Essays on Regional and Public Economics

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Summary

At the time of writing this thesis, the world faces profound challenges: geopolitical conflicts, economic turbulence, and the climate crisis among them. In response, governments are implementing large-scale investment programs in defense, infrastructure, and green technologies. While the political discussion focuses on the source of the funds, the purpose and the destination, deeper questions about efficiency and spatial allocation of public resources, particularly with regard to how governments procure goods and services and fund innovation, remain insufficiently addressed.

This dissertation contributes to understanding and improving public spending allocation by examining two core instruments: public procurement and R&D subsidies. It addresses three central questions:

1. Is there home bias in public procurement and in which contracts predominantly?
2. Do local fiscal incentives lead to a home bias in public procurement?
3. What are the direct and spillover effects of public R&D funding to private firms?

Using large-scale data and applied empirical methods, the three chapters of this thesis explore these research questions. Together, they investigate how institutional, spatial, and fiscal factors shape public spending decisions.

Chapter 1: Home Bias in Public Procurement: Proximity, Digitalization and Thresholds

This chapter analyzes contract-level procurement data across EU countries from 2008 to 2023. Using a gravity model, it documents a home bias in public procurement: public authorities prefer firms located near themselves, even when the place of performance is located farther away. However, this spatial preference largely disappears in digital industries, where distance plays a minimal role in supplier selection. This contrast suggests that specialized high-tech industries can mitigate home bias, while networks with local suppliers may sustain in more traditional sectors.

It also finds that greater competition and digitalized procurement processes mitigate home bias, while

complex tender designs tend to reinforce it. Finally, the chapter explores strategic bunching of contract values just below legislative thresholds and finds that contracting authorities who engage in such bunching exhibit home bias, whereas those who do not bunch do not show such bias.

Chapter 2: Local Business Tax Rates and Localization in Public Procurement

Chapter 2 investigates one potential mechanism behind home bias: fiscal incentives tied to local business taxation in Germany. Municipalities can set their own tax rates and benefit directly when local firms receive contracts via a tax-cashback from the firm's increased taxable profit. Using a three-stage control function approach and exploiting exogenous variation from census-based population shocks, the chapter shows that higher local business tax rates are associated with a higher likelihood that contracts are awarded to local firms relative to non-local competitors.

These results highlight a tension between EU-level goals of open procurement and local incentives for revenue recapture, raising broader questions about policy alignment in federal systems.

Chapter 3: Subsidizing R&D in Germany: Direct and Spillover Effects

The final chapter shifts focus to innovation policy, analyzing the effects of public R&D subsidies in Germany from 2000 to 2020. Using matched data at the firm and cluster levels, and relaxing traditional SUTVA assumptions, the chapter estimates both direct effects on recipient firms and spillover effects on nearby non-funded firms.

Findings reveal that while funded firms benefit directly (via increased fixed assets, employment or increased innovation), spillover effects on innovation outputs measured in quality-adjusted patents, are even stronger in clustered environments. This suggests that targeted R&D funding within regional ecosystems can foster broader innovation capacity.

Contribution

This thesis contributes to the broader discussion on the allocation of public funds by offering empirical evidence on procurement decisions and innovation funding. In doing so, the dissertation contributes to a growing body of research at the intersection of public economics, regional economics, and innovation economics. It critically assesses the policy objectives of EU public procurement rules and identifies potential pathways to mitigate home bias and improve market openness. The findings in Chapter 3 provide actionable insights for designing innovation funding policies that maximize both direct and spillover effects, supporting more effective and inclusive innovation ecosystems.

Beyond the policy implications, the dissertation advances empirical methodology. While grounded in established econometric frameworks, the thesis applies and extends these approaches in novel ways tailored to the research question, for example by extending the classic two-location gravity model to three locations.

Moreover, the analysis draws on comprehensive, up-to-date, and large-scale datasets that allow for detailed analyses at multiple levels of observation.

CHAPTER 1

Home Bias in Public Procurement: Proximity, Digitalization and Thresholds

Chapter Abstract

Public procurement, a major component of government spending, is often treated in aggregate terms, with limited attention to the spatial dynamics that shape contract allocation. This paper examines spatial patterns in public procurement contracts across Europe from 2008 to 2023 using a gravity model commonly applied in the trade literature. By analyzing contracts where the location of the contracting authority differs from the place of work, service, or delivery, I identify a preference for firms located near the procuring authority as the primary spatial driver of contract awards, rather than actual trade costs such as travel or shipping expenses. This preference can be interpreted as a form of home bias in public procurement.

The paper additionally analyzes digital industries, which often face minimal or zero travel and shipping costs. In contrast to traditional sectors, the spatial preference largely disappears in digital industries, indicating that specialized high-tech industries can mitigate home bias. Furthermore, I explore how contract-specific factors, including the number of bidders, digitalized tendering, and contract complexity, moderate the impact of spatial proximity on procurement decisions. I find that digitalized tendering can decrease home bias slightly, while complex tenders reinforce it.

To assess strategic behavior in procurement, I also use a regression-discontinuity density estimator to detect manipulation of award values below EU thresholds. The analysis shows that contracting authorities engaging in such manipulation exhibit a home bias, while non-bunching authorities do not.

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1.1 Introduction

Public procurement, the purchase of goods and services by governments and public entities, accounts for approximately 12% of GDP in OECD countries (OECD, 2021). Given its economic importance, policymakers have set the goal of making procurement more efficient, transparent, and aligned with broader policy objectives (European Commission, 2024; OECD, 2019, 2024). Unlike private-sector purchasing, public procurement is subject to political, administrative, and regulatory constraints, as public authorities must balance economic efficiency with goals including social welfare, sustainability, equity, and innovation. However, these complexities also make procurement prone to inefficiencies, favoritism, and corruption (European Commission, 2015; Organisation for Economic Co-operation and Development (OECD), 2024).

One persistent issue is home bias, where contracting authorities disproportionately award contracts to local firms, beyond what would be economically reasonable. Despite the 2004 EU Procurement Directive, which aimed to harmonize rules, promote competition, and increase transparency, procurement remains highly localized (European Parliament and Council of the European Union, 2004; Herz and Varela-Irimia, 2020). This raises concerns that hidden barriers, such as political discretion, social networks, or administrative preferences continue to impact procurement decisions, limiting market openness and efficiency.

This paper investigates whether home bias exists in EU public procurement and how it varies across industries, institutional settings, and tender design. I ask whether public contracts are awarded primarily based on economic trade costs or whether discretionary favoritism plays a dominant role, leading authorities to prefer firms located near them regardless of efficiency.

To answer this question, I estimate extended gravity models of procurement flows on the contract and regional level using data on EU public procurement contracts with contract award values as the main outcome. Unlike standard gravity models, which typically use two locations, I incorporate three: the location of the contracting authority, the location of the winning firm, and the place of performance, service, or delivery. This structure allows me to identify and compare the effects of each bilateral distance on procurement outcomes. I define home bias as the relative importance of the firm-authority distance versus the firm-place distance. In the absence of home bias, mostly the distance between the firm and the place of performance should matter, since it reflects the actual cost of execution. If, instead, the firm-authority distance dominates, this suggests that authorities disproportionately favor nearby firms, potentially due to local ties or favoritism.

I find that in most settings, the distance between the contracting authority and the winning firm is the strongest predictor of procurement outcomes, suggesting a home bias. By contrast, this pattern is not present in digital industries, where firm–authority proximity does not play a significant role. Instead, in digital industries, proximity to the place of performance matters, likely reflecting implementation or on-site service requirements rather than favoritism. Additionally, digital tendering processes, such as fully electronic auctions, appear to mitigate home bias. I also show that home bias tends to be stronger in more complex tenders, suggesting that contracting authorities may use design complexity to steer awards toward preferred bidders. Finally, I examine whether authorities strategically bunch contract values just below EU procurement thresholds to avoid stricter regulation. Strikingly, I find that home bias is present only among these bunching authorities, reinforcing the view that discretion and regulatory evasion play a role in enabling favoritism.

This paper contributes to the literature on home bias, favoritism, and inefficiencies in public procurement. Prior studies highlight the prevalence of localization in procurement markets (Cox et al., 2024) and show that regional public authorities exhibit stronger home bias than national authorities (Garcıa-Santana and Santamarıa, 2021). Herz and Varela-Irimia (2020) further document border effects in procurement flows, indicating that administrative and political frictions shape market access. Beyond documenting home bias, recent work emphasizes its economic costs. Baránek and Titl (2024) find that politically connected firms in the Czech Republic receive procurement contracts at inflated prices—approximately 6% above market rates, without quality improvements, increasing total procurement expenditures by 0.36%. Similarly, Celis Galvez et al. (2025) show that greater discretion in procurement leads to higher prices and reduced competition, reinforcing concerns about inefficient public spending (Palguta and Pertold, 2017; Szucs, 2024).

Several studies investigate the mechanisms that sustain this localization, distinguishing between benign frictions and discretionary favoritism. Wei et al. (2025) use bid-level data from China to disentangle information frictions from incentive distortions. Their findings suggest that bureaucratic discretion and career incentives often drive local favoritism, even when more competitive or efficient bids exist. Theoretical insights by Celentani and Ganuza (2002) reinforce this view, showing that increasing competition does not always reduce corruption; in fact, under certain institutional settings, it can exacerbate it when discretionary power is high. These mechanisms suggest that proximity may be favored not for efficiency reasons, but due to relational contracting, informational advantages for insiders, or political incentives.

This paper contributes to the literature on public procurement, trade frictions, and institutional favoritism in four ways. First, I develop a novel approach to identifying home bias by comparing the relative effects of firm-authority and firm-place distances. Second, I extend the standard gravity framework by incorporating three distinct locations allowing me to model procurement flows using location triplets as the unit of observation. Third, the use of highly granular contract-level data enables a detailed analysis of how home bias varies across contract-level characteristics in the tendering process. Finally, I provide new evidence that home bias is concentrated among authorities that strategically bunch contract values just below EU regulatory thresholds.

The remainder of the paper is structured as follows. Section 2 describes the dataset and key variables. Section 3 outlines the empirical methodology, including the gravity model framework. Section 4 presents the results and robustness checks. Section 5 discusses policy implications and concludes.

1.2 Data

To analyze the extent and drivers of home bias in public procurement, I use data from Tenders Electronic Daily (TED), the official platform for EU-wide procurement notices (GROW G4, European Commission, 2024), complemented by the OpenTender database from the DigiWhist platform (Digiwhist Project Team, 2025). These datasets provide a comprehensive record of public procurement contracts across EU member states and Norway, the United Kingdom, Switzerland, Iceland, Liechtenstein, and North Macedonia from 2008 to 2023. The TED dataset contains detailed procurement information submitted by public authorities through standardized forms, including contract values, contract types (works, services, or supplies), and the locations of both the contracting authority and the winning firm. Additionally, it records the place of performance, where the contracted work or service is carried out. This allows the analysis to distinguish between the firms' proximity to the authority and proximity to the location where the contract is executed

To enrich this information submitted through TED, I incorporate data from OpenTender, which aggregates procurement data from national and international sources and enhances it with web-scraped details from tender portals. This supplementary dataset provides additional contract characteristics, including the length of tender descriptions, the number of award criteria, which serve as proxies for tender complexity. It also includes information on the mode of tendering, distinguishing between digital and in-person auctions, as well as the number of total and electronic bids submitted. These additional details allow for a more nuanced analysis of the determinants of procurement outcomes and the extent to which digitalization influences contract allocation patterns.

A key feature of the dataset is its ability to distinguish between three geographical locations in procurement contracts, as illustrated in Figure 1.1: the contracting authority, which is the public entity issuing the tender; the winning firm, which is the firm awarded the contract; and the place of performance, where the contracted goods or services are delivered. This distinction is crucial for assessing whether procurement decisions prioritize proximity to the contracting authority, or proximity to the place of performance. The identifying variation arises in contracts where the NUTS3 region of the contracting authority differs from the NUTS3 region of the place of performance. This occurs in 26.3% of contracts, while in 73.7% of contracts, the NUTS3 regions of both the contracting authority and the place of performance align.

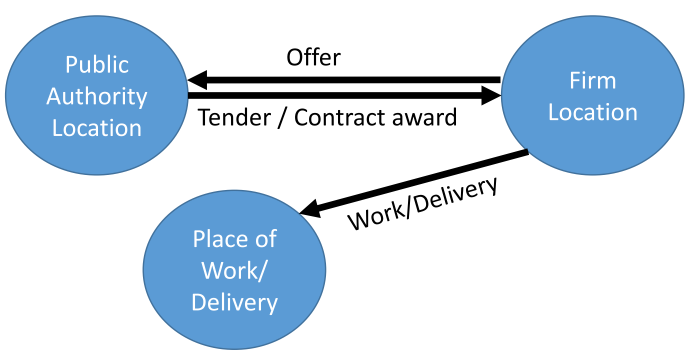

Figure 1.1: Three different locations covered in the data: Firm location, Public Authority location and location of work, service, or delivery

To quantify these spatial relationships, I aggregate all location data to the NUTS3 regional level. I geocode the exact locations of winning firms and public authorities for contracts that do not provide a NUTS3 region, and add the NUTS3 region using a spatial join with shapefiles of European NUTS3 regions. I then calculate geocoded centroid-to-centroid distances to compute three key distance measures: the distance between the winning firm and the contracting authority, the distance between the winning firm and the place of performance, and the distance between the contracting authority and the place of performance. Note that for any pair of locations that fall within the same NUTS3 region, the centroid-to-centroid distance is recorded as zero. In addition to these distance measures, I construct indicators, including whether two NUTS3 regions share a border, whether they belong to the same NUTS2 region, and whether they are located within the same country. The dataset also includes industry classifications via Common Procurement Vocabulary (CPV) codes, which allow for contract categorization at varying levels of aggregation. Based on these classifications, I construct an indicator for digital industries, covering sectors such as telecommunications, software, and IT services (see Table A.2 in the appendix for a complete list of CPV codes I define as digital).

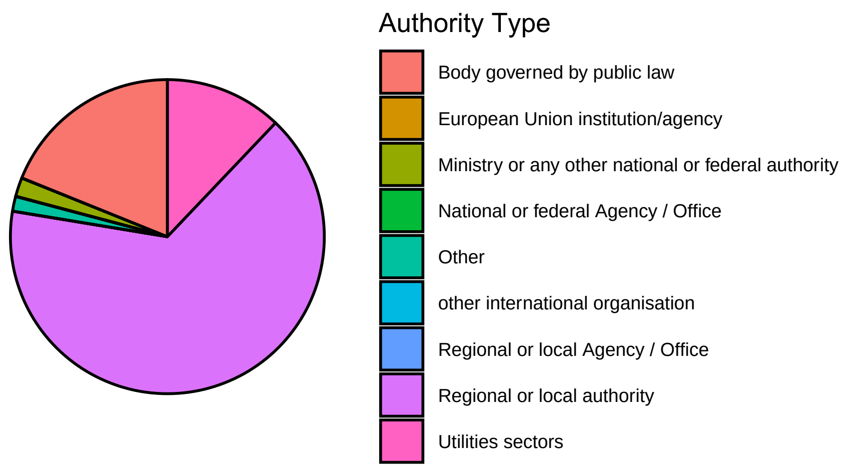

Figure 1.2: The different types of contracting authorities

Figure 1.2 illustrates the different types of public procurement authorities. The majority of contract value is awarded by regional and local authorities, which is important because the type of contracting authority influences the spatial distribution of decision-making. Regional authorities typically make decisions regarding local public procurement, in contrast to national governments that award contracts for locations that may be far from their own jurisdiction. Consequently, local authorities have a direct impact on their regions and are often motivated to act in ways that benefit their local areas. Another notable category of public authorities with a relatively large share of contract value are bodies governed by public law, e.g. public university hospitals or student service organizations at universities.

Figures 1.3 to 1.6 display maps of the EU NUTS3 regions included in my study, illustrating their shares in public procurement markets as origin (firms) or destination (contracting authorities). The maps in Figures 1.3 and 1.4 show the number of contracts per capita by destination NUTS3 region (Figure 1.3) and origin NUTS3 region (Figure 1.4). The maps in Figure 1.5 and 1.6 show the value awarded per

1,000 inhabitants in Euro by destination NUTS3 region (Figure 1.5) and origin NUTS3 region (1.6). Purple shades represent less contracts or less award value, respectively, while yellow shades represent more contracts or value, respectively. The values for the maps are log-transformed.

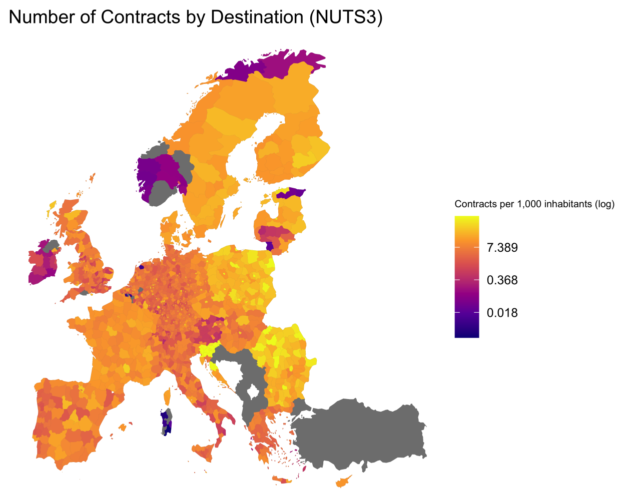

Figure 1.3: Map with number of contracts concluded by contracting authorities in the respective regions

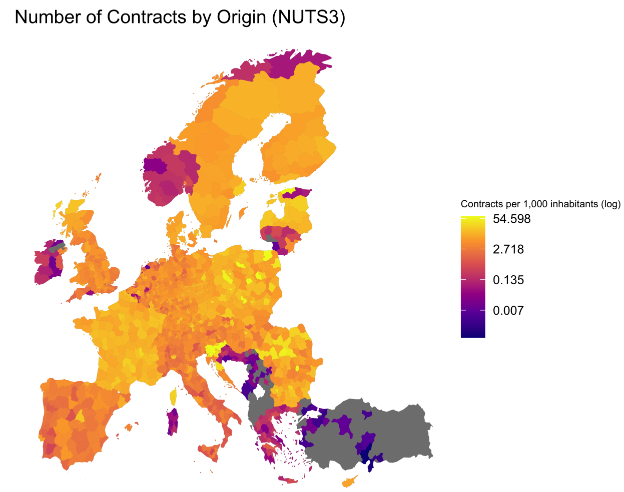

Figure 1.4: Map with number of contracts won by firms in the respective regions

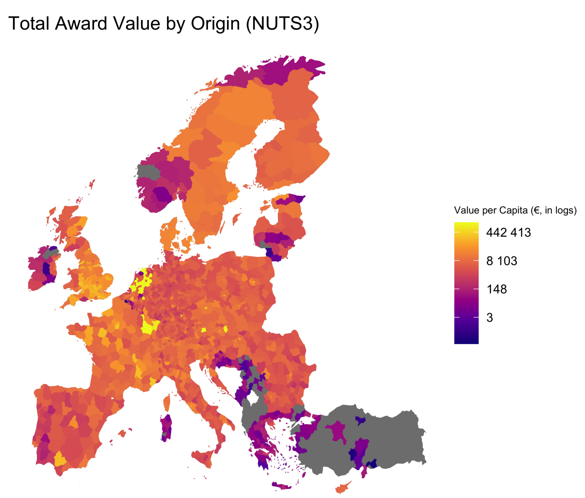

Figure 1.5: Map with sum of contract values of contracts concluded by contracting authorities in the respective regions

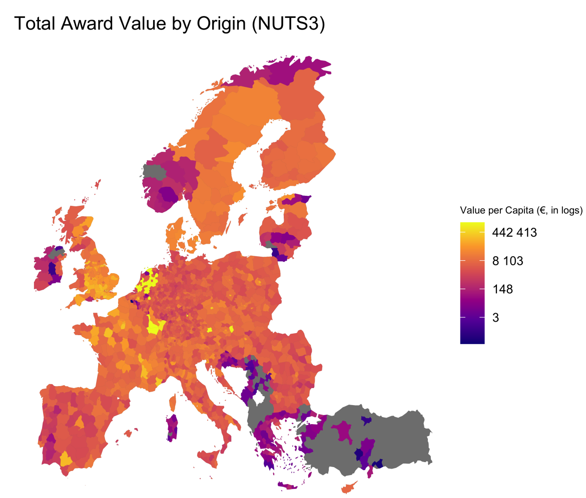

Figure 1.6: Map with sum of contract values of contracts won by firms in the respective regions

Taken together, the maps reveal that contract frequency (counts) and contract volume (monetary award) exhibit different spatial signatures: counts are relatively widely distributed across many NUTS3 regions, whereas cumulative award values are markedly concentrated in a much smaller set of regional hubs, producing a more pronounced, spiky pattern on the value maps.

The map of contract counts by destination shows substantial activity across Central Europe and also in many Eastern European NUTS3 regions, indicating a broad geographic footprint of public buyers. By contrast, the destination value map displays clear outlier regions with very high per-capita award values, which suggests that a limited number of administrative or economic hubs account for a disproportionate share of total procurement spending. The origin maps demonstrate a more even spatial dispersion of winning firms compared with destination values, but with notable density peaks in core regions of Central Europe, Benelux and large German NUTS3 units, and a lower presence toward the EU external borders.

This pattern implies that while suppliers are geographically widespread, their winning power and participation cluster more strongly in established economic centers. The divergence between count-based and value-based patterns is important because it indicates two interacting mechanisms — (i) proximity and local networks that shape the probability of winning contracts across many regions, and (ii) concentrated demand or institutional capacity in a few hubs that generate very large contract values.

To assess whether public procurement exhibits home bias, I descriptively examine whether contracts are more localized than would be economically justified on the country level. A strong localization bias would indicate that public authorities favor local firms for reasons beyond efficiency considerations, such as political or network-based influences, rather than simply minimizing trade costs. To explore this, I compare the localization of public procurement with general trade patterns in goods and services. Table 1.1 presents a comparison of trade shares and procurement shares, drawing on WIOD input-output tables (Timmer et al., 2015) for traded goods and TED data (GROW G4, European Commission, 2024) for public procurement flows across EU countries. For each country, I contrast the share of domestically produced goods and services traded internationally with the share of those exchanged within national borders. This comparison is conducted separately for public procurement contracts and general trade flows, allowing me to assess whether localization patterns in procurement deviate systematically from those observed in broader trade relations.

Table 1.1: Comparison of domestic vs. non-domestic trade with procurement flows

	Trade Share %		Procurement Share %	
Country	Foreign	Domestic	Foreign	Domestic
BEL	78.97	21.03	0.64	99.36
BGR	75.46	24.54	5.21	94.79
GER	73.43	26.57	0.00	100.00
ESP	75.58	24.42	1.17	98.83
FIN	74.60	25.40	3.09	96.91
FRA	77.12	22.88	0.01	99.99
ITA	72.30	27.70	1.92	98.08
POL	73.15	26.85	5.50	94.50
SWE	75.82	24.18	4.50	95.50

Table 1.1 presents a comparison of domestic versus foreign trade share and domestic versus foreign procurement share for various EU countries. While trade flows for most countries tend to have a substantial

share of foreign goods ranging from 70% to 80%, public procurement flows show a much stronger preference for domestic firms. The procurement share for domestic suppliers in these countries is overwhelmingly high, often approaching 100%[1]. This stark contrast suggests that public procurement in the EU tends to be highly localized on the country level, deviating from what might be expected in the case of goods trade, where cross-border transactions are more prevalent.

To provide an initial assessment of localization in public procurement on the sub-national level, Table 1.2 presents summary statistics on the localization of public procurement contracts. I differentiate between two spatial relationships: (i) between the contracting authority and the winning firm's location, and (ii) between the winning firm's location and the place of performance.

Table 1.2: Localization: The share of cumulated contract value and number of contracts, respectively, that were concluded within the same region, the same or neighboring region or the same country of the actors.

	All observations		Electronic Auction		Digital Industries	
	Value	Contract	Value	Contract	Value	Contract
Firm-Authority						
Same NUTS3	55	26.3	38.6	29.6	0.6	37.3
Same Country	99.6	71.9	79.7	76.7	99.4	75.5
Neighboring NUTS3	94	75.2	80.3	83.7	1.1	75.5
Firm-Place						
Same NUTS3	0.1	12.8	13.4	19.8	5	11.3
Same Country	99.9	85.3	89.3	93.7	73.2	81.4
Neighboring NUTS3	99.98	86.5	45.5	86.9	81.9	75.6

The data show that public authorities predominantly award contracts to firms within the domestic country. In terms of NUTS3 regions, most contracts are awarded within the same NUTS3 region or to a firm in a neighboring region. Notably, the localization is stronger for the relationship between the contracting authority and the firm compared to the relationship between the place of performance and the winning firm's location. Specifically, 26.3% of contracts involve both the contracting authority and the winning firm being located within the same NUTS3 region, whereas only 12.8% of contracts have the place of performance and the firm in the same NUTS3 region.

At the country level, the pattern reverses. This likely reflects the broader geographic scope of some contracting authorities, particularly within the EU, which operate on a larger, sometimes international, scale. This is especially true for entities like European Union institutions and national governments, which tend to source contracts from across broader geographic areas. For instance, 71.9% of contracts are awarded to firms located within the same country as the contracting authority, while the share for the place of performance and firm location within the same country is higher at 85.3%.

The localization statistics for contracts concluded via electronic auctions reveal a nuanced pattern. In terms of the number of contracts, electronic auctions appear even more localized than the overall sample: 29.6% of contracts are awarded to firms located in the same NUTS3 region as the contracting authority,

[1]Note that values are rounded, which means 100% does not reflect complete domestic procurement.

and 19.8% to firms located in the same region as the place of performance - both higher shares than in the full sample. However, the relative localization between the authority–firm and place–firm relationships remains very similar to the overall pattern, suggesting that electronic auctions do not substantially alter home bias when considering the count of contracts. However, when examining contract values, the pattern shifts. For electronic auctions, the share of contract value awarded to firms in the same NUTS3 region as the contracting authority is lower than in the overall sample, while the share awarded to firms located in the same region as the place of performance is higher. This indicates a rebalancing: high-value contracts in electronic auctions tend to be sourced closer to the place of performance and less close to the contracting authority. This change in relative localization is consistent with the interpretation that electronic auctions may slightly reduce home bias by weakening the advantage of local authority–firm proximity.

In digital industries, which include sectors such as software development and IT infrastructure services, home bias appears stronger than in the overall sample when measured by the number of awards. Public authorities award 37.3% of contracts in digital industries to firms located in the same NUTS3 region as the authority, substantially higher than in the full sample. By contrast, only 11.3% of contracts are awarded to firms located in the same region as the place of performance, a share that is similar to the overall pattern. However, when considering contract values rather than counts, the pattern reverses. Contract values in digital industries are less localized with respect to the contracting authority and more localized with respect to the place of performance. This suggests that high-value digital projects are not necessarily awarded to locally favored firms. Instead, for these larger and more complex contracts, public authorities appear to rely less on local supplier familiarity—indicating weaker home bias—and more on ensuring that the contractor is geographically aligned with the actual location of service delivery or implementation.

These initial descriptive findings suggest that localization may not be solely driven by economic factors such as trade costs. Instead, it may also reflect networks of contracting authorities and political economy considerations, where local or regional connections influence procurement decisions. To formally assess these mechanisms, I employ a gravity model framework, a standard empirical approach for analyzing bilateral trade flows. This allows me to control for trade barriers and to quantify the relative importance of economic trade costs versus discretionary factors in explaining procurement localization.

1.3 Empirical Methodology

The gravity model is well-suited for analyzing public procurement flows due to its ability to account for spatial frictions such as distance and institutional barriers. It enables me to estimate the effect of spatial frictions, such as regional characteristics and disentangle those from other factors influencing procurement decisions. Therefore, it helps reduce unobserved spatial heterogeneity, but also to estimate observed spatial frictions explicitly.

I begin by specifying a standard gravity-type equation to analyze public procurement flows between NUTS3 regions. Using the standard gravity terminology, I define the region where the buying public authority is located as destination i and the region where the contract-winning firm is located as origin j. The dependent variable is the procurement flows X_{ijt} between these regions in year t. Procurement flows are measured as the aggregated contract award values in Euro between firms in region j and public authorities in region i.

Following Anderson and Van Wincoop (2003), I incorporate fixed effects for origin-time I_{it} and destination-time I_{jt}, as well as year fixed effects I_t, capturing time-varying factors specific to regions and general trends.

Multilateral resistance terms capture the relative ease or difficulty of engaging in trade with all potential partners, while bilateral resistance terms capture the relative ease of bilateral trade for a region pair. Allowing the region fixed effects to vary over time controls for the multilateral resistance terms. However, the bilateral resistance terms are not part of the fixed-effects as those are the main coefficients of interest and need to be estimated explicitly.

Given the prevalence of NUTS3 region pairs that do not engage in procurement from each other, the distribution of the dependent variable is skewed, with a high number of zero observations. To address this, I use the Pseudo-Poisson Maximum Likelihood (PPML) estimator, which has become standard for zero trade flows in gravity models (Silva and Tenreyro, 2006).

$$X_{ijt} = \exp(\beta_0 + \beta_1 \log dist_{ij} + \beta_2 bilateral_{ij} + \beta_3 I_{it} + \beta_4 I_{jt} + \beta_5 I_t) + u_{ijt} \tag{1.1}$$

with

- X_{ijt}: The aggregate public procurement award value (volumes) bought by public authorities in NUTS3 region i (destination) from a firm located in NUTS3 region j (origin) in year t.
- $\log dist_{ij}$: log of kilemeter distance between NUTS3 region i and NUTS3 region j.
- $bilateral_{ij}$: A vector of bilateral control variables including border dummies for country and NUTS2-region and a contiguit dummy.
- I_{it}, I_{jt}, I_t: Fixed effects for origin-time, destination-time, and year, respectively.

The unique feature of the public procurement data is the observation of three distinct locations for each contract: the location of the public authority (destination), the location of the firm (origin), and the place of work, service or delivery, the place of performance k. This allows me to extend the gravity model by introducing triplets of locations that consist of unique combinations of NUTS3 regions for the place of performance k, the winning firm-location j and the contracting authority location i, as the unit of observation.

Introducing the place of performance as a third location is critical, as it enables me to disentangle the effects of delivery and trade costs from potential preferences for local procurement. In many cases, contracts are awarded to firms located far from the contracting authority but closer to the place of performance, reflecting cost minimization in travel or logistical expenses. Conversely, public authorities may exhibit a preference for suppliers located near their own offices, potentially due to familiarity, administrative convenience, or network effects. A key identifying assumption in this framework is that the place of performance is exogenous. That is, the location where procurement is required arises independently of firms' locational choices. This assumption holds in cases where procurement needs are determined by external constraints, such as infrastructure requirements, legal mandates, or fixed service locations (e.g., schools, hospitals, or roads that must be built in a given area). While public authorities may have some discretion in selecting specific locations, I assume that these choices are primarily driven by exogenous factors—such as demand for public services or regulatory requirements, rather than strategic firm considerations. This assumption is critical for identification, as it ensures that the estimated distance effects reflect actual travel and logistical costs rather than endogenous location choices by firms or public authorities.

I extend the bilateral variables to all bilateral combinations of the three regions. Furthermore, I extend the fixed-effects by adding region-time fixed-effects for the place of performance I_{kt}. Standard Errors are clustered at the triplet level. Equation 1.2 shows the extended gravity specification.

$$X_{ijkt} = exp(\beta_0 + \beta_1 \log dist_{ij} + \beta_2 \log dist_{ik} + \beta_3 \log dist_{jk} + \\ \beta_4 bilateral_{ij} + \beta_5 bilateral_{ik} + \beta_6 bilateral_{jk} + \beta_7 I_{it} + \beta_8 I_{jt} + \beta_9 I_{kt} + \beta_{10} I_t) + u_{ijkt} \tag{1.2}$$

- With X_{ijkt}: the trade flow between destination i, origin j, served at place of performance k in year t.
- $\log dist_{ij}$: The distance between destination NUTS3 region i and origin NUTS3 region j.
- $\log dist_{ik}$: The distance between destination NUTS3 region i and place of performance k.
- $\log dist_{jk}$: The distance between origin NUTS3 region j and place of performance k.
- $bilateral_{ij}$: A vector of bilateral controls for destination NUTS3 region i and origin NUTS3 region j.
- $bilateral_{ik}$: A vector of bilateral controls for destination NUTS3 region i and place of performance k.
- $bilateral_{jk}$: A vector of bilateral controls for origin NUTS3 region j and place of performance k.
- I_{it}: Destination (buying public authority's region) fixed-effects
- I_{jt}: Origin (contract-winning firm's region) fixed-effects
- I_{kt}: Place of performance region fixed-effects
- I_t: Time fixed-effects

The gravity framework outlined above operates at the triplet level, aggregating procurement flows across all contracts for each combination of contracting authority, firm, and place of performance. To study the role of tender process digitalization, contract- and industry characteristics, which vary at the level of individual contracts, I shift the analysis to the contract level. In this specification, each contract serves as a separate observation, and I interact contract-level variables with the different distance measures to examine how these factors influence the relative importance of each distance and modify the associated coefficients.

I examine whether digital tendering processes mitigate geographic barriers by interacting distance terms with an indicator for a digital digital tendering process. Additionally, I analyze whether procurement dynamics differ for contracts in digital industries, which face fewer spatial frictions due to reduced or even zero travel costs. Furthermore, I explore the role of contract complexity by interacting distance terms with measures for contract complexity. This allows me to determine whether more specialized contracts exhibit greater home bias. These interactions provide insight into the underlying mechanisms driving spatial patterns and home bias in public procurement.

Although the PPML estimator is widely used in the gravity literature and has several attractive properties, it presents certain drawbacks in this context. The primary motivation for using PPML in standard trade gravity models is the prevalence of zero trade flows between regions. Since a logarithmic transformation cannot accommodate zeroes, traditional OLS with logged trade flows excludes these observations, potentially introducing selection bias. PPML overcomes this issue by allowing zero trade flows to enter the estimation, preserving valuable variation.

However, at the individual contract level, zero trade flows do not occur. Every contract represents a realized transaction, meaning that a logarithmic transformation of contract values does not result in data loss. This makes the key advantage of PPML redundant. Furthermore, high-dimensional fixed effects, which are crucial for controlling for location-specific and time-specific heterogeneity, pose a challenge for nonlinear estimators such as PPML. This issue, commonly referred to as the incidental parameters problem, arises because the number of observations cannot grow enough without also increasing the number of fixed effects, potentially leading to unreliable estimates. Given these limitations, an OLS-based approach appears to be a more reliable alternative (Greene, 2004; Lancaster, 2000).

Still, both PPML and log-transformed OLS suffer from another important drawback related to the logarithmic transformation of distances: observations with zero distances must be excluded. Since I compute distances using NUTS3 region centroids, contracts where the firm and the authority are located within the same NUTS3 region have a calculated distance of zero. This is highly problematic, as it removes a substantial portion of the variation relevant to my research question: local procurement, where a contracting authority awards contracts to firms within the same NUTS3 region. Moreover, even when the place of performance NUTS3 region coincides with the contracting authority's NUTS3 region, the contract might still be awarded to a more distant firm, which is an essential aspect of the analysis. Excluding these cases would systematically underestimate the extent of local contracting and distort the estimated distance effects.

To address this issue, I adopt the Inverse Hyperbolic Sine (IHS) transformation of distances in OLS estimations. The IHS transformation retains the desirable properties of the logarithmic transformation, such as compressing extreme values and approximating a logarithmic transformation for large distances. However, unlike the logarithm, IHS is defined for zero values, meaning that contracts with zero distance between firm and authority, firm and place of performance, or authority and place of performance remain in the estimation (Aihounton and Henningsen, 2021).

To further account for the importance of local contracts, I introduce separate dummy variables for each of the three bilateral region pairs: authority–firm, authority–place of performance, and firm–place of performance, indicating whether the two regions in each pair belong to the same NUTS3 region. These dummies allow to explicitly control for contracts awarded within the same administrative region, while still preserving the continuous distance-based variation in procurement patterns.

Equation 1.3 shows the contract-level regression, including the local dummies and IHS-transformed distance variables. To examine how contract-specific characteristics moderate the distance effects, I either interact the distance measures with contract-level variables Z_c or estimate the model on relevant subsamples of contracts.

$$\begin{aligned}\log X_{cijkt} = \beta_0 + \beta_1 \sinh^{-1} dist_{ij} + \beta_2 \sinh^{-1} dist_{ik} + \beta_3 \sinh^{-1} dist_{jk} + \\ \beta_4 \sinh^{-1} dist_{ij} * Z_c + \beta_5 \sinh^{-1} dist_{jk} * Z_c + \beta_6 Z_c + \beta_7 bilateral_{ij} + \\ \beta_8 bilateral_{ik} + \beta_9 bilateral_{jk} + \beta_{10} I_{it} + \beta_{11} I_{jt} + \beta_{12} I_{kt} + \beta_{13} I_t + u_{cijkt}\end{aligned} \tag{1.3}$$

- X_{cijkt}: The contract volume awarded by public authorities in NUTS3 region i (destination) to firms in NUTS3 region j (origin), with services or goods delivered in NUTS3 region k (place of performance), in year t.
- $\sinh^{-1} dist_{ij}$: The IHS-transformed geographic distance in kilometers between the public authority's location i and the firm's location j.
- $\sinh^{-1} dist_{ik}$: The IHS-transformed geographic distance in kilometers between the public authority's location i and the place of performance k.
- $\sinh^{-1} dist_{jk}$: The IHS-transformed geographic distance in kilometers between the firm's location j and the place of performance k.
- Z_c: Contract level variables.
- $bilateral_{ij}$, $bilateral_{ik}$, $bilateral_{jk}$: Bilateral controls for the three region pairs.
- I_{it}, I_{jt}, I_{kt}, I_t: Origin-, destination-, and place-of-performance–year fixed effects (time-varying multilateral resistance terms).
- u_{cijkt}: Error term.

With this specification, I ensure that local procurement observations remain in the dataset while maintaining a flexible functional form that captures spatial frictions in public contract allocation. By applying this framework, I can now assess how geographic distance, digitalization, and contract complexity influence procurement patterns. In the following section, I present the estimation results, beginning with the baseline gravity model.

1.4 Results

The results are structured into the three levels of analysis presented before, each capturing different dimensions of procurement trade flows:

1. **Bilateral Gravity Model:** I begin with a standard bilateral gravity model (Equation 1.1), regressing aggregate procurement flows between the firm's and authority's NUTS3 regions on their bilateral distance and border-related characteristics. This allows for a first test of whether and how spatial frictions affect procurement decisions.

2. **Triplet Gravity Model:** The second level expands the analysis by incorporating unique three-location triplets as unit of observation: public authority location, firm location, and place of performance.

3. **Contract-Level Model:** The final level shifts the unit of observation to individual contract awards rather than aggregated trade flows. This enables the inclusion of contract- and tender-specific characteristics, providing insights into how procurement mechanisms, tender design, and contract complexity shape trade flows and trade barriers.

I begin by estimating Equation 1.1, which examines how the geographical distance between the procuring authority's region and the winning firm's region affects procurement award values. The underlying intuition is that procurement markets are often characterized by localization, where authorities may prefer awarding contracts to geographically closer firms. I expect a negative coefficient for Firm-Authority Distance, indicating that firms farther away from the contracting authority receive smaller aggregate contract values. The magnitude of this coefficient provides insights into the extent of localization in public procurement contract award decisions.

Table 1.3 presents the results from estimating the bilateral gravity model, examining the effect of geographic distance between public authorities and firms on procurement award values. Column (1) includes firm and authority NUTS3 fixed effects along with year fixed effects, Column (2) further introduces firm-year and authority-year fixed effects, ensuring that time-varying region-specific factors are accounted for. Column (3) extends the model by including additional bilateral factors: a dummy for NUTS3-region contiguity and cross-border procurement.

Across all specifications, the coefficient on Firm-Authority Distance is negative and statistically significant, confirming that procurement flows decline with increasing distance between contracting authorities and firms. The positive and significant coefficient on Firm-Authority Contiguity suggests that firms located in adjacent regions receive higher aggregate award values. Similarly, the positive coefficient on Firm-Authority Same Country indicates that procurement flows are significantly higher when firms and authorities are located within the same country, reflecting national procurement preferences. The decreasing coefficient on Firm-Authority Distance from column 1 to column 2 indicates that parts of the original effect is captured by time-varying region-specific effects, e.g., the economic development. In column 3,

Table 1.3: Estimation Results for the Bilateral Gravity Model

Dependent Variable:		Award Value	
Model:	(PPML)	(PPML)	(PPML)
Variables			
Firm-Authority Distance	-0.9306***	-0.8110***	-0.5734***
	(0.1436)	(0.0276)	(0.0337)
Firm-Authority Contiguity			0.1755**
			(0.0712)
Firm-Authority Same Country			2.385***
			(0.1584)
Fixed-effects			
Firm NUTS3	Yes		
Authority NUTS3	Yes		
Year	Yes	Yes	Yes
Firm NUTS3-Year		Yes	Yes
Authority NUTS3-Year		Yes	Yes
Fit statistics			
Observations	478,342	476,024	476,024
Squared Correlation	0.63968	0.99980	0.99980
Pseudo R^2	0.79620	0.98415	0.98516
BIC	1.2×10^{14}	9.33×10^{12}	8.74×10^{12}

Clustered (Firm NUTS3-Authority NUTS3) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

parts of the original effect from column 1 are captured by the additional dummies, indicating that the distance-effect is not perfectly linear.

While the results in Table 1.3 confirm the expected negative effect of distance on procurement flows in the bilateral case, they raise a more nuanced question: to what extent does this distance effect stem from the spatial separation between the winning firm and the contracting authority, as opposed to the distance between the winning firm and the place of performance? In other words, does procurement favor firms that are geographically closer to the contracting authority, or does proximity to the place of performance play a more dominant role?

To address this question, the next step estimates Equation 1.2, introducing the place of performance to differentiate between spatial frictions related to accessing tenders and those linked to executing contracts.

Table 1.4 shows that all three bilateral distances influence procurement outcomes, but their effects differ across specifications. In Column (1), both Firm-Authority Distance and Firm-Place Distance have negative and significant coefficients, though the former has a larger magnitude, implying stronger frictions in the tendering phase than in the delivery phase. Authority–Place Distance also enters negatively, suggesting that performance locations tend to be geographically closer to the contracting authority. This pattern is consistent with the nature of many local and regional public contracts, which are often implemented

Table 1.4: Extended Gravity Model with three locations: The winning firm's region, the contracting authorities' region and the place of performance

Dependent Variable:	Award Value		
Model:	(1)	(2)	(3)
	PPML	PPML	PPML
Variables			
Firm-Authority Distance	-0.2597*	-0.7304***	-0.5952***
	(0.1382)	(0.2774)	(0.1523)
Firm-Place Distance	-0.1749**	-0.0728	0.0420
	(0.0846)	(0.0968)	(0.0959)
Authority-Place Distance	-0.3916***	-0.1516***	-0.1687***
	(0.0833)	(0.0508)	(0.0504)
Firm-Authority Contiguity			-0.7354***
			(0.2219)
Firm-Place Contiguity			1.017***
			(0.3130)
Authority-Place Contiguity			0.2078*
			(0.1216)
Firm-Authority Same Country			0.1690
			(0.6576)
Firm-Place Same Country			-0.7716
			(0.6566)
Authority-Place Same Country			1.114
			(1.146)
Fixed-effects			
Firm NUTS3	Yes		
Authority NUTS3	Yes		
Place NUTS3	Yes		
Year	Yes	Yes	Yes
Firm NUTS3-Year		Yes	Yes
Authority NUTS3-Year		Yes	Yes
Place NUTS3-Year		Yes	Yes
Fit statistics			
Observations	97,699	92,862	92,862
Squared Correlation	0.86605	0.99218	0.99835
Pseudo R^2	0.76331	0.96626	0.96875
BIC	1.53×10^{12}	2.16×10^{11}	2×10^{11}

Clustered (Firm NUTS3-Authority NUTS3-Place NUTS3) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

within or near the authority's jurisdiction

After introducing full region-year fixed effects in Column (2), only the Firm-Authority Distance remains significant. This suggests that unobserved time-varying regional factors, such as local productivity or firm capabilities, may have driven the earlier significance of the firm-place relationship.

Column (3) adds spatial indicators for contiguity and national borders. The qualitative pattern from the distance measures remains: Authority–Firm Distance continues to exhibit a negative and significant effect, while Firm–Place Distance remains statistically insignificant. However, the spatial indicators introduce non-linearity in how distance affects procurement flows and therefore require a more nuanced interpretation. Although the overall Firm–Place Distance does not matter on average, there is a significantly positive effect when the firm and the place of performance are located in contiguous NUTS3 regions. This suggests that distance between firms and performance locations matters primarily at very short ranges. For larger distances, the relationship appears flat. The negative coefficient on the Authority–Firm contiguity indicator points to the opposite pattern: the marginal effect of distance between authorities and firms is relatively small at very short ranges but becomes stronger as distances increase. This indicates that the authority–firm relationship exhibits a smoother distance decay, where localization strengthens gradually with geographic separation rather than being concentrated at regional boundaries.

In light of home bias, these findings suggest that spatial preferences in procurement decisions are more strongly tied to the authority–firm relationship than to the firm–place relationship. Authorities appear to prioritize working with firms located closer to them, while the execution of the contract at the place of performance is less sensitive to distance except in very local settings.

The triplet-level analysis provides insights into aggregate spatial patterns in procurement but does not allow for a detailed examination of contract-specific characteristics. Since each triplet aggregates multiple contracts, variations in complexity and procurement procedures remain unaccounted for. To better understand the underlying mechanisms driving the observed distance effects, I shift the analysis to the contract level.

Table 1.5 presents the baseline OLS estimates examining the role of geographical distances in public procurement. Column (1) reports a simple OLS gravity model where only the distances between the firm, the contracting authority, and the place of performance are included as regressors, alongside fixed effects for all three locations and the years. Column (2) extends this specification by adding time-varying fixed effects at the NUTS3 level for firms, contracting authorities, and places of performance. Finally, Column (3) introduces additional gravity controls, indicating whether the respective locations are in the same NUTS2 region or the same country, to control for the impact of administrative boundaries on contract values.

Note that the number of observations at the contract level is only moderately larger than at the regional level because the PPML specification includes all potential region pairs and triplets, including those with zero observed contracts, whereas the OLS specification excludes cases where one of the three distances equals zero.

Across all three specifications, firm-authority distance is negatively and significantly associated with

Table 1.5: Baseline OLS estimations on the contract level

Dependent Variable:	Award Value		
Model:	(1)	(2)	(3)
	OLS	OLS	OLS
Variables			
Firm-Authority Distance	-0.0812**	-0.0669***	-0.0846**
	(0.0352)	(0.0254)	(0.0329)
Firm-Place Distance	0.0180	-0.0230	-0.0239
	(0.0309)	(0.0225)	(0.0291)
Authority-Place Distance	-0.0493	-0.0055	-0.0099
	(0.0439)	(0.0384)	(0.0503)
Firm-Authority Same Country			-0.1801
			(0.3847)
Firm-Place Same Country			-0.4587
			(0.3803)
Authority-Place Same Country			-0.0740
			(0.6347)
Firm-Authority Same NUTS2			-0.0502
			(0.0675)
Firm-Place Same NUTS2			0.0205
			(0.0521)
Authority-Place Same NUTS2			-0.0180
			(0.0909)
Fixed-effects			
Firm NUTS3	Yes		
Authority NUTS3	Yes		
Place NUTS3	Yes		
Year	Yes	Yes	Yes
Firm NUTS3-Year		Yes	Yes
Authority NUTS3-Year		Yes	Yes
Place NUTS3-Year		Yes	Yes
Fit statistics			
Observations	150,349	150,349	150,349
R^2	0.43402	0.64470	0.64504
Within R^2	0.00040	0.00033	0.00129

** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

contract value. This consistent effect suggests that firms located closer to contracting authorities tend to receive contracts in higher values, possibly reflecting informational advantages, administrative proximity, or stronger reputational ties. In contrast, neither firm-place nor authority-place distance is statistically significant, indicating that contract value is less sensitive to the distance between the place of performance and the firm.

The addition of administrative boundary indicators in Column (3) does not yield statistically significant coefficients. This suggests that, once geographical distances are accounted for, being located in the same country or NUTS2 region does not systematically affect contract values on the contract level. Notably, Column (1) reports a positive, though insignificant, coefficient on firm-place distance, which is counterintuitive given the likely cost implications of longer distances. This effect disappears once time-varying fixed effects are included in Column (2), suggesting that regional productivity or specialization patterns may confound the relationship between distance and contract value in the simpler model.

As discussed in Chapter 1.3, both OLS and PPML have drawbacks in the case of contract-level estimations. Table 1.6 presents the results for the two different estimation approaches: log-transformed OLS (Column 1) and OLS with Inverse Hyperbolic Sine (IHS)-transformed distances (Column 2).

In both models, the distance between the firm and the contracting authority has a negative and statistically significant effect on contract value. This effect is stronger in the IHS model, suggesting that, even when accounting for zero-distance contracts, contracts tend to be of lower value when awarded to firms located farther from the contracting authority. In contrast, the distance between the firm and the place of performance does not show a statistically significant relationship with contract value in either specification.

Notably, the negative coefficient on the local dummy and the negative coefficient on the same-country dummy for firm-authority pairs indicate a non-linear distance relationship: relative to the estimated average distance effect, contract values are lower for very local procurement (within the same NUTS3) and also for same-country allocations. This pattern suggests that the negative effect of distance on procurement value is particularly pronounced at short distances, while the overall negative relationship persists across longer distances.

Regarding the Authority-Place Distance, a positive coefficient would be expected if this variable serves as a proxy for the geographic scope or complexity of a procurement project. Authorities commissioning projects outside their own region, particularly across national borders, are typically larger institutions managing larger, more complex contracts. In contrast, local procurement projects, where the authority and the place of performance lie within the same NUTS3 region, are generally smaller in scope and value. Although the coefficients for Authority-Place Distance are not statistically significant, the smaller scope and complexity of local contracts in terms of authority-place is reflected in the significantly negative coefficient on the local dummy.

Overall, the results show that distance effects in public procurement are robust to different specifications and the inclusion of zero-distance observations. The negative effect of firm-authority distance remains consistent, while firm-place and authority-place distances are slightly more sensitive. The IHS transformation allows inclusion of zero-distance contracts while compressing larger distances. Based on these

Table 1.6: Comparison of the baseline contract-level gravity regression with standard OLS and OLS with IHS-transformed distances

Dependent Variable:	Award Value	
Model:	(1)	(2)
	OLS	OLS (IHS)
Variables		
Firm-Authority Distance	-0.0526*	-0.0932***
	(0.0278)	(0.0241)
Firm-Place Distance	-0.0370	-0.0339
	(0.0255)	(0.0244)
Authority-Place Distance	0.0320	-0.0113
	(0.0448)	(0.0367)
Firm-Authority Same Country	-0.6607**	-1.516***
	(0.3078)	(0.3582)
Firm-Place Same Country	-0.2603	0.8770**
	(0.3067)	(0.3624)
Authority-Place Same Country	0.0718	1.457***
	(0.5307)	(0.5072)
Firm-Authority Same NUTS2	-0.0416	0.0471
	(0.0531)	(0.0528)
Firm-Place Same NUTS2	0.0032	0.0026
	(0.0457)	(0.0518)
Authority-Place Same NUTS2	-0.0475	0.2148***
	(0.0773)	(0.0717)
Firm-Authority Local		-0.9596***
		(0.2793)
Firm-Place Local		-0.4444
		(0.2844)
Authority-Place Local		-0.7039*
		(0.4183)
Fixed-effects		
Firm NUTS3-Year	Yes	Yes
Authority NUTS3-Year	Yes	Yes
Place NUTS3-Year	Yes	Yes
Year	Yes	Yes
Fit statistics		
Observations	181,402	593,931
R^2	0.63462	0.41591
Within R^2	0.00361	0.00562

Clustered (Firm NUTS3-Year) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

considerations, I present the results using IHS-transformed distances.

While the previous analysis identifies a home bias in public procurement - where firms located closer to the contracting authority win higher valued contracts, while the distance between the firm and the place of performance shows no significant impact on award values - the mechanisms underlying this pattern remain open to investigation. The following analysis explores how contracting authorities may implement this preference in practice. Specifically, it investigates the mechanisms through which authorities can favor local firms within the formal constraints of equal treatment and open competition. One such mechanism is the strategic use of tender specifications. In this view, authorities may design tender documents in ways that implicitly advantage preferred local firms, even when non-local competitors may offer more competitive bids on objective grounds.

This form of tailoring the tender design may be reflected in specific features of the tender documents, such as the length of the contract description, the number of award criteria, or the structure of the bidding process itself (e.g., how many bids are ultimately received). Rather than being neutral tools of competition, these design choices may serve as instruments of discretion, enabling authorities to steer procurement outcomes toward preferred firms.

The mechanism explored here builds on two economic strategies that contracting authorities may use to favor local firms: increasing specificity and manipulating information frictions. First, authorities may increase the specificity of tender descriptions and technical requirements in ways that closely match the capabilities of a preferred local supplier, effectively tailoring the tender to reduce the pool of eligible bidders. Second, they may exploit information frictions by keeping descriptions intentionally vague or increasing procedural complexity, making it more difficult for non-local firms to assess their chances or prepare competitive bids. In such cases, firms without direct access to informal communication channels or local networks may be at a disadvantage. Similarly, adding numerous or ambiguous award criteria can further obscure the selection process and favor insiders with local knowledge. Both approaches serve to deter broader participation and increase the likelihood that a contract is awarded to a preferred local firm, while formally complying with procurement rules.

To assess these mechanisms, I examine the interaction between the two key distance coefficients and three tender characteristics: (i) the logarithmic description length (in 1,000 symbols), (ii) the number of award criteria, and (iii) the number of bids, which serves as a proxy for competition. If authorities systematically adjust these variables to favor nearby firms, the strength of home bias should vary with these tender features.

The following regression results extend the baseline regression from equation 1.3 by interacting measures of tender complexity and bidder participation with firm-authority and firm-place distances.

Table 1.7: Regression Results With Tender-Specific Aspects

Dependent Variable:	Award Value		
Model:	(1)	(2)	(3)
	OLS (IHS)	OLS (IHS)	OLS (IHS)
Variables			
Firm-Authority Distance	-0.0905***	-0.0849***	-0.0908***
	(0.0241)	(0.0261)	(0.0244)
Firm-Place Distance	-0.0342	-0.0415	-0.0419*
	(0.0244)	(0.0267)	(0.0245)
Authority-Place Distance	-0.0122	-0.0138	-0.0111
	(0.0367)	(0.0362)	(0.0367)
Description Length	0.1553***		
	(0.0328)		
Firm-Authority Distance × Description Length	-0.0104**		
	(0.0044)		
Firm-Place Distance × Description Length	0.0009		
	(0.0045)		
No. Criteria		0.0966***	
		(0.0142)	
Firm-Authority Distance × No. Criteria		-0.0046	
		(0.0029)	
Firm-Place Distance × No. Criteria		0.0025	
		(0.0029)	
No. Bids			-0.0144**
			(0.0064)
Firm-Authority Distance × No. Bids			-0.0003
			(0.0005)
Firm-Place Distance × No. Bids			0.0014***
			(0.0005)
Fixed-effects			
Firm NUTS3-Year	Yes	Yes	Yes
Authority NUTS3-Year	Yes	Yes	Yes
Place NUTS3-Year	Yes	Yes	Yes
Year	Yes	Yes	Yes
Bilateral Geographic Controls	Yes	Yes	Yes
Fit statistics			
Observations	593,931	589,011	593,931
R^2	0.37698	0.37956	0.41681
Within R^2	0.00601	0.00821	0.00715

Clustered (Firm NUTS3-Year) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

In Column (1), longer tender descriptions are positively associated with award values, suggesting that more detailed and complex tenders are of higher value. The negative interaction term between Firm–Authority

Distance and Description Length implies that longer descriptions actually reinforce the disadvantage of geographically distant firms relative to the contracting authority: the negative effect of distance becomes stronger the more detailed the tender description. In contrast, the interaction between Description Length and Firm–Place Distance is statistically insignificant, providing no evidence that longer descriptions systematically moderate the effect of distance from the place of performance. Put differently, while detailed descriptions raise overall award values, they disproportionately favor firms located closer to the contracting authority, whereas they do not change award values with regard to how distant they are located to the place of performance. This indicates that detailed descriptions do not uniformly reduce information frictions, but rather increase the importance of being close to the contracting authority relative to the importance of being close to the place of performance.

Column (2) introduces the number of award criteria as another measure of tender complexity. The positive and highly significant coefficient on the number of award criteria suggests that tenders with more award criteria tend to have higher award values. This is intuitive, as more complex contracts are usually of higher value. The negative sign on the interaction with firm–authority distance and the positive sign on the interaction with firm–place distance are consistent with the hypothesis that contract complexity increases the relative importance of proximity to the contracting authority compared to proximity to the place of performance. However, both interaction terms are statistically insignificant. Therefore, based on the number of award criteria, there is no evidence that contracting authorities systematically structure selection criteria in a way that favors nearby firms, for instance by making the evaluation process more ambiguous or tailoring requirements to the capabilities of preferred bidders.

In column (3), a higher number of bids is associated with lower award values, consistent with greater competition driving down prices. This pattern may also reflect that less complex contracts attract more bidders, and such contracts are typically of smaller value, as indicated by the positive coefficient on Description Length and No. Criteria. The number of bids is not associated with a change in home bias. The interaction between Firm-Authority Distance and the number of bids is not statistically significant, and the interaction between Firm-Place Distance and the number of bids is positive and very small, resulting in Firm-Place Distance and its interaction term being jointly insignificant. This indicates that increased competition does not meaningfully alter the relative importance of a firm's proximity to authorities and places of performance.

To further investigate the role of digitalization in moderating home bias in public procurement, I examine its effect across different contract types. Specifically, I compare the impact of geographic distance in (1) service contracts, (2) contracts in digital industries, and (3) contracts in non-digital industries. The central question is whether home bias, identified through the difference in the effects of firm-authority distance versus firm-place distance, varies systematically across these categories.

From an economic perspective, the extent of home bias may depend on the nature of the good or service procured, particularly in terms of transportability, specialization, the importance of face-to-face interaction, and the reliance on localized knowledge or labor. Digital industries, such as IT services, software development, or online platforms, often produce goods and services that are easily scalable, require little physical transportation, and can be delivered remotely, which may in general reduce the relevance of geographic proximity. Additionally, firms in these sectors tend to be more specialized and operate in competitive, internationally integrated markets, which may reduce the importance of connections to local firms. As a result, I expect home bias to be smaller in digital industries, with Firm-Authority Distance having a smaller (or even statistically insignificant) negative effect on award outcomes. Due to the reduced importance of geographic frictions, I also expect the coefficient on Firm-Place Distance to be non-significant.

In contrast, non-digital industries often involve physical production, transportation, or infrastructure services that are more location-bound. These contracts typically incur higher transportation costs, require local infrastructure, and may involve more frequent on-site coordination and monitoring. Furthermore, contracting authorities may rely more on established local networks or face higher perceived risks when engaging distant firms. Consequently, I expect home bias to be stronger in non-digital industries, with a more pronounced negative effect of firm-authority distance.

For service contracts, the role of geographic proximity is more ambiguous and likely heterogeneous. Services such as consulting or digital design can often be provided remotely, while others, like building maintenance or healthcare, require physical presence. Thus, the strength of home bias in service contracts may vary depending on the degree to which services are location-dependent or standardized.

Table 1.8 presents the results separately for service contracts, digital industries, and non-digital industries. The coefficients on Firm-Authority Distance and Firm-Place Distance are compared across these subsamples to assess whether the strength and pattern of home bias differ by contract type.

For service contracts, both the coefficient on Firm-Authority Distance and the coefficient on Firm-Place Distance are statistically insignificant. This suggests that geographic proximity, whether to the contracting authority or the place of performance, does not strongly influence awarded contract values in this category. This finding is consistent with the hypothesis that services are heterogeneous in nature, with some requiring on-site delivery and others being deliverable remotely. The lack of significance may also reflect a composition of both location-dependent and standardized services within the same category, leading to a net zero effect. While statistical power may be lower due to smaller subsample, the results point toward a weaker or more diffuse pattern of home bias in service contracts.

The results for digital industries partially confirm the hypothesis. As expected, firm-authority distance is statistically insignificant, which aligns with the idea that home bias is weaker in digital sectors due to the more specialized work and internationalization of digital services. However, Firm-Place Distance is significantly negative, indicating that geographic proximity to the place of performance still matters in digital contracts—contrary to the expectation that such contracts would be largely location-agnostic. This may reflect that many digital contracts still involve region-specific implementation or support tasks (e.g., on-site IT services, installation, or compliance with regional regulations). Moreover, authority-place distance is significantly positive. This suggests that the scope of the projects and the government level of the authority is an important indicator for contract values of contracts in digital industries. One possible explanation is that digital industry contracts are more frequently issued by higher-level authorities—such as national or supranational bodies—reducing the relevance of local proximity to the authority. Additionally, contracting at higher administrative levels may be subject to stricter regulatory

Table 1.8: Differences across Service Industries, Digital Industries and non-digital industries

Dependent Variable:		Award Value	
	Service Contracts	Digital Industries	Non-Digital Industries
Model:	(IHS)	(IHS)	(IHS)
Variables			
Firm-Authority Distance	-0.0227	0.0491	-0.0797***
	(0.0161)	(0.0647)	(0.0210)
Firm-Place Distance	0.0133	-0.1494**	0.0107
	(0.0158)	(0.0658)	(0.0215)
Authority-Place Distance	-0.0239	0.3637***	-0.0170
	(0.0253)	(0.1308)	(0.0315)
Fixed-effects			
Firm NUTS3-Year	Yes	Yes	Yes
Authority NUTS3-Year	Yes	Yes	Yes
Place NUTS3-Year	Yes	Yes	Yes
Year	Yes	Yes	Yes
Bilateral Gravity Control	Yes	Yes	Yes
Fit statistics			
Observations	676,539	65,852	342,188
R^2	0.51159	0.59036	0.55589
Within R^2	0.00541	0.00628	0.03770

Clustered (Firm NUTS3-Year) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

oversight or standardized procedures, which could limit the scope for local favoritism. In such contexts, while proximity to the authority is less critical, firms may still need to operate near the designated place of performance, explaining the persistent effect of firm-place distance. Overall, Firm-Place Distance is realtively more important than Firm-Authority Distance, indicating a smaller Home Bias in digital industries.

The findings for non-digital industries are strongly in line with the hypothesis of pronounced home bias. Firm-Authority distance is significantly negative, indicating that firms located farther from the contracting authority receive lower award values. This supports the notion that proximity to the authority is a key factor in awarding contracts for physical or infrastructure-intensive projects. At the same time, firm-place distance is statistically insignificant, which fits with the idea that once a firm is selected—often based on authority proximity, the place of performance is less relevant.

I now shift to examining how digitalization in the tendering process may influence home bias. I aim to understand whether technology-driven procurement practices foster a more competitive and geographically inclusive tendering environment. In particular, I focus on two key dimensions of digitalization that could help level the playing field for firms across different locations.

First, I test whether fully electronic auctions are associated with weaker home bias than non-digital procedures. I interact a dummy for fully electronic auctions with the key spatial distance variables. Electronic auctions reduce administrative burdens, increase transparency, and lower participation costs, especially for firms located farther from the contracting authority. By removing logistical and informational barriers, digital procedures may reduce the advantages enjoyed by local firms and make tendering more competitive across geographic space. I expect the negative effect of Firm-Authority Distance to be attenuated in electronic tenders, while the relative role of the Firm-Place Distance may become more prominent, suggesting a weaker home bias in fully digital procedures.

Second, I examine whether the number of electronically submitted bids, a proxy for the reach and accessibility of the tender, influences the spatial allocation of contracts by interacting this variable with the distance regressors. A higher number of digital offers likely reflects broader participation and more intense competition. In addition, it may indicate that firms from a wider geographic range, including more distant firms, find it feasible to submit bids. I expect the negative effect of firm-authority distance to be mitigated as the number of electronic bids increases, pointing to a more competitive and geographically neutral allocation of contracts.

Table 1.9 presents the results on how digitalization during the tendering process interacts with spatial frictions. The analysis focuses on two types of tender-process digitalization: fully electronic auctions (Column 1) and the number of electronically submitted offers (Column 2).

The results in Column (1) suggest that electronic auctions are associated with a modest reduction in home bias. The baseline coefficient for Firm-Authority Distance is significantly negative, indicating that, on average, firms located farther from the contracting authority receive smaller award values. However, the interaction between electronic auctions and Firm-Authority Distance is small and statistically insignificant, while the interaction between electronic auctions and Firm-Place Distance is significantly negative. This implies that in electronic auctions, the negative effect of distance to the place of performance becomes more pronounced, while the firm-authority distance remains largely unaffected.

If we conceptualize home bias as the relative importance of firm-authority distance compared to firm-place distance, then this shift suggests a weakening of home bias in electronic auctions. That is, contract allocation in digital tenders appears to place more weight on the location of the actual performance, service, or delivery and less on the geographic proximity to the contracting authority. Moreover, the strongly positive main effect of the electronic auction dummy on award value indicates that electronic tenders may be associated with larger or more commercially attractive contracts, possibly due to their broader reach or greater formality.

Column (2) explores the role of digital participation intensity, measured by the number of electronically submitted bids. Here, neither of the interaction terms with distance is statistically significant. Although the coefficient on firm-authority distance remains significantly negative, the lack of significant interaction effects suggests that increasing digital participation alone does not systematically alter the spatial pattern of awards. The negative coefficient on the number of electronic offers itself can have three reasons. (1) It may reflect a higher number of bids in general and therefore a more competitive bidding in such tenders. (2) There are more bids in general on smaller-value contracts or (3) electronic bids may be associated directly with smaller award values to reduce the transaction costs relative to the award value especially

Table 1.9: Regression Results for Electronic Auctions and Electronic Bids

Dependent Variable:	Award Value	
Model:	(1)	(2)
	OLS (IHS)	OLS (IHS)
Variables		
Firm-Authority Distance	-0.1002***	-0.1069**
	(0.0282)	(0.0435)
Firm-Place Distance	-0.0228	-0.0436
	(0.0285)	(0.0420)
Authority-Place Distance	-0.0566	0.0144
	(0.0500)	(0.0665)
Electronic Offers		-0.0117***
		(0.0029)
Electronic Auction	0.4626***	
	(0.1363)	
Firm-Authority Distance × Electronic Auction	0.0113	
	(0.0176)	
Firm-Place Distance × Electronic Auction	-0.0280*	
	(0.0169)	
Firm-Authority Distance × Electronic Offers		0.0008
		(0.0011)
Firm-Place Distance × Electronic Offers		0.0006
		(0.0011)
Fixed-effects		
Firm NUTS3-Year	Yes	Yes
Authority NUTS3-Year	Yes	Yes
Place NUTS3-Year	Yes	Yes
Year	Yes	Yes
Bilateral Geographic Controls	Yes	Yes
Fit statistics		
Observations	351,347	180,227
R^2	0.47712	0.44839
Within R^2	0.02466	0.00707

Clustered (Firm NUTS3-Year) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

for smaller contract values.

Taken together, these results suggest that digitalization in the form of fully electronic auctions modestly reshapes spatial frictions in procurement, potentially mitigating home bias by shifting attention away from the location of the contracting authority. However, increasing the number of digital bids alone does not appear to have spatial effect.

1.5 Bunching

Bunching refers to the strategic behavior of economic agents who cluster just below a regulatory threshold to avoid stricter rules or higher costs associated with exceeding it. In the context of public procurement, contracting authorities may set contract values just below the EU procurement thresholds to circumvent more stringent tendering requirements. These requirements often involve additional administrative burdens, extended bidding periods, and more rigorous transparency obligations. By keeping contract values below the threshold, authorities can exert greater control over the selection of suppliers.

This behavior connects to the broader literature on the role of discretion in public procurement outcomes. Several studies highlight how public authorities strategically manipulate procurement thresholds to gain greater discretion in awarding contracts. Palguta and Pertold (2017) analyze procurement data from the Czech Republic and find that contracting authorities systematically cluster contract values just below regulatory thresholds to avoid stricter procurement rules. This behavior allows authorities to exercise greater discretion, potentially reducing competition and increasing favoritism in contract awards. Similarly, Tas (2023) examines bunching in European public procurement and finds that 10–13% of authorities engage in this manipulation. His results indicate that bunching authorities are less likely to use competitive procurement procedures and more likely to repeatedly award contracts to the same local firms.

Discretion in procurement has broader implications beyond threshold manipulation. Celis Galvez et al. (2025) examine reforms in the Czech Republic and show that greater discretion in procurement leads to higher contract prices and reduced competition. They find that limiting discretion improves efficiency, while expanding it often results in politically motivated favoritism. These findings align with those of Tas (2023), who demonstrates that authorities engaging in threshold manipulation systematically favor local firms, potentially at the expense of cost-effectiveness and transparency.

I follow the methodology used by Tas (2023). This involves examining the bunching behavior separately for each public authority. This allows me to construct a binary indicator at the contract level that indicates whether the issuing authority engages in bunching. I then examine whether the contracts issued by bunching authorities differ in terms of the relative importance of the two distance measures: the distance between the contracting authority and the winning firm and the distance between the winning firm and the place of performance.

To detect bunching behavior for each public authority separately, I employ the manipulation test proposed by Cattaneo et al. (2018) and Cattaneo et al. (2024), implemented via the RDDensity tests. This method tests for an excess mass of observations just below a given threshold, which would indicate strategic bunching. The method estimates the density function of the contract values around the EU procurement thresholds and tests for a discontinuity at the threshold. Following Chetty et al. (2011), I use a counterfactual with the running variable being a continuous function around the threshold. This counterfactual reflects the situation in which the authorities do not engage in bunching. If a significant discontinuity is found, it suggests that authorities systematically adjust contract values to remain below the threshold.

The estimator relies on local polynomial density estimation to estimate the density of the running variable. In contrast to the previous analyses, I use the award values that are estimated by the contracting authority before publishing the contract notice $Award\,Value_{est}$. I use these pre-award estimated values instead of the eventually realized award values, as those are relevant for the procedure. A rejection of the null hypothesis indicates a significant discontinuity in the distribution, which in this case would imply strategic bunching by contracting authorities. Formally, the null hypothesis of the manipulation test is that the density of the estimated award value is continuous at the threshold $Thresh_c$; that is

$$H_0 : \lim_{Award\,Value_{est} \uparrow Thresh_c} f(Award\,Value_{est}) = \lim_{Award\,Value_{est} \downarrow Thresh_c} f(Award\,Value_{est}) \tag{1.4}$$

EU public procurement regulations impose different financial thresholds for contract values, above which stricter tendering procedures apply. These thresholds vary by contract type, such as goods, services, or construction works, and are updated every two years to reflect inflation and policy adjustments. Find a full list of thresholds for different years and contract types in A.1.

To detect strategic bunching, I examine whether contracting authorities systematically concentrate contract values just below the relevant thresholds. I first standardize the estimated award values by subtracting the applicable threshold, such that

$$c = Award\,Value_{est} - Thresh_c \tag{1.5}$$

This transformation centers c around zero, such that positive values in c indicate estimated award values above the threshold and negative values below the threshold.

The bunching analysis proceeds in three steps:

1. Data preparation
    - Compile a complete list of EU procurement thresholds by year, contract type, and authority type (see Table A.1).
    - Assign the appropriate threshold and c to each contract based on its year, contract type, and contracting authority type.
    - Standardize contracting authority names to ensure consistent identification across contracts.
2. Testing for discontinuities
    - For each authority, apply the RDDensity function to estimate the density of c around zero.
    - Conduct formal statistical tests to determine whether there is a significant discontinuity at the threshold.
3. Constructing a bunching indicator
    - Assign a dummy variable equal to 1 if a significant discontinuity is detected.
    - If no discontinuity is found, assign a value of 0.

This approach allows me to classify contracting authorities into two groups: those that exhibit statistically significant bunching behavior and those that do not.

Figure 1.7 shows the discontinuity plot for contracts from all authorities. The vertical line at $c = 0$ marks the threshold. Contracts to the left fall below it, while those to the right exceed it. The figure reveals a visible spike in the density just below the threshold, suggesting that some contracting authorities may strategically set estimated award values to remain below the threshold.

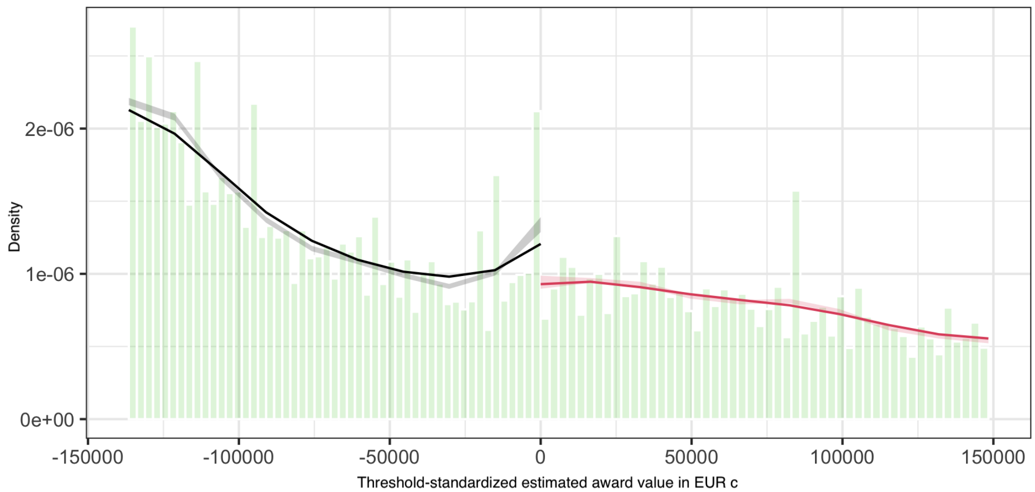

Figure 1.7: The Discontinuity at the threshold c, taking into account all contracts, with estimated densities of estimated award values below (black) and above (red) the threshold.

To further explore this pattern, I separate the sample by bunching behavior. Figure 1.8 shows the estimated density distribution for authorities classified as bunching, while Figure 1.9 shows the same for non-bunching authorities.

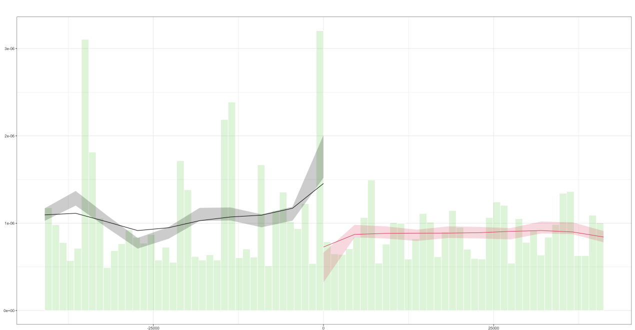

Figure 1.8: The Discontinuity at the threshold c, taking into account contracts from authorities classified as bunching, with estimated densities of estimated award values below (black) and above (red) the threshold

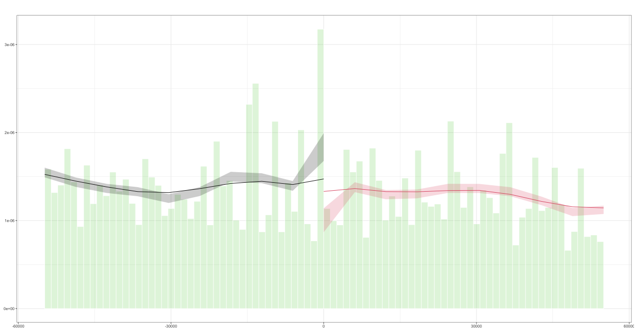

Figure 1.9: The Discontinuity at the threshold c, taking into account contracts from authorities classified as non-bunching, with estimated densities of estimated award values below (black) and above (red) the threshold

Among authorities identified as bunching (Figure 1.8), the density of contract values rises sharply just below the threshold. This indicates a systematic tendency to adjust estimated values downward.

Table 1.10: Comparison of Bunching and Non-Bunching Authorities

Variable	Bunching Authorities	Non-Bunching Authorities	t statistic	p value
Award Value	$1,545,712$	$1,487,274$	-3.225	0.001***
Description Length	343.891	345.811	0.990	0.322
Award Criteria	1.462	1.465	0.489	0.625
Number of Offers	6.754	6.723	-0.788	0.431

Figure 1.9, which presents the distribution for non-bunching authorities, also exhibits a discontinuity at the threshold, though the spike just below it is less pronounced. This indicates that even among authorities classified as non-bunching, some degree of strategic behavior may still be present in aggregate, but to a lesser extent and insignificant for individual contracting authorities.

The comparison between the two figures highlights that strategic adjustments in contract values are not exclusive to explicitly bunching authorities if we do not look at the authorities separately, but across all contracts issued by bunching and non-bunching authorities, respectively. However, the magnitude of the discontinuity suggests that bunching authorities engage in this behavior more systematically.

Having shown descriptive evidence of authorities clustering contract award values just below EU thresholds, I now turn to the analysis at the contracting authority level. The RDDensity-based indicator introduced earlier assigns a binary bunching classification to each authority based on their individual contract value distributions. This classification enables comparisons at the micro-level between authorities that exhibit bunching behavior and those that do not.

Table 1.10 presents summary statistics for these two groups. The average awarded contract value is slightly higher for bunching authorities (€1.55 million) compared to non-bunching authorities (€1.49 million). This suggests that bunching authorities may tend to operate in larger procurement markets or allocate larger contracts, although they reduce estimated award values locally around the threshold to fall below the threshold.

The descriptive comparison indicates that bunching and non-bunching authorities are similar along observable tender characteristics. Description length, the number of award criteria, and the number of offers per tender do not differ meaningfully between the two groups, and none of these differences are statistically significant.

These descriptive results give a first impression that tender complexity and competitiveness might not be influenced by the bunching of public authorities below thresholds. The lack of significant differences in award criteria count and number of offers suggests that bunching authorities do not strategically provide longer descriptions or more award criteria to pre-select bidders. The higher average award value for bunching authorities, while statistically significant, may indicate that these authorities operate in larger procurement markets where local networking and informal agreements with preferred firms play a more prominent role.

To assess whether spatial frictions influence procurement outcomes differently across contracting authority types, I estimate the gravity model from equation 1.3 separately for bunching and non-bunching authorities. The dependent variable is the realized award value, while the key independent variables measure distances between the firm, the contracting authority, and the place of performance. By estimating the model separately for the two groups, I examine whether the degree of home bias, proxied by the relative importance of contract values to firm-authority and firm-place distances, varies systematically. I hypothesize that home bias is more pronounced among bunching authorities. These authorities may deliberately keep contract values just below EU procurement thresholds in order to retain greater discretion, potentially allowing them to favor local firms with whom they have pre-existing relationships.

Table 1.11: Effect of Spatial Distances on Award Values for Bunching and Non-Bunching Authorities

Dependent Variable:	Award Value	
Model:	(1)	(2)
	Bunching Authorities	Non-Bunching Authorities
Variables		
Firm-Authority Distance	-0.1316**	-0.0498
	(0.0514)	(0.0473)
Firm-Place Distance	-0.0820	-0.0893*
	(0.0544)	(0.0485)
Authority-Place Distance	-0.1145	0.0622
	(0.0901)	(0.0717)
Fixed-effects		
Firm NUTS3	Yes	Yes
Authority NUTS3	Yes	Yes
Place NUTS3	Yes	Yes
Year	Yes	Yes
Bilateral Geographic Controls	Yes	Yes
Fit statistics		
Observations	102,146	120,739
R^2	0.37986	0.37237
Within R^2	0.01110	0.00901

Clustered (Firm NUTS3) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

The results in Table 1.11 support the hypothesis that bunching authorities, those that strategically set contract values just below EU procurement thresholds, exhibit a stronger home bias in procurement decisions. For non-bunching authorities (Column 2), the coefficient on Firm-Place Distance is statistically significant, while the effect of Firm-Authority Distance is not. This pattern aligns with the expectation that trade costs are typically captured by the distance between the firm and the place of performance, which should influence procurement outcomes when decisions are guided by efficiency considerations.

In contrast, for bunching authorities (Column 1), the Firm-Place Distance is not significant, while the Firm-Authority Distance has a statistically significant negative effect. This suggests that proximity to the contracting authority - rather than to the place of performance - plays a more important role. Such a pattern is consistent with the idea that social networks, local familiarity, or informal relationships with decision-makers may influence supplier selection. This finding supports the proposed mechanism in which contracting authorities bunch contract values to remain below EU thresholds, thereby retaining greater discretion and the ability to favor nearby firms with whom they may have established ties.

Non-bunching authorities, by contrast, are subject to stricter transparency and competition requirements, which may reduce the role of local ties and shift emphasis toward objective considerations like proximity to the project location. However, it remains an open question whether bunching authorities exhibit stronger home bias because they operate under less restrictive rules, or whether they already hold stronger local preferences and strategically bunch contract values to circumvent regulatory constraints.

This analysis has an important limitation: As shown in Table 1.10, bunching authorities tend to award higher valued contracts. Therefore it is not clear whether the results are driven by the contract size or the actual bunching behavior. The estimated higher home bias may reflect bunching behavior, larger contract size, or a combination of both.

These results contribute to the discussion on discretion in procurement and align with Palguta and Pertold (2017) and Tas (2023) in two respects. First, the findings confirm that contracting authorities manipulate estimated contract values to remain just below regulatory thresholds. Second, such discretion appears to be used to favor certain firms, which are often local. The contribution of this study is to extend the existing literature by providing a new definition of home bias by distinguishing between the location of the contracting authority and the place of performance. This differentiation allows me to examine whether authorities that manipulate award values also exhibit a stronger home bias.

1.6 Conclusion

This paper studies home bias in public procurement by disentangling the location of the buying public authority and the place of performance, service, or delivery. I define home bias as the relative importance of the distance between the winning firm and the awarding authority versus the distance between the winning firm and the place of performance. If procurement decisions were driven purely by trade or travel costs, only the distance between the firm and the place of performance should matter. In contrast, a stronger role of firm-authority proximity suggests local favoritism or the influence of social and institutional networks.

To test this, I estimate an extended gravity model at the NUTS3 level using contract award values as the outcome. This model extends the standard gravity framework by incorporating a third location, the place of performance, alongside firm and public authority locations, thereby allowing the use of location triplets as the unit of observation.

The results show that firm-authority distance is a stronger determinant of procurement outcomes than firm-place distance. This implies that proximity to the contracting authority plays a more important role in determining contract values, consistent with the presence of home bias driven by local ties or favoritism.

To examine reasons and find mechanisms of this home bias, I analyze how the effect varies across different contexts. First, home bias is stronger for more complex contracts, those with longer descriptions, suggesting that authorities may tailor specifications to favor a preferred firm. Furthermore, I look at digital industries and do not find evidence of a home bias, in contrast to non-digital industries. Fully electronic tender procedures are associated with a modest reduction in home bias, indicating that digitalization can help constrain discretion. Finally, I examine bunching behavior at procurement thresholds, where authorities appear to manipulate estimated contract values to fall just below EU regulatory limits. By classifying authorities as bunching or non-bunching, I find that only the bunching authorities exhibit a home bias. This indicates that authorities actively reduce regulatory oversight in order to retain discretion and favor local firms.

Overall, the study shows a home bias for all contracts, except electronically auctioned contracts, contracts awarded by non-bunching authorities and contracts in digital industries. Furthermore, the descriptive analysis in Section 1.2 suggests that home bias is stronger for contracts spanning wider distances and for contracts with a larger scope. Although this is contradictory with more complex tender leading being associated with higher contract award values and greater home bias at the same time, future research could disentangle whether the scope of the contracts, and the contracting authority level may drive those results. Section A.5 provides a first descriptive step in this direction.

While a growing literature investigates home bias in procurement, this paper offers a novel spatial approach to identify it by using three locations and comparing the relative importance of proximity to the buyer versus proximity to the place of delivery. This definition of home bias offers new insights into how public authorities may deviate from purely efficiency-based allocation. Methodologically, this paper also contributes to the gravity model literature by incorporating a three-location framework with location triplets as the unit of analysis. The contract-level structure further enables fine-grained, microeconomic insights into procurement behavior.

This paper also opens several avenues for future research. A key limitation is that the direction of causality remains difficult to establish: do certain authorities exhibit home bias a priori and then seek ways to circumvent regulation, or does regulatory discretion enable bias that would otherwise not occur?

Policy implications follow directly. Encouraging fully electronic tendering systems, strengthening transparency through formalized award criteria, and monitoring for threshold bunching can help reduce discretion and limit the scope for home bias.

CHAPTER 2

Local Business Tax Rates and Localization in Public Procurement

with Sanne Kruse-Becher (DHBW Stuttgart)

Chapter Abstract

This paper investigates whether municipal-level business taxes incentivize local authorities to award contracts to firms within their municipalities. When municipalities purchase from local businesses, they benefit indirectly through increased local business tax revenue, as these contracts boost the profits of the contracted firms. We use a gravity model of procurement contracts based on data from German municipalities and exploit exogenous fiscal variation introduced by a census shock to estimate the influence of local business tax rates on the likelihood of contracting local firms. Our analysis reveals that a one percentage point increase in the local business tax rate shifting factor is associated with a 2.4% higher increase in the number of contracts awarded to local firms compared to non-local firms.

2.1 Introduction

Public procurement refers to the process by which public governments or firms in public ownership purchase goods and services. It is a critical issue for policymakers of quantitative economic importance. In OECD countries, public procurement accounts for approximately 12% of GDP. As a result, there has been growing interest in making public procurement more efficient and transparent. Beyond its scale, public procurement is unique because it is directly influenced by policymakers and public authorities, who are responsible for achieving broader social objectives such as maximizing social welfare, promoting sustainability, ensuring equity, and fostering innovation in public spending (OECD, 2021). Due to the complexity of laws in different countries and the mix of public and private decision makers, it is vulnerable to misconduct and potential conflicts with policy goals of higher federal authorities (Bosio et al., 2022).

In 2004, the European Union took a step towards harmonizing procurement practices by implementing a directive to coordinate public procurement across member states. This initiative aimed to improve competition and transparency. However, many public procurement contracts are still awarded at the local level, often to local firms, which leads to a substantial home bias in public procurement (Mulabdic and Rotunno, 2022). This disconnect between local procurement practices and the broader goals of higher federal levels raises an important question: do local incentives, such as municipal-level business taxes, influence procurement decisions in ways that may counteract national or EU-wide policy goals?

The incentive for local authorities to favor local firms in procurement arises from a tax cashback effect: When a municipality awards a contract to a local firm, the firm's increased operating profit is subject to local business taxation of the buying municipality, increasing the municipality's tax revenue. If the authority had bought the good or service from a non-local firm, i.e., a firm from another municipality, the additional profits from the procured good would have been subject to local business taxation by another municipality.

In this paper, we investigate whether local business tax (LBT) rates act as a mechanism through which local authorities prioritize procurement from firms within their own jurisdiction. Our main research question is: Do municipalities with higher LBT rates procure more from local firms? Specifically, we examine the effect of higher LBT rates on public procurement practices in German municipalities.

This paper pioneers the empirical assessment of tax incentives for localization in public procurement. Thereby, it first contributes to the literature on public procurement by offering a mechanism, which can explain that the home bias in trade between public and private firms is more pronounced than trade between solely private firms. Second, it offers a quantification of the cashback effect of local taxes on public procurement. Importantly, the German local business tax system offers a unique setting, as tax rates vary across municipalities and are frequently adjusted. However, other rules of the business tax, e.g., the liability criteria, are set at higher federal levels. Thus, the variation allows us to observe effects directly at municipal boundaries.

To this end, we construct a municipality-level gravity model based on data from bilateral public procurement contracts in Germany, where the dependent variable is the number of contracts awarded by a contracting authority in one municipality to firms in another. We distinguish between local procurement as contracts awarded to firms located within the same municipality as the contracting authority and non-local procurement as contracts awarded to firms in other municipalities. To assess the impact of LBT rates, we interact a local procurement dummy with the business tax rate of the contracting authority. Additionally, we include origin-destination fixed effects to control for municipality-specific characteristics as well as bilateral resistance terms. The coefficient of the interaction term provides an estimate of the effect of an increased local business tax rate on the decision whether to procure locally or non-locally.

To address potential endogeneity concerns, we exploit exogenous variation from a fiscal shock induced by the 2011 German census, known as the census shock. The census replaced outdated population projections with newly collected data, leading to downward revisions in some municipalities' official population figures. As a result, these municipalities received lower transfers from the municipal equalization scheme, tightening their budgets. In response, fiscally constrained municipalities had stronger incentives to increase LBT rates to compensate for lost revenue. This mechanism allows us to isolate fiscal incentives from other factors influencing procurement behavior.

The remainder of the paper is structured as follows: Section 2 describes the literature our paper speaks to. Section 3 explains the institutional background of the local business tax in Germany and public procurement. Section 4 describes the data used for the analysis, Section 5 outlines the empirical methodology, Section 6 presents the results including robustness checks, and Section 7 concludes with a discussion of the policy implications of our findings.

2.2 Literature

Evidence about the role of taxes in public procurement is a field that has yet to be explored. Weichenrieder (2001) pioneers the investigation of a tax incentive for public procurement under capital taxation in a theoretical model. In this model, the capital tax incentivizes local procurement to promote local capital accumulation, ultimately resulting in a tax cashback effect. However, there is no empirical evidence for this mechanism.

Our perspective on tax incentives and public procurement relates to two other strands of literature. First, we provide evidence for one mechanism giving rise to a home bias in trade, which is unique for trade between public and private actors, yet absent for trade between private firms. Home bias in public procurement has been studied by several papers: As a starting point, Shingal (2015) utilizes World Trade Organization data on procurement contracts from Japan and Switzerland to examine how various determinants influence the share of foreign procurement. His findings suggest that productivity differentials play a central role, with governments prioritizing efficiency. However, Herz and Varela-Irimia (2020) identify trade-reducing border effects within the European Union's integrated market using gravity regressions. Mulabdic and Rotunno (2022) use a similar gravity-approach to show that home bias in government procurement on the country-level is systematically larger than in private markets. They find that trade agreements increase cross-border trade, but only for service procurement. Similarly, Garcıa-Santana and Santamarıa (2021) demonstrate that home bias, unrelated to productivity gains, accounts for a substantial portion of border effects in Spanish and French procurement contracts. They also find those results on sub-national levels. Hoekman (2018) and Shingal (2015) further find that public procurement agreements fail to mitigate home bias effectively. Arozamena et al. (2023) explores favoritism in procurement auctions and demonstrate that even in the absence of explicit discrimination, post-award renegotiation allows local or politically connected firms to receive preferential treatment, leading to a distortion of competition.

The literature identifies several causes and mechanisms underlying home bias. One category pertains to bribery, corruption, favoritism, and political economy objectives. For instance, Coviello and Gagliarducci (2017) reveal that mayoral tenure significantly impacts procurement outcomes, with longer tenure associated with higher procurement costs and an increased likelihood of local firms winning contracts. Additionally, governments may include home procurement in their objective functions to stimulate Keynesian multiplier effects, as Evenett (2009) observe increases in home procurement during recessions. Furthermore, trade literature highlights consumer preferences for domestic goods. Morey (2016) finds that preferences for domestic products result in lower-than-expected trade volumes, and governments may similarly exhibit such preferences in their procurement policies. By demonstrating that localized tax systems act as an incentive for local procurement, we provide a novel explanation for home bias in public procurement.

Moreover, we contribute to a literature strand, where recent studies have leveraged the German Local Business Tax system, which provides within-country variation in tax rates without granting municipalities control over the tax rules. This setting is particularly suitable for our analysis because it combines local variation in tax rates with a uniform national regulatory framework, ensuring that observed differences in outcomes are not confounded by broader institutional differences. Unlike cross-country studies, this framework minimizes variation in determinants of the tax base beyond the LBT rate. For instance, Fuest et al. (2018) estimate the incidence of LBT changes on wages, while Becker et al. (2012) show that reductions in LBT rates stimulate activity of multinational enterprises in the respective municipalities, with an increased number of headquarters of multinational enterprises. Link et al. (2024) utilize the timing of LBT rate changes in Germany—municipalities decide at the end of the year whether to adjust the LBT rate—to find that firms reduce their planned investments after local tax rate increases. They find that more firms reduce their planned investments after local tax rate increases than they would have done without an increase in the tax. By evaluating whether the local business tax system in Germany counteracts policy goals on the EU level, our study contributes to the discourse on discriminatory tax policies and fiscal federalism. Variations in business tax rates across municipalities can counteract other policy objectives and result in efficiency losses. For example, Fajgelbaum et al. (2019) analyze discriminatory tax policies and their effects on the location decisions of workers and firms in the U.S., finding that heterogeneity in state taxes leads to welfare losses.

2.3 Institutional Background

2.3.1 Local Business Tax

The local business tax is a key component of Germany's municipal financing system. The LBT is levied on the operating profits of both corporate and non-corporate firms. Although the liability criteria and tax base are federally regulated, municipalities have full control over the local tax rate, which they adjust annually. The total tax revenue of municipalities consists of property taxes, the municipal share of the income tax, the municipal share of the consumption taxes and the local business tax. Compared to the other taxes, the local business tax plays an important role due to several reasons. (1) The tax revenue is directly dependent on the economic performance of local firms and therefore the performance of the municipality as a business location. (2) By setting the local business tax rate, the municipality has sovereignty on the tax policy and (3) with 38%, it is the tax with the highest share of the total tax revenue after the municipal share of the income tax (according to calculations with data from Bertelsmann Stiftung, 2024). Therefore, this tax plays a crucial role in financing local government budgets and influences the decision-making of municipal policymakers.

The LBT is based on the operating profits of businesses located within the respective municipality. The tax rate is determined by the municipality and consists of two main components: (1) The base rate applied uniformly across all municipalities and (2) the local business tax multiplier. Each municipality sets its own multiplier, which can vary from one municipality to another. This multiplier adjusts the base rate to reflect local fiscal needs and policy priorities.

Municipalities can and often do adjust their multipliers annually, which introduces a dynamic element to the tax system. In the municipalities' annual budgetary meetings, a decision on the multiplier has to be made, no matter if they want to keep it or decide to change it. These adjustments are influenced by local budgetary needs, economic conditions, and political considerations.[1]

The revenue generated from the local business tax is retained by the municipality and used to finance local services and infrastructure. This includes public transportation, education, and municipal administration. This variation in LBT rates can influence business location decisions, potentially encouraging businesses to relocate to municipalities with lower business tax rates.

Municipalities might use the local business tax as a tool to align with broader policy goals, such as promoting local economic development or adjusting fiscal policies in response to economic conditions. The ability of municipalities to set their own tax rates can lead to conflicts between local and higher-level policy goals. For instance, local governments might prioritize attracting businesses through lower tax rates, which could be at odds with national policies aimed at standardizing tax practices, addressing broader economic imbalances or promoting competition among firms, the latter being a core policy concern of the EU (Becker et al., 2012; Fossen and Steiner, 2018).

2.3.2 Public Procurement

The public procurement regulation in Germany is based on policy directives set by the European Union. These EU regulations aim to promote competition, ensure transparency, and provide equal access to procurement opportunities across all member states. The framework is designed to prevent discrimination and favoritism while encouraging the efficient use of public funds. A key aspect of this system is the introduction of thresholds that determine when a procurement contract is considered of EU-wide interest and, therefore, must be open to all firms within the European Union. For contracts involving goods

[1] In our estimation, we control for these factors through inclusion of municipality-time fixed effects at the local level among others.

and services, this threshold is set at €143,000 for central governments and €221,000 for sub-central governments, while for public works contracts it is €5,538,000 (European Commission, 2024; European Parliament and Council of the European Union, 2004).

For contracts exceeding the established thresholds, all procurement notices must be published in the Supplement to the Official Journal of the European Union, which is accessible online via the Tenders Electronic Daily (TED) platform, while contracts below the threshold might still be published on the platform. Depending on the procedure, the contracting authority may issue multiple notices.

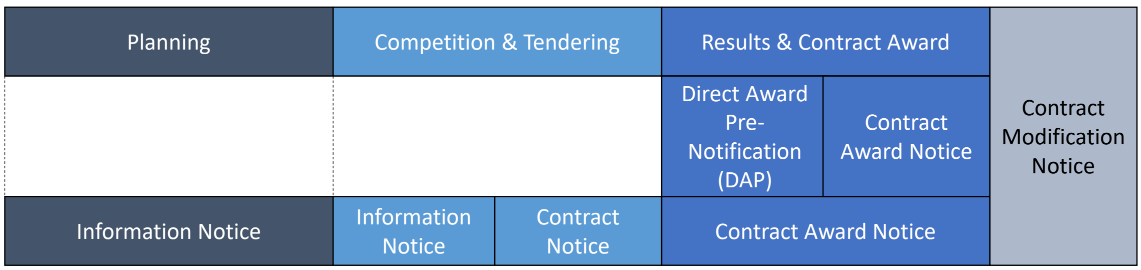

Figure 2.1: Public Procurement Notices & Process for EU-wide tenders (TED eForms Documentation, 2024)

Figure 2.1 shows the different types of notices and their use throughout the process. Contracting authorities in the EU can issue preliminary information notices to outline the anticipated scope and timing of future projects. These notices, released before any formal competition, serve to inform potential bidders about upcoming opportunities, facilitating early preparation. Following one or more of these information notices, a detailed contract notice is issued, including a unique contract identifier and a call for competition. Even after issuing the contract notice, authorities may publish additional information notices if necessary.

Contracting authorities may opt to publish a Direct Award Pre-Notification (DAP), which signals their intention to award a contract directly to a specific firm, bypassing the competitive process. In this, the buyer has to state a reason for the direct award with extreme urgency being the most common reason. This pre-notification is released before the contract is signed and aims to ensure transparency by clarifying situations where direct awards are justified.

Typically, after the tendering phase, a contract award notice is published to announce the outcome, including details of the selected bidder. This notice is released once the contract has been signed. In the case of direct awards, the publication of a contract award notice supplements the earlier DAP. Moreover, if there are modifications to the contract post-award that do not initiate a new tendering process, Contract Modification Notices are issued to inform the public of these changes.

The mechanism that drives the effect we investigate follows a timeline of subsequent events for the decision-makers, affecting their incentives. Figure 2.2 shows an example chronology of the events happening throughout a procurement process and the decisions on the municipality level.

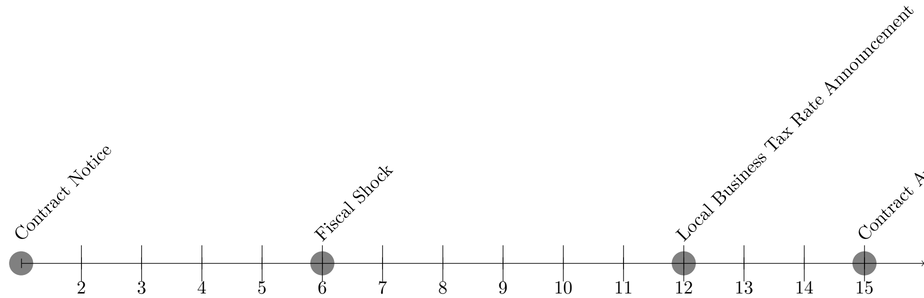

Figure 2.2: Illustrative timeline (in months) showing the sequence of key events: contract notice, fiscal shock, tax rate announcement, and contract award notice.

First, the public authority publishes a contract notice to announce the upcoming contract and to specify its requirements. Subsequently, the fiscal shock we use as an instrument is realized. This shock alters the municipal budget, leaving some municipalities with exogenously reduced financial resources, thereby increasing their reliance on tax revenues. At the annual budgetary meeting held every December, each municipality decides on its LBT rate. In response to the negative fiscal shock, municipalities may choose to raise the LBT rate to increase their tax revenues.

Following the decision to adjust the LBT rate, the municipality decides which firm will be awarded the contract. Anticipating a future tax cashback from an increased tax base generated by the contract, municipalities with higher LBT rates have higher incentives to award the contract to a local firm, compared to a municipality with a lower LBT rate. The decision to choose a local supplier ensures that the tax cashback applies, in contrast to awarding the contract to a non-local firm, where the contract would not contribute to the municipality's tax base.

2.4 Data

To investigate the impact of LBT rates on municipal procurement decisions, we merge municipality-level data with public procurement contract data to create a gravity panel dataset. This dataset contains all combinations of German municipalities engaged in public procurement from 2008 to 2019. We restrict our analysis to the years before 2020 to avoid distortions caused by the COVID-19 pandemic, which in Germany began in 2020 and influenced government spending, fiscal policies, and procurement activities, as well as the feasibility of initiating and completing projects. Leveraging municipality-level variation in LBT rates, we can analyze how these rates influence incentives for municipalities to procure from local firms.

2.4.1 Municipality Data

We analyze LBT rate shifting factors across German municipalities from 2008 to 2019. The data originates from Wegweiser Kommune, a project by the Bertelsmann Stiftung, which aggregates information from various German administrative sources, such as the federal statistical offices and the Federal Employment Agency. Specifically, the data on LBT rates is derived from the regional statistical offices of the German federal states and supplemented by data from the German national statistics office (Bertelsmann Stiftung, 2024).

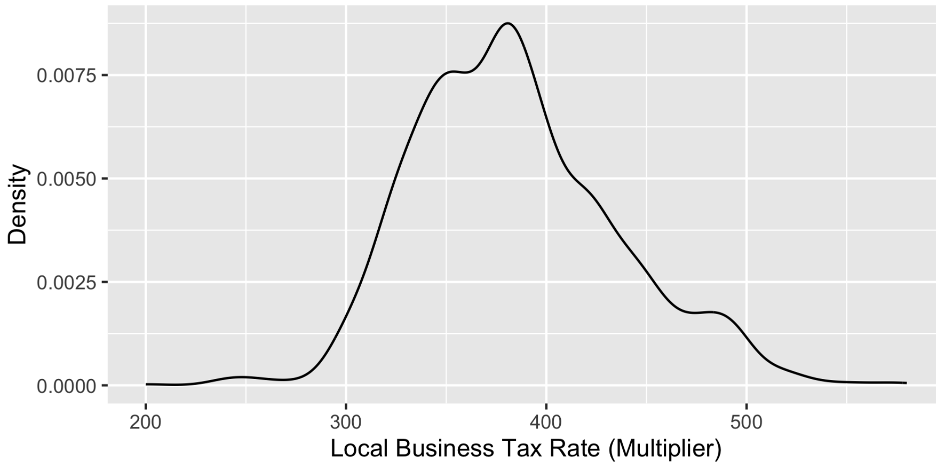

Figure 2.3: Local Business Tax Rates in Germany

Figure 2.3 shows the kernel density estimate of the LBT rate shifting factors among the German municipalities. While 90% of the municipalities scale the base tax rate by 315% to 480%, the rate is distributed heterogeneously among the municipalities with the smallest scaling factor being 200% and the largest 580%. Throughout this paper, we use the shifting factor as a variable in the regressions, not the complete tax rate. Therefore, "LBT rate" refers to the LBT rate shifting factor, unless noted otherwise.

The map in Figure 2.4 depicts the spatial distribution of LBT rates across German municipalities, showing considerable regional variation. Darker areas represent higher tax rates, which are primarily concentrated in economically developed urban and industrial regions, especially in western and southern Germany. Firms located in the Rhine-Ruhr Metropolitan area are subject to especially large LBT rates. In contrast, lighter regions, indicating lower tax rates, are more prevalent in rural and more deprived areas, notably in parts of eastern Germany. The map highlights the variation in LBT rates, which could reflect different dimensions of regional economic policies, disparities in economic development, and strategic fiscal decisions by municipalities aiming to attract or retain businesses.

The map in Figure 2.5 illustrates the frequency of changes in LBT rates across German municipalities from 2008 to 2019, with darker-red colors representing a higher number of tax rate adjustments. In contrast, regions depicted in yellow experienced fewer adjustments. Higher frequencies of tax rate modifications are predominantly clustered in central and eastern Germany. Our identification strategy exploits this rich variation in LBT rate changes across time, as we use municipality fixed effects throughout our specifications.

The spatial pattern suggests a differentiated fiscal strategy, where some municipalities actively adjusted tax rates to respond to economic conditions, possibly reflecting efforts to attract or retain businesses. Meanwhile, municipalities with fewer changes in tax rates may indicate either stable economic conditions or a preference for long-term tax consistency to promote predictability for businesses. Furthermore,

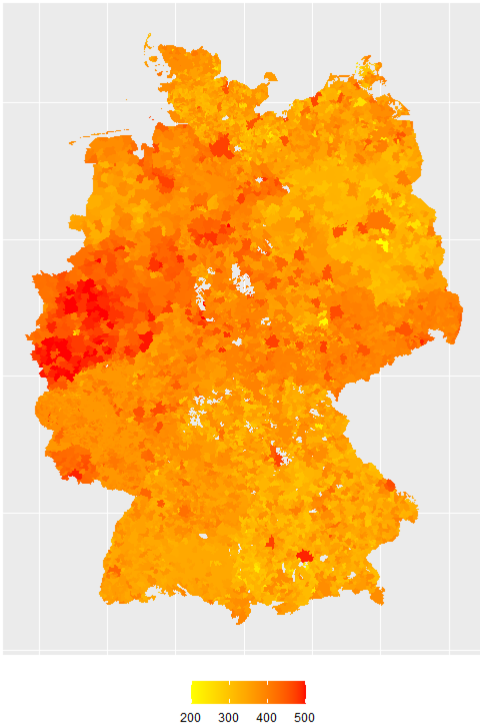

Figure 2.4: Map of German Local Business Tax Rates Shifting Factor (Average 2008-2019)

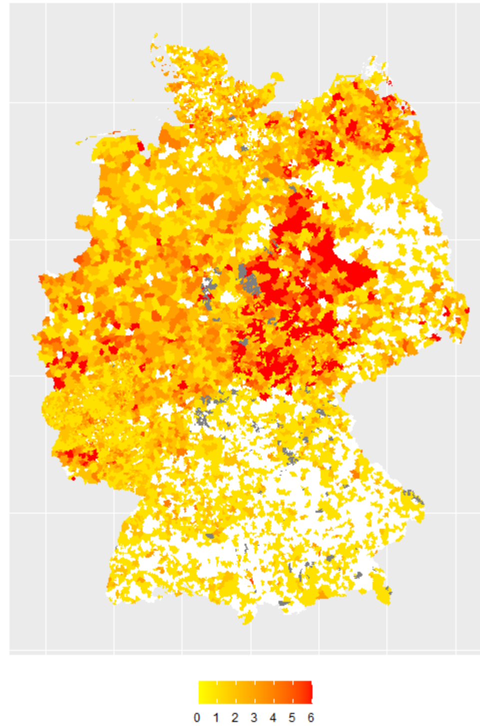

Figure 2.5: Map of German Local Business Tax Hikes (Number of hikes from 2008 to 2022)

municipalities with smaller LBT rates have mechanically smaller numbers of tax hikes, as municipalities always increase the local business tax rate and almost never decrease. Therefore, there are very few changes in rural areas in eastern and southern Germany, where LBT rates are lower in general.

In order to account for heterogeneity of fiscal policy at the municipality level, we include various fiscal variables in our data. These include debt levels, total tax revenue, and per capita payments from the German municipal equalization scheme. This scheme redistributes funds to municipalities based on a calculated fiscal need and their tax revenue. The equalization payments are highly variable and are designed to ensure a comparable level of public sector quality across municipalities. The fiscal need calculation is heavily influenced by population size, making the payments from the equalization scheme strongly correlated with population.

Additionally, we incorporate data on population, area, and NUTS regions for each municipality. To compute the census shock, we also use data from the 2011 census survey. Population figures for other years are derived from projections based on demographic flow variables such as births, deaths, immigration, and emigration. Since the 2011 census was conducted in May, we use population projections from December 2010 and the official census data from May 2011 to calculate the census shock (see chapter 4 for details).

We calculate the distances and contiguity between all pairs of municipalities and categorize the spatial environment into three groups: local, regional, and non-regional. Local refers to activities within the same municipality. Regional refers to nearby municipalities, which are either contiguous (sharing a border) or within a specified distance of 50 kilometers. All other municipalities, which are non-contiguous and located farther away, are categorized as non-regional.

2.4.2 Procurement Data

For our analysis of public procurement, we rely on data from Tenders Electronic Daily (TED)[2], the European Union's online portal for public procurement tenders (GROW G4, European Commission, 2024). This dataset covers on average about 43,000 public procurement contract award notices per year for German municipalities, spanning the period from 2008 to 2019. The data is submitted directly by public authorities responsible for procurement, who provide the necessary forms electronically for public tenders. These forms are then published in the Supplement to the Official Journal of the European Union, accessible through TED.

The dataset includes key information such as the value of contract awards, as well as the locations of both the winning firms and the public authorities issuing the tenders. Additionally, TED employs a classification system known as the Common Procurement Vocabulary (CPV), which allows us to categorize tenders into specific industry sectors. The CPV system consists of a 9-digit code, with an additional digit indicating whether the contract pertains to services, supplies, or works. Each contract award notice contains a primary CPV code along with supplementary codes. In the case of German contract award notices, we observe 2,210 unique classifications under the main CPV codes. Our dataset includes contracts from all procurement procedures.

The vast majority (86%) are awarded through open procedures, which allow unrestricted participation. Restricted procedures account for 4% of contracts, where only pre-qualified bidders are invited to submit bids, and no negotiations take place. Additionally, 1% of contracts are awarded without a prior published contract notice, indicating the use of direct award mechanisms. Finally, 5.7% of contracts are procured through negotiated procedures with a call for competition, where contracting authorities engage in negotiations after an initial competitive phase.

Figure 2.6 illustrates the different types of government public procurement authorities and their award value share of the total contracts. Most of the contract value is awarded by regional and local authorities, which is crucial because these authorities directly influence procurement decisions within their regions. Most importantly, they are in charge of their own municipal budget and can influence it with public procurement decisions. In contrast, national governments make decisions on public procurement that impact distant locations, and are therefore not affected by the mechanism presented in this paper. Local authorities, as the key decision-makers, have strong incentives to act in the best interest of their municipalities, particularly when their fiscal policies, such as tax rebates from firms located within the same municipality, are involved.

To take the relevance of the mechanism for different types of buyers into account, we exclude contracts

[2]As we use only German contracts, we abstain from using OpenTender data as a complementary dataset, as it covers the same set of contracts for Germany (above and below thresholds) and does only add variables that are not relevant for this study.

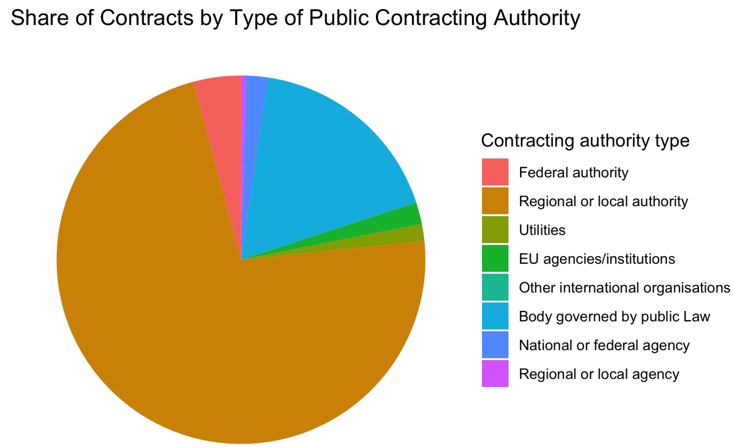

Figure 2.6: The different types of contracting authorities

decided at higher federal levels, focusing only on those awarded by regional or local authorities and agencies, where municipality-level tax incentives are relevant. We are left with 66% of the total contracts. Additionally, we omit contracts from public firms, as they are not directly influenced by municipal budgets or fiscal decisions. This leaves us with contracts awarded by municipal governments and administrative bodies. To avoid the influence of more restrictive EU guidelines for EU-funded projects, we exclude those projects.

2.4.3 Estimation Data Set

We merge the data using a spatial join. We start by geocoding the locations of all unique contracting authorities and winning firms. This process involves using the Nominatim database to input the address, postal code, and city name provided with the public procurement contract data from the European Union. We then use shape files from the federal and state governments, created as part of the 2022 census, which include the shapes of each German municipality and their official German municipality identifier ('Amtlicher Gemeindeschlüssel').

For each contracting authority and firm in the data, we identify the municipality polygon of the shape file that contains its geocode and assign the corresponding municipality identifier to the contracting authorities and winning firms. We use this identifier to merge contracting authorities and winning firms with data at the municipality level, such as the local business tax rate.

Through the Nominatim geocoding process, we successfully obtain geocodes for 99% of the contracting authorities and 87% of the winning firms. After performing the spatial join with the municipality shape files, 4.5% of the contracts have missing municipality identifiers for the contracting authorities, and 14.5% have missing identifiers for the winning firms.

2.4.4 Descriptive Analysis

The identifying assumption is that the authorities benefit from the tax cashback by choosing a local firm, where the benefit ends sharply at the municipal border. Table 2.1 shows the distribution of public procurement contracts awarded to firms based on their location relative to the municipality issuing the contract. For works and supply contracts, the majority of contracts (81% and 83%, respectively) are awarded to non-local and non-contiguous firms, with only 11% of works and supply contracts going to local firms within the same municipality, and 8% and 6% to firms within a contiguous municipality. In contrast, service contracts exhibit a higher proportion awarded to local firms (25%), with 12% going to firms in neighboring municipalities and 63% to firms from other regions. This indicates that while non-local firms dominate works and supply contracts, local firms play a more important role in service procurement. While procurement opportunities are typically larger in all neighboring municipalities combined than in the local municipality, the authorities still procure more from local firms. This suggests that there might be incentives for the contracting authorities that end at the own municipal border.

Table 2.1: Percentage of contracts procured from firm within own municipality (local), a neighboring municipality (neighbor) or from other municipalities

	Works	Supply	Services
% Local	11%	11%	25%
% Neighbour	8%	6%	12%
% Others	81%	83%	63%

To provide an initial illustration of the relationship between LBT rates and the share of contracts awarded to local firms (i.e., the share of locally procured goods and services), Figure 2.7 presents a scatter plot of the data. Each point represents a municipality, and the fitted regression line illustrates the general correlation of municipalities' average LBT rates and their share of local procurement. The share of locally procured goods is the number of contracts a municipality procured from a firm within their own municipality divided by the total number of contracts the municipality procured during the observation period. This figure shows the positive correlation between LBT rates and the share of local procurement, suggesting that municipalities with higher tax rates might have stronger incentives to procure locally.

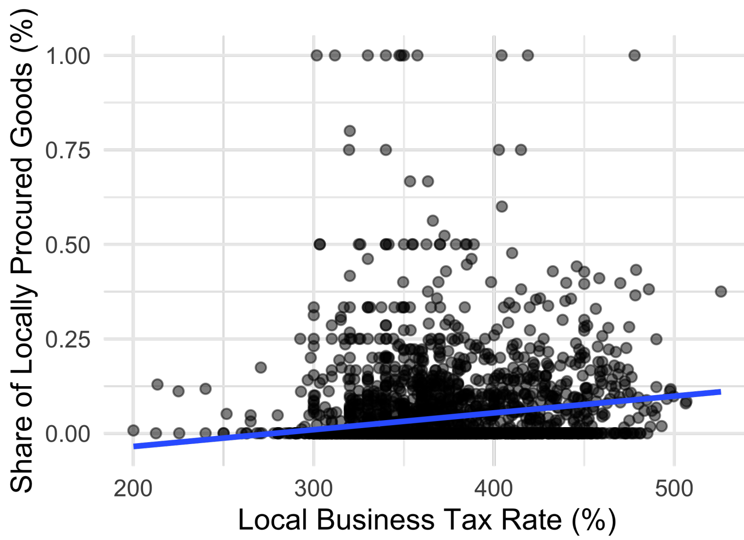

Figure 2.7: Scatter plot showing the correlation between the LBT rate and the share of locally procured goods. Each point represents a municipality.

To further quantify the correlation between local procurement and the LBT rate, we begin with an ordinary least squares (OLS) cross-sectional analysis at the municipality level, including all municipalities active in public procurement in the specific year, as represented in Equation 2.1. The percentage of contracts procured locally serves as the dependent variable, while the LBT rate is our primary independent variable.

$$\text{Share Local}_{it} = \alpha + \beta \text{LBT}_{it} + \varepsilon_{it} \tag{2.1}$$

Table 2.2 presents the results: Column 1 shows the cross-sectional estimates for the year 2008, and Column 2 for the year 2019. In Column 3, we average the LBT rate and calculate the share of local procurement over the full observation period, providing a long-term cross-sectional perspective.

Table 2.2: OLS estimates: Correlation between share of local procurement and local business tax rate

Dependent Variable:	Share Local		
	Cross-Section 2008	Cross-Section 2019	Cross-Section Avg. 2008-2019
Model:	(1)	(2)	(3)
Variables			
Constant	-0.0991**	-0.0754***	-0.1238***
	(0.0487)	(0.0280)	(0.0146)
LBT	0.0004***	0.0003***	0.0004***
	(0.0001)	(7.31×10^{-5})	(4.04×10^{-5})
Fit statistics			
Observations	752	1,857	3,667
R^2	0.01396	0.01175	0.03194
Adjusted R^2	0.01264	0.01122	0.03167

IID standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

Table 2.2 reveals a positive correlation between the LBT rate and the share of local procurement. Specifically, a 1-percentage-point increase in the LBT rate shifting factor is associated with an increase of 0.0004 percentage points in the share of locally procured contracts.

Table 2.3: Percentage of contracts procured from firm within own municipality (local), a neighboring municipality (neighbor) or from municipalities in a 100 kilometer radius

	Small (< 25%)	Medium (25% – 50%)	Large (> 75%)
% Local	4.3%	5%	16.4%
% Neighbour	0.4%	0.9%	5.7%
% 100km radius	11.2%	11.5%	22.9%
Local Business Tax rate	343.8%	362.4%	401.5%

Table 2.3 provides an overview of public procurement contracts awarded based on the size of the contracting authority and the location of the firm. The municipality sizes in this table are categorized into small (smallest quartile of total procurement value), medium (second quartile of total procurement value), and large (fourth quartile of total procurement value). For small municipalities, 4.3% of the total contracts are awarded to firms within the same municipality, while 0.4% are awarded to firms from neighboring municipalities. In contrast, for large municipalities, the share of contracts awarded to local firms increases to 16.4%, with 5.7% awarded to firms in neighboring municipalities. Additionally, firms within a 100 kilometer radius around the contracting municipality receive a larger share of contracts as the municipality size increases, with 11.2% of contracts from small municipalities, 11.5% from medium municipalities, and 22.9% from large municipalities are awarded to firms located within a 100-kilometer radius. The table also shows that the average local business tax rate increases with municipality size, from 343.8% in small

municipalities to 401.5% in large municipalities, indicating a correlation between municipality size and local tax rates.

This raises an important concern: Are larger municipalities more likely to procure from local firms because they have a greater number and variety of suppliers, or do they procure more locally because they benefit more from the tax cashback due to higher LBT rates? To further examine this relationship between municipality size and local business tax rate, table 2.4 shows summary statistics on the contracting authorities' municipality level.

Table 2.4: Summary Statistics on the Municipality Level

Statistic	Mean	St. Dev.	Min	Max
LBT	359.738	42.023	200	580
Total Number of Contracts	57	353	1	12,013
Local Contracts	7.632	83.454	0	2990
Population	27,360	99,832	4,478	3,495,887
Local Share of Contracts	0.0361	0.098	0	1

Table 2.4 presents summary statistics for the 3,755 municipalities with contracting authorities. The LBT rate is positively correlated with municipality size, measured by population and total number of contracts, with correlation coefficients of 0.26 and 0.23, respectively. Similarly, the share of contracts awarded locally is positively correlated with municipality size, with coefficients of 0.23 and 0.16.

Smaller municipalities are less likely to procure locally due to limited availability of local firms capable of fulfilling contracts. As a result, they often procure regionally, not being able to benefit from the tax-recapture mechanism. In contrast, larger municipalities are more likely to source locally due to a larger variety in local industries with a higher likelihood of having suitable local firms. This discrepancy introduces endogeneity: Larger municipalities have higher LBT rates and are more likely to procure locally, independent of the tax-recapture mechanism.

2.5 Empirical Methodology

Our empirical strategy to identify the impact of local business taxation on municipal procurement decisions addresses three major concerns. First, larger municipalities may generally procure more from local firms due to a broader local supply, as shown in the previous section. Second, we must account for tax competition between municipalities. This concern relates to the endogeneity of tax rates in a general equilibrium setting, where municipalities strategically set tax rates. Additionally, firms located in high-tax municipalities may be less competitive due to a higher tax burden, making them less likely to win procurement contracts. This dynamic potentially counteracts the mechanism we seek to identify in this paper. Third, there is considerable heterogeneity in economic, geographic, and fiscal conditions across municipalities.

To address the first concern, we restrict the sample to contracts that could plausibly be sourced locally and regionally. Using Common Procurement Vocabulary (CPV) codes, we classify procurement categories and identify firms capable of supplying these goods and services. We use detailed 9-digit CPV codes to precisely classify contracts. For each municipality, we compile a list of CPV codes that represent categories in which the firms located in that municipality have won at least one contract during the observation period. We create a separate list of CPV codes for each municipality, representing categories in which firms from contiguous or proximal (50 kilometer radius) municipalities have won at least one contract during the observation period. Our analysis then is restricted to contracts that potentially could have been served by at least one firm located in the contracting authority's municipality and by at least one firm located in a regional municipality. Therefore, the sample specifically reflects contracts where the contracting authority had the option to award to either a firm locally or in a regional municipality. The restricted sample consists of 67% of the total number of contracts. Table **??** compares the full and restricted samples. While the restriction reduces the number of municipalities and contracts, there are no substantial differences in most variables. Municipalities in the restricted sample tend to have larger populations and a higher share of contracts awarded locally. By restricting the sample, the correlation between the share of contracts sourced locally and municipality size, measured by population and total number of contracts, declines from 0.26 to 0.09 and from 0.23 to 0.05, respectively. The correlation between municipality size and the LBT rate remains largely unchanged. This finding supports the hypothesis that smaller municipalities are less likely to source locally due to limited availability of suitable local suppliers. The restriction helps to identify only contracts that are relevant for the discussed mechanism. To mitigate that bias from local procurement potential, we apply the restricted sample consistently throughout the analysis.

To further investigate the drivers of the initial correlation between the LBT rate and procurement patterns, we conduct a bilateral analysis of municipalities' procurement activities. Instead of aggregating procurement at the municipality level, we analyze individual procurement flows between municipality pairs, where the buying municipality is the destination and the contract-winning firm's municipality is the origin. This allows us to apply a gravity model approach, commonly used in the trade literature, which offers two key advantages in our setting, helping to solve the second and third concern.

First, the gravity framework enables us to control for unobserved bilateral heterogeneity, meaning we do not have to assume homogeneous procurement costs across municipalities. Instead, we absorb these bilateral procurement costs using pairwise fixed effects, which account for geographical frictions such as distance, transportation costs, and institutional differences. Second, the gravity model allows us to explicitly account for bilateral tax competition. Firms in high-tax municipalities face a competitive disadvantage when bidding for contracts in lower-tax municipalities. Higher tax burdens force them to submit higher bids, reducing their likelihood of winning contracts compared to firms in lower-tax locations. A municipality-level approach would fail to capture these cross-municipality tax interactions, potentially biasing estimates of the impact of LBT rates on procurement outcomes.

Equation 2.2 presents the gravity equation for the baseline estimations. We conduct theory-consistent gravity estimations to quantify the impact of higher LBT rates on the number of contracts awarded by a public authority in the destination municipality i to firms in the origin municipality j. The unit of observation is a directed municipality pair (i, j) in year t, capturing the number of contracts awarded by contracting authorities located in municipality i to firms located in municipality j. Thus, i and j represent municipal locations rather than individual institutions.

Our primary coefficient of interest is the interaction between the LBT rate and a local binary variable. The local variable takes a value of 1 if the origin and destination municipalities are the same - that is, if the winning firm is located in the same municipality as the contracting authority - and 0 otherwise. In the municipality-pair (origin-destination) data, this variable therefore identifies intra-municipality procurement flows (self-pairs on the diagonal of the origin–destination matrix), distinguishing them from inter-municipality procurement flows.

The total effect of the LBT rate on the number of local contracts is given by $\beta_1 + \beta_2$. β_2 captures the effect of the LBT rate on the number of non-local contracts, while β_1 represents the differential effect for local contracts, relative to non-local contracts. The analysis focuses on this differential effect, which addresses the question of whether local contracts respond more strongly to changes in LBT rates than non-local contracts. For completeness, we provide F-test statistics for the joint significance of LBT_{it} and $LBT_{it} * Local_{ij}$ in the respective regression tables.

To account for the competitive tax environment among municipalities and to absorb spatial trade frictions, we include bilateral origin-destination (municipality-pair) fixed effects in our model, along with time fixed effects. These absorb all time-invariant differences between municipality pairs, such as geographic distance, historical trade relationships, and industrial composition. The origin and destination fixed effects are implicitly included via origin-destination fixed effects.

Given the prevalence of municipality pairs that do not engage in procurement from each other, the distribution of the dependent variable is skewed, with a high number of zero observations. To address this, we employ the Pseudo-Poisson Maximum Likelihood (PPML) estimator, which has become standard for handling skewed data and zero trade flows in gravity models (Silva and Tenreyro, 2006).

$$X_{ijt} = exp[\beta_1 LBT_{it} * Local_{ij} + \beta_2 LBT_{it} + \beta_3 Pop_{it} + \beta_4 Pop_{jt} + \beta_5 I_i * I_j + \beta_6 I_t] + u_{ijt} \tag{2.2}$$

With

- X_{ijt}: The number of contracts awarded by contracting authorities in municipality i to firms in municipality j in year t.
- LBT_{it}: The Local Business tax rate in municipality i in year t.
- $Local_{ij}$: A binary indicator equal to 1 if municipalities i and j are the same, and 0 otherwise. This variable identifies contracts awarded by a contracting authority to a firm located in the same municipality. If $Local_{ij} = 1$, then $i = j$ and then the aggregated procurement flow in number of contracts $X_{ijt} = X_{iit}$, which describes the intra-municipality procurement between authorities and firms in the same municipality in year t. Put differently, $Local_{ij} = 1$ on the diagonal of the $i \times j$ matrix, and 0 off-diagonal.
- Pop_{it}: The population of municipality i in year t, serving as a proxy for municipality size. In trade gravity models, GDP is typically used, but since GDP data is only available at the NUTS3 level,

we use population as an alternative measure when necessary.

- Pop_{jt}: The Population in municipality j in year t.
- $I_i \times I_j$: Bilateral Resistance terms for the procuring and contract-winning municipality-pairs.
- I_t: Time fixed-effects.
- u_{ijt}: Error term.

A potential endogeneity issue arises from the LBT rate's influence on both municipal behavior and firm competitiveness. Municipalities engage in tax competition with neighboring jurisdictions, strategically setting their LBT rates to attract businesses and influence location decisions. In addition, firms located in municipalities with lower LBT rates have a competitive advantage in public tenders, as lower tax obligations enable them to offer more attractive bids. Consequently, municipalities with higher LBT rates can be incentivized to source from firms in neighboring municipalities with lower tax rates, aiming to benefit from the greater competitiveness of these firms.

The discussion surrounding tax competition has gained significance in the economic literature, particularly in the context of local corporate taxation. Tax competition refers to the strategic decisions made by jurisdictions to lower their tax rates in order to attract mobile capital flows. This dynamic can lead to a "race to the bottom," where states or municipalities reduce their tax rates to levels that fall below optimal thresholds, potentially resulting in adverse long-term effects on public finances.

Becker et al. (2012) demonstrate in their study on the impact of corporate taxes on multinational enterprises (MNEs) that lower local tax rates are significantly correlated with a higher establishment of MNEs. Their findings suggest that municipal tax policy and its competitiveness are critical factors influencing the investment behavior of foreign companies. These insights underscore the necessity for careful consideration of tax competition when analyzing local taxation decisions.

Furthermore, Agrawal et al. (2025) show that intermunicipal cooperation alters the nature of tax competition between municipalities. While cooperation generally weakens strategic interactions among member municipalities, it can simultaneously intensify competition with non-members. Their results highlight that cooperation can mitigate some parasitic effects of tax competition but also generate new competitive dynamics, depending on how cooperation is structured.

In addition, Janeba and Osterloh (2013) propose a novel theoretical model addressing local tax competition dynamics between large cities and smaller hinterlands. Their research indicates that larger jurisdictions may utilize less distortionary taxes compared to smaller ones due to heightened competitive pressures from both neighboring communities and more distant cities. This finding emphasizes how size differences among jurisdictions significantly impact their approach to taxation strategies.

Moreover, Buettner and Poehnlein (2024) examine how municipalities adjust their local business-tax rates in response to the introduction of a federal minimum tax rate. They show that while municipalities below the minimum are forced to raise their rates, municipalities above the threshold do not systematically adjust, which has implications for the intensity of tax competition.

Additionally, Buettner (2001) provides empirical evidence on spatial interactions in local business taxation within Germany, demonstrating that municipalities engage in strategic behavior concerning their business tax rates as they respond to competitors nearby.

Given these insights, it is essential to consider the influence of tax competition on the determination of local corporate tax rates and to treat LBT rates as endogenous. To address this endogeneity and to isolate the fiscal effect of municipalities purchasing locally to benefit from tax recapture, we employ a fiscal shock as an instrumental variable. Municipalities experiencing a negative fiscal shock are hypothesized to rely more heavily on the tax revenue, adjusting their LBT rates to mitigate fiscal constraints.

We rely on an exogenous fiscal revenue shock based on exogenous population changes due to a census survey, as first introduced by Serrato and Wingender (2016). They use changes in the census methodology as exogenous variation in the population numbers. Specifically, we use the special setting of the German census shock as used by Helm and Stuhler (2024). Municipalities receive payments from the fiscal equalization scheme, a system designed to balance fiscal disparities across municipalities. These payments are heavily influenced by municipal population numbers, as fiscal needs are assumed to increase with population size. Consequently, population size is the primary factor influencing the allocation formula. On average, transfers from the fiscal equalization scheme account for 14% of total municipal revenue, with some municipalities relying on these payments for up to 46% of their revenue (Bertelsmann Stiftung, 2024; Helm and Stuhler, 2024).

The census shock arises from discrepancies between the population projections for the year 2010 made from the 1978 census numbers and the actual population counts recorded in the 2011 census. These discrepancies led to substantial and instantaneous adjustments of population numbers between 2010 and 2011. These adjustments affected the payments from the equalization scheme, resulting in either increases or decreases in fiscal transfers to municipalities. Because these population projection errors are unrelated to municipalities' current procurement or tax policies and could not be anticipated before the reveal of census counts, the resulting fiscal shocks provide a source of exogenous variation.

$$censusshock_{i,2011} = population^{census}_{i,2011} - population^{projection}_{i,2010} \tag{2.3}$$

Equation 2.3 defines the census shock as the difference between the population recorded in the May 2011 census survey $population^{census}_{i,2011}$ and the projected population in December 2010 $population^{projection}_{i,2010}$. The December 2010 population projection is based on the 1978 census, adjusted for observed population flows between 1978 and 2010. However, over such a long period, measurement errors and missing data in the projections are unavoidable. The census survey in 2011 corrects the population numbers. The discrepancies between the projected and census-reported populations is the exogenous population change.

Importantly, municipalities were unaware of these adjustments before the census results were published, and the actual population at the time of the census was not influenced by changes in administrative population data. As a result, the census shock represents an exogenous change in the population numbers, which subsequently affects payments from the equalization scheme in the following year.

To make use of this exogenous variation, we perform a control function approach. First, we regress the payment from the equalization scheme on the population change from the census shock. We expect the municipalities with a negative population shock to receive less money from the equalization scheme in the year after the census shock. This step quantifies the mechanical reaction of payments from the fiscal equalization scheme to the exogenous population change of the census shock. This reaction is due to the calculation of fiscal needs, that are based on population numbers and determine the payments from the equalization scheme. This step ensures, that we only include the exogenous, census-induced variation in the equalization payments into the second stage regression. We are taking this initial step for two reasons: (1) First, using equalization payments directly as an instrument would not only capture the adjustments triggered by the census but also endogenous population changes occurring in the months after the survey (May to December) and other non-random factors affecting fiscal transfers. (2) Furthermore, if we directly used the census shock as the instrument, we would not only capture changes in the municipality revenue due to the census shock, but also other effects of the population change that influence the LBT rate decision. For example, municipalities experiencing population declines may respond by lowering LBT rates to attract businesses and counteract economic stagnation. Moreover, changes in population data might include demographic changes influencing broader policy choices, such as infrastructure investments or initiatives aimed at attracting younger population.

$$equalization\ payment_{it+1} = \beta_0 + \beta_1 census\ shock_{it} + \beta_2 I_i + \beta_3 I_t + e_{it} \tag{2.4}$$

With $equalization\ payment_{it+1}$ being the payment from the equalization scheme received by municipality i in year $t+1$. $census\ shock_{it}$ represents the population discrepancies caused by the census shock, as defined in equation 2.3. Since changes in fiscal equalization payments take effect only in the year following the census shock, we estimate their impact with a one-year lag.

In the first stage, we use the fitted values from equation 2.4 $equalizatio\hat{n}\ payment_{it}$ to estimate the impact of the census-induced fiscal shock on the local business tax rate (LBT_{it}).

$$LBT_{it} = \beta_0 + \beta_1 equalizatio\hat{n}\ payment_{it} + \beta_2 Population_{it} + \beta_3 I_i + \beta_4 I_t + u_{it} \tag{2.5}$$

Here, LBT_{it} is the local business tax rate shifting factor set by municipality i in year t, and $equalizatio\hat{n}\ payment_{it}$ are the fitted payments derived from equation 2.4. We assume that municipalities adjust their LBT rate in response to census shock-induced changes in fiscal equalization payments at the same time these payment changes take effect. Since municipalities are informed of their fiscal equalization payments for the upcoming year before setting the LBT rate at the end of the current year, their decision reflects anticipated revenue adjustments rather than a delayed reaction. We control for population, year and municipality fixed effects. This step captures the indirect effect of the census shock on LBT rate decisions through fiscal constraints. Buettner (2006) finds that the volume of payments received from the equalization payment scheme is inversely related to the tax rate. Therefore, we expect to find an increased LBT rate in response to an exogenous reduction in payments after the population corrections.

The second stage estimates the effect of LBT rates on procurement flows between municipalities, as shown in 2.6. To account for the high prevalence of zero flows and the skewed distribution of procurement contracts, we use Poisson Pseudo-Maximum Likelihood (PPML) regressions.

Following Wooldridge (2014) and Wooldridge (2010), a standard Two-Stage-Least-Squares (2SLS) estimation procedure does not provide consistent estimators when applied to a non-linear second stage. Since we use OLS for the first stage and PPML for the second stage, we employ the control function approach. This approach, while equivalent to 2SLS in the linear case, provides consistent estimators for the instrumented non-linear second stage. However, a key limitation is that there is no closed-form solution for the standard errors in this setup. To address this, we bootstrap the standard errors for all estimations incorporating the control function approach. Specifically, we perform 200 bootstrap replications at the origin-destination level, resampling unique pairs of procuring municipalities and winning firm municipalities with replacement. We then compute the standard errors from the distribution of coefficients across these 200 bootstrap replications.

$$\begin{aligned} X_{ijt} = \exp[\,&\beta_1 LBT_{it} \times Local_{ij} + \beta_2 LBT_{it} + \beta_3 Pop_{it} + \beta_4 Pop_{jt} \\ &+ \beta_5 \Delta LBT_{ijt} + \beta_6 u_{it} + \beta_7 I_i \times I_j + \beta_8 I_t\,] + v_{ijt} \end{aligned} \tag{2.6}$$

where X_{ijt} is the number of public procurement contracts awarded by authorities located in municipality i to firms located in municipality j in year t, and $local_{ij}$ is a dummy variable equal to 1 if $i = j$ (indicating local procurement) and 0 otherwise. $LBT_{it} \times Local_{ij}$ is the main regressor of interest indicating the additional effect of an increased LBT rate on the number of procurement contracts for local procurement, relative to non-local procurement. We also include the effect of LBT_{it} on non-local procurement separately, while $Local_{ij}$ alone is collinear with the fixed effects and therefore not included separately. The coefficient β_2 captures the baseline effect of the LBT rate on non-local procurement, while β_1 measures the additional effect for local procurement. The total effect of the LBT rate on local contracts is therefore given by $\beta_1+\beta_2$. u_{it} are the residuals from equation (2.5). Additionally, we control for the difference in the LBT rate to capture endogeneity from tax competition bilaterally through ΔLBT_{ijt}. We include bilateral fixed effects $I_i \times I_j$ and year fixed effects I_t.

Our instrumental variables approach relies on the relevance assumption, which implies that the exogenous population shock had an impact on the LBT rate. The first stage reveals a strong significance of the coefficient of the population-shock induced equalization payment change on the LBT rate and a high F-value, underlining the relevance of the census shock. Furthermore, there is a literature examining the effect of equalization payments on the LBT rate with Buettner (2006) finding an effect for the German equalization scheme, where municipalities' LBT rate decreases with increased payments from the municipal equalization scheme. Helm and Stuhler (2024) use the same exogenous variation and find decreasing LBT rates after positive population and therefore payment changes. However, in the latter case, municipalities adjust the rate slowly as a reaction to the persistent shock. As an additional assumption, we must not violate the exclusion restriction. While exogenous population changes and therefore revenue changes do affect total volumes and contract numbers in public procurement, the exclusion restriction is only violated if the fitted equalization payments directly predict the number of contracts in the bilateral gravity specification. Although it does not serve as a formal test for the exclusion restriction, Table B.4 in the Appendix provides results for the second stage gravity estimation with the added direct impact of the instrument. The findings indicate that the fitted equalization payments do not significantly predict the number of contracts, providing empirical support for the validity of the instrument.

As an additional estimation, we add origin-time and destination-time fixed-effects, as shown in Equation 2.7. These fixed effects account for time-varying characteristics of both the contracting municipality (destination) and the supplier municipality (origin). By doing so, we control for temporal changes in municipality-specific factors, such as economic conditions, administrative practices, and structural characteristics, that might otherwise influence procurement decisions. Furthermore, these additional fixed effects capture changes in the LBT rate in the buying municipality as well as in the municipality of the contract-serving firm.

$$X_{ijt} = \exp[\beta_1 LBT_{it} \times Local_{ij} + \beta_2 I_i \times I_j + \beta_3 I_{it} + \beta_4 I_{jt} + \beta_5 I_t] + v_{ijt} \tag{2.7}$$

It is important to note that the coefficient of interest, the interaction between the LBT rate and the local supplier dummy, remains identified. This identification is achieved through the combination of a variable with bilateral variation ($Local_{ij}$) with a variable with municipality-time variation (LBT_{it}). However, municipality-specific effects, as for example LBT_{it}, are collinear with the fixed-effects and are therefore omitted from this specification. Furthermore, this model does not allow to estimate the total effect of an increase in LBT_{it} on local contracts $\beta 1 + \beta 2$, but only the difference between the two groups $\beta 1$, the coefficient on $LBT_{it} \times Local_{ij}$.

2.6 Empirical Results

The baseline gravity estimates presented in Table 2.5 reveal that the interaction between the LBT rate and local procurement changes substantially across model specifications. In Column (1), the interaction coefficient is negative and statistically significant, suggesting that — conditional only on municipality fixed effects and year fixed effects — higher LBT rates are initially associated with fewer contracts awarded to firms located in the same municipality. This counterintuitive result may reflect unobserved heterogeneity or omitted variable bias related to stable, unobserved bilateral characteristics.

Table 2.5: Results of the baseline gravity estimation specification presented in equation 2.2

Dependent Variable:	Number of Contracts		
	PPML		
Model:	(1)	(2)	(3)
Variables			
LBT	-0.0035*	-0.0084***	
	(0.0018)	(0.0008)	
Local	9.351***		
	(0.4877)		
LBT × Local	-0.0109***	0.0216***	0.0255***
	(0.0011)	(0.0022)	(0.0019)
Fixed-effects			
Destination	Yes		Yes
Origin	Yes		Yes
Year	Yes	Yes	Yes
Origin-Destination		Yes	Yes
Origin-Year			Yes
Destination-Year			Yes
Fit statistics			
Observations	106,377,780	478,040	305,489
Squared Correlation	0.75718	0.81315	0.94773
Pseudo R^2	0.51532	0.42233	0.53414
BIC	1,190,468.0	1,001,902.0	1,327,881.0

** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

Column (2) introduces origin-destination fixed effects, which absorb all time-invariant characteristics specific to each pair of municipalities, e.g., bilateral distances. Once these bilateral fixed effects are included, the sign of the interaction coefficient turns positive and remains highly significant. This change indicates that the initial negative association in Column (1) was likely confounded by persistent differences between municipality pairs. For instance, some municipalities may have both high LBT rates and structural disadvantages in attracting local firms, biasing the naive interaction downward.

Column (3) further includes time-varying fixed effects for both the procuring and winning municipalities, as shown in 2.7. These control for evolving local factors such as economic development, administrative changes, or political cycles. Importantly, the interaction between LBT and local procurement remains positive and statistically significant also in this very restrictive specification.

Table 2.6 presents the results of the control function approach, including fixed effects and control variables such as origin-destination fixed effects and origin population, which account for time-variant changes in the size of the local economy.

Table 2.6: Regression Results for the three stages of the control function approach

Dependent Variables:	Equalisation Payment	LBT	Number of Contracts
Model:	(1)	(2)	(3)
	OLS	OLS	PPML
Variables			
Census Shock	10,174.8***		
	(144.6)		
Equalization Payment (fitted)		-1.71×10^{-9}**	
		(7.86×10^{-10})	
LBT			-0.0052***
			(0.0012)
Residuals (u_{it})			-0.0050***
			(0.0012)
LBT Difference			0.0024***
			(0.0000)
LBT × Local			0.0241***
			(0.0001)
Fixed-effects			
Destination	Yes	Yes	Yes
Origin	Yes	Yes	Yes
Origin-Destination	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	310,353	310,353	310,353
Squared Correlation	0.97231	0.96099	0.79160
Pseudo R^2	0.08253	0.30224	0.42467
BIC	12,785,586.5	2,735,575.0	757,258.6

Joint F-test for LBT and LBT × Local for the total effect: $F(2, 281821) = 83.08$, $p < 0.001$***.

Clustered (Procuring Municipality-Winning Firm Municipality) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

Column (1) examines the impact of the census shock on equalization payments. The results confirm that a decrease of 1,000 inhabitants due to the census adjustment is associated with a €10,174 decrease in payments from the equalization scheme in the subsequent year. This finding highlights the strong dependence of fiscal transfers on population estimates. Municipalities experiencing a downward correction in population are shown to face tighter budget constraints.

Column (2) explores the relationship between fitted equalization payments and LBT rates. Using the fitted values from the equalization payments from column (1), we find that a reduction in payments from the equalization due to the exogenous population number adjustment scheme leads to a small but statistically significant increase in the LBT rate. This result suggests that municipalities facing exogenous fiscal shortfalls adjust their tax policy to compensate for the loss of financial resources, consistent with the hypothesis that tighter budgets incentivize the municipalities to increase their LBT rates. This result is in line with previous studies, as e.g., Buettner (2006).

Column (3) investigates the effect of LBT rates on public procurement flows. The coefficient on the interaction term $LBT_{it} \times Local_{ij}$ measures the effect of a change in the LBT rate shifting factor on local contracts relative to non-local contracts. In other words, it shows how a change in LBT_{it} affects the number of contracts awarded to firms located in the same municipality, relative to municipalities located elsewhere; it does not by itself equal the total effect of LBT_{it} on the raw number of local contracts.

In our baseline specification, the estimated interaction coefficient is $\hat{\beta}_1 = 0.0241$ (Column (3)). Because the model is estimated by PPML, the percentage change in expected contract counts associated with a change Δ in the scaling factor is computed exactly as

$$\%\,\Delta X \;=\; \big(\exp(\hat{\beta}_{int} \cdot \Delta) - 1\big) \times 100.$$

Thus a one percentage-point increase in the LBT scaling factor implies a $(\exp(0.0241) - 1) \times 100 \approx 2.44\%$ larger number of local contracts relative to non-local contracts. For the most common change of $\Delta = 5$ percentage points, the implied relative increase is

$$(\exp(0.0241 \times 5) - 1) \times 100 \approx 12.81\%.$$

This result supports the home-bias mechanism: higher LBT rates are associated with relatively more procurement contracts awarded to local firms, consistent with municipalities favoring local suppliers to recapture tax revenues when fiscal constraints bind.

To illustrate the effect in terms of total tax rates, consider a representative municipality with an LBT scaling factor of 350% applied to a base rate of 3.5%, which yields a total tax rate of $3.5\% \times 3.5 = 12.25\%$. A typical 5 percentage-point increase in the scaling factor raises the total tax rate to $3.5\% \times 3.55 = 12.425\%$, i.e. a $12.425/12.25 - 1 \approx 1.43\%$ increase in the total tax rate. Combining this with the relative effect above implies that a 1% increase in the total tax rate (derived) is associated with approximately $12.81/1.43 \approx 8.96\%$ more local contracts relative to non-local contracts.

For completeness, one can also consider the total effect of an LBT increase on local versus non-local contracts. Non-local contracts respond only to the main LBT coefficient $\hat{\beta}_2 = -0.0052$, implying that a one percentage-point increase in the LBT scaling factor reduces non-local contracts by about 0.5% (and by 2.6% for a typical 5 percentage-point increase). Local contracts respond to the sum $\hat{\beta}_2 + \hat{\beta}_1 = 0.0189$, corresponding to an increase of about 1.9% per percentage point (or 9.8% for 5 percentage points). Hence the interaction coefficient $\hat{\beta}_1 = 0.0241$ captures the additional relative response, i.e., that local contracts grow about 2.4% faster than non-local contracts per percentage-point increase in the LBT rate shifting factor (or 12.8% faster for 5 percentage-points).

To identify which contracts are driving the results, we categorize them into three distinct types: service, supply, and works contracts. These contract categories differ considerably in terms of trade costs and the degree of specialization required, which influences firms' ability to compete across distances.

Works contracts, which encompass construction and infrastructure projects, typically entail the highest trade costs. These contracts often require specialized labor on-site, along with the transportation of heavy materials and adherence to regulatory constraints. As a result, works contracts are generally less flexible in terms of geographic reach.

Supply contracts, involving the procurement of goods, incur moderate trade costs. While many goods can be transported relatively easily, logistical challenges, delivery reliability, and transport costs still play crucial roles in determining competitiveness. Goods in supply contracts tend to be more standardized, which allows contracting authorities to choose from a broader pool of suppliers.

Service contracts, such as consulting, IT, and maintenance services, generally involve the lowest trade costs. These services can often be provided remotely or with minimal physical presence, making them less sensitive to geographic constraints. The flexibility in delivery allows firms to compete over longer distances.

Given these differences, the effects of localization may vary across contract types. Table 2.7 presents the results of the final stage for each category.

Table 2.7: Estimation Results for contracts from the different categories Services, Supply, and Works

Dependent Variable:	Number of Contracts		
	Services	Supply	Works
Model:	(1)	(2)	(3)
Variables			
LBT	-0.0085**	-0.0050*	-0.0010
	(0.0032)	(0.0027)	(0.0014)
Residuals (u_{it})	-0.0060*	-0.0018	-0.0067***
	(0.0032)	(0.0027)	(0.0014)
LBT Difference	0.0042***	0.0022*	0.0009***
	(0.0001)	(0.0001)	(0.0000)
LBT × Local	0.0231***	0.0307***	0.0161***
	(0.0002)	(0.0002)	(0.0002)
Fixed-effects			
Origin-Destination	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	97,867	81,755	181,416
Squared Correlation	0.73982	0.46013	0.54333
Pseudo R^2	0.45901	0.31376	0.22608
BIC	238,541.1	174,446.9	362,586.1
F-Test LBT and LBT × Local	35.16***	43.00***	12.80***

Clustered (Procuring Municipality-Winning Firm Municipality) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

The results, broken down by contract type, reveal heterogeneity in how LBT rates interact with local procurement decisions across services, supply, and works contracts. The interaction term $LBT_{it} \times Local_{ij}$

is positive and statistically significant for all contract types, suggesting that as LBT rates increase, municipalities allocate relatively more contracts to local firms compared to non-local firms. However, the magnitude of the coefficients varies, reflecting differences in trade costs, the degree of specialization required for each contract type, and the role of tax recapture incentives in procurement decisions.

Supply contracts exhibit the largest coefficient, implying that municipalities are particularly inclined to prioritize local procurement when tax recapture benefits are higher. The relatively standardized nature of many supply contracts facilitates local sourcing. In markets where goods are commoditized and regulation is stringent, municipalities may have greater leeway to favor local suppliers without incurring substantial additional costs. This makes local procurement a more attractive option when the tax incentives are strong.

In contrast, service contracts and works contracts show smaller, though still significant, coefficients. This suggests that while tax recapture incentives remain relevant, other factors tend to moderate their impact.

For service contracts, which typically involve specialized labor or intangible goods such as consulting or IT services, the relatively low trade costs make it easier for firms outside the municipality to compete. Despite the potential tax incentives, high skill requirements and the importance of firm reputation can make it more challenging to find a suitable local supplier. Therefore, while municipalities may still be influenced by tax recapture incentives, the need for specialized expertise or well-established service providers often limits the extent to which local procurement can be prioritized.

For works contracts, which include construction and infrastructure projects, the highest trade costs arise from on-site labor requirements, the transportation of heavy materials, and the need to comply with complex regulatory frameworks. The interaction term remains significant but relatively smaller, suggesting that, even with tax recapture incentives, municipalities often face the necessity of contracting firms with specific expertise or capacity that may not be locally available. In such cases, municipalities may prioritize local procurement to a lesser extent, as the specialized nature of the work and the logistical constraints take precedence over tax incentives.

To explore potential heterogeneity in the effects based on the size of municipalities, we split the analysis by procurement activity of municipalities, according to the total number of contracts awarded. This approach allows us to examine whether the observed relationships vary systematically with the procurement activity or economic scale of municipalities. The results of this split analysis are presented in Table 2.8.

Table 2.8: Main Results of the third stage split by size of the municipality

Dependent Variable:	Number of Contracts		
	All	Small	Large
Model:	(1)	(2)	(3)
Variables			
LBT	-0.0052***	-0.0050*	-0.0058***
	(0.0012)	(0.0027)	(0.0017)
Residuals	-0.0050***	-0.0020	-0.0067***
	(0.0012)	(0.0027)	(0.0017)
LBT Difference	0.0024***	0.0030***	0.0022***
	(0.0000)	(0.0001)	(0.0001)
LBT × Local	0.0241***	0.0243***	0.0235***
	(0.0001)	(0.0002)	(0.0002)
Fixed-effects			
Origin-Destination	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	310,353	154,735	155,024
Squared Correlation	0.79160	0.17577	0.83152
Pseudo R^2	0.42467	0.17929	0.51244
BIC	757,258.6	316,446.0	368,402.4
F-Test LBT and LBT × Local	83.08***	43.02***	5.48***

Clustered (Procuring Municipality-Winning Firm Municipality) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

The results in Table 2.8 show that the interaction effect between the LBT rate and the local procurement indicator is positive and statistically significant across all municipality size categories. Notably, the magnitude of the effect is very similar for both small and large municipalities. This indicates that municipalities of different sizes alike are more inclined to procure locally as LBT rates increase.

For larger municipalities, the significant interaction effect may reflect their stronger administrative capacity and more developed procurement systems, which can facilitate the strategic alignment of procurement choices with fiscal considerations. Their access to a broad supplier base may further enhance their ability to leverage local procurement in response to tax incentives.

Smaller municipalities also exhibit a positive and significant interaction effect, suggesting that they, too, respond to LBT incentives when choosing local suppliers. Although the effect is not markedly larger than in larger municipalities, it still indicates that local procurement can serve as an important fiscal mechanism even for smaller administrations, particularly when procurement choices are feasible and competitive local firms are available. Without restricting the sample to contracts where local or regional procurement is feasible, as described in Section 5, the results for smaller municipalities are not significant. After this adjustment, the results indicate that even smaller municipalities actively utilize local procurement as a fiscal tool when the conditions permit. The stark difference underscores the importance of controlling for potential procurement choices to address endogeneity concerns related to municipality size.

To enhance the robustness of the baseline analysis and better address potential confounding factors, we incorporate origin-time and destination-time fixed effects as shown in Equation 2.7.

Table 2.9: Specification with additional origin-time and destination-time fixed effects

Dependent Variable:	Number of Contracts		
	PPML		
Model:	(All)	(Large)	(Small)
Variables			
LBT × Local	0.0184***	0.0193***	0.0172**
	(0.0021)	(0.0031)	(0.0074)
Fixed-effects			
Origin-Destination	Yes	Yes	Yes
Origin-Year	Yes	Yes	Yes
Destination-Year	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	65,689	32,835	32,590
Squared Correlation	0.98584	0.98742	0.99573
Pseudo R^2	0.52593	0.63293	0.27146
BIC	953,627.9	433,029.7	549,744.6

Clustered (Procuring Municipality-Winning Firm Municipality) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

Table 2.9 presents the results from the more restrictive specification with origin-time and destination-time fixed effects. These fixed effects absorb any time-varying municipality characteristics, which implies that the main effect of the LBT rate cannot be separately identified in this specification. The remaining coefficient of interest is therefore the interaction term $LBT_{it} \times Local_{ij}$, which captures the differential response of local procurement relative to non-local procurement. Consistent with the baseline specification in Table 2.6, the interaction term remains positive and statistically significant across all municipality size groups, although being slightly smaller than in the baseline estimation.

2.7 Robustness

The results presented in Section 6 are computed using the restricted sample to reflect only contracts, where the contracting authority could plausibly buy from a local and a regional supplier. To further examine the robustness to different restrictions of contracts, we run the regressions with sample restrictions based on different criteria. Table 2.10 examines the relationship between LBT rates and the number of contracts awarded to local firms, presenting three different ways to restrict the sample: (1) contracts within CPV categories where at least one firm located in the contracting municipality has been active ("Local CPVs"),

(2) contracts in categories with at least one active firm located in the contracting municipality and at least one firm in one of its neighboring municipalities ("Local & Contig. CPVs"), and (3) contracts in categories with at least one active firm located in the contracting municipality and at least one firm in one of its neighboring municipalities or within a 50 kilometer radius around the contracting municipality ("Local & Regional (50km) CPVs").

Table 2.10: Robustness: Different restrictions of contracts

Dependent Variable:	Number of Contracts		
	Local CPVs	Local & Contig. CPVs	Local & Regional (50km) CPVs
Model:	(1)	(2)	(3)
Variables			
LBT	-0.0050***	-0.0038**	-0.0052***
	(0.0015)	(0.0018)	(0.0012)
Residuals (u_{it})	-0.0047***	-0.0001	-0.0050***
	(0.0015)	(0.0018)	(0.0012)
LBT Difference	0.0023***	-0.0002*	0.0024***
	(0.0000)	(0.0001)	(0.0000)
LBT × Local	0.0230***	0.0134***	0.0241***
	(0.0001)	(0.0003)	(0.0001)
Fixed-effects			
Origin-Destination	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	365,826	50,979	310,353
Squared Correlation	0.78627	0.85707	0.79160
Pseudo R^2	0.41570	0.44089	0.42467
BIC	781,271.6	143,597.7	757,258.6
F-Test LBT and LBT × Local	71.56***	3.85**	83.08***

Clustered (Procuring Municipality-Winning Firm Municipality) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

The coefficient on the interaction term $LBT_{it} \times Local_{ij}$ is positive and significant across all columns, suggesting that an increase in the local business tax rate is associated with an increased propensity to award contracts to firms in the own municipality relative to non-local firms. This means that the baseline results from table 2.6, here reflected in Column 3, still hold, also when we change the methodological details of how we restrict the contracts. Specifically, the magnitude of the coefficient is driven by the existence of a potential local supplier, as we find a magnitude similar to our baseline in Column 1. It declines when we restrict the contracts to include only contracts with potential local and contiguous suppliers. This difference is driven by a lack of potential suppliers in the contiguous municipalities. Once we restrict to include additionally contracts with a regional supplier, the effect is similar to Column 1. This pattern is consistent with the hypothesis that higher LBT rates incentivize municipalities to favor local suppliers more strongly, but this effect depends on the actual possibility to decide between a local and a regional supplier.

We conduct a placebo test to examine whether the observed relationship between LBT rates and public procurement decisions is genuinely driven by the hypothesized tax cashback mechanism. In this test, we replace the local dummy variable with a placebo indicator equal to 1 if the firm awarded the tender is located near the procuring municipality i, but outside its borders.

In Column 1 of Table 2.11, "near" is defined as the neighboring municipalities of the procuring municipality. Columns 2 and 3 expand this definition to include municipalities within a 10 kilometer and 50 kilometer radius, respectively. The underlying tax cashback mechanism depends on a sharp discontinuity at the municipal border: Municipalities only benefit from tax revenues if the firm is located within their jurisdiction. Consequently, firms located just beyond the municipal boundary, even if geographically close, do not contribute to the local business tax revenues of the procuring municipality.

Based on this, we expect no positive interaction between the LBT rate and the placebo dummy. Specifically, procuring municipalities with higher LBT rates have no fiscal incentive to favor firms in neighboring municipalities since the resulting business tax revenues accrue elsewhere. Thus, higher LBT rates should not lead to an increased likelihood of awarding contracts to firms located outside the municipal boundary, even in contiguous or nearby municipalities.

The results of the placebo test, presented in Table 2.11, align with our identification logic. For all three placebo definitions (neighbors, within 10km, within 50km), the interaction coefficient $LBT_{it} \times Placebo_{ij}$ is negative and statistically significant. This indicates that, as LBT rises, contracts to geographically proximate but non-local firms decline relative to other non-local contracts. Read together with the positive $LBT_{it} \times Local_{ij}$ in the main specification, this pattern is consistent with a reallocation of awards from nearby non-local firms toward local firms—i.e., a tax–recapture (home-bias) mechanism that operates at municipal borders rather than in broader neighboring areas. These placebo results remain robust when adding origin–year and destination–year fixed effects, as presented in table 2.12.

Table 2.11: Placebo test: Local dummy is replaced by a placebo dummy, that is equal to 1, if the origin municipality is close to the destination dummy.

Dependent Variable:	Number of Contracts		
	Neighbours	10km	50km
Model:	(1)	(2)	(3)
Variables			
LBT	0.0016	0.0018	0.0029**
	(0.0012)	(0.0012)	(0.0012)
Residuals (u_{it})	-0.0049***	-0.0049***	-0.0049***
	(0.0012)	(0.0012)	(0.0012)
LBT Difference	-0.0004***	-0.0004***	-0.0003***
	(0.0000)	(0.0000)	(0.0000)
LBT × Placebo	-0.0035***	-0.0028***	-0.0042***
	(0.0001)	(0.0001)	(0.0001)
Fixed-effects			
Origin-Destination	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	310,353	310,353	310,353
Squared Correlation	0.78729	0.78742	0.78795
Pseudo R^2	0.42310	0.42311	0.42317
BIC	707,568.0	707,564.7	707,528.5
F-Test LBT and LBT × Local	1.54	1.29	6.66***

Clustered (Origin-Destination) bootstrapped standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

Table 2.12 presents the results of the placebo test using a more restrictive fixed-effects specification, as in equation 2.7. Across all three distance thresholds, the interaction term between the LBT rate and the placebo indicator remains negative. While the effect is not statistically significant for neighboring municipalities, it becomes significant at the 10 km radius and at the 50 km radius. This reinforces the idea that the tax cashback mechanism operates strictly within municipal boundaries. The inclusion of more restrictive fixed effects ensures that the observed patterns are not driven by time-invariant characteristics at the municipality level, further strengthening our identification strategy.

Table 2.12: Placebo test using Regional but not Local instead of Local municipalities

Dependent Variable:	Number of Contracts		
	Neighbors	10km	50km
Model:	(1)	(2)	(3)
Variables			
LBT × Placebo	-0.0006	-0.0045*	-0.0075***
	(0.0035)	(0.0026)	(0.0017)
Fixed-effects			
Origin-Destination	Yes	Yes	Yes
Origin-Year	Yes	Yes	Yes
Destination-Year	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	65,503	65,503	65,503
Squared Correlation	0.98515	0.98518	0.98533
Pseudo R^2	0.52601	0.52603	0.52610
BIC	950,948.1	950,942.6	950,919.6

Clustered (Origin-Destination) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

2.8 Conclusion

We investigate a mechanism that incentivizes municipalities to procure locally: the local business tax rate. Municipalities with tight budget constraints can partially refinance their public investments through the tax revenues generated by awarding procurement contracts to firms within their jurisdiction. To address the endogeneity of LBT rate decisions, we leverage the exogenous population corrections introduced by the 2011 census. These corrections triggered changes in intergovernmental transfers, creating budgetary pressures that influenced municipalities' LBT rate adjustments.

Our empirical findings indicate that a 1 percentage-point increase in the LBT rate shifting factor leads to a 2.4% higher share of contracts awarded to firms in the same municipality, relative to non-local firms. This differential effect suggests that fiscal incentives linked to local tax revenues shape procurement decisions in favor of local suppliers, reinforcing the localized nature of public procurement.

In contrast to much of the literature that examines home bias at the national and regional level, we focus on the local level to highlight regional dynamics that are often overlooked. This approach provides new insights into how localized fiscal policies within integrated markets, such as municipal tax rates, shape procurement behavior and contribute to home bias in ways that may differ from national behavior.

This research contributes to understanding why public procurement remains localized despite initiatives such as the EU's efforts to foster an integrated market with open tenders. It highlights a structural challenge: fiscal incentives at the municipal level can undermine policy goals aimed at enhancing cross-border competition and market integration. More broadly, the findings speak to debates in fiscal federalism about the trade-off between local autonomy and higher-level policy coordination. While allowing municipalities to set their own tax rates can improve responsiveness to local economic needs, it may also create counterproductive incentives that impede integration and efficiency. In this case, municipal tax policy functions as a barrier to the EU's objective of a unified procurement market and may induce procurement decisions that are not cost-minimizing.

While our findings suggest that municipalities with higher LBT rates are more likely to procure locally to maximize the tax recapture effect, alternative explanations may also play a role in this pattern. One such mechanism is the compensation effect, where municipalities increase LBT rates but seek to offset the burden on local businesses by favoring them in procurement decisions. In this case, local procurement might function as a form of indirect compensation rather than as a strategic response to tax incentives. Another potential explanation lies in political economy considerations, where local decision-makers prioritize local firms for reasons beyond fiscal optimization. Foremny and Riedel (2014) show that business tax increases are reduced before the election and increased afterwards. However, policymakers may use awarding contracts to local firms as an alternative means of boosting their electoral support despite increasing taxes or after a revenue cut due to the census shock. Additionally, local procurement could be seen as a way to address unemployment by supporting the local economy. While our study emphasizes the fiscal dimension of these mechanisms, future research could explore and disentangle these politically motivated channels to better understand the broader factors influencing local procurement decisions.

Future research could further explore the broader implications of this mechanism, including its impact on market efficiency, competition, and the economic development of municipalities. Understanding these dynamics is essential for designing policies that balance the autonomy of local governments with the goals of larger economic unions and integrated markets.

CHAPTER 3

Subsidizing R&D in Germany: Direct and Spillover Effects

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Chapter Abstract

We estimate the effects of R&D funding on firm performance and innovation activity in Germany for the period 2000-2020. Since R&D collaborations between funded and non-funded firms can lead to positive externalities, our study accounts not only for the direct effects of the policy on funded firms but also for indirect (spillover) effects on non-funded firms. We use multiple databases (DAFNE, PATSTAT, Förderkatalog) and conduct empirical estimations on both firm and cluster levels. Our findings suggest that, in general, R&D funding has a positive impact on both funded and non-funded firms, which varies with the share of funded firms in the cluster. Interestingly, the indirect impact on innovations is larger than the direct impact. The latter finding highlights the benefits of this broad form of R&D funding (without sectoral or regional concentration) in fostering innovation spillovers, but also its limitation in generating stronger direct effects.

3.1 Introduction

Recently, considerable effort has been made for the development of R&D and innovation policies worldwide. This growing trend is justified by the positive effects of private sector R&D on sustainable growth (Aghion and Howitt, 2009; Doraszelski and Jaumandreu, 2013; Griliches, 1979; Grossman and Helpman, 1994), innovation (increased number of patent applications and new products) (Czarnitzki and Delanote, 2015; Czarnitzki and Hottenrott, 2011; Czarnitzki and Licht, 2006; David et al., 2000; Howell, 2017), and additional R&D expenses (Aerts and Schmidt, 2008; Almus and Czarnitzki, 2003; Czarnitzki and Fier, 2002; Czarnitzki and Hussinger, 2004; Czarnitzki et al., 2007; Hussinger, 2008). However, R&D funding can also lead to the crowding out of private investments by public funding (Montmartin and Herrera, 2015; Wallsten, 2000). R&D funding can affect firms from two perspectives: it will either stimulate firms to increase innovation activity due to the enhanced capability to afford innovation costs or it will give them a possibility to use the R&D subsidy for their own funding because they would have implemented their R&D projects anyway.

In this paper, we want to contribute to the existing debate on the effects of R&D funding. We study the causal impact of R&D funding on the performance and innovation outcomes of German firms over the period of 2000-2020. The main research questions are (1) whether R&D funding improves firm performance and innovative output and (2) whether there are any spillovers for non-funded firms due to the presence of funded firms. The latter question is inspired by the work of earlier scholars, who claim that the presence of FDI firms (Girma et al., 2015) can generate positive externalities via collaboration.

For the first time, we integrate multiple datasets by leveraging the comprehensive German Förderkatalog dataset (includes information on all R&D projects funded in Germany) and linking it with the DAFNE database and the EPO Worldwide Patent Statistical Database (PATSTAT). Our empirical strategy includes several steps. First of all, to address the selection problem implying that the firms with certain characteristics were selected for the funding, we implement matching on several covariates (fixed assets, number of employees, firm age, and industry dummy) on the firm as well as the cluster level. We define as clusters unique industry-region combinations (by using German NUTS1 regions (16 Bundesländer) and NACE1 industry classification (9 broad industries)) resulting in 144 unique clusters. Then, we perform firm- and cluster-level estimations to obtain the direct, indirect, and total effects of funding for different proportions of funded firms in the cluster following Girma et al. (2015).

Although there is a broader literature on R&D policy evaluations, most of the papers analyzing the effect of R&D subsidies either focus on the general effect of funding on the funded firms or collaboration and spillover effects without disentangling the direct from the indirect effects. With this paper, we want to add more insights to the existing literature by making a clear distinction between direct and indirect (spillover) effects of R&D funding on the firms' financial and innovation outcomes, and understanding what drives the overall effect. To the best of our knowledge, only the study by Czarnitzki et al. (2007) looks at both effects by regressing innovation activity on collaboration and separately on R&D funding. Their results suggest that there is no direct effect of R&D funding on private R&D. In contrast, they find that collaboration increases the firms' R&D activity and might generate positive spillovers for other non-funded firms, which drives the overall positive effects of R&D funding on innovation. However, the latter study does not explore how the effect of R&D varies when we have different shares of treated firms in the cluster. Our key contribution lies in using the share of funded firms as a mechanism to identify spillovers, thereby uncovering insights into the optimal share of funded actors and relevant thresholds to enhance spillover effects.

The remainder of the paper is structured as follows. In the following section, we present the existing literature. Section 3.3 presents the data and the main descriptive statistics. The empirical estimation strategy is explained in Section 3.4. The main findings are discussed in Section 3.5. Finally, Section 3.6 concludes.

3.2 Literature Review

Traditionally, economists ascribe economic growth to technological progress, which can be stimulated via research and development (R&D) funding (Romer, 1990). The latter one is very important, because it develops a systematic approach towards innovation and stimulates the diffusion of knowledge and interactive learning among different public and private actors (Clausen, 2009).

Another justification for public funding of R&D is the existence of two types of market failures. The first one is related to the classical market failure argument by Arrow (1962) which states that firms have fewer incentives to invest in R&D. The main reason is the existence of knowledge externalities and incomplete appropriability, which makes it difficult to claim the ownership rights and allows free-riding by competitors. The second type of market failure implies that due to very high costs of external capital, firms do not have sufficient incentives to invest in R&D from the societal point of view, thus some innovations will not be developed. This problem is particularly severe for small, young, and cash-limited firms (Hall, 2002a, 2002b).

To fill this gap between private and social return rates to R&D investments, governments design different R&D initiatives, which are usually offering either direct funding or a tax incentive (Hall, 2002a, 2002b) by stimulating private R&D investments (Jaffe, 2002). Existing research analyzes the impact of R&D funding from two different perspectives by concentrating on the direct and spillover effects. The overwhelming majority of studies tackling the direct effects of R&D policies focus on input additionally. Some of them claim that R&D funding increases R&D expenditures of funded firms (Almus and Czarnitzki, 2003; Czarnitzki and Fier, 2002; Czarnitzki and Licht, 2006; Duguet, 2004; González et al., 2005; Hud and Hussinger, 2015; Hussinger, 2008; Li and Bosworth, 2018) by supporting the R&D complementarity hypothesis, while the others argue that it leads to the crowding out of R&D spending of funded firms (Montmartin and Herrera, 2015; Wallsten, 2000). Another strain of research concentrates on output additionality and provides positive evidence of R&D subsidies on innovation (Aschhoff, 2009; Bronzini and Piselli, 2016; Czarnitzki and Fier, 2002; Czarnitzki and Hussinger, 2004; Czarnitzki and Licht, 2006; Czarnitzki and Hussinger, 2018; Czarnitzki and Lopes Bento, 2012; Kleine et al., 2022) and collaboration (Czarnitzki et al., 2007; Kleine et al., 2022). Particularly, the importance of R&D collaboration raised tremendously, which is seen by the firms as a possibility to access new resources, and knowledge (Badillo and Moreno, 2015; Kaiser, 2002; Li and Bosworth, 2018), and share the risks and costs of R&D projects (Antonioli et al., 2016; Belderbos et al., 2004) by leading to the improvement of firm-level outcomes (Falk, 2010; Hall et al., 2009; Lerner, 1999).

However, R&D collaborations do not necessarily occur between the firms funded within the same scope of certain R&D policies but can be established between different non-funded actors by thus generating positive spillovers and indirect effects of the funding for non-funded firms. This brings us to the second strain of recent literature concentrating on the spillover effects of R&D policies.

In general, inter-firm spillovers are the result of knowledge exchange between scientists (Jaffe, 1986). Due to the limited mobility of scientists (Breschi and Lissoni, 2009), knowledge exchange is more likely to be stimulated and successfully transferred at a close distance (Audretsch and Feldman, 1996; Jong and Freel, 2010; Spithoven et al., 2019) by generating geographical spillovers. The latter is the most studied direction in the spillover literature and is found to lead to positive externalities (Acemoglu et al., 2016; Breschi and Lissoni, 2005; Petruzzelli, 2011). For instance, Petruzzelli (2011) asserts that R&D collaboration at a close geographic distance leads to more innovation.

However, proximity is not only constrained to a spatial context. There is a higher probability of knowledge exchange not only in geographic proximity but also in the same (Bernstein and Nadiri, 1989) or more related industries (Jaffe et al., 1993). Bloom et al. (2013) support the idea that technological connectedness leads to more positive spillovers and higher social returns of R&D. They also add that the impact is lower for small firms, which is the reason for their operation in "technological niches" and fewer linkages to other firms in technology space. Further, Li and Bosworth (2018) verifies in the study of the British firms, that own-sector R&D spillovers have the largest and most significant effect on labor productivity.

Building on these concepts, additional studies suggest that agglomeration economies (such as region-industry targeted clusters) can be more advantageous for the growth of innovative firms due to economies of scale and spillover effects (Akcigit et al., 2018; Breschi and Lissoni, 2009; Crass et al., 2017; Engel et al., 2017; Nishimura and Okamuro, 2010; Uyarra and Ramlogan, 2016).

Finally, the study by Czarnitzki et al. (2007) looks both at the direct and indirect (spillover) effects of R&D funding. They argue that R&D collaborations have a more pronounced impact on firms' R&D activity through positive spillovers than direct funding itself.

3.3 Data

Our final dataset results from linking three types of data sources on the firm level. Firm financial information is taken from DAFNE database (extracted in September 2020). It is geo-referenced data, which provides information on firm location, sector, financial outcomes (e.g., fixed assets, employment costs, sales, revenue, etc.), legal entity details, corporate structures, ownership, etc. In our analysis, we concentrate on the sub-sample of German firms (around 2 million firms) over the period of 2000-2020.

DAFNE data is linked to the EPO Worldwide Patent Statistical Database (PATSTAT), which contains detailed information on all patented innovations including the names and addresses of applicants and inventors, priority year, patent families, the title and abstract of patent applications, citation links, and patents classification by technology class. We restrict PATSTAT data also to the patent applications by German firms.

Finally, the information on the direct R&D funding of firms by the German Federal Government is taken from the Förderkatalog (funding catalog) database, which is jointly created by the Federal Ministry of Education and Research (BMBF) and the Federal Ministry of Economics and Technology (BMWI). The Förderkatalog is comprehensive, unique data that contains detailed information (i.e., funding type, amount, duration, area, recipient groups, responsible institutions/federal states/countries) on 90,000 individual R&D projects that have been funded since 1970. The growing number of funding recipients from 1970 to 2020 led to 28,013 firms with a total budget of 44 bn € (BMBF (2023)). It's noteworthy that this extensive dataset, encompassing all R&D-funded projects in Germany, has not been previously utilized.

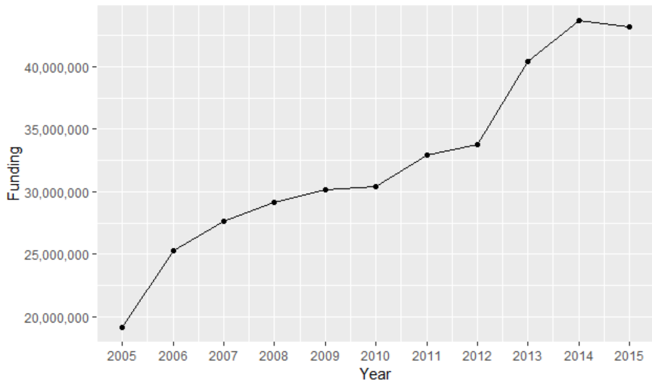
Dependent variables: Firm-level outcomes are divided into the firm's fixed assets and employment (number of employees). Assessing innovation outcomes often relies on measuring patents, but this poses challenges as patents vary in their commercial value and technological impact. Research suggests that while most patents hold little value, a select few represent significant technological breakthroughs (Gambardella et al., 2008). When explicit value information is lacking, citation-based measures offer the best approximation of patent quality (Gambardella et al., 2008; Reitzig, 2004). Patents that are frequently cited by subsequent patents are generally considered higher quality, provided that differences in citation propensity are controlled for (Harhoff et al., 1999; Jaffe and De Rassenfosse, 2019; Trajtenberg, 1990).

However, comparing citation counts across countries has limitations. For one, applicants tend to protect more valuable patents abroad (due to the elevated expenses associated with filing and translating patents), making direct comparisons between domestic and foreign applications less informative (Harhoff et al., 2003). Additionally, variations in examination practices across national patent offices result in significant differences in citation counts. Furthermore, patent examiners commonly demonstrate a tendency to favor citing patents from their native country (Michel and Bettels, 2001). To address these challenges, we adopt a patent quality measure proposed by Boeing and Mueller (2016) that enables technology-specific cross-country comparisons and incorporates forward citations. This measure accounts for factors such as the number of technology classes, family size (number of countries in which a patent application was filed), and the number of forward citations received by the patent.

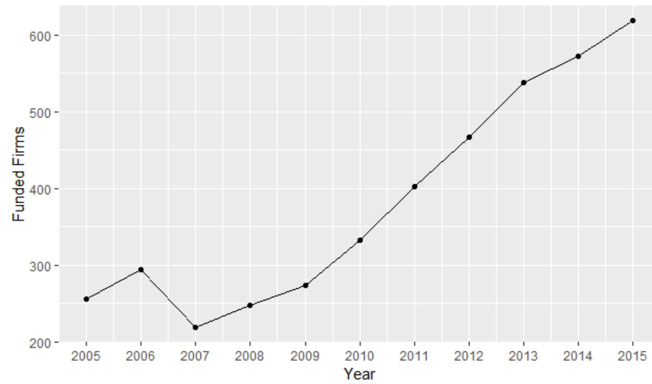
Figure 3.1 shows summarising graphs for funding. Both funding (a) as well as funding recipients[1] (b) increased over the years of observation. The funding programs were widened and R&D funding increased in general. Graph (c) shows the decrease in the share of funding recipients/funded firms from 2005 to 2007 due to a slight decrease in funding recipients and an increase in the overall total number of firms.

Table 3.1 presents summary statistics for variables of interest categorized by R&D funding status. The first column illustrates the average outcomes of funded firms, the second column - the average outcomes of non-funded firms, and the third column - the average overall outcome from 1970 to 2020. Firm-level metrics encompass firm age (computed as the difference between the current year and the year of in-

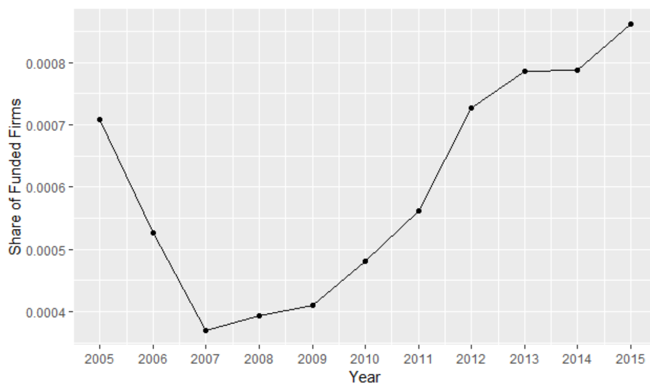
[1]Funding recipient means the firm that received the funding.



(a) Funding increase over time



(b) Funding recipients increase over time



(c) Share of funded firms over time

Figure 3.1: Descriptive statistics for funding and patents

Table 3.1: Summary statistics on firm-level characteristics

	Funded (N=207164)	Non-Funded (N=12498684)	Overall (N=12705848)
Firm age	27.2 (31.6)	17.7 (22.3)	17.9 (22.5)
Firm size	2.22 (1.74)	0.805 (0.972)	0.834 (1.01)
Patent (simple)	19.8 (170)	5.42 (85.1)	9.12 (113)
Patent (adjusted)	52.0 (457)	14.1 (214)	23.8 (296)
Fixed assets	107 (1560)	6.12 (303)	8.17 (373)
Total assets	170 (2320)	9.99 (448)	13.2 (554)
Turnover	270 (2420)	20.5 (536)	27.7 (670)
Sales	259 (2430)	23.5 (465)	33.7 (682)
Number of employees	442 (4430)	30.9 (726)	39.1 (953)
Labor costs	46.6 (390)	7.67 (330)	10.9 (336)

Notes: Fixed and total assets, turnover, sales, and labor costs values are in Mln.

corporation), firm size (log of total assets), simple and quality-adjusted number of patents, fixed and total assets, turnover, sales, number of employees, and labor costs. Significantly, substantial discrepancies emerge between R&D funded and non-funded firms, underscoring the necessity of adopting a treatment effects evaluation framework. The raw data indicates that, on average, funded firms are approximately 10 years older and twice as large as non-funded firms. Funded firms also boast around four times more patents and greater firm capital and labor. Regarding industry distribution, funded firms tend to dominate in knowledge-intensive sectors such as Education, Professional, Scientific and Technical Activities,

Table 3.2: Summary statistics on industry characteristics

Industry	Funded (N=207164)	Non-Funded (N=12498684)	Overall (N=12705848)
Accommodation and Food Service Activities	897 (0.4%)	138562 (1.1%)	139459 (1.1%)
Administrative and Support Service Activities	914 (0.4%)	136456 (1.1%)	137370 (1.1%)
Arts, Entertainment and Recreation	1042 (0.5%)	46172 (0.4%)	47214 (0.4%)
Construction of Buildings	1932 (0.9%)	417631 (3.3%)	419563 (3.3%)
Education	5019 (2.4%)	97475 (0.8%)	102494 (0.8%)
Financial and Insurance Activities	509 (0.2%)	182500 (1.5%)	183009 (1.4%)
Human Health and Social Work Activities	2729 (1.3%)	84619 (0.7%)	87348 (0.7%)
Information and Communication	3067 (1.5%)	91675 (0.7%)	94742 (0.7%)
Manufacturing	3118 (1.5%)	62820 (0.5%)	65938 (0.5%)
Other Service Activities	2870 (1.4%)	129965 (1.0%)	132835 (1.0%)
Professional, Scientific and Technical Activities	4677 (2.3%)	350926 (2.8%)	355603 (2.8%)
Public Administration and Defense: Compulsory Social Security	1569 (0.8%)	17802 (0.1%)	19371 (0.2%)
Real Estate Activities	3450 (1.7%)	1060267 (8.5%)	1063717 (8.4%)
Transportation and Storage	1668 (0.8%)	84346 (0.7%)	86014 (0.7%)
Water Supply	862 (0.4%)	18567 (0.1%)	19429 (0.2%)
Wholesale and Retail Trade	5749 (2.8%)	819881 (6.6%)	825630 (6.5%)
Activities of households as employers	0 (0%)	290 (0.0%)	290 (0.0%)

Table 3.3: Summary statistics on some cluster-level characteristics

	Mean	SD	Min	Max
Number of funded firms	1438	1905	12	12090
Proportion of funded firms	0.025	0.022	0.002	0.126
Average firm age	17.8	22.4	-249	1014
Average firm size	0.83	1.01	-1.81	11.8
Average patents (simple)	9.11	113.4	1	7116
Patent (adjusted)	23.8	296.2	0	21151.8
Fixed assets	8.16	373.3	-15.57	456537.5
Total assets	13.2	553.5	-6.89	668236.2
Turnover	27.7	670.4	-164.2	221674.2
Sales	33.7	681.8	-4.23	190262.7
Number of employees	39.1	953.4	1	420000
Labor costs	10.9	335.76	-53.46	293906.7
Number of clusters	144			

Notes: Fixed and total assets, turnover, sales, and labor costs values are in Mln.

Information and Communication, etc. (refer to Table 3.2).

As will be discussed in detail in the next section, our identification strategy relies on the partial interference assumption, where firm interaction is freely allowed within well-defined clusters. In this study, we categorize firms into clusters based on 16 geographic areas and 9 broadly defined industries (detailed explanation provided in section 3.4 and the list of regions and industries in Appendix C.1). This classification yields 144 clusters, each comprising an average of 1438 funded firms. Table 3.3 presents summary statistics of cluster-level variables. The average proportion of funded firms within a cluster is approximately 2.5 percent. Moreover, it is evident from the various measures of dispersion presented in Table 3.3 that clusters exhibit considerable heterogeneity in terms of average characteristics. Table 3.4 showcases the number of funded firms and the proportion of funded (treated) firms by cluster indicating substantial heterogeneity across clusters.

Table 3.4: The number and the proportion of funded (treated) firms by clusters

C	NF	PF	C	NF	PF	C	NF	PF	C	NF	PF
1	12	0.002	37	2619	0.035	73	1242	0.0098079	109	2968	0.054
2	12090	0.055	38	317	0.016	74	763	0.0241609	110	2110	0.006
3	2270	0.021	39	303	0.009	75	523	0.0537623	111	461	0.09
4	330	0.017	40	3696	0.058	76	976	0.0100608	112	310	0.022
5	126	0.013	41	339	0.005	77	1807	0.0219408	113	2086	0.011
6	652	0.017	42	1189	0.014	78	1022	0.0053821	114	565	0.004
7	1219	0.05	43	1008	0.006	79	269	0.0149769	115	349	0.017
8	349	0.021	44	899	0.029	80	1653	0.0834469	116	322	0.014
9	1108	0.06	45	3453	0.008	81	371	0.0237683	117	449	0.009
10	174	0.014	46	1126	0.023	82	4341	0.0089880	118	101	0.01
11	207	0.012	47	332	0.017	83	51	0.0124299	119	504	0.03
12	1246	0.021	48	378	0.008	84	447	0.0247152	120	3968	0.045
13	453	0.006	49	504	0.025	85	3522	0.0141429	121	503	0.014
14	4904	0.01	50	738	0.024	86	996	0.0299405	122	71	0.006
15	674	0.036	51	1111	0.008	87	1104	0.0287134	123	1032	0.021
16	860	0.024	52	404	0.02	88	615	0.0217437	124	534	0.021
17	2130	0.054	53	4507	0.014	89	361	0.0384124	125	718	0.074
18	446	0.007	54	794	0.032	90	313	0.0110445	126	805	0.008
19	2550	0.034	55	955	0.008	91	1710	0.0103238	127	1210	0.0143984
20	711	0.009	56	298	0.005	92	6288	0.0068924	128	475	0.0063220
21	284	0.027	57	577	0.019	93	634	0.0372350	129	5478	0.1264134
22	211	0.008	58	8083	0.057	94	912	0.0805938	130	2588	0.0110198
23	295	0.01	59	206	0.02	95	596	0.0417425	131	3799	0.0549195
24	11547	0.071	60	1163	0.015	96	1573	0.0105336	132	867	0.0149550
25	239	0.044	61	739	0.004	97	2715	0.0073573	133	5198	0.0076556
26	948	0.02	62	1645	0.094	98	468	0.0207898	134	228	0.0244530
27	3182	0.115	63	205	0.008	99	72	0.0089186	135	2877	0.0212318
28	121	0.009	64	1414	0.053	100	229	0.0126394	136	762	0.0603803
29	759	0.016	65	823	0.018	101	387	0.0148395	137	1509	0.0292187
30	670	0.048	66	321	0.031	102	401	0.0309318	138	160	0.0306279
31	458	0.041	67	356	0.075	103	779	0.0123749	139	4313	0.0091855
32	6516	0.01	68	702	0.006	104	1546	0.0216739	140	246	0.0109362
33	511	0.005	69	107	0.039	105	2207	0.0097025	141	2152	0.0259437
34	4231	0.021	70	1292	0.058	106	1434	0.0197360	142	2660	0.0194057
35	481	0.006	71	1828	0.03	107	1264	0.0061842	143	347	0.0070236
36	646	0.007	72	2345	0.014	108	262	0.0198050	144	200	0.0075259

Note: C = Cluster (region-sector) number; NF = Number of funded firms;
PF = Proportion of funded (treated) firms.

3.4 Empirical Framework

3.4.1 Identification Strategy

The estimation of direct and spillover effects has an inherent identification problem. The Stable Unit Treatment Value Assumption (SUTVA) is violated by construction. For the assumption to be reasonable, the outcome of units must be independent of the treatment status of the other units. If we now consider spillover effects, such that the firms benefit from other firms' increased R&D activity in response to the funding, then SUTVA is violated. Additionally, the funding of firms strengthens their position in a competitive market with other funded and non-funded firms causing changes in the competition of the firms due to the funding. Therefore, for SUTVA to hold we can only compare firms that do not have any innovation spillovers and that do not compete with each other in a common market.

Second, public policy programs usually target certain firms which raises a selection problem in the identification. In this case, there is selection on two levels. The authority deciding who receives the funding might be more interested in specific regions or in specific industries. This might be due to future expectations about the economic outcomes in the industry or region or due to personal preference. Furthermore, the firm selection into the treatment is not random but the result of a decision process. It is a decision both from the firm side to apply for funding and from the side of a policymaker to choose a specific firm. The third identification problem comes from region-industry-specific effects. Industry-specific productivity shocks can affect productivity and the innovation outcomes from innovation inputs such as R&D expenditure. This might lead to treatment as a response to the shock and biased estimates.

Since standard approaches are not able to tackle aforementioned issues, we introduce a non-standard approach by combining the methods suggested by Fong et al. (2018), Girma et al. (2015), and Hudgens and Halloran (2008). First of all, to consistently estimate both direct and spillover effects, we relax the SUTVA assumption as suggested by the recent econometric literature (Baird et al., 2018; Ferracci et al., 2014; Girma et al., 2015; Hudgens and Halloran, 2008; Rosenbaum, 2007). As per this literature, treatment externalities, also known as spillovers, occur when an individual's potential outcome is influenced by the treatment status of others within a group. As the effect of the funding is not limited to funded firms, the treatment pollutes the control units. As the funding does not affect all control units, but only groups of them, we refer to partial interference. In contrast to standard methods controlling for the funding of neighboring firms, we can form groups into the spatial dimension as well as the industry dimension to also control for the effect of funding on technologically close non-treated firms. This is important to capture knowledge spillovers and effects on competition in markets as two potential sources of spillover effects. Furthermore, defining clusters and allowing the firms within clusters to interact enables us to perform matching on an additional level, the cluster level. Following the approach outlined by Hudgens and Halloran (2008), we utilize the proportion of treated firms within a group as a metric for interaction among individual firms. Consequently, potential outcomes are modeled as a function of the firm's treatment status and the fraction of R&D funded firms within a specific group or cluster.

Following Girma et al. (2015), we conduct estimations at two distinct levels: the firm level and the cluster level, with clusters defined as unique region-industry combinations. Our main identification assumption is the Partial Interference Assumption, where we posit that the SUTVA holds across clusters but not within clusters. This implies that the potential outcomes of an observed unit are unaffected by the treatment status of other units in other clusters. To satisfy this assumption, we require a cluster definition that provides a sufficient number of clusters with substantial variation to facilitate estimations while ensuring diverse levels of interaction within the clusters. On the other hand, it's essential to minimize interaction in innovative activity and competition across the clusters as much as possible. For this purpose, we utilize region-industry combinations with German NUTS 1 regions (Bundesländer) and the broad NACE 1 industry classification. Our Partial Interference Assumption dictates that interactions occur solely between firms within the same industry and region. This approach aligns with findings in the literature on innovative spillovers, which indicate a sharp spatial decay and limited interactions within broader industries (Acemoglu et al., 2016; Breschi and Lissoni, 2005; Glaeser et al., 1992). Moreover, this definition, rather than relying solely on industry classification or regions, offers a sufficiently broad range of clusters. The 9 NACE 1 industries and 16 NUTS 1 regions in Germany give us 144 unique combinations of broad industries and regions. An illustrative example of a cluster could be the manufacturing industry in North Rhine-Westphalia (NRW) or the Information and Communication industry in Bavaria.

3.4.2 Estimation Strategy

We use the empirical methodology suggested by the literature on causal estimands under partial interference that was introduced by Hudgens and Halloran (2008) and shifted to infinite populations by Baird et al. (2018). As suggested by Girma et al. (2015), we perform the first part of the analysis separately for each cluster and then shift the analysis to the cluster level. To give a brief overview, the methodology we use involves the following steps:

Steps on the firm level (separately for each cluster):

1. **Firm-Level Matching**
   We obtain inverse probability weights by regressing the binary treatment variable (1 if a firm was funded, 0 otherwise) on pre-treatment firm characteristics.

2. **Computing cluster-specific treatment effects**
   We regress the firm-level outcome (fixed assets, number of employees, or quality-adjusted number of patents) on the binary treatment variable and control variables.

3. **Computing potential outcomes**
   We obtain the potential outcomes for each of these firm-level outcomes based on the estimated coefficients from step 2 and the corresponding observations for those variables. We calculate cluster-average potential outcomes for treated and non-treated units.

Steps on the cluster level:

4. **Aggregating the variables**
   We compute the cluster-means for the firm-specific control variables and the binary treatment variable.

5. **Generalized Propensity Scores**
   We compute Generalized Propensity Scores following Fong et al. (2018) regressing the average binary treatment variable in a cluster on the average firm characteristics in a cluster.

6. **Computing coefficients for the treatment intensity and the GPS**
   We regress the cluster-average potential outcomes across treated firms and across non-treated firms on the Generalized Propensity Scores and the treatment intensity.

7. **Computing Cluster-potential outcomes**
   Based on the coefficients obtained from step 6, we compute the cluster-level potential outcomes for different levels of treatment intensities.

8. **Treatment effect calculation**
   We compute the direct, indirect and overall treatment effects comparing the cluster-level potential outcomes.

We start with the analysis on the firm level. Addressing the effectiveness of R&D funding poses several challenges, primarily stemming from the selective nature of such funding initiatives. Comparing funded and non-funded firms is inherently complex due to existing disparities in outcomes. These disparities give rise to various identification issues. Firstly, the endogeneity of treatment arises as firms may self-select into receiving R&D funding based on unobservable traits that also influence their outcomes. This self-selection introduces bias into estimates of the causal impact of R&D funding on firm performance and innovation outcomes.

Secondly, sample selection bias emerges as the sample of funded firms may not accurately represent the broader population of firms. This discrepancy can lead to biased estimates if treated and untreated groups differ systematically in ways that influence the outcomes of interest.

The latter can be observed in Table 3.5 which summarizes the means of the main variables for the treated firms and the non-treated firms before the first observed treatment. The outcome variables and covariates differ across treated firms and non-treated firms. Treated firms are larger in terms of number of employees and fixed assets. Also, they are slightly older (3 years) and the innovation outcomes (quality-adjusted patents) were already higher before the funding programs were rolled out. We conclude from differences in means between the treatment and control group that firms are non-randomly selected into treatment based on pre-treatment characteristics.

Table 3.5: Pre-treatment differences between treated and non-treated firms

Variables	Treated	Non-treated
Firm age	30	27
Yearly Patents (adjusted)	2.49	0.16
Fixed assets	69036	8012
Number of employees	332	38

Note: Fixed assets are in thousand dollars

Finally, the heterogeneous treatment effects add another layer of complexity as the impact of R&D funding may vary across firms based on diverse characteristics such as size, industry, and age. Failing to consider these differences can result in biased estimates of the average treatment effect.

To overcome these identification issues on the firm level, we perform inverse probability weighting on pre-treatment firm-level characteristics. This approach relies on the assumption of unconfoundedness, which states that conditional on the observed covariates, treatment assignment is independent of potential outcomes. Additionally, matching methods assume common support, which means that there are units in both the treatment and control groups with propensity scores overlapping across the entire range. These assumptions help ensure that the weighted samples are comparable, allowing for a more credible estimation of treatment effects (Hill and Su, 2013; Huber, 2013).

We define a treatment dummy d_{ict} on the firm level that takes the value of 1 if firm i in cluster c received funding in year t, and 0 otherwise. To compute the inverse probability weights, we perform the logistic regression from equation (3.1) for each cluster separately. We regress the treatment dummy d_{ict} on a vector X_{ict} of firm-level covariates before treatment. Those covariates include firm age, number of employees, fixed assets, and a dummy indicating whether the firm is a low-tech, medium-tech, or high-tech firm. The dummy is based on the NACE 2 industry classification of the firm. We exclude the respective outcome variable from the firm covariates in the logistic regressions.

$$P(d_{ict} = 1) = \frac{1}{1 + e^{-b_0 + b_1 X_{ict}}} \tag{3.1}$$

We obtain the inverse probabilities by calculating $\frac{1}{\hat{d_{ict}}}$ for treated firms and $\frac{1}{1-\hat{d}_{ict}}$ for non-treated firms. We use the generated propensity scores as inverse probability weights in the OLS regressions on the firm level.

Table 3.6: Determinants for selection into treatment

	Dependent variable:
	Treatment
Fixed Assets	0.169***
	(0.011)
Number of Employees	0.398***
	(0.016)
Firm Age	0.045
	(0.034)
High-Tech	0.035
	(0.080)
Low-Tech	−1.066***
	(0.057)
Constant	−5.950***
	(0.172)
Observations	39,595
Log Likelihood	−6,913.510
Akaike Inf. Crit.	13,839.020
Note:	*p<0.1; **p<0.05; ***p<0.01

Table 3.6 shows the determinants of the treatment on the firm level. The size of the firm in terms of fixed assets and number of employees have a positive impact on selection into treatment and low-tech firms are less likely to receive the treatment compared to medium- or high-tech firms.

Using the inverse-probability weights from the logistic regressions for each cluster separately, we regress the outcomes on firm-level control variables X_{ict} and the treatment dummy d_{ict},

$$y_{ict} = \alpha + \beta d_{ict} + \delta X_{ict} + \epsilon_{ict}; \quad i = 1, \ldots, N_{ct} \tag{3.2}$$

With y_{ict} being the firm-level (fixed assets, number of employees) and innovation (quality-adjusted number of patents) outcome of firm i in cluster c, X is the vector of firm-level covariates including firm age, number of employees, and fixed assets, and the error term ϵ. Here, we obtain separate estimates for the effect of receiving funding on the outcome.

In the third step, we calculate the potential outcomes separately for each cluster c and each year t. For that, we use the OLS estimates from equation (3.2) and the firm-level observations for control variables X_{ict}. We set the treatment dummy $d_{ict} = 1$ for the potential outcome under treatment $\bar{y}^1_{ct}$ and $d_{ict} = 0$ for the potential outcome under no treatment $\bar{y}^0_{ct}$. We aggregate those potential outcomes across the firms in a respective cluster to obtain the average potential outcomes for each cluster and year, for treated and untreated firms.

$$\bar{y}^1_{ct} = \frac{1}{N_{ct}} \sum_{i=1}^{N_{ct}} \hat{\alpha} + \hat{\beta}_c + \hat{\delta}_c X_{ict} \quad \text{and} \quad \bar{y}^0_{ct} = \frac{1}{N_c} \sum_{i=1}^{N} \hat{\alpha} + \hat{\delta}_c X_{ict} \tag{3.3}$$

After obtaining the potential outcomes for each cluster and year separately, we shift our methodology to the cluster level. To solve potential selection issues also on the cluster-level, we calculate propensity scores. The treatment variable now needs to be adapted, as we now do not observe the treatment of each firm individually, but the overall treatment intensity within each cluster. Therefore we use the proportion of treated firms as suggested by Hudgens and Halloran (2008) in the cluster (p_c). Note that $p_c \in (0, 1)$ and is continuous. We denote the number of firms in cluster c and year t as N_{ct}. The proportion of treated firms in a cluster indicates the intensity of the funding within the cluster and at the same time it is a proxy for the probability of interaction of treated firms in a cluster.

$$p_{ct} = \frac{\sum_{i=1}^{N_{ct}} d_{ict}}{N_{ct}} \tag{3.4}$$

To check the validity of the SUTVA assumption on the cluster level, we observe the correlation between the share of treated firms in the cluster and other cluster-level variables (see Table 3.7). The clusters show heterogeneity regarding their firm composition and the selection of funding is likely to be non-random but in parts driven by differences in cluster-level characteristics. To overcome a potential selection bias on observed cluster-level variables, we match also on the cluster-level.

Table 3.7: Correlation matrix of cluster-level variables

	% Treated Firms	No. Employees	Fixed Assets	Firm Age	Pot. Outcome
% Treated Firms	1.00***	0.06***	-0.00	-0.22***	0.14***
No. Employees	0.06***	1.00***	0.08***	0.25***	0.51***
Fixed Assets	-0.00	0.08***	1.00***	0.04***	0.18***
Firm Age	-0.22***	0.25***	0.04**	1.00**	0.41***
Pot. Outcome	0.14***	0.51***	0.18***	0.41***	1.00***

Table 3.8: Pre-treatment in clusters

Variables	25% quantile p_c	50% quantile p_c	75% quantile p_c
Firm age	37.0	37.6	37.9
Yearly Patents (adjusted)	25.28	31.13	36.81
Fixed assets	63947	63364	83366
Number of employees	430	464	585

Note: Fixed assets are in thousand dollars

Table 3.8 shows the firm-level characteristics averaged over the cluster. We split by different treatment intensities p_{ct}. However, there are still observable differences in the characteristics between different levels of treatment. The clusters with a higher proportion of treated firms tend to be larger, with a larger number of employees and higher fixed assets. Additionally, they already had higher patent counts before the observed treatment. Also, they are slightly older. To account for those differences in the characteristics that increase the overall level of treatment in the cluster and at the same time increase average patent outcomes in the clusters, we perform matching also on the cluster level.

Due to the treatment variable now being continuous, we calculate the conditional density of the treatment using the Covariate Balancing Generalized Propensity Score estimation procedure suggested by Fong et al. (2018). We use the same covariates as in the firm-level matching, but now we average them across the firms in the cluster. As we did on the firm-level, we again exclude the outcome variable from the cluster-level covariates. We fit different estimators of the Generalised Propensity Score and pick the one with the best Covariate Balancing. The Covariate Balancing Generalized Propensity Score is the one with the best Covariate Balancing properties. See Appendix C.3 for more details on the generalized propensity score estimation procedure.

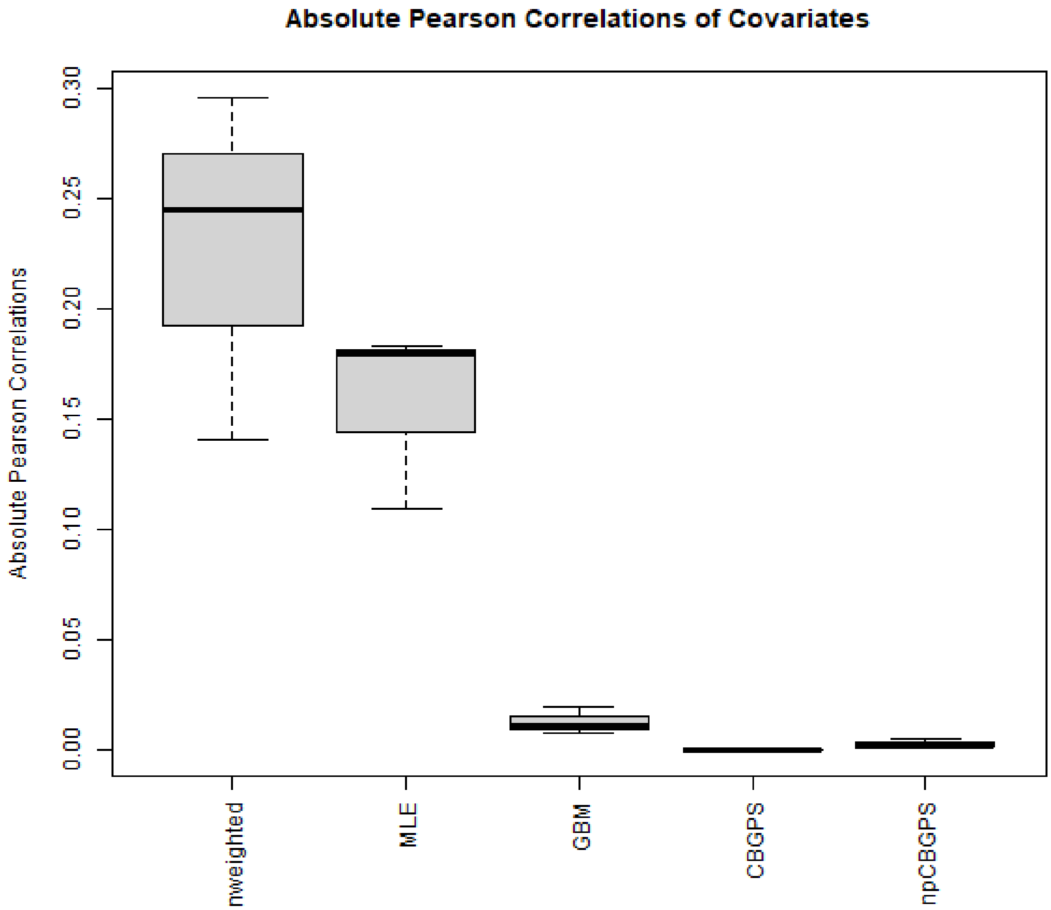


Figure 3.2: Covariate balancing propensity score estimation

Figure 3.2 shows the covariate balancing statistics for the generalized propensity score estimations. We use the propensity scores from the Covariate Balancing Generalised Propensity Score (CBPS) estimation in the further procedure, as it minimizes the Correlation of the covariates considerably compared to Generalized Boosted Models (GBM) and Maximum Likelihood (MLE). The non-parametric version of the CBPS (npCBPS) achieves similar, but slightly worse Covariate Balancing.

We use the estimates of the conditional density of the continuous treatment $G\hat{P}S_{ct}$ and the proportion of treated firms within a cluster p_{ct} to approximate the expected values of the cluster-level potential outcomes under treatment and no treatment $\bar{y}^d_{ct}$ we generated in the firm-level estimation in equation 3.3. Following **Hirano_2004**, we approximate the potential outcomes under treatment $d = 1$ and potential outcomes under no treatment $d = 0$. We use polynomial approximations of the second-order

$$\bar{y}^d_{ct} = \beta_0 + \beta_1 G\hat{P}S_{ct} + \beta_2 p_{ct} + \beta_3 G\hat{P}S^2_{ct} + \beta_4 p^2_{ct} + \beta_5 G\hat{P}S_{ct} p_{ct}. \tag{3.5}$$

The approximation in Equation 3.5 gives us estimates $\hat{\beta}_1$ to $\hat{\beta}_5$ to calculate the average potential cluster outcomes for different levels of proportion of treated firms $\bar{y}_p^d$, where p is the proportion of treated firms.

In the final step, we use a grid of possible values of p_c. We define a grid over the values from the minimum proportion of treated firms in a cluster to the maximum proportion. Therefore, the grid goes from 0 to 0.22 with 0.01 steps. For each of these values of the grid, we calculate the sample average potential outcomes across all clusters under treatment and no treatment. For this, we use the estimated coefficients from equation 3.5 and the conditional treatment density estimates $G\hat{P}S_c$. We follow

$$\bar{y}_p^d = \frac{1}{C}\sum_{r=1}^{C}(\hat{\beta}_0 + \hat{\beta}_1 G\hat{P}S_{ct} + \hat{\beta}_2 p_{ct} + \hat{\beta}_3 G\hat{P}S_{ct}^2 + \hat{\beta}_4 p_{ct}^2 + \hat{\beta}_5 G\hat{P}S_{ct} p_{ct}) \tag{3.6}$$

We obtain the cluster-level potential outcomes from Equation 3.6 for all different intensities of treatment on the cluster-level. We can now make comparisons of those to calculate treatment effects. We distinguish three types of treatment effects: the direct effect, the spillover effect, and the total effect. For the direct effect γ_{pp}^{10}, we compare cluster potential outcomes with the same treatment intensity p_{ct} within the cluster under treatment and no treatment. Therefore, we hold the interaction of funded firms in the cluster constant while we assume no interaction across clusters. The potential outcomes only differ due to the treatment status of the respective firm.

$$\bar{\gamma}_{pp}^{10} = \bar{y}_p^1 - \bar{y}_p^0 \tag{3.7}$$

With $\bar{\gamma}_{pp}^{10}$ being the direct treatment effect, $\bar{y}_p^1$ is the average potential outcome under treatment for a cluster with the proportion of treated firms p and $\bar{y}_p^0$ is the average potential outcome under no treatment for a cluster with the same proportion of treated firms p.

For the spillover effect γ_{p0}^{00}, we compare only non-treated cluster potential outcomes with different levels of the proportion of funded firms in the cluster. The outcomes only differ due to their different levels of treatment in the cluster. Therefore, there is a different level of interaction with funded firms in the compared clusters.

$$\bar{\gamma}_{p0}^{00} = \bar{y}_p^0 - \bar{y}_0^0 \tag{3.8}$$

With $\bar{\gamma}_{p0}^{00}$ being the spillover effect, $\bar{y}_p^0$ the average potential outcome under no treatment for a cluster with proportion of treated firms p and $\bar{y}_0^0$ the average potential outcome under no treatment for a cluster without treated firms, i.e., the proportion of treated firms $p = 0$.

The total effect can be decomposed into direct and indirect effects. It compares treated and untreated potential outcomes at different levels of the proportion of funded firms in the cluster.

$$\bar{\gamma}_{p0}^{10} = \bar{y}_p^1 + \bar{y}_0^0 \tag{3.9}$$

With $\bar{\gamma}_{p0}^{10}$ being the total effect, $\bar{y}_p^1$ the average potential outcome under no treatment for a cluster with proportion of treated firms p and $\bar{y}_0^0$ the average potential outcome under no treatment for a cluster without treated firms, i.e., the proportion of treated firms $p = 0$.

3.5 Results

Our main findings regarding the direct and spillover effects of R&D funding on the firm-level and innovation outcomes are depicted in Figure 3.3. The latter shows that the average treatment effect (i.e., the changes in firm financial and innovation outcomes due to the R&D subsidy) is highly dependent on the proportion of R&D funded firms in the cluster. For example, the direct effect of funding on the quality-adjusted number of patents enhances by around 3.5% when we increase the share of R&D funded firms up to 1.6 %.

When we further increase the share of R&D funded firms to 1.7% and above, its direct effect on patents starts to decline. The picture is different when we observe the spillover effect of R&D funding on innovation, which will be around 10% and 30% when the proportions of R&D funded firms in the cluster are 1% and 2%. This might indicate that the R&D funding indirectly initiated new collaborations between funded and non-funded firms, thus leading to the generation of more patents. Analogous results regarding more pronounced indirect effects were observed by Czarnitzki et al. (2007) who identified no direct impact but rather significant indirect effects on private R&D stemming from positive spillovers facilitated by collaborations.

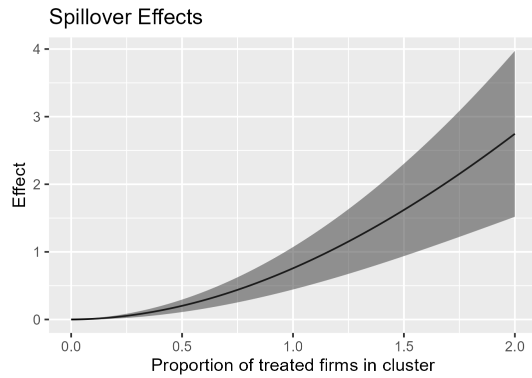
Further, we uncover a significant positive direct impact on the financial outcomes of treated firms. For example, the direct effects of R&D subsidy on fixed assets and the number of employees are 10% and 20% when the proportion of funded firms in the cluster is 1%. When we increase the share of funded firms up to 2%, the direct effect on fixed assets and the number of employees almost triples as well by reaching up to 30% and 20%, respectively. The positive trend holds for the spillover effects of R&D subsidy on fixed assets of non-funded firms. However, the indirect effect on the number of employees is negative and reduces by 25% when we increase the share of funded firms up to 2%. This can indicate that the R&D funding of single firms triggered negative competition effects for employment.

Our findings suggest that after the treatment, the funded firms increase their capital and labor. The non-funded firms increase their knowledge output, which can be seen from the increase in the number of quality-adjusted patents for non-funded firms with exposure to funded firms. If we assume high-skill labor for innovative activity to be limited within the cluster, the non-funded firms increase their innovative input by increasing capital but suffer from competing against funded firms in the high-skill labor market. Therefore, they increase fixed assets and the number of employees is decreased, while the funded firms increase their number of employees and fixed assets thus increasing innovative output. Similar results regarding negative competition effects for labor are reported in the previous research on cutting-edge innovation clusters conducted by Lehmann and Menter (2017). While they observed positive impacts on innovation outcomes similar to our results, they also identified 'beggar-thy-neighbor' effects in sunrise regions affecting neighboring areas. They argue that focusing on 'winning' regions increases their capacity and the demand for highly skilled labor, causing neighboring regions to suffer more from the 'power of attraction' of these leading-edge clusters in comparison to non-neighboring regions.

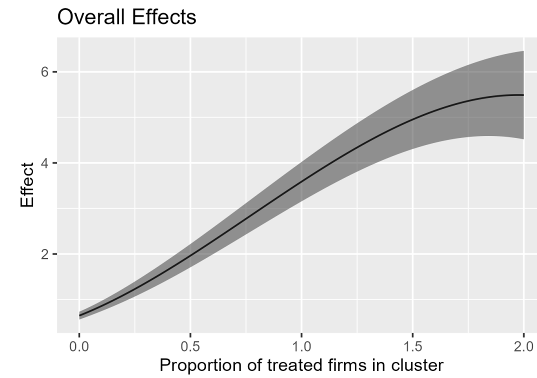
Finally, we obtain the total effect by combining the direct and indirect effects together. We find, that the total effects on both innovation (patents) and firm-level (fixed assets and employment) outcomes are uniformly positive and increasing with the share of funded firms in the cluster. However, for the number of employees and quality-adjusted number of patents, the total effect stabilizes when the proportion of funded firms in the cluster goes beyond 1.6%. Despite having negative spillovers from employment, here the direct effect dominates, and thus the total effect of R&D funding on the number of employees is positive.

These findings align with the broader consensus in the innovation literature, which suggests that R&D funding enhances innovation (Aschhoff, 2009; Bronzini and Piselli, 2016; Czarnitzki and Fier, 2002; Czarnitzki and Licht, 2006; Czarnitzki and Hussinger, 2018; Czarnitzki and Lopes Bento, 2012; Kleine et al., 2022) and firm-level outcomes (Falk, 2010; Hall et al., 2009; Lerner, 1999). They also support the ideas of Akcigit et al. (2018), Breschi and Lissoni (2009), Crass et al. (2017), Engel et al. (2017), Nishimura and Okamuro (2010), and Uyarra and Ramlogan (2016), who highlight the importance of agglomeration for firm-level development. Furthermore, the findings reinforce the spillover literature's arguments regarding the positive effects of spatial (Acemoglu et al., 2016; Breschi and Lissoni, 2005; Petruzzelli, 2011) and industrial (Bloom et al., 2013; Li and Bosworth, 2018) proximity demonstrated by the unique region-industry clusters in our analysis. They also verify the findings of Girma et al. (2015) regarding FDI spillovers in innovation setup, as the prevalence of R&D funded firms suggests a greater propensity for positive externalities through increased collaboration.

(a) Patents (quality-adj. number)

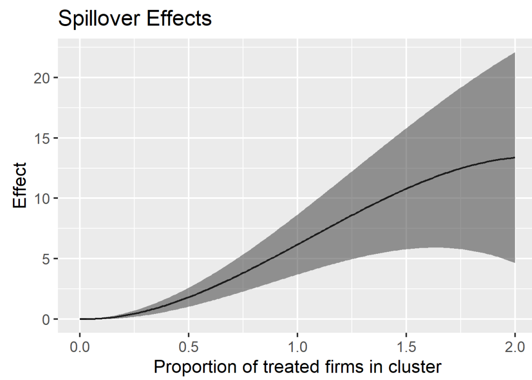


(b) Patents (quality-adj. number)



(c) Patents (quality-adj. number)

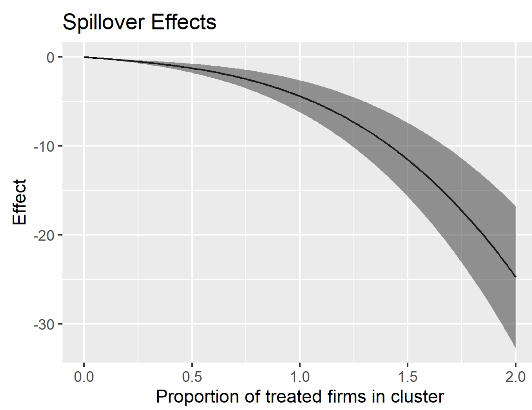
(d) Fixed Assets



(e) Fixed Assets

(f) Fixed Assets

(g) Number of Employees



(h) Number of Employees

(i) Number of Employees

Figure 3.3: Causal effects of R&D funding on innovation and firm-level performance of funded and non-funded firms with 95% confidence intervals

3.6 Conclusion

This research implements the causal analysis of the direct and spillover effects of R&D funding on firm-level performance and innovation activity of funded and non-funded firms. This study reveals a crucial insight: the effects of research and development (R&D) funding, both direct and spillover impact, exhibit variability contingent on the proportion of funded firms within the cluster. While we see a positive direct impact on firm capital and labor when we increase the proportion of funded firms, the direct effect on patents decreases when the share of funded firms exceeds 1.7%. For spillovers on non-funded firms, we find that they are positive for patents and fixed assets, however, they are negative for the number of employees. What is also interesting, the indirect effects for the quality-adjusted number of patents and fixed assets increase monotonically with the proportion of funded firms. Consequently, policymakers should encourage the concentration of more R&D-funded firms in a cluster, however, not above the threshold of 1.7% to maintain innovation effects.

Another striking finding is that the direct effect of R&D funding on patents is less pronounced compared to the indirect effect: for the clusters with a higher proportion of treated firms, the indirect effect starts to dominate. This signals that funding R&D activities of every single firm in an isolated setup is not a good approach. Rather, the policy aiming at the R&D funding of several firms close to each other (either spatially or industry-wise) will enforce both direct and indirect positive effects. One working example of the aforementioned policy setup can be the cluster type of funding, which according to the existing literature (Acemoglu et al., 2016; Audretsch et al., 2018; Breschi and Lissoni, 2005; Crass et al., 2017; Lehmann and Menter, 2017) does not only have a positive direct impact on the innovation activity of funded firms but also enforces positive spillovers for non-funded neighbor firms. Finally, the total effect is positive and significant for all outcome variables.

Generally, the results of our study show that the estimation of both direct and indirect treatment effects provides a clearer understanding of different mechanisms through which the share of funded firms impacts the potential outcomes of funded and non-funded firms. In this way, our research significantly contributes to the policy debate regarding the benefits of R&D funding and optimal allocation from a regional development perspective and aligns well with the earlier studies reporting positive direct (Aschhoff, 2009; Bronzini and Piselli, 2016; Czarnitzki and Hussinger, 2018; Falk, 2010; Hall et al., 2009) and indirect effects (Czarnitzki et al., 2007).

However, our research also has some potential limitations. The results are obtained under the Partial Inference Assumption, assuming that SUTVA holds across the clusters but not within. There are scenarios where this might not be the case. A potential threat to the SUTVA assumption is the presence of ownership groups. Subsidiaries within these ownership groups are linked across different clusters and might interact as part of the same group, implying inter-firm interactions across clusters, and potentially violating the SUTVA assumption. Additionally, input and output linkages might similarly challenge the validity of SUTVA.

Additionally, the assumption of random treatment assignments conditional on observable characteristics might not hold perfectly because, in an ideal scenario, we would require more detailed information about the firms to ensure genuine randomness. Relying only on observable characteristics means that unobserved variables could still affect treatment assignment, introducing potential biases. More comprehensive data on firm-specific attributes, such as internal capabilities and market conditions, would enhance the reliability and ensure a more accurate analysis of the causal effects. Hence, it might be interesting for future research to observe these linkages and utilize more detailed data to improve empirical validity.

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APPENDIX A

Chapter 1

A.1 EU Public Procurement Thresholds for different Types of Contract

Table A.1: Public Procurement Thresholds in the European Union (2008–2025), in Euros

Year	Contract Type	Works	Services (National)	Services (Regional)	Services (Utilities)	Supply (National)	Supply (Regional)
2008	All Types	4,150,000	133,000	206,000	412,000	133,000	206,000
2009	All Types	4,150,000	133,000	206,000	412,000	133,000	206,000
2010	All Types	4,845,000	125,000	193,000	387,000	133,000	206,000
2011	All Types	4,845,000	125,000	193,000	387,000	133,000	206,000
2012	All Types	5,000,000	130,000	200,000	400,000	135,000	209,000
2013	All Types	5,000,000	130,000	200,000	400,000	135,000	209,000
2014	All Types	5,186,000	134,000	207,000	414,000	139,000	214,000
2015	All Types	5,186,000	134,000	207,000	414,000	139,000	214,000
2016	All Types	5,225,000	135,000	209,000	418,000	139,000	214,000
2017	All Types	5,225,000	135,000	209,000	418,000	139,000	214,000
2018	All Types	5,548,000	144,000	221,000	443,000	140,000	215,000
2019	All Types	5,548,000	144,000	221,000	443,000	140,000	215,000
2020	All Types	5,350,000	139,000	214,000	428,000	139,000	214,000
2021	All Types	5,350,000	139,000	214,000	428,000	139,000	214,000
2022	All Types	5,382,000	140,000	215,000	431,000	140,000	215,000
2023	All Types	5,382,000	140,000	215,000	431,000	140,000	215,000
2024	All Types	5,538,000	143,000	221,000	443,000	143,000	221,000
2025	All Types	5,538,000	143,000	221,000	443,000	143,000	221,000

A.2 CPV Codes for Digital Industries Dummy

Table A.2: CPV codes and descriptions for the digital industries dummy

CPV Code	Description	CPV Code	Description
30200000	Computer equipment and supplies	32400000	Networks
30210000	Data-processing machines (hardware)	32410000	Local area network
30213000	Personal computers	32411000	Ethernet network
30213100	Portable computers	32412000	Communications network
30213200	Tablet computers	32413000	Wide area network
30213300	Desktop computer	32414000	Network infrastructure
30214000	Workstations	32415000	Network cabling
30230000	Computer-related equipment	32416000	Network equipment
30231000	Computer screens and consoles	32417000	Network routers
30232000	Printers and plotters	32418000	Network switches
30233000	Media storage and reader devices	32420000	Telecommunications network
30233100	Media storage devices	32421000	Telephone network
30234000	Storage media	32422000	Data network
30236000	Miscellaneous computer equipment	32423000	Voice network
30237000	Parts, accessories and supplies for computers	32424000	Video network
30237100	Parts of computers	32425000	Wireless network
30237200	Computer accessories	32426000	Optical network
30237300	Computer supplies	32427000	Satellite network
30237400	Computer-related devices	32428000	Mobile network
30238000	Computer hardware	32429000	Internet network
30239000	Computer-related services	32430000	Wide area network services
30240000	Computer hardware consultancy services	32431000	Local area network services
30241000	Computer hardware acceptance testing consultancy services	32432000	Network management services
30242000	Computer hardware disaster recovery consultancy services	32433000	Network planning services
30243000	Computer hardware integration consultancy services	32434000	Network design services

CPV Code	Description	CPV Code	Description
30244000	Computer hardware maintenance consultancy services	32435000	Network implementation services
30245000	Computer hardware support services	32436000	Network maintenance services
30246000	Computer hardware installation consultancy services	32437000	Network support services
30247000	Computer hardware project management consultancy services	32438000	Network security services
30248000	Computer hardware procurement consultancy services	32439000	Network consultancy services
30249000	Computer hardware quality assurance consultancy services	32440000	Network infrastructure services
30250000	Computer hardware training services	32441000	Network cabling services
30251000	Computer hardware training consultancy services	32442000	Network equipment installation services
30252000	Computer hardware training courses	32443000	Network equipment maintenance services
30253000	Computer hardware training materials	32444000	Network equipment support services
30254000	Computer hardware training software	64200000	Telecommunications services
30260000	Computer hardware rental services	64210000	Telephone and data transmission services
30261000	Computer hardware leasing services	64220000	Telecommunication services except telephone and data transmission services
30262000	Computer hardware maintenance services	64230000	Telecommunication cable services
30263000	Computer hardware repair services	64300000	Telecommunications network
30264000	Computer hardware support services	64310000	Telephone network
30265000	Computer hardware testing services	64320000	Digital network
30266000	Computer hardware upgrading services	72000000	IT services: consulting, software development, Internet and support

CPV Code	Description	CPV Code	Description
30267000	Computer hardware warranty services	72100000	Hardware consultancy services
30268000	Computer hardware disposal services	72200000	Software programming and consultancy services
30269000	Computer hardware recycling services	72210000	Programming services of packaged software products
30270000	Computer hardware consultancy services	72211000	Programming services of systems and user software
30271000	Computer hardware acceptance testing services	72212000	Programming services of application software
30272000	Computer hardware disaster recovery services	72213000	Programming services of communication software
30273000	Computer hardware integration services	72214000	Programming services of database software
30274000	Computer hardware maintenance services	72215000	Programming services of graphics software
30275000	Computer hardware support services	72216000	Programming services of financial software
30276000	Computer hardware installation services	72217000	Programming services of security software
30277000	Computer hardware project management services	72218000	Programming services of multimedia software
30278000	Computer hardware procurement services	72219000	Programming services of development software
30279000	Computer hardware quality assurance services	72220000	Systems and technical consultancy services
30280000	Computer hardware training services	72221000	Business analysis consultancy services
30281000	Computer hardware training consultancy services	72222000	Information systems or technology strategic review and planning services
30282000	Computer hardware training courses	72223000	Information technology requirements review services
30283000	Computer hardware training materials	72224000	Project management consultancy services
30284000	Computer hardware training software	72225000	System quality assurance assessment and review services
32400000	Networks	72226000	System implementation planning services

CPV Code	Description	CPV Code	Description
32410000	Local area network	72227000	Software integration consultancy services
32411000	Ethernet network	72228000	Hardware integration consultancy services

A.3 Reconstruction of Missing Award Criteria Data

The information on the number of award criteria originates from the TED (Tenders Electronic Daily) structured fields. In the complementary DigiWhist dataset, the criteria count variable was only available for a subset of tenders. Consequently, the number of award criteria was non-randomly missing, with missingness concentrated among tenders that appeared more complex (e.g., with multiple award criteria beyond price). In the DigiWhist data, the average number of criteria among available observations was 1.6, suggesting that primarily single-criterion tenders (price-only) were captured.

To address this limitation, I use the unstructured text fields containing award criterion weightings to infer the number of non-price criteria. These weightings are typically separated by triple dashes ("—"), e.g. `15,00---10,00---10,00---5,00`, which represents four non-price criteria in addition to the price criterion, resulting in a total of five criteria.

The number of criteria was therefore reconstructed as: number of $" - - - " + 2$ (for the first criterion and the price criterion) whenever a valid weighting list was available and the structured field was missing. This approach allowed recovering values for the majority of previously missing tenders.

After reconstruction, the mean number of criteria increased from 1.6 to 2, confirming that tenders with multiple award criteria had been underrepresented in DigiWhist data. The expanded dataset thus provides a more comprehensive view of procurement procedures, particularly those using more elaborate evaluation schemes.

In the regression analysis presented in table 1.7, the inclusion of these additional tenders modestly affects the estimated relationship between distance and tender complexity. Specifically, the interaction between Firm–Authority Distance and Number of Criteria remains negative but loses statistical significance (p 0.11). This attenuation likely reflects the broader coverage of complex tenders across different contract types and geographies, reducing the previously exaggerated concentration of locally awarded complex tenders.

A.4 Distance Density

Figures A.1 and A.2 display the distance density distributions between the contracting authority and the firm (red) and between the place of service and the firm (green), respectively. The density patterns for both types of distances are strikingly similar, with a notable spike around 60 km for both relationships.

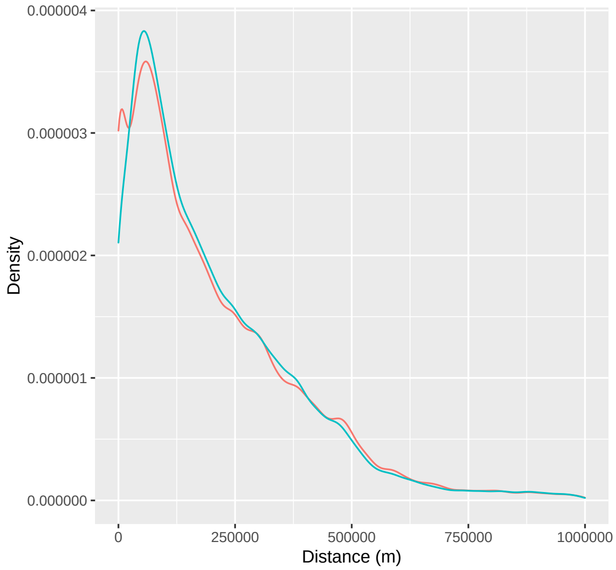


Figure A.1: Density of the distance between the contracting authority and the winning firm (red) and between the place of performance and the winning firm (green)

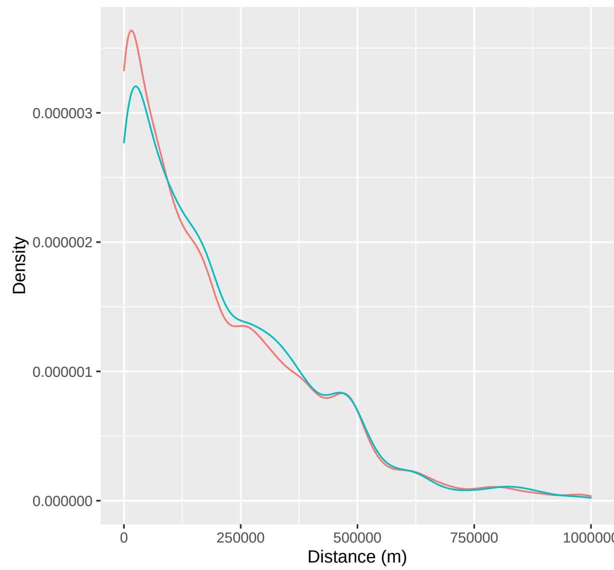


Figure A.2: Density of the distance between the contracting authority and the winning firm (red) and between the place of performance and the winning firm (green) for contracts from digital industries

Despite heterogeneity across public authorities and contracts, the two distance measures show no striking differences overall. In digital industries (Figure A.2), the distance between the contracting authority and the winning firm (green line) is more dispersed than the distance to the place of performance (red line). This pattern reflects weak home bias in digital sectors: the location of the contracting authority has little influence on contract awards, while proximity to the place of performance still matters. Digital contracts are often standardized and tradable across regions, allowing authorities to select firms from a wider geographic area. In contrast, the narrower distribution of Firm–Place distances indicates that operational or implementation constraints, such as on-site IT services, maintenance, or compliance with local regulations, still require firms to be located relatively close to the project site.

A.5 Higher Scope and Award Values

The split-sample analysis by contract size supports the interpretation that home bias is primarily a feature of large-value contracts. When award values are divided at the median, high-value contracts exhibit clear home bias, with negative and statistically significant coefficients for cross-border distance and positive coefficients for same-country and local relationships. In contrast, low-value contracts show no systematic home bias. This pattern aligns with the broader evidence in the paper: home bias persists in the aggregate but is driven disproportionately by larger, more complex, or higher-stakes procurements. Award Values are higher for bunching authorities, as shown in Section 1.5 and show home bias, in contrast to non-bunching authorities. Therefore, it is not immediately clear whether the results are driven by bunching authorities being larger and having higher valued contracts or bunching authorities having home bias and therefore leading to a home bias in higher- valued contracts. However, contracts auctioned via electronic tenders

Table A.3: Results split by below and above median award values

Dependent Variable:	Award Value		
Model:	(All)	(Low)	(High)
Variables			
Firm-Authority Distance	-0.0932***	0.0443	-0.0441***
	(0.0241)	(0.0465)	(0.0123)
Firm-Place Distance	-0.0339	-0.1107**	0.0179
	(0.0244)	(0.0480)	(0.0123)
Authority-Place Distance	-0.0113	0.0365	-0.0166
	(0.0367)	(0.1008)	(0.0192)
Firm-Place Same Country	0.8770**	-0.2302	0.1150
	(0.3624)	(0.7311)	(0.1545)
Authority-Place Same Country	1.457***	-1.607	0.4154
	(0.5072)	(3.113)	(0.3482)
Firm-Authority Same Country	-1.516***	0.3103	-0.6888***
	(0.3582)	(0.7265)	(0.1529)
Authority-Place Same NUTS2	0.2148***	0.1221	-0.0096
	(0.0717)	(0.2296)	(0.0416)
Firm-Authority Same NUTS2	0.0471	0.0407	-0.0111
	(0.0528)	(0.1040)	(0.0270)
Firm-Place Same NUTS2	0.0026	-0.0649	0.0171
	(0.0518)	(0.1035)	(0.0264)
Firm-Authority Local	-0.9596***	0.5681	-0.4613***
	(0.2793)	(0.5240)	(0.1409)
Firm-Place Local	-0.4444	-1.308**	0.2027
	(0.2844)	(0.5429)	(0.1401)
Authority-Place Local	-0.7039*	0.0539	-0.4323**
	(0.4183)	(1.106)	(0.2180)
Fixed-effects			
Firm NUTS3-Year	Yes	Yes	Yes
Authority NUTS3-Year	Yes	Yes	Yes
Place NUTS3-Year	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	593,931	233,801	360,128
R^2	0.41591	0.54507	0.28246
Within R^2	0.00562	0.00058	0.01073

Clustered (Firm NUTS3-Year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

have smaller average award values (with 435881 Euro for electronic auctioned and 448783.6 for non-electronically auctioned contracts). The same holds for digital industries with smaller average contract values compared to non-digital industries (428,419 vs. 458,714 euros). Therefore, those results seem to not be merely driven by contract size.

APPENDIX B

Chapter 2

B.1 Restricted vs. Unrestricted Sample

To complement the cross-sectional correlations presented in Section 2.4.4, Table B.1 and Table B.2 extend the analysis to a panel setting at the contracting authority–municipality level, estimated with OLS. Table B.1 shows the full sample, while Table B.2 refers to the selected sample discussed in Section 2.5. The correlation persists in a model with year fixed effects shown in table B.1, Column 1. However, when we add municipality fixed effects (column 2) and additionally control for the population (column 3), the correlation becomes insignificant.

Table B.1: Correlation between Local Procurement and Local Business Tax rate (LBT) in the panel with fixed effects

Dependent Variable:		Share Local	
	Year FE	Municipality FE	Mun. FE with Population
Model:	(1)	(2)	(3)
Variables			
LBT	0.0005***	9.34×10^{-6}	-5.78×10^{-5}
	(3.83×10^{-5})	(0.0001)	(0.0002)
Population			-1.43×10^{-7}
			(1.29×10^{-7})
Fixed-effects			
Year	Yes	Yes	Yes
Procuring Municipality		Yes	Yes
Fit statistics			
Observations	14,093	14,093	11,737
R^2	0.02337	0.34791	0.32735
Within R^2	0.02108	5.44×10^{-7}	4.04×10^{-5}

Clustered (Year) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

This decline in significance suggests that the initial correlation may be driven by unobserved heterogeneity, such as municipality size or other location-specific characteristics. Larger municipalities are likely to exhibit higher LBTs and a greater propensity for local procurement, which can confound the observed relationship. Furthermore, the competitive dynamics between neighboring municipalities—especially under tax competition—may also play a role. Firms in high-LBT areas could be disadvantaged, leading local governments to procure more from firms in neighboring, lower-tax areas where competitive offers are more attractive. Table B.2 presents this correlation between the share of locally awarded contracts and the LBT rate only for contracts that could have been awarded locally and regionally, as discussed in the empirical methodology. Here, the coefficient on LBT is statistically significant throughout all models, still it loses some significance when adding further controls. This suggests that the selection-method of the sample takes care of some of the confounders in Table B.1.

Table B.2: Correlation between the share of domestically awarded contracts and the LBT rate, considering only contracts that could have been awarded locally.

Dependent Variable:	Share Local		
	Year FE	Municipality FE	Municipality FE with Population
Model:	(1)	(2)	(3)
Variables			
LBT	0.0004***	0.0006**	0.0005*
	(4.96×10^{-5})	(0.0002)	(0.0002)
Population			-1.56×10^{-6}***
			(3.29×10^{-7})
Fixed-effects			
Year	Yes	Yes	Yes
Procuring Municipality		Yes	Yes
Fit statistics			
Observations	7,588	7,588	7,188
R^2	0.16006	0.46774	0.45046
Within R^2	0.00678	0.00091	0.00309

Clustered (Year) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

Table **??** compares key characteristics of contracting authorities and contracts between the full estimation sample and the restricted sample used in Section 2.5 and 2.6. The restricted sample includes only contracts for which local and regional suppliers were potentially available. While the restriction reduces the number of municipalities and contracts, the overall characteristics remain similar across samples. Contract-level variables such as average award value and number of offers remain similar, indicating that the restriction does not substantially alter the contract composition. The main difference is that municipalities in the restricted sample are larger and exhibit a higher share of local procurement, consistent with larger municipalities having a greater likelihood of hosting suitable local firms.

Table B.3: Comparison of contracting authority-level characteristics between full and restricted sample

	Mean		SD	
Variable	Full Sample	Restricted Sample	Full Sample	Restricted Sample
Municipalities (Contracting Authorities)				
Debt (per capita)	278.385	273.055	232.636	234.212
Equalization Payment	3,593,720.790	3,779,704.932	3,404,501.210	3,883,115.962
Tax Income (per capita)	448.079	419.584	267.852	277.154
LBT Shifting Factor	359.776	369.174	39.370	44.764
Population	27,360.214	37,127.528	99,831.921	123,288.476
Share Local	3.6%	14%	0.347	0.053
N Procuring Municipalities	3,755	1,802		
Contracts				
Award Value (EUR)	440,517.4	481,475.7	964,178.3	1,031,044
Offers	6.16	6.64	7.77	7.46
N Contracts	214037	144313		

B.2 Direct Impact of the Instrument in the Second Stage

Table B.4 presents the regression results assessing the direct impact of the fitted equalization payment on the number of contracts in the bilateral gravity model using PPML. Column (1) shows that the equalization payment is not a significant predictor of the number of contracts. Column (2) adds the key regressor of interest, the interaction between the local business tax (LBT) rate and the local dummy, while the equalization payment remains insignificant. Column (3) includes the full second-stage specification with the instrumented variable, and Column (4) replaces the instrument with the census shock as a predictor. Across all specifications, the instrument does not significantly affect the number of contracts, suggesting that it operates only through the LBT rate. Although there is no formal test for the exclusion restriction, the results indicate that the instrument does not directly influence the outcome variable, supporting the validity of the identification strategy.

Table B.4: Direct impact of the instrument in the second stage

Dependent Variable:	Number of Contracts			
Model:	(PPML)	(PPML)	(PPML)	(PPML)
Variables				
Census Shock				-6.4×10^{-6}
				(3.94×10^{-6})
Equalization Payment (fitted)	2.63×10^{-10}	1.59×10^{-10}	-6.29×10^{-10}	
	(3.8×10^{-10})	(3.87×10^{-10})	(3.87×10^{-10})	
LBT		-0.0084***	-0.0051***	-0.0051***
		(0.0008)	(0.0013)	(0.0013)
LBT $\times$ Local		0.0227***	0.0242***	0.0242***
		(0.0025)	(0.0026)	(0.0026)
Residuals (u_{it})			-0.0050***	-0.0050***
			(0.0010)	(0.0010)
LBT Difference			0.0023***	0.0023***
			(0.0007)	(0.0007)
Origin Population			$-1.08 \times 10^{-6***}$	$-1.08 \times 10^{-6***}$
			(3.86×10^{-7})	(3.86×10^{-7})
Destination Population			$5.3 \times 10^{-6***}$	$5.3 \times 10^{-6***}$
			(6.95×10^{-7})	(6.95×10^{-7})
Authority GDP			0.0022***	0.0022***
			(0.0008)	(0.0008)
Firm GDP			0.0014	0.0014
			(0.0018)	(0.0018)
Fixed-effects				
Origin-Destination	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	310,353	310,353	310,353	310,353
Squared Correlation	0.77207	0.78010	0.79194	0.79194
Pseudo R^2	0.42229	0.42403	0.42474	0.42474
BIC	707,996.6	706,972.3	706,622.3	706,622.3

Clustered (Origin-Destination) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

B.3 Placebo: Only Non-Local Authorities

In the main sample, we only use public authorities that tend to operate local, as in the mechanism, we want to capture the decision-making on the local level. As an additional placebo test, we reduce the sample to only non-local public authorities. Those are labeled as "Ministry or any other national or federal authority, including their regional of local subdivisions", "National or federal Agency / Office", "European Union institution/agency" and "other international organisation". If the effects in the main analysis were driven by local decision-making and the mechanism of the tax cashback effect, we would not see significant effects in the interaction of the LBT rate with the Local dummy. Table B.5 confirms this.

Table B.5: Results considering only contracting authorities that tend to be non-local, categories include "Ministry or any other national or federal authority, including their regional of local subdivisions", "National or federal Agency / Office", "European Union institution/agency" and "other international organisation"

Dependent Variable:	Number of Contracts
Model:	(1)
Variables	
LBT $\times$ Local	0.0122
	(0.0160)
Local	-0.0516
	(5,811.3)
Fixed-effects	
Procuring Municipality-Winning Firm Municipality	Yes
Year	Yes
Winning Firm Municipality-Year	Yes
Procuring Municipality-Year	Yes
Fit statistics	
Observations	9,436
Squared Correlation	0.98511
Pseudo R^2	0.54327
BIC	135,741.4

Clustered (Procuring Municipality-Winning Firm Municipality) standard-errors in parentheses
** statistically significant at the 10% level, ** at the 5% level, *** at the 1% level*

APPENDIX C

Chapter 3

C.1 Cluster Definition

	NUTS1	Region	Share of funded firms
1	DE1	Baden-Württemberg	0.0208890
2	DE2	Bayern	0.0138296
3	DE3	Berlin	0.0170714
4	DE4	Brandenburg	0.0175150
5	DE5	Bremen	0.0233157
6	DE6	Hamburg	0.0107204
7	DE7	Hessen	0.0127690
8	DE8	Mecklenburg-Vorpommern	0.0203811
9	DE9	Niedersachsen	0.0148482
10	DEA	Nordrhein-Westfalen	0.0148137
11	DEB	Rheinland-Pfalz	0.0132487
12	DEC	Saarland	0.0187119
13	DED	Sachsen	0.0325534
14	DEE	Sachsen-Anhalt	0.0232295
15	DEF	Schleswig-Holstein	0.0106457
16	DEG	Thüringen	0.0302741

Table C.1: Geographical classification used in this paper

	Industry	Share of funded firms
1	Manufacturing	0.0163587
2	Manufacturing	0.0663014
3	Manufacturing	0.0419899
4	Construction	0.0072075
5	Wholesale trade and retail	0.0093676
6	Transport and communication	0.0101057
7	Real estate activities	0.0129624
8	Education	0.0196599
9	Other service activities	0.0218824

Table C.2: Industrial classification used in this paper

C.2 Generating Inverse Probability Weights

On the firm level, we perform logistic regressions for each cluster separately. For each cluster, we use the same set of covariates with pre-treatment values. The set of covariates includes firm age, number of employees, and fixed assets. We regress the treatment dummy d_{ict} on the set of covariates to generate the predicted treatment values $\hat{d_{ict}}$. We obtain the inverse probabilities by calculating the $\frac{1}{\hat{d_{ict}}}$ for treated firms and $\frac{1}{1-\hat{d_{ict}}}$ for non-treated firms. We use these weights afterward in the inverse probability-weighted OLS regressions (Equation 3.3 on the firm-level for each cluster separately).

C.3 Generalized Propensity Score Matching

For the methodology on Generalized Propensity Score Matching, we follow Fong et al. (2018). We start by averaging the firm-level variables across each cluster and defining the treatment variable as the proportion of treated firms within the cluster. This proportion is the average of the treatment dummy on the firm level. The distribution of the treatment variable is skewed to the left, many clusters do not receive any funding or only a few firms receive funding. Therefore, we use the Box-Cox transformation in equation C.1 to transform the distribution of the treatment variable closer to normal. We find the most suitable transformation by computing the distribution correlation between the Box-Cox transformed treatment and the normal distribution. We find the correlation maximizing λ searching on a grid from 150 to -150, where the optimal λ is at -49.

$$\frac{(p_r+1)^{\lambda}-1}{\lambda} \tag{C.1}$$

We then fit four different models, a Maximum Likelihood Estimator (MLE), Generalized Boosted Regression Modelling (GBM), the regular Covariate Balancing Generalized Propensity Score (CBPS), and the non-parametric version of the Covariate Balancing Generalized Propensity Score (npCBPS). Figure C.1 shows the absolute Pearson correlation between each covariate and the transformed treatment variable. The Maximum Likelihood Estimator improves covariate balancing, while GBM, CBPS and the non-parametric CBPS perform better. The CBPS esitmates have the smallest absolute correlation of the covariates. GBM and npCBPS perform slightly worse and come with higher computational effort. Therefore, we use CBPS to estimate Generalized Propensity Scores on the cluster-level. CBPS iteratively optimizes covariate balancing and the treatment prediction resulting in a good model fit and covariate balance while retaining computational feasibility.

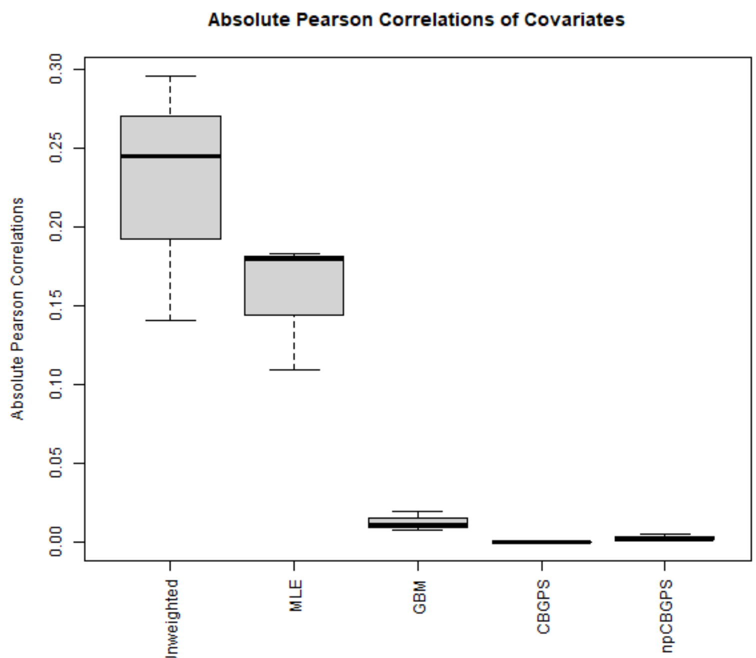
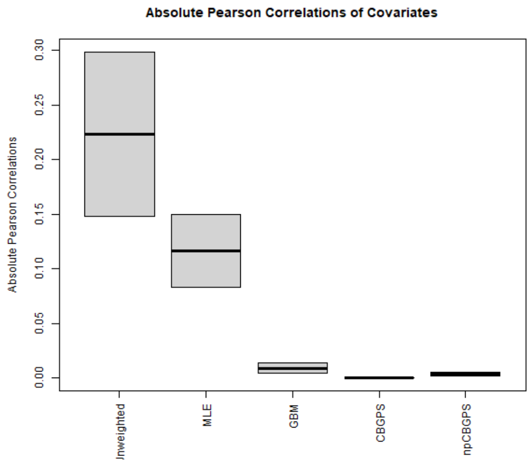


Figure C.1: Covariate balancing propensity score estimation

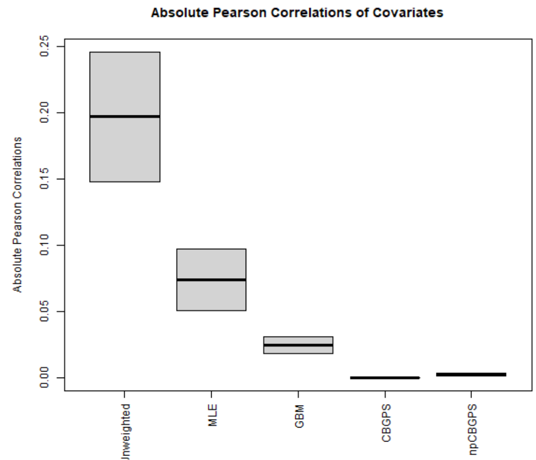
Additionally, we perform the generalized matching procedure on the cluster level with different sets of covariates. To reflect the covariates for the estimations with fixed assets and number of employees as outcomes, we exclude the fixed assets or number of employee variables, respectively, from the generalized propensity score estimation and calculate covariate balancing statistics for the different estimators. Figure C.2 shows that for all sets of covariates, the CBPS estimator minimizes covariate correlation. Therefore, we use CBPS throughout our analysis for all cluster-level generalized propensity score estimations.

C.4 Bootstrapped Standard Errors

As our analysis includes calculating potential outcomes and involves a multi-step methodology, we bootstrap the standard errors for the confidence intervals shown throughout the paper to assess the variability or uncertainty in our treatment effect estimates. We randomly select samples of firms from our dataset and calculate treatment effects for each sample. We do this process 180 times, creating 180 bootstrap replications. For each bootstrap replication, we calculate different types of treatment effects, such as the direct effect $\bar{\gamma}^{10}_{pp}$, the indirect effect $\bar{\gamma}^{00}_{p0}$, and the overall effect $\bar{\gamma}^{10}_{p0}$. These effects are estimated for various levels of the proportion of treated firms. We follow Girma et al. (2015) for calculating the bootstrap standard errors according to equation (C.2). We simplify the notation by using $\bar{\gamma}$ as a placeholder for the different types of treatment effects $\bar{\gamma}^{10}_{pp}$, $\bar{\gamma}^{00}_{p0}$ and $\bar{\gamma}^{10}_{p0}$. $\overline{\overline{\gamma}_B}$ is the mean of the obtained treatment effects



(a) Covariate balancing statistics, excluding fixed assets



(b) Covariate balancing statistics, excluding number of employees

Figure C.2: Covariate balancing statistics: Pearson correlation of covariates

across the bootstrap replications, while $\bar{\gamma}_B$ is the treatment effect obtained from bootstrap replication B.

$$se(\bar{\gamma}) = \sqrt{\frac{1}{180}\sum_{B=1}^{180}(\bar{\gamma}_B - \overline{\bar{\gamma}_B})^2} \tag{C.2}$$