discrimination with the treatment variable. In this section, we use municipality-level data for two reasons. First, the characteristics of local population and the vote shares are available at the municipality level. Secondly, because the precincts and the employment zones overlap, it is preferable to study the relationship between the electoral and job-market discrimination at

Table 2.5: Votes for political opponents of the right-wing pairs

(A)	(1)	(2)	(3)	(4)
Right-Wing Woman First	0.508 (0.196)***	0.360 (0.187)*	0.402 (0.189)**	0.334 (0.186)*
R^2	0.44	0.57	0.58	0.58
N	4,333	4,321	4,321	4,321
Indiv. Controls	N	Y	Inter.	Inter.
Precinct characteristics	N	N	N	Y
Number of candidates	Y	Y	Y	Y
(B)	(1)	(2)	(3)	(4)
Right-Wing Woman First	0.383 (0.327)	0.264 (0.293)	0.305 (0.294)	0.221 (0.292)
Woman First	0.147 (0.362)	0.091 (0.313)	0.080 (0.314)	0.049 (0.313)
Right-Wing Woman First*Woman First	0.255 (0.521)	0.190 (0.452)	0.193 (0.455)	0.225 (0.454)
R^2	0.44	0.57	0.58	0.58
N	4,333	4,321	4,321	4,321
Indiv. Controls	N	Y	Inter.	Inter.
Precinct characteristics	N	N	N	Y
Number of candidates	Y	Y	Y	Y

OLS Regressions. Each column considers the opponents of the right-wing pair within the restricted sample of precincts including only one right-wing pair. The outcome variable is the share of votes received by the considered competing pair in the first round of the election. Panel (A) reports the effect, for a political opponent of the right-wing pair, of having a female listed first on the right-wing ballot. Panel (B), interacts this effect with the treatment status of the considered political opponents. Column (1) controls only for the number of candidates in the precinct and the party of the considered competing pair. Column (2) also controls for the age, socioprofessional categories and political experience of male and female candidates within the considered pair. Column (3) controls for the same variables but interacts the age of man and woman, the socioprofessional categories of man and woman, and the political experience of man and woman. Column (4) adds to these controls the unemployment rate, the average age of the population, the share of individuals with a graduate degree and the share of voters living in rural areas within the precincts level in parentheses.

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

Table 2.6: Heterogeneity with respect to local characteristics

Share of votes in the first round	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Woman First	-0.788 (0.938)	-0.690 (5.839)	-1.350 (1.309)	0.022 (2.574)	-0.636 (0.942)	-1.565 (5.771)	-0.902 (1.261)	-0.192 (2.522)
Unemployment Rate	-38.170 (4.618)***				-40.522 (4.734)***			
Woman First*Unemployment Rate	-4.374 (6.293)				-5.134 (6.301)			
Mean Age		0.278 (0.084)***				0.345 (0.085)***		
Woman First*Mean Age		-0.013 (0.120)				0.008 (0.119)		
Share Graduate			3.769 (3.522)				2.082 (3.560)	
Woman First*Share Graduate			-0.019 (4.888)				-1.269 (4.753)	
Unexplained Wage Gap				-0.422 (0.185)**				-0.393 (0.182)**
Woman First*Unexplained Wage Gap				0.153 (0.271)				0.109 (0.267)
R^2	0.31	0.30	0.29	0.29	0.31	0.31	0.31	0.31
N	25,964	25,966	25,966	25,966	25,964	25,964	25,964	25,964
Indiv. Controls	Inter.	Inter.	Inter.	Inter.	Inter.	Inter.	Inter.	Inter.
Municipality characteristics	N	N	N	N	Y	Y	Y	Y
Number of candidates	Y	Y	Y	Y	Y	Y	Y	Y

OLS Regressions at the municipality level. Each column considers the restricted sample of right-wing pairs of candidates for which we could observe the ballot. The outcome variable is the share of votes received by the pair of candidates in the first round of the election. Columns (1) to (4) control for the number of candidates in the precinct, the interacted age of the man and woman, the interacted socioprofessional categories of the man and woman, and the interacted political experience of the man and woman. Columns (5) to (8) adds to these controls the unemployment rate, the average age of the population, the share of individuals with a graduate degree within the municipality and a dummy variable indicating whether this municipality is located in a rural area. Standard errors clustered at the precinct level in parentheses.

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

the municipality level¹⁰.

We present the results of these interactions in Tables 2.6, 2.7 and 2.8. In Table 2.6, we interact the treatment variable directly with the different local characteristics of interest. For sake of brevity, we present only the results for the two most stringent specifications. For all the variables considered, we detect no interaction effect. However, one might worry that this absence of result comes from non-linear interactions. Therefore, in Tables 2.7 and 2.8, we present interactions with respect to the top and bottom deciles of each of the considered local characteristic variable.

Overall, we find no interaction effect with the top and bottom deciles of age, education and unemployment. However, we do find that discrimination is greater in areas belonging to the top decile of unexplained wage gap on the labor market. In particular, we find that in these areas, the discriminatory effect is greater by 2.7 to 3.2 percentage points, depending on the specification.

10. Note that all the previous results also hold at the municipality level.

Table 2.7: Heterogeneity with respect to local characteristics

Share of votes in the first round	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Woman First	-1.244 (0.595)**	-1.278 (0.576)**	-1.371 (0.618)**	-1.008 (0.624)	-1.155 (0.595)*	-1.187 (0.559)**	-1.163 (0.599)*	-0.870 (0.607)
Top Decile Unemployment	-3.329 (0.588)***				-3.307 (0.597)***			
Woman First*Top Decile Unemployment	-0.546 (0.770)				-0.651 (0.772)			
Top Decile Mean Age		2.476 (0.810)***				2.734 (0.805)***		
Woman First*Top Decile Unemployment		-0.606 (1.145)				-0.282 (1.141)		
Top Decile Share Graduate			1.288 (0.612)**				1.086 (0.614)*	
Woman First*Top Decile Share Graduate			0.135 (0.890)				-0.050 (0.875)	
Top Decile Unexplained Wage Gap				2.937 (1.178)**				2.768 (1.162)**
Woman First*Top Decile Unexplained Wage Gap				-3.237 (1.521)**				-2.736 (1.495)*
R^2	0.30	0.29	0.29	0.29	0.30	0.31	0.31	0.31
N	25,966	25,966	25,966	25,966	25,966	25,964	25,964	25,964
Indiv. Controls	Inter.	Inter.	Inter.	Inter.	Inter.	Inter.	Inter.	Inter.
Municipality characteristics	N	N	N	N	Y	Y	Y	Y
Number of candidates	Y	Y	Y	Y	Y	Y	Y	Y

OLS Regressions at the municipality level. Each column considers the restricted sample of right-wing pairs of candidates for which we could observe the ballot. The outcome variable is the share of votes received by the pair of candidates in the first round of the election. Columns (1) to (4) control for the number of candidates in the precinct, the interacted age of the man and woman, the interacted socioprofessional categories of the man and woman, and the interacted political experience of the man and woman. Columns (5) to (8) adds to these controls the unemployment rate, the average age of the population, the share of individuals with a graduate degree within the municipality and a dummy variable indicating whether this municipality is located in a rural area. Standard errors clustered at the precinct level in parentheses.

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

Table 2.8: Heterogeneity with respect to local characteristics

Share of votes in the first round	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Woman First	-1.411 (0.569)**	-1.321 (0.606)**	-1.401 (0.581)**	-1.477 (0.630)**	-1.340 (0.571)**	-1.185 (0.591)**	-1.233 (0.566)**	-1.305 (0.614)**
Bottom Decile Unemployment	3.670 (0.568)***				3.349 (0.557)***			
Woman First*Bottom Decile Unemployment	0.638 (0.860)				0.645 (0.867)			
Bottom Decile Mean Age		-0.567 (0.581)				-0.727 (0.573)		
Woman First*Bottom Decile Mean Age		-0.461 (0.825)				-0.634 (0.813)		
Bottom Decile Share Graduate			0.717 (0.744)				1.011 (0.717)	
Woman First*Bottom Decile Share Graduate			0.247 (0.969)				0.427 (0.926)	
Bottom Decile Unexplained Wage Gap				-0.876 (0.931)				-0.816 (0.909)
Woman First*Bottom Decile Unexplained Wage Gap				0.853 (1.595)				1.098 (1.551)
R^2	0.30	0.29	0.29	0.29	0.30	0.31	0.31	0.31
N	25,966	25,966	25,966	25,966	25,966	25,964	25,964	25,964
Indiv. Controls	Inter.	Inter.	Inter.	Inter.	Inter.	Inter.	Inter.	Inter.
Municipality characteristics	N	N	N	N	Y	Y	Y	Y
Number of candidates	Y	Y	Y	Y	Y	Y	Y	Y

OLS Regressions at the municipality level. Each column considers the restricted sample of right-wing pairs of candidates for which we could observe the ballot. The outcome variable is the share of votes received by the pair of candidates in the first round of the election. Columns (1) to (4) control for the number of candidates in the precinct, the interacted age of the man and woman, the interacted socioprofessional categories of the man and woman, and the interacted political experience of the man and woman. Columns (5) to (8) adds to these controls the unemployment rate, the average age of the population, the share of individuals with a graduate degree within the municipality and a dummy variable indicating whether this municipality is located in a rural area. Standard errors clustered at the precinct level in parentheses.

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

These results call for two comments. First the absence of interaction with the characteristics of the population in the precinct might reflect an aggregation effect, coming from the fact that different types of population might be subject to limited attention and discriminatory behaviors¹¹. Secondly, the fact that gender discrimination in politics and on the labor market are linked suggests that policies aiming at reducing discrimination should tackle those different aspects simultaneously.

6 Conclusion

Among the numerous reasons which might explain why women are under-represented in politics, gender-bias of voters is frequently considered as a potential candidate. While several pieces of research argue that gender-biases are unlikely to play a role, isolating such effects using actual electoral data can prove complicated, due to the presence of selection effects.

In this paper, we isolate gender-biases from selection effects using a natural experiment in France. Using the fact that the candidates of the *Départementales* elections of 2015 had to run for the first time by gender-balanced pairs, and considering that the order of the candidates on the ballot is determined by alphabetical order, we show that the gender of the first candidate on the ballot is as good-as-random. This framework therefore allows us to disentangle cleanly selection effects and gender-biases, since we compare pairs of candidates which are on average similar, but which differ only in the order of male and female candidates on the ballot.

We detect a sizable gender-bias affecting right-wing female candidates, due to voters who arguably were simultaneously subject to limited attention concerning the rules of the election and to discriminatory behaviors. Overall, the right-wing pairs where the female candidate was listed first on the ballot saw their score in the first round decrease by about 1.5 percentage points, and their probability of going to the second round or of winning the election in the first round decreasing by 4 percentage points. Furthermore, we provide evidence that this discrimination is rather statistical than taste-based.

Such results call for several important comments. First and foremost, while we find evi-

11. In fact, identifying the respective roles of these two effects among different categories of population cannot be done with aggregate administrative data, and calls for field or laboratory experiments which we reserve for future research.

dence of gender-biases against right-wing candidates, the absence of evidence concerning the candidates of other parties does not necessarily imply that they are not also affected by gender biases. Indeed, not detecting evidence of discrimination for other parties can be either explained by the fact that the voters are less subject to limited attention or by the fact that they discriminate less.

Secondly, since limited attention seems to be at the heart of our result, it is crucial to understand what are its determinants. Indeed, as acknowledged by DellaVigna (2009), understanding limited attention requires to know the cost of acquiring relevant information about the decision which is made - in our case, about the electoral rule. While our setting prevents us from investigating this matter further, such findings raise important questions about how the electoral rules and the governmental action are perceived by the citizens.

Thirdly, since the information available on the ballot on the day of election seems to affect both the overall electoral performances of the candidates and the discrimination that women face, a broader consideration should be paid about to the design of electoral ballots.

Finally, since we find greater electoral discrimination in places where discrimination against women on the labor market is higher, gender discrimination among voters in politics is unlikely to be reduced without other coordinated policies.

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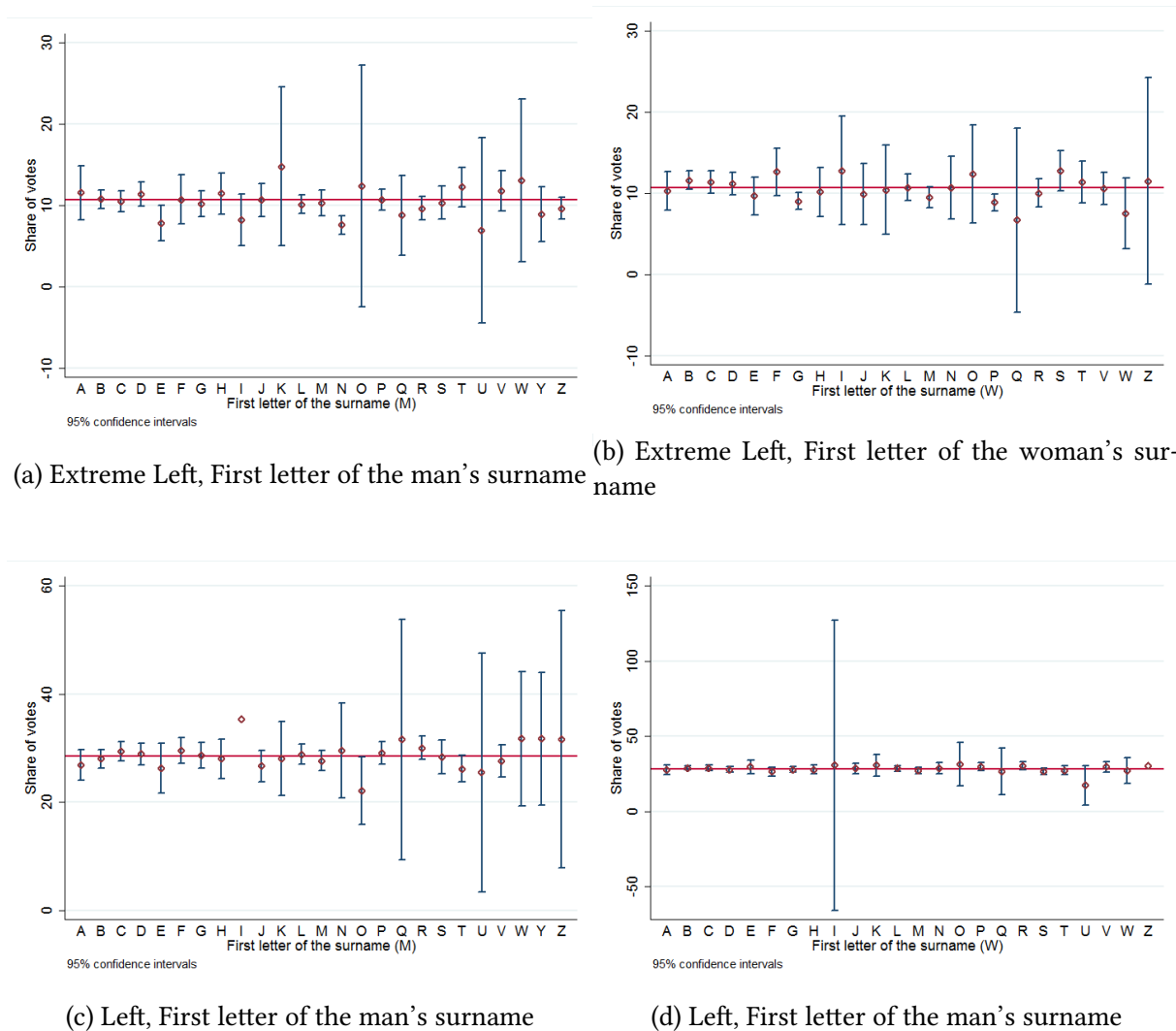
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Appendix

2.A Distribution of vote shares in each subsample, across first letter of surnames

Figure 2.A.1: Distribution of vote shares in the first round across first letter of candidates' surname (Restricted samples, Extreme-Left and Left)



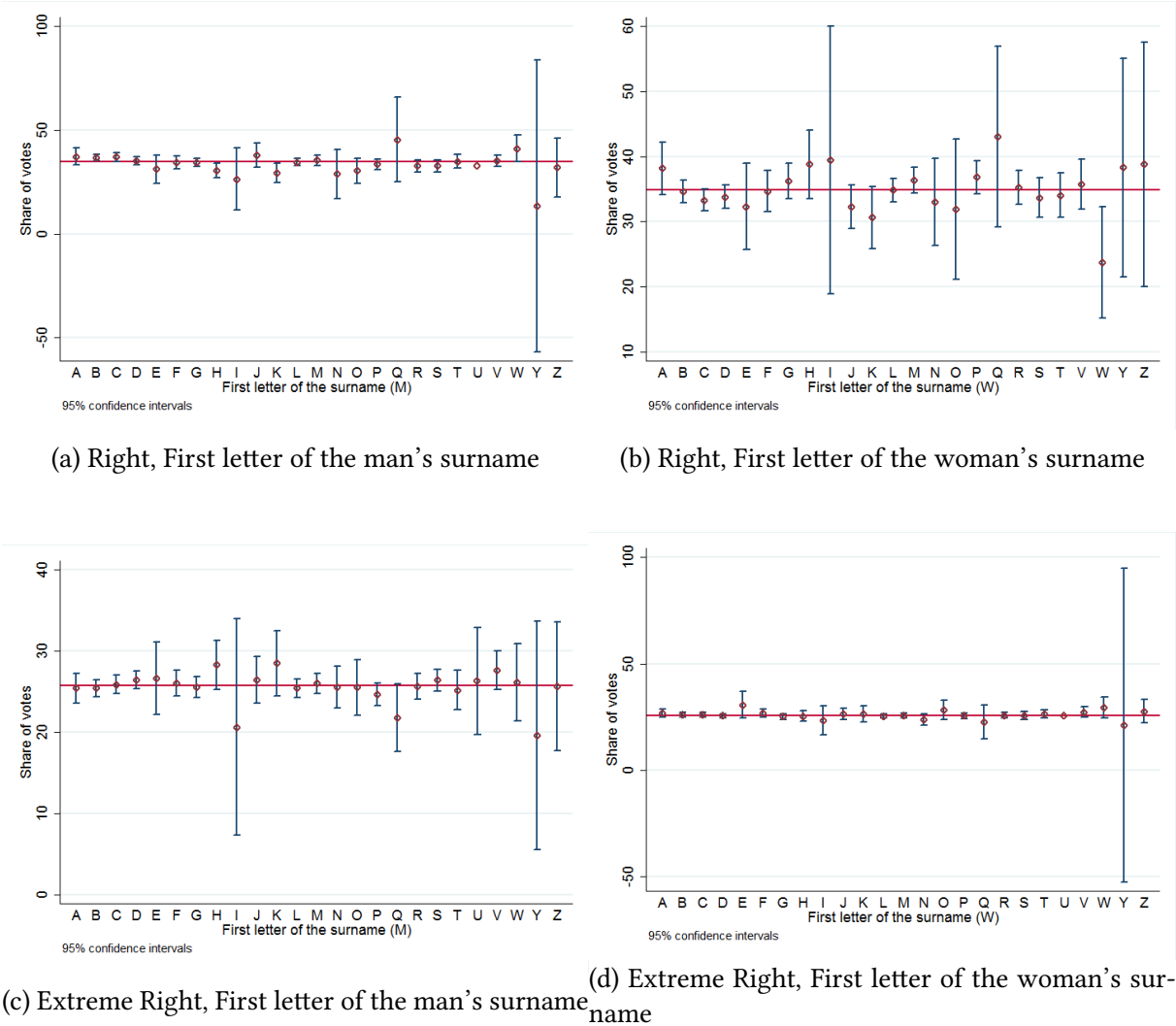
(a) Extreme Left, First letter of the man's surname

(b) Extreme Left, First letter of the woman's surname

(c) Left, First letter of the man's surname

(d) Left, First letter of the man's surname

Figure 2.A.2: Distribution of vote shares in the first round across first letter of candidates' surname (Restricted samples, Right and Extreme-Right)



Chapter 3

”Dismantling the ”Jungle”: Migrant Relocation and Extreme Voting in France

This paper is co-authored with Max Viskanic

1 Introduction and Background

Is there a link a between the recent migrant crisis and the raise of far-right votes in Europe? In the last years, the number of asylum applications in the European Union increased dramatically, from 431 thousand in 2013, to 627 thousand in 2014 and close to 1.3 million in 2015. Arguably this influx, which is double the amount of the peak asylum application in the aftermath of the Yugoslavian conflict in the 1990s (Eurostat (2016)), had electoral repercussions in numerous European countries. Recent literature in the context of large immigration inflows has documented that large flows of immigrants have led to increases in radical votes and especially far-right votes (represented by parties such as FPÖ (Austria), AfD (Germany) or Lega Nord (Italy)). On the other hand little is known regarding the impact of *small scale* migrant inflows and their electoral repercussions.

In this paper we try to fill this gap by examining as an event study the dismantlement of the Calais “Jungle”, an encampment just outside the city of Calais, in the North of France. During the migrant crisis this illegal squatter camp, increased in population reaching nearly 6,400 inhabitants in October 2016 (Le Monde (2016)), shortly before it was closed and the

inhabitants, mostly migrants, relocated. Those migrants were relocated to about 200 to 400 temporary migrant centres called *Centres d'Accueil et d'Orientation* (CAOs) all over the country. We link municipality level variation in the exposure to small numbers of migrants to electoral outcomes. We focus specifically on the vote share of the *Front National* (National Front), the major far-right wing party in France. During the campaign prior to the presidential election in May 2017 the *Front National's* rhetoric was generally anti-immigrant, which brought the migrant crisis at the heart of the presidential debate. This was demonstrated most prominently in the general media, but also on the party's social media, their public gatherings as well as election manifesto.¹

In order to achieve exogenous variation in the exposure of French municipalities to relocated migrants we instrument the presence of a CAO with the presence and size of holiday villages in the same municipality. The reason why we expect a high positive correlation between the presence of the CAO and the holiday villages is the fact that one of the many criteria of the location of the CAOs was potential additional space in those holiday villages, given that the resolution of the "Jungle" took place mostly in October 2016. The holiday villages would be unoccupied at that time and could thus be used as temporary shelters for migrants. At the same time the stock of holiday villages is determined much before the current migrant surge that led to the creation of the CAOs. Thus our exclusion restriction is likely warranted and we are thus able to estimate the causal effect of the migrant relocation on votes in favour of the *Front National*. Carrying out our empirical analysis, we find the presence of a CAO to have a negative effect on the vote share of the *Front National*. The growth in vote share of the *Front National* between the 2012 and 2017 presidential elections is decreased by 15.7 percentage points in those municipalities. Given that the average increase of FN votes over this period corresponded to about 20%, this indicates that the increase in *Front National* vote of municipalities with a CAO was 25% the one of municipalities without a CAO. These results point towards direct effects of exposure to migrants consistent with the contact hypothesis (Allport (1954)). Indeed, migrants were meant to stay for a short period of time (typically less than three months), and they were also unlikely to affect the local economy for several rea-

1. See for example La Croix (2017), BBC (2017) and Le Monde (2017) amongst others.

sons. First, the cost of relocation was fully taken charge of by the government. Secondly, they did not have the right to work and received no financial transfers. In fact, we show that their arrival does not seem to have impacted local economic activity.

Our main interpretation of our findings is that citizens developed a greater degree of acceptance towards migrants and hence were less likely to vote for the *Front National*. These results seem to be confirmed by the fact that we observe an increase in the share of votes received by the far-left party *Front de Gauche*, which has a more open stance towards migrants, but similar political platform on other issues. Furthermore, we find spillover effects of the presence of the CAOs on neighbouring municipalities. Municipalities within a five km radius had a lower growth rate of vote share for the *Front National* by about 1.8 percentage points. Overall, we also find a stronger decrease of vote shares of the *Front National* in more diverse municipalities with a larger share of younger people. On the other hand effects are dampened in municipalities which were exposed to more migrants and where the mayors volunteered to welcome them. Importantly our calculations suggest that in municipalities that had over 39 beds per 1000 inhabitants the impact on the *Front National* vote outcome is positive. This finding reconciles the fact that large inflows of immigrants contributed to the rise of Right wing parties as will be discussed in the following paragraphs.

We add to the literature in two ways. First of all, this paper is part of a large strand of literature documenting the electoral repercussions of immigration. Whereas most of the literature has focused on large and long-lasting impacts of immigrants on voting behavior, little is known about the effects of short and small-scale exposure to migrants. Studies examining large inflows of immigrants have generally found a positive impact on far-right votes (Barone et al. (2016), Halla, Wagner, and Zweimueller (forthcoming), Harmon (forthcoming), Otto and Steinhardt (2014), Mendez and Cutillas (2014), Brunner and Kuhn (2014), Becker and Fetzer (2016), Viskanic (2017)). Most of those papers rely to some degree on the instrument proposed by Card (2001) which uses the prior allocation of immigrants as way to obtain exogenous variation in immigrant allocation and thus solve the issue of geographical selection.

In the wake of the migrant crisis, recent contributions analyzed the effects of exposure to migrants on voting behaviors and attitudes toward migrants, with diverging results. More

specifically, Hangartner et al. (2017a) and Hangartner et al. (2017b) found that voters on Greek Islands which were more exposed to large inflows of migrants were more likely to develop hostility towards them, and to vote for the Golden Dawn party, one of the major far-right parties in Greece. Conversely, Steinmayr (2016) shows that municipalities of Upper Austria which received migrants were less likely to vote for far-right parties. On the other hand Dustmann, Vasiljeva, and Damm (2016) show that the effects of exogenous migrant relocation on voting behavior in Denmark are heterogeneous and depend crucially on the characteristics of the localities: in particular, while positive effects on anti-immigration parties are found in rural areas, this effect is reversed in urban areas. These results highlight the importance of taking into account both municipality characteristics and the intensive margin of exposure to migrants.

Our paper combines these approaches by focusing on the electoral effects of receiving a small number of migrants (typically a few dozens), conditionally on long-term exposure to immigrants. Furthermore, our rich dataset allows us to explore how the results vary at the intensive margin (number of migrants) and depending on the characteristics of the population. From this point of view, the threshold effect that we find (above 39 migrants per 1000 inhabitants, the *Front National* vote increases), reconciles it with findings on large inflows of migrants.

Secondly, our framework allows us to isolate a direct effect of migrant relocation on voting behavior, which is unlikely to occur through intermediary variables. A large literature in economics has considered the links between immigration and the labour market (Card (1990), Altonji and Card (1991), Borjas (2003), Ortega and Peri (2009), Ottaviano and Peri (2012), Guriev and Vakulenko (2002), among others), public finance (Gott and Johnstone (2002), OECD (2015), Vargas-Silva (2015)) or crime (Moehling and Piehl (2009), Bianchi, Buonanno, and Pinotti (2012), Mastruboni and Pinotti (2016)), which in turn are likely to affect electoral outcomes. In particular, variations on the labour market affect extreme votes, notably through trade shocks (Autor et al. (2016), Malgouyres (2017), Dippel et al. (2017)), or unemployment (Algan et al. (2017)). In this paper, we argue that our results are not affected by variations on the labour market or in local public finance. Overall, while national exposure to immi-

gration shapes attitudes towards migrants (Hainmueller and Hopkins (2014)), we show that small-scale contacts are also likely to play an important role.

The paper is organised as follows. Section 2 provides the institutional framework and data description, section 3 presents the empirical specification and identification, Section 4 presents the main results on the allocation of the migrants together with the main results on the vote share of the *Front National*, Section 5 provides some heterogeneous effects, robustness checks as well as falsification exercises can be found in section 6, whereas section 7 concludes.

2 Institutional Framework and Data

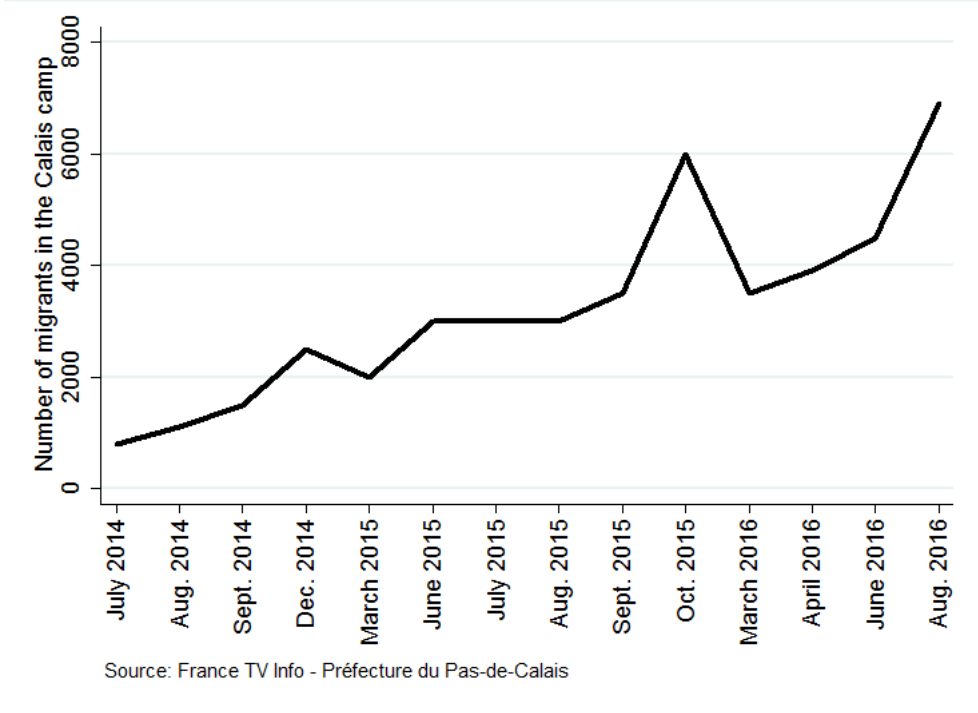
In the following subsections we first provide qualitative and quantitative details on the Calais Camp and its dismantlement. Then we outline the functioning of the French presidential elections and outline our various data sources used and controls employed.

2.1 Migrants and the Calais “Jungle”

The Calais “Jungle” was an informal migrant camp, which first took form in the late 1990s, was progressively extended during the 2000s, and grew massively following the European migrant crisis in 2014-2015, reaching a peak of more than 7,000 inhabitants in late 2015 (Figure 3.1). Following this massive inflation of the “Jungle”, the government decided to progressively dismantle the camp starting from October 2015, through the creation of CAOs (*Centres d’Accueil et d’Orientation*). These centres, whose creation was ordered on October 27th 2015, aim at receiving migrants who have not yet started any procedure to obtain refugee status. Migrants allocated to the CAOs are thus meant to stay only for a short period of time, typically for less than three months. During this period, they are offered administrative assistance and bed and board, but they do not receive any financial allocation (nor do they have the right to work legally). The average cost of a day in a CAO is about 25 euros. However, it is the government and not the municipalities which pay for it (Ministère de l’Intérieur (2017)). The migrants who have started a procedure to obtain a migrant status are redirected to the CADA (*Centres d’Accueil pour Demandeurs d’Asile*), which also offers bed and board together with administrative assistance, while awaiting decision. The first of these centres were created in the 1970s,

and could host up to 25,000 migrants as of 2015. (Ministère de l’Intérieur (2017)). Between 2015 and 2017, the number of places in CADA increased to around 40,000 places (La Cimade (2017)). Although the network of CADAs is the largest structure used to host asylum-seekers, other structures were created over time, such as the AT-SA (*Accueil Temporaire du Service de l’Asile* - 6,000 places as of 2017), the HUDA (*Hebergement d’Urgence des Demandeurs d’Asile* - 15,000 places as of 2017), the CPH (*Centre Provisoire d’Hebergement* - 2,300 places as of 2017), and PRAHDA (*Programme d’Accueil et d’Hebergement des Demandeurs d’Asile* - 5,351 as of 2017) (La Cimade (2017)).

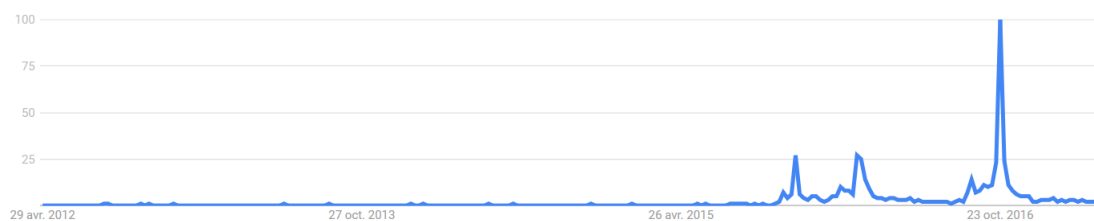
Figure 3.1: Evolution of the number of migrants in the Calais camp



The dismantling of the Calais camp occurred in several stages from October 2015 to October 2016. Overall, the government reports having relocated 13,366 migrants since October 2015, and more than 7,000 inhabitants during the sole dismantling of October 2016. This event received considerable media attention, as we can see from Figure 3.2, showing the number of Google searches for “Jungle de Calais” (“Jungle of Calais”) over time.

Focusing on the dismantling of the “Jungle” raises different challenges. First of all, the criteria of allocation of the CAOs have not been clearly defined, which makes the use of an

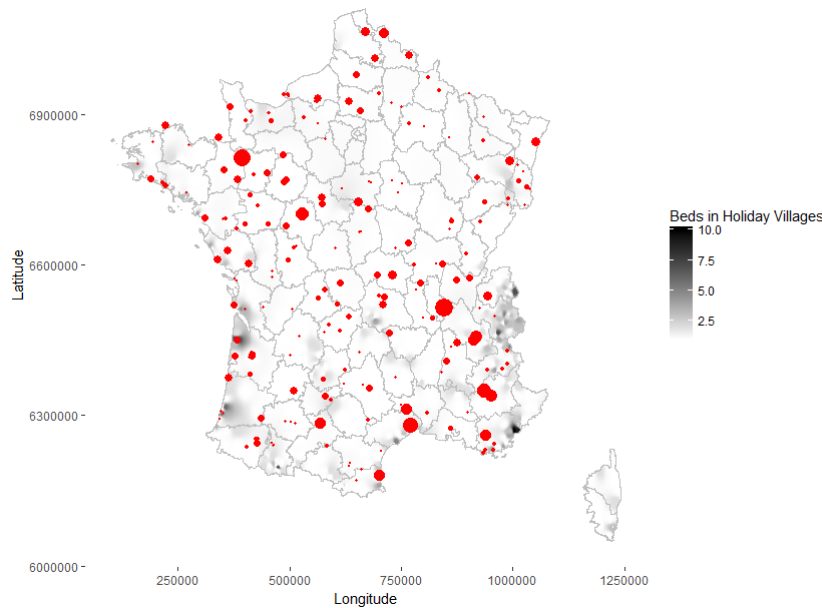
Figure 3.2: Google Trends for the expression “Jungle de Calais”



instrument for its assignment mandatory. During the final dismantling of October 2016, even though the government announced that the allocation of CAOs across regions would be based on “socio-demographic criteria” (Ministère de l’Intérieur (2017)), no comprehensive list of factors was provided. Therefore our paper will also be devoted to documenting, on observables, which municipalities were chosen to host migrants. The only indication that was given was that the Parisian agglomeration (Ile-de-France) and Corsica would not be considered. Those two regions are thus excluded from our analysis and Corsica will be used as an additional robustness check in section 6. Since no migrants were allocated to Corsica, if our instrument is valid, then holiday villages in Corsica should not be systematically related to any political outcomes. Another issue to consider is the extent to which the mayors of concerned municipalities were involved in the process of the allocation of the CAOs. Although many mayors were contacted to receive migrants (Le Monde (2015), Association des Maires de France (2016)), during the final dismantling, the Minister of Interior, Bernard Cazeneuve, entrusted the final decision to the local representatives of the government i.e. the *préfets*.² The *préfets* would first identify suitable premises without prior consultation of the concerned municipalities, and then negotiate with the mayors. In our analysis, even though the compliance of mayors is not generally observed, we exploit additional information about a list of mayors who publicly declared, in September 2015, their willingness to welcome migrants. We do this in order to investigate whether the effects are stronger in those municipalities.

2. The *préfets* have authority at the provincial level of the *département*.

Figure 3.3: CAOs and density of holiday villages capacity



2.2 French Presidential Elections

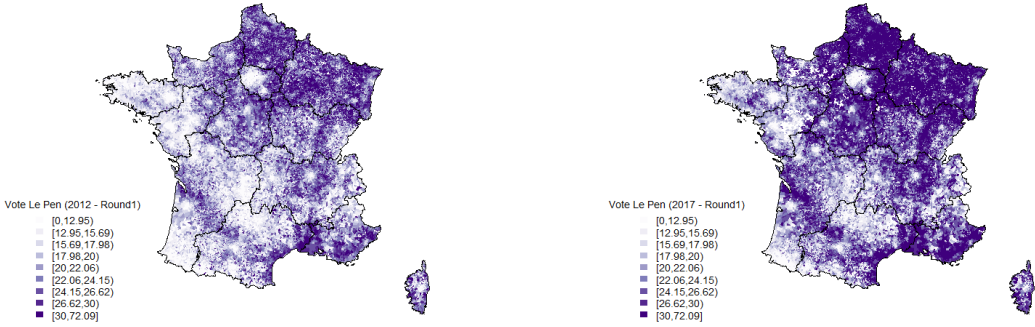
French presidential elections are held every five years since 2002, using a two-round majoritarian system. After the first round, if no candidate received more than 50% of the expressed votes, a second round is held between the two candidates with the largest vote share. We collect the vote shares of all the candidates in the presidential elections in 1995, 2002, 2007, 2012 and 2017, for each French municipality.

Our main outcome of interest is the share of votes received by the *Front National* candidates in the first round of the presidential election. The candidates from this party over the last three decades were all members of the Le Pen family: Jean-Marie Le Pen (founder of the *Front National*) was candidate from 1988 to 2007, while his daughter Marine Le Pen was candidate in 2012 and 2017.³ Figure 3.4 shows the geographic repartition of FN voters in the presidential elections of 2012 and 2017 in France. The *Front National*'s strongholds are located in the south-eastern and north-eastern parts of France, where more than 30% of the population voted in favour of Marine Le Pen both in 2012 and 2017. As indicated by the common scale of colours

3. The *Front National* was not the only far-right party represented in these elections. Other conservative candidates, sharing some of the rhetoric of the *Front National* were also represented in the 2007 election (Philippe de Villiers), as well as in the 2012 and 2017 elections (Nicolas Dupont-Aignan).

used for both maps, the *Front National* vote increased substantially between 2012 and 2017 (by 20% on average).

Figure 3.4: FN vote shares in the first round of 2012 and 2017 presidential elections



(a) FN vote share - 2012

(b) FN vote share - 2017

2.3 Data Description

In order to conduct our empirical analysis we use multiple data sources. Presidential election results in 1995, 2002, 2007, 2012 and 2017 at the municipality level are taken from the Ministry of Interior. In each of those elections the vote share of the *Front National* is expressed in percentage points. The location and size of holiday villages is taken from the 2016 survey of tourism capacity at the municipal level carried out by the French national statistical institute (INSEE). From the same data source we also collect the number of hotel beds per municipality, which we introduce as a control in order to filter out the component in migrant relocation not related to tourism.

Holiday villages are defined as individual or collective housing, with common sports and entertainment facilities, dedicated to host leisure stays for a fixed fee. Our dataset lists the number of holiday villages and how many beds they contain per municipality in 2016.

In order to proxy the compliance of French mayors in the implementation of the CAOs we use a list of mayors who declared to being willing to welcome migrants as of September 2015. This dataset, which is taken from the National French Television (France Télévision (2015)), is neither official nor exhaustive, but contains 417 municipalities.

We use the 2013 French Census from the INSEE, which is the most recent one available. In particular, we consider the total population, the share of vacant housing, of home owners and social housing for each municipality. We also collect the share of individuals aged between 15 and 29, 30 and 44, 45 and 59, 60 and 74, or over 75 respectively per municipality. We consider the share (among the population above 15 years-old) of individuals belonging to each of the eight official socio-professional categories (farmers, independent, white collars, intermediary professions, employees, blue collars, retired and inactive). Similarly, we consider the share of unemployment among the population aged between 15 and 64. Finally, we also report the share of migrants within the total population of the municipality, where migrants are defined as individuals who are foreign-born but live in France. From the 2013 version of the INSEE file on disposable income, we also collect information on the median disposable income by consumption unit in Euros at the municipality level. Those are available only for municipalities of more than 50 inhabitants. All the aforementioned variables in this paragraph are also collected for 2006 and we use the variation over time as controls as well as the stock in 2013 in our regressions in order to capture the evolution of municipalities post the major 2008 financial crisis as well as current economic conditions.

From the INSEE, we also collect information about the type of each municipality, which can be either central, suburban, independent or rural.

All the aforementioned socio-economic characteristics are part of the controls in our regressions. In order to extensively control for political characteristics of the municipalities in question, we collect background information on the mayors, using the *Repertoire National des Elus* from the Ministry of Interior. This dataset provides information on the occupation of the mayor i.e. if she is a private employee or a civil servant, a teacher, a farmer, or an individual working in an industrial or liberal occupation. It also indicates the age of the mayor, and her party affiliation which we reclassify in 5 categories: left wing, right wing, extreme left, extreme right or others.

Since the French government did not provide official information on the location of the CAOs, we use a non-official dataset. Our preferred dataset is from the *CIMADE* - a French association working with migrants - which, based on local media and associations, indicated

the location of 203 CAOs by late October 2016. Such a figure is much lower than the one provided by the Government, which is of 374 as of February 2017 (Ministère de l'Intérieur (2017)). However, the number of available beds in CAOs reported by the *CIMADE* (7,585) roughly corresponds to the number of migrants who were relocated during the dismantling of October 2016. Importantly, the data from the *CIMADE* reports simultaneously CAOs that were created before the final dismantlement, and those that were created between September and October 2016. We therefore broadly interpret the CAOs contained in the *CIMADE* dataset as centres which received migrants any time between October 2015 and October 2016. Importantly, this source also indicates the capacity of the centres as of October 2016. Since the *CIMADE* data is not official, it is likely that some existing CAOs were not reported. Since it assigns some treated municipalities into the control group, it therefore artificially reduces the observed differences between treated and non-treated municipalities. Thus our results are likely to represent a lower bound of the true effect of migrant relocation. In Section 6, we use an alternative source of data from *InfoCAO*,⁴ a website from two associations assisting the Calais migrants (*L'Auberge des Migrants* and *Utopia 56*), which reports the location of 375 CAOs in France. Even though this dataset reports twice as many CAOs as the one from the *CIMADE*, it does not report the size of the centres. Yet, using this dataset yields very similar results for our main specification.

From the *Cimade*, we also collect information on the presence of other types of migrant centres (as of July 2017), including CADA, HUDA, AT-SA, CPH and PRAHDA. The data is most detailed for the CADA, where we are able to obtain the number of places between 2012 and 2016 on a yearly basis. This allows us to compute the evolution of the number of places in the CADA at the municipality level during this period. Combining all this information with a GIS dataset of French municipalities (provided by the French national geographic institute (IGN)), we are able to compute, for each municipality, the distance to each of these centres i.e. the distance to the closest centre among all CADA, HUDA, AT-SA, CPH and PRAHDA. Furthermore, we also use this GIS data to compute, for each municipality, the distance to the closest CAO, which is used to estimate spillover effects.

Finally, in order to identify whether our results can be attributed to a variation of economic

4. <http://www.infocao.net/>

activity at the local level, we use a dataset of from Trendeo - Observatoire de l'investissement et de l'emploi (2017), which reports job destructions and creations at the municipal level in France between January 2009 and June 2017. This dataset has the advantage of providing a measure of local employment dynamics at the municipal level with higher frequency than traditional indicators. However, in the context of our study, it might suffer from two drawbacks. First, since it is based on monitoring, it might only cover job destructions and creations that are of a magnitude to be actually mentioned in local media (for example local newspapers). Furthermore, this data is likely to be more accurate in depicting labor markets at the level of the employment zone than at the level of the municipality, which is the administrative unit of interest in this paper. We therefore do not include this data in our main analysis, but we investigate their relationship to migrants inflows in Section 5.3.

3 Empirical Specification and Instrumental Variable Approach

We estimate the effect of temporary migrant centres on the evolution of FN vote between 2012 and 2017. Because of data limitations we only know the presence of a CAO and how many sleeping places this CAO contains per municipality, but not how many migrants were finally moved there. We therefore estimate the following equation:

$$\Delta FN \equiv \log(FN_{2017})_i - \log(FN_{2012})_i = \beta_0 + \beta_1 CAO_i + \delta X_i + \epsilon_i \quad (3.1)$$

Where $\log(FN_{2017})_i - \log(FN_{2012})_i$ is the difference of log voting shares for the *Front National* in 2017 and 2012; CAO_i is a dummy equal to 1 if the municipality i has a CAO and 0 otherwise, while X_i are control variables for municipality i , which were outlined in the data description. Particularly we use all the socio-economic controls (notably the evolution between 2013 and 2006), the log of distance to the closest permanent migrant centre, the evolution in the number of CADA places between 2012 and 2016, the log of hotel rooms, as well as political and administrative characteristics of the municipality and demographics of the mayors. All the regressions include provincial (*département*) fixed effects, and the standard errors

are clustered at the *département* level.

However, the assignment of the CAOs is not random, and is likely to be endogenous to political outcomes. First of all, as we show in the next section, municipalities which volunteered to receive migrants were also more likely to eventually receive a CAO. Since this measure is only an imperfect measure of municipality compliance, and as we do not observe the bargaining which might have taken place between municipalities and the government, simple OLS estimates are likely to be biased towards zero, given that citizens of volunteering cities are arguably more tolerant toward migrants and less likely to be affected by the presence of a CAO. Furthermore, many CAOs were established in vacant buildings owned or rented by the state such as for example old military bases or hospitals, and as we show in the next section, they were also more likely to be located in places with a higher number of vacant housing units and in rural areas. Simple OLS estimations might therefore capture part of these effects which are likely to be factors increasing the share of votes in favour of the *Front National* over time.

Consequently, in order to circumvent these potential biases, we propose to instrument the probability of location of a CAO with the number of beds available in the “Village Vacances” (VV) i.e. the aforementioned holiday villages, as of 2016.⁵ Even though several types of venues were considered by the government, a strong emphasis was put on holiday villages (and especially the ones belonging to companies such as *La Poste* or *EDF*) (Libération (2016)). We argue that, controlling for overall tourism (i.e. the number of sleeping places in hotels), holiday villages provide a good instrument to achieve exogenous variation in the assignment of migrants. The residency in those holiday villages is seasonal rather than permanent and thus most likely not associated with any political characteristic of a municipality. What re-enforces this argument is that the holiday villages were established historically in the past and certainly not for the purpose of hosting migrants. In fact, the stock of beds in holiday villages seems to be very stable over time: for example, the correlation coefficient between the number of beds in a municipality in 2014 and in 2016 is equal to 0.98. On the other hand ancient military bases or hospitals as well as total vacant units might indicate a progressive isolation of the municipality. We therefore think that holiday villages can capture exactly this exogenous variation

5. Using as instrument the mere presence of a “Village Vacances” also gives a very strong first stage.

in migrant allocation that we are looking for.

Since our first stage is a Probit, we posit that the more beds in a certain “Village Vacances”, the higher the probability of a migrant centre being located there. Therefore our first stage can be written as:

$$Pr(CAO_i) = \Phi(\log(1 + bedsVV)_i, X_i) \quad (3.2)$$

Where $\log(1 + bedsVV)_i$ is the natural logarithm of 1 + the number of beds provided in the “Village Vacances”.

To confirm the validity of this instrumentation strategy, we run several tests in Section 6. In particular, we show that before the dismantling of the Calais camp, municipalities with a CAO did not seem to be on different electoral pre-trends than municipalities without a CAO, and that controlling for past evolutions of FN vote does not affect our results. We also show our results are unaffected by instrumenting with the number of beds in holiday villages in 2014. Finally, we run a falsification test using the particular case of Corsica: while this region has several holiday villages, it did not receive any CAOs. Yet, in this region, we do not find that municipalities with a greater number of beds in holiday villages had different trends of vote for the *Front National* between 2012 and 2017.⁶

Finally, we investigate the presence of spillover effects of migrant relocation by estimating the effect of distance to the closest CAO (using radiuses of 5km, 10km and 15km). In order to estimate spillovers we have to assume that the decision to create a CAO in a given municipality is unrelated to politics in localities in the radius of 5km, 10km and 15km. This assumption seems warranted given the high number of observations and is re-enforced when looking at our empirical results: the estimate of β_1 is affected only slightly when spatial dummies are introduced.

6. In fact, in the general case, we do not find any significant correlation between the number of beds in holiday villages and the evolution of *Front National* vote between 2017 and 2012.

4 Empirical Results

In the following sections, we first show the main drivers behind the migrant relocation. We then show the main estimates of the migrant relocation on voting shares of the *Front National* in the 2017 presidential election.

4.1 Where were the migrants relocated?

In this subsection, we examine our rich dataset to document the characteristics of municipalities which received migrants in CAOs between October 2015 and October 2016.

A first important question is related to the magnitude of the inflows in each of the 203 municipalities for which we observe a CAO. First of all, based on the data provided by the *CIMADE*, we find that a municipality which received migrants in CAOs had on average 36 beds (standard deviation of 26, the minimum being equal to 2 and the maximum being equal to 150). These municipalities had on average 17 beds per 1000 inhabitants (standard deviation of 36, with a minimum of 0.06 and a maximum of 251).

In Tables 3.1 and 3.2 we report the characteristics of municipalities with and without CAOs. They differ in many observable characteristics. Importantly for our identification strategy, municipalities with CAOs include many more beds in holiday resorts than other municipalities. They are also more likely to be among the municipalities whose mayor publicly mentioned to be willing to welcome migrants, and they had a lower share of *Front National* vote in 2012. We also find that these municipalities are larger, closer to other migrant centres, with more hotel rooms and vacant housing units. Their population, which has lower median income and a higher share of unemployment, is also younger, includes more migrants, and hosts more beneficiaries of social housing. migrants seem to have been relocated evenly between municipalities at the centre of urban units, suburban cities and rural municipalities. Most of these municipalities had right-wing or left-wing mayors, who were also slightly younger, more likely to work in liberal occupations and less likely to be retired.

However, these effects are largely driven by composition effects. Indeed, if we regress the probability of having a CAO on these variables as well as *département* fixed-effects in a Probit model, only a few variables are found to significantly affect the probability of having a

CAO. Overall, the only significant variables at the 5% level are: the number of beds in holiday villages, the distance to the closest permanent migrant centre, the willingness to receive migrants, the share of farmers and the dummy indicating that the municipality is rural. Two additional variables are significant at the 10% level: the number of vacant housing units and the share of individuals aged between 15 and 29 in 2013. Interestingly, once all these factors are controlled for, the presence of a CAO is uncorrelated to the share of FN vote in 2012.⁷

4.2 Main Results

In Table 3.3 one can see that while the coefficient of the instrument is slightly affected by the presence of controls, the magnitude and significance still remain important. Our first stage is very strong, the F-Statistic for the excluded instrument with controls is over 15, which is much higher than the customary value of 10 and the weak instrument guidelines given in Stock and Yogo (2005). We observe a negative correlation between the presence of a CAO and the evolution of *Front National* voting shares when looking at the OLS regression (Column (3)). When we use our instrumental variables approach, the effect is more negative and highly significant. As we previously discussed, not instrumenting the allocation of CAOs biases our estimates towards zero. The presence of a CAO decreases the growth rate of *Front National* votes by 15.7 percentage points (Column (4)). Since the FN vote increased by 20% on average in French municipalities between 2012 and 2017 (which corresponds to a 5-points increase on average), this estimation suggests that the growth rate of FN vote in municipalities with a CAO was only 25% the one of municipalities without a CAO (corresponding to an increase lower by 4 points - which amounts to what we find using shares as outcome variables rather than logs).

5 Further Analysis of the Effects of Migrant Relocation

In the following sections we estimate heterogeneous effects of migrant relocation in order to determine particular factors that are driving our results. We also estimate the impact on other electoral outcomes, particularly the impact of votes on the extreme left. Lastly, we analyse

7. Results of this regression are available upon request.

Table 3.1: Characteristics of the municipalities of relocation (Part 1)

	No CAO	Obs	CAO	CAO	Diff	T-Test
Beds in holiday resorts	6.418	33422	106.734	203	-100.316	-17.471
Log hotel rooms (2016)	0.525	33422	3.845	203	-3.319	-35.881
Share of FN votes (2012)	21.492	33422	17.208	203	4.284	8.534
Share of FN votes (2017)	26.550	33422	20.454	203	6.097	9.579
$\log(FN_{2017})-\log(FN_{2012})$	0.207	33331	.1542	203	0.052	3.207
Log min. distance to migrant center	2.899	33422	1.689	203	1.209	26.219
Evol number places in CADA	0.279	33422	11.990	203	-11.711	-31.754
Voluntary to welcome migrants	0.011	33422	0.266	203	-0.255	-33.283
Net job creation per 1000 inhabitants (2012-2014)	0.229	33422	0.045	203	0.185	0.086
City characteristics - 2013						
Log population (2013)	6.153	33422	8.984	203	-2.831	-31.335
Log vacant housing units (2013)	2.982	33422	5.885	203	-2.903	-32.848
Share 15-29 (2013)	0.166	33422	0.213	203	-0.047	-14.691
Share 30-44 (2013)	0.237	33422	0.216	203	0.021	4.935
Share 45-59 (2013)	0.268	33422	0.241	203	0.027	8.792
Share 60-74 (2013)	0.209	33422	0.195	203	0.014	3.649
Share 75+ (2013)	0.120	33422	0.135	203	-0.015	-4.142
Share farmers (2013)	0.036	33417	0.008	203	0.028	7.495
Share independant (2013)	0.043	33417	0.034	203	0.009	3.646
Share white collars (2013)	0.053	33417	0.066	203	-0.013	-4.049
Share intermediary professions (2013)	0.130	33417	0.127	203	0.002	0.530
Share employees (2013)	0.154	33417	0.161	203	-0.006	-1.534
Share blue collars (2013)	0.156	33417	0.134	203	0.022	4.302
Share retired (2013)	0.308	33417	0.307	203	0.001	0.093
Share inactive (2013)	0.120	33417	0.162	203	-0.043	-10.506
Share unemployed (15-64) (2013)	0.077	33422	0.103	203	-0.027	-11.243
Share of homeowners (2013)	0.786	33422	0.568	203	0.218	29.733
Share of social housing (2013)	0.031	33422	0.157	203	-0.125	-30.544
Log median income (2013)	9.880	30085	9.851	201	0.028	2.613
Share of migrants (2013)	0.039	33422	0.075	203	-0.036	-12.978
City characteristics - Evolution (2006-13)						
Evol share farmers (2006-13)	-0.011	33416	-0.002	203	-0.009	-2.250
Evol share independant (2006-13)	0.004	33416	0.001	203	0.003	1.057
Evol share white collars (2006-13)	0.005	33416	0.006	203	-0.001	-0.167
Evol share intermediary professions (2006-13)	0.011	33416	0.001	203	0.010	2.058
Evol share employees (2006-13)	0.005	33416	-0.005	203	0.010	1.978
Evol share blue collars (2006-13)	-0.009	33416	-0.009	203	0	0.052
Evol share retired (2006-13)	0.017	33416	0.020	203	-0.004	-0.541
Evol share inactive (2006-13)	-0.023	33416	-0.013	203	-0.010	-2.115
Evol share 15-29 (2006-13)	-0.012	33422	-0.013	203	0.001	0.420
Evol share 30-44 (2006-13)	-0.025	33422	-0.021	203	-0.004	-1.179
Evol share 45-59 (2006-13)	-0.001	33422	-0.005	203	0.004	1.152
Evol share 60-74 (2006-13)	0.029	33422	0.024	203	0.005	1.545
Evol share 75+ (2006-13)	0.098	33422	0.075	203	0.023	5.269
Evol share unemployed (15-64) (2006-13)	0.014	33422	0.022	203	-0.008	-3.597
Evol log median income (2006-13)	0.198	27929	0.162	200	0.036	7.415
Evol share migrants (2006-13)	0.002	33422	0.007	203	-0.005	-3.962
Evol share homeowners (2006-13)	0.003	33422	0.002	203	0.001	0.262
Evol share social housing (2006-13)	0.001	33422	-0.002	203	0.002	2.123
Evol log vacant housing units (2006-13)	0.298	33422	0.340	203	-0.042	-1.061
Evol log population (2006-13)	0.053	33422	0.018	203	0.035	4.296

Notes: All shares are expressed in decimals, except for voting shares. Distances are expressed in km.

Table 3.2: Characteristics of the municipalities of relocation (Part 2)

	No CAO	Obs	CAO	CAO	Diff	T-Test
Type of municipality						
Suburb	0.120	33422	0.276	203	-0.156	-6.796
Center	0.039	33422	0.379	203	-0.340	-24.498
Independant	0.029	33422	0.099	203	-0.069	-5.811
Rural	0.812	33422	0.246	203	0.565	20.521
Mayor party						
Age of mayor (in 2014)	58.703	33341	58.271	203	0.432	0.662
Right-wing Mayor	0.368	33244	0.475	202	-0.107	-3.158
Left-wing Mayor	0.214	33244	0.356	202	-0.143	-4.925
Extreme Right Mayor	0.001	33244	0	202	0.001	0.390
Extreme Left Mayor	0.011	33244	0.059	202	-0.048	-6.356
Mayor occupation						
Farmers	0.141	33339	0.039	203	0.101	4.150
Others	0.030	33339	0.059	203	-0.029	-2.367
Teaching/Education	0.043	33339	0.079	203	-0.036	-2.519
Civil Servants	0.101	33339	0.138	203	-0.036	-1.713
Industrial and Commercial	0.061	33339	0.054	203	0.007	0.398
Liberal Occupations	0.037	33339	0.143	203	-0.106	-7.858
Retired	0.429	33339	0.345	203	0.084	2.416
Private employees	0.157	33339	0.143	203	0.014	0.563

Notes: All shares are expressed in decimals, except for voting shares. Distances are expressed in km.

Table 3.3: Main Results on the impact of migrants on the *Front National* Vote

	(1)	(2)	(3)	(4)	(5)
	Pr(CAO)	Pr(CAO)	Δ_{FN}	Δ_{FN}	Δ_{FN}
$\log(1 + VVlit)$	0.155*** (0.017)	0.099*** (0.025)			
CAO			-0.020*** (0.007)	-0.157*** (0.033)	-0.161*** (0.033)
Spillover (5 kms)					-0.018*** (0.006)
Spillover (10 kms)					-0.003 (0.004)
Spillover (15 kms)					-0.003 (0.003)
Regression	Probit	Probit	OLS	IV	IV
Controls	No	Yes	Yes	Yes	Yes
<i>Département</i> Fixed Effects	No	Yes	Yes	Yes	Yes
Observations	33625	26813	27938	26812	26812
Adjusted R^2			0.118	0.114	0.114

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

Columns 1 and 2 report the coefficients of a first stage probit regression where the dummy variable indicating the presence of a CAO is regressed on the log of 1+the number of beds in holiday villages. Column 1 includes no controls, while column 2 controls for municipality sociodemographic characteristics (in 2013 and in evolution between 2006 and 2013), the log of the number of hotel rooms, whether the municipality volunteered to receive migrants, the log of distance to the closest permanent migrant center, the evolution of the number of places in CADAs, the mayor's party and characteristics, and *département* fixed effects. Column 3 presents the results of an OLS regression where the variation of log shares of FN votes between 2012 and 2017 is regressed on the presence of a CAO and the full set of controls. Columns 4 and 5 present the results of IV regressions where the first-stage regression is the one presented in column 2. Both regressions include the set of controls described above, and column 5 adds different of radiuses of distance to the closest CAO. Standard errors clustered at the *département* level in parentheses.

whether our results could be driven by enhanced economic activity brought by migrants or whether evidence rather points towards the contact hypothesis

5.1 Heterogeneous Effects of Migrant Relocation

As part of our main analysis we conduct regressions showing heterogeneous effects in Table 3.1. We interact our prediction from the first stage with various indicators provided at the micro level to instrument for the interaction terms outlined in Table 3.1. We want to test whether communities with certain characteristics respond in differing ways to migrants. First, we try to see whether migrants have a stronger effect on communities when there are already many immigrants to begin with. In column (1) we can see that the decrease of votes of the *Front National* is more pronounced in places with a higher share of immigrants. This could be the case as already pre-existing communities from the same country of origin of the migrants could facilitate initial contact. We also find a stronger decrease in municipalities with a larger share of younger inhabitants (column (2)). This could be due to the fact that younger people have less fortified opinions towards migrants and thus might be more willing to get in touch with the new people joining their municipality. Furthermore, we find a smaller decrease in municipalities in which mayors publicly volunteered to welcome migrants (column (3)): this might be due to the fact that citizens living in volunteering cities are also less likely to be prejudiced against migrants, so that actual contact with them is less likely to affect their political choices. However, we do not find that the treatment effect is different in places where the FN vote was historically low (column (4)). Finally, the decrease seems to be higher in larger municipalities (column (5)), even though the point estimate is not significant.

The analysis of the intensive margin yields important results for the understanding of electoral reaction to migrant inflows. We indeed find that the negative effect on FN vote is stronger in municipalities with fewer beds per inhabitant (column (6)). Based on this heterogeneity analysis, we estimate that municipalities which decreased their FN vote upon receiving migrants were those that had less than 39 beds per 1,000 inhabitants. Above this threshold, the effect of CAO on FN vote seems to be positive. This is in line with a large literature on the impacts of large inflows of immigrants on political outcomes.

Overall, combining all these effects together (column (7)) it appears that the most significant margin of heterogeneity is related to whether mayors were voluntary to receive migrants: municipalities where the FN vote decreased the most in relative terms are those whose mayor did not explicitly call to receive migrants.

5.2 Other Election Results

In this subsection, we refine our analysis by investigating what impact the relocation of migrants had on abstention and votes on the extreme left-wing political spectrum. In Table 3.2 we can see that the location of a CAO is associated with a slightly lower abstention, therefore a higher turnout. There seems to be some evidence that migrants have causally increased turnout in those municipalities. Controlling for the change in abstention, we can see that the electoral effects on the vote of the *Front National* are unaffected (Column(3)). Though CAOs are located in municipalities with a slightly higher share of votes for the *Front de Gauche*. After instrumenting, we find a pronounced effect in favour of votes of the *Front de Gauche*, which is similar in magnitude to the negative effect on the votes of the *Front National*.⁸ Therefore we can establish that the causal impact of migrant relocation has led to a decrease in votes of the *Front National* and an increase in both turnout and votes in favour of the major left-wing pro-immigrant party. The next section will outline and discuss two potential mechanisms behind those findings.

5.3 Mechanism: Local Economic Activity or Contact Hypothesis?

In this section, we analyse a potential alternative mechanism to the contact hypothesis: the effect of migrants on local economic activity. Indeed, while migrants in CAOs do not legally have the right to work on the French territory and do not receive any monetary allocation, their arrival might have an effect on local activity through increased demand in the catering or building sectors. In turn, these potential variations in local economic activity might affect electoral outcomes. To check that these effects are unlikely to drive our results, we use

8. We do not carry out a separate analysis for electoral outcomes in favour of centre-left and centre-right parties, given that the candidacy of Emmanuel Macron, an ex-socialist minister and self-proclaimed centrist, makes it difficult to compare those votes with the election in 2012.

Table 3.1: Heterogeneous Effects of the impact of migrants on the *Front National* Vote

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Δ_{FN}	Δ_{FN}	Δ_{FN}	Δ_{FN}	Δ_{FN}	Δ_{FN}	Δ_{FN}
CAO	-0.071 (0.044)	-0.085* (0.050)	-0.240*** (0.057)	-0.191*** (0.059)	-0.017 (0.101)	-0.154*** (0.032)	-0.100 (0.149)
CAO $\times \frac{Immigrants}{Population}$	-1.542** (0.636)						-1.214 (0.737)
CAO $\times \frac{Young(15-29)}{Pop(over15)}$		-0.638** (0.297)					-0.625 (0.576)
CAO $\times Voluntary - Mayors$			0.135** (0.051)				0.192** (0.075)
CAO $\times FN_{2007}$				-0.007 (0.008)			-0.004 (0.009)
CAO $\times \log(Population)$					-0.030 (0.019)		-0.011 (0.041)
CAO $\times \frac{CAObeds}{Population} \times 1000$						0.004*** (0.001)	0.003 (0.003)
Regression	IV	IV	IV	IV	IV	IV	IV
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Département Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	26812	26812	26812	26812	26812	26812	26812
Adjusted R^2	0.116	0.116	0.110	0.113	0.117	0.116	0.114

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

All columns correspond to IV regressions where the presence of a CAO is instrumented by the log of beds in holiday villages, and where the outcome variable is the difference between log FN vote shares between 2012 and 2017. All specifications control for municipality sociodemographic characteristics (in 2013 and in evolution between 2006 and 2013), the log of the number of hotel rooms, whether the municipality volunteered to receive migrants, the log of distance to the closest permanent migrant center, the evolution of the number of places in CADAs, the mayor's party and characteristics, and *département* fixed effects. Standard errors clustered at the *département* level in parentheses.

Table 3.2: Effect of migrant Relocation on Abstention and Extreme-left wing votes

	(1)	(2)	(3)	(4)	(5)
	Δ_{Abst}	Δ_{Abst}	Δ_{FN}	Δ_{FG}	Δ_{FG}
CAO	-0.015 (0.009)	-0.102*** (0.039)	-0.157*** (0.033)	0.006 (0.009)	0.151*** (0.049)
Δ_{Abst}			0.000 (0.005)		
Regression	OLS	IV	IV	OLS	IV
Controls	Yes	Yes	Yes	Yes	Yes
<i>Département</i> Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	27926	26800	26799	27925	26802
Adjusted R^2	0.062	0.060	0.114	0.060	0.060

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

Column 1 reports the coefficients of an OLS regression where the variation of abstention rate between the presidential elections of 2012 and 2017 is regressed on the presence of a CAO. Column 2 reports the coefficient of CAO on the variation of abstention after instrumenting it with the number of holiday villages. Column 3 reports the second stage of the main instrumental variable specification, where the outcome variable is the variation of FN log vote shares between 2012 and 2017, but controlling for the variation in the abstention rate. Column 4 reports the CAO coefficient in an OLS regression where the outcome variable is the variation in log vote shares obtained by the *Front de Gauche* between 2012 and 2017. Column 5 reports the estimated effect of CAO on the variation of the *Front de Gauche* vote share after instrumenting it with the presence of a holiday village. All specifications control for municipality sociodemographic characteristics (in 2013 and in evolution between 2006 and 2013), the log of the number of hotel rooms, whether the municipality volunteered to receive migrants, the log of distance to the closest permanent migrant center, the evolution of the number of places in CADAs, the mayor's party and characteristics, and *département* fixed effects. Standard errors clustered at the *département* level in parentheses.

a dataset provided by Trendeo - Observatoire de l'investissement et de l'emploi (2017), which indicates the number of job creations and destructions at the municipality level from January 2009 to June 2017. Using this data, we compute the net job creation per inhabitant at the municipality level for three time periods: from 2012 to 2014, after the beginning of the whole relocation process (from October 2015 to June 2017), and after the beginning of the final step of the dismantling (from October 2016 to June 2017). First, as we showed in Table 3.1, we find no significant difference of net job creation per inhabitant over the period 2012-2014 between municipalities which eventually received a CAO and those that did not. In Table 3.3, we estimate whether CAO creations are related to different labor market dynamics in the following months. Whether we consider OLS or IV estimates, controlling for previous net job creation per inhabitant over the period 2012-2014, does not lead to any significant relationship between the presence of CAO and net job creation. Similarly, controlling for net job creation per inhabitant before and after the creation of CAO does not affect our IV estimate of the impact of CAOs on the evolution of FN vote.

6 Robustness Checks and Falsification Exercises

In the following sections we carry out a battery of robustness checks and falsification exercises. First, we use an alternative dataset from the website *InfoCAO* that enumerates 375 CAOs in France. Then we vary our measure of holiday villages by only including holiday villages in 2014. Lastly we check for political pre-trends in order to make sure that we are not picking up persistent political trends in certain municipalities.

6.1 Alternative Dataset of CAOs

Using the data provided by the website *InfoCAO*, which provides the location of 375 CAOs, we estimate the effects of migrants on the French presidential elections. As we can observe (Table 3.1, column (1)) the first stage is still highly significant, beds in holiday villages do predict well the assignment of a CAO. CAOs are slightly negatively correlated with electoral outcomes of the *Front National*, but after instrumenting, we find a highly significant negative effect of a magnitude similar to the one found in our main estimation.

Table 3.3: Effect of migrant Relocation on Net job creation

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>NJC</i>	<i>NJC</i>	<i>NJC</i>	<i>NJC</i>	Δ_{FN}	Δ_{FN}
	<i>Post</i> – 10/2015	<i>Post</i> – 10/2016	<i>Post</i> – 10/2015	<i>Post</i> – 10/2016		
CAO	0.899 (2.049)	0.934 (1.494)	-5.015 (3.138)	-1.554 (1.953)	-0.156*** (0.033)	-0.157*** (0.033)
Regression	OLS	OLS	IV	IV	IV	IV
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Control: <i>NJC</i> _{2012–2014}	Yes	Yes	Yes	Yes	Yes	Yes
Control: <i>NJC</i> _{<i>Post</i>–10/2015}	No	No	No	No	Yes	No
Control: <i>NJC</i> _{<i>Post</i>–10/2016}	No	No	No	No	No	Yes
<i>Département</i> Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27940	27940	26813	26813	26812	26812
Adjusted <i>R</i> ²	0.025	0.016	0.025	0.016	0.114	0.114

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

Columns 1 and 2 report the coefficients of an OLS regression where we regress the net creation rate per 1,000 inhabitant after October 2015 (Column 1) and after October 2016 (Column 2) on the presence of a CAO. Columns 3 and 4 report the coefficients of the same specification where the presence of a CAO is instrumented by the log of beds in holiday villages. Column 5 is an instrumental variable regression where the outcome variable is the variation of log FN vote share between 2012 and 2017, where we control for the net creation rate per 1,000 inhabitant after October 2015. Column 6 is the same specification as Column 5, but controlling for net creation rate per 1,000 inhabitant after October 2016. All regressions control for municipality sociodemographic characteristics (in 2013 and in evolution between 2006 and 2013), the log of the number of hotel rooms, whether the municipality volunteered to receive migrants, the log of distance to the closest permanent migrant center, the evolution of the number of places in CADAs, the mayor's party and characteristics, and *département* fixed effects. Standard errors clustered at the *département* level in parentheses.

NJC stands for Net Job Creation (per thousand inhabitants)

Table 3.1: Effect of migrant Relocation using alternative dataset of CAOs

	(1)	(2)	(3)	(4)
	CAO_{Alt}	CAO_{Alt}	Δ_{FN}	Δ_{FN}
$\log(1 + VVlit)$	0.162*** (0.016)	0.099*** (0.021)		
CAO_{Alt}			-0.022*** (0.008)	-0.120*** (0.022)
Regression	Probit	Probit	OLS	IV
Controls	No	Yes	Yes	Yes
<i>Département</i> Fixed Effects	Yes	Yes	Yes	Yes
Observations	33625	27922	27938	27920
Adjusted R^2			0.118	0.114

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

Columns 1 and 2 report the coefficients of a first stage probit regression where the dummy variable indicating the presence of a CAO (as measured according to the dataset from *InfoCAO*) is regressed on the log of 1+the number of beds in holiday villages. Column 1 includes no controls, while column 2 controls for municipality sociodemographic characteristics (in 2013 and in evolution between 2006 and 2013), the log of the number of hotel rooms, whether the municipality volunteered to receive migrants, the log of distance to the closest permanent migrant center, the evolution of the number of places in CADAs, the mayor's party and characteristics, and *département* fixed effects. Column 3 presents the results of an OLS regression where the variation of log shares of FN votes between 2012 and 2017 is regressed on the presence of a CAO (as measured according to the dataset from *InfoCAO*). Column 4 presents the results of an IV regression where the first-stage regression is the one presented in column 2. Both regressions include the set of controls described above. Standard errors clustered at the *département* level in parentheses.

Table 3.2: Effect of Refugee Relocation using Beds in holiday villages in 2014

	(1)	(2)	(3)
	CAO	CAO	Δ_{FN}
$\log(1 + VVlit)_{14}$	0.154*** (0.017)	0.101*** (0.024)	
CAO			-0.157*** (0.033)
Observations	33625	26813	26812
Adjusted R^2			0.114
Regression	Probit	Probit	IV
Controls	No	Yes	Yes
<i>Département</i> Fixed Effects	No	Yes	Yes
Observations	33625	26813	26812
Adjusted R^2			0.114

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

Columns 1 and 2 report the coefficients of a first stage probit regression where the dummy variable indicating the presence of a CAO is regressed on the log of 1+the number of beds in holiday villages (as of 2014). Column 1 includes no controls, while column 2 controls for municipality sociodemographic characteristics (in 2013 and in evolution between 2006 and 2013), the log of the number of hotel rooms, whether the municipality volunteered to receive migrants, the log of distance to the closest permanent migrant center, the evolution of the number of places in CADAs, the mayor's party and characteristics, and *département* fixed effects. Column 3 presents the results of an IV regression where the first-stage regression is the one presented in column 2. and includes the set of controls described above. Standard errors clustered at the *département* level in parentheses.

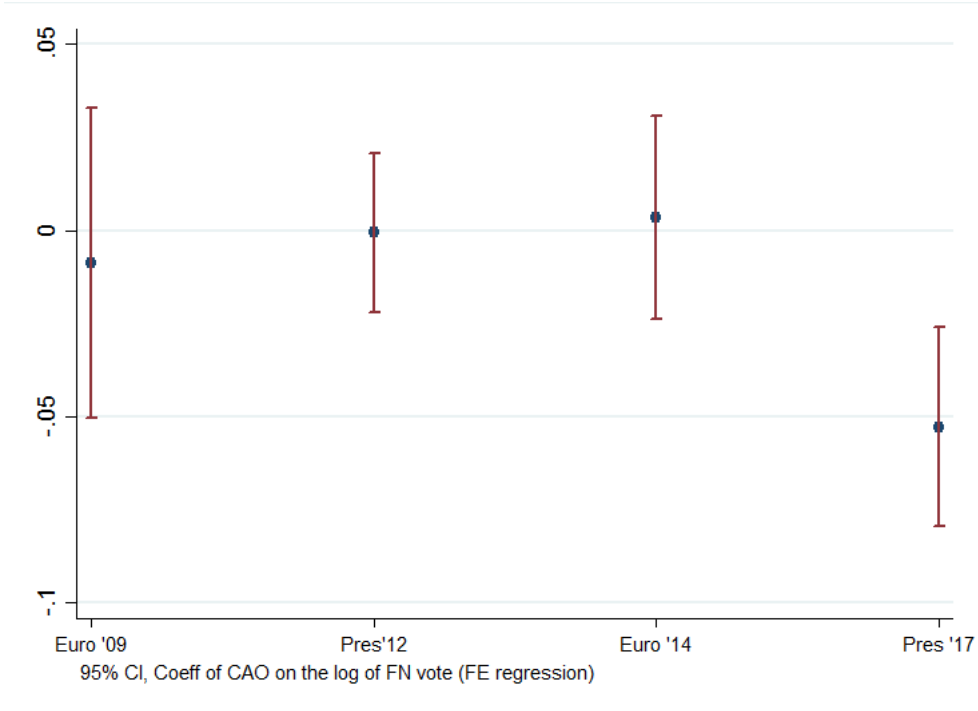
6.2 Alternative Measure of Beds in Holiday Villages

In order to provide more evidence on the robustness of our results, we resort to an alternative measure of beds in holiday villages. In our previous estimation, we used the number of beds in holiday villages for 2016, as it is the most recent measure on the subject. In order to rule out that the presence of migrants might have affected this variable, we carry out the same regressions using observations for the year 2014 (prior to the dismantlement). We can see that both the first stage as well as the coefficient on the *Front National* vote are very similar compared to our previous measure (Table 3.2).

6.3 Other Falsification Exercises and Robustness Checks

In this section we conduct a set of falsification exercises as well as robustness checks. First we consider whether we might be picking up a pre-eminent electoral trend in certain municipalities. To do so, we run a panel regression at the municipality level, where we evaluate the effect of CAO presence on various elections between 2007 and 2017 (namely, the Presidential elections of 2007, the European elections of 2009, the Presidential elections of 2012, the European elections of 2014 and the Presidential election of 2017), controlling for municipality and election fixed-effects. In Figure 3.1, where the effect of CAO in the Presidential elections of 2007 is normalized to be zero, the coefficient on CAO is never statistically different from zero except for the 2017 Presidential elections. This gives us some evidence that the treated municipalities were not on different political pre-trends prior to the election.

Figure 3.1: Absence of Pretrends



In Table 3.3 we confirm this result by showing that the presence of CAOs seems to be unrelated to long-run evolutions of FN vote in Presidential elections. In Column (1), (2) and (3) we can see that regressing the variation of log FN vote between the Presidential elections 1995, 2002, 2007 and 2012 on the posterior presence of CAO yields small and insignificant point

Table 3.3: Pre-Trends: CAO coefficients on past Presidential Elections.

	(1)	(2)	(3)	(4)
	$\Delta FN_{1995-2002}$	$\Delta FN_{2002-2007}$	$\Delta FN_{2007-2012}$	$\Delta FN_{2012-2017}$
CAO	0.017 (0.012)	-0.003 (0.011)	-0.008 (0.009)	-0.178*** (0.034)
Regression	OLS	OLS	OLS	IV
Controls	Yes	Yes	Yes	Yes
Controls: $\Delta FN_{1995-2002}$	No	No	No	Yes
Controls: $\Delta FN_{2002-2007}$	No	No	No	Yes
Controls: $\Delta FN_{2007-2012}$	No	No	No	Yes
<i>Département</i> Fixed Effects	Yes	Yes	Yes	Yes
Observations	27898	27924	27932	26766
Adjusted R^2	0.191	0.254	0.142	0.199

Columns 1 to 2 report the results of OLS regressions where the outcome variable is the variation of log FN votes between the presidential elections of 1995 and 2002 (Column 1), 2002 and 2007 (Column 2) and 2007 and 2012 (Column 3). The reported coefficient is the effect of the presence of a CAO between October 2015 and October 2016. Column 4 reports the results of an instrumental variable regression of the variation of log FN votes between the presidential elections of 2012 and 2017 on the presence of a CAO, where the presence of a CAO is instrumented by the log of beds in holiday villages. All specifications control for municipality sociodemographic characteristics (in 2013 and in evolution between 2006 and 2013), the log of the number of hotel rooms, whether the municipality volunteered to receive migrants, the log of distance to the closest permanent migrant center, the evolution of the number of places in CADAs, the mayor's party and characteristics, and *département* fixed effects. Column 4 also controls for past variations of log FN vote between the presidential elections of 1995 and 2002, 2002 and 2007, as well as 2002 and 2007. Standard errors clustered at the *département* level in parentheses.

estimates. In column (4) we can see that the effect of the CAO (instrumented), controlling for said electoral trends, is barely affected (and if anything, our main effect is reinforced).

As a last check we consider Corsica (Table 3.4), which represents an interesting indirect test of our exclusion restriction. Indeed, no migrants were relocated to Corsica, but given its appeal as holiday destination, it contains many holiday villages. In order to re-enforce the fact that our regressions are not picking up a pre-eminent trend in very touristic places, we regress our instruments on voting outcomes for the *Front National* vote in the French Presidential elections. Table 3.4 shows that no coefficient is significant. These additional regressions additional underline the validity of our instrumental variable approach.⁹

9. Furthermore, for all municipalities, regressing the evolution of log FN votes between 2012 and 2017 on the number of holiday villages yields insignificant point estimates, which reinforces the plausibility of our exclusion restriction.

Table 3.4: No link between holiday villages and FN trend in Corsica

	(1)	(2)	(3)
	Δ_{FN}	Δ_{FN}	Δ_{FN}
$\log(1 + bedsVV)$	-0.005	-0.013	-0.013
	(0.002)	(0.005)	(0.005)
Regression	OLS	OLS	OLS
Controls	No	Yes	Yes
<i>Département</i> Fixed Effects	No	No	Yes
Observations	352	199	199
Adjusted R^2	-0.002	0.151	0.202

Columns 1 to 2 report the results of OLS regressions of the variation of log FN votes between the presidential elections of 2012 and 2017 on the log of beds in holiday vilages. All regressions control for municipality sociodemographic characteristics (in 2013 and in evolution between 2006 and 2013), the log of the number of hotel rooms and mayor's party and characteristics. Column 3 controls for *département* fixed effects. Standard errors clustered at the *département* level in parentheses.

7 Concluding Remarks

In this paper we have tried to answer some important questions regarding both the assignment of migrants subsequent to the dismantlement of the Calais “Jungle” and the impact of the relocation of those migrants on electoral outcomes in the 2017 Presidential election. We find a negative effect on the share of votes for the *Front National*, which is consistent with the contact hypothesis. We also show heterogeneous effects, as stronger negative effects on the vote share of the *Front National* occur in municipalities with a younger population and with more migrants. However, in municipalities where the mayor pronounced her willingness to accept migrants in the first place, the decrease is dampened. Finally, the effect is particularly negative for cities which received fewer migrants, and not seem to be driven by potential economic effects. Overall our results suggest that there exists a difference in perceived immigration through the media compared with actual immigration, and that the electoral reaction to actual migration seems to depend crucially on the size of the inflow.

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Chapter 4

Conclusion and research agenda

In this thesis, I tackled empirically issues which are at the heart of contemporary thinking about democracy: imbalanced electoral representation and the rise of populism. In a nutshell, the results presented in this thesis suggest that imbalanced political representation depends crucially on institutional frameworks and voters preferences, confirm that this imbalanced representation is not neutral (at least for public finance) and suggest that, under certain circumstances, populist rhetoric against migrants can be counterbalanced with actual interactions between natives and migrants. These results, which shed new light on a number of results emphasized in past literature, also call for further research.

The first chapter shows that dynastic politicians in Italy are different from non-dynastic politicians, both in terms of characteristics (they are younger, less experienced and electorally more successful) and in terms of behavior while in office: even if cities ran by dynastic politicians do not seem to have lower performance than cities ran by non-dynastic politicians, dynastic politicians are more likely to engage in opportunistic behaviors. Overall, more work still needs to be done to understand the impact of families on economic policies, and it would be particularly interesting to extend such an analysis to other countries.

The second chapter argues that right-wing female candidates in France were discriminated against by voters in the *départementales* elections of 2015. The impact of such discrimination was sizable, since it prevented some pairs from being elected, and the effect was likely to be driven by statistical discrimination. This paper raises many important questions, notably regarding the role of information and of ballot layouts in elections, the policies which can be implemented to dampen gender discrimination. However, because of the nature of the data,

some questions remain unanswered as of now. It would be particularly interesting to replicate the setting of this election in a laboratory experiment, to analyze the individual determinants of voter discrimination, and estimate more precisely the role played by information asymmetries (both about the electoral rules and the candidates) in electoral outcomes. Furthermore, it will be crucial to test whether such results hold in the next *départementales* elections (provided that the electoral rules remain unchanged).

The third chapter shows that small-scale and short-term exposure to Calais migrants was likely to slow down the progression of far-right votes in French municipalities which received them. This effect is however likely to be reversed if the number of relocated refugees is above an estimated threshold of 39 beds per 1,000 inhabitants. While this paper bridges results from several strands of the literature studying the links between immigration and populism, and suggests that negative views towards refugees can be counterbalanced with actual interactions, it would be particularly interesting to directly evaluate how the attitude of citizens towards migrants was affected by these interactions. Furthermore, as the causes of the rise of populism are numerous and complex, many more efforts need to be done to understand them, by studying their institutional, economic and cultural roots.

Understanding the determinants of electoral supply and demand is key to assess the challenges faced by democracy. The renewed understanding of politics that economic modeling and empirical analyses enable is only at its beginning, as innovative methodologies and increasingly accurate data will progressively enable researchers to bring new answers to already existing questions, and to ask questions that could not be imagined before. It is with this in mind that I wish to continue and extend the research efforts developed in this thesis.

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