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Public Procurement's Long COVID: Emergency Responses and Shifting Corruption Risks in Europe

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ABSTRACT

The COVID-19 pandemic caused an unprecedented surge in spending to combat the pandemic with widespread accusations of corruption. The magnitude, drivers, and trajectory of corruption across the pandemic response remain underexplored. This paper approximates the impacts of the pandemic on public procurement corruption risks and the mechanisms underpinning their persistence and diffusion. Administrative data on all high-value, regulated government tenders across Europe from 2016 to 2023 are analyzed. Fixed-effects regressions on the contract level estimate the impact of the pandemic-induced spending shock on corruption risks. We find that the COVID-19 spending surge was followed by sustained increases in corruption risks, particularly in medium-integrity countries. This is largely driven by market entrants without COVID-19-product experience exploiting weaker pandemic corruption controls to secure large contracts. We also find small spillover effects from corruption risks in COVID-19-related products to healthcare and wider procurement markets, driven by buyers normalizing emergency procurement practices beyond emergency contracts.

1 | Introduction

Public procurement accounts for about one-third of general government spending. It is heavily regulated to ensure value for money, fair competition, and transparency, which enables citizens and stakeholders to monitor public funds. However, public procurement is also highly vulnerable to corruption due to the large amounts of money involved, as well as its technical and legal complexity. These pressures may become even stronger during an emergency, which makes hiding corruption easier as spending exceptionally large amounts in a short period is often required. Thus, competition, integrity, and transparency may be temporarily weakened during crises, and they may permanently suffer as a result of one-off shocks.

The COVID-19 pandemic has been a major health emergency, requiring rapid responses to protect citizens' health and lives, presenting public procurement systems with unprecedented challenges. Governments had to procure massive quantities of specific medical supplies and services under challenging emergency conditions, often sidestepping usual competitive procedures. This meant that many countries (seemingly) faced the short-term choice between speed and integrity in public procurement and typically chose speed in the face of mounting health pressures.[1] Hence, it is unsurprising that the main short-term effect of emergencies, such as the pandemic, was a considerable increase in corruption risks in both high and low integrity countries (Bandiera et al. 2021, Part I and III). Many governments spent large sums through emergency procurement procedures such as single-sourcing and direct awards, rather than using standard open and competitive procurement procedures. However, it is unclear to what degree the short-term impacts of emergency spending persisted and in what ways they shaped integrity systems across the whole healthcare system and beyond. Examples from other policy areas such as labor markets or transportation have identified large one-off and sizable long-term effects of COVID-19 (Ketter et al. 2025; Mohammadi et al. 2023; Liu et al. 2020). In addition, the logic of grand corruption posits that the extra-profit earned from corrupt deals during the pandemic, and the new organizational norms established in the emergency period are likely to expand to other, adjacent areas. Taken together, there are fundamental, yet unexplored questions around the persistence and spillover of COVID-19-induced corruption risks across both high and low integrity countries. To address these gaps, we set out to approximate the short- and mid-term impacts of the COVID-19 pandemic on public procurement corruption and to map the mechanisms underpinning persistence and spillovers of these impacts.

To these ends, we develop a methodology for tracking changes in corruption risks in government contracts in Europe. This is made possible by using large-scale public procurement data from a publicly available official government source: the EU-wide Tenders Electronic Daily (TED) public procurement portal. The analysis is conducted over a 7-year period, between July 2016 and June 2023 with the onset of the emergency period in the middle. We define the start of the pandemic in the EU as February 1st, 2020, and contrast the 3.5 years prior to this date with the 3.5 years that followed. We employ a range of fixed effects regression models using contract-level data from across Europe. We exploit the reasonably exogenous onset of the pandemic and estimate the impact of the ensuing, pandemic-linked additional spending on public procurement corruption risks. The analysis benefits from a wide heterogeneity of corruption risks pre-pandemic across countries, public buyers and private sector suppliers to unpack mechanisms.

First, we establish that there was a large, immediate, and sustained increase in spending on COVID-19-related goods following February 2020 and a much smaller yet discernible increase in spending on other healthcare goods. Importantly, there is no statistically significant change in non-health, general public procurement spending volume. Second, we confirm that the surge in COVID-19-related spending is followed by heightened corruption risks in the procurement of COVID-19-related products. Specifically, we find a 0.073-point increase on the Corruption Risk Index (CRI—a composite score tracking individual red flags such as single bidding or direct awards) in months with the highest spending increase over the country's pre-pandemic baseline (top quartile) compared to the months with low spending (bottom quartile). This increase is substantial, amounting to about 1/3rd of the average pre-pandemic CRI. The identified corruption risk effects are largely down to countries with medium-level pre-pandemic corruption risks, with high pre-pandemic risk countries remaining similarly high-risk and low-risk countries quickly reverting back to low risks. Third, on the micro-level, the corruption-enhancing effect of the spending surge is mainly driven by new market entrants without prior COVID-related product experience who took advantage of the opportunities and won large and high-risk contracts. Fourth, we find small, yet consistent spillover effects both from COVID-19 markets to healthcare and from healthcare to general (non-healthcare) procurement. Specifically, the size of spillovers from COVID-19 CRI to healthcare CRI tripled from 0.024 to 0.085 points due to a unit increase in COVID-19 CRI. These effects are persistent over time, also considering several months of lagged effects. Relating to our findings on the spending shock size, the strength of the spillover from COVID-19 to healthcare is greater when the initial spending shock is larger. Fifth, we confirm one of the main mechanisms behind such spillovers going through buyers where COVID-related corruption risks are entrenched and then high-risk practices become acceptable in non-COVID-related purchases too.

These findings present a range of contributions to the literature. First, we show that the corruption-inducing effect of the COVID-19 pandemic persisted over time going beyond the well-documented initial risk increases, supporting recent findings from Italian natural disasters (Fazekas et al. 2025). The paper contributes to our understanding of how emergency response spending can permanently shift stable corruption-control equilibria, and subsequently why similar procedural relaxations have different implications across countries with different starting levels of corruption controls. Second, we show that the major driver of pandemic-induced corruption is down to the size and speed of the emergency spending shock (Gawronski et al. 2023; McEvoy 2022). While the starting levels of corruption control mattered, most negative effects are concentrated in medium integrity countries where countervailing corruption and anti-corruption forces were tipped to the worse by the pandemic. Third, the diffused effects of the pandemic on public procurement corruption risks are, at least partially, driven by normalizing high-risk procurement practices of buyers and the strengthened market position of risky suppliers. These findings contribute to the wider literature conceptualizing corruption in terms of social norms and defined by the balance between opportunities and constraints of corruption (Mungiu-Pippidi 2006).

2 | Conceptual Framework

The theoretical literature on disasters and emergencies analyzed how corruption in emergency procurement undermines provided products and services and helped identify the key factors which determine how corruption evolves during an emergency, including development of mitigation strategies and coordinated activities (Schultz and Søreide 2008). While public perceptions and media tend to emphasize the corruption-increasing impact of emergencies, theoretical research rather points out the diversity of countervailing factors whose relative strength eventually determines whether corruption increases or decreases during and following a disaster.

Among those which tend to increase corruption a few stand out: large value spending in a short period of time and weakened procedural controls in public procurement such as direct award of contracts instead of open competitive selection of the suppliers, foregoing the publication of call for tenders, or increased concentration of awarded contracts (Compte et al. 2005; Blanco-Varela et al. 2022). Furthermore, internal oversight mechanisms are often sidelined during emergency periods. Administrative checks are greatly weakened with the use of purchasing techniques without supervision, fewer compliance rules followed, or rushed practices. When standard procurement rules are bypassed for speed and large spending volumes, corruption risks increase (Lalliot and Yukins 2020). A second group of scholars argue that corruption risks in emergencies tend to decrease, due to disasters being typically accompanied by greater public scrutiny over public decisions, such as government contracts awarded to selected suppliers. Moreover, the imminent pressures of a disaster also come with the moral imperative of saving human lives which may constrain corrupt actors. In sum, there are theoretically opposing arguments regarding corruption to increase throughout emergencies such as the COVID-19 pandemic in Europe.

In the context of the COVID-19 emergency, it was imperative that governments secure critical products in time. Much of COVID-19-related spending was directed toward the acquisition of scarce medical products—masks, ventilators, and protective equipment—under extreme time pressures and at unprecedented volumes (Gereffi 2020). Due to the urgency of healthcare needs, emergency spending rules were used for many purchases. This implied far fewer corruption controls than usual, both within government and by non-governmental actors. Governments frequently relied on accelerated, simplified, direct, or negotiated procedures without prior publication, thus substantially reducing competitive and procedural safeguards present in standard procurement processes (Griffore et al. 2023). Often, the layers that were sidestepped included preparation and review of tender documentation, multi-tiered sign-off procedures, and most importantly the reliance on competitive pressure generated by multiple bidders that serves as a check on collusion and favoritism (Bauhr et al. 2020).

Moreover, transparency of tenders and contract award decisions was typically weakened throughout the pandemic to lower the administrative burden of procurement officials and allow them to procure quicker. This meant that information on public procurement decisions was often late and less precise than standard, compromising the capacity of civil society and other external actors to scrutinize government public procurement decisions (e.g., the UK's infamous VIP lane for COVID suppliers, Germany's mask suppliers cases, or Slovenia's award of a substantial contract to a gambling mogul) (Transparency International 2021a, 2021b; Baranek and Titl 2021; Sparrow 2021; Nyreröd and Spagnolo 2020). Taken together, the unprecedented surge in emergency spending was accompanied by a significant weakening of corruption controls in public procurement which created favorable conditions for corruption. Evidence on corruption scandals in both high and low integrity countries support this interpretation of the corruption increasing impact of COVID-19 pandemic spending. Taking the scale of emergency spending as the key starting point for public procurement corruption risks, we hypothesize:

Hypothesis 1a. *Increasing spending on COVID*-19*–related products* is *followed by higher corruption in their procurement tenders*.

Countries entered the pandemic with very different pre-COVID integrity levels; hence they are likely to have reacted to the same pressures differently. According to Hypothesis 1a above, the COVID-19-induced spending surge was followed by weakened procedural controls, concentrated discretion of public officials, circumvented ordinary procedures, and weakens competition and horizontal accountability (Silverio-Murillo et al. 2024; Rose-Ackerman 2021; Blanco-Varela et al. 2022). Taken together, weakened oversight effectively lowered the probability of detection. By reducing the expected costs (punishment) of corruption and substantially increasing the rewards of procurement contracts, the pandemic made rent-seeking a highly rational choice (Becker and Stigler 1974). Moreover, COVID-19 pandemic represented a systemic shock to public procurement and the broader governance systems, not only impacting institutional constraints of corruption, but also expanding opportunities for corruption, potentially recalibrating the governance equilibrium prevailing prior to the pandemic (Mungiu-Pippidi 2015).

The starting levels of constraints and opportunities naturally determine the degree to which COVID-19 impacted corruption risks in public procurement, overall. In countries with inconsistently enforced procedural controls, few meritocratic bureaucrats, and weak competition among companies, corruption safeguards continued to be circumvented or ignored. Thus, emergency procurement rules may have merely “legalized” already widespread de-facto practices. At the same time, the pandemic may have increased the opportunities side of the equilibrium by providing additional funds to steal from; even though spending is redirected from other areas (e.g., there is less funding for large infrastructure, to fund additional healthcare spending), the net effect across the country might be null.

By contrast, countries with strong pre-COVID integrity frameworks, the process of relaxing the procurement procedures represented a substantial break with established practices and unleashed previously well-contained corruption incentives. For example, while an official in a high integrity country, say Denmark, could not single-handedly sign off on a large contract to a supplier with no track record in the healthcare market before the pandemic, it suddenly became possible during the pandemic. While these countries experienced the most significant break with prior procurement practices due to COVID-19, they are also the ones with the most robust and diverse anti-corruption frameworks. Within this set of countries, an efficient control of corruption rested on the underlying governance order shaped by effective formal institutions and continued to rely on a broader governance equilibrium of effective and credible enforcement of impersonal rules and social and institutional accountability mechanisms (Mungiu-Pippidi 2015). Hence, we can expect that strong and well-established institutions can withstand the temporary loosening of corruption controls opened up by the emergency situation. At the same time, these are the countries which can quickly reassert their strong corruption controls once the immediate emergency dissipates, and the corruption opportunities opened up by additional spending needs to decline.

Theoretically and empirically, the most interesting set of countries are those where pre-COVID corruption controls were intermediate, as these countries experienced a substantial break with established norms, yet they lack the robustness of anti-corruption institutions to bounce back from the shock once immediate spending needs lower (Gnaldi and Del Sarto 2024; Griffore et al. 2023). The combination of rapid spending and partially effective controls exposed them as most vulnerable to risk increases both in the short and mid-terms. It is this set of countries where the spending surge may translate into the greatest increases in corruption risks.

Hypothesis 1b. *The effect of COVID*-19 *emergency spending on public procurement corruption is the largest in countries with medium level pre-COVID corruption control*.

Tracking the mechanism of how corruption risks increase due to emergency spending is challenging, considering the potential for multiple pathways at play. A plausible channel, often highlighted by qualitative examples, is when new suppliers without prior relevant experience, but with capacity and experience to engage in corrupt transactions, enter the COVID-19 market. The unprecedented demand for COVID-19-related products, which were not previously purchased at such a large scale, combined with lax entry conditions and accelerated procurement timelines, allowed companies with little prior relevant experience to gain access to lucrative contracts and gain a foothold for the longer term. Due to lack of established compliance systems or experience, newcomers may rely on political-bureaucratic relations or opportunistic practices (Baranek and Titl 2021; Thomann et al. 2024). Numerous cases show how politically connected, and yet inexperienced, often newly created firms, were awarded large contracts for emergency products such as masks and protective covers, but only to deliver substandard products (Lawrence 2021; Transparency International 2021a, 2021b). A second channel may involve already present suppliers, who have already competed on fair terms, but have identified the pandemic as an opportunity to expand their operations and seek extra profit by circumventing rules or colluding with corrupt officials (Atkinson et al. 2020; Saeed and Kohler 2025; Thomann et al. 2024). These two mechanisms are not mutually exclusive and may be at play simultaneously, thus reinforcing each other. For instance, new and established suppliers might have simultaneously adapted their behavior to the extraordinary procurement context.

These impacts are unlikely to play out in all markets equally. The COVID-19 crisis represented an asymmetric shock, first and foremost, affecting COVID-19-related products, such as masks and ventilators, and only indirectly impacting the rest of the healthcare market and general procurement. This product-specificity was also evidenced by the emergency guidance of the European Commission applying only to a defined list of products such as surgical masks (European Commission 2020). Thus, one would expect corruption risks to increase primarily in the procurement of COVID-19 products, without immediate effect on other markets. However, prior literature has demonstrated that once emergencies weaken corruption control and corruption risks increase, corrupt actors have strong incentives to prolong and extend the scope of emergency arrangements (Schultz and Søreide 2008; Klitgaard 1988, chapter 1). Hence, we can argue that if emergency procurement increases corruption risks in the markets where emergency spending takes place, it may also spill over into adjacent markets (Fazekas et al. 2021), particularly those that are organizationally and technologically similar, such as other healthcare product markets. In this regard, COVID-19 procurement serves as a gateway: once actors establish corrupt networks in pandemic-related products (e.g., masks, tests, ventilators), they may leverage these capabilities to expand into adjacent markets with compatible procurement structures and partially overlapping networks (Kipfer and Mohamud 2021; Yan and Cao 2022). Over time, as exceptional procurement becomes normalized, these spillovers may even reach unrelated sectors (Lalliot and Yukins 2020).

We therefore consider two possible spillover scenarios. First, a limited spillover impacting the healthcare sector that is all health care products other than COVID-19 products, including diverse items such as cancer drugs or medical machinery only. Second, a full spillover transcends risks beyond health sectors into the general public procurement market, even for very different products such as building football stadiums or buying school meals. The first type, limited spillover, is far more likely, given the organizational and technological similarities between COVID-19-related and other healthcare markets. The second type, full spillover, is much less likely given how distant many markets are from healthcare. In addition, it is expected that only once limited spillover takes place, it is possible that a more widespread, full spillover arises.

At any rate, spillover pathways go through learning and routinization. Once a buyer and supplier have established practices under emergency COVID-19 procedures, such as using direct procurement or being a single bidder, they have become easier to replicate for other purchases. Over time, the same "informal" rules and discretionary practices can be transferred to unrelated markets, such as office equipment, vehicle purchases or construction projects, exemplifying how corruption risks move from sector-specific to system-wide risks. For instance, studies on disaster aid and reconstruction show that, once networks and incentives are in place, corruption that often begins in the designated sector tends to expand to unrelated sectors (Gawronski et al. 2023; Yamamura 2014; Khurana et al. 2022). These represent extreme cases and are likely to develop from each other. Hence, we hypothesize:

Hypothesis 2a. *(Limited spillover): Increased corruption in COVID-19-related products is followed by higher corruption risks in health-related products.*

Hypothesis 2b. *(Full spillover): Increased corruption in health-related products is followed by higher corruption risks in general public procurement.*

Importantly, the spillover is likely conditional on the scale of the original spending shock and associated corruption risk impacts. Where emergency procurement remained limited, increases in corruption risks are expected to be small or nonexistent. By contrast, where there was substantial pandemic-related spending, the size of the shock provided greater rents and opportunities, increasing the likelihood and magnitude of spillovers. Therefore, heterogeneity in spending volume and intensity helps explain why some systems experienced limited disruptions, while others a more widespread shock.

Tracking the mechanism of the post-pandemic patterns of spillovers originating from the initial COVID-19 spending shock is challenging given the multitude of factors at play. One key mechanism goes through public buyers who initially loosen their corruption controls in COVID-19-related spending by routinely applying non-competitive tendering practices among others. Then the public officials use their newly established informal networks and the corrupt gains extracted to behave similarly in adjacent domains. The expectation is consistent with research on fiscal relief spendings or natural disaster relief funds that cluster to specific and adjacent domains (Leeson and Sobel 2008; Fenner and Mahlstein 2009; Schultz and Søreide 2008). If corruption controls and standard norms of open and fair competition in public procurement are not re-established, these practices can spread to entirely unrelated fields such as office supply purchases by hospitals. A key proposition here is that it is public buyers' learning processes which account for much of the spillover effects from COVID-19-related spending to healthcare and even potentially to general public procurement.

3 | Research Design

3.1 | Data

We analyze administrative data on public procurement tenders and contracts, which is collected from the EU-wide publication portal, Tenders Electronic Daily (TED). The dataset covers 26 EU member states (excluding Malta), as well as the United Kingdom (until its withdrawal from the EU), Iceland, Norway, and Switzerland. The analysis focuses on comparable periods before and during the COVID-19 emergency in Europe, specifically covering of August 1, 2016 to 30th June 30, 2023 (42 months before and 41 months during the onset of the emergency period). In most complete models, we analyze the full dataset of over 1.5 million contracts. This data contains information related to the purchasing of goods, services, and works by government agencies, public utilities, and other publicly funded organizations. Among the key variables available are descriptions of the goods or services being purchased, the suppliers or contractors providing them, the type of the procurement process (e.g., open, restricted), the value of the contract(s), and the number of bidders. Further details of the data collection and preparation process are reported in Fazekas et al. (2024).

In this study, we categorize contracts into three main procurement categories reflecting the degree to which the pandemic impacted them: COVID-19-related, healthcare (other than COVID-19-related), and all other products. We rely on the European Commission's official list of COVID-19; Table A1 in Appendix A provides a detailed list of items classified as COVID-19-related in this analysis.[2] Contracts are classified by using common procurement vocabulary (CPV) codes and matching keywords to tender titles. We validate the results manually to ensure that duplicates and ambiguous cases are discarded. We classify healthcare contracts as those that fall under the CPV division 33, but are not related to COVID-19 products. For example, medical breathing devices are classified as COVID-19 related, whereas dialysis equipment is classified as a healthcare product. Goods that are not specified as COVID-19-related or healthcare-related are referred to as general goods. Table 1 reports the total number of contracts before and during the pandemic, along with overall totals. Table A2 presents the definitions of all variables used in the analysis. Tables A3 and A4 in Appendix A show the distribution of each variable for the entire analysis period and for the period during the pandemic.

In the main analysis, the unit of observation is the awarded contract, which allows us to capture more granular features and relationships than would be possible with higher-level averages, such as country–month. While contracts represent the primary unit of analysis, some covariates (such as procurement spending or monthly lags of corruption risks) are measured at the country–month level and incorporated accordingly. For the analysis examining impact mechanisms, we shift the level of observation to the organization–month or organization levels to better capture dynamics playing out within organizations.

3.2 | Indicators

The analyses make use of indicators capturing corruption risks, pandemic period, procurement spending, contract-related control factors, and product groups. While the latter has been defined above (COVID-19-related, healthcare, and general procurement), the other indicator groups are introduced briefly here.

To measure corruption risks in public procurement, we calculate the corruption risk index (CRI) which is based on typologies of corruption that are specific to public procurement and detectable with open public procurement data. The CRI is a composite score of individual red flags: single bidding, the length of the advertisement and decision periods, the incidence of non-open procedure types, the lack of a call for tender, supplier registered in tax haven,[3] Benford's Law, and the Buyer's dependence on suppliers (the detailed description of the individual indicators comprising the CRI is available in Table A5 in Appendix A). These factors aim to capture high-risk scenarios associated with deviations from open and fair competition in public procurement benefiting selected bidder(s) to the detriment of others. The composite CRI score is defined at the contract level and ranges between 0 and 1, with 0 representing no red flags of corruption (e.g., single bidding). For a more detailed discussion of the CRI methodology and measurement validity, see Fazekas and Kocsis (2020).

While the validity and reliability of the risk indicators used in this analysis have extensively been tested in "normal" times, their behavior in emergency periods is less often studied (Gnaldi and Del Sarto 2024). Corruption risk indicators only

TABLE 1 | Number of contracts before and during COVID-19 pandemic.

Product group	Before pandemic 1st of August	During pandemic	Total
Time period	1st of August 2016–31st of January 2020	1st of February 2020–30th of June 2023	1st of August 2016–30th of June 2023
COVID-19 products	8525	7222	15,747
Healthcare products	110,269	95,061	205,330
General products	550,569	513,019	1,063,588

approximate actual corrupt transactions in public procurement, yet their tailoring to context, considering only relevant, competitive markets, and combining multiple indicators at once all aim to minimize false positives. These strategies may be less effective in assuring indicator validity in emergency situations when non-corrupt reasons for some red flags become more prevalent. For example, expedited, non-competitive tenders may become unavoidable due to the urgency of purchasing needs. Hence, some red flags may suffer from higher false positive rates in emergency situations. However, emergencies also weaken broader institutional controls such as institutional checks and balances, making the corrupt abuse of procedural weaknesses easier, for example, exploiting non-competitive procedures for awarding contracts to connected firms (Fazekas et al. 2025). These countervailing arguments imply that a red flags approach like ours should be carefully applied to emergency situations, such that individual red flags some of which are less directly influenced by emergency rules (e.g., Benford's Law violations in contract value digits) should be separately analyzed. In addition, research designs which limit the use of before-after comparisons across emergencies should be priorities. As will be shown below, we follow these principles in the below analysis.

Given the central importance of COVID-19-induced spending surge, we calculate a range of spending variables capturing aggregate spending in each country within a month on COVID-19-related products, but also on the 2 other product groups. We track absolute spending on each product group (log transformed) while we also derive relative change indicators looking at monthly increases in spending and also increases compared to the pre-pandemic average.

In order to control for a range of contract, organization, and country-level confounders, we also use indicators on supply type (goods, works, services); organization size and type; and GDP (Log), liberal democracy index, number of COVID cases (Log), and population vaccinated (%) for details see Table A2 in Appendix A.

3.3 | Methods

The analysis exploits the fact that a range of European countries with different degrees of corruption control faced a comparably severe emergency at the same time, while the overall regulatory and institutional framework remained constant. For our identification approach, it is beneficial that the COVID-19 pandemic was clearly exogenous to European public procurement systems. However, the pandemic response influenced a wide variety of policies and behaviors, making the interpretation of the coefficient of a simple before-during binary variable problematic. As an alternative, difference-in-differences set-up is not feasible either as there is no suitable control group not affected by the pandemic in Europe. Hence, we opt to look at the main, albeit not the only, driver of the procurement response: the unprecedented surge in spending on specific products necessary for fighting the disease. This makes interpretation more straightforward yet rules out many alternative interpretations as no other major reason for additional spending on COVID-19-related goods can be identified during the pandemic period. Overall, this set-up allows us to identify temporal associations of the pandemic-induced spending surge, even though causal identification akin to randomized experiments is not feasible.

All countries had to follow the EU Public Procurement Directives, establishing a uniform regulatory framework that remained unchanged throughout the study period, including the state of emergency. These Directives already contained emergency clauses intended to address exceptional situations, such as the COVID-19 pandemic. The activation of these clauses, however, effectively normalized the use of direct awards and accelerated procedures, momentarily overriding the traditional public procurement preference for competitive tendering. To minimize legal interpretation challenges, the European Commission issued additional guidance (European Commission 2020) on how to apply emergency clauses during COVID-19. The guidance clarified how contracting authorities can proceed to invoke emergency provisions for the pandemic-related procurements, with reference to critical and impacted products such as masks, protective equipment, ventilator oxygen supplies (Arrowsmith et al. 2021; European Commission 2020).

Despite the uniform regulatory environment, countries diverged sharply in their spending choices—both in terms of the scale of expenditures (amount spent) and its timing (when the spending took place). We argue that spending decisions (largely) followed medical and pandemic-related needs, such as where masks were urgently needed, which were reasonably exogenous drivers of spending. Spending variability is particularly relevant for our analysis when considered against European countries' diverse starting positions, such as their existing corruption controls (Bozzay and Magina 2020). Using spending choices as the main variable of interest, nevertheless, can be followed by confounding results if those organizations within each country got allocated larger amounts which had weaker pre-pandemic corruption controls. We tested this hypothesis and found that organizations of all levels of pre-pandemic average corruption risks significantly and substantially increased spending, with the lowest pre-pandemic average corruption risk public bodies getting most of the additional spending, rather than the highest risk ones (Table A17 in Appendix B). This assures that the additional spending allocation is not simply reinforcing existing institutional differences; rather, changing organizational behavior.

Beyond spending levels, other potential confounding factors, such as administrative overload, staff shortages or supply chain disruptions, also need to be considered and acknowledged, as these can independently influence corruption risks in public procurement. Many if not all of these factors operated systematically across time periods or varied uniformly across countries and contracting authorities. One example is the global supply chain disruption during 2020. The fixed-effects for time and country absorb these common shocks and time-invariant heterogeneities. As a result, what remains is variation in spending that cannot be attributed to these background conditions. Nevertheless, there are also time-varying confounders that can differently operate across contracting authorities for example administrative overload hitting different buyers at different times. Unfortunately, some of these factors may not be fully accounted for. Finally, reverse causality also poses a potential problem for identifying causal effects which we can only partially address by specifically testing time lags and the sequence of events (e.g., 1. state of emergency, 2. spike in spending, 3. increase in corruption risks).

Our analysis proceeds in three steps. First, we provide a descriptive overview of the pandemic shock to public procurement, motivating the subsequent explanatory analysis. This analysis shows that while emergency-driven public procurement is followed by sharp increases in spending on COVID-19-related products, no comparable increase was observed in healthcare or general procurement markets. At the same time, corruption risks rose across all three product groups. Second, we estimate regression models to test Hypothesis 1a, which evaluates whether increased spending on COVID-related products is associated with higher corruption risks in those specific purchases. We also explore the mechanisms behind this relationship. In particular, we interact spending shocks with pre-pandemic corruption risk levels and examine the role of new suppliers without prior COVID-related procurement experience in shaping observed patterns. Third, we test Hypothesis 2a and Hypothesis 2b, which address the underlying spillover dynamics. These models assess whether corruption risks originating in COVID-19 procurement overflowed into adjacent healthcare markets (limited spillover, Hypothesis 2a) or diffused further into general procurement markets (full spillover, Hypothesis 2b). To unpack these mechanisms, we conduct buyer-level regressions, focusing on how the size of the initial shock and within-buyer behavioral change drive spillovers.

In order to test hypotheses, we estimate several OLS regressions at the contract level, with country and month fixed effects to account for time-invariant heterogeneity and within-year seasonality. We estimate the following regressions:

$$\text{Covid products CRI} = \alpha + \beta \cdot \text{Spending} + \gamma \cdot \text{Controls} + \delta c + \tau t + \epsilon \tag{1}$$

where Covid products CRI is the contract-level CRI scores for each contract. Spending is different variations of within-country monthly spending to capture the policy response in the post COVID period. Controls are various country level controls such as GDP (logged), the liberal democracy index (V-Dem's LDI) (Coppedge et al. 2024). We also include covid specific controls such as the percentage of population vaccinated and the logged number of COVID cases.[4] δ^c and τ^t are country and month fixed effects, respectively. ε are the standard error term. A positive and significant β would indicate that the surge in COVID-19-related spending is positively associated with corruption risk scores.

During the COVID-19 pandemic, many jurisdictions adopted streamlined procurement procedures (such as direct awards, shortened advertisement periods, and simpler procurement procedures) as administrative responses to COVID-19 pandemic. These emergency provisions, while continuing to represent enhanced risks of corruption, will mechanically affect certain CRI components. Specifically, the increased use of direct awards inflates indicators such as single bidding, non-open procedure types, and shortened advertisement periods, even in the absence of corruption. As we are only looking at the post-pandemic period in this analysis exploiting the variation in additional spending across countries and periods, this is less of a concern for our empirical strategy. Nevertheless, to explore potential confounders and offer a more detailed view of our results, we conduct disaggregated analyses examining individual red flags separately rather than using the composite CRI. This approach allows us to distinguish between components that more directly reflect regulatory responses (e.g., call for tender publication, procedure type, advertisement duration) and those that capture behavioral choices less directly affected by emergency rules (e.g., supplier registered in tax haven, Benford's Law deviations, buyer dependence on suppliers). The disaggregated results are presented in Tables A22–A29 in Appendix B.

$$\begin{aligned}\text{Covid products CRI} = {} & \alpha + \beta_1 \text{Log(Spending deviation)} \\ & + \beta_2 (\text{Pr e} - \text{Pandemic Risk Group}) \\ & + \beta_3 \text{Log(Spending deviation)}(\text{Pr e} - \text{Pandemic Risk Group}) \\ & + \delta_c + \pi + \varepsilon\end{aligned} \tag{2}$$

where Covid products CRI is the contract-level corruption risk score for each COVID-19 contract. Spending deviation captures the deviation of monthly COVID-19-related spending from pre-pandemic country-level baselines, reflecting the scale of the emergency procurement response. Pre-pandemic Risk Group is the country-level average CRI score measured between August 2016 and February 2020, categorized into low-, medium-, and high-risk tertiles to capture structural differences in institutional procurement quality prior to the pandemic. β_2 and β_3 are the key parameters of interest, capturing the interaction between the spending surge and pre-pandemic risk levels, that is, whether the corruption risk-enhancing effect of emergency spending varied systematically depending on a country's starting institutional conditions. As in Equation (1), controls include GDP (logged) and the liberal democracy index (V-Dem's LDI), with COVID-specific controls, vaccination rates, and logged case counts are added for robustness. δ_c and τ_t are country and month fixed effects respectively, and ϵ are the standard error terms.

$$\begin{aligned}\text{CRI Adjacent Market} = {} & \alpha + \beta_1 \text{ CRI Affected Market} \\ & + \beta_1 \text{POST COVID} + \beta_2 \text{POST COVID} \\ & + \beta_3 \text{ (POST COVID} \cdot \text{CRI Affected Market)} \\ & + \beta_4 \text{ Spending Affected Market} \\ & + \beta_5 \text{ Spending Adjacent Market} \\ & + \gamma \cdot \text{Controls} + \delta c + \tau t + \epsilon\end{aligned} \tag{3}$$

where CRI Affected Market is the market where we hypothesize the spillovers originate from. CRI Adjacent Market is the market where spillovers are expected to occur. POST COVID is a binary that captures post-pandemic contracts. POST COVID · CRI Affected Market is an interaction term to capture the effect of increases in corruption risk scores in the post pandemic period. Spending Affected Market and Spending Adjacent Market capture the aggregated monthly level of spending in each market, Finally, we add the same country level controls in addition to country and month effects used in Hypothesis 1a. A positive and significant $\beta 3$ would indicate that spillovers indeed occur across markets.

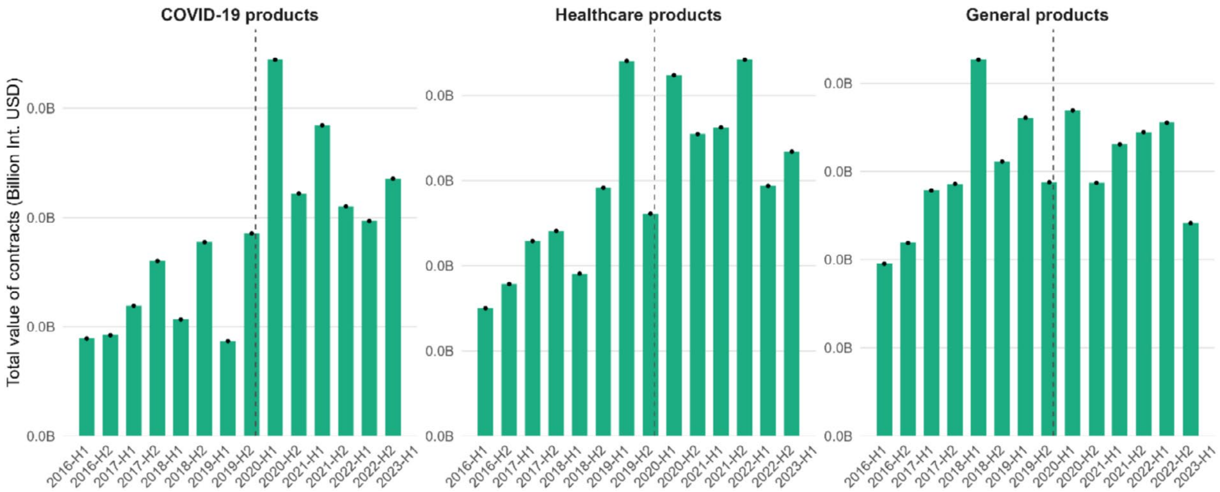

FIGURE 1 | Public procurement half yearly total spending across Europe by product groups, 2016–2023 (TED Data).

In order to test Hypothesis 1a and Hypothesis 2a hypotheses, we rely on the specific timing of the pandemic onset: February 2020.[5] As we may not account for time trends with this set-up, we conducted placebo tests with three different cut-off dates (February 2017, 2018, and 2019) and corresponding analysis periods. The placebo tests estimate whether similar spending patterns emerge in the periods preceding the pandemic and whether spending results in similar corruption risk increases compared to the actual pandemic period. If results using the placebo, earlier than the actual cut-off dates yield insignificant and unreliable results, we can rule out existing pre-pandemic trends driving our main results.

As a final analytical step, the study extends the methodology applied by Abdou et al. (2022) to estimate the budgetary costs of corruption risks during the pandemic. This approach relies on OLS regressions of relative prices (final contract prices divided by estimated contract values) on CRI in the presence of controls. Comparing observed prices with counterfactual prices reflecting a hypothetical no-corruption risks scenario allows us to estimate the financial losses attributed to heightened corruption risks during the pandemic.

4 | Results

4.1 | Descriptive Analysis: Additional Spending Triggered by COVID-19

To contextualize the subsequent analysis, Figure 1 illustrates half-yearly total spending in the three product groups: (a) COVID-19–related, (b) general health–related, and (c) all other public contracting, covering the period from August 2016 to June 2023 (please note the slight discrepancy of our full observation period and the periods used for the chart which was necessary for producing equal, half-yearly aggregates). We observe a sharp and immediate increase in total spending on COVID-19–related products following the onset of the emergency period (Figure 1, left panel). Beginning in February 2020, governments ramped up procurement to meet urgent pandemic-related needs. Total spending increased by 201 million USD for COVID-19–related products, approximately doubling spending from before to during COVID-19. This increase is statistically significant and substantially very large. Spending in healthcare and broader, non-healthcare markets also increased somewhat (Figure 1, middle and right-hand panels), but the additional spending was much smaller compared to the size of the markets and, in the case of general procurement, the increase was not statistically significant (Table A6 in Appendix A). This pattern provides the basis for our identification strategy: COVID-19–related product groups can be interpreted as being subject to an exceptionally large exogenous spending shock, while other product groups serve as controls without a comparable large spending surge.

The rise of COVID-19-related spending was not constrained to specific countries, but occurred across all European macro regions: Western, Northern, Eastern, and Southern. Figure A1 in Appendix A reveals that, despite differences in procurement systems or pre-pandemic baselines, most regions experienced a sharp increase in spending on COVID-19–related products following the onset of the pandemic. This strengthens the interpretation of the spending surge as a general shock across Europe, making procurement authorities face similar pressures and constraints.

4.2 | Hypothesis 1: Spending and Corruption Risks

To assess our first hypothesis, we first review simple trends in corruption risks using the CRI. Consistent with the surge in total spending, corruption risks increased sharply during the early stages of the pandemic. This pattern is consistent with expectations, given the unprecedented, wide-ranging, and fast-paced circumstances, combined with exceptional procurement efforts to acquire medical products without delay. Notably, however, the average CRI remains elevated across all product groups throughout the entire study period compared to the pre-pandemic baseline, suggesting a sustained weakening of corruption control mechanisms. This pattern is supported by statistical tests across the three product groups, which show that CRI scores for COVID-19–related products increased by 0.040 points (from 0.148 to 0.188 points); compared to a 0.023 points increase for general health–related products and 0.024 points for all other procurement categories (see Table A7 in Appendix A).[6]

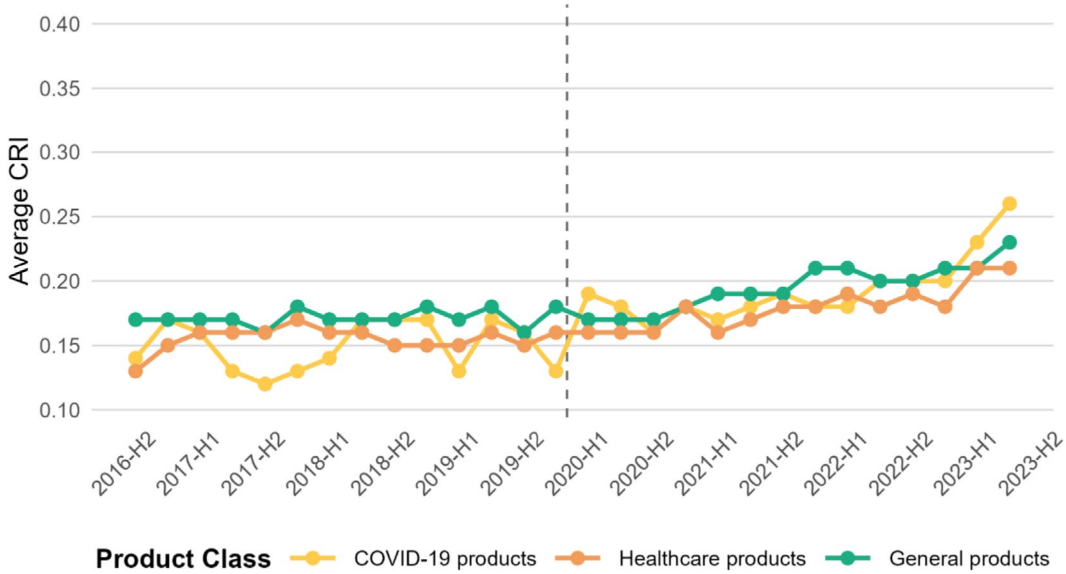

FIGURE 2 | Public procurement CRI across Europe, 2016–2023, Quarterly averages by product groups (TED Data).

COVID-19–related products experienced an immediate increase in corruption risk scores coinciding with the introduction of pandemic restrictions in February 2020, with levels continuing to rise over time and peaking in the final two quarters of the study period (Figure 2, red line).[7] By contrast, general health–related products showed little change in CRI scores in the immediate aftermath of the pandemic's onset, followed by a delayed increase that suggests a lagged adjustment to the new risk environment (Figure 2, green line). The broader public procurement market in Europe also remained relatively stable for roughly 6 months before beginning to rise, broadly mirroring the delayed pattern observed in the general health group (Figure 2, blue line).

We also examine CRI trends by product groups in different European regions (Figure A2). The charts indicate broadly similar patterns of increasing CRI scores related to COVID-19 products, following the onset of the pandemic. The exception is Northern Europe, where risks rose more gradually over time. Such a pattern suggests that distinct mechanisms may account for why rising spending triggered higher corruption risks in some regions but not in others. Although the immediate increase observed elsewhere was absent in Northern Europe, risk levels nonetheless rose steadily. Taken together, the evidence indicates that COVID-related risks increased across Europe, manifesting either immediately or with a delay, depending on each jurisdiction's control mechanisms. Figure A2 also shows that risks for general health products and other procurement categories also rose consistently after the pandemic across European regions. These trends support our first hypothesis: while spending initially surged only for COVID-19 products, increases in corruption risks were observed across procurement categories seemingly unrelated to COVID-19 spending.

4.2.1 | Hypothesis 1a Corruption Risk Increases Following Additional Spending

Our first hypothesis (Hypothesis 1a) proposes that increased spending would be accompanied by higher corruption risks. The OLS regression results (Table 2) confirm this expectation, showing that during the pandemic higher spending is consistently and significantly associated with higher CRI scores. By contrast, no statistically significant relationship between spending and CRI is found in the pre-pandemic period (Table A8, Appendix A), indicating that the rise in corruption risks can be attributed to extraordinary spending.

In Model 1, logged spending on COVID-19 products is positively and significantly associated with CRI scores: more spending appears to increase corruption risks. Specifically, doubling spending—which is about the observed increase in average monthly spending—is associated with an increase of approximately 0.0028 points on the CRI score (i.e., 1.9% increase of the average pre-pandemic CRI of 0.148). To test the robustness of this relationship, we consider two alternative measures of spending. Model 2 uses growth in monthly COVID-related spending and reveals a similarly small, positive, and statistically significant effect. A hypothetical doubling of COVID-related spending from 1 month to the next is associated with an increase in CRI of 0.0034 points, corresponding to 2% of the average CRI in the month before the start of the pandemic. Model 3 uses a log-transformed measure of spending deviation relative to the pre-pandemic country-level COVID-19 spending. Consistent with the previous specifications, the results show that higher deviations from pre-pandemic spending are associated with higher CRI scores ($\beta = 0.008$). When additional controls for pandemic intensity, vaccination rates, and the logarithm of COVID-19 cases are included in Model 4 ($\beta = 0.007$), the results remain stable, indicating the robustness of the relationship.

Given the highly skewed distribution of COVID-19-related spending and our underlying theoretical framework emphasizing the impact of surging spending, we can expect that the impact of COVID-19 spending is non-linear. To test this claim, we estimate the effect of different degrees of spending increases compared to the pre-pandemic baseline. We created 4 quartiles of the pandemic period spending deviations compared to the pre-pandemic country average.[8] Model 5 in Table 2 finds

TABLE 2 | Effects of additional spending on CRI of COVID-19 products during the COVID-19 pandemic 2020–2023.

DV: COVID products CRI	Model 1	Model 2	Model 3	Model 4	Model 5
Spending (log)	0.004*** (0.001)				
Spending monthly growth (log difference)		0.005*** (0.001)			
Spending deviation (log transformed)			0.008** (0.002)	0.007** (0.002)	
Spending deviation log difference groups (moderate decrease/no change)					0.047** (0.015)
Spending deviation log difference groups (moderate increase)					0.066*** (0.015)
Spending log difference deviation groups (large increase)					0.073*** (0.016)
GDP (log)	Yes	Yes	Yes	Yes	Yes
Liberal democracy index	Yes	Yes	Yes	Yes	Yes
Population vaccinated (%)	No	No	No	Yes	Yes
Number of cases (log)	No	No	No	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
R^2	0.194	0.198	0.195	0.196	0.198
Adjusted R^2	0.188	0.192	0.189	0.190	0.192
RMSE	0.129	0.128	0.129	0.129	0.129
Observations	10,131	9307	9664	9664	9998

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $^*p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$.

that, compared to months when spending was well below the pre-pandemic baseline, corruption risks steadily increase as a function of additional spending on COVID-19 products. The estimated CRI increases are particularly large when spending deviations grow larger: moderate deviations (3rd quartile) are associated with a 0.066-point increase, while larger deviations (4th quartile) correspond to a 0.073-point increase (35% of average pre-pandemic CRI). These patterns are illustrated in Figure A3 in Appendix A.

We run a range of robustness tests. Appendix B, Tables A22–A29, unpack the main effects by rerunning the main models by each individual red flag composing the CRI. These disaggregated results show that the observed effects hold not only for those risk factors which are directly resulting from pandemic-induced policy changes such as the use of direct award procedures, but also for those which are less automatic and only indirectly relate to the emergency rule changes such as Benford's Law deviation. Appendix B, Table A19 shows that there is no positive significant effect of additional spending on CRI using placebo pandemic onset dates from the pre-pandemic period. This is not surprising also because we find no notable increase in spending around the placebo cut-off dates (Table A18 in Appendix B).

The explanatory power of the models remains stable (adjusted $R^2 \approx 0.19$), with country and month fixed effects accounting for institutional and temporal variation. Taken together, these findings provide evidence for Hypothesis 1a, underlining that spending surges in COVID-19 procurement contracts are closely followed by increased corruption risks. Crucially for our theory, marginal effect sizes are relatively small (0.003–0.008) while predicted impacts of very large spending increases are relatively large (0.05–0.07), exceeding the observed average CRI increase throughout the pandemic (0.044 points increase, from 0.151 to 0.195 points). This suggests that it is the extraordinary nature of quick and large spending which accounts for the bulk of the risk increases.

4.2.2 | Hypothesis 1b: Heterogeneous Effects by Pre-Pandemic COVID-19 Corruption Risk Levels

To assess the impact of starting corruption controls on the corruption risk-enhancing effect of the pandemic (Hypothesis 1b), we look at the interaction between pre-pandemic procurement corruption risk levels and the already investigated spending surge. Pre-pandemic risks are measured as country-level average CRI scores for COVID-19-related products between August 2016 and February 2020. We categorize countries as high-, medium-, and low-risk groups based on the tertiles of the country-level CRI distribution (Table A9, Appendix A). The interaction analysis uses OLS models where the dependent variable is the contract-level CRI score for COVID-19 products during the pandemic. The key explanatory variables are the interaction between pandemic-related spending deviations (log-transformed) and pre-pandemic country risk levels. All models in Table A10 in Appendix A include country and month fixed effects to account for country-specific and temporal factors influencing CRI scores. In addition, Model 1 controls for GDP and degree of democracy (liberal democracy index), while Model 2 adds COVID-specific controls.

As hypothesized, we find that the corruption-enhancing impact of the COVID-19-induced spending surge is largest in countries with medium levels of pre-pandemic corruption risks. The impact of additional spending is close to zero and insignificant in both high and low starting corruption risk countries. The effect size for medium pre-pandemic corruption risk countries is more than six times as big as the average effect across Europe, identified above in Table 2, which shows that the main effect observed is driven by countries with moderate levels of corruption controls in Europe, representing the most vulnerable category regarding pandemic-induced corruption risks. Figure 3 depicts these different estimated relationships between spending deviation and corruption risks. High risk countries remain consistently high, while medium risk countries show a pronounced upward trajectory as the spending deviation increases. These findings support our theoretical considerations that in countries with medium levels of pre-pandemic public procurement corruption risk, the pandemic could tip their corruption control equilibrium over to the worse. These countries were characterized during the pre-pandemic period by some degree of effective corruption control through functioning formal institutions and procurement procedural controls. In these contexts, the large spending surge induced by COVID-19 most likely strengthened corrupt groups and normalized weaker control processes to the degree that reversing corruption risk increases have become very hard. Unlike the low pre-pandemic corruption risk group, these countries could not rely on a dense and well-functioning set of control institutions and effective civil society to bounce back once the COVID-19 emergency dissipated.

Three placebo tests using alternative starting dates (January 2017, January 2018, and January 2019) yield no comparable positive significant effect (Table A20). This difference in the results for different time windows (placebo vs. actual) indicates that our hypothesized mechanism is likely to drive the effects rather than pre-pandemic time trends.

4.2.3 | Micro-Mechanisms: Suppliers With and Without COVID-19 Product Experience

Having shown that the COVID-19-induced spending increases corruption risks overall and across specific contexts, the next step is to understand the mechanism through which such risks develop. We investigate one key mechanism central to Hypothesis 1a: whether corruption risks were driven by established or by new suppliers capitalizing on the opportunities provided by the onset of the pandemic. We restrict the data to COVID-19 contracts and create a binary variable, defined at the supplier-year level, equal to one for a supplier's contract when the supplier wins a COVID-related contract after having no COVID-related contracts in the previous 2 years.

In Table 3, Model 1 gathers evidence that it is market entrants of the COVID period who are the main drivers of the overall increased corruption risks. Market entrants in general are no different from market incumbents in terms of their corruption risk levels as the coefficient of no COVID experience is small and statistically insignificant. As expected, the post-February 2020, COVID period is of generally higher corruption risks for all types of suppliers. Crucially for our mechanism interpretation, it is the suppliers who entered COVID-19 product markets during the pandemic which are of particularly high risk, even higher than the already increasing pandemic period corruption risk level. This is captured by the positive, significant interaction term (no COVID experience * Post period). In this model, we account for heterogeneity across firms by controlling for supplier size, measured as the total number of contracts over the analysis period (August 1, 2016 to June 30, 2023). To hold the overall context constant, we also control for country-level GDP (log), the

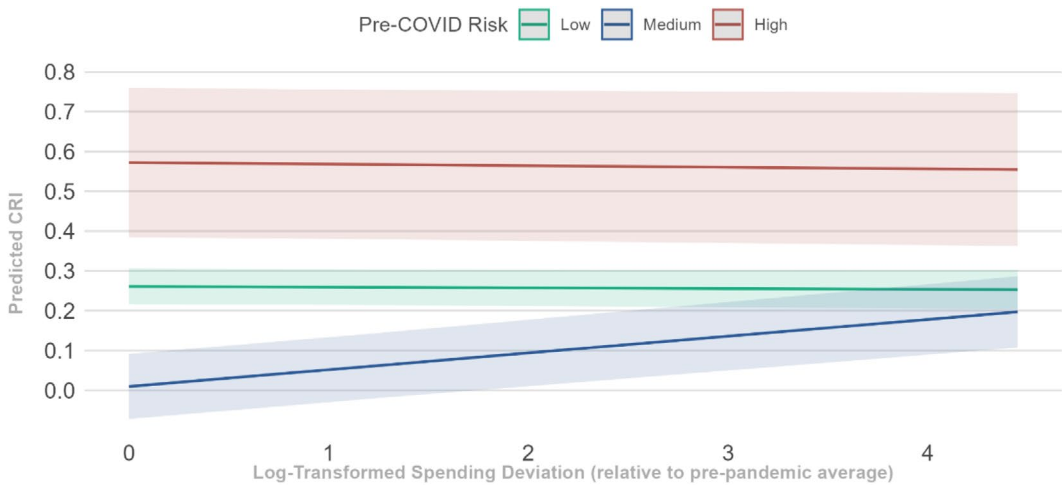

FIGURE 3 | Effects of COVID-19 Spending Deviation by pre-pandemic corruption risk levels, during the COVID-19 pandemic 2020–2023. Predicted values are marginal effects from Model 2 (Table A10). Predictions are adjusted for log GDP = 10.75, liberal democracy index = 69.69, population vaccinated = 31.85%, log number of cases = 13.95, for country = France, and date = 1 March 2020.

TABLE 3 | Interaction between supplier size and its COVID-products experience, contact and organization level linear regression models, 2016–2023.

DV: COVID CRI	Model 1	Model 2	Model 3
Sample	**Covid-19 contracts**	**Established suppliers (no COVID experience = 0)**	**Market entrants (during pandemic, POST COVID = 1)**
No COVID experience	−0.007 (0.008)		0.016* (0.008)
POST COVID	0.126*** (0.020)	0.099*** (0.029)	
No COVID experience × POST COVID	0.018+ (0.009)		
GDP (log)	Yes	Yes	Yes
Liberal democracy index	Yes	Yes	Yes
Overall supplier size (iog)	Yes	Yes	Yes
Country FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
R^2	0.200	0.297	0.199
Adjusted R^2	0.194	0.280	0.193
RMSE	0.131	0.120	0.129
Observations	18,619	3913	10,224

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

Liberal Democracy Index, and include country and month fixed effects.

To further explore the different role incumbents and market entrants play, Models 2 and 3 restrict the sample along two complementary dimensions. Model 2 isolates contracts awarded to established suppliers (those with prior COVID-19 product experience) and examines how their corruption risks evolved across the pandemic period. Among this group, corruption risks rose significantly after February 2020 (0.099), confirming that the pandemic period was associated with higher risks even among experienced suppliers, though the absence of the interaction term here reflects that this model captures the incumbent baseline trajectory rather than the entrant premium. Model 3 restricts the sample to the pandemic period only and re-estimates the effect of having no COVID-19 experience on corruption risk during this window. The results show that market entrants during the pandemic period exhibit significantly higher corruption risks relative to their experienced counterparts (interaction term coefficient is 0.016). Together, Models 2 and 3 reinforce the core finding from Model 1; the mechanism behind Hypothesis 1a operates primarily through the entry of suppliers without prior COVID-19 product experience who secured contracts during the surge, with established suppliers showing elevated but comparatively lower risks.

4.3 | Hypothesis 2: Spillover Effects

We now turn to testing the spillover hypothesis (Hypothesis 2a). Specifically, we investigate whether the heightened corruption risks observed in COVID-19 procurement extended into healthcare (Hypothesis 2a) and, ultimately, into the broader public procurement market (Hypothesis 2b). Table 4 (Model 1) examines spillovers from the COVID-19 market to the healthcare market (i.e., excluding COVID-19 products). First, the post-COVID coefficient indicates a 0.048 CRI (≈0.32 SD) increase in healthcare contracts at the onset of the pandemic. More importantly, the spillover effect from COVID-19 procurement risk strengthened substantially after the pandemic began. Prior to COVID-19, moving form no to maximum CRI in COVID-related procurement was associated with a 0.024 point (≈0.16 SD) increase in healthcare CRI. During COVID-19, this association rose to 0.085 points, more than a three-fold increase. This suggests that corruption risks observed during emergency procurement did not remain confined to COVID-19 products but extended to other health products, albeit the magnitude of the impact remained relatively small. This empirical evidence lends support to Hypothesis 2a (limited spillover).

In Table 4 (Model 2) tests the full spillover effect, investigating whether the rise in healthcare corruption risks further spilled over into the broader public procurement market, including products unrelated to healthcare. The interaction between healthcare corruption risks and the post-COVID period is again positive and highly significant. It shows that spillover effects from healthcare corruption risk extended into the general procurement market and increased after the onset of the pandemic. Before COVID-19, the observed level of healthcare CRI (0.152) contributed approximately 0.016 points to overall corruption risks (0.152 * 0.1035 = 0.0157). During COVID-19, with healthcare CRI rising to 0.174 and the spillover effect strengthening, this contribution

TABLE 4 | Spillover effects of healthcare and COVID-19 procurement risk on adjacent markets corruption risk, 2016–2023.

	Health CRI	**Other CRI**
DV: CRI	**Model 1**	**Model 2**
POST COVID	0.048*** (0.004)	0.036*** (0.002)
CRI COVID (monthly)	0.024*** (0.004)	
POST COVID × CRI COVID	0.061*** (0.006)	
CRI healthcare		0.104*** (0.003)
POST COVID × CRI healthcare		0.033*** (0.003)
Spending COVID (log)	Yes	
Spending health (log)	Yes	Yes
Spending other (log)		Yes
GDP (log)	Yes	Yes
Liberal democracy index	Yes	Yes
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.230	0.148
Adjusted R^2	0.230	0.148
RMSE	0.131	0.142
Observations	285,227	1,247,205

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

increased to about 0.024 points (0.174 * (0.1035 + 0.0326) = 0.0237). While these effects are small, they nevertheless highlight that the pandemic amplified corruption risk spillovers beyond the healthcare sector. This suggests that the routinization of emergency practices (such as discretionary awards and reduced competition) extended beyond healthcare and shaped broader procurement dynamics. In a series of placebo tests using earlier, pre-pandemic cut-off dates, we find largely confirmatory results, in all but one model there is a positive significant increase in spillover effect (see Table A21 in Appendix B).

Taken together, these findings provide empirical support for the spillover hypotheses, both for the limited spillover from COVID to healthcare (Hypothesis 2a) and for the full spillover from healthcare to general, non-healthcare procurement (Hypothesis 2b), even though effect sizes are modest. The magnitude of the coefficients indicates that the largest effects are concentrated where the original shock was most intense (COVID-19 procurement), but their influence did not stop there. Over time, emergency procurement appears to have acted as a gateway, normalizing practices that gradually diffused into adjacent and ultimately into unrelated markets. This mirrors the conceptual pathway described in Hypothesis 2a and Hypothesis 2b, where limited spillover is a necessary precursor for the more extensive, system-wide effects.

A natural concern with the spillover hypothesis is whether these observed effects are short-lived responses to sudden pandemic-related procurement shocks or whether they persist over time. To address this, we extend the baseline models by including monthly lags of CRI and run the regressions on the post-COVID period. The inclusion of both contemporaneous and lagged CRI scores confirms that spillover effects are not transitory, rather persistent over time (Table A11 in Appendix A). For health-related procurement, we find small but significant coefficients not only for the contemporaneous COVID-19 CRI, but also for the first and second monthly lags. Similarly, when analyzing spillovers from healthcare into broader procurement, the coefficients remain positive and significant across contemporaneous, one-month, and two-month lags, indicating that spillover effects decrease over time but do not disappear. This indicates that increased corruption risks in COVID-19 procurement continued to exert influence in the months that followed, spilling over into healthcare and further into general procurement markets. The persistence of these effects highlights that the pandemic not only generated an immediate corruption risk shock but also set in motion dynamics that extended beyond the initial period of crisis.

Hypothesis 1a proposed that spillovers are conditional on the magnitude of emergency spending for which we found supporting evidence. Bringing this relationship into the spillover analysis proposes that when procurement volumes remain limited, spillovers should be modest, but when spending surges, opportunities and incentives for corruption expand, strengthening spillover effects (not only from COVID to healthcare, but also from healthcare to general procurement). Evidence from Table A12 (Appendix A) and illustrated through predictive margins in Figure 4 supports these expectations. When COVID-related or healthcare-related spending remains low, the models estimate minimal spillovers (green lines in Figure 4, both panels). However, in both health-related and general procurement markets, the interaction between sector-specific CRI scores and logged spending is positive and significant. For COVID-19 procurement, the spillover effect turns positive, already around average monthly COVID-19-related spending during the pandemic (0.014*16 > 0.168). When spending on COVID-19 products is high, we see a substantial spillover from COVID-19 corruption risks to healthcare corruption risks (Figure 4, left-hand panel). The overall pattern is largely the same for the spillover from healthcare to general procurement (Figure 4, right hand panel).

These results reinforce the notion that the COVID-19 crisis did not produce a uniform shift in corruption risks. Rather, the persistence and breadth of spillovers depended crucially on the size of the underlying procurement spending shock, helping explain why some countries experienced broader corruption challenges than others.

4.3.1 | Mechanisms for Hypothesis 2a and Hypothesis 2b

In this section, we explore the mechanisms through which corruption risks in emergency procurement during the COVID-19

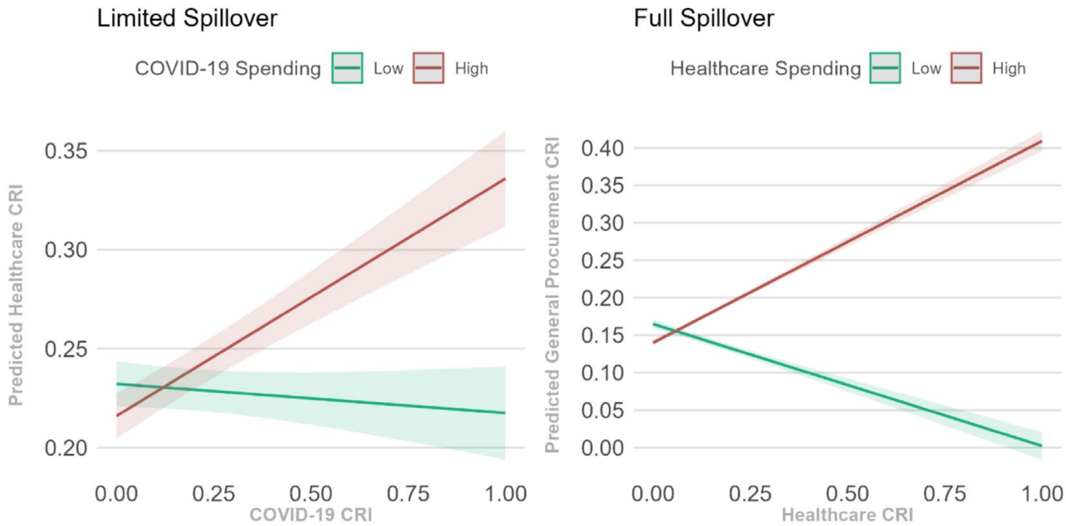

FIGURE 4 | Spillover Effects of Spending–CRI Interactions Across Markets During the COVID-19 Pandemic, 2020–2023.

pandemic spilled over into adjacent markets. Conceptually, buyers which exhibited high corruption risks during the pandemic may have shifted to a generally higher risk pattern permanently, by normalizing risking behaviors across the board. The central question is whether COVID-19 procurement served merely as a temporary disruption confined to the health domain (limited spillover), or whether it created broader systemic vulnerabilities (full spillover), and more precisely, what those channels of risk propagation are.

Table A13 in Appendix A investigates the underlying mechanisms for Hypothesis 2a and Hypothesis 2b on the buyer-month level. We filter the data for each product market (healthcare and other procurement) and retain only large buyers, defined as those above the 25th percentile in total contract volume. Small buyers are excluded for two reasons: (1) they tend to exhibit greater variability in outcomes, and removing them allows us to focus on more robust and meaningful effects among large buyers; and (2) larger buyers are more likely to operate across multiple markets, which aligns with our interest in cross-market spillovers. Models 1 and 2 (Table A13) estimate these spillover mechanisms through the behavior of large buyers. Both specifications control spending in the relevant and adjacent markets, buyer type, and buyer size (total number of contracts), and include country and month fixed effects to account for time-invariant and temporal confounders.

For limited spillover (Hypothesis 2a), increases in COVID-19 related products corruption risk are positively associated with higher corruption risk in healthcare procurement more broadly. Among large buyers (Model 1 in Table A13), the post-COVID interaction term for COVID-related CRI is positive and statistically significant (0.063 or ≈0.42 SD), indicating that pandemic procurement practices propagated into adjacent healthcare markets through these large buyers. These results suggest that well-resourced buyers were able to extend corrupt practices from COVID-related products into other healthcare products, such as medical equipment or specialty drugs.

For full spillover (Hypothesis 2b), we find evidence of broader propagation of corruption risks into general procurement markets through large buyers (Model 2 in Table A13). The post-COVID interaction term for healthcare CRI remains positive and significant (0.023 or ≈0.15 SD), while the main effect of healthcare CRI is strongly positive (0.139 or ≈0.93 SD). This suggests that higher healthcare corruption risk already spills over into the general procurement sector, and that this spillover increased further following the onset of the COVID-19 pandemic. Figure 5 depicts the limited and full spillover mechanisms using the predicted CRI scores in adjacent markets as the CRI scores for buyers in the affected market increase.

We explore these spillover mechanisms further by taking lagged effects on the buyer level into account and by considering the pre-pandemic organizational CRI levels. First, we include both contemporaneous and lagged CRI scores in the models to capture the dynamics of post-COVID spillovers (Table A14, Appendix A). For health-related procurement, both the contemporaneous COVID-19 CRI (0.054 or ≈0.36 SD) and its one-month lag (0.055 or ≈0.37 SD) are positive and statistically significant, indicating that corruption risks persist over time and are not transitory at the buyer level. Similarly, for spillovers from healthcare into general procurement, the contemporaneous healthcare CRI (0.102 or ≈0.68 SD) and its lag (0.039 or ≈0.26 SD) remain positive and significant, showing that spillovers decrease over time but continue to affect adjacent markets. These results demonstrate that the persistence of spillovers identified at the contract level is observable at the buyer level as well. Second, we interact with pre-pandemic buyer CRI with post-COVID sector-specific CRI and split buyers into high and low pre-COVID CRI groups (Table A15 in Appendix A). For the limited spillover models, we find that buyers with low pre-pandemic CRI are more affected by increases in COVID-related corruption risk, resulting in spillovers into the healthcare market. In contrast, buyers with high pre-pandemic CRI demonstrate stronger spillovers into the general procurement market compared to buyers with low pre-pandemic risk (See Figure A4 in Appendix A).

Analyzing the individual red flags composing the CRI in separate models allows us to show which specific behaviors were most likely to take hold and propagate across markets within the same buyers. Tables A30–A37 in Appendix B replicate the main

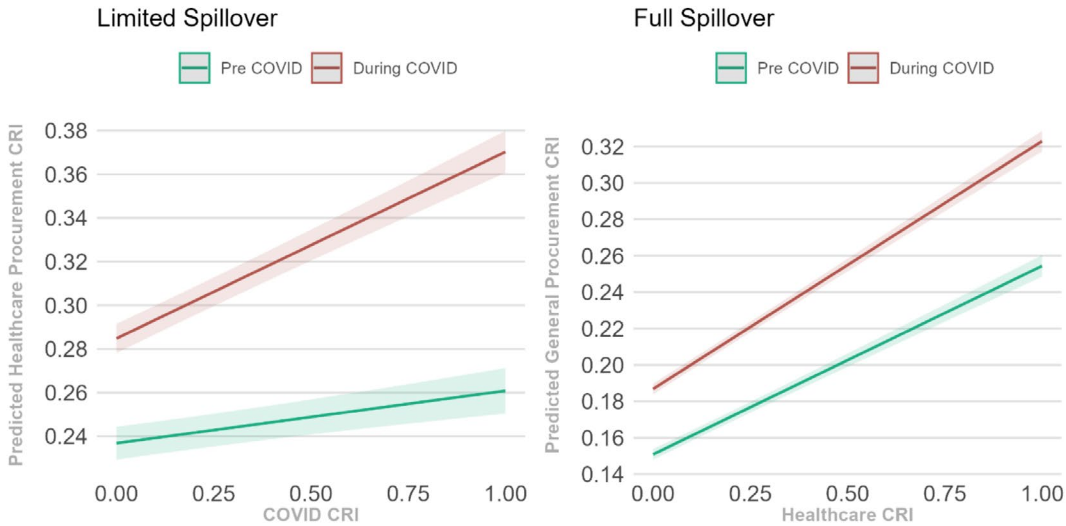

FIGURE 5 | Buyer-level predicted adjacent market CRI by affected market CRI across the COVID-19 pandemic, 2016–2023.

spillover models using each CRI component as the dependent variable separately, with a detailed explanation of results in the same appendix. Overall, we find that the buyer learning mechanism operated through distinct and evolving channels at different stages of spillover. Early spillovers from COVID-products to healthcare were driven by procedural risks, such as non-open procedure types. During the later spillovers from healthcare to general procurement, more discretionary and less formalistic risk factors, such as advertisement period length, became dominant.

Taken together, the results indicate that spillover pathways are both sequential and conditioned by buyer characteristics, particularly pre-pandemic corruption risks. Pandemic procurement initially destabilized corruption safeguards in COVID-19 products, with large, low-risk buyers primarily propagating these practices into adjacent healthcare markets (limited spillover), while historically high-risk buyers acted as the main conduits for broader systemic diffusion into general procurement (full spillover). This pattern supports the conceptual framework: large buyers' greater capacity and integrated procurement portfolios facilitate propagation within similar markets, whereas high-risk larger buyers' post-pandemic adjustments and supplier relationships produce wider, cross-sectoral spillovers. Overall, the evidence underscores that corruption risks initially concentrated in pandemic-related procurement can extend beyond their origin. However, the scale, direction, and intensity of such spillovers are shaped by the combination of buyer size, spending amounts, and pre-existing corruption vulnerabilities; in line with theories of corruption equilibria (Mungiu-Pippidi 2015).

5 | Conclusions and Policy Lessons

This article examines how the COVID-19 pandemic reshaped corruption risks in public procurement. Our analysis provides evidence that the pandemic-induced emergency spending was a key, albeit clearly not the only, driver of increased corruption risks in European public procurement: higher spending on COVID-19 products is associated with higher corruption risks throughout the pandemic. However, this effect is not uniform across countries, rather shaped by pre-pandemic corruption risk levels with medium pre-pandemic corruption risk countries accounting for the bulk of the observed corruption risk increases across Europe. A key mechanism for such increases goes through new suppliers which have no prior COVID-19 product experience, consistent with many reported scandals and anecdotes. COVID-19 market entrants, especially those which became big and stayed on the market for long, account for the bulk of increased risks following February 2020. This highlights that market entry under emergency conditions is notably different from market entry during non-emergency periods.

Even though the COVID-19 spending surge was confined to specific products needed for fighting the pandemic, some corruption risks spilled over to adjacent healthcare markets, and from there into general procurement markets. These dynamics were not temporary; rather, they were persistent, with prior corruption risks in one sector influencing higher subsequent risks in others. A key mechanism underlying observed spillovers goes through large public buyers, which bought COVID-19-related and many other products throughout the pandemic. These buyers, once normalizing emergency and other high-risk procurement approaches, started to use them in unrelated markets and purchases.

Our study design faces limitations in terms of identifying the causal effects of pandemic-induced additional spending. The main challenge is that a range of factors and policies changed at about the same time as additional spending kicked in. We develop a time series estimation strategy, using not only the composite CRI but also individual red flags, and we also carefully examine the underlying mechanisms. Still, other concurrent events and mechanisms may bias our estimates, which would need to be further investigated in subsequent research. Hence, the results should be interpreted in the context of the overall emergency period and the corresponding constrained institutional context. Fully disentangling spending from other factors, such as administrative overload, falls beyond the scope of this paper. The analysis isolates only one pathway through which emergency conditions affect corruption risks, and other potential mechanisms that may operate in parallel are beyond the scope of the analysis.

To assess the fiscal magnitude of these risk increases, we estimated the budgetary costs of corruption in pandemic procurement (Abdou et al. 2022). We regress relative prices (i.e., the ratio of contract value to estimated tender price) on the CRI. There is a positive and significant association between CRI and relative prices within all three product groups (Table A16). For COVID-19 products, regression results suggest a total corruption cost of USD 331 million, or 3.26% of overall pandemic procurement spending. When looking only at the additional corruption risk increase during the pandemic, compared to the pre-pandemic levels, the additional corruption costs amount to roughly USD 249.4 million, equivalent to 2.46% of all COVID-19 procurement spending. While these figures are substantive, they surely represent a lower-bound estimate as many purchases resulted in faulty and unusable products (Transparency International 2021a, 2021b).

Taken together, our findings highlight how emergency spending, while necessary to save lives, created systemic vulnerabilities persisting across time and impacting adjacent markets. By relaxing competitive safeguards and mobilizing unprecedented resources in a compressed time frame, governments not only increased risks in COVID-19 procurement itself but also set in motion spillover processes that reshaped corruption vulnerabilities across the broader procurement landscape.

Given the persistent and diffuse nature of the emergency spending-induced corruption risk increases, effective anti-corruption in emergency response should not stop with the end of the emergency period. Instead, ex post controls should be strengthened, emergency spending practices effectively limited to a short period and specific product group; and market entrants who could have made unfair advantage of relaxed tendering conditions should be interrogated for their capacity and corruption controls.

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Conflicts of Interest

The authors declare no conflicts of interest.

Data Availability Statement

The data that support the findings of this study are openly available in opentender at https://opentender.eu/all/download.

Endnotes

[1] It must be noted that, some countries reached such high corruption levels in the name of speed over integrity, that many purchases resulted in faulty, unusable or unnecessary products. Hence, it is questionable whether there was really a trade-off between speed and integrity in many cases.

[2] We use the list of products from the European Commission website to define COVID-related products. https://health.ec.europa.eu/system/files/2022-02/guidanceclinicaltrials_covid19_en_1.pdf.

[3] We use data from the Tax Justice Network (TJN) to identify tax haven jurisdictions using their Financial Secrecy Index https://fsi.taxjustice.net/.

[4] As the Oxford COVID-19 Government Response Tracker (OxCGRT; https://github.com/OxCGRT/covid-policy-tracker/tree/master) ceased publishing updates at the end of 2022, we carry forward the last available values for subsequent periods. This assumes that the final reported levels represent the maximum reached in terms of COVID-19 cases and vaccination coverage.

[5] Please note that we are not running a difference-in-differences estimator, as there is no suitable, never treated control group we could rely on.

[6] Note: The *t*-test for the CRI is based on the monthly average index for each product group.

[7] We examined the contracts underlying the peak observed in the final two quarters. The number of contracts during this period was relatively low (529 and 364, respectively), which makes the CRI scores more sensitive to individual tenders. In addition, a substantial share of the contracts involved equipment such as monitors and PPE, where the elevated CRI values appear to reflect genuine procurement risks.

[8] The groups are defined based on the logarithmic difference in spending as: Large Decrease (−4.11 to −1.65), Minimal Decreases/No Change (−1.65 to 0), Moderate Increase (0–3.25), Large Increase (3.25–5.71).

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Appendix A

TABLE A1 | COVID-19 products classification keywords based on European commission guidance.

CPV code	CPV code description
33157500	Hyperbaric chambers
33157300	Oxygen tents
33192160	Stretchers
33172200	Resuscitation device
33157400	Medical breathing device
33195000	Patient-monitoring system
33195100	Central monitoring station
33195200	Respiratory monitors
33195110	Infusion pumps
33194110	Coverall
18114000	Protective and safety clothing
35113400	Garments for biological or chemical protection
35113410	Safety visor
18142000	Protective gear
18143000	Protective goggle
33735100	Frames and mountings for goggle
33735200	Goggle
33735000	Disposable glove
18424300	Surgical gloves
33141420	Medical gas mask
33157100	Air filter
42514310	Antiseptic and disinfectant
33631600	Sterilization, disinfection and hygiene devices
33191000	Thermograph
33111640	Oxygen administration unit
33157800	Oxygen therapy unit
33157810	Oxygenator
33186100	Immuno-analysis devices
33127000	Autopsy fluid collection vacuum aspirators or tubing
33926000	Syringe
33141310	Medical needles
33141320	Diagnostic supplies
33124130	Intensive-care unit construction work
45215142	Antiseptics and disinfectants
33631600	Sterilization, disinfection and hygiene devices
33191000	Sterilizer
33191100	Autoclaves

(Continues)

TABLE A1 | (Continued)

CPV code	CPV code description
33191110	Hospital beds
33192120	Gas-therapy and respiratory devices
33157000	Medical gas masks
33157100	Oxygen mask
33157110	Oxygen kits
33157200	Oxygen tents
33157300	Medical breathing devices
33157400	Hyperbaric chambers
33157500	Blow bottle
33157700	Oxygen administration unit
33157800	Oxygen therapy unit
33157810	Disinfection equipment
39330000	Protective and safety clothing
35113400	Oxygen mask
33157110	Medical breathing devices
33157400	Diagnostic agents
33694000	Surgical gloves
33141420	Respiratory monitors
33195110	Medicinal products for the respiratory system
33670000	Medicinal products for obstructive airway diseases
33673000	Cough and cold preparations
33674000	Antihistamines for systemic use
33675000	Protective gear
18143000	Disposable gloves
18424300	Mobile medical services air evacuation stretchers

TABLE A2 | Definitions of variables used in the analysis.

Variable name	Definition
CRI	The average of all non-missing corruption risk indicators for a given contract, representing the overall level of corruption risk in the tendering process. Pre-pandemic CRI level – Average contract-level CRI scores before the pandemic.
POST COVID	A binary indicator denoting the beginning of the COVID pandemic (1st of February 2020).
Spending COVID (log)	Monthly total contract value for COVID-related products (logarithmic scale).
Spending healthcare (log)	Monthly total contract value for healthcare products (logarithmic scale).
Spending other (log)	Monthly total contract value for healthcare products (logarithmic scale).
Spending monthly growth	The logarithmic difference between current total monthly spending and the previous month total spending. Calculated as (log(spending in month *t*) – log(spending in month *t* – 1)).
Spending deviation (log difference)	The logarithmic difference between current monthly COVID-related spending and the country's average monthly pre-pandemic spending. Calculated as (log(spending in month *t*) – log(pre-pandemic average spending)).
Spending deviation groups	A categorical version of the spending deviation variable, dividing observations into four intervals based on the size of deviation from pre-pandemic spending.
Spending deviation (log transformed)	The log transformed ratio between current monthly COVID-related spending and the country's average monthly pre-pandemic spending. The ratio is calculated as (spending in month *t* – Pre-pandemic average spending)/pre-pandemic average spending, we then log-transform the ratio using log(2 + ratio)
No COVID experience	A binary indicator takes the value 1 for contracts where the supplier did not win any COVID-related contracts in the 2 years prior.
Supplier size	The total number of contracts awarded to a supplier, expressed in logarithmic scale.
Buyer type	Classification of buyers according to their institutional category (e.g., National Authority, European Agency, Utilities).
Buyer size	The total number of contracts awarded by a buyer.
Supply type	The category of the tender based on its nature (works, services, goods).
GDP (log)	The gross domestic product, PPP (constant 2021 international $) indicator from the world bank data https://data.worldbank.org/indicator/NY.GDP.MKTP.PP.KD
Liberal democracy index	The Liberal democracy index is a country-year level index from the V-dem dataset. It measures the degree that liberal democracy is achieved. It's values range from 0 to 1, where values of closer to 1 shows a high degree of liberal democracy. Data can be downloaded from https://www.v-dem.net/
Number of COVID cases (log)	The Number of COVID cases by country and year. The data comes from the Oxford COVID-19 government response tracker (OxCGRT; https://github.com/OxCGRT/covid-policy-tracker/tree/master) Since, the data ceased publishing updates at the end of 2022, we carry forward the last available values for subsequent periods. This assumes that the final reported levels represent the maximum reached in terms of COVID-19 cases and vaccination coverage.
Population vaccinated (%)	The yearly percentage of a country's total population that has received a dose of a COVID-19 vaccine. The data is from the xford COVID-19 government response tracker.
Country	The country where the tendering authority is located, based on the buyer's registered location.
Month	The month in which the contract was awarded.
Year	The year in which the contract was awarded.

TABLE A3 | Variable distribution across the analysis period (from 1st of August 2016 to 30th of June 2023).

Variable	Missing rate	Average	SD	Min.	Median	Max.	Histogram
Composite corruption risk index (CRI)	0.00	0.18	0.15	0.00	0.17	1.00	▇▂▁▁▁
CRI—COVID products (monthly average)	0.10	0.18	0.10	0.00	0.17	1.00	▇▅▁▁▁
CRI—Healthcare products (monthly average)	0.01	0.15	0.09	0.00	0.15	1.00	▇▁▁▁▁
CRI—Other products (monthly average)	0.00	0.19	0.07	0.00	0.18	0.45	▂▇▇▂▁
CRI—COVID products (lagged) (monthly average)	0.11	0.18	0.10	0.00	0.18	1.00	▇▅▁▁▁
CRI—Healthcare products (lagged) (monthly average)	0.02	0.15	0.08	0.00	0.15	1.00	▇▂▁▁▁
CRI—Other products (lagged) (monthly average)	0.01	0.19	0.06	0.00	0.18	0.42	▁▆▇▂▁
Log spending—COVID products (monthly total)	0.13	15.90	1.70	11.04	15.96	22.61	▁▆▇▂▁
Log spending—Healthcare products (monthly total)	0.01	18.84	1.78	10.00	19.27	23.38	▁▁▃▇▂
Log spending—Other products (monthly total)	0.00	21.74	1.20	12.46	21.91	27.19	▁▁▃▇▁
Log spending—COVID products (monthly lag) (monthly total)	0.15	15.89	1.68	11.04	15.96	22.61	▁▆▇▂▁
Log spending—Healthcare products (monthly lag) (monthly total)	0.02	18.78	1.76	10.00	19.19	23.38	▁▁▃▇▂
Log spending—Other products (monthly lag) (monthly total)	0.01	21.68	1.20	12.46	21.87	27.19	▁▁▃▇▁
Spending growth—COVID products (monthly)	0.22	0.04	1.85	−8.05	0.03	8.82	▁▃▇▁▁
Spending deviation (log difference)—COVID products (monthly)	0.55	1.06	1.41	−4.10	0.94	5.70	▁▃▇▃▁
Spending deviation (log transformed)—COVID products (monthly)	0.55	1.01	0.84	0.00	0.78	4.46	▇▆▁▁▁
Pre-pandemic CRI—COVID products (country average)	0.00	0.15	0.06	0.05	0.12	0.36	▁▇▂▁▁
Pre-pandemic CRI—Healthcare products (country average)	0.00	0.18	0.05	0.08	0.17	0.30	▃▃▇▂▃
Pre-pandemic CRI—Other products (country average)	0.00	0.14	0.06	0.02	0.13	0.32	▃▇▃▃▁
Relative price—Contract	0.42	125.25	9596.65	0.00	1.00	998,710	▇▁▁▁▁
Population vaccinated (%) (oxford data)	0.54	35.81	33.58	0.00	36.68	86.62	▇▁▁▃▆
Confirmed COVID-19 cases (oxford data)	0.54	6,049,997	9,632,576	0.00	2216,363	38,266,999	▇▁▁▁▁
Liberal democracy index (0–100) (V-DEM)	0.00	74.80	12.64	32.50	79.50	89.20	▁▁▁▂▇

TABLE A4 | Variable distribution during the pandemic period (1st of February 2020 to 30th of June 2023).

Variable	Missing rate	Average	SD	Min.	Median	Max.	Histogram
Composite corruption risk index (CRI)	0.00	0.19	0.15	0.00	0.17	1.00	
CRI—COVID products (monthly average)	0.09	0.20	0.10	0.00	0.19	1.00	
CRI—Healthcare products (monthly average)	0.01	0.16	0.09	0.00	0.16	0.70	
CRI—Other products (monthly average)	0.00	0.20	0.07	0.00	0.19	0.45	
CRI – COVID products (lagged) (monthly average)	0.10	0.20	0.10	0.00	0.19	1.00	
CRI—Healthcare products (lagged) (monthly average)	0.01	0.16	0.09	0.00	0.16	0.70	
CRI—Other products (lagged) (monthly average)	0.00	0.20	0.06	0.00	0.20	0.42	
Log spending—COVID products (monthly total)	0.12	16.28	1.67	11.04	16.30	20.71	
Log spending—Healthcare products (monthly total)	0.01	19.00	1.79	10.00	19.47	22.67	
Log spending—Other products (monthly total)	0.00	21.80	1.15	13.10	22.00	27.19	
Log spending—COVID products (monthly lag) (monthly total)	0.13	16.23	1.66	11.04	16.30	20.71	
Log spending—Healthcare products (monthly lag) (monthly total)	0.01	18.94	1.78	10.00	19.40	22.67	
Log spending—Other products (monthly lag) (monthly total)	0.00	21.75	1.16	13.10	21.95	27.19	
Spending growth—COVID products (monthly)	0.19	0.07	1.73	−5.00	−0.03	5.78	
Spending deviation (log difference)—COVID products (monthly)	0.14	1.06	1.41	−4.10	0.94	5.70	
Spending deviation (log transformed)—COVID products (monthly)	0.14	1.01	0.84	0.00	0.78	4.46	
Pre-pandemic CRI—COVID products (country average)	0.00	0.15	0.06	0.05	0.12	0.36	
Pre-pandemic CRI—Healthcare products (country average)	0.00	0.18	0.05	0.08	0.17	0.30	
Pre-pandemic CRI—Other products (country average)	0.00	0.14	0.06	0.02	0.13	0.32	
Relative price—Contract	0.40	204	13,079	0.00	1.00	998,710	
Population vaccinated (%) (oxford data)	0.13	36.54	33.52	0.00	39.55	86.62	
Confirmed COVID-19 cases (oxford data)	0.13	6,173,454	9,691,119	0.00	2,441,142	38,266,999	
Liberal democracy index (0–100) (V-DEM)	0.00	73.53	13.53	32.50	79.40	88.70	

TABLE A5 | List of public procurement corruption risk indicators included in the CRI.

Variable name	Definition
Single bidding	The indicator flags lots which received only one bid during the tendering process. Single bidding is a well-documented corruption risk indicator as it is the result of non-competitive tendering activities.
Use of non-open procedures	The indicator flags contracts awarded through a non-open procedure type as the use of less transparent procedures allows for higher discretion in awarding contracts to preferred suppliers.
Call for tender document not published	The indicator tracks tenders where the call for tender documents were not published. Not publishing the call for tender makes it less likely that eligible bidders notice the bidding opportunity
Length of submission period	The indicator captures corruption risk associated with the length of the submission period. Contracts are flagged if the submission period length is significantly related to higher probability of single bidding.
Length of decision period	The indicator captures corruption risk associated with the length of the decision period. Contracts are flagged if the decision period length is significantly related to higher probability of single bidding.
Benford's law	The indicator flags contracts awarded by procurement authorities whose awarded contract prices are in violation of Benford's law.
Supplier registered in a tax haven	The indicator flags suppliers registered in a country with a high financial risk—Based on the FSI from the Tax justice network.
Buyer dependence on supplier	The indicator is the share of the total amount (based on contract value) awarded to a specific supplier from a given buyer (i.e., higher the values refer to bigger spending concentration).

TABLE A6 | One-sided *t*-tests of total spending (USD) in product groups from before to during the pandemic period (full period effects, 3.5 years before versus 3.4 years during).

Class	Pre-spending	Pre-*N*	During spending	During *N*	Diff.	*p*	*t*	df	Sig.
COVID-19 products	201,471,209		8,391,406,205,578		10,224,204,734,369	0.000	4.48	65.8	***
Healthcare products	4,269,664,816		131,0125,855,624,833		154,2161,585,960,016	0.002	2.96	78.6	**
General products	50,416,098,921		603,66,453,078,407,027		643,1302,662,308,106	0.224	0.76	2.24	

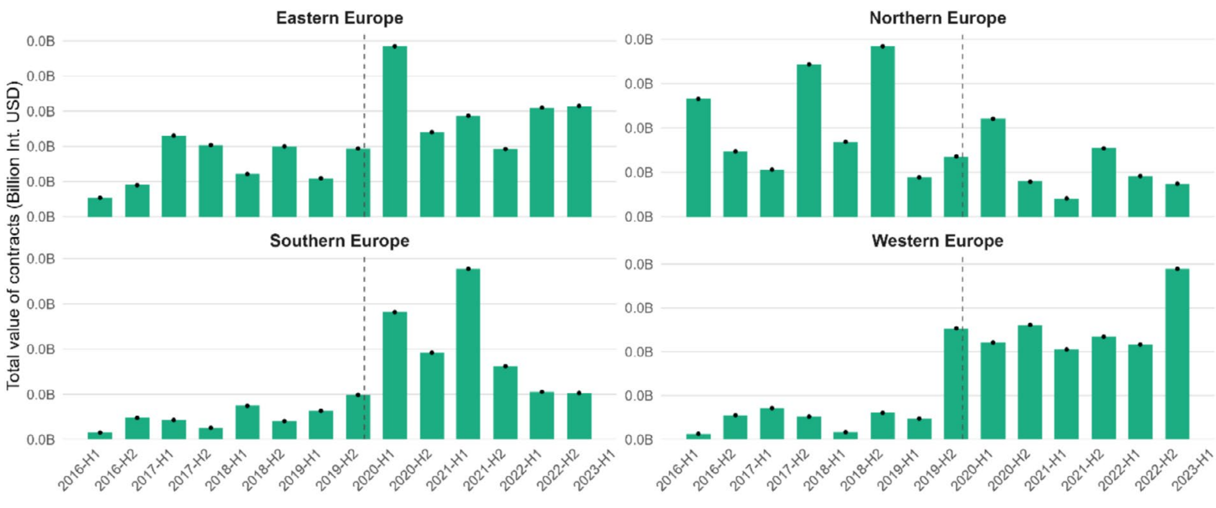

FIGURE A1 | Public procurement half-yearly total spending on COVID-19 products across European regions, 2016–2023 (TED Data). Countries are grouped by region as follows: Western Europe: Austria, Belgium, Switzerland, Germany, France, Luxembourg, Netherlands; Northern Europe: Denmark, Finland, Iceland, Ireland, Norway, Sweden, United Kingdom; Southern Europe: Cyprus, Spain, Greece, Italy, Portugal; Eastern Europe: Bulgaria, Czechia, Estonia, Croatia, Hungary, Lithuania, Latvia, Poland, Romania, Slovakia, Slovenia.

TABLE A7 | One Sided *t*-tests of CRI scores in product groups across the pandemic periods.

Class	Pre-CRI	Pre-*N*	During CRI	During *N*	Diff.	*p*	*t*	df	Sig.
COVID-19 products	0.148	7222	0.188	8525	0.040	0.000	16.9	15,103	***
Healthcare products	0.157	110,269	0.180	95,061	0.023	0.000	35.0	199,172	***
General products	0.171	550,569	0.196	513,019	0.024	0.000	82.6	1,060,258	***

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

FIGURE A2 | Public procurement CRI across European regions, 2016–2023, half-yearly average by product groups (TED data). Countries are grouped by region as follows:—Western Europe: Austria, Belgium, Switzerland, Germany, France, Luxembourg, Netherlands; Northern Europe: Denmark, Finland, Iceland, Ireland, Norway, Sweden, United Kingdom; Southern Europe: Cyprus, Spain, Greece, Italy, Portugal; Eastern Europe: Bulgaria, Czechia, Estonia, Croatia, Hungary, Lithuania, Latvia, Poland, Romania, Slovakia, Slovenia.

TABLE A8 | Effects of additional spending on CRI of COVID-19 products, before the pandemic.

DV: COVID products CRI		
	Model 1	**Model 2**
Spending (log)	−0.002 (0.001)	
Spending monthly growth (log difference)		0.001 (0.001)
GDP	Yes	Yes
Liberal democracy index	Yes	Yes
Population vaccinated (%)	No	No
Number of cases (log)	No	No
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.195	0.191
Adjusted R^2	0.188	0.183
RMSE	0.130	0.130
Observations	8213	7229

Note: By design, spending deviation models cannot be estimated for the pre-pandemic period. Standard errors in parentheses; significance stars: $+p < 0.1$, $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

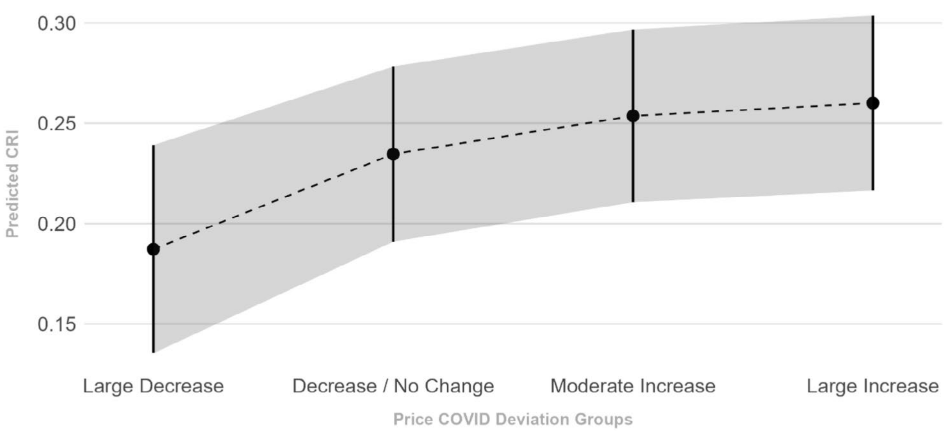

FIGURE A3 | Predicted CRI for different levels of deviation from pre-COVID spending, during the pandemic. Predicted values are marginal effects from Model 5 (Table 3). Predictions are adjusted for log GDP = 10.75, liberal democracy index = 69.69, population vaccinated = 31.85%, log number of cases = 13.95, for country = France, and date = 1 March 2020.

TABLE A9 | Average COVID-19 related products CRI before the pandemic by Country (TED data), 2016–2020.

Country	CRI	Risk level
Austria	0.081	Low
Belgium	0.101	Low
Cyprus	0.152	Low
Germany	0.120	Low
Estonia	0.053	Low
France	0.125	Low
United Kingdom	0.117	Low
Croatia	0.115	Low
Ireland	0.093	Low
Italy	0.119	Low
Latvia	0.081	Low
Netherlands	0.060	Low
Portugal	0.101	Low
Romania	0.123	Low
Sweden	0.116	Low
Switzerland	0.240	Mid
Spain	0.172	Mid
Finland	0.163	Mid
Greece	0.247	Mid
Hungary	0.204	Mid
Iceland	0.205	Mid
Lithuania	0.215	Mid
Norway	0.203	Mid
Poland	0.212	Mid
Slovakia	0.188	Mid
Slovenia	0.204	Mid
Bulgaria	0.256	High
Chech Republic	0.270	High
Denmark	0.363	High
Luxembourg	0.312	High

TABLE A10 | Interaction of pre-pandemic risk levels and COVID-19 spending deviations on contract-level CRI, during the COVID-19 pandemic 2020–2023.

DV: COVID products CRI	Model 1	Model 2
Medium pre-pandemic CRI level	−0.237*** (0.039)	−0.251*** (0.040)
High pre-pandemic CRI level	0.331*** (0.085)	0.311*** (0.085)
Spending deviation log-transformed	−0.001 (0.003)	−0.002 (0.003)
Medium pre-pandemic CRI × spending deviation log-transformed	0.043*** (0.006)	0.044*** (0.006)
High pre-pandemic CRI × spending deviation log-transformed	−0.006 (0.007)	−0.002 (0.007)
GDP (log)	Yes	Yes
Liberal democracy index	Yes	Yes
Population vaccinated (%)	No	Yes
Number of cases (log)	No	Yes
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.200	0.201
Adjusted R^2	0.194	0.195
RMSE	0.129	0.129
Observations	9664	9664

Note: Standard errors in parentheses; significance stars: +$p < 0.1$, *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

TABLE A11 | Spillover effects of contemporaneous and lagged CRI on overall corruption risk, during the COVID-19 pandemic (2020–2023).

	Health CRI	Other CRI
DV: CRI	**Model 1**	**Model 2**
CRI COVID	0.049*** (0.004)	
CRI COVID lag 1	0.019*** (0.004)	
CRI COVID lag 2	0.057*** (0.004)	
CRI healthcare		0.107*** (0.002)
CRI healthcare lag 1		0.074*** (0.003)
CRI healthcare lag 2		0.012*** (0.003)
Spending COVID	Yes	No
Spending healthcare	Yes	Yes
Spending other	No	Yes
GDP (log)	Yes	Yes
Liberal democracy index	Yes	Yes
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.290	0.149
Adjusted R^2	0.289	0.148
RMSE	0.125	0.142
Observations	154,216	1,247,205

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $^*p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$.

TABLE A12 | Spillover effects of spending–CRI interactions on adjacent markets corruption risk, during the COVID-19 pandemic 2020–2023.

	Health CRI	Other CRI
DV: CRI	**Model 1**	**Model 2**
CRI COVID	−0.168*** (0.042)	
Spending COVID (log)	−0.002** (0.001)	
CRI COVID × spending COVID (log)	0.014*** (0.003)	
CRI healthcare		−0.504*** (0.023)
Spending healthcare (log)	−0.005*** (0.000)	−0.002*** (0.000)
CRI healthcare × spending healthcare (log)		0.034*** (0.001)
Spending other (log)		0.001* (0.000)
GDP (log)	Yes	Yes
Liberal democracy index	Yes	Yes
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.289	0.148
Adjusted R^2	0.289	0.148
RMSE	0.125	0.142
Observations	154,216	1,247,205

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $^*p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$.

TABLE A13 | Spillover mechanisms across the pandemic: Limited (Hypothesis 2a) and Full (Hypothesis 2b) spillovers at the buyer-month level, 2016–2023.

	Limited spillover (healthcare)	Full spillover (general procurement)
DV: Sector-specific CRI	**Model 1**	**Model 2**
Sample	Large buyers	Large buyers
Post-COVID	0.045*** (0.015)	0.033*** (0.005)
COVID products CRI (monthly)	0.028+ (0.015)	
COVID products CRI × POST COVID	0.063** (0.021)	
Healthcare products CRI (monthly)		0.139*** (0.007)
Healthcare products CRI × POST COVID		0.023** (0.007)
Spending COVID-products	Yes	No
Spending healthcare-products	Yes	Yes
Spending general procurement	No	Yes
GDP (log)	Yes	Yes
Liberal democracy index	Yes	Yes
Buyer type	Yes	Yes
Buyer size	Yes	Yes
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.222	0.165
Adjusted R^2	0.218	0.164
RMSE	0.129	0.135
Observations	22,195	225,042

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$. The buyer size threshold used is 50 total contracts before the pandemic.

TABLE A14 | Buyer level spillover effects of contemporaneous and lagged CRI on overall corruption risk, during the COVID-19 pandemic (2020–2023).

	Limited spillover (healthcare)	Full spillover (general procurement)
DV: Sector-specific CRI	**Model 1**	**Model 2**
Sample	Large buyers ($N>$ 25th percentile ~ 86 contracts)	Large buyers ($N>$ 25th percentile ~ 18 contracts)
COVID products CRI	0.054*** (0.016)	
COVID products CRI (lagged)	0.055*** (0.016)	
Healthcare products CRI		0.102*** (0.009)
Healthcare products CRI (lagged)		0.039*** (0.010)
Spending COVID-products	Yes	No
Spending healthcare-products	Yes	Yes
Spending general procurement	No	Yes
GDP (log)	Yes	Yes
Liberal democracy index	Yes	Yes
Buyer type	Yes	Yes
Buyer size	Yes	Yes
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.257	0.181
Adjusted R^2	0.252	0.180
RMSE	0.122	0.160
Observations	12,220	116,089

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$. The buyer size threshold used is based on the 25th percentile of the total number of contracts by buyer in each sample.

TABLE A15 | Buyer level spillover effects—Interactions between pre-pandemic risks and CRI, during the COVID-19 pandemic (2020–2023).

DV: Sector-specific CRI	Limited spillover (healthcare)	Full spillover (general procurement)
	Model 1	Model 2
Sample	Large buyers ($N > 25$th percentile ~86 contracts)	Large buyers ($N > 25$th percentile ~18 contracts)
COVID products CRI	0.094*** (0.019)	
Pre-COVID buyer CRI (high)	0.099***	−0.018***
Baseline: Below average	(0.006)	(0.005)
Pre-COVID buyer CRI (high) × COVID products CRI	−0.100*** (0.027)	
Healthcare products CRI		0.102*** (0.015)
Pre-COVID buyer CRI (High) × healthcare products CRI		0.052* (0.023)
Spending COVID-products	Yes	No
Spending healthcare-products	Yes	Yes
Spending general procurement	No	Yes
GDP (log)	Yes	Yes
Liberal democracy index	Yes	Yes
Buyer type	Yes	Yes
Buyer size	Yes	Yes
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.305	0.181
Adjusted R^2	0.299	0.180
RMSE	0.116	0.160
Observations	11,516	116,089

Note: Standard errors in parentheses; significance stars: $+p < 0.1$, $*p < 0.05$, $**p < 0.01$, $***p < 0.001$. The buyer size threshold used is based on the 25th percentile of the total number of contracts by buyer in each sample.

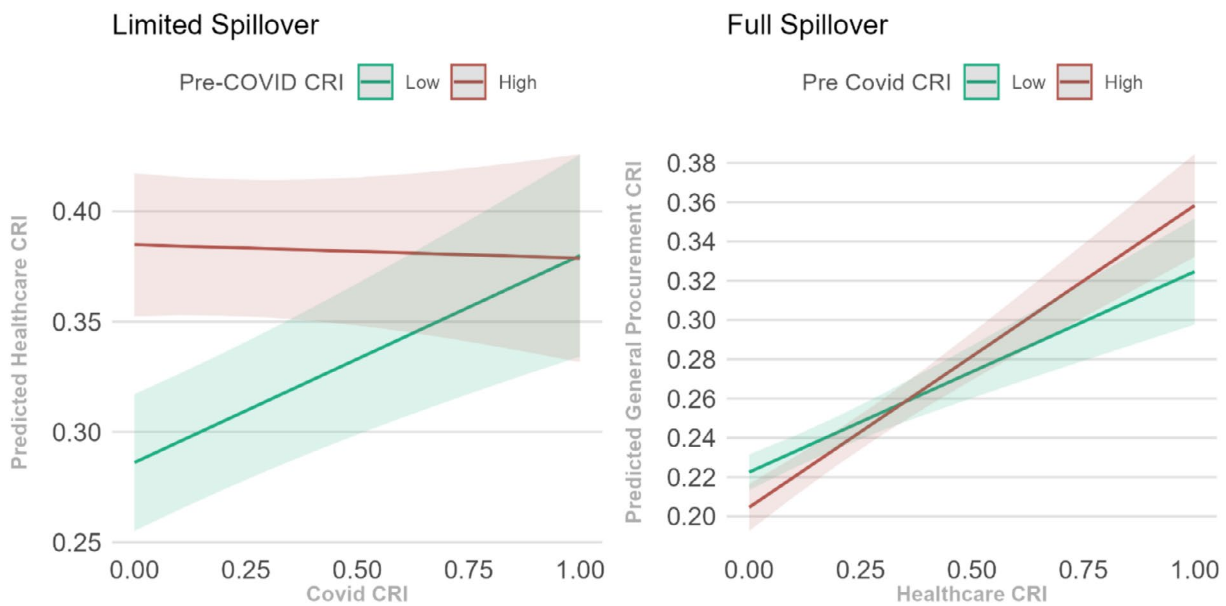

FIGURE A4 | Buyer-level spillover effects by pre-pandemic CRI, during the COVID-19 pandemic (2020–2023).

TABLE A16 | Relationship between corruption risk (CRI) and relative prices in different product markets during the COVID-19 pandemic, 2020–2023.

DV: Relative price	COVID-related products Model 1	Healthcare products Model 2	General procurement products Model 3
CRI	0.155*** (0.017)	0.103*** (0.004)	0.064*** (0.002)
Supply type	Yes	Yes	Yes
Buyer type	Yes	Yes	Yes
GDP (Log)	Yes	Yes	Yes
Liberal democracy index	Yes	Yes	Yes
Country FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
R^2	0.318	0.279	0.091
Adjusted R^2	0.306	0.278	0.091
RMSE	0.132	0.134	0.132
Observations	4634	73,461	285,767

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$. The data is filtered to keep relative prices between 0.5 and 1.3.

TABLE A17 | One sided *t*-tests of total spending (USD) by pre-pandemic risk groups from before to during the pandemic period (full period effects, 3.5 years before versus 3.4 years during).

COVID products CRI pre-pandemic	Pre-spending	Pre-*N*	During spending	During *N*	Diff.	*p*	*t*	df	Sig.
Low	139,429,623	5978	249,394,636	6532	109,965,014	0.001	3.042	74.571	**
Medium	44,322,613	1959	105,500,462	2738	61,177,849	0.015	2.256	45.294	*
High	18,151,143	454	51,310,480	954	33,159,337	0.001	3.216	49.685	**

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

Appendix B

Robustness: Selection on Pre-Pandemic Corruption Risk Level

A potential concern is that authorities with weaker pre-existing procurement capacity may have received disproportionately more emergency funds, such that observed correlations between pandemic spending and corruption risks reflect pre-existing institutional weaknesses rather than any causal influence of emergency procurement conditions. To assess this, Table A17 presents *t*-tests comparing total COVID-related spending before and during the pandemic across pre-pandemic risk

TABLE A18 | Placebo *t*-tests of total spending (USD) by product groups using arbitrary cutoff dates and 1 year pre/post windows.

COVID products CRI pre-pandemic	Diff.	*p*	*t*-statistic	Significance
Cutoff date: 1st February 2017				
COVID-19 products	−9,005,962	0.542	−0.112	
Healthcare products	700,485,294	0.079	1.492	
General products	5,982,543,039	0.147	1.127	
Cutoff date: 1st February 2018				
COVID-19 products	69,782,694	0.126	1.195	
Healthcare products	−66,083,688	0.558	−0.148	
General products	16,022,353,679	0.036	1.949	*
Cutoff date: 1st February 2019				
COVID-19 products	−69,668,976	0.832	−0.988	
Healthcare products	2,618,967,504	0.021	2.251	*
General products	−3,024,223,529	0.629	−0.337	
Cutoff date: 1st February 2020				
COVID-19 products	290,890,293	0.000	4.254	***
Healthcare products	894,210,970	0.729	−0.622	
General products	2,565,305,308	0.680	−0.474	

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$.

TABLE A19 | Effects of additional spending on CRI in placebo cutoff dates.

	DV: COVID products CRI								
	Cutoff date: Feb 2017			Cutoff date: Feb 2018			Cutoff date: Feb 2018		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Spending (log)	−0.010*** (0.003)			−0.010*** (0.003)			−0.008*** (0.003)		
Spending monthly growth		0.001 (0.002)			−0.002 (0.002)			−0.009*** (0.002)	
Spending deviation			−0.024*** (0.006)			−0.015*** (0.005)			−0.018** (0.007)
GDP (log)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Liberal democracy index	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.206	0.192	0.179	0.212	0.215	0.213	0.220	0.230	0.221
Adjusted R^2	0.195	0.181	0.167	0.198	0.200	0.199	0.204	0.215	0.206
RMSE	0.131	0.132	0.132	0.120	0.118	0.120	0.126	0.123	0.126
Observations	2973	2740	2882	2319	2106	2216	2153	1849	2104

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$.

groups. The results do not support the premise of this selection mechanism. While all three risk groups experienced statistically significant spending increases during the pandemic period, the absolute increase was largest among low-risk organizations, which saw spending rise by approximately USD 110 million compared to USD 61 million and USD 33 million for medium- and high-risk groups respectively. If weaker governance capacity had driven disproportionate fund allocation, we would expect high-risk authorities to have absorbed the greatest share of emergency expenditure. Instead, the pattern is reversed: authorities with stronger pre-pandemic governance profiles received the largest monetary increases, suggesting that the spending surge reflects a well-allocated distribution of emergency funds, guided by pre-existing institutional capacity rather than a selection process confounded by pre-existing institutional weakness.

Robustness: Placebo Cut-Off Dates

As a robustness check and in response to concerns regarding causal identification, we implement a series of placebo cutoff tests to verify that the spending increase observed around the February 1st, 2020 pandemic onset is not an artifact of spurious temporal variation. Applying the same one-year pre/post window to three arbitrary cutoff dates (February 1st, 2017, 2018, and 2019) we find no consistent or statistically significant increases in COVID-19 product spending around any of these placebo dates. Where weakly significant differences do emerge across the placebo years, they are confined to general or healthcare product categories and do not replicate across cutoffs, suggesting these are chance findings rather than systematic patterns. In contrast, the true cutoff of February 1st, 2020 yields a highly significant increase in COVID-19 product spending ($t=4.254$, $p<0.001$), while healthcare and general product spending show no significant change at this date, further isolating the effect to pandemic-specific procurement. This pattern of results of statistical significance emerging exclusively for COVID-19 products at the true cutoff and nowhere else provides strong evidence that the observed spending surge is causally anchored to the pandemic onset rather than reflecting pre-existing trends or arbitrary temporal fluctuations.

To further validate the causal interpretation of the main findings, we replicate Models 1, 2, and 3 from Table 2 using placebo cutoff dates of February 2017, 2018, and 2019 (Table A19). We apply a one-year window pre and post the placebo cut-off dates to isolate the effect of the actual pandemic period. In contrast to the positive and significant associations between spending and corruption risk index (CRI) scores observed during the true pandemic period, the placebo regressions yield systematically negative and, where significant, inversely signed coefficients across all three spending measures and all three cutoff dates. Notably, where statistical significance does appear in the placebo models (such as the negative coefficients on logged spending and spending deviation) the direction of the effect is opposite to that found in the main analysis, indicating that higher spending in non-pandemic periods was if anything associated with lower corruption risks rather than higher ones. This pattern of reversed or null effects across all placebo windows provides strong evidence that the positive spending-corruption risk relationship identified in Table 2 is specific to the COVID-19 pandemic period and is not a statistical artifact of general temporal trends or model specification. The absence of any consistent pre-pandemic relationship between spending and CRI across three independent placebo windows lends additional credibility to the main findings, suggesting that the observed association between spending and corruption risks is more plausibly connected to the extraordinary procurement conditions of the pandemic than to pre-existing structural factors.

To assess whether the interaction between pre-pandemic risk levels and spending deviations observed in the main analysis (Table A10) is specific to the COVID-19 period, we replicate the interaction models using three placebo cutoff dates (February 2017, 2018, and 2019) applying the full observation period for each. The placebo results do not reproduce the pattern found in the main analysis. Most notably, the defining feature of the main findings, that medium pre-pandemic risk countries show a pronounced and significant positive relationship between spending

TABLE A20 | Interaction of pre-pandemic risk levels and COVID-19 spending deviations on contract-level CRI, using placebo cutoff dates.

	DV: COVID products CRI		
	Cutoff date: Feb 2017	**Cutoff date: Feb 2018**	**Cutoff date: Feb 2019**
	Model 1	**Model 2**	**Model 3**
Medium pre-cutoff CRI level	−0.216*** (0.026)	−0.034 (0.032)	−0.110*** (0.031)
High pre-cutoff CRI level	−0.065* (0.029)	−0.352*** (0.035)	0.214*** (0.025)
Spending deviation log-transformed	0.009* (0.005)	0.003 (0.003)	0.007* (0.003)
Medium pre-cutoff CRI × spending deviation log-transformed	−0.022*** (0.005)	−0.003 (0.004)	0.011*** (0.005)
High pre-cutoff CRI × spending deviation log transformed	−0.011+ (0.006)	0.089*** (0.008)	−0.010 (0.014)
GDP (log)	Yes	Yes	Yes
Liberal democracy index	Yes	Yes	Yes
Country FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
R^2	0.186	0.205	0.204
Adjusted R^2	0.181	0.199	0.199
RMSE	0.130	0.128	0.129
Observations	15,295	13,457	11,441

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

deviation and CRI, is largely absent across the placebo cutoffs. The interaction term for medium-risk countries is negative and significant at the February 2017 cutoff, negative and insignificant at February 2018, and while it turns positive and significant at February 2019, the estimated coefficient ($\beta=0.011$) is notably smaller than the effect observed in the true pandemic model ($\beta=0.043$–0.044) and should be interpreted with caution given its proximity to the true cutoff and including true pandemic time periods in the effect. The interaction patterns for high-risk countries are similarly inconsistent across placebo windows, with coefficients shifting in sign and magnitude, suggesting these are not stable structural relationships. Taken together, the absence of a consistent and comparably sized medium-risk interaction effect across placebo periods suggests that the vulnerability of moderately governed countries to pandemic-induced procurement risks is more plausibly tied to the specific conditions of the COVID-19 period than to pre-existing or generalized trends.

To evaluate whether the spillover patterns identified in Table 4 are specific to the COVID-19 period, we replicate the spillover models using placebo cutoff dates of February 2017, 2018, and 2019. Several features of the placebo results support the validity of the main findings. A positive and significant time effect in the healthcare market is observed at the 2017 and 2018 cutoffs but loses significance by 2019, and the baseline association between COVID-19 product CRI and healthcare CRI is positive and significant before all placebo windows, as would be expected given that these markets are structurally related regardless of the pandemic. Crucially, however, the defining interactions from the main analysis do not replicate in the placebo period. The interaction

TABLE A21 | Spillover effects of healthcare and COVID-19 procurement risk on adjacent markets corruption risk, using placebo cutoff dates.

	DV: CRI					
	Cutoff date: Feb 2017		Cutoff date: Feb 2018		Cutoff date: Feb 2019	
	Health CRI Model 1	Other CRI Model 2	Health CRI Model 1	Other CRI Model 2	Health CRI Model 1	Other CRI Model 2
POST-cutoff	0.077*** (0.008)	0.000 (0.003)	0.039*** (0.005)	−0.001 (0.002)	0.006 (0.004)	0.010*** (0.002)
CRI COVID	0.125*** (0.016)		0.029*** (0.008)		0.031*** (0.009)	
POST-cutoff × CRI COVID	−0.143*** (0.017)		−0.009 (0.011)		0.015 (0.011)	
CRI healthcare		0.123*** (0.008)		0.054*** (0.006)		0.077*** (0.005)
POST-cutoff × CRI healthcare		−0.003*** (0.008)		0.029*** (0.007)		−0.053 (0.006)
Spending COVID (log)	Yes	Yes	Yes	Yes	Yes	Yes
Spending healthcare (log)	Yes	Yes	Yes	Yes	Yes	Yes
Spending other (log)	Yes	Yes	Yes	Yes	Yes	Yes
GDP (log)	Yes	Yes	Yes	Yes	Yes	Yes
Liberal democracy index	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.196	0.141	0.176	0.143	0.178	0.137
Adjusted R^2	0.197	0.141	0.176	0.143	0.177	0.137
RMSE	0.145	0.144	0.138	0.140	0.127	0.139
Observations	54,255	237,469	75,997	347,555	79,425	377,535

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

between the post-cutoff indicator and COVID-19 CRI (central argument to Hypothesis 2a) is negative and significant at the 2017 cutoff, and insignificant at both 2018 and 2019, indicating that the amplification of spillovers from COVID-19 procurement into the healthcare market following the true pandemic onset is not reproduced under arbitrary cutoffs. Similarly, for Hypothesis 2b, the interaction between healthcare CRI and the post-cutoff period yields only one significant result across the three placebo models, at the February 2018 cutoff, with no consistent pattern otherwise. Taken together, the placebo results suggest that the systematic strengthening of cross-market spillovers observed in the main analysis is more plausibly associated with the specific conditions of the COVID-19 pandemic than with pre-existing structural relationships between procurement markets.

Robustness: Effect Decomposition by Individual Red Flags

The following section further explores the main effects reported in the paper by disaggregating the composite Corruption Risk Index (CRI) into its constituent red flags. Rather than relying solely on the composite measure, we replicate the core regression models for Hypothesis 1a, Hypothesis 2a, and Hypothesis 2b using each individual red flag as the dependent variable. This allows us to assess which specific procurement behaviors are driving the aggregate patterns observed and to address concerns about whether the CRI retains its interpretive validity under emergency regulatory conditions.

Hypothesis 1a: Spending and Individual Red Flags

The results in Tables A22–A29 largely corroborate the main findings. We run the same specification in Table 2 using each individual red flag as the main dependent variable. Higher pandemic-related spending is positively and significantly associated with increased procedure type risk, buyer spending concentration, advertisement period risk, call for tender publication risk, and Benford's Law risk, which is consistent with the composite CRI results reported in Table 2. Two red flags move in the opposite direction: single bidding risk and decision period risk are negatively associated with spending, suggesting that as procurement volumes grew, contracts were not systematically more likely to attract only one bidder, and decision periods did not increase in risk. The tax haven risk results, while directionally consistent, do not reach statistical significance and are based on a substantially smaller number of observations, as this analysis is restricted to contracts awarded to foreign suppliers and should therefore be interpreted with caution.

TABLE A22 | Effects of additional spending on procedure type risk of COVID-19 products, during the COVID-19 pandemic 2020–2023.

DV: COVID products procedure type risk				
	Model 1	**Model 2**	**Model 3**	**Model 4**
Spending (log)	0.005*** (0.001)			
Spending monthly growth		−0.002** (0.001)		
Spending deviation			0.020*** (0.002)	0.020*** (0.002)
GDP (log)	Yes	Yes	Yes	Yes
LDI	Yes	Yes	Yes	Yes
Population vaccinated (%)	No	No	No	Yes
Number of cases (log)	No	No	No	Yes
Country FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
R^2	0.262	0.202	0.276	0.281
Adjusted R^2	0.256	0.197	0.270	0.276
RMSE	0.098	0.081	0.099	0.099
Observations	10,097	9292	9630	9630

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

TABLE A23 | Effects of additional spending on buyer spending concentration risk of COVID-19 products during the COVID-19 pandemic 2020–2023.

DV: COVID products buyer spending concentration				
	Model 1	**Model 2**	**Model 3**	**Model 4**
Spending (log)	0.010*** (0.001)			
Spending monthly growth		0.002** (0.001)		
Spending deviation			0.003 (0.002)	0.004 (0.002)
GDP (log)	Yes	Yes	Yes	Yes
LDI	Yes	Yes	Yes	Yes
Population vaccinated (%)	No	No	No	Yes
Number of cases (log)	No	No	No	Yes
Country FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
R^2	0.172	0.067	0.066	0.066
Adjusted R^2	0.164	0.059	0.057	0.057
RMSE	0.114	0.106	0.107	0.107
Observations	7998	7381	7663	7663

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

TABLE A24 | Effects of additional spending on single bidding risk of COVID-19 products during the COVID-19 pandemic 2020–2023.

DV: COVID products single bidding risk				
	Model 1	**Model 2**	**Model 3**	**Model 4**
Spending (log)	−0.015*** (0.003)			
Spending monthly growth		0.000 (0.003)		
Spending deviation			−0.021** (0.007)	−0.022** (0.007)
GDP (log)	Yes	Yes	Yes	Yes
LDI	Yes	Yes	Yes	Yes
Population vaccinated (%)	No	No	No	Yes
Number of cases (log)	No	No	No	Yes
Country FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
R^2	0.194	0.198	0.198	0.195
Adjusted R^2	0.188	0.193	0.188	0.189
RMSE	0.363	0.364	0.364	0.364
Observations	10,115	9295	9651	9651

Note: Standard errors in parentheses; significance stars: $+p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

TABLE A25 | Effects of additional spending on advertisement period risk of COVID-19 products, during the COVID-19 pandemic 2020–2023.

DV: COVID products advertisement period risk				
	Model 1	**Model 2**	**Model 3**	**Model 4**
Spending (log)	0.014*** (0.004)			
Spending monthly growth		0.011*** (0.003)		
Spending deviation			0.019* (0.008)	0.016* (0.008)
GDP (log)	Yes	Yes	Yes	Yes
LDI	Yes	Yes	Yes	Yes
Population vaccinated (%)	No	No	No	Yes
Number of cases (log)	No	No	No	Yes
Country FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
R^2	0.324	0.328	0.320	0.322
Adjusted R^2	0.319	0.323	0.315	0.316
RMSE	0.384	0.380	0.385	0.384
Observations	9224	8439	8822	8822

Note: Standard errors in parentheses; significance stars: $+p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

TABLE A26 | Effects of additional spending on decision period risk of COVID-19 products, during the COVID-19 pandemic 2020–2023.

DV: COVID products decision period risk				
	Model 1	**Model 2**	**Model 3**	**Model 4**
Spending (log)	−0.010*** (0.003)			
Spending monthly growth		0.001 (0.002)		
Spending deviation			−0.010+ (0.005)	−0.010+ (0.005)
GDP (log)	Yes	Yes	Yes	Yes
LDI	Yes	Yes	Yes	Yes
Population vaccinated (%)	No	No	No	Yes
Number of cases (log)	No	No	No	Yes
Country FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
R^2	0.235	0.239	0.236	0.237
Adjusted R^2	0.229	0.233	0.229	0.231
RMSE	0.258	0.255	0.260	0.260
Observations	8807	8122	8416	8416

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $p<0.05$, $**p<0.01$, $***p<0.001$.

TABLE A27 | Effects of additional spending on call for tender publication risk of COVID-19 products during the COVID-19 pandemic 2020–2023.

DV: COVID products call for tender publication risk				
	Model 1	**Model 2**	**Model 3**	**Model 4**
Spending (log)	0.001+ (0.000)			
Spending monthly growth		0.001*** (0.000)		
Spending deviation			0.001 (0.001)	0.001 (0.001)
GDP (log)	Yes	Yes	Yes	Yes
LDI	Yes	Yes	Yes	Yes
Population vaccinated (%)	No	No	No	Yes
Number of cases (log)	No	No	No	Yes
Country FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
R^2	0.209	0.239	0.210	0.210
Adjusted R^2	0.201	0.230	0.201	0.201
RMSE	0.030	0.029	0.030	0.030
Observations	6234	5560	6006	6006

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

TABLE A28 | Effects of additional spending on tax haven risk of COVID-19 products during the COVID-19 pandemic 2020–2023.

	DV: COVID products tax haven risk			
	Model 1	**Model 2**	**Model 3**	**Model 4**
Spending (log)	0.024 (0.018)			
Spending monthly growth		0.003 (0.018)		
Spending deviation			0.055 (0.040)	0.050 (0.039)
GDP (log)	Yes	Yes	Yes	Yes
LDI	Yes	Yes	Yes	Yes
Population vaccinated (%)	No	No	No	Yes
Number of cases (log)	No	No	No	Yes
Country FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
R^2	0.398	0.476	0.411	0.437
Adjusted R^2	0.218	0.291	0.215	0.243
RMSE	0.245	0.240	0.249	0.244
Observations	275	231	250	250

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

TABLE A29 | Effects of additional spending on Benford's law risk of COVID-19 products during the COVID-19 pandemic 2020–2023.

	DV: COVID products Benford's law risk			
	Model 1	**Model 2**	**Model 3**	**Model 4**
Spending (log)	0.012+ (0.006)			
Spending monthly growth		0.006 (0.005)		
Spending deviation			0.032* (0.012)	0.031* (0.012)
GDP (log)	Yes	Yes	Yes	Yes
LDI	Yes	Yes	Yes	Yes
Population vaccinated (%)	No	No	No	Yes
Number of cases (log)	No	No	No	Yes
Country FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
R^2	0.301	0.280	0.298	0.301
Adjusted R^2	0.291	0.269	0.288	0.290
RMSE	0.397	0.402	0.397	0.397
Observations	4241	4007	4087	4087

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

Hypothesis 2a and Hypothesis 2b: Spillover Mechanism and Individual Red Flags

To further unpack the spillover mechanisms identified in the main analysis, we replicate the Table A13 buyer-month level spillover models (Tables A30–A37), using each individual red flag as the dependent variable. On the one hand, these models allow for distinguishing between procedural red flags that may reflect legitimate emergency adaptations and more behavioral indicators suggestive of underlying corruption dynamics. On the other hand, they also allow for tracking which exact behaviors were propagated across markets underpinning overall risk spillovers. For limited spillover (Hypothesis 2a), the results show that single bidding and procedure type risk are the primary drivers, with positive and significant interaction terms, suggesting that emergency procurement practices, in particular the use of non-competitive procedures, propagated from COVID-19 markets into broader healthcare procurement in large buyers. Decision period and buyer concentration move in a positive direction but do not reach significance, while advertisement period, CFT publication, and Benford's Law show negative or null interactions, indicating these dimensions did not systematically spill over. For full spillover (Hypothesis 2b), advertisement period and decision period risks emerge as significant positive drivers of propagation into general procurement, while procedure type risk shows a significant negative interaction. These suggest that by the time risks diffused into non-healthcare markets, the mechanism shifted away from procedure-based risks toward more discretionary and less formalistic dimensions. Tax haven and Benford's Law are positive but insignificant across both spillover models. Given the substantially smaller sample sizes for tax haven (restricted to foreign suppliers), these results should be interpreted cautiously. Taken together, the red flag decomposition suggests that the limited spillover from COVID-19 to healthcare procurement was primarily carried through procedural shortcuts, while the broader diffusion into general markets operated through different channels, pointing to a gradual normalization of emergency behaviors rather than a simple replication of the original procurement shock.

TABLE A30 | Spillover mechanisms across the pandemic: Limited (Hypothesis 2a) and full (Hypothesis 2b) spillovers at the buyer-month level, 2016–2023—Single bidding.

DV: Single bidding	Limited spillover (healthcare)	Full spillover (general procurement)
Sample	Large buyers	Large buyers
Post-COVID	0.113** (0.041)	0.101*** (0.014)
COVID products single bidding (monthly)	−0.008 (0.015)	
COVID products single bidding × POST COVID	0.059** (0.019)	
Healthcare products single bidding (monthly)		0.026** (0.009)
Healthcare products single bidding × POST COVID		−0.007 (0.009)
Spending COVID-products	Yes	No
Spending healthcare-products	Yes	Yes
Spending general procurement	No	Yes
GDP (log)	Yes	Yes
Liberal democracy index	Yes	Yes
Buyer type	Yes	Yes
Buyer size	Yes	Yes
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.339	0.053
Adjusted R^2	0.336	0.052
RMSE	0.360	0.422
Observations	22,153	222,924

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

TABLE A31 | Spillover mechanisms across the pandemic: Limited (Hypothesis 2a) and full (Hypothesis 2b) spillovers at the buyer-month level, 2016–2023—Procedure type.

DV: Procedure type	Limited spillover (healthcare)	Full spillover (general procurement)
Sample	Large buyers	Large buyers
Post-COVID	0.009 (0.006)	−0.017*** (0.004)
COVID products procedure type (monthly)	−0.011 (0.015)	
COVID products procedure type × POST COVID	0.029+ (0.018)	
Healthcare products procedure type (monthly)		0.050*** (0.009)
Healthcare products procedure type × POST COVID		−0.048*** (0.011)
Spending COVID-products	Yes	No
Spending healthcare-products	Yes	Yes
Spending general procurement	No	Yes
GDP (log)	Yes	Yes
Liberal democracy index	Yes	Yes
Buyer type	Yes	Yes
Buyer size	Yes	Yes
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.148	0.167
Adjusted R^2	0.143	0.166
RMSE	0.050	0.110
Observations	22,097	222,728

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

TABLE A32 | Spillover mechanisms across the pandemic: Limited (Hypothesis 2a) and full (Hypothesis 2b) spillovers at the buyer-month level, 2016–2023—Advertisement period.

DV: Advertisement period	Limited spillover (healthcare)	Full spillover (general procurement)
Sample	Large buyers	Large buyers
Post-COVID	0.175*** (0.048)	0.026* (0.012)
COVID products advertisement period (monthly)	0.203*** (0.016)	
COVID products advertisement period × POST COVID	−0.026 (0.019)	
Healthcare products advertisement period (monthly)		0.405*** (0.005)
Healthcare products advertisement period × POST COVID		0.017*** (0.005)
Spending COVID-products	Yes	No
Spending healthcare-products	Yes	Yes
Spending general procurement	No	Yes
GDP (log)	Yes	Yes
Liberal democracy index	Yes	Yes
Buyer type	Yes	Yes
Buyer size	Yes	Yes
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.309	0.439
Adjusted R^2	0.304	0.439
RMSE	0.380	0.323
Observations	19,932	198,482

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

TABLE A33 | Spillover mechanisms across the pandemic: Limited (Hypothesis 2a) and full (Hypothesis 2b) spillovers at the buyer-month level, 2016–2023—Decision period.

DV: Decision period	Limited spillover (healthcare)	Full spillover (general procurement)
Sample	Large buyers	Large buyers
Post-COVID	0.081* (0.034)	0.060*** (0.009)
COVID products decision period (monthly)	0.001 (0.019)	
COVID products decision period × POST COVID	0.035 (0.024)	
Healthcare products decision period (monthly)		0.061*** (0.007)
Healthcare products decision period × POST COVID		0.024*** (0.008)
Spending COVID-products	Yes	No
Spending healthcare-products	Yes	Yes
Spending general procurement	No	Yes
GDP (log)	Yes	Yes
Liberal democracy index	Yes	Yes
Buyer type	Yes	Yes
Buyer size	Yes	Yes
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.297	0.253
Adjusted R^2	0.293	0.252
RMSE	0.288	0.266
Observations	20,548	214,344

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

TABLE A34 | Spillover mechanisms across the pandemic: Limited (Hypothesis 2a) and full (Hypothesis 2b) spillovers at the buyer-month level, 2016–2023—Call for tender publication.

DV: CFT publication	Limited spillover (healthcare)	Full spillover (general procurement)
Sample	Large buyers	Large buyers
Post-COVID	−0.010+ (0.006)	0.002 (0.003)
COVID products CFT publication (monthly)	0.043*** (0.010)	
COVID products CFT publication × POST COVID	−0.197*** (0.018)	
Healthcare products CFT publication (monthly)		−0.022*** (0.004)
Healthcare products CFT publication × POST COVID		−0.012 (0.012)
Spending COVID-products	Yes	No
Spending healthcare-products	Yes	Yes
Spending general procurement	No	Yes
GDP (log)	Yes	Yes
Liberal democracy index	Yes	Yes
Buyer type	Yes	Yes
Buyer size	Yes	Yes
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.197	0.126
Adjusted R^2	0.189	0.125
RMSE	0.036	0.064
Observations	11,070	102,401

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

TABLE A35 | Spillover mechanisms across the pandemic: Limited (Hypothesis 2a) and Full (Hypothesis 2b) spillovers at the buyer-month level, 2016–2023—Tax haven.

DV: Tax haven	Limited spillover (healthcare)	Full spillover (general procurement)
Sample	Large buyers	Large buyers
Post-COVID	−0.027 (0.132)	0.187*** (0.054)
COVID products tax haven (monthly)	−0.006 (0.078)	
COVID products tax haven × POST COVID	0.046 (0.096)	
Healthcare products tax haven (monthly)		−0.010*** (0.024)
Healthcare products tax haven × POST COVID		0.039 (0.032)
Spending COVID-products	Yes	No
Spending healthcare-products	Yes	Yes
Spending general procurement	No	Yes
GDP (log)	Yes	Yes
Liberal democracy index	Yes	Yes
Buyer type	Yes	Yes
Buyer size	Yes	Yes
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.142	0.067
Adjusted R^2	0.093	0.056
RMSE	0.304	0.317
Observations	2167	10,107

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

TABLE A36 | Spillover mechanisms across the pandemic: Limited (Hypothesis 2a) and full (Hypothesis 2b) spillovers at the buyer-month level, 2016–2023—Buyer concentration.

DV: Buyer concentration	Limited spillover (healthcare)	Full spillover (general procurement)
Sample	Large buyers	Large buyers
Post-COVID	−0.023* (0.011)	−0.011+ (0.007)
COVID products buyer concentration (monthly)	−0.015 (0.012)	
COVID products buyer concentration × POST COVID	0.018 (0.019)	
Healthcare products buyer concentration (monthly)		0.037** (0.012)
Healthcare products buyer concentration × POST COVID		−0.041** (0.015)
Spending COVID-products	Yes	No
Spending healthcare-products	Yes	Yes
Spending general procurement	No	Yes
GDP (log)	Yes	Yes
Liberal democracy index	Yes	Yes
Buyer type	Yes	Yes
Buyer size	Yes	Yes
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.052	0.036
Adjusted R^2	0.046	0.035
RMSE	0.087	0.180
Observations	20,127	188,295

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.

TABLE A37 | Spillover mechanisms across the pandemic: Limited (Hypothesis 2a) and full (Hypothesis 2b) spillovers at the buyer-month level, 2016–2023—Benford's law.

DV: Benford's law	Limited spillover (healthcare)	Full spillover (general procurement)
Sample	Large buyers	Large buyers
Post-COVID	0.016 (0.054)	0.003 (0.026)
COVID products Benford's law (monthly)	0.030 (0.018)	
COVID products Benford's law × POST COVID	−0.026 (0.023)	
Healthcare products Benford's law (monthly)		0.000 (0.011)
Healthcare products Benford's law × POST COVID		0.005 (0.012)
Spending COVID-products	Yes	No
Spending healthcare-products	Yes	Yes
Spending general procurement	No	Yes
GDP (log)	Yes	Yes
Liberal democracy index	Yes	Yes
Buyer type	Yes	Yes
Buyer size	Yes	Yes
Country FE	Yes	Yes
Month FE	Yes	Yes
R^2	0.365	0.231
Adjusted R^2	0.360	0.230
RMSE	0.341	0.413
Observations	13,216	66,992

Note: Standard errors in parentheses; significance stars: $+p<0.1$, $*p<0.05$, $**p<0.01$, $***p<0.001$.