Regulation & Governance

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ORIGINAL ARTICLE OPEN ACCESS

The Impact of Emergencies on Corruption Risks: Italian Natural Disasters and Public Procurement

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Received: 30 August 2023 | **Revised:** 18 December 2024 | **Accepted:** 20 December 2024

Funding: The authors received no specific funding for this work.

Keywords: corruption | disaster | emergency | Italy | public procurement

ABSTRACT

Theory and case studies suggest that emergencies and disasters increase corruption, especially in public procurement, hampering relief and reconstruction efforts. Despite a growing interest in the topic, including in research, there is still little systematic evidence about these effects, their structure and trajectories. We set out to investigate the medium-term impact of disasters on corruption risks, using large-scale administrative data on public tenders in Italy from 2007 to 2020, combined with data on 5 natural disasters. We employ logistic regression, coarsened exact matching and difference-in-differences estimators. We find that disasters increase corruption risks in the medium-term (3 or more years after the disaster), even more than on the short term (1 year after the disaster). In the matched and diff-in-diff analyses, we find 3%–10% points more non-open procedures used, 19%–21% points fewer call for tenders published, 19%–29% points more tenders with short advertisement period and 14%–17% points more single bidding tenders. Our findings highlight the importance of ring-fencing corruption risks associated with disaster response, especially in the medium to long term.

1 | Introduction

Public procurement constitutes a major part of government spending around the world. In the EU, it represented 29% of general government expenditure in 2019.[1] To ensure that public interests are well served during various purchases of goods and services, principles and safeguards of "value for money" are core elements of public procurement regulations and implementation. Hence, public procurement is an extensively regulated area of public spending with strict limitations of discretionary authority and expectations of transparency and procedural fairness (Ladi and Tsarouhas 2017; Križić 2021). For example, transparent tendering conditions are expected and the opportunity to bid is made open to all competent companies.

However, the attractiveness of large sums and the complexity of contracts motivate the corrupt to steal. Officials who intend to illicitly collect extra income can abuse their discretionary powers and bend formal rules to award contracts at inflated prices to companies that offer bribes or are related in other ways (Gong and Zhou 2015), for example, because they belong to friends, family members, or well-connected politicians (Rose-Ackerman 1975; Aidt 2003). At the same time, the discretionary authority that allows some public officials to allocate contracts in exchange for private benefits is valuable if used by clean and competent bureaucrats, as it ensures flexibility and reduces transaction costs.[2] Corruption in public procurement means society pays inflated prices and/or receives poor-quality goods and services. If serious, it may have significant consequences, including environmental damage and hampered economic development (Rose-Ackerman and Palifka 2016).

The usual decision-making procedures and hence corruption controls fundamentally change when a natural disaster hits a society (Sommer, Parent, and Li 2024). Disasters pose serious challenges due to the immediate loss of life and property. Disaster response tends to imply substantial amounts of public spending in a short window of time. Essential goods and services, such as medicines and food, need to be procured immediately for the sake of mitigating damages and for commencing the subsequent recovery efforts. Following standard (non-emergency) procurement rules, which favor open competition and value-for-money, may result in unacceptable delays in delivering much-needed relief to disaster-affected areas. Therefore, procurement laws usually allow for the relaxation of some procedures and requirements during emergencies. These emergency clauses grant broad discretionary powers to public officials to deviate from otherwise strict procurement regulations and apply fast-track processes to fulfill urgent local needs.

Although there is a well-founded need to have emergency clauses in procurement regulations, they may lead to a higher risk of corruption. Along with greater discretionary powers embedded in emergency clauses, affected areas also attract a large amount of funds for disaster-relief. These funds are exposed to officials in charge who may seek to enrich themselves. In a system already affected by corruption, the occurrence of disasters may further heighten corruption risks (Atkinson and Sapat 2012). Those benefiting from corruption can also seek to prolong the emergency situation. Indeed, as Schultz and Søreide (2008) highlight, a lack of consensus on what constitutes an emergency and how long it should last may motivate officials to continue to abuse the discretionary powers given by emergency clauses for illegal rent-seeking.

However, it is far from obvious that these emergency-induced corruption risks materialize. Officials may refuse to distort decisions in exchange for bribes because of the importance of saving lives. Their decisions are also not without oversight and corrupt acts may be brought to light through ex post investigations by auditors, aid donors,[3] or other branches of government. Finally, large disaster response spending often comes with far greater than usual public attention on spending decisions and results, making it harder to conceal corruption. Moreover, if the extent of corruption increases during an emergency, it is unclear whether the problem decreases once circumstances are back to normal or continues after the disaster.

This article reflects on these theoretical and empirical challenges and investigates the short- to medium-term impacts of natural disasters on corruption risks in public procurement. It employs innovative data and measurement and a variety of empirical methods. We combine the de facto data on five natural disasters in Italy with administrative data on public procurement tenders and conduct a contract-level analysis. Several indicators from different stages of the procurement process are used as proxies for corruption risks: (a) non-open procedure types, (b) non-publication of tender calls on-line, (c) too-short advertisement periods, and (d) one bid submitted on a tender (i.e., single bidding). To understand how disasters affect these dependent variables, we carry out regression and coarsened exact matching on the before-after population of contracts in the disaster-affected areas and a difference-in-differences comparison of affected and unaffected areas. The nature of the treatment (i.e., random timing of natural disasters) and the diversity of the methods we apply mean we can closely approximate causal effects. In addition to the strict exogeneity of the disasters, our within affected area before-after comparison holds institutional factors constant. Our matching estimators additionally make sure that the observed effects are not simply the result of a different composition of spending (e.g., more spending on food and repairs once the disaster hit). Finally, our difference-in-differences estimator also takes into account time trends that would have impacted our dependent variables even in the absence of the treatment.

We selected Italy as a case for several reasons: first, the Italian government has taken several steps to increase efficiency in the procurement process, such as the inclusion of the public procurement authority into the anti-corruption agency and the introduction of laws which harmonize procurement with EU standards. Second, the availability of a rich micro-level administrative dataset allows us to directly investigate our research question. Third, the natural disasters that have affected Italy in the last decade or so have all been local in nature, implying that the overall institutional framework for public procurement and corruption control has remained unchanged. The existence of a capable and well-resourced central government with an extensive array of anti- corruption institutions means that Italy should be able to limit the impact of local natural disasters on corruption risks, in spite of its general corruption problems.

We find that disasters increase corruption risks in the medium term (3 or more years after the disaster), even more than in the short term (1 year after the disaster). In the matched and diff-in-diff analyses we find that disasters increase corruption risks on the mid-term: 3%–10% points more non- open procedures used, 19%–21% points fewer call for tenders published, 19-29% points more tenders with short advertisement period, and 14-17% points more single bidding tenders; even though the diff-in-diff estimates are insignificant for single bidding and non-open procedures. Comparing short-term (1 year) and mid-term (3 years or more) disaster impacts, we see a consistent climbing of risks after the disaster, rather than a drop after the initial crisis ebbs. For example, the use of non-open procedures increases by 6% points in 1 year after the disaster (statistically insignificant) while the increase is 10% points in the 3 or longer time window (statistically significant).

Our results contribute to the literature in multiple ways. We confirm our theoretical predictions as natural disasters lead to sizeable increases in a range of public procurement risk factors. Looking at the medium term, emergencies seem to have persistently increased the local risk of corruption even in Italy, a country with strong central government and anti-corruption institutions. This is important because, while an immediate increase in risk is more understandable, the persistence of heightened risks may be due to a shift in the equilibrium from low corruption to high corruption. A natural disaster can act as a temporary shock with persistent effects and cause changes which do not necessarily disappear even long after the disaster situation has subsided. This is consistent with the understanding of corruption as a self-reinforcing process, that is, corruption corrupts (Andvig and Moene 1990; Bardhan 1997), whereby disasters push localities from a low-corruption equilibrium to a high-corruption equilibrium. This could be due to a decrease in the moral cost of corruption (i.e., shifting social norms) or because a temporary increase in corruption creates tight-knit networks of the corrupt which in turn makes it easier to engage in corruption in the longer term (i.e., shifting power balance).

The rest of the article is organized as follows: first, drawing results in the literature, we outline our theoretical framework and put forward our hypotheses. Second, we describe our data, indicators and empirical strategy. Third, we show and discuss our results. Finally, we conclude by drawing policy lessons and assessing the limitations of our analysis.

2 | Theory and Empirical Expectations

The problem of corruption in public procurement, specifically, arises when public contracts are awarded and implemented by bending universalistic rules of open and fair access to government contracts to favor a certain bidder while denying access to others (Fazekas and Kocsis 2020). The bidder is often part of a closed network of allies that collude to secure illegitimate benefits. The exclusion of those without the right connections (e.g., family ties, friendship), or those unwilling to offer bribes, lies at the heart of corruption (North, Wallis, and Weingast 2009). Public procurement, being the context in which the problem plays out, is an area of government spending that requires considerable discretionary authority to function efficiently and ensure value for public money (Coviello, Guglielmo, and Spagnolo 2018; Decarolis et al. 2020). Such discretion can be misused for private benefit, not only by the public officials involved but also by their administrative leaders or elected officials. Corrupt enrichment in this context results from inflated prices for a given quality and quantity and/or from compromising on quality and quantity. In order to balance the need for discretion with the risks of abusing it, governments have devised a series of corruption controls which are widely used across the world, including in Italy. Procurement tenders are highly regulated administrative procedures where open competition is expected to be the norm, meaning there is at least a few qualified bidders competing against each other. Effective competition depends on transparent and open tendering procedures which are safeguarded, for example, by publishing an open call for bidders to submit bids and allowing a wide set of companies to participate in the competition. Furthermore, allowing companies to prepare and submit bids in a sufficient amount of time is required to further support fair and intensive competition for government contracts (Piga 2011).

When natural disasters hit and emergencies arise, the standard balance of incentives for and controls of corruption fundamentally changes (Schultz and Søreide 2008). Delivering specific goods and services quickly, in often difficult conditions (e.g., food to people stuck in an earthquake zone), and of quantities multiple times larger than usual (e.g., buying masks when a pandemic threatens to overwhelm hospitals) become imperative. Hence, many of the standard safeguards of corruption control requiring time, multiple checks, and careful consideration become impractical for good reasons, such as saving lives (Thomann, Marconi, and Zhelyazkova 2023). To accommodate the extraordinary conditions arising from emergencies, public procurement regulations foresee emergency clauses allowing for quicker, simpler, and less controlled spending. With diminishing controls, discretion increases greatly, as does the risk of corruption.

However, emergency-induced corruption risks may not materialize. Officials may refuse to distort decisions in exchange for bribes because of the importance of saving lives. Their decisions are also not without oversight, and corrupt acts may be brought to light through ex post investigations by auditors, aid donors, or other branches of government. Finally, large disaster response spending often comes with far greater than usual public attention on spending decisions and results, making it harder to conceal corruption.

While the theoretical arguments are well-articulated, both for and against intensified problems of corruption in emergency response procurement, there are few studies of the degree, trajectory and enabling conditions of corruption in disaster responses. However, the concerns motivating this study, are supported among others by Nikolova and Marinov (2017), who investigate flood-related transfers to municipalities and associated spending infringements in Bulgaria after torrential rains in 2004 and 2005. Using publicly available audit reports, they found that a 10% increase in the per capita amount of disbursed funds led to a 9.8% increase in corruption in the disaster-affected areas. The ex post disaster persistence of the problem, that we are concerned about, is not part of their study. Long-term effects are included in a cross-country study by Wenzel (2021). She investigates the influence of droughts on public sector corruption in 120 countries during the period 1985–2013, and confirms that, in general, more severe drought exposure is followed by more corruption. Exploring the likely causes of the problem, she points at how drought in developing countries triggers significantly larger aid inflows, which she finds are exposed to corruption due to weak democratic accountability, weaker law and order, and low transparency. With respect to developed countries, she finds higher estimated corruption as a consequence of governmental drought relief payments, and hence, the increased revenue appears to cause corruption-related challenges despite wealthier countries' better institutions. Variations between drought-affected areas and non-affected areas within countries are not part of her analysis, however, and therefore, it is not possible to learn from that study whether the extent of corruption evolves differently in crisis-affected areas compared to other parts of a country. In a study of natural disasters in Honduras, Birch and Mart'ınez i Coma (2023), find extreme weather conditions intensify the extent of political corruption at the local level, in this context described as *clientelism*. With the help of a survey and interviews, they explain disaster relief seems to nurture corrupt networks. However, they also argue that "multiple severe disasters may overwhelm clientelist networks," as the combination of flooding and a challenging COVID-situation seemed to make it difficult for politicians to exchange benefits with their network of allies for personal and electoral benefits. Given the methodology applied, the magnitude and persistence of the effects could not be quantified.

On balance, and considering prior theoretical and empirical material, our first hypothesis is the following:

Hypothesis 1. *Corruption risks of public procurement processes and outcomes increase during the immediate aftermath of a natural disaster compared to non-emergency periods.*

It is up for discussion what the immediate aftermath exactly entails, but given the limited scale of disasters we study, we argue that short-term corruption risk-increasing effects should last for about half to 1 year. While natural disasters and emergencies represent distinct and typically short-term events, they carry the potential to displace the previous low corruption equilibrium and embed a higher corruption equilibrium.

This argument hinges on an assumed dynamic nature of corruption, suggesting that the extent of corruption in society depends on how prevalent the problem is already. Upon the first sophisticated economic analysis of corruption, conducted in the 1970s by Rose-Ackerman (1978), many researchers across several social science disciplines studied the presence of corruption equilibria, also described as corruption "stickiness" or "collective action problems,"—which suggests, the problem depends on how many other members of society are involved (Klitgaard 1988; Bardhan 1997; Marquette and Peiffer 2018; Persson, Rothstein, and Teorell 2013). Andvig and Moene (1990) conducted an early analysis of the phenomenon, describing how "corruption corrupts," pointing out how the extent of the problem contributes to explaining the ease with which an individual or firm finds a corrupt counterpart, the opportunity to bribe oneself out of the problem if detected, and the limited reputational burden if detected (e.g., if corruption is already a problem, those involved will more easily get a new job or contract even if caught in the crime, while in societies with less corruption, this might be a problem). Slingerland (2018) developed a related but different path of argumentation, describing the network features of corruption, specifically explaining and demonstrating empirically how a network of contacts may develop a practice of *tilting* decisions to the benefit of one another, in ways that would not necessarily fit a criminal law definition of corruption, yet in sum, create similar distorting consequences.

This literature suggests there is a risk that a shift in the extent of corruption in a society may intensify the problem over a longer period than the actual presence of the reasons that created the shift, in our case, a natural disaster. In our context, this is facilitated by a number of concurrent mechanisms. First, a disaster and corruption happening in the accompanying relief efforts can create a sense of normality for corrupt behavior. If what used to be an outlier behavior becomes accepted behavior among actors in public procurement, we see a norm shift with corruption embedding as accepted. With normative constraints on corruption shifting, the equilibrium level of corruption increases (Mungiu-Pippidi 2013). Second, disasters can shift a local corruption equilibrium by increasing the resources, hence the power of corrupt actors in comparison to non-corrupt actors. This can happen through large volumes of procurement spending taking place in a short period with relatively little controls, hence leading to large corrupt incomes. When large sums are captured by groups of corrupt actors, interpersonal relationships among the corrupt can grow and hence corrupt networks can arise and solidify, locking a high corruption equilibrium in. Third, the disaster can destroy or weaken the institutional foundations of corruption control by eroding public trust in public institutions that failed to adequately prepare or respond to a disaster. Lower trust in institutions is likely to make citizens and firms less ready to report or protest against corruption in public procurement. Anti-corruption institutions can also be directly weakened by the destruction of physical and human capital which played a key role in controlling corruption prior to the disaster. Finally, the inherent ambiguity of when an emergency ends and the potential corrupt manipulation of the end date allow for an initial short-term relaxation of rules to become long-term or even permanent (Schultz and Søreide 2008). These considerations lead to our second hypothesis:

Hypothesis 2. *The corruption-increasing effect of local natural disasters persists well beyond the actual emergency situation.*

Regarding the critical question of how long the persistent effect may be, we revert to the empirical material, as our theory is not specific enough. The starting point is that corruption equilibrium theories argue in terms of years of equilibrium shifts (sometimes even in decades (Rothstein 2011)), and our time series covers at least 3 years before and after the studied disasters. Hence, we define persistent effects lasting 3 or more years after the natural disaster hits. Such longer-term view also allows for tracking larger and/or more complex contracts, which inevitably take longer to tender and implement (please note that we control for both contract size and product category in our main models, hence bias from contract complexity is a limited problem).

3 | Italy and the Empirical Context

3.1 | De Facto Data on Disasters

The Emergency Events Database (EM-DAT)[4] is maintained by the Centre for Research on the Epidemiology of Disasters (CRED) which has data on 22,000 disasters from all over the world from year 1900. For each disaster, the database provides detailed information about the type of disaster, the start and end dates, the region (up to city level) affected, the origin, and associated disasters[5], the magnitude, the number of people affected (injured and dead), and the damage caused. Since this dataset also contains information on very small-scale disasters with hardly any damage, we selected disasters which are sufficiently large to have an impact on the procurement process and corruption in public spending. Thus, we applied the below selection criteria for disasters.

We selected those disasters from the EM-DAT dataset which have (a) a fixed location on land in Italy, (b) a fixed start and end date, (c) are geophysical or hydrological in nature, (d) caused more than 10 deaths, and (e) occurred between 2008 and 2020.[6] These conditions ensure that we have distinct, exogenous, and important interventions to study, specifically condition (a) and (b) deliver a sharp boundary for the location and timing of the disasters; condition (c) ensures that the disasters are exogenous, that is, strictly non-man-made; and condition (d) limits our investigation to sizable disasters triggering a procurement response, at least locally. We have to limit the set of disasters to the 2008–2020 period as acceptable quality procurement data was only available for 2007–2020 (note that we retained at least 1 year before any disaster to be able to track changes from before to after).

After applying these selection criteria, the following five disasters which lie in mutually exclusive regions remain (the regions where they occurred are depicted in Figure 1):

1. **Floods in Messina Province** which occurred on the 1st of October, 2009. The flood mainly affected Giampilieri, Taormina, Scaletta Zanclea, Molino towns in Sicily and killed a total of 35 people. The floods caused an estimated total damage of US $20 million.
2. **Earthquake in Modena Province** which occurred on the 29th of May, 2012. The earth-quake had a magnitude of 6 on the Richter scale and caused 17 deaths. According to the EM-DAT dataset, the earthquake caused total insured damage of US $1.3 billion. Others estimated the total damage to be around US $4 billion (Dirani 2012).
3. **Floods in Sassari Province** which occurred on the 18th of November, 2013. The floods mainly affected the Olbia and Arzachena cities and caused 18 deaths. The floods caused total damage of US $780 million.
4. **Earthquake in Rieti and Ascoli Piceno Provinces** which occurred on the 24th of August, 2016. The earthquake had a magnitude of 6 on the Richter scale and caused 296 deaths. The earthquake caused total damage worth US $5 billion.
5. **Avalanche in Pescara Province** which occurred on the 18th of January, 2017 and led to 29 deaths due to the destruction of a hotel. The avalanche was triggered by a set of earthquakes in the same area which themselves did not cause any deaths. The total damage caused by the avalanche was US $6 million.

FIGURE 1 | Map of Italy depicting the treated and untreated areas.

3.2 | Italian Public Procurement and Anti-Corruption Policies

Italian public procurement roughly accounts for 10% of the Italian GDP (European Commission 2016) and is carried out under the auspices of the *Autorit'a Nazionale Anticorruzione* (ANAC) which is the Italian anti-corruption body. A further level of oversight is maintained by Italy's Court of Audit.

The Italian government has stipulated strict rules regarding public procurement, including procedures for the tendering process and for awarding contracts. The current regulations evolved in several steps, most importantly the introduction of the first public procurement code in 2006 and a subsequent one in 2016, primarily to align with new European directives.[7] In general, all contracts that have a value of more than 1 million Euros need to go through a full open tendering process. Contracts below this threshold can be awarded through non-open procedures like direct award or a negotiated procedure. In the case of emergencies or in cases where previous open procedures received no bids, the rules allow the use of non-open procedures, even for contracts above the 1 million Euro threshold.

In the past few years, there have been several convictions for procurement-related corruption in Italy. Between 2001 and 2012, 22% (68) of corruption convictions involved procurement-related offenses (ANAC 2012). In the period 2016-2019, 152 cases of corruption in public procurement came to light (ANAC 2019). Several reforms have been implemented to reduce the risk of corruption in general and specifically in public procurement too. For example, a national anti-corruption law was passed in 2012 which mandated the approval of the National Anti-Corruption Plan as well as corresponding regional plans (Span'o et al. 2017). The 2012 Anti-Corruption law also makes it obligatory to make public all data related to awards of public contracts. The formation of the Public Procurement Observatory to track financial flows is another anti-corruption step undertaken by the Italian authorities.

While the evolution in national regulations most likely impacted procurement practices in Italy, the rules apply to *all* regions, including all the regions that are part of this analysis. Naturally, implementation of national procurement and anti-corruption policies will vary to some degree across regions, also including the well-documented, North-South divide (Feldman 2020). This is why, our main analysis is based on comparisons within regions hit by disasters.

3.2.1 | Data on Italian Public Procurement

For the public procurement data, we rely on administrative data collected under the DIGI-WHIST project.[8] The data consists of Italian public contracts between a supplier and a buyer (public authority) published on Tenders Electronic Daily (TED; which is the dedicated portal for European public procurement) from 2007 to 2020. For each contract, the data contains information regarding the procedure followed for awarding the contracts, the location of the buyer (NUTS3 code and city name), the type and name of the buyer, the CPV code, the size of the contract, the number of bidders, dates of the first call, last call and the date of contract award. We drop all the contracts without location information or relevant dates.

All the contracts that lie in the NUTS3 code[9] (or city when NUTS3 code is missing) of an area affected by the five disasters discussed above are included in the analysis. The total number of such contracts is 11,128. In addition, we included all other Italian contracts in our database to be used in matching underpinning the difference-in-differences analysis. Tables 1, 2, and 3, respectively show disaster-wise, year-wise, and sector-wise composition of our sample for both disaster-affected and disaster-unaffected areas.

TABLE 1 | The total number of contracts by disaster from 2007 to 2020.

Group	Number of contracts
Disaster 1	2915
Disaster 2	2366
Disaster 3	2106
Disaster 4	615
Disaster 5	3126
Total	11,128

TABLE 2 | The yearly composition of contracts in disaster-affected areas.

	Number of contracts	Number of contracts
Year	Disaster-affected areas	Disaster-unaffected areas
2007	6	995
2008	254	5867
2009	409	18,567
2010	388	15,122
2011	1402	43,992
2012	731	33,408
2013	1227	34,128
2014	988	29,666
2015	1260	33,996
2016	1171	29,954
2017	1274	38,706
2018	773	40,410
2019	952	39,827
2020	254	17,330
Total	11,128	382,048

3.2.2 | Dependent Variables: Measuring Corruption Risks in Public Procurement

Our approach to measuring the risk of corruption, that is, our dependent variable, is based on our theoretical expectations above as well as recent innovations in the measurement literature (Klaˇsnja 2015; Charron et al. 2017; Decarolis and Giorgiantonio 2020). This literature developed a set of objective indicators of corruption risk in public procurement. This approach has been described as the "red flag approach" where deliberate deviations from open and transparent procurement practices are used as indicators of corruption risk (Fazekas, T'oth, and King 2016; Fazekas and Kocsis 2020). Such indicators aim to directly gauge corruption as limited access to public resources in line with our definition above. Table 4 provides an overview of the dependent variables included in the analysis. All dependent variables are ordered so that a higher number reflects higher corruption risks. Continuous variables such as bidder number and advertisement period length (days), are categorized into high/low-risk categories to minimize false positives and better align them with a corruption risk interpretation.[10]

These corruption risk indicators have been thoroughly validated against proven cases as well as other indicators of corruption (e.g., self-reported corruption experiences). For example, Charron et al. (2017) finds that our corruption risk indicators such as single bidding correlate with both population survey-based corruption perceptions and self-reported bribery across European regions. Similarly, Fazekas and Kocsis (2020) finds strong correlations between public procurement corruption risks and both expert scores (e.g., WGI Control of Corruption indicator) and survey of bidding firms across Europe on the country level. Specifically in Italy, Fazekas, Sberna, and Vannucci (2022) shows that proven cases of local public procurement corruption can be predicted using red flags such as single bidding or non-open procedures.

However, there is only limited evidence on corruption risk indicators' validity in emergency situations specifically. We argue that indicator validity applies to emergency situations too. A frequently put-forward counterargument is that in emergencies such risks are inevitable as public buyers have little time to write open calls and wait weeks for a range of companies to bid. However, this still means that the opportunities for corruption increase in the presence of red flags. Moreover, in localized emergencies, such as the ones we investigate, supply markets of goods, works and services remain intact, meaning that getting competing offers is possible even on accelerated timelines, for example, with the use of existing framework agreements or the purchase of standard products (Schultz and Søreide 2008; Arrowsmith et al. 2021).

4 | Methods

We exploit the exogeneity of the exact timing of hydrological and geophysical disasters to conduct an impact analysis where the treatment is the occurrence of the disaster. There may be certain areas where disasters occur frequently (e.g., in zones of high seismic activity where earthquakes may strike frequently), but since it is difficult to know exactly when an earthquake or flood will occur, the exact timing can be assumed to be randomly assigned.

Secondly, even though the occurrence of floods may be known a few days in advance, through flood or heavy rain forecasts, it is unlikely to influence our results since public procurement operates on much longer time-frames during non-emergency periods. A mid to large-size contract is likely to be awarded after months of preparation and tendering.

TABLE 3 | The total number of contracts by main sector of the product (CPV).

Type of procurement	Number of contracts — Disaster-affected areas	Number of contracts — Disaster-unaffected areas
Medical equipment, pharmaceuticals, and personal care	7180	195,880
Financial and insurance services	722	22,830
Health and social work services	565	13,113
Sewerage, refuse, cleaning, and env. services	551	21,922
Repair and maintenance	222	11,646
Transport services (excluding waste transport)	206	5117
Other	1682	111,540
Total	11,128	382,048

All the contracts that had call-for-tenders dates prior to the disaster are part of the untreated- within group,[11] while all those that had call-for-tenders dates after the disaster are part of the treated-within group. This is because any new procedural rules only apply to tenders that are yet to begin, and not to those tenders which are already underway at the time of the disaster, but lead to contract award after that. In cases where the call-for-tenders dates are missing, we use the publication date of the first contract award announcement.

Despite the exogeneity of disasters, our analysis is not free of confounding factors. Disasters have a range of effects, some of which directly impact corruption behaviors, while others may change environmental conditions, only indirectly related to corruption. In our analysis, we would like to isolate the direct effect of behavioral change due to disasters. This is difficult to do with an unmatched comparison between treated-within and untreated-within contracts. The unmatched comparison will capture both the direct effect of behavioral changes and the indirect effect of changes in spending composition and total volume due to disasters. The composition effect may pose challenges to our identification strategy as different product groups and markets have different inherent corruption risks. Some markets are natural monopolies while others are highly competitive. The total spending effect may also pose an identification challenge as higher total spending on its own may attract more corrupt actors. As a result, disasters are expected to drastically alter procurement spending composition and volumes and hence we could conflate corruption risk changes due to actors taking advantage of the emergency with more spending flowing into inherently higher-risk products such as construction or more spending in general. To control for the

TABLE 4 | Dependent variables and the associated corruption risks.

Indicator name	Corruption risk involved	Interpretation of values
Non-open procedure type	Non-open procedures reduce transparency. These may be used to favor certain suppliers.	*proceduretype* = 1, non-open procedure *proceduretype* = 0, open procedure
Non-publication of tender call	Non publication of tender calls may limit advertisement and consequently restrict the number of firms involved in the bidding phase.	*tendercall* = 1, no call published *tendercall* = 0, call published
Too-short advertisement period	A too-short advertisement period could be because the procedure is skewed in favor of one particular firm. The information about the tender is only available for a short while and it does not give competitors sufficient time to prepare bids.	*advertperiod* = 1, too-short advertisement period *advertperiod* = 0, normal advertisement period
Single bidder	Single bidding could be because the contract is narrowly defined to suit one firm. Alternatively, it could be due to the presence of a bidding cartel where the winner is pre-determined.	*singlebid* = 1, only one bidder *singlebid* = 0, multiple bidders

Note: Non-open procedure types include: restricted, restricted with publication, negotiated without publication, competitive dialog, outright award, negotiated, innovation partnership.

indirect effect of such changes in spending composition and volume, we conduct both a regression analysis and a matched before-and-after treatment analysis controlling for contract value and product group, among others.

For the regression analysis, we run the following logistic regression for each of our binary dependent variables:

$$log_e\left(\frac{pr[DV_i = 1]}{1 - pr[DV_i = 1]}\right) = \beta_0 + \beta_1 \times Disaster_i + \alpha_i + \in_i \quad (1)$$

where DV_i refers to the various dependent variables and $Disaster_i$ is the time dummy which is 0 before the disaster and 1 after the disaster. α_i refers to a set of control variables which account for main product and institutional confounders (this list largely overlaps with the list of matching covariates, see below):

1. Buyer type (national, regional, public body, utilities, or others).
2. Main sector of the product purchased (CPV).
3. Contract value (natural logarithm of amount in euros).
4. Contract month.
5. Contract year.

Note that for all the binary dependent variables, a higher value indicates increased corruption risk.[12]

For the matched before-after analysis, we match contracts in the treatment-within group with those that are in the untreated group to find a suitable control-within group. For this purpose, we use Coarsened Exact Matching (CEM) (Iacus, King, and Porro 2012) at the contract level.[13] According to Iacus, King, and Porro (2009), the procedure does exact matching by coarsening covariates and minimizing covariate imbalance between treatment and control groups.[14] CEM is advantageous since it relies on fewer assumptions about common support than other procedures. We match the contracts from the treated group based on five covariates which include 1, 2, 3, and 4 from the above list of control variables along with the pre-treatment corruption risks of the buyer organization.

An advantage of focusing on disaster-affected areas is that we are able to control for unobserved institutional quality. In particular, analyzing the same areas ensures that the range of buyers is limited. In the matched comparison, however, we do this explicitly by restricting the analysis to only those buyers that are present in the sample both before and after the disaster. The estimates from a within disaster-affected areas analysis, might, however, be driven by long-term trends where corruption risks may be increasing or decreasing due to long-run factors like changes in institutional quality or the deep economic crisis of 2008–2010.

To rule out the effects of such long-term trends, we conduct a difference-in-differences analysis. To create suitable treatment and control groups, we carry out both longitudinal and cross-sectional matching. For longitudinal matching, we use the same groups (treated-within and control-within) created for the before-after analysis. For cross-sectional matching, we use the before-after matched groups for disaster-affected areas and create before-after groups for areas not affected by disasters. The cross-sectional matching is done based on the five covariates used as control variables in the logit regression. We argue that the matching procedure generates a control group which is second-best since we cannot observe the same contract before and after disasters. Our approach is based on the work of D'avid-Barrett and Fazekas (2020) who propose matching on these covariates because they are measured at the contract level and are more relevant for causal estimation. Nevertheless, comparing matched regions in and outside of disaster regions may not be able to account for all unobserved confounders. While the timing of disasters is reasonably exogeneous, our disaster regions may be more likely to experience disasters in the long term. If governance quality is adversely affected by repeated disasters the treated disaster regions may systematically suffer from higher corruption risks and other institutional weaknesses.

There are also drawbacks to our comprehensive approach even though it combines methods each of which alleviates different threats to causal identification. First, the NUTS3 region of contract implementation might be broader than the actual disaster impact zone. Since the public procurement data does not go below the NUTS3 level, it is difficult to improve our measurement. Second, similar to D'avid-Barrett and Fazekas (2020), when using the pre-treatment average of the dependent variable for matching, we may get matches across buyers potentially presenting unobserved variable bias. Third, since the disasters are of varying magnitudes, not all of them will have the same impact on public procurement. Hence, our average effects may hide relevant heterogeneity.

5 | Results

5.1 | Unmatched *t*-Tests and Regression Analysis

In this section, we discuss the unmatched comparisons of the treated and control groups with respect to all our dependent variables.

To test whether exogenous disasters lead to short-term and/or mid-term changes in corruption risks, we first conduct simple *t*-tests for all dependent variables (Table 5). Given the limitations of our time series, we consider the comparison of 1 year before and after the disaster for estimating short-term effects, and our full time series for estimating mid-term effects. Robustness tests using a 3-year time window around the disasters are reported in Table B1.[15] If there is an increase in the corruption risks following disasters, we should see an uptick in the shares of awarded contracts with red flags.

t-Tests show that in the 1-year time window there are statistically significant increases in corruption risks for two dependent variables. More precisely, disasters lead to a 3% increase in non-publication of tender calls, and 35% increase in the use of non-open procedures. However, we observe a statistically insignificant decrease in single bidding (13%). Moreover, there is a significant decrease in contract awards with too-short advertisement periods (10%). An explanation for this pattern contradicting Hypothesis 1 is that the increase in the non-publication of call for tenders decreases the use of tenders where we observe advertisement periods (i.e., when there is no call for tenders advertised there is no advertisement period defined). The observed increases in corruption risks in the short-run are unsurprising since officials ought to use discretionary powers in emergency times. However, the change in the non-publication of call for tenders persists in the mid-term (full period), garnering initial support for Hypothesis 2. Furthermore, changes in the too-short advertisement period risk also become positive significant in the longer term.

These results are indicative of the effects we aim to measure, albeit imperfectly. They might be confounded by changing spending composition such as higher spending on construction projects or healthcare supplies critical for crisis response. And if these markets have inherently higher or lower corruption risk, they confound our results, pointing at spending composition, rather than behavioral changes. Therefore, to better gauge the effects of natural disasters, we proceed to the regression analysis to control for such confounding factors.

Tables 6 and 7 report the results of the binary logistic regression analysis for all four dependent variables (average marginal effects), during the short- (1-year time window before and after the disasters) and mid-terms (full period). The results for the 3-year analysis are available in Table B2.

Across our regression models, we find evidence that disasters have a significant impact on public procurement corruption risks in both the short and mid-terms. In the short term (the 1-year window), disasters increase the probabilities of non-open procedures (6%), non-publication of tender calls (51%), and single bidding (0.2%), albeit the latter is not significant at traditional significance cut-offs. On the other hand, there is a 25% decrease in the probability of too-short advertisement periods. As discussed above, this could be driven by the steep increase in non-publication of tender calls with the remaining, advertised tenders run with sufficiently long advertisement periods. All results considered, we have some, albeit not unequivocal support for Hypothesis 1. The mid-term (full period) results in Table 7 present a much clearer picture underpinning the claim that short and mid-term effects of disasters are different. In the mid-term, we find a consistent, 6%–9%, increase in corruption risks across all our dependent variables, with all results being statistically significant. The 3-year window results are available in Table B2, they also confirm our results. Comparing the two time periods, we see that the no call for tenders effect greatly decreases

TABLE 5 | Simple *t*-tests for differences of means between the treated and control contracts by dependent variable (*t*-value in the brackets).

Window size	Non-open procedure (after–before)	No tender call (after–before)	Too-short advert period (after–before)	Single bidding (after–before)
1-year window	0.351***	0.0334***	−0.102***	−0.130
	(12.358)	(1.183)	(−3.3327)	(2.834)
Full period	−0.0081	0.0629***	0.382***	−0.043
	(−1.431)	(8.4975)	(45.248)	(0.64)

Note: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

TABLE 6 | Binary logistic regression results: The table reports the average marginal effects from the binary logistic regression of treatment dummy on each of the dependent variables (std. errors are in the brackets).

	Binary logistic regression			
	Non-open Procedure	No tender call	Too-short advertisement period	Single bidding
Treatment (1-year)	0.0605**	0.510***	−0.250***	0.0024
	(0.0321)	(0.073)	(0.040)	(0.0207)
Control variables				
Contract value (log)	Y	Y	Y	Y
CPV (medical or not)	Y	Y	Y	Y
Contract year	Y	Y	Y	N
Contract month	Y	Y	Y	Y
Buyer type (regional or not)	Y	Y	Y	Y
N (total)	935	935	935	394
N (after disaster)	364	364	364	227
Cox and Snell's R^2	0.56	0.36	0.45	0.11

Note: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

TABLE 7 | Binary logistic regression results; the table reports the average marginal effects from the binary logistic regression of treatment dummy on each of the dependent variables (std. errors are in the brackets).

	Binary logistic regression			
	Non-open procedure	**No tender call**	**Too-short advertisement period**	**Single bidding**
Treatment (full period)	0.0667**	0.0599***	0.0932***	0.071***
	(0.0071)	(0.009)	(0.010)	(0.0142)
Control variables				
Contract value (log)	Y	Y	Y	Y
CPV (medical or not)	Y	Y	Y	Y
Contract year	Y	Y	Y	Y
Contract month	Y	Y	Y	Y
Buyer type (regional or not)	Y	Y	Y	Y
N (*total*)	9322	9322	9321	3648
N (*after disaster*)	5624	5623	6624	1138
Cox and Snell's R^2	0.17	0.22	0.42	0.17

Note: $*p < 0.1$; $**p < 0.05$; $***p < 0.01$.

in magnitude while the short advertisement period risk factor turns from negative to positive significant. Non-open procedure types remain about the same magnitude and also significant. Single bidding remains positive and becomes significant in spite of dropping to about half its magnitude compared to the short term. This is most likely due to the increase in sample size for the medium-term results. Taken together, these results lend support to our Hypothesis 2. While we observe some moderation of risks related to limited openness and competition, the consistent corruption risks increasing effects of the disasters persist. Crucially, while criticism around the inevitability of some risks to increase immediately after disasters, the longer term persistence of the observed corruption risk factors also increase our confidence in measurement validity. In other words, supply markets and tender timelines should normalize a few years following a disaster making the risk indicators more likely to indicate risky choices made by public buyers rather than necessities.

However, the unmatched regression analysis does not fully isolate the effect of change in behavior due to disasters. Recall that the shock due to disasters can affect procurement practices through (a) changes in the spending composition and volume, and (b) changes in corrupt behavior. The coefficients of the unmatched comparison capture both. The matched analysis, presented in the next section, allows us to control for risks that arise due to changing spending composition and isolate the effect of behavioral changes due to disasters.

5.2 | Matching Estimations

We apply the matching procedure outlined in Section 4 to identify matched control- treatment groups. The matching takes place over time within the same buyers in the same region, with the pre-disaster period serving as the control group and the post-disaster period as the treated group. This explicitly controls for institutional quality and allows us to compare contracting behaviors by similar entities throughout the disasters. In addition, matching on contract characteristics, such as value and product group, controls for corruption risks stemming from higher spending overall as well as different kinds of goods and services that municipalities may require after disasters which may be inherently more susceptible to corruption risks (for matching balance see Table A1). Following the matching, we conduct *t*-tests on the matched samples to estimate the causal impact of the disasters. Since Coarsened Exact Matching is more demanding for our sample size, the results are less reliable and hence comparable to the mid-term results. By implication, we will mainly focus on the mid-term results in the subsequent discussion. This is also suitable from a theoretical perspective as short-term increases in corruption risks are expected with mid- to longer-term effects being more ambiguous.

Our matched comparisons further confirm the regression analysis results (Table 8): all impacts are positive significant, that is, disasters lead to increased risk of corruption in the mid-term. Specifically, we find a 10% increase in contract awards through non-open procedures, a 20% increase in no call for tenders publication and a 39% increase for too-short advertisement periods, due to emergencies. The matched analysis also shows that disasters result in an increase of 14% in the share of single bidder contracts, though the change is significant only at the 10% level (please note a smaller sample size due to some missing values). These results are qualitatively confirmed by the 3-year time window, albeit single bidding loses its significance due to an even lower sample size (Table B3).

Unfortunately, the comparison with the short-term effects is limited by the small number of observations and hence the less refined matching algorithm. Nevertheless, the results for short-term (1-year window) impacts qualitatively overlap with the mid-term results (Table B4). Effects are similar in magnitude and directions are always the same. The lack of call for tenders increases significantly from before to after the disaster, by 20%. The other effects become insignificant due to the much smaller sample sizes. All results considered, we have some support for Hypothesis 1 and strong support for Hypothesis 2.

5.3 | Difference-in-Differences Analysis

The comparison of affected areas before and after disasters so far could mitigate a range of challenges to isolating the causal effect of disasters on corruption risks. However, these methods cannot take into account exogenous time trends or shocks which affect all regions independently of the disasters. For example, the results in the previous section could simply be driven by long- term changes in demand. To make our results robust to such potential confounding factors, we conduct a difference-in-differences analysis which is well-suited to controlling for exogenous time trends. Bringing in the comparison regions which were not affected by disasters, arguably, carries the risk that we compare inherently different areas as the long-term likelihood of disasters varies by region, potentially affecting governance quality. To minimize such risk to our causal identification strategy, we compare contracts awarded in disaster-affected areas with those that were awarded in very similar, non-disaster-affected areas. Among others, we match on pre-disaster dependent variable averages, aiming to balance governance quality among compared regions. Further details on the matching procedure are outlined in Appendix A. Due to data limitations for shorter time periods, we only conduct the diff-in-diff analysis for the full period. The use of shorter time periods in this sort of analysis leads to difficulties such as pre-treatment imbalance between the treated and control groups and is therefore not reliable.

First, we note a large and statistically significant increase in the prevalence of non-publishing call for tenders (10% increase) and too-short advertisement periods (29% increase) in disaster-affected areas after the disaster compared to the control group (Table 9). The impacts on the two other dependent variables, while positive, remain insignificant: contract awards through non-open procedures increase by 3% while single bidding increases by 17% following the disasters. Given that the sample size is considerably smaller for single bidding, it may explain the low significance level.

All results considered, we find that emergencies increase corruption risks both in the short and medium terms, supporting both Hypotheses 1 and 2. If anything, longer-term corruption risk- enhancing impacts of disasters are even greater than short-term impacts. As mentioned in the introduction, this could be due to long-run effects of temporary shocks, when the governance sys- tem moves from a low corruption equilibrium to a high corruption equilibrium and high corruption persists. However, these results on the sorts of deviations from standard public procurement procedures that increase the risk of corruption, should not be interpreted as exact measures of increases in corruption itself.

6 | Discussion and Conclusions

We investigated whether there is an increase in corruption risks in the public procurement processes and outcomes due to natural disasters by combining public procurement data from Italy with de facto data on five natural disasters. The results of regressions, matched and difference-in-differences analyses show that disasters lead to an increase in the use of procurement procedures that in practice reduce transparency and result in weaker competition. Our results shed light on how procurement officials might use their discretionary

TABLE 8 | Matched *t*-tests; the table reports the results of the difference in means of the matched sample of contracts after and before disaster (*t*-values are in the brackets).

	t-Tests			
	Non-open procedure (after–before)	**No tender call (after–before)**	**Too-short advertisement period (after–before)**	**Single bidding (after–before)**
Treatment (full period)	0.096***	0.203***	0.395***	0.142*
	(3.429)	(3.904)	(21.79)	(1.952)
Matching variables				
Contract value (log)	Y	Y	Y	Y
Main sector CPV	Y	Y	Y	Y
Contract month	Y	Y	Y	Y
Buyer type	Y	Y	Y	Y
Buyer prior DV avg.	Y	Y	Y	Y
N (total)	673	643	2614	115
N (after disaster)	124	99	966	57

Note: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

TABLE 9 | Difference-in-differences results: The table reports the results of difference-in-differences regression results following matching (std. errors are in the brackets).

	(Diff-in-diff (OLS))			
	Non-open procedure	**No tender call**	**Too-short advertisement period**	**Single bidding**
Diff-in-diff (full period)	0.034	0.205***	0.287***	0.166
	(0.021)	(0.037)	(0.022)	(0.121)
Timing (full period)	−0.107***	−0.040**	−0.181***	−0.039
	(0.018)	(0.015)	(0.012)	(0.096)
Location (full period)	−0.049***	−0.016	−0.267***	−0.040
	(0.008)	(0.012)	(0.014)	(0.071)
Control variables				
Contract value (log)	Y	Y	Y	Y
Main sector CPV	Y	Y	Y	Y
Contract month	Y	Y	Y	Y
Buyer type	Y	Y	Y	Y
Contract year	Y	Y	Y	Y
N (*total*)	12,569	8006	24,529	1799
Adj. R^2	0.107	0.245	0.290	0.102

Note: $^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

powers to alter procedures and outcomes in the years following a disaster.

Our results demonstrate that disasters increase the risks that officials engage in rent-seeking, because they have a greater opportunity to abuse their extended discretionary authority. Moreover, we find that such effects persist in the medium-run, possibly due to a shift from a low corruption equilibrium to a high corruption equilibrium. However, this analysis of procedures and outcomes cannot provide a complete picture of the extent of the increases in corruption risks. Discretionary powers may help reduce delays in procurement (Coviello, Guglielmo, and Spagnolo 2018; Decarolis et al. 2020) and effective disaster-response calls for quick acquisitions of necessities and the "value for money" standard may need to be relaxed. The pressure to use relief-aid quickly may also result in more single bidding and accelerated procedures (Schultz and Søreide 2008). Furthermore, it is difficult to define what is an adequate time-period during which emergency-related procurement is allowed. In this study, we have considered the risk that officials may prolong the state-of-emergency to continue to receive illicit gains. However, officials may also need discretionary powers to legitimately prolong the period of emergency-related procurement since some disasters could have particularly severe long-run effects.

Our results face a number of limitations which subsequent research can improve on. First, we only looked at a small set of natural disasters in Italy, further research could compare a wider set of disasters across countries to test how widely our findings travel. Second, while we enumerated specific mechanisms which would increase corruption in disaster response and perpetuate a higher corruption equilibrium in public procurement, we could not compare and differentiate these mechanisms empirically. Further research should track impact mechanisms and test the conditions under which some are more influential than others.

In spite of its limitations, our work presents some policy implications. First, besides emergency clauses, policymakers should ensure that there is a clear framework for disaster procurement which clarifies the legitimate uses of discretionary power under emergency clauses. The rules should clarify which groups of goods and services can be procured through emergency clauses. This should allow for flexibility in choosing the exact goods and services needed for disaster response while limiting abuses of emergency situations. Second, there must be criteria, albeit flexible, for how an emergency is defined and a roadmap for returning to normalcy. Third, "real-time evaluations," which are quick assessments of disaster responses, need to be carried out while relief efforts are underway, ideally aided by real-time data (Schultz and Søreide 2008; Fazekas and S'anchez 2021). Such evaluations can help to flag any potential abuse of powers during the response effort and also aid in returning to normalcy. Fourth, ex-post evaluations should take place which (a) focus on policy implementation and unnecessary procurement procedures run and (b) have the power to sanction offenders.

In general, policymakers face the tough task of balancing the benefits of discretionary authority with the risks of corruption. This challenge becomes even more acute during emergencies. Striking the right balance is important as incorrect decisions could cost lives or result in huge losses due to, for example, prolonged abuse of emergency clauses. Further work may focus on contract implementation to study the welfare effects of emergency-related procurement.

Acknowledgments

The authors are thankful for the outstanding research assistantship offered by Ahmed Alshaibani.

Conflicts of Interest

The authors declare no conflicts of interest.

Data Availability Statement

The data and codes that underpin the findings of this study are openly available in Mendeley Data at https://data.mendeley.com/datasets/gpfdstkzyd/1, with DOI number: 10.17632/gpfdstkzyd.1.

Endnotes

[1] See the most recent OECD figures: https://doi.org/10.1787/888934258363.

[2] Coviello, Guglielmo, and Spagnolo (2018) and Decarolis et al. (2020) document that discretion may not always introduce inefficiencies.

In fact, it may reduce transaction costs by reducing delays in acquisitions.

[3] Lambert and de La Maisonneuve (2007) is an example of an ex-post investigation. The authors investigate the use of relief-aid after the 2004 Indian Ocean Tsunami.

[4] Available at: https://www.emdat.be/.

[5] These are disasters that are caused by other disasters. For example, an earthquake may trigger an avalanche or a tsunami.

[6] These criteria rule out migrant boat accidents, heat waves, famine, industrial accidents, and other small-scale disasters.

[7] EU directives 2004/18/EC and 2007/17/EC culminated into the Italian Legislative Decree 163/2006, and 10 years later EU directives 23/2014/EU, 24/2014/EU, and no. 25/2014/EU culminated into Italian Legislative decree 50/2016. For a more detailed review of public procurement in Italy, see Decarolis and Giorgiantonio (2015).

[8] DIGIWHIST refers to the Digital Whistleblower Project. More information is available at: http://digiwhist.eu/about-digiwhist/. The data can be freely downloaded from https://opentender.eu/start. The database and portal are maintained by the Government Transparency Institute: www.govtransparency.eu/.

[9] NUTS refers to Nomenclature of Territorial Units for Statistics. For Italy, the NUTS3 code allocates a unique alphanumeric code to each of the 107 Italian provinces.

[10] To give an example, corruption risks are much higher when there is 1 bid submitted on a competitive market compared to, say 5 bids. However, there is little change in corruption risks when bidder number increases from 6 to 10. In other words, while the underlying variables are continuous, corruption risks are non-linear necessitating a transformation (for an extended discussion on these see Fazekas and Kocsis (2020)). Nevertheless, to show that our results are not sensitive to the specific binary transformations of the continuous dependent variables (advertisement period length in days and number of bidders), we rerun all main analyses with the continuous versions. Results are confirmatory and placed in Appendix B (Tables B5, B6, and B7).

[11] Note that for ease of exposition, we use the “within” notation to refer to groups that lie in the disaster-affected.

[12] Please note that we do not cluster standard errors for the main regression results (both unmatched regressions and difference-in-differences regressions), as we work with full population data (full set of clusters and all observations within those clusters are included in the analysis), rather than a random sample. Nevertheless, we also show that our logistic regression results are robust to clustering standard errors in Appendix B.4 (Tables B8 and B9).

[13] The CEM code was written in R with the help of the R library due to Iacus, King, and Porro (2009).

[14] Coarsening of variables, that is selecting cut-points for splitting the sample, was done balancing sample size and sufficient specificity of analysis.

[15] We aim to keep the before–after time windows symmetric as much as possible in order to balance the comparison samples in terms of seasonality and keep compared samples balanced. Nevertheless, we also estimate our models using an asymmetric before-after sample excluding the 1st year after the disaster hits to offer a perspective on long-term effects without the immediate disaster relief effects. For details see Appendix B.5 (Tables B10, B11, and B12). All results are confirmatory, with the exception of no call for tenders which switches sign in the Diff-in-Diff model.

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Appendix A

Matching Procedure

To create the treatment and control groups, we conduct longitudinal as well as cross-sectional matching. For longitudinal matching, we retain the before-after groups created for disaster-affected areas.

- Before-after matching in the treated area
- Matching the before group lying in the treated area with the before group in the untreated area
- Matching the after group lying in the treated area with the after group in the untreated area Matching variables:
- Before-after matching in the treated area:

1. Buyer type (national, regional, public body, utilities, or others)
2. Main sector of the product purchased (CPV)
3. The contract value (natural logarithm of amount in euros)
4. Buyer organization (pre-treatment average of the corruption risks)
5. Contract month

- Cross-sectional matching

1. Buyer type (national, regional, public body, utilities, or others)
2. Main sector of the product purchased (CPV)
3. The contract value (natural logarithm of amount in euros)
4. Contract month
5. Contract year

Treatment and Control Groups

TABLE A1 | The table presents results of *t*-tests showing that the treatment and control groups are similar on average before treatment.

Dependent variable	Control–treatment	*p* value
Non-open procedure type	0.001	0.157
No tender call published	0.022	0.204
Too-short advertisement period	0.020	0.122
Single bidding	0.110	0.126

Appendix B

Additional Results Tables

Unmatched Models With 3-Year Window

TABLE B1 | *t*-Test results for the treated contracts awarded by dependent variable (*t*-value in the brackets).

Window size	Non-open procedure (after–before)	No tender call (after–before)	Too-short advertisement period (after–before)	Single bidding (after–before)
3 years	0.068***	0.139***	0.273***	−0.048*
	(7.992)	(10.884)	(20.25)	(1.94)

Note: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

TABLE B2 | Binary logistic regression results; the table reports the average marginal effects from the binary logistic regression of treatment dummy on all the dependent variables (std. errors in the brackets).

	Binary logistic regression			
	Non-open procedure	**No tender call**	**Too-short advertisement period**	**Single bidding**
Treatment (3-years)	0.0429***	0.119***	0.178***	0.0792***
	(0.0079)	(0.011)	(0.010)	(0.030)
Control variables				
Contract value (log)	Y	Y	Y	Y
CPV (medical or not)	Y	Y	Y	Y
Contract year	Y	Y	Y	Y
Contract month	Y	Y	Y	Y
Buyer type (regional or not)	Y	Y	Y	Y
N (*total*—3 *years*)	4214	4214	4214	1588
N (*after disaster*—3 *years*)	1734	1734	1734	1065
Cox and Snell's R^2—3 years	0.19	0.26	0.29	0.18

Note: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Matched *t*-Tests With 1- and 3-Year Windows

TABLE B3 | Matched *t*-tests; the table reports the results of the difference of means of contracts 3 years before and after disasters (*t*-value in the brackets).

	t-Tests			
	Non-open procedure (after–before)	**No tender call (after–before)**	**Too-short advertisement period (after–before)**	**Single bidding (after–before)**
Treatment (3 year)	0.094***	0.132**	0.344***	0.095
	(3.299)	(2.513)	(12.62)	(1.224)
Matching variables				
Contract value (log)	Y	Y	Y	Y
Main sector CPV	Y	Y	Y	Y
Contract month	Y	N	Y	Y
Buyer type	Y	N	Y	Y
Buyer prior DV avg.	Y	Y	Y	Y
N (*total*)	529	499	1577	95
N (*after disaster*)	122	103	385	46

Note: For single bidding, we have relaxed the matching to obtain as many observations as possible. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

TABLE B4 | Matched *t*-tests; the table reports the results of the matched *t*-test of contracts 1 year before and after disasters (*t*-value in the brackets).

	t-Tests			
	Non-open procedure (after–before)	**No tender call (after–before)**	**Too-short advertisement period (after–before)**	**Single bidding (after–before)**
Treatment (1 year)	0.12	0.302**	0.072	0.062
	(1.228)	(2.69)	(1.622)	(0.28)
Matching variables				
Contract value (log)	Y	Y	Y	Y
Main sector CPV	Y	Y	Y	Y
Contract month	N	N	N	N
Buyer type	N	N	N	N
Buyer prior DV avg.	Y	Y	Y	Y
N (*total*)	128	112	330	32
N (*after disaster*)	64	56	165	16

Note: For single bidding, we have relaxed the matching to obtain as many observations as possible. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Models With Continuous Dependent Variables

TABLE B5 | Linear regression results for continuous dependent variables: Advertisement period length in days (corresponding to short advertisement period risk) and number of bidders (corresponding to single bidding risk).

	Unmatched linear regression	
	Days advertised/365	**Number of bidders**
Treatment	−0.0185***	−0.191
	(0.004)	(0.550)
Control variables		
Contract value (log)	Y	Y
CPV	Y	Y
Contract year	Y	Y
Contract month	Y	Y
Buyer type	Y	Y
N (*total*)	5159	1719
N (*after disaster*)	1922	993
Adj. R^2	0.2331	0.23

Note: These dependent variables indicate higher risk with lower values. Standard errors are in the brackets. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

TABLE B6 | Matched *t*-tests for continuous dependent variables: Advertisement period length in days (corresponding to short advertisement period risk) and number of bidders (corresponding to single bidding risk).

	t-Tests	
	Days advertised/365 (after–before)	**Number of bidders (after–before)**
Treatment	−0.0395***	8.153***
	(−18.425)	(3.4572)
Matching variables		
Contract value (log)	Y	Y
Main sector CPV	Y	Y
Contract month	Y	Y
Buyer type	Y	Y
Buyer prior DV avg.	Y	Y
N (*total*)	2301	121
N (*after disaster*)	850	61

Note: The table reports the results for the difference in means for contracts before and after disasters (*t*-values in the brackets). Note that these dependent variables indicate higher risk with lower values. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

TABLE B7 | Difference-in-differences results for continuous dependent variables: Advertisement period length in days (corresponding to short advertisement period risk) and number of bidders (corresponding to single bidding risk).

	(Diff-in-diff (OLS))	
	Advertisement period days/365	**Number of bidders**
Diff-in-diff (full period)	−0.020***	−1.500
	(0.005)	(1.15)
Timing (full period)	0.006*	−0.188
	(0.003)	(1.000)
Location (full period)	0.021***	2.239***
	(0.003)	(0.710)
Control variables		
Contract value (log)	Y	Y
Main sector CPV	Y	Y
Contract month	Y	Y
Buyer type	Y	Y
Contract year	Y	Y
N (*total*)	18,229	1767
Adj. R^2	0.079	0.258

Note: These dependent variables indicate higher risk with lower values. Standard errors are in the brackets. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

TABLE B8 | Binary logistic regression results with clustered standard errors at the disaster-year level (1 year window): The table reports the average marginal effects from the binary logistic regression of treatment dummy on each of the dependent variables (std. errors are in the brackets).

	Binary logistic regression			
	Non-open procedure	**No tender call**	**Too-short advertisement period**	**Single bidding**
Treatment (1-year)	0.0605	0.510***	−0.250*	0.0024
	(0.044)	(0.179)	(0.134)	(0.012)
Control variables				
Contract value (log)	Y	Y	Y	Y
CPV (medical or not)	Y	Y	Y	Y
Contract year	Y	Y	Y	N
Contract month	Y	Y	Y	Y
Buyer type (regional or not)	Y	Y	Y	Y
N (*total*)	935	935	935	394
N (*after disaster*)	364	364	364	227
Cox and Snell's R^2	0.56	0.36	0.45	0.21

Note: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

TABLE B9 | Binary logistic regression results with clustered standard errors at the disaster-year level (full period): The table reports the average marginal effects from the binary logistic regression of treatment dummy on each of the dependent variables (std. errors are in the brackets).

	Binary logistic regression			
	Non-open procedure	**No tender call**	**Too-short advertisement period**	**Single bidding**
Treatment (full period)	0.0667*	0.0599	0.0932*	0.0071
	(0.034)	(0.049)	(0.052)	(0.007)
Control variables				
Contract value (log)	Y	Y	Y	Y
CPV (medical or not)	Y	Y	Y	Y
Contract year	Y	Y	Y	Y
Contract month	Y	Y	Y	Y
Buyer type (regional or not)	Y	Y	Y	Y
N (*total*)	9322	9322	9321	3648
N (*after disaster*)	5624	5623	6624	1138
Cox and Snell's R^2	0.17	0.22	0.42	0.17

Note: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Regressions Excluding the 1st Year of Disaster: Replication of Tables 7, 8, and 9

TABLE B10 | Binary logistic regression results without the 1st year after the disaster; the table reports the average marginal effects from the binary logistic regression of treatment dummy on each of the dependent variables (std. errors are in the brackets).

	Binary logistic regression			
	Non-open procedure	**No tender call**	**Too-short advertisement period**	**Single bidding**
Treatment (full period)	0.034***	0.025***	0.128***	0.016
	(4.622)	(2.712)	(11.774)	(1.432)
Control variables				
Contract value (log)	Y	Y	Y	Y
CPV (medical or not)	Y	Y	Y	Y
Contract year	Y	Y	Y	Y
Contract month	Y	Y	Y	Y
Buyer type (regional or not)	Y	Y	Y	Y
N (total)	8850	8850	8849	3424
N (after disaster)	5270	5270	5270	2416
Cox and Snell's R^2	0.168	0.247	0.433	0.067

Note: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

TABLE B11 | Matched *t*-tests-without the 1st year after disaster; the table reports the results of the difference in means of the matched sample of contracts after and before disaster (*t*-values are in the brackets).

	***t*-Tests**			
	Non-open procedure (after–before)	**No tender call (after–before)**	**Too-short advertisement period (after–before)**	**Single bidding (after–before)**
Treatment (full period)	0.096***	0.203***	0.395***	0.142*
	(3.429)	(3.904)	(21.79)	(1.952)
Matching variables				
Contract value (log)	Y	Y	Y	Y
Main sector CPV	Y	Y	Y	Y
Contract month	N	N	N	N
Buyer type	N	N	N	N
Buyer prior DV avg.	Y	Y	Y	Y
N (total)	673	643	2614	115
N (after disaster)	124	99	966	57

Note: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

TABLE B12 | Difference-in-differences results without the 1st year after disaster: The table reports the results of difference-in-differences regression results following matching (std. errors are in the brackets).

	(Diff-in-diff (OLS))			
	Non-open procedure	**No tender call**	**Too-short advertisement period**	**Single bidding**
Diff-in-diff (full period)	0.048*	−0.365***	0.207***	0.184
	(0.026)	(0.041)	(0.024)	(0.129)
Timing (full period)	−0.056***	−0.335***	−0.114***	0.042
	(0.015)	(0.024)	(0.017)	(0.107)
Location (full period)	−0.040**	0.411***	−0.170***	−0.081
	(0.017)	(0.026)	(0.016)	(0.068)
Control variables				
Contract value (log)	Y	Y	Y	Y
Main sector CPV	Y	Y	Y	Y
Contract month	Y	Y	Y	Y
Buyer type	Y	Y	Y	Y
Contract year	Y	Y	Y	Y
N (*total*)	4211	1877	18,287	1099
Adj. R^2	0.169	0.485	0.307	0.120

*$p < 0.1$.
**$p < 0.05$.
***$p < 0.01$.