CORRUPTION IN THE TIMES OF PANDEMIA

JORGE GALLEGO†, MOUNU PREM‡, AND JUAN F. VARGAS⋆

Abstract. The public health crisis caused by the COVID-19 pandemic, coupled with the subsequent economic emergency and social turmoil, has pushed governments to substantially and swiftly increase spending. Because of the pressing nature of the crisis, public procurement rules and procedures have been relaxed in many places in order to expedite transactions. However, this may also create opportunities for corruption. Using contract-level information on public spending from Colombia's e-procurement platform, and a *difference-in-differences* identification strategy, we find that municipalities classified by a machine learning algorithm as traditionally more prone to corruption react to the pandemic-led spending surge by using a larger proportion of discretionary non-competitive contracts and increasing their average value. This is especially so in the case of contracts to procure crisis-related goods and services. Additionally, in places that rank higher on our corruption scale, contracts signed during the emergency are more likely to have cost overruns, be awarded to campaign donors, and exhibit implementation inefficiencies. Our evidence suggests that easing procurement rules in response to large negative shocks may increase corruption, and thus governments that encourage spending should also bolster instances of monitoring and oversight.

Keywords: Corruption, COVID-19, Public procurement, Machine learning

Date: July 25, 2020.
We would like to thank Gabriele Gratton, Jennifer Hunt, Nelson Ruíz, Mateo Uribe, and seminar participants at DNP and OVERS for helpful comments. We also thank Misión de Observación Electoral, Contraloría General de la República, and Luis Martínez for sharing with us data used in this project. Erika Corzo and Andrés Rivera provided excellent research assistance. Declaration of interest: none.
†Department of Economics, Universidad del Rosario. email: jorge.gallego@urosario.edu.co.
‡Department of Economics, Universidad del Rosario. email: francisco.munoz@urosario.edu.co.
⋆Department of Economics, Universidad del Rosario. email: juan.vargas@urosario.edu.co.

1. Introduction

The COVID-19 pandemic, caused by the SARS-CoV-2 novel coronavirus, has rapidly expanded throughout the world, creating an unprecedented public health crisis in both developed and developing countries (Ferguson et al., 2020). In an effort to control the spread of the disease, many governments in different regions have established stringent lockdown measures, restricting the mobility of hundreds of millions and paralyzing large sectors of the economy (Acemoglu et al., 2020; Ludvigson et al., 2020). Lockdowns have hit developing countries particularly hard. There, a considerable proportion of the population derive their livelihood from informal activities –most of which cannot be executed remotely–and that lack any access to safety nets such as unemployment insurance (Loayza and Pennings, 2020; Mobarak and Barnett-Howell, 2020). Consequently, in addition to the large investments in medical infrastructure and related supplies that governments have had to make to face the health crisis, billions have been allocated for poverty relief and to help survive small and medium businesses that are more vulnerable to large periods of economic inactivity. This paper shows that the need of governments to quickly spend large amounts of resources may create corruption opportunities in public procurement.

Traditional norms governing public procurement generally require –at least for purchases greater than certain values–lengthy and thorough procedures that seek to limit the discretion of public officials and promote competition among sellers. However, in many countries, the need to quickly spend large amounts of resources to offset the effects of the pandemic has forced government to take extraordinary measures, such as relaxing public procurement protocols (De Michele and Cruz, 2020). This has been the case, over the past weeks, of Argentina, Australia, Brazil, Canada, Chile, Colombia, France, Germany, Hungary, Israel and New Zealand, just to mention a few examples.[1] Easing procurement rules increases the discretion of public officials, and this may create corruption opportunities that could offset the full potential benefits of the policies aimed at promoting short-term relief spending in

[1]To track emergency legislation around the world see: https://www.lexology.com/library/detail.aspx?g=d75c6657-a3f7-4312-b341-7ba8da835fd8 (last accessed on May 12, 2020).

the face of large negative shocks.[2] Using administrative data for almost 360,000 contracts procured in Colombia around the first confirmed COVID-19 case in March 2020, we show that the pandemic increased the incidence and the value of discretionary contracts, especially in places that have traditionally had low levels of state capacity and high levels of corruption. In these places, contracts signed during the emergency are more likely to have cost overruns, be awarded to campaign donors, and exhibit implementation inefficiencies.

Our *difference-in-differences* identification strategy uses weekly longitudinal variation across the country's roughly 1,100 municipalities, exploiting the timing of the first detected case of COVID-19 in the country, as well as the cross-sectional variation provided by the baseline probability that a municipality is corrupt. We estimate the latter using an ensemble model that aggregates the corruption predictions of four canonical *machine learning* algorithms: random forest, gradient boosting machine, lasso, and neural network. These models leverage on almost 150 municipality-level characteristics to predict observed cases of corruption prosecutions from 2008 to 2015.

After the outbreak of COVID-19 in Colombia, places that rank higher on our predicted baseline corruption scale differentially increase the use of discretionary contracts, which are awarded using a direct selection procedure and allow for no competition whatsoever among bidders. The effect is economically large: a one-standard-deviation increase in the predicted baseline probability of corruption increases the probability of awarding a discretionary contract (the average value of a discretionary contract) by 7% (7.5%) relative to the pre-COVID-19 period *weekly* mean. The effect is larger for contracts that procure crisis-related items such as food and medical supplies: after the first detected COVID-19 case in Colombia, a one-standard-deviation increase in the predicted baseline probability of corruption increases the average value of a food-related contract by 13%. Moreover, the differential

[2] There is indeed widespread evidence that discretion in procurement may result in inefficiencies and corruption (Transparency International, 2010; Tran, 2011; Palguta and Pertold, 2017; Decarolis et al., 2020; Baltrunaite et al., 2018; Kang and Miller, 2017). However, in situations in which there is limited information about the quality of sellers, or the preferences of officials are aligned with those of the society as a whole, more discretion can in fact reduce corruption and improve efficiency (Coviello et al., 2018; Duflo et al., 2018; Bandiera et al., 2009). See CAF (2019) for a recent review of the pros and cons of providing more discretion to public officials.

effect of the outbreak on discretionary spending in corrupt municipalities is substantially larger after the government declared a *state of emergency* that enabled local governments to avoid competitive procurement processes.

Importantly, we control municipality and week fixed effects throughout, as well as the interaction of the latter with baseline levels of population, population density, and a poverty index, which indirectly capture the latent municipal demand for public procurement to face the crisis. Moreover, our results remain unchanged if we control for the spread of the disease across the country. Indeed, we find that there is a null correlation between the number of cases and deaths caused by COVID-19 and municipality-level corruption. This allows us to rule out the idea that the most corrupt places have been more affected by the disease, and accordingly, have had to adapt their procurement strategies more intensively than their less corrupt counterparts. Our results are also robust to the inclusion of a control that captures municipalities' access to ports and markets, thus suggesting that the results are not driven by corrupt places facing more difficulties and higher prices in the acquisition of crisis-related items. Finally, the main findings are substantively the same if instead of using our predicted baseline corruption measure as the treatment variable, we use any of two official transparency and institutional capacity indices calculated by the Colombian government.

To be sure, while compelling, our quantitative evidence is only suggestive about the corruption effects of the pandemic, but do not have a smoking gun that could be used as evidence in court. Because its illegal nature, objectively measuring corruption is challenging (Olken, 2007; Olken and Pande, 2012; Fisman, 2001). We posit that corruption is very likely in this setting because: i) As mentioned, in many settings more discretionary and less competitive procurement generate graft opportunities (see references on footnote 2); ii) We show that in places that rank higher on our corruption scale, contracts signed amid the emergency are more likely to receive cost overrun alerts from the Office of the Comptroller General, be awarded to campaign donors, and exhibit time and budget extensions, inefficiencies that are highly correlated with corruption (Gallego et al., 2020); iii) We discuss anecdotal evidence in section 2 that provides strong support to our interpretation that procurement rules relaxations in response to the crisis have led to more corruption.

This paper contributes to the literature studying the governance challenges imposed by episodes of crisis and catastrophic events, such as wars (Querubin and Snyder, 2013), natural disasters (Leeson and Sobel, 2008), and epidemics (Khemani, 2020). One strand of the literature has studied the political economy of epidemics, focusing on how these events relate to and affect political outcomes. Studies have focused on different dimensions, such as electoral behavior (Beall et al., 2016; Adida et al., 2019; Campante et al., 2020), state legitimacy (Fluckiger et al., 2019), women empowerment (Bandiera et al., 2019), and conflict (Gonzalez-Torres and Esposito, 2017). More related to our findings, Maffioli (2020) finds that in Liberia the government's response to the Ebola outbreak was strategic in such a way that relief efforts were not allocated efficiently but privileged electorally swing villages. We contribute to this literature by showing that public health crises, like the one caused by the COVID-19 pandemic, may affect other important political outcomes, such as corruption, which can arguably make recovery slower and more difficult.

Another strand of literature has studied the connection between disasters and corruption. Some authors argue that disaster relief may be allocated strategically to win elections (Garret and Sobel, 2003; Gasper and Reeves, 2011), even in a clientelistic fashion (Gallego, 2018). Other show that natural disasters create resource windfalls that may trigger corruption and fraud (Leeson and Sobel, 2008; Nikolova and Marinov, 2017). In general, the argument is that catastrophic events imply the mobilization of resources in the form of relief, which may be strategically appropriated by political actors. We delve deeper into the channels explaining corruption in the midst of a catastrophe, by showing how relief spending differentially changes as a function of historical corruption. We also show that not only foreign aid is at risk (Andresen et al., 2020), but also other forms of public resources. Moreover, we contribute to this literature showing a novel mechanism: After a catastrophic event –such as a pandemic– corruption can be exacerbated because public procurement rules need to be relaxed, which in turn can make spending to address the emergency more troublesome and less efficient.

Finally, our paper is connected to the vast literature on corruption, by providing an objective measure of malfeasance and by exploring some of the economic consequences of this phenomenon.[3] In line with previous research (Gallego et al., 2020; Colonnelli et al., 2020), we construct an objective measure based on observable characteristics and a machine learning approach.[4] One advantage of this index, compared to an approach based solely on observed detections by anti-corruption agencies or perceptions of citizens and key actors (Olken and Pande, 2012), is that the algorithms are able to pinpoint where corruption is most likely to occur, even if it has not been previously detected by agencies or citizens. With this measure in hand, we disentangle the effects of corruption on public procurement in the midst of an emergency.

This paper is composed of six sections, including this Introduction. In Section 2 we describe the Colombian context, emphasizing on the emergency legislation that has simplified public procurement in the midst of the emergency. Section 3 describes the data we use while Section 4 our identification strategy, underscoring the way in which we construct the municipality-level corruption index, the contract-level information gathered from Colombia's e-procurement platform, and the difference-in-differences approach used to identify the results. These are reported in Section 5, and Section 6 concludes.

2. Context

The first detected case of COVID-19 in Colombia was announced on March 6th, 2020, corresponding to a 19-year old woman that had recently traveled back from Milan, Italy. Initially, the virus spread at a lower pace compared to other countries in the region, such as

[3]For reviews, see Banerjee et al. (2012), Olken and Pande (2012), and Fisman and Golden (2017), and see Colonnelli and Prem (2020) for recent evidence on the economic costs of corruption.

[4]For other studies using procurement data and data science to disentangle corruption, see Adam and Fazekas (2019) and Fazekas and Saussier (2018).

Brazil, Ecuador, or Peru, perhaps due to the rapid response from the central government. By April 27, the last day included in our analysis, a total of 5,597 people had tested positive and 253 had officially died from the disease. 180 municipalities, out of the 1103 in the country, had reported cases by then. Panel A of Figure A1 in the Appendix reports the temporal evolution of cases in Colombia.

Soon after the first case was detected, President Ivan Duque announced a series of measures to contain the spread of the disease and to mitigate its economic impact, which became apparent after a nationwide lockdown which began on March 24, 2020 and the end of which has been repeatedly extended through April and May. On March 17, Duque appealed article 215 of the Constitution to declare an "economic and social emergency" in the country, which allowed the executive to issue emergency decrees. Shortly afterwards, the National Public Procurement Agency (*Colombia Compra Eficiente*, CCE) issued an official statement clarifying the implications of the emergency on public procurement and inviting contracting entities, (including local governments) to invoke instances of *manifest urgency* to speed up procurement when necessary. This entitled officials to directly select contractors without any previous bidding stage in order to expedite the procurement of goods and services considered necessary to face the emergency.[5] However, as we show in this paper, the relaxation of public procurement rules also promotes the very activities that the rules were designed to discourage to begin with, namely corruption and malfeasance.

Indeed, a few weeks after the state of emergency was invoked by the president, Colombia's control agencies –the Comptroller General, the Inspector General, and the Attorney General–revealed that several local and regional governments were taking advantage of the situation by likely engaging in corruption. These organization released a list of a handful of mayors and governors that awarded suspicious contracts, in which overcosts were apparent. For instance, in one such case, revealed by the Office of the Comptroller General, authorities

[5]The statement literally establishes that "even though the public tender is the awarding mechanism that constitutes the general rule for entities governed by the General Statute of Procurement of the Public Administration (...) Law 1150 of 2007 establishes some exceptions to free competition and to the plurality of bidders" (p. 1). See CCE's statement here:
https://www.colombiacompra.gov.co/sites/cce_public/files/cce_documentos/comunicado_covid_19.pdf (last accessed May 12th, 2020).

in the town of La Palma had bought crisis-related items with price premiums up to 330%.[6] In Arauca, food items were bought at prices over five times the market value.[7] The data journalism unit of *La Silla Vacía*, a Colombian independent news website, studied a sample of 48 contracts awarded in 17 departments in the midst of the emergency.[8] The report found overprices in more than 40% of the contracts analyzed, with food items such as sugar cane paste, salt, pasta, rice, beans, oil, sugar, and lentils being where the highest premiums were found.[9]

3. Data

3.1. **Public procurement.** Public procurement data used for this paper comes from the *Sistema Electrónico para la Contratación Pública* (SECOP), a web-based platform established by the Colombian government to digitize transactions held by public entities in the country. The first version of the platform, SECOP I, was created simply to publicize procurement but it recently evolved into SECOP II, a site where transactions can actually take place online. Compliance with the regulation that mandates the use of SECOP has increased over time since its creation and its usage is currently quite comprehensive. Today, the platform details approximately 10 million contracts from national and subnational-level public entities.[10]

For this analysis, we focus on contracts held by local-level governments from January 1 to April 27 of 2020, covering a total of 357,875 contracts with a total value of US$ 4.5 billion. Contract-level information includes important features such as the total budget of the contract, its approval date and duration, a unique standard identifier (UNSPCSC code), a

[6]See https://www.pulzo.com/nacion/cuarentena-alcalde-palma-explica-sobrecostos-contrato-mercados-PP877697 (last accessed May 11th, 2020).
[7]See https://forbes.co/2020/04/08/actualidad/contralor-denuncia-sobrecostos-de-80-000-millones-en-ayudas-por-covid-19-en-el-pais/ (last accessed May 11th, 2020).
[8]Departments are equivalent to U.S. states.
[9]See https://lasillavacia.com/plata-mas-48-contratos-mercados-se-habrian-alimentado-68-mil-familias-76388 (last accessed July 25th, 2020).
[10]See https://www.datos.gov.co/Gastos-Gubernamentales/SECOP-Integrado/rpmr-utcd (last accessed May 12th, 2020).

textual description of the good or service under contract, indicators of whether the contract required time or budget extensions, the nature of the contracting agency (national or subnational) and the type of process that was used in the procurement, among other. The type of process refers to the awarding instruments available for agencies, including public tenders, selection based on the qualification of merits, auctions, special regime, minimum-value, and direct selection. Importantly, the direct selection procedure is the least competitive mechanism, and its use is legally restricted to special cases, including situations of manifest urgency caused by particular events such as natural disasters. In turn, the textual description of each contract's purpose allows us to use text-analysis techniques to identify which contracts correspond to crisis-related spending. In particular, using a dictionary method, we identify all contracts related to the procurement of food and health-related goods and services.[11]

We thus have access to fine-grained information on public spending at the municipality-week level and for the different awarding instruments used by local governments. This allows us to confidently track whether a specific municipality used the direct selection method (and for what value), in a particular week of 2020, and if such a contract corresponded to the procurement of a crisis-related good or service. Figure A1 in the Appendix presents the evolution of discretionary contracts –i.e. those awarded through the direct selection method– as a share of the sum of discretionary and competitive contracts, both for the total number of contracts and the amount awarded by these.[12]

3.2. **Corruption.** Measuring corruption is challenging (Olken, 2007; Olken and Pande, 2012). More so measuring corruption for all the districts of a particular country.[13] Rather

[11]For the food category we used the Spanish words "alimentos", "víveres", "comida", "mercados", "kits", while for health-related procurement, we used the words "médico", "salud", "medicamentos", "hospitalización", "farmacia".

[12]We define 'competitive' contracts that are awarded using public tenders, auctions, selection based on merits, or a special regime, as they imply potential competition between two or more bidders.

[13]Ferraz and Finan (2008) and subsequent papers by the same authors measure corruption using the results of public audits in Brazil, that are only available for a subset of municipalities. In the case of Colombia, Transparencia por Colombia (2017) produced an index that focused on 28 department capitals. There are over 1,000 municipalities in Colombia. These are the third administrative layer of the country, and are equivalent to counties in the U.S.

than relying on perceptions of citizens or key actors –a common approach to measure corruption championed by international organizations–we follow Gallego et al. (2020) and Colonnelli et al. (2020) and use a machine learning approach to predict corruption based on factual corruption detections and observable characteristics of municipalities.

Using information from the Office of the Inspector General of Colombia and originally collected by Martinez (2019), we construct a dummy variable indicating if the mayor of each municipality was prosecuted by this anticorruption agency in the 2008-2011 or 2012-2015 mayoral periods.[14] We define this measure as our outcome variable and combine it with a large set of municipal characteristics to train several machine learning models: random forest, gradient boosting machine, lasso, and neural network.[15] We ensemble these using (Polley et al., 2011)'s super learner approach to construct a municipal-level risk score in each of the two periods. We use a random 70% of our dataset to train the models, and the remaining 30% to test their performance. We use a 5-fold, 10-time repeated cross-validation procedure to train our models and choose the optimal combination of parameters. In the case of the super learner the optimal weights used by the ensemble are 0.25 for the random forest, 0.26 for the boosting machine, 0.02 for the neural network, and 0.47 for the lasso.

We asses the predictive performance of our models in the testing set using conventional metrics, such as the Area Under the ROC curve (AUC) and the accuracy. Overall, our ensemble model achieves a high level of performance, at an accuracy of 0.84 and a AUC of 0.71. Given these numbers, we proceed to construct the corresponding predicted values for each municipality-period, which indicate the probability of being classified as corrupt. The final score for each municipality is the average of the two probabilities, one for each period. Note that the correlation between the probabilities of each period is quite high (0.7), which suggests that municipal corruption may be quite persistent over time. This is important for

[14]These prosecutions are carried out for violations by public officials of the disciplinary code. Violations, in turn, can be due to different causes, such as mismanagement of public resources (e.g. irregularities in public procurement, embezzlement, etc.), violation of electoral rules (e.g. non-compliance with legal requirements, undue participation in politics, etc.), among others. Consequently, this variable should be understood as a broad measure of corruption.

[15]We use a total of 147 municipality-level predictors, grouped into ten categories and measured for the same period as the outcome. The categories are: financial sector, conflict, crime, human capital, local politics, public sector, local demographics, economic activity, illegal activity, and natural resources.

our analysis, because it implies that the predicted probability of corruption until 2015, can be a good proxy for the latent probability of corruption in 2020.

We prefer to use this predicted measure throughout the analysis, because algorithms allow diagnosing corruption even if agencies have not detected it before. A municipality that shares all the characteristics with those that have traditionally been corrupt is assigned a high probability of being corrupt as well, even if in the past it was not classified as such by any agency. However, as we show below, our results are robust to the use of alternative transparency measures issued by government agencies. In Appendix B we describe in more detail how we constructed our predicted corruption measure. Figure 1 maps the baseline geographic distribution of predicted corruption in Colombia, according to our model.

3.3. **Other Variables.** Throughout the analysis, we use other variables either as controls, alternative treatment measures, or as additional outcomes. Information on the distance from municipalities to the departmental capital comes from the municipal panel of Universidad de los Andes, while distance to the nearest port was measured by ourselves using standard GIS techniques. The National Health Institute publishes daily information on the spread of COVID-19 in Colombia. The 2016 Open Government Index (IGA by its Spanish acronym) was calculated by the Office of the Inspector General, while the 2017 Integral Development Index (IDI by its Spanish acronym) was built by the National Planning Department. Finally, the data on cost overrun alerts was provided by the Office of the Comptroller General, while the information on campaign donors comes from the National Electoral Council, and was provided to us by the NGO *Electoral Observation Mission.* Table A1 in the Appendix reports the descriptive statistics.

4. Empirical strategy

4.1. **Main specification.** Our identification strategy exploits the timing of the first detected case of COVID-19 in Colombia (March 6th, 2020), as well as the cross-sectional variation provided by the baseline probability of a municipality being corrupt. More formally, using the subindex m to denote municipalities and t to denote weeks, we estimate the following difference-in-differences model:

$$y_{mt} = \alpha_m + \lambda_t + \beta(\text{Post Outbreak}_t \times \text{Corrupt}_m) + \sum_{c \in X_m} \gamma'(c \times \lambda_t) + \epsilon_{mt} \tag{4.1}$$

where y_{mt} are different measures of public procurement in municipality m in week t; Corrupt_m is a standardized measure of the predicted probability of corruption described in Section 3.2; Post Outbreak_t is a dummy that takes the value one after March 6th, 2020; and X_m is a set of municipality-level characteristics measured in 2019 that include total population, population density, and a poverty index. We interact these characteristics with the week fixed effects, λ_t, to allow for differential flexible trends parametrized by these municipality features. Additional to the week fixed effects, we include municipality fixed effects, α_m, that control for any observed or unobserved municipal-level time invariant heterogeneity. In turn, the non-interacted week dummies control for any time shock that affects simultaneously all the municipalities on the same week. Finally, ϵ_{mt} is the error term. Given that our treatment variable, $Corrupt$, is the result from the predicted values of a previously-estimated model, we estimate wild bootstrap standard errors that are clustered at the municipality level.

Our coefficient of interest, β, captures the average differential change in the use of discretionary public procurement contracts, before and after the outbreak of COVID-19 in municipalities with a high estimated baseline probability of corruption, relative to municipalities with a low estimated probability. In order to measure the use of discretionary contracts, for the outcome y_{mt} we focus on a dummy variable indicating whether a contract was awarded using the direct selection procedure in municipality m and in week t, and the natural log of the average amount of money awarded through this procedure. We also focus on discretionary contracts associated to crisis-related spending, in particular to procure food and health-related services and supplies.

4.2. **Identifying assumption.** The main assumption behind our empirical model is that in the absence of the outbreak of COVID-19, the usage of discretionary public procurement contracts in municipalities with a high estimated baseline probability of corruption would have followed a similar trajectory to the usage of discretionary public procurement contracts in municipalities with a low estimated probability. The validity of this "parallel trends" assumption can be partially assessed by estimating the following non-parametric regression:

$$y_{mt} = \alpha_m + \lambda_t + \sum_{j \in J} \beta_j (\text{Corrupt}_m \times \delta_t) + \sum_{c \in X_m} \gamma'(c \times \lambda_t) + \varepsilon_{mt} \tag{4.2}$$

where J includes all weeks in our sample except from the week before the first COVID-19 case in Colombia. Therefore, the parameters β_j can be interpreted as the differential usage of, for example, discretionary public procurement contracts in municipalities with a high predicted probability of corruption relative to municipalities with a low probability, in week j, relative to the week prior to the first detected COVID-19 case in the country.

5. Results

In this section, we present the main results of our analysis. First, we show that after the detection of the first COVID-19 case in Colombia, there is a greater increase in the use of discretionary contracts in places with higher baseline levels of corruption. Second, we show that this result is robust to different tests and alternative specifications, including the use of official measures of transparency and institutional capacity at the municipality level, instead of our predicted corruption index. Third, we show evidence in favor of the main identifying assumption of our empirical strategy, the parallel trends assumption, through a dynamic difference-in-differences specification. Fourth, we explore the mechanisms behind the results, showing that the effects are greater after President Duque declared the economic emergency and public procurement rules were relaxed, that there are no differential impacts for competitive contracting, and that the effects are found to occur mainly in the purchase of crisis-related items, especially food. Finally, we show additional evidence supporting the hypothesis that the pandemic increased corruption in public procurement, because in the post-treatment period, baseline corruption is positively correlated with the existence of judicial detection of cost overruns, with the allocation of contracts to campaign donors, and with contractual inefficiencies reported in SECOP.

5.1. **Corruption and Discretionary Contracts.** Table 1 presents the main results from specification (4.1). We include municipality and week fixed effects throughout, as well as the interaction of the latter with baseline controls: population, population density, and a poverty index, which indirectly capture the latent municipal demand for public procurement to face the crisis. The dependent variable in Columns 1 to 3 is the indicator of that at least one discretionary contract was awarded in municipality m and week t. In Columns 4 to 6 we focus on the (log) value of discretionary contracts. We find that, after the first COVID-19 case in Colombia, there is a differential increase in the probability of using a discretionary contract in municipalities with higher predicted baseline probability of corruption. Focusing on Column 1, a one-standard-deviation increase in the predicted probability of corruption increases the probability of issuing a discretionary contract in 5 percentage points (pp.), which represents an increase of 7% with respect to the average in the pre-COVID-19 period. Moreover, we find that the average value of a contract increased by 7.5% (see Column 4).[16]

In addition, Columns 2 and 5 control for the municipal prevalence of the disease by including the number of COVID-19 cases. Controlling for the prevalence of the virus is arguably a 'bad control', but we still do so to make sure that our results are explained by the proclivity of engaging in corruption and not by the severity of the disease. The point estimates are largely robust to this control, suggesting that the intensity of the COVID-19 infection is not a likely channel.[17] Overall, these results support the idea that municipalities with a higher probability of corruption did not change their public procurement behavior due to the needs caused by the pandemia.

Finally, Columns 3 and 6 control for market access by including the distance of each municipality to the nearest port and the department capital interacted with the week fixed effects. We do this to test whether our results are explained by municipalities with a higher

[16]Note that for the (log) value of contracts, the number of observations falls, since not all municipalities have a direct contract every week. Table A2 in the Appendix shows that the results are robust if we use a hyperbolic sine transformation of this value, which takes into account the zeros in the dependent variable.

[17]Furthermore, in Table A3 of the appendix we run a cross-sectional regression where the dependent variable is the number of COVID-19 cases (Column 1) and the number deaths associated with this disease (Column 2), while our treatment variable is the estimated baseline probability of corruption. We do not find any significance associated.

probability of corruption also finding it more difficult to access supplies, a channel which could explain the increase in the use and value of discretionary contracts. We do not find support for this alternative story: while the point estimates slightly fall in magnitude, they are not statistically different from our baseline coefficients.

5.2. **Robustness Tests and Alternative Specifications.** Table 2 of the Appendix reports additional robustness tests. First, Columns 1 to 4 assess the robustness of our inference. Columns 1 and 2 follow Conley (1999) and Conley (2016) and control for cross-sectional dependence and first order time dependence in the error term. Columns 3 and 4 follow Bertrand et al. (2004) and collapse the data before and after the first COVID-19 case to deal with potential serial correlation in the dependent variable. Our results are robust to both of these corrections. Second, in Figure 4, we present the robustness to the exclusion of one department at a time, and we find coefficients to be stable to the exclusion of any department. Consequently, our results are not driven by any particular department, which is important given the role that some parts of Colombia have had in terms of both historical corruption and the spread of the pandemic.

Furthermore, instead of our predicted corruption index, we use two alternative official cross-sectional measures of transparency and institutional capacity, at the municipality level.[18] On the one hand, we use the Open Government Index (IGA for its Spanish acronym), which was developed by the Office of the Inspector General.[19] This composite indicator is an official measure of transparency, since it summarizes the level of information reporting and the state of progress in the implementation of some regulations that seek to promote the strengthening of territorial public management.[20] Higher IGA values, on a scale of 0 to 100, indicate higher levels of transparency. We interact the 2016 IGA measure (the last year for which it is available) with the post-COVID time indicator, to determine if there is

[18]In other specifications, available upon request, we use the average *observed* corruption cases (instead of the machine-learning prediction) as the treatment variable. The results are quite similar to those presented in Table 1.

[19]See https://www.procuraduria.gov.co/portal/que_es_IGA.page (last accessed July 21, 2020).

[20]The index is made up of three dimensions: organization, display, and dialogue of the information. Each of these dimensions in turn has its own set of categories.

a differential change in the use of discretionary contracts in municipalities with high levels of transparency, relative to less transparent places. Columns 5 and 6 of Table 2 show that a one standard deviation increase in the IGA index implies a *reduction* of 3.1 percentage points in the probability of issuing a discretionary contract and a decrease of 5.4% in the average value of this type of contracts. Both effects are statistically significant.

On the other hand, we use the Integral Development Index (IDI for its Spanish acronym), constructed by the National Planning Department, to measure institutional capacity at the municipality level in Colombia.[21] This indicator, also measured on a scale of 0 to 100, seeks to evaluate public management in terms of planning, execution, and monitoring capacity, as well as the overall decision-making in the use of municipal resources.[22] As in the previous case, we interact the 2017 IDI with the post-COVID time dummy, to determine if there are differential changes in the use of discretionary contracts as a function of the level of municipality institutional performance. Columns 7 and 8 of Table 2 show that a one standard deviation increase in the IDI index implies a *reduction* of 2.7 percentage points in the probability of issuing a discretionary contract and a decrease of 4.6% in the average value of this type of contracts. Once more, both effects are statistically significant. In sum, it is reassuring that our results are robust to these two alternative and *official* measures of transparency and institutional capacity. This implies that our main result does not depend exclusively on the measure of corruption that we built using machine learning techniques.

Finally, Figure 5 plots the distribution of estimates from a randomization inference test, which involves 500 simulations in each of which we randomly assign the estimated values of the predicted probability of corruption across municipalities and estimate the main regression of interest –equation (4.1)–for each outcome. Panel A plots the results for the probability of issuing a discretionary contract, while Panel B is the analogous exercise for the natural logarithm of the average value of discretionary contracts. In both cases, the probability of

[21]See https://www.dnp.gov.co/programas/desarrollo-territorial/Estudios-Territoriales/Indicadores-y-Mediciones/Paginas/desempeno-integral.aspx (last accessed July 21, 2020).

[22]This index is made up of 6 dimensions, namely: effectiveness, efficiency, compliance with legal requirements, administrative and fiscal management, fiscal performance, and administrative capacity.

finding an estimate as the one presented in Table 1 is below 1%, showing that our results are unlikely to be driven by pure chance.

5.3. **Parallel Trends and Dynamics.** In Figure 2, we present the results from equation (4.2). In both panels we find no differential trends in the six weeks before the first COVID-19 case for municipalities with a higher predicted baseline probability of corruption relative to the ones with a lower probability. Overall, this evidence provides support for the main identifying assumption of our empirical strategy. In addition, Figure 3 reports equivalent results for the IGA and IDI, the alternative official measures of transparency and institutional capacity. Moreover, the figures suggest that, in contrast to the estimated (null) effects for the weeks prior to the first COVID-19 case in Colombia, after the arrival of the pandemic there is a differential increase in the usage and value of discretionary public procurement contracts in municipalities with a higher estimated baseline probability of corruption, and lower levels of transparency and institutional capacity. Interestingly, these effects grow over time suggesting that the effect of the pandemic on contract inefficiency and corruption is persistent.

5.4. **Mechanisms.** We corroborate this pattern in a more parametric way, by splitting the post COVID-19 period into two, taking into account the March 17 presidential decree that enabled municipalities to declare a *manifest urgency* in order to increase the use of discretionary contracts. In Table 3, we include a time dummy that takes the value of one for weeks between the first case and the release of the presidential decree (called *Post 1*), and a second dummy that equals one for weeks after the release of the presidential decree (*Post 2*). Consequently, Post 2 identifies the period in which public procurement regulations were *de jure* relaxed. We find that for both of our main dependent variables there is a larger and statistically different effect after the relaxation of the requirements for the usage of discretionary contracts. Note that for the probability of awarding a discretionary contract, there is a positive and significant effect of 3.4 percentage points in *Post 1*. This result suggests that even if the emergency had not officially been declared, in some places local governments may have anticipated the crisis generated by the arrival of COVID-19 in the country and increased the use of discretionary contracts prior to the official announcement of the state of emergency. In any case, our evidence shows that the use of this awarding mechanism is significantly greater after the relaxation of procurement rules took place.

We then study whether the pattern encountered for discretionary contracts can be also found for competitive procurement, which limits the scope for graft and was also not directly affected by any presidential decree. This falsification is important for the validity of our results. We find that there is no differential usage in this type of contracts in municipalities with a higher baseline corruption index after the outbreak of the pandemic in Colombia, and that their average value does not differentially increase in these places (see Table A4 and Figure A2 in the Appendix). This result is important because it suggests that in the post-COVID period there is no differential change in general government spending between corrupt and non-corrupt municipalities. Similarly, it suggests that there is no differential change, after the arrival of the disease, in the citizen demand for public services not related to the emergency. The differential change in public procurement only occurs through discretionary contracts, which are precisely those that were affected by the relaxation of procurement rules following the declaration of state of emergency in the country.

Finally, using text analysis, we grouped contracts distinguishing between crisis needs (food and health-related items), versus other contracts. When exploiting this distinction, we find that after the first COVID-19 case in Colombia municipalities that are one-standard-deviation more likely to be corrupt at baseline experience an increase in the average value of crisis-related contract of 7% (see Column 1 of Table 4 and Figure 6). Moreover, when we separate the post COVID-19 period by taking into account the decree that encouraged the usage of discretionary contracts, the size of the effect doubles to 13% (Column 2). When we split contracts between food and health-related purchases, we find that most of the effect is driven by a large increase in the value of food-related items in municipalities predicted to be more corrupt at baseline. A one-standard-deviation increase in the predicted corruption increases the value of these contracts after the presidential decree in 21% (Column 4). The observed increase in contracts that procure health-related items (5%) is not statistically significant at conventional levels. These results go in line with the anecdotal evidence described in Section 3.2, and with the statistical evidence based on judicial decisions that we present in the following subsection, which suggests that malfeasance was mainly driven by cost overruns in the acquisition of food relief.

5.5. **Is it Corruption?** So far, we have shown that discretionary contracting, particularly to procure food in the midst of the crisis, had a differential increase in places with higher levels of estimated baseline corruption, that this phenomenon occurs mainly after the state of emergency was declared, and that there is no equivalent result for competitive contracts. We have also discussed the anecdotal evidence that points to the relationship between COVID-driven procurement and corruption in Colombia, and discussed the large literature that correlates discretion to malfeasance opportunities.[23] However, it is natural to ask whether our empirical results necessarily imply that corruption increased due to the pandemic. As we recognized in the introduction to this paper, we do not have a smoking gun allowing us to verify, with an absolute level of certainty, that acts of corruption were in fact committed in Colombian municipalities when the spread of the pandemic generated the need to accelerate public expenditure. Nevertheless, in this subsection we present suggestive evidence consistent with the observation that after the outbreak of COVID-19 in Colombia, places with higher levels of baseline corruption showed a greater number of irregularities in public procurement. For this purpose, we use information from judicial investigations carried out during the pandemic by an anti-corruption agency, data on campaign donations in the 2019 local elections, and information from SECOP on contracts that required extensions (in time or money) after being awarded.

Soon after the start of the emergency, a team of analysts and data scientists from the Office of the Comptroller General started scraping and analyzing, in real time, all contracts

[23]Using information from Italian auctions, Decarolis et al. (2020) question the connection between discretion and corruption in public procurement. However, the authors find that the combination of discretion and lack of competition does correlate with corruption. This is precisely what has happened in Colombia during the emergency, after the declarations of manifest urgency and what we refer to as "discretionary contracts": a total absence of competition, because contracts awarded under this procedure, are assigned directly to a single bidder. The evidence shows that corruption is very likely in such cases.

related to the pandemic.[24] By comparing reported contract prices with official prices of goods and services computed by the National Statistics Office for the different regions of the country, this team was able to identify contracts that presented significant cost overruns. Based on this information, the Office of the Comptroller General issued alerts addressed to the responsible local governments. We had confidential access to the classified information on the *cross section* of municipalities that, as of June 2020, had received at least one such alert for cost overruns during the pandemic. Columns 1 and 2 of Table 5 show the results of cross-sectional regressions in which we assess the correlation between our estimated baseline corruption measure and the probability of receiving an alert from the Comptroller's Office. The correlation is positive and highly significant. According to Column 2, the specification that controls for baseline characteristics and department fixed effects, a one standard deviation increase in the corruption index is associated with a probability of receiving an alert 7.5 percentage points higher, which represents a 63% increase with respect to the mean for this outcome. Although this evidence is only suggestive and correlational, it indicates that during the pandemic, contracts with cost overruns were mainly awarded in places that have historically been more corrupt according to our measure.

Next, using information from the National Electoral Council, we identified all natural and legal persons who were campaign donors in the 2019 local elections and who received a contract in the post-COVID period in Colombia. We focus on that year's campaign, as it corresponds to the race in which the mayors who currently rule during the pandemic period were elected. Although the allocation of contracts to campaign donors does not necessarily prove the occurrence of malfeasance, there is an empirical literature that shows that this phenomenon is often associated with favoritism (Baranek and Titl, 2020), has negative implications on the provision of public goods (Mironov and Zhuravskaya, 2016), and is positively correlated with different measures of corruption (Fazekas and Cingolani, 2017; Baltrunaite, 2020; Hummel et al., 2019). In fact, Gulzar et al. (2020) show that in Colombia, "donors to mayoral campaigns are typically local business owners seeking to gain

[24] For information on this team, see https://www.contraloria.gov.co/contraloria/la-entidad/organigrama-y-dependencias/direccion-de-informacion-analisis-y-reaccion-inmediata-diari- (last accessed July 24, 2020).

preferential treatment in public procurement assignment." Consequently, the allocation of contracts to campaign donors can be interpreted as a red flag alerting that politicians seek to reward those who helped them in the past election, or who could help them in future electoral races.

Table 5 presents cross-sectional evidence that in the post-COVID period, the allocation to campaign donors of contracts of any type (Columns 3 and 4), and of direct contracts (Columns 5 and 6), is more frequent in municipalities with higher levels of baseline corruption. Importantly, all columns include department fixed effects and control for the lag of the dependent variable measured since the beginning of 2020 until the outbreak. Columns 4 and 6 imply, for example, that an additional standard deviation in the corruption index is associated with about 5.9 and 4.5 percentage points higher probabilities of awarding any type of contract and a direct contract to a campaign donor, respectively. It is important to note that in this case we use cross-sectional evidence for the post-treatment period, because the allocation of contracts to a campaign donor is an infrequent event, which presents very little variation in our municipality/week difference-in-differences specification. Again, these results show that in the midst of the pandemic, the allocation of (direct) contracts to campaign donors was higher in the most corrupt places, which is a sign of favoritism and could result in worse execution of these contracts.

In fact, SECOP itself provides information that proxies for the quality of a contract. The platform records whether each contract required any addition in money, or an extension in time, with respect to the original budget and the timeline initially established in the contract. Over costs and time extensions are certainly proxies of inefficiency (what Bandiera et al. (2009) call *passive* waste), and although they do not necessarily imply corruption (*active* waste), the empirical literature suggests that both forms of waste tend to correlate positively (Bandiera et al., 2009; DalBo et al., 2013; Gallego et al., 2020; Bosio et al., 2020). In fact, there is evidence that suggests that in Colombia, a significant part of corruption in public procurement occurs through over costs and time extensions, with respect to what was originally stipulated in contracts (Henao and Isaza, 2018). Table 6 shows that, in the post-COVID period, the frequency of over costs (Panel A) and time extensions (Panel B) in direct contracts, and to buy food and health-related items, is increasing in baseline corruption. As before, these regressions control for the lag of the dependent variable measured since the beginning of 2020 until just before the outbreak of the pandemic, and include department fixed effects. Column 2 of Panel A shows, for instance, that an additional standard deviation in baseline corruption is associated with a 5 percentage points higher probability of having a direct contract with over costs. As for time extensions, the difference is 4.7 percentage points. The coefficients are similar, and also always significant, for contracts to procure food and health-related items.

In sum, although proving corruption is hard, the evidence presented in this section shows that after the arrival of COVID-19 in Colombia, contracts are more likely to have judicially proven cost overruns, to be assigned to campaign donors, and to report over costs and time extensions in places with higher baseline levels of corruption.

6. Conclusion

This paper studies the evolution of public procurement during the COVID-19 crisis in Colombia. Using the first case of COVID-19 as our time variation, and a machine learning-estimated baseline probability of corruption at the municipality level, we find that the spending rush led by the pandemic increases the usage and value of discretionary procurement contracts –which are more likely to be corrupt–in places initially more likely to be corrupt. We also show that these effects are higher after the relaxation of procurement requirements, and are more pronounced in the procurement of crisis-related items such as food. Moreover, we find that in the post-pandemic period, contracts signed in places with higher levels of baseline corruption are more likely to have judicially-proven cost overruns, be awarded to campaign donors, and exhibit implementation inefficiencies often linked to malfeasance.

Our findings have important policy implications. It is obvious that in the midst of a catastrophe, such as the COVID-19 pandemic, governments need to relax contracting procedures in order to guarantee the expedited procurement of relief and other crises-related items. However, in order to curb the potential negative effects of these policies in terms of graft, top-down accountability tools such as audits –which have proven effective in other contexts (Olken, 2007; Avis et al., 2018)–should accompany the relaxation of contracting rules. More transparency, coupled with technology and data science, could also help reducing corruption. Our analysis exemplifies how machine learning techniques can be applied to the information provided by e-procurement platforms to identify places in which malfeasance is more likely to occur. In these contexts, preventing corruption may well save lives.

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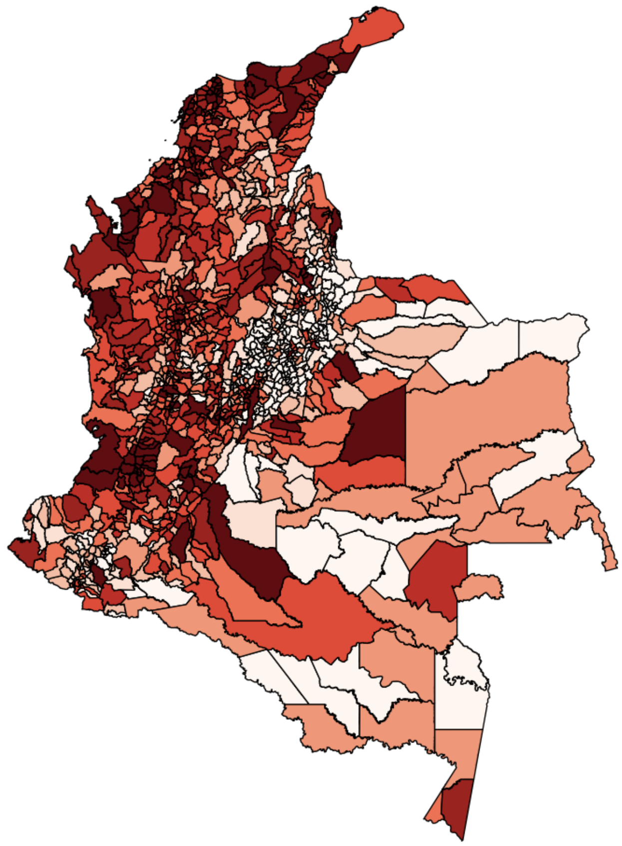
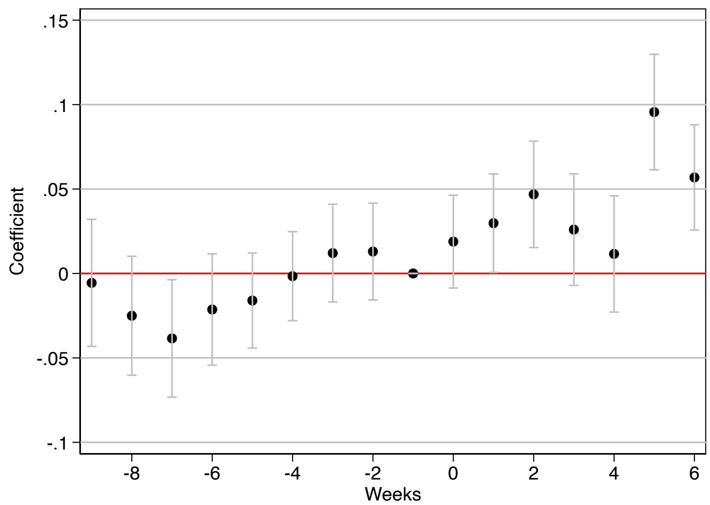
Figure 1. **Spatial distribution of corruption**



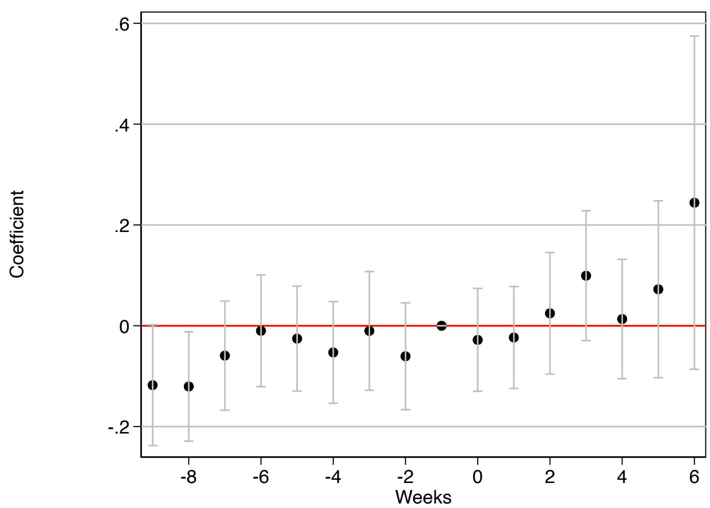
A. Corruption

Notes: This figure presents the spatial distribution of the predicted probability of corruption as discussed in Section 3.2.

Figure 2. **Corruption and discretionary contracts**



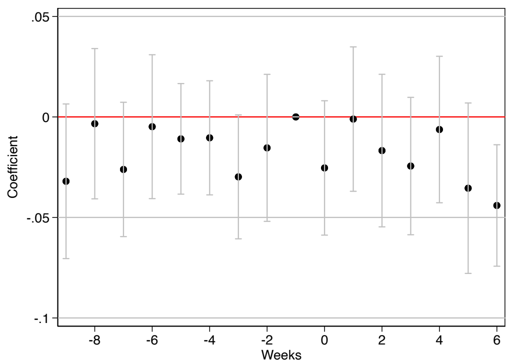
A. Any contract



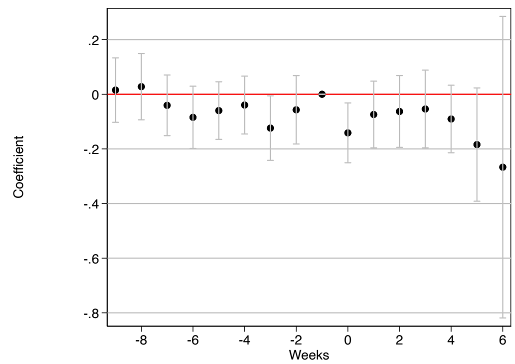
B. Ln average contract

Notes: This figure presents the coefficients from our dynamic specification presented in equation (4.2). Panel A presents the results for a dummy equal to one if there was at least one discretionary contract, while Panel B uses as dependent variable the logarithm of average amount per contract. We present the point estimates of the regression and the confidence of interval at the 95%.

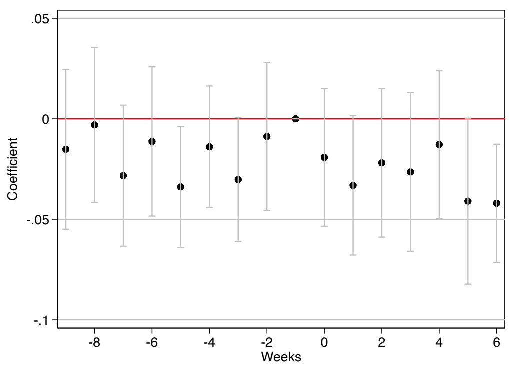
FIGURE 3. **Measures of transparency and discretionary contracts**



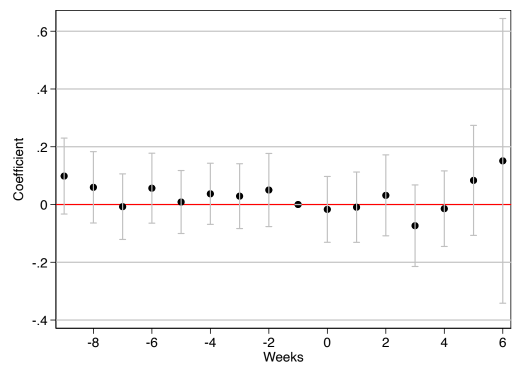
A. IGA-Any contract



B. IGA-Ln avg amount



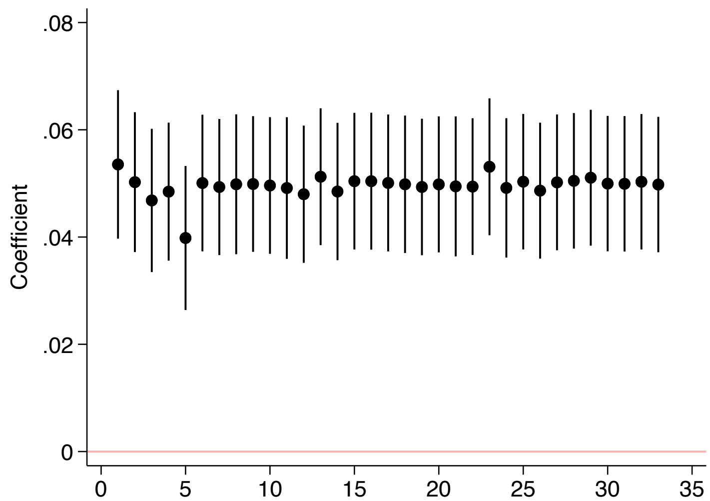
C. IDI-Any contract



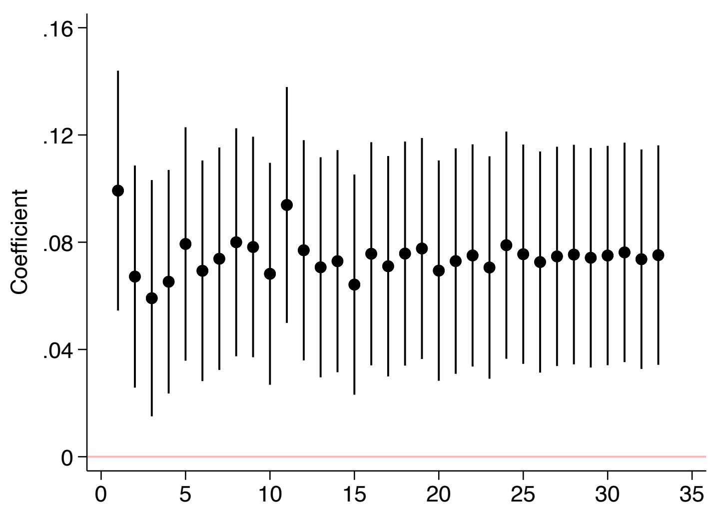
D. IDI-Ln avg amount

Notes: This figure presents the coefficients from our dynamic specification presented in equation (4.2). Panels A and B use as treatment variable the transparency index IGA, while Panels C and D use as treatment the institutional capacity index IDI. Panels A and C presents the results for a dummy equal to one if there was at least one discretionary contract, while Panels B and D uses as dependent variable the logarithm of average amount per contract. We present the point estimates of the regression and the confidence of interval at the 95%.

FIGURE 4. **Robustness to excluding one department at the time**



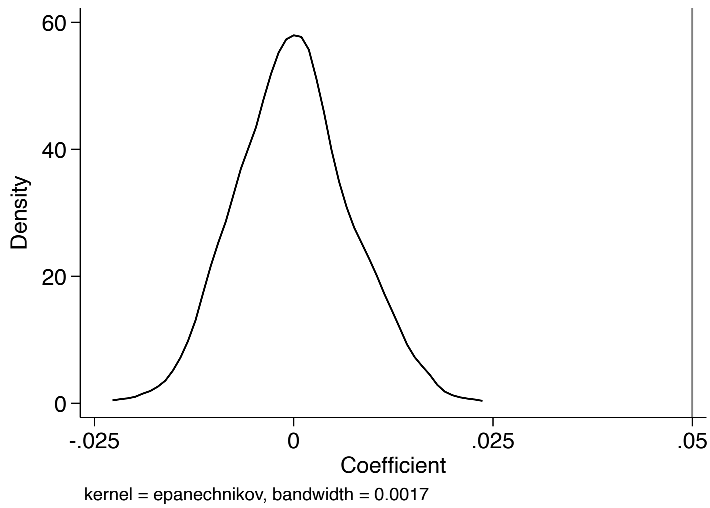
A. Any contract



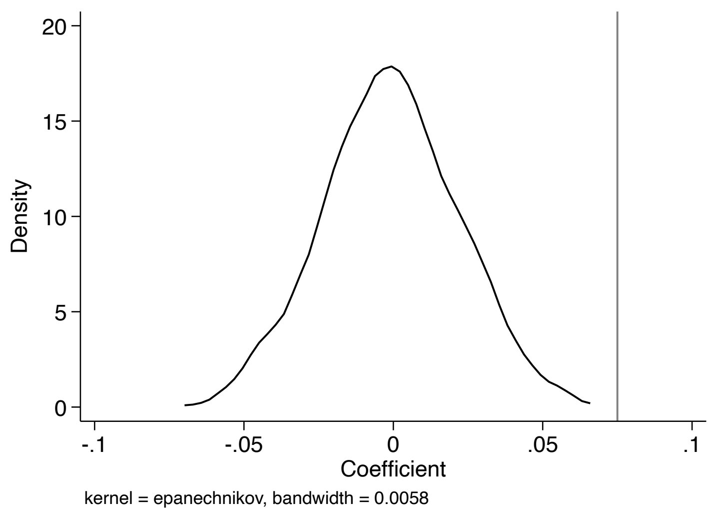
B. Ln average contract

Notes: This figure presents the robustness to excluding one department at the time and estimate the main specification (4.1). We present the point estimates of the regression and the confidence of interval at the 95%.

Figure 5. **Randomization inference**



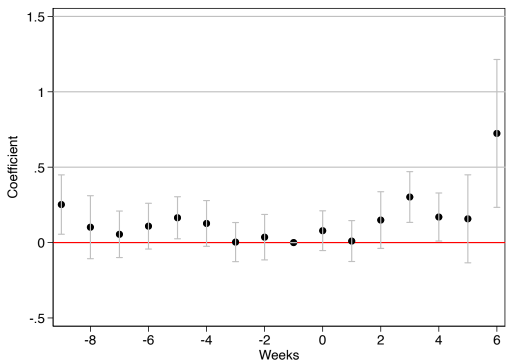
A. Any contract



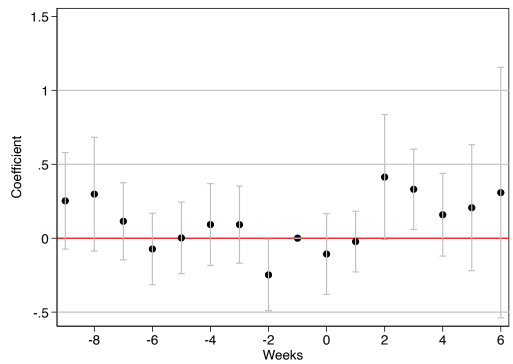
B. Ln average contract

Notes: This figure presents the distribution of placebo estimates from the main specification (4.1), where we randomly assign the predicted probability of corruption to municipalities 500 times. In both cases the probability of finding an estimate as the one presented in Table 1 is below 1%.

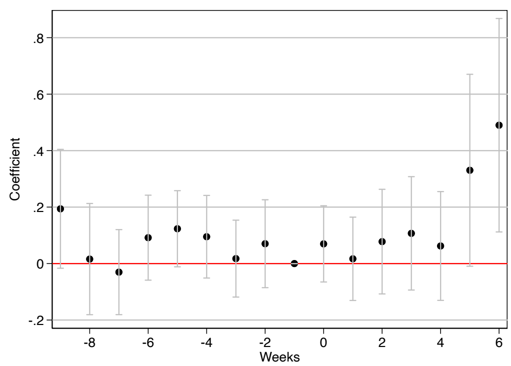
FIGURE 6. **Corruption and crisis related contracts**



A. Ln average contract



B. Ln average contract: Food



C. Ln average contract: Health

Notes: This figure presents the coefficients from our dynamic specification presented in equation (4.2). We use as dependent variable the logarithm of the average amount of a contract for different types of contracts. In Panel A we use contracts related to food and health combined, while Panels B and C present the results for food and health-related contracts separately. We present the point estimates of the regression and the confidence of interval at the 95%.

Table 1. Corruption and discretionary contracts

	(1)	(2)	(3)	(4)	(5)	(6)
	Any contract			Ln average contract amount		
Corrupt × Post outbreak	0.050***	0.049***	0.046***	0.075***	0.068***	0.057*
	(0.008)	(0.008)	(0.008)	(0.025)	(0.025)	(0.029)
Observations	17,552	17,552	17,552	10,995	10,995	10,995
R-squared	0.424	0.424	0.428	0.320	0.321	0.326
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 cases	No	Yes	No	No	Yes	No
Market access	No	No	Yes	No	No	Yes
Average dependent var	0.728	0.728	0.728	16.47	16.47	16.47

Notes: This table presents the results from the main specification in equation (4.1). *Corrupt* is a standardized version of the predicted probability of corruption. *Post outbreak* takes the value one after the first case of COVID-19 in Colombia. *Baseline controls* include population, population density, and a poverty index all of them interacted with week fixed effects. COVID-19 cases is the actual number of cases of COVID-19 in the municipality m in week t. *Market access* includes the distance to the nearest port and the distance to the department capital interacted with week fixed effects. Standard errors are computed using wild bootstrap and clustered at the municipality level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 2. Robustness exercises

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Spatial SE		Collapse pre/post outbreak		Alternative measure of transparency			
Treatment	Corrupt				IGA		IDI	
	Any contract	Ln average contract	Any contract	Ln average contract	Any contract	Ln average contract	Any contract	Ln average contract
Treatment × Post outbreak	0.050***	0.075***	0.050***	0.082***	-0.031***	-0.054**	-0.027***	-0.046**
	(0.008)	(0.024)	(0.010)	(0.041)	(0.006)	(0.022)	(0.006)	(0.022)
Observations	17,536	11,013	2,192	2,052	17,472	10,981	17,472	10,981
R-squared	0.423	0.322	0.838	0.725	0.420	0.319	0.420	0.319
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Average dependent var	0.729	16.47	0.729	16.46	0.730	16.47	0.730	16.47

Notes: This table presents the results from the main specification in equation (4.1). *Corrupt* is a standardized version of the predicted probability of corruption. *Post outbreak* takes the value one after the first case of COVID-19 in Colombia. *Baseline controls* include population, population density, and a poverty index all of them interacted with week fixed effects. Columns 1 and 2 present the main results using standard errors that control for spatial and first-order time correlation (see Conley, 1999, Conley, 2016). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. Columns 3 and 4 collapse the data to pre and post COVID-19 outbreak. Columns 5 and 6 use as our treatment variable the transparency index *IGA*, while columns 7 and 8 use the institutional capacity index *IDI*. Standard errors are computed using wild bootstrap and clustered at the municipality level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 3. Corruption, discretionary contracts, and policy response to COVID-19

	(1) Any contract	(2) Ln average contract
(2) Corrupt × Post 2	0.057***	0.107***
	(0.009)	(0.032)
(1) Corrupt × Post 1	0.034***	0.023
	(0.010)	(0.028)
Observations	17,552	10,995
R-squared	0.424	0.321
Municipality FE	Yes	Yes
Week FE	Yes	Yes
Baseline controls	Yes	Yes
Average dependent var	0.728	16.47
pvalue difference (1) and (2)	0.010	0.040

Notes: This table presents the results from the main specification in equation (4.1). *Corrupt* is a standardized version of the predicted probability of corruption. *Post 1* takes the value one between the first case of COVID-19 in Colombia and the release of a government decree relaxing public procurement requirements. *Post 2* takes the value one after the release of a government decree relaxing public procurement requirements. *Baseline controls* include population, population density, and a poverty index all of them interacted with week fixed effects. Standard errors are computed using wild bootstrap and clustered at the municipality level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 4. Corruption and crisis related contracts

	(1)	(2)	(3)	(4)	(5)	(6)
	Crisis		Food		Health	
Corrupt × Post outbreak	0.073*		0.134*		0.026	
	(0.046)		(0.087)		(0.050)	
(1) Corrupt × Post 2		0.130***		0.212***		0.055
		(0.055)		(0.104)		(0.063)
(2) Corrupt × Post 1		-0.044		-0.118		-0.020
		(0.047)		(0.087)		(0.050)
Observations	5,369	5,369	1,678	1,678	4,604	4,604
R-squared	0.426	0.428	0.548	0.551	0.404	0.404
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Average dependent var	16.35	16.35	16.51	16.51	16.33	16.33
pvalue difference (1) and (2)		0.000		0.000		0.170

Notes: This table presents the results from the main specification in equation (4.1). The dependent variable is the logarithm of the average value of a contract. *Crisis* includes food and health related contracts. *Corrupt* is a standardized version of the predicted probability of corruption. *Post outbreak* takes the value one after the first case of COVID-19 in Colombia. *Post 1* takes the value one between the first case of COVID-19 in Colombia and the release of a government decree relaxing public procurement requirements. *Post 2* takes the value one after the release of a government decree relaxing public procurement requirements. *Baseline controls* include population, population density, and a poverty index all of them interacted with week fixed effects. Standard errors are computed using wild bootstrap and clustered at the municipality level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 5. Corruption, alerts for cost overruns, and campaign donors

	(1)	(2)	(3)	(4)	(5)	(6)
			Campaign donors			
	Cost overrun alerts		Any contract		Any direct contract	
Corrupt	0.060***	0.075***	0.105***	0.059***	0.097***	0.045**
	(0.015)	(0.018)	(0.016)	(0.020)	(0.016)	(0.019)
Observations	1,091	1,091	1,091	1,091	1,091	1,091
R-squared	0.083	0.087	0.147	0.162	0.133	0.164
Department FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	No	Yes	No	Yes	No	Yes
Average dependent var	0.120	0.120	0.191	0.191	0.120	0.120

Notes: This table presents the results from a cross-section specification. The dependent variables are a dummy that takes the value one if there was at least one alert for cost overruns in public procurement and a dummy that equals one if at least one contract was awarded to a campaign donor. *Corrupt* is a standardized version of the predicted probability of corruption. All regressions control for the lag of the dependent variable measured since the beginning of 2020 until just before the outbreak of the pandemic, and include department fixed effects. *Baseline controls* include population, population density, and a poverty index. Standard errors are computed using wild bootstrap. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

TABLE 6. Contracts with extensions

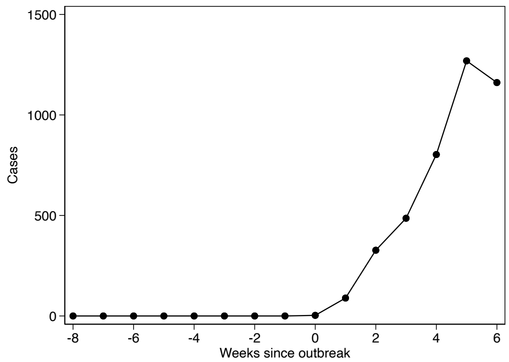
	(1)	(2)	(3)	(4)	(5)	(6)
	Direct contract		Food		Health	
Panel A: Budget						
Corrupt	0.087***	0.050**	0.049***	0.049***	0.055***	0.038***
	(0.015)	(0.021)	(0.014)	(0.017)	(0.014)	(0.015)
R-squared	0.199	0.207	0.064	0.068	0.113	0.125
Average dependent var	0.302	0.302	0.116	0.116	0.0802	0.0802
Panel B: Time						
Corrupt	0.094***	0.047**	0.100***	0.060***	0.109***	0.069***
	(0.015)	(0.020)	(0.015)	(0.017)	(0.016)	(0.019)
Observations	1,091	1,091	1,091	1,091	1,091	1,091
R-squared	0.221	0.234	0.204	0.223	0.157	0.171
Department FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	No	Yes	No	Yes	No	Yes
Average dependent var	0.291	0.291	0.124	0.124	0.167	0.167

Notes: This table presents the results from a cross-section specification. The dependent variable is a dummy that takes the value one if there was at least one contract with additions in budget and time for different types of contracts. All regressions control for the lag of the dependent variable measured since the beginning of 2020 until the outbreak. *Corrupt* is a standardized version of the predicted probability of corruption. All regressions control for the lag of the dependent variable measured since the beginning of 2020 until just before the outbreak of the pandemic, and include department fixed effects. *Baseline controls* include population, population density, and a poverty index. Standard errors are computed using wild bootstrap. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

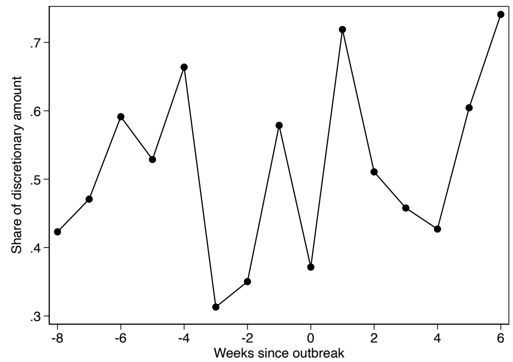
Online Appendix

Appendix A. Additional Graphs and Tables

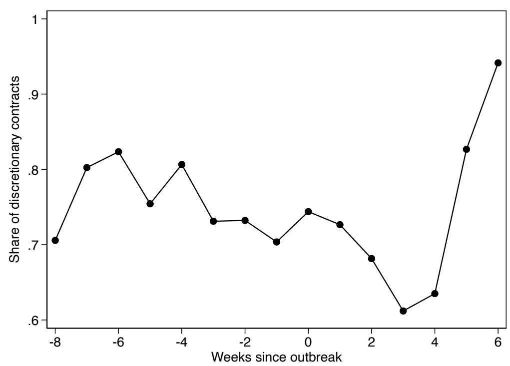
Figure A1. **Evolution of discretionary contracts and COVID-19 cases**



A. COVID-19 cases



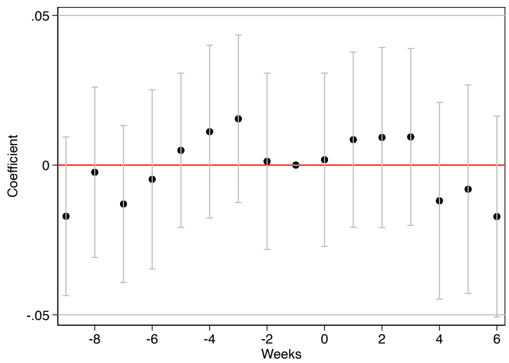
B. Share of discretionary contracts over competitive



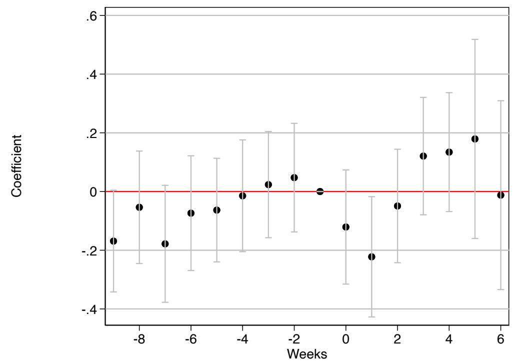
C. Share of discretionary amount over competitive

Notes: This figure presents the evolution of COVID-19 cases, the share of discretionary contracts over discretionary and competitive, and share of the amount in discretionary contracts over the total value in competitive and discretionary.

Figure A2. **Corruption and competitive contracts**



A. Any contract: Competitive



B. Ln average contract: Competitive

Notes: This figure presents the coefficients from our dynamic specification presented in equation (4.2). Panel A uses as dependent variable a dummy for at least one competitive contract, while Panel B uses as dependent variable the logarithm of the average amount of a competitive contract. We present the point estimates of the regression and the confidence of interval at the 95%.

TABLE A1. Summary statistics

	(1) Mean	(2) Median	(3) Standard deviation
Any discretionary contract	0.63	1.00	0.48
Ln average contract value for discretionary contracts	16.53	16.45	1.07
Ln average contract value for crisis related items	16.56	16.42	1.23
Ln average contract value for food items	16.95	16.80	1.47
Ln average contract value for health items	16.41	16.31	1.13
Any competitive contract	0.34	0.00	0.47
Ln average contract value for competitive contracts	16.58	16.32	1.74
Predicted probability of corruption	0.20	0.18	0.10
IGA index	64.45	65.75	9.51
IDI index	72.84	75.61	11.57
Cost overrun alerts	0.12	0.00	0.33
Any contract to campaign donor after outbreak	0.19	0.00	0.39
Any direct contract to campaign donor after outbreak	0.12	0.00	0.33
Any budget additions to discretionary contracts after outbreak	0.30	0.00	0.46
Any budget additions to food-related items after outbreak	0.12	0.00	0.32
Any budget additions to health-related items after outbreak	0.08	0.00	0.27
Any time additions to discretionary contracts after outbreak	0.29	0.00	0.45
Any time additions to food-related items after outbreak	0.17	0.00	0.37
Any time additions to health-related items after outbreak	0.12	0.00	0.33

Notes: This table presents summary statistics for the main variables used in the empirical analysis.

TABLE A2. Robustness to use the hyperbolic sine transformation of the average amount of discretionary contracts

	(1)	(2)	(3)
Corrupt × Post outbreak	0.883***	0.855***	0.883***
	(0.132)	(0.133)	(0.132)
Observations	17,552	17,552	17,552
R-squared	0.426	0.426	0.426
Municipality FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes
COVID-19 cases	No	Yes	No
Market access	No	No	Yes
Average dependent var	12.50	12.50	12.50

Notes: This table presents the results from the main specification in equation (4.1) for the hyperbolic sine transformation of average contract amount. *Corrupt* is a standardized version of the predicted probability of corruption. *Post outbreak* takes the value one after the first case of COVID-19 in Colombia. *Baseline controls* include population, population density, and a poverty index all of them interacted with week fixed effects. COVID-19 cases is the actual number of cases of COVID-19 in the municipality m in week t. *Market access* includes the distance to the nearest port and the distance to the department capital interacted with week fixed effect. Standard errors are computed using wild bootstrap and clustered at the municipality level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

TABLE A3. Corruption and COVID-19 exposure

	(1) Total cases	(2) Total deaths
Corrupt	-2.528	-0.061
	(1.700)	(0.168)
Observations	1,090	1,090
R-squared	0.753	0.514
Department FE	Yes	Yes
Baseline controls	Yes	Yes
Average dependent var	4.896	0.220

Notes: This table presents the results from a cross-section specification. *Corrupt* is a standardized version of the predicted probability of corruption. *Baseline controls* include population, population density, and a poverty index all of them interacted with week fixed effects. Standard errors are computed using wild bootstrap. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A4. Corruption and competitive contracts

	(1) Any contract	(2) Ln average contract amount
Corrupt × Post outbreak	-0.001	0.026
	(0.008)	(0.049)
Observations	17,552	5,871
R-squared	0.546	0.496
Municipality FE	Yes	Yes
Week FE	Yes	Yes
Baseline controls	Yes	Yes
Average dependent var	0.411	16.50

Notes: This table presents the results from the main specification in equation (4.1). *Corrupt* is a standardized version of the predicted probability of corruption. *Post outbreak* takes the value one after the first case of COVID-19 in Colombia. Baseline controls include population, population density, and a poverty index all of them interacted with week fixed effects. Standard errors are computed using wild bootstrap and clustered at the municipality level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Appendix B. Construction of the Municipality Corruption Index

To construct our predicted baseline corruption measure, we use a total of 147 municipality-level predictors. Each variable corresponds to one of the following ten categories: financial sector, conflict, crime, human capital, local politics, public sector, local demographics, economic activity, illegal activity, and natural resources. For the outcome variable used to train the models, we construct a dummy variable indicating if the mayor of each municipality was prosecuted by this anti-corruption agency in the 2008-2011 or 2012-2015 mayoral periods. The information to build this measure comes from the Office of the Inspector General and was collected by Martinez (2019). Using this outcome variable, and the 147 aforementioned predictors, we trained four canonical machine learning models for each period of government. In particular, we trained random forests, gradient boosting machine, neural network, and lasso. The results are not very different if other algorithms are used instead.

In each case, we follow the following steps:

(1) The dataset is divided into a training set consisting of 70% of the observations and a testing set with the remaining 30%.
(2) 5-fold cross-validation is performed in the training set in order to select the optimal combination of parameters for each algorithm and to train the models.
(3) The previous step is repeated 10 times varying randomly the partitions. Hence, 10 optimal sets of parameters are obtained. The final optimal parameter set is the average of these.
(4) Using this optimal parameters the predictive performance of the models is assessed in the test set that was not used for training purposes.
(5) Finally, individual models are ensembled using the Super Learner procedure (Van der Laan et al., 2007; Polley et al., 2011), in order to stack the individual predictions.

We use ensemble methods to finally construct the corruption measure as it is the case that the combination of different models perform better than their individual components. The models achieve acceptable levels of predictive performance, with a precision of 84% and an area under the ROC curve (AUC) of 0.71. These final models are used to estimate the level of corruption in each period of government, and the final measure we use is the standardized average of the score obtained by each municipality in these two periods. The correlation between the probabilities of each period is quite high (0.7), which suggests that municipal corruption may be quite persistent over time. This is important for our analysis, because it implies that the predicted probability of corruption until 2015, can be a good proxy for the latent probability of corruption in 2020.