Organized Crime, Public Procurement and Firms

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Abstract

How do transparency requirements affect firms with suspected criminal ties? To study this question, I leverage Italy's 2013 anti-mafia reform, which mandated police vetting for firms bidding on public contracts. Using detailed procurement records and newly collected police investigation data, I flag suspected firms that systematically avoid vetting despite high predicted bidding activity. The reform hampers these firms' ability to secure public contracts. They contract in size, shift business away from procurement, and, conditional on winning, secure lower-value contracts—possibly to avoid scrutiny. At the local level, procurement becomes more competitive and less geographically concentrated, with increased entry and more contracts awarded to out-of-province firms. Despite this shift away from local contractors, I find no adverse effects on local employment or income. In the most affected areas, new firm creation actually increases, suggesting the reform stimulates entrepreneurship rather than harming local economies.

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1 — Introduction

Investment in the legal economy is a defining feature of modern organized crime. Criminal organizations are estimated to reinvest up to 70% of their proceeds—more than USD 1.4 billion annually—into legal businesses that provide cover, diversification, and access to state resources (UNODC 2011; Europol 2024). As these organizations become increasingly sophisticated at infiltrating legal markets (Mirenda, Mocetti, and Rizzica 2022), they impose substantial economic and social costs by undermining competition (Slutzky and Zeume 2024), facilitating illegal activities (Sviatschi 2022; Blattman et al. 2021), and hindering economic growth (Pinotti 2015a, 2015b).

Policymakers worldwide have responded to this challenge through a combination of enforcement and prevention. Repressive strategies rely on direct enforcement against criminal organizations and infiltrated institutions (Colonnelli and Prem 2022; Fenizia and Saggio 2024). While such interventions can effectively punish detected criminal activity, they are costly, limited by finite investigative capacity, and frequently escalate violence as criminal groups retaliate (Lindo and Padilla-Romo 2018; Dell 2015). A complementary approach uses transparency requirements in the form of disclosure, monitoring, and screening. The effectiveness of these preventive strategies is *ex ante* ambiguous. On the one hand, by mandating openness and subjecting market participants to scrutiny, transparency policies raise the expected costs of criminal infiltration and encourage self-selection out of legal markets. On the other hand, if disclosure rules can be circumvented or if monitoring lacks credibility, they may fail to deter criminal participation while imposing compliance costs on legitimate firms. Concrete implementations span multiple domains and have gained global traction over the last decade: beneficial ownership registries eliminate corporate anonymity, financial monitoring systems flag suspicious transactions, and procurement vetting excludes compromised firms from public contracts.[1] Yet despite this widespread adoption, empirical evidence on whether transparency requirements actually reduce organized crime infiltration remains scarce. This paper addresses this gap by developing a machine-learning approach to identify firms with suspected criminal ties and exploiting a major Italian procurement reform to study transparency's causal effects on criminal infiltration and market outcomes.

1. International institutions like the World Bank, the United Nations and agencies under the U.S. Federal Acquisition Regulation maintain systematic vendor vetting processes. Notable implementations include the Corporate Transparency Act, EU AML directives (4th and 5th), and banking KYC standards.

The Italian context provides an ideal setting to answer these questions. First, organized crime represents a substantial challenge in Italy: the Italian Parliament's Antimafia Commission estimated that the total turnover of endogenous organized crime in Italy valued at 150 billion Euros in 2012, around 9% of GDP (Antimafia 2012). Second, precisely because of the magnitude of this issue, Italy has developed one of the most comprehensive legislative frameworks to combat organized crime. A key element of this framework is the 2013 amendment to the Antimafia Information Law, which significantly increased legality transparency requirements for firms participating in public procurement contracts. The policy made police vetting a condition for market access: firms seeking to bid on public tenders must undergo screening for organized crime connections. Successfully screened firms are listed on publicly available vendors registries (*white lists*).

Evaluating the implications of transparency requirements—particularly on criminally infiltrated firms—presents two key empirical challenges. First, organized crime infiltration is inherently difficult to detect. Most existing indicators measure infiltration at aggregate geographic levels, such as provinces or regions. Even when firm-level information is available, it focuses on firms that have already been detected and prosecuted. The second challenge is establishing causal effects. For example, both firms' compliance with transparency mandates and criminal infiltration might respond to underlying economic conditions like market opportunities or financial distress. Such joint determination would lead to a spurious correlation between infiltration status and transparency requirements.

To tackle the measurement challenge, I develop a three-step methodology that combines new data collection with machine learning techniques. First, I build a unique dataset covering 14 years (2008–2022), hand-collecting historical white lists of firms vetted by local anti-mafia police from 30 provincial offices.[2] I extract and classify information from over 5,000 pages of official records to identify around 30,000 vetted firms between 2013 and 2022, and merge this with firm-level balance sheets, income statements, and detailed data on procurement activity. Second, I train a gradient boosting classifier on pre-policy data (2008–2012) to predict each firm's likelihood of participating in public procurement absent the vetting requirement. The model

2. The 30 provincial offices account for 20% of Italy's GDP and encompass 62% (236 of 378) of municipalities where, since 1991, local governments have been dissolved due to confirmed mafia connections.

incorporates more than 50 firm-level metrics spanning performance, leverage, liquidity, size, age, and procurement history—including bidding activity, contracts won, and contract values—to isolate firms with strong economic incentives to bid. Trained on purely pre-period data, the model correctly predicts procurement participation for nearly 90% of the firms, demonstrating strong reliability in identifying which firms would naturally be expected to bid. Third, I classify as suspected of having ties to organized crime those firms with high predicted participation probabilities that are systematically absent from white lists in the post-reform period. These suspected firms are identified as those with strong expected net benefits from procurement participation, yet who systematically avoid—or fail to obtain—vetting clearance.

To address the identification challenge, I employ a difference-in-differences strategy leveraging the introduction of the 2013 transparency reform. My main specification compares outcomes between firms suspected of organized crime ties and non-suspected firms that were incumbent in the public procurement market at the onset of the policy. The specification includes firm fixed effects to control for time-invariant firm characteristics. Additionally, I group firms into 10-percentage-point bins based on their predicted pre-policy procurement participation probabilities and include bin-by-year-by-province fixed effects. This design ensures that comparisons are made exclusively among firms with nearly identical procurement participation likelihoods within the same province and year, effectively controlling for local economic conditions, market opportunities, and time-varying factors that might simultaneously affect both firm performance and procurement behavior. The key identifying assumption is that, conditional on these highly restrictive fixed effects, unobserved factors influencing firm outcomes are not systematically correlated with firms' suspected organized crime status and the timing of the reform.

I begin by identifying and characterizing firms suspected of having ties to organized crime. The machine learning model, combined with novel white list data, classifies approximately 1,200 firms as potentially connected to criminal networks—around 18% of incumbents in the public procurement market at the onset of the policy. Before the introduction of the white list provision, these suspected firms secured almost 8 billion Euros in public contracts—20% of total contract value.

The paper documents effects of transparency requirements at two levels. At the firm level, transparency requirements generate substantial deterrent effects on suspected firms, either di-

rectly constraining their operations or forcing them into costly avoidance strategies. The policy successfully drives these firms out of public procurement markets, with a 30% decline in tenders won relative to non-suspected firms compared to their pre-reform baseline difference. This effect builds gradually, with the most significant impact emerging 2-3 years after implementation and persisting throughout the decade following policy introduction. Exclusion from procurement markets forces suspected firms into broader organizational decline: total assets decline by 40% and employment falls by 35% relative to non-suspected firms. The cumulative impact is severe—six years after policy implementation, half of the suspected incumbents have ceased operations entirely, while most remaining firms have been effectively excluded from procurement markets. When direct enforcement fails to eliminate suspected firms completely, it compels them into costly avoidance behaviors. Conditional on winning contracts, surviving firms systematically shift toward smaller-valued tenders that typically face less regulatory scrutiny, suggesting strategic attempts to evade detection. These survivors gradually pivot their business models away from public contracts, with non-procurement revenues eventually recovering after an initial adjustment period as firms adapt to the new regulatory environment.

At the local market level, the paper analyzes how local economies adjust when suspected firms are removed from public procurement—and often from the market entirely. The ex ante impact is ambiguous: removal could improve market efficiency by eliminating corrupt participants or create supply disruptions if these firms provided critical services. To study these dynamics, I construct a municipality-level exposure index, defined as the pre-reform share of tenders won by suspected firms in each municipality, and employ it in a difference-in-differences design comparing high versus low exposure municipalities.

The results reveal substantial market restructuring with predominantly positive outcomes. Tenders left vacant by suspected firms are captured almost entirely by new entrants rather than existing incumbents, and competition, as measured by bids per tender, rises by 3.8%. Procurement becomes less geographically concentrated, with affected municipalities 8.3% more likely to award contracts to firms located outside their province after the reform.

These market changes raise two key concerns. First, displaced firms might attempt to re-enter the market through more indirect roles. In the data, subcontracting rises by 5.2% in more affected municipalities. This increase could simply reflect the shift toward more competitive and

geographically diverse procurement markets documented above, where subcontracting helps match the capacity and expertise of new entrants with local project requirements. At the same time, subcontracting is generally less transparent and harder to monitor,[3] which makes it a potential channel for regulatory circumvention (Bosio et al. 2022). To assess this risk, I use the subset of subcontracted tenders with available data on subcontractors (around 30%) and show that the share of contracts having suspected firms as subcontractors actually decreases. This suggests that the reform effectively limited their ability to regain market access both directly and indirectly. Second, the shift away from local contractors might harm local economies. Using quartile regressions to examine municipalities with the highest exposure levels and event-study models to trace dynamic effects, I find no evidence of adverse impacts on employment or per capita income. Instead, the most heavily affected municipalities experience a surge in entrepreneurship. The number of newly incorporated construction firms rises immediately after the reform, while total firm counts remain stable initially as new entrants replace exiting suspected firms, then grow substantially within several years. This pattern indicates that transparency requirements successfully drive away suspected firms while simultaneously encouraging business formation, leading to a more competitive and legitimate contractor base over time.

Related Literature. The findings in this paper contribute to several strands of the literature. First, this work adds to a growing body of research on the economic effects of policies aimed at curbing corruption and organized crime. Most existing studies focus on repressive interventions targeting public officials and local institutions: Colonnelli et al. (2022) and Colonnelli and Prem (2022) examine anti-corruption crackdowns in Brazilian municipalities, Chen, Jin, and Xu (2021) studies high-profile corruption investigations in China, and Fenizia and Saggio (2024) analyzes the dismissal of mafia-infiltrated municipal councils in Italy. Other work examines firm-level interventions such as corporate debarment (Szerman 2023) and large-scale asset seizures (Slutzky and Zeume 2024).

In contrast, transparency-based approaches have received far less attention in economics. Evidence is more developed in the accounting literature, which has examined how mandatory

3. Moreover, the initial 2013 legislation did not regulate subcontracting, creating a potential loophole that was only closed a year later through Article 29 of Law n. 114/2014, which extended anti-mafia vetting requirements to subcontracting arrangements. See https://www.bosettiegatti.eu/info/norme/statali/2014_0114.htm.

disclosure affects firm valuations (Samuels 2021), reporting choices (Kays 2022), and compliance behavior (Duro, Heese, and Ormazabal 2019; Aobdia 2018; De Simone and Olbert 2022). A smaller set of studies in economics and finance explores equilibrium effects of transparency-enhancing reforms, such as anti–money-laundering rules on bank lending (Slutzky, Villamizar-Villegas, and Williams 2020), credit restrictions for high-risk sectors (Sachdeva et al. 2023), and tax information exchange agreements on offshore evasion and firm value (Bennedsen and Zeume 2018). These studies shed light on the market-wide consequences of transparency reforms but do not address whether such policies deter criminally connected firms from participating in the market in the first place.

This paper fills that gap by focusing on a transparency regime that directly targets firms suspected of organized crime ties. Using a novel classification of suspected firms and rich administrative data, I study how the exclusion of these firms affects market structure, competition, and local economic activity. Closest to this work is Daniele and Dipoppa (2023), who document strategic bunching below disclosure thresholds in a related Italian setting. While their results highlight regulatory avoidance, this paper goes further by providing the first causal evidence on the deterrent and restructuring effects of a transparency-based policy, showing how it reshapes both procurement markets and the local business environment.

Second, this paper contributes to the literature on how organized crime infiltrates firms and how such infiltration affects firm performance. Mirenda, Mocetti, and Rizzica (2022) proxy infiltration using mafia-sounding names on boards of limited liability companies, while Arellano-Bover et al. (2024) rely on Financial Intelligence data to identify firms involved in suspicious transactions. Bianchi et al. (2022) and Bianchi and Pecchiari (2025) use intelligence records on firm managers to detect infiltration. This paper complements these approaches in three ways. First, it draws on granular administrative data that captures both large and small firms—including smaller businesses often excluded from financial surveillance but crucial to criminal operations due to their lower visibility (Ganz 2019). Second, existing metrics tend to focus on cases that have been detected and prosecuted. By contrast, I propose a revealed-preference approach based on firms' strategic decisions to avoid vetting, which allows for the identification of a potentially broader set of infiltrated firms. Third, it uses the 2013 amendment to inquire how infiltrated firms respond to hightened transparency standards.

Third, this paper connects to the large literature that studies corruption in public procurement settings. In Chen (2024) and Decarolis et al. (2020), the authors investigate the effects of public officials' corruption on procurement dynamics, contract characteristics, and execution. I add to this literature by examining the influence of organized crime, specifically focusing on the role of infiltrated firms and their ability to secure public contracts.

Fourth, this work contributes to an emerging body of literature that applies machine learning methods to detect and predict corruption. In López-Iturriaga and Sanz (2018) and Gallego, Rivero, and Martínez (2021), the authors introduce algorithms that draw from observable characteristics to predict corruption in Spanish provinces and Colombian public contracts, respectively. The closest works are from Ash, Galletta, and Giommoni (2020), Colonnelli, Gallego, and Prem (2022), and Campedelli, Daniele, and Le Moglie (2024) where the authors employ time-varying budgetary and non-budgetary characteristics to predict corruption in Brazilian municipalities and Italian city council infiltration by the mafia. In this paper I follow Ash, Galletta, and Giommoni (2020) and use a tree-based gradient boosting classifier that combines pre-policy firm and market characteristics to predict firms' probability of participating in public procurement.

Finally, this paper contributes to ongoing policy efforts to strengthen transparency-based enforcement by proposing a predictive framework that combines expected firm behavior with observed compliance. The methodology identifies firms that systematically avoid vetting despite strong predicted incentives to participate, offering a potentially widely applicable tool to flag anomalous behavior in contexts where direct evidence of criminal infiltration is scarce. While developed for the Italian Antimafia framework, this approach can inform transparency-based monitoring strategies in other procurement systems or regulatory domains.

Outline. The rest of the paper proceeds as follows. Section 2 describes the institutional framework and the changes introduced by the 2013 amendment to the Antimafia Information Law. Section 3 presents the data. Section 4 outlines the empirical strategy, including the conceptual framework behind the firm-level infiltration measure and its estimation. Section 5 presents the results at the firm and market levels. Section 6 concludes.

2 — Institutional Background

2.1. The Antimafia Information Law

The Antimafia Information Law, part of a broader framework of anti-corruption measures by the Italian state, represents a comprehensive legislative effort to curb organized crime's influence on the economy. This law originated from the heightened need for anti-mafia legislation in the 1990s, following a series of high-profile mafia-related assassinations. Initially enacted in 1965 and subsequently updated in 1994 and 1998, the law primarily aims to prevent mafia-linked firms from accessing government subsidies and procurement contracts.

In 2013, the Italian government introduced significant amendments to this legislative framework, particularly through the 18 April 2013 presidential decree.[4] The scope and effectiveness of controls were expanded, with enhanced police investigative tools extending checks to friends and family members.[5] The law harmonized the roles of contracting authorities and police forces and created a centralized database of mafia-related information.

2.2. The White List Provision

A central provision of the 2013 anti-mafia decree introduced mandatory screening for firms operating in sectors considered at high risk of mafia infiltration and seeking to participate in public procurement. The reform required each of Italy's 103 provincial police offices (*Prefetture*) to establish and maintain a publicly available *white lists* of vetted firms eligible to bid for public contracts. High-risk sectors include construction, waste management, transportation, catering, and environmental services, among others.[6]

To be included in the white list, firms must submit an application to the local police office (*Prefettura*) in the province where their headquarters are located. The local police then conduct a rigorous investigation into the firm, which includes reviewing financial accounts, examining the firm's history of interactions with contracting authorities, and—when deemed necessary—carrying

4. Law 6 November 2012, n. 190: https://www.bosettiegatti.eu/info/norme/statali/2012_0190.htm; D.P.C.M. 18 April 2013: https://www.gazzettaufficiale.it/eli/gu/2013/05/06/104/sg/pdf

5. The reform expanded background checks beyond the primary individuals to include their social and family networks, allowing authorities to identify potential indirect connections to organized crime.

6. The full list is defined in Law 6 November 2012, n. 190: https://www.bosettiegatti.eu/info/norme/statali/2012_0190.htm. Construction firms account for approximately 60% of all firms on the white list.

out on-site inspections. These inspections may involve interviews with managers, employees, and, in some cases, family members.[7]

Firms that successfully pass the investigation are added to the white list and become eligible to participate in public tenders.[8] Applications can be denied if proof of infiltration emerges, and registration can be revoked at any time if new evidence arises. Listings are valid for one year and must be renewed annually.

3 — Data

My analysis draws from a number of different data sources, which I describe below.

White List My main data source is a comprehensive set of white list records manually collected and compiled for 30 provincial police offices (Figure 2a). This novel dataset covers over 28,000 firms successfully vetted by the Italian police from 2013 to 2022. Lists are updated periodically as new firms join, but only the most recent versions are made available online. To reconstruct historical registries, I submitted FOIA requests to provincial police authorities.[9] The obtained documents contain key identifying information such as the firm's name, headquarters, and sector (Figure 2b), though formatting and completeness varied across provinces and over time. Notably, I was not granted access to records of firms that did not pass the vetting procedure. As a result, firms that do not appear on the white list may either have never applied or may have been rejected following the police investigation.

Firms I use data on firms' balance sheets and income statements from Bureau Van Dijk for the years 2008 to 2022. To construct the most comprehensive sample possible, I combined different

7. The investigation assesses both formal disqualifying conditions (Art. 67) and evidence of attempted mafia infiltration (Art. 84) under the Anti-Mafia Code: https://www.brocardi.it/codice-antimafia/libro-i/titolo-v/capo-i/art67.html; https://www.brocardi.it/codice-antimafia/libro-ii/capo-ii/art84.html.

8. To minimize compliance costs and avoid disruptions to legitimate business activity, firms are allowed to submit bids as long as they are either already listed or have a pending application at the time of bid submission. This feature ensures that administrative processing times do not block market access for compliant firms (Ministero dell'Interno 2016). If proof of infiltration emerges, any awarded contracts are automatically voided.

9. I submitted FOIA requests to all 103 Italian provincial police offices and obtained viable information from 30 provinces: Arezzo, Ascoli Piceno, Avellino, Benevento, Caltanissetta, Terni, Campobasso, Cosenza, Ferrara, Lecce, Lodi, Aosta, Catanzaro, Messina, Napoli, Potenza, Reggio Calabria, Reggio Emilia, Salerno, Treviso, Venezia, Rimini, Varese, Savona, Trieste, Ravenna, Vibo Valentia, Trapani, Ragusa, and Siracusa.

vintages and sources from Orbis and Amadeus, using data from multiple providers, including WRDS and the Orbis BvD interface. This approach is crucial for tracing small and medium-sized firms, which are often hard to track due to sporadic reporting but are key participants in tenders and frequent targets of organized crime infiltration. As Orbis drops non-reporting firms after a certain period, I complement it with data from WRDS Amadeus, and the BvD Interface to maximize information coverage. This process follows the methodologies of Kalemli-Ozcan et al. (2015) and Díez, Fan, and Villegas-Sánchez (2021), aiming to extend the time series and improve firm coverage.

Public Procurement I use data on public procurement contracts from 2008 to 2022, collected by the Italian Authority for Public Contracts (ANAC). This dataset includes all public works contracts with a reservation price above 40,000 euros. For my analysis, I focus on construction within the OG (*Opere Generali*) categories, which cover a wide range of activities for public buildings and infrastructure, such as civic buildings (OG01) and transportation infrastructure like roads and bridges (OG03), among others. These categories together account for more than half of all contracts by both number and total expenditure.

The dataset provides detailed insights into the contracting phase for each contract, encompassing the start and end dates of the bidding process, the type of contracting authority, the auction procedure, the selection criteria, the number of bidders, and the identity of the winning bidder. Despite the richness of this data, its full potential is often underutilized, as approximately a third of the tenders lack relevant demographic information about the winners, such as their headquarters and sector. This gap typically results in dropping these tenders from empirical analysis. However, by leveraging Bureau Van Dyke and Chambers of Commerce information,[10] I was able to reconstruct the missing details for these tenders. The resulting dataset identifies more than 90% of the winners.

10. Chamber of Commerce information on headquarters locations and sectors were accessed through https://www.ufficiocamerale.it/trova-azienda a private provider which offers a search engine into the Chamber of Commerce database.

4 — Empirical Strategy

In this section, I first discuss the construction of a novel index of mafia infiltration at the firm level. Second, I describe how I combine this index and the 2013 reform for my empirical strategy.

4.1. Firm-Level Measure of Mafia Infiltration

Existing measures of Mafia infiltration mostly rely on extreme events such as high-profile mafia arrests, and local violence (Transcrime 2014), violence against politicians (Pulejo and Querubín 2023), or extreme cases of political corruption (dismissal of city councils in Fenizia and Saggio (2024)). While insightful, these indices focus on aggregate, often extreme manifestations of organized crime and do not provide granular information about individual firms or the business activities of organized crime within the legal economy.

To address this limitation, researchers have adopted more indirect approaches to investigate mafia infiltration into firms and its ability to launder money into the legal economy. Examples include flagging board members with mafia-sounding names (Mirenda, Mocetti, and Rizzica (2022)) or identifying firms engaging in suspicious transactions (Arellano-Bover et al. (2024)). My index belongs to this second category but differs fundamentally from existing approaches: instead of relying on already detected criminal interventions or observable manager characteristics, it focuses on firms behaving in ways that diverge systematically from their predicted economic incentives in a manner plausibly explained only by ties to organized crime. This approach leverages the transparency reform as a natural experiment to reveal infiltration through firms' revealed preferences regarding anti-mafia screening procedures.

The following two subsections lay out the conceptual and empirical foundations of the firm-level infiltration measure. First, I develop a simple economic model to formalize the trade-offs that firms face when deciding whether to undergo vetting. This framework provides the theoretical basis for distinguishing infiltrated firms from those with legitimate reasons not to participate. Second, I describe how I translate this model into an empirical classification procedure using supervised machine learning.

4.1.1. Conceptual Framework

To rationalize firms' post-reform behavior, I develop a simple binary-type model in which participation requires undergoing mandatory anti-mafia vetting. The reform fundamentally altered incentives: before 2013, firms entered the procurement market whenever expected benefits were positive; after the reform, they also had to weigh these benefits against the risk of detection.

Firms differ along two dimensions. First, they have observable characteristics $\mathbf{X}_i$ that determine their expected net benefits from procurement, denoted $B_i = f(\mathbf{X}_i)$. Second, they may be either clean ($\theta = 0$) or infiltrated ($\theta = 1$). Clean firms face no risk of sanction, while infiltrated firms are detected with probability $p > 0$ if they undergo vetting and incur a penalty $P > 0$ if caught.

Payoffs from vetting are therefore type-specific. Clean firms obtain

$$V_i(0) = B_i,$$

while infiltrated firms obtain

$$V_i(1) = (1 - p)B_i - pP,$$

which discounts benefits by the expected penalty. Abstaining from vetting yields zero for all firms.

The decision rules follow directly. Clean firms participate whenever $B_i > 0$. Infiltrated firms participate only if procurement benefits are large enough to outweigh detection risk, that is, if $B_i > \overline{B} \equiv \frac{p}{1-p}P$. The consequence is a wedge in behavior: for sufficiently high B_i, both types participate; for non-positive B_i, neither does; but in the intermediate region

$$0 < B_i < \overline{B},$$

only clean firms vet. I refer to this interval as the *revelation zone*, illustrated in Figure 1. Clean firms in this zone are observed on police white lists, whereas infiltrated firms strategically abstain.

This framework provides the foundation for the empirical strategy. By estimating B_i as a function of firm observables $\mathbf{X}_i$ using pre-reform data, I obtain a benchmark for expected participation absent vetting. Post-reform, I compare these predicted benefits with white list registrations. Firms with high predicted B_i that appear on the lists are consistent with clean participation, while those with similarly high B_i but absent from the lists are likely infiltrated. In

practice, the empirical histogram of predicted B_i combined with white list information mirrors the conceptual partition in Figure 1, separating clean from infiltrated firms in the revelation zone.

4.1.2. Machine Learning Classification Procedure

This section describes how I operationalize the conceptual framework by estimating firm-level net benefits (B_i) from public procurement and classifying firms based on their post-reform presence in the white lists. The strategy hinges on predicting which firms have strong economic incentives to participate, and flagging those that avoid or fail anti-mafia screening despite high predicted benefits.

Step 1: Predicting Net Benefits with Machine Learning. I estimate predicted procurement participation using a gradient boosting model trained on more than 50 pre-policy firm characteristics. Features include: *i.* procurement history, such as participation and win indicators, contract counts, and total amounts—capturing experience and expected gains; *ii.* financial variables, including firm size, leverage, and profitability—measuring capacity to execute contracts and bear administrative costs; and *iii.* firm characteristics like year of incorporation, reflecting market tenure. All inputs are observed over a four-year window (t-1 to t-4) to capture the persistence and evolution of firm characteristics, performance, and temporal variation in procurement engagement. These pre-reform variables collectively proxy each firm's expected net benefit B_i from participating in pubilc procurement under a no-policy counterfactual.

The model achieves strong out-of-sample performance, capturing the structural persistence of participation decisions. The model attains a precision of 86.2% at the 50% classification threshold, meaning that among firms predicted to participate, nearly 9 out of 10 actually do.[11] The model's overall discriminatory power, as measured by the area under the ROC curve, is close to 90%, indicating solid separation between likely and unlikely participants.[12] This high predictive accuracy validates machine learning as an effective methodology for modeling procurement participation, particularly given the dominance of observable firm characteristics in driving predictions. Table

11. This substantially exceeds the precision of 0.72 reported in Ash, Galletta, and Giommoni 2020, whose machine learning methodology I closely follow.

12. An AUC of 0.9 means that if we randomly select one firm that actually participates in procurement and one firm that does not participate, the model will correctly assign a higher participation probability to the actual participant 90% of the time.

1 reports the 20 most important predictors according to the *gain* metric. Procurement history emerges as the dominant predictor: participation in the previous year (t-1) alone accounts for 18.2% of the model's predictive power, while past procurement indicators collectively contribute over 40%. This finding reinforces a fundamental pattern in procurement markets: firms with prior procurement experience exhibit strong persistence in participation, continuing to engage unless deterred by factors beyond pure economic incentives.

Step 2: Generating Participation Probabilities. The trained model assigns each firm a predicted probability of participating in procurement post-2013, conditional on no reform. These probabilities serve as empirical proxies for the latent net benefit B_i in the theoretical framework. Figure 3a presents a binned histogram showing the distribution of predicted participation probabilities. The x-axis reports the predicted participation probability (ranging from 0 to 1), while the y-axis shows the number of firms falling within each probability bin (binwidth = 0.05). The distribution reveals a bimodal pattern: firms tend to cluster around very low or very high participation probabilities. This reflects a structural sorting in the data: some firms treat public procurement as a core business strategy, while others engage only sporadically.

At the end of steps 1 and 2, the methodology successfully separates firms based on their expected procurement benefits B_i. Firms with high predicted participation probabilities fall to the right of the right of the y-axis ($B_i > 0$) in Figure 1, representing those with strong economic incentives to participate (green region). Conversely, firms with low predicted probabilities fall to the left ($B_i < 0$), corresponding to the region of low-benefit firms.

Step 3: White List Cross-Validation. I compare predicted participation probabilities to actual white lists. Figure 3b shades firms by whether they ever appear on provincial white lists. As expected, the share of vetted firms increases with predicted probability. This pattern is reassuring: firms flagged by the model as having strong procurement incentives are also those most likely to comply with vetting procedures. The model appears to successfully capture the underlying economic logic driving transparency compliance.

Step 4: Infiltration Classification. Firms with predicted probabilities exceeding 50% are identified as likely participants in post-policy public procurement markets under a no-policy

scenario. Among these firms, those that never appear on the lists are labeled as potentially infiltrated. Figure 3c highlights these firms in red. These firms map to the red line in Figure 1. For firms laying on this line positive net benefits ($B_i > 0$) are outweighed by high detection costs, leading to systematic avoidance of the vetting process or failure to complete it.

At the completion of steps 3 and 4, the methodology achieves its goal: distinguishing infiltrated firms (red line) from clean firms (blue line) within the high-benefit population (revelation zone) in Figure 1.

4.1.3. Comparison with literature metrics

In this section I validate the index proposed in section 4.1 against three established measures of organized crime presence from the literature: city council dismissals due to mafia infiltration (Fenizia and Saggio 2024), mafia-style attacks against public officials (Pulejo and Querubín 2023), and firms confiscated from organized crime groups (Slutzky and Zeume 2024). Since all metrics capture organized crime at the municipality level, I transform my firm-level classification by computing the share of tenders won by infiltrated firms in each municipality during the pre-period. This share reflects the relative predominance of infiltrated incumbents in local procurement markets at the onset of the policy. Figure 4 presents the validation results. While median shares are always zero for municipalities without extreme mafia events, they rise to approximately 10% in municipalities that experienced dismissals, violence, or asset confiscations. Mean shares show similar patterns, with municipalities experiencing extreme mafia manifestations exhibiting statistically significantly higher shares (of around 3 percentage points across the three measures) of infiltrated winners. These results confirm that municipalities with extreme mafia manifestations show significantly higher infiltration rates, as expected. However, even municipalities without such extreme events present positive shares on average, suggesting that my measure captures milder forms of infiltration that, while economically harmful, may not reach public attention like council dismissals, violent attacks, or asset confiscations. This fine-grained approach allows identification of subtle economic infiltration beyond the most visible manifestations of organized crime.

4.2. Research Design

This section outlines the empirical strategy used to identify the causal effects of the transparency requirements introduced by the Antimafia Information Law amendment on infiltrated firm outcomes and their broader impact on local markets. I implement two complementary identification strategies that together provide a comprehensive picture of the policy's impact: a firm-level analysis examining direct effects on infiltrated firms, and a municipality-level analysis capturing broader market consequences of removing these firms from procurement markets.

4.2.1. Firm-Level Consequences of Transparency Requirements

I employ a difference-in-differences (DiD) research design to study the causal effects of the introduction of the white list provision on the economic outcomes of mafia-infiltrated firms. My design exploits the heterogeneous impact of this regulation across firm types. While the law applies de jure to all firms, its low administrative and economic compliance costs make it de facto a targeted shock to infiltrated incumbents only. As explained in Section 2.2, legitimate firms can easily satisfy the transparency and disclosure requirements at minimal cost, whereas infiltrated firms face structural barriers to compliance due to their illicit organizational ties.

The analysis compares changes in firm-level outcomes before and after the law's introduction between infiltrated and non-infiltrated incumbents in the public procurement market. My main econometric specification is:

$$Y_{i,t} = \alpha_{\text{i}} + \delta_{\text{pbt}} + \sum_{\substack{k=2008 \\ k \neq 2012}}^{2022} \beta_k \cdot \text{Infiltrated}_i \cdot \mathbf{1}\{\text{t=k}\} + \epsilon_{i,t} \tag{1}$$

where the dependent variable is the outcome of interest for firm i in year t. I restrict the sample to firms with predicted participation probabilities exceeding 50%, ensuring comparisons among firms with positive net benefits from procurement participation. These firms are grouped into 10-percentage-point bins based on their predicted probabilities. On the right-hand side, I control for firm fixed effects (α_{i}) and province-year-probability bin fixed effects (δ_{pbt}). The variable Infiltrated_i is a dummy equal to 1 if firm i is classified as infiltrated according to the machine learning procedure described in Section 4.1. The coefficients β_k estimate the differential treatment effects in year k relative to the baseline year 2012. Standard errors are clustered at the firm level.

Discussion of Assumptions. I include firm fixed effects to account for all time-invariant firm characteristics, such as sectoral factors or baseline productivity differences. These fixed effects ensure that my identification strategy relies on within-firm variation over time to estimate the causal effects of the transparency requirements. Additionally, I include province-year-likelihood group fixed effects to control for regional shocks and differential policy impacts across firms with varying procurement propensities. The key assumption is that, absent the reform, infiltrated and non-infiltrated incumbents would have followed parallel trends in outcomes. This assumption could be violated if infiltrated firms were already on declining trajectories due to increased law enforcement pressure, changing market conditions, or anticipation effects beginning before 2013. I assess this assumption by examining the pre-policy coefficients β_k for $k < 2013$ in the results section. Moreover, placebo and robustness tests in section C rule out a wide array of alternative explanations for the results.

4.2.2. Market-Level Consequences of Transparency Requirements

The second identification strategy evaluates the impact of the reform on local procurement markets and firm dynamics by exploiting variation in treatment intensity across municipalities. I construct a municipality-level index of infiltration exposure by aggregating the tenders won by infiltrated firms in the pre-reform period (2008–2013) and expressing it as a share of total tenders held within each municipality. This index captures the heterogeneous exposure of local procurement markets to criminal infiltration prior to the reform, creating a continuous measure of treatment intensity.

My identification strategy leverages the insight that while the white list provision applies uniformly across all Italian municipalities, its economic impact should be proportional to the pre-existing presence of infiltrated firms in local markets. Municipalities with higher pre-2013 infiltration should experience larger disruptions in firm composition and market dynamics following the reform, as a greater share of their incumbent suppliers face binding compliance constraints. Conversely, municipalities with minimal pre-reform infiltration should exhibit limited changes in outcomes, as most local firms can comply with the new regulations at negligible cost.

I exploit this cross-municipal variation using a difference-in-differences framework, comparing changes in outcomes before and after the reform between municipalities with above versus below median infiltration exposure:

$$Y_{mt} = \alpha_m + \gamma_{pt} + \delta_{dt} + \beta \cdot \text{Above Median Infiltration}_m \cdot \text{Post}_t + \epsilon_{mt} \quad (2)$$

where the dependent variable is the outcome of interest in municipality m and year t. The variable Above Median Infiltration$_m$ is a dummy equal to 1 if the pre-reform share of tenders won by infiltrated firms in municipality m exceeds the median across all municipalities. Post$_t$ is a dummy equal to 1 in the post-reform period. I include municipality fixed effects (α_m), province-by-year fixed effects (γ_{pt}), and demographic category-by-year fixed effects (δ_{dt}). The demographic categories are defined by population size, creating twelve groups ranging from municipalities with fewer than 500 residents to those with more than 500,000 residents, allowing for flexible time trends across municipalities of different sizes. The coefficient β captures the differential evolution of outcomes in municipalities with higher versus lower pre-reform infiltration exposure. Standard errors are clustered at the municipality level.

Discussion of Assumptions. I include municipality fixed effects to account for all time-invariant local characteristics, such as economic structure, geographic factors, or institutional quality differences. Province-by-year fixed effects control for provincial shocks and policy changes that might differentially affect municipalities over time, while demographic category-by-year fixed effects allow municipalities of different population sizes to follow distinct time trends. The key identifying assumption underlying this approach is that municipalities with different levels of pre-reform infiltration exposure would have followed parallel trends in procurement outcomes absent the regulatory intervention. This assumption could be violated if municipalities with different infiltration levels were on divergent trends for reasons unrelated to the policy. I address this concern by including province-by-year and demographic category-by-year fixed effects that absorb broader regional and size-related dynamics.

5 — Results

This section shows how transparency requirements introduced with the 2013 Antimafia Information Law amendment affected infiltrated firms' performance in and out of the public procurement market. Figure 5 already clearly shows descriptively the sharp decline in infiltrated incumbents'

ability to secure public contracts. The share of total tenders won by infiltrated incumbents drops from approximately 15% in 2012 to under 10% by 2015, with the steepest decline occurring immediately after policy implementation. The erosion continues over the following years, with infiltrated firms' market share falling to roughly 6% by 2020, demonstrating the policy's sustained effectiveness in reducing organized crime's presence in public procurement.

5.1. Firm-Level Consequences of Transparency Requirements

Figures 6a through 8 show results from the causal event study design. They report β_k coefficients from equation 1.

First, I examine procurement participation at the extensive and intensive margins. Figure 6a shows the extensive margin results, where infiltrated firms experience an immediate and significant decline in the number of contracts won. The effect is substantial, with a negative coefficient of approximately -0.2 in 2014, indicating that infiltrated firms won 20% fewer contracts than non-infiltrated incumbents in the first year after policy implementation. This effect intensifies over time, reaching -0.4 by 2016.

Figure 6b examines the intensive margin—the value of contracts won conditional on winning. The results show that infiltrated firms not only win fewer contracts but also secure lower-value contracts when they do win. This effect is consistent with avoidance behavior: lower-valued contracts usually face less scrutiny from public authorities, making it more likely that a non-vetted firm slips through the cracks and can still secure some contracts despite its absence from white lists.

Second, Figure 7 examines how the policy affects firm size. Both total assets (Figure 7a) and employment (Figure 7b) decline for infiltrated firms. While both outcomes register statistically significant drops already in 2014, larger effects build up gradually as firms adjust their scale to reduced procurement opportunities, with the magnitude of impacts intensifying through 2015-2016.

Finally, Figure 8 reveals how infiltrated firms adjust their revenue composition. Figure 8a shows that total operating revenues decline substantially following the policy implementation. However,

Figure 8b reveals that revenues from non-procurement activities actually increase. This suggests that surviving infiltrated firms partially substitute lost procurement income by expanding other business lines, though the compensation remains incomplete—total revenues still fall significantly. Conditional on survival, infiltrated firms become smaller and shift their business focus away from the public procurement market.

5.2. Market-Level Consequences of Transparency Requirements

This section studies the effects of the 2013 transparency reform on local procurement markets. Table 2 reports estimates for the β coefficient in equation 2.[13]

Market Composition and Competition. The first three columns examine shifts in winner composition due to the policy. Consistent with the firm-level results, suspected firms' share of tenders won decreases by 11.9 percentage points in treated municipalities in the post-period. Vetted incumbents' share remains largely constant; instead, the market share lost by suspected firms flows to entrants, whose share increases by 2.5 percentage points. Since entrants are defined as firms that never participated in public tenders in a given municipality during the pre-period, their pre-treatment share is mechanically zero. This reallocation suggests the reform successfully disrupted existing networks while creating opportunities for entirely new market participants.

Column (4) further highlights this finding by showing that the reform reduced local favoritism. The probability that the winning firm is incorporated in the same province as the contracting municipality decreases by nearly 5 percentage points, suggesting that procurement becomes less geographically concentrated and more open to outside competition.

The reform also intensifies competition: the number of bidders per tender rises by 10.4% (column 9), translating to approximately 4 additional bidders competing for each contract. This increased competition does not translate into statistically significant changes in winning discounts (column 5) or project delays (column 6), suggesting that service quality is maintained despite the market restructuring.

13. Following the corner solution literature, we maintain a balanced panel by assigning zero to all outcome categories in municipality-years with no tenders, thereby capturing both intensive margin effects (changes in market composition when tenders occur) and extensive margin effects (changes in the probability of tender activity).

Circumvention Through Subcontracting. Columns (7) and (8) examine potential avoidance behavior through subcontracting arrangements. Subcontracting increases by 2.7 percentage points in affected municipalities (column 7). This finding warrants attention because subcontracting arrangements are inherently harder to oversee than direct procurement relationships (Bosio et al. 2022), and the initial version of the 2013 anti-mafia law did not clearly discipline subcontracting activities. However, column (8) shows that the share of subcontracted work going to suspected firms actually decreases, indicating that the reform effectively limits both direct and indirect access to procurement markets.

Local Economic Effects. A key concern is whether the displacement of local contractors might harm municipal economies. Column (10) provides initial evidence against this concern, showing that the number of newly incorporated construction firms increases by 8.2% in more affected municipalities. Figure 9 provides a more detailed dynamic analysis using event study specifications that separately examine municipalities in the third and fourth quartiles of pre-reform exposure intensity.

Panel (a) shows that total employment in construction remains stable in both Q3 and Q4 municipalities, with no evidence of job losses following the reform. Panel (b) reveals that the total number of construction firms initially remains constant but grows substantially in the most affected municipalities (Q4) starting three years after the reform. Panel (c) shows that this growth is driven by new firm creation: the number of newly incorporated construction firms increases immediately after the reform in Q4 municipalities and remains elevated throughout the observation period. Panel (d) demonstrates that income per capita is unaffected by the reform, even in the most heavily treated areas.

These patterns indicate that transparency requirements achieve a dual effect: they successfully exclude suspected firms while stimulating entry by legitimate businesses, ultimately creating a more competitive and trustworthy contractor base. Rather than harming local economies, the removal of suspected networks appears to create space for legitimate entrepreneurship to flourish.

6 — Conclusion

This paper examines whether transparency mandates can effectively deter organized crime from operating within legal markets. I study the 2013 amendment to Italy's Antimafia Information Law, which introduced mandatory police vetting for firms bidding on public contracts and published vetted firms in white lists. By integrating novel police data with a machine learning model of predicted participation, I identify firms plausibly affiliated with organized crime based on their systematic avoidance of vetting despite strong predicted incentives to engage in public procurement.

The results show that transparency requirements generate substantial deterrent effects. Infiltrated firms were systematically pushed out of public procurement. Their presence in tenders dropped sharply, total assets and employment contracted, and over half ceased operations entirely within six years of implementation. Where direct enforcement did not fully remove them, these firms responded by shifting to lower-value tenders and gradually pivoting their activities away from procurement, indicating strategic adaptation to avoid detection.

At the local level, procurement markets in areas heavily exposed to suspected firms underwent substantial restructuring, with outcomes that were largely positive. The exit of these actors created space for legitimate competition to expand: market share shifted primarily to new entrants rather than existing incumbents, bidding became more competitive, and procurement became less geographically concentrated. Crucially, concerns that removing local contractors might harm municipal economies are not borne out by the data. Even in the most heavily affected areas, employment and income remained stable, while firm creation—particularly in construction—rose, suggesting that legitimate businesses stepped in to replace displaced networks.

One important question is to what extent the results presented in this paper are informative for other jurisdictions. Among others, both the European Union and the United States are actively legislating to strengthen ownership disclosure and improve enforcement against infiltration and money laundering.[14] The methodology developed here—combining predictive modeling with

14. In the European Union, recent legislation includes the 6th Anti-Money Laundering Directive (6AMLD), which reinforces the obligation to identify beneficial owners especially in high-risk sectors (6AMLD on Eur-Lex). In the United States, the Corporate Transparency Act mandates most companies to report beneficial ownership information to FinCEN to help detect illicit financial flows (FinCEN Beneficial Ownership Information reporting).

causal inference—can inform ongoing efforts to evaluate and calibrate these policies. More broadly, it provides a framework for assessing the effectiveness of transparency-based enforcement in settings where counterfactual behavior (e.g., participation absent vetting) can be credibly predicted, and firms' response to disclosure requirements can be used to infer underlying compliance status.

Finally, while this paper documents clear behavioral adaptation consistent with avoidance, important questions remain about how infiltrated firms systematically reorganize following enforcement actions. Future research should investigate whether observed adaptations represent temporary disruptions or systematic evolution toward more sophisticated evasion strategies that exploit regulatory gaps.

7 — Tables

Table 1: **Feature Importance: Participation Prediction Model**

Feature	Gain
N. contracts participated t-1 (log)	0.211
N. contracts participated t-2 (log)	0.069
Participant t-3	0.068
Participant t-1	0.047
Year of incorporation	0.046
Participant t-2	0.034
ROA t-1	0.026
Operating revenues t-1 (log)	0.025
Profit margin t-2	0.022
Equity t-1 (log)	0.022
ROA t-2	0.021
Profit margin t-4	0.020
N. contracts participated t-3 (log)	0.018
ROA t-4	0.018
Fixed assets t-1 (log)	0.017
Operating revenues t-3 (log)	0.017
Contract value t-1 (log)	0.016
Profit margin t-1	0.016
Contract value t-2 (log)	0.016
Operating revenues t-2 (log)	0.015
Total features	53

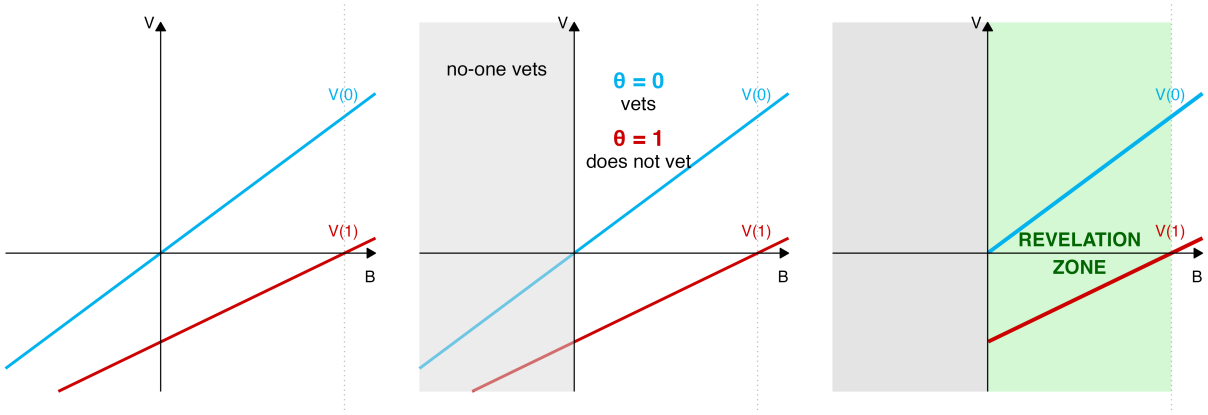
Notes: Gain measures each feature's relative contribution to the model's predictive accuracy, aggregated over all decision tree splits. The model uses 53 features and achieves an AUC of 0.732.

Table 2: **Effect of White List Provision on Local Procurement Markets**

	Share tenders won by									
	Infiltrated	Vetted incumbents	Entrants	Same prov.	Win. discount	Delay	Subcontracting	Bad subcontr.	Bids per tender	New firms
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Infiltration × Post	-0.119***	0.014	0.025***	-0.049***	-0.005	-0.084	0.026*	-0.026*	0.102**	0.010
	(0.004)	(0.009)	(0.009)	(0.014)	(0.004)	(0.064)	(0.015)	(0.015)	(0.046)	(0.012)
R^2	0.17659	0.42090	0.51670	0.32135	0.44176	0.26116	0.32355	0.35699	0.54379	0.74599
Observations	27,225	27,225	27,225	18,359	18,336	9,903	18,359	5,416	17,122	27,225
E[Y]	0.067	0.409	0.000	0.592	0.184	0.802	0.523	0.065	2.758	0.500
E[Y] non-transformed									39.529	1.522
Infiltration share mean	0.501									
Muni fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year × Prov fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year × Pop fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

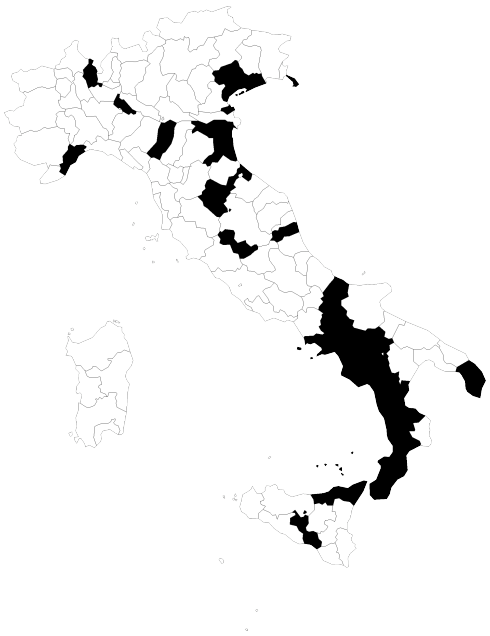
Notes. Observations are at the municipality-year level. The table presents difference-in-differences estimates from the regression $Y_{m,t} = \alpha_m + \gamma_{p,t} + \delta_{d,t} + \beta \cdot \text{Above Median Infiltration}_m \cdot \text{Post}_t + \epsilon_{m,t}$, where $Y_{m,t}$ represents procurement outcomes for municipality m in year t. Treatment is defined as municipalities with above-median pre-reform infiltration exposure (Above Median Infiltration$_m$). Post$_t$ is an indicator for years after 2013. Columns (1)-(3) report share of tenders won by infiltrated firms, vetted incumbents, and entrants respectively. Column (4) reports share of tenders won by firms from the same province. Column (5) reports winning discount expressed as a percentage of the reserve price. Column (6) reports project delays expressed as a percentage. Column (7) reports share of contracts involving subcontracting. Column (8) reports share of subcontracted contracts with potentially infiltrated subcontractors. Column (9) reports number of bids per tender (log transformation). Column (10) reports the number of newly incorporated construction firms (log transformation). Fixed effects include municipality fixed effects (α_m), year×province fixed effects ($\gamma_{p,t}$), and year×demographic category fixed effects ($\delta_{d,t}$). Demographic categories are defined by population size: I (<500), II (500-999), III (1,000-1,999), IV (2,000-2,999), V (3,000-4,999), VI (5,000-9,999), VII (10,000-19,999), VIII (20,000-59,999), IX (60,000-99,999), X (100,000-249,999), XI (250,000-499,999), XII (≥500,000 residents), allowing for flexible time trends across municipalities of different sizes. E[Y] reports the mean of the dependent variable in the pre-reform period. For transformed variables, E[Y] (non-transf.) reports the mean of the untransformed variable. Standard errors clustered at the municipality level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

8 — Figures

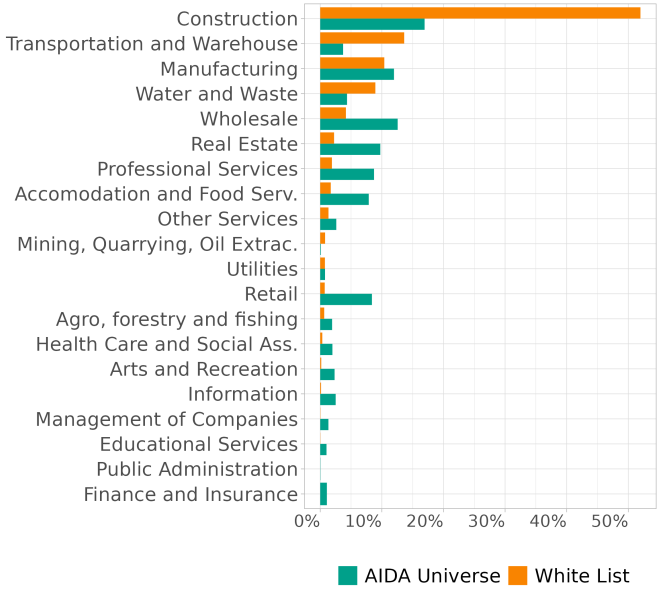


(a) Utility functions by firm type (b) Vetting zones and detection risk (c) The revelation zone

Figure 1 — Theoretical Foundation for Machine Learning Classification. Panel (a) establishes the utility functions where clean firms (blue, $V(0)$) participate whenever expected benefits $B > 0$, while infiltrated firms (red, $V(1)$) require benefits above threshold $\overline{B}$ due to detection risk $(1-p)B - pP$. Panel (b) shows how vetting parameter θ determines detection probability, creating behavioral zones where $\theta = 0$ (no vetting) allows all firms to participate freely, while $\theta = 1$ (mandatory vetting) deters infiltrated firms. Panel (c) highlights the revelation zone ($0 < B < \overline{B}$) where only clean firms undergo vetting, enabling identification through observed participation patterns. In the empirical implementation, machine learning-predicted procurement benefits serve as proxies for B, while white list registration indicates vetting compliance, allowing classification of high-benefit firms that systematically avoid transparency requirements.

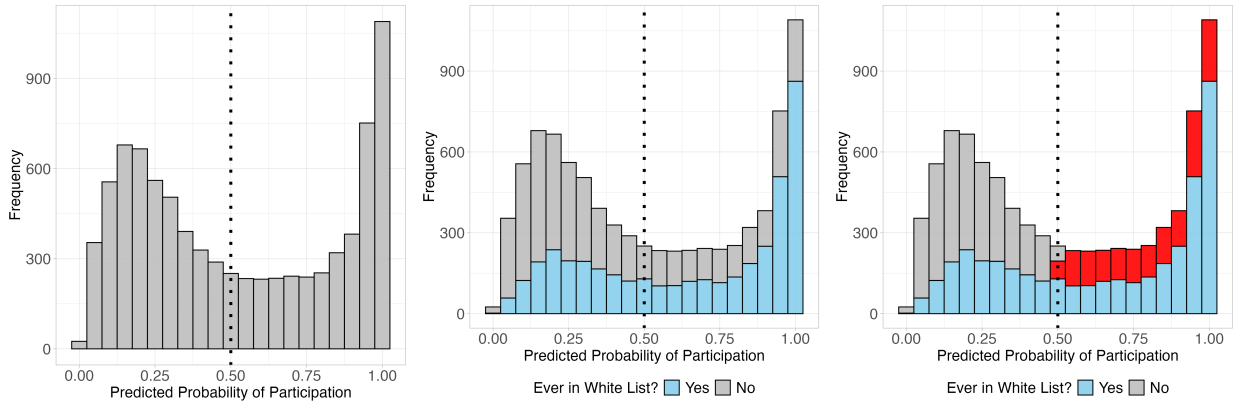


(a) Provincial police offices that provided data



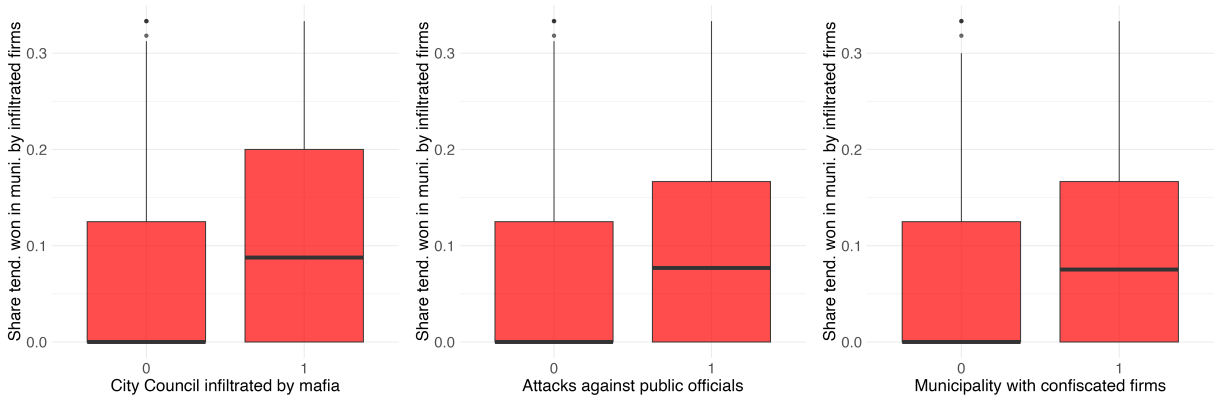
(b) Sectoral composition of firms on the white list compared to the entire population of Italian firms

Figure 2 — **White List Sample**



(a) Predict firm-level procurement participation (b) Identify firms that never enter white lists post-2013 (c) Firms with high probabilities that never enter white lists

Figure 3 — **Construction of the Firm-Level Infiltration Index**



(a) City council dismissed for mafia infiltration (b) Mafia-style attacks against public officials (c) Firms confiscated from organized crime

Figure 4 — Validation Against Literature Measures of Organized Crime. Box plots show the distribution of infiltrated firms' tender share across municipalities, trimmed to the 10th-90th percentile range. Panel (a) compares municipalities where city councils were dismissed for mafia infiltration versus non-dismissed councils. Panel (b) compares municipalities experiencing mafia-style attacks against public officials versus municipalities without attacks. Panel (c) compares municipalities with firms confiscated from organized crime before 2014 versus municipalities without confiscated assets. The thick horizontal line shows the median, boxes represent interquartile ranges.

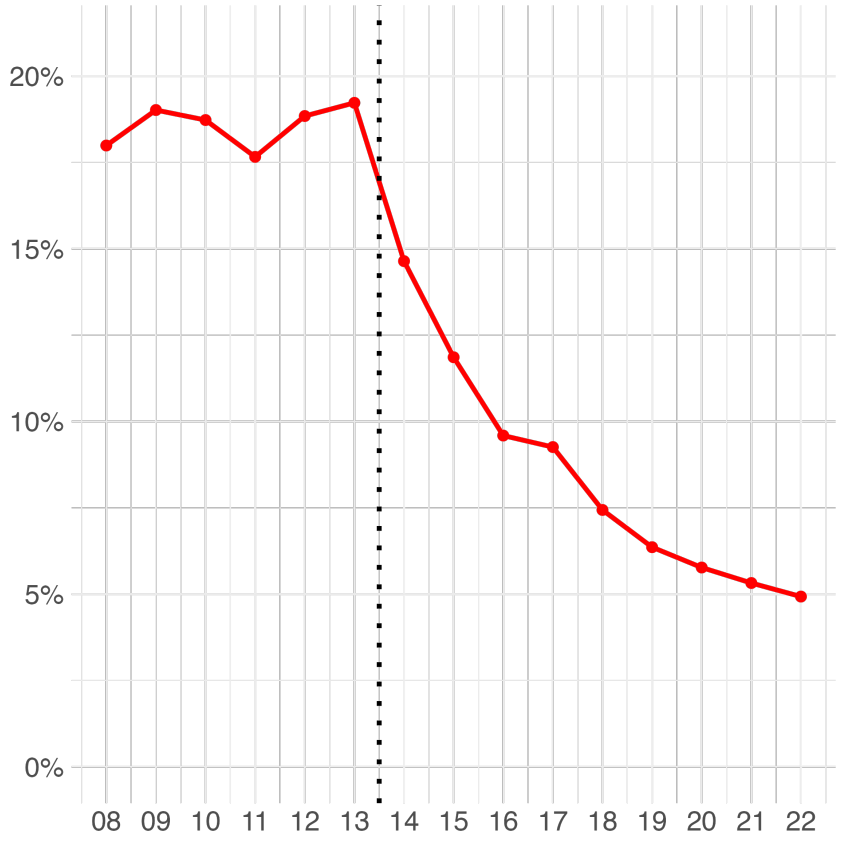
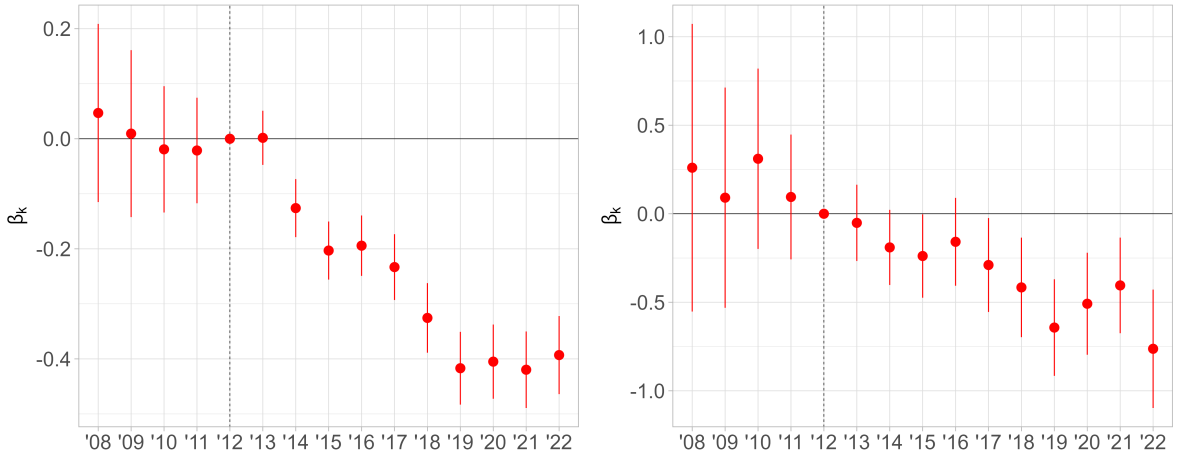


Figure 5 — **Tenders Won by Infiltrated Incumbents**. The y-axis represents the share of tenders won by infiltrated incumbents (bad types) out of all tenders won by incumbents each year (x-axis). The vertical dotted line marks the 2013 policy implementation.

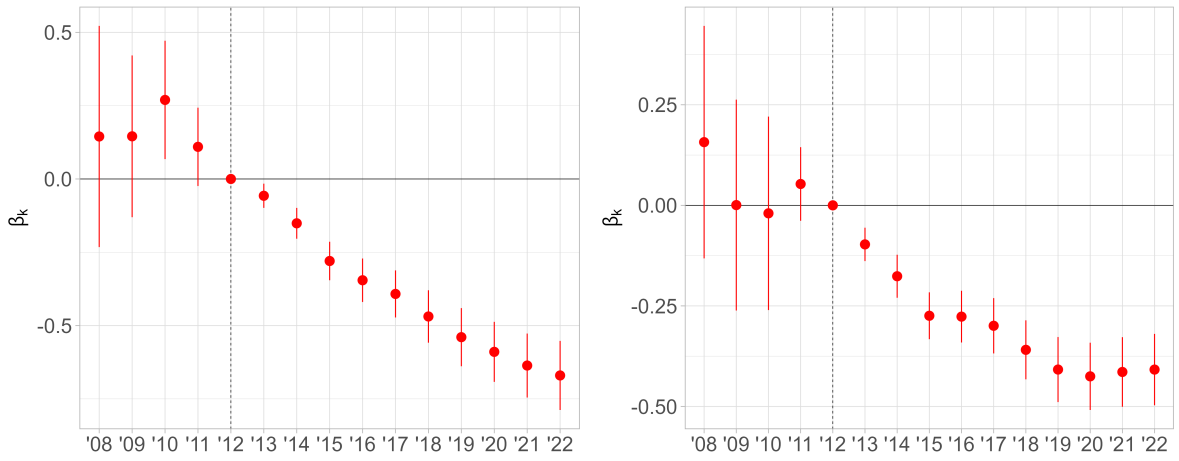


(a) No. Tenders Won (Log)

(b) Amount Won (Log)

Figure 6 — **Infiltrated Firms Activity in Public Procurement**

Notes. Observations are at the firm-year level. Both panels present the estimated coefficients β_k from regression $Y_{i,t} = \delta_{\text{pbt}} + \gamma_{\text{m}} + \alpha_{\text{i}} + \sum_{\substack{k=09 \\ k \neq 13}}^{22} \beta_k \cdot \text{Infiltrated}_i \cdot \mathbf{1}\{\text{t=k}\}_k + \epsilon_{i,t}$
along with 95% confidence intervals. In panel 6a, the dependent variable is the number of contracts won by firm i in year t. In panel 6b, the dependent variable is the total value of contracts (in Euros) won by firm i in year t, conditional on winning. Both variables are expressed in logs. The sample is restricted to firms with a predicted participation probability of over 50%, grouped into 10-percentage-point bins based on predicted probabilities. Fixed effects include province-year-likelihood group fixed effects (δ_{pbt}), municipality fixed effects (γ_{m}), and firm fixed effects (α_{i}). Standard errors are clustered at the firm level to account for the level of treatment.

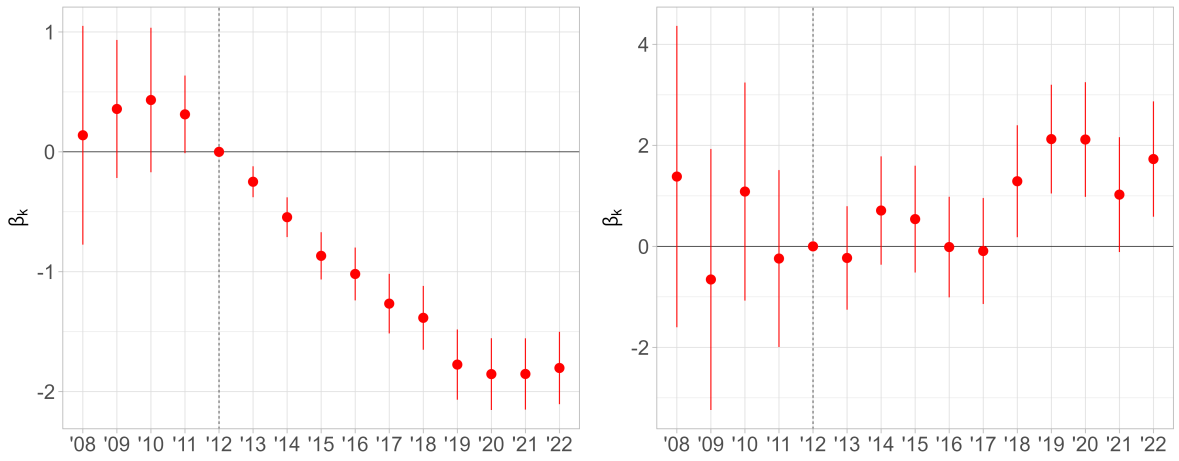


(a) Total Assets (Log)

(b) N. of Employees (Log)

Figure 7 — **Infiltrated Firms Size**

Notes. Observations are at the firm-year level. Panels present the estimated coefficients β_k from regression $Y_{i,t} = \delta_{\text{pbt}} + \gamma_{\text{m}} + \alpha_{\text{i}} + \sum_{\substack{k=09 \\ k \neq 13}}^{22} \beta_k \cdot \text{Infiltrated}_i \cdot \mathbf{1}\{\text{t=k}\}_k + \epsilon_{i,t}$ along with 95% confidence intervals. Panels 7a, 7b report results for total assets and number of employees for firm i in year t. All variables are expressed in logs. The sample is restricted to firms with a predicted participation probability of over 50%, grouped into 10-percentage-point bins based on predicted probabilities. Fixed effects include province-year-likelihood group fixed effects (δ_{pbt}), municipality fixed effects (γ_{m}), and firm fixed effects (α_{i}). Standard errors are clustered at the firm level to account for the level of treatment.

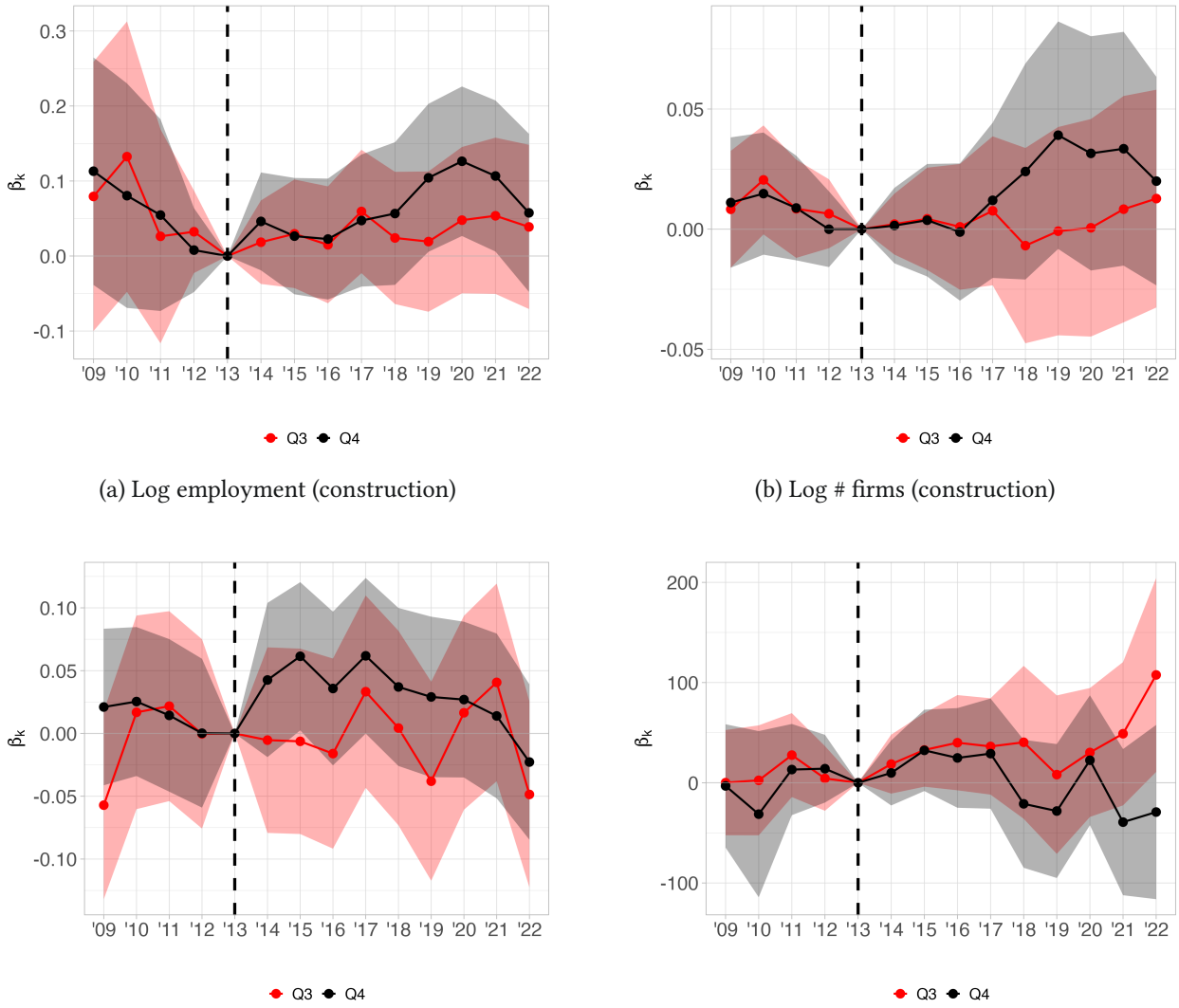


(a) Revenues (Log)

(b) Revenues not from public procurement (IHS)

Figure 8 — **Infiltrated Firms Performance**

Notes. Observations are at the firm-year level. Panels present the estimated coefficients β_k from regression $Y_{i,t} = \delta_{\text{pbt}} + \gamma_{\text{m}} + \alpha_{\text{i}} + \sum_{\substack{k=09 \\ k \neq 13}}^{22} \beta_k \cdot \text{Infiltrated}_i \cdot \mathbf{1}\{\text{t=k}\}_k + \epsilon_{i,t}$
along with 95% confidence intervals. Panels 8a, 8b report results for total revenues and revenues not from public procurement for firm i in year t. Both variables are expressed in logs. The sample is restricted to firms with a predicted participation probability of over 50%, grouped into 10-percentage-point bins based on predicted probabilities. Fixed effects include province-year-likelihood group fixed effects (δ_{pbt}), municipality fixed effects (γ_{m}), and firm fixed effects (α_{i}). Standard errors are clustered at the firm level to account for the level of treatment.



(a) Log employment (construction)

(b) Log # firms (construction)

(c) Log # new firms (construction)

(d) Income per capita

Figure 9 — **Local Economic Effects of Anti-Mafia Reform**

Notes. Observations are at the municipality-year level. Panels present the estimated coefficients β_k^{Q3} and β_k^{Q4} from regression $Y_{mt} = \sum_{k \neq 2013} \beta_k^{Q3} \cdot \text{Q3}_m \times \mathbf{1}[t = k] + \sum_{k \neq 2013} \beta_k^{Q4} \cdot \text{Q4}_m \times \mathbf{1}[t = k] + \alpha_m + \gamma_{pt} + \delta_{dt} + \varepsilon_{mt}$ along with 95% confidence intervals. Treatment intensity is measured as the share of procurement contracts won by firms with suspected criminal ties in the pre-reform period (2009-2013), with Q3 representing municipalities in the third quartile (50th-75th percentiles) and Q4 representing the fourth quartile (above 75th percentile) of this distribution. Panel (a) shows log total employment in construction firms; panel (b) shows log number of active construction firms; panel (c) shows log number of newly incorporated construction firms; panel (d) shows municipal income per capita calculated as total taxable income divided by resident population. Year 2013 is omitted as the reference period. Fixed effects include municipality fixed effects (α_m), year $\times$ province fixed effects (γ_{pt}), and year $\times$ population category fixed effects (δ_{dt}). Standard errors are clustered at the municipality level.

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A — Appendix

B — Machine Learning Algorithm for Participation in Procurement Market

In this section I discuss the machine learning model training, selection and evaluation.

B.1. Dataset Preparation

The dataset contains firm-level information on public procurement contracts and firm participation for the years leading the policy (2009–2013). For data preparation I follow Ash, Galletta, and Giommoni (2020) and apply the following steps.

- **Subsetting:** The data is restricted to contracts closed before 2014 to focus on the pre-2014 period. Key variables include `p_iva`, `municip`, `province`, and contract details.
- **Count Variables:** The number of contracts won (`n_contr_win`) and participated (`n_contr_part`) are defined for each firm in each year.
- **Balanced Panel:** We create a balanced panel dataset by merging firm IDs with all possible year combinations (2009-2013), ensuring that each firm has data for all years.
- **Imputation:** Missing values for balance sheet variables (e.g., `TOAS`, `OPRE`, `LTDB`) are filled using mean imputation within firms. For categorical variables like `municip` and `province`, missing values are filled using forward and backward filling.

The full count dataset includes all firms incorporated in the provinces under consideration, totaling 230,391 firms. Out of these, 115,907 firms have balance sheet information, while 114,484 do not. Many of these firms neither participate in nor win any public contracts during the period of interest. To enhance the predictive power of the models, we consider a subset of firms that have either won or participated in at least one public procurement tender between 2009 and 2013. This subset includes 8569 firms, 6765 of which have balance sheet information and 1804 that do not. All the models are run separately for firms with balance sheet data and without.

The final set of variables is:

- **won_dummy_t1, won_dummy_t2, won_dummy_t3, won_dummy_t4:** Binary variables indicating whether a firm won at least one public procurement tender in years $t-1$, $t-2$,

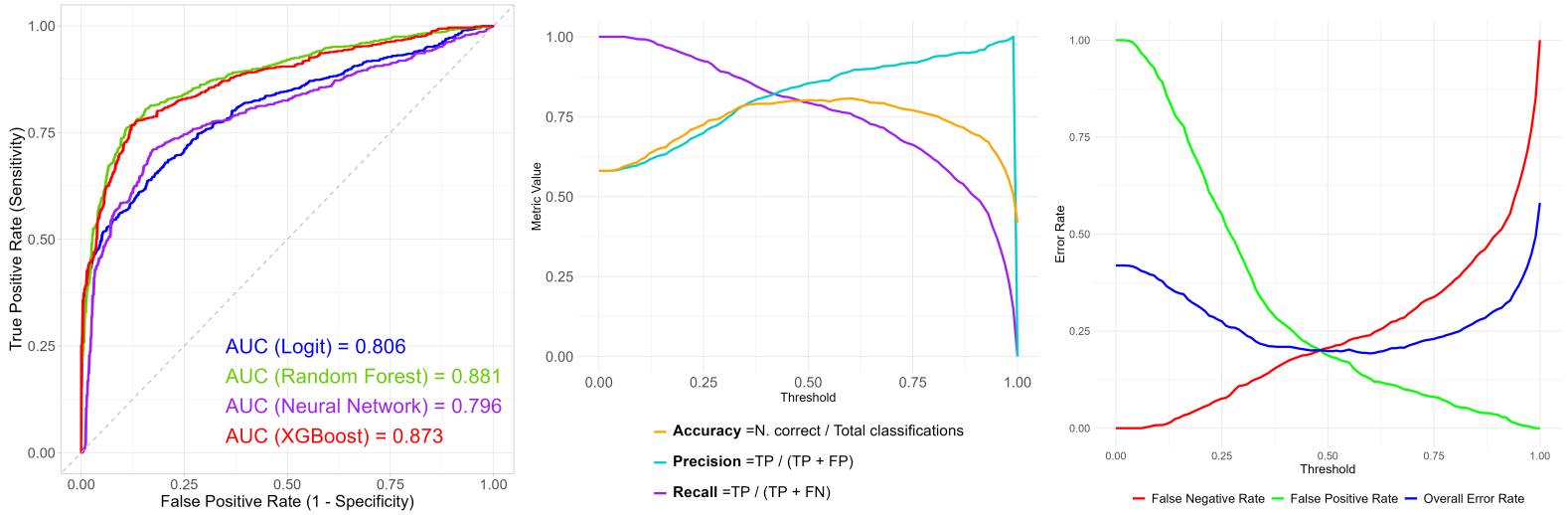
$t-3$, and $t-4$, respectively.

- **part_dummy_t1, part_dummy_t2, part_dummy_t3, part_dummy_t4:** Binary variables indicating whether a firm participated in at least one public procurement tender in years $t-1$, $t-2$, $t-3$, and $t-4$, respectively.
- **log_n_contr_won_t1, log_n_contr_won_t2, log_n_contr_won_t3, log_n_contr_won_t4:** Natural logarithm of the number of tenders won by the firm in years $t-1$, $t-2$, $t-3$, and $t-4$.
- **log_n_contr_part_t1, log_n_contr_part_t2, log_n_contr_part_t3, log_n_contr_part_t4:** Natural logarithm of the number of tenders participated in by the firm in years $t-1$, $t-2$, $t-3$, and $t-4$.
- **log_importo_lotto_t1, log_importo_lotto_t2, log_importo_lotto_t3, log_importo_lotto_t4:** Natural logarithm of the value of tenders (in Euros) won by the firm in years $t-1$, $t-2$, $t-3$, and $t-4$.
- **log_TOAS_t1, log_TOAS_t2, log_TOAS_t3, log_TOAS_t4:** Natural logarithm of the firm's total assets in years $t-1$, $t-2$, $t-3$, and $t-4$.
- **log_OPRE_t1, log_OPRE_t2, log_OPRE_t3, log_OPRE_t4:** Natural logarithm of the firm's operating revenues in years $t-1$, $t-2$, $t-3$, and $t-4$.
- **log_LTDB_t1, log_LTDB_t2, log_LTDB_t3, log_LTDB_t4:** Natural logarithm of the firm's long-term debt in years $t-1$, $t-2$, $t-3$, and $t-4$.
- **log_LOAN_t1, log_LOAN_t2, log_LOAN_t3, log_LOAN_t4:** Natural logarithm of the firm's short-term loans in years $t-1$, $t-2$, $t-3$, and $t-4$.
- **log_SHFD_t1, log_SHFD_t2, log_SHFD_t3, log_SHFD_t4:** Natural logarithm of the firm's shareholder funds in years $t-1$, $t-2$, $t-3$, and $t-4$.
- **log_FIAS_t1, log_FIAS_t2, log_FIAS_t3, log_FIAS_t4:** Natural logarithm of the firm's fixed assets in years $t-1$, $t-2$, $t-3$, and $t-4$.
- **RTAS_t1, RTAS_t2, RTAS_t3, RTAS_t4:** firm's Return on Assets in years $t-1$, $t-2$, $t-3$, and $t-4$.
- **ETMA_t1, ETMA_t2, ETMA_t3, ETMA_t4:** firm's EBITDA margin (EBITD/Revenues) in years $t-1$, $t-2$, $t-3$, and $t-4$.

• **YEARINC:** Year of the firm's incorporation.

B.2. Training and Model Selection

The data is separated into two subsamples: firms with balance sheet data (budget firms) and firms without balance sheet data (non-budget firms). For both subsamples, I split the data into 80% training and 20% testing sets. The training set is used to fit the models, while the testing set is reserved for out-of-sample predictions. More specifically, after training the models on the 80% training data, I apply these models to the 20% testing data to generate predictions. This allows me to evaluate the model's performance on data it has not seen before. Four different types of classifiers are used in the analysis: logit, random forest, neural networks and extreme gradient boosting. The different models ability to correctly predict outcomes in the testing dataset is summarized in Figure B1a. The area under the curve (AUC) is maximal with the extreme gradient boosting model.



(a) ROC Curves (b) Performance Metrics (c) Error Rates

Figure B1 — Model Performance for Procurement Participation Prediction. Models (Logistic Regression, XGBoost, Random Forest, Neural Network) trained on 2009-2012 firm data using lagged financial indicators and procurement history from the previous 4 years. Data split 80% training, 20% test on incumbent firms. Panel (a) shows ROC curves plotting True Positive Rate against False Positive Rate at various classification thresholds, with AUC values indicating discriminatory power. Panel (b) displays the trade-off between Recall, Precision, and Accuracy across thresholds for the XGBoost model. Panel (c) shows False Negative Rate, False Positive Rate, and Overall Error Rate variations across classification thresholds for XGBoost. Performance metrics: Recall = TP/(TP + FN); Precision = TP/(TP + FP); Accuracy = (TP + TN)/(TP + TN + FP + FN); False Negative Rate = FN/(TP + FN); False Positive Rate = FP/(TN + FP).

C — Robustness and Placebo Tests

This appendix presents comprehensive robustness and placebo tests to validate the main empirical findings, addressing concerns about sample selection, survivor bias, and alternative explanations for the observed effects.

C.1. Sample Composition Robustness Tests

C.1.1. Firms Alive in 2018

The effects observed around the policy may reflect unrelated shocks in 2013 (e.g. European sovreign debt crisis) that affect treatment and control trajectories differently.

Intuition Firms that survive beyond 2018 are likely more resilient and less exposed to short-term macro or regulatory shocks.

I estimate the main specification from equation 1 restricting the sample to firms that remain active (with balance sheet data available) through at least 2018. Additionally, I exclude the year 2008 to focus on the core analysis period and maintain statistical power. This restriction ensures that both treatment and control groups contain only firms with demonstrated long-term viability, eliminating potential bias from differential survival rates. The test confirms that the policy impact reflects changes in performance among continuing firms rather than selective exit.

C.1.2. Exclusion of Direct Competitors

Observed effects might be predominantly driven by competitive dynamics rather than the direct impact of transparency requirements (SUTVA violation).

Intuition I estimate equation 1 excluding firms that directly competed with infiltrated firms in procurement tenders during the pre-reform period (2008-2013). The identification procedure follows these steps:

1. Identify all tenders where at least one infiltrated firm participated before 2013
2. Identify all non-infiltrated firms that participated in these same tenders
3. Exclude these "direct competitor" firms from the analysis sample

This specification isolates the pure policy effect by removing firms that might have benefited from reduced competition due to infiltrated firms' exit from procurement markets.

C.2. Survivor (Loser) Bias Tests

I develop formal tests to address concerns that the results might be driven by survivor bias rather than a causal effect of the anti-mafia policy. The survivor bias critique operates at two levels: first, that firms absent from the white list may simply be "natural losers" with unobserved characteristics predicting poor performance that are neither detected by the machine learning algorithm nor related to criminal connections; second, that even within prediction buckets of firms with identical predicted participation probabilities, white listing may merely identify "natural winners" from residual unobservable variation unrelated to organized crime.

This section presents the analytical framework demonstrating that, under specific assumptions, survivor bias alone cannot explain our empirical findings.

Analytical Framework I define the following notation:

- $Y_{i,t}$: Outcome variable for firm i at time t
- D_i: Treatment indicator (1 if firm is not on white list, 0 if on white list)
- P_i: Predicted probability of procurement participation
- $Post_t$: Indicator for post-reform period
- ε_i: Unobservable factors affecting both selection and outcomes

Within the potential outcomes framework, we define:

- $Y_{i,t}(1)$: Outcome if firm is not white-listed
- $Y_{i,t}(0)$: Outcome if firm is white-listed

The observed outcome is:

$$Y_{i,t} = D_i \cdot Y_{i,t}(1) + (1 - D_i) \cdot Y_{i,t}(0) \tag{C.1}$$

The true treatment effect for firm i is:

$$\tau_i = Y_{i,t}(1) - Y_{i,t}(0) \tag{C.2}$$

Selection Mechanism The selection into treatment (non-listing) is determined by:

$$D_i = g(P_i, \varepsilon_i) \tag{C.3}$$

where unobservable factors ε_i affect both selection and outcomes:

$$Cov(\varepsilon_i, Y_{i,t}) \neq 0 \text{ and } Cov(\varepsilon_i, D_i) \neq 0 \tag{C.4}$$

These two conditions establish that unobservable factors ε_i simultaneously influence both the treatment assignment D_i and the potential outcomes $Y_{i,t}$. When both of these conditions hold, this generates the of endogeneity known as selection bias.

To see why, consider that for any firm i, we observe only one potential outcome—either $Y_{i,t}(1)$ or $Y_{i,t}(0)$—depending on whether the firm is treated or not. If treatment assignment were random or independent of potential outcomes, we could write:

$$E[Y_{i,t}(0)|D_i = 1] = E[Y_{i,t}(0)|D_i = 0] \tag{C.5}$$

However, due to the correlation between ε_i and both D_i and $Y_{i,t}$, this equality does not hold. Instead, we have:

$$E[Y_{i,t}(0)|D_i = 1] \neq E[Y_{i,t}(0)|D_i = 0] \tag{C.6}$$

This inequality is the formal definition of selection bias: the potential outcome under no treatment differs systematically between treated and untreated firms, even in the absence of any true treatment effect.

Difference-in-Differences Estimands The difference-in-differences (DiD) approach aims to identify causal effects by comparing changes in outcomes between treated and control groups before and after a policy intervention.

Our DiD estimator is:

$$\begin{aligned}\hat{\tau} = &\{E[Y_{i,t}|D_i = 1, Post_t = 1] - E[Y_{i,t}|D_i = 0, Post_t = 1]\} \\ &- \{E[Y_{i,t}|D_i = 1, Post_t = 0] - E[Y_{i,t}|D_i = 0, Post_t = 0]\}\end{aligned} \tag{C.7}$$

To understand what this estimand captures, substitute the observed outcomes with potential outcomes:

$$E[Y_{i,t}|D_i = 1, Post_t = 1] = E[Y_{i,t}(1)|D_i = 1, Post_t = 1] \tag{C.8}$$

$$E[Y_{i,t}|D_i = 0, Post_t = 1] = E[Y_{i,t}(0)|D_i = 0, Post_t = 1] \tag{C.9}$$

$$E[Y_{i,t}|D_i = 1, Post_t = 0] = E[Y_{i,t}(0)|D_i = 1, Post_t = 0] \tag{C.10}$$

$$E[Y_{i,t}|D_i = 0, Post_t = 0] = E[Y_{i,t}(0)|D_i = 0, Post_t = 0] \tag{C.11}$$

Note that in the pre-reform period ($Post_t = 0$), all firms effectively face the same potential outcome $Y_{i,t}(0)$ since the policy has not yet been implemented.

Analytical Decomposition I now decompose the estimand to identify both the true treatment effects and potential selection bias:

$$\begin{aligned}\hat{\tau} = &\{E[Y_{i,t}(1)|D_i = 1, Post_t = 1] - E[Y_{i,t}(0)|D_i = 0, Post_t = 1]\} \\ &- \{E[Y_{i,t}(0)|D_i = 1, Post_t = 0] - E[Y_{i,t}(0)|D_i = 0, Post_t = 0]\}\end{aligned} \tag{C.12}$$

To isolate the treatment effect, add and subtract $E[Y_{i,t}(0)|D_i = 1, Post_t = 1]$:

$$\begin{aligned}\hat{\tau} = &\{E[Y_{i,t}(1)|D_i = 1, Post_t = 1] - E[Y_{i,t}(0)|D_i = 1, Post_t = 1]\} \\ &+ \{E[Y_{i,t}(0)|D_i = 1, Post_t = 1] - E[Y_{i,t}(0)|D_i = 0, Post_t = 1]\} \\ &- \{E[Y_{i,t}(0)|D_i = 1, Post_t = 0] - E[Y_{i,t}(0)|D_i = 0, Post_t = 0]\}\end{aligned} \tag{C.13}$$

The first term in braces is the true average treatment effect:

$$\tau = E[Y_{i,t}(1)|D_i = 1, Post_t = 1] - E[Y_{i,t}(0)|D_i = 1, Post_t = 1] \tag{C.14}$$

The remaining terms capture the selection bias:

$$\begin{aligned} &\{E[Y_{i,t}(0)|D_i = 1, Post_t = 1] - E[Y_{i,t}(0)|D_i = 0, Post_t = 1]\} \\ &- \{E[Y_{i,t}(0)|D_i = 1, Post_t = 0] - E[Y_{i,t}(0)|D_i = 0, Post_t = 0]\} \end{aligned} \tag{C.15}$$

Under the parallel trends assumption, we have:

$$\begin{aligned} &E[Y_{i,t}(0)|D_i = 1, Post_t = 1] - E[Y_{i,t}(0)|D_i = 1, Post_t = 0] \\ &= E[Y_{i,t}(0)|D_i = 0, Post_t = 1] - E[Y_{i,t}(0)|D_i = 0, Post_t = 0] \end{aligned} \tag{C.16}$$

This assumption states that, in the absence of treatment, the average outcomes for both groups would have followed parallel paths.

Rearranging:

$$\begin{aligned} &E[Y_{i,t}(0)|D_i = 1, Post_t = 1] - E[Y_{i,t}(0)|D_i = 0, Post_t = 1] \\ &= E[Y_{i,t}(0)|D_i = 1, Post_t = 0] - E[Y_{i,t}(0)|D_i = 0, Post_t = 0] \end{aligned} \tag{C.17}$$

This means that the selection bias in the post-period is equal to the selection bias in the pre-period. Under this assumption, the difference-in-differences estimator perfectly controls for selection bias, and we get:

$$\hat{\tau} = \tau \tag{C.18}$$

However, if the parallel trends assumption is violated due to time-varying unobservables, we

have:

$$E[Y_{i,t}(0)|D_i = 1, Post_t = 1] - E[Y_{i,t}(0)|D_i = 0, Post_t = 1]$$
$$\neq E[Y_{i,t}(0)|D_i = 1, Post_t = 0] - E[Y_{i,t}(0)|D_i = 0, Post_t = 0] \tag{C.19}$$

If unobservable factors affecting selection remain constant over time, then $\Delta = \Delta_0$, and we recover $\hat{\tau} = \tau$. However, if these factors change, our estimator captures both the true treatment effect and the change in selection bias.

For simplicity, and under the assumption that pre-period selection bias is effectively controlled for by our event study design, we can write:

$$\hat{\tau} = \tau + \Delta \tag{C.20}$$

where Δ now represents any remaining selection bias not controlled for by the DiD design.

C.2.1. Test Based on Probability Distribution Homogeneity

Key Assumption (Homogeneous Treatment Effects Across Probability Distribution) I assume that the true causal effect of the anti-mafia policy is homogeneous across firms with different predicted participation probabilities:

$$\tau_H = \tau_L = \tau \tag{C.21}$$

This assumption allows me to isolate selection bias effects from true policy effects in the subsequent analysis.

Empirical Test To test whether selection bias alone can explain our results, I estimate equation 1 separately for firms with different predicted participation probabilities:

- $H_i = \mathbf{1}\{P_i \geq 0.5\}$: High predicted probability firms (main sample)
- $L_i = \mathbf{1}\{P_i < 0.5\}$: Low predicted probability firms (placebo sample)

Intuition Under the survivor bias hypothesis, white listing identifies firms with inherent performance advantages that are not captured by the machine learning algorithm and are unrelated to

ties with organized crime. If this were true, the absence of white listing would signal poor performance potential regardless of its predicted procurement participation based on observables, since the underlying quality differential would be orthogonal to procurement participation patterns.

Applying the decomposition to both subgroups, we have:

$$\hat{\tau}_H = \tau + \Delta_H \tag{C.22}$$

$$\hat{\tau}_L = \tau + \Delta_L \tag{C.23}$$

where Δ_H and Δ_L are the selection bias terms for high and low probability firms, respectively.

The empirical analysis yields the following results:

- $\hat{\tau}_H < 0$ and statistically significant (high probability firms)
- $\hat{\tau}_L \approx 0$ and not statistically significant (low probability firms)

This implies:

$$\hat{\tau}_H - \hat{\tau}_L = (\Delta_H - \Delta_L) \neq 0 \tag{C.24}$$

Null Hypothesis (Selection Bias Only) If the observed effects are entirely driven by selection bias, specifically **survivor** bias where white listing merely identifies firms that would have succeeded regardless of the policy, then the vetting process should be equally effective at identifying "natural winners" across the entire distribution of predicted participation probabilities. Under this hypothesis, we would expect:

$$\Delta_H = \Delta_L \tag{C.25}$$

and therefore:

$$\hat{\tau}_H - \hat{\tau}_L = 0 \tag{C.26}$$

However, our empirical finding that $\hat{\tau}_H - \hat{\tau}_L \neq 0$ contradicts this expectation, providing evidence that our results reflect true causal effects rather than pure selection bias. These results are shown in Figure C1, where the placebo test on low predicted probability firms (green points) yields null effects, while the main specification on high probability firms (red points) shows significant negative effects.

C.2.2. Test Based on Fictional Treatments Within Prediction Buckets

Key Assumption (Homogeneous Treatment Effects Across Time) We assume the true causal effect of the anti-mafia policy is constant across time periods:

$$\tau_t = \tau \text{ for all } t \tag{C.27}$$

This allows us to compare treatment effects across different time periods while allowing for heterogeneous effects across the probability distribution.

Fictional Treatment Design I create fictional treatments by modifying equation 1, replacing the infiltration dummy with a fictional treatment indicator based on contract outcomes within prediction buckets:

$$F_{i,t} = \mathbf{1}\{n_contr_part_{i,t} > 0 \text{ and } n_contr_won_{i,t} = 0\} \tag{C.28}$$

I implement two fictional treatments[15]:

- **Lost in 2013:** Firms that participated in tenders but won zero contracts in 2013, using 2013 ML predictions for sample selection
- **Lost in 2014:** Firms that participated in tenders but won zero contracts in 2014, using 2014 ML predictions for sample selection

Crucially, this occurs within strata defined by:

$$S_{i,t} = P_{i,t} \times Post_t \times Province_i \tag{C.29}$$

This ensures we compare firms with identical predicted participation probabilities in the same time period and province.

Intuition If white listing merely identifies "natural winners" within prediction buckets, then any mechanism that distinguishes winners from losers—including fictional treatments based on

15. This exercise is limited to estimating 2013 and 2014 probabilities due to data constraints. The machine learning model requires four-year lagged variables, and since procurement outcomes are only available starting in 2008, earlier predictions cannot be reliably generated with the selected machine learning model.

contract outcomes in 2013 or 2014—should yield similar negative effects across all outcomes and time periods. Conversely, if our results reflect genuine policy impacts, fictional treatments should produce muted effects since they lack the underlying causal mechanism, with any mechanical effects on contract outcomes confined to the specific treatment year and no persistent spillovers to other performance measures.

Identification Strategy The identification relies on within-stratum variation:

$$\hat{\tau}_t^{fictional} = E[Y_{i,s}|F_{i,t} = 1, S_{i,t}] - E[Y_{i,s}|F_{i,t} = 0, S_{i,t}] \tag{C.30}$$

By conditioning on $S_{i,t} = P_{i,t} \times Post_t \times Province_i$, we compare firms that are:

- Identical in predicted participation probability
- In the same time period and province
- Different only in whether they won or lost contracts in year t

Empirical Results The fictional treatment test results are presented in Figure 1 for core outcomes, more in detail in Figure C2 for the number of contracts won. For most outcomes, fictional treatments yield muted effects compared to the main specification, providing evidence against the survivor bias hypothesis. Contract outcomes show mechanical effects only in the treatment years, with no persistent post-treatment impacts, consistent with the true causal effect hypothesis.

The combination of both tests—one requiring homogeneous treatment effects across probability distributions and the other across time periods—provides robust evidence against various forms of selection bias.

C.3. Alternative Sample Definitions

C.3.1. Low Predicted Probability Firms

Concern: Firms labeled as infiltrated may be those with private knowledge of future decline and opt out of vetting in anticipation of exit or hardship.

Intuition: If selection into vetting merely captures firms' own private expectations about performance, we should observe similar negative outcomes even among firms unlikely to bid in procurement.

As a placebo test, I estimate equation 1 using only firms with predicted participation probabilities below 0.5. These firms represent those with low expected benefits from public procurement participation, making them less likely to be systematically affected by transparency requirements.

The theoretical framework suggests that these firms should show minimal response to the policy, as they have limited engagement with public procurement markets. Finding significant effects in this subsample would suggest spurious correlation or model misspecification.

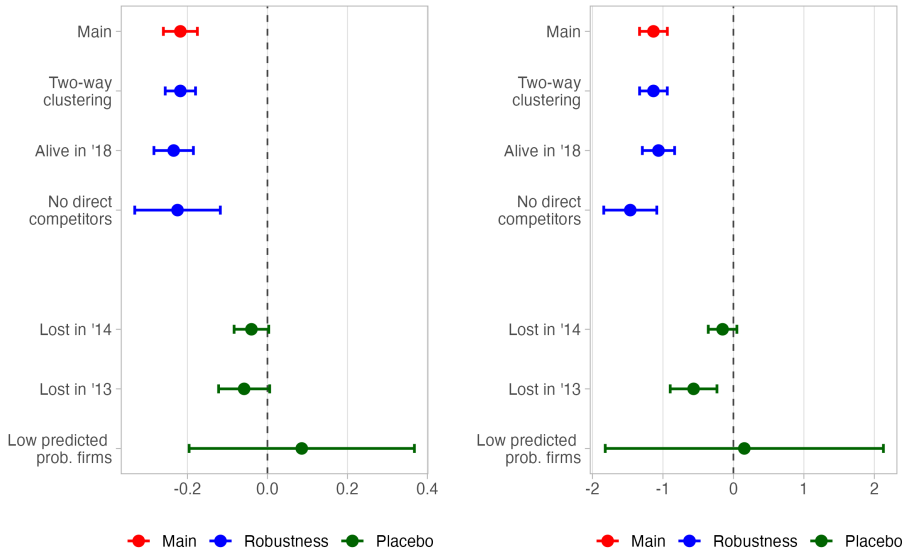
The sample is constructed using firms with $\max(\text{predicted_part_dummy_2014}, \text{predicted_won_dummy_2014})$ 0.5, and firms are grouped into probability bins using 0.1-unit intervals between 0 and 0.5.

C.4. Robustness Tests Summary

Figure C1 presents a comprehensive summary of all robustness tests across four key outcome variables: number of contracts won, operating revenues, return on assets, and financial distress. The figure displays coefficient estimates and 95% confidence intervals for each specification, organized into three categories:

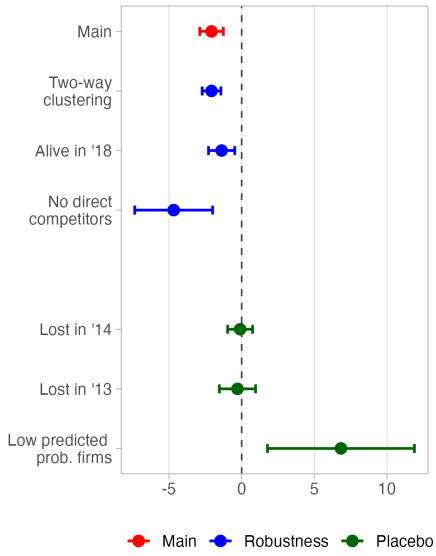
- **Main:** Primary specification (red)
- **Robustness:** Alternative sample definitions that should yield similar results (blue)
- **Placebo:** Tests that should yield null results if the main findings are valid (green)

The consistency of results across robustness specifications and the null findings in placebo tests provide strong support for the validity of the main empirical findings. The robustness tests demonstrate that the estimated effects are not driven by sample selection, survivor bias, competitive spillovers, or other alternative explanations.

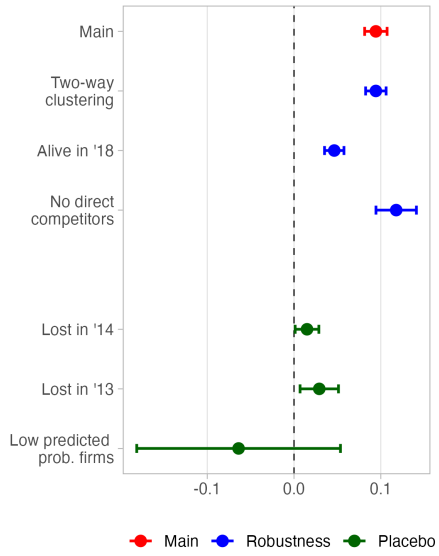


(a) Contracts Won

(b) Operating Revenues

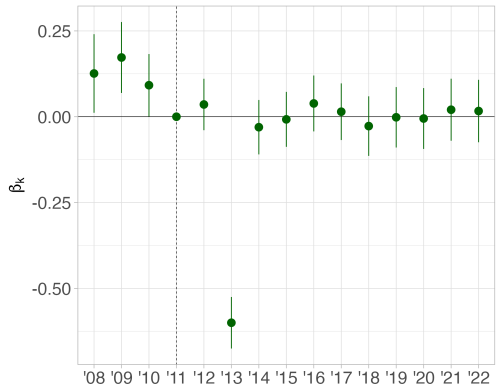


(c) Return on Assets

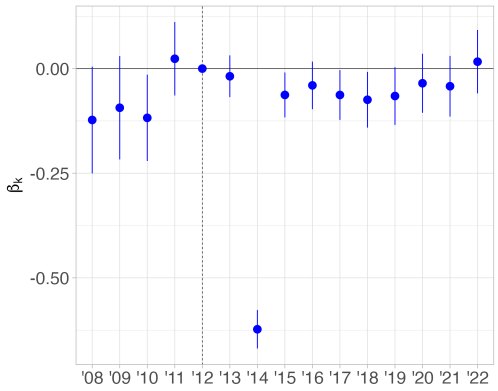


(d) Financial Distress

Figure C1 — Robustness Tests for Main Specification. Coefficient estimates and 95% confidence intervals for the interaction term β in the difference-in-differences specification: $Y_{it} = \alpha + \beta \cdot Bad_i \times Post_t + \gamma X_{it} + \theta_{bin \times province \times t} + \mu_i + \epsilon_{it}$. **Main (red):** Baseline specification with firm fixed effects μ_i, bin×province×year fixed effects $\theta_{bin \times province \times t}$, and clustering by firm. **Robustness checks (blue):** *Two-way clustering* uses firm × province clustering; *Alive in 2018* restricts to firms surviving until 2018; *No direct competitors* excludes firms that competed directly with infiltrated firms pre-2013. **Placebo tests (green):** *Low prob. firms* applies the same specification to firms with predicted participation probability < 0.5; *Lost in '13/'14* uses fictional treatment dummies for firms that lost contracts in 2013/2014 respectively, excluding mechanical years for contract outcomes. The vertical dashed line indicates zero effect.

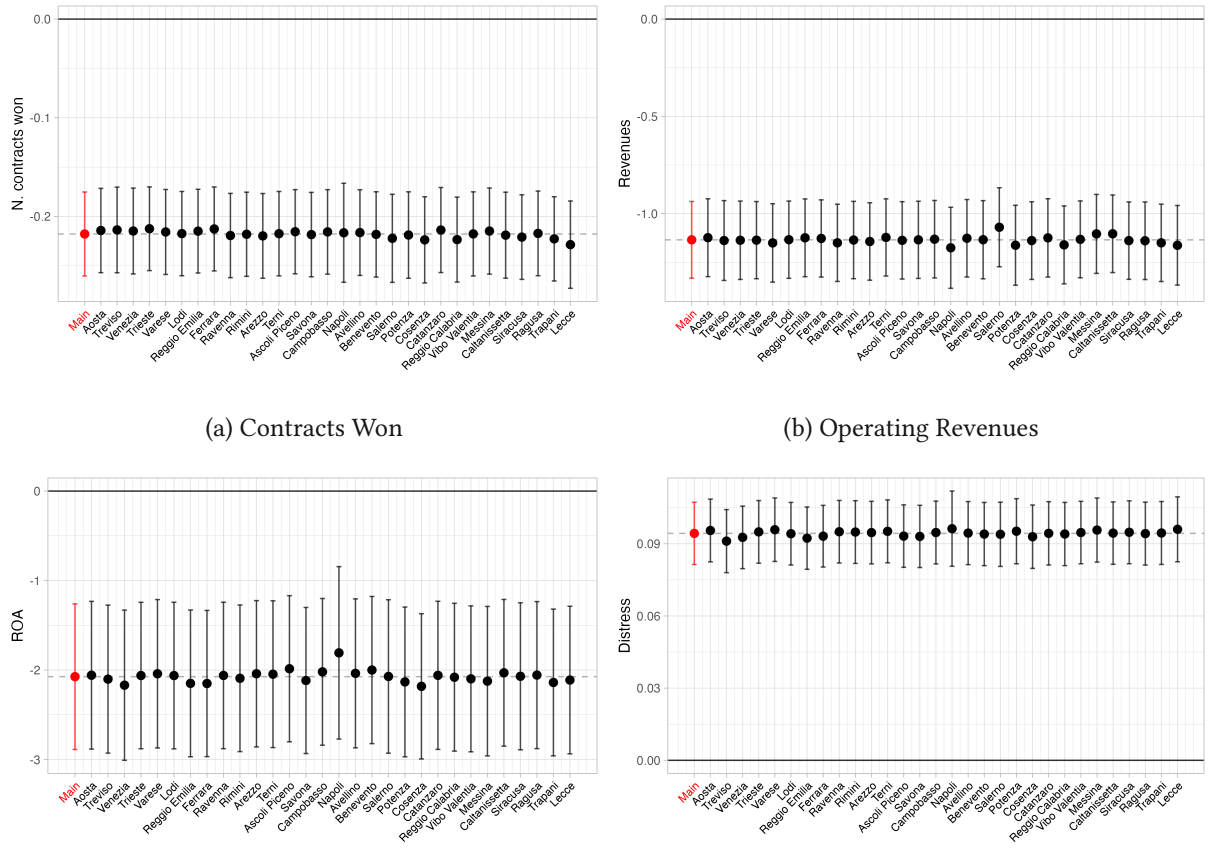


(a) Lost in 2013



(b) Lost in 2014

Figure C2 — Fictional Treatment Test: Contracts Won. Results from fictional treatment specification testing for survivor bias within prediction buckets using firms that lost contracts in specified years. The fictional treatment is defined as $F_{i,t} = \mathbf{1}\{n_contr_part_{i,t} > 0 \text{ and } n_contr_won_{i,t} = 0\}$, comparing firms with identical predicted participation probabilities that differ only in contract outcomes. Under survivor bias, both should show persistent negative effects. Under true causal effects, effects should be muted with no persistent post-treatment impacts.



(a) Contracts Won

(b) Operating Revenues

(c) Return on Assets

(d) Financial Distress

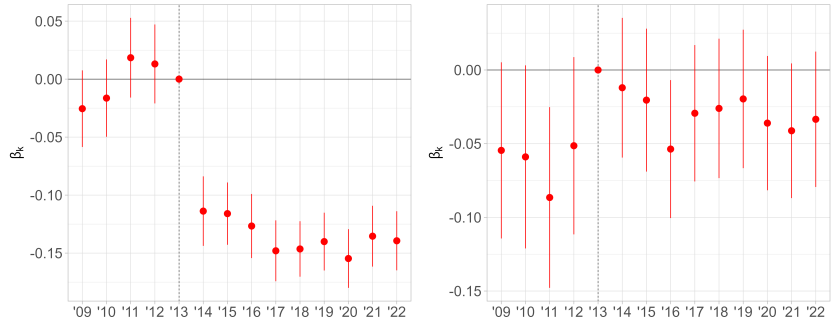
Figure C3 — Leave-One-Province-Out Robustness Tests. Coefficient estimates and 95% confidence intervals for the interaction term β in equation $Y_{it} = \alpha + \beta \cdot Bad_i \times Post_t + \gamma X_{it} + \theta_{bin \times province \times t} + \mu_i + \epsilon_{it}$, estimated by sequentially excluding each province from the sample. Each black dot represents the coefficient estimate when a specific province is excluded, ordered geographically from North to South Italy (left to right). The red dot labeled "Main" shows the baseline coefficient using the full sample. The solid horizontal line at zero indicates no effect, while the dashed gray line shows the main estimate for reference. Provinces are included only if they have at least 50 observations. The stability of coefficients across all leave-one-out specifications demonstrates that the main results are not driven by any particular geographic region and are robust to potential province-specific confounders or outliers.

D — Additional Tables and Figures

Table D1: **Balance Table: Pre-Treatment Characteristics of Infiltrated vs. Vetted Firms**

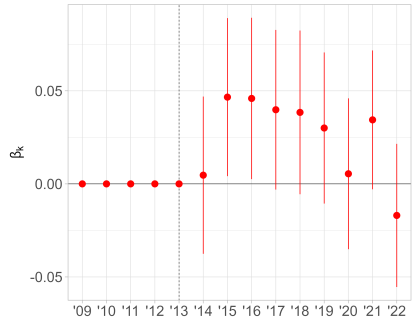
Variable	Infiltrated Firms (1)	Vetted Firms (2)	Difference (1) - (2) (3)
Year of birth	1998.24	1997.39	0.5
Winning rate	0.24	0.26	0.02
Contract amount (log)	13.56	13.59	-0.17
Assets (log)	13.93	14.21	-0.42**
Revenues (log)	13.40	13.71	-0.65**
Debt (log)	8.16	8.69	-0.75
ROA	1.59	2.37	0.39
Profits/Sales	8.01	8.39	1.26
No. employees (log)	2.40	2.61	-0.23
N. of firms	1204	2283	

Notes: This table presents pre-treatment (2009-2011) characteristics of firms classified as infiltrated versus vetted based on predicted participation and winning probabilities. The sample includes firms with predicted participation or winning probability above 50% that appear in the balanced sample. Infiltrated firms are those predicted to have connections to organized crime, while vetted firms are those without such connections. Columns (1) and (2) report mean values for each group. Column (3) presents the coefficient from univariate regressions of each characteristic on an infiltrated firm dummy, controlling for predicted probability bins, province fixed effects, and 2-digit NAICS industry fixed effects. The regression specification is: $Y_i = \alpha + \beta \cdot \text{Infiltrated}_i + \gamma_b + \delta_p + \theta_s + \epsilon_i$, where Y_i is the firm characteristic, γ_b are predicted probability bin fixed effects, δ_p are province fixed effects, and θ_s are industry fixed effects. Financial variables (assets, revenues, debt, equity, employees) are expressed in logarithms. Winning rate is defined as the ratio of contracts won to contracts participated in during the pre-treatment period. Standard errors are clustered at the firm level. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

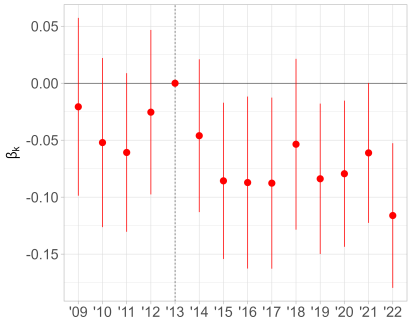


(a) Share of tenders won by infiltrated firms

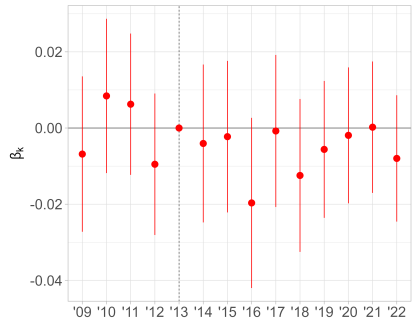
(b) Share of tenders won by vetted incumbents



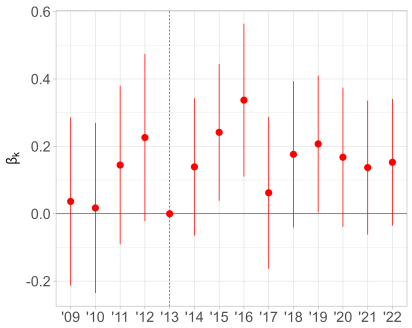
(c) Share of tender won by entrants



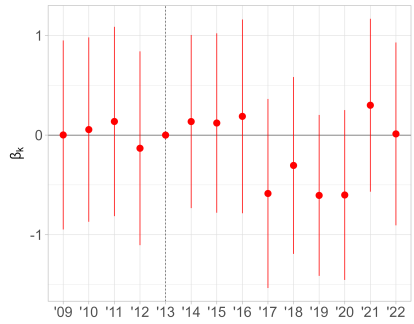
(d) Winners incorporated in tender province



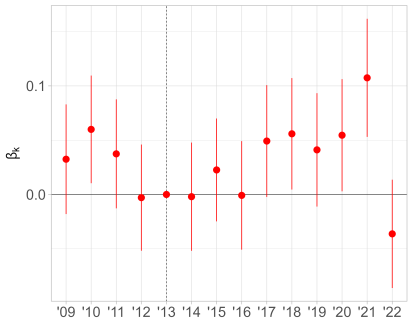
(e) Winning discount on posted price



(f) Bids per tender



(g) Delay



(h) New firms in construction

Figure D1 — **Procurement Market Outcomes: Event Study Analysis**

Notes. Observations are at the municipality-year level. Panels present the estimated coefficients β_k from regression $Y_{m,t} = \delta_{\text{pt}} + \gamma_{\text{m}} + \sum_{\substack{k=2009 \\ k\neq 2013}}^{2022} \beta_k \cdot \text{Infiltrated}_m \cdot \mathbb{1}\{\text{t=k}\} + \epsilon_{m,t}$ along with 95% confidence intervals. Panels D1a-D1c report results for the share of tenders won by infiltrated