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

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## Abstract

A classic problem in public finance is the over-expenditure of local governments in expectation of a bailout from higher-level administrations. While monitoring could mitigate agency problems, it can itself be rendered ineffective if auditors are corruptible. Vannutelli evaluates whether limiting auditors' conflicts of interest improves effectiveness and affects the financial health of local governments. She exploits the staggered introduction of a reform that removed the control of auditors' appointment from local politicians and introduced a random assignment mechanism. She obtains four main findings. First, random matching severs auditors-mayors connections. Second, treated municipalities significantly improve their net surpluses and debt repayments, per national government objectives. Third, the fiscal improvement results from a sizeable increase in tax capacity. Fourth, treatment effects are a combination of selection, matching and incentive effects. These findings highlight the value of auditor independence and illustrate how changes in the organizational design of the state can improve government performance.

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# 1 Introduction

Local governments are among the most important providers of public goods and services worldwide. While delegating responsibilities to subnational entities may bring decision-making closer to local interests, it also creates opportunities for abuse by local officials, who might put their own interests, and the ones of their immediate constituents, ahead of those of the country. Emblematic of this moral hazard problem is the debt accumulated by lower-level governments in expectation of an eventual bailout from the central government. It is thus unsurprising that one of the longest-standing debates around decentralization is how to ensure local fiscal discipline.

Monitoring by external auditors is a common tool used to mitigate agency problems in decentralized organizations. To be effective watchdogs, auditors should be independent from the audited party. 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.[1] Even if not deliberately corrupt, auditors are likely to be unconsciously biased toward the party that hires them (Bazerman, Morgan and Loewenstein (1997)). Understanding how to design effective monitoring institutions is thus central to modeling decentralized organizations and promoting their effective functioning.

This paper examines the consequences of changes in the design of monitoring institutions. Leveraging a large-scale reform that changed the appointment system of auditors overseeing municipal government budgets in Italy, I provide new evidence that removing the discretion of monitors' appointments from local politicians significantly improves local governments' performance. Italian municipalities are required to draft financial statements to be reviewed and approved by professional auditors, who act as an intermediate layer of oversight between municipalities and the central monitoring performed by the Italian National Court of Accounts (comparable to the Government Accountability Office in the U.S.). The audit system aims to ensure responsible spending practices in compliance with national fiscal rules. 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 of municipalities and

[1]As one example of a potential benefit, a locally appointed auditor might have better information on whether to relax the rules in response to local economic shocks. A large theoretical literature has investigated the broader trade-offs involved in delegating authority (Aghion and Tirole 1997) and the optimal design of monitoring institutions (Strausz 1997; Ortner and Chassang 2018; Mookherjee and Tsumagari 2018; Prendergast 2015; Prendergast and Topel 1996). I provide an empirical test of how changes in the information asymmetries and bargaining power of the supervisor vis-a-vis the monitored agent affect monitoring quality.

can thus influence municipal financial decisions ex-ante.

The main feature of the reform was a switch from a discretionary "quasi-patronage" system, in which mayors could choose their auditors, to a random assignment mechanism in order to strengthen oversight of local entities' budgets. This change was part of a larger package of austerity measures adopted in August 2011, at the peak of the sovereign debt crisis, in order to comply with the fiscal adjustment efforts required by the European Union. [2] While all the other provisions went immediately into effect, the implementation of the new appointment system took place when the current auditor's term expired, which occurred on different dates across municipalities for historical reasons unrelated to the reform and beyond mayors' control.

I exploit the staggered introduction of the new appointment system and apply a generalized difference-in-differences (DiD) methodology. I compare the outcomes of municipalities treated earlier ($Treatment$) and those treated later ($Control$), before and after the arrival of an independently appointed auditor. Under the identifying assumption that the treatment timing is uncorrelated with the evolution of outcomes over time, this approach allows me to 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, I adopt a stacked-by-event design (Cengiz, Dube, Lindner and Zipperer 2019; Deshpande and Li 2019) that ensures the treatment effects are estimated only based on comparisons of units switching into treatment to not-yet-treated units. As a robustness check, I also apply the alternative estimator proposed by de Chaisemartin and D'Haultfœuille (2020) and Callaway and Sant'Anna (2021) and obtain very similar estimates.

I start by studying the reform's effects on the selection and allocation of auditors. I find that the reform significantly changes auditor-mayor matching and severs connections between municipal auditors and mayors along several dimensions likely to affect auditors' independence. The share of auditors residing in the municipality they were hired to monitor declined from 31% before the reform to just 1% afterward. Similarly, the share of auditors reappointed for a second term drops from 57% before the reform to 1% after, and the percentage of those sharing the same surname as a local politician falls from 7% to 1%. The reform also induces a change in the composition of the auditors' pool: almost 14,000 new auditors join the list, representing around 60% of the pool of auditors. While these new auditors have no municipal-specific experience, they look otherwise very similar to the existing set of incumbents on other observables. Most importantly, they are equally experienced in

[2] In Italy, as in many other countries, local entities cannot default on their debts, and deficits (or surpluses) run by local governments are consolidated in the national budget. Facing pressure to reduce the national debt and to improve credibility on the global financial markets, local governments' fiscal health became increasingly important from the national government's perspective.

terms of general private-sector accounting.

I then move to study the causal effects of random auditors' assignments on municipal financial health. I find that, when subject to more external oversight, municipalities significantly improve their fiscal performance, as per national government objectives: they increase their expenditure in debt repayments by 9% and their net budget surplus by 8%.[3] The magnitude of the effects is comparable to that of Grembi, Nannicini and Troiano (2016), who analyze the effect of relaxed fiscal rules in Italy in 2001. This result suggests that draft-appointed auditors are more effective enforcers of fiscal rules and provides empirical evidence on the importance of enforcement issues when considering rules' effectiveness.[4] 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.

Intuitively, to improve their fiscal stance, municipalities can either increase their revenue (e.g., by collecting more taxes) or cut spending (e.g., by cutting investments or current expenditures). I investigate responses along these different margins and I find that municipalities do not cut spending but instead improve their fiscal stance through revenue-based adjustment—specifically by increasing revenue from the local property tax by over 20%. I also provide evidence that the increase in revenue comes from an expansion in the tax base rather than from a change in tax rates, consistent with an improvement in the enforcement of tax collection upon the arrival of independent auditors. In line with this explanation, I find that the effects are stronger in municipalities that have a higher share of housing units that are undeclared and where there is a higher share of buildings hidden from tax authorities, as documented in Casaburi and Troiano (2016). This result speaks to the literature on the determinants of fiscal capacity and is consistent with the findings of Xu (2018), which shows that patronage-appointed governors raised fewer taxes and invested less in the revenue capacity of British colonies.

The switch from discretionary appointment to random assignment is potentially acting along three different margins that are considered to be crucially related to government performance: selection, matching, and incentives of public servants (Besley, Burgess, Khan and Xu 2022). First, by removing barriers to entry based on patronage networks, the new ap-

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

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

pointment system alters the selection of auditors by changing the applicant pool. While I find that new entrants look fairly similar to incumbents along observables (e.g., age, gender and private sector tenure), they might be better along other unobservable dimensions, such as honesty. On the other hand, they are less experienced in municipal auditing than incumbent auditors, and this might impair their monitoring effectiveness. Second, the reform changes the matching of auditors to municipalities, moving from a system of endogenous matches to one of random ones. This change severs ties between auditors and mayors that could impair monitoring but might come at the cost of sacrificing the potential value of discretionary appointments and inducing misallocation. Third, the reform alters auditors' incentives. Under the pre-reform system, auditors could be reappointed for a second term by mayors. This provides strong incentives for them to please the mayor who appointed them, at the cost of potential ex-post sanctions from the Court of Auditors should the auditors be found liable for lax or corrupt monitoring. Under the new system, auditors have the incentive to please the Court.

Exploiting the richness of my design, I investigate the role played by these different aspects. I find that, in small municipalities, the treatment effects are largely driven by municipalities that receive a new entrant as opposed to an incumbent auditor, suggesting that selection is indeed an important driver of the results. However, I still detect sizable effects when focusing only on large municipalities that can only receive an auditor drafted among the pool of incumbent auditors due to the presence of minimal experience requirements in the law. I then explore the role of matching effects by investigating how the treatment effect varies with proxies of auditor-mayor collusion risk both in the pre- and post-reform period. I consider (a) past financial health since lax or corrupt monitoring might allow for less responsible financial behavior, (b) an indicator for whether any auditor appointed before the reform was a local resident, and (c) a measure of local corruption investigations. For all measures, I find that the reform was more effective in places that had a higher risk of collusion in the pre-reform period. I also find that the treatment effects are driven by municipalities where the randomly assigned auditor resides in a municipality at least one hour away from the audited one. Distant auditors are less likely to have connections and have fewer incentives to collude with the local mayor, but they might also be less knowledgeable about the municipality and thus be less effective in monitoring. The absence of treatment effects for municipalities that randomly happen to receive a local auditor is particularly intriguing as it further sheds light on the strength of social ties and on how home bias might adversely affect bureaucratic performance even in the presence of random assignment (Xu, Bertrand and Burgess 2021; Chu, Fisman, Tan and Wang 2020). Third, I investigate the role played by changes in incentives in two ways. First, I exploit quasi-exogenous within-auditor variation in connections to the incumbent mayor in the pre-reform period. I show that during the years in which mayors and auditors are connected, municipalities run higher deficits and decrease their debt repayments, thus confirming that indeed incentives played a role in affecting auditors' behavior. Second, I compare the effect of the reform for municipalities in which the last appointed auditor was in his first versus second term. Intuitively, auditors in their second term should have had fewer incentives to be lenient toward the mayor as, for them, career incentives are muted. Indeed, I find suggestive evidence in this direction, as the effects are significantly stronger in municipalities with a first-term, as opposed to a second-term auditor.

This paper contributes to several strands of the literature. First, it contributes to the literature that investigate the organization of the state, the trade-offs involved in decentralized organizations, and the role of monitoring as a tool to solve agency problems. While several papers have documented the effects of audit findings on corruption and accountability (Olken 2007; Ferraz and Finan 2011; Avis, Ferraz and Finan 2018; Bobonis, Fuertes and Schwabe 2016), taking audit quality as given, very little is known about the determinants of monitoring effectiveness. A notable exception is the work of Duflo, Greenstone, Pande and Ryan (2013a), analyzing the effect of randomly assigning third-party environmental auditors to 236 industrial plants in India. This paper builds on their findings by providing evidence on the effect of removing discretion in the selection of auditors in a public sector setting, in which the oversight problem is complicated by political economy considerations, and for a different type of audit regime.[5] Furthermore, my analysis evaluates the consequences of a nationwide reform involving over 6,000 municipalities, thus corroborating the external validity of Duflo et al. (2013a)'s findings outside of a randomized controlled setting and in a developed country.

More generally, this paper provides three distinct contributions to the growing literature on the selection, allocation, and incentives of personnel in the public sector and their implications for state performance (Finan, Olken and Pande 2017; Besley et al. 2022). First, I study a quasi-experimental intervention that allows me to analyze the distinct roles of selection and matching, as well as their interplay, in affecting key dimensions of government performance, such as fiscal capacity. Previous papers have separately documented the adverse effects of patronage for the selection of elite and lower-level bureaucrats (Xu 2018; Colonnelli, Prem and Teso 2020) and the effects of the allocation of socially tied bureaucrats

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

across localities (Xu et al. 2021; Chu et al. 2020) and of decision-making rights within an organization (Bandiera, Best, Khan and Prat 2021), holding selection constant. Second, my experimental variation also allows me to study how changes in selection rules affect outcomes by changing endogenously the quality of the applicant pool. This then allows me to connect two different strands of the literature that have separately looked at the importance of the applicant pool for public services versus the role of selection rules (see Besley et al. 2022 for a review). Third, this is among the first papers to focus on agents that are at the intersection of the private and public sectors, as independent consultants who work for the public sector but are not bureaucrats.

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

# 2 Institutional Context

## 2.1 Fiscal Monitoring

Italy is a highly decentralized democracy, with 2 levels of subnational governments: 20 regions and over 8000 municipalities. Each municipality has its local government composed of an elected mayor (Sindaco), an executive body (Giunta) appointed by the mayor, and an elected council (Consiglio Comunale). The mayor is directly elected for a 5-year mandate with a 2-term limit, holds executive power at the municipal level, and is responsible for all key policy decisions, including the annual budget. Municipalities are granted large autonomy, they manage around 8% of total public expenditure (over €55 billion) and have full control of a wide range of essential public services, such as waste management, social services, childcare and nursery schools, school-related services, local police, road maintenance, housing, culture, recreation, and economic development. Spending is financed by municipal fiscal revenues (87%) plus transfers from the central government (13%), while borrowing is allowed only to finance investment expenditures and is subject to strict quantitative limits (Grembi et al. (2016),Chiades and Mengotto (2015)).[6] Fiscal revenues come from two main sources: (1) local taxes, among which the most relevant is the property tax; (2) local fees (e.g. building permits, traffic fines, and fees for other services). One of the main responsibilities of mayors 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 tax collection, tax rates, and budget allocations.

From 1999 onwards[7], , 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 limit the growth of their so-called fiscal gap - defined as the deficit, net of transfers, and debt service - below a given threshold (Grembi et al. 2016).It is important to note that in Italy, and in general in many countries in the European Union, local entities cannot default on their debts, deficits (or surpluses) run by local governments are consolidated in the national budget, and the bailout of sub-national entities is formally regulated by law.[8] Subnational government debt accounts for 11% of GDP and represents 7% of general government debt. Therefore, especially during the economic crisis, the national government has frequently made use of the pact to shift part of the national debt reduction required by the European Union towards local entities.[9] In this context, enforcing the respect of fiscal rules and ensuring the fiscal sustainability of local governments became critical for the national government, motivating a surge in attention to fiscal monitoring procedures.

## 2.2 The Role of Municipal Auditors

Since 1990, all municipal budgets need to be audited and certified by a board of auditors.[10] The board is composed of one member for municipalities with less than 15,000 inhabitants - which represent 90% of municipalities - and three members for larger municipalities. Prior

[6] Municipalities can undertake new debt only if the total amount of debt service does not exceed 15% of current revenues of the two preceding years. While in theory they can borrow from private banks, the vast majority of debt (89%) is granted by the so-called "Cassa Depositi & Presiti", a state-owned body that operates to promote national and local governments' investment projects. Most of the municipal debt is therefore implicitly guaranteed directly by the national government.

[7] *Legge Finanziaria* 23 December 1998, n. 448

[8] 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`.

[9] For example, between 2010 and 2017, the national government cut resources for local governments by over €12 billion: partly via a reduction of inter-governmental transfers (9 billion), and partly through a tightening of the rules of the pact (3.3 billion).

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

to the reform, the board used to be nominated with a majority vote of the city council[11], 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.

From its very first inception, the board of auditors was assigned a double role of both monitoring and advising. The original law instituting the board of auditors describes its role as follows: *The board of auditors collaborates with the municipal council in its control and governing function, supervises the accounting and financial regularity of the management of the entity and certifies the correspondence of the balance sheet to the economic outcomes, drawing up a specific report, which accompanies the proposed board resolution of the balance sheet. The auditors shall provide advice and proposals aimed at achieving better efficiency, productivity, and cost-effectiveness in local government management.* .

The tasks of auditors have progressively been detailed and expanded over time, in particular in the years 2000 and 2003 [12]. Appendix Table A1 provides a summary list of the auditors' main tasks, as indicated in the law. Importantly, and differently from the typical auditor in a private sector context, municipal auditors are not simply auditing and signing off the balance sheets ex-post, but they have a significant role in influencing policy-making ex-ante. According to the National Association of Municipal Auditors (ANCREL), auditors need to perform over 200 tasks every year, summing up advising and supervising ones.[13] For example, in his advising role, the auditor is required to provide written opinions on several important acts, including municipal protocols and regulations about tax collection. The auditor is also charged with verifying the accuracy of the information reported in the budget/balance sheet and it should also provide advice on how to improve efficacy, efficiency, and cost-effectiveness in the management of public funds. Last but not least, auditors are required to verify the respect of fiscal rules.

The results of the audit review, as well as the set of auditors' opinions and suggestions, should be included in an audit report that is reviewed by the council at the moment of final approval of the budget/balance sheet. After the approval, the auditor is required to fill in a detailed questionnaire and to transmit all of the relevant documents, including the approved budget and the audit review, to the *National Court of Accounts*, which is the central monitor. The Court is in charge of reviewing all the material transmitted by the auditors and can decide to start a judicial procedure against municipalities that failed to comply with the rules or that displayed improper financial practices. On average, the Court

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

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

[13] `www.ancrel.it`

emits around 2000 judicial acts of ex-post verifications per year (Corte dei Conti 2022). Most of these pertain to recommendations and other preliminary acts that warn municipal governments and invite them to take corrective actions within 60 days, while the number of subsequent judicial prosecutions is relatively limited to an average of 300 cases per year. While it was originally instituted with the scope of providing a form of surveillance over municipal financial practices, the effective monitoring action of the board of auditors has been fairly limited for many years, consisting only of a "formal check" of the compliance with budget laws.[14]

The Italian market for auditors is characterized by a high degree of competition. Differently from the U.S., where auditors are mostly belonging to large private firms, the auditing job in Italy is carried out by self-employed Certified Public Accountants, who work for municipal governments but also offer professional accounting and auditing services to individuals and small businesses. There are over 150,000 registered CPAs that are authorized to work as auditors, in the public and the private sector. Working as a municipal auditor provides visibility and is likely to allow auditors to expand their private clients' network, and thus represents a potentially attractive career choice, beyond its monetary compensation. As common in markets for experts (Ronen 2010, White 2010), however, discretionary appointments by mayors likely limited the reliance on a restricted set of "connected" auditors based on patronage networks.

### 2.2.1 Introducing Random Assignment of Auditors

In August 2011, at the peak of the European Sovereign Debt Crisis, the national government suddenly changed the mechanism of auditors' appointment (*Law 148/2011*).[15] 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, so as to avoid sending un-experienced auditors to audit large municipalities. For municipalities up to 4,999 inhabitants, auditors shall have been certified public accountants for at least 2 years. For those between 5000 and 14,999 inhabitants, they shall have been certified public accountants for at least 5 years and have been appointed as municipal auditors at least once before. For those above 15,000 inhabitants and above:

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

[15] 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.

auditors shall have been certified public accountants for at least 10 years and have been appointed as municipal auditors at least twice before. Due to administrative constraints and the time needed to form the public lists, the new drafting procedure entered into effect on December 10, 2012. The reform does not apply to the 5 "Special Status" Regions of Italy (Valle d'Aosta, Trentino Alto Adige, Friuli Venezia Giulia, Sardinia, and Sicily), which are granted extra-autonomy along many margins, including regulation of fiscal monitoring procedures.[16] 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[17], via a standardized, computer-based system provided by the Ministry of the Interior. Municipalities are required by law to inform their local prefecture at least 2 months before the current auditor's term expires.[18] For each draft, the number of drafted candidates is equal to $3 \times N$, where N is the number of auditors to be appointed. 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 she is eligible for the specific appointment.[19] The formal appointment notice also contains the wage offered to the auditor for the three-year mandate. Auditors' compensation is regulated nationally, including a minimum floor, that is estimated to cover the expenses to effectively perform the job, and maximum caps, that vary by population threshold, leaving mayors very limited margins of discretion in setting the exact compensation within this small bandwidth, and thus very limited variation in the actual compensation across municipalities of similar size.[20] This aspect of the auditors' appointment was not affected by the reform.Shall the first amongst the drafted auditors refuse the appointment or be deemed to be incompatible, the municipality

[16]In 2016, Friuli Venezia Giulia, Sardinia, and Sicily adopted laws introducing drafting procedures that are similar to the national one but that generally preserve some discretion for local governments. In Friuli Venezia Giulia, municipalities can choose among the $3 \times N$ 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 toward the local auditors. 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.

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

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

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

[20]The presence of a minimum floor is particularly important, as Duflo, Greenstone, Pande and Ryan (2013b) show that a major reason for the lack of effectiveness of third-party auditors in India was the fact that the audit price was too low to cover the effective cost of conducting an audit.

proceeds to contact the second-drafted auditor. Thus, mayors cannot choose amongst the set of drafted auditors but have to proceed following the order of the draft.[21]

# 3 Data

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. 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 government, minus total expenditure, net of interest payments on outstanding debt. The $NetSurplus$ is a clean measure of the actual fiscal sustainability of municipal finances, as it nets out factors that are not under the direct control of the current local administration, such as changes in the level of transfers from higher levels of government or the consistency of interests on debts undertaken by previous administrations. Furthermore, this variable is the main target of the national government's fiscal rules (Grembi et al. (2016)).

Second, I look at $DebtRepayments$, capturing the overall amount of municipal finances devoted to reducing the stock of existing debt. This variable is also closely monitored, as municipal debt is part of the national debt.[22] 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 variable is a signal 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, such as any expense incurred that was not previ-

[21]The data shows that rejections are a relatively rare occurrence: around 20% of first-drafted auditors in total reject the appointment, and in most of the cases the second amongst the drafted auditors accepts the appointment. In less than 3% of municipalities, a new draw was needed as none of the candidates accepted the position. Table A2 investigates the determinants of auditors' rejections. The most significant predictor is the distance between the municipality of residence and the municipality of appointment. All other characteristics have very limited explanatory power. Importantly, rejections are uncorrelated with indicators of the financial performance of municipalities.

[22]In the last 10 years, the national government has pushed municipalities to repay their outstanding debts as quickly as possible, especially those that were undertaken in the past, as they were subject to significantly higher interest rates.

ously authorized and accounted for in the provisional budget. [23] 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. Municipalities are allowed to recognize OBS liabilities that arose in previous years and had not been previously recognized. To finance $OBSDebtRepayments$, municipalities may employ any existing current surplus and, residually, they may undertake new debt, provided they justify the impossibility of financing $OBSDebtRepayments$ out of their own resources.

Auditors are explicitly asked by the Court to closely monitor the presence of OBS debts and the process of recognition. They are asked to review the reasons why the OBS debts arose and evaluate the proposed repayment methods. The auditors are also required to report about $OBSDebtRepayments$ in a specific section of the questionnaire they need to complete for the Court after the balance sheet has been approved. Given that they represent an exceptional violation of the rules, $OBSDebtRepayments$ are a relatively rare phenomenon, and this variable is equal to 0 for over 80% of the sample.

All variables are measured in per-capita terms (winsorized at the 1%), expressed in 2018 euros equivalent, and reflect accrual accounting.[24] All spending variables, which are always greater than 0, are transformed using the inverse hyperbolic sine transformation. Table 1 shows the summary statistics of all the outcome variables used in the analysis.

The key treatment assignment variable for my analysis is the date when each municipality appoints an auditor with the drafting procedure for the first time. All information about currently draft-appointed auditors is available on the website of the Ministry of the Interior, separately for each municipality. I created a complete historical database for the universe of municipalities by scraping the website and obtained a dataset containing information on the draft dates and identities of all the auditors drafted from 2012 onwards. I combine this data with information from the candidates' pool. The list contains information about age, gender, municipality of birth, municipality of residence, region of residence, level of professional experience (proxied by the number of years they have been certified as public accountants), and information on the previous service as an auditor. This allows me to overcome, at least partially, one important data limitation, namely the fact that I don't have information about

[23]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.

[24]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. 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.

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

| | A. Fiscal Sustainability | | | | | |
|---|---|---|---|---|---|---|
| | Mean | S.D. | Median | p10 | p90 | p99 |
| Net Surplus | -306.97 | 204.81 | -258.73 | -554.19 | -125.73 | 31.30 |
| Debt Repayments | 59.96 | 48.95 | 47.72 | 21.63 | 107.07 | 279.40 |
| OBS Debts Repayments | 3.61 | 13.01 | 0.00 | 0.00 | 7.74 | 89.05 |
| | **B. Revenue and Spending Choices** | | | | | |
| | Mean | S.D. | Median | p10 | p90 | p99 |
| Current Exp. | 886.77 | 401.74 | 770.17 | 566.10 | 1,312.69 | 2,804.00 |
| Capital Exp. | 556.88 | 783.00 | 295.62 | 88.94 | 1,230.57 | 5,012.36 |
| Property Tax | 140.82 | 140.24 | 123.58 | 0.00 | 268.57 | 779.23 |
| Income Tax | 38.32 | 28.84 | 37.61 | 0.00 | 76.26 | 113.14 |
| Capital Rev. | 487.38 | 744.31 | 232.78 | 69.08 | 1,112.87 | 4,704.39 |
| New Debt | 132.34 | 255.13 | 25.94 | 0.00 | 379.03 | 1,446.98 |

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

auditors' appointments for the pre-reform period. By exploiting the information reported by auditors when they sign up for the lists, I am able to recover the identity of the last auditor appointed before the reform for over 40% percent of my sample.[25] I supplement the above information with further data to account for municipal characteristics that might affect fiscal sustainability. For a description of controls as well as sample restrictions, see Appendix Section A2. My analysis sample consists of 5603 municipalities in Ordinary regions for the years 2007-2015.

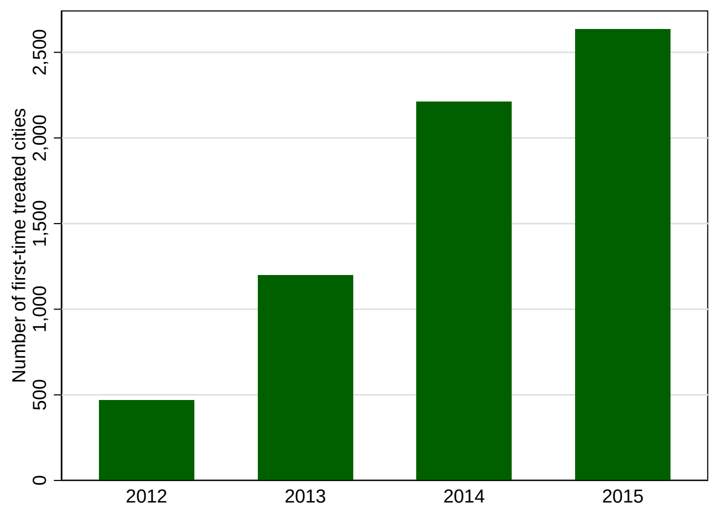
# 4 Empirical Strategy

My identification strategy exploits the staggered introduction of the reform across municipalities. As explained in Section 2, while the auditors' appointment reform became effective in December 2012, the effective treatment date varies across municipalities depending on the expiration date of the current auditor's term. For example, municipalities that appointed auditors in August 2011, just before the announcement of the reform, would be "treated" with a draft-appointed auditor only from September 2014 onwards. A key factor for my identification is that there exists wide variation in the auditor's appointment date before the reform

[25] 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.

due to historical reasons, which are uncorrelated with other municipal characteristics.[26] Because municipalities had appointed auditors at different points in time, treatment timing is plausibly exogenous. My design, therefore, compares municipalities that are treated earlier to municipalities that are treated later. Figures 1 and A1 display the sources of identifying variation. Figure 1 shows the total number of municipalities with independently-appointed auditors over time. Figure A1 displays their geographic distribution, highlighting the lack of any geographic patterns by treatment timing.

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




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

Given my design, the most standard specification would be the following:

$$Y_{mt} = \alpha_m + \delta_t + \beta Treated_{mt} + X'_{mt}\zeta + \epsilon_{mt} \tag{1}$$

Where $Y_{mt}$ is one of the possible outcomes measured at the municipal level, $Treated_{mt}$ is an indicator variable that is equal to 1 if an independent auditor is active in year $t$, $X_{mt}$ is a matrix of time-varying controls, $\alpha_m$ are municipality fixed effects. However, naively applying this specification could potentially pose a set of empirical challenges that have been recently

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

highlighted by a growing literature on the pitfalls of two-way fixed effects estimators with staggered adoption (Goodman-Bacon (2021), de Chaisemartin and D'Haultfœuille (2020), Borusyak, Jaravel and Spiess (2021)). The coefficient $\beta$ from equation 1 is a weighted average of all the possible 2x2 comparisons in my sample. Therefore, it is also estimated using comparisons among already-treated units and not-yet-treated units, where the already-treated units serve as controls. This induces a bias in the presence of heterogeneous treatment effects across groups experiencing treatment at different points in time. In the absence of a never-treated group, a straightforward solution is to limit attention only to comparisons between treated and not-yet-treated units, where only the not-yet-treated units serve as controls. To do so, there are two alternative options. One way is to use one of the recently developed robust alternative estimators (de Chaisemartin and D'Haultfœuille 2020, Callaway and Sant'Anna 2021, Sun and Abraham 2021). Alternatively, and more flexibly, I adopt a stacked-by-event design (Deshpande and Li 2019, Cengiz et al. 2019).[27] This technique simply amounts to using a "rolling control group", by constructing my estimation dataset as follows. First, I create a separate dataset for each of the 3 treatment waves before the last one (2012, 2013, 2014). In each of these datasets, municipalities that receive the randomly-drafted auditor in that year are considered treated, while municipalities that will experience the treatment in later years serve as a control. Second, in every dataset, I create event-time dummies relative to the year of treatment.[28] 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. The resulting dataset has 3410 treated municipalities and a total of 114,028 municipality-year observations. My main estimating equation then becomes:

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

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;[29] $Post_{mt}$ 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

[27] I show the robustness of my estimates to the use of the alternative estimators in Section 5.4.

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

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

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 $\delta_{pt}$. I estimate separate calendar time effects $\delta_{pt}$ for different dummies population bins so that municipalities belonging to different population size classes are allowed to evolve along different trends. The matrix of time-varying controls $X_{mt}$ includes 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. Standard errors are clustered at the municipality level (Bertrand, Duflo and Mullainathan 2004), accounting for the possibility of serial correlation over time and 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:

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

In this specification, the coefficients of interest are the $\gamma_k$'s, measuring the change in outcomes of treated municipalities $k$ years after treatment, relative to the pre-treatment year, relative to the change in outcomes of control municipalities, who have yet to be treated. I estimate treatment effects up to three periods from treatment onset (i.e. $k = 2$) thus covering the entire term of the draft-appointed auditor.[30]

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

[30]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 $\gamma_2$ is estimated only using the comparison of the change in the outcomes between 2011 and 2014 of the municipalities treated in 2012 and the municipalities that will be treated in 2015. The coefficient $\gamma_1$, 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 in 2015.

in 2010. Table A3 shows that there are no significant effects across columns except for population-size dummies. In all of my analyses, I, therefore, control non-parametrically for the presence of differential trends by population size, by including population-by-year fixed effects. I construct a set of dummies for the following population bins: 0 to 1,000 inhabitants; 1,000 to 5,000; 5,000 to 10,000; 10,000 to 15,000 inhabitants; 15,000 to 20,000; 20,000 to 60,000 and above 60,000 inhabitants. I then interact each category with year fixed-effects to estimate the population-by-year fixed effects $\delta_{pt}$.[31]. These controls also allow me to account for potential effects induced by changes in the composition of the pool of auditors for municipalities of different sizes due to the minimum experience requirements for municipalities above 5,000 and 15,000 inhabitants. The presence of significant coefficients should not be interpreted as evidence of differential selection of municipalities across cohorts, as mayors cannot manipulate in any way the expiration date of their current auditor, which is the only variable determining the assignment across cohorts. Results simply suggest that, for historical reasons, larger municipalities are slightly more likely to have their auditors' terms expiring in 2012 or 2013, as opposed to later years.

Second, I look at whether municipalities treated at different times display any significant differences in the levels of outcomes in the pre-reform period. In particular, the first four columns of Table A4 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 small significant unconditional differences, after conditioning on the set of covariates used in the regressions, no significant difference can be detected. Notably, testing for the absence of a significant difference in pre-treatment *levels* of outcomes by treatment timing is a stronger test, as the standard difference-in-differences assumptions only require no counterfactual differences in the trends of the outcomes.

Third, and most importantly, one can inspect whether outcomes exhibit parallel trends in the pre-reform period as a way to assess the plausibility of the assumption of parallel trends in the post-reform period.[32] In Figure 2 I estimate the flexible dynamic specification of equation 3 and show that I do not detect any evidence of differential pre-event trends

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

[32]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),and my results are robust to allow for large degrees of possible non-linearity in the violation of parallel trends.

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

In a standard difference-in-differences design with never-treated units, one could easily inspect the presence of either type of anticipation effect by looking at event studies relative to the announcement moment. This type of exercise is not feasible in this setting, due to the absence of never-treated units. Instead, I can exploit one of the stacked-by-event design features to provide suggestive evidence of the type of anticipation effects potentially at play. As can be seen in equation 3, the design allows me to separately control for both calendar-time fixed effects (the $\delta_p t$) and event-time fixed effects (the terms $\sum_{k=-7}^{k=2} \beta_k * D^k$). The latter are estimated off of the control group.By looking at the $\beta_k$ coefficients, one can thus inspect if, and in which direction, the control units change their behavior before actually becoming treated.[33] Appendix Figure A3 shows the results. The important thing to notice in the figures is that, if anything, control units seem to display an anticipatory behavior of the first kind, meaning that they start behaving in a way that is similar to the one of the treatment group, before being treated. In fact, the coefficients of Figure A3 are of the same sign, though of a much smaller magnitude, as those of Figure 2, which displays the coefficients $\gamma_k$, which represent the event-studies for the treated group. In this sense, my estimates could be considered conservative lower-bound estimates of the true effects in the presence of anticipatory effects.

Finally, a source of concern might be the presence of other time-varying shocks that occur in the same period, in a way that is correlated with the staggered introduction of the reform across municipalities. The most significant change that occurred during this time period is

[33] In my stacked-by-event design, when constructing the dataset, for each event, control units are assigned a "placebo" shock at event time = 0 for that specific event, which ensures that their potentially anticipatory responses are accounted for explicitly in the estimation of the dynamic treatment effect.

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 6 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, which have been always subject to the same rules since 2001. Relatedly, one might worry about the fact that the change in auditors' appointment was adopted as part of a large emergency reform, which might have affected local governments' finances in other ways beyond the change in auditor's appointment.[34] The most important one for this paper is the temporary inclusion of owner-occupied dwellings amongst houses subject to the property tax in 2012. However, it is important to remember that these changes affected contemporaneously all municipalities, while the change in auditors' appointments entered in place in a staggered way, depending on the timing of the expiration of the previous municipal auditor. Therefore, the effect of any other change related to the reform is captured by the presence of calendar time effects in my regressions.

# 5 Results

## 5.1 The Impact of Random Assignment on the Selection and Allocation of Auditors

Before moving to investigate the effect of random auditor assignment on fiscal discipline, I inspect how the reform affects the selection and allocation of auditors across municipalities. Table 2 compares the characteristics of the auditor-municipality match under discretionary appointments (pre-reform) as opposed to the ones resulting from random assignment. Columns (1) and (2) show average characteristics in the pre and post-reform periods, respectively. Column (3) shows the difference in means between the two, and column (4) the standardized differences, so as to make it easier to compare the change across different variables.[35]

Consistently with the removal of barriers to entry in the auditors' market, there is a sizeable increase in the number of municipal auditors: 22,000 auditors join the list, 60% are

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

[35] As robustness, Appendix Table A5 reports a regression version of the same Table using the main empirical specification in equation 1, where the characteristics of the auditors and auditor-municipality matches are on the left-hand side. All the outcome variables are standardized.

Table 2
Reform-induced 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 post-reform periods. Column (3) shows the difference in means between columns (1) and (2), as well as the significance level (p-values are calculated using heteroskedasticity-robust standard errors). Significance: * significant at 10%; ** significant at 5%; *** significant at 1%,. *Experience as auditor* refers to the number of years the individual has previously served as municipal auditor, *Experience as accountant* refers to the number of years as CPA, *Re-appointed* is an indicator equal to 1 if the auditor serves for 2 consecutive terms in the same municipality, *Local resident* is an indicator equal to 1 if the auditor is resident in the same municipality where she serves as an auditor, *Distance from residence* represents the distance in minutes between the municipality of appointment and the municipality of residence.

new entrants. The large entry response implies that auditors in the post-reform period are, on average, less experienced in terms of municipal auditing. On the other hand, they have a similar level of experience in terms of years spent in the accounting profession and are of similar age as in the pre-reform system, around 55 years old. This suggests that the new entrants are not young accountants that are new to the profession, but rather established accountants that were not previously appointed as municipal auditors. I then look at the average probability that auditors are re-appointed for a second term. In the pre-reform system, this happened in 57% of the cases, while in the post-reform system this becomes an extremely rare occurrence (1%), as it is the result of the same auditor being drafted again for the same municipality. Similarly, the introduction of random assignment makes it significantly less likely for municipalities to have a local resident as a municipal auditor. Under the pre-reform patronage appointment system, 32% of municipalities appointed a hometown auditor. This number goes down to 1% by virtue of the random assignment. One consequence of this change is the large increase in the average distance (in travel time) between the auditors' municipality of residence and the one where he operates as municipal auditor, increasing from 17 to over 60 minutes. Finally, I look at the probability that the auditor and the mayor share some relevant characteristics, such as gender, birthplace, or surname. In the pre-reform period, 20% of the auditors were born in the same place as the mayor, and 7% shared the same surname with a local politician. Together, these preliminary findings suggest that random assignment significantly changed the allocation of auditors across municipalities in a way that likely severed previously-existing ties between auditors and mayors. Previous research has shown that these types of connections are likely to negatively affect monitoring performance (Chu et al. (2020), Xu et al. (2021), Xu (2018)). On the other hand, auditors in the post-reform period look fairly similar to the old ones along characteristics that might impact their monitoring ability, such as age and experience.[36]

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

## 5.2 The Impact of Random Assignment on Municipal Fiscal Performance

My analysis begins with a series of graphs comparing the distribution of fiscal sustainability outcomes by treatment status. In the top panel of Figure A2 , the histogram of $NetSurplus$ shows a clear rightward shift of the distribution for values below 0 under the new random assignment system, with a much more sizeable fraction of the distribution around 0. On the other hand, the two distributions still overlap for values above 0. This suggests that the auditors operate by improving the fiscal stance of municipalities that were running deficits, while they do not necessarily induce overshoot in municipalities that were already running non-negative surpluses. The second panel shows the distribution of $DebtRepayments$. In this case, fiscal rules do not require municipalities to hit a particular target, but simply to repay their outstanding debts in a timely way. Consistent with this, I observe a rightward shift in the overall distribution. Finally, in the bottom panel, I show the distribution of $OBSDebtRepayments$. Here, no clear pattern emerges.[37] Table 3 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 stance of municipal finances.

In columns (1) and (2), I investigate the effect on $NetSurplus$. Upon the arrival of an independent auditor, treated municipalities improve on this margin by €16 per capita, an increase of 8% relative to the pre-treatment mean. In columns (3) and (4), I look at $DebtRepayments$. Again, I find a significant positive effect of comparable 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 due to the presence of strongly heterogeneous dynamics across different types of municipalities.Across all outcomes, the inclusion of controls leaves results essentially unchanged. Figure 2 displays the coefficients from the event-study specification in equation 3 and allows us to inspect the presence of differential pre-trends, as well as the dynamic of the treatment effect of the auditor's term. The first thing to notice is that the graphs do not display any significant evidence of differential pre-trends across all three outcomes. Secondly, the figures allow for inspecting the dynamics of the treatment effect over time. Interestingly, for both the $NetSurplus$ and

[37] This graph only shows the distribution of the variable for municipalities that have positive $OBSDebtRepayments$, which happens for less than 20% of my sample, given that $OBSDebtRepayments$ are a rare event.

Table 3
The Effect of Auditor's Independence on Fiscal Sustainability

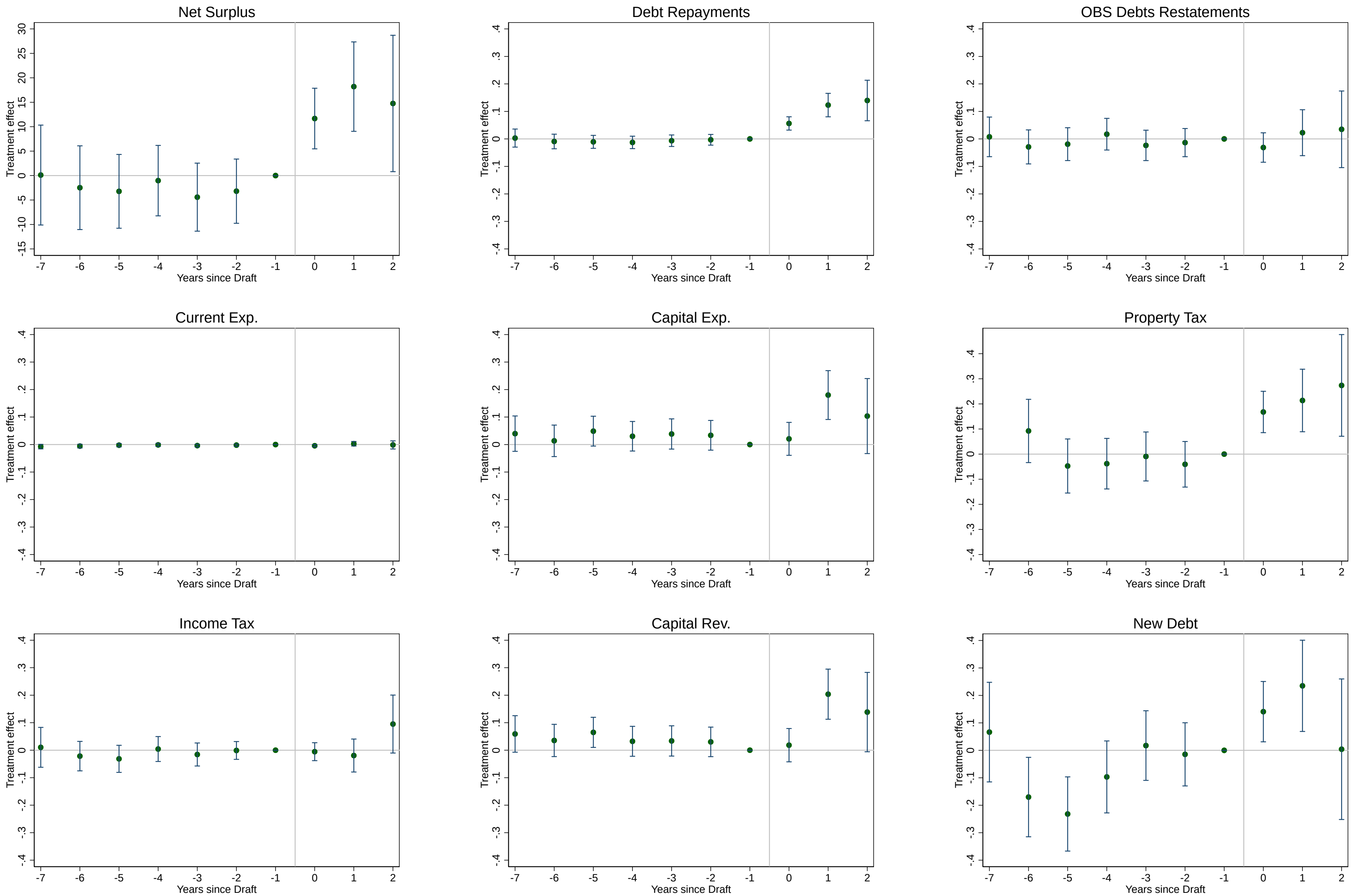
| | Net Surplus | | Debt Repayments | | OBS Debt Repayments | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated × Post | 16.45*** [3.240] | 15.88*** [3.236] | 0.0885*** [0.0140] | 0.0872*** [0.0139] | -0.00218 [0.0253] | -0.00293 [0.0253] |
| Controls | No | Yes | No |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

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

*DebtRepayments*, treatment effects appear right away, from the very first year of arrival of an independent auditor, and then remain fairly constant across the three years of the auditor's term. This pattern seems to be suggestive of a sudden and persistent shift in municipal budgetary practices in response to the treatment. Finally, the figure confirms the absence of any significant effect for *OBSDebtRepayments*.

Figure 2

The Dynamic Effect of Auditor's Independence on Fiscal Sustainability and Aggregate Spending and Revenue Choices




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

## 5.3 Margins of Adjustment: Revenues vs. Spending

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

In Table 4 I investigate the margins of adjustments of municipal governments by looking at their spending and revenue choices. All variables are measured in per capita terms and transformed using the inverse hyperbolic sine transformation.

Table 4
The Effect of Auditor's Independence on Aggregate Spending and Revenue Choices

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

*Notes:* Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms and transformed using the inverse hyperbolic sine transformation, but their non-transformed mean is reported at 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's initial age (in log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

In columns (1) and (2) I look separately at total current and capital expenditures. Results show 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%, though the effect is only marginally statistically significant. The absence of an effect on current expenditures should not be surprising. The vast majority of current expenditures are due to personnel costs and are therefore rigid, as local governments cannot fire local bureaucrats, nor can they adjust their wages. On the other hand, the increase in capital investment is a potentially desirable outcome. A frequently debated side-effect of fiscal rules' constraints imposed by the DSP pact is that municipalities have usually responded by cutting investments (Chiades and Mengotto (2015)). A potential explanation for the observed increase is that it results from political bargaining between the auditor and the mayor. In exchange for a significant improvement in fiscal sustainability, which is mostly achieved through improvements on the revenue side, mayors are allowed to expand spending on investments. The increased investment spending might be used by mayors to justify the revenue consolidation efforts with their local constituents. Consistent with this explanation, when looking separately at investment spending across different categories,. only four categories display significant increases: general administration, education, culture, and social welfare. Interestingly, these are categories that translate into directly observable goods for local residents, and might thus be particularly important from a political perspective (see Appendix Table A7 ).

In the subsequent columns, I look at the response on the revenue side. Municipalities' main sources of current revenues are the property tax and the local income tax surcharge. Column (3) shows that municipalities improve their fiscal stance by significantly increasing revenues from the local property tax, which rises by over 20%, an amount corresponding to an average increase of over €30 per capita. Specifically, I look only at revenues from property tax on properties that are not owner-occupied, as the vast majority of owner-occupied dwellings have been exempted from the property tax by national-level legislative changes for the largest part of my sample period.[38]On the other hand, no significant change can be detected for the local income tax (column (4)). Finally, in columns (5) and (6) I look at $CapitalRevenues$ and $NewDebt$. $CapitalRevenues$ increase by 4%, while $NewDebt$ increases by over 22 %. Given that, according to Italian law, municipalities can undertake new debt only to finance new investments, it is not surprising to observe an increase in $NewDebt$ in parallel with the observed increase in investment expenditures. Notably, however, the increase in total revenues to be used for investment financing ($NewDebt$ and $CapitalRevenues$) is higher than the increase in capital expenditures, thus leading to an improvement in the surplus.

The bottom rows of Figure 2 display results from event-study specifications for the outcomes in Table 4. Looking at the first row, no significant trends could be detected. However, some interesting dynamics emerge. While current revenues respond immediately from the very first period of arrival of the new auditor, both capital revenues and capital expenditures only react from the second period. The increase in $NewDebt$ instead is present only in the first two periods and then disappears, while the opposite is true for the increase in the local income tax, suggesting potential substitution patterns across different sources of revenues.

[38]The distinction between revenues from owner-occupied and other properties is not available for 2007, the first year of the sample, and therefore the analysis for this variable is restricted to the window 2008-2014. As explained in Section 4, the only year in which taxes were collected on owner-occupied units was 2012. This temporary change is not problematic for my estimates as it is common across all municipalities and thus accounted for by calendar time effects.

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

A natural question to ask is how municipalities increase their tax revenues. On the one hand, they could increase the property tax rate. On the other hand, they could expand the tax base by improving the enforcement of tax collection and reducing tax evasion upon the arrival of independent auditors. While my data are not well-suited to fully address this question, I provide some suggestive evidence on the specific margins in Table A6 . Column (1) simply reports the results of column (3) of the previous Table. In column (4), I look at the total amount of property tax revenues that is effectively collected by the end of the year. Here, the results look almost identical to the ones found in the previous columns, suggesting that the effect is coming from an expansion of the reported tax base, rather than an improvement in collection ability. In Column (7), instead, the dependent variable is the (log of) property tax rate. I obtain coefficient estimates that are insignificant and very close to 0 in magnitude. The absence of a sizeable effect on the tax rate margin further suggests that the improvement in revenues is coming from an expansion of the tax base. In line with this explanation, in columns (2) and (3), I show that the effects are stronger in municipalities that have a higher share of non-declared units, based on the matching between tax declarations and cadastral data done by the National Tax Agency, and where there was a higher share of so-called "ghost buildings", as documented in Casaburi and Troiano (2016).[39] This result speaks to the literature on the determinants of fiscal capacity and is consistent with the findings of Xu (2018), which shows that patronage-appointed governors raised fewer taxes and invested less in the revenue capacity of British colonies.[40]

Taking stock of the results presented so far, I argue that the arrival of draft-assigned

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

[40] In contrast, Balan, Bergeron, Tourek and Weigel (2022) find that delegating tax collection to local leaders, as opposed to state officers, improves tax collection capacity in the Democratic Republic of Congo (DRC). A key distinction is that in my context, the local versus central trade-off involves the appointment of monitors who check on the behavior of local leaders, who still retain the power of tax collection. Furthermore, the value of local information is potentially significantly higher in a context like the DRC, with respect to Italy.

auditors indeed improved monitoring and spurred more fiscal discipline, with municipalities running higher surpluses and achieving the adjustment through a sizeable improvement in property tax collection.

## 5.4 Robustness

In this section, I discuss a series of empirical tests that I perform to corroborate the robustness of my analyses. First, I apply three alternative estimators, the standard TWFE estimator, as well as two alternative robust estimators recently proposed by de Chaisemartin and D'Haultfœuille (2020) and Callaway and Sant'Anna (2021) to solve issues with treatment effect heterogeneity in TWFE estimators. Results are displayed in Table A8 and A9 and Figure A6 . Reassuringly, estimates are all very similar to the ones in Table 3. Further details on these alternative estimators are provided in Appendix A3. I then perform a series of alternative specifications to further corroborate the robustness of my results. In Table A10 , I re-estimate the main model but include flexible trends in the log of population and municipal income in the pre-reform period as controls. This ensures that results are not driven by the differential effect over time of different pre-treatment municipalities' characteristics. In Table A11 , I include a set of 135 $Region * Year$ fixed effects, which ensure that the results are not driven by differences in region-specific trends over time. While lower in magnitude, results remain highly significant and qualitatively similar. In Table A12 , I show that results are robust to account for the presence of spatial correlation in the data, by re-estimating the model using (Conley 1999; 2010) standard errors. Furthermore, I verify whether effects are potentially driven by specific groups of municipalities. First, I show that I am able to detect significant effects for both small and large municipalities (see Panel B of Table 5). This specification compares treated and control municipalities of similar sizes (say, below 5,000 inhabitants) ensuring that results are not driven by potentially differential selection in treatment timing by population size. Second, I perform a heterogeneity split by geography and show that significant effects are present across the whole country (see Table A13 ). While I find significant effects for all groups, I observe responses along different margins for different groups. In the South and in large municipalities, effects are concentrated on the $DebtRepayments$ margin, with negligible effects on the $NetSurplus$, while the opposite is true for the North and for small municipalities.[41]

[41] In a concurrent working paper, Barone, Conti, Narciso and Tonello (2020) similarly use a DiD approach to evaluate the reform's effects on a slightly different set of outcomes. In contrast to my findings, they find that independent audits are associated with a worsening of municipal public finances. This difference may be due to differences in the estimation sample (which, in Barone et al. 2020, includes only municipalities in special regions), the definition of the treatment variable (which depends on the number of months an auditor has been in place), and the empirical design (a two-way fixed effects model, potentially leading to bias from

# 6 Mechanisms

In this section, I discuss the mechanisms through which the reform most plausibly operates. The switch from discretionary appointment to random assignment is affecting three different margins. First, it is potentially changing the selection of auditors—who gets hired and who decides to participate in the auditors' pool; Section 6.1 investigates the role played by selection. Second, it is changing the matching of the auditors to municipalities—who gets allocated where; Section 6.2 assesses the role played by matching. Third, it affects the incentives of auditors—what they are supposed to do and how are they motivated to do it; Section 6.3 explores the role of incentives in explaining the results. Finally, using the lenses of the standard models of crime (Becker and Stigler 1974), the strengthening in monitoring effectiveness can improve outcomes through a direct effect (detection) and an indirect effect (deterrence). Sections A4.1 and A4.2 explore the respective roles of these channels.[42]

## 6.1 Selection

By removing barriers to entry based on patronage networks, the new appointment system is changing the pool of potential auditors, thus affecting the selection margin. As common in markets for experts (Ronen 2010, White 2010), despite the high degree of potential competition in the market, the pre-reform discretionary appointment system was characterized by a relatively limited number of auditors who performed audit tasks for multiple municipalities.[43] Removing discretionary appointments allowed new auditors to enter the market. If these new entrants are of higher quality, less corruptible, or more likely to enforce national government objectives, then the selection margin might drive the observed treatment effects. Indeed, as discussed in Section 5.1, the reform induced a change in the composition of the auditor pool: almost 14,000 new auditors joined the list, representing around 60% of the pool of potential auditors. While these new entrants have no municipal-specific experience, they look otherwise very similar to the existing pool of incumbents on other observable dimensions (see Table 2). Most importantly, they are equally experienced in terms of general private-sector accounting, so the change in the selection method does not seem to come at

negative weighting problems (Goodman-Bacon 2021). In Appendix Table A14 I replicate the main results of Barone et al. (2020) and conclude that the differences are driven by the inclusion of special regions in the estimation sample.

[42] All heterogeneity analyses are performed using the same specification of the main results, the one of equation 4. As a robustness check, I also repeat all the analyses using the standard two-way fixed effects estimator of 1 in Table A17 and obtain very similar results.

[43] Note, however, that auditors, by law, could only have at most eight concurrent appointments, so this naturally put a limit on market concentration before the reform.

the cost of compromising quality. However, it could well be that new entrants are different from incumbent auditors along other unobservable dimensions, for example, honesty.

To assess the role of selection in explaining my results, I perform three different types of exercises. To test whether the effects are driven by the reform-induced change in auditors' characteristics, I first adopt the same DiD specification as in equation 4 but include the observable characteristics of auditors as controls. Panel A of Table 5 displays the results. Note that by virtue of random assignment, these observables are all orthogonal to municipal characteristics. Therefore, comparing the magnitude of these coefficients to the ones in Table 3 is informative about the role played by changes in auditors' observable characteristics. Overall, the results suggest that these characteristics account for 30% to 50% of the reform effect.

Because in these three groups the law imposes different experience requirements, I then repeat the main analysis separately for a) municipalities with less than 5,000 inhabitants, b) municipalities that have between 5,000 and 15,000 inhabitants, and c) municipalities with more than 15,000 inhabitants. In municipalities with more than 5,000 inhabitants, the selection channel is shut off by construction as only auditors with experience can be drafted. Panel B of Table 5 shows significant results across the three groups of municipalities, thus rejecting the possibility that selection is the only driver of the effects.[44]

I next gauge the exogeneity of random assignment in the post-reform period to compare the outcomes of municipalities that, by chance, are assigned a new entrant to those that are assigned an already experienced one. Note that in this comparison, I am limiting attention to municipalities with less than 5,000 inhabitants as these are the only ones that could be assigned new entrants. Table 5, Panel C displays the results and shows that, at least for the subsample of smaller municipalities, the effects are essentially driven by the assignment of new entrants. Overall, the results suggest that selection indeed plays an important role but cannot explain the entirety of the effects.

## 6.2 Matching

By removing control of appointment from the mayor, the reform affects the allocation of auditors to municipalities, moving from a system of endogenous matches to one of random ones. This change has two potential effects. On the one hand, random matching increases the auditor's bargaining power as it is essentially removing both psychological and rational

[44]The heterogeneity also suggests that the effects on debt repayments are significantly stronger in larger municipalities. This should not be surprising as larger municipalities are the ones more likely to be indebted. Consistent with this explanation, I also detect a significant positive treatment effect on $OBSDebtRepayments$ for municipalities that have more than 15,000 inhabitants.

Table 5
Impact of the Reform on Fiscal Sustainability, the Role of Selection

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

| | PANEL B: | By Population Size | |
|---|---|---|---|
| | (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 |

| | PANEL C: | New Entrants vs. Incumbent Auditors | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Treated × | 2.311 | -0.00252 | -0.0174 |
| Post | [6.719] | [0.0208] | [0.0382] |
| Treated × | 21.29*** | 0.0439** | 0.00218 |
| Post × New Entrant | [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 |

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

motives to please the mayor and thus reduces the risk of collusion or the auditor's capture by the mayor. This, however, might come at the cost of sacrificing the potential value of discretionary appointment and could induce misallocation. Mayors are likely to be better informed about the characteristics of auditors who best match their specific needs, and discretion might have thus enabled mayors to choose skilled, well-informed auditors. This type of tension is a classic example of a delegation problem, leading rise to a capture-vs.-information trade-off (Holmström 1977; Holmström 1984; Kelman 1990), where mayors might use discretion for good (Liu and Zhang 2021, Voth and Xu 2019)—to select better auditors—or abuse of it—to hire more corruptible ones (Xu 2018, Xu et al. 2021, Colonnelli et al. 2020). A useful insight from optimal delegation models is that the benefits of discretion increase with alignment with the organizational objectives. Thus, we might expect random assignment to be more valuable in places where mayors are less aligned with the interests of the central government in the pre-reform period.

I, therefore, adopt several different proxies to identify places where lax or corrupt monitoring was more likely to occur before the reform, including unhealthy financial conditions, the appointment of a local resident as auditor, and the presence of corruption investigations.

If independent auditors act to efficiently ensure the fiscal sustainability of municipal finances, then we should not expect to observe any increase in surpluses in municipalities that were already in good standing before the arrival of the draft-appointed auditors. If anything, independent auditors might spur a reduction in surpluses to push municipalities that were running excess surpluses closer to a balanced budget. In Panel A of Table 6 I repeat the analysis of Table 3 but include an interaction with an indicator that 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 in all pre-treatment years, is always 0. Therefore, for this variable, I construct an indicator equal to 1 if the municipality has never had a positive amount of $OBSDebtRepayments$ in the pre-period and 0 otherwise.

The results indeed show that the treatment effects are significantly larger in municipalities that were less fiscally sustainable before the reform. The treatment effect for the $NetSurplus$ is twice as large for municipalities that had a $NetSurplus$ below the median value in 2011. An even more striking difference emerges when looking at the effects on $DebtRepayments$, where the treatment effect is four times as large. However, in both cases, the treatment effect, while smaller, is also still positive and significant for municipalities that were already in a relatively good fiscal stance in the pre-period.

Finally, the last column shows that the treatment effect for municipalities that never had $OBSDebtRepayments$ in the past is positive, significant, and very large in magnitude. In contrast, the treatment effect for those that 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 3 and suggests the presence of two very different underlying phenomena. Differently from all the other components of the budget, $OBSDebtRepayments$ represent a self-declaration of an illicit spending act. Thus, auditors can act on two different margins: they can pressure the mayor and/or the city council to report anything about $OBSDebtRepayments$, or they can make it impossible or very costly to commit illicit spending. While I cannot effectively separate the two mechanisms without having information about the unobserved amounts of illicit spending, the divergent signs of the effects in the sample split suggest the presence of two types of municipalities. In "honest" municipalities—those 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 $OBSDebtRepayments$. Whereas in "dis-honest" municipalities, the arrival of the independent auditor induces the reporting of illicit spending, thus leading to an increase in $OBSDebtRepayments$.

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. I first use an indicator for whether the last auditor appointed before the reform was born or resided in the same municipality. Intuitively, a "local" auditor is more likely to have connections with the mayors and/or be more sensitive to local interests and thus be willing to collude to favor local spending vis-a-vis fiscal sustainability (Chu et al. 2020; Xu et al. 2021; Xu 2018). Table 6, Panel B displays the results, where I include a triple interaction with an indicator for municipalities that had a local auditor before the reform. Across all outcomes, the 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 did not have a local auditor is not only insignificant but also negative. In contrast, 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 $OBSDebtRepayments$, reviewing and expressing an opinion on the restatement process and signalling 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, the results suggest that municipalities with local auditors had a larger amount of unrecognized OBS debtsand upon the arrival of an independent auditor were more likely to restate them.

Last, 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*).[45] The final data contain 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.[46]

Appendix Figure A7 shows a map of municipalities that have the *Corrupt* dummy equal to 1. While one would ideally like to know directly whether local politicians have been investigated for corruption, these 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 occur. Furthermore, corruption-related crimes are frequently linked to local public procurement or other forms of local public spending, which are factors that should be subject to auditors' monitoring. Thus, a context in which corruption-related crimes occur is more likely to be one in which the mayor might have the incentive to hire a lenient local auditor who would not uncover illicit practices.

Table 6, Panel C displays the results. The patterns of the results are very similar to the ones found in Panel A, with a much stronger gap displayed in the last two outcomes. For *DebtRepayments*, the treatment effect is four times larger in corrupt municipalities (0.06 versus 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%. In contrast, in corrupt municipalities, *OBSDebtRepayments* increases by 27%. Again, such a large divergence might also explain the absence of a significant average treatment effect in Table 3, as it results from a composition of very different phenomena. The negative treatment effect in non-corrupt municipalities suggests a reduction in the actual size of OBS debts. On the other hand, the significant increase in *OBSDebtRepayments* in corrupt municipalities suggests the presence of a large amount of previously hidden OBS debts in these places that are brought to the table by the arrival of randomly assigned auditors.

I also investigate the role of the characteristics of the post-reform municipality-auditor match. In particular, one would expect to observe stronger treatment effects in places where the randomly assigned auditor is less likely to have previous connections and be biased or captured. Vice versa, if the results are entirely driven by the change in auditors' allocation,

[45]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.

[46]These include bribery, corruption, malfeasance, and embezzlement.

we should not expect to observe any significant change if, by chance, random assignment re-creates a situation similar to the pre-reform system. On the other side of the independence-embeddedness trade-off, it could be that a local auditor has more valuable information to leverage that can improve their monitoring effectiveness.

To proxy for the strength of auditors' social proximity in the post-reform matching, I leverage the fact that I have information on the auditor's residence and auditors are randomly assigned to municipalities. I thus calculate the travel distance between the auditor's municipality of residence and the municipality she is assigned to audit. While not perfect, distance is a viable proxy for social proximity (Xu et al. 2021; Chu et al. 2020). Furthermore, the results in the previous subsection show that the treatment effects are larger in municipalities where the previous auditor was a local resident, thus suggesting that distance indeed matters for audit outcomes.

The results in Panel D of Table 6 show that the effect of the reform is entirely driven by municipalities that, by chance, receive an auditor who lives at least one hour away, which is the median distance in the sample.The absence of treatment effects for municipalities that randomly happen to receive a local auditor is particularly intriguing, as it further sheds light on the strength of social ties and on how home bias might adversely affect bureaucratic performance (Xu et al. 2021; Chu et al. 2020) even in the presence of random assignment. To further shed light on the mechanisms behind this result, I investigate whether the role of the post-reform distance varies depending on pre-reform choices. Intuitively, we would expect the value of being assigned an outsider to be largest for municipalities that were previously selecting local residents as auditors. In Table A15 , I thus further split the sample between municipalities that had appointed a local resident as an auditor in the pre-reform period and municipalities that instead already appointed an auditor from a different municipality.[47] Indeed, the results confirm that the treatment effects of being assigned a distant auditor are twice as large for municipalities that used to appoint local residents in the pre-reform period.

Combined, the results suggest that both selection and matching are important drivers of the overall effects, both pointing toward the importance of ensuring the presence of an independent and "external" eye in the monitoring process.

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

Table 6
The Effect of Auditor's Independence on Fiscal Sustainability, the role of Matching

| | PANEL A: Pre-Reform Fiscal Stance | | |
|---|---|---|---|
| | Net Surplus | Debt Repayments | OBS Debt Repayments |
| | (1) | (2) | (3) |
| Treated × Post | 10.90*** [3.559] | 0.0350** [0.0150] | -0.176*** [0.0466] |
| Treated × Post × Below Median | 12.42** [5.616] | 0.106*** [0.0237] | 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 |

| | PANEL B: Pre-Reform Local Auditor | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Treated × Post | 10.35** [4.984] | 0.0876*** [0.0226] | -0.0432 [0.0416] |
| Treated × Post × Local Auditor | 0.461 [7.483] | 0.0755* [0.0428] | 0.110 [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 C: Corrupt Municipality | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Treated × Post | 15.88*** [3.483] | 0.0637*** [0.0137] | -0.0330 [0.0249] |
| Treated × Post × Corrupt | -3.545 [8.432] | 0.181*** [0.0551] | 0.282*** [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 |

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

*Notes:* Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are in parentheses. $BelowMedian$ 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. $LocalAuditor$ is equal to 1 if any of the municipality's appointed auditors before the reform was either born or resident in the municipality. $Corrupt$ is an indicator flagging whether there was at least one investigation for corruption-related crimes from 2004 to 2013 in the municipality. $AtLeast1hr$ is an indicator flagging whether the driving distance between the auditor's residence and the municipality is at least 1 hour (average distance is 65 min). The sample here is restricted to municipalities below 5,000 inhabitants.

## 6.3 Incentives

Beyond the selection and allocation of auditors, the reform is likely to change auditors' incentives and behavior. Under the pre-reform system, auditors could be reappointed for a second term by mayors. However, this provides strong incentives for the auditors to please the mayor who appointed them during their first term, and it gives the mayor a clear retaliation mechanism. Pleasing the mayor, however, would come at the cost of potential ex-post sanctions from the Court of Auditors should the auditors be found liable for lax or corrupt monitoring. In addition, auditors had a more general incentive to build a reputation for "leniency" under the old patronage system since they could be appointed by other mayors. This, however, might have reduced the private market value of auditors as firms might not be willing to hire a corrupt auditor. Thus, under the old system, auditors experienced a clear tension: career incentives pushed them to be lax to please the mayor while being at risk of getting sanctioned by the Court. In the post-reform system, auditors have incentives to monitor effectively in order to please the Court of Auditors as well as to preserve their reputation in the private market.

To identify the role played by the change in incentives, I perform two different exercises. First, I exploit within-auditor variation in connections to the incumbent mayor in the pre-reform period to assess whether changes in incentives to please the mayor affect auditors' behavior and municipal fiscal outcomes. I exploit the fact that, even before the reform, auditors were not always working for the mayor that appointed them due to the difference in the length of auditors' and mayors' mandates. Italian mayors' terms last five years, while auditors' terms last only three (renewable for one time). This gives rise to within-auditor exogenous variation in the connection between auditor and mayor (see the bottom panel of Figure A9 and Appendix Figure A8 for an example). 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 the years in which mayors controlled the appointment. On the other hand, 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:

$$Y_{mt} = \alpha_m + \delta_p t + \beta Control_{mt} + X'_{mt}\zeta + \epsilon_{mt}, \tag{4}$$

where $Control_{mt}$ isolates the effect of the shock in connections with the mayor.

These estimates are based on within-auditor variation, so they net out any unobserved fixed auditor-specific confounders and hold constant any selection margin. Importantly, the probability of being connected with the mayor increases the longer a mayor stays in power. 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 $\beta$ is estimated by comparing auditors who serve for mayors who are at similar points of the election cycle but, for exogenous reasons, either were or were not appointed by them.

Table 7, Panel A shows the results. In the odd columns, I also include all second-term mayors and introduce a control for them interacted with the pre-election dummy. In the even columns, I instead restrict attention to first-term mayors. Notably, I find that during the years in which mayors and auditors are connected, municipalities run higher deficits and decrease their debt repayments. This suggests that under the old quasi-patronage system perform, auditors are less effective when working for the mayor who appointed them. These results are in line with previous findings from (Xu 2018, Xu et al. 2021) and show how, respectively, connected governors in the British Empire and connected bureaucrats perform significantly worse while connected to higher-level officials. From a policy perspective, these results also provide evidence that introducing a gap between the political and the audit cycle could be an alternative system to at least strengthen oversight partially by improving auditors' incentives to monitor effectively.

Second, I compare the effect of the reform for municipalities in which the last appointed auditor was in his first versus second term. Intuitively, auditors in their second term should have had fewer incentives to be lenient toward the mayor as, for them, career incentives are muted. This would mean that we should expect to observe stronger effects for municipalities with first-term auditors. On the other hand, it could have been that, having learned about the reform, first-term auditors know that they can no longer be re-appointed, so their career incentives are also muted, assuming that they indeed know that the reform is coming and change their monitoring behavior. It is therefore an interesting empirical question. Table 7 reports the results. Indeed, I find suggestive evidence in the direction of the former hypothesis, as the effects are significantly stronger in municipalities with a first-term, as opposed to a second-term, auditor.[48]

[48] I also assess whether auditors perform differently in the pre-reform period. While I lack the power to detect significant changes, the results in Appendix Table A16 are at least suggestive that auditors in their first term are more lenient than the ones in their second term and that municipalities that re-appointed their auditors tend to perform worse.

Table 7
The Effect of Auditor's Independence on Fiscal Sustainability, The Role of Incentives

| | PANEL A: | | by Connection in the Pre-Reform Period | | | |
|---|---|---|---|---|---|---|
| | Net Surplus | | Debt Repayments | | OBS Debt Repayments | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Connected | -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 |

| | PANEL B: | | by Term of Pre-Reform Auditor | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated | 9.299** | 10.27** | 0.0952*** | 0.132*** | -0.0201 | -0.0543 |
| × Post | [3.715] | [4.825] | [0.0181] | [0.0268] | [0.0333] | [0.0462] |
| Treated | | -1.957 | | -0.0744** | | 0.0686 |
| × Post× Second | | [6.407] | | [0.0295] | | [0.0545] |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Dep. Var Mean | -191.7 | -191.7 | 65.09 | 65.09 | 3.204 | 3.204 |
| Observations | 91846 | 91846 | 91846 | 91846 | 91846 | 91846 |
| Adj. R-sq | 0.693 | 0.693 | 0.757 | 0.757 | 0.427 | 0.427 |

*Notes:* Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation. *Connected* is a dummy equal to 1 for years in which the mayor in power is the one that had appointed the auditor, and is constructed using exogenous variation arising from the difference between the auditor (3 years) and election (5 years) cycle. The sample is restricted to pre-treatment years. *Second* is an indicator equal to 1 if the last auditor before the introduction of the random draft was in her second term (as opposed to the first). This information is not available for the entire sample, so even columns repeat the baseline regression for the restricted sample for which *Second* is not missing. 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's initial age (in log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

### 6.4 Additional Mechanisms

Using the lenses of the standard models of corruption (Becker and Stigler 1974, Shleifer and Vishny 1993), the strengthening in monitoring effectiveness can improve outcomes through both a direct effect, whereby randomly assigned auditors detect inappropriate practices and induce local politicians to change their behavior; and an indirect (deterrence effect), leading to a change in behavior even without any action of auditors. To obtain the first effect, I investigate the interplay between auditors' appointments and electoral accountability and I find consistent evidence that the response was somewhat stronger in municipalities where mayors faced lower reelection pressures. Results are reported in Appendix Table A18 and discussed more in detail in Appendix Section A4.1. To identify the deterrence effect, I investigate the presence of the reforms' spillover effects. Specifically, I exploit the staggered introduction across municipalities and the geographic variation in the exact timing of the audits to identify the spillover effects on nearby yet-to-be-treated municipalities, assuming that proximity makes the audits salient even for yet-to-be-treated municipalities. I detect sizable spillover effects since being closer to a treated municipality has an impact on a yet-to-be-treated municipality that is around 60% the size of the main effects. I then re-estimate the main results, excluding from the control group all municipalities that neighbor a treated one. I obtain estimates that are almost twice as large as the main effects, highlighting the importance of accounting for spillover effects. Results are reported in Appendix Table A19 and discussed more in detail in Appendix Section A4.2.

## 7 Discussion and Conclusion

While considered a crucial tool for good governance, effective monitoring is frequently impaired by conflicts of interest. However, this paper highlights that changes in the design of monitoring institutions can significantly improve governance outcomes. Taking advantage of a large-scale reform that changed the appointment system of auditors for municipal government budgets in Italy, in this paper, I investigate the causal effects of removing appointment control from mayors and introducing a random assignment of auditors. Three main findings arise. 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, the treatment effects are significantly larger for municipalities that were more at risk of collusion before the reform and for those matched to a more distant or less connected auditor in the post-reform period.

These findings have important policy implications and can inform the global debate about policies to curb corruption and solve conflicts of interest in monitoring. They also provide two important policy takeaways. One pertains to the reform's cost-effectiveness. Rather than introducing a new auditing system (e.g., federal audits in Brazil) or an additional supervisory board (e.g., the PCAOB for U.S. 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 takeaway relates to the auditors' identity. As distinct from other types of reforms or experimental studies, the auditors here are not central government bureaucrats—as in the well-known case of Brazil (Ferraz and Finan 2008) or Puerto Rico—nor are they village members—as in the case of Indonesia (Olken 2007). They are Certified Public Accountants hired directly by municipalities to carry out auditing duties, and auditing governments is only a limited part of their work activities. This suggests a potentially important role for external professionals to carry out audit roles in other settings as well.

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

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 did not 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, Favero and Giavazzi 2019) and negatively affect local welfare. In a concurrent work in progress, I am evaluating the electoral consequences of the reform, at the local and at the national level. The results suggest that local welfare is indeed negatively impacted, as the local electorate responds negatively to the reform, but it correctly attributes the responsibility of the increased austerity to the national government.

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# Online Appendix

# A1 Additional Tables and Figures

Table A1
Auditor's Main Tasks *ex lege 239 TUEL & dlgs 118/2011*

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

*Notes:* The table represents a summary of the main tasks of auditors, as regulated by Italian law, *ex lege 239 TUEL & dlgs 118/2011* . Additional details can be found in Miani, Sequi and Di Russo (2019) and ODCEC (2009).

Table A2
Predictors of Drafted Auditors' Rejecting Appointment

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

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

Table A3
Municipal Characteristics that Predict Treatment Timing.

| | 2012 Cohort | 2013 Cohort | 2014 Cohort | 2015 Cohort |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| 1Year After Election | 0.00273 | 0.0405 | 0.0222 | -0.0655** |
| | [0.0178] | [0.0264] | [0.0281] | [0.0301] |
| 2Years After Election | -0.0235** | -0.0318** | 0.0530*** | 0.00231 |
| | [0.00962] | [0.0143] | [0.0169] | [0.0180] |
| 2Years Before Election | 0.0110 | 0.0520* | -0.0646** | 0.00158 |
| | [0.0189] | [0.0266] | [0.0262] | [0.0312] |
| 1Year Before Election | 0.00352 | 0.00435 | 0.0178 | -0.0257 |
| | [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. 15-60k | 0.104*** | 0.0491** | -0.200*** | 0.0472* |
| | [0.0189] | [0.0223] | [0.0212] | [0.0263] |
| Pop. Above 60k | 0.202*** | 0.0959* | -0.224*** | -0.0735 |
| | [0.0562] | [0.0583] | [0.0452] | [0.0577] |
| Municipal Union | -0.00900 | 0.0100 | -0.0182 | 0.0171 |
| | [0.00873] | [0.0141] | [0.0173] | [0.0181] |
| Mayor Age (log) | 0.00816 | 0.00678 | 0.0501* | -0.0651** |
| | [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 Mayor | 0.00921 | 0.0299*** | -0.0179 | -0.0212 |
| | [0.00778] | [0.0115] | [0.0136] | [0.0143] |
| Mayor Resignation | -0.00275 | 0.0503** | -0.0374 | -0.0101 |
| | [0.0163] | [0.0255] | [0.0261] | [0.0290] |
| Mayor Term-limited | -0.00566 | -0.00513 | 0.0145 | -0.00370 |
| | [0.00696] | [0.0107] | [0.0130] | [0.0136] |
| Council Dismissal | -0.00336 | 0.0819 | -0.0140 | -0.0646 |
| | [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 are reported in square brackets.

Table A4
Levels of Oucomes in 2010, by Treatment Timing

| | A. Fiscal Sustainability | | | | | |
|---|---|---|---|---|---|---|
| | 2012 Cohort | 2013 Cohort | 2014 Cohort | 2015 Cohort | Uncond. F-test | Cond. F-test |
| Net Surplus | -332.48 | -336.70 | -327.95 | -310.64 | 0.001 *** | 0.251 |
| Debt Repayments | 60.60 | 61.92 | 64.54 | 62.91 | 0.413 . | 0.959 |
| OBS Debts Repayments | 5.85 | 5.16 | 2.70 | 3.36 | 0.000 *** | 0.791 |

| | B. Revenue and Spending Choices | | | | | |
|---|---|---|---|---|---|---|
| | 2012 Cohort | 2013 Cohort | 2014 Cohort | 2015 Cohort | Uncond. F-test | Cond. F-test |
| Current Exp. | 895.74 | 885.98 | 907.66 | 871.69 | 0.044 ** | 0.406 |
| Capital Exp. | 505.93 | 544.56 | 543.26 | 507.68 | 0.367 . | 0.678 |
| Property Tax | 126.03 | 119.58 | 144.18 | 144.77 | 0.000 *** | 0.384 |
| Income Tax | 40.05 | 36.82 | 40.61 | 41.11 | 0.001 *** | 0.233 |
| Capital Rev. | 458.40 | 471.34 | 460.69 | 427.97 | 0.295 . | 0.735 |
| New Debt | 122.87 | 150.55 | 131.16 | 126.59 | 0.115 . | 0.718 |

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

Table A5
Changes in Auditors' Characteristics, Regression Analysis

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

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

Table A6

The Effect of Auditor's Independence on Tax Capacity

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

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

Table A7
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 × Post | 0.0719* [0.0375] | -0.0334 [0.0396] | 0.0952** [0.0441] | 0.0726** [0.0319] | -0.0211 [0.0265] | 0.00414 [0.0451] | 0.0362 [0.0436] | 0.0726* [0.0392] |
| Dep. Var Mean | 65.86 | 22.24 | 35.04 | 14.36 | 17.72 | 128.6 | 170.3 | 27.34 |
| Observations | 112320 | 60500 | 113004 | 113025 | 113024 | 112909 | 112756 | 113004 |
| R-sq | 0.485 | 0.471 | 0.343 | 0.365 | 0.431 | 0.386 | 0.453 | 0.318 |

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

Table A8
Impact of the Reform on Fiscal Sustainability, TWFE model, Testing Robustness to Heterogeneous Treatment Effects as in de Chaisemartin and D'Haultfœuille (2020)

| | Net Surplus | | Debt Repayments | | OBS Debt Repayments | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Indep. Auditor=1 | 17.81*** [3.233] | 16.12*** [3.038] | 0.0866*** [0.0123] | 0.0804*** [0.0125] | -0.0100 [0.0229] | -0.0115 [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 | 0.603 | 0.624 | 0.713 | 0.720 | 0.362 | 0.363 |
| % ATTs with negative weights | 29.5 | 29.5 | 29.5 | 29.5 | 29.5 | 29.5 |
| Sum of negative weights | -0.106 | -0.106 | -0.106 | -0.106 | -0.106 | -0.106 |
| $\underline{\sigma_{fe}}$ | | 12.62 | | 0.06 | | 0.009 |
| $\underline{\underline{\sigma_{fe}}}$ | | 50.35 | | 0.26 | | 0.04 |

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

Table A9
Impact of the Reform on Fiscal Sustainability, Using the Estimator Developed by Callaway and Sant'Anna (2021)

| | Net Surplus | | Debt Repayments | | OBS Debt Repayments | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ATT | 9.070*** | 7.494** | 0.0695*** | 0.0587*** | -0.0111 | -0.00548 |
| | [3.369] | [3.400] | [0.0134] | [0.0135] | [0.0300] | [0.0305] |
| Controls | No | Yes | No | Yes | No | Yes |
| Dep. Var Mean | -205.8 | -205.8 | 64.47 | 64.47 | 3.435 | 3.435 |
| Observations | 44824 | 44824 | 44824 | 44824 | 44824 | 44824 |

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

Table A10
Impact of the Reform on Fiscal Sustainability, Including Municipal Pre-treatment Characteristics as Controls

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

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

Table A11
Impact of the Reform on Fiscal Sustainability, Allowing for Region-Specific Non-parametric Trends

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

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

Table A12
Impact of the Reform on Fiscal Sustainability, accounting for Spatial Correlation in the Data

| | Net Surplus | Debt Repayments | OBS Debt Repayments | | | |
|---|---|---|---|---|---|---|
| | Surplus | | Net Surplus | | Debt Repayments | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated=1 × Post=1 | 15.88*** | 15.88*** | 0.0872*** | 0.0872*** | -0.00293 | -0.00293 |
| | [3.236] | [3.566] | [0.0139] | [0.0149] | [0.0253] | [0.0192] |
| Conley S.E. | No | Yes | No | Yes | No | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 114028 | 114028 | 114028 | 114028 | 114028 | 114028 |
| Adj. R-sq | 0.700 | | 0.757 | | 0.413 | |

*Notes:* Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. In odd columns, standard errors clustered at the municipality level are reported in parentheses. In even columns, I reported standard errors adjusted for spatial correlation in the data for up to 50 km using (Conley 1999; 2010) methodology. 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's initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A13
Impact of the Reform on Fiscal Sustainability, by 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:* Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation. All regressions include municipality, year, election cycle fixed effects, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A14
Impact of the Reform on Fiscal Sustainability, Replication of Table 3 in Barone et al. (2020)

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

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

Table A15
Impact of the Reform on Fiscal Sustainability, by Pre-reform and Post-Reform Distance

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

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

Table A16
Impact of Auditor's Career Incentives on Fiscal Sustainability in the Pre-Reform Period

| | Net Surplus | | Debt Repayments | | OBS Debts Restatements | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Re-appointed=1 | -5.278<br>[4.614] | | -0.0193<br>[0.0139] | | 0.0271<br>[0.0346] | |
| Second-Term Auditor=1 | | 4.886<br>[4.773] | | -0.00121<br>[0.0105] | | -0.0182<br>[0.0360] |
| Dep. Var Mean | -214.1 | -182.8 | 62.70 | 66.76 | 3.813 | 3.016 |
| Observations | 19824 | 14838 | 19824 | 14838 | 19824 | 14838 |
| R-sq | 0.773 | 0.721 | 0.820 | 0.823 | 0.538 | 0.483 |

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

Table A17
Impact of the Reform on Fiscal Sustainability, Heterogeneity Analysis using TWFE model

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

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

Table A18
The Effect of Auditor's Independence on Fiscal Sustainability, the role of Electoral Accountability

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

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

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

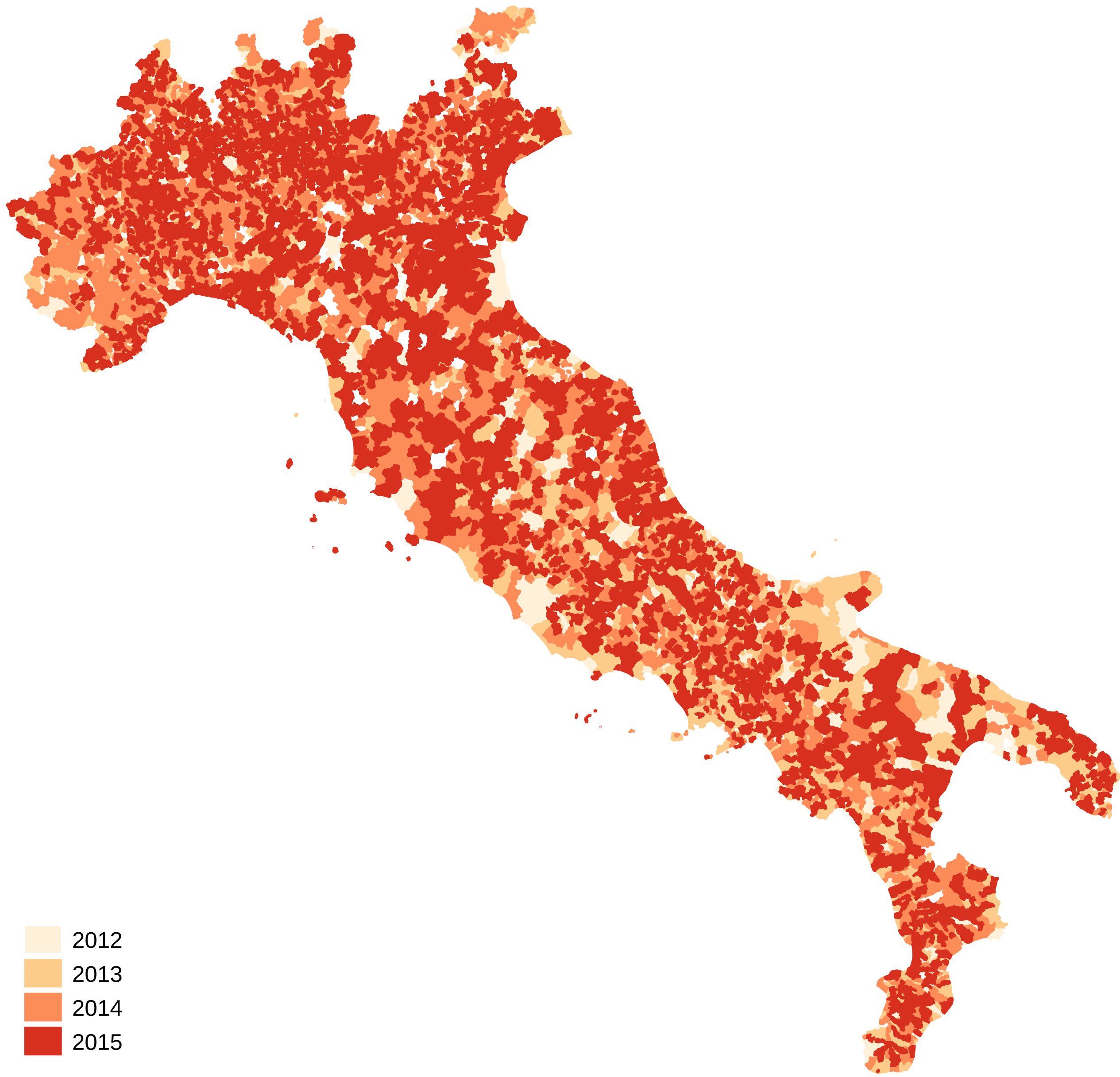
*Notes:* Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation. $Pre-ElectionYears$ and $Term-Limited$ are two proxies for the strength of electoral accountability faced by the 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's initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A19
The Effect of Auditor's Independence on Fiscal Sustainability, Spillover Effects

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

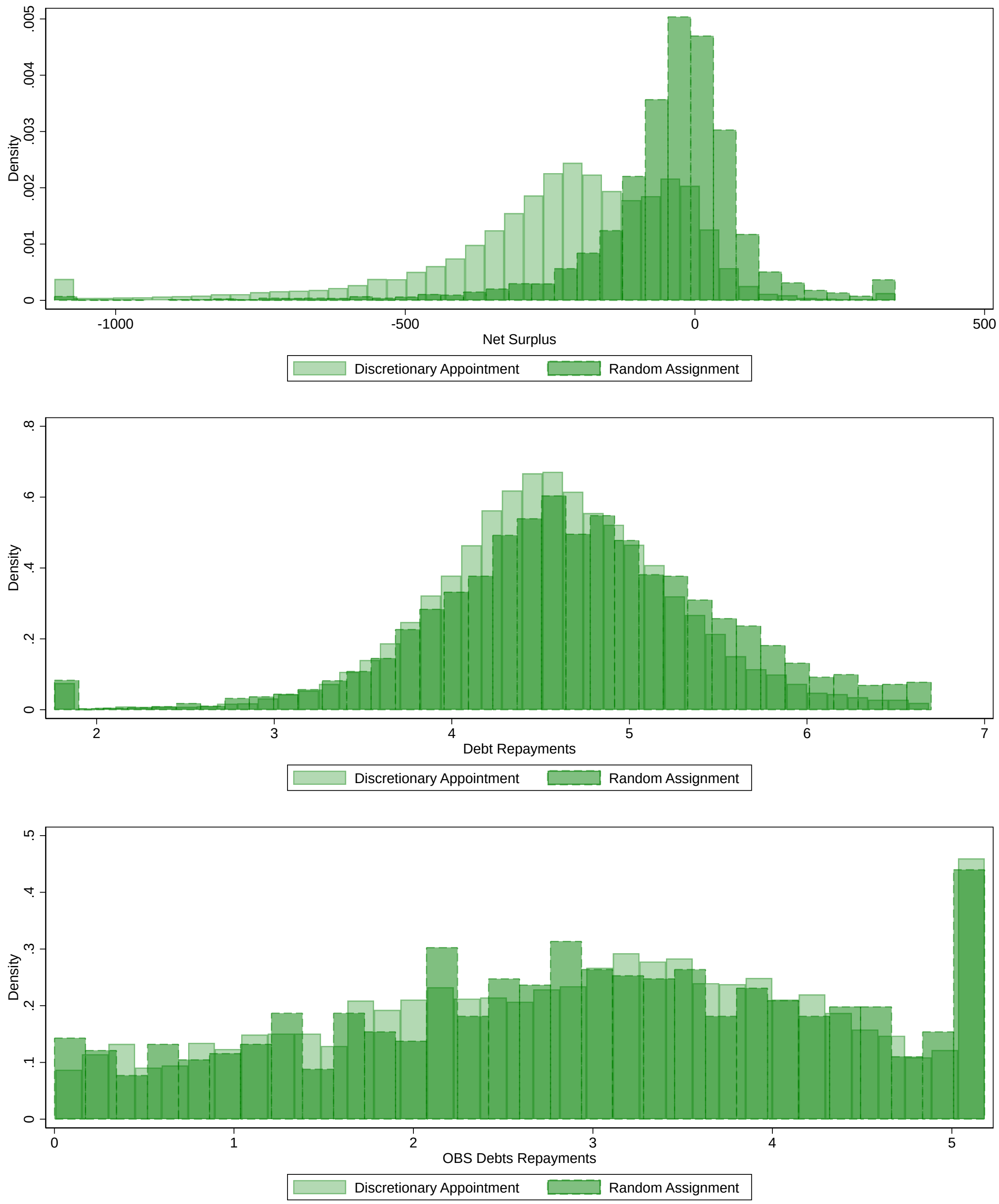
*Notes:* Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation, but their non-transformed mean is reported at 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's initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Figure A1
Staggered Treatment, Geographic Variation



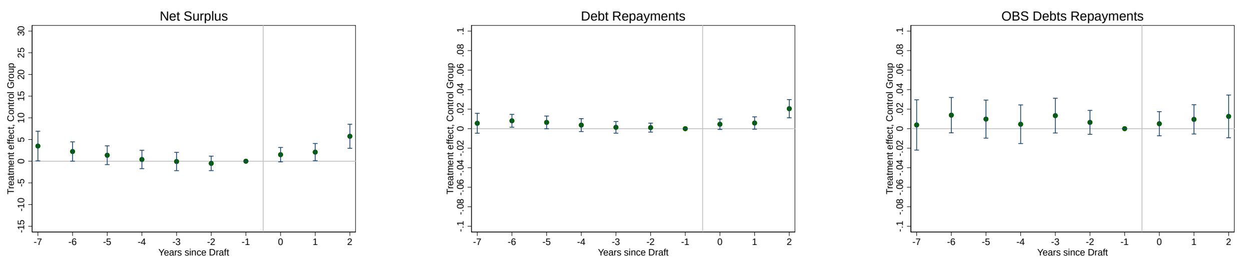

*Notes:* The figure shows the geographic variation in treatment timing. Darker gradation reflects later treatment timing.

Figure A2
Distribution of Outcomes, by Treatment Status



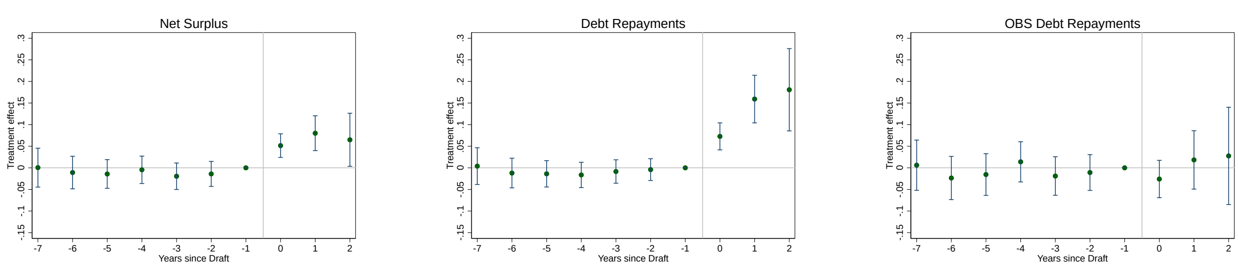

*Notes:* The figure shows histograms of the three outcomes variables, $NetSurplus$, $DebtRepayments$ and $OBSDebtRepayments$, by treatment status. All dependent variables are in per capita terms, $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation.

Figure A3
The Dynamic Effect of Auditor's Independence on Fiscal Sustainability, Anticipation Effects in the Control Group




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

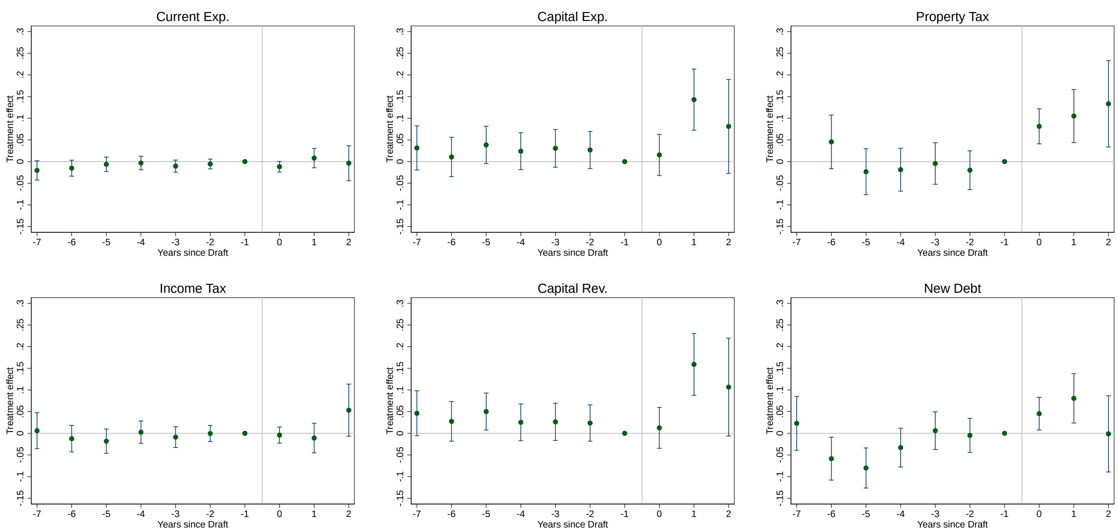
Figure A4
The Dynamic Effect of Auditor's Independence on Fiscal Sustainability, Standardized Outcomes




*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 the city council, mayor's age (in logs) gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Figure A5

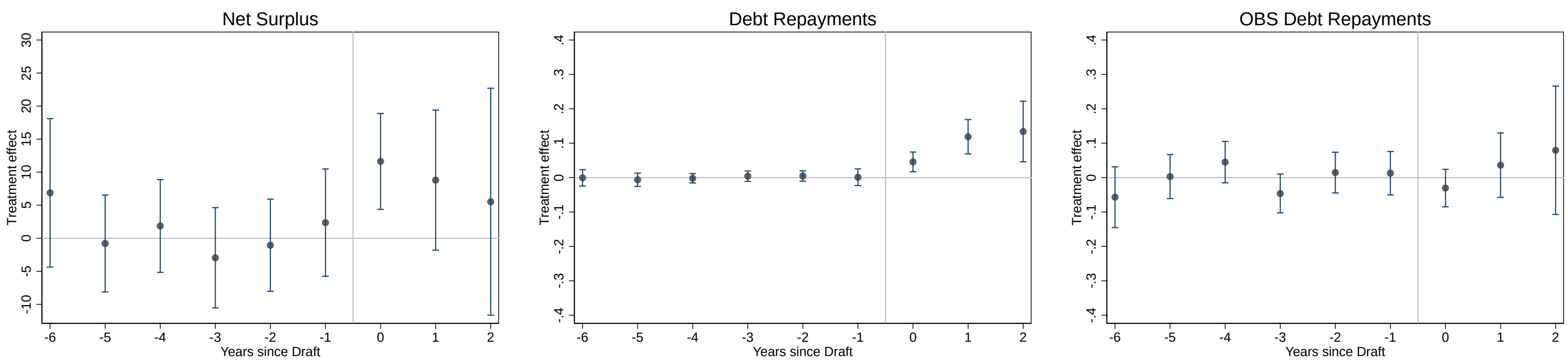
The Dynamic Effect of Auditor's Independence on Aggregate Spending and Revenue Choices, Standardized Outcomes




*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: a dummy for early termination of the city council, mayor's initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Figure A6

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

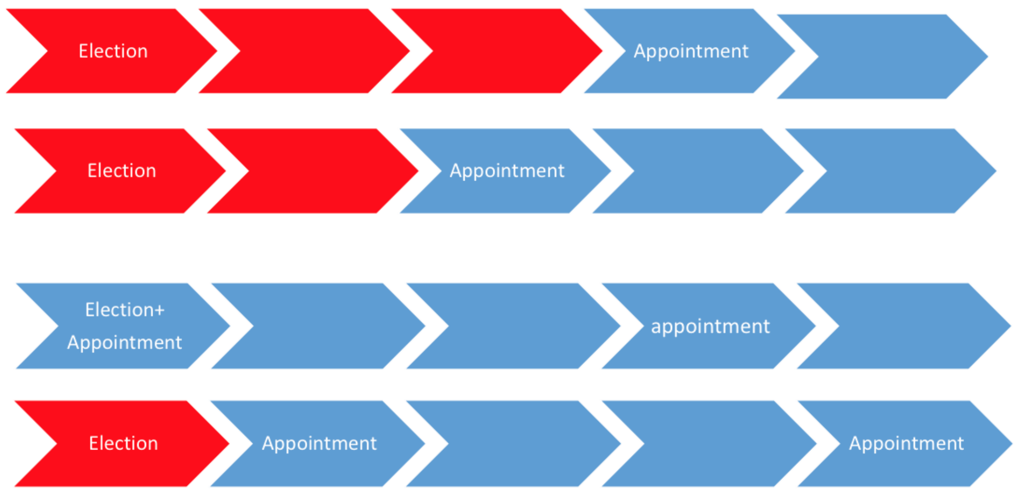



*Notes:* The graphs report coefficients and confidence intervals of the $DID_M$ estimators estimated according to de Chaisemartin and D'Haultfœuille (2020) methodology using the did_multipleGT command in Stata. Standard errors clustered at the municipality level. All dependent variables are in per capita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

Figure A7
Municipal Corruption Flags

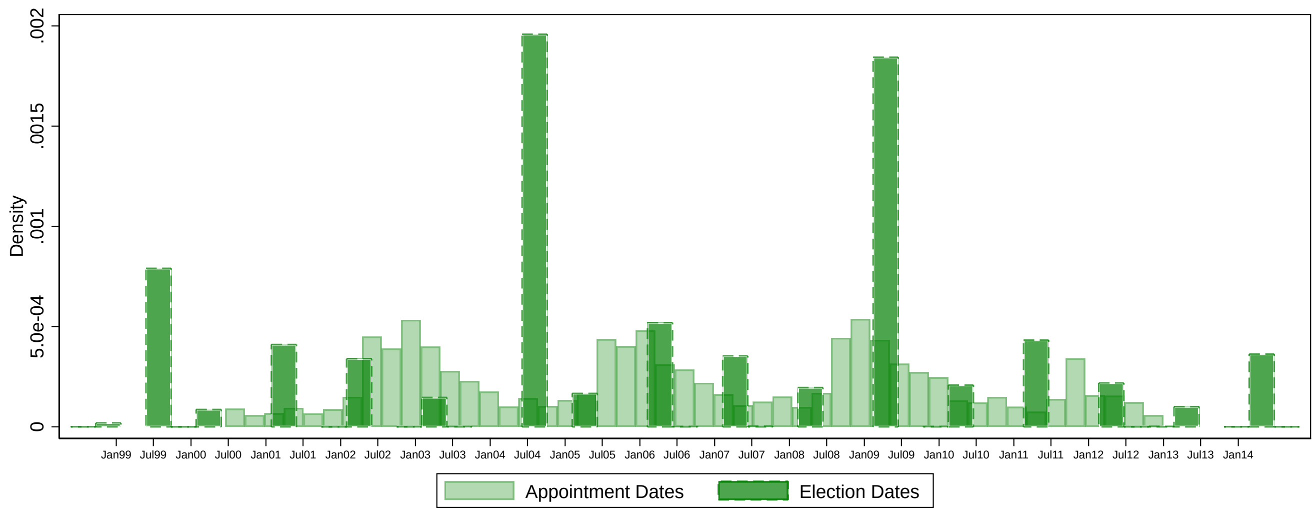

*Notes:* The figure displays a map of the municipalities having the *Corrupt* dummy equal to 1. To construct this variable, I exploit restricted-access data from the *Sistema D'Indagine Interforze* (SDI), a centralized investigation archive that contains reports of all individuals investigated by any of the Italian police forces. The final data contains information, for each municipality, on the total number of 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.

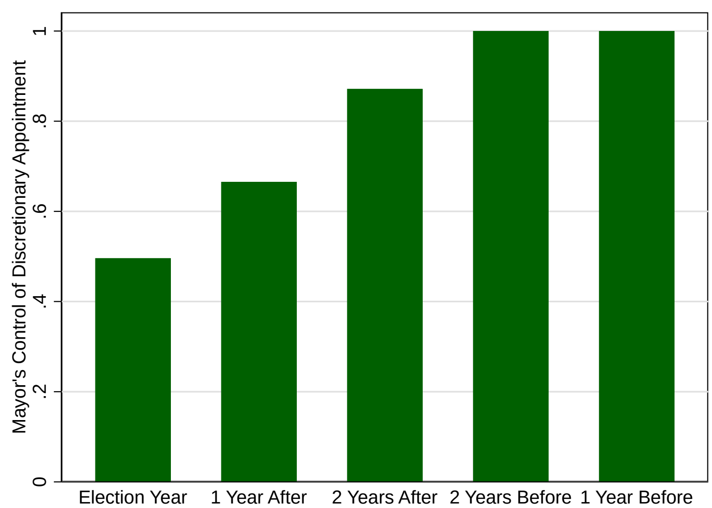
Figure A8
Auditors' Appointment Control, Example




*Notes:* The figure shows an example of how the interplay between 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 A9
Auditors' Appointment Dates and Municipalities' Election Cycles




*Notes:* The top panel of the figure displays the distribution of auditors' appointment dates in light green, as well as election dates in dark green. The bottom panel of the figure shows the share of mayors that had control of the auditor's appointment, by the moment of the election cycle. This is the variation used for identification in Table 7. Municipalities are on different electoral cycles as well as auditors' cycles for historical reasons, unrelated to the reform.

# A2 Additional Details on Data and Sample Construction

I supplement my main data with further data sources to account for municipal characteristics that might affect fiscal sustainability. First, I use detailed data on the universe of municipal elections provided by the Ministry of the Interior, including information about election dates, results, and 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' decrees. Italian municipal councils can be dismissed for several different reasons, the predominant one being mafia infiltration, but also for a 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). My analysis sample is based on the 6,627 municipalities located in Ordinary regions. To keep a balanced panel during the analysis period, I exclude all those municipalities that were dissolved or newly created between 2008 and 2015. Furthermore, I exclude 265 municipalities that were affected by earthquakes during the analysis period, as they were granted several fiscal exceptions following the catastrophic events. I also exclude 341 municipalities that voluntarily participated in the experimental phase, between 2013 and 2015, of the so-called "harmonized accounting system" that became binding from 2016 onwards. This new system significantly changed both the structure of the financial reports as well as the set of fiscal rules. The final sample consists of 5603 municipalities observed between 2007 and 2015.

# A3 Alternative Estimators

To corroborate my analysis, I apply the alternative estimator recently proposed by de Chaisemartin and D'Haultfœuille (2020), which is robust to negative-weighting issues.[49] I start by estimating the "naive" two-way fixed effects model outlined in equation 1. Results are displayed in Table A8 . Reassuringly, the estimates are all very similar to the ones in Table 3. I then estimate the weights attached to each of the average treatment effects (ATTs) to

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

compute the overall $\beta_{fe}$ estimate.[50] Results are displayed in the bottom part of Table A8 . The $\beta_{fe}$ coefficients in Table A8 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.[51] Furthermore, across all outcomes, $\underline{\sigma}_{fe}$, which reflects the minimal value under which it would be possible to have a $\beta_{fe}$ which is of the opposite sign of the true ATT, 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. I then estimate the alternative Wald estimator proposed by de Chaisemartin and D'Haultfœuille (2020), the $DIDm$ estimator, which is robust to treatment effect heterogeneity across groups and periods. In my case, the $DIDm$ is estimated only by comparing "joiners", i.e. units whose treatment status changes between periods, to untreated stable units, i.e. units who remain untreated between periods.Similarly, one can also compute $DIDm$ placebo estimators by 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 A6 . Results look very similar to the ones in Figure 2.

As an additional robustness test, in Table A9 I also repeat my analysis using the estimator developed by Callaway and Sant'Anna (2021). Their approach amounts to estimate all the different group-time average treatment effects on the treated, $ATT(g, t) = E[Y_{it}(g) - Y_{it}(\infty)|G_i = g]$, which gives the average treatment effect at time t for the cohort first treated in time g. Under standard assumptions of parallel trends and no anticipation, each of the $ATT(g, t)$ can be identified by comparing the expected change in outcome for cohort g between periods g-1 and t to that for a control group not-yet treated at time t., i.e. any group g' such that $g' > t$, or an average over some set of comparisons G such that $g' > t$ for all $g' \in G$ . Callaway and Sant'Anna (2021) consider two possibilities for G, one using only never-treated units and the second uses all not-yet-treated units. In my case, I can only use the second option, given the absence of a never-treated group in my sample. In this case, this estimator is analogous to the one proposed by Sun and Abraham (2021).

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

[51] 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 periods and reflects the minimal value under which it would be possible to have a $\beta_{fe}$ 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 $\beta_{fe}$ 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 $\beta_{fe}$, which is a weighted average of all the ATEs, of opposite sign to them.

# A4 Additional Mechanisms

## A4.1 Direct Effects: Detection and the Role of Electoral Accountability

A common justification to introduce audits is to provide citizens with valuable information that they can use to hold local politicians accountable (Ferraz and Finan 2008, Pande 2011). On the one hand, one would expect that mayors subject to strong re-election pressures should have stronger incentives to perform, restraining from inappropriate or wasteful financial practices. On the other hand, local interests may frequently clash with national ones, and thus local accountability may push mayors to act in a different direction, particularly if the push toward fiscal sustainability from the national government comes at the cost of cutting expenditures for local services or raising higher taxes for residents.

To understand the role of electoral accountability, I perform two different types of exercises. I first exploit the fact that, for historical reasons, municipalities are on different electoral cycles that do not overlap with the audit cycles.[52] 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. Panel A of Table A18 shows the results. For both $NetSurplus$ and $DebtRepayments$, the treatment effects are significantly larger for municipalities treated at the beginning of the electoral term, when the mayor had lower re-election concerns.

I then 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. The results, presented in Panel B of Table A18 , are very similar to the ones obtained in Panel A: term-limited mayors are more responsive than their re-electable counterparts. This seems to suggest that the adjustment was somewhat costly for mayors and not necessarily in line with local constituents' interests. Finally, in Panel C, I exploit heterogeneity in the strength of the mayor's majority in the municipal council. Here I find evidence consistent with an empowering role of independent auditors for the local opposition: mayors with a relatively stronger opposition are more responsive to the reform.

## A4.2 Indirect Effects: Deterrence and the Role of Spillovers

Beyond direct detection effects, a further explanation for the observed effects is the presence of deterrence effects, whereby mayors change their behavior in expectation of potentially

[52]The electoral term of Italian mayors is five years, while the term for auditors is three years.

facing a stricter auditor in the future. The key hypothesis here is that the change in auditors' appointment—or its consequences— is not fully salient to mayors before their municipality or a nearby municipality is subject to the rule change, but they become more salient afterward. To isolate the role played by deterrence effects, I exploit the staggered introduction across municipalities and the geographic variation in the exact timing of the audits.

Following Colonnelli and Prem (2021), I proceed in two steps. I first identify the spillover effects of the reform on yet-to-be-treated municipalities. I consider a municipality to be treated whenever a neighboring municipality has been treated. The control group consists of all municipalities, aside from the neighboring ones, that are yet to be treated. Table A19 reports the results. As evident from Panel A, I detect sizable spillover effects that are over 60% the size of the main effects, a magnitude similar to the one found by Colonnelli and Prem (2021) for Brazilian anti-corruption audits.

Given the quantitative importance of spillover effects and deterrence, I then re-estimate the main equation to include both the direct and indirect effects of the audits. To do so, in Panel B of Table A19 , I exclude from the control group all not-yet-treated municipalities that neighbor the one experiencing treatment in a given year. This ensures that the estimates are based on comparisons of treated municipalities relative to municipalities that were plausibly neither directly nor indirectly affected. The estimated effects of the reform are almost twice as large as the average effects of the reform in Table 3, as one would expect given the size of the spillover effects detected in Panel A.

Taken together, the results suggest that the plausible "general equilibrium" effects of the audit would be even higher than the effects estimated in Section 5.2, and they highlight the importance of considering both direct and indirect effects when assessing the effectiveness of monitoring.