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FROM LAPDOGS TO WATCHDOGS:  
RANDOM AUDITOR ASSIGNMENT AND MUNICIPAL FISCAL PERFORMANCE

Silvia Vannutelli

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### **ABSTRACT**

A central challenge in public finance is how to design oversight institutions that align local governments' incentives with national fiscal objectives. While monitoring can mitigate agency problems, it may itself be rendered ineffective if monitors are corruptible. In this paper, I evaluate the consequences of changes in the design of monitoring institutions for organizational performance. I exploit the staggered introduction of a reform that removed the control of municipal auditors' appointments 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 and persistently improve their net surpluses and debt repayments, in line with national government objectives. Third, the fiscal improvement results from a sizeable increase in tax capacity. Fourth, treatment effects are significantly larger where the risk of auditor capture was highest before the reform, but also increasing in auditors' expertise—suggesting the potential presence of a bias-information trade-off. Overall, the results highlight the value of monitors' independence and illustrate how changes in organizational design can substantially improve governance.

Silvia Vannutelli  
Northwestern University  
and NBER  
silvia.vannutelli@northwestern.edu

# 1 Introduction

Local governments provide a large share of public goods and services, but their fiscal autonomy creates well-known agency problems: local officials may undertax, overspend, and accumulate debt, often in anticipation of eventual bailouts from the central government. A central challenge in public finance is thus how to design oversight institutions that align local governments' incentives with national fiscal objectives. One important mechanism is the use of external monitors—such as auditors—who provide an intermediate layer of supervision between local officials and central authorities. This raises the important question of who should select these monitors.

In many settings, the entities being overseen choose who audits them. In the private sectors, firms are typically allowed to pick and pay their own external auditors, credit agencies and regulatory compliance inspectors. In the public sector, similar arrangements are common: school districts choose their own accreditation bodies, hospitals select peer-review organizations to certify their compliance. While such discretion may generate benefits—allow agents to choose someone they trust, with whom they communicate well, and who can provide tailored advice—it also raises concerns about conflicts of interest ([Bazerman et al. 1997](#)).

The prevalence of such arrangements suggests that the perceived benefits of leaving discretion may outweigh the potential costs. Yet, it is difficult to know whether this is truly the case, since monitoring institutions are typically endogenous and rarely undergo reforms that allow for causal inference. This paper fills this gap by providing causal evidence on how the rules governing auditor appointments shape the fiscal performance of the audited party and the conditions under which oversight institutions effectively serve the principal's objectives. I leverage a unique large-scale reform in Italy that removed discretion from local politicians and introduced a random assignment mechanism for the appointment of municipal auditors. These professional auditors act as an intermediate layer of oversight between municipalities and the central monitoring performed by the National Court of Audit. The audit system aims to ensure sound financial practices and compliance with national fiscal rules.

Differently from compliance or anti-corruption auditors, who merely verify outcomes ex post, municipal auditors provide guidance along the entire budgetary process and can thus continuously influence municipal decisions throughout their three-year long terms. Their advisory role is particularly important in settings where local politicians and bureaucrats have limited financial expertise, as is often the case in small municipalities. In this context, allowing mayors discretion in auditors' appointment could plausibly have important advantages. First, mayors are well placed to identify auditors with the right expertise to understand local conditions. Local knowledge could also allow auditors to apply fiscal rules more flexibly, for example in the presence of temporary local shocks. Working with a trusted, locally chosen

auditor also facilitates the exchange of information. Moreover, removing mayors' autonomy risks provoking resistance, as autonomy itself is highly valued by local officials and can lead to higher effort and improved performance (Kala 2024, Bandiera et al. 2021). In this sense, discretion may have enhanced not only the quality of advice municipalities received, but also the motivation and cooperation of local officials themselves. Yet the same features that make discretion potentially valuable also open the door to distortions. Local knowledge may make auditors more sympathetic to local political pressures, more willing to justify excess deficits or to overlook weak tax collection when enforcement is politically costly. Discretion thus presents a fundamental *bias-information* tradeoff, typical of models of delegation in organizations (Alonso and Matouschek 2008, Mookherjee 2012). The context I study maps directly onto this theoretical tradeoff and allows me to causally evaluate whether the benefits of discretion outweighed its costs.

The reform under study was adopted amidst the European Sovereign Debt Crisis. Facing market pressure to reduce the national debt, the Italian government became increasingly concerned about local governments' fiscal health: local entities cannot default, and their deficits are consolidated in the national accounts. The new appointment system was introduced gradually as incumbent auditors' terms expired, which occurred at different times across municipalities between 2012 and 2015 for historical reasons unrelated to the reform and beyond mayors' control. This generates exogenous, staggered variation in treatment timing that I exploit for identification. I apply a generalized difference-in-differences methodology, comparing municipalities treated earlier to those treated later, before and after the arrival of a randomly assigned auditor. Under the assumption that treatment timing is uncorrelated with the evolution of outcomes over time, this identifies the effect of the appointment reform on municipal fiscal health. In support of this assumption, I document the absence of differential selection into treatment timing across a wide range of municipal characteristics, and show that early and later-treated municipalities exhibit similar pre-treatment levels and trends in all outcome variables. To address concerns raised by the recent econometric literature on staggered DiD designs, I adopt a stacked-by-event approach (Cengiz et al. 2019, Deshpande and Li 2019), which restricts treatment effect estimation to comparisons between units switching into treatment and not-yet-treated units.

I start by investigating how random assignment affects the selection and allocation of auditors. I document that the reform severs connections between municipal auditors and mayors along several dimensions likely to impair monitoring effectiveness. The share of auditors residing in the municipality they monitor falls from 31% before the reform to less than 1% afterward; those sharing a surname with a local politician drop from 7% to 1%. The reform also transforms the auditor pool: nearly 14,000 new entrants join, representing around 60% of the post-reform pool. While these newcomers lack prior municipal auditing

experience, they are otherwise very similar to the incumbents, with equal years of experience as accountants.

Second, I study the causal effects of random auditors’ assignments on municipal finances. I find that treated municipalities significantly improve their financial performance in line with national government objectives, increasing debt repayments by 9% and their net budget surplus by 8%.<sup>1</sup> The effects materialize immediately upon the arrival of the randomly assigned auditor and persist throughout the three-year term, suggesting a sudden and durable shift in budgetary practices. I further provide suggestive evidence that the effects persist in the long run, up to seven years after the adoption of the reform. Overall, these results suggest that randomly appointed auditors are more effective enforcers of fiscal rules, underscoring the importance of enforcement mechanisms for rules’ effectiveness — a point raised in recent theoretical work (Halac and Yared 2019, Dovis and Kirpalani 2020) yet lacking empirical support until now.

I then investigate the margins of adjustment. Municipalities can improve their fiscal stance by expanding revenues or cutting spending. I find that they sizeably increase revenues from the property tax — the main local tax — by over 20%, with no evidence of expenditure cuts. The revenue increase stems from an expansion of the tax base rather than higher tax rates, consistent with improved enforcement of tax collection upon the arrival of randomly assigned auditors.<sup>2</sup> Corroborating this mechanism, effects are substantially larger in municipalities with a higher share of housing units and buildings undeclared to tax authorities, and treated municipalities increase investments in administrative capacity, particularly in high-evasion settings. Overall, the evidence suggests that tighter oversight strengthens mayors’ incentives to pursue property tax delinquents. Furthermore, I rule out any negative effects on public good provision or the local economy.

In the final part of the paper, I investigate the mechanisms behind the estimated effects. Random assignment removes mayors’ ability to appoint auditors who may be more lenient toward fiscally irresponsible choices—but also their ability to select auditors on the basis of expertise. This creates a canonical trade-off (Alonso and Matouschek 2008, Mookherjee 2012): eliminating capture improves outcomes, while losing the ability to select skilled monitors worsens them. I provide evidence for both sides of the tradeoff. On the one hand, I find significantly larger treatment effects in municipalities more at risk of auditor’s capture before the reform. I further show that effects are larger where the randomly-assigned auditor

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<sup>1</sup>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’ capture, suggesting that this is an important proxy for inappropriate practices.

<sup>2</sup>Italian municipalities have full discretion on local tax collection procedures. A critical auditor responsibility is to examine these procedures and monitor collection effectiveness, including proposing improvements such as cross-checks across data sources to detect evasion or nudges to increase compliance.

resides at least one hour away, consistent with social proximity adversely affecting monitoring effectiveness (Xu et al. 2021). On the other hand, I also show that treatment effects are smaller where pre-reform auditors were more experienced and larger where random assignment delivers an auditor more experienced than the predecessor—pointing to a genuine advisory contribution of expertise. Taken together, the large positive average treatment effects imply that, in this context, the gains from eliminating capture dominated the costs of potential loss of expertise.

Finally, using the lens of corruption monitoring models (Becker and Stigler 1974, Banerjee et al. 2012, Olken and Pande 2012), I document evidence of both a direct *detection* effect and an indirect *deterrence* effect. Consistent with detection, treatment effects are larger when mayors face weaker re-election pressures, suggesting external oversight substitutes for missing political discipline. Consistent with deterrence, I document sizable spillover effects to neighboring untreated municipalities.

Overall, these findings underscore the importance of monitoring institution design in mitigating agency problems. Removing appointment discretion from agents can significantly enhance fiscal performance by preventing capture and better aligning agents’ behavior with the principal’s objectives.

**Related Literature:** First and foremost, I contribute to the growing literature on principal-agent problems in the public sector and their implications for state performance. While some empirical papers have documented the effects of *introducing* monitoring tools to reduce corruption and improve accountability (Olken 2007, Ferraz and Finan 2008, Bobonis et al. 2016), taking audit quality as given, very little is known about the determinants of monitoring effectiveness. A notable exception is Duflo et al. (2013), who use an RCT with 236 industrial plants in India to estimate the combined effects of random environmental auditors’ assignments and stronger monetary incentives. I build on their findings in four ways. First, the reform I study isolates the appointment mechanism, holding monetary incentives constant. Second, I focus on a context where political economy considerations complicate oversight in ways absent from private markets. Third, auditors in my setting serve multi-year terms and advise as well as monitor, introducing a tradeoff between information and capture that is absent in their context. Fourth, the reform I exploit is a national policy change, allowing me to corroborate the external validity of Duflo et al. (2013)’s findings at scale.

Second, I contribute to the broad debate on the optimal design of organizations (Gibbons et al. 2013). The allocation of decision-making power and the structure of monitoring are key elements of organizational design. This paper focuses on the allocation of authority over a particularly consequential decision, the selection of monitors. A large theoretical literature has highlighted the complications that arise as auditors and agents might enter into

collusive agreements (Strausz 1997, Ortner and Chassang 2018, Strulovici 2021, Mookherjee and Tsumagari 2023), but also the potential informational benefits from local, as opposed to central, supervision (Dessein 2002, Repullo 2018, Dessein et al. 2025). My research demonstrates empirically that the costs of discretion might outweigh its benefits, as agents might select auditors whose interests align with their own rather than with those of the principal.

More generally, the fiscal improvements I document also connect this paper to the literature on state capacity and tax enforcement. My results complement previous evidence on the role of tax design (Best et al. 2015), individual ability (Best et al. 2023), local knowledge (Balan et al. 2022), technology (Dzansi et al. 2022), and tax collectors' incentives (Khan et al. 2016) for fiscal performance. I expand this work by providing evidence of a new, key mechanism behind improvements in local tax collection, that is the presence of effective monitoring institutions that allow for a realignment of local leaders' incentives with national objectives and that limit moral hazard problems that may lead to under-investment in tax enforcement.

Finally, my results speak to the literature on fiscal crowd-out in public finance, which documents that central transfers to subnational governments do not always translate into higher local spending because recipients adjust their own tax effort in response (Knight 2002, Gordon 2004). This literature identifies the design of intergovernmental transfers as a key lever for local fiscal outcomes. My findings highlight a complementary lever: the design of oversight institutions. The reform I study does not alter the flow of resources to municipalities, yet changing only the appointment mechanism of local auditors substantially raises local fiscal effort. This suggests that improving oversight over how municipalities manage their own finances can be as consequential for local compliance as re-engineering the transfer schedule itself.

## 2 Institutional Context

### 2.1 Fiscal Monitoring

Italy is a highly decentralized democracy with over 8,000 municipalities. Each municipality has 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 five-year mandate with a two-term limit, holds executive power, 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 and have full control of a wide range of essential public services. Municipal revenues are drawn primarily from local taxes, most importantly, the property tax, and municipalities enjoy a substantial amount of discretion over tax collection,

tax rates, and budget allocations.

From 1999 onwards, all Italian sub-national entities have been subject to the “Domestic Stability Pact” (DSP), the national counterpart of the EU Stability and Growth Pact. The pact requires municipalities to limit the growth of their fiscal gap—defined as the deficit net of transfers and debt service—below a given threshold (Grembi et al. 2016). Local deficits and surpluses are consolidated into the national budget, and sub-national debt accounts for approximately 11% of GDP. Additional details are provided in Appendix A2.

## 2.2 The Role of Municipal Auditors

Since 1990, all municipal budgets must be audited and certified by a board of auditors. The board consists of one member for municipalities with fewer than 15,000 inhabitants—representing 90% of municipalities—and three members for larger ones. Board members can hold at most eight simultaneous appointments and cannot be dismissed unless the council can prove faulty inaction or breach of official duties.

Prior to the reform, the board used to be nominated with a majority vote of the municipal council. However, this formal procedure did not confer any meaningful independence to the council. Under Italian municipal electoral law, the coalition supporting the elected mayor automatically receives at least two-thirds of the council seats — a majority bonus designed to ensure governability. The council’s survival is moreover tied to the mayor’s by the *simul stabunt, simul cadent* principle: a council vote against the mayor triggers the automatic dissolution of both bodies and the calling of new elections. The council therefore has no incentive to oppose the mayor’s preferred choice of auditor, and its vote amounts to a formal ratification of a decision taken by the executive. Pre-reform auditor appointments were, in effect, a mayoral prerogative.

The auditing job is carried out by self-employed Certified Public Accountants rather than large audit firms.<sup>3</sup>

From its very first inception, the board was assigned a dual role of monitoring and advising. Municipal auditors do not merely sign off on balance sheets ex-post: they perform over 200 tasks per year spanning both supervision and advice. These include providing formal written opinions on proposed budgets and tax collection protocols, verifying the accuracy of revenue projections, and certifying compliance with fiscal rules. Table A1 provides a full enumeration of auditors’ tasks.

The results of audit reviews and auditors’ opinions are compiled in a report reviewed by the council at the time of budget approval. While opinions are not legally binding, the

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<sup>3</sup>With over 150,000 registered CPAs eligible to serve as municipal auditors, the potential pool is large. Yet discretionary appointments by mayors likely limited reliance to a restricted set of “connected” auditors.



council is expected to follow them or formally justify any inaction. Audit documents are subsequently transmitted to the National Court of Audit, a Supreme Audit Institution which can initiate judicial proceedings against non-compliant municipalities. Non-compliance with auditors' recommendations is itself a trigger for increased Court scrutiny. Appendix A2 provides further details on auditors' statutory mandates and the oversight process.

To situate the municipal auditors I study within the broader literature, it is useful to contrast their role with that of purely ex-post audit programs, such as the federal anti-corruption audit lotteries studied in Brazil (Ferraz and Finan 2008, ?). Those programs involve occasional, backward-looking investigations into past misconduct, conducted by an external body of bureaucrats appointed by the central government with no ongoing relationship to the audited entity. Italian municipal auditors, by contrast, are locally-hired external consultants who are continuously embedded in municipal governance throughout their three-year term. They attend council sessions, review documents as they are produced, and must formally provide their opinions before the council adopts the annual budget, the provisional balance sheet, and a range of other key financial instruments. This ex-ante, advising dimension of their activity can have consequential effects that go in opposite directions depending on auditors' behavior. An independent auditor can flag risks before they materialize, push back on unrealistic revenue projections, encourage compliance with fiscal rules, and advise on ways to improve tax collection and expenditure management. A captured auditor, by contrast, can exploit the same position to validate irregular financial practices, issue favorable opinions on inflated budgets, or remain silent about off-balance-sheet liabilities. The auditor's role is thus both dual and continuous-encompassing oversight of past decisions and advisory input on future ones-which implies that the identity and independence of the auditor shape not only the detection of past violations but the quality of fiscal governance prospectively.

## 2.3 The Auditors' Appointment Reform

During the European Sovereign Debt Crisis, the national government progressively tightened the pact's requirements to shift part of the EU-mandated debt reduction onto local entities, making fiscal sustainability at the municipal level a central concern and motivating the push to strengthen fiscal monitoring procedures. In August 2011, at the peak of the crisis, the national government 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"*. Due to administrative constraints and the time needed to form the public lists, the new procedure entered into

effect on December 10, 2012.

The random draw is carried out by prefectures—the local offices of the Ministry of the Interior—via a standardized, computer-based system. Municipalities must notify their local prefecture at least two months before the current auditor’s term expires. The prefecture then draws a slate of candidates, equal to  $3XN$ , where  $N$  is the number of auditors to be appointed, and the municipal council must appoint them in the order drawn; mayors cannot choose among drafted candidates. If the first-ranked candidate refuses or is deemed ineligible, the municipality contacts the second-ranked, and so on. Refusals are relatively rare: approximately 20% of first-drafted candidates decline the appointment, and in fewer than 3% of cases a new draw was required because no candidate from the original slate accepted. However, this partial non-compliance does not undermine the validity of the design: municipalities experiencing a rejection are immediately assigned another randomly drawn auditor, so that the random assignment is preserved. Moreover, descriptive evidence reported in Appendix Table A3 shows that rejections are mainly predicted by distance (and, to a lesser extent, gender), while other professional or municipal characteristics are balanced across accepted and rejected cases. Crucially, refusals are uncorrelated with indicators of municipal financial performance.<sup>4</sup>

Finally, it is important to note that the reform does not apply to the 5 “Special Status” Regions (Valle d’Aosta, Trentino Alto Adige, Friuli Venezia Giulia, Sardinia, and Sicily), which retain autonomy over their own fiscal monitoring procedures. My analysis is therefore limited to municipalities in the 15 “Ordinary” regions. Full details of the appointment procedure, experience requirements, eligibility rules, and the rate of first-draw refusals are provided in Appendix A2.

### 3 Data

To measure financial outcomes, I use detailed data on all municipal balance sheets (*Bilanci Consuntivi*) provided by the Italian Ministry of the Interior. The data include detailed information about municipal governments’ spending and revenue sources, such as local taxes, current expenditures, investments, debts, and transfers. I focus specifically on a set of indicators that are used by the National Court of Audit to monitor municipalities’ fiscal sustainability. 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

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<sup>4</sup>In the main analysis, I perform a series of further robustness checks that confirm that treatment effects are quantitatively and qualitatively similar across municipalities independently on whether or not they experience a rejection, and robust to the inclusion of controls for auditors’ rejection (see Appendix Tables A4 and A5).

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. Third, I look at *OBSDebtRepayments*, representing the total amount of Off-Balance-Sheet (OBS) Debt that is recognized and re-paid by municipalities in a given year. This variable is a signal of potential profound imbalances. The National Court of Audit defines OBS Debt as 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.<sup>5</sup> Municipalities are allowed to recognize OBS liabilities that arose in previous years and had not been previously recognized.<sup>6</sup> To finance *OBSDebtRepayments*, municipalities may employ any existing current surplus and, residually, they may undertake new debt. Auditors are explicitly asked by the Court to closely monitor the presence of OBS debts and the process of recognition. They are asked to review the reasons why the OBS debts arose and evaluate the proposed repayment methods. The auditors are also required to report about *OBSDebtRepayments* in a specific section of the questionnaire they need to complete for the Court after the balance sheet has been approved. Given that they represent an exceptional violation of the rules, *OBSDebtRepayments* are a relatively rare phenomenon, occurring in only about 20% 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 revenue and spending variables, which are always non-negative, are then transformed using the inverse hyperbolic sine transformation. Table A2 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 randomly 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 drafted auditors. I combine this data with information from the candidates’ pool, published yearly by the Ministry of the

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<sup>5</sup>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.

<sup>6</sup>Municipal bureaucrats that approved unexpected expenses that count as OBS liabilities but cannot be recognized in the balance sheet, remain liable for those expenses.

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. Furthermore, I obtain information about auditors’ appointments for the pre-reform period from the annual questionnaire for the Annual Account of the Italian General Accounting Office submitted by municipalities to the Ministry of Economics and Finance.

Finally, I supplement the above information with further data to account for municipal characteristics that might affect fiscal sustainability. My main analysis sample consists of 5603 municipalities in Ordinary regions for the years 2007-2015. For a detailed description of controls as well as sample restrictions, see Section A3.

## 4 Empirical Strategy

My identification strategy exploits the staggered introduction of the reform, comparing the fiscal outcomes of municipalities treated earlier against those treated later. As described in Section 2, while the reform became effective in December 2012, treatment timing varies across municipalities depending on when each incumbent auditor’s term expires: a municipality whose auditor was appointed in August 2011, for instance, would receive its first randomly assigned auditor only in September 2014. Treatment timing therefore varies widely across municipalities for historical reasons unrelated to local fiscal conditions, and is independent of the electoral cycle, which also varies across municipalities but does not overlap with auditors’ three-year terms. Crucially, mayors have no ability to influence auditors’ appointment dates or term lengths, ruling out any strategic manipulation of treatment timing. Figures 1 and A1 illustrate the sources of identifying variation: Figure 1 shows the rollout of randomly assigned auditors over time, and Figure A1 shows their geographic distribution, confirming the absence of systematic geographic patterns in treatment timing.

Given the presence of staggered treatment assignment, I adopt a stacked-by-event design to perform my estimation ([Deshpande and Li 2019](#), [Cengiz et al. 2019](#), [Aneja and Xu 2024](#)). This estimator allows me to account for the recently highlighted potential pitfalls of two-way fixed effects estimators in the presence of staggered adoption ([Goodman-Bacon 2021](#), [de Chaisemartin and D’Haultfoeuille 2020](#), ?). In Section 5.3, I also show the robustness of my estimates to the use of alternative estimators recently developed by [Callaway and Sant’Anna \(2021\)](#) and [de Chaisemartin and D’Haultfoeuille \(2020\)](#).<sup>7</sup> My main estimating equation is as

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<sup>7</sup>The “stacked” method simply amounts to consider each reform’s wave of adoption as a separate sub-experiment, creating a separate dataset for each of the three treatment waves before the last one (2012, 2013, 2014). In each of these datasets, municipalities that receive the randomly-drafted auditor in that year are

follows:

$$Y_{mct} = \alpha_m + \delta_{pt} + \beta_0 Treated_{mc} + \beta_{DD} Treated_{mc} \times Post_{ct} + \sum_{k=-7}^{k=2} \beta_k D_{ct}^k + X'_{mt} \zeta + \epsilon_{mct} \quad (1)$$

Here,  $\alpha_m$  are municipality fixed effects, allowing me control for time-invariant differences in outcomes across municipalities;  $Treated_{mc}$  is a dummy that takes the value of 1 if the municipality  $m$  is a treated municipality in cohort  $c$ ;  $Post_{ct}$  is a cohort-time dummy equal to 1 for all years  $t$  on or after the treatment date of cohort  $c$ , for *all* municipalities in cohort  $c$ 's stack; and  $D_{ct}^k$  is a set of relative event-time dummies defined within each cohort-stack, that take the value of 1 if year  $t$  is  $k$  periods after (or before, if  $k$  is negative) the treatment date of cohort  $c$ , for all municipalities in that stack. 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  $\delta_{pt}$  for seven population bins so that municipalities belonging to different population size classes are allowed to evolve along different trends, allowing me to control for other shocks that affect the evolution of outcomes over time in a way that is common across municipalities of similar size, such as changes in macroeconomic conditions or fiscal rules.<sup>8</sup> 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:

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

In this specification, the coefficients of interest are the  $\gamma_k$ 's, measuring the change in outcomes of treated municipalities  $k$  years after treatment, relative to the pre-treatment year, relative

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considered treated, while municipalities that will experience the treatment in later years serve as a control, and event-time dummies relative to the year of treatment for the specific sub-experiment. Municipalities that experience treatment in the last year, 2015, serve only as controls.

<sup>8</sup>I construct dummies for the following population bins (following Italian Statistical Office classifications): 0–1,000; 1,000–5,000; 5,000–10,000; 10,000–15,000; 15,000–20,000; 20,000–60,000; and above 60,000 inhabitants, each interacted with year fixed effects to form the  $\delta_{pt}$  terms.

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 first draft-appointed auditor.

## 4.1 Identifying Assumptions and Threats to Identification

The key identifying assumption of my design is the absence of differential trends between municipalities experiencing treatment at different points in time; that is, absent the change in auditor’s appointment, the outcomes of municipalities belonging to different treatment cohorts would have evolved similarly over time. Given the exogenous and historical nature of the difference in audit cycles across municipalities, the timing of expiration of the existing auditor, and thus the timing of treatment, should be uncorrelated with the evolution of outcomes over time.

To provide evidence in favor of this assumption, Table A6 examines whether municipalities treated at different times display significant differences in observable characteristics (Panel A) and pre-reform outcome levels (Panels B and C). The first four columns report cohort-specific means, while column 5 reports the p-value of a joint F-test for the null that all cohort dummies are equal to zero. In Panel A, I fail to reject this null for all variables except population. The population imbalance reflects the fact that, for historical reasons, larger municipalities are slightly more likely to have their auditors’ terms expiring in 2012 or 2013. This fact should not threaten identification for several reasons. First, treatment timing is mechanically determined by the staggered expiration of existing appointments-mayors have no discretion over when they are treated and cannot manipulate in any way the expiration date of their current auditor. Second, I present results separately by population-size group, focusing in particular on the 5,000 and 15,000-inhabitant thresholds that govern eligibility for different types of auditors. Within each group, the identifying variation comes from municipalities of similar size subject to the same assignment rules, removing concerns that effects are driven by cross-size differences. Third, this within-group strategy also addresses potential confounding from differences in auditor composition across size categories, since comparisons are drawn among municipalities exposed to the same type of auditor but treated at different points in time. Finally, I control non-parametrically for population-related trends by including population-by-year fixed effects throughout. These controls also absorb potential effects from differences in the composition of the auditor pool across size categories due to minimum experience requirements for municipalities above 5,000 and 15,000 inhabitants. Taken together, these steps ensure that population-related heterogeneity does not confound the main estimates.

Panels B and C of Table A6 repeat the same exercise for outcome levels in 2010. While

some outcomes display small unconditional differences across cohorts, none remains significant after conditioning on the observables in Panel A. Notably, testing for pre-treatment level differences is a stronger requirement than the parallel trends assumption, which only requires similar counterfactual *trends*, not levels.

Furthermore, and most importantly, I 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. In Figure 2, I estimate the flexible dynamic specification of Equation 2 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. I exploit one of the stacked-by-event design features to test for the presence of anticipation effects. Because the design separately identifies calendar-time effects and event-time effects, I can directly inspect whether control municipalities change their behavior before becoming treated.<sup>9</sup> Figure A2 shows that, if anything, control units display anticipation of the first kind: their coefficients are of the same sign as those in Figure 2, though of substantially smaller magnitude. My estimates therefore represent conservative lower-bound estimates. This interpretation is further corroborated by the positive spillover effects on neighboring not-yet-treated municipalities documented in Section 6.3.<sup>10</sup>

Finally, other time-varying shocks correlated with the reform’s staggered rollout could confound identification. The most relevant change was the extension of the Domestic Stability Pact rules to municipalities below 5,000 inhabitants in 2014. Population-by-year fixed effects address this directly. Furthermore, I show that treatment effects are also present when restricting the sample to municipalities above 5,000, which have been subject to the same rules since 2001 (Section 6). More broadly, the 2012 emergency reform introduced additional changes affecting local finances. Because these changes affected all municipalities simultaneously rather than in a staggered fashion, their effects are fully absorbed by calendar time fixed effects. I provide a broader discussion of the interaction with other reforms in Section 5.3.2.

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<sup>9</sup>In the stacked-by-event design, each control unit is assigned a placebo shock at event time=0 for every event in which it serves as control, so that potentially anticipatory responses are explicitly accounted for in the estimation of the dynamic treatment effect.

<sup>10</sup>In Figure A2, a modest upward movement in Net Surplus appears in the years just before the reform for municipalities in the control group, likely reflecting municipalities starting to smooth adjustment ahead of treatment once they learned about the reform. To the extent that such effects exist, they would bias the estimates downward, as they reduce the contrast between treated and untreated municipalities after treatment.

## 5 Results

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

I begin by examining how the reform affected the selection and allocation of auditors across municipalities. This analysis serves two purposes: confirming whether the random assignment was effectively implemented and identifying key changes in auditor characteristics brought by the reform with potential implications for monitoring effectiveness. As a preliminary step, Table A7 tests the integrity of the randomized draw by showing that drafted auditors are statistically indistinguishable from the pool of eligible candidates across all observable characteristics.

I then move to compare the characteristics of the auditors and auditor-municipality match under discretionary appointments as opposed to the ones resulting from random assignment. Table 1, 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, to make it easier to compare the magnitude of the changes across different variables.<sup>11</sup>

The results show that the reform produced large and statistically significant changes along several key dimensions. The first notable result concerns the size and composition of the auditor pool. Following the reform, 22,000 auditors joined the list, 60% of whom are individuals with no prior experience in municipal auditing. This large entry response implies that post-reform auditors are, on average, less experienced in municipal auditing than their pre-reform counterparts. At the same time, the new entrants share similar characteristics with incumbents in other important respects, such as age and professional experience in accounting, suggesting that pre-reform barriers had restricted the entry of otherwise competent professionals. The reform thus appears to have broadened the auditor market considerably.

The second major change concerns re-appointment rates. Under the pre-reform discretionary system, incumbent auditors were re-appointed in 63% of cases—a pattern consistent with mayors systematically favoring familiar or aligned auditors. After the reform, re-appointment became an extremely rare occurrence, falling to just 1%, as it can now only arise from the same auditor being randomly drawn again for the same municipality. This feature has important implications for the incentives of those auditors who remain active: knowing that re-appointment no longer depends on mayoral discretion fundamentally alters

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<sup>11</sup>Furthermore, in Table A8 and Figure A3 I report results using the main empirical specifications in Equation 1 and 2, where the characteristics of the auditors and auditor-municipality matches are on the left-hand side. The event studies confirm a lack of pre-trends and a sudden and persistent change in auditors' characteristics.



the strategic incentives that may previously have induced leniency.

Third, looking at the characteristics of the auditor-municipality match, the reform dramatically changed the geographic and social proximity between auditors and the municipalities they oversee. Under the pre-reform system, 24% of municipalities appointed a local resident as auditor; this share falls to effectively zero after the reform. One consequence is a large increase in the average travel time between auditors' place of residence and their assigned municipality, from 19 to over 65 minutes. Similarly, the reform also curtailed social and family connections between auditors and local politicians. In the pre-reform period, 15% of auditors were born in the same place as the mayor, and 6% shared a surname with a local politician—patterns suggestive of deliberate selection based on personal ties. These shares fall to 2% and 1%, respectively, after the reform. This shift toward greater physical and social distance is likely to reduce the informal ties and local embeddedness that may have compromised auditor independence under the discretionary system. Indeed, previous research has shown that these types of connections are likely to negatively affect monitoring performance (?).

Taken together, the reform produced a fundamental transformation in the selection and allocation of auditors. These changes map directly onto a classic bias-information trade-off (?): by removing mayoral discretion, the reform improved independence but potentially sacrificed the informational advantage that local and experienced auditors might have possessed. The implications of these changes for auditors' effectiveness are ambiguous. The reduction in political alignment and local embeddedness plausibly strengthened monitoring effectiveness, which could translate into improvements in municipal fiscal performance. On the other hand, if connected auditors appointed under the status-quo discretionary system had higher municipality-specific knowledge and were thus able to provide more tailored advice on local fiscal practices, we could observe a null or even a worsening of fiscal performance. In the following section, I shed light on this question by investigating the causal effect of the reform on municipal fiscal performance.

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

Table 2 displays the main regression results for the effect of the arrival of a randomly-assigned auditor on indicators of municipal fiscal sustainability. Odd columns present results from Equation 1 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*. Treated municipalities increase their net surplus by €16 per capita, an 8% rise

relative to the absolute value of the pre-treatment mean, upon the arrival of a randomly-assigned auditor. In columns (3) and (4), I look at *DebtRepayments*. I find a significant and positive effect of comparable magnitude, with treated municipalities increasing their debt repayments by 9% after the arrival of draft-appointed auditors. 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 due to the presence of strongly heterogeneous dynamics across different types of municipalities.

Figure 2 displays coefficients from the event-study specification in Equation 2, allowing inspection of differential pre-trends and treatment effect dynamics over the auditor’s three-year term. The graphs show no significant differential pre-trends across all three outcomes. For both *NetSurplus* and *DebtRepayments*, treatment effects appear in the first year of the auditor’s arrival and remain consistent across the three years. This suggests a sudden and persistent shift in municipal budgetary practices in response to the treatment. The figure also confirms no significant effect on *OBSDebtRepayments*.

Overall, these results underscore the effectiveness of randomly assigning auditors in strengthening oversight and improving municipal fiscal health, thus emphasizing the importance of organizational design changes in enhancing monitoring.

## 5.3 Robustness and Persistence of the Effects

### 5.3.1 Alternative Estimators and Specifications

In this section, I discuss a series of empirical tests that I perform to corroborate the robustness of my analyses. I first apply three alternative estimators—the standard TWFE estimator and two robust alternatives proposed by [de Chaisemartin and D’Haultfœuille \(2020\)](#) and [Callaway and Sant’Anna \(2021\)](#) to address treatment effect heterogeneity—and obtain qualitatively similar estimates to those in Table 2 (Tables A9 and A10; Figure A4).

I then perform a series of alternative specifications to further corroborate the robustness of my results. First, I verify that the results are not sensitive to the choice of functional form. In particular, I replicate the analysis using both untransformed levels and log transformations. Results, reported in Appendix Tables A11, A12, remain both quantitatively and qualitatively very similar, confirming that the substantive conclusions are not driven by the use of the inverse hyperbolic sine transformation. In Table A13, I re-estimate the main model but include non-parametric trends in the log of population, municipal income and all the other municipal characteristics included in Table A14. This ensures that results are not driven by the differential effect over time of different pre-treatment municipalities’ characteristics. In Table A15, I estimate a version of Equation 1 fully saturated with cohort-specific municipality fixed effects and obtain nearly identical estimates. In Table A16, I include a set of 135 region-

times-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 A17 I show that results are robust to account for the presence of spatial correlation in the data, by re-estimating the model using [Conley \(1999\)](#) 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 Table A18). This specification compares treated and control municipalities of similar sizes (say, all those below 5,000 inhabitants), ensuring that results are not driven by potentially differential selection in treatment timing by population size. It also allows me to rule out the possibility that the effects are entirely driven by the minimum experience requirements introduced by the reform, which only apply to municipalities above 5,000 inhabitants. Moreover, I perform a heterogeneity split by geography and show that significant effects are present across the whole country (see Table A19).<sup>12</sup>

### 5.3.2 Disentangling the Reform from Concurrent Fiscal Policies

The reform was introduced in 2011 as part of a broader package of fiscal tightening measures (Law 148/2011), which included: 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. The most important one was the temporary inclusion of owner-occupied dwellings among houses subject to the property tax in 2012. I do not view these concurrent measures as competing explanations—independent and randomly appointed auditors likely provided the credible enforcement capacity necessary for the new fiscal rules to be implemented effectively, and without such enforcement it is unlikely that municipalities would have responded as strongly or uniformly to the national rules. Nonetheless, it is important to establish that the treatment effects I document reflect the specific role of the auditor reform.

Three features of the reform design and timing support this interpretation. First, even before the reform, municipalities regularly experienced auditor turnover at the end of six-year

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<sup>12</sup>While the reform improved fiscal performance throughout the country, the specific margins of adjustment differed across regions. In Northern municipalities, the adjustment primarily takes the form of higher net surpluses, consistent with greater fiscal space. In the Center, effects are positive but more muted, while in the South I find no significant increase in net surpluses but a sizeable rise in debt repayments. These differences suggest that municipalities adjusted along different margins: Northern municipalities through deficit reduction, and Southern municipalities by prioritizing debt servicing. Institutional features—such as the uneven distribution of municipality sizes across areas, baseline levels of debt repayment, and differences in access to EU structural funds—likely contributed to these heterogeneous responses.

terms. What changed with the reform was not that new auditors arrived, but the *mechanism* of appointment—the introduction of centralized and randomized assignment that reduced the scope for local political ties. The relevant discontinuity is therefore in the incentives and perceived independence associated with the position, not in auditor turnover per se. Second, while the broader omnibus law attracted substantial public attention, the specific provision changing auditor appointment received little political scrutiny, as public debate focused on major national fiscal adjustments. The arrival of a randomly assigned auditor is therefore unlikely to have served as a salient national signal triggering responses through media or public discourse. Third, while most concurrent fiscal provisions entered into force immediately and uniformly across all municipalities, the auditor reform was phased in gradually. This staggered implementation allows comparisons between municipalities equally exposed to the national context but differing in when they receive a randomly assigned auditor. Importantly, this equal exposure is ensured conditionally: as described in Section 4, the empirical strategy includes population-bin-by-year fixed effects that absorb any differential impact of the national reforms across municipality size categories, and treatment is identified from within-group variation in timing among municipalities facing the same regulatory environment. Consistent with this interpretation, treatment effects emerge throughout the treatment window and persist well beyond the peak of the crisis period (Figure A5), suggesting they reflect structural changes in monitoring behavior rather than short-term responses to national pressure.

Finally, the heterogeneity analysis discussed below provides further support. Treatment effects are larger in municipalities with weaker pre-reform fiscal conditions—consistent with greater scope for improvement—and in those matched to auditors with more experience or weaker local ties, despite the fact that assignment was random within each roster. These patterns are difficult to reconcile with a story attributing the observed changes solely to the tightening of national rules, and instead point to the specific role of auditors in shaping local fiscal outcomes.

### 5.3.3 Long-Run Persistence of Effects

A natural question to ask is whether the observed effects would persist in the longer run, or if instead they just represent a temporary improvement. On the one hand, we would expect that, by changing the selection and allocation of auditors, the removal of discretion of appointment should have a persistent impact on public finances. On the other hand, mayors might learn over time how to influence, and potentially capture, even randomly-assigned auditors. Policy events suggest the reform likely had long-lasting significance, as the randomized assignment system remained a political concern for mayors seven years after its implemen-

tation. In December 2019, during fiscal negotiations with the national government, mayors of large municipalities secured a partial reversal of the reform by reintroducing patronage appointments for the head of the auditors' committee. To move beyond anecdotal evidence, I now specifically investigate empirically whether the effects persist beyond the first randomly assigned auditor's term. While the features of my design do not naturally lend themselves to analyzing long-run outcomes, I provide suggestive evidence of the persistence of effects for 7 years after treatment, that is until the second round of randomly assigned auditors is completed. To do so, I extend the sample until 2019 and estimate the following dynamic specification:

$$Y_{mt} = \alpha_m + \delta_t + \sum_{k=-7}^{k=6} \gamma_k * D^k + X'_{mt}\zeta + \epsilon_{mt} \quad (3)$$

Given that all units are treated by 2015, the estimation of longer-run effects exploits already-treated units as controls, and rest on the following two additional assumptions, as discussed in ?: a) a limited influence of negative weights, and b) homogeneity of short-run effects across cohorts. As discussed in the previous section, I provide evidence in support of the first assumption in Table A9. As for the second assumption, in Table A21 I re-estimate the same model but allow for cohort-specific treatment effects. I perform an F-test and fail to reject the hypotheses that the treatment effects are the same across cohorts for all three outcomes of interest. Given these premises, the estimates are unlikely to be biased and can be interpreted as the long run effects of the reform. Overall, results reported in Figure A5 suggest that the treatment effects of the reform are remarkably stable and persistent over the long run, consistent with a long-lasting shift in budgetary practices induced by the improvement of monitoring effectiveness.<sup>13</sup>

## 5.4 Margins of Adjustment: Revenues vs. Spending

The results above show that, upon the arrival of randomly-assigned auditors, treated municipalities run higher net surpluses and reduce their outstanding debts, in accordance with the national government's objectives. How did municipalities manage to improve their fiscal stance? Intuitively, they could either increase their revenues (e.g. by raising taxes) or cut expenditures (e.g. cut investments and/or current expenditures), or a combination of both.

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<sup>13</sup>An alternative way of estimating long run effects could be to use municipalities in special regions, which are exempted from the initial application of the reform. However, these regions are also granted special autonomy in setting their own fiscal rules. Furthermore, they were allowed to choose whether or not to opt in into the reform, which lead to endogenous sorting into treatment which could be correlated with their trends of outcomes over time. For these reasons, they do not represent a viable control group and are excluded from the analysis.

In Table 3, 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 *CurrentExpenditures* and *CapitalExpenditures*. The results reveal that fiscal sustainability is not achieved through expenditure cuts. While current expenditures remain essentially unaffected, capital expenditures increase by over 4%, although this effect is only marginally statistically significant. The absence of an effect on current expenditures is plausibly explained by the fact that 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 observed increase in capital investment is a potentially desirable outcome.

Because investment spending was easier to compress than rigid current expenditures such as wages or interest payments, many municipalities had disproportionately relied on investment cuts in the pre-reform period to comply with fiscal rules (Chiades et al. 2016). Both the national government and the Court of Audit voiced concerns about this dynamic, arguing that the Pact had excessively penalized investment while leaving less efficient spending untouched. Against this background, it seems plausible that the introduction of randomly-assigned auditors shifted compliance strategies away from indiscriminate cuts to investment and toward more meaningful adjustments, such as improving tax collection or reducing arrears, in line with the preferences of central authorities. Thus, the positive impact on capital spending may reflect .

Consistent with this explanation, I provide evidence that the post-reform investment response is concentrated in treated municipalities subject to the Pact and with below-median pre-period investment. Results are reported in Columns (1)–(2) of Appendix Table A22. These results are in line with previous evidence by Chiades et al. (2016), who document that investment was compressed most strongly where the Pact applied. Thus, results suggest that randomly-assigned auditors facilitated a shift in the way municipalities complied with the Pact, away from indiscriminate investment cuts and toward previously compressed areas of spending. I also examine regional heterogeneity. Column (3) of Table A22 shows that the response is concentrated in the South, consistent with the availability of EU structural funds exempt from the Pact’s constraints. These resources likely enabled capital projects once alternative fiscal space was mobilized; accessing them required additional administrative effort and capacity, which auditors may have helped catalyze. In line with this explanation, Column (4) of Table A22 shows that treatment effects are substantially larger for municipalities that experienced improvements in auditors’ expertise as a result of the reform. This pattern is consistent with more experienced auditors guiding municipalities toward alternative compliance strategies that restored room for investment.

A potential further explanation for the observed increase in investment spending is that

it results from political bargaining between the auditor and the mayor. In exchange for significant improvements in fiscal sustainability, primarily through revenue enhancements, mayors may be permitted to increase investment spending. This spending can help justify the revenue consolidation efforts to local constituents. Notably, significant increases are observed in investment categories related to general administration, education, culture, and social welfare - areas directly observable and beneficial to local residents and thus likely to be politically relevant (see Table A23).

In the subsequent columns of Table 3, I investigate adjustments on the revenue side. Municipalities' main sources of current revenues are the property tax and the local income tax surcharge. Column (3) shows a significant increase in *PropertyTax* revenues, rising by over 20%, equivalent to an average increase of over 30 euros per capita. This analysis focuses on non-owner-occupied properties, as most owner-occupied dwellings were exempt from the property tax during most of the sample period. On the other hand, I fail to detect any significant change in 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 %.<sup>14</sup> Under Italian law, municipalities may contract new debt exclusively to finance capital investment or to refinance existing obligations.<sup>15</sup>

I examine the decomposition of this new borrowing by instrument and purpose in Appendix Table A24. The results show that the effect on *NewDebt* is driven entirely by long-term loans, while there is no increase short-term borrowing, the instrument most closely associated with opportunistic fiscal management. The additional long-term debt is directed toward increased principal repayments of past loans and new capital investment. Loans for investment increase significantly and the share of investment financed through new borrowing rises by roughly 3.3 percentage points. Taken together, these results are consistent with municipalities under more effective fiscal oversight borrowing more, but doing so to finance more investment and servicing existing obligations more assiduously.

Figure 3 displays results from event-study specifications for the outcomes in Table 3. While property tax revenues increase immediately with the auditor's arrival, the effect on new debt only appears initially and then dissipates in the last period. On the other hand, both capital revenues and expenditures show similar delayed response patterns from period two onwards, indicating that expanded investment activities require some time to occur.<sup>16</sup>

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<sup>14</sup>This result should be interpreted 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, leading to a slight overestimate in the magnitude of the effect.

<sup>15</sup>Refinancing can be undertaken only when doing so generates debt service savings (see Appendix A2 for further details). Such savings were particularly attainable after 2012, when ECB policy drove interest rates near zero while most outstanding municipal debt had been contracted at the substantially higher rates of the 1990s and early 2000s.

<sup>16</sup>As a robustness, in Figure A6, I also investigate the response of transfers from higher levels of government.

Overall, results show that, upon the arrival of a randomly assigned auditor, municipalities improve their fiscal stance through revenue-based adjustment, largely 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](#)).

How did municipalities manage to expand tax revenues? The observed expansion could be driven by either an increase in the property tax rate or an expansion of the tax base. I provide evidence of the drivers behind the expansion in tax revenue in Table 4. Column (1) replicates the results from Table 3, where I look at the total amount of property taxes assessed in a given year. 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 effects are slightly larger than in column (1), suggesting that the enlargement of the reported tax base is also associated with a slight improvement in collection effectiveness. In Column (7), I test explicitly for whether there is any change in the speed of tax collection, that is the ratio between collected and assessed property tax revenue ([Gagliarducci and Nannicini 2013](#)), and I detect a small but statistically significant improvement, consistent with a faster conversion of assessed taxes into collected revenues. Column (10) shows no significant change in the average property tax rate; however, column (12) reveals a significant negative interaction with the share of undeclared units: in municipalities with more non-compliant taxpayers, the property tax rate falls significantly following the reform. This is consistent with a base expansion large enough that municipalities can lower their rate while still raising total revenues. 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 had 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 \(2016\)](#).<sup>17</sup> Overall, the evidence points to an effect concentrated on the extensive margin of tax collection, namely the expansion of the reported property tax base through the inclusion of previously unregistered or misclassified properties. This is a dimension of enforcement that auditors can directly influence in the short run by verifying cadastral records and ensuring

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Previous research on Brazil ([Brollo and Troiano 2016](#), [Brollo 2008](#)) shows that audited municipalities who are found liable for misbehavior receive fewer transfers from the federal government. I find suggestive evidence of a very short-run mild decrease of transfers from the state, while transfers from other levels of government (Regions or the EU) are unaffected.

<sup>17</sup>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 sizable 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”.



that new properties are brought into the tax base. From this perspective, the presence of independent auditors is central: they provide the institutional mechanism through which stricter national rules translate into a broader and more accurately reported tax base. By contrast, improvements on the intensive margin—collecting more effectively from already registered taxpayers—are less likely to materialize quickly, as they typically involve legal procedures, reliance on external collection agencies, and multi-year enforcement lags. It is therefore unsurprising that I detect only small short-run effects on this margin.

To put this finding in context, it is also important to note that mayors have complete discretion over tax collection procedures. For instance, the responsibility of verifying if a property qualifies for the owner-occupied property tax exemption lies with the municipal police. 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. Consistent with an increased effort of mayors in increasing tax collection capacity, in the last three columns of the Table, I show that treated municipalities increase by 7% their investment spending for administrative functions, which include investments targeted towards expanding tax collection functions. Again, the treatment effects are sizably larger in municipalities with a higher share of non-declared units. Overall, the evidence suggests that, when facing more independent auditors that enforced tighter budget constraints, mayors have stronger incentives to fight property tax delinquents — a behavior they might have previously overlooked due to political or economic considerations.<sup>18</sup>

## 5.5 The Broader Consequences of the Reform

The results presented so far provide robust empirical evidence that the arrival of draft-assigned auditors improved monitoring effectiveness and spurred more fiscal stability, with municipalities running higher surpluses and achieving the adjustment through a sizable improvement in tax revenue and collection capacity. This raises the question of whether such a reform also affects the provision of public goods and the broader economy. Given the previous evidence showing no reduction in current spending and expansion of investment, it's reasonable to hypothesize that public goods provision should not suffer adverse effects and we might even expect improvements, albeit they might take time to materialize.

Turning to the broader impacts on the real economy, in principle, the direction of such

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<sup>18</sup>Law 148/2011, which introduced random auditor assignment, also temporarily expanded the property tax base by including owner-occupied dwellings among properties subject to the municipal property tax in 2012. The staggered timing of the auditor reform ensures this raises no identification concerns. It is nonetheless possible that independent auditors were themselves the mechanism through which the expanded rules translated into actual behavioral change — a point I discuss at length in the following section

effects is ambiguous. On the one hand, higher tax compliance could reduce disposable income and dampen local consumption, especially if the burden fell on households with high marginal propensities to consume. On the other hand, municipalities also responded by expanding capital revenues and public investment, which could have stimulated local activity via procurement, employment, and construction. The overall net impact on local economic performance is therefore an empirical question.

To shed light on the potential broader consequences of the reform, in Table 5 I look at a wide range of outcomes related to public good provision and economic activity. In columns 1 to 5, I look at a series of measures coming from the local public goods’ performance assessment survey that municipalities have to fill in alongside their balance sheet and report to the Ministry of the Interior. In columns 1 and 2, I start by looking at two metrics of basic local public good provision, the percentage of housing units with access to waste collection services and the extent of public lighting coverage across local streets. I then move to investigate the impact on one of the most important public services provided by local governments, public nurseries for kids between ages 0 and 3. Here, I look at three different metrics. First, I investigate whether there are changes in the extensive margin, i.e. in the presence of nurseries in the municipality. Then, I look at the acceptance rate and the teacher-pupil ratio. In column 6, I look at the number of sports facilities per 10,000 inhabitants. Across all these metrics, the findings indicate negligible and statistically insignificant impacts.

In the last three columns, I turn to investigate the consequences on the real economy. Here, I exploit data coming from the Ministry of Economics and Finance on the universe of tax declarations. In column 7, I look at total income per capita, a close proxy of GDP per capita. In column 8, I look at the total number of taxpayers. Finally, in column 9, I look at the log of the total population, which would capture any migration responses. Again, I find results that are both negligible in magnitude and statistically insignificant. This null result can be best understood in light of the design of the property tax and the specific channels through which the reform operated. Most of the additional revenue came from enforcing compliance on non-owner-occupied residential properties—second homes and rental units—where tax payments are relatively modest (with a median annual bill of around €200) and unlikely to induce large consumption responses. Compliance gains were concentrated at the extensive margin, bringing in previously undeclared properties, rather than substantially raising the burden on already-taxed households. Moreover, in municipalities with higher initial evasion, the broadened base allowed for lower average tax rates, further reducing the likelihood of contractionary effects. Finally, while higher revenues were partly channeled into greater public investment, the scale of these increases was not large enough to produce detectable impacts in aggregate income or employment statistics. Overall, these findings suggest that while the reform improved fiscal stability and revenue collection, it did not negatively

impact public good provision or the broader economy.

## 6 Mechanisms

By replacing discretionary auditor appointments—where mayors controlled who monitored them—with random assignment by an independent authority, the reform eliminates a potential source of capture while also removing mayors’ ability to select auditors on the basis of expertise or local knowledge. This creates a canonical trade-off (Alonso and Matouschek 2008, Mookherjee 2012): mayors might have used appointment discretion for good—selecting auditors with deeper expertise or local knowledge—or abused it to hire more lenient or corruptible ones. The average treatment effect estimated above reflects the net balance of these two forces. In this section, I examine each side in turn. I first provide evidence that, before the reform, discretion was systematically abused where local and national interests were most misaligned, and use post-reform random variation in auditor distance to identify the independence channel through which the reform operated (Section 6.1). I then examine the other side of the tradeoff: whether auditor expertise matters for monitoring effectiveness, and what this implies about the potential informational cost of removing appointment discretion (Section 6.2). Finally, I consider two additional channels: the reform’s impact as a function of mayoral electoral incentives, and deterrence effects on untreated neighboring municipalities (Section 6.3).

### 6.1 The Costs of Discretion

**Pre-Reform:** A useful insight from optimal delegation models (Alonso and Matouschek 2008, Mookherjee 2012) is that the costs of discretion increase with the agents’ misalignment 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 and thus more likely to abuse discretion in the pre-reform period. I adopt several proxies to identify places where lax or corrupt monitoring was more likely to occur before the reform. I start by investigating how the treatment effects vary with pre-reform financial health. Intuitively, lax or corrupt monitoring might have allowed for more irresponsible financial behavior. In Panel A of Table 6, I repeat the analysis of Table 2 but include an interaction term with an indicator that is equal to one for municipalities for which the dependent variable was below the median value in 2011. 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 municipalities without prior off-balance sheet debt repayments show a substantial and significant positive treatment effect, contrasting with a significant negative effect in those with previous debt repayments. The sign divergence explains the overall zero effect in Table 2 and suggests the presence of two different underlying phenomena. *OBSDebtRepayments* represent a self-declaration of an illicit spending act. Thus, auditors can act on two different margins. On the one hand, in municipalities with previously undisclosed liabilities, the appointment of an external auditor may lead to the recognition and reporting of hidden debts, thereby increasing recorded *OBSDebtRepayments*. In contrast, in municipalities that were already compliant with reporting requirements, the presence of an independent auditor may discourage the issuance of new off-balance-sheet obligations, reducing the occurrence of these transactions. As a result, the reform can simultaneously promote transparency by uncovering previously hidden liabilities and strengthen fiscal discipline by preventing the creation of new ones. While I cannot effectively separate the two mechanisms without having information about the unobserved amounts of illicit spending, the divergent signs of the effects suggest the presence of both phenomena across different contexts. 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 “dishonest” municipalities, the reform induces the reporting of illicit spending, thus leading to an increase in *OBSDebtRepayments*.

In Panels B and C of Table 6, I look at two other proxies of local misalignment and risk of auditors’ capture in the pre-reform period. I first use an indicator for whether any of the auditors appointed before the reform were born or resided in the municipality. Furthermore, using restricted-access data from the *Sistema D’Indagine Interforze* (SDI)<sup>19</sup> I construct an indicator variable flagging whether, in any given municipality, there was at least one investigation for corruption-related crimes (bribery, corruption, malfeasance, and embezzlement) before treatment. 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-

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<sup>19</sup>The SDI is a centralized investigation archive that contains reports of all individuals investigated by any of the Italian police forces, which include state police (*Polizia di Stato*), finance police (*Guardia di Finanza*), military police (*Carabinieri*), and environmental police (*Guardia Forestale*). The SDI data have been previously used in research by Decarolis et al. (2025). The data contains information, for each municipality, on the total number of investigations for all types of crimes in the years 2004-2013. See Decarolis et al. (2025) for further details on the data.

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 not uncover illicit practices. Table 6, Panels B and C display the results. Treatment effects are generally stronger for municipalities that had a local auditor or corruption investigations. For example, for *DebtRepayments*, the treatment effect is four times larger in corrupt municipalities (0.06 versus 0.24). Furthermore, I again detect diverging patterns when looking at *OBSDebtRepayments* similar to the one detected in panel A, suggesting a reduction in the actual size of off-balance sheet debts in non-corrupt places, and the emergence of a large amount of previously hidden OBS debts in corrupt places that are brought to the table by the arrival of randomly assigned auditors. Complementing these results, Appendix Table A25 documents systematic differences in the types of auditors appointed before the reform in municipalities with and without prior corruption investigations. In “corrupt” municipalities, auditors were significantly less likely to be female -a characteristic that has been associated in the literature with lower propensity for corrupt behavior (Brollo and Troiano 2016, Decarolis et al. 2023), more likely to be local residents, and more frequently had prior experience in financially troubled or irregular municipalities. They also tended to have lower overall professional experience. These patterns are consistent with the idea that discretion in the pre-reform system was strategically used to select more pliable or less stringent auditors, reinforcing the scope for political capture. By removing such discretion, the reform likely curtailed these opportunities and contributed to the stronger improvements observed in high-risk environments.

Taken together, the results up to now provide suggestive evidence of the presence of significant costs of discretion in the pre-reform period: in settings where local and national interests were most likely to diverge, the reform produced the strongest improvements. This lends direct empirical support to the theoretical prediction that the value of removing discretion is greatest precisely where the risk of capture is highest.

To provide further piece of evidence on the costs of pre-reform discretion, I exploit a natural source of within-municipality variation: because mayors serve 5-year terms while auditors serve 3-year renewable terms, there is exogenous variation in whether a sitting mayor appointed the current auditor. If mayors used discretion to appoint lenient auditors, fiscal performance should be worse in years when mayors controlled the appointment; if discretion was used productively, no difference should emerge. Results, reported in Table A20 and discussed in detail in Appendix A4, confirm the former: when mayors control appointments, municipalities run higher deficits and reduce debt repayments. The magnitude of the independent auditor effect in Table 2 is 2/4 times larger, suggesting that random assignment is a more effective disciplining mechanism than term misalignment alone.

**Post-Reform:** I next test more directly whether the reform operates by breaking local ties, using post-reform random variation in auditor-municipality distance. As shown in Table 1, one of the most critical observable changes induced by the reform is a substantial increase in the mean travel distance between auditors’ residence and the municipality of assignment, from around 20 minutes to over 60. Distance is a viable and commonly used proxy for social proximity (Xu et al. 2021; ?), but its implications for monitoring effectiveness are theoretically ambiguous. On the one hand, a local auditor is more likely to have connections with the mayors and to be more sensitive to local interests and thus be more willing to favor local spending vis-a-vis fiscal sustainability. On the other hand, closer auditors might have better local knowledge and face lower costs in acquiring information, which might make them more effective monitors. The evidence in Panel D of Table 6 resolves this ambiguity in favor of the capture channel. The estimated effect of the reform is substantially larger in municipalities that, through the random assignment mechanism, receive an auditor residing at least one hour away—that is, where auditor-municipality social ties are least likely to have pre-existed. This result speaks directly to the role of local embeddedness in shaping bureaucratic behavior: even under a system of random assignment, residual variation in distance generates meaningful differences in auditor independence, and those differences translate into measurably different fiscal outcomes.

Together, these results point to auditor independence as a central driver of monitoring effectiveness.<sup>20</sup>

## 6.2 The Value of Discretion: The Role of Auditor Expertise

Having established the potential costs of discretion, I now examine the other side of the tradeoff. By removing mayors’ ability to select auditors, the reform may also have forfeited the informational benefit of discretionary appointment—the ability to hire auditors whose professional expertise would make them more effective. This is particularly relevant given the advisory dimension of municipal auditors’ role, as discussed in Section 2: auditors are responsible not only for detecting irregularities but also for guiding local administrations through complex fiscal rules. More experienced auditors are likely to have a deeper understanding of the legal framework, greater familiarity with common implementation challenges, and more developed strategies for translating rules into actionable guidance. In a context where fiscal rules are complex and municipal administrations often face significant capacity constraints,

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<sup>20</sup>Further evidence in this direction comes from Appendix Table A18, which reports heterogeneity by municipality size. Municipalities above 5000 inhabitants—which, due to minimum experience requirements, could only be assigned auditors already active before the reform—also display significant treatment effects. Since the composition of the auditor pool is unchanged for these municipalities, their improvements cannot reflect a change in who is assigned but must instead reflect the severing of the social ties that had formed under the prior discretionary system.

auditors with stronger professional backgrounds may therefore act not only as enforcers of compliance but also as effective advisors. To assess whether this advisory dimension matters empirically, I exploit variation in auditor experience both before and after the reform.

**Pre-Reform:** I first examine whether the impact of the reform varies with the experience of auditors hired in the pre-reform period. Because municipalities had discretion in selecting auditors before the reform, some consistently relied on individuals with substantial prior experience while others did not. While this variation is admittedly endogenous, it provides suggestive evidence on the potential importance of advisory capacity. Using the comprehensive MEF appointment data, I construct for each municipality a measure of the average experience of its pre-reform auditors-defined as the total number of prior appointments accumulated by each auditor across all municipalities up to the year of appointment.

I first exploit the granularity of the experience dimension by estimating municipality-specific treatment effects using an imputation-based estimator developed in ?.<sup>21</sup> The results, presented in Figure A8, reveal a strong and consistent negative correlation between the estimated treatment effects and the degree of prior experience. Municipalities that had relied on more experienced auditors before the reform tend to display smaller treatment effects, consistent with the interpretation that the reform introduced less additional advisory capacity in those settings.<sup>22</sup>

I then confirm this pattern formally by augmenting the baseline stacked difference-in-differences specification with an interaction between the treatment indicator and standardized pre-reform experience. The results in Panel A of Table 7 broadly confirm the non-parametric pattern, as treatment effects are systematically smaller where pre-reform auditors were more experienced.

Because pre-reform experience is endogenous, this analysis is best interpreted as suggestive evidence rather than a clean identification of the informational cost of removing discretion.

**Post-Reform:** I then turn to exploit the reform-induced exogenous change in auditors' experience. I construct  $\Delta \text{exp}_i$  as the difference in average experience between the randomly assigned auditor and the last discretionally appointed one, and augment the baseline stacked difference-in-differences specification with an interaction between the treatment indicator and

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<sup>21</sup>Municipality-specific treatment effects are estimated using the imputation-based estimator of ? : a model for the untreated potential outcome is first estimated on clean control observations by regressing outcomes on municipality and year fixed effects as well as the standard set of controls included in my regressions; the estimates are then used to impute the counterfactual for each treated observation, and the unit-level treatment effect is recovered as the average post-treatment gap between actual and imputed outcomes. This approach yields a consistent estimate of the unit-level average treatment effect under parallel trends without imposing homogeneity across units or time.

<sup>22</sup>The one exception is *OBSDebtRepayments*, for which I do not detect a clear relationship with prior auditor experience. This is consistent with the absence of a significant average treatment effect for this outcome overall, suggesting that the advisory channel does not operate along this particular margin.

this variable. The results in Panel B of Table 7 show a positive and significant interaction coefficient: municipalities that received a more experienced auditor relative to their pre-reform baseline improved more. This pattern is inconsistent with a purely enforcement-based mechanism—if monitoring operated only through detection and sanctioning, the experience of the assigned auditor would likely play a limited role and points to a genuine advisory contribution of auditor expertise.

Taken together, these results clarify the information side of the tradeoff. Expertise genuinely matters: municipalities assigned more experienced auditors under random assignment improve their fiscal outcomes more. This implies that discretion had real potential informational value—if consistently used to select the most expert available monitors. At the same time, the features of the reform’s design limited the informational cost of removing it. In particular, the structured roster system with minimum experience requirements ensured that the most complex municipalities remained served by experienced auditors.

### 6.3 Additional Channels

**Electoral Accountability** A common justification for external auditing is to provide citizens with information they can use to hold local politicians accountable (Ferraz and Finan 2008, Pande 2011). Electoral incentives, however, may have an ambiguous effect on local fiscal behavior. On the one hand, mayors subject to strong re-election pressures have stronger incentives to perform well, refraining from inappropriate or wasteful financial practices. On the other hand, local interests may frequently clash with national ones, so local electoral accountability may push mayors to act in a different direction—particularly if the push toward fiscal sustainability from the central government imposes costs for local residents. To shed light on the interplay between external monitoring and electoral incentives, I perform two exercises.

First, I exploit the fact that, for historical reasons, municipalities are on different electoral cycles that do not overlap with audit cycles, as the electoral term of Italian mayors is five years while the auditor term is three years. This allows me to compare treatment effects in municipalities where the randomly assigned auditor arrives at the beginning of the electoral cycle against municipalities treated when mayors are closer to an election. Results are displayed in Panel A of Table A26. Treatment effects are significantly larger for municipalities treated at the beginning of the electoral term, when re-election concerns were weakest.

Second, I exploit the two-term limit for Italian mayors to test whether mayors who can be re-elected respond differently from those serving their final term. Results in Panel B of Table A26 are very similar: term-limited mayors are more responsive to the reform than their re-electable counterparts. This pattern suggests that fiscal adjustment was somewhat costly



for mayors and not necessarily aligned with the preferences of local constituents.

Taken together, both exercises point in the same direction: audits had a larger disciplining effect precisely where electoral accountability provided weaker incentives for fiscal discipline, suggesting that external monitoring substitutes for—rather than complements—the incentive effects of electoral competition.

**Deterrence and the Role of Spillovers** Finally, through the lens of the standard model of crime ([Becker and Stigler 1974](#)), the reform can improve outcomes directly through an improvement in the detection of lax financial practices, but also act indirectly through deterrence, inducing mayors to change their behavior in expectation of potentially facing a stricter auditor in the future. By design, my baseline estimates capture the effects of the reform, as all municipalities became aware of the future change in the appointment system of auditors at the moment of the reform adoption. Thus, a general deterrence effect cannot immediately be isolated and would likely lead to a downward bias in my baseline estimates of Table 2. However, we can hypothesize that the consequences of the change in auditors' appointment are not fully clear or salient to mayors before a nearby municipality is subject to the rule change, but they become more salient afterwards. Under this assumption, I can exploit the staggered introduction across municipalities and the geographic variation in the exact timing of the audits to isolate the role played by deterrence effects. Following [Colonnelli and Prem \(2022\)](#), I proceed in two steps. First, I identify the spillover effects of the reform on municipalities that are yet-to-be-treated but neighboring a treated municipality. I consider a municipality to be exposed to the spillover effects of the treatment 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. Results are reported in Table A27. As evident from Panel A, I detect sizable spillover effects, that are over 60% the size of the main effects, a magnitude that is similar to the one found by [Colonnelli and Prem \(2022\)](#) for Brazilian anti-corruption audits. Given the quantitative importance of spillover effects and deterrence, I then re-estimate my main equation in order to isolate the unattenuated direct effect of the treatment. To do so, in Panel B of Table A27, I exclude from the control group all not-yet-treated municipalities that neighbour 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 neither directly nor indirectly affected. The estimated effects of the reform in this context are almost twice as large as the average effects of the reform in Table 2, as one would expect given the size of the spillover effects detected in Panel A, thus highlighting that the results estimated in Table 2 represent a lower bound of the overall effects of the reform.

Taken together, these results suggest that the effects estimated in Section 5.2 represent a lower bound for the overall effects of the reform, and highlight the importance of taking

into account both direct and indirect effects when assessing the effectiveness of monitoring.

## 7 Discussion and Conclusion

In most organizations, crucial decisions are frequently delegated locally, giving rise to agency problems. While considered a crucial tool to align incentives, effective monitoring is frequently impaired by conflicts of interest, especially when the audited party itself is charged with appointing the auditor. Although this discretionary system may reduce information frictions and promote cooperation, it inherently introduces potential bias into the monitoring process. 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 random assignment of auditors—a reform that trades the risk of auditor capture against the potential informational value of discretionary appointment. Four findings arise. First, random matching severs pre-existing auditor-mayor connections that likely impaired monitoring effectiveness. Second, treated municipalities improve their financial health, in accordance with national government objectives. Third, the improvement is achieved through revenue-based consolidation rather than expenditure cuts, as treated municipalities show substantial improvements in their tax capacity, a key measure of organizational performance in governments. Fourth, treatment effects are significantly larger where the risk of auditor capture was highest before the reform, but also increasing in the relative expertise of the randomly assigned auditor—suggesting that both the independence and the informational value channels are operative.

Importantly, the effects documented in the data likely do not operate solely through the direct monitoring actions of auditors. The arrival of a newly randomly-assigned auditor may also act as a catalyst that triggers or facilitates a broader set of responses by the local government. In particular, the appointment of a new, randomly-assigned auditor may serve as a focal point or credible signal that oversight from the central state is intensifying, prompting local officials to accelerate compliance and enforcement efforts. In this sense, the effects of the reform may extend beyond the auditor’s direct actions and reflect how the appointment reshapes local political incentives, especially in a context in which municipal public finances are under heightened scrutiny.

Overall, the findings in this paper can inform the global debate about policies to curb misconduct and resolve conflicts of interest in monitoring, and they provide two important policy takeaways. The first pertains to the reform’s cost-effectiveness. Rather than introducing a new auditing system—such as federal audits in Brazil—or an additional supervisory

board—such as the PCAOB for U.S. audit firms—the reform I study operates by introducing frictions that make potential collusive agreements harder while leaving the broader features of institutional design unchanged. In this sense, the benefits come with very low implementation costs. The second takeaway relates to auditor identity. Unlike other reforms or experimental studies, the auditors in my setting are neither central government bureaucrats—as in the well-known cases of Brazil (Ferraz and Finan 2008) and Puerto Rico (Bobonis et al. 2016)—nor village members—as in Indonesia (Olken 2007). They are Certified Public Accountants contracted directly by municipalities to carry out auditing duties, for whom auditing governments represents only a limited part of their professional activities. This points to a potentially important role for external professionals in audit functions in other settings. A similar system is in place in the United States, where local governments are required to hire external auditors to conduct so-called “single audits,” an important process aimed at certifying the proper spending of federal awards (Tassin et al. 2019, Cuneo et al. 2023).

More generally, the results speak to a long-standing debate in accounting and finance around the challenges posed by conflicts of interest in private-sector auditing (Moore et al. 2006, Lennox and Wu 2018) and bank supervision (Agarwal et al. 2014, Fernández-Villaverde et al. 2013). While most research has focused on diagnosing and quantifying the problems of regulatory and auditor capture, little is known about potential solutions, as changes in the design of regulatory institutions are rare. This paper provides evidence that mandatory rotation policies, when combined with random assignment and a sufficiently large pool of potential monitors, can effectively limit conflicts of interest and improve monitoring outcomes.

The findings also invite reflection on the distributional dimension of fiscal oversight. Throughout the analysis, I adopt the perspective of the central government as the primary principal of the oversight system, and from this vantage point the reform is clearly beneficial: it strengthened fiscal sustainability by raising revenues and reducing deficits without crowding out local public services. Yet this framing conceals an underlying tension. Local citizens—who bear the cost of higher taxation without receiving commensurate gains in local expenditure or economic activity—may reasonably view the reform less favorably. This is precisely the logic of the soft budget constraint: when local voters internalize only their own municipality’s costs and benefits, they may favor policies that appear privately optimal but impose fiscal externalities on taxpayers elsewhere. The oversight mechanism—and the reform itself—is designed precisely to correct this distortion: by insulating auditors from local political pressure and reorienting their accountability toward national fiscal rules, the system internalizes the externality that decentralized governance would otherwise leave unaddressed. This asymmetry between local and aggregate welfare is therefore not incidental to the design of the system but intrinsic to it. The reform can thus be understood as a

deliberate reorientation of auditor incentives: away from monitors selected by mayors and attentive to local preferences, toward monitors randomly assigned and primarily accountable to national fiscal rules. Whether this reorientation constitutes a welfare improvement depends on whose welfare is being assessed. From the perspective of aggregate public finance and interregional fiscal equity, the answer is yes. From the perspective of local constituents, the calculus is more ambiguous-and that ambiguity is itself informative about the political economy of decentralized fiscal governance.

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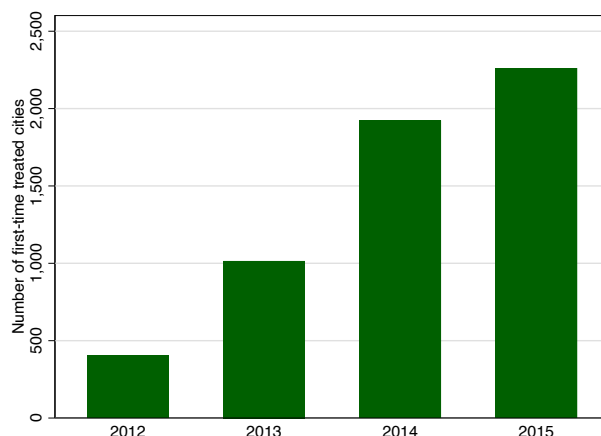
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# Tables and Figures

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).

Table 1

Reform-induced Changes in Auditors' Characteristics

|                                      | Pre Draft | Post Draft | Difference | Standardized Difference |
|--------------------------------------|-----------|------------|------------|-------------------------|
|                                      | (1)       | (2)        | (2)-(1)    | (3)/S.D(1)              |
| Female                               | 0.25      | 0.27       | 0.02 ***   | 0.046                   |
| Age                                  | 53.57     | 53.92      | 0.35 ***   | 0.041                   |
| Experience as auditor                | 19.22     | 14.99      | -4.23 ***  | -0.145                  |
| Experience as accountant             | 15.22     | 15.99      | 0.77 ***   | 0.086                   |
| Re-appointed                         | 0.63      | 0.01       | -0.62 ***  | -1.281                  |
| Local resident                       | 0.24      | 0.00       | -0.23 ***  | -0.542                  |
| Distance from Residence (in minutes) | 19.13     | 65.38      | 46.25 ***  | 1.466                   |
| Same gender of mayor                 | 0.70      | 0.67       | -0.03 ***  | -0.066                  |
| Same birthplace of mayor             | 0.15      | 0.02       | -0.12 ***  | -0.337                  |
| Same surname of local politician     | 0.06      | 0.01       | -0.05 ***  | -0.219                  |
| Observations                         | 12,557    | 10,602     |            |                         |

*Notes:* The table shows the average characteristics of appointed auditors, before and after the draft appointment was introduced. 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 2

The Effect of Random Auditor Assignment on Fiscal Sustainability

|                   | Net Surplus         |                     | Debt Repayments       |                       | OBS Debts Repayments |                      |
|-------------------|---------------------|---------------------|-----------------------|-----------------------|----------------------|----------------------|
|                   | (1)                 | (2)                 | (3)                   | (4)                   | (5)                  | (6)                  |
| Treated<br>× Post | 16.45***<br>[3.240] | 15.96***<br>[3.234] | 0.0885***<br>[0.0140] | 0.0874***<br>[0.0139] | -0.00218<br>[0.0253] | -0.00239<br>[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.

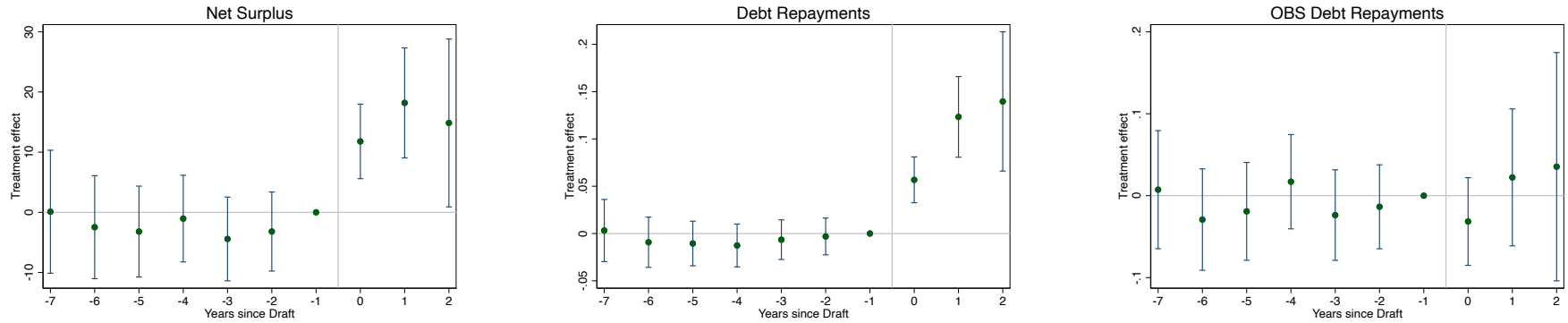
Table 3

The Effect of Random Auditor Assignment on Aggregate Spending and Revenue Choices

|                   | (1)                   | (2)                 | (3)                  | (4)                 | (5)                 | (6)                  |
|-------------------|-----------------------|---------------------|----------------------|---------------------|---------------------|----------------------|
|                   | Current Exp.          | Capital Exp.        | Property Tax         | Income Tax          | Capital Rev.        | New Debt             |
| Treated<br>× Post | 0.000184<br>[0.00309] | 0.0423*<br>[0.0254] | 0.205***<br>[0.0434] | 0.00758<br>[0.0254] | 0.0445*<br>[0.0266] | 0.225***<br>[0.0560] |
| Dep. Var Mean     | 875.0                 | 465.3               | 158.7                | 43.73               | 398.8               | 123.4                |
| Observations      | 114028                | 114028              | 99040                | 114028              | 114028              | 114028               |
| Adj. R-sq         | 0.941                 | 0.488               | 0.425                | 0.836               | 0.480               | 0.415                |

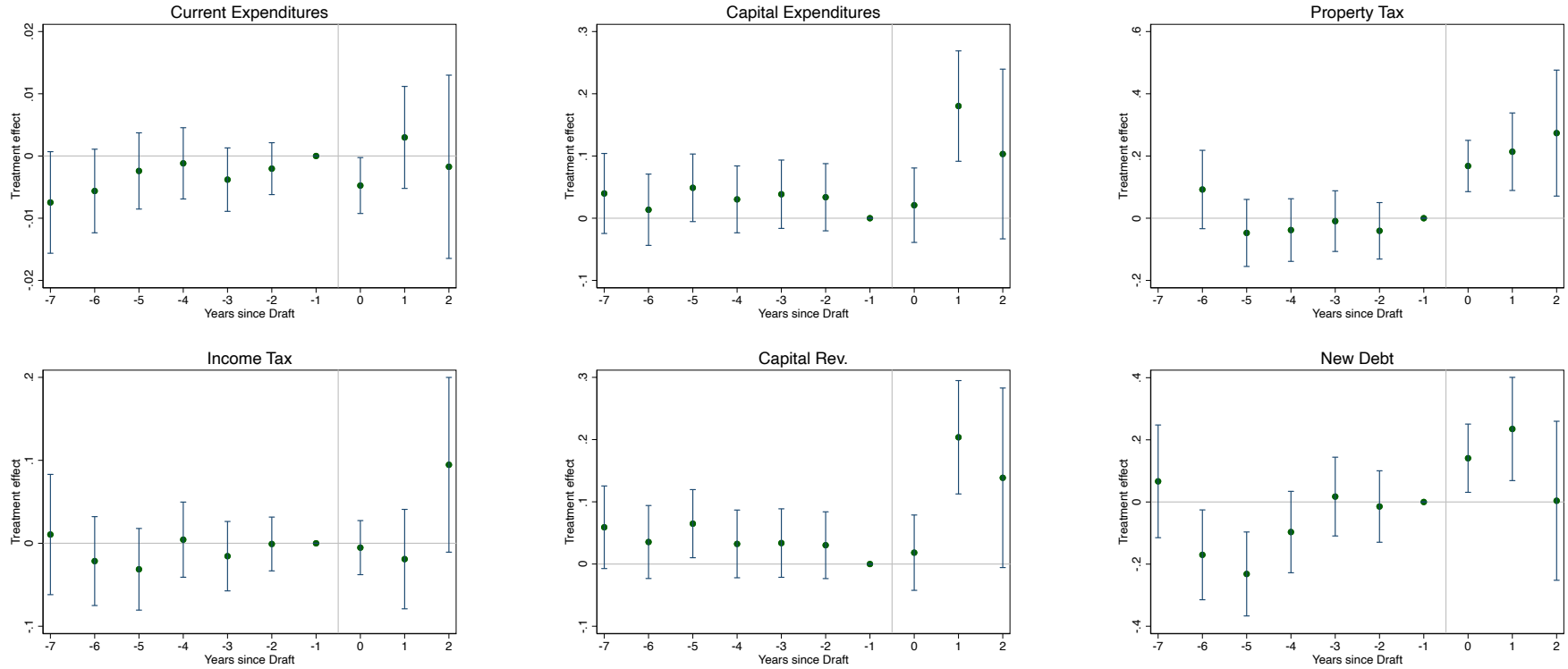
*Notes:* Significance: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms and transformed using the inverse hyperbolic sine transformation, but their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

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



*Notes:* The graphs report coefficients and 95% confidence intervals estimated according to specification 2. Standard errors are clustered at the municipality level. All dependent variables are in per capita terms, *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation. All regressions include municipality, population-bins-by-year fixed effects, relative time fixed effects and election cycle fixed effects, as well as the following controls: mayor's age at the beginning of the term (in logs), mayor's gender, term in office and a dummy equal to one if the mayor was born in the municipality.

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



*Notes:* The graphs report coefficients and confidence intervals estimated according to specification 2. Standard errors are clustered at the municipality level. All dependent variables are in per capita terms, and transformed using the inverse hyperbolic sine transformation. All regressions include municipality, 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 4  
The Effect of Random Auditor Assignment on Tax Capacity

|                       | Property Tax Revenue,<br>Assessed |                      |                      | Property Tax Revenue,<br>Collected |                      |                      | Speed of Property<br>Tax Collection |                     |                     | Property Tax Rate    |                      |                        | Investments for<br>Administrative Capacity |                     |                      |
|-----------------------|-----------------------------------|----------------------|----------------------|------------------------------------|----------------------|----------------------|-------------------------------------|---------------------|---------------------|----------------------|----------------------|------------------------|--------------------------------------------|---------------------|----------------------|
|                       | (1)                               | (2)                  | (3)                  | (4)                                | (5)                  | (6)                  | (7)                                 | (8)                 | (9)                 | (10)                 | (11)                 | (12)                   | (13)                                       | (14)                | (15)                 |
| Treated<br>× Post     | 0.202***<br>[0.0435]              | 0.188***<br>[0.0432] | 0.185***<br>[0.0430] | 0.229***<br>[0.0461]               | 0.213***<br>[0.0458] | 0.205***<br>[0.0453] | 1.397***<br>[0.525]                 | 1.388***<br>[0.520] | 1.359***<br>[0.518] | -0.00327<br>[0.0221] | -0.00253<br>[0.0221] | 0.00164<br>[0.0220]    | 0.0688*<br>[0.0372]                        | 0.0648*<br>[0.0376] | 0.0534<br>[0.0369]   |
| TreatXPostXGBI        |                                   | 0.121***<br>[0.0352] |                      |                                    | 0.144***<br>[0.0367] |                      |                                     | 0.109<br>[0.424]    |                     |                      | -0.00636<br>[0.0161] |                        |                                            | 0.0348<br>[0.0256]  |                      |
| TreatXPostXUndeclared |                                   |                      | 0.160***<br>[0.0337] |                                    |                      | 0.225***<br>[0.0362] |                                     |                     | 0.798<br>[0.520]    |                      |                      | -0.0454***<br>[0.0173] |                                            |                     | 0.142***<br>[0.0292] |
| Dep. Var Mean         | 158.9                             |                      |                      | 4.600                              |                      |                      | 80.20                               |                     |                     | 7.218                |                      |                        | 3.238                                      |                     |                      |
| Het. Var Mean         |                                   | 0.0268               | 0.0182               |                                    | 0.0268               | 0.0182               |                                     | 0.0262              | 0.0175              |                      | 0.0268               | 0.0182                 |                                            | 0.0268              | 0.0182               |
| Het. Var SD           |                                   | 0.0209               | 0.0137               |                                    | 0.0209               | 0.0137               |                                     | 0.0206              | 0.0132              |                      | 0.0209               | 0.0137                 |                                            | 0.0209              | 0.0137               |
| Observations          | 98401                             | 98401                | 98401                | 98401                              | 98401                | 98401                | 85042                               | 85042               | 85042               | 98401                | 98401                | 98401                  | 98401                                      | 98401               | 98401                |
| R-sq                  | 0.425                             | 0.425                | 0.425                | 0.479                              | 0.479                | 0.480                | 0.541                               | 0.541               | 0.541               | 0.868                | 0.868                | 0.868                  | 0.466                                      | 0.466               | 0.466                |

*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. All Lower-order interactions with heterogeneity dimension are also included but not shown. *Property Tax Revenue, Assessed* is the total amount of property taxes assessed in a given year, *Property Tax Revenue, Collected* is the total amount of property taxes collected in a given year, *Speed of Property Tax Collection* is the ratio between collected and assessed property tax revenues within the year; *Investments for Administrative Capacity* is the total amount of investment spending devoted to administrative affairs, which include investments for expanding the local tax office. All outcomes variables, except for *Speed of Property Tax Collection*, are transformed using the inverse hyperbolic sine transformation. *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 5  
The Effect of Random Auditor Assignment on Public Good Provision and the Real Economy

|                     | Housing Units w.<br>Waste Collection (%) | Public Lighting<br>Coverage | Any Public<br>Nursery | Nursery Acceptance<br>Rate | Teacher-Pupil<br>Ratio | Sports Facilities<br>per 10,000 Inhabitants | Total Income<br>per Capita | Total Number of<br>Taxpayers | Total<br>Population     |
|---------------------|------------------------------------------|-----------------------------|-----------------------|----------------------------|------------------------|---------------------------------------------|----------------------------|------------------------------|-------------------------|
|                     | (1)                                      | (2)                         | (3)                   | (4)                        | (5)                    | (6)                                         | (7)                        | (8)                          | (9)                     |
| Treated<br>× Post=1 | 0.00110<br>[0.00414]                     | -0.000788<br>[0.00316]      | -0.00566<br>[0.00449] | -0.00114<br>[0.00657]      | 0.00397<br>[0.00294]   | -0.0583<br>[0.128]                          | 0.00110<br>[0.000899]      | 0.000371<br>[0.000464]       | -0.000434<br>[0.000906] |
| Controls            | Yes                                      | Yes                         | Yes                   | Yes                        | Yes                    | Yes                                         | Yes                        | Yes                          | Yes                     |
| Pre-T. DV Mean      | 0.899                                    | 0.580                       | 0.277                 | 0.860                      | 0.199                  | 5.477                                       | 9.429                      | 0.708                        | 7.805                   |
| Pre-T. DV SD        | 0.207                                    | 0.308                       | 0.448                 | 0.202                      | 0.110                  | 9.712                                       | 0.267                      | 0.0681                       | 1.254                   |
| Observations        | 103914                                   | 109570                      | 114028                | 26452                      | 19262                  | 114028                                      | 114028                     | 114028                       | 114028                  |
| Adj. R-sq           | 0.636                                    | 0.897                       | 0.833                 | 0.737                      | 0.838                  | 0.786                                       | 0.987                      | 0.953                        | 1.000                   |

*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.

Table 6

## The Effect of Random Auditor Assignment on Fiscal Sustainability, the Role of Capture

|                                        | Net Surplus         | Debt Repayments       | OBS Debt Repayments   |
|----------------------------------------|---------------------|-----------------------|-----------------------|
| PANEL A: Fiscal Stance                 |                     |                       |                       |
|                                        | (1)                 | (2)                   | (3)                   |
| Treated × Post                         | 10.93***<br>[3.560] | 0.0354**<br>[0.0150]  | -0.177***<br>[0.0466] |
| Treated × Post × Below Median          | 12.49**<br>[5.612]  |                       |                       |
| Treated × Post × Below Median          |                     | 0.106***<br>[0.0237]  |                       |
| Treated × Post × Below Median          |                     |                       | 0.316***<br>[0.0461]  |
| Dep. Var Mean                          | -194.791            | 65.292                | 3.255                 |
| Sum of Coefs.                          | 23.425***           | .141***               | .139***               |
| Observations                           | 114028              | 114028                | 114028                |
| Adj. R-sq                              | 0.694               | 0.757                 | 0.413                 |
| PANEL B: Local Auditor (Pre-Reform)    |                     |                       |                       |
|                                        | (1)                 | (2)                   | (3)                   |
| Treated × Post                         | 15.32***<br>[3.992] | 0.0406***<br>[0.0148] | -0.00662<br>[0.0267]  |
| Treated × Post × Local Auditor=1       | -6.551<br>[6.296]   | 0.155***<br>[0.0355]  | 0.000574<br>[0.0659]  |
| Dep. Var Mean                          | -193.271            | 64.969                | 3.295                 |
| Sum of Coefs.                          | 8.764*              | .195***               | -.006                 |
| Observations                           | 106590              | 106590                | 106590                |
| Adj. R-sq                              | 0.703               | 0.759                 | 0.415                 |
| PANEL C: Corruption Investigations     |                     |                       |                       |
|                                        | (1)                 | (2)                   | (3)                   |
| Treated × Post                         | 15.98***<br>[3.481] | 0.0640***<br>[0.0137] | -0.0323<br>[0.0249]   |
| Treated × Post × Corrupt               | -3.725<br>[8.434]   | 0.181***<br>[0.0551]  | 0.280***<br>[0.102]   |
| Dep. Var Mean                          | -194.791            | 65.292                | 3.255                 |
| Sum of Coefs.                          | 12.254              | .245***               | .248**                |
| Observations                           | 114028              | 114028                | 114028                |
| Adj. R-sq                              | 0.700               | 0.758                 | 0.413                 |
| PANEL D: Distant Auditor (Post-Reform) |                     |                       |                       |
|                                        | (1)                 | (2)                   | (3)                   |
| Treated × Post                         | 7.131<br>[5.371]    | 0.00538<br>[0.0191]   | -0.00807<br>[0.0331]  |
| Treat × Post × At least 1 hr           | 18.85***<br>[6.240] | 0.0434**<br>[0.0207]  | -0.0134<br>[0.0358]   |
| Dep. Var Mean                          | -224.226            | 70.303                | 2.540                 |
| Sum of Coefs.                          | 25.981***           | .049***               | -.021                 |
| Observations                           | 82041               | 82041                 | 82041                 |
| Adj. R-sq                              | 0.685               | 0.789                 | 0.323                 |

*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. For off-balance-sheet debt repayments (*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. *LocalAuditor* is equal to 1 if any of the municipality's appointed auditors before the reform were 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. 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. All lower-order interactions with heterogeneity dimension are also included but not shown.

Table 7

The Effect of Random Auditor Assignment on Fiscal Sustainability, the Role of Experience

|                                                | Net Surplus         | Debt Repayments        | OBS Debt Repayments  |
|------------------------------------------------|---------------------|------------------------|----------------------|
| PANEL A - Pre-Reform Experience:               |                     |                        |                      |
|                                                | (1)                 | (2)                    | (3)                  |
| Treated $\times$ Post                          | 14.45***<br>[3.249] | 0.0763***<br>[0.0136]  | -0.00215<br>[0.0250] |
| Treated $\times$ Post $\times$ Past Experience | -2.224<br>[3.250]   | -0.0388***<br>[0.0117] | 0.00239<br>[0.0192]  |
| Dep. Var Mean                                  | -194.791            | 65.292                 | 3.255                |
| Sum of Coefs.                                  | 12.224***           | .038**                 | 0                    |
| Observations                                   | 114028              | 114028                 | 114028               |
| Adj. R-sq                                      | 0.701               | 0.759                  | 0.413                |
| PANEL B - Post-Reform Experience               |                     |                        |                      |
|                                                | (1)                 | (2)                    | (3)                  |
| Treated $\times$ Post                          | 15.42***<br>[3.534] | 0.0644***<br>[0.0140]  | -0.0238<br>[0.0254]  |
| Treat $\times$ Post $\times$ Delta Experience  | 10.16***<br>[2.633] | 0.0669***<br>[0.00984] | -0.00501<br>[0.0131] |
| Dep. Var Mean                                  | -194.797            | 65.289                 | 3.255                |
| Sum of Coefs.                                  | 25.578***           | .131***                | -.029                |
| Observations                                   | 113424              | 113424                 | 113424               |
| Adj. R-sq                                      | 0.700               | 0.760                  | 0.410                |

Notes: Significance: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Standard errors clustered at the municipality level in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation. *PastExperience* is the standardized average pre-reform auditor experience in the municipality; *DeltaExperience* is the difference in average experience between the randomly assigned post-reform auditor and the last discretionarily appointed pre-reform auditor. All regressions include municipality, year, 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. All lower-order interactions with heterogeneity dimension are also included but not shown.



# Appendix For Online Publication

## A1 Additional Tables and Figures

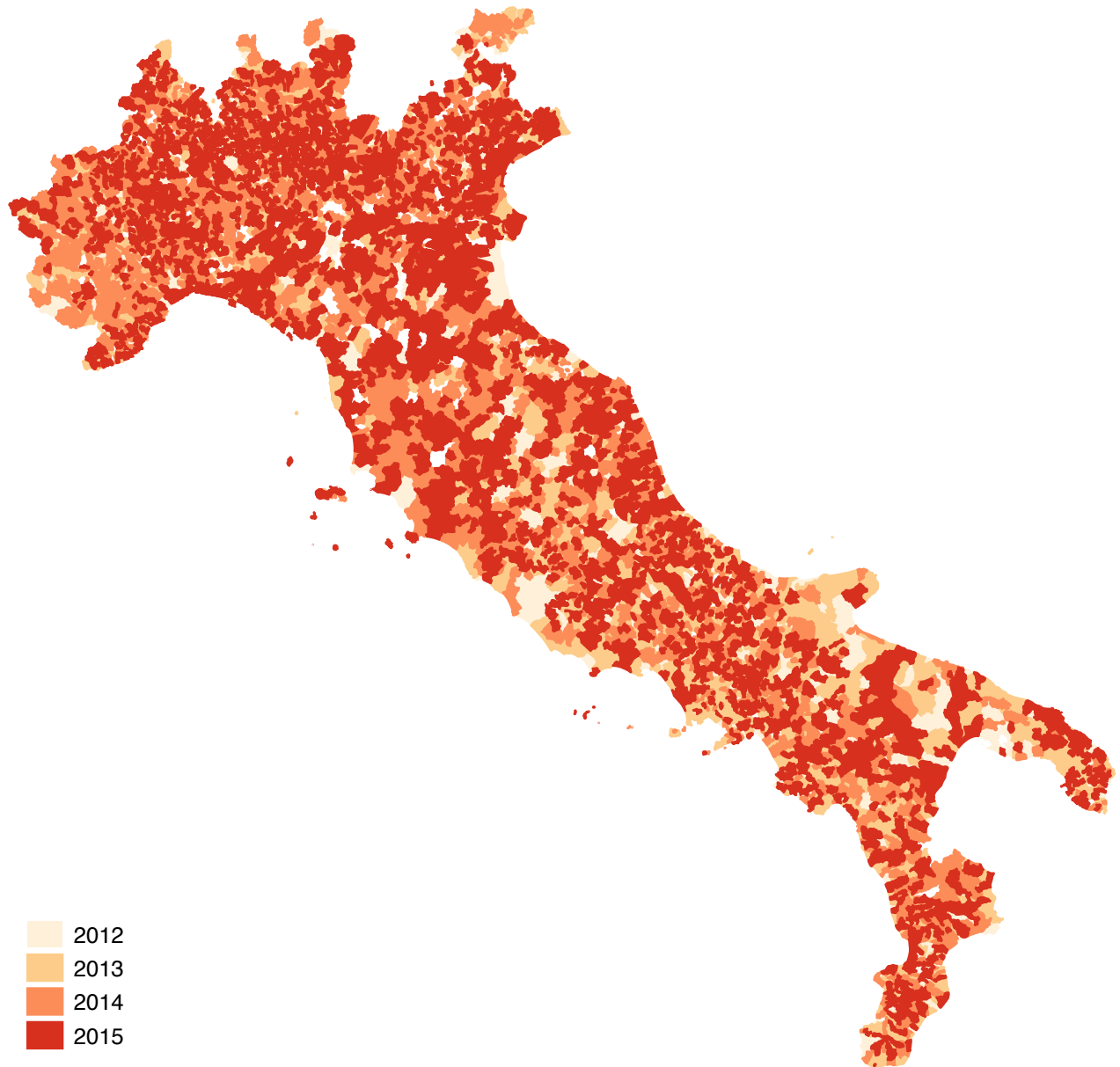
Table A1

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

| Advising                                                                              | Monitoring                                                                |
|---------------------------------------------------------------------------------------|---------------------------------------------------------------------------|
| <b>Mandatory written opinions on:</b>                                                 | <b>Audit and Certification of:</b>                                        |
| 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       |                                                                           |
|                                                                                       | <b>Verification of:</b>                                                   |
| 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 \(2009\)](#).

Figure A1  
Staggered Treatment, Geographic Variation



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

Table A2  
Summary Statistics for the Main Outcomes in the Pre-Reform Period

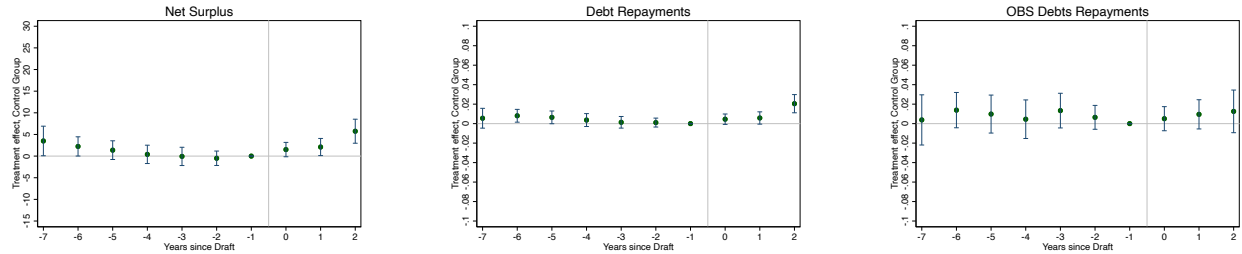
| A. Fiscal Sustainability |         |        |         |         |         |        |
|--------------------------|---------|--------|---------|---------|---------|--------|
|                          | Mean    | S.D.   | Median  | p10     | p90     | p99    |
| Net Surplus              | -306.97 | 204.81 | -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 Expenditures            | 886.77 | 401.74 | 770.17 | 566.10 | 1,312.69 | 2,804.00 |
| Capital Expenditures            | 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.

Figure A2  
The Dynamic Effect of Random Auditor Assignment on Fiscal Sustainability, Anticipation Effects in the Control Group



*Notes:* The graphs report the  $\beta_k$  coefficients and 95% confidence intervals estimated according to Equation 2. 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.

Table A3  
Predictors of Drafted Auditors' Rejecting Appointment

|                                  | Auditor Rejects Appointment |                          |                          |                          |                          |
|----------------------------------|-----------------------------|--------------------------|--------------------------|--------------------------|--------------------------|
|                                  | (1)                         | (2)                      | (3)                      | (4)                      | (5)                      |
| Long Distance                    | 0.0664***<br>[0.0125]       | 0.0653***<br>[0.0124]    | 0.0600***<br>[0.0122]    | 0.0594***<br>[0.0120]    | 0.0600***<br>[0.0119]    |
| Female                           |                             | 0.0278**<br>[0.0126]     | 0.0278**<br>[0.0127]     | 0.0280**<br>[0.0127]     | 0.0279**<br>[0.0127]     |
| Age                              |                             | -0.00371<br>[0.00832]    | -0.00296<br>[0.00847]    | -0.00312<br>[0.00844]    | -0.00290<br>[0.00842]    |
| Age <sup>2</sup>                 |                             | 0.0000548<br>[0.0000717] | 0.0000478<br>[0.0000729] | 0.0000491<br>[0.0000725] | 0.0000472<br>[0.0000723] |
| New Entrant                      |                             | -0.0164<br>[0.0139]      | -0.0173<br>[0.0141]      | -0.0170<br>[0.0141]      | -0.0170<br>[0.0141]      |
| Experience                       |                             | -0.00138<br>[0.00775]    | -0.00199<br>[0.00773]    | -0.00186<br>[0.00775]    | -0.00191<br>[0.00774]    |
| Experience <sup>2</sup>          |                             | 0.0000879<br>[0.000255]  | 0.000106<br>[0.000251]   | 0.000103<br>[0.000251]   | 0.000103<br>[0.000251]   |
| Mountain                         |                             |                          | 0.0234<br>[0.0169]       | 0.0212<br>[0.0179]       | 0.0211<br>[0.0180]       |
| Sea                              |                             |                          | 0.0375<br>[0.0265]       | 0.0347<br>[0.0275]       | 0.0358<br>[0.0265]       |
| Province Capital                 |                             |                          | -0.0536<br>[0.0329]      | -0.0553<br>[0.0332]      | -0.0501<br>[0.0321]      |
| Net Surplus Below Median         |                             |                          |                          | -0.0116<br>[0.0126]      | -0.0122<br>[0.0126]      |
| Debt Repayments Below Median     |                             |                          |                          | -0.0154<br>[0.0111]      | -0.0153<br>[0.0111]      |
| OBS Debt Repayments Below Median |                             |                          |                          | 0.0107<br>[0.00989]      | 0.0107<br>[0.00975]      |
| Corruption                       |                             |                          |                          |                          | 0.0110<br>[0.0167]       |
| Mafia Infiltrations              |                             |                          |                          |                          | -0.0570**<br>[0.0264]    |
| Constant                         | 0.168***<br>[0.00636]       | 0.208<br>[0.182]         | 0.183<br>[0.187]         | 0.196<br>[0.182]         | 0.190<br>[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 A4

The Effect of Random Auditor Assignment on Fiscal Sustainability, by Occurrence of Auditors' Rejections

|                           | Net Surplus         |                   | Debt Repayments       |                      | OBS Debt Repayments |                    |
|---------------------------|---------------------|-------------------|-----------------------|----------------------|---------------------|--------------------|
|                           | (1)                 | (2)               | (3)                   | (4)                  | (5)                 | (6)                |
| Treated<br>× Post         | 17.20***<br>[3.526] | 14.30*<br>[8.081] | 0.0959***<br>[0.0160] | 0.0588**<br>[0.0269] | -0.0166<br>[0.0291] | 0.0500<br>[0.0506] |
| First-Drafted Ever Reject | No                  | Yes               | No                    | Yes                  | No                  | Yes                |
| Dep. Var Mean             | -192.6              | -204.9            | 64.62                 | 68.43                | 3.387               | 2.645              |
| Observations              | 93000               | 21006             | 93000                 | 21006                | 93000               | 21006              |
| Adj. R-sq                 | 0.706               | 0.677             | 0.750                 | 0.789                | 0.409               | 0.428              |

Notes: Significance: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation, but their mean is reported as non-transformed. 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 A5

The Effect of Random Auditor Assignment on Fiscal Sustainability, controlling for Auditors' Rejections

|                   | Net Surplus         | Debt Repayments       | OBS Debt Repayments |
|-------------------|---------------------|-----------------------|---------------------|
|                   | (1)                 | (2)                   | (3)                 |
| Treated<br>× Post | 18.85***<br>[3.861] | 0.0891***<br>[0.0171] | -0.0484<br>[0.0352] |
| Controls          | Yes                 | Yes                   | Yes                 |
| Dep. Var Mean     | -194.8              | 65.30                 | 3.255               |
| Observations      | 114006              | 114006                | 114006              |
| Adj. R-sq         | 0.700               | 0.757                 | 0.413               |

Notes: Significance: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation, but their mean is reported as non-transformed. 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 A6  
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 A7  
Comparison of Characteristics of Drafted Auditors vs. Pool of Eligible Auditors

|                          | DRAFTED | POOL  | $\beta$ | STANDARDIZED DIFFERENCE |
|--------------------------|---------|-------|---------|-------------------------|
|                          | (1)     | (2)   | (3)     | (4)                     |
| 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 the left-hand-side on a dummy equal to 1 if the individual has been drafted and *RegionXYearXPopulationSize* fixed effects, which represent the randomization strata. 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 variances.

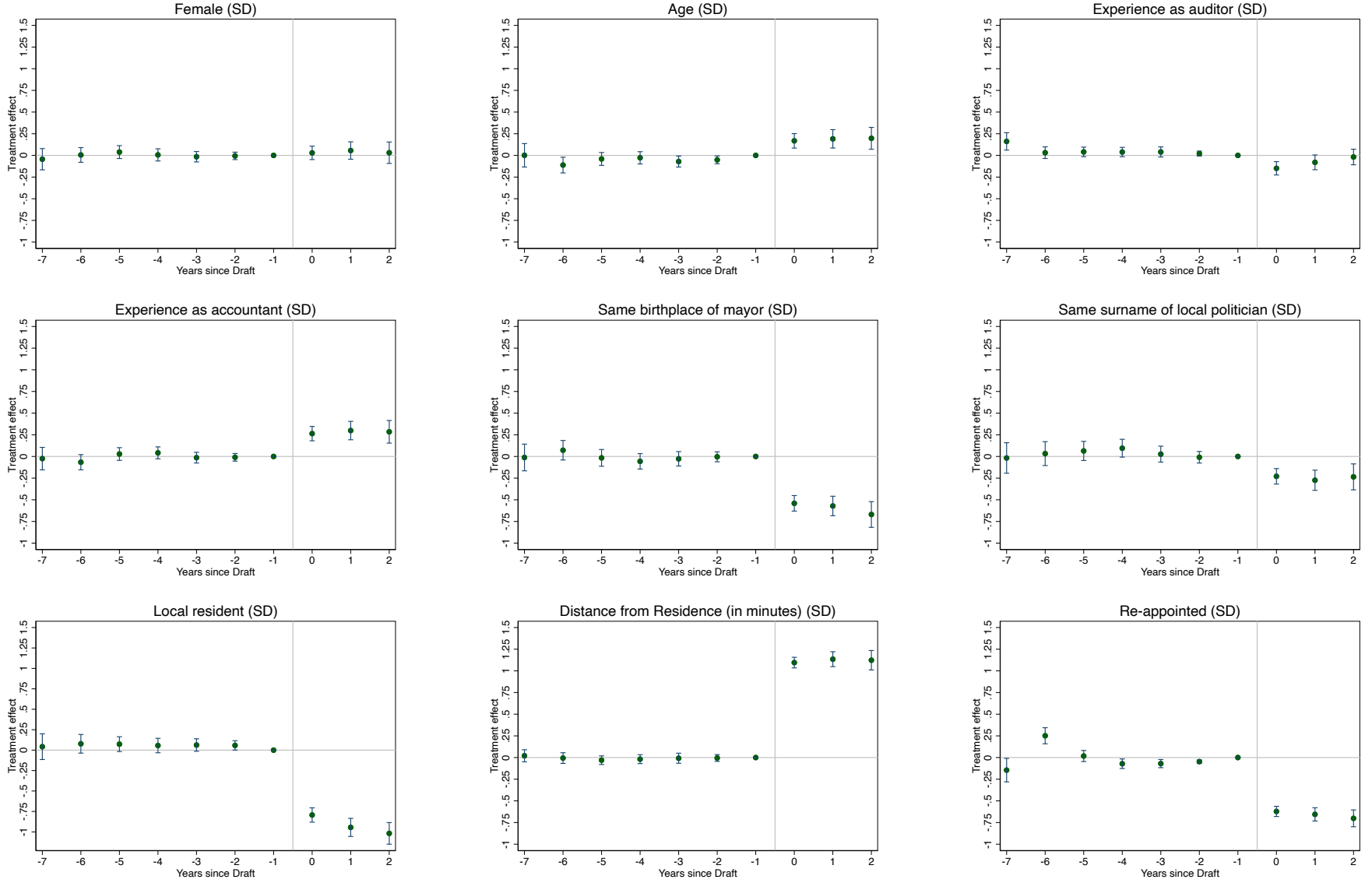
Table A8  
The Effect of Random Auditor Assignment on the Selection and Allocation of Auditors

|                     | (1)<br>Female      | (2)<br>Age           | (3)<br>Exp. as auditor | (4)<br>Exp. as accountant | (5)<br>Re-appointed  | (6)<br>Local resident | (7)<br>Distance      | (8)<br>Same gender | (9)<br>Same birthplace | (10)<br>Same surname  |
|---------------------|--------------------|----------------------|------------------------|---------------------------|----------------------|-----------------------|----------------------|--------------------|------------------------|-----------------------|
| treat=1<br>× post=1 | 0.0436<br>[0.0438] | 0.190***<br>[0.0452] | -0.145***<br>[0.0417]  | 0.268***<br>[0.0449]      | 1.640***<br>[0.0330] | -0.887***<br>[0.0486] | 1.288***<br>[0.0361] | 0.0204<br>[0.0432] | -0.542***<br>[0.0483]  | -0.341***<br>[0.0542] |
| Dep. Var Mean       | 0.266              | 53.21                | 20.97                  | 14.84                     | 0.345                | 0.208                 | 16.51                | 0.648              | 0.146                  | 0.0508                |
| Dep. Var SD         | 0.442              | 8.606                | 31.75                  | 9.024                     | 0.475                | 0.406                 | 17.07                | 0.478              | 0.353                  | 0.220                 |
| Observations        | 34521              | 30337                | 33186                  | 30337                     | 34521                | 30324                 | 26790                | 34521              | 34521                  | 34521                 |
| R-sq                | 0.793              | 0.742                | 0.805                  | 0.750                     | 0.880                | 0.785                 | 0.845                | 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. *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. The sample is restricted to the subset of municipalities for which I have information about auditors' characteristics for at least 1 term in the pre-period. All dependent variables are standardized. All regressions include municipality, 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 A3

The Effect of Random Auditor Assignment on the Selection and Allocation of Auditors, Event-Study Estimates



Notes: The graphs report coefficients and 95% confidence intervals estimated according to Equation 2. Standard errors are clustered at the municipality level. All dependent variables are standardized. 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 A9

The Effect of Random Auditor Assignment 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)                  |
| Treated                      | 18.04***<br>[3.385] | 15.07***<br>[3.174] | 0.0891***<br>[0.0135] | 0.0801***<br>[0.0135] | -0.00817<br>[0.0243] | -0.00791<br>[0.0245] |
| Dep. Var Mean                | -201.4              | -201.4              | 64.92                 | 64.92                 | 3.291                | 3.291                |
| Observations                 | 50427               | 50427               | 50427                 | 50427                 | 50427                | 50427                |
| 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               |
| $\sigma_{fe}$                |                     | 12.62               |                       | 0.06                  |                      | 0.009                |
| $\sigma_{fe}$                |                     | 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. 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

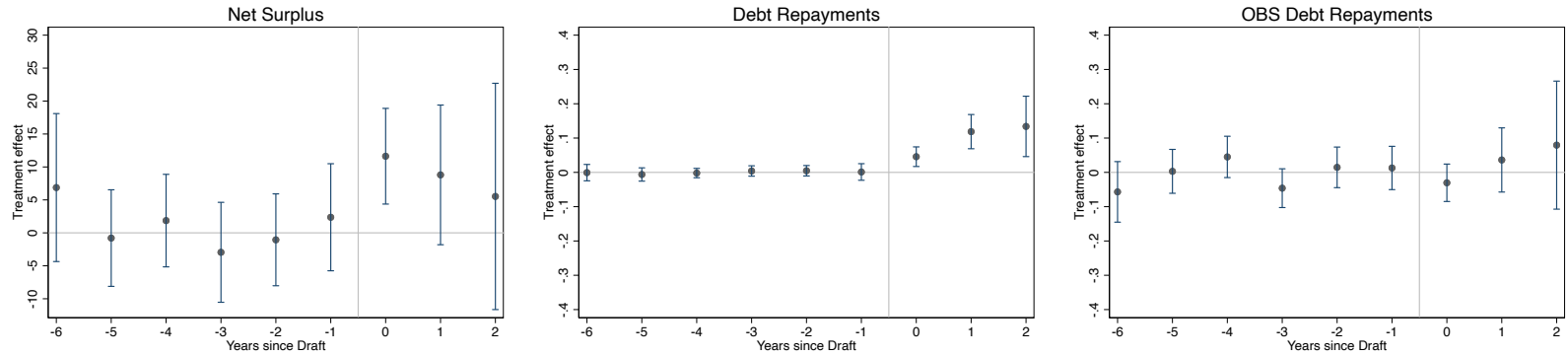
The Effect of Random Auditor Assignment on Fiscal Sustainability, Using the Estimator Developed by [Callaway and Sant'Anna \(2021\)](#)

|               | Net Surplus         |                    | Debt Repayments       |                       | OBS Debt Repayments |                      |
|---------------|---------------------|--------------------|-----------------------|-----------------------|---------------------|----------------------|
|               | (1)                 | (2)                | (3)                   | (4)                   | (5)                 | (6)                  |
| Treated       | 9.070***<br>[3.369] | 7.494**<br>[3.400] | 0.0695***<br>[0.0134] | 0.0587***<br>[0.0135] | -0.0111<br>[0.0300] | -0.00548<br>[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 the estimator developed by [Callaway and Sant'Anna \(2021\)](#). 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 A4

The Dynamic Effect of Random Auditor Assignment on Fiscal Sustainability, using [de Chaisemartin and D'Haultfoeuille \(2020\)](#) Methodology



*Notes:* The graphs report coefficients and confidence intervals of the  $DID_M$  estimators estimated according to [de Chaisemartin and D'Haultfoeuille \(2020\)](#) 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.

Table A11

The Effect of Random Auditor Assignment on Fiscal Sustainability, All Variables in Per Capita Terms

|                   | Net Surplus         |                     | Debt Repayments     |                     | OBS Debts Repayments |                   |
|-------------------|---------------------|---------------------|---------------------|---------------------|----------------------|-------------------|
|                   | (1)                 | (2)                 | (3)                 | (4)                 | (5)                  | (6)               |
| Treated<br>× Post | 16.45***<br>[3.240] | 15.96***<br>[3.234] | 8.402***<br>[1.550] | 8.310***<br>[1.543] | 0.0306<br>[0.295]    | 0.0316<br>[0.295] |
| Controls          | Yes                 | Yes                 | Yes                 | Yes                 | Yes                  | 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               | 0.677               | 0.678               | 0.261                | 0.261             |

*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, 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 A12

The Effect of Random Auditor Assignment on Fiscal Sustainability, Log-Transformed Only positive Outcomes

|                   | Debt Repayments       |                       | OBS Debts Repayments |                    |
|-------------------|-----------------------|-----------------------|----------------------|--------------------|
|                   | (1)                   | (2)                   | (3)                  | (4)                |
| Treated<br>× Post | 0.0860***<br>[0.0136] | 0.0849***<br>[0.0135] | 0.0735<br>[0.0589]   | 0.0708<br>[0.0588] |
| Controls          | Yes                   | Yes                   | Yes                  | Yes                |
| Dep. Var Mean     | 65.29                 | 65.29                 | 19.03                | 19.03              |
| Observations      | 114028                | 114028                | 19561                | 19561              |
| Adj. R-sq         | 0.760                 | 0.760                 | 0.522                | 0.523              |

*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, 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 A13

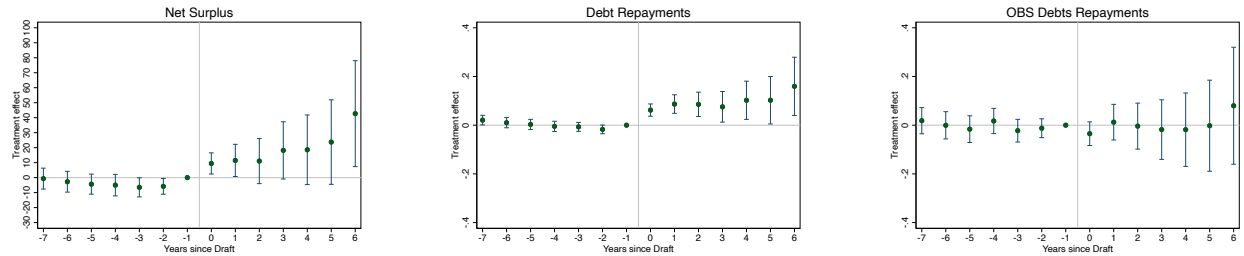
The Effect of Random Auditor Assignment on Fiscal Sustainability, including Non-parametric Trends by Pre-treatment Characteristics

|                   | Net Surplus         |                     | Debt Repayments       |                       | OBS Debt Repayments  |                     |
|-------------------|---------------------|---------------------|-----------------------|-----------------------|----------------------|---------------------|
|                   | (1)                 | (2)                 | (3)                   | (4)                   | (5)                  | (6)                 |
| Treated<br>× Post | 16.45***<br>[3.240] | 11.15***<br>[3.300] | 0.0885***<br>[0.0140] | 0.0609***<br>[0.0135] | -0.00218<br>[0.0253] | 0.00173<br>[0.0263] |
| 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               | 0.704               | 0.757                 | 0.764                 | 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. All regressions include municipality, population-bins-by-year fixed effects, relative time fixed effects 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 A5

The Dynamic Effect of Random Auditor Assignment on Fiscal Sustainability, Persistent Effects in the Long Run



*Notes:* The graphs report the  $\beta_k$  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.

Table A14  
Municipal Characteristics that Predict Treatment Timing.

|                                | 2012 COHORT            | 2013 COHORT           | 2014 COHORT           | 2015 COHORT          |
|--------------------------------|------------------------|-----------------------|-----------------------|----------------------|
|                                | (1)                    | (2)                   | (3)                   | (4)                  |
| Election Cycle                 | 0.00628**<br>[0.00311] | 0.000807<br>[0.00448] | -0.00827<br>[0.00522] | 0.00118<br>[0.00556] |
| 1-5k pop.                      | -0.00791<br>[0.00779]  | -0.0149<br>[0.0127]   | 0.0137<br>[0.0162]    | 0.00909<br>[0.0163]  |
| 5-10k pop.                     | 0.0174<br>[0.0111]     | -0.0160<br>[0.0156]   | -0.125***<br>[0.0187] | 0.124***<br>[0.0206] |
| 10-15k pop.                    | 0.0440***<br>[0.0154]  | 0.00723<br>[0.0198]   | -0.161***<br>[0.0222] | 0.109***<br>[0.0255] |
| 15-20k pop.                    | 0.115***<br>[0.0249]   | 0.0791***<br>[0.0287] | -0.195***<br>[0.0261] | 0.000487<br>[0.0322] |
| 20-60k pop.                    | 0.183***<br>[0.0589]   | 0.114*<br>[0.0621]    | -0.187***<br>[0.0516] | -0.110*<br>[0.0620]  |
| Above 250k pop.                | 0.366*<br>[0.188]      | -0.0267<br>[0.115]    | -0.379***<br>[0.0287] | 0.0395<br>[0.163]    |
| Mayor Age (log)                | 0.00537<br>[0.0158]    | 0.0317<br>[0.0257]    | 0.0196<br>[0.0312]    | -0.0567*<br>[0.0331] |
| Male Mayor                     | -0.00562<br>[0.0107]   | 0.0105<br>[0.0153]    | -0.0170<br>[0.0207]   | 0.0122<br>[0.0210]   |
| Local Mayor                    | 0.00316<br>[0.00839]   | 0.00739<br>[0.0122]   | -0.00289<br>[0.0146]  | -0.00766<br>[0.0154] |
| Mayor Term-limited             | -0.00966<br>[0.00686]  | -0.00564<br>[0.0105]  | 0.0286**<br>[0.0130]  | -0.0133<br>[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 A15

The Effect of Random Auditor Assignment on Fiscal Sustainability, including Cohort-by-Municipality Fixed Effects

|                   | Net Surplus         |                     | Debt Repayments       |                       | OBS Debt Repayments  |                      |
|-------------------|---------------------|---------------------|-----------------------|-----------------------|----------------------|----------------------|
|                   | (1)                 | (2)                 | (3)                   | (4)                   | (5)                  | (6)                  |
| Treated<br>× Post | 16.06***<br>[3.228] | 15.50***<br>[3.224] | 0.0860***<br>[0.0138] | 0.0848***<br>[0.0138] | -0.00281<br>[0.0253] | -0.00356<br>[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 cohort-by-municipality, population-bins-by-year fixed effects, relative time fixed effects 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.

Table A16

The Effect of Random Auditor Assignment on Fiscal Sustainability, allowing for Region-Specific Non-parametric Trends

|                   | Net Surplus         |                     | Debt Repayments       |                       | OBS Debt Repayments |                    |
|-------------------|---------------------|---------------------|-----------------------|-----------------------|---------------------|--------------------|
|                   | (1)                 | (2)                 | (3)                   | (4)                   | (5)                 | (6)                |
| Treated<br>× Post | 8.783***<br>[3.118] | 8.720***<br>[3.117] | 0.0395***<br>[0.0127] | 0.0392***<br>[0.0127] | 0.0108<br>[0.0251]  | 0.0108<br>[0.0251] |
| 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         | 0.714               | 0.714               | 0.777                 | 0.777                 | 0.416               | 0.416              |

*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. Regressions include municipality, population-bins-by-year fixed effects, region-times-year fixed effects, relative time fixed effects 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.

Table A17

The Effect of Random Auditor Assignment on Fiscal Sustainability, accounting for Spatial Correlation in the Data

|                   | Net Surplus         |                     | Debt Repayments       |                       | OBS Debt Repayments  |                      |
|-------------------|---------------------|---------------------|-----------------------|-----------------------|----------------------|----------------------|
|                   | (1)                 | (2)                 | (3)                   | (4)                   | (5)                  | (6)                  |
| Treated<br>× Post | 15.88***<br>[3.236] | 15.88***<br>[3.566] | 0.0872***<br>[0.0139] | 0.0872***<br>[0.0149] | -0.00293<br>[0.0253] | -0.00293<br>[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 \(1999\)](#) 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, 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 A18

The Effect of Random Auditor Assignment on Fiscal Sustainability, by Population Size

|                                      | Net Surplus         | Debt Repayments      | OBS Debt Repayments |
|--------------------------------------|---------------------|----------------------|---------------------|
|                                      | (1)                 | (2)                  | (3)                 |
| Treated=1 ×<br>Post=1                | 16.78***<br>[4.380] | 0.0328**<br>[0.0137] | -0.0113<br>[0.0226] |
| Treated=1 ×<br>Post=1 × Pop. 5-15 k. | -2.702<br>[5.916]   | 0.152***<br>[0.0387] | -0.0318<br>[0.0570] |
| Treated=1 ×<br>Post=1 × Above 15k    | -2.863<br>[7.993]   | 0.197***<br>[0.0542] | 0.143*<br>[0.0848]  |
| Dep. Var Mean                        | -194.8              | 65.29                | 3.255               |
| Observations                         | 114028              | 114028               | 114028              |
| R-sq                                 | 0.715               | 0.773                | 0.435               |

*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's initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality. All lower-order interactions with heterogeneity dimension are also included, but not shown.

Table A19

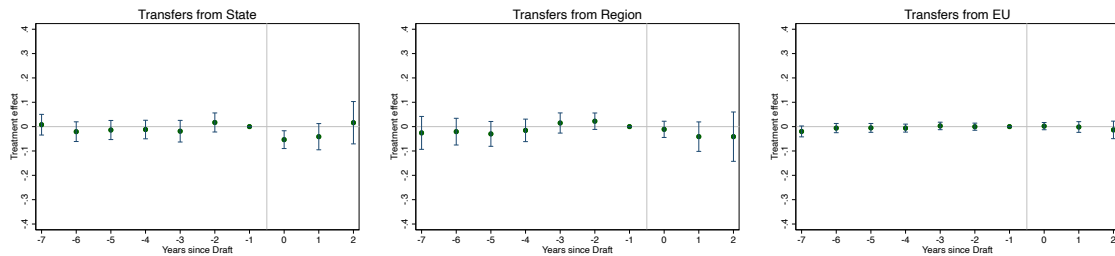
The Effect of Random Auditor Assignment on Fiscal Sustainability, by Geography

|                                | Net Surplus         | Debt Repayments      | OBS Debt Repayments |
|--------------------------------|---------------------|----------------------|---------------------|
|                                | (1)                 | (2)                  | (3)                 |
| Treated=1 ×<br>Post=1          | 14.54***<br>[3.881] | 0.0141<br>[0.0118]   | -0.0266<br>[0.0185] |
| Treated=1 ×<br>Post=1 × South  | -14.04**<br>[6.565] | 0.123***<br>[0.0293] | 0.0635<br>[0.0485]  |
| Treated=1 ×<br>Post=1 × Center | -1.849<br>[7.869]   | 0.0400<br>[0.0312]   | 0.0614<br>[0.0556]  |
| Dep. Var Mean                  | -194.8              | 65.29                | 3.255               |
| Observations                   | 114028              | 114028               | 114028              |
| R-sq                           | 0.719               | 0.779                | 0.435               |

*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. All lower-order interactions with heterogeneity dimension are also included, but not shown.

Figure A6

The Dynamic Effect of Random Auditor Assignment on Transfers from Other Levels of Government



*Notes:* The graphs report coefficients and confidence intervals estimated according to Equation 2. 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, 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.



Table A20

Discretionary Appointments and Fiscal Sustainability in Pre-Reform Period

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

*Notes:* Significance: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation. *Control of Appointment* is a dummy equal to 1 for years in which the mayor had appointed the auditor, and is constructed using exogenous variation arising from the difference between audit (3 years) and election (5 years) cycle. See Figure A9 and Appendix Figure A10 for further details. The sample is restricted to pre-treatment years. All regressions include municipality, population bins-times-year fixed effects and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor initial age (in log), gender, term in office and a dummy equal to one if the mayor was born in the municipality.

Table A21

The Effect of Random Auditor Assignment on Fiscal Sustainability, Cohort-Specific Treatment Effects

|                               | Net Surplus         |                     | Debt Repayments       |                       | OBS Debt Repayments |                     |
|-------------------------------|---------------------|---------------------|-----------------------|-----------------------|---------------------|---------------------|
|                               | (1)                 | (2)                 | (3)                   | (4)                   | (5)                 | (6)                 |
| Treated in 2012 $\times$ Post | 13.19*<br>[7.920]   | 13.08*<br>[7.907]   | 0.0215<br>[0.0185]    | 0.0201<br>[0.0185]    | 0.0968<br>[0.0665]  | 0.0954<br>[0.0666]  |
| Treated in 2013 $\times$ Post | 11.21**<br>[5.659]  | 10.84*<br>[5.672]   | 0.0601***<br>[0.0230] | 0.0602***<br>[0.0230] | 0.00628<br>[0.0396] | 0.00490<br>[0.0396] |
| Treated in 2014 $\times$ Post | 12.37***<br>[4.297] | 12.42***<br>[4.298] | 0.0409**<br>[0.0165]  | 0.0404**<br>[0.0165]  | -0.0467<br>[0.0322] | -0.0470<br>[0.0322] |
| Controls                      | No                  | Yes                 | No                    | Yes                   | No                  | Yes                 |
| Dep. Var Mean                 | -210.1              | -210.1              | 63.59                 | 63.59                 | 3.258               | 3.258               |
| F-Stat (T.E. Heterogeneity)   | 0.0237              | 0.0346              | 0.881                 | 0.950                 | 2.050               | 2.011               |
| P-Value                       | 0.977               | 0.966               | 0.414                 | 0.387                 | 0.129               | 0.134               |
| Observations                  | 103498              | 103498              | 103498                | 103498                | 103498              | 103498              |
| Adj. R-sq                     | 0.701               | 0.701               | 0.786                 | 0.786                 | 0.423               | 0.423               |

*Notes:* Significance: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. In this exercise, I restrict attention to the short run effect in  $t = 0$ , so as to maximize comparability across cohorts, as the cohort treated in 2014 is only observed as treated for one year, before municipalities in the control group, that is those treated in 2015, receive a randomly-assigned auditor. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation. 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 A22

The Effect of Random Auditor Assignment on Capital Investments, Heterogeneous Effects

|                                    | Capital Expenditures |                    |                        |                      |
|------------------------------------|----------------------|--------------------|------------------------|----------------------|
|                                    | (1)                  | (2)                | (3)                    | (4)                  |
| Treated<br>× Post                  | 0.00203<br>[0.0334]  | 0.0185<br>[0.0345] | -0.0919***<br>[0.0262] | 0.0243<br>[0.0253]   |
| Treated<br>× Post × Above 5k       | 0.129***<br>[0.0472] |                    |                        |                      |
| Treated<br>× Post × Below Median   |                      | 0.0469<br>[0.0444] |                        |                      |
| Treated<br>× Post × South          |                      |                    | 0.289***<br>[0.0531]   |                      |
| Treated<br>× Post × $\Delta_{exp}$ |                      |                    |                        | 0.0403**<br>[0.0197] |
| Dep. Var Mean                      | 465.329              | 465.329            | 465.329                | 465.329              |
| Sum of Coefs.                      | .131***              | .065**             | .197***                | .065**               |
| Observations                       | 114028               | 114028             | 114028                 | 114028               |
| R-sq                               | 0.514                | 0.514              | 0.520                  | 0.515                |

Notes: Significance: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. *DeltaExperience* is the difference in average experience between the randomly assigned post-reform auditor and the last discretionarily appointed pre-reform auditor. 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. All lower-order interactions with heterogeneity dimension are also included, but not shown.

Table A23

The Effect of Random Auditor Assignment on Investment, by Investment Category

|                   | Administrative<br>Capacity | Local<br>Police     | Education            | Culture              | Tourism             | Public<br>Transport | Local Public<br>Goods | Social<br>Welfare   |
|-------------------|----------------------------|---------------------|----------------------|----------------------|---------------------|---------------------|-----------------------|---------------------|
| Treated<br>× Post | 0.0745**<br>[0.0370]       | 0.00110<br>[0.0234] | 0.0860**<br>[0.0437] | 0.0716**<br>[0.0316] | -0.0196<br>[0.0263] | 0.0129<br>[0.0448]  | 0.0375<br>[0.0431]    | 0.0718*<br>[0.0390] |
| Dep. Var Mean     | 3.309                      | 0.920               | 2.068                | 0.762                | 0.399               | 4.039               | 3.818                 | 1.932               |
| Observations      | 114028                     | 99040               | 114028               | 114028               | 114028              | 114028              | 114028                | 114028              |
| R-sq              | 0.483                      | 0.361               | 0.342                | 0.365                | 0.431               | 0.385               | 0.452                 | 0.317               |

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. All regressions include municipality, year, 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 A24  
The Effect of Random Auditor Assignment on New Debt Issuances and Debt Repayments

|                   | Total New Borrowing  | Short-Term Borrowing | Bonds                 | Long-Term Loans      | Loans for Investments | Debt-Financed Investment Share | Principal (Long-Term Loans) | Interest Payments    |
|-------------------|----------------------|----------------------|-----------------------|----------------------|-----------------------|--------------------------------|-----------------------------|----------------------|
|                   | (1)                  | (2)                  | (3)                   | (4)                  | (5)                   | (6)                            | (7)                         | (8)                  |
| Treated<br>× Post | 0.225***<br>[0.0560] | 0.00719<br>[0.0505]  | 0.0117**<br>[0.00528] | 0.249***<br>[0.0545] | 0.0773**<br>[0.0367]  | 0.0331***<br>[0.00765]         | 0.101***<br>[0.0168]        | -0.00582<br>[0.0496] |
| Controls          | Yes                  | Yes                  | Yes                   | Yes                  | Yes                   | Yes                            | Yes                         | Yes                  |
| Dep. Var Mean     | 123.4                | 53.25                | 0                     | 211289.2             | 98765.8               | 0.150                          | 266450.7                    | 53.44                |
| Observations      | 114028               | 113429               | 113429                | 113429               | 98534                 | 109347                         | 113429                      | 114028               |
| Adj. R-sq         | 0.415                | 0.612                | 0.292                 | 0.283                | 0.302                 | 0.230                          | 0.707                       | 0.615                |

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

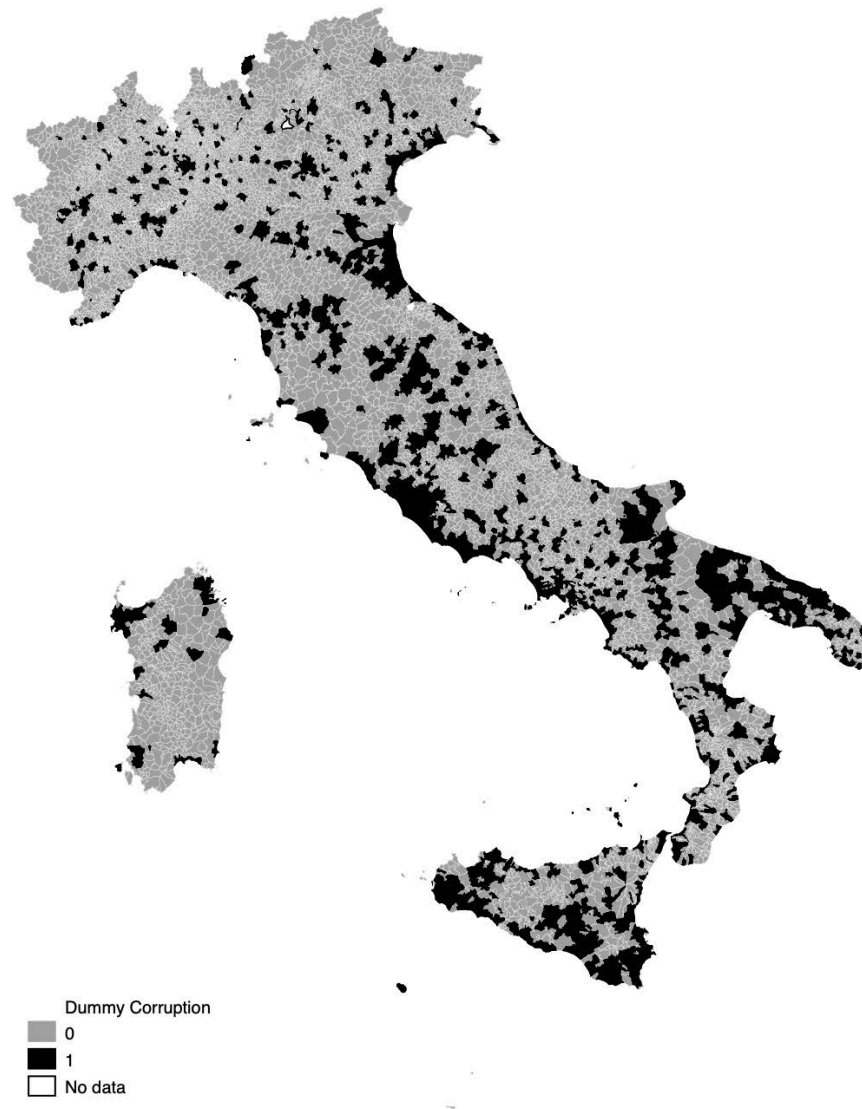
Table A25

Pre-Reform Auditors' Characteristics, Corrupt vs. Non-Corrupt Municipalities

|                                       | Corrupt | Non-Corrupt | Diff.      | Std Diff.  |
|---------------------------------------|---------|-------------|------------|------------|
|                                       | (1)     | (2)         | (1)-(2)    | (3)/S.D(1) |
| Female                                | 0.22    | 0.25        | -0.03 ***  | -0.073     |
| Age                                   | 56.90   | 55.84       | 1.05 ***   | 0.116      |
| Structural Def Muni                   | 0.16    | 0.08        | 0.08 ***   | 0.217      |
| Dissolved Muni                        | 0.16    | 0.04        | 0.11 ***   | 0.303      |
| Mafia Muni                            | 0.11    | 0.02        | 0.08 ***   | 0.261      |
| Local Auditor                         | 0.50    | 0.12        | 0.38 ***   | 0.760      |
| Avg Distance                          | 19.25   | 25.32       | -6.06 ***  | -0.207     |
| Tot N Munis                           | 2.98    | 5.36        | -2.37 ***  | -0.677     |
| Tot N Years Muni Auditor Appointments | 14.04   | 27.95       | -13.91 *** | -0.737     |

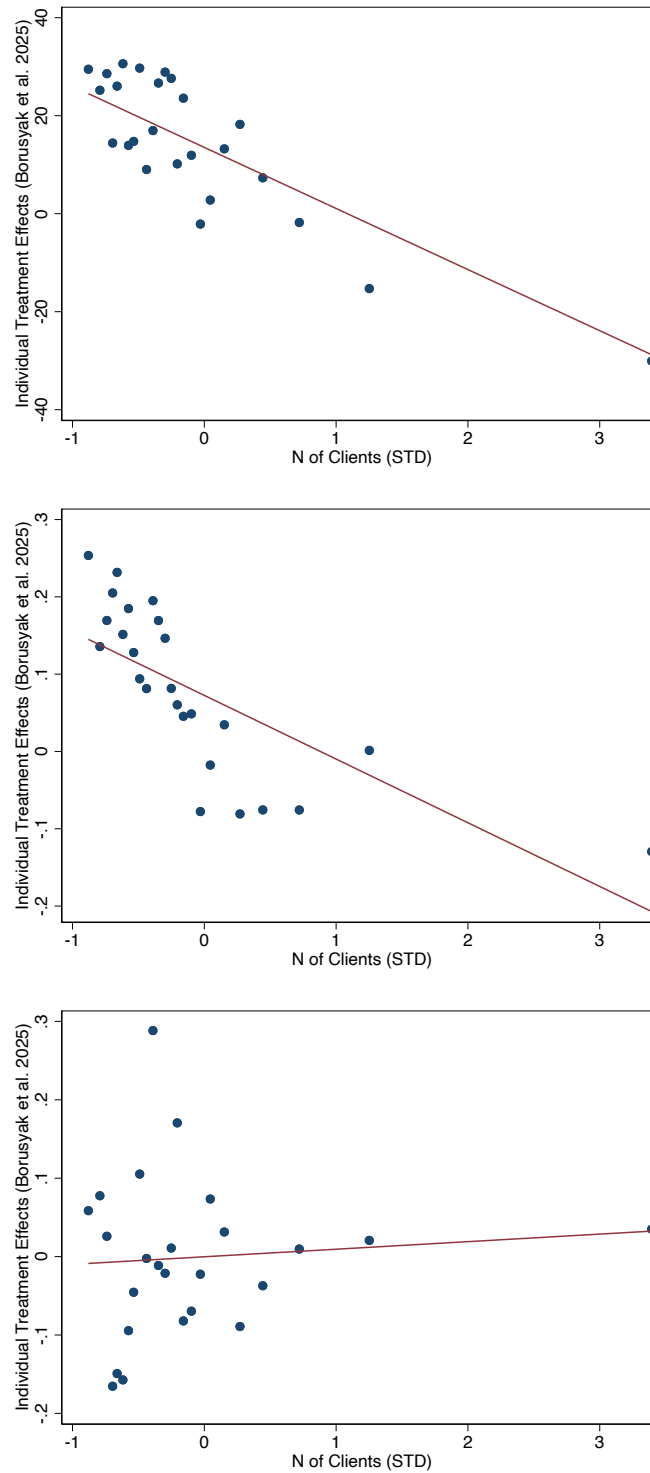
*Notes:* The table shows the average characteristics of appointed auditors, by size of the mayor's coalition in the pre-reform period. Column (3) (resp. (4)) shows the difference (standardized difference) in means between columns (1) and (2), as well as the significance level (p-values are calculated 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.

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  
Treatment Effect Heterogeneity by Pre-reform Auditors' Experience



*Notes:* The figure plots a binned scatterplot of individual treatment effects, estimated following ?, against the standardized number of municipalities previously served by the pre-reform auditor (*N of Clients*, STD), a measure of auditors' experience. Panels from top to bottom display results for *Net Surplus*, *Debt Repayments*, and *OBS Debt Repayments*, respectively. A negative slope indicates that municipalities previously served by more experienced auditors exhibit smaller treatment effects, consistent with those auditors already providing more effective monitoring prior to the reform.

Table A26

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 $\times$ Post                             | 15.71***<br>[3.861] | 0.127***<br>[0.0176]  | 0.0162<br>[0.0318]  |
| Treated $\times$ Post $\times$ Pre-Election Years | -2.350<br>[6.148]   | -0.106***<br>[0.0256] | -0.0426<br>[0.0475] |
| Dep. Var Mean                                     | -194.791            | 65.292                | 3.255               |
| Sum of Coefs.                                     | 13.36***            | .021                  | -.026               |
| Observations                                      | 114028              | 114028                | 114028              |
| Adj. R-sq                                         | 0.700               | 0.757                 | 0.413               |
| PANEL B: Term Limit                               |                     |                       |                     |
|                                                   | Net Surplus         | Debt Repayments       | OBS Debt Repayments |
|                                                   | (1)                 | (2)                   | (3)                 |
| Treated $\times$ Post                             | 19.83***<br>[5.494] | 0.130***<br>[0.0280]  | 0.0345<br>[0.0503]  |
| Treated $\times$ Post $\times$ Re-Electable       | -4.017<br>[6.197]   | -0.0517*<br>[0.0307]  | -0.0477<br>[0.0550] |
| Dep. Var Mean                                     | -194.791            | 65.292                | 3.255               |
| Sum of Coefs.                                     | 15.81***            | .078***               | -.013               |
| Observations                                      | 114028              | 114028                | 114028              |
| Adj. R-sq                                         | 0.700               | 0.757                 | 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. *Pre – ElectionYears* is equal to 1 if the mayor was in the last 2 years of her election cycle at the moment of treatment. *Term – Limited* is a dummy equal to 1 if the mayor in power at the moment of treatment was facing a term limit and could thus not run for re-election. All regressions include municipality, relative event-time fixed effects, election cycle fixed effects, population bins-times-year fixed effects, and the following controls: a dummy for dissolution of the city council, a dummy for mayor's resignation, mayor's initial age (in logs), gender, term in office and a dummy equal to one if the mayor was born in the municipality. All lower-order interactions with heterogeneity dimension are also included but not shown.



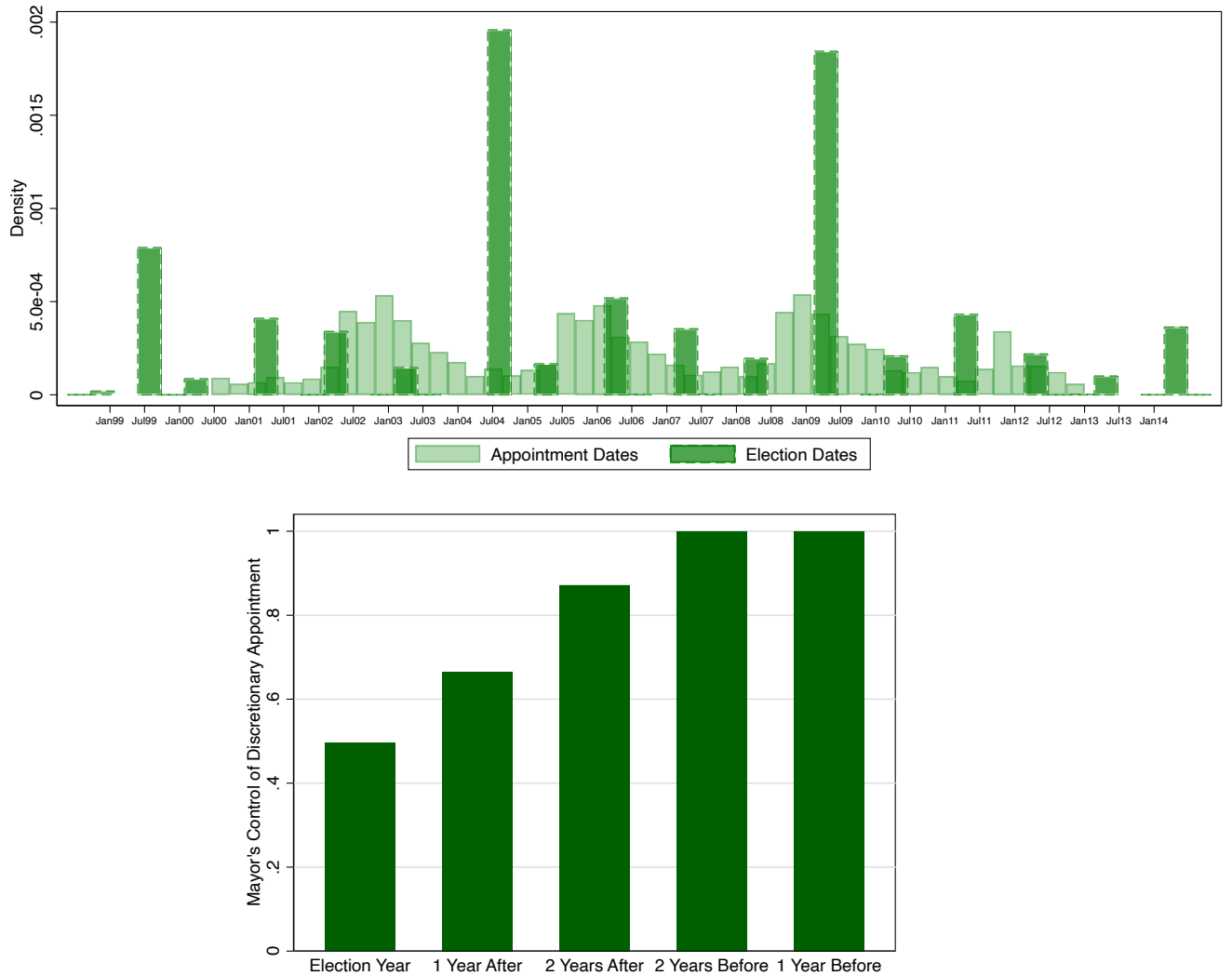
Table A27

The Effect of Random Auditor Assignment on Fiscal Sustainability, Deterrence and the Role of Spillovers

|                                           | Net Surplus |          | Debt Repayments |           | OBS Debt Repayments |          |
|-------------------------------------------|-------------|----------|-----------------|-----------|---------------------|----------|
| PANEL A: Estimating Spillover Effects     |             |          |                 |           |                     |          |
|                                           | (1)         | (2)      | (3)             | (4)       | (5)                 | (6)      |
| Treated                                   | 11.01***    | 10.34*** | 0.0653***       | 0.0631*** | -0.0208             | -0.0220  |
| Neighbor $\times$ Post                    | [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: Accounting for Spillover Effects |             |          |                 |           |                     |          |
|                                           | (1)         | (2)      | (3)             | (4)       | (5)                 | (6)      |
| Treated                                   | 30.94***    | 29.47*** | 0.155***        | 0.151***  | -0.00813            | -0.0110  |
| $\times$ Post                             | [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    |

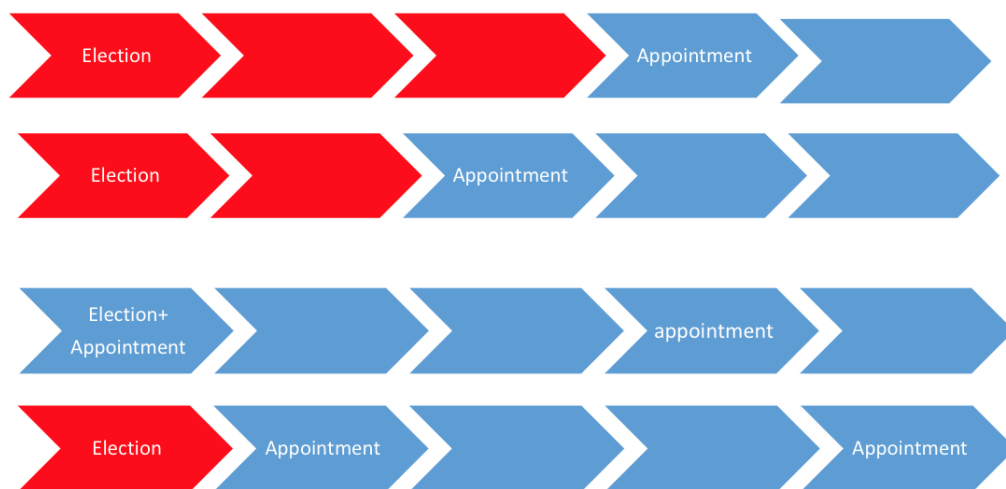
*Notes:* Significance: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Standard errors clustered at the municipality level are reported in parentheses. All dependent variables are in per capita terms. *DebtRepayments* and *OBSDebtRepayments* are transformed using the inverse hyperbolic sine transformation, but their non-transformed mean is reported at the bottom of the table. 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. *Neighbor* is a dummy equal to 1 if a municipality is not yet treated but shares a border with an already treated municipality. Panel A excludes treated municipalities from the sample. Panel B excludes neighboring municipalities from the sample.

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 who had control of the auditor's appointment, by the moment of the election cycle. This is the variation used for identification in Table A20. Municipalities are on different electoral cycles as well as auditors' cycles for historical reasons, unrelated to the reform.

Figure A10  
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.

## A2 Additional Institutional Details

### Municipal Finances

Municipalities manage around 8% of total public expenditure (over €55 billion) and have full control of a wide range of essential public services, including 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 permitted only to finance investment expenditures and is subject to strict quantitative limits.

Fiscal revenues come from two main sources: (1) local taxes, among which the most relevant is the property tax; and (2) local fees, such as building permits, traffic fines, and fees for other services. The mayor enjoys substantial discretion over tax collection, tax rates, and budget allocations, and is responsible for proposing the annual provisional and final budgets to the municipal council for approval by majority rule.

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. The central government also allows municipalities to undertake new debt to refinance existing debt or 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*).

Subnational debt accounts for approximately 11% of GDP. The Domestic Stability Pact, in force since 1999, requires municipalities to limit the growth of their fiscal gap below a given threshold ([Grembi et al. 2016](#)). During the economic crisis of the late 2000s and early 2010s, the central government progressively tightened these requirements, reducing the threshold and expanding the set of fiscal aggregates subject to the pact, in order to shift part of the EU-mandated fiscal consolidation onto local entities. These successive tightenings substantially increased the fiscal pressure on municipalities and raised the stakes of effective local fiscal monitoring.

## Appointment Procedure: Eligibility and Experience Requirements

Participation in the regional lists from which auditors are drawn is subject to experience requirements that vary with the population of the municipality being audited, so as to avoid assigning inexperienced auditors to larger, more complex entities. For municipalities with up to 4,999 inhabitants, candidates must have been certified public accountants for at least 2 years. For those between 5,000 and 14,999 inhabitants, candidates must have at least 5 years of certification and have held at least one prior municipal auditor appointment. For those above 15,000 inhabitants, candidates must have at least 10 years of certification and at least two prior appointments.

For each draw, the prefecture compiles a slate of  $3 \times N$  candidates, where  $N$  is the number of auditor seats to be filled. The municipality must appoint candidates in the order drawn. A candidate may be ineligible if she: (a) is or has been in the past two years a member of the municipal council or executive body; (b) is a local government employee; or (c) has already reached the maximum of eight concurrent appointments.

## Auditors' Compensation

Auditors' compensation is regulated nationally. A minimum floor covers expenses associated with performing the role, and maximum caps vary by population threshold. Mayors retain very limited discretion within this narrow bandwidth, resulting in minimal variation in actual compensation across municipalities of similar size. This aspect of the auditors' appointment was not affected by the 2011 reform.

## Statutory Mandate of Municipal Auditors

The dual monitoring-and-advising role of municipal auditors has been part of Italian law since the institution was established in 1990. The original statutory mandate (*Law 142/1990, Article 57*) reads 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 were progressively expanded between 1990 and the early 2000s (*art. 239, TUEL; d.p.r. n. 97/2003, attachment 17*) but were not affected by the 2011 reform. Table A1 in the main text provides a full enumeration of these tasks as codified in law.

## **The Oversight Process and the Role of the National Court of Audit**

Once the municipal council approves the final balance sheet, the auditor is required to compile a detailed questionnaire and transmit it, together with the approved balance sheet and the full audit report, to the National Court of Audit (*Corte dei Conti*). The Court is a Supreme Audit Institution that serves as the central oversight body for all levels of Italian government. It reviews the material submitted by auditors across all municipalities and can initiate judicial proceedings against municipal governments that fail to comply with applicable rules or engage in improper financial practices.

The auditors' questionnaire is a structured instrument designed to elicit systematic information about the municipality's financial management. Its scope covers the regularity of accounting practices, the accuracy of revenue and expenditure classifications, compliance with fiscal rules, the presence of off-balance-sheet liabilities, and the adequacy of internal controls. Crucially, the first question in the questionnaire asks the auditor to report whether any irregularity was identified during the review period and, if so, whether the municipality followed the auditor's recommendations in response. This design means that the auditor's independence is directly observable to the Court: an auditor who identifies no irregularities in a financially distressed municipality, or who fails to report persistent non-compliance, is likely to attract scrutiny. The questionnaire therefore creates a formal accountability link between the auditor's conduct and the Court's enforcement activity.

Non-compliance with auditors' recommendations is itself a trigger for heightened Court scrutiny. Municipalities that fail to act on auditor findings are flagged in subsequent reviews, and the Court's enforcement actions are graduated according to the severity and persistence of the non-compliance. On average, the Court issues approximately 2,000 judicial acts per year across Italian municipalities ([Corte dei Conti 2022](#)). The majority of these are initial warnings (*pronunce di accertamento*), which alert the municipal government to specific irregularities and require corrective action within 60 days. Formal judicial prosecutions are considerably rarer, averaging around 300 cases per year, indicating that most municipalities comply with the Court's initial demands. This graduated enforcement structure means that the Court's deterrent effect operates primarily through the threat of escalation rather than through frequent prosecution.

## The Italian Auditor Market

The auditing job in Italy is carried out by self-employed Certified Public Accountants (CPAs), unlike the United States where municipal and corporate audits are predominantly conducted by large private firms. Italian CPAs work simultaneously for municipal governments and for private clients, offering accounting and auditing services to individuals and small businesses. There are over 150,000 registered CPAs authorized to work as auditors in both the public and private sectors. Working as a municipal auditor provides visibility and is likely to help auditors expand their private client networks, making it an attractive career choice beyond its direct monetary compensation. This large and competitive pool of potential auditors is what makes a random drafting procedure operationally feasible. As common in markets for experts (Ronen 2010, White 2010), however, discretionary appointments under the pre-reform system likely concentrated appointments in a restricted set of “connected” auditors based on patronage networks.

## Special Status Regions

The reform does not apply to the five Special Status Regions (Valle d’Aosta, Trentino Alto Adige, Friuli Venezia Giulia, Sardinia, and Sicily), which are granted autonomy along many margins, including fiscal monitoring procedures. In 2016, Friuli Venezia Giulia, Sardinia, and Sicily adopted their own drafting procedures, but in ways that preserve greater local discretion than the national system. In Friuli, municipalities may choose from among the  $3 \times N$  drafted candidates. In Sicily, the drafting procedure was introduced following pressure from the Regional Council of Auditors, which had repeatedly denounced political interference with local auditors; however, the draw is carried out by the City Council itself, which exposes it to a higher risk of manipulation. These regional variants are excluded from the main analysis.

## A3 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 most severe one being mafia infiltration, but also for a failure to approve the budget. Council dismissals have proven to affect economic activity ([Fenizia and Saggio 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 results 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.



## A4 Alternative Design: Fiscal Performance with Discretionary Appointments

Even before the reform, mayors did not always control auditor appointments. Because mayors serve 5-year terms while auditors serve 3-year renewable terms, exogenous variation in mayoral control arises from the mismatch in mandate lengths (see Figure A9 and A10 for an illustration). I exploit this variation to test whether discretionary appointment power induced worse fiscal outcomes. Concretely, I estimate:

$$Y_{mt} = \alpha_m + \delta_t + \beta \text{Control}_{mt} + X'_{mt}\zeta + \epsilon_{mt} \quad (4)$$

where  $\text{Control}_{mt}$  is an indicator for whether the sitting mayor appointed the current auditor. Because the probability of controlling the appointment rises over the electoral term, I include a dummy for the two pre-election years among the controls  $X_{mt}$ , so that  $\beta$  is identified by comparing mayors at similar stages of the election cycle who differ in whether they appointed the current auditor for exogenous reasons.

Results are shown in Table A20. Odd columns include all mayors with a control for second-term status interacted with the pre-election dummy; even columns focus on first-term mayors. Across specifications, when mayors control appointments municipalities run higher deficits and reduce debt repayments, indicating that discretionary appointment power is associated with less fiscally responsible behavior. Comparing magnitudes, the positive effect on Net Surplus from an independent, draft-appointed auditor (Table 2) is roughly twice as large as the negative effect from discretionary control here, while the effect on Debt Repayments is over four times as large. This suggests that centralized random assignment is a more powerful disciplining mechanism than simple term misalignment between mayor and auditor.

Overall, these results reinforce the main findings: changes in control over auditor appointments have meaningful consequences for monitoring effectiveness and municipal fiscal performance. From a policy perspective, they also suggest that introducing a gap between the political and audit cycles—without full randomization—could be a lower-cost alternative for strengthening auditor independence.