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

Silvia Vannutelli*

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Abstract

Monitoring is a common tool used to mitigate agency problems. Monitors themselves, however, may be biased or corrupt, in particular if they feel obliged to please the party that appoints them. In this paper, I evaluate whether shifting control over auditor assignments improves monitoring effectiveness and impacts outcomes of the audited party. In 2011, Italy switched from allowing mayors to appoint municipal auditors to a system of random assignment, to strengthen oversight and ensure the financial soundness of municipal budgets. My identification exploits the reform's staggered introduction across municipalities in a generalized difference-in-differences setting. I obtain three main findings. First, treated municipalities increase their net surpluses by 9% and debt repayments by 8%, 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. Taken together, these findings provide novel quantitative evidence on the importance of independence in auditing, and highlight the improvement in outcomes that may result from changes in the design of monitoring institutions. **JEL**: H72, D73, G3, M42, H83

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1 Introduction

A common tool to overcome agency problems in decentralized organizations – whether corporate or governmental – is the use of monitoring by external auditors (Strausz (1997), Mookherjee (2012)). To be effective watchdogs, auditors should be independent from the audited party. Otherwise, they risk being corrupted, turning a blind eye to favor the interests of their client (Ronen (2010)). The lack of auditor independence has been cited as a cause in numerous scandals, involving both private (e.g., Enron, Lehman Brothers, WeWork, WireCard) and public entities (e.g., the bailouts of sub-national governments in Argentina and Brazil).

A natural source of this lack of independence is that the audited party itself is charged with appointing the auditor. While this discretionary system may have its benefits - for example reducing information frictions and facilitating cooperation - it leads to inherent conflicts of interest. Even if not deliberately corrupt, auditors are likely to be unconsciously biased toward the party that hires them (Bazerman, Morgan and Loewenstein (1997)). Despite the perception that discretionary appointments corrupt the audit process, reforms to sever the links between the auditor and audited party are scant (most likely because both the auditor and audited party may be motivated to lobby against any change to the status quo (Moore, Tetlock, Tanlu and Bazerman (2006)).[1]

In this paper, I evaluate the consequences of independent auditing for the financial outcomes of municipal governments in Italy. I exploit a unique natural experiment, resulting from a reform to the system governing the appointment of auditors overseeing municipal governments' budgets. The main feature of the reform was a switch from a discretionary system in which mayors could choose their auditors, to a random assignment mechanism.

Italian municipalities are required to draft budgets and balance sheets that need to be reviewed and approved by professional auditors, who thus act as an intermediate layer of oversight between municipalities and the central monitoring performed by the National Court of Accounts.[2] The audit system aims to ensure responsible spending practices, in compliance with national fiscal rules, which dictate no deficits and require effort towards repayment of outstanding debts. As distinct from 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

[1] The only existing example is documented in the work by Duflo, Greenstone, Pande and Ryan (2013), which experimentally studies the effect of randomly appointing third-party auditors for firms' compliance with environmental regulations in two Indian regions.

[2] The National Court of Accounts is the Italian analog to the U.S. Government Accountability Office and the U.K. National Audit Office.

of municipalities and can thus influence municipal financial decisions ex-ante.

The reform, enacted by the national government in August 2011, established that auditors be chosen centrally via random selection from a list of participating certified public accountants. The motivation for the reform was to increase auditors' independence and to disrupt potential collusion between the mayor and their discretionally-appointed auditors. This change was part of a larger package of austerity measures that aimed to reduce public spending and comply with the fiscal adjustment effort required by the European Union.[3] Facing pressure to reduce national debt and improve credibility on the global financial markets, local governments' fiscal health became increasingly important in the national government's perspective.[4]

The reform had a rapid and significant impact on the characteristics of municipal auditors: the share of auditors resident in the municipality they were hired to monitor was reduced from 31% before the reform to just 0.1 % afterward.[5] Similarly, the share of auditors reappointed for a second term dropped from 57 % before the reform to 0.1 %.

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 causally identify the effect of the change in the appointment system on the financial health of municipalities. To address concerns raised by the recent econometric literature on staggered-adoption DiD designs (Athey and Imbens (2018), Goodman-Bacon (2018), de Chaisemartin and D'Haultfoeuille (2019)), I adopt a stacked-by-event design (Autor, Donohue and Schwab (2006), Cengiz, Dube, Lindner and Zipperer (2019), Deshpande and Li (2019), Fadlon and Nielsen (2019)) that ensures that my treatment effects are estimated only off comparisons

[3]This in turn was a response to the sovereign debt crisis and financial market pressures resulting from rising Italian debt. 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.

[4]In Italy, as 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.

[5]To limit travel costs, the drafting of auditors is performed within regional strata, so that that only auditors resident in the same region can be drafted for appointment in a given municipality.

of units switching into treatment to not-yet-treated units.[6] As a robustness, I also apply the alternative estimator proposed by de Chaisemartin and D'Haultfoeuille (2019).

My analysis proceeds in three steps.

First, I look at the effect of auditors' independence on municipal financial health. I find that, upon the arrival of a draft-appointed auditor, municipalities increase their expenditure in debt repayments by 9% and their net budget surplus by 8%.[7] This corresponds, respectively, to an increase of €16 per capita in net surplus and €5 per capita in debt repayments in the average municipality. To give a sense of magnitudes, total annual per-capita spending in the average municipality is equal to €1600 per capita, so the increase in the net surplus is about 1% of the overall annual budget. The magnitude of this effect is comparable to the findings of Grembi, Nannicini and Troiano (2016) in analyzing the effect of relaxing fiscal rules in Italy in 2001.

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.

Second, I investigate the margins of adjustment. 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 expenditures), or a combination of the two. This is not a neutral choice, as previous research has shown that tax-based adjustment is likely to have a more negative impact on output than expenditure-based ones at the macro level (Alesina and Ardagna (2013), Alesina, Favero and Giavazzi (2019)). My results show that municipalities do not cut spending, but instead improve their fiscal stance through revenue-based adjustment, and specifically by increasing revenues from the local property tax of over 20 %, an increase corresponding on average to €30 per capita.[8]

Third, in my final set of analyses, I perform a series of heterogeneity tests with the objective of better understanding the set of mechanisms through which the reform most plausibly

[6] My methodology is close to the one used by Deshpande and Li (2019). In particular, I create a separate dataset for each of the treatment "cohorts". 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, and event-time dummies are specified relative to the specific year of treatment for that cohort. I then append all datasets together and estimate a DiD regression in which I can separate control for both calendar and event-time trends.

[7] I also investigate the response of off-balance-sheet debt repayments, which represent a rare but critical indicator of potentially profound budget imbalances. While I do not detect any effect on average, significantly diverging patterns of treatment effects emerge when looking at heterogeneity tests, which collectively suggest it represents an important proxy for inappropriate practices.

[8] This is likely to produce a larger negative fiscal multiplier than a counterfactual adjustment based on expenditure cuts (Alesina et al. (2019)). On the other side, given the rigidity of current spending, expenditure cuts would have resulted in cutdowns on investment, negatively impacting public good and public service provision.

operates. First, I investigate how the treatment effect varies with pre-reform financial health. Intuitively, lax or corrupt monitoring might allow for more irresponsible financial behavior and thus greater scope for improvement under more active auditing. Consistent with this story, I find that the treatment effect of independent auditors are up to four times as large in municipalities with worse financial conditions before the reform.

I also look at more direct proxies of collusion between auditor and mayor 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 which 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 investigate the role of the characteristics of the post-reform municipality-auditor match. In particular, one would expect to observe the strongest 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. To proxy for these types of dynamics, I look at whether the newly appointed auditor is an"outsider", meaning that (a) she has never been an auditor before or (b) she is resident in a municipality that is at least one hour away from the audited one. Investigating heterogeneity along these margins also allows me to speak about the trade-off between discretion and collusion. In fact, more distant auditors, as well as outsiders, are also less likely to be knowledgeable about the municipality and the appropriateness of financial practices. In this sense, it is not ex-ante clear what we would expect to observe. Both measures deliver a similar message: treatment effects are largely driven by municipalities that receive an auditor that is an outsider.

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 central government's objectives. Secondly, heterogeneity analyses provide suggestive evidence that the effects operate via breaking up collusive ties between auditors and mayor, thus fostering more effective monitoring.

This paper contributes to the literatures that investigate the tradeoffs involved in delegation in complex organizations, and the role of monitoring as a tool to solve agency problems.

A large theoretical literature models the design of optimal monitoring mechanisms in the presence of collusion between supervisor and agent (Tirole (1986), Strausz (1997),Rahman (2012),Khalil and Lawarrée (2006), Celik (2009), Ortner and Chassang (2018)). I provide an empirical test of how changes in the information asymmetries and bargaining power of the supervisor vis-a-vis the agent affect the potential for collusion (Mookherjee and Tsumagari (2018), Khalil, Lawarrée and Yun (2010)). In fact, my results are consistent with the predic-

tions of a model in which strong collusion and extortion possibilities exist between an agent and a partially informed supervisor. The reform shifts bargaining power in favor of the less informed party (the auditor), thus allowing the principal to introduce frictions within the coalition and reduce rent-extraction from the agent (the audit target), and benefits from the information provided by the supervisor to reduce the costs of collusion.

Despite the vaste theoretical debate, empirical evidence on the effects of altering agency relationships in monitoring institutions is scant. In their seminal paper, Duflo et al. (2013) analyze the effect of randomly assigning third-party auditors to 236 industrial plants in Gurajat, India, for a period of two years. My work builds on their findings by providing evidence on audit reforms in a distinct setting, one in which auditor independence plausibly has somewhat different implications. First, the audit targets in my setting are elected officials, rather than private firms. While the latter is a more straightforward case of regulatory enforcement, monitoring of elected officials by higher levels of government is complicated by political economy considerations (e.g. electoral incentives and common pool problems). Second, I look at a different form of auditing. Distinct from third-party auditors examined by Duflo et al. (2013), who verify ex-post firm compliance annually, 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, ambiguity and lower verifiability, making collusion problems more likely to arise and more difficult to restrain. Third, my analysis evaluates the consequences of a nation-wide reform involving over 6,000 municipalities, thus corroborating the external validity of Duflo et al. (2013)'s findings outside of a randomized controlled setting.[9]

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. In particular, my paper is closely related to work that investigates the use of fiscal rules as a tool to solve the so-called "soft budget constraint problem" (Kornai, Maskin and Roland (2003), Bouton, Lizzeri and Persico (2016), Azzimonti, Battaglini and Coate (2016)).Recent theoretical contributions have proven how fiscal rules can be effective in solving trade-offs of commitment versus flexibility, but also that enforcement is crucial for their efficacy (Halac and Yared (2019), Dovis and Kirpalani (2020)). There is, however, scant empirical evidence on the channels through

[9]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, 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 estimation design (a two-way fixed effect model, potentially leading to bias from negative weighting problems (Goodman-Bacon (2018)).

which fiscal rules are enforced, and I help to fill this gap, by documenting the role played by independent auditors in improving enforcement.[10]

Fourth, by investigating how changes in monitoring systems translate into changes in local governance outcomes, my paper also contributes to research that examines the role of monitoring as a tool to improve public service delivery and limit corruption/mismanagement of public funds. While several papers have documented the effects of ex-post anti-corruption audits (Olken (2007),Ferraz and Finan (2011),Avis, Ferraz and Finan (2018), Bobonis, Fuertes and Schwabe (2016), Chu, Fisman, Tan and Wang (2020)), very little is known about how ex-ante, more persistent forms of monitoring affect local governance. My results thus offer important insights for the personnel economics of the public sector, and on the tradeoffs involved in the design of effective monitoring mechanisms.

Finally, a large body of accounting literature has investigated the challenges posed by the conflicts of interest that arise in the context of private-sector auditing (Moore et al. (2006), Lennox and Wu (2018)). Two debated policy tools to address these issues are mandatory audit firm rotation and public audit oversight. While some prior work has analyzed the effects of mandatory rotation and public oversight on audit quality using reforms, they frequently obtain conflicting results (Gipper, Hail and Leuz (2020), Gipper, Leuz and Maffett (2019)). Furthermore, the scope of the analysis is limited by the high concentration in the U.S. and global audit market.[11] While focusing on the public sector, this paper contributes to this strand of the literature by showing the effects of a form of mandatory rotation that is stronger than the current US standard for public firms and more significant in scope, given the highly decentralized characteristic of the market for public-sector auditors in Italy.

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

[10] In particular, in the Italian context, several papers have looked at the consequences of fiscal rules for local public spending (Grembi et al. (2016), Bonfatti and Forni (2019)), firms' dynamics (Coviello, Marino, Nannicini, Persico et al. (2017)), corruption (Daniele and Giommoni (2020)) or electoral outcomes (Gamalerio (2020)). They all exploit 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.

[11] In the U.S., in 2010, the "Big Four" audit firms (Ernst & Young, Deloitte, KMPG and PwC, were responsible for 67 % of publicly traded companies' audits and collected 94 % of audit fees (Gerakos and Syverson (2015)).

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 on 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.[12] 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[13], 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.[14] It is important to note that in Italy,

[12] In particular, municipalities can take out new debt only if the total amount of debt service paid on past debt and new debt does no 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 municipal debt is

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

[14] From 2017 onwards, DSP has been replaced by the "zero-deficit rule", which became part of the Italian constitution from 2012 and is now applied both at the national and at the local level. Under the new rule, local entities are required to reach a zero-deficit rule on the 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 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

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.[15] 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 intergovernmental 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 on fiscal monitoring procedures. Since 1990, all municipal budgets need to be audited and certified by a board of auditors. The board is composed of one member for municipalities with less than 15,000 inhabitants, and of three members for larger municipalities. The board is nominated with a majority vote of the city council[16], 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.[17] Auditors' compensation is set by the city council at the moment of the appointment.

From its very first inception, the board of auditors was assigned a double role of both monitoring and advising. In particular, the board is in charge of 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. 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

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).

[15] 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`

[16] 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

[17] Further restrictions are imposed depending on the 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.

judicial procedure against municipalities that failed to comply with the rules or which display improper financial practices. In practice, the Court frequently acts through recommendations and other informal acts that warn municipal governments, while the number of real judicial procedures is very low.

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[18].

2.2.1 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 .[19] 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:[20]

- Up to 4,999 inhabitants: 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 municipal auditor at least once before.
- 15,000 and above: shall have been certified public accountants for at least 10 years and have been appointed municipal auditor at least twice before.

Due to administrative constraints and time needed to form the public lists, the new drafting procedure entered into effect from December 10, 2012.[21] The reform does not apply to the 5

[18] 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.

[19] 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

[20] Note that, before the reform, there was no increasing experience requirement to become auditor in large municipalities, except for being a CPA.

[21] On November 27, 2012, the Ministry of the Interior published the final version of the list from which auditors are drawn. A few days later, on December 4th 2012, the government published the announcement that the new procedure will be effectively in place from December 10th, the date of the first draw. At the time of the announcement some municipalities had already started their discretionary selection process but were forced to switch to the drafting. Instead, some municipalities that appointed their new auditors just before the announcement, i.e. up to December 3th, were the last to be treated at the end of 2015.

"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.[22] My analysis is therefore limited to the sample of 6627 municipalities in the 15 "Ordinary" regions.

The random drafting procedure is carried out by the local prefectures[23], via a standardized informatized 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.[24] 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 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 not ineligible for the specific appointment[25]. The formal appointment notice also contains the wage offered to the auditor for the three-year mandate.[26] This aspect of the auditors' appointment was not affected by the reform, thus mayors retained control over wage setting for auditors. Note that the municipal council is not required by law to contact the auditors before the appointment, but it is very common for municipalities to contact the first candidate and check whether she would accept the appointment at the offered wage. In fact, 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 inappropropriate. 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

[22]In 2016, Friuli Venezia Giulia, Sardinia, and Sicily adopted laws that introduce 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 one 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.

[23]Prefectures are the local offices of the Ministry of the Interior. Mutuated from the French model, they officially represent the National Government in each Province. In total, there are 103 prefectures

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

[25]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;l 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.

[26]Notably, auditors' compensation is subject to nationally mandated population-based maximum caps, but no legal minimum floor.

appointment.

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 national sovereign debt.[27]

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

[27] 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.

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 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 (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.[28] 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 repayed 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 to the rules, $OBSDebtRepayments$ are a relatively rare phenomenon, and in fact 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.[29] All spending variables, which are always

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

[29] 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 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.

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.

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 exact draft dates and identities of all the drafted candidates for each draft conducted from 2012 onwards. 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 the vast majority of municipalities, those with population up to 15,000 inhabitants, three potential candidates are drafted. I then matched this database with another database containing information on the appointed auditors, in order to check if the first of the drafted candidates accept the appointment or if the supplementary candidates are appointed.[30] 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 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 any information about auditors' appointments for the pre-reform period. In particular, 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.[31]

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

[30] In very few cases (3% of municipalities) there needs to be another draw as not one of the candidates accepted the position.

[31] In particular, 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.

election dates, 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 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 along 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 entered officially in place from 2016 onwards. The 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 2.2.1, 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 who 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 aspect 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.[32] Because municipalities had appointed auditors at different points in time, treatment timing is plausibly exogenous. My design, therefore, compares municipalities who are treated earlier to municipalities who 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, 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 (2018), de Chaisemartin and D'Haultfoeuille (2019), Borusyak and Jaravel (2017)). In particular, the 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 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 control.[33]

[32] A concern would arise if mayors could differentially select into treatment, by, for example, manipulating the auditors' appointment date in order to delay the entry into effect of the reform. However, mayors cannot affect the auditors' appointment date nor auditors' term length in any way, thus selection into treatment timing is unlikely to be a concern.

[33] 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

Second, in every dataset, I create event-time dummies relative to the year of treatment.[34] Note that municipalities that experience treatment in 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 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;[35] $Post_{mt}$ 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)).Note that 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, compared to the change in outcomes of control municipalities, who have yet to be treated. I estimate

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.

[34]For example, in the dataset for the first cohort of treatment, event-time dummies are defined in time relative to 2012.

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

treatment effects up to three periods from treatment onset (i.e. $k = 2$) thus covering the entire term of the draft-appointed auditor.[36] The matrix of time-varying controls X_{mt} includes 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.

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

[36] 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 in 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 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 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.

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 standard difference-in-differences assumption only requires 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.[37]

Another source of potential concern is the presence of anticipatory behavior, due to the fact that, at least in theory, municipalities knew exactly the timing of treatment.

The first thing to note in this respect is that, if anything, anticipatory effects would tend to bias my estimates downward, 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. In this sense, my β estimates could be considered as conservative lower-bound estimates of the true effects in the presence of anticipatory effects. Second, the use of a stacked-by-event design allows me to control separately for both event-time trends (the terms $\sum_{k=-7}^{k=2} \beta_k * D^k$ in equation 3) and calendar-time trends (δ_t). This 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).[38]

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 only significant change that occurred in 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.

[37]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.

[38]In my stacked-by-event design, I have a control group made of units which will experience the treatment only later in time. 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.

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' finances in other ways beyond the change in auditor's appointment.[39] However, it is important to remember that these changes affected contemporaneously all municipalities, while the change in auditors' appointment 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 changes related to the reform is captured by the presence of calendar time effects in my regressions.

5 Results

5.1 Fiscal Sustainability under Draft Appointment

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), the β_{DD} coefficient, I investigate the effect on $NetSurplus$. Upon the arrival of an independent auditor, treated municipalities increase their Net Surplus by €16 per capita, an increase of 8% relative to the pre-treatment mean. In column (3) and (4), I look at the $DebtRepayments$. 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 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 A2 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. Sec-

[39] 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.

ondly, the figures highlight the presence of heterogeneous dynamics of the treatment effect over time. The treatment effect for the $NetSurplus$ is fairly constant across the three years of the auditor terms, while the effect on $DebtRepayments$ is increasing over time. Finally, the figure confirms the absence of any significant effect for $OBSDebtRepayments$, albeit point estimates display a slightly increasing pattern over time.

5.2 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 side, 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 improvements in current revenues by significantly increasing the local property tax, which rises by over 20%, for an amount that corresponds on average to an increase of over €30 per capita.[40] On the other side, no significant change

[40]Specifically, I look only at revenues from property tax on properties that are not owner-occupied. In fact, 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. The distinction between revenues from owner-

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 investment, 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 A3 displays results from event-study specifications for the outcomes in Table 6. Looking at the first row, no significant trends could be detected. On the other hand, some interesting dynamics emerge. In particular, 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, and specifically by increasing revenues from the local property tax.[41] 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)).

Secondly, 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 negative and sustained trend towards a contraction of the investments that occurred from 2007 onwards, as municipalities achieved the required adjustments of the DSP pact by mainly cutting investments (Chiades and Mengotto (2015)).

Taking stock of the results presented so far, I argue that independent monitoring by draft-assigned auditors indeed improves fiscal sustainability, with municipalities running higher surpluses via an increase in local property tax, and it also spurs municipalities to allocate more funds to investments.[42]

occupied and other properties is not available for 2007, the first year of sample, and therefore the analysis for this variable is restricted to the window 2008-2015.

[41] When investigating the channels behind the increase in revenues, I do not detect any significant change ni the local property tax rate, thus suggesting that the effect is driven by an expansion of the tax base.

[42] 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 A2 , I look at total expenditures for these eight categories as dependent variables. Interestingly, we see that the

6 Robustness

6.1 Alternative Estimator

To corroborate my analysis, I apply the alternative estimator recently proposed by de Chaisemartin and D'Haultfoeuille (2019), which is robust to negative-weighting issues.[43]. 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.[44] 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, $\underline{\underline{\sigma}}_{fe}$, 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. Nevertheless, as a further robust test, I estimate the alternative Wald estimator proposed by de Chaisemartin and D'Haultfoeuille (2019), 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.[45] Similarly, one can also compute $DIDm$ placebo estima-

increase in expenditures is not homogeneously distributed. Only four categories display significant increases: general administration, education, culture, and social welfare.

[43]For further details, see, in particular, section 5.2 of de Chaisemartin and D'Haultfoeuille (2019)

[44]I use the $twowayfeweights$ command, developed by the authors, and available in STATA repository.

[45] 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.

tors looking at outcomes' evolution in pre-treatment periods, as well as the different dynamic treatment effects over time. I present results for the full dynamic specifications in Appendix Figure 4. Results look very similar to the ones in Figure A2 .

6.2 Fiscal Sustainability under Discretionary Appointment

To corroborate my results, I investigate whether, in the pre-reform period, granting discretion in auditors' appointment 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 5 and Appendix Figure A4 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 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 have or have not appointed the current auditor. Furthermore, mayors in their second term of office always have control of appointments. 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. Results are displayed in Table 11. Notably, 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 responsible way. It is interesting to compare the magnitude of the effect of the arrival of an

independent auditor in Table 5 with the effects obtained in Table 11, 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 discretionary appointment, 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, results go in the same direction as my previous findings. From a policy perspective, they also provide an alternative "policy tool" to achieve fiscal consolidation, which is the introduction of a gap between the political and the audit cycle.

7 Heterogeneity

In this Section, I perform a series of tests for heterogeneity to better understand why the reform was effective. I focus on three different margins. First, I investigate the role of pre-reform characteristics. In particular, one would expect to observe the largest treatment effects in municipalities that were in poor fiscal stance and where collusion between mayor and auditor, impairing auditor's activity, was more likely to take place before the reform. Then, I investigate the role of the characteristics of the post-reform municipality-auditor match. In particular, one would expect to observe the strongest 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 the auditor and mayor or 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 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 who were running excess surpluses closer to a balanced budget. In Table 7 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 instead construct an indicator equal to 1 if the municipality 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. Interestingly, 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 and significant, and very large in magnitude. On the other side, 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, $OBSDebtRepayments$ represents 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; 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 effect 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 $OBSDebtRepayments$.

7.2 Pre-Reform Collusion Risk

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, Bertrand and Burgess (2018), Xu (2019)). Table 8, Panel A displays the results, where I in-

clude 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 side, 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 somewhat proportional to the underlying size of OBS Debts) and upon the arrival of an independent auditor were more likely to restate them.

Second, 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*)[46]. The final data contains information, for each municipality, 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[47]. Appendix Figure A5 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 factors that should be, in theory, under 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

[46]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

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

to collude with a local auditor to cover illicit practicesTable 8, 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 4 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 side, 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 side, the significant increase in $OBSDebtRepayments$ in corrupt municipalities suggests the presence of a large amount of previously hidden OBS Debts in these corrupt municipalities.

7.3 New Auditor-Municipality match

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 appointment, the pre-reform market was characterized by a relatively limited number of auditors who perform audit tasks for multiple municipalities.[48] The new draft system allowed many outsiders to add their names to the list for the draft and have a chance to enter the market, and this changes 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. The law, however, imposes some minimal pre-reform audit experience requirements for larger municipalities, and thus the selection channel might operate only in municipalities up to 5,000 inhabitants, that can receive a complete outsider as an auditor. On the other side, the reform is likely to change the behavior of existing auditors due to the gained independence. 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. Even if random assignment per se weakens the bargaining power of the agent (the mayor) by taking away from him the choice of both auditor's identity and auditor's renewal, if random assignment by chance recreates a situation that is similar to the pre-reform appointment

[48]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 to market concentration.

(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 independence , I leverage the fact that I have information on the auditor's residence. I, therefore, 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 potentially to 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 local (so distance was 0), thus suggesting that indeed distance matters for audit outcomes. I, therefore, do three different types of sample splits. First, I repeat my main analysis but separately for a) municipalities below 5,000 inhabitants, b) municipalities above 5,000 inhabitants.[49] In fact, in municipalities above 5,000 inhabitants, the selection channel is shut off by construction, as only auditors with experience can be drafted. Table A4 presents the results. Then, for the subsample of municipalities below 5,000, I split the sample between those that receive an outsider as an auditor and those that receive an auditor with previous experience. This allows me to further distinguish between the two alternative channels. Table 9, Panel A displays the results. Finally, in Panel B and C of Table 9, I look at the heterogeneity based on distance, separately in municipalities above and below 5,000. In those above 5,000, this should matter more, as all auditors are necessarily experienced and thus more likely to have previous connections. Contrary to the expectations, however, I do not detect any significant differential effect of higher distance in larger municipalities, while it seems to matter much more in smaller ones.

7.4 Electoral Accountability

Finally, I investigate the interplay between the auditor's appointment and electoral accountability. On one side, 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 side, local interests may frequently clash with national interests, and thus local accountability may push mayors in a direction, particularly if the push towards

[49] In Appendix Table A6 , I further split municipalities above 5000 to distinguish between those above and below 15,000 inhabitants, as those above 15,000 inhabitants have a three-person audit committee and have even stronger experience requirements (at least 2 past auditor mandates).

fiscal sustainability from the national government comes at the cost of cutting expenditures for local services or raising higher taxes for local citizens. 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.[50] 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 10. 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 10. 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.

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 conflict-of-interest problems in monitoring. There are two important policy takeaways. One pertains to the cost-effectiveness of the reform.

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

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 identity of the auditors. As distinct from other types of reforms, the auditors here are not central government's bureaucrats, nor are they village members; 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 also in other settings.

While arguably a strength of this article is to provide direct evidence of the reform 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 auditor's 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 et al. (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 over-reacted 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 act 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 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 has for the auditors' market. The reform significantly altered auditors' career concerns and reputational incentives, so a natural question to investigate is how would auditors react. I plan to shed light on this question in future work 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.

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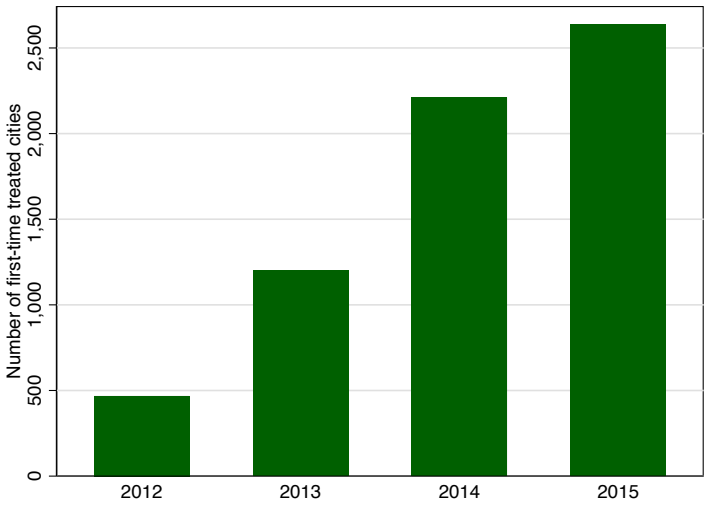
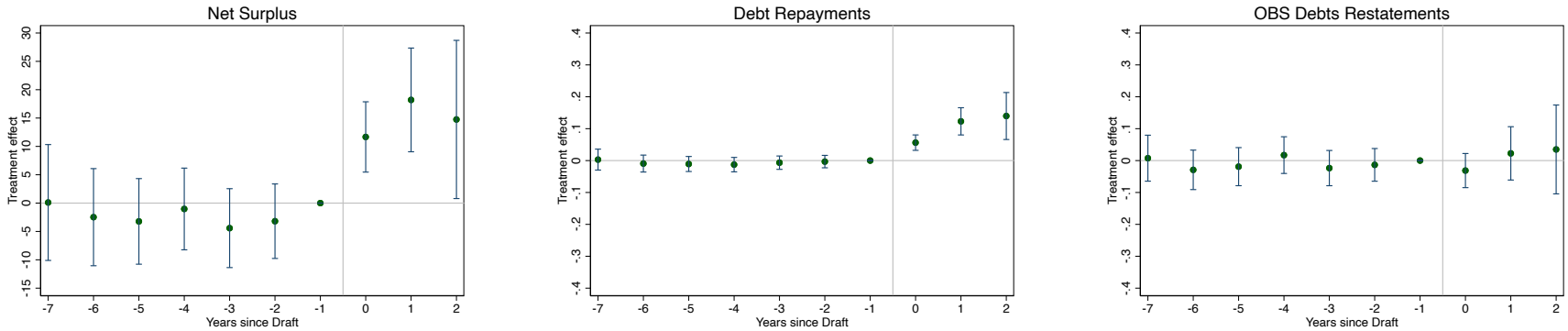
Figure 1
Number of Municipalities Receiving a Draft-appointed Auditor for the First Time in a Given Year



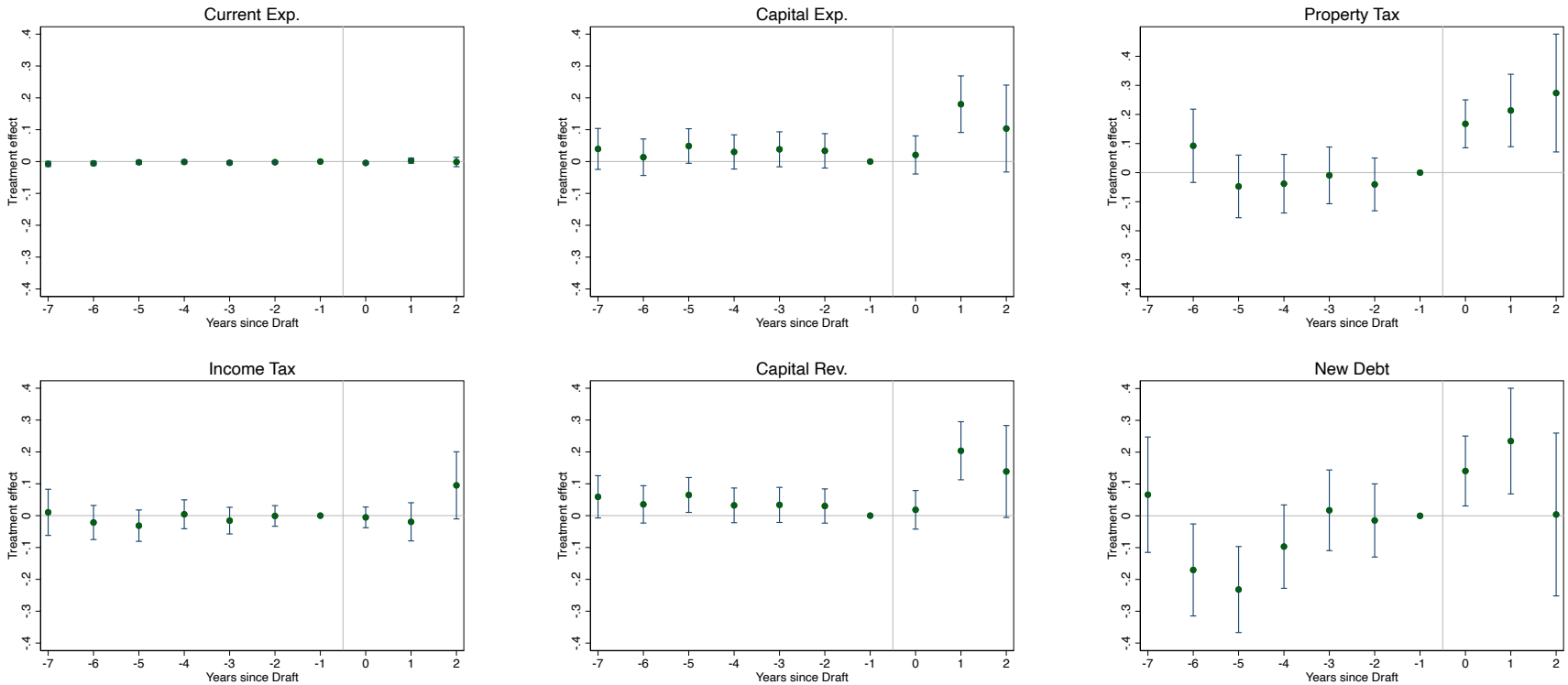
Notes: The figure shows the margins of variation used in the empirical analysis. The bar graph shows the number of municipalities (y-axis) that had a draft-appointed auditor operative for at least 3 months at the moment of adoption of the balance-sheet adoption, in a given year (x-axis).

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



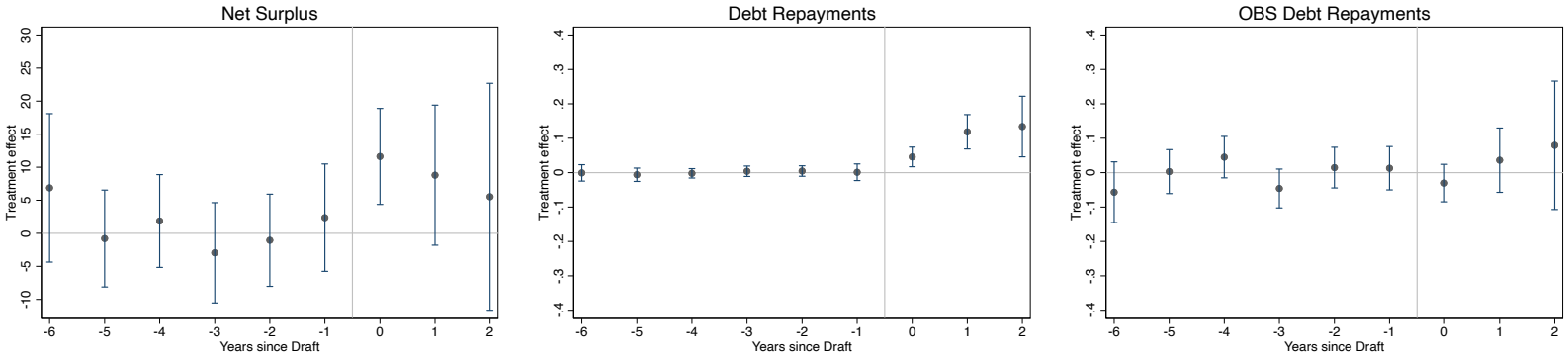
Notes: The graphs report coefficients and 95% confidence intervals estimated according to specification 3. Standard Errors clustered at the municipality level. All dependent variables are in per capita terms, $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation. 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 3
The Dynamic Effect of Auditor's Independence on Aggregate Spending and Revenue Choices



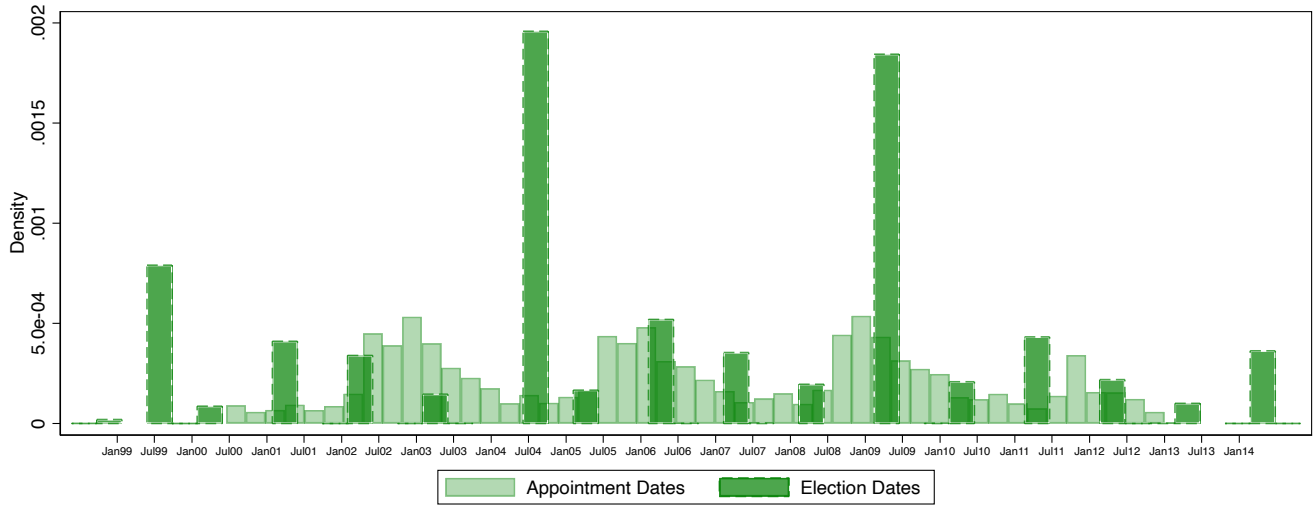
Notes: The graphs report coefficients and confidence intervals estimated according to specification 3. Standard Errors clustered at the municipality level. All dependent variables are in per capita terms, and transformed using the inverse hyperbolic sine transformation. All regressions include municipality, 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 initial age (in log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

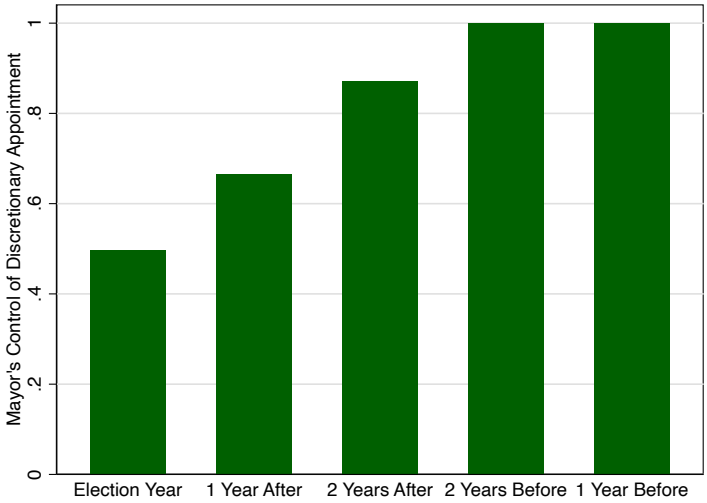
Figure 4
Event Studies, using de Chaisemartin and D'Haultfoeuille (2019) methodology



Notes: The graphs report coefficients and confidence intervals of the DID_M estimators estimated according to de Chaisemartin and D'Haultfoeuille (2019) 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 log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Figure 5
Auditors' Appointment Cycle and Election Cycle





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 auditorappointment, by the moment of the election cycle. This is the variation used for identification in Table 11. 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 Restatements	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 before 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. Years of Treatment

	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 in parentheses.

Table 3
Levels of Oucomes in 2010, by Treatment Timing

A. Fiscal Sustainability						
	2012 Cohort	2013 Cohort	2014 Cohort	2015 Cohort	Uncond. F-test p-value	Cond. F-test p-value
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 Restatements	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 p-value	Cond. F-test p-value
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 log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table 4
Changes in Auditors' Characteristics

	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 accountanrt* 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: Signifiance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard Errors clustered at the municipality level are reported in parenthesis. 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 log), 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: Signifiance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard Errors clustered at the municipality level are reported in parenthesis. 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 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: Signifiance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard Errors clustered at the municipality level are reported in parenthesis. 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 8
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: Signifiance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level in parenthesis. 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. *SameMuni* and *Corrupt* are proxies for the pre-reform collusion risk. *SameMuni* 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 ifrom 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 9
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: Signifiance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard Errors clustered at the municipality level are reported in parenthesis. All dependent variables are in per capita terms. $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation. $NoExperience$ 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, $NoExperience$ 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 10
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]
Dep. Var Mean	-194.791	65.292	3.255
Sum of Coefs.	13.288**	.021	-.027
Observations	114028	114028	114028
R-sq	0.715	0.769	0.442
	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]
Dep. Var Mean	-194.791	65.292	3.255
Sum of Coefs.	15.696***	.078***	-.014
Observations	114028	114028	114028
R-sq	0.715	0.769	0.442
	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: Signifiance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard Errors clustered at the municipality level are reported in parenthesis. 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 log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table 11
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=1	-7.417***	-7.360**	-0.0152*	-0.0179*	0.0199	0.0244
	[2.785]	[3.198]	[0.00885]	[0.0100]	[0.0247]	[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: Signifiance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard Errors clustered at the municipality level are reported in parenthesis. 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 yrs) and election(5 yrs) cycle. See Figure 5 and Appendix Figure A4 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.

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'Haultfoeuille (2019)

	Net Surplus		Debt Repayments		OBS Debt Repayments	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	16.82***	15.48***	0.0846***	0.0795***	-0.0123	-0.0130
	[1.971]	[3.034]	[0.0123]	[0.0124]	[0.0230]	[0.0232]
Dep. Var Mean	-205.8	-205.8	64.47	64.47	3.435	3.435
Observations	50427	50427	50427	50427	50427	50427
Adj. R-sq	0.604	0.624	0.714	0.721	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: Signifiance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard Errors clustered at the municipality level are reported in parenthesis. 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 log), 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 Investment, by Investment category

	Investment							
	(1) admin	(2) police	(3) education	(4) culture	(5) tourism	(6) transport	(7) local public goods	(8) social
Treated=1	0.0719*	-0.0334	0.0952**	0.0726**	-0.0211	0.00414	0.0362	0.0726*
× Post=1	[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: Signifiance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard Errors clustered at the municipality level are reported in parenthesis. 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 log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A3
The Effect of Auditor's Independence on Fiscal Sustainability, by Municipality Size

	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated ×	16.62***	0.0331**	-0.0131
Post	[4.383]	[0.0140]	[0.0269]
Treated ×	-2.501	0.158***	-0.0409
Post × Pop. 5-15 k.	[5.919]	[0.0400]	[0.0681]
Treated ×	-2.545	0.205***	0.169*
Post × Above 15k	[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: Signifiance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard Errors clustered at the municipality level are reported in parenthesis. 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 log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A4
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 × Post	16.81***	0.0679***	-0.0176
	[3.627]	[0.0140]	[0.0256]
Treated × Post × Local Auditor	-6.807	0.0941**	0.0826
	[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: Signifiance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard Errors clustered at the municipality level are reported in parenthesis. 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 log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A5
Impact of the Reform on Fiscal Sustainability, by Geography

	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated × Post	14.67***	0.0167	-0.0329
	[3.876]	[0.0124]	[0.0220]
Treated × Post × South	-14.21**	0.123***	0.0740
	[6.549]	[0.0299]	[0.0575]
Treated × Post × Center	-2.078	0.0390	0.0797
	[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

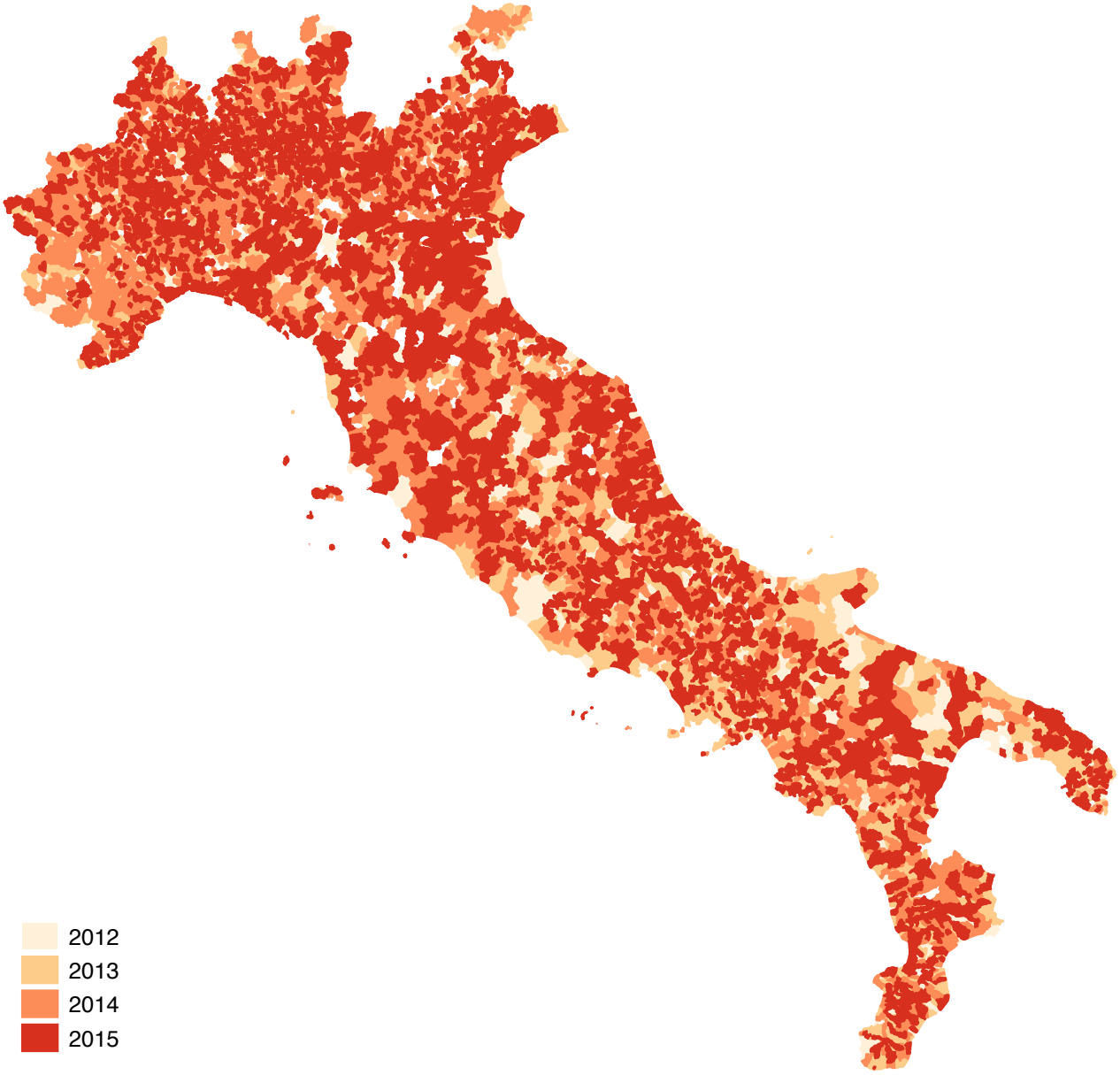
Notes: Signifiance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard Errors clustered at the municipality level are reported in parenthesis. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic 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 log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A6
Impact of the Reform on Fiscal Sustainability, allowing for Region-Specific Nonparametric Trends

	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
treat=1 × post=1	10.47***	0.0427***	0.0142
	[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

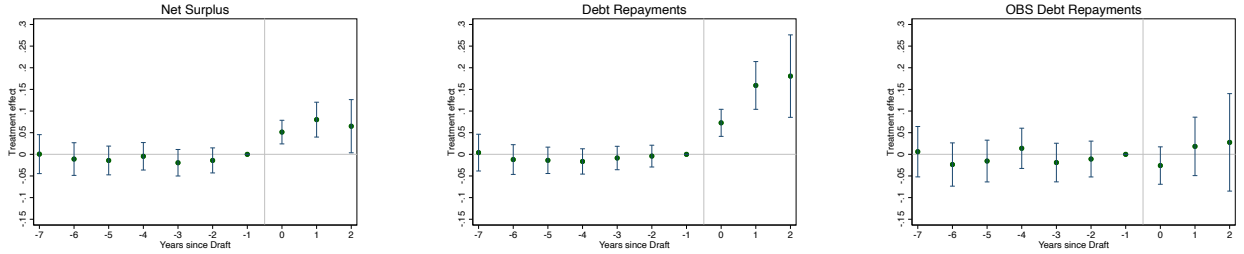
Notes: Signifiance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard Errors clustered at the municipality level are reported in parenthesis. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic 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 log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Figure A1
Staggered Treatment, Geographic Variation



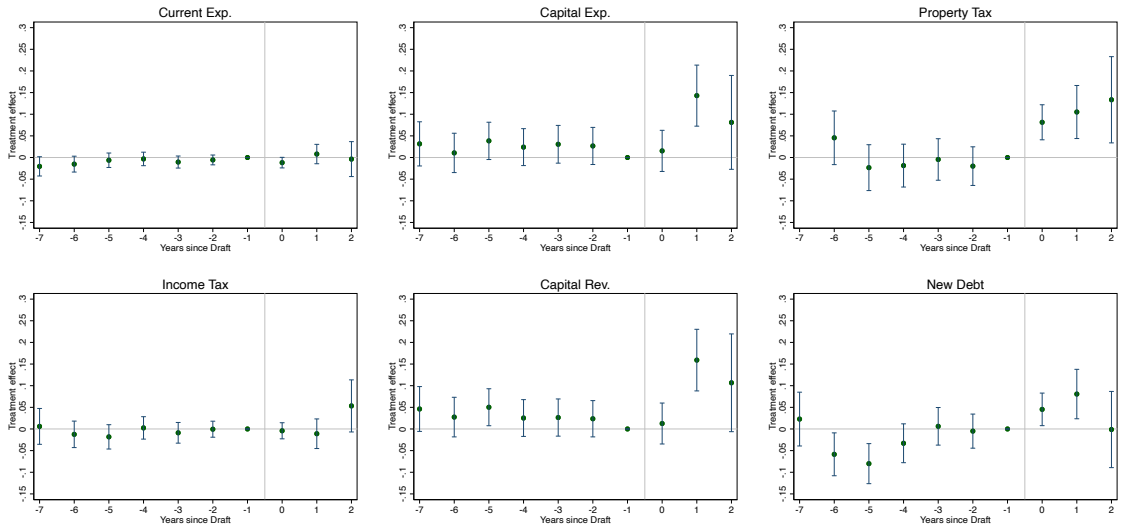
Notes: The figure shows the geographic variation in treatment timing. Darker gradation reflects later treatment timing.

Figure A2
The Dynamic Effect of Auditor's Independence on Fiscal Sustainability, Standardized Outcomes



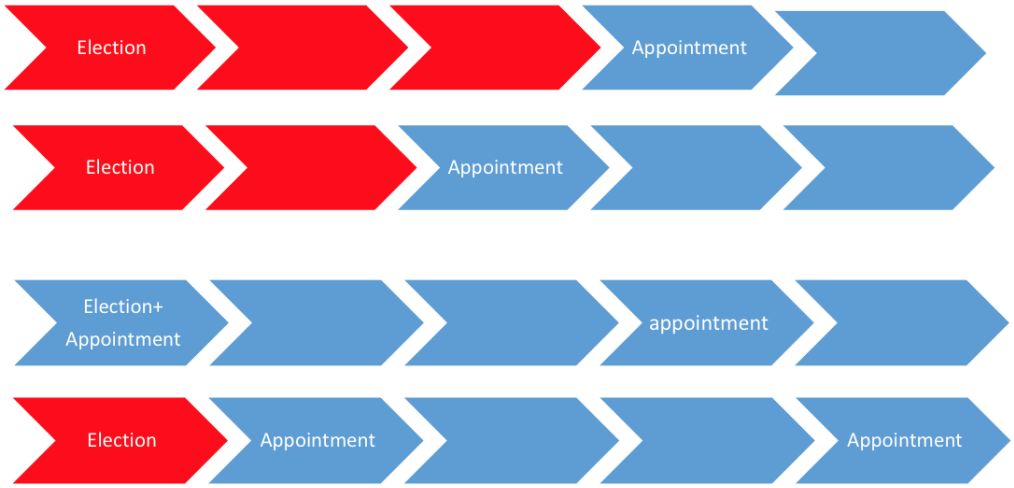
Notes: The graphs report coefficients and 95% confidence intervals estimated according to specification 3. Standard Errors clustered at the municipality level. All dependent variables are in per capita terms, $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation. 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 A3
The Dynamic Effect of Auditor's Independence on Aggregate Spending and Revenue Choices, Standardized Outcomes



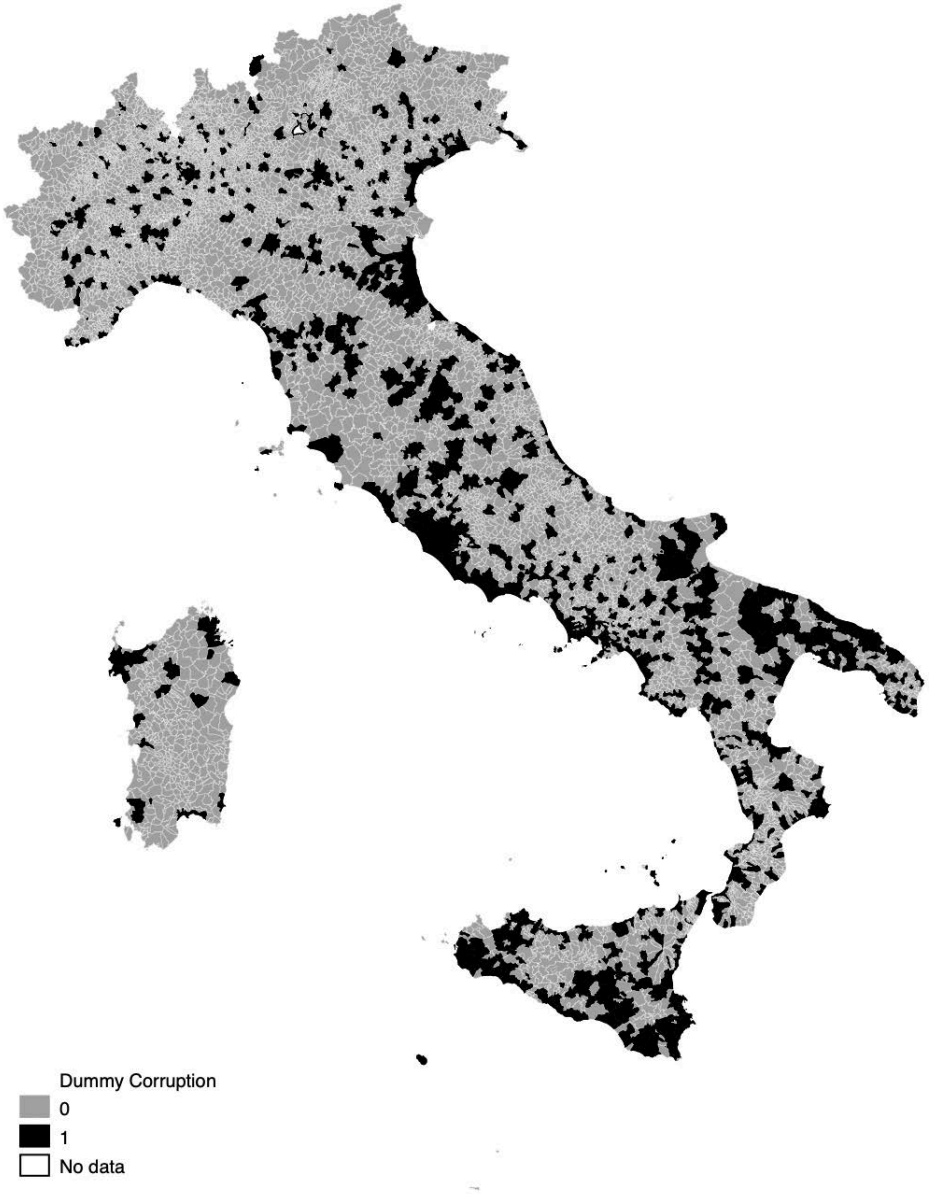
Notes: The graphs report coefficients and confidence intervals estimated according to specification 3. Standard Errors clustered at the municipality level. All dependent variables are in per capita terms, and transformed using the inverse hyperbolic sine transformation. All regressions include municipality, 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, gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Figure A4
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 A5
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.