Organized Crime, Public Procurement and Firms

Elena Stella*

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Abstract

Can transparency requirements deter organized crime, and what are the long-run consequences on local economies? I exploit Italy's 2013 anti-mafia reform, which mandated police vetting for firms bidding on public contracts. Using detailed procurement records and newly collected police data, I develop a machine-learning approach to identify suspected firms that systematically avoid vetting despite high predicted bidding activity. The reform effectively deters suspected firms from participating in procurement: they contract in size, exit at higher rates, and shift business away from public contracts. Procurement becomes more competitive and less geographically concentrated, with increased entry and reallocation toward out-of-province contractors. Despite the shift away from local firms, employment and income remain stable and new firm creation increases, suggesting that transparency stimulates entrepreneurship rather than harming local economies.

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1 — Introduction

Criminal organizations channel large shares of illicit profits into legal markets (UNODC 2011; Europol 2024). As these organizations become increasingly sophisticated at infiltrating firms that provide cover, diversification, and access to public resources, policymakers face two key challenges. First, identifying and removing infiltrated firms despite having limited information and screening capacity. Second, understanding whether removing infiltrated firms may come at the cost of harming growth in areas deeply entrenched with criminal organizations. While infiltrated firms distort competition (Slutzky and Zeume 2024) and misallocate resources (Pinotti 2015), their removal can disrupt local employment (Szerman 2023), credit allocation (Slutzky, Villamizar-Villegas, and Williams 2020; Ferraz et al. 2023), and shrink already thin local markets (Auriol and Søreide 2017).

In this paper, I examine this trade-off in the context of transparency requirements mandating background checks as a condition for market access. Governments increasingly try to combat criminal infiltration *ex ante* by mandating ownership transparency through disclosure, screening, or audits. Despite the global adoption of these policies (Rossi et al. 2025), little is known about their effectiveness. Can transparency requirements deter infiltration, and what are the long-run consequences for local economies?

I answer these questions by leveraging a major Italian reform that expanded transparency requirements for firms participating in public procurement. Starting in 2013, any firm operating in high-risk sectors must undergo annual criminal background checks conducted by local anti-mafia police before it can bid on public contracts.[1] Firms that pass the screening are included in *white lists* maintained by local police offices. I assemble novel data on police investigations, firms financials and procurement activity, and apply machine-learning methods to identify firms that are likely to have criminal ties. I then use a difference-in-differences strategy to quantify the deterrent effect of transparency requirements and their long run implications for local economic activity.

Evaluating the effects of transparency requirements—particularly on criminally infiltrated firms—poses two empirical challenges. First, infiltration status is not simply unobserved; it is deliberately hidden. Criminal firms operate to appear legitimate, so absence of detection is itself

1. High-risk sectors include construction, waste management, transportation, catering, and environmental services.

an equilibrium outcome (Pinotti 2020). Existing indicators capture only detected cases, typically at broad geographic levels such a provinces or regions, and thus reflect enforcement intensity as much as underlying wrongdoing. The second challenge is establishing causal effects. For example, both firms' compliance with transparency requirements 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 challenge of measuring criminal infiltration, I develop a methodology that infers suspected infiltration from systematic deviations between observed and predicted firm choices. This approach combines newly collected administrative data with machine-learning inference and consists of three steps. 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 for firms operating in the sector most affected by the reform: construction.[3]

As a second step, I train a machine learning algorithm on pre-policy data (2008–2012) to predict each firm's likelihood of participating in public procurement absent the vetting requirement. The model 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 criminal ties those firms with high predicted participation probabilities that are systematically absent from white lists in the post-reform period. The intuition is that firms with high expected benefits from procurement participation that systematically avoid vetting for a decade following policy implementation exhibit suspicious behavior.

Using this methodology I classify approximately 1,200 firms as suspected of criminal infiltra-

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.

3. Construction firms onstitute more than 60% of firms subject to these requirements.

tion—around 18% of incumbents in the public procurement market at the onset of the policy.[4] Before the introduction of the white list provision, these suspected firms secured almost 8 billion Euros in public contracts—20% of total contract value. I validate my measure against external indicators of criminal activity. Suspected firms operate disproportionately in municipalities with documented mafia presence, and an AI-assisted firm-level web search shows that they are significantly more likely to have documented criminal investigations in news outlets.

To address the identification challenge, I focus my analysis on firms with high predicted participation probabilities that were incumbent in the public procurement market at the onset of the 2013 reform. I employ a difference-in-differences strategy comparing outcomes between firms suspected of organized crime ties and non-suspected firms. 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.

The first part of the paper investigates whether transparency requirements can deter infiltration. The policy successfully drives suspected firms out of procurement markets: after the reform these companies win 30% fewer tenders relative to non-suspected firms, compared to pre-reform baseline differences. 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 leads suspected firms to a broader performance 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.

4. This scale of suspected criminal infiltration is consistent with Decarolis et al. (2025), who find that 17% of public works contracts in Italy between 2000 and 2016 were awarded to firms investigated for corruption by law enforcement authorities.

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 (Daniele and Dipoppa 2023), 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.

A potential concern is that my results are driven by misclassification of suspected firms. If firms classified as suspected exited procurement due to adverse economic shocks rather than criminal ties, my findings would reflect economic distress rather than anti-mafia enforcement effects. I address this concern in several complementary ways. First, the suspected classification is based on pre-reform predictions of which firms would have participated in public procurement absent the vetting requirement, ensuring that treatment reflects *ex ante* incentives rather than post-reform behavior.[5] Second, I rule out misclassification bias by showing that prediction errors from the machine-learning model are not systematically correlated with white list presence.[6] Third, any alternative shock must account for a more than doubled exit rate among high-predicted probability firms right when the policy becomes effective, with no comparable pattern in other years.[7] Finally, extensive robustness tests across alternative samples and placebo treatments show no comparable effects.

In the second part of the paper, I study the long run economic effects of transparency requirements. I investigate 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

5. Even if in a different setting, my methodology is close in spirit to Card, Colella, and Lalive (2025). The authors use pre-policy employers characteristics to predict employers' likelihood of stating gender preferences on job postings. They then use the passage of a ban to examine how policy changes affect predicted users of the banned practice.

6. This correlation test addresses concerns about systematic measurement error in ML-generated treatment variables, as discussed in Battaglia et al. (2024). The absence of correlation between prediction errors and white list status suggests that ML misclassification does not systematically bias treatment assignment. See Appendix Table B5 for detailed results.

7. To establish the abnormal exit rate I apply the machine learning algorithm to years outside of the policy. For each cohort of high-predicted participation probability firms for years 2010 to 2015 I compute their permanent exit rates from procurement. In 2014 the exit rate more than doubles from its baseline of 3%, with no comparable increases in any other year.

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 that tenders left vacant by suspected firms are captured almost entirely by 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 questions. 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, which makes it a potential channel for regulatory circumvention (Bosio et al. 2022).[8] Data on subcontractor identities is typically unavailable, making this channel difficult to study. To assess this risk, I compile a unique dataset identifying subcontractors for 30% of subcontracted tenders. I 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.

A second key question is that the shift away from local contractors might imply adverse effects on local employment and income. To explore the long run effects on local economic growth and entrepreneurship, I focus on municipalities with the highest exposure to suspected criminal firms and use quartile regressions to capture non-linear responses, alongside event-study models to trace dynamic effects over time. I find no evidence of negative impacts on employment or per capita income. Instead, the most exposed municipalities show signs of increased new firm formation: the number of newly incorporated construction firms appears to rise in the years immediately following the reform, while total firm counts initially remain relatively stable as new entrants replace exiting suspected firms, then show signs of growth over the long run. By 2021, the number of construction firms in the most affected municipalities suggests an increase relative to the pre-reform baseline.

8. 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.

These findings provide evidence that transparency requirements are effective in removing suspected criminal firms. The policy also transforms local procurement markets, making them more competitive and less geographically concentrated, while fostering entrepreneurship in the most affected areas.

Related Literature. The findings in this paper contribute to several strands of the literature. First, this paper contributes to the literature that measures organized crime's ability to infiltrate firms and its effects on their 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. These existing metrics predominantly focus on cases that have been detected and often prosecuted. Moreover, they typically capture larger firms while missing smaller businesses that are often the primary targets of criminal infiltration (Transcrime 2017). By contrast, I propose a revealed-preference approach based on firms' strategic decisions to avoid vetting, in the spirit of what Zitzewitz (2012) reviews as the "forensic economics" method. This framework enables the identification of a broader set of potentially infiltrated firms that remain invisible to traditional detection methods.

Second, 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; Colonnelli and Prem 2022; Chen, Jin, and Xu 2021; Fenizia and Saggio 2024) or firm-level interventions such as corporate debarment, asset seizures, and corruption investigations (Szerman 2023; Slutzky and Zeume 2024; Ferraz et al. 2023).

In contrast, transparency-based approaches have received far less attention in economics. While the accounting literature has extensively examined mandatory disclosure effects on firm valuations, reporting choices, and compliance behavior (Samuels 2021; Kays 2022; Duro, Heese, and Ormazabal 2019; Aobdia 2018; De Simone and Olbert 2022), economics and finance research on transparency reforms remains limited. A smaller set of studies explores equilibrium effects of transparency-enhancing reforms: anti-money laundering rules on bank lending (Slutzky,

Villamizar-Villegas, and Williams 2020), credit restrictions for high-risk sectors (Sachdeva et al. 2023), tax information exchange agreements on offshore evasion and firm value (Bennedsen and Zeume 2018), and strategic bunching below disclosure thresholds in grants programs (Daniele and Dipoppa 2023). These studies shed light on 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.

Third, this paper connects to the literature on corruption in public procurement. Prior work studies how procurement design affects corruption outcomes (Bandiera, Prat, and Valletti 2009; Coviello, Guglielmo, and Spagnolo 2018; Auriol, Straub, and Flochel 2016; Decarolis et al. 2025), the role of transparency mechanisms (Lewis-Faupel et al. 2016), and corrupt behavior by public officials (Chen 2024; Brierley 2020). This paper contributes by leveraging a new measure of suspected criminal status to establish causal evidence on the effectiveness of a widely adopted but understudied feature of procurement systems: vendor integrity requirements.[9]

Fourth, this work contributes to an emerging literature applying machine learning to detect corruption. Studies have developed algorithms to predict corruption (Colonnelli, Gallego, and Prem 2022; López-Iturriaga and Sanz 2018; Ash, Galletta, and Giommoni 2025) and criminal infiltration (Campedelli, Daniele, and Le Moglie 2024) of local governments, and in public contracting (Gallego, Rivero, and Martínez 2021). In this paper I follow Ash, Galletta, and Giommoni (2025) 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.

9. As noted in Auriol and Søreide (2017), vendor integrity standards date back to 1884 in the United States when Congress required contracts to be awarded only to the lowest "responsible" bidder (Act of July 5, 1884, Ch. 217, 23 Stat. 109), and are currently implemented by major procurement systems worldwide, including the UN Global Marketplace, World Bank, and WHO procurement processes.

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. Organized Crime and Public Procurement

Criminal organizations have systematically infiltrated Italian public procurement for decades, making it one of their most lucrative and strategically important revenue streams. Since the 1960s economic boom, traditional mafias evolved from parasitic ties with legitimate enterprises to direct participation in legal markets (Ravenda et al. 2020). Cosa Nostra, 'Ndrangheta, and Camorra transformed from extortion-based operations into business entities capable of competing directly with legitimate firms while exploiting their criminal networks and coercive power. Public contracts, in particular, offer both financial and strategic advantages: they provide a steady flow of public funds, confer social legitimacy, and secure control over local territories and employment (Transcrime 2017).

Public procurement offers particularly favorable conditions for criminal infiltration. It channels large, predictable financial flows (roughly 10% of Italian GDP annually) and involves complex tender procedures dispersed across many contracting authorities. These features create information asymmetries and multiple entry points for manipulation through bid rigging, fraudulent documentation, and collusive arrangements (Transcrime 2017). Criminal organizations exploit these weaknesses by corrupting local politicians, establishing front companies, and intimidating competitors (Calderoni, Caneppele, et al. 2009).

Recognition of these systemic vulnerabilities has prompted successive government interventions to strengthen anti-mafia legislation and regulatory oversight. Over time, policy efforts have evolved from reactive interventions targeting individual cases to comprehensive transparency frameworks aimed at preventing infiltration *ex ante*. The 2013 amendments to the Antimafia Information Law represent the culmination of this process.

2.2. 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.[10] The scope and effectiveness of controls were expanded, with enhanced police investigative tools extending checks to friends and family members.[11] The law harmonized the roles of contracting authorities and police forces and created a centralized database of mafia-related information.

2.3. 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.[12]

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 out on-site inspections. These inspections may involve interviews with managers, employees, and, in some cases, family members.[13]

10. Law 6 November 2012, n. 190; D.P.C.M. 18 April 2013

11. 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.

12. The full list is defined in Law 6 November 2012, n. 190. In my data construction firms account for more than 60% of all firms on the white list (see section 3).

13. The investigation assesses both formal disqualifying conditions (Art. 67) and evidence of attempted mafia

Firms that successfully pass the investigation are added to the white list and become eligible to participate in public tenders.[14] 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

In this section, I describe the data used in my analysis. First I provide details on each data source and outline the dataset construction proces. I then describe the sample selection and present the summary statistics on the analysis sample.

3.1. Data Construction

My analysis draws from a number of different data sources. In this subsection I describe each data used and explain how the data are collected and standardized.

3.1.1. White Lists

My dataset is structured around historical white list records obtained from Italian provincial police offices. To create the dataset, I collected, digitized, and standardized white list archives from 30 provincial police offices that maintained registries of firms passing mandatory anti-mafia screening following the 2013 reform. As explained in Section 2.3, each provincial police office began maintaining these registries after the reform, with the most updated versions posted online for contracting authorities to verify bidder eligibility. However, studying the long-run consequences over the 10 years that followed the reform implementation is impossible without access to historical versions, which are not publicly available. To reconstruct historical white list records, I primarily relied on Freedom of Information Act (FOIA) requests, requesting white list documents from 2014 onward.[15] I complemented this dataset in several ways. First, I systematically

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.

14. 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.

15. In Italian *Domanda di Accesso Pubblico Generalizzato*. The data collection involved two successive rounds of FOIA requests, followed by several interviews with local police offices to verify data completeness and accuracy.

searched archived versions of police office websites using the Internet Archive's Wayback Machine to retrieve past versions of the lists. Second, I downloaded the most updated versions of white lists for the whole of Italy and cross-checked firm registration patterns to identify potential gaps in historical coverage. In total, 30 offices provided usable historical data, either directly through FOIA responses or supplemented with these complementary sources.[16] The historical white list records span 10 years from 2014 to 2023, covering the entire post-reform period. Starting from the obtained sources, I extracted and parsed more than 5,000 pages of unstructured PDF documents, harmonizing formats across provinces and years, and standardizing firm identifiers (tax codes and VAT numbers), headquarters locations, and sector classifications using NAICS industry codes. The resulting dataset covers 28,208 unique firms vetted by the Italian antimafia police between 2013 and 2023.[17] The dataset focuses on firms that successfully completed the vetting process, as records of rejected applications are not made publicly available by police offices. However, aggregate statistics from two provinces reveal that rejection rates were below 2% of total applications, indicating that the policy operates primarily through deterrence rather than enforcement.[18] This pattern suggests that firms not appearing on the white list predominantly chose not to apply rather than being rejected following police investigation.[19]

3.1.2. Firms

To obtain firm-level information on performance and financial health, 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 combine different 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

16. The offices with viable information are: 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. In the cases of Venezia, Reggio Emilia and Trapani, FOIA responses were incomplete and were complemented using archived web snapshots.

17. The dataset includes 6,843 firms that were recorded as having requested registration but for which no successful enrollment was reported; I classify these firms as vetted to be conservative, as many likely abandoned the process due to lost interest or business exit.

18. Reggio Emilia and Napoli provided aggregate rejection rates.

19. This finding is corroborated by interviews with local police forces, which indicated that criminal firms typically avoid applying for white list certification altogether, knowing they would not pass the screening process.

due to sporadic reporting but are key participants in tenders and the most common targets of organized crime infiltration (Transcrime 2014a). As Orbis drops non-reporting firms after a period of 10 years, 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. Following this procedure, I match white-listed firms to their financial records: among firms subject to mandatory reporting requirements (primarily corporations and limited liability companies), over 90% are successfully linked to their balance sheet and income statement information.[20]

3.1.3. Public Procurement

To track firm-level procurement outcomes, I use a database provided by the Public Contracts Observatory at the Italian Anticorruption Authority (ANAC), the public entity that oversees public procurement in Italy. ANAC monitors all public contracts with a reservation price above 40,000 euros, and my dataset covers the universe of ANAC records for public infrastructure between 2008 and 2022.[21] Within public infrastructure, the most relevant contract types are the OG (*Opere Generali*) categories, which include construction of public buildings (OG01) and transportation infrastructure such as roads and bridges (OG03). Together, these categories account for the majority of contracts and procurement expenditure. The dataset reports key contract characteristics, including the identity of the winning bidder, the number of bidders, the percentage discount offered relative to the reservation price, flags for subcontracted tenders, expected contractual duration, and actual completion time for a subset of contracts. The latter allows me to compute delivery delays as the difference between expected and realized duration. Following Decarolis et al. (2025), I apply an inverse hyperbolic sine transformation to the delay measure to address the presence of both negative delays (early delivery) and extreme outliers. A key feature of the dataset is that, for tenders awarded between 2008 and 2016, it reports identities of the full list of bidders, not just the winners. This information is essential for measuring firms' pre-reform presence and tenure in procurement markets. Despite the richness of this data, key firm-level information such

20. Roughly 40% of firms appearing in the white lists are sole proprietorships, which are exempt from mandatory financial reporting in Italy.

21. Italian procurement is broadly divided into three categories: works, goods, and services. My analysis focuses on public works, which represent roughly 25% of total contract value and over 30% of all procurement contracts (Decarolis et al. 2025).

as exact headquarters locations and sector classifications are missing for approximately one-third of participating firms. To address this limitation, I reconstructed headquarters locations and sector classifications using Bureau Van Dijk records and Chamber of Commerce files.[22] This data recovery effort resulted in complete firm-level identifiers for more than 90% of all participants.

3.1.4. Subcontractors

To complement firm-level procurement information and capture firms' indirect participation through subcontracting networks, I use subcontracting records obtained from ANAC. While measuring firms' more indirect roles in procurement is typically challenging due to limited data availability, I was able to obtain data covering approximately 30% of the tenders flagged as involving subcontracted work. These records identify the subcontractors selected by winning bidders and report the value of subcontracted portions relative to the main contract. The subcontracting data allows me to trace firms' complete participation patterns across both primary contracting and secondary subcontracting markets, providing a comprehensive view of market dynamics in response to the regulatory change.

3.2. Sample Statistics

In this subsection I provide descriptive statistics for the key datasets and variables used in my analysis.

Figure 3a shows the geographical distribution of the 30 provincial offices that provided historical records. These provinces account for roughly 20% of Italy's GDP and feature predominantly the south of the country, where the problem of criminal infiltration is particularly severe. In fact, the sample includes 62% (236 out of 378) of municipalities that have experienced a local government dissolution due to confirmed mafia infiltration since 1991. This coverage ensures that the sample is not only representative of the Italian economy, but particularly representative of areas where organized crime poses a significant threat to public procurement.

Figure 1 displays annual white list enrollments across the sampled provinces. Starting from around 1,000 firms in 2014, registrations increased substantially during 2015-2017 and stabilized at

22. Chamber of Commerce information on headquarters locations and sectors was accessed through https://www.ufficiocamerale.it, a private search engine for Chamber of Commerce records.

approximately 2,000 firms annually. This enrollment pattern indicates a gradual policy implementation, with an initial learning period during which firms and contracting authorities familiarized themselves with the vetting process and requirements.

Figure 3b compares the sectoral composition of white-listed firms with that of all Italian firms. Construction accounts for more than 60% of vetted firms, confirming that the policy primarily targets sectors structurally exposed to infiltration risks. At the same time, the overall distribution broadly resembles the national sectoral composition, indicating that white list participation is not restricted to a narrow industrial niche.

In Table 1 I provide a broader characterization of white listed firms compared to the universe of Italian firms operating in the high-risk sectors.[23] To construct this comparison, I restrict the sample to firms operating in sectors broadly subject to white list requirements and with available balance sheet information that meet mandatory reporting requirements.[24] I classify firms as white-listed if they successfully obtained anti-mafia certification after 2013, while non-white list firms are those that never appeared on any white list during the sample period. Characteristics are presented at baseline (2009-2012). The first observation is that 8,828 of the white listed firms in the 10 years following the policy implementation were already operating in relevant sectors at the onset of the policy. Incumbents that eventually seek white list certification are systematically different from the general population of Italian firms across multiple dimensions. While similar in age, white-listed firms are larger, with significantly higher assets, revenues, and number of employees. They also exhibit higher debt levels and, as expected, are substantially more likely to participate in public procurement markets, with 25% participating compared to only 1% of non-white list firms. This means that about one-quarter of firms that eventually obtained white list certification were already active procurement participants when the policy was introduced. For these firms, procurement represents 8% of their revenues on average, compared with essentially zero in the general population. Conditional on procurement participation, white-listed firms secure contracts of similar size and exhibit comparable winning rates to other participants. This

23. The sample is restricted to firms operating in sectors broadly subject to white list requirements: mining and oil extraction (NAICS 21), manufacturing (NAICS 32-33), construction (NAICS 23), transportation and warehousing (NAICS 48), administrative and waste management services (NAICS 56), accommodation and food services (NAICS 72), other services (NAICS 81), and real estate (NAICS 53).

24. Out of the white list sample of 28,208 unique firms vetted by the Italian antimafia police, 15,522 firms are subject to mandatory reporting requirements and present viable financial information.

descriptive evidence suggests that white list certification attracts firms already established in public procurement rather than systematically selecting the highest-performing bidders.

In Table 2 I describe the procurement data. The dataset provides information for 94,819 unique public tenders issued between 2008 and 2022 across 1,815 Italian municipalities. Panel A provides summary statistics at the auction level for the whole sample of just under 95,000 unique tenders. The average contract value is 700,000 euros, though the median is substantially lower at 170,000 euros, reflecting the presence of large infrastructure projects. Winners offer an average discount of 17% relative to the reserve price, with a median discount of 15%. Fifty-eight percent of contracts involve subcontracting arrangements. Delivery delays, calculated as the ratio of actual delay days to the originally planned contract duration, are available for approximately 30% of tenders due to incomplete reporting of project completion dates. For contracts with available completion data, projects experience substantial delays, with completion times averaging 75% longer than the originally planned duration, indicating significant challenges in project execution and timeline adherence in Italian public works. The average number of bidders per tender is 14, but the median is only 3, indicating substantial variation in competition across tenders, with some attracting very large numbers of participants.

Panel B reports summary statistics at the municipality level for contracting authorities. We observe 1,815 municipalities across 27,225 municipality-year observations. The geographical distribution reflects the sample's focus on areas with higher organized crime presence: 64% of municipalities are in the South, 31% in the North, and only 5% in the Center. The average municipality awards 4 tenders per year, though the median is only 1, indicating that many small municipalities have infrequent procurement activity. Sixty percent of contracts are won by firms headquartered in the same province as the contracting authority, suggesting a strong local bias in procurement outcomes. At the municipality level, the average winner discount is 16% and delays average 78%, while 55% of municipal contracts involve subcontracting.

3.3. Sample Selection

My analysis employs two main datasets to inquire into the firm-level and market-level consequences of the transparency reform. The first dataset varies at the firm-year level and captures direct effects on individual firms' behavior and performance. The second dataset varies at the

municipality-year level and examines broader market dynamics and local economic outcomes. This subsection outlines the sample restrictions applied to construct each dataset.

Firm-Level Sample. The firm-level analysis focuses on incumbent firms that were active in public procurement markets at the onset of the policy. I define incumbents as firms that participated in at least one public works tender between 2008 and 2013. This criterion identifies 26,325 firms.[25] The sample is restricted to firms with complete financial data, specifically requiring non-missing values for total assets, operating revenues, debt and equity. The analysis focuses predominantly on construction firms, which represent the primary target of the white list policy and account for around 90% of the final sample.[26] The sample is further refined following the machine learning classification procedure explained in detail in section 4. After applying the machine learning classification to identify firms with strong procurement participation incentives, the final sample includes 3,487 incumbent firms. These firms are classified into treatment and control groups based on their suspected infiltration status. Table A1 demonstrates balance between treatment and control groups across observable characteristics in the pre-reform period.

Municipality-Level Sample. To examine the effects of transparency requirements on local procurement markets, I construct a second dataset that varies at the municipality-year level and comprises relevant information on procurement characteristics and outcomes. The dataset covers 1,815 municipalities across the 30 provinces that provided historical white list records, spanning the period 2008-2022 and yielding 27,225 municipality-year observations. The sample includes municipalities that issued at least one public tender during the observation period, with the final analytical sample restricted to municipalities where treatment intensity can be meaningfully measured.[27]

25. These 26,325 incumbent firms represent 10% of all firms with available balance sheet information in the dataset.

26. The sample also includes firms from other high-risk sectors (mining, manufacturing, transportation, waste management, accommodation and food services, and other services) that participated in public works tenders. These firms likely engage in construction-related activities as a secondary business line or provide specialized services to construction projects, explaining their participation in public works procurement despite their primary sectoral classification.

27. The final sample excludes 70 municipalities that issued tenders during the pre-reform period but lacked sufficient information to classify winning firms as clean or suspected of criminal infiltration, preventing the construction of treatment intensity measures (Section 4).

4 — Measure of Suspected Mafia Infiltration

In this section, I discuss the construction of a novel index of mafia infiltration at the firm level.

Existing measures of Mafia infiltration mostly rely on extreme events such as high-profile mafia arrests, and local violence (Transcrime 2014b), 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. 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 outweigh detection risk, that is, if $B_i > \overline{B} \equiv \frac{p}{1-p}P$. The key insight is that vetting choices reveal type only when economic incentives to participate are sufficiently strong. For $B_i < 0$, both types abstain, making choices uninformative. But in the intermediate region

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

clean firms vet while infiltrated firms strategically abstain. I refer to this interval as the *revelation zone*, illustrated in Figure 2. Only here do vetting decisions separate types: clean firms appear on police white lists, whereas infiltrated firms with comparable economic incentives remain absent.

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. The empirical histogram of predicted B_i combined with white list information thus mirrors the conceptual partition in Figure 2, separating clean from infiltrated firms in the revelation zone.

4.2. 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 XGBoost (extreme gradient boosting), selected after comparing performance across multiple algorithms including logistic regression, random forests, and neural networks. XGBoost demonstrates superior performance in terms of both predictive accuracy and interpretability, achieving the highest F1-score (0.838) and maintaining clear feature importance rankings that align with economic intuition.[28] The model is trained on more than 50 pre-policy firm characteristics from a sample of 6,585 incumbent firms with complete balance sheet data. The target variable exhibits a reasonably balanced distribution with 59.7% of firms participating in procurement in 2013 and 40.3% not participating, avoiding class imbalance issues that typically complicate machine learning classification tasks.

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 public 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.[29] The model's

28. Detailed model comparison and performance metrics are provided in Appendix B.

29. I follow closely the model from Ash, Galletta, and Giommoni 2025 who predict corruption in Brazilian munici-

overall discriminatory power, as measured by the area under the ROC curve, is close to 90%, indicating solid separation between likely and unlikely participants.[30] Comprehensive threshold analysis demonstrates that the 50% classification cutoff provides optimal performance, though results remain robust across alternative thresholds.[31] 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 B4 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 28.1% 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 4a 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 2, 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.

palities and achieve a precision of 0.72.

30. 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.

31. Detailed threshold robustness analysis is provided in Appendix B.

Step 3: White List Cross-Validation. I compare predicted participation probabilities to actual white lists. Figure 4b 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: Suspected Firms 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 4c highlights these firms in red. These firms map to the red line in Figure 2. 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 suspected firms (red line) from clean firms (blue line) within the high-benefit population (revelation zone) in Figure 2.

4.3. Suspected Firms Characteristics

Table 3 provides a comprehensive characterization of suspected criminal firms compared to the universe of Italian firms operating in high-risk sectors subject to white list requirements. To construct this comparison, I restrict the sample to firms with available balance sheet information that meet mandatory reporting requirements, examining characteristics during the baseline period (2009-2012). Suspected criminal firms represent 1,251 companies.

The descriptive evidence reveals that suspected criminal firms differ systematically from the general population across multiple dimensions. These firms are slightly older,substantially larger across all financial metrics: assets, revenues, debt levels, and employment are all significantly higher than the population average. As expected, suspected firms exhibit much higher engagement with public procurement markets. While only 3% of firms in the general population participate in public tenders, 67% of suspected firms are active procurement participants. For these firms, procurement represents 22% of total revenues on average—compared to essentially zero in the

general population—indicating their heavy dependence on public sector contracts.

Conditional on procurement participation, suspected firms exhibit slightly lower winning rates (26% versus 30%) but secure contracts of comparable size to other participants. Despite their lower profitability margins (8% versus 11.7% profit-to-sales ratio), their substantial scale and procurement focus suggest these firms occupy strategically important positions within local public procurement markets. This descriptive evidence indicates that suspected criminal firms are established, large-scale operators with significant economic footprints in public procurement markets.

4.3.1. Comparison with literature metrics

In this section I test my suspected firm measure against two benchmarks: municipality-level crime indicators derived from detected mafia activity and firm-specific investigations reported in online news outlets.

Geographic Validation I validate my suspected firm classification 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. Across the three measures, suspected firms secured a median ranging from 8 to 10% of public contracts in high-crime municipalities during the pre-reform period, while winning no contracts in municipalities without such indicators. 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. 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.

Firm-Level Validation To complement the geographic validation, I implement a systematic AI-assisted web search algorithm to identify criminal investigations involving individual firms in local news sources. The methodology combines comprehensive Google-API searches with LLM for automated content analysis and manual verification of results. For each firm, the algorithm conducts multiple targeted searches using combinations of firm name, province, municipality, and crime-related keywords.[32] The search process involves approximately 15,000 firm-municipality queries, followed by automated analysis of roughly 500 retrieved articles to identify explicit mentions of criminal investigations, with manual verification of flagged cases to ensure accuracy. This systematic approach reveals that 9% of suspected firms have documented criminal investigations in local news sources.[33]

5 — Empirical Strategy

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. I discuss several identifying assumptions and conclude with a wide set of robustness tests.

5.1. 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

32. The keywords used are *mafia*, *indagine*, *interdittiva*, *sequestro antimafia*, *criminalità organizzata*, and *riciclaggio*, with news articles retrieved only when at least one keyword appears alongside the firm name and municipality of incorporation within the same article text. All pulled articles are inspected combining manual and LLM-assisted review.

33. The same analysis found 5% of mentions for vetted incumbents. Manual inspection of the vetted firms' news articles revealed that 32% involve firms under court-appointed administration (which are eligible for white list registration by judicial decree) and notably, a significant portion (16%) involve firms that were victims of intimidation campaigns. A smaller set of articles refers to dismissed charges (10%).

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 2.3, 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. 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.

5.2. 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:

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

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.

6 — Results

In this section, I present the empirical results. First, I show that the introduction of the 2013 Antimafia Information Law amendment had an immediate impact on suspected firms ability to secure public contracts and triggered broader organizational decline. I then address identification concerns through extensive robustness analyses, including placebo tests across multiple years and fictional treatment designs. Finally, I examine how local procurement markets restructured when suspected firms were excluded, analyzing changes in competition, geographical concentration, and firm entry patterns. I conclude by discussing long-run implications for the local economic activity.

6.1. Firm-Level Consequences of Transparency Requirements

6.1.1. Procurement Participation and Enforcement

I start by assessing the effects of transparency requirements on suspected firms ability to bid and secure public contracts. Figures 7a and 7b focus on participation and winning probability as outcomes and report β_k estimates from Equation 1. The results demonstrate that the transparency

requirements successfully drove suspected firms out of procurement markets across both bidding and winning margins. The probability of bidding in tenders drops by 34% in the post-policy period for suspected firms relative to the vetted baseline. The event study specification reveals that this effect builds gradually, with the reduction reaching 54% by 2016. A similar pattern characterizes the probability of winning contracts, which reduces by 22% on average and reaches a 42% reduction through the years.

The progressive intensification of enforcement reflects two key factors. First, once white lists are in place, contracting authorities as well as firms have to learn how to navigate the new requirements. Extensive discussions with local police officers suggested that there was a learning period, both from the contracting authorities' side (who should actively consult the white lists) and the firms' side.[34] Second, the number of tenders participated and contract secured by vetted incumbents (the control group), does not stay fully constant over time. As detailed in Section 6.1.5, local procurement markets experience increased entry, and the share of tenders won by incumbents reduces somewhat over time as new competitors enter.

6.1.2. Organizational Decline Among Suspected Firms

The first set of results shows significant deterrent effects of the transparency requirements. I now ask what are the broader implications for suspected firms outside of the public procurement space. Figure 8 examines how the policy affects firm size. The coefficients show that suspecte firms shrink substantially in the post reform period: number of employees and assets reduce by 30 and 40% respectively. Also in this case effects build over time with the most severe consequences manifesting 5 years into the policy. Beyond organizational contraction, suspected firms experience severe performance deterioration.[35] Finally, I examine whether suspected firms exit the market completely. Exit is measured as a binary indicator that equals one when year t represents the final year firm i filed a balance sheet. Figure 9 shows that suspected firms face a 12 percentage point higher probability of complete exit. The scale of this effect is remarkable. By the sixth year of policy implementation, nearly half of suspected firms have ceased operations entirely.

34. This was particularly true for firms operating across sectoral boundaries, where classification as high-risk sectors subject to vetting requirements was not immediately clear.

35. Complementary results in Figure A2, confirm the organizational decline. Return on assets drops by more than 2 percentage points in the post-policy period. Total revenues decline dramatically following a similar pattern.

6.1.3. Strategic Adaptation by Surviving Firms

Having established widespread organizational decline, I examine whether surviving suspected firms implement strategic adaptations along two dimensions: revenue diversification and regulatory evasion.

Revenue Diversification. I first investigate whether suspected firms pivot away from public procurement toward alternative revenue sources. To measure this, I combine firm-level revenue data from income statements with procurement contract values, allowing me to calculate the portion of revenues not derived from public contracts.[36]

My findings support strategic adaptation through two complementary patterns. First, the share of revenues from non-procurement sources increases by 17% after the policy (Figure 10a). Second, I examine absolute levels of non-procurement revenues to assess whether firms actively develop new business lines (Figure 10b). These revenues remain stable during the initial years following policy implementation but begin increasing substantially among firms that survive six years into the reform. This temporal pattern indicates that while suspected firms experience overall revenue decline, those that survive eventually restructure their business models by developing alternative revenue streams beyond public procurement.

Regulatory Evasion. Suspected firms also adapt within the procurement market itself. Despite the strong enforcement effects documented above, a subset of suspected firms continue to win a limited number of contracts in the post-reform period, even while absent from white lists. Prior research shows that firms strategically bunch below regulatory thresholds to avoid transparency requirements (Daniele and Dipoppa 2023). In public procurement, contract size is a key threshold: larger tenders require additional third-party verification and full compliance checks, whereas smaller tenders face lighter scrutiny and fewer administrative controls.[37] This regulatory structure potentially creates incentives for suspected firms to concentrate their bidding efforts on smaller,

36. Multi-year procurement contracts complicate direct comparison between annual revenues and contract awards. For contracts with available duration data, I allocate contract value across years starting from the award year to approximate annual payments. I then subtract this from total revenues to estimate non-procurement income. For contracts without duration data, I assign the full value to the award year.

37. Procurement systems worldwide feature different regulatory requirements based on contract size. In the United States, the Federal Acquisition Regulation allows procurements below USD 250,000 to use simplified procedures with less documentation, while larger contracts require full oversight.

less monitored tenders where enforcement effort is lower. This regulatory structure potentially creates incentives for suspected firms to concentrate their bidding efforts on smaller, less monitored tenders where enforcement effort is lower.

To examine this channel, I analyze how the value of contracts won by suspected firms changes over time. Figure 10c shows that, conditional on winning, suspected firms secure contracts that are roughly 30% smaller relative to the pre-reform difference with vetted incumbents. This pattern is consistent with strategic adaptation: surviving firms remain active, but only by targeting contract sizes where enforcement intensity is lowest.[38]

6.1.4. Magnitude of the Effects and Robustness Analysis

Firm turnover is a regular feature of procurement markets. Therefore, it is important to benchmark the results documented above against typical procurement market dynamics. In particular, I examine permanent exit rates among high-probability firms across different years around the reform. First, I apply the machine learning model to each year from 2011-2015 to identify firms with high predicted participation probability.[39] Second, for each year between 2011 and 2015, I define permanent exit as the share of high-probability firms in year t that never bid again from year t through 2016 nor win contracts through 2022. Third, I track how the permanent exit rates change across the period 2011-2015.

Results are reported in Figure 6. During the pre-policy years 2011-2013, permanent exit rates remained stable between 3-5%, reflecting normal market dynamics. However, the policy year exhibits a sharp break in this pattern. Exit rates jump to 10% in 2014 and remain elevated at 12% in 2015. This represents a two- to four-fold increase relative to baseline levels, demonstrating that the transparency requirements generated exit patterns far exceeding normal market volatility and confirming that the observed firm-level consequences reflect genuine policy impacts rather than coincidental economic trends.

To further validate these findings, I conduct comprehensive robustness and placebo tests that

38. In Figure A3, I also specifically inquire the threshold of 150,000 Euros, used by Daniele and Dipoppa 2023 and find similar results.

39. Because procurement participation information is only available until 2016, I can only apply the model to years 2011-2015. Additionally, I cannot use the full original model as it requires 4 lags, which would exclude years 2011 and 2012. Therefore, I calibrate a 2-lag model that allows me to produce estimates for all years 2011-2015. The two models exhibit similar performance as shown by Figure B1.

rule out alternative explanations for the results presented in this section. First, I address the concern that results capture firms that were exiting anyway due to unrelated economic shocks by restricting my sample to firms that survive beyond 2018. This specification isolates changes in performance among continuing firms and estimates yield the same results, confirming that effects reflect genuine policy impacts rather than differential exit patterns. Second, I test whether absence from white lists represents firms' private information about future decline rather than criminal connections. I implement placebo tests using firms with low predicted participation probabilities and show that absence from white lists itself is not sufficient to explain the results; it is particularly the combination between strong ex ante economic incentives and systematic avoidance of white listing that produces my results. Third, I address the possibility that white lists systematically captures "procurement winners" even within one narrow predicted probability bin in equation 1. To assess this possibility I create a fictional treatment that assigns treatment status based on contract outcomes within prediction buckets, comparing firms with identical predicted participation probabilities that differ only in whether they won contracts in specific years. Under the survivor bias hypothesis, any mechanism distinguishing winners from losers should produce similar effects, but the fictional treatments yield muted impacts compared to the actual policy. The consistency of results across robustness specifications and null findings in placebo tests provide strong evidence that the estimated effects reflect genuine policy impacts (detailed results in Section C).

6.1.5. Market Composition and Competition

The transparency requirements fundamentally restructured local procurement markets by altering both the composition of winning firms and competitive dynamics. Table 4 reports estimates for the β coefficient in equation 2, examining municipality-level outcomes in the post-reform period.[40]

Winner Composition. Columns (1)-(3) document a substantial reallocation of market share away from suspected firms toward new market entrants. Suspected firms' share of tenders won decreases by 11.9 percentage points in treated municipalities (column 1). Given the pre-reform

40. Following the corner solution literature, I 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).

baseline of 6.7%, this eliminates most of their market presence. This reduction does not primarily benefit vetted incumbents, whose market share shows no statistically significant change (column 2). Instead, the displaced market share flows to entrants—firms that never participated in public tenders in a given municipality during the pre-period. The share of tenders won by entrants increases by 2.5 percentage points (column 3), representing entirely new competitive entry since their pre-treatment share is mechanically zero.

Competition. Column (4) demonstrates that the reform reduced geographical concentration in procurement markets. The probability that the winning firm is incorporated in the same province as the contracting municipality decreases by 4.9 percentage points. Given the pre-reform baseline where 59.2% of tenders were won by same-province firms, this reduction suggests that procurement became more open to outside competition.

The market restructuring also intensified competitive participation. Column (9) shows that the number of bidders per tender increases by 10.2%, which translates to approximately 4 additional bidders competing for each contract. This substantial increase demonstrates that the removal of suspected networks attracted new participants rather than discouraging overall market engagement.

Service Quality and Efficiency. Despite the significant market disruption, procurement outcomes remained stable along key quality dimensions. The reform produces no statistically significant changes in winning discounts (column 5) or project delays (column 6), indicating that increased competition and network disruption did not compromise service delivery or project completion efficiency.

6.1.6. Subcontracting and Regulatory Evasion

Market restructuring creates opportunities for displaced firms to attempt re-entry through indirect channels. Subcontracting represents a particularly relevant pathway for regulatory circumvention, as these arrangements are inherently less transparent and harder to monitor than direct procurement relationships (Bosio et al. 2022). Moreover, the initial 2013 anti-mafia legislation did not regulate subcontracting activities, creating a potential loophole that was only addressed

a year later through Article 29 of Law n. 114/2014.[41] This regulatory gap makes it essential to examine whether suspected firms exploited subcontracting as an evasion mechanism.

Column (7) shows that subcontracting increases by 2.6 percentage points in affected municipalities, representing a 5.0% increase relative to the pre-reform baseline of 52%. This rise could reflect two distinct mechanisms. First, it might represent regulatory evasion, where suspected firms collaborate with winning bidders to maintain market access through indirect roles. Second, it could reflect the natural adjustment process as new entrants and geographically diverse winners rely on local subcontractors to provide specialized knowledge and capacity for project execution.

To distinguish between these explanations, I examine the identity of subcontractors using a unique dataset that identifies subcontractor firms for approximately 30% of subcontracted tenders. This analysis addresses a critical data limitation, as subcontractor identities are typically unavailable in procurement databases, making this channel of potential evasion difficult to study systematically.

Column (8) provides direct evidence against the regulatory evasion hypothesis. The share of subcontracted work going to suspected firms actually decreases by 2.6 percentage points. Given the pre-reform baseline where 6.5% of subcontracted work involved suspected firms, this represents a substantial reduction in their indirect market access. This finding indicates that the reform effectively limited suspected firms' ability to regain market participation through subcontracting arrangements, despite the initial regulatory gap.

The combination of increased subcontracting activity alongside reduced participation by suspected subcontractors suggests that the observed rise in subcontracting reflects legitimate market adjustments rather than systematic evasion. New market entrants appear to rely more heavily on subcontracting to access local expertise and capacity, but they systematically avoid partnerships with suspected firms, indicating that transparency requirements successfully extended beyond direct procurement relationships to influence indirect market participation as well.

41. Article 29 of Law n. 114/2014 extended anti-mafia vetting requirements to subcontracting arrangements. See https://www.bosettiegatti.eu/info/norme/statali/2014_0114.htm.

6.1.7. Local Economic Impact

The exclusion of suspected firms from procurement markets, and often from the market entirely, raises key questions about the long-run consequences for local employment and economic activity. Prior research highlights the economic complexity of removing criminal networks: Le Moglie and Sorrenti 2022 find that mafia presence actually shielded employment during the 2008 financial crisis, while Szerman 2023 quantifies substantial negative employment spillovers from corporate debarment programs.

I examine long-run effects on local economic outcomes using event study specifications that separately analyze municipalities in the third and fourth quartiles of pre-reform exposure intensity. I use a quartile regression approach to capture any potential non-linear responses where the most affected areas might experience different adjustment patterns than moderately affected municipalities. This specification is particularly important for detecting threshold effects, where economic disruption might only become visible above certain levels of criminal firm presence. Figure 11 provides a comprehensive dynamic analysis of these effects.

Panel (a) examines total employment in the construction sector and shows that employment remains stable in both Q3 and Q4 municipalities throughout the observation period. There is no evidence of job losses following the reform, indicating that the displacement of suspected firms did not create widespread unemployment in the construction industry. Panel (b) reveals the adjustment process in the number of construction firms. Total firm counts initially remain constant in both quartiles but grow substantially in the most affected municipalities (Q4) starting three years after the reform. This delayed response suggests an initial period of market adjustment followed by sustained growth in business formation.

Panel (c) demonstrates that this growth is driven by entrepreneurial entry rather than firm survival. The number of newly incorporated construction firms increases immediately after the reform in Q4 municipalities and remains elevated throughout the observation period. This immediate response indicates that market opportunities created by suspected firm exit quickly attracted new business formation. Panel (d) shows that per capita income remains unaffected by the reform, even in the most heavily treated areas. This stability indicates that any economic disruption from removing established contractors was offset by legitimate business activity,

preventing adverse welfare effects for local residents.

The dynamic patterns reveal that transparency requirements achieved a beneficial market transformation rather than economic disruption. The most exposed municipalities experience sustained increases in business formation, with new construction firms replacing suspected networks and ultimately expanding total business activity. The absence of employment or income losses, combined with increased entrepreneurship, indicates that removing suspected firms created space for businesses to thrive rather than destroying essential economic capacity.

7 — 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, 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 largely positive outcomes. The exit of these actors created space for expanded competition: contract awards shifted primarily to firms that had not previously participated 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 heavily affected areas, employment and income remained stable, driven by increased entrepreneurship in affected sectors. New firm creation rose immediately, initially offsetting exits, then generating net growth with the

total stock of firms increasing substantially over time.

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.[42] The methodology developed here—combining predictive modeling with 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.

42. 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).

8 — Tables

Table 1: **Characteristics of White Listed firms**

Variable	White List Firms (1)	Non-White List Firms (2)	Difference (1) - (2) (3)
Year of incorporation	1999.22	2000.35	-1.4**
Assets (log)	13.66	12.47	1.28***
Revenues (log)	13.00	9.73	3.22***
Debt (log)	7.67	5.48	2.44***
Profits/Sales	9.29	11.94	-0.17
No. employees (log)	2.35	1.89	0.54***
Procurement as a share of revenues	0.08	0.00	0.08***
Probability of participating	0.25	0.01	0.25***
Conditional on procurement participation			
Winning rate	0.33	0.25	0.06**
Contract amount (log)	13.27	13.36	0.05
N. of firms	8,828	144,329	

Notes: This table presents pre-treatment (2009-2012) characteristics of firms that eventually obtained white list certification versus firms that never appeared on any white list. The sample is restricted to firms operating in sectors broadly subject to white list requirements: mining and oil extraction (NAICS 21), manufacturing (NAICS 32-33), construction (NAICS 23), transportation and warehousing (NAICS 48), administrative and waste management services (NAICS 56), accommodation and food services (NAICS 72), other services (NAICS 81), and real estate (NAICS 53). The sample includes firms with balance sheet information that meet reporting requirements. White list firms are those that obtained anti-mafia certification at any point in time between 2014 and 2023, while non-white list firms never appeared on any white list. Columns (1) and (2) report mean values for each group. Column (3) presents the coefficient from univariate regressions of each characteristic on a white list firm dummy, controlling for province fixed effects, municipality fixed effects, and 2-digit NAICS industry fixed effects. The regression specification is: $Y_i = \alpha + \beta \cdot \text{WhiteList}_i + \delta_p + \mu_m + \theta_s + \epsilon_i$, where Y_i is the firm characteristic, δ_p are province fixed effects, μ_m are municipality fixed effects, and θ_s are industry fixed effects. Financial variables (assets, revenues, debt, employees) are expressed in logarithms. Procurement as a share of revenues is winsorized at the 95th percentile to address outliers. Variables in the "Conditional on procurement participation" section are calculated only for firms that participated in at least one procurement tender during the pre-treatment period. Standard errors are clustered at the firm level. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 2: **Summary Statistics for Procurement Data**

	Mean (1)	Median (2)	SD (3)	N (4)
A. Auction Level				
Contract value (M EUR)	0.70	0.17	10.05	94,971
Winner discount (%)	16.94	15.46	14.04	94,602
Subcontracting	0.58	1.00	0.49	94,994
Delay (%)	0.75	0.39	2.77	28,871
No. bidders	13.66	3.00	35.72	94,994
B. Municipality Level				
No. tenders	3.83	1.00	18.35	27,225
Same province	0.60	0.67	0.38	18,359
Winner discount (%)	0.16	0.15	0.11	18,302
Delay (%)	0.78	0.51	1.31	9,903
Subcontracting	0.55	0.57	0.40	18,359
Total municipalities				1,815
By area:				
North				558
Center				96
South				1,161

Notes: This table presents summary statistics for the procurement dataset spanning 2008-2022. Panel A reports statistics at the auction level for 94,819 unique public tenders from the 30 provinces with available white list data. Panel B presents statistics at the municipality level, covering 1,815 municipalities across 27,225 municipality-year observations. The sample is restricted to public works contracts above 40,000 euros in sectors broadly subject to white list requirements: mining and oil extraction (NAICS 21), manufacturing (NAICS 32-33), construction (NAICS 23), transportation and warehousing (NAICS 48), administrative and waste management services (NAICS 56), accommodation and food services (NAICS 72), other services (NAICS 81), and real estate (NAICS 53). Contract values are expressed in millions of euros. Winner discount represents the percentage discount offered by the winning bidder relative to the reserve price. Delay measures the percentage difference between actual and expected contract duration. Subcontracting indicates the share of tenders involving subcontracted work. Same province indicates the share of contracts won by firms headquartered in the same province as the contracting authority. No. bidders represents the average number of firms participating in each tender. The geographical distribution shows municipalities concentrated in the South (64%), followed by the North (31%) and Center (5%). Municipality-level statistics are calculated as averages across municipality-year observations, with varying sample sizes due to municipalities having no tenders in certain years.

Table 3: **Characteristics of Suspected Criminal Firms vs. Full Sample**

Variable	Suspected Criminal Firms (1)	Full Sample Firms (2)	Difference (1) - (2) (3)
Year of incorporation	1998.38	2000.27	-2.55***
Assets (log)	13.92	12.54	1.44***
Revenues (log)	13.32	9.92	3.81***
Debt (log)	8.19	5.62	2.97***
Profits/Sales	8.00	11.67	-2.94***
No. employees (log)	2.30	1.94	0.56***
Procurement as a share of revenues	0.22	0.01	0.2***
Probability of participating	0.67	0.03	0.6***
Conditional on procurement participation			
Winning rate	0.26	0.30	-0.03**
Contract amount (log)	13.38	13.19	0.12
N. of firms	1,251	156,609	

Notes: This table presents pre-treatment (2009-2012) characteristics of firms identified as suspected criminal organizations versus the full sample of firms. Suspected criminal firms are those identified through machine learning algorithms as having high probability of organized crime connections but systematically absent from anti-mafia white lists. The sample is restricted to firms operating in sectors broadly subject to white list requirements: mining and oil extraction (NAICS 21), manufacturing (NAICS 32-33), construction (NAICS 23), transportation and warehousing (NAICS 48), administrative and waste management services (NAICS 56), accommodation and food services (NAICS 72), other services (NAICS 81), and real estate (NAICS 53). The sample includes firms with balance sheet information that meet reporting requirements. Columns (1) and (2) report mean values for each group. Column (3) presents the coefficient from univariate regressions of each characteristic on a suspected criminal firm dummy, controlling for province fixed effects, municipality fixed effects, and 2-digit NAICS industry fixed effects. The regression specification is: $Y_i = \alpha + \beta \cdot \text{SuspectedCriminal}_i + \delta_p + \mu_m + \theta_s + \epsilon_i$, where Y_i is the firm characteristic, δ_p are province fixed effects, μ_m are municipality fixed effects, and θ_s are industry fixed effects. Financial variables (assets, revenues, debt, employees) are expressed in logarithms. Procurement as a share of revenues is winsorized at the 95th percentile to address outliers. Variables in the "Conditional on procurement participation" section are calculated only for firms that participated in at least one procurement tender during the pre-treatment period. Standard errors are clustered at the firm level. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: **Effect of White List Provision on Local Procurement Markets**

	Share tenders won by								
	Suspected	Vetted incumbents	Entrants	Same prov.	Win. discount	Delay	Subcontracting	Suspected subcontr.	Bids per tender
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Infiltration × Post	-0.119***	0.014	0.025***	-0.049***	-0.005	-0.084	0.026*	-0.026*	0.102**
	(0.004)	(0.009)	(0.009)	(0.014)	(0.004)	(0.064)	(0.015)	(0.015)	(0.046)
R^2	0.17659	0.42090	0.51670	0.32135	0.44176	0.26116	0.32355	0.35699	0.54379
Observations	27,225	27,225	27,225	18,359	18,336	9,903	18,359	5,416	17,122
E[Y]	0.067	0.409	0.000	0.592	0.184	0.802	0.523	0.065	2.758
E[Y] non-transformed									39.529
Infiltration share mean	0.501								
N. of Municipalities	1,815								
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 suspected 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 suspected subcontractors. Column (9) reports number of bids per tender (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.

9 — Figures

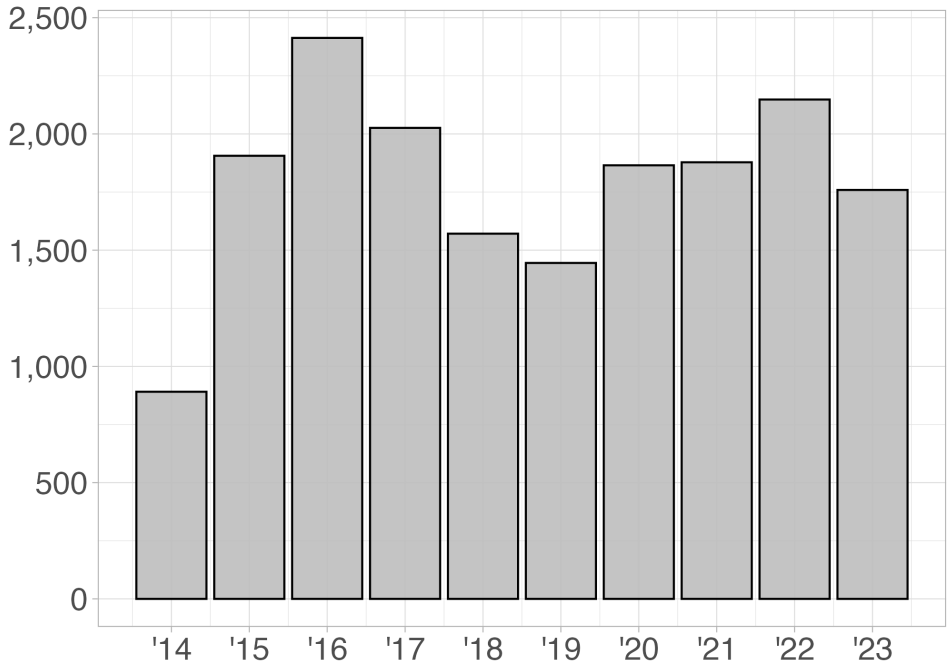
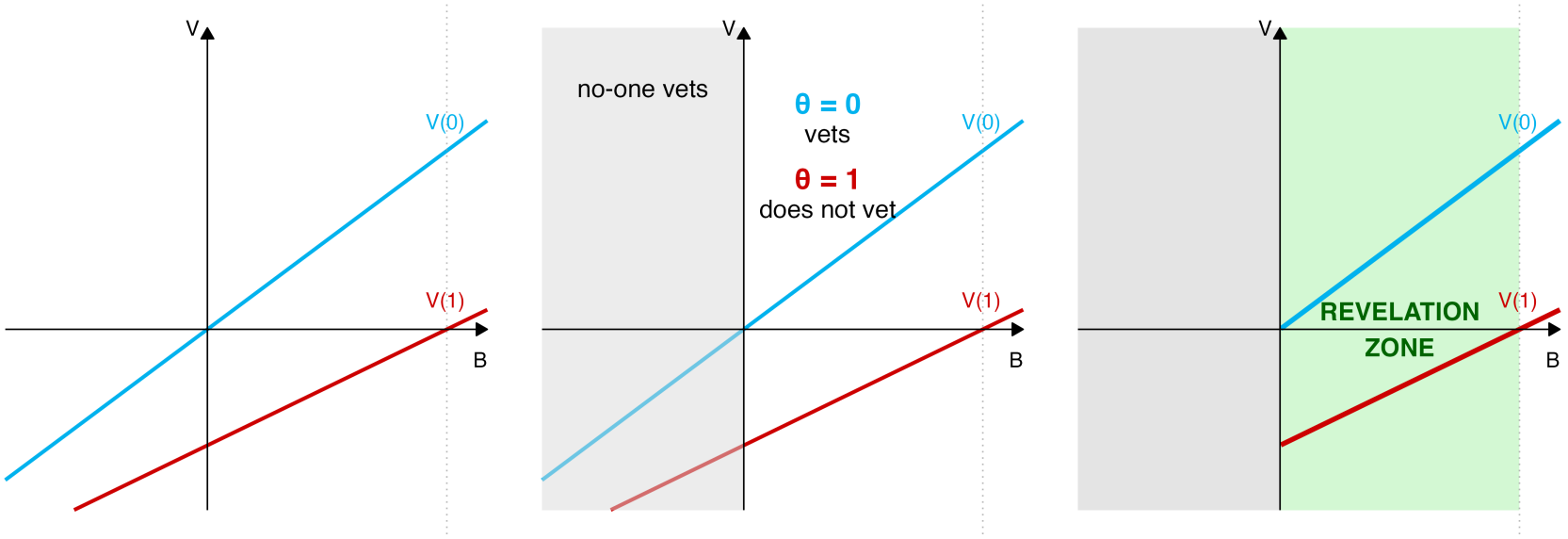


Figure 1 — White List Enrollment Over Time

Notes. This figure shows the annual number of firms entering white lists from 2014 to 2023. The data contains 64% of all firms that ever entered the white list system. The exact date of when each firm entered the white list for the first time was not reported by all prefectural offices.

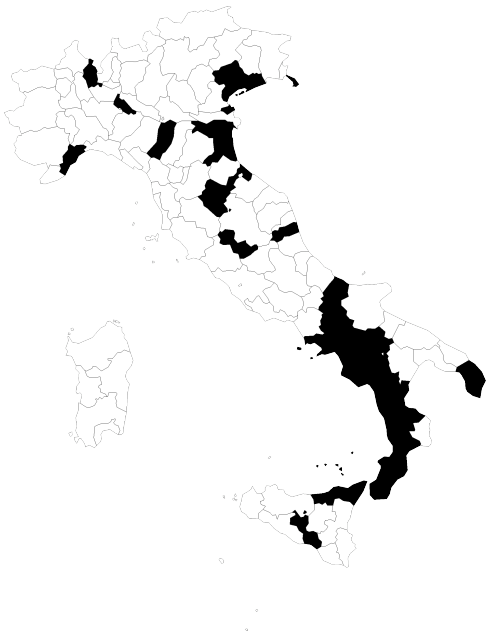


(a) Utility functions by firm type

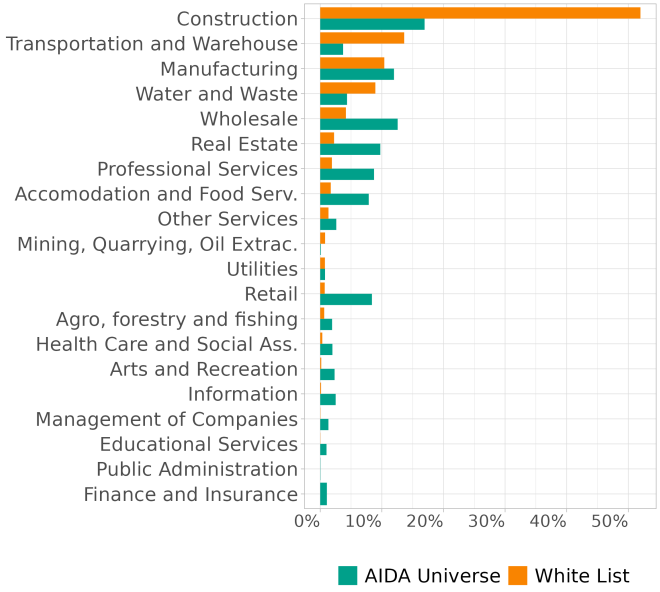
(b) Vetting zones and detection risk

(c) The revelation zone

Figure 2 — 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.

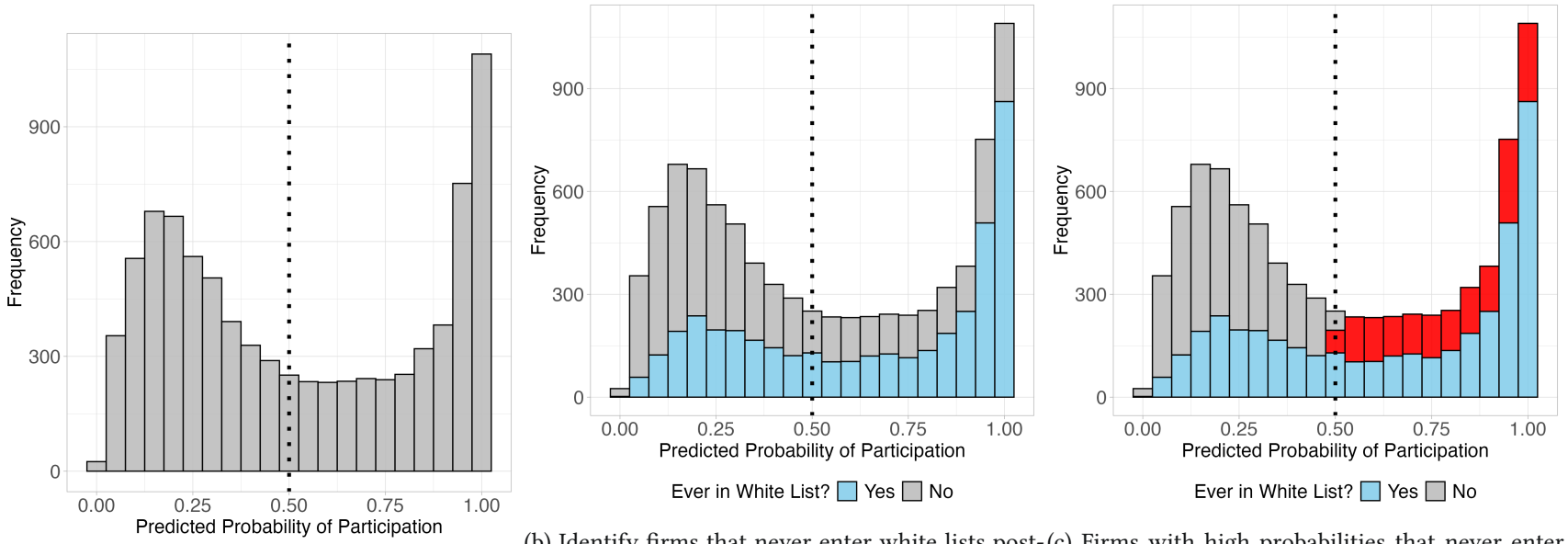


(a) Provincial police offices that provided data



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

Figure 3 — **White List Sample**

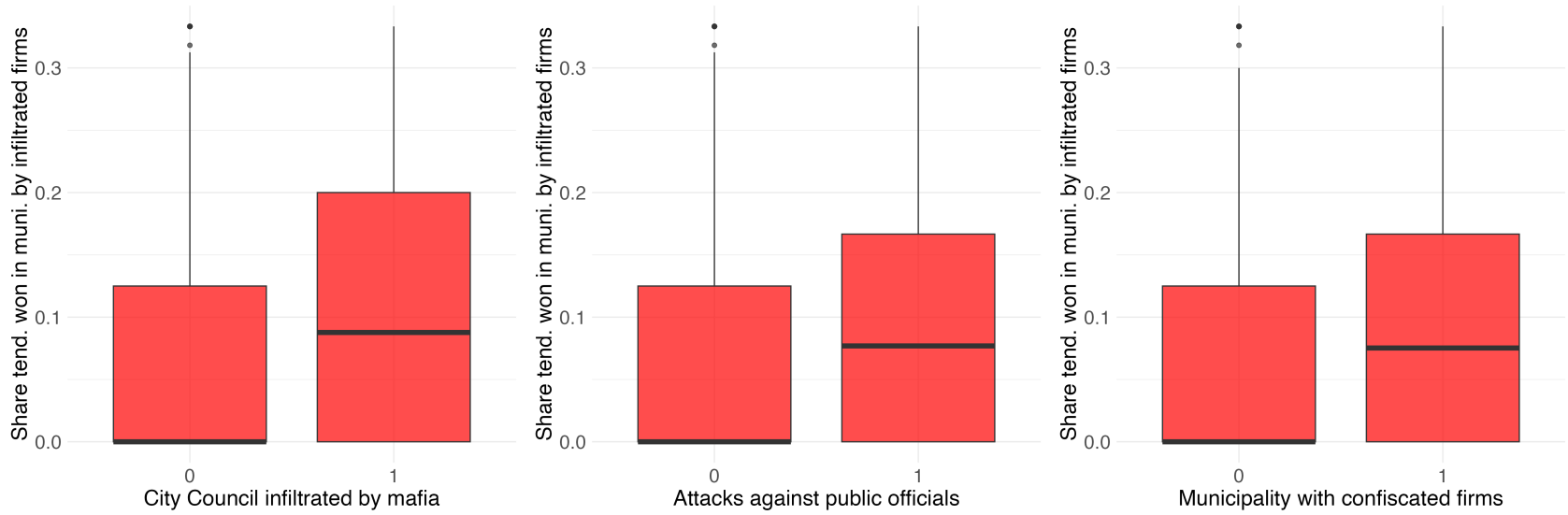


(a) Predict firm-level procurement participation

(b) Identify firms that never enter white lists post-2013

(c) Firms with high probabilities that never enter white lists

Figure 4 — **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 5 — 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.

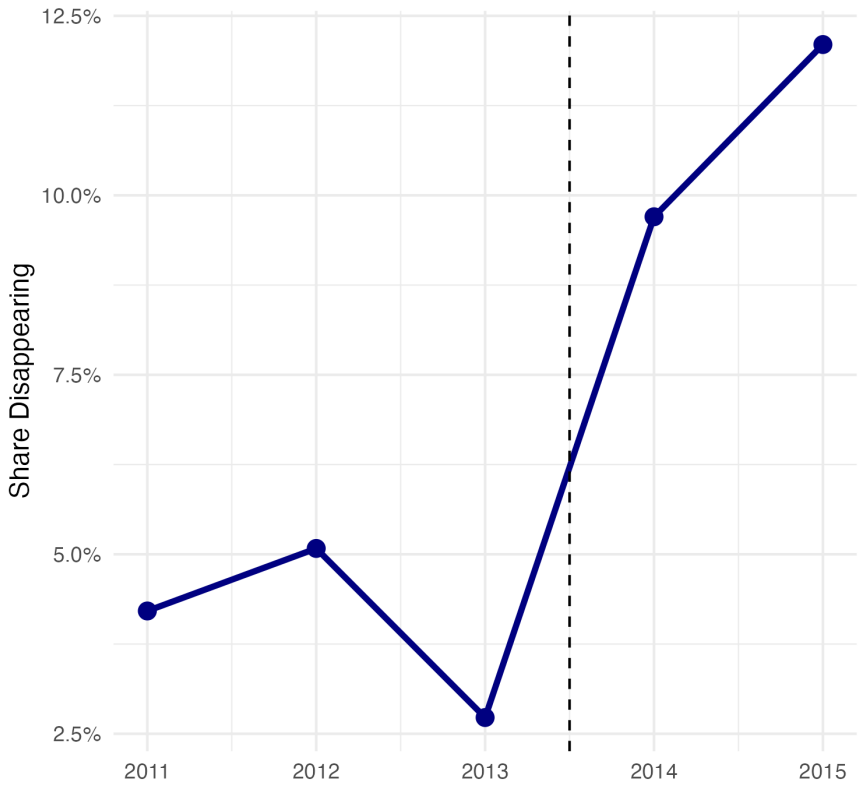
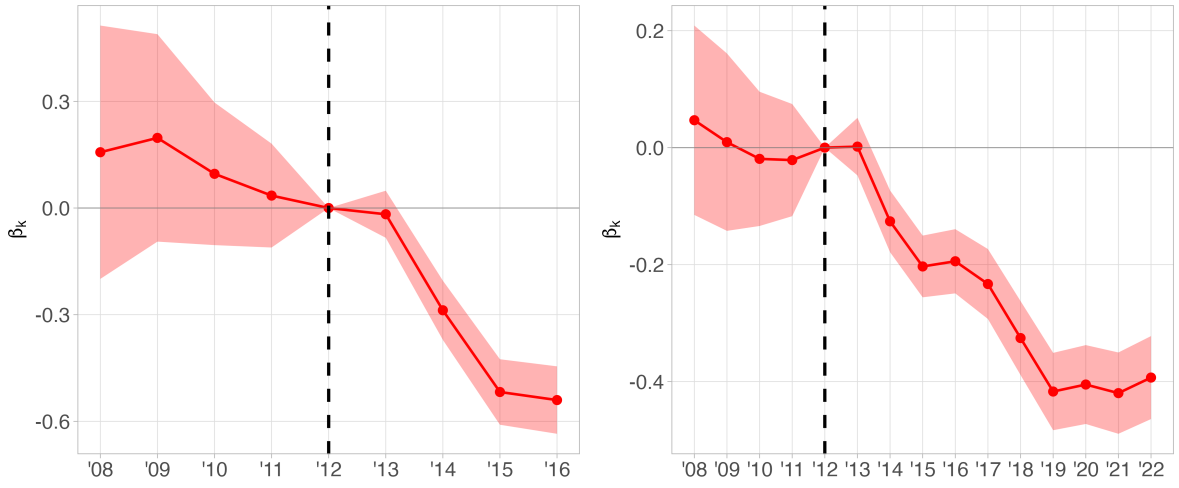


Figure 6 — **Share of High-Probability Incumbents That Disappear by Year**. This figure displays the annual permanent exit rate for firms with high predicted participation probability ($\geq$ 50%) in Italian public procurement markets from 2011-2015. For each year t, I apply a 2-lag XGBoost model trained on firm balance sheet variables and procurement history to identify firms with predicted participation probability exceeding 50%. The exit rate is computed as the percentage of these high-probability firms that permanently disappear from procurement (never bid again from year t through 2016) and never win contracts through 2022. The vertical dashed line marks 2013 when the anti-mafia transparency reform was announced, with white lists becoming operational in 2014. The sharp increase from 3% in 2013 to 10% in 2014 demonstrates abnormal market dynamics precisely when the policy took effect.

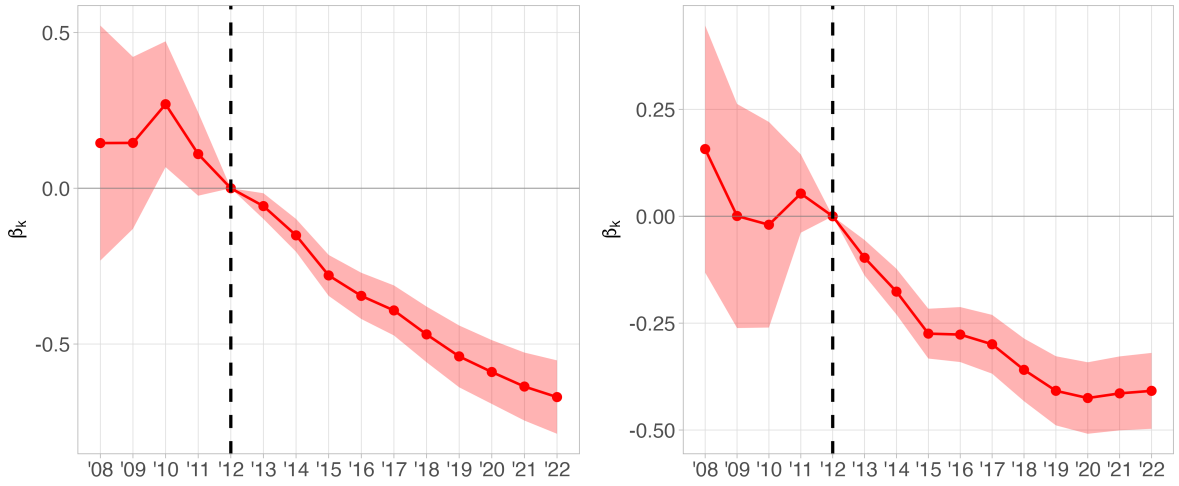


(a) No. Tenders Won (Log)

(b) Amount Won (Log)

Figure 7 — **Suspected Firms Activity in Public Procurement**

Notes. Observations are at the firm-year level. Both panels present the estimated coefficients β_k from regression $Y_{i,t} = \delta_{\text{pbt}} + \gamma_{\text{m}} + \alpha_{\text{i}} + \sum_{\substack{k=08 \\ k \neq 12}}^{22} \beta_k \cdot \text{Suspected}_i \cdot \mathbf{1}\{\text{t=k}\}_k + \epsilon_{i,t}$ along with 95% confidence intervals. In panel 7a, the dependent variable is the number of bids posted by firm i in year t. In panel 7b, the dependent variable is the total number of contracts won by 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) Total Assets (Log)

(b) N. of Employees (Log)

Figure 8 — **Suspected Firms Size**

Notes. Observations are at the firm-year level. Panels present the estimated coefficients β_k from regression $Y_{i,t} = \delta_{\text{pbt}} + \gamma_{\text{m}} + \alpha_{\text{i}} + \sum_{\substack{k=08 \\ k \neq 12}}^{22} \beta_k \cdot \text{Suspected}_i \cdot \mathbf{1}\{\text{t=k}\}_k + \epsilon_{i,t}$ along with 95% confidence intervals. Panels 8a, 8b 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.

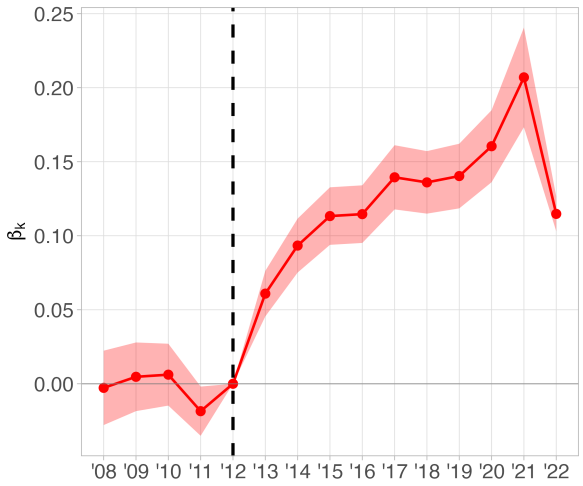
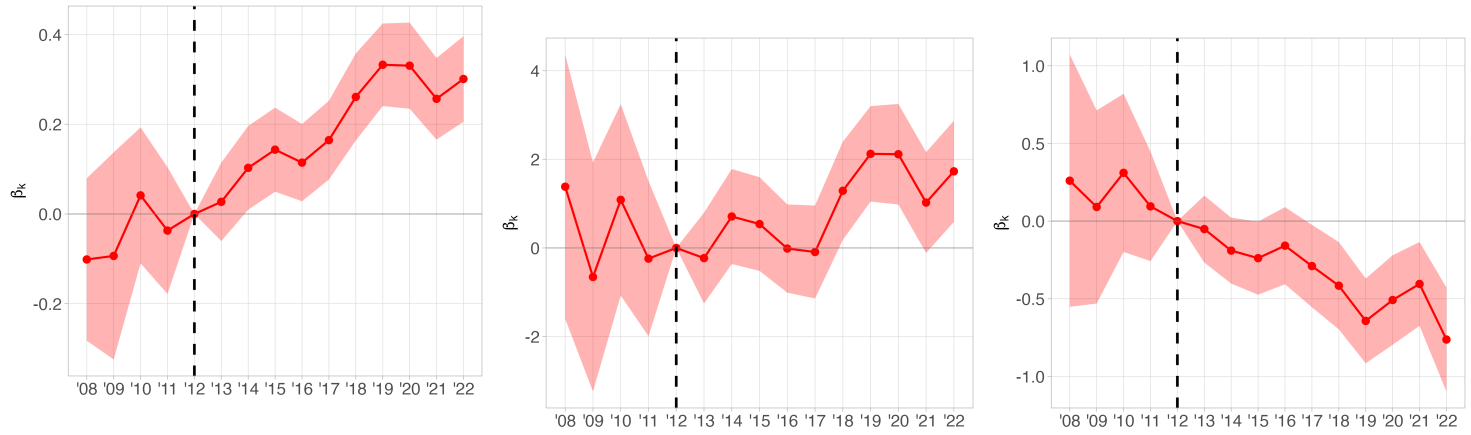


Figure 9 — **Suspected Firms Exit Probability**

Notes. Observations are at the firm-year level. Panels present the estimated coefficients β_k from regression $Y_{i,t} = \delta_{\text{pbt}} + \gamma_{\text{m}} + \alpha_{\text{i}} + \sum_{\substack{k=08 \\ k \neq 12}}^{22} \beta_k \cdot \text{Suspected}_i \cdot \mathbf{1}\{\text{t=k}\}_k + \epsilon_{i,t}$
along with 95% confidence intervals. The dependent variable is a dummy taking value 1 if year t is the last year firm i reports a viable balance sheet. 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.



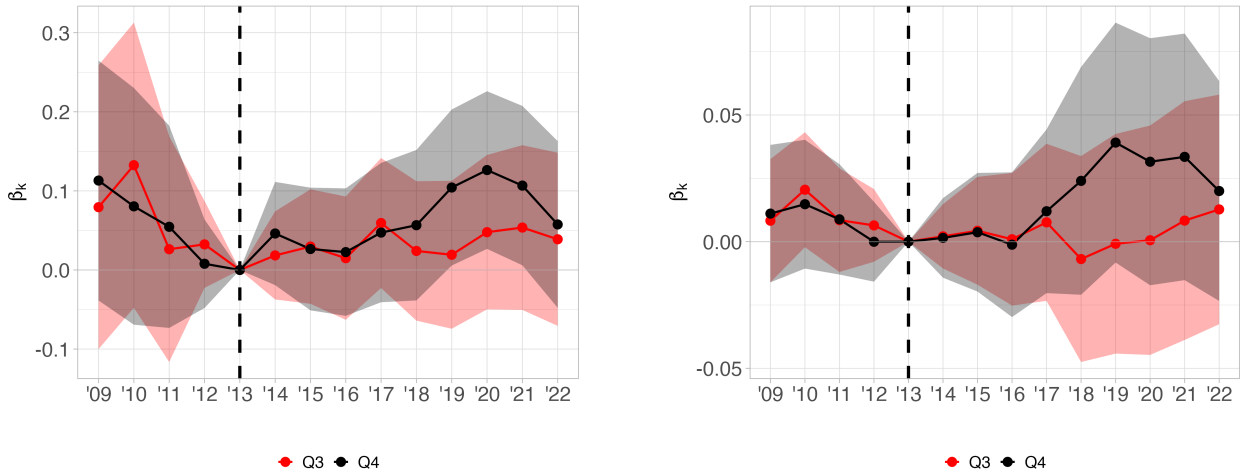
(a) Revenues not from procurement as a share of total revenues

(b) Revenues not from procurement

(c) Amount won

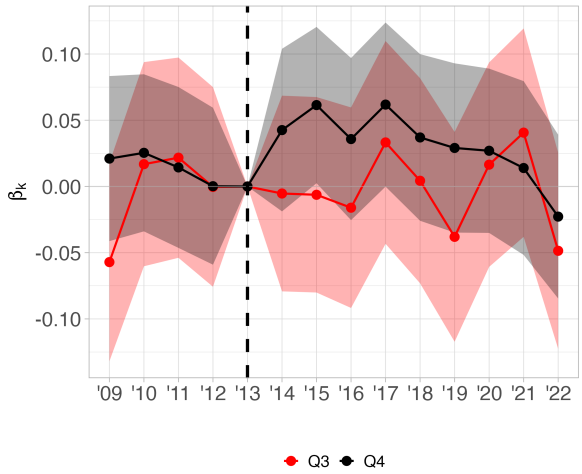
Figure 10 — **Strategic Adaptation by Surviving Suspected Firms**

Notes. Observations are at the firm-year level. All panels present the estimated coefficients β_k from regression $Y_{i,t} = \delta_{\text{pbt}} + \gamma_{\text{m}} + \alpha_{\text{i}} + \sum_{\substack{k=08 \\ k\neq 12}}^{22} \beta_k \cdot \text{Suspected}_i \cdot \mathbf{1}\{\text{t=k}\}_k + \epsilon_{i,t}$ along with 95% confidence intervals. Panel 10a shows the share of total revenues not derived from procurement contracts, calculated as (total revenues - procurement contract values) over total revenues. Panel 10b shows the absolute level of revenues not from procurement, calculated as total revenues minus procurement contract values. This variable is transformed using the inverse hyperbolic sine function to handle cases where the subtraction yields negative or zero values, which can occur due to the assignment strategy between multi-year contract values and annual revenues or when procurement represents the firm's primary revenue source. Panel 10c shows the total value of contracts (in Euros) won by firm i in year t, conditional on winning, 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.

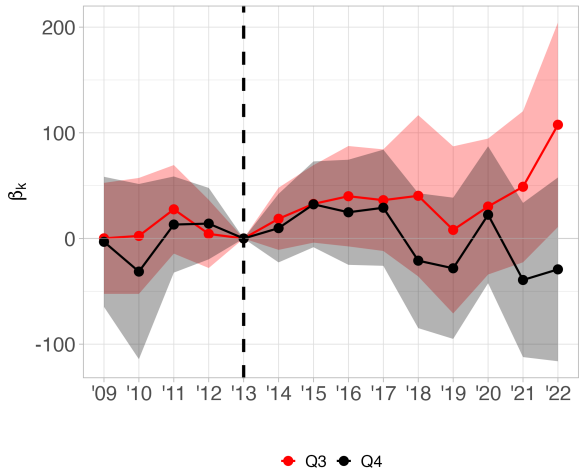


(a) Log employment (construction)

(b) Log # firms (construction)



(c) Log # new firms (construction)



(d) Income per capita

Figure 11 — **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 — Additional Tables and Figures

Table A1: **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.

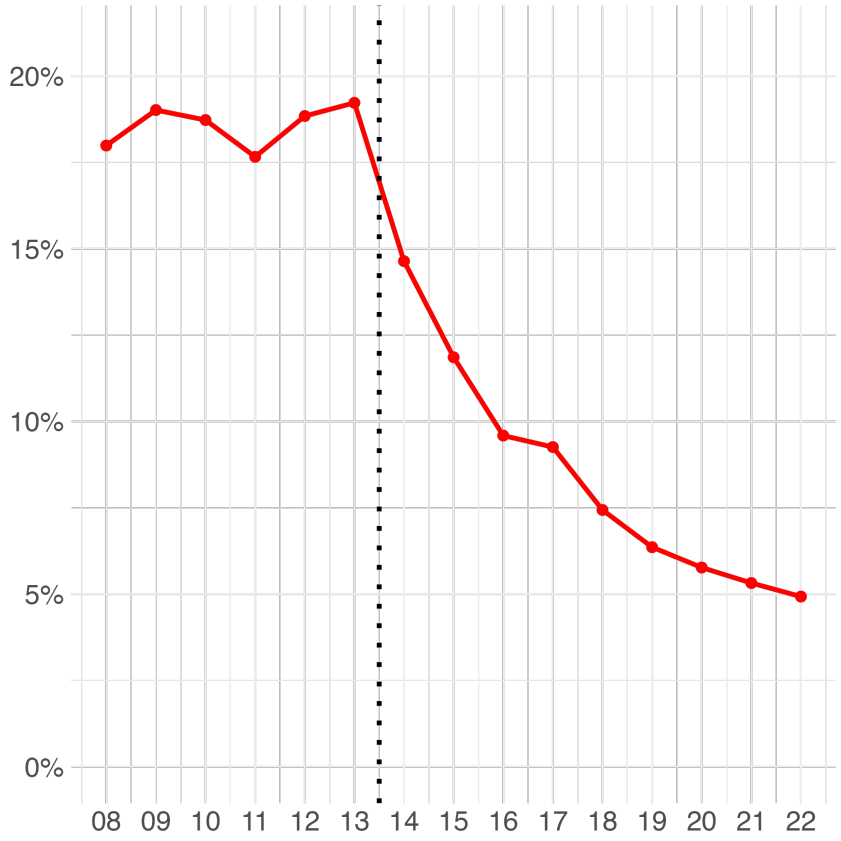
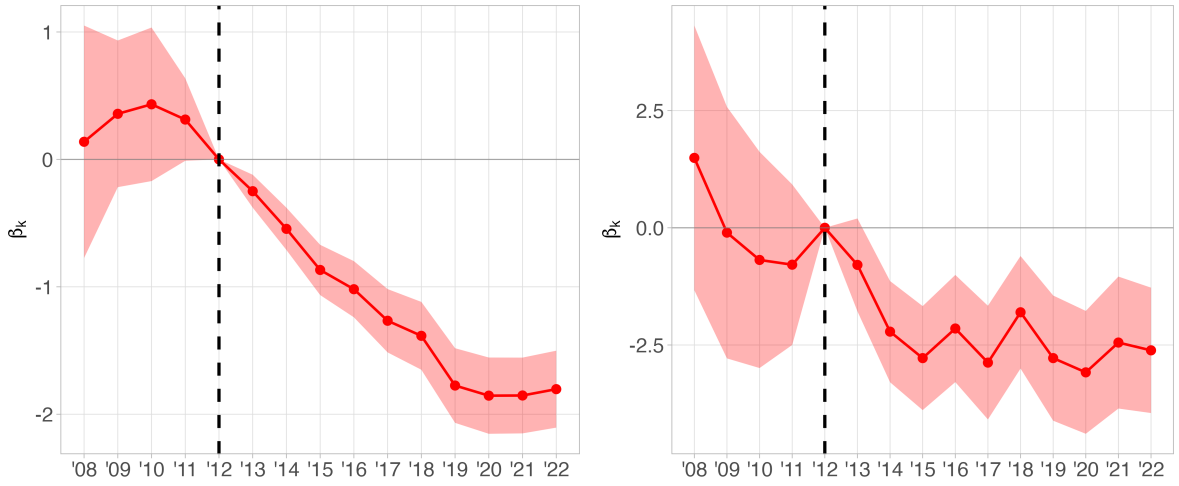


Figure A1 — **Tenders Won by Infiltrated Incumbents**. The y-axis represents the share of tenders won by infiltrated incumbents (bad types) out of all tenders won by incumbents each year (x-axis). The vertical dotted line marks the 2013 policy implementation.

Table A2: **Effect of Transparency Requirements on Suspected Firms Behavior and Performance**

	Procurement tenders					Non-procurement Revenues		
	Participated	Won	Assets	Employees	Exit Prob.	Share	Value	Contract Amount
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Suspected × Post	-0.340***	-0.218***	-0.377***	-0.270***	0.119***	0.173***	0.644*	-0.275***
	(0.038)	(0.022)	(0.037)	(0.027)	(0.006)	(0.030)	(0.355)	(0.081)
R^2	0.75482	0.62903	0.89603	0.86142	0.76951	0.31046	0.29443	0.62135
Observations	18,495	35,383	35,383	32,480	35,383	34,874	35,383	18,743
E[Y]	1.589	0.544	14.097	2.371	0.001	0.517	10.396	13.220
E[Y] non-transformed	9.256	1.216	13.365	35.804			4.030	2.479
Suspected dummy mean	0.332							
Prob. Bin-Year-Prov. fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes. Observations are at the firm-year level. The table presents difference-in-differences estimates from the regression $Y_{i,t} = \alpha_i + \gamma_{b,p,t} + \beta \cdot \text{Suspected}_i \cdot \text{Post}_t + \epsilon_{i,t}$, where $Y_{i,t}$ represents outcomes for firm i in year t. Treatment is defined as firms suspected of criminal ties (Suspected_i), identified through machine learning predictions combining police vetting data with firm characteristics. Post_t is an indicator for years after 2012. Columns (1)-(2) report procurement tender outcomes: number of tenders participated in and number of tenders won (log transformation). Columns (3)-(5) report firm characteristics: total assets (log), number of employees (log), and exit probability. Columns (6)-(7) report non-procurement revenue outcomes: share of revenues from non-procurement activities (inverse hyperbolic sine transformation) and value of non-procurement revenues (inverse hyperbolic sine transformation). Column (8) reports contract amount (log transformation). Fixed effects include firm fixed effects (α_i) and probability bin×year×province fixed effects ($\gamma_{b,p,t}$), where probability bins are defined by deciles of the predicted probability of having criminal ties (ranging from 0.5 to 1.0). E[Y] reports the mean of the dependent variable in the pre-reform period (before 2013). For transformed variables, E[Y] non-transformed reports the mean of the untransformed variable; values for total assets, non-procurement revenues, and contract amount are expressed in millions of euros. Suspected dummy mean reports the share of suspected firms in the analysis sample. Standard errors clustered at the firm level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.



(a) Revenues (Log)

(b) Return on Assets

Figure A2 — **Suspected Firms Performance: Additional Metrices**

Notes. Observations are at the firm-year level. Panels present the estimated coefficients β_k from regression $Y_{i,t} = \delta_{\text{pbt}} + \gamma_{\text{m}} + \alpha_{\text{i}} + \sum_{\substack{k=08 \\ k \neq 12}}^{22} \beta_k \cdot \text{Suspected}_i \cdot \mathbf{1}\{\text{t=k}\}_k + \epsilon_{i,t}$ along with 95% confidence intervals. Panels A2a, A2b report operating revenues and ROA for firm i in year t. Operating revenues 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.

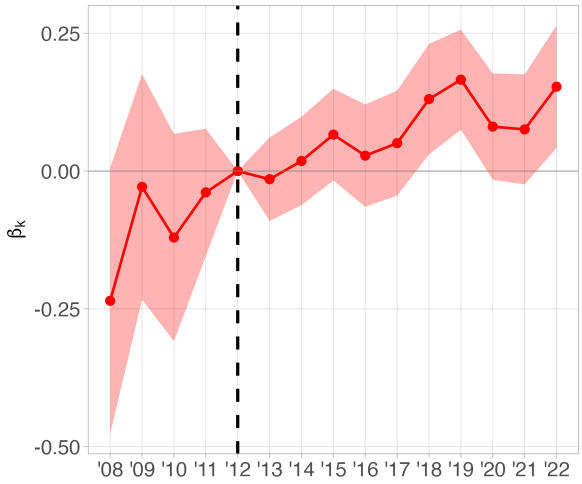
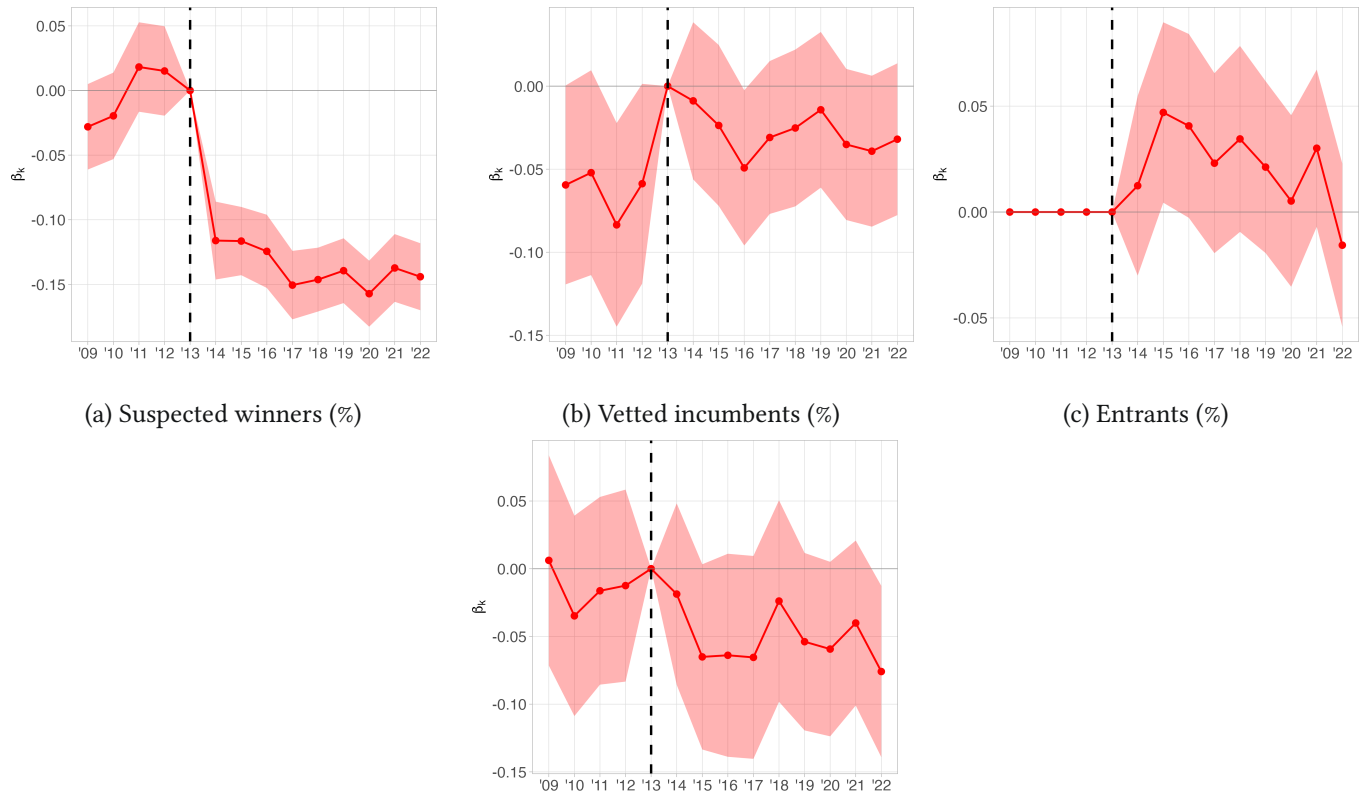


Figure A3 — **Strategic Adaptation by Surviving Suspected Firms: Bunching Below 150,000 Euros**

Notes. Observations are at the firm-year level. Panels present the estimated coefficients β_k from regression $Y_{i,t} = \delta_{\text{pbt}} + \gamma_{\text{m}} + \alpha_{\text{i}} + \sum_{\substack{k=08 \\ k\neq 12}}^{22} \beta_k \cdot \text{Suspected}_i \cdot \mathbf{1}\{\text{t=k}\}_k + \epsilon_{i,t}$ along with 95% confidence intervals. The dependent variable is a dummy taking value 1 if the value of the contract secured by firm i in year t is below 150,000 Euros. 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) Suspected winners (%)

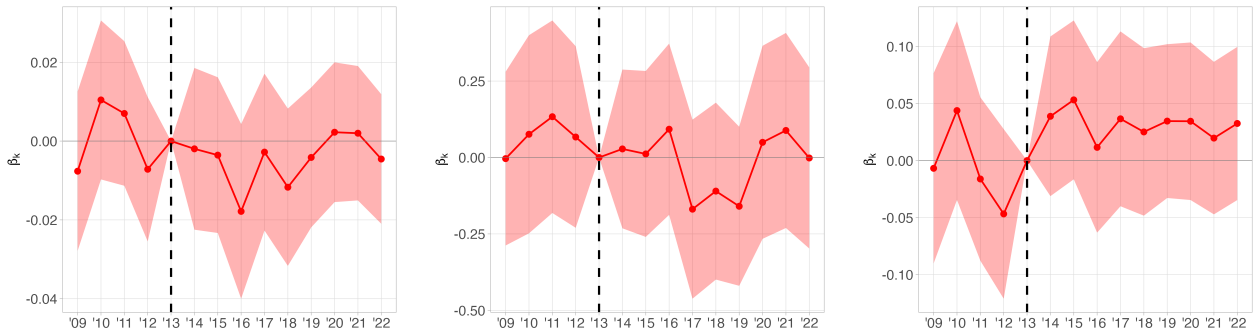
(b) Vetted incumbents (%)

(c) Entrants (%)

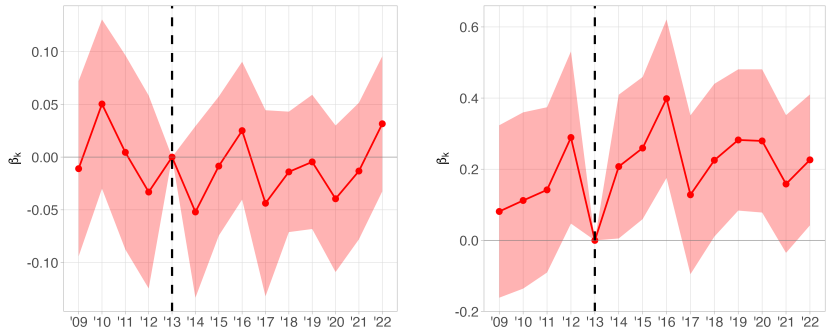
(d) Same province (%)

Figure A4 — **Procurement Market Composition: Event Study Analysis**

Notes. Observations are at the municipality-year level. Panels present the estimated coefficients β_k from the regression $Y_{m,t} = \alpha_m + \gamma_{p,t} + \delta_{d,t} + \sum_{\substack{k=2009 \\ k \neq 2013}}^{2022} \beta_k \cdot \text{Above Median Infiltration}_m \cdot \mathbf{1}\{t = k\} + \epsilon_{m,t}$, 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$_m$), measured as the share of tenders won by suspected firms during 2008-2013. Year 2013 serves as the reference period (omitted year). Shaded areas represent 95% confidence intervals. Panel A4a shows the share of tenders won by firms suspected of criminal ties, identified through machine learning predictions combining police data with firm characteristics. Panel A4b shows the share won by vetted incumbent firms, defined as those approved for the white list or predicted to have low probability of criminal ties who participated in procurement before 2014. Panel A4c shows the share won by entrant firms, defined as firms making their first appearance in procurement markets after 2013. Panel A4d presents the share of tenders won by firms incorporated in the same province as the contracting municipality, measuring the geographic concentration of procurement markets. Fixed effects include municipality fixed effects (α_m), year×province fixed effects ($\gamma_{p,t}$), and year×demographic category fixed effects ($\delta_{d,t}$), where demographic categories are defined by population size quintiles. Standard errors are clustered at the municipality level.



(a) Winning discount (%) (b) Project delay (%) (c) Subcontracting (%)



(d) Suspected subcontractors (%) (e) Bids per tender (log)

Figure A5 — **Procurement Performance and Competition: Event Study Analysis**

Notes. Observations are at the municipality-year level. Panels present the estimated coefficients β_k from the regression $Y_{m,t} = \alpha_m + \gamma_{p,t} + \delta_{d,t} + \sum_{\substack{k=2009 \\ k \neq 2013}}^{2022} \beta_k \cdot \text{Above Median Infiltration}_m \cdot \mathbf{1}\{t = k\} + \epsilon_{m,t}$, 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$_m$), measured as the share of tenders won by suspected firms during 2008-2013. Year 2013 serves as the reference period (omitted year). Shaded areas represent 95% confidence intervals. Panel A5a shows the average winning discount expressed as a percentage of the reserve price, measuring competitive pressure in bidding. Panel A5b reports project delays expressed as a percentage, capturing contract execution quality. Panel A5c shows the share of contracts involving subcontracting arrangements, indicating the use of secondary contractors. Panel A5d presents the share of subcontracted contracts where at least one subcontractor is a suspected firm, measuring continued infiltration through indirect channels. Panel A5e shows the log-transformed number of bids received per tender, a direct measure of procurement market competition. Fixed effects include municipality fixed effects (α_m), year×province fixed effects ($\gamma_{p,t}$), and year×demographic category fixed effects ($\delta_{d,t}$), where demographic categories are defined by population size quintiles. Standard errors are clustered at the municipality level.

B — Machine Learning Algorithm for Participation in Procurement Market

This section describes the machine learning methodology used to predict firm participation in public procurement markets. The analysis focuses exclusively on predicting participation behavior using data from firms with complete balance sheet information, as these firms provide the richest feature set for accurate prediction and constitute the primary sample for subsequent causal analysis.

B.1. Dataset Preparation

The dataset contains firm-level information on public procurement contracts and firm participation for the years leading to the policy implementation (2009–2013). For data preparation I follow Ash, Galletta, and Giommoni (2025) and apply the following steps.

- **Subsetting:** The data is restricted to contracts closed before 2014 to focus on the pre-policy 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:** A balanced panel dataset is created 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 analysis begins with 115,907 firms incorporated in the sample provinces with available balance sheet data between 2009 and 2013. From this universe, I identify 8,569 incumbent firms that won or participated in at least one public procurement tender during this period. The final analytical sample consists of 6,585 incumbent firms with complete balance sheet information across all years. This sample exhibits a reasonably balanced distribution of participation outcomes: 59.7% of firms participated in procurement in 2013 (3,933 firms) versus 40.3% that did not participate (2,652 firms), yielding a balance ratio of 0.68 (Table B1).

The final set of predictor variables includes:

- **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 (EBITDA/Revenues) in years $t-1$, $t-2$, $t-3$, and $t-4$.

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

B.2. Model Training and Selection

The data is split into 80% training and 20% testing sets at the firm level, yielding approximately 5,268 firms for training and 1,317 firms for out-of-sample evaluation. This ensures performance assessment on data the models have never seen. Four different classification algorithms are implemented and compared: logistic regression (baseline), random forest, neural networks, and XGBoost (extreme gradient boosting).

Table B1 presents comprehensive performance metrics for all four models evaluated on the test set. XGBoost demonstrates superior performance across key metrics, achieving an F1-score of 0.838, precision of 0.855 at the 50% threshold, and AUC-ROC of 0.878. These results indicate that XGBoost balances precision and recall while maintaining high discriminatory power between participants and non-participants.

Table B2 provides detailed confusion matrices for all four models at the 50% classification threshold. The matrices reveal that XGBoost achieves the best balance between true positives (657) and true negatives (406), while minimizing both false positives (111) and false negatives (143). This performance translates to reliable identification of both participating and non-participating firms.

Figure B2 illustrates the comparative performance of all models through ROC curves and detailed performance metrics. Panel (a) shows that XGBoost and Random Forest achieve comparable AUC values (0.878 and 0.879 respectively), both substantially outperforming logistic regression (0.806) and neural networks (0.799). Panels (b) and (c) provide detailed threshold analysis for the XGBoost model specifically, demonstrating the trade-offs between precision, recall, and accuracy across different classification thresholds.

B.3. XGBoost Performance Analysis

Motivated by XGBoost's performance and interpretability, I select it as the primary algorithm and proceed with further performance analysis. Table B3 presents comprehensive performance metrics across thresholds ranging from 10% to 90%. The analysis confirms that the standard 50% threshold provides an optimal balance between precision (0.855) and recall (0.821), yielding the

highest F1-score (0.838). This performance level supports confidence in using XGBoost predictions for subsequent treatment assignment in the causal analysis.

B.4. Model Interpretability

Table B4 reports the 20 most important predictors according to XGBoost's gain metric, which measures each feature's relative contribution to the model's predictive accuracy. Procurement history emerges as the dominant predictor, with participation in the previous year (t-1) alone accounting for 28.1% of the model's predictive power. Past procurement indicators collectively contribute over 40% of total predictive capacity, reinforcing the fundamental pattern that firms with prior procurement experience exhibit strong persistence in participation behavior.

Beyond procurement history, firm characteristics like year of incorporation (4.9% contribution) and financial variables including operating revenues and return on assets feature prominently. This pattern indicates that the algorithm successfully learns economically meaningful relationships: larger, more established firms with stronger financial positions and extensive procurement experience are more likely to continue participating in public procurement markets. The dominance of lagged participation variables validates the theoretical expectation that past behavior strongly predicts future procurement engagement.

B.5. Model Validation Exercises

Two validation exercises address concerns about systematic measurement error in ML-generated treatment variables (Battaglia et al. 2024; Ludwig, Mullainathan, and Rambachan 2025). Table B5 tests whether prediction errors correlate with white list status for firms with predicted participation $\geq 50\%$. If the ML systematically mispredicts for criminal firms, this bias should correlate with absence from vetted firm lists. The results show no significant correlation ($\rho = 0.053$, $p = 0.144$), indicating that ML errors are not correlated with the treatment assignment. This rules out the concern that the classification captures firms the model predicts poorly rather than genuine avoidance behavior.

Threshold robustness analysis further validates the ML approach by testing whether main econometric results remain stable across different ML classification cutoffs. The exercise creates five datasets using ML probability thresholds of 50%, 60%, 70%, 80%, and 90% to classify firms

as "suspected" criminal, then runs identical difference-in-differences regressions on each sample. Figure B3 shows that treatment effects maintain consistent directions and economically meaningful magnitudes across all thresholds for key outcomes including participation, revenues, ROA, and employment. This stability indicates that results do not depend on the specific 50% classification cutoff, supporting the robustness of the ML-based identification strategy.

Table B1: Out-of-Sample Performance Metrics for Participation Prediction

	Logistic	Random Forest	Neural Network	XGBoost
Accuracy	0.73	0.82	0.781	0.807
AUC-ROC	0.806	0.879	0.799	0.878
F1	0.768	0.849	0.812	0.838
Sample size	1317 firms			
Participation rate	59.7%			
Balance ratio	0.68			

Notes: Performance metrics calculated on test set of firms with complete balance sheet data. Accuracy = (TP+TN)/(TP+TN+FP+FN); Precision = TP/(TP+FP); Recall = TP/(TP+FN); F1-Score = 2×(Precision×Recall)/(Precision+Recall); AUC-ROC measures discriminatory power across all classification thresholds. TP=True Positive, TN=True Negative, FP=False Positive, FN=False Negative.

Table B2: Confusion Matrices for Participation Prediction Models

	Prediction	
	Not Participate	Participate
Panel A. Logistic Regression		
Not Participate	374	143
Participate	212	588
Panel B. XGBoost		
Not Participate	406	111
Participate	143	657
Panel C. Random Forest		
Not Participate	415	102
Participate	135	665
Panel D. Neural Network		
Not Participate	407	110
Participate	178	622

Notes: Confusion matrices for all four models at 50% classification threshold on test set of firms with complete balance sheet data. Sample size: 1,317 firms. Values represent counts of True Negatives (TN), False Positives (FP), False Negatives (FN), and True Positives (TP).

Table B3: XGBoost Performance Across Different Classification Thresholds

Threshold	Accuracy	Precision	Recall	Specificity	F1-Score
0.10	0.635	0.625	0.994	0.079	0.768
0.15	0.658	0.643	0.981	0.159	0.777
0.20	0.694	0.672	0.970	0.267	0.794
0.25	0.719	0.701	0.939	0.379	0.802
0.30	0.754	0.741	0.915	0.505	0.819
0.35	0.788	0.786	0.895	0.623	0.837
0.40	0.805	0.819	0.871	0.702	0.844
0.45	0.813	0.843	0.851	0.754	0.847
0.50	0.807	0.855	0.821	0.785	0.838
0.55	0.802	0.873	0.789	0.822	0.829
0.60	0.801	0.889	0.769	0.851	0.824
0.65	0.794	0.900	0.744	0.872	0.815
0.70	0.783	0.913	0.710	0.896	0.799
0.75	0.771	0.932	0.671	0.925	0.781
0.80	0.745	0.939	0.620	0.938	0.747
0.85	0.710	0.948	0.552	0.954	0.698
0.90	0.672	0.953	0.484	0.963	0.642

Notes: Performance metrics calculated on test set of firms with complete balance sheet data. Sample size: 1,317 firms. Accuracy = (TP+TN)/(TP+TN+FP+FN); Precision = TP/(TP+FP); Recall = TP/(TP+FN); Specificity = TN/(TN+FP); F1-Score = 2×(Precision×Recall)/(Precision+Recall). TP=True Positive, TN=True Negative, FP=False Positive, FN=False Negative.

Table B4: **Feature Importance: Participation Prediction Model**

Feature	Gain
N. contracts participated t-1 (log)	0.281
N. contracts participated t-2 (log)	0.049
Year of incorporation	0.049
Participant t-1	0.040
Participant t-4	0.040
Participant t-2	0.027
Operating revenues t-1 (log)	0.026
ROA t-1	0.026
Equity t-1 (log)	0.021
Contract value t-1 (log)	0.019
ROA t-2	0.019
ROA t-4	0.019
Profit margin t-3	0.018
Profit margin t-2	0.017
Profit margin t-1	0.017
Profit margin t-4	0.017
ROA t-3	0.016
Fixed assets t-1 (log)	0.016
Operating revenues t-3 (log)	0.016
Operating revenues t-2 (log)	0.014
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 0.878.

Table B5: ML Prediction Error Analysis by White List Status

WL Status	N	Mean Error	Actual Part.	Predicted Part.
High Probability Firms ($\geq$ 50% predicted participation)				
Never in WL	297	0.019	0.798	0.817
Ever in WL	471	-0.0159	0.8917	0.8758
Correlation Test Results				
Error-WL Correlation		$\rho = 0.053$, $p = 0.144$		Difference: 0.035

Notes: This table analyzes ML prediction errors for firms with predicted participation $\geq$ 50% using out-of-sample test set evaluation. Mean Error = Predicted Participation - Actual Participation. Positive errors indicate over-prediction of participation rates. The correlation test examines whether prediction errors systematically relate to white list status, addressing concerns about measurement error bias in treatment assignment. Non-significant correlation (p > 0.05) supports the absence of systematic bias.

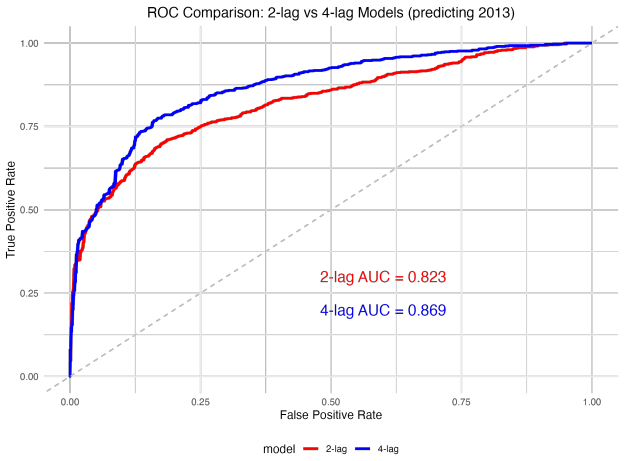
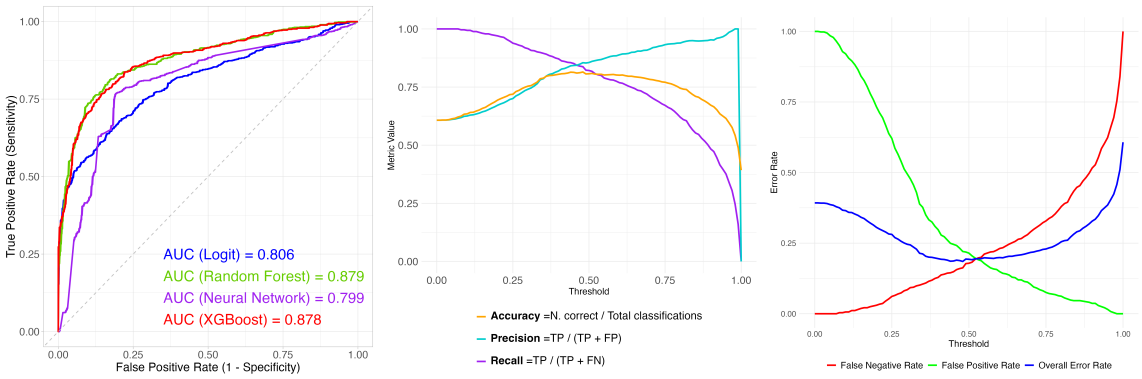
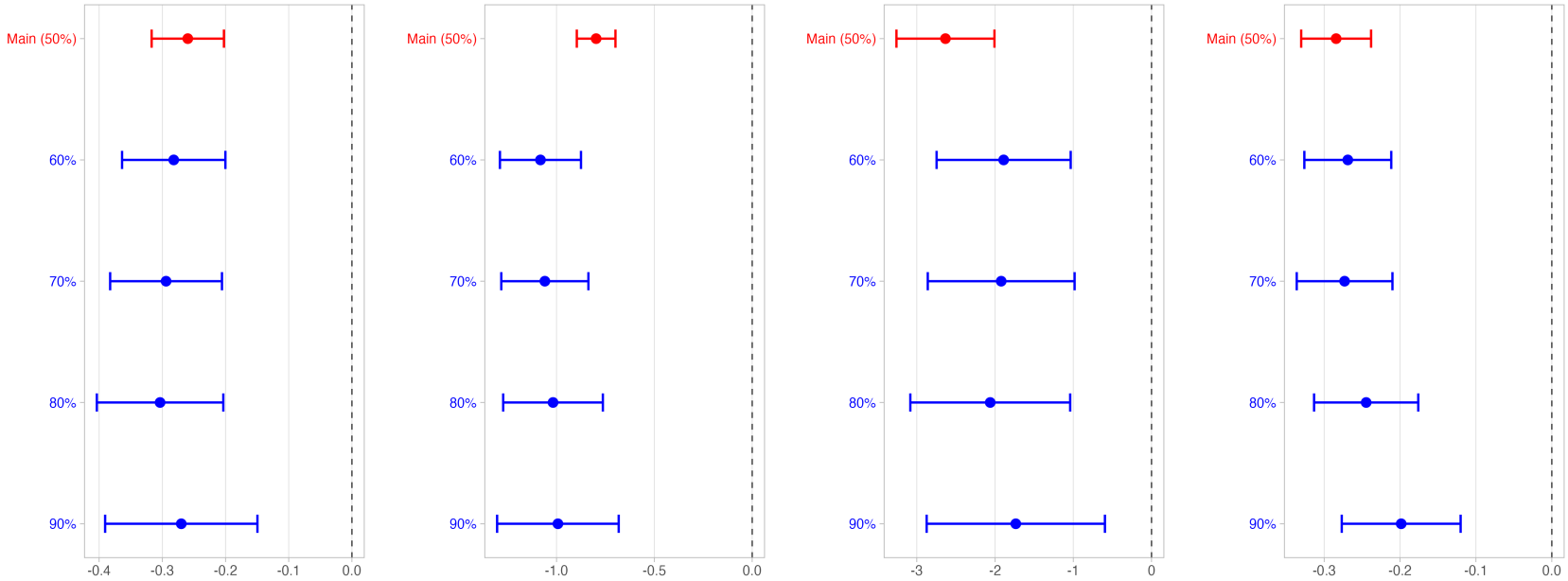


Figure B1 — Model Performance for Procurement Participation Prediction: 2-lag vs. 4-lag model. Both models are trained on 2011-2012 data to predict 2013 participation using identical variable sets. The 2-lag model uses variables from years $t-2$ and $t-1$, while the 4-lag model incorporates variables from years $t-4$ through $t-1$. The ROC curves demonstrate comparable predictive performance between the two specifications, validating the use of the 2-lag model for extended time series analysis where 4-lag data is unavailable.



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

Figure B2 — Model Performance for Procurement Participation Prediction. Models trained on 2009-2012 firm data using lagged financial indicators and procurement history. Data split 80% training, 20% test on balance sheet firms. Panel (a) shows ROC curves with AUC values indicating discriminatory power. Panel (b) displays trade-offs between Recall, Precision, and Accuracy across thresholds for XGBoost. Panel (c) shows error rate variations across classification thresholds. Performance metrics: Recall = TP/(TP + FN); Precision = TP/(TP + FP); Accuracy = (TP + TN)/(TP + TN + FP + FN).



(a) Participation (b) Operating Revenues (c) Return on Assets (d) Employment

Figure B3 — Threshold Robustness Analysis. Each panel shows treatment effect coefficients from difference-in-differences regressions using different ML probability thresholds to classify suspected firms. The main specification (50% threshold) appears in red, while alternative thresholds (60%-90%) appear in blue. Consistent effect directions and magnitudes across thresholds demonstrate that results do not depend on arbitrary ML classification choices.

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[43]:

- **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

43. 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

$$\max(\text{predicted_part_dummy_2014}, \text{predicted_won_dummy_2014}) < 0.5$$

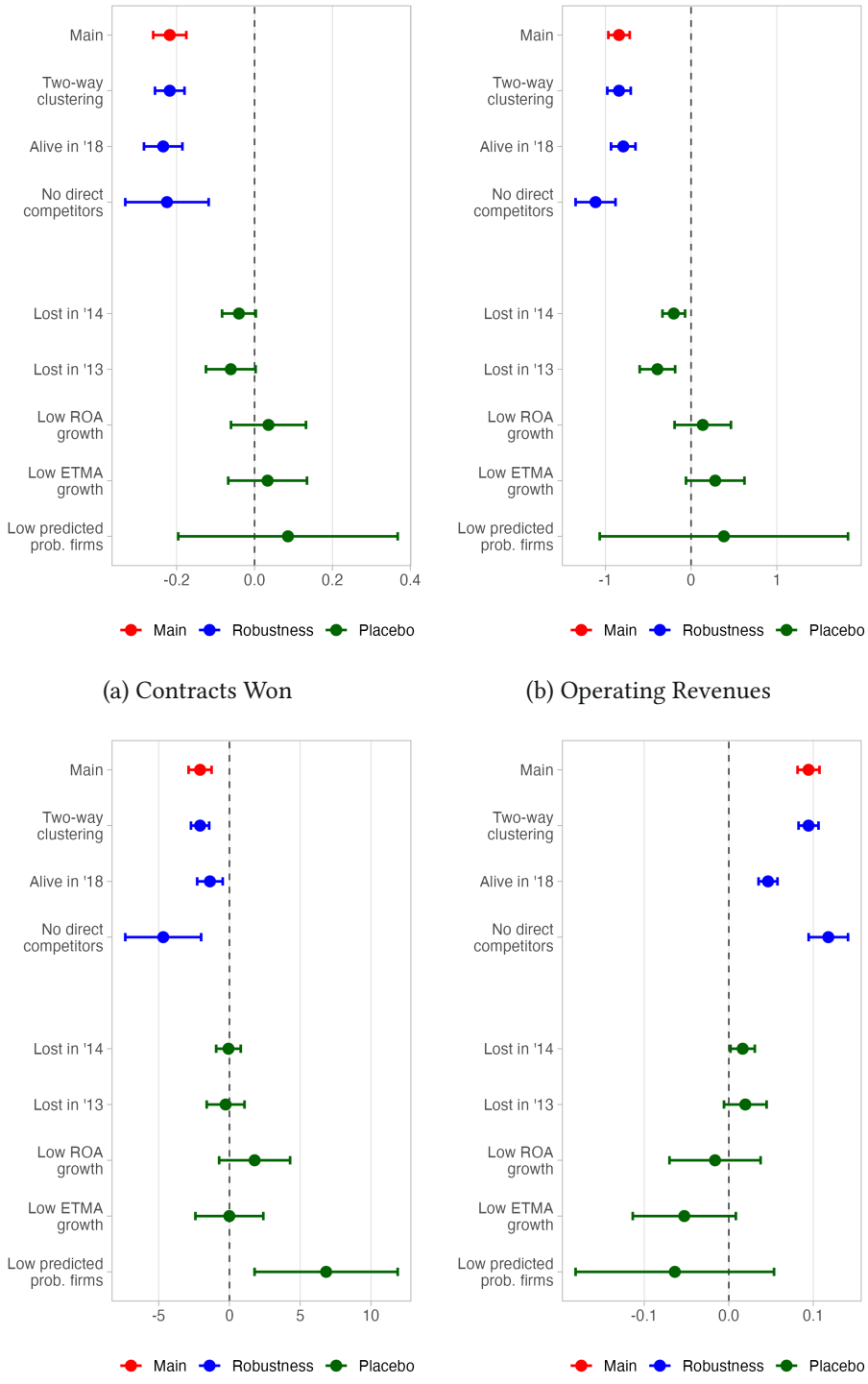
, and firms are grouped into probability bins using 0.1-unit intervals between 0 and 0.5.

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.



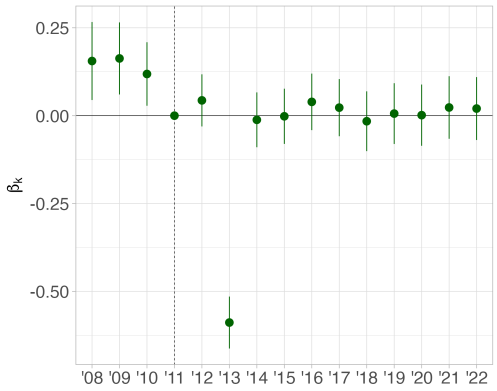
(a) Contracts Won

(b) Operating Revenues

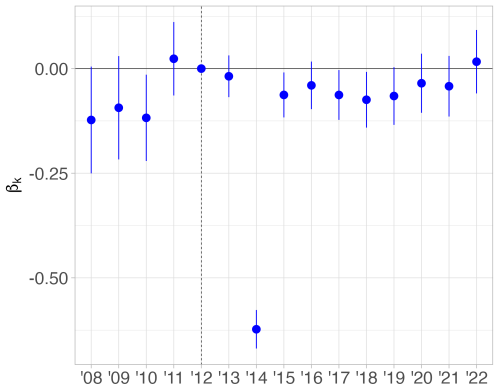
(c) Return on Assets

(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.

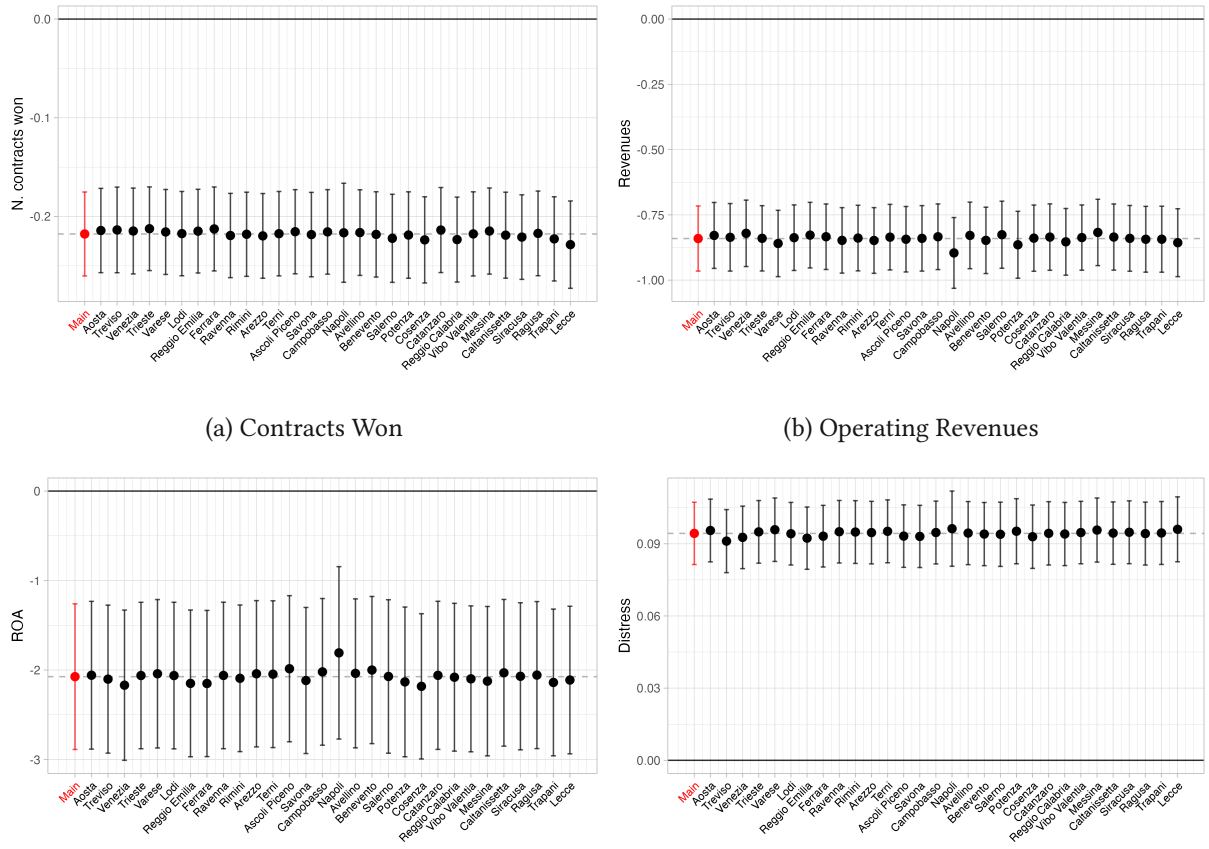


(a) Lost in 2013



(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.