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

A fundamental question in organizational economics is how to structure organizations to better align incentives across levels. While monitoring could mitigate agency problems, it can itself be rendered ineffective if auditors are corruptible. In this paper, I evaluate the consequences of changes in the design of monitoring institutions that limit auditors' conflicts of interest. I exploit the staggered introduction of a reform that removed the control of auditors' appointment from local politicians and introduced a random assignment mechanism. I obtain 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 governance.

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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 capture and abuse by local officials (Aghion and Tirole 1997, Bardhan and Mookherjee 2000, Martinez-Bravo et al. 2022).Emblematic of this moral hazard problem is the debt accumulated by lower-level governments in expectation of an eventual bailout from the central government (Rodden 2002, Halac and Yared 2022). It is thus unsurprising that one of the longest-standing debates around decentralization is how to ensure local fiscal sustainability.

Monitoring by external auditors is a common tool used to mitigate agency problems in decentralized organizations, whether corporate or governmental. Understanding how to create well-functioning monitoring institutions is thus central for decentralization. To be effective watchdogs, auditors should be independent from the audited party. However, the audited party itself is usually charged with appointing the auditor. While discretion may have its benefits - for example reducing information frictions and allowing for greater flexibility - it leads to inherent conflicts of interest and might compromise auditors' independence. Even if not deliberately corrupt, auditors are likely to be unconsciously biased toward the party that hires them (Bazerman et al. 1997). Despite the perception that discretionary appointments corrupt the audit process, reforms to sever the links between the auditor and audited party are rare, likely due to lobbying against changes from both parties involved. This helps explain why, despite the extensive theoretical research on the problems of monitoring in the presence of collusion,[1] empirical evidence is scant.

This paper fills this gap by providing empirical evidence on the consequences of changes in the design of monitoring institutions. Leveraging a unique 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' fiscal performance, aligning local behavior to the national government's objectives.

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 National Court of Auditors - a Supreme Audit Institution, 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 for regulatory compliance, who usually only

[1] See, for instance, Tirole (1986), Aghion and Tirole (1997), Kofman and Lawarrée (1993), Strausz (1997), Ortner and Chassang (2018), Mookherjee and Tsumagari (2018), Bizzotto and Chiara (2022).

engage in ex-post checks, municipal auditors in Italy serve a dual role of both monitoring and advising. During their three-year terms, auditors are required to assist and oversee the entire budgetary process of municipalities and can thus substantially influence municipal financial decisions ex-ante. However, the presence of a long-standing relationship and repeated interactions with the mayor also exposes auditors to a serious risk of capture.

As in many countries, Italian local entities cannot default on their debts, and their deficits are consolidated in the national budget. During the European Sovereign Debt Crisis of 2011, facing pressure to reduce the national debt and improve credibility on the financial markets, the national government became increasingly concerned about local governments' fiscal health. Thus, in August 2011 Italy adopted a reform that introduced a switch from a discretionary system, in which mayors could choose their auditors, to a random assignment mechanism, aiming at strengthening oversight of municipal budgets.[2] The implementation of the new appointment system took place gradually, when the current auditor's term expired, which occurred at different moments across municipalities between 2012 and 2015 for historical reasons unrelated to the reform and beyond mayors' control.[3] This gave rise to exogenous, staggered variation in the timing of the introduction of the new appointment that I exploit for identification, in a similar vein to Martinez-Bravo et al. (2017). I apply a generalized difference-in-differences (DiD) methodology and compare the outcomes of municipalities treated earlier ($Treatment$) to those treated later ($Control$), before and after the arrival of a randomly-assigned auditor.[4] 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 et al. 2019; Deshpande and Li 2019) which ensures that 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 estimators proposed by de Chaisemartin and D'Haultfœuille (2020) and Callaway and Sant'Anna (2021) and obtain very similar estimates.

I start by studying the effects on the selection and allocation of auditors. I document that the reform significantly changes auditor-mayor matching and severs connections between mu-

[2]This change was part of a larger package of austerity measures adopted to comply with the fiscal adjustment efforts required by the European Union. However, all other measures went immediately into effect, and therefore do not coincide with the implementation of the auditors' reform.

[3]Treatment timing cannot be manipulated by mayors in any way and it is not correlated with other factors that might affect local fiscal policy, such as the election cycle, which also varies across municipalities but in a way that is not overlapping with auditors term - see Appendix Figure A9.

[4]The reform affected all municipalities in Ordinary Regions but excluded those in Special Regions. However, these Regions are also granted special autonomy in setting their own fiscal rules, and therefore do not represent a viable control group and are excluded from the analysis.

nicipal 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 less than 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 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%.[5] This result suggests that draft-appointed auditors are more effective enforcers of fiscal rules. It thus emphasizes the importance of taking into account enforcement mechanisms for rules' effectiveness, a point that has been highlighted in recent theoretical work (Halac and Yared 2019, Dovis and Kirpalani 2020).

Inspecting the dynamics of the treatment effects, I find that they appear immediately upon the arrival of the (randomly assigned) auditor and remain essentially constant over the auditor's three-year term, suggesting a sudden but persistent shift in budgetary practices.[6]

Intuitively, to improve their fiscal stance, municipalities can either increase their 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 then 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 randomly-assigned auditors. In line with this explanation, I find that the effects are stronger in municipalities that have a higher share of housing units

[5] I also look at off-balance-sheet debt repayments, a rare but critical indicator of budget imbalances. While I do not detect any effect on average, there are significantly heterogeneous patterns based on the risk of pre-reform auditors' collusion, suggesting that this is an important proxy for inappropriate practices.

[6] The research design used doesn't allow me to inspect longer-run effects of the reform, for two reasons: a) all municipalities are treated by 2015 and b) a new accounting system enters in place in 2016, introducing changes in the measurement of the key outcome variables. However, policy events suggest that the randomized assignment system remained a significant political concern even seven years after its initial implementation. In December 2019, during negotiations with the national government regarding fiscal measures, large municipalities were able to partially reverse the reform by reintroducing patronage appointments for the head of the auditors' committee.

that are undeclared and where there is a higher share of buildings hidden from tax authorities, as documented in Casaburi and Troiano (2016).[7] Overall, the evidence suggests that, when facing tighter budget constraints, mayors have stronger incentives to fight property tax delinquents they might have tolerated before for political economy considerations. This result speaks to the large literature on the determinants of fiscal capacity (Besley and Persson 2009, Sánchez De La Sierra 2020, Allen et al. 2022) 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 et al. 2022).

Exploiting the richness of my design, I investigate the role played by these different margins. To explore the role of selection, I explore heterogeneity along municipal population size, as the law imposes minimal experience requirements for municipalities with more than 5,000 inhabitants. Focusing on small municipalities, I can compare the effect of receiving a new entrant as opposed to an incumbent, thus isolating the role of changes in selection and separating the causal effect of the change in the appointment from the effect of minimal experience requirements introduced by the reform, which only affect larger municipalities. I find that the treatment effects are largely driven by localities that randomly receive a new entrant as opposed to an incumbent auditor, suggesting that selection indeed plays an important role. On the other hand, I still detect sizable effects when focusing only on large municipalities that are mandated to receive an auditor drafted among the pool of incumbent auditors, thus ruling out the possibility that selection is the only driver of the results.

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 mighft allow for less responsible financial behavior, (b) an indicator of 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

[7] I also show that the effects are not simply driven by an improvement in the so-called speed of tax collection (Gagliarducci and Nannicini 2013), meaning that municipalities are not simply improving at collecting revenues that they had already assessed, but they are effectively expanding the tax base, i.e. increasing the size of the assessed revenues.

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). Third, I investigate the role played by changes in auditors' incentives in two ways. First, I exploit quasi-exogenous variation in connections to the auditor 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.

Related Literature: This paper contributes to the literature that investigates the organization of the state and the role of monitoring as a tool to solve agency problems in decentralized organizations. While several papers have documented the effects of audit adoption on corruption and accountability (Olken 2007, Ferraz and Finan 2011, Avis et al. 2018, Bobonis et al. 2016), taking audit efficacy as given, very little is known about the determinants of monitoring effectiveness. Closest to the spirit of this paper, Duflo et al. (2013) analyze the effect of randomly assigning third-party environmental auditors to 236 industrial plants in India. I expand 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. As distinct from third-party auditors, who simply annually verify firms' regulatory compliance and whose reports can be easily back-checked, municipal auditors are hired for longer terms and can influence budget decisions both ex-ante and ex-post. The wider scope of their mission also implies higher margins of discretion, introducing a stronger trade-off between bias and information.

More generally, this paper speaks to the growing literature on principal-agent problems in the public sector and their implications for state performance (Finan et al. 2017, Besley et al. 2022). I study a quasi-experimental intervention that allows me to analyze the distinct roles of selection, allocation, and incentives, as well as their interplay, in affecting key dimensions of government performance, such as fiscal capacity. Previous papers have separately documented the effects of discretion in the selection of bureaucrats (Xu 2018, Colonnelli et al. 2020), and of changes in incentives within an organization (Bandiera et al. 2021), holding selection constant. [8] Furthermore, to the best of my knowledge, this is the first paper to

[8]My experimental variation also allows me to study how changes in selection rules affect outcomes by

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, a role that is becoming increasingly common across countries.[9]

Lastly, by analyzing issues of monitoring and enforcement 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 decentralization (Bardhan and Mookherjee 2006) and the use of fiscal rules as a tool to limit moral hazard in fiscal policy (Yared 2019). While some papers have documented the effects of fiscal rules (Poterba 1996; Grembi et al. 2016), and the heterogeneity in countries' rules abidance (Eyraud et al. 2018), the determinants of fiscal rules' effectiveness have been overlooked. 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.[10] The central government also allows municipalities to undertake new debt to refinance existing debt or

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 (Besley et al. 2022).

[9]For example, in the United States, local governments are required to hire external auditors to conduct a so-called "single audit", aimed at reviewing and certifying the proper spending of federal awards (Tassin et al. 2019, Cuneo et al. 2023).

[10]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.

to refund previously emitted bonds, provided that this allows them to achieve debt service savings and that the new funds are still used to finance investment spending (*Law 311/2004*).

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, 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. Subnational government debt accounts for 11% of GDP and represents 7% of general government debt.[11] Therefore, especially during the economic crisis, the national government has frequently made use of the pact by strengthening its requirements so as to shift part of the national debt reduction required by the European Union towards local entities.[12] 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.[13] 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 to the reform, the board used to be nominated with a majority vote of the municipal council,[14] for a 3-year term, renewable for an extra term. The board cannot be dismissed for any reason

[11] 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`.

[12] 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).

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

[14] Given that a mayor always enjoys a majority in the council, she is the one in charge of the decision.

unless the council can prove auditors' faulty inaction or breach of official duties. Each of the board members can have at most eight contemporaneous appointments. 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.

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 between 1990 and the early 2000s,[15] but they were not affected by the reform. Appendix Table A1 provides a summary list of these 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. 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 in terms of both sources of revenues as well as chapters of expenditures and could ask for clarifications regarding revenue projections. Auditors are also required to advise municipalities on ways 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 auditors' opinions and suggestions, are

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

included in an audit report. This report is reviewed by the council at the time of final budget or balance sheet approval. While the auditors' opinions are not legally binding, the council is encouraged to either follow their suggestions or formally justify any inaction. After the approval, the auditor is required to fill in a detailed questionnaire and transmit all of the relevant documents, including the approved balance sheet and the audit review, to the *National Court of Auditors*. This body is a Supreme Audit Institution, it serves as the central monitor and reviews all the material sent by the auditors. It can initiate judicial proceedings against municipalities that fail to comply with the rules or engage in improper financial practices. Non-compliance with auditors' recommendations can lead to increased scrutiny from the Court. In fact, the very first question in the Court's questionnaire to auditors pertains to whether they have identified any irregularity and, if so, whether the municipality has followed the recommendations of the auditors. On average, the Court issues around 2000 judicial acts of ex-post verifications per year (Corte dei Conti 2022). The majority of these relate to recommendations and preliminary acts designed to warn municipal governments and encourage them to take corrective actions within 60 days. By contrast, the number of subsequent judicial prosecutions is relatively limited, averaging around 300 cases per year.

2.3 The Auditors' Appointment Reform

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*). 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, 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 the possibility of setting their own fiscal rules and 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 prefectures, the local offices of the Ministry of the Interior, via a standardized, computer-based system. Municipalities are required by law to inform their local prefecture at least 2 months before the current auditor's term expires. 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.[17] 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. 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 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.[18]

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" (*Bilanci Consuntivi*), reporting effective revenues and expenses for the previous year, which need to

[16] In 2016, Friuli Venezia Giulia, Sardinia, and Sicily adopted laws introducing similar drafting procedures that generally preserve some discretion for local governments. In Friuli, municipalities can choose among the $3 \times N$ drafted candidates. In Sicily, the drafting procedure was adopted upon the pressure of the Regional Council of Auditors, which repeatedly denounced political pressures toward the local auditors. In contrast to the national system, however, the drafting is carried out directly by the City Council, which exposes it to a higher risk of illicit acts.

[17] Causes of ineligibility include: a) being a member (or have been a member in the past 2 years) of the municipal council or executive body; b) being a local bureaucrat; c) having already reached the maximum number of concurrent appointments (8).

[18] The data shows that rejections are a relatively rare occurrence: around 20% of first-drafted auditors in total reject the appointment, and in most 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.

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 randomly-assigned auditors provide more independent monitoring and thus improve the fiscal sustainability of municipalities, as per the national government's objectives. Therefore, I focus on a set of indicators that are used by the national government and/or by the National Court of Auditors 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. Municipalities are required by the national government to use any surplus towards the repayment of existing debt.[19] 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 previously authorized and accounted for in the provisional budget. [20] 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

[19] Particularly from 2010 onwards, the national government pushed municipalities to quickly repay their outstanding debts undertaken in the past, as they were subject to significantly higher interest rates.

[20] These include, for example, unexpected expenses incurred for emergency interventions or other expenses motivated by local public necessities, but also expenses arising from recapitalizing SOEs.

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. All spending variables, which are always greater than 0, are then 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, published yearly by the Ministry of the Interior. 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 partially overcome the fact that I don't have information about auditors' appointments for the pre-reform period. By exploiting the information reported by auditors when they sign up for the lists, I am able to recover the identity of the last auditor appointed before the reform for over 40% percent of my sample.[21] I supplement the above information with further data to account for municipal characteristics that might affect fiscal sustainability. For a detailed 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. From 2016 onwards, municipalities are mandated to adopt the so-called "harmonized accounting system". This new system significantly changed both the structure of the financial reports as well as the calculation of the deficit targets for fiscal rules and therefore makes it unfeasible to extend the analysis beyond 2015.

4 Empirical Strategy

My identification strategy leverages the staggered introduction of the auditors' appointment reform across municipalities. As explained in Section 2, while the reform became effective in December 2012, the treatment date varies across municipalities depending on the expiration date of the current auditor's term. For example, municipalities that appointed auditors in

[21] 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.

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 across municipalities in the auditor's appointment date before the reform due to historical reasons, which are uncorrelated with other municipal characteristics. Thus, treatment timing is plausibly exogenous.[22] My design, therefore, compares the evolution over time of outcomes of 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 randomly-assigned auditors over time. Figure A1 displays their geographic distribution, highlighting the lack of any geographic patterns by treatment timing.

Given my design, the most standard specification would be the following two-way-fixed-effects (TWFE) model:

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

where Y_{mt} represents an outcome for municipality m at time t, α_m are municipality fixed effects, $Treated_{mt}$ is an indicator variable that is equal to 1 if a randomly-assigned auditor is active in municipality m in year t, and X_{mt} is a matrix of time-varying controls. However, naively applying this specification could potentially pose a set of empirical challenges that have been recently highlighted by a growing literature on the pitfalls of two-way fixed effects estimators with staggered adoption (Goodman-Bacon 2021, de Chaisemartin and D'Haultfœuille 2020, Borusyak et al. 2021).[23] 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).[24] 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).

[22]A concern would arise if mayors could differentially select into treatment, for example by manipulating the auditors' appointment date. 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.

[23]The main concern raised by the literature is that the coefficient β from Equation 1 is a weighted average of all the possible 2x2 comparisons in my sample. Therefore, it is also estimated using comparisons among already-treated units and not-yet-treated units, where the already-treated units serve as controls. This induces a bias in the presence of heterogeneous treatment effects across groups experiencing treatment at different points in time.

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

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.[25] 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 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;[26] $Post_{mt}$ is a dummy equal to 1 for the years in which a randomly-assigned auditor is active, while the D^k is a set of relative event-time dummies, that take the value of 1 if year t is k periods after (or before, if k is negative) the treatment. The inclusion of these event-time dummies allows me to control for event-time trends that are not captured by the calendar year fixed effects. I also estimate separate calendar year effects δ_{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, and a set of pre-determined characteristics of the mayor in power at the time of auditors' appointment, such as a dummy for whether the mayor is term-limited, a dummy for whether the mayor was born in the municipality, as well as the gender and the age of the mayor at the time of election (measured in logs). Standard errors are clustered at the municipality level (Bertrand et al. 2004), accounting for the possibility of serial correlation over time and 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:

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

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

[26] 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.

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

The key identifying assumption of my design is the absence of differential trends between municipalities experiencing treatment at different points in time; that is, absent the change in auditor's appointment, the outcomes of municipalities belonging to different treatment cohorts would have evolved similarly over time.

My empirical design already allows me to rule out various concerns that could otherwise impair my ability to interpret the results as causal. First, the inclusion of municipality fixed-effects rules out that the results are driven by time-invariant differences in outcomes across municipalities. Second, the inclusion of calendar time effects allows me to exclude that results are driven by factors that affect the evolution of outcomes over time in a way that is common across municipalities, such as changes in macroeconomic conditions.

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 evidence in favor of this assumption, in Table A3 I look at whether municipalities treated at different times display any significant differences in their observable characteristics (Panel A) and in the levels of outcomes (Panel B and C) in the pre-reform period. The first four columns of the table show the average levels of each variable, by cohort of treatment. I then perform a regression of each variable on cohort dummies and in column 5 I display the p-value of a joint F-test for the null hypothesis that all are jointly equal to zero. I fail to reject that all dummies are jointly equal to zero for all variables, except for population.[27]

It is important to note that 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. To account for this imbalance, in all of my analyses, I control non-parametrically for the presence of differential trends by population size, by including population-by-year fixed effects.[28] These controls also allow me to account for

[27]This is further confirmed in Table A4, where I directly look at predictors of treatment timing. Here, I perform two different F-tests, whose p-values are reported at the bottom of the Table. When excluding population dummies from the test, across columns, I fail to reject the null hypothesis that all coefficients are jointly equal to zero.

[28]I construct a set of dummies for the following population bins (reflecting the ones used by the Italian

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.

Panel B and C of Table A3 repeat the same exercise as Panel A, but look at the levels of outcomes in 2010. Here, I add an additional column, where I report the p-value of the joint F-test of the joint significance of cohort dummies in a regression that also includes the observable characteristics of Panel A. While some outcomes display small significant unconditional differences, after conditioning on the set of covariates, 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.[29] 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 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 overspending 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

Statistical Office to classify municipalities along population size): 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 δ_{pt}.

[29]Kahn-Lang and Lang (2020), Roth (2020), and Rambachan and Roth (2020) have cautioned against the use of pre-event trends testing as these tests are frequently underpowered and can lead to type-2 errors. I have conducted the sensitivity tests as in Rambachan and Roth (2020), and my results are robust to allow for large degrees of non-linearity in the violation of parallel trends.

anticipation effects potentially at play. As can be seen in Equation 3, the design allows me to separately control for both calendar-time fixed effects (the δ_{pt}) 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.[30] By looking at the β_k coefficients, one can thus inspect if, and in which direction, the control units change their behavior before actually becoming treated.[31] 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 γ_k, which represent the event-studies for the treated group. In this sense, my estimates could be considered conservative lower-bound estimates of the true effects in the presence of anticipatory effects.

Finally, a source of concern might be the presence of other time-varying shocks that occur in the same period, in a way that is correlated with the staggered introduction of the reform across municipalities. The most significant change that occurred during this time period is the extension of the rules of the Domestic Stability Pact to municipalities with a population below 5000 inhabitants in 2014. This change is accounted for by the inclusion of population-size-by-year fixed effects. In Section 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.[32] The most important one was 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 into 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.

[30]In particular, they are estimated thanks to the fact that the same municipality appears multiple times in the dataset. As an alternative, I also estimate a specification that includes fully saturated cohort-specific municipality fixed effects and obtain nearly identical results. See Table A12.

[31]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.

[32]The reform also introduces: a) a tightening of fiscal rules and cuts of national government transfers for the year 2012, b) 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) a reduction in the number of members of the municipal council e) collection of property tax on owner-occupied units.

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 local fiscal policy, I inspect how the reform affects the selection and allocation of auditors across municipalities. Table 2 compares the characteristics of the auditors and auditor-municipality match under discretionary appointments (pre-reform) as opposed to the ones resulting from random assignment.[33] 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.[34] 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, and 60% are 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, it becomes an extremely rare occurrence (1%), as it can only happen as a result of the same auditor being drafted again for the same municipality. The large change in re-appointment rates highlights how the reform might have also changed auditors' incentives. Looking at the characteristics of the auditor-municipality match, 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

[33] In Appendix Table A5, I provide evidence that the assignment was indeed random under the new system. To do so, I compare the average characteristics of the drafted individuals to the ones of the relevant pool of auditors, and I fail to reject that the two are statistically the same.

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

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.[35]

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.[36] 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 regression results for the effect of the introduction of randomly-assigned auditing on indicators of municipal fiscal sustainability. Odd columns present results

[35] 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 policy for municipalities above 5,000 inhabitants, that could only be assigned auditors with previous experience.

[36] As shown in Appendix Table **??**, municipalities belonging to different cohorts of treatment display similar pre-treatment levels of outcomes, so this histogram is not confounded by potential level differences across early vs. later treated municipalities.

[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.

from Equation 4 without controls, while even columns include controls. Results indeed provide evidence that the random assignment of auditors contributes significantly to improving the fiscal stance of municipal finances. In columns (1) and (2), I investigate the effect on $NetSurplus$. Upon the arrival of a randomly-assigned 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 me to inspect the presence of differential pre-trends, as well as the dynamics of the treatment effect along the auditor's three-year 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 $DebtRepayments$, treatment effects appear from the very first year of the arrival of a randomly-assigned 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 $OBSDebtRepayments$.

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. 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. 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. 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 in more detail at the different types of investment spending, 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 A8).

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 %.[39] Given that the observed increase in total revenues to be used for investment financing ($NewDebt$ and $CapitalRevenues$) is higher than the increase in capital expenditures, it is likely that municipalities are using part of the $NewDebt$ to re-finance pre-existing debt, thus partially explaining the observed increase in $DebtRepayments$. As explained in Section 2, according to Italian law, municipalities can undertake new debt to finance new investments or to re-finance pre-existing debt, provided that they achieve substantial debt service savings. $NewDebt$ is substantially more convenient than the old one, as most of the pre-existing debt was undertaken in the 1990s and 2000s, periods of relatively high interest rates, while rates in the post-reform period (2012-2015) were close to zero, mostly as a consequence of the ECB interest rate policies at the time.

[38]The distinction between revenues from owner-occupied and other properties is not available for 2007, therefore the analysis for this variable is restricted to 2008-2014.

[39]This result should be taken with some caution as $NewDebt$ is the only variable for which I detect a significant negative pre-trend for the relative event coefficients -5 and -6, thus it could be that the magnitude of the effect is slightly over-estimated.

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. Overall, results show that, upon the arrival of a randomly-assigned 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 et al. 2019).

A natural question to ask is how municipalities increase their tax revenues upon the arrival of randomly-assigned auditors. 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. While my data are not perfectly suited to fully address this question, I provide some suggestive evidence on the specific margins in Table A7. Column (1) simply reports the results of column (3) of Table 4. 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), I test explicitly for whether there is any change in the speed of tax collection Gagliarducci and Nannicini (2013), a measure that is constructed as the ratio between collected and assessed property tax revenue, and I fail to detect any significant improvement. Finally, in Column (10), I look at the (log of) property tax rate, and 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), and similarly in columns (4) and (6), I show that the effects on both the assessed and collected revenues are significantly 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).[40] Overall, this pattern

[40] 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

seems to support the hypothesis that mayors are expanding tax revenues by combating past tax evaders. In line with this theory, in columns (8) and (9), I find a negative heterogeneous effect on the speed of tax collection, suggesting that, despite the effort to fight evasion by taxing previously un-assessed properties, it is slightly more difficult to effectively collect taxes from former tax evaders.

The fact that the revenue increase seems to come from a sizeable improvement in tax collection from potential tax evaders should not sound surprising, as in Italy, mayors have complete discretion over tax collection procedures. For example, it is entirely up to the municipal police to verify whether a property is owner-occupied (and thus exempted from paying the property tax by law), or not. According to the Italian Revenue Agency, the false declaration of residence in a different property to benefit from the owner-occupied exemption is one of the most common forms of property tax evasion and is estimated to affect almost 150,000 properties.

Overall, the evidence suggests that, when facing tighter budget constraints, mayors have stronger incentives to fight property tax delinquents, which they might have tolerated before for political economy considerations. This result speaks to the literature on the determinants of fiscal capacity and is consistent with the findings of Xu (2018), who shows that patronage-appointed governors raised fewer taxes and invested less in the revenue capacity of British colonies.

Taking stock of the results presented so far, I argue that the arrival of draft-assigned auditors indeed improved monitoring and spurred more fiscal stability, 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 A9 and A10 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 A11, I re-estimate the main model but include flexible trends in the log of population and municipal

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".

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 A12, I estimate a version of Equation 4 fully saturated with cohort-specific municipality fixed effects, and obtain nearly identical estimates. In Table A13, 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 A14 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, all of those 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 A15). 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]

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

[41] In a concurrent working paper, Barone et al. (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 municipalities in special regions), the definition of treatment (which in their case depends on the number of months an auditor has been in place), and the empirical design (a potentially biased two-way fixed effects model (Goodman-Bacon 2021)). In Appendix Table A16 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.

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). Section 6.4 explores 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 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 randomly-assigned auditors as controls. Panel A of Table 5 displays the results. Note that by virtue of random assignment, these observables are 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

[42] All heterogeneity analyses are performed using the same specification of the main results, the one of Equation 4, augmented with a triple interaction term for the relevant heterogeneity margin, as well as interactions between the $Treated$ and $Post$ indicators with the relevant heterogeneity covariate. This ensures that heterogeneous effects are not driven by differential trends along the specific heterogeneity margin. As a robustness check, I also repeat all the analyses using the standard two-way fixed effects estimator of 1 in Table A19 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.

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 endogenous matches to random ones. This change has two potential effects. On the one hand, random matching reduces the risk of auditors' capture. On the other hand, it might come at the cost of sacrificing the potential value of discretionary appointments and inducing misallocation. Mayors are likely to be better informed about the characteristics of auditors that 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; 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 investiga-

[44] The heterogeneity also shows that the effects on debt repayments are stronger in larger municipalities. This should not be surprising as larger municipalities are the ones more likely to have large debts. Consistently, I also detect a significant positive treatment effect on $OBSDebtRepayments$ for large municipalities.

tions. Overall, one can think of these types of places as being more likely to switch from endogenous, captured matches to non-connected, less-captured ones as a result of the reform.

As for the first proxy, if randomly-assigned 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. In Panel A of Table 6, I repeat the analysis of Table 3 but include an interaction term 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.

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 a randomly-assigned auditor reduces the occurrence of the illicit spending phenomena, thus leading to a decrease in $OBSDebtRepayments$. Whereas in "dis-honest" municipalities, the reform induces the reporting of illicit spending, thus leading to an increase in $OBSDebtRepayments$.

While I cannot directly measure the risk of auditors' capture in the pre-reform period, I collect information on potential proxies or factors that are expected to increase the probability of capture. 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 signaling 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 debts and upon the arrival of a randomly-assigned auditor were more likely to restate them.

Furthermore, 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 the inspection and detection of corrupt practices is not the auditors' main task, corruption cases usually involve illegal transactions between private parties and members of the local bureaucracy and are thus a proxy of an environment where illegal practices occur, that might or might not involve directly local politicians. Furthermore, corruption-related crimes are frequently linked to 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

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

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

not uncover illicit practices.

Table 6, Panel C displays the results. The patterns 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 a randomly-assigned auditor, the amount of $OBSDebtRepayments$ decreases by 5%. In contrast, in corrupt municipalities, I observe an increase for $OBSDebtRepayments$ of 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 off-balance sheet 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.

Lastly, I exploit likely exogenous variation in the strength of ties between auditor and mayor arising from the random nature of the post-reform municipality-auditor match. To proxy for the strength of auditors' social proximity, 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 presented before 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. On the one hand, one could 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. On the other hand, it could be that a local auditor has more valuable information to leverage that can improve its monitoring quality, and thus the random assignment of a distant auditor makes monitoring less effective.

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 A17, 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.

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. The possibility of re-appointment likely gave strong incentives to the auditors to please the mayor who appointed them during their first term, and it also provided the mayor with 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 the variation in connections between the auditor and the mayor in the pre-reform period. 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 lee-way vis-a-vis fiscal rules and spending, we should observe a worsening of fiscal sustainability during the years in which auditors work for mayors that

[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. I am comparing two municipalities that made the same endogenous selection in the pre-reform period but were randomly assigned either a closer or more distant auditor.

appointed them. 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. Importantly, the probability of an auditor-mayor connection 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 political budget cycles (Rogoff 1987). For this reason, I restrict attention to mayors in their first term and I always include a dummy equal to 1 for the two pre-election years, so that the β coefficient 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 report the results from Equation 4. In the even columns, I also include auditor fixed-effects. These estimates are based on within-auditor variation, so they net out any unobserved fixed auditor-specific confounders and hold constant any effect operating on the selection margin. 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 were 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. The bottom Panel of 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]

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 investigate the presence of a detection effect, I look at 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 re-election pressures. Results are reported in Appendix Table A20 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 effect 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 A21 and discussed more in detail in Appendix Section A4.2.

Overall, the sizeable presence of spillovers suggests a potentially broader role of the reform in changing compliance norms across municipalities.

7 Discussion and Conclusion

While considered a crucial tool for good governance, effective monitoring is frequently impaired by conflicts of interest. 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, I investigate the causal effects of removing appointment control from mayors and introducing a random assignment of auditors. Three main findings arise. First, treated munic-

[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 A18 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.

ipalities 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 take-away 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. For example, a similar system is in place in the United States, where local governments are required to hire external auditors to conduct a so-called "single audit", aimed at reviewing and certifying the proper spending of federal awards (Tassin et al. 2019, Cuneo et al. 2023).

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 et al. 2006). Furthermore, even though the reform was not formally accompanied by a change in the ex-post audit practices of the central monitor (the National Court of Auditors), 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 having some form of third-party measures to use as a comparison (e.g., auditors' reports to the National Court of Auditors), a topic that I aim to address in future research.

While providing a well-identified causal evaluation of the effects of the introduction of random auditor assignment in the short run, this paper is silent about the long-run effects of the reform. Although it is not feasible to empirically assess these effects given the research design, examining policy events can provide valuable insights. For example, in December 2019, during negotiations with the national government regarding fiscal measures, major municipalities were able to partially reverse the reform by reintroducing patronage appointments for the head of the auditors' committee. This suggests that the randomized assignment system remained a significant political concern even seven years after its initial implementation.

Finally, this paper is silent about the overall welfare consequences of the reform. The analysis takes the perspective of the central government and documents how changes in the institutional design of monitoring institutions can make them more effective and align the behavior of local governments with national objectives. The discussion of whether the national policies themselves - which in this case entail fiscal austerity - are socially optimal is beyond the scope of the paper. On the other hand, results show that municipalities achieve better fiscal health not by cutting local spending - if anything, they even increase local investments - but by increasing revenues, and most of the tax revenue increase comes from a reduction in tax evasion, thus suggesting limited negative local welfare consequences.

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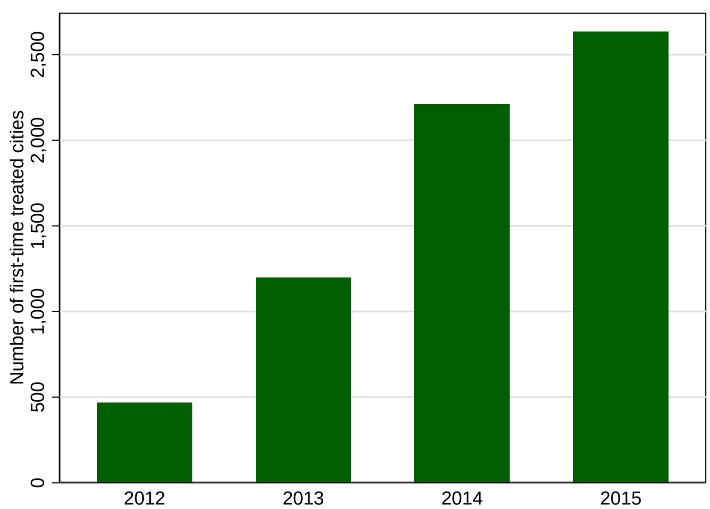
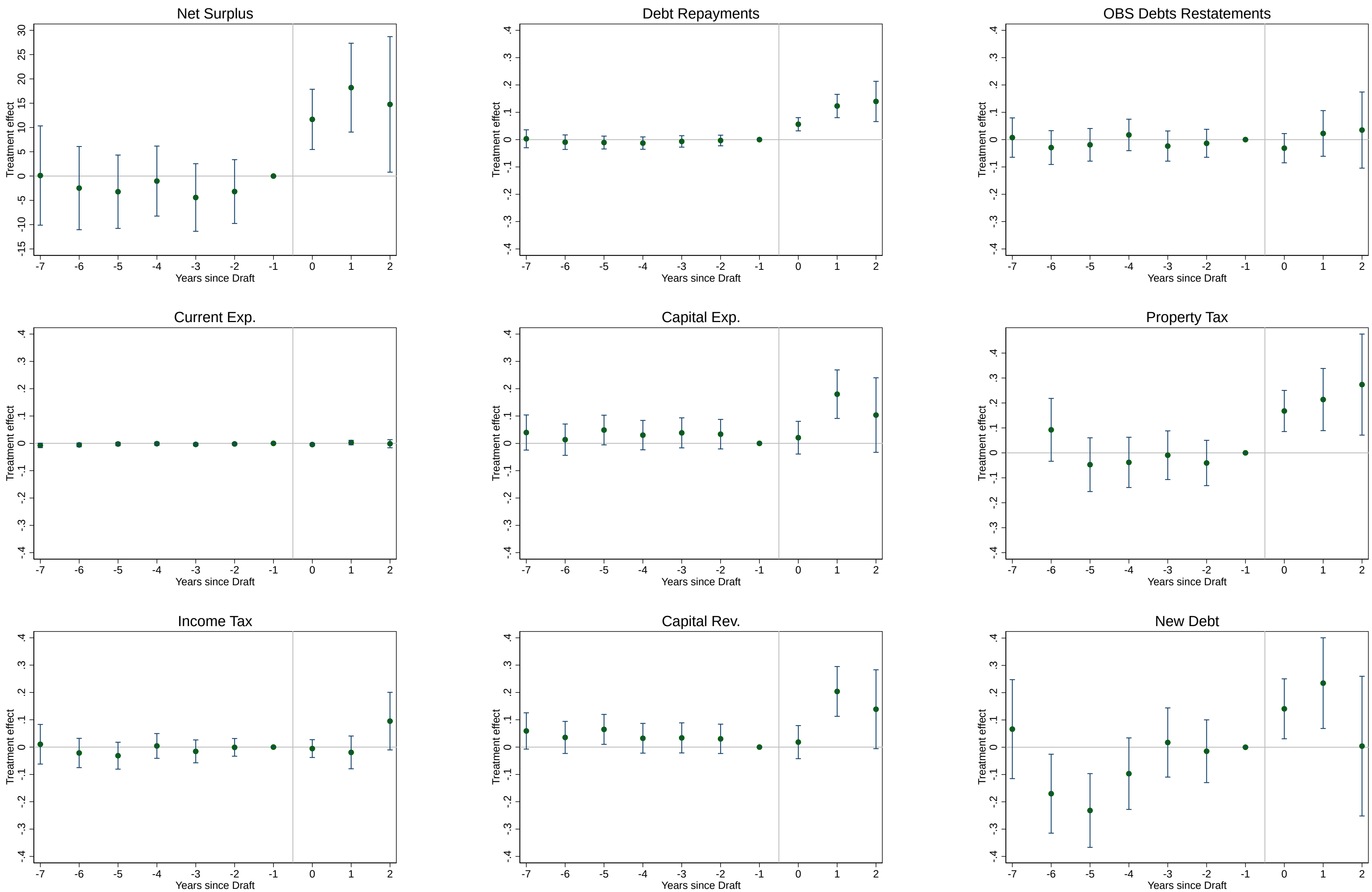
Tables and Figures

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



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

Figure 2
The Dynamic Effect of Auditor's Independence on Fiscal Sustainability and Aggregate Spending and Revenue Choices



Notes: The graphs report coefficients and 95% confidence intervals estimated according to Equation 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: 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.

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

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

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

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

Table 2
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) (resp. (4)) shows the difference (standardized 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.

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

	Surplus		Net Surplus		Debt Repayments	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated=1	16.45***	16.02***	0.0885***	0.0869***	-0.00218	-0.00222
× Post=1	[3.240]	[3.236]	[0.0140]	[0.0139]	[0.0253]	[0.0253]
Controls	No	Yes	No	Yes	No	Yes
Dep. Var Mean	-194.8	-194.8	65.29	65.29	3.255	3.255
Observations	114028	114028	114028	114028	114028	114028
Adj. R-sq	0.699	0.700	0.757	0.757	0.412	0.413

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

Table 4
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: mayor's initial age (in log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

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

	PANEL A:	Randomly-assigned Auditors' characteristics as Controls	
	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated × Post	8.777** [3.419]	0.0373*** [0.0129]	0.0186 [0.0256]
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

	PANEL B:	By Population Size	
	(1)	(2)	(3)
Treated × Post	16.62*** [4.383]	0.0331** [0.0140]	-0.0131 [0.0269]
Treated × Post × Pop. 5-15 k.	-2.501 [5.919]	0.158*** [0.0400]	-0.0409 [0.0681]
Treated × Post × Above 15k	-2.545 [7.997]	0.205*** [0.0558]	0.169* [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 × Post	2.311 [6.719]	-0.00252 [0.0208]	-0.0174 [0.0382]
Treated × Post × New Entrant	21.29*** [6.974]	0.0439** [0.0217]	0.00218 [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, and the following controls: 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 A, controls also include the following randomly-assigned auditors' characteristics: gender, age, years of experience as an accountant and as an auditor, and auditors' distance from the municipality. In Panel C, *NewEntrant* is an indicator equal to 1 if the municipality is assigned as an auditor an individual who 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. All specifications in Panel B and C also include interactions between the *Treated* and *Post* indicators with the relevant heterogeneity covariate.

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. All regressions include municipality, relative event-time fixed effects, election cycle fixed effects, population bins-times-year fixed effects, and the following controls: mayor initial age (in log), gender, term in office and a dummy equal to one if the mayor was born in the municipality, as well as interactions between the $Treated$ and $Post$ indicators with the relevant heterogeneity covariate.

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)
Control of Appointment=1	-7.347**	-10.13*	-0.0180*	-0.0302*	0.0246	0.0547
	[3.197]	[5.872]	[0.0100]	[0.0168]	[0.0291]	[0.0465]
Auditor FE	No	Yes	No	Yes	No	Yes
Dep. Var Mean	-208.2	-183.0	64.45	65.77	3.678	3.932
Observations	24442	17172	24442	17172	24442	17172
R-sq	0.760	0.790	0.812	0.855	0.514	0.616

	PANEL B:		by Term of Pre-Reform Auditor			
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	9.299**	10.27**	0.0952***	0.132***	-0.0201	-0.0543
	[3.715]	[4.825]	[0.0181]	[0.0268]	[0.0333]	[0.0462]
Treated × Post× Second		-1.957		-0.0744**		0.0686
		[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: mayor's initial age (in log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Appendix For Online Publication

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 et al. (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
Pre-treatment Municipal Characteristics and Levels of Outcomes, by Treatment Timing

A. Municipal Characteristics						
	2012 Cohort	2013 Cohort	2014 Cohort	2015 Cohort	Uncond F-test	
Election Cycle	1.85	1.83	1.85	1.84	0.994	
Population	23700.84	8605.04	4040.70	6359.44	0.000	
Mayor Age (log)	3.87	3.87	3.86	3.85	0.064	
Male Mayor	0.90	0.91	0.88	0.88	0.849	
Local Mayor	0.41	0.40	0.34	0.34	0.425	
Mayor Term-limited	0.36	0.39	0.42	0.40	0.552	
B. 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	0.196
Debt Repayments	60.60	61.92	64.54	62.91	0.413	0.962
OBS Debts Repayments	5.85	5.16	2.70	3.36	0.000	0.775
C. Revenue and Spending Choices						
	2012 Cohort	2013 Cohort	2014 Cohort	2015 Cohort	Uncond. F-test	Cond. F-test
Current Expenditures	895.74	885.98	907.66	871.69	0.044	0.345
Capital Expenditures	505.93	544.56	543.26	507.68	0.367	0.780
Property Tax	126.03	119.58	144.18	144.77	0.000	0.417
Income Tax	40.05	36.82	40.61	41.11	0.001	0.207
Capital Rev.	458.40	471.34	460.69	427.97	0.295	0.780
New Debt	122.87	150.55	131.16	126.59	0.115	0.737

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 region fixed effects and 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 pre-treatment characteristics of panel A as controls.

Table A4
Municipal Characteristics that Predict Treatment Timing.

	2012 Cohort	2013 Cohort	2014 Cohort	2015 Cohort
	(1)	(2)	(3)	(4)
Election Cycle	0.00628**	0.000807	-0.00827	0.00118
	[0.00311]	[0.00448]	[0.00522]	[0.00556]
1-5k pop.	-0.00791	-0.0149	0.0137	0.00909
	[0.00779]	[0.0127]	[0.0162]	[0.0163]
5-10k pop.	0.0174	-0.0160	-0.125***	0.124***
	[0.0111]	[0.0156]	[0.0187]	[0.0206]
10-15k pop.	0.0440***	0.00723	-0.161***	0.109***
	[0.0154]	[0.0198]	[0.0222]	[0.0255]
15-20k pop.	0.115***	0.0791***	-0.195***	0.000487
	[0.0249]	[0.0287]	[0.0261]	[0.0322]
20-60k pop.	0.183***	0.114*	-0.187***	-0.110*
	[0.0589]	[0.0621]	[0.0516]	[0.0620]
Above 250k pop.	0.366*	-0.0267	-0.379***	0.0395
	[0.188]	[0.115]	[0.0287]	[0.163]
Mayor Age (log)	0.00537	0.0317	0.0196	-0.0567*
	[0.0158]	[0.0257]	[0.0312]	[0.0331]
Male Mayor	-0.00562	0.0105	-0.0170	0.0122
	[0.0107]	[0.0153]	[0.0207]	[0.0210]
Local Mayor	0.00316	0.00739	-0.00289	-0.00766
	[0.00839]	[0.0122]	[0.0146]	[0.0154]
Mayor Term-limited	-0.00966	-0.00564	0.0286**	-0.0133
	[0.00686]	[0.0105]	[0.0130]	[0.0135]
Observations	5603	5603	5603	5603
R-sq	0.0292	0.0354	0.0568	0.0327
P-value Joint F-test, w/o pop.	0.25	0.58	0.15	0.43
P-value Joint F-test, w. pop.	0.00	0.04	0.00	0.00

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. The specification also includes region fixed-effects. Robust standard errors are reported in square brackets.

Table A5
Testing for Effective Randomization

	Drafted	Pool	β	Standardized Difference
Female	0.26	0.28	-.01	-0.028
Resident in Municipality	0.008	0.004	0	0.053
Born in Municipality	0.003	0.001	0	0.049
Experience as Accountant	16.89	15.43	-.09	0.123
Age	48.56	47.17	-.07	0.105

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Column (3) displays the coefficient of a separate regression of each of the characteristics on a dummy equal to 1 if the individual has been drafted and $RegionXYearXPopulationSize$ fixed effects. These fixed effects account for the level at which the drafting is performed. Column (4) displays the standardized difference between Column (1) and Column (2), which is equal to the difference in means divided by the square-root of the sum of the standard deviations. *Local* is a dummy equal to 1 if the auditor's municipality of residence coincides with the municipality of appointment, *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.

Table A6
Changes in Auditors' Characteristics, Regression Analysis

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

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

Table A7
The Effect of Auditor's Independence on Tax Capacity

	Property Tax Revenue, Assessed			Property Tax Revenue, Collected			Speed of Property Tax Collection			Property Tax Rate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated × Post	0.201***	0.186***	0.183***	0.183***	0.171***	0.170***	0.406	0.451	0.423	-0.00320	-0.00243	0.00177
	[0.0436]	[0.0432]	[0.0430]	[0.0423]	[0.0420]	[0.0417]	[0.413]	[0.411]	[0.411]	[0.0222]	[0.0222]	[0.0221]
TreatXPostXGBI		0.126***			0.111***			-0.645*			-0.00680	
		[0.0351]			[0.0339]			[0.336]			[0.0161]	
TreatXPostXUndeclared			0.164***			0.151***			-0.679*			-0.0452***
			[0.0337]			[0.0329]			[0.406]			[0.0173]
Dep. Var Mean	158.9			4.584			80.17			7.218		
Het. Var Mean		0.0268	0.0182		0.0267	0.0181		0.0262	0.0174		0.0268	0.0182
Het. Var SD		0.0209	0.0137		0.0209	0.0137		0.0205	0.0132		0.0209	0.0137
Observations	98401	98401	98401	97531	97531	97531	84267	84267	84267	97677	97677	97677
R-sq	0.458	0.458	0.458	0.544	0.544	0.544	0.657	0.657	0.657	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: 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 A8

Impact of the Reform on Investment, by Investment Category

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

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

Table A9

Impact of the Reform on Fiscal Sustainability, 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.	18.04***	15.07***	0.0891***	0.0801***	-0.00817	-0.00791
Auditor=1	[3.385]	[3.174]	[0.0135]	[0.0135]	[0.0243]	[0.0245]
Dep. Var Mean	-201.4	-201.4	64.92	64.92	3.291	3.291
Observations	114028	114028	114028	114028	114028	114028
Adj. R-sq	0.679	0.700	0.751	0.757	0.412	0.413
% 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: 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, 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: 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 A11
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 × Post	9.051***	0.0384***	0.0170
	[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, population-bins-by-year fixed effects, relative time fixed effects and election cycle fixed effects, as well as the following controls: mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A12

Impact of the Reform on Fiscal Sustainability, including Cohort-by-Municipality Fixed Effects

	Net Surplus		Debt Repayments		OBS Debt Repayments	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated=1 × Post=1	16.06*** [3.228]	15.50*** [3.224]	0.0860*** [0.0138]	0.0848*** [0.0138]	-0.00281 [0.0253]	-0.00356 [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	0.672	0.673	0.736	0.737	0.359	0.359

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, population-bins-by-year fixed effects, relative time fixed effects and election cycle fixed effects, as well as the following controls: mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A13

Impact of the Reform on Fiscal Sustainability, 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, population-bins-by-year fixed effects, region-times-year fixed effects, relative time fixed effects and election cycle fixed effects, as well as the following controls: 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, 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*** [3.236]	15.88*** [3.566]	0.0872*** [0.0139]	0.0872*** [0.0149]	-0.00293 [0.0253]	-0.00293 [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, population-bins-by-year fixed effects, relative time fixed effects and election cycle fixed effects, as well as the following controls: 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 A15
Impact of the Reform on Fiscal Sustainability, by Geography

	Net Surplus	Debt Repayments	OBS Debt Repayments
	(1)	(2)	(3)
Treated × Post	14.67*** [3.876]	0.0167 [0.0124]	-0.0329 [0.0220]
Treated × Post × South	-14.21** [6.549]	0.123*** [0.0299]	0.0740 [0.0575]
Treated × Post × Center	-2.078 [7.850]	0.0390 [0.0319]	0.0797 [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: 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 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 A17
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	1.703	0.000784	0.0158	-0.0257	0.0150
	[6.189]	[9.887]	[0.0196]	[0.0459]	[0.0346]	[0.0806]
Treat × Post × At least 1 hr	24.74***	38.15***	0.0445**	0.0864*	0.0111	-0.0543
	[7.114]	[13.52]	[0.0217]	[0.0522]	[0.0378]	[0.0944]
Dep. Var Mean	-226.061	-218.240	71.998	64.774	2.319	3.261
Observations	63475	18566	63475	18566	63475	18566
R-sq	0.696	0.722	0.818	0.727	0.358	0.350

Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation. All regressions include municipality, population-bins-by-year fixed effects, relative time fixed effects and election cycle fixed effects, as well as the following controls: 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
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		-0.0193		0.0271	
	[4.614]		[0.0139]		[0.0346]	
Second-Term Auditor=1		4.886		-0.00121		-0.0182
		[4.773]		[0.0105]		[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.

Table A19
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 × Local Auditor	15.12**	0.135***	0.0419
	[6.366]	[0.0240]	[0.0492]
Indep. Auditor=1	14.05***	0.0632***	-0.0183
	[3.167]	[0.0124]	[0.0230]
Indep. Auditor=1 × Corruption=1	12.73*	0.144***	0.0464
	[6.828]	[0.0311]	[0.0656]
Indep. Auditor=1	10.29*	0.00338	-0.0364
	[5.987]	[0.0169]	[0.0310]
Indep. Auditor=1 × noexperience=1	8.101	0.0392**	0.0215
	[6.293]	[0.0155]	[0.0274]
Indep. Auditor=1	12.17***	0.0608***	-0.0253
	[3.729]	[0.0145]	[0.0254]
Indep. Auditor=1 × t_distant=1	6.679	0.0377***	0.0246
	[4.359]	[0.0135]	[0.0250]
Indep. Auditor=1	13.98***	0.0880***	0.0105
	[3.509]	[0.0141]	[0.0261]
Indep. Auditor=1 × Pre-Election Years=1	3.602	-0.0205	-0.0563**
	[4.911]	[0.0150]	[0.0276]
Indep. Auditor=1	22.04***	0.0818***	-0.00647
	[4.854]	[0.0188]	[0.0339]
Indep. Auditor=1 × Re-Electable	-8.933*	-0.00318	-0.00894
	[5.079]	[0.0179]	[0.0323]
Indep. Auditor=1	18.65***	0.0876***	-0.0110
	[3.718]	[0.0141]	[0.0256]
Indep. Auditor=1 × Strong Mayority	-7.666*	-0.0197	-0.00492
	[4.593]	[0.0142]	[0.0259]
Indep. Auditor=1	-7.314**	0.0849***	-0.403***
	[3.562]	[0.0130]	[0.0276]
Indep. Auditor=1 × Below Median=1	47.45***	-0.00850	0.169***
	[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, population-bins-by-year fixed effects, relative time fixed effects and election cycle fixed effects, as well as the following controls: mayor initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A20
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

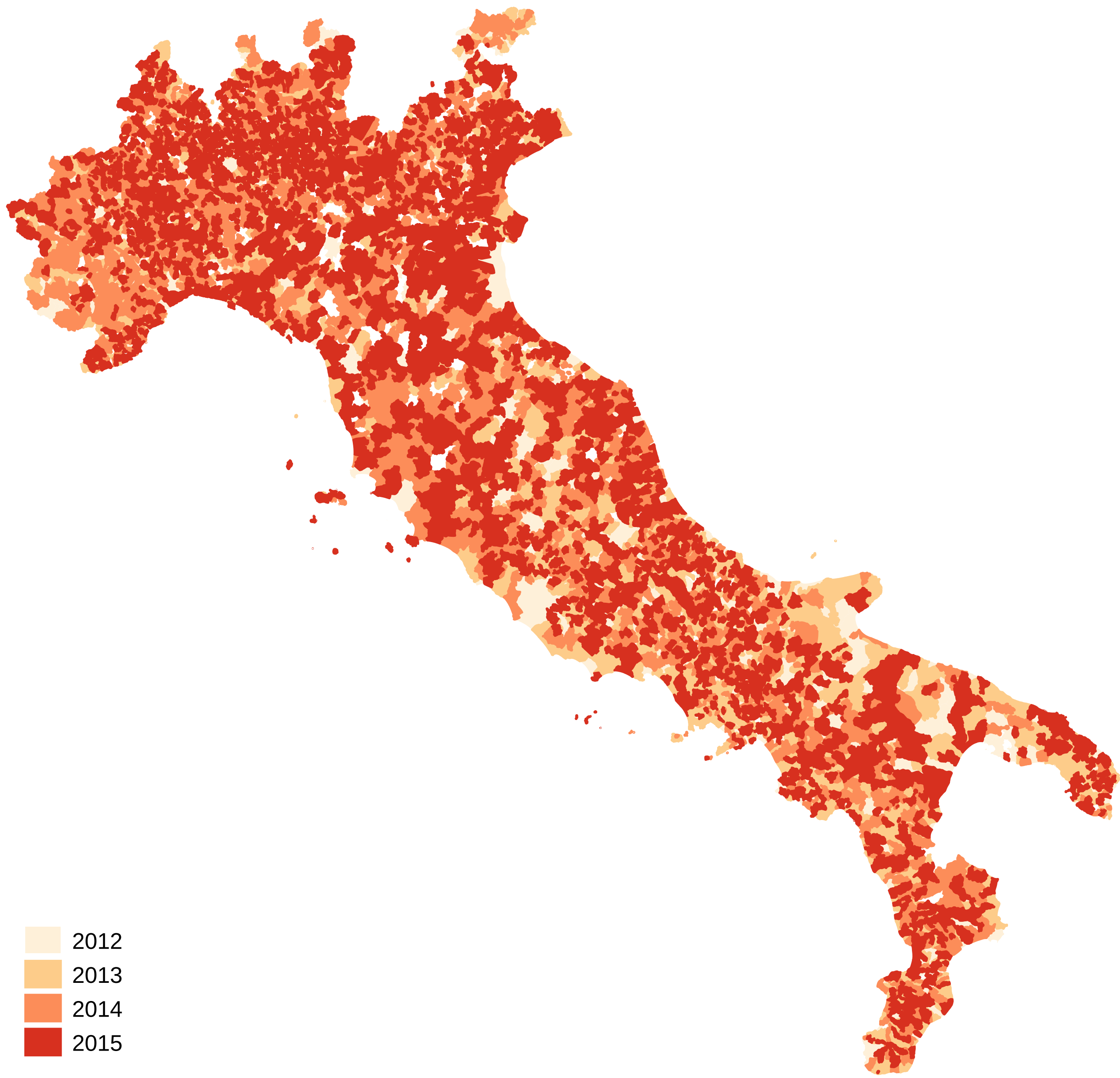
Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic 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: 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 A21
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

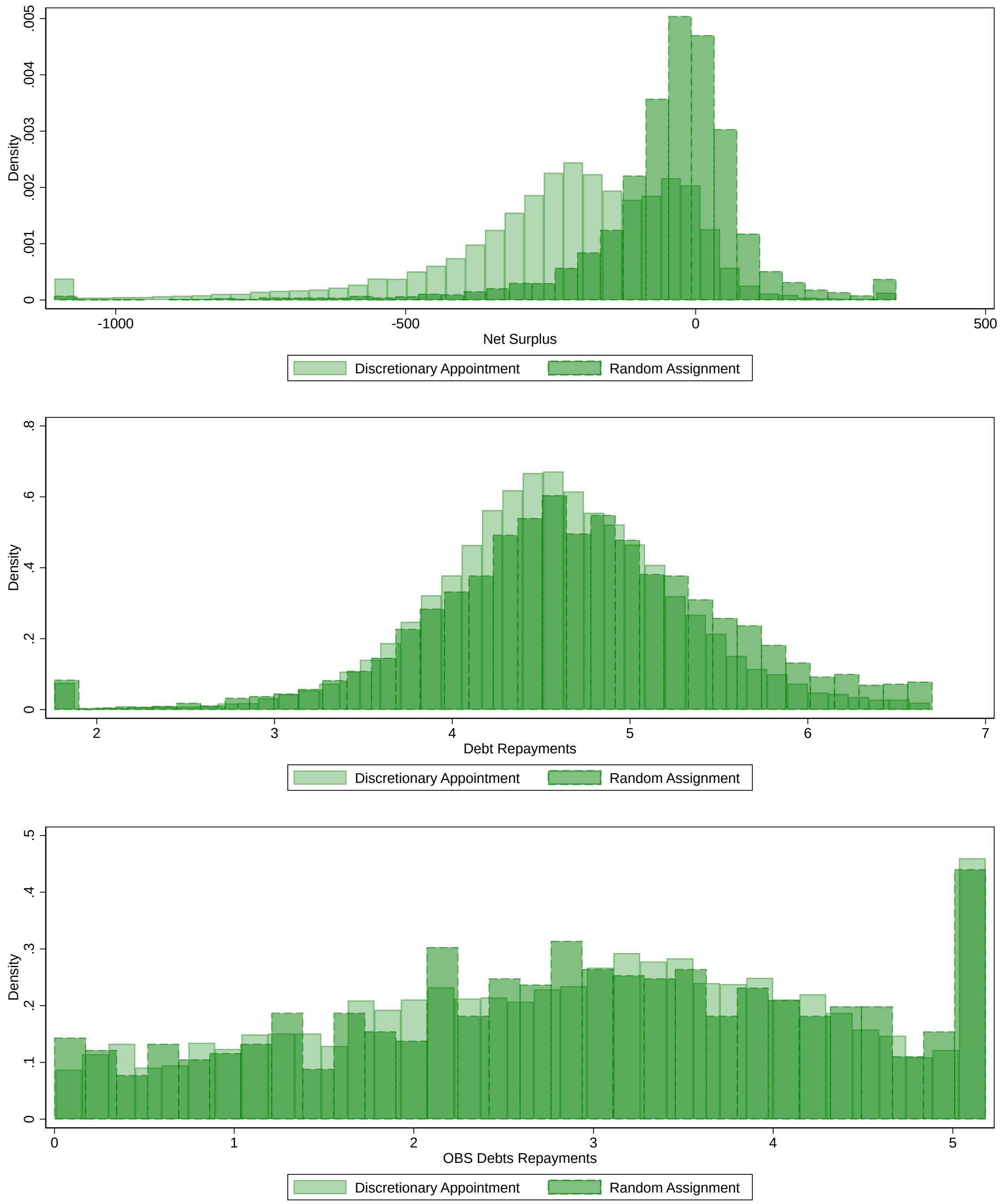
Notes: Significance: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic 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: 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



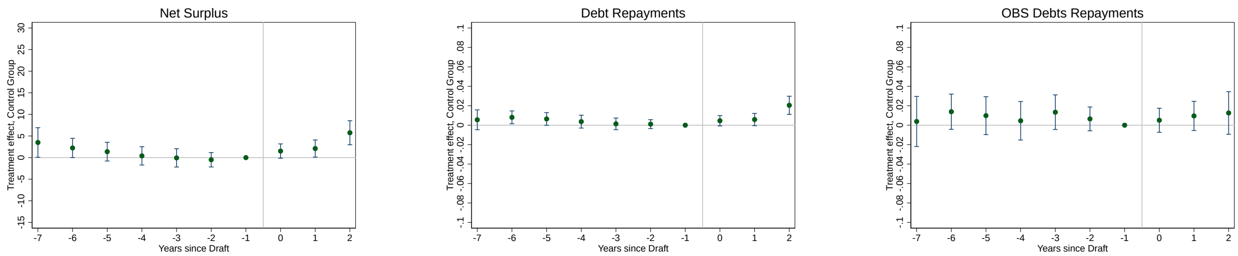
Notes: The figure shows the geographic variation in treatment timing. Darker gradation reflects later treatment timing.

Figure A2
Distribution of Outcomes, by Treatment Status



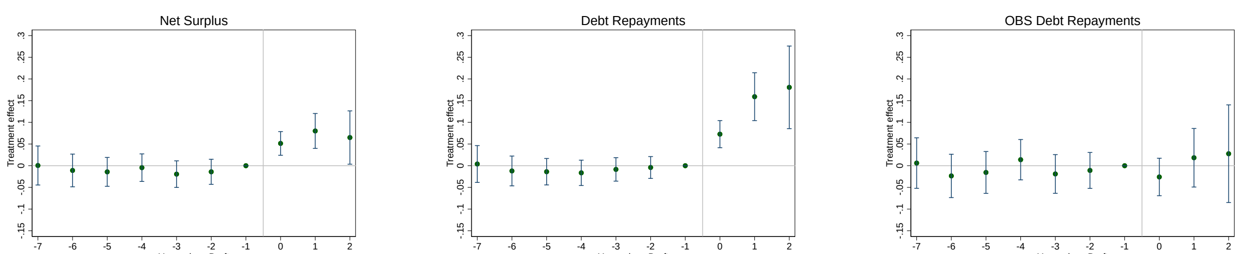
Notes: The figure shows histograms of the three outcome variables, $NetSurplus$, $DebtRepayments$ and $OBSDebtRepayments$, by treatment status. All dependent variables are in per capita terms, $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine transformation.

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



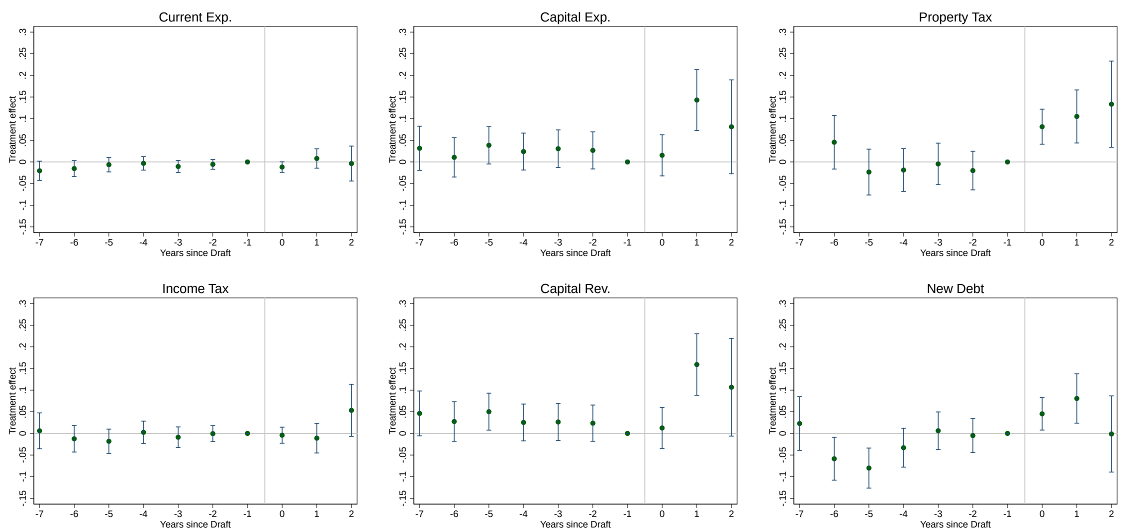
Notes: The graphs report the β_k coefficients and 95% confidence intervals estimated according to Equation 3. Standard errors clustered at the municipality level. All dependent variables are in per capita terms, $DebtRepayments$ and $OBSDebtRepayments$ are transformed using the inverse hyperbolic sine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

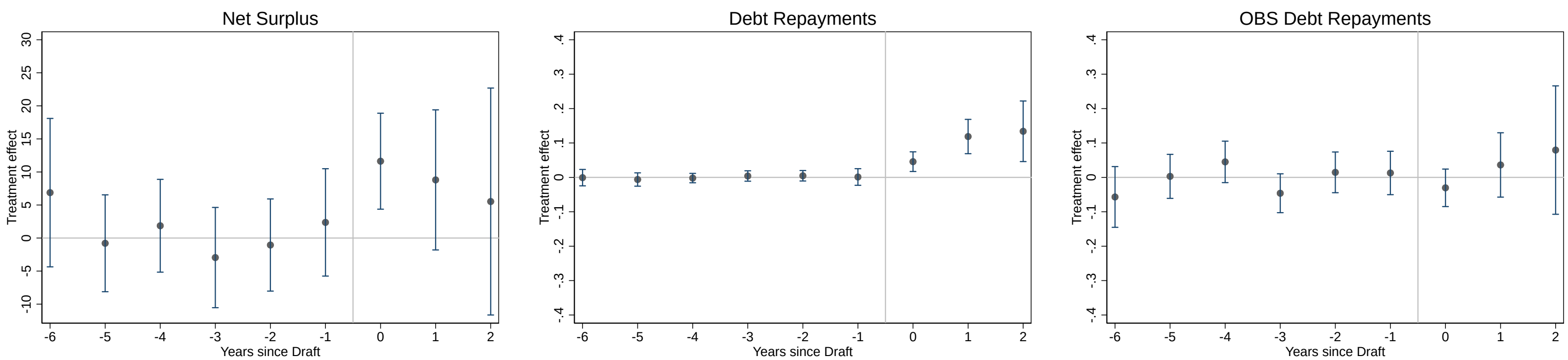
Figure A5
The Dynamic Effect of Auditor's Independence on Aggregate Spending and Revenue Choices, Standardized Outcomes



Notes: The graphs report coefficients and confidence intervals estimated according to Equation 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: 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

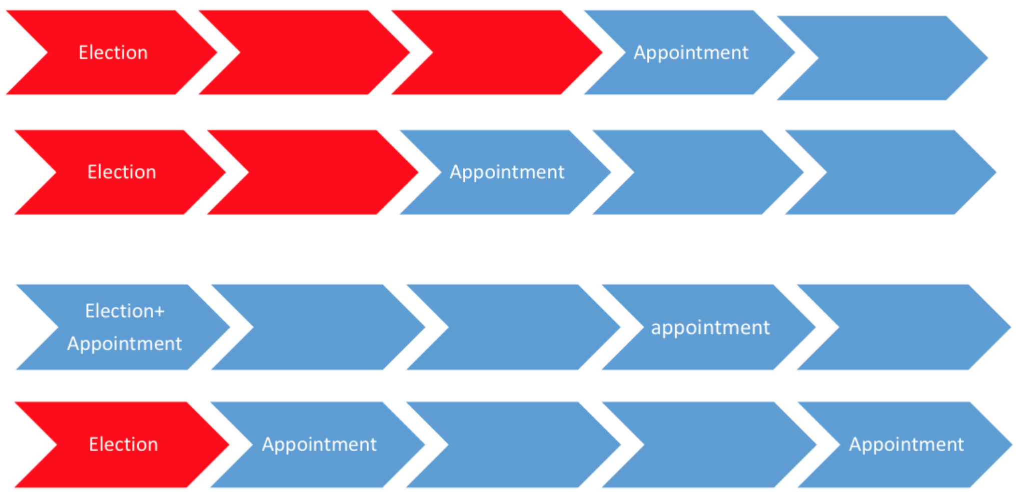


Notes: The graphs report coefficients and confidence intervals of the DID_M estimators estimated according to de Chaisemartin and D'Haultfœuille (2020) methodology using the did_multipleGT command in Stata. Standard errors clustered at the municipality level. All dependent variables are in per capita 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: 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

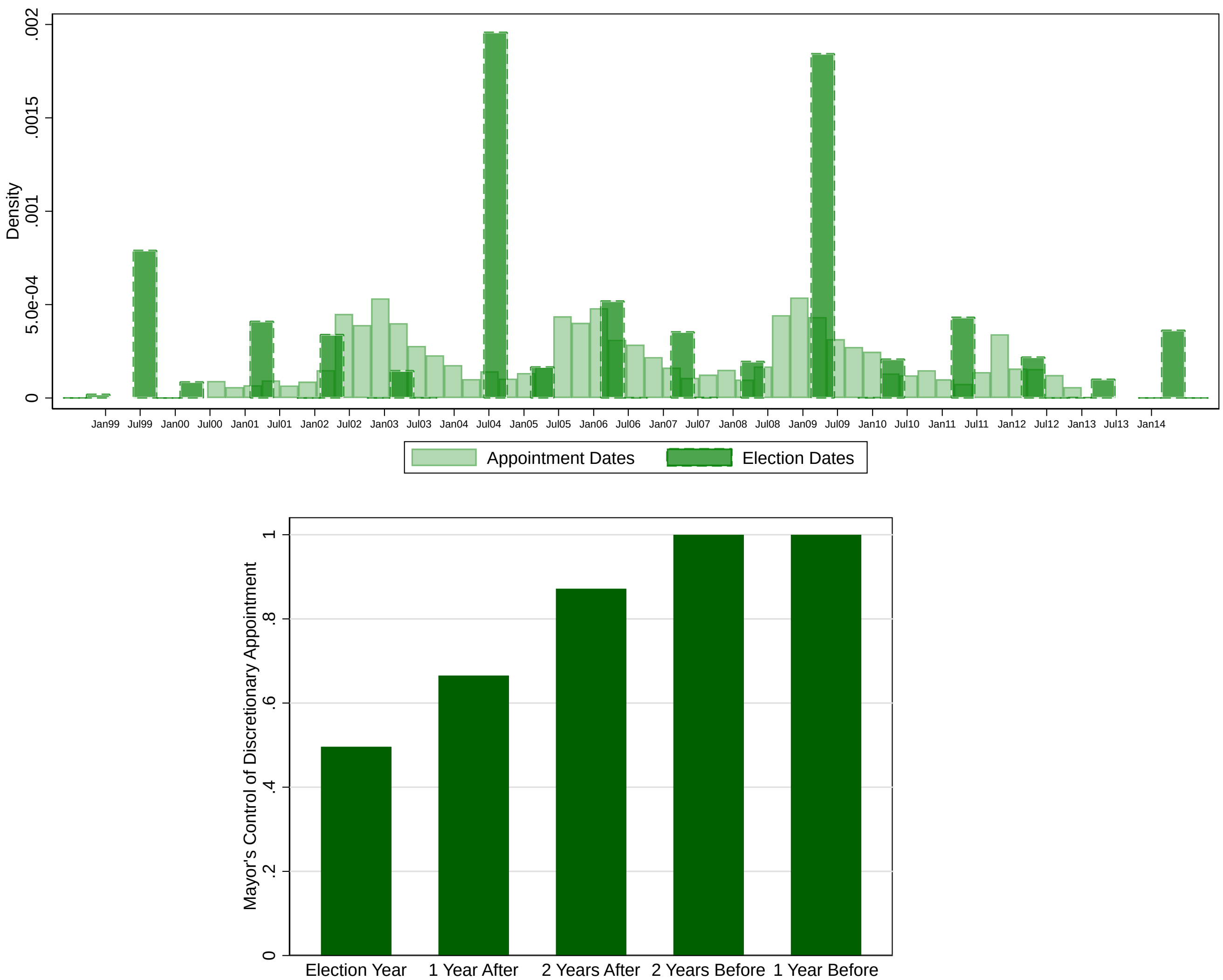
Notes: The figure displays a map of the municipalities having the *Corrupt* dummy equal to 1. To construct this variable, I exploit restricted-access data from the *Sistema D'Indagine Interforze* (SDI), a centralized investigation archive that contains reports of all individuals investigated by any of 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 the 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 (Di Cataldo and Mastrorocco 2022, Fenizia et al. 2020), and this is likely to be reflected in the municipal budgets. While I prefer not to include this as a control in my main specifications, as council dismissal could in principle be itself an outcome, all of my results control for council dismissals are robust to controlling for or excluding 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 A9. Reassuringly, the estimates are all very similar to the ones in Table 3. I

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

then estimate the weights attached to each of the average treatment effects (ATTs) to compute the overall β_{fe} estimate.[50] Results are displayed in the bottom part of Table A9. The β_{fe} coefficients in Table A9 are obtained as a weighted average of 10,747 ATTs, of which 3175 receive a negative weight, but overall the negative weights only sum up to -0.106, suggesting that the relative importance of ATTs receiving negative weights is limited. To have a better sense of whether this represents a problem, one can look at the two diagnostic measures to assess the robustness of the $\hat{\beta_{fe}}$ estimate to treatment effect heterogeneity. The first one $\underline{\sigma}_{fe}$, corresponds to the ratio between the $\hat{\beta}_{fe}$ and the standard deviation of the weights, which is a proxy of the (unobserved) degree of heterogeneity in ATEs across treated groups and periods and reflects the minimal value under which it would be possible to have a β_{fe} which is of the opposite sign of the true ATT. The second one,$\underline{\underline{\sigma}}_{fe}$, is a proxy of the minimal amount of treatment effect heterogeneity under which it would be possible to obtain a β_{fe} which is of the opposite sign of all the ATEs. Note that this second measure is defined only if at least one of the weights is negative, otherwise, it would be impossible to obtain a β_{fe}, which is a weighted average of all the ATEs, of opposite sign to them. Furthermore, across all outcomes, $\underline{\sigma}_{fe}$, which reflects the minimal value under which it would be possible to have a β_{fe} which is of the opposite sign of the true ATT, 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.[51] 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 A10 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

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

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

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).

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 A20 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 A20, 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

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

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 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 A21 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 A21, 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.