The Value of Information for Regulatory Enforcement*

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Abstract

Regulatory enforcement often depends on information produced by agents who may have incentives to distort it. We study this tension in the context of the Italian Court of Auditors, where local public finance auditors report to national judges. We model this process as involving two frictions: imperfect detection of irregularities and incentives to underreport them. A reform that randomized auditor assignments provides a natural experiment that potentially increases the first friction (by reducing local knowledge) while reducing the second one (by increasing independence). Combining novel administrative data with a machine-learning-based measure of fiscal risk, we find that enforcement increased and became better targeted toward high-risk municipalities. We trace this improvement to the transmission channel: randomly assigned auditors report more irregularities, especially in high-risk municipalities and where pre-reform local ties were strongest. Experienced judges translate these improved signals into enforcement, concentrating deliberations on high-risk cases. The results suggest that in this setting, the benefits of auditor independence dominate the costs of reduced local knowledge.

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1 Introduction

Regulatory enforcement is a cornerstone of good governance, yet its effectiveness depends critically on two ingredients that are often in tension: the quality of information available to decision-makers and the incentives of those who produce it. Since Becker (1968), economists have recognized that enforcement outcomes depend on the probability and severity of sanctions; a large empirical literature has since documented how monitoring design shapes corruption, compliance, and public-sector performance (Olken, 2007; Ferraz and Finan, 2008; Avis et al., 2018; Bobonis et al., 2016). A parallel literature on hierarchical organizations emphasizes that information is often gathered by agents with private incentives to distort it before it reaches those who act on it (Aghion and Tirole, 1997; Dessein, 2002; Stein, 2002). Yet direct measurement of these internal communication frictions has been rare, because internal reports are seldom observable and their informational content is hard to score. This paper provides the first evidence on how reforming the design of information acquisition within an enforcement hierarchy—specifically, randomized assignment of front-line monitors—reshapes the quality, transmission, and ultimate use of information for enforcement.

We focus on the municipal audit system in Italy, where auditors review local government accounts and transmit reports to judges at the National Court of Auditors, who decide whether to initiate formal enforcement proceedings. We study a national reform that centralized and randomized the assignment of municipal auditors to local governments. By replacing mayoral appointment with random assignment, the reform tightened auditors' independence without altering the Court's formal decision rules. We show that this change increased judicial action—the probability that the Court opens a deliberation against a municipality—and improved the targeting of decisions toward high-risk municipalities. We trace these effects to an upgrade in the *judge-visible* signal produced by auditors.

Our analysis is guided by a model of the enforcement pipeline in which each municipality-year has a latent fiscal state T, an auditor observes a private signal and transmits a report r to a judge, and the judge combines r with her own signal s_J to decide whether to enforce ($d = 1$). The model yields two families of equilibria distinguished by the judge's aggregation rule. In the conservative ("AND") equilibrium, the judge enforces only when both the auditor's report and her own signal indicate irregularities; in the aggressive ("OR") equilibrium, she enforces whenever at least one source flags a problem. The key difference is the auditor's veto power: under "AND" auditors can always block enforcement by concealing irregularities; under "OR", the judge can convict on her own signal alone, making concealment less valuable. This generates sharply different predictions about (i) whether the reform unam-

biguously reduces misreporting; (ii) how auditor signal quality affects reporting choices; (iii) whether judicial experience amplifies or dampens the distortion caused by local ties; and (iv) whether enforcement gains from the reform are largest for experienced or inexperienced judges.[1] These opposing predictions allow us to use the empirical heterogeneity patterns to discriminate between the two regimes.

The Italian institutional setting offers a rare combination of features that allow us to take this framework to the data. Italy's over 8,000 municipalities are overseen by a two-tier monitoring architecture: approximately 14,000 local auditors produce standardized reports that feed into a centralized judicial body, the National Court of Auditors, whose judges decide whether to open enforcement proceedings. This structure maps cleanly onto the theoretical framework of an informed sender and a partially informed receiver. Crucially, a 2012 reform centralized and randomized the assignment of municipal auditors, replacing a system in which mayors handpicked their own monitors. The reform simultaneously broke pre-existing local ties between auditors and municipal governments *and* generated quasi-random matches between auditors and judges—providing sharp experimental variation in the independence of the front-line monitor without changing the legal framework, the judge pool, or the Court's decision rules.

We exploit this reform using administrative micro-data that track both communication within the organization and its enforcement consequences. We theoretically and empirically link four components: the full set of auditor questionnaires, which capture what auditors report to the Court; Court deliberations and judge identities, which capture enforcement decisions and judicial experience; municipal financial records, which we use to construct a machine-learning measure of fiscal risk; and exogenous fiscal distress events recorded by the Treasury.

This structure allows us to observe both stages of the enforcement pipeline: what auditors report and how judges respond. That dual observation resolves a central identification problem in the literature, which typically observes only final enforcement outcomes. When enforcement changes, it is usually unclear whether this reflects better underlying information, more honest reporting, or different decision-making. Because we observe the auditor's report directly, we can separate changes in transmission from changes in judicial response, and attribute shifts in enforcement to changes in the information reaching judges rather than inferring them from outcomes alone.

Our identification strategy exploits the quasi-exogenous variation in treatment timing arising from the staggered introduction of random auditor assignment, based on pre-determined variation in the expiration dates of previously-appointed auditors' terms (Vannutelli, 2021).

[1] These predictions are summarized in Table 2 and discussed further in the paper.

Because these expiration dates reflect historical appointment cycles that are uncorrelated with current municipal shocks or the electoral cycle, both the timing and the identity of the new auditors are orthogonal to contemporaneous determinants of fiscal policy and enforcement.

We establish four sets of results that collectively trace the reform's impact through the information pipeline and allow us to discriminate between the AND (conservative) and OR (aggressive) equilibria.

First, information transmission improves. Following the reform, auditors report substantially more irregularities: the probability that any irregularity is flagged rises by 1.12 percentage points, a 95% increase over the baseline. The increase is concentrated in high-risk municipalities and in places with strong pre-reform local ties, suggesting that auditors were previously understating problems in precisely those settings. This pattern is consistent with a simple prediction: increasing auditor independence should lead to more complete reporting of irregularities.

Second, enforcement increases. Event studies show flat pre-trends and a discrete, persistent rise in the probability that the Court opens a deliberation. The average effect is 1.72 percentage points, about 6.3% of the pre-reform mean. The clear increase suggests that improved auditor independence translates into more enforcement, consistent with the idea that reducing local ties leads auditors to surface more problems rather than offsetting those gains through reduced local knowledge.

Third, enforcement becomes more precisely targeted to underlying risk. The increase in enforcement is concentrated in municipalities with higher underlying fiscal risk. Using a pre-reform machine-learning risk score, we find that deliberations rise by 1.77 percentage points among high-risk municipalities (a 5% increase relative to the pre-treatment mean), with little to no change among low-risk ones. The effect increases nearly monotonically across risk quintiles, indicating that the reform directs enforcement toward the municipalities where problems are most likely.

Fourth, heterogeneity evidence points to the underlying mechanism. On the supply side, gains in reported irregularities are largest when auditors are new entrants with less local knowledge, suggesting that independence matters more than familiarity for improving reporting. On the demand side, experienced judges drive the enforcement response, increasing deliberations by 1.61 percentage points (an 8.2% increase relative to the baseline), with effects concentrated among high-risk municipalities, while inexperienced judges show no significant effect. Taken together, these patterns indicate that improved reporting drives the results and that judicial capacity to act on better signals is essential for translating auditor independence into targeted enforcement.

These findings speak to a central tension in the design of monitoring institutions. Policymakers often face a trade-off between local knowledge and independence: locally embedded monitors may be better informed but more susceptible to capture, while independent monitors may be less informed but more willing to report problems. Our results suggest that, in settings with structured reporting and decision-makers who can recognize improved signals, the gains from independence outweigh the costs of reduced local expertise. This insight extends beyond fiscal oversight to any hierarchical organization—such as banking supervision, environmental regulation, or health inspections—where front-line monitors operate under potential conflicts of interest.

This research contributes to the economics literature on regulatory enforcement. Closest to our work, Duflo et al. (2013) and Duflo et al. (2018) show that the design of auditor incentives and rotation shapes the quality of environmental regulation in India. These papers measure the overall effect of monitoring interventions on downstream outcomes such as pollution or fiscal compliance; by contrast, we open the black box of the enforcement hierarchy by observing both the internal reports (the message) and the judicial actions (the decision). That allows us to identify the specific channel—information transmission versus information production—through which monitoring design affects enforcement. This distinction matters for policy, because it implies that reforming who produces information can improve enforcement even when the decision-maker's authority and incentives remain unchanged.

The broader literature on public auditing and accountability documents that top-down government audits reduce corruption (Olken, 2007), that random audits in Brazil expose malfeasance and affect electoral outcomes (Ferraz and Finan, 2008, 2011), and that the threat of audits deters future non-compliance (Avis et al., 2018; Bobonis et al., 2016). Vannutelli (2021) uses the same Italian reform to study auditor independence and fiscal outcomes. We extend this body of work along two dimensions. First, we model and trace the full information pipeline from auditor signal to judicial enforcement, providing direct evidence on the information-transmission channel through the standardized questionnaires. Second, we show that the reform improves not only the *level* but also the *targeting* of enforcement—a result that the existing literature, which typically measures average effects, has not been able to establish.

A third contribution is to bridge the literatures on auditing and judicial decision-making. Abrams et al. (2012) and Cohen and Yang (2019) document substantial unexplained variation in sentencing across judges, Coviello et al. (2019) study how task specialization affects judicial productivity, and Chan et al. (2022) develop methods to estimate judicial preferences from decisions. These papers study judges in isolation from the upstream information they receive. By jointly modeling the auditor-judge pipeline, we show that the value of better

monitoring information depends critically on who receives it: experienced judges translate improved signals into sharply targeted enforcement, while inexperienced judges do not. This finding demonstrates that the returns to improving information supply are endogenous to the decision-maker's capacity—a point with implications for the design of any organization in which information producers and decision-makers are distinct agents.

Our theoretical framework draws on canonical models of strategic communication and delegation in hierarchies (Crawford and Sobel, 1982; Aghion and Tirole, 1997; Dessein, 2002; Tirole, 1986; Laffont and Martimort, 1997). A growing empirical literature documents how information is transmitted—and lost—within organizations: Stein (2002) argues that hierarchies lose soft information in transmission, Liberti and Petersen (2019) shows that such information degrades as it travels through hierarchical layers in banking, and recent work by Kala et al. (2025), and Nambiar et al. (2025) studies communication patterns in corporate and bureaucratic settings. Our paper expands on these works by providing causal evidence on how changes in selection and incentives of individuals in charge of information collection and production change communication patterns within a hierarchy.

Finally, our setting—a two-tier system with local auditors feeding information to national judges—speaks directly to the literature on supervisory architecture. Dell'Ariccia and Marquez (2006) and Agarwal et al. (2014) show that the allocation of supervisory authority between federal and local regulators affects financial regulation, with proximity to regulated entities biasing monitoring. Haselmann et al. (2022) demonstrate that supervisory distance matters for bank oversight quality, and Calzolari et al. (2019) studies optimal regulatory architecture in multi-tier systems. Our setting offers a cleaner design than is typically available in this literature: quasi-random auditor assignment with staggered timing, a fixed reporting template, and a homogeneous enforcement body let us isolate the effect of changing the front-line monitor's independence while holding the decision-maker fixed. The reform provides evidence that independence of the front-line monitor is a first-order design choice, with benefits that dominate the costs of reduced local knowledge.

2 Institutional Setting

2.1 Municipal Governance and Fiscal Monitoring

Italy is a highly decentralized democracy, with two levels of subnational governments: 20 regions and over 8,000 municipalities. Each municipality has its local government composed of an elected mayor (*Sindaco*), an executive body (*Giunta*) appointed by the mayor, and an elected council (*Consiglio Comunale*). The mayor is directly elected for a five-year mandate

with a two-term limit, holds executive power at the municipal level, and is responsible for all key policy decisions, including the annual budget.

Municipalities enjoy substantial fiscal autonomy. They manage around 8% of total public expenditure (over €55 billion) and control a wide range of essential public services, including waste management, social services, childcare, school-related services, local police, road maintenance, housing, culture, recreation, and economic development. Fiscal revenues come from two main sources: local taxes (primarily the property tax) and local fees (building permits, traffic fines, and fees for other services). Spending is financed by municipal fiscal revenues (87%) plus transfers from the central government (13%), while borrowing is allowed only to finance investment expenditures and is subject to strict quantitative limits.[2] The mayor enjoys substantial executive power and 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 subnational debt accounts for approximately 11% of GDP. During the economic 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.

Given the high degree of decentralization, municipalities are subject to national fiscal rules and a stringent monitoring system led by the National Court of Auditors (*Corte dei Conti*). This body is a Supreme Audit Institution, responsible for overseeing the proper management of public finances and the legality of public administration activities. It operates as an independent body, reporting directly to Parliament, and can initiate judicial proceedings against municipalities that fail to comply with fiscal rules or engage in improper financial practices. The Court operates across the entire national territory, with central sections based in Rome and regional sections located in the capital of each region, as well as in the autonomous provinces of Trento and Bolzano.

The composition of each regional panel is determined by the Council of the Presidency of the Court, the body responsible for managing the careers of judges and the internal organization of the institution. Each regional section is led by a President, appointed based

[2]Municipalities can undertake new debt only if total 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 state-owned *Cassa Depositi e Prestiti*, so most municipal debt is implicitly guaranteed by the national government.

on merit and seniority, who is responsible for assigning cases to judges. The assignment of judges to regional offices is institutionalized and regulated; by contrast, the allocation of judges to municipalities within each region follows more informal practices. In most cases, judges are assigned to municipalities of a specific province, with this assignment reviewed annually—a distribution we learned about through direct conversations with judges that is widespread but not codified by formal rules. Judges remain in the same region for an average of five years, with a maximum of ten years, after which they may be reassigned to a different region or to the central office.

Once a case is assigned, the designated judge conducts the investigation. When the investigation is complete, the judge submits the case, along with a report, to the President, who convenes the full panel and the involved municipality for review. The process ensures compliance with adversarial principles and provides for appeal to a central section. The final deliberation is carried out by a commission of no fewer than three members. On average, the Court issues around 2,000 judicial acts of *ex-post* verification per year (Corte dei Conti, 2022). The majority of these are initial warnings (*pronunce di accertamento*) that alert municipalities 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.

2.2 The Role of Municipal Auditors

Italy's 8,000 municipalities pose a significant oversight challenge for the Court, which counts 477 judges—only half of whom are assigned to regional control sections. Given these constraints, the monitoring system relies on a hierarchical structure. Approximately 14,000 local auditors (*Revisori dei conti*) act as the Court's operational backbone, conducting first-line financial checks and feeding critical information into the judicial process.

Since 1990, all municipal budgets must be audited and certified by a board of auditors. The board is composed of one member for municipalities with fewer than 15,000 inhabitants—representing about 90% of municipalities—and three members for larger ones. Unlike in the United States, where municipal audits are predominantly conducted by large private firms, Italian municipal auditors are self-employed Certified Public Accountants (CPAs) who simultaneously serve municipal governments and provide freelance accounting services to individuals and small businesses. With over 150,000 registered CPAs authorized to work as municipal auditors, the potential pool is large. Working as a municipal audi-

tor provides professional visibility and helps expand private client networks, making it an attractive career beyond its direct monetary compensation. As is common in markets for experts, however, the pre-reform system of discretionary appointments likely concentrated auditing jobs within a restricted set of "connected" auditors based on patronage networks.

From its inception, the board of auditors was assigned a dual role of monitoring and advising. The original statutory mandate (*Law 142/1990, Article 57*) specifies that auditors "collaborate with the municipal council in its control and governing function, supervise the accounting and financial regularity of the management of the entity, and certify the correspondence of the balance sheet to the economic outcomes." Importantly, municipal auditors do not merely sign off on balance sheets *ex post*: according to the National Association of Municipal Auditors (ANCREL), 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—all before key financial decisions are adopted by the council. The auditors' tasks were progressively expanded between 1990 and the early 2000s but were not affected by the 2011 reform.

The results of the audit review and the auditors' opinions are compiled in a report reviewed by the council at the time of budget approval. While opinions are not legally binding, the council is expected to follow them or formally justify any inaction. Following the review, auditors submit the approved balance sheet, audit report, and a detailed questionnaire to the Court. The 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 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, and municipalities that fail to act on auditor findings are flagged in subsequent reviews.

Prior to the reform, the board of auditors was selected by the municipal council with a majority vote, contracts were renewed every three years, and the boards' remuneration was directly sourced from the municipality's budget and left to the municipality's discretion. The

board cannot be dismissed unless the council can prove faulty inaction or breach of official duties, and each board member can hold at most eight contemporaneous appointments.

2.3 The Policy Reform

In August 2011, at the peak of the European Sovereign Debt Crisis, the national government changed the mechanism of auditor appointment (*Law 148/2011*). The new law mandated that *"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. For each draw, the prefecture compiles a slate of $3N$ candidates, where N is the number of auditor seats to be filled. The municipality must appoint candidates in the order drawn; mayors cannot choose among drafted candidates. Participation in the regional lists is subject to experience requirements that vary with the population of the municipality, 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 for at least 2 years; between 5,000 and 14,999, at least 5 years of certification and one prior appointment; above 15,000, at least 10 years and two prior appointments. Refusals are relatively rare: approximately 20% of first-drafted candidates decline, typically due to distance, and in fewer than 3% of cases a new draw was required because no candidate from the original slate accepted. Auditors' compensation was set nationally, including a minimum floor covering expenses and maximum caps varying by population, with minimal variation in actual compensation across municipalities of similar size.

The reform does not apply to the five Special Status Regions (Valle d'Aosta, Trentino Alto Adige, Friuli Venezia Giulia, Sardinia, and Sicily), which retain autonomy over their own fiscal monitoring procedures. Our analysis is therefore limited to municipalities in the 15 Ordinary regions.

3 Model

We develop a simple model of the information pipeline from auditor investigation to judicial enforcement. The model captures two key frictions: a *production* friction, whereby noisy signals limit diagnostic accuracy, and a *transmission* friction, whereby locally tied auditors may strategically distort their reports. The framework delivers sharp comparative statics that map directly to our empirical tests.

Environment. A municipality has an unobserved fiscal condition $T \in \{0, 1\}$, where $T = 1$ denotes fiscal irregularities and $T = 0$ denotes fiscal soundness. The prior probability of irregularities is $\pi \equiv \Pr(T = 1)$. Two players interact sequentially: an *auditor* (A) who investigates the municipality and transmits a report, and a *judge* (J) who decides whether to initiate enforcement proceedings ($d = 1$) or not ($d = 0$).

Each player receives a private binary signal $s_i \in \{0, 1\}$ about the true state, drawn from a symmetric channel with precision $p_i \in [\frac{1}{2}, 1]$: the signal equals the true state with probability p_i and is incorrect with probability $1 - p_i$. Signals are conditionally independent given T. After observing s_A, the auditor sends a binary report $r \in \{0, 1\}$ to the judge, who then observes both r and her own signal s_J before choosing d.

Local ties and auditor incentives. A central feature of the model is that auditors may have local ties to the municipality they oversee. Let $\tau \in \{0, 1\}$ indicate whether the auditor is locally connected ($\tau = 1$) or not ($\tau = 0$), with $\theta \equiv \Pr(\tau = 1)$. When tied, the auditor draws a bias $b > 0$ from a continuous distribution F on $(0, \infty)$, capturing the strength of her relational incentives to avoid enforcement. Both τ and b are independent of T.

The auditor's payoff is

$$u_A(d, r, s_A; \tau, b) = \tau b \, \mathbf{1}_{\{d=0\}} - \mathbf{1}_{\{r \neq s_A\}}. \tag{1}$$

The first term reflects the tied auditor's benefit from no enforcement; the second is a misreporting cost representing career or reputational concerns.[3] In the comparative statics, the reform that randomizes auditor assignment corresponds to a reduction in θ.

Judge's objective. The judge minimizes a weighted loss from classification errors:

$$u_J(d, T) = -\kappa \, \mathbf{1}_{\{d=1,\, T=0\}} - (1 - \kappa) \, \mathbf{1}_{\{d=0,\, T=1\}}, \tag{2}$$

[3]This formulation is without loss of generality. Allowing heterogeneous misreporting costs $c > 0$ and dividing through by c yields an equivalent model with rescaled bias $\tilde{b} = b/c$; see Appendix B for details.

where $\kappa \equiv c_1/(c_1 + c_2) \in (0, 1)$ is the normalized cost of a Type I error (enforcement against a sound municipality) relative to a Type II error (no enforcement when irregularities are present). The judge initiates enforcement whenever her posterior belief that $T = 1$ exceeds κ.

Timing. Nature draws T and the auditor's type (τ, b). The auditor observes s_A and sends report r. The judge observes (r, s_J) and chooses d.

Assumptions. We maintain three assumptions throughout. First, after observing $s_A = 1$, irregularities are at least as likely as no irregularities: $\Pr(T = 1 \mid s_A = 1) \geq \frac{1}{2}$. Second, if the judge could observe both signals directly, the optimal decision would be unambiguous: two positive signals would warrant enforcement, while two negative signals would not. These two conditions rule out degenerate equilibria in which the judge always convicts or always acquits. Third, the auditor's signal is more precise than the judge's ($p_A > p_J$), reflecting the auditor's specialized diagnostic role. This rules out an equilibrium in which the judge ignores the report entirely (see Appendix B for formal statements and proofs).

3.1 Two Equilibria

Given these assumptions, two natural equilibria emerge, distinguished by how the judge aggregates the auditor's report with her own signal.[4]

Conservative ("AND") equilibrium. The judge enforces if and only if *both* sources indicate irregularities: $d = 1 \Leftrightarrow (r = 1 \text{ and } s_J = 1)$. Under this rule, a tied auditor who observes $s_A = 1$ can suppress enforcement entirely by reporting $r = 0$. Formally, she lies whenever her bias exceeds a threshold:

$$b \geq \frac{1}{\Gamma_1},$$

where $\Gamma_1 \equiv \Pr(s_J = 1 \mid s_A = 1)$ is the probability that the judge's signal corroborates the auditor's. It increases in both p_A and p_J: a more precise auditor signal raises the posterior $\Pr(T = 1 \mid s_A = 1)$, making it more likely the judge also observes $s_J = 1$. The overall misreporting rate is

$$m^{\wedge} = \theta \left[1 - F\left(\frac{1}{\Gamma_1}\right)\right]. \tag{3}$$

[4] In both equilibria, untied auditors always report truthfully. Tied auditors always report truthfully when $s_A = 0$, since misreporting upward is costly and never benefits them. The strategic tension arises only when a tied auditor observes $s_A = 1$.

Aggressive ("OR") equilibrium. The judge enforces whenever *at least one* source indicates irregularities: $d = 1 \Leftrightarrow (r = 1 \text{ or } s_J = 1)$. Here, lying only prevents enforcement when the judge's own signal happens to be $s_J = 0$, so the threshold for lying is higher:

$$b \geq \frac{1}{1 - \Gamma_1}.$$

The misreporting rate is

$$m^{\vee} = \theta \left[1 - F\Big(\frac{1}{1 - \Gamma_1}\Big)\right]. \tag{4}$$

Since $\Gamma_1 > \frac{1}{2}$ under our assumptions, we have $1/\Gamma_1 < 1/(1 - \Gamma_1)$, so fewer tied auditors find it worthwhile to lie under OR than under AND. Intuitively, lying is less effective under the aggressive regime because the judge's own signal can still trigger enforcement.

3.2 Comparative Statics

The two equilibria generate sharply contrasting predictions. We state the main results here and relegate formal derivations to Appendix B.

Effect of ties on misreporting and welfare. In both equilibria, more ties increase misreporting and reduce judicial welfare:

$$\frac{\partial m^{\wedge}}{\partial \theta} \geq 0, \quad \frac{\partial V_J^{\wedge}}{\partial \theta} \leq 0; \qquad \frac{\partial m^{\vee}}{\partial \theta} \geq 0, \quad \frac{\partial V_J^{\vee}}{\partial \theta} \leq 0.$$

This is the direct channel through which the reform—by reducing θ—can improve enforcement.

Effect of auditor signal quality. Here the equilibria diverge. Under AND, a better-informed auditor ($\uparrow p_A$) *increases* misreporting: a more precise signal $s_A = 1$ makes the judge's signal more likely to also equal 1, raising the probability that a truthful report leads to conviction, which strengthens the tied auditor's incentive to suppress it. Under OR, the logic reverses: a more precise $s_A = 1$ makes $s_J = 0$ less likely, which is the only event where lying prevents enforcement, so fewer tied auditors find it worthwhile to lie. Formally:

$$\frac{\partial m^{\wedge}}{\partial p_A} \geq 0 \qquad \text{versus} \qquad \frac{\partial m^{\vee}}{\partial p_A} \leq 0.$$

Interaction between ties and judge experience. The equilibria also deliver opposite predictions about how judge expertise (p_J) modulates the effect of ties. Under AND, more

experienced judges *amplify* the distortion from ties: as p_J rises, the judge is more likely to observe $s_J = 1$ when the state is indeed $T = 1$, making it more valuable for a tied auditor to suppress her positive signal. Under OR, more experienced judges *attenuate* the distortion: the event $s_J = 0$ (the only scenario where lying matters) becomes rarer, reducing the return to misreporting. Formally:

$$\frac{\partial^2 m^{\wedge}}{\partial p_J \partial \theta} \geq 0 \qquad \text{versus} \qquad \frac{\partial^2 m^{\vee}}{\partial p_J \partial \theta} \leq 0.$$

The welfare cross-derivatives mirror this pattern: the welfare gain from reducing ties is *larger* for experienced judges under AND and *smaller* under OR.

3.3 The Reform as a Reduction in Territorial Familiarity

To evaluate the reform, we interpret θ as an index of territorial familiarity: higher θ increases both the likelihood of relational ties and the auditor's signal precision through locally acquired knowledge. Formally, let $p_A = p_A(\theta)$ with $p_A'(\theta) > 0$, and model the reform as a reduction in θ that lowers both ties and auditor precision.

Under AND, the reform *unambiguously reduces misreporting*—both the direct reduction in ties and the loss of local expertise work in the same direction (the latter lowers Γ_1, further reducing the incentive to lie). Under OR, the effect on misreporting is *ambiguous*: the loss of local expertise can increase misreporting, partially offsetting the benefit of fewer ties.

In both equilibria, the welfare effect of the reform trades off two channels: a *misreporting channel* (fewer ties improve report quality) and a *signal quality channel* (less local knowledge reduces diagnostic precision for truthful auditors). The empirical analysis in Sections 6.1–6.4 evaluates the net effect and uses the contrasting predictions—particularly heterogeneity by judge experience—to determine which equilibrium better describes the Court's enforcement behavior. Table 2 maps each comparative static to its corresponding empirical test.

4 Data

4.1 Data Sources

Auditor Questionnaires 2010-2015. Each year, Italian municipalities are assigned a board of auditors. This board prepares an audit report that is submitted to the national Court of Auditors. One part of this audit report is a standardized questionnaire that remains largely consistent year to year. The questionnaire is a structured text document that

solicits information about financial distress, structural deficit indicators, and accounting irregularities.

Between 2010 and 2015, the format and content of these questionnaires were highly standardized, with only minor revisions introduced to reflect changes in financial regulation or oversight priorities. The dataset we assembled includes approximately one million observations, corresponding to about forty-four thousand unique questionnaires. Each entry is associated with a specific municipality, fiscal year, and question, and derives from the standardized forms submitted by auditors to the Court of Auditors. We digitized and systematized these forms to create a consistent panel suitable for empirical analysis. Examples of the standardized questionnaires used during the period 2010–2015 are reproduced in Appendix A3.

Each questionnaire provides a comprehensive account of the financial condition of the municipality. It begins with an identification header reporting basic information about the local entity, population, and the responsible auditor, followed by references to relevant financial documents and deliberations approving the budget or the final account. Subsequent sections focus on the main dimensions of municipal finance, including budget and cash-flow results, debt evolution and guarantees, compliance with internal stability constraints, and the management of participations in publicly owned entities. The questionnaires also contain sections on personnel expenditure, assessing compliance with statutory limits, and on patrimonial and economic accounts, which verify the consistency of balance-sheet items and the valuation of assets and liabilities. Each form concludes with a certification statement, signed by the auditors, attesting that all reported data correspond to the official municipal accounts.

The questionnaires combine both structured and narrative elements. Closed-ended questions (typically Yes/No/Not Applicable) are complemented by numerical tables and qualitative explanations that allow auditors to clarify specific irregularities or exceptional circumstances. This hybrid format enables systematic comparison across municipalities and over time, while retaining valuable qualitative insights into local financial management.

Court Deliberations, 2009–2021. The Italian Court of Auditors (Corte dei conti) produces "deliberations" – formal judicial documents assessing the financial management of local governments and other public entities. Each deliberation corresponds to an official judgment concerning the compliance of a municipality's financial statements with accounting and fiscal regulations. The structure and language of these documents follow a relatively standardized institutional format, allowing for systematic textual analysis and extraction of key information.

The deliberations dataset comprises approximately thirty-six thousand observations, each corresponding to a specific deliberation–municipality pair. For each document, we identify the regional section issuing the judgment, the names and positions of the participating magistrates (including the president, councilors, and rapporteur judge), and the fiscal years of the municipal accounts under review. The deliberations were collected directly from the regional websites of the Court of Auditors and processed to extract relevant information through automated text recognition and structured parsing.

An example deliberation document is reproduced in Appendix A4. Each deliberation begins with a formal header reporting the unique identification number, the regional control section (e.g., "Regional Control Section for Tuscany"), and the composition of the judging panel. The main body of the document is divided into sections that follow a quasi-legal structure. The introductory paragraphs ("*VISTO*" or "*VISTA*") list the relevant legal references and precedents. The subsequent section ("*FATTO*") presents the factual elements emerging from the audit, including irregularities or imbalances detected in municipal accounts. The following section ("*DIRITTO*") provides the legal reasoning that connects the factual findings to the applicable laws and accounting standards. The deliberation concludes with a decision section ("*DELIBERA*" or "*DECIDE*"), often followed by a final operative part ("*P.Q.M.*" or "*ORDINA*"), which specifies the measures to be adopted by the municipality or other competent authorities.

Beyond their formal structure, the deliberations frequently discuss recurring issues in local public finance, such as the accumulation of arrears, the misclassification of expenditures, or breaches of fiscal discipline under the internal stability pact. They also reference the work of municipal auditors, linking the oversight activity of the *revisori dei conti* to the subsequent judicial evaluation of compliance. This makes the deliberations a valuable complement to the auditor questionnaires, providing an ex post institutional assessment of local financial behavior.

Municipal Balance Sheets, 2000-2022. Every year, each Italian municipality must prepare a municipal balance sheet. This balance sheet is submitted to the board of auditors and also submitted to the national Court of Auditors as a part of the audit report. The municipal balance sheet represents the official accounting document summarizing the financial position, operating performance, and patrimonial situation of the local government. It provides the quantitative foundation for the assessments carried out by the auditors and for the subsequent judicial review by the Court of Auditors.

Our dataset covers the period 2000–2022 and contains approximately two hundred thousand observations at the municipality–year level. For each municipality, the data include a

wide range of accounting variables that together describe the structure of revenues, expenditures, assets, and liabilities. These variables allow for the construction of financial indicators that capture the fiscal sustainability, liquidity, and performance of local governments. The balance sheets were collected from official administrative sources and harmonized over time to ensure consistency across changes in accounting frameworks.

Each municipal balance sheet is composed of three main sections: the budget account, the economic account, and the patrimonial account. The budget account reports realized revenues and expenditures, distinguishing between current and capital operations, and provides measures of the annual budget balance and the overall management result. The economic account records flows of costs and revenues on an accrual basis, reflecting the economic performance of the municipality. The patrimonial account provides a stock measure of assets and liabilities, including the composition of debt, receivables, and patrimonial resources. Together, these components allow for a comprehensive assessment of both the financial and economic equilibrium of local administrations.

Using these data, we compute a set of key indicators that summarize the financial condition of municipalities. These include measures of indebtedness and debt service costs, compliance with legal borrowing limits, off-balance-sheet obligations, liquidity ratios, budget balances, and indicators of revenue collection and payment execution efficiency. We also derive measures of fiscal independence, investment activity, and per capita debt burden, which provide additional insights into local fiscal behavior and sustainability.

Random Auditor Assignment, 2012–2019. The key treatment assignment variable for our analysis is the date when each municipality first appoints an auditor through the random draw procedure. All information on currently appointed auditors is available on the website of the Ministry of the Interior, separately for each municipality. We constructed a comprehensive historical database covering the universe of municipalities by scraping the Ministry's website, obtaining the draw dates and identities of all auditors selected through the random assignment system since 2012. We combined these data with information from the yearly candidate pools published by the Ministry, which include details on candidates' age, gender, municipality and region of birth and residence, level of professional experience (measured by years of CPA certification), and previous service as auditors. Furthermore, we recover the identity of the last auditor appointed before the reform from the annual questionnaire for the Annual Account of the Italian General Accounting Office submitted by municipalities to the Ministry of Economics and Finance, allowing us to measure pre-reform auditor characteristics—including whether the auditor was local (born or residing in the same municipality) and their accumulated experience—for a substantial share of our

sample.

Municipal Financial Distress Procedures, 1990–2021. This dataset reports all municipalities that activated either the bankruptcy (*dissesto*) or financial rebalancing (*riequilibrio*) procedures between 1990 and 2021. *Dissesto* represents the more severe procedure, where municipalities formally declare bankruptcy and cannot meet their financial obligations. *Riequilibrio*, introduced as a preventive alternative, allows municipalities to propose recovery plans while maintaining greater autonomy. After the highest bankruptcy incidence in the 1990s, these instruments stayed mostly unused until the 2011 reform, when *riequilibrio* became the more frequently used mechanism as municipalities sought to avoid the drastic consequences of formal bankruptcy.[5]

Municipal Elections, 2004–2019. We use detailed data on the universe of municipal elections provided by the Ministry of the Interior, including election dates, results, and mayor characteristics (gender, age, and education). These data allow us to control for election cycle fixed effects and mayoral term, which is important because mayoral terms (five years) do not overlap with auditor terms (three years), providing an additional source of exogenous variation.

Supplementary Sources. We supplement the above with municipal geographic and sociodemographic characteristics from the Italian Statistical Census (ISTAT), and data on the universe of municipal council dismissals digitized by OpenPolis. Italian municipal councils can be dismissed for several reasons—predominantly mafia infiltration, but also failure to approve the budget. Council dismissals affect economic activity and are likely reflected in municipal budgets; while we prefer not to include them as controls in our main specifications (since dismissal could in principle be an outcome), all results are robust to controlling for or excluding dismissed municipalities (165 municipalities, less than 3% of our sample).

4.2 Sample Construction and Key Variables

Our analysis sample is based on the 6,627 municipalities located in Ordinary regions. To maintain a balanced panel during the analysis period, we exclude municipalities that were dissolved or newly created between 2008 and 2015, as well as 265 municipalities affected by earthquakes during the analysis period (which were granted fiscal exceptions following the catastrophic events). We also exclude 341 municipalities that voluntarily participated in the

[5] *Riequilibrio* is similar to Chapter 11 reorganization in the US, while *dissesto* resembles Chapter 7 liquidation.

experimental phase of the "harmonized accounting system" between 2013 and 2015, as this system significantly changed both the structure of financial reports and the set of applicable fiscal rules. The final sample consists of 5,603 municipalities observed between 2007 and 2015.

To measure financial outcomes, we rely on the "final balance sheets" (*Bilanci Consuntivi*), which report realized revenues and expenditures for the previous fiscal year and must be submitted and approved by April 30 of the following year. We focus on a set of fiscal indicators that the central government and the National Court of Auditors routinely use to monitor the fiscal stance of local administrations.

First, we consider *Off-balance Sheet Debts*, defined as municipal liabilities not covered by any revenues. These arise outside the approved budget framework and are not absorbed into current or capital expenditures. Their presence signals violations of budgetary discipline and typically reflects accounting irregularities or unauthorized commitments that must eventually be recognized and repaid. Auditors are explicitly asked by the Court to closely monitor the presence of off-balance-sheet debts and the process of recognition, and to report on them in a specific section of the questionnaire.

Second, we analyze the *Debt Limit Index*, which measures the ratio of expenditures on debt repayments to total available revenues. This indicator reflects the extent to which a municipality's resources are absorbed by servicing outstanding obligations and provides a forward-looking measure of fiscal pressure associated with compliance with nationally mandated debt limits.

Third, we examine *Cash Advances*, representing short-term cash inflows used to cover temporary liquidity shortages. Although these advances must be repaid, recurrent reliance on them highlights underlying liquidity constraints and weaknesses in cash-flow management.

Finally, we study the *Budget Surplus* (or deficit), calculated as the ending cash balance plus active residuals, minus passive residuals and restricted funds. This indicator summarizes a municipality's year-end fiscal position and its alignment with the balanced-budget targets emphasized by national fiscal rules.

All variables are expressed in per-capita terms, winsorized at the 1% level, expressed in 2018 euro equivalents, and reflect accrual accounting. Spending variables, which are strictly positive, are transformed using the inverse hyperbolic sine. Table A2 presents summary statistics for these fiscal indicators in the pre-reform period.

5 Methods

5.1 Machine Prediction of Fiscal Distress

We use supervised machine learning (ML) to predict municipal fiscal distress based on financial statement data. The goal is to build a reliable early-warning system that identifies municipalities most at risk of entering formal financial distress, enabling prioritized oversight by the Court of Auditors. This predictive exercise faces two main challenges. First, fiscal distress is a multi-dimensional phenomenon that reflects both structural and cyclical imbalances in municipal finances. Second, distress events are rare—occurring in only about two percent of municipality-year observations—so the problem involves severe class imbalance. ML algorithms are well-suited to address these issues because they can flexibly model non-linearities and interactions among financial indicators while employing regularization to prevent over-fitting and handle sparse positive outcomes (e.g., Hastie et al., 2009; Mullainathan and Spiess, 2017; Athey, 2018; Ash et al., 2025).

Data Preparation. Our dataset combines annual municipal balance sheets and audit questionnaires submitted to the Court of Auditors over 2005–2022. We focus on 24 financial indicators that are routinely monitored by the Court and capture the main dimensions of fiscal capacity, debt exposure, liquidity, and revenue performance (see Table 1). These include variables such as current and capital account balances, off-balance-sheet liabilities, tax revenues, and debt service ratios.

To account for temporal dynamics in fiscal deterioration, we augment the dataset with one- to five-year lags of each financial indicator. Missing values are imputed using within-municipality means across available years—a conservative approach that preserves cross-sectional comparability while avoiding spurious temporal patterns. We also include demographic controls (population, income per capita, and dependency ratios) and geographic fixed effects to capture persistent structural differences across regions and municipality size classes.

The resulting panel provides a rich set of fiscal and demographic predictors for each municipality-year observation. In contrast to traditional fiscal risk assessments that rely on single thresholds (e.g., debt-to-revenue ratios), this dataset allows the model to learn complex, non-linear combinations of indicators that jointly predict distress. The final matrix X contains roughly 24 base features, 120 lagged features, and several demographic controls, for a total of about 150 predictors and over 100,000 municipality-year observations. The binary outcome variable $Y_{it} \in 0, 1$ equals one if municipality i enters formal financial distress in year t, and zero otherwise.

Machine Learning Approach. We aim to learn a conditional expectation function $Y(X) = \mathbb{E}\{Y|X\}$ that provides a predicted probability, based on observed financial indicators X, that a municipality-year will enter fiscal distress. The feature space is moderately high-dimensional, with correlated fiscal and demographic predictors relative to the limited number of distress events. Classical linear models often over-fit and fail to capture important non-linearities or interactions, limiting their ability to generalize to unseen data (Hastie et al., 2009). Given our goal of predicting distress in municipalities that have not yet exhibited financial problems, we adopt ML algorithms rather than the standard linear models from applied economics (Mullainathan and Spiess, 2017; Athey, 2018).

ML algorithms provide two key advantages for this task. First, they introduce *regularization*—algorithmic penalties that constrain model complexity and reduce over-fitting. For example, our first approach is regularized logistic regression, a linear classifier that estimates a logit link between fiscal variables and the distress indicator. The parameters in this model are estimated by minimizing a penalized loss function,

$$\min_{\theta} \sum(-L(\theta) + R(\theta)),$$

where $L(\theta)$ is the log-likelihood and $R(\theta)$ is a penalty on the magnitude of the coefficients, such as the L1 (Lasso) or L2 (Ridge) norm (Tibshirani, 1996; Zou and Hastie, 2005). Regularization shrinks coefficients toward zero, improving out-of-sample stability and interpretability.

Second, ML models can efficiently capture complex, non-linear relationships among fiscal indicators without requiring the researcher to manually specify interactions or polynomial terms (Hastie et al., 2009). This flexibility motivates our second ML algorithm, random forests (Breiman, 2001), which are ensembles of decision trees trained on bootstrap samples of the data. Each tree recursively partitions the predictor space to reduce classification error, capturing non-linearities and interactions. Our third ML algorithm, XGBoost (Friedman, 2001; Chen and Guestrin, 2016), adds gradient boosting to sequentially fit trees to the residuals of the forests, gradually improving predictive accuracy while controlling over-fitting through shrinkage and subsampling. These enhancements yield state-of-the-art performance on structured tabular data (Feurer et al., 2018; Grinsztajn et al., 2022). We therefore take XGBoost as our main specification for predicting fiscal distress.

Model Training and Selection. Model training is restricted to post-2011 data, when financial reporting standards became consistent following national accounting reforms. We partition the data into training and test sets using five-fold cross-validation at the munic-

ipality level, ensuring that all observations for a given municipality fall within the same fold. This design prevents information leakage from lagged predictors and provides unbiased estimates of out-of-sample predictive performance. The rare-event nature of fiscal distress—roughly two percent of observations—creates substantial class imbalance, which we address by weighting the loss function inversely to class frequencies (King and Zeng, 2001; He and Garcia, 2009). For tree-based models, we also implement `scale_pos_weight` parameters equal to the imbalance ratio.

For each model, hyperparameters are tuned by grid search within the cross-validation folds (Varma and Simon, 2006; Cawley and Talbot, 2010; Feurer et al., 2018). For logistic regression, we select the optimal regularization strength λ and the elastic-net mixing parameter using the validation loss. For the random forest, we tune the number and depth of trees, the number of features sampled per split, and the minimum node size. For XGBoost, we tune the number of boosting rounds, learning rate, maximum tree depth, and regularization parameters (α and λ). Early stopping is applied when the validation loss fails to improve over a fixed number of iterations.

Across models, performance is evaluated on held-out test data using accuracy, precision, recall, F1 score, and the area under the receiver operating characteristic curve (AUC-ROC). As in other prediction tasks with tabular data, we find that gradient-boosted trees (XGBoost) consistently outperform logistic regression and random forests in both accuracy and calibration (Friedman, 2001; Chen and Guestrin, 2016; Feurer et al., 2018; Grinsztajn et al., 2022; Kleinberg et al., 2018; Ash et al., 2025). Consequently, XGBoost serves as our preferred predictive model for fiscal distress.

Model Evaluation. We evaluate model performance using held-out test data to assess predictive accuracy and robustness. Figure 1 presents Receiver Operating Characteristic (ROC) curves comparing the discriminative ability of the three classifiers. The diagnostics summarize the trade-off between sensitivity (the ability to detect true distress cases) and specificity (the ability to avoid false alarms). The area under the ROC curve (AUC-ROC) provides a threshold-independent measure of performance, interpretable as the probability that a randomly selected distressed municipality is assigned a higher predicted risk than a non-distressed one. While all models perform well, XGBoost has the highest AUC-ROC (0.838).

To better understand classification behavior, Table A3 reports how precision, recall, and F1 scores vary across different decision thresholds for the XGBoost model. This analysis highlights how policy priorities—such as minimizing false positives versus maximizing early detection—map onto alternative decision boundaries. In our baseline results, we report

outcomes at a balanced operating point that optimizes the F1 score, while appendix figures present full threshold-sensitivity curves. Figure A5 shows a calibration plot, illustrating that as predicted distress increases, true distress rates increase in held out test data.

5.2 Empirical Design

Our identification strategy leverages the staggered introduction of the random auditor assignment reform across municipalities. The reform became effective in December 2012, but treatment timing varies across municipalities depending on when their incumbent auditor's term expired. This variation creates a natural experiment where municipalities are randomly assigned to different treatment cohorts based on historical appointment patterns unrelated to municipal characteristics. Figure A2 displays municipalities geographic distribution, highlighting the lack of any geographic patterns by treatment timing.

To estimate the causal effects of random audits, we employ an event study specification:

$$Y_{it} = \sum_{k \neq -1} \beta_k \cdot \mathbf{1}[t - t_i^* = k] + \alpha_i + \delta_{rpt} + \varepsilon_{it} \tag{5}$$

where Y_{it} represents our outcome of interest for municipality i in year t, t_i^* denotes the year municipality i first receives a random auditor, and k represents event time relative to treatment. The coefficients β_k capture the dynamic treatment effects at each horizon relative to the year of first random auditor assignment. Municipality fixed effects α_i control for time-invariant characteristics. The fixed effects δ_{rpt} are risk-population-year fixed effects, constructed by interacting each municipality's pre-reform predicted fiscal distress quartile with its population size category and year.[6] These fixed effects allow municipalities that are similar in both ex-ante fiscal risk and population size to serve as comparison units, absorbing differential trends across groups that may differ systematically in their pre-reform characteristics.

The main identifying assumption underlying our empirical design is that municipalities exposed to the reform at different points in time would not have followed distinct trends in the absence of treatment; in other words, without the change in the auditor's appointment, the trajectories of outcomes across treatment cohorts would have evolved in a comparable manner over time.

Our empirical specification already addresses several potential issues that might otherwise challenge the causal interpretation of our findings. First, by including municipality fixed

[6]Population size categories follow the Italian Statistical Office (ISTAT) classification: municipalities with fewer than 1,000 inhabitants; 1,000–4,999; 5,000–9,999; 10,000–14,999; 15,000–19,999; 20,000–59,999; and 60,000 or more inhabitants.

effects, we remove the influence of any time-invariant municipal characteristics that could explain differences in outcomes. Second, the inclusion of year fixed effects ensures that we are not capturing shocks that affect all municipalities simultaneously, such as macroeconomic fluctuations or nationwide policy changes.

Given the exogenous and historically determined variation in audit cycles across municipalities, the timing of auditor expiration—and hence the treatment onset—should be uncorrelated with the evolution of municipal outcomes. To support this assumption, Table A1 examines predictors of treatment timing. The table reports two F-tests at the bottom. When population dummies are excluded, the null hypothesis that all coefficients are jointly zero cannot be rejected across specifications.

Third, and perhaps most crucially, for all of our analyses, we assess whether outcomes evolve in parallel before the reform, providing evidence on the plausibility of post-reform parallel trends.

A further potential issue concerns anticipation effects (Malani and Reif, 2015), given that municipalities may have known the timing of their auditor changes. Two opposite anticipation patterns could theoretically emerge. On one hand, later-treated municipalities might start adjusting early in anticipation of a stricter audit, gradually adapting their fiscal behavior; such a response would bias our estimates downward, as the "control" group would already begin acting like the treated one. On the other hand, they could instead increase spending or debt accumulation before treatment, expecting future restrictions—an effect that would bias the estimates upward.

Finally, we address the possibility that other contemporaneous, time-varying shocks coincide with the staggered reform rollout. The main event of concern is the 2014 extension of the Domestic Stability Pact to municipalities under 5,000 inhabitants, which we capture through the inclusion of population-size-by-year fixed effects.

Another possible worry is that the change in auditor appointments occurred alongside a broader emergency reform package that may have influenced local finances through other channels.[7] However, these concurrent measures were implemented simultaneously across all municipalities, whereas the change in auditor appointments occurred gradually, depending on the expiry date of the incumbent auditor. Consequently, any effects of the broader reform are absorbed by the inclusion of year fixed effects in our specifications.

[7]The 2012 reform also introduced (a) stricter fiscal rules and cuts in central transfers, (b) progressive municipal income tax rates, (c) compulsory inter-municipal management of public services for towns below 1,000 residents, (d) a reduction in the size of municipal councils, and (e) temporary taxation of owner-occupied dwellings.

6 Results

We present results in the order of the information pipeline: we first document that auditor reports improve (upstream), then show that enforcement increases and becomes better targeted (downstream), and finally explore the mechanisms—auditor characteristics, local ties, and judicial experience—that explain these patterns. Table 2 maps each model prediction to its empirical test.

6.1 Information Transmission: Auditor Reports Improve

We begin by examining the upstream channel: whether the reform improves the informational content of audit reports. Both the AND and OR equilibria predict that reducing local ties ($\downarrow \theta$) should increase reported irregularities (Comparative Static 1 in both regimes). The key outcome is whether the auditor flags any accounting irregularity in the standardized questionnaire submitted to the Court—the first question on the form, and the most direct measure of the auditor's willingness to report problems to the judicial authority.

Figure 2(a) confirms this prediction: following the reform, the probability that auditors report accounting irregularities rises by 1.12 percentage points, a 95% increase over the pre-reform baseline of 1.18%. The event study displays flat pre-trends and a discrete, persistent post-treatment increase, ruling out differential pre-existing dynamics between early- and late-treated municipalities. The magnitude is striking: it implies that under the discretionary system, auditors were concealing a near-doubling of the irregularities they would otherwise have reported.

This improvement in reporting is concentrated among high-risk municipalities, as shown in Figure 2(b). Municipalities in the upper half of the pre-reform ML risk score distribution experience substantially larger increases in reported irregularities than those in the lower half. The reform thus leads to better reporting that more accurately reflects the underlying fiscal condition as measured by our distress index. This pattern aligns with our model's prediction that a more informative reported signal r pushes borderline high-risk cases over the judicial threshold, concentrating reporting gains where $\mathcal{M}(X)$ is highest. This finding is a necessary condition for both equilibria but does not yet discriminate between them, since both AND and OR predict that $\downarrow \theta$ increases r on average.

6.2 Enforcement Increases

We next examine whether the improved upstream signal translates into more enforcement downstream. In both equilibria, a reduction in misreporting m leads to higher enforcement

rates, conditional on the judge's aggregation rule. The AND equilibrium predicts an unambiguous increase (since both fewer ties and lower p_A reduce concealment), whereas the OR equilibrium predicts an ambiguous effect (since lower p_A may offset fewer ties).

Figure 3(a) presents event study estimates of the causal effect of random audits on the probability that the Court opens a deliberation against a municipality. The event-time coefficients display flat pre-trends and a discrete, persistent post-treatment rise. In Appendix Table A4, we perform a static DID specification to obtain the average treatment effect: the probability of a deliberation increases by 1.47 percentage points, or 5.4% relative to the pre-reform mean. The effect appears immediately with the arrival of the first randomly assigned auditor and remains stable across the three years of the auditor's term, suggesting a sudden and persistent shift in the Court's enforcement posture rather than a transient adjustment.

The unambiguous direction of this effect provides initial support for the AND regime. Under AND, both fewer ties ($\downarrow \theta$) and the loss of local knowledge ($\downarrow p_A$) reduce concealment, so the reform unambiguously increases enforcement. Under OR, lower p_A can increase misreporting by reducing the auditor's signal precision, potentially offsetting the benefit of fewer ties and making the net effect ambiguous. The clear positive effect we observe is therefore more consistent with the conservative aggregation rule.

To benchmark these magnitudes, it is useful to compare our estimates with the broader monitoring and anti-corruption literature. On the information-quality margin, the 95% increase in irregularity reporting we document is comparable in scale to Duflo et al. (2013), who find that randomly assigning third-party auditors in India reduced false pollution reporting by 80%. Both estimates point to large distortions in the baseline information environment: in our setting, auditors were concealing roughly half of the irregularities they would have reported under independence; in Gujarat, only 7% of plants were reported as violating emission standards when 59% actually were. On the enforcement margin, our 5.4% increase in judicial deliberations is of a similar order to the deterrence effects estimated in Brazil's random audit program: Avis et al. (2018) find an 8% reduction in future corruption among previously audited municipalities, and Lichand and Marcos (2023) estimate a comparable magnitude with additional geographic spillovers. These effects are more modest than the 8 percentage point (31%) reduction in missing road expenditures that Olken (2007) attributes to increasing government audit probability from 4% to 100%, or the 64% reduction in corrupt violations from timely pre-election audits in Bobonis et al. (2016)—but the comparison is informative: those interventions change whether or at what frequency monitoring occurs, whereas our reform changes only *who* does the monitoring while holding the monitoring institution fixed. The fact that reforming auditor independence alone generates enforcement gains on the order of 5–8% of the pre-treatment mean suggests that the organizational de-

sign of information production is a quantitatively important margin, even relative to the extensive margin of introducing monitoring itself.

Beyond judicial enforcement, we also document that the reform had a direct impact on municipal fiscal performance. Using the same staggered difference-in-differences framework applied to balance-sheet data, we find that treated municipalities increase their net surplus by approximately €16 per capita (an 8% rise relative to the pre-treatment mean) and increase debt repayments by a comparable magnitude. These fiscal improvements are achieved primarily through revenue-side adjustment—specifically, a significant expansion of property tax revenues driven by a broader tax base rather than rate increases—suggesting that more independent auditors compelled mayors to combat tax evasion they might otherwise have tolerated. Importantly, the reform does not lead to cuts in current or capital expenditures, and we detect no adverse effects on public goods provision or local economic activity, indicating that the fiscal gains come without meaningful costs to municipal services. The 8% fiscal improvement is notable in light of the broader literature: Ferraz and Finan (2011) estimate that reelection incentives reduce resource misappropriation by 27% in Brazilian municipalities, and Olken (2007) finds a 31% reduction in missing expenditures from full audit coverage. Our effect is smaller in absolute terms, which is expected given that we change the identity of the monitor rather than the presence or intensity of monitoring. Still, an 8% gain in net surplus from a reform that cost the government essentially nothing to implement—randomization replaced patronage at no additional fiscal outlay—represents a high benefit-to-cost ratio.

6.3 Targeting Improves

We then investigate whether the reform affects not only the level of enforcement, but its targeting. Our model predicts that an improved report r pushes borderline high-risk municipalities across the judicial enforcement threshold, concentrating enforcement gains where latent risk is highest. This prediction holds in both equilibria: a less noisy r shifts the posterior $\mu_J(r, s_J)$ above the judge's enforcement threshold κ precisely in cases where the true state $T = 1$, generating a positive correlation between latent risk and the reform's marginal effect on d.

To test this, we exploit our ML-produced risk score $\mathcal{M}(X)$—trained on pre-reform balance-sheet data to predict formal fiscal distress—and interact treatment with municipalities' pre-reform risk classification. Figure 3(b) demonstrates that high-risk municipalities experience substantially larger increases in deliberation probability compared to low-risk municipalities. The effect is 1.77 p.p. among high-risk municipalities (a 5% increase rela-

tive to the pre-treatment mean), while low-risk municipalities show negligible changes. The gradient across risk quintiles is near-monotonic: treatment effects increase steadily from the lowest to the highest quintile of $\mathcal{M}(X)$, indicating that the reform does not simply shift enforcement indiscriminately but channels it toward municipalities where the latent probability of irregularities is greatest.

This result has a direct policy implication: random auditor assignment functions as an implicit targeting mechanism. Without any explicit algorithmic allocation of enforcement resources, the reform achieves better targeting by improving the signal that judges observe, allowing the existing judicial threshold to sort municipalities more accurately. The improvement in targeting also speaks to the welfare consequences of the reform: enforcement gains are concentrated where the social cost of missed irregularities is highest, rather than generating costly false positives among fiscally sound municipalities.

A parallel pattern emerges on the fiscal side. The reform's effects on municipal budget outcomes—net surplus, debt repayments, and off-balance-sheet debt recognition—are significantly larger in municipalities with weaker pre-reform fiscal conditions, prior corruption investigations, and local auditors, consistent with the model's prediction that the value of independence is greatest where the risk of capture was highest. The targeting result also invites comparison with Duflo et al. (2018), who show that discretionary inspections by Indian pollution regulators generate three times more pollution abatement than randomly allocated inspections of equal number, highlighting the value of regulators' private information about which firms to target. Our finding is complementary: even without giving the regulator targeting discretion, improving the quality of front-line information allows the existing judicial threshold to sort cases more accurately, concentrating enforcement gains among high-risk municipalities. The two results together suggest that both algorithmic targeting and institutional-design reforms that improve information quality can enhance the precision of enforcement, through distinct channels.

6.4 Mechanisms

We now explore the mechanisms through which the reform improved both reporting and enforcement, tracing the effects through auditor characteristics, local ties, and judicial experience. The AND and OR equilibria generate sharply different predictions about these heterogeneity patterns (see Table 2), allowing us to discriminate between the two regimes.

Before examining heterogeneity in treatment effects, it is useful to document how the reform changed the composition of auditors and auditor-municipality matches. Comparing pre- and post-reform characteristics reveals several striking shifts. The reform attracted approximately 22,000 new auditors to the national registry, 60% of whom were new entrants with no prior municipal auditing experience. While these new auditors are less experienced in municipal oversight, they share similar professional credentials (age, years of CPA certification) with incumbents, suggesting that the pre-reform patronage system was restricting entry by competent professionals. Most importantly for our theoretical framework, the share of municipalities with a hometown auditor drops from 25% to essentially zero, and the median travel distance between auditor residence and municipality increases from 15 to over 60 minutes. The probability that auditor and mayor share a birthplace falls from 15% to 2%, and surname-sharing with local politicians drops from 6% to 1%. Re-appointment rates collapse from 63% to near zero (since re-drafting the same auditor is extremely unlikely). These compositional changes confirm that the reform dramatically reduced the scope for local capture ($\downarrow \theta$) while simultaneously reducing average local knowledge ($\downarrow p_A$)—precisely the variation our model exploits.

Improved transmission Figure 2(a) shows that random audits significantly increase the probability that auditors report accounting irregularities, consistent with our framework's prediction that reduced local ties and capture ($\downarrow \text{ties}_{im}$ and $\downarrow \tau_a$) improve the transmission of auditors' private assessments to judge-visible reports.

Better targeting at high risk This improvement in reporting is concentrated among high-risk municipalities, as shown in Figure 2(b), indicating that the reform leads to better reporting that more accurately reflects the underlying fiscal risk of municipalities as measured by our ML-based distress index. Table A6, column (2) confirms this: the effect is entirely driven by high-risk municipalities, where independent auditors raise irregularity reporting by 1.13 p.p. (a 96% increase over the baseline), while the effect for low-risk municipalities is small and statistically insignificant. This pattern aligns with our model's prediction that a more informative reported signal R pushes borderline high-risk cases over the judicial threshold, concentrating enforcement gains where $\mathcal{M}(X)$ is highest.

Breaking local capture. The largest increases in irregularities reporting occur in municipalities that were employing local auditors (residents of the municipality) in the years before the reform (Figure 4). This concentration of effects among municipalities with local auditors provides direct evidence supporting our framework's transmission channel: random assignment helps break local capture (reducing θ), enabling auditors to report problems they might otherwise have concealed due to personal or professional ties with municipal officials. The pattern is consistent with the costs-of-discretion hypothesis: in the pre-reform system, mayors with misaligned incentives—those in worse fiscal condition, those with prior corruption investigations, and those who had appointed local auditors—display the largest fiscal improvements after the reform. These are precisely the settings where the model predicts the largest reduction in concealment from lowering θ.

We investigate this anti-capture mechanism further by exploiting exogenous variation in auditor characteristics generated by random assignment. Random matching creates a natural trade-off between local knowledge and independence. While local auditors may possess superior information about municipal operations, they face stronger incentives to conceal irregularities due to social ties. Conversely, distant or less experienced auditors may lack local expertise but enjoy greater reporting independence.

Figure 5 demonstrates that our results are driven by auditor characteristics that favor independence over local knowledge. Effects are substantially larger in municipalities receiving auditors residing at least one hour away (Panel 5(b)). Greater geographic distance weakens local ties, improving information transmission by reducing social pressure to conceal findings. Similarly, the reform's impact concentrates among municipalities assigned new auditors with no prior municipal experience (Panel 5(c)).[8] These auditors have reduced concealment incentives through the absence of established relationships. The concentration of effects among distant and new municipal auditors reveals that independence and integrity outweigh local knowledge when capture risks are high.

Improved production capacity ($T \to S$). Figure 6 reveals that the increase in irregularities reporting is concentrated among municipalities that previously employed auditors with limited professional and territorial experience. To establish this result, we construct two measures of auditor experience, each capturing different aspects of professional competence. We classify each municipality based on the average characteristics of auditors assigned

[8]The new entrants could only be assigned to municipalities with population below 5,000 inhabitants due to minimum experience requirements, creating exogenous variation in auditor experience across municipality size. Importantly, municipalities above 5,000—which could only receive incumbent auditors reshuffled by the random draw—also display significant treatment effects, confirming that the reform operates through severing ties rather than simply through auditor replacement.

to a municipality in the pre-reform period ($t < 0$) and estimate separate event studies for municipalities with above- and below-median auditor experience along each dimension.

The experience measures capture distinct aspects of auditor capability. Experience as a Court auditor (median: 13 years) measures specialized knowledge of municipal oversight procedures and legal requirements. Experience across different provinces (median: 1 province) measure territorial breadth, reflecting exposure to diverse institutional contexts and administrative practices.[9] The results consistently show that the largest increases in irregularities reporting occur in municipalities whose pre-reform auditors had below-median experience across all four dimensions. This pattern supports our framework's production channel, indicating that random assignment systematically upgrades audit quality by improving diagnostic skill and effort ($\uparrow \kappa_a$), with the largest benefits occurring where auditor expertise was previously weakest. Importantly, this pattern is consistent with Comparative Static 2 of the AND equilibrium, where lower p_A reinforces the effect of fewer ties on reducing concealment. Under the OR equilibrium, lower p_A would instead partially offset the reform's benefit (see Table 2), which is not what we observe.

Judicial capital and enforcement decisions ($R \to D$). We test our framework's prediction that judicial capital (ϕ_j) mediates how judges translate improved information into enforcement action. We decompose deliberations according to the rapporteur's pre-reform characteristics. We classify judges based on their baseline experience (2009-2011) across two dimensions that capture different aspects of judicial capital. Total deliberations as rapporteur reflects experience as the primary judge on each case. Rapporteurs handle the investigation and draft the final decision, making this the most demanding judicial role. Prior experience in the specific province measures local institutional knowledge and familiarity with regional administrative practices.

Figure 7 shows the results for the full sample of municipalities. Experienced judges consistently drive larger increases in deliberations following random audits. This pattern holds across both measures of judicial experience, suggesting that different dimensions of judicial capital enhance enforcement effectiveness. The results provide initial evidence that higher judicial capital ($\uparrow \phi_j$) enhances judges' ability to act on improved information signals.

Differential filtering capacity. We next examine how this relationship varies with underlying municipal risk. We estimate separate event studies for high-risk and low-risk municipalities, splitting deliberations by judge experience within each risk category. This approach

[9] Figure A6 displays an additional measure of experience: general accounting experience (median: 17 years), which captures broader professional competence in financial analysis and auditing standards.

allows us to test whether judicial capital interacts with case characteristics in determining enforcement responses.

The results reveal an important interaction between judicial experience and municipal risk level. Among high-risk municipalities (Figures 7(b),7(e)), deliberations increase sharply when conducted by experienced judges but show no increases under inexperienced judges. We interpret this as evidence that higher judicial capital ($\uparrow \phi_j$) enables judges to effectively recognize and act upon credible signals from improved auditor reports. This effect is particularly valuable in complex cases where interpretive skill matters most.

These findings directly test Comparative Statics 3 and 4 of our model. Under AND, judge experience amplifies the distortion caused by ties and therefore also amplifies the reform's gains ($\partial^2 m^\wedge / \partial p_J \partial \theta \geq 0$; $\partial^2 V_J^\wedge / \partial p_J \partial \theta < 0$). Under OR, the predictions are reversed: experienced judges would dampen rather than amplify the reform's effect ($\partial^2 m^\vee / \partial p_J \partial \theta \leq 0$). The data clearly support the AND predictions. The differential response provides direct evidence of the $r \xrightarrow[p_J]{\text{decision}} d$ channel: judicial capital amplifies information improvements in a risk-dependent manner, enhancing both efficiency and targeting of judicial oversight.

Taken together, these patterns—reform gains driven by experienced judges in high-risk municipalities, new-entrant auditors reinforcing the effect of fewer ties, and $\downarrow p_A$ reducing rather than increasing concealment—are all consistent with the AND equilibrium and inconsistent with the OR equilibrium's predictions (see Table 2).

7 Conclusion

This paper shows that changing the *institutional design of information* inside an enforcement pipeline improves not only the *level* of enforcement but its *targeting*. In the context of the Italian Court of Auditors, random assignment of municipal auditors increased judicial enforcement, concentrated enforcement gains among high-risk municipalities, and improved the quality of auditor reports—all without altering the Court's formal rules or institutional structure.

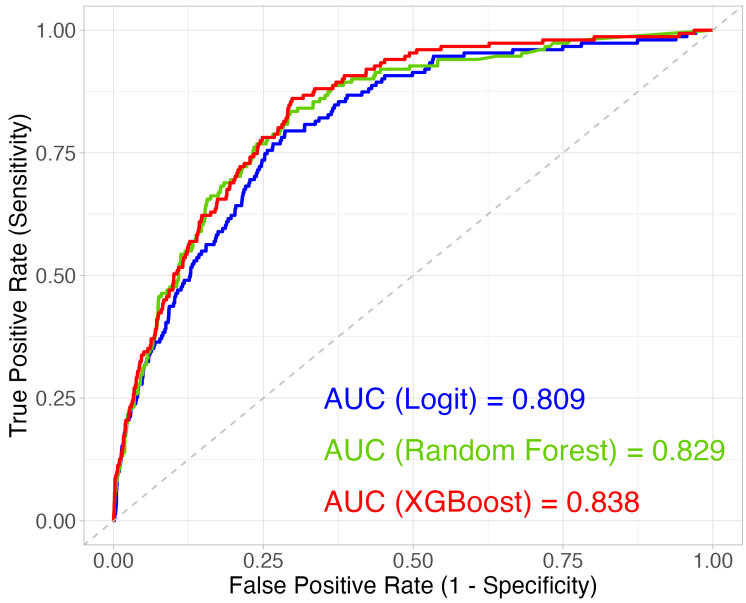
Our evidence identifies the specific points in the oversight pipeline where the reform mattered. Upstream, random assignment improved the reported signal that judges observe: auditors report more irregularities, especially where pre-reform local ties were strongest and where baseline auditor experience was weakest. The mechanism evidence points to simultaneous gains in both information production and information transmission, with new-entrant auditors driving the largest increases—a pattern consistent with the AND equilibrium of our model, where both fewer ties ($\downarrow \theta$) and lower local knowledge ($\downarrow p_A$) reduce concealment. Downstream, experienced judges amplify these information gains by concentrating

enforcement on high-risk municipalities, while less experienced judges increase enforcement less discriminately.

These patterns carry a knowledge trade-off: lower p_A raises reporting under the AND regime but reduces the welfare value of honest reports regardless of the aggregation rule. In this setting, the benefits of auditor independence dominate the costs of reduced local knowledge. The fiscal evidence reinforces this conclusion: treated municipalities achieve significant improvements in net surplus and debt repayment without cuts to current expenditures or public services, and the adjustment is achieved primarily through expanded property tax revenues—suggesting that more independent auditors compelled mayors to combat tax evasion rather than reduce spending. Importantly, the reform's effects persist for at least seven years and do not generate adverse consequences for public goods provision or local economic activity.

More broadly, our findings underscore that "optimal targeting" is not only an algorithmic problem—it is an *institutional design* problem. Strengthening independence at the monitoring stage can raise the precision of enforcement without altering judicial discretion, provided that decision-makers can recognize and act on improved signals. Reforms that shift incentives and assignments within hierarchical institutions can enhance governance by making internal communication more informative and less pliable, especially where fiscal risks are concentrated. The Italian experience demonstrates that even a relatively simple institutional change—replacing patronage with randomization in the selection of front-line monitors—can generate substantial and lasting improvements in both the information available to enforcement authorities and the quality of the decisions they make.

Figure 1: ROC Curves for Financial Distress Prediction Models



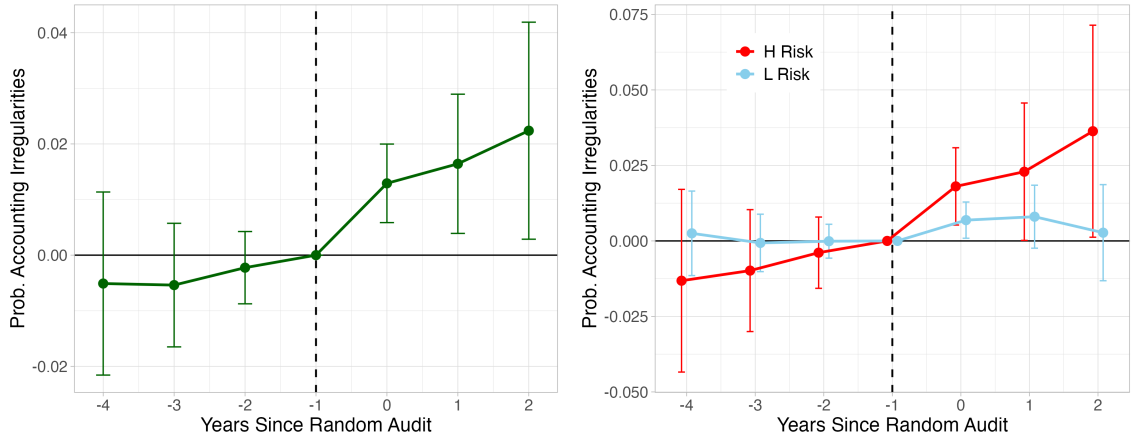
Notes. Receiver Operating Characteristic curves comparing the discriminative performance of XGBoost, Logistic Regression, and Random Forest models on the test set. Area Under the Curve (AUC) values indicate each model's ability to distinguish between municipalities that will and will not enter financial distress.

Table 1: List of Variables and Their Descriptions

#	**Variable**	**Description**
1	Off-balance sheet debts	Liabilities not covered by any revenues. Absolute value of off-balance-sheet debts over sum of current and capital expenditures.
2	Total debt service	Sum of principal and interest payments due in the year.
3	Debt limit index	Expenditures on debt repayments to total available revenues.
4	Cash advances	Short-term cash inflows used to cover liquidity shortages.
5	Budget surplus (deficit)	Ending cash balance plus active residuals, minus passive residuals and restricted funds.
6	Management balance	Total receipts minus total payments.
7	Current account balance	Ongoing revenues minus routine expenditures.
8	Capital account balance	Capital revenues minus long-term investments.
9	Passive carryovers	Outstanding payment obligations from previous periods.
10	Active carryovers	Uncollected revenues from previous periods.
11	Active carryovers to current revenues	Ratio of carryovers to current year revenues.
12	Active carryovers to total revenues	Ratio of carryovers to total revenues.
13	Total expenditures	Sum of all municipal spending.
14	Total capital expenditures	Investment spending on infrastructure and assets.
15	Total current expenditures	Operating expenses for municipal services.
16	Revenues from taxes	Income from local taxation.
17	Other revenues	Non-tax income sources.
18	Cash	Liquid assets available.
19	Financial autonomy index	Own-source revenues to total revenues.
20	Debt per capita	Total outstanding debt divided by population.
21	Investments	Capital expenditures relative to current expenditures.
22	Collecting capacity	Tax collections relative to total tax commitments.
23	Ability to pay	Total payments to total commitments.
24	Resident population	Number of residents in municipality.

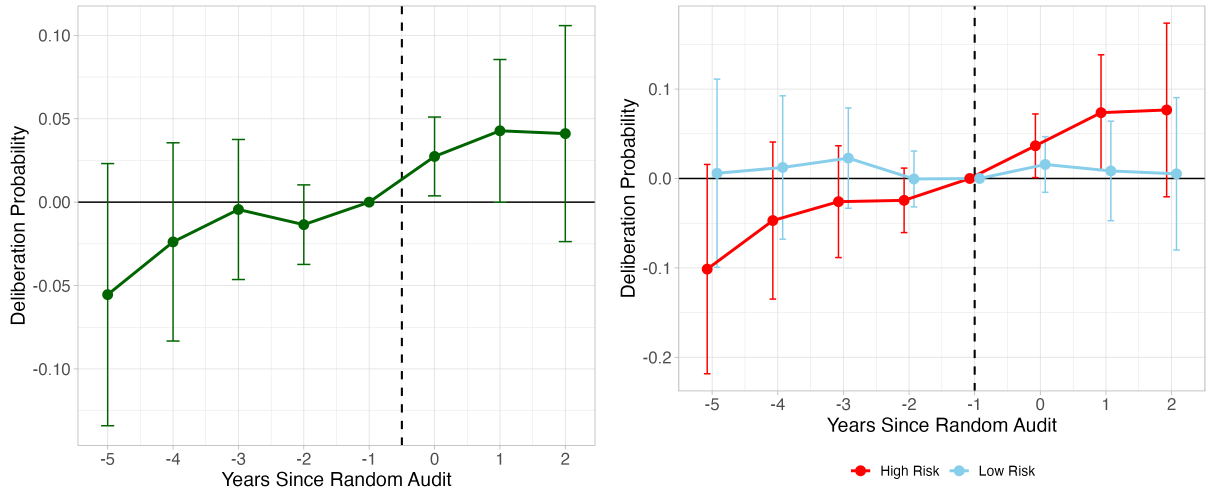
Table 2: Mapping Model Predictions to Empirical Evidence

Model Prediction	**Section**	**Empirical Finding**	**Equilibrium Support**
CS 1: $\uparrow \theta \rightarrow \uparrow m$ (both AND & OR)	§6.1	Reform increases irregularity reports by 1.12 p.p. (95%); effects concentrated where pre-reform local auditors served	Both equilibria
CS 2 (AND): $\uparrow p_A \rightarrow \uparrow m^{\wedge}$	§6.4	Effects largest for new-entrant auditors (lower p_A), consistent with lower local knowledge reinforcing fewer ties	Supports AND; OR predicts offset
CS 3 (AND): $\frac{\partial^2 m^{\wedge}}{\partial p_J \partial \theta} \geq 0$	§6.4	Experienced judges drive larger enforcement gains in high-risk municipalities (1.61 p.p., 8.2%)	Supports AND; OR predicts opposite
CS 4 (AND): Reform gains larger for experienced judges	§6.4	High-risk: experienced judges show sharp increases; low-risk: experienced judges show no response	Supports AND; OR predicts gains largest for inexperienced
Reform on m: unambiguous (AND) vs. ambiguous (OR)	§6.1, §6.2	Unambiguous increase in both reporting and enforcement	Supports AND
Court aggregation rule	§6.4	$\Pr(D{=}1 \mid r{=}1)$ falls; $\Pr(D{=}0 \mid r{=}0)$ rises post-reform	Direct evidence of AND rule



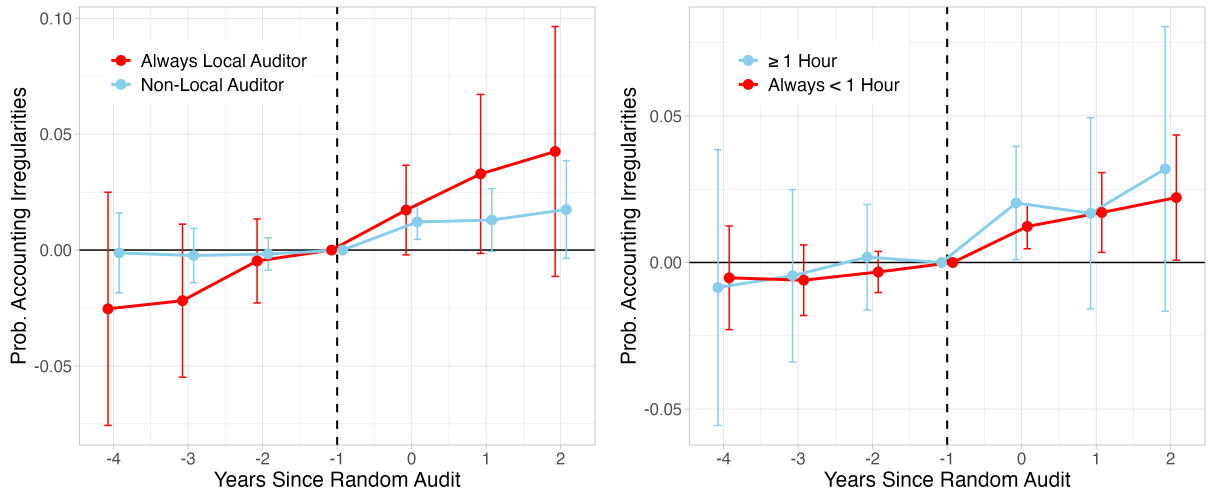
(a) **Overall Effect** (b) **By Municipal Risk Level**

Figure 2: Random Audit Effects on Accounting Irregularities Reporting
Panel (a) shows event study estimates of the causal effect of random audits on the probability that a municipal auditor reports accounting irregularities in the yearly questionnaires for the Court of Auditors. Panel (b) compares high-risk municipalities (H Risk, above median predicted financial distress, shown in red) with low-risk municipalities (L Risk, below median, shown in light blue). The reference period is $t = -1$ (one year before the first random audit). Standard errors are clustered at the municipality level. 95% confidence intervals are shown.



(a) **Overall Effect** (b) **By Municipal Risk Level**

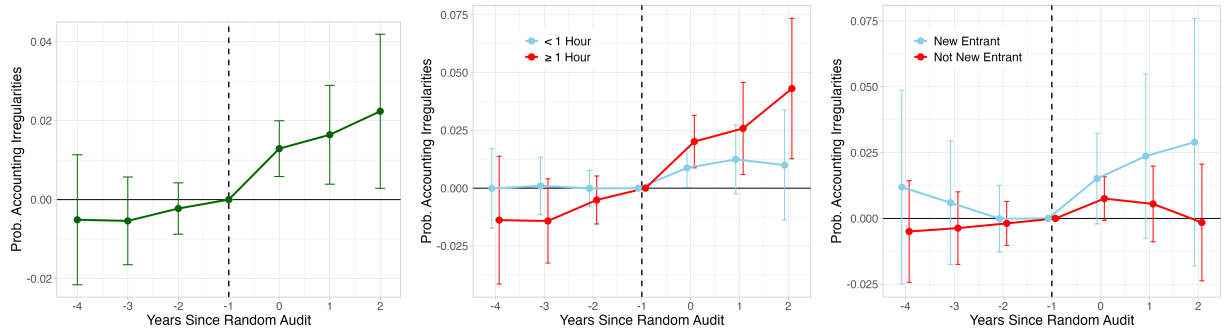
Figure 3: Random Audit Effects on Court Deliberation Probability
Panel (a) shows event study estimates of the causal effect of random audits on the probability that a municipality receives a Court deliberation. Panel (b) compares high-risk municipalities (above median predicted financial distress, shown in red) with low-risk municipalities (below median, shown in light blue). The reference period is $t = -1$ (one year before the first random audit). Standard errors are clustered at the municipality level. 95% confidence intervals are shown.



(a) **Local Auditor** (b) **Close Auditor**

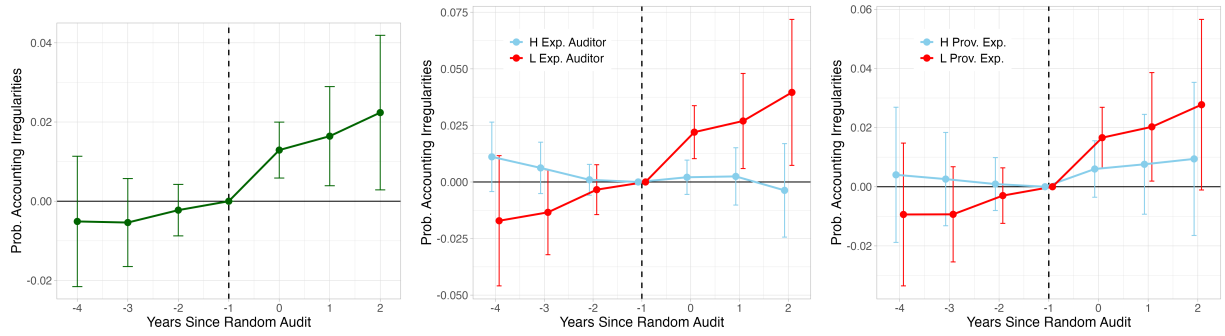
Figure 4: Irregularity Reporting: Heterogeneity by Pre-Reform Auditor Ties

Each panel plots event-study estimates of equation (5) estimated separately on two subsamples defined by the characteristics of auditors assigned *before* the reform ($t < 0$). The split is based on the full pre-reform auditor history of each municipality. Panel (a): Locality. Red: municipalities that *always* had a local auditor prior to the reform, where local means the auditor resides in the same municipality they audit (N = 5,533 obs., 976 municipalities; pre-period mean 1.55%). Light blue: municipalities with at least one non-local auditor in the pre-reform period (N = 32,162 obs., 5,490 municipalities; pre-period mean 1.13%). Panel (b): Distance. Red: municipalities that *always* had an auditor residing within 60 minutes driving time prior to the reform (N = 31,255 obs., 5,364 municipalities; pre-period mean 1.04%). Light blue: municipalities with at least one auditor beyond the 60-minute threshold (N = 6,440 obs., 1,102 municipalities; pre-period mean 1.87%). The reference period is $t = -1$. Standard errors clustered at the municipality level. Bands show 95% confidence intervals.



(a) **Overall Effect** (b) **By Auditor Distance** (c) **By New Entrant Status**

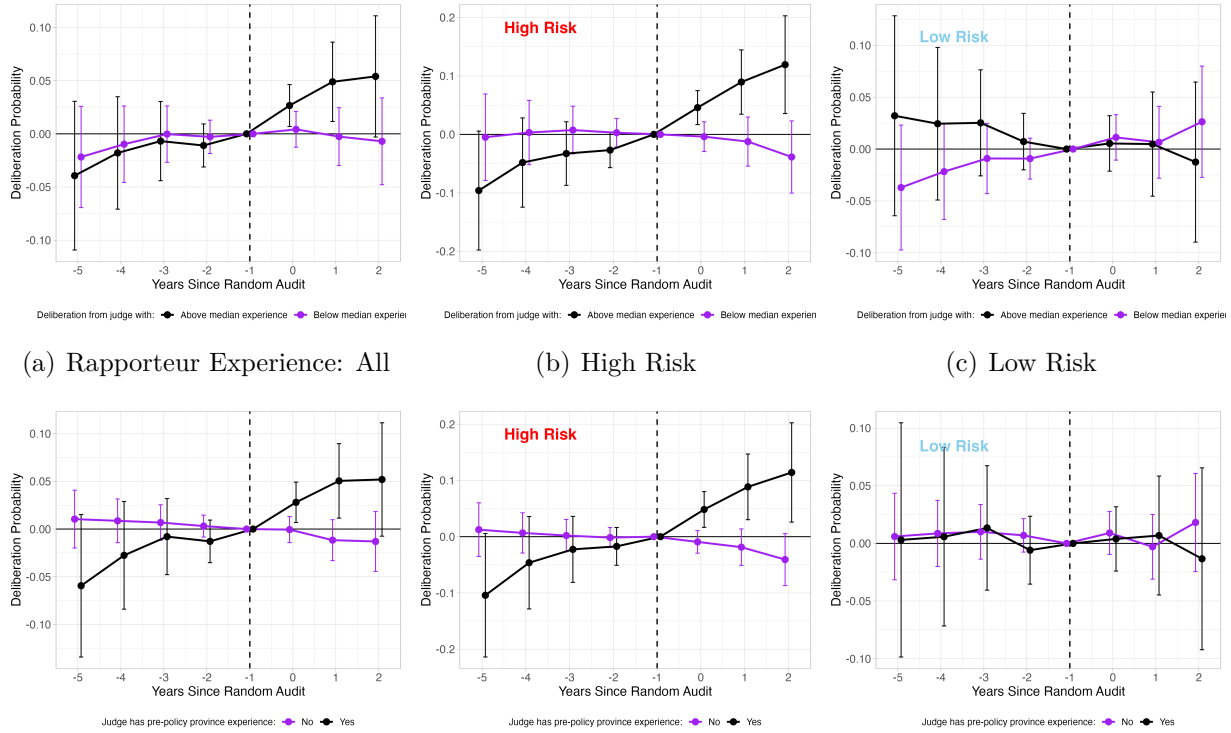
Figure 5: Auditor Independence and Professional Renewal Effects
Panel (a) shows the overall effect of random audits on the probability that auditors report accounting irregularities. Panel (b) compares effects by auditor travel distance: municipalities assigned auditors residing less than one hour away (<1 Hour, shown in light blue) versus those assigned more distant auditors (≥ 1 Hour, shown in red). Panel (c) compares effects by auditor experience: municipalities assigned experienced auditors (Not New Entrant, shown in red) versus those assigned auditors with no prior municipal experience (New Entrant, shown in light blue). The reference period is $t = -1$ (one year before the first random audit). Standard errors are clustered at the municipality level. 95% confidence intervals are shown.



(a) **Main Effect** (b) **Experience as an Auditor** (c) **Experience in Different Provinces**

Figure 6: Random Audit Effects on Accounting Irregularities Reporting: Heterogeneity by Pre-Period Auditor Experience

Panel (a) shows the main event study estimate of random audit effects on the probability that auditors report accounting irregularities. Panels (b) and (c) examine heterogeneity based on the *average characteristics of auditors assigned to a municipality in the pre-reform period* ($t < 0$). Each municipality is classified once based on the cross-sectional median of its pre-period mean. Panel (b) splits municipalities by their auditors' average experience as court auditors: red lines show municipalities whose pre-period auditors had below-median average experience (L Exp. Auditor), while light blue lines show those with above-median experience (H Exp. Auditor). Panel (c) splits by whether pre-period auditors exclusively served municipalities within a single province (L Prov. Exp., shown in red) versus those with experience across multiple provinces (H Prov. Exp., shown in light blue). The reference period is $t = -1$ (one year before the first random audit). Standard errors are clustered at the municipality level. 95% confidence intervals are shown.



(a) Rapporteur Experience: All (b) High Risk (c) Low Risk

(d) Local Experience: All (e) High Risk (f) Low Risk

Figure 7: Random Audit Effects by Judge Experience

Event study estimates of random audit effects on deliberation probability by judge experience type. Panels (a)-(c) examine rapporteur experience: experienced judges are those above the pre-period median of 44 deliberations as rapporteur (2009-2011). Judges that deliberate for the first time post 2011 are assigned experience equal to zero for both measures. Panels (d)-(f) examine local experience: experienced judges had prior experience in the municipality's province during 2009-2011 (4.2% of deliberations). Black lines show effects for experienced judges, purple lines for inexperienced judges. Each panel reports coefficients from equation 5 where the dependent variable equals 1 if the municipality received deliberations from the respective judge type. Reference period is $t = -1$. Standard errors clustered at municipality level. 95% confidence intervals shown.

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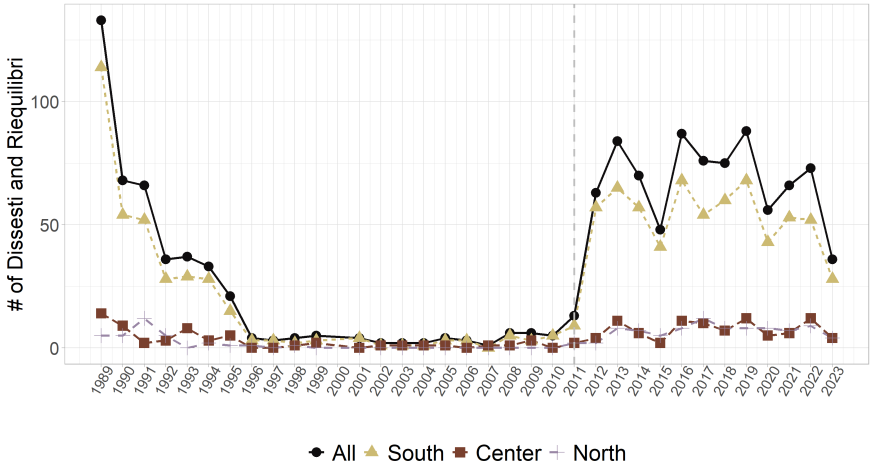
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A Appendix

Figure A1: Municipal Financial Distress Procedures Over Time



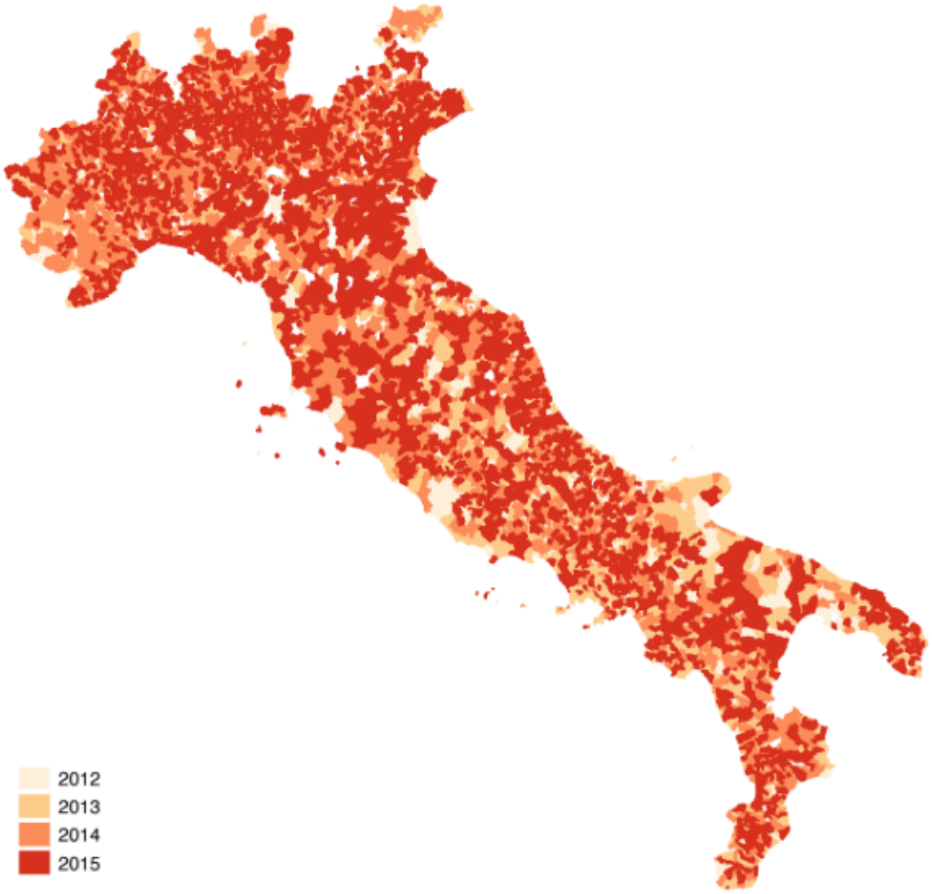
Notes. Time series of municipal financial distress procedures from 1989 to 2023, showing the number of dissesti (bankruptcy) and riequilibri (financial rebalancing) by geographic region. The black line with circles shows national totals, while triangular markers represent the South, square markers the Center, and cross markers the North.

Table A1: Municipal Characteristics that Predict Treatment Timing.

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

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

Figure A2: Staggered Treatment, Geographic Variation

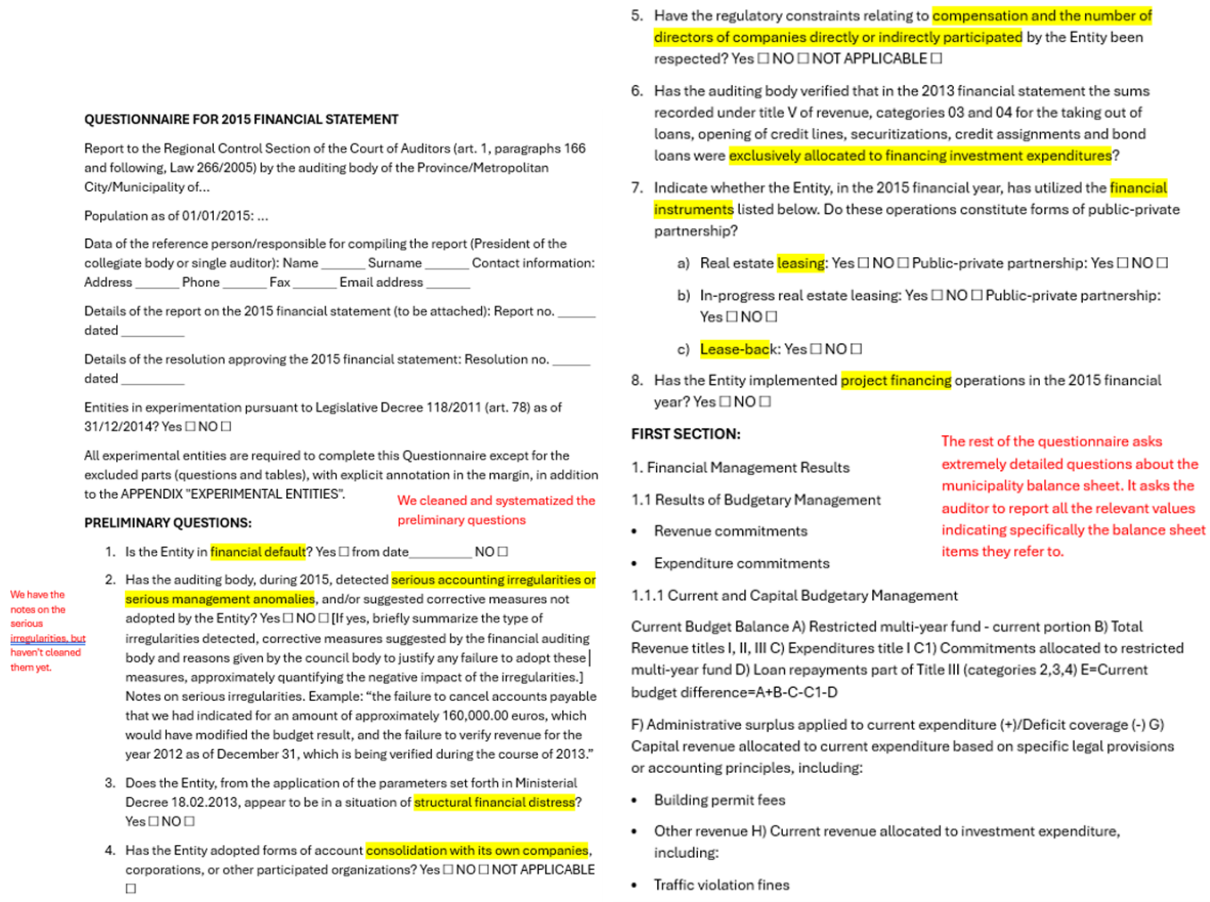


Notes. The figure shows the geographic variation in treatment timing. Darker gradation reflects later treatment timing.

Table A2: Summary Statistics for Municipal Financial Indicators

	Mean	S.D.	Median	p10	p90	p99
A. Fiscal Sustainability						
Off-balance sheet debts	0.003	0.011	0.000	0.000	0.008	0.074
Debt limit index	0.045	0.026	0.041	0.015	0.082	0.113
Cash advances	49.044	174.434	0.000	0.000	103.250	958.582
Budget surplus	171.977	311.397	77.650	5.693	404.464	1591.521
B. Revenue and Spending						
Total expenditures	7.743	1.250	7.795	7.308	8.610	9.559
Total capital expenditures	6.226	1.340	6.259	5.086	7.670	8.923
Total current expenditures	7.251	1.138	7.323	6.941	7.941	8.733
Revenues from taxes	6.331	1.092	6.459	5.666	7.140	7.920
Other revenues	5.535	1.135	5.594	4.598	6.672	7.819
Cash	412.115	655.029	216.366	4.188	921.745	3769.820
Management balance	-9.429	240.078	-0.019	-201.826	165.037	934.981
Current account balance	-53.190	216.057	-30.300	-234.473	109.857	592.055
Capital account balance	-469.742	587.142	-262.335	-1079.818	-81.160	0.000
C. Performance Indicators						
Financial autonomy	0.643	0.213	0.656	0.330	0.931	0.972
Passive carryovers	1807.409	1954.051	1114.682	441.439	3893.866	10683.087
Active carryovers	1562.628	1751.893	953.314	300.785	3480.008	9475.112
Active carryovers to current revenues	0.299	0.143	0.280	0.130	0.500	0.650
Active carryovers to total revenues	0.862	0.591	0.690	0.320	1.650	2.952
Investments	0.482	0.422	0.351	0.128	1.001	2.332
Collecting capacity	0.739	0.162	0.760	0.580	0.906	0.993
Ability to pay	0.689	0.156	0.723	0.522	0.833	0.908
Default	0.001	0.031	0.000	0.000	0.000	0.000

Notes. Summary statistics for Italian municipalities covering 2009-2011. All monetary variables are expressed in per-capita terms and winsorized at the 1% level. Expenditure variables are transformed using the inverse hyperbolic sine transformation. Sample restricted to municipalities subject to random auditor assignment.



QUESTIONNAIRE FOR 2015 FINANCIAL STATEMENT

Report to the Regional Control Section of the Court of Auditors (art. 1, paragraphs 166 and following, Law 266/2005) by the auditing body of the Province/Metropolitan City/Municipality of...

Population as of 01/01/2015: ...

Data of the reference person/responsible for compiling the report (President of the collegiate body or single auditor): Name _____ Surname _____ Contact information: Address _____ Phone _____ Fax _____ Email address _____

Details of the report on the 2015 financial statement (to be attached): Report no. _____ dated _____

Details of the resolution approving the 2015 financial statement: Resolution no. _____ dated _____

Entities in experimentation pursuant to Legislative Decree 118/2011 (art. 78) as of 31/12/2014? Yes ☐ NO ☐

All experimental entities are required to complete this Questionnaire except for the excluded parts (questions and tables), with explicit annotation in the margin, in addition to the APPENDIX "EXPERIMENTAL ENTITIES".

We cleaned and systematized the preliminary questions

PRELIMINARY QUESTIONS:

1. Is the Entity in financial default? Yes ☐ from date_____ NO ☐
2. Has the auditing body, during 2015, detected serious accounting irregularities or serious management anomalies, and/or suggested corrective measures not adopted by the Entity? Yes ☐ NO ☐ [If yes, briefly summarize the type of irregularities detected, corrective measures suggested by the financial auditing body and reasons given by the council body to justify any failure to adopt these measures, approximately quantifying the negative impact of the irregularities.] Notes on serious irregularities. Example: "the failure to cancel accounts payable that we had indicated for an amount of approximately 160,000.00 euros, which would have modified the budget result, and the failure to verify revenue for the year 2012 as of December 31, which is being verified during the course of 2013."
3. Does the Entity, from the application of the parameters set forth in Ministerial Decree 18.02.2013, appear to be in a situation of structural financial distress? Yes ☐ NO ☐
4. Has the Entity adopted forms of account consolidation with its own companies, corporations, or other participated organizations? Yes ☐ NO ☐ NOT APPLICABLE ☐
5. Have the regulatory constraints relating to compensation and the number of directors of companies directly or indirectly participated by the Entity been respected? Yes ☐ NO ☐ NOT APPLICABLE ☐
6. Has the auditing body verified that in the 2013 financial statement the sums recorded under title V of revenue, categories 03 and 04 for the taking out of loans, opening of credit lines, securitizations, credit assignments and bond loans were exclusively allocated to financing investment expenditures?
7. Indicate whether the Entity, in the 2015 financial year, has utilized the financial instruments listed below. Do these operations constitute forms of public-private partnership?
   a) Real estate leasing: Yes ☐ NO ☐ Public-private partnership: Yes ☐ NO ☐
   b) In-progress real estate leasing: Yes ☐ NO ☐ Public-private partnership: Yes ☐ NO ☐
   c) Lease-back: Yes ☐ NO ☐
8. Has the Entity implemented project financing operations in the 2015 financial year? Yes ☐ NO ☐

FIRST SECTION:

The rest of the questionnaire asks extremely detailed questions about the municipality balance sheet. It asks the auditor to report all the relevant values indicating specifically the balance sheet items they refer to.

1. Financial Management Results

1.1 Results of Budgetary Management

- Revenue commitments
- Expenditure commitments

1.1.1 Current and Capital Budgetary Management

Current Budget Balance A) Restricted multi-year fund - current portion B) Total Revenue titles I, II, III C) Expenditures title I C1) Commitments allocated to restricted multi-year fund D) Loan repayments part of Title III (categories 2,3,4) E=Current budget difference=A+B-C-C1-D

F) Administrative surplus applied to current expenditure (+)/Deficit coverage (-) G) Capital revenue allocated to current expenditure based on specific legal provisions or accounting principles, including:

- Building permit fees
- Other revenue H) Current revenue allocated to investment expenditure, including:
- Traffic violation fines

We have the notes on the serious irregularities, but haven't cleaned them yet.

Figure A3: Example pages from the Auditor Questionnaire (2010 and 2015 editions)

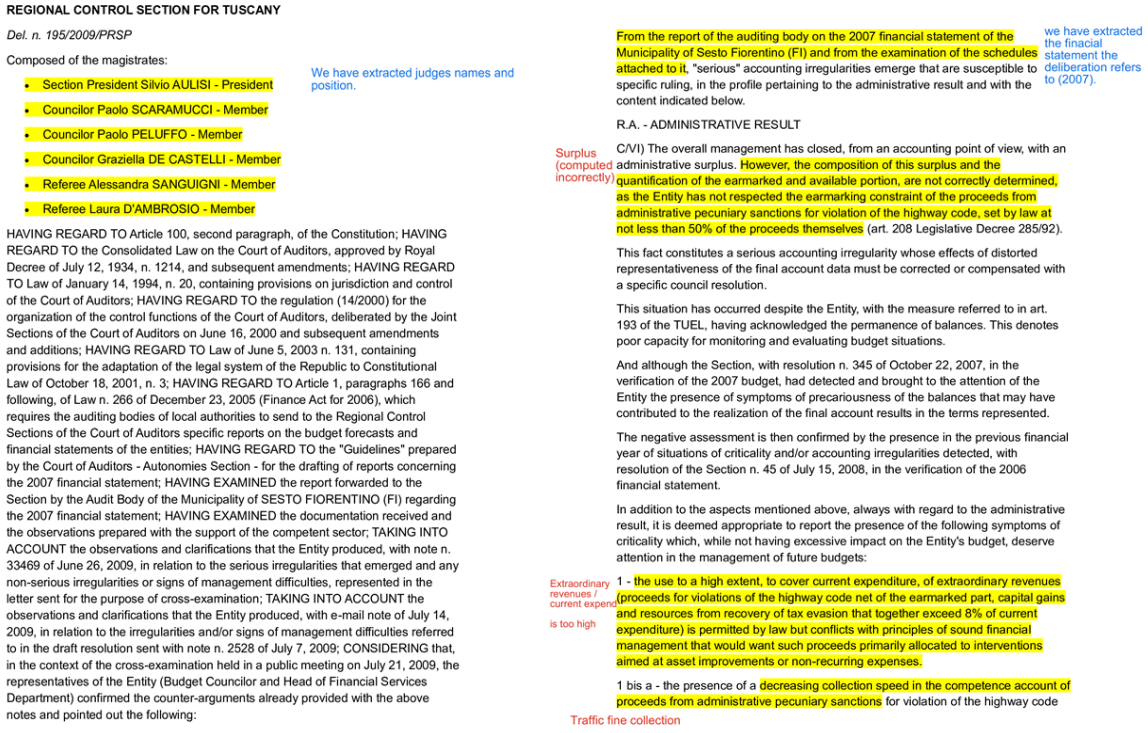


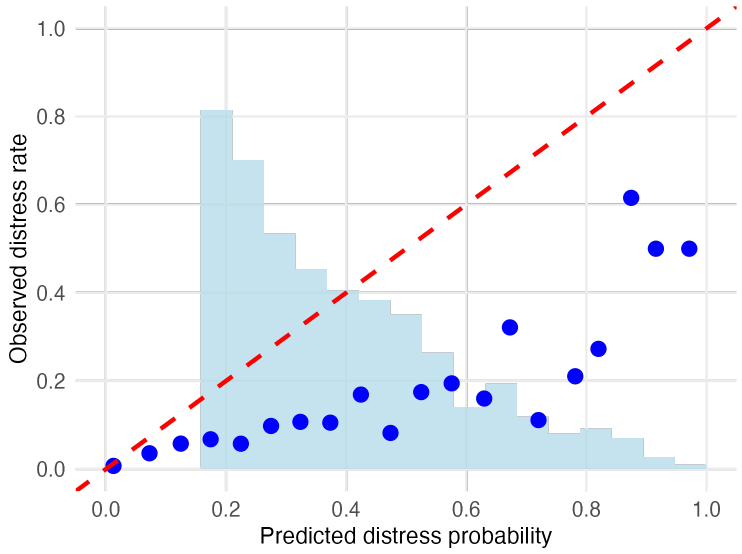
Figure A4: Excerpt from a Court of Auditors deliberation addressed to the Municipality of Sesto Fiorentino (2009)

Table A3: XGBoost Performance Across Different Classification Thresholds

Threshold	Accuracy	Precision	Recall	Specificity	F1-Score
0.10	0.716	0.113	0.821	0.711	0.199
0.15	0.768	0.126	0.742	0.769	0.215
0.20	0.804	0.137	0.675	0.810	0.228
0.25	0.839	0.156	0.623	0.849	0.249
0.30	0.860	0.166	0.563	0.874	0.257
0.35	0.879	0.178	0.503	0.896	0.263
0.40	0.896	0.194	0.450	0.916	0.271
0.45	0.909	0.200	0.371	0.934	0.260
0.50	0.924	0.233	0.338	0.950	0.276
0.55	0.936	0.256	0.265	0.966	0.261
0.60	0.942	0.275	0.219	0.974	0.244
0.65	0.947	0.305	0.192	0.980	0.236
0.70	0.949	0.299	0.132	0.986	0.183
0.75	0.953	0.367	0.119	0.991	0.180
0.80	0.957	0.467	0.093	0.995	0.155
0.85	0.958	0.579	0.073	0.998	0.129
0.90	0.957	0.500	0.020	0.999	0.038

Notes. Performance metrics calculated on test set. Accuracy = (TP+TN)/(TP+TN+FP+FN); Precision = TP/(TP+FP); Recall = TP/(TP+FN); Specificity = TN/(TN+FP); F1-Score = 2×(Precision×Recall)/(Precision+Recall). TP=True Positive, TN=True Negative, FP=False Positive, FN=False Negative.

Figure A5: Model Calibration Plot for XGBoost Financial Distress Predictions



Notes. Calibration plot evaluating the reliability of predicted probabilities from the XGBoost model. The plot combines a histogram (light blue bars) showing the distribution of predicted distress probabilities across municipalities in the test set, with calibration points (blue dots) representing the observed distress rates within binned probability ranges. Each bin contains municipalities with similar predicted probabilities, and the corresponding blue dot shows the actual proportion that experienced distress in that bin. The red dashed line represents perfect calibration, where predicted probabilities would exactly match observed rates. The plot construction uses 20 probability bins of equal width (5% each) to assess model reliability across the full probability spectrum.

Table A4: Random Audit Effects on Court Deliberation Probability

	=1 if received Court deliberation		
	All	High Risk	Low Risk
	(1)	(2)	(3)
Indep. Auditor = 1	0.0172**	0.0190	0.0164
	(0.0085)	(0.0130)	(0.0109)
R^2	0.37635	0.36193	0.36830
Observations	45,262	22,631	22,631
Municipality fixed effects	✓	✓	✓
Risk-Pop-Year fixed effects	✓	✓	✓
Dep. Var Mean	0.2731	0.3450	0.2027

Notes: Static difference-in-differences estimates of random audit effects on probability of receiving Court deliberation. Treatment = 1 for municipalities subject to random auditor assignment under the reform. Post = 1 for treatment year and after ($t \geq 0$). Column (1) shows overall effect; columns (2)-(3) split by municipal risk (above/below median predicted financial distress in the pre-period). Standard errors clustered at municipality level. Fixed effects: municipality and risk-population-year. Sample: treated municipalities, 2009-2021. Dependent variable mean calculated for pre-treatment period ($t < 0$). *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Random Audit Effects on Accounting Irregularities Reporting

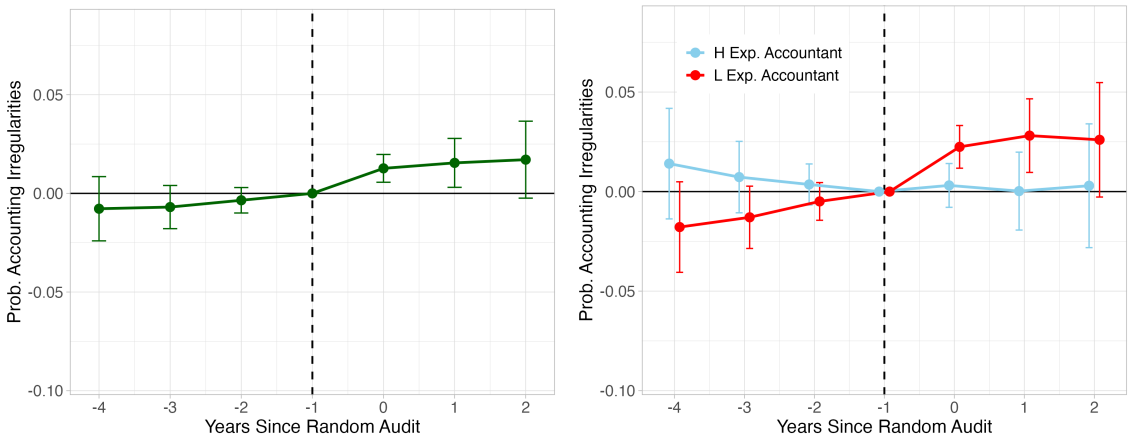
	=1 if accounting irregularities		
	All	High Risk	Low Risk
	(1)	(2)	(3)
Indep. Auditor = 1	0.0113***	0.0131**	0.0085***
	(0.0030)	(0.0052)	(0.0028)
R^2	0.34455	0.35724	0.26877
Observations	37,695	18,786	18,909
Municipality fixed effects	✓	✓	✓
Risk-Pop-Year fixed effects	✓	✓	✓
Dep. Var Mean	0.0118	0.0206	0.0033

Notes: Static difference-in-differences estimates of random audit effects on probability of reporting an accounting irregularity. Treatment = 1 for municipalities subject to random auditor assignment under the reform. Post = 1 for treatment year and after ($t \geq 0$). Column (1) shows overall effect; columns (2)-(3) split by municipal risk (above/below median predicted financial distress in the pre-period). Standard errors clustered at municipality level. Fixed effects: municipality and risk-population-year. Sample: treated municipalities, 2009-2021. Dependent variable mean calculated for pre-treatment period ($t < 0$). *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Random Audit Effects on Accounting Irregularities Reporting: Interaction with Risk

	=1 if accounting irregularities	
	Baseline	Interaction
	(1)	(2)
Indep. Auditor = 1	0.0113***	0.0032
	(0.0030)	(0.0030)
Indep. Auditor × High Risk		0.0157***
		(0.0037)
R^2	0.34455	0.34517
Observations	37,695	37,695
Municipality fixed effects	✓	✓
Risk-Pop-Year fixed effects	✓	✓
Dep. Var Mean	0.0118	0.0118

Notes: Static difference-in-differences estimates of random audit effects on the probability of reporting an accounting irregularity. Column (1) replicates the baseline specification from equation 5. Column (2) augments it with an interaction term between the post-treatment indicator and an indicator for high-risk municipalities. The estimating equation is: $Y_{it} = \beta_1 \text{Post}_{it} + \beta_2 \text{Post}_{it} \times \text{HighRisk}_i + \alpha_i + \gamma_{qpt} + \varepsilon_{it}$, where $\text{Post}_{it} = \mathbf{1}[t \geq 0]$, $\text{HighRisk}_i = \mathbf{1}[\bar{d}_i > \text{median}]$ with $\bar{d}_i$ denoting mean predicted financial distress in the pre-period. Median predicted distress probability in the pre-period is equal to 1.3%. α_i are municipality fixed effects, and γ_{qpt} are risk-quartile × 2011 population-bin × year fixed effects. β_1 captures the effect for low-risk municipalities; $\beta_1 + \beta_2$ captures the effect for high-risk municipalities. Standard errors clustered at the municipality level. Sample: treated municipalities, 2010–2015. Dependent variable mean calculated over the pre-treatment period ($t < 0$). *** p<0.01, ** p<0.05, * p<0.1.



(a) **Main Effect** (b) **Experience as an Accountant**

Figure A6: Random Audit Effects: Accounting Experience Heterogeneity
Panel (a) shows the main event study estimate of random audit effects on accounting irregularities reporting probability. Panel (b) examines heterogeneity based on the general accounting experience of the *last auditor before the reform* (measured at $t = -1$). The analysis splits municipalities by their last auditor's general accounting experience (median: 17 years). Red lines show municipalities whose last auditor had below-median accounting experience (L), while light blue lines show those with above-median experience (H). The reference period is $t = -1$. Standard errors are clustered at the municipality level. 95% confidence intervals are shown.

B Model: Detailed Derivations

This appendix provides the complete formal development of the model introduced in Section 3. We state all assumptions precisely, characterize equilibrium strategies, derive closed-form expressions for misreporting rates and judicial welfare, and prove every comparative static. Throughout, we maintain the notation established in the main text.

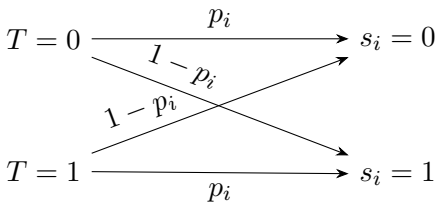
B.1 Setup

State and players. A municipality has an unobserved fiscal condition $T \in \{0, 1\}$, where $T = 0$ denotes fiscal soundness and $T = 1$ denotes fiscal irregularities. The prior probability of irregularities is $\pi \equiv \Pr(T = 1) \in (0, 1)$. There are two players: an auditor (A) who investigates and reports, and a judge (J) who decides on enforcement.

Information structure. Each player $i \in \{A, J\}$ receives a private signal $s_i \in \{0, 1\}$ through a binary symmetric channel with precision $p_i \in [\frac{1}{2}, 1]$:

$$s_i = \begin{cases} T, & \text{with probability } p_i, \\ 1 - T, & \text{with probability } 1 - p_i. \end{cases}$$

When $p_i = \frac{1}{2}$ the signal is uninformative; when $p_i = 1$ it is perfectly revealing. Signals are conditionally independent across players given T.



After observing s_A, the auditor sends a report $r \in \{0, 1\}$ to the judge. This report is the only strategic message in the model. The judge then observes (r, s_J) and chooses the enforcement decision $d \in \{0, 1\}$.

Local ties and auditor payoff. Let $\tau \in \{0, 1\}$ indicate whether the auditor has local ties to the municipality ($\tau = 1$) or not ($\tau = 0$), with $\theta \equiv \Pr(\tau = 1)$. Conditional on $\tau = 1$, the auditor draws a bias $b > 0$ from an absolutely continuous distribution F with density f supported on $(0, \infty)$. Both τ and b are independent of T.

The auditor's utility is

$$u_A(d, r, s_A; \tau, b) = \tau b \, \mathbf{1}_{\{d=0\}} - \mathbf{1}_{\{r \neq s_A\}}. \tag{1}$$

The first term captures the tied auditor's preference for no enforcement; the second is the cost of misreporting, reflecting career or reputational concerns.

[Generality of the payoff normalization] The unit normalization of the misreporting cost is without loss of generality. Consider the more general payoff $\tau b \, \mathbf{1}_{\{d=0\}} - c \, \mathbf{1}_{\{r \neq s_A\}}$ with $c > 0$ and (b, c) drawn from some joint distribution G. Since $c > 0$, dividing through by c yields $\tau \tilde{b} \, \mathbf{1}_{\{d=0\}} - \mathbf{1}_{\{r \neq s_A\}}$ where $\tilde{b} \equiv b/c \in (0, \infty)$. Letting F denote the marginal distribution of $\tilde{b}$ recovers our formulation.

Judge's payoff. The judge minimizes a weighted loss from Type I and Type II errors. Normalizing payoffs from correct decisions to zero:

$$u_J(d, T) = -c_1 \, \mathbf{1}_{\{d=1,\, T=0\}} - c_2 \, \mathbf{1}_{\{d=0,\, T=1\}}, \tag{2}$$

where $c_1 > 0$ is the cost of initiating enforcement against a sound municipality (Type I error) and $c_2 > 0$ is the cost of failing to enforce when irregularities are present (Type II error). Dividing by $c_1 + c_2$ and defining $\kappa \equiv c_1/(c_1 + c_2)$, we obtain the normalized payoff

$$u_J(d, T) = -\kappa \, \mathbf{1}_{\{d=1,\, T=0\}} - (1 - \kappa) \, \mathbf{1}_{\{d=0,\, T=1\}}. \tag{3}$$

Judge's decision rule. Given information (r, s_J), let $\mu(r, s_J) \equiv \Pr(T = 1 \mid r, s_J)$ denote the judge's posterior. The expected loss from $d = 1$ is $c_1(1 - \mu)$ and from $d = 0$ is $c_2\mu$, so the judge initiates enforcement if and only if

$$c_1(1 - \mu) \leq c_2\mu \quad \Longleftrightarrow \quad \mu \geq \kappa \equiv \frac{c_1}{c_1 + c_2}.$$

A lower κ (greater concern about Type II errors) makes enforcement more likely.

Timing.

1. Nature draws $T \in \{0, 1\}$ and the auditor's type (τ, b).
2. The auditor privately observes s_A and sends report $r \in \{0, 1\}$ to the judge.
3. The judge privately observes s_J, sees report r, and chooses $d \in \{0, 1\}$.

B.2 Preliminary Definitions and Assumptions

Posterior beliefs. By Bayes' rule, the auditor's posterior upon observing s_A is

$$\mu_1^A(p_A, \pi) \equiv \Pr(T = 1 \mid s_A = 1) = \frac{p_A \pi}{p_A \pi + (1 - p_A)(1 - \pi)}, \tag{4}$$

$$\mu_0^A(p_A, \pi) \equiv \Pr(T = 1 \mid s_A = 0) = \frac{(1 - p_A)\pi}{(1 - p_A)\pi + p_A(1 - \pi)}. \tag{5}$$

Signal agreement probabilities. For the analysis of both equilibria, it is useful to define the conditional probability that the judge's signal agrees with the auditor's:

$$\Gamma_1(p_A, p_J, \pi) \equiv \Pr(s_J = 1 \mid s_A = 1) = p_J \mu_1^A + (1 - p_J)(1 - \mu_1^A), \tag{6}$$

$$\Gamma_0(p_A, p_J, \pi) \equiv \Pr(s_J = 0 \mid s_A = 0) = p_J(1 - \mu_0^A) + (1 - p_J)\mu_0^A. \tag{7}$$

These are obtained by conditioning on T and using conditional independence. The quantity Γ_1 plays a central role: it measures the probability that the judge would independently corroborate a positive auditor signal.

After observing $s_A = 1$, irregularities are weakly more likely than no irregularities:

$$\mu_1^A \equiv \Pr(T = 1 \mid s_A = 1) \geq \tfrac{1}{2}.$$

This is equivalent to $\pi \geq 1 - p_A$, a mild condition requiring that the prior is not too low relative to the auditor's signal precision.

If the judge observed both signals directly, her optimal decision would be unambiguous:

$$\Pr(T = 1 \mid s_A = 1, s_J = 1) \geq \kappa, \tag{8}$$

$$\Pr(T = 1 \mid s_A = 0, s_J = 0) \leq \kappa. \tag{9}$$

Using Bayes' rule and conditional independence, these are equivalent to the likelihood-ratio conditions

$$\frac{p_A}{1 - p_A} \cdot \frac{p_J}{1 - p_J} \cdot \frac{\pi}{1 - \pi} \geq \frac{\kappa}{1 - \kappa}, \tag{10}$$

$$\frac{1 - p_A}{p_A} \cdot \frac{1 - p_J}{p_J} \cdot \frac{\pi}{1 - \pi} \leq \frac{\kappa}{1 - \kappa}. \tag{11}$$

[No degenerate equilibria] Under Assumption B.2, there is no equilibrium in which the judge always acquits or always convicts.

Always-acquit. Suppose $d = 0$ for every (r, s_J). Since d does not depend on r, the auditor

strictly prefers to avoid the misreporting cost and reports truthfully: $r = s_A$. At the on-path history $(r = 1, s_J = 1)$,

$$\Pr(T = 1 \mid r = 1, s_J = 1) = \Pr(T = 1 \mid s_A = 1, s_J = 1) \geq \kappa,$$

by (8). The judge's best response is $d = 1$, a contradiction.

Always-convict. Suppose $d = 1$ for every (r, s_J). Again, the auditor reports truthfully. At $(r = 0, s_J = 0)$,

$$\Pr(T = 1 \mid r = 0, s_J = 0) = \Pr(T = 1 \mid s_A = 0, s_J = 0) \leq \kappa,$$

by (9). The judge's best response is $d = 0$, a contradiction.

The auditor's signal is more precise than the judge's: $p_A > p_J$.

[No judge-only equilibrium] Under Assumption B.2, there is no equilibrium in which the judge ignores the auditor's report and decides based solely on s_J.

Consider a candidate equilibrium in which $d = \mathbf{1}_{\{s_J=1\}}$, ignoring r. Since the report is irrelevant, the auditor reports truthfully: $r = s_A$. The judge's strategy requires

$$\Pr(T = 1 \mid s_J = 0) \leq \kappa \quad \text{and} \quad \Pr(T = 1 \mid s_J = 1) \geq \kappa.$$

For the judge to rationally ignore the truthful report, it must be the case that the auditor's report never tips the decision. In particular, we need

$$\Pr(T = 1 \mid s_A = 1, s_J = 0) \leq \kappa \quad \text{and} \quad \Pr(T = 1 \mid s_A = 0, s_J = 1) \geq \kappa.$$

Defining $q_i \equiv p_i/(1 - p_i)$ for $i \in \{A, J\}$, these conditions become

$$q_A \cdot \frac{1}{q_J} \cdot \frac{\pi}{1 - \pi} \leq \frac{\kappa}{1 - \kappa} \quad \text{and} \quad \frac{1}{q_A} \cdot q_J \cdot \frac{\pi}{1 - \pi} \geq \frac{\kappa}{1 - \kappa}.$$

Combining the two inequalities yields $q_A^2 \leq q_J^2$, i.e., $p_A \leq p_J$, which contradicts $p_A > p_J$.

[No babbling] Unlike standard cheap-talk models, this framework does not admit a babbling equilibrium. The misreporting cost in (1) ensures that if the judge ignores the report, the auditor reports truthfully. But when $p_A > p_J$, a truthful report is valuable to the judge, so she cannot rationally ignore it.

B.3 Conservative ("AND") Equilibrium

We characterize the equilibrium in which the judge enforces if and only if both her own signal and the auditor's report indicate irregularities:

$$d = 1 \quad \Longleftrightarrow \quad (r = 1 \text{ and } s_J = 1).$$

B.3.1 Auditor's Optimality

Case $s_A = 0$. If the auditor reports $r = 0$ (truthfully), then $d = 0$ regardless of s_J, yielding payoff τb. If she reports $r = 1$ (lying), she incurs the misreporting cost and $d = 0$ occurs only when $s_J = 0$, yielding $-1 + \tau b \Pr(s_J = 0 \mid s_A = 0) = -1 + \tau b \Gamma_0$. Since $\Gamma_0 \leq 1$, the payoff from lying is strictly lower: $-1 + \tau b \Gamma_0 < \tau b$. The auditor truthfully reports $r = 0$.

Case $s_A = 1$. If the auditor reports $r = 0$ (lying), she gets $\tau b - 1$ since enforcement is blocked regardless of s_J. If she reports $r = 1$ (truthfully), enforcement occurs when $s_J = 1$, so her payoff is $\tau b(1 - \Gamma_1)$, where $\Gamma_1 = \Pr(s_J = 1 \mid s_A = 1)$. The auditor lies if and only if

$$\tau b - 1 \geq \tau b(1 - \Gamma_1) \quad \Longleftrightarrow \quad \tau b \Gamma_1 \geq 1 \quad \Longleftrightarrow \quad \tau b \geq \frac{1}{\Gamma_1}.$$

For untied auditors ($\tau = 0$), this condition never holds; they always report truthfully. For tied auditors ($\tau = 1$), it holds when $b \geq 1/\Gamma_1$.

Summary. The auditor's equilibrium strategy is:

$$r = \begin{cases} 0, & \text{if } s_A = 0, \\ 0, & \text{if } s_A = 1,\ \tau = 1,\ \text{and } b \geq 1/\Gamma_1, \\ 1, & \text{if } s_A = 1 \text{ otherwise.} \end{cases} \tag{12}$$

The conditional misreporting rate is

$$m^{\wedge}(\theta, p_A, p_J, \pi) \equiv \Pr(r = 0 \mid s_A = 1) = \theta \left[1 - F\left(\frac{1}{\Gamma_1(p_A, p_J, \pi)} \right) \right]. \tag{13}$$

B.3.2 Judge's Optimality

The judge convicts if and only if $\Pr(T = 1 \mid r, s_J) \geq \kappa$, which can be expressed via likelihood ratios:

$$\frac{\Pr(r \mid T = 1)}{\Pr(r \mid T = 0)} \cdot \frac{\Pr(s_J \mid T = 1)}{\Pr(s_J \mid T = 0)} \cdot \frac{\pi}{1 - \pi} \geq \frac{\kappa}{1 - \kappa}.$$

Report likelihood ratios. Marginalizing over s_A:

$$\Pr(r \mid T=1) = \Pr(r \mid s_A=1)\, p_A + \Pr(r \mid s_A=0)\,(1-p_A),$$
$$\Pr(r \mid T=0) = \Pr(r \mid s_A=1)\,(1-p_A) + \Pr(r \mid s_A=0)\, p_A.$$

Since $\Pr(r=1 \mid s_A=0)=0$ (the auditor never lies upward) and $\Pr(r=1 \mid s_A=1)=1-m^{\wedge}$, we obtain

$$\Pr(r=1 \mid T=1) = (1-m^{\wedge})p_A, \qquad \Pr(r=1 \mid T=0) = (1-m^{\wedge})(1-p_A).$$

Crucially, the misreporting rate cancels in the ratio:

$$\frac{\Pr(r=1 \mid T=1)}{\Pr(r=1 \mid T=0)} = \frac{p_A}{1-p_A}. \tag{14}$$

For $r=0$:

$$\Pr(r=0 \mid T=1) = 1-(1-m^{\wedge})p_A, \qquad \Pr(r=0 \mid T=0) = p_A + (1-p_A)m^{\wedge},$$

so

$$\frac{\Pr(r=0 \mid T=1)}{\Pr(r=0 \mid T=0)} = \frac{1-(1-m^{\wedge})p_A}{p_A+(1-p_A)m^{\wedge}}. \tag{15}$$

Equilibrium conditions. For the AND rule to be consistent with the judge's optimality, four conditions must hold:

(i) At $(r=1, s_J=1)$, the judge must prefer $d=1$:

$$\frac{p_A}{1-p_A}\cdot\frac{p_J}{1-p_J}\cdot\frac{\pi}{1-\pi} \geq \frac{\kappa}{1-\kappa}. \tag{16}$$

This is simply condition (10) from Assumption B.2.

(ii) At $(r=1, s_J=0)$, the judge must prefer $d=0$:

$$\frac{p_A}{1-p_A}\cdot\frac{1-p_J}{p_J}\cdot\frac{\pi}{1-\pi} \leq \frac{\kappa}{1-\kappa}. \tag{17}$$

(iii) At $(r=0, s_J=1)$, the judge must prefer $d=0$:

$$\frac{1-(1-m^{\wedge})p_A}{p_A+(1-p_A)m^{\wedge}}\cdot\frac{p_J}{1-p_J}\cdot\frac{\pi}{1-\pi} \leq \frac{\kappa}{1-\kappa}. \tag{18}$$

(iv) At $(r = 0, s_J = 0)$, the judge must prefer $d = 0$:

$$\frac{1-(1-m^\wedge)p_A}{p_A+(1-p_A)m^\wedge}\cdot\frac{1-p_J}{p_J}\cdot\frac{\pi}{1-\pi}\leq\frac{\kappa}{1-\kappa}. \tag{19}$$

Condition *(iv)* is implied by *(iii)* since $(1-p_J)/p_J \leq p_J/(1-p_J)$ for $p_J \geq \frac{1}{2}$. The binding constraints are *(i)*, *(ii)*, and *(iii)*.

B.3.3 Equilibrium Payoff

We compute the judge's expected payoff by deriving the Type I and Type II error probabilities.

Type I error probability. Under AND, $d = 1$ requires $r = 1$ and $s_J = 1$. When $T = 0$:

$$\begin{aligned}\Pr(d=1\mid T=0) &= \Pr(d=1\mid T=0, s_A=0)\ \Pr(s_A=0\mid T=0)\\ &\quad+\Pr(d=1\mid T=0, s_A=1)\ \Pr(s_A=1\mid T=0)\\ &= 0\cdot p_A+\Pr(r=1\mid s_A=1)\ \Pr(s_J=1\mid T=0)\,(1-p_A)\\ &= (1-m^\wedge)(1-p_J)(1-p_A).\end{aligned}$$

Type II error probability. When $T = 1$, $d = 0$ occurs unless both $r = 1$ and $s_J = 1$:

$$\begin{aligned}\Pr(d=0\mid T=1) &= \Pr(s_A=0\mid T=1)+\Pr(d=0\mid T=1, s_A=1)\ \Pr(s_A=1\mid T=1)\\ &= (1-p_A)+\left[1-p_J(1-m^\wedge)\right]p_A\\ &= 1-p_Ap_J(1-m^\wedge).\end{aligned}$$

The intermediate step uses: conditional on $s_A = 1$, $d = 0$ occurs when $s_J = 0$ *or* $r = 0$. By inclusion-exclusion, $\Pr(s_J = 0 \text{ or } r = 0 \mid T = 1, s_A = 1) = (1-p_J) + m^\wedge - (1-p_J)m^\wedge = 1 - p_J(1-m^\wedge)$.

Judge's expected payoff. Combining:

$$\begin{aligned}V_J^\wedge &= -\kappa(1-\pi)(1-m^\wedge)(1-p_J)(1-p_A)-(1-\kappa)\pi\left[1-p_Ap_J(1-m^\wedge)\right]\\ &= (1-m^\wedge)\underbrace{\left[(1-\kappa)\pi p_Ap_J-\kappa(1-\pi)(1-p_J)(1-p_A)\right]}_{\equiv\ B^\wedge}-(1-\kappa)\pi.\end{aligned} \tag{20}$$

Note that $B^\wedge \geq 0$ is equivalent to condition (16). The payoff is linear in the truthful-reporting rate $(1-m^\wedge)$, with slope $B^\wedge \geq 0$: more truthful reporting improves welfare in the

AND regime.

B.3.4 Comparative Statics

[Effect of ties] An increase in the probability of local ties raises misreporting and reduces judicial welfare:

$$\frac{\partial m^{\wedge}}{\partial \theta} = 1 - F\left(\frac{1}{\Gamma_1}\right) \geq 0, \qquad \frac{\partial V_J^{\wedge}}{\partial \theta} = -\frac{\partial m^{\wedge}}{\partial \theta} B^{\wedge} \leq 0.$$

The first result follows directly from differentiating (13) with respect to θ. For the second, differentiating (20):

$$\frac{\partial V_J^{\wedge}}{\partial \theta} = -\frac{\partial m^{\wedge}}{\partial \theta} B^{\wedge}.$$

Since $\partial m^{\wedge}/\partial \theta \geq 0$ and $B^{\wedge} \geq 0$, we have $\partial V_J^{\wedge}/\partial \theta \leq 0$.

Intuition. A higher θ increases the fraction of tied auditors. Under AND, a tied auditor who observes $s_A = 1$ can block enforcement entirely by reporting $r = 0$, so more tied auditors means more suppressed positive signals. Since truthful reporting is valuable to the judge ($B^{\wedge} \geq 0$), increased misreporting reduces welfare.

[Effect of auditor signal quality] An increase in auditor precision raises misreporting. The effect on judicial welfare is ambiguous:

$$\frac{\partial m^{\wedge}}{\partial p_A} \geq 0, \qquad \frac{\partial V_J^{\wedge}}{\partial p_A} = (1 - m^{\wedge})\frac{\partial B^{\wedge}}{\partial p_A} - B^{\wedge}\frac{\partial m^{\wedge}}{\partial p_A}, \quad \text{sign ambiguous.}$$

Differentiating $m^{\wedge}$ with respect to p_A:

$$\frac{\partial m^{\wedge}}{\partial p_A} = \theta\, f\left(\frac{1}{\Gamma_1}\right) \frac{1}{\Gamma_1^2} \frac{\partial \Gamma_1}{\partial p_A}.$$

From (6) and (4):

$$\Gamma_1 = (1 - p_J) + (2p_J - 1)\mu_1^A \quad \Longrightarrow \quad \frac{\partial \Gamma_1}{\partial p_A} = (2p_J - 1)\,\frac{\partial \mu_1^A}{\partial p_A},$$

where

$$\frac{\partial \mu_1^A}{\partial p_A} = \frac{\pi(1-\pi)}{\left[p_A\pi + (1-p_A)(1-\pi)\right]^2} > 0.$$

For $p_J \geq \frac{1}{2}$, we have $\partial \Gamma_1/\partial p_A \geq 0$, so $\partial m^{\wedge}/\partial p_A \geq 0$.

For welfare, differentiating (20):

$$\frac{\partial V_J^\wedge}{\partial p_A} = -\frac{\partial m^\wedge}{\partial p_A} B^\wedge + (1 - m^\wedge)\frac{\partial B^\wedge}{\partial p_A},$$

where $\partial B^\wedge/\partial p_A = (1-\kappa)\pi p_J + \kappa(1-\pi)(1-p_J) > 0$. The first term is non-positive (more misreporting) and the second is positive (higher signal value conditional on truthful reporting), so the net sign is ambiguous.

Intuition. Higher p_A makes $s_A = 1$ more informative, raising Γ_1—the probability that $s_J = 1$ as well. Under AND, this increases the chance that a truthful report $r = 1$ leads to conviction, strengthening the tied auditor's incentive to suppress it. At the same time, conditional on truth-telling, a more precise auditor signal improves the judge's decision quality ($\partial B^\wedge/\partial p_A > 0$). The two forces pull in opposite directions.

[Interaction of ties and judge experience] The positive effect of ties on misreporting is larger for more experienced judges. The negative effect of ties on welfare is also larger:

$$\frac{\partial^2 m^\wedge}{\partial p_J \partial \theta} \geq 0, \qquad \frac{\partial^2 V_J^\wedge}{\partial p_J \partial \theta} \leq 0.$$

Misreporting cross-derivative. Differentiating $\partial m^\wedge/\partial\theta = 1 - F(1/\Gamma_1)$ with respect to p_J:

$$\frac{\partial^2 m^\wedge}{\partial p_J \partial \theta} = f\left(\frac{1}{\Gamma_1}\right)\frac{1}{\Gamma_1^2}\frac{\partial \Gamma_1}{\partial p_J}.$$

From (6): $\partial\Gamma_1/\partial p_J = 2\mu_1^A - 1 \geq 0$ by Assumption B.2. Hence $\partial^2 m^\wedge/(\partial p_J \partial\theta) \geq 0$.

Welfare cross-derivative. Differentiating $\partial V_J^\wedge/\partial\theta = -(\partial m^\wedge/\partial\theta)\, B^\wedge$ with respect to p_J:

$$\frac{\partial^2 V_J^\wedge}{\partial p_J \partial \theta} = -\frac{\partial^2 m^\wedge}{\partial p_J \partial \theta} B^\wedge - \frac{\partial m^\wedge}{\partial \theta}\frac{\partial B^\wedge}{\partial p_J},$$

where $\partial B^\wedge/\partial p_J = (1-\kappa)\pi p_A + \kappa(1-\pi)(1-p_A) > 0$. Both terms are non-positive, so $\partial^2 V_J^\wedge/(\partial p_J \partial\theta) \leq 0$.

Intuition. Under AND, the auditor has veto power over enforcement. As p_J rises, the judge is more likely to observe $s_J = 1$ conditional on $T = 1$, which makes it more valuable for a tied auditor to block enforcement by reporting $r = 0$. Hence, ties distort more when judges are experienced. For welfare, two forces compound: (i) ties cause more misreporting when p_J is high, and (ii) undistorted reports are more valuable when the judge's signal is precise ($\partial B^\wedge/\partial p_J > 0$). Both amplify the welfare cost of ties.

B.3.5 Effect of the Reform

We model the reform as a reduction in territorial familiarity θ, recognizing that θ jointly determines both the probability of ties and auditor signal precision. Let $p_A = p_A(\theta)$ with $p'_A(\theta) > 0$.

Effect on misreporting. The total derivative of the misreporting rate with respect to θ is

$$\frac{dm^\wedge}{d\theta} = \underbrace{\frac{\partial m^\wedge}{\partial \theta}}_{\geq 0} + \underbrace{\frac{\partial m^\wedge}{\partial p_A}}_{\geq 0} p'_A(\theta) > 0.$$

Both terms are non-negative: reducing θ decreases ties (direct channel) and reduces auditor precision, which lowers Γ_1 and further reduces the incentive to lie (indirect channel). The reform therefore *unambiguously reduces misreporting* under AND.

Effect on welfare.

$$\frac{dV_J^\wedge}{d\theta} = -B^\wedge \frac{dm^\wedge}{d\theta} + (1 - m^\wedge)\frac{\partial B^\wedge}{\partial p_A} p'_A(\theta).$$

The first term captures the *misreporting channel*: lower θ reduces misreporting and raises welfare ($B^\wedge \geq 0$). The second captures the *signal quality channel*: lower θ reduces p_A, decreasing the value of truthful reports. The net welfare effect is ambiguous.

B.4 Aggressive ("OR") Equilibrium

We now characterize the equilibrium in which the judge enforces whenever at least one source indicates irregularities:

$$d = 1 \quad \Longleftrightarrow \quad (r = 1 \text{ or } s_J = 1).$$

B.4.1 Auditor's Optimality

Case $s_A = 0$. If the auditor reports $r = 0$, then $d = 1$ occurs only when $s_J = 1$, so $d = 0$ with probability Γ_0, yielding payoff $\tau b \Gamma_0$. If she reports $r = 1$, then $d = 1$ regardless of s_J, yielding payoff -1. Since $\tau b \Gamma_0 \geq 0 > -1$, the auditor always reports truthfully when $s_A = 0$.

Case $s_A = 1$. If the auditor reports $r = 0$ (lying), enforcement occurs only when $s_J = 1$, which happens with probability Γ_1. Her payoff is $\tau b(1 - \Gamma_1) - 1$. If she reports $r = 1$

(truthfully), enforcement is certain, giving payoff 0. She lies when

$$\tau b(1-\Gamma_1)-1\geq 0 \quad\Longleftrightarrow\quad \tau b\geq \frac{1}{1-\Gamma_1}.$$

Since $\Gamma_1 > \frac{1}{2}$ (by Assumption B.2), the threshold $1/(1-\Gamma_1) > 1/\Gamma_1$: lying is harder to justify under OR than under AND, because it only prevents enforcement when $s_J = 0$.

Summary. The auditor's equilibrium strategy is:

$$r=\begin{cases}0, & \text{if } s_A=0,\\ 0, & \text{if } s_A=1,\ \tau=1,\ \text{and } b\geq 1/(1-\Gamma_1),\\ 1, & \text{if } s_A=1 \text{ otherwise.}\end{cases} \tag{21}$$

The conditional misreporting rate is

$$m^{\vee}(\theta,p_A,p_J,\pi)\equiv \Pr(r=0\mid s_A=1)=\theta\left[1-F\left(\frac{1}{1-\Gamma_1(p_A,p_J,\pi)}\right)\right]. \tag{22}$$

B.4.2 Judge's Optimality

For the OR rule, the equilibrium conditions are:

(i) At $(r=0, s_J=0)$, the judge must prefer $d=0$:

$$\frac{1-(1-m^{\vee})p_A}{p_A+(1-p_A)m^{\vee}}\cdot\frac{1-p_J}{p_J}\cdot\frac{\pi}{1-\pi}\leq\frac{\kappa}{1-\kappa}. \tag{23}$$

(ii) At $(r=1, s_J=1)$, the judge must prefer $d=1$:

$$\frac{p_A}{1-p_A}\cdot\frac{p_J}{1-p_J}\cdot\frac{\pi}{1-\pi}\geq\frac{\kappa}{1-\kappa}. \tag{24}$$

(iii) At $(r=0, s_J=1)$, the judge must prefer $d=1$:

$$\frac{1-(1-m^{\vee})p_A}{p_A+(1-p_A)m^{\vee}}\cdot\frac{p_J}{1-p_J}\cdot\frac{\pi}{1-\pi}\geq\frac{\kappa}{1-\kappa}. \tag{25}$$

(iv) At $(r=1, s_J=0)$, the judge must prefer $d=1$:

$$\frac{p_A}{1-p_A}\cdot\frac{1-p_J}{p_J}\cdot\frac{\pi}{1-\pi}\geq\frac{\kappa}{1-\kappa}. \tag{26}$$

Condition *(iv)* is the strongest requirement on the OR side: even when the signals disagree ($r = 1$, $s_J = 0$), the judge must still prefer to enforce.

B.4.3 Equilibrium Payoff

The judge's expected payoff in the OR equilibrium is

$$V_J^{\vee} = -\kappa(1-\pi)\left[1 - p_A p_J\right] - (1-\kappa)\pi(1-p_J)(1-p_A) + B^{\vee}\, m^{\vee}, \tag{27}$$

where

$$B^{\vee}(p_A, p_J, \pi, \kappa) \equiv \kappa(1-\pi)(1-p_A)p_J - (1-\kappa)\pi p_A(1-p_J) \leq 0, \tag{28}$$

with the sign following from condition (26). Note that $B^{\vee} \leq 0$: under OR, misreporting *reduces* the expected loss because it offsets the aggressive regime's tendency toward over-enforcement. This contrasts with AND, where $B^{\wedge} \geq 0$ and misreporting is unambiguously harmful.

B.4.4 Comparative Statics

The OR equilibrium yields comparative statics that contrast sharply with the AND case.

[Effect of ties under OR] As under AND, more ties increase misreporting and reduce judicial welfare:

$$\frac{\partial m^{\vee}}{\partial \theta} = 1 - F\left(\frac{1}{1-\Gamma_1}\right) \geq 0, \qquad \frac{\partial V_J^{\vee}}{\partial \theta} = B^{\vee}\frac{\partial m^{\vee}}{\partial \theta} \leq 0.$$

Differentiating (22) and (27) with respect to θ is immediate. The welfare sign follows from $B^{\vee} \leq 0$ and $\partial m^{\vee}/\partial\theta \geq 0$.

[Effect of auditor signal quality under OR] In contrast with AND, higher auditor precision *reduces* misreporting and *unambiguously increases* welfare:

$$\frac{\partial m^{\vee}}{\partial p_A} \leq 0, \qquad \frac{\partial V_J^{\vee}}{\partial p_A} \geq 0.$$

Differentiating (22) with respect to p_A:

$$\frac{\partial m^{\vee}}{\partial p_A} = -\theta\, f\left(\frac{1}{1-\Gamma_1}\right)\frac{1}{(1-\Gamma_1)^2}\frac{\partial \Gamma_1}{\partial p_A} \leq 0,$$

since $\partial\Gamma_1/\partial p_A \geq 0$. Higher p_A raises Γ_1, reducing $1-\Gamma_1$ (the probability of the only event where lying helps), which tightens the threshold $1/(1-\Gamma_1)$ and deters misreporting.

Intuition. Under OR, lying only avoids enforcement when $s_J = 0$. Higher p_A makes $s_J = 0$ less likely conditional on $s_A = 1$ (since Γ_1 rises), so the return to lying falls. This is opposite to AND, where higher p_A makes lying more valuable by raising the probability that a truthful report would trigger conviction.

[Interaction of ties and judge experience under OR] In contrast with AND, the effect of ties on misreporting is smaller for experienced judges, and ties are less harmful to welfare when judges are more expert:

$$\frac{\partial^2 m^\vee}{\partial p_J \partial \theta} \leq 0, \qquad \frac{\partial^2 V_J^\vee}{\partial p_J \partial \theta} \geq 0.$$

The misreporting cross-derivative is

$$\frac{\partial^2 m^\vee}{\partial p_J \partial \theta} = -f\left(\frac{1}{1-\Gamma_1}\right) \frac{1}{(1-\Gamma_1)^2} \frac{\partial \Gamma_1}{\partial p_J} \leq 0,$$

since $\partial \Gamma_1 / \partial p_J = 2\mu_1^A - 1 \geq 0$ by Assumption B.2. For welfare, an analogous calculation shows $\partial^2 V_J^\vee / (\partial p_J \partial \theta) \geq 0$.

Intuition. Under OR, misreporting matters only when $s_J = 0$. As p_J rises, $s_J = 0$ becomes less likely conditional on $T = 1$, so the "window" in which lying affects outcomes shrinks. Hence ties are less damaging when judges are experienced—the opposite of the AND prediction.

B.4.5 Effect of the Reform

Under OR, lowering θ has an *ambiguous* effect on misreporting:

$$\frac{dm^\vee}{d\theta} = \underbrace{\frac{\partial m^\vee}{\partial \theta}}_{\geq 0} + \underbrace{\frac{\partial m^\vee}{\partial p_A}}_{\leq 0} p_A'(\theta).$$

The direct effect (fewer ties) reduces misreporting, but the indirect effect (lower p_A from loss of local knowledge) *increases* misreporting under OR by reducing Γ_1 and thus lowering the threshold $1/(1-\Gamma_1)$. These forces work in opposite directions, so the net effect depends on magnitudes. This contrasts starkly with AND, where both channels reinforce each other.

B.5 Summary: Contrasting Predictions

Table B7 summarizes the comparative statics across the two equilibria. The key discriminating predictions concern (i) the interaction between ties and judge experience ($\partial^2 m / \partial p_J \partial \theta$),

(ii) the effect of auditor signal quality on misreporting ($\partial m/\partial p_A$), and (iii) whether the reform unambiguously reduces misreporting. These opposing signs allow us to use the empirical patterns to discriminate between the two enforcement regimes.

Table B7: Comparative statics: AND versus OR equilibrium

Comparative static	AND	OR
$\partial m/\partial\theta$ (ties $\rightarrow$ misreporting)	≥ 0	≥ 0
$\partial V_J/\partial\theta$ (ties $\rightarrow$ welfare)	≤ 0	≤ 0
$\partial m/\partial p_A$ (auditor quality $\rightarrow$ misreporting)	≥ 0	≤ 0
$\partial^2 m/(\partial p_J \partial\theta)$ (ties $\times$ judge exp.)	≥ 0	≤ 0
$\partial^2 V_J/(\partial p_J \partial\theta)$ (ties $\times$ judge exp. on welfare)	≤ 0	≥ 0
Reform effect on misreporting	unambiguous $\downarrow$	ambiguous