From Lapdogs to Watchdogs: Random Auditor Assignment and Municipal Fiscal Performance

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Abstract

A classic problem in public finance is the over-expenditure of local governments as a result of expectations of a bailout from higher-level administrations. While monitoring can in theory mitigate such agency problems, it can itself be rendered less effective as a result of corruptible auditors. In this paper, I evaluate whether shifting control over auditor appointments improves monitoring effectiveness and affects the financial health of local governments. In 2011, Italy switched from allowing mayors to appoint municipal auditors to a system of random assignment. My identification exploits the reform's staggered introduction across municipalities in a generalized difference-in-differences setting. I obtain four main findings. First, the reform significantly changes auditor-mayor matching, severing connections between municipal auditors and mayors. Second, treated municipalities increase their net surpluses by 9% and debt repayments by 8%, per national government objectives. Third, the improvement results from a sizeable increase in municipalities' tax revenue, rather than cuts in expenditures. Fourth, treatment effects are significantly larger for municipalities that were more likely to have a corruptible auditor before the reform, and for those that are randomly matched to a less connected auditor. Taken together, these findings highlight the value of auditor independence and illustrate how changes in the organizational design of the state can improve government performance. **JEL**: H72, D73, H83, H11, H71, H81, H77, M42

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1 Introduction

Local governments are amongst the most important providers of public goods and services, accounting on average for 25% percent of total government expenditures worldwide (OECD (2019)). While delegating responsibilities to subnational entities may bring decision-making closer to local interests, it also creates opportunities for abuse by local officials, who might put their own interests, and the ones of their immediate constituents, ahead of those of the country. Emblematic of this moral hazard problem is the debt accumulated by lower-level governments, in expectation of an eventual bailout from the central government. It is thus unsurprising that one of the longest-standing debates around decentralization is how to ensure local fiscal discipline. Frequently, decentralized governments have fiscal rules in place to limit spending (Kornai, Maskin and Roland (2003), Grembi, Nannicini and Troiano (2016), Carreri and Martinez (2021)). However, rules themselves are ineffective if they are not properly enforced.[1] A common solution is to put in place oversight institutions to enforce rules and watch over the behavior of local governments. This is naturally imperfect, however, as monitors themselves may be captured by those who they are intended to watch over, particularly if the monitor is hired by the local government itself, as is frequently the case. Understanding how to design effective monitoring institutions is thus central to models of decentralized organizations, and to promote their effective functioning. While discretion over appointments can have its benefits – reducing information frictions and allowing for a more flexible application of rules – it leads to inherent conflicts of interest.[2]

This paper examines the consequences of changes in the design of monitoring institutions for the fiscal discipline of local governments. I provide new evidence that removing discretion of monitors' appointments from local politicians significantly improves municipal financial health. To do so, I leverage a unique natural experiment, resulting from a reform to the system governing the appointment of auditors overseeing municipal governments' budgets in Italy. Italian municipalities are required to draft financial statements to be reviewed and approved by professional auditors, who act as an intermediate layer of oversight between municipalities and the central monitoring performed by the Italian National Court of Accounts

[1] Recent theoretical contributions have highlighted the importance of taking into account issues of credibility and limited enforcement when considering the effectiveness and optimality of fiscal rules (Halac and Yared (2019), Dovis and Kirpalani (2020)).

[2] As one example of a potential benefit, a locally-appointed auditor might have better information on whether to relax the rules in response to local economics shocks. A large theoretical literature has investigated the broader tradeoffs involved in delegating authority (Aghion and Tirole (1997)) and the optimal design of monitoring institutions (Strausz (1997), Ortner and Chassang (2018), Mookherjee and Tsumagari (2018)Prendergast (2002), Prendergast (2015), Prendergast (2003)). I provide an empirical test of how changes in the information asymmetries and bargaining power of the supervisor vis-a-vis the monitored agent affect monitoring quality.

(comparable to the Government Accountability Office in the U.S.). The audit system aims to ensure responsible spending practices, in compliance with national fiscal rules. It is worth emphasizing that their functions are broader than those of third-party auditors, who merely certify documents ex-post. Municipal auditors in Italy serve a dual role of both monitoring and advising: during their three-year terms, auditors are required to assist and oversee the entire budgetary process of municipalities and can thus influence municipal financial decisions ex-ante.

The main feature of the reform was a switch from a discretionary, "quasi-patronage" system, in which mayors could choose their auditors, to a random assignment mechanism, in order to strengthen oversight of local entities' budgets. This change was part of a larger package of austerity measures adopted in August 2011, at the peak of the sovereign debt crisis, in order to comply with the fiscal adjustment efforts required by the European Union. In Italy, as in many other countries, local entities cannot default on their debts, and deficits (or surpluses) run by local governments are consolidated in the national budget. Facing pressure to reduce the national debt and improve credibility on the global financial markets, local governments' fiscal health became increasingly important from the national government's perspective. Importantly, while all the other provisions went immediately into effect, the implementation of the new appointment system took place upon the expiration of the current auditor's term, which occurred at different dates across municipalities for historical reasons unrelated to the reform and beyond a mayor's control.

I exploit the staggered introduction of the new appointment system and apply a generalized difference-in-differences (DiD) methodology to identify the impact of the change in rules governing auditors' appointments on the financial health of municipal governments. I compare the outcomes of municipalities treated earlier ($Treatment$) and municipalities treated later ($Control$), before and after the arrival of an independently-appointed auditor. Under the identifying assumption that treatment timing is uncorrelated with the evolution over time of outcomes, this approach allows me to identify the effect of the change in the appointment system on the financial health of municipalities.[3] To address concerns raised by the recent econometric literature on staggered-adoption DiD designs, I adopt a stacked-

[3]Interpreting the post-reform changes in outcomes between early-treated and later-treated municipalities as the effect of changes in auditors' appointments requires the assumption that any pre-existing differences between these two groups of municipalities did not change after the reform and that any other provisions of the reform (which applied to all municipalities) affected early-treated and later-treated municipalities in the same way. In support of these assumptions, I perform a series of robustness checks. First, I provide evidence of the absence of differential selection into treatment timing across a wide range of municipal characteristics that might potentially affect outcomes. Second, I show that early-treated and later-treated municipalities are on parallel pre-trends with respect to all my outcome variables. Third, I demonstrate that the results are not affected by controlling for a wide range of time-varying observable factors and flexible non-parametric trends in pre-determined municipal characteristics.

by-event design (Cengiz, Dube, Lindner and Zipperer (2019), Deshpande and Li (2019)) which ensures that my treatment effects are estimated only based on comparisons of units switching into treatment to not-yet-treated units. As a robustness check, I also apply the alternative estimator proposed by de Chaisemartin and D'Haultfœuille (2020) and obtain nearly identical estimates.[4]

I document four sets of results. First, I study the effects of the reform on the selection and allocation of auditors. I show that the reform significantly changes auditor-mayor matching, severing connections between municipal auditors and mayors along several dimensions that are likely to affect auditors' independence. The share of auditors resident in the municipality they were hired to monitor declined from 31% before the reform to just 1% afterward. Similarly, the share of auditors reappointed for a second term dropped from 57% before the reform to 1%, and the percentage of those sharing the same surname of a local politician fell from 7% to 1%. The reform also induces a change in the composition of the auditors' pool: almost 18,000 new auditors joined the list, representing around 70% of the pool of auditors. While these new auditors have no municipal-specific experience, they look otherwise very similar to the existing set of incumbents on other observables. Most importantly, they are equally experienced in terms of general private-sector accounting.

Second, I show that, when subject to more external oversight, municipalities significantly improve their fiscal performance. Upon the arrival of a draft-appointed auditor, municipalities increase their expenditure in debt repayments by 9% and their net budget surplus by 8%.[5] The magnitude of the effects is comparable to that of Grembi et al. (2016), which analyzes the effect of relaxed fiscal rules in Italy in 2001, thus suggesting that draft-appointed auditors are more effective enforcers of fiscal rules. Inspecting the dynamics of the treatment effects, I find that they appear immediately upon the arrival of the (randomly assigned) auditor and remain essentially constant over the auditor's three-year term, suggesting a sudden but persistent shift in budgetary practices.[6]

Intuitively, to improve their fiscal stance, municipalities can either increase their revenues (e.g., by collecting more taxes) or cut spending (e.g., by cutting investments or current

[4]This ensures that results are not driven by the artificial increase in the number of observations that occurs when performing the stacked-by-event design.

[5]I also investigate the response of off-balance-sheet debt repayments, which represent a rare but critical indicator of potentially severe budget imbalances. While I do not detect any effect on average, significantly diverging patterns of treatment effects emerge when looking at heterogeneity tests based on the risk of auditors' collusion in the pre-reform setting, which collectively suggest that this outcome represents an important proxy for inappropriate practices.

[6]Furthermore, exploiting exogenous variation arising from non-overlapping electoral and audit cycles in the pre-reform period, I show that during years in which mayors had control of auditors' appointment, net surpluses and debt repayments are significantly lower. This suggests that lax monitoring by discretionally-appointed auditors allowed mayors to run higher deficits without being reported to the Court.

expenditures), or a combination of the two. My third result shows that municipalities do not cut spending, but instead improve their fiscal stance through revenue-based adjustment, specifically by increasing revenues from the local property tax by over 20%, an increase corresponding on average to €30 per capita.[7] I provide evidence that the increase in revenues comes from an expansion in the tax base rather than a change in tax rates, consistent with an improvement in the enforcement of tax collection upon the arrival of independent auditors. In line with this explanation, I show that the effects are stronger in municipalities that have a higher share of housing units that are undeclared, and where there was a higher share of buildings hidden from tax authorities, as documented in Casaburi and Troiano (2016). This result speaks to the literature on the determinants of fiscal capacity and is consistent with the findings of Xu (2018), which shows that patronage-appointed governors raised fewer taxes and invested less in the revenue capacity of British Colonies.[8]

Finally, I perform a series of heterogeneity tests with the objective of better understanding the set of mechanisms through which the reform most plausibly operates. I start by investigating how the treatment effect varies with proxies of pre-reform collusion risk. I first look at financial health, as lax or corrupt monitoring might allow for less responsible financial behavior and thus greater scope for improvement under active auditing. Consistent with this view, I find that the treatment effect of independent auditors is up to four times as large in municipalities with worse pre-reform financial conditions. I also consider more direct proxies of auditor-mayor collusion in the pre-reform period, including (a) an indicator for whether any auditor appointed before the reform was a local resident; and (b) a measure of corruption as captured by an indicator variable that flags municipalities where there was at least one investigation for corruption-related crimes during 2004-2013. For both measures, I find that the reform was more effective in places that had a higher risk of collusion.

I then show that treatment effects are driven by municipalities where the randomly assigned auditor is less likely to have previous connections and has less incentive to collude with the local mayor. When instead the random assignment reproduces a situation similar to the pre-reform one, I do not detect any change in outcomes. In particular, I look at

[7] This is likely to produce a larger negative fiscal multiplier than a counterfactual adjustment based on expenditure cuts at the macro level (Alesina and Ardagna (2013), Alesina, Favero and Giavazzi (2019)). On the other hand, given the rigidity of current spending, and the difficulty of rapidly achieving large savings only through improvements in spending efficiency, expenditure cuts would have resulted in investment reductions, negatively impacting public goods and public service provision.

[8] On the other hand, Balan, Bergeron, Tourek and Weigel (2020) find that delegating tax collection to local leaders, as opposed to state officers, improves tax collection capacity in the Democratic Republic of Congo. A key distinction is that in my context, the local-vs-central trade-off involves the appointment of monitors who check on the behavior of local leaders, who still retain the power of tax collection. Furthermore, the value of local information is potentially significantly higher in a context like the DRC, with respect to Italy.

whether the newly appointed auditor is an "outsider," as defined by either (a) having no prior experience as a municipal auditor; or (b) she is resident in a municipality that is at least one hour away from the audited one. Investigating heterogeneity along these margins allows me to speak about the trade-off between discretion and collusion, as more distant auditors or those with less public sector experience may also be less knowledgeable about the municipality and the appropriateness of financial practices. Both measures deliver a similar message: treatment effects are largely driven by municipalities that receive an auditor that is an outsider. The absence of treatment effects for municipalities that randomly happen to receive a local auditor is particularly intriguing, as it further sheds light on the strength of social ties and on how home bias might adversely affect bureaucratic performance (Xu, Bertrand and Burgess (2018), Chu, Fisman, Tan and Wang (2020)), even in the presence of random assignment.

Lastly, I show that the effect of the reform is stronger whenever local interests are less aligned with national ones. To proxy for this, I investigate the interplay between auditor's appointment and electoral accountability. On the one hand, we might expect that mayors subject to strong reelection pressures would have stronger incentives to perform well, restraining from inappropriate or wasteful financial practices. Alternatively, local interests may frequently clash with national ones, particularly if the national government's push toward fiscal sustainability comes at the cost of cutting expenditures for local services or taxes for local citizens. I investigate the interplay with electoral accountability along two different margins. First, I exploit the fact that, for historical reasons, municipalities are on different electoral cycles that do not overlap with the audit cycles.[9] I thus compare the treatment effects in municipalities that receive a randomly appointed auditor for the first time in the first part of the electoral cycle to municipalities that are treated when mayors are closer to elections. Second, I exploit the existence of a two-term limit for Italian mayors to explore whether mayors who can be re-elected respond differently than mayors who are in their second and final term. Third, I look at the relative strength of the city council opposition. I find similar results along all of these margins, suggesting that the response was somewhat stronger in municipalities where mayors faced lower reelection pressures. These results also provide suggestive evidence that the adjustment was costly for mayors, and not necessarily in line with local constituents' interests.

Collectively, my results show that random assignment of auditors significantly improves the financial health of municipalities, thus emphasizing the value of independence in achieving effective oversight and enforcing the central government's objectives. Furthermore, heterogeneity analyses provide suggestive evidence that the effects operate via breaking up collusive

[9]The electoral term of Italian mayors is 5 years while the auditor term is 3 years.

ties between auditors and mayors, thus fostering more effective monitoring.

This paper contributes to the literatures that investigate the organization of the state, the tradeoffs involved in decentralized organizations, and the role of monitoring as a tool to solve agency problems. While several papers have documented the effects of audit findings on corruption and accountability (Olken (2007a), Ferraz and Finan (2011), Avis, Ferraz and Finan (2018), Bobonis, Fuertes and Schwabe (2016)), taking audit quality as given, very little is known about the determinants of monitoring effectiveness. In their seminal paper, Duflo, Greenstone, Pande and Ryan (2013) analyze the effect of randomly assigning third-party environmental auditors to 236 industrial plants in India. My work builds on their findings by providing evidence on the effect of removing discretion in the selection of auditors in a public sector setting, in which the oversight problem is complicated by political economy considerations, and for a different type of audit regime.[10] Furthermore, my analysis evaluates the consequences of a nationwide reform involving over 6,000 municipalities, thus corroborating the external validity of Duflo et al. (2013)'s findings outside of a randomized controlled setting and in a developed country.[11]

By analyzing the effects of independent monitoring in the context of local governments, this paper also speaks to a large literature in public finance that has highlighted the tradeoffs resulting from increased delegation to local governments (Bardhan and Mookherjee (2006)). In particular, while a number of papers document the use of fiscal rules in decentralized entities, their impacts on policy outcomes (Poterba (1996), Grembi et al. (2016)), as well as the heterogeneity in countries' rules abidance (Eyraud, Debrun, Hodge, Lledo and Pattillo (2018)), there is scant empirical evidence regarding the determinants of fiscal rules' effectiveness. This paper fills this gap by documenting the role played by independent auditors in improving enforcement.[12]

[10]Distinct from third-party auditors examined by Duflo et al. (2013), who simply annually verify firms' compliance with regulation, and whose reports can be easily back-checked, municipal auditors are hired for longer terms and can influence municipal budget decisions both ex-ante and ex-post. The wider scope of their mission also implies higher margins of discretion, making collusion problems more likely to arise, and introducing a trade-off between bias and information.

[11]In a concurrent working paper, Barone, Conti, Narciso and Tonello (2020) similarly use a difference-in-differences approach to evaluate the effects of the reform on a slightly different set of outcomes. In contrast to my findings, they find that independent audits are associated with a worsening of municipal public finances. This difference may be due to differences in the estimation sample (which, in Barone et al. (2020), includes only municipalities in special regions), the definition of the treatment variable (which depends on the number of months an auditor has been in place) and the empirical design (a two-way fixed effect model, potentially leading to bias from negative weighting problems (Goodman-Bacon (2021)). In Appendix Table A3 I replicate the main results of Barone et al. (2020) and conclude that the differences are driven by the inclusion of special regions in the estimation sample.

[12]In the Italian context, several papers have looked at the consequences of fiscal rules for local public spending (Grembi et al. (2016)), firms' dynamics (Coviello, Marino, Nannicini, Persico et al. (2017)), corruption (Daniele and Giommoni (2020)) or electoral outcomes (Gamalerio (2020)). These studies all exploit

More generally, this paper speaks to the growing literature on the selection, allocation, and incentives of personnel in the public sector and their implications for state performance (Finan, Olken and Pande (2017), Besley, Burgess, Khan and Xu (2021)). My paper makes two distinct contributions. First, I study a quasi-experimental intervention that allows me to analyze the distinct roles of selection and matching, as well as their interplay, in affecting key dimensions of government performance, such as fiscal capacity. Previous papers have separately documented the adverse effects of patronage for the selection of elite and lower-level bureaucrats (Xu (2018),Colonnelli, Prem and Teso (2020)), or the effects of the allocation of socially-tied bureaucrats across localities (Xu et al. (2018), Chu et al. (2020)), holding selection constant. Finally, my work relates to Bandiera, Best, Khan and Prat (2021), who investigate delegation in procurement contracting and show that shifting decision-making rights from monitors to officers improves outcomes. My findings are distinct but complementary, in that I focus on the causal effect of a shift in the control of the appointment of monitors, rather than in procurement decision-making power. Second, my paper is the first paper to focus on agents that are at the intersection of the private and public sectors, as independent consultants that work for the public sector but are not bureaucrats.

Lastly, a large body of literature in accounting and corporate governance has investigated the challenges posed by the conflicts of interest that arise in the private sector, as a result of audited firms selecting and paying external auditors (Moore, Tetlock, Tanlu and Bazerman (2006), Lennox and Wu (2018)). As a partial solution to the problem, the U.S. introduced time limits and mandatory audit-firm rotations, but the effectiveness of these measures is compromised by the high level of concentration of the private-sector market of auditors (Gipper, Hail and Leuz (2020), Gipper, Leuz and Maffett (2019)).While focusing on the public sector, this paper provides evidence of how mandatory rotation, if combined with competition in the market for auditors, might be an effective solution to curb conflicts-of-interests and improve audit effectiveness.

2 Institutional Context

2.1 Mayors and The Municipal Budget

Italy is a highly decentralized democracy, with 3 levels of subnational governments: regions, provinces, and municipalities. Each of Italy's 8000 municipalities has its local government composed of an elected mayor (Sindaco), an executive body (Giunta) appointed by the

differences in the applicability or tightness of rules but take their enforcement for granted. In this sense, my paper contributes to the understanding of the mechanisms operating behind the observed effects of fiscal rules on economic outcomes.

mayor, and an elected council (Consiglio Comunale). The mayor is directly elected for a 5-year mandate with a 2-term limit for consecutive terms, holds executive power at the municipal level, and is responsible for the administration of the local government. Municipalities are granted large autonomy and manage around 8% of total public expenditure (over €55 billion). In particular, since 1993, increased decentralization allowed municipalities to have full control of a wide range of essential public services: environment protection and waste management, social services to elderly and disabled persons, childcare and nursery schools, school-related services (school meals and transportation), local police, maintenance of municipal roads, management of civil registries, town planning, culture, recreation, and economic development. Current expenditure is financed by municipal fiscal revenues (87%) plus transfers from the central government (13%), while borrowing is allowed only to finance investment expenditures and is subject to strict quantitative limits (Grembi et al. (2016), Bonfatti and Forni (2019),Chiades and Mengotto (2015)).[13] Fiscal revenues come from two main sources: (1) local taxes, among which the most relevant are the property tax and the local income tax surcharge; (2) local fees related to building permits, traffic fines, and other services. One of the main responsibilities of each mayor is to propose the annual provisional budget and final budget to the municipal council that approves them with majority rule. The mayor enjoys a substantial amount of executive power and discretion over budget allocations, as well as over the components of municipal revenues.

2.2 Fiscal Monitoring

From 1999 onwards[14], all Italian sub-national entities are subject to the so-called "Domestic Stability Pact" (DSP), the national counterpart of the European Union's Stability and Growth Pact, adopted in 1997. The pact prescribes a set of fiscal rules which has undergone several changes over time, but which generally requires municipalities to run a balanced budget and limit their net surplus below a given threshold.[15] It is important to note that

[13] In particular, municipalities can undertake new debt only if the total amount of debt service paid on past debt and new debt does not exceed 15% of current revenues of the two preceding years. While in theory they can borrow from private banks, the vast majority of debt is granted by the so-called "Cassa Depositi & Presiti", a state-owned body that operates to promote national and local governments' investment projects. De facto, therefore, most of the municipal debt is guaranteed directly by the national government.

[14] *Legge Finanziaria* 23 December 1998, n. 448, substantially modified by the subsequent *Legge Finanziaria* 23 December 2000, n. 388.

[15] From 2017 onwards, DSP has been replaced by the "zero-deficit rule", which became part of the Italian constitution in 2012 and is now applied both at the national and the local level. Under the new rule, local entities are required to reach a zero-deficit rule on an accrual basis. The old DSP rule instead prescribed a zero-deficit rule on a mixed basis: accrual basis for current revenues and expenditures, cash basis for capital revenues, and expenditures. Thus, the current rule relaxes constraints on capital expenditure, allowing local

in Italy, and in general in many countries in the European Union, local entities cannot default on their debts, deficits (or surpluses) run by local governments are consolidated in the national budget, and the bailout of sub-national entities is formally regulated by law.[16] Therefore, especially during the economic crisis, the national government has frequently made use of the pact to shift part of the burden of national debt reduction, required by the European Union, towards local entities. For example, between 2010 and 2017, the national government cut resources for local governments by over €12 billion: partly via a reduction of inter-governmental transfers (9 billion), and partly through a tightening of the rules of the pact (3.3 billion). In this context, enforcing the respect of fiscal rules and ensuring the fiscal sustainability of local governments became critical for the national government, motivating a surge in attention to fiscal monitoring procedures.

2.3 The Role of Municipal Auditors

Since 1990, all municipal budgets need to be audited and certified by a board of auditors.[17] The board is composed of one member for municipalities with less than 15,000 inhabitants, and three members for larger municipalities. The board is nominated with a majority vote of the city council[18], for a 3-year term, renewable for an extra term. The board cannot be dismissed for any reason unless the council can prove faulty inaction or breach of official duties. Each of the board members can have at most eight contemporaneous appointments.[19] Auditors' compensation is set by the city council at the moment of appointment.

From its very first inception, the board of auditors was assigned a double role of both monitoring and advising. The original law instituting the board of auditors describes its role as follows: *The board of auditors collaborates with the municipal council in its control and governing function, supervises the accounting and financial regularity of the management of the entity and certifies the correspondence of the balance sheet to the economic outcomes, drawing up a specific report, which accompanies the proposed board resolution of the balance*

authorities who have margins of liquidity, in an accrual sense, to make investment decisions. Finally, under the old rules, municipalities were required to run a strictly positive surplus and contribute to the reduction of national debt with their surpluses, while under the current rule they need to run balanced budgets, i.e. they can have zero surpluses. For further details, see Grembi et al. (2016).

[16] In the OECD, in 2013, sub-central government debt represented on average 17% of total public debt. See `https://www.oecd-ilibrary.org/economics/national-accounts-at-a-glance-2015_na_glance-2015-en`.

[17] *Law 142/1990, Article 57.*

[18] In practice, given that a minority vote would cause the resolution of the local government, the choice of the auditor is directly made by the mayor.

[19] Further restrictions are imposed depending on size of the audited municipalities: at most 4 contemporaneous appointments in municipalities up to 5000 inhabitants, 3 for municipalities between 5,000 and 100,000, 1 for those above 100,000.

sheet. The auditors shall provide advice and proposals aimed at achieving better efficiency, productivity, and cost-effectiveness in local government management. [20]. The tasks of auditors have progressively been detailed and expanded over time, in particular in the years 2000 and 2003 [21]. Appendix Table B1 provides a summary list of the auditors' main tasks, as indicated in the law. Importantly, and differently from the typical auditor in a private sector context, municipal auditors are not simply auditing and signing off the balance sheets ex-post, but they have a significant role in influencing policy-making ex-ante. According to the National Association of Municipal Auditors (ANCREL), auditors need to perform over 200 tasks every year, summing up advising and supervising ones.[22]. For example, in his advising role, the auditor is required to provide written opinions on several important acts, including municipal protocols and regulations about tax collection. The auditor is also charged with verifying the accuracy of the information reported in the budget/balance sheet and it should also provide advice on how to improve efficacy, efficiency, and cost-effectiveness in the management of public funds. Last but not least, auditors are required to verify the respect of fiscal rules. The results of the audit review, as well as the set of suggestions, should be included in an audit report that is reviewed by the council at the moment of final approval of the budget/balance sheet. After the approval, the auditor is required to fill in a detailed questionnaire and to transmit all of the relevant documents, including the approved budget and the audit review, to the *National Court of Accounts*, which is the central monitor. The Court is in charge of reviewing all the material transmitted by the auditors and can decide to start a judicial procedure against municipalities that failed to comply with the rules or which displayed improper financial practices. On average, the Court emits around 2000 judicial acts of ex-post verifications per year (*Banca Dati della Corte dei Conti della Giurisdizione e del Controllo* (2022)). Most of these pertain to recommendations and other preliminary acts that warn municipal governments and invite them to take corrective actions within 60 days, while the number of subsequent judicial prosecutions is relatively limited to an average of 300 cases per year.

While it was originally instituted with the scope of providing a form of surveillance over municipal financial practices, the effective action of the board of auditors has been fairly limited for many years, consisting only in a "formal check" of the compliance with budget laws.[23]

[20] *Law 142/1990, Article 57.*

[21] *art. 239, TUEL, law, and d.p.r. n. 97/2003, attachment 17*

[22] `www.ancrel.it`

[23] Based on personal interviews with judges of the National Court of Accounts, the poor quality of monitoring provided by politically-appointed auditors was a diffused and well-known practice.

2.3.1 Introducing Random Assignment of Auditors

In August 2011, in the midst of the European Sovereign Debt Crisis, the national government suddenly changed the mechanism of auditors' appointment (*Law 148/2011*).[24] According to the new law, *"Starting from the first renewal after the adoption of the law, financial auditors of local entities will be chosen by a random draw from a list in which the following subjects can ask to be included: a) those currently included in the regional list of auditors, b) any officially authorized Certified Public Accountant".* In February 2012, the government outlined the procedures to participate in the list, including experience requirements varying with population thresholds, as follows:[25]

- Up to 4,999 inhabitants: auditors shall have been certified public accountants for at least 2 years
- Between 5000 and 14,999 inhabitants: auditors shall have been certified public accountants for at least 5 years and have been appointed as municipal auditors at least once before.
- 15,000 inhabitants and above: auditors shall have been certified public accountants for at least 10 years and have been appointed as municipal auditors at least twice before.

Due to administrative constraints and the time needed to form the public lists, the new drafting procedure entered into effect on December 10, 2012.The reform does not apply to the 5 "Special Status" Regions of Italy (Valle d'Aosta, Trentino Alto Adige, Friuli Venezia Giulia, Sardinia, and Sicily), which are granted extra-autonomy along many margins, including regulation of fiscal monitoring procedures.[26] My analysis is therefore limited to the sample of 6627 municipalities in the 15 "Ordinary" regions.

[24]The reform was adopted in the form of a Law Decree, which is a special law adopted in the context of urgency, with immediate effectiveness.

[25]Note that, before the reform, there was no increasing experience requirement to become an auditor in large municipalities, except for being a CPA. The inclusion of gradual experience requirements was mainly a result of a bargaining process with the existing auditors, worried about protecting their interests against outsiders.

[26]In 2016, Friuli Venezia Giulia, Sardinia, and Sicily adopted laws introducing drafting procedures that are similar to the national one. In Friuli Venezia Giulia, $3 \times N$ auditors were drafted and the municipality was left with some discretion in choosing among the drafted candidates. In Sicily, the adoption of the drafting procedure was spurred by the pressure of the Regional Council of Auditors, which repeatedly denounced political pressures and asked for a reform that would align the local regulations to the national ones and guarantee their independence. The existing system has however been strongly criticized as, in contrast to the national system, the drafting in Sicily is carried out directly by the City Council, which exposes it to a higher risk of illicit acts.

The random drafting procedure is carried out by the local prefectures[27], via a standardized, computer-based system provided by the Ministry of the Interior. Municipalities are required by law to inform their local prefecture at least 2 months before the current auditor's term expires.[28] For each draft, the number of drafted candidates is equal to $3 \times N$, where N is the number of auditors to be appointed. For over 90 percent of municipalities, those with a population up to 15,000 inhabitants, three potential candidates are drafted. The prefecture immediately informs the municipality of the draft results, and the municipal council shall proceed with the appointment of the first drafted candidate(s), after having checked that the drafted auditor is eligible for the specific appointment.[29] The formal appointment notice also contains the wage offered to the auditor for the three-year mandate. Auditors' compensation is subject to nationally-mandated population-based minimum and maximum caps, leaving mayors very limited margins of discretion in setting the exact compensation within this small bandwidth. This aspect of the auditors' appointment was not affected by the reform.[30] Shall the first amongst the drafted auditors refuse the appointment or be deemed to be incompatible, the municipality proceeds to contact the second-drafted auditor. Thus, mayors cannot choose amongst the set of drafted auditors but have to proceed following the order of the draft.[31]

[27]Prefectures are the local offices of the Ministry of the Interior. They officially represent the National Government in each Province. There are 103 prefectures in total.

[28]In case of an early auditor's resignation, the municipality is required to inform the prefecture within 3 days.

[29]Causes of ineligibility include: a) being currently a member (or having been a member in the past 2 years) of the municipal council or municipal executive body; b) being an employee of the municipal bureaucracy or of any of the local bureaucracies (region, province, municipal union) to which the municipality belongs; c) having already reached the maximum number of concurrent appointments (8). In this latter case, however, the auditor may decide to resign from one of the existing appointments in order to accept the new one.

[30]In practice, however, there is very little variation in the compensation offered. The municipal council is not required by law to contact the auditors before the appointment, but it is very common for municipalities to informally reach out to the first candidate and check whether she would accept the appointment at the offered wage. Auditors can refuse the appointment without having to provide any formal justification, and thus they could refuse the appointment if they found the wage offer inappropriate. However, the absence of an appointed auditor makes it impossible for the municipality to adopt any official financial document, and this rules out the possibility for mayors to strategically offer excessively low wages so as to force auditors to reject the appointment.

[31]The data shows that rejections are a relatively rare occurrence: around 20% of first-drafted auditors in total reject the appointment, and in most of the cases the second amongst the drafted auditors accepts the appointment. In less than 3% of municipalities, a new draw was needed as none of the candidates accepted the position. Appendix Table A5 investigates the determinants of auditors' rejections. The most significant predictor is the distance between the municipality of residence and the municipality of appointment. All other characteristics have very limited explanatory power. Importantly, rejections are uncorrelated with indicators of the financial performance of municipalities.

3 Data

3.1 Municipal Financial Accounts

To measure financial outcomes, I use detailed data on all municipal budgets provided by the Italian Ministry of the Interior. These are the so-called "final balance sheets", reporting effective revenues and expenses for the previous year, which need to be presented and approved by April 30th of the following year. The data include detailed information about municipal governments' spending and revenue sources, such as local taxes, current expenditures, investments, debts, and transfers.

3.1.1 Indicators of Fiscal Sustainability

For the sake of my analysis, I want to inspect whether independent monitoring improves the fiscal sustainability of municipalities. I, therefore, focus on a set of indicators that are used by the national government and/or by the National Court of Accounts to monitor the fiscal stance of local governments. First, I look at the $NetSurplus$, which is calculated as total revenues, net of transfers from other levels of governments, minus total expenditure, net of interest payments on outstanding debt. The $NetSurplus$ is a clean measure of the actual fiscal sustainability of municipal finances, as it nets out factors that are not under the direct control of the current local administration, such as changes in the level of transfers from higher levels of government or the consistency of interests on debts undertaken by previous administrations. Furthermore, this variable is the main target of the national government's fiscal rules (Grembi et al. (2016)).

Second, I look at $DebtRepayments$, capturing the overall amount of municipal finances devoted to reducing the stock of existing debt. This variable is also closely monitored, as municipal debt is part of the national debt.[32]

Third, I look at $OBSDebtRepayments$, representing the total amount of Off-Balance-Sheet (OBS) Debt that is recognized and re-payed by municipalities in a given year. This is the most critical variable among the four, as it signals situations of potential profound imbalances. The National Courts of Account defines as OBS Debt any liability that is undertaken in violation of the municipal budget rules. In particular, any expense that was not previously authorized and accounted for in the provisional budget constitutes an OBS liability. The municipal budget law then legislates which types of OBS liability can be recognized and should

[32]In the last 10 years, the national government has pushed municipalities to repay as quickly as possible their outstanding debts, especially those that were undertaken in the past, as they were subject to very high interest compared to the present close-to-zero rates.

be included in the balance sheet. These include, for example, unexpected expenses incurred for emergency interventions or other expenses motivated by local public necessities, but it also includes expenses arising from recapitalizing SOEs (state-owned enterprises) and other entities that deliver local public goods and services. Importantly, in theory, any of those unexpected expenses occurring, for example, between December 31, 2011 (the last day to approve the provisional budget for the fiscal year 2012) and December 31, 2012, should be included as OBS debt in the 2012 final balance sheet. However, municipalities are also allowed to recognize ex-post OBS liabilities that arose in previous years and had not been previously recognized.[33] Municipal bureaucrats that approved unexpected expenses that count as OBS liabilities but cannot be recognized in the balance sheet remain liable for those expenses. In other words, they should pay out of their pockets for those expenses. Any OBS debt that is recognized should be fully repaid in the current year or, if impossible, paid in installments within the following two years. To finance $OBSDebtRepayments$, municipalities may employ any existing current surplus and, residually, they may undertake new debt, provided they justify the impossibility of financing $OBSDebtRepayments$ with their own resources.

Auditors are explicitly asked by the Court to closely monitor the presence of OBS debts and the process of recognition, both ex-ante and ex-post. Ex-ante, the auditor is required to review the existence of OBS debts and give an opinion about the Council act that officially recognizes OBS debts. At this moment, the auditor is required to: (a) review the reasons why the OBS debts arose in the first place and evaluate if it falls in the categories that can be recognized; (b) evaluate the proposed repayment methods. Ex-post, the auditors are required to report about $OBSDebtRepayments$ in a specific section of the questionnaire they need to complete for the Court after the balance sheet has been approved. Given that they represent an exceptional violation of the rules, $OBSDebtRepayments$ are a relatively rare phenomenon, and this variable is equal to 0 for over 80% of the sample.

All variables are measured in per-capita terms (winsorized at the 1%), expressed in 2018 euros equivalent, and reflect accrual accounting.[34] All spending variables, which are always greater than 0, are transformed using the inverse hyperbolic sine transformation. Table 1 shows the summary statistics of all the outcome variables used in the analysis.

[33]Unfortunately, I am not able to distinguish between these two cases in my data.

[34]Italian municipal accounting has a parallel accrual and cash accounting. Accrual accounting refers to the revenues and expenditures that pertain to the fiscal year t. However, not all revenues (expenditures) are effectively collected (paid) in the current year, so cash accounting might differ from accrual accounting. Cash accounting includes both revenues and expenditures that refer to the fiscal year t and are effectively collected/paid in t, as well as the so-called residual cash accounting, referring to revenues/expenditures that are collected/paid in year t but were adopted in terms of accrual accounting in previous years. Accrual accounting is the most relevant reflection of the decisions made by the current administration, and therefore it is the focus of the present analysis.

3.2 Municipal Auditors

The key treatment assignment variable for my analysis is the date when each municipality is assigned an independent auditor for the first time. All information about currently draft-appointed as well as previously draft-appointed auditors is available on the website of the Ministry of the Interior. However, information is reported separately for each municipality, so I created a complete historical database for the universe of municipalities by scraping the website. I obtained a dataset containing information on the draft dates and identities of all the auditors drafted from 2012 onwards. For each case, the number of drafted candidates is equal to $3 \times N$, where N is the number of auditors to be appointed. For the vast majority of municipalities, those with a population of up to 15,000 inhabitants, three potential candidates are drafted. I matched this database with another one containing information on the appointed auditors, in order to check if the first person drafted accepted the appointment or if the supplementary candidates were appointed. Finally, I combine together information from all the public lists of potential auditors. The list contains information about candidates' characteristics, such as age, gender, municipality of birth, municipality of residence, region of residence, level of professional experience (proxied by the number of years they have been certified as public accountants), and information on the previous service as an auditor. In particular, because of the increasing experience requirements, auditors who want to compete for larger municipalities have to prove that they have served as auditors in the past. This allows me to overcome, at least partially, one important data limitation, namely the fact that I don't have information about auditors' appointments for the pre-reform period. By exploiting the information reported by auditors when they sign up for the lists, I am able to recover the identity of the last auditor appointed before the reform for over 40% percent of my sample.[35]

3.3 Other Municipal Characteristics

I supplement the above information with further data to account for municipal characteristics that might affect fiscal sustainability.

First, I use detailed data on the universe of municipal elections, including information about election dates, and results as well as information about the mayor's characteristics (gender, age, and education). Local electoral data allows me to control for election cycle fixed effects as well as the mayor's term.

Second, I include data on the universe of municipal council dismissals, which have been

[35] I have information for those municipalities that appointed auditors who: a) decide to participate in the drafting procedure, b) want to participate in drafts for larger municipalities.

put together by a non-profit research entity, OpenPolis, by digitizing each of the dismissals' decree. Italian municipal councils can be dismissed for a number of different reasons, the predominant one being mafia infiltration, but also resignation and/or incompatibility of the mayor, and, most importantly, failure to approve the budget. Council dismissals have proven to affect economic activity (Acconcia, Corsetti and Simonelli (2014)), and this is likely to be reflected in the municipal budgets. All of my results control for council dismissals and are robust to the exclusion of dismissed municipalities (165 municipalities, which amount to less than 3% of my sample).

Finally, I use data on municipal geographic and socio-demographic characteristics from the Italian Statistical Census (ISTAT).

3.4 Sample Construction

My analysis is based on the 6,627 municipalities located in Ordinary regions. In order to keep a balanced panel during the analysis period, I exclude all those municipalities that were dissolved or newly created between 2008 and 2015. Furthermore, I exclude 265 municipalities that were affected by earthquakes during the analysis period, as they were granted a number of fiscal exceptions following the catastrophic events. I also exclude 341 municipalities that voluntarily participated in the experimental phase, between 2013 and 2015, of the so-called "harmonized accounting system" that became binding from 2016 onwards. This new system significantly changed both the structure of the financial reports as well as the set of fiscal rules. The final sample consists of 5603 municipalities observed between 2007 and 2015.

4 Empirical Strategy

My identification strategy exploits the staggered introduction of the reform across municipalities. As explained in Section 2.3.1, while the auditors' appointment reform was adopted in August 2011, the effective treatment date varies across municipalities depending on the expiration date of the current auditor's term, which has a total fixed duration of three years. For example, municipalities that appointed auditors in August 2011, just before the adoption of the reform, would be "treated" with a draft-appointed auditor only from September 2014 onwards. A key factor for my identification is that there exists wide variation in the auditor's appointment date before the reform due to historical reasons, which are uncorrelated with other municipal characteristics.[36] Because municipalities had appointed auditors at different

[36] A concern would arise if mayors could differentially select into treatment, for example by manipulating the auditors' appointment date in order to delay the entry into effect of the reform. However, mayors cannot affect the auditors' appointment date or auditors' term length in any way, thus selection into treatment

points in time, treatment timing is plausibly exogenous. My design, therefore, compares municipalities that are treated earlier to municipalities that are treated later. Figures 1 and A1 display the sources of identifying variation. Figure 1 shows the total number of municipalities with independently-appointed auditors over time, while Figure A1 displays their geographic distribution.

The most standard specification would be the following:

$$Y_{mt} = \alpha_m + \delta_t + \beta Treated_{mt} + X_{mt} + \epsilon_{mt} \tag{1}$$

Where Y_{mt} is one of the possible outcomes measured at the municipal level, $Treated_{mt}$ is an indicator variable that is equal to 1 if an independent auditor is active in year t, X_{mt} is a matrix of time-varying controls, α_m are municipality fixed effects. However, naively applying this specification would pose a set of empirical challenges that have been recently highlighted by a growing literature on the pitfalls of two-way fixed effects estimators with staggered adoption (Goodman-Bacon (2021), de Chaisemartin and D'Haultfœuille (2020), Borusyak and Jaravel (2017)). In particular, the coefficient β from equation 1 is a weighted average of all the possible 2x2 comparisons in my sample. Therefore, it is also estimated using comparisons among already-treated units and not-yet-treated units, where the already-treated units serve as controls. This induces a bias in the presence of heterogeneous treatment effects across groups experiencing treatment at different points in time. In the absence of a never-treated group, a straightforward solution is to limit attention only to comparisons between treated and not-yet-treated units, where only the not-yet-treated units serve as controls. To do so, there are two alternative options. One way is to use the last treated cohort as control (Abraham and Sun (2020)). Alternatively, and more flexibly, I am going to use a "rolling control group", by constructing my estimation dataset as follows. First, I create a separate dataset for each of the 3 treatment waves before the last one (2012, 2013, 2014). In each of these datasets, municipalities that receive the randomly-drafted auditor in that year are considered treated, while municipalities that will experience the treatment in later years serve as a control.[37] Second, in every dataset, I create event-time dummies relative to the year of treatment.[38] Note that municipalities that experience treatment in

timing is unlikely to be a concern.

[37]This approach is very similar to the one used by Deshpande and Li (2019). The main difference between mine and their approach is the fact that they keep a balanced post-period window, and thus include as controls only units that experience treatment later than the post-treatment window, which in their case is set to be 2 years. In my case, I am interested in investigating the effect for up to 2 years into treatment. Thus, applying their restriction would be equivalent to restricting the control group to only the last cohort (2015). Instead, I decide to use an unbalanced sample with a rolling control group to maximize power. Results are qualitatively and quantitatively similar across the two specifications and are available upon request.

[38]For example, in the dataset for the first cohort of treatment, event-time dummies are defined in time

the last year, 2015, serve only as controls, as by 2015 everyone is treated so they would not have a "good" control group in the sample. For the same reason, all the observations from the calendar year 2015 are excluded from estimation. The resulting dataset has 3410 treated municipalities and a total of 114,028 municipality-year observations. My main estimating equation then becomes:

$$Y_{mt} = \alpha_m + \delta_t + \beta_0 Treated_{mc} + \beta_{DD} Treated_{mc} \times Post_{mt} + \sum_{k=-7}^{k=2} \beta_k * D^k + X'_{mt}\zeta + \epsilon_{mt} \quad (2)$$

Where $Treated_{mc}$ is a dummy that takes the value of 1 if the municipality m is a treated municipality in the cohort c. This variable is not collinear with the municipality fixed effect as, given the data structure, the same municipality can appear multiple times both as treated and as control;[39] $Post_{mt}$ is a dummy equal to 1 for the years in which an Independent auditor is active, while the D^k are a set of relative event-time dummies, that take the value of 1 if year t is k periods after (or before, if k is negative) the treatment. The inclusion of these event-time dummies allows me to control for event-time trends that are not captured by the calendar year fixed effects δ_t. Standard errors are clustered at the municipality level, allowing for serial correlation over time (Bertrand, Duflo and Mullainathan (2004)). This level of clustering also accounts for the repeated appearance of municipalities in the datasets as both treatment and control units.

To investigate pre-trends, as well as the dynamic evolution of the treatment effect, I also estimate a non-parametric event-study specification:

$$Y_{mt} = \alpha_m + \delta_t + \beta_0 Treated_{mc} + \sum_{k=-7}^{k=2} \gamma_k * D^k \times Treated_{mc} + \sum_{k=-7}^{k=2} \beta_k * D^k + X'_{mt}\zeta + \epsilon_{mt} \quad (3)$$

In this specification, the coefficients of interest are the γ_k's, measuring the change in outcomes of treated municipalities k years after treatment, relative to the pre-treatment year, relative to the change in outcomes of control municipalities, who have yet to be treated. I estimate treatment effects up to three periods from treatment onset (i.e. $k = 2$) thus covering the entire term of the draft-appointed auditor.[40] The matrix of time-varying controls X_{mt} includes

relative to 2012.

[39]For example, a municipality treated in 2013 would appear as a control municipality for the cohort of 2012 and as treated for the cohort of 2013.

[40]Given the structure of my data and the absence of a never-treated group, the coefficients of different relative $event-time$ periods are estimated out of different groups. For example, the coefficient γ_2 is estimated only using the comparison of the change in the outcomes between 2011 and 2014 of the municipalities treated in 2012 and the municipalities that will be treated in 2015. The coefficient γ_1, instead, is a weighted average

population-bins-time-year and election-cycle fixed effects, a dummy for whether the mayor is term-limited, a dummy for whether the mayor is from the municipality, the gender and age of the mayor at the time of election (measured in logs) and a dummy for the mayor's anticipated resignation.

4.1 Identifying Assumptions and Threats to Identification

The key identifying assumption of my design is the absence of differential trends between municipalities experiencing treatment at different points in time. Given the exogenous and historical nature of the difference in audit cycles across municipalities, the timing of expiration of the existing auditor, and thus the timing of treatment, should be uncorrelated with the evolution of outcomes over time. To provide suggestive evidence in favor of this assumption, I first look at whether any observable characteristics of municipalities consistently predict the timing of treatment. To do so, I perform a regression of indicators for the four cohorts of treatment (2012, 2013, 2014, and 2015) on a set of characteristics measured in 2010. Results are displayed in Table 2. The Table shows that the only characteristics that significantly predict treatment timing across different columns are population-size dummies. In all of my analyses, I will therefore control non-parametrically for the presence of differential trends by population size, by including population-by-year fixed effects. I construct a set of dummies for the following population bins: 0 to 1,000 inhabitants; 1,000 to 5,000 inhabitants; 5,000 to 10,000 inhabitants; 10,000 to 15,000 inhabitants; 15,000 to 20,000 inhabitants; 20,000 to 60,000 inhabitants and above 60,000 inhabitants. I then interact each category with year fixed-effects.[41]. This means that the estimates are obtained by comparing municipalities of similar size, which are treated earlier versus later. These controls also allow me to account for potential effects induced by changes in the composition of the pool of auditors for municipalities of different sizes due to the minimum experience requirements for municipalities above 5,000 and 15,000 inhabitants. Importantly, the presence of significant

of two comparisons: a) the change in the outcomes between 2011 and 2013 of municipalities treated in 2012, compared to the change between 2011 and 2013 of municipalities treated in 2014 and 2015; b) the change in the outcomes between 2012 and 2014 of municipalities treated in 2013, compared to the change between 2012 and 2014 of municipalities treated in 2015. Finally, the coefficient γ_0 is a weighted average of 3 effects: a) the change in the outcomes between 2011 and 2012 of municipalities treated in 2012, compared to the change between 2011 and 2012 of municipalities treated in 2013, 2014, and 2015; b) the change in the outcomes between 2012 and 2013 of municipalities treated in 2013, compared to the change between 2012 and 2013 of municipalities treated in 2014 and 2015; c) the change in the outcomes between 2013 and 2014 of municipalities treated in 2014, compared to the change between 2013 and 2014 of municipalities treated in 2015.

[41] In the construction of the bins, I keep the population fixed at the level of 2011. The categories reflect the ones used by the Italian Statistical Office and other governmental institutions to classify municipalities along population size.

coefficients should not be interpreted as evidence of differential selection of municipalities across cohorts, as municipalities cannot manipulate in any way the expiration date of their current auditor, which is the only variable determining the assignment across cohorts. Results simply suggest that, for historical reasons, larger municipalities are slightly more likely to have their auditors' terms expiring in 2012 or 2013, as opposed to later years.

Second, I look at whether municipalities treated at different times display any significant differences in the levels of outcomes in the pre-reform period. In particular, the first four columns of Table 3 show average levels of outcomes by treatment timing. In the last two columns, instead, I display the results of the Unconditional and Conditional F-tests for the difference in means across the four cohorts of treatment. While some outcomes display significant unconditional differences, after conditioning on the set of covariates used in the regressions, no significant difference can be detected. Notably, testing for the absence of a significant difference in pre-treatment levels of outcomes by treatment timing is a stronger test, as the standard difference-in-differences assumptions only require no counterfactual differences in the trends of the outcomes.

Third, and most importantly, one can inspect whether outcomes exhibit parallel trends in the pre-reform period as a way to assess the plausibility of the assumption of parallel trends in the post-reform period.[42]

Another source of potential concern is the presence of anticipation effects (Malani and Reif (2015)), due to the fact that, at least in theory, municipalities knew exactly the timing of treatment. There are two types of anticipation effects that could be happening in this setting. On the one hand, one could think that later-treated units, in expectation of the arrival of a stricter auditor, would start responding earlier, in order to smooth the adjustment of their finances. This type of anticipatory behavior is not particularly problematic, as it would lead to an under-estimate of my treatment effect, as I would compare my treated units to later-treated units who are already starting to behave like treated ones before they switch on to be treated. On the other hand, one might think of a scenario in which the later-treated units respond in the opposite direction, meaning that they start over-spending and accumulating debt in the expectation of not being able to do so anymore in the future. This second type of anticipatory behavior is more worry-some, as it would potentially lead to an over-estimate of the treatment effects.

[42]Some recent papers (Kahn-Lang and Lang (2020), Roth (2020), Rambachan and Roth (2020)) have cautioned against the use of pre-event trends testing as these tests are frequently under-powered and researchers can commit type-2 errors when taking a failure to reject the null of non-parallel pre-trends as evidence in favor of the assumption of counterfactual post-treatment parallel trends. I have conducted sensitivity tests, in the spirit of the one suggested by Rambachan and Roth (2020), using the R package *HonestDiD* created by the authors, and my results are robust to allow for large degrees of possible non-linearity in the violation of parallel trends. Results are available upon request.

In a standard difference-in-differences design with never-treated units, one could easily inspect the presence of either type of anticipation effects by looking at event studies relative to the announcement moment. This type of exercise is not feasible in this setting, due to the absence of never-treated units. Instead, I can exploit one of the stacked-by-event design features to provide suggestive evidence of the type of anticipation effects potentially at play. As can be seen in equation 3, the design allows me to separately control for both calendar-time fixed effects (the δ_t) and event-time fixed effects (the terms $\sum_{k=-7}^{k=2} \beta_k * D^k$). The latter are estimated off of the control group.[43] By looking at the β_k coefficients, one can thus inspect if, and in which direction, the control units change their behavior before actually becoming treated.[44] Appendix Figure A2 shows the results. The important thing to notice in the figures is that, if anything, control units seem to display an anticipatory behavior of the first kind, meaning that they start behaving in a way that is similar to the one of the treatment group, before being treated. In fact, the coefficients of Figure A2 are of the same sign, though of a much smaller magnitude, of those of Figure 2, which displays the coefficients γ_k, which represent the event-studies for the treated group. In this sense, my estimates could be considered conservative lower-bound estimates of the true effects in the presence of anticipatory effects.

Finally, a source of concern might be the presence of other time-varying shocks that occur in the same period, in a way that is correlated with the staggered introduction of the reform across municipalities. The most significant change that occurred during this time period is the extension of the rules of the Domestic Stability Pact to municipalities with a population below 5000 inhabitants in 2014. This change is accounted for by the inclusion of population-size-by-year fixed effects. In Section 7 I also investigate the heterogeneity in treatment effects by municipality size and show that the effects are present also when restricting the sample only to municipalities above 5,000, that have been always subject to the same rules since 2001. Relatedly, one might worry about the fact that the change in auditors' appointment was adopted as part of a large emergency reform, which might have affected local governments'

[43] In particular, they are estimated thanks to the fact that the same municipality appears multiple times in the dataset. Separately pin down calendar-time effects and event-time effects would not be possible in a "pure" event study design in the absence of a control group, as with unit fixed effects one cannot separately identify the passing of calendar time and relative time - see Borusyak and Jaravel (2017) for a clear description of the problem.

[44] In my stacked-by-event design, when constructing the dataset, for each event, control units are assigned a "placebo" shock at event time = 0 for that specific event, which ensures that their potentially anticipatory responses are accounted for explicitly in the estimation of the dynamic treatment effect. For example, when calculating the treatment effect for the 2012 cohort, all control units are assigned "placebo" relative time around 2012, so for example, for units treated in 2015, the calendar years in which they could display anticipatory behavior, namely 2012, 2013 and 2014, are labeled as 0, 1, and 2, while in a "standard" event study they would be -3, -2 and -1 and thus their anticipatory behavior would not be distinguishable from pre-event trends.

finances in other ways beyond the change in auditor's appointment.[45] The most important one for this paper is the temporary inclusion of owner-occupied dwellings amongst houses subject to the property tax. However, it is important to remember that these changes affected contemporaneously all municipalities, while the change in auditors' appointments entered in place in a staggered way, depending on the timing of the expiration of the previous municipal auditor. Therefore, the effect of any other change related to the reform is captured by the presence of calendar time effects in my regressions.

5 Results

5.1 The impact of Random Assignment on the Selection and Allocation of Auditors

Before moving to investigate the effect of random auditor assignment on fiscal discipline, I inspect how the reform affects the selection and allocation of auditors across municipalities. Table 4 compares the characteristics of the auditor-municipality match under discretionary appointments (pre-reform) as opposed to the ones resulting from random assignment. Column (1) (respectively, (2)) shows average characteristics in the pre (post) reform period, while column (3) shows the difference in means between the two, and column (4) the standardized differences, so as to make it easier to compare the change across different variables.[46] Consistently with the ease in access to the auditors' market in the post-reform system, auditors in the post-reform period are less experienced in terms of municipal auditing. On the other hand, if anything, they have even a slightly higher level of experience as accountants. This is partially explained by the fact that they are not any younger, but actually even slightly older, suggesting that the new entrants are not young accountants that are new to the profession, but rather established accountants that were not previously appointed as municipal auditors. I then look at the average probability that auditors are re-appointed for a second term. In the pre-reform system, this happened in 57% of the cases, while in the post-reform system this becomes an extremely rare occurrence (1%), as it is the result of the same auditor being drafted again for the same municipality. Similarly, the introduction of random

[45] In particular, the reform introduces a set of provisions that affected municipalities: a) tightening of fiscal rules and cuts of national government transfers for the year 2012, b) introduction of progressive tax rates for the municipal additional income tax, c) mandatory collective management of public goods and public services for municipalities up to 1000 inhabitants, d) reduction in the number of members of the municipal council e) collection of property tax on owner-occupied units.

[46] As robustness, Appendix Table A6 reports a regression version of the same Table using the main empirical specification in equation 1, where the characteristics of the auditors and auditor-municipality matches are on the left-hand side. All the outcome variables are standardized.

assignment makes it significantly less likely for municipalities to have a local resident as a municipal auditor. In the pre-reform discretionary system, hometown auditors were present in 32% of the sample. This number goes down to 1% by virtue of the random assignment. This fact is mirrored in the large increase in the average distance (in travel time) between the auditors' municipality of residence and the one where he operates as municipal auditor, increasing from 17 to over 60 minutes. Finally, I look at the probability that the auditor and the mayor share some relevant characteristics, such as gender, birthplace or surname. In the pre-reform period, 20% of the auditors were born in the same place as the mayor, and 7% shared the same surname with a local politician. Together, these preliminary findings suggest that random assignment significantly changed the allocation of auditors across municipalities in a way that is likely to have severed previously-existing ties between auditors and mayors, that are likely to affect monitoring performance (Chu et al. (2020), Xu et al. (2018), Xu (2018)). On the other hand, auditors in the post-reform period look fairly similar to the old ones along characteristics that might impact their monitoring ability, such as age and experience.[47]

5.2 Fiscal Sustainability under Random Assignment

Table 5 displays the main results for the effect of the introduction of independent auditing on indicators of municipal fiscal sustainability. Odd columns present results from the specification 4 without controls, while in even columns I augment the specifications with time-varying controls. Results indeed provide evidence that independence contributes significantly to improve the fiscal sustainability of municipal finances. Across all outcomes, the inclusion of controls leaves results essentially unchanged. In columns (1) and (2), I investigate the effect on $NetSurplus$. Upon the arrival of an independent auditor, treated municipalities improve on this margin by €16 per capita, an increase of 8% relative to the pre-treatment mean. In columns (3) and (4), I look at $DebtRepayments$. Again, I find a significant positive effect, of similar magnitude, with treated municipalities increasing their debt repayments by 8% after the arrival of draft-appointed auditors. Finally, in the last two columns, I show results for $OBSDebtRepayments$. Here, I do not find any significant effect. As I will explain in later sections, this is likely to be due to the presence of strongly heterogeneous dynamics across

[47] A distinct, interesting comparison would be the one between the pool of auditors in the pre vs. post period. While I can observe the pool of auditors subscribing to the list in the post-reform period, I do not have access to a full list of all the potential candidates in the pre-reform period, given that mayors could choose among any certified public accountant. Relatedly, I cannot separately identify whether the reform induced the exit of "bad" auditors, who decided not to sign up on the list. However, as it will be clear in the following section, I am able to rule out that this type of "differential sorting" is the only driver of the effects as I am still able to detect a significant change in fiscal discipline for municipalities above 5,000 inhabitants, that could only be assigned auditors with previous experience.

different types of municipalities, as well as to the fact that the variable is equal to 0 in over 80% of my sample, given that $OBSDebtRepayments$ are a rare event.

Figure 2 displays the coefficients from the event-study specification in equation 3 and allows us to inspect the presence of differential pre-trends, as well as the dynamic of the treatment effect of the auditor's term. The first thing to notice is that the graphs do not display any significant evidence of differential pre-trends across all three outcomes. Secondly, the figures allow inspecting the dynamics of the treatment effect over time. Interestingly, for both the $NetSurplus$ and $DebtRepayments$, treatment effects appear right away, from the very first year of arrival of an independent auditor, and then remain fairly constant across the three years of the auditor's term. This pattern seems to be suggestive of a sudden and persistent shift in budgetary practices of municipalities in response to the treatment. Finally, the figure confirms the absence of any significant effect for $OBSDebtRepayments$, albeit point estimates display a slightly increasing pattern over time.

5.3 Margins of Adjustment: Revenues vs. Spending

The results above show that treated municipalities indeed improve their fiscal sustainability by running higher net surpluses and reducing their outstanding debts. This is likely to have implications for spending and revenue choices. To improve their fiscal stance, local governments can either increase their revenues (e.g. by raising higher taxes) or cut other expenditures (e.g. cut investments and/or current expenditures), or a combination of both.

In Table 6 I investigate the margins of adjustments of municipal governments by looking at their spending and revenue choices. As before, all variables are measured in per capita terms and transformed using the inverse hyperbolic sine transformation. In columns (1) and (2) I look separately at total current and capital expenditures. Interestingly, here we see that local governments do not improve their fiscal sustainability by cutting expenditures. On the contrary, while $CurrentExpenditures$ remain essentially unaffected, $CapitalExpenditures$ increase by over 4%. The absence of an effect on current expenditures should not appear surprising, as the vast majority of current expenditures are due to personnel costs and are therefore rigid, as local governments cannot fire local bureaucrats, nor can they adjust their wages. On the other hand, the increase in capital investment is a potentially desirable outcome. A frequently debated side-effect of fiscal rules' constraints imposed by the DSP pact is that municipalities have usually responded by cutting investments (Chiades and Mengotto (2015)). This can be a way to go around the fiscal restraints, but can also be related to the nature of investments: as public infrastructure spending requires long planning (e.g. because of the procurement process), it is frequent that investment decisions made at year t are

completed and paid only in later years.

In the subsequent columns, I look at the response on the revenue side. Municipalities' main sources of current revenues are the local property tax and the local income tax. Column (3) shows that municipalities achieve the improvement by significantly increasing revenues from the local property tax, which rise by over 20%, for an amount that corresponds on average to an increase of over €30 per capita. Specifically, I look only at revenues from property tax on properties that are not owner-occupied, as the vast majority of owner-occupied dwellings have been exempted from the property tax by national-level legislative changes for the largest part of my sample period.[48]On the other hand, no significant change can be detected for the local income tax (column (4)). Finally, in columns (5) and (6) I look at $CapitalRevenues$ and $NewDebt$. $CapitalRevenues$ increase by 4%, while $NewDebt$ increases by over 22 %. Given that municipalities, by law, can undertake new debt only to finance new investments, it is not surprising to observe an increase in $NewDebt$ in parallel with the observed increase in investment expenditures. Notably, however, the increase in total revenues to be used for investment financing ($NewDebt$ and $CapitalRevenues$) is higher than the increase in capital expenditures, thus leading to an improvement in the surplus. Figure 3 displays results from event-study specifications for the outcomes in Table 6. Looking at the first row, no significant trends could be detected. However, some interesting dynamics emerge. While current revenues respond immediately from the very first period of arrival of the new auditor, both capital revenues and capital expenditures only react from the second period. The increase in $NewDebt$ instead is present only in the first two periods and then disappears, while the opposite is true for the increase in the local income tax, suggesting potential substitution patterns across different sources of revenues.

Overall, results show that, upon the arrival of an independent auditor, municipalities improve their fiscal stance through revenue-based adjustment, specifically by increasing revenues from the local property tax. This result is in line with previous evidence from Italy, showing that municipalities mainly respond to cuts in tax transfers from the central government through revenue-based adjustments (Grembi et al. (2016), Marattin, Nannicini and Porcelli (2019)).

A natural question to ask is how municipalities increase their tax revenues. On the one hand, they could increase the property tax rate. On the other hand, they could expand the tax base by improving enforcement of tax collection and reducing tax evasion upon

[48]The distinction between revenues from owner-occupied and other properties is not available for 2007, the first year of the sample, and therefore the analysis for this variable is restricted to the window 2008-2014. As explained in Section 4.1, the only year in which taxes were collected on owner-occupied units was 2012. This temporary change is not problematic for my estimates as it is common across all municipalities and thus accounted for by calendar time effects.

the arrival of independent auditors. While my data are not well-suited to fully address this question, I provide some suggestive evidence on the specific margins in Table 7. Column (1) simply repeats the same analysis of column (3) of the previous Table. In column (4), I look at the total amount of property tax revenues that is effectively collected by the end of the year. Here, the results look almost identical to the ones found in the previous columns, suggesting that the effect is coming from an expansion of the reported tax base, rather than an improvement in collection ability. In Column (7), instead, the dependent variable is the (log of) property tax rate. I obtain coefficient estimates that are insignificant and very close to 0 in magnitude. The absence of a sizeable effect on the tax rate margin further suggests that the improvement in revenues is coming from an expansion of the tax base. In line with this explanation, in columns (2) and (3), I show that the effects are stronger in municipalities that have a higher share of un-declared units, based on the matching between tax declarations and cadastral data done by the National Tax Agency, and where there was a higher share of c.d. "ghost buildings", as documented in Casaburi and Troiano (2016).[49]

Moreover, municipalities increase spending on investments. This second result is particularly relevant and goes in the direction of the efforts of the national government to revert the persistent negative trend towards a contraction of the investments that occurred from 2007 onwards. This contraction occurred mostly because municipalities initially responded to the tightened budget constraint induced by the DSP pact mainly by cutting investments (Chiades and Mengotto (2015)).

Taking stock of the results presented so far, I argue that the arrival of draft-assigned auditors indeed improved monitoring and spurred more fiscal discipline, with municipalities running higher surpluses and achieving the adjustment by better enforcing the collection of revenues from the local property tax.[50]

[49]In 2007, the national government started a large anti-evasion program to identify buildings that were previously not included in the land registry and thus hidden from tax authorities. Thanks to the use of aerial photography, the program detected over 2 million parcels with at least one ghost building. As documented in Casaburi and Troiano (2016), "the program led to a substantial wave of building registration and a sizeable increase in total tax revenues (almost half a billion €), with a large share of these extra revenues coming from local property taxes.[...] The intensity of the additional tax enforcement varied significantly across towns".

[50]One natural question is to understand which implications these adjustments have for public goods and public services provision. As explained in Section 2.1, local governments in Italy provide a large set of goods and services, which can be grouped into 8 categories: general administration, local police, education, culture, tourism, transportation, local public goods, and social welfare. In columns (1) to (8) of Appendix Table A4 , I look at total expenditures for these eight categories as dependent variables. Interestingly, we see that the increase in expenditures is not homogeneously distributed. Only four categories display significant increases: general administration, education, culture, and social welfare.

6 Robustness

6.1 Alternative Estimators

To corroborate my analysis, I apply the alternative estimator recently proposed by de Chaisemartin and D'Haultfœuille (2020), which is robust to negative-weighting issues.[51] I start by estimating the "naive" two-way fixed effects model outlined in equation 1. Results are displayed in Table A1 . Reassuringly, the estimates are all very similar to the ones in Table 5. I then estimate the weights attached to each of the average treatment effects (ATTs) to compute the overall β_{fe} estimate.[52] Results are displayed in the bottom part of Table A1 . The β_{fe} coefficients in Table A1 are obtained as a weighted average of 10,747 ATTs, of which 3175 receive a negative weight, but overall the negative weights only sum up to -0.106, suggesting that the relative importance of ATTs receiving negative weights is limited. To have a better sense of whether this represents a problem, one can look at the two diagnostic measures to assess the robustness of the $\hat{\beta_{fe}}$ estimate to treatment effect heterogeneity. The first one $\underline{\sigma}_{fe}$, corresponds to the ratio between the $\hat{\beta}_{fe}$ and the standard deviation of the weights, which is a proxy of the (unobserved) degree of heterogeneity in ATEs across treated groups and time periods and reflects the minimal value under which it would be possible to have a β_{fe} which is of the opposite sign of the true ATT. The second one, $\underline{\sigma}_{fe}$, is a proxy of the minimal amount of treatment effect heterogeneity under which it would be possible to obtain a β_{fe} which is of the opposite sign of all the ATEs. Note that this second measure is defined only if at least one of the weights is negative, otherwise, it would be impossible to obtain a β_{fe}, which is a weighted average of all the ATEs, of opposite sign to them. Reassuringly, across all outcomes, $\underline{\sigma}_{fe}$ is as large as the $\hat{\beta}_{fe}$ estimate per se, thus implying that a substantial amount of treatment effect heterogeneity across $municipalityXyear$ cells would be required to invalidate the naive estimates. As a further robust test, I estimate the alternative Wald estimator proposed by de Chaisemartin and D'Haultfœuille (2020), the $DIDm$ estimator, which is robust to treatment effect heterogeneity across groups and time periods. In my case, the $DIDm$ is estimated only comparing "joiners", i.e. units whose treatment status changes between periods, to untreated stable units, i.e. units who remain untreated between periods.[53] Similarly, one can also compute $DIDm$ placebo estimators looking at outcomes' evolution in pre-treatment periods, as well as the different dynamic treatment

[51] For further details, see, in particular, section 5.2 of de Chaisemartin and D'Haultfœuille (2020).

[52] I use the most recent versions of $twowayfeweights$ and $did_multiplegt$ commands, developed by the authors, and available in STATA repository.

[53] So, for example, to estimate the treatment effect for the first year of treatment (t = 0), the $DIDm$ compares the evolution of outcomes between $t-1$ and t for units that become treated at time t to units that are not yet treated in t.

effects over time. I present results for the full dynamic specifications in Appendix Figure A5 . Results look very similar to the ones in Figure 2.[54]

6.2 Fiscal Sustainability under Discretionary Appointment

To corroborate my results, I investigate whether, in the pre-reform period, granting discretion in auditors' appointments indeed induced adverse effects in terms of fiscal sustainability. To do so, I exploit the fact that, even before the reform, mayors did not always have full control of the appointment of auditors, due to the difference in the length of auditors' and mayors' mandates. Italian mayors' terms last 5 years while auditors' one last only 3 (renewable for one time). This gives rise to exogenous variation in the mayoral control of appointment (see the bottom panel of Figure 4 and Appendix Figure A6 for an example), which can be used to inspect the use or abuse of discretion in the pre-reform period. Intuitively, if mayors were appointing friendly and lenient auditors who would allow them leeway vis-a-vis fiscal rules and spending, we should observe a worsening of fiscal sustainability during years in which mayors had control of the appointment. On the other side, if mayors were using discretion in a good way to appoint efficient and experienced auditors, then we should not observe any significant difference. I test this hypothesis by running the following regression:

$$Y_{mt} = \alpha_m + \delta_t + \beta Control_{mt} + X'_{mt}\zeta + \epsilon_{mt} \tag{4}$$

Importantly, the probability of having control of appointment increases along the electoral term. and second-term mayors always have control of appointment. Thus, one might worry that the control of appointment is in fact picking up distortions related to the political budget cycle problems (Alesina and Paradisi (2017)). For this reason, among the controls, I always include a dummy equal to 1 for the two pre-election years, and therefore β is estimated by comparing mayors that are at similar points of the election cycle but, for exogenous reasons, either did or did not appoint the current auditor. Furthermore, mayors in their second term of office always have control of appointments. Results are displayed in Table 8. In the odd columns, I include also all second-term mayors and introduce a control for second-term mayors interacted with the pre-election dummy. In the even columns, instead, I restrict attention to first-term mayors. Notably, I find that, during years in which mayors have control of appointments, municipalities run higher deficits and decrease their debt repayments. This suggests that, indeed, mayors that have discretionary appointment power act in a less fiscally

[54] As additional robustness, I also repeat the analysis using the estimator developed by Callaway and Sant'Anna (2021) and obtain estimates that are qualitatively analogous to the ones of the main analysis. Results are reported in Appendix Table A2 .

responsible way. It is interesting to compare the magnitude of the effect of the arrival of an independent, draft-appointed auditor in Table 5 with the effects obtained in Table 8, with the caveat of the difference in the estimating samples and equations. The positive effect on the $NetSurplus$ of the independent auditor's appointment is twice as large as the negative effect found for the discretionary appointment effect, while the effect on $DebtRepayments$ is over 4 times as large. This suggests that the draft appointment mechanism is a more effective disciplining tool than the simple term misalignment between mayor and auditor in the pre-reform period. Overall, the results go in the same direction as my previous findings, confirming the importance played by changes in the control of auditors' appointments. From a policy perspective, these results also provide evidence that the introduction of a gap between the political and the audit cycle could be an alternative system to strengthen oversight and improve auditors' independence.

7 Mechanisms

In this Section, I perform a series of tests for heterogeneity with the objective of better understanding the set of mechanisms through which the reform most plausibly operates. The results can be organized according to three different directions. First, I investigate the role of pre-reform characteristics. Intuitively, one would expect to observe larger treatment effects in municipalities that were more likely to have lax or corrupt monitoring in place in the pre-reform period. I, therefore, adopt several different proxies to identify places where collusion between mayor and auditor, impairing auditor's activity, was more likely to take place before the reform, including un-healthy financial conditions, the appointment of a local resident as auditor, and the presence of corruption investigations. Second, I investigate the role of the characteristics of the post-reform municipality-auditor match. In particular, one would expect to observe stronger treatment effects in places where the randomly assigned auditor is less likely to have previous connections and has fewer incentives to collude with the local mayor. Viceversa, we should not observe any significant difference in financial outcomes if, by chance, random assignment re-created a situation in which previous connections between auditor and mayor or the incentives to collude are likely to be stronger. Finally, I investigate the interplay between auditor's appointment and electoral accountability, to understand whether increased monitoring acts as a complement or substitute for monitoring from the local electorate and/or from the local political opposition.

7.1 Pre-Reform Collusion Risk

7.1.1 Pre-Reform Fiscal Stance

If independent auditors act to efficiently ensure the fiscal sustainability of municipal finances, then we should not expect to observe any increase in surpluses in municipalities that were already in good standing before the arrival of the draft-appointed auditors. If anything, independent auditors might spur a reduction in surpluses, to push municipalities that were running excess surpluses closer to a balanced budget. In Table 9 I repeat the analysis of Table 5 including an interaction with an indicator which is equal to 1 for municipalities for which the dependent variable was below the median value in 2011. For $OBSDebtRepayments$, the median value in 2011, and all pre-treatment years, is always 0. Therefore, for this variable, I construct an indicator equal to 1 if the municipality has ever had a positive amount of $OBSDebtRepayments$ in the pre-period and 0 otherwise. Results indeed show that treatment effects are significantly larger in municipalities that were less fiscally sustainable before the reform. The treatment effect for the $NetSurplus$ is twice as large for municipalities that had a $NetSurplus$ below the median value in 2011. An even more striking difference emerges when looking at the effects on $DebtRepayments$, where the treatment effect is four times as large. However, in both cases, the treatment effect, while smaller, is still positive and significant also for municipalities that were already in a relatively good fiscal stance in the pre-period. Finally, looking at the last column, we see that the treatment effect for municipalities that never had $OBSDebtRepayments$ in the past is positive, significant, and very large in magnitude. On the other hand, the treatment effect for those that had already had at least a dollar of $OBSDebtRepayments$ in the pre-reform period is negative and significant. The sign divergence of the treatment effects explains the overall zero effect in Table 5 and suggests the presence of two very different underlying phenomena. Differently from all the other components of the budget, $OBSDebtRepayments$ represent a self-declaration of an illicit spending act. Thus, auditors can act on two different margins: they can pressure the mayor and/or the city council to report any about $OBSDebtRepayments$; b) they can make it impossible or very costly to commit illicit spending. While I cannot effectively separate the two mechanisms without having information about the unobserved amounts of illicit spending, the divergent signs of the effects in the sample split suggest the presence of two types of municipalities. In "honest" municipalities, that correctly report illicit spending in the pre-reform period, the arrival of an independent auditor reduces the occurrence of the illicit spending phenomena, thus leading to a decrease in $OBSDebtRepayments$. On the other side, in "dis-honest" municipalities, the arrival of the independent auditor induces the reporting of illicit spending, thus leading to an increase in $OBSDebtRepayments$.

7.1.2 Appointment of Local Residents as Auditors

While I cannot directly measure collusion risk in the pre-reform period, I collect information on potential proxies for collusion and/or factors that increase the probability of collusion. First, I use an indicator for whether the last auditor appointed before the reform was born or a resident in the same municipality. Intuitively, a "local" auditor is more likely to have connections with the mayors and/or be more sensitive to local interests, and thus be willing to collude to favor local spending vis-a-vis fiscal sustainability (Chu et al. (2020), Xu et al. (2018), Xu (2018)). Table 10, Panel A displays the results, where I include a triple interaction with an indicator for municipalities that had a local auditor before the reform. We see that, across all outcomes, treatment effects for municipalities that had a local auditor before are stronger. While the difference is not significant for the $NetSurplus$, much starker differences emerge when looking at the last two outcomes. The treatment effect on $DebtRepayments$ is 30% larger. Most notably, when looking at $OBSDebtRepayments$, we see that the treatment effect for municipalities that didn't have a local auditor is not only insignificant but also negative. On the other hand, the treatment effect for municipalities with a local auditor is positive and significant. This indeed seems to suggest a more significant improvement in monitoring in these types of municipalities. As previously mentioned, auditors have a crucial role in the process of $OBSDebtRepayments$. They shall a) review and express an opinion on the restatement process and b) signal to the Court if they are aware of any OBS Debts that have not been properly restated in the balance sheet. While I cannot measure the underlying size of OBS Debts, results suggest that municipalities with local auditors had a larger amount of unrecognized OBS debts, as proxied by the larger pre-treatment mean in $OBSDebtRepayments$ (assuming that the size of $OBSDebtRepayments$ is proportional to the real underlying size of OBS Debts) and upon the arrival of an independent auditor were more likely to restate them.

7.1.3 Corruption Investigations

Lastly, I exploit an indicator of municipal corruption. I use restricted-access data from the *Sistema D'Indagine Interforze* (SDI), a centralized investigation archive that contains reports of all individuals investigated by any of the Italian police forces: state police (*Polizia di Stato*), finance police (*Guardia di Finanza*), military police (*Carabinieri*), and environmental police (*Guardia Forestale*).[55] The final data contains information, for each munici-

[55]The SDI data have been previously used in research by Pinotti (2017) and Decarolis, Fisman, Pinotti and Vannutelli (2019). See Decarolis et al. (2019) for further details on the data.

pality, on the total number of investigations for all types of crimes in the years 2004-2013. I construct an indicator variable flagging whether, in any given municipality, there was at least one investigation for corruption-related crimes in the entire period.[56] Appendix Figure A7 shows a map of municipalities having the *Corrupt* dummy equal to 1. While, ideally, one would like to know directly whether local politicians have been investigated for corrupt practices, corruption cases usually involve illegal transactions between private parties and members of the local bureaucracy and are thus a proxy of an environment where illegal practices take place. Furthermore, corruption-related crimes are frequently linked to local public procurement or other forms of local public spending, which are factors that should be, in theory, subject to auditor's monitoring. Thus, a context in which corruption-related crimes take place is more likely to be a context where monitoring might fail and/or in which the mayor might have an incentive to collude with a local auditor to cover illicit practices. Table 10, Panel B displays the results. The patterns of results are very similar to the one found in Panel A, with a much stronger gap displayed in the last two outcomes. For *DebtRepayments*, the treatment effect is four times larger in corrupt municipalities (0.06 vs 0.24). When looking at *OBSDebtRepayments*, we see an even more striking gap. The treatment effect is negative and significant: upon the arrival of an independent auditor, the amount of *OBSDebtRepayments* decreases by 5%. On the other hand, in corrupt municipalities, *OBSDebtRepayments* increase by 27%. Again, such a large divergence might also explain the absence of a significant average treatment effect in Table 5, as it results from a composition of very different phenomena. The negative treatment effect in non-corrupt municipalities suggests a reduction in the overall size of OBS Debts. On the other hand, the significant increase in *OBSDebtRepayments* in corrupt municipalities suggests the presence of a large amount of previously hidden OBS Debts in these places.

7.2 Post-Reform Collusion Risk

In this section, I aim at investigating how much the characteristics of the new auditor-municipality match explain the observed effects. On the one hand, treatment effects might be at least partially due to a selection effect, as the reform induces a change in the composition of the auditors' pool. As common in markets for experts with the presence of discretional appointments, the pre-reform market was characterized by a relatively limited number of auditors who perform audit tasks for multiple municipalities.[57] The new draft system allowed

[56]Corruption-related crimes include bribery, corruption, malfeasance, and embezzlement.

[57]Note, however, that the market was still much more decentralized than the Italian and U.S. market for private auditing, which is characterized by a 4-companies oligopoly. Furthermore, auditors, by law, can have at most 8 concurrent appointments, so this naturally puts a limit on market concentration.

many outsiders to add their names to the list for the draft and have a chance to enter the market, and this changed the composition of the auditors' pool. If these new "outsiders" are less corrupt and more likely to enforce national government objectives, then this might drive the observed treatment effects. Indeed, the reform induced a change in the composition of the auditors' pool: almost 18,000 new auditors joined the list, representing around 70% of the pool of potential auditors. While these new entrants have no municipal-specific experience, they look otherwise very similar to the existing pool of incumbents on other observable dimensions, as it can be seen from the descriptive statistics in Table 11. Most importantly, they are equally experienced in terms of general private-sector accounting. To test more formally whether effects are driven by the reform-induced change in auditors' characteristics, I adopt the same difference-in-differences specification, but including the characteristics of auditors in Table 11 as controls. Results are displayed in Table A11 . Comparing the magnitude of coefficients to the ones in Table 5, changes in auditors' characteristics account for 30 to 50% of the effects. To further assess the role of selection, for the subsample of municipalities below 5,000 inhabitants, I split the sample between those that receive an outsider as a new entrant and those that receive an already-operating auditor. Table 11, Panel A displays the results. The analysis shows that, at least for the sub-sample of smaller municipalities, effects are essentially driven by the assignment of new entrants.

Beyond selection, the reform is likely to change the incentives and behavior of auditors due to the gained independence and the improvement in their bargaining power vis-a-vis the mayor. Such a change in behavior is expected to be larger the less likely the existing auditor is to have pre-existing connections with the mayor. Intuitively, although random assignment *perse* weakens the bargaining power of the agent (the mayor) by taking away from him the choice of both auditor's identity and reappointment, ifthe outcome of the draftt by chance recreates a situation that is similar to the pre-reform appointment (e.g. by assigning someone who is from the municipality or who is likely to have pre-existing links with the mayor), then pre-reform dynamics are likely to arise again despite the random assignment. To proxy for the strength of auditors' independence in the post-reform matching, I leverage the fact that I have information on the auditor's residence. I thus calculate the travel distance between the auditor's residence and the municipality she is assigned to audit. While not perfect, distance is a viable proxy for "proximity" and thus for independence vs. collusion risk (Xu et al. (2018), Chu et al. (2020)), for at least three reasons. First, a more distant auditor is less likely to have previous connections with the local mayor. Second, a more distant auditor is less likely to value local interests (vis-a-vis national ones). Third, a distant auditor is less likely to have an incentive to build a reputation for leniency locally in order to potentially receive other types of discretionary appointments in the future. Furthermore, results in the previous

subsection show that treatment effects are larger in municipalities where the previous auditor was a local resident, thus suggesting that indeed distance mattered for audit outcomes. I, therefore, do three different types of sample splits. First, I repeat my main analysis separately for a) municipalities below 5,000 inhabitants, b) municipalities between 5,000 inhabitants and 15,000 c) municipalities above 15,000 inhabitants, as in these three groups the law imposes different experience requirements.

The analysis of the results on the two larger groups of municipalities is informative of the pure matching effects. In municipalities above 5,000 inhabitants, the selection channel is shut off by construction, as only auditors with experience can be drafted. Table A8 presents the results. There are two important takeaways from this Table. First, results are present across the three groups of municipalities, thus rejecting the possibility that selection is the only driver of the effects. Secondly, the heterogeneity suggests that the effects on debt repayments are significantly stronger in larger municipalities. This should not be surprising, as larger municipalities are the ones that are more likely to be indebted. Consistently with this explanation, I also detect a significant positive treatment effect on OBS Debt Repayments for municipalities above 15,000 inhabitants.

Finally, in Panel B of Table 11, I look at the heterogeneity based on the auditor-municipality distance. Results show that the effect of the reform is entirely driven by municipalities that, by chance, receive an auditor that resides at least 1 hour away, which is the median distance in the sample. The absence of treatment effects for municipalities that randomly happen to receive a local auditor is particularly intriguing, as it further sheds light on the strength of social ties and ton how home-bias might adversely affect bureaucratic performance (Xu et al. (2018), Chu et al. (2020)), even in the presence of random assignment.

To further shed light on this result, I investigate whether the role of the post-reform distance varies depending on pre-reform choices. Intuitively, we would expect the value of being assigned an outsider to be largest for municipalities that were previously selecting local residents as auditors. In Table A13 , I thus further split the sample between municipalities that had appointed a local resident as auditor in the pre-reform period and municipalities that instead already appointed an auditor from a different municipality.[58] Indeed, results confirm that the treatment effects of being assigned a distant auditor are twice as large for municipalities that used to appoint local residents in the pre-reform period. Combined, the results suggest that both selection and matching are important drivers of the overall

[58]While this choice was indeed endogenous in the pre-reform period, it is orthogonal to the assignment of the auditor in the post-reform period. Thus, I'm comparing two municipalities that made the same endogenous selection in the pre-reform period, one that randomly received a closer auditor and one that randomly received a more distant one in the post-reform period.

effects, both pointing towards the importance of ensuring the presence of an independent and "external" eye in the monitoring process.

7.3 Direct Effects: Detection and the Role of Electoral Accountability

In this section, I investigate the interplay between the auditor's appointment and electoral accountability. On the one hand, one would expect that mayors subject to strong re-election pressures should have stronger incentives to perform, restraining from inappropriate or wasteful financial practices. On the other hand, local interests may frequently clash with national interests, and thus local accountability may push mayors to act in a different direction, particularly if the push towards fiscal sustainability from the national government comes at the cost of cutting expenditures for local services or raising higher taxes for local residents. To understand the role of electoral accountability, I perform two different types of exercises. First, I exploit the fact that, for historical reasons, municipalities are on different electoral cycles that do not overlap with the audit cycles.[59] I, therefore, compare the treatment effects in municipalities that receive the randomly appointed auditor for the first time in the first part of the electoral cycle to municipalities that are treated when mayors are closer to elections. Results are displayed in Panel A of Table 12. We see that, for both $NetSurplus$ and $DebtRepayments$, treatment effects are significantly larger for municipalities treated at the beginning of the electoral term, when the mayor had lower re-election concerns. Second, I exploit the existence of a two-term limit for Italian mayors to see whether mayors who can be re-elected respond differently than mayors who are serving their second and last term. Results are presented in Panel B of Table 12. Despite failing to detect significant differences, I find suggestive evidence that term-limited mayors are more responsive than their re-electable counterparts. Overall, I find similar results along both margins of heterogeneity, suggesting that the response was stronger in municipalities where mayors were facing lower re-election pressures. This seems to suggest that adjustment was somewhat costly for mayors and not necessarily in line with local constituents' interests.

7.4 Indirect effects: Deterrence and the Role of Spillovers

Beyond direct detection effects, a further explanation for the observed favorable average effects is the presence of deterrence effects, whereby mayors change their behavior in expectation of potentially facing a stricter auditor in the future. The key hypothesis is that the

[59] The electoral term of Italian mayors is five years while the auditor term is three years.

change in auditors' appointment is not fully salient to mayors before their municipality or a nearby municipality is subject to the rule change, but they become more salient afterward. I am going to exploit the staggered introduction across municipalities and the geographic variation in the exact timing of the audits to isolate the role played by deterrence effects. Following Colonnelli and Prem (2021), I proceed in two steps. First, I identify the spillover effects of the reform on municipalities that are yet to be treated. I consider a municipality to be treated whenever a neighboring municipality has been treated. The control group consists of all municipalities, aside from the neighboring ones, that are yet to be treated. Second, I net out the effect of spillovers. To do so, I exclude from the control group all neighboring municipalities relative to the one experiencing treatment. Results are reported in Table 13. As evident from Panel A, I detect sizable spillover effects, that are about 80% the size of the main effects. This is further confirmed when looking at Panel B, as the effects of the reform are almost twice as large when excluding neighboring municipalities from the control group.

8 Discussion and Conclusion

While considered a crucial tool for good governance, effective monitoring is frequently impaired by conflicts of interest. My results highlight that changes in the design of monitoring institutions can significantly improve governance outcomes.

I take advantage of a large-scale reform that changed the appointment system of auditors for municipal governments' budgets in Italy, removing appointment control from mayors and introducing random assignment of auditors. There are three main findings. First, treated municipalities improve their financial health, in accordance with national government objectives. Second, the improvement is achieved through revenue-based consolidation, rather than by cutting expenditures. Third, treatment effects are significantly larger for municipalities that were more at risk of collusion before the reform, and for those that are matched to a more distant or less connected auditor.

These findings have important policy implications and can inform the global debate about policies to curb corruption and solve conflicts of interest in monitoring. There are two important policy takeaways. One pertains to the cost-effectiveness of the reform. Rather than introducing a new auditing system (like the federal audits in Brazil) or an additional supervisory board (like the PCAOB for US audit firms), the reform operates by introducing frictions that make potential collusive agreements harder, while leaving features of institutional design unchanged. In this sense, the benefits come with zero implementation cost. The second lesson relates to the auditors' identity. As distinct from other types of reforms or experimental studies, the auditors here are not central government bureaucrats - as in the

well-known case of Brazil (Ferraz and Finan (2008)) or Puerto Rico - nor are they village members - as in the case of Indonesia (Olken (2007b)); they are certified public accountants hired directly by municipalities to carry out auditing duties. Auditing governments is only a limited part of their work activities. This suggests a potentially important role of external professionals to carry out audit roles in other settings as well.

While arguably a strength of this article is to provide direct evidence of the reform's impact on the ultimate outcome of interest, one open question is to investigate the channels through which the acquired independence improved financial outcomes. By altering the market structure for auditors' appointments, the reform improved auditors' bargaining power vis-a-vis the mayor, as it eliminated the incentive for the auditor to act leniently to secure re-appointment or higher pay from the mayor. This likely induced a change in the auditors' behavior, both ex-ante, in the "consulting" phase, decreasing the likelihood of cooperation to hide information, and ex-post, in the reporting phase, increasing the likelihood of reporting bad outcomes. Indeed, previous research has shown that auditors tend to be "morally seduced" and are psychologically impaired towards retaining an independent judgment when they are facing conflicts of interest with their clients (Bazerman, Morgan and Loewenstein (1997) Moore et al. (2006)). Furthermore, even though the reform was not formally accompanied by a change in the ex-post audit practices of the central monitor (the National Court of Accounts), auditors are now more likely to signal faulty behavior to the Court, thus making targeting of ex-post inspections more accurate and increasing the risk of punishment. Third, it could be that municipalities overreacted in response to the lower expected quality of auditors. In particular, it might be that local governments feared an increase in reporting from auditors who are not experienced and acted out of excessive conservatism. Unfortunately, I am currently unable to distinguish between alternative channels, as this would require the collection of detailed information about the auditing process and to have some form of third-party measures to use as a comparison (e.g. auditors' reports to the National Court of Accounts), a topic that I aim to address in future research.

Relatedly, an important dimension to investigate in future work is the implications the reform had for the auditors' market. The reform significantly altered auditors' concerns and reputational incentives, so a natural question to investigate is if and how would auditors respond and the implications for their careers. In a current work in progress, I'm investigating this question by combining original auditors' survey data with information from auditors' curricula, political appointments' data, and firms' registry data, to see whether and how the reform changes the market for experts and the private value of political connections for auditors.

Finally, a natural question to ask is whether the reform was overall welfare-improving.

While a full cost-benefit assessment is beyond the scope of this paper, it might be instructive to think about the sources of potential costs. While the reform didn't introduce any extra cost for the central government, removing discretion from mayors in auditors' appointments might indeed come at the cost of worsening cooperation and information flows between the two parties, thus affecting the efficiency of policy-making. Furthermore, the revenue-based adjustment might have negative consequences on overall local economic activity (Alesina et al. (2019)). However, the size of the estimated effects would require negative fiscal multipliers and efficiency costs of policy-making of implausibly large size to offset the estimated benefits. In another concurrent work in progress, I'm evaluating the electoral consequences of the reform, both at the local and at national level. Results suggest that local welfare is indeed negatively impacted, as the local electorate responds negatively to the reform, but it correctly attributes the responsibility of the increased austerity to the national government.

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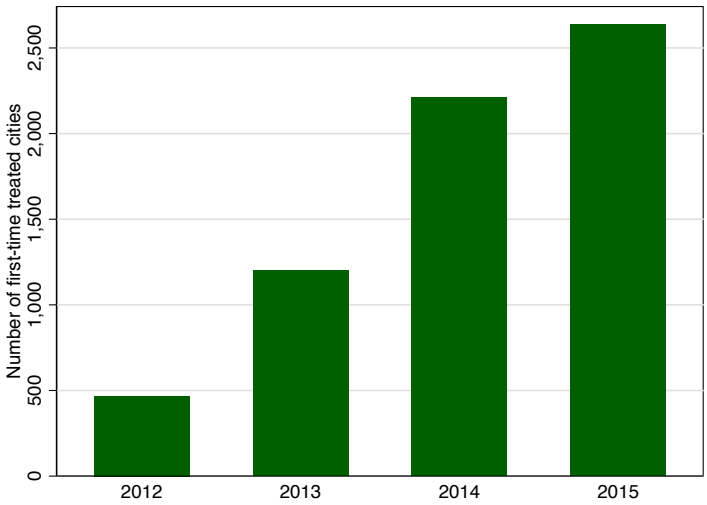
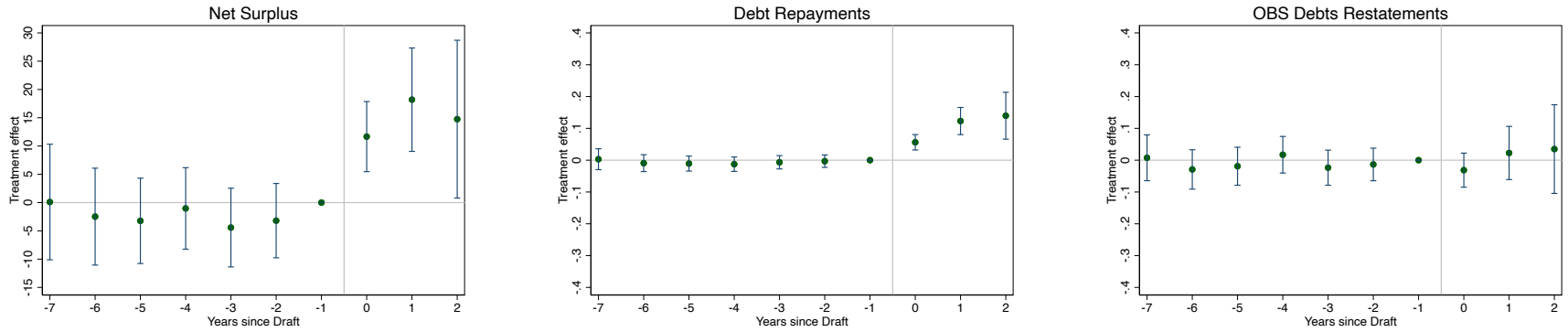
Figure 1
Number of Municipalities Receiving a Draft-appointed Auditor for the First Time in a Given Year



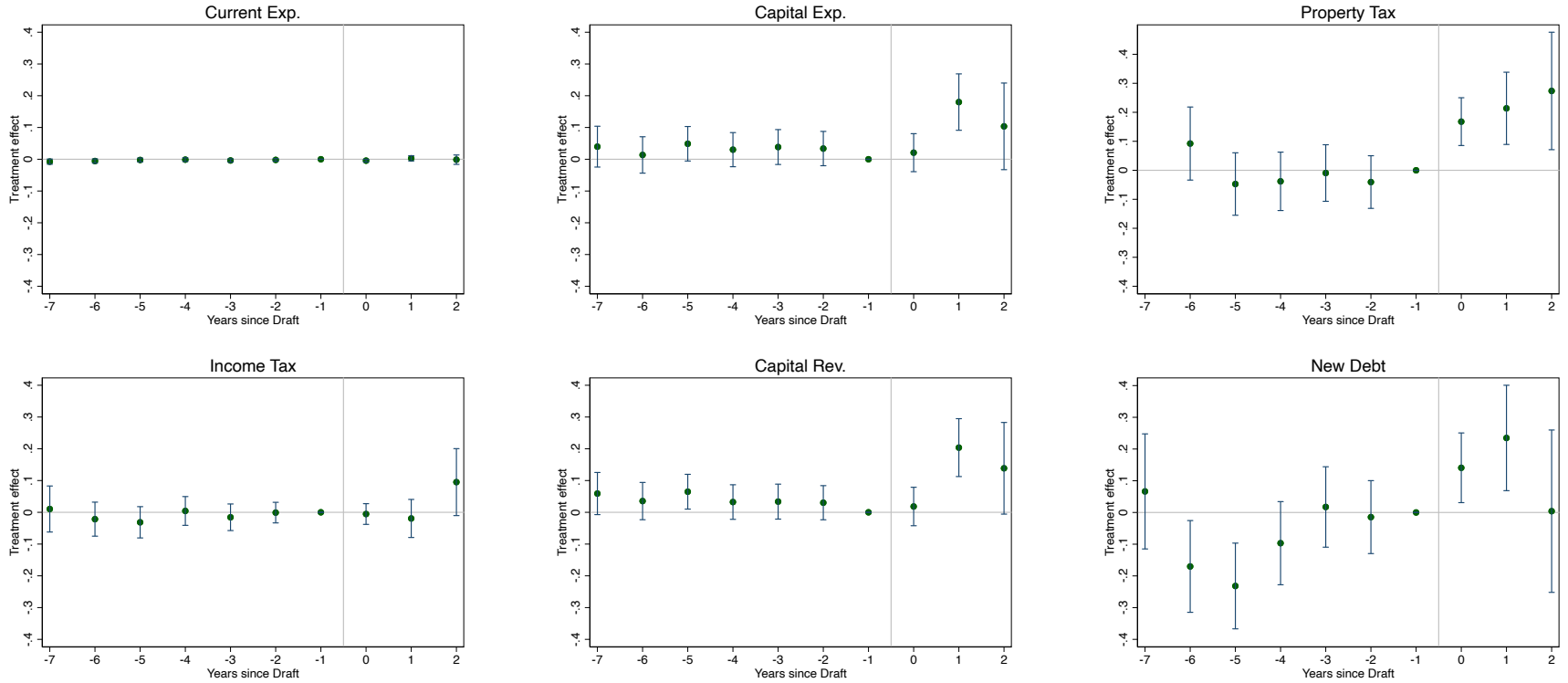
Notes: The figure shows the main margin of variation used in the empirical analysis. The bar graph shows the number of municipalities (y-axis) that had a draft-appointed auditor operative for at least 3 months at the moment of the balance-sheet adoption, in a given year (x-axis).

Figure 2
The Dynamic Effect of Auditor's Independence on Fiscal Sustainability



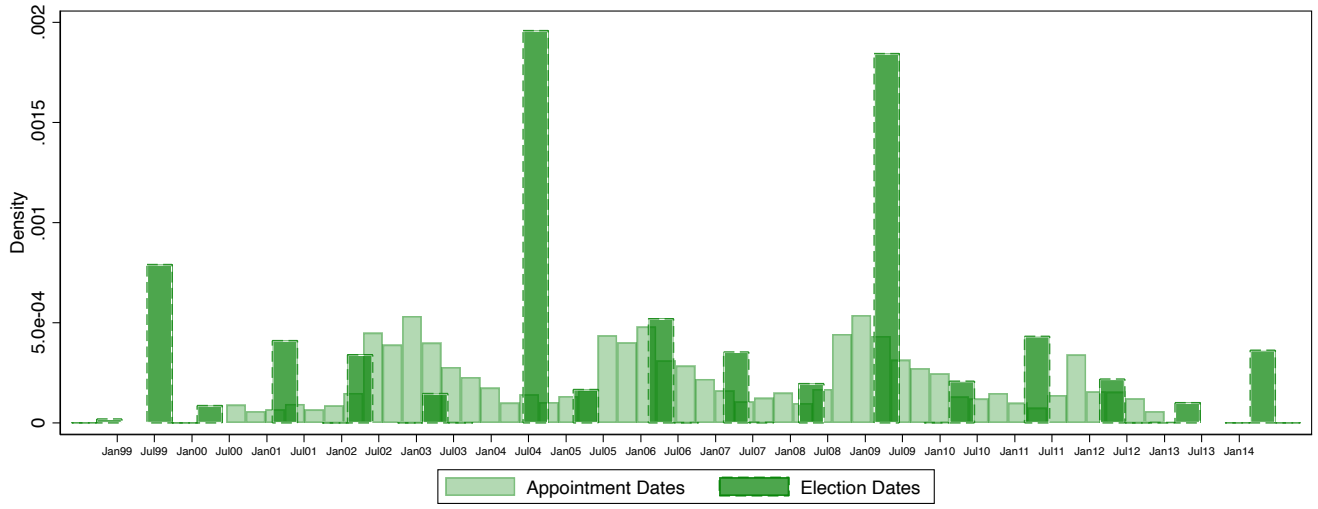
Notes: The graphs report coefficients and 95% confidence intervals estimated according to specification 3. Standard errors are clustered at the municipality level. All dependent variables are in per capita terms, $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation. All regressions include municipality, population-bins-by-year fixed effects, relative time fixed effects and election cycle fixed effects, as well as the following controls: a dummy for early termination of city council, mayor's age at the beginning of the term (in logs), mayor's gender, term in office and a dummy equal to one if the mayor was born in the municipality.

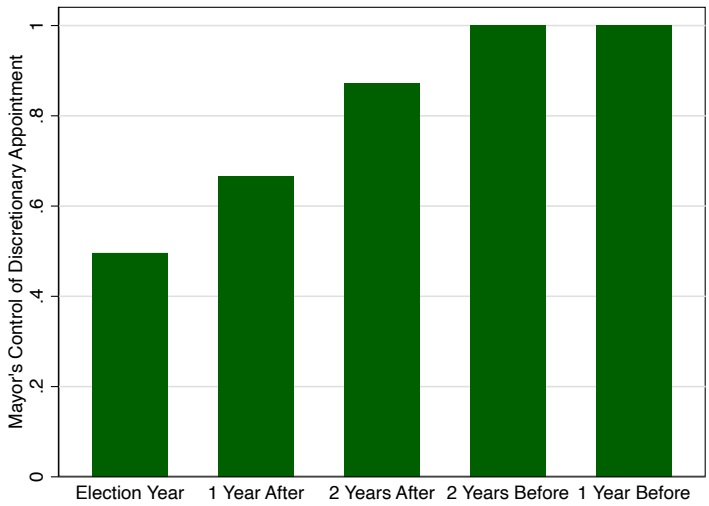
Figure 3
The Dynamic Effect of Auditor's Independence on Aggregate Spending and Revenue Choices



Notes: The graphs report coefficients and confidence intervals estimated according to specification 3. Standard errors are clustered at the municipality level. All dependent variables are in per capita terms, and transformed using the inverse hyperbolic sine transformation. All regressions include municipality, year and election cycle fixed effects, and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor's age at the beginning of the term (in logs), mayor's gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Figure 4
Auditors' Appointment Dates and Municipalities' Election Cycles





Notes: The top panel of the figure displays the distribution of auditors' appointment dates in light green, as well as election dates in dark green. The bottom panel fo the figure shows the share of mayors that had control of the auditor's appointment, by the moment of the election cycle. This is the variation used for identification in Table 8. Municipalities are on different electoral cycles as well as auditors' cycles for historical reasons, unrelated to the reform.

Table 1
Summary Statistics for the Main Outcomes in the Pre-Reform Period

	A. Fiscal Sustainability					
	Mean	S.D.	Median	p10	p90	p99
Net Surplus	-306.97	204.81	-258.73	-554.19	-125.73	31.30
Debt Repayments	59.96	48.95	47.72	21.63	107.07	279.40
OBS Debts Repayments	3.61	13.01	0.00	0.00	7.74	89.05

	B. Revenue and Spending Choices					
	Mean	S.D.	Median	p10	p90	p99
Current Exp.	886.77	401.74	770.17	566.10	1,312.69	2,804.00
Capital Exp.	556.88	783.00	295.62	88.94	1,230.57	5,012.36
Property Tax	140.82	140.24	123.58	0.00	268.57	779.23
Income Tax	38.32	28.84	37.61	0.00	76.26	113.14
Capital Rev.	487.38	744.31	232.78	69.08	1,112.87	4,704.39
New Debt	132.34	255.13	25.94	0.00	379.03	1,446.98

Note: The table shows summary statistics of outcome variables for years up to 2010. All variables are in per-capita terms, expressed in 2018 euros and winsorized at the 1% to remove outliers.

Table 2
Municipal Characteristics that Predict Treatment Timing.

	2012 Cohort	2013 Cohort	2014 Cohort	2015 Cohort
	(1)	(2)	(3)	(4)
1Year	0.00273	0.0405	0.0222	-0.0655**
After Election	[0.0178]	[0.0264]	[0.0281]	[0.0301]
2Years	-0.0235**	-0.0318**	0.0530***	0.00231
After Election	[0.00962]	[0.0143]	[0.0169]	[0.0180]
2Years	0.0110	0.0520*	-0.0646**	0.00158
Before Election	[0.0189]	[0.0266]	[0.0262]	[0.0312]
1Year	0.00352	0.00435	0.0178	-0.0257
Before Election	[0.0154]	[0.0214]	[0.0243]	[0.0261]
Pop. 5-15k	0.0225**	-0.0119	-0.145***	0.134***
	[0.00896]	[0.0127]	[0.0150]	[0.0168]
Pop.	0.104***	0.0491**	-0.200***	0.0472*
15-60k	[0.0189]	[0.0223]	[0.0212]	[0.0263]
Pop. Above	0.202***	0.0959*	-0.224***	-0.0735
60k	[0.0562]	[0.0583]	[0.0452]	[0.0577]
Municipal	-0.00900	0.0100	-0.0182	0.0171
Union	[0.00873]	[0.0141]	[0.0173]	[0.0181]
Mayor Age	0.00816	0.00678	0.0501*	-0.0651**
(log)	[0.0156]	[0.0249]	[0.0302]	[0.0319]
Male Mayor	-0.000330	0.0266*	-0.0102	-0.0161
	[0.0104]	[0.0152]	[0.0203]	[0.0206]
Local	0.00921	0.0299***	-0.0179	-0.0212
Mayor	[0.00778]	[0.0115]	[0.0136]	[0.0143]
Mayor	-0.00275	0.0503**	-0.0374	-0.0101
Resignation	[0.0163]	[0.0255]	[0.0261]	[0.0290]
Mayor	-0.00566	-0.00513	0.0145	-0.00370
Term-limited	[0.00696]	[0.0107]	[0.0130]	[0.0136]
Council	-0.00336	0.0819	-0.0140	-0.0646
Dismissal	[0.0388]	[0.0598]	[0.0564]	[0.0631]
Observations	5603	5603	5603	5603
R-sq	0.0233	0.0128	0.0356	0.0151

Note: The table displays results from 4 separate OLS regressions where the dependent variables are indicators for independent auditor appointment starting in 2012, 2013, 2014 and 2015. The explanatory variables are measured in 2010. Robust standard errors reported in square brackets.

Table 3
Levels of Oucomes in 2010, by Treatment Timing

A. Fiscal Sustainability						
	2012 Cohort	2013 Cohort	2014 Cohort	2015 Cohort	Uncond. F-test	Cond. F-test
Net Surplus	-332.48	-336.70	-327.95	-310.64	0.001 ***	0.251
Debt Repayments	60.60	61.92	64.54	62.91	0.413 .	0.959
OBS Debts Repayments	5.85	5.16	2.70	3.36	0.000 ***	0.791
B. Revenue and Spending Choices						
	2012 Cohort	2013 Cohort	2014 Cohort	2015 Cohort	Uncond. F-test	Cond. F-test
Current Exp.	895.74	885.98	907.66	871.69	0.044 **	0.406
Capital Exp.	505.93	544.56	543.26	507.68	0.367 .	0.678
Property Tax	126.03	119.58	144.18	144.77	0.000 ***	0.384
Income Tax	40.05	36.82	40.61	41.11	0.001 ***	0.233
Capital Rev.	458.40	471.34	460.69	427.97	0.295 .	0.735
New Debt	122.87	150.55	131.16	126.59	0.115 .	0.718

Note: The table shows summary statistics of outcomes in municipalities for which independent auditor appointment starts, respectively, in 2012, 2013, 2014 and 2015 (first four columns). All variables are measured in 2010. The last 2 columns display, respectively, the p-value of unconditional and conditional F-Tests for equality of means across all four groups. For each variable, the unconditional F-test is obtained by running an OLS regression of the outcome variable on a set of indicators for the different cohorts and then testing equality of the indicators' coefficients. The conditional F-test is obtained by running an analogous OLS regression but also including the following as covariates: election cycle dummies, geographic area dummies, population size dummies, a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table 4
Changes in Auditors' Characteristics, Summary Statistics

	Pre Draft	Post Draft	Difference	Standardized Difference
	(1)	(2)	(2)-(1)	(3)/S.D(1)
Female	0.24	0.24	-0.00	0.000
Age	54.01	55.28	1.26 ***	0.161
Experience as auditor	6.34	2.45	-3.89 ***	-1.204
Experience as accountant	16.51	17.03	0.52 ***	0.061
Re-appointed	0.57	0.01	-0.56 ***	-1.130
Local resident	0.32	0.01	-0.31 ***	-0.666
Distance from Residence (in minutes)	16.54	62.46	45.92 ***	1.568
Same gender of mayor	0.73	0.70	-0.03 ***	-0.067
Same birthplace of mayor	0.19	0.02	-0.16 ***	-0.410
Same surname of local politician	0.07	0.01	-0.06 ***	-0.230
Observations	6,966	9,331		

Notes: The table shows the average characteristics of appointed auditors, before and after the draft appointment was introduced. the sample is restricted to the 3591 municipalities that I observe in both the pre and the post period. Column (3) shows the difference in means between columns (1) and (2), as well as the significance level (p-values are calculated using heteroskedasticity-robust standard errors). Significance: * significant at 10%; ** significant at 5%; *** significant at 1%,. *Experience as auditor* refers to the number of years the individual has previously served as municipal auditor, *Experience as accountant* refers to the number of years as CPA, *Re-appointed* is an indicator equal to 1 if the auditor serves for 2 consecutive terms in the same municipality, *Local resident* is an indicator equal to 1 if the auditor is resident in the same municipality where she serves as auditor, *Distance from residence* represents the distance in minutes between the municipality of appointment and the municipality of residence.

Table 5
The Effect of Auditor's Independence on Fiscal Sustainability

	Net Surplus		Debt Repayments		OBS Debt Repayments	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	16.45***	15.88***	0.0885***	0.0872***	-0.00218	-0.00293
× Post	[3.240]	[3.236]	[0.0140]	[0.0139]	[0.0253]	[0.0253]
Controls	No	Yes	No	Yes	No	Yes
Dep. Var Mean	-194.8	-194.8	65.29	65.29	3.255	3.255
Observations	114028	114028	114028	114028	114028	114028
Adj. R-sq	0.699	0.700	0.757	0.757	0.412	0.413

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation, but their mean is reported as non-transformed. All regressions include municipality, event time fixed effects, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table 6
The Effect of Auditor's Independence on Aggregate Spending and Revenue Choices

	(1) Current Exp.	(2) Capital Exp.	(3) Property Tax	(4) Income Tax	(5) Capital Rev.	(6) New Debt
Treated	0.000184	0.0423*	0.205***	0.00758	0.0445*	0.225***
× Post	[0.00309]	[0.0254]	[0.0434]	[0.0254]	[0.0266]	[0.0560]
Dep. Var Mean	875.0	465.3	158.7	43.73	398.8	123.4
Observations	114028	114028	99040	114028	114028	114028
R-sq	0.944	0.514	0.458	0.844	0.506	0.445

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms and transformed using the inverse hyperbolic sine transformation, but their non-transformed mean is reported in the bottom of the table. All regressions include municipality, relative event-time fixed effects, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table 7
The Effect of Auditor's Independence on Tax Capacity

	Property Tax Revenue			Property Tax Revenue, Collected			Property Tax Rate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated	0.204***	0.186***	0.186***	0.184***	0.171***	0.171***	-0.00309	-0.00278	0.00194
× Post	[0.0434]	[0.0432]	[0.0429]	[0.0421]	[0.0420]	[0.0416]	[0.0221]	[0.0222]	[0.0221]
TreatXPostXGBI		0.126***			0.111***			-0.00677	
		[0.0351]			[0.0339]			[0.0161]	
TreatXPostXUndeclared			0.165***			0.152***			-0.0460***
			[0.0337]			[0.0329]			[0.0173]
Dep. Var Mean	158.7			158.4			7.218		
Het. Var Mean		0.0268	0.0182		0.0267	0.0181		0.0268	0.0182
Het. Var SD		0.0209	0.0137		0.0209	0.0137		0.0209	0.0137
Observations	99040	98401	98878	98156	97531	97994	98316	97677	98154
R-sq	0.458	0.458	0.458	0.544	0.544	0.545	0.875	0.875	0.875

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All regressions include municipality, relative event-time fixed effects, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in log), gender, term in office and a dummy equal to one if the mayor was born in the municipality. *GBI* is the standardized share of "Ghost Buildings Intensity" at the municipality level, as calculated by Casaburi and Troiano (2016). *Undeclared* is the standardized share of houses that the National Tax Agency found to be undeclared by cross-checking tax declarations with cadastral data.

Table 8
Discretionary Appointment and Fiscal Sustainability in Pre-Reform Period

	Net Surplus		Debt Repayments		OBS Debts Restatements	
	(1)	(2)	(3)	(4)	(5)	(6)
Control of Appointment	-7.417*** [2.785]	-7.360** [3.198]	-0.0152* [0.00885]	-0.0179* [0.0100]	0.0199 [0.0247]	0.0244 [0.0291]
Second-Term Mayors	Yes	No	Yes	No	Yes	No
Dep. Var Mean	-205.8	-208.2	64.50	64.45	3.427	3.678
Observations	39329	24442	39329	24442	39329	24442
R-sq	0.727	0.760	0.789	0.813	0.457	0.515

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation. *Control of Appointment* is a dummy equal to 1 for years in which the mayor had appointed the auditor, and is constructed using exogenous variation arising from the difference between audit (3 years) and election (5 years) cycle. See Figure 4 and Appendix Figure A6 for further details. The sample is restricted to pre-treatment years. All regressions include municipality, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table 9
The Effect of Auditor's Independence on Fiscal Sustainability, by Pre-Reform Fiscal Stance

	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated × Post	10.90***	0.0350**	-0.176***
	[3.559]	[0.0150]	[0.0466]
Treated × Post × Below Median	12.42**		
	[5.616]		
Treated × Post × Below Median		0.106***	
		[0.0237]	
Treated × Post × Below Median			0.314***
			[0.0461]
Dep. Var Mean	-194.791	65.292	3.255
Sum of Coefs.	23.315***	.141***	.137***
Observations	114028	114028	114028
R-sq	0.709	0.769	0.442

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation, but their non-transformed mean is reported in the bottom of the table. $BelowMedian$ is a dummy variable equal to 1 if the value of the outcome variable in a given municipality was below the cohort-specific median value in 2011. All regressions include municipality, relative event-time fixed effects, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table 10
The Effect of Auditor's Independence on Fiscal Sustainability, by Pre-Reform Collusion Risk

	PANEL A: Local Auditor		
	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated × Post	10.35**	0.0876***	-0.0432
	[4.984]	[0.0226]	[0.0416]
Treated × Post × Local Auditor	0.461	0.0755*	0.110
	[7.483]	[0.0428]	[0.0791]
Dep. Var Mean	-180.278	61.281	4.072
Sum of Coefs.	10.811*	.163***	.067
Observations	62394	62394	62394
R-sq	0.727	0.734	0.453

	PANEL B: Corrupt Municipality		
	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated × Post	15.88***	0.0637***	-0.0330
	[3.483]	[0.0137]	[0.0249]
Treated × Post × Corrupt	-3.545	0.181***	0.282***
	[8.432]	[0.0551]	[0.102]
Dep. Var Mean	-194.791	65.292	3.255
Sum of Coefs.	12.332	.245***	.249**
Observations	114028	114028	114028
R-sq	0.715	0.770	0.442

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation, but their non-transformed mean is reported in the bottom of the table. *LocalAuditor* and *Corrupt* are proxies for the pre-reform collusion risk. *LocalAuditor* is equal to 1 if any of the municipality's appointed auditors before the reform was either born or resident in the municipality. *Corrupt* is an indicator flagging whether, in any given municipality, there was at least one investigation for corruption-related crimes from 2004 to 2013. All regressions include municipality, relative event-time fixed effects, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table 11
The Effect of Auditor's Independence on Fiscal Sustainability, Characteristics of the New Match

	PANEL A: New Entrants		
	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated × Post	2.311	-0.00252	-0.0174
	[6.719]	[0.0208]	[0.0382]
Treated × Post × New Entrant	21.29***	0.0439**	0.00218
	[6.974]	[0.0217]	[0.0386]
Dep. Var Mean	-224.226	70.303	2.540
Sum of Coefs.	23.606***	.041***	-.015
Observations	82041	82041	82041
R-sq	0.701	0.800	0.357

	PANEL B: Distance		
	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated × post=1	3.185	0.00218	-0.0161
	[5.301]	[0.0184]	[0.0322]
Treat × Post × At least 1 hr	27.76***	0.0517**	0.000473
	[6.281]	[0.0203]	[0.0357]
Dep. Var Mean	-224.226	70.303	2.540
Sum of Coefs.	30.941***	.054***	-.016
Observations	82041	82041	82041
R-sq	0.701	0.800	0.357

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation. $NewEntrant$ and $AtLeast1hr$ are two proxies of the level of auditor's independence in the new municipality-auditor match arising from random assignment. In Panel A, $NewEntrant$ is an indicator equal to 1 if the municipality is assigned as auditor an individual which has never been an auditor before. In this panel, the sample is restricted to municipalities below 5,000 inhabitants as only those can receive non-experienced auditors.$AtLeast1hr$ is an indicator flagging whether the driving distance between auditor's residence and the municipality is at least 1 hour (average distance is 65 min). All regressions include municipality, relative event-time fixed effects, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table 12
The Effect of Auditor's Independence on Fiscal Sustainability, by Electoral Accountability

	PANEL A: Electoral Cycle		
	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated × Post	15.61***	0.127***	0.0155
	[3.862]	[0.0176]	[0.0318]
Treated × Post × Pre-Election Years	-2.322	-0.106***	-0.0424
	[6.150]	[0.0256]	[0.0475]

	PANEL B: Term Limit		
	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated × Post	19.81***	0.130***	0.0344
	[5.496]	[0.0280]	[0.0503]
Treated × Post × Re-Electable	-4.116	-0.0520*	-0.0484
	[6.200]	[0.0307]	[0.0550]

	PANEL C: Council Majority		
	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated × Post	19.53***	0.106***	-0.0150
	[4.173]	[0.0181]	[0.0322]
Treated × Post × Strong Mayority	-9.860*	-0.0446*	0.0217
	[5.509]	[0.0240]	[0.0432]
Dep. Var Mean	-194.791	65.292	3.255
Sum of Coefs.	9.672**	.062***	.007
Observations	114028	114028	114028
R-sq	0.715	0.769	0.442

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation. $Pre-ElectionYears$ and $Term-Limited$ are two proxies for the strength of electoral accountability faced by mayor. $Pre-ElectionYears$ is equal to 1 if the mayor was in the last 2 years of her election cycle at the moment of treatment. $Term-Limited$ is a dummy equal to 1 if the mayor in power at the moment of treatment was facing a term limit and could thus not run for re-election. All regressions include municipality, relative event-time fixed effects, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table 13
The Effect of Auditor's Independence on Fiscal Sustainability, Spillover Effects

	Net Surplus		Debt Repayments		OBS Debt Repayments	
Panel A: Spillover effects	(1)	(2)	(3)	(4)	(5)	(6)
Treated	11.01***	10.34***	0.0653***	0.0631***	-0.0208	-0.0220
Neighbor=1 × Post=1	[3.848]	[3.843]	[0.0132]	[0.0131]	[0.0243]	[0.0243]
Controls	No	Yes	No	Yes	No	Yes
Dep. Var Mean	-218.5	-218.5	63.47	63.47	2.882	2.882
Observations	83424	83424	83424	83424	83424	83424
Adj. R-sq	0.699	0.699	0.769	0.769	0.405	0.405
Panel B: Excluding spillover effects	(1)	(2)	(3)	(4)	(5)	(6)
Treated	30.94***	29.47***	0.155***	0.151***	-0.00813	-0.0110
Neighbor=1 × Post=1	[4.929]	[4.921]	[0.0182]	[0.0182]	[0.0362]	[0.0363]
Controls	No	Yes	No	Yes	No	Yes
Dep. Var Mean	-218.5	-218.5	63.47	63.47	2.882	2.882
Observations	62263	62263	62263	62263	62263	62263
Adj. R-sq	0.700	0.701	0.768	0.768	0.401	0.401

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation, but their non-transformed mean is reported in the bottom of the table. *Above*5*k* is an indicator equal to 1 if the municipality's population in 2011 was above 5,000 inhabitants. All regressions include municipality, relative event-time fixed effects, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Appendix A: Additional Tables and Figures

Table A1
Impact of the Reform on Fiscal Sustaibaility, "Naive" DID, testing robustness heterogeneous treatment effects as in de Chaisemartin and D'Haultfœuille (2020)

	Net Surplus		Debt Repayments		OBS Debt Repayments			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Indep. Auditor=1	2.875	2.149	17.81***	16.12***	0.0866***	0.0804***	-0.0100	-0.0115
	[1.984]	[1.985]	[3.233]	[3.038]	[0.0123]	[0.0125]	[0.0229]	[0.0232]
Dep. Var Mean	-9.753	-9.753	-205.8	-205.8	64.47	64.47	3.435	3.435
Observations	50427	50427	50427	50427	50427	50427	50427	50427
Adj. R-sq	0.114	0.119	0.603	0.624	0.713	0.720	0.362	0.363
% ATTs with negative weights	29.5	29.5	29.5	29.5	29.5	29.5		
Sum of negative weights	-0.106	-0.106	-0.106	-0.106	-0.106	-0.106		
$\underline{\sigma_{fe}}$		12.62		0.06		0.009		
$\underline{\underline{\sigma_{fe}}}$		50.35		0.26		0.04		

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. The table reports coefficient estimates based on equation 1. All dependent variables are in per capita terms, $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation. All regressions include municipality, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A2
Impact of the Reform on Fiscal Sustaibaility, using the estimator developed by Callaway and Sant'Anna (2021)

	Net Surplus		Debt Repayments		OBS Debt Repayments	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	9.070***	7.494**	0.0695***	0.0587***	-0.0111	-0.00548
	[3.369]	[3.400]	[0.0134]	[0.0135]	[0.0300]	[0.0305]
Dep. Var Mean		-205.8		64.47		3.435
Observations	44824	44824	44824	44824	44824	44824
R-sq						

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. The table reports coefficient estimates based on equation 1. All dependent variables are in per capita terms, *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation. All regressions include municipality, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A3
Impact of the Reform on Fiscal Sustaibaility, replication of Table 3 in Barone et al. (2020)

	Net Surplus								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
treat4=1	-28.29***	-24.40***	-28.81***						
	[4.680]	[4.513]	[4.733]						
treat6=1				-33.19***	-30.36***	-33.62***			
				[4.891]	[4.742]	[4.989]			
treat8=1							-30.99***	-28.83***	-31.08***
							[4.861]	[4.743]	[4.949]
Dep. Var Mean	-341.0	-341.0	-335.8	-341.0	-341.0	-335.8	-341.0	-341.0	-335.8
Observations	54273	54273	53367	54273	54273	53367	54273	54273	53367
Adj. R-sq	0.844	0.846	0.842	0.844	0.846	0.842	0.844	0.846	0.842

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. The table reports coefficient estimates based on the following equation: x. The sample includes municipalities in special regions, i.e. never-treated units, as controls, for years 2008-2015.

Table A4
Impact of the Reform on Investment, by Investment category

	Investment							
	(1) admin	(2) police	(3) education	(4) culture	(5) tourism	(6) transport	(7) local public goods	(8) social
Treated	0.0719*	-0.0334	0.0952**	0.0726**	-0.0211	0.00414	0.0362	0.0726*
× Post	[0.0375]	[0.0396]	[0.0441]	[0.0319]	[0.0265]	[0.0451]	[0.0436]	[0.0392]
Dep. Var Mean	65.86	22.24	35.04	14.36	17.72	128.6	170.3	27.34
Observations	112320	60500	113004	113025	113024	112909	112756	113004
R-sq	0.485	0.471	0.343	0.365	0.431	0.386	0.453	0.318

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms and transformed using the inverse hyperbolic sign transformation. All regressions include municipality, year, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy controlling for extension of fiscal rules to municipalities below 5000 from 2014 onwards, a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A5
Predictors of Drafted Auditors' Rejecting Appointment

	Auditor Rejects Appointment				
	(1)	(2)	(3)	(4)	(5)
Long Distance	0.0664***	0.0653***	0.0600***	0.0594***	0.0600***
	[0.0125]	[0.0124]	[0.0122]	[0.0120]	[0.0119]
Female		0.0278**	0.0278**	0.0280**	0.0279**
		[0.0126]	[0.0127]	[0.0127]	[0.0127]
Age		-0.00371	-0.00296	-0.00312	-0.00290
		[0.00832]	[0.00847]	[0.00844]	[0.00842]
Age2		0.0000548	0.0000478	0.0000491	0.0000472
		[0.0000717]	[0.0000729]	[0.0000725]	[0.0000723]
New Entrant		-0.0164	-0.0173	-0.0170	-0.0170
		[0.0139]	[0.0141]	[0.0141]	[0.0141]
Experience		-0.00138	-0.00199	-0.00186	-0.00191
		[0.00775]	[0.00773]	[0.00775]	[0.00774]
Experience2		0.0000879	0.000106	0.000103	0.000103
		[0.000255]	[0.000251]	[0.000251]	[0.000251]
Mountain			0.0234	0.0212	0.0211
			[0.0169]	[0.0179]	[0.0180]
Sea			0.0375	0.0347	0.0358
			[0.0265]	[0.0275]	[0.0265]
Province Capital			-0.0536	-0.0553	-0.0501
			[0.0329]	[0.0332]	[0.0321]
Net Surplus Below Median				-0.0116	-0.0122
				[0.0126]	[0.0126]
Debt Repayments Below Median				-0.0154	-0.0153
				[0.0111]	[0.0111]
OBS Debt Repayments Below Median				0.0107	0.0107
				[0.00989]	[0.00975]
Corruption					0.0110
					[0.0167]
Mafia Infiltrations					-0.0570**
					[0.0264]
Constant	0.168***	0.208	0.183	0.196	0.190
	[0.00636]	[0.182]	[0.187]	[0.182]	[0.181]
Dep. Var Mean	0.202	0.202	0.202	0.202	0.202
Observations	6705	6705	6705	6705	6705
Adj. R-sq	0.115	0.118	0.119	0.119	0.119

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All regressions include $RegionXYearXPopulationSize$ fixed effects. These fixed effects account for the level at which the drafting is performed. The dependent variable, $AuditorRejectsAppointment$ is a dummy equal to 1 if the first drafted auditor rejects the appointment and 0 otherwise on a set of explanatory variables. $LongDistance$ is a dummy equal to 1 if the driving distance between auditor's municipality of residence and the municipality of appointment is larger than 60 minutes, $NewEntrant$ is a dummy equal to 1 if the auditor has never been a municipal auditor before the reform, $Experience$ measures the years of general auditing experience, $Mountain$ is a dummy equal to 1 if the municipality is located in the mountains, Sea is a dummy equal to 1 if the municipality is located on the coasts, $NetSurplusBelowMedian$ is a dummy equal to 1 if the municipality had a value of fiscal surplus below the median in 2010, $Corruption$ is a dummy equal to 1 if there was at least one investigation for corruption in the municipality, $MafiaInfiltrations$ is a dummy equal to 1 if there had ever been investigations for mafia infiltrations in the municipality.

Table A6
Changes in Auditors' Characteristics, Regression Analysis

	(1) Female	(2) Age	(3) Experience as Auditor	(4) Experience as Accountant	(5) Re-appointed	(6) Local Resident	(7) Distance	(8) Same Gender of Mayor	(9) Same Birthplace of Mayor	(10) Same surname of Local Politician
Treated	0.0436	0.247***	-0.771***	0.405***	-0.0797**	-0.874***	1.217***	0.0204	-0.542***	-0.341***
× Post	[0.0438]	[0.0418]	[0.0346]	[0.0419]	[0.0326]	[0.0447]	[0.0345]	[0.0432]	[0.0483]	[0.0542]
Dep. Var Mean	0.266	52.82	7.543	13.57	0.255	0.209	22.52	0.648	0.146	0.0508
Dep. Var SD	0.442	7.932	6.358	9.335	0.436	0.406	18.10	0.478	0.353	0.220
Observations	34521	34521	34521	34521	34521	34521	34521	34521	34521	34521
R-sq	0.793	0.784	0.879	0.798	0.844	0.827	0.814	0.762	0.717	0.662

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation, but their mean is reported as non-transformed. All regressions include municipality, event time fixed effects, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A7
The Effect of Auditor's Independence on Fiscal Sustainability, by Municipality Size

	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated × Post	16.62***	0.0331**	-0.0131
	[4.383]	[0.0140]	[0.0269]
Treated × Post × Pop. 5-15 k.	-2.501	0.158***	-0.0409
	[5.919]	[0.0400]	[0.0681]
Treated × Post × Above 15k	-2.545	0.205***	0.169*
	[7.997]	[0.0558]	[0.101]
Dep. Var Mean	-194.8	65.29	3.255
Observations	114028	114028	114028
R-sq	0.715	0.770	0.442

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation, but their non-transformed mean is reported in the bottom of the table. $Above5k$ is an indicator equal to 1 if the municipality's population in 2011 was above 5,000 inhabitants. All regressions include municipality, relative event-time fixed effects, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A8
The Effect of Auditor's Independence on Fiscal Sustainability, by Auditor's Residence, Full Sample

	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated ×	16.81***	0.0679***	-0.0176
Post	[3.627]	[0.0140]	[0.0256]
Treated ×	-6.807	0.0941**	0.0826
Post × Local Auditor	[6.824]	[0.0398]	[0.0734]
Dep. Var Mean	-194.791	65.292	3.255
Sum of Coefs.	10.002*	.162***	.065
Observations	114028	114028	114028
R-sq	0.715	0.770	0.442

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation, but their non-transformed mean is reported in the bottom of the table. $Pop.5-15k$ is an indicator equal to 1 if the municipality's population in 2011 was between 5,000 and 15,000 inhabitants. $Above15k$ is an indicator equal to 1 if the municipality's population in 2011 was above 15,000 inhabitants. All regressions include municipality, relative event-time fixed effects, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A9
Impact of the Reform on Fiscal Sustainability, by Geography

	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated ×	14.67***	0.0167	-0.0329
Post	[3.876]	[0.0124]	[0.0220]
Treated ×	-14.21**	0.123***	0.0740
Post × South	[6.549]	[0.0299]	[0.0575]
Treated ×	-2.078	0.0390	0.0797
Post × Center	[7.850]	[0.0319]	[0.0665]
Dep. Var Mean	-194.8	65.29	3.255
Observations	114028	114028	114028
R-sq	0.719	0.776	0.442

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation. All regressions include municipality, year, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A10
Impact of the Reform on Fiscal Sustainability, allowing for Region-Specific Nonparametric Trends

	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated	10.47***	0.0427***	0.0142
× Post	[3.411]	[0.0129]	[0.0255]
Controls	Yes	Yes	Yes
Dep. Var Mean	-194.8	65.29	3.255
Observations	114028	114028	114028
Adj. R-sq	0.696	0.772	0.415

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation. All regressions include municipality, year, election cycle fixed effects, region-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A11
Impact of the Reform on Fiscal Sustainability, including auditors' characteristics as controls

	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated	11.55***	0.0389***	-0.0155
× Post	[3.996]	[0.0142]	[0.0274]
Controls	Yes	Yes	Yes
Dep. Var Mean	-196.8	66.02	2.996
Observations	92877	92877	92877
Adj. R-sq	0.699	0.773	0.383

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation. All regressions include municipality, year, election cycle fixed effects, region-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A12
Impact of the Reform on Fiscal Sustainability, including municipal pre-treatment characteristics as controls

	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated	9.051***	0.0384***	0.0170
× Post	[3.371]	[0.0131]	[0.0261]
Controls	Yes	Yes	Yes
Dep. Var Mean	-194.8	65.29	3.255
Observations	114028	114028	114028
Adj. R-sq	0.703	0.774	0.415

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation. All regressions include municipality, year, election cycle fixed effects, region-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A13
Impact of the Reform on Fiscal Sustainability, by Pre-reform and Post-Reform Distance

	Net Surplus	Debt Repayments	OBS Debt Repayments			
	(1)	(2)	(3)	(4)	(5)	(6)
Treated=1 ×	3.715	1.703	0.000784	0.0158	-0.0257	0.0150
Post=1	[6.189]	[9.887]	[0.0196]	[0.0459]	[0.0346]	[0.0806]
Treat × Post	24.74***	38.15***	0.0445**	0.0864*	0.0111	-0.0543
× At least 1 hr	[7.114]	[13.52]	[0.0217]	[0.0522]	[0.0378]	[0.0944]
Dep. Var Mean	-226.061	-218.240	71.998	64.774	2.319	3.261
Observations	63475	18566	63475	18566	63475	18566
R-sq	0.696	0.722	0.818	0.727	0.358	0.350

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation. All regressions include municipality, year, election cycle fixed effects, region-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A14
Impact of the Reform on Fiscal Sustainability, Heterogeneity Analysis using TWFE model

	Net Surplus	Debt Repayments	OBS Debt Repayments
Indep. Auditor=1	8.127*	0.0627***	-0.0317
	[4.601]	[0.0191]	[0.0375]
Indep. Auditor=1	15.12**	0.135***	0.0419
× Local Auditor	[6.366]	[0.0240]	[0.0492]
Indep. Auditor=1	14.05***	0.0632***	-0.0183
	[3.167]	[0.0124]	[0.0230]
Indep. Auditor=1	12.73*	0.144***	0.0464
× Corruption=1	[6.828]	[0.0311]	[0.0656]
Indep. Auditor=1	10.29*	0.00338	-0.0364
	[5.987]	[0.0169]	[0.0310]
Indep. Auditor=1	8.101	0.0392**	0.0215
× noexperience=1	[6.293]	[0.0155]	[0.0274]
Indep. Auditor=1	12.17***	0.0608***	-0.0253
	[3.729]	[0.0145]	[0.0254]
Indep. Auditor=1	6.679	0.0377***	0.0246
× t_distant=1	[4.359]	[0.0135]	[0.0250]
Indep. Auditor=1	13.98***	0.0880***	0.0105
	[3.509]	[0.0141]	[0.0261]
Indep. Auditor=1	3.602	-0.0205	-0.0563**
× Pre-Election Years=1	[4.911]	[0.0150]	[0.0276]
Indep. Auditor=1	22.04***	0.0818***	-0.00647
	[4.854]	[0.0188]	[0.0339]
Indep. Auditor=1	-8.933*	-0.00318	-0.00894
× Re-Electable	[5.079]	[0.0179]	[0.0323]
Indep. Auditor=1	18.65***	0.0876***	-0.0110
	[3.718]	[0.0141]	[0.0256]
Indep. Auditor=1	-7.666*	-0.0197	-0.00492
× Strong Mayority	[4.593]	[0.0142]	[0.0259]
Indep. Auditor=1	-7.314**	0.0849***	0.169***
	[3.562]	[0.0130]	[0.0221]
Indep. Auditor=1	47.45***	-0.00850	-0.403***
× Below Median=1	[4.361]	[0.0136]	[0.0276]
Dep. Var Mean	-205.802	64.467	3.435
Observations	50427	50427	50427
R-sq	0.661	0.751	0.438

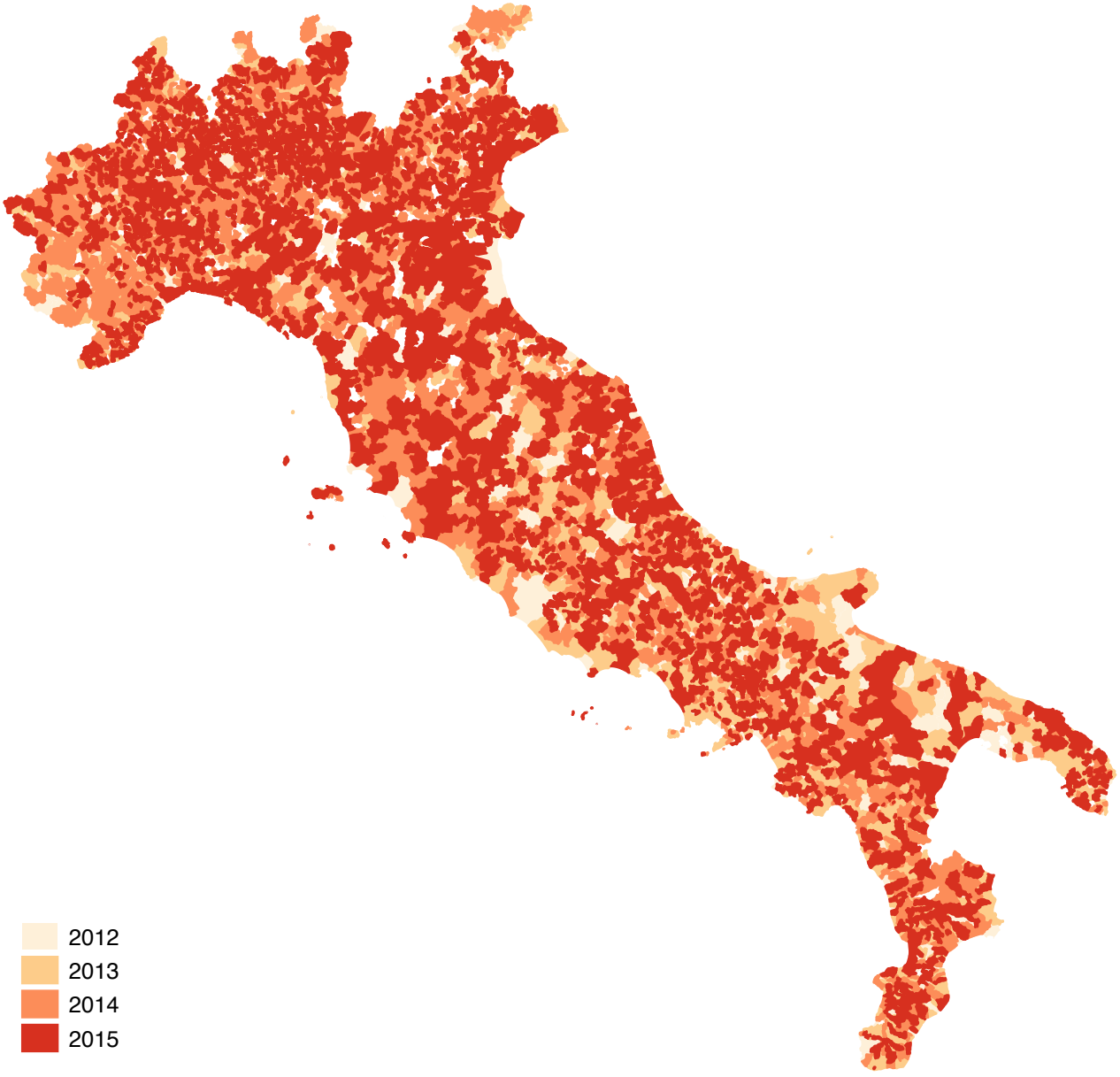
Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation. All regressions include municipality, year, election cycle fixed effects, region-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A15
Testing Data Manipulation Benford's law

	Net Surplus	Debt Repayments
	(1)	(2)
Treated=1	0.00364	0.00257
	[0.00408]	[0.00179]
Dep. Var Mean	0.0202	0.0241
Observations	36	36
Adj. R-sq	0.902	0.892

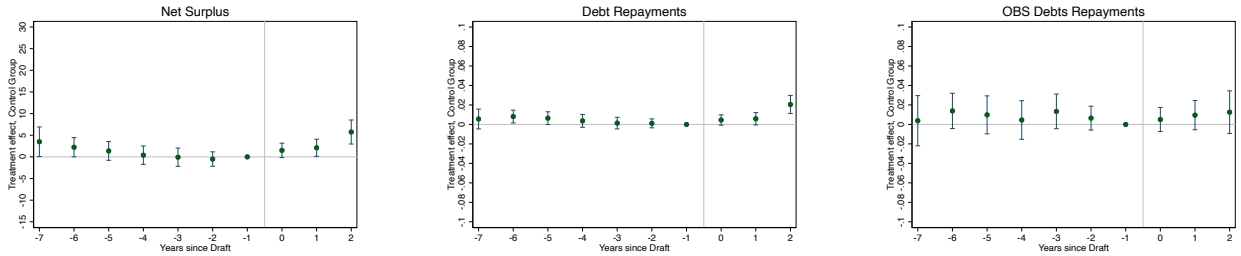
Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. *DebtRepayments* is transformed using the inverse hyperbolic sine transformation, but its non-transformed mean is reported in the bottom of the table.

Figure A1
Staggered Treatment, Geographic Variation



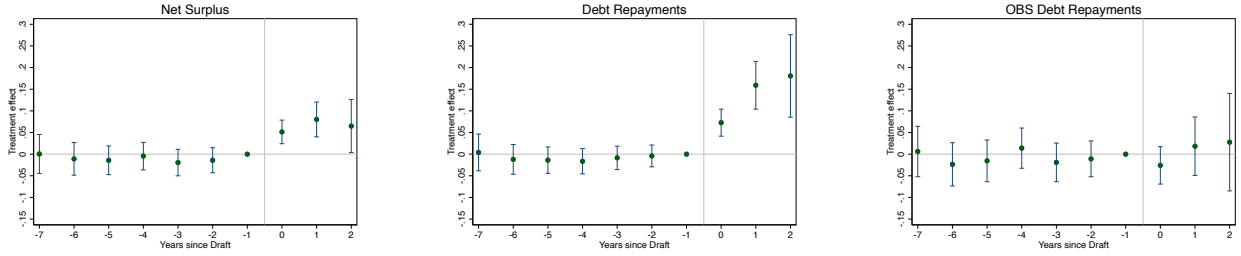
Notes: The figure shows the geographic variation in treatment timing. Darker gradation reflects later treatment timing.

Figure A2
The Dynamic Effect of Auditor's Independence on Fiscal Sustainability, Anticipation Effects in the Control Group



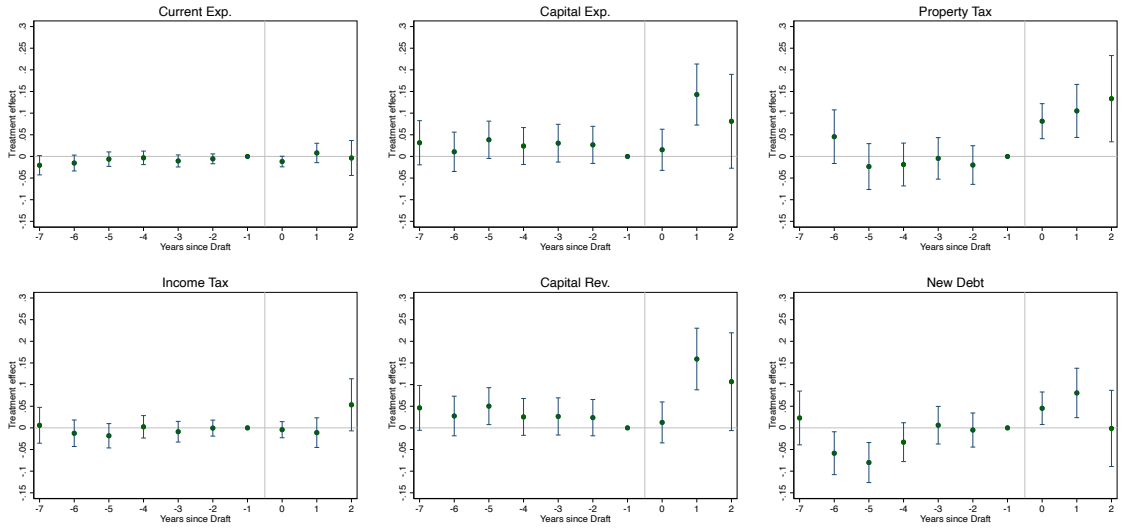
Notes: The graphs report the β_k coefficients and 95% confidence intervals estimated according to specification 3. Standard errors clustered at the municipality level. All dependent variables are in per capita terms, $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation. All regressions include municipality, population-bins-by-year fixed effects and election cycle fixed effects, as well as the following controls: a dummy for early termination of city council, mayor iage (in logs) gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Figure A3
The Dynamic Effect of Auditor's Independence on Fiscal Sustainability, Standardized Outcomes



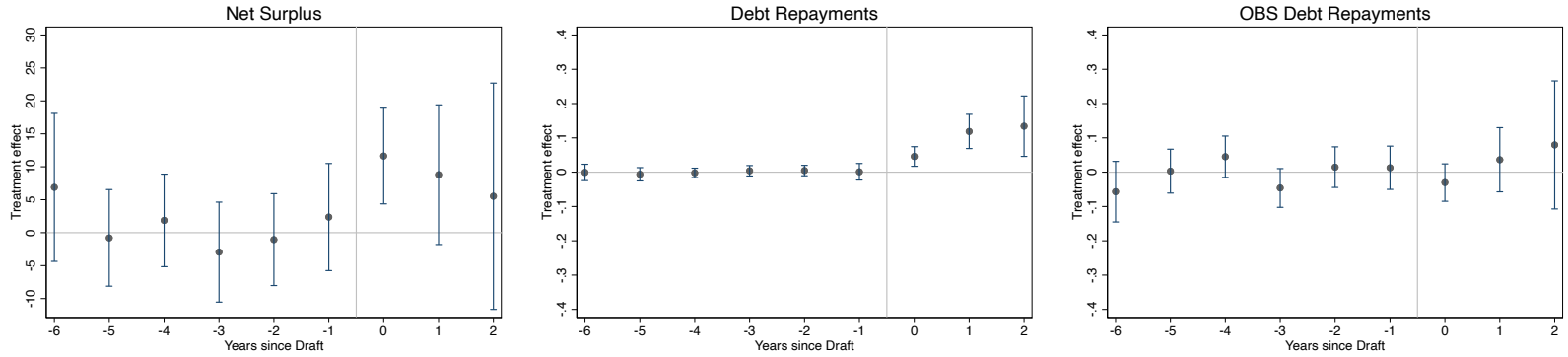
Notes: The graphs report coefficients and 95% confidence intervals estimated according to specification 3. Standard errors clustered at the municipality level. All dependent variables are in per capita terms, $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation. All regressions include municipality, population-bins-by-year fixed effects, relative time fixed effects and election cycle fixed effects, as well as the following controls: a dummy for early termination of city council, mayor iage (in logs) gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Figure A4
The Dynamic Effect of Auditor's Independence on Aggregate Spending and Revenue Choices, Standardized Outcomes



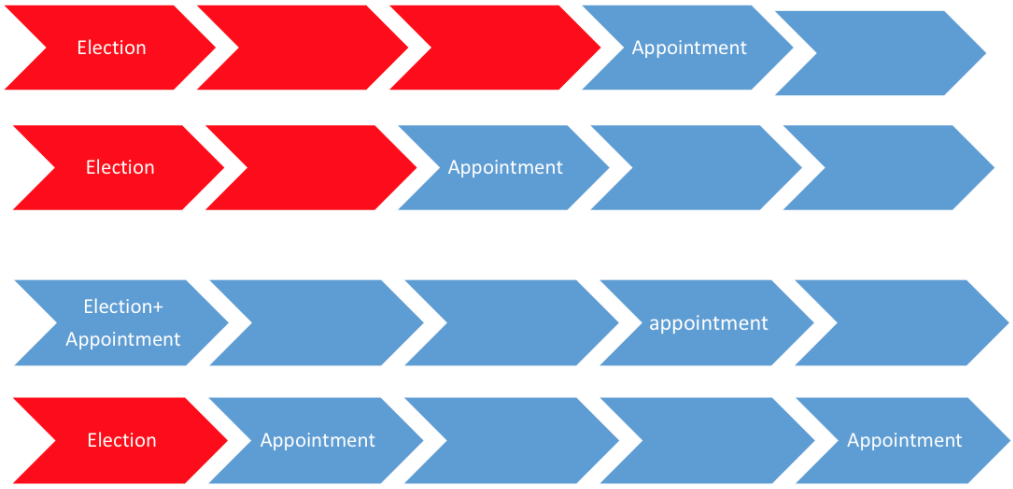
Notes: The graphs report coefficients and confidence intervals estimated according to specification 3. Standard errors clustered at the municipality level. All dependent variables are in per capita terms, and transformed using the inverse hyperbolic sine transformation. All regressions include municipality, year and election cycle fixed effects, and the following controls: dummy for use of special harmonised accounting system, a dummy for early termination of city council, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Figure A5
Event Studies, using de Chaisemartin and D'Haultfœuille (2020) methodology



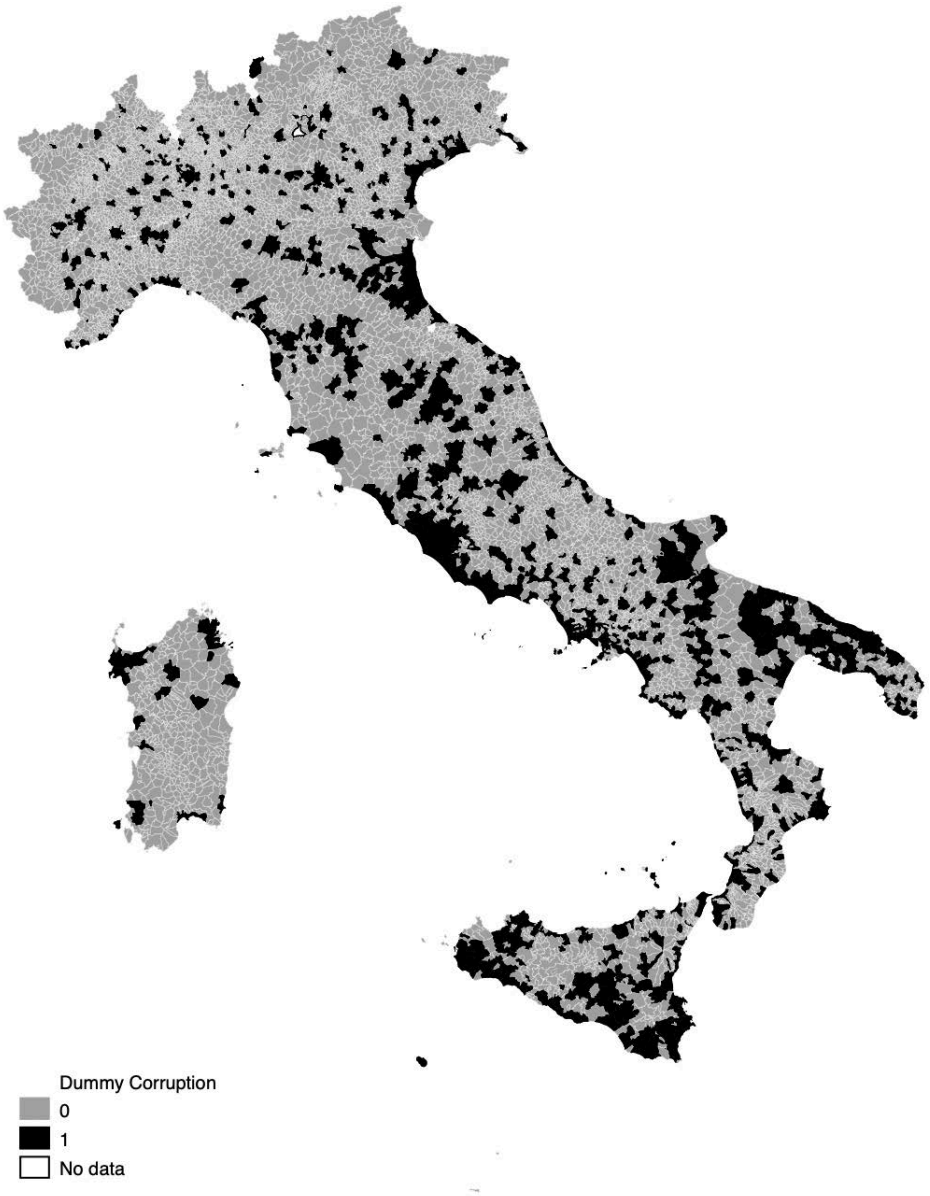
Notes: The graphs report coefficients and confidence intervals of the DID_M estimators estimated according to de Chaisemartin and D'Haultfœuille (2020) methodology using the did_multipleGT command in Stata. Standard errors clustered at the municipality level. All dependent variables are in per capita terms. $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation. All regressions include municipality, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Figure A6
Auditors' Appointment Control, Example



Notes: The figure shows an example of how the interplay beyween the audit and election cycle across municipalities creates variation in control of appointment by the mayor. Each of the rows represents a different municipality. In the first case (top row), the auditor was just appointed when the election takes place, so the newly elected mayor gets to appoint the auditor only after 3 years. In the third row, the opposite happens: the auditor cycle ends right after the election,and so the mayor gets to appoint a new auditor immediately after being elected.

Figure A7
Municipal Corruption Flags



Notes: The figure displays a map of the municipalities having the *Corrupt* dummy equal to 1. To construct this variable, I exploit restricted-access data from the *Sistema D'Indagine Interforze* (SDI), a centralized investigation archive that contains reports of all individuals investigated by any of the Italian police forces. The final data contains information, for each municipality, on the total number of investigation for all type of crimes in the years 2004-2013. I construct an indicator variable flagging whether, in any given municipality, there was at least one investigation for corruption-related crimes in the entire period.

Appendix B: Additional Institutional Details

Table B1
Auditor's Main Tasks *ex lege 239 TUEL & dlgs 118/2011*

Advising	Monitoring
Mandatory written opinions on:	**Audit and Certification of:**
mayoral three-years programmatic document	preliminary budget
budgetary planning tools	balance sheet
preliminary budget draft	prospectus of mayor's political campaign and entertainment expenses
management of public services	end-of-mandate mayoral report
creation or participation in city-owned-enterprises	prospectus on the respect of fiscal rules
assignment of public services management to private firms or city-owned-enterprises	prospectus on the respect of other limits to public spending
proposals of new debt undertakings	prospectus on the respect of limits to personnel costs
proposals of underwritings of derivatives and other exceptional financial instruments	prospectus on the respect of limits to advertising expenses
proposals of inclusion of off-balance-sheet debt in the financial statements	prospectus on the respect of limits to employees' training expenses
changes to the local tax code	prospectus on the respect of limits to expenses for public vehicles
changes to the local tax management regulations	prospectus on the respect of limits to new real estate acquisitions
re-assessments of un-collected credits	prospectus on the government participations in city-owned enterprises
financial recovery plans	three-year local procurement and public works plans
debt repayment plans	three-year hiring plan
extraordinary maintenance plans of local public buildings, roads and utilities	
	Verification of:
budget adjustments	respect of mandatory transparency regulations and transmission of data
anti-evasion tax collection plans	reliability of tax revenue projections
plans to achieve a more efficient management of public services	sustainability of debt repayment plans
plans to reduce personnel costs	existence of off-balance-sheet debts
three-year hiring plans	current account budget balance
three-year public works plans	capital account budget balance
three-year procurement plans	reliability of anti-evasion tax collection plans
	tax collection procedures
	public spending procedures and respect of payment times
	proper use of earmarked revenue sources

Notes: The table represents a summary of the main tasks of auditors, as regulated by italian law, *ex lege 239 TUEL & dlgs 118/2011* . Additional details can be found in Miani, Sequi and Di
Locali dell'ODCEC del Circondario del Tribunale di Cassino (2009).