

Better to be direct? Evidence from the abolition of direct elections in Italian local governments

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Abstract

I provide evidence for the importance of direct electoral processes by investigating the consequences for public spending of an unexpected reform that repealed direct elections as the way to select local politicians in Italy. Provincial elections were substituted with an indirect process, 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 where the provincial presidents were born tend to receive disproportionately more public funds after the reform. Second, the share of provincial resources spent on public goods drops in favor of bureaucratic costs. I discuss suggestive evidence hinting that these results are driven by lower electoral incentives rather than by the selection of worse politicians.

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1 Introduction

Since the end of WWII, western countries have progressively pushed towards democratization, with more and more political offices being assigned by citizens through general elections. Although elections may generate distortions in public spending and electoral cycles (Persson and Tabellini (2003), Maskin and Tirole (2004), Hessami (2018)), a vast political-economic literature shows that democratization allows voters to hold politicians accountable, reducing corruption and helping newly enfranchised groups to receive a bigger share of public resources (Fujiwara (2015), Cascio and Washington (2013), Martinez-Bravo et al. (2012), Aidt and Dallal (2008)). The intuition behind this result is straightforward: accountable politicians need to efficiently provide public goods and services to the voters in order to be (re-)elected.

Democratization may actually come in very different shades, and each peculiar electoral law yields a different degree of representativeness. In a broader categorization, public offices may be assigned through direct elections, namely a popular vote, or through indirect elections, whereby voters only choose the final appointers.¹ The two categories mainly differ in two aspects: the selection process and the electoral incentives they generate. Direct choice comes with a fully democratic selection of politicians and a strong bundle of electoral incentives generated by the need to please voters and by the high public attention. Indirect elections may reduce the salience of the process and partially isolate appointed officials from electoral incentives, while selection may be based on criteria other than representativeness (competence, seniority, party loyalty, friendship etc.). Due to the interplay of selection and accountability, whether the passage to indirect elections (or the other way around) leads to distortions in public spending is an empirical question that the literature has only partially answered.

In this paper I cast some light on the importance of the incentives generated by direct elections for an efficient provision of public good. To do so, I analyze an electoral reform in Italy, which replaced popular elections in the local governments of the provinces (middle sized entities, including on average 80 municipalities and presided over by a president) with a form of indirect election whereby the president of the province is chosen by all the mayors and councilors

¹See Akzin (1960) for a discussion on the overlapping of the notions of indirect election and appointment in democratic electoral processes.

of the municipalities (civic local governments) located inside a province. Moreover, the pool of eligible candidates is restricted to municipal mayors, who are still directly (s)elected by citizens. Interestingly, the reform's permanence was arguably a *historical accident*: the change in the electoral law was supposed to be a minor, transitory step towards the complete elimination of the provincial governments. However, the constitutional referendum necessary to change the constitution and eliminate provincial governments unexpectedly failed, 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 a very rare event in a stable democracy, which makes this a precious opportunity to evaluate this phenomenon in the contemporaneous western world; second, the reform was relatively exogenous as it was imposed from an central government to a local one with the only scope of saving the public resources for the provincial politics, before the provincial governments could be eliminated.

After the reform, the pool from which provincial politicians are chosen is still democratically selected via a direct municipal election: if elections only matter because citizens are able to select better candidates, this channel was not jeopardized. However, the final indirect election of provincial leaders isolates politicians from provincial voters *de facto* reducing accountability; this is made more severe by the reduced salience of the indirect electoral process. This setting suggests that the observed consequences of the reform are driven by the drop in electoral incentives, rather than by a change in the selection process. I provide evidence that the reform did not affect politicians' education, which I take as a proxy for quality (as in Baltrunaite et al. (2014), Galasso and Nannicini (2011), Kotakorpi and Poutvaara (2011)), and that it did not affect their average age. This confirms the important role played by (*direct*) accountability.

While presidents are held accountable by the provincial council both before and after the reform, several reasons suggest that accountability towards citizens was severely undermined when elections became indirect. First, the reform dramatically reduced the number of people directly holding provincial politicians accountable from hundred of thousands to few hundreds: if the pass-trough of accountability from citizens to municipal politicians is not perfect, provincial politicians may end up being unaccountable to the general public.² Second, politicians rather

²Municipal politicians' vote is weighted by the size of the city they represent, meaning that the support of a small number of politicians from relatively big cities may be enough to win; clearly there is no guarantee that

than citizens are the final electors: since they are members of a party, they may have strong biases towards the provincial leaders (or a higher cost of crossing party lines), and may vote only based on party affiliation. Third, the assembly of all municipal electors only meets for the provincial elections and its members are different every time (each year a share of municipalities holds elections and renew part of their politicians): this makes it hard for municipal politicians to monitor provincial leaders and to enforce future punishments.³ Finally, but perhaps most importantly, municipal electoral campaigns focus almost exclusively on municipal issues, making provincial politicians' appointment an extremely low-salience topic for citizens. This drop in salience, exacerbated by the fact that municipal and provincial elections are not synchronized, strongly dilutes provincial leaders' accountability. Consistent with this, I find evidence of a severe drop in the salience of provincial elections after they became indirect, and of an almost complete lack of discussion on provincial issues during municipal elections.⁴

With a difference-in-differences strategy I show that the introduction of indirect elections negatively affects both the geographic allocation of resources and the general composition of public spending. First, I demonstrate that, since the reform, the municipalities connected to the provincial presidents receive 10-30% more public transfers per capita compared to the unconnected ones. This effect is driven by the municipalities where the presidents were born, more than by those where they were elected mayors. I don't observe a similar favoritism towards other municipalities whose mayors share their ideology with the president, suggesting that presidents take the support of same-party politicians for granted and only act in their own self-interest. This analysis provides a concrete example of the types of misbehavior fostered by the reform and it rules out other possible channels, such as that presidents are trying to please some mayors (their new electors) to secure their vote. Second, I provide a more general analysis of how the provincial budget changed. As the electoral reform targeted provinces and not municipalities, I compare the change in provincial public spending on transport, education and administration (the main responsibilities of provincial governments) to the corresponding types of expenditure

their interest is in line with the general interest of provincial citizens.

³The assembly of mayors, a secondary provincial entity, can only approve changes in the provincial statute.

⁴Figure A3 shows how few people searched for the terms *president* and *province* during municipal or provincial elections after the reform, compared to the pre-reform period: information on provincial elections severely shrank both during the provincial ballot itself and during municipal elections (when citizens vote the final appointers).

performed by the municipalities contained within each province, before and after the reform. I find that provinces reduced the *share* of current expenditure directed to public goods by 3.9 (in transport) and 5.5 (in education) percentage points and increased the share directed to bureaucracy by 10 percentage points. My main outcome is the share, rather than the total amount of resources allocated to different sectors, to highlight the local political decision of resource allocation. This choice is in line with the literature on budget composition (Rauch (1995), Hessami (2014)), which studies how politicians allocate available resources assuming a fixed budget. Indeed, this is exactly what happens in the case of provinces: Italian local politicians have a sufficient discretion to influence the resource allocation across sectors (Grembi et al., 2016), but provinces have limited discretion on the overall size of their budget, which largely depends on grants from the government or on very inflexible taxes. Consistently, the effect on absolute expenditure goes in the same direction as that on relative expenditure. Using a similar strategy, I provide suggestive evidence of an increase in car accidents in provincial roads compared to urban ones. I interpret these findings as evidence in favor of the hypothesis that drops in accountability cause a less equitable and less efficient allocation of public resources.

This paper contributes to the literature on the importance of the direct election of political representatives, on the one hand by empirically showing that the indirect elections of the leaders of local governments are associated with a distortion in public spending, and on the other by developing a simple theoretical model explaining how rent seeking increases when the salience of the indirect election drops. Part of the quasi-experimental evidence on this topic has relied on what is more formally a change from a parliamentary system to a presidential one, whereby the president is directly elected: this approach makes it impossible to disentangle the role of the direct vote from that of the increase in presidential powers and from the broader change in the political system. This was the case of the replacement of indirect with direct elections in Indonesian local administrations, which was associated with more political budget cycles (Sjahrir et al. (2013)) but higher health expenditure (Skoufias et al. (2014)). The Italian reform is exceptional in this sense because the provincial government remained a presidential system before and after the reform, but provincial leaders are now only indirectly accountable to citizens: this allows me to identify the role of direct accountability, isolating it from other

institutional changes.⁵ Other papers relied on the passage of the Seventeenth Amendment in the US, to show that the change to direct election of senators in 1912 made them more responsive to the electorate (Gailmard and Jenkins (2009)), less polarized (Bernhard and Sala (2006)) and more active in terms of bill-sponsorship and roll-call participation (Meinke (2008)). Crucially, the fact that in my case study the full provincial government (and not just a branch) becomes indirectly elected, and the focus on multiple local governments (rather than on members of a single central government), allow me to study the effects of indirect elections on the activity of the all government, and more precisely on its public spending, rather than on the productivity and responsiveness of single politicians. The paper closer to mine in terms of contribution is Hessami (2018), which compares directly elected and appointed mayors in Germany to show that elected mayors obtain more grants in election years, while appointed politicians do not indulge in political business cycles. My peculiar setting allows me to study the reverse process (a move towards a *less* direct vote), and a different set of outcomes (local favoritism and budget allocation). Similarly, my findings are in line with the literature comparing elected and appointed officials: for instance, Besley and Coate (2003) find that elected regulators in the US behave more in favor of consumers than the appointed ones.

I then speak to the research on birthplace favoritism, a phenomenon documented both in settings with authoritarian institutions (Hodler and Raschky (2014), Do et al.(2007)) and strong democratic traditions (Baskaran and Lopes da Fonseca (2021), Folke et al. (2021), Fiva and Halse (2016), Maaser and Stratmann (2016)). With respect to these papers, I contribute by showing that the local lawmakers' bias towards their birthplaces rises only after direct popular accountability drops, which provides an interesting insight for policy interventions. Consistent with Fiva and Halse (2016), I present suggestive evidence indicating that president' preference to favor their hometown exists even absent a direct electoral incentive to do so. My results are thus consistent with Carozzi and Repetto (2016) that show that the hometowns of Italian national MPs receive more transfers, but for MPs not representing their hometown district such bias vanishes close to elections. In this case, I contribute by instead focusing on a different change

⁵Formally, before the reform the system was a *neo-presidential* one (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 councilors mandate.

in accountability, namely a permanent transition to indirect elections, and on local governments rather than on national individual politicians.

Finally, this paper speaks to the literature on democratization (Fujiwara (2015), Cascio and Washington (2013), Martinez-Bravo et al. (2012), Aidt and Dallal (2008)). My first contribution is to show that (partially) disenfranchising citizens has detrimental effects on the efficient provision of public good. Indeed, symmetry with enfranchisement should not be taken for granted since path dependence may preserve an efficient provision of public goods even after elections are abandoned. Moreover, differently from most of the aforementioned studies, I study this in the context of a political natural experiment within a democratic country and in the absence of other major political or ideological shocks. This rare event can be extremely informative on the consequences of the elimination of general elections in favor of appointment by a restricted elite. As a second contribution, while other studies often fail to identify whether selection or accountability are driving the result, the peculiarity of the Italian reform allows me to assess the behavior of politicians whose electoral *incentives* have dramatically changed, but who still need to pass through a direct and democratic form of pre-selection at the municipal level. I can thus attribute the effect of disenfranchisement to the change in accountability rather than to a change in the type of representatives. In this sense, my setting provides an alternative and to some extent better test to the use of term limits to study changes in politicians' accountability while holding selection constant (Alt et al. (2011), Aruoba et al. (2019), Ferraz and Finan (2011)). First, politicians who reach the term limit or who run for term-limited offices may substantially differ from the others in terms of quality, experience and ambitions. Second, (the absence of) term limits only discipline politicians through one type of electoral incentive, namely the possibility of re-election, but elections introduce a wider set of incentives. Visibility, pre-electoral promises and campaigns, sense of responsibility towards those who voted for them are examples of electoral incentives that bind even those directly elected politicians who are in their last term. The peculiarity of the Italian reform allows me to capture the effect of this more complete set of incentives affecting accountability.

The paper is organized as follows: Section 2 provides an institutional background, Section 3 discusses the conceptual framework(s) and Section 4 describes my data. I present my identifica-

tion strategy and the main results in Sections 5 (hometown favoritism) and 6 (sectoral allocation of resources); finally Section 7 discusses threats to identification and alternative explanations.

2 Institutional Background

The Italian constitution subdivides the national territory into three main layers of government, from larger to smaller: regions (20), provinces (110), and municipalities (around 8,000), as exemplified in Figure 1. Since 1951, the politicians ruling provinces and municipalities have been selected with direct elections. The electoral laws have changed over time but have always preserved universal suffrage. In particular, since 1993 voters have directly elected the president of the province and the provincial council through a dual ballot election. Regions, provinces, and municipalities are in charge of different duties, which have remained virtually unchanged since 2000 (Law 267). Specifically, provinces are mainly responsible for public transport, construction and maintenance of roads and schools and some standard administrative functions (local tax collection, bureaucratic procedures etc.). Altogether these duties account for more than 70% of the overall provincial spending, while the residual ones only play a marginal role.⁶

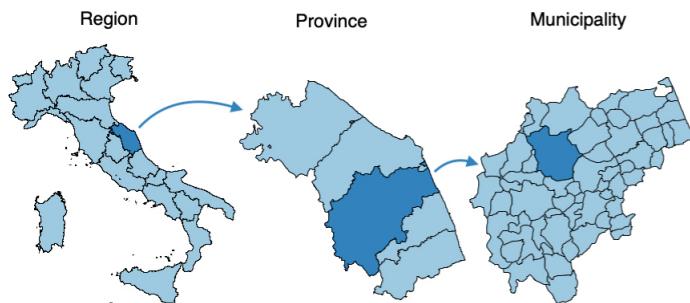


Figure 1: Territorial division: local governments

Recently, a strong demand for a public-spending review led many parties to demand the elimination of the provincial governments and the transfer of their responsibilities to other institutions (mainly the regions). However, since provinces are explicitly mentioned in the Constitution, a constitutional reform followed by a popular referendum is needed to remove them (see Longo and Mobilio (2016) for a detailed pre-referendum discussion on the expectations about the fu-

⁶These include environmental protection (parks, cultural heritage, laws on hunting and fishing, water policies, natural disasters), waste management, inter-municipalities coordination.

ture role of provinces).⁷ Therefore, the government planned a two-step political action: first, the Parliament issued the transitory Delrio Act (April 2014), which substituted direct elections for provincial leaders with political appointment (to momentarily reduce the politics-related costs); afterwards, a provision within the much broader constitutional reform - whose first version was presented in the Senate on April 8th, 2014 - would have canceled every reference to the provinces from the Constitution, allowing for their complete abolition. The project was not successful: while the Parliament approved the Delrio Act, the constitutional reform was rejected by a popular referendum in December 2016. During the drafting and parliamentary discussion, a rejection of the reform was unexpected as opinion polls reported a landslide approval until July 2016.⁸ Interestingly, there was very little public discussion and awareness of the relevant changes that the Delrio Act was bringing about, since the incoming constitutional referendum was monopolizing public attention.⁹

As a consequence of the failure of the constitutional reform, provinces remained in place and the Delrio Act became the permanent law regulating them. In particular, the new law mainly regulated the electoral process to select the leaders of the provinces (and metropolitan areas) and had only minor provisions regarding other layers of administration.¹⁰ The new electoral law totally abolished general elections for the roles of councilor and president of the province. Instead, only the mayors and councilors of the municipalities (i.e. the politicians at the lowest administrative level) can now vote to appoint provincial politicians. Furthermore, only municipal mayors are eligible as presidents, which is the more powerful provincial office. Elected officials hold both positions but initially only earned wages as mayors; additional wages for the presidents were re-introduced in 2020. In practice, a province includes on average 80 municipalities: each

⁷A similar reform was attempted in 2012: the government postponed 2012 and 2013 provincial elections and introduced a comprehensive reform of the provinces, effective since 2014. However, in July 2013 the Constitutional Court rejected the law, which was thus never implemented (and whose only effect was to postpone some elections).

⁸Figure A2 shows that opinion polls forecast a landslide approval for the referendum between October 2015 (first approval of the definitive version of the reform in the Lower House) and April 2016 (final parliamentary approval). The approval's lead started then shrinking and finally disappeared around July, 2016.

⁹Figure A1 plots the Google searches for the word *province* and *referendum*. The absence of peaks in searches for the term *province* in April 2014 (when the first version of the constitutional reform was also presented in the Senate) suggests a low salience of the event.

¹⁰The reform had only indirect effects on municipalities: first, it incentivized the *mergers* of very small municipalities (mergers were regulated even before the law and only about 2% of municipalities merged after 2014: these were only the smaller ones with negligible expenditure); second, it defined more stringent gender quotas in municipal councils; third it slightly modified the organization of the *union of municipalities*, a long existing inter-municipality partnerships helping small neighboring municipalities to jointly provide services.

of them has a mayor and a municipal council. After the reform, only these 80 mayors can run for the office of provincial president, and only the mayors and the members of the 80 municipal councils will have the right to vote for the provincial government (their vote is weighted by their municipality's population).¹¹ Figure 2 exemplifies this mechanism. On average, 13 provincial seats must be filled, a president and 12 councilors, but the council size increases with the provincial population.¹² To run for presidents, candidates need their municipal office to last at least 18 more months and the written support of 15% of eligible voters. Provincial presidents remain in charge for four years.¹³ Almost all the provinces had elections in 2014, as shown in Figure 3.

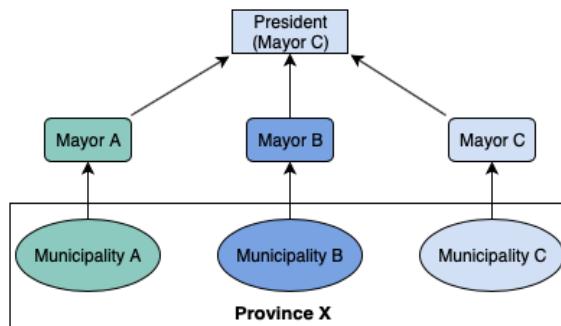


Figure 2: Post-reform indirect electoral system (here the mayor of municipality C becomes president)

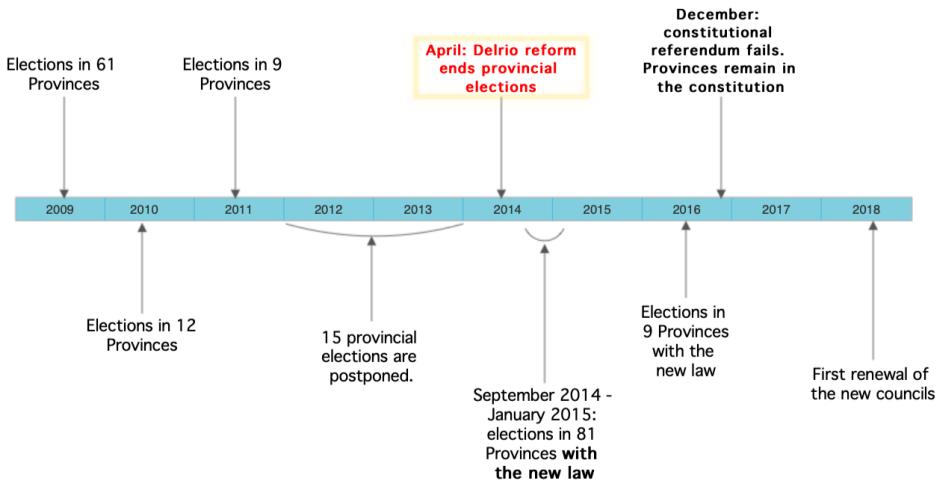


Figure 3: Timeline of the provincial elections

¹¹More specifically, municipalities are divided into nine population bins, and each bin is associated to a weight. The number of votes cast by a municipality's representatives is multiplied by its weight and the candidate-president with more weighted votes is elected. Then the D'Hondt method is applied to attribute the number of councilors to each coalition. Implicitly, a majority bonus exists via the election of municipal politicians. Before the reform, the candidate with an absolute majority of votes (in the first round or, alternatively, in a runoff election) would be elected president and its party would get 60% of the seats allocated with the D'Hondt method.

¹²The average council size shrank from 30 to 24 members in 2011 and to 12 members in 2014. In the robustness checks sections I provide evidence to exclude that this change explains my results.

¹³Councilors are elected every two years. Before 2014, presidents' and councilors' term length was five years.

The new system made the provincial government directly accountable not to the provincial citizens, but only to a pool of municipal politicians that changes every year and that only gathers for provincial elections. Moreover, since municipal electoral campaigns never deal with the provincial vote and citizens are typically unaware of which provincial politician will municipal representatives vote for, the pass through of accountably is severely undermined.¹⁴

The reform explicitly listed the province's responsibilities, which remained stable, maintaining education (school infrastructure) and transport (public transport, road maintenance) as the most relevant functions. While provinces were formally allowed to transfer functions towards the regions, responsibilities remained stable, as confirmed by the 2015 *Council for Local Autonomies* report and by wide journalistic evidence.¹⁵ A change of responsibilities prescribed by the reform could confound my results; thus, in Section 7 I perform a range of tests to show the robustness of my results to this potential identification threat.

The analysis of the reform is complicated by the progressive reduction in governmental funds to the provinces, which was perceived as a serious threat for public good provision.¹⁶ ¹⁷ To overcome this issue, I focus on the share of total spending allocated to each sector, in order to shed light on the political (and local) process of resource allocation. Notice that around one third of the (current and capital) expenditure of local Italian governments is considered non rigid (Grembi et al., 2016), leaving a large discretion to local politicians in the allocation of resources.

¹⁴The terms *president* and *province* are rarely searched on Google during municipal elections (see Figure A3).

¹⁵"... provincial responsibilities remained the same: the maintenance of 135.000 Km of roads (...) and the maintenance of 6000 schools," from Il Corriere della Sera Jan/06/17.

¹⁶"The average incidence of the cut in funds in the budget laws between 2012 and 2018 on the provinces' revenues was 60.4% - notes the Union of Italian provinces - (...) they will cause a disastrous effects on the provision of essential services to citizens." Il Fatto Quotidiano, 2019/05/03. "provinces are in full stagnation... and having cut funds without cutting responsibilities delays or reduces services to the citizens." Cuneo24.it, 2019/11.

¹⁷See Figure C7. From time to time the reduction of resources forced the central government to increase funds to provinces to make sure essential services were provided. At the same time, the budget law in 2015 mandated a 50% reduction of provincial personnel, through non-replacement of retired personnel and transfers of employees to other administrations, mostly the regions. In practice, this reduction was progressive and it accelerated the already existing trend of reduction in provincial personnel. According to the census of local public administrations, provinces had 54644 active workers in 2009; 48034 workers in 2014; and 23274 in 2020. A similar, though slower trend existed among municipalities that employed 383480 workers in 2009; 350155 in 2014; and 299276 in 2020.

3 Conceptual framework(s)

3.1 Selection and Incentives

The replacement of direct with indirect elections could potentially affect the selection of politicians, besides their incentives. Conceptually, I argue that changes in selection should be limited in this specific setting because the pool of province-eligible politicians are still selected with a direct popular vote in the first place (in municipal elections). However, it could be the case that the worst mayors are appointed as provincial presidents by their colleagues or that the reform induced worse politicians to run for mayor in the first place; similarly, if provincial politicians were relatively (un-)experienced compared to the municipal ones before the reform, it is possible that (older) younger presidents are now selected. While almost half of the mayors in power in 2015 had been elected before the reform, so that their decision to run in the municipality could not have been affected, it is important to check whether politicians elected after the reform are systematically different. Therefore, I compare provincial (treated) and municipal (control) leaders' age and level of education around the reform date, using a simple diff-in-diff framework.

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

	(1) Years of education	(2) Age
ProvincLeader*PostReform	-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 years of education and age of local politicians. ProvincLeader takes value 1 for provincial presidents and 0 for municipal mayors. PostReform becomes 1 after 2013. Robust standard errors, clustered at the province level, are in parentheses, * p<0.10, ** p<0.05, *** p<0.01.

Table 1 reports the results of this exercise, while Figure A4 plots the same outcomes over time: both analyses exclude a change in presidents' characteristics.¹⁸ These results are thus incompat-

¹⁸Given the introduction of gender quotas in municipal councils mentioned in footnote 10, I do find some imbalance in terms of gender, that caused a relative increase in female representation at the municipal level. This change in selection at the *municipal* level cannot explain results in Section 5, where the municipal control groups is not used at all (transfers are not determined by the municipalities which only receive them); moreover, it is also at odd with results in Section 6 where, looking at the figures, we see that the discontinuous behavior is coming from the provincial budget and not from the municipal one. In any case, I include the reported gender among the controls in Table 3: its inclusion actually increase the point estimate and the significance of the estimated effect.

ible with a story of less educated or experienced representatives being selected after the reform. Taking education as a proxy for the quality of politicians (as in Baltrunaite et al. (2014) or Galasso and Nannicini (2011)), they help ruling out a negative selection mechanism.

A second issue concerns the drop in presidential wages after the reform. Since presidents only maintain their wages as mayors, their income-related incentive to perform well in the province could have decreased. The government's budget law approved in late 2019 offers an occasion to disentangle the effect of the relative decrease in wage from that of electoral accountability. Indeed, provincial presidents' salaries were raised in 2020 to a level similar to the pre-reform 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. Results in Section 6 will show no evidence of such reversal, while only a minor evidence can be detected in Section 5, when really the COVID-19 pandemic tended to level out the amount of transfers across municipalities, regardless of the level adjustments in politicians' salaries.

3.2 Theoretical model(s)

I present here two simple theoretical models - one originally developed in this paper and one taken from Persson et al. (1997) - that help conceptualizing my findings that indirect elections translate into higher hometown favoritism and a more costly bureaucracy.¹⁹ Indeed, in a modern democracy, where an independent judiciary power punishes the most severe forms of corruption or diversion, reducing accountability can induce more subtle forms of rent-seeking behaviors

¹⁹In Appendix E I also discuss a third model, which is an extension of the standard accountability model in Persson and Tabellini (2000). I compare a case with the direct election of both layers of government to one with a direct election of the municipal government that in turn elects the provincial government. Results depend on the assumptions on how municipal politicians vote; however, assuming both politicians are simply maximizing rents, one can show that, with indirect elections, the prominent position of the provincial government allows it to act as a first mover in the rent diversion and thus obtain more rents than in the direct election case. If instead we assume that the municipal politicians always vote for their party's provincial candidate, the latter could even get away with all the rents she can steal and be reelected. My results are instead inconsistent with models that rely on the creation of a minimum winning coalition (MWC). Having municipal politicians as final electors, the president can target each elector with transfers. Theoretically, this would allow the president to identify a majority of the municipal electors, give them just enough resources to secure their vote and keep all remaining resources for herself. In practice, what we observe in the indirect provincial elections is that municipal politicians always vote based on their party membership. This makes the outcome of the indirect provincial election quite deterministic, which in turns makes the MWC unnecessary since the provincial politicians can take those votes for granted and keep more resources for themselves. All in all, I do not find much evidence of groups of municipalities receiving extra shares of resources (apart from presidential hometowns), suggesting the MWC is not the mechanism explaining my results. Moreover, as I will discuss below, Figure 6 shows no evidence of party favoritism in the allocation of resources following the reform.

that benefit the politicians: hometown favoritism and increased bureaucratic costs are classic instances of such milder types of rent seeking.

The first and preferred theoretical model, which I have developed for this paper and I discuss in more details in Appendix D, is an original extension of the probabilistic voting model with rents from Persson and Tabellini (2000). In this model, voters derive utility from the public spending of two layers of government (municipalities and provinces), but they are only allowed to vote for the municipal government. Under the assumption that municipal politicians always vote a provincial candidate based on their party affiliation, voters end up choosing a bundle of two policies-politicians from the same party. The model predicts that the amount of provincial rents depends on how salient the provincial policy/election is - captured by a parameter ρ - when citizens are electing their municipal politicians. In particular, provincial rents increase when less popular attention is devoted to the provincial dynamics. This approach highlights the efficiency cost citizens may suffer if the indirect election distracts them away from the provincial policy. Indeed, when ρ is sufficiently small, the amount of rents with indirect elections is 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 dramatically reduced the attention devoted to provincial elections, as evident from the drop in the number of Google searches plotted in Figure A3.²⁰ Notice that the role of salience is crucial in this model: if an indirect election manages to maintain a very high level of salience (take for instance the election of a national president in a parliamentary system), then this result is not granted.

A second prominent model describing how indirect elections can generate an increase in presidential rents is presented in Persson et al. (1997). The paper compares the amount of public good and presidential rents in three political systems: one with directly elected president; one with directly elected president and legislature; one with a directly elected legislature which in turn indirectly elects the president. The president has agenda setting powers on the allocation of the budget, while the legislature has veto power on the budget proposal. Under these assumptions, a system with direct elections of the president generates fewer presidential rents than one with

²⁰Notice that not only the amount of Google searches for the words *Province* and *President* dropped during provincial elections, once the vote became indirect, but the number of searches became also virtually negligible during the municipal election, that is when the municipal appointers are really selected. This suggests that the salience of the future provincial vote dropped dramatically, when citizens are selecting municipal politicians.

indirect election. The 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 to collude. Such threat forces the voters to set their reservation utility so to let the president get away with an additional amount of rents compared to the case of direct elections.²¹ The model is extremely insightful on how indirect elections can increase the bargaining power of the agent that is indirectly appointed, while leaving the directly elected agents as well off. This conceptual framework partially departs from my setting because the universe of municipal politicians, as opposed to a parliament, do not have explicit veto powers on the amount of transfers proposed by the president. Nevertheless, it is reasonable to conjecture that presidents still need a basic level of agreement with municipal authorities.

4 Data and Sample

The core of my data comes from AIDA PA, a restricted database that provides information on the type of public expenditure and revenues of the Italian local administrations. The main advantage of AIDA PA is that it provides budget data up to 2020 that are already homogenized throughout years, places, type and sector of spending, which allows me to avoid any subjective categorization.²² In my main analyses I focus on current spending, as it accounts for almost 70% of total revenues; however, I also discuss the capital expenditure dynamics, when available.²³ More specifically, in my geographic favoritism analysis I rely on AIDA PA data to study

²¹A 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 of reappointing the presidents, the latter can propose a budget that gives the legislature a higher utility than what they would obtain if reelected, making them prefer to forgo reelection. In this case, both presidents and legislature would 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 at its status quo rents; the president divers more and both are reelected.

²²There was a relevant error in the AIDA database. The *Transport* sector of spending is constructed by summing up 5 main subcategories: the two most relevant ones (accounting for more than 95% of it) are ‘expenditure on public transport’ and ‘expenditure on roads’. Before 2016, however, the ‘transport’ category erroneously coincided with ‘expenditure on public transport’ alone. I checked the financial statements of some provinces in different years to make sure this was indeed a mistake. I fixed the problem by generating a new variable that equals the sum of the 2 main subcategories. In Table C8 and Figure C23, as a robustness check, I sum the missing subcategory (namely, expenditure on roads) to the denominator: results are virtually unaffected.

²³Current spending refers to the current provision of public services, such as yearly contracts of services or yearly maintenance of existing infrastructure; capital spending refers to longer-term investments. The former

the amount of current transfers that municipalities have received from any other institutions between 2011 and 2020. I mainly rely on this aggregate measure since AIDA does not provide disaggregated data for municipal transfers for the relevant years, but I also integrate this measure with the information from the SIOPE dataset, to also isolate transfers coming from local public administrations. Instead, in my analysis on the composition of public spending I focus 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 for provinces before the reform, as well as information on the candidates' level of education and birthplace. I then collected post-reform electoral data at the province level and presidents' personal information from newspaper and provinces' websites. Finally, I use data from the Italian Institute of Statistics on the number of car accidents as a measure of the quality of the roads. Specifically, accidents located on non-urban roads, among which provincial roads are the most represented, are a proxy for the low quality of provincial transportation, while urban accidents are associated with the low quality of municipal transportation.²⁴

Table 2: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Total curr. 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 current 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 current 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 current 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-municipality level between 2011-2020 in rows 1-3 and at the year-province level between 2009-2020 (2015 for administration) in rows 4-13. 'Shares' are divided over total current expenditure.

accounts for about 70% of total revenues both in provincial and municipal budgets whereas the latter accounts for less than 20% (see Figure C5). AIDA PA provides detailed capital spending data at the province level up to 2015 and never for the education sector.

²⁴Non-urban roads also include state roads, but not high-ways.

In the analysis on the spending composition (Section 6) I restrict my sample to the 94 regular provinces, which all had provincial leaders appointed by directly elected local politicians. I thus exclude three autonomous provinces that were not affected by the law and the provinces containing ten large metropolitan-cities, which were subject to a very different set of rules.²⁵ In the analysis on geographic favoritism (Section 5) I further exclude the autonomous regions of Sicily, Sardinia and Friuli and only focus on the remaining 5,603 municipalities.²⁶ Table 2 shows the summary statistics of the main variables of interest.

5 Indirect elections and geographic favoritism

5.1 Identification Strategy

After the introduction of indirect elections, the general population lost any direct control over provincial politicians. While severe misbehavior is still equally punished by the judiciary, the members of the provincial government may now find it less politically risky to mis-allocate resources. A natural question is thus whether provincial leaders provide disproportionately more resources to specific geographic areas, which may benefit themselves. There are mainly two such types of areas, which in practice tend to coincide after the reform: the municipalities where they were born and the ones where they are elected mayors. In the first case, presidents may respond to a classic rent-seeking behavior, whereby they want their own community to receive more public resources. In the second case, presidents may respond to an electoral/career incentive: maintaining the title of mayor is a necessary condition to remain in power in the province and sending resources to their own municipal voters may help them accomplish this goal.

To study this phenomenon, I investigate whether municipalities more connected to the provincial president receive more resources after the reform. In my main specification, I define the treatment group ($T_{m,t}$) as the time-varying set of municipalities that are birthplaces of the provincial presidents in office in each year; the control group is composed of all the remaining municipal-

²⁵These are respectively: Aosta, Bolzano, Trento; and Milan, Rome, Turin, Genoa, Bari, Naples, Bologna, Florence, Venice and Reggio Calabria.

²⁶Out of the 94 targeted provinces, 77 were equally affected while the remaining 17 (in Sicily, Sardinia and Friuli) were explicitly affected but the regions had some degree of autonomy in defining who could vote for the new legislative body (still local politicians, not the general population); in practice, Friuli abolished 3 of its 4 provincial governments, while in Sicily and Sardinia the (directly elected) regional governments appointed administrators (typically not mayors, but *commissari*) to take the place of provincial presidents.

ties. Since the first renewal of the provincial government after the reform, the great majority of presidents' hometowns also coincide with the municipalities they are mayors of.²⁷ My sample includes all municipalities of non-autonomous regions and non-metropolitan cities, observed between 2011 and 2020.²⁸ On average, the treated municipalities are larger than the control ones, but the median treated unit is still small, with a population of 10,100 inhabitants.²⁹ After the introduction of the DelRio Act in April 2014, provincial presidents became aware that no popular election would be held anymore, making (part of) 2014 the first treated period (results are robust to considering 2015 the first treated period).³⁰ I thus define equation (1), as follows:

$$Y_{p,m,t} = \alpha + \beta Treat_{m,t} + \delta(Treat_{m,t} * After_t) + \psi_{p,t} + \chi_m + \epsilon_{p,m,t} \quad (1)$$

where $T_{m,t}$ is a dummy equal to 1 if municipality m is the birthplace of the provincial president in office in year t and $After_t$ is 1 for years after 2013. $Y_{p,m,t}$ is a measure of public transfers to municipality m (in province p) in year t , while $\psi_{p,t}$ and χ_m are province-by-year and municipality-fixed effects, respectively. I thus exploit time-changing variation within-province and between municipalities. Every other specification, I include a control for the number of inhabitants in m and for politicians' characteristics such as gender, age, education and party. δ captures the effect of indirect elections on treated municipalities with respect to the control group. I cluster standard errors at the municipality level. My sample starts in 2011, after the fiscal reform that made municipalities less dependent from Central Government's transfers.³¹

The advantage of this specification is that the treatment group only contains those units that are connected to a president in the specific year in which she is in office, both before and after 2014. The main disadvantage, however, is that the treatment and control groups are time-varying. This could threaten my identification strategy if the reform also affected the

²⁷ 68 among the provinces relevant in this analysis had their first elections in 2014, 4 in 2015 and 6 in 2016.

²⁸ I also exclude provincial capital cities from the sample. Capitals tend to be bigger and they receive far more 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 Figures B3 and B4. To increase power, I include capitals in the analysis in Appendix B.1.5 and show that results are preserved.

²⁹ See Table B1 and B2 for additional summary statistics.

³⁰ In Appendix B.1.1 I show results from a slightly difference identification strategy (an event-study), in which each ever-treated unit's first treatment period is the year in which a president born in that municipality is elected for the first time after the reform. Figure B6 shows that such exercise yields results that are qualitatively identical to my main specification.

³¹ This reform is better discussed in Appendix B.1.5, where a longer time period is used for the analysis

characteristics of the municipalities that are connected to the president (e.g. if presidents elected after 2013 were born in larger municipalities and, in turn, larger municipalities had more transfers per capita). In order to maintain the treatment and control groups consistent throughout the whole period, I rely on a different and complementary identification strategy. More specifically, I include in the treatment group all the municipalities whose mayor became president of a province *at some point after 2013*, regardless of the exact year in which this happened. A municipality is thus part of the treatment group from the beginning of the sample, even if its mayor became president only in 2018; the treatment period still coincides with the years after 2013.³² Using the municipality where the president is mayor closely proxies the president's birthplace, but it will also provide suggestive evidence on the relative importance of hometown favoritism with respect to electoral incentives: indeed, presidents remain directly accountable to the citizens who chose them as mayors (notice that a specification like equation 1 would not be possible in this case, since presidents are mayors only after 2013). I will thus run the following regression:

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

where $Treat_m$ is a time-constant indicator equal to 1 if municipality m 's mayor becomes president of the province any time after the reform, and $After_t$ becomes 1 for years following 2013.

My main dependent variable in both specifications is the aggregate measure of municipal transfers provided by AIDA PA, 97% of which originates from public administrations. In my preferred specification I focus on transfers per capita as dependent variable, to directly measure how hometown favoritism affects citizens' access to public resources, accounting for the city size. Results are similar when normalizing by total revenues, rather than by population, or using the total 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 (results are confirmed when applying a 5% winsorization to the linear version of the outcome).³³ Direct

³²In this second approach, municipalities whose mayor become president from 2018 are already regarded as treated in 2014 (the great majority of provincial presidents were replaced in 2014 and 2018). This allows observing time-invariant treatment and control groups over time. This approach thus yields a lower bound of the true effect since some municipalities marked as treated in all years after 2013 are actually treated for a shorter period. As an alternative approach, I also construct an event-study in which each municipality is only treated when its mayor becomes president. This exercise, reported in Figures B7 and B8, confirms the results of my main specification.

³³Like the logarithm, winsorizing helps reducing the influence of outliers, while preserving the observations and

transfers from the province account for a small share of total transfers, but the president of the province can facilitate some municipalities to get access to funds from other local administrations (unions of municipalities, *comunità montane* etc.) or from higher administrative levels such as the region and the Central Government.³⁴ Indeed, it is common for presidents to act as intermediaries and lobby higher governments for additional funds to municipalities.³⁵ With direct popular accountability, the presidents would lobby for higher-level resources to those municipalities that are more likely to electorally reward their efforts; however, once accountability is reduced, even this lobbying process may be affected by geographic favoritism. In Appendix B.2 I use data from SIOPE to show that my results are robust to using an alternative dependent variable, that focuses on current and non-current transfers from local governments only (Regions or lower), as local entities are more likely than national ones to be affected by the presidents' lobbying.

In this analysis, I am implicitly assuming that the control group is completely unaffected by the reform (SUTVA). SUTVA may be violated if the additional resources directed to the treated group were indeed subtracted from those assigned to the control group. In practice, the total amount of transfers is not necessarily fixed (this is not a pure cake-splitting problem among municipalities) as additional resources can be introduced by the province or by higher administrative levels. Moreover, the fact that more than 98% of the municipalities are in the control group alleviates quantitatively this concern: even if additional funds to the treated units were taken from the control group, the average amount of transfers in the latter would hardly be affected on average. While I cannot prove or rule out that the reform had some impact on the overall amount of transfers, this possibility does not undermine my results, which should be interpreted as the effect on my specific treatment group *relative* to the control group.

5.2 Results

The impact of the introduction of indirect elections on the amount of transfers to municipalities per capita is reported in Figure 4. The time-varying treatment group is composed of the home-
their relative position. See results in columns 5 and 6 of Tables B4 and B5 and Figures B11 and B12.

³⁴On average provincial transfers account for 3% of overall transfers to municipalities, and for a comparable share of the provinces' total expenditure.

³⁵Some anecdotal evidence is reported here: <https://www.giornaledibrescia.it/brescia-e-hinterland/mancano-7-5-milioni-per-bus-e-corriere-rischio-tagli-da-gennaio-1.3533205>.

towns of the presidents in office. The figure shows a virtually perfect parallel trend in the years between 2011 and 2013, suggesting the two groups were extremely similar when provincial presidents were directly elected. This changes exactly in concurrence with the reform, after which the treated municipalities start receiving disproportionately more transfers compared to the control group. The gap keeps widening until 2020, when the outbreak of the COVID-19 pandemic caused a sharp increase in transfers to municipalities, which leveled out previous differences. Importantly, treated municipalities were connected to the president also before 2014, however it is only after the reform that connections generated the increase in transfers. Table 3 tests these results more formally, using equation (1) and confirms that the reform significantly increased the amount of transfers to treated municipalities by 25%, 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 and politicians' characteristics.

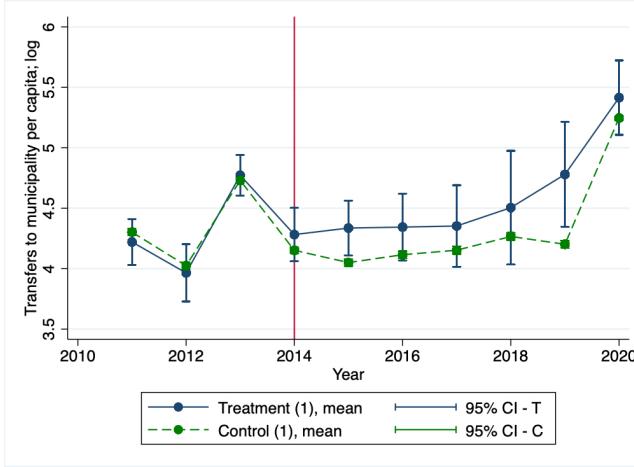


Figure 4: Trend of (the logarithm of) transfers per capita. The treatment group changes over time and it is composed of the municipalities that are place of birth of the president in office, excluding provincial capitals.

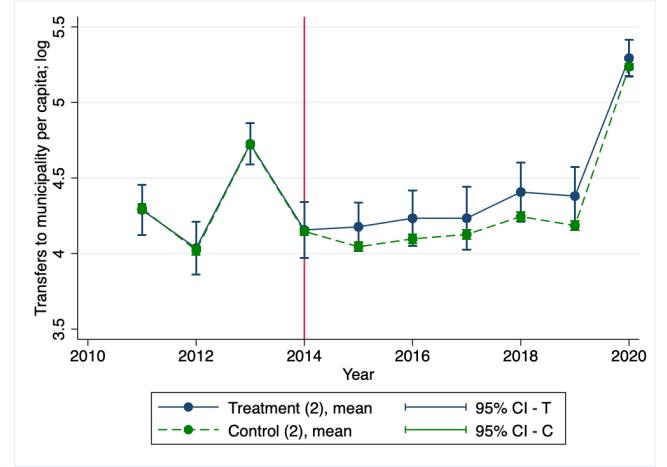


Figure 5: Trend of (the logarithm of) transfers to municipality per capita. The treatment group is consistent over time and it is composed of the municipalities whose mayor became president since 2014, excluding capitals.

A possible concern arising with this first identification strategy is whether the treatment units 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 relatively more represented among presidents' birthplaces after the reform, and at the same time they tended to receive more transfers per capita, 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.³⁶ Nevertheless, to account for other types of endogenous selection into treatment, I introduce here the results from my second identification strategy, whereby the treatment group is stable over time and it is composed of the municipalities whose mayor became president after 2013.

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	0.262*** (0.0819)	0.290*** (0.0807)	0.106* (0.0539)	0.113** (0.0553)
Treatment (time-variant)	-0.160** (0.0721)	-0.177** (0.0708)		
Controls	No	Yes	No	Yes
Municipality FE	Yes	Yes	Yes	Yes
Province-by-Year FE	Yes	Yes	Yes	Yes
Observations	54,720	53,558	52,308	51,123
R-squared	0.759	0.760	0.756	0.757

The dependent variables is (the logarithm of) transfers to municipality per capita. Treated municipalities in column 1 and 2 are those that are the birthplaces of the presidents in office each year, thus they change over time. Treated municipalities in column 3 and 4 are those whose mayor became president after 2013 and are consistent over time. Controls include municipal population and municipal mayors' characteristics (sex, age, education, whether from the left). The coefficient of interest is that on the interaction term. The sample includes the 5,431 municipalities outside autonomous regions or metropolitan cities; province capitals are also excluded. Robust standard errors, clustered at the municipality level, are in parentheses, * p<0.10, ** p<0.05, *** p<0.01.

Figure 5 plots this second specification and shows a perfectly parallel trend in transfers per capita between the treated and the control municipalities, before 2014. Things change right after the reform, when the treated units start receiving disproportionately more funds. Columns 3 and 4 of Table 3 display the results from running equation (2) and show that indirect elections significantly increased transfers to the treated group by more than 10%, compared to the control group. Just like with equation (1), including controls for politicians' characteristics marginally increases both magnitude and significance of 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 selection into treatment.³⁷ Adopting

³⁶See in the Appendix Figure B1, where the treatment group is stable over time and Figure B2, where the small decrees in population in 2016 and 2017 is compensated in the following years.

³⁷The second specification alone would be flawed if presidents *always* favored their hometowns. Indeed, while in equation (1) treatment municipalities are birthplaces of the presidents both before and after 2014, making this issue inconsistent, in equation (2) a treatment-municipality is only connected to the president after 2013. Reassuringly, Figure 4 shows that presidents' birthplaces were not disproportionately receiving more funds before the reform (Table B3 formally rejects this hypothesis), suggesting this mechanism cannot explain the results.

a multiple time periods approach for both equations (1) and (2) to look at the effect year-by-year, the coefficient of the interaction term is positive in all post-intervention years and always significant with the exception of 2017 and 2020.³⁸

Some readers may also be concerned that the reform not only reduced accountability towards the voters, but it also generated a new form of accountability towards the new electors (mayors and councilors) who elected the president, or that the reform could foster party favoritism (as in Curto-Grau et al.(2018)). In the new provincial elections, municipal politicians typically vote based on party-membership, so that presidents can often take these votes for granted. Moreover, part of the municipal politicians changes every year and the assembly of mayors and councilors only gathers to elect provincial leaders, making it hard for them to implement any real form of control and making it unnecessary for presidents to maintain their favor in the long run.³⁹ Still, the provincial candidates may try to "buy" their vote through the promise of additional funds to specific cities or simply prefer to help ideologically similar mayors. I test this hypothesis in Figure 6, by plotting the amount of funds received by cities whose mayors share their ideology with the president. Under this scenario, we would 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 potential appointees.⁴⁰

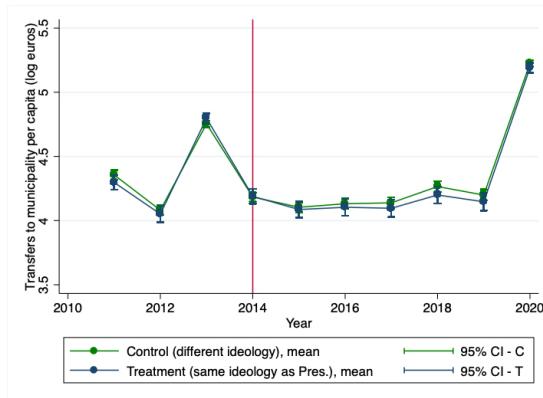


Figure 6: Trend of (the logarithm of) transfers per capita. The treatment group is time-invariant and composed of the municipalities that, after the reform, are of the same political orientation as the President. Capitals are excluded.

³⁸See Figure B9 and column 2 in Table B6 for the precise year-specific coefficients in the main specification. Also, see Figure B10 and column 4 in Table B6 for the equivalent analysis using the second strategy.

³⁹The assembly of mayors (but without councilors) also votes for changes in the provincial statute.

⁴⁰Using a time-variant treatment group, with municipalities that share ideology with the president as the treatment group both before and after the reform, we see a less constant trend, with same ideology municipality receiving constantly less transfers, and no evidence of divergence after 2013 (see Figure B5).

A second concern is that the possibility of promotion as a provincial politician may affect the quality of those who run as municipal politicians. In a robustness test, I restrict the treatment group to those presidents who had been elected mayors before 2014, whose decision to run cannot have depended on the reform. Despite the small sample, this test confirms the general trend, suggesting that results are not driven by changes in municipal selection connected to the reform.⁴¹

Taken together, these results provide solid evidence that indirect elections allowed presidents to increase the flow of funds towards municipalities closely connected to them, but the exact mechanism is still unclear. Presidents may either have an intrinsic preference for hometown favoritism, which only turns to actions when accountability drops, or it may be the overlapping of the roles of president and mayor that generates electoral motive (chasing re-election in the municipal office as a requisite for the presidency) and administrative opportunity (the overlapping of powers) to benefit specific places. Both mechanisms are caused by the weaker accountability towards provincial voters, but being mayors is a necessary condition only in the second case. To disentangle these two alternative explanations, I exploit the fact that in Italy it is not necessary to be born in a city in order to become its mayor; thus, a president can be born in city A and be the mayor of city B. This allows me to partially disentangle the two alternative explanations.

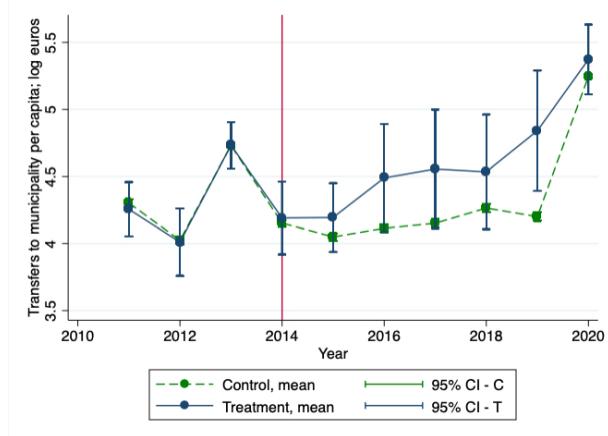


Figure 7: Trend of (the logarithm of) transfers per capita to municipalities. Treated municipalities are the hometowns of the presidents, excluding those where the president is mayor. Provincial capitals are not in the sample.

I plot in Figure 7 the flow of transfers towards the municipalities where the presidents were

⁴¹This test is shown in Figure B14 in the Appendix.

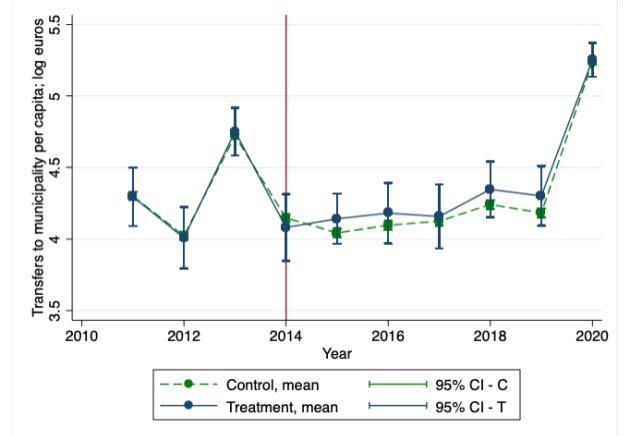


Figure 8: Trend of (the logarithm of) transfers per capita to municipalities. Treated municipalities are those whose president became mayor after the reform, excluding the hometowns of the presidents. Provincial capitals are not in the sample.

born, but of which they are not mayors. Symmetrically, Figure 8 restricts the treatment group to municipalities where the presidents are mayors, but excluding those where they were born. The way I selected the treated cities in these figures is endogenous, but the trend in transfers is only compatible with an inherent preference for favoring one's own birthplace rather than with the overlapping of offices. This is in line with Fiva and Halse (2016), Baskaran and Lopes da Fonseca (2021) and Carozzi and Repetto (2016), and may be caused by personal ties or loyalty to one's own roots. Interesting, the fact that results are driven by presidents' birthplaces, rather than municipalities where they are mayors, is also incompatible with a future career concern. In conclusion, the loss of direct accountability caused presidents to switch from spreading the available money across all municipalities in order to maximize their probability of reelection, to distribute transfers based on their personal preferences.

6 Indirect elections and the composition of public spending

6.1 Identification strategy

In the previous section, I provided a concrete example of how indirect elections generated geographical distortions in public spending. I will now assess the general impact on provincial budget, whose three main components (accounting for 80% of current expenditure), as subdivided by AIDA, 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 operated by municipal governments. More specifically, for each *real* province, I construct a control group by summing up municipal public spending issued by all municipalities located within the corresponding provincial territory. Figure 9 shows an example of treated and control unit: the two entities share the same borders (and population, wealth, etc.), but the reform only affected public spending issued by the former, as nothing changed at the municipal level.⁴² 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 trend for the control group in the absence of the reform.

⁴²Consider for example the province of Siena, which includes many municipalities such as Siena, Montepulciano, San Gimignano, Pienza, etc. In this case, the treated observation is the (expenditure of the) province of Siena while the corresponding control observation aggregates (the expenditure of) all the aforementioned municipalities.

Therefore, my identification relies on the assumption that the reform caused no changes in the responsibilities assigned to provinces or to municipalities (e.g. transfer of duties among administrations). More broadly, the SUTVA requires that municipalities were not directly affected by the reform.⁴³ As already discussed, the reform only addressed municipalities through the introduction of more stringent gender quotas and, more indirectly, through a slight reorganization of the unions of municipalities and the introduction of incentives for municipal mergers (but only a negligible number of very small municipalities merged following 2014). Importantly, the 2015 report of the *Council for Local Autonomies* and a wide journalistic evidence confirm that provinces remained in charge of the exact same functions as before the reform. Section 7 provides more evidence and robustness tests on this assumption.

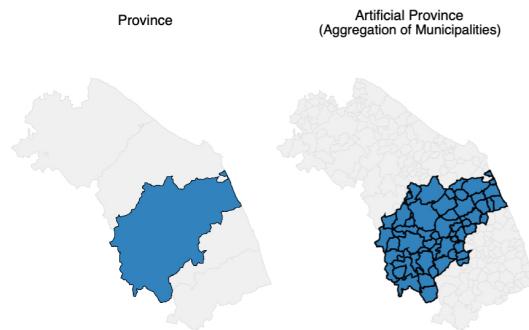


Figure 9: An actual province and the counterfactual province, which sums up contained municipalities

A crucial concern is whether the type of functions performed by the provincial and the municipal governments are comparable, and ultimately whether the aggregated municipalities are a valid control group. Indeed, the municipal and provincial type public spending on *transport* are virtually the same: roads' maintenance and public transport. The only difference is that the former deals with urban roads and buses while the latter deals with provincial roads and buses, namely the ones connecting different municipalities. The two types of roads do not strictly correspond to urban and rural areas: even two fully urbanized neighboring municipalities will typically have a provincial road connecting their centers, and people living in urban areas will

⁴³Notice that results in Section 5 do not invalidate using municipalities as a control group: in the average province, only one out of 80 municipalities are associated to a president (i.e. treated) each year, so that the overall control group is hardly affected. Moreover, my main dependent variable is the share of resources allocated to each specific sector (rather than the level) precisely to isolate the choice made by the political authority from the changes in the total amount of available resources, which largely depends on higher levels of government.

have to use provincial roads to reach other urban areas. Since both administrations provide the same services, just on different roads, I argue that aggregate municipalities are indeed a good control group. As for *bureaucracy*, the two functions are again very similar: in both cases this item captures expenditure connected to tax collection, functioning of institutional bodies, bureaucrats' salaries etc., making the two groups ex-ante comparable. Finally, *education* refers to spending on schools' maintenance and school accessibility. In this case, municipal and provincial spending is partly complementary: they both act on the same schools and cooperate on similar duties. Overall, all these functions are arguably comparable and while some degree of discrepancy does exist, this should translate into a distance in the levels of spending rather than in the trends, which will be taken into account by the diff-in-diff strategy.

In the resulting dataset all the treated provinces have a counterfactual with the same geographic area and socioeconomic characteristics, but public spending choices made at different institutional levels. Thus, I run the following diff-in-diff, using actual provinces as the treatment group ($T_p=1$) and years after 2013 as treatment period ($After_t=1$):

$$Y_{p,t} = \alpha + \delta(T_p * After_t) + \eta_p + \chi_t + \epsilon_{p,t} \quad (3)$$

$Y_{p,t}$ is the outcome of interest (mainly public expenditure) in province p and year t. I include year and province fixed effects. The coefficient of interest is δ and it captures how provinces diverge from the control group after the reform. $\epsilon_{p,t}$ is the error term; 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 results are robust to restricting the treatment period to 2015-2020. To have a better sense of the timing, I also plot in Appendix C.2 the corresponding year-by-year coefficients.

In terms of outcome, I mainly focus on the share of total current expenditure allocated to the two main types of provincial public good, namely transport and education and to administrative costs. The choice of focusing on the *share* is the natural solution to shed light on the political (and local) process of resource allocation: the *overall* amount of provincial expenditure across sectors is indeed an imprecise indicator for the effective operational decisions and the incentives generated by the reform, as the overall budget strongly depends on the exogenous amount of

funds that provinces have received from the Central Government in a given year, while provincial taxes are highly inflexible.⁴⁴ On the contrary, the *share* of the budget allocated to specific sectors provides a better representation of how provincial politicians chose to exactly allocate the available resources. A possible concern is whether different sectors were subject to different elasticity to changes in revenues. If politicians had discretion only on some types of expenditure, while others were exogenously fixed, changes in revenues may imply changes in shares of resources across sectors. Reassuringly, the share of expenditure across sectors remained constant between 2009 and 2014 (see Figures 10, 11 and 12), despite the incessant cut in resources.⁴⁵ 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

I will now describe how the introduction of indirect elections has affected the composition of provincial public spending in the two sectors than are the main provincial responsibilities, namely transport and education. My main results are plotted in Figures 10 and 11, which show the yearly group-means of the share of current expenditure allocated to transport and education for the treated group - provinces - and for the control group, namely all municipalities within each province. In both cases, the treated group's share sharply decreases in concomitance with the reform, after at least five years of parallel and constant trend. In contrast, the control group remains unaffected.

Table 4 tests this result more formally, using equation (3). The coefficients of the interaction term in columns 1 and 2 are negative and highly significant. An advantage of the way I constructed my control group is that, for every treated unit, there exists a counterfactual, which refers to exactly the same geographic area. Thus, all the geographic and socioeconomic time-varying unobservables are forced to move in the same way in the two groups. This is clearly not true for changes in the budget of the two different levels of government (municipalities and

⁴⁴ After 2011, only 40% of provincial revenues comes from provincial taxes. Moreover, the main provincial tax revenue source (accounting for more than 50% of total tax revenues) is a car-insurance tax (RC auto), whose tax rate is fixed by the central government at 12.5% and the province can only modify it by up to 3.5%. See Bracco and Revelli (2018) for a discussion of the source of provincial funds before 2011.

⁴⁵ Figures C7 and C9 show the level decrease in provincial revenues (mainly due to central government cuts).

provinces): indeed my identification exploits exactly these differential changes. While the share of expenditure in both sectors was in general reducing after 2013, the sign and size of the interaction term confirm that this happened 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 a 14% and 29% reduction with respect to the pre-reform mean.

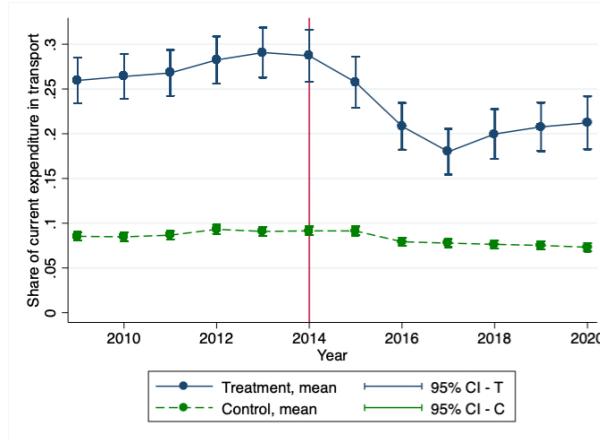


Figure 10: Trend of the current expenditure on transport as a share of total current expenditure. Provinces (T) and aggregated municipalities (C).

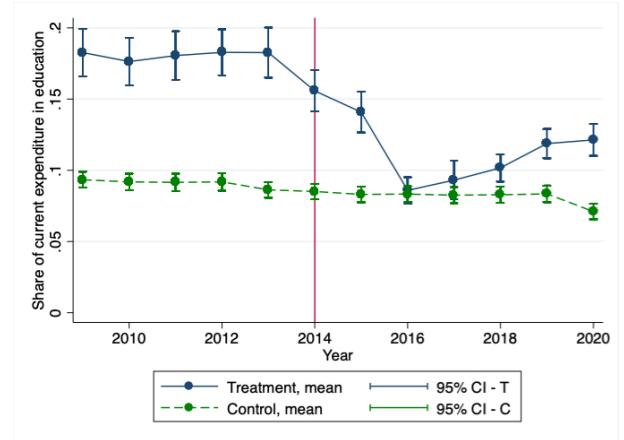


Figure 11: Trend of the current expenditure on education as a share of total current expenditure. Provinces (T) and aggregated municipalities (C).

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

	(1)	(2)	(3)
FULL SAMPLE	Transport Share	Education Share	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 include current expenditure on transport (col. 1), education (col. 2), administration (col.3) as a share of total current expenditure. The independent variables include post-treatment dummy (After), dummy for treatment group (Treat), and the interaction term. In column 3 the sample ends in 2015, due to change in the data structure. Province and year fixed effects are included. 11 Autonomous provinces and metropolitan cities are excluded. Robust standard errors, clustered at the province level, in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The evidence presented so far is very suggestive, but it comes with a potential issue. The trends in Figures 10 and 11 for the treatment and the control group are parallel but distant from one another, thus one may be concerned that the share of expenditure on transport and education in the control group is already too low to decrease. However, the complete lack of evidence of a reduction in these two sectors - that, taken together, account for almost 20% of current expenditure - is at odd with this concern. In any case, I explore in Section C.1 an alternative design in which I construct a synthetic control group for provincial expenditure, by selecting those municipalities with consistently high levels of expenditure in education and transport. Results from this alternative strategy match almost exactly those in my main specification.

Expenditure on Bureaucracy

I now provide evidence showing that the drop in the share of resources allocated to public goods was compensated for by the fast rise in administrative costs, the only sector whose share actually increased. *Administration* is one of the 20 categories in which AIDA PA divides provincial ordinary expenditure and it refers to a wide range of tasks and services that are commonly associated with *bureaucracy*. This includes the functioning of the political and bureaucratic bodies, the cost of tax collection, the benefits for bureaucrats, (non-sectoral) transfer of funds outside the province, etc.⁴⁶

The trend in share of expenditure allocated to such category is shown in Figure 12. Interestingly, not only are the lines of the treated and the control groups parallel and constant over time, but they are also very close to one another, with a share of expenditure around 30%. Starting from 2014, however, the rise in the share allocated to bureaucratic costs is evident.⁴⁷ Column 3 of Table 4 shows the results of equation (3) using bureaucratic costs as dependent variable: the interaction term is positive and highly significant even with only two post-treatment year (and robust to considering 2014 as a pre-reform year); it is also robust to including socioeconomic

⁴⁶The main subcategories of this sector are the following: ‘Institutional Bodies’, ‘General Secretariat’, ‘General Secretariat, personnel and organization’, ‘Management of public-owned land’, ‘Economic Management’, ‘Fiscal Management and fiscal services’, ‘Technical Office’, ‘Statistical Office’, ‘Elections’, ‘Human resources’, ‘Other general services’, ‘General functions of administration and control’, etc.

⁴⁷In 2016 the administrative sector was relabeled ‘General Institutional Services’. The new definition should include the same categories of bureaucratic expenditure; nevertheless, I exclude observations after 2015 from the main analysis, to avoid the risk of generating an artificial discontinuity. In Figure 12 I highlight such change by breaking the line after 2015. In any case, data up to 2015 are very suggestive of a strong effect right after 2013.

controls. The reform caused a 10 percentage points increase in the cost share of bureaucracy that corresponds to a 30% increase with respect to the pre-reform mean. Appendix Section C.6 uses AIDA PA and SIOPE data to provide some further insights into the origin of such increase, suggesting that extraordinary-current costs (a category containing a broad variety of short-term non-expected costs, such as failure to collect credits, thefts, damages, non-expected bonuses, etc.) and administrative transfers (i.e. classified by AIDA as unrelated to other missions such as transport or education) to private and non-central public entities are driving the results, at least in 2015.

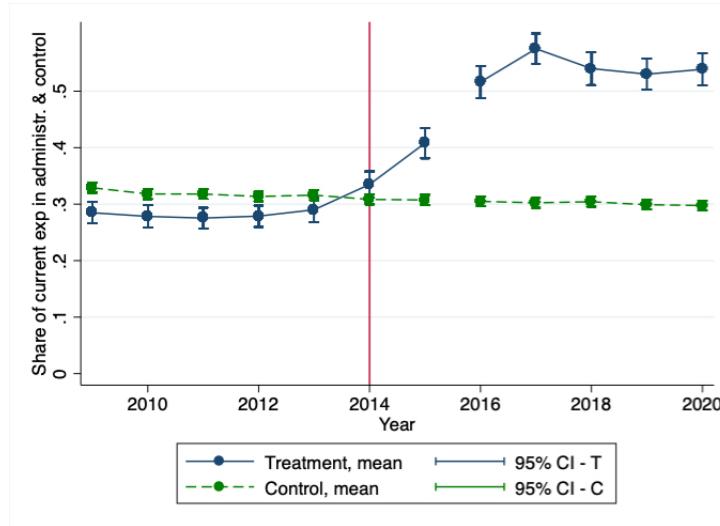


Figure 12: Bureaucratic current spending as a share of total current spending. In 2016 the category was relabeled; I break the line, as I can't exclude the chance that small discrepancies may have arisen. Provinces (T) and aggregated municipalities (C)

An important strong point of the analysis presented so far is the fact that the pre-reform shares of expenditure remained constant despite significant declines in provincial revenues. This suggests the presence of an equilibrium in which a virtually fixed share of resources was consistently allocated to each sector. This abruptly changed with after elections were made indirect, with relatively more resources spent on bureaucratic costs.

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 expanding from 2014.⁴⁸

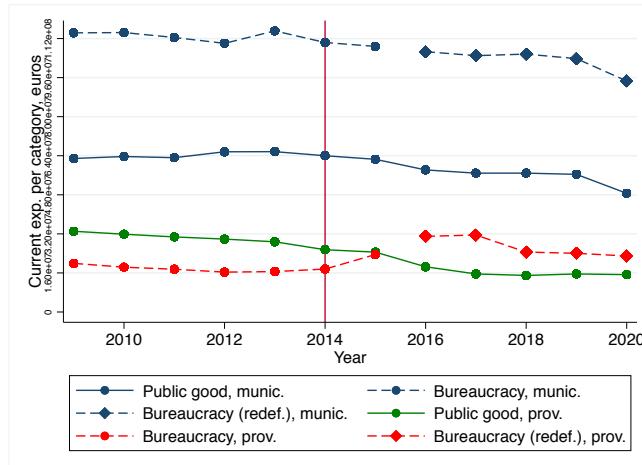


Figure 13: Absolute level of provincial spending. Bureaucracy and public good (transport, education).

Specifically, Figure 13 shows that the absolute rise in provincial bureaucratic costs was sufficient to compensate the reduction in public goods, which accelerated following the reform, and to generate an increase in total spending.⁴⁹ More in details, the amount of euros spent both on transport and education significantly dropped right after the reform: a standard diff-in-diff equation suggests that the indirect elections had a negative and highly significant impact on the level of expenditure as well.⁵⁰ The same pattern is not visible when we focus on the municipal expenditure in bureaucracy and public good (the blue lines): in this case, the two lines remain virtually parallel. The dynamics in level are thus consistent with the analysis in share of expenditure, rejecting the hypothesis that my results in share simply reflect different elasticities to budget cuts between administrative costs and public good provision.

Quality of the Services

I will now provide suggestive evidence that the different allocation of provincial resources caused by the reform had concrete consequences on the quality of public good provided, focusing on roads' quality as a consequence of the reduction in the share of provincial resources allocated to

⁴⁸Table C2 show that the increase was significant when compared to the municipal trend.

⁴⁹The overall size of provincial personnel shrank considerably, but possibly this happened more among the employees involved in public good provision and less among bureaucrats. Given results in Section C.6 one can conjecture that a lower concern for inefficiencies, and more clientelistic transfers have played an important role.

⁵⁰See Table C2 and Figures C11 and C12.

transport. Specifically, I use the number of accidents on provincial and urban roads as a proxy for road maintenance. 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 14. Before the reform, the trend is decreasing and virtually parallel for the two groups; from 2014 the number of accidents keeps decreasing in urban roads but starts to increase in provincial roads.⁵¹

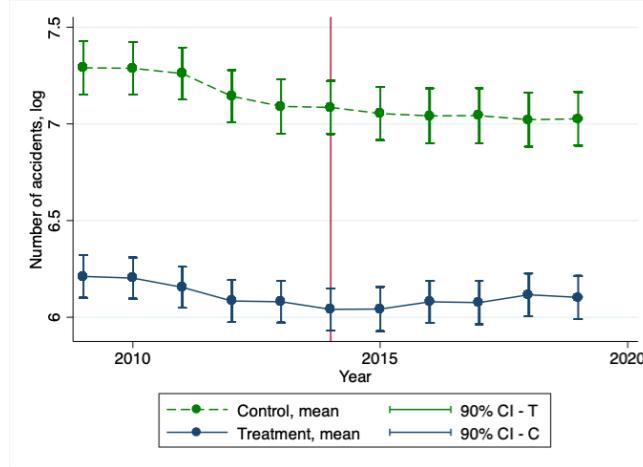


Figure 14: Trend of the (logarithm of the) number of accidents.

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

	(1) Accidents	(2) Ln(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 (col. 1) and the logarithm of the number of accidents (col. 2) in urban vs provincial roads. Provinces are matched with provincial roads; municipal counterfactuals are matched with urban roads. Treatment period: 2014-2019. Autonomous provinces and metropolitan cities excluded. Robust standard errors, clustered at the province level, in parentheses,
* p<0.10, ** p<0.05, *** p<0.01.

⁵¹2020 is excluded because the lock-down following the Covid-19 pandemic dramatically reduced accidents.

More formally, I estimate equation (1) using the 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 was consequential in terms of the quality of the public good provided.

7 Alternative Explanations and Robustness Checks

The crucial assumption of both my sets of results is that the reform has affected the functioning of the provincial governments only through the different electoral system. Indeed, the goal of the Delrio Act was to reduce the “cost” of provincial representation, until the result of the constitutional referendum would have allowed to dramatically downsize, if not dismiss, provinces. 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 results in Section 6. The reform specifically listed public transport, educational programming and maintenance of road and school infrastructures as the main provincial responsibilities; however, it still allowed potential agreements between provinces and regions to redefine how to exactly allocate responsibilities among them. While many journalistic sources confirm that responsibilities remained unchanged, some anecdotal evidence may generate some concerns. For instance, in 2017 the region of Lombardy committed to turning 200km of provincial roads into regional roads within 2018. However, this promise was not fulfilled and those roads remained under the jurisdiction of the provinces.⁵² Reassuringly, according to the report released on the 21st of September 2015 by the *Council for Local Autonomies*, among all regions that had signed an agreement with their respective provinces, only Marche (on transport) and Umbria (on education) had implemented some minor transfer of responsibilities to the region in main sectors. As for the other sectors, only some minor ones such as tourism, cultural heritage, fishing and hunting occasion-

⁵²As a further concern, anecdotal evidence suggests that in recent years some provinces delegated part of the public transport sector to external companies, potentially reducing the relative expenditure share in transport. In many cases, however, the provinces maintain control of these external companies. While even the choice of outsourcing a public service and contemporaneously increasing administrative costs would still be a relevant consequence of the reform, note that both expenditure in public transport and road-maintenance were affected, as shown in Figure C24.

ally passed from the provinces to the regions (see Figure C10 for the trend in residual sectors) but, if anything, this should mechanically raise the share of resources allocated to education and transport. In fact, my estimates on transport are only marginally affected by the exclusion of the provinces in Umbria and Marche. To the best of my knowledge no other major change in responsibility happened. Since I cannot exclude the existence of minor changes, I present some descriptive evidence on the general trend in regional expenditure on transport and education (Figures C25 to C27). While non-conclusive, those graphs are incompatible with a transfer of responsibilities toward the regions since 2014.

A second threat for both analyses in Sections 5 and 6 is the change in the size of provincial councils. Indeed, the reform prescribed a consistent reduction of the council size, from 19-28 to 10-16 councilors, depending on the provincial population. A possible alternative explanation for my results is that a smaller council may be less able to constrain the 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 we take this alternative explanation seriously, we would expect to see in 2011 an effect similar to what we observe in 2014. On the contrary, 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 B.1.5 I replicate my analyses on a larger time period, including all years after 2007. Indeed, despite the dramatic changes in municipal budget before 2011, Figures B17 shows no evidence of early divergence whatsoever for municipalities whose president became mayor after the reform. We do, however, see some minor evidence of pre-trend when focusing on presidents' birthplaces in a sample that includes province capitals (see Figure B18). In Appendix B.1.5 I discuss more extensively how I take such pre-trend into account, but Figure B19 shows that the issue vanishes when excluding capital cities from the analysis, as I do in my main specifications. Finally, to further investigate the role of the council size on birthplace favoritism, I directly test whether favoritism may decrease with council size. To do so, I exploit the provincial population thresholds at which the number of councilors increases and construct a RDD using transfers to presidents' birthplace as dependent variable. Figure B15 and Table B7 show that the decrease in council size does not affect transfers at all, with, if anything, a point estimate going in the opposite direction.

Another possible confounder is the reduction in presidential salaries brought about by the reform, which may have worsened politicians' quality or incentive to avoid waste of resources as suggested in Gagliarducci and Nannicini (2013). I have already argued and presented evidence against a worse selection of politicians after the reform; however, it is not easy to entirely rule out that the change in salary may have played a role. 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 re-set to a level comparable to the pre-reform 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 shows any evidence of reversal, making this explanation unlikely.⁵³ Second, I exploit once again the provincial population threshold, focusing now on the cutoffs used to determine presidential wages before the reform. Figure B16 and Table B8 show that passing the population thresholds, and thus raising wages, was not associated to a significantly lower amount of transfers to presidents' birthplaces.

An alternative explanation for the differential impact on the share allocated to each sector, points to a different elasticity to changes in total revenues for public goods or bureaucratic spending. If a fixed amount of resources needs to be spent on bureaucracy, while cuts in public services are feasible, a reduction in total revenues would increase the share spent on bureaucracy (even holding the 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 actually increased in 2015, as shown in Figure C7.⁵⁴ Third, the trend in the pre-reform share of expenditure in the different sectors is virtually constant between 2009 and 2013 (as shown in Figures 10, 11 and 12), even if the overall amount of resources and current expenditure dropped by about one quarter in the same period. The fact that resources increased in 2015, while the absolute level of expenditure on public goods kept falling, confirms that the

⁵³More specifically, the presidents' wages were equated to those of the mayors of the largest city in the province (this roughly corresponds to the presidents' pre-reform wages). In this sense, the presidential wages rose significantly in general but remained stable for presidents that were mayors of the capital cities. Separately looking at the dynamics of capital cities shows that the closure of the gap in transfers between the treatment and the control groups in Section 5 also happened for this supposedly unaffected subgroup (see Figure B4). This suggests that the extraordinary response to COVID-19 simply leveled out municipal differences in the amount of transfers.

⁵⁴The spike in Figure C7 may be partially overstated by the presence of 'transfers to the state' (discussed below). In practice, high revenues may be reported but the provinces will have to send money back to the Central Government. However, since 'transfers to the state' became relevant from 2016, this cannot entirely explain the peak. Indeed, the spike in 2015 remains if I drop those provinces with high 'transfers to the state'.

additional resources were wasted in less efficient bureaucracy.

A final issue worth discussing is the possibility that my results are driven by a more accurate choice 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 becoming presidents are more informed of available applications for funds. However Figure 5 rules out the possibility that treated municipalities were special and that they were already receiving more funds before 2014, when they were not associated to a president. Similarly, Figure 7 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 choose to elect its mayor as president to favor it. Since presidents did not provide more resources to their hometowns before 2014 (see Figure 4), the latter case implies that municipal politicians anticipate that, after the reform, the president will indulge in such favoritism. In this sense, the conclusion does not change substantially: the reform did increase regional favoritism. The former case is instead harder to rule out but, to the best of my knowledge, there is no evidence whatsoever of parties choosing their candidates on the base 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 in bureaucracy at the expenses of public good, 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 non-necessarily problematic in the absence of direct evidence of reduced quality in publicly-provided services. However, the expansion of administrative costs at the expense of services that more directly provide public good to the citizens, both in relative and absolute terms, and the suggestive evidence on road quality indicate that, in the absence of an effective political control, public good may end up being more poorly provided.

Finally, Appendix C.7 further discusses a wide range of additional robustness checks for my findings in Section 6. To sum up, results are robust to using different specifications, namely com-

puting shares over total revenues instead of expenditure; including 2014 in the pre-intervention period or dropping it; dropping years after 2015; and they cannot be explained by a switch from current to capital expenditure.

8 Conclusion

This paper analyzes the consequences for public spending of the electoral reform of the Italian provinces, which drastically reduced politicians' accountability to their citizens by getting rid of popular elections. Direct elections of the provincial leaders were replaced by an indirect mechanism whereby the municipal politicians elect the 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 find that, with indirect elections, municipalities where the president was born received 10% to 30% more public transfers compared to the other cities. Second, analyzing how the composition of provincial expenditure changed after the reform, I find that the share of expenditure allocated to public good fell by 3.9 to 5.5 percentage points (depending on the sector), while the share in bureaucracy increased by 10 percentage points. A very similar dynamics applies for the absolute level of spending.

The reform allows me to assess the impact of a transition from direct to indirect elections, without the passage from a presidential to a parliamentary system. I can thus identify the negative impact of the end of direct popular selection of representatives, without confounding it with a complete change of the governing system. Introducing direct elections in a context where political appointment is the status quo leads to changes in both the selection of politicians and their incentives. Direct elections increase the salience of the process and force politicians to change their behavior to seek citizens' approval; at the same time, they may be superior in terms of selection, with citizens able to choose better (or more representative) types of candidates as opposed to an internal choice among politicians. The peculiarity of the reform of the Italian provinces generates a dramatic change in incentives while maintaining democratic selection, at the municipal level, for the pool of eligible provincial candidates, which suggests that the negative impact of the reform is more compatible with a change in incentives rather than a change in selection: an analysis on politicians' observable characteristics confirms this hypothesis.

Indirect elections may be introduced to reduce electoral 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, causing a general reduction in politicians' accountability and a consequential mis-allocation of public funds.

A natural future question would be to assess whether the introduction of indirect elections limited the rise in citizens' polarization or politicians' incumbency advantage. Assessing the evolution of citizens' approval towards the indirectly elected leaders would be crucial for understanding whether these types of reforms may affect institutional legitimization and consequently citizens' behavior such as protests or tax evasion.

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Appendix

A Salience of the reform and the elections; politicians' selection

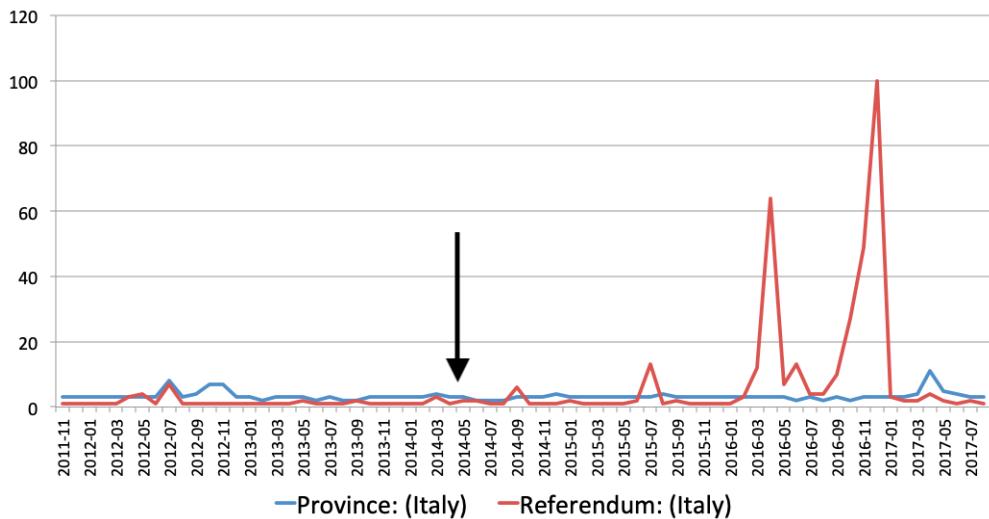


Figure A1: Trend of the searches of the word *province* (red) and *referendum* (blue) in Google. Both lines are normalized with respect to the highest concentration of searches in the period considered (here *referendum* in November 2016) No peaks in the blue line around the reform date (April, 2014) suggests little attention was devoted to the reform.

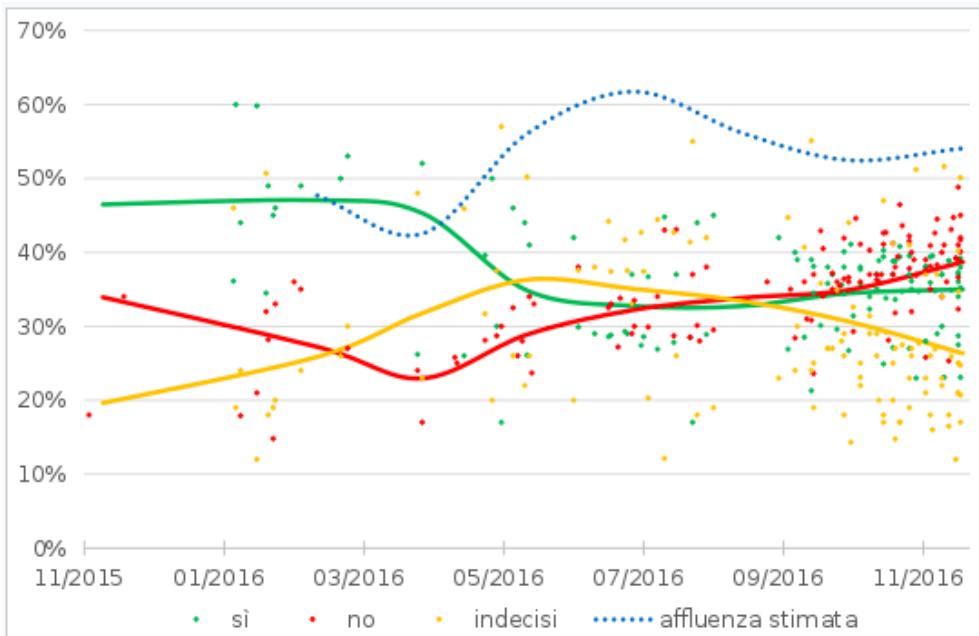


Figure A2: Mean of opinion polls for the referendum held on December 4th, 2016. Approve (*sì*), reject (*no*), undecided (*indecisi*). Source: Wikipedia. The final version of the constitutional reform was approved by the Senate in January 2016 and by the Lower House in April 2016. At that point, *approve* was leading *reject* by more than 20%. After July 2016, polls started predicting an uncertain election with no clear lead.

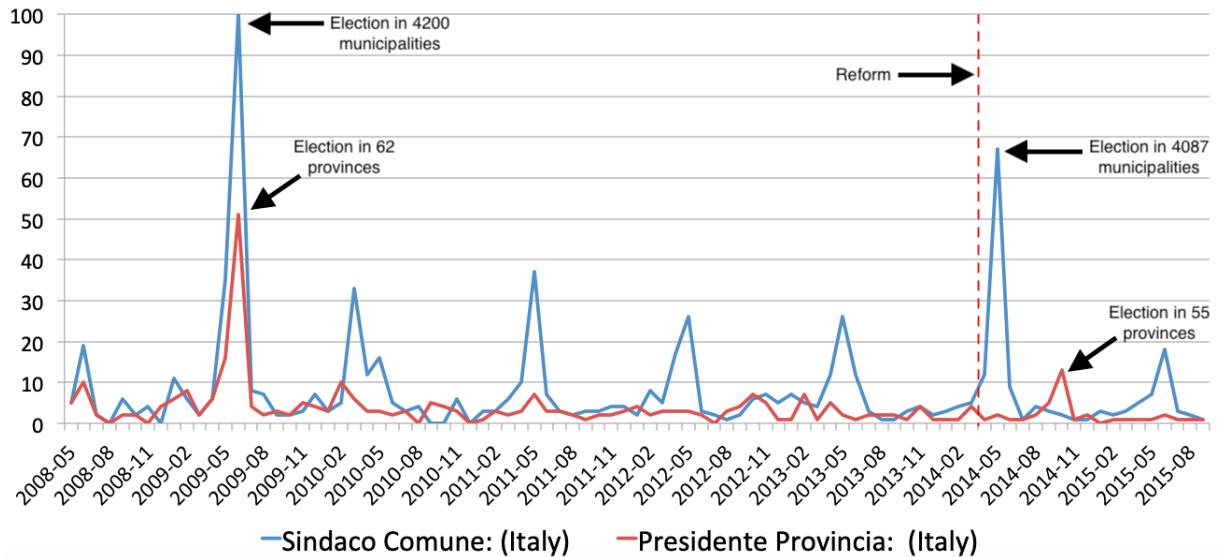


Figure A3: Trend of the Google web searches (news) of the words *Province & President* (red) or *Municipality & Mayor* (blue). All searches are proportional to the month with more searches (that is June 2009, for *Municipality & Mayor*). The vertical dotted line indicates the reform. Each blue peak coincides with a municipal election. No peak in searches for Province-related terms take place during municipal elections (the future provincial vote is not a salient topic during municipal elections). At the same time, the peak during provincial elections is way smaller compared to the pre-reform period (indirect provincial elections are less salient overall).

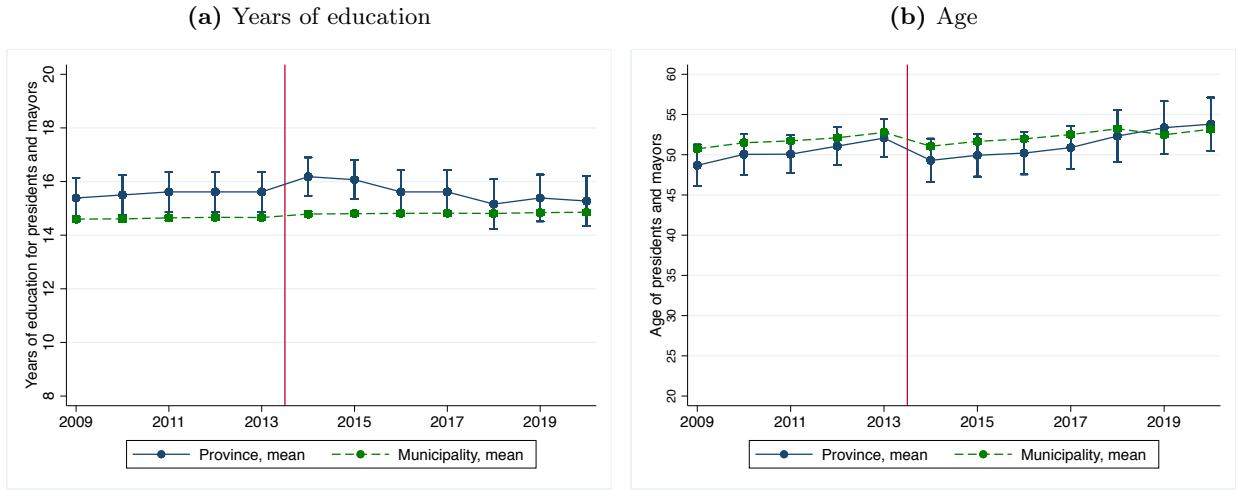


Figure A4: Provincial and municipal politicians' characteristics over time (age and years of education).

B Indirect elections and geographic favoritism

B.1 Presidents and mayors: Transfers to municipalities

Table B1: Summary statistics: municipalities where the presidents are mayors 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	

Sample: 2011-2020. Std deviations in parenthesis.

Table B2: Summary statistics: presidents' birthplaces vs control municipalities.

	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.

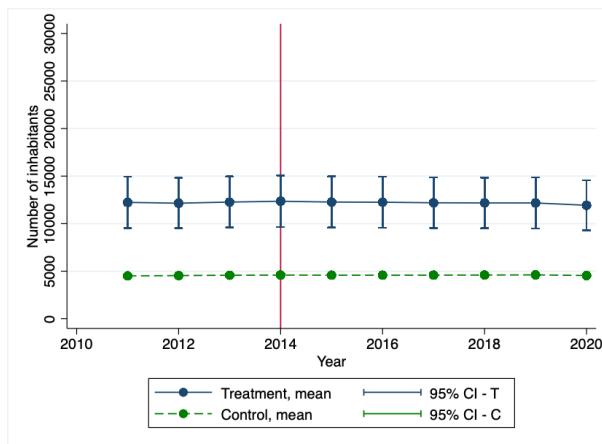


Figure B1: Number of inhabitants in the treated (municipalities whose presidents became mayor after the reform) and control municipalities, over time.

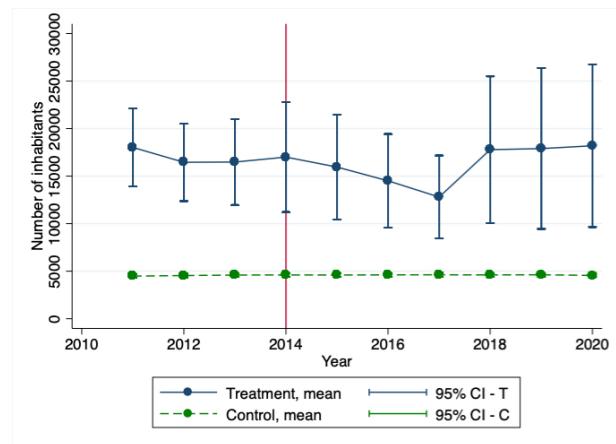


Figure B2: Number of inhabitants in the treated (where the president was born) and control municipalities, over time.

Table B3: 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.

Table B4: Alternative outcomes. Treatment group: presidents' birthplace municipalities (2011-2020)

	(1) (Log) Total transfers	(2) (Log) Total transfers	(3) (Log) Transfers share	(4) (Log) Transfers share	(5) Transfers p.c. (winsor.)	(6) Transfers p.c. (winsor.)
Treatment (time-variant)	-0.170** (0.080)	-0.172** (0.072)	-0.142* (0.086)	-0.130* (0.073)	-18.526* (10.075)	-18.471** (8.331)
Treatment*After	0.317*** (0.091)	0.276*** (0.082)	0.269*** (0.093)	0.232*** (0.084)	36.736*** (11.896)	31.324*** (9.586)
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	54,723	54,723	54,723	54,723	54,817	54,817
R-squared	0.843	0.865	0.600	0.653	0.719	0.757

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 those (5,431) municipalities not in autonomous provinces or regions. I also exclude metropolitan cities. Treated municipalities are those that were birthplace of the presidents in the years when they was president. Treatment period corresponds to 2014-2020. Columns 1 and 2 match figure 5. I only include years between 2011 and 2020. Robust standard errors, clustered at the municipality level, are in parentheses, * p<0.10, ** p<0.05, *** p<0.01.

Table B5: Alternative outcomes. Treatment group: municipalities whose mayor become provincial president (2011-2020)

	(1) (Log) Total transfers	(2) (Log) Total transfers	(3) (Log) Transfers share	(4) (Log) Transfers share	(5) Transfers p.c. (winsor.)	(6) Transfers p.c. (winsor.)
Treatment*After	0.115** (0.0563)	0.117** (0.0538)	0.0716 (0.0529)	0.0833 (0.0527)	9.252 (5.879)	9.971* (5.702)
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 only includes those (5,431) municipalities not in autonomous provinces or regions. I also exclude metropolitan cities. Treated municipalities are those whose mayor eventually becomes provincial president. Treatment period corresponds to 2014-2020. Columns 1 and 2 match figure 5. I only include years between 2011 and 2020. Robust standard errors, clustered at the municipality level, are in parentheses, * p<0.10, ** p<0.05, *** p<0.01.

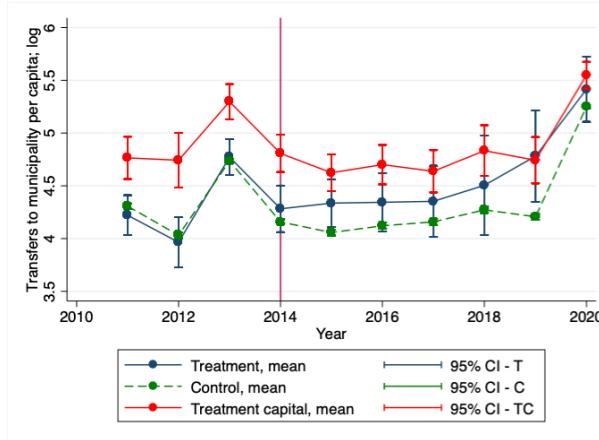


Figure B3: Trend of the current transfers per capita to municipalities. Treatment group: municipalities that are birthplace of the provincial presidents. The red line corresponds to treated units that are also provincial capitals (these are excluded in the main analysis).

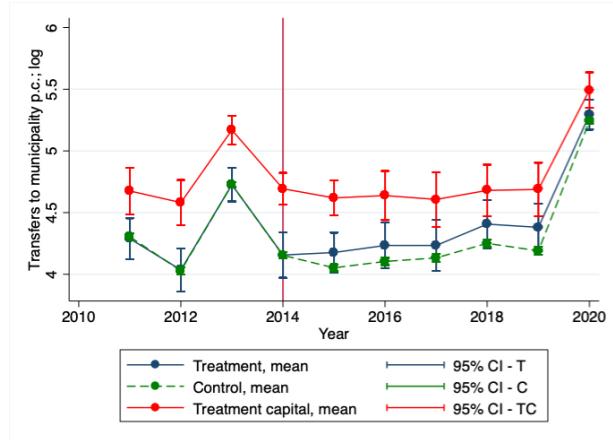


Figure B4: Trend of the current transfers per capita to municipalities. Treatment group: municipalities where the provincial presidents is mayor. The red line corresponds to treated units that are also provincial capitals (these are excluded in the main analysis).

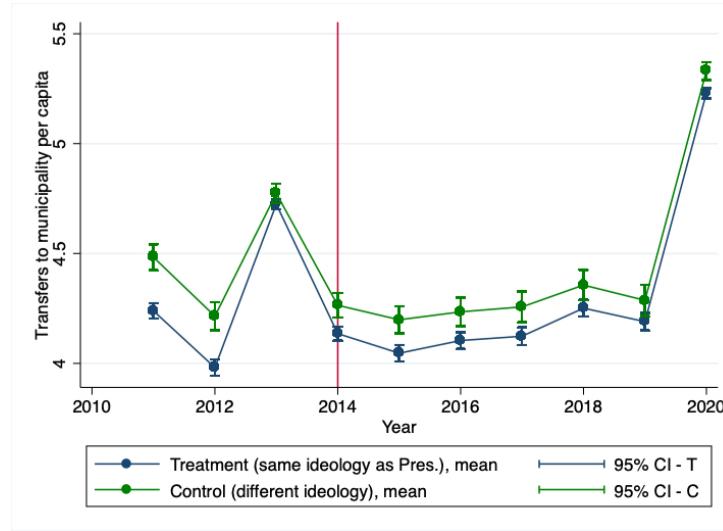


Figure B5: Trend of (the logarithm of) transfers per capita. The treatment group is time-variant and composed of the municipalities whose mayors share their ideologies with the provincial president in office in each specific year.

B.1.1 Event Studies

In this section I report two event-study approaches aimed at exploiting the exact timing in which a connection between a city and a president becomes effective.

In Figure B6 I identify for each province the first president elected after 2013 and the year in which this happened (*my event*); I then use her birthplace as treated unit and the other

municipalities of the province as control group. Finally, I plot the logarithm of transfers per capita around the *event* date. In Figure B7 I use an identical approach, but I now focus on municipalities where presidents are mayors, rather than their birthplaces. Both figures mirror the trend in my main specifications.

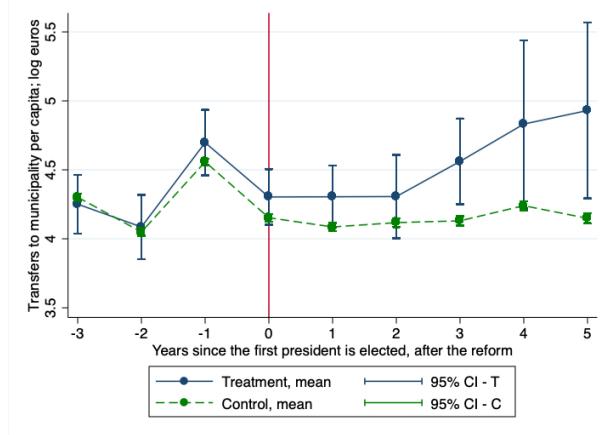


Figure B6: Staggered Diff-in-Diff. Trend of the current transfers per capita to municipalities. Treatment group: municipalities that are birthplace of the provincial presidents in each year. The reference year for both the treated and control municipalities is the year in which the first president is elected with the new system. Focus on 2011-2019. Provincial capitals are excluded.

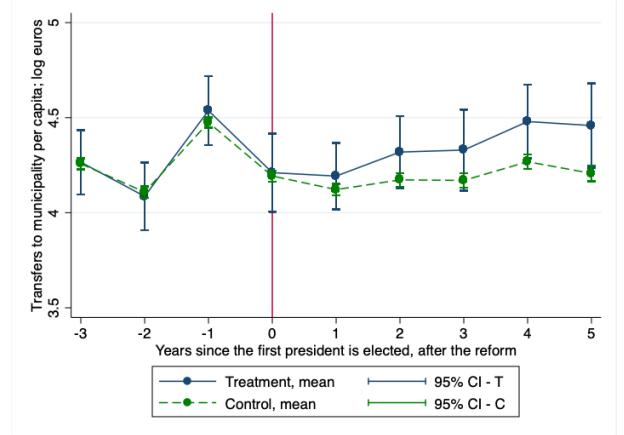


Figure B7: Staggered Diff-in-Diff. Trend of the current transfers per capita to municipalities. Treatment group: municipalities whose mayor becomes the provincial presidents after the reform. The reference year for both the treated and control municipalities is the year in which the first president is elected with the new system. Focus on 2011-2019. Provincial capitals are excluded.

While the birthplace of a provincial president is always identifiable, presidents are chosen among mayors virtually only after the reform. This means that, when focusing on being mayors, rather than birthplaces, I can construct a more standard event-study where I simply define an *event* as the time a municipality has its mayor becoming president (without time restrictions). I can thus run the following specification:

$$Y_{m,t} = \alpha + \sum_{x=-3}^{+6} Event_x \gamma_x + \chi_m + \omega_{p,t} + \epsilon_{m,t} \quad (B1)$$

Figure B8 plots the coefficients from equation (B1), 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.

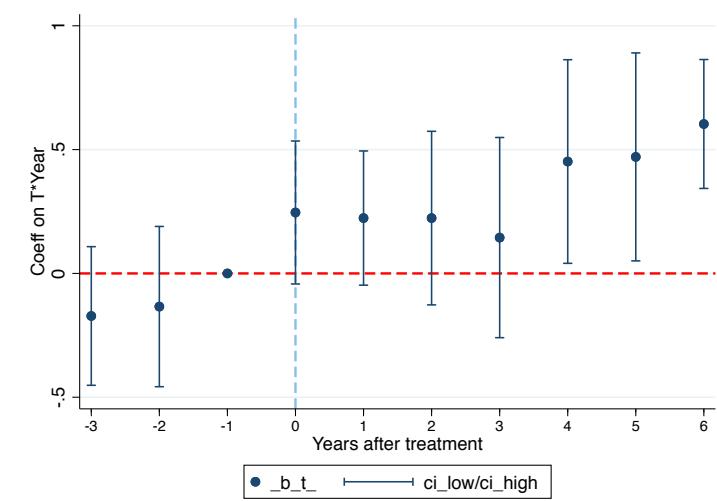


Figure B8: Trend of (the logarithm of) transfers to municipality per capita. Event defined as the first year in which the mayor of a municipality is elected president of a province. Province-by-year and municipality fixed effect included; clustering at the municipality-level.

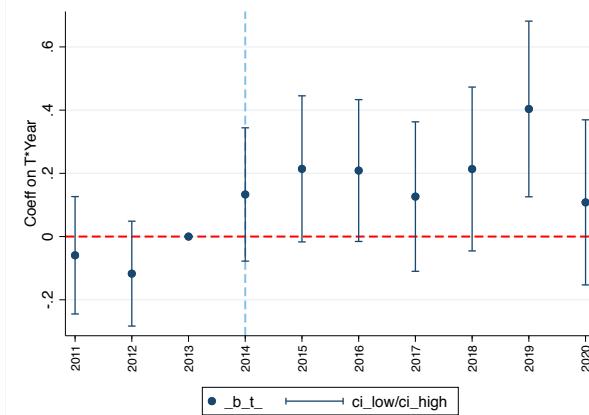
B.1.2 Multiple periods Diff-in-Diff

To have a better sense of when exactly the reform begins having its effects, I also run the following specification:

$$Y_{m,t} = \alpha + Treat_{m(t)} * \sum_{t=2011}^{2020} year_t \gamma_t + \chi_m + \omega_{p,t} + \epsilon_{m,t} \quad (B2)$$

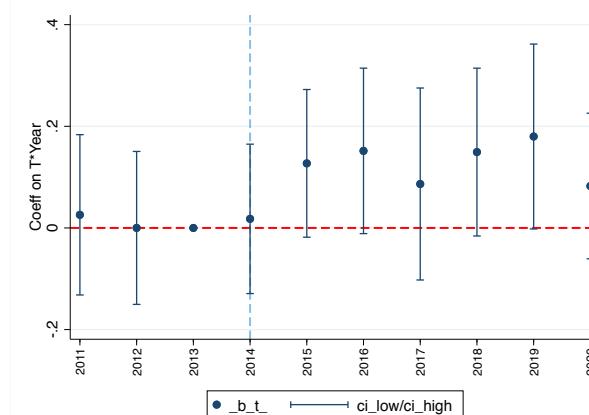
where all variables are defined as in equation (1), $+\omega_{p,t}$ are province-by-year fixed effects and $year_t$ is a set of year-specific dummies.

Figure B9: Strategy 2: (log) transfers p.c. Treatment based on president's birthplace. Coefficients of the interaction between treatment status and year dummies.



Treatment group (varying every year): municipalities that are the birthplace of the presidents. The regression includes year, municipality and province-by-year fixed effects. Cluster at municipal level. Province capital excluded. 95% confidence intervals.

Figure B10: Strategy 1: (log) transfers p.c. Treatment based on where presidents serve as mayors. Coefficients of the interaction between treatment status and year dummies.



Treatment group (consistent in time): municipalities whose mayor is elected president after the reform. The regression includes year, municipality and province-by-year fixed effects. Std. errors clustered at the municipality level. Province capital excluded. 95% confidence intervals.

Table B6: Trend of municipal transfers per capita. Coefficients of the interaction between treatment status and year dummies.

	(1)	(2)	(3)	(4)
	(Log)Transfers p.c.	(Log)Transfers p.c.	(Log)Transfers p.c.	(Log)Transfers p.c.
Treat (time-variant)	-0.066 (0.120)	-0.100 (0.090)		
Treat*2011	-0.134 (0.114)	-0.059 (0.095)	0.005 (0.086)	0.026 (0.080)
Treat*2012	-0.133 (0.120)	-0.117 (0.085)	0.019 (0.087)	0.000 (0.077)
Treat*2014	0.079 (0.129)	0.133 (0.108)	0.022 (0.081)	0.018 (0.075)
Treat*2015	0.207 (0.137)	0.214* (0.118)	0.134 (0.084)	0.127* (0.074)
Treat*2016	0.210 (0.139)	0.209* (0.114)	0.144 (0.095)	0.152* (0.083)
Treat*2017	0.178 (0.159)	0.127 (0.121)	0.096 (0.110)	0.086 (0.096)
Treat*2018	0.254 (0.175)	0.214 (0.132)	0.151 (0.097)	0.149* (0.084)
Treat*2019	0.473*** (0.166)	0.404*** (0.142)	0.184* (0.103)	0.180* (0.093)
Treat*2020	0.063 (0.175)	0.108 (0.133)	0.029 (0.078)	0.082 (0.073)
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Province-by-year FE	No	Yes	No	Yes
Observations	54,720	54,720	52,308	52,308
R-squared	0.719	0.759	0.720	0.756

The dependent variables is (the logarithm of) transfers per capita. The coefficients are the interaction terms between treatment and year dummies (where the reference is treat*2013). The sample includes the municipalities outside autonomous regions or metropolitan cities. Treated municipalities are those where the president is born (col. 1-2) and those whose mayor became president after 2013 (col. 3-4). Robust standard errors, clustered at the municipality level, are in parentheses, * p<0.10, ** p<0.05, *** p<0.01.

B.1.3 Transfers to Municipalities as a share of total municipal revenues

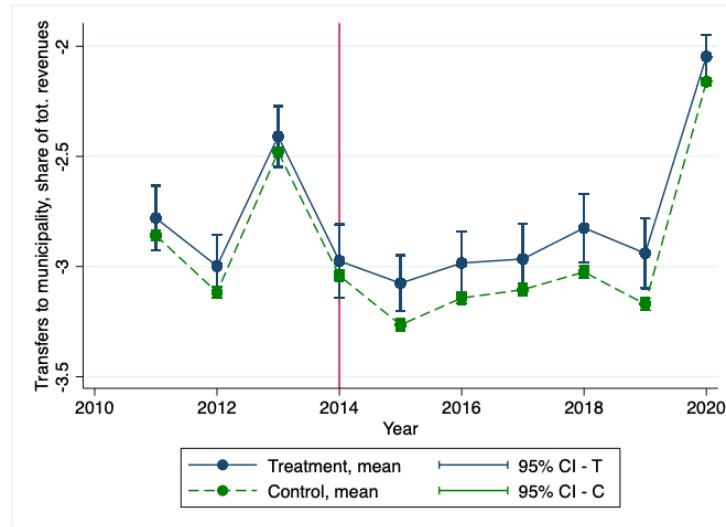


Figure B11: Trend of (the logarithm of) transfers as a share of total municipal revenues. Treatment group: municipalities in which the provincial president is serving as mayor, after the reform.

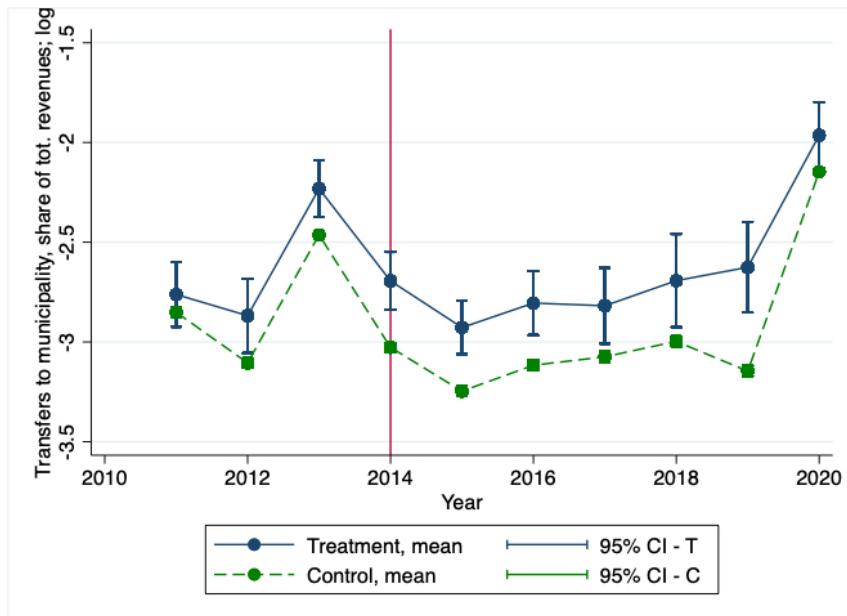


Figure B12: Trend of transfers as share of municipal revenues. The treatment group is composed of the municipalities that are place of birth of the presidents (regardless of whether they are the mayors or not).

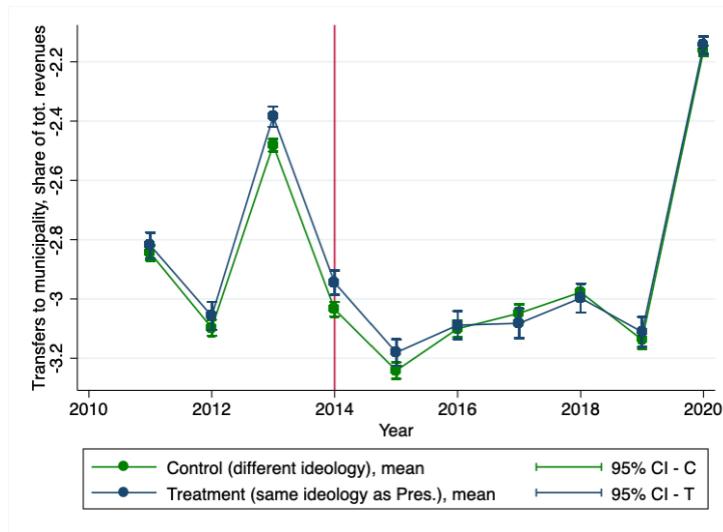


Figure B13: Trend of transfers as share of municipal revenues. The treatment group is composed of the municipalities that, after the reform, are of the same political orientation as the President.

B.1.4 Robustness tests and placebos

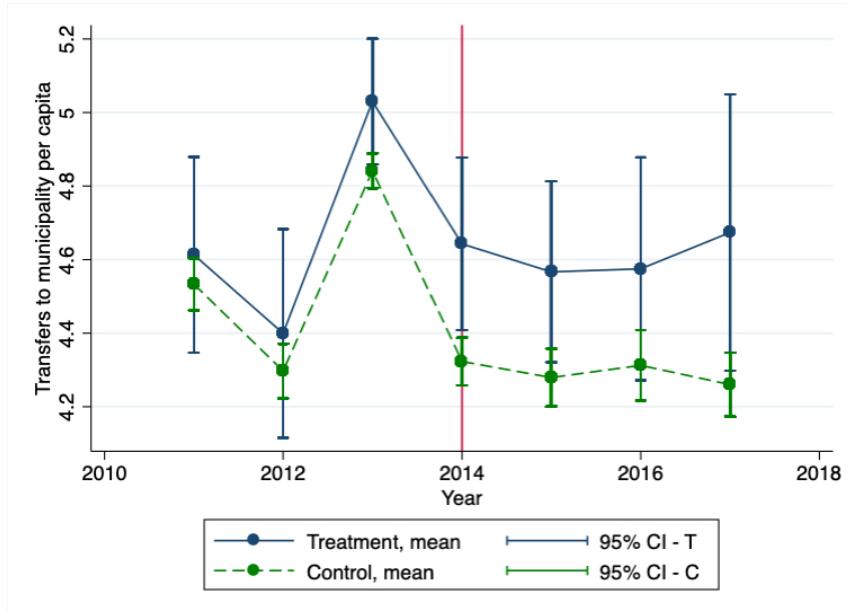


Figure B14: Trend of the transfers per capita to municipalities. Treated municipalities are those whose mayor-president had become mayor before the reform was passed. Only municipalities with mayors elected before the reform in control group (up to 2017, before replacement). I include capitals not to further reduce a very small sample, but results are qualitatively similar excluding them.

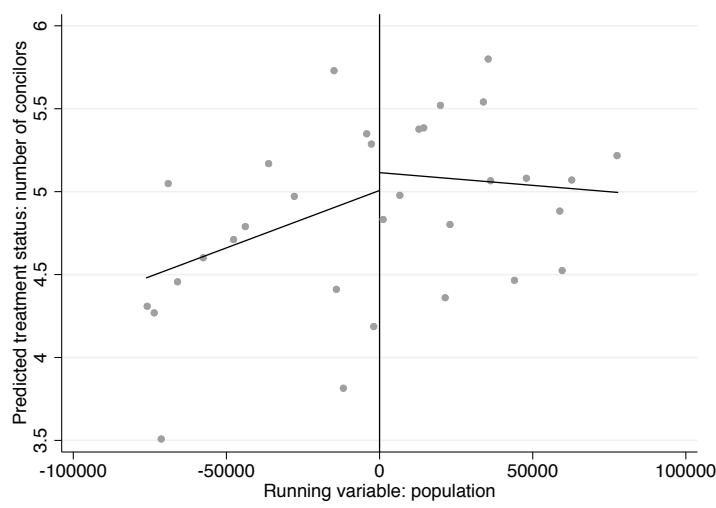


Figure B15: (Log) Transfers per capita to provincial president's municipality of birth. The running variable is provincial population normalized around the closest threshold: the thresholds are at 700,000 and 300,000 inhabitants. Passing the threshold, the size of provincial council increases by 4 councilors before 2014 and 3, on average, after 2014. The sample is the universe of presidents' birthplaces (2008-2020): 464 municipality-years.

Table B7: Municipal transfers per capita. Effect of larger provincial council on municipal transfers to presidents' hometowns (2008-2020). Total sample: 464 observations. Provincial capitals included.

	Transfers to municipality per capita (ln)		
	(1)	(2)	(3)
Treatment effect	0.283 (0.271)	0.273 (0.322)	0.331 (0.236)
Robust p-value	0.508	0.336	0.599
Observations	163	111	264
Polynomial order	1	1	1
Bandwidth	CCT (75,927.9)	50,000	100,000

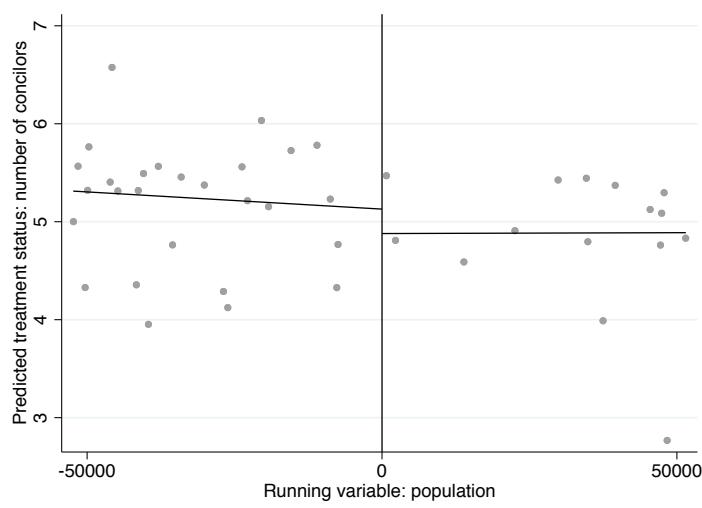


Figure B16: (Log) Transfers per capita to provincial president's municipality of birth. The running variable is provincial population normalized around the closest threshold: the thresholds are at 1,000,000, 500,000 and 250,000 inhabitants. Before 2014, the wage of provincial presidents increased after passing the population threshold. The sample is the universe of presidents' birthplaces (2000-2014): 589 municipality-years.

Table B8: Municipal transfers per capita. Effect 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.1.5 Transfers to Municipalities. Longer sample: 2008-2020

I report here the same analysis but using a broader sample, which includes years from 2008 to 2020 and that includes provincial capitals both in the treated and control group, unless differently specified. Overall, Figures B17 and B18 and Tables B9 and B10 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, 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 big fluctuations and the frequent tax reforms, the treated and the control municipalities were extremely similar before the reform and their trends virtually overlap in the pre-reform periods.

While the trend in transfers is perfectly parallel in the analysis focusing on cities whose mayor became president (see Figure B17), small pre-trend seems to anticipate the reform when the treated group is defined as the presidents' birthplaces (see Figure B18). This may be caused by the 2012 reform project described in note 7, soon declared unconstitutional, which postponed the 2012 and 2013 provincial elections and would have prescribed provincial leaders to be chosen among municipal politicians. Those years were thus a transition period and presidents may have started modifying their behavior before the Delrio reform was enacted. I address this issue in multiple 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). Finally, I restrict my main analysis to 2011-2020, that is, to years after the fiscal reform that dramatically changed municipal budgets, where the treated and control lines move in a parallel way.

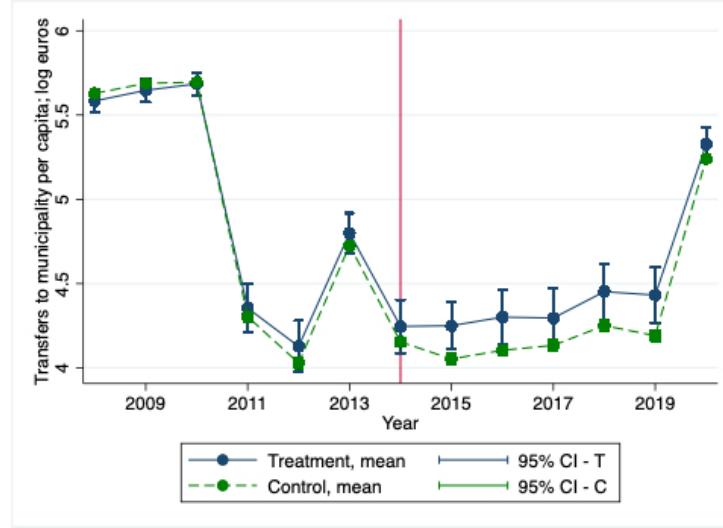


Figure B17: Trend of (the logarithm of) transfers to municipality per capita. Treatment group: municipalities in which the provincial president is serving as mayor, after the reform.

Table B9: Change in the amount of municipal transfers (2008-2020). Treatment group: municipalities in which the provincial president is serving as mayor, after the reform.

	(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*After	0.144*** (0.0491)	0.119** (0.0467)	0.160*** (0.0488)	0.134*** (0.0473)	0.113** (0.0458)	0.0979** (0.0461)
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	69,295	69,295	69,298	69,298	69,298	69,298
R-squared	0.751	0.786	0.869	0.887	0.679	0.719

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 only includes those (5,507) municipalities not in autonomous provinces or regions. I also exclude metropolitan cities. Treated municipalities are those whose mayor was elected president of the province after the reform (2014-2020). Robust standard errors, clustered at the municipality level, are in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

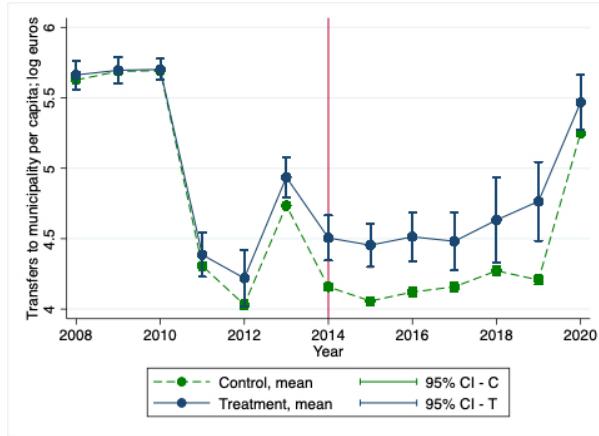


Figure B18: Trend of (log) transfers per capita. The treatment group is composed of the municipalities that are place of birth of the presidents (regardless of whether they are the mayors or not).

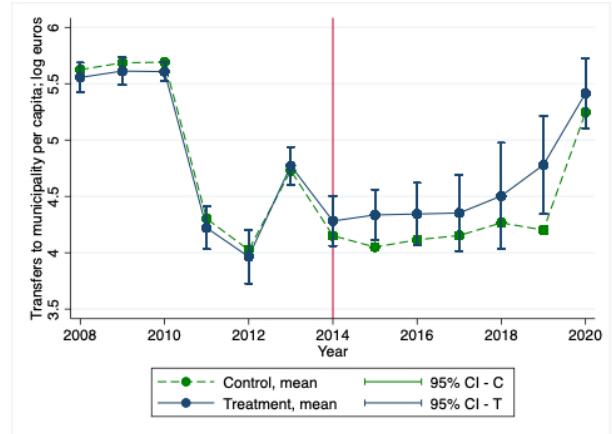


Figure B19: Trend of (log) transfers per capita. The treatment group is composed of the municipalities that are place of birth of the presidents (regardless of whether they are the mayors or not). Capitals excluded.

Table B10: Change in the amount of municipal transfers (2008-2020). The treatment group is composed of the municipalities that are place of birth of the president.

	(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 only includes those (5,431) municipalities not in autonomous provinces or regions. I also exclude metropolitan cities. To maintain a consistent group and perform a standard Diff-in-Diff, Treated municipalities are those that were birthplace of the president exactly in the considered year. Using a constant treatment groups (all cities that were a birthplace at least once), the interaction term remains similar. Capitals included. Treatment period: 2014-2020. Robust standard errors, clustered at the municipality level, are in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

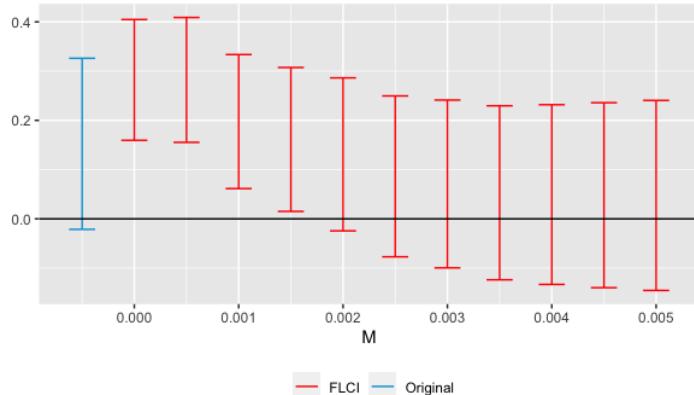


Figure B20: Rambachan and Roth (2020) inference method. Impact in 2015 using 2013 as last pre-reform year (2000 is the first). Province by years and municipality fixed effects included, standard errors clustered at the municipality level. $M=0$ implies a linear pre-trend, larger M allows larger deviations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

B.2 SIOPE data: transfers from local governments

In this section I replicate some results using an alternative data source: SIOPE. While AIDA PA provides data on public expenditure and revenues, homogeneously aggregated by sectors and consistent over time, data from SIOPE are less homogeneous and are subject to relevant changes in reporting over time (with a break in consistency in 2017), which make their systematic use more problematic. At the same time, SIOPE has the advantage of reporting more disaggregated subcategories of revenues. I thus replicate below the main results of Section 5 using a different measure of transfers: instead of the total amount of current transfers received by each municipality (the only measure of transfers from AIDA PA that is consistently available for the all period), 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 revenues (the ones 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). The results, plotted in Figures B21 to B24 qualitatively confirm the results of Section 5, indicating an increase in local-public funds toward municipalities connected to a president.

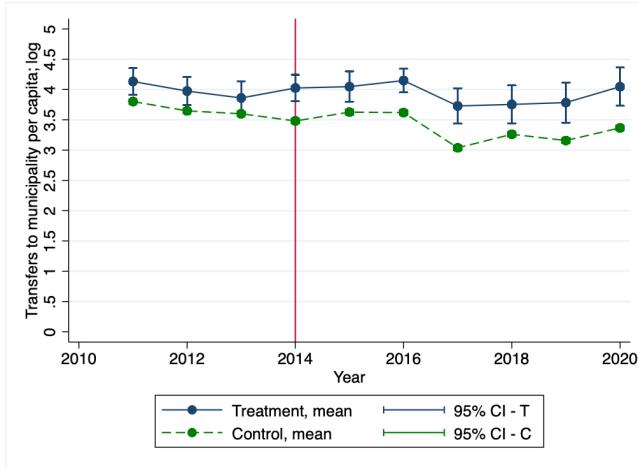


Figure B21: Trend of (the logarithm of) transfers per capita: sum of current and capital transfers from any local government. The treatment group changes over time and it is composed of the municipalities that are place of birth of the president in office, excluding provincial capitals.

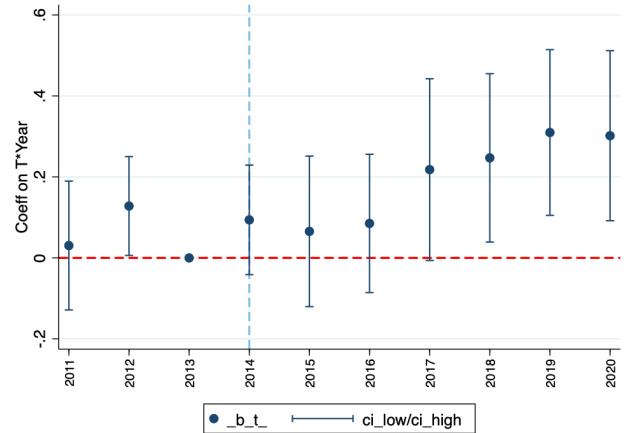


Figure B22: Trend of (the logarithm of) transfers per capita: sum of current and capital transfers from any local government. The treatment group changes over time and it is composed of the municipalities that are place of birth of the president in office, excluding provincial capitals.

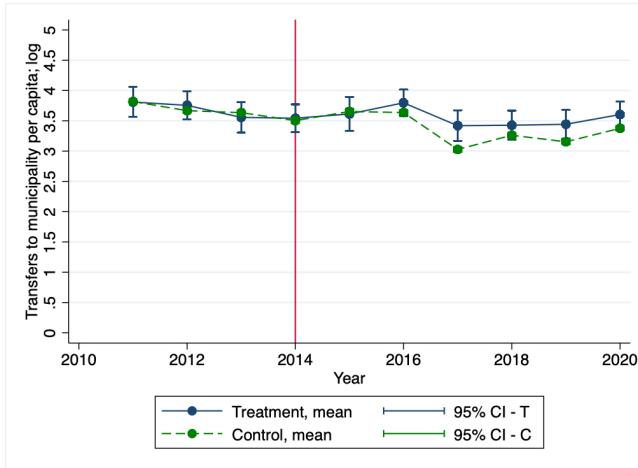


Figure B23: Trend of (the logarithm of) transfers to municipality per capita: sum of current and capital transfers from any local government. The treatment group is consistent over time and it is composed of the municipalities whose mayor became president since 2014, excluding capitals.

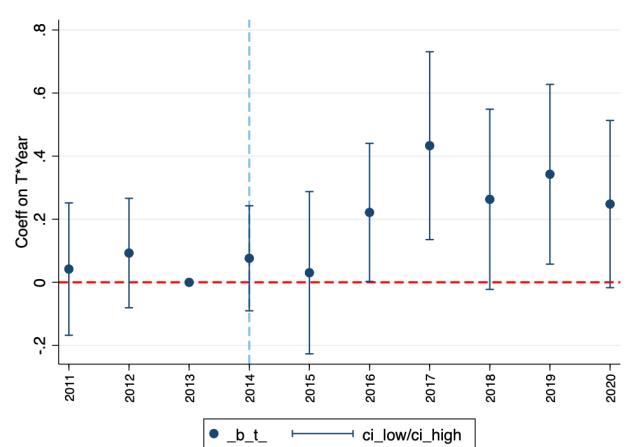


Figure B24: Trend of (the logarithm of) transfers to municipality per capita: sum of current and capital transfers from any local government. The treatment group is consistent over time and it is composed of the municipalities whose mayor became president since 2014, excluding capitals.

C Indirect elections and the composition of public spending

C.1 Synthetic control

The evidence presented in Section 6.2 comes with a potential issue. The trends in Figures 10 and 11 for the treatment and the control group are parallel but distant from one another. One may be concerned that the share of expenditure on transport and education in the control group is already 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 municipalities spend a smaller share of resources in transport and education; thus, even the highest counterfactual province has a share of expenditure on transport below 15% throughout the whole period.⁵⁵

Therefore, 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.⁵⁶ 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 10-year average.⁵⁷

⁵⁵ Municipalities have a wider range of responsibilities than provinces.

⁵⁶ This is not a formal synthetic control method, as discussed in the next note.

⁵⁷ An alternative solution to better match the level of the treatment group would be to construct the control group by selecting only those municipalities with high expenditure shares in the pre-treatment period (2009-2013). The problem with this approach is that, since on average the 8165 municipalities have an expenditure share in transport and education below 10%, when selecting only those municipalities with a pre-period share high enough to match the treatment group, these observations will mechanically suffer from reversal to the mean. Indeed, only those (few) municipalities with transitory exceptional expenses would be selected. For instance, think of a municipality with an average expenditure share in transport. If it 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. However, it will 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. As a placebo, if I restrict to a share in transport expenditure above 20% in 2009-2011, the trend progressively drops from about 23% to 15% in 2016. The same would happen if I restricted to my whole pre-intervention period.

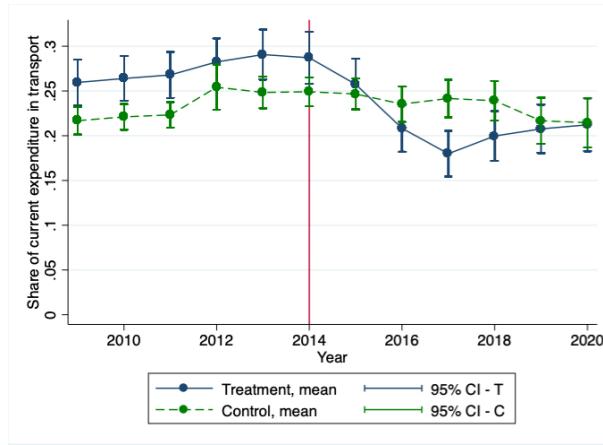


Figure C1: Trend of the share of current expenditure on transport, for all the 219 municipalities (from 47 different provinces) whose full-period (2009-2020) mean of expenditure on transport is above 20%.

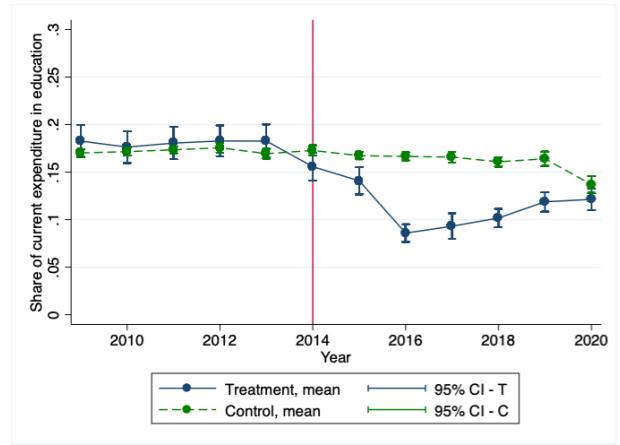


Figure C2: Trend of the share of current expenditure on education when restricting to all the 914 municipalities (from 75 provinces) whose full-period mean of expenditure on education is above 15%.

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 confirmed in Table C1.

Table C1: Impact of the reform on expenditure on transport and education (share of total expenditure). Restricted sample.

	(1)	(2)
RESTRICTED SAMPLE	Transp. Curr. Share	Educ. Curr. Share
Interaction	-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 are current share of expenditure on transport (col. 1-2) and education (col. 3-4). Independent variables include post-treatment dummy (After), dummy for treatment group (Treat), and the interaction term, which is the coefficient of interest. Autonomous provinces and metropolitan cities are excluded. The control group is reconstructed as follows: for share of expenditure on transport it includes 219 (from 47 provinces) municipalities with 12-year average expenditure share in transport above 20% in control group. For share of expenditure on education: 914 municipalities (from 75 provinces) above 15% of current expenditure share in education. Province level fixed effects. 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

To have a better sense of when exactly the reform begins having its effects, I also run the following specification:

$$Y_{p,t} = \alpha + T_p * \sum_{t=2009}^{2020} year_t \gamma_t + \psi_t + \gamma_p + \epsilon_{p,t} \quad (C3)$$

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 11 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.

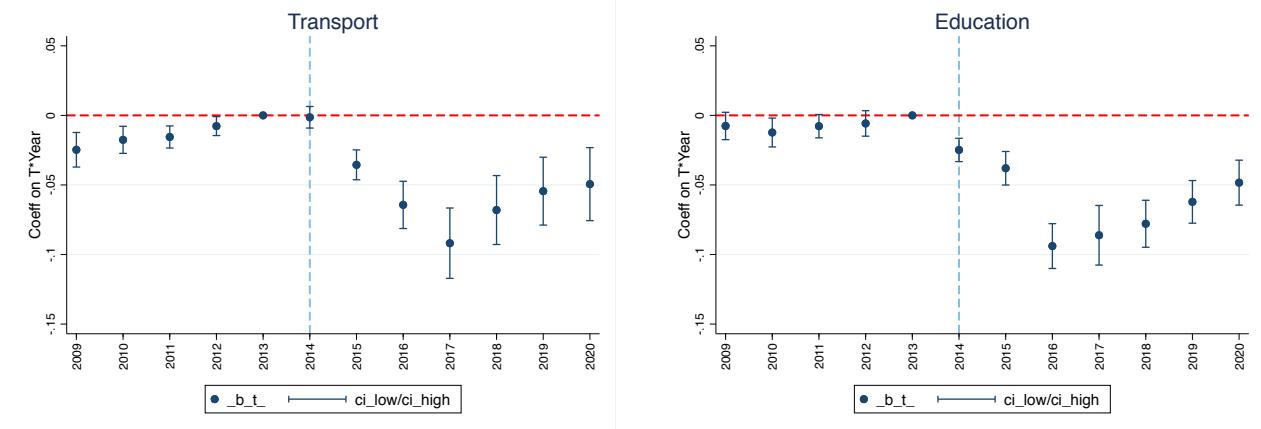


Figure C3: Share of current expenditure on transport and education. Treatment by year coefficients are plotted. Reference omitted year: 2013. Province and year FE included. Cluster at the province level.

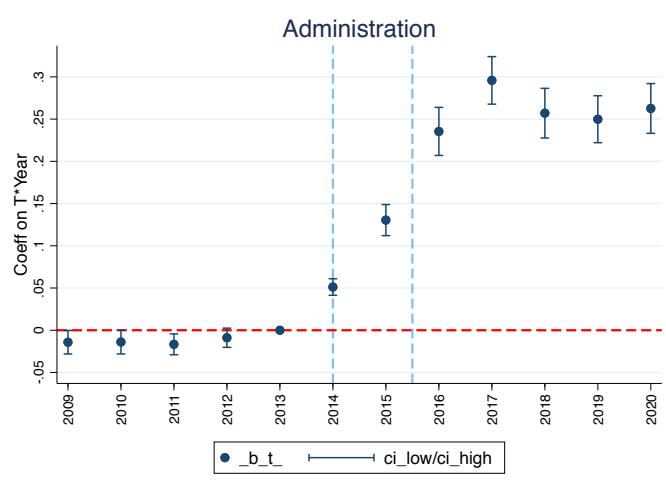


Figure C4: Event study: share of current expenditure on administration and control. Treatment by year coefficients are plotted. Reference omitted year: 2013. Province and year FE included. Cluster at the province level.

C.3 Overall expenditure and revenues

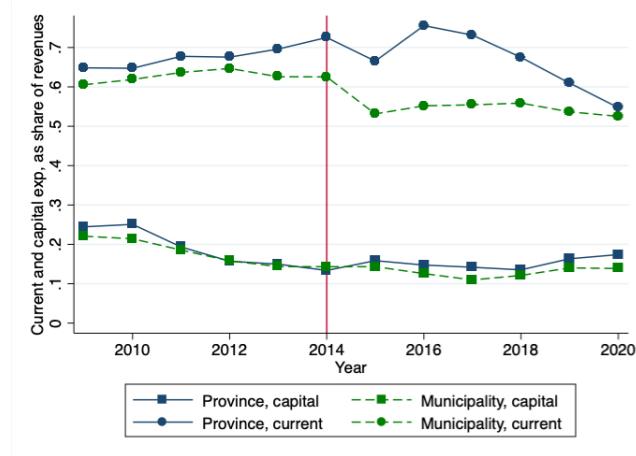


Figure C5: Trend of provincial and municipal current and capital expenditure as a share of total revenues

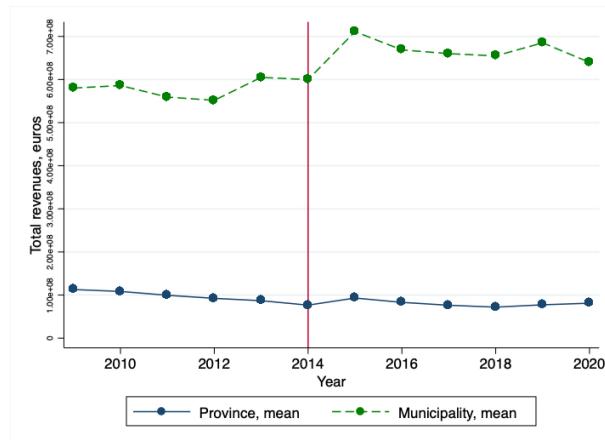


Figure C6: Trend of total revenues for provinces and Municipalities.

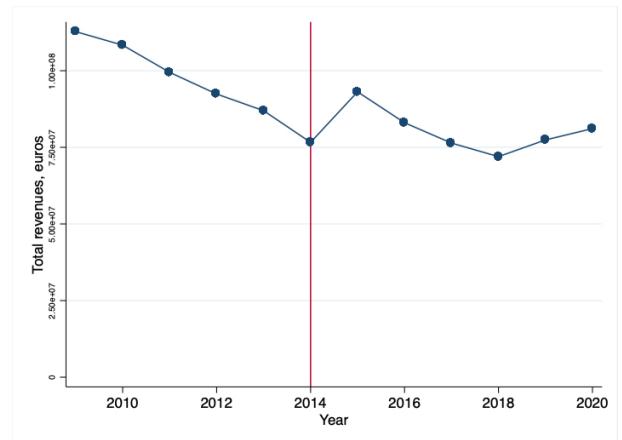


Figure C7: Trend of total revenues, focus on provinces.

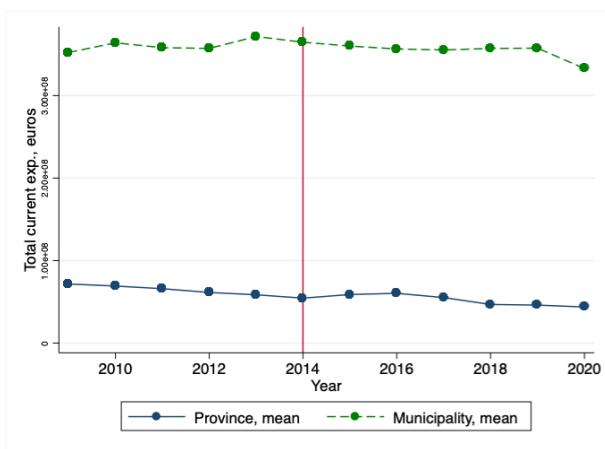


Figure C8: Trend of Total Current Expenditure.

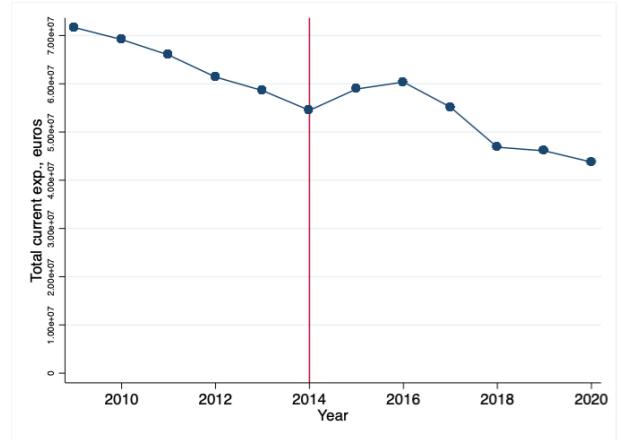


Figure C9: Trend of total current expenditure, focus on provinces.

C.4 Complementary results

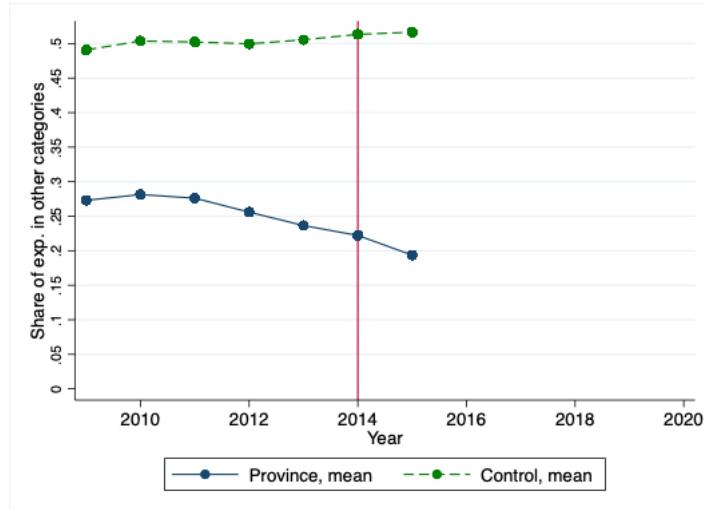


Figure C10: Share of current expenditure in sectors other than transport, education, administration

C.5 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, which is the coefficient of interest. Post-intervention period: 2014-2020 (2014-2015 for administrative exp. due to data availability). Fixed effects are at the province and year level. 11 autonomous provinces and metropolitan cities are excluded (not affected or 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.

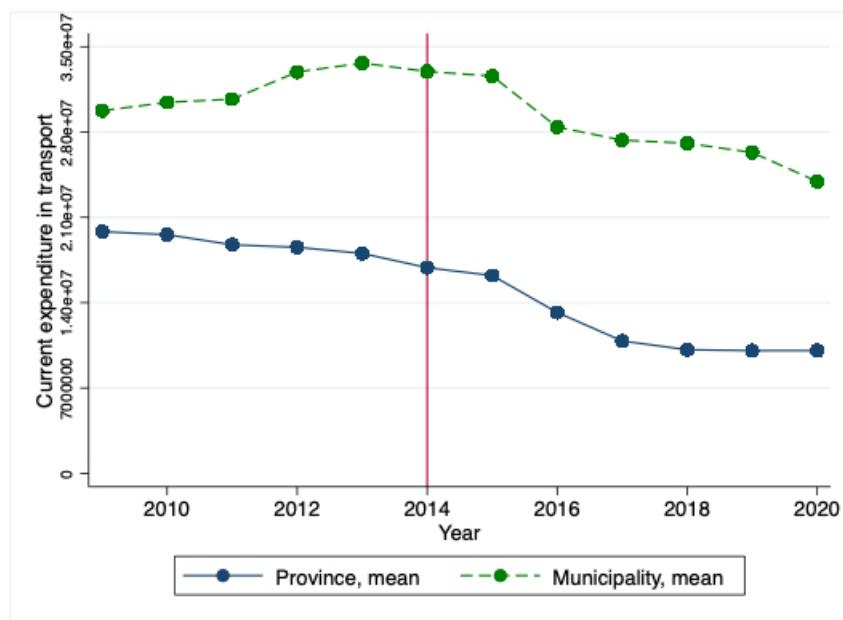


Figure C11: Trend of the current transport expenditure, euros

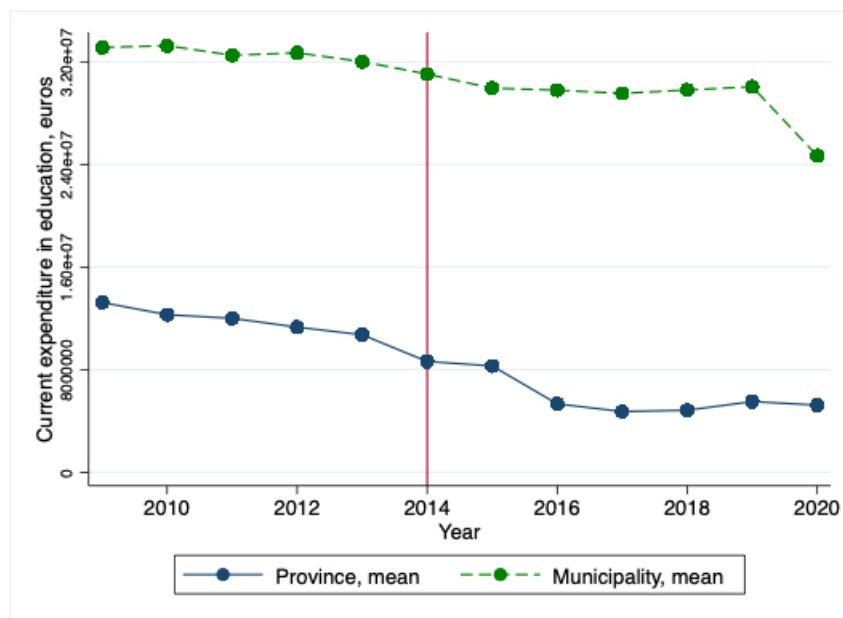


Figure C12: Trend of current expenditure on education, euros.

C.6 Decomposition of administrative expenditure

In this section I provide some additional information of the areas in which provincial bureaucratic costs increased the most. More specifically, for each *mission* - 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, whose sector is related to each specific mission.

Figure C13 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. As shown in panels (c) to (e), 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 towards private entities (firms, families and NGOs), and on transfers to public entities. I then sum up the different categories and show the result in panel (f), which confirms a very stark increase in administrative spending starting from 2015.

In light of its relative importance, panel (d) deserves a more detailed discussion. The types of transfers included in this category, are the ones directed to public entities, whose purpose is not specifically connected to another mission. For instance, this would exclude transfers made with the purpose of providing transport-related public good, as this would be contained in the *transport* mission. Originally, this category would include transfers to the central government, which sharply increased after 2013, due the 2014 budget law that required the provinces to contribute to the central government's budget. This could confound my results since the reason for the increase was totally exogenous to the provinces' choice; thus, I used the *SIOPE* dataset to collect the amount of funds each province sent to the central government each year and I subtracted 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.

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 C14, which reassuringly, excludes any large increase in transfers of funds toward municipalities.

Finally, to provide a more complete picture of the described dynamics and match my graphs in Section 6, Figure C15 reports the share of total administrative spending 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 12, confirming that transfers to the central government do not play a central role in explaining my results.

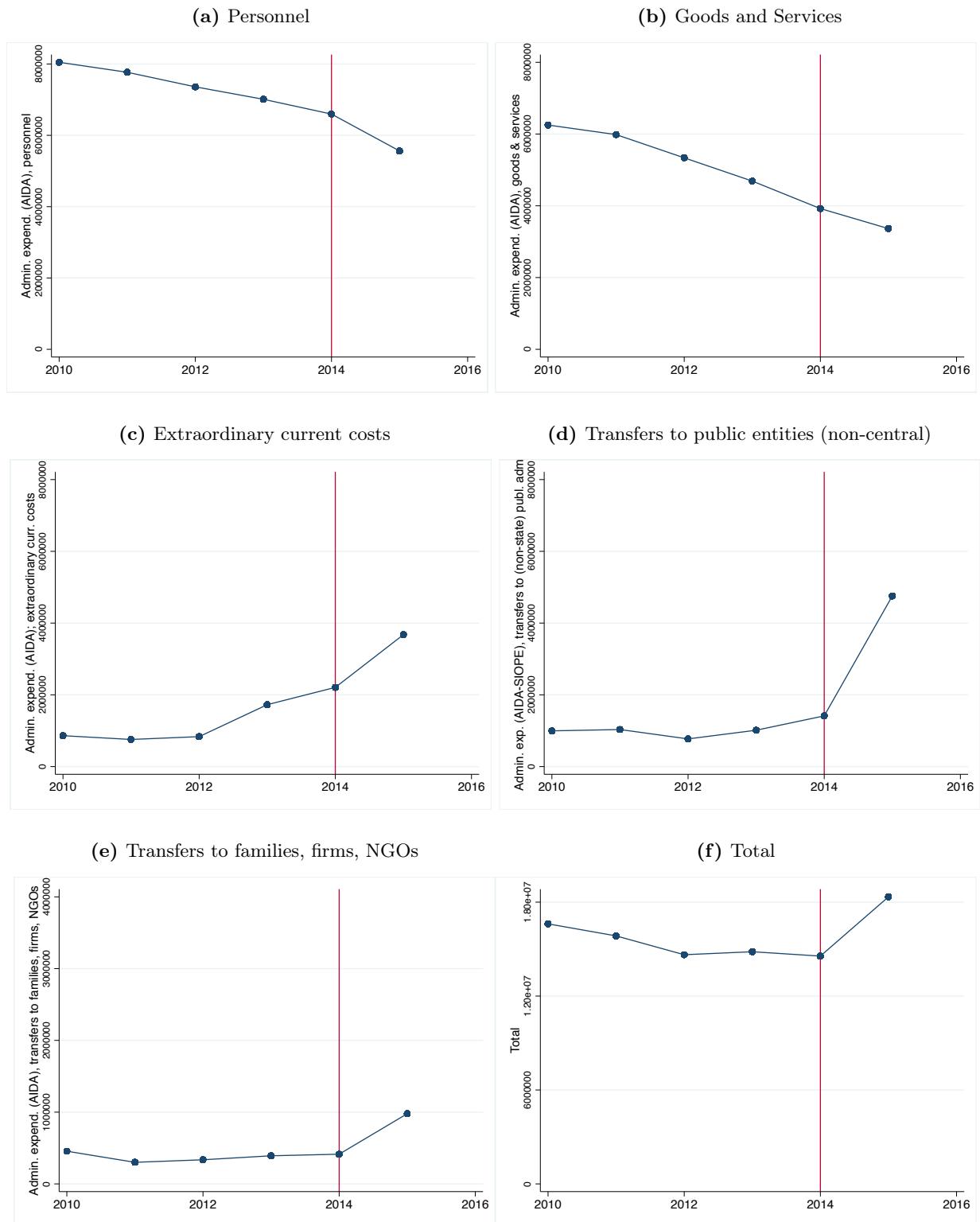


Figure C13: Absolute administrative expenditure, euros. Total and subcategories.

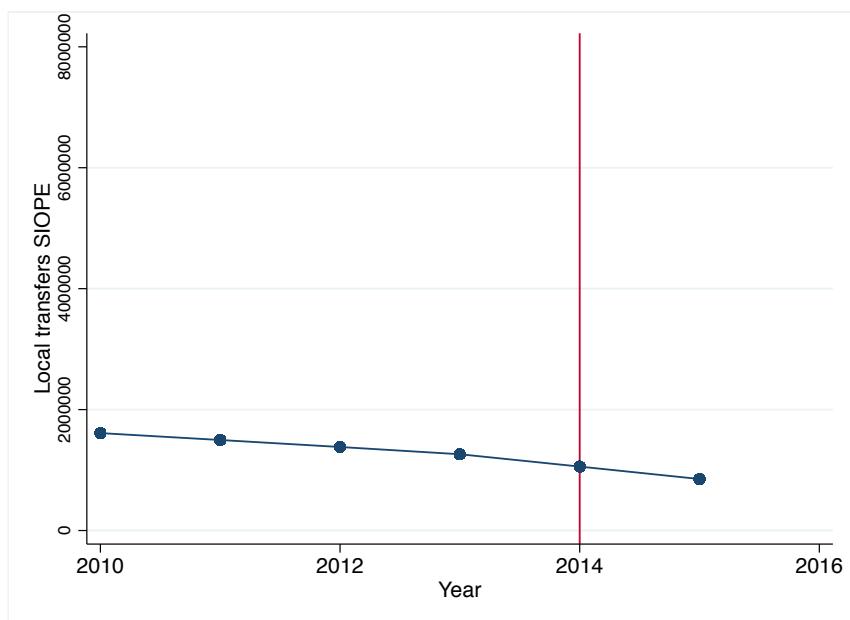


Figure C14: Total amount of transfers from provinces to municipalities, unions of municipalities, comunità montane.

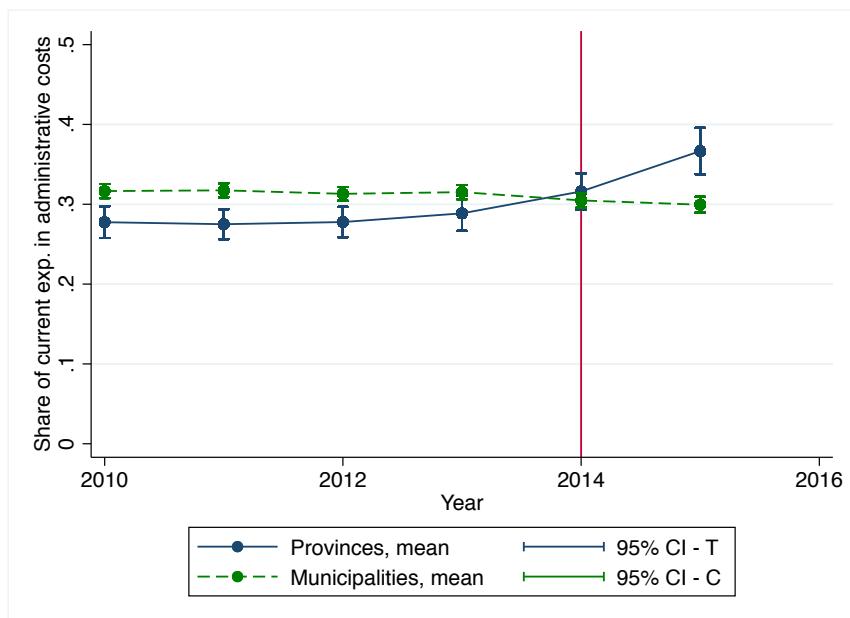


Figure C15: Bureaucratic costs as a share of current spending, net of transfers to central government.

C.7 Robustness Checks

In this section I discuss some major 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.⁵⁸ 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 C16 to C18). 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. Figures C20, C21 and C22 plot the coefficient of the interaction term per year for those provinces that elected their president in (late) 2014 and for those with later elections. In education and administration, we see that provinces with late elections show a small effect already in 2014, and catch up in 2015: this pattern suggests that the drop in accountability played a role even before turnover (highlighting the importance of incentives vs selection), and fortified after the election. The effect on transport in 2014 was not significant for either group, but even for this category we see the same happening in 2015.

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 only 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 signifi-

⁵⁸Figure C5 shows current expenditure is actually quite a constant share.

cantly 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 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 C19 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. 1-2), education (3-4) and administration (col. 5-6). Independent variables include post-treatment dummy (After), dummy for treatment group (Treat) and the interaction term, which is the coefficient of interest. 11 autonomous provinces and metropolitan cities are excluded (not affected or 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.

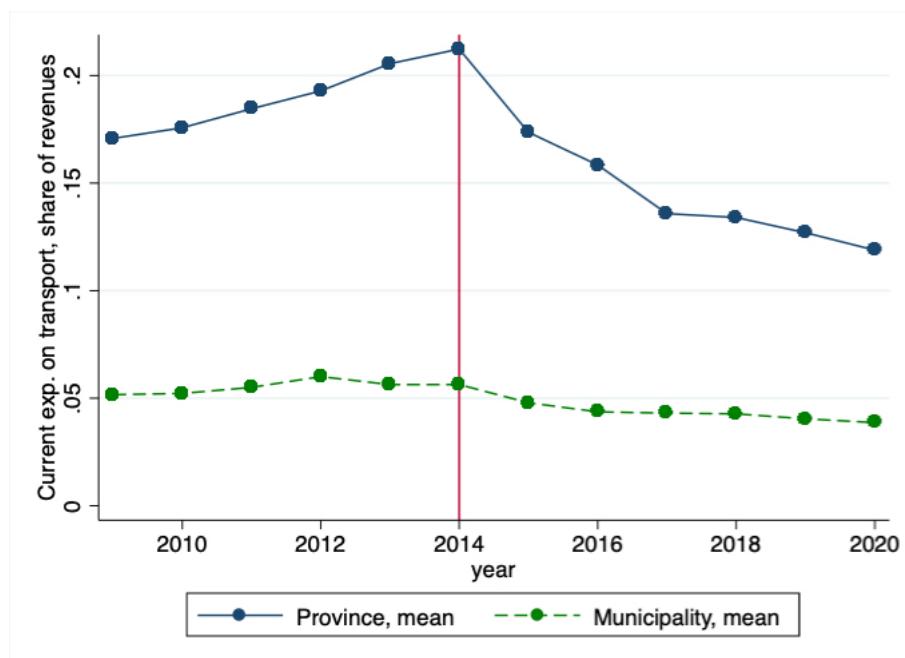


Figure C16: Transport expenditure, share of total revenues

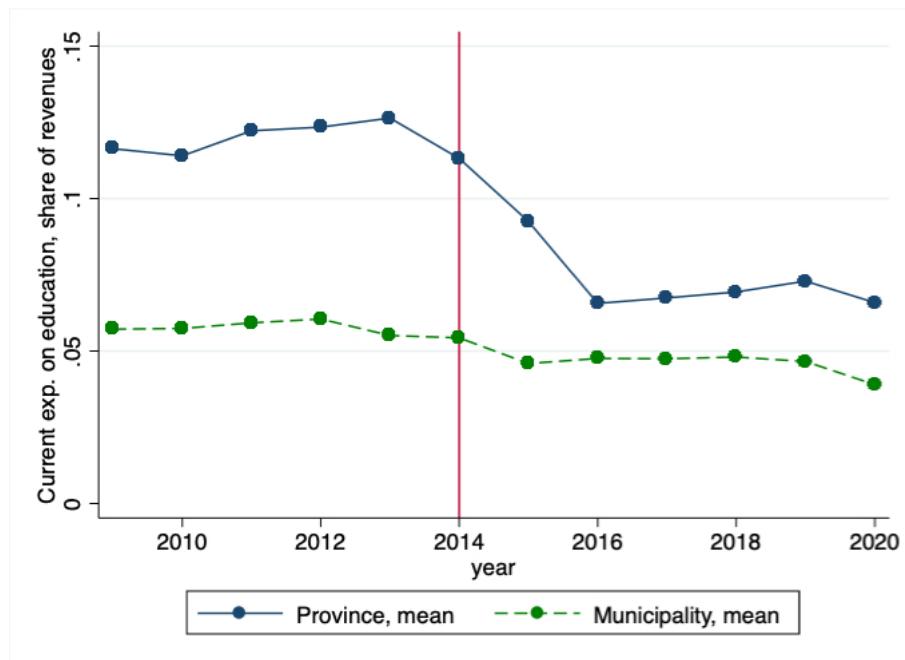


Figure C17: Education expenditure, share of total revenues

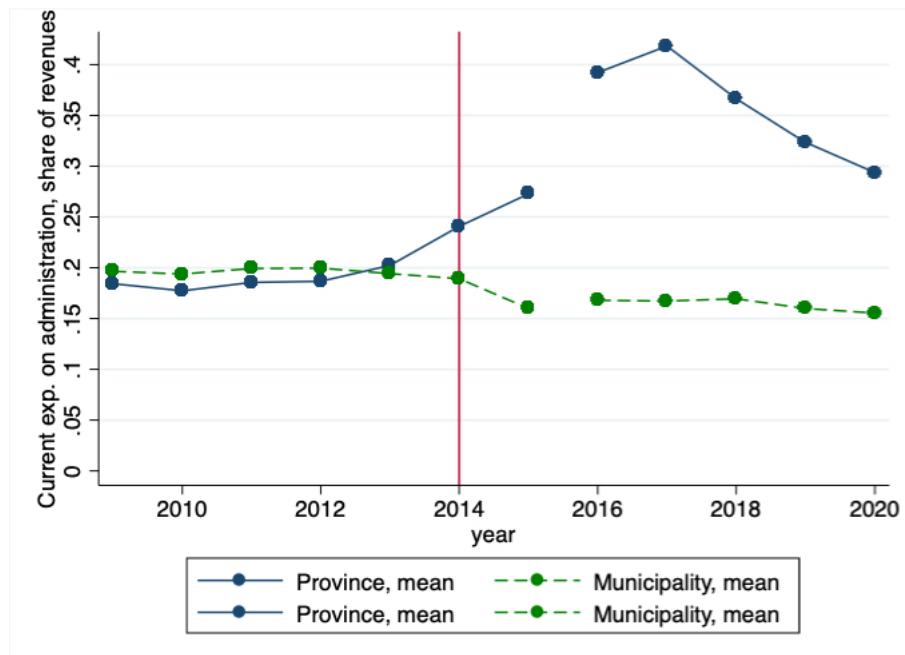


Figure C18: Administration expenditure, share of total revenues

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, share in education and share in administration expenditure among total current expenditure. Independent variables include post-treatment dummy (After), dummy for treatment group (Treat) and the interaction term, which is the coefficient of interest. Fixed effects are at the provincial level. 11 autonomous provinces and metropolitan cities are excluded (not affected or 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, share in education and share in administration expenditure among total current expenditure. Independent variables include post-treatment dummy (After is 1 from 2015 to 2020), dummy for treatment group (Treat) and the interaction term, which is the coefficient of interest. Fixed effects are at the province level. 11 autonomous provinces and metropolitan cities are excluded (not affected or 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 (Treat), and the interaction term, which is the coefficient of interest. I also control for the provincial population. 11 autonomous provinces and metropolitan cities are excluded (not affected or 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)		
	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 in transport, share in education and share in administration expenditure among total current expenditure. Independent variables include post-treatment dummy (After is 1 for 2014 and 2015), dummy for treatment group (Treat) and the interaction term, which is the coefficient of interest. Years 2016 to 2020 are dropped. 19 provinces and controls in 2015 (those with high transfers) are also dropped. Fixed effects are at the provincial level. 11 autonomous provinces and metropolitan cities are excluded (not affected or differently affected by the reform). Robust standard errors, clustered at the province level, are in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure C19: Spending in transport and education (public good). Excluding any provincial transfer to the central government from the numerator and denominator.

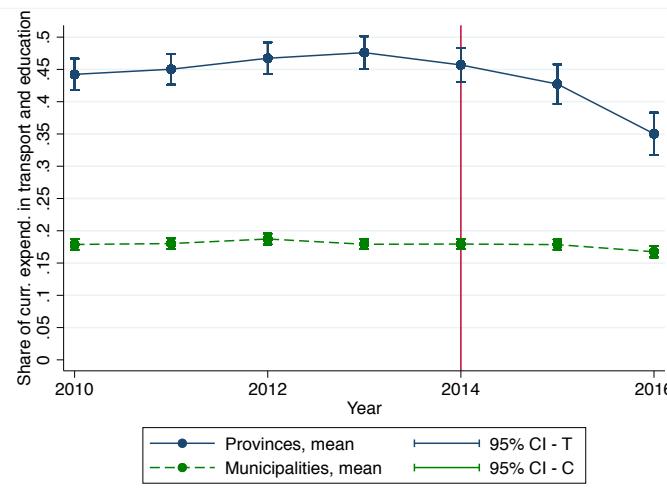
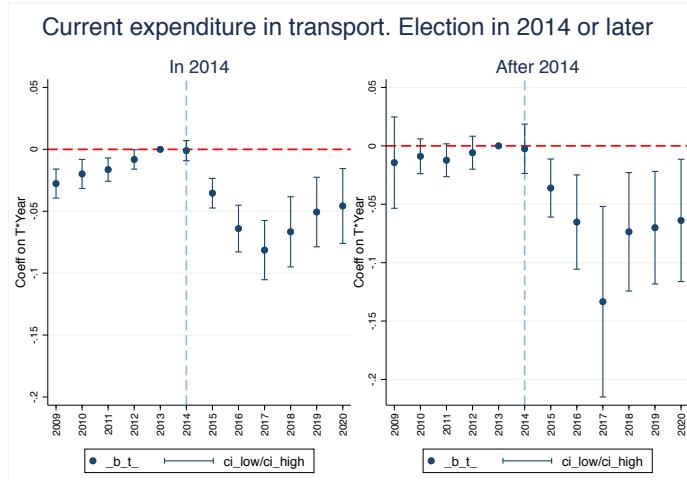
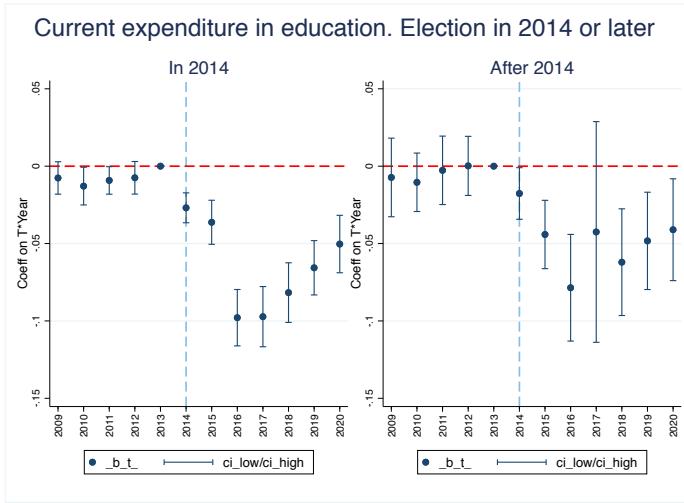


Figure C20: provinces with election in 2014 vs those with successive elections: transport



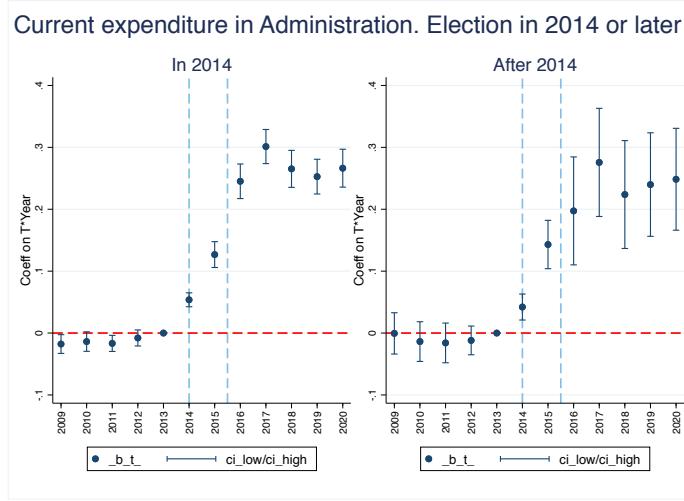
Note: Reference year 2013. Number of observations 1762 and 465, respectively. Province and year FE. Cluster at province level.

Figure C21: provinces with election in 2014 vs those with successive elections: education



Note: Reference year 2013. Number of observations 1762 and 465, respectively. Province and year FE. Cluster at province level.

Figure C22: provinces with election in 2014 vs those with successive elections: administration



Note: Reference year 2013. Number of observations 1762 and 465, respectively. Province and year FE. Cluster at province level.

Table C8: Robustness Check: summing expenditure on maintenance of road infrastructures and total current expenditure

(1)	
Transport share (adjust.)	
Treatment*After	-0.002 (0.005)
Provincial Fe	Yes
Year Fe	Yes
Observations	1,271
R-squared	0.970

The dependent variable is the share in transport over the sum of current expenditure and road costs (since this category was erroneously excluded from the transport sector). Independent variables include post-treatment dummy (After), dummy for treatment group (Treat) and the interaction term, which is the coefficient of interest. Province fixed effect in column 2 and 3. Autonomous provinces and metropolitan cities are excluded (not affected or 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.

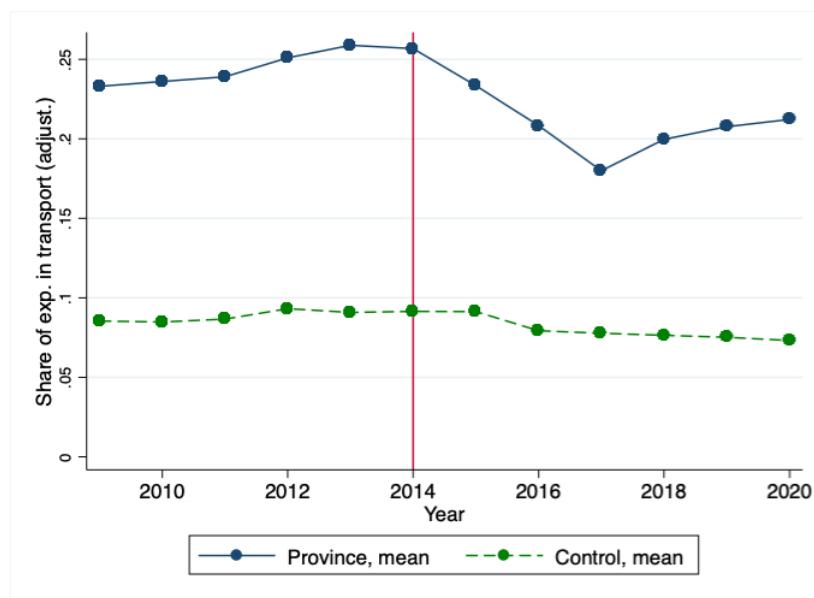


Figure C23: Trend in current expenditure on transport with adjusted total current expenditure

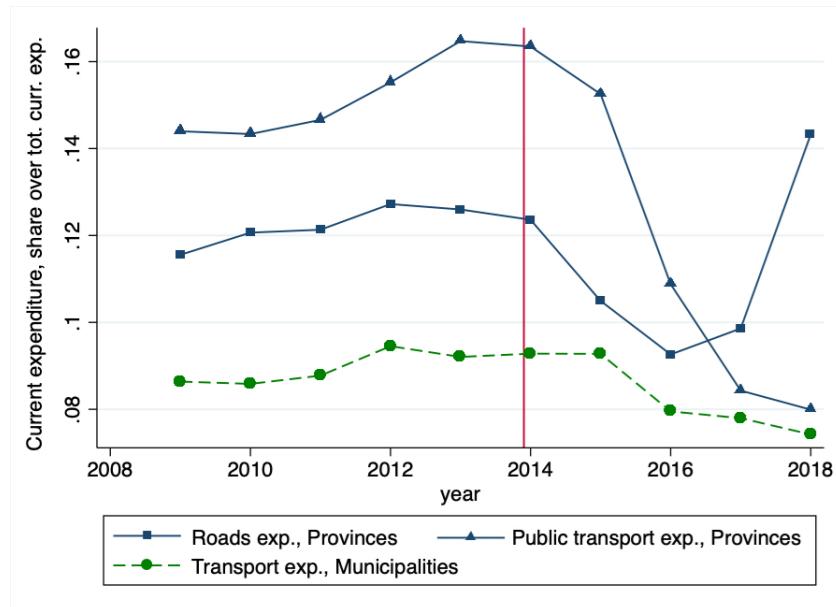


Figure C24: Trend of the share of current expenditure on transport over overall current expenditure divided into the two main programs: public transport and road expenditure

C.8 Regional trends

I am presenting here the trend of the expenditure in the transport- and education-related sectors coming from the database ‘SIOPE.it’. What I want to convey with these graphs is the idea that compared to 2013 (and partly 2014) there was no stark increase in the level or share of expenditure at the regional level in transport and education. This provides evidence for the assumption that no major responsibilities were moved to the regions immediately after the reform. A first important problem 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 included in their capital component. A second problem is that in 2011 and 2012 there were huge changes in the level of expenditure of the regions, due to a national law that imposed maximum levels of general and sector-specific expenditure for the region. The overall regional expenditure in 2012 was one third of that in 2013. From 2013 on, the situation stabilized. Having highlighted all these issues, we can still see that after 2014 there was no increase in 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.

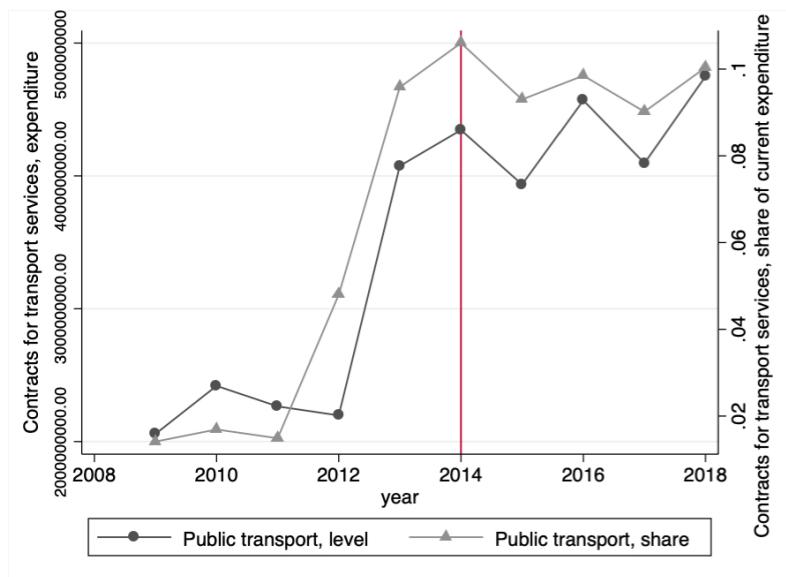


Figure C25: Trend of regional current expenditure on public transport. Level and share of total current expenditure

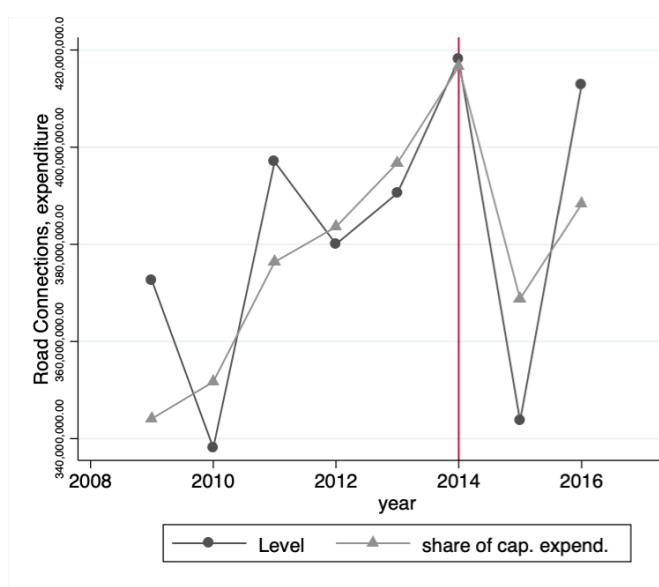


Figure C26: Trend of regional capital expenditure on road infrastructure. Level and share of total capital expenditure

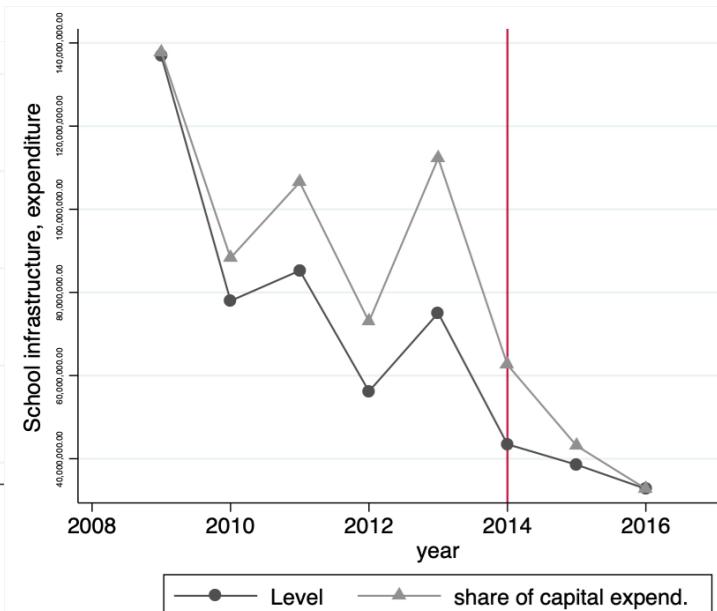


Figure C27: Trend of regional capital expenditure on school infrastructure. Level and share of total capital expenditure

D Probabilistic voting model (forward-looking voters)

A possible approach is to conceptualize politicians' decisions in the context of rent-motivated politicians facing forward-looking voters. For this, I will mainly rely on the classic probabilistic voting model with rents as in Persson and Tabellini (2000) section 4.2 (henceforth PT), with forward looking voters and binding electoral platform, based on which voters select a politician.

Politicians only raise taxes from a level-specific portion of income. The budget constraint is:

$$\tau^m * y^m = (g^m + r^m) \quad \text{and} \quad \tau^p * y^p = (g^p + r^p) \quad \text{with} \quad \tau^m, \tau^p \leq 1 \quad (\text{D4})$$

Citizens derive utility from both private consumption and public good:

$$W^m(g^m, r^m) = (y^m - g^m - r^m) + H(g^m) \quad \text{and} \quad W^p(g^p, r^p) = (y^p - g^p - r^p) + U(g^p) \quad (\text{D5})$$

In the real world, municipal politicians (henceforth M) typically vote for the provincial president (henceforth P) based on their 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). When voters cannot directly choose both politicians,

they vote for the candidate M of party A, M_A , if her platform and that of the candidate-president from party A, P_A , is sufficiently good for them. That is if:

$$W^j(g_A^m, r_A^m) + \rho W^j(g_A^p, r_A^p) \geq W^j(g_B^m, r_B^m) + \rho W^j(g_B^p, r_B^p) + \sigma^{ij} + \delta \quad (\text{D6})$$

Since people only directly vote for M, the subsequent provincial election (and public good) becomes less salient: this is captured by $\rho (> 0)$. This means that when choosing how to vote, citizens mis-calculate their utility from the province as they pay less attention to it. As in PT, I then assume that voters' party bias σ has the same distribution in every group j. The distribution is uniform on $[-1/2\phi; 1/2\phi]$, the popularity shock δ is also uniformly distributed on $[-1/2\psi; 1/2\psi]$.

Municipal politicians care of endogenous and exogenous rents per se - which they only obtain 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 (I implicitly assume here that $r^p > 0$ and focus on internal solutions). M's objective function is thus:

$$E(V_A^m) = p_A(\gamma r_A^m + R_A) - \frac{1}{r_A^p} \quad (\text{D7})$$

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 here is that the municipal politicians running the campaign can present a credibly binding platform for the provincial policy as well.⁵⁹ Thus, one can derive the probability of victory for the mayor of party A in municipal election, which is given by:

$$p_A = 1/2 + \psi[W^j(g_A^m, r_A^m) - W^j(g_B^m, r_B^m) + \rho W^j(g_A^p, r_A^p) - \rho W^j(g_B^p, r_B^p)] \quad (\text{D8})$$

What are the optimal g^m and g^p ? Taking the FOC of (13) we get:

$$\frac{\partial E(V_A^m)}{\partial g_A^m} = \frac{\partial p_A}{\partial g_A^m}(\gamma r_A^m + R_A) \longrightarrow (\gamma r_A^m + R_A)\psi W_g(g_A^m, r_A^m) = 0 \quad (\text{D9})$$

⁵⁹This 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.

Since $W_{gr} = 0$, then $W_g = 0$ for each level of r . Given the definition of W , this means that public good is optimally provided (see PT). The same is true for g^p . Notice that, by symmetry, in equilibrium both parties will converge on the same policy: $g_A = g_B = g^*$. This also implies $p_A = p_B = 1/2$. What about rents? Since:

$$\frac{\partial p_A}{\partial r_A^m} = -\psi \quad \text{and} \quad \frac{\partial p_A}{\partial r_A^p} = -\rho\psi \quad (\text{D10})$$

then,

$$\frac{\partial E(V_A^m)}{\partial r_A^m} = \frac{\partial p_A}{\partial r_A^m}(\gamma r_A^m + R_A) + \gamma p_A \rightarrow -\psi(\gamma r_A^m + R_A) + \gamma/2 = 0; \quad [r^m \geq 0] \quad (\text{D11})$$

$$\frac{\partial E(V_A)}{\partial r_A^p} = \frac{\partial p_A}{\partial r_A^p}(\gamma r_A^m + R_A) + \frac{1}{(r_A^p)^2} \rightarrow -\rho\psi(\gamma r_A^m + R_A) + \frac{1}{(r_A^p)^2} = 0 \quad (\text{D12})$$

therefore:

$$r^m = \max[0, \frac{1}{2\psi} - \frac{R_A}{\gamma}] \quad \text{and} \quad r^p = \sqrt{\frac{1}{\rho\psi(\gamma r^m + R_A)}} \quad (\text{D13})$$

Plugging the expressions in (D12) one into the other, we get: $r^m = \max[0, \frac{1}{2\psi} - \frac{R_A}{\gamma}]$ and:

$$r^p = \begin{cases} \sqrt{\frac{1}{\rho\psi R_A}} & \text{if } r^m = 0 \\ \sqrt{\frac{2}{\rho\gamma}} & \text{if } r^m > 0 \end{cases} \quad (\text{D14})$$

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. Indeed, having direct election both at municipal and provincial level, with voters choosing each politician directly, would still produce the optimal amount of public spending while rents are just like the standard case in PT, that is:

$$r^m = \max[0, \frac{1}{2\psi} - \frac{R^m}{\gamma}] \quad \text{and} \quad r^p = \max[0, \frac{1}{2\psi} - \frac{R^p}{\gamma}] \quad (\text{D15})$$

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 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 thus suggests that it is not indirectness per se that causes the drop in accountability, 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 where the salience is all on the final election (think of the Electoral College in the US).

E Accountability Model

This model is an extension of the classic accountability model in section 4.4 of Persson and Tabellini (2000), henceforth PT, where politicians only care of rents and voters decide on reappointment based on a reservation utility set to minimize rents.

In my setting there are two layers of government: the province, whose leader is P, and the municipality, whose leader is M. Both provide public good to voters and both only care about endogenous (r) and exogenous (R) rents from office (including reputation and future expected rents). For $i \in [P, M]$ the objective function is:

$$E(V_I^i) = \gamma r^i + p_I R^i \quad (\text{E16})$$

where p_I is the reelection probability and $\gamma \in (0, 1)$ indicates how transparent or salient the budget process is (difficulty in diverting resources). The budget constraint for each politician is:

$$\tau^m * y = (g^m + r^m) \quad \text{and} \quad \tau^p * y = (\theta g^p + r^p) \quad \text{with} \quad \tau^m + \tau^p \leq 1 \quad (\text{E17})$$

Politicians can thus potentially tax the all economy, but of course the sum of the two taxes cannot exceed 1 (they cannot extract more than y). Voters enjoy private consumption c as well as provincial and municipal public consumption (with concave utilities $H(g)$ and $U(g)$); their objective function $W(g, r)$ is thus:

$$W(g, r) = c + H(g^m) + U(g^p) = (y - \theta g^m - \theta g^p - r^m - r^p) + H(g^m) + U(g^p) \quad (\text{E18})$$

Voters are backward looking and reappoint the incumbent if and only if she has provided them with $W(g, r) \geq \omega$, i.e. at least their reservation utility. If a politician is not reappointed, an ex-ante identical challenger is elected. I am assuming that voters only observe their total welfare and if reservation utility is violated accordingly punish both politicians irrespectively of who is misbehaving. This may capture distrust in politicians: if one is caught stealing more money than expected, all politicians will pay that behavior. Reelection probability is thus:

$$p_I = \begin{cases} 1 & \text{if } W(g, r) \geq \omega \\ 0 & \text{otherwise} \end{cases} \quad (\text{E19})$$

Direct election. P and M are the incumbent politicians in office. Both are directly elected by voters simultaneously, thus the timing is the same in both cases: voters set a reservation utility $\omega \rightarrow$ the politician freely chooses policy (public good and rents) \rightarrow elections are held to confirm or replace the incumbents (with an identical alternative).

Given direct election, this is solved exactly like in section 4.4 of PT. To win reelection and satisfy voters, politicians need to provide them with $W(g, r) = \omega$. Substituting the expression for $W(g, r)$ from (E17) and isolating rents implies that they can appropriate:

$$r^i = y - \theta g^m - g^p - r^{-i} - \omega + H(g^m) + U(g^p) \quad (\text{E20})$$

Since p_I is discrete, the only choice of the incumbent is between satisfying the voter (winning reelection) or forgoing reelection. In the former case she will get $\gamma r^i + 1 * R^i$; in the latter, the best option is to steal all available resources y . In the standard accountability model in PT with one incumbent, she prefers reappointment iff:

$$\gamma r^i + R^i \geq \gamma y \quad (\text{E21})$$

Anticipating this (and assuming that indifferent politicians prefer reelection), voters allow them to divert the r^{i*} that satisfies (E20) with equality, i.e. $r^{i*} = \text{Max}[0, y - R^i/\gamma]$.

However, in my setting we have two incumbents, separate budget constraints and voters'

utility derived by both municipal and provincial policy and a common reservation utility jointly disciplining both governments. Thus, deviating from the equilibrium, a politician can cause the non-reelection of the other as well.

Approach 1. A possible solution to this inefficiency is to add the following *assumption (A1)*: *non re-election (of both politicians) triggers competition between P and M on available resources, which are split based on exogenous shares ($\alpha; 1 - \alpha$) that are common knowledge.* Under (A1), P and M can credibly threaten to steal only fractions αy and $(1 - \alpha)y$ if no-election is triggered (indeed, stealing all y would cause the non-reelection of both politicians). In this case, provincial equilibrium rents will be smaller, and precisely: $r^{p*} = \text{Max}[0, \alpha y - R^p/\gamma]$ and $r^{m*} = \text{Max}[0, (1 - \alpha)y - R^m/\gamma]$. To see this is an equilibrium, suppose one of the two deviates and gets more rents: this will violate voters' reservation utility causing non-reelection of both. In this case they would be able to only get away with αy and $(1 - \alpha)y$ leaving them indifferent. Notice that no equilibrium exists with fewer rents, as politicians would find it profitable to unilaterally deviate and trigger non-reelection.

Approach 2. If citizens know the government's budget constraint, they know that it is not possible for both M and P to divert y . Given an equilibrium taxation with $\hat{\tau}^p$ and $\hat{\tau}^m$, the politician who unilaterally deviates will at most get $(1 - \hat{\tau}^{-i})y$, where $\hat{\tau}^i + \hat{\tau}^{-i} \leq 1$. This would imply an equilibrium rent solved by $\gamma r^m + R^m = (1 - \hat{\tau}^p)\gamma y$ and $\gamma r^p + R^p = (1 - \hat{\tau}^m)\gamma y$.⁶⁰

Indirect election. P and M are the two incumbent politicians in office. Citizens elect the mayors and in turn the mayors elect the president. Given the backward looking type of vote in accountability models, the natural timing of the election is top-down and as follows: citizens set their reservation utility \rightarrow M sets a reservation utility $\omega^m \rightarrow$ P freely chooses policy (public good and rents) \rightarrow provincial elections are held (i.e M chooses on P's reelection) \rightarrow M chooses policy and rents \rightarrow municipal popular elections are held.

In this case P is accountable to M who is accountable to voters. The best M can do to discipline the incumbent is to set a reservation utility ω^m and a reelection probability as in

⁶⁰Solving this yields: $r^m = y/(1 + \gamma) + [\gamma(R^m) - R^p + (g^p - g^m)]/(1 - \gamma^2) = r^m(g^m, g^p)$ and $r^p = y/(1 + \gamma) + [\gamma(R^p) - R^m + (g^m - g^p)]/(1 - \gamma^2) = r^p(g^m, g^p)$. In the classic model, g is residually determined given the minimum amount of rents that voters are forced to leave to politicians. Notice here that we can actually identify an equilibrium of the form: $\hat{r}^m(g^{*m}, g^{*p})$; $\hat{r}^p(g^{*m}, g^{*p})$. This is an equilibrium: if voters set the reservation utility in this way, each politician cannot deviate in a profitable way, as her deviation will leave her with at most $r^d = (1 - \tau^{-i})y\gamma$, that is, indifferent.

(E18). Given its utility function however, the utility is based on the possibility to divert some rent. Note that voters here cannot do better: if they were to set reservation utilities that force M not to reappoint P when he steals some rent, this would trigger the disastrous action by P, namely forgoing reelection and steal all y .

Given the probability of election as in (E18), P will have the same two options, namely diverting all revenues and forgo reelection or diverting fewer rents and being reelected. The problem will be the same as in equation (E20), forcing M to allow provincial politicians getting away with $r^{p*} = \text{Max}[0, y - R^p/\gamma]$.

Maintaining (A1), then M can threaten P to forgo reelection triggering the split of the economy. Therefore, this equilibrium is only sustainable if $(1 - \alpha)y \leq y - r^{p*} = \text{Min}(y, R^p/\gamma)$; i.e. if the municipal outcome triggered by no reelection is sufficiently small. For the same reason P won't deviate as $\alpha y \leq y$.

Note that, given r^{p*} , ω^m can be set so that P uses the remaining resources to provide the optimal feasible public good g^{*p} or to drive g to 0. This is precisely the highest threat M can make to the voters, namely to set its reservation utility so to force P to set $g^p = 0$ and threat to divert all available resources $y - r^{*p} - 0$. To avoid this, voters will allow M to get away with rents that satisfy the following with equality:

$$\gamma r^m + R^m \geq \gamma(y - r^{p*}) \implies r^{*m} = y - r^{p*} - R^m/\gamma = \text{Min}[y - R^m/\gamma; (R^p - R^m)/\gamma] \quad (\text{E22})$$

The reservation utility set by M is thus equal to $\gamma r^{*m} + R^m$ or more simply: reelect iff $r^p \leq r^{*p}$. Aware of all of this, voters will choose their reservation utility. If the remaining resources, namely $y - r^{*m} - r^{*p}$ leave enough for the optimal provision of public good g^{*m} and g^{*p} voters will set:

$$\omega = y - g^{*m} - g^{*p} - r^{*p} - r^{*m} + H(g^{*m}) + U(g^{*p}) \quad (\text{E23})$$

otherwise, if not enough resources are left, voter will choose the best (smaller) amount of public good, with the available resources.

Comparative statics. If positive, provincial rents are higher in the indirect election case since: $y - R^p/\gamma > \alpha y - R^p/\gamma$; the more so, the lower is *alpha*, i.e. the relative strength of provincial leaders. In the second approach $y - R^p/\gamma > \alpha(1 - \hat{\tau}^m)y - R^p/\gamma$. As for municipal

rents, they may increase or decrease with indirect elections depending on the relative size of R^p and R^m . Finally, rents are higher the higher is γ , that is, as more of the diverted resources turn into benefits for the incumbent. One may speculate that indirect elections cause an increase in γ .