

Better to Be Direct? Evidence from the Abolition of Direct Elections in Italian Local Governments

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Abstract

This paper provides evidence for the importance of direct electoral processes by investigating the consequences for public spending of an unexpected reform that repealed direct elections for local (provincial) politicians in Italy. Direct elections were substituted with indirect ones, whereby directly elected municipal politicians choose a municipal mayor to serve as provincial president. Using a difference-in-differences strategy, I document two main consequences of the reform. First, municipalities connected to the provincial presidents tend to receive disproportionately more public funds after the reform, suggesting geographic favoritism increased. Second, the share of provincial resources spent on public goods drops in favor of bureaucratic costs. I discuss suggestive evidence that these results are driven by weaker electoral incentives rather than by the selection of worse politicians.

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1 Introduction

Since the end of World War II, Western countries have become more democratic, with more and more political officials being selected by citizens through general elections. A vast political-economic literature shows that elections allow voters to hold politicians accountable, thereby reducing corruption and helping newly enfranchised groups receive a bigger share of public resources (Fujiwara (2015), Cascio and Washington (2013), Martinez-Bravo et al. (2012)). Democratization comes in very different shades, and each electoral law has a different degree of representativeness. In a broad categorization, public offices may be assigned through direct elections (a popular vote) or through indirect elections (whereby citizens only choose the appointers).¹ The two categories mainly differ in two aspects: the selection process and the electoral incentives the laws generate. Direct choice comes with grassroots selection of politicians and a bundle of strong electoral incentives generated by the more stringent need to please the citizens and by high levels of public attention to the electoral process. In contrast, indirect elections reduce the salience of the vote and partially isolate appointed officials from electoral incentives vis-à-vis citizens; at the same time, the quality of representatives could improve or worsen, as their selection may be based on criteria other than representativeness, such as competence, seniority, party loyalty, or friendship. Since selection and incentives may affect public spending in opposite ways, and because of the dearth of reforms transitioning from direct to indirect elections, the literature has only partially answered the question whether more indirect elections lead to more inefficient provision of public goods. Moreover, it is still unclear whether the distortions from indirect elections are mainly driven by the changes in representatives' selection or in their incentives.

In this paper I cast some light on the importance of the incentives generated by direct elections for efficient provision of public goods. To do so, I analyze an electoral reform in Italy that replaced direct elections in provinces (midsized local governments, including on average eighty municipalities each, that are presided over by a president) with a form of indirect election whereby the president of the province is chosen by all the mayors and councillors of the municipalities located inside the province. Moreover, the pool of eligible candidates is restricted to municipal mayors, who are still directly elected by citizens. The reform's permanence was arguably a historical accident: the change was supposed to be a minor, transitory step toward the complete elimination of the provincial

¹See Akzin (1960) for a discussion on the overlap between the notions of indirect election and appointment.

governments through a constitutional referendum. However, the referendum failed for reasons arguably unrelated to the reform of the provinces, turning the transitory step into a permanent electoral law. These circumstances are particularly advantageous for studying the effects of indirect elections. First, the abolition of direct elections is very rare in a modern democracy, especially absent a comprehensive constitutional reform, which makes this a precious opportunity to evaluate this phenomenon in isolation. Second, from the perspective of the provincial governments - whose behavior I am studying - the reform was an exogenous shock, as it was imposed by the central government with the only purpose of saving public resources.

Theoretically, the passage from direct to indirect elections could have changed both the selection and the incentives of the politicians. I find suggestive evidence that selection does not play a role in this context and that the effects on public spending are associated with a drop in accountability. In fact, data on politicians elected to provincial governments before and after the reform reveal no changes in politicians' education, a proxy for quality (as in Baltrunaite et al. (2014), Galasso and Nannicini (2011), Kotakorpi and Poutvaara (2011)), or age, a proxy for experience. This is consistent with the fact that the pool of eligible politicians is still selected via direct municipal election. At the same time, many elements suggest a drop in provincial governments' accountability to citizens. First, the reform reduced the number of people directly holding provincial politicians accountable from all the citizens receiving provincial public goods to a few hundred politicians, who often vote based on party affiliation. Second, the municipal politicians across the province only meet for the provincial vote, and their composition changes every year, following municipal elections. This makes it hard for them to monitor provincial leaders and to enforce future punishments. Third, municipal electoral campaigns focus almost exclusively on municipal issues, making provincial politicians' appointment an extremely low-salience topic for citizens. Consistent with this, I find evidence of a steep drop in the salience of provincial elections, as measured by Google searches and an almost-complete lack of discussion on provincial issues during municipal elections, after elections became indirect. This evidence suggests that the changes in public spending observed after the reform can be attributed to a change in politicians' incentives. I present a simple model showing that a reduction in election's salience facilitates politicians' rent-seeking, which is consistent with the results of my empirical analysis.

I use a difference-in-differences strategy to demonstrate that the introduction of indirect elections increases geographic favoritism in the allocation of resources and vitiates the general composition

of public spending. First, I find that after the reform the municipalities connected to the provincial presidents receive 10%–30% more public transfers per capita compared to the unconnected ones. The effect is larger in municipalities in which the presidents were born than in those in which they were elected mayors, and no additional funds were sent to municipalities sharing political ideology with the president, suggesting that leaders only act in their own self-interest rather than catering to their new electors (the mayors) to secure votes. Second, I show that after the reform the share of the provincial budget devoted to public-good provision declines in favor of administrative costs. To do so, I compare the main categories (or sectors) of provincial spending (education, transport, administration) to the corresponding categories in the municipalities in each province, which maintained direct vote. Specifically, I find that provinces reduced the share of current expenditure on transport and education by 3.9 and 5.5 percentage points respectively, and they increased the share spent on bureaucracy by 10 percentage points. In line with the literature on budget composition (Rauch (1995), Hessami (2014)), I focus on the share of spending in each sector, rather than total spending. This is the right outcome to consider when, as in the Italian case (Grembi et al., 2016), politicians have sufficient discretion on how to allocate resources but have limited influence on the total size of budget, which largely depends on grants from the central government or on very inflexible taxes. In any case, the effect on absolute expenditure goes in the same direction as that on relative expenditure. Additionally, I provide suggestive evidence of an increase in car accidents on provincial roads compared to municipal ones that is consistent with a diminished quality of provincial public goods. I interpret these findings as evidence that drops in accountability cause a less equitable and less efficient allocation of public resources. Indeed, in high-income democracies, in which corruption is severely punished, hometown favoritism and high bureaucratic costs are two subtle forms of rent-seeking that politicians can implement with low risk of judicial punishment and which can only be reduced by high accountability to voters.

This paper contributes to the literature on direct elections of politicians by documenting two novel effects of a passage from direct to indirect elections, namely increases in geographic favoritism and in bureaucratic costs. Additionally, I contribute to this literature by isolating the specific role played by electoral incentives and conceptualizing this with a simple theoretical model. Many quasi-experimental papers on this topic exploit changes from parliamentary to presidential systems with a directly elected president, which does not allow researchers to isolate the introduction of the direct vote from additional presidential powers and adjustments in checks and balances. This

sort of transition happened in Indonesian local elections, which produced more health expenditure (Skoufias et al. (2014)) and political budget cycles and had no effect on local public investments (Sjahrir et al. (2013), Kis-Katos and Sjahrir (2017)). The Italian reform is exceptional in this sense because the provincial government remained a presidential system but provincial leaders are not directly accountable to citizens anymore.² Other papers have studied the passage of the Seventeenth Amendment in the US, which made senators directly elected, and showed that senators became more responsive to the electorate (Gailmard and Jenkins (2009)), less polarized (Bernhard and Sala (2006)), and more active in sponsoring bills and participating in roll calls (Meinke (2008)). My setting allows me to study the effect of indirect elections on the overall government's spending activity, arguably what ultimately matters for citizens, rather than focusing on the behavior of single politicians in one of the branches of government.

Moreover, while all the existing studies analyze the effect of moves toward direct elections, this paper investigates the effects of going from direct to indirect elections. Symmetry with the adoption of direct elections should not be taken for granted since path dependence and established norms could theoretically preserve an efficient provision of public goods even after general elections are abandoned. Furthermore, unlike most of the aforementioned studies, mine examines this in the context of a political natural experiment within a modern democracy and in the absence of other major political or constitutional shocks. To my knowledge only Hessami (2018), who studies the introduction of the direct election of German mayors, analyzes this matter in a similarly clean scenario. While she shows that directly elected mayors seek more grants in election years, my setting allows me to study a different and complementary set of outcomes: local favoritism and budget allocation. Finally, I contribute to this literature by discussing whether changes in representatives' characteristics or in their incentives are driving the effects of direct elections. In this sense, my setting allows for an exercise that is similar in principle to the use of term limits to separate accountability from politicians' characteristics (Alt et al. (2011), Aruoba et al. (2019), Ferraz and Finan (2011)) but can be applied to study moves between types of election. My findings are also in line with the literature that compares elected and appointed nonpolitical officials and suggests that the former are more in favor of consumers (Besley and Coate (2003)).

Finally, I speak to the research on birthplace favoritism, a phenomenon documented both in

²Formally, before the reform the system was *neopresidential* (Bin and Pitruzzella, 2010), as the council could unseat the president with a no-confidence motion. This was very unlikely since the president's party enjoyed a large (60%) majority and because unseating the president automatically caused the end of the councillors' mandate.

settings with authoritarian institutions (Hodler and Raschky (2014), Do et al.(2007)) and those with strong democratic traditions (Baskaran and Lopes da Fonseca (2021), Folke et al. (2021), Maaser and Stratmann (2016)). Consistent with Fiva and Halse (2016) and Carozzi and Repetto (2016), I find that incentives to favor one's hometown exist even beyond electoral incentives. My paper is the first to show that geographic favoritism increases with more indirect forms of election, providing an important insight for policy intervention. Indeed, high-income countries interested in reducing geographic favoritism at the local level and in reducing the cost of local bureaucracies should consider increasing, rather than reducing, the use of direct election for local leaders.

The paper is organized as follows: Section 2 provides the institutional background, Section 3 discusses the conceptual framework, and Section 4 describes my data. I present my identification strategy and results in Sections 5 (hometown favoritism) and 6 (sectoral allocation of resources). Section 7 discusses threats to identification and alternative explanations, and Section 8 concludes.

2 Institutional Background

The Italian constitution subdivides the national territory into three main levels of government - the regions (20), the provinces (110), and the municipalities (around 8,000) - with smaller units nested into larger ones. Each level is in charge of different duties, virtually unchanged since 2000, and each was historically run by politicians selected through direct election. Provinces are mainly responsible for providing public transport, constructing and maintaining roads and schools, and performing standard administrative functions (local tax collection, bureaucratic procedures, and so on), which together account for 70% of the provincial budget.

Following the European debt crisis, the need to reduce public spending led many political parties to request a constitutional reform in order to eliminate the provincial governments and to redistribute their responsibilities to other institutions, such as the regions. Therefore, the government planned a two-step political action: first, Parliament would pass the Delrio Act to substitute direct elections for provincial leaders with political appointment in order to momentarily reduce the costs of provincial politicians' elections and wages; second, a provision within a broad constitutional reform - initially presented to Parliament in April 2014 - would remove every reference to the provinces from the constitution, allowing for their complete abolition (Longo and Mobilio (2016) discuss the general

ex-ante expectations on the postreferendum role of provinces).³ The plan was not successful: while Parliament passed the Delrio Act in April 2014, the constitutional reform was rejected by popular referendum in December 2016. The act was passed with the expectation that the reform would be approved. The reform's rejection was largely unrelated to voters' ideas about provincial governments and mostly connected to another provision downgrading the Italian Senate and to the chance of weakening the left-wing government. Moreover, the constitutional referendum monopolized public attention, reducing awareness of the changes that the Delrio Act was bringing about.⁴

After the failure of the constitutional reform, provinces remained in place; thus, the Delrio Act remained the main law regulating them and the process for selecting their leaders (namely, a president and twelve councillors, on average).⁵ The new law abolished general elections for provincial president and councillors. Instead, only the mayors and councillors of the municipalities can now vote to appoint provincial politicians. Furthermore, as illustrated in Figure 1, only municipal mayors are eligible for president, the most powerful provincial office. To run for president, candidates need the written support of 15% of eligible voters (the municipal politicians) and their municipal office must not expire within the next eighteen months. Provincial elected officials maintain both municipal and provincial offices and for some time only earned wages as mayors until provincial wages were reintroduced in 2020.⁶ Figure 2 shows the timeline of provincial elections.

The new law did not modify the provinces' responsibilities, explicitly maintaining education (school infrastructure) and transport (public transport, road maintenance) as provincial functions. While provinces were allowed to transfer functions to regions, their responsibilities remained stable, as confirmed by the 2015 *Council for Local Autonomies* report and by vast journalistic evidence.⁷

³A similar reform, which was supposed to take effect in 2014, was rejected by the Constitutional Court in 2013; because of that, the 2012 and 2013 provincial elections were postponed to 2014.

⁴Appendix Figure A2 plots the Google searches for the words *province* and *referendum*. The absence of a peak in searches for *province* when the Delrio Act was voted confirms the relatively low salience of the event.

⁵The council size depends on the provincial population. It shrank from a range of twenty-four to forty-five members before 2011, to a range of nineteen to thirty-six after 2011, to a range of ten to twenty-four members since 2014. In Section 7 I discuss why this change cannot explain my results.

⁶To better illustrate how the elections work, consider the median province: it contains sixty municipalities ruled by sixty mayors and around seven hundred councillors, all entitled to express a vote for the provincial government. Municipalities are divided into nine population bins, used to assign weights to the votes cast by each municipality's representatives. The president, elected every four years, is only chosen among mayors (councillors have no voting right) as the candidate that wins more weighted votes. The provincial council is elected every two years by the municipal councillors and the mayors, and seats are assigned to parties using a proportional vote with the D'Hondt method, while a candidate preference is used to select candidates within party lists. Before the reform, elections for all provincial offices were held every five years and the candidate with an absolute majority of votes (in the first round or, alternatively, in a runoff election) would be elected president and their party would get 60% of the seats (still allocated with the D'Hondt method).

⁷"Provincial responsibilities remained the same: the maintenance of 135.000 Km of roads ... and the maintenance of 6000 schools." Il Corriere della Sera (Jan, 2017).

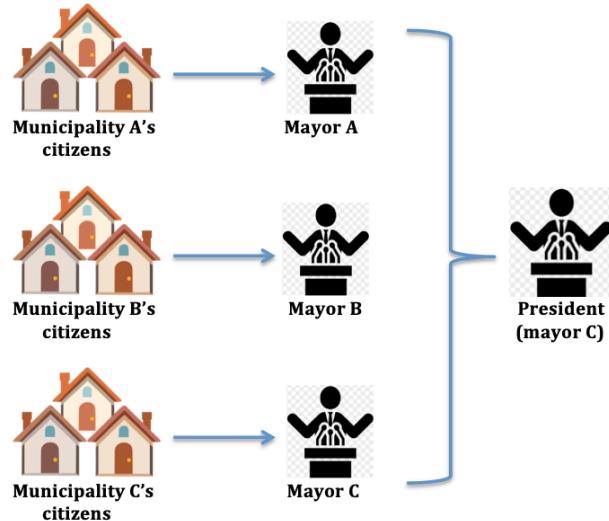


Figure 1: Indirect presidential electoral system (mayor of municipality C becomes provincial president)

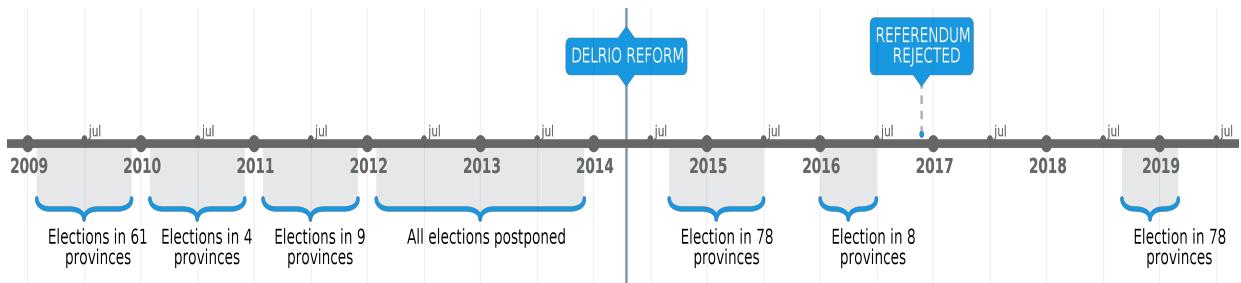


Figure 2: Timeline of the election for the provincial presidents

Since 2008 the central government has constantly reduced funding to the provinces.⁸ However, Grembi et al. (2016) classify at least one-third of Italian local governments' current and capital spending as nonrigid, attesting to the discretion of local politicians in allocating available resources.

3 Conceptual Framework

3.1 Selection and Incentives

The replacement of direct with indirect elections could affect both the selection of provincial politicians - potentially improving or worsening their characteristics - and their incentives, by diluting accountability to citizens. The fact that these two forces may go in opposite directions or reinforce each other makes

⁸"Provinces are in full stagnation ... having cut funds without cutting responsibilities delays or reduces services to the citizens." Cuneo24.it (Nov, 2019). While transfers from the central government decreased overall, anecdotal evidence suggests the government intervened by sending resources to provinces - for instance, in 2016 - to make sure essential services were provided (provincial and municipal revenues are plotted in Appendix Figure C7). Simultaneously, the 2015 budget law demanded a 50% reduction of provincial personnel through nonreplacement of retiring personnel and transfers of employees to other administrations, mostly the regions. In practice, this reduction was progressive and only accelerated an already-existing trend of reducing provincial personnel.

it hard to predict what will happen to public spending when we transition from direct to indirect elections. In this section, I discuss how selection and incentives have been affected by the reform and the extent to which one of the two forces can be isolated.

First, I argue that changes in selection are limited in this setting. After all, the pool of province-eligible politicians is still selected by the citizens with a direct vote in municipal elections. However, it could still be the case that the worst mayors are now appointed as provincial presidents if citizens are better than politicians at choosing leaders; or it could be the case that the reform induced worse politicians to run for mayor in the first place (though this is unlikely since half of the mayors in power in 2015 had been elected before the reform). Similarly, if provincial politicians were (in)experienced compared to municipal ones before the reform, it is possible that (older) younger presidents are now selected. To understand whether the reform led to changes in politicians' characteristics, I test whether the composition of provincial leaders in terms of age and education changed after the reform and I compare it to the control group of municipal leaders by employing a simple difference-in-differences framework. Table 1 reports the results of this exercise, and Appendix Figure A3 plots the same outcomes over time; both analyses exclude a change in presidents' characteristics.⁹ Taking education as a proxy for the quality of politicians (as in Baltrunaite et al. (2014) or Galasso and Nannicini (2011)) and age for experience, these results rule out a story of different selection based on these characteristics.

Table 1: Balance test: change in presidents' characteristics after the reform

	(1) Years of education	(2) Age
ProvLeader*Post	-0.114 (0.528)	0.479 (1.478)
Office FE	Yes	Yes
Year FE	Yes	Yes
Observations	56,579	56,592
R-squared	0.655	0.559

The dependent variables are politicians' years of education and age. ProvLeader takes value 1 for provincial presidents and 0 for municipal mayors. Post is 0 in 2009-13 and 1 in 2014-20. Years and Office fixed-effects included. Robust standard errors, clustered at the province level, are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

⁹Given a strengthening of gender quotas at the municipality level (discussed in Section 6.1), I find a relative increase in municipal female representation. This change is irrelevant for the results in Section 5, in which both treatment and control groups are municipalities passively receiving higher-level transfers. Moreover, it looks irrelevant for results in Section 6, in which the discontinuous behavior in the raw averages is entirely driven by the behavior of the provincial budget rather than by the municipal one. In any case, I include reported gender among the controls in Table 3. Its inclusion actually increases the point estimate and the significance of the estimated effect.

Second, I discuss several reasons why the reform led to a drop in accountability. The indirect voting system prevents citizens from expressing a vote on provincial leaders, which used to induce them to pay attention to provincial dynamics; at the same time, municipal electoral campaigns focus exclusively on municipal issues, excluding the issue of provincial politicians' appointment from public debate. This drop in salience, exacerbated by the fact that municipal and provincial elections are not synchronized anymore, strongly dilutes provincial leaders' accountability to the citizens. Consistently with this, Appendix Figure A1 shows evidence of a drop in the salience of provincial elections after the reform, as measured by Google searches. In addition, the people directly holding provincial politicians accountable dropped from hundreds of thousands of citizens to a few hundred politicians. Since the latter are often members of a party, they may vote based on party affiliation and be less responsive to changes in public spending. Finally, the municipal electors across the province only meet for the provincial elections, and their composition changes every year following municipal elections. This makes monitoring very difficult, as future punishments cannot be enforced by the same individuals over time.¹⁰

Third, to attribute politicians' misbehavior to a drop in accountability, it is important to rule out changes in other types of incentives, such as monetary ones. Indeed, a possible confounding factor is the change in wages of provincial presidents after the reform. Since the presidents only earn wages as mayors, their average income decreased. The government's budget law approved in late 2019 offers an occasion to disentangle the effect of the decrease in wage from that of the decrease in electoral accountability. Provincial presidents' salaries were raised in 2020 to a level similar to the prereform period, leaving the other features of the reform unaffected. If wages were the main determinants of my results, we should expect a reversal of the reform's impact on public spending from 2019 to 2020. I find no evidence of such a reversal, corroborating my hypothesis that changes in public spending are due to changes in electoral accountability. Overall, the context and the evidence suggest that any changes in politicians' behavior should be attributed to changes in electoral accountability.

3.2 Theoretical Model

I present here a simple theoretical model that conceptualizes how moving from direct to indirect elections, by reducing the salience of the electoral process, leads to worse public spending and more rent-seeking. The model, which I develop and discuss in detail in Appendix D, is an original extension

¹⁰The assembly of mayors, a secondary provincial entity, can only approve changes in the provincial statute.

of the probabilistic voting model with rents by Persson and Tabellini (2000). In my case, voters derive utility from private consumption and the public spending of two tiers of government, the municipality and the province, while politicians derive utility from the (endogenous and exogenous) rents they can extract from tax revenues. I compare voters' utility and politicians' rents in two scenarios. In the first one, voters are only allowed to vote for the municipal governments, which then choose the provincial leaders. In the second, voters can elect directly both the provincial and municipal governments. In the first scenario, under the assumptions that policy platforms are binding and that municipal politicians always vote for a provincial candidate based on their party affiliation - a reasonable assumption given that most provincial leaders in the real world have a party affiliation - voters end up choosing a bundle of a municipal and a provincial politician from the same party. Given the reduced salience of indirect provincial elections, voters discount the part of their utility $W(\cdot)$ derived from provincial activity. Municipal voter i in group j thus votes for party A rather than for party B if

$$W^j(g_A^m, r_A^m) + \rho W^j(g_A^p, r_A^p) \geq W^j(g_B^m, r_B^m) + \rho W^j(g_B^p, r_B^p) + \sigma^{ij} + \delta \quad (1)$$

where g_I^m and r_I^m (g_I^p and r_I^p) are respectively public spending and rents proposed by party I in municipality m (province p), σ^{ij} is voter i 's idiosyncratic party bias, and δ is the popularity shock.

The model predicts that, in equilibrium, the amount of provincial rents depends on the salience of the provincial election - captured by the parameter ρ - when citizens elect their municipal politicians. In particular, provincial rents are positive and increase when less popular attention is devoted to provincial dynamics. This approach highlights the efficiency cost citizens may suffer if the indirect-election process causes them to pay little attention to provincial policy. As derived in Appendix D, there exists a threshold ρ^* for the salience of the provincial election below which the amount of rents under indirect election is always higher than the amount with the direct vote. This is the conceptual framework that better fits the setting I study, whereby the lack of a direct vote significantly reduced the attention devoted to provincial elections, as evident from the drop in Google searches plotted in Appendix Figure A1.¹¹ The role of salience is crucial in this model: if an indirect election manages to maintain very high salience (as with, for example, the election of a

¹¹Not only did the amount of Google searches for the words *Province* and *President* drop during provincial elections once the vote became indirect, but the number of such searches does not peak during municipal elections, when the future appointers are selected. This suggests that the overall salience of the provincial vote severely dropped.

president in a parliamentary system), then this result is not guaranteed.

The model I developed is not the first one finding a detrimental effects of indirect elections on public spending. Persson et al. (1997) compare the amount of public goods and presidential rents in three political systems: one with a directly elected president; one with a directly elected president and legislature; one with a directly elected legislature that in turn elects the president. The president has agenda-setting powers regarding the allocation of the budget, while the legislature (municipal mayors and councillors, in my case) has veto power over the budget proposal. The model predicts that a system in which the president is indirectly elected generates more presidential rents than one with direct election.¹² However, it partially departs from my setting because municipal politicians, unlike a parliament, do not have veto powers over the amount of transfers proposed by the president.

4 Data and Sample

My main dataset was obtained from AIDA PA, a database providing information on Italian local administrations' sources of revenues and on their sectoral public spending. The database provides municipality-by-year budget data subdivided into sectors and types of spending, which allows me to avoid subjective categorizations.¹³ I mainly focus on current spending, as it accounts for almost 70% of total revenues; however, I also discuss capital-expenditure dynamics, when the data are available.¹⁴ More specifically, in my geographic-favoritism analysis I rely on AIDA data to study the

¹²The model relies on the further assumption that the president can remain in power even if the legislature is not reappointed. In this case, with indirect elections, the president and the legislature can threaten the voters that they will collude, thus forcing the voters to set their reservation utility in a way that allows the president to get more rents compared to the case of direct elections. A sketch of the argument goes as follows: As in a classic accountability model, voters set a reservation utility above which they reappoint the incumbent. With direct election, the equilibrium amount of permissible rents leaves the president indifferent between being reelected and diverting all the rents that they can (the legislature is given its status quo amount of rents). With indirect election and collusion, under the binding promise by the legislature to reappoint the presidents, the latter can propose a budget that gives the legislature higher utility than what they would obtain if reelected, making them prefer to forgo reelection. In this case, the president and the legislature would both get more rents, but the voters could only dismiss the legislature facing the same president (and problem) over and over. This would make voters worse off; thus, in order to avoid it, voters end up allowing the president to divert more rents. In equilibrium, the legislature is left with its status quo rents, the president diverts more rents and both are reelected.

¹³The database comes with a relevant error: Spending on the transport category is constructed by summing up five main spending subcategories, the two most important of which (accounting for 95% of it) are public transport and expenditure on roads. Before 2016, however, the overall transport category erroneously coincided with public transport alone. I checked the financial statements of some provinces to make sure this was indeed a mistake and fixed the problem by generating a new transport variable equal to the sum of the main subcategories. In Appendix Figure C15, as a robustness check, I also add the missing subcategory (namely, expenditure on roads) to current spending; the results are virtually unaffected.

¹⁴Current spending captures recurring expenses such as yearly wages or maintenance of existing infrastructure; capital spending captures longer-term investments. Appendix Figure C6 shows that the former accounts for 70% of total revenues both in provincial and municipal budgets, whereas the latter accounts for less than 20%. AIDA PA provides detailed capital-spending data at the province level only up to 2015 and never for the education sector.

amount of current transfers that municipalities received from any higher institutions between 2011 and 2019; 2020 is only included in some robustness checks, as municipal transfers that year were strongly affected by the government's response to COVID. AIDA does not provide disaggregated data for municipal transfers in all relevant years, so I integrate this measure with information from the SIOPE database to isolate transfers coming from local public administrations. For my analysis on the composition of public spending, I use AIDA PA's data on municipal and provincial revenues and on expenditure on transport, education, and administration between 2009 and 2020.

The Italian Ministry of the Interior provides electoral data for municipalities and pre-reform provinces, as well as information on candidates' level of education and birthplace. I then integrate postreform provincial electoral data using information from the websites of the provinces. Finally, I use data from the Italian Institute of Statistics on the number of car accidents as a measure of the quality of roads. Specifically, I use accidents on nonurban roads, among which provincial roads are the most represented (highways are excluded), as a proxy for low quality of provincial transportation, and I use urban-road accidents as a proxy for low quality of municipal transportation.

I exclude from my sample three autonomous provinces that were not affected by the law and ten large cities, which were subject to a very different set of rules.¹⁵ All the ninety-four remaining provinces had provincial leaders appointed by directly elected local politicians. In the analysis on geographic favoritism (Section 5) I further exclude the autonomous regions of Sicily, Sardinia, and Friuli and focus on the remaining seventy-seven provinces and 5,603 municipalities.¹⁶ Table 2 shows the summary statistics for the main variables of interest.

5 Indirect Elections and Geographic Favoritism

5.1 Identification Strategy

With the introduction of indirect elections, citizens lost direct electoral control over provincial leaders. In this section I analyze whether this reform led provincial politicians to channel disproportionately more resources to municipalities they are connected to, a relatively subtle and legal form of rent-seeking

¹⁵These are, respectively, Aosta, Bolzano, and Trento; and Milan, Rome, Turin, Genoa, Bari, Naples, Bologna, Florence, Venice, and Reggio Calabria.

¹⁶Out of the ninety-four affected provinces, seventy-seven were equally impacted, whereas for the remaining seventeen (in Sicily, Sardinia, and Friuli) the regions had some autonomy in defining who could appoint the new legislative body. Friuli abolished three of its four provincial governments, while in Sicily and Sardinia the (directly elected) regional governments started appointing provincial leaders. Since the leaders are not necessarily mayors, I exclude these areas from my analysis of geographic favoritism.

Table 2: Summary statistics

Variable	Obs	Mean	Std dev	Min	Max
Total current transfers, million euros	55,564	0.658	2.766	0	242
Transfers per capita, euros	55,561	137.3	292.9	0	17180.1
Log. transfers per capita	55,464	4.3	1.06	-11.8	9.8
Administrative curr. spending, million euros	1,307	66.2	68.2	4.603	506
Administration curr. spending, share	1,307	0.311	0.085	0.103	0.639
Transport curr. expenditure, million euros	2,227	22.6	19.1	0.003	147
Transport curr. expenditure, share	2,227	0.162	0.125	0.000	0.713
Education curr. expenditure, million euros	2,227	19.9	19.8	0	135
Education curr. expenditure, share	2,227	0.114	0.065	0	0.425
Population	2,249	437573.6	266447.2	81918	1749040
Accidents provincial roads	1,125	530.8	325.7	42	1722
Accidents urban roads	1,125	1592.1	1155.3	72	5468

Observations are at the year-by-municipality level between 2011 and 2020 in rows 1-3 and at the year-by-province level between 2009 and 2020 (2009–15 for administration) in rows 4-13. Shares are out of total current spending.

that is likely to occur even in high-income democracies, where more severe forms of corruption are discouraged by the judicial system. There are two such types of connected municipalities: the municipalities where presidents were born and the ones where they are elected mayors. In the first case, presidents may want their own community to receive more resources. In the second, they may respond to an electoral incentive: remaining the mayor is necessary to stay in power in the province, and directing resources to their own voters may help accomplish this goal. In this case, presidents remain partially accountable to voters in their own municipality. These two types of connection have often coincided since the reform.

To shed light on this outcome, I investigate whether the reform increased the amount of resources directed to municipalities connected to the provincial president. Conceptually, before the reform, direct accountability forced presidents to equally distribute resources and effort across municipalities or to target the most responsive voters; since the reform, weaker incentives have allowed presidents to indulge more in hometown favoritism. In my main specification, I define the treatment group as the time-varying set of municipalities that are birthplaces of the individuals who are presidents in year t ; the control group is composed of all the remaining municipalities. My sample includes all municipalities between 2011 - the first year after a fiscal reform dramatically decreased the central government's transfers to municipalities - and 2019, except for autonomous regions and metropolitan cities.¹⁷ As Appendix Table B1 shows, treated municipalities are bigger than control ones, but they

¹⁷I also exclude provincial capital cities from the sample. Capitals tend to be bigger, and they receive far more

prove to behave similarly over time in the pre-period. Since popular elections were abandoned in April 2014, immediately affecting presidents' incentives, I consider 2014 as my first treated year. Results are robust to using 2015 or the year of the first postreform election instead. I thus write Equation (2) as follows:

$$Y_{m,t} = \alpha + \beta Treat_{m,t} + \delta(Treat_{m,t} * After_t) + X_{m,t} + \psi_{p,t} + \chi_m + \epsilon_{m,t} \quad (2)$$

where $Treat_{m,t}$ is an indicator equal to 1 if municipality m is the birthplace of the provincial president in office in year t , and $After_t$ takes value 1 for years after 2013. $Y_{m,t}$ is public transfers per capita to municipality m in year t ; $\psi_{p,t}$ and χ_m are province-by-year and municipality fixed effects, respectively. I thus exploit variation in incentives within provinces and across municipalities over time. I include controls $X_{m,t}$ for municipal population and for politicians' characteristics such as gender, age, education, and party. δ captures the effect of indirect elections on treated municipalities relative to the control group. I cluster standard errors at the municipality level.

The advantage of this specification is that the treatment group contains municipalities that are connected to presidents in the years in which they are in office, both before and after 2014. This allows me to observe whether presidents used to benefit municipalities they are connected to even before the reform and whether favoritism to connected municipalities increased. One disadvantage, however, is that the treatment and control groups are time varying. This could threaten my identification strategy if the reform also affected the characteristics of the municipalities that are connected to the president (for example, if presidents elected after 2013 were born in larger municipalities and, in turn, larger municipalities received more transfers per capita). I thus rely on a different and complementary identification strategy, that keeps the treatment and control groups consistent throughout the whole period. More specifically, I include in a time-invariant treatment group all the municipalities whose mayor became president of a province at some point after 2013, and I use the remaining municipalities as the control. The treatment period consists of the years after the first president is indirectly elected in a given province. A municipality is thus part of the treatment group from the beginning of the sample, before it forms any connection with the president, and it is considered treated when the first link with the presidency in the province is created.¹⁸ This transfers than the average municipality; moreover, they host the provincial government. For this reason they may be favored even by presidents born elsewhere. The trend of transfers in capital cities is shown in Appendix Figures B3 and B4. In Appendix Section B.1.4 I include capitals and show that results remain unchanged.

¹⁸For clarity, consider a province in which directly elected president Y, born in city A, was only in office between

definition of treatment closely proxies the use of the president's birthplace in my first strategy, but it allows me to seek evidence on the relative importance of electoral incentives in explaining hometown favoritism. After all, presidents remain directly accountable to the subset of citizens who live in the municipality that chose them as mayors. Unfortunately, testing a specification symmetric to Equation 2 is conceptually impossible in this case since presidents have the role of mayors only after 2013. I thus estimate the following regression:

$$Y_{m,t} = \alpha + \delta(Treat_m * After_{p,t}) + X_{m,t} + \psi_{p,t} + \chi_m + \epsilon_{m,t} \quad (3)$$

where $Treat_m$ is a time-invariant indicator equal to 1 if municipality m 's mayor becomes provincial president anytime after the reform, $After_{p,t}$ equals 1 following the first postreform presidential election in province p . Other terms are defined as in Equation 2.¹⁹

The preferred dependent variable in both Equations 2 and 3 is per capita transfers received by the municipality, which allows me to directly measure the degree to which hometown favoritism affects citizens' access to public resources. I also provide results normalizing by total revenues, rather than by population, and using the absolute amount. I use a logarithmic transformation of the outcomes to normalize their distribution and to reduce the weight of outliers while maintaining their relative position (I validate this strategy by applying a 5% winsorization to the linear version of the outcome).²⁰ My measure of municipal transfers is an aggregate one, 97% of which originates from public administrations. Transfers from the province only account for around 3%-5% of the total; however, it is common for provincial presidents to lobby other local administrations (for example, unions of municipalities and *comunità montane*) or higher-level administrative units such as the regional or central government to secure additional funds for some municipalities, and this

2010 and 2013, while indirectly elected president X, born in (and mayor of) city B, was only in office between 2014 and 2018. In the first identification strategy, city A is in the treatment group only between 2010 and 2013, while city B only between 2014 and 2018. In the second strategy, B is in the treatment group for the all time period, while A is always a control unit, since president Y, elected before 2014, is never a mayor.

¹⁹A drawback of this specification is that not all treated units are treated at time zero: in cases (about a third) in which a new president from a different municipality is elected in the second postreform election, the treatment for the newly linked municipality takes effect in period 3 or 4, thus underestimating the effect. I choose this specification because it allows me to define a common and meaningful treatment period for the control municipalities - namely, the year in which the first municipality of the province is treated - and to observe a sufficient number of pre- and post-treatment periods. Since presidents are reelected at most every four years, precisely defining the treatment for each municipality would only allow me to observe at most two pre- and two post-periods. To mitigate concerns, Appendix Section B.1.1 presents an event study without a control group (Equation B3 and Appendix Figure B8), in which every municipality is treated the first year in which its own mayor becomes president.

²⁰As with the use of logarithms, winsorizing reduces the weight of outliers, thus maintaining the observation and its relative position.

measure of transfers reflects this additional effort.²¹ Indirect elections also undermine the incentive of presidents to lobby high-level governments for resources to municipalities that may reward their effort more, once again to the benefit of the presidents' hometowns. Since local entities are more likely than national ones to be affected by a president's lobbying, Appendix Section B.2 uses data from SIOPE to show that results are robust to using current and capital transfers from local governments only (regions or lower) as an alternative dependent variable.

In this analysis, I am implicitly assuming that the control group is completely unaffected by the reform (the stable unit treatment values assumption). The assumption may be violated if the additional resources for the treated group came at the expense of the control group. In practice, the total amount of transfers is not fixed, as additional resources can be sent by the province or higher-level administrative units. Moreover, the fact that more than 98% of the municipalities are in the control group quantitatively alleviates this concern: even if additional funds to the treated units came at the expense of the control group, the average amount of transfers in the latter would barely be affected. My results should thus be interpreted as the effect on my specific treatment group relative to the control group.

5.2 Results

The impact of introducing indirect elections on the per capita amount of transfers to municipalities is reported in Figure 3. The time-varying treatment group is composed of the birthplaces of the provincial presidents in office. The figure shows a virtually perfect parallel trend in the years between 2011 and 2013, suggesting the treatment and control groups were extremely similar when presidents were directly elected. After the reform, though, the treated municipalities started receiving disproportionately more transfers compared to the control group and the gap kept widening until 2019. Importantly, treated municipalities were connected to the president before 2014 also; however, it is only after the reform that connections generated the increase in transfers. Table 3 tests these results more formally using Equation (2) and confirms that the reform significantly increased the amount of transfers to treated municipalities by 29% compared to the control group. This is robust to the inclusion of municipality and province-by-year fixed effects and to controlling for the municipal population size and politicians' characteristics.

A possible concern arising with this first identification strategy is that the composition of the

²¹An example is reported here

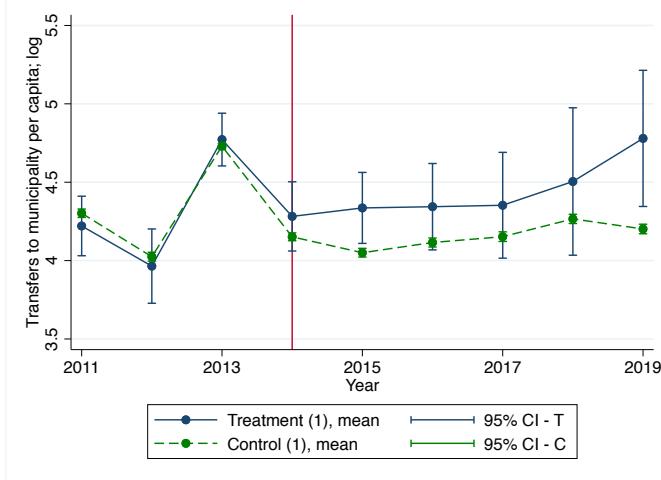


Figure 3: Log transfers per capita. The treatment group changes over time and is composed of the municipalities that are the place of birth of the president in office, excluding provincial capitals. Treatment period: 2014-2020.

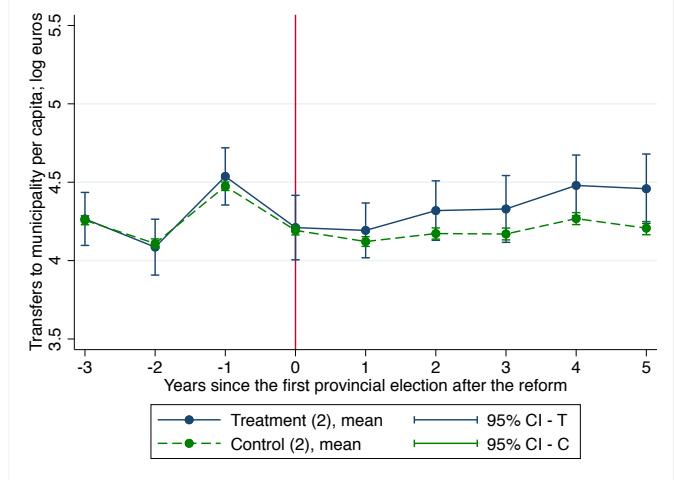


Figure 4: Log transfers to municipality per capita. Treatment group (time invariant): municipalities whose mayor eventually became president, excluding capitals. Reference year for treated and control municipalities: first postreform presidential election.

treatment group might change because of the reform in a way that is ex-ante correlated with the amount of transfers. For instance, if the new electoral law caused larger cities to be more represented among presidents' birthplaces, this could bias my results. Reassuringly, the trend in the number of inhabitants in treated and control municipalities remains similar before and after the reform.²² Nevertheless, to account for other types of endogenous selection into treatment, I introduce the results from my second identification strategy, whereby the treatment group is stable over time and is composed of the ever-treated municipalities - that is, those whose mayor became provincial president after 2013.

Figure 4 plots the second specification and shows a perfectly parallel trend in transfers per capita between the treated and control municipalities before a municipality forms a connection with the president. Things change right after the first connection, when the treated units start receiving disproportionately more funds. Columns 3 and 4 of Table 3 display the results from running the regression in Equation (3) and show that indirect elections significantly increase transfers to the treated group by around 13% compared to the control group. Once again, including controls for population size and politicians' characteristics does not substantially change my estimates. Since the treatment group is now stable over time, the effect must be driven by an increase in transfers obtained by municipalities after their mayor becomes provincial president, rather than by a different

²²See Appendix Figure B1, in which the treatment group is stable over time, and Appendix Figure B2, in which the small decrease in population in 2016 and 2017 is compensated for in the following years.

Table 3: Impact of the reform on the amount of municipal transfers

	(1) Log(transf. p.c.) (Birthplace)	(2) Log(transf. p.c.) (Birthplace)	(3) Log(transf. p.c.) (Mayor)	(4) Log(transf. p.c.) (Mayor)
Treatment*After	0.285*** (0.082)	0.313*** (0.081)	0.130** (0.058)	0.129** (0.056)
Treatment (time variant)	-0.166 (0.071)	-0.184*** (0.070)		
Controls	No	Yes	No	Yes
Municipality FE	Yes	Yes	Yes	Yes
Province-by-year FE	Yes	Yes	Yes	Yes
Observations	49,808	48,739	36,460	35,605
R-squared	0.750	0.751	0.770	0.770

The dependent variable is (the logarithm of) transfers to municipalities per capita. In columns 1 and 2 the treated municipalities are the birthplaces of the presidents in office each year; thus they change over time. In columns 3 and 4 the treated group is time invariant and consists of the municipalities whose mayor became president at some point after 2013; the sample is restricted to the period between three years before the election and five years after. Treated period: 2014-2019 (columns 1 and 2) and years after a mayor becomes president (columns 3 and 4). Controls include municipal population and mayors' characteristics (sex, age, education, whether they are left-wing). The coefficient of interest is that on the interaction term. The sample includes the 5,431 municipalities outside autonomous regions and metropolitan cities between 2011 and 2019; provincial capitals are also excluded. Robust standard errors, clustered at the municipality level, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

type of selection into treatment.²³ When estimating a slightly modified version of Equations (2) and (3) to obtain year-by-year coefficients conditional on my set of fixed effects, virtually all years are significant at the 10% level.²⁴ Similarly, results are confirmed when using slightly different dependent variables, such as absolute transfers, transfers normalized by total municipal revenues, or winsorized (not log) variables, or when using SIOPE's measure of transfers only from local governments.²⁵

Some readers may be concerned that the reform not only reduced accountability to voters but generated a new form of accountability to the new electors (mayors and councillors) who choose the president and thus fostered party favoritism (as in Curto-Grau et al.(2018)). In the new provincial elections, municipal politicians vote based on party membership and many of them are replaced every year, so presidents have very weak incentives to win their vote and to maintain their favor in the long run. Still, the provincial candidates may try to win their vote through promising additional funding for their cities or may simply prefer to help ideologically similar mayors. I test this hypothesis in Appendix Section B.1.2 by plotting the amount of funds received by cities whose mayors share their

²³The second specification alone would overestimate the effect if presidents *always* favored their hometowns. While in Equation (2) this issue is nonexistent since treatment municipalities are birthplaces of the presidents both before and after 2014, in Equation (3) a treatment municipality is only connected to the president after 2013. Reassuringly, Figure 3 shows that presidents' birthplaces were not disproportionately receiving more funds before the reform (Appendix Table B2 rejects this hypothesis), suggesting this mechanism cannot explain the results.

²⁴Appendix Figures B5 and B7 show year-specific coefficients for the two strategies that include fixed effects.

²⁵See Appendix Tables B3 and B4, Appendix Figures B13 and B14 and Appendix Section B.2.

ideology with the president. Under this scenario, we expect more transfers to flow to the aligned municipalities. Instead, we see no divergence in transfers received, suggesting that the presidents do not favor their ideologically similar new electors. A second concern is that the possibility of rising to the provincial presidency may affect the quality of those who run as municipal politicians. In Appendix Figure B15 I restrict the treatment group to presidents who were elected mayors before 2014 and thus whose decision to run as mayors cannot have depended on the reform. Despite the small sample, this robustness test confirms the general trend, suggesting that the results are not driven by changes in municipal selection connected to the reform.

Taken together, these results provide solid evidence that indirect elections allowed presidents to increase the flow of funds to municipalities closely connected to them, but the exact mechanism is still unclear. It may be that presidents have an intrinsic preference for hometown favoritism and it only manifests in action when accountability drops, or it may be that the overlapping of the roles of president and mayor generates an electoral motive (chasing reelection to the municipal office as a requisite for the presidency) and administrative opportunity (the overlapping of powers) to benefit specific places. Both mechanisms arise from the weaker accountability to provincial voters, but being mayor is a necessary condition only in the second one. To partially disentangle these two alternative explanations, I exploit the fact that in Italy it is not necessary to be born in a city to become its mayor. Figure 5 mirrors Figure 3 in showing the trend in transfers per capita to presidential birthplaces, but it excludes cases in which the presidents are mayors of their birthplace. Similarly, Figure 6 mirrors 4 in showing the trend in transfers after a city elects a mayor as president but excludes cases in which mayors govern their birthplace. There may be endogenous reasons for why a president falls into one of the two categories, but these figures suggest that the transfers to connected municipalities increase for both types of connections. Overall, both the stronger electoral incentives to please one's constituency and the inherent preference for favoring one's birthplace seem to help explain the increase in transfers to connected municipalities relative to unconnected ones. However, the effect is larger and more precisely estimated in the case of birthplace favoritism. This is in line with Fiva and Halse (2016), Baskaran and Lopes da Fonseca (2021), and Carozzi and Repetto (2016), and the favoritism may reflect presidents' personal ties or connection to their roots.

In conclusion, the loss of direct accountability caused presidents to switch from equally channeling available resources across all municipalities, potentially targeting citizens more likely to reward public spending with their vote, to distributing funds based on their personal connections.

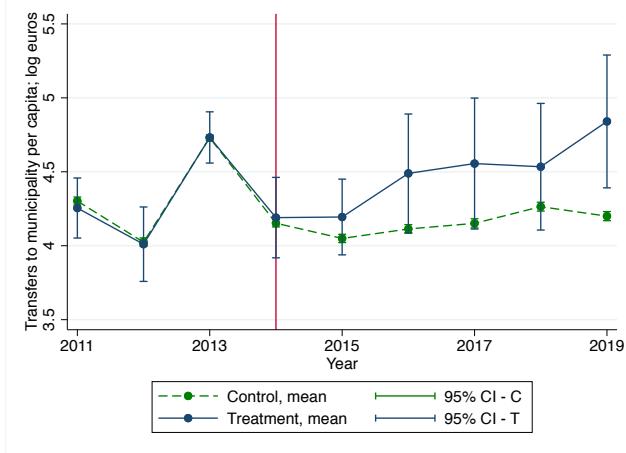


Figure 5: Logarithm of transfers per capita to municipalities. Treated municipalities are the birthplace of the presidents, excluding those where the president is mayor. Provincial capitals are not in the sample.

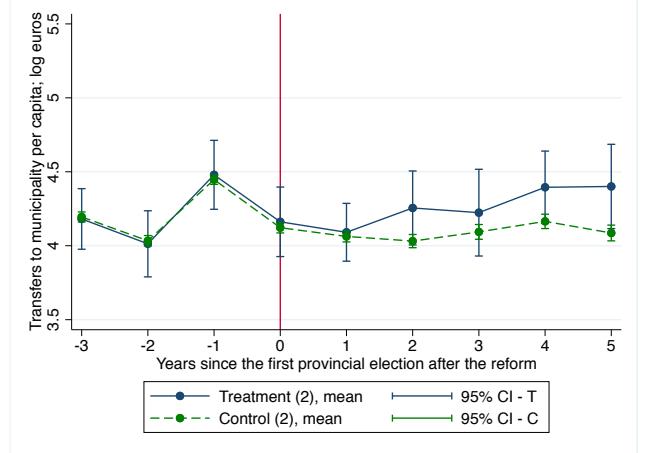


Figure 6: Logarithm of transfers per capita to municipalities. Treated municipalities are those whose president became mayor after the reform, excluding the birthplace of the presidents. Provincial capitals are not in the sample.

6 Indirect Elections and the Composition of Public Spending

6.1 Identification Strategy

A second possible way in which politicians may divert resources without transgressing the limits of legality is altering the composition of public spending in a way that they prefer. I thus assess the impact of less direct accountability on the composition of the provincial budget, whose three main components, accounting for 80% of current expenditure, are transport, education, and bureaucracy. To do so, I compare sectoral spending in the 94 provincial governments - my treatment group - to similar types of expenditure by municipal governments, which maintained direct election both before and after the reform. More specifically, for each real province, I construct an artificial control group by summing up spending by all municipalities within the province. Figure 7 shows an example of treated and control units. The two entities share the same borders (and population and wealth), but the reform only affected spending by the former, as nothing changed at the municipal level. Consequently, my dataset includes 188 units: the 94 provinces and the 94 artificial controls.

My identification assumption is that the trends in the outcome variables for the treated group would have followed the corresponding trends for the control group in the absence of the reform. The stable unit treatment values assumption requires that municipalities were not directly affected by the reform. Therefore, my identification requires that the law repealing direct elections caused

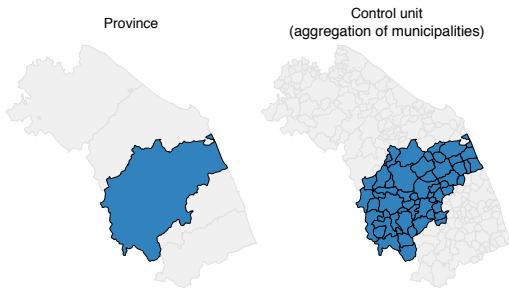


Figure 7: An actual province and its corresponding counterfactual province, the latter of which aggregates the former's municipalities

no changes in the responsibilities assigned to provinces or to municipalities.²⁶ The reform only marginally affected municipalities: first, by defining more stringent gender quotas for municipal executive committees, which anyway did not affect top-level positions (Spaziani (2022)); second, by incentivizing the mergers of very small municipalities, which were incentivized before the reform as well (and only about 2% of municipalities, with negligible budgets, merged after 2014); third, more indirectly, by slightly reorganizing the unions of municipalities, a long-existing intermunicipality partnership. Importantly, the 2015 report of the Council for Local Autonomies and wide-ranging journalistic evidence confirm that provinces remained in charge of the exact same functions as before the reform. Section 7 provides robustness tests to validate this.

For the comparison between provinces and municipalities to be meaningful, it is essential that they perform comparable functions; and they do. First, municipal and provincial public spending on transport is virtually identical, as these entities provide the same services - road maintenance and public transport (mainly buses) - but on different types of roads. The municipal spending concerns urban roads, while the provincial spending concerns provincial roads - that is, transport between municipalities. The two types of roads do not merely correspond to urban and rural areas: even two neighboring urban municipalities typically have a provincial road connecting their centers, and people living in urban areas have to use provincial roads to reach other urban areas. As for bureaucracy, the two functions are again very similar: in both municipalities and provinces this item

²⁶The findings described in Section 5.2, in which we saw that funds are disproportionately given to the municipality associated with the president, do not invalidate the use of municipalities as the control group because in the average province, only one out of eighty municipalities is associated with a president (and thus treated) each year such that the overall control group is hardly affected. Moreover, my main dependent variable is the share of resources allocated to each sector (rather than the level) to isolate the choice made by the local authorities from the changes in the total amount of available resources, which largely depends on higher levels of government.

captures expenditure connected to tax collection, functioning of institutional bodies, bureaucrats' salaries, and the like. Finally, the education category concerns spending on school maintenance and accessibility. In this case, municipal and provincial spending is complementary: they both concern the same school structures and work to ensure accessibility to disadvantaged citizens.

Overall, these functions are highly comparable, and even if some discrepancies do exist, they translate into a difference in the levels of spending rather than in the trends; and the levels are taken into account by the difference-in-differences strategy. In the resulting dataset, each treated province has a counterfactual with the same geographic area and socioeconomic characteristics but with public spending decisions made at a different institutional level. I thus estimate the following difference-in-differences equation, using actual provinces as the treatment group ($T_p=1$) and years after 2013 as treatment period ($After_t=1$):

$$Y_{p,t} = \alpha + \delta(T_p * After_t) + \eta_p + \chi_t + \epsilon_{p,t} \quad (4)$$

$Y_{p,t}$ is the outcome of interest (share of public expenditure in a sector, in most specifications) in province p and year t . η_p and χ_t are year and province fixed effects, respectively. The coefficient of interest is δ , which captures how provinces diverge from the control group after the reform. I cluster standard errors at the province level. The reform was implemented in April 2014, suggesting that 2014 might have been a transition year. I consider 2014 as the first treated year, but the results are robust to restricting the treatment period to 2015-20. In Appendix Section C.2 I also estimate the corresponding event-study equation with year-by-treatment interaction terms.

My main outcome variable is the share of total current expenditure allocated to the two main types of provincial public goods and to administrative costs. The choice to focus on shares is the natural one to shed light on the local political process of resource allocation. The overall amount of provincial expenditure in each sector would imprecisely reflect provincial politicians' decisions since the size of the budget strongly depends on the amount of funds that provinces exogenously receive from the central government and because provincial taxes are highly inflexible.²⁷ In contrast, the share of the budget allocated to specific sectors provides a better representation of how provincial politicians choose to allocate the available resources. A possible concern is that different sectors are

²⁷Since 2011, only 40% of provincial revenues has come from provincial taxes. Moreover, the main source of provincial tax revenue (accounting for more than 50% of total tax revenues) is a car-insurance tax, whose rate is anchored at 12.5% by the central government, though provinces can modify it by up to 3.5%. See Bracco and Revelli (2018) for a discussion of the source of provincial funds before 2011.

subject to different elasticities with respect to changes in revenues. If politicians had discretion only for some types of expenditure, while other types were exogenously fixed, changes in revenues might imply changes in shares of resources across sectors. Reassuringly, the share of expenditure across sectors remained constant between 2009 and 2014 (see Figures 8, 9 and 10), despite the continuing cuts in resources.²⁸ In any case, I show that the change in the level of expenditure qualitatively resembles that in the share.

6.2 Results

Expenditure on Public Goods

How did the introduction of indirect elections affect the provision of public goods? My main results are plotted in Figures 8 and 9, which show the yearly group means of the share of current expenditure allocated to the transport and education sectors, respectively, for the treated provinces and the control group. In both cases, the treated group's share sharply decreases with the reform, after at least five years with a constant trend that was parallel to the control group's trend. In contrast, the control group is unaffected. This is confirmed by Appendix Figure C3, which reports the coefficients of the year-by-treatment interaction terms with fixed effects.

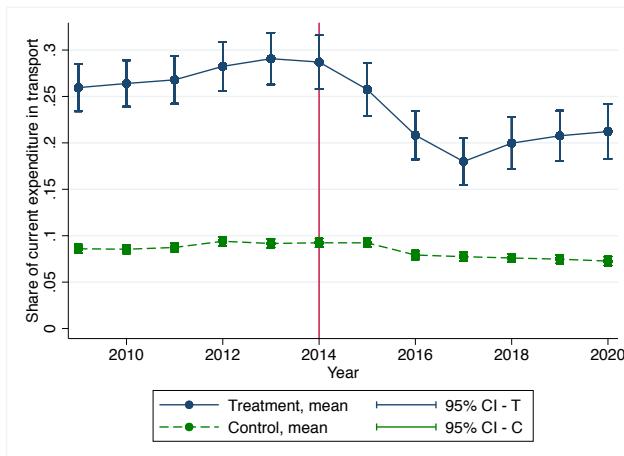


Figure 8: Trend of current spending on transport as a share of total current spending. Provinces (Treatment) and aggregated municipalities (Control).

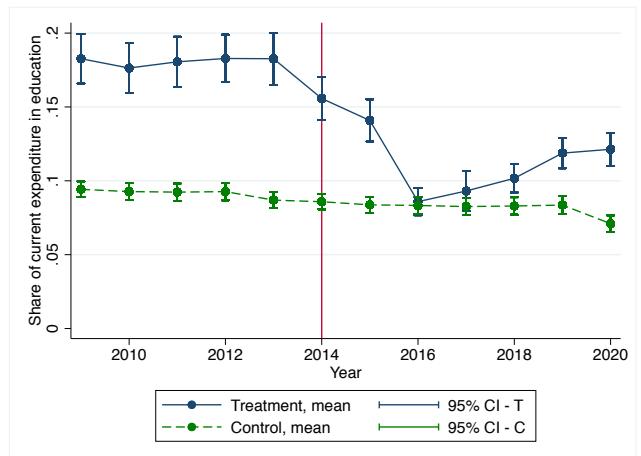


Figure 9: Trend of current spending on education as a share of total current spending. Provinces (treatment) and aggregated municipalities (control).

Table 4 tests this result more formally, using Equation 4. The coefficients of the interaction term in columns 1 and 2 are negative and highly significant. An advantage of the way I construct my control group is that for every treated unit there is a counterfactual that covers exactly the same geographic

²⁸Appendix Figures C7 and C8 show the continuous decrease in provincial budgets following the government's cuts.

area. Thus, all the geographic and socioeconomic time-varying unobservables are constrained to move in the same way in the two groups. While the share of expenditure in both sectors was in general falling after 2013, the sign and size of the interaction term confirm that the share fell far more in the treated group compared to the control. Overall, the reform caused a drop in the share of expenditure of about 3.9 percentage points for transport and 5.5 percentage points for education. On average this corresponds, respectively, to 14% and 29% reductions relative to the prereform mean.

Table 4: Impact of the reform on the share of expenditure on transport and education.

	(1) Transport share	(2) Education share	(3) Administration share
Treatment*After	-0.039*** (0.007)	-0.055*** (0.006)	0.102*** (0.007)
Province FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	2,227	2,227	1,307
R-squared	0.882	0.762	0.844

The dependent variables are current expenditure on transport (column 1), education (column 2), and administration (column 3) as shares of total current expenditure. The independent variables include a post-treatment dummy (After), a dummy for the treatment group (Treatment), and the interaction term; province and year fixed effects are included. In column 3, because of a change in the data structure, I take a conservative approach and only include years up to 2015, which explains the smaller sample size. Province and year fixed effects are included. Eleven autonomous provinces and metropolitan cities are excluded. Robust standard errors, clustered at the province level, are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

The trends in Figures 8 and 9 for the treatment and control groups are parallel but distant from one another, raising the concern that the shares of expenditure on transport and education in the control group are too low to decrease. However, the absence of any evidence of a reduction in the control group's shares for the two sectors, which together account for almost 20% of current municipal expenditure, alleviates this concern. In any case, Appendix Section C.1 discusses an alternative design in which I construct a synthetic control group for provincial spending by selecting municipalities with consistently high expenditures on education and transport. Results from this alternative strategy almost exactly match those in my main specification.

Expenditure on Bureaucracy

I now provide evidence that the drop in the share of resources allocated to public goods was offset by a fast rise in administrative costs, the only sector whose budget share increased. Administration is one of the categories in which AIDA PA divides provincial ordinary spending, and it includes a wide

range of tasks that are commonly associated with bureaucracy. These include the costs of collecting taxes, functioning of political and bureaucratic bodies, benefits for bureaucrats, and (nonsectoral) transfer of funds outside the province,²⁹

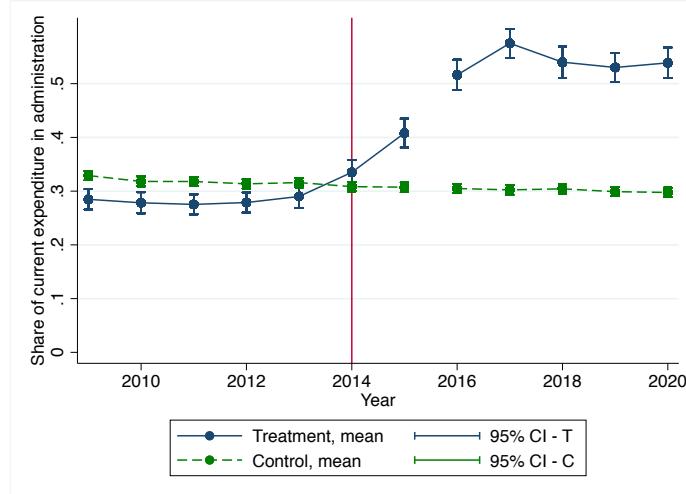


Figure 10: Bureaucratic current spending as a share of total current spending. In 2016 the category was relabeled. I thus break the line, as I cannot exclude the chance that small discrepancies may have arisen. Provinces (treatment) and aggregated municipalities (control)

The trends in the share of expenditure allocated to this category is shown in Figure 10. Not only are the lines of the treated and control groups parallel and constant before 2014, but they are also very close to one another, with a share of expenditure around 30%. Starting in 2014, however, a rise in the share allocated to bureaucratic costs is evident. The results are confirmed by Appendix Figure C4, which plots the coefficients of the year-by-treatment interaction term conditional on fixed effects. Similarly, column 3 of Table 4 shows the results of Equation 4 using bureaucratic costs as the dependent variable. The interaction term is positive and highly significant even if, because of a slight redefinition of the category by AIDA, I only include two post-treatment years.³⁰ The reform caused a ten percentage-point increase in the cost share of bureaucracy, which corresponds to a 30% increase relative to the prereform mean. Appendix Section C.5 uses AIDA and SIOPE data to provide further insights into the origin of the increase; it suggests that the results are driven by extraordinary current costs (a category comprising a broad variety of short-term unexpected

²⁹Other subcategories include general secretariat, personnel and organization, management of public-owned land, economic management, technical and statistical office, elections, and other general services.

³⁰In 2016 the administrative sector in AIDA PA was relabeled “General Institutional Services.” The new label includes virtually the same categories of bureaucratic expenditure; nevertheless, I exclude observations after 2015 from the main analysis to avoid any risk of generating an artificial discontinuity. In Figure 10 I highlight the change by breaking the line after 2015. In any case, even data between 2013 and 2015 are very suggestive of a strong postreform effect.

costs, such as theft, damages, unexpected bonuses, and failure to collect credits) and administrative transfers (residual transfers classified by AIDA as unrelated to other categories such as transport or education) to private and non-central-government public entities.

A strength of the analysis presented so far is that the prereform shares of expenditure remained constant despite significant declines in provincial revenues. This suggests an equilibrium in which a virtually fixed share of resources was consistently allocated to each sector. This abruptly changed after the introduction of indirect elections, when a larger share of resources was spent on bureaucracy.

Expenditure in Levels

A closer look at the absolute amount of spending in different sectors provides a more complete picture of the dynamics in place. After years of moderate reduction in the level of provincial expenditure on both public goods and bureaucracy, the latter started increasing in 2014.

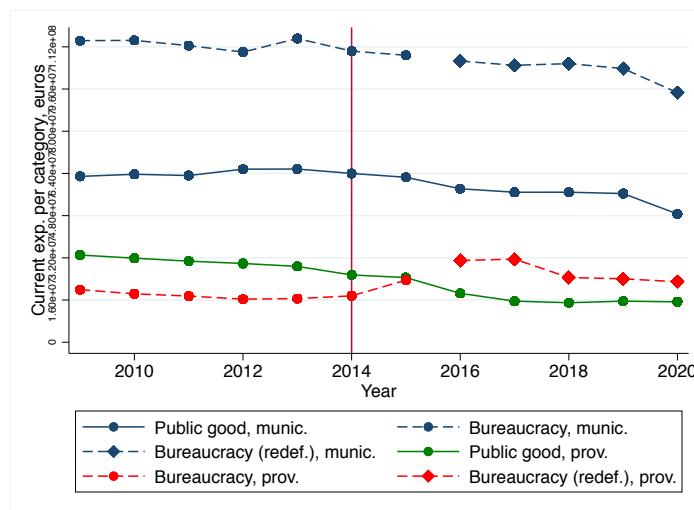


Figure 11: Level of spending in public good (transport, education) and bureaucracy; the latter is broken to highlight the slight redefinition in 2016 discussed in footnote 30

Specifically, Figure 11 shows that the absolute rise in provincial bureaucratic costs was sufficient to compensate for the reduction in public-goods spending, which accelerated following the reform, and to generate an increase in total spending.³¹ The reform had a negative and highly significant impact on the level of expenditure both on transport and education as confirmed by the standard difference-in-differences equation in Appendix Table C2. The same pattern is not evident when we focus on municipal spending on bureaucracy and public goods (the blue lines): in this case, the

³¹The overall number of provincial personnel fell considerably, but the fall may have been greater among employees involved in providing public goods and smaller among bureaucrats. Given the results in Appendix Section C.5 I conjecture that less concern for inefficiencies and more clientelistic transfers have played an important role.

two lines remain parallel. The dynamics in levels are thus consistent with the analysis of shares of expenditure and inconsistent with the hypothesis that results regarding shares simply reflect different elasticities with respect to budget cuts between bureaucratic costs and public-goods provision.

Quality of Services

I now look at trends of car accidents on provincial and municipal roads - which I take as a proxy for road maintenance - to investigate whether the reduction in the share of provincial resources allocated to transport had consequences for the quality of public goods provided. On average, there are about three times more accidents on urban roads than provincial ones; thus, to make the trends more comparable, I plot the logarithm of this variable in Figure 12. Before the reform, the trends are decreasing and virtually parallel for the two groups; starting in 2014 the number of accidents keeps decreasing on urban roads but starts to increase on provincial roads. More formally, I estimate Equation 4 using the absolute number of accidents and its natural logarithm as dependent variables and display the results in Table 5. Columns 1 and 2 confirm a positive and significant impact on the number of accidents. These results suggest that the shift in resource allocation negatively affected the quality of provincial roads.

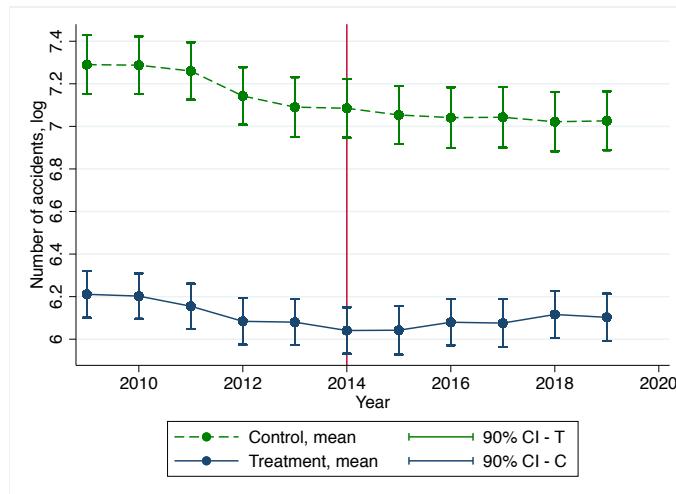


Figure 12: Logarithm of the number of accidents. 2020 is excluded because of COVID-19 lockdowns.

7 Alternative Explanations and Robustness Checks

The crucial assumption underlying both sets of results is that the reform has affected the functioning of the provincial governments only through the change in the electoral system. Indeed, the goal of

Table 5: Impact of the reform on the number of accidents

	(1)	(2)
	Accidents	Log(Accidents)
Treatment*After	235.202*** (22.785)	0.101*** (0.017)
Province FE	Yes	Yes
Year FE	Yes	Yes
Observations	2,062	2,062
R-squared	0.985	0.983

The dependent variables are the number of accidents (column 1) and the logarithm of the number of accidents (column 2) on provincial roads (maintained by provinces and thus treated) and urban roads (maintained by municipalities). Province and year fixed effects included. Treatment period: 2014–19. Autonomous provinces and metropolitan cities are excluded. Robust standard errors, clustered at the province level, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the Delrio Act was to reduce the cost of provincial representation until the upcoming constitutional amendment would allow the central government to dissolve provincial governments. While no other reform directly targeted provinces in the same period, some other characteristics of the Delrio Act could represent a threat to my identification strategies.

A change in responsibilities assigned to provinces represents the main threat to the results in Section 6. The reform specifically listed public transport and maintenance of road and school infrastructures as the main provincial responsibilities; however, it allowed agreements between provinces and regions to redefine how exactly to allocate responsibilities. While many journalistic sources confirm that responsibilities went unchanged, some anecdotal evidence gives cause for concern. For instance, in 2017 the region of Lombardy committed to turning 200 km of provincial roads into regional roads by 2018. This promise was not fulfilled, and those roads remained under the jurisdiction of the provinces.³² Reassuringly, according to the report released in the end of 2015 by the Council for Local Autonomies, among all regions only Marche and Umbria as of that date had transferred any responsibilities to the region in the main sectors (transport and education, respectively), and excluding them does not change my results. As for the residual sectors (see the

³²Over time some provinces delegated part of the responsibility for bus-related transport to companies, maintaining only partial control over the companies. Separating spending on public transport from that on road maintenance, I find an effect on both, suggesting that the outsourcing of public transport is not driving the results.

trend in Appendix Figure C5)), only minor ones such as cultural heritage, fishing, and hunting occasionally were transferred to the regions, and, if anything, this should mechanically raise the share of resources allocated to education and transport. To the best of my knowledge no other major transfer of duties happened; moreover, in Appendix Section C.7 I discuss suggestive evidence on regional spending that looks incompatible with a transfer of duties to the regions.

A second threat, for the analyses in both Sections 5 and 6, is the change in the size of provincial councils. The reform prescribed a consistent reduction of councils' size, from a range of nineteen to thirty-six councillors to a range of ten to twenty-four, depending on the provincial population. A possible alternative explanation for my results is that smaller councils are less able to constrain presidents or more prone to collude with them. I address this threat in two ways: First, I look at a similar policy that reduced the council size by 20% in 2011. If this alternative explanation is true, we should expect to see in 2011 a similar effect to what we observe in 2014. But no figure in Section 6 shows any evidence of such an effect. In order to assess the presence of an earlier divergence for the results in Section 5, in Appendix Section B.1.4 I replicate my analyses on a longer period, including all years between 2007 and 2020. Appendix Figure B19 shows no evidence of early divergence of presidents' birthplaces compared to the control group. We do see small evidence of divergence in Appendix Figure B18 when using a sample that includes provincial capitals, but Appendix Section B.1.4 discusses extensively why the pre-trend is of minor concern. Finally, to further investigate the role of council size in birthplace favoritism, I directly test whether favoritism decreases with council size. To do so, I exploit the provincial population thresholds at which the number of councillors increases and construct a regression discontinuity design using transfers to presidents' birthplace as the dependent variable. Appendix Figure B16 and Appendix Table B5 show that the decrease in council size does not affect transfers at all; in fact, the point estimate goes in the opposite direction.

Another possible confounder is the reduction in presidential salaries brought about by the reform, which may have worsened politicians' quality and incentives as suggested in Gagliarducci and Nannicini (2013). I have already argued against the conjecture that selection of politicians worsened after the reform; but it is harder to rule out the role of reduced wages in weakening politicians' incentives. I address this concern in two ways: First, I exploit the fact that the reduction in salaries was reversed in 2020, when presidents' wages were reset at a level comparable to the prereform period. If the change in salaries was a main determinant of my results, we would expect the effect

to be mostly offset in 2020, but none of the figures in Section 6 show any evidence of reversal, making this explanation unlikely.³³ Second, I run a regression discontinuity design exploiting other provincial population thresholds, focusing now on the cutoffs used to determine presidential wages before the reform. Appendix Figure B17 and Appendix Table B6 show that before the reform, higher wages were not associated with a significantly lower amount of transfers to presidents' birthplaces.

An alternative explanation for the differential impact of the reform on the share allocated for public goods and administrative costs points to a different elasticity with respect to changes in total revenues for these two sectors. If a fixed amount of resources needs to be spent on bureaucracy, while cuts in public services are feasible, a reduction in total revenues will increase the share spent on bureaucracy (even holding its level fixed) at the expense of public goods. Several pieces of evidence suggest this was not the case. First, the *level* of bureaucratic costs also increased after 2013. Second, the amount of resources did not decrease in 2015, as shown in Appendix Figure C7.³⁴ Third, the trend in the prereform share of expenditure in the different sectors is virtually constant between 2009 and 2013 (as shown in Figures 8, 9 and 10)), even if the overall amount of resources and current expenditure dropped by about one-quarter in the same period. That resources increased in 2015 while the level of expenditure on public goods kept falling confirms that the additional resources were wasted on less efficient bureaucracy.

Appendix Section C.6 discusses a wide range of additional robustness checks for the findings in Section 6. To sum up, the results are robust to computing shares of total revenues instead of expenditure, including 2014 in the preintervention period or dropping it, and dropping years after 2015, and they cannot be explained by a switch from current to capital expenditures.

A final issue is that my results might be driven by an unobservable but more accurate selection by municipal politicians, who are - one could argue - more informed than the average citizen on how to spend resources. One could thus speculate either that more informed politicians choose the president among those mayors coming from municipalities in need of more resources or that mayors who become presidents are more informed of available uses for funds. However, Figure 4 rules out

³³More specifically, presidents' wages were set to equal those of the mayor of the largest city in the province (which roughly equal the presidents' prereform wages). Thus, presidential wages rose in general but remained stable for presidents that were mayors of province-capital cities. A separate look at the dynamics of this supposedly unaffected subgroup of treated cities shows that the gap in transfers with the control group also closed for them (figures available upon request), suggesting that what leveled out municipal differences in the amount of transfers in 2020 was the extraordinary response to COVID-19 rather than the raise in presidential wages.

³⁴The increase of resources in 2015 shown in Appendix Figure C7 may be artificially inflated by the presence of transfers to the state, discussed in detail in Appendix Sections C.5 and C.6. However, the spike remains after dropping the provinces with large transfers to the state.

the possibility that treated municipalities were special and that they were already receiving more funds before 2014, when they were not associated with a president. Similarly, Figure 5 shows that favoritism dramatically increased, even after excluding presidents who are mayors of their hometown, making the second explanation less plausible. Thus, a threat to my identification only exists either if municipal politicians anticipate that a city will receive extra funds for some events and therefore select its mayor as president to better deal with it or if they know that a city has been in need of resources and elect its mayor as president to favor it. Since presidents did not provide more resources to their hometowns before 2014 (see Figure 3), the latter case implies that municipal politicians anticipated that, after the reform, the president would indulge in such favoritism. Thus, the conclusion does not change substantially: the reform did increase regional favoritism. The former case is harder to rule out, but, to the best of my knowledge, there is no evidence of parties choosing their candidates on the basis of where future events are located. In any case, this alternative explanation entirely fails to explain the second part of my results - namely, the increased expenditure on bureaucracy at the expense of public goods, which is only consistent with a less efficient administration.

Similarly, one could deem my conclusions in Section 6 somehow normative and see the growth of the bureaucratic sector as not necessarily problematic in the absence of direct evidence of reduced quality in publicly provided services. However, the expansion of administrative costs, both in relative and absolute terms, at the expense of sectors that more directly provide public goods to the citizens and the evidence on road quality indicate that, in the absence of effective political control, public goods may end up being more poorly provided.

8 Conclusion

This paper has analyzed the consequences for public spending of an electoral reform of the Italian provinces, which reduced politicians' accountability to their citizens by getting rid of popular elections. Direct elections of provincial leaders were replaced by an indirect mechanism whereby municipal politicians elect provincial ones and only municipal mayors are eligible as provincial presidents. My results show that the reform generated significant inefficiencies in the provision of public goods. First, I found that, with indirect elections, municipalities connected to the president receive 10% to 30% more public transfers compared to other cities. Second, analyzing how the composition of provincial expenditure changed after the reform, I found that the share of expenditure

allocated to public goods fell by 3.9 to 5.5 percentage points (depending on the sector), while the share allocated to bureaucracy increased by 10 percentage points. A very similar dynamic applies for the absolute level of spending. My results are consistent with my model, which predicts higher rent-seeking due to reduced salience of the electoral process. Indeed, in developed democracies, in which an independent judiciary system punishes and partly prevents the most severe types of corruption, politicians may choose more subtle forms of rent-seeking such as hometown favoritism and increased bureaucratic costs.

The reform allowed me to assess the impact of a transition from direct to indirect elections in isolation from any other constitutional change (such as an accompanying passage from a presidential to a parliamentary system). I can thus identify the negative impact of the end of direct popular election of representatives without confounding it with a complete change of the governing system. Introducing direct elections in a context in which political appointment is the status quo leads to changes in both the selection of politicians and their incentives. Directness increases the salience of the process and forces politicians to change their behavior to seek citizens' approval; at the same time it is possible that compared with politicians, citizens can choose better or more representative types of candidates. The peculiar reform of the Italian provinces greatly changed politicians' incentives while maintaining direct municipal selection for eligible provincial candidates, suggesting that the negative impact of the reform is more compatible with a change in incentives than a change in selection. An analysis of politicians' observable characteristics confirmed this hypothesis.

Government may consider introducing indirect elections to reduce political business cycles in public spending or to save on public resources spent on general elections. However, my results suggest that indirect electoral laws reducing the salience of the electoral process may severely backfire, reducing politicians' accountability and causing misallocation of public funds. Future research should assess whether the introduction of indirect elections limited the rise in citizens' polarization or politicians' incumbency advantage. Assessing the evolution of citizens' approval of indirectly elected leaders would also be crucial for understanding whether this type of reform affects institutional legitimization and consequently citizens' behaviors such as protests and tax evasion.

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Appendix

A Salience, public attention and politicians' characteristics

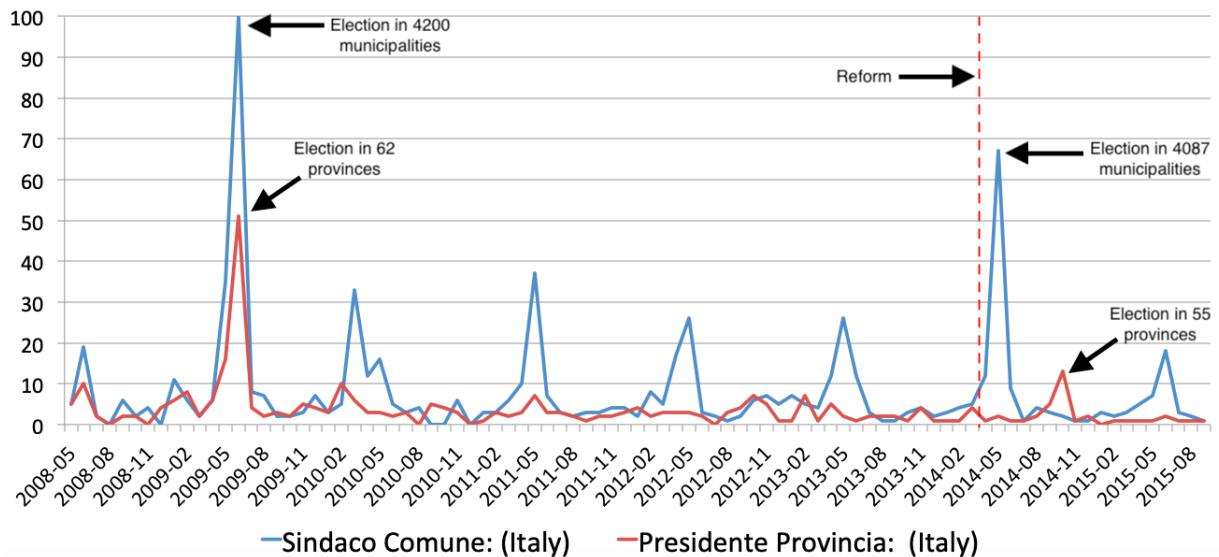


Figure A1: Trend of the Google web searches (news) of the words *Province & President* (red) or *Municipality & Mayor* (blue). All searches are proportional to the month with more searches (that is June 2009, for *Municipality & Mayor*). The vertical dotted line indicates the reform. Each blue peak coincides with a municipal election. No peak in searches for Province-related terms takes place during municipal elections after the reform, suggesting that the future provincial vote is not a salient topic during the municipal electoral campaign. Moreover, the peak during provincial elections is way smaller compared to the pre-reform period, despite de-synchronization should avoid crowding out searches. Overall, indirect provincial elections are less salient after the reform.

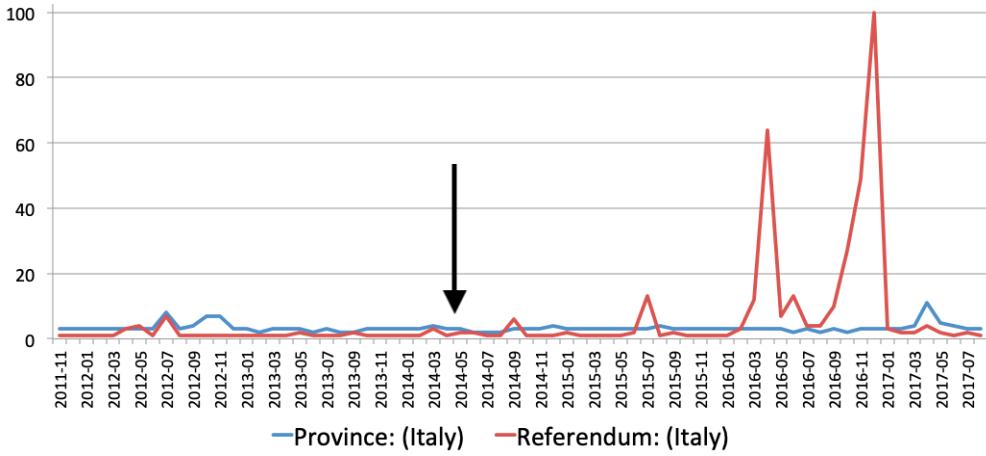


Figure A2: Trend of the searches of the word *province* (red) and *referendum* (blue) in Google. Both lines are normalized with respect to the highest concentration of searches in this period (here *referendum* in November 2016). The lack of any peak in the blue line (even relatively to other existing peaks in the blue line itself) when the reform passed in April, 2014 confirms the lack of public attention.

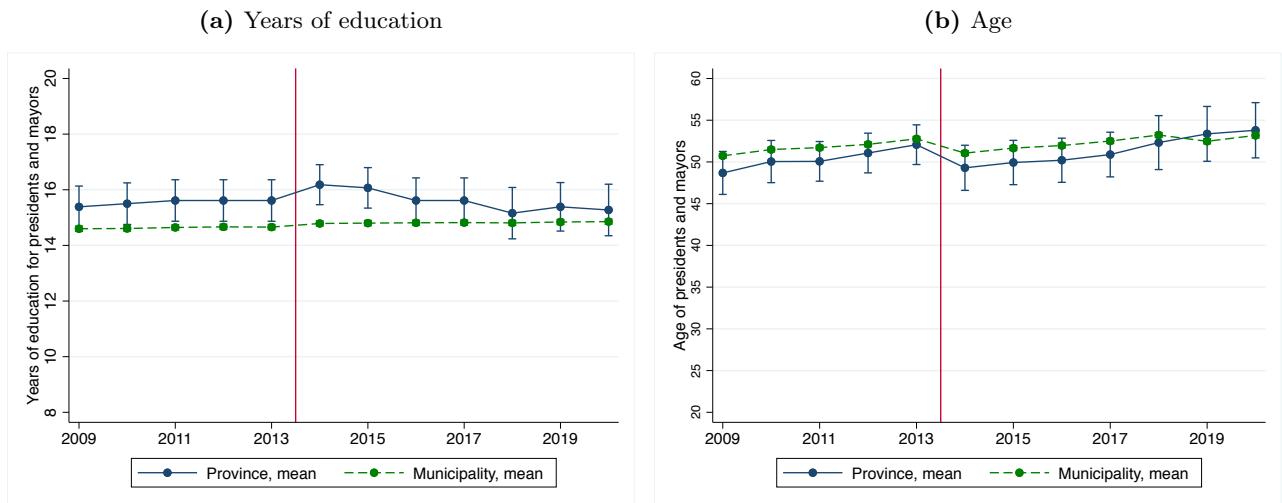


Figure A3: Provincial and municipal politicians' characteristics over time (age and years of education).

B Indirect elections and geographic favoritism

B.1 Presidents and mayors: Transfers to municipalities

Table B1: Summary statistics. Presidents' municipal offices and presidents' birthplaces vs control municipalities.

	President-mayor Municip.			Control Municip.		
	Mean	(SD)	Median	Mean	(SD)	Median
Population	12,202	(14,316)	7,312	4,571	(6,935)	2,235
Observation		1,108			52,038	

	Birthplace Municip.			Control Municip.		
	Mean	(SD)	Median	Mean	(SD)	Median
Population	16,540	(17,385)	10,100	4,583	(7,038)	2,206
Observations		350			55,222	

Sample: 2011-2020. Std deviations in parenthesis.

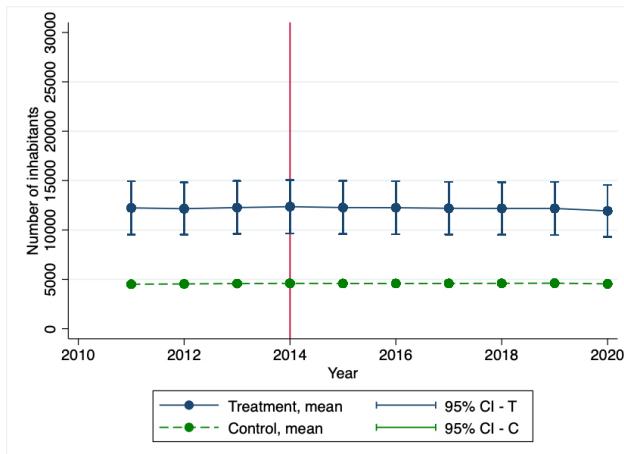


Figure B1: Number of inhabitants in the treated (municipalities whose mayor became presidents at any point after 2013) and control municipalities.

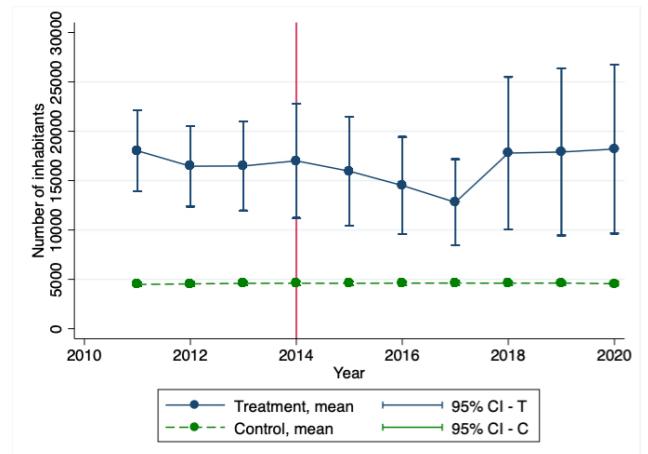


Figure B2: Number of inhabitants in the treated (where the president was born) and control municipalities.

Table B2: Balance table: absence of pre-reform geographic favoritism. T-test on the equality of means.

	Birthplace municipalities			Control municipalities			Difference	(SE)
	Mean	(SD)	Obs.	Mean	(SD)	Obs.		
Log(Transfers p.c.), 2011-2013	4.29	(0.72)	118	4.35	(0.99)	16,660	0.06	(0.09)

The table tests whether (log) transfers per capita before the reform are different between the treatment group (the presidents' birth-municipality, in the years in which they are in office) and the remaining municipalities. Capitals excluded. Std deviations or std errors in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

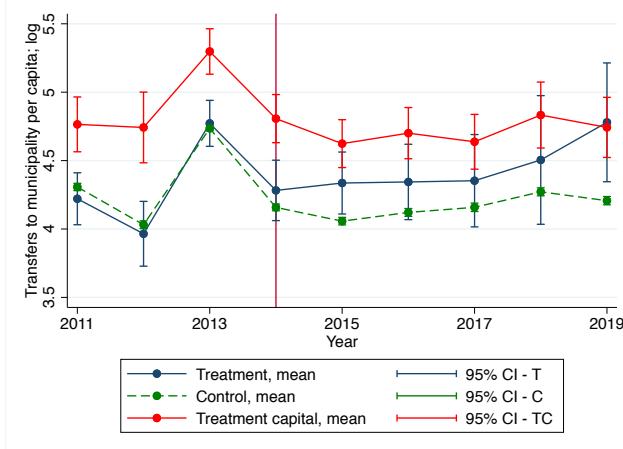


Figure B3: Current transfers per capita to municipalities. Treatment group: municipalities that are birthplace of the provincial presidents. The red line corresponds to treated units that are also provincial capitals (these are excluded in the main analysis).

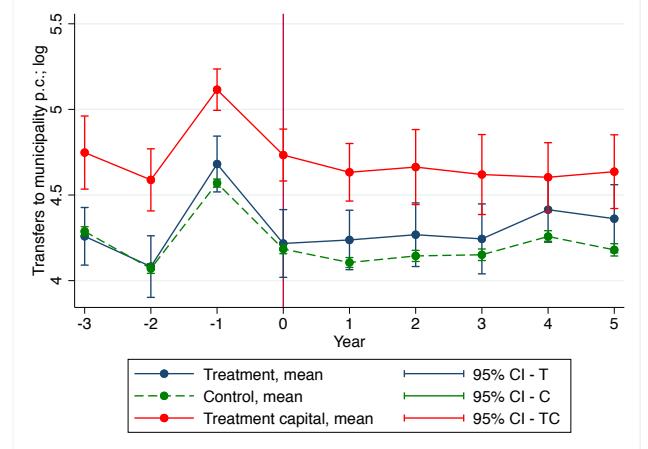


Figure B4: Current transfers per capita to municipalities. Treatment group: municipalities where the provincial president is mayor. The red line corresponds to treated units that are also provincial capitals (these are excluded in the main analysis).

B.1.1 Event Studies

In this section I report some event-study approaches aimed at exploiting the exact timing in which a connection between a city and a president becomes effective. Equation (B1) replicates the structure of Equation (2), but reports the coefficient on each post reform year, that is:

$$Y_{m,t} = \alpha + Treat_{m,t} * \sum_{t=2011}^{2019} year_t \gamma_t + \chi_m + \psi_{p,t} + \epsilon_{m,t} \quad (B1)$$

where all variables are defined as in Equation (2), and $year_t$ is a set of year-specific dummies. The result, plotted in Figure B5, shows a positive effect for all the post-reform periods. The coefficients are always significant at the 10% level, with the exception of 2017.

While the birthplace of a provincial president is always identifiable, presidents are chosen among mayors virtually only after the reform. In Equation (B2) I thus construct my time-invariant treatment group as those municipalities whose mayor eventually becomes provincial president (after

the reform) and I define the *event*, common for all the municipalities in the province, as the first time a mayor is elected president after the reform. This allows me to use all non-treated municipalities of the provinces as a control group and compare them to the treated group in the same year in which they are treated. I thus run the following specification:

$$Y_{m,t} = \sum_{x=-3}^{+5} \gamma_x \mathbb{1}\{TimeToEvent_{t,p} = x\} * Treat_m + \chi_m + \omega_{p,t} + \epsilon_{m,t} \quad (B2)$$

where $TimeToEvent_{p,t}$ measures time relative to the first election of a post-reform provincial president p , after the reform. Figure B7 plots the coefficients from Equation (B2), showing that even with this strategy, we have strong evidence of a sharp increase in resources once a municipality established a connection with the provincial president. The drawback of this strategy is that, around a third of the treated municipalities are effectively treated when $TimeToEvent_{t,p} = 2, 3, 4$. These are cases in which at the first post-reform reelection of the president, a new president, mayor of a different municipality is elected. This choice is driven by the need to define an event time for the control municipalities and a sufficiently long pre- and post treatment period. To have a sense of the size of the effect when defining the correct timing for each municipality I rely on a pure event study strategy, with no control group, whereby I only keep ever-treated units and define the event for each municipality as the year in which its mayor is elected president:

$$Y_{m,t} = \sum_{x=-3}^{+5} \gamma_x \mathbb{1}\{TimeToEvent_{t,m} = x\} + \chi_m + \omega_{p,t} + \epsilon_{m,t} \quad (B3)$$

where everything is defined as in equation(B2) but $TimeToEvent_{t,m}$ can vary among different municipalities within the same province. Results, plotted in Figure B8, show a stark increase in the point estimate right after treatment and similar positive coefficients (a 10% effect), standard errors however are significantly larger due to the reduction in sample size.

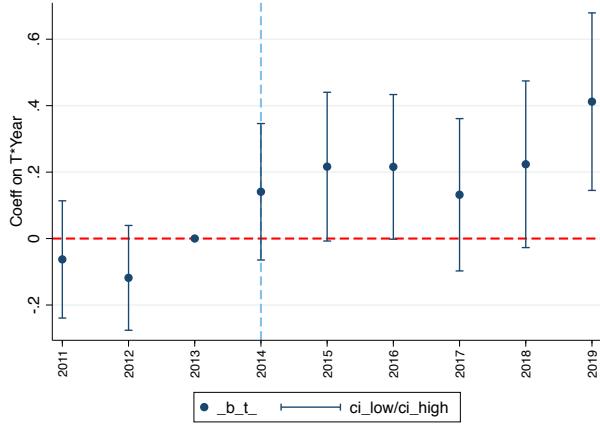


Figure B5: Strategy 1: (log) transfers to municipality per capita. Treatment group (time-varying): municipalities that are birthplace of the president. The regression includes year, municipality and province-by-year fixed effects. Cluster at municipal level. Province capital excluded. 95% confidence intervals.

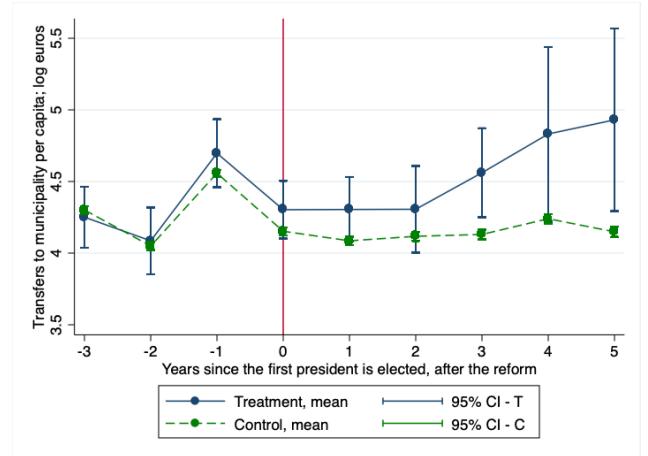


Figure B6: (Log) transfers per capita to municipalities. Treatment group: municipalities that are birthplace of the provincial presidents in office. Reference year for treated and control municipalities: when the first post-reform president is elected. Provincial capitals are excluded.

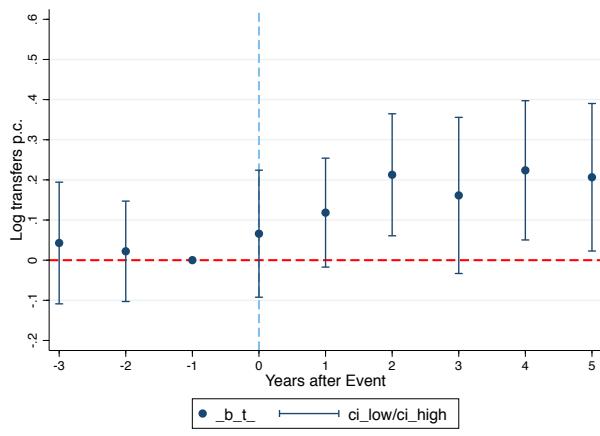


Figure B7: Strategy 2: (log) transfers to municipality per capita. Treatment group (time-invariant): municipalities whose mayor eventually becomes president. Event defined as the first year a mayor in the province is elected provincial president. Province-by-year and municipality fixed effects included; cluster at the municipal level. 95% confidence intervals.

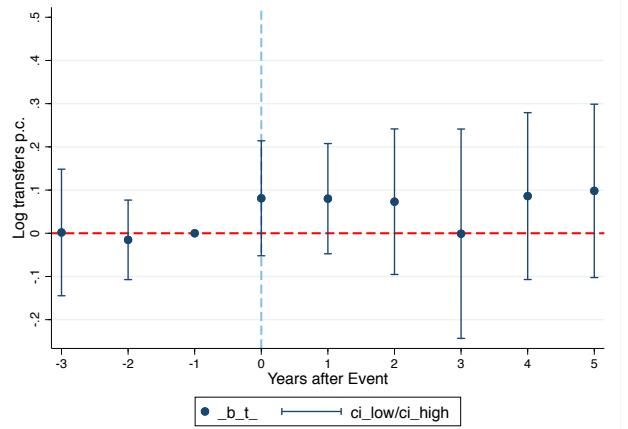
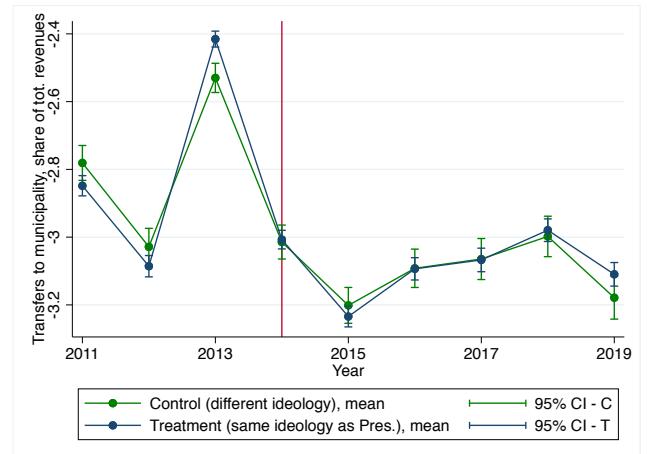
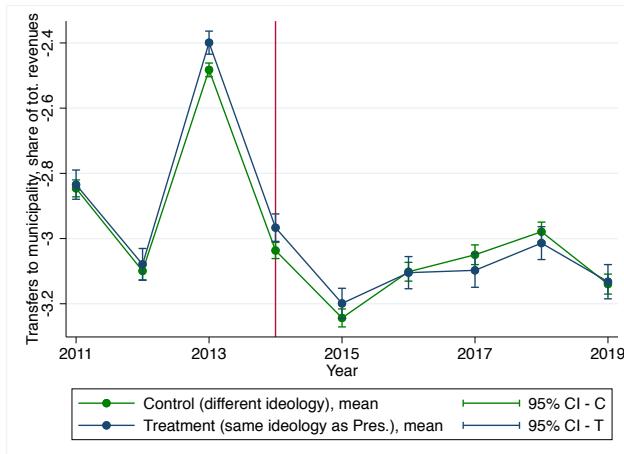
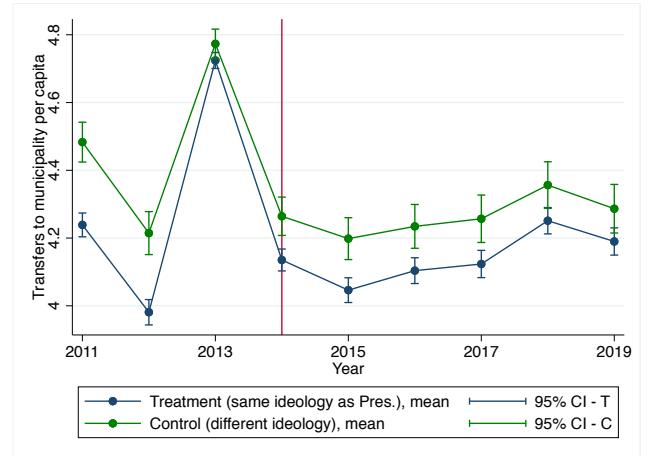
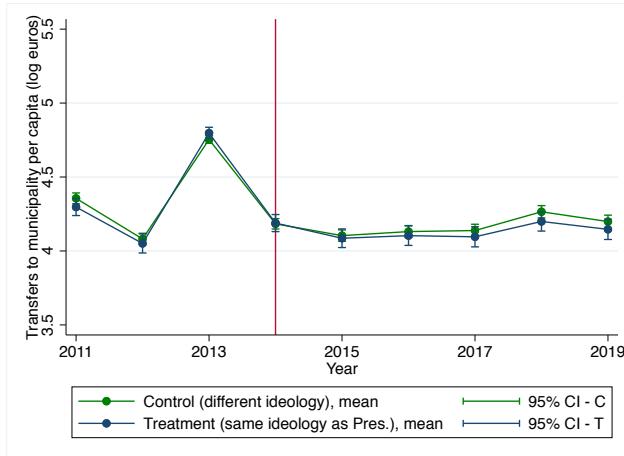


Figure B8: Event study: (log) transfers to municipality per capita. The sample only includes ever treated municipalities whose mayor eventually becomes president (no control group). The event is the first year the municipality's mayor becomes president. Province-by-year and municipality fixed effects; cluster at municipal level. 95% confidence intervals.

For completeness, Figure B6 shows results from the equivalent of Equation (B2), but defining connections based on birthplaces: each ever-treated (and control) municipality's first treatment period is the first year in which a post-reform president is elected in the province. Results are qualitatively identical to my main specification, but a slight pre-trend is visible, suggesting presidents started misbehaving as soon as the incentives dropped, even before the new election.

B.1.2 Transfers to Municipalities sharing ideology with the president



B.1.3 Robustness tests and placebos

Table B3: Other outcomes. Treatment group: presidents' birthplace municipalities.

	(1)	(2)	(3)	(4)	(5)	(6)
	(Log) total transfers	(Log) total transfers	(Log) transfers share	(Log) transfers share	Transfers p.c. (winsor.)	Transfers p.c. (winsor.)
Treatment (time-variant)	-0.172** (0.079)	-0.178** (0.071)	-0.137 (0.084)	-0.130* (0.073)	-16.712* (9.399)	-17.400** (7.630)
Treatment*After	0.343*** (0.091)	0.300*** (0.082)	0.288*** (0.092)	0.254*** (0.083)	36.748*** (11.246)	32.943*** (9.024)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Province by year FE	No	Yes	No	Yes	No	Yes
Observations	49,811	49,811	49,811	49,811	49,904	49,904
R-squared	0.840	0.860	0.597	0.646	0.707	0.745

The dependent variables include the logarithm of total municipal transfers (col. 1-2), the logarithm of transfers as a share of municipal revenues (col. 3-4) and municipal transfers per capita in absolute terms, winsorized at the 5% level (col. 5-6). The unit of observation is municipality-year, and the sample only includes 5,431 municipalities not in autonomous regions or metropolitan cities. Treated municipalities (time-varying) are those that are birthplace of the presidents in office in that year. Sample period: 2011-2019 (treatment: 2014-2019). Robust standard errors, clustered at the municipality level, are in parentheses, * p<0.10, ** p<0.05, *** p<0.01.

Table B4: Other outcomes. Treatment group: municipalities with eventually a mayor as president.

	(1)	(2)	(3)	(4)	(5)	(6)
	(Log) Total transfers	(Log) Total transfers	(Log) Transfers share	(Log) Transfers share	Transfers p.c. (winsor.).	Transfers p.c. (winsor.)
After	0.0618*** (0.0141)		0.0232 (0.0145)		5.178*** (1.368)	
Treatment*After	0.155*** (0.0582)	0.141** (0.0575)	0.150*** (0.0553)	0.129** (0.0565)	11.08* (5.706)	10.38* (5.723)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Province by year FE	No	Yes	No	Yes	No	Yes
Observations	52,311	52,311	52,311	52,311	52,404	52,404
R-squared	0.846	0.866	0.600	0.651	0.723	0.754

The dependent variables include the logarithm of total municipal transfers (col. 1-2), the logarithm of transfers as a share of municipal revenues (col. 3-4) and municipal transfers per capita in absolute terms, winsorized at the 5% level (col. 5-6). The unit of observation is municipality-year, and the sample includes the 5,431 municipalities not in autonomous regions or metropolitan cities. Treated municipalities (time-constant) are those whose mayor eventually becomes provincial president. Sample: 2011-2019. Treatment period (After) varies by province and it is the first time a mayor in a province is elected president. Robust standard errors, clustered at the municipality level, are in parentheses, * p<0.10, ** p<0.05, *** p<0.01.

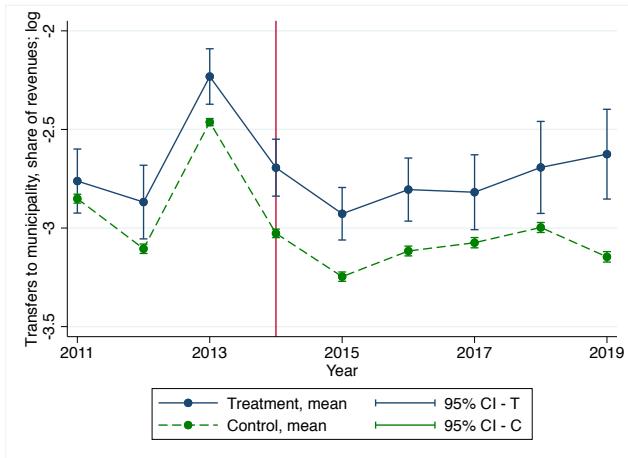


Figure B13: (Logarithm of) transfers as *share* of municipal revenues. Treatment group (time-varying): municipalities that are the presidents' birthplace.

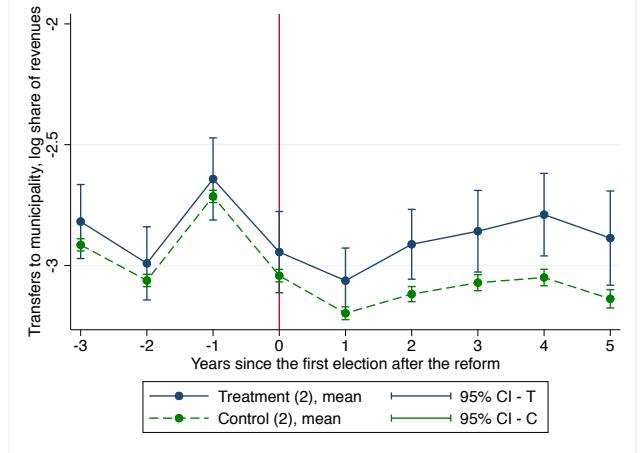


Figure B14: (Logarithm of) transfers as a *share* of total municipal revenues. Treatment group: municipalities in which the provincial president serves as mayor, anytime after the reform.

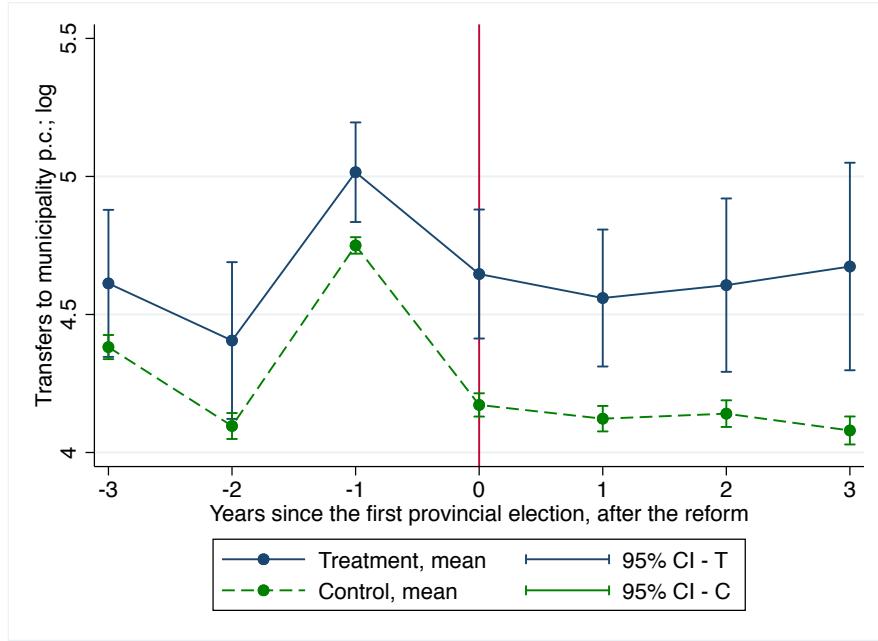


Figure B15: Transfers to municipalities per capita. Treated municipalities: those whose mayor that will become president had become mayor *before* the reform. Control municipalities: other municipalities from the same provinces as the considered subsample of presidents. Sample stops in 2017 i.e. before first presidential replacement. Capitals excluded.

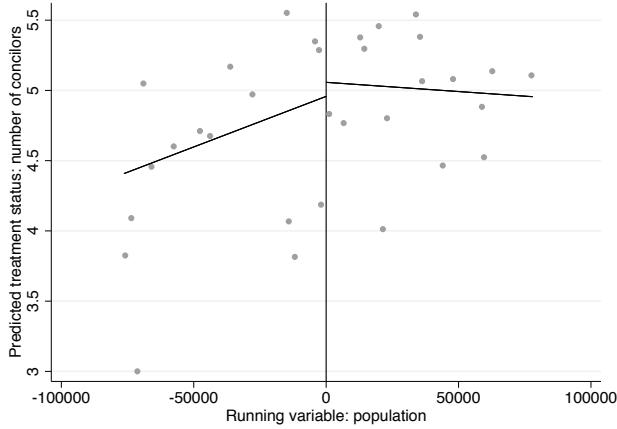


Figure B16: Log transfers per capita to provincial president's birth-municipality, RDD for increase of the council size. The running variable is provincial population normalized around the closest threshold: cutoffs at 700,000 and 300,000 inhabitants). Passing the threshold, the size of provincial council increases by 4 councillors before 2014 and 3, on average, after 2014. The sample is the universe of presidents' birthplaces (2008-2019): 437 municipality-years.

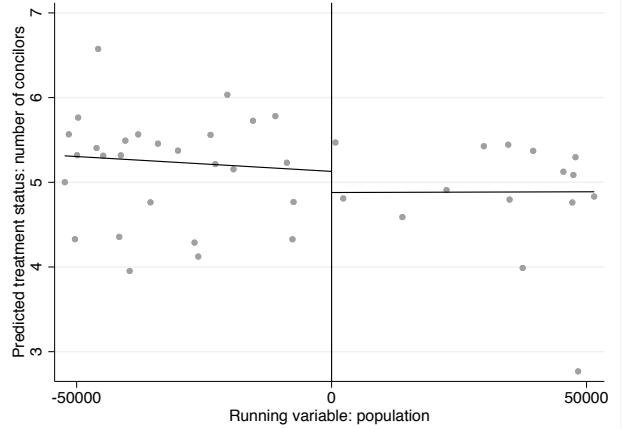


Figure B17: (Log) Transfers per capita to provincial president's municipality of birth, RDD for increase of the wage. The running variable is provincial population normalized around the closest threshold: the thresholds are at 250,000, 500,000 and 1,000,000 inhabitants. Before 2014, presidents' wages increased when passing the population threshold. The sample is the universe of presidents' birthplaces (2000-2014): 589 municipality-years.

Table B5: Municipal transfers per capita. RDD effect of larger provincial council on municipal transfers to presidents' hometowns (2008-2019). Total sample: 437 observations. Provincial capitals included.

	Transfers to municipality per capita (ln)		
	(1)	(2)	(3)
Treatment effect	0.254 (0.277)	0.268 (0.333)	0.325 (0.247)
Robust p-value	0.540	0.811	0.570
Observations	163	103	249
Polynomial order	1	1	1
Bandwidth	CCT (80,051)	50,000	100,000

Table B6: Municipal transfers per capita. RDD effect of higher wage for provincial president on presidents' hometown transfers (2000-2014). Total sample: 589 observations. Provincial capitals included.

	Transfers to municipality per capita (ln)		
	(1)	(2)	(3)
Treatment effect	-0.306	-0.294	-0.413
Robust p-value	0.282	0.339	0.636
Observations	136	191	362
Polynomial order	1	1	1
Bandwidth	CCT (42,630)	50,000	100,000

B.1.4 Transfers to Municipalities. Longer sample: 2008-2020

I replicate here my main results using a longer sample period, which includes years from 2008 to 2020, and provincial capitals both in the treated and control group, unless differently specified. Overall, Figures B18 and B19 and Table B7 confirm the results in the main analysis. Notice that the big fluctuations in transfers before 2011 are entirely driven by national changes in the municipal tax system. In 2011, the so-called municipal federalism reduced municipal dependence from the central government's transfers by allowing cities to raise more local taxes. Conversely, municipal tax reforms in 2007-2008 and 2013 forced the government to increase transfers to compensate for municipal resource shortages. The high level of transfers before 2011 was due to the virtual abolition of the municipal estate tax (ICI), the main municipal source of revenues. Similarly, the peak in 2013 was a consequence of the abolition of the second installment of the new municipal estate tax (IMU). This was compensated for, from 2014, by the TASI municipal tax. The most striking and reassuring characteristic of the pre-reform trends is that, despite the fluctuations due to frequent tax reforms, the treated and the control municipalities were extremely similar before the reform and their trends virtually overlap in the pre-reform period.

A small effect seems to anticipate the reform when defining the treated group as the presidents' birthplaces (see Figure B18). This may be caused by the 2012 reform project described in note 3, which postponed the 2012 and 2013 provincial elections and prescribed provincial leaders to be chosen among municipal politicians, but that was declared unconstitutional. 2012 and 2013 are thus a transition period, possibly with weaker incentives, thus presidents may have started modifying their

behavior even before the Delrio Act. While in the main analysis I restrict to 2011-2019, to keep constant the level of central government's transfers, below I address this pre-reform effects in two ways. First, I exclude province capitals, which are peculiar in multiple dimensions, from the analysis in the main text: Figure B19 shows the long sample having excluded capitals. Since the headquarter of the provincial government is located in these cities, they may benefit even from presidents that were not born there; moreover, they consistently receive way more transfers per capita than the average municipality (see Figure B3 and B4), suggesting that changes over time in the number of capitals included in the treated group may threaten my identification. Excluding capitals from my sample, eliminates the pre-trend. Second, I implement the Rambachan and Roth (2021) inference method in the presence of pre-trends for the sample that includes capitals: in this case, my results are confirmed after assuming that the pre-trend is sufficiently linear (see Figure B20).

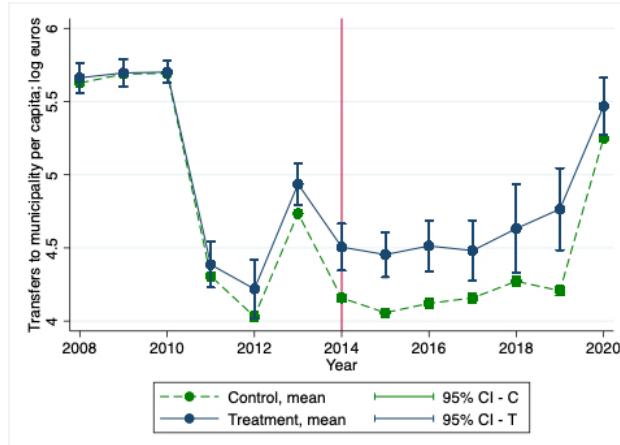


Figure B18: (Log) transfers per capita. Treatment group: municipalities that are birthplace of the presidents (time-varying). Capitals included.

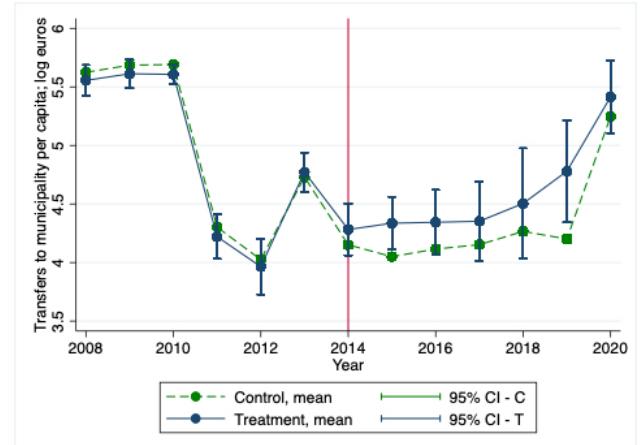


Figure B19: (Log) transfers per capita. Treatment group: municipalities that are birthplace of the presidents (time-varying). Capitals excluded.

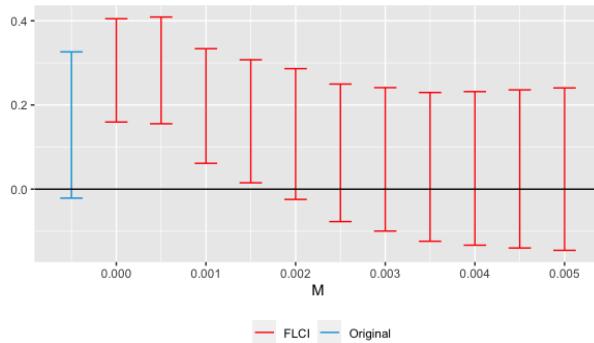


Figure B20: Rambachan and Roth (2020) inference method. Impact in 2015 using 2013 as last pre-reform year (2000 is the first). Province by years and municipality fixed effects included, standard errors clustered at the municipality level. M=0 implies a linear pre-trend, larger M allows larger deviations from linearity. Treated group: presidents' birthplace. Outcome: log transfers per capita.

Table B7: Municipal transfers (2008-2020). Treatment group: cities that are the president's birthplace.

	(1)	(2)	(3)	(4)	(5)	(6)
	(Log)Transfers p.c.	(Log)Transfers p.c.	(Log) Total transfers	(Log) Total transfers	(Log) Transfers share	(Log) Transfers share
Treatment (time-variant)	-0.142*** (0.051)	-0.124*** (0.047)	-0.158*** (0.050)	-0.140*** (0.047)	-0.113** (0.053)	-0.103** (0.047)
Treatment*After	0.309*** (0.062)	0.256*** (0.058)	0.334*** (0.061)	0.282*** (0.057)	0.246*** (0.067)	0.217*** (0.060)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Province by year FE	No	Yes	No	Yes	No	Yes
Observations	72,468	72,468	72,471	72,471	72,471	72,471
R-squared	0.747	0.786	0.866	0.886	0.675	0.718

The dependent variables include the logarithm of: total municipal transfers, transfers per capita and transfers as a share of municipal revenues. The unit of observation is municipality-year, and the sample includes the 5,431 municipalities not in autonomous regions or metropolitan cities. Treated municipalities are those that are birthplace of the president in office in year t , and change over time (using a constant treatment groups i.e. all cities that were a birthplace at least once, yields similar results). Capitals are included. Time period: 2008-2020 (treatment period: 2014-2020). Robust standard errors, clustered at the municipality level, are in parentheses, * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

B.2 SIOPE data: transfers from local governments

In this section I replicate the main results on Section 5 using municipal transfer data from SIOPE. While AIDA PA provides data on public spending and revenues homogeneously aggregated by sectors and consistent over time, SIOPE is less homogeneous and subject to a relevant break in consistency in 2017, which make its use more problematic. However, SIOPE has the advantage of reporting more disaggregated subcategories of revenues. Instead of the total amount of current transfers received by each municipality (the only consistently available measure of transfers from AIDA), I use here the sum of current and capital transfers to municipalities from any local governments. This measure only includes regions, provinces, union of municipalities, comunità montane and other municipalities, which are the administrations that are most likely to be influenced by the provincial president's direct or indirect (lobbying) activity. Besides adding capital revenue (used to support long-term investments, rather than current spending), this measure excludes transfers coming from any central government's institution, from the European Union and from any non-public sources (e.g. transfers from firms and citizens). Since the large increase in transfers to municipalities in 2020 were from the central government, here excluded, I include 2020 in the sample. Figures B21, B22, B23, B24 qualitatively confirm the results of Section 5, indicating an increase in local-public funds toward municipalities connected to a president.

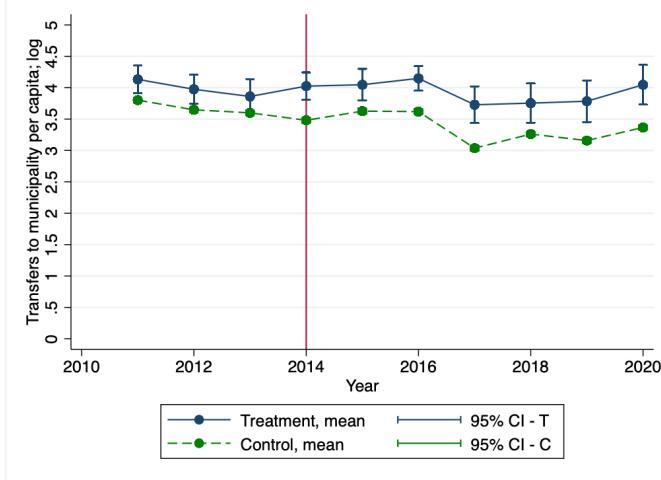


Figure B21: (Log) transfers per capita: sum of current and capital transfers from any local government. Treatment group (time-varying): municipal birthplace of the president in office, excluding provincial capitals.

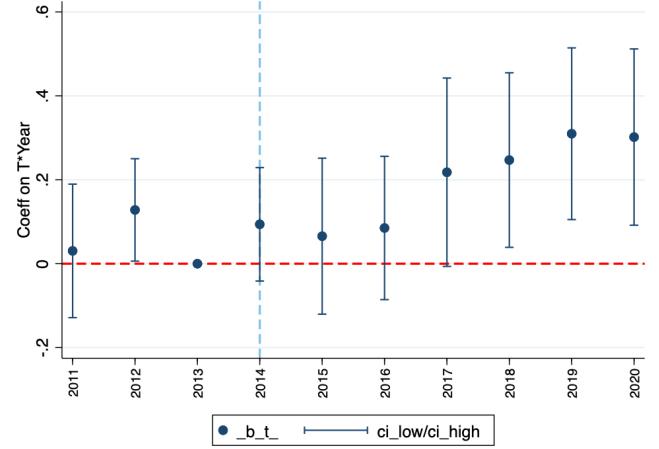


Figure B22: (Log) transfers per capita: sum of current and capital transfers from any local government. Treatment group (time-varying): municipal birthplace of the president in office, excluding provincial capitals.

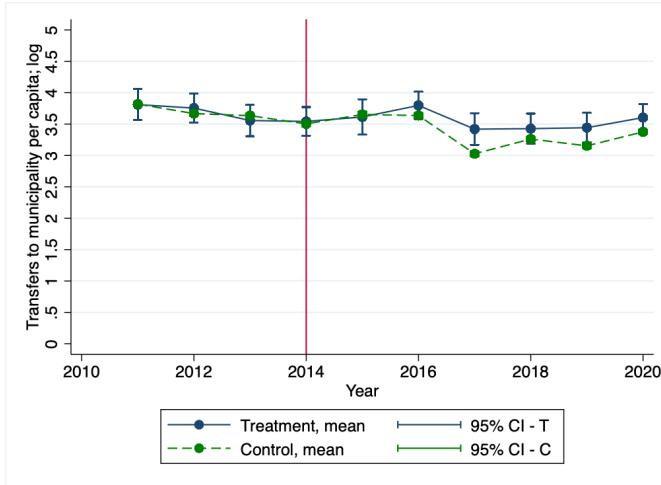


Figure B23: (Log) transfers to municipality per capita: current and capital transfers from any local government. Treatment group (time-constant): municipalities whose mayor is president after 2014, no capitals.

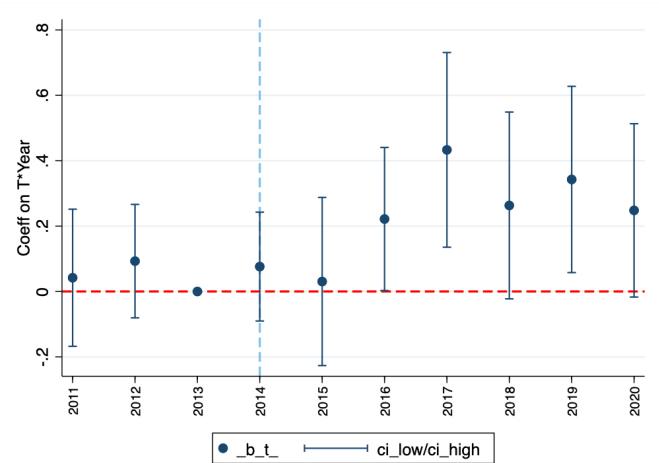


Figure B24: (Log) transfers to municipality per capita: current and capital transfers from any local government. Treatment group (time-constant): municipalities whose mayor is president after 2014, no capitals.

C Indirect elections and the composition of public spending

C.1 Synthetic control

The trends in Figures 8 and 9 for the treatment and the control group are parallel but distant from one another, raising concerns that the share of expenditure on transport and education in the control group is too low to decrease. In what follows, I construct a control group that better

matches the treatment group's expenditure share in the two sectors in the pre-reform period. The main difficulty in doing so is that - having a wider range of responsibilities - municipalities spend a smaller share of resources in transport and education; thus, even the highest counterfactual has a share of spending on transport below 15% throughout the whole period. In practice, to match the level of the treatment group, I construct a 'synthetic' control group by picking only those municipalities with sufficiently high 10-year averaged expenditure shares. In this way, given the long time frame, I restrict to the municipalities with a permanently high share of expenditure, while excluding those which experienced exceptionally high expenditure for only a few years (the latter would indeed be affected by mean reversal). Importantly, I still allow the selected control group to vary over time, potentially dropping after 2013, provided they maintain a sufficiently high 12-year average.³⁵

The resulting control groups are depicted in Figures C1 and C2, where I restrict to a 12-year average share above 20% for transport and above 15% for education, to better match the treated group in the pre-intervention period. Consider Figure C1: given the pre-treatment average (about 23%), the constraint to be above the 20% share of expenditure implies that the control group average could drop as low as 17.8% in the overall post-intervention period (clearly each single year could reach far lower values, depending on the other post-reform values). Reassuringly, the post-treatment mean is very stable and similar to the pre-trend. To sum up, these figures show that even reconstructing the control group by restricting it to those municipalities with a share of expenditure comparable to that of provinces, no drop in the share is visible after 2013. This result is formally tested and confirmed in Table C1.

³⁵This approach differs from the classic synthetic control method which would construct the control group by only selecting municipalities with high expenditure shares in the pre-treatment period (2009-2013). The problem with using the classic approach here is that, as municipalities devote to transport and education shares of spending below 10%, when selecting only those municipalities with a pre-period share high enough to match the provinces, these observations will mechanically suffer from mean reversal in the unrestricted period. Indeed, only the (few) municipalities with exceptional expenses in the short run would be selected. For instance, consider an average municipality, that implements an exceptional road renovation program in 2010-2011 the expenditure share in transport will rise dramatically in those years and the municipality will be selected into the control group, but it will then quickly revert to its historical low mean: this problem will be more relevant the smaller the selected sample and the shorter the period on which the average is computed. Indeed, by restrict as to a share in transport expenditure above 20% in the placebo period 2009-2011, the trend progressively drops from about 23% to 15% within 5 years. The same happens if I restricted to my whole pre-intervention period.

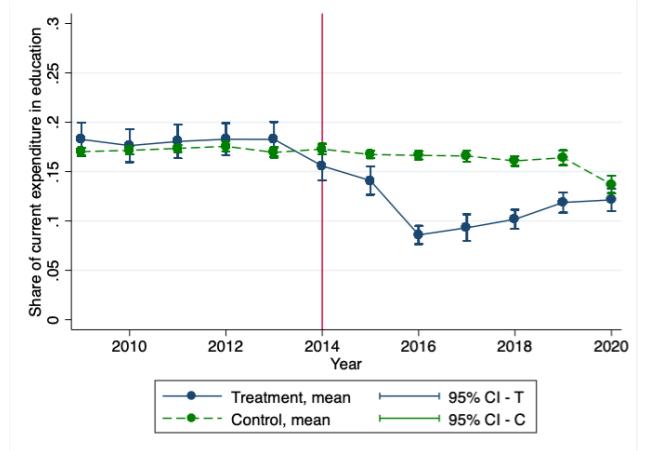
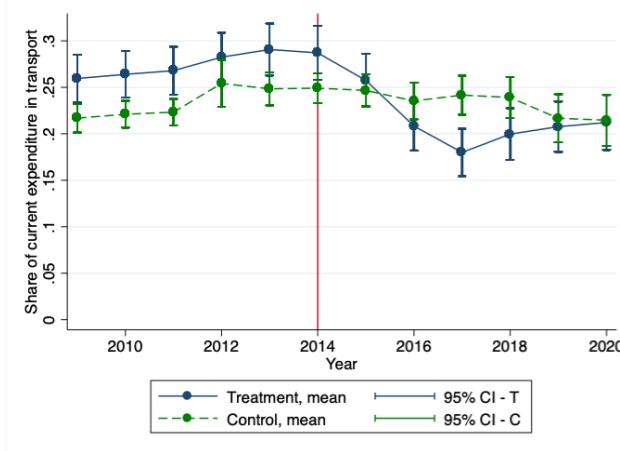


Table C1: Impact on share of spending on transport and education. Synthetic control.

	(1)	(2)
SYNTHETIC CONTROL	Transp. Curr. Share	Educ. Curr. Share
Treatment*After	-0.049*** (0.012)	-0.055*** (0.007)
Province Fe	Yes	Yes
Year Fe	Yes	Yes
Observations	1,510	1,865
R-squared	0.778	0.661

Dependent variables: current share of expenditure on transport (col. 1) and education (col. 2). Independent variables include the interaction between post-treatment dummy (After) and dummy for treatment group (Treatment), which are absorbed by the fixed effects. Autonomous provinces and metropolitan cities are excluded. For transport, the control group includes 219 municipalities (from 47 provinces) with 12-year average expenditure share in transport above 20%. For education it includes 914 municipalities (from 75 provinces) with more than 15% of current spending in education. Province and year fixed effects included. Robust standard errors, clustered at the province level, are in parentheses, * p<0.10, ** p<0.05, *** p<0.01.

C.2 Multiple periods Diff-in-Diff

For a better sense of the size of the effect in each year, I also run the following specification:

$$Y_{p,t} = \alpha + T_p * \sum_{t=2009}^{2020} year_t \gamma_t + \psi_t + \gamma_p + \epsilon_{p,t} \quad (C4)$$

where all variables are defined as in equation (3) and $year_t$ is a set of year-specific dummies. Standard errors are still clustered at the province level.

Figure 9 suggests that the impact of the reform may begin as early as in 2014. To have a better sense of the exact timing, I thus run equation (5) for the expenditure in transport and education and I plot the estimates from the event study in Figure C3. The figure confirms that the effect becomes significant between 2014 and 2015. The event study estimates for the administrative costs are plotted in Figure C4, which shows that the effect of the reform had already begun in 2014 but its magnitude increased in 2015. As in the main graph I highlight that results after 2015 are to be interpreted with caution, because of the reclassification of the bureaucratic category.

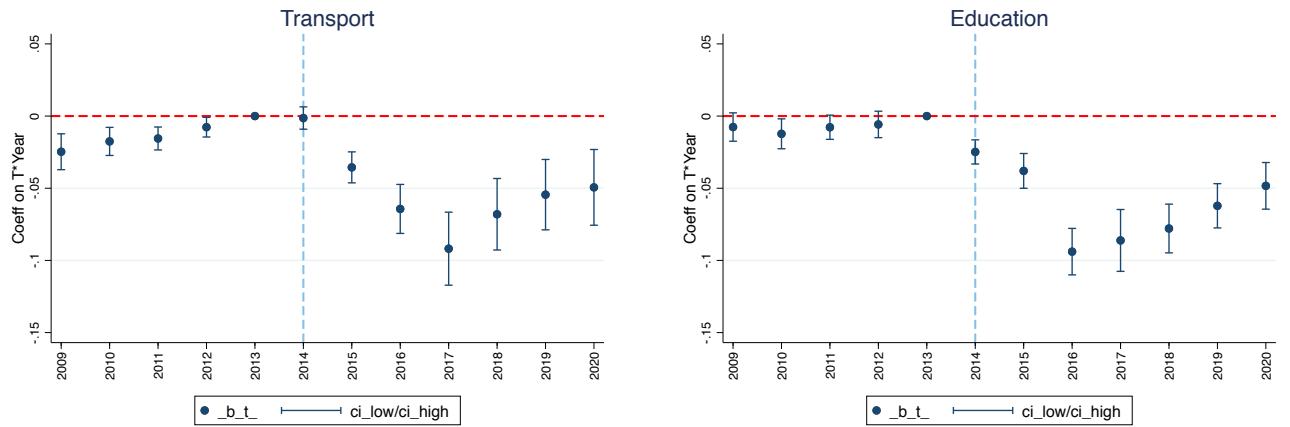


Figure C3: Share of current expenditure on transport and education. Treatment by year coefficients are plotted. Reference omitted year: 2013. Province and year FE included. Cluster at the province level.

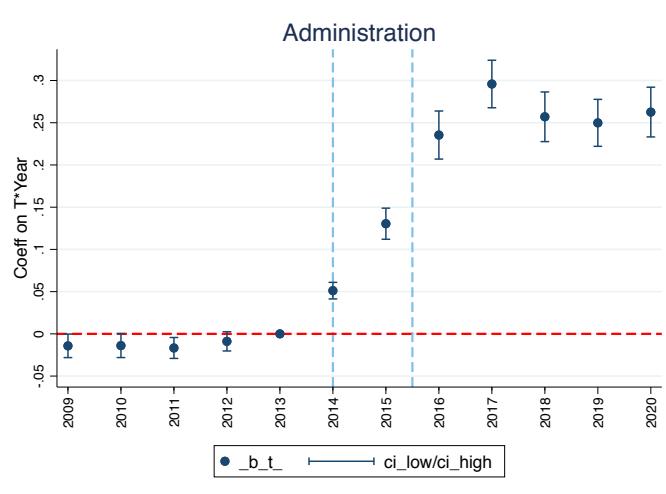


Figure C4: Event study: share of current spending on administration. Treatment by year coefficients are plotted. Province and year FE included. Cluster at the province level.

C.3 Context: residual responsibilities; total expenditure, total revenues

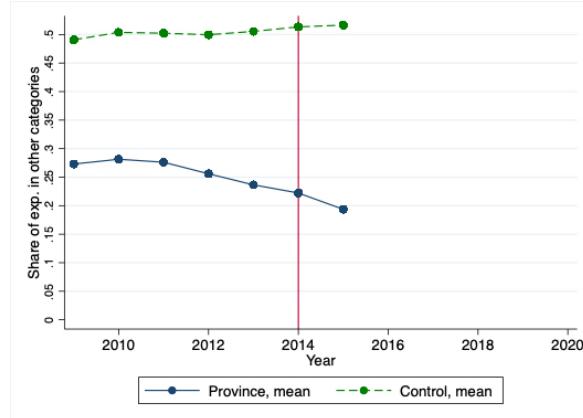


Figure C5: Share of current expenditure in sectors other than transport, education, administration

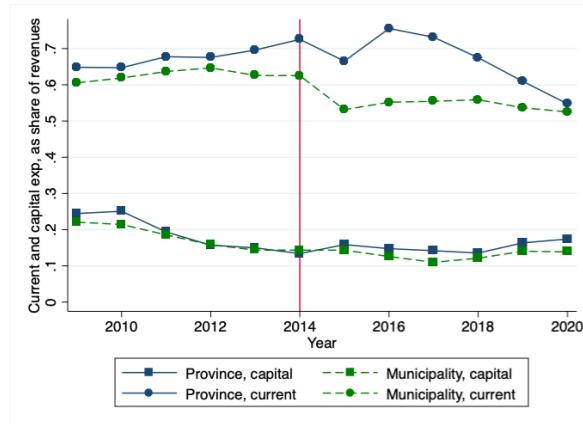


Figure C6: Provincial and municipal current and capital expenditure as a share of total revenues

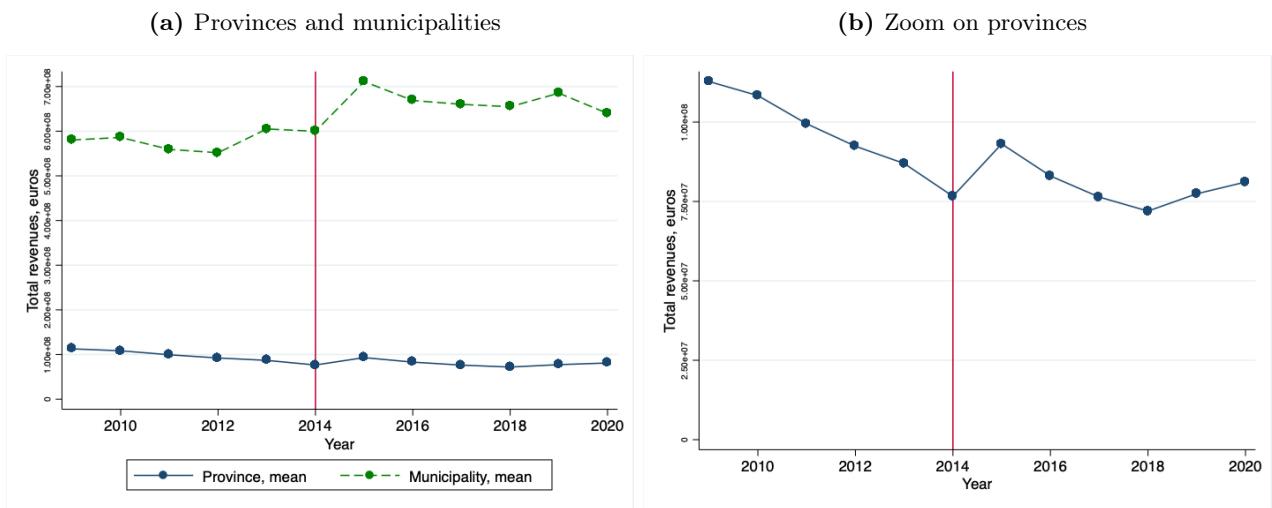
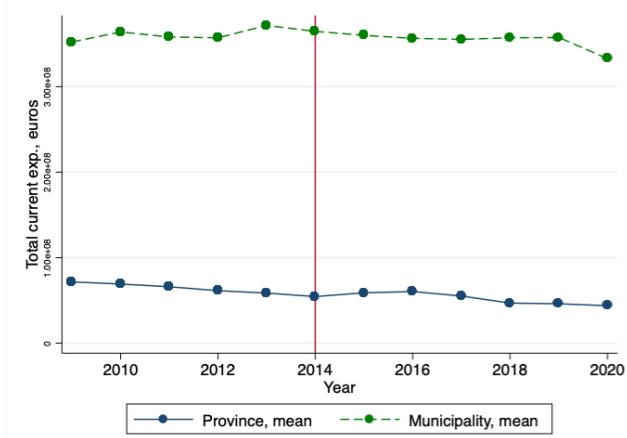


Figure C7: Total revenues

(a) Provinces and municipalities



(b) Zoom on provinces

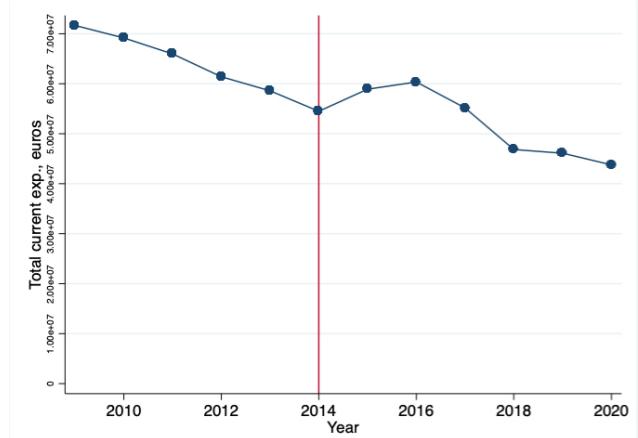


Figure C8: Total Current Expenditure

C.4 Level of Expenditure

Table C2: Impact on current expenditure on transport, education and administration (in Euros)

	(1)	(2)	(3)
	Transport	Education	Administration
Treatment*After	-2978574.1*** (1034551.7)	-2385340.6*** (667,234.3)	6591199.2*** (1598564.3)
Province Fe	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes
Observations	2,227	2,227	1,307
R-squared	0.910	0.974	0.990

The dependent variables are current expenditure on transport, education and administration. Independent variables include post-treatment dummy (After), dummy for treatment group (Treat) and the interaction term. Post-intervention period: 2014-2020 (2014-2015 for administrative spending due to data inconsistency). Fixed effects are at the province and year level. The three autonomous provinces and the metropolitan cities are excluded (differently affected by the reform). Robust standard errors, clustered at the province level, are in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.5 Decomposition of administrative expenditure

In this section I provide additional information of the areas in which provincial bureaucratic costs increased the most. More specifically, for each *mission* (sector) - transport, education, administration - AIDA PA provides an additional decomposition of spending (only available up to 2015) into some categories such as personnel, materials and services, extraordinary current costs, transfers to public and non-public entities. Figure C9 shows such decomposition for the administrative sector. Panel (a) shows a progressively decreasing trend in the personnel expenditure, which accelerates after the reform, as an expected effect of the reduction of provincial personnel previously discussed. Panel (b) shows a similar progressive reduction in the cost of good and services, which in this case slightly decelerates following 2014. Panels (c) to (e) show that the aforementioned decreasing trends are more than compensated by the large increases in spending on *extraordinary expenses of the current administration*, a category containing a broad variety of short-term non-expected costs (failure to collect credits, thefts and damages, non-expected bonuses, etc.); on direct transfers of funds to private entities (firms, families and NGOs); and on transfers to public entities. Summing up the different categories, panel (f) confirms the stark increase in administrative spending starting from 2015.

In light of its relative importance, panel (d) deserves a closer discussion. The types of transfers included in this category are the ones directed to public entities, whose purpose is not specifically connected to other missions. For instance, this excludes transfers to other institutions constrained to provide transport-related public good, which instead would be included in the *transport* mission. Originally, this category would include provincial transfers to the central government, which sharply increased after 2013 due to a budget law that required the provinces to contribute to the central government's budget. This could confound my results; thus, I use *SIOPE* data to collect the amount of funds each province sent to the central government each year and I subtract this amount from the *transfers to public administrations* category provided by AIDA. Panel (d) is precisely the result of such operation and it still shows a very stark increase after 2013. In line with this, Figure C10 matches Figure 10 and reports the overall share of spending in the administration mission over total current spending, *net of the amount of transfers to the central government*. The figure shows a very similar pattern to the one in Figure 10, confirming that transfers to the central government are not driving results.

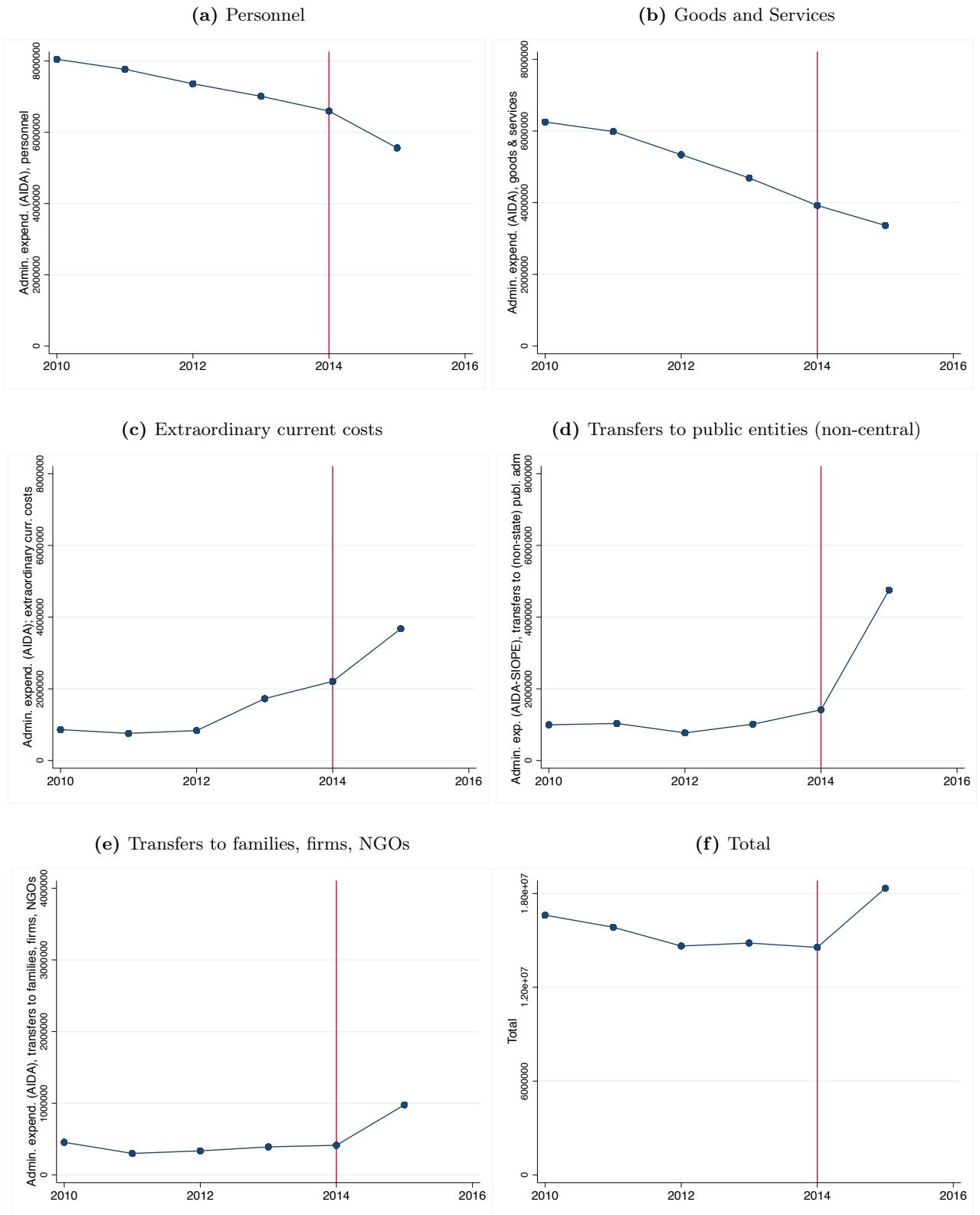


Figure C9: Absolute administrative expenditure, euros. Total and subcategories.

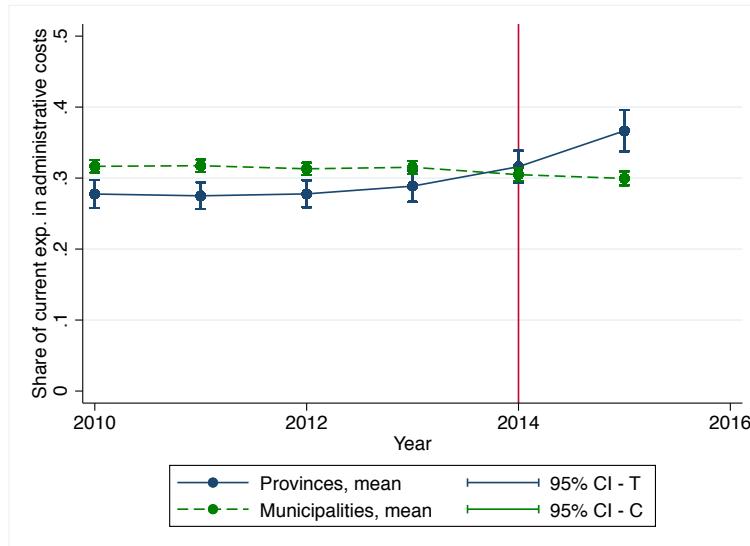


Figure C10: Bureaucratic costs as a share of current spending, net of transfers to central government.

While I do not observe the amount of transfers divided by mission *and* recipient, SIOPE does provide the total amount of provincial transfers to local administrations (municipalities, union of municipalities, comunità montane). A very large amount of transfers to local administrations would cast some doubts on my identification strategy in Section 6, which uses municipalities as the control group for provincial governments. I plot this amount in Figure C11, which reassuringly, excludes any large increase in transfers of funds toward municipalities.

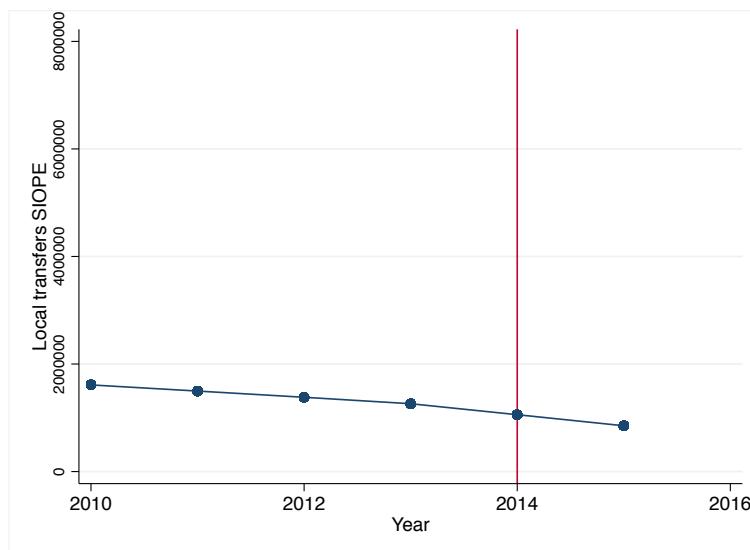


Figure C11: Transfers from provinces to: municipalities, unions of municipalities, comunità montane.

C.6 Robustness Checks

In this section I discuss several robustness checks, in which I use different specifications to address minor concerns. First, since current expenditure is not a fixed share of total revenues, one might wonder how results would change if I focused on shares of total revenues instead.³⁶ I therefore re-run the main regressions, dividing current expenditure in each sector over total revenues. Even though the pre-trend is not as parallel as in the main specification, sharp changes of slope are still observable in 2014 (see Figures C12 and C13). Table C3 confirms that the coefficients of the interaction term maintain the sign of the main analysis, remaining highly significant.

Second, as the reform was implemented in mid-2014, I slightly redefine the pre- and post-intervention periods. Results are robust both to considering 2014 as a post-period or to excluding 2014, as shown in Tables C4 and C5, respectively. What about the timing of the elections? The effect on provinces with elections happening after 2014 is not straightforward: indeed even non-replaced incumbents became aware in early 2014 that they would have not been reelected with direct elections, reducing their incentives right after the reform passed. Figure C16 plots the coefficient of the interaction term per year for the provinces that elected their first president in 2014 and for those with later elections. Provinces with late elections show a smaller but significant effect in 2014 for education and administration, but not for transport. This pattern suggests that the drop in electoral incentives was stronger after the elections, but played a role even before politicians' turnover (highlighting the role of incentives rather than politicians' characteristics).

Third, it could be possible that the results I find simply reflect a transfer of resources from current to capital expenditure (and the other way around in bureaucracy). I only have data on capital expenditure for transport and administration before 2016, but Table C6 shows a small and non-significant effect in capital spending. These considerations suggest that the change in current expenditure is not driven by a shift of resources towards capital expenditure.

One last concern is the fact that in 2018, 2017 and partially in 2016 some provinces significantly increased their ‘transfers to the state’. This was a fiscal phenomenon with provinces both receiving funds from and sending funds to the state. To maintain a conservative approach, I perform a separate analysis in which not only do I drop the years after 2016 but I also exclude those 15 provinces (and relative artificial controls) that were affected by such a rise in 2015. Table C7 shows that, even

³⁶Figure C6 shows current expenditure is actually quite a constant share.

for this reduced subgroup, the impact of the reform remains significant. I then directly control for the amount of transfers to the central government (as consistently measured by the dataset SIOPE, between 2010 and 2016) by subtracting them from the total current expenditure (the denominator of my main dependent variables). Figure C14 shows that this does not affect my results.

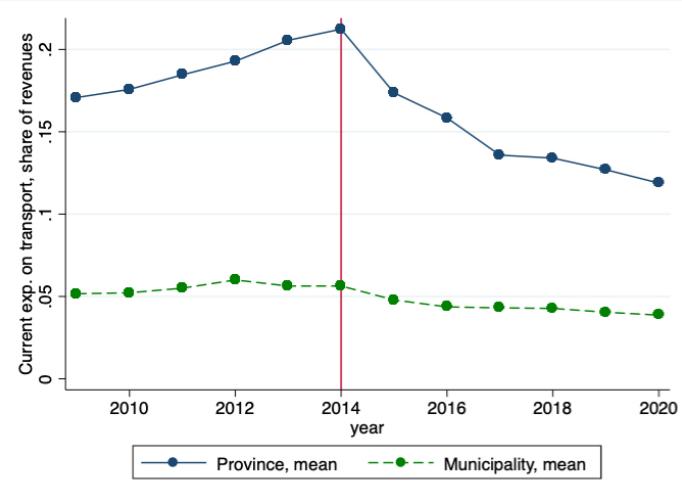
Share of total revenues

Table C3: Check: share calculated over total revenues

	(1)	(2)	(3)
	Transp. share rev.	Educ. share rev.	Admin. share rev.
Treatment*After	-0.021*** (0.005)	-0.031*** (0.004)	0.093*** (0.008)
Province Fe	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes
Observations	2,224	2,224	1,307
R-squared	0.859	0.773	0.687

The dependent variables are the share of total revenues spent on transport (col. 1), education (3) and administration (col. 5). Independent variables include post-treatment dummy (After), dummy for treatment group (Treat) and the interaction term. Three autonomous provinces and all metropolitan cities are excluded (differently affected by the reform). Robust standard errors, clustered at the province level, are in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(a) Transport



(b) Education

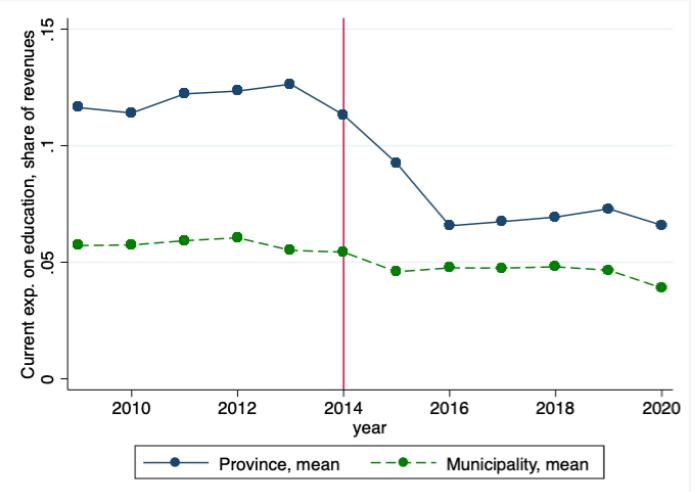


Figure C12: Expenditure on public good as share of total revenues

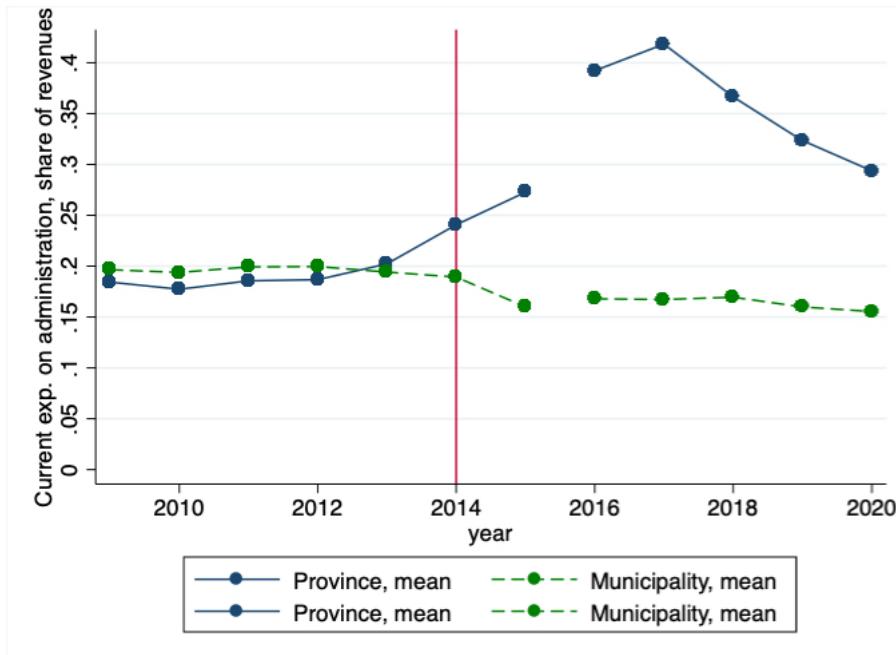


Figure C13: Administrative spending as a share of total revenues

Robustness to different post-treatment periods

Table C4: Check: 2014 excluded from the analysis since the law was in the middle of the year

	(1)	(2)	(3)
	Transport Exp. Share	Education Exp. Share	Admin. Exp. Share
Treatment*After	-0.048*** (0.008)	-0.061*** (0.007)	0.142*** (0.010)
Province Fe	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes
Observations	2,040	2,040	1,120
R-squared	0.880	0.762	0.856

The dependent variable is the share in transport, education and administrative costs over total current expenditure. Independent variables include post-treatment dummy (After), dummy for treatment group (Treatment) and the interaction term. Year and province fixed effects. Three autonomous provinces and all metropolitan cities are excluded (differently affected by the reform). Year 2014 is excluded. Robust standard errors, clustered at the province level, are in parentheses, * p<0.10, ** p<0.05, *** p<0.01.

Table C5: Check: 2014 is now pre-reform period

	(1)	(2)	(3)
	Transport Exp. Share	Education Exp. Share	Admin. Exp. Share
Treatment*After	-0.050*** (0.008)	-0.058*** (0.006)	0.132*** (0.010)
Province Fe	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes
Observations	2,227	2,227	1,307
R-squared	0.886	0.768	0.843

The dependent variable is the share in transport, education and administrative costs among total current expenditure. Independent variables include post-treatment dummy (After, 1 from 2015 to 2020), dummy for treatment group (Treatment) and the interaction term. Fixed effects are at the province level. Three autonomous provinces and all metropolitan cities are excluded (differently affected by the reform). Robust standard errors, clustered at the province level, are in parentheses, * p<0.10, ** p<0.05, *** p<0.01.

Other checks

Table C6: Impact of the reform on capital expenditure on transport (share of total capital expenditure).

	(1)	(2)	(3)	(4)
	Transp. cap. share	Transp. cap. share	Admin. cap. share	Admin. cap. share
Treatment	0.192*** (0.017)		-0.040** (0.017)	
Treatment*After	-0.011 (0.021)	-0.009 (0.023)	-0.014 (0.016)	-0.016 (0.018)
Province Fe	No	Yes	No	Yes
Year Fe	Yes	Yes	Yes	Yes
Observations	1,306	1,306	1,306	1,306
R-squared	0.211	0.498	0.022	0.506

The dependent variables are share of capital expenditure on transport and administration. Independent variables include post-treatment dummy (After, which refers to only 2014 and 2015), dummy for treatment group (Treatment), and the interaction term. Three autonomous provinces and metropolitan cities are excluded (differently affected by the reform). Robust standard errors, clustered at the province level, are in parentheses, * p<0.10, ** p<0.05, *** p<0.01.

Table C7: Check: drop years and provinces with transfers

	(1)	(2)	(3)
	Transport Exp. Share	Education Exp. Share	Admin. Exp. Share
Treatment*After	-0.004 (0.005)	-0.023*** (0.005)	0.095*** (0.008)
Province Fe	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes
Observations	1,271	1,271	1,271
R-squared	0.968	0.908	0.848

The dependent variable is the share on transport, education and administrative costs among total current expenditure. Independent variables include post-treatment dummy (After is 1 for 2014 and 2015), dummy for treatment group (Treatment) and the interaction term. Years 2016 to 2020 are dropped. 19 provinces and 19 counterfactuals are also dropped in 2015 (those with high transfers). Three autonomous provinces and all metropolitan cities are excluded (differently affected by the reform). Year and province fixed effects. Robust standard errors, clustered at the province level, are in parentheses, * p<0.10, ** p<0.05, *** p<0.01.

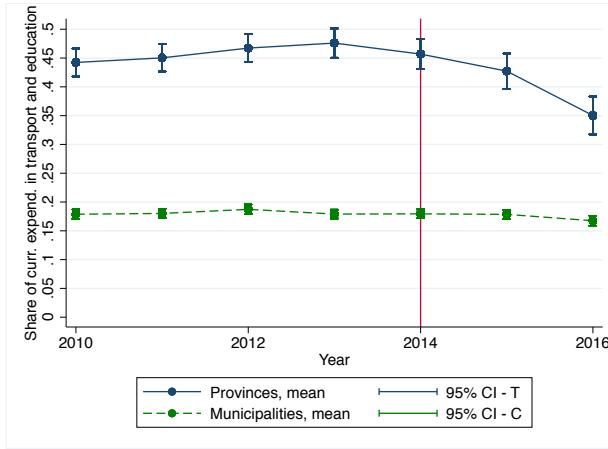


Figure C14: Spending in transport and education (public good). Excluding any provincial transfer to the central government from the numerator and denominator.

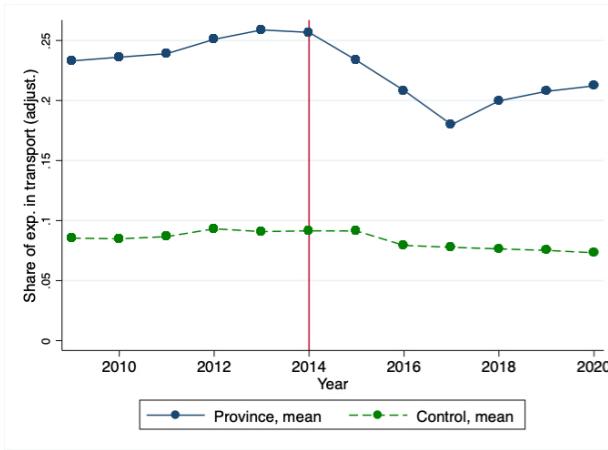


Figure C15: Share of current spending on transport over adjusted total current expenditure, i.e the sum of total current expenditure and road costs. The latter was erroneously excluded from the transport sector in the original data.

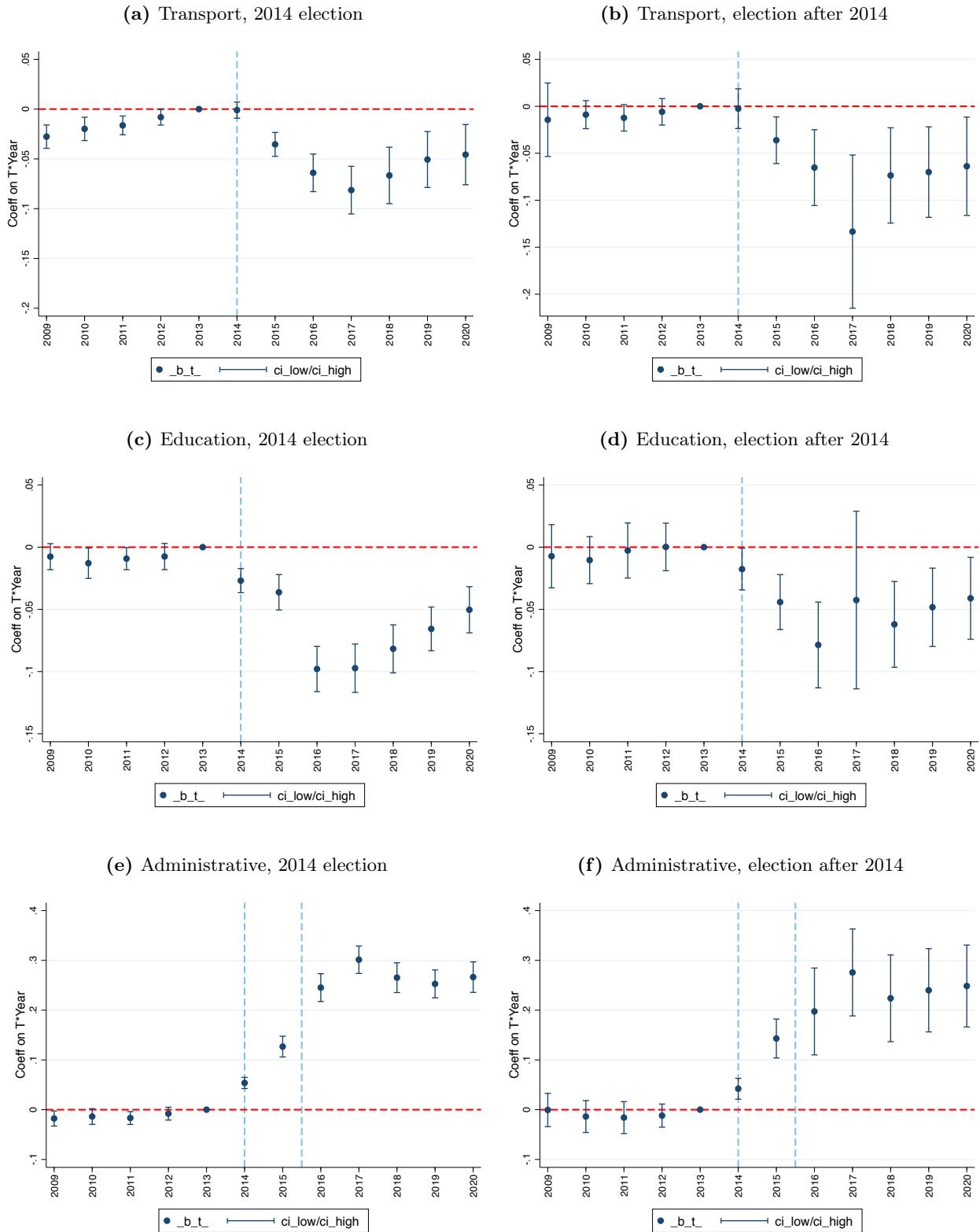


Figure C16: Provinces with first election in 2014 (1762 observations) and those whose first election after 2014 (465 observations). Province and year FE. Cluster at province level.

C.7 Regional trends

In order to rule out the hypothesis that provinces transferred part of their duties to the regions, Figure C17 plots the regional total spending in transport and education and the share of total (capital or current) spending in each sector, as provided by SIOPE. Compared to 2013 and 2014, there was no stark increase in the level or share of expenditure at the regional level in transport and education. This is consistent with the journalistic evidence that no major responsibilities were transferred from the provinces to the regions immediately after the reform. A shortcoming of the data is that the consolidated balance sheet is not available by missions (as it was for the provinces) and that expenditure on school and road infrastructures are all considered capital spending at the regional level. Moreover, in 2011 and 2012 there were large changes in regional spending due to a national law that imposed maximum levels of general and sector-specific spending, which caused overall expenditure in 2012 to be about one third of that in 2013. From 2013 on, however, spending stabilized. Despite these shortcomings, the figures suggest that, after 2014, there was no increase in the level or share of expenditure compatible with a transfer of responsibilities from the provinces to the regions. In fact, if anything, there was a decrease.

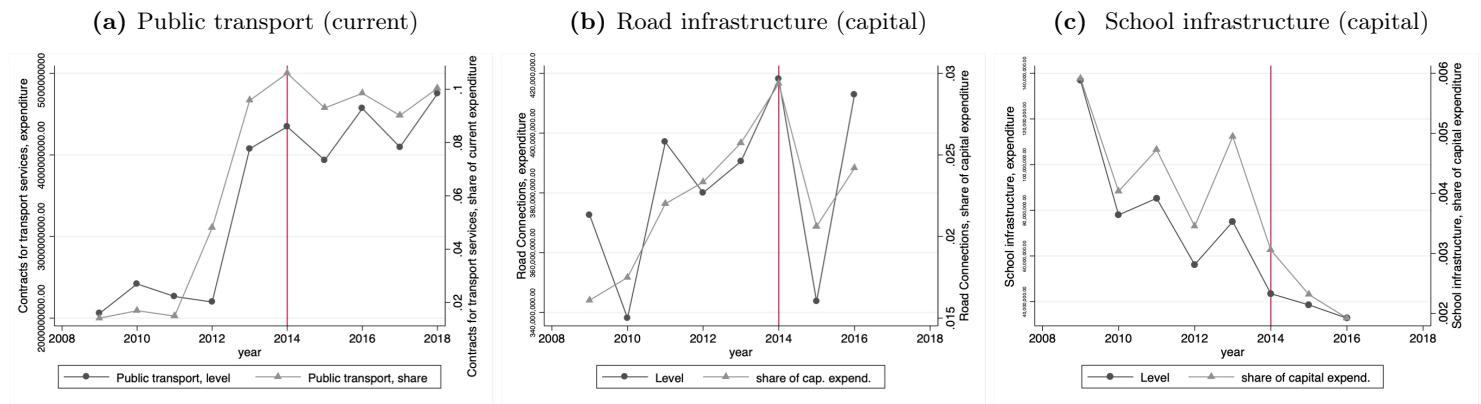


Figure C17: Regional capital (current) spending; level and sectoral share of capital (current) spending.

D Probabilistic voting model (forward-looking voters)

In this model, I conceptualize the agents' decisions in the context of rent-motivated politicians facing forward-looking voters who select their representatives based on their electoral promises. In particular, I develop an extension of the classic probabilistic voting model with rents (section 4.2 in Persson and Tabellini (2000), henceforth PT), with forward looking voters and binding electoral

platforms. Borrowing the notation from PT, the model develops as follows:

There are two parties (A, B) and two tiers of government: the municipality (m) and the province (p). Politicians raise taxes τ^t from a tier-specific portion of the citizens' income y^t , consistent with the idea that each tier of government can only raise taxes on specific activities/types of wealth (e.g property tax, tax on waste etc.). The collected resources can be extracted as rent r by the politician or used to deliver public good g . The budget constraint is thus:

$$\tau^m * y^m = (g^m + r^m) \quad \text{and} \quad \tau^p * y^p = (g^p + r^p) \quad \text{with} \quad \tau^m, \tau^p \leq 1 \quad (\text{D5})$$

Citizens derive utility from both private consumption $c^i = (1 - \tau)y^i$ and public good, as follows:

$$W^m(g^m, r^m) = (y^m - g^m - r^m) + H(g^m) \quad \text{and} \quad W^p(g^p, r^p) = (y^p - g^p - r^p) + U(g^p) \quad (\text{D6})$$

where $H(\cdot)$ and $U(\cdot)$ are concave and increasing functions.

In the real world, the provincial president (henceforth P) is typically party-affiliated, and municipal politicians (henceforth M) thus vote based on their own party affiliation. Thus, I assume that in an indirect election, voters give it for granted that M will vote for the provincial candidate from the same party (voters thus choose a “package” of politicians). Therefore, as voters cannot directly choose both politicians, they vote for party A’s candidate - M_A - if her platform joint with the one of party A’s candidate-president - P_A - is sufficiently good for them. That is, if:

$$W^j(g_A^m, r_A^m) + \rho W^j(g_A^p, r_A^p) \geq W^j(g_B^m, r_B^m) + \rho W^j(g_B^p, r_B^p) + \sigma^{ij} + \delta \quad (\text{D7})$$

Since citizens only directly elect M, the subsequent provincial election (and provincial public good) becomes less salient to them; thus, they mis-calculate and under-weight their province-based utility, as captured by $\rho > 0$. As in PT, I assume that voter i ’s idiosyncratic party-bias σ^{ij} has the same distribution in every group j , and it is uniformly distributed on $[-\frac{1}{2\phi}, \frac{1}{2\phi}]$; the popularity shock δ is common to all voters and uniformly distributed on $[-\frac{1}{2\psi}, \frac{1}{2\psi}]$.

Municipal politicians care of endogenous (r^m) and exogenous rents (R) - which they only get in case of victory - but they also care of provincial rents, because they could face internal-party retaliation if they do not propose sufficient rents for the provincial candidate (r^p). I implicitly assume here that $r^p > 0$ and focus on internal solutions. M’s objective function is thus:

$$E(V_A^m) = p_A(\gamma r_A^m + R_A) - \frac{1}{r_A^p} \quad (\text{D8})$$

M's proposal is thus binding for P's rent-seeking behavior, but M internalizes the internal party-pressure to allow P diverting rents as well. The Ms from both parties compete at the elections on a binding platform $[g^m, g^p, r^m, r^p]$ so to maximize their expected utility. The crucial assumption of the model is thus that the municipal politicians running the campaign can present a credibly binding platform for the provincial policy as well.³⁷ Thus, one can derive the probability of victory for the mayor of party A in municipal election, which is given by:

$$p_A = 1/2 + \psi[W^j(g_A^m, r_A^m) - W^j(g_B^m, r_B^m) + \rho W^j(g_A^p, r_A^p) - \rho W^j(g_B^p, r_B^p)] \quad (\text{D9})$$

What are the optimal g^m and g^p ? Taking the FOC of D8, we get:

$$\frac{\partial E(V_A^m)}{\partial g_a^m} = \frac{\partial p_A}{\partial g_A^m}(\gamma r_A^m + R_A) \longrightarrow (\gamma r_A^m + R_A)\psi W_g(g_A^m, r_A^m) = 0 \quad (\text{D10})$$

Since $W_{gr} = 0$, then $W_g = 0$ for each level of r ; thus, given the definition of W , this implies that municipal public good is optimally provided (as defined in PT) and the same is true for g^p . Notice that, by symmetry, in equilibrium both parties will converge to the same policy: $g_A = g_B = g^*$. This also implies $p_A = p_B = \frac{1}{2}$.

What about rents? Since:

$$\frac{\partial p_A}{\partial r_A^m} = -\psi \quad \text{and} \quad \frac{\partial p_A}{\partial r_A^p} = -\rho\psi \quad (\text{D11})$$

then,

$$\frac{\partial E(V_A^m)}{\partial r_A^m} = \frac{\partial p_A}{\partial r_A^m}(\gamma r_A^m + R_A) + \gamma p_A \longrightarrow -\psi(\gamma r_A^m + R_A) + \frac{\gamma}{2} = 0; \quad [r^m \geq 0] \quad (\text{D12})$$

$$\frac{\partial E(V_A)}{\partial r_A^p} = \frac{\partial p_A}{\partial r_A^p}(\gamma r_A^m + R_A) + \frac{1}{(r_A^p)^2} \longrightarrow -\rho\psi(\gamma r_A^m + R_A) + \frac{1}{(r_A^p)^2} = 0 \quad (\text{D13})$$

therefore:

$$r^m = \text{Max}[0, \frac{1}{2\psi} - \frac{R_A}{\gamma}] \quad \text{and} \quad r^p = \sqrt{\frac{1}{\rho\psi(\gamma r^m + R_A)}} \quad (\text{D14})$$

Plugging the expressions in D14 one into the other, we get: $r^m = \text{Max}[0, \frac{1}{2\psi} - \frac{R_A}{\gamma}]$ and:

³⁷This is a strong assumption, but one can think as the reputation-cost of breaking the promise to be so high that the internal party decision on who is the party candidate in the province necessarily enforces the promises.

$$r^p = \begin{cases} \sqrt{\frac{1}{\rho\psi R_A}} & \text{if } r^m = 0 \\ \sqrt{\frac{2}{\rho\gamma}} & \text{if } r^m > 0 \end{cases} \quad (\text{D15})$$

Therefore, **an increase in ρ reduces provincial rents**, suggesting that the level of popular attention, or salience, is crucial in determining provincial rents. As for municipal rents, notice that they are exactly equal to the direct-election case.

Under **direct elections** both at municipal and provincial level , with voters choosing each politician directly, the optimal amount of public spending would still be reached, while rents are just like the standard case in PT, that is:

$$r^m = \text{Max}[0, \frac{1}{2\psi} - \frac{R^m}{\gamma}] \quad \text{and} \quad r^p = \text{Max}[0, \frac{1}{2\psi} - \frac{R^p}{\gamma}] \quad (\text{D16})$$

This means that if provincial rents are zero in the direct election case, then they are always higher with indirect elections (since rents are always positive in that case). Otherwise, there is a threshold ρ^* , below which the indirect election generates more rents than the direct case. Also notice that as $\rho \rightarrow \infty$, provincial rents go to zero, and that as ρ gets close to zero, rents will only be constrained by a provincial tax $\tau^p = 1$ and consequently by the maximum available diversion: y^p (when $\rho = 0$ the probability of victory does not depend on provincial welfare at all, thus it will be optimal to set $r^p = y^p$). After making reasonable assumption on the parameters of the model it is possible to calculate the value of ρ that generates the observed increase in rents (between 10% and 20%) when indirect elections are introduced.

Overall, this model suggests that it is not indirectness per se that causes the drop in accountability and the increase in rents, but rather the intrinsically connected reduced popular attention. This is consistent with the fact that accountability may still remain extremely high in formally indirect electoral processes whereby the salience is all on the second level electoral result (think of the Electoral College in the US).