Voter turnout and fiscal policy

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Abstract

In this paper, we examine whether shocks in voting costs can impact elected representatives' quality, defined as the capacity to fund projects at the lowest cost. Using data on French municipalities and local variations in seasonal infections incidence as a shock on voting cost, we estimate that higher incidence lowers voter turnout, increases subsidies obtained by a municipality, decreases harmful financial decisions, and increases the municipality's investment in infrastructure. We present a model where these predictions would hold, in particular for municipalities with a high base level of turnout.

1 Introduction

In an electoral competition, candidates may differ in policy preferences, as well as in quality, defined as the capacity to fund projects at the lowest cost for constituents. This capacity could reflect better knowledge or training that may give access to better sources of funding, either public or private. In this paper, we ask whether changes in voting cost can affect the quality of elected representatives and the unit cost for their constituents of the policies they implement.

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To address this question, we use as a shock on voting cost the incidence of digestive infections at the time of elections. We choose this particular shock for two reasons. First, its realization is observed only at the time of the election and therefore does not affect the set of candidates running nor the platforms they propose. Second, it can potentially affect the entire population, unlike other shocks considered in the literature, such as enfranchisement laws. We use data on French municipalities over the period 2001-2013, which covers all the years of two consecutive municipal terms. Two elections took place over the period studied, the first in March 2001, and the second in March 2008.

We estimate the effects of the average weekly incidence of digestive infections in March on four (sets of) dependent variables: voter turnout, two measures of the cost of public goods, infrastructure expenses and other fiscal outcomes, and characteristics of the elected representatives. We include as control variables a set of municipal variables that capture demographic and economic conditions at the beginning of a term. Since we observe municipalities for two terms, all our estimations include municipal fixed effects.

Our first result is that an increase in infections incidence decreases voter turnout in the first round of the election. Sick people are less likely to vote. The incidence of digestive infections indeed significantly affects voting costs. We can then estimate the effect of voting cost on two variables that measure the cost of public policy following an election. Both variables depend on municipal councilmen's actions, and hence reflect their quality.

Our first measure uses transfers between the municipality and other governments. Every municipality can receive funds from these upper government levels by applying for targeted subsidies used for infrastructure expenses only. The allocation of these subsidies is left to the discretion of the higher levels of government.[1] The ability to obtain these funds depends on the knowledge of the politician and the quality of the project she proposes. Every municipality also transfers funds to other levels of governments. We define net discretionary transfers as the difference between targeted subsidies received and transfers paid.

The cost of a given investment in infrastructure borne by municipal residents is obviously lower if net transfers are bigger. We therefore estimate the effect of infections incidence at the time of the election on the ratio of net transfers over total infrastructure expenses in the years of the term that follow the election. We find that this effect is positive and significant.

[1]Some subsidies are automatic and depend on observable characteristics of a municipality (its population, its surface, etc.) We do not count them here.

These effects are robust if we control for infrastructure expenses.

The second measure of unit costs of implemented policies (and thus quality of the candidates) is the probability to hold financial products that became known as “toxic loans”. In 1995, some banks started to propose to municipalities structured loans with variable interest rates. These loans turned out to be extremely costly for the borrowers, who often ended up having to pay several times the amount they would have paid if they had chosen other types of loans. The decision to contract this type of loan seems to have been driven by lack of financial literacy; holding toxic loans thus indicates lower quality.[2] We estimate that infections incidence decrease the probability to hold such a loan.[3] Higher interest rates mean a higher cost of public policy borne by municipal residents.

We then estimate that an increase in infections increases the infrastructure expenses. We find no significant impact on other fiscal outcomes.

Finally, we assess the effect of average infections incidence on characteristics of the elected representatives. Each municipality is governed by a municipal council, headed by a mayor. The mayor is typically the local leader of the party that won the largest number of seats in the council. We find that infections incidence has no significant impact on the political affiliation (left or right) of the mayor.[4] Then, using information on the professional activity of mayors, we estimate that infections incidence increases the probability that the mayor’s profession requires at least a Master’s degree. This result shows that infections incidence can indeed impact electoral outcomes, but not through political affiliation.

We then use infections incidence to define instrumental variables for the log of voter turnout and estimate the elasticity of infrastructure expenses to voter turnout. This elasticity is around -7 if each municipality has the same weight, and around -2.5 if we weigh each municipality by its number of inhabitants.

The identification assumption is that infections incidence at the time of an election is not correlated with some unobserved factor that explains

[2]An official report published in 2011 (Cour des Comptes 2011) explicitly warns municipalities against risky loans. The report explains to mayors that bankers’ financial incentives are different from mayors’ constituents’ interests. It also reports that many toxic loans have deceiving names that may induce borrowers to believe they have taken loans with fixed rates. Several suits against the banks offering such loans have claimed that borrowers were misinformed.

[3]We use an indicator variable as dependent because we have no trustworthy information on the actual interest paid by the municipality.

[4]The number of mayors belonging to extremist parties at the time of study is so small that our estimations cannot be attributed to their electoral results.

voter turnout, electoral outcomes, or municipal policy following the election. Such an unobserved factor could impact infections incidence directly, or could create an error in the measurement of incidence correlated with municipal policy. Consider, for instance, a decline in economic conditions (not captured in the set of control variables) that triggers changes in municipal policy. Such a decline might make the municipal population either more vulnerable to infections (direct impact on infections incidence) or less likely to visit a doctor if they are sick (measurement error). In either case, such a decline would be a source of endogeneity.

We test the validity of the identification assumption in three ways. First, we estimate the effect of the average weekly incidence in the months before and after the election, i.e. February and April, on the main dependent variables considered. Neither variable has any significant effect or substantially alters the value of the coefficient of infections incidence in the election month. Any omitted variable that would affect infections incidence in general, either directly or through measurement error, such as economic decline as described above, should affect infections incidence in any month of the election year, not in March specifically. Of course, average weekly incidence in March is a noisy measure of the actual number of individuals who were sick in the week of the election and had symptoms on the election days. However, if the noise in March specifically is not correlated to omitted factors explaining municipal policy (or turnout, or electoral results), the previous estimations reflect a causal effect of infections on the dependent variables.[5]

Then, we propose two series of falsification tests. The first series are the regressions of past values of the main dependent variables on infections incidence at the time of an election.[6] The second series are the regressions of current values of the main dependent variables on infections incidence measured in the month of March in a non-election year.[7] None of the main coefficients of interest in these tests are statistically significant.

To summarize, a shock on voting cost will decrease voter turnout, increase the probability that the higher-quality candidate wins (as measured

[5]Infections incidence could directly impact infrastructure expenses, independent of its impact on voter turnout. For instance, a high incidence may justify the building of medical facilities. These tests also address this point: if infrastructure expenses respond to infections incidence, they should be affected by infections in February (which is comparable to infections incidence in March) and/or in April, which is not the case.

[6]We have no information on voter turnout or electoral results in municipal elections before 2001. Due to data limitations, we cannot observe all the years of the municipal term ending in 2001.

[7]We report only the results for one year before and two years before an election year.

by her ability to decrease the unit cost of public policy borne by municipal residents, through increasing transfers to the municipality or through decreasing interests on loans) and increase the quantity of public goods provided (by increasing investment in municipal infrastructure). We interpret these results as evidence that higher infections incidence disproportionately reduces the ratio of voters who favor the candidate associated with lower infrastructure expenses.

Turning to the interpretation of the results, a natural explanation could be that those individuals more likely to get sick might have specific preferences. For instance, infections may disproportionately affect older people, who may favor less public good. We cannot measure political preferences directly, but one test suggests that this explanation is unlikely to be the main one: the effect of infections incidence on voter turnout is not significantly different in municipalities with an older population compared with the rest of the sample. Nor is it significantly different in richer municipalities.[8]

Instead of trying to relate the empirical results to the effect of infections on a specific demographic or economic group, we examine whether our main empirical results could arise in a model with no correlation between voting costs and political preferences. Based on minimal assumptions about candidates and voters, we derive a general condition for our results to hold and derive an additional prediction that is borne out in the data.

The model involves two candidates (A and B) who make campaign promises about the level of expenses on a public good so as to maximize their vote share. We assume that candidate A is of higher quality than candidate B in the sense that A can provide a unit of public good at a lower cost for the municipality than B. In equilibrium he proposes a higher level of expenses and all voters agree on his higher quality. In addition to the utility they derive from the public good, all voters derive an idiosyncratic utility from the election of candidate B, which can be positive or negative, and is drawn from a continuous distribution with a median at zero and support on $\mathbb{R}$.[9] Any eligible voter votes if the absolute difference between her utility if one candidate wins and her utility if the other wins is larger than some cost of voting c, uniform across individuals. Solving the model provides the necessary and sufficient condition that must be satisfied for

[8]This effect is also consistent with epidemiological reports regarding the infections we consider, which find little correlation between economic or demographic conditions and the probability of infection (Herikstad et al. 2002).

[9]This payoff may represent the ideological proximity of a voter with candidate B, for instance.

our empirical predictions to hold.

We show that this condition is satisfied in particular if the cost of voting is small.[10] The intuition is as follows. If the cost of voting is null, every eligible voter votes and the higher-quality candidate gets a higher vote share than the other. If the voting cost increases, only individuals for whom the gain from voting is larger than the voting cost vote; both candidates may lose votes. If they both lose the same number of votes, the vote share of the higher-quality candidate will increase, since the proportion of votes lost is smaller for him than for the lower-quality candidate, who has a smaller number of votes to begin with. When the cost of voting remains in an interval close enough to zero, voter turnout is high, and a marginal increase in the cost of voting (for instance because of an infection) causes both candidates to lose approximately the same number of votes, which then helps the higher-quality candidate.

Empirically, the model implies that one should observe that a small increase in voting cost will favor the higher-quality candidate in municipalities where the "base level" of voter turnout is large. This seems consistent with the context of French municipalities.[11] To check this point further, we propose a proxy for the base level of voter turnout that we use to estimate the specific impact of infection incidence in municipalities with a larger voter turnout than its median value. We find that this impact is indeed large and statistically significant. In fact, the estimated effects of infections incidence on municipal policy are driven mostly by the municipalities with a low cost of voting.

Our paper is related to several studies that have examined the effect of variations in voter turnout. Most of these papers study a major change with a large effect on the voting cost of a specific group of the population. They show, in general, that changes in turnout bring about changes in public policy consistent with inferred preferences of the affected groups. This is the case with the enfranchisement of African-Americans or of women (Husted and Kenny 1997, Cascio and Washington 2014, Miller 2008), with the turnout increase among the poor and less educated due to new voting technology (Fujiwara 2015), and with changes in registration costs (Braconnuer, Dormagen and Pons 2014). Unlike in these papers, the shock on voting cost studied here may affect anyone. In addition, the shock is relatively moderate in the sense that eligible voters may be sick and vote nevertheless. These features fit better the properties of voting cost in the

[10] There is no converse result: for some distributions of preferences, the condition may hold for c arbitrarily large.

[11] 80 percent of the eligible population votes in municipal elections, on average.

model we propose.

Our paper is also related to Hodler, Luechinger and Stutzer 2015, who show that predictable shocks (in their case the introduction of postal voting) affect the average information held by those who vote. They consider a model where the decision to acquire information is endogenous and depends on the voting cost (only those who plan to vote seek information). Our approach is different: in our case, the shock occurs at the time of the election, and modifies neither voters' decision to acquire information nor candidates' strategies. In addition, we use explicit measures of the cost of public policy.

A few papers in the literature examine the effect of variations in turnout on the effort made by elected officials or on the rents they attempt to extract. Aldashev 2015 considers a situation where turnout is endogenous and depends on the cost of acquiring information for a group of mobile voters. A lower cost of voting has a disciplining effects on the candidates as they can anticipate the higher turnout of informed voters who will refrain from voting for them if they extract too much rents. The situation we consider is different: turnout will also have an effect on the resulting quality of the policies, but not because the candidates can anticipate the level of turnout (they cannot, except perhaps a few weeks before the election), but because higher turnout affects who is elected. Note that we adopt a similar model for the turnout decision. In a similar spirit Joe 2016 shows that exogenous shocks on turnout can increase the efforts made by elected legislators. Using data from Korean elections Joe 2016 shows that variation in turnout increases the number of bills proposed by the officials. We have a similar identification strategy but our focus is on the quality of the elected officials and the mechanism we consider is very different.

Finally, our paper belongs to the general literature examining factors that have been shown to impact turnout. These include, apart from demographic or economic characteristics, the weather (Hansford and Gomez 2010, Gomez et al. 2007), general rules of governance (Hinnerich and Pettersson Lidbom 2014, Herrera, Morelli and Palfrey 2014), voting systems (Braconnuer, Dormagen and Pons 2014), candidates' ethnicity (Washington 2006), and availability of certain information technology (Stromberg 2004, Enikolopov, Petrova and Zhuravskaya 2010, Gentzkow, Shapiro and Sinkinson 2011, Gentzkow 2006 and Gavazza, Nardotto and Valletti 2014). Like infections incidence, these factors may impact public policy, through their effect on variables other than voting cost or through their impact on the political affiliation of elected representatives – that is, directly or indirectly.

The rest of the paper is organized as follows. Section 2 describes French municipal institutions, the data used in the estimations and our measures of candidates' quality. Sections 3 and 4 present the empirical specification and results. Section 5 proposes a simple model to understand the results and discusses their possible interpretations.

2 Institutional Background and Data on French Municipalities

2.1 Institutional Background

Since the French Revolution, in 1789, the French territory has been divided in municipalities, the smallest administrative unit in the country. France now counts more than 36,000 municipalities. The rules that govern municipal elections, and the rights and duties of municipal councilmen, are set in national law, and apply uniformly across the French territory.[12] Elections are held on the same days nationwide, always on a Sunday, usually in March. Any adult above 18, French or EU national, whose main residence is in the municipality, can register to vote. To be allowed to vote in a given year, an eligible individual must have registered before the end of the previous year. The number of registered voters is thus publicly known several months before the actual election.

Municipal councilmen are elected through direct universal suffrage. After the election, councilmen elect a mayor among themselves, who will be the agenda setter for the municipal term. Mayors are responsible for the enforcement of decisions passed at the council. A mayor has no extra decision power, however, since every decision taken by a council must be approved by a simple majority of councilmen.

The number of councilmen, and other rules of the electoral system of a municipality, depend on the number of inhabitants. Details of the electoral system are reported in Appendix A. In practice, most candidates (more than 80 percent[13]) are affiliated with a local group or *list* of candidates. Each list is headed by a leader who is usually the main proponent of the

[12] Our sample contains no data on the roughly 500 municipalities located in Corsica or in overseas territories and departments.

[13] The data we use in this paper do not give detailed information on candidates who do not win a seat in the municipal council, so we use information on around 5000 municipalities located in the West of France, provided by a local newspaper for the 2001 election (*Ouest-France*) to compute the statistics presented in this paragraph.

program of his/her list in the media, such as the local press. Besides single candidates, who usually obtain few votes and are not elected, most elections are run between two lists.[14] Unlike a majoritarian system, the electoral system is such that the list that does not obtain the largest number of votes may still obtain seats in the municipal council.[15]

For all municipalities, municipal elections may comprise two rounds, held a week apart. Any candidate (or party of candidates) who obtains a number of votes larger than half the size of voter turnout in the first round is elected directly.[16] This direct election is likely to happen when only two lists of candidates are running and, empirically, a substantial number of councilmen are indeed elected in the first round: almost 90 percent of municipalities in the sample had no second round at all, either in 2001 or 2008. We therefore focus here on voter turnout in the first round.

The three most recent first rounds of municipal elections took place on Sunday, March 11th, 2001, on Sunday, March 9th, 2008, and on Sunday, March 23rd, 2014.[17] The most recent election before 2001 took place in 1995. Our fiscal data span the years 1998 to 2013, and we have electoral data for 2001 and 2008 only, so that, unless otherwise specified, our estimations cover the two terms 2001 to 2007 and 2008 to 2013.[18]

2.2 Measures of cost of policy

Based on the institutional features of the environment we study, we propose two novel measures of the cost of public policy on municipal residents. We argue that both measures reflect the quality of the elected representatives. The first measure reflects the capacity of the candidate to obtain subsidies from higher levels of government. The second measure reflects the ability to make sound borrowing decisions.

2.2.1 Net discretionary transfers

Municipal revenues are composed of transfers received by the municipality from other governments, local taxes and other sources of revenues.

[14]In 90 percent municipalities, no more than two lists would run in an election

[15]More than half municipalities have 2 lists or more represented in the municipal council.

[16]This rule applies only if the number of voters in this first round is larger than a quorum of 25 percent of registered individuals.

[17]These are the days of the first rounds; the second rounds took place a week after.

[18]The incumbent council can make fiscal decisions in January, including the January of the election year. However, these decisions can be canceled by the incoming council after the election in March.

Transfers to a municipality from other levels of government come in two forms: automatic and discretionary subsidies. Automatic subsidies comprise subsidies granted by the national government according to a formula set in the national law that integrates observable municipal characteristics and reimbursement of value-added tax paid by the municipality for its infrastructure. Discretionary subsidies are designed to finance infrastructure expenses. They are usually granted by local governments, at a larger administrative level than a municipality. In most cases, to obtain a subsidy, a municipality's council is required to make a detailed application, describing the project and the financing plan. The application is then reviewed by the authority of the upper level of government or administrative unit that may grant the subsidy.

Municipalities also transfer funds to local governments. These transfers are effectively a form of tax paid by a municipality to the local governments or to any other institutional body a municipality may belong to,[19] which finances part of the subsidies these local governments grant. There is no single formula to determine the amount of transfers a municipality is going to pay to a specific body. In fact, these rules may be fixed by the national government, by local governments, or by the members of that group. Although the amount of transfers a municipality will pay another governmental body usually depends on municipal characteristics, there are variations that depend on the ability of the municipality to bargain over these transfers.

We define net discretionary transfers as the difference between discretionary subsidies to a municipality and transfers from this municipality to other governments. We use as our first indicator of the candidate's quality the amount of net discretionary transfers divided by the total amount of infrastructure expenses. This dimension seems to fit unambiguously the definition of quality we proposed above: if more targeted subsidies can be obtained, this will reduce the cost of funding, which appears unambiguously good for all inhabitants of the municipality. The attribution of these subsidies reflects the candidate's quality in terms of quality of the project, knowledge of the system (knowing that these subsidies exist) and knowledge of how to prepare these applications.

2.2.2 Toxic loans

Our second measure of a mayor's quality is whether he contracted a so-called "toxic loan". Since the end of the 1990s, many French municipalities

[19] Some municipalities may decide to form groups that can pool part of their budget.

have contracted structured loans with a fixed low rate for a number of years followed by a variable rate.[20] All municipalities with similar characteristics had access to the same type of loans: indeed this was a period of fierce competition in that banking sector, since from the mid-1990s municipalities had started decreasing their debt levels (after a period of soaring debt).[21] These loans have become extremely costly to borrowers over time.[22]

In 2011, given the growing crisis, a parliamentary group was formed to examine in detail the origins of the toxic debt crisis hitting French municipalities (as well as other public institutions such as hospitals). They reached a number of conclusions. First, the mayor was often deciding alone on what type of loan to contract.[23] Second, the loans were contracted because of poor decision-making and information by the mayors and/or deceiving behavior by the banks. The report in fact explicitly claims that in many cases, contracting these loans was not a deliberate choice made to maximize gains.[24] They claim that the decisions were often made with little understanding of these products.[25] The bank called Dexia, the main provider of these loans to municipalities, is involved in a number of lawsuits based on the claim that it failed in its duty of providing appropriate information.[26] In some cases the banks are accused of giving deceiving names

[20]The article in the *Guardian*, 6th September 2011, takes the example of the city of Saint-Etienne which had as much as 70 percent of its 370m euro debt in structured loans

[21]As indicated in Cour des Comptes 2011 (p.28), competition became more fierce:"in such a small market, competition between banks had become fierce". They add: "it is in such a context of abondance of liquidity available for the municipalities... that started a fierce commercial war between the banks" (p.61).

[22]We have no reliable data on the financial details of these loans that would inform the formula used to compute interests, the exact amount already paid etc . However, we computed that, controlling for observable characteristics, municipalities that took such loans at some point between 2001 and 2011 saw their annuities increase by 30 percent more than the other municipalities on average. This increase is an average on annuities from all loans, not only the toxic loans.

[23]See Cour des Comptes 2011 (p.52). The report of the commission states that "given the technical constraints and the urgency that characterizes the contracting of a loan, the assemblies often delegated the decision to the local official". In other words, even though the municipal council had the right to decide on the type of loan they most often delegated that power to the mayor.

[24]"in a number of cases, the municipalities did not make a deliberate choice to contract these risky products in order to maximize their gains" (p.49).

[25]The commission cites an official in Saint Etienne, admitting naive and uninformed decisions "We were dragged into taking important risk by making us believe (the banks) that we were escaping risk. We might have been naive, I am ready to admit" (p.51)

[26]In 2013, Dexia was involved in 219 lawsuits with municipalities, most still underway and several lost by Dexia

to these products.[27] In addition, most of these loans did not include a cap on the interest rate, which is usually included in these kinds of contracts.

The probability to have a toxic loan thus seems to be a reasonable indicator of the quality of municipal councilmen, because this probability reflects their capacity to fund municipal investment at the lowest financial cost.

2.3 Data

Table 1 contains the list and descriptive statistics of all variables used in this study. We have data on turnout and fiscal variables for around 35,000 municipalities, social and demographic data for around 30,000 municipalities, and data on electoral outcomes for smaller samples of municipalities.

Data on municipal elections. We obtained data on municipal elections from the French Ministry of Home Affairs, which has kept records on every election since 2001 only. These data contain information on the number of registered individuals and electoral participation for the 2001 and 2008 municipal elections. We define turnout as the number of individuals who vote in the first round of an election.

The data also contain information on the party affiliation of elected councilmen for some municipalities in 2001 and 2008, as well as information on the age and on professional activity of municipal councilmen (including mayors) for a subsample of municipalities in 2008. We observe the professional activity of roughly 14,000 mayors in each election. We define an indicator variable equal to 1 if and only if the professional activity of the mayor requires a Master's degree or its equivalent.[28]. In addition, we obtained the names of incumbent mayors from an online unofficial database to define the indicator variable "incumbent wins", equal to 1 if the incumbent mayor is again the mayor after the election, and 0 otherwise.[29]

Data on municipal finances. Data on municipal finances were provided by the French Ministry of Finances and the French Ministry of the Interior for each year of the period 1998-2013.[30] For every year of that

[27]For instance, one of the main type of loans offered by the main bank was called "Tofix" which sounds like "Fixed Rate" in French

[28]A substantial number of mayors are civil servants, who are required to have certain degrees depending on their activity and status (although there are exceptions). Professions that require a Master's degree are: medical doctors, pharmacists, dentists, veterinarians, lawyers, accountants, judges, teachers, professors, engineers, architects, journalists, and senior executives.

[29]The source is the website FranceGenWeb: http://www.francegenweb.org/mairesgenweb/

[30]Some data are available online: http://www.colloc.bercy.gouv.fr/. Others were pro-

period, and for almost every municipality, we observe the amount raised through each of the following sources of municipal revenues: transfers received by the municipality from other governments or subsidies, local taxes and other sources of revenues. We also observe the amount spent in each of the following expense items, for most municipalities: transfers from the municipality to other governments, infrastructure expenses, and other expenses. By definition, the sum of revenues from all possible sources is equal to the sum of expenditures. We refer to this sum as the total budget of a municipality.[31]

In addition to data from the French Ministry of Finances, we use data on "toxic loans" that were made public by the French newspaper *Liberation* in 2011.[32] We construct the probability to hold a "toxic loan" as 1 if the municipality was included in the list published by the newspaper. For each loan, we know the starting year and the ending year of the loan. The data covers municipalities that contracted a toxic loan before 2011.[33]

Data on municipal characteristics

Data on municipal social, economic and geographic characteristics are available from the *Institut National des Statistiques et Etudes Economiques*.[34] They are based mainly on the last two censuses, which took place in 1999 and 2007. We observe the following information about a municipality: total taxable income of the municipal population, median income, total population and adult population (15 and older - we do not observe people 18 or older), senior population (60 and older), labor force participation and unemployment. These data are not available for around 5000 municipalities, mainly the smallest ones due to confidentiality restrictions.

Data on infections incidence

We use data on the incidence of digestive infections from the *Reseau Sentinelles* (roughly equivalent to the Center for Disease Control), an organization that gathers data from general practitioners in France.[35] The data contain estimations of average weekly incidence in a month, for the

vided to us directly by the Direction Generale des Collectivites Locales, which is part of the French Ministry of the Interior.

[31]Fiscal outcomes for 2001 and 2008 result from decisions taken over all these years. A newly elected municipal council will usually meet in the weeks that follow an election and cancel or confirm fiscal decisions made between January and March.

[32]http://s0.libe.com/fremen/maps/carte-emprunts-toxiques/. This list does not give information on the exact amount paid or to be paid each year.

[33]Official warnings against this type of loans were issued after 2010. It is unlikely that any municipality took any such loan after that date.

[34]Available online at http://www.insee.fr/fr/bases-de-donnees/. There is no consistent data on average municipal income over the period.

[35]http://websenti.b3e.jussieu.fr/sentiweb/.

months of February, March or April that is the average number of new patients per 100,000 persons and per week, who visited a general practitioner with symptoms of digestive infection, for around 700 evenly geographically distributed locations of observation.[36]

Digestive infections last for a few days, so that most people who fall sick in the week before the election (sick enough to visit a doctor) could be still sick or recovering on the day of the election.

In three ways, the measure of infections incidence we use is a noisy measure of the actual number of people in a municipality with symptoms on election days, which would be the "ideal" variable. First, we do not observe the number of individuals sick at the municipal level, but only for around 700 observation locations. We define the incidence in a municipality as the incidence at the closest observation point. Second, we only observe the average weekly incidence in any given month; incidence varies across the weeks of a month. Third, some sick people may not visit a doctor, leading to an under-declaration of the number of individuals feeling the effect of a disease. Due to the limited number of visits a doctor can take in a day, the under-estimation of the actual number of sick people is more likely when incidence is large. Sick individuals may recover before they could actually visit a doctor (a seasonal infection typically lasts a few days), and therefore not be reported. We discuss the possible effect of these measurement errors when we present the results.

3 Specification

For a municipality i and a year t, our main specification is:

$$Y_{i,t} = \alpha_i + \delta_t + \beta I_{i,e(t)} + \gamma X_{i,t} + \epsilon_{i,t} \tag{1}$$

where $e(t)$ is the last election year that took place before year t and $I_{i,e(t)}$ is the log of of infections incidence at the time of the last election $e(t)$. The terms α_i and δ_t stand for municipal fixed effects and year indicator variables. $X_{i,t}$ is a vector of control variables.

The sample of observations depends on the nature of the outcome Y. If the outcome is observed only at the time of the election, e.g. Y is voter turnout in the election, t takes only two values, 2001 and 2008. If the outcome is observed in every year between elections, e.g. Y is a fiscal outcome, t takes all the values between 2001 and 2013, unless otherwise

[36]We are grateful to Clement Turbelin for his help in obtaining the data, and for his generous advice. Data are provided by a network of medical doctors.

specified.

The fact that infections incidence is not observed at the municipal level may create a correlation in standard errors of all municipalities sharing a point of observation of infections incidence. Unless otherwise specified, we thus estimate standard errors clustered at the level of observation of infections incidence.

Our main identification assumption is that infections incidence is not correlated with factors that explain voter turnout, electoral outcomes, or municipal policy independent of their effect on voting costs. We discuss this assumption as we present each estimation in the next section.

4 Empirical results

4.1 Effect of infections incidence on voter turnout

Table 2 reports the effect of the log of average weekly infections incidence in the month of March on the log of voter turnout in the first round of the municipal election. There is one observation by municipality and by year for each of the two election years 2001 and 2008. Standard errors are clustered at the level of observation of infections incidence.

All estimations include municipal fixed effects, since municipality-specific factors that are constant over time may be correlated with voter turnout and infections incidence. All estimations include year indicator variables as well.

Columns 2 to 5 also include indicator variables for the number of councilmen, as well as the logs of total income of the municipal population, median income of the municipal population, size of labor force in the municipal population, number of unemployed individuals, total population, adult population, and senior population. All these variables control for the economic and demographic characteristics of a municipality, which could explain both infections incidence and voter behavior.

In addition, we include the number of registered voters as control variables. This number is known several months before an election and could be correlated with municipal characteristics, voter turnout and candidates' platforms since candidates might strategically respond to it. We also include as a control variable the log of debt in the election year (i.e. at the beginning of a term) since it can affect future borrowing and expenditure decisions, including the type of loan to contract. The difference in the number of observations between the first column and the other ones is due to a lack of information on voter turnout or municipal characteristics, primarily in small municipalities. We refer to these variables as the full set of control variables.

Column 2 shows that an increase in the number of sick individuals decreases voter turnout; being sick makes voting more costly. An increase of one percent in incidence decreases voter turnout by 0.003 percent once we control for observable municipal characteristics.

Two sources of endogeneity could bias these estimations. First, turnout could be affected by some omitted variable correlated with the vulnerability of the municipal population to infections in general. If this were the case, turnout would also be correlated with infections incidence in other months, not specifically in March. To check this possibility, columns 3 to 5 show the same estimations as before, with the log of average weekly infections incidence in February and/or the log of infections incidence in April added to the vector of control variables. Neither infections incidence in February, which is comparable in size to infections incidence in March as shown in Table 1, nor infections incidence in April has a significant impact on voter turnout. Including these two incidences has no effect on the value or the significance of the coefficient of infections incidence in March. Unless some variable affects voter turnout and infections incidence in March specifically, our estimations thus reflect the causal effect of infections on voter turnout.

A simulation based on the estimations indicates that the number of voters decreases by 2.9 persons (with a 95% confidence interval $[2.50, 6.59]$) on average for one extra person who is sick. The fact that this number is larger than 1 could stem from the fact that sick people may not visit their doctor. A recent estimation for several developed countries (Scallan et al. 2005) shows that 20 percent of people sick with the same seasonal digestive infections we consider here visit a doctor. Applied to the previous estimation, this percentage suggests in fact that many people who have symptoms do vote on election days.

In general, measurement error may affect the precision of the estimation of the effect of the infection incidence at the time of the election on voter turnout. This is a limitation of our approach, but it does not in itself question the validity of a causal interpretation of this coefficient.

The causal interpretation would be invalidated if measurement errors for the number of sick people on the day of the election depended on omitted factors that could also explain voter turnout. For instance, if a municipality becomes poorer in ways that are not well captured by the control variables already included, its population may be less likely to visit a medical doctor when sick, which would create an error in the measure of infections incidence, and also be less likely to vote. In this example, however, as for any source of error in the measurement of infections incidence, the incidence in February and April would suffer from the same problem, and should also have a significant impact on turnout. Columns 3 to 5 again show that this is not the case. There is thus no evidence that any source of measurement error that would affect infections incidence in general, and not in March only, has an effect of voter turnout.

4.2 Effect of infections incidence on fiscal outcomes

Net discretionary transfers. Columns 1 to 3 in Table 3 report the estimation of the effect of the log of infections in March incidence on the ratio of net discretionary transfers between the municipality and other governments over infrastructure expenditures. There is one observation by municipality and by year for every year from 2001 to 2008. Standard errors are clustered at the level of observation of infections incidence. Both columns include the full set of municipal characteristics described in the previous estimations, measured at the time the elections.

Column 1 shows a positive and significant impact of infections incidence on the ratio of transfers over infrastructure expenditures, and Column 2 shows that this effect remains if we control for the log of infections incidence in February or April. In addition, there is no significant impact of these two control variables on transfers.

Column 3 shows that an increase in infections incidence increases the ratio even if we control for infrastructure expenses and the log of infrastructure expenses. By law, the type of subsidies that we included in the measure of net transfers must finance infrastructure expenditures almost exclusively. The results thus show that, when the cost of voting is higher at the time of the election, the elected government will be better at obtaining inter-governmental subsidies for a given level of infrastructure expenditures. This is our first evidence of an effect of changes in voting costs on the quality of the elected official.

Toxic loans. In Columns 4 and 5, we estimate the effect of infections incidence on an indicator variable equal to 1 if and only if the municipality i has a "toxic loan" in year t. All estimations are run for the sample of observations between 2001 and 2011 (we have no data on toxic loans after 2011). We find that average weekly incidence in March decreases the probability to have a toxic loan. This last result indicates that municipalities that face a higher voting cost in an election are less likely to suffer from poor financial decisions in the following term.

We also find that municipalities that had a larger debt in the election year were more likely to take such loans. This result may reflect the fact that municipalities with larger debt were more credit-constrained, and had to resort to loans with bigger interest rates on average. Including this variable and other municipal characteristics has no substantial effect on the coefficient of infections incidence.

Both sets of results show that an increase in voting cost leads to the election of higher-quality politicians making decisions more beneficial for the municipality's finances. These effects do not result from a general impact of infections on these outcomes, since they are not significantly impacted by infections incidence in the February and April of the election years.

4.3 Effect of infections incidence on infrastructure expenses and other fiscal outcomes

Columns 6 to 7 in Table 3 report the effect of the log of infections incidence in March on the log of municipal infrastructure expenses. As in columns 1 to 3, there is one observation by municipality and by year for every year from 2001 to 2013. All standard errors are clustered at the level of observation of infections incidence. All estimations include the full set of control variables. For each outcome, we report both the estimation of the main regression, and the estimation of the main regression with the log of infections incidence in February and in April as control variables.

Column 6 shows a positive and significant impact of infections incidence on infrastructure expenses. Infrastructure is, by law, the only public good a municipality can invest in, and the only investment that can be discretionarily subsidized. The estimation is therefore consistent with the model: a shock on voting cost favors candidates who are better at getting funds and invest more in public goods. Column 7 shows that incidence in February and April have no significant impact on infrastructure expenses.

We also ran estimations (not reported here) of the impact of the log of infections incidence in March on the log of other expenses, on the log of taxes, on the log of other revenues, and the log of the total budget of a municipality. None of these estimations shows any significant impact of infections incidence in March – the election month – in February or April, on these dependent variables.

4.4 Effect of infections incidence on electoral outcomes

This section explores the effect of infections incidence on electoral outcomes. Due to the paucity of data on elected representatives, estimations are run on a smaller number of observations than before, and focus, when possible, on characteristics of the mayor (and not the other councilmen), who is, in most cases, also the leader of the group of candidates who won the largest number of seats in the municipal council.

In Table 4, Columns 1 and 2, we estimate the effect of voting costs on an indicator equal to 1 if and only if the elected mayor's professional activity requires at least a Master's degree or its equivalent. We find that infections incidence increases the probability that the elected mayor has such a degree. In Columns 3 and 4, we find that infections incidence increases the age of the elected mayor, though this effect is significant at 90 percent only. In additional estimations (Columns 5 to 8) we estimate no significant impact of infections on the incumbent mayor is reelected (both in the council and as mayor) and on the probability that the new mayor is affiliated with the left. These estimations suggest that variations in voting costs may favor certain types of candidates, but types that are not incumbency status or political affiliation.

4.5 Effect of voter turnout on fiscal policy

The objective of the previous estimations was to establish the qualitative effect of an increase in the cost of voting on certain outcomes of municipal policy, using infections incidence as a source of such a cost. Since an increase in the cost of voting decreases voter turnout, infections incidence can also be used as an instrumental variable to estimate the impact of voter turnout on public policy. In Table 5, we estimate the effect of voter turnout on the three main fiscal outcomes we consider: net discretionary transfers, probability to have a toxic loan, and log of infrastructure expenses. The first column shows the first stage of the estimation of infections incidence on turnout in the 2SLS estimation of voter turnout on net discretionary transfers (this first stage is similar to the first stage of the other 2SLS estimations, and similar to the results in Table 2). The other columns show, for each outcome, first the standard fixed effects estimation, then the 2SLS fixed effects estimation of the regression of policy outcomes on voter turnout. In addition, we include at the bottom of the first column the F-statistics of the significance of the instrument in the first stage. Since it is quite low, we report the p-value of the Anderson-Rubin test of the null hypothesis for the log of voter turnout in the first round. All estimations show a significant impact of voter turnout on the fiscal outcomes.

The table shows substantial differences between 2SLS and standard fixed effect estimations. In general, one would expect that an election with large stakes, i.e. with big and costly projects to be implemented by the elected administration, to draw more voters. This mechanism would induce a positive correlation between voter turnout and municipal expenses after the election. This correlation could explain the difference between OLS and 2SLS estimations of the regression of the log of infrastructure expenses on log voter turnout: in the 2SLS estimation, the coefficient of the log of voter turnout is much smaller than in the standard fixed effect estimation. Similarly, an electoral competition in which only higher-quality candidates run should attract more voters than a competition in which only lower-quality candidates run. In this case, the standard fixed effect estimation of the effect of voter turnout on any indicator or quality would show a larger increase in quality due to voter turnout than what a causal estimation would show, which is what we observe in the tables.

By construction, our measure of infections incidence is a noisy measure of the actual number of newly sick people in a municipality at a given time (see Section 2). This measurement error may partly explain why the log of infections incidence is not a strong instrumental variable, which in turn may bias the 2SLS estimations. As mentioned in Section 2, the actual incidence is more likely to be underestimated when it is high, so that a convex function of infections incidence may therefore be a better proxy for the log of the actual incidence. To check this point, we run the same estimations as before using the square of infections incidence as the instrumental variable for the log of voter turnout. The estimations show that the first-stage F-statistic – equal to 11 – is indeed larger when we used the log of infections as the instrumental variable. The impact of the log of voter turnout on the probability of holding toxic assets, the ratio of transfers and the log of infrastructure investment are around 2, -7 and -7 respectively, which is very close to what we estimated before.[37]

What can explain this large impact? We note that municipalities are extremely heterogenous in terms of population and budget, and the elasticity of voter turnout likely differs across them. For a small municipality with infrastructure expenses close to zero, a positive transfer or an investment in infrastructure may amount to a massive increase in percentage of infrastructure expenses, even if it amounts to a small absolute change in

[37]We report the results in Appendix B.

terms of euros. For instance, almost 10 percent of municipalities have fewer than 100 residents. In addition, the number of voters necessary to change the outcome of an election in a small municipality is usually very small. In addition, anecdotal evidence suggests that candidates in small municipalities may vary a lot in terms of observable characteristics, and thus in terms of competence as well. To understand better the impact of small municipalities on the elasticity, we run the same 2SLS estimations as before, but each municipality is now weighted by its number of inhabitants. We report the results for the square of infections only, since it is a stronger IV than the log of infections.[38] The results, reported in Table 6, show a much smaller effect of voter turnout on transfers or infrastructure expenses. The impact of turnout on the probability to have a toxic asset is also smaller than before, but not significantly different from zero.[39] These results are consistent with the facts that small municipalities are more responsive to changes in voter turnout than bigger ones, that political changes may yield large relative changes in these municipalities, and that these changes may amount to small absolute changes. The estimations of Tables 5 and 6 imply that the elasticity of municipal investment in infrastructure to voter turnout is approximately -7 on average across French municipalities, and -2.5 on average across French people.

4.6 Falsification tests

We conduct two sets of falsification tests. The first set of tests estimates the impact of the log of infections incidence (respectively the squared infections incidence) in a given election year on the main fiscal outcomes of interest in the years of the term *before* that election year (the term before 2001 started in 1995, but we only have data from 1998 and later), with non-weighted observations (resp. with observations weighted by the size of the population in a municipality). The results, reported in Table 7, show that the log of infections (resp. the squared infections incidence) incidence is never significant at the 5 percent level. Infections incidence in March is thus not correlated with past changes in fiscal outcomes from one term to the next. The second set of tests controls for the log of infections incidence (respectively the squared infections incidence) in a year *before* an election

[38]The 2SLS results using the log of infections as an instrumental variable provide slightly bigger estimates, but the F statistics is much lower and the standard errors are large.

[39]Larger municipalities may be more likely to be governed by councilmen who are aware of the risks involved with toxic assets.

year, namely one year and two years before the election, on the main fiscal outcomes of interest in the years of the term starting in that election year, with non-weighted observations (resp. with observations weighted by the size of the population in a municipality). The results, unreported here, again show no significant impact of that additional control variable.

5 Interpretation

The empirical results show that the marginal voter is in favor of the candidate who will decrease infrastructure expense. Infections might affect different groups of voters differently. It is natural to think for instance that older voters might be more affected than the rest of the population. If that were the case, infections would affect the composition of the electorate in a way that might favor certain types of spending. Infections could for instance have a smaller impact on eligible voters who favor infrastructure investment, either because people who favor smaller expenses are more likely to get sick, or because sickness disproportionately discourages people who favor smaller expenses to vote.

We cannot test this directly since we do not observe eligible voters' preferences and there is no data on turnout by socioeconomic groups at the municipal level. Instead, we examine this question indirectly by estimating whether the impact of infections incidence is different in municipalities with a richer/older population than the rest of the sample. To do so, we define an indicator equal to 1 if and only if a municipality's median income is among the 50 percent of municipalities with the larger median income, and an indicator equal to 1 if and only if a municipality's ratio of inhabitants above 60 is among the 50 percent of municipalities with the larger such ratio. We then run the main estimations, adding the interaction of either indicator with the log of infections incidence in the control variables. By definition, the coefficient of this interaction is the difference between the effect of infections incidence in the municipalities for which the value of the indicator is 1 and the other municipalities. The results, reported in the first two columns of Table 8, show no significant different effect for richer/older municipalities.

Moreover, we focused on outcomes likely to be favored by the entire population, unlike total spending or tax levels, for instance. Every voter should, everything else equal, wish to elect candidates who are more able to obtain subsidies for a given level of infrastructure expenses, and less likely to make poor borrowing decisions, which will eventually be paid by constituents' taxes.

We control for most observable dimensions of municipal decision-making, but we cannot exclude that there could be unobservable consequential dimensions. One could be that the politician who is spending time studying loan conditions is not spending it on choosing the type of infrastructure most needed by his constituents. But official reports suggest that the mayors contracting toxic loans did so because of lack of financial literacy and not because of lack of effort.

Based on these additional results, we ask the following question: under what conditions would our empirical results hold if we suppose there is no correlation between the cost of voting and political preferences? We examine in the following section a general model that addresses this question.

5.1 Model

The extensive theoretical literature on voter turnout can be broadly separated into two strands. The first strand assumes that the probability to be pivotal and to influence the outcome of an election explains the decision to vote or not (Palfrey and Rosenthal 1983, Feddersen and Pesendorfer 1996, and the survey by Feddersen 2004). The second strand studies non-instrumental motives, or benefits derived from voting independent of affecting electoral outcomes (e.g. Braconnuer, Dormagen and Pons 2014).[40] There is still no consensus on which approach to adopt. The theories based on instrumental motives suffer from the paradox of voting in large elections where the chance of being pivotal is null. At the same time, there is evidence that closeness of the election impacts turnout, consistent with the idea that some notion of pivotality might play a role (Blais 2000). We do not directly address this debate here, but given that we do consider relatively large electorates and that the focus is not on the model of turnout itself, we choose to align ourselves with the second strand. We suppose that the instrumental benefits of voting are measured by the difference in utility the voter obtains if one candidate is elected rather than another. An alternative is to assume that the instrumental benefit is either independent of the candidates (benefit of doing civic duty) or is given by a random taste parameter, as in Aldashev 2015.

The model is an extension of the model of voting proposed in Persson and Tabellini 2002. Two new elements are introduced. First, there is a source of possible heterogeneity between candidates, namely their quality.

[40] A third strand studies group-based voting behaviors (see survey by Feddersen 2004.)

Second, there is a random cost of voting that can lead to variations in voter turnout.[41]

We consider an environment with a continuum of potential voters and two candidates $\{A, B\}$ who make binding campaign promises on a level of public good in order to maximize their vote share. We cannot include all the subtleties of the French electoral system, but these assumptions seem consistent with the facts that in most elections, no more than two lists, each headed by a main leader, are running, and they obtain seats in the municipal council that are partly a function of their vote shares (see Section 2.1 and Appendix A).

The utility of voter i if candidate j wins (not including voting costs) is given by $W_i(j) = H(g_j) - \alpha_j g_j + \delta_i \mathbf{1}_{j=B}$, where δ_i is an individual preference shock drawn from a distribution f. We assume that f is continuous and its support is $\mathbb{R}$. g_j denotes the level of public good provided by candidate j in power and H is a concave and strictly increasing function. $\alpha_j g_j$ represents voter i's contribution to the cost of the public good through taxes. We assume that all voters contribute equally to the public good.

The candidates may differ in their quality, defined as their ability to provide the public good at the lowest cost possible. Specifically, we assume without loss of generality that $\alpha_A \leq \alpha_B$ (A is the higher- quality candidate). This is the counterpart to the empirical section where quality was measured by the level of subsidies candidates manage to obtain or the type of loans they contract. We assume that everyone observes the respective qualities. We could introduce a more general case where an exogenously given proportion of the population is informed, but this would not affect our results.

When comparing candidate A and candidate B, voters thus have a common view based on the qualities of the candidates $(H(g_A) - \alpha_A g_A - (H(g_B) - \alpha_B g_B))$ and a private perception based on ideology $(\delta_i \mathbf{1}_{j=B})$. The relative importance of these two dimensions will determine the respective vote shares. The only restriction we impose on the distribution f of the ideology is that the median δ_i is smaller or equal to zero (i.e. $F(0) \geq 1/2$), which implies that the population is not biased against candidate A.[42]

We assume that an individual votes if and only if the difference in utilities she derives from candidates A and B's elections is larger than

[41] As explained in Section 2, the electoral system is a mix of proportional and winner-take-all. Furthermore, some budgetary decisions need a supermajority to be approved, so the vote share will affect the quality of the final decisions.

[42] Our results would still be valid if the population were moderately biased in favor of B.

some uniform voting cost, denoted by c. Thus, voter i will turn out and vote for candidate A if and only if $W_i(A) - W_i(B) \geq c$. The timing of the model is as follows:

1. The candidates make campaign promises (g_A, g_B).
2. The draw of the preference shocks δ_i and the voting cost c is made and the voters make their voting decisions.
3. The elected candidate j implements her platform g_j.

We restrict attention to pure strategy equilibria.

5.2 Results

The optimal strategy for both candidates, regardless of the strategy of their opponent, is to promise a level of spending such that the marginal cost of spending equals the marginal returns:

$$H'(g_j) = \alpha_j$$

We denote the solution g_j^*. Due to the concavity of H, in equilibrium *candidate A will thus propose a higher level of spending than candidate B.*

Given our model of turnout, voter i, for a given voting cost c, will turn out to vote for A if and only if:

$$H(g_A) - \alpha_A g_A - H(g_B) + \alpha_B g_B - \delta_i \geq c$$

Conversely, voter i, for a given voting cost c, will turn out to vote for B if and only if:

$$H(g_B) - \alpha_B g_B + \delta_i - H(g_A) + \alpha_A g_A \geq c$$

so the respective numbers of votes for A and B are $F(\overline{\delta} - c)$ and $1 - F(\overline{\delta} + c)$, where

$$\overline{\delta} = H(g_A) - \alpha_A g_A - (H(g_B) - \alpha_B g_B) > 0$$

$\overline{\delta}$ represents the common perception shared by all voters on the difference *in equilibrium* between candidates A and B. By definition, it is the difference in utilities from candidates A and B for an individual who has no other gain from the election of A or B. This common value is tilted in favour of the higher-quality candidate, i.e $\overline{\delta}$ is positive. Furthermore, naturally, $\overline{\delta}$ is decreasing in α_A and increasing in α_B.

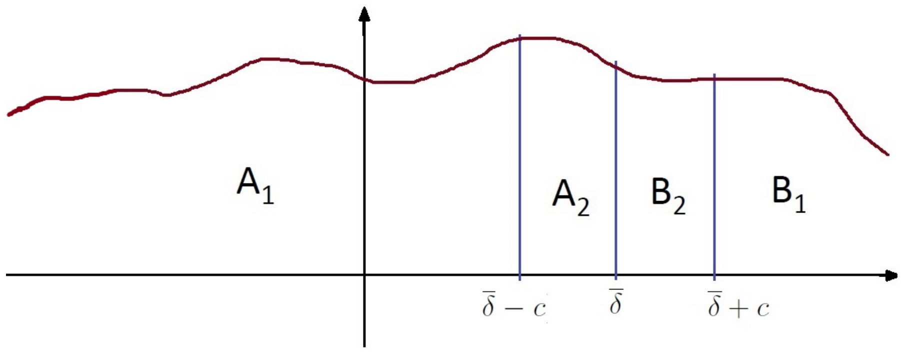

Figure 1: Increase in voting costs from 0 to c

We represent in Figure 1 the variations in votes as c varies for a distribution centered in 0.[43] If the voting cost is zero, candidate B gets $B_1 + B_2$ votes while candidate A gets $A_1 + A_2$. As voting costs jump to c, candidate A loses A_2 votes and B loses B_2. This implies that how the vote share of A varies with c critically depends on the respective size of A_2 and B_2.

There is however one systematic effect that drives our results. If $A_2 = B_2$, A's vote share strictly increases with c. Indeed the loss A_2 relative to the smaller baseline $B_1 + A_2$ is much larger for candidate B than for A who loses A_2 relative to a large baseline $A_1 + A_2$.

There are a number of factors that make A_2 and B_2 comparable and thus guarantee that A's vote share is increasing in c. We focus on one such condition that may be partly tested in the data. If the voting cost is close to zero, variations in c will lead to similar decrease in votes for candidates A and B and thus as explained above, will imply that the vote share of A increases in c.

Proposition 1. *In equilibrium the high quality candidate A spends more on the public good than B. Candidate A's vote share is increasing in c if and only if condition (2) is satisfied*

$$F(\overline{\delta} - c) \times f(\overline{\delta} + c) - \left(1 - F(\overline{\delta} + c)\right) \times f(\overline{\delta} - c) > 0 \qquad (2)$$

[43]To simplify the picture, we represent the situation in which the voting cost goes from 0 to c. The discussion would be the same if it went to some positive cost to some larger cost.

Furthermore, there exists $\bar{c}$ such that if $c \leq \bar{c}$, condition (2) is satisfied.[44]

5.3 Discussion and Additional Estimations

Empirically, the main prediction of the model implies that when the cost of voting is small enough, which is equivalent to saying that the base level of voter turnout is large enough, a marginal decrease in voter turnout due to a marginal increase in voting cost will be beneficial to the candidate with the bigger budget and the lower cost per unit of public good. In our sample, around 65 percent of the total eligible population voted in the 2001 and 2008 French municipal elections.[45] This large number and the main estimations are consistent with this prediction.

Everything else equal, the condition "the voter turnout is large enough" is more likely to hold in municipalities with a larger level of voter turnout with respect to other municipalities. The ideal test to check further the validity of our interpretation would thus be to estimate the specific effect of infections incidence in the former municipalities, in comparison with the effect in the latter municipalities. We obviously cannot use voter turnout in municipal elections for that purpose since it is a dependent variable of interest and is endogenously related to municipal policy outcomes. Instead, we use voter turnout at the municipal level in an election different from the municipal elections, namely the presidential elections of 2007, as a proxy for voter turnout in municipal elections. Except maybe in the largest cities, such as Paris, the stakes of presidential elections are unrelated to municipal

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[44]Proof. Candidate A gets votes $F(\bar{\delta} - c)$ while B gets $1 - F(\bar{\delta} + c)$, so that candidate A's a vote share is given by:

$$\frac{F(\bar{\delta} - c)}{F(\bar{\delta} - c) + 1 - F(\bar{\delta} + c)}$$

The denominator is always strictly positive, so A's share of votes is increasing with c if and only if:

$$f(\bar{\delta} + c)F(\bar{\delta} - c) - f(\bar{\delta} - c)\Big(1 - F(\bar{\delta} + c)\Big) > 0 \tag{3}$$

The term on the left is continuous in c. It is equal to $f(\bar{\delta})\Big(2F(\bar{\delta}) - 1\Big)$ if $c = 0$, which is strictly positive since $\bar{\delta} > 0$ and $F(0) \geq 1/2$ by assumption. Therefore there exists $\bar{c}$ such that if $c \leq \bar{c}$, the vote share of A is increasing in c.

[45]Voter turnout is larger in smaller municipalities. On average across municipalities, around 80 percent of the eligible population voted in the 2001 and 2008 French municipal elections

elections. However, the main factors that affect the cost of voting in a municipality, such as the number of voting stations, the sense of civic duty of its residents, etc., are likely to be constant over time. Indeed, we find (in estimations unreported here) that the correlation in voter turnout between municipal elections in any year and presidential elections is close to 1.[46]

We define an indicator equal to 1 if and only if a municipality is among the 50 percent of municipalities with the larger turnout in the 2007 presidential elections, and estimate the impact of the interaction of that indicator and the log of infections incidence, on the main dependent variables of interest. In Columns 4 to 6 of Table 8, we show a difference in the effect of infections incidence in municipalities with a larger base level of turnout on the fiscal policy variables. These results are consistent with the prediction of the model: the impact of the log of infections on fiscal policy outcomes is systematically stronger in municipalities with a larger base level of voter turnout. Finally, we show that infections incidence has no significantly different impact on voter turnout in municipalities with a large base level of voter turnout (Column 3 of Table 8). The general impact of infections incidence on voter turnout - which is the same in both high and low turnout municipalities - thus cannot explain its different impact on policy outcomes. It suggests that it is the relative effect of the infections incidence on marginal voters for each candidate that may be different in high-turnout and low-turnout municipalities, which is consistent with the prediction of the model.

6 Conclusion

Whereas the literature has mainly examined the effect on policy of changes in turnout among specific segments of the population, we focus on a shock on voting cost that does not affect any specific group. We show that such a shock can affect the quality of elected representatives defined as their capacity to access good sources of funding.

We note that, unlike in more recent studies (e.g. Hodler, Luechinger and Stutzer 2015 or Aldashev 2015), these results cannot be explained by voters or candidates' anticipations regarding the cost of voting, since

[46]By definition in the model, the voting cost refers to the total voting cost. Empirically, we interpret it as the sum of all losses from any possible source of cost (independent of the gain obtained from electoral outcomes). The voting cost may thus be the cumulative effect of one's general health, one's distance to the vote station, one's value of time spent doing something else, etc. Most of these costs are likely to change little over the few years between either municipal elections and 2007.

the shocks we consider cannot be anticipated. Instead, we observe that any potential voter can derive a gain or a loss from the implementation of any policy. But all voters gain from having policies implemented at the lowest cost possible. Everything else equal, this gives an advantage to the higher-quality candidate. With a simple model, we show that, under certain conditions, this advantage will be stronger if the cost of voting marginally increases. In comparison with most other empirical studies, this paper also shows that exogenous variations in voter turnout may affect policies in systematic ways without being obviously related to a specific demographic/economic group of eligible voters.

7 Tables

Table 1: Summary statistics

Variable	Mean	Std. Dev.	N
Voter Turnout	712.47	2340.1	69841
Log Voter Turnout	5.65	1.17	69841
Incumbent wins	0.66	0.47	26258
Left wins	0.37	0.48	55966
Mayor has Master's Degree	0.34	0.47	39431
Average Weekly Infections Incidence in March 2001,2008	171.02	73.67	71351
Average Weekly Infections Incidence in February 2001,2008	182.15	79.3	71351
Average Weekly Infections Incidence in April 2001,2008	110.39	63.75	71351
Total Budget (/1000 euros of 2000)	2185.97	13188.88	571011
Taxes	1186.57	7428.69	571011
Transfers to Municipality - automatic	362.18	1970.66	571011
Transfers to Municipality - discretionary	91.60	435.88	571007
Other Revenues	545.62	3858.76	571011
Transfers from Municipality	112.72	1038.73	571011
Infrastructure Expenses	434.23	2236.53	570869
Other Expenses	836.17	5256.56	571011
Municipal Debt (/1000 euros of 2000)	1206.36	7740.93	571011
$\frac{\text{Net Discretionary Transfers}}{\text{Infrastructure Expenses}}$	0.19	1.43	562140
Has a "Toxic Loan"	0.02	0.13	499829
Population	1512.23	6840.43	69841
Total Income (/1000 euros of 2000)	13982.62	65862.58	69283
Median Income	23886.81	5917.95	60816
Labor Force	857.12	4035.22	69838
Unemployed Individuals	83.43	497.22	69838
Adult Population (15+)	1252.41	5801.71	69831
Senior Population (60+)	330.46	1426.52	69831
Registered Individuals	1057.9	4097.92	69841
Number of Councilmen	13.7	4.26	71351
Year	2004.49	3.5	71351

Table 2: **Infections incidence and voter turnout in the first round of municipal elections**

	Log Voter Turnout				
	(1)	(2)	(3)	(4)	(5)
Log Incidence in March	-0.014***	-0.004***	-0.003***	-0.003**	-0.003**
	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)
Log Incidence in February			-0.000		-0.000
			(0.002)		(0.002)
Log Incidence in April				-0.001	-0.001
				(0.001)	(0.001)
Log Total Income		0.012**	0.012**	0.012**	0.012**
		(0.005)	(0.005)	(0.005)	(0.005)
Log Median Income		0.010	0.010	0.010	0.010
		(0.008)	(0.008)	(0.008)	(0.008)
Log Labor Force		0.023*	0.024*	0.023*	0.024*
		(0.014)	(0.014)	(0.014)	(0.014)
Log Unemployed		-0.006***	-0.006***	-0.006***	-0.006***
		(0.001)	(0.001)	(0.001)	(0.001)
Log Registered		0.628***	0.628***	0.628***	0.628***
		(0.050)	(0.050)	(0.050)	(0.050)
Log Population		0.057***	0.057***	0.057***	0.057***
		(0.013)	(0.013)	(0.013)	(0.013)
Log Adult Pop.		0.083***	0.083***	0.083***	0.083***
		(0.012)	(0.012)	(0.012)	(0.012)
Log Senior Pop.		0.012***	0.012***	0.012***	0.012***
		(0.004)	(0.004)	(0.004)	(0.004)
Log Debt in Election Year		-0.001	-0.001	-0.001	-0.001
		(0.001)	(0.001)	(0.001)	(0.001)
# Municipalities	35760	29303	29303	29303	29303
# Clusters	726	726	726	726	726
# Obs.	69841	53271	53271	53271	53271
R2	0.169	0.556	0.556	0.556	0.556

NOTES. This table reports the fixed effect estimations of the regression of the log of voter turnout in the first round of municipal elections on the log of average weekly infections incidence in the month of the election (i.e. March 2001 and March 2008). There is one observation by municipality and by election year (2001 and 2008). In addition to the control variables reported on the left of the table, all regressions include indicator variables for year, and columns 2 and up include indicator variables for the number of councilmen. All regressions include municipal fixed effects. Infections incidence is observed at every point of a given set of locations of observations. We define the infection incidence in a municipality as the incidence in the closest location of observation. Standard errors are clustered at the location of observation level. The number of clusters is reported at the bottom of each column. $*$ Significant at the 10 percent level. $**$ Significant at the 5 percent level. $***$ Significant at the 1 percent level.

Table 3: **Infections incidence and fiscal outcomes**

	Transfers			Has Toxic Loan		Log Infrastructure Expenses	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log Incidence in March	0.026***	0.027***	0.026***	-0.008**	-0.010***	0.024***	0.023**
	(0.007)	(0.008)	(0.008)	(0.003)	(0.004)	(0.008)	(0.010)
Log Incidence in February		-0.016			0.001		-0.002
		(0.011)			(0.004)		(0.012)
Log Incidence in April		0.010			0.005		0.004
		(0.007)			(0.004)		(0.008)
# Municipalities	28868	28868	28868	28868	28868	28868	28868
# Clusters	726	726	726	726	726	726	726
# Obs.	337534	337534	337534	286030	286030	337534	337534
R2	0.000	0.000	0.002	0.067	0.068	0.013	0.013

NOTES. This table reports the fixed effect estimations of the regression of municipal fiscal outcomes in a year t between 2001 and 2013 on the log of average weekly infections incidence in the month of the last election before t (i.e. March 2001 or March 2008). The fiscal outcomes are: the ratio of net discretionary transfers over infrastructure expenses (Columns 1 and 2), the probability to hold a "toxic loan" in a year t (Columns 3 and 4), the log of infrastructure expenses (Columns 5 and 6). Net transfers are by definition the difference between non-automatic subsidies obtained by a municipality from other local governments and transfers paid by the municipality to other levels of governments. There is one observation by municipality and by year in the period 2001 to 2013 (to 2011 in Columns 3 and 4 - we do not observe toxic loans after 2011.). All regressions include municipal fixed effects and all control variables included in Column 2 of Table 2. Standard errors are clustered at the location of observation of incidence level. $*$ Significant at the 10 percent level. $**$ Significant at the 5 percent level. $***$ Significant at the 1 percent level.

Table 4: **Effect of infections incidence on electoral outcomes**

	Mayor has Master's		Mayor's Age		Incumbent Wins		Left Wins	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Incidence in March	0.005**	0.006**	0.107**	0.118*	-0.010	-0.006	0.002	0.003
	(0.002)	(0.002)	(0.052)	(0.064)	(0.013)	(0.016)	(0.006)	(0.007)
Log Incidence in February		-0.001		-0.064		-0.010		-0.002
		(0.003)		(0.071)		(0.016)		(0.007)
Log Incidence in April		-0.002		0.018		-0.002		-0.001
		(0.002)		(0.047)		(0.011)		(0.005)
# Municipalities	19627	19627	25051	25051	11620	11620	26891	26891
# Clusters	726	726	726	726	726	726	726	726
# Obs.	29709	29709	38303	38303	21470	21470	43369	43369
R2	0.003	0.003	0.749	0.749	0.014	0.014	0.007	0.007

NOTES. This table reports the fixed effect estimation of the regression of indicator variables describing the elected mayor on the log of average weekly infections incidence in the month of the election (i.e. March 2001 and March 2008). There is one observation by municipality and by election year (2001 and 2008). In addition to the control variables reported on the left of the table, all regressions include indicator variables for year and for the number of councilmen. All specifications include municipal fixed effects and all control variables included in Column 2 of Table 2. Infections incidence is observed at every point of a given set of locations of observations. Standard errors are clustered at the location of observation of incidence level. The number of clusters is reported at the bottom of each column. $*$ Significant at the 10 percent level. $**$ Significant at the 5 percent level. $***$ Significant at the 1 percent level.

Table 5: **Effect of voter turnout on the main fiscal outcomes**

	Log Voter Turnout	Transfers		Log Infra. Expenses		Has Toxic Loan	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log Incidence in March	-0.003***						
	(0.001)						
Log Voter Turnout		0.017	-7.368**	0.125**	-6.972**	0.068***	2.316**
		(0.057)	(3.256)	(0.050)	(3.284)	(0.019)	(1.118)
Method		FE	2SLS FE	FE	2SLS FE	FE	2SLS FE
F stat.	7.86						
AR p-value			.001		.004		.011
# Municipalities	28868	28868	28868	28868	28868	28868	28868
# Clusters	726	726	726	726	726	726	726
# Obs.	337534	337534	337534	337534	337534	337534	337534
R2	0.555	0.000	-0.070	0.013	-0.118	0.071	-0.656

NOTES. The first column reports the first stage of the 2SLS estimation of the regression of the log of voter turnout on net transfers. The other columns report the fixed effect estimations (Columns 2, 4, 6) and the 2SLS fixed effect (Columns 3, 5, 7) estimations of the regression of the main outcomes of interest on the log of voter turnout in the first round of the French municipal elections. The instrumental variable used for the 2SLS fixed effect estimations is the log of infections incidence in the month of an election. There is one observation by municipality and by year, for each year between 2001 and 2013. All specifications include municipal fixed effects and all control variables included in Column 2 of Table 2. F stat. is the F statistics of the significance of the instrument in the first stage. AR p-value is the probability that the Anderson-Rubin statistics is larger than the actual Anderson-Rubin statistics under the hypothesis that the coefficient of the Log of Turnout is equal to 0. The dependent variable "Transfers" is the ratio of net discretionary transfers over infrastructure expenses. Standard errors are clustered at the location of observation of incidence level. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

Table 6: **Effect of voter turnout on the main fiscal outcomes - weighted observations**

	Log Voter Turnout	Transfers		Log Infra. Expenses		Has Toxic Loan	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Squared Incidence in March	-0.001***						
	(0.000)						
Log Voter Turnout		0.065	-2.503**	0.110	-2.225**	0.342***	0.934
		(0.051)	(0.985)	(0.078)	(1.013)	(0.081)	(0.991)
Method		FE	2SLS FE	FE	2SLS FE	FE	2SLS FE
Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F stat.	13.5						
AR p-value			.003		.007		.364
# Municipalities	28808	28808	28808	28808	28808	28808	28808
# Clusters	726	726	726	726	726	726	726
# Obs.	337174	337174	337174	337174	337174	337174	337174
R2	0.615	0.006	-0.025	0.042	0.012	0.270	0.262

NOTES. The first column reports the first stage of the 2SLS estimation of the regression of the log of voter turnout on net transfers. The other columns report the fixed effect estimations (Columns 2, 4, 6) and the 2SLS fixed effect (Columns 3, 5, 7) estimations of the regression of the main outcomes of interest on the log of voter turnout in the first round of the French municipal elections. The instrumental variable used for the 2SLS fixed effect estimations is the square of infections incidence in the month of an election. There is one observation by municipality and by year, for each year between 2001 and 2013. Every observation is weighted by the size of the population of the municipality. All specifications include municipal fixed effects and all control variables included in Column 2 of Table 2. The first column reports the first stage of the 2SLS estimation of the effect of log of voter turnout on net transfers. F stat. is the F statistics of the significance of the instrument in the first stage. AR p-value is the probability that the Anderson-Rubin statistics is larger than the actual Anderson-Rubin statistics under the hypothesis that the coefficient of the Log of Turnout is equal to 0. The dependent variable "Transfers" is the ratio of net discretionary transfers over infrastructure expenses. Standard errors are clustered at the location of observation of incidence level. * Significant at the 10 percent level. ** Significant at the 5 percent level. * * * Significant at the 1 percent level.

Table 7: **Falsification tests: Infections incidence and *past* fiscal outcome**

	Past Transfers		Had Toxic Loan		Past Log Infrastructure Expenses	
	(1)	(2)	(3)	(4)	(5)	(6)
Log Incidence in March	-0.001		-4.609		0.002	
	(0.001)		(4.598)		(0.009)	
Squared Incidence in March		-0.002		-9.233		-0.001
		(0.002)		(22.279)		(0.002)
Weights	No	Yes	No	Yes	No	Yes
# Municipalities	28862	28802	28862	28802	28861	28801
# Clusters	726	726	726	726	726	726
# Obs.	232776	232416	232776	232416	231688	231329
R2	0.025	0.184	0.003	0.027	0.014	0.042

NOTES. This table reports the fixed effect estimations of the regression of the main fiscal outcomes of interest in a year t between 1998 and 2007 on the log of (Columns 1 to 3) or on the squared (Columns 4 to 6) average weekly infections incidence in the month of the closest election *after* t (i.e. March 2001 or March 2008). There is one observation by municipality and by year in the period 1998 to 2007. In Columns 4 to 6, every municipality is weighted by its population. All specifications include municipal fixed effects and all control variables included in Column 2 of Table 2. The dependent variable "Transfers" is the ratio of net discretionary transfers over infrastructure expenses. Infections incidence is observed at every point of a given set of locations of observations. Standard errors are clustered at the location of observation of incidence level. $*$ Significant at the 10 percent level. $**$ Significant at the 5 percent level. $***$ Significant at the 1 percent level.

Table 8: **Effect of infections incidence interacted with municipal characteristics**

	Log Turnout			Transfers	Log Infra. Expenses	Has Toxic Loan
	(1)	(2)	(3)	(4)	(5)	(6)
Log Incidence in March	-0.002*	-0.004**	-0.004**	0.014	0.010	-0.003
	(0.001)	(0.002)	(0.002)	(0.010)	(0.010)	(0.005)
Richer Municipality x Log Incidence in March	-0.002					
	(0.002)					
Older Municipality x Log Incidence in March		0.000				
		(0.002)				
Higher Turnout Municipality x Log Incidence in March			0.001	0.024*	0.030**	-0.010*
			(0.002)	(0.013)	(0.013)	(0.005)
# Municipalities	29303	29303	29303	28868	28868	28868
# Clusters	726	726	726	726	726	726
# Obs.	53271	53271	53271	337534	337534	286030
R2	0.556	0.556	0.556	0.000	0.013	0.068

NOTES. Columns 1 to 3 report the fixed effect estimations of the regression of the log of voter turnout on the log of average weekly infections incidence in the month of the last election before t (i.e. March 2001 or March 2008) and on the interaction of this variable with an indicator equal to 1 if the municipality is among the 50 percent richer (Column 1), or an indicator equal to 1 if the municipality is among the 50 percent older (Column 2), or an indicator equal to 1 if the municipality is among the 50 percent with higher turnout in the 2007 presidential elections (Column 3). See Section 5 for details on the definition of these indicator variables. Columns 4 to 6 report the fixed effect estimations of the regressions of the main outcomes of interest on the log of average weekly infections incidence in the month of the last election before t (i.e. March 2001 or March 2008) and on the interaction of this variable with an indicator equal to 1 if the municipality is among the 50 percent with higher turnout in the 2007 presidential elections. All specifications include municipal fixed effects and all control variables included in Column 2 of Table 2. The dependent variable "Transfers" is the ratio of net discretionary transfers over infrastructure expenses. Standard errors are clustered at the location of observation level. $*$ Significant at the 10 percent level. $**$ Significant at the 5 percent level. $***$ Significant at the 1 percent level.

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A Electoral System of French Municipal Councils

Above 3500 inhabitants - apart from the three biggest cities, Paris, Lyon and Marseille - council seats are distributed according to a party-list proportional representation system. Candidates run in groups, called *lists*, which may also be linked to a national party, and every voter can vote for at most one list. The list with the most votes obtains half the seats (rounded to the closest higher integer); the remaining seats are distributed according to the Highest Average Method among all groups (including the group that obtained the most votes). Below 3500 inhabitants, the electoral system follows a first-past-the-post voting method. Candidates run individually and every voter can give at most one vote to any arbitrary number of candidates smaller than the number of seats. The candidates fill the available positions in order of highest vote.[47] Even in municipalities with fewer than 3500 inhabitants, most candidates are affiliated with a list.[48] In any municipality, a list is headed by the leader who is the main proponent of the program of the list.

The number of councilmen also depends on the number of municipal residents, as shown in the following table.

[47]A candidate can be elected in the first round only if more than half voters and at least a quarter of registered voters voted for her or him.

[48]We do not have detailed information on the candidates who were not elected in the data we use here, but from detailed data on municipal in the West of France provided by the newspaper *Ouest-France*, it appears that at least 80 percent candidates belong to a list.

Table 9: **French Municipal Councils**

# Residents	# Municipal councilmen	Voting System
Less than 100	9	Individual/First-past-the-post (FPTP)
100 - 499	11	Individual/FPTP
500 - 999	15	Individual/FPTP
1000 - 2 499	19	Individual/FPTP
2 500 - 3 499	23	Individual/FPTP
3500 - 4999	27	Party-list /Proportional
5000-9999	29	Party-list /Proportional
10000-19999	33	Party-list /Proportional
20000-29999	35	Party-list /Proportional
30000-39999	39	Party-list /Proportional
40000-49999	43	Party-list /Proportional
50000-59999	45	Party-list /Proportional
60000-79999	49	Party-list /Proportional
80000-99999	53	Party-list /Proportional
100000-149999	55	Party-list /Proportional
150000-199999	59	Party-list /Proportional
200000-249000	61	Party-list /Proportional
250000-299999	65	Party-list /Proportional
300000 or more	69	Party-list /Proportional

B NOT FOR PUBLICATION. Additional results

Table 10: **Effect of voter turnout on the main fiscal outcomes using the square of infections incidence as the instrumental variable**

	Log Voter Turnout	Transfers		Log Infra. Expenses		Has Toxic Loan	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Squared Incidence in March	-0.001***						
	(0.000)						
Log Voter Turnout		0.017	-6.677**	0.125**	-7.356**	0.068***	1.891**
		(0.057)	(3.050)	(0.050)	(3.147)	(0.019)	(0.840)
Method		FE	2SLS FE	FE	2SLS FE	FE	2SLS FE
F stat.	11.6						
AR p-value			0		0		.067
# Municipalities	28868	28868	28868	28868	28868	28868	28868
# Clusters	726	726	726	726	726	726	726
# Obs.	337534	337534	337534	337534	337534	337534	337534
R2	0.555	0.000	-0.058	0.013	-0.133	0.071	-0.407

NOTES. The first column reports the first stage of the 2SLS estimation of the regression of the log of voter turnout on net transfers. The other columns report the fixed effect estimations (Columns 2, 4, 6) and the 2SLS fixed effect (Columns 3, 5, 7) estimations of the regression of the main outcomes of interest on the log of voter turnout in the first round of the French municipal elections. The instrumental variable used for the 2SLS fixed effect estimations is the square of infections incidence in the month of an election. There is one observation by municipality and by year, for each year between 2001 and 2013. All specifications include municipal fixed effects and all control variables included in Column 2 of Table 2. The first column reports the first stage of the 2SLS estimation of the effect of log of voter turnout on net transfers. F stat. is the F statistics of the significance of the instrument in the first stage. AR p-value is the probability that the Anderson-Rubin statistics is larger than the actual Anderson-Rubin statistics under the hypothesis that the coefficient of the Log of Turnout is equal to 0. The dependent variable "Transfers" is the ratio of net discretionary transfers over infrastructure expenses. Standard errors are clustered at the location of observation of incidence level. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.