# Essays on Private Firms' Information Environment

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# Declaration

I declare that the work presented in this dissertation is, to the best of my knowledge and belief, original and my own work. The material has not been submitted, either in whole or in part, for a degree at this, or any other university. I further declare that Chapter 3 is co-authored with Dr. Kalash Jain, while Chapter 4 is co-authored with Prof. Justin Chircop. For these two chapters, I was responsible for developing the research ideas and questions, designing and conducting empirical analyses, and writing the chapters. The contributions of the co-authors have been limited to the reasonable level expected in a doctoral thesis at a research university in the United Kingdom. 

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# Abstract

This thesis investigates how interactions with government agencies shape private firms' information environment. Chapter 2 examines how government certifications on firms' regulatory compliance affect firms' access to public procurement and the efficiency of procurement contract allocation. I explore the introduction of the Legality Rating in Italy, a government certification rating firms based on their efforts to prevent misconduct and criminal infiltration. I find that the certification improves firms' access to public procurement. Furthermore, I show that certified firms execute their contracts more efficiently with fewer cost overruns, modifications, and delays. Overall, the results show that government certifications can improve the allocation of public resources.

Chapter 3 investigates how information spillovers among product market peers mitigate private firms' information frictions regarding government programs. Using a novel definition of product market peers based on firms' common bids for public procurement contracts, we find that firms are more likely to obtain the Legality Rating after competing in a public procurement contract with a certified peer. Cross-sectional tests reveal that firms obtain the certification primarily to reduce certified peers' competitive advantage. This study identifies a novel channel through which firms acquire information—public procurement networks.

Chapter 4 examines the effect of participating and losing a bid for a public procurement contract on firms' tax avoidance. We predict that disclosing the outcome of a public procurement contract allows firms to learn about their competitiveness relative to peers. Consistent with this prediction, we find that firms engage in more tax avoidance to improve their competitive position after losing a bid for a public procurement contract. Furthermore, we show that increased tax avoidance raises firms' likelihood of winning a subsequent public procurement contract. This paper provides novel evidence on how disclosing public procurement outcomes facilitates learning about participating firms' competitiveness.

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*Alla mia famiglia, ma soprattutto a Nonna Rosa e Nonno Carmine*
*(To my family, but especially to my grandma Rosa and my grandpa Carmine)*

# Chapter 1

# Introduction

Private firms have always constituted the overwhelming majority of firms in the world, accounting for a significant share of the total GDP and workforce (Beuselinck et al., 2023). However, their relevance in the global economy has remarkably increased over the last two decades. While many firms have voluntarily delisted from stock exchanges, high-growth firms are increasingly reluctant to go public, as shown by declining IPO volumes in developed countries (e.g., Stulz, 2020). Academics and policymakers generally attribute this reduced propensity to go public to two concurrent explanations. First, because some regulations apply only to public firms, their regulatory burden—ranging from compliance costs (e.g., SOX 404) to the mandatory disclosure of proprietary information—might outweigh the benefits of being public (Aghamolla and Thakor, 2022a; Slutzky, 2021). Second, high-growth private firms can access private capital more easily than before, whether through investments from private equity funds or established peers, ultimately reducing their reliance on public capital markets to finance growth (Cunningham et al., 2021; Ewens and Farre-Mensa, 2020; Stulz, 2020).

Given the growing economic relevance of private firms, their information environment has attracted increasing regulatory and academic interest. However, while regulators are debating whether to impose stricter reporting mandates on private firms (e.g., Crenshaw, 2022), empirical evidence on their benefits is more nuanced. Lacking strong capital market incentives, private firms disclose information primarily in response to stakeholders' demands (Atz et al., 2023; Breuer et al., 2017). Thus, reporting mandates might impose unnecessary costs on firms with fewer incentives for voluntary disclosure (e.g., low stakeholder demand). Furthermore, although public disclosures generally lead to positive spillovers, recent studies show how information frictions might prevent private firms from using peers' disclosures (Christoffersen et al., 2024; Gassen and Muhn, 2025). Given this mixed evidence, understanding the implications of private firms' disclosures and factors influencing their information acquisition is crucial for informing the academic and regulatory debate.

In this thesis, I investigate how interactions with government agencies—a crucial stakeholder—shape private firms' information environment. Given the heterogeneity of these agencies' tasks, they act both as providers and users of firms' information. On the one hand, they have privileged access to confidential data (e.g., firms' tax or regulatory compliance records). Therefore, by providing this information to market participants, their disclosures can affect private firms' access to product and capital markets through a certification or shaming effect (Bonfim et al., 2023; Johnson, 2020). On the other hand, because government agencies are a major corporate customer through the public procurement process (Cohen and Li, 2020), they are also active users of private firms' information, e.g., when screening potential bidders for awarding public procurement contracts. Furthermore, when bidding for public procurement contracts, firms share valuable cost- or product-related information with their peers. Therefore, participating in public procurement might allow firms to improve their information set by observing peers' bids.

For my analyses, I rely on the Italian private firm setting which provides three desirable features. First, the role of private firms in the local economy is sizeable and comparable to other developed countries, such as the U.S. or the UK (Beuselinck et al., 2023). Second, because Mafia infiltration in public procurement is widespread (e.g., Marcolongo, 2023; Ravenda et al., 2020), there is a strong demand for increased firm transparency to avoid allocating public procurement contracts to criminal or corrupt firms. However, the majority of government contractors are private SMEs with limited external monitoring and reduced disclosure requirements. Thus, acquiring and evaluating information on these firms is crucial but also challenging. Third, the wide availability of data on firms' financial information and public procurement contracts allows large-scale empirical analyses.

My doctoral thesis consists of three papers. The first paper, presented in Chapter 2 and titled "*Government Certification in Public Procurement: Evidence from the Italian Legality Rating*" investigates how government certifications on private firms' regulatory compliance affect procurement agencies' allocation decisions and aggregate welfare. The second and third papers focus on how private firms acquire information through the public procurement process. Specifically, the second paper, presented in Chapter 3 and titled "*Product Market Networks and the Take-up of Government Programs*" explores how information spillovers in public procurement networks increase the take-up of government programs among private firms. The third paper, presented in Chapter 4 and titled "*Learning when Losing: Evidence from Public Procurement Contracts*" shows how disclosing the outcome of a public procurement contract allows firms to learn about their competitiveness relative to peers.

In particular, in the solo-authored paper "*Government Certification in Public Procurement: Evidence from the Italian Legality Rating*" I study how government certifications on firms' regulatory compliance affect firms' access to public procurement and the efficiency of procurement contract allocation. To answer this research question, I explore the introduction of the Legality Rating in Italy, a government certification rating firms based on their efforts to prevent misconduct and criminal infiltration. I find that the certification improves firms' access to public procurement. Furthermore, I show that certified firms execute their contracts more efficiently with fewer cost overruns, modifications, and delays. However, I uncover wide heterogeneity among certified firms: primarily firms with higher certification scores experience improved access to public procurement and execute contracts systematically better. By estimating aggregate effects, I document that the certification increases participation in public procurement and is revenue-positive for the government.

This paper relates to three streams of literature. First, it contributes to literature examining the effects of government transparency initiatives on resource allocation. While prior literature has focused on the role of reporting and auditing mandates (e.g., Breuer, 2021; Breuer et al., 2023), this paper documents that, if coupled with a discrete rating system, government certifications can be an alternative and cost-effective method to improve the allocation of public resources. Second, by showing how increased transparency on supplier firms' regulatory compliance can be informative to procurement agencies, this paper adds to the literature examining the effects of transparency in public procurement (e.g., Coviello and Mariniello, 2014; Nathan, 2024; Seregni, 2024). Third, this paper relates to the emerging literature examining the effects of organized crime on the legal economy. While prior literature has primarily focused on the negative externalities imposed by organized crime on legitimate firms (e.g., Fenizia and Saggio, 2024; Slutzky and Zeume, 2023), this study documents how voluntary disclosure can allow firms to signal their legitimacy in contexts with high criminal infiltration.

In the paper "*Product Market Networks and the Take-up of Government Programs*" (co-authored with Kalash Jain), we investigate how information spillovers among product market peers mitigate private firms' information frictions regarding government programs. Using a novel definition of product market peers based on firms' common bids for public procurement contracts, we find that firms are more likely to obtain the Legality Rating after competing in a public procurement contract with a certified peer. Cross-sectional tests reveal that firms obtain the certification primarily to reduce certified peers' competitive advantage.

This study contributes to four streams of literature. First, this paper adds to the emerging literature examining the effects of information frictions on government programs' take-up (e.g., Gupta et al., 2023; Zwick, 2021). Our study shows that information spillovers in product markets can mitigate these frictions and ultimately increase take-up rates. Second, by identifying public procurement networks as a novel channel through which firms learn about government programs, our study contributes to the literature on peer effects in program participation (Dahl et al., 2014; Mora-García and Rau, 2023). Third, by documenting the spillover effects of private firms' certification decisions, a form of voluntary disclosure, this study relates to the literature examining spillover effects of private firms' disclosure (e.g., Bernard et al., 2021; Breuer et al., 2022; Kim and Olbert, 2022). Fourth, our study contributes to the literature on industry classifications (e.g., Hoberg and Phillips, 2016; Kaustia and Rantala, 2021; Lee et al., 2015) by constructing a novel classification of product market peers for private firms.

In the paper "*Learning when Losing: Evidence from Public Procurement Contracts*" (co-authored with Justin Chircop), we investigate the effect of participating and losing a bid for a public procurement contract on firms' tax avoidance. We predict that disclosing the outcome of a public procurement contract allows firms to learn about their competitiveness relative to peers. Consistent with our prediction, we find that firms engage in more tax avoidance to improve their competitive position after losing a bid for a public procurement contract. This relation is stronger for firms facing stiffer public procurement competition and financially constrained firms. Furthermore, we show that increased tax avoidance raises firms' likelihood of winning a subsequent public procurement contract.

This study informs the debate about the relationship between competition and tax avoidance (Cai and Liu, 2009; Kubick et al., 2015). Indeed, we show that losing a bid for a public procurement contract, hence learning that the firm is uncompetitive relative to its peers, incentivizes firms to improve their competitiveness by engaging in tax avoidance. Second, by documenting the unintended consequences of public procurement participation on losing firms, this study informs the debate about the role of public procurement on tax avoidance activities (Mills et al., 2013) Given that multiple losing firms exist for each winning firm (i.e., government contractor), investigating this research question is economically relevant. Third, this study also contributes to the emerging literature examining the real effects of transparency in public procurement (e.g., Coviello and Mariniello, 2014; Duguay et al., 2023) by showing how disclosing public procurement outcomes facilitates learning about participating firms' competitiveness.

Overall, by examining how government certifications affect procurement agencies' allocation decisions and how firms acquire information through the public procurement process, this thesis contributes to the timely debate on private firms' information environment. As private firms have become more prominent in the global economy over the last two decades, there have been growing calls to impose stricter reporting mandates on these firms. However, evidence on the net benefits of these mandates and, in general, on private firms' usage of public disclosure is nuanced. In this respect, this thesis contributes to this academic and regulatory debate in two key ways. First, it shows how government certifications can be an alternative and cost-effective method to improve the allocation of public resources. Second, it documents how improved transparency on government initiatives—such as disclosing public procurement outcomes or recipients of government programs—can facilitate information spillovers that ultimately mitigate private firms' information frictions.

The rest of the thesis is organized as follows. Chapter 2 is based on the paper "*Government Certification in Public Procurement: Evidence from the Italian Legality Rating*". Chapter 3 is based on the paper "*Product Market Networks and the Take-up of Government Programs*". Chapter 4 is based on the paper "*Learning when Losing: Evidence from Public Procurement Contracts*". Chapter 5 concludes this thesis.

# Chapter 2

# Government Certification in Public Procurement: Evidence from the Italian Legality Rating

## 2.1 Introduction

Due diligence is a crucial aspect of customer-supplier transactions. Selecting suppliers with poor compliance track records (e.g., regulatory violations) carries significant reputational and monetary costs (Dai et al., 2021; She, 2022).[1] While regulators and activist groups demand increased disclosures of suppliers' compliance, customers often lack the adequate infrastructure and expertise to gather, verify, and evaluate such information (Christensen et al., 2021; Kim and Davis, 2016). Transparency around suppliers' compliance is particularly relevant to public procurement agencies, a major corporate customer (Cohen and Li, 2020). Given the sheer size of contracts and their material effect on supplier firms (e.g., Ferraz et al., 2015; Hvide and Meling, 2022), public procurement is notoriously vulnerable to corruption and criminal infiltration (Colonnelli et al., 2022; Marcolongo, 2023). In this respect, limited transparency around suppliers' compliance can lead to a misuse of public resources by increasing the risk of allocating public procurement contracts to criminal or corrupt firms.

In this paper, I examine whether and how a government certification of firms' compliance improves firms' access to public procurement and the efficiency of the allocation of procurement contracts. I explore a unique institutional setting in Italy, where the government introduced a certification program (the Legality Rating, hereafter "LR") in 2012. The LR is available to firms at no cost and rates recipients in terms of a score—ranging from one to three stars—based on their efforts to prevent misconduct and criminal infiltration. To obtain the baseline one-star LR, firms must meet some minimum compliance requirements (e.g., no prior regulatory sanctions or criminal infiltration), have sales above EUR 2 million in the year preceding the request, and have been included in the Italian Business Register for at least two years. Firms can earn two-star or three-star scores by implementing additional internal control mechanisms, such as adhering to anti-Mafia protocols or adopting organizational structures to prevent corruption.[2] The list of LR recipients is publicly available, and several firms advertise the award on their website.

Two institutional features make the LR particularly relevant to the Italian context. First, given that public procurement accounts for a sizeable fraction of economic activity in Italy (11% of GDP), it provides an attractive source of revenues for criminal firms. According to

[1] As studies use the term "compliance" primarily to indicate compliance with laws and regulations (e.g., Kalmenovitz, 2023; Trebbi et al., 2023), I adopt a similar approach.

[2] I discuss the requirements for LR eligibility and the criteria for LR scores in more detail in Section 2.2.1 below.

a large body of judicial and empirical evidence (e.g., Marcolongo, 2023; Tulli, 2024), Mafia organizations participate in public procurement through apparently legitimate firms for securing lucrative procurement contracts. Second, as in other European countries, most Italian firms are private small and medium enterprises (SMEs). Although these firms win the majority of public procurement contracts in the European Union (European Commission, 2019), they are subject to limited external monitoring and reduced disclosure requirements. Thus, acquiring and evaluating information on their compliance is crucial but also challenging.

The LR might improve firms' access to public procurement and contract allocation efficiency by increasing transparency around supplier firms. Italian law requires procurement agencies to *ex ante* verify bidding firms' compliance by checking multiple sources, such as criminal records and databases of prior regulatory violations. By providing a single and coarse signal of supplier firm quality, the LR might reduce the cost of searching, acquiring, and evaluating such information. It can thus help procurement agencies identify and allocate contracts to superior supplier firms, thereby improving allocative efficiency.

However, the impact of the LR depends on its prominence and credibility, which are *a priori* unclear. The LR might not be informative to procurement agencies that could have already inferred supplier firms' quality through internal controls or past interactions. Moreover, given weak incentives and favoritism in government organizations (e.g., Prendergast, 2007), the screening of LR applicants might lack sufficient rigor. Therefore, criminal and corrupt firms might obtain the LR to enhance their reputation (Daniele and Dipoppa, 2023; Luca and Zervas, 2016), ultimately impairing its credibility. This reduced credibility might, in turn, lead procurement agencies to disregard the LR in their contract allocation decisions. Alternatively, if they rely on it, the LR might distort the allocation of resources by easing Mafia firms' access to lucrative procurement contracts.

Investigating the effect of the LR on firms' access to public procurement is empirically challenging. First, given the voluntary nature of the LR, the main challenge is selection bias: certified firms might differ from non-certified firms across different dimensions. Second, it is crucial to separate the certification effect of the LR from firms' underlying characteristics, such as their degree of compliance. To mitigate these challenges, I employ a matching strategy that identifies, for each certified firm, a never-certified control firm based on a set of firm-level characteristics in the year preceding the LR award, such as size and sector (e.g., Colonnelli et al., 2022; Lagaras, 2023). In addition, I ensure that both certified and control firms participate in public procurement and match them on their access to public procurement in the year preceding the LR award. Given that the requirements for participating in public procurement significantly overlap with those for LR eligibility, this matching strategy allows me to plausibly isolate LR's effects from firms' underlying degree of compliance.

Two key findings show that the LR improves firms' access to public procurement. First, after the LR award, certified firms experience a 4% (21%) increase in the number (value) of public procurement contracts won relative to control firms. These findings suggest that the LR reduces procurement agencies' information processing costs in contract allocation decisions by allowing certified firms to stand out from their peers and gain visibility (Bourveau et al., 2022). Second, given that the LR publicly disseminates information on supplier firms' quality, it reduces information asymmetry with respect to a large set of procurement agencies (Breuer et al., 2018; Sufi, 2009). Consistent with this reasoning, I show that the LR allows certified firms to transact with more public procurement agencies and at greater geographical distances. By examining dynamic effects, I document no evidence of pre-treatment trends, further indicating that improved access to public procurement coincides with the LR award.

The improvement observed in firms' access to public procurement can emerge from two non-mutually exclusive mechanisms: reduced information frictions or increased external monitoring. On the one hand, the coarse nature of the LR scores—which rate firms in terms of stars—simplifies complex and hard-to-access information on firms' compliance by enabling easier rankings and comparisons among supplier firms. Specifically, procurement agencies might rely on LR scores as a signal of supplier firm quality in their contract allocation decisions (Bourveau et al., 2022; Howell, 2020).[3] On the other hand, by providing a public signal of supplier firms' quality, the LR might allow external monitors (e.g., regulators, media) to better scrutinize procurement agencies' allocation decisions (Darendeli et al., 2022; Seregni, 2024). Thus, in response to this increased monitoring, procurement agencies might allocate contracts to certified firms to show that their decisions are consistent with a defensible signal (Tian and Xia, 2021).

Cross-sectional analyses reveal that reduced information frictions are the primary driver of the observed results. Although certified firms on average experience improved access to

[3] Procurement agencies can evaluate bidding firms also through qualitative characteristics. I provide a detailed overview of the Italian public procurement setting in section 2.2.2.

public procurement, the prominence of their LR score substantially influences the extent of this access. Firms with a one-star score do not experience relevant improvements in their access to public procurement. Conversely, the effect is nearly twice as large for those with two stars and three times as large for those with three stars relative to the baseline estimates. Moreover, by examining the characteristics of contracts awarded to certified firms, I show that these firms win a higher share of contracts through direct awards, that have lower transparency requirements and afford greater discretion in supplier firm selection. Notably, contrary to what the external monitoring mechanism would suggest, certified firms do not secure a higher share of contracts through auctions (e.g., open procedures), which involve more external monitoring and stricter control mechanisms.

Next, I investigate the effects of the LR on procurement contract allocation efficiency. Certified firms' improved access to public procurement could imply either the LR's role in enhancing contractual transparency or its potential to inadvertently distort contract allocation decisions. On the one hand, the LR might allow procurement agencies to select superior supplier firms by reducing uncertainty about firms' quality. This benefit can be particularly significant in direct awards where agencies have greater discretion in supplier selection. On the other hand, the LR might facilitate contract allocation to connected firms (Schoenherr, 2019). For instance, procurement agencies might strategically use direct awards to favor connected certified firms, regardless of their ability to effectively execute contracts (Tulli, 2024). Additionally, criminal organizations might exploit seemingly legitimate firms to obtain the LR and profit from lucrative procurement contracts. Because these alternative explanations carry different implications for the LR's ability to signal firm quality and improve allocative efficiency, understanding which of these explanations predominates is relevant from a policy perspective.

To investigate whether the LR improves contract allocation efficiency, I examine whether contracts allocated to certified firms have superior execution performance relative to those allocated to matched controls. I exploit data on contract performance measures, such as delays, cost overruns, and modifications, which are available for a subset of public works contracts. These measures reflect the efficiency of procurement allocation, since increased delays, cost overruns, or contract modifications lead to additional costs for procurement agencies and impose negative externalities on citizens. Consistent with this reasoning, prior literature documents that allocating contracts to criminal or connected firms leads to worse contract execution, resulting in a misuse of public funds (e.g., Ravenda et al., 2020; Schoenherr, 2019).

I find that contracts awarded to certified firms are 2 to 3 percentage points less likely to experience cost overruns, modifications, and delays. However, the aggregate results mask substantial heterogeneity among certified firms. The reduction in cost overruns and modifications occurs primarily in contracts awarded to firms with two-star or three-star scores. For instance, contracts awarded to three-star firms are 13 percentage points less likely to experience cost overruns relative to those awarded to their matched controls. In contrast, contracts awarded to one-star firms have a similar likelihood of cost overruns and modifications as those awarded to control firms. Back-of-the-envelope calculations suggest that allocating public works contracts to three-star recipients instead of one-star recipients resulted in a total estimated saving of EUR 201 million, which exceeds the estimated cost of the program substantially during my sample period (EUR 15.65 million).

Finally, to further alleviate endogeneity concerns stemming from the LR's voluntary adoption, I conduct an aggregate analysis that leverages more exogenous treatment variation due to the differential exposure of local markets to the LR. Specifically, I consider two eligibility thresholds for the LR, which provide the features of a quasi-experiment. To obtain the LR, firms must have sales above EUR 2 million and be included in the Business Register for at least two years. In the spirit of Breuer (2021) and Breuer et al. (2025), these thresholds create variation in the exposure of local markets (defined at the province-industry level) to the introduction of the LR in public procurement, depending on markets' pre-existing structure (i.e., the share of eligible firms). I show that markets with a greater *ex ante* share of eligible firms experience a larger allocation of public procurement contracts after the introduction of the LR in public procurement. Moreover, I find that these markets experience an increase in procurement participation, as shown by a larger number of firms bidding for procurement contracts. This finding suggests that by publicly disclosing information on supplier firms' quality, the LR reduces incumbents' information advantage and ultimately facilitates entry into public procurement (e.g., Breuer, 2021; Breuer et al., 2018).

This paper relates to the literature examining the effects of government transparency initiatives on resource allocation. In the last decade, governments have imposed increased reporting and auditing mandates on private firms, especially in the context of environmental, social and governance (ESG) reporting.[4] However, previous studies

[4]In the United States, the California Transparency in Supply Chains Act requires firms operating in California to disclose how they conduct due diligence to address suppliers' human rights abuses. More recently, the 2023 California Climate Corporate Data Accountability Act requires firms to report their Scope 1, 2 and 3 greenhouse gas emissions. In the European Union, the Corporate Sustainability

find mixed evidence on the net benefits of reporting and auditing mandates (Breuer, 2021; Breuer et al., 2023). This paper explores government certifications as an alternative initiative to improve resource allocation. By reducing information frictions, certifications can help market participants evaluate complex or hard-to-access information (e.g., Bernstein et al., 2023; Bonfim et al., 2023; Bourveau et al., 2022; Howell, 2020). Given privileged access to confidential data, governments have a unique information advantage in evaluating firms' compliance relative to other information intermediaries. Importantly, government certifications impose lower costs on firms and regulatory bodies than mandates. This paper shows that, if coupled with a discrete rating system, government certifications can improve the allocation of public resources.

This paper contributes to the literature examining the effects of transparency in public procurement. Previous studies primarily documented the positive effects of increased publicity of tender offers (e.g., Carril et al., 2022; Coviello and Mariniello, 2014; Lewis-Faupel et al., 2016), government audits (e.g., Colonnelli et al., 2022), or fair pricing requirements (Nathan, 2024). However, recent studies highlight how these initiatives can have unintended consequences on procurement agencies' allocation decisions (Duguay et al., 2023; Gerardino et al., 2017) or firms' bidding incentives (He et al., 2024). This paper indicates that increased transparency on supplier firms' regulatory compliance can be informative to procurement agencies, especially in contracts where they have more discretion in supplier selection. In this respect, this paper complements a related work by Seregni (2024) who observes increased procurement competition after the introduction of beneficial ownership registries in Europe.

Finally, this paper adds to the emerging literature examining the effects of organized crime on the legal economy. Prior literature has primarily examined the negative externalities imposed by organized crime on legitimate firms (e.g., Fenizia and Saggio, 2024; Slutzky and Zeume, 2023). Indeed, criminal firms increase legitimate firms' business costs by using violence and bribery to obtain preferential access to public contracts or better terms from suppliers (e.g., Bianchi et al., 2022; Chircop et al., 2023; Mirenda et al., 2022; Ravenda et al., 2020). This study documents how voluntary disclosure can allow firms to signal their legitimacy in contexts with high criminal infiltration.

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Reporting Directive requires firms to disclose information on the risks, opportunities, and impact of ESG issues. Notably, even if these mandates primarily apply to large private firms, they have substantial spillover effects on SMEs given the additional information requested by their supply-chain partners or lenders.

## 2.2 Institutional background

### 2.2.1 The Legality Rating

Corruption and organized crime have historically imposed costs on the Italian economy, ranging from the misallocation of public funds to reduced competition and access to credit (e.g., Bonaccorsi Di Patti, 2009; Daniele and Dipoppa, 2023; Fenizia and Saggio, 2024). To increase the competitiveness of legitimate firms, the Italian Competition Authority ("ICA"), the equivalent of the U.S. Federal Trade Commission, introduced the LR (*Rating di Legalità*) in 2012. The LR certifies that recipient firms meet the highest standards of legality. To obtain the LR, firms must: (1) have no prior criminal conviction or administrative offense; (2) have no violations of antitrust, tax, unfair commercial practices, workplace safety, anti-corruption, and environmental regulation in the two years preceding the LR request; (3) respect the legal threshold for cash-based transactions; and (4) have managers without criminal convictions and ties to criminal organizations. Additionally, to prevent criminal organizations from acquiring the LR through fictitious shell companies (Acconcia et al., 2021), firms must have been included in the Italian Business Register for at least two years and have revenues higher than EUR 2 million in the year preceding the request of the LR.

The LR rates recipient firms using a score ranging from one star ($\star$) to three stars ($\star\star\star$). To receive higher LR scores, firms must implement additional compliance mechanisms to prevent corruption and criminal infiltration. These include: (1) adhering to anti-mafia protocols; (2) implementing a system of corporate compliance (e.g., a compliance supervisory body); (3) adopting organizational structures to prevent corruption; (4) adopting organizational structures to promote Corporate Social Responsibility; (5) joining the province-level list of companies and suppliers without ties to organized crime ("White List"); (6) ensuring traceability for all payments, even those below the legal threshold; (7) adhering to ethical self-regulation codes promoted by trade associations. Meeting any of these conditions increases the baseline LR by one plus sign (+)—three plus signs equal one additional star. No other certifications use the same eligibility criteria.

Firms apply for the LR by submitting all required documentation electronically.[5] To verify the veracity of the information provided by applicants, the ICA consults various sources, such as the Italian Anti-corruption Authority ("ANAC "), law enforcement agencies, the

[5]Furthermore, the ICA has no regional offices, thereby reducing the likelihood that certified firms have ties with ICA officials.

Ministry of the Interior, and the Ministry of Justice. Verification lasts about 60 days, and if successful, the ICA awards the LR to applicants. The LR lasts two years, after which recipient firms can freely renew it if they still meet the award requirements. During these two years, the ICA still monitors recipient firms' compliance with LR eligibility criteria. For instance, the ICA revoked the LR for 72 firms in 2020. The list of current LR recipients is publicly available on the ICA website. Furthermore, several firms advertise the LR award on their websites.[6]

The LR aims to increase the competitiveness of legitimate firms by providing a public signal of their compliance. The Italian Government has encouraged banks to use the LR for assessing borrowers' credit risk and speeding up loan approval times.[7] Furthermore, when awarding public subsidies (e.g., direct transfers, tax cuts) to firms, public administrations can grant preferential access to LR recipients. In this regard, survey and empirical evidence show that firms primarily seek the LR to enhance their reputation (Ginesti et al., 2018; ICA, 2022; La Rosa and Bernini, 2022). Secondary motives for LR application include potential improvements in access to credit and public funding.

Despite the LR's apparent benefits, however, only 11,220 firms obtained it between 2013 and 2020. This limited uptake is common to other government programs (e.g., Bonfim et al., 2023; Custodio et al., 2021) and stems from limited awareness of the LR and poor understanding of its benefits (ICA, 2022). As such, Panel (a) of Figure 2.1 documents an upward trend in LR uptake, suggesting that more firms became aware of the LR's availability and benefits over my sample period. Panel (b) reports the distribution of LR scores. Only 6% of LR recipients achieve the highest score (three stars), while more than half receive the minimum score of one star. Panel (c) shows the wide geographical variation of LR recipients across Italian provinces. The density of LR uptake is highest in Northern Italy, the most industrialized area of the country. However, the percentage of LR recipients is also high in some areas of Southern Italy, especially in regions with a strong presence of criminal organizations (e.g., Campania or Puglia).

The high LR uptake in Southern Italy suggests its prominence in regions with greater corruption and criminal infiltration. While this might indicate legitimate firms obtaining

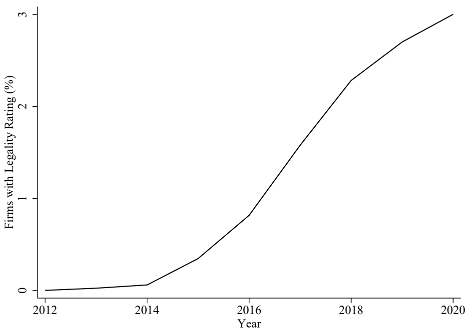
[6] The list of current LR recipients is available at the following link: https://www.agcm.it/competenze/rating-di-legalita/elenco-rating. Figure A.1 provides some examples of companies' disclosures of the LR award.

[7] Banks must also report yearly to the Bank of Italy how they considered the LR in their lending decisions.

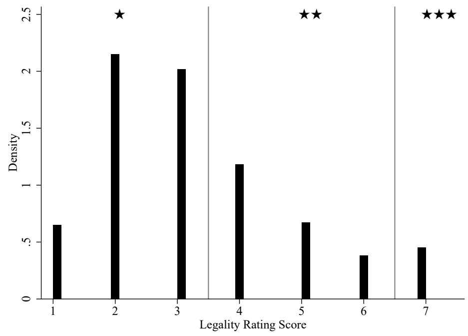
the LR to distinguish themselves, it might also suggest strategic exploitation by criminal firms. For example, Mafia organizations might obtain the LR through seemingly legitimate firms to secure lucrative procurement contracts. In this respect, a crucial caveat applies: LR awards depend on both firm compliance and external enforcement efficacy (Colonnelli et al., 2022). In this context, firms with the strongest Mafia ties might more successfully elude enforcement and obtain the LR.

**Figure 2.1:** Legality Rating Recipients

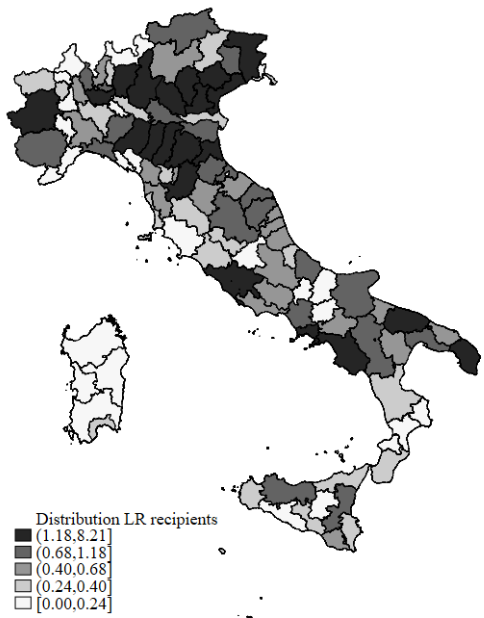
This figure provides descriptive evidence on Legality Rating recipients. Panel A plots the yearly percentage of Legality Rating recipients over total sample firms. Panel B plots the distribution of Legality Rating scores. Panel C shows the distribution of Legality Rating recipients across Italian provinces.




**(a)** LR Take-up




**(b)** Distribution of Legality Rating Scores




**(c)** Geographic Distribution

### 2.2.2 Public procurement in Italy

The Italian public procurement system is highly decentralized. Indeed, local procurement agencies (e.g., regions, municipalities, hospitals) award most procurement contracts. For each procurement contract, the relevant agency designates a procurement official (*Responsabile Unico del Procedimento*), typically chosen from a pool of senior-level bureaucrats. Procurement officials have substantial discretion in designing the contracting process within the boundaries set by national and European regulations (Decarolis et al., 2021). The primary public procurement regulation is the Public Procurement Code (Legislative Decree N. 50 of 18 April 2016), which transposed the EU procurement Directives (24/2014 and 25/2014) into national legislation.[8] Thus, the Italian procurement regulation largely resembles that of other European countries.

Procurement officials can award contracts through public competitive auctions or direct awards. In public competitive auctions, the procurement officials must disclose a tender notice detailing contract conditions (e.g., type of good/service to deliver, reserve price), after which firms can submit their sealed bids. The procedures for selecting bidding firms vary within these auctions. In open procedures, any qualified firm can participate. In negotiated procedures, officials pre-select a number of bidding firms and negotiate contract conditions with them. In public competitive auctions, officials award contracts based on one of two criteria. Under the lowest price criterion, the contract is awarded to the firm that offers the lowest price. Alternatively, under the scoring rule criterion, procurement officials select the winning firm based on a range of parameters beyond the price. These parameters include hard and soft elements, such as the expected quality of the work or service to be delivered (e.g., Baltrunaite et al., 2021; Decarolis et al., 2021).

Aside from auctions, officials can allocate contracts through direct awards, where they directly source products and services from selected contractors. After requesting quotes from one or more potential contractors, procurement officials finalize the purchase, and disclose information on the different quotes received (Gerardino et al., 2017). Due to the higher subjectivity in the choice of contractors, direct awards are prone to more favoritism and corruption than public competitive auctions. Hence, to promote transparency and competitiveness, international organizations recommend public competitive auctions (e.g., OECD, 2015). In this respect, Italian procurement regulation allows direct awards only for contracts below a given monetary threshold or in specific circumstances.

[8] During my sample period, the Public Procurement Code underwent two minor revisions in April 2017 and June 2019.

To participate in public procurement, firms must meet various technical and financial requirements, such as the previous award of similar contracts, adequate revenue relative to the contract award, and the availability of suitable equipment and labor. Importantly, firms must also fulfill specific compliance requirements, which largely overlap with those for LR eligibility.[9] To verify bidding firms' compliance, procurement agencies must check approximately ten different sources.

Given that the requirements for participating in public procurement largely overlap with those for obtaining the LR, the availability of the LR might reduce the cost of searching, acquiring, and evaluating information on bidding firms' compliance. Furthermore, the LR might provide a confirmatory signal on the quality of bidding firms. In this respect, the Procurement Code allowed procurement officials to include the LR among the non-mandatory, additional parameters (*criteri premiali*) when awarding contracts using the scoring rule criterion in public competitive auctions.[10] Procurement officials thus have significant discretion on: i) whether to include the LR in their assessment of potential bids, and ii) the relative weight assigned to the LR in bid evaluation.[11]

## 2.3 Conceptual Framework

The onset of a transaction involves substantial information asymmetry between customers (e.g., public procurement agencies) and supplier firms. While supplier firms are aware of their quality, they might struggle to credibily communicate this information to their potential customers (Stahl and Strausz, 2017). Besides, given the lack of complete contracts, customers cannot verify or contract on supplier firms' future activities, e.g., whether these firms will deliver high-quality goods on time and without extra costs (Giese et al., 2024). By increasing adverse selection problems, this information asymmetry can,

[9]Specifically, firms must i) have no criminal convictions, ii) have managers without criminal convictions and ties to organized crime, iii) have no sanctions for noncompliance with tax, workplace safety, and environmental regulation, iv) have no ongoing bankruptcy proceedings, v) have no evidence of anti-competitive behavior. For more details, see Repubblica Italiana (2016).

[10]Additionally, when awarding supply or services contracts to LR recipients, procurement officials might reduce the amount held in escrow. However, survey evidence shows that this practice is not widespread (ICA, 2022).

[11]I emphasize the distinction between public *procurement* (i.e., public sector demand for goods and service) and public *subsidies* (i.e., public support to firms through direct transfer or tax cuts). LR recipients can have preferential access to public subsidies, but not to public procurement. Notably, the Anti-corruption Authority explicitly prohibits procurement officials from including the LR among the mandatory bids participation criteria. Since only Italian firms with sales above EUR 2 million can obtain the LR, including the LR among the mandatory criteria in the selection of bidders would automatically exclude some firms, thus infringing the EU Procurement Directives' principles of fair competition.

in turn, constrain resource allocation to high-quality firms. As a result, high-quality firms undertake actions to distinguish themselves from low-quality firms (Jensen and Meckling, 1976; Kausar et al., 2016). One example of such actions is delegating the assessment of their quality to a trusted third party through a certification, such as the LR (Bonetti and Ormazabal, 2023).

The decision to obtain a certification depends on firms' *ex ante* assessment of the net benefits associated with it. On the one hand, certifications benefit high-quality firms by reducing information asymmetry around their quality. Indeed, certifications create a separating equilibrium as high-quality firms commit to providing a signal that is difficult for low-quality firms to mimic (Guasch and Weiss, 1981; Kausar et al., 2016). On the other hand, the certification process imposes some costs on firms, e.g., management time spent overseeing the application and certifiers' fees. Thus, firms will obtain a certification only if its benefits (i.e., reducing information asymmetry) outweigh its costs.

Certifications can improve transparency in customer-supplier transactions by reducing customers' information processing costs. Even if the information underlying the certification is publicly accessible, customers might face significant costs in gathering, verifying and evaluating such information (Christensen et al., 2021; Kim and Davis, 2016). By aggregating multidimensional information into a single, coarse signal, certifications might allow supplier firms to stand out from their peers and gain customers' attention (Bernstein et al., 2023; Bourveau et al., 2022; Howell, 2020). Furthermore, certifications publicly disseminate and/or harden relationship-specific information on supplier firms' quality (Breuer et al., 2018). Therefore, by reducing incumbent customers' information advantage (e.g., knowledge of supplier firms' quality acquired over repeated interactions), certifications allow supplier firms to transact with more customers.

Nevertheless, the positive effects of certifications depend on their prominence and credibility, which are not guaranteed. Given concurrent sources of information, certifications might lack sufficient prominence to influence customers' decisions. Besides, certifications might fail to convey a credible signal of firms' quality for two interrelated reasons. First, certifiers' business model might introduce a systematic bias in their certifications. For instance, for-profit certifiers might issue overly positive certifications to secure future revenues (e.g., Becker and Milbourn, 2011; Ettredge et al., 2014). While government certifiers lack direct financial motives (Dranove and Jin, 2010), weak incentives and corruption in government organizations might lead to inadequate applicant screening (e.g., Olken, 2007; Prendergast, 2007). For instance, Ho (2012) finds that government officials in San Diego award the highest sanitation grades to nearly all restaurants. Therefore, customers might mistrust government certifications, especially in environments with high government inefficiency and corruption (Colonnelli et al., 2024; Olken, 2007). Second, the design of certification programs creates strategic incentives for low-quality applicants to obtain the certifications (e.g., Daniele and Dipoppa, 2023; Forbes et al., 2015). For instance, Luca and Zervas (2016) document that low-quality restaurants are more likely to leave fake reviews on Yelp platform to improve their reputation. Therefore, if applicant screening is inadequate, these strategic incentives would lead to a decrease in the quality of certified firms, and ultimately to a loss of trust in the certification. In light of these arguments, whether the LR improves firms' access to public procurement and the efficiency of contract allocation remains an open empirical question.

## 2.4 Empirical Strategy

### 2.4.1 Data

I obtain the list of LR recipients from the ICA for each year from 2013 to 2020. Data contains the name of the firm receiving the LR, its tax identifier, the rating score received, and the date of the LR award. Next, I collect accounting data for Italian firms for the period 2011-2020 from the AIDA database of Bureau Van Dijk.

I obtain data on public procurement contracts from the Italian Anti-corruption Authority (e.g., Decarolis et al., 2021; Fenizia and Saggio, 2024). In addition to supervising public procurement agencies, ANAC aggregates and discloses information on the contracts awarded by individual public procurement agencies. The dataset offers an extensive coverage of public procurement contracts awarded in Italy during my sample period (Cappelletti et al., 2024). For each contract, I observe a unique contract identifier, the year of the award, the contract value, the tax identifiers of the winning firm and the procurement agency awarding the contract, the awarding procedure, and the number of bidding firms. Furthermore, for the subset of public works contracts, data also includes measures of contract execution performance (e.g., delays, cost overruns, and modifications). I use these data to (1) identify firms that participate in public procurement, (2) aggregate procurement information at the firm-year level, and (3) compute measures of contract performance for the subset of public works contracts.

A shortcoming of the ANAC data is its focus solely on firms that win public procurement contracts, omitting information on all participating firms. To mitigate this shortcoming, I complement ANAC data with a novel public dataset of public procurement contracts.[12] Unlike ANAC, this dataset allows me to observe every firm involved in the bidding process, irrespective of whether they win the contract. Using these data, I compute my market-level measure of procurement participation based on firms' bids for procurement contracts.

To construct my sample, I begin by merging the list of LR recipients with accounting data from AIDA. Next, I combine this dataset with firm-year public procurement information from my procurement datasets. Because the listing status can affect firms' compliance requirements, I exclude 509 listed firms. The resulting sample consists of 274,191 unique firms, including 10,614 LR recipients. When focusing on firms participating in public procurement during the sample period, the sample size reduces to 44,221 firms, of which 5,172 are LR recipients.

Table A.1 reports key characteristics of LR recipients in the year preceding their award relative to non-certified firms. On average, LR recipients are larger, with average sales of EUR 23 million and 116 employees, compared to EUR 11 million and 31 employees for non-certified firms. This larger size results in a higher share of LR recipients subject to extensive reporting and auditing requirements. Furthermore, LR recipients participate more in public procurement, with a 35% average likelihood of securing a contract in a given year, relative to a 6% average for non-certified firms. Finally, LR recipients are more likely to operate in areas and sectors with a high presence of organized crime.

### 2.4.2 Matching Certified firms to Control Firms

Given the voluntary nature of the LR, the primary challenge in estimating its effects is selection bias. Since certified firms voluntarily obtain the LR, they might differ from non-certified firms along a set of observable and unobservable characteristics. In this case, certified firms are, on average, larger than non-certified firms and are clustered within some specific sectors. To mitigate selection bias from observable characteristics, I complement a fixed effects specification with a matching estimator. This empirical strategy, which prior literature has largely used in the presence of non-random treatment (e.g., Colonnelli et al., 2022; Graham et al., 2023; Lagaras, 2023), allows me to identify a set of control firms based on a set of observable firm-level characteristics.

I use a two-step matching approach to match certified firms to potential control firms. In

[12] The dataset is publicly available on Kaggle at the following link https://www.kaggle.com/datasets/sebastianomm/italian-public-tender-data-as-at-q1-2021

the first step, for each firm obtaining the LR in year $t$, I select a potential control firm that (1) never obtained the LR during my sample period, (2) operates in the same two-digit sector, (3) participates in public procurement, (4) is in the same decile for the number of contracts won in the year before the LR award, and (5) is in the same quintile for size, profitability, and employment in the year before the LR award. Matching on existing access to procurement provides a key benefit. Because the requirements to participate in public procurement significantly overlap with those for LR eligibility, this matching strategy allows me to plausibly isolate the effect of the certification from the underlying degree of compliance. In the second step, if there are multiple potential controls for a certified firm, I select the control firm with the closest propensity score. To do so, I estimate a linear probability model using a set of firm-level characteristics, such as leverage, growth, cash, and asset tangibility.

At the end of the matching procedure, I am able to match 3,772 certified firms with a unique control firm.[13] Table 2.1 presents matching statistics for certified and control firms in the year before the award. Columns (1) to (3) compare certified firms with non-certified firms in the year before the award. Columns (4) to (6) restrict the analysis to matched certified and control firms. After the matching procedure, I observe no significant difference in access to public procurement, size, employment, profitability, and growth between certified and control firms.

## 2.5 LR and Firms' Access to Public Procurement

To investigate the effects of the LR on firms' access to public procurement, I use four different outcome variables: the number and value of public procurement contracts won, the number of transacting public procurement agencies, and the maximum geographical distance to a procurement agency. By reducing information processing costs in contract allocation decisions, the LR award should increase the volume (i.e., number and total award value) of public procurement contracts allocated to certified firms. Furthermore, by publicly disseminating information on the quality of certified firms, the LR might reduce informational distance between firms and a larger set of customers (Breuer et al., 2018; Sufi, 2009). Therefore, the LR should allow certified firms to transact with (1) more procurement agencies, and (2) more relatively uninformed procurement agencies, such as

[13] The reduction in the number of matched observation is common in studies complementing a difference-in-differences design with a matching stategy (e.g., Colonnelli and Prem, 2022; Lagaras, 2023)

**Table 2.1:** Matching Statistics

This table reports matching statistics for certified firms and their matched controls. Columns (1) and (2) present the average and standard deviation for certified firms and all non-certified sample firms. Columns (4) and (5) present the average and standard deviation for certified firms and their matched controls. Column 3 ([7]) presents the average difference between columns 1 and 2 ([5 and 6]), and the significance level of the difference. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Before Matching | | | After Matching | | |
| | Certified | Non-certified | Diff. | Certified | Controls | Diff. |
| Any Contract | 0.33 | 0.06 | 0.28*** | 0.63 | 0.63 | 0.00 |
| | (0.47) | (0.23) | | (0.48) | (0.48) | |
| Contracts Won (Number) | 2.74 | 0.32 | 2.42*** | 5.18 | 6.01 | -0.83 |
| | (11.12) | (6.88) | | (16.07) | (32.06) | |
| Contract Value (EUR Thousands) | 4,535.64 | 388.30 | 4,147.34*** | 8,327.96 | 8,143.35 | 184.61 |
| | (39,745.16) | (17,938.60) | | (58,118.89) | (71,812.72) | |
| % Competitive (Number) | 0.25 | 0.04 | 0.21*** | 0.48 | 0.48 | -0.00 |
| | (0.40) | (0.19) | | (0.45) | (0.45) | |
| % Competitive (Value) | 0.26 | 0.04 | 0.22*** | 0.49 | 0.49 | -0.00 |
| | (0.41) | (0.19) | | (0.46) | (0.46) | |
| % Non-competitive (Number) | 0.08 | 0.02 | 0.06*** | 0.15 | 0.15 | 0.00 |
| | (0.22) | (0.12) | | (0.28) | (0.28) | |
| % Non-competitive (Value) | 0.07 | 0.02 | 0.06*** | 0.14 | 0.14 | 0.00 |
| | (0.22) | (0.12) | | (0.28) | (0.28) | |
| Size | 8.87 | 7.89 | 0.98*** | 8.92 | 8.96 | -0.04 |
| | (1.21) | (1.54) | | (1.25) | (1.40) | |
| ROA | 0.08 | 0.10 | -0.02 | 0.06 | 0.07 | -0.00 |
| | (0.45) | (25.09) | | (0.10) | (0.11) | |
| Growth | 0.56 | 2.89 | -2.32 | 0.79 | 0.15 | 0.64 |
| | (24.58) | (442.54) | | (39.20) | (1.22) | |
| Employees | 3.54 | 2.28 | 1.25*** | 3.77 | 3.75 | 0.02 |
| | (1.20) | (1.41) | | (1.24) | (1.26) | |
| Mafia-sector | 0.06 | 0.05 | 0.01*** | 0.07 | 0.07 | -0.00 |
| | (0.24) | (0.23) | | (0.26) | (0.26) | |

those located at greater geographical distances (Bernard et al., 2020; Giroud, 2013).[14]

Specifically, I estimate the following fixed effects specification on the sample of certified and control firms:

$$y_{i,t} = \beta_0 + \beta_1 Certified_{i,t-1} + \gamma X_{i,t-1} + \alpha_i + \alpha_t + \epsilon_{i,t} \tag{2.1}$$

where $y$ is the outcome variable (e.g., number of contracts won) for firm $i$ in year $t$. *Certification* is an indicator variable taking values of one if a firm has the LR for more than ninety days (i.e., one quarter) in a year, and zero otherwise.[15] $X$ refers to a set of time-varying controls to absorb firm-specific characteristics. Following previous literature (e.g., Goldman et al., 2013; Tahoun, 2014), I control for a set of variables related to the allocation of public procurement contracts, i.e., size (*Size*), profitability (*ROA*), profit margin (*Margin*), growth (*Growth*), asset tangibility (*PPE*), and number of employees (*Employees*). In all specifications, I include firm fixed effects ($\alpha_i$) to control for time-invariant firm characteristics and year fixed effects ($\alpha_t$) for year-level shocks. I cluster standard errors at the firm-level. The coefficient of interest $\beta_1$ captures the change in my outcome variable (e.g., number of contracts won) for certified firms after the LR relative to control firms, taking into account time-invariant firm characteristics and year-level shocks.

Panel A of Table 2.2 reports summary statistics for the variables used in the firm-level analysis. Sample firms win a median of one procurement contract per year, with a median total contract value of EUR 172,000, and transact with an average of three procurement agencies per year. Consistent with previous studies on Italian private firms (e.g., Bianchi et al., 2022; Chircop et al., 2023), sample firms are, on average, small- to mid-sized. Indeed, the median firm has assets of EUR 6 million.

[14]For each year, I compute the geographical distance between a firm and all of its transacting procurement agencies. To calculate geographic distance, I obtain geographical coordinates for the municipalities where firms and procurement agencies are located and calculate the linear distance (in kilometers) between them. Because ZIP codes can include multiple municipalities in Italy, this approach ensures a more precise calculation of the distance.

[15]Using the availability of the LR at year-end might not fully capture the effect of the LR. Because the ICA continuously assigns the LR throughout the year, some firms would be classified as having the certification in a given year even if they obtained it later (e.g., December). Nonetheless, my results are robust to using this alternative definition.

**Table 2.2:** Summary Statistics

This table reports estimated summary statistics for the different samples used in the empirical analysis. Panel A reports summary statistics for the variables used in the firm-level analysis, which includes certified firms and their matched controls. Panel B reports summary statistics for variables used in the contract-level analysis, which includes public works contracts allocated to certified firms and their matched controls. Panel C reports summary statistics for the variables used in the market-level analysis, where the level of observation is the province-industry-year.

| | Panel A: Matched Firm-level sample | | | | | |
|---|---|---|---|---|---|---|
| | N | Mean | Std. Dev | p25 | p50 | p75 |
| Contracts Won (Number) | 56,261 | 5.628 | 28.094 | 0.000 | 1.000 | 4.000 |
| Contracts Won (Log) | 56,261 | 0.994 | 1.070 | 0.000 | 0.693 | 1.609 |
| Contract Value (EUR Thousands) | 56,261 | 8,156.498 | 61,408.766 | 0.000 | 172.610 | 1,874.051 |
| Contract Value (Log) | 56,261 | 8.585 | 7.043 | 0.000 | 12.059 | 14.444 |
| No. Agencies (Number) | 56,261 | 2.975 | 8.081 | 0.000 | 1.000 | 3.000 |
| No. Agencies (Log) | 56,261 | 0.842 | 0.884 | 0.000 | 0.693 | 1.386 |
| Distance (Number) | 56,261 | 148.611 | 235.084 | 0.000 | 19.654 | 209.500 |
| Distance (Log) | 56,261 | 2.723 | 2.651 | 0.000 | 3.028 | 5.349 |
| Size | 56,261 | 8.876 | 1.392 | 7.931 | 8.635 | 9.577 |
| ROA | 56,261 | 0.062 | 0.330 | 0.018 | 0.040 | 0.078 |
| Margin | 56,261 | 0.018 | 1.850 | 0.017 | 0.039 | 0.073 |
| Growth | 56,261 | 0.549 | 26.898 | -0.072 | 0.039 | 0.181 |
| Employees | 56,261 | 3.682 | 1.284 | 2.833 | 3.497 | 4.382 |
| PPE | 56,261 | 0.142 | 0.156 | 0.025 | 0.083 | 0.208 |
| Assets (EUR Thousands) | 56,261 | 44,790.144 | 707,101.457 | 2,781.000 | 5,627.000 | 14,427.000 |

| | Panel B: Contract-level sample | | | | | |
|---|---|---|---|---|---|---|
| | N | Mean | Std. Dev | p25 | p50 | p75 |
| Cost Overrun | 27,107 | 0.528 | 0.499 | 0.000 | 1.000 | 1.000 |
| % Cost Overrun | 27,107 | 0.056 | 0.167 | -0.001 | 0.000 | 0.111 |
| Modification | 73,062 | 0.229 | 0.420 | 0.000 | 0.000 | 0.000 |
| Modifications (Number) | 73,062 | 0.332 | 0.781 | 0.000 | 0.000 | 0.000 |
| Modification (Log) | 73,062 | 0.194 | 0.380 | 0.000 | 0.000 | 0.000 |
| Delay | 34,702 | 0.582 | 0.493 | 0.000 | 1.000 | 1.000 |
| Days of Delay | 34,702 | 2.614 | 1,107.922 | -1.000 | 23.000 | 122.000 |
| Days of Delay (Asinh) | 34,702 | 2.120 | 3.965 | -0.881 | 3.829 | 5.497 |
| Reserve Price (EUR Thousands) | 73,062 | 1,254.579 | 17,091.513 | 101.328 | 221.970 | 619.305 |
| Reserve Price (Log) | 73,062 | 12.552 | 1.349 | 11.526 | 12.310 | 13.336 |

| | Panel C: Market-level Sample | | | | | |
|---|---|---|---|---|---|---|
| | N | Mean | Std. Dev | p25 | p50 | p75 |
| Firms (Number) | 69,470 | 39.440 | 155.168 | 3.000 | 8.000 | 27.000 |
| Share Treated | 69,470 | 0.449 | 0.280 | 0.273 | 0.455 | 0.605 |
| Average Contracts Won | 69,470 | 0.564 | 3.595 | 0.000 | 0.000 | 0.333 |
| % Bidding Firms | 69,470 | 0.088 | 0.184 | 0.000 | 0.000 | 0.091 |

### 2.5.1 Results

#### 2.5.1.1 Main Effect

I begin my empirical analysis by examining whether the LR affects the volume of procurement contracts allocated to certified firms. Panel A of Table 2.3 reports the results. In columns (1) and (2), I document that certified firms win more procurement contracts after obtaining the LR. The estimated coefficient ($\beta_1$) indicates a relative increase in the number of procurement contracts won by about 4% after the LR award. In columns (3) and (4), I explore the effect of the LR on the total value of procurement contracts won. The positive and significant coefficients show that the LR increases the total value of procurement contracts awarded by 21%. These results suggest that the LR reduces procurement agencies' information processing costs in contract allocation decisions by allowing certified firms to stand out from their peers and gain visibility (Bourveau et al., 2022). Given these reduced information processing costs, certified firms experience an increase in the number and value of procurement contracts won.

Next, in Panel B of Table 2.3, I investigate how the LR affects informational distance between firms and procurement agencies. By publicly disseminating information on firms' quality, the LR should allow certified firms to transact with more public procurement agencies. Consistent with this reasoning, in columns (1) and (2), I show that the number of transacting procurement agencies increases after the LR award. The estimated coefficient in column (2) indicates a 3% relative increase in the number of transacting procurement agencies. Furthermore, this reduced informational distance should allow certified firms to transact more with relatively uninformed procurement agencies. Accordingly, in columns (3) and (4), I find that, on average, the LR increases certified firms' distance from their farthest procurement agency by 9%.

Collectively, the results in Table 2.3 document that the LR improves firms' access to public procurement. However, the internal validity of these findings relies on the assumption that if the LR is absent, the differences in my outcome variables are likely to remain constant between certified and control firms. To investigate the validity of this assumption in my setting, I estimate a dynamic version of the model in Equation (2.1) by introducing relative time indicator variables up to four years before and after the LR award. Figure 2.2 plots the point estimates for every indicator variable in my dynamic model and the 95% confidence interval for specifications with my main outcome variables.[16] Panels (a)

[16]Recent studies show that two-way fixed effects models in frameworks with staggered treatments are likely to be biased in the presence of treatment effect heterogeneity (e.g., Baker et al., 2022; Callaway and

## **Table 2.3:** Firm-level Access to Public Procurement

This table reports estimated coefficients for Equation (2.1). In Panel A, *Contracts Won* is the natural logarithm of one plus the number of public procurement contracts won. *Contract Value* is the natural logarithm of the value of procurement contracts won. In Panel B, *No. Agencies* is the natural logarithm of one plus the number of transacting procurement agencies. *Distance* is the natural logarithm of one plus the kilometer distance between a firm and its farthest public procurement agency relation. *Certified* is an indicator taking values of one if a firm had the Legality Rating for more than ninety days in $t-1$, and zero otherwise. *Size* is the natural logarithm of total assets in $t-1$. *Margin* is the ratio of EBIT to sales in $t-1$. *ROA* is the ratio of EBIT to lagged total assets in $t-1$. *Growth* is the sales growth rate from $t-2$ to $t-1$. *PPE* is the ratio of property, plant and equipment to total assets in $t-1$. *Employees* is the natural logarithm of the number of employees in $t-1$. Standard errors (in parentheses) are clustered by firm. All estimations include firm and year fixed effects. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | Panel A: Procurement Volume | | | |
|---|---|---|---|---|
| | (1) Contracts Won | (2) Contracts Won | (3) Contract Value | (4) Contract Value |
| Certified | 0.050*** | 0.038*** | 0.304*** | 0.212** |
| | (0.011) | (0.011) | (0.092) | (0.091) |
| Size | | 0.131*** | | 1.012*** |
| | | (0.013) | | (0.096) |
| Margin | | -0.000 | | 0.007 |
| | | (0.001) | | (0.011) |
| ROA | | 0.014 | | 0.006 |
| | | (0.014) | | (0.056) |
| Growth | | -0.000* | | -0.001 |
| | | (0.000) | | (0.001) |
| PPE | | 0.017 | | -0.013 |
| | | (0.054) | | (0.466) |
| Employees | | 0.122*** | | 0.951*** |
| | | (0.011) | | (0.086) |
| Firm FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.7482 | 0.7533 | 0.5258 | 0.5328 |
| Observations | 56,261 | 56,261 | 56,261 | 56,261 |

| | Panel B: Informational Distance | | | |
|---|---|---|---|---|
| | (1) No. Agencies | (2) No. Agencies | (3) Distance | (4) Distance |
| Certified | 0.036*** | 0.027*** | 0.115*** | 0.086** |
| | (0.009) | (0.009) | (0.035) | (0.035) |
| Size | | 0.101*** | | 0.354*** |
| | | (0.010) | | (0.036) |
| Margin | | -0.000 | | 0.003 |
| | | (0.001) | | (0.004) |
| PPE | | 0.017 | | -0.044 |
| | | (0.043) | | (0.171) |
| Growth | | -0.000 | | -0.000 |
| | | (0.000) | | (0.000) |
| Employees | | 0.099*** | | 0.283*** |
| | | (0.009) | | (0.032) |
| ROA | | 0.005 | | 0.004 |
| | | (0.007) | | (0.018) |
| Firm FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.7555 | 0.7601 | 0.5315 | 0.5367 |
| Observations | 56,261 | 56,261 | 56,261 | 56,261 |

to (d) in Figure 2.2 do not show evidence of pre-trends for any of my outcome variables. There is no statistical difference in my outcome variables between certified and control firms before the LR award. The effect of the LR on firms' access to public procurement materializes in the year of the award (year 0), and stabilizes in subsequent years (years +1 to +4).[17]

#### 2.5.1.2 Endogeneity Concerns and Robustness Tests

My matching strategy aims to mitigate selection concerns based on observable characteristics. A remaining concern is that unobservable characteristics might drive both firms' decision to obtain the LR and their subsequent improvement in access to public procurement. For instance, firms that obtain the LR might decide to become more transparent overall (Samuels, 2021). Thus, this increased transparency can reduce information asymmetry with procurement agencies and improve firms' access to public procurement. To mitigate this concern, I manually collect information on a sample of firms that applied for the LR unsuccessfully. The underlying intuition is that these firms could act as counterfactuals for certified firms if they did not obtain the LR. Unfortunately, due to data limitations, this information is available only for a small subset of firms and only for the 2021 applicant cohort.

Following Lagaras (2023), I identify for each of these rejected firm a control firm that never obtained the rating using the matching procedure outlined in Section 2.4.2. I am able to match 41 rejected firms with a unique control firm. I then aggregate procurement information at the firm-quarter level and estimate Equation (2.1) on the set of rejected firms and their matched controls.[18] Table A.3 reports the results. I find that rejected

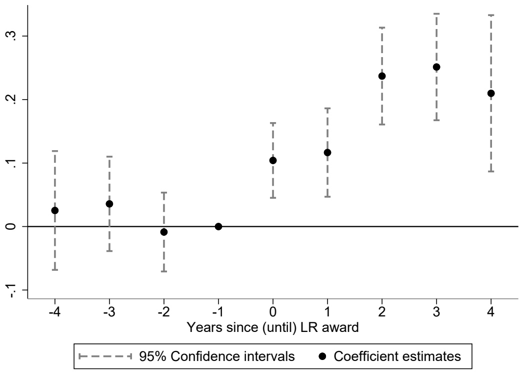
Sant'Anna, 2021). Because treatment effects might vary across groups and over time, this heterogeneity might introduce bias, especially when already treated units act as control for treated units. Therefore, the average treatment effects might have negative signs. However, my empirical strategy identifies, for each certified firm, a never-certified control firm, ultimately avoiding the issue of negative weights. Additionally, when examining dynamic effects, I further mitigate this concern by restricting my analysis to certified firms having the LR for at least four years with their matched controls. Therefore, as illustrated in Colonnelli and Prem (2022) and Lagaras (2023), my empirical strategy resembles a stacked cohort empirical design in the spirit of Cengiz et al. (2019).

[17] In Table A.2, I investigate whether, by improving firms' access to public procurement, the LR also affects certified firms' dynamics. Previous studies document that winning firms often increase their employment to meet the increased demand for their products (e.g., Ferraz et al., 2015). Consistent with this, I find that certified firms experience, on average, a 2% increase in the number of employees after the LR award.

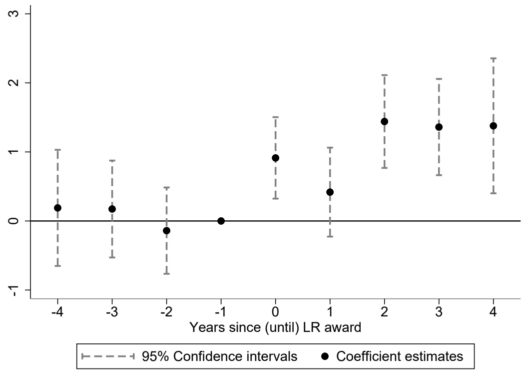
[18] My firm-level data spans up to 2020, while my procurement data extends to 2022. Consequently, I match applicants rejected in 2021 with control firms based on their observable characteristics in 2020. Given the small sample size, I use a quarterly specification to ensure sufficient observations for regression estimation. However, also univariate tests show that rejected firms do not experience improved access to

**Figure 2.2:** Access to public procurement: Firm-level analysis

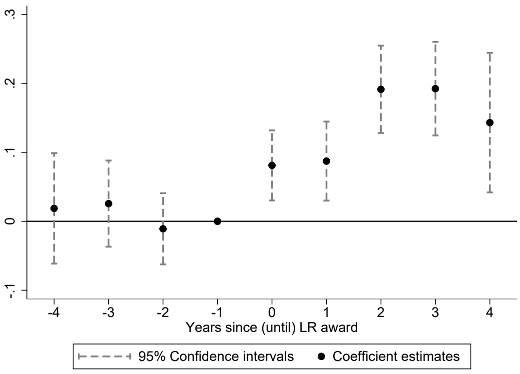
This figure reports the dynamic coefficients and 95% confidence interval obtained from estimating Equation (2.1) with indicators for every year relative to the LR adoption date excluding the minus-one indicator variable. The sample includes certified firms having the LR for four years and their matched controls. In Panel A, *Contracts Won* is the natural logarithm of one plus the number of public procurement contracts won. In Panel B, *Contract Value* is the natural logarithm of one plus the total value of public procurement contracts won. In Panel C, *No. Agencies* is the natural logarithm of one plus the number of transacting procurement agencies. In Panel D, *Distance* is the natural logarithm of one plus the kilometer distance between a firm and its farthest public procurement agency relation. The regressions include controls, firm and year fixed effects. Standard errors are clustered at the firm level.



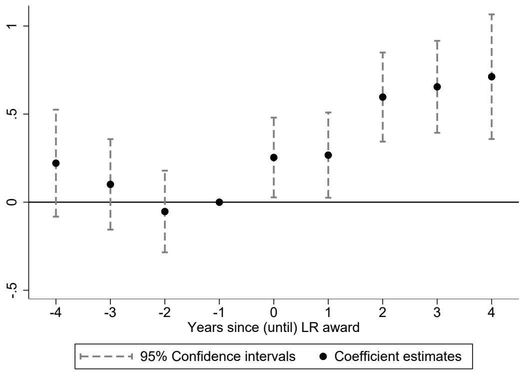

**(a)** Contracts Won




**(b)** Contract Value




**(c)** No. Agencies




**(d)** Distance

firms do not experience improved access to public procurement after their unsuccessful application. The coefficient estimates are not statistically significant, and often have opposite signs relative to my baseline estimates.

Subsequently, I assess the robustness of my findings using various alternative specifications. First, I restrict the matching procedure described in Section 2.4.2 to firms operating in the same region. This restriction reduces the number of matched certified firms to 1,392. As shown in Figure A.2, the coefficient estimates align with my baseline analysis but are much noisier, likely due to lower match quality. Second, to control for differences in characteristics between certified and non-certified firms, I check the sensitivity of my result using an entropy-balanced method (Hainmueller, 2012), which ensures a covariate balance between certified and non-certified firms.[19] Table A.4 reports the results from estimating Equation (2.1) on an entropy-balanced sample. The coefficient for *Certified* is positive and statistically significant across all specifications, with a magnitude similar to that of the baseline tests. Third, Figure A.3 reports coefficient estimates obtained from estimating Equation (2.1) with a fixed-effects Poisson specification (Cohn et al., 2022).[20] Fourth, I more directly attempt to control for unobserved variation by triangulating my results across different fixed effects structures. In Figure A.4, I augment Equation (2.1) with municipality-year and industry-year (4-digit) fixed effects. Fifth, in Figure A.5, I estimate Equation (2.1) using standard errors clustered at the broader province-industry level, as potential negative spillovers from certified to non-certified firms might upwardly bias my baseline estimates. Overall, across these different specifications, I obtain qualitatively similar results to my baseline tests.

### 2.5.2 Mechanism

In this section, I examine two non-mutually exclusive mechanisms through which the LR might improve firms' access to public procurement: reduced information frictions and improved external monitoring.

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public procurement. Notably, I obtain qualitatively similar results when my baseline analysis on certified firms at the firm-quarter level.

[19]Entropy-balancing weights observations such that the moments of certified firms' control variables match the moments of non-certified firms' control variables. This procedure offers three advantages with respect to other matching methods (e.g., propensity score-matching): (1) retaining all data; (2) matching on more moments, rather than only the mean; and (3) fewer subjective research design choices are required. Specifically, to ensure convergence, I match on the mean and the variance of my control variables.

[20]It is important to note that, by requiring certified and control firms to participate in public procurement, my matching procedure already reduces the skewness of my procurement variables.

On the one hand, the discrete LR scores might reduce information frictions in procurement contract allocation decisions. Literature documents that discrete ratings simplify comparisons and help market participants evaluate information that is available but difficult to access (e.g., Bernstein et al., 2023; Carpenter et al., 2021). By rating firms in terms of stars, the discrete LR scores allow procurement agencies to easily rank and compare supplier firms. For instance, while low LR scores primarily signal firms' past compliance, higher LR scores also signal firms' low likelihood of future misconduct, given their better compliance mechanisms. Therefore, procurement agencies might rely on the LR scores to identify and select superior supplier firms when awarding contracts.

On the other hand, the LR might reduce information processing costs not only for procurement agencies but also for external monitors, such as regulators or media (Darendeli et al., 2022). By providing a public signal of supplier firms' quality, the LR might allow these monitors to better evaluate the quality of firms winning procurement contracts. Research shows that procurement agencies adjust their contract allocation decisions in response to increased public scrutiny (Duguay et al., 2023). For example, Seregni (2024) finds that under higher scrutiny, procurement agencies allocate fewer contracts to firms with riskier beneficial owners. Consequently, by increasing external monitoring on procurement agencies, the LR might influence their award decisions. Specifically, in response to this heightened monitoring, procurement agencies might prefer allocating contracts to certified firms to minimize reputational risks or demonstrate alignment with a defensible signal (Tian and Xia, 2021).

To investigate which of these mechanisms predominates, I conduct two cross-sectional tests. I begin by estimating Equation (2.1) separately for certified firms with one-star, two-star, and three-star scores, and their respective matched controls.[21] Table 2.4 presents the results. The improved access to public procurement occurs primarily among certified firms with higher LR scores. One-star firms show no statistically significant increase in any of my outcome variables. Two-star firms experience improved access to public procurement after the LR award, with magnitudes comparable to the baseline estimation. Of note, despite the small sample size, three-star firms show large improvements in their access to public procurement, with magnitudes nearly triple the baseline estimation. For instance, after receiving the LR, three-star recipients experience an average increase of

[21] To ensure a clean identification of the effects of different LR scores, I exclude from this analysis certified firms that experience a change in their LR score (e.g., from one to two stars) during my sample period.

16% in the number and 64% in the value of procurement contracts won.[22]

**Table 2.4:** Mechanism: LR Scores

This table reports estimated coefficients for Equation (2.1), separately for certified firms with one-star, two-star, and three-star scores, and their respective matched controls. In Panel A, *Contracts Won* is the natural logarithm of one plus the number of public procurement contracts won. *Contract Value* is the natural logarithm of the value of procurement contracts won. In Panel B, *No. Agencies* is the natural logarithm of one plus the number of transacting procurement agencies. *Distance* is the natural logarithm of one plus the kilometer distance between a firm and its farthest public procurement agency relation. *Certified* is an indicator taking values of one if a firm had the Legality Rating for more than ninety days in $t-1$, and zero otherwise. Standard errors (in parentheses) are clustered by firm. All estimations include controls, firm and year fixed effects. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Procurement Volume

| | (1) | (2) Contracts Won | (3) | (4) | (5) Contract Value | (6) |
|---|---|---|---|---|---|---|
| | ★ | ★★ | ★★★ | ★ | ★★ | ★★★ |
| Certified | 0.022 | 0.040** | 0.160*** | 0.151 | 0.307** | 0.640** |
| | (0.016) | (0.019) | (0.039) | (0.142) | (0.145) | (0.255) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.6957 | 0.7649 | 0.7845 | 0.4922 | 0.5300 | 0.5425 |
| Observations | 21,764 | 19,907 | 5,089 | 21,764 | 19,907 | 5,089 |

Panel B: Informational Distance

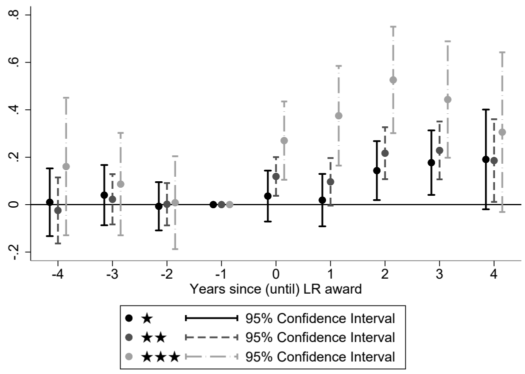
| | (1) | (2) No. Agencies | (3) | (4) | (5) Distance | (6) |
|---|---|---|---|---|---|---|
| | ★ | ★★ | ★★★ | ★ | ★★ | ★★★ |
| Certified | 0.014 | 0.038*** | 0.118*** | 0.071 | 0.128** | 0.348*** |
| | (0.013) | (0.014) | (0.030) | (0.055) | (0.054) | (0.101) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.6962 | 0.7796 | 0.8016 | 0.4771 | 0.5645 | 0.5675 |
| Observations | 21,764 | 19,907 | 5,089 | 21,764 | 19,907 | 5,089 |

I next exploit cross-sectional variation in two dimensions of procurement contracts:

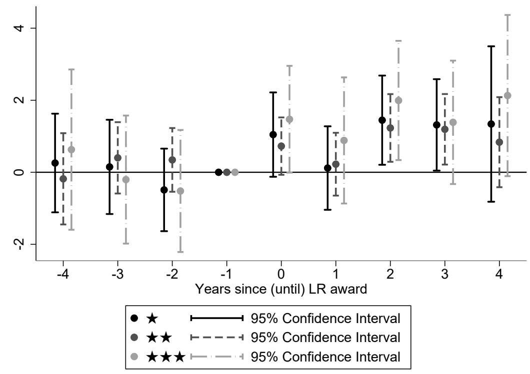
[22] I also estimate a dynamic specification of Equation (2.1) separately for certified firms with one-star, two-star, and three-star scores, and their respective matched controls. As shown in Figure 2.3, three-star firms experience larger improvements in their access to public procurement compared to one-star and two-star firms. Furthermore, these differences are often statistically significant.

## **Figure 2.3:** LR Scores

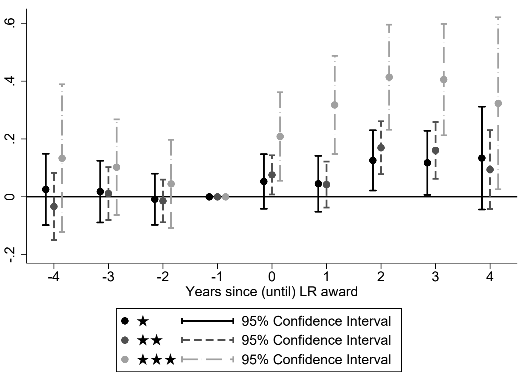
This figure reports the dynamic coefficients and 95% confidence interval obtained from estimating Equation (2.1) with indicators for every year relative to the LR adoption date excluding the minus-one indicator variable. The sample includes firms treated for four years and their matched controls. In Panel A, *Contracts Won* is the natural logarithm of one plus the number of public procurement contracts won. In Panel B, *Contract Value* is the natural logarithm of one plus the total value of public procurement contracts won. In Panel C, *No. Agencies* is the natural logarithm of one plus the number of transacting procurement agencies. In Panel D, *Distance* is the natural logarithm of one plus the kilometer distance between a firm and its farthest public procurement agency relation. Each panel compares the estimates of the effect of the LR, separately for firms with a ⋆ (black lines), ⋆⋆ (dark grey lines), and ⋆⋆⋆ (light grey lines) LR score. The regressions include controls, firm and year fixed effects. Standard errors are clustered at the firm level.



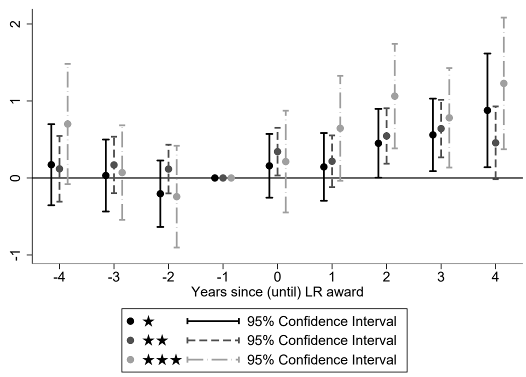

**(a)** Contracts Won




**(b)** Contract Value




**(c)** No. Agencies




**(d)** Distance

awarding procedure and degree of competitiveness. First, because public competitive auctions (e.g., open or negotiated procedures) require procurement agencies to specify contract conditions *ex ante* and ensure a formal competitive process, they involve greater monitoring and transparency than direct awards. In contrast, direct awards grant agencies significant discretion in supplier selection and are harder for regulators to monitor (Tulli, 2024). Second, contracts with multiple bidders naturally undergo more monitoring, as losing firms are motivated to scrutinize the fairness of the process (Gerardino et al., 2017). Therefore, if improved external monitoring enhances certified firms' access to public procurement, this effect should be most evident in contracts awarded through auctions or involving multiple bidders. Conversely, since the LR summarizes many non-price components of bidder quality, its ability to alleviate information frictions should be greater in direct awards, that have lower transparency requirements, or contracts with only one bidder, where price becomes a secondary consideration without market competition. As a result, if the LR primarily reduces information frictions in contract allocation decisions, this effect should be more visible in discretionary procedures.

Thus, I proceed to analyze how the characteristics of contracts won by certified firms change after receiving the LR award, compared to their matched controls. Table 2.5 reports the results. Panel A examines changes in the share of contracts won with different characteristics (transparency and competitiveness) relative to total contracts won. Panel B applies a similar approach, but focuses on changes in the share of contract value with these specific characteristics. Columns (1) to (3) focus on the degree of transparency of three different award procedures: open procedures, negotiated procedures, and direct awards. Certified firms show no significant increase in contracts awarded through auctions (open and negotiated procedures), but secure a higher share through direct awards. Columns (4) and (5) focus on the degree of competition of procurement contracts. Certified firms experience a statistically significant increase only in the share of noncompetitive contracts, i.e., those involving only one bidding firm. These findings indicate that post-LR award, certified firms secure a higher share of contracts subject to limited external monitoring. Overall, this cross-sectional evidence suggests that reduced information frictions, rather than improved monitoring, primarily drive the observed improvement in certified firms' access to public procurement.

My findings are consistent with two possible explanations. First, by providing a coarse signal of supplier firms' quality, the LR and the related scores might allow procurement agencies to identify and allocate contracts to superior supplier firms. This benefit is

## **Table 2.5:** Mechanism: Contract Types

This table reports estimated coefficients for Equation (2.1). In Panel A (B), *% Open* is the ratio of the number (value) of open contracts won to the total number (value) of contracts won. *% Negotiated* is the ratio of the number (value) of negotiated contracts won to the total number (value) of contracts won. *% Direct Awards* is the ratio of the number (value) of contracts won through a direct award to the total number (value) of contracts won. *% Competitive* is the ratio of the number (value) of competitive contracts won to the total number (value) of contracts won. *% Non-competitive* is the ratio of the number (value) of non-competitive contracts won to the total number (value) of contracts won. I define a contract as competitive (non-competitive) if it involves multiple (only one) bidding firm. *Certified* is an indicator taking values of one if a firm had the Legality Rating for more than ninety days in $t-1$, and zero otherwise. Standard errors (in parentheses) are clustered by firm. All estimations include controls, firm and year fixed effects. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Contracts Won

| | (1)<br>% Open | (2)<br>% Negotiated | (3)<br>% Direct Awards | (4)<br>% Competitive | (5)<br>% Non-competitive |
|---|---|---|---|---|---|
| Certified | 0.000 | -0.007 | 0.018*** | -0.005 | 0.017*** |
| | (0.004) | (0.006) | (0.005) | (0.007) | (0.004) |
| Controls | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.2316 | 0.2556 | 0.2564 | 0.3554 | 0.3294 |
| Observations | 56,261 | 56,261 | 56,261 | 56,261 | 56,261 |

Panel B: Contract Value

| | (1)<br>% Open | (2)<br>% Negotiated | (3)<br>% Direct Awards | (4)<br>% Competitive | (5)<br>% Non-competitive |
|---|---|---|---|---|---|
| Certified | 0.009* | -0.011* | 0.014*** | -0.004 | 0.016*** |
| | (0.005) | (0.006) | (0.005) | (0.007) | (0.005) |
| Controls | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.2781 | 0.2186 | 0.1706 | 0.3450 | 0.2497 |
| Observations | 56,261 | 56,261 | 56,261 | 56,261 | 56,261 |

particularly salient in contracts where procurement agencies have greater discretion in supplier selection (e.g., direct awards or noncompetitive contracts), rather than in more rigid procurement procedures (e.g., open or negotiated). For instance, using direct awards, procurement agencies can more easily allocate contracts to supplier firms they consider legitimate and able to execute the contract effectively (e.g., without undue cost overruns or delays). Therefore, a direct implication of this argument is that procurement contracts awarded to certified firms should have superior execution performance. Supporting this explanation, literature documents that increased discretion improves contractual efficiency by allowing agents to complement incomplete contracts with more informal information on supplier firms' quality (e.g., Banerjee and Duflo, 2000; Coviello et al., 2018; Decarolis et al., 2021).

Second, the LR might also inadvertently favor corrupt practices in contract allocation decisions. Complicit procurement agencies might leverage direct award procedures to more easily allocate contracts to connected or criminal firms, regardless of their ability to effectively execute contracts (Baltrunaite et al., 2021; Boland and Godsell, 2021; Gerardino et al., 2017; Tulli, 2024). Literature extensively documents how favoritism and connections in public procurement distort the allocation of procurement contracts. This misallocation often results in prolonged delays and inflated costs, as awarding decisions prioritize relationships and personal gains over efficiency (e.g., Ravenda et al., 2020; Schoenherr, 2019; Szucs, 2023). According to this argument, therefore, contracts awarded to certified firms should exhibit poorer execution performance.

## 2.6 LR and Contract Allocation Efficiency

To investigate the effects of the LR on contract allocation efficiency, I compare contract execution performance between certified firms and their matched controls. For the subset of completed public works contracts, ANAC provides information on the expected and actual delivery date, the total completion costs, and the number of modifications. Following previous studies (Coviello et al., 2018; Decarolis et al., 2021; Schoenherr, 2019), I use this information to compute three variables capturing *ex post* contract execution performance: cost overruns, delays, and contract modifications. These measures reflect the effectiveness of procurement allocation. Increased delays, cost overruns, or contract modifications result in additional resource outflows for the procurement agency and impose negative externalities on citizens, who are the ultimate beneficiaries of public works (Ravenda et al., 2020).

Specifically, I estimate the following specification at the individual contract-level $c$:

$$y_{c,t} = \beta_0 + \beta_1 Certified_{c,t} + \gamma X_{c,t} + \alpha_p + \alpha_w + \alpha_t + \epsilon_{c,t} \tag{2.2}$$

where $y$ is the outcome variable (e.g., delays) for contract $c$ in year $t$. *Certified* is an indicator taking values of one for contracts allocated to certified firms, and zero otherwise. A potential concern is that contracts allocated to certified firms might be different from contracts allocated to controls, ultimately driving differences in execution performance. To mitigate this concern, I control for several observable contract characteristics correlated with execution performance (e.g., Bafundi et al., 2024; Decarolis et al., 2021; Errico et al., 2024). First, because larger contracts are more likely to experience adverse outcomes, I include in all specifications a control for the reserve price (*Reserve Price*), i.e., the maximum amount the procurement agency is willing to pay for the contract. Second, I include province fixed effects ($\alpha_p$) to absorb time-invariant differences in contract execution across different geographical areas. Third, because execution performance might differ based on the type of work delivered, I include work-type ($\alpha_w$) fixed effects. Fourth, I use year fixed effects ($\alpha_t$) to absorb year-level shocks.

Panel B of Table 2.2 reports summary statistics on my contract-level sample. These statistics are in line with prior studies examining public works in Italy (e.g., Bafundi et al., 2024). The median reserve price is EUR 222 thousand, while more than half of my contracts experience a modification or delay.

Table 2.6 reports the results of estimating Equation (2.2) on the sample of public works contracts performed by certified firms and their matched controls. My outcome variables are indicators capturing the occurrence of three adverse outcomes: cost overruns, modifications, and delays in delivery. Across all these measures of contract performance, I find that contracts allocated to certified firms exhibit superior execution performance. Contracts awarded to certified firms are 3 and 2 percentage points less likely to experience cost overruns and modifications, respectively. Furthermore, certified firms are 2 percentage points less likely to deliver contracts with delays.[23] In Table A.6, I replace my indicator variables with variables capturing the intensity of adverse

[23] A potential concern relates to how these results might change depending on the contract awarding procedure (e.g., direct awards versus open procedures). For instance, complicit procurement agencies might allocate contracts to certified firms through direct awards, regardless of these firms' ability to execute contracts effectively. Therefore, these contracts might have lower execution performance compared to those awarded on a competitive basis. To mitigate this concern, I estimate Equation 2.2 adding award procedure fixed effects. As shown in Table A.5, the results remain qualitatively similar to my baseline analysis.

outcomes. Specifically, I use the percentage of cost overruns relative to the initial award price (column [1]), the natural logarithm of one plus the number of modifications (column [2]), and the inverse hyperbolic sine transformation of the number of days of delay (column [3]).[24] The results show that certified firms deliver contracts with lower extra costs, fewer modifications, and fewer days of delay.

**Table 2.6:** Contract Execution Performance

This table reports estimated coefficients for Equation (2.2). *Cost Overrun* is an indicator taking values of one if the final cost exceeds the award amount, and zero otherwise. *Modifications* is an indicator taking values of one if a contract experiences a subsequent modification, and zero otherwise. *Delay* is an indicator taking values of one if a contract experiences a delay, and zero otherwise. *Certified* is an indicator for contracts awarded to firms with the Legality Rating, and zero otherwise. *Reserve Price* is the natural logarithm of the reserve price. Standard errors (in parentheses) are clustered by firm. All estimations include year, work-type and province fixed effects. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1) Cost Overrun | (2) Modification | (3) Delay |
|---|---|---|---|
| Certified | -0.035*** | -0.024*** | -0.022** |
| | (0.012) | (0.007) | (0.009) |
| Reserve Price | 0.072*** | 0.055*** | 0.107*** |
| | (0.004) | (0.002) | (0.003) |
| Year FE | Yes | Yes | Yes |
| Work Type FE | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0588 | 0.0805 | 0.0843 |
| Observations | 24,617 | 58,624 | 30,419 |

I next investigate whether these results differ among certified firms with different LR scores. My previous analysis suggests that procurement agencies rely on LR scores to benchmark supplier firms, assuming higher scores as a signal of higher quality. Therefore, examining how contract execution performance differs across firms with varying LR scores allows me to assess whether these scores effectively discriminate between high-quality and low-quality firms. Furthermore, it sheds light on whether procurement agencies' interpretation of these scores aligns with actual firm quality.

I estimate Equation (2.2) separately for certified firms with one-star, two-star, and three-star scores, and their respective matched controls.[25] Table 2.7 reports the results. The reduction in cost overruns and modifications occurs primarily in contracts allocated to two- or three-star firms. Contracts allocated to three-star firms are 13 and 5 percentage

[24]Following Decarolis et al. (2021), I use the inverse hyperbolic sine transformation to reduce the impact of outliers and capture the possibility of both positive and negative delays, depending on whether works are completed late or early, respectively.

[25]I restrict the sample to certified firms and controls that both perform public works.

points less likely to experience cost overruns, modifications, and delays. In contrast, contracts allocated to one-star firms do not exhibit a lower likelihood of cost overruns and modifications relative to controls, but have a lower likelihood of delays. Table A.7 repeats the analysis, focusing on the intensity of adverse outcomes. Consistent with my previous findings, I document that contracts allocated to three-star firms have superior execution performance, as shown by the lower number of modifications, days of delays and percentage of cost overruns.[26] Again, while firms with a one-star score show lower delays, they do not exhibit reduced cost overruns or modifications.[27] These somewhat puzzling findings are consistent with strategic behavior, as documented by Decarolis (2014). Specifically, firms might prioritize avoiding delays and associated penalties, but opt to renegotiate contract conditions. This strategy might result in fewer delays, but more cost overruns and modifications.

These findings suggest that LR scores can provide a valuable signal of supplier firm quality. To further validate these empirical results, I cross-reference LR recipients with firms confiscated for Mafia connections. According to a large body of judicial and empirical evidence (e.g., Marcolongo, 2023; Tulli, 2024), Mafia organizations participate in public procurement through apparently legitimate firms for securing lucrative procurement contracts. In this respect, the high LR uptake in Southern Italy might suggest strategic exploitation by criminal firms. However, I find that, as of 2023, no LR recipients from 2013-2020 have subsequently been confiscated for Mafia-related crimes.[28]

Overall, my results suggest that the discrete LR scores, rather than the LR itself, improve contract allocation efficiency. While there is a positive association between LR awards and contract execution performance, this effect is primarily driven by two-star and three-star firms. In contrast, one-star firms show similar execution performance to control firms. This suggests a specific selection pattern where lower-quality firms might apply for the LR and settle for a low score merely to demonstrate compliance. Hence, these findings underscore the relevance of discrete ratings in certifications to more adequately discriminate between high- and low-quality applicants (Bernstein et al., 2023).

[26] In a concurrent work, Iossa and Latour (2025) also observe a positive correlation between LR scores and procurement efficiency.

[27] In Tables A.8 and A.9, I examine contract execution performance using exclusively the set of contracts awarded to LR recipients. My main inferences are qualitatively unaffected.

[28] I observe eight LR recipients obtaining the LR *after* being confiscated and entering into judicial administration. This is explicitly allowed by the LR regulation under the assumption that the judicial administration removes existing ties with organized crime while allowing for firms' continuation as a going concern. For more information on the Italian judicial administration procedure, see Calamunci and Drago (2020)

**Table 2.7:** Contract Execution Performance: Heterogeneous Effects

This table reports estimated coefficients for Equation (2.2), separately for contracts awarded to certified firms with one-star, two-star, and three-star scores, and their respective matched controls. In Panel A, the dependent variable is *Cost Overrun. Cost Overrun* is an indicator taking values of one if the final cost exceeds the award amount, and zero otherwise. In Panel B, the dependent variable is *Modifications. Modifications* is an indicator taking values of one if a contract experiences a subsequent modification, and zero otherwise. In Panel C, the dependent variable is *Delay. Delay* is an indicator taking values of one if a contract experiences a delay, and zero otherwise. *Certified* is an indicator for contracts awarded to firms with the Legality Rating, and zero otherwise. Standard errors (in parentheses) are clustered by firm. All estimations include a control for the reserve price, year, work-type and province fixed effects. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Cost Overrun

| | (1)<br>★ | (2)<br>★★ | (3)<br>★★★ |
|---|---|---|---|
| Certified | 0.024 | -0.058*** | -0.133*** |
| | (0.022) | (0.016) | (0.045) |
| Control | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Work Type FE | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0530 | 0.0695 | 0.1223 |
| Observations | 9,647 | 11,232 | 2,789 |

Panel B: Modifications

| | (1)<br>★ | (2)<br>★★ | (3)<br>★★★ |
|---|---|---|---|
| Certified | -0.008 | -0.037*** | -0.051*** |
| | (0.011) | (0.010) | (0.019) |
| Control | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Work Type FE | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0696 | 0.0901 | 0.1103 |
| Observations | 21,727 | 28,093 | 7,416 |

Panel C: Delay

| | (1)<br>★ | (2)<br>★★ | (3)<br>★★★ |
|---|---|---|---|
| Certified | -0.035** | -0.020 | -0.047* |
| | (0.014) | (0.013) | (0.027) |
| Control | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Work Type FE | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0828 | 0.0852 | 0.1273 |
| Observations | 12,018 | 13,926 | 3,394 |

## 2.7 LR and Aggregate Effects

While I have so far studied the effects of the LR at a micro-level, I explore the aggregate effects of the LR in public procurement in this section.

My firm-level analyses show that the LR improves certified firms' access to public procurement. However, given the voluntary adoption of the LR by certified firms, a potential concern is that unobservable characteristics might drive the LR adoption and the subsequent improved access to public procurement. Furthermore, my firm-level estimates might be upwardly biased because of potential negative spillovers from certified to non-certified firms. Although my matching strategy and previous tests already mitigate these concerns, I further assess the robustness of my findings using an aggregate design. This analysis leverages the more plausibly exogenous variation in treatment due to the differential exposure of local markets to the introduction of the LR. Therefore, it attenuates concerns related to selection bias or potential spillovers from certified to non-certified firms, since such spillovers are less likely in aggregate designs (Breuer et al., 2025).

I exploit two thresholds for LR eligibility, which provide the features of a quasi-experiment. To obtain the LR, firms must have sales above EUR 2 million and be included in the Business Register for at least two years. These thresholds create exogenous variation in the exposure of local markets, defined at the province-industry level, to the introduction of the LR. The intuition is that industries in provinces with a larger *ex ante* share of eligible firms (i.e., firms with sales above EUR 2 million and included for at least two years in the Business Register) should be more affected by the introduction of the LR in public procurement (Breuer, 2021; Breuer et al., 2025).[29] This greater province-industry exposure should, in turn, explain the changes in procurement contract allocation after the introduction of the LR.

I estimate the following difference-in-differences specification with continuous treatment:

$$y_{j,p,t} = \beta_0 + \beta_1 Share_{j,p} * Post_t + \alpha_{j,p} + \alpha_{j,t} + \alpha_{p,t} + \epsilon_{j,p,t} \tag{2.3}$$

where $y$ is the outcome variable for industry $j$ (using two-digit industry classification),

[29] I examine the validity of this assumption by regressing the actual share of LR recipients in a given province-industry-year group on the average share of eligible firms in the pre-procurement-reform. The estimated F-stat is 107.87, well above the critical value of 8.96 suggested by Stock et al. (2002) and Larcker and Rusticus (2010).

in province $p$, in year $t$. *Share* captures the average share of eligible firms in the period 2013-2015 for industry $j$, in province $p$. *Post* is an indicator variable taking values of one for all years after the introduction of the LR in the public procurement process (i.e., from 2016).[30] I control for systematic differences across provinces and industries using province-industry fixed effects ($\alpha_{j,p}$). Furthermore, I use industry-by-year ($\alpha_{j,p}$) and province-by-year ($\alpha_{p,t}$) fixed effects to absorb time-varying shocks at the industry- and province-level, respectively. I cluster standard errors at the province-industry level. Panel C of Table 2.2 reports summary statistics on my market-level sample. An average of 39 firms operate in a given province-industry-year group, but only an average of 9% participate in public procurement.

Table 2.8 reports the results of my aggregate analyses. In column (1), I examine the effects of introducing the LR in the public procurement process on the aggregate allocation of procurement contracts. I find a positive and statistically significant coefficient for the interaction between *Share* and *Post*. After the introduction of the LR in the public procurement process, markets with a greater *ex ante* exposure experience a greater allocation of public procurement contracts. This result shows that, by improving firms' access to public procurement, the LR led to a reallocation of procurement contracts across different markets.

In column (2), I investigate the effects of the LR on aggregate participation in public procurement. By publicly disseminating information on firms' quality, the LR might reduce incumbent firms' information advantage, such as knowledge of supplier firms' quality acquired over repeated interactions (Breuer et al., 2018; Sufi, 2009). The LR might, therefore, facilitate participation in public procurement by reducing the relevance of existing relationships in contract allocation decisions (e.g., Breuer, 2021; Rajan and Zingales, 2003). In line with this reasoning, I observe that markets with higher exposure experience an increase in the share of firms bidding for public procurement contracts. The estimated coefficient indicates that the share of firms participating in public procurement increases by 6 percentage points after the introduction of the LR in the public procurement process for markets with higher exposure to the certification.[31]

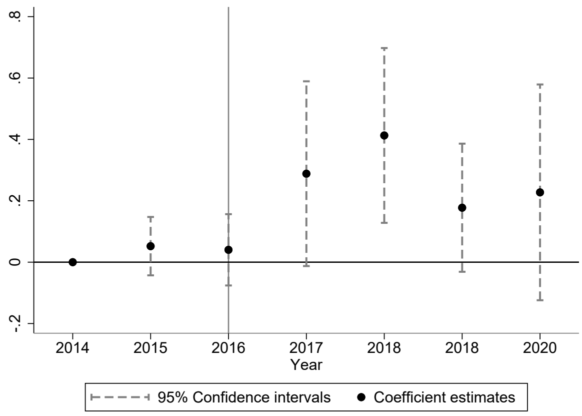
I conclude my analysis of the LR's aggregate effects with back-of-the-envelope estimates

[30] Even though the ICA introduced the LR in 2012, the LR gained relevance in the public procurement process only after the 2016 Procurement Reform(ICA, 2017).

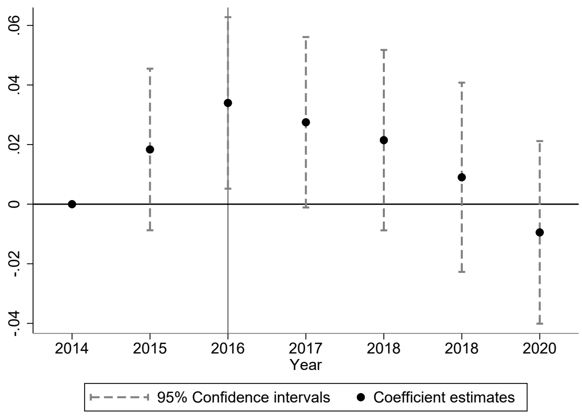
[31] A potential concern is that an unobserved source of variation might drive my aggregate results. To mitigate this concern, I also estimate Equation (2.3) using as explanatory variable the actual share of LR recipients in a given province, industry, year. I obtain qualitatively similar results.

## **Figure 2.4:** Access to public procurement: Market-level analysis

This figure reports the dynamic coefficients and 95% confidence interval from estimating Equation (2.3) on the share of affected province industry groups. The share of affected province-industry groups is computed as the average fraction of potentially-eligible firms in a given province-industry in the 2013-2015 period. The annual estimates represent difference-in-differences coefficients relative to the base year 2014. In Panel A, the dependent variable is *Average Contracts Won*, computed as the total number of procurement contracts won scaled by total firms in a given province, industry, and year. In Panel B, the dependent variable is *% Bidding Firms*, computed as the number of firms bidding for public procurement contracts scaled by total number of firms in a given province, industry, year. The regressions include province-industry, industry-year and province-year fixed effects. Standard errors are clustered at the province-industry level.




**(a)** Average Contracts Won




**(b)** % Bidding Firms

**Table 2.8:** Market-level Analysis

This table reports estimated coefficients for Equation (2.3). *Average Contracts Won* is the number of procurement contracts won scaled by total firms in a given province, industry, and year. *% Bidding Firms* is the number of firms bidding for a public procurement contract scaled by total firms in a given province, industry, and year. Standard errors (in parentheses) are clustered at the province-industry level. The estimations include province-industry, province-year and industry-year fixed effects. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1)<br>Average Contracts Won | (2)<br>% Bidding Firms |
|---|---|---|
| Share × Post | 0.295** | 0.058*** |
| | (0.119) | (0.010) |
| Province-Industry FE | Yes | Yes |
| Province-Year FE | Yes | Yes |
| Industry-Year FE | Yes | Yes |
| Adjusted $R^2$ | 0.7912 | 0.5537 |
| Observations | 69,400 | 69,400 |

of the costs and benefits of the LR in the public procurement process. The costs of the LR in public procurement coincide with the costs the ICA faces in administering the program, such as analyzing applications or monitoring *ex post* compliance. To estimate the costs directly pertaining to the LR program, I leverage a feature of Italian law that requires government agencies to disaggregate costs and revenues by operational segment in their annual financial statements. Specifically, the ICA separately discloses the costs of its "Conflict of Interest, Rating and Business Legality "(*Conflitto di interessi, rating e legalità imprese*) segment, which includes the tasks related to the LR. The average annual expenditure for this segment from 2018 to 2020 is EUR 1.96 million, primarily consisting of wage expenses.[32] Extending this average figure over the 2013-2020 period, I estimate the total cost of the program to be EUR 15.65 million (approximately USD 19.20 million).[33]

From the perspective of a public buyer, a crucial benefit of the LR is a lower misuse of public resources. My previous findings document that the LR and the related LR scores allow procurement agencies to select superior supplier firms, leading to lower cost overruns. Specifically, the coefficients reported in Table A.9 indicate that for every EUR 1 allocated to three-star firms, there is a saving of EUR 0.030 compared to similar contracts allocated to one-star firms. Given that the total value of public works contracts awarded

[32]ICA financial statements are not publicly available before 2018.

[33]This amount represents an upper-bound estimate. Given the ICA's multiple responsibilities, the annual average (1.96 million) likely includes wage expenses for staff involved in other tasks within the same operational segment, such as monitoring potential conflicts of interests.

to three-star firms during my sample period is EUR 6.71 billion, back-of-the-envelope calculations suggest a total saving of EUR 201 million (around USD 247 million in 2020 prices).[34] This amount is approximately 0.01% of the Italian GDP in 2021 prices. Overall, this analysis indicates that the LR is revenue-positive for the Italian government, as its estimated savings largely exceed the costs of implementing the program.

## 2.8 Conclusion

In most developed economies, public procurement accounts for a significant fraction of the countries' economic activity. However, given the sheer size of procurement contracts and their material effect on supplier firms (e.g., Ferraz et al., 2015; Hvide and Meling, 2022), public procurement is also prone to corruption and criminal infiltration (Colonnelli et al., 2022; Marcolongo, 2023). In this respect, limited transparency around supplier firms may lead to a misuse of public resources by facilitating the allocation of public procurement contracts to criminal or corrupt firms. In this study, I explore the introduction in Italy of a government certification, the Legality Rating, and examine how it affects firms' access to public procurement and the efficiency of procurement contracts' allocation. I document that, after the certification, certified firms experience improved access to public procurement. Furthermore, certified firms execute their contracts more efficiently with fewer cost overruns, modifications and delays. However, only firms with higher certified ratings experience improved access to public procurement and systematically execute contracts better. Through the estimation of aggregate effects, I document that the certification increases participation in public procurement and is revenue-positive for the government.

Overall, my results show that government certifications can improve resource allocation. Despite the limitations of a single-country study, the evidence reported should be of interest for policy purposes. In the last decade, governments have implemented several reporting and auditing mandates to improve private firms' transparency. This study shows that government certification can constitute an alternative mechanism to improve private firms' transparency, thereby reducing their frictions in accessing product markets. I see two main avenues of future research. First, while this paper focused on the public procurement setting, it is relevant to examine whether and how the LR improves

[34]During my sample period, 2,442 contracts were allocated to certified firms with a three-star score, with an average award value of EUR 2.75 million. To obtain the estimated savings, I multiply the total award amount (6,711,989,582) by the estimated coefficient of (0.030).

transactions with private sector customers, which might have different informational demands than procurement agencies. Second, given the low take-up of the LR and other government programs, understanding the factors driving the take-up of government programs among SMEs is of first-order importance.

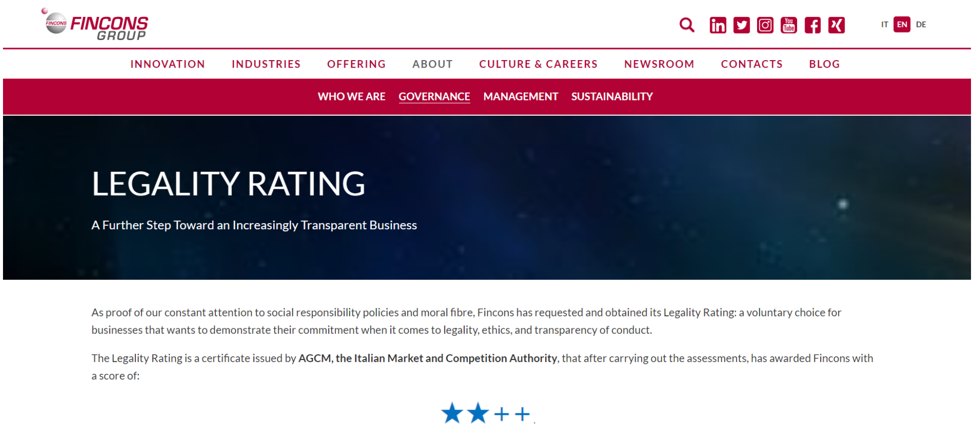
# Appendix A

## A.1 Supplementary Material to Chapter 2

**Figure A.1:** Examples of Company Disclosures of Legality Rating Award

This appendix contains two examples for mentions of the Legality Rating award on companies' webpages.

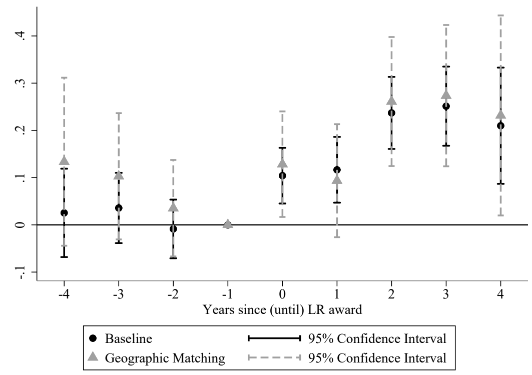

(a) MTA (Advanced Automotive Solutions)



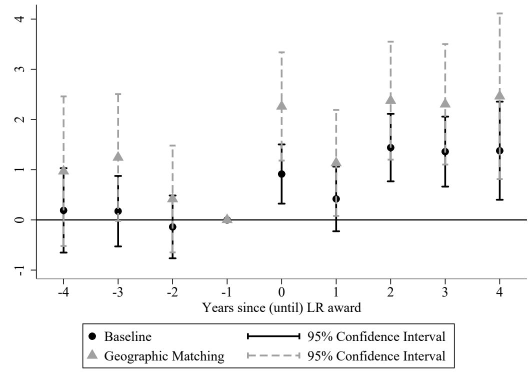

(b) Fincons Group

## **Figure A.2:** Alternative Matching

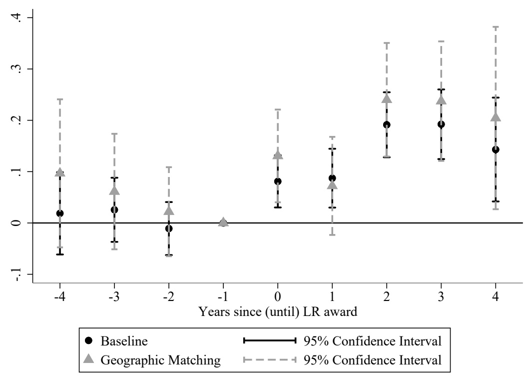
This figure compares the dynamic coefficients and 95% confidence interval obtained from estimating Equation (2.1) with indicators for every year relative to the LR adoption date excluding the minus-one indicator variable. The baseline estimates from Figure 2.2 are reported for comparability and are denoted by the dark lines in all panels. These estimates are compared to those obtained when restricting the matching procedure to firms operating in the same region (light gray lines). The sample includes certified firms having the LR for four years and their matched controls. In Panel A, *Contracts Won* is the number of public procurement contracts won. In Panel B, *Contract Value* is the total value of public procurement contracts won. In Panel C, *No. Agencies* is the number of transacting procurement agencies. In Panel D, *Distance* is the natural logarithm of one plus the kilometer distance between a firm and its farthest public procurement agency relation. The regressions include controls, firm and year fixed effects. Standard errors are clustered at the firm level.



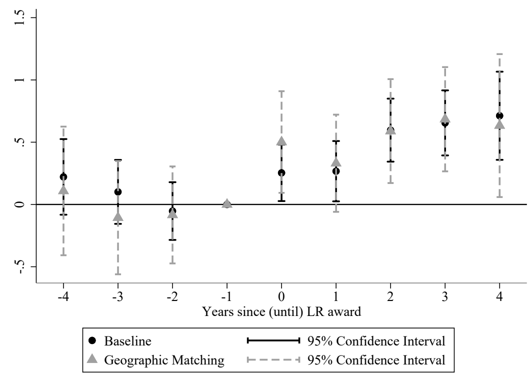

**(a)** Contracts Won




**(b)** Contract Value



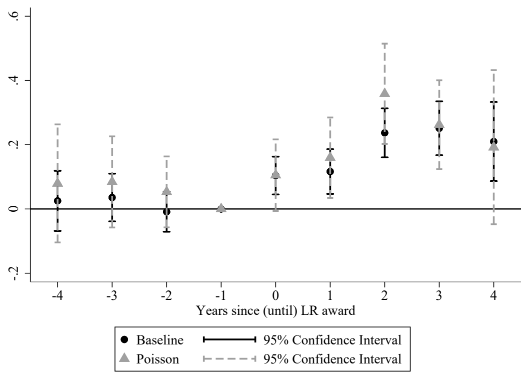

**(c)** No. Agencies



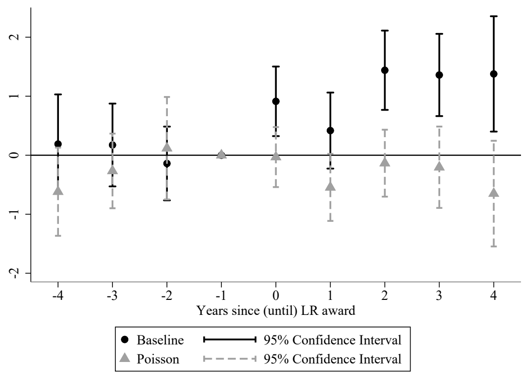

**(d)** Distance

## **Figure A.3:** Poisson Specification

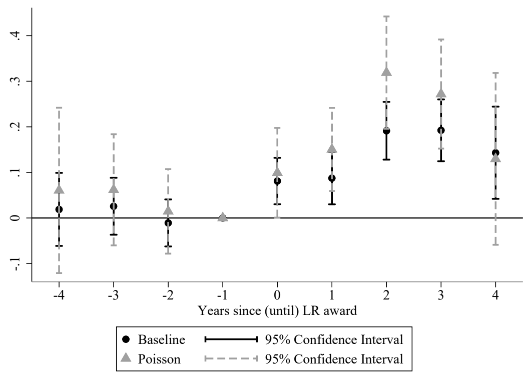
This figure compares the dynamic coefficients and 95% confidence interval obtained from estimating Equation (2.1) with indicators for every year relative to the LR adoption date excluding the minus-one indicator variable. The baseline estimates from Figure 2.2 are reported for comparability and are denoted by the dark lines in all panels. These estimates are compared to those obtained when estimating Equation (2.1) using a Poisson specification (light gray lines). The sample includes certified firms having the LR for four years and their matched controls. In Panel A, *Contracts Won* is the natural logarithm of one plus the number of public procurement contracts won. In Panel B, *Contract Value* is the natural logarithm of one plus the total value of public procurement contracts won. In Panel C, *No. Agencies* is the natural logarithm of one plus the number of transacting procurement agencies. In Panel D, *Distance* is the natural logarithm of one plus the kilometer distance between a firm and its farthest public procurement agency relation. The regressions include controls, firm and year fixed effects. Standard errors are clustered at the firm level.



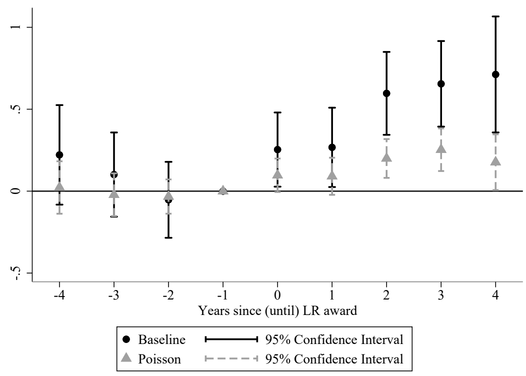

**(a)** Contracts Won




**(b)** Contract Value



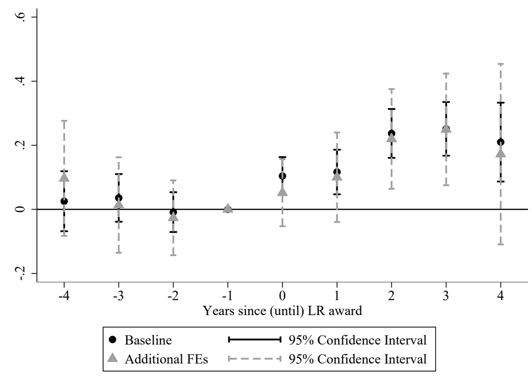

**(c)** No. Agencies



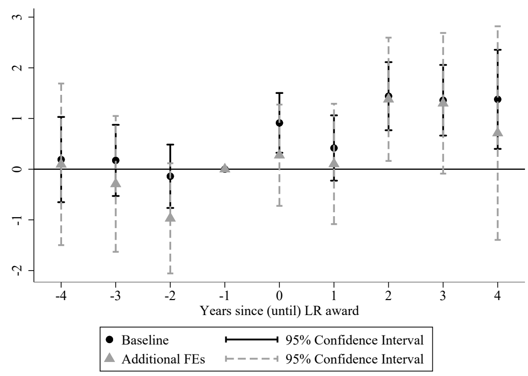

**(d)** Distance

## **Figure A.4:** Alternative Fixed Effects

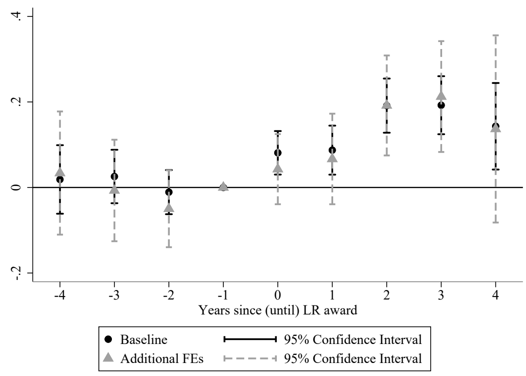
This figure compares the dynamic coefficients and 95% confidence interval obtained from estimating Equation (2.1) with indicators for every year relative to the LR adoption date excluding the minus-one indicator variable. The baseline estimates from Figure 2.2 are reported for comparability and are denoted by the dark lines in all panels. These estimates are compared to those obtained when estimating Equation (2.1) using municipality-year and industry-year (4-digit) fixed effects. The sample includes certified firms having the LR for four years and their matched controls. In Panel A, *Contracts Won* is the natural logarithm of one plus the number of public procurement contracts won. In Panel B, *Contract Value* is the natural logarithm of one plus the total value of public procurement contracts won. In Panel C, *No. Agencies* is the natural logarithm of one plus the number of transacting procurement agencies. In Panel D, *Distance* is the natural logarithm of one plus the kilometer distance between a firm and its farthest public procurement agency relation. Standard errors are clustered at the firm level.



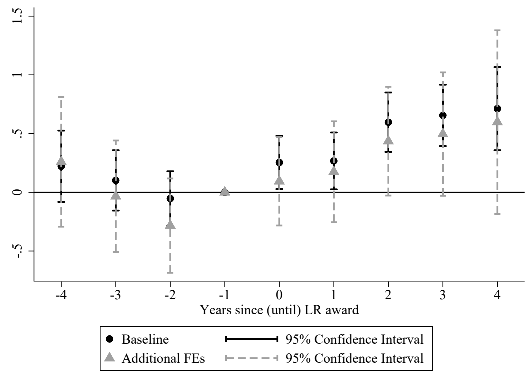

**(a)** Contracts Won




**(b)** Contract Value



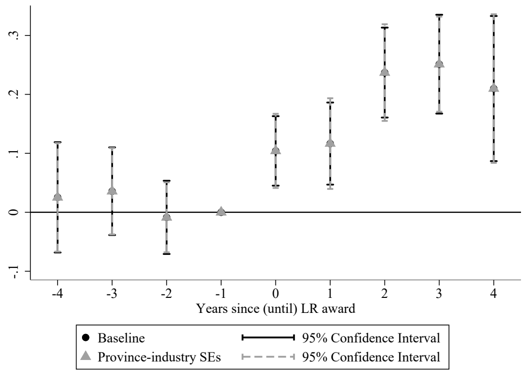

**(c)** No. Agencies



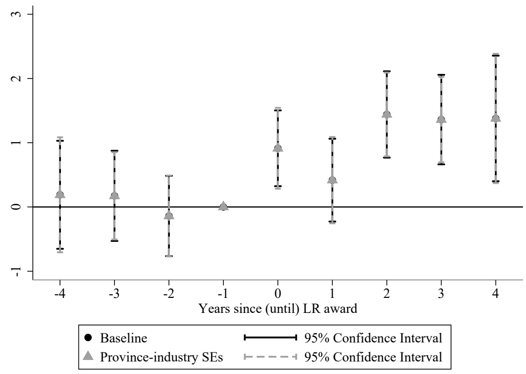

**(d)** Distance

# **Figure A.5:** Alternative Standard Errors

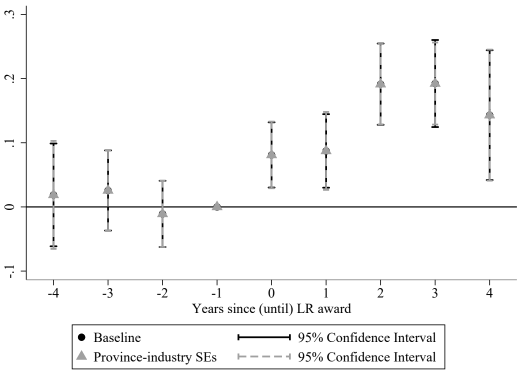
This figure compares the dynamic coefficients and 95% confidence interval obtained from estimating Equation (2.1) with indicators for every year relative to the LR adoption date excluding the minus-one indicator variable. The baseline estimates from Figure 2.2 are reported for comparability and are denoted by the dark lines in all panels. These estimates are compared to those obtained when estimating Equation (2.1) using standard errors clustered by province-industry. The sample includes certified firms having the LR for four years and their matched controls. In Panel A, *Contracts Won* is the natural logarithm of one plus the number of public procurement contracts won. In Panel B, *Contract Value* is the natural logarithm of one plus the total value of public procurement contracts won. In Panel C, *No. Agencies* is the natural logarithm of one plus the number of transacting procurement agencies. In Panel D, *Distance* is the natural logarithm of one plus the kilometer distance between a firm and its farthest public procurement agency relation. The regressions include controls, firm and year fixed effects.



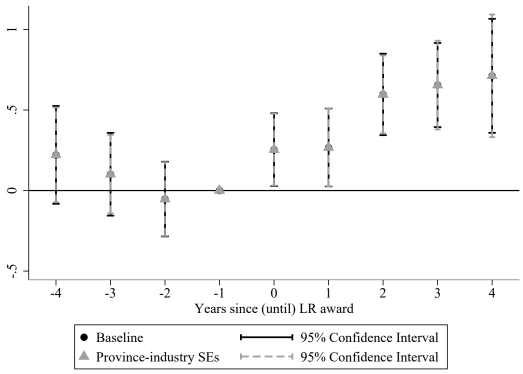

**(a)** Contracts Won




**(b)** Contract Value




**(c)** No. Agencies




**(d)** Distance

## **Table A.1:** Characteristics of LR recipients

This table reports the key characteristics of LR recipients in the year before the award and compares them to those of non-certified sample firms.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | LR Recipients | | Non-certified firms | |
| | Mean | Median | Mean | Median |
| Assets (EUR Thousands) | 28,426.655 | 6,996.000 | 23,120.410 | 2,406.000 |
| Sales (EUR Thousands) | 23,244.651 | 6,833.000 | 11,102.327 | 2,337.000 |
| Employees (Number) | 115.730 | 32.000 | 31.046 | 10.000 |
| Reporting and Auditing Mandate | 0.389 | 0.000 | 0.079 | 0.000 |
| ROA | 0.056 | 0.040 | 0.104 | 0.034 |
| Growth | 0.058 | 0.033 | 2.886 | 0.014 |
| Any Contract | 0.349 | 0.000 | 0.058 | 0.000 |
| Contracts Won (Number) | 3.462 | 0.000 | 0.321 | 0.000 |
| Mafia-Sector | 0.062 | 0.000 | 0.054 | 0.000 |
| Mafia-Region | 0.128 | 0.000 | 0.109 | 0.000 |
| Mafia-Municipality | 0.048 | 0.000 | 0.044 | 0.000 |

**Table A.2:** Firm Dynamics

This table reports estimated coefficients for Equation (2.1), focusing on the effects of the LR on firm dynamics. *Employees* is the natural logarithm of the number of employees. I include as controls the lagged value of *Size*, *Margin*, *ROA*, *PPE*, *Growth*, *Employees*, *Contracts Won* and *Contract Value*. Standard errors (in parentheses) are clustered by firm. All estimations include firm and year fixed effects. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1) Employees | (2) Employees |
|---|---|---|
| Certified | 0.051*** | 0.018*** |
| | (0.009) | (0.006) |
| Controls | No | Yes |
| Firm FE | Yes | Yes |
| Year FE | Yes | Yes |
| Adjusted $R^2$ | 0.9197 | 0.9492 |
| Observations | 55,658 | 55,658 |

## **Table A.3:** Rejected Firms

This table reports estimated coefficients for Equation (2.1) on the sample of rejected firms and their matched controls. *Contracts Won* is the natural logarithm of one plus the number of public procurement contracts won by a firm in a given quarter. *Contract Value* is the natural logarithm of the value of procurement contracts won by a firm in a given quarter. *No. Agencies* is the natural logarithm of one plus the number of transacting procurement agencies in a given quarter. *Distance* is the natural logarithm of one plus the kilometer distance between a firm and its farthest public procurement agency relation in a given quarter. *Rejected* is an indicator taking values of one after a firm applies for the Legality Rating unsuccessfully, and zero otherwise. Standard errors (in parentheses) are clustered by firm. All estimations include firm and quarter-year fixed effects. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1)<br>Contracts Won | (2)<br>Contract Value | (3)<br>No. Agencies | (4)<br>Distance |
|---|---|---|---|---|
| Rejected | -0.036 | -0.472 | 0.001 | -0.128 |
| | (0.059) | (0.580) | (0.062) | (0.283) |
| Firm FE | Yes | Yes | Yes | Yes |
| Quarter-Year FE | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.4444 | 0.4217 | 0.3749 | 0.3490 |
| Observations | 1,558 | 1,558 | 1,558 | 1,558 |

## **Table A.4:** Entropy-balanced Sample

This table reports estimated coefficients for Equation (2.1) on an entropy-balanced sample. *Contracts Won* is the natural logarithm of one plus the number of public procurement contracts won by a firm in a given year. *Contract Value* is the natural logarithm of the value of procurement contracts won by a firm in a given year. *No. Agencies* is the natural logarithm of one plus the number of transacting procurement agencies. *Distance* is the natural logarithm of one plus the kilometer distance between a firm and its farthest public procurement agency relation. *Certified* is an indicator taking values of one if a firm had the Legality Rating for more than ninety days in $t-1$, and zero otherwise. Standard errors (in parentheses) are clustered by firm. All estimations include controls, firm and year fixed effects. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1)<br>Contracts Won | (2)<br>Contract Value | (3)<br>No. Agencies | (4)<br>Distance |
|---|---|---|---|---|
| Certified | 0.050*** | 0.123*** | 0.028*** | 0.036** |
| | (0.007) | (0.047) | (0.005) | (0.018) |
| Controls | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.8941 | 0.8184 | 0.8958 | 0.7913 |
| Observations | 1,475,304 | 1,475,304 | 1,475,304 | 1,475,304 |

**Table A.5:** Contract Execution Performance: Alternative Fixed Effects

This table reports estimated coefficients for Equation (2.2). *Cost Overrun* is an indicator taking values of one if the final cost exceeds the award amount, and zero otherwise. *Modifications* is an indicator taking values of one if a contract experiences a subsequent modification, and zero otherwise. *Delay* is an indicator taking values of one if a contract experiences a delay, and zero otherwise. *Certified* is an indicator for contracts awarded to firms with the Legality Rating, and zero otherwise. All estimations include a control for the reserve price, and year, work-type, province, and award procedure fixed effects. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1) Cost Overrun | (2) Modification | (3) Delay |
|---|---|---|---|
| Certified | -0.026** | -0.019*** | -0.016* |
| | (0.012) | (0.006) | (0.009) |
| Control | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Work Type FE | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes |
| Award Procedure FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0773 | 0.0989 | 0.0950 |
| Observations | 24,456 | 58,109 | 30,204 |

## **Table A.6:** Contract Execution Performance: Intensive Margin

This table reports estimated coefficients for Equation (2.2). *% Cost Overrun* is the ratio of the difference between final cost and award amount to award amount. *Modifications (Log)* is the natural logarithm of one plus the number of modifications a contract experiences. *Delay Days (Asinh)* is the inverse hyperbolic sine transformation of the number of days of delay. *Certified* is an indicator for contracts awarded to firms with the Legality Rating, and zero otherwise. Standard errors (in parentheses) are clustered by firm. All estimations include a control for the reserve price, year, work-type and province fixed effects. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1)<br>% Cost Overrun | (2)<br>Modifications (Log) | (3)<br>Delay Days (Asinh) |
|---|---|---|---|
| Certified | -0.011** | -0.021*** | -0.203*** |
| | (0.005) | (0.006) | (0.078) |
| Control | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Work Type FE | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0458 | 0.0974 | 0.0895 |
| Observations | 24,617 | 58,624 | 30,419 |

**Table A.7:** Contract Execution Performance: Heterogeneous Effects

This table reports estimated coefficients for Equation (2.2). *% Cost Overrun* is the ratio of the difference between final cost and award amount to award amount. *Modifications (Log)* is the natural logarithm of one plus the number of modifications a contract experiences. *Delay Days (Asinh)* is the inverse hyperbolic sine transformation of the number of days of delay. *Certified* is an indicator for contracts awarded to firms with the Legality Rating, and zero otherwise. Standard errors (in parentheses) are clustered by firm. All estimations include a control for the reserve price, year, work-type and province fixed effects. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: % Cost Overrun

| | (1)<br>★ | (2)<br>★★ | (3)<br>★★★ |
|---|---|---|---|
| Certified | 0.003 | -0.014** | -0.042* |
| | (0.008) | (0.006) | (0.022) |
| Control | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Work Type FE | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0481 | 0.0582 | 0.0898 |
| Observations | 9,647 | 11,232 | 2,789 |

Panel B: Modifications (Log)

| | (1)<br>★ | (2)<br>★★ | (3)<br>★★★ |
|---|---|---|---|
| Certified | -0.009 | -0.034*** | -0.040** |
| | (0.010) | (0.009) | (0.019) |
| Control | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Work Type FE | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0829 | 0.1090 | 0.1342 |
| Observations | 21,727 | 28,093 | 7,416 |

Panel C: Delay Days (Asinh)

| | (1)<br>★ | (2)<br>★★ | (3)<br>★★★ |
|---|---|---|---|
| Certified | -0.318*** | -0.208* | -0.602** |
| | (0.116) | (0.110) | (0.248) |
| Control | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Work Type FE | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0888 | 0.0973 | 0.1316 |
| Observations | 12,018 | 13,926 | 3,394 |

## **Table A.8:** Contract Execution Performance: LR Recipients

This table reports estimated coefficients for Equation (2.2). *Cost Overrun* is an indicator taking values of one if the final cost exceeds the award amount, and zero otherwise. *Modifications* is an indicator taking values of one if a contract experiences a subsequent modification, and zero otherwise. *Delay* is an indicator taking values of one if a contract experiences a delay, and zero otherwise. *Two-star* is an indicator for contracts awarded to firms with the a two-star Legality Rating score, and zero otherwise. *Three-star* is an indicator for contracts awarded to firms with the a three-star Legality Rating score, and zero otherwise. Standard errors (in parentheses) are clustered by firm. All estimations include a control for the reserve price, year, work-type and province fixed effects. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: ★ vs. ★★

| | (1)<br>Cost Overrun | (2)<br>Modification | (3)<br>Delay |
|---|---|---|---|
| Two-star | -0.064*** | -0.023** | -0.006 |
| | (0.018) | (0.009) | (0.014) |
| Year FE | Yes | Yes | Yes |
| Work Type FE | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0646 | 0.0647 | 0.0749 |
| Observations | 4,170 | 13,387 | 6,626 |

Panel B: ★ vs. ★★★

| | (1)<br>Cost Overrun | (2)<br>Modification | (3)<br>Delay |
|---|---|---|---|
| Three-star | -0.069** | -0.024* | -0.033 |
| | (0.031) | (0.014) | (0.023) |
| Year FE | Yes | Yes | Yes |
| Work Type FE | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0785 | 0.0650 | 0.1024 |
| Observations | 2,471 | 8,708 | 3,967 |

Panel C: ★★ vs. ★★★

| | (1)<br>Cost Overrun | (2)<br>Modification | (3)<br>Delay |
|---|---|---|---|
| Three-star | -0.077** | -0.032** | -0.031 |
| | (0.031) | (0.014) | (0.023) |
| Year FE | Yes | Yes | Yes |
| Work Type FE | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0775 | 0.0676 | 0.1019 |
| Observations | 2,491 | 8,952 | 4,016 |

**Table A.9:** Contract Execution Performance: LR Recipients (Intensive Margin)

This table reports estimated coefficients for Equation (2.2). *% Cost Overrun* is the ratio of the difference between final cost and award amount to award amount. *Modifications (Log)* is the natural logarithm of one plus the number of modifications a contract experiences. *Delay Days (Asinh)* is the inverse hyperbolic sine transformation of the number of days of delay. *Two-star* is an indicator for contracts awarded to firms with the a two-star Legality Rating score, and zero otherwise. *Three-star* is an indicator for contracts awarded to firms with the a three-star Legality Rating score, and zero otherwise. Standard errors (in parentheses) are clustered by firm. All estimations include a control for the reserve price, year, work-type and province fixed effects. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: ★ vs. ★★

| | (1)<br>% Cost Overrun | (2)<br>Modifications (Log) | (3)<br>Delay Days (Asinh) |
|---|---|---|---|
| Two-star | -0.020*** | -0.015* | -0.113 |
| | (0.006) | (0.009) | (0.112) |
| Year FE | Yes | Yes | Yes |
| Work Type FE | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0436 | 0.0777 | 0.0785 |
| Observations | 4,170 | 13,387 | 6,626 |

Panel B: ★ vs. ★★★

| | (1)<br>% Cost Overrun | (2)<br>Modifications (Log) | (3)<br>Delay Days (Asinh) |
|---|---|---|---|
| Three-star | -0.030*** | -0.027* | -0.308 |
| | (0.010) | (0.014) | (0.194) |
| Year FE | Yes | Yes | Yes |
| Work Type FE | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0843 | 0.0730 | 0.0798 |
| Observations | 2,565 | 8,437 | 4,175 |

Panel C: ★★ vs. ★★★

| | (1)<br>% Cost Overrun | (2)<br>Modifications (Log) | (3)<br>Delay Days (Asinh) |
|---|---|---|---|
| Three-star | -0.018* | -0.023* | -0.146 |
| | (0.009) | (0.013) | (0.193) |
| Year FE | Yes | Yes | Yes |
| Work Type FE | Yes | Yes | Yes |
| Province FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0695 | 0.0862 | 0.1053 |
| Observations | 2,471 | 8,708 | 3,967 |

# Chapter 3

# Product Market Networks and the Take-up of Government Programs

Authors: Kalash Jain; Carmine Pizzo.

## 3.1 Introduction

Small and medium enterprises ("SMEs") face severe constraints in attracting capital and visibility (Bourveau et al., 2022). To mitigate these constraints, governments worldwide have implemented programs such as loan guarantee schemes, certifications, and small business grants. However, the take-up of these programs among SMEs is often low, as SMEs have limited awareness of government programs, lack a complete understanding of their benefits, or perceive the application process as overly complex (Custodio et al., 2021; Gassen and Muhn, 2025).[1] Thus, given SMEs' relevance in the economic landscape and the substantial resources governments devote to such programs, understanding factors increasing the take-up of government programs is of first-order importance.

In this study, we investigate how information spillovers in product market networks increase the take-up of government programs. While firms can acquire information on government programs through several public and private sources (e.g., banks, media, or external consultants), peers' actions also represent a relevant source of information (Bernard et al., 2020). According to models of observational learning (e.g. Bustamante and Frésard, 2021), firms use peers' actions to evaluate uncertain investment opportunities or identify valuable corporate policies. In this setting, when a firm joins a government program, information about the program, such as its availability and benefits, becomes more available to peers. Consequently, as firms observe their peers enrolling in government programs, they become more informed and are more likely to apply themselves.

We exploit the introduction in Italy of a government certification program, the Legality Rating (*Rating di Legalità*, hereafter "LR"). Through the LR, the Italian government aimed to increase the competitiveness of legitimate firms by providing a public signal of their compliance. Firms can freely request the LR if they did not receive regulatory sanctions, have no criminal infiltration, have managers without criminal convictions, and have sales over EUR 2 million in the year preceding the request.[2] Recent studies document several benefits of the LR, such as improved reputation, better access to financing, and increased access to public procurement contracts (Acconcia et al., 2021; La Rosa and Bernini, 2022; Pizzo, 2024).

Three features make the LR a valuable setting for investigating how information spillovers

[1] For instance, Humphries et al. (2020) and Custodio et al. (2021) document how information frictions limited SMEs' uptake of government sponsored credit programs in the U.S. and Portugal, respectively.

[2] For expositional purposes, I direct the reader to Sections 2.2.1 and 2.2.2 for a detailed overview of the Italian Legality Rating and public procurement framework, respectively.

increase government programs' take-up. First, despite the LR's apparent benefits, information frictions have severely limited its take-up among SMEs (ICA, 2022). Second, the LR is public, free, and not based on disclosing commercially sensitive information (e.g., financial data or patents). Thus, it provides a suitable channel for peer learning while mitigating the concern that direct or proprietary costs of disclosure discourage firms from applying. Third, because Italian SMEs are comparable to SMEs located in other European countries and in the U.S., we can potentially extend our inferences to other developed countries.

However, identifying information spillovers in product market networks requires overcoming two major empirical challenges. First, commonly used peer group definitions (e.g., firms in the same industry code) often fail to identify firms operating in the same product market (Aghamolla and Thakor, 2022b; Hoberg and Phillips, 2016). Second, peer effects are difficult to identify due to the reflection problem (Manski, 1993), which refers to the inability to separate the effect of information spillovers from other confounding factors affecting all firms within an industry.

We circumvent these challenges by developing a novel definition of product market peers based on firms' common bids for public procurement contracts. What makes our analysis possible is the availability of open public procurement data in the European Union (Duguay et al., 2023). We exploit a new public procurement dataset containing detailed information on over 2.3 million public procurement contracts awarded in Italy from 2012 to 2020. These data resemble those used in recent studies (e.g., Decarolis et al., 2021; Duguay et al., 2023), but with a significant difference - we can observe all firms bidding for a procurement contract, regardless of the award outcome.

We define product market peers as firms that bid for the same public procurement contracts as the focal firm in a quarter. This peer group definition provides three benefits. First, because we capture firms that supply the same product to public authorities, we can identify firms operating in the same product market space. Thus, we can overcome concerns that commonly used peer group definitions (e.g., firms in the same industry code) fail to identify economically-related peers (Aghamolla and Thakor, 2022b; Hoberg and Phillips, 2016; Jain, 2024). Accordingly, we document that our product market peer definition outperforms existing industry classifications in explaining variation in firm fundamentals, time-varying product markets, and across-industry relatedness. Second, since each firm bids for a different set of procurement contracts, we can construct firm-specific, time-varying, and partially overlapping peer groups, which addresses the reflection problem (Bramoullé et al., 2009; De Giorgi et al., 2010). Third, by leveraging granular contract and LR award dates, we can isolate the effect of product market interactions on LR's take-up from other sources of variation.

Using this peer group definition, we find strong evidence that information spillovers in product market networks increase the take-up of government programs. Specifically, after participating in a public procurement contract with an LR-certified peer, the focal firms' probability of obtaining the LR in the following four quarters increases by 71%. As the percentage of LR-certified peers increases, firms are more likely to obtain the LR. A one standard deviation increase in the percentage of LR-certified peers increases firms' probability of obtaining the LR by 11% relative to the mean. These effects hold after controlling for time-varying firm- and year-level shocks.

Because the LR can provide certified firms with financial (e.g., improved growth opportunities) and reputational benefits (e.g., by signaling firms' legitimacy), the observed information spillovers can emerge from two complementary mechanisms: competition or social legitimacy concerns. On the one hand, when a firm receives government support, it might gain a competitive advantage over its product market peers. In our setting, LR-certified firms might benefit from improved access to public procurement contracts or financing (Acconcia et al., 2021; Pizzo, 2024). Therefore, by observing how peers benefit from the LR, focal firms might obtain the LR to reduce their peers' competitive advantage (Cao et al., 2019). On the other hand, social legitimacy concerns might drive focal firms' certification decisions (Bursztyn and Jensen, 2015). Previous studies document that individuals conform to peer behavior to avoid social stigmatization (e.g., Frakes and Wasserman, 2021). In our setting, because the LR signals the lack of criminal convictions and ties with criminal organizations, not-yet-certified firms might obtain the LR to avoid social stigmatization from being regarded as criminal.

Our cross-sectional results suggest that competitive concerns are the main driver of focal firms' certification decisions. Specifically, we show that the effects are concentrated among firms operating in more competitive product markets. We proxy for competition using the percentage of open procedure procurement contracts (Duguay et al., 2023), i.e., contracts more open to qualified bidders. Furthermore, the focal firms' propensity to obtain the LR is higher after an LR-certified peer wins a procurement contract. Conversely, we perform two tests and fail to find evidence in favor of social legitimacy concerns driving focal firms' certification decisions. First, we do not observe any incremental take-up among firms located in high-Mafia areas, which are likely to face higher stigma from being non-certified. Second, we posit that social legitimacy concerns would increase when a greater share of geographically close peers obtains the certification. However, we find that geographical proximity to LR-certified peers has little effect on focal firms' subsequent certification decisions.

This study contributes to four streams of literature. First, this paper adds to the emerging literature examining the effects of information frictions on government programs' take-up (e.g., Gupta et al., 2023; Zwick, 2021). While information frictions might, in principle, improve targeting efficiency, they, in practice, impair firms that would benefit the most from accessing to government programs (Custodio et al., 2021; Humphries et al., 2020). Our study shows that information spillovers in product markets can mitigate these frictions and ultimately increase take-up rates. Thus, our study has relevant policy implications. Indeed, our results suggest that improved transparency on the recipients of these programs or the benefits (e.g., winning a procurement contract) can foster take-up through information spillovers.

Second, our study contributes to the literature on peer effects in program participation by identifying a novel channel through which firms learn about government programs: public procurement networks. Prior studies have examined workplace and family networks as pathways for information dissemination about government programs (e.g., employment subsidies and paternity leave as shown by Dahl et al., 2014; Mora-García and Rau, 2023, respectively). We extend this literature by documenting how procurement peers play a similar role for SMEs, acting as an information intermediary that facilitates program adoption.

Third, this study relates to the literature examining spillover effects of private firms' disclosure. A burgeoning literature has focused on the spillover effects of public firms' disclosures (see Roychowdhury et al., 2019). For instance, Seo (2021) finds that the frequency of peers' management forecasts has a strong impact on the frequency of the focal firm's management forecast. However, due to private firms' low transparency and reporting requirements, examining private firms' disclosure behavior is challenging (Beuselinck et al., 2023). Accordingly, prior studies primarily examine spillover effects from private firms' mandatory disclosures, e.g., by using the availability of financial statements or the number of disclosed line items (e.g., Breuer et al., 2022; Kim and Olbert, 2022). For instance, Bernard et al. (2021) find that private firms mimic incumbent firms' capital structure by learning from their financial disclosures. Our study complements this literature by documenting the spillover effects of private firms' certification decisions, a form of voluntary disclosure. Further, by using granular disclosure data, we can precisely investigate the timing through which private firms learn from peers.

Fourth, our study contributes to the literature on industry classifications. Given the limitations of traditional industry classifications in identifying economically related firms, prior studies developed classification systems based on e.g., analyst overlap (Kaustia and Rantala, 2021), 10-K business descriptions (Hoberg and Phillips, 2016), or EDGAR searches (Lee et al., 2015). However, due to data constraints, these classification systems apply only to U.S. publicly listed firms. Our study constructs a novel classification of product market peers for private firms.[3] While our measure exploits Italian public procurement data, it can be easily leveraged in many other settings where complete information on procurement bids is available (e.g., Brazil, Norway).

## 3.2 Conceptual Framework

According to seminal disclosure and signaling theory (Grossman, 1981; Milgrom, 1981; Spence, 1973), and assuming no information frictions, eligible firms should obtain the LR immediately and full unraveling should occur. Specifically, the highest-quality firms (e.g., firms with better compliance mechanisms) will obtain the LR first to avoid being pooled with average-quality firms. Observing the highest-quality firms' behavior, an increasing number of lower-quality firms will also obtain the certification. This unraveling process continues until all eligible firms obtain the certification. However, the unraveling prediction relies on several assumptions (e.g., costless disclosure, rational expectations) that appear demanding in our setting. Indeed, SMEs face significant information frictions, which might prevent them from obtaining the LR.

Previous literature identifies two main information frictions driving the incomplete take-up of government programs: lack of knowledge and transaction costs (Finkelstein and Notowidigdo, 2019). First, firms might not be aware of the availability of government programs. For instance, Humphries et al. (2020) show that small firms were less aware of the Paycheck Protection Program and, in turn, less likely to apply. Further, firms might under-estimate their eligibility or the magnitude of government programs' benefits (Bhargava and Manoli, 2015). Second, the perceived complexity of the application process might discourage firms from applying for the LR. For instance, in the context of the *SME-*

[3]To the best of our knowledge, the only study developing classification systems for private firms is Hoberg et al. (2024), who use firms' website disclosure to identify competitor networks.

*Leader Program* in Portugal, Bonfim et al. (2023) document that some managers are aware of the program but do not apply due to the complexity of the application. Relatedly, Zwick (2021) shows that only 37% of eligible firms claim their tax refund due to the perceived complexity of the tax code. These factors might be particularly acute for SMEs, which might not have specialized human resources for these activities (Humphries et al., 2020).

However, while these frictions may be large for an individual firm making decisions in isolation, the actions of product market peers might constitute an informal source of information, significantly reducing such frictions (Maturana and Nickerson, 2019). Consistent with models of observational learning (e.g., Bustamante and Frésard, 2021), several studies show that firms use the actions of their peers to evaluate uncertain investment opportunities or identify valuable corporate policies. For instance, Bernard et al. (2021) document that entrants use financial disclosures to learn about and mimic incumbents' capital structure. Besides, Cao et al. (2019) find that firms react to their product market peers' adoption of CSR practices by adopting similar CSR practices.

In this setting, after observing a product market peer obtaining the LR, focal firms' information about the LR might increase. Thus, by becoming more aware of the LR's availability, eligibility criteria and potential benefits, firms might become more likely to apply for the LR. However, while peers' decision to obtain the LR might generate an information spillover to not-yet-certified firms, the effect might not be salient enough to increase LR's take-up for two reasons. First, limited attention and procrastination might limit firms' ability to learn from peers' actions (Bourveau et al., 2020; Ponce et al., 2017). Second, firms might not perceive any net benefit in obtaining the LR. This perception might be due to a lack of trust in government certifications or an intangible cost in applying (Custodio et al., 2021). Hence, whether peers' decision to obtain the LR generates information spillovers and thus increases the LR's take-up is ultimately an empirical question.

## 3.3 Data & Research Design

### 3.3.1 Data

Our analysis relies on multiple data sources. We obtain the list of LR recipients from the ICA for each year from 2013 to 2020. The ICA data contains the name of the firm receiving the LR, its tax identifier, the rating score received, and the date of the LR award. Figure 3.1 provides some descriptive evidence on LR recipients. Panel A shows an upward trend in the number of LR recipients, suggesting that more firms are becoming aware of the LR's availability and benefits. Panel B shows the wide geographical variation of LR recipients across Italian provinces. The density of LR uptake is highest in Northern Italy, the most industrialized area of the country. However, the percentage of LR recipients is also high in some areas of Southern Italy, especially in regions with a strong presence of criminal organizations (e.g., Campania or Puglia). Next, we obtain financial data for a sample of 274,700 unique Italian firms (2,746,918 firm-year observations) for the period 2011-2020 from the AIDA database of Bureau van Dijk.

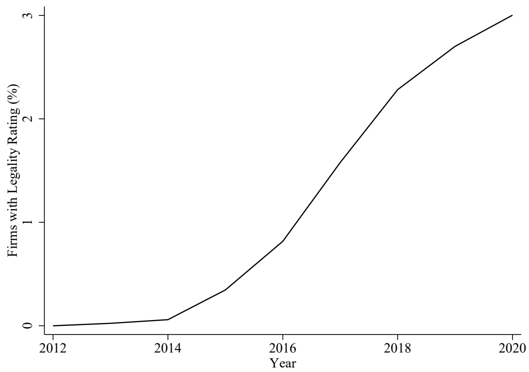
Our primary source of public procurement data is a novel public database of procurement contracts issued in Italy. This database covers over 2.3 million public procurement contracts awarded by 3,669 unique public authorities, such as municipalities, regional governments, and other public authorities (e.g., hospitals or universities). These data resemble those used in recent studies (e.g., Decarolis et al., 2021; Duguay et al., 2023), but with two major differences. First, we observe all firms bidding for each contract, regardless of the award outcome. Second, by individually scraping data from public authorities' websites, this dataset offers more extensive coverage of public procurement contracts than other publicly available procurement datasets (e.g., Digiwhist or ANAC).

Each contract is assigned a unique contract identifier. For each contract, we observe the year of the award, the contract value, the tax identifier of the public authority awarding the contract, and the procurement procedure. Because data quality and coverage dramatically increase from 2014, we focus on contracts awarded from 2014 to 2020, the last year for which we have accounting and LR data.

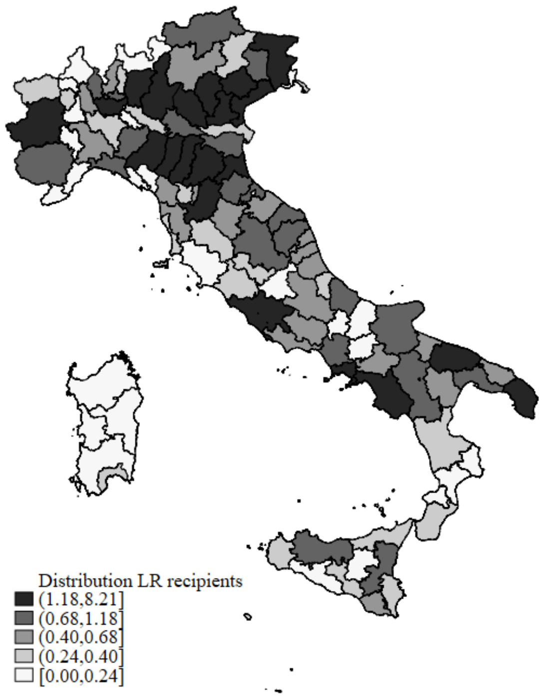
To construct our procurement peer groups, we focus on contracts involving at least two bidding firms for which we have accounting data available. Further, since our identification strategy leverages granular contract dates, we match these public procurement contracts to ANAC data, which provides the exact award dates of public procurement contracts. These criteria yield a sample of 26,417 unique public procurement contracts across 18,379 unique firms. We then exclude observations of ineligible firms (firms with sales under EUR 2 million in the preceding year) and aggregate information at the firm-quarter level. Our final merged dataset includes fast-moving information on LR awards and public procurement data for a wide cross-section of Italian SMEs, which allows for the precise identification of peer effects.

**Figure 3.1:** Legality Rating Recipients

This figure provides descriptive evidence on Legality Rating recipients. Panel A plots the yearly percentage of Legality Rating recipients over all firms in our sample. Panel B shows the geographic distribution of Legality Rating recipients across Italian provinces.




**(a)** LR Take-up




**(b)** Distribution of Legality Rating Recipients across Italian Provinces

### 3.3.2 Product Market Peer Definition

Identifying firms operating in the same product space is a key issue in both academic and practitioners' work (Kaustia and Rantala, 2021). To address this issue, agents often rely on industry classifications, which group economically-related firms into "industry"groups (Jain, 2024; Lee et al., 2024). Examples of industry classifications include the SEC's Standard Industry Classification (SIC), the North American Industry Classification System (NAICS), or Standard & Poor's Global Industry Classification Standard (GICS). In Europe, the most common industry classification is the European Commission's *Nomenclature statistique des activités économiques dans la Communauté européenne* (NACE), which classifies firms based on 2-digit divisions, 3-digit groups, 4-digit classes.[4]

Most industry classification systems share two key constraints: transitivity and non-overlapping peer groups. In a transitive classification system, if firms A and B have firm C as a peer, all three firms belong to the same group. In a non-overlapping system, each firm appears in only one group. While transitivity and non-overlapping systems simplify the structure of classification, they can lead to imprecise groupings (Hoberg and Phillips, 2016; Jain, 2024), especially for firms operating in multiple product markets.

We propose a novel definition of product market peers based on firms' common bids for public procurement contracts. Specifically, for any given firm $i$, we define a procurement peer firm $j$ as a firm that bids for the same public procurement contract in quarter $t$ as firm $i$. Since each firm bids for a different set of contracts each quarter, our peer group definition creates firm-specific, time-varying, and partially overlapping peer groups, which relax both constraints of industry classifications.

As an example of our peer group definition, consider Carl Zeiss as a focal firm. Carl Zeiss is a manufacturer and seller of optical systems (4-digit NACE: 46.6.9: *Wholesale of other machinery and equipment*). In Q3 of 2019, Carl Zeiss participated in a public procurement contract to provide the University of Milan with laboratory equipment. In this case, we identify as Carl Zeiss' peers the two additional firms participating in that same public procurement contract: Illumina (4-digit NACE: 33.2.0: *Installation of industrial machinery and equipment*) and Becton Dickinson (4-digit NACE: 21.1.0: *Manufacture of basic pharmaceutical products*), which ultimately won the contract. However, in that same quarter, Becton Dickinson participated in an additional public procurement contract for providing medical equipment to the Calabria Regional Authority. Due to Becton

[4]In Italy, ATECO codes transpose NACE codes. ATECO codes are developed by ISTAT, the equivalent of the U.S. Census Bureau, and exactly match NACE codes.

Dickison's different bidding behavior, it has two more peers (B. Braun and Pikdare, 4-digit NACE: 46.4.6: *Wholesale of pharmaceutical goods*) relative to Carl Zeiss. Thus, Carl Zeiss and Becton Dickison have distinct peer groups, which overlap only partially.

In the spirit of Hoberg and Phillips (2016), we argue that our procurement peer definition has four advantages relative to industry classifications. First, our definition captures better the changing nature of product markets. Due to changes in economic conditions (e.g., innovation or competition), firms adapt their product offerings over time. Because we identify the specific products a firm supplies in a given quarter, our definition identifies firms that operate in the same product space as the focal firm in a fast-moving manner. Consider, for example, Dream Distribution, which started as an electronic and telephone wholesaler (4-digit NACE is 46.5.2: *Wholesale of electronic and telecommunications equipment and parts*). Dream Distribution's NACE code implies that it should operate in the same product space as Motorola Solutions or Hitachi. However, during the COVID-19 pandemic, the firm shifted its business model and focused on the distribution of individual protection devices (e.g., FFP2 masks). Accordingly, we observe Dream Distribution participating and winning some procurement contracts for providing protection-related products to public authorities in 2020. Thus, relative to NACE codes, using procurement contracts to identify peer groups provides more timely information on the product market space a firm competes in.

Second, our procurement peer definition provides a better overview of the product market space of multi-segment firms. Because multi-segment firms offer multiple products and services, they are difficult to categorize through industry classifications (Jain, 2024). In this respect, by identifying product market peers for each product multi-segment firms provide, our procurement peer definition can improve multi-segment firms' classification quality. Consider, for example, Leonardo (4-digit NACE 30.3.0: *Manufacture of air and spacecraft and related machinery*), which operates in three segments: Aerospace, Defense, and Security. Due to its multiple segments, Leonardo offers a wide range of products, such as helicopters, maritime traffic control systems, or cybersecurity services. In this respect, we observe Leonardo participating in procurement contracts related to helicopter maintenance, surveillance, and software systems. Thus, its procurement peers include both firms operating helicopters (e.g., Airgreen or Alidaunia) and providing software solutions (e.g., IBM, Accenture Technology).

Third, our procurement peer definition captures across-industry relatedness, i.e., industries catering to the same customers. As shown by the Carl Zeiss example, industry classifications might treat as unrelated firms providing the same products. We investigate this aspect further in Panel A of Table 3.1, where we report descriptive statistics on our procurement peers. We find that, on average, only 36% (21%) of procurement peers belong to the same 2-digit (4-digit) NACE. We also investigate whether our measure captures firms operating in the same geographical area (Jennings et al., 2017). We find that, on average, procurement peers are geographically dispersed. The percentage of procurement peers headquartered in the same region is 37%, while the average distance between two procurement peers is 208 kilometers.

Fourth, in addition to this largely qualitative evidence, we empirically document that our procurement peer definition has higher informativeness than industry classifications. Following Hoberg and Phillips (2016), we examine the extent to which different classifications generate higher across-industry variation in firm fundamentals, i.e., size, profitability, growth, asset tangibility, and cash holdings. Specifically, for each classification (e.g., 4-digit NACE, Procurement peers), we first compute the mean of a given fundamental among its industry peers. We then compute the standard deviation of these means across all observations in our sample. If a classification provides more explanatory power, it should exhibit higher across-industry variation. Panel B of Table 3.1 reports the results. Across all fundamentals, we find that our procurement peer definition outperforms industry classifications. Across-industry variation in ROA is 0.029 and 0.033 for 3-digit NACE and 4-digit NACE, respectively. However, across-industry variation in ROA increases by 72% to 0.057 for our procurement peers definition.

While there are benefits to our approach, we recognize two primary limitations in our definition of procurement peers. First, our definition captures firms primarily transacting with public procurement authorities. While public procurement authorities are the largest buyer of goods and services in developed countries (Cohen and Li, 2020; Goldman, 2020), their purchases might be more prevalent for some specific products, e.g., for finished goods relative to raw materials. Second, limiting our sample to firms transacting with public procurement may create some selection bias. Indeed, in untabulated analysis, we find that firms participating in public procurement are, on average, larger than other Italian firms.

### 3.3.3 Empirical Methodology

A major empirical challenge when estimating peer effects is the reflection problem (Manski, 1993). According to Manski (1993), when the variable of interest (peers' actions)

## **Table 3.1:** Procurement Peers

Panel A reports descriptive statistics on the degree of correspondence between our procurement peers and alternative groupings. For any given firm $i$, we define a peer firm $j$ as a firm that bids for the same public procurement contract in quarter $t$ as firm $i$. We report the percentage of procurement peers belonging to the same industry classification (2-digit NACE, 3-digit NACE and 4-digit NACE) or headquartered in the same geographical area (Region, Province or Municipality). *Distance* indicates the average distance (in kilometers) between two procurement peers. Panel B reports across-industry standard deviations of firm fundamentals. For a given variable indicated in the left-hand column, across-industry standard deviations are computed as the standard deviation of the industry (equal-weighted) average of the given variable across all firms in our sample. *Size* is the natural logarithm of total assets. *ROA* is the ratio of operating income to lagged total assets. *ROE* is the ratio of net income to lagged equity. *Growth* is the annual sales growth rate. *Leverage* is the ratio of total liabilities to total assets. *PPE* is the ratio of property, plant and equipment to total assets. *Cash* is the ratio of cash and cash equivalents to total assets.

Panel A: Descriptive statistics

| Year | Same NACE code | | | Same Geographical Unit | | | Distance |
|---|---|---|---|---|---|---|---|
| | 2-digit | 3-digit | 4-digit | Region | Province | Municipality | |
| 2013 | 0.318 | 0.251 | 0.222 | 0.384 | 0.172 | 0.025 | 147 |
| 2014 | 0.360 | 0.261 | 0.208 | 0.305 | 0.168 | 0.066 | 197 |
| 2015 | 0.379 | 0.314 | 0.226 | 0.337 | 0.155 | 0.043 | 212 |
| 2016 | 0.398 | 0.320 | 0.246 | 0.429 | 0.219 | 0.042 | 186 |
| 2017 | 0.378 | 0.310 | 0.242 | 0.333 | 0.150 | 0.037 | 221 |
| 2018 | 0.317 | 0.175 | 0.135 | 0.409 | 0.134 | 0.033 | 179 |
| 2019 | 0.411 | 0.326 | 0.265 | 0.358 | 0.170 | 0.049 | 259 |
| 2020 | 0.389 | 0.318 | 0.303 | 0.364 | 0.130 | 0.022 | 246 |
| All Sample | 0.361 | 0.264 | 0.214 | 0.371 | 0.150 | 0.038 | 208 |

Panel B: Across-industry variation

| | Size | ROA | ROE | Growth | Leverage | PPE | Cash |
|---|---|---|---|---|---|---|---|
| 2-digit NACE | 0.511 | 0.023 | 0.088 | 0.183 | 0.062 | 0.080 | 0.038 |
| 3-digit NACE | 0.586 | 0.029 | 0.0105 | 0.210 | 0.072 | 0.087 | 0.043 |
| 4-digit NACE | 0.621 | 0.033 | 0.124 | 0.235 | 0.080 | 0.094 | 0.045 |
| Procurement Peers | 1.209 | 0.0571 | 0.289 | 0.316 | 0.139 | 0.111 | 0.080 |

is linearly dependent on other regressors, it is difficult to disentangle the peer effect from peers' characteristics. In our setting, the reflection problem results in the impossibility of separating the effect of peers' certification decision from common shocks or characteristics affecting all firms within an industry.

Two features of our data mitigate the reflection problem. First, by identifying firms bidding for the same procurement contract as the focal firm, we can construct time-varying and partially overlapping peer groups, which solve the reflection problem (Bramoullé et al., 2009; De Giorgi et al., 2010). Indeed, the reflection problem holds as long as all peers within a group have the same set of peers. When peer groups overlap only partially, the peer action regressor varies within the same group. Since firm $i$ and firm $j$'s peer groups do not overlap, we can identify the effect of having an LR-certified peer for firm $i$, relative to firm $j$, which has no LR-certified peers (Aghamolla and Thakor, 2022b).

Second, due to our highly granular disclosure data, we include firm-year fixed effects across all specifications. Thus, we remove the influence of (yearly) shocks to the focal firm that might drive the certification decisions. Further, using quarter-year fixed effects controls for quarter-specific shocks affecting all firms' LR take-up. Finally, since the requirements to participate in public procurement significantly overlap with those required for the LR, we can control for firms' underlying eligibility for the LR.

Hence, following Aghammola and Thakor (2022b), we estimate the following OLS specification on our set of procurement peers:

$$Certification_{i,t} = \beta_0 + \beta_1 CertifiedPeer_{i,[t-4,t-1]} + \gamma X_{i,t-1} + \alpha_{iy} + \alpha_t + \epsilon_{i,t} \tag{3.1}$$

where *Certification* is an indicator taking values of one in the quarter a firm obtains the LR, and zero otherwise. We remove a firm from our sample after it obtains the LR. The main explanatory variable, *Certified Peer* is an indicator taking values of one if a firm participated in a public procurement contract with an LR-certified peer in the past four quarters, and zero otherwise. We also use a specification where we replace the binary *Certified Peer* with a continuous measure, *% Certified Peers*, computed as the average share of LR-certified peers in the past four quarters. $X_{i,t-1}$ is a vector of time-varying controls. Specifically, we control for the number of procurement contracts a firm participates in a given quarter, and the related number of procurement peers. We cluster standard errors at the firm-level. The coefficient ($\beta_1$) captures the focal firm's propensity to obtain the certification after participating in a public procurement contract with an

LR-certified peer relative to when it participates in a public procurement contract without LR-certified peers, taking into account time-varying firm- and year-level shocks.

Table 3.2 reports summary statistics for our final sample. Consistent with the notion that LR's take-up is low, the focal firm's propensity to obtain the LR in a given quarter is 0.8%. Conversely, the average likelihood of having an LR-certified peer in a given quarter is 8.9%, suggesting that a sizeable number of sample firms observe a peer firm obtaining the LR. Further, in line with previous studies examining Italian private firms (e.g., Bianchi et al., 2022; Chircop et al., 2023), the median firm is mid-sized (EUR 4.9 million in assets and 25 employees).[5]

**Table 3.2:** Summary statistics

This table reports summary statistics for the variables used in the analysis. *Certification* is an indicator taking values of one in the quarter a firm obtains the LR, and zero otherwise. *Certified Peer* is an indicator taking values of one if a firm participated in a public procurement contract with an LR-certified peer in the past four quarters, and zero otherwise. *% Certified Peers* is the average proportion of LR-certified peers in the past four quarters. *Number of Contracts* is the natural logarithm of one plus the number of procurement contracts a firm participates in a given quarter. *Number of Peers* is the natural logarithm of one plus the number of procurement peers in a given quarter. *Open* is an indicator taking values of one for firms in the top tercile of the full sample distribution of open procedure contracts. *Winner Peer* is an indicator taking values of one for firms that observe an LR-certified peer winning a public procurement contract. *Proximity* is the inverse of the average distance (in kilometers) between the focal firm and all of its LR-certified peers. We set *Proximity* equal to zero for firms without LR-certified peers. For ease of interpretation, we divided *Proximity* by one thousand. *High-crime* is an indicator taking values of one if the focal firm is headquartered in one of the three regions where Mafia originated, i.e., Sicily, Campania, or Calabria. *Total Assets* is the value of total assets (in thousand euros). *Employees* is the number of employees.

| | N | Mean | SD | Median |
|---|---|---|---|---|
| Certification | 374,400 | 0.008 | 0.087 | 0.000 |
| Certified Peer | 374,400 | 0.089 | 0.285 | 0.000 |
| % Certified Peers | 374,400 | 0.019 | 0.088 | 0.000 |
| Number of Contracts | 374,400 | 0.091 | 0.311 | 0.000 |
| Number of Peers | 374,400 | 0.197 | 0.712 | 0.000 |
| Open | 374,400 | 0.249 | 0.433 | 0.000 |
| Winner Peer | 374,400 | 0.127 | 0.333 | 0.000 |
| Proximity | 374,400 | 0.128 | 1.570 | 0.000 |
| High-crime | 374,400 | 0.068 | 0.252 | 0.000 |
| Total Assets (EUR 000) | 339,449 | 261,040 | 6,685,821 | 4,908 |
| Employees | 321,168 | 150 | 1,257 | 25 |

[5]For a small number of observations, accounting variables are missing. This is broadly due to two factors. First, some firms might not file their financial statements in a given year. Second, while we construct a balanced panel for each firm (32 quarters), we do not observe accounting data after a merger or a bankruptcy. In untabulated results, we show that our results hold if we restrict to observations with non-missing accounting data

## 3.4 Results

### 3.4.1 Baseline Result

We begin our analysis by examining how information spillovers in product market networks improve the take-up of the LR. Table 3.3 reports the results of this analysis. In Panel A, the main explanatory variable is *Certified Peer*, an indicator taking values of one if a firm participated in a public procurement contract with an LR-certified peer in the past four quarters, and zero otherwise. Column (1) reports the result only with quarter-year fixed effects. We add firm and firm-year fixed effects separately in columns (2) and (3). Column (4) reports the full specification with controls, firm-year and quarter-year fixed effects.

Across all specifications, we observe a positive and statistically significant relationship between the focal firm's certification decision and the previous certification decision of a product market peer. The estimated effect is sizeable. In our most stringent specification, the coefficient in Column (4) suggests that, after participating in a public procurement contract with an LR-certified peer, the average firms' probability to obtain the LR increases from a baseline probability of 0.7% to 1.2% per quarter. Thus, participating in a public procurement contract with an LR-certified peer increases the average firms' probability to obtain the LR by 71%.

In Panel B, we use as the main explanatory variable *% Certified Peers*, i.e., the average percentage of LR-certified peers between *t-4* and *t-1*. Consistent with our previous results, we document that the focal firm's propensity to obtain the LR increases when a larger percentage of product market peers have the LR. The estimated coefficient of 0.009 indicates that one standard deviation increase in the proportion of LR-certified peers (i.e., from 1% to 10%) increases the focal firm's propensity to obtain the LR by 9 basis points, or 11% relative to the mean value of the dependent variable.

### 3.4.2 Mechanism

Our main results show that information spillovers in product market networks increase the take-up of government programs. Specifically, by interacting with LR recipients in product markets, firms might become aware of the certification, its eligibility criteria, and the potential benefits. In this section, we explore two complementary mechanisms behind the observed information spillovers: competitive and social legitimacy concerns

**Table 3.3:** Peer Effects in Take-up of Government Certification

This table reports estimated coefficients for Equation (3.1). *Certification* is an indicator taking values of one in the quarter a firm obtains the LR, and zero otherwise. *Certified Peer* is an indicator taking values of one if a firm participated in a public procurement contract with an LR-certified peer in the past four quarters, and zero otherwise. *% Certified Peers* is the average proportion of LR-certified peers in the past four quarters. *Number of Contracts* is the natural logarithm of one plus the number of procurement contract a firm participates in a given quarter. *Number of Peers* is the natural logarithm of one plus the number of procurement peers in a given quarter. Standard errors are clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Discrete Certified Peer Indicator

| | (1)<br>Certification | (2)<br>Certification | (3)<br>Certification | (4)<br>Certification |
|---|---|---|---|---|
| Certified Peer | 0.010*** | 0.008*** | 0.005*** | 0.005*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Number of Contracts | | | | 0.000 |
| | | | | (0.001) |
| Number of Peers | | | | 0.000 |
| | | | | (0.000) |
| Firm FE | No | Yes | No | No |
| Quarter-Year FE | Yes | Yes | Yes | Yes |
| Firm-Year FE | No | No | Yes | Yes |
| Adjusted $R^2$ | 0.0039 | 0.0375 | 0.1330 | 0.1331 |
| Observations | 374,400 | 374,385 | 373,770 | 373,770 |

Panel B: Continuous Certified Peers Measure (%)

| | (1)<br>Certification | (2)<br>Certification | (3)<br>Certification | (4)<br>Certification |
|---|---|---|---|---|
| % Certified Peers | 0.026*** | 0.023*** | 0.010*** | 0.009** |
| | (0.003) | (0.003) | (0.004) | (0.004) |
| Number of Contracts | | | | 0.000 |
| | | | | (0.001) |
| Number of Peers | | | | 0.001 |
| | | | | (0.000) |
| Firm FE | No | Yes | No | No |
| Quarter-Year FE | Yes | Yes | Yes | Yes |
| Firm-Year FE | No | No | Yes | Yes |
| Adjusted $R^2$ | 0.0036 | 0.0374 | 0.1330 | 0.1330 |
| Observations | 374,400 | 374,385 | 373,770 | 373,770 |

(Aghamolla and Thakor, 2022b; Bursztyn and Jensen, 2015; Cao et al., 2019).

#### 3.4.2.1 Competition

By design, government programs aim to benefit recipient firms, e.g., through improved access to financing. Hence, when a firm takes up a government program, it gains a competitive advantage over its product market peers that do not take up the government program. In our setting, prior studies show that LR recipients benefit from improved access to financing and public procurement (Acconcia et al., 2021; Pizzo, 2024). As a consequence, by observing how peers benefit from the LR, firms may apply for the LR to reduce the LR-certified peers' competitive advantage (Cao et al., 2019).

We explore the importance of competition concerns in two ways. First, we study how information spillovers vary depending on the degree of competition in the product market in which the focal firm operates. The underlying assumption is that firms operating in more competitive product markets have greater incentives to acquire information from peers and adopt peer behavior. To do so, we leverage the richness of our data and construct a measure of product market competition based on the number of open procedure procurement contracts a firm bids for. Specifically, for each firm, we compute the average percentage of open procedure contracts that the firm bids for during our sample period. Then, we construct an indicator variable *Open* taking values of one for firms in the top tercile of this distribution, and zero otherwise.

In Panel A of Table 3.4, we augment Equation (3.1) by interacting *Open* with our main explanatory variables, i.e., *Certified Peer* and *% Certified Peers*. While the coefficients on *Certified Peer* and *% Certified Peers* capture the average effect of participating in a public procurement contract with an LR-certified peer on the focal firm's subsequent certification decision, the interaction terms capture the incremental effect for firms operating in competitive product markets. The interacted coefficient is positive and significant, while the main effect is non-significant across both specifications. This result suggests that the LR peer effect primarily occurs among firms operating in competitive product markets.

Next, we study whether information spillovers are greater for firms observing an LR-certified peer winning a public procurement contract. To do so, we augment Equation (3.1) by interacting our explanatory variables with *Winner Peer*, an indicator taking values of one for focal firms that observe an LR-certified peer winning a public procurement contract. Panel B of Table 3.4 reports the results. We again find that focal firms' propensity to obtain the LR is 3 times higher when an LR-certified peer wins a public procurement contract, from 0.3 pp to 0.9 pp, suggestive of the competition mechanism. A similar relative effect can be found in the *% Certified Peers* regression, although the coefficients are non-significant.

#### 3.4.2.2 Social Legitimacy

A large literature in economics documents that individuals conform to peer behavior to avoid social stigmatization (e.g., Bursztyn and Jensen, 2015). In our setting, the LR signals that firms do not have criminal convictions or ties with criminal organizations. As a consequence, as more product market peers obtain the certification, not-yet-certified firms might obtain the LR to avoid social stigmatization from being regarded as criminal.

To explore the role of social legitimacy concerns, we conduct two empirical tests. First, we hypothesize that social stigmatization from being non-certified might be greater for firms located in areas with significant criminal infiltration. Thus, we interact our main explanatory variable with *High-Mafia*, an indicator taking values of one for firms headquartered in the three regions where Mafia originates, i.e., Sicily, Calabria, or Campania. Panel A of Table 3.5 reports the results. We do not observe any incremental information spillover for firms headquartered in areas with high criminal infiltration.

Second, we study how geographical proximity to LR-certified peers affects focal firms' certification decisions. The underlying assumption is that, if a greater share of LR-certified peers operates in the same geographical area as the focal firm, social stigmatization from being non-certified increases. To capture geographic proximity with LR-certified peers, we construct a variable (*Proximity*) equal to the inverse of the average geographical distance (in kilometers) between the focal firm and its LR-certified peers. We set *Proximity* equal to 0 for firms without LR-certified peers. Panel B of Table 3.5 reports the results obtained when interacting our main explanatory variables with *Proximity*. Across all specifications, the interacted coefficients are not statistically significant. Overall, our cross-sectional evidence suggests that competitive concerns are the main driver of focal firms' decision to obtain the LR. Conversely, we do not find evidence that social legitimacy concerns stemming from geographically proximate certified peers or from the focal firm's geographical location drive the documented results.

**Table 3.4:** Competition Cross-Sectional Splits

This table analyzes the role of competition concerns in driving information spillovers. *Certification* is an indicator taking values of one in the quarter a focal firm obtains the LR, and zero otherwise. *Certified Peer* is an indicator taking values of one if a firm participated in a public procurement contract with an LR-certified peer in the past four quarters, and zero otherwise. *% Certified Peers* is the average proportion of LR-certified peers in the past four quarters. In Panel A, *Open* is an indicator for firms in the top tercile of the full-sample distribution of open procedure contracts. In Panel B, *Winner Peer* is an indicator taking values of one for firms that observe an LR-certified peer winning a public procurement contract. Controls include *Number of Contracts* and *Number of Peers*. Standard errors are clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Competitiveness of Bids

| | (1) Certification | (2) Certification |
|---|---|---|
| Certified Peer | 0.003 | |
| | (0.002) | |
| Certified Peer × Open | 0.005* | |
| | (0.003) | |
| % Certified Peers | | 0.003 |
| | | (0.004) |
| % Certified Peers × Open | | 0.019** |
| | | (0.009) |
| Controls | Yes | Yes |
| Firm-Year FE | Yes | Yes |
| Quarter-Year FE | Yes | Yes |
| Adjusted $R^2$ | 0.1331 | 0.1330 |
| Observations | 373,770 | 373,770 |

Panel B: Winning Peers

| | (1) Certification | (2) Certification |
|---|---|---|
| Certified Peer | 0.003** | |
| | (0.002) | |
| Certified Peer × Winner Peer | 0.006* | |
| | (0.004) | |
| % Certified Peers | | 0.005 |
| | | (0.005) |
| % Certified Peers × Winner Peer | | 0.010 |
| | | (0.008) |
| Controls | Yes | Yes |
| Firm-Year FE | Yes | Yes |
| Quarter-Year FE | Yes | Yes |
| Adjusted $R^2$ | 0.1331 | 0.1330 |
| Observations | 373,770 | 373,770 |

## **Table 3.5:** Social Legitimacy Cross-Sectional Splits

This table analyzes the role of social legitimacy concerns in driving information spillovers. *Certification* is an indicator taking values of one in the quarter a firm obtains the LR, and zero otherwise. *Certified Peer* is an indicator taking values of one if a firm participated in a public procurement contract with an LR-certified peer in the past four quarters, and zero otherwise. *% Certified Peers* is the average proportion of LR-certified peers in the past four quarters. In Panel A, *High-crime* is an indicator taking values of one if the focal firm is headquartered in one of the three regions where Mafia originated, i.e., Sicily, Campania, or Calabria. In Panel B, *Proximity* is the inverse of the average distance (in kilometers) between the focal firm and all of its LR-certified peers. We set *Proximity* equal to zero for firms without LR-certified peers. For ease of interpretation, we divided *Proximity* by one thousand. Controls include *Number of Contracts* and *Number of Peers*. Standard errors are clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: High Crime Area

| | (1) Certification | (2) Certification |
|---|---|---|
| Certified Peer | 0.005*** | |
| | (0.001) | |
| Certified Peer × High Crime | -0.007 | |
| | (0.008) | |
| % Certified Peers | | 0.011*** |
| | | (0.004) |
| % Certified Peers × High Crime | | -0.039 |
| | | (0.024) |
| Controls | Yes | Yes |
| Firm-Year FE | Yes | Yes |
| Quarter-Year FE | Yes | Yes |
| Adjusted $R^2$ | 0.1331 | 0.1330 |
| Observations | 373,770 | 373,770 |

Panel B: Geographic Peers

| | (1) Certification | (2) Certification |
|---|---|---|
| Certified Peer | 0.005*** | |
| | (0.001) | |
| Certified Peer × Proximity | -0.000 | |
| | (0.000) | |
| % Certified Peers | | 0.009** |
| | | (0.004) |
| % Certified Peers × Proximity | | -0.001 |
| | | (0.000) |
| Controls | Yes | Yes |
| Firm-Year FE | Yes | Yes |
| Quarter-Year FE | Yes | Yes |
| Adjusted $R^2$ | 0.1330 | 0.1330 |
| Observations | 373,770 | 373,770 |

## 3.5 Robustness Tests

In this section, we perform a battery of tests to examine the sensitivity of our results to different empirical specifications.

### 3.5.1 Instrumental Variable Specification

To mitigate the reflection problem, our main analysis exploits the partial overlap of procurement peer groups and a granular fixed effect structure. However, our procurement peer definition provides a natural setting to use an Instrumental Variable (IV) specification. Specifically, in the spirit of Aghamolla and Thakor (2022b) and De Giorgi et al. (2010), we instrument for the focal firm's certification decision by using the certification decision of a peer of the peer firm that is not a direct peer of the focal firm. Using an IV specification allows us to further mitigate concerns of correlated effects affecting all firms within a group.

To illustrate this strategy, consider the previous Carl Zeiss example: Becton Dickinson is a peer of Carl Zeiss and B. Braun, but B. Braun and Carl Zeiss are not peers. Assuming Carl Zeiss is our focal firm, our strategy instruments for Becton Dickinson's effect on Carl Zeiss through the effect of B. Braun's decision on Becton Dickinson. Since Carl Zeiss and B. Braun are not peers, the exclusion restriction plausibly holds in this specification (Aghamolla and Thakor, 2022b).

We report the results of our IV specification in Table 3.6. In Columns (1) and (3), we report our first-stage results. We find a positive and statistically significant relationship between *Certified Peer* and *Certified Peer of Peers*. The F-stat values of 408.95 and 3462.55, respectively, suggest the relevance condition is satisfied. In Columns (2) and (4), we report our second-stage results. We observe a positive and significant coefficient for *Certified Peer*.[6] Overall, these results further confirm that correlated effects do not bias our main result.

[6] The coefficients of our IV estimates are larger than those obtained using OLS. Two factors mitigate potential concern around this difference in coefficients. First, this result is relatively common when using instrumental variable specification with partially overlapping peer groups (e.g., Aghamolla and Thakor, 2022b; De Giorgi et al., 2010). Second, in this setting, the OLS estimate does not necessarily overestimate the magnitude of the effects. Indeed, as De Giorgi et al. (2010) note, because peers share only a subset of group shocks and different group shocks can have different signs, predicting in advance the magnitude of the OLS estimator relative to the IV is impossible.

**Table 3.6:** Instrumental Variable Specification

This table reports estimated coefficients from using an instrumental variable specification. *Certification* is an indicator taking values of one in the quarter a firm obtains the LR, and zero otherwise. *Certified Peer* is an indicator taking values of one if a firm participated in a public procurement contract with an LR-certified peer in *t-1*, and zero otherwise. *Certified Peer of Peers* is an indicator taking values of one if firm $i$ has a procurement peer firm that in turn has a procurement peer (but that is not a peer to firm $i$) that obtained the certification in t-2. $\widehat{\textit{Certified Peer}}$ is instrumented *Certified Peer*. Controls include *Number of Contracts* and *Number of Peers*. Standard errors are clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1) First Stage Certified Peer | (2) Second Stage Certification | (3) First Stage Certified Peer | (4) Second Stage Certification |
|---|---|---|---|---|
| $\widehat{\text{Certified Peer}}$ | | 0.104*** | | 0.052*** |
| | | (0.023) | | (0.011) |
| Certified Peer of Peers | 0.046*** | | 0.097*** | |
| | (0.002) | | (0.002) | |
| Controls | No | No | Yes | Yes |
| Firm-Year FE | Yes | Yes | Yes | Yes |
| Quarter-Year FE | Yes | Yes | Yes | Yes |
| F-stat | 408.95 | | 3462.55 | |
| Observations | 348,173 | 348,173 | 348,173 | 348,173 |

### 3.5.2 Firm-year Results

By exploiting granular LR and contract award dates, our main analysis examines LR's take-up at the firm-quarter level. In this section, we document that our results are robust to using a firm-year specification. Table B.1 reports the results. In Panel A, the main explanatory variable is a dummy taking values of one after a firm participates in a public procurement contract with an LR-certified peer, and zero otherwise. In Panel B, we use as main explanatory variable the percentage of LR-certified peers in *t-1*.

The results are consistent with our main analysis. Firms are, on average, 150% more likely to obtain the LR after observing the certification decision of a peer bidding for the same procurement contracts. Specifically, firms' propensity to obtain the LR increases to 5%, from an unconditional baseline propensity of 1.8% per year.

### 3.5.3 Alternative Fixed Effects

A potential concern in peer effects models is that other common shocks unrelated to information spillovers might drive firms' LR take-up. For instance, some peers might operate in the same geographic area. Hence, they might obtain the LR in response to a local shock, such as an anti-mafia enforcement action. Alternatively, some procurement authorities might value more LR-certified firms. Therefore, firms bidding for those contracts might obtain the LR because the procurement authority values it.

While using partially overlapping groups and firm-year fixed effects already mitigates this concern, we more directly attempt to control for this by including additional fixed effects in our main specification. As suggested by Armstrong et al. (2022), we triangulate our results across different fixed effects structures. Specifically, we augment Equation (3.1) with municipality-year, and procurement authority fixed effects.[7] As shown by Table B.2, the coefficients of *Certified Peer* and *% Certified Peers* remain positive and statistically significant, with a magnitude similar to that of previous tests.

### 3.5.4 Logit Specification

To investigate the effect of information spillovers on firms' take-up of the LR, we use a dynamic OLS model in our main analysis. The OLS model allows us to include firm-year and quarter-year fixed effects to control for time-varying firm- and year-level shocks, respectively. Conversely, logit models with fixed effects might be difficult to interpret and generate biased coefficients. In this section, we document that our results are robust to re-estimating Equation (3.1) with a logit specification. Table B.3 reports the results. Consistent with our previous results, the coefficients of *Certified Peer* and *% Certified Peers* are positive and statistically significant across all specifications.

## 3.6 Conclusion

SMEs constitute the overwhelming majority of firms in Europe and the U.S., accounting for a significant share of the total GDP and workforce (Beuselinck et al., 2023). To alleviate the constraints SMEs face in accessing capital and gaining visibility, governments have introduced various programs, such as loan guarantees, certifications, and small business grants. However, despite the significant resources devoted to these initiatives, SME participation often remains low due to limited awareness, misunderstandings about the benefits, and perceived complexities in the application process (Custodio et al., 2021; Gassen and Muhn, 2025; Humphries et al., 2020). These challenges raise concerns about the effectiveness of these programs in reaching their intended beneficiaries.

In this study, we investigate whether information spillovers in product market networks increase the take-up of government programs. For this purpose, we exploit the

[7]When the focal firm bids for multiple contracts with multiple public authorities, we select the public authority with which the focal firm transacts the most.

introduction of a government certification, the Legality Rating, in Italy. Using a novel definition of product market peers based on firms' bids for public procurement contracts, we show that firms are more likely to obtain the certification after competing with an LR-certified peer in a public procurement contract. The estimated effect is sizeable: after observing a peer obtain the LR certification, the average firms' propensity to obtain the LR in the following four quarters increases by 71%. Cross-sectional tests reveal that competitive concerns are the main driver of the documented effects, as opposed to social legitimacy concerns.

When interpreting these findings, two institutional limitations should be considered. First, the generalizability of our results from Italian SMEs to other contexts may be questioned, particularly given Italy's unique institutional environment. Italy has higher corruption rates and a greater prevalence of criminal activity compared to most developed countries (Transparency International, 2023), which makes the Legality Rating (LR) certification especially salient. However, the take-up rates for the LR are comparable to those observed for similar government programs in other developed economies (Bonfim et al., 2023; Zwick, 2021), suggesting that our findings may hold in broader settings. As such, Italy provides a relevant and reasonable context for evaluating mechanisms to enhance government program participation.

Second, our analysis focuses on firms engaged in public procurement, which, while addressing key empirical challenges, may limit the representativeness of our sample relative to the broader population of Italian SMEs. Firms participating in public procurement are often concentrated in specific industries (e.g., services or finished goods) and tend to be larger on average. However, smaller firms, which typically face greater information frictions, are likely to experience even stronger information spillover effects than those documented in our study. This suggests that our results may represent a conservative estimate of the broader potential for peer-driven information spillovers.

Future research could explore several avenues to build on our findings. First, while this study focuses on product market networks in a public procurement context, future studies could investigate whether similar peer-driven information spillovers exist in other types of networks, such as across supply chains. Second, future research could explore the dynamics of information spillovers in emerging markets or countries with different regulatory environments to assess the external validity of our findings. To the extent that complete procurement data is publicly available in other markets (e.g., Brazil), our methodology can be directly applied. Finally, while this study identifies competition as a key driver of program take-up, future work could investigate how other firm-level factors (financial constraints, managerial characteristics, or organizational learning capabilities) moderate these spillover effects. We leave these questions to future research.

# Appendix B

## B.1 Supplementary Material to Chapter 3

**Table B.1:** Baseline Analysis: Yearly Frequency

This table reports estimated coefficients for Equation (3.1). *Certification* is an indicator taking values of one in the year a firm obtains the LR, and zero otherwise. *Certified Peer* is an indicator taking values of one after a firm participated in a public procurement contract with an LR-certified peer, and zero otherwise. *% Certified Peers* is the average proportion of LR-certified peers. Controls include *Number of Contracts*, *Number of Peers*, *Size*, *ROA*, *Growth*, *Leverage*, *Cash* and *PPE*. Standard errors are clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Discrete Certified Peer Indicator

| | (1)<br>Certification | (2)<br>Certification | (3)<br>Certification |
|---|---|---|---|
| Certified Peer | 0.025*** | 0.027*** | 0.022*** |
| | (0.001) | (0.001) | (0.001) |
| Controls | No | No | Yes |
| Firm FE | No | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0127 | 0.0954 | 0.0976 |
| Observations | 216,648 | 216,648 | 216,648 |

Panel B: Continuous Certified Peers Measure (%)

| | (1)<br>Certification | (2)<br>Certification | (3)<br>Certification |
|---|---|---|---|
| % Certified Peers | 0.077*** | 0.084*** | 0.073*** |
| | (0.004) | (0.005) | (0.005) |
| Controls | No | No | Yes |
| Firm FE | No | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0110 | 0.0951 | 0.0981 |
| Observations | 216,648 | 216,648 | 216,648 |

## **Table B.2:** Additional Fixed Effects

This table reports estimated coefficients for Equation (3.1) using alternative fixed effects structures. *Certification* is an indicator taking values of one in the quarter a firm obtains the LR, and zero otherwise. *Certified Peer* is an indicator taking values of one if a firm participated in a public procurement contract with an LR-certified peer in the past four quarters, and zero otherwise. *% Certified Peers* is the average proportion of LR-certified peers in the past four quarters. Controls include *Number of Contracts* and *Number of Peers*. Standard errors are clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1)<br>Certification | (2)<br>Certification | (3)<br>Certification | (4)<br>Certification |
|---|---|---|---|---|
| Certified Peer | 0.005*** | 0.005*** | | |
| | (0.001) | (0.001) | | |
| % Certified Peers | | | 0.009** | 0.010** |
| | | | (0.004) | (0.004) |
| Controls | Yes | Yes | Yes | Yes |
| Quarter-Year FE | Yes | Yes | Yes | Yes |
| Firm-Year FE | Yes | Yes | Yes | Yes |
| Municipality-Year FE | Yes | No | Yes | No |
| Procurement Authority FE | No | Yes | No | Yes |
| Adjusted $R^2$ | 0.0649 | 0.1454 | 0.0649 | 0.1453 |
| Observations | 372,240 | 201,041 | 372,240 | 201,041 |

## **Table B.3:** Robustness Test: Logit

This table reports estimates coefficients for Equation (3.1) using a logistic regression. *Certification* is an indicator taking values of one in the year a firm obtains the LR, and zero otherwise. *Certified Peer* is an indicator taking values of one after a firm participates in a public procurement contract with an LR-certified peer, and zero otherwise. *% Certified Peers* is the average proportion of LR-certified peers. Controls include *Number of Contracts* and *Number of Peers*. Standard errors are clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Discrete Certified Peer Indicator

| | (1)<br>Certification | (2)<br>Certification | (3)<br>Certification |
|---|---|---|---|
| Certified Peer | 0.764***<br>(0.047) | 1.964***<br>(0.073) | 1.880***<br>(0.078) |
| Controls | No | No | Yes |
| Firm FE | No | Yes | Yes |
| Quarter-Year FE | Yes | Yes | Yes |
| Observations | 374,400 | 48,367 | 48,367 |

Panel B: Continuous Certified Peers Measure (%)

| | (1)<br>Certification | (2)<br>Certification | (3)<br>Certification |
|---|---|---|---|
| % Certified Peers | 1.551***<br>(0.118) | 5.723***<br>(0.279) | 5.124***<br>(0.279) |
| Controls | No | No | Yes |
| Firm FE | No | Yes | Yes |
| Quarter-Year FE | Yes | Yes | Yes |
| Observations | 374,400 | 48,367 | 48,367 |

# Chapter 4

# Learning When Losing: Evidence from Public Procurement Contracts

Authors: Justin Chircop; Carmine Pizzo

## 4.1 Introduction

In most developed economies, public procurement accounts for a significant fraction of the countries' economic activity. For example, in 2020, U.S. government procurement spending represented about 11% of the U.S. gross domestic product ("GDP"). Along with constituting a valid policy tool for governments, public procurement is a crucial channel for transferring resources from governments to firms (Bosio et al., 2022). Given the impact of public procurement on firm growth (Ferraz et al., 2015; Hvide and Meling, 2022), regulators encourage firms' participation in the public procurement process (e.g., European Court of Auditors, 2023).

In this study, we investigate the effect of participating and losing a bid for a public procurement contract on firms' tax avoidance.[1] Publication of the outcome of a public procurement contract provides participating firms with information about their competitiveness relative to peer firms.[2] Losing firms learn that they are uncompetitive relative to the winning firm, while the winning firm learns that it is competitive relative to peer firms. Given that there are potentially multiple losing firms for each winning firm, examining how public procurement participation affects losing firms' tax avoidance is economically important.

We expect firms that lose bids for public procurement contracts to undertake greater tax avoidance activities. After losing a bid, firms learn that they face more competition than previously anticipated. Thus, losing firms have greater incentives to undertake tax avoidance to improve their competitive position. Consistent with this argumentation, Cai and Liu (2009) suggest that more disadvantaged firms and firms facing stiffer competition undertake tax avoidance activity to increase their investment money to improve their competitiveness.

However, it is unclear whether incentives arising from losing a bid for a government

[1] We refer to tax avoidance as a set of activities firms undertake to reduce the amount of taxes paid (Chircop et al., 2023). Similar to prior literature (e.g., Khan et al., 2017), we do not identify the type of tax avoidance activity (e.g., investing in a tax advantageous asset or participating in aggressive tax avoidance schemes) nor attempt to distinguish between legal or illegal tax avoidance (i.e., tax evasion) activities. Further, the distinction between legal and illegal tax avoidance activities is difficult since the tax treatment of specific activities is often unclear and requires professional judgment (Chircop et al., 2023).

[2] While participation in a public procurement contract is endogenous to the firm, the outcome of the public procurement contract is exogenous since the firm has arguably no control on the outcome of the public procurement contract. The exogenous nature of the public procurement contract attenuates endogeneity concerns in our setting.

procurement contract are strong enough to influence the firms' tax avoidance. First, due to their lower-than-expected profitability, losing firms might not realize the benefits of tax avoidance activities. Second, as potential adverse outcomes resulting from the detection of tax avoidance activities might be particularly detrimental to uncompetitive firms, knowledge of the loss of public procurement contracts reduces the incentives for such firms to undertake tax avoidance activity. Indeed, these firms might prefer strategies other than tax avoidance (e.g., pricing decisions) to improve their competitiveness. Third, the publication of the outcome of a public procurement contract might not be a salient enough signal to allow firms to learn about their competitiveness.

To examine our research question, we use a comprehensive dataset of 2.3 million procurement contracts awarded by 3,669 unique Italian public authorities from 2014 to 2020. Two features make the Italian public procurement setting apt for investigating our research question. First, due to the availability of open public procurement data (Duguay et al., 2023), we can observe all bidding firms for each procurement contract, regardless of the final winner. Second, public procurement regulation and its impact on local economic activity in Italy (11% of GDP) are comparable to those of other European countries and the U.S.[3] Hence, inferences from our study should be generalizable to other countries.

We focus on public procurement contracts involving at least two firms. To be included in the sample, a firm must have lost a bid for a public procurement contract. Further, to attenuate potential confounding effects, we require firms to participate and lose only one bid for a public procurement contract in a four-year window. Using a staggered difference-in-differences estimation, we compare changes in tax avoidance around the loss of the bid for these firms (treatment group) with changes in tax avoidance for control firms. We define control firms as firms operating in the same municipality and industry as treated firms but not participating in public procurement during our sample period.

We find that firms increase their tax avoidance activity relative to control firms after losing a bid for a public procurement contract. Specifically, after losing a bid for a public procurement contract, firms experience a 2% reduction in their effective tax rates ("ETR") relative to control firms. This effect is larger when losing firms (1) face greater competition in public procurement contracts and (2) are more financially constrained. These results suggest that learning about firms' uncompetitiveness incentivizes firms to increase their tax avoidance activities. Such incentives are greater when the firm is

[3]For expositional purposes, I direct the reader to Section 2.2.2 for a detailed overview of the Italian public procurement framework.

uncompetitive relative to a larger number of firms and has less financial slack to improve its competitiveness.

We subject our baseline results to multiple robustness tests. First, we test whether our results are sensitive to including firms with multiple public procurement bids in our sample period. Second, since our tax avoidance measures might be subject to short-term earnings management, we use long-term measures of tax avoidance. Third, to ensure that systematic differences between our treatment and control firms do not unduly influence our results, we run our baseline analysis on an entropy-balanced sample (Hainmueller, 2012). Fourth, given the limitations of a staggered difference-in-differences approach, we supplement our analysis with a stacked regression (Cengiz et al., 2019). Inferences from these robustness tests support our baseline results.

Next, to corroborate our conjecture that firms improve their competitiveness after losing bids for public procurement contracts, we conduct two additional tests. First, we find that after losing a bid for a public procurement contract, firms increase their pre-tax returns on assets and asset turnover. Second, we find that the likelihood of subsequently winning a procurement contract depends on the magnitude of tax avoidance undertaken by the firm. Interestingly, we do not find evidence that firms change their tax avoidance activities after winning a procurement contract. These results suggest that after losing a bid for a public procurement contract, firms seek to improve their operating efficiency in several ways, including tax avoidance. Further, tax avoidance increases the likelihood of subsequently winning public procurement contracts.

This study informs the debate about the relationship between competition and tax avoidance. Cai and Liu (2009) find that greater competition leads to more tax avoidance, while Kubick et al. (2015) find that firms sheltered from competition undertake more tax avoidance activity. We inform this debate by showing that losing a bid for a public procurement contract, hence learning that the firm is uncompetitive relative to its peers, incentivizes firms to improve their competitiveness by engaging in tax avoidance.

Further, this study informs the debate about the role of public procurement on tax avoidance activities. In a related study, Mills et al. (2013) find that political sensitivity arising from government contracting is associated with an increase in tax payments. This relationship weakens as government contractors increase their bargaining power. We add to this stream of literature by providing empirical evidence relating to the effects of the public procurement process on firms that participate but do not win public procurement contracts. Given that multiple losing firms exist for each winning firm (i.e., government contractor), examining the effect of public procurement on the tax avoidance of losing firms is economically relevant.

This study also contributes to the emerging literature examining the real effects of transparency in public procurement. Coviello and Mariniello (2014) show that higher publicity of public procurement auctions improves contract execution performance, while Duguay et al. (2023) find that procurement officials are more likely to award contracts through competitive procedures after the open data initiative in the European Union. We provide initial evidence on the firm-level effects of increased transparency in public procurement. Specifically, our study suggests that disclosing public procurement outcomes facilitates learning about participating firms' competitiveness.

## 4.2 Background and Hypothesis Development

### 4.2.1 Corporate Income Taxes in Italy

In Italy, there are two taxes on corporate income: a national corporate income tax (*Imposta sul reddito delle società*, hereafter "IRES") and a regional tax on production (*Imposta regionale sulle attività produttive*, hereafter "IRAP"). On the one hand, the IRES rate is flat and ranges from 27.5% (from 2011 to 2016) to 24% (after 2017) during our sample period. Pre-tax income reported in the financial statements prepared under Italian Generally Accepted Accounting Principles (*GAAP*) is the starting point for computing IRES taxable income. The Italian tax framework requires firms to recognize gains and expenses on an accrual basis, with few exceptions (e.g., board compensation). To deduct an expense from taxable income, it must meet two conditions. First, it must identifiable and objectively measurable. Second, it must relate directly to the business activity—it must be functional for generating revenues (Bianchi et al., 2019).

On the other hand, the baseline IRAP rate is 3.9%, but each region can increase it by up to 0.92 percentage points. The IRAP taxable income is the difference between the value and costs of production—roughly equivalent to operating income before some cost items (e.g., labor costs or impairments). Because firms use *GAAP* income figures to compute taxable income for both IRES and IRAP and tax rules require few adjustments, Italy shows a high degree of book-tax conformity.

The Italian Revenue Agency (*Agenzie delle entrate*) and Italian tax police (*Guardia* *di Finanza*) monitor firms' tax compliance and conduct tax audits, which can lead to preventive asset seizures and monetary fines. However, despite increased tax collection efforts over the last two decades, Italy continues to have a low level of tax compliance relative to other European countries. As of 2016, the estimated total tax gap is EUR 109.1 billion, roughly equivalent to 9.4% of Italian GDP (European Commission, 2020).

Overall, these arguments indicate how Italy provides a valuable setting to investigate the effect of public procurement participation on tax avoidance for two reasons. First, the combination of a high statutory tax rate (averaging 27.9%) and a weak tax enforcement creates strong incentives to engage in tax avoidance. Second, because public procurement accounts for a relevant fraction of local economic activity and is a key value driver, firms have strong incentives to improve their competitiveness to ultimately secure procurement contracts.

### 4.2.2 Losing a Public Procurement Bid and Tax Avoidance

Tax avoidance refers to activities undertaken by the firm intended to reduce the amount of taxes paid (Chircop et al., 2023). Several studies have examined the determinants and consequences of tax avoidance activities (Hanlon and Heitzman, 2010). However, there is limited understanding of how participating in the public procurement process affects firms' tax avoidance. When bidding for public procurement contracts, firms compete with peers based on *ex ante* set rules and regulations. While firms learn about peers through several channels (e.g., financial disclosures), the outcome of a public procurement contract provides firms with a unique opportunity to learn about their competitiveness relative to peers. Specifically, firms winning public procurement contracts learn that they are more competitive than their peers, while firms losing public procurement contracts learn that they are uncompetitive relative to their peers. This learning should spur, in turn, losing firms to improve their competitiveness.

Tax avoidance constitutes a mechanism to compensate for firms' uncompetitiveness relative to their peers (Chircop et al., 2023). By increasing after-tax profit and cash flows, tax avoidance reduces the cost of investments. Consistent with this reasoning, Cai and Liu (2009) document a negative relationship between firms' competitiveness and their level of tax avoidance. Uncompetitive firms undertake more tax avoidance to reduce their tax liability and increase their investment money. By improving firms' margins, tax avoidance allows firms to offer lower prices for procurement contracts, thus increasing their chances of winning future contracts. Based on this argument, we conjecture a positive relation between losing a bid for a public procurement contract and firm tax avoidance.

Nonetheless, the effect of losing a bid for a public procurement contract on firms' tax avoidance is unclear. First, losing firms are less able to realize the benefits of tax avoidance activities (Kubick et al., 2015). When insulated from competitive threats, firms can more easily realize the benefits of tax avoidance activities (Mayberry et al., 2015) and have a natural hedge against adverse outcomes of tax avoidance, such as public scrutiny (Dyreng et al., 2016).

Second, losing firms might prefer alternative strategies to improve their competitiveness (e.g., pricing decisions). Indeed, aggressive tax avoidance strategies might carry detection risks, which could result in the exclusion from future procurement bids in our setting. Specifically, the Italian Public Procurement Code mandates the exclusion of firms with severe tax violations from bid participation.[4] Furthermore, when submitting bid documentation, firms must provide a specific certificate (*Documento Unico di Regolarità Contributiva*) documenting their compliance with social security contribution requirements. Therefore, since any adverse outcomes (e.g. exclusion from future procurement bids) from tax avoidance will damage losing firms' already weak competitive position, knowledge of the uncompetitiveness of the firm relative to its peers narrows the scope for the firm to undertake tax avoidance (e.g., Rego and Wilson, 2012).

Third, the assumption that the outcome of a public procurement contract allows firm learning might be demanding in our setting. Literature documents that information frictions often impede private firms from following best practices (e.g., Gassen and Muhn, 2025). Thus, disclosing the outcome of a public procurement contract might not provide a salient enough signal to allow firm learning. These three argumentations suggest that losing a bid for a public procurement contract should have little or no effect on firms' tax avoidance. Given that it is *ex ante* unclear whether losing a bid for a public procurement contract influences the firms' tax avoidance activities, we express our hypothesis in the null form:

> H1: Losing a bid for a public procurement contract has no effect on firms' tax avoidance activity.

[4] Severe tax violations refer to cases where firms' unpaid taxes exceed a certain threshold. During our sample period, the threshold was EUR 10,000 from 2011 to 2018, after which The Italian Government lowered it to EUR 5,000.

## 4.3 Methodology

### 4.3.1 Data

Our primary source for public procurement contracts is a novel public database of public procurement contracts issued in Italy. By individually collecting data from procurement agencies' websites, this dataset offers more extensive coverage of procurement contracts relative to other publicly available procurement datasets (e.g., Digiwhist or ANAC). Indeed, this database has information on 2.3 million procurement contracts awarded by 3,669 unique public authorities, such as municipalities, regional governments, and other public authorities (e.g., hospitals or universities). Data quality and coverage dramatically increase from 2014. Hence, we focus on contracts awarded from 2014 to 2020, the last year for which we have accounting data.

Each contract is assigned a unique contract identifier. For each contract, we observe the year of the award, the contract value, the tax identifier of the public authority awarding the contract, and the procurement procedure. Importantly, we observe the tax identifier of all firms bidding for each contract. We use bidding firms' tax identifiers to aggregate at the firm-level the total number of firm bids for procurement contracts and the number of contracts won throughout our sample period. We obtain firm-level financial data from the Bureau Van Dijk AIDA database. Specifically, we collect accounting data for 274,400 unique Italian private firms from 2011 to 2020 to calculate the variables required for our baseline analysis.

### 4.3.2 Sample Selection

For our analysis, we define treated firms as private firms participating in public procurement contracts but losing these contracts to other firms. To identify our treated firms, we focus on procurement contracts involving at least two private firms, one of which does not win the contract. Next, we restrict our sample to firms for which we have accounting data available. Then, to observe losing firms for at least two years before and after losing the procurement contract, we consider only contracts awarded from 2014 to 2018. Hence, we exclude all firms bidding for a contract from 2019 to 2020. Finally, we omit firms bidding for more than one procurement contract over our sample period. While essential to ensure a clean identification, these criteria restrict our sample of treated firms from 54,289 to 1,814 unique firms. Figure 4.1 shows the wide geographic distribution of treated firms across Italian provinces (Panel A) and municipalities (Panel

B). Treated firms are spread throughout the Italian peninsula, although we observe a greater concentration in Lombardy, Veneto, and Tuscany.

Next, we retrieve the municipality and industry in which treated firms operate. We define control firms as private firms that (1) operate in the same municipality and industry as treated firms, and (2) never participate in public procurement during our sample period. The underlying assumption is that, since control firms operate in the same municipality and industry as treated firms, control firms have characteristics similar to those of our treated firms but are unaffected by the procurement process. Our final sample comprises 129,190 firm-year observations for which we can estimate all our empirical variables.

### 4.3.3 Research Design

To investigate the relation between losing a public procurement contract and future tax avoidance, we employ the following staggered difference-in-differences estimation:

$$y_{i,t} = \beta_0 + \beta_1 LostBid_{i,t-1} + \gamma X_{i,t-1} + \alpha_i + \alpha_t + \epsilon_{i,t} \tag{4.1}$$

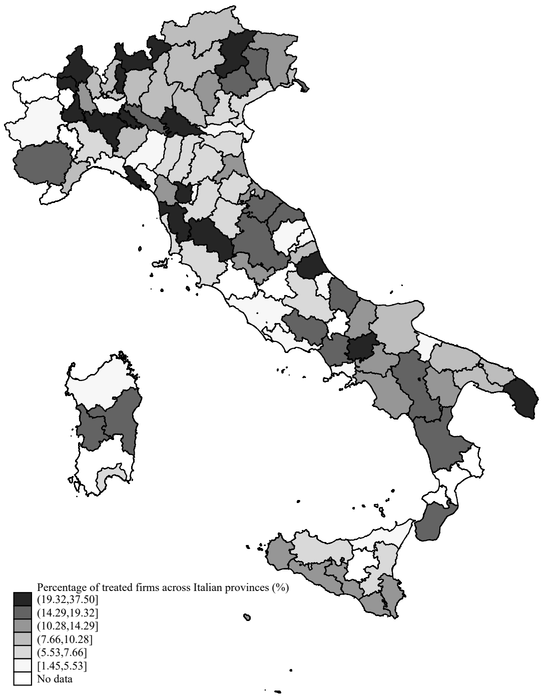
where $y$ is the outcome variable (i.e., tax avoidance) for firm $i$, in year $t$. Following previous studies (e.g., Bianchi et al., 2019; Chircop et al., 2023; Hanlon and Heitzman, 2010), we use two proxies for tax avoidance: *GAAP ETR* and *Current ETR*. *GAAP ETR* is the ratio of total (the sum of current, prepaid and deferred) tax expenses to pre-tax income, while *Current ETR* is the ratio of current tax expenses to pre-tax income. Both measures reflect nonconforming tax avoidance and capture the average tax per euro of income.[5] Because effective tax rates are difficult to interpret for negative income observations, we eliminate observations with negative pre-tax income. Further, like Chircop et al. (2023), we constrain our tax avoidance variables to take a value between zero and one. Finally, to facilitate interpreting our results, we multiply *GAAP ETR* and *Current ETR* by minus one so that higher values of our outcome variables indicate higher tax avoidance.

*Lost Bid*, the independent variable of interest, is an indicator variable taking values of one after a treated firm loses a bid for a public procurement contract, and zero

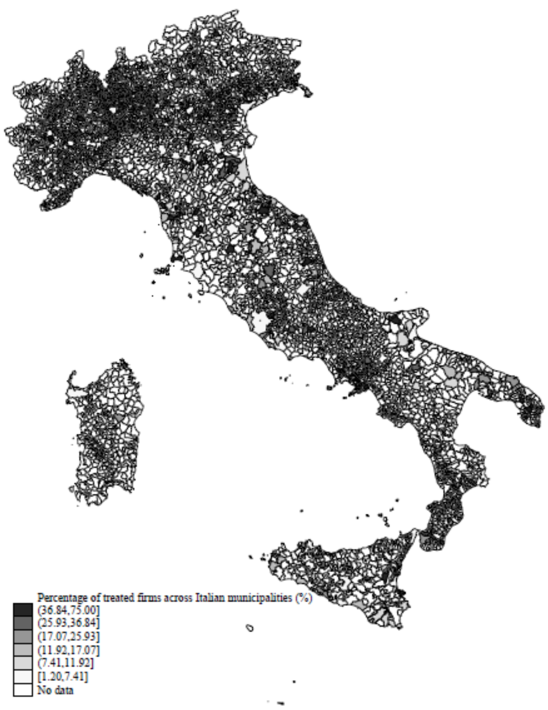
[5] Because tax returns are not publicly available in Italy, we compute our tax avoidance measures using income statement figures. Furthermore, since several sample firms have reduced reporting requirements, we lack cash flow data to compute alternative tax avoidance measures (e.g., Cash ETR). However, given Italy's high degree of book-tax conformity, we expect our proxies to adequately capture firms' nonconforming tax avoidance activities.

**Figure 4.1:** Geographic Distribution of Treated Firms

This figure reports the distribution of treated firms across Italian provinces (Panel A) and municipalities (Panel B). Darker areas indicate a greater percentage of treated firms.




**(a)** Province-level Distribution




**(b)** Municipality-level Distribution

otherwise. $X$ refers to a set of time-varying controls that absorb time-varying firm-specific characteristics. Specifically, we control for firm characteristics associated with tax avoidance, including size (*Size*), profitability (*ROA*), leverage (*Debt*), sales growth (*Growth*), and property, plant, and equipment (*PPE*). We winsorize all continuous control variables at the 1st and 99th percentile and cluster standard errors at the firm level. In all specifications, we include firm fixed effects ($\alpha_i$) to control for time-invariant firm characteristics and year fixed effects ($\alpha_t$) to control for year-level shocks, such as changes in the statutory tax rate.

### 4.3.4 Descriptive Statistics

Table 4.1 reports descriptive statistics for sample firms. As shown in Panel A, we observe the largest (smallest) number of observations in 2019 (2013), accounting for 14.1% (9.1%) of the total sample. Panel B reports the distribution of sample firms across industries. Most firms operate in Wholesale and Retail or in the Construction sector.

Table 4.2 reports summary statistics for the variables employed in the empirical analysis. Our sample firms' average (median) tax rate is 43.1% (36.5%). The high tax burden of our sample firms is consistent with the estimates documented by previous studies examining Italian firms (e.g., Bianchi et al., 2019; Chircop et al., 2023). Our sample firms are, on average, small- or mid-sized. The average firm in our sample has *Size* of 7.945, roughly equivalent to EUR 13 million. These statistics are consistent with the prevalence of small and medium enterprises in Italy.

Table 4.3 provides univariate tests comparing the effect of losing a public procurement contract on tax avoidance for treated and control firms. For this analysis, we use the residuals computed from regressing *GAAP ETR* and *Current ETR* on year fixed effects to account for time-varying changes in tax rates. We partition treated observations into before and after relative to the year they lose the bid for a public procurement contract. We partition control observations into before and after relative to the first year in which a firm in the same municipality-industry loses a bid for a public procurement contract. The differences in the means of *Abnormal GAAP ETR* and *Abnormal Current ETR* are negative and statistically significant for treated firms. Conversely, the differences in means for control firms are not statistically significant. These univariate tests provide initial evidence of the effect of losing a bid for a procurement contract on future tax avoidance.

## **Table 4.1:** Descriptive Statistics

This table reports descriptive statistics for sample firms. Panel A reports the number of treated and control observations by year. Panel B reports the number of treated and control observations by industry. Treated firms are firms that participate in one public procurement bid and lose that bid during our sample period. Control firms are firms that never participate in public procurement during our sample period and are located in the same municipality and industry as treated firms.

Panel A: Observations by Year

| Year | Treated Observations | Control Observations | Total |
|---|---|---|---|
| 2013 | 773 | 11,066 | 11,839 |
| 2014 | 987 | 14,321 | 15,308 |
| 2015 | 1,040 | 15,003 | 16,043 |
| 2016 | 1,093 | 15,760 | 16,853 |
| 2017 | 1,117 | 16,571 | 17,688 |
| 2018 | 1,115 | 17,036 | 18,151 |
| 2019 | 1,104 | 17,072 | 18,176 |
| 2020 | 964 | 13,997 | 14,961 |
| Total | 8,193 | 120,826 | 129,019 |

Panel B: Observations by Industry

| Industry | Treated Observations | Control Observations | Total |
|---|---|---|---|
| Agriculture and mining | 22 | 200 | 222 |
| Manufacturing | 1,451 | 16,763 | 18,214 |
| Utilities | 89 | 1,260 | 1,349 |
| Construction | 1,774 | 12,237 | 14,011 |
| Wholesale and Retail | 2,334 | 52,189 | 54,523 |
| Transportation | 399 | 8,553 | 8,952 |
| Hospitality | 100 | 2,081 | 2,181 |
| IT | 554 | 4,663 | 5,217 |
| Financial Firms | 47 | 1,369 | 1,416 |
| Real estate | 156 | 6,028 | 6,184 |
| Professional activities | 569 | 8,093 | 8,662 |
| Leasing, Traveling and Service | 506 | 4,972 | 5,478 |
| Education | 37 | 460 | 497 |
| Healthcare | 121 | 1,312 | 1,433 |
| Sport and entertainment | 21 | 414 | 435 |
| Other services | 13 | 232 | 245 |
| Total | 8,193 | 120,826 | 129,019 |

## **Table 4.2:** Summary Statistics

This table reports summary statistics for the main variables used in the empirical analysis. *GAAP ETR* is the ratio of total income tax expense divided by pre-tax book income, multiplied by minus one. *Current ETR* is the ratio of current income tax expense over pre-tax book income, multiplied by minus one. *GAAP ETR* and *Current ETR* are bounded between zero and minus one. *Size* is the natural logarithm of total assets. *ROA* is the ratio of operating income over lagged total assets. *Growth* is the yearly sales growth rate. *Debt* is the ratio of total liabilities to total assets. *PPE* is the ratio of property, plant and equipment to total assets.

| | Panel A: All Firms | | | | | |
|---|---|---|---|---|---|---|
| | N | Mean | Std. Dev | p25 | p50 | p75 |
| GAAP ETR | 129,019 | -0.432 | 0.250 | -0.552 | -0.361 | -0.288 |
| Current ETR | 129,019 | -0.427 | 0.254 | -0.545 | -0.357 | -0.284 |
| Size | 129,019 | 7.945 | 1.315 | 7.057 | 7.760 | 8.676 |
| ROA | 129,019 | 0.109 | 0.164 | 0.026 | 0.055 | 0.124 |
| Growth | 129,019 | 0.204 | 1.278 | -0.087 | 0.031 | 0.185 |
| Debt | 129,019 | 0.623 | 0.257 | 0.439 | 0.674 | 0.834 |
| PPE | 129,019 | 0.131 | 0.198 | 0.010 | 0.043 | 0.164 |

| | Panel B: Treated Firms | | | | | |
|---|---|---|---|---|---|---|
| | N | Mean | Std. Dev | p25 | p50 | p75 |
| GAAP ETR | 8,193 | -0.454 | 0.256 | -0.619 | -0.386 | -0.292 |
| Current ETR | 8,193 | -0.448 | 0.260 | -0.614 | -0.380 | -0.286 |
| Size | 8,193 | 8.112 | 1.226 | 7.300 | 7.945 | 8.692 |
| ROA | 8,193 | 0.085 | 0.108 | 0.027 | 0.051 | 0.104 |
| Growth | 8,193 | 0.111 | 0.632 | -0.077 | 0.039 | 0.176 |
| Debt | 8,193 | 0.630 | 0.222 | 0.474 | 0.671 | 0.809 |
| PPE | 8,193 | 0.128 | 0.158 | 0.018 | 0.062 | 0.176 |

| | Panel C: Control Firms | | | | | |
|---|---|---|---|---|---|---|
| | N | Mean | Std. Dev | p25 | p50 | p75 |
| GAAP ETR | 120,826 | -0.431 | 0.249 | -0.547 | -0.360 | -0.288 |
| Current ETR | 120,826 | -0.425 | 0.253 | -0.541 | -0.356 | -0.284 |
| Size | 120,826 | 7.933 | 1.320 | 7.041 | 7.744 | 8.675 |
| ROA | 120,826 | 0.110 | 0.167 | 0.026 | 0.056 | 0.125 |
| Growth | 120,826 | 0.210 | 1.310 | -0.087 | 0.031 | 0.186 |
| Debt | 120,826 | 0.623 | 0.259 | 0.437 | 0.674 | 0.836 |
| PPE | 120,826 | 0.132 | 0.201 | 0.009 | 0.041 | 0.163 |

**Table 4.3:** Univariate Tests

This table reports the univariate analysis for the effect of losing a bid for a public procurement contract on future tax avoidance separately for treated and control firms. For treated firms, we define before and after relative to the year in which they lose a bid for procurement contracts. For control firms, we define before and after relative to the first year in which a firm in the same municipality-industry loses a procurement contract. *Abnormal GAAP ETR* and *Abnormal Current ETR* are the residuals obtained by regressing *GAAP ETR* and *Current ETR* on year fixed effects. *GAAP ETR* is the ratio of total income tax expense divided by pre-tax book income, multiplied by minus one. *Current ETR* is the ratio of current income tax expense over pre-tax book income, multiplied by minus one. *GAAP ETR* and *Current ETR* are bounded between zero and minus one.

| | Treated only | | |
|---|---|---|---|
| | Before | After | Difference |
| Abnormal GAAP ETR | -0.043 | -0.021 | -0.022*** |
| Abnormal Current ETR | -0.046 | -0.018 | -0.028*** |

| | Control only | | |
|---|---|---|---|
| | Before | After | Difference |
| Abnormal GAAP ETR | 0.001 | 0.002 | -0.001 |
| Abnormal Current ETR | 0.001 | 0.002 | -0.001 |

## 4.4 Results

In this section, we present results from estimating Equation 4.1, which examines the effect of losing a bid for a public procurement contract on future tax avoidance. We conjecture that when losing a bid for a public procurement contract, firms learn they are uncompetitive relative to their peers leading these firms to change their tax avoidance activities to improve their competitiveness. Accordingly, we expect the coefficient on *Lost Bid* to be significant.

Table 4.4 presents the results for Equation 4.1. In columns (1) and (2), we estimate Equation 4.1 using *GAAP ETR* as the dependent variable, while in columns (3) and (4), we estimate Equation 4.1 using *Current ETR* as the dependent variable. In both specifications, our coefficient of interest ($\beta_1$) is positive and statistically significant at the 1% level (coeff: 0.021 in Column [2]; coeff: 0.023 in Column [4]). After losing a procurement contract, firms experience a 2.1% (2.3%) reduction in their *GAAP ETR* (*Current ETR*). In line with previous studies (e.g., Chircop et al., 2023), we find that smaller and more levered firms have higher tax avoidance. These results suggest that treated firms increase tax avoidance after losing a public procurement contract.

However, the internal validity of these results relies on the assumption that absent the loss of the bid for a public procurement contract, differences in outcome variables are

## **Table 4.4:** Tax Avoidance after Losing a Bid

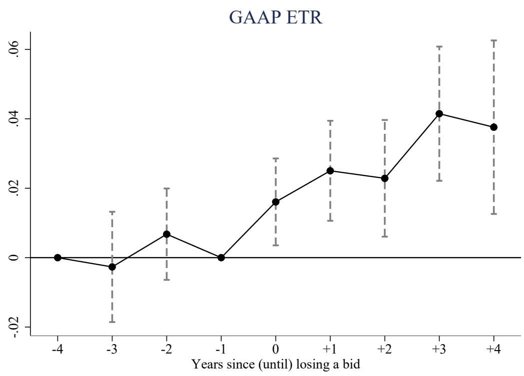
This table reports the results examining the effect of losing a bid for a public procurement contract on future tax avoidance. *GAAP ETR* is the ratio of total income tax expense divided by pre-tax book income, multiplied by minus one. *Current ETR* is the ratio of current income tax expense over pre-tax book income, multiplied by minus one. *GAAP ETR* and *Current ETR* are bounded between zero and minus one. *Lost Bid* is a dummy variable taking values of one after a firm loses a bid for a public procurement contract in *t-1*, and zero otherwise. *Size* is the natural logarithm of total assets in *t-1*. *ROA* is the ratio of operating income over lagged total assets in *t-1*. *Growth* is the sales growth rate from *t-2* to *t-1*. *Debt* is the ratio of total liabilities to total assets in *t-1*. *PPE* is the ratio of property, plant and equipment to total assets in *t-1*. All estimations include firm and year fixed effects. Control variables are winsorized at the 1% and 99% level. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level.

| | (1)<br>GAAP ETR | (2)<br>GAAP ETR | (3)<br>Current ETR | (4)<br>Current ETR |
|---|---|---|---|---|
| Lost Bid | 0.023*** | 0.021*** | 0.024*** | 0.023*** |
| | (0.006) | (0.006) | (0.006) | (0.006) |
| Size | | -0.017*** | | -0.020*** |
| | | (0.002) | | (0.002) |
| ROA | | 0.066*** | | 0.064*** |
| | | (0.005) | | (0.005) |
| Growth | | -0.001 | | -0.001** |
| | | (0.000) | | (0.000) |
| Debt | | 0.057*** | | 0.061*** |
| | | (0.007) | | (0.007) |
| PPE | | -0.009 | | -0.003 |
| | | (0.011) | | (0.011) |
| Firm FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.5319 | 0.5337 | 0.5263 | 0.5281 |
| Observations | 129,019 | 129,019 | 129,019 | 129,019 |

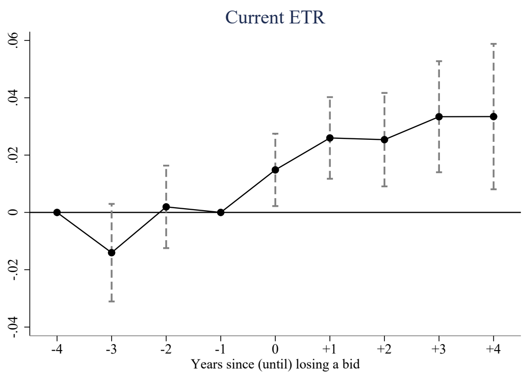
constant between treated and control firms. To investigate the validity of this assumption in our setting and ensure that the parallel trend assumption holds, we estimate a dynamic version of the model in Equation 4.1 by introducing relative time indicator variables up to four years before and after the year of the bid. In this respect, recent studies show that two-way fixed effects models in frameworks with staggered treatments are likely to be biased in the presence of treatment effect heterogeneity (Baker et al., 2022; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021), which can undermine the testing of pre-trends using lead and lag coefficients. To mitigate this concern, we estimate a dynamic model based on the approach proposed by Sun and Abraham (2021), which gauges cohort-specific treatment effects on the treated periods and averages them across all cohorts. Figure 4.2 plots the point estimates for every indicator variable in our dynamic DiD model and the 95% confidence interval for specifications with *GAAP ETR* (Panel A) and *Current ETR* (Panel B). The figures do not show evidence of pre-trends for our tax avoidance variables. There is no statistical difference in the effective tax rate between treated and control firms before the bid. The effect of losing the contract on treated firms' tax avoidance materializes in the year of the bid (year 0) and stabilizes in subsequent years (years +1 to +4).

**Figure 4.2:** Parallel Trends

These figures plot the dynamic effect of losing a public procurement bid on *GAAP ETR* and *Current ETR* following the estimation procedure in Sun and Abraham (2021). *GAAP ETR* is the ratio of total income tax expense divided by pre-tax book income, multiplied by minus one. *Current ETR* is the ratio of current income tax expense over pre-tax book income, multiplied by minus one. *GAAP ETR* and *Current ETR* are bounded between zero and minus one. We centered the year the bid was lost at year zero and estimated a model with indicators for every year relative to the adoption date. We exclude the minus-one and minus-four indicator. The estimation includes year and firm fixed effects. Standard errors are clustered by firm.




**(a)** GAAP ETR




**(b)** Current ETR

## 4.5 Cross-Sectional Analysis

In this section, we examine the cross-sectional determinants of our results. Specifically, we investigate how the degree of competition in the public procurement process and the firms' financial constraints affect the relationship between losing a bid for a public procurement contract and tax avoidance.

### 4.5.1 Degree of Competition in Public Procurement

To mitigate sample selection bias, in our baseline analysis we include all public procurement contracts with at least two participating firms. As discussed in Section 2.2.2, public procurement contracts can be awarded through public competitive auctions or direct contracting. Public competitive auctions include open procedures (17% of our baseline sample), where the bidding is open to all firms; restricted procedures (1%), where bidding is open to firms that pass a prequalification phase; and negotiated procedures (22%), where bidding is only available to firms invited to bid by the procurement agency. Direct contracting (60% of our baseline sample) refers to public procurement contracts that involve direct negotiations between the procurement agency and specific firms. Since public competitive auctions are less prone to favoritism and corruption than direct contracting (e.g., Gerardino et al., 2017) we conjecture that there is greater scope for learning about one's competitiveness from these public procurement contracts. Thus, we predict that the relationship between losing a bid for a public procurement contract and tax avoidance is stronger for public competitive auctions.

To test this conjecture, we include the interaction term *Lost Bid* × *High Competition* in Equation 4.1. *High Competition* is an indicator variable taking values of one for public competitive auctions and zero otherwise. While the coefficient on *Lost Bid* captures the average effect of losing a public procurement contract on tax avoidance activity, the interaction term *Lost Bid* × *High Competition* captures the incremental effect of losing a public procurement contract awarded through a public competitive auction on tax avoidance activity. Table 4.5 presents the results of this analysis. In line with our conjecture that there is greater scope for learning about a firm's competitiveness from public procurement contracts awarded through auctions, the coefficient on *Lost Bid* × *High Competition* is positive for both measures of tax avoidance. Further, this coefficient is significant at the 5% level when *Current ETR* is the dependent variable.

**Table 4.5:** Cross-sectional Test: Degree of Competition

This table reports the results examining the effect of losing a bid for a public procurement contract on future tax avoidance, depending on the degree of competition in procurement contracts. *GAAP ETR* is the ratio of total income tax expense divided by pre-tax book income, multiplied by minus one. *Current ETR* is the ratio of current income tax expense over pre-tax book income, multiplied by minus one. *GAAP ETR* and *Current ETR* are bounded between zero and minus one. *Lost Bid* is a dummy variable taking values of one after a firm loses a bid for a public procurement contract in *t-1*, and zero otherwise. *High Competition* is a dummy variable taking values of one for procurement contracts awarded through competitive award procedures, and zero otherwise. *Size* is the natural logarithm of total assets in *t-1*. *ROA* is the ratio of operating income over lagged total assets in *t-1*. *Growth* is the sales growth rate from *t-2* to *t-1*. *Debt* is the ratio of total liabilities to total assets in *t-1*. *PPE* is the ratio of property, plant and equipment to total assets in *t-1*. All estimations include firm and year fixed effects. Control variables are winsorized at the 1% and 99% level. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level.

| | (1)<br>GAAP ETR | (2)<br>Current ETR |
|---|---|---|
| Lost Bid | 0.016** | 0.013* |
| | (0.007) | (0.007) |
| Lost Bid × High Competition | 0.013 | 0.025** |
| | (0.012) | (0.012) |
| Size | -0.017*** | -0.020*** |
| | (0.002) | (0.002) |
| ROA | 0.066*** | 0.064*** |
| | (0.005) | (0.005) |
| Growth | -0.001 | -0.001** |
| | (0.000) | (0.000) |
| Debt | 0.057*** | 0.061*** |
| | (0.007) | (0.007) |
| PPE | -0.010 | -0.003 |
| | (0.011) | (0.011) |
| Firm FE | Yes | Yes |
| Year FE | Yes | Yes |
| Adjusted $R^2$ | 0.5337 | 0.5281 |
| Observations | 129,019 | 129,019 |

### 4.5.2 Firms' Financial Constraints

Next, we investigate whether financial constraints affect the relationship between losing a procurement contract and tax avoidance. Financial constraints limit firms' ability to improve their competitiveness through price reductions or improvements in operating efficiency. Thus, we conjecture that after learning about their uncompetitiveness, losing firms with greater financial constraints are more likely to increase their tax avoidance. To test this conjecture, we include variables *High Financial Constraint* and *Lost Bid* × *High Financial Constraint* in Equation 4.1, where *High Financial Constraint* is a dummy variable taking values of one for observations in the top quintile of the distribution of short-term bank debt over total assets, and zero otherwise. The interaction term *Lost Bid* × *High Financial Constraint* captures the incremental effect of losing a bid on tax avoidance activity for financially constrained firms.

Table 4.6 presents the results of this analysis. Irrespective of the measure of tax avoidance used, the coefficient on the interaction term is positive and significant at the 5% level. In line with our conjecture, these results suggest that the relationship between losing a public procurement contract and tax avoidance activity is stronger for financially constrained firms.

### 4.5.3 Robustness Test

We perform a battery of tests to examine the sensitivity of our results to different research design choices. First, we examine the robustness of our results to our sample selection criteria. In our baseline analysis, we define treated firms as firms that bid and lose only one public procurement contract during our sample period. As this sample selection criteria significantly reduces our sample, in this robustness test, we include firms bidding and losing multiple public procurement contracts in our sample period. This change in our sample selection criteria increases our sample of treated observations from 1,814 to 4,736. For this analysis, the variable *Lost Bid* takes the value of one after a firm loses its first bid for a public procurement contract, and zero otherwise. Table 4.7 shows the results of this analysis. Column 1 (2) shows the results for the specification with *GAAP ETR* (*Current ETR*) as the dependent variable. In line with our baseline results, the coefficient on *Lost Bid* is positive and significant at the 1% level in both specifications (coeff: 0.014 in Column [1]; coeff: 0.015 in Column [2]). Coefficients for control variables are in line with those presented in our baseline analysis.

## **Table 4.6:** Cross-sectional Test: Financial Constraints

This table reports the results examining the effect of losing a bid for a public procurement contract on future tax avoidance, depending on treated firms' financial constraints. *GAAP ETR* is the ratio of total income tax expense divided by pre-tax book income, multiplied by minus one. *Current ETR* is the ratio of current income tax expense over pre-tax book income, multiplied by minus one. *GAAP ETR* and *Current ETR* are bounded between zero and minus one. *Lost Bid* is a dummy variable taking values of one after a firm loses a bid for a public procurement contract in *t-1*, and zero otherwise. *High Financial Constraints* is a dummy variable taking values of one for observations in the top quintile of the distribution of short-term bank debt over total assets, and zero otherwise. *Size* is the natural logarithm of total assets in *t-1*. *ROA* is the ratio of operating income over lagged total assets in *t-1*. *Growth* is the sales growth rate from *t-2* to *t-1*. *Debt* is the ratio of total liabilities to total assets in *t-1*. *PPE* is the ratio of property, plant and equipment to total assets in *t-1*. All estimations include firm and year fixed effects. Control variables are winsorized at the 1% and 99% level. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level.

| | (1)<br>GAAP ETR | (2)<br>Current ETR |
|---|---|---|
| Lost Bid | 0.016** | 0.017*** |
| | (0.006) | (0.006) |
| High Financial Constraints | -0.015*** | -0.016*** |
| | (0.003) | (0.003) |
| Lost Bid × High Financial Constraints | 0.026** | 0.027** |
| | (0.011) | (0.012) |
| Size | -0.017*** | -0.020*** |
| | (0.002) | (0.002) |
| ROA | 0.065*** | 0.063*** |
| | (0.005) | (0.005) |
| Growth | -0.001 | -0.001** |
| | (0.000) | (0.000) |
| Debt | 0.060*** | 0.064*** |
| | (0.007) | (0.007) |
| PPE | -0.010 | -0.004 |
| | (0.011) | (0.011) |
| Firm FE | Yes | Yes |
| Year FE | Yes | Yes |
| Adjusted $R^2$ | 0.5339 | 0.5283 |
| Observations | 129,019 | 129,019 |

## **Table 4.7:** Multiple Bids

This table reports the results examining the effect of losing a bid for a public procurement contract on future tax avoidance, using a sample of firms participating in multiple bids over our sample period. *GAAP ETR* is the ratio of total income tax expense divided by pre-tax book income, multiplied by minus one. *Current ETR* is the ratio of current income tax expense over pre-tax book income, multiplied by minus one. *GAAP ETR* and *Current ETR* are bounded between zero and minus one. *Lost Bid* is a dummy variable taking values of one after a firm loses its first bid for a public procurement contract in *t-1*, and zero otherwise. *Size* is the natural logarithm of total assets in *t-1*. *ROA* is the ratio of operating income over lagged total assets in *t-1*. *Growth* is the sales growth rate from *t-2* to *t-1*. *Debt* is the ratio of total liabilities to total assets in *t-1*. *PPE* is the ratio of property, plant and equipment to total assets in *t-1*. All estimations include firm and year fixed effects. Control variables are winsorized at the 1% and 99% level. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level.

| | (1)<br>GAAP ETR | (2)<br>Current ETR |
|---|---|---|
| Lost Bid | 0.014*** | 0.015*** |
| | (0.004) | (0.004) |
| Size | -0.017*** | -0.020*** |
| | (0.002) | (0.002) |
| ROA | 0.080*** | 0.075*** |
| | (0.004) | (0.004) |
| Growth | -0.001* | -0.001*** |
| | (0.000) | (0.000) |
| Debt | 0.060*** | 0.064*** |
| | (0.006) | (0.006) |
| PPE | -0.001 | 0.006 |
| | (0.008) | (0.009) |
| Firm FE | Yes | Yes |
| Year FE | Yes | Yes |
| Adjusted $R^2$ | 0.5348 | 0.5259 |
| Observations | 233,290 | 231,911 |

Second, to ensure that our results are not sensitive to short-term earnings management activities, we use long-term tax avoidance measures. In our baseline analysis, limited data availability does not allow us to compute several proxies of tax avoidance. Dyreng et al. (2008) document that annual ETR measures might be cyclical and asymmetrically persistent. In this section, to reduce yearly variation in our dependent variables, we substitute the dependent variables in Equation 4.1 with *3-year GAAP ETR* and *3-year Current ETR* computed as the three-year mean of *GAAP ETR* and *Current ETR*, respectively. Table 4.8 shows the results of this test. Column 1 (2) shows the results for the specification with *3-year GAAP ETR* (*3-year Current ETR*) as the dependent variable. In line with our baseline results, the coefficient on *Lost Bid* is positive and significant at the 1% level in both specifications (coeff: 0.021 in Column [1]; coeff: 0.025 in Column [2]). Interestingly, the adjusted $R^2$ for these specifications is around 23.5% larger than for the baseline results, suggesting that the effect of losing a bid for a public procurement contract on tax avoidance is a strategic decision.

**Table 4.8:** Average ETR

This table reports the results examining the effect of losing a bid for a public procurement contract on future tax avoidance, using as dependent variables the long-term ETRs. *3-year GAAP ETR* is the three-year average of the ratio of total income tax expense divided by pre-tax book income, multiplied by minus one. *3-year Current ETR* is the three-year average of the ratio of current income tax expense over pre-tax book income, multiplied by minus one. *GAAP ETR* and *Current ETR* are bounded between zero and minus one. *Lost Bid* is a dummy variable taking values of one after a firm loses a bid for a public procurement contract in *t-1*, and zero otherwise. *Size* is the natural logarithm of total assets in *t-1*. *ROA* is the ratio of operating income over lagged total assets in *t-1*. *Growth* is the sales growth rate from *t-2* to *t-1*. *Debt* is the ratio of total liabilities to total assets in *t-1*. *PPE* is the ratio of property, plant and equipment to total assets in *t-1*. Control variables are winsorized at the 1% and 99% level. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level.

| | (1)<br>3-year GAAP ETR | (2)<br>3-year Current ETR |
|---|---|---|
| Lost Bid | 0.021*** | 0.025*** |
| | (0.005) | (0.005) |
| Size | -0.003 | -0.007*** |
| | (0.002) | (0.002) |
| ROA | 0.096*** | 0.100*** |
| | (0.003) | (0.004) |
| Growth | 0.001*** | 0.001*** |
| | (0.000) | (0.000) |
| Debt | -0.091*** | -0.079*** |
| | (0.006) | (0.006) |
| PPE | -0.010 | -0.014 |
| | (0.009) | (0.009) |
| Firm FE | Yes | Yes |
| Year FE | Yes | Yes |
| Adjusted $R^2$ | 0.7686 | 0.7635 |
| Observations | 129,019 | 129,019 |

Third, to ensure that systematic differences between treated and control firms do not drive our results, we use entropy balancing (Hainmueller, 2012). Entropy balancing weights observations such that the mean, variance, and skewness of treated firms' control variables match the mean, variance, and skewness of control firms' control variables. This procedure offers three advantages relative to other matching methods (e.g., propensity score-matching): (1) retaining all data; (2) matching treated and control firms on three moments (i.e., mean, variance and skewness), rather than only variable means; and (3) fewer subjective research design choices are required. Table 4.9 shows the results of estimating Equation 4.1 on an entropy-balanced sample. In line with the baseline analysis, the coefficients on *Lost Bid* are positive and significant, irrespective of whether *GAAP ETR* (coeff.: 0.011) or *Current ETR* (coeff.: 0.019) is the dependent variable.

**Table 4.9:** Entropy-balanced Sample

This table reports the results examining the effect of losing a bid for a public procurement contract on future tax avoidance, using an entropy-balanced sample. *GAAP ETR* is the ratio of total income tax expense divided by pre-tax book income, multiplied by minus one. *Current ETR* is the ratio of current income tax expense over pre-tax book income, multiplied by minus one. *GAAP ETR* and *Current ETR* are bounded between zero and minus one. *Lost Bid* is a dummy variable taking values of one after a firm loses a bid for a public procurement contract in *t-1*, and zero otherwise.*Size* is the natural logarithm of total assets in *t-1*. *ROA* is the ratio of operating income over lagged total assets in *t-1*. *Growth* is the sales growth rate from *t-2* to *t-1*. *Debt* is the ratio of total liabilities to total assets in *t-1*. *PPE* is the ratio of property, plant and equipment to total assets in *t-1*. All estimations include firm and year fixed effects. Control variables are winsorized at the 1% and 99% level. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level.

| | (1)<br>GAAP ETR | (2)<br>Current ETR |
|---|---|---|
| Lost Bid | 0.011* | 0.019*** |
| | (0.007) | (0.007) |
| Size | -0.018*** | -0.016** |
| | (0.007) | (0.007) |
| ROA | 0.130*** | 0.111*** |
| | (0.018) | (0.019) |
| Growth | -0.001 | 0.000 |
| | (0.002) | (0.002) |
| Debt | 0.078*** | 0.072*** |
| | (0.021) | (0.020) |
| PPE | 0.018 | 0.017 |
| | (0.028) | (0.028) |
| Firm FE | Yes | Yes |
| Year FE | Yes | Yes |
| Adjusted $R^2$ | 0.5701 | 0.5615 |
| Observations | 129,019 | 129,019 |

Fourth, because recent econometric theory (e.g., Baker et al., 2022; Callaway and Sant'Anna, 2021) suggests that staggered DiD methodologies are biased, we use a stacked estimator approach as in Cengiz et al. (2019). In this approach, we estimate Equation 4.1 using event-specific datasets. This approach mitigates the concern that already-treated observations bias our results. Table 4.10 shows the results of this analysis. Consistent with our baseline results, the coefficient on *Lost Bid* is positive and significant, irrespective of whether *GAAP ETR* (coeff.: 0.018) or *Current ETR* (coeff.: 0.020) is the dependent variable.

**Table 4.10:** Stacked regression

This table reports the results examining the effect of losing a bid for a public procurement contract on future tax avoidance, using the stacked regression estimator approach (Cengiz et al., 2019). *GAAP ETR* is the ratio of total income tax expense divided by pre-tax book income, multiplied by minus one. *Current ETR* is the ratio of current income tax expense over pre-tax book income, multiplied by minus one. *GAAP ETR* and *Current ETR* are bounded between zero and minus one. *Lost Bid* is a dummy variable taking values of one after a firm loses a bid for a public procurement contract in *t-1*, and zero otherwise. *Size* is the natural logarithm of total assets in *t-1*. *ROA* is the ratio of operating income over lagged total assets in *t-1*. *Growth* is the sales growth rate from *t-2* to *t-1*. *Debt* is the ratio of total liabilities to total assets in *t-1*. *PPE* is the ratio of property, plant and equipment to total assets in *t-1*. All estimations include firm-cohort and year-cohort fixed effects. Control variables are winsorized at the 1% and 99% level. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level.

| | (1) GAAP ETR | (2) Current ETR |
|---|---|---|
| Lost Bid | 0.018*** | 0.020*** |
| | (0.006) | (0.006) |
| Size | -0.018*** | -0.021*** |
| | (0.001) | (0.001) |
| ROA | 0.058*** | 0.056*** |
| | (0.002) | (0.002) |
| Growth | -0.000** | -0.001*** |
| | (0.000) | (0.000) |
| Debt | 0.061*** | 0.067*** |
| | (0.004) | (0.004) |
| PPE | -0.009* | -0.004 |
| | (0.006) | (0.006) |
| Firm × Cohort FE | Yes | Yes |
| Year × Cohort FE | Yes | Yes |
| Adjusted $R^2$ | 0.6349 | 0.6307 |
| Observations | 501,001 | 498,059 |

Overall, these results suggest that biases arising from our research design choices are unlikely to affect our inferences, i.e., that firms that lose bids for public procurement contracts seek to increase their competitiveness by undertaking tax avoidance activities.

## 4.6 Further Analyses

In this section, we present further analyses to examine the effects of losing a public procurement contract. We conjecture that, after learning about their uncompetitiveness, treated firms increase their tax avoidance to improve their competitiveness. Besides increasing their tax avoidance, treated firms might take other actions to improve their operating efficiency. Further, if tax avoidance increases firms' competitiveness, it should increase their chances of winning subsequent public procurement contracts.

### 4.6.1 Improvement in Firm Operating Efficiencies

Aware of their lack of competitiveness, firms losing a bid for a public procurement contract likely engage in several activities to improve their competitiveness. The baseline analysis shows that treated firms increase their tax avoidance. However, these firms might also undertake other actions to improve their operations. To test this conjecture, we substitute our proxies for tax avoidance in Equation 4.1 with two measures capturing different dimensions of operating efficiency: (1) the return on operating assets (*ROA*), capturing the profitability of firms' operating activities, and (2) asset turnover (*Asset Turnover*), measuring the amount of sales generated for each unit of total assets. If firms losing a bid for a public procurement contract improve their operating efficiency, we should observe a positive and significant coefficient on *Lost Bid* for both specifications. Table C.1 shows the results for this test. In Column (1), we show the results using *ROA* as the dependent variable. In Column (2), we show the results using *Asset Turnover* as the dependent variable. In both specifications, the coefficient on *Lost Bid* is positive and significant at the 1% level. This result suggests that firms losing public procurement contracts improve their operating efficiency, as captured by improved *ROA* (coeff.: 0.009) and *Asset Turnover* (coeff.: 0.063).

### 4.6.2 Likelihood of Winning a Future Public Procurement Contract

We conjecture that firms losing a bid for a public procurement contract increase their tax avoidance activities to improve their competitiveness. If this conjecture is correct, then such firms should have a greater likelihood of winning future public procurement contracts. To test this conjecture, we use the sample of firms that lost bids for public procurement contracts issued in 2014 and examine whether their likelihood of winning a future public procurement contract is conditional on their level of tax avoidance activity in the period following 2014. We focus on public procurement contracts issued in 2014 for two reasons. First, 2014 is the first year for which we have data on public procurement contracts. Second, we can observe the effects of tax avoidance for the longest period before firms bid again for a public procurement contract.

Specifically, we substitute the dependent variable in Equation 4.1 with an indicator variable, *Contract Won*, which takes the value of one for the first year after 2014 in which the firm wins a public contract and zero otherwise. To avoid tainting our sample with winning firms, we drop a firm from our sample after it wins a public procurement contract. We also substitute our independent variable of interest in Equation 4.1 with variables capturing tax avoidance at *t-1.* If the likelihood of winning a contract is conditional on firm competitiveness as captured by the level of tax avoidance, we expect positive coefficients on our measures of tax avoidance.

Table C.2 shows the results of this test. Column (1) shows the results when *GAAP ETR* is our independent variable of interest, while Column (2) shows the results when *Current ETR* is our independent variable of interest. Results for both specifications show a positive coefficient on our tax avoidance measures, albeit only significant for *GAAP ETR* (coeff.: 0.045). These results validate the conjecture that increased tax avoidance makes firms more competitive in public procurement contracts, as captured by the higher likelihood of winning a public procurement contract.

Further, we test whether firms that lost a bid for a public procurement contract, engaged in tax avoidance, and subsequently won a public procurement contract, change their tax avoidance behavior after winning. Winning a public procurement contract might cause firms to reconsider the costs and benefits of undertaking tax avoidance activities. On the one hand, winning a public procurement contract suggests that the firm has achieved its objective and might consider reducing its tax avoidance activities. On the other hand, if the firm's objective is not to win just one contract but to continue winning public procurement contracts in the future, it will have to maintain its competitiveness by continuing to undertake tax avoidance activities. Hence, it is unclear whether winning a public procurement contract changes firms' tax avoidance behavior.

To undertake this test, we use the sample of firms that lost public procurement contracts in 2014 (the first year in our sample period) and substitute the variable *Lost Bid* in Equation 4.1 with an indicator variable *Won Bid* that takes the value of one for periods after a firm wins a public procurement contract, zero otherwise. Put differently, *Won Bid* captures whether firms that lose one public procurement contract and subsequently win another change their tax avoidance activities after winning. Table C.3 shows the results of this analysis. Column (1) shows the results when *GAAP ETR* is the dependent variable, while Column (2) shows the results when *Current ETR* is the dependent variable. In both specifications, the coefficient on *Won Bid* is insignificant, suggesting no changes in tax avoidance activity after winning a public procurement contract.

## 4.7 Conclusion

Public procurement constitutes a pivotal channel for transferring resources from governments to firms (Bosio et al. 2023). Given the impact of public procurement on firm growth (Ferraz et al. 2015; Hvide and Meling, 2023), regulators increasingly encourage firms' participation in the public procurement process (e.g., European Court of Auditors, 2023). In this study, we investigate the effect of losing a bid for a public procurement contract on firms' tax avoidance. We conjecture that the outcome of a public procurement contract provides firms with a unique opportunity to directly learn about their competitiveness relative to peers. We posit that firms participating and losing a public procurement contract learn that they are uncompetitive relative to the winning firm. This learning incentivizes firms to change their tax avoidance behavior. On the one hand, firms might seek to improve their competitiveness by lowering their expenses through tax avoidance. On the other hand, losing a public procurement contract reduces firms' anticipated profits, and hence the marginal benefits of tax avoidance.

Using a sample of Italian public procurement contracts from 2014 to 2020, we find that firms increase their tax avoidance after losing a bid for a public procurement contract. These effects are more pronounced when firms face more competition for public procurement contracts and have less financial slack to improve their competitiveness. By improving competitiveness through higher tax avoidance, firms increase their likelihood of winning a subsequent public procurement contract. Further, we document that losing firms improve their operating efficiency in other ways beyond tax avoidance.

Our findings are subject to the following caveats. First, due to limited data availability, we do not observe the amount of cash taxes paid but capture tax avoidance using the effective tax rate. In this respect, previous literature documents several limitations of effective tax rates, such as its inability to capture the deferral of tax payments or tax avoidance from artificially lowering pre-tax income (Hanlon and Heitzman, 2010). Second, due to the composition of our sample, we cannot directly speak to the effect of losing a public procurement contract in other settings. Despite these limitations, our study identifies a potential distortion of increased competition in public procurement contracts.

# Appendix C

## C.1 Supplementary Material to Chapter 4

**Table C.1:** Improvements in Operating Efficiency

This table reports the results examining the effect of losing a bid for a public procurement contract on operating efficiency. *ROA* is the ratio of operating income over lagged total assets. *Asset Turnover* is the ratio of sales over lagged total assets. *Lost Bid* is a dummy variable taking values of one after a firm loses a bid for a public procurement contract in *t-1*, and zero otherwise. *Size* is the natural logarithm of total assets in *t-1*. *Growth* is the sales growth rate from *t-2* to *t-1*. *Debt* is the ratio of total liabilities to total assets in *t-1*. *PPE* is the ratio of property, plant and equipment to total assets in *t-1*. All estimations include firm and year fixed effects. Control variables are winsorized at the 1% and 99% level. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level.

| | (1) ROA | (2) Asset Turnover |
|---|---|---|
| Lost Bid | 0.009*** | 0.063*** |
| | (0.002) | (0.014) |
| Size | -0.089*** | -0.038*** |
| | (0.002) | (0.012) |
| ROA | 0.097*** | 0.093*** |
| | (0.008) | (0.030) |
| Growth | -0.002*** | 0.032*** |
| | (0.000) | (0.002) |
| Debt | 0.054*** | 0.566*** |
| | (0.008) | (0.041) |
| PPE | -0.006 | -0.410*** |
| | (0.008) | (0.051) |
| Firm FE | Yes | Yes |
| Year FE | Yes | Yes |
| Adjusted $R^2$ | 0.5976 | 0.8292 |
| Observations | 129,019 | 129,019 |

## **Table C.2:** Likelihood of Winning

This table reports the results examining how tax avoidance after losing a bid for a public procurement contract affects the likelihood of winning subsequent contracts. The estimation sample is restricted to firms losing a public procurement contract in 2014. *Contract Won* is a dummy variable taking values of one in the year in which a firm wins a public procurement contract, and zero otherwise. *GAAP ETR* is the ratio of total income tax expense divided by pre-tax book income in *t-1*, multiplied by minus one. *Current ETR* is the ratio of current income tax expense over pre-tax book income in *t-1*, multiplied by minus one. *GAAP ETR* and *Current ETR* are bounded between zero and minus one. *Size* is the natural logarithm of total assets in *t-1*. *ROA* is the ratio of operating income over lagged total assets in *t-1*. *Growth* is the sales growth rate from *t-2* to *t-1*. *Debt* is the ratio of total liabilities to total assets in *t-1*. *PPE* is the ratio of property, plant and equipment to total assets in *t-1*. Control variables are winsorized at the 1% and 99% level. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level.

| | (1)<br>Won Bid | (2)<br>Won Bid |
|---|---|---|
| GAAP ETR | 0.045** | |
| | (0.023) | |
| Current ETR | | 0.032 |
| | | (0.022) |
| Size | 0.015*** | 0.015*** |
| | (0.004) | (0.004) |
| ROA | -0.069 | -0.066 |
| | (0.054) | (0.053) |
| Growth | -0.000 | -0.000 |
| | (0.004) | (0.004) |
| Debt | -0.010 | -0.013 |
| | (0.028) | (0.028) |
| PPE | 0.095** | 0.097** |
| | (0.038) | (0.038) |
| Year FE | Yes | Yes |
| Adjusted $R^2$ | 0.0824 | 0.0820 |
| Observations | 5,171 | 5,156 |

**Table C.3:** Tax Avoidance after Winning a Bid

This table reports the results examining the effect of winning subsequent bids for public procurement contracts on future tax avoidance. The estimation sample is restricted to firms losing a public procurement contract in 2014. *GAAP ETR* is the ratio of total income tax expense divided by pre-tax book income, multiplied by minus one. *Current ETR* is the ratio of current income tax expense over pre-tax book income, multiplied by minus one. *GAAP ETR* and *Current ETR* are bounded between zero and minus one. *Won Bid* is a dummy variable taking values of one after a firm wins its first bid for a public procurement contract, and zero otherwise. *Size* is the natural logarithm of total assets. *ROA* is the ratio of operating income over lagged total assets. *Growth* is the sales growth rate from *t-2* to *t-1*. *Debt* is the ratio of total liabilities to total assets. *PPE* is the ratio of property, plant and equipment to total assets. All estimations include firm and year fixed effects. Control variables are winsorized at the 1% and 99% level. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level.

| | (1)<br>GAAP ETR | (2)<br>Current ETR |
|---|---|---|
| Won Bid | 0.012 | 0.013 |
| | (0.008) | (0.008) |
| Size | -0.021* | -0.018 |
| | (0.011) | (0.011) |
| ROA | 0.067** | 0.046 |
| | (0.031) | (0.031) |
| Growth | 0.006*** | 0.005** |
| | (0.002) | (0.002) |
| Debt | 0.050 | 0.050 |
| | (0.037) | (0.038) |
| PPE | 0.063 | 0.052 |
| | (0.054) | (0.051) |
| Firm FE | Yes | Yes |
| Year FE | Yes | Yes |
| Adjusted $R^2$ | 0.4981 | 0.4905 |
| Observations | 7,599 | 7,590 |

# Chapter 5

# Conclusion

This thesis investigates how interactions with government agencies—a crucial stakeholder—shape private firms' information environment. It consists of three thematically linked chapters exploring the Italian private firm setting. While Chapter 2 explores the implications of government certifications on resource allocation, Chapters 3 and 4 explore how private firms acquire information through the public procurement process.

Chapter 2 examines how government certifications on firms' regulatory compliance affect firms' access to public procurement and the efficiency of procurement contract allocation. I explore the introduction of the Legality Rating in Italy, a government certification rating firms based on their efforts to prevent misconduct and criminal infiltration. I find that the certification improves firms' access to public procurement. Furthermore, I show that certified firms execute their contracts more efficiently with fewer cost overruns, modifications, and delays. By estimating aggregate effects, I show that the certification increased participation rates in public procurement. Overall, these results document how government certifications can be a cost-effective method to improve resource allocation.

Chapter 3 investigates how information spillovers among product market peers mitigate private firms' information frictions regarding government programs. We develop a novel definition of product market peers based on firms' common bids for public procurement contracts. Using this definition, we find that firms are more likely to obtain the Legality Rating after competing in a public procurement contract with a certified peer. In cross-sectional tests, we show that firms obtain the certification primarily to reduce certified peers' competitive advantage. This study identifies a novel channel through which firms acquire information—public procurement networks.

Chapter 4 examines the effect of participating and losing a bid for a public procurement contract on firms' tax avoidance. We predict that disclosing the outcome of a public procurement contract allows firms to learn about their competitiveness relative to peers. Consistent with this prediction, we find that firms engage in more tax avoidance to improve their competitive position after losing a bid for a public procurement contract. Furthermore, we show that increased tax avoidance raises firms' likelihood of winning a subsequent public procurement contract. This paper provides novel evidence on how disclosing public procurement outcomes facilitates learning about participating firms' competitiveness.

Overall, these three studies enhance our understanding of the factors shaping private firms' information environment. In the last two decades, private firms' relevance in the global economy has remarkably increased owing to their lower regulatory burden and the increased availability of private capital. As a result, there is a growing regulatory and academic debate on the optimal disclosure and auditing requirements for private firms. By showing how interactions with government agencies shape private firms' information environment, these three studies contribute to this debate. In particular, they document how government certifications can improve the allocation of public resources and remove some constraints firms face in product markets. Furthermore, they show how participating in public procurement can improve private firms' information set.

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