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A Dirty Deed Done Dirt Cheap: Reporting the Blame of a National Reform on Local Politicians

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Abstract

This paper tests the hypothesis that upper-level governments can transfer the accountability of the costs of a reform to a lower one. The reform of the school rhythm in France provides the ground for a verification of this hypothesis, as it was nationally decided and locally implemented, right before a municipal election. The results confirm that local incumbents have taken the blame of the reform, especially in larger cities and if they belong to the governing coalition. In this case, thus, the cost of the reform is borne twice by the lower level of government, financially and politically, offering a double gain to the government. That mayors who have announced a boycott of the reform have received electoral gains confirms the perception of the local cost of the reform.

Keywords: Reforms, Elections, Municipalities, Reform.

JEL Classification: D04, D72, D78, H77, I28

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1. Introduction

In the aftermath of a series of deceiving results in PISA tests (OECD, 2014), the French government decided a reform of the school rhythm, starting from the elementary school level. Such a reform was considered necessary to obtain better results in the medium term. As such, this reform lies in conformity with the “crisis-induces-reform” hypothesis (see, e.g., Drazen and Grilli, 1993, or Agnello et al., 2015). However, in this case, it is the repetition of bad results of French pupils that creates the impetus for the reform, a scenario that would be in line with the argument on the dynamics of learning made by Tommasi and Velasco (1996), although in a very different context.¹

Moreover, the timing of the French school rhythm reform, implemented right after a Presidential election, could also be expected, as the political resistance to the reform is lower the further looms the next election (see, e.g., Alesina and Drazen, 1991, or Lora and Olivera, 2004). Also, as reforms lead to redistribution impacts, electoral losses can be expected, reinforcing the incentive to push reforms early during the mandate (Dewatripont and Roland, 1992, 1995; Padovano and Petrarca, 2013). The French government has apparently followed this conventional path, enforcing a reform of the French education system at the beginning of its mandate.

However, what makes this reform particularly interesting is that, in this particular case, the newly elected government could also have a reduced incentive to postpone the reform by transferring the costs of the reform to a lower level of government. In other words, upper-level politicians have anticipated that the costs of the reform would be borne by the local politicians, and that they would be gone by the next Presidential election. Such a framework has, to our knowledge, not been explored in the literature. Tandon (2012) explores the regional impact of a reform of Indian tariffs, looking at national elections, while Joanis (2013) examines the impact of different degrees of centralization on electoral accountability, we here focus on the “boomerang” effect of a national reform on local elections.

¹ As the authors look at the situation of developing countries. Remark that Rodrik (1996) considers the link between crises and reforms as tautological, but his argument does not apply here, as the reform we consider takes place after a series of disappointing results, and not after a brutal and exogenous shock.

In this paper, we thus profit from the fact that local elections happen between national ones to analyze if voters punish the party of a newly elected national government that enforces a reform whose effects and implementation are local. We look at the impact of the implementation of a national reform locally implemented on local incumbent's chances of reelection. So doing, we show how reforms could be accelerated, if their costs can, say, trickle down to a lower level of government. The French school rhythm reform thus provides a first-rate opportunity to study the response of voters to a reform, if only because the largest opposition party strongly opposed the reform (both at the local and national level), even though it shared the government's verdict on the need for reforming the school system.

We first show that the option to adopt the reform early was clearly perceived by local politicians as costly. The variables related to the costs of implementing the reform are significantly related to an attempt to postpone it. Except for the municipalities in the most dire financial situation, where the subsidies offered in case of an early adoption appear to have skewed the politicians' decisions.

Then, we show that the hypothesis of a transfer of accountability from the upper-level of government to the lower one is confirmed, as incumbents who implemented the reform suffer from a reduction of their electoral margin, and the impact goes beyond a traditional midterm effect. Interestingly, mayors that decided to boycott the reform have been rewarded by the voters for doing so and left-wing boycotters have even been more rewarded than other boycotters. This confirms that (at least some of) the local incumbents clearly perceived the transfer of accountability and tried to deflect the blame.

Beyond these results on voters' behavior, our results can be put in perspective, as they tend to show that, if different political parties face different costs in implementing the same reform (Cukierman and Tommasi, 1998, Tandon, 2012), delegating the costs to a lower level of government provides a way to bypass the status-quo bias (Alesina and Drazen, 1991), in effect doing a dirty deed to local politicians. They also reveal that an option for a politician that wants to reveal her ability to voters can be to report the blame of a reform on others, which could reduce the incentive to engage in socially detrimental reforms (Fu and Li, 2014), and further the prospects of welfare-enhancing ones.

The remainder of the paper is structured as follows. The next section presents the institutional context of the reform. Section 3 presents the data and the results. Finally, section 4 concludes.

2. The reform of the French school rhythm

In France, the organization and functioning of education is traditionally under the jurisdiction of the central government. However, in the 1980s, some competences have been devolved to municipalities, departments and regions. Actually, municipalities are in charge of building, renovating and maintaining the public schools that are located in their territory, and they manage all related expenditures, whatever their nature (investment or functioning). If several schools are present, the municipal council decides upon the rules of allocation of children to schools (zoning).

Importantly for our purpose, municipalities can (but are not obliged to) organize any complementary activity (educational or cultural as well as sports) they consider relevant, and they decide upon the opening hours. Also, for the kindergarten and elementary schools, they manage and organize the canteens, taking care of everything, from prices to menus to staff recruitment (outsourcing is of course allowed, although it is quite customary to have municipal staff in charge of the provision). Parents contribute financially to the feeding of their children, with contributions depending on their revenues. Municipalities also are in charge of the non-teaching staff, especially in kindergartens.

The implemented reform intended to modify the school rhythm. In fact, since the institution of universal public education in the late 19th century in France, schoolchildren have benefited from a weekly day off (for a long time on Thursdays, for religious reasons, then on Wednesdays - since 1972). To make up for the lost teaching times, schools opened their doors on Saturday mornings but, in 2008, under Conservative President Nicolas Sarkozy, it was decided to compress the school week into a four day schedule and Saturday has become a no-school day.

However, shortly after the 2012 Presidential election – won by the Socialist François Hollande –, the government announced a reform of the school week structure, to shorten the school day for primary school pupils, which is currently deemed as too long to allow for

effective learning. Teaching hours would be spread out over the week to make up the hours by extending the current system of 4 days of classes per week to 4.5 days. More importantly for our purpose, municipalities could implement immediately or postpone the implementation of the reform.

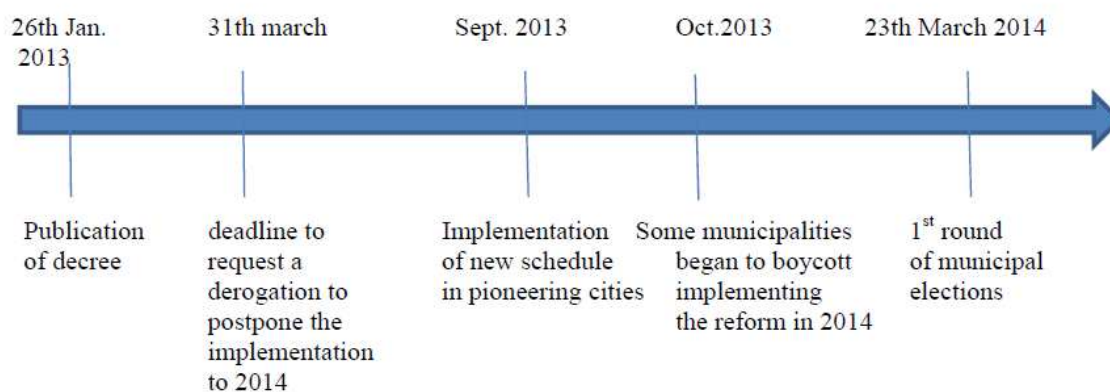
The reform essentially concerns kindergartens and elementary schools, hence directly impacting the municipalities, in the following ways. First, given the reduced school time, municipalities may have to organize more extracurricular activities, which has a direct financial impact (if only for petty stuff – paper, pens, balls, etc.). Second, if they ask (or request) the teachers to take care of the extracurricular activities, the extra-hours will have to be paid by municipalities (although the teachers are civil servants paid by the Ministry of Education for the normal time spent in class). Of course, municipalities can decide relying on extra staff, which they will nevertheless have to pay. The transition period can also be costly, as they have to recruit and (potentially learn to) manage new workers (in particular, specialized helpers in pre-school and after-school activities and extra canteen staff). Third, and important, the school transportation system will have to adapt, with an extra day of transport to be organized. The municipality may nevertheless share this last impact with its neighbors, if it belongs to a union of cities, or if the transportation system is managed by the upper-level of government (the “département”). The presence of such vertical links with the departmental council may in turn affect the diffusion of the reform.

Concretely, the official launch of the reform is a decree (dated 26th, January 2013) stating that municipalities had to decide upon the adoption of the reform before the 31st, March 2013. If the mayor refuses to answer or to ask the council to take a formal vote (as they have done in a majority of cases), the Ministry would consider the refusal as an obligation to implement the reform in 2013. From September 2013, some mayors decided to boycott the reform and not to implement it, even in 2014. Figure 1 displays the sequence of events.

Importantly, the municipal elections were programmed and held in March 2014, and the announcements by incumbents that they would boycott the reform increased strongly: while it first was a very marginal phenomenon before November 2013, at the eve of the first round of

municipal elections, 13% of incumbent mayors in municipalities with more than 3,500 inhabitants planned to boycott the implementation in September 2014.²

Figure 1. Timeline of the reform



Source: authors

Hence, this offers a case study of a reform decided by an upper-level government (here, the national government) whose cost is fully borne by the lower-level governments (here, municipalities).

Moreover, it has to be added that, as the time schedule of class was modified, this would have some impact on the parents' own rhythms, which explains that the reform was resented as disturbing by most parents (i.e., voters) and teachers, not to re-insist on the mayors, on whom the empirical part focuses. The question thus arises to know if, with local elections looming, the electoral cost would be supported by the local incumbents or by the national one at the next Presidential election. With the next Presidential election held in 2017, we here focus on verifying the transfer of accountability hypothesis.

² In the end, the government threatened to use all the legal powers it has to force the municipalities into implementing the reform and, at the start of the next school year (September 2014), the reform was enforced across the whole territory.

3. Assessing the local electoral consequences of the reform

To assess if voters have held the incumbent local politicians accountable of the implementation of the national reform (“responsibility hypothesis”), we follow the comprehensive approach developed by Padovano and Petrarca (2014), consisting in the estimation of both a policy setting equation and a vote popularity equation.

3.1. Data

Our sample includes all the municipalities with more than 3,500 inhabitants. This threshold is related to the fact that two different voting rules exist, depending upon the size of the municipality, with the two rounds list system applying above 3,500 inhabitants. 2615 French municipalities (Corsica excluded) are subjected to this two-round electoral rule in 2008, i.e., 7% of French municipalities, but 60% of the French metropolitan population.³ The threshold is also induced by the absence of school in many of the smaller municipalities, meaning that they often share the school with several other municipalities (often belonging to the same inter-communal structure). In such a case, municipalities must cooperate to determine if they wish to implement the school rhythm reform in 2013, which makes it more difficult to identify the determinants of the choice.

We choose to focus on the effect of the reform in the first round incumbent’s margin (as the election ends at the first round in more than 60% of the cities we consider). Three sets of explanatory variables are gathered. Table 1 summarizes the data sources and descriptive statistics. To save on space, we below precise the expected sign for each variables only when it may not be obvious with regard to our hypothesis.

--- Insert Table 1 about here ---

Local school context

³ The electoral rule has been changed in 2014. Before that, the rule was different between the municipalities hosting less than 3,500 inhabitants, and the ones hosting more. The threshold has been reduced to 1,000 inhabitants for the election of 2014. Hence, for consistency reasons, we focus on cities with more than 3,500 inhabitants.

The first set of variables relates to the local school context itself. First, it has to be acknowledged that practical and budgetary difficulties may arise when applying the reform to larger numbers of school-age children and public schools. The larger the proportion of school-age children in the municipal population is, the larger the global cost of extracurricular activities to be financed by the municipality. 80% of municipalities have 10 public schools at most. We thus introduce a dummy variable, equal to 1 if there are less than 10 public schools in the municipality. The large number of schools can create problems to hire and manage group leaders able to organize games, cultural and sporting activities during extracurricular activities. Moreover, school directors and parents could have conflicting preferences on the new organization of the school week between educational institutions, which may complicate the municipal choice and delay the implementation of the reform.

It is a common sense argument that long school days can result in a lower performance of pupils, with the worst impact to be experienced by children living in deprived urban areas. The literature so far has, to our knowledge, not proved the point. However, existing results show that extending the length of a school day is at best neutral and could even be detrimental, if the end of the week interruption is too long (see, Delvolvé and Jeunier, 1999, for the latter point, and De Cicca, 2007, Mayer and Klaveren, 2013, Taylor, 2014, for results in different contexts and levels).⁴ Schools in those areas belong to a Priority Education Network whose objective is to attenuate the impact of socio-economic inequalities on school performance. We introduce a dummy variable that takes the value 1 if some schools in the municipality belong to a priority education network, and 0 otherwise. Children in these municipalities would greatly benefit from a reform whose aims are to improve learning and to foster educational success and we can expect mayors to be more inclined to quickly implement the reform there.

Then, as soon as the decree has been published, some mayors of rural municipalities voiced concern with regard to the recruitment of qualified staff, the physical availability of space and the cost of school busing. We introduce a dummy variable to account for the specific difficulties that could delay the implementation of the reform in rural areas.

⁴ The consequences of a long school day are different from those of a longer school year, the latter tending to benefit the pupils and students (as shown by, e.g., Kikuchi, 2014, Parinduri, 2014, Agüero and Beleche, 2013, in very different contexts).

We also take into account existing vertical links between layers of government that come from their respective competencies relative to pre-schools and primary schools. As explained above, city unions can be endowed with competencies over extracurricular activities and school transportation. Thus, we introduce two variables that capture, respectively, if competences on extracurricular activities and school transport have been devolved to the city union. Finally, municipalities within a city union observe the core city's choice and could follow it.

The 2010 territorial authorities reform planned the completion of the inter-municipal map by the 1st of January 2014. Many municipalities joined a city union during this period while mergers of city unions also took place. In both cases, competences devolved to municipalities and the city union are likely to be modified, especially in extra-curricular activities and school transportation. As the competences changed, mayors might prefer to delay the implementation of the reform.

Budget data

As municipalities are endowed with competence over primary school, they must finance the after school activities generated by the reform. The government has created a specific subsidy ("fonds d'amorçage") and has budgeted 250 millions euros to cover the transition costs for the fast-adopters. Viewed as an incentive to adopt the reform in 2013, this subsidy aims at rewarding municipalities for the signal they send in adopting the reform in 2013. However, municipalities and their representative bodies have denounced the cost of the reform, providing various estimates of the per-pupil cost of it.

This not only suggests that budgetary concerns are an important determinant of the willingness to implement the reform, but that municipalities may have to raise local taxes to finance the reform, which they may be reluctant to do (especially right before an election and even more so in municipalities where the tax burden is already high). By the same reasoning, the level of municipal debt should also affect mayors' choices. We thus assume that local choices by mayors are constrained by the structure of their municipal budget. Debt is expressed in thousands of euros per capita, while the tax effort – a proxy of the tax burden computed by the government - is calculated by dividing the amount of taxes collected by the municipality on households (property tax on developed and undeveloped property and housing tax) by an estimate of how much tax the municipality should be able to collect given

its tax bases. This measure enables the comparison of the actual taxes and the expected taxes if the municipality applied the average national tax rates. 2012 data are used as the choice to implement the reform has been taken between January and March 2013 (between September 2013 and March 2014 as for the choice to boycott it, see Figure 1).

Besides, poorer municipalities receive additional grants from the State to implement the reform (atop from the "fonds d'amorçage", a lump-sum grant which amounts to 50€ per pupil and an additional grant of 40€ per pupil if the municipality is located in a poor surrounding). The additional State aid is dedicated to the poorest municipalities, the ones that receive the targeted urban solidarity grant ("*DSU-cible*") or the targeted rural solidarity grant ("*DSR-cible*"). To analyze if the additional State aid has an incentive effect on the probability to implement the reform in 2013, we introduce two dummy variables: the first one ("aid to poor urban municipalities") is equal to 1 if the municipality receives the targeted urban solidarity grant and 0 otherwise, while the second one ("aid to poor rural municipalities") is equal to 1 if the municipality receives the targeted rural solidarity grant.

As voters may also be sensitive to several aspects of local public management, we include three other items in the vote function: staff costs per capita, operating expenditures per capita (excluding staff costs) and equipment spending per capita. For these, we use 2013 financial data, because elections are held in March of the following year.

Political variables

A third set of data is related to the mayor's personal and political characteristics. Even if the municipal council is the decision-making body, power is centralized in the hands of the mayor, who has authority over the municipality's civil servants and takes all decisions relative to the implementation of its budget. As a consequence, personal characteristics of the mayor could affect the probability of an early implementation of the reform.

Detailed information on personal characteristics of mayors is provided through the national directory of elected officials (French interior ministry). A dummy "Less than 60 years old" is included as to reflect potential nostalgia for school weeks that run on four and a half days as it was the norm up to 2008. Even though the proportion of women mayors is still extremely low in France (11%, see Table 1), a dummy variable accounts for this fact. The experimental economics literature on gender differences acknowledges that women are more risk averse

than men when facing a risky situation (see, for example, Croson and Gneezy, 2009). Implementing a controversial reform can be considered as such a risky move, and we thus expect a negative sign for this variable.

Four specific socio-professional categories are used as proxies of the mayors' sense of public service and of the capacity to focus on the children's interests. Teachers (from preschool teachers to higher-education teaching personnel) are first considered. On the one hand, with a significant knowledge of how schoolchildren and students learn best, they should be the best motivated to an early implementation of the reform. On the other hand, they could be unfavorable to a reform that increases the spread of weekly working hours of their fellows without pay compensation. Civil servants (other than teachers) and workers in public enterprises should bear in mind –to some extent- the public service values and the will to ensure quality education to children. Physicians should be responsive to the impact of the school time schedule on children's health. They are in a position to promote a reform directed towards the interests of children, not their parents'. We create a dummy variable for each category.

Another subgroup of political variables accounts for the links between the local and national political contexts. We include a dummy equal to 1 if the incumbent mayor and the majority in Parliament are from the same political party and 0 if not. Likewise, another dummy expresses the similarity of political leaning between the challenger and the majority in Parliament. These variables control for the potential influence of the national government's popularity on local elections. French voters commonly consider municipal elections as midterms and use them to penalize government and the parliamentary majority for poor performance. We use a variable to control for the vote share received by presidential candidate from the incumbent mayor's party in the second round of the last presidential election. Dummies for the incumbent's national standing are included and are equal to 1 if she is a deputy or a senator, and 0 otherwise.

Also, a weak electoral support should restrict the available policy space the mayor needs to implement a controversial reform, while strong past electoral results should provide more leeway for local public choices. Electoral support can be expressed either by a dummy variable equal to 1 if the mayor was elected in the first round in the last election, or by the absolute margin between the mayor and her main challenger at the final round.

The number of consecutive mandates won by the mayor also deals with the past electoral support. We build three dummy variables to account for the number of terms of office and the ability of the mayor to be reelected: “*2nd term*”, “*3rd term*” and “*more than 3 terms*”. The expected sign for all these variables is positive: implementing a controversial reform should not prevent the reelection of the mayor, which could increase the probability of starting the new school schedule in 2013.

Finally, to account for the incumbent and the challenger candidates' characteristics without increasing the number of variables introduced into the model, we also introduce variables that relate the gap between the incumbent and the challenger in terms of gender and of age in the vote popularity equation.

Dependent variables

The dependent variable of interest, with regard to the hypothesis we test, is the margin between incumbent's score and the challenger's one. However, to evaluate how voters value mayors that have implemented a costly reform, we have to take into account a selection issue: only the mayors who have implemented the reform can be made accountable. This leads us to implement a two-step procedure, and to consider for the first step a dependent variable that takes the value 1 when the reform has been implemented, 0 otherwise.

3.2. Method

As stated right above, to control for selection issues related to the choice of an early (resp. postponed) implementation of the reform, we follow a two-step procedure. In the first step, the choice of early implementation ($Reform2013_i$) is our first binary dependent variable.⁵ This observed decision takes the value 1 if the municipality decided to implement the reform in 2013 and 0 otherwise. This choice depends on the difference in utilities between the two alternatives (early reform and postponed reform): $U_{1,i} - U_{0,i}$. The probit model assumes that this difference, $U^* = U_{1,i} - U_{0,i}$, follows a normal distribution. However, U^* is not observable, as only the real choices are known, which is reflected in:

⁵ We also perform and report the estimates with boycotting the implementation in 2014 as the dependent variable.

$$Reform2013_i = 1 \text{ if } U_i^* \geq 0$$

$$Reform2013_i = 0 \text{ if } U_i^* < 0$$

In the model, we consider the impact the characteristics of the municipality i that we have described over the net utility of the mayor, and thus on the probability to implement the reform in 2013. The Probit decision model is thus:

$$\begin{aligned} Reform2013_i &= 1 \text{ if } U_i^* = c + \alpha \Omega_i + e_i > 0, \\ Reform2013_i &= 0 \text{ otherwise,} \end{aligned} \tag{1}$$

where U_i^* is the unobservable latent dependent variable, c is the intercept, Ω_i is the set of relevant (school, financial and political) characteristics of the municipality, and $e_i \sim N(0,1)$ is a disturbance term. In addition to the standard White correction for heteroskedasticity, we correct for clustering at the departmental level using the Froot's correction (Froot, 1989).

In robustness checks, we will interact some of our control variables to strengthen and refine the analysis. However, the magnitude and standard errors of the interaction effect in nonlinear models are not correctly estimated. As stated by Ai and Norton (2003), the magnitude of the interaction effect in nonlinear models does not equal the marginal effect of the interaction term, and can be of opposite sign. We will thus introduce interaction terms in the estimation following the methodology built by Norton et al. (2004) to compute the magnitude of the interaction effect.

In the second step, we study the incumbent's margin in the first round of the 2014 municipal election by standard linear equation. We compute the margin ($INC2014_i$) as the difference between the share of votes received by the incumbent and that of her principal challenger. First, we test whether the implementation of the reform in 2013 affects the incumbent's result in municipal elections held in March 2014 (equation 2). For obvious (if technical) reasons, we cannot introduce in the same equation a dummy variable indicating those who promote boycotting the reform (equation 3). In addition to the key variables related to the reform, the three set of determinants of the mayor's popularity that have been described above are included:

$$INC2014_i = \delta_1 Reform2013_i + \theta_1 \Omega_i + \varepsilon_i, \quad (2)$$

$$INC2014_i = \delta_2 Boycott_i + \theta_2 \Omega_i + \varepsilon_i, \quad (3)$$

The expected share of votes for the incumbent in the first round clearly influences the probability of adopting an electorally risky reform in 2013. Endogeneity concerns in a dummy variable should be controlled for by estimating a treatment effect model based on Heckman control function. Endogeneity arises in this case because the treatment (*REFORM2013*) is correlated with the error term in the outcome (vote function) equation. The treatment effect model simultaneously estimates equations for the likelihood of treatment (*REFORM2013*, equation 1) and the outcome of the treatment (the incumbent margin, equation 2). This simultaneous estimation allows the elimination of endogeneity, although with the trade-off of making the assumption that the error terms are jointly normally distributed. Maximum likelihood techniques were used to estimate the model.

However, it seems relevant to consider that treatment effects vary with the political characteristics of the municipalities that implemented the reform in 2013. The estimated model is thus the following:

$$INC2014_i = \delta_3 Reform2013_i + \delta_4 (Reform2013_i * Pol_i) + \theta_3 \Omega_i + \varepsilon_i, \quad (4)$$

$$Reform2013_i = 1 \text{ if } U_i^* = c + \alpha \Omega_i + e_i > 0, \quad (5)$$

Reform2013_i = 0 otherwise.

In the presence of such interactions between the treatment variable and another explanatory variable, there are two endogenous variables in the model (*Reform2013_i* and the interaction term, *Reform2013_i * Pol_i*). Nevertheless, the maximum likelihood technique presented above is not affected by the introduction of this second endogenous variable and estimates remain consistent (as shown by Brown and Mergoupis, 2010).

4. Results

Tables 2 and 3 display the results of the simultaneous estimates of the above equations (4) and (5). Table 2 reports the estimation results for the implementation year (equation 5) and details,

in the first two columns, our baseline regression results for all municipalities above the 3,500 inhabitants electoral threshold, whereas columns 3 to 6 deal with the subsamples of municipalities with less than 9,000 inhabitants and more than 9,000 inhabitants.

--- Insert Table 2 about here ---

Results about internal determinants reveal that the reform has been more easily embraced in smaller municipalities, the ones that host less than 10 schools. The proportion of school age children significantly, strongly and negatively influences the probability of adoption in 2013. Interestingly, and somewhat paradoxically, belonging to the Priority Education Network has not statistically influenced the adoption.

The level of taxes has been a clear impediment to the quick adoption of the reform, probably for fear of the increase in spending related to the extra-curricular activities. Interestingly, though, the level of debt has a positive impact on the dependent variable. This may be due to the fact that mayors in relatively highly indebted municipalities embrace the reform because they are searching for new sources of funds, even if temporary ones, and thus respond to get the extra transfers from the government. In the robustness checks provided in columns 7 to 12, we interact the municipal debt with the fact that the municipality receives an aid dedicated to poor municipalities, confirming that only poor and heavily indebted municipalities are more likely to implement the reform in 2013.

As regards vertical links, we show that city union competencies have an impact on the early implementation of the reform but only in the subsample of municipalities above 9,000 inhabitants. When the city union has competency over extracurricular activities, it increases the probability of an early implementation in the municipality (which should not have to bear the cost of these activities). However, when the city union is in charge of school transportation, it significantly reduces the probability of early implementation, as municipalities should then cooperate on the modalities of the reform. Finally, municipalities follow the core city's decision: when the core city chooses to implement the reform in 2013, it has a positive impact on the early implementation by the other municipalities within the city union and a negative impact on the decision to boycott the reform in 2014.

Few variables related to the mayor's appear to be significant, except for the age and a profession in the civil service. The latter can be understood as either a revelator of a form of loyalty towards the state legislation, or a bias towards values related to the Left. This is confirmed by the fact that teachers have a positive leaning towards the reform, as they are less likely to have boycotted it).⁶ The impact of age can be related to a nostalgia effect of the rhythm schools had before 2008.

However, the most important coefficients are the ones attached to the alignment of the mayor with regard to the governmental coalition. Left-wing mayors are more prone to support the reform and to undertake it without delay. And the variables that are related to the local political context (past electoral results, number of consecutive mandates won by the incumbent mayor) are not significant, highlighting the strong impact of the national situation. The higher François Hollande's score (in the 2012 presidential election in the municipality), the higher the probability of adoption in 2013. It means that mayors also take left-leaning voters into account, even when the former do not belong to the governmental coalition.

All in all, then, these first results reveal a strong impact of practical, financial and political characteristics of the municipality.

We now turn to the second part of the model, which forms the core of our investigation. Table 3 reports results about the incumbent margin in the first round of 2014 municipal election, to assess the local political cost of the national reform. Estimations are performed on the whole sample, and then on several subsamples, based on the population size of the municipality (see table 3, columns 3 to 6 for baseline results, and 9 to 12 for robustness checks). We can reject the null hypothesis of no correlation between the treatment errors and the outcome errors except for municipalities under 9,000 inhabitants.⁷

--- Insert Table 3 about here ---

The main coefficients of interest here are those related to the implementation of the reform as we want to test whether voters hold mayors responsible for the implementation of the reform.

⁶ In Saint-Paul's (2010) theoretical model, the left-leaning of the teachers is shown to have an impact on agents' preferences.

⁷ For this specific subsample, there is thus no need to estimate a treatment effect model and it is possible to use standard OLS.

No evidence is found that the implementation of the reform (early adoption or boycott) has affected voters' choice in municipalities with less than 9,000 inhabitants (table 3, columns 3-4 and 9-10). However, the responsibility hypothesis is strongly verified in larger municipalities, where political stakes are stronger for political parties and politicians. There, it clearly appears that incumbents who implemented the reform in September 2013 are sanctioned: they would have had a 2% higher margin if they had not applied the reform. On the opposite, they are rewarded if they announced the boycott of the reform before the election, and the average reward is as large as 3% (compared to the incumbents who would not have announced the boycott). In columns 7 to 12, we refine the analysis to determine if this sanction/reward mechanism differs according to the incumbent's political side and find that, whatever her political affiliation, the early adopter incumbent receives a smaller margin, hence enduring a political cost from the reform. In contrast, left-wing boycotters are even more rewarded than other boycotters, which confirms that our hypothesis of a transfer of the political cost of a reform from one decision level to the other has been perceived by (at least a part of) the local politicians.

If a mayor belongs to the leading coalition in parliament, the margin is reduced, a standard result in French municipal electoral studies, corresponding to a midterm effect. Reciprocally, the incumbent margin increases if her main challenger belongs to the governmental coalition.

As for budgetary variables, results are in line with existing studies of the vote function in France (see, e.g., Dubois and Paty, 2010, or Cassette et al., 2013). Staff costs have a positive impact on the incumbent's margin, supporting the idea that voters consider this to be more useful spending than standard operating expenditures. Spending on equipment also appears to be valued by voters. Incumbents in heavily indebted municipalities benefit from a lower margin, whatever the size of the municipality, which also can be related to our starting hypothesis.

Finally, being a deputy or a senator is favorable to the incumbent but only in municipalities above the 9,000 inhabitants threshold. If the incumbent has been elected at the first round of the election at the preceding election, it has no impact on her 2014 margin. And the number of consecutive mandates has a non-linear impact, first increasing the margin, until electoral fatigue effect takes place, (slightly) reducing the incumbency premium.

5. Conclusion

This paper studies how voters do reward or blame their local politicians for the implementation of reform decided at the national level. The results confirm that local incumbents have taken the blame of implementing the reform, especially in larger cities and if they belong to the governing coalition. In this case, thus, the cost of the reform is borne twice by the lower level of government, financially and politically, offering a double gain to the government. The fact that left-wing boycotters have received higher electoral gains than other boycotters suggests that they expected it, and feared more their voters' sanction than their party's blame.

The hypothesis of a transfer of accountability is thus supported by the evidence coming from the French school rhythm reform. Mayors not only bear the budgetary cost of the reform but also the electoral ones. Both explain their reluctance to apply it. Test of the transfer of accountability hypothesis can only be complete with the next Presidential election (2017), but then disentangling the effects will be made harder as many more than the education reform will concur to explain the results.

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Table 1. Summary statistics and expected effects

Variables		Sources	Obs.	Summary statistics			
				Mean	Std Dev	Min	Max
MAYOR'S POLICY							
REFORM2013	1 if the school rhythm reform has been implemented in the municipality as soon as September 2013	Departmental services of the Ministry of National Education	2615	0,23	0,42	0	1
BOYCOTT	1 if the municipality refuses to implement the reform in 2014	www.clrdrs.fr	2615	0,10	0,30	0	1
BUDGET DATA							
Tax effort	amount of taxes collected by the municipality on households divided by an estimate of how much tax the municipality should be able to collect given its tax bases if the municipality applied the average national tax rates.	Census of the Ministry of Finance	2612	1.02	0,28	0,08	2.64
LOCAL DEBT	Municipal Debt (thousands of euros per capita)		2612	0,95	0,69	0	11,45
MAYOR'S PERSONAL CHARACTERISTICS							
Less than 60 y.o	1 if the mayor is less than 60 y.o., 0 otherwise	Ministry of Internal Affairs	2612	0.37	0.48	0	1
WOMAN	1 if the mayor is a woman, 0 otherwise		2612	0,11	0,31	0	1
TEACHER	1 if the mayor if a teacher, 0 otherwise		2612	0,15	0,36	0	0
CIVIL SERVANT	1 if the mayor is a civil servant, 0 otherwise		2612	0,15	0,36	0	1
PUBLIC ENTERPRISES	1 if the mayor works in a public enterprise, 0 otherwise		2612	0,05	0,21	0	1
PHYSICIANS	1 if the mayor if a physician, 0 otherwise		2612	0,04	0,19	0	1
links between the local and national political contexts							
COALGOV	1 if the mayor and the majority in Parliament belong to the same political party, 0 otherwise	Ministry of Internal Affairs	2612	0,33	0,46	0	1
OTHER_LEFT	1 if the mayor is from other leftist parties, 0 otherwise		2612	0,18	0,38	0	1
DEPUTY	1 if the mayor is a deputy, 0 otherwise		2612	0,06	0,24	0	1
SENATOR	1 if the mayor is a senator, 0 otherwise		2612	0,03	0,16	0	1
PRESID_LEFT	Incumbent party share of votes at the presidential election		2612	51.79	9.76	14.25	78.22
Mayors' past electoral results							
1 ST ROUND	1 if the mayor was elected in the first round of the preceding election, 0 otherwise	Ministry of Internal Affairs	2612	0,67	0,46	0	1
INCPREV	incumbent party's margin in the last municipal election		2612	23.63	27.04	0	100

2 nd term	1 if the mayor spends her 2nd term in office, 0 otherwise		2612	0,37	0,47	0	1
3 rd term	1 if the mayor spends her 3rd term in office, 0 otherwise		2612	0,15	0,36	0	1
More than 3 terms	1 if the mayor already spent more than 3 terms in office, 0 otherwise		2612	0,15	0,35	0	1
local school context							
POP 2-14	Share of school-age children (2 to 14 y.o.)	INSEE	2612	0,16	0,02	0,08	0,26
Less than 10 schools	1 if there are less than 10 schools in the municipality, 0 otherwise		2615	0,81	0,39	0	1
Priority Education Network	1 if some schools in the municipality belong to a priority education network, 0 otherwise		2612	0,21	0,40	0	1
Aid to poor urban municipalities	1 if municipality received the targeted urban solidarity grant, 0 otherwise	Census of the Ministry of Finance	2612	0,11	0,31	0	1
Aid to poor rural municipalities	1 if municipality received the targeted rural solidarity grant, 0 otherwise		2612	0,16	0,36	0	1
Vertical links							
Extraact _cityunion	1 if competences on extracurricular activities have been devolved to the city union, 0 otherwise	BANATIC, Ministry of Internal Affairs	2612	0,21	0,41	0	1
Transport _cityunion	1 if competences on school transportation have been devolved to the city union, 0 otherwise		2612	0,30	0,46	0	1
Core city of the city union	1 if municipality is the core city of the city union, 0 otherwise		2615	0,41	0,49	0	1
intensity of electoral competition in 2014							
NB1_same	Number of candidates at the first round, same wing	Ministry of Internal Affairs	2612	1.35	1.18	0	9
NB1_opp	Number of candidates at the first round, opposite wing		2612	0.74	1.17	0	6

Table 2: Early vs. postponed implementation of the reform: baseline results and robustness checks

VARIABLES	1	2	3	4	5	6	7	8	9	10	11	12
	REFORM2013	BOYCOTT	REFORM2013	BOYCOTT	REFORM2013	BOYCOTT	REFORM2013	BOYCOTT	REFORM2013	BOYCOTT	REFORM2013	BOYCOTT
	all		3500-9000 inhab.		>9000 inhab.		all		3500-9000 inhab.		>9000 inhab.	
Less than 10 schools	0.076*** (0.027)	-0.030 (0.023)			0.094*** (0.035)	-0.022 (0.019)	0.073*** (0.027)	-0.033 (0.023)			0.093*** (0.033)	-0.023 (0.018)
POP 2-14	-1.152*** (0.368)	0.571** (0.270)	-0.943* (0.519)	0.468** (0.223)	-1.638*** (0.549)	0.983* (0.537)	-1.229*** (0.381)	0.650** (0.266)	-1.006* (0.516)	0.500** (0.229)	-1.698*** (0.597)	1.013* (0.533)
Priority education network	-0.051** (0.025)	0.022 (0.023)	-0.058* (0.034)	0.030 (0.027)	-0.043 (0.028)	-0.003 (0.027)	-0.042* (0.024)	0.027 (0.022)	-0.049 (0.033)	0.055* (0.030)	-0.041 (0.029)	-0.005 (0.028)
Aid to poor urban municipalities	0.063* (0.038)	0.007 (0.024)	0.048 (0.059)	0.170*** (0.061)	0.058 (0.043)	-0.017 (0.031)						
Aid to poor rural municipalities	0.008 (0.025)	0.002 (0.028)	0.017 (0.029)	-0.003 (0.020)	-0.038 (0.101)	0.060 (0.128)						
Aid to poor municipalities tax effort							-0.034 (0.033)	0.036 (0.056)	-0.040 (0.039)	0.041 (0.048)	-0.023 (0.065)	0.042 (0.078)
	-0.010 (0.039)	0.028 (0.026)	-0.047 (0.052)	0.048* (0.025)	0.014 (0.052)	0.010 (0.043)	-0.014 (0.040)	0.034 (0.028)	-0.054 (0.053)	0.053* (0.028)	0.009 (0.052)	0.010 (0.043)
Debt per capita	0.026** (0.013)	0.006 (0.007)	0.028* (0.015)	-0.009 (0.010)	0.025 (0.020)	0.026*** (0.009)	0.016 (0.015)	0.011* (0.006)	0.019 (0.018)	-0.004 (0.008)	0.011 (0.023)	0.033*** (0.011)
Debt per capita*aid to poor municipalities							0.060** (0.031)	-0.050 (0.043)	0.075 (0.046)	-0.047 (0.049)	0.065 (0.052)	-0.053 (0.051)
COAL GOV	0.290*** (0.028)	-0.100*** (0.019)	0.237*** (0.031)	-0.070*** (0.014)	0.370*** (0.049)	-0.133*** (0.036)	0.291*** (0.028)	-0.100*** (0.019)	0.236*** (0.031)	-0.072*** (0.013)	0.372*** (0.050)	-0.134*** (0.036)
OTHER LEFT	0.071** (0.028)	-0.032** (0.014)	0.054 (0.033)	-0.043*** (0.012)	0.127** (0.063)	-0.016 (0.028)	0.071** (0.028)	-0.032** (0.014)	0.053* (0.032)	-0.041*** (0.012)	0.127** (0.065)	-0.016 (0.029)
WOMAN	0.034 (0.029)	0.011 (0.014)	0.007 (0.034)	0.009 (0.019)	0.074 (0.054)	0.003 (0.023)	0.032 (0.029)	0.011 (0.013)	0.006 (0.034)	0.008 (0.019)	0.070 (0.053)	0.004 (0.023)
LESS than 60 years old	-0.036*** (0.013)	0.004 (0.011)	-0.035* (0.019)	-0.012 (0.012)	-0.035 (0.024)	0.025 (0.022)	-0.035*** (0.013)	0.005 (0.011)	-0.032* (0.019)	-0.010 (0.012)	-0.037 (0.024)	0.025 (0.022)
TEACHER	0.024 (0.027)	-0.029** (0.013)	0.046 (0.038)	-0.027** (0.013)	-0.015 (0.035)	-0.025 (0.024)	0.025 (0.027)	-0.027** (0.013)	0.046 (0.038)	-0.026** (0.013)	-0.016 (0.036)	-0.023 (0.023)
CIVIL SERVANT	0.065*** (0.022)	0.003 (0.019)	0.053* (0.028)	0.005 (0.022)	0.094*** (0.037)	-0.003 (0.024)	0.065*** (0.022)	0.004 (0.019)	0.055** (0.027)	0.005 (0.022)	0.090** (0.037)	0.001 (0.024)

VARIABLES	1	2	3	4	5	6	7	8	9	10	11	12
	REFORM2013	BOYCOTT	REFORM2013	BOYCOTT	REFORM2013	BOYCOTT	REFORM2013	BOYCOTT	REFORM2013	BOYCOTT	REFORM2013	BOYCOTT
	all		3500-9000 inhab.		>9000 inhab.		all		3500-9000 inhab.		>9000 inhab.	
PUBLIC	0.075	-0.047***	0.076	-0.041***	0.083	-0.068**	0.075	-0.047***	0.077	-0.037***	0.081	-0.068**
ENTERPRISES	(0.046)	(0.014)	(0.050)	(0.010)	(0.082)	(0.027)	(0.046)	(0.014)	(0.051)	(0.011)	(0.083)	(0.027)
PHYSICIANS	0.010	0.033	0.063	0.040	-0.063	0.018	0.011	0.034	0.064	0.047	-0.065	0.018
	(0.046)	(0.025)	(0.063)	(0.036)	(0.058)	(0.041)	(0.046)	(0.025)	(0.064)	(0.038)	(0.058)	(0.041)
DEPUTY	0.027	0.040*	0.104	0.032	0.000	0.052*	0.025	0.042*	0.098	0.031	0.000	0.053*
	(0.043)	(0.021)	(0.084)	(0.045)	(0.051)	(0.032)	(0.043)	(0.022)	(0.084)	(0.045)	(0.052)	(0.032)
SENATOR	-0.024	0.023	0.005	0.004	-0.039	0.040	-0.028	0.025	0.001	0.007	-0.042	0.043
	(0.044)	(0.032)	(0.087)	(0.049)	(0.045)	(0.046)	(0.042)	(0.032)	(0.085)	(0.050)	(0.044)	(0.046)
MARGIN 2008	0.000	0.000	0.001**	0.000	-0.001	0.001*	0.000*	0.000	0.001**	0.000	-0.001	0.001*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
1st ROUND	-0.011	-0.005	-0.032	0.012	0.051	-0.051*	-0.011	-0.005	-0.031	0.011	0.049	-0.049*
	(0.022)	(0.013)	(0.030)	(0.012)	(0.036)	(0.028)	(0.022)	(0.012)	(0.030)	(0.012)	(0.036)	(0.028)
2nd term	0.016	0.016	0.018	-0.007	0.020	0.042	0.016	0.016	0.018	-0.005	0.021	0.041
	(0.022)	(0.015)	(0.028)	(0.015)	(0.036)	(0.027)	(0.022)	(0.015)	(0.028)	(0.015)	(0.036)	(0.026)
3rd term	0.044	0.026	0.040	0.011	0.066	0.032	0.046*	0.025	0.042	0.014	0.069	0.030
	(0.027)	(0.022)	(0.035)	(0.022)	(0.049)	(0.041)	(0.027)	(0.022)	(0.034)	(0.023)	(0.049)	(0.041)
More than 3 terms	0.013	0.034	0.008	0.017	0.021	0.040	0.017	0.033	0.012	0.019	0.024	0.038
	(0.029)	(0.029)	(0.035)	(0.027)	(0.051)	(0.045)	(0.029)	(0.028)	(0.035)	(0.026)	(0.052)	(0.044)
PRESID_LEFT 2012	0.007***	-0.000	0.008***	-0.001	0.007***	-0.001	0.007***	-0.000	0.008***	-0.001	0.007***	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
joining a city union	-0.048*	-0.016	-0.059*	-0.012	-0.015	-0.036	-0.048*	-0.015	-0.057*	-0.006	-0.018	-0.036
in 2014	(0.028)	(0.016)	(0.033)	(0.014)	(0.040)	(0.026)	(0.028)	(0.016)	(0.033)	(0.014)	(0.039)	(0.027)
Extraact _ city	0.055	-0.022	0.028	0.003	0.091***	-0.060**	0.057	-0.021	0.028	0.003	0.097***	-0.062**
union	(0.040)	(0.019)	(0.050)	(0.017)	(0.033)	(0.027)	(0.041)	(0.018)	(0.050)	(0.017)	(0.033)	(0.027)
Transport _ city	-0.031	0.018	-0.010	-0.002	-0.063**	0.050**	-0.031	0.018	-0.010	-0.002	-0.064**	0.050**
union	(0.024)	(0.019)	(0.032)	(0.019)	(0.028)	(0.023)	(0.024)	(0.019)	(0.032)	(0.019)	(0.027)	(0.023)
rural areas	0.004	0.014	-0.004	0.032	0.104		-0.003	0.015	-0.007	0.030	0.082	
	(0.034)	(0.024)	(0.036)	(0.025)	(0.177)		(0.034)	(0.023)	(0.036)	(0.024)	(0.172)	
Core city of the city	0.086***	0.002	0.106***	-0.010	0.048	0.025	0.082**	0.003	0.101**	-0.014	0.046	0.027
union	(0.032)	(0.020)	(0.039)	(0.015)	(0.038)	(0.028)	(0.032)	(0.020)	(0.039)	(0.015)	(0.038)	(0.028)
Reform 2013 in the	0.261***	-0.054**	0.252***	-0.044**	0.283***	-0.054*	0.257***	-0.054**	0.249***	-0.046**	0.280***	-0.052*
core city	(0.044)	(0.025)	(0.052)	(0.023)	(0.059)	(0.030)	(0.044)	(0.024)	(0.052)	(0.023)	(0.060)	(0.030)
Observations	2,612	2,612	1,614	1,614	998	988	2,612	2,612	1,614	1,614	998	988
Pseudo R ²	0.2064	0.1070	0.1832	0.1016	0.2726	0.1639	0.2075	0.1101	0.1848	0.0945	0.2735	0.1655
Percent of correct												
predictions	79.2%	89.9%	80.1%	91.7%	80.5%	85.9%	79.7%	89.9%	79.9%	91.7%	80.5%	86.2%

* denotes significance at 10%; ** at 5%; *** at 1%. Marginal effects computed at means. Robust errors into parentheses. Froot (1989) correction for departmental-level cluster correlation.

Table 3. Incumbent margin, first round of the 2014 municipal election

	-1	-2	-3	-4	-5	-6	-7	-8	-9	-10	-11	-12
	INC MARGIN1		INC MARGIN1		INC MARGIN1		INC MARGIN1		INC MARGIN1		INC MARGIN1	
	all		3500-9000 inhab.		>9000 inhab.		all		3500-9000 inhab.		>9000 inhab.	
REFORM2013	-0.133***		-0.103		-0.159***		-0.161***		-0.118*		-0.177***	
	(0.048)		(0.097)		(0.038)		(0.040)		(0.063)		(0.048)	
REFORM2013 X INC_COALGOV							0.028		0.014		0.019	
							(0.024)		(0.032)		(0.036)	
BOYCOTT2014	0.177***		0.084		0.197***		0.178***		0.102		0.193***	
	(0.042)		(0.208)		(0.071)		(0.036)		(0.090)		(0.046)	
BOYCOTT X INC_COALGOV							0.084**		0.083		0.071*	
							(0.034)		(0.053)		(0.043)	
Past win margin_1st round	0.002***	0.002***	0.002***	0.002***	0.004***	0.004***	0.002***	0.002***	0.002***	0.002***	0.004***	0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Elected in the first round of the preceding election	0.011	0.010	0.002	0.002	-0.004	-0.007	0.011	0.011	0.002	0.002	-0.003	-0.007
	(0.011)	(0.011)	(0.014)	(0.015)	(0.022)	(0.023)	(0.012)	(0.012)	(0.015)	(0.015)	(0.021)	(0.020)
WOMAN_GAP	-0.041***	-0.041***	-0.051***	-0.050***	-0.024*	-0.028**	-0.041***	-0.041***	-0.050***	-0.050***	-0.024**	-0.028**
	(0.007)	(0.007)	(0.009)	(0.010)	(0.015)	(0.014)	(0.008)	(0.008)	(0.011)	(0.011)	(0.012)	(0.012)
AGE-GAP	-0.001***	-0.001***	-0.001**	-0.001**	-0.002***	-0.002***	-0.001***	-0.001***	-0.001**	-0.001**	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
INC_COALGOV	-0.144***	-0.159***	-0.137***	-0.153***	-0.146***	-0.163***	-0.146***	-0.164***	-0.138***	-0.155***	-0.147***	-0.169***
	(0.020)	(0.018)	(0.030)	(0.032)	(0.024)	(0.025)	(0.016)	(0.014)	(0.022)	(0.020)	(0.022)	(0.021)
CHALL_COALGOV	0.101***	0.097***	0.097***	0.092***	0.102***	0.105***	0.102***	0.096***	0.097***	0.092***	0.102***	0.104***
	(0.012)	(0.012)	(0.015)	(0.015)	(0.019)	(0.019)	(0.010)	(0.010)	(0.013)	(0.013)	(0.016)	(0.016)
Share of votes at the presidential election	0.078***	0.065***	0.055**	0.044**	0.090***	0.076***	0.076***	0.065***	0.054***	0.043**	0.089***	0.076***
	(0.015)	(0.014)	(0.022)	(0.020)	(0.023)	(0.023)	(0.014)	(0.013)	(0.019)	(0.018)	(0.019)	(0.019)
# of candidates, same wing	-0.031***	-0.031***	-0.043***	-0.042***	-0.030***	-0.029***	-0.031***	-0.031***	-0.043***	-0.042***	-0.030***	-0.029***
	(0.006)	(0.006)	(0.009)	(0.009)	(0.007)	(0.006)	(0.005)	(0.005)	(0.008)	(0.008)	(0.006)	(0.006)
# of candidates, opposite wing	0.019***	0.018***	0.002	0.002	0.012	0.013	0.019***	0.018***	0.003	0.002	0.012*	0.013**
	(0.006)	(0.006)	(0.012)	(0.012)	(0.008)	(0.008)	(0.005)	(0.005)	(0.010)	(0.010)	(0.007)	(0.007)
# of consecutive years of mandate	0.011***	0.011***	0.011***	0.011***	0.011***	0.011***	0.011***	0.011***	0.011***	0.011***	0.011***	0.011***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Mandate ²	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)

	-1	-2	-3	-4	-5	-6	-7	-8	-9	-10	-11	-12
	INC MARGIN1		INC MARGIN1		INC MARGIN1		INC MARGIN1		INC MARGIN1		INC MARGIN1	
	all		3500-9000 inhab.		>9000 inhab.		all		3500-9000 inhab.		>9000 inhab.	
Deputy	0.092***	0.079***	0.038	0.025	0.073***	0.066***	0.091***	0.079***	0.039	0.026	0.072***	0.067***
	(0.019)	(0.017)	(0.032)	(0.029)	(0.019)	(0.019)	(0.019)	(0.019)	(0.043)	(0.044)	(0.020)	(0.020)
Senator	0.071***	0.063***	0.058	0.046	0.058**	0.054**	0.070**	0.065**	0.057	0.048	0.057*	0.055*
	(0.020)	(0.022)	(0.051)	(0.045)	(0.023)	(0.026)	(0.029)	(0.029)	(0.060)	(0.060)	(0.032)	(0.032)
Operating costs net from interest charges (per capita)	0.067	0.072	0.097	0.114	0.076	0.063	0.066	0.072	0.096	0.112	0.074	0.066
	(0.067)	(0.070)	(0.096)	(0.096)	(0.083)	(0.090)	(0.056)	(0.055)	(0.082)	(0.082)	(0.076)	(0.076)
Staff costs (per capita)	0.075***	0.075***	0.042	0.035	0.050*	0.060*	0.076***	0.072***	0.043	0.035	0.050	0.058*
	(0.029)	(0.029)	(0.042)	(0.039)	(0.030)	(0.032)	(0.026)	(0.026)	(0.042)	(0.042)	(0.034)	(0.034)
Equipment spending (per capita)	0.032**	0.039**	0.046**	0.053**	0.022	0.025	0.031*	0.040**	0.046*	0.054**	0.022	0.025
	(0.016)	(0.016)	(0.022)	(0.023)	(0.016)	(0.016)	(0.017)	(0.017)	(0.025)	(0.024)	(0.023)	(0.022)
Debt (per capita)	-0.036***	-0.042***	-0.040***	-0.043***	-0.024***	-0.036***	-0.036***	-0.042***	-0.040***	-0.043***	-0.024**	-0.036***
	(0.007)	(0.008)	(0.011)	(0.012)	(0.009)	(0.009)	(0.007)	(0.007)	(0.010)	(0.010)	(0.010)	(0.010)
Observations	2384	2384	1410	1410	974	974	2384	2384	1410	1410	974	974
Log pseudolikelihood	-597.27	-296.52	-398.47	-161.24	-143.06	-75.01	-596.61	-293.55	-398.38	-160.02	-142.91	-73.66
Wald Test of indep. Eq.: Chi(2)	10.06***	8.52***	0.47	0.08	16.24***	8.69***	12.40***	19.17***	1.74	1.03	19.03***	14.15***

Robust errors into parentheses. Significance levels: *10%, **5%, ***1%. Intercept not reported. Froot (1989) correction for departmental-level cluster correlation. Results of the treatment equation not reported

