



RESPOND

Rescuing Democracy from Political
Corruption in Digital Societies

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Methodology Report on Novel Forms of Corruption Measurement

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Introduction

This Deliverable offers an update on corruption measurement activities as part of the RESPOND project, based on our comprehensive literature review, consultations with stakeholders, and selected initial data analysis activities. The indicators compiled in this report provide the foundation upon which many subsequent RESPOND research activities, papers and policy briefs will be based. The literature on measuring favoritism, corruption risks, and related activities has grown vast in the last decade, making this report also extensive. The authors compiled a comprehensive literature review surveying the relevant literature while also offering initial empirical research supporting novel indicators and research directions in the domains of legislation, public procurement and intra-government transfers.

In each of the 3 domains of government activity (legislation, public procurement, grants), the report starts by offering foundational definitions delimiting the study topic, then moving on to a comprehensive literature review of measurement approaches and indicators; and finally offering a selected case study demonstrating the kinds of novel corruption measurement activities RESPOND is embarking on. The 3 sections can be read in isolation even though the overall approach to measurement, the selection of the relevant literature and empirical work are similar across.

We believe that the below material offers a rich basis for subsequent research as well as policy analysis with selected promising avenues for filling measurement gaps for more comprehensive corruption risk assessment.

1. Legislation/regulations

1.1 Definitions

Legislation and regulation are analytically distinct but closely intertwined modes of rulemaking in contemporary governance. While legislation is typically associated with acts enacted by legislatures and regulation with rules developed and implemented by executive or administrative bodies, the boundary between the two is often blurred in both theory and practice (Kosti et al. 2019). Due to this conceptual overlap and for reasons of analytical parsimony, we treat legislation and regulation jointly throughout this section.

Lawmaking and regulatory production constitute critical arenas of organizational conflict (Schattschneider 1960) and corporate influence (Ferguson 1995). Firms deploy a range of strategies to shape the statutory environment governing their activities and those of their competitors; such interventions may aim either to influence policy outputs or to prevent policymakers from acting altogether. To the extent that these efforts succeed, legislation or regulation may incorporate provisions that seek to advantage targeted economic actors over others and redistribute competitive advantages. We treat such outcomes as instances of favoritistic legislation or regulation, defined as measures that create or modify legal rules in ways that disproportionately benefit specific firms within an economic sector.¹ Conceptualized in this way, legislative or regulatory favoritism constitutes a specific kind of particularistic policy that focuses on targeted or narrow rather than universal policy appeals (see e.g., Mayhew 1974; Carey and Hix 2003). Unlike typical pork-barrel legislation where the focus is on geographically targeted local public goods (e.g., Ferejohn 1974), favoritistic legislation instead centers on policies intended to benefit more or less narrow *business* constituencies (some firms in a given sector) and provide them with “rents”, such as subsidies and special tax provisions (Cox and McCubbins 2001; c.f. Krueger 1974; Buchanan, Tollison and Tullock 1980).²

The inclusion of favoritistic elements in laws and regulations should not necessarily be interpreted as a deviation from conventional democratic practice or decision-making standards. Across democratic polities, firms – alongside other organized interests such as labor representatives and consumer groups – are widely regarded as legitimate sources of expertise, data, and real-world feedback for policymakers (Bitonti 2025). More generally, favoritism can also result from electoral incentives (e.g., Carey and Shugart 1995; Catalinac 2016; Hee Park and Jensen 2007; McGillivray 1997, 2003; Motolina 2021), disproportionate yet legitimate industry influence (e.g., Rogowski and Kayser 2002), and potentially a public or general interest rationale (e.g., Carpenter 2013, 64).³ Consequently, favoritistic legislation need not be part of a relatively direct quid-pro-quo or clientelistic exchange (Kitschelt 2000; Carrol and Lyne 2007). However, to the extent that favoritistic legislation is actively promoted with the intent of securing personal or political gain from the beneficiaries of such provisions, it more closely approximates what we term corrupt

¹ This potentially includes legislation to change the constitution but does not include legislative motions that do not change the law, such as simple resolutions in the U.S. Congress. We note that this goes beyond the definition of legislative favoritism in Benoît et al. (2025) which focuses on adopted legislation.

² We note that Mayhew (1974, 57) also allows for particularistic policies in the form of tax loopholes that benefit recipients outside the district and may be rewarded with campaign contributions. Similarly, Carey and Shugart (1995) allow for particularistic policies in the form of amendments that benefit particular groups.

³ The argument by Carpenter (2013) is on regulatory actions, but a similar logic applies.

favoritistic legislation. This distinction builds on scholarship that differentiates between individual and institutional conceptions of corruption, where the former centers on intentional exchanges for private benefit and the latter highlights systemic distortions of institutional purposes or procedures (Thompson 2018).

1.2 Corruption Schemes and Indicators

Pathways of Influence in Corrupt Favoristic Legislation

The distinction drawn above between the presence of provisions that advantage certain market actors and corrupt favoritistic legislation or regulation is analytically important, as similar policy outcomes may warrant different normative interpretations. Provisions that de facto advantage certain market actors can emerge as byproducts of routine interactions that serve legitimate institutional functions. Consequently, the mere presence of favoritistic provisions is insufficient to classify a policy as corrupt under our definition. Rather, such a classification requires evidence not only of the advantages conferred, but also of intentional efforts by firms to secure these outcomes and the involvement of identifiable political actors or organizations as targets of influence. In this sense, our understanding of favoritistic corrupt legislation follows the logic of corrupt exchanges as described for example by Della Porta and Vannucci (1999).

Beyond outright bribery – widely regarded as relatively uncommon in advanced democracies (Laffont and Tirole 1993; see also Harstad and Svensson 2011) – the literature identifies three principal channels through which quid pro quo-like exchanges may occur and generate favoritistic outcomes in the lawmaking and regulatory process: (1) lobbying, (2) campaign contributions, and (3) political connections.

(1) *Lobbying* provides the most extensively studied – and analytically most direct – illustration of our distinction. The literature contrasts informational theories of lobbying, in which firms supply policymakers with policy-relevant expertise, with exchange-based accounts that emphasize quid pro quo arrangements (Bombardini and Trebbi 2020). In the former view, lobbying is understood as a mechanism through which organized interests reduce informational asymmetries and may contribute to policy efficiency; in the latter, it is conceptualized as a vehicle for securing preferential treatment in exchange for political support. This distinction closely mirrors our differentiation between favoritistic legislation and corrupt favoritistic legislation. Informational lobbying may generate policy outcomes that advantage specific firms without necessarily implying corruption,⁴ whereas quid pro quo lobbying entails intentional exchanges between firms and policymakers that align more directly with our definition of corrupt favoritistic intervention (see also Grossman and Helpman 1994).

While lobbying can be broadly understood as “any activity oriented towards shaping public policy outcomes” (Drutman 2015, 31), much of the scholarly literature adopts (either implicitly or explicitly) the informational approach by conceptualizing it more narrowly as the communication or transfer of information from interest groups to policymakers (Figueiredo 2002, 126; Leech 2011, 604). This perspective builds on the idea that policymakers often lack detailed knowledge about the sectors they regulate, whereas firms and other organized

⁴ Whether informational lobbying leads to better informed policymaking or results in agenda distortion is however debated in the literature, e.g. see Cotton and Déllis (2016).



interests possess specialized expertise that can inform policy design (Potters and van Winden 1992, 270).

Importantly though, lobbying interactions often extend beyond the neutral provision of expertise. They may also involve “threats, commitments, [and] signals” (Figueiredo 2002, 125). Firms have clear preferences and may present information strategically, framing it in ways that advance their interests. Moreover, while market-based accounts of politics often abstract from ideology, alternative approaches emphasize the role of shared preferences and policy goals in structuring exchanges between interest groups and policymakers (Hall and Deardorff 2006). These perspectives suggest that lobbying encompasses a spectrum of interactions, ranging from the provision of policy-relevant information to more strategic forms of influence rooted in access, relationships, and repeated exchange.

Given the difficulty of empirically disentangling these dimensions, informational lobbying is often treated as encompassing a broad set of communicative and strategic interactions between firms and political actors, some of which may blur into exchange-based dynamics (see also Awad 2024). In this context, other perspectives situate lobbying at the limit of this continuum, where interactions take the form of more direct quid pro quo exchanges. In canonical models of political influence, policy outcomes are understood as the result of exchange relationships in which firms provide political support in return for favorable regulatory or legislative decisions (e.g., Grossman and Helpman 1994; Helpman and Persson 1998). More recent work formalizes this logic by emphasizing how strategic transfers or support can shape policy outcomes through legislative bargaining processes. Although such exchanges are rarely directly observable, a substantial empirical literature documents systematic policy and economic advantages associated with lobbying activity that are consistent with these exchange-based accounts. For example, the number of lobbying contacts has been shown to significantly influence the voting behavior of committees (Wright 1990). It has also been shown that firms that increase their lobbying expenditures in a given year tend to benefit from lower effective tax rates in subsequent periods (Richter et al. 2009). At the firm level, lobbying is also associated with improved financial performance, higher stock returns (Chen et al. 2015) and higher profits (Chalmers and Macedo 2021). Importantly, these effects extend beyond lobbying firms themselves. Using variation in product similarity, Neretina (2019) shows that lobbying generates redistributive effects across firms, with non-lobbying competitors experiencing heterogeneous impacts depending on their proximity to lobbying firms.

While these findings do not provide direct evidence of explicit quid pro quo agreements, they are difficult to reconcile with purely informational accounts of lobbying. Instead, they suggest that lobbying may operate, at least in part, as a mechanism through which firms secure targeted policy advantages, consistent with exchange-based interpretations. In this sense, the empirical literature complements theoretical approaches that conceptualize lobbying as a structured form of influence-seeking behavior, situated between informational provision and more explicit forms of political exchange (see also Harstad and Svensson 2011).

(2) Although they are sometimes treated as a subtype of lobbying (Bombardini and Trebbi 2020), *campaign contributions* are better understood as a specific mechanism of influence, one in which firms directly transfer monetary resources to political actors in the expectation of securing favorable legislative or regulatory treatment. Unlike much of the lobbying literature, which



rarely studies the motivations of policymakers on the receiving end of influence attempts, the literature on campaign finance offers a clearer account of political actors' incentive structures. Campaign contributions provide an immediately intelligible benefit to officeholders and candidates because they are directly tied to electoral competition, political survival, and career advancement. For this reason, campaign contributions also constitute one of the most plausible channels through which quid pro quo exchange may occur.

The empirical literature provides substantial, though not uniform, support for this interpretation. McKay (2020), for example, shows that firm requests submitted to the US Senate Finance Committee were significantly more likely to be incorporated into committee amendments and final legislation when they were preceded by campaign contributions. Fellowes and Wolf (2004) similarly find that aggregate business contributions affect congressional voting, particularly on regulatory and tax legislation, where the connection between legislative choices and business interests is especially direct. These findings are important because they indicate that campaign contributions may shape not only access, but also concrete policy outputs. At the same time, the literature also documents important limits to this mechanism. A meta-analysis conducted in 2003 concluded that only a minority of studies reports statistically significant effects of campaign contributions on legislative voting, and that the estimated effects are generally small (Ansolabehere, de Figueiredo, and Snyder 2003). Fourinaies and Fowler (2022), moreover, show that in some sectors campaign contributions have little or no measurable effect. These findings caution against treating all contributions as straightforward purchases of policy. In part, this may reflect the fact that politicians remain constrained by constituency pressures, party commitments, and institutional rules. It may also reflect strategic behavior on both sides: as Katic and Hillman (2023) argue, firms and politicians alike have reasons to avoid arrangements that appear too explicitly corrupt, since overt quid pro quo exchange carries reputational and legal risks (see further).

Yet these limitations do not weaken the plausibility of campaign contributions as a quid pro quo mechanism so much as they specify the conditions under which this mechanism is likely to operate. The effects of contributions appear strongest where the exchange is politically valuable but difficult to observe directly. McKay (2020) shows precisely that firm-specific requests can be translated into legislative outputs without public visibility of the underlying demand. Fellowes and Wolf (2004) similarly suggest that business money is most consequential in policy domains such as taxation and regulation, rather than in areas involving more visible distributive transfers. In other words, campaign contributions may be most effective not when they buy openly identifiable favors, but when they help secure less visible forms of policy responsiveness. This interpretation is reinforced by the broader literature on strategic political giving. Corporate and trade-group contribution patterns often reflect sophisticated attempts to influence policymakers with whom firms may not substantively agree (Thieme 2020). Likewise, Fowler, Garro, and Spenkuch (2020) find little evidence that firms benefit on average from backing winning candidates, which suggests that campaign contributions do not uniformly generate returns and may be ineffective in many cases. But this finding is fully compatible with a more selective quid pro quo view: returns may be concentrated in particular institutional settings, particular stages of the legislative process, or particular forms of exchange that are difficult to capture through average electoral effects alone. Consistent with this, Großer, Reuben, and Tymula (2013) emphasize that tacit quid pro quo arrangements are hard to observe directly but can emerge under repeated interaction, especially when reciprocity can be informally enforced.



In this vein, Robertson, et al. (2016) argue that ordinary observers frequently perceive common campaign-finance practices as resembling quid pro quo corruption, even when these practices fall short of explicit bribery.

Finally, the relative transparency of campaign contributions in some contexts (particularly in the United States) may itself push firms toward adjacent but less visible financial channels. Because contributions above relatively low thresholds are publicly disclosed, firms may prefer mechanisms that preserve the material logic of quid pro quo exchange while reducing public scrutiny. Bertrand et al. (2020), for example, show that firms strategically direct charitable donations to nonprofit organizations located in the districts of relevant legislators and affiliated with them, and that these donations increase when legislators acquire committee positions of particular value to firms. McKay (2018) likewise finds that fundraising events organized for members of Congress are associated with a greater likelihood that firm requests are introduced as amendments, with effects stronger than those of campaign contributions themselves. These findings suggest that campaign contributions should be understood not in isolation, but as part of a broader family of financial inducements through which firms may reward, cultivate, and activate politically useful relationships.

(3) Besides lobbying and campaign contributions, *political connections* constitute a third (though closely related) channel through which firms may secure policy favors. In this literature, influence is exercised not primarily through the provision of information or the transfer of campaign resources, but through the cultivation, acquisition, or activation of privileged ties to policymakers and regulators. These ties may take several forms: firms may appoint current or former politicians to their boards, hire former legislators and staff as lobbyists, recruit former regulators into executive roles, or otherwise rely on revolving-door movements between public and private office. What unifies these practices is the expectation that personal relationships, insider knowledge, procedural familiarity, and reputational capital can be converted into privileged access, regulatory accommodation, or favorable legislative and regulatory outcomes.

Recent research provides strong evidence that firms treat such connections as valuable assets. Green and Homroy (2022), for example, show that after the United Kingdom removed restrictions that had previously prevented MPs with corporate interests from participating in related parliamentary proceedings, firms responded by increasingly appointing MPs to their boards. Politicians with corporate affiliations subsequently became more active in parliamentary select and joint committees, while connected firms experienced both short-term abnormal stock returns and longer-term increases in firm value. This finding is important because it suggests that firms benefit not merely from the prestige of connected individuals, but from their capacity to intervene in policy-relevant arenas. A broader revolving-door literature reinforces this interpretation. Vidal, Draca, and Fons-Rosen (2012) show that the value of revolving-door lobbyists depends heavily on whether their former political contacts remain in office: when connected legislators leave Congress, the revenue of those lobbyists declines significantly. McCrain (2018) reaches a similar conclusion in showing that former congressional staffers command greater lobbying revenue when they maintain denser networks of staff-to-staff connections. Strickland (2020) demonstrates that the value of revolving-door lobbyists declines as legislative turnover erodes those ties, while Strickland (2023) shows more broadly that former legislators deliver greater value when their insider relationships remain politically relevant. These studies confirm that firms use revolving-door recruitment as a strategy for acquiring



politically productive relationships that can later be mobilized on their behalf. Some of the literature also goes beyond access to show direct policy consequences. Lobbying organizations receive more earmarks and more earmarked funds when they lobby, and that there is an additional positive effect when revolving-door lobbyists are involved (McKay and Lazarus 2023). This is especially relevant because it links political connections not merely to access or market valuation, but to identifiable distributive outcomes. More generally, these findings suggest that political connections matter not only because they open doors, but also because they shape who gets heard, whose proposals are advanced, and whose interests are translated into policy.

Yet the value of political connections is not unconditional. Egerod, Rasmussen, and van der Ploeg (2024) find limited overall evidence that hiring revolvers increases access to the European Commission, though they do identify positive effects in subsets of meetings concerning the broader legislative agenda and among contract lobbying firms. Chalmers et al. (2022), looking at financial regulatory authorities in the European Union, likewise show that repeated movements through the revolving door are concentrated among individuals whose careers center on law, policy, and government affairs, indicating that the value of such trajectories depends on the accumulation of specialized expertise and durable network ties. These studies therefore qualify, but do not undermine, the broader conclusion that political connections can serve as meaningful channels of influence for firms. Broader comparative evidence shows that the value of political connections also varies across institutional contexts (see Olar 2025). For instance, Li (2023) finds that in China revolving-door recruitment became costly to firms during an anticorruption campaign, as political ties turned from assets into liabilities. Baturo and Arlow (2018), by contrast, find a more limited revolving-door pattern in Irish politics, though they still identify a nontrivial movement of former politicians into consulting, lobbying, and corporate board positions. These findings suggest that the effects of political connections depend on institutional structure, political risk, and the credibility of connected actors.

Overall, the literature supports the view that political connections are consequential channel of influence by embedding firms in durable networks of public authority. Their effects may be mediated by turnover, institutional design, transparency rules, or political shocks, but the underlying mechanism is clear: firms seek to transform proximity to officeholders, regulators, and former insiders into preferential access and, under favorable conditions, into advantageous policy outcomes. However, influence channels should not be understood in isolation. A growing literature shows that firms often combine lobbying, campaign contributions, and political connections in order to increase the effectiveness of their political interventions. For instance, recent research establishes that requests for meetings are significantly more likely to be granted when accompanied by information indicating prior campaign support (Kalla and Broockman 2016). Large-scale evidence similarly shows that campaign contributions increase the likelihood of subsequent lobbying directed at the same politicians (Kim et al. 2026). Bertrand et al. (2014), moreover, suggest that patterns of interaction are shaped at least as much by relational ties between legislators and lobbyists as by the substantive content of policy issues.

Legislative Risk Factors

Identifying channels of influence is only a first step in explaining how firm-politician exchanges are translated into policy outcomes. Indeed, detecting undue influence requires attention not only to the exchanges linking firms and political actors, but also to the institutional mechanisms

through which those exchanges are transformed into concrete statuses or regulatory frameworks. The central question here is how and under what conditions policymakers insert, preserve, or shield favoritistic provisions. The discussion that follows focuses first on legislation, before extending the same approach to regulation.

Because undue influence may be exercised at different stages of the lawmaking process, the first analytical task is to disaggregate that process into its constituent steps – as favoritistic provisions may be introduced at drafting, amendment, review, or implementation stages, and later preserved through procedures that limit revision or contestation.

A second important consideration is that favoritistic provisions are unlikely to be introduced most often through routine lawmaking procedures. Because favoritism imposes potential political, reputational, and potentially legal costs, policymakers seeking to advance such provisions have incentives to rely on legislative or regulatory routes in which scrutiny is reduced. To be sure, favoritistic provisions may sometimes survive ordinary and visible procedures. Yet the checks and balances ordinarily present in standard lawmaking processes – committee review, debate, amendment, public consultation, and administrative oversight – make their insertion and concealment more difficult.

The analytical task, then, is not only to disaggregate the stages of the legislative and regulatory process, but also to account for variation in the scrutiny applied at each stage. Put differently, our argument is that conditions of reduced scrutiny should be treated as risk factors heightening the probability of favoritistic policy making.

Table 1.1 – Legislative indicators

Indicator group	Indicator Name	Measurement
Parliamentary scrutiny	Emergency procedure	Use of restricted procedures or compressed parliamentary debates
Parliamentary scrutiny	Extraordinary legislative session	
Parliamentary scrutiny	Legislative Initiator	MP versus government
Parliamentary scrutiny	Committee involvement	No/few committees involved
Parliamentary scrutiny	Stages	Reduced number of parliamentary stages
Parliamentary scrutiny	Plenary debate (duration)	Plenary debate duration relative to bill length
Parliamentary scrutiny	Plenary debate (size/extent)	
Parliamentary scrutiny	Parliamentary debate timing	Total parliamentary debate duration



Parliamentary scrutiny	Amendments	Number of tabled amendments Length/size Timing (last minute amendment) Wording similarity with interest groups needs
Parliamentary scrutiny	Final vote timing: suspicious	Held during national holidays, important events, or late hours
Parliamentary scrutiny	Final vote timing: weak legitimacy	Number of legislators participating in the vote
Parliamentary scrutiny	Modifications of procedure rules	Rules that reduce scrutiny
Parliamentary scrutiny	Legislative overload	Number of laws passed (monthly quarterly/yearly)
External scrutiny	Lack of public scrutiny	Limited public consultations or draft-text disclosure
External scrutiny	Lack of technical scrutiny	Absence or low quality of regulatory impact assessment
Law/bill lifecycle	Quick modifications	Average annual number of modifications
Law/bill lifecycle	Frequent modifications	Average annual number of modifications
Law/bill lifecycle	Unusual modification	Distance between the modifying and modified laws in the network of laws
Law/bill lifecycle	Unusual/typical modification	Weighing factor for the other indicators
Law/bill lifecycle	Unusual lack of modifications of laws	Deviation from average modification frequency
Law/bill content	Omnibus legislation	Size of the law/number of affected sectors
Law/bill content	Skeleton legislation	Legislation that contains a large number of modifications
Law/bill content	Law text targeting: specificity of language	See Motolinia 2021
Law/bill content	Law text targeting: mentioning of specific firms or individuals	Occurrence of company names or legal entity markers
Law/bill content	Law text targeting: topics catering to narrow interests	See Motolinia 2021

Parliamentary metadata	Transparency of parliamentary data and activities	Share of information published on legislative website
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Our indicators, which are summarized in Table 1.1, can be grouped into three broad dimensions. The first concerns internal (here parliamentary) *scrutiny*, that is, the extent to which a legislative or regulatory decision is exposed to deliberation, amendment, oversight, and contestation within formal decision-making arenas. The second concerns *external scrutiny*, meaning the possibility for review by actors outside the immediate decision-making process, including the media, civil society, and technical or legal experts. The third concerns the *law/bill text lifecycle and content*, namely the features of the text itself and its subsequent trajectory over time, including amendment patterns, legal stability, and implementation predictability.

The indicators proposed here are designed to travel across institutional contexts. Legislative rules differ substantially across systems, especially with respect to committee powers, minority rights, executive agenda control, and amendment procedures (Döring 1995; Cox 2000; Shugart 2008). Yet important differences also exist within a given legislature. Even where formal rules remain constant, some proposals are exposed to extensive committee review, consultation, and amendment, whereas others proceed under compressed timelines, restricted debate, or expedited procedures. Policymakers therefore operate within a set of procedural tools and agenda powers that can be deployed selectively (Cox 2000). Fast-track provisions, committee gatekeeping, bundling practices, negative agenda control, amendment restrictions, guillotine motions, and delegated rulemaking all affect the degree of scrutiny applied to a decision (Huber 1992; Gailmard and Jenkins 2007; Calvo and Sagarzazu 2011; Fong 2022). For present purposes, the relevant question is not only how these instruments redistribute influence inside government (the focus of much of the existing literature on these procedures and instruments), but how they affect the probability that favoristic provisions can be inserted, preserved, or shielded from detection.

(1) The first set of indicators focuses on *parliamentary scrutiny*. The underlying intuition is straightforward. Favoristic provisions are more likely to escape detection where review is compressed, bypassed, or strategically limited. One obvious indicator is the use of extraordinary or emergency procedures, including unusually short debate. Such procedures may be justified under exceptional circumstances. Yet they also reduce the time available for legislators, parliamentary staff, journalists, and affected actors to identify hidden distributive effects. OECD (2025) thus treats legislation enacted within ten days of introduction as exceptional. Although the empirical literature has not yet shown systematically that policymakers use fast-track procedures specifically to confer favors on particular firms, recent work does show that extraordinary procedures can be used to pass measures affecting economic actors without a clear justification for bypassing ordinary scrutiny (Čellár 2022; Bárd and Kazai 2022). Accordingly, the use of emergency procedure should be treated as a clear risk factor, particularly where the underlying decision concerns market regulation, taxation, subsidies, procurement, or licensing.

A related indicator is the use of an extraordinary legislative session. The logic is similar. Extraordinary sessions may be called for legitimate reasons, but they also alter visibility, compress preparation time, and potentially reduce the range of actors able to monitor developments (see Waismel-Manor et al. 2024). The origin of the proposal also matters.



Government-sponsored bills usually endure substantial ex ante coordination within ministries, executive offices, coalition structures, and party organizations before formal introduction (Martin and Vanberg 2004). This internal (administrative) vetting can resolve technical ambiguities and distributive conflicts before a bill ever reaches the floor. By contrast, proposals introduced by individual legislators may enter the process with fewer centralized safeguards. This does not imply that government bills are inherently less risky or that member bills are inherently more suspect. It does mean, however, that the point at which scrutiny occurs differs. Proposal origin can therefore serve as a useful contextual indicator, especially when read alongside more direct measures of review.

The more direct indicators concern the extent and structure of parliamentary review itself. The absence of committee scrutiny, limited committee involvement, or unusually few legislative stages are all relevant warning signs. Committees remain one of the core institutional sites in which ambiguities, exemptions, technical flaws, and distributive side effects are identified and contested (Gilligan and Krehbiel 1987; Krehbiel 1992). Where that stage is truncated or skipped, the probability that problematic provisions remain unchallenged increases. Likewise, the size and duration of plenary debate can themselves be informative. Debate is not meaningful simply because it occurs; its extent should be considered in relation to the length, technicality, and distributive significance of the proposal. A very limited debate on a lengthy or highly specialized text may indicate weak review. The timing of debate also matters. Debate scheduled at unusual hours, near recess periods, or under conditions of low attention may reduce participation and visibility. Similar considerations apply to final vote timing, for instances when votes are held late at night or under severe time pressure. The number of legislators participating in the vote is also itself informative, as low participation may reflect reduced salience or deliberate avoidance of contestation.

Finally, amendments, too, deserve close attention within this first dimension. The number of tabled amendments can matter, although its interpretation depends on context: a high number may indicate vigorous scrutiny, but it may also signal strategic complexity or overload. The length or size of amendments is often more clearly informative, since long amendments are harder to review in time. Timing is again crucial. Last-minute amendments sharply reduce the capacity of legislators, media actors, and outside experts to assess their effects. The literature on congressional procedure and omnibus bills has repeatedly emphasized the importance of late-stage amendment and bundling as techniques through which scrutiny may be weakened (Sinclair 2017; Gluck, O'Connell, and Po 2015). The content of amendments may also matter. Where amendment wording is highly similar to proposals publicly advanced by business associations, lobbying organizations, or specific firms, this may indicate a closer linkage between organized interests and legal text. Such an indicator requires textual analysis and external data, but it would be especially valuable where available. Even modification of the procedural rules themselves during the process should be treated as suspicious, because procedural change can be used strategically to alter review conditions for a particular decision.

At a more aggregate level, parliamentary scrutiny may also be weakened by legislative overload. Even if formal procedures remain intact, a high volume of legislative output in a given period can strain the oversight capacity of legislators, committees, and other reviewers. Rozenberg (2021) notably argues that legislative overload weakens effective monitoring because institutions face an informational burden, they cannot adequately process information. In such cases, scrutiny is



reduced in practice rather than formally. This matters because favoritism need not depend on dramatic procedural exceptionalism; it may also emerge under ordinary but congested conditions, where too many decisions move too quickly for meaningful review to occur.

(2) The second group of indicators concerns *external scrutiny*. Even where internal scrutiny is weak, outside review can expose problematic provisions and raise the political or reputational costs of maintaining them. External scrutiny has at least two analytically distinct forms. The first is public scrutiny: media attention, civil-society monitoring, public contestation, and general visibility. The second is technical scrutiny: review by legal experts, specialized agencies, auditors, consultative bodies, and other actors capable of identifying hidden distributive effects or implementation problems. These two forms do not always move together. A decision may be politically visible but technically opaque, or highly technical but publicly invisible. In either case, the absence of scrutiny raises risk. This point follows directly from a large literature on information, oversight, and legislative quality. Committees, hearings, and open debate can force clarification of statutory language and expose distributive trade-offs before adoption; when those channels are absent, uncertainty is shifted into the implementation stage (Epstein and O'Halloran 1999; Huber and Shipan 2002; Bawn 1995). External scrutiny is therefore not merely a democratic norm; it is part of the informational environment through which hidden favors become detectable.

(3) This leads to the third dimension – namely the law/bill text lifecycle and content. Favoritism is not always evident at the process of enactment. It may become visible only through the subsequent amendment history of the decision or the features of the legal text. One important set of indicators therefore concerns post-enactment modifications. Legislative predictability refers to the stability of enacted laws and regulations over time. Where decisions are clear, precise, and internally coherent, there is relatively little need for repeated revision, apart from changes induced by new external conditions. By contrast, rapid or frequent amendment may signal poor initial quality, unresolved conflict, deliberate ambiguity, or efforts to correct provisions that were inserted without adequate review. Fazekas et al. (2024) treat instability as an important governance signal, while Brenner and Fazekas (2023) show that laws exposed to stricter scrutiny are amended less frequently after adoption. This does not mean that all ex-post modification is problematic. It does mean that quick modifications, frequent modifications, and unusual modification patterns are useful indicators of heightened risk. In some cases, even an unusual lack of modifications may be informative, particularly where a technical or politically sensitive domain suddenly becomes highly stable, despite changing external conditions. In such cases, the question is whether the enacted framework is serving a narrow set of interests well enough to avoid correction.

A second set of lifecycle indicators concerns law or bill content. Some textual structures are intrinsically harder to scrutinize. As suggested before, omnibus legislation, common in various institutional settings (typically at the European Union level) bundles multiple provisions or policy issues into a single package, making it harder to isolate the effects of particular clauses. On the opposite side of this continuum, skeleton legislation that leaves substantial or substantive discretion to future implementation mechanically shifts politically important choices away from the primary moment of legislative review (see McCann and Shipan 2021). These forms are not necessarily improper, but both reduce visibility and complicate contestation. More direct indicators concern targeting in the legal text itself. One is the specificity of language: unusually

narrow or tailored provisions may indicate that a decision is designed to fit a small number of actors (see Motolinia 2021). A stronger indicator is the explicit mention of specific firms or individuals, though such direct targeting is likely to be rare in advanced democracies. More common are provisions addressing topics that cater to narrow interests while remaining formally general. Content-based indicators are particularly important because they focus on the text itself rather than only on the process that produced it.

Regulatory Risk Factors

The same three-dimensional framework can be extended to regulation (Table 1.2). Regulatory risk factors can likewise be organized around internal scrutiny, external scrutiny, and the regulatory lifecycle. Internal scrutiny concerns the degree of review that occurs within the executive or administrative process, including the type of regulatory instrument used, the number of procedural stages, the extent of inter-agency review, and the use of accelerated or exceptional procedures. External scrutiny concerns the extent to which draft regulations are exposed to public consultation, technical evaluation, impact assessment, and, where relevant, parliamentary review or oversight. The lifecycle dimension captures what happens after adoption, including the timing of withdrawal, amendment, repeal, or judicial challenge.

Table 1.2 – Regulatory risk factors

Indicator group	Indicator Name	Measurement
Executive scrutiny	Regulatory instrument type	Decree, statutory instrument, rule, ordinance, guidance, etc.
Executive scrutiny	Originating body type	Ministry, agency, regulator, executive office, delegated body
Executive scrutiny	Number of procedural stages	Count of formal stages before adoption
Executive scrutiny	Use of accelerated procedure	Whether adopted under emergency or expedited procedure
Executive scrutiny	Inter-agency review	Whether reviewed by other executive or advisory bodies
External scrutiny	Draft publication	Whether draft text was published before adoption
External scrutiny	Existence of public consultation	Whether consultation occurred
External scrutiny	Consultation period duration	
External scrutiny	Volume of consultation comments	Total number of submitted comments
External scrutiny	Consultation openness	Open consultation vs. targeted consultation
External scrutiny	Response to consultation	Whether authority published responses or summary of comments



External scrutiny	Existence of impact assessment	Whether an impact assessment was conducted
External scrutiny	Quality of impact assessment	Presence of cost-benefit analysis, alternatives, distributional effects, etc.
Parliamentary oversight	Existence of parliamentary review	Whether parliament or committee reviewed the regulation
Parliamentary oversight	Intensity of political oversight	Hearing, debate, objection, approval, annulment power
Parliamentary oversight	Timing of political review	Pre-adoption or post-adoption review
Regulation lifecycle	Draft withdrawal or suspension	Whether draft was withdrawn, paused, or substantially delayed
Regulation lifecycle	Regulatory status	Still in force, amended, repealed, withdrawn
Regulation lifecycle	Time to first amendment	Days until first modification
Regulation lifecycle	Amendment frequency	Number of amendments over a defined period

The rationale is the same as in the legislative setting. Favoritism in regulation is unlikely to be introduced most often through highly visible and fully scrutinized processes. Where public decisions advantage specific firms or narrow economic actors, policymakers and regulators have incentives to rely on routes in which scrutiny is weaker, faster, or less transparent. In the regulatory domain, this may involve low-visibility instruments, limited consultation, weak technical review, or procedural compression. For that reason, the indicators listed in the table should be understood as risk factors rather than proof. No single feature conclusively demonstrates favoritism. However, the coexistence of multiple indicators across these three dimensions increases the likelihood that a regulation was developed under conditions in which favoritistic provisions could be inserted, preserved, or shielded from detection.

1.3 Case-study: Legislative Procedural Scrutiny and Firm Performance in the UK

Legislation and regulation routinely advantage some market actors over others. Yet inferring the existence of a corrupt favoritistic exchange is empirically far more demanding. Doing so requires more than identifying a favorable policy outcome. It also requires tracing a source of influence on the firm side, identifying a political actor who actively sought to advance that firm's interests, and linking both to a concrete legislative or regulatory outcome. In other words, the empirical challenge lies in reconstructing the full causal chain connecting firm-level influence attempts, political action, and legally consequential policy outputs.

In practice, however, scholars often face substantial obstacles in recovering that chain in full. In some cases, there is evidence of firm influence – through lobbying, donations, or political connections – but little direct evidence that policymakers acted to translate those pressures into favoritistic legal provisions. In others, a suspicious legislative or regulatory outcome may be observable, but the underlying influence channel remains opaque. More generally, one or more



elements of the exchange may be missing from the available record: the firm's strategy may be visible while the political response is not; the political response may be visible while the intended beneficiary is uncertain; or both sides may be partly observable without a clear link to the final policy text. These difficulties are especially acute where influence is exercised through informal access, low-visibility procedures, or routinized interactions that leave few explicit traces.

One way to address this problem is to adopt a more indirect empirical strategy. Rather than requiring conclusive observation of every component of the exchange, such an approach identifies combinations of risk factors and policy outcomes that make corrupt favoritistic intervention more plausible. It then becomes possible to assess whether laws and regulations enacted under conditions of reduced scrutiny are associated with systematically uneven distributive consequences, notably where firms already well positioned within their sectors benefit disproportionately relative to their competitors. Rather than reconstructing the full exchange directly, this strategy evaluates whether the procedural risk factors identified above coincide with outcomes that are consistent with favoritistic intervention.

This is the strategy illustrated in this section. Using the United Kingdom as a case study, we show how the framework developed above can be used to investigate likely instances of corrupt favoritistic legislation or regulation even where the full causal chain cannot be directly observed. In a nutshell, our approach links detailed legislative-process data for all enacted laws between 2009 and 2015 to firm-level financial data covering all registered companies, in order to examine whether laws adopted under higher favoritism risk are associated with systematically uneven market outcomes. Rather than attempting to identify concealed exchanges directly, the analysis uses observable features of the legislative process as proxies for favoritism risk – specifically, excessive legislative production, limited parliamentary scrutiny, frequent amendment, and the use of non-standard procedures. These indicators are then matched to sector-level legal change and related to variation in firm profits, revenues, costs, and profit margins through firm-level fixed-effects models.

The United Kingdom provides a particularly useful setting for this analysis. It combines a long-established parliamentary democracy, an advanced market economy, and unusually extensive transparency around both legislative activity and firm-level economic data, making it possible to connect lawmaking processes to market outcomes at scale. The study draws on legislative data covering all enacted laws in the UK and matches these to sectoral classifications and firm-level financial information for the 2009-2015 period. This design allows the analysis to move beyond case-based evidence and examine whether laws adopted under our high-risk conditions leave systematic traces even in a high-accountability democratic setting. In that sense, the UK is not treated as an extreme or anomalous case, but rather as a demanding test: if laws passed under weak scrutiny are associated with concentrated gains for selected firms even here, the broader concern with legislative favoritism is likely to extend beyond more overtly clientelistic or weakly institutionalized systems.

The analysis examines whether the same legislative environment has systematically different effects on firms with distinct prior growth profiles by interacting the scrutiny indicators with categories for anomalous high-growth and anomalous low-growth firms, using medium-growth firms as the reference group. This makes it possible to test whether laws passed under weaker scrutiny disproportionately benefit some firms while disadvantaging others operating in the

same sector. To approximate causal effects, the models include firm and year fixed effects, and economy-wide legislative controls. The results point to a consistent redistributive pattern (Figure 1.1).

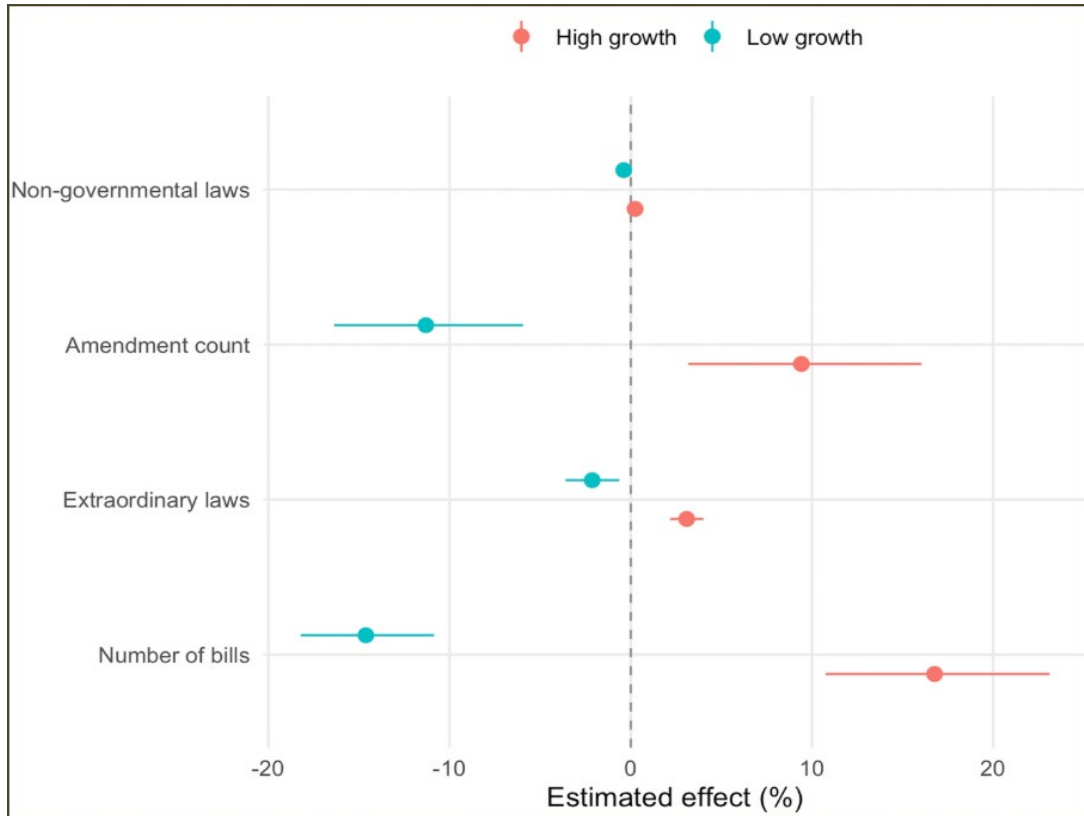


Figure 1.1 – Estimated profit effects of weak legislative scrutiny

Note: Points represent estimated coefficients transformed into percentage effects; whiskers denote 95% confidence intervals. Estimates are derived from firm-level fixed-effects models using annual UK firm data for 2009-2015. All specifications include firm fixed effects, year fixed effects, and controls for economy-wide legislative activity through the multisector counterparts of the four legislative scrutiny indicators. Legislative indicators are measured at the sector-year level and lagged by one period relative to firm outcomes. Standard errors are clustered at the sector level. Reported effects for anomalous high-growth and anomalous low-growth firms are estimated relative to medium-growth firms. The figure reports estimated effects on log profits.

Across all four indicators of weak legislative scrutiny, anomalous high-growth firms systematically benefit relative to medium-growth firms, while anomalous low-growth firms are disadvantaged. The strongest effects on profits are associated with legislative volume and amendment intensity: one additional sectoral law is linked to roughly 16-19 percent higher profits for anomalous high-growth firms and 16-21 percent lower profits for anomalous low-growth firms, while ten additional amendments are associated with profit gains of about 9-17 percent for the former and losses of 12-21 percent for the latter. Extraordinary laws and non-governmental bills show smaller but still asymmetric effects.

The mechanism analysis suggests that these profit effects operate primarily through revenues rather than cost reductions (Figure 1.2). Weak scrutiny appears to increase revenues and profit

margins for anomalous high-growth firms while reducing both for anomalous low-growth firms, indicating that legislation passed under reduced scrutiny tends to redistribute market advantages within sectors rather than raising sector-wide performance overall.

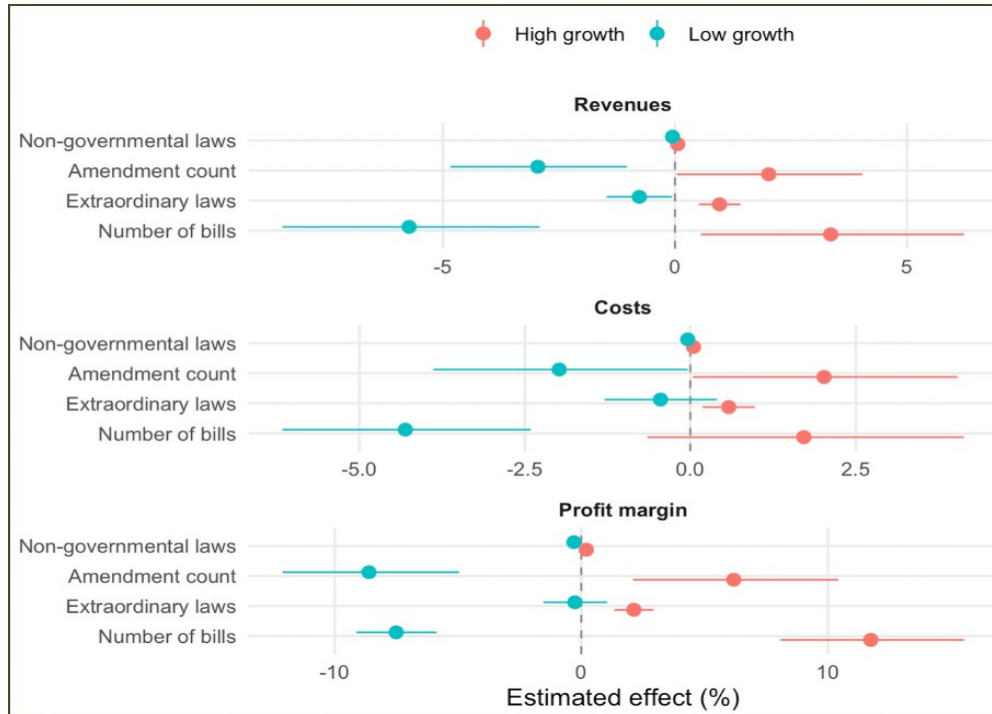


Figure 1.2 – Estimated channel effects of weak legislative scrutiny

Note: Same specification and estimation strategy as in Figure 1.1. This figure reports estimated effects on log revenues, log costs, and log profit margins.

To be sure, the UK study does not satisfy the demanding evidentiary standard required to demonstrate the existence of corrupt favoritistic legislation in a specific instance. It does not directly identify a particular firm as the source of influence, a particular political actor as the recipient, and a fully reconstructed chain linking that exchange to a concrete legislative outcome. What it does show, however, is a clear and systematic association between our legislative risk factors and the accrual of economic advantages to firms already well positioned within their sectors. In that sense, the findings are consistent with the broader idea that legislation adopted under conditions of reduced scrutiny is associated with distributive outcomes that favor a subset of firms over their competitors.

These results admit at least two interpretations, which are not mutually exclusive. One is that reduced legislative scrutiny creates greater uncertainty and shorter adjustment windows, thereby disproportionately benefiting firms with stronger legal capacity, better regulatory monitoring, and greater organizational resources. On this reading, the observed gains reflect uneven capacity to anticipate and exploit legislative change. The other interpretation is more directly political: reduced scrutiny may also make it easier for selective advantages to be incorporated into legal texts through exemptions, narrow definitions, delegated implementation, or low-visibility clauses. A large literature suggests that organized interests adapt strategically to procedural opportunities, and that concentrated interests are more likely



to shape policy where monitoring is weak (Denzau and Munger 1986; Balla 2001; Pedersen et al. 2014; Fourirnaies and Hall 2018; Stigler 1971; Peltzman 1976). Recent work likewise finds measurable advantages for politically connected firms in fiscal and regulatory domains (Fisman 2001; Faccio 2006; Claessens, Feijen, and Laeven 2008).

The UK results do not allow us to adjudicate conclusively between these mechanisms. They do show, however, that reduced scrutiny is not distributively neutral. This is precisely the value of the risk-factor approach. Its purpose is not to prove favoritism through any single variable, but to identify the institutional conditions under which favoristic outcomes become more plausible. No single indicator can demonstrate that a legislative or regulatory decision is favoristic. Yet where weak scrutiny, procedural compression, instability, and other warning signs coincide with concentrated gains for already advantaged firms, concern becomes harder to dismiss. Read in this way, the UK case study does offer strong indirect evidence that the procedural conditions identified in this report are associated with patterns of market advantage consistent with favoritism.

2. Public procurement

For many years, most quantitative corruption indicators were drawn from non-administrative sources, in particular expert assessments (Andersson and Heywood 2009; Kaufmann et al. 2010; Rose and Peiffer 2012). However, the measurement landscape has evolved to include a range of other approaches and indices. As a result, different corruption indicators fall into the following broad categories (adapted from Fazekas and Ferrali (2023)):

- Expert scoring (e.g. V-Dem or Transparency International's CP)
- Public opinion surveys (perception, experience) (e.g. World Bank Enterprise Survey)
- Enforcement-based indicators (e.g. Cordis and Milyo, 2016; Escresa and Picci, 2015)
- Proxy indicators such as
 - Gap-based indicators of public spending (e.g. Olken, 2006)
 - *Public procurement risk indicators* (Main focus below)
 - Company risk indicators (e.g. Caneppele et al., 2009)
 - Personal connections and conflict of interest indicators (e.g. Boas et al., 2014; Amore and Bennedsen, 2013)
 - Legislative favoritism metrics (see section 1 above).

Considering this wide list, the below sections concentrate on those indicators which rely on administrative data and are tailored to public procurement transactions. A range of systematic literature reviews have been published in recent years on indicators and methods of corruption measurement in public procurement (e.g. dos Santos et al, 2025; Lyra et al, 2022; Fazekas et al, 2018). We draw on these studies but convey a broader range of articles, also including policy and grey literature. In addition, we organize the material specifically to support indicator building and subsequent research making use of such indicators.

2.1 Defining corruption in public procurement

In public procurement, corrupt rent extraction typically requires favoring the connected winning supplier throughout the procurement process, in order to secure control over the distribution of benefits. Favoring select suppliers typically takes place during the tendering phase when suppliers are selected, either due to initial budgeting decisions (e.g. deciding to buy products with a unique seller) or during the tendering and award decision (e.g. unfairly scoring the connected bidder higher than its competitors; or using direct award procedures without sufficient justification). Hence, to create an objective measure suitable for cross-country and over-time analysis, indicators typically capture practices that undermine competition. Nevertheless, favoritism and rent extraction can also take place when budgeting and competition appears fair and impartial because corrupt deals can also be pushed to the implementation phase (Dávid-Barrett and Fazekas, 2020). In such corrupt schemes, the favored bidder wins competitively, knowing that it will not be sanctioned for not delivering according to its written offer or relying on inside knowledge about future implementation details (e.g. required quantities changing during the implementation process) (World Bank, 2009). Corrupt public procurement transactions are typically orchestrated by high-level, well-organized actors in an institutionalized and recurrent fashion due to the high value of rents and the need for developing elaborate detection evasion techniques (Fazekas et al, 2016). The very nature of these corrupt transactions imply that regulatory loopholes and discretionary public resource allocation decisions are exploited for corrupt ends; hence, corruption is commonly against the



principles of open and fair public procurement while being strictly speaking non-illegal, especially not in a criminal law sense.

Indicators of corruption risk in public procurement are most suitably organized according to how they relate key conceptual elements of corrupt transactions (Figure 2.1): i) Tendering risks; ii) buyer risks, and iii) supplier risks. In addition, political connections linking suppliers and buyers also play a major role; however, we do not review them here as they are looked at by Work Package 4 and 5 of the RESPOND project (for an in-depth review see: Fazekas et al, 2018). Tendering risks can be further broken down according to the stages of the tendering cycle, such as the submission/tendering, evaluation/award, and implementation phases. Beyond process-related risks, both contracting authorities (buyers) and awarded firms (suppliers) can display traits associated with corrupt arrangements (Fazekas et al, 2018). Although the contract itself represents the mechanism through which rents are captured and distributed, buyers control the full procurement cycle - from drafting specifications and evaluating offers to overseeing contract delivery. Suppliers, meanwhile, serve as the main channel through which illicit rents are realized. The relationship between the contracting body and the winning bidder - often a particularistic or preferential tie - forms an additional distinct category of corruption risk.

Beyond classifying and grouping corruption risk or proxy indicators according to the substantive characteristics of the underlying corrupt behaviors, it is also important to understand how close the approximations are to the corrupt transactions (direct vs indirect, structural conditions) and whether they capture causes or enabler versus effects or impacts of such transactions. Specifically, *direct* measures represent immediate proxies of corrupt practices, directly capturing illicit transactions themselves even if some are only risk factors or red flags. Examples include repeated buyer-supplier interactions, supplier concentration, or criminal charges against officials. *Indirect* indicators, by contrast, reflect broader environmental or structural conditions that only shape corruption exposure, such as a supplier being located in regions with high corruption risk or weak institutional oversight. In turn, *causes* of corruption are micro-level factors that explicitly drive or limit illegal acts, as seen in the levels of meritocracy or institutional autonomy within a public buyer. Finally, corruption *effects* embody micro-level consequences of corrupt transactions – for instance, high wealth of the official in charge of procurement or abnormal returns of firms-suppliers.

Crucially, while the literature on corruption risk indicators using administrative data has greatly expanded in recent years, data constraints remain problematic in many countries and for many specific indicators (Poltoratskyia and Fazekas, 2024). Many administrative datasets are limited in scope vis a vis the total public procurement spending, while others systematically lack key variables for risk flagging such as organization IDs or payments data; and a wide set of countries' datasets suffer from missing and incorrect values in spite of extensive regulations and e-procurement systems implemented.

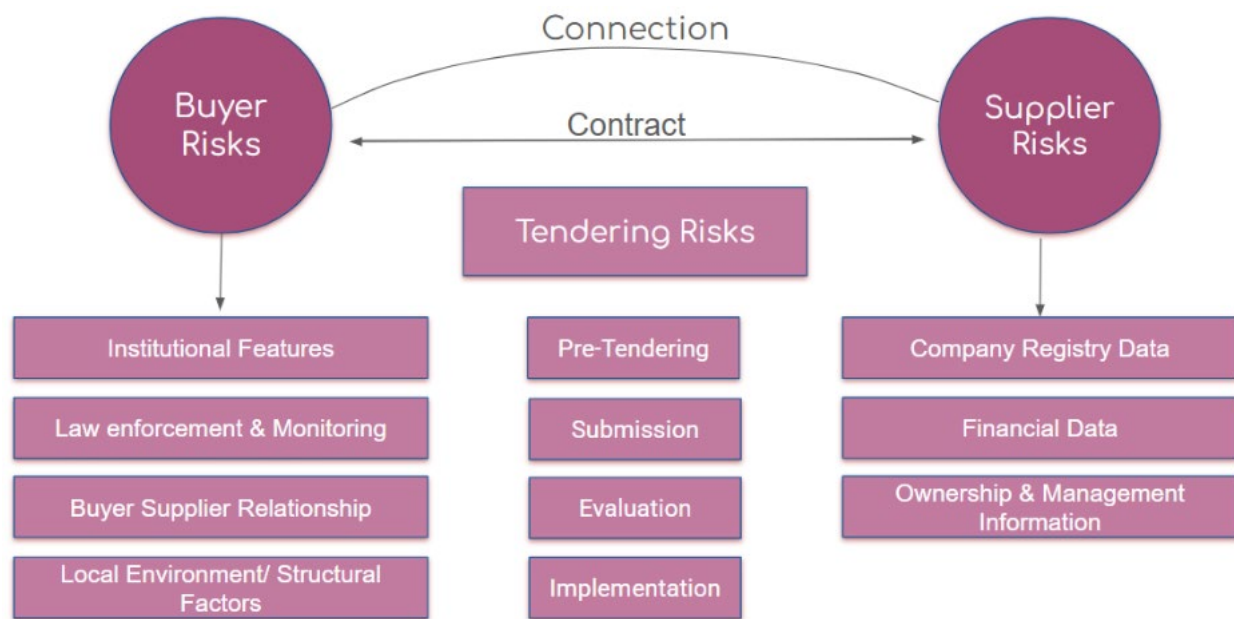


Figure 2.1 – Stages of public procurement process

2.2 Corruption schemes and corresponding indicators

2.2.1 Tendering risk indicators

Risk indicators are often developed to capture corrupt behaviors specific to each public procurement phase (Figure 2.1), though many studies address multiple stages simultaneously by combining indicators. All tender risk indicators are by definition *direct proxy* measures of corrupt transactions in the procurement process.

The **pre-tendering phase** can involve inflating or falsifying project needs or budgets to justify unnecessary contracts and to increase contract value. Moreover, defining needs which can only be met by one or very few suppliers is another sure way to incorporate favoritism in the procurement process early on. Unfortunately, manipulations at this stage are very hard to detect using quantitative data.

The **submission phase** focuses on mechanisms that restrict competition among actual or potential bidders, such as shortened advertisement periods, non-open procedures, and the absence of publication of the call for tenders. At this stage, corruption schemes can manifest in several coordinated ways. A common tactic involves advertising tenders with deliberately shortened submission periods, leaving insufficient time for most potential bidders to prepare and submit proposals, except for a pre-selected company that has been informally notified in advance. Another approach is to craft highly specific or tailored technical specifications that only the favored supplier can fulfill, effectively excluding competitors. In some cases, the contracting authority may bypass open competition altogether by using non-open procedures, such as negotiated or restricted tenders, under the pretext of urgency or special circumstances. These methods create the appearance of legality while ensuring that only a particular bidder or a small,

colluding group of firms, have a real chance of winning the contract, thereby undermining competition and fair access to public resources.

There are several ways to capture such corruption schemes using data. For instance, Fazekas, Tóth, and King (2016) develop a Corruption Risk Index based on indicators such as the absence of a published call for tender, the type of procurement procedure used, and the length of the submission period. By directly identifying red flags at the submission stage and validating these indicators against instances of single bidding, the authors flag tenders with no publication, restricted procedures, and excessively short submission windows as high-risk. Similarly, Ferwerda, Deleanu, and Unger (2017) examine whether a shortened bidding period or the absence of publicly available selection documents is more likely to be associated with confirmed cases of corruption in procurement. Expanding on these approaches, Decarolis and Giorgiantonio (2022) provide a comprehensive review of existing indicators by applying a random forest model to predict corruption based on proven corruption convictions of individuals linked to the supplier. Their analysis highlights key predictors within Italian public procurement data, including missing information in the call for tender, shortened submission periods, the use of urgent or negotiated procedures, reliance on MEAT⁵ criteria - which emphasize qualitative over quantitative assessment - and, more broadly, features that increase the potential for discretionary evaluation of bids.

Restrictive or discretionary procedure types and their association with corruption have received particular attention in the literature, with substantial evidence pointing to a positive correlation between the two. Auriol, Straub, and Flochel (2016) demonstrated, in the case of Paraguay, that more corrupt public institutions tend to allocate a larger share of their budgets through exceptional procedures across all sectors. Chong, Klien, and Saussier (2016) examined the use of discretionary procurement procedures and their effects on competition, efficiency, and potential corruption in EU public procurement. Their findings highlighted a clear association with reduced competition, increased market concentration, and higher price variance. Finally, Fazekas, Nishchal, and Søreide (2025) illustrate how a general increase in discretion in public procurement procedures can elevate the risk of misuse of public funds by analyzing a series of emergency procurement cases from Italy.

The texts of calls for tenders can also serve as a valuable indicator of corruption risk in procurement. In their study of 119,000 tenders issued in Hungary between 2011 and 2020, Katona and Fazekas (2024) apply a range of statistical methods to predict the likelihood of single-bidder outcomes. They combine conventional risk indicators, such as the length of the submission and decision periods, the absence of a published call for tender, and the procedure type, with a text-as-data approach that analyzes the language used in tender announcements. Their findings reveal that certain recurring phrases in eligibility criteria, product specifications, and evaluation rules are strong predictors of single bidding, even in markets where competition would typically be expected. The detailed look at the specific terms contributing to limited competition on otherwise competitive markets suggests that subtle tender text manipulations such as tailored product descriptions or overly restrictive eligibility conditions can increase

⁵ The Most Economically Advantageous Tender (MEAT) is a method of assessment that can be used as the selection procedure, allowing the contracting party to award the contract based on aspects of the tender submission other than just price



corruption risks even when formal and visible tender features, such as procedure type, are seemingly open.

In the **evaluation phase**, indicators typically focus on signs of limited competition, delays or unusually swift decision-making in awarding the contract, as well as substantial increases in the final contract price compared to the initial estimated value. Corruption schemes during this stage can take various forms. For instance, selection criteria may remain undisclosed, enabling evaluators to justify the choice of a preferred bidder without transparency or accountability. The decision-making process may be deliberately delayed, creating space for informal negotiations or interference. Bid prices may display irregular patterns such as identical pricing across bids or consistent rounding, that suggest coordination among bidders, possibly indicating collusion or bid rotation. In some cases, the winning bid could be significantly higher than the original estimated contract value, despite no clear justification for the increase. This suggests that evaluators may have disregarded or reinterpreted cost criteria during the evaluation process to favor a preferred bidder. The behavior of the winning bidder in the wider network of buyers and suppliers (i.e. contracting network) has also emerged as a major cue for corruption in recent years (Hudon and Garzon, 2016). Network centrality metrics, or the suspicious lack of network connections in local networks as well as sudden reconfigurations of network structures following government change are some of the key metrics pointing at corrupt organization of public procurement transactions at the mezo and macro scales (Wachs et al, 2021).

Several studies have sought to capture these schemes using data-driven approaches. For example, Ferwerda, Deleanu, and Unger (2017) find that the absence of publicly available award and selection documents is positively correlated with confirmed cases of corruption. Similarly, Kenny and Musatova (2012), in their analysis of World Bank-financed water projects, identify common red flags such as limited competition in procurement auctions, unreasonable delays in contract signing, and low participation by international firms. The authors argue that corruption is closely linked to restricted competition; in essence, limited competition is not merely a byproduct but a necessary condition of corrupt exchanges, as it enables public officials to award contracts to favored suppliers while masking the preferential treatment.

Finally, the **delivery phase** focuses on outcomes such as contract modifications, delays, and cost overruns. Corruption schemes at this stage often unfold as follows: after the contract is awarded, project specifications and pricing are quietly altered, resulting in increased costs and an expanded scope beyond what was originally agreed. The contractor may inflate expenses, leading to substantial cost overruns that are approved without proper scrutiny or justification. Project delivery could be repeatedly delayed, often explained by bureaucratic inefficiencies or unforeseen circumstances. When the project is eventually completed, the delivered work frequently falls short of the agreed specifications, featuring incorrect quantities or substandard quality, yet payments are processed in full. This occurs due to weak oversight mechanisms and missing or manipulated documentation, allowing the scheme to go unchecked.

Unfortunately, much of the relevant information needed for systematic assessment is either not published or made available in unstructured formats that are unsuitable for quantitative analysis. Nonetheless, some studies have attempted to use indicators at this stage to reflect corruption risks. For example, Lisciandra, Milani, and Millemaci (2022) analyzing data on Italian public works from 2007 to 2017, found significant correlations between tender complexity -

measured by planned contract duration and budgeted costs - and implementation length and cost overruns. The authors argue that large, complex tenders that experience delays and cost increases not only correlate with corruption but also create conditions that facilitate it. Similarly, Baldi et al. (2016) examine the use of negotiated procedures in Italy and find that such projects are more likely to suffer from longer execution delays and higher price rebates.

Table 2.1 – Tendering risk indicators according to public procurement phases

Submission phase					
Indicators	Key references	Countries	Sectors	Years	Quality
Use of exceptional, emergency or restricted procedures	Auriol, Straub & Flochel 2016; Fazekas, Tóth & King 2016; Baldi et al. 2016; Chever, Saussier & Yvrande-Billon 2017; Søreide 2002; Decarolis et al. 2020; Decarolis & Giorgiantonio 2022; Fazekas, Nishchal & Søreide 2021	Paraguay, Italy, Hungary, France, Romania, general EU	General procurement, construction, healthcare	2004-2020	High quality systematic evidence
No call for tender publication	Fazekas, Tóth & King 2016; Fazekas, Nishchal & Søreide 2021; Ware, Moss & Campos 2007	Hungary, Italy, Romania	General procurement	2009-2020	High quality systematic evidence
Tailored/restrictive tendering terms, overly specific eligibility criteria	Fazekas, Tóth & King 2016; Katona & Fazekas 2024; Ware, Moss	Hungary, EU	General procurement	2011-2020	Limited systematic evidence



	& Campos 2007; Acikalin et al, 2023				
Missing information from tender requirements	Decarolis & Giorgiantonio 2022; Ferwerda, Deleanu & Unger 2016; Ware, Moss & Campos 2007	France, Hungary, Italy, Lithuania, Netherlands, Poland, Romania, Spain	General procurement, construction	2009-2015	Limited systematic evidence
Short advertisement period	Ferwerda, Deleanu & Unger 2016; Decarolis & Giorgiantonio 2022; Fazekas, Tóth & King 2016	France, Hungary, Italy, Lithuania, Netherlands, Poland, Romania, Spain	General procurement, construction	2009-2015	High quality systematic evidence
Tender size and complexity	Bandiera, Prat & Valletti 2009; Søreide 2002; Lisciandra, Milani & Millemaci 2022; Baldi et al. 2016; Decarolis & Giorgiantonio 2022; Ferwerda, Deleanu & Unger 2016	Italy, France, Hungary, Lithuania, Netherlands, Poland, Romania, Spain	General procurement, construction	2000-2017	High quality systematic evidence
Evaluation phase					
Limited competition (single bidding, lack of non-local suppliers)	Coviello & Gagliarducci 2010; Fazekas, Tóth & King 2016; Kenny & Musatova	Italy, Hungary, World Bank procurement projects	General procurement, construction sector	2009-2024	High quality systematic evidence



	2012; Doroftei 2016				
Abusing discretionary bid evaluation	Decarolis & Giorgiantonio 2022; Ware, Moss & Campos 2007	Italy, EU	General procurement	2009-2015	Limited systematic evidence
Non-transparent bid assessment	Ware, Moss & Campos 2007	-	General procurement	-	Limited systematic evidence
Contract signature delays	Kenny & Musatova 2012; Fazekas, Tóth & King 2016	Hungary, EU	General procurement	2009-2015	High quality systematic evidence
Bid price irregularities: Benford's Law	Ensminger & Leder-Luis, 2025; Czibik et al, 2025	Kenya, Albania, Spain, Bulgaria, Poland, France, Italy, Romania	General procurement	1993-2024	Limited systematic evidence
Politically driven winning patterns	Dávid-Barrett & Fazekas 2020; Romero 2025	Guatemala UK, Hungary	General procurement	2009-2019	Limited systematic evidence
Network position	Fazekas & Wachs, 2020; Wachs et al, 2021; Luna-Pla & Nicolás-Carlock, 2020	Czech Republic, Hungary, EU Mexico	General procurement	2008–2016	High quality systematic evidence
Delivery phase					
Contract modifications & scope changes	Ware, Moss & Campos 2007; Søreide 2002; Fazekas et al, 2023	US	General procurement	2004-2015	Limited systematic evidence



Cost overruns	Guccio, Pignataro & Rizzo (2012); Cavalieri, Guccio & Rizzo (2017); Ravenda et al. (2020); Lisciandra, Milani & Millemaci (2022); Catalão, Cruz & Sarmento (2023)	Italy, Portugal	General procurement, construction	1980-2017	High quality systematic evidence
Delayed contract implementation	Baldi et al. (2016); Cavalieri, Guccio & Rizzo (2017); Catalão, Cruz & Sarmento (2021); Lisciandra, Milani & Millemaci (2022)	Italy, Portugal	Construction	1980-2016	High quality systematic evidence

2.2.2 Supplier risk indicators

This section reviews indicators used to identify corruption risks linked to companies winning public contracts. In corrupt procurement transactions, suppliers play a central role in extracting, distributing, and concealing illicit gains, while they often differ from non-corrupt firms in observable ways. However, detecting corrupt suppliers using public data is inherently difficult, as different schemes produce varying patterns, and theoretical expectations are often ambiguous. Relying on a single indicator can be misleading, as some red flags, like rapid growth or local proximity, may also describe efficient or well-positioned firms. Many indicators are also limited by data availability. To reduce false positives, supplier-level risks should be interpreted alongside other signs of corruption, such as tendering risk indicators. For instance, a company based in the same municipality as the buyer may not raise concern on its own, but if it is also recently established and linked to politically exposed persons, it may indicate a high risk of corruption. The below discussion groups supplier risk indicators by 3 key areas, corresponding to main data types: company registry, financial performance, and ownership and management. Each category highlights specific supplier characteristics that may signal illegal behaviors, while emphasizing the need for corroboration with other evidence (see Table 2.2).

Company Registry Information

The first group of indicators is based on essential company registry data such as the firm's HQ location or date of incorporation. These pieces of information are usually relatively easily extractable, which makes them a valuable source of potential red flags.

Several studies have highlighted registration in high-risk location as an *indirect* measure of illicit behavior in procurement. In their review of objective corruption proxies, Fazekas, Cingolani & Tóth (2016) take into account registration in a "corrupt region", as defined by independent corruption indices. Although a distant approximation, this geographic feature may point to the presence of the environment where misconduct is condoned. In turn, Caneppele, Calderoni, and Martocchia (2009) examine the infiltration of organized crime in public contracting in Southern Italy. Along with revealing a heightened risk of infiltration in regions like Sicily, Calabria, and Campania, the authors emphasize the role of company profile characteristics in signaling business capture by criminal groups. Location of the firm's headquarters is seen as one of such markers, which can range from registration in a place suspiciously remote from the contract's origin to establishment in a foreign country or frequent change of locations.

A related measure is registration of multiple firms at the same address. This *indirect* proxy indicator of corruption signals that co-located entities might be colluded or even be part of a shell companies network used to simulate competition or distribute illicit gains. Indeed, Caneppele, Calderoni, and Martocchia (2009) note that sharing one address may conceal illicit connections, often involving firms that serve as forefronts with no real operations while subcontracting the actual work. In constructing a support system for fraud detection in Brazilian procurement, Velasco et al. (2021) includes shared address of two bidders as a marker of potential collusion between them, whereas Fazekas, Cingolani, and Tóth (2016) consider firms co-registration among their location-based proxies of corruption. Similarly, in the report on corruption in procurement within the EU, Ferwerda and Deleanu (2013) reveal that the suspect supplier connected to the procurer through side-works and eventually receiving inflated invoices was also registered at one address with other multiple contractors of the those works (Ferwerda & Deleanu 2013: 118).

Company age represents another *indirect* proxy of illicit transactions: companies established shortly before winning large public contracts may have been created specifically to channel funds to insiders, often lacking the capacity to deliver contracted services. As argued by Fazekas, Lukács, and Tóth (2015), the timing of a firm's establishment may signal corrupt intent - particularly when it coincides with political transitions or occurs shortly before securing major contracts. On the examples of Hungary and Sweden, Fazekas and Tóth (2017) empirically demonstrate that the value of contracts won by very young firms (incorporated one year before the contract) is significantly larger in Hungary, the country with a higher perceived corruption risk. Velasco et al. (2021) similarly account for a company-level risk pattern of how close the first contract or bid is to the entity's registration based on the Brazilian procurement data,

Supplier registration in tax havens – i.e. jurisdiction imposing little or no taxes, lacking corporate data transparency and effective exchange of tax-related information (OECD 1998) – is similarly used to *indirectly* approximate fraud instances in public contracting. Thus, on the example of Hungary, Fazekas, Tóth and King (2016) demonstrate that suppliers residing in tax havens exhibit

a significantly larger Corruption Risk Index (CRI) than businesses from more transparent countries. The result is even more remarkable given that only 2% of winners are registered outside Hungary (Fazekas, Tóth & King 2016: 395). On the contract data from five countries from different regions of the world, Abdou et al. (2022) further include tax haven residence as one of the CRI components, showing that it is positively associated with the outcomes like single bidding or supplier contract share. De Willebois et al. (2011) also point out that major corruption cases across the globe frequently involve tax havens residence and reliance on offshore entities.

Overall, company registry offers several useful, low-cost indicators for detecting illicit behavior in procurement. While straightforward, however, these markers might not be inherently suggestive of corruption, so that scholars need to analyze them in context and combine with other red flags.

Financial information

The second group of supplier risk indicators covers financial information such as firms' revenue patterns, productivity, and size. Here, sudden spikes in assets, abnormally high profit margins, or discrepancies between firm and contract size may connote that an entity is receiving overvalued orders, possibly in exchange for bribes or kickbacks.

Prior research has consistently interpreted patterns like the disproportionate growth of turnover, sales, or profitability of a firm as *effects* of corrupt contracting. According to this logic, improved firm performance results from rents acquired through public contracts. Fazekas, Poltoratskaia, Schiffbauer and Tóth (2025), for instance, combine procurement data with company-level statistics and data on political connections to explore the impact of favoritism on private sector in Bulgaria. As the findings suggest, businesses with contracts won in uncompetitive procedures feature significantly higher profit margins, which is driven by the contract overpricing of 9-11% -- even though their growth remains flat. In turn, Wittberg and Fazekas (2023) reveal a similar pattern on the example of Swedish construction firms during 2010-2015, finding that companies with a bigger ratio of single-bid wins consequently have a 0.2-0.6 percentage-point higher sales margins. Moreover, the logic also holds on the global sample: Cheung, Rau, and Stouraitis (2012), who examine a 166 procurement-related bribe cases of 107 companies in 52 countries over 1971-2007, report that market capitalization of bribing suppliers rises by about 11 dollars per each dollar of the bribe paid (Cheung, Rau, & Stouraitis 2012: 5).

Moreover, the link between enhanced firm performance and corruption is substantively evidenced in the wider literature. In a large-scale survey of 106,805 businesses across 132 developing countries, Williams, Martinez-Perez and Kedir (2016) find that enterprises reporting the use of bribes to public officials experience 14% higher average sales growth rates and 48% higher productivity growth rates annually. In Danish municipalities, companies related by family to local politicians are found to observe significantly higher profitability (Amore & Bennedsen 2013), while in Italy, local political connections tend to increase company revenue and profits by about 6% (Cingano & Pinotti 2013). Importantly, the effects are driven by industries reliant on supplying the public administration, such that biased procurement is seen as a channel of rent allocation.

While the above work suggests (short-run) financial returns to uncompetitive public contracts, the picture is different when firms' efficiency is concerned. Existing evidence equally implies that suppliers engaging in illicit deals tend to underperform their counterparts, such that low productivity can be seen as an effect or cause of corrupt procurement. In particular, Cheung, Rau, and Stouraitis (2012) report that bribe-paying businesses score significantly lower on asset turnover, return on assets, and profit margins several years before as well as after the contract award, although underperformance vanishes three years thereafter. As the authors conclude, the very fact of this lagging behind might drive companies to give bribes for contracts (Cheung, Rau & Stouraitis 2012: 18-19). Similarly, the Bulgarian case study reveals flat productivity and employment growth of suppliers who are awarded contracts via uncompetitive procedures, as opposed to winners of competitive tenders who feature substantially improved performance in subsequent years (Fazekas et al. 2025). On the Russian example, Mironov and Zhuravskaya (2016) similarly show that procurement goes to less efficient firms: recipients of government awards have significantly lower labor productivity compared to non-recipients, especially in corrupt regions where contract allocation is associated with the occurrence of illegal cash transfers around elections.

More broadly, research also identified the share of procurement income to total revenue of a business as typically resulting from corrupt contracting (Fazekas et al, 2016). Studying the relationship between illegal cash outflows ("tunneling"), government contracts, and political cycle in Russia, Mironov and Zhuravskaya (2016) find that firms with a larger share of procurement-to-total revenue are more involved in tunneling around regional elections. Moreover, a positive association between transfers and consequent procurement income highlights corrupt goals of election-driven tunneling, which is used to secure future public contracts, particularly in regions with higher perceived corruption. Likewise, for Vietnamese businesses, Hoang (2022) considers revenue dependence from government and SOE contracts to show that state-dependent firms incur 23%-higher bribery costs.

Financial information can similarly give insight about the discrepancy between a company's ongoing operations and amount of procurement: disproportionately small firm size signals that it might be a shell company, typically established to conceal beneficial owners and perform activities like routing illicit profits. Shell company is defined as a legal entity that is lacking independent business operations, adequate assets, or personnel (de Willebois et al. 2011: 34). Yet, as multiple cases of grand corruption attest, it is commonly misused for corrupt purposes, being combined with other techniques to disguise actual owners such as business control through ad hoc arrangements and bearer shares, layering multiple corporate vehicles, or placing beneficiaries in secrecy jurisdictions (Ibid.: 35-36). Thus, the presence of a bidder who deviates from the average firm size and/or wins disproportionately large contracts raises legitimate questions about the legitimacy of the supplier selection process (Caneppele, Calderoni & Martocchia 2009). Shell-company bidders may furthermore form clusters connected by shared ownership or management, as Nicolás-Carlock and Luna-Pla (2024) demonstrate on Mexican procurement. The authors construct a bipartite buyer-supplier network to show how groups of shell companies operate together to extract public funds.

In contrast to bogus "hollow" entities, practices fostering disclosure of financial information are supposed to foster transparency of the procurement process. Indeed, accounting and auditing standards can be seen as a factor reducing the risk of uncompetitive contract awards.

Particularly, Nathan (2022) examines the effects of the US Truth in Negotiations Act, according to which suppliers of government contracts above a threshold price are obliged to disclose their accounting information. The study shows that above-threshold contracts indeed observe more competitive bidding as well as improved contract execution. While Nathan (2022) does not relate his findings to the benefits of reduced favoritism, stricter accounting requirements do represent a meaningful indicator of corruption worthy of further exploration.

Ownership and Management

The final group of corruption risk indicators leverages ownership and management information to proxy misconduct in public contracting. These measures focus on concealed beneficiaries and complex corporate structures, links to politically exposed persons, as well as more generally connections with political elites. Besides capturing potentially corrupt procurement procedures, these indicators are also valuable for pointing at related crimes such as money laundering, tax evasion and more broadly hiding the ultimate recipients of illicit rents.

To start with, hidden or opaque ownership represents a major red flag of ongoing diversion of public funds, since it is the beneficial owner who eventually profits from the business's illicit profits. Concealing actual beneficiaries is a widespread tactic in grand corruption, including the simple absence of registration or the omission of ownership changes (de Willebois et al. 2011). The use of fake representatives serves as another technique: Caneppele, Calderoni, and Martocchia (2009: 165) observe that criminal groups often resort to “dummy representatives” in order to disguise their presence in the contract, while Valesco et al. (2021: 44) mention “straw persons”, i.e. individuals legally appearing as partners of a business but actually doing so on behalf of a hidden actual owner. This way, unknown or untraceable beneficial owners should be viewed as an important supplier risk indicator, as pointed out by Fazekas, Cingolani and Tóth (2016).

Moreover, opaque ownership is in practice tightly interlinked with the presence of shell or front companies (see subsection on company financial data-linked indicators above), as the latter are aimed at obscuring connections between companies and public officials. In one typical kickback scheme, a prime contractor of the public procurer would hire a subcontractor (shell) firm to perform some side tasks, which are in reality used to mask a kickbacks paid back to a public official from the procuring agency (Ware, Moss & Campos 2007: 304). As Ware et al. (2007) note, such suspicious subcontractors commonly involve previously unknown companies, with bogus owners like law firms rather than identifiable individuals, registered in secrecy jurisdictions, missing complete contact information, and having no visible operational facilities (Ibid.: 305-306). Complex ownership structures comprise yet another widespread characteristic. In particular, Riccardi and Savona (2013) highlight how convoluted ownership models resembling the “Chinese box” schemes are deployed to hide identities of true beneficiaries, such as those found in mafia-influenced businesses in Italy.

The next proxy indicator of corrupt suppliers is whether the bidding company belongs to an owner investigated or persecuted for corruption. As Caneppele, Calderoni, and Martocchia (2009) posit, the contractor's involvement or connection to economic crimes, even if perpetrated by another legal entity, is a primary predictor of public contract infiltration. Decarolis et al. (2025) further develop a novel measure of corruption in public procurement based on confidential



police records in Italy. The scholars explore conditions under which contracts are awarded to firms with at least one owner or employee suspected of corruption, finding that such outcome is significantly more likely with discretionary procedure auctions – however, only when procedural discretion is abused by procuring officers in order to limit competition (Decarolis et al. 2025: 214). In a related piece, Decarolis and Giorgiantonio (2022) consider whether any of the supplier’s owners or top managers has been investigated for economic crimes, reporting that this proxy is highly consistent with established corruption measures based on judicial evidence and contract execution metrics.

Finally, a sizeable strand of research studies the effect of political connections on procurement (Please note that political connections of government suppliers is not a focus of this report, as it is covered by another work package of RESPOND. Here, we only mention some notable papers and briefly discuss the topics to offer a comprehensive overview.). From one perspective, political ties can be understood as direct links between a firm’s leadership and political elites, whether through party affiliation, local elite networks, or connection to politically exposed persons. For example, Goldman, Rocholl, and So (2013) relate procurement outcomes to the presence of board members who previously served as senior bureaucrats. Following the election, businesses linked to the winning party are found to undergo a considerable increase in contracts, while those associated with the losing party face significant declines. The effect of party affiliation is further evidenced by Baránek and Titl (2024), who show that Czech companies with representatives belonging to political parties get awards overpriced by about 6%.

Co-partisanship can also be inferred from the shifts in companies’ total procurement value after a government turnover, as done in the study of partisan favoritism in Hungary and the UK (Dávid-Barrett & Fazekas 2020). The paper reveals that politically connected firms secure up to 60% of the procurement market in Hungary, while in the UK, where institutional constraints are stronger, the figure is limited to 10%. Alongside those measures, Romero (2025) predicts an ongoing link between a firm and local mayor by considering how much rents were accrued in the municipalities ruled by the mayor’s “clique”. Using this novel proxy, it demonstrates that the presence of a competent yet politically controlled bureaucracy actually raises the risk that connected businesses win via less competitive procedures. Moreover, Fazekas, Poltoratskaia, and Tóth (2023) leverage the data on politically exposed persons in Bulgaria, tracing their status in a company as owners or directors. Their results support the idea that contracts won by connected firms during 2011-2019 exhibit significantly higher corruption risks.

From another perspective, political connections can manifest through more institutionalized channels, such as state ownership or financial contributions to political parties. Tkachenko, Yakovlev & Kuznetsova (2017), for instance, document allocation bias towards public firms in Russia, showing that the latter win tenders at significantly inflated prices compared to private firms. Notably, the effect is strongest when contracts are awarded through non-transparent processes. In turn, Doroftei (2016) relies on preceding party donations and received privileged treatment to establish that Romanian firms with political ties receive considerably more contracts, especially in pre-election years.

Overall, the information on managers and owners offers a major advancement not only for detecting rigged procurement processes but also untangling complex corruption schemes and trace ultimate profiteers. Certain indicators, such as the presence of shell companies or owners investigated for economic crimes, emerge as particularly strong predictors of corruption risks.

Constructing these proxies, however, often requires access to restricted data sources like beneficial ownership registries, as well as the ability to link public procurement and company records with asset or interest declarations. Moreover, the links between political connections of company owners and managers and the expected unusually profitable, high-flying company performance may not be as straightforward as assumed in most of the studies. Political connections can also be exploitative, as pointed out by Bai et al (2026) using Chinese data, when politicians extract rents from companies to the degree that their performance severely deteriorates, rather than making them achieve above normal financial performance.

Table 2.2 – Supplier risk indicators grouped by company data type

Indicators	Key references	Countries	Sectors	Years	Quality	Cause/Effect/Direct/Indirect
Company registry information						
Registration in high-risk location	Caneppele, Calderoni, & Martocchia (2009); Fazekas, Cingolani & Tóth (2016)	Italy	General procurement	n.a.	Limited systematic evidence	Indirect
Multiple companies registered at the same address	Caneppele, Calderoni & Martocchia (2009); Ferwerda & Deleanu (2013); Fazekas, Cingolani & Tóth (2016)	EU, Italy	General procurement	2000 - 2010	High quality systematic evidence	Indirect
Company age	Fazekas, Lukács, & Tóth (2015); Fazekas & Tóth (2017); Velasco et al. (2021)	Hungary, Sweden, Brazil	General procurement, construction	2009 - 2019	High quality systematic evidence	Indirect
Tax havens	De Willebois et al. (2011); Fazekas, Tóth & King (2016); Abdou et al. (2022)	World, Hungary, Georgia, Indonesia, Paragua	General procurement	2007 - 2020	High quality systematic evidence	Indirect



		y, Uganda, Romania				
Debarred/sanctioned suppliers	Aldana, Falcón-Cortés & Larralde (2022); Falcón-Cortés, Aldana & Larralde (2022); Medina-Hernández, Kertesz & Fazekas (2025)	Mexico	General procurement	2011 - 2022	High quality systematic evidence	Direct
Financial data						
Disproportionate turnover / sales / profitability	Cheung, Rau & Stouraitis (2012); Wittberg & Fazekas, (2023); Fazekas et al. (2025)	World, Sweden, Bulgaria	General procurement, construction	1971 - 2018	High quality systematic evidence	Effect
Low efficiency & productivity	Cheung, Rau & Stouraitis (2012); Mironov & Zhuravskaya (2016); Fazekas et al. (2025)	World, Russia, Bulgaria	General procurement	1971 – 2018	High quality systematic evidence	Cause/Effect
High share of procurement income to total revenue	Fazekas, Cingolani & Tóth (2016); Mironov & Zhuravskaya (2016); Hoang (2022)	Russia, Vietnam	General procurement	1999 - 2017	High quality systematic evidence	Effect
Disproportionately small /shell companies	Caneppele, Calderoni & Martocchia (2009); de Willebois et al. (2011); Nicolás-	World, Mexico, Italy	General procurement	2014 - 2019	Limited systematic evidence	Indirect



	Carlock & Luna-Pla (2024)					
Accounting & auditing standards	Nathan (2022)	US	General procurement	2015 - 2018	Limited systematic evidence	Cause
Ownership and management information						
Hidden / opaque ownership	Ware, Moss & Campos (2007); Caneppele, Calderoni & Martocchia (2009); de Willebois et al. (2011); Fazekas, Cingolani & Tóth (2016); Velasco et al. (2021)	World, Italy, Brazil	General procurement	2003 - 2019	High quality systematic evidence	Indirect
Risky owners: corruption investigations and convictions	Caneppele, Calderoni & Martocchia (2009); Decarolis & Giorgiantonio (2022); Decarolis et al. (2025)	Italy	General procurement, construction	2000 - 2016	High quality systematic evidence	Direct
Political connections*	Goldman et al (2013); Doroftei (2016); Auriol, Straub & Flochel (2016); Tkachenko, Yakovlev & Kuznetsova (2017); Dávid-Barrett &	US, Romania, Paraguay, Russia, Bulgaria, Czech Republic	General procurement, construction, food	1994 - 2019	High quality systematic evidence	Direct



	Fazekas (2020); Fazekas, Poltoratskaia & Tóth (2023); Baránek & Titl (2024); Romero (2025)					
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*Note: *please note that political connections of government suppliers is not a focus of this report, as it is covered by another work package of RESPOND. Here, we only mention some notable papers and briefly discuss the topics to offer a comprehensive overview.*

2.2.3 Buyer risk indicators

Contracting authorities, as defined by EU procurement directives, include public entities with legal independence and funding from the state (OECD, 2011). Along with suppliers, they are equally important for detecting corruption in public contracting. Their organizational frameworks, shaped by both formal and informal rules, significantly influence opportunities for corruption. The independence and integrity of these authorities are therefore central to mitigating risks, with institutional factors such as autonomy, oversight, and accountability playing a defining role. At the level of the contracting authority or buyer, corruption risk indicators can be grouped along several dimensions, namely institutional design, law enforcement and monitoring, buyer's relationship with suppliers, and broader structural conditions (see Table 2.3).

Institutional Features

The first group of indicators centers around institutional features of the procurer, such as autonomy and staff composition. These can be broadly categorized as *causes* of corruption being able to deter or prompt the occurrence of misconduct.

To start with, the literature views organizational autonomy as an important predictor of favoritism in public contracting. Procuring agencies are assumed to be influenced by intervention from the central government: the stronger formal supervision and informal pressure, the more bureaucrats tend to contribute to politically motivated contract allocation. On the US example, Dahlström et al. (2021) explore whether patterns of partisan favoritism vary with agencies' political responsiveness. The paper distinguishes between cabinet departments, separate bureaus within departments, independent administrations, and independent commissions, finding that executive departments -- as more politicized bodies -- are more likely to observe contracts with noncompetitive procedures and outcomes.

Likewise, studying if campaign donors receive larger contracts, Zarit (2018) demonstrates that the effect indeed holds for the executive offices that are least insulated from the president. On the contrary, the advantage vanishes in field offices, which are usually more distant from central headquarters and are staffed by career civil servants. Looking at Italian procurement, Bandiera et al. (2009) similarly differentiate between central ministries, local offices, and semi-autonomous agencies in studying the determinants of wasteful spending. The latter is indeed



shown to be higher in large central bureaucracies and lower in semi-autonomous organizations, even though the effect is insignificant when it comes to active waste (including corruption). While the evidence is systematic and of high quality, the autonomy indicator focuses on overarching structures, thus being highly aggregated and potentially rigid. Especially when operationalized through institutional types, the measure might overlook micro-level behavioral dynamics of corruption.

A related line of research concentrates on staff composition: as opposed to merit-based recruitment, the one involving political and/or personal connections tends to increase corruption risks. Dahlström et al. (2021) consider the number of political appointees relative to career civil servants within the procuring body, finding a positive relationship with bias-suggesting patterns such as single-bidding. Although this measure is less static than the institutional categorization used in the same paper, one could still note its high level of aggregation. Somewhat addressing it, other works point to the critical role of political/personal connections observed at the individual level – yet they are not dedicated to procurement itself. In the case of Colombia, public officials with family ties to senior bureaucrats are more likely to engage in corrupt transactions, whereas at the agency level, a higher share of such connections correlates with poorer government quality indices (Riaño 2021). Through a survey across several developing countries, Harris et al. (2023) further evidence that servants recruited via political or personal connections are indeed more lenient towards corrupt practices, although the two types exhibit distinct motivations and behavioral logics.

Another determinant of corruption risks concerns material incentives, like remuneration policies. Intuitively, adequate pay and stable career prospects are supposed to reduce the will to seek illicit rents. Using a survey of procurement officials in Buenos Aires's hospitals, Di Tella and Schargrodsky (2003) evince that higher efficiency wages (higher pay relative to outside options) reduce prices paid for basic inputs. However, the negative effect holds only when the perceived risk of punishment through audits is at intermediate levels. Though a relatively early study, it elegantly combines contracting outcomes with procurer-level treatment. Going beyond the field of procurement, literature also deploys measures such as the ratio of government to manufacturing wages (Rijckeghem 2001) or public to the general average wage (Dong & Torgler 2013), revealing a negative association with corruption proxies. Survey-based papers similarly confirm this negative relationship: perceived corruption decreases with higher average public wage (Alt & Lassen 2003), while the propensity for misconduct is mitigated by admitted salary sufficiency (Harris et al. 2023).

Finally, civil service meritocracy, including hiring, promotion, and firing policies, constitute a more generic, yet increasingly recognized cause of corruption. Predominantly survey-based, this strand of research increasingly emphasizes “Weberian” administrative practices grounded in meritocracy, professionalism, and rule-based behavior. A recent paper applying this approach to procurement is by Charron et al. (2017), who integrate a survey from over 18,000 public employees with the data on 1.4 million procurement contracts across 212 EU regions. Evaluating whether career success in the public sector depends on *hard work* vs. *luck and connections*, the authors find that regions with stronger meritocratic incentives exhibit a significantly lower share of single-bid contracts as well as lower corruption index. This piece indeed stands out for leveraging individual-level responses and its broad geographical coverage, even though

surveyed officials are not procurers themselves whereas the treatment variables could be enhanced by additional survey measures.

The importance of meritocracy is similarly highlighted within the broader public sector and across the globe. Civil servants' assessment of corruption decrease when they observe higher levels of merit-based recruitment and lower levels of politicization (Meyer-Sahling & Mikkelsen 2016); stronger application of disciplinary codes (Meyer-Sahling & Mikkelsen 2022) and awareness of the anti-corruption law (Lee, Meyer-Sahling & Park 2025); greater extrinsic and intrinsic motivation (Kwon 2014); as well as overall predominance of “Weberian” rules-based practices as opposed to discretionary ones (Schuster, Meyer-Sahling & Mikkelsen 2020). Cross-country expert surveys by Rauch and Evans (2000) and Dahlström, Lapuente and Teorell (2012) further confirm that meritocratic recruitment significantly reduces perceived corruption, akin to Neshkova and Kostadinova (2012) concluding that the adoption of civil service legislation is negatively related to general corruption indices. Overall, while civil service meritocracy is indeed well-documented in general research, its application to public procurement remains relatively underexplored. Besides, more “objective” measures of meritocracy that move beyond surveys could substantially enrich methodology.

Law enforcement & Monitoring

The second category of buyer risk indicators concerns external monitoring and law enforcement. Since corruption is a criminal offence, at least in principle, the state's effort to investigate and sanction misconduct should shape exposure to corruption risks.

A prominent strand of this research looks at local capture and mafia infiltration within local governments, which in turn adversely affects procurement. Specifically, most papers focus on the case of Italy: the latter offers a quasi-experimental setup in the light of Law No. 164/1991, which authorized the dissolution of municipal councils that had been infiltrated by organized crime. For instance, Fazekas et al. (2022) relate the fact of infiltration – defined by whether a contract is awarded in the 0–36 months preceding government dissolution – to contract-level risk indicators like single bidding or non-open procedures. Their results evince that municipalities featuring mafia presence display systematically elevated corruption risks prior to dissolution.

Ravenda et al. (2020), instead, consider tenders published during the office of 127 municipal councils dissolved for infiltration, as well as contracts awarded to firms later seized for mafia control. Using winning rebates and cost overrun outcomes, they find evidence of inflated costs and reduced competitiveness in procurement tenders. Finally, Di Cataldo and Mastrorocco (2022) exploit the time elapsed between the last local election and the council's subsequent dissolution as their treatment variable, showing that mafia infiltration leads to distortions in the number of awards and overall municipal finances. Collectively, these works leverage law-enforcement events (dissolutions) to construct *direct* measures of illicit transactions in contracting, given the ex post criminal interventions. Yet, despite confirming the explicit link between mafia infiltration and biased procurement, these case studies rely on rare and quite specific intervention events, which may limit generalizability of results besides overlooking subtler, everyday forms of corruption.

A further measure of corruption risks is based on corruption-related investigations and convictions. This indicator speaks most closely to earlier measures of corruption that relied on



judicial and prosecutorial statistics. Examining procurement auctions in Italy, Decarolis et al. (2025) consider whether the responsible procurer and any official in the contracting authority have ever been investigated for corruption, deploying it as a *direct* proxy of suspected behavior. The paper shows that auctions managed by suspected bureaucrats exhibit greater procedural discretion and a higher probability of being awarded to an investigated firm. Litschig and Zamboni (2024), on the other hand, highlight the deterrent effect of judicial accountability on local bureaucrats, although their framework is not procurement specific. Looking at whether a municipality hosts a state judiciary, the authors reveal that the presence of a local judicial office decreases corruption risk, measured by the share of inspections reporting irregularities. While explicit and accurate, the prosecution-based indicator suffers from a similar pitfall of relying on confirmed offences, thus omitting a potentially big share of unobserved instances of corruption. Moreover, such data are often confidential or difficult to access, limiting their use for research purposes.

Lastly, auditing represents a central example of a monitoring-related indicator. Often exploiting the (close-to-) experimental design, literature stresses the potential of audits to curb occurrence of favoritism. A seminal work investigating this effect in public contracting is Olken (2007), who conducts a field experiment with construction projects in Indonesia. As the results suggest, villages randomly informed that their projects would be audited by the national agency experience significantly lower cost inflation. Similarly, Bobonis et al. (2016) exploit the timing of audit report releases in Puerto Rico, varying whether the report was released before or after the next municipal election. Earlier audits are indeed found to reduce subsequent report-based corruption levels. Complementing these studies, Avis et al. (2018) implement a randomized assignment of Brazilian municipalities to receive a government audit of federal funds, showing that prior audit exposure lowers the number of irregularities detected and increases the likelihood of legal action against public officials. Taken together, these pieces offer strong evidence of the causal link between audits and corruption, although deploying exogenous variation can be costly, especially in the studies on public procurement.

Buyer–Supplier Relationships

A third class of metrics focuses on relational dependencies between buyers and suppliers which are closely related to network metrics highlighted in Table 2.1. Patterns of interactions within existing procurement networks offer a revealing lens on exposure to corruption risk, serving as a *direct* proxy for illicit transactions.

First, literature leverages recurrent interactions -- i.e. repeated contracting between the same buyer and supplier (or a narrow set of suppliers) -- to measure collusion as well as broader capture in contracting. Using Hungarian procurement data, Fazekas and Tóth (2016) examine how favoritism is distributed and clustered across procurement awards, uniting the phenomenon of grand corruption with state capture. By mapping the Corruption Risk Index of individual contracts onto the network of buyers and suppliers, the authors pinpoint densely connected clusters of high-CRI actors as manifestations of state capture. Furthermore, the study reveals that with the 2010 change in the governing elite, high-risk organizations became more central in the network, which is consistent with an increase in centralized state capture. Deploying the similar logic of network analysis, Wachs et al. (2021) analyze over 4 million procurement contracts from 26 EU member states during 2008–2016. The results underline a strong positive association between market centralization and contract-level corruption risk

proxied with single bidding; the latter is also found to be significantly clustered, although its core vs periphery location varies by country.

In a related study, Popa (2019) uses machine-learning techniques on a large dataset of European public contracts to identify when “close connections” between the state and private business emerge. Defining connections as the number of firm-authority matches, the scholar reveals that close ties are more likely in environments with weaker external enforcement of procurement rules, i.e. poorer governance quality indicators. On a related note, Goryunova et al. (2021) predict suspicious single-bidder auctions in Russia, considering whether the winning bidder has had prior interactions with the buyer. As the findings suggest, auctions with the winner previously contracted by the buyer are indeed more likely to be flagged as suspicious.

Second, to reflect illicit transactions between a procurer and firm, papers often rely on concentration metrics. The latter most often imply supplier concentration – i.e. the ratio of the supplier’s contract value from the buyer to the annual contract value of that buyer -- which is widely found to positively correlate with corruption risks. Abdou et al. (2021), for example, use winner contract share as one of the indicators for restricted competition when composting their corruption risk index. Meanwhile, in the study of Italian mafia infiltration, Fazekas et al. (2022) evince that the share of supplier in buyer’s annual spending is the strongest predictor of local capture. An alternative, buyer-centered definition of concentration is also possible, such as the share of the firm’s victories with procurer to total victories (Goryunova et al. 2021), even though it is less common.

Geographic proximity between buyers and suppliers is yet another direct measure of collusive behavior in procurement. Either because “geographical proximity enhances personal relationships, or because local bidders represent an easier target” (Coviello & Cagliarducci 2017: 14), the research highlights that local winners exhibit higher corruption risks. On the example of Swedish municipalities, Wittberg and Fazekas (2023) point to likely favoritism towards local firms. As their analysis shows, a winner from the same municipality is positively related to the probability of a single-bidder contract. In turn, Coviello and Gagliarducci (2010, 2017) find that local bidders are more likely to win contracts with reduced competition and inflated prices in Italian municipalities where mayors were repeatedly re-elected between 2000 and 2005 – the pattern suggesting political favoritism. Finally, in her definition of “close ties”, Popa (2019) additionally relies on the geographical distance between procurer and business, demonstrating that closely-tied firms indeed tend to win multiple contracts.

Thus, the relational indicators are particularly advantageous in capturing micro-level dynamics and network effects specific to procurement data. Their scalability from contract to market- or country-wide level also makes them a valuable metric of both localized and systemic corruption risks. However, these proxies remain data-intensive as well as may require cross-validation with external sources if being derived from procurement records themselves.

Local environment / Structural factors

The final group of indicators captures broader contextual influences, like operating in regions with high baseline corruption risk or weak institutional oversight, which affects both buyers and suppliers in an indirect manner. Though less frequently examined in the literature, these factors



capture how the surrounding environment can subtly condition procurement integrity, thus representing *indirect* measure of illicit behaviors.

Quite interestingly, research underlines the role of social capital in shaping procurement outcomes. On social-network data from Hungarian settlements, Wachs et al. (2019) explore the variation between fragmented, inward-looking (“bonding social capital”) and diverse, externally connected (“bridging social capital”) networks. As the paper shows, bonding capital is associated with elevated corruption risks in contracting at the two level, reflected in a share of single-bid or closed-procedure awards as well as a composite corruption risk index. Speaking to these findings, Harris (2007) demonstrates a positive correlation between bonding social capital and levels of perceived corruption in a cross-country analysis with the World Values Survey data. Public and political corruption is found to be increased both directly, through fostered inward trust and exclusion of outsiders, as well as indirectly, via reduced opportunism and reinforced within-group reciprocity.

Another example of contextual dimension involves area-specific criminality of buyers, similarly to the arguments applied to suppliers above. To assess the risk of organized crime infiltration in Italian procurement, Caneppele et al. (2009) construct a geographical criminal index based on the attributes like mafia-style killings, conspiracy cases, asset confiscations, and city council dissolutions. Further combining it with the ratio of contract value to regional GDP, the work reveals that infiltration risks are highest in Calabria, Campania, and Sicily – regions historically associated with entrenched mafia organizations – particularly where public investments are large relative to the size of local economies. Overall, however, contextual indicators should be seen as a complementary layer of analysis, which rather underlines broader preconditions for corruption. Their usage alongside direct and causal indicators would provide a more nuanced picture of favoritism in public procurement.

Table 2.3 – Buyer risk indicators

Indicator	Key references	Countries	Sector	Years	Quality	Cause/effect/direct/indirect
Institutional Features						
Institutional autonomy	Bandiera, Prat & Valletti (2009), Dahlström, Fazekas & Lewis (2021), Zarit (2018)	Italy, US	General procurement	2000 - 2016	High quality systematic evidence	Cause



Staff composition	Dahlström, Fazekas & Lewis (2021)	US	General procurement	2003 - 2015	Limited systematic evidence	Cause
Civil service meritocracy	Charron et al. (2017)	EU	General procurement	2009 - 2013	Limited systematic evidence	Cause
Material Incentives: Remuneration	Di Tella & Schargrods ky (2003)	Argentina	Healthcare	1996 - 1997	Limited systematic evidence	Cause
Law Enforcement & Monitoring						
Mafia infiltration	Fazekas, Sberna & Vannucci (2021), Ravenda et al. (2020), Di Cataldo & Mastrorocco (2022)	Italy	General procurement	1998 - 2017	High quality systematic evidence	Direct
Corruption-related Investigations & convictions	Decarolis et al. (2025)	Italy	Construction	2000 - 2016	Limited systematic evidence	Direct
Audits	Olken (2007), Di Tella & Schargrods ky (2003)	Indonesia, Argentina	Construction	2003 - 2004	High quality systematic evidence	Cause
Buyer-Supplier Relationships						
Recurrent interactions	Fazekas & Toth (2016), Wachs, Fazekas, Kertész (2021), Popa (2019), Goryunova	Hungary, EU, Russia	General procurement	2008 - 2018	High quality systematic evidence	Direct

	et al. (2021)					
Spending concentration	Abdou et al. (2021), Fazekas et al. (2022), Goryunova, Baklanov & Ianovski (2021)	Romania, Italy, Russia	General procurement	2008 - 2021	High quality systematic evidence	Direct
Proximity	Wittberg & Fazekas (2023), Coviello & Gagliarducci (2010, 2017), Popa (2019)	Sweden, Italy, Europe	Construction, General procurement	2000 - 2015	High quality systematic evidence	Direct
Local Environment/Structural Factors						
Social capital	Wachs et al. (2019)	Hungary	General procurement	2006 - 2014	Limited systematic evidence	Indirect
Area-specific criminality	Caneppele, Calderoni & Martocchia (2009)	Italy	General procurement	2000 - 2005	Limited systematic evidence	Indirect

2.3 Case study: Hidden barriers: Using text mining to uncover deliberate restrictions to competition in public procurement

Introduction

This combination of large amounts of public resources allocated, high risk of corruption, and unprecedented open data spawned a large literature in the last 10-15 years, proposing novel measurements for corruption and corruption risks (*Lyra et al, 2022*). Aside from qualitative studies, the literature has almost exclusively focused on structured information on government tenders such as the procedure type followed (e.g. open tenders versus direct awards), fulfilling publication requirements (e.g. publishing the call for tenders) or supplier characteristics (e.g. tax haven registered company) (*Gnaldi and Sarto, 2024*). It has largely neglected text which is



particularly suitable for hiding favouritism because the high degree of technical, financial, and legal complexity makes the insertion of seemingly benign, but competition-restricting conditions easy. Hence, in spite of intense scholarly and policy interest, we still know too little about subtle forms of corruption, their magnitude, and what drives them.

To address this gap, this study makes use of underutilized text in government tenders to study corruption and favouritism in competition for government contracts. Specifically, we aim to

identify strategies for limiting competition for government contracts by tailoring bidding conditions to a potentially favoured company.

Our starting point is the understanding of corruption as limited access to public resources, that is unjustified restriction of competition in public tenders for the benefit of a connected bidder(s) (World Bank, 2009; North et al, 2009). A large number of corruption strategies have been identified in the qualitative literature, many of which have also been estimated in large-scale administrative datasets. Against this background, we argue that subtle, text-based corruption strategies are employed by corrupt actors when more visible strategies for corruption, such as non-competitive tendering procedures, are undesirable or impractical. Hence, this study contributes to existing corruption risk measurement frameworks by expanding on the repertoire of identified corruption strategies and gauging trade-offs and complementarities between more and less visible strategies. These insights strengthen the evidence base for data-driven governance and support the development of policy instruments that can more effectively detect corruption in public procurement.

Many corruption risk indicators are identified and validated using predictive models with single bidding on competitive markets as the dependent variable (Fazekas and Kocsis, 2020). This literature predicts single bidding with the use of structured procurement information on the products purchased, the characteristics of the tender and its outcomes. This study takes these models as a starting point and improves them by developing a combined outcome variable that incorporates both single bidding and supplier's spending share for a more comprehensive approximation of limited competition; in addition it also adds text-based indicators as predictors. These refinements not only improve corruption risk prediction methods, but also help generate insights for policymakers seeking to design targeted monitoring and oversight mechanisms. Given that procurement texts can describe a range of tendering features, we explore which types of texts are most important for predicting corruption risks, understood as limited competition in public tenders. The results' robustness is also tested by using the commonly used indicator of restricted competition, single bidding on its own as the outcome variable.

We analyze online available, official government data on 82,590 Hungarian government contracts from 2012-2020. The Hungarian dataset is part of the Government Transparency Institute's Global Public Procurement Database and can be accessed at opentender.eu/hu/download. We use text mining methods to extract, pre-process and analyze the textual and structured information. First, we replicate past research predicting competition restriction, making use of a host of structured product and market features such as contract value (structural constraints), in combination with well-documented corruption risk indicators



such as non-open procedure types (baseline models). Then we train Logistic Regression, Random Forest, and Boosting models adding word n-grams and text meta-features to the baseline models. For external validity tests, we correlate predicted scores of Random Forest models trained only on text fields with two established proxies for favoritism: suppliers' political connections and tender prices. We expect that firms with political connections are more likely to benefit from restricted competition, and that higher prices are associated with restricted competition as captured by text fields.

Our findings point out that the models using textual information outperform the baseline models in predicting competition restriction. This confirms our expectation that subtle, text-based corruption strategies can be used in addition to other, more visible strategies for achieving corrupt ends. To explore which texts are most important for measuring corruption risks, we trained different models using texts containing requirements for bidding firms, the award criteria used to score bids, and product descriptions. We found that eligibility criteria are the least impactful for predicting competition restriction, while the text in product description has the highest predictive power. Unpacking the highest prediction accuracy model demonstrates that frequent restrictive technical and financial conditions coupled with highly specific product descriptions greatly increase the probability of competition restriction. These insights highlight how text mining can expand corruption monitoring frameworks and equip policymakers with practical tools to detect competition restriction.

Our text-as-data approach contributes to both the academic literature and policy applications aimed at understanding, measuring, and identifying corruption. First, unlike most text-as-data approaches which only use textual information for predicting single bidding and related outcomes, we explicitly combine already established models using structured variables with text-based features. This allows us to assess the added value of text mining methods in the literature (e.g. Modrušan et al, 2020). Second, we depart from the text-as-data literature in the field by building on all readily available text fields in public procurement announcements. This allows us to better understand different types of competition restrictions hidden in different legal and technical texts, some applying to the product, some to the bidder, some to the bidding process itself. Qualitative evidence pointed out the simultaneous relevance of all 3 text types, yet explicit testing and comparisons in large-scale text analysis has not been done yet. Third, we unpack our complex and flexible machine learning models on top of identifying the best performing model, again, going beyond the state-of-the-art in this field (e.g. Acikalin et al, 2023, Lima et al, 2023). Opening up the black box of predictive models allows both to directly relate to qualitative studies predicting certain types of restrictive terms, and to offer risk predictions which are more actionable for practitioners. Finally, our novel results in corruption risk measurement aligned with prior literature can also serve as a valuable input into better targeting anti-corruption policies and for corruption investigations such as the precise identification of investigative leads (OECD, 2019).

The manuscript is structured as the following: First, we outline the conceptual framework of the analysis defining corruption in public procurement and how corrupt transactions are conducted. This section derives empirical expectations guiding the empirical analysis. Second, we introduce our large-scale administrative data and methods. Third, we review our findings and finally we outline our conclusions and areas for further development.

Conceptual framework

Following prior literature analysing corruption risks in public procurement, we understand corruption predominantly as a limitation of open access to public resources (*North et al, 2009*). This framing departs from often used principal agent theories or accounts equating corruption with bribery. The key expectation of non-corrupt, well-functioning public procurement markets as laid down in laws and also supported by academic theories is open and fair competition. This means that all those companies who reasonably can deliver the requested goods and services should be able to bid and should be assessed fairly (*Yukins, 2007*). These principles are violated when certain bidders are treated unequally, for example by being excluded even though they could reasonably participate in the tender. This violation of principles of good public procurement chimes with broader concepts in political science revolving around impartiality in the implementation of public policies (*Rothstein and Teorell, 2008*).

Hence, we define corruption in public procurement as the allocation and performance of public procurement contracts by bending prior explicit rules and principles of open and fair public procurement in order to benefit a closed network while denying access to all others (*Fazekas et al, 2016*). This definition implies a specific measurement approach. Indicators follow from this definition which capture biases in the procurement process that are typically used for favoring a selected bidder. Further indicators also follow from this definition which point at successful competition restriction, that is tendering outcomes indicating limited competition and repeated success of the same firm. For example, when a public buyer artificially creates a situation of emergency (e.g. deliberately announcing the tender late given known project deadlines) and uses it to award a non-competitive contract to a connected company, we talk about a corrupt scenario. However, it is important to bear in mind that low competition is not equal to corruption, instead when competition is deliberately limited to favour a particular bidder is when we can talk about corruption.

A number of measurement instruments have been proposed on the basis of this theoretical framework leading to a wide range of promising and a few validated corruption risk indicators, or proxies (*Fazekas et al, 2018; Gnaldi et al, 2021, Villamil et al, 2023*). Almost exclusively, these indicators make use of structured fields such as procedure type used, contract value, or bidder location (**Table A1**). The attractiveness of these indicators is that they rest on readily available or at least readily processible information in public procurement administrative records and datasets (*Fazekas and Saussier, 2018*).

This rich prior literature has identified and validity tested a range of indicators which proxy corrupt strategies in public procurement. The underlying strategies typically make use of features of the tender which are easily visible, verifiable to outsiders. For example, not advertising a call for tenders on a government publication website is by default verifiable for auditors, civil society or interested bidders. Implementing these strategies requires some legal expertise regarding key features of public procurement rules, e.g. rules defining when direct awards can be made as opposed to running an open tender. However, they rarely require sophisticated technical and economic skills which are needed for tailoring tendering terms to a pre-selected bidder. These different features, visibility versus technical sophistication, are what set aside hitherto extensively studied compared to understudied corrupt strategies.

A corrupt group, in particular a corrupt procuring entity, will resort to visible, but easy to implement strategies when it is confident that monitoring agents will not uncover or punish it for corruption; or when it is confident that its corrupt acts will look legal. However, whenever it needs to hide its corrupt dealings more carefully or the legal framework makes it hard to conceal corruption, it will resort to more subtle methods of excluding non-connected bidders and favouring those with connections. Among the wide range of corruption techniques identified in the academic and policy literature (e.g. OECD, 2007), tailoring tendering terms favouring a particular bidder are by far the most widely used, at least based on case studies. Hence, tailoring tendering terms to a particular company and its products can be used to constrain competition in public procurement (i.e. limit access to public resources) *in addition to* visible, formal corruption strategies. These arguments lead to the following hypothesis:

H1: Constraining conditions and criteria in tender texts represent an avenue to unjustified limitation to competition in addition to formalistic, visible procurement process biases.

However, not all sections of the tendering documents can be used to limit competition in the same way. There are 3 major areas of the tender documentation which can be used for subtly favouring a certain bidder:

1. **Product description:** This section of the tender documentation precisely defines the products (goods, works, and services) that are purchased (*Gorgun et al, 2020*). Hence, this is where the specific products of the favoured company can be targeted, in essence excluding all non-connected competitors with substitute products.
2. **Eligibility criteria:** This section of the tender documentation defines the preconditions whose fulfilment is necessary for any eligible bidder (*Rabuzin and Modrušan (2019); Modrušan et al., 2020*). This is where unwanted competitors can be excluded without being even considered as bidders.
3. **Award criteria:** This section of the tender documentation defines the scoring rule for eligible bids, that is once a company passed the eligibility criteria and its products fit the product description. Typically, this section only defines price as the main criteria for ranking submitted bids. However, when different price-related criteria and quality features are scored, subjective or biased scoring rules can be inserted which favour the connected bidder.

Nevertheless, each of these tender texts may only partially represent additional strategies to formalistic, visible competition restrictions. When the reason for non-competitive procurement procedures is product specificity or uniqueness, restrictions in the product description may correspond to the use of non-open procedure type and the non-publication of call for tenders. However, biases in award criteria are only needed for corrupt goals when there is competition expected between the connected and non-connected bidders, that is the procedure type is open and there was a call for tenders published.

More broadly, we can expect from an efficiently operating corrupt group to use these different tendering sections flexibly, placing the competition constraining conditions in the parts which fit the given context best. For example, if a favoured firm has a unique product, tailoring the product description is the optimal way for corrupting the tender. Hence, there is no need to place further competition constraining conditions in the other sections. However, if the favoured company only sells generic products such as office chairs, inserting favoritistic conditions in the

product description will be hard. This will make the other 2 sections more attractive avenues for corruption, by for example enabling competition but including unfair or easily gamed award criteria (i.e. conditions which are subjective hence can be used for favouring a connected bidder). Given the different functions of the 3 sections of the tender documentation and their correspondingly different corruption potential, we hypothesize:

H2: The 3 different main parts of the tender documentation (eligibility criteria, product description, and award criteria) can be used in additive, partially overlapping corruption strategies.

Data and indicators

Public procurement data

We use official governmental data on Hungarian public procurement between 2012 and 2020. The database is derived from online published public procurement announcements at the national publication portal www.kozbeszerzes.hu as collected and processed by the Government Transparency Institute. For full information see: <https://opentender.eu/hu>. The database contains all public procurement tenders and contracts conducted under Hungarian Public Procurement Law. The information is published in standard publications forms such as

1. Calls for tenders,
2. Contract award notices, and
3. Contract modification and correction notices.

As not all these kinds of announcements appear for each procedure (e.g. most non-competitive tendering procedures do not need to publish a call for tenders), we only have the variables deriving from contract award notices consistently across every procurement procedure.

As the source announcements are published as html pages, a web scraper algorithm was used to collect the source information. Then a structured database was created by parsing the html data into a pre-defined structure, containing variables with clear meaning and well-defined categories such as standardized procedure types or contract award announcement dates following the DDMMYYYY format. Furthermore, errors, inconsistencies, and omissions found in the source data were corrected or removed. After the data cleaning processes the final database has sufficient quality for scientific research as prior publications with this data demonstrated it (e.g. Fazekas et al, 2016). For a full description of database development, see Fazekas et al (2024).

The novel aspect of this database, which has not yet been analysed, is a corpus of raw texts. Six different text fields are available, which can be grouped into three main types:

1. Product description: tender title, tender description;
2. Eligibility criteria: personal, technical, and economic requirements; and
3. Award criteria: scoring rules for submitted bids.

Crucially, for the interpretation of the results and for understanding the limitations of the data, these text fields do not correspond to the full tender documentation, rather only encompass the summary and key fields in the public announcements. In all cases, there is more detailed and specific documentation which is only available for registered bidders, hence could not be

collected. In our analysis we analyse the collected texts following careful pre-processing steps, for more details see below.

While the dataset we use is extensive covering a long time period and very rich in detail, it suffers from a number of quality and scope limitations. First, as the source information follows standard publication formats which frequently change, the database is not always consistent over time (e.g. some variables may be missing for some publication types) and some errors might remain due to inconsistent source information. Second, the dataset only contains contracts above the mandatory publication thresholds of about 50,000 EUR where regulatory requirements, including transparency norms, are more stringent (for more information see: http://europam.eu/?module=country-profile&country=Hungary#info_PP). Moreover, there are other exceptions from publication requirements in the Hungarian Public Procurement Law, for example sectors such as high value defence spending are typically exempted hence not contained in our database. Third, there might be missing texts due to some public buyers not fully following legal requirements or exploiting loopholes for avoiding public scrutiny.

Structured variables: Red flags, structural constraints and validation variables

Three groups of variables are derived from structured data: corruption risk indicators (both dependent and independent variables), structural constraints and validation variables. Corruption risk indicators, or red flags, approximate corruption occurring in individual public procurement tenders and contracts. Importantly, they should not be interpreted as indications of whether corruption has actually occurred.

We develop a composite indicator for competition restriction, as the outcome variable for our models, which captures both the intensity of the competition in a given tender and the market dominance of the winning supplier. Specifically, we combine the single bidding indicator, that is one bid submitted on an otherwise competitive market; and the supplier's spending share which represents the share of contract value won by the supplier within the total spending of the buyer within each year.

Single bidding on competitive markets is the simplest indicator of competition restriction, hence a core risk factor of corruption (Titl, 2025). It has been shown across a wide set of countries, including Hungary, that single bidding is associated with overpricing at the bidding stage, other risk factors such as tax haven registration of the supplier (Fazekas and Kocsis, 2020), and also perceptions and self-reported experiences with corruption (Charron *et al*, 2017). Supplier's spending share captures how the local market is dominated by a single firm. Hence, high spending share supplies which purport more than 50% of the buyer's spending, are considered risky as they can potentially capture the buyer as it becomes highly dependent on them. Whereas suppliers with low spending shares, less than 50%, do not have a dominant position typically hence are considered low risk of competition restriction (Fazekas *et al*, 2016). For calculating this indicator, buyers with fewer than five contracts per year are excluded to prevent skewed results.

We construct the combined metric of competition restriction based on the two binary indicators: single bidding (yes/no) and supplier's spending share (above/below 50%). Each indicator takes the value of 1 if a risk condition is present, and 0 otherwise. We assign a value of 1 to the combined metric if at least one indicator takes the value of 1, otherwise 0.

Furthermore, we calculate risk factors to be used on the independent side of the model. These factors signal behaviors or situations often leading to deliberately restricted competition in public procurement (*Fazekas and Kocsis, 2020*). They point at process biases which can be used and in fact often are used to exclude unwanted, unconnected bidders. Of the longer list of such indicators (e.g. *Ferwerda et al, 2017; Gnaldi and Sarto, 2024*), we only adopt those indicators for the subsequent analysis which are applicable to Hungary, while also potentially can be calculated for a wider set of countries. In addition, we also excluded red flags which overlap with text-based indicators, for which we develop a wider set of new indices. Our red flag list, used as predictors in the models is:

- **Non-open procedure type:** using procedure types which exclude bidders by definition such as direct awards or negotiated procedure without prior publication represent a straightforward way for favouring a connected bidder. Moreover, those procedure types which retain some, but only partially requirements of open competition such as invitation tenders, can also be abused for favoritistic and corrupt ends.
- **No call for tenders published:** When the call for tenders are not published in the official journal, it is much harder for interested bidders to learn about tendering opportunities. Hence, informing the connected bidder about a tender while avoiding a public announcement can restrict competition and disadvantage non-connected bidders.
- **Suspiciously short submission period length:** When an open tender has to be run following transparency requirements, defining an unusually short submission or advertisement period (i.e. the number of days between publishing the call for tenders and the bid submission deadline) can put non-connected bidders at a disadvantage. This happens when bidders who only learn about the tender from the public announcement have too little time to put together high quality, competitive bids, compared to a connected bidder who received the information about the tender earlier.
- **Suspiciously short decision period length:** When the decision period is very short (i.e. the number of days between bid submission deadline and contract award decision date) it can indicate that the buyer did not consider bidders carefully, rather made snap decisions in favour of the connected bidder.

In addition to red flags, our models include a range of structural constraints which take account of different degrees of complexity, product market specificities, and somewhat different regulations (e.g. state-owned enterprises face different procedure type thresholds than central government ministries). These variables are contract value, economic sector (2-digit Common Procurement Vocabulary (CPV) codes), year (based on contract award announcement), buyer location (region of the buyer), buyer type (e.g. central government entity), and buyer main activity (e.g. education or healthcare).

In order to provide evidence for external model validity, we additionally correlate modelling predictions with established metrics and outcomes of corruption. To this end, we used political



connections of suppliers and relative prices. Relative price is defined as the ratio of the contract value to its estimated value. A value below 1 indicates a discount offered by the winning supplier; for example, a relative price of 0.97 signifies a 3% discount compared to the auction reference price. To measure political connections, we combined multiple lists collected by different institutions to identify the widest set of politically connected firms. First, we use our own collection based on official records, supplemented by databases from Transparency International's Tenderbajnok⁶ initiative and the Corruption Research Center Budapest (CRCB)⁷ compilation. Combining sources using different data collection methods, allowed us for a comprehensive mapping of politically connected firms.

Text-based indicators and text processing

We add text-based indicators to the structured variables described above, inspired by a small but growing, diverse literature (*Winters, 2014; Fazekas and Kocsis, 2020; Gorgun et al, 2022*). This includes creating n-grams, that is combinations of 3, 4, and 5 words as predictors. We use an n-gram vectorizer because of our focus on interpretability, also they align closely with how corrupt buyers typically operate (by inserting small, restrictive conditions) making patterns easier to detect and understand. Beyond the word vectors we include meta-characteristics of the text fields such as length or uniqueness in the model. We calculate these variables for each of the 3 different text field types introduced above: product description, eligibility criteria, and award criteria.

In order to calculate these variables, the first step is to clean raw text data. We conduct careful preprocessing to standardize texts and to remove noise as much as possible. We carry out preprocessing in multiple steps, trying out different methods and degrees of preprocessing as they can greatly influence modelling outcomes and accuracy (*Denny and Spirling 2018*). Python was used to perform preprocessing. We used the Spacy and NLTK packages, which are available in Hungarian.

Our first step of preprocessing is lemmatization (i.e. finding the dictionary form or root of words). For Hungarian (being an agglutinating language), the otherwise faster stemming does not work. The difference between the two is that the former looks for the actual lemma, while the latter only cuts off the suffixes from the end of the word. We use the HuSpaCy⁸ package for this task. Second, stop-word removal follows. We use the stop-word list available for Hungarian from the NLTK package. We remove a range of frequently occurring stop-words such as the Hungarian equivalents of “a”, “the”, “and”, etc. As part of stop-word removal, we also remove numbers. While numbers may be important - as for example they can indicate laws and sections of laws or specific eligibility parameters - removing numbers improves model interpretability. This is because competition restriction happens through overspecifying legal and technical conditions. However, it is not central to our claim whether restrictions happen through any particular paragraph or eligibility condition, rather their frequency of use.⁹ Moreover, we also eliminate words shorter than 2 characters, in addition to the removal of stop words. Looking at the word

⁶ <https://tenderbajnok.transparency.hu>

⁷ <https://www.crcb.eu>

⁸ <https://github.com/huspace/huspace>

⁹ Underpinning these claims, our overall model accuracy did not meaningfully improve by retaining numbers through pre-processing. By implication retaining numbers would have increased model complexity at little to no model accuracy gain.

frequency list and the results of the models, such a strict pre-processing delivers the best balance between interpretability and model accuracy.

After careful text pre-processing, we create predictors from n-grams by converting the processed text data to numerical vectors – in the form of a sparse matrix – suitable for analysis. We use the TF-IDF (Term Frequency-Inverse Document Frequency) vectorizer from the scikit-learn¹⁰ package in Python. TF-IDF reflects the importance of a term in a document relative to a collection of documents.

We also calculate text meta characteristics, but please note that we calculated them on the raw, unprocessed texts. These meta variables aim at capturing the macro features of each text either on its own (e.g. law reference) or compared to other texts (e.g. uniqueness score or lexical diversity). Calculating the ratio of numbers and tracking references to laws allows us to retain some of the information removed during pre-processing, i.e. removing numbers. Moreover, including the length of texts (normalized by economic sector average) allows the models to consider the overall frequency of words in a text in addition to the occurrence of specific words and word combinations.

Final dataset

The final, cleaned dataset used in the analysis contains 82,590 contracts from 2012-2020. The number of contracts follows a cyclical distribution across years (**Figure A1**). **Table A2** lists the variables used in the analysis along with their definitions and descriptive statistics. First, we show the outcome variables used in the analysis: single bidding, and the combined variable. Second, we describe the structural constraints used: contract value, product sector (2-digit CPV code), tender year, buyer location, buyer type (e.g. central government bodies), and buyer main activities. Third, we present structured red flags of corruption used in prior research. Our models incorporate widely used corruption risk indicators such as submission period length, no call for tender publication, non-open procedure type, and decision period length. No call for tenders is the most widespread risk factor in our dataset. Third, we describe the 3 main text field types in public procurement announcements, highlighting that product descriptions are the most widely available text field. Then, we show the text-based meta variables such as uniqueness, lexical diversity, the mention of numbers and laws. Finally, we present the validation variables: relative price and politically connected firms.

Methods: Predictive Models

In order to test our hypotheses we build on a small but quickly growing literature making use of texts and specific terms in public procurement and more broadly project finance (e.g. *Winters, 2014; Modrusan et al, 2020*). Specifically, we estimate supervised learning models which predict restricted competition (1 = restricted competition; 0 = no restriction of competition) with the help of structured data as well as text. We estimate a wide range of models with the goal of identifying the most accurate models including text-as-data on top of already tried and used structured information. When deciding which kinds of predictive models to estimate, we aim to balance interpretability with predictive power. Hence, we estimate 1) binary logistic regression,

¹⁰ <https://scikit-learn.org/stable/>



2) random forest, and 3) XGBoost models (*Gareth et al, 2021*).¹¹ Specifically, we use the built-in logistic regression¹² and random forest models from the scikit-learn package in Python. We divide our data into two parts: 80% of the observations belong to the train and 20% to the test set, we use the default settings of the models. To ensure comparability, we use the same training and test sets for all models. To test the robustness of the models, we also ran the same models using single bidding as the outcome variable, in addition to the combined variable (see: Annex).

Logistic regression is a statistical method used for binary classification problems, where the outcome variable is categorical with two possible values. The model estimates coefficients for the input features to make predictions, which makes the interpretation of the model easy and understandable. Random Forests and XGBoost are ensemble models built on multiple decision trees. Ensemble techniques let us train more accurate and more stable predictive models, by combining the output from a large number of individual models (in this case each individual decision tree), each of which have been estimated on a slightly different sample with somewhat different parameters. In Random Forest models each tree is built independently, and the final prediction is a majority vote of the individual tree predictions. With a higher number of trees we can achieve better performance but running a model with a high number of trees is very resource intensive (i.e. takes a long time to run on ordinary machines). An advantage of Random Forest models is that they handle imbalanced datasets more effectively than regression models. In XGBoost models, trees are built sequentially, and each new tree corrects the errors of the combined ensemble of the previous trees.

To choose the most accurate predictive model, we compare the observed single bidding outcome with the predicted value on a dataset ‘not seen’ by the model (test set). We calculate 4 goodness-of-fit metrics: Precision, Recall, F1-score, Accuracy.

In order to provide additional evidence for the validity of our text-based scoring of competition restriction, we run a series of external validity tests using suppliers’ political connections and relative prices of the awarded contracts. In these cases, the logic is that if corrupt competition restriction is taking place, we expect political connections to be more prevalent and prices to be higher. First we trained a Random Forest model on the text-based features (n-grams and meta-characteristics) only and saved the model’s predictions into a probability score. We used t-test to understand the relationship between the binary variable measuring political connections and the predicted continuous score. Moreover, we used OLS regressions on the contract-level (with the structural constraints as control variables) to explain relative price with the predicted score.

Results

Understanding the role of text fields

The results section starts by estimating baseline regressions only including structured data on structural constraints and red flags (Table 2.4 for the combined variable and Table A3 for single bidding). Then we test H1 by adding different text parts independently and H2 by adding them

¹¹ We also run Support Vector Machines models, but they did not perform well compared to the other methods, so the results are not reported in this paper.

¹² With a higher number of iterations, to achieve accurate convergence.

all together. At each step, we show performance of logistic regression, random forest and XGBoost models.

The baseline models largely replicate Fazekas et al, 2016, but using not only single bidding as outcome variable but the combined metric also. Additionally, the baseline models are using longer time series and fewer red flags which are more readily applicable in a wide range of countries (*see for example, Fazekas and Kocsi, 2020*). The baseline models achieve moderate accuracy (ranging between 69% and 72% for the combined variable and somewhat higher 77% – 78% for the single bidding outcome) and f1-score (62% – 70% for the combined variable and 70% – 77% for single bidding) of outcomes correctly classified (Table 2 for the combined variable and Table A3 for single bidding). By way of comparison, given that restricted competition is observed for about 30% of contracts, a naive estimation classifying all contracts as not restricted would achieve 70% accuracy.¹³ However, f1 scores balance against such a naive estimation and the baseline models achieve moderately high scores compared to a naive estimation's f1 score (f1 of classifying all cases as zeros would be 0). As it will be systematically shown, random forest models perform generally better than traditional logistic regression and XGBoost models.

Table 2.4 – Baseline models, binary logistic regressions, Random Forests, XGBoosts using structural constraints and red flags, Hungary, 2012-2020

	precision	recall	f1-score	accuracy
Logistic Regression: structural constraints only	0.66	0.70	0.61	0.70
Random Forest: structural constraints only	0.68	0.69	0.68	0.69
Boosting: structural constraints only	0.70	0.62	0.70	0.72
Logistic Regression: structural constraints and red flags	0.67	0.70	0.62	0.70
Random Forest: structural constraints and red flags	0.69	0.70	0.69	0.70
Boosting: structural constraints and red flags	0.69	0.70	0.69	0.70

Now we turn to testing H1 by adding different text-based variables to the baseline models (Table 2.5 and Table A4). The text fields referring to different parts of the tendering documents - and hence different types of constraints on competition - are added separately: eligibility criteria texts, product description texts, and award criteria texts. Each Random Forest model considerably outperforms the baseline models achieving accuracy between 76% and 79% (f1-score of 0.74-0.76) for the composite competition outcome variable. For single bidding, accuracy ranges between 78% and 83% and f1 score between 71% and 81%. Logistic regressions and XGBoost models are less accurate, they perform similarly to the baseline models. As the Random Forest models achieve accuracies considerably above the baseline models, which already include structural constraints and validated red flags, we conclude that texts confer additional explanatory power for competition restriction in addition to structured information. This suggests that constraining conditions and criteria in tender texts allow for limiting competition within narrowly defined product markets, even when formalistic, visible procurement process biases are not present. This hypothesis is also confirmed by the robustness tests.

¹³ Single bidding is observed for about 23% of contracts, a naive estimation classifying all contracts as not single bidder would achieve 77% accuracy.

Table 2.5 – Text based models, binary logistic regressions, Random Forests, and XGBoost using red flags, structural constraints, and texts, Hungary, 2012-2020

	precision	recall	f1-score	accuracy
PRODUCT DESCRIPTION				
Logistic Regression: texts, red flags and structural constraints	0.66	0.70	0.62	0.70
Random Forest: texts, red flags and structural constraints	0.79	0.79	0.76	0.79
Boosting: texts, red flags and structural constraints	0.72	0.71	0.61	0.71
ELIGIBILITY CRITERIA				
Logistic Regression: texts, red flags and structural constraints	0.67	0.70	0.62	0.70
Random Forest: texts, red flags and structural constraints	0.74	0.76	0.74	0.76
Boosting: texts, red flags and structural constraints	0.70	0.71	0.61	0.70
AWARD CRITERIA				
Logistic Regression: texts, red flags and structural constraints	0.66	0.70	0.61	0.70
Random Forest: texts, red flags and structural constraints	0.77	0.78	0.76	0.78
Boosting: texts, red flags and structural constraints	0.70	0.70	0.60	0.70

Note: Best model is highlighted in bold.

Now, we turn to testing H2 by adding the different text-based variables all at once and comparing model performance with previous models containing texts (Table 2.6 and Table A5). The Random Forest model achieves the best prediction accuracy, correctly classifying 79% of contracts (f1-score=0.77, for single bidding accuracy is 84% and f1-score 82%). The slightly improving model fit suggests that hard-to-identify constraints to competition in different parts of the tender documentation offer complementary avenues to achieving corrupt ends.

Table 2.6 – Text based models, binary logistic regressions, and Random Forests using red flags, structural constraints, and all three text parts, Hungary, 2012-2020

	precision	recall	f1-score	accuracy
Logistic Regression: texts, red flags and structural constraints	0.67	0.70	0.62	0.70
Random Forest: texts, red flags and structural constraints	0.79	0.79	0.77	0.79
Boosting: texts, red flags and structural constraints	0.72	0.71	0.61	0.71

Note: Best model is highlighted in bold.

Next, we investigate H2 further by looking at the impact of text fields under 2 competition scenarios: 1) when there is no call for tenders published - little to no open competition is expected, and 2) when a call for tenders is published - open competition is expected. We argue that texts can be used to justify the use of non-competitive procedure types (restrictive product description), but they can also bias the formally open competition (subjective award criteria).

Hence, the role of product descriptions and award criteria may vary by call for tenders publication status.

As a starting point for this analysis, we establish the different availability of different text fields in the 2 sub-samples: with/without call for tenders publication (Table 2.7). Clearly, eligibility criteria are only available in tenders with call for tenders publication, so they are not suitable for comparing across the 2 subsamples. As for product description and award criteria, missing rates vary somewhat, still allowing for comparisons across subsamples. Moreover, price-only award criteria are used approximately to the same degree in the 2 subsamples too, warranting meaningful comparisons.

Table 2.7 – Share of missing and price only texts by text type and publication of call for tenders status (yes/no), Hungary, 2012-2020

Text type	Share of	Call for tenders published	No call for tenders published
product description	no text in title	0.00%	0.02%
product description	no text in description	0.08%	0.2%
eligibility criteria	no text in personal requirements	0.48%	100%
eligibility criteria	no text in economic requirements	0.20%	100%
eligibility criteria	no text in technical requirements	0.21%	100%
award criteria	no text in award criteria	0.07%	0.25%
award criteria	price only in award criteria	55.44%	53.03%

Turning to the impact of text fields on model performance in the 2 subsamples, we see marked differences (Table 2.8 and Table A6). Product descriptions improve model performance both with and without call for tenders, but the impact is much larger in the no call published subsample with accuracy increasing from 69% to 79%. This confirms our expectation that specific or tailored product descriptions are more often used as a justification for non-competitive tenders rather than to bias open tenders. As for award criteria, the results are opposite, that is accuracy improves further in the call for tenders published subsample 76% vs 74% while accuracy even slightly decreases in the no call for tenders published subsample (78% vs 79%). This suggests that the relative importance of award criteria biases is higher when competition is expected. Both conclusions are essentially the same in the robustness models.

Table 2.8 – Random Forest models using red flags, structural constraints, and texts, for tenders with and without a published call, Hungary, 2012-2020

	Call published		No call published	
	f1-score	accuracy	f1-score	accuracy
NO TEXT: red flags and structural constraints only	0.73	0.72	0.69	0.69
PRODUCT DESCRIPTION: texts, red flags and structural constraints	0.72	0.74	0.77	0.79
AWARD CRITERIA: texts, red flags and structural constraints	0.75	0.76	0.77	0.78

Model validation

To validate our best model and assess its robustness, we trained a Random Forest model using text data only. We then applied t-tests to evaluate the relationship between political connections and the predicted risk score. Additionally, we conducted OLS regressions to analyze the effect of the predicted score on relative prices.

First, we examine political connections. We expect that firms with political connections would have a higher probability of restricted competition, inferred from text data. To test this, we perform a t-test (Table A7) comparing the predicted scores of politically connected and non-connected firms. The scores for politically connected firms are slightly higher than those for non-connected ones. This suggests that our model behaves as expected, there may be substantial measurement errors, for example many false negatives in identifying politically exposed firms, likely due to the inherent difficulty of tracking political connections from publicly available sources (for a recent advance in identifying political connected firms see Titl et al, 2024).

The second test focuses on relative prices. We run OLS regressions with structural constraints and risk scores, using relative prices as the dependent variable and the Random Forest-based predicted score as the key predictor (Table A8). The regression models include red flags and control variables such as tender year, bid price, CPV code, NUTS code, buyer type, and buyer main activities. The results indicate a consistent increase in prices (1.4-2.7%) when the text-based risk indicators are present. This suggests that text-based competition restrictions do benefit the winning supplier allowing it to charge higher prices, in line with our expectations.

Although the results of the validation are only moderately strong, we consistently find relationships consistent with a corruption-related interpretation of our text-based risks scores. The Random Forest-based probability scores show a small but statistically significant association with political connections and larger and significant relative price increases. These findings support the validity and robustness of our results.

Unpacking text-based models

While the overall performance of different models offer supporting evidence for H1 and H2, the models have remained a black box so far. In order to improve the interpretability of our results and link them back to our theoretical framework, we explore the tender texts qualitatively. We hope to identify specific terms and conditions which are tailored to favoured firms by looking into our models.

We look at the *full, raw texts of description, eligibility criteria and award criteria fields* in tenders where the best model without texts incorrectly predicts no competition restriction, while the best model with texts correctly predict competition restriction.¹⁴ Such tenders and contracts should represent those cases where the inclusion of text crucially contributes to a more precise identification of competition restrictions, in spite of formally open procedures.

¹⁴ For tractability, our review concentrates on contracts with full tender text information in all text fields from major buyers.

While there is a considerable amount of noise, especially a large volume of texts which are short and vague (recall we only work with official announcement texts, while full technical details are in separate tender documentations we do not have access to), we can confirm theoretically sound tendencies. The frequent, lengthy and complicated exclusion criteria appear to co-occur with competition restriction in spite of formally open procedural features (e.g. there are personal requirement fields which are 2-3 standard deviation above the mean length of such fields and contain a bewildering array of legal references to certificates and proofs to be submitted). Moreover, criteria and requirement specificity also comes up as associated with correctly predicting the incidence of competition restriction. Among eligibility criteria, some surprisingly specific conditions are associated with competition restriction in spite of no apparent, structural red flag for competition limitation: *“Has reference(s) with CPV code 33111720-4 for angiographic equipment within the last 3 years from the date of dispatch of the invitation to tender (see point VI.5 of this notice), with the following quantities of a reference delivered in accordance with the specifications and the contract: 19 for the 1st part, 193 for the 2nd part, 193 for the 3rd part 23 for the 4th part [...] 28 for 20th part, 19 for the 21st part, 19 for the 22nd part, 8 for 23rd part. [etc.]”*. It is hard to fathom why so specific numbers of delivery references are required instead of a general minimum amount or interval. Finally, when it comes to award criteria texts, the overwhelming majority of tenders award contracts based on price with some further tenders also considering objective, numerical award criteria such as delivery speed, guarantee length, or damages payments. In a few cases, arguably subjective criteria co-occurs with competition restriction even in the absence of formal, visible competition restrictions. For example, a contract award was partially based on the “quality of the organisational plan”, coinciding with earlier examples cited in Fazekas and Kocsis (2020).

Conclusions and further work

We built a comprehensive database of public procurement tenders and contracts for Hungary, spanning over a decade, accounting for roughly one third of public spending. The analysis identifies subtle, text-based strategies for limiting competition by tailoring the bidding conditions to a potentially favoured company. This can include specifying the purchase of a unique product, the excessive use of exclusion conditions, putting high weight on idiosyncratic, specific experience, and the requirement of unreasonably extensive prior experience (e.g. past turnover). We found evidence that these potentially corrupt strategies are mostly employed by actors when more visible strategies for favouritism, such as non-competitive tendering procedures, are undesirable or impractical. The inclusion of text based information improves overall model prediction accuracy from 70% to 79%, with each text field type additionally contributing to model performance. Nevertheless, text-based strategies can also support visible corruption strategies, such as the use of non-competitive procedure types justified by the requirement for purchasing a highly specific product. Robustness tests (single bidding as an alternative outcome variable) and external validity tests, using politically connected firms and tender prices, confirm our model’s validity and reliability.

These results can be incorporated into existing risk prediction tools used by anti-corruption and other law enforcement bodies. New indicators could pinpoint new investigative targets or help identify risky tenders of already investigated organisations. The broader, pro-competition applications of the tool could support the better use of standard bidding documents and tendering conditions which are already in use across the globe. By identifying typical competition



restrictions, official guidance could discourage their use in future tenders. Moreover, buyers more frequently using restrictive tendering conditions could be targeted by administrative interventions such as offering capacity development or enhanced supervision of planned tenders.

While we gather evidence on the importance and strategic use of texts for furthering deliberate competition restrictions and hence we contribute to the growing literature; our analysis is limited in a number of ways. Most importantly, our dataset contains a high rate of missing data which is most likely due to lots of texts being delegated to full tender documents rather than the official tender announcements (recall, we collected data from the latter but could not access the former due to its irregular structure and varying formats such as scanned pdfs). Further work along the lines of this paper should improve model prediction accuracy, for example by drilling deeper into sectoral differences. If the approach turns out to be fruitful and valuable, it should be extended to other countries using different languages in public procurement (following for example, Gorgun et al (2020)). The long-term ambition is to extend the regular toolkit of corruption red flagging by researchers and policy actors using text-as-data. Further research could explore other proxies for corruption and limited competition, such as sanctioned firms. These new dependent variables could in particular unpack the complex dynamics in multiple bidder tenders where favouritism is at play and hence, among others, award criteria is applied in a biased manner to favour the connected bidder over the other bidders in the tender.

3. Intra-government transfers / grants

3.1 Definitions

Political favoritism in intra-governmental transfers refers to the practice where political actors allocate public resources not based on objective, public-regarding criteria such as need or efficiency, but rather according to political considerations. This often manifests in preferential treatment of subnational governments or localities that share political or other forms of alignment with higher-level authorities. While not all forms of political influence in resource distribution are inherently corrupt, certain patterns of favoritism signal deviations from the principles of equitable governance. It is therefore important to distinguish between different types of political favoritism that may occur in the allocation of resources across government tiers.

Political favoritism takes various forms depending on the context. One key type is favoritism based on **political alignment**, when funds flow to jurisdictions ruled by the same party or coalition as the national or regional government. Such alignment helps politicians to establish reliable connections with local elites, who in turn secure the incumbent's electoral success in their constituencies as well as ensure support of national policies. Another type of favoritism emerges from voter preferences and **the extent of electoral competition** within localities. In this setting, incumbent elites face a strategic choice between investing in areas of "core" support or targeting "swing" uncertain districts, conditional on which would bring higher electoral returns. Lastly, **personal motives** similarly bias the allocation process, whereby funding decisions are determined by a politician's birthplace, ethnic affiliation, career path, or other sorts of connections. These various forms of political favoritism can operate simultaneously, reflecting the complexity of political incentives underlying spending priorities.

In essence, political favoritism fundamentally differs from universalistic or formula-based distribution, which is ideally driven by objective criteria such as population size, economic need, or development gaps. For instance, funds may be channeled to regions with aligned local government or many swing voters, rather than to places in greater need. That is, favoritism distorts resource allocation by privileging political loyalty or personal interests over efficiency and equity. This in turn leads to inefficiencies, reduced transparency, and potential inequalities in public service delivery, undermining the legitimacy of fiscal federalism and democratic accountability. Understanding these distinctions is crucial for designing policies that promote transparent allocation of public resources.

3.2 Indicators of favoritism in intra-government transfers

3.2.1 Favoritism due to political alignment

As research suggests, incumbent elites are inclined to allocate more funds, especially those of discretionary nature, to aligned subnational authorities (Table 3.1). Political alignment can take various forms, depending on the type of political system, level of decentralization and other factors, but in principle refers to the situation when lower-level governments (municipalities, districts, states) share the same party affiliation or coalition with the upper-level government or its leader like the president. Even though the eventual composition of local elected governments is determined by voters, it is the role of those elites and the fact of their (non-)alignment with



the upper level which is emphasized in this strand of literature. As expected, aligned local elites serve as compliant agents of the center who are more likely to back its agenda and secure electoral support at the respective constituencies.

Importantly, however, not all forms of political alignment imply favoritism-driven spending. While some indeed lead to diversion of tax-financed public resources, others mostly capture convergence between different levels of government on non-corrupt grounds like common ideology and policy objectives.

One of the rationales behind particularistic transfers is *investment in electoral success*. This type is particularly problematic because it diverts public resources consisting of taxpayers' money to advance the private interests of political elites – most notably, to stay in power. Re-election is therefore seen as the main goal of public grants disbursement, whereby loyal municipal authorities, proxied by alignment, function as optimal political machines in maximizing votes. Either through rewarding them for past performance or co-opting ahead of the upcoming elections, the system creates a feedback loop centered around potential rotations of power.

In Spain, for example, funds flow to aligned municipalities because there is a clear political return in the form of votes (Solé-Ollé & Sorribas-Navarro 2008), whereas in Portugal, aligned municipalities get almost 20% larger targetable transfers and this further translates into the higher vote share for one of the two major parties (Migueis 2013). Aligned Italian communes similarly accrue more national grants, especially before municipal elections such that co-partisan mayors are more likely to remain in office (Bracco et al. 2015). In the case of Ghana, where intergovernmental transfers exhibit a pronounced electoral dynamics, those are predominantly channeled to aligned districts throughout the entire cycle (Brunnschweiler & Obeng 2022). A mutually beneficial relationship between local and national elites is also observed in Colombia: incoming presidential candidates get significantly more votes from constituencies where mayors are affiliated with the candidates' coalition. Those mayors, in turn, are subsequently rewarded with discretionary funds from the center (Bonilla-Mejía & Higuera-Mendieta 2017).

Arguably, the logic may not apply to all grants universally but rather to “visible” projects that can bring tangible electoral gains and attribute spent money to the respective party, as Muraközy and Telegdy (2016) demonstrate on Hungarian construction projects. Likewise, in the years preceding local elections in Brazil, municipalities currently aligned with the federal level get considerably larger transfers for infrastructure (Brollo & Nannicini 2012). Comparable evidence from disaster declarations in the US reveals that areas where the governor is co-partisan with the president receive almost twice as many relief declarations, the effect appearing only after hurricanes of medium strength (Schneider & Kunze 2025). Alternatively, the positive association between transfers and popular support might hold only when spending actually corresponds to local needs (Dellmuth & Chalmers 2018).

From another perspective, central elites may favor aligned constituencies in order to *strengthen the vertical of power* – that is, enhance coherence across different levels of government and/or ruling party. Instead of being primarily election-centered, such transfers are aimed at facilitating the governance process more broadly. Specifically, the government might need to prevent policy diversion or shirking of lower-level authorities, advance own legislative agenda, ensure national policy implementation, as well as improve party discipline. While more policy-oriented and

potentially socially beneficial, this pattern of funds distribution remains not entirely impartial, especially in its tendency to empower the incumbent party vis-à-vis the opposition.

Particularly, cooperation on major investment projects which benefit both local and central actors can help establish reliable connections within the party across its multiple layers, also ensuring that the projects do not face resistance from the opposition (Muraközy & Telegdy 2016). Baskaran and Hessami (2017) equally find that in Germany, where municipalities have discretion over execution of national policies, there is still a lack of tools to secure their compliance with the federal agenda. Increased discretionary grants can therefore buy off cooperation and reduce resistance of local elites.

In turn, several studies on the US highlight presidential influence in tightening the party line, such as through federal outlays flowing to the states with co-partisan governors (Larcinese, Rizzo & Testa 2006) or funds given out to states/counties whose representatives are predominantly from the president’s party (Larcinese, Rizzo & Testa 2006; Berry, Burden & Howell 2010). Moreover, Curto-Grau & Zudenkova (2018) reveal that party loyalty can be electorally costly for elites, going against the interests of their constituents. In this case, allegiance to the party has to be remunerated with higher discretionary spending to avert the risk of sabotage.

Finally, transfers driven by partisan alignment do not necessarily involve favoritism. In contrast to the two above rationales, alignment can be positively associated with spending for non-corrupt reasons, namely *shared ideology and policy goals*. Although potentially advantaged too, aligned municipalities accrue more resources primarily because they are viewed as reliable agents whose cooperation is assured through an ideational bond. Rather than partisan favoritism, the literature emphasizes broader structural inequality between subnational regions, especially in decentralized contexts where substantial competences are not matched by adequate fiscal resources (Falleti 2005).

The importance of ideological alignment is illustrated by Kleider et al. (2020), who consider the closeness between central and subnational governments on the “state-market” ideological dimension. The results across 14 OECD countries over 20 years show that besides partisan alignment, it is ideological proximity which equally leads to higher discretionary regional spending on policy areas such as education. Indeed, as the paper pinpoints, ideological alignment is an often overlooked factor capturing classic left-right policy preferences, which the current partisan-based measures fail to reflect. However, while conceptually relevant in distinguishing actual favoritism from value-aligned funding priorities, this indicator is somewhat sidelined in the existing literature.

Table 3.1 – Political alignment behind territorial funds allocation

Authors	Main predictor	Operationalisation	Country, years	Outcome variable
Baskaran & Hessami (2017)	Partisan alignment	Whether the municipality is ruled by the party bloc that is in power at the regional level	Germany, 1989–2010	Budget support transfers

Bracco, Lockwood, Porcelli & Redoano (2015)	Partisan alignment	Whether the mayor's party-coalition is the same as the coalition in power at the central level	Italy, 1998–2010	Central government transfers; local tax revenue
Larcinese, Rizzo & Testa (2006)	Partisan alignment	Whether the governor belongs to the same party as the president; whether a majority of the state's House members belong to the same party as the president	USA, 1982 - 2000	Federal outlays
Bonilla-Mejía & Higuera-Mendieta (2017)	Partisan alignment	Whether the mayor's party is within the presidential coalition (current or incoming)	Colombia, 1994-2015	Central discretionary transfers for road investments
Kleider, Röth & Garritzmann (2020)	Partisan and ideological alignment	Partisan - whether the largest government party is the same at the national and the regional level Ideological - proximity between the incumbent regional and national government on the 'state-market' ideological dimension	14 OECD countries, 1990-2010	Subnational expenditures on education
Solé-Ollé & Sorribas-Navarro (2008)	Partisan alignment	Four categories based on the relationship between municipal and upper-tier governments (regional, national, upper-local): single-party alignment, leader (party)	Spain, 1993-2003	Regional spending on education

		alignment, municipal partner alignment, and upper-layer partner alignment		
Brollo & Nannicini (2012)	Partisan alignment	Whether the mayor's party is within the federal government coalition; whether the mayor belongs to the same party as the president	Brazil, 1999- 2010	Federal discretionary transfers for infrastructure projects
Migueis (2013)	Partisan alignment	Whether the municipality is ruled by the party that is in power at the national level	Portugal, 1992-2005	Targetable transfers
Muraközy & Telegdy (2016)	Partisan alignment	Whether the mayor's party is within the central government	Hungary, 2004-2012	The value of won EU structural funds
Schneider & Kunze (2025)	Partisan alignment	Whether the governor belongs to the same party as the president	USA, 1965- 2018	Public relief funds (disaster declarations)
Berry, Burden & Howell (2010)	Partisan alignment	Whether Members of Congress belong to the same party as the president	USA, 1984 - 2007	Federal outlays
Curto-Grau & Zudenkova (2018)	Partisan loyalty	Party unity score	USA, 1986 - 2010	Federal discretionary spending
Brunnschweiler & Obeng (2022)	Partisan alignment	Whether the district's local administrator and member of Parliament belong to the same party as the central government	Ghana, 1994-2018	Government fiscal outcomes



3.2.2 Favoritism due to electoral competition

Besides the political alignment of local governments, the scholarship is also interested in the degree of electoral competition within target localities. Here, the main emphasis shifts from elite composition to voter preferences and how the changes in support for the incumbent influences her decision to target certain constituents. There is long-standing debate in the literature about whether investing in “core” or “swing” voters is more beneficial (Cox & McCubbins 1986; Lindbeck & Weibull 1993; Dixit & Londregan 1998; Diaz-Cayeros et al. 2016). In line with this, grants distribution prioritizes one of those groups (Table 3.2).

On the one hand, national officeholders may seek to reward their “core supporters”, i.e. loyal constituencies yielding large electoral margins, in order to strengthen own support base and improve chances of reelection (be it their own or of aligned local elites). Core voters are more likely to receive discretionary funds because it is a safer and more predictable way to secure electoral success, particularly in systems where voter loyalty matters more than uncertain swings (Kauder et al. 2016). Germany could be one such example, since regional authorities there tend to channel EU structural funds to districts that are strongholds of the Land prime minister’s party, rather than marginal ones (Dellmuth & Stoffel 2012). Provinces in Argentina exhibit parallel dynamics, with federal funding being channeled primarily to strongholds of the dominant coalition (Calvo & Victoria Murillo 2004).

In the US – a prominent case in this literature -- federal outlays are directed to the states that observed strong support for the current president during the last elections, contrary to swing states that do not get these perks (Larcinese et al. 2006). This type of co-partisan favoritism is further corroborated in a study relying on survey-based as opposed to election-based measures of public support, which should reduce endogeneity concerns (Larcinese et al. 2013). Dynes & Huber (2015) similarly show that the prevalence of the president’s party supporters significantly increases future federal spending to those districts.

On the other hand, contested elections – particularly close races – create distinct political incentives for governments to strategically allocate fiscal resources to those localities. In such marginal (aka “swing”) environments, where small shifts in voter behavior can easily change election outcomes, politicians may be incentivized to use transfers in order to maximize their electoral chances. As Brollo and Nannicini (2012) show in Brazil, before next local elections, federal transfers increase specifically to municipalities where aligned mayors previously won by a tight margin. It arguably reflects the efforts to “tie the hands” of rival local elites by limiting their resources and bolster allies’ prospects in the upcoming elections.

In Ghana, swing districts with a low margin of victory in the last parliamentary elections benefit the most in terms of transfers and spending, while districts having large margins of support for the ruling party do not receive higher fiscal allocations (Brunnschweiler & Obeng 2022). Moreover, the evidence of strong electoral cycle in spending and its weakening for aligned districts during election years likely suggests that resources are re-directed towards uncertain swing areas (Brunnschweiler & Obeng 2022). In the same vein, Swedish municipalities featuring more swing voters are significantly more likely to get a grant from the central government (Dahlberg & Johansson 2002), whereas EU funds in Poland target localities with a smaller vote-share difference between the two main parties (Banaszewska & Bischoff 2017).

Further exploring the impact of alignment in Spanish municipalities, Curto-Grau et al. (2018) reveal that capital transfers are substantially larger for constituencies where the regional incumbent's bloc won local seats by a small margin. In turn, Bonilla-Mejía and Higuera-Mendieta (2017) evidence that the increasing effect of alignment on transfers holds specifically in close mayoral races, where an aligned candidate barely wins or loses, and declines as the margin grows. Finally, the US example also highlights that districts in states that were highly contested in the last presidential elections as well as districts held by electorally vulnerable members of the House receive more federal allocations later on (Dyner & Huber 2015).

Yet, the core-swing divide is not necessarily as deep. Somewhat bridging the gap between the two targets, Kriner and Reeves (2015) observe that US presidents actually favor counties of both types, with the main difference in timing. Steering grants towards swing state is particularly acute during the years of presidential re-election, while benefits to core supporters generally do not follow electoral cycles. In other words, investing in swing voters appears more worthwhile right before the moments of potential change, in contrast to partisan states that are rewarded more consistently. Equally, Magaloni et al. (2007) find that Mexican municipalities with greater "added" support for the dominant party as well as persistently waning vote share experience highest inflow of transfers.

As a possible alternative mechanism, however, the incumbent's winning margin within localities might have no additional effect on the amount of central grants, beyond that of political alignment (Migues 2013). Besides, it can be competition in the upper-level government (and not local) which matters more, so that favoritism towards aligned municipalities actually disappears as regional elections get more competitive (Curto-Grau et al. 2018).

Table 3.2 – Political competition behind territorial funds allocation

Paper	Main predictor	Operationalization	Country, years	Outcome variable
Dellmuth & Stoffel (2012)	Vote share	Vote share of the Land prime minister's party	Germany, 2000-2006	EU structural funds expenditure
Magaloni, Díaz-Cayeros, Estévez (2007)	Vote share	Predicted vote share, given the party's national vote share; average trend in the vote share (18-years based measures)	Mexico, 1989-1994	Poverty relief funds
Calvo & Victoria Murillo (2004)	Vote share	Vote share of the two major parties	Argentina, 1987-2000	Federal financing and revenue sharing ratios
Kauder, Potrafke & Reischmann (2016)	Vote share	Vote share of the incumbent party	Germany, 2008-2011	Discretionary project grants



Dahlberg & Johansson, (2002)	Share of swing voters	Survey-based distribution of voter preferences	Sweden, 1994-1998	Ecological-sustainability grants
Banaszewska & Bischoff (2017)	Winning margin	Vote share difference between the two leading parties	Poland, 2007-2011	EU funds
Bonilla-Mejía & Higuera-Mendieta (2017)	Winning margin	Vote share difference between the top two mayoral candidates (where at least one is aligned)	Colombia, 1994-2015	National discretionary transfers for road investments
Curto-Grau, Solé-Ollé, & Sorribas-Navarro (2018)	Winning margin	Votes needed for the incumbent's bloc to win/lose the majority of seats in the local council	Spain, 1995-2007	Regional transfers
Brollo & Nannicini (2012)	Winning margin	Vote share difference between the top two/three mayoral candidates (where at least one is aligned)	Brazil, 1999-2010	Discretionary federal transfers for infrastructure projects
Brunnschweiler & Obeng (2022)	Winning margin	Vote share difference between the parliamentary candidate of the party winning the national elections and candidate of the party losing the national elections	Ghana, 1994-2018	Local government fiscal outcomes
Kriner & Reeves, 2015	Vote share	Share of the two-party vote for the president's party in the past three elections ("core" state if >55% of the vote; "swing" state if 45-55%; "hostile" state if <45%)	USA, 1984 - 2008	Federal grants
Larcinese, Rizzo & Testa (2006)	Vote share	Vote share of the president in the last election	USA, 1982 - 2000	Federal outlays
Larcinese, Snyder & Testa (2013)	Vote share	Survey-based share of swing votes; share of Democrat and Republican partisans	USA, 1978-2002	Federal transfers
Dynes & Huber (2015)	Winning margin, vote share	Vote share difference between the winning and losing presidential candidate; whether the House member won by less than 5%	USA, 1984 - 2010	Discretionary federal spending

3.2.3 Favoritism due to personal motivations

In contrast to alignment- or competition-based factors, intergovernmental grants can also involve favoritism toward specific localities due to leaders' personal motivations. While not always a direct exchange for tangible benefits, such as electoral gains (though they may be ancillary), these transfers are principally rooted in personal attachments of officials in control of spending. Cutting across political coalitions, institutional levels, and public-private spheres, this form of allocation bias is particularly pervasive, commonly leading to exclusionary resource distribution and sidelining of certain areas or societal groups. In the literature, individual motivations are typically operationalized as incumbents' birthplace, location of family members, ethnic background, career path, as well as other characteristics of personal background (Table 3.3).

Birthplace (or "hometown") favoritism is widely discussed in the research. As the argument goes, the incumbent's place of origin is significantly advantaged in terms of public spending, infrastructure, or other public goods compared to the rest of the country across a wide range of settings (Hodler & Raschky 2014; Gonschorek, 2021a, 2021b; Carozzi & Repetto 2016; Huang & Du 2022). The same logic applies to funds: indeed, a number of studies indicates that birthplace considerations override (re-)election motives when it comes to spending priorities.

Exploring the distribution patterns of discretionary grants in Indonesia, Gonschorek (2021a) finds that the governors' birth districts gain a considerably bigger share of funds. Notably, it is governors in their second or last term who engage in such targeting, meaning that reelection is not the primary concern and that electoral accountability stemming from local elections might actually limit favoritism. Likewise, the allocation bias among Italian national legislators is observed only for birth towns that are not MPs' electoral districts, where their voters reside (Carozzi & Repetto 2016). Localism of this kind can also smoothly operate through appointed positions rather than elected offices, as demonstrated by the study of home districts of the Budget Commission members in Indonesia (Gonschorek 2021b).

Ethnicity represents a further determinant of financial targeting, since elites may direct resources predominantly toward ethnically aligned territories. Indeed, a large body of literature documents this type of favoritism, showing that co-ethnic areas receive disproportionate benefits across infrastructure (Burgess et al. 2015; André et al. 2018; De Luca et al. 2018; Ejdemoir et al. 2018) and broader development outcomes (Franck & Rainer 2012; Kramon & Posner 2013, 2016).

Specifically, the study on Benin reveals that the number of school constructions in a district almost doubles under a co-ethnic education minister. Interestingly, however, there is no significant increase when ethnicity is shared with a new president – which is because the latter has to share power with other ethnic groups, allowing them (but not herself) to favor co-ethnics (André et al. 2018). Besides, ethnic bias can co-exist with birthplace-related favoritism. An interesting comparison is done by Bommer et al. (2018), who look at the distribution of humanitarian aid following natural disasters across 38 countries. It is revealed that regions inhabited by powerful ethnic groups are indeed more likely to get aid, yet its amount is mostly associated with being a birth region of the country's leader.

Among other motivations, career path often comes to the forefront. It can be closely intertwined with the hometown bias, whether as a locus of an early political career (Gonschorek 2021a) or as a source of future post-retirement opportunities (Carozzi & Repetto 2016). Alongside networks embedded in one's birthplace, professional incentives may similarly arise from the surrounding system of patronage. On the example of China, Jiang and Zhang (2015) illustrate how city leaders connected to the provincial secretary benefit from increased fiscal transfers, which in turn suggests how aspiring elites strategically allocate resources to advance their local-level clients. This effect is intensified by factors like proximity of potential promotion (i.e., Party Congress), bigger career prospects of the city leaders, as well as status and seniority of the patron provincial secretary.

Lastly, personal motives behind spending decisions can come together with partisan alignment, too. As Fiva and Halse (2016) show on Norwegian municipalities, transfers to hometowns of regional council members significantly rise when they belong to the party of a governor. More to that, this dynamics is self-reinforcing as citizens predominantly vote for parties whose candidates originate from their own municipalities. Indeed, features including shared birthplace or ethnicity tend to become information cues about a candidate's likely behavior once in office (Shugart et al. 2005).

Personally-driven favoritism is particularly hard to eradicate: unlike political alignment or electoral competition, which are relatively temporary, personal characteristics are part of individual identity and thus extend beyond a single electoral cycle. Nevertheless, the literature provides insights into potential remedies, or at least into the ways to alleviate resulting allocation disparities. These range from greater international oversight -- as seen in the context of foreign aid, when donors ensure that even discriminated ethnicities have access to funds (Bommer et al. 2018) -- to structural changes inside the political system. As a matter of fact, favoritism seems to persist even under power-sharing arrangements so long as there is no proper system of checks and balances (André et al. 2018). That is, reducing discretion, promoting transparency, and consolidating the rule of law can offer a solution to biased allocation of intergovernmental transfers arising from both political incentives or personal background.

Table 3.3 – Personal motivations behind territorial funds allocation

Paper	Main predictor	Operationalisation	Country, years	Outcome variable
Gonschorek (2021a)	Birth districts of governors	Whether the district is a governor's birth district	Indonesia, 2004-2017	Share of subnational discretionary grants
Gonschorek (2021b)	Home districts of members of the national Budget Commission	Whether at least one member of the Budget Commission lives in the district	Indonesia, 2005–2013	Intergovernmental transfers
Carozzi & Repetto (2016)	Birth towns of national legislators	Whether the municipality is a legislator's birthplace	Italy, 1994-2006	National transfers



André, Maarek & Tapo (2018)	District's ethnic alignment	Whether the district shares a politician's ethnic group	Benin, 1960-2004	Number of school constructions
Jiang & Zhang (2015)	Patron-client connection	Whether the provincial secretary was in office during the city leader's first promotion	China, 2001-2009	Fiscal transfers
Bommer, Dreher, & Perez-Alvarez (2018)	Birth region of country leaders, ethnic alignment	Whether a disaster occurred in a leader's birth region; share of disaster victims belonging to the politically powerful ethnic group	38 countries, 1964-2015	Humanitarian aid
Fiva & Halse (2016)	Birth town of council members, partisan alignment	Whether the municipality is a legislator's birth town; whether she belongs to the same party as the governor	Norway, 1976–2011	Regional government funding for local investments

3.3 Case study: Partisan Favoritism in Public Procurement: How Political Alignment Shapes Public Fund Distribution in Eastern Europe

Introduction

In this study, we present quantitative evidence on the relationship between political alignment and local public procurement spending in three countries in and around the Balkan region: Hungary, Romania, and Croatia. Our research focuses on observable and quantifiable patterns of political favoritism and related corruption risks at the local level. Political alignment is defined as a situation in which the political coalition backing a municipality's administrative leader (i.e. the mayor) shares at least one political party with the central government. Local spending is measured using country-specific definitions intended to capture spending that may be influenced, either directly or indirectly, by local governments.

This study builds on the working assumption that political favoritism is observable through disproportionately allocated funds favoring politically aligned local governments. On one hand, this may be observed across all years, independently of the national coalition parties. On the other hand, this phenomenon should be particularly visible after alignment shifts following local elections. To test these assumptions, we introduce three hypotheses:

H1: Politically aligned municipalities have a significantly higher total spending compared to non-aligned municipalities.

H2: Changing out of alignment is associated with a punishment mechanism from the central government and results in less local spending post-election, compared to those municipalities that stayed aligned.

H3: Changing into alignment is associated with a reward mechanism from the central government and results in higher local public spending, compared to those municipalities that stayed non-aligned.

The methodology includes a preliminary assessment of the relationship between alignment and local spending using a two-way fixed-effects specification estimated by OLS to test hypothesis H1. We find that during the analyzed period there is a significant and positive relationship in line with theoretical expectations in Hungary and Romania. To understand the effect of local elections and alignment changes around the elections, we introduce difference in differences Two-Way Fixed Effects (TWFE) models, comparing municipalities that changed into alignment with those that remained non-aligned, and municipalities that changed out of alignment with those that remained aligned, while controlling for municipality and year fixed effects.

We first present pooled results by country, then - after excluding periods that do not comply with the parallel trends assumption - we present and analyze the results by election cycle and by country. Our findings support the hypotheses linked to punishing (H2) and rewarding (H3) municipalities in Hungary and support the hypothesis of punishing (H2) in Romania, to an extent. The results in Romania are overshadowed by the lack of parallel trends assumption compliance in two out of the three periods, providing sufficient evidence only on the first election period analyzed. In Croatia, we do not find evidence of comparable punishment or reward mechanisms. This may reflect either comparatively more universalistic tendering practices or limitations in data availability.

Methodology

The research uses multiple methodological approaches to adequately capture the relationship between political alignment and local public spending. First, we estimate the relationship using a baseline two-way fixed-effects specification estimated by OLS. Second, we employ a Difference-in-Differences (DiD) framework to evaluate the effect of changes in political alignment on local spending, leveraging staggered treatment timing across multiple local election cycles. The primary specification of this research is a Two-Way Fixed Effects (TWFE) model at the municipality-half year level, incorporating municipality and year fixed effects. While a baseline OLS setup was used to establish the relationship between alignment and spending, the TWFE framework is used to estimate the effects of transitions into and out of alignment during local elections, while also accounting for unobserved time-invariant municipal heterogeneity and common time shocks (Callaway & Sant'Anna, 2021; Sun & Abraham, 2021; Wooldridge, 2025).

The TWFE approach identifies the average treatment effect under the parallel trends assumption, which posits that treated and control units would have followed similar outcome trajectories in the absence of treatment. Rüttenauer & Aksoy (2025) argue that ensuring the validity of the parallel trends assumption is more important for credible inference than applying other, increasingly complex estimators designed to address treatment effect heterogeneity. This research builds heavily on these foundations and uses an event study setup to formally assess

the validity of the parallel trends assumption through a falsification test. In this model, we include indicator variables for the periods leading up to the change in political alignment; if the parallel trends assumption holds, these pre-event coefficients should be statistically indistinguishable from zero and show no visible trend. For each TWFE model in this research, we present event-study plots relative to the election to assess whether treated and control municipalities were on similar trends prior to the treatment (Callaway & Sant'Anna, 2021; Miller, 2023; Sun & Abraham, 2021).

To ensure the reliability of these findings across the different national contexts studied, the analysis is conducted both using pooled specifications and separately by election period. The pooled specification uses event time - i.e. a redefined time variable where years are assigned based on the relative distance to treatment - instead of conventional years as the time-specific fixed effects, following the recommendations of Rüttenauer & Aksoy (2025). This reduces the risk that staggered treatment timing biases estimates through time heterogeneity in treatment effects. Furthermore, the disaggregated approach addresses the econometric concern that standard TWFE estimators may be biased in staggered designs when treatment effects vary over time or across groups. By examining periods individually, the period-specific design avoids "forbidden comparisons" where previously treated units are used as controls for later-treated ones, which can induce sign reversals in estimated effects (Callaway & Sant'Anna, 2021; Rüttenauer & Aksoy, 2025; Sun & Abraham, 2021).

Finally, the Hungarian case presents an additional classification challenge that may bias comparisons between aligned and non-aligned municipalities between 2010 and 2022: the distinction between what we term "pseudo-Fidesz" independents and "true independents." Hajdu (2024) shows that in municipalities with fewer than 5,000 inhabitants, the national coalition led by Fidesz may support a formally independent candidate, or a candidate may lean toward the governing party without official party membership. More specifically, Hajdu (2024) finds that, between 2022 and 2024, independent candidates who faced a Fidesz challenger received less national funding than independent candidates who faced no strong Fidesz contender. To account for this phenomenon, we examine the top three candidates in each local election to determine the competitive landscape. We define "true independents" as those who successfully competed against and defeated a candidate whose party affiliation contains "Fidesz" among the second- or third-placed finishers. In contrast, "pseudo-Fidesz" independents are categorized as those who faced no Fidesz-affiliated opposition, which we interpret as a proxy for tacit support or alignment with the central government's interests. In the analysis, municipalities led by "pseudo-Fidesz" independents are grouped with Fidesz-aligned municipalities.

Data and definitions

The research combines two main types of datasets for each country: public procurement data and electoral data. Public procurement data are collected from public sources, combining information from Tenders Electronic Daily (TED) and national e-procurement portals using automated web scraping techniques. These data are processed through data cleaning pipelines that produce harmonized data structures, substantially improving within-country and cross-country comparability, while acknowledging that some country-specific reporting differences remain. The resulting dataset provides a comprehensive contract-level record of municipal

procurement activity, allowing us to compute total spending per half year for all municipalities. Table 3.4 shows the main descriptives of these datasets for the three countries included in the analysis.

Table 3.4 – Public procurement data descriptives

	Hungary	Romania	Croatia
Years available	2007-2022	2007-2023	2008-2023
Number of contracts	41009	4241130	108395
Total contract value	14.06 bn EUR	11.04 bn EUR	14.66 bn EUR

Electoral data are also collected from publicly available sources and focus on the results of municipal-level elections. These data include information on the mayor, the political coalition backing the mayor, and the opposing candidates and their respective coalitions. They are supplemented with information on national governing coalitions, enabling the construction of political alignment variables. Political alignment is defined as a situation in which the political coalition backing a municipality’s mayor shares at least one political party with the political coalition of the central government. Table 3.5 shows the main descriptives of the electoral datasets for the three countries included in the analysis.

Table 3.5 – Electoral data descriptives

	Hungary	Romania	Croatia
Years available	2010-2022	2010-2023	2013-2023
Number of municipalities	3152	3180	552
Local election years	2010, 2014, 2019, 2024	2008, 2012, 2016, 2020	2013, 2017, 2021
National election years	2010, 2014, 2018, 2022	2008, 2012, 2016, 2020	2015, 2016, 2020
Election cycles analyzed	2014-2019, 2019-2024	2012-2016, 2016-2020, 2020-2023	2013-2017, 2017-2021

Tables 3.4 and 3.5 also highlight the main implications of data availability for the empirical design. First, the intersection of the yearly availability (restricted to years with high-quality data) of the two datasets determines the election cycles that can be included in the analysis. Second, differences in the number of municipalities across countries have implications for statistical power and the precision of estimates. Hungary and Romania contain a relatively large number of municipalities, whereas Croatia has an order of magnitude fewer local government units. This raises concerns about whether the Croatian data provide sufficient statistical power to detect the relationship between political alignment and local public spending.

Furthermore, the timing of national and local elections in Croatia and Romania shapes the event window used in the DiD analysis. We define the half-year of the local election as the treatment period and include three half-years prior to and four half-years following the election, to avoid overlaps across election cycles. Although Hungarian local elections are held every five years since



2014 rather than every four, allowing for two more half years to be added to our analysis, we apply the same event window to ensure comparability across countries.

Finally, because the outcome of interest is local public spending that may be influenced by municipal leadership, we construct country-specific definitions of local political control using buyer-level information from the procurement data. Direct political control refers to buyers that are clearly and institutionally linked to municipal leadership, such as mayor's offices or local councils. Indirect political control applies a broader definition, also including sub-national public bodies whose budgets or operations may be indirectly influenced by local political authority. We estimate results using both definitions; however, as findings are similar, we focus on the direct political control definition in the main analysis.

In Hungary, direct political control is defined by restricting the procurement dataset to contracts issued by buyers whose names contain "polgármesteri hivatal" (mayor's office) or "önkormányzat" (municipality/local council), excluding buyers located in Budapest. In Romania, the equivalent filter targets buyers whose names contain keywords associated with municipal institutions, including "comuna" (commune), "primaria" / "primăria" (mayor's office), "consiliul local" (local council), "municipiul" / "municipiului" (municipality), "orașul" / "orasul" / "oras" / "orasului" (town/city), or "orasesc" (municipal/town-level), excluding buyers located in Bucharest. In Croatia, the filter is necessarily broader given the structure of Croatian public administration. It targets buyers whose names contain keywords associated with municipal institutions, including "grad" (city), "opcin" (municipality), "županijsk" (county), "javn" (public), "komunal" (utilities), "vodovod" (waterworks), "vatrogas" (firefighters), "term" (thermal/gas), "lucka uprava" (port authority), or "odvodnja" (drainage services). The definition is further extended to include buyers whose names contain keywords identifying hospitals ("bolni"), clinical hospital centers ("kbc"), elementary schools ("oš"), secondary schools ("sš"), or the names of over 120 Croatian cities and towns. Buyers located in Zagreb are excluded throughout the analysis.

Results

Results for Hungary

The baseline results presented in Table 3.6 examine the relationship between political alignment and local public procurement spending across Hungary, Romania, and Croatia, providing a first test of Hypothesis 1. The unit of observation is the municipality–half year, restricted to periods in which at least one procurement contract with an available contract value is recorded, with local public procurement spending measured as the logarithm of bid prices in EUR ($\ln(\text{price} + 1)$). Across Hungary and Romania, the results indicate a positive and statistically significant association between political alignment and local spending, consistent across specifications including municipality and year fixed effects. These findings remain stable when additional controls are introduced, as shown in the (C) specifications, suggesting that the observed relationship is not driven by differences in population or related structural characteristics. In contrast, the estimates for Croatia are not statistically significant, although the direction of the effect remains positive, as expected. The absence of statistical significance in Croatia may reflect either a weaker association between political alignment and public spending compared to



Hungary and Romania, or limited statistical power due to the substantially smaller number of municipalities and observations available in the data.

Table 3.6 – Baseline OLS estimates with Two-Way Fixed Effects of political alignment and local public procurement spending (cross-country comparison)

	Hungary	Hungary (C)	Romania	Romania (C)	Croatia	Croatia (C)
Aligned	0.137*	0.137*	0.048***	0.049***	0.024	0.023
	(0.053)	(0.053)	(0.012)	(0.012)	(0.044)	(0.044)
Log population / proxy		0.000		0.480***		–0.058
		(0.000)		(0.087)		(0.084)
Std. Errors	by: Municipality	by: Municipality	by: Municipality	by: Municipality	by: Municipality	by: Municipality
FE: Municipality	Yes	Yes	Yes	Yes	Yes	Yes
FE: Year	Yes	Yes	Yes	Yes	Yes	Yes
Num.Obs.	13528	13528	57701	57701	5903	5903
R2	0.430	0.430	0.460	0.469	0.611	0.611
RMSE	1.23	1.23	1.03	1.02	1.11	1.11

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Standard errors clustered by municipality in parentheses.

(C) includes log population controls: log_pop + municipality_status for HU; ln_est_pop for HR; ln_total_votes (log total votes, used as population proxy) for RO.

Reference category: Non-aligned municipalities (HU: non-aligned + non-Fidesz-associated independents).

Outcome: ln(bid_digiwhist_price + 1) for all countries.

HU: Extended alignment definition (Fidesz/KDNP + Fidesz-associated independents vs rest). RO/HR: Simple alignment (party coalition match).

All models: OLS with municipality + year fixed effects, combined (full available) period.

Taken together, these results suggest that politically aligned municipalities tend to exhibit higher levels of discretionary public procurement spending. While these findings remain associational, they provide consistent support for Hypothesis 1 and motivate a more detailed examination of the dynamics of alignment changes. Next, we build on this baseline evidence by turning to country-specific Difference-in-Differences analyses to assess whether shifts into and out of political alignment are associated with systematic changes in local spending, corresponding to Hypotheses 2 and 3.

The above OLS estimates show a time-averaged link between a municipality's political alignment and its procurement spending. The findings are consistent with the theoretical framework presented earlier, which suggests that favoritism based on political alignment influences procurement allocation. However, this association alone isn't enough to prove that alignment causes preferential procurement. It's possible that aligned and non-aligned municipalities differ in other significant ways, such as administrative capacity, urban-rural status, or historical procurement activity, which could independently affect their spending levels.

To address these limitations and move towards a more causal interpretation, we employ a Difference-in-Differences (DiD) approach. This framework allows us to exploit changes in alignment status induced by local elections, comparing variations within municipalities over

time. Specifically, we estimate whether municipalities that change their political alignment subsequently experience procurement trajectories that diverge from those municipalities whose alignment status remains stable throughout the same period. Under this methodological design, each municipality serves as its own counterfactual, assuming that, in the absence of an alignment change, both treated and control municipalities would have followed similar procurement trends. We test this crucial assumption through the by-cycle event study plots presented in (Annex B).

Among the three countries examined, Hungary provides the most statistically precise evidence of an alignment-based procurement spending advantage (Table 3.7). This outcome is consistent with Hungary's institutional environment. During the analysis period, the Fidesz-led government, which remained continuously in power, significantly consolidated administrative and fiscal authority. This consolidation made the channeling of procurement resources towards aligned municipalities not only possible but also expected. The mechanism was, at times, explicitly articulated. For instance, prior the 2018 mayoral election in Hódmezővásárhely, government minister János Lázár publicly declared that residents of a non-aligned municipality "cannot expect any support or cooperation from the government." (Source: Rádió 7, Közös Nevező: 2023. 06. 15.). This statement directly illustrates the reward-punishment logic under investigation.

Table 3.7 – Political alignment and public procurement spending in Hungary: Difference-in-Differences results

	Out of Alignment	Into Alignment
DiD Estimate (Treatment x Post)	-0.475**	0.723**
	(0.145)	(0.219)
Num.Obs.	58092	10604
R2	0.371	0.410
R2 Within	0.000	0.003
RMSE	3.98	4.51
FE: Municipality	X	X
FE: Year	X	X

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Standard errors clustered by county in parentheses.

Out of Alignment: Treatment = changed out, Control = stayed aligned at election halfyear

Into Alignment: Treatment = changed into, Control = stayed non-aligned at election halfyear

DV: $\log(\text{sum_bid_digiwhist} + 1)$

The pooled TWFE estimates support this expectation. Municipalities that switched out of alignment with the governing Fidesz party experienced an approximate 37.8% decline in local procurement spending ($\beta = -0.475$, $SE = 0.145$, $p < 0.01$) in the post-election period relative to aligned municipalities. Conversely, municipalities that transitioned into alignment saw their procurement spending increase by roughly 106.1% ($\beta = +0.723$, $SE = 0.219$, $p < 0.01$) relative to non-aligned municipalities, compared to the pre-election period.

Both coefficients are statistically significant at the 1% level and consistent with the hypothesized reward-punishment mechanisms, providing robust empirical support for both H2 (punishment) and H3 (reward) in the Hungarian context. Note the observed asymmetry in effect magnitudes, where the reward effect surpasses the punishment effect in absolute terms. The precision of these estimates is particularly noteworthy given a structural characteristic of the Hungarian data: the substantial political dominance of Fidesz throughout the observation period structurally limits the number of municipalities switching out of alignment. The fact that statistically significant and theoretically consistent effects emerge despite this limited variation strengthens the interpretation of our findings.

The validity of the TWFE estimates rests on the parallel trends assumption. The by-cycle event study plots for Hungary, shown in Annex B, provide support to this assumption across both available electoral cycles (2013-2016 and 2018-2021). Pre-treatment coefficients are not significantly different from zero throughout the pre-period in both the into alignment and out of alignment plots. While confidence intervals are wide due to the limited number of switching municipalities, the absence of a systematic pre-trend is consistent across cycles and directions. The parallel trends condition is considered sufficiently satisfied to support a causal interpretation of the pooled estimates.

Table 3.8 – Political alignment and public procurement spending in Hungary: Difference-in-Differences results by electoral cycle

	2013-2016 (Out)	2013-2016 (Into)	2018-2021 (Out)	2018-2021 (Into)
DiD Estimate (Treatment x Post)	-0.115	0.558*	-0.428	0.370
	(0.264)	(0.237)	(0.404)	(0.363)
Num.Obs.	24168	5544	33924	5060
R ²	0.458	0.463	0.392	0.423
R ² Within	0.000	0.001	0.000	0.000
RMSE	3.64	4.10	3.96	4.67
FE: Municipality	X	X	X	X
FE: Year	X	X	X	X

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Standard errors clustered by county in parentheses.

(Out) = Changed out of alignment vs. stayed aligned at election halfyear

(Into) = Changed into alignment vs. stayed non-aligned at election halfyear

DV: $\log(\text{sum_bid_digiwhist} + 1)$

Table 3.8 shows the by-cycle estimates, breaking down the pooled Hungarian results across the two electoral cycles. This decomposition reveals differences that the aggregated estimator might hide. Specifically, the effect for municipalities transitioning into alignment during the 2013-2016 cycle demonstrates statistical significance ($\beta = + 0.558$, $SE = 0.237$, $p < 0.05$). This result indicates that municipalities aligning with the Fidesz government in that period saw an approximate 74.7%



increase in procurement spending post-election relative to the control group of municipalities that remained non-aligned, compared to pre-election.

The other three cycle-direction combinations, namely, municipalities moving out of alignment in 2013-2016 ($\beta = -0.115$), and both directions in 2018-2021 ($\beta = -0.428$ and $\beta = +0.370$), do not reach the usual significance levels individually. However, it is worth noting that all coefficients associated with municipalities moving out of alignment are negative, and all coefficients for those moving into alignment are positive. This consistent directionality across all models aligns with the expected reward-punishment mechanism.

The difference between the pooled and by-cycle results likely reflects statistical power limitations in the Hungarian data rather than the absence of actual effect. In both electoral cycles, the number of municipalities switching into alignment is relatively small (5544 municipality-half year level observations in 2013-2016 and 5060 in 2018-2021), which limits the precision of by-cycle estimates. By pooling these cycles into a larger sample of 10,604 observations, we gain enough statistical power to identify the effect. The consistent direction of the estimates, combined with the statistically significant pooled results, supports the interpretation that the political favoritism is a stable feature of Hungarian procurement system over the entire period, rather than a result driven by a single electoral cycle. The 2013-2016 into-alignment estimate, being the most precisely measured among the by-cycle results, is also the easiest to interpret, particularly in light of Fidesz's early consolidation of administrative and fiscal control after returning in power in 2010.

Results for Romania

Following the analysis of the Hungarian case, we now turn to the results for Romania. While the Hungarian results show a clear pattern of alignment-based favoritism, the Romanian findings are more nuanced. We treat these as tentative results, as the analysis is complicated by data quality issues in the early years and violations of the parallel trends assumption in several of our estimates.

Table 3.9 – Political alignment and public procurement spending in Romania: Difference-in-Differences results

	Out of Alignment	Into Alignment
DiD Estimate (Treatment x Post)	0.061	0.075
	(0.088)	(0.089)
Num.Obs.	57856	47088
R2	0.626	0.640
R2 Within	0.000	0.000
RMSE	3.20	2.99
FE: Municipality	X	X
FE: Year	X	X

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Standard errors clustered by jurisdiction in parentheses.

Out of Alignment: Treatment = changed out, Control = stayed aligned at election halfyear

Into Alignment: Treatment = changed into, Control = stayed non-aligned at election halfyear

DV: $\log(\text{sum_bid_digiwhist} + 1)$

The pooled TWFE estimates for Romania do not show a statistically significant average treatment effect for either direction of alignment change. As shown in the pooled results Table 3.9, municipalities that switched out of alignment with the governing party show an estimate of 0.061 (SE = 0.088) in the post-election period relative to municipalities that stayed aligned, while municipalities that transitioned into alignment show an estimate of 0.075 (SE = 0.089) relative to non-aligned control municipalities, compared to the pre-election period. In both specifications, the coefficients fail to reach conventional significance thresholds, meaning we do not find evidence of a systemic reward or punishment mechanism in the pooled Romanian data, leaving H2 and H3 unsupported at the aggregate level.

Several factors may explain the absence of a clear pooled effect. Unlike Hungary's relatively stable political environment, Romania is characterized by frequent changes in government coalitions and a more fragmented party system. In addition, local political dynamics in Romania are marked by a relatively high degree of party switching among mayors, both following electoral losses and prior to local elections, as a strategy to maximize their re-election chances. This fluidity may obscure the mechanisms linking political alignment and public spending and weaken the consistency of any observable reward–punishment pattern. Furthermore, pooled estimators may mask substantial heterogeneity across electoral cycles, especially given the variation in data quality over the sample period.

Table 3.10 – Political alignment and public procurement spending in Romania: Difference-in-Differences results by electoral cycle

	2011-2014 (Out)	2011-2014 (Into)	2015-2018 (Out)	2015-2018 (Into)	2019-2022 (Out)	2019-2022 (Into)
DiD Estimate (Treatment x Post)	-0.391+	-0.002	0.726**	0.797**	0.036	0.226***
	(0.230)	(0.261)	(0.224)	(0.239)	(0.052)	(0.039)
Num.Obs.	20128	13664	17512	13272	20216	20152
R2	0.493	0.499	0.490	0.502	0.716	0.716
R2 Within	0.001	0.000	0.003	0.003	0.000	0.004
RMSE	3.67	3.65	3.20	3.11	0.93	0.92
FE: Municipality	X	X	X	X	X	X
FE: Year	X	X	X	X	X	X

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Standard errors clustered by jurisdiction in parentheses.

(Out) = Changed out of alignment vs. stayed aligned at election halfyear

(Into) = Changed into alignment vs. stayed non-aligned at election halfyear

DV: log(sum_bid_digiwhist + 1)

The by-cycle estimates (Table 3.10) present a more complex picture and highlight several methodological challenges that make a causal interpretation difficult for most periods. In the 2011-2014 cycle, we find a marginally significant negative effect for municipalities moving out of alignment ($\beta = -0.391$, $p < 0.1$), suggesting an approximate 32.4% decline in procurement spending relative to municipalities that stayed aligned, compared to the pre-election period, consistent with a punishment mechanism (H2). However, these early results should be interpreted with caution. Data coverage is particularly poor in this period, with 70-78% of bid price observations missing in the pre-treatment half-years, which likely introduces significant noise.

Data quality improves substantially in the later cycles, with missing rates falling to 1-5% from 2018 onward. Although we observe statistically significant coefficients in the 2015-2018 and 2019-2022 cycles, such as the into alignment rewards of $\beta = +0.797$ ($p < 0.01$) and $\beta = +0.226$ ($p < 0.001$) respectively, these results cannot be given a causal interpretation due to violations of the parallel trends assumption in both periods. The event study plots (Annex B), suggest that treated and control municipalities were already following different trajectories prior to the alignment change. Despite this limitation, the by-cycle DiD estimates point in the theoretically expected direction, suggesting a potential reward mechanism present in Romania, a hypothesis that matched treatment and control groups could allow us to test most rigorously.

Results for Croatia

The final case focuses on Croatia, where the analysis reveals no statistically significant evidence of a clear relationship between political alignment and local public procurement spending. Similar to the pooled results in Romania, the Croatian estimates indicate that alignment shifts after the elections do not produce noticeable changes in procurement trajectories.

Table 3.11 – Political alignment and public procurement spending in Croatia: Difference-in-Differences results

HR_data_d_extended - DiD Analysis | Dependent Variable: log(Bid Price) Pooled Results

	Out of Alignment	Into Alignment
DiD Estimate (Treatment x Post)	-0.069	-0.108
	(0.235)	(0.275)
Num.Obs.	5578	5741
R2	0.520	0.524
R2 Within	0.000	0.000
RMSE	4.55	4.54
Std.Errors	by: county_normalized	by: county_normalized
FE: buyer_city_cleaned	X	X
FE: year	X	X

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001
Standard errors clustered by county in parentheses.
Out of Alignment: Treatment = changed out, Control = stayed aligned at election halfyear
Into Alignment: Treatment = changed into, Control = stayed non-aligned at election halfyear
DV: log(sum_bid_digiwhist + 1)

The pooled TWFE estimates for Croatia do not show a statistically significant average treatment effect for either direction of alignment change (Table 3.11). As shown in the pooled results table, the estimated effect for municipalities moving into alignment is small and not statistically different from zero ($\beta = -0.108$, $SE = 0.275$). Similarly, municipalities losing alignment show no significant decline in spending ($\beta = -0.069$, $SE = 0.235$). These null results stand in contrast to the robust reward-punishment patterns observed in Hungary, suggesting that the mechanism of political favoritism through procurement is either absent or undetectable in Croatia during the studied period/ leading us to reject H2 and H3 for this case.

Two primary factors likely contribute to the absence of a clear pooled effect. First, Croatia's institutional setting is characterized by higher political fragmentation. The national government, often operating through fragile multi-party coalitions, may lack the centralized administrative capacity required to systematically reward or punish local administrations via procurement allocations. Second, the analysis is constrained by significant data limitations. The Croatian sample is substantially smaller than those of Hungary or Romania, and missing bid price data is widespread, affecting between 31% and 63% of observations

Table 3.12 – Political alignment and public procurement spending in Croatia: Difference-in-Differences results by electoral cycle

	2015-2019 (Out)	2015-2019 (Into)	2019-2023 (Out)	2019-2023 (Into)
DiD Estimate (Treatment x Post)	–0.362 (0.532)	0.196 (0.330)	–0.050 (0.375)	–0.586 (0.520)
Num.Obs.	2688	3094	2890	2647
R2	0.585	0.566	0.539	0.571
R2 Within	0.000	0.000	0.000	0.001
RMSE	4.25	4.29	4.38	4.26
FE: Municipality	X	X	X	X
FE: Year	X	X	X	X

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Standard errors clustered by county in parentheses.

(Out) = Changed out of alignment vs. stayed aligned at election halfyear

(Into) = Changed into alignment vs. stayed non-aligned at election halfyear

DV: log(sum_bid_digiwhist + 1)

The by-cycle estimates further confirm the absence of a consistent reward-punishment mechanism and highlight the methodological challenges in the Croatian data (Table 3.12). In the 2015-2019 cycle, the point estimates for both into alignment ($\beta = 0.196$, SE = 0.330) and out of alignment ($\beta = -0.362$, SE = 0.532) are statistically insignificant. In the subsequent 2019-2023 cycle, the estimates remain insignificant and, in the case of into-alignment transitions, even reverse direction ($\beta = -0.586$, SE = 0.520). This lack of consistent direction across cycles distinguishes Croatia from Romania, where estimates were at least theoretically consistent even when they lacked statistical power. The absence of a systematic trend suggests that procurement spending in these municipalities is driven by local factors rather than a coordinated national strategy of political favoritism.

Event study plots (Annex B) provide additional support for these findings. For both into alignment and out of alignment transitions, the pre-treatment coefficients are not statistically significant, indicating that treated and control municipalities followed similar trajectories prior to the elections. Because the parallel trend assumption holds, the null results reflect a real absence of a measurable effect rather than a problem with the research design, further confirming the lack of support for our hypotheses in Croatia.

Conclusions

This study provides quantitative evidence on the relationship between political alignment and local public procurement spending across Hungary, Romania, and Croatia. Using harmonized contract-level procurement data, electoral data, baseline two-way fixed-effects OLS specifications, and Difference-in-Differences models, the analysis identifies patterns of resource allocation that are consistent with political favoritism and discretionary fund distribution. These findings contribute to a broader understanding of how central-local political dynamics may

distort public spending priorities and create observable corruption risks within South-Eastern European procurement systems.

In Hungary, the results provide robust and statistically significant evidence of reward and punishment mechanisms. The observed 106% increase in procurement spending for municipalities switching into alignment, contrasted with a 37% decline for those losing alignment, suggests that procurement may function as an important instrument of political consolidation. This pattern is consistent with political machine logic, where loyal local administrations are rewarded with increased fiscal capacity to secure electoral support and ensure the implementation of central policies. The results remain meaningful after accounting for “pseudo-independent” candidates, pointing to a broader erosion of competitive neutrality, where administrative and fiscal authority appears to be used to favor politically aligned jurisdictions.

In Romania, the findings are more cautious but still theoretically relevant. The results point in a direction consistent with alignment-based punishment, but violations of the parallel trends assumption in two out of three electoral cycles prevent causal interpretation beyond the earliest period. This limitation is informative in itself: it suggests that changes in alignment may be endogenous to pre-existing local spending trajectories. The prevalence of party switching among Romanian local politicians may reinforce this interpretation, as strategic realignment can blur the distinction between treatment and control groups.

In contrast, the findings for Croatia do not support the hypotheses of alignment-based reward or punishment. The lack of statistical significance may be attributed to several factors, including the country’s higher degree of party fragmentation and smaller sample size. In a multi-party coalition environment, the centralized administrative capacity needed to systematically reward or punish local governments is likely weaker. The absence of statistically significant results offers an informative contrast to Hungary and Romania, suggesting that institutional structures and the nature of political competition may play an important role in determining whether procurement systems become politicized.

This analysis already offers a systematic overview of quantifiable political favoritism and related corruption risks at the local level in the selected South-Eastern European context. There are multiple viable paths to refine and extend the findings of this research, however. First, future research could introduce additional methods to address violations of the parallel trends assumption in non-compliant periods and test more complex methodological approaches. For example, complementary strategies could include matching approaches or synthetic control methods that align treatment and control municipalities on pre-treatment spending trends before applying the TWFE. These approaches could reduce systematic differences between groups and help restore the conditions needed for causal inference. Second, improving data quality and excluding low-quality periods or municipalities may further strengthen the findings. Third, the scope of the analysis could be extended to further countries in the region, both introducing new country-specific findings and allowing pooled analyses that may reveal broader regional patterns. Finally, the scope of the analysis may be extended to test the effect of local political competitiveness on alignment-based public spending allocation on the local level.



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ANNEX A: Supporting Chapter 2 case study

Table A1 – Overview of selected corruption risk indicators in public procurement

Source	Indicator(s) used	Country	Year	Sector
Di Tella and Schargrodsky (2003)	Difference in prices of standardised products such as ethyl alcohol	Argentina	1996–2007	Health care procurement
Olken (2007)	Differences between the officially reported and independently audited prices and quantities of road construction	Indonesia	2003–2004	Infrastructure (roads)
Hyytinen, Lundberg, and Toivanen (2008)	Number and type of invited firms; use of restricted procedure	Sweden	1990–2008	Cleaning services
Bandiera, Prat, and Valletti (2009)	Price differentials for standard goods purchased locally or through a national procurement agency	Italy	2000–2005	Standardised goods (e.g. paper)
Klašnja (2015)	Single bidder auctions; non-open procedure types	Romania	2008–2012	General procurement
Chong, Klien, and Saussier (2015)	Negotiated procedure type	European Union	2008–2012	General procurement
Auriol, Flochel, and Straub (2016)	Exceptional procedure type	Paraguay	2004–2007	General procurement
Coviello and Gagliarducci (2017)	Number of bidders; same firm awarded contracts recurrently; level of competition	Italy	2000–2005	General procurement
Ferwerda, Deleanu, and Unger (2017)	Contract level elementary risk indicators such as short advertisement period	EU	2006–2010	General procurement
Fazekas et al, 2016	Composite risk score including elementary indices such as single bidding and supplier's spending share	Hungary	2009–12	General procurement
Luciánodra, Milani, Millemaci (2022)	Composite score with focus on contract complexity	Italy	2007–2017	Public works
Decarolis, Giorgiantonio (2022)	Composite risk score including absence of tender call, page and word number of calls, open tender days.	Italy	2009–2015	Public works (sector of goods, services, and works)

Source: adapted and extended from Fazekas et al. (2018)



Figure A1 – Number of contracts awarded by year, Hungary, 2012-2020

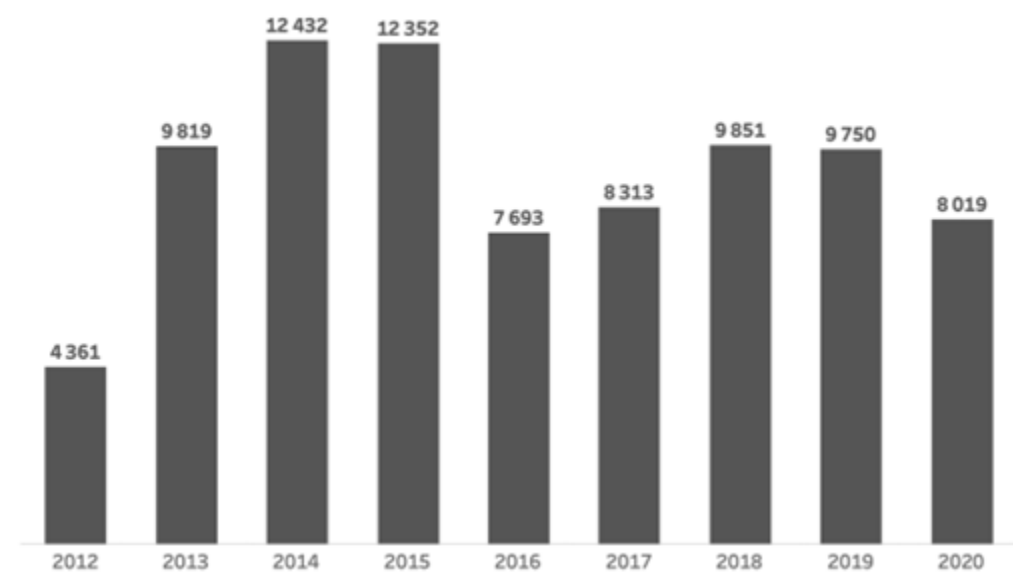


Table A2 – Variables used in predictive models, variable definitions, and distributions,
Ncontracts = 82,590

Variable	Definition	Distribution	
Outcome variable			
Single bidding	0 = more than 1 bid received 1 = 1 bid received	0 1	63696 18894
Supplier's spending share	0 = low 1 = high	0 1 missing	57039 7411 18140
Combined variable	0 = no competition restriction 1 = competition restricted	0 1	57170 25420
Structural constraints			
Bid price	Contract values coded into deciles are used instead of actual contract values because the contract value distribution is highly skewed with a few large contracts distorting results.	Mean (bid price) Missing	50403756 0
Tender CPV divisions	CPV = Common procurement vocabulary CPV stands for the market division of the tender Top 20 CPV + other	top 5 largest categories 45 33 Other 79 71 Missing	37124 6257 6193 5428 4109 0
Tender year	Year of contract (2011-2020)	2012 2013 2014 2015 2016 2017 2018 2019 2020	4361 9819 12432 12352 7693 8313 9851 9750 8019
Buyer NUTS	NUTS = Nomenclature of territorial units for statistics. Hungary's geographical regions: West, Central and Eastern Hungary plus the whole country for national markets	HU11 HU12 HU21 HU22 HU23 HU31 HU32	27761 5445 6709 6326 7080 9005 10571



		HU33	9693
		Missing	
Buyer type	Main type of the buying organisation as defined by EU's Procurement Directive	National agency National authority Other Public body Regional agency Regional authority Utilities	9616 1312 7053 15813 37704 640 5804
Buyer main activities	Main sector of the buying organisation	General public services Other Health Education Recreation culture and religion Economic and financial affairs Urban transport Environment Public order and safety Social protection Water Railway Electricity Housing and community amenities Defence Gas and heat production Airport Postal	32983 9857 9142 6994 5715 2709 1935 1927 1886 1302 1082 501 462 440 345 335 148 47
Red Flags based on structured data			



Submission period length	number of days between publication of call for tenders and submission deadline 0 = long submission period (no risk) (>= 38 days) 1 = short submission period (risky) (2 to 37 days)	0 1 Missing	1397 11939 69254
No call for tender publication	0 = call for tenders published on publication portal (no risk) 1 = call for tenders not published on publication portal (no notice URL) (risky)	0 1	13374 69216
Decision period length	number of days between submission deadline and announcing the contract award 0 = long decision period (no risk) (>= 41 days) 0.5 = moderately short decision period (medium risk) (21 to 40 days) 1 = very short decision period or missing decision period (high corruption risk) (<= 20 days)	0 0.5 1	7133 3462 71995
Procedure Type	0 = open (no risk) 1 = non-open (high risk)	0 1	13234 69356
Unstructured text fields used		Missing ratios	
Tendering requirements (Eligibility criteria)	Eligibility criteria define which companies are allowed to bid, what prior experience is required from eligible bidders. Personal requirements (e.g. compliance with laws) Technical requirements (e.g. different qualifications, professional knowledge) Economic requirements (e.g. financial resources)	Personal requirements Technical requirements Economic requirements	85% 85% 85%
Product description	Tender and/or lot title : subject of the contract Tender description : short description of the subject of the contract	Title Description	12% 12%



Award criteria	After the bids are received, the procuring body evaluates them and selects the winner according to the award criteria. The result is published in a contract award announcement. If no valid bids were received or the prices were too high for the institution, an announcement is published about the reason of the failure	Price only ratio Price and quality ratio Missing award criteria information	41% 43% 16%
Text-based meta variables			
Uniqueness score	This variable provides a measure of how unique or distinctive the language is in each document within the same CPV category. It is calculated for each text type and aggregated on tender level. Higher scores indicate that the document contains words or phrases that are relatively unique within the dataset. Theoretical minimum: 0 Theoretical maximum: 1	mean	0.00012
Normalized length	The variable represents the normalized length of each document's combined text content, considering the total length of text in various columns and normalizing it with respect to the mean length of documents within the same CPV category.	min mean max	0 1 35.9
Law reference	This variable measures the presence of legal references in a document within its respective CPV category. It is calculated for each text. A higher value indicates that the text contains more references to laws, regulations, or other legal provisions. Theoretical minimum: 0 Theoretical maximum: 1	Mean of... Title Description Personal requirements Technical requirements Economic requirements Award criteria	0.00025 0.00025 0.00025 0.00025 0.00025 0.00025

Number ratio	Number ratio shows the ratio of the count of numbers in each text to the sum of counts of numbers for the corresponding CPV sector. Theoretical minimum: 0 Theoretical maximum: 1	Mean of the variable: Title Description Personal requirements Technical requirements Economic requirements Award criteria	0.000254 0.000254 0.000254 0.000254 0.000254 0.000254
Lexical diversity	Lexical diversity is defined here as the ratio of the number of unique words to the total number of words in a text (considering CPV category). A high value means higher lexical diversity score, which indicates that the text has a greater variety of unique words relative to the total number of words, suggesting a richer and more varied use of language. Theoretical minimum: 0 Theoretical maximum: 1	Mean of the variable: Title Description Personal requirements Technical requirements Economic requirements Award criteria	0.74 0.76 0.93 0.94 0.95 0.96
Validation variables			
Relative price	The relative price variable measures the ratio of the bid price to the estimated lot price in a procurement process. It indicates how a bid compares to the expected cost.	Std min max mean	0.126 0.5 1.5 0.98
Politically connected firms	To produce this variable, we combined multiple lists collected by different institutions to identify the most politically connected firms.	0 1	61605 20985

Table A3 – Baseline models predicting single bidding: binary logistic regressions, Random Forests, XGBoosts using structural constraints and red flags, Hungary, 2012-2020

	precision	recall	f1-score	accuracy
Logistic Regression: structural constraints only	0.72	0.77	0.70	0.77
Random Forest: structural constraints only	0.75	0.77	0.76	0.77
Boosting: structural constraints only	0.75	0.77	0.76	0.77
Logistic Regression: structural constraints and red flags	0.72	0.77	0.70	0.77
Random Forest: structural constraints and red flags	0.77	0.78	0.77	0.78
Boosting: structural constraints constraints and red flags	0.77	0.78	0.77	0.78



Table A4 – Text based models predicting single bidding: binary logistic regressions, Random Forests, and XGBoost using red flags, structural constraints, and texts, Hungary, 2012-2020

	precision	recall	f1-score	accuracy
PRODUCT DESCRIPTION				
Logistic Regression: texts, red flags and structural constraints	0.74	0.78	0.73	0.78
Random Forest: texts, red flags and structural constraints	0.82	0.83	0.81	0.83
Boosting: texts, red flags and structural constraints	0.78	0.78	0.71	0.78
ELIGIBILITY CRITERIA				
Logistic Regression: texts, red flags and structural constraints	0.73	0.78	0.71	0.78
Random Forest: texts, red flags and structural constraints	0.81	0.82	0.80	0.82
Boosting: texts, red flags and structural constraints	0.77	0.79	0.72	0.79
AWARD CRITERIA				
Logistic Regression: texts, red flags and structural constraints	0.73	0.78	0.72	0.78
Random Forest: texts, red flags and structural constraints	0.82	0.83	0.81	0.83
Boosting: texts, red flags and structural constraints	0.76	0.78	0.71	0.78

Table A5 – Text based models predicting single bidding: binary logistic regressions, and Random Forests using red flags, structural constraints, and all three text parts, Hungary, 2012-2020

	precision	recall	f1-score	accuracy
Logistic Regression: texts, red flags and structural constraints	0.74	0.78	0.72	0.78
Random Forest: texts, red flags and structural constraints	0.83	0.84	0.82	0.84
Boosting: texts, red flags and structural constraints	0.78	0.78	0.71	0.78

Table A6 – Random Forest models predicting single bidding using red flags, structural constraints, and texts, for tenders with and without a published call, Hungary, 2012-2020

	Call published		No call published	
	f1-score	accuracy	f1-score	accuracy
NO TEXT: red flags and structural constraints only	0.76	0.77	0.77	0.78
PRODUCT DESCRIPTION: texts, red flags and structural constraints	0.75	0.77	0.82	0.85
AWARD CRITERIA: texts, red flags and structural constraints	0.77	0.79	0.82	0.83

Figure A2 – Distribution of the predicted score

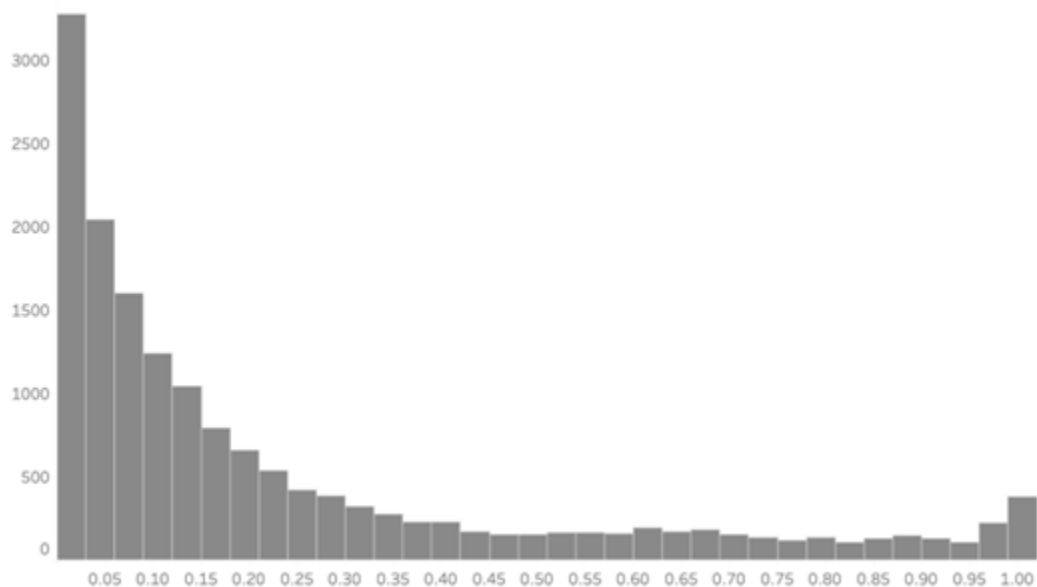


Table A7 – T-test using political connections as independent variable and probability score as dependent variable

T-test:	N	Probability score (RF texts only)
pep = 0	12234	0.240
pep = 1	4228	0.243
p-value		0.025



Table A8 – OLS Regression using probability score as independent variable (with and without red flags and control variables) and relative price as dependent variable

	coef	std. err	t	p
TX score	0.0146	0.004	4.154	0.000
TX score + controls	0.0235	0.004	6.478	0.000
TX score + controls + redflags	0.0269	0.004	7.420	0.000

ANNEX B: Supporting Chapter 3 case study

Figure B1 – Hungary: Event Study coefficients, log Bid Price, Change into alignment

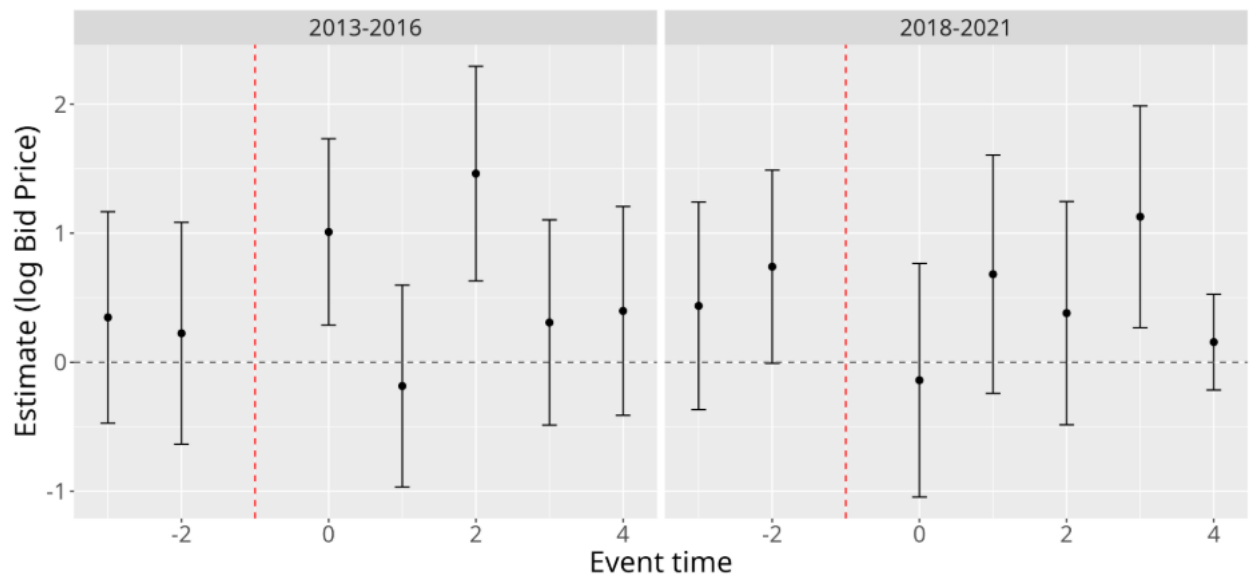


Figure B2 – Hungary: Event Study coefficients, log Bid Price, Change out of alignment

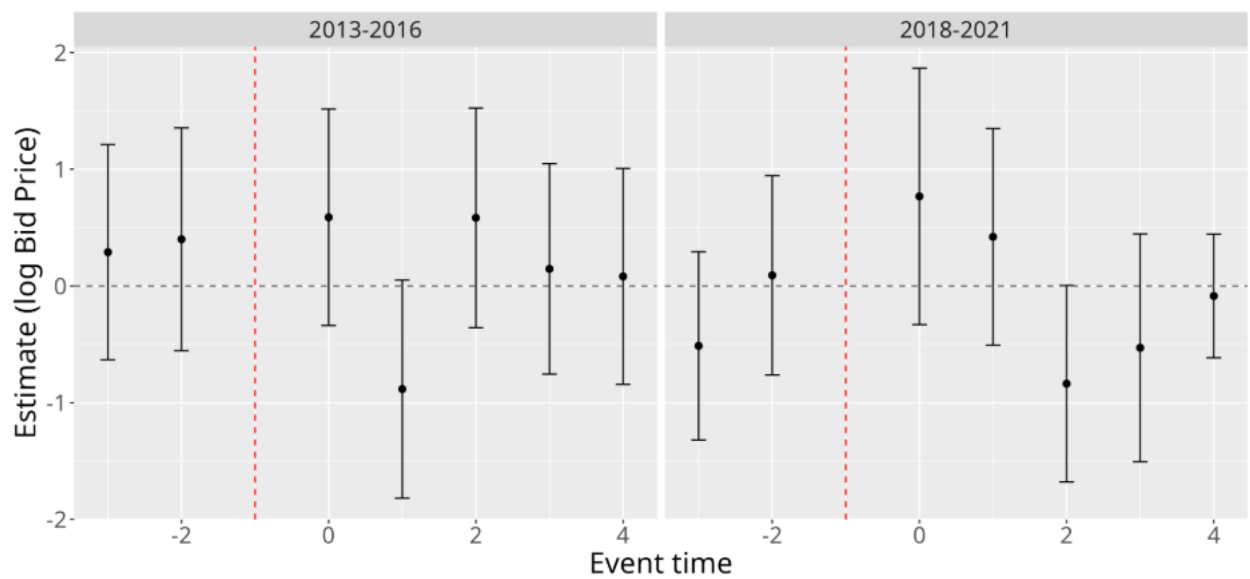


Figure B3 – Romania: Event Study coefficients, log Bid Price, Change into alignment

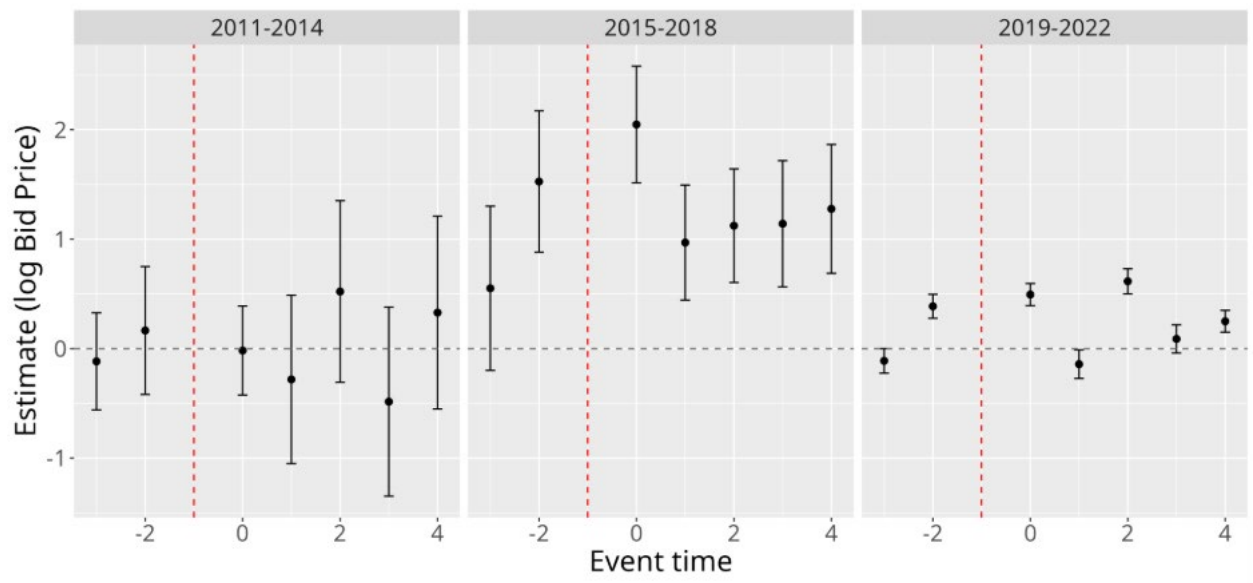


Figure B4 – Romania: Event Study coefficients, log Bid Price, Change out of alignment

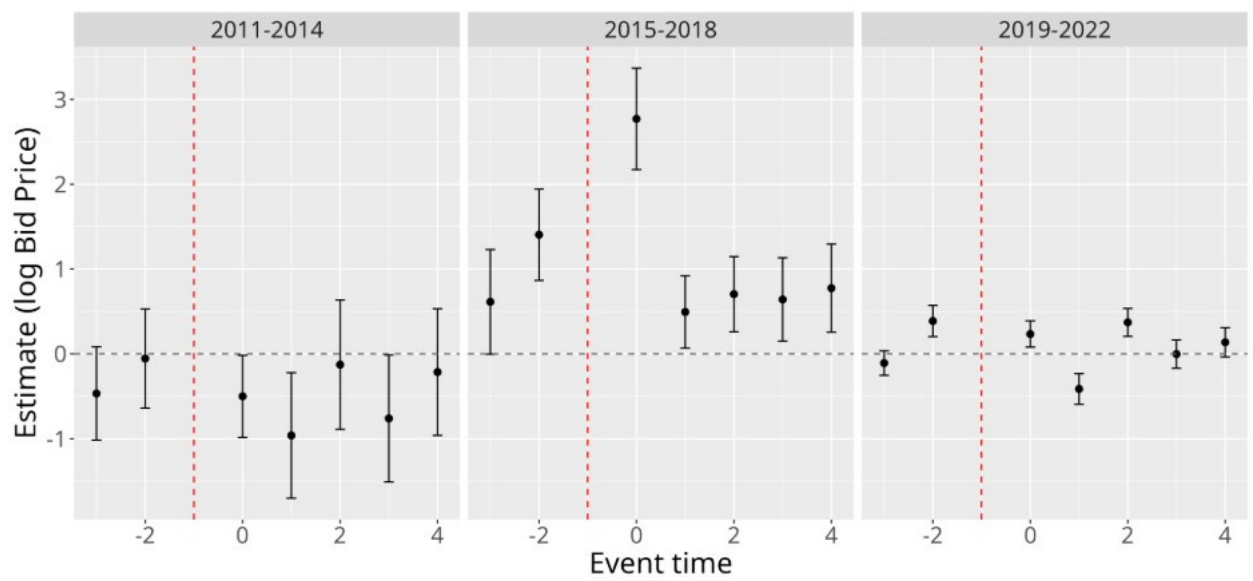


Figure B5 – Croatia: Event Study coefficients, log Bid Price, Change into alignment

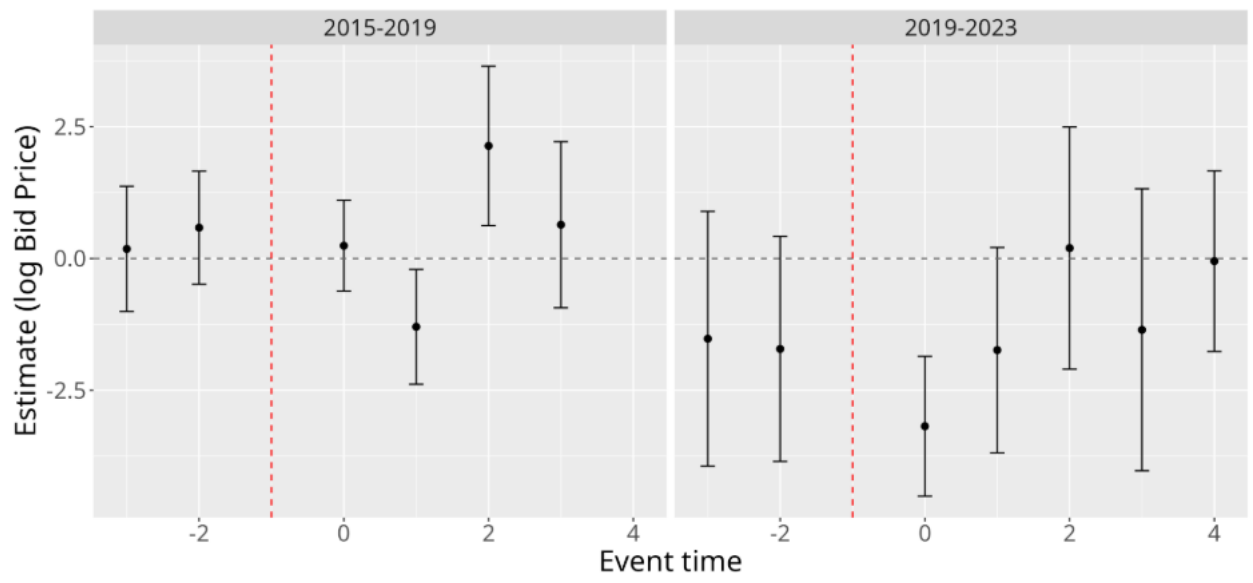


Figure B6 – Croatia: Event Study coefficients, log Bid Price, Change out of alignment

