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Ballot-Structure and Corruption:

A Natural Experiment from French Municipal Elections

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Abstract

Electoral reforms continue to be a highly debated topic in many democracies. However, although great progress has been made in recent years, our knowledge of the effects of various institutional specifications remain unsatisfactory. Importantly, there is a lacuna to be bridged with regards to our knowledge of the specific causal mechanisms through which electoral systems influence levels of corruption. This paper contributes to the literature by utilising a rare natural experiment in small French municipalities. It uses a regression discontinuity design and examines the effect of closed- and open-list ballots in local elections on the probability of corruption in municipal councils in France. Municipalities with populations equal to or larger than 3,500 are required to use a closed-list proportional-representation (PR) system, while municipalities with less than 3,500 inhabitants are prescribed an open-list plurality system. I take advantage of this population threshold to test the theoretical prediction made by Persson and Tabellini (2000) that voting for a closed lists rather than an open list should attenuate incumbents' incentives to signal competence and so increase rent extraction relative to open-list elections. The results of the analysis suggest that closed-list elections cause the probability that local politicians are convicted of corruption to rise markedly. The finding might have implications for the cost-benefit calculation of those seeking to reform local elections.

Contents

1 Introduction

Corruption is not only a significant issue in the developing world. On the contrary, the cost of corruption in the EU, for instance, is estimated to lie between €179 and €990 billion annually (Hafner et al., 2016). Hence, learning more about the causes of corruption in these developed countries is potentially highly beneficial.

One of the factors that have been identified as an important determinant of corruption is political institutions. Yet, although empirical research abound, our understanding of which causal mechanisms play important roles remains poor. A recurring issue is the difficulty of isolating the causal effects of particular institutions. However, recently, a promising strand of research has emerged that tests model predictions by utilizing natural experiments that occur at subnational levels where institutional rules are often arbitrarily set and more frequently changed.

This study aims to contribute to this strand of research. It focuses on political corruption and asks whether different electoral rules are more or less successful in stifling political corruption by holding incumbent politicians accountable at the polls. More specifically, it empirically tests the relationship between ballot-structure and corruption using a natural experiment in France. French law stipulates that municipalities with 3,500 inhabitants or more must use a proportional closed-list system, while municipalities with less than 3,500 inhabitants must use a block vote open-list system. Utilizing this exogenous and arbitrary population threshold, I use a regression discontinuity design (RDD) to identify the causal effect of going from open to closed lists in small French municipalities. The dependent variable is an objective corruption measure based on reported cases of corruption in France.

I find evidence that municipalities with closed-list elections have a markedly higher probability of corruption than they would have had, had they held open-list elections. The finding is consistent with empirical findings elsewhere and theoretical predictions. However, other institutional rules also change at the threshold. While it is impossible to control for these directly, robustness checks indicate that the estimated effect is not caused by another policy-change.

In the following sections, I review the current literature and present the theoretical framework that links corruption to ballot structure, before turning to the empirical investigation.

2 Literature Review

How do political institutions affect corruption? While this question has been of academic investigation for a while, only more recently have the effects of electoral rules been explicitly linked with corruption, and many questions still remain unanswered. This section reviews a selection of this literature. Following a quick summary of early research on corruption, I first look at the major cross-country studies linking the electoral rules and corruption literatures. I then highlight important limitations of previous studies. Finally, I discuss studies using natural experiments to identify causal mechanisms through which institutional features might affect corruption.

Due to the elusive nature of corruption and thus lack of data, early corruption literature was mainly theoretical (see, e.g. Huntington, 1968/2006). However, in response to multinational companies' growing reliability on information about corruption internationally, various consulting firms developed multiple corruption indices. Most popular are those published by International Country Risk Guide and Business International, the World Bank Institute, and Transparency International's (TI) Corruption Perception Index (CPI). These have transformed the study of corruption by enabling empirical investigations of causes of corruption, and since the late 1990s, a vast literature has emerged correlating corruption indices with historical and institutional factors.

Initially, most research focused on cultural, religious, and historical factors. The main lessons from these studies are that corruption is robustly correlated with income (Mauro, 1995; Knack & Keefer, 1995; Ades & Di Tella, 1999; La Porta, Lopez-de-Silanes, Shleifer, & Vishny, 1999; Treisman, 2000), and that perceived corruption is lower in established, developed democracies with a free press, high openness to trade, and more gender equality in politics (Ades & Di Tella, 1999; Dollar, Fisman, & Gatti, 2001; Swamy, Knack, Lee, & Azfar, 2001; Treisman, 2000; Brunetti & Weder, 2003; Djankov, McLiesh, Nenova, & Shleifer, 2003; Treisman, 2007). Additionally, corruption is higher in countries that have intrusive business regulations or economies that rely heavily on fuel export (Ades & Di Tella, 1999; La Porta et al., 1999; Treisman, 2007). Although interesting for many reasons, these observations are purely correla-

tional and (with the exception of female participation) of limited policy interest as they identify variables largely out of societies' control.

As the incidence of corruption varies substantially among countries with similar socioeconomic characteristics, political institutions have naturally always occupied a central position in the corruption literature. For instance, Gerring and Thacker (2004) look at the effect of unitarism and parliamentarism on corruption and conclude that unitary parliamentary states have less corruption, and Fisman and Gatti (2002) find a negative association between fiscal decentralization and corruption. Treisman (2000) investigated the relationship between democracy and corruption. Yet, although the role of specific political accountability mechanisms have been extensively theoretically discussed in this context (e.g. Fackler & Lin, 1995; Linz & Stepan, 1996; Bailey & Valenzuela, 1997; Persson, Roland, & Tabellini, 1997; Rose-Ackerman, 1999; Besley, 2006), it has only received cursory empirical treatment. It is to these studies, bridging the literature on electoral rules and corruption, I now turn.

Persson, Tabellini, and Trebbi (2003) empirically test three arguments, developed by Myerson (1993); Persson, Roland, and Tabellini (2000); Persson and Tabellini (1998), respectively: (1) larger district magnitude and lower thresholds for representation generate more political competition and thus less corruption; (2) a larger fraction of representatives elected on an open- rather than closed-list ballot generates a stronger link of accountability and thus less corruption; and (3) relative to PR in large districts, plurality rule in small districts are associated with less corruption. They find that switching from a party list PR system to a system where all candidates are elected by plurality as individuals, reduce perceptions of corruption. Moreover, they argue that although electoral system has observable partial effects on corruption, the total effect of changing from one electoral system to another is zero. The reason is that institutions that usually come together have opposite effects on corruption: open-list elections usually go with smaller districts, which they argue raise entry-barriers for competitors and therefore corruption, so the net effect is zero.

Covering similar ground, Kunicová and Rose-Ackerman (2005) investigate the relationship between corruption, electoral formula, and ballot structure. Although agreeing with Persson et

al. (2003) that closed-list ballot structure raises corruption, concerning district magnitude they argue that although entry-barriers are higher in small districts, political opponents' incentives to monitor each other are stronger. They find that countries with PR, federalism and presidentialism have higher corruption. This contradicts Persson et al. (2003) who report that electoral rules alone have no influence on corruption levels, and Persson and Tabellini (2003), who argue that presidentialist systems should be less corrupt.

Another major study finds that more years under democratic rule lowers corruption, and that transitioning from parliamentarism to presidentialism increases the probability of having high levels of corruption (Lederman, Loayza, & Soares, 2005). However, freedom of press, common law, and openness to trade are not statistically significant when institutional covariates are controlled for. The authors argue that political institutions themselves determine these factors so that these institutions have no independent effect. The study seemingly provides evidence that some correlations noticed in the early literature might be driven by underlying political institutions, which might play an even larger role than initially imagined.

Some key conclusions to draw from this literature are that, firstly, political institutions matter, and secondly, there is no consensus about the causal mechanisms accounting for the observed associations, nor about the overall effects of different electoral formulas and ballot structures.

Although providing important lessons, the literature reviewed above generally suffers from some serious shortcomings that raise questions about the credibility of their findings altogether. I highlight the two most severe: (1) omitted variables and sample selection bias, and (2) problems associated with the corruption perception indices.

(1) The inability of multiple regression on cross-country data to account for the full set of institutional configurations that determine corruption, has meant that causal identification has been impossible and findings less practically interesting. Furthermore, another limitation is that cross-national studies often suffered from data shortages that may lead to sample bias: in some regressions, La Porta et al. (1999), Treisman (2000), and Persson et al. (2003) use as few as 47, 81, 80 and often fewer observations, respectively.

(2) Another problem is that the dependent variable has been perceived rather than actual

experienced corruption. Worryingly, the subjective measure of corruption that is based on the perception of businessmen and citizens is the measure that actually corroborate the commonly held hypotheses; once objective measures are used, most of the associations disappear (Treisman, 2007). This is worrying since perceptions are inherently unreliable. No empirical evidence robustly links experts' opinions about corruption with experienced levels of corruption. Quite to the contrary, Razafindrakoto and Roubaud (2005) find that there is no statistically significant correlation between expert judgement of corruption and the extent to which the general population actually experience corruption (the coefficient is even negative). However, they do find that the experts' opinions were correlated with the corruption ratings of Kaufmann, Kraay, and Zoido-Lobatón (1999). Might TI's CPI provide a better proxy, since it only includes countries for which at least one survey based on the population is available, as claimed by Lipset and Lenz (2000)? It seems not. Abramo (2008) finds that the general public are likely to be as poor at judging actual corruption as experts are: the public's judgements about the pervasiveness of corruption correlates with the World Bank's index but not their household's experience with corruption.

Furthermore, it is questionable whether measurements can meaningfully be compared across countries. The sources of the frequently used indices change over time and across countries (Knack, 2007), and it is tough to define corruption in the abstract (Tanzi, 1998). Thus, not only might there be problems relating to different surveys using different implicit definitions of corruption for different purposes (e.g., the ICRG treats corruption primarily as a threat to foreign investment while the World Development Report treats corruption as an obstacle to business in general) 'corruption' might be an inherently culture conditional concept (e.g. Johnston, 1986).

Finally, some studies and indices weigh correlated sources more, assuming that agreement indicates accuracy. Yet, these weighing schemes may exacerbate measurement error as the agreement may result from experts consulting each other and relying on the same information.

All these considerations make the cross-national empirical literature dubious. Albeit, one would have to be an obstinate sceptic to dismiss all the conclusions reached in the literature above. The findings are probably best viewed as interesting correlations that draw an informative picture of what corrupt and non-corrupt countries look like. The findings so far are essen-

tially reduced-form econometric results demonstrating that there exist systematic associations consistent with underlying theoretical predictions. As stated by Kunicová and Rose-Ackerman (2005), the natural next step is to explore empirically the underlying causal mechanisms to decide between conflicting causal stories.

A more recent study by Ferraz and Finan (2011) has been a step in that direction. They use audit reports in Brazil and an RDD to identify the effect of term-limits on corruption. They compare municipalities in which the mayor barely won or barely lost elections. Effective term limit is among these politicians as-good-as randomly distributed. They find that corruption is higher among politicians that do not face re-election. Although their research explicitly tests the model developed by Besley (2006), it is an encouraging observation for other theories that suggest that politicians consider the electoral rules and make strategic choices that determine levels of corruption.

In addition to using a natural experiment, by using an objective measure of corruption, many of the concerns raised in the previous paragraphs are settled. While there are studies that utilize objective data to identify the causes and effects of rent-seeking and corruption (e.g. Fisman, 2001; Duggan & Levitt, 2002; Svensson, 2003; Di Tella & Schargrodsky, 2003; Reinikka & Svensson, 2004; Fisman & Wei, 2004; Olken, 2007; Vicente, 2010) such studies investigating the effect of democratic institutions on corruption are scant.

In sum, although previous literature provides interesting correlational insights into the nature of corruption, there is a gap to be filled with regards to which causal mechanisms produce the observed outcomes. Hence, the future of this literature consists in accruing more narrow and well-identified analyses from various different contexts to draw a more comprehensive picture of the causal determinants of rent-seeking. A first step has been taken by Ferraz and Finan (2011), and it is as a continuation of this research agenda that the current study positions itself.

As will be detailed in ensuing sections, this study exploits a natural experiment in small French municipalities that has made it possible to analyse the causal effect of closed- versus open-list ballots on corruption. An exogenously set rule stipulates that municipalities below

a certain population threshold must use en open-list majoritarian system while those above must use a closed-list PR system. Comparing observations just below and above this arbitrary threshold, the present study aims to discern the causal effect of closed- versus open-list ballots on rent-seeking behaviour by elected politicians. In the next section, I present the theoretical framework that will be used to interpret the empirical findings.

3 Theoretical Framework

To inform the interpretation of the empirical findings, I utilize the simple *career-concern* model of Persson and Tabellini (2000), but modified slightly to fit the current single-district case. The model focuses on elections as a means for voters to select the most competent politicians, and correspondingly incumbency as a means for politicians to signal competence. The model is specified as follows.

There are two periods. Assume that taxes are fixed at $\bar{\tau}$ and that the government budget must be balanced in the two periods. Throughout this exposition, I assume that there are 3 incumbents, $J = \{1,2,3\}$. Moreover, suppose that J also corresponds to a given policy area and that there are J groups of voters (for simplicity, each group is equally large) that vote based on g_t^J. Persson and Tabellini (2000) imagines that there are J localities where voters in locality J care only about public goods local to J. However, in the local context, it is equally likely that there are different types of voters that vote on the basis of different things. Voters form preferences over candidates as follows:

$$w_t^J = y(1-\bar{\tau}) + \alpha g_t^J \tag{1}$$

where $\alpha \geq 1$ is an exogenous parameter, g_t denotes the public good provided by incumbent J at time t (where $\partial w_t / \partial g_t$ is a constant), and y denotes income. Hence, voters judge politicians only according to how much income they can keep and how much public goods the politician provides. A separate budget constraint applies to each politician and in each period. Each politician chooses how to allocate tax revenues between public goods and rents for themselves.

The amount of public goods provided by politician J is given by

$$g_t^J = \eta^J(\bar{\tau}^J - r_t^J) \tag{2}$$

The parameter η^J denotes politician J's competence and r_t^J denotes rents extracted by politician J. For all politicians, η^J is randomly distributed over $[1-1/2\xi, 1+1/2\xi]$ (so that, $E[\eta^J] = 1$ and its density is ξ). Every politician has the same competence, η^J across periods. Rents must be non-negative and less than or equal to $\bar{r}$, which must be less than $\bar{\tau}y$ to ensure (as will become evident) that voters have an incentive to vote for the most competent politicians. Assume that $\bar{r}$ is the same for all incumbents.

The incumbent politicians' period 1 objective is

$$v_J = r_1^J + p_J\beta(R + r_2^J) \tag{3}$$

where $0 < \beta < 1$ is a discount factor and p_J is the probability that the incumbent is re-elected. The quantity r_t denotes rents appropriated in period t, and R denotes the exogenous rents from holding office. The timing of this game is as follows. All incumbent politicians in office in period 1 simultaneously choose rents for that period, r_1^J, without knowing their competence, η^J. After rents are determined, the values of η^J are realised and public good provisions, g_1^J, are residually determined to satisfy the budget constraint. Voters then observe their own utility, but neither r_1^J nor η^J. After voters have observed their utility, elections are held. If an incumbent wins, his competence remains η^J. If he loses, an opponent is elected with competence drawn at random from the same distribution distribution of η^J described above. Lastly, the elected politicians set period 2 rents, r_2^J, and public goods are residually determined again to satisfy the budget constraint.

The sub-game perfect Nash equilibrium of this game requires that each politician behave optimally in each period, given the decision rule of the voters. As the game ends in period 2, period 2 politicians have no incentives to behave in the interest of the voters; that is, they always ap-

propriate maximum rents, $r_2^J = \bar{r}$, which implies public spending at $g_2^J = \eta^J(\bar{\tau}y - \bar{r})$ for all J. Since, by assumption $\bar{\tau}y - \bar{r} \neq 0$, voters are better off with more competent politicians. Hence, they use the elections to reappoint competent politicians and remove incompetent ones from office, taking into account their observed utility in period 1 and knowing that the opponent's expected value at the elections is $E[\eta^J] = 1$.

The period 2 sub-game Nash equilibrium is trivial. However, period 1 equilibrium behaviour yields interesting insights into how ballot structure influences how the incumbents' actions influence the probability of victory and consequently equilibrium rents. First, consider the open-list set-up.

Since each candidate is held accountable individually, the voters consider each candidate separately. Voters may allocate 3 votes and for each incumbent judges whether it is best to re-elect the incumbent or elect a challenger. Consider the equilibrium behaviour from the perspective of one of the candidates.

At the time of the election, voters know that the incumbent maximizes $\upsilon_J = r_1^J + p_J\beta(R + r_2^J)$. The rent extraction of politician J is thus associated with specific public goods provided by J. Let $\tilde{r_1}$ denote the solution to the incumbent's optimisation problem in period 1, $\tilde{r}_1^J = \bar{\tau}y - \xi\beta(R + \bar{r})$. At the time of the election, voters know g_1^J and $\bar{\tau}$ and, knowing ξ, β, R, and $\bar{r}$, can compute $\tilde{r_1}^J$. Hence, from $g_t^J = \eta^J(\bar{\tau}y - r_t^J)$,

$$\tilde{\eta}^J = \frac{g_1^J}{\bar{\tau}y - \tilde{r_1}^J}, \tag{4}$$

where $\tilde{\eta}^J$ is the voters' estimate of incumbent J's competence. Since voters are better off with a more competent politician, they vote for each candidate J if and only if $\tilde{\eta}^J \geq E[\eta^J] = 1$. This means that $\tilde{p}_J = 1$ if and only if $\tilde{\eta}^J \geq 1$ and 0 otherwise.

From the perspective of the incumbent, since he does not know his own competence, his probability of re-election, p_J, is given by $\Pr(\tilde{p}_J = 1) = \Pr(\tilde{\eta}^J \geq 1)$. Knowing this and knowing that g_1^J is residually determined from the government budget constraint, the incumbent chooses

r_1^J. In equilibrium, $\tilde{\eta}^J = \eta^J$, so plugging 2 into 4, we have

$$\frac{\bar{\tau}y - \tilde{r_1}^J}{\bar{\tau}y - r_1^J} = 1$$

which, since we have that the event $\tilde{\eta}^J \geq 1$, entails that

$$\Pr(\tilde{p_J} = 1) = \Pr\left(\eta^J \geq \frac{\bar{\tau}y - \tilde{r_1}^J}{\bar{\tau}y - r_1^J}\right)$$

Under our assumption that the distribution of η^J is uniform, the incumbents objective is to choose r_1^J such that,

$$\max\ \upsilon_J = r_1^J + p_J\beta(R + r_2^J),\ \text{s.t. } p_J = \frac{1}{2} + \xi\left[1 - \frac{\bar{\tau}y - \tilde{r_1}^J}{\bar{\tau}y - r_1^J}\right]$$

The resulting first-order condition is

$$\frac{\partial \upsilon_I}{\partial r_1} = 1 - \frac{\xi(\bar{\tau}y - \tilde{r_1}^J)}{(\bar{\tau}y - r_1^J)^2}\beta(R + \bar{r}) = 0$$

In equilibrium, $\tilde{r_1} = r_1^J$, hence solving for r_1^J, we have

$$r_1^J = \bar{\tau} - \xi\beta(R + \bar{r}) \text{ for all } J$$

and total rents $r_1 = \sum_J r_1^J$ under majoritarian elections are given by

$$r_1 = 3\bar{\tau} - 3\xi\beta(R + \bar{r}) \tag{5}$$

Next, consider closed-list elections. Suppose all three incumbents belong to the same list. The order of candidates on the party list corresponds to the number of their policy-areas. Voters either vote for the incumbents' list or the opposition's list. Assume for simplicity that the electoral system is perfectly proportional. In this set-up, voters of type J vote for incumbent J's list if the incumbent setting policy in area J performed well in period 1, in the sense that voters

infer that $\tilde{\eta}^J \geq 1$. If the incumbent's inferred competence is lower than 1, however, voters vote for the challenger. Thus, voters' optimal behaviour has not changed from the open-list scenario.

The incumbents' behaviour does change, however. In the closed-list scenario, re-election is not purely reliant on the incumbent's receiving the blessing of his supporters. The re-election chances of one incumbent are now also partially determined by the electoral support of his party colleagues. In equilibrium, each incumbent has a 50 percent chance of pleasing his voters, so that $p_{-J} = 1/2$, where $-J$ denotes all other incumbents except for the one from whose perspective we take, J^*. Hence, we can write p_J^* as

$$p_1^* = p_1[(1-p_2)(1-p_3)] + [p_2p_3 + p_3(1-p_2) + p_2(1-p_3)] = p_1\frac{1}{4} + \frac{3}{4}$$
$$p_2^* = p_2[p_1(1-p_3) + p_3(1-p_1)] + p_1p_3 = p_2\frac{1}{2} + \frac{1}{4}$$
$$p_3^* = p_1p_2p_3 = p_3\frac{1}{4}$$

In words, the first incumbent, since he ranks the highest on the list, needs only 1/3 of the voters to vote for his list and therefore only that at least one of his party colleagues is competent, to be re-elected. This means that, from his perspective, only in 1 election out of 4 is his own performance pivotal for his own re-election. The second ranking incumbent needs either that both of his colleagues are perceived as competent or that one of his colleagues are so perceived and that he himself is so perceived to win. Hence, sometimes his perceived competence does not matter because both his colleagues are perceived as competent, and sometimes his perceived competence does not matter because neither of his colleagues are perceived as competent. For incumbent number 3, re-election prospects are even bleaker. Only when both colleagues are perceived as competent and he himself is perceived as competent will he be re-elected. Thus, in 3 in 4 elections, he loses anyway so that his own efforts to seem competent are in vain.

In sum, the highest ranking incumbent free-rides on the effort of his colleagues and so has weak incentives to appear competent; the lowest ranking incumbent has a low probability of being re-elected anyway and so has weak incentives to appear competent; and the middle ranking incumbent has stronger incentives than his colleagues to appear competent, but they are

still weaker than under majoritarian election.

Repeating the analysis of optimal period 1 rents but with closed-list ballots, yields that total period 1 rents are given by

$$r_1 = 3\bar{\tau} - \xi\beta(R + \bar{r}) \tag{6}$$

Comparing 6 and 5 it is evident that, in equilibrium, total period 1 rents are higher under closed-list elections. In open-list elections, a candidate can guarantee his re-election by satisfying his voters. In closed-list elections, however, re-election is partially determined by party colleagues and so there are incentives to rely on good luck and extract higher rents.

In sum, because politicians depend less on appearing competent to be re-elected, the risk of corruption should be higher in closed-list than open-list elections. Thus, one can formulate a research hypothesis: Everything else equal, rent-seeking should be higher in municipalities holding closed-list elections than in municipalities holding open-list elections.

4 Municipal Elections and Political Corruption in France

French local governments have important responsibilities. Municipalities manage local development, build and maintain libraries, museums, and sports and tourist facilities; are in charge of pre-elementary and elementary schools, maintain roads and local public order through the police power of the mayor, and implements some social welfare functions (Loughlin, 2007; Arkwright et al., 2018). Municipalities are governed by the municipal council, which elects a mayor among its members. Normally, elections are held every sixth year. The four previous elections were held in 2014, 2008, 2001, and 1995.

Between 1982 and 2013, France's *Code Électoral* stipulated that municipalities with less than 3,500 inhabitants use a two-round block vote system with panachage. The threshold was changed to 1,000 in 2013. The system works as follows. If the municipality has n municipal council seats, voters can vote for up to n different candidates. In the first round, candidates are elected if they obtain an absolute majority and at least 25 percent of registered voters voted for them. In the second round, the n highest vote-getters receive seats. Voters can vote for any combination of candidates regardless of list. Thus, although candidates often run as groups, the

ballots represent candidate-level preferences.

Contrarily, municipalities with 3,500 inhabitants or more use a two-round, closed-list PR system with a 50 percent winner's bonus. Presented with lists, voters may vote for one only. If one list receives more than 50 percent of votes, that party receives half the seats and the remaining seats are allocated proportionally between the qualified lists — including the winning list. If no one party receives more than 50 percent of the votes, a second round ensues where the list with the most votes is awarded half of the seats while the remaining seats are distributed proportionally among all of the qualified lists — again, including the winning list. With regards to ballot structure, the important point is that voters are allowed to express list-level, but not candidate-level, preferences, and the Code stipulates that seats are allocated to candidates according to their ranking on their list ("Code Électoral - Article L264", 1983).

While praised for combining minority representation and effective government (e.g. by Alliès, 1995), the system has its critics. The electoral system and the practice of holding multiple offices (the system of *cumul des mandats*) have been blamed for enabling political corruption (Mény, 1992). Mény (1992) argues that, in local elections in municipalities with 3,500 inhabitants or more, due to weakly organized parties, parliamentarism is reversed so that the mayor co-opts the council rather than vice versa. This means that the mayor's party colleagues at the council are effectively discretionary selected lacqueys who will do the bidding of the mayor in return for favours and rewards. Making matters worse, the requirements of transparency towards the council on the part of the mayors executive team are arguably too lenient. For instance, the mayor may authorize the demolition of buildings or allocate building permits without the council knowing until the projects have commenced. Amplifying the discretionary powers of the executive, the largest party is often without real opposition. The 50 percent winner's bonus means that minority parties have very limited powers to meaningfully influence policy. In conclusion, it is a widespread opinion among observers of the French electoral system that the closed-lists PR system presents ample opportunities for abuse of power.

Given these observations and the fact that France has almost 1 elected local politician per 100 citizens according to the data used in this study, it is perhaps unsurprising that more than

40 percent of all French corruption cases involve local politicians. While the cost of corruption is impossible to estimate precisely, some speculate that corruption costs the French €30 billion annually (Lenglet & Touly, 2013) — it is not unrealistic to suppose that a significant proportion of this cost is due to political corruption at local levels. Common types of corruption are favoritism, accepting bribes and embezzlement. Generally, the most recurring crimes committed are highly visible abuses of power such as awarding contracts or selling and regulating properties for personal gain or the enrichment of friends and family.

Nevertheless, the fight against corruption has not become a political priority, and, while legal systems are in place, the law is poorly enforced (Phélippeau, Bastard, Lalevée, Le Berre, & Belle, 2011). Due to these adverse incentives that operate within local governments, an important way to constrain local corruption would presumably be through the ballot box. The efficiency of this constraint, as argued in section 3, depends on the precise ballot structure which changes above and below the 3,500 population threshold.

5 Data

To date, there exists no public database of corruption convictions in France. However, TI's French branch has created a database listing cases where individuals or organizations have been convicted of corruption by the French courts. Their sources of information have been national and regional newspapers, and the Internet sites of newspapers, radio, and television stations. They have also utilized the work of Pascot and Riou Harchaoui (2014) to enrich their data. Moreover, the information was cross-checked wherever possible, using multiple articles for the same case. The result is a detailed dataset documenting 763 corruption cases between 1995-2013. The dataset includes information about the parties involved, the crimes committed, the punishments' severity, the politicians' municipality and department, the date of the crimes, and the date of sentencing. This information made it possible to filter out the cases that involved mayors or municipal councillors and locate the case in time and space.

Appealingly, this data provides an objective measure of corruption. As argued in section 2, perceptions of corruption is an unreliable proxy for actual corruption. Moreover, by allowing the French judicial system to decide what constitutes corruption, the measure is comparable

across treatment and control groups.

Yet, the obvious shortcoming is that the data almost definitely only covers the tip of the iceberg. Although sub-optimal for my purposes, this limitation might not be that severe. The main reason is that the type of corruption that most likely escapes media attention, or prosecution altogether, are the less visible types of corruption, as these are less likely sensitive to electoral incentives. Ferraz and Finan (2011) test this argument in the context of term limits and find that, indeed, re-election incentives only affect *visible* forms of corruption, but not invisible corruption nor corruption that does not affect voters' welfare. Thus, the proportion of corruption cases covered in the subset that feature in voters' utility functions (and thus matter for electoral outcomes) is likely to be significantly higher than the proportion of corruption cases covered overall. Another reason the data limitations might be mitigated is that the unusually high number of observations (France's more than 36,000 municipalities, more than the old EU15 combined) will mean that even small effect sizes may be detected. Thus, although the absolute values of the probability of corruption (and so also the absolute value of the effect) will be small, the risk of a type II error is smaller than what the data-shortage would indicate.

Population, income, profession, and immigration data are provided by *Institut national de la statistique et des études économiques* (INSEE). An important benefit of using INSEE's population data is that INSEE also determines whether a municipality is above or below the population threshold. Thus, the risk that population counts are inaccurate is mitigated. Used as a proxy for religious affiliation, data on the existence of an active protestant community is provided by *L'Observatoire du patrimoine religieux*. The presence of an active protestant community is admittedly a rather crude measure and the dataset is somewhat incomplete — religious buildings in all departments are enumerated except for Moselle, Haute-Saone, Doubus, and Loire, which are currently being enumerated. However, due to French law prohibiting the collection of detailed data on ethnicity and religion, this data must suffice.

With regards to coding the variables, due to the number of subjects in this study (the 36,000 municipalities) and the relatively low number of corruption cases (no small- or medium-sized municipality ever experienced more than one corruption case in the duration of one electoral

cycle) the most natural way to code the dependent variable is as a dummy variable that takes on the value of 1 if the municipality has experienced corruption during electoral cycle t, and 0 otherwise. Thus, I define the outcome of interest as the *probability of visible corruption*. While this data is far from as detailed as that of for instance Ferraz and Finan (2011) and so will no allow me to explore more deeply the character of corruption in France, it suffices for the purpose of testing my research hypothesis.

I divide the data into 3 periods: 1995-2001, 2002-2008, 2008-2013, corresponding to the municipal election cycles. If a municipality experienced corruption at any time during an electoral cycle, the variable *corruption* is coded 1 for that municipality in that electoral cycle. For each electoral cycle, I record the population at the year of the election at the end of the cycle. The reasoning behind this is that during that election cycle, politicians form expectations about how they will be held accountable at the next election and act accordingly. It is likely that local politicians have quite accurate information about whether they will be held collectively or individually accountable in the upcoming election during their time in office. Whenever possible, the other covariates are from the same years as the population data, but if not, they are as temporally proximate as possible — erring on the side of staying within the same electoral cycle.

As mentioned, the population threshold was moved in 2013. Thus, 2014 is not included in the third electoral cycle. However, I include the time up until then, since, in the years after 2008 before the law was passed, local politicians should have acted as if the threshold were 3,500.

I have excluded the period before 1995 and after 2013 since there are very few corruption cases recorded. While it would have been interesting to examine the 1,000 population threshold post-2013 since fewer other policy-changes take place there, at the present, there is not enough data. If local corruption data quality improves, this could be an interesting avenue for future research.

6 Empirical Strategy

To measure the effect of the ballot structure on corruption in small French municipalities, I apply a regression discontinuity design (Angrist & Pischke, 2009; Cattaneo, Idrobo, & Titiu-

nik, 2018a, 2018b; Calonico, Cattaneo, Farrell, & Titiunik, 2018; Thistlethwaite & Campbell, 1960), utilizing the fact that the electoral system is determined at a population threshold. I use the local, non-parametric, linear regression and examine whether the results are robust to various modelling choices.

For point estimation of the treatment effect at the 3,500 threshold, I define a sample of municipalities close to the threshold and regress the dependent variable on the threshold-centred population interacted with an indicator for whether the municipality is above the threshold. I use the triangular kernel function so that observations close to the threshold are weighted more. In some specifications, I include social and economic factors as covariates. Since municipalities in the neighbourhood of the 3,500 threshold should be similar on average in these and other dimensions, covariates are included primarily to increase precision rather than to control for confounding factors.

I estimate the local average treatment effect, $\hat{\alpha}_{\text{LATE}}$ using the following specification:

$$\Pr(\text{Corruption})_{it} = \beta_0 + \alpha_{\text{LATE}} T_{it} + \beta_1 \text{Pop}_{it} + \beta_2 T_{it} \text{Pop}_{it} + \eta_{it}$$

Where $T_{it} = \mathbb{1}\{\text{Pop}_{it} \geq 3,500\}$, and $\mathbb{1}\{\cdot\}$ is the indicator function, which takes on the value 1 if and only if the population in municipality i at the end of electoral cycle t is greater or equal to 3,500 and 0 otherwise. Pop_{it} denotes the population of municipality i at the end of electoral cycle t. α_{LATE} is the local average treatment (main) effect.

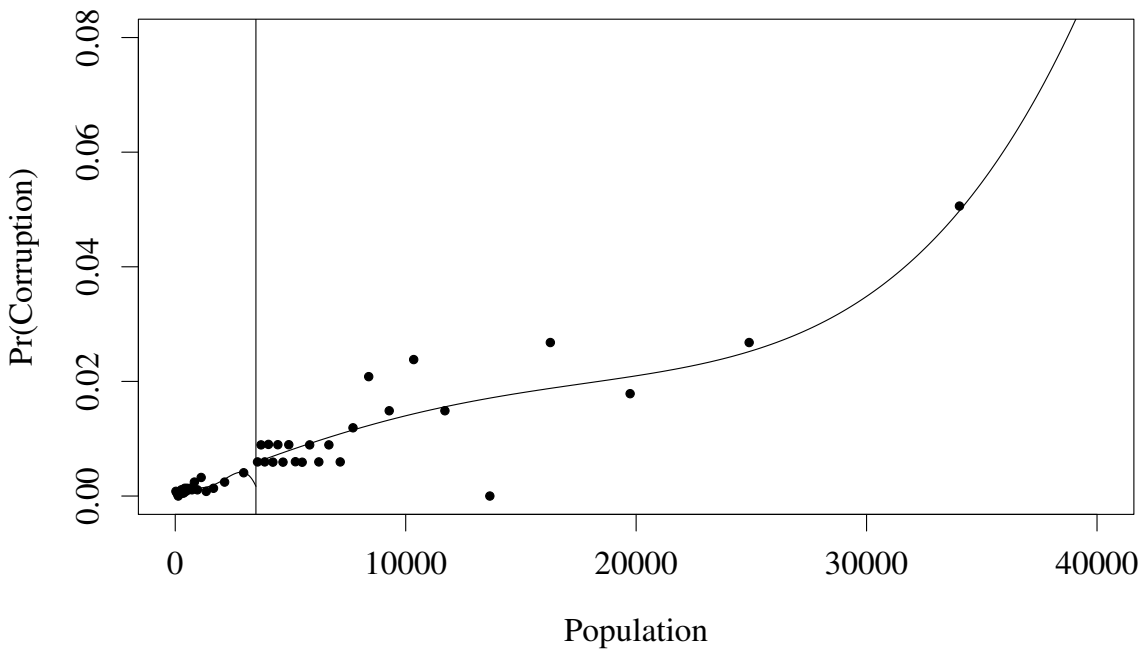
As pointed out by Lee and Lemieux (2010), since the treatment is purely predetermined exogenously and thus does not correlate with covariates, fixed-effects are unnecessary for causal identification in RDD. Robust standard errors are clustered at municipality level.

In addition to model-specification, another crucial parameter is the width of the neighbourhood around the threshold within which one conducts the local linear regression. As empirical results are usually sensitive to bandwidth choice, it is important that it is conducted in a data-driven, automatic manner. This will reduce the ability of the researcher to select bandwidths in an ad hoc manner that might be influenced by biases and render the research hard to replicate.

As a preliminary analysis, although it provides a poor local approximation of the underlying

model, figure 1 gives an overview of the data by displaying the binned data and including a global polynomial approximation. Encouragingly, the plot reveals a positive jump at the threshold. Three additional things can be noted. Firstly, there are a lot more observations in the control than treated group (100,621 versus 8,610). Secondly, the variance is larger in the treatment group. Lastly, the probability of corruption is quite linear with respect to population between 3,500 and approximately 7,000 inhabitants.

FIGURE 1: OVERVIEW OF SMALL AND MEDIUM SIZED MUNICIPALITIES



Note: The lines are fourth degree polynomial approximation of the underlying regression function, fitted individually above and below the threshold on the original data — ie., not the binned data. The points mark quantile-spaced binned averages where bins have been chosen to have approximately equal numbers of observations and minimize integrated mean-squared error. Municipalities with populations greater than 40,000 are excluded to make the plot more informative. The vertical solid line marks the 3,500 population threshold.

I implement the MSE-optimal bandwidth selector (Imbens & Lemieux, 2008; Calonico et al., 2018). As the variances in the treatment and control groups differ considerably ($var(\text{Corruption}|\text{Population} \geq 3500) = 0.017$ while $var(\text{Corruption}|\text{Population} < 3500) = 0.001$) the optimal bandwidths are estimated separately on each side of the threshold (Calonico, Cattaneo, & Titiunik, 2014; Cattaneo et al., 2018a). Another solution to the problem would be to use the logarithmic transformation of population as the running variable. However, to keep the model as parsimonious as possible, and because, according to my analysis, corruption does not increase

linearly in log population, I have chosen not to do so. Symmetrical bandwidths are reported for completeness. Using this procedure, I obtain a substantially larger bandwidth above the threshold than below. The reason why the optimal bandwidth is so large above the threshold appears to be that most of the outcomes examined are rather linear in population — which is hinted at in figure 1. Therefore, the bias resulting from using a larger bandwidth is minimal.

RDD depends on the treatment being exogenous and so would be invalid if politicians were somehow able to manipulate the population numbers in their municipality and effectively choose electoral system. Two tests are utilized to verify whether such bundling at the threshold takes place. If there is no self-selection in the vicinity of the threshold, the distribution of treated and untreated should follow a binomial distribution with $\Pr(D = 1) = 0.5$ (Cattaneo, Titiunik, & Vazquez-Bare, 2017). The results of running the binomial test within various windows around the threshold are displayed in table 1. The null hypothesis that the distribution is binomial is not rejected around the threshold, indicating no bundling.

Another falsification test involves examining whether, near the threshold, treated and control units are similar in observable characteristics. That is, if there is no selection into treatment, the null hypothesis of no treatment effect on predetermined covariates should not be rejected. I analyse every covariate as I do the variable of interest, and in each case, the treatment effect (not reported) was close to 0 and non-significant.

As a final falsification test, I test for a jump in the density around the threshold (McCrary, 2008). The results are displayed in table 2. Again, the results indicate that there is no discontinuity in the density of the running variable around the threshold, and that populations are not manipulated to self-select into treatment.

As a complement and robustness check to the continuity-based analysis, I implement the localized randomization framework. This framework is not based on approximation by regression functions and extrapolation at the threshold, but instead on the idea that the exogenous discontinuity of treatment status can lead to a situation that, in the immediate vicinity of the threshold, mimics a randomized experiment. For the implementation of the localized randomization, I use a uniform kernel and estimate the difference in means. The falsification tests in table 1 indicate

that the treatment is as-good-as randomly distributed up until the bandwidths are approximately 200.

7 Results

Table 3 reports the estimated coefficient $\hat{\alpha}_{LATE}$; i.e., how much the probability of corruption increases at the threshold. Column 1 displays the results for the bandwidths chosen by the MSE-optimal procedure. Columns 2 and 3 show the results when the bandwidths have been decreased to 50 and 75 percent, respectively, of the MSE-optimal. Columns 4-6 include covariates. Column 7 fits the symmetrical MSE-optimal bandwidths. Lastly, column 8 reports the results for the analysis in the local randomization framework.

The coefficients are of the expected sign and consistently statistically significant at the 5 percent level when covariates are included. When covariates are not included, the estimate is statistically significant at the 10 or 5 percent level. Unsurprisingly, the estimated effect within the symmetrical bandwidths is non-significant. The analysis (not reported) shows that as the symmetrical bandwidths increase, the point estimate converges on the same estimate that was obtained using asymmetrical bandwidths, but the standard errors are much larger and the estimate is never statistically significant at conventional levels. This is probably because it picks a right-side bandwidth that is too small given that the variance is much larger in the treated group so that more observations are necessary to form precise estimates. The results indicate an effect of crossing the threshold on the probability of corruption that ranges between 0.004 and 0.008. Overall, the results indicate that changing from open- to closed-list ballots causes about a threefold increase in the probability of corruption measured as the probability of a local politician being convicted of corruption.

Figure 2 panel (A) shows the local linear regression line, and panel (B) shows the non-parametric relationship between population and the estimated jump in corruption as a function of bandwidths. In panel (B), it emerges that the finding is not especially sensitive to the particular width of the bandwidths.

Since there is always a danger of the local linear regression being a poor approximation of the underlying model, I fit a local cubic regression on the same sample displayed in figure 3.

TABLE 1: BINOMIAL TESTS AT THE 3,500 INHABITANTS THRESHOLD

	Binomial tests		
h	N_-	N_+	p-value
1	1	1	≈ 1
10	21	26	0.56
50	113	119	0.74
100	252	236	0.50
150	374	359	0.58
200	519	479	0.22

Note: Binomial tests testing the probability of obtaining the populations around the threshold if population was randomly distributed with a binomial distribution with $q = 0.5$. h denotes the window within which the test is implemented.

TABLE 2: DENSITY TESTS AT THE 3,500 INHABITANTS THRESHOLD

	Density tests				
	h_-	h_+	N_-	N_+	p-value
Method					
Unrestricted, 2-h	125	125	301	298	0.31
Unrestricted, 1-h	125	125	301	293	0.31
Restricted	254	254	673	571	0.75

Note: The density tests implemented are unrestricted density estimation (i.e. estimation not assuming equal c.d.f.s nor equal higher-order derivatives) for both symmetrical and asymmetrical bandwidths, and restricted density estimation. (Cattaneo, Jansson, & Ma, 2017)

TABLE 3: THE EFFECT OF CROSSING THE 3,500 POPULATION THRESHOLDS ON CORRUPTION

	Model							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimate	0.006* (0.003)	0.007** (0.003)	0.006* (0.003)	0.005** (0.002)	0.007** (0.003)	0.005** (0.002)	0.004 (0.004)	0.008* 0.005
N:	6,906	4,623	6,008	5,910	3,889	5,036	2,689	998
$N_-\|N_+$:	1,457|5,449	727|3,896	1,167|4,841	857|5,053	388|3,501	634|4,402	1,554|1,135	519|479
$h_-\|h_+$:	498|5,488	272|2,764	409|4,147	324|4,740	162|2,370	243|3,555	528|528	200|200
Covariates:	no	no	no	yes	yes	yes	no	no
Bandwidths:	MSE-optimal	50%	75%	MSE-optimal	50%	75%	MSE-optimal	Local randomization

Note: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Robust clustered standard errors are reported in parentheses. $h_-|h_+$ indicate the bandwidth below and above the threshold. N_- and N_+ denote the number of observations in the control and treatment group respectively.

Changing the functional form, while the local regression seems to fit better, does not alter the substantive conclusion: the estimate is now $\hat{\alpha}_{LATE} = 0.007$, and it is significant at the 5 percent level. In terms of sensitivity to bandwidth size, the pattern that emerges is very similar to when the local linear specification is used, and so is not reported. Thus, the finding is not sensitive to model specification.

Figure 4 shows how the estimate and standard errors change as bandwidths change when covariates are included. The same pattern emerges but with a more efficient estimator.

Lastly, figure 5 reveals that when the different bandwidths are used within the window where population is as-good-as random, the estimate is usually significant at the 10 percent level, but when the bandwidths dips below a bit less than 170, the estimates fall and are non-significant.

8 Discussion and Robustness Checks

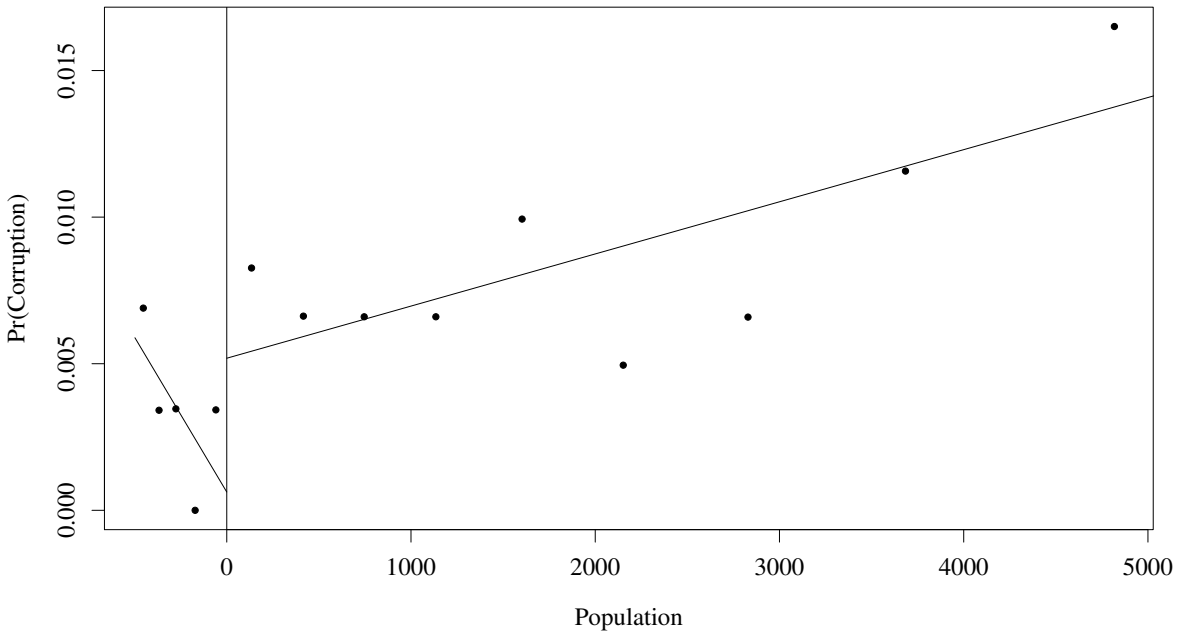
Theory suggests that closed-list ballots weaken the incentives for incumbents to appear competent before the electorate and so produce an equilibrium outcome where rent-seeking (specifically here the probability of corruption) is higher relative to when incumbents' perceived competence directly influences re-election chances. Although not overwhelmingly robust, the finding from the sample of small French municipalities is broadly consistent with this theory: municipalities using closed-list ballots have a higher probability of at least one of its local politicians being convicted of corruption than the comparable control group of municipalities using open-list ballots. It seems that when local politicians can more easily get away with behaving counter to the interests of the voters, they actually do so.

This finding is also consistent with empirical findings discussed in section 2. For example, Persson et al. (2003) also find that ballot structure matters for perceived corruption, and Kunicová and Rose-Ackerman (2005) find that perceived corruption is higher in closed-list PR systems than in majoritarian systems. Hence my findings are in line with previous research.

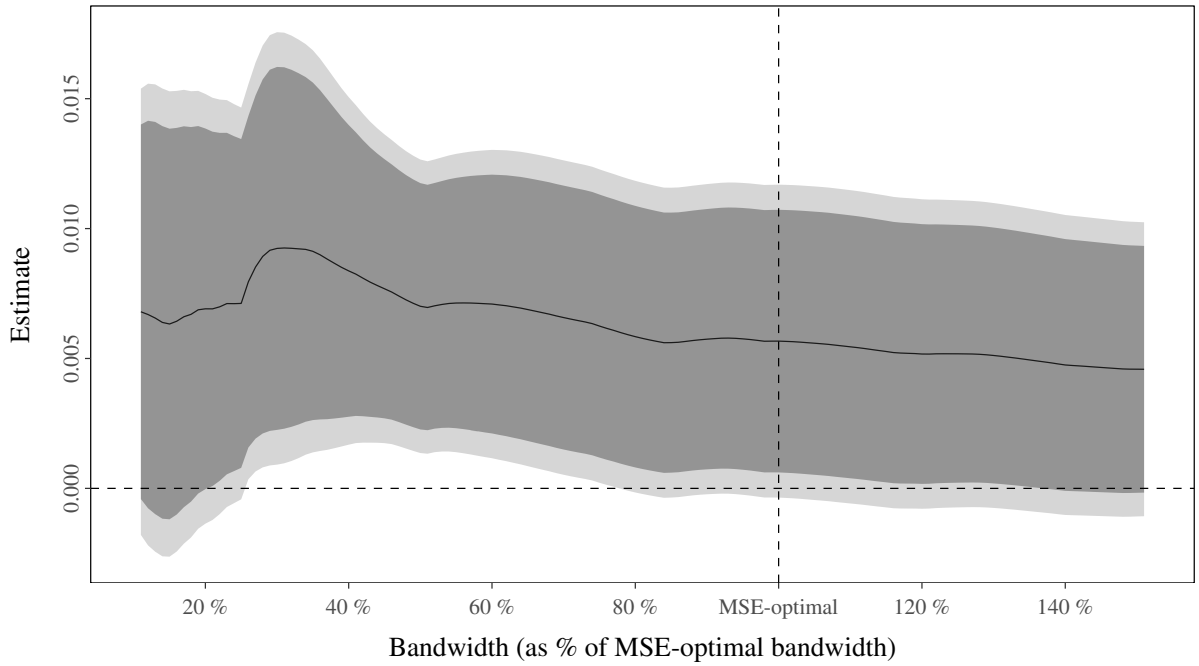
However, the present study has an issue of compound treatment, and therefore the possibility of alternative explanations. At the 3,500 population threshold, the following changes occur: (1) open versus closed list; (2) council size; (3) maximum number of deputy mayors; (4) mayor and

FIGURE 2: RESULTS FOR THE LOCAL LINEAR REGRESSION

(A) THE LOCAL LINEAR REGRESSION

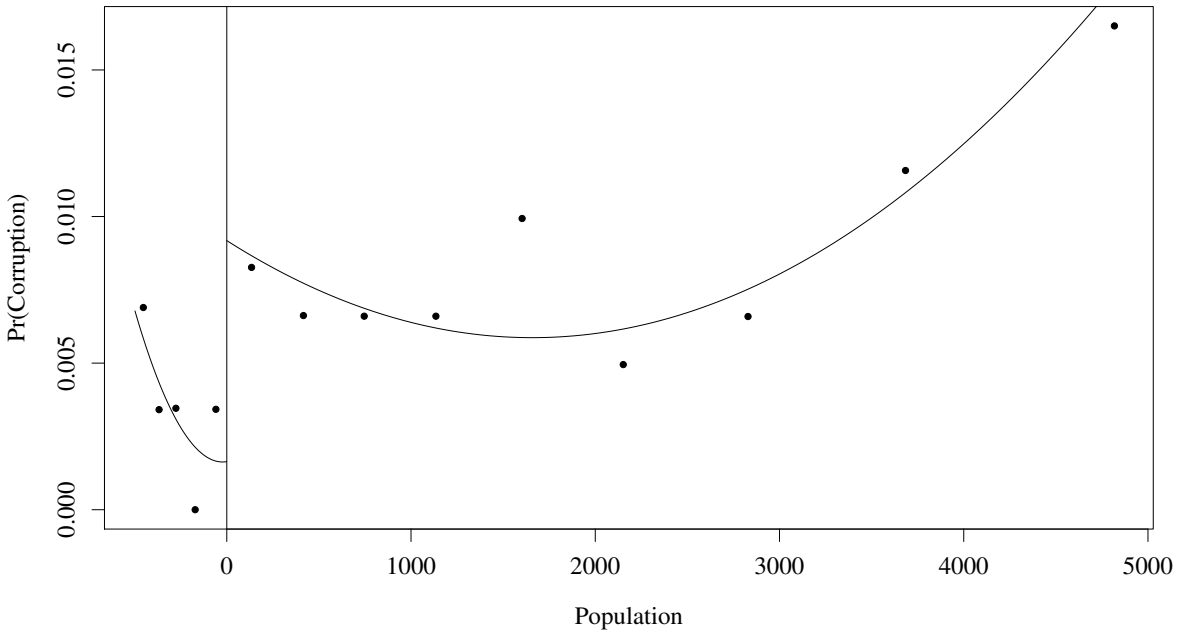


(B) ESTIMATES AND CONFIDENCE INTERVALS FOR DIFFERENT CHOICES OF BANDWIDTHS



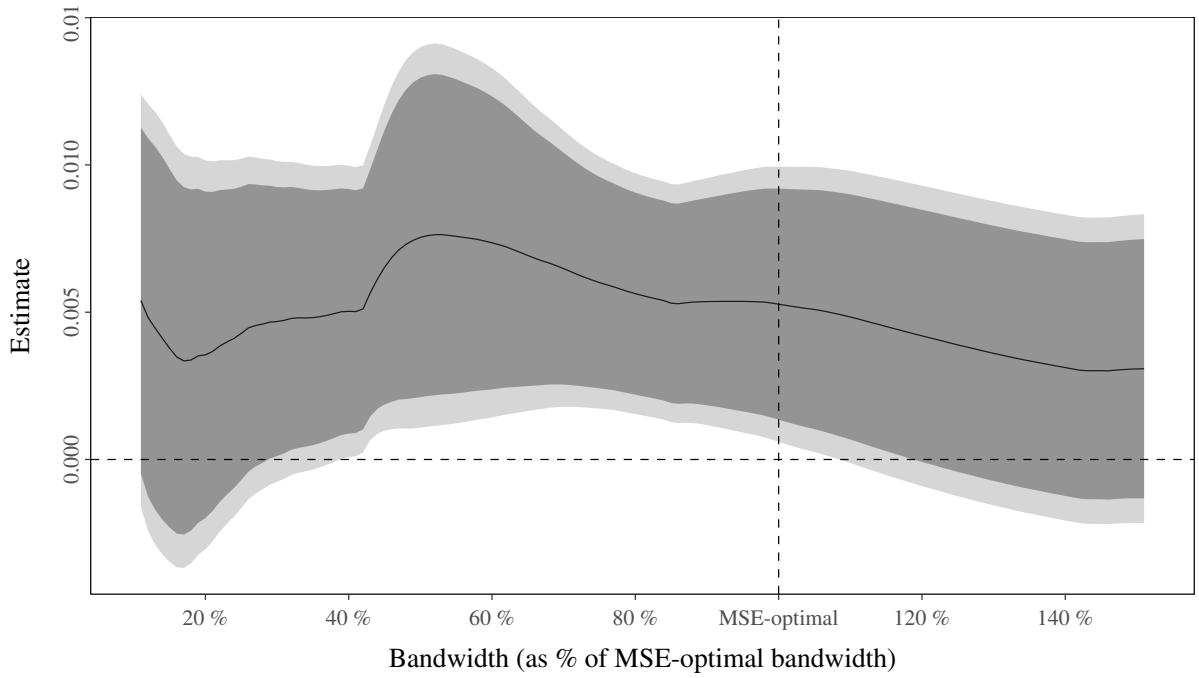
Note: Panel (A) shows the local linear fit. The points indicate binned averages using IMSE-optimal, quantile-spaced bins. Panel (B) shows the non-parametric relationship between population and the estimated jump in corruption as a function of bandwidths. The vertical dotted line indicates the MSE-optimal bandwidths. The light grey area marks the 95 percent confidence intervals while the dark grey area marks the 90 percent confidence intervals.

FIGURE 3: RESULTS FOR THE LOCAL CUBIC REGRESSION



Note: The local cubic regression fitted at the same bandwidths as the bandwidths optimised for the local linear regression. The points indicate binned averages using IMSE-optimal, quantile-spaced bins.

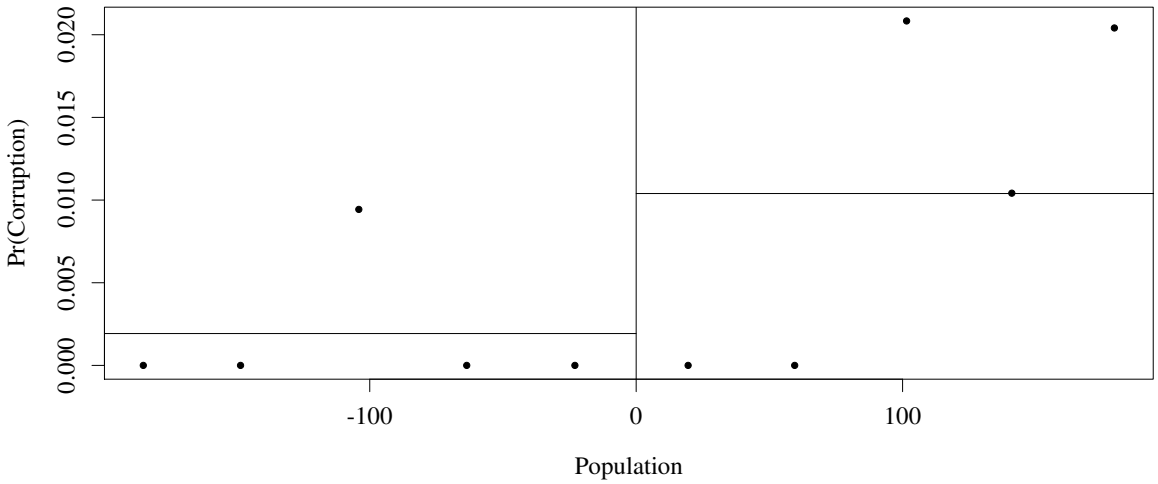
FIGURE 4: ESTIMATES AND CONFIDENCE INTERVALS FOR DIFFERENT CHOICES OF BANDWIDTHS FOR THE LOCAL LINEAR REGRESSION WITH COVARIATES



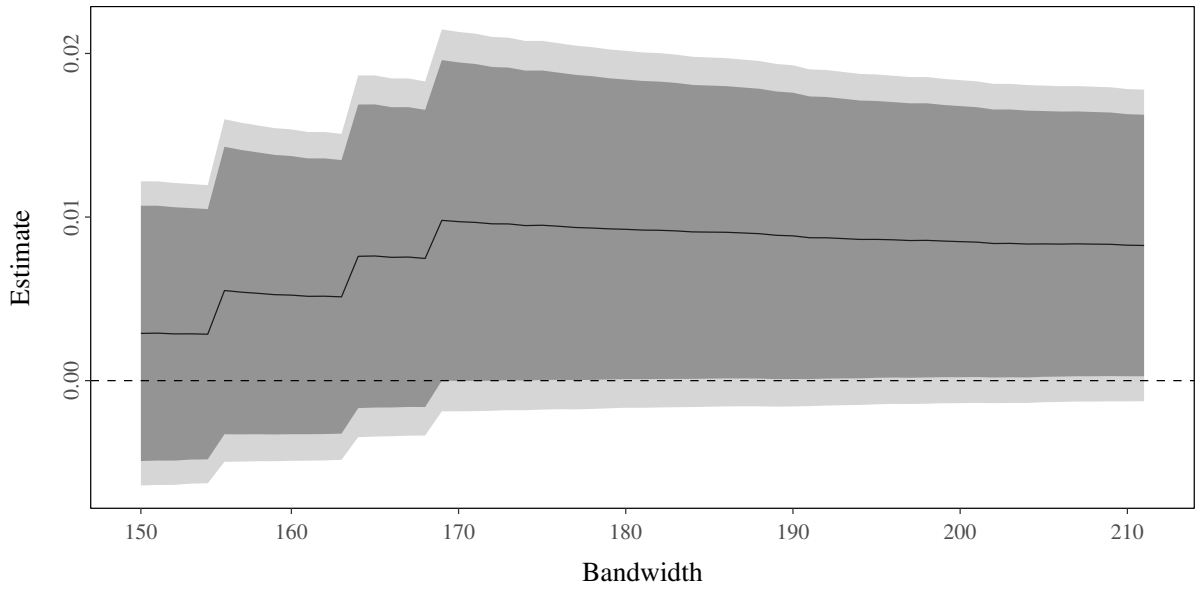
Note: The line draws the non-parametric relationship between population and the estimated jump in corruption as a function of bandwidths when covariates are added. The vertical dotted line indicates the MSE-optimal bandwidths. The light grey area marks the 95 percent confidence intervals while the dark grey area marks the 90 percent confidence intervals.

FIGURE 5: LOCALIZED RANDOMIZATION

(A) THE LOCAL DIFFERENCE IN MEANS



(B) ESTIMATES AND CONFIDENCE INTERVALS FOR DIFFERENT CHOICES OF BANDWIDTHS



Note: Panel (A) shows the local difference in means. The points mark binned averages where the bins are IMSE-optimal evenly-spaced. Panel (B) shows the non-parametric relationship between population and the estimated jump in corruption as a function of bandwidths when covariates are added. The vertical dotted line indicates the MSE-optimal bandwidths. The light grey area marks the 95 percent confidence intervals while the dark grey area marks the 90 percent confidence intervals.

deputy mayor salary; (5) majoritarian versus proportional representation; (6) gender parity; (7) outsourcing scrutiny; (8) council must debate budget prior to vote; (9) committees follow PR principle; and (10) amount of paid leave for council work (Eggers, Freier, Grembi, & Nannicini, 2018; Lepinard & Lieber, 2015).

Thus, for instance, some models predict that salary should decrease rent-seeking by leading to more competent politicians, or politicians with preferences more congruent to voters' preferences, to be elected (Besley, 2004; Mattozzi & Merlo, 2008). Persson et al. (2003) argue that higher wage is likely to suppress corruption since voters can exert more control by threatening to oust the politician from a more luxurious position. Lastly, higher wages might increase morale (Gagliarducci & Nannicini, 2013).

Some of these changes' effects may be indirectly tested since they also change on other population thresholds. In table 4, I list the different population thresholds and relevant policies that change on those thresholds. I run the same analysis on these thresholds that I did on the 3,500 threshold. Moreover, to verify that the relationship between population and corruption is not fundamentally discontinuous so that the jump at the threshold is contaminated by other factors (i.e. treatment effect is zero when it should be) I also test two placebo thresholds close to the 3,500 threshold.

As can be seen in table 4, at none of the thresholds is there an effect on corruption as large and as consistent as the effect of crossing the 3,500 threshold — most thresholds are neither. For instance, the estimate for mayor and deputy mayor salary is zero and non-significant at both the 500 and 1,000 threshold. This indicates that salary does not confound the estimate. Even if it did however, it would downwardly bias the estimate and so not serve as an alternative explanation for the jump. Encouragingly, there is no jump at the placebo thresholds.

Only at 1 of 5 thresholds were the effects of increasing council size and number of deputy mayors distinguishable from zero. This is strong evidence that council size should not matter. Nevertheless, since there is some indication that crossing the 2,500 population threshold increases corruption, this policy change warrants some discussion. The jump suggests that council size or the number of deputy mayors might influence the discontinuity at the 3,500 threshold where council size changes from 23 to 27 and the maximum number of deputy may-

TABLE 4: THE EFFECT ON CORRUPTION OF CROSSING OTHER POPULATION THRESHOLDS

		Model					
Population	Policy change(s)	(1)	(2)	(3)	(4)	(5)	(6)
100	CS; MDM	0 (0.001)	0 (0.001)	0 (0.001)	—	—	—
500	CS; SM; MDM	0 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0 (0.001)	0.001 (0.001)
1,000	SM; PR	0 (0.001)	0.001 (0.001)	0 (0.001)	0.001 (0.001)	0 (0.001)	0.001 (0.001)
1,500	CS; MDM	−0.001 (0.0013)	−0.002 (0.002)	0.002 (0.001)	−0.001 (0.001)	−0.002 (0.001)	0 (0.001)
2,500	CS; MDM	0.006* (0.003)	0.004 (0.004)	0.006* (0.003)	0.005* (0.003)	0.005 (0.003)	0.006* (0.003)
3,000		−0.002 (0.003)	−0.001 (0.004)	−0.003 (0.003)	−0.001 (0.003)	−0.001 (0.004)	−0.002 (0.003)
4,000		0 (0.002)	0 (0.004)	−0.001 (0.003)	−0.001 (0.003)	0 (0.004)	0 (0.003)
5,000	CS; MDM	0 (0.004)	0 (0.005)	0 (0.004)	0.002 (0.004)	0.004 (0.004)	0.003 (0.004)
Covariates:		no	no	no	yes	yes	yes
Bandwidths:		MSE-optimal	50%	75%	MSE-optimal	50%	75%

Note: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Clustered robust standard errors are reported in parentheses. "CS" indicates council size; "SM" indicates salary of mayor and deputy mayor(s); and "MDM" indicates maximum number of deputy mayors. Asymmetrical bandwidths were used wherever computationally possible, if not, symmetrical bandwidths were used if possible. Regressions where neither were possible have been marked with a dash.

ors changes from 7 to 8. Might it be that more potential reprobates with power means more corruption? Details of the data suggest that if this is the case, it should only have a minor effect. Although council sizes range from 7 to 69, almost 80 percent of corruption cases recorded are committed by mayors alone, suggesting the council size matters less. Interestingly, it might be noted that this fact too is consistent with the model of Persson and Tabellini (2000), in which the highest-ranking politician on the list has particularly strong incentives to misbehave.

Moreover, theoretical predictions with regards to district magnitude are ambiguous. For instance, on one hand, Persson and Tabellini (2003), Persson et al. (2003) argue that smaller districts (i.e., fewer elected politicians) increase entry barriers and therefore rent-seeking. On the other hand, Kunicová and Rose-Ackerman (2005) argue that although entry barriers are higher in small districts, incentives for political opponents to police each other are stronger.

In sum, while the data does not entirely rule out that council size contributes to the jump at the 3,500 threshold, there are strong reasons to doubt it plays a major part in explaining it.

With regards to the majoritarian versus PR change, it is commonly thought that more proportionality leads to more rent-seeking. More coalition executives means that it is more difficult for voters to assign praise or blame. It is unlikely, however, to be an important factor here. The reason is that, as argued by Eggers (2015), the change involves only a small "dose" of increased proportionality. The change involves going from a pure majoritarian system to a system where the largest list receives 50 percent of the seats while the remaining seats are allocated proportionally. Thus, one party will always win 50 percent of the seats plus seats allocated proportionally, and therefore form a majority. This, in turn, means that the accountability-diluting mechanism of PR systems is non-existent in this case.

In fact, Chang and Golden (2007) have found evidence that intra-party competition in open-list elections in pre-1994 Italy impelled candidates to corruption to finance their election campaigns. Thus, if the PR element introduced at the 3,500 threshold biases the estimate, it is more likely to downwardly than upwardly bias it.

Regarding gender, a gender parity law took effect in 2000, which stipulates that, for municipalities with 3,500 inhabitants or more, each list must contain an equal number of males and females. The proportion of female politicians might influence corruption levels as there is some

evidence that females are less likely to be corrupt, as discussed in section 2. Unfortunately, corruption data prior to year 2000 is poor and so there is no way of testing whether gender plays a major role for corruption levels in this context. However, as with wages, gender parity has been linked to lower rather than higher levels of corruption. Thus, if gender does play a role, it is likely to suppress rather than inflate the causal estimate.

The same is true of points 7. through 10. on the list. It is impossible to examine empirically whether they play a role in corruption levels. Yet, they are all factors that one would a priori think would mitigate the adverse effects of lower accountability, not amplify them.

As a final note, it is worth mentioning a subtly different causal story, which is less theoretically developed in this particular context, but might nevertheless be a competing explanation for the estimate. Rent-seeking among politicians can be generalized by applying the public good logic (Olson, 1965/2003; Persson et al., 2003). A brief exposition of the argument suffices for our purposes. In a system where politicians are held individually accountable, political office is a private good in the sense that the politicians' efforts to appeal to the electorate benefits her alone. In systems where politicians are to varying degrees held accountable together with other politicians, political office a public good: the effort one of one politician benefits her party colleagues or otherwise politicians that may be associated with her.

Thus, the particular mechanism of the career concern model is a sufficient but not necessary condition for free-riding behaviour among incumbents. Within the general public goods framework, free-riding incentives among politicians might be generated by a host of factors. Under the assumptions of the career concern model, the *closedness* of the list and the probabilistic nature of elections generate higher rent-seeking. However, under certain conditions, other mechanisms might be important. If voters lack information about individual politicians' efforts; do not care about politics much and vote based on vague criteria; or vote according to ideology, free-riding may become possible when when several politicians appear as "teams" — for instance, by representing the same ideology or social group. In these situations, individual politicians might free-ride on the reputation and clout of the party. Worryingly for the current study: if using closed-list ballots strengthens the salience of parties (for instance by strength-

ening the party leadership) in the eyes of the voters, going from a open to a closed list system might generate more rent-seeking although the closing of the lists itself has no influence on politicians' perceived probability of being re-elected due to ranking on the list.

Empirical evidence suggests that, in practice, it is likely that both causal mechanisms play partial roles (Persson et al., 2003), although the high degree of collinearity between open-lists ballots and individualized elections and conversely closed-list ballots and partisan elections at national levels, cross-country data make these findings unreliable.

The same is true in the current case. In municipalities below the threshold, elections are unpredictable for every candidate. Mayors may suddenly find their leadership challenged by outsiders for personal reasons, and so the local political elites are completely dependent on nurturing their personal reputations of competence and trustworthiness (Le Bart, 2003). In municipalities above the threshold, on the other hand, elections are highly partisan and the party leader is a lot more powerful since her leadership is not at the mercy of the electorate alone. Le Bart (2003) argues that these elections are highly personalized in the sense that as far as the electorate is concerned, it is choosing between mayors. This suggests that the perceived performance of one local government is largely a public good for incumbents: on the one hand, the mayor may free-ride on the efforts of the rest of the party and the rest of the party may free-ride on the reputation of the mayor.

Due to the size of the estimated effect (about a threefold increase in the probability of corruption) and the particular nature of local politics in municipalities above the threshold as outlined in section 4, it is likely that both mechanisms play partial roles.

Elections is just one of many constraints on politicians. However, the present analysis indicates that even in a country with strong institutions and legal traditions and where corruption levels are not especially high, electoral incentives play an important role in constraining corruption. In 2013, the threshold for when municipalities must use closed-list PR was lowered. This has allowed French reformers to attain higher degree of gender parity and, as Eggers (2015) find, provide better conditions for higher voter turnout. Unfortunately, however, this study suggests that such reforms come at a cost.

To what extent do these results help us understand the role of ballot structure in explaining corruption outside the specific context of small French municipalities? Firstly, it is a strength that this study is unique in presenting evidence that is relevant in the European context. The findings presented here might be more relevant to reformers in other European countries who seek to reform their local governments. So far, other studies on corruption have often focused on developing countries. Ferraz and Finan (2011), though they are able to argue very convincingly that they find a causal effect, analyse Brazil, which is a very different country from the European ones. Secondly, the finding presented here claims to measure the effect of a universal model. Everything else equal, the model finds that the equilibrium outcome is more corruption in closed-list elections. Factors such as for example culture, climate, and other administrative structures do not feature in the model. Hence it should be true anywhere that, everything else equal, closed-list elections should increase corruption — the present study find empirical support for this argument.

Nevertheless, "everything else equal" is a strong caveat. In practice, we need a better understanding of how the effect of accountability interacts with other features such as district size and other institutional features. As mentioned, some evidence indicate that heightened accountability can, in some contexts, generate perverse incentives such as committing crimes to fund the electoral campaign (Chang & Golden, 2007). Another important thing to consider is that, in France, local parties are very weakly organized, and so party strength matters little regardless of electoral system. In other contexts where strong parties are important for the functioning of the democratic system, open-list ballots might have a destructive effect by weakening the ability of the party leadership to control its members. In other words, ballot structure might interact with a series of other cultural or institutional factors. In general, the local average treatment effect is not generalizable to outside the immediate neighbourhood of the threshold. This means, in practice, not only that the effect might be different or non-existent for other levels of governments, but also for larger or smaller municipalities. Ultimately, these considerations highlight the need to continue to accrue more narrow and well-identified analyses from various different contexts in order to draw a more comprehensive picture of the effect of ballot structure on rent-seeking.

Interesting directions for future research would be to find natural experiments of the career concern model at different levels of government and in countries with different political cultures. Another interesting avenue for future research might be to continue in the vain of Eggers (2015) and explore other consequences of ballot structure such as the strength of political parties or more generally the consequences for voters' welfare.

9 Conclusion

This article has utilized a rare natural experiment in small French municipalities that has made it possible to investigate the causal effect of closed- versus open-list ballots on corruption. Using a regression discontinuity design, I find that the probability of serious visible corruption is higher in small municipalities that use closed-list ballots than in similarly sized municipalities that use open-list ballots. I also find that the result is robust to bandwidth selection, conceptual framework, and various robustness checks.

The finding suggests that reformers who seek to attain various social goals by stipulating that local elections must use a closed-list system should consider this cost as well as the benefits. As with most things, there is no such thing as a free lunch.

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