Does buyer discretion facilitate home bias in procurement? Cross-border procurement of medical supplies under Covid-19

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Abstract

Public procurement markets are often national despite agreements against national preferencing. Is this due to home bias or real constraints such as language, regulatory hurdles or transportation cost? The beginning of the Covid-19 pandemic allow us to study two important margins of procurement policy. We study the effects of *crisis urgency* and increased *buyer discretion* on cross-border procurement in two difference-in-differences analyses that exploit local infection rates and a regulation change as natural experiments. Introducing a unique data set of contract awards for medical supplies in 27 European countries 2018 - 2020, we find a large, albeit temporary surge in cross-border awards during the first wave of the Covid-19 pandemic, overturning home bias. While previously just 1.5 percent of medical supplies were purchased cross-border, an increase in local infection rates by one standard deviation locally increases cross-border procurement by 19.3 percentage points. Some categories of medical supplies were subject to deregulation that increased buyer discretion. Drawing on additional data for products from related product groups that were not exempt from standard regulation, we find that deregulation caused cross-border procurement to increase by more than 35 percentage points. These effects persist also when accounting for local capacity constraints, so they are not solely driven by domestic supply shortages. Informing our findings with an agency model where the pandemic shifts the costs of monitoring a procurement agent, the findings suggest that national procurement is characterized by home bias rather than technical barriers.

JEL Codes: H12 Crisis Management, H57 Procurement, L51 Economics of Regulation

Keywords: public procurement, home bias, regulation, difference-in-differences, Covid-19

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1 Introduction

Public sector procurement accounts for a large share of the economy, at 15-20% of gross domestic product in the European Union (EU). Economists have identified sources of misallocation that result in inefficiencies, for example, due to favoritism of public buyers towards firms from their own country (home bias). Home bias can emerge when buyers care not only about the purchase itself, but also about secondary goals, such as boosting local jobs or pursuing political goals. In the EU, persistent home bias also undermines the policy goal of completing the "Single Market". This is in spite of regulation aimed at promoting competition and a level playing field. Instead, most contracts are awarded nationally, fragmenting public procurement markets along national borders.

In this paper, we use the impact of the early Covid-19 pandemic to study home bias. We find large effects on home bias of two factors surrounding procurement in an emergency: crisis urgency and deregulation of buyer discretion. We show that these effects are not explained by potential supply-side limitations that may have pushed buyers to purchase abroad. We use the natural experiment that the pandemic posed for certain regions and certain product categories to draw lessons for procurement, and we argue that the effective lever to reduce home bias is in buyer incentives. However, given that the costs of the pandemic are estimated anywhere between 12.5 trillion USD globally through 2024 (by the IMF)[1] to as much as 16 trillion USD for the US alone (Cutler and Summers, 2020), also avoiding misallocation in the context of emergencies is an important application of our results.

In 2020, the Covid-19 pandemic shocked the global economy on many levels, impacting international supply chains and firm activities. Changing market conditions overturned the conventional wisdom of the economics of procurement. As the crisis intensified, rules which prioritize transparency and competition were abandoned. In some places, public authorities found themselves competing against private sector buyers and each other. In the EU, the European Commission eventually dropped publication and transparency requirements and gave buyers full discretion in their purchasing decisions for certain goods. The focus of our analysis is on this deregulation, together with the effect of crisis urgency.

Potential misallocation in the procurement for medical supplies came under scrutiny and garnered high levels of public attention. Anti-corruption activists cautioned against the risk that vested private interests capture public resources and distort decisions as procurement is highly vulnerable to corruption.[2] Media reports uncover problems ranging from low-quality products in Austria[3] and Switzerland[4], over failures to organize distribution[5] and payment of deliveries[6] to even major irregularities in contract awards due to political influence-taking in Germany[7] and Finland[8]. Examples from other countries abound, making it clear that misallocation and inefficiencies did not fully disappear in this crisis situation.

This article makes three main contributions to the study of home bias and cross-border procurement: We create a novel data set that allows us to study procurement of medical supplies. We analyze tender documents published between 2018 and 2020 in Tenders Electronic Daily (TED), an online register of European procurement contracts. Our descriptive statistics show that a temporary and large surge in cross-border procurement during the first wave of infections coincides with a period of few competitive tenders and many direct awards. The prevalence of direct cross-border awards already suggests that mutual unawareness of buyers and foreign sellers is likely not an important driver of

[1] Reuters, 20.01.2022: https://www.reuters.com/business/imf-sees-cost-covid-pandemic-rising-beyond-125-trillion-estimate-2022-01-20/

[2] Transparency International, 29.03.2021: https://www.transparency.org/en/blog/g20-italy-covid-19-recovery-corruption-priorities

[3] Der Standard, 09.12.2020: https://www.derstandard.de/consent/tcf/story/2000122115865/mangelhafte-ffp2-masken-werden-zurueckgeholt-und-beschaffung-rechtlich-geprueft [in German]

[4] blue News, 08.03.2021: https://www.bluewin.ch/de/news/international/armee-tauscht-schutzmasken-der-firma-emix-um-616790.html [in German]

[5] Osnabrücker Zeitung, 05.03.2021: https://www.noz.de/lokales/osnabrueck/artikel/2246556/osnabrueck-loest-corona-materiallager-auf [in German]

[6] Die Zeit 13.06.2020: https://www.zeit.de/politik/deutschland/2020-06/mundschutz-atemschutzmasken-lieferproblem-zahlungsverzug-coronavirus [in German]

[7] Deutsche Welle, 08.03.2021: https://www.dw.com/en/german-lawmaker-resigns-over-face-mask-scandal/a-56798497

[8] Politiikka, 09.04.2020: https://www.hs.fi/politiikka/art-2000006469197.html [in Finnish]

home bias.

Furthermore, we investigate the separate effects of buyer discretion and crisis urgency with two analyses. First, we compare procurement of Covid-19 related medical products between regions, using differences in local infection rates as a staggered treatment. Our analysis finds that an increase in infection rates by one standard deviation increases the likelihood of a cross-border award for medical supplies by more than 19 percentage points (over a pre-pandemic baseline of 1.5 percent). A country-by-country leave-one-out analysis finds a still sizeable lower bound on the effect size of 9.3 percentage points.

Second, we analyze the lifting of restrictions on buyers purchasing certain kinds of Covid-19 related medical supplies. Under the new rules, buyers purchased directly from sellers rather than posting calls for tender. We compare the share of cross-border procurement for these supplies with medical supplies from related product categories that were unaffected by deregulation. Following this increase in buyer discretion, we estimate an increase in the probability of a cross-border award as large as 35.7 percentage points for affected products and services (with a lower bound of 16 percentage points in a country-by-country leave-one-out analysis).

Our third and final contribution is a monitoring model involving a government and its purchasing agent that yields testable predictions that help to interpret the empirical results. We propose that the observed changes in buyer behavior have to do with the changed incentives and informational advantages of the buyers.

Importantly, our results persist in an analysis on a subsample of contracts where there is domestic spare capacity, conservatively approximated as the extensive margin of supply (additional domestic firms standing ready to compete for a contract but not winning it). Therefore, a lack of domestic production capacity alone does not explain the surge in cross-border awards during the pandemic. At the same time, this implies that the low share of cross-border awards outside of the emergency is not driven by a lack of competitive foreign sellers, for example due to potential transportation cost differentials or other potential frictions related to regulatory, language or cultural barriers.

The empirical results show that home bias can subside temporarily in an emergency. Our findings then vindicate the crisis response of lifting restrictions. However, a return to pre-pandemic levels of cross-border procurement in late 2020 shows that both effects were just transitory. Overcoming home bias in procurement therefore requires more permanent policies. If regulation to limit buyer discretion has the straightforward effect of lowering this monitoring cost, we would expect regulation to decrease misallocation. However, the above-mentioned scrutiny and public attention on procurement of medical supplies during the pandemic might have worked in the opposite direction by disciplining buyer behavior.

The rest of the paper is organized as follows: In Section 2 we review the literature on misallocation in procurement. In Section 3 we presents the data set. We outline our empirical strategy and regression results in Section 4. Section 5 supports our findings with a model of monitoring. Section 6 concludes.

2 Literature

This paper relates most closely to the empirical literature on the effect of procurement design, in particular buyer discretion, on various outcomes of the procurement process. Coviello, Guglielmo, and Spagnolo (2017) study procurement in Italy with 2000-2005 data and find that increased buyer discretion on average improves the functioning of the procurement system. In contrast, Baltrunaite et al. (2021) find that an increase in bureaucrat discretion in Italy increased the rents of politically well-connected and less efficient firms in data from 2009-2013. This paper is the first to study the economic channels of crisis urgency and increase of buyer discretion on cross-border awards. Our analysis is not limited to contracts around specific thresholds and contributes a broad, European overview.

An older strand of the literature studies how misallocation in procurement can occur due to favoritism in auctions (McAfee and McMillan, 1989; Laffont and Tirole, 1991), outright corruption (Burguet, 2017), or "buy national" policies (Cernat and Kutlina-Dimitrova, 2015). Allocation of procurement contracts in the EU is heavily skewed towards domestic firms (Vagstad, 1995), indicating

strong discrimination against foreign firms. Based on an empirical analysis of procurement in 29 OECD countries, Hessami (2014) documents misallocation in procurement due to political corruption and rent-seeking behavior in OECD countries.

We call this skew towards domestic firms "home bias", which need not arise from corruption, but which can also be a deliberate policy choice.[9] Procurement processes can encourage home bias to promote small-and-medium-sized firms, jobs, growth, or innovation. Dynamic considerations can justify home bias economically (Barbosa and P. C. Boyer, 2012). Laffont and Tirole (1991) suggest that the share of cross-border awards can serve as a simple screen for favoritism. They specifically analyze collusion of domestic buyers and firms against the government and conclude that under some conditions a "law of large numbers" should hold for awards to the disfavored group. We study this outcome empirically in the context of European medical supplies.

In spite of its richness and accessibility, few articles have used procurement data published on TED. Prier, Prysmakova, and McCue (2018) describe a consolidated data set published by TED for the years 2009-2015. La Cour and Ølykke (2018) find that data completeness differs depending on the country submitting tender information. La Cour and Milhøj (2013) attempt a mainly statistical exploration of the data on Danish contracts. Similar to our study, Kutlina-Dimitrova and Lakatos (2016) use data for cross-border contract awards in Europe and argue that strong product market regulation may act as a hidden anticompetitive barrier. Carboni, Iossa, and Mattera (2018) also discuss empirical methods to study discrimination of foreign firms in procurement.[10] We discuss the issues raised by previous scholars with regards to our data set for sample selection and identification. Although missing values are common in our data set, they seem uncorrelated with the award of contracts to foreign or domestic firms. This is the first paper to our knowledge that uses TED data to investigate procurement in the context of the Covid-19 pandemic and creates an original data set from individual contract award notices, rather than using the consolidated data sets which are only published with a lag of several years.

So far, the effect of Covid-19 on procurement has been studied by legal scholars (Lalliot and Yukins, 2020; Sanchez-Graells, 2020) while (Hoekman et al., 2021) focuses on the trade dimension and the implications for public-private partnerships in the global medical industry (Casady and Baxter, 2020; Vecchi, Cusumano, and E. J. Boyer, 2020). These early responses to the pandemic lay out the policy challenges. By studying empirically the outcomes of procurement for medical supplies during the Covid-19 pandemic, this paper helps to quantify these concerns and to draw lessons for the future.

3 Data

We briefly describe each data set here. Additional details can be found in Appendix A.

3.1 Administrative data on contract award notices

We obtain data on contract award notices (CAN) for public procurement 2018-2020 in Europe from "TED: tenders electronic daily, Supplement to the Official Journal of the EU" (TED).[11] According to its website, "TED publishes 746 thousand procurement award notices a year, including 235 thousand calls for tenders which are worth approximately €545 billion." Each CAN describes the outcome of a tender, including information on the buyer (name, type and location of the authority), the object (total value of the procurement, product category), and, where applicable, the division of the contract into individual lots.

For each lot, the CAN reports the number of companies that bid for the contract (bidders), as well as the number of bidders that are foreign, non-EU, and small-and-medium-sized firms. The bidder

[9] The European Commission claims that "the public sector can use procurement to boost jobs, growth and investment, and to create an economy that is more innovative, resource and energy efficient, and socially-inclusive" (https://ec.europa.eu/growth/single-market/public-procurement_en). Loader (2007) writes that "UK government and small firms believe that public sector procurement is a good way of helping to support small business". See also Loader (2016) on preferencing for small-and-medium-sized firms.

[10] This is also the subject of a study commissioned by the European Commission using TED data (Ramboll/HTW Chur, 2011).

[11] https://ted.europa.eu

winning the lot, the contractor, is listed with its location as well as an initial estimate and final reported value of the award.

The unit of observation is a separate contract award to a contractor. We study the cross-section of awards as there are too few repeat observations for firms and buyers to create a panel. A contract award is defined by a contract date and a contractor and may represent an entire contract or just a contract lot.

3.2 Administrative data on regional infection rates for Covid-19

We combine this procurement data with data on regional infection rates from the European Centre for Disease Prevention and Control (ECDC).[12] These are reported as average cases per 100,000 inhabitants over a 14-day period. Regions are listed by NUTS2-code.

3.3 Population data

We take population data at national and regional (by NUTS2-code) levels from Eurostat for a robustness check for which we compute average national infection rates excluding individual regions.

3.4 Exchange rates

Non-Euro currencies are converted to Euro using data from the European Central Bank (supplemented in very few cases by online sources). For details we again refer to Appendix A.

3.5 Sample selection

We drop entries with incomplete data or token values. Especially the largest countries often do not report the total values of procurement contracts or only report token values (such as 1 EUR) which we treat as missing. Of the 295 documents for medical supplies where no total value is reported (out of a total of 8,054 CAN that remain after dropping failed tenders from the original 9,322 CAN), 75 percent have buyers from Germany, France, and the UK. This is consistent with differences in reporting described by previous researchers (see Section 2).

We restrict our sample to contract awards by national governments and public authorities (excluding EU bodies) from 2018 - 2020 with contract values reported. Thus, we obtain a regression data set of 67,638 observations of individual awards for medical supplies.[13] These contracts were designated by TED as related to the Covid-19 pandemic. Based on belonging to the same larger product groups, we identify another 252,575 observations which are used as the control group in Section 4.4. These are listed in detail in Table 7.

3.6 Descriptive statistics

Tables 1 and 2 present summary statistics for Covid-19-related tenders and the control group of other medical supplies, respectively. Further information, graphs and tables on contract awards are in Appendix B with brief summaries of the main findings below. The most important findings of this section are that the value of cross-border awards increases from 1.5% on average pre-pandemic to more than 50% between April and June 2020 and that a majority of tenders in April and May 2020 were direct awards to foreign sellers.

Spending patterns: Contract awards in our sample are mostly worth between 1,000 and 10,000 EUR (Figure 8). Figures 9 and 10 compare spending before and during the pandemic for different countries. Monthly spending showing similar patterns is reported separately in Appendix B. Table 8 summarizes the total spending on medical supplies in Euro equivalent at prevailing exchange rates without VAT. It is computed from the total value of tenders in our data published between February and December 2020, measured at the document-level. This reflects the categorization by TED for Covid-19-related tenders. It is apparent that the amount procured by a country is not just a function

[12] https://qap.ecdc.europa.eu/public/extensions/COVID-19/COVID-19.html

[13] The number of observations exceeds the number of CAN because one CAN may include several individual lots.

Table 1: Descriptive statistics of tenders for Covid-19-related product groups, n = 67,638

	Mean	Std. dev.	Minimum	Median	Maximum
Lot value (excluding VAT) in EUR	137,115.50	3,640,437.11	0.02	1,999.87	420,499,749
Indicator: domestic award	0.99	0.08	0	1	1
14-day average infection rate per 100 inhabitants	0.99	0.08	0	1	1
Infection rate at seller location	0.03	0.12	0	0	2.35
Total number of bidders	5.00	10.52	1	2	350
Share of foreign bidders	0.00	0.06	0	0	1

Table 2: Descriptive statistics of tenders for product groups in the control group, n = 252,575

	Mean	Std. dev.	Minimum	Median	Maximum
Lot value (excluding VAT) in EUR	162,292.00	7,076,075.20	0.07	1,380.92	1,990,000,000
Indicator: domestic award	0.99	0.07	0	1	1
14-day average infection rate per 100 inhabitants	0.02	0.10	0	0	1.87
Infection rate at seller location	0.02	0.10	0	0	1.87
Total number of bidders	4.18	7.26	1	3	350
Share of foreign bidders	0.00	0.06	0	0	1

of the size of its economy or population.[14] The five countries in our data set that procured the greatest amount during the pandemic are the United Kingdom (UK), Ireland, Germany, Norway and Romania. In both tables, we find that the UK represents a large amount of spending. This is due to the report of some exceptionally large contracts of goods from China and Hong Kong. The ten largest individual lots are reported in Table 10. As a result, to avoid one country from impacting the estimation by too much, we conduct a robustness check by leaving one country out at a time.

Finally, there are purchases completely outside the EU procurement system that do not appear in the data set. For example, the German "open house" purchase of face masks in March 2020 accepted face masks from all sellers who committed to a minimum delivery amount. According to media reports, circa a billion face masks at a unit price of 4.50 EUR were ordered.[15] While the contract notice is posted on TED, no CAN exist. In contrast with traditional contract awards, all firms that fulfilled certain conditions could deliver goods at the posted price.[16]

Net change in cross-border awards: Our outcome of interest is the share of contracts that were awarded cross-border. We define an indicator variable that takes the value 1 if buyer and seller are located in the same country and 0 if not. At the start of the pandemic, we observe a sudden and drastic increase in the value cross-border awards.[17] Figure 1 shows the average monthly share of domestic procurement. The overall level of domestic procurement before the pandemic is consistent with previous studies on overall European procurement in Vagstad (1995).[18]

[14] An economic explanation is that countries with a well-funded healthcare system may already possess large stocks of commodities such as disinfectant or PPE and need to procure less in an emergency than countries with an under-funded healthcare system. However, we do not observe such stocks and in any case this would only represent a part of the spending. The overall decisions to procure medical supplies also depend on the incentives of buyers (e.g. differences in the funding of the healthcare system between countries, Beveridge vs. Bismarck systems) and differences in capacity planning for goods such as intensive care units (ICU) or medical labs (we do observe construction services for both ICU and medical labs in our data).

[15] See Tagesschau, 16.05.2021: https://www.tagesschau.de/investigativ/ndr-wdr/open-house-101.html [in German]

[16] See https://ted.europa.eu/udl?uri=TED:NOTICE:147548-2020:TEXT:EN:HTML&tabId=1

[17] We do not observe local subsidiaries of foreign firms or foreign value-added to contracts awarded to domestic firms, e.g., through traded inputs or sub-contracting. Accounting for these alternative channels would result in a larger value share to foreign companies. See e.g. Cernat and Kutlina-Dimitrova (2015).

[18] Before the pandemic, buyers awarded over 90% of total contract value domestically. Over 99% of all contracts are

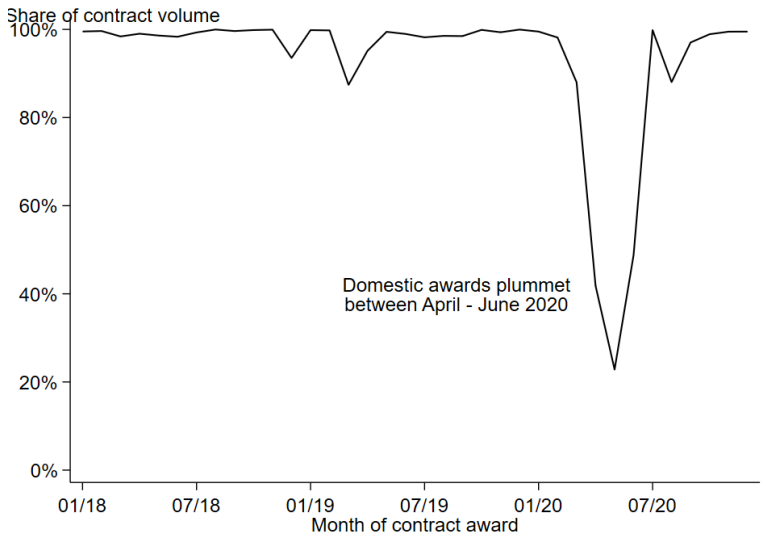

Figure 1: Share of contract value awarded to domestic firms

Figure 1 shows that in a sudden reversal, the volume of domestic procurement dips in April 2020. Less than 50% of contract volume were awarded to domestic companies between April and June 2020, reaching a low of 22.8% in May 2020. Purchases in several European countries, including France, the UK, and Italy, contribute to this dip. Countries that received a large number of foreign orders include large non-European trade partners of several European countries, including the US and China, and within Europe Switzerland and Austria. Overall, the value of domestic contract awards are 98.5% of the total in the pre-pandemic period from January 2018 to January 2020 and 60.3% in the pandemic period from February to December 2020.

Competition and direct awards: We observe less competition for contract awards at the start of the pandemic. Figure 2 describes the share of the total value that buyers awarded competitively in each month. We define competitive contract awards as having more than one bidder and not being categorized as "Contract awards without prior publication" or "Negotiated without a prior call for competition". Buyers awarded over 90% of contract volume non-competitively in April and May 2020. Direct awards to foreign sellers represent 56.5% and 77.1% of the total observed contract volume in these two months. While the presence of several bidders does not guarantee a competitive auction, it is noteworthy that buyers placed many contracts directly with foreign sellers. This suggests that search frictions, such as a lack of awareness of buyers about foreign sellers or of firms about foreign procurement tenders, are unlikely to explain home bias absent the pandemic.

Explanatory variables: One of our main explanatory variables is the average 14-day infection rate per 100 inhabitants. The overall shape of the infection rate in our data set is plotted in Figure 3.[19] We average the infection rate by months, weighting regions by their monthly contract value in our data. This curve tracks closely the total European infection rate reported by the ECDC with a first wave in early 2020 and a much higher second wave in late 2020, as plotted in Figure 4. The ECDC figure contains weekly data separate by country. Although there are some differences, such as a slightly steeper drop in infection rates towards the end of the sample period, this comparison suggests that the weight of different European regions in our data indeed tracks the general trend of the pandemic.

The variable "Regulation change" is a dummy that takes the value 1 for contracts signed after April 1, 2020, the publication date of the "European Commission guidelines on procurement" and 0 before. It always takes the value 0 in the control group. The regulation changes broadly increased buyer discretion. We test the hypothesis that following the publication of these guidelines, as buyers had more discretion in awarding contracts, the share of cross-border procurement and home bias increased.

We investigate missing data and sample selection in Appendix B.1. Procurement documents are

awarded domestically, which implies that cross-border awards are of higher value on average.

[19]Scaled down by a factor of 1,000 from the original data for readable regression coefficients.

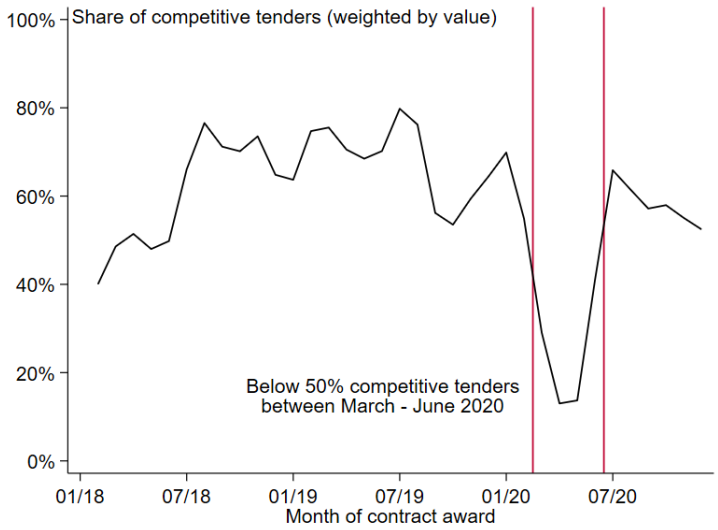

Figure 2: Share of competitive tenders (moving average)

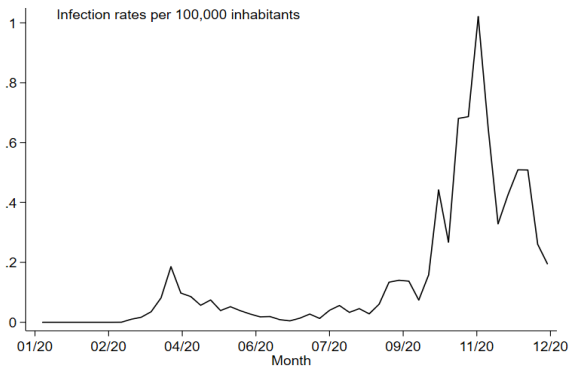

Figure 3: Monthly averages of the 14-day infection rate per 100k inhabitants in 2020 (regions weighted by purchase value).

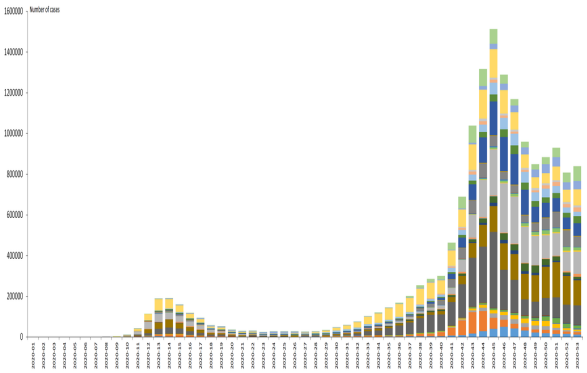

Figure 4: Weekly European number of cases by country, (c) 2021 European Centre for Disease Prevention and Control.

sometimes incomplete, leading to missing values. These values are most likely not missing at random, yet a regression analysis shows that they likely just result in a downward bias of regression estimates, leaving us with a lower bound on possible effect sizes. Also, contracts below an administrative notification threshold don't have to be reported. Contracts that should normally fall above a notification threshold can be split or shaded intentionally to fall below the threshold and go unreported. While we cannot exclude the possibility that this also affects tenders for medical supplies, we do not think that they impact our estimates. We would only be concerned about systematic differences in contract size between times of high cross-border awards and other periods. The main period of high cross-border awards, April to June 2020, is not obviously anomalous from visual inspection beyond usual fluctuations. There is also no significant seasonal variation in the main outcome or independent variables.

3.7 Summary of the data

We document heterogeneous purchasing patterns across countries and product-groups, also with regards to purchases during vs. before the pandemic. Also, lot values are highly dispersed and range from small purchases to multi-million Euro contracts. The largest cross-border contracts include several purchases of PPE from the UK, but also from other European countries, which is why we conduct a leave-one-out robustness check by country. Descriptive statistics show that the net effect of the pandemic and deregulation was a temporary surge in cross-border procurement. Direct awards to foreign sellers represent a high share of the total observed contract volume, especially in April and May 2020. While the presence of many bidders does not guarantee a competitive auction, it is noteworthy that buyers placed most contracts directly with foreign sellers in these months. This suggests that there is no lack of mutual awareness of buyers and sellers across borders driving home bias in normal times.

4 Estimating the impact of the emergency and buyer discretion

We estimate economically large effects of emergency (measured through local infection rates) as well as increased buyer discretion (measured through a change in regulation) on the likelihood of cross-border procurement. We first analyze medical supplies that are designated as Covid-19 related. Using local infection rates as a treatment for a staggered difference-in-difference analysis, we find an increase in the likelihood of a cross-border award by 19.3 percentage points following a one-standard deviation increase in infection rates.

The contracts in the first analysis were all subject to deregulation that increased buyer discretion. For a second analysis, we draw upon additional data for similar products from the same product categories but for which regulations were not lifted. We find that the likelihood of a cross-border award increased by 35.7 percentage points for products and services for which the regulation was lifted relative to the control group. However, each analysis finds a large increase in cross-border procurement compared to the pre-pandemic baseline when only 1.5 percent of the total value was awarded internationally. Even the lower bound effect sizes in a "leave-one-out" analysis remain economically very large, at 9.3 and 16 percentage points, respectively.[20]

4.1 Empirical strategy

What would be the ideal experiment to causally identify the effect of increased *buyer discretion*? Consider a counterfactual Europe with identical infection rates but without the lifting of regulation. Comparing this to our data would allow a direct causal interpretation of the impact of regulation. Alternatively, with complete information on the bidding history of all firms, we could analyze firms' likelihood of winning bids (conditional on their bid and other bids) around the regulation change. This data is not available, as individual bids are not released, nor is the identity of non-winning bidders known. It is apparent that the ideal experiment does not exist: non-European procurement markets,

[20]We emphasize that the two analyses have different treatment and control groups and effects are not like-for-like or additive.

such as in Asia or the United States enacted their own regulatory responses to the pandemic. Both the pandemic and policy responses evolved at different speeds in different parts of the world.

A potential challenge for our analysis of infection rates as a measure of *crisis urgency* could be that the outcome variable impacts infection rates. That is, less misallocation and better procurement might, for some reason, directly result in lower infection rates. However, we are exploiting variation in infection rates within country-month-product group brackets, so any impact of procurement on infection rates would have to occur within the same month to create this kind of endogeneity. The effects of emergency and the regulation are robust to an alternative approach to measuring emergency that specifically circumvents such potential feedback effects: the infection rate in other regions of the same country (computed via population statistics and national infection rates, see Table 15). However, these measures result in a noisier estimation than the main estimation, so we only consider them as robustness checks.

To allow companies across Europe to compete on a level playing field, there are procedural restraints (open and competitive tenders, transparent award criteria, review by external authorities). These procedural restraints were lifted in an announcement by the European Commission on April 1st, 2020, two months after the start of the pandemic.[21] From that point on, buyers could award contracts directly and as fast as possible. The "Guidance from the European Commission on using the public procurement framework in the emergency situation related to the COVID-19 crisis" (2020/C 108 I/01) states:

> "for a situation such as the current COVID-19 crisis which presents an extreme and unforeseeable urgency, the EU directives do not contain procedural constraints. [...] [P]ublic buyers may negotiate directly with potential contractor(s) and there are no publication requirements, no time limits, no minimum number of candidates to be consulted, or other procedural requirements. No procedural steps are regulated at EU level. In practice, this means that authorities can act as quickly as is technically/physically feasible – and the procedure may constitute a de facto direct award only subject to physical/technical constraints related to the actual availability and speed of delivery."

The use of these expedited rules is also mentioned directly in some procurement documents. Generally, these rules apply only to tenders for goods and services that are directly related to the pandemic, not to procurement overall. We note that this communication from the European Commission was not a change in hard law. It merely clarified how to use the procurement framework. However, verbal notes to procurement contracts refer explicitly to the use of expedited rules under the conditions of the pandemic.[22]

We present three distinct regression analyzes. In the first step, we analyze the data set of procurement tenders for medical supplies using a naive OLS regression and logit. This serves as an alternative way of summarizing the data displayed graphically in Section 3. The effects of deregulation and infection rates admit a causal interpretation only if infection rates and regulation were randomly assigned. This is clearly not the case: infection rates are endogenous, as they depend on

[21] The WHO declared the novel Coronavirus outbreak a public health emergency of international concern (PHEIC), WHO's highest level of alarm, on January 30, 2020, see https://www.who.int/emergencies/diseases/novel-coronavirus-2019/interactive-timeline.

[22] One example is contract award notice 600222-2020, a British purchase of PPE from Austria, concluded on May 30, 2020. It says "In March the NHS experienced severe shortages of PPE [...]. In these circumstances, a procurement following the usual timescales under the PCR 2015, including accelerated options, was impossible. PPE manufacturers and supply chains were under immediate and unprecedented global pressure to provide products. A delay in engaging with the market by running a usual procurement process ran the risk of failing to acquire the necessary stock of PPE equipment and presenting a significant risk to life. The Department is content the tests permitting use of the negotiated procedure without prior publication (Regulation 32(2)(c)) are met: 1) The purchasing of PPE was identified as strictly necessary to meet anticipated demand. 2) It is responding to Covid-19 immediately because of public health risks presenting a genuine emergency. 3) The events that led to the need for extreme urgency were unforeseeable: the Commission itself confirmed: 'The current coronavirus crisis presents an extreme and unforeseeable urgency – precisely for such a situation our European rules enable public buyers to buy within a matter of days, even hours, if necessary.' 4) There was no time to run an accelerated procurement under the open, restricted or competitive procedures with negotiation that would secure products within the required timescales. 5) The situation is not attributable to the contracting authority: It has not done anything to cause or contribute to the need for extreme urgency."

previous infection rates, infection rates in neighboring regions, and factors that are correlated with factors such as the efficiency of the public sector or existing trade links. Changes in infection rates might not be random, as they might be influenced, e.g., government quality which might impact the likelihood of cross-border procurement and influence infection rates.

The second step focuses on the identification of the emergency effect of the pandemic. Using the data set on medical supplies, we consider local infection rates as a treatment in a difference-in-differences setup. The third part of the regression analysis focuses on the regulation effect. To this end, we use a data set of contracts in product groups which are adjacent to the previously analyzed products. As these fall outside the classification as "Covid-19 related" they were therefore unaffected by deregulation. Here, the regulation action on April 1st, 2020, provides a treatment that only affects the tenders for certain medical supplies, which allows us to identify a control group of similar products for which the procurement rules were unaffected. We show that trends in procurement for these products are indeed comparable to our treatment group.

4.2 Naive estimation

The naive estimation via linear regression on the data set of medical supplies serves primarily as a statistical summary of the data set and secondarily as a benchmark for the remaining analyzes. Our outcome variable in this and future regressions is an indicator variable y_{it} that takes the value 1 if the contractor is located in the same country as the buyer and 0 otherwise. Our main regression equation is

$$y_{it} = \beta_1 I_{it} + \vec{\beta}_x X_i + \vec{\beta}_d \delta_{it} + \epsilon_{it} \tag{1}$$

where I_{it} is the infection rate of the region where contract i is performed,[23] X_i is a vector of contract-specific control variables, the infection rate at the seller location, the share of foreign bidders, the total number of bidders, and the number of bidders that are small and medium-sized firms. δ_{it} is a vector of dummies for the product category, year-month, and country and $\vec{\beta}_d$ is the vector of the associated coefficients.[24]

We are interested in the effect on the value awarded abroad, so each observation is weighted by the value of the award. Thus we avoid distortion from potentially arbitrary divisions of contracts into more lots. We use White's heteroskedasticity-robust standard errors to account for the well-known fact that linear regressions in a binary outcome framework have heteroskedastic residuals. The results of this regression are reported in column 1 of Table 3. Additionally, we test non-linear functions of the infection rate as alternative measures of crisis intensity: we include a squared term for the infection rate in column 2, and use the natural logarithm of the infection rate in column 3. The ease of interpreting these alternative, non-linear transformations of the infection rate is the main reason for the use of a linear probability model.

The results suggest that an increase in the infection rate at the location of contract performance is strongly correlated with a decreased probability of a domestic award, while higher infection rates at seller location is correlated with a higher likelihood of a domestic award. The former effect is likely nonlinear, as shown by the statistically significant coefficients on the linear and quadratic term of the infection rate in column 2. Domestic contract awards are correlated with the total number of bidders, but not the share of foreign bidders. As these are merely correlations, we refrain from interpreting them at this point.

We estimate the effect of regulation as a before-after comparison of the month dummies around the date of regulation. We subtract the average of the month-dummies of April-December 2020 from the average of the month-dummies January 2018 - March 2020. The result of this linear hypothesis test is reported below the regression coefficients in Table 3. The effects are negative and large: after regulation, depending on specification, cross-border awards are 20 to 30 percentage points more likely. This is unsurprising given the descriptive data, but doesn't admit a causal interpretation because we cannot rule out trends or other important contemporary factors. This approach is distinct from the

[23] We rescale the 14-day average rate of infection per 100,000 inhabitants published by the ECDC to the rate of infection per 100 inhabitants to improve readability of the coefficient estimates.

[24] The constant is absorbed in the full set of dummies and therefore not required for unbiased estimation.

Table 3: Linear regression for contract award to domestic companies

	Linear model	Squared term	Log-infection rate
Dep. var.: Contract awarded to domestic company			
Buyer infection rate	-1.366***	-2.398***	
	(0.299)	(0.552)	
(Buyer infection rate)^2		0.675**	
		(0.300)	
Log of buyer infection rate			-2.382***
			(0.434)
Total number of bidders	0.004***	0.004***	0.004***
	(0.001)	(0.001)	(0.001)
Share of foreign bidders	-0.040	-0.037	-0.040
	(0.090)	(0.088)	(0.089)
Seller infection rate	2.369***	2.491***	2.517***
	(0.423)	(0.443)	(0.430)
Difference of month dummies pre-/post 04/20	-0.31***	-0.22***	-0.24***
Standard error of regulation effect	0.05	0.06	0.05
Dummies	yes	yes	yes
Adj. R-squared	0.930	0.931	0.931
N	67,638	67,638	67,638

Robust standard errors in parentheses, ** p<0.05, *** p<0.01. Infection rates are 14-day moving average per 100 inhabitants at the NUTS-region reported as performance location (buyer infection rate) or as location of the contractor (seller infection rate). Share of foreign bidders computed as number of foreign bidders from EU and non-EU countries divided by total number of bidders.

difference-in-differences estimation in Section 4.4 which exploits the presence of a control group of similar products that were unaffected by the regulation. Quantitatively, the results are reasonable, however, as we observe only small increases in cross-border procurement in the control group (see Figure 7).

In line with the literature, and appropriately for a binary outcome variable, we estimate a logit model. Results are reported in Appendix B.2. Most effects go in the same direction as in the linear model. Two differences to the linear probability model stand out: the share of foreign bidders is now a strong predictor for foreign awards while the regulation effect and average marginal effects of regulation and infection rates are insignificant. The results are not materially different for common "rare events" logit approaches. This is not surprising, as even though foreign awards are rare relative to domestic procurement, we have several thousand observations of such events. As discussed above, this mainly summarizes the correlation between cross-border awards, deregulation, and infection rates in our data. We proceed to compare these results with the causal effects identified in the following sections.

4.3 Emergency effect

The first research hypothesis concerns the effect of emergency, measured through infection rates, on the likelihood of cross-border awards. Our model in Section 5 motivates the effect of the pandemic on cross-border awards through a change in priorities of the decision-maker: in an emergency, procuring well is of the essence, and the buyers' knowledge about the differences between firms becomes more important than political aspects, such as handing out lucrative contracts to potentially inefficient domestic firms to gain political favors. Measuring the degree of emergency implied by the pandemic through local infection rates, we investigate the impact of emergency on cross-border awards in this section.

We test the hypothesis that greater levels of emergency reduce the extent of misallocation. If the absence of cross-border awards is associated with misallocation, we should observe more cross-border procurement when *crisis urgency* is greater. Note that the model is silent on why or how the govern-

ment values purchases from one or the other firm. Consistent with the descriptive statistics presented in Section 3.6, it is clear, however, that the effect of infection rates on cross-border procurement is, if anything, transitory. The overall plummet in cross-border procurement coincides with the first wave of Covid-19 infections, but not the higher, second wave.

Therefore, the data used in Section 4.3 lends itself to a two-way fixed effects analysis with staggered treatment (Callaway and Sant'Anna, 2020; Athey and Imbens, 2021). A two-way fixed effects analysis generalises the canonical two-period difference-in-differences approach to multiple periods. The characteristic of the staggered treatment is that units are treated at different times. The unit that is treated is not the contract award (our unit of observation) itself, but the geographic region for which the contract is specified. The control group for treated units (regions) consists both of never-treated units and potentially also units that have been treated in the past. This setting also features varying treatment intensity as the treatment variable, infection rates, vary across locations and periods. Wooldridge (2021) shows that also with a staggered treatment, our estimator is equivalent to a two-way fixed effects estimator while accounting for treatment intensity, covariates and interactions.

Our context differs from a standard setup of the staggered difference-in-difference estimator first because of the binary outcome variable. Both linear probability models and non-linear alternatives such as probit are available and have been used in these cases (Finkelstein, 2002) although the interpretation of the treatment effect in the non-linear model is not straightforward (Puhani, 2012). We refrain from repeating the exercise with a non-linear model as our focus is on the demonstration of an effect of considerable economic size, rather than a precise estimation of magnitude.

Difference-in-differences diagnostics: We start by investigating the standard "parallel trends" assumption and potential spillover in treatment or outcomes. A standard approach to demonstrate parallel trends is to plot control and treatment group around the threshold and visually confirm that their trends are approximately parallel. Here, this is difficult due to the large number of regions with distinct infection profiles. We therefore aggregate regions into two groups depending on the date when infections were first recorded.

We observe the following weeks as weeks of first infection in our data: the weeks starting on January 20, 27, February 3, 17, 24, and March 9, 16. We group all observations by whether the associated region was among those first infected early or late. For easier visualization, we designate regions that first experienced infections in the weeks starting January 20 or 27 as "early" and the rest as "late". Figure 5 compares the evolution of the share of domestic purchases and the average infection rates.

The figure shows differences with two visible spikes in cross-border awards for the "early" group and a spike in-between for the "late" group. Looking at the flat section of the graphs in Figure 6 until the beginning of April, it is also clear that both groups do not experience a fundamentally different development in the infection numbers for the first few weeks. This strengthens the case for areas that were infected a few weeks later as a control group for those that were infected earlier. In both figures, two red vertical lines mark the weeks beginning January 20 and 27, as a marker of the "early infection" group.

Estimation results: Based on these considerations, we estimate the following baseline equation which permits an interpretation of the effect of infection rates on cross-border awards.

$$y_{it} = \alpha_i + \lambda_t + \beta I_{it} + \vec{\beta} X_{it} + \epsilon_{it} \tag{2}$$

α_i denotes a contract's performance location, λ_t is the week. Recall that the indicator variable y_{it} takes the value 1 if the contractor is located in the same country as the buyer and 0 otherwise. I_{it} is the infection rate at the buyer's location (more precisely, the infection of the region which is identified for the location of contract performance) for observation i. X_i is a vector of control variables, the infection rate at the seller location, the share of foreign bidders, the total number of bidders, and dummies for country and product-group. The results are summarized in Table 4.

The estimated average treatment effect on the treated (ATET) is for a unit increase of the 14-day average infection rate per 100 inhabitants. It is best interpreted relative to observed standard deviations during the pandemic. We compute the standard deviation of the infection rate for observations

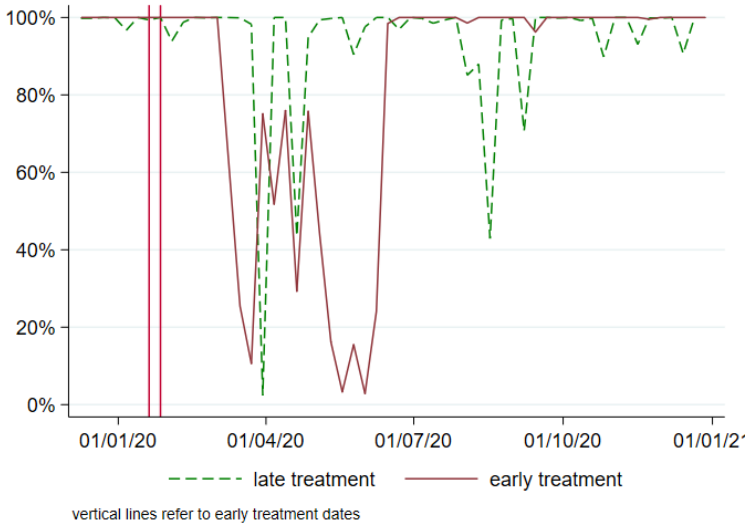

Figure 5: Share of domestic purchases in regions infected early/late

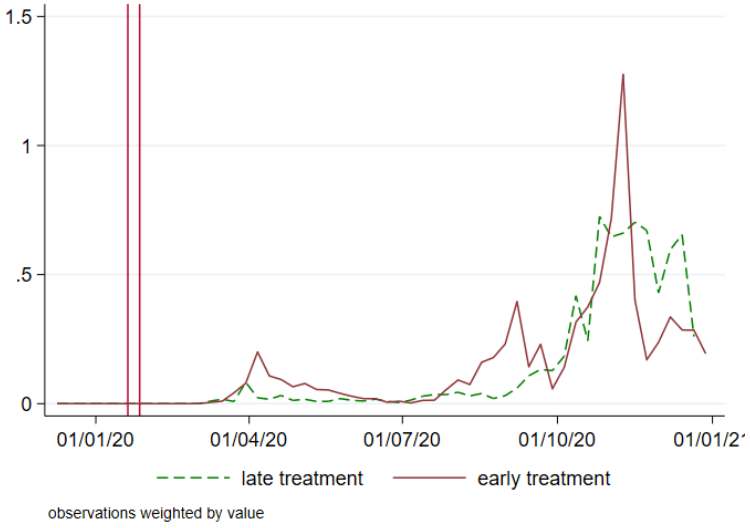

Figure 6: Average infection rates per 100 inhabitants in regions infected early/late

with non-zero infection rates.[25] In column one, we use all observations for medical supplies where the lot value is not missing. In the whole sample, the effect of a one-standard deviation increase in infection rates is an increase of 19.3 percentage points in the likelihood of a cross-border award.

Due to heterogeneity between countries and the presence of outliers, we conduct a "leave-one-out" analysis in which we exclude one country at a time. For all of these tests, the effect remains statistically significant and when leaving out Czech Republic, it even increases by about 50% relative to the full sample. Unsurprisingly in light of the previously documented size of orders from the UK, leaving out the UK diminishes the coefficient the most. Even this case, however, we find that a one-standard deviation increase in infections increases the likelihood of a cross-border awards by 9.3 percentage points (see column 2 of Table 4). Given that cross-border contracts only represent 1.5 percent of value pre-pandemic (see Figure 1), even an increase by more than 9 percentage points is economically large.

Spillover might occur if the treatment status of some units influences the treatment of other units. In the context of a pandemic, this is clearly a concern as our treatment literally spreads and is transmitted by people moving between regions. However, whether this spread of infections also influences procurement decisions is less clear, as buyers should react to local infection rates

[25] Otherwise the standard deviation of the infection rate would be artificially diminished by pre-pandemic observations.

Table 4: Difference-in-difference analysis with staggered treatment through infection rates

	All obs.	No UK
Dep. var.: Contract awarded to domestic company		
ATET		
14-day average infection rate per 100	-1.376***	-0.463***
	(0.318)	(0.127)
Controls		
Infection rate at seller location	2.242***	0.529***
	(0.400)	(0.183)
Total number of bidders	0.002***	0.000
	(0.001)	(0.000)
Share of foreign bidders	-0.363***	-0.428***
	(0.100)	(0.061)
Standard error of infection rate	0.14	0.20
1-std.dev. increase	-0.193	-0.093
Dummies	yes	yes
N	67,638	67,387

Robust standard errors in parentheses, *** p<0.01. Infection rates are 14-day moving average per 100 inhabitants at the NUTS-region reported as performance location (buyer infection rate) or as location of the contractor (seller infection rate). Share of foreign bidders computed as number of foreign bidders from EU and non-EU countries divided by total number of bidders.

independent of whether local infections arise because of movement between regions or infections within a region. More importantly, are buyers likely influenced by information from other regions when making purchasing decisions? Do infections in other regions provide additional information beyond what local (lagged) infection rates predict?

To test this, for each region we test for Granger-causality of infection rates against own lags and lags of other regions in the same country (see Appendix B.3). Allowing for up to six lags of the weekly updated infection data, we cannot reject the Null-hypothesis that the infection rates in one region are not Granger-caused by infection rates in other regions (see Table 13).[26]

We also test for spillover in outcomes (Table 14 in Appendix B.3): Does treatment status by one region impact outcomes in another region? Using data on population and infection rates, we compute for every region and every week the national infection rate excluding that region. In other words, we use a national measure of infection rates that excludes the contract location. Note that this measure is strongly correlated with local infection rates: this tells us that most regions at most times did not diverge too far from national trends in the infection rate. Still, even in the small subset of observations (842 observations representing ca. 45 million Euros worth of contracts) where local infections were below the overall median infection rate while national rates were above, we do not find a significant impact of the national infection rate on outcomes.

4.4 Suspension of regulation

As described in Section 4.1, the European Commission published guidance on using the procurement framework in which the normally very restrictive rules were widely suspended. We argue that these suspensions increased buyer discretion, which previous scholars identified as a potential source of better procurement and misallocation (Section 2). This hypothesis follows also from our theoretical model (Section 5). We want to test whether the suspension of these rules led to additional cross-border procurement. Previous scholarship identified buyer discretion as a potential source of misallocation,

[26]We flexibly allow for up to nine lags and then choose the model based on Bayesian Information Criterion. Allowing for longer or shorter lag structures, e.g. six weeks or twelve weeks of past information, yields the same result.

but also as a potential source of efficiency.

The European Commission has confirmed that these rules were indeed applied restrictively to Covid-related tenders. We leverage the TED classification of Covid-related tenders to identify those products that fell under these rules. Then, we select a control group from all product groups of the CPV that are at the same hierarchy level as the Covid-related tenders. Thus, we obtain a selection of closely related products.

For example, from the category "beds for medical use" (33192100), the product group "hospital beds" (33192120) is Covid-related and all contract award notices 2018-2020 are part of the treatment group. Contract award notices for the remaining products in the category "beds for medical use", namely "orthopaedic beds" (33192110), "motorised beds" (33192130), "psychiatric couches" (33192140), "therapy beds" (33192150), and "stretchers" (33192160), enter the control group. A complete list of relevant product groups is provided in Table 7.

For our regression analysis, we assume that products in the control group are similar enough to the products in the treatment group. This could mean that for a contractor, there is a high degree of supply-side substitutability between, say, "intensive-care unit construction work" (treatment group) and "diagnostic screening room construction work" (control group). It could also mean that, even if different firms produce "protective gear" (treatment group) on the one hand and "work gloves" and "safety visors" (control group) on the other hand, changes in market conditions and input costs affect these firms similarly.

Difference-in-differences diagnostics: We construct a control group based on product similarity in the product classification system. [27]

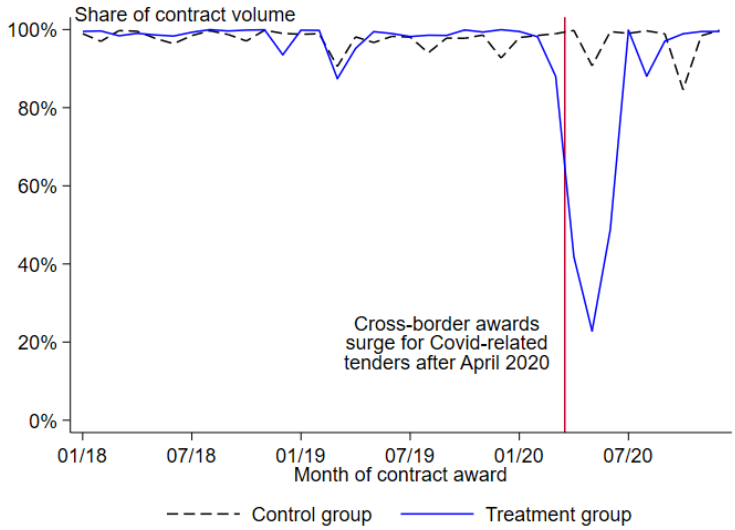

Figure 7: Average share of cross-border awards in the treatment group with greater buyer discretion and the control group

Figure 7 shows the average monthly rates of cross-border procurement for the products in the treatment and control group. A slightly lower share of domestic awards already in March suggests some anticipation effects. This is unsurprising given the soft nature of the suspension of buyer discretion: instead of representing a change in "hard law", it is a clarification on the interpretation of existing rules. Nonetheless, a clear impact of the regulation on the impacted goods, but not similar goods can be seen. At the same time, the graph shows broadly parallel trends pre-April 2020, save for a handful of spikes in individual months.

One possible concern is that the two sets of products are not comparable due to a surge in demand

[27] A similar method has been used by Eurostat, the statistical agency of the EU, to analyze trends in international trade related to Covid. Based on the trade statistics in their Comext data base, they compare Covid-19 specific product groups with other, similar product groups which are selected by chapters of the "Harmonised System" product classification. See Eurostat, 31.03.2021: https://ec.europa.eu/eurostat/statistics-explained/index.php?title=EU_trade_in_COVID-19_related_products

for the products in the treatment group, which might not carry over to the products in the control group. We should be worried if total purchases for products in the control group remained constant while purchases in the treatment group spiked. We already mentioned that average monthly purchases by product group are reported in Figures 15 and 16. For comparison, the average monthly purchases of the 20 largest product groups in the control group are shown in Figures 17 and 18.

For most product groups, even in the control group the average monthly spending during the pandemic exceeds spending before the pandemic by a large amount. Only in two product groups (albeit including the largest by spending, "various medicinal products" alongside "antineoplastic agents"), do we observe slightly lower spending during the pandemic. For most product groups both in the control and treatment group, average spending during the pandemic exceeds average spending before the pandemic, sometimes by an order of magnitude. This suggests that differences in cross-border procurement between goods in the treatment and control group are likely not driven by an uneven spike in demand that would only affect the treatment group.

Estimation results: Finally, we estimate the following equation

$$y_{it} = \tau_i + \lambda_t + \beta D_{it} + \vec{\beta} X_{it} + \epsilon_{it} \tag{3}$$

The regression equation has a few differences with equation 2. τ_i now denotes whether a contract awards belongs to the treatment or control group, the set of control variables does not include product group dummies (to avoid co-linearity with treatment group status), and D_{it} takes the value 1 for observations in the treatment group after the treatment date (April 1st, 2020). All treated units are now treated at the same date. Again, we conduct a leave-one-out analysis by country to investigate the importance of outliers.

Our estimation results are reported in Table 3. The baseline result on the full sample indicates that the increase in buyer discretion increased the share of cross-border awards for Covid-19 related tenders by circa 36 percentage points. We treat the smallest absolute regression coefficient in the leave-one-out analysis as a lower bound of this effect. This value is only 16 percentage points when the UK is removed, reflecting large cross-border contracts from the UK. The overall results are unsurprising, given the size of the drop in cross-border procurement, as well as the visually clear difference between the treatment group and the control group in Figure 7.

Table 5: Difference-in-difference analysis with targeted deregulation

	All obs.	No UK
Dep. var.: Contract awarded to domestic company		
ATET		
Regulation change	-0.357***	-0.160***
	(0.055)	(0.061)
Controls		
Infection rate at seller location	0.394***	0.058*
	(0.121)	(0.033)
Total number of bidders	0.001	0.001***
	(0.000)	(0.000)
Share of foreign bidders	-0.030	-0.504***
	(0.036)	(0.075)
dummies	yes	yes
N	320,213	319,173

Robust standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01. Infection rates are 14-day moving average per 100 inhabitants at the location of the contractor (seller infection rate). Share of foreign bidders computed as number of foreign bidders from EU and non-EU countries divided by total number of bidders.

4.5 Robustness

We consider potential challenges to the empirical strategy, including the impact of the pandemic on the manufacturers of medical supplies, but also the measurement of buyer discretion and emergency. The pandemic has lead to disruptions for manufacturers as well as for distribution and international supply chains which could impact our ability to make inference on awarding practice. For example, a decrease in contract awards to foreign companies could be related to border closures or export restrictions on medical supplies that were applied in 2020.

All companies may face higher costs or lower production capacity due to sick workers, new safety measures, or general uncertainty arising from the circumstances of the pandemic and therefore decide to pursue fewer contracts or only bid on nearby (domestic) contracts. Depending on a firm's industry, production technology and location, rising infection rates can imply

- decreased production capacity, including factory shutdowns, due to worker sickness;
- decreased labor productivity, or increased production cost, due to increased safety standards (e.g. encumbering safety clothing, regular disinfection protocols, maintaining physical distance between workers);
- lack of physical access or uncertain access to raw materials due to interrupted supply chains;
- lack of physical access or uncertain access to buyers due to border shutdowns and export restrictions.

To rule out that the observed contracting practices are driven by these supply-side factors, ideally we would want to control for participation of companies of different sizes. However, we only observe the total number of bidders, and the numbers of bidders from other EU and non-EU countries. The control variables in our main regression, the number of firms bidding and the share of foreign bidders, already account for some of these supply-side effects. However, we also consider effects that would impact foreign awards even after controlling for participation.

Capacity constraints on the supply-side: A buyer might turn to foreign sellers because domestic industries cannot supply the short-term need for all goods and services. Then we would expect an increase in cross-border awards even absent any misallocation. However, even applying a very conservative definition of spare capacity, our findings remain the same.

We identify contracts for medical supplies in which there was excess domestic capacity, identified as a larger number of domestic bidders than domestic winners. Within each contract, we look for the lot with the largest number of domestic bidders and count the distinct number of domestic winners by firm names after cleaning inconsistently spelled firm names. This likely underestimates domestic spare capacities because we only count entire domestic companies that would stand ready to serve a contract, the extensive margin of domestic supply, but we do not observe intra-firm spare capacity, the intensive margin. Our measure is also conservative because bidders are anonymous, so if different firms bid for different lots on a contract, we cannot distinguish between them: by looking for the lot with the largest number of domestic firms, we are just identifying the lower bound of distinct domestic firms ready to supply a certain product at the time of contract award.

The results are reported in Tables 16 and 17. Overall, we restrict our sample for the difference-in-differences analysis by regional infection rates to 23,443 (34.7% of the full sample). The effect of the infection rate remains significant and the effect size of similar magnitude at -19.7 percentage points for a one-standard deviation increase in infections. For the analysis on the regulation effect using a control group of additional products, we use the full control group. Here, the effect size is lower, at -8.8 percentage points post regulation. The coefficients are still significant but less so than in the main analysis. The leave-one-out robustness check does not change the results much, except that is pushes up the standard error for the regulation effect so that it is no longer significant at the 5% level. Overall, the analysis shows that a lack of domestic spare capacity is not a sufficient explanation for the surge in cross-border procurement. Large effects, especially from rising infection rates remain for contracts on which domestic suppliers bid.

Another supply-side concern could arise from changes in the pool of sellers. For example, the increase in domestically awarded contracts could be due to buyers using their increased discretion to purchase via resellers or trading companies. These companies may not qualify for procurement contracts for medical supplies under normal circumstances but might have privileged access to foreign manufacturers. Anecdotally, the German government called upon companies with trading links to China to purchase additional face masks around Easter 2020. While we do not have sufficiently rich firm data to describe composition differences in their industries, we have checked that companies which have as name components "trade", "logistic" or "distribution" are not more common among contractors during the pandemic than before and make up only a small portion of total sales, suggesting that this is not a large concern.

We cannot replicate our results in an unweighted regression. Without weighting, using all observations including those that are missing in the main sample due to missing values, we find no effect of the infection rate and a negative effect of the regulation change, and zero effect of the regulation change and a negative effect of the infection rate when only looking at the main sample (see Table 18). This is unsurprising, as this check is very much driven by two countries whose contract award notices are typically split into many, sometimes very small lots: Romania, representing 63% of all contract awards, and Poland, representing 20% of the absolute number of lots.[28]

Finally, in a subset of countries where we can observe a smooth increase in the sum of failed procurement tenders during the pandemic, we cannot find an impact of failed procurement on contract awards (Table 19). We also test whether correlation between procurement contracts within countries, for example due to similarity in regulatory and legal environments or similarities in training, doctrine, and perspective among public sector buyers, play a role. We re-estimate our difference-in-differences analyzes with country-clustered standard errors, as well as standard errors that are clustered by contract. Both types of clustered standard errors are moderately larger than White-robust standard errors (see Tables 20 and 21), but our estimates remain significant at conventional significance levels. Therefore, they do not change our conclusions regarding the impact of crisis urgency and buyer discretion.

4.6 Discussion

We find that that the overall surge in cross-border awards is driven by two channels: both *crisis urgency* and increased *buyer discretion* lead to an increase in cross-border awards. This does not exclude the possibility that other channels may have an impact, too. At least the result on buyer discretion is surprising as it may in principle also contribute to misallocation.

The effects are statistically significant, even when accounting for across-country heterogeneity, and economically large: the increase in the likelihood of a cross-border award following a one-standard deviation increase in infection rates is over 10 times larger than the baseline rate, and over 20 times in the case of the deregulation. Interpreting this in light of the descriptive statistics and charts we note that the large drop in value awarded to domestic companies coincides with the first, lower wave of infections, and does not repeat itself when infection rates rise much higher during a second wave in late 2020. This suggests that any mitigating effect of urgency on home bias was at best temporary.

While the absence of cross-border procurement is not direct evidence of misallocation, there are good reasons to presume that low shares of cross-border procurement indicate misallocation. Firstly, large variations in cross-border procurement as a result of a change in buyer discretion is inconsistent with a hypothesis of equal treatment. Secondly, a pure cost-based explanation could not explain the drastic surge in cross-border procurement from almost nothing pre-pandemic to more than 50 percent in some months of 2020. As argued above, our results are not driven by a lack of competitive foreign sellers. The magnitude of the observed effects is economically large relative to potential exclusionary effects of, say, fixed costs that might arise from translation of tender documents.

[28] EU regulation suggests that contracts may be aggregated to achieve cost savings, while a division into smaller lots might be motivated by a desire to allow smaller companies to participate, for example in Directive 2014/24/EU, preamble, paragraph 59. However, these competing forces determining contract size and lot division are beyond the scope of the paper.

A model of monitoring in the following section can help us rationalize these findings and explain especially the puzzling effect of buyer discretion. We translate the concept of emergency to a differential in the payoff that the principal (e.g., a government) receives in the case of a collusive or non-collusive outcome. For many repeated purchases, consider this the difference in average payoff of purchasing from the unconditionally best supplier vs. the supplier favored by the buyer in the presence of misallocation. It is plausible that this differential is higher in an emergency due to the health threat posed by the pandemic. Buyer discretion enters the model through the cost of monitoring.

When regulation on buyer discretion decreases these costs and makes the collusive outcome less likely, we expect the lifting of such regulation to result in more misallocation. We see evidence to the contrary in the empirical analysis. The conclusion within the proposed model is that in the context of the pandemic, the lifting of regulation does not increase the cost of monitoring collusion. Indeed, as the media reports cited in the introduction show, misallocation and collusive practices were under great scrutiny. While normally, procurement outcomes rarely make headlines, in the pandemic there was great public interest in the procurement of medical supplies. Therefore, it seems plausible that even though we do know of instances of misallocation, overall buyers were put under more pressure to avoid misallocation, potentially reducing the cost of uncovering and fighting undesirable outcomes. This rationalizes the observed surge in cross-border procurement.

5 Model

This section briefly illustrates the impact of urgency and buyer discretion in a simple model of monitoring. Even though this setting is simplified, it is informative to predict the presence of misallocation in procurement. The key comparative statics include a government's cost of reviewing and punishing collusion and the importance of a procurement agent's information.

The former may represent the time and effort that needs to be expended to review a tender decision, litigate an outcome, and enforce a compensation for foregone benefits. Regulation that limits the discretion of buyers can be understood to directly reduce these costs. For example, when a regulator limits buyer discretion by mandating transparent tenders, prescribing scoring rules, and making the bidding process more transparent, he effectively reduces the cost of verifying violations of such rules.

We think of the value of the agent's information as increasing during an emergency. In particular, consider the multitude of policy objectives regarding procurement, some of which are completely unrelated to each tender (such as promoting jobs or small businesses). We assume that an increase in the urgency of a crisis makes objectives related to the tender relatively more important than secondary policy goals which may not require such information. For example, a good tender decision might require detailed information about the quality the seller, while simply spending money to secure local jobs does not. So, an increase in the urgency of a crisis should increase the importance of the bureaucrat's information. From our model, we predict that an increase in crisis urgency should result in less misallocation.

It is inspired by the mechanism-design models based upon McAfee and McMillan (1989) and Laffont and Tirole (1991). These models concern optimal auction contracts in a principal-agent-firm framework where the agent can collude with the firm (as opposed to collusion among firms). Whereas Laffont and Tirole (1991) is concerned with collusion-proof auctions and how they differ from the first-best, other models generate collusion in equilibrium (Burguet and Che, 2004; Burguet, 2017), or study dynamics and steady-state levels of corruption (Menezes and Monteiro, 2006).

Branco (1994) argues that foreign profits should not enter the utility function of the principal when the principal is a national government. In this case, favoritism may be an efficient outcome also for the principal, although it implies an international coordination problem between governments. This problem has been compared to a prisoner's dilemma where governments "want domestic protectionism and foreign liberalization, but they may prefer mutual liberalization to mutual protectionism" (Rickard and Kono, 2014). Arozamena and Weinschelbaum (2011) offer a different view and suggest that when entry into the auction is important, the principal should not favor a subset of bidders even if it cares about their payoffs, but not about the payoffs of other bidders.

However, these models are not straightforward to take to the data as they often consider different cases, such as verifiable and unverifiable information, often with different implications. This model considers an explanation that Laffont and Tirole (1991) suggest to explain the small fraction of cross-border procurement, a fact which they suggest as a screen for misallocation, but that to our knowledge has not received much attention in this literature: rather than designing the auction himself, the principal might have to contend with reviewing procurement decisions and punishing collusion ex-post.

To be sure, Laffont and Tirole (1991) think of this as a reputation game: concerns about a reputation to "keep their mouths shut" might disincentivize firms that were unfairly treated to blow the whistle on a buyer who discriminates against them to avoid losing whatever rents they were left with. In contrast, we study a simple monitoring game between a bureaucrat who decides whether to bias his decision and a government that decides to review (and punish) to generate predictions relative to our application, without modeling firms explicitly. We propose that the ease of monitoring the buyer and the size of his informational advantage are important channels in this context.

5.1 Setup

Consider a simultaneous game of complete information between a bureaucrat and a government. For a procurement tender, the bureaucrat has to decide whether to bias his decision towards a domestic supplier (cheat, C) or not (NC). We call this "cheat" for brevity, but it represents home bias as a form of misallocation in the widest sense such that not the procurement outcome is prioritized, but some other objective of the bureaucrat, say, because of regulatory capture.

If the bureaucrat plays C, this results in a low-quality product being delivered to the government from which the government obtains a payoff of $\underline{S}$, and the bureaucrat receives a private benefit, or bribe, $B > 0$. If the bureaucrat doesn't cheat, a high-quality product of value $\bar{S} > \underline{S}$ is delivered to the government and the bureaucrat receives a payoff normalized to 0. The government decides to either review the tender (R) or not (NR). If the government plays R, the government imposes a cost of k_G onto itself and k_B onto the bureaucrat. In case of review and if the bureaucrat plays C, the bureaucrat compensates the government for the foregone quality $\bar{S} - \underline{S} = \Delta S$, effectively increasing government payoff to $\bar{S}$ (before subtracting the review cost). This is summarized in Table 6 which depicts a 2x2 matrix in which the government is the row player and receives the first payoff listed in each cell and the bureaucrat is the column player and receives the second payoff listed in each cell.

Table 6: Review-and-collusion game between government and bureaucrat

		Bureaucrat: C	Bureaucrat: NC
Gov't	R	$\bar{S} - k_G$, $B - k_B - \Delta S$	$\bar{S} - k_G$, $-k_B$
	NR	$\underline{S}$, B	$\bar{S}$, 0

We assume two conditions hold to ensure that the game is interesting: $\Delta S > k_G$ ensures that the government has an interest in playing R if the bureaucrat plays C. If k_G is too high, then the government will strictly prefer to play NR whatever the bureaucrat's strategy. The other condition is $\Delta S > B$, the difference in quality must be greater than the benefit, so the bureaucrat prefers NC when the government plays R. If this was not the case and C was the bureaucrat's dominant strategy, the government's best response would be R. If these two conditions hold, we obtain a well-known kind of monitoring game which has no Nash Equilibrium in pure strategies, but only in mixed strategies where the government plays R with some positive probability p and the bureaucrat plays C with some positive probability q.

These probabilities p, q are chosen such that the other player is indifferent between his strategies, so:

$$p(B - k_B - \Delta S) + (1 - p)B = p(-k_B) + (1 - p) \cdot 0 \tag{4}$$

$$q(\bar{S} - k_G) + (1 - q)(\bar{S} - k_G) = q\underline{S} + (1 - q)\bar{S} \tag{5}$$

After some simplification, this is straightforward to solve for

$$p = \frac{B}{\Delta S} \tag{6}$$

$$q = \frac{k_G}{\Delta S} \tag{7}$$

As for comparative statics, it is easy to see that the equilibrium probability p of the government playing R (reviewing a tender) increases with the size of the benefit B and decreases with the difference in the two quality levels ΔS. The equilibrium probability q of the bureaucrat playing C (favoring domestic suppliers) increases with the cost of review and enforcement k_G and decreases with the difference in the two quality levels ΔS.

5.2 Discussion of the results

The comparative statics for equation 7 are measurable within our empirical framework as was suggested in Section 4.6. We understand the lifting of buyer discretion, which we analyze in Section 4, to amount to an increase in k_G. Our model predicts misallocation to become more frequent when k_G increases. However, we have found an effect in the opposite direction. This suggests that in spite of the suspension of rules on buyer discretion, government review cost need not have increased. Indeed, one might argue that scrutiny of procurement of medical supplies was higher than usual, also thanks to the pandemic. The media reports cited in the introduction are an example of how procurement received unusual public attention.

We describe ΔS as the difference between the high quality and the low quality product. Rather than just narrowly describing product quality in the sense of vertically differentiated products, this quality parameter S has been interpreted and described, e.g. by Laffont and Tirole (1991), as the fit of a seller with tender requirements, or more broadly as the value of the information of an agency. This motivates our focus on procurement of medical supplies. In the context of an emergency, such as the Covid-19 pandemic, we can imagine that the value of information on the actual quality of the seller of medical supplies is increased. In Section 3, we measure urgency through the local Covid-19 infection rate, but also consider alternative urgency measures. The empirical analysis confirms the model prediction that an increase in the value of information should decrease misallocation (and also lead to fewer reviews which we do not observe or study, however).

What justifies the assumption that the quality differential is larger than the bribe? If this condition wasn't true, then collusion would be efficient in our model in the sense that it maximizes total surplus. The idea behind the second condition is that the bribing firm cannot offer a bribe that is greater than ΔS which is motivated by the notion of bilateral interim efficiency developed in Laffont and Tirole (1991). This idea was developed for the case where an agency can collude symmetrically with two firms and information about firm quality is verifiable. Then under a bilateral interim efficient auction, there are no incentive-compatible side transfers between the bureaucrat and any firm, and no announcement strategy by the agency or that firm, given the auction which is designed by the principal in the Laffont-Tirole model when taking truth-telling by the other firm as given.

Why is the bureaucrat capable of compensating for shortcomings in procurement tenders? While this question might arise of the agencies in the mechanism-design models, which are bureaucratic agencies that are merely paid to screen sellers on behalf of the principal in the spirit of Laffont and Tirole (1991), the buyers which we observe in our data in Section 3 typically purchase goods out of their own budgets rather than just running auctions for goods and services for which the government pays. The implications and comparative statics of the model will not change drastically if we assume that recovery of foregone surplus was imperfect (i.e., if the bureaucrat pays the government ΔS if caught cheating, but the government only receives a fraction $\lambda \in (0, 1)$ of that sum, q would increase to $\frac{k_g}{\lambda \Delta S}$).

6 Discussion and conclusions

We analyze home bias in procurement using a novel data set of procurement tenders in Europe, exploiting the natural experiment provided by the onset of the Covid-19 pandemic. We observe a temporary surge in cross-border awards as the net effect of suspended rules on buyer discretion and an emergency effect of the pandemic. Our results suggest that *buyer discretion* and *crisis urgency* are important channels of procurement. We analyze these channels in two separate analyses, one exploiting regional variation of infection rates and one exploiting a change in regulation for some medical supplies. We find that both channels exert an economically large impact on cross-border procurement, with effect sizes an order of magnitude greater than the baseline rate.

Some open questions merit follow-up investigation. While this paper investigates the channels that affect home bias, our data is silent on contract performance indicators, such as cost overruns, delays, or terminations. Additional data could directly demonstrate the effect of misallocation. The paper also does not account explicitly for the role of trade policy instruments. An interesting subject for follow-up research would also be a network analysis of buyer-seller decisions following the pandemic to study the long-term effects on buyer-seller relationships in Europe. An interesting question for follow-up research would then be a network analysis of buyer-seller decisions following the pandemic to study the long-term effects on the buyer-seller relationships in Europe.

For procurement policy, this suggests that simply making it easier for sellers to enter their bids into foreign procurement tenders, for example by reducing language barriers, is likely not the most effective remedy against home bias. This does not mean that existing regulation in this direction, such as Europe-wide publication requirements for large tenders, are not helpful for market integration. For example, they might have built up existing mutual awareness of buyers and sellers that enabled cross-country procurement in the pandemic. The results also do not suggest that lifting the regulations was wrong in 2020, in spite of the transitory nature of the effect. The net effect of the Covid-19 pandemic and resulting policies was an increase in cross-border purchases during the first peak of the emergency.

Instead, we draw two conclusions from our data. First, buyers can rapidly adjust their behavior in emergencies. In spite of decades of low shares of cross-border procurement, contradicting the EU policy goal of completing the "Single Market", we have seen a surge of cross-border procurement at the onset of the pandemic. Both the local extent of emergency, measured through infection rates, and increasing buyers' discretion, had contributed to this surge. We explain this finding through a model in which the costs of monitoring buyers and the extent of their informational advantage impact misallocation. While the monitoring cost decreased due to heightened media attention, the informational advantage increased as procuring well for medical supplies is of heightened interest during a medical emergency.

Second, in normal times as well as during emergencies, buyer incentives and regulatory constraints are important, also to foster EU Common Market policy. The prevalence of direct awards and non-competitive tenders in spring 2020 suggests that buyers and sellers are aware of each other. Search frictions, such as mutual unawareness of buyers and sellers, are then likely not an important causes of the low volume of cross-border contracts absent the pandemic. To the contrary, buyers seem capable of selecting foreign sellers if they wish to do so.

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A Creation of the data set

We create our data set from individual XML files related to Contract Award Notices published on the website "TED Tenders electronic daily - Supplement to the Official Journal of the European Union".[29] The selection of relevant procurement contracts is via the pre-selected filter that is offered on a "COVID-19 dedicated page for tenders related to medical equipment needs".[30] The default filter is for contracts published between February 1, 2020 and December 31, 2020. For our descriptive statistics, we adopt this datum to distinguish between pre- and post-Covid contracts, which is in line with the WHO designation (see footnote 21).

We expand the default filters to include notices published as early as January 2018 to obtain a comparison period immediately preceding the pandemic. Note that the contract date for any individual lot, which we use in our analysis, may precede the publication date. The earliest contracts observed in our data go back as far as the year 2000. The filter also includes pre-selected codes from the Common Procurement Vocabulary (CPV).[31] In this sample, we have 461 distinct CPV codes at different levels of granularity. The first five digits of a CPV code describe the category, while three additional digits provide greater detail about the products. We aggregate different individual products by category, using the first 5 digits of the CPV code, resulting in 311 different product categories in our data set.

We download a total of 9,233 contract award notices and voluntary ex-ante transparency notices.[32] These documents are complex and rich in features, each describing a procurement tender, giving information on the buyer (name, type and location of the authority), the object (total value of the procurement and product categories, of which we only observe the first, main product category if there are several), and possibly division of the contract into individual lots. For each lot, the document reports the number of companies that bid for the contract, as well as the number of foreign, non-EU, and small and medium-sized firms as bidders.

To create a data set at the level of individual contract awards, we process the files in two stages: first, we collect data that is common to all lots within one contract award notice, such as the date of publication, the location of the buyer, and the total value and currency of the procurement tender. In the second step, we take the section regarding the individual contract award, split it at lot numbers and extract features that are specific to individual awards, such as the location and name of the contractor, the initially estimated and final value of the award, the contract date, as well as the number of bidders, further divided into bids from other EU countries, bids from outside the EU, and bids from small and medium-sized firms. These award features are then combined with the contract-specific information.

The unit of observation is a separate contract award to a contractor. So, when a lot is awarded to a single company, this is one observation. When several contract awards are listed under one lot number, each contract award is also counted as one observation. We understand entries where no contractor name or contract date are to represent failed procurement tenders. In these cases, no contract was awarded. We remove these observations from our main analysis. We do, however, construct a weekly panel by country of the total amount of failed procurement at the start of each week, which we combine with our cross-section of contract awards for a robustness check (see Appendix C). In the remaining data set, there are 146 awards where no contract date has been listed. In these cases, we approximate contract date with publication date.

We construct our main outcome variable as an indicator that takes the value 1 if the country of the contractor is identical to the country of the buyer. Therefore, we exclude procurement by EU agencies because our outcome variable related to domestic firms does not apply to procurement by the EU itself. Thus, we obtain a total of 125,301 observations of individual awards, some of which do not go into our main regression analysis due to lack of data on contract values.

[29] https://ted.europa.eu

[30] https://simap.ted.europa.eu/web/simap/covid-related-tenders

[31] "The CPV establishes a single classification system for public procurement aimed at standardising the references used by contracting authorities and entities to describe the subject of procurement contracts." https://simap.ted.europa.eu/web/simap/cpv.

[32] The latter, which only make up 0.1% of documents and 0.3% of awarded value, do not enter the regression analysis, because they contain no information about the number of bidders or foreign bidders - they all refer to directly negotiated contract awards. They are considered in Figure 2 to assess the share of non-competitive tenders and Table 8 to describe the total spending by country observed in the data.

We combine this data with monthly average exchange rates for all non-Euro currencies. We use data from the Statistical Data Warehouse of the European Central Bank, data set "EXR : Exchange Rates", supplemented by individual values for the Macedonian Denar and Icelandic Kronur for six different months from "currencies.zone", an exchange-rate information provider. All non-Euro values are then converted to Euro using the prevailing exchange rate. In a few cases, token values such as 0.01, 1, 99,999,999 or 999,999,999 appear in the data set. These are set to missing. We manually verified for the large values that these are indeed token or placeholder values, which is sometimes explicitly noted as a comment in the document or apparent from inconsistencies between total value and the value of awards.

Some contracts don't specify a total value of the procurement at the top of the contract, but instead report a lower and upper range. We compute the midpoint between the higher and lower bound of this range and replace missing values for the total contract with this average. To be conservative and not introduce noise (e.g. from very imprecise ranges), we only do this in cases when the upper bound is less than 10 times larger than the lower bound, so we are not off by more than an order of magnitude. We use this value (total value of procurement) only for descriptive statistics and in a robustness check where we compute the total amount of failed procurement for some countries as an additional control variable. The main regressions use the value at the award level for which the value is always either an exact amount or missing.

Next, we combine this data with data on infection rates from the European Centre for Disease Prevention and Control (ECDC) at a national and sub-national level.[33] We match infection rates with the performance regions of the contracts. This way, we can measure the pandemic intensity at the location of interest to the buyer. Some data cleaning is required to deal with inconsistent entries (e.g. "CZ0" appears as well as "CZ") and to match infection rates and contract location at the same level. Infection rates are available at different levels of aggregation depending on the country. Where only infection rates for the region at the NUTS3 level is available, we compute an approximate value for matching with contract information available at the NUTS2-level by matching the contract with the first NUTS3-region in the data. In most cases, this location is reported at a more detailed level (NUTS3), which we can easily match with a NUTS2 code through the first two numbers following the country code.

In a few cases, the performance location is listed as a NUTS1 code, in which case it is matched to the first applicable NUTS2 code. When only the country is listed as location of performance, we use national data on the infection rate. We also use national data to fill a few days before the 14-day sub-national average is started being reported. Dates before the first reported case in a country are assumed to have 0 infections. The procedure is repeated to match infection rates with seller location NUTS-codes and Covid-death rates for robustness checks reported in Appendix C. Finally, we add recent population data for the different NUTS codes from Eurostat to compute the average national infection rate excluding individual regions one at a time for a robustness check.

The data for the control group of goods closely related to the Covid-19 related medical supplies are retrieved and treated similarly. We select a control group from all product groups of the CPV that are at the same hierarchy level as the Covid-related tenders. The CPV of Covid-19 related tenders are pre-defined by TED. Table 7 shows which CPV were chosen to complement treatment product groups:

[33] https://qap.ecdc.europa.eu/public/extensions/COVID-19/COVID-19.html#subnational-transmission-tab

Table 7: Selection of product groups by CPV for treatment and control group

Treatment	Control
45215142 Intensive-care unit construction work	45215141 Operating theatre construction work, 45215143 Diagnostic screening room construction work, 45215144 Screening rooms construction work, 45215145 Fluoroscopy room construction work, 45215146 Pathology room construction work, 45215147 Forensic room construction work, 45215148 Catheter room construction work
35113400 Protective and safety clothing	35113100 Site-safety equipment, 35113200 Nuclear, biological, chemical and radiological protection equipment, 35113300 Safety installations
18143000 Protective gear	18143100 Work gloves, 18143200 Safety visors
18424300 Disposable gloves	18424400 Mittens, 18424500 Gauntlets
33141420 Surgical gloves	33141410 Wire cutter and bistoury
33157000 Gas-therapy and respiratory devices	33151000 Radiotherapy devices and supplies, 33152000 Incubators, 33153000 Lithotripter, 33154000 Mechanotherapy devices, 33155000 Physical therapy devices, 33156000 Psychology testing devices
33192120 Hospital beds	33192110 Orthopaedic beds, 33192130 Motorised beds, 33192140 Psychiatric couches, 33192150 Therapy beds, 33192160 Stretchers, 33192200 Medical tables, 33192300 Medical furniture except beds and tables, 33192400 Dental workstations, 33192500 Test tubes, 33192600 Lifting equipment for health care sector
33195110 Respiratory monitors	33195200 Central monitoring station
33670000 Medicinal products for the respiratory system	33610000 Medicinal products for the alimentary tract and metabolism, 33620000 Medicinal products for the blood, blood-forming organs and the cardiovacular system, 33640000 Medicinal products for the genitourinary system and hormones, 33650000 General anti-infectives for systemic use, vaccines, antineoplastic and immunodulating agents, 33660000 Medicinal products for the nervous system and sensory organs, 33680000 Pharmaceutical articles, 33690000 Various medicinal products
33363600 Antiseptics and disinfectants	33363100 Antifungals for dermatological use, 33363200 Emollients and protectives, 33363300 Antipsoriatics, 33363400 Antibiotics and chemotherapeutics for dermatological use, 33363500 Corticosteroids for dermatological use and dermatological preparations, 33363700 Anti-acne preparations, 33700000 Personal care products, 33900000 Post-mortem and mortuary equipment and supplies
39330000 Disinfection equipment	39310000 Catering equipment, 39340000 Gas network equipment, 39350000 Sewerage works equipment, 39360000 Sealing equipment, 39370000 Water installation

B Data description

This section reports additional summary statistics for the main data set, including the summed value of individual contract awards and lots by buyer country in our data set. All sums are converted to Euros, where necessary, and exclude VAT. Table 9 reports the sum of lot values for all contracts in the data set (published between 2018 and 2020, but including some contracts with earlier contract dates) and thus represents the weights of different countries in our regression analyzes.

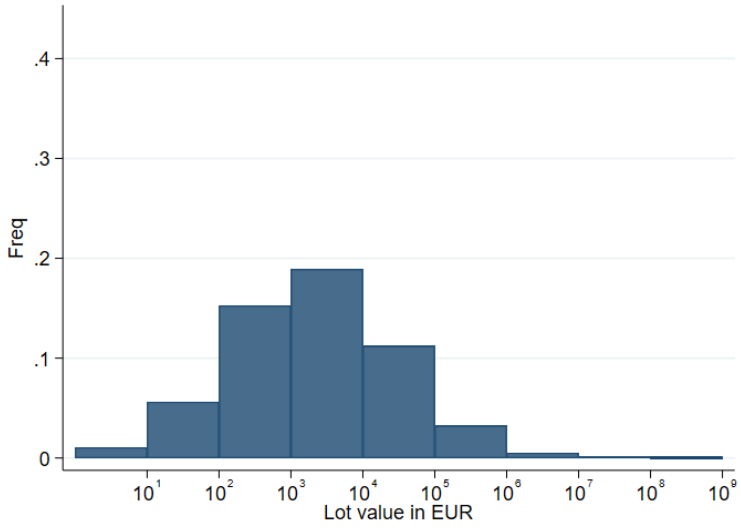

Figure 8: Distribution of lot value for contract awards, Covid-19 related tenders in EUR (log scale)

Table 8: Total value of contract award notices published February - December 2020 by buyer country (EUR equivalent, excluding VAT)

Country	Total value	Country	Total value	Country	Total value
United Kingdom	9,005,126,690.03	Czech Republic	177,231,200.62	Croatia	8,915,339.79
Ireland	1,934,554,280.00	Sweden	158,995,275.51	Switzerland	8,905,421.95
Germany	1,415,021,574.51	Spain	105,107,673.78	Greece	4,470,425.59
Norway	1,370,994,977.44	Lithuania	73,816,291.34	Latvia	4,448,462.10
Romania	982,951,739.61	Finland	73,498,257.00	Luxemburg	1,612,510.48
Poland	953,452,881.24	Hungary	65,918,660.67	Malta	1,231,841.70
Austria	578,323,685.38	Slovenia	63,251,154.62	Portugal	1,030,179.80
France	556,193,188.75	Bulgaria	40,919,459.08	Cyprus	741,500.00
Italy	533,255,369.29	Slovakia	32,943,139.96	North Macedonia	262,834.19
Denmark	376,466,222.33	Estonia	30,216,805.50		
Netherlands	218,548,939.44	Belgium	29,994,238.76		

Sum of the total contract values by buyer country, published as "result[s] of tenders related to COVID-19" on https://ted.europa.eu/TED/.

Figure 19 shows the share of contracts of different sizes in 2020. The categories follow the different thresholds that the EU uses to identify contracts presumed to be of cross-border interest.[34] A dashed box marks the three months with the highest share of cross-border awards.

We look at the largest lots to better understand the impact of potential outliers. Table 10 lists the 10 largest cross-border contract awards after February 2020. Except for one, they are all purchases from the UK. All of these purchases were under the CPV code for "Garments for biological or chemical protection" which encompasses the most common types of PPE, including face masks. These contracts

[34]See the website of the European Commission under "Internal Market, Industry, Entrepreneurship and SMEs", https://ec.europa.eu/growth/single-market/public-procurement/rules-implementation/thresholds_en.

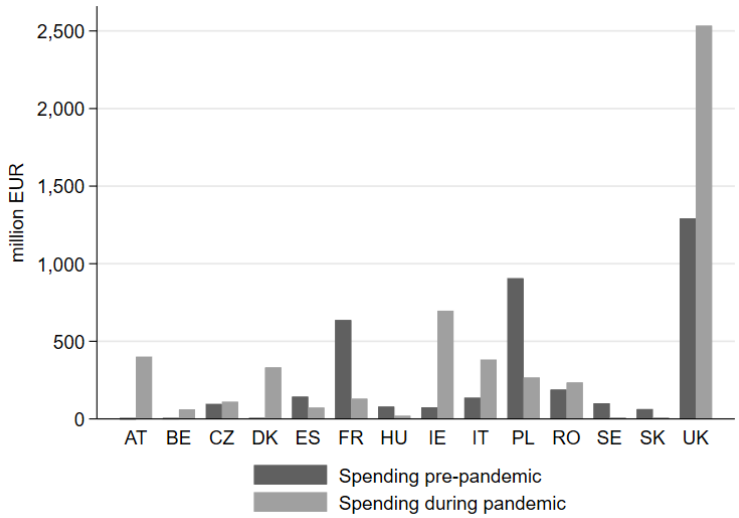

Figure 9: Spending 2018-2020 on Covid-19-related product groups, top 14 countries

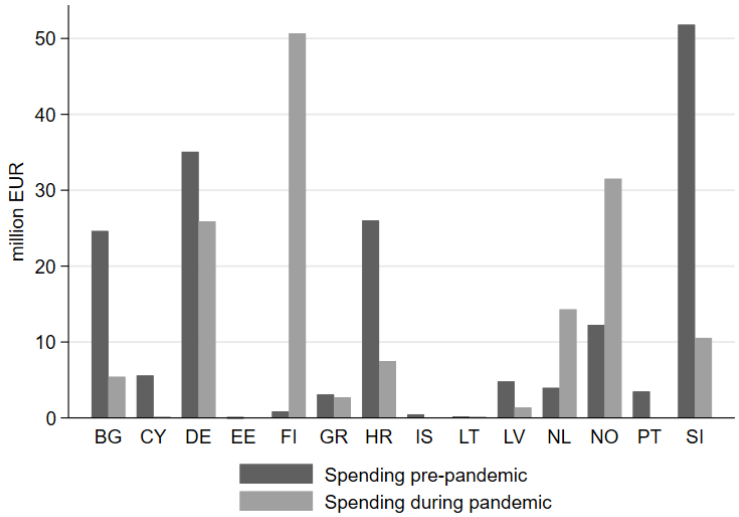

Figure 10: Spending 2018-2020 on Covid-19-related product groups, other countries

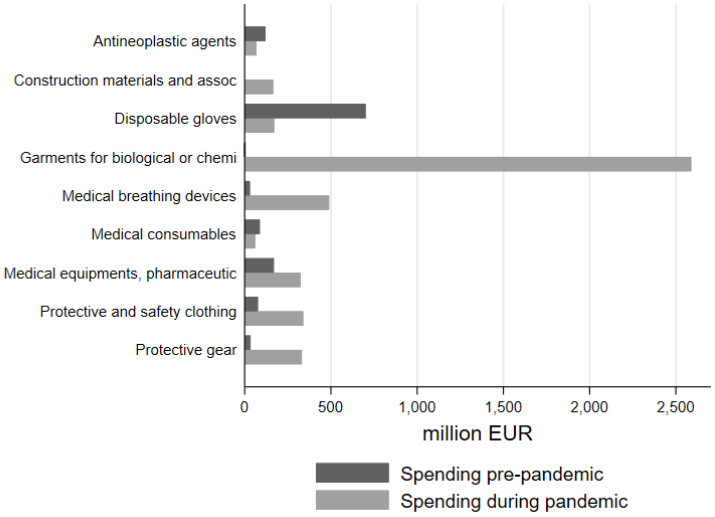

Figure 11: Spending 2018-2020 by product category, top 9 categories

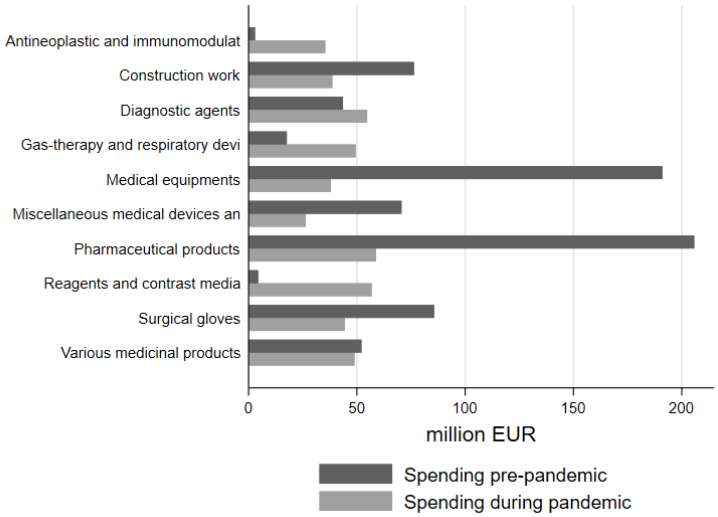

Figure 12: Spending 2018-2020 by product category, other categories

range in value from almost 350 million Euros to just under 80 million Euros and most have been awarded to non-EU countries. This suggests that while we observed unprecedented levels of cross-border procurement in the pandemic, Covid-19 did not "integrate the Single Market", but rather pushed buyers that previously purchased domestically to an international procurement strategy.[35]

Expanding on the discussion of missing values in Section 3, we might be concerned about selection bias and unreported values if the distribution of contract award sizes was highly abnormal in this time period. Although the share of contracts in these different size categories exhibits some fluctuation year-round, the average within each category seems to be not too far off for the three months within the box and the remaining nine months. We might be concerned about an absence of large contracts in the period of high cross-border awards, because this might suggest that large contracts would have been deliberately not reported or shaded to avoid exceeding reporting thresholds. Visual inspection of Figure 19 does not suggest lower reporting of large contracts in the period of large cross-border awards.

To compare monthly spending in the countries of our sample and in the 20 largest product categories, we reproduce Figures 9, 10, 11 and 12, but comparing monthly values by dividing the pre-pandemic sum by 25 and the sum during the pandemic by 11, reflecting the time covered in our data set. Of interest is the observation that for some countries average spending was higher in the two years preceding the pandemic than in the first 11 months of the pandemic, including Germany, Croatia, and Slovenia. This comparison also shows that monthly average spending did remain approximately constant in some product groups, including construction (related to medical work), which might reflect long-term planning for some kinds of products and services. In most of the largest product groups (Figure 15), average monthly spending during the pandemic exceeded average monthly spending before the pandemic by a wide margin.

B.1 Sample selection

We investigate missing data and sample selection in our data set. Procurement documents are sometimes incomplete, leading to missing values. These values are most likely not missing at random, yet a regression analysis shows that they likely just result in a downward bias of regression estimates, leaving us with a lower bound on possible effect sizes. Table **??** presents a least-squares regression of

[35] Among the 50 largest lots, which includes contracts above 2.8 million Euros, we also find three French purchases of protective gear from Germany and Switzerland, one Czech purchase of medicinal products from Ireland, one Romanian purchase of medical breathing devices from Germany, three Italian purchase of medical consumables and garments from Singapore and China, while the remainder are UK purchases mostly again of garments for biological or chemical protection from mostly non-EU countries.

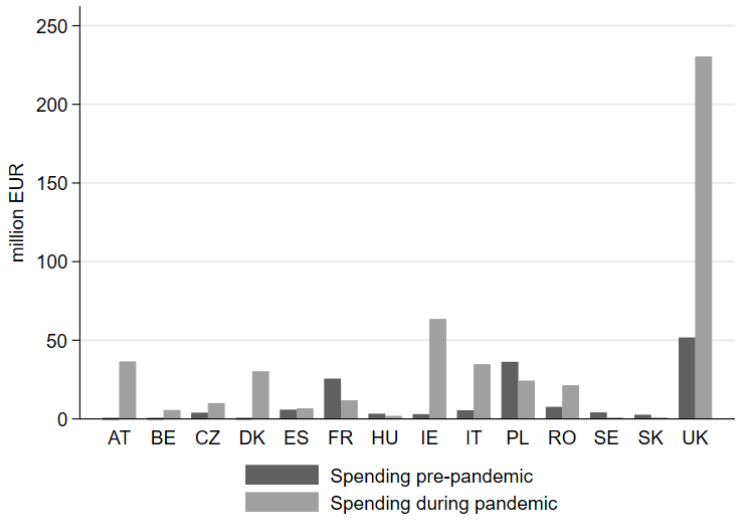

Figure 13: Average monthly spending 2018 - 2020 by country on Covid-19-related products, top 14 spenders

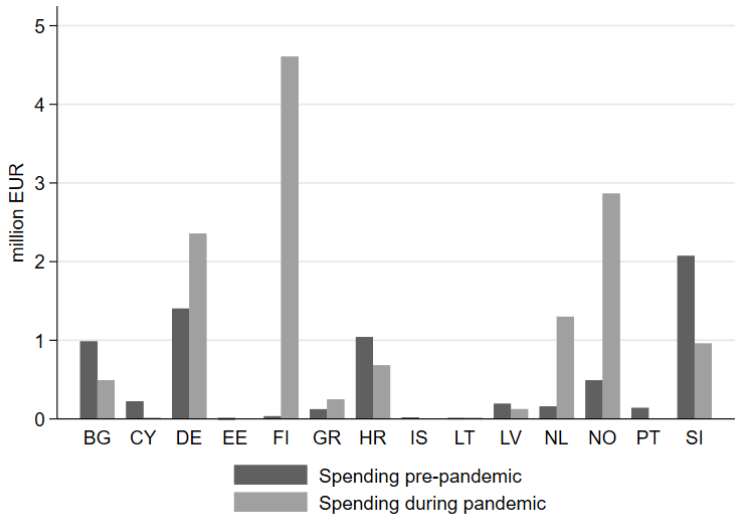

Figure 14: Average monthly spending 2018 - 2020 by country on Covid-19-related products, other spenders

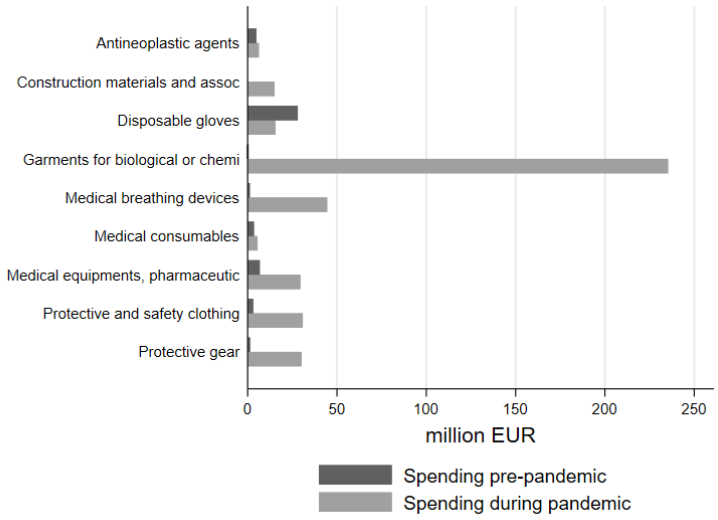

Figure 15: Average monthly spending 2018 - 2020 by Covid-19-related product category, top 9 categories

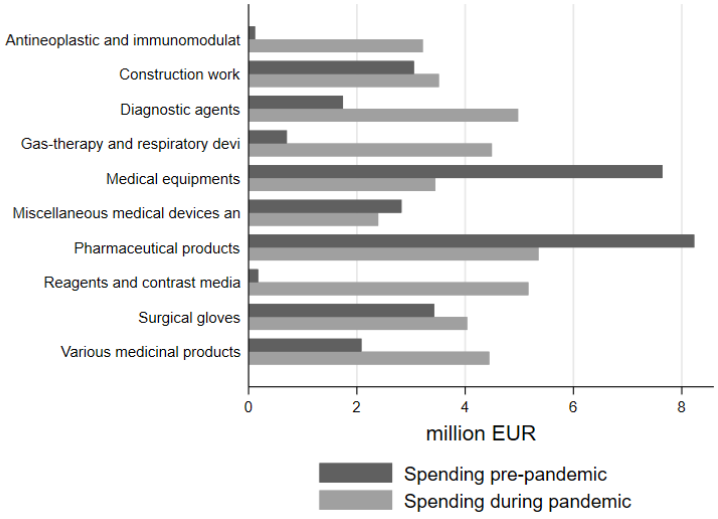

Figure 16: Average monthly spending 2018 - 2020 by Covid-19-related product category, other categories

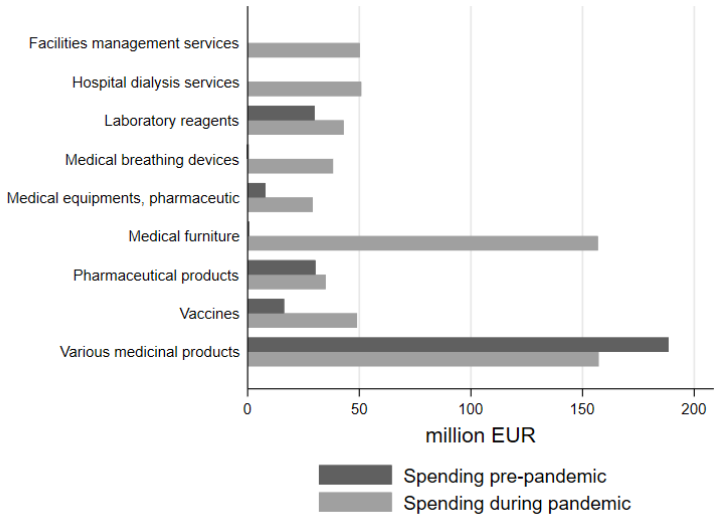

Figure 17: Average monthly spending 2018 - 2020 by product category, top 9 categories (control group products Section 4.4)

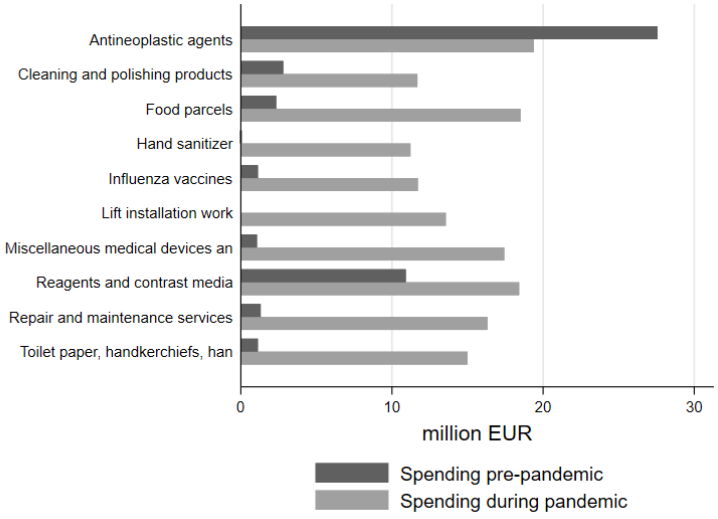

Figure 18: Average monthly spending 2018 - 2020 by product category, other categories (control group products Section 4.4)

Table 9: Total value of individual contract awards published 2018-2020 by buyer country (EUR equivalent, excluding VAT)

Country	Total value	Country	Total value	Country	Total value
United Kingdom	3,825,440,159.92	Hungary	98,826,759.14	Greece	5,806,930.67
Poland	1,172,433,382.38	Slovakia	72,286,834.36	Cyprus	5,735,658.40
Ireland	771,395,491.32	Belgium	65,088,034.40	Portugal	3,481,540.76
France	767,024,944.62	Slovenia	62,360,505.66	Iceland	475,862.22
Italy	517,696,183.13	Germany	60,982,075.65	Lithuania	336,143.81
Romania	423,224,861.66	Finland	51,526,898.00	Estonia	10,190.00
Austria	402,774,108.00	Norway	43,775,788.39		
Denmark	336,247,169.13	Croatia	33,510,701.05		
Spain	215,683,821.81	Bulgaria	30,046,518.24		
Czech Republic	205,983,843.36	Netherlands	18,257,500.94		
Sweden	109,544,231.72	Latvia	6,166,053.67		

Sum of the individual contract awards by buyer country, published as "result[s] of tenders related to COVID-19" on a page with "COVID-19-related tenders" on https://ted.europa.eu/TED/.

Table 10: Largest cross-border purchases

Buyer country	Seller country	Contract date	Contract value (EUR equivalent)
United Kingdom	China	20/05/2020	346,725,371
Ireland	China	03/04/2020	225,128,510
United Kingdom	Hong Kong	28/05/2020	126,582,278
United Kingdom	China	06/06/2020	110,173,256
United Kingdom	United States	04/06/2020	91,642,825
United Kingdom	Austria	30/05/2020	87,951,739
United Kingdom	Hong Kong	04/05/2020	82,553,660
United Kingdom	China	03/06/2020	81,705,908
United Kingdom	China	21/05/2020	79,801,871
United Kingdom	United States	04/06/2020	74,041,759

a dummy variable that takes the value 1 if the value of a contract award is missing and 0 if not. As regressor we use the main dependent variable (an indicator variable for contract award to a domestic company).

Absent control variables, we find no strong correlation between domestically awarded contracts and missing information on contract value. Controlling for local infection rates for the buyer and seller, number of bidders, share of foreign bidders, as well as dummies for buyer country, product group and month-year, we find that domestic contracts are on average circa eight percentage points more likely to not have information on award values. This regression analysis excludes 158 observations with missing values for the number of bidders. This finding is consistent with buyers being more careful when filling in procurement documents for larger contracts because cross-border contracts are, on average, larger than domestically awarded contracts. Contracts below an administrative notification threshold don't

Table 11: Likelihood of missing value conditional on domestic awards

Dep. var.: Award value is missing		
Domestic award	0.013	0.084***
	(0.019)	(0.019)
controls & dummies	no	yes
N	121,120	120,962

Robust standard errors in parentheses,
*** p<0.01.

have to be reported. Contracts that should normally fall above a notification threshold can be split or shaded intentionally to fall below the threshold and go unreported. While we cannot exclude the possibility that this also affects tenders for medical supplies, we do not think that they impact our estimates. We would only be concerned about systematic differences in contract size between times of high cross-border awards and other periods.

We categorize contracts by size and plot their relative frequency by month in 2020 in Figure 19.[36] The main period of high cross-border awards, April to June 2020, is not obviously anomalous from visual inspection beyond usual fluctuations. There is also no significant seasonal variation in the main outcome or independent variables. Also, we would expect that contracts where misallocation plays a role to be more likely to go unreported. Suppose such contract awards are more likely to be concealed, for example to avoid scrutiny by oversight authorities. Then this would imply that we oversample cross-border awards. However, since the baseline of cross-border awards is already very small (less than 1% as a simple share of all contracts), such an effect would likely be small as well.

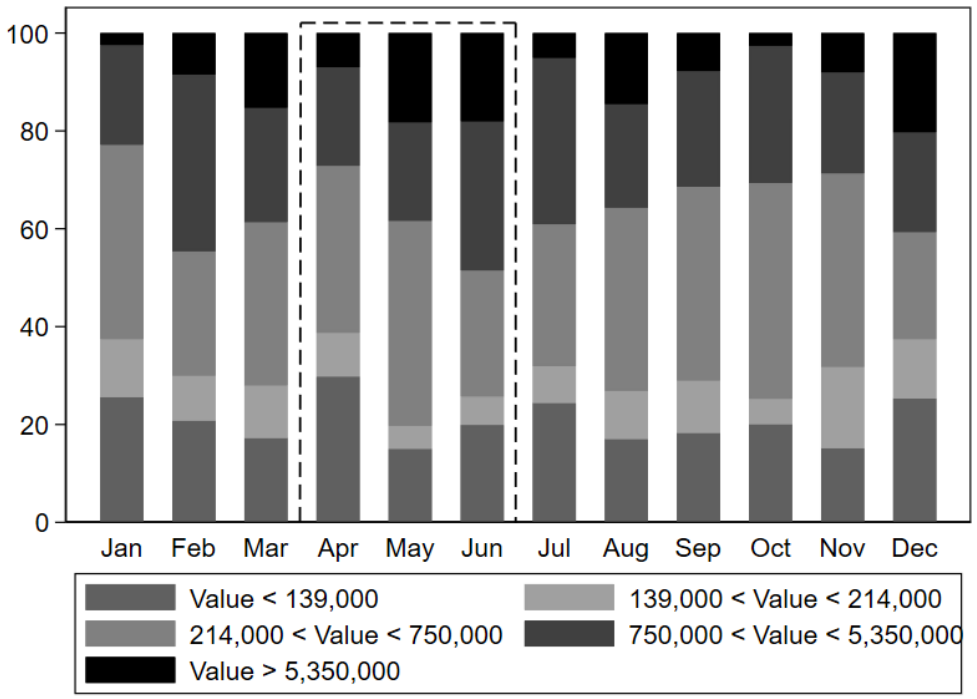

Figure 19: Contract award sizes in 2020 in EUR

B.2 Logit regression

In line with the literature, and appropriately for a binary outcome variable, we estimate a logit model with a slightly changed regression equation. In the non-linear model, we cannot compare the coefficients of the month dummies, as the effect of their coefficients depends on the level of other variables, we use instead a dummy variable that takes the value 1 during the deregulation period and the value 0 before. Results are reported in Table 12. This regression rests on fewer observations (53,971 rather than 67,638) as perfect predictor variables and their associated observations are dropped. If, for example, in a certain product group or country we observe no cross-border procurement, this perfectly predicts a "success" and the observation is dropped to improve numerical stability. This has no effect on the likelihood or estimates of the remaining variables.

[36]The main value thresholds above which contracts have to be reported are 135,000 EUR for goods and services and 5,350,000 EUR for (subsidized) works contracts, which in our context can apply to construction work. A few other thresholds may apply in special circumstances which we cannot identify from contract observables. See https://ec.europa.eu/growth/single-market/public-procurement/rules-implementation/thresholds_en

Table 12: Logit regression coefficients and average marginal effects for contract award to domestic companies

	Linear infections
Logit, dep. var.: Contract awarded to domestic company	
Buyer infection rate	-2.317*
	(1.251)
Seller infection rate	3.588**
	(1.470)
Regulation change	-0.252
	(0.179)
Total number of bidders	0.069***
	(0.025)
Share of foreign bidders	-7.386***
	(0.275)
Average marginal effects across observations	
Buyer infection rate	-0.278*
	(-1.85)
Regulation change	-0.0305
	(-1.40)
N	53,971

Robust standard errors of coefficients and z-statistics of marginal effects in parenthesis, * p<0.1, ** p<0.05, *** p<0.01.

Table 13: Test for Granger-causality of local infection rates

Dep. var.: Local infection rate	
Infection rate other regions	-0.017
	(0.017)
Number of periods	349
Number of units	46

Standard errors allowing for cross-sectional heteroskedasticity in parenthesis. Up to nine lags of the explanatory variable tested, six lags chosen based on Bayesian Information Criterion.

Most effects go in the same direction as in the linear model. Two differences to the linear probability model stand out: the share of foreign bidders is now a strong predictor for foreign awards while the regulation effect and average marginal effects of regulation and infection rates are insignificant. The results are not materially different for common "rare events" logit approaches. This is not surprising, as even though foreign awards are rare relative to domestic procurement, we have several thousand observations of such events.

B.3 Granger causality and spillover

This Section contains the tables referenced in Section 4.3 to test for the information value of infection rates in other regions and possible spillover effects.

Table 14: Domestic contract awards in countries below the national median

Dep. var.: Domestic award	
Infection rate other regions	0.092
	(0.085)
Constant	0.990***
	(0.006)
R-squared	0.001
N	842

Robust standard errors in parenthesis,
*** p<0.01.

C Robustness

In this section, we report robustness checks to the regression analysis in Section 4.

Alternative emergency variables

Our claim of the infection rates as a measure for crisis emergency, or urgency, should persist for alternative measures. In particular, one might worry about possible feedback effects from procurement on local infection rates. To alleviate such concerns, we test an alternative measure of crisis urgency that specifically addresses feedback effects from procurement on infection rates. To do this, we compute the infection rate in all other regions of a country except for the one of the performance location of the contract. This way we can exclude effects of local procurement outcomes on the infection rate.

We construct this measure by computing the absolute number of infections in each country and the region indicated in the contract award using population and the infection rate, then computing the difference between the two, and dividing by country population minus regional population to obtain the average infection rate in the country excluding the region of the observation. Even though we look at spillover effects separately in Section 4.3 and find no additional information about future infections from other regions of a country, this measure helps us capture the common trend of infections within a country while excluding potential local feedback effects.

Using this alternative variable for our regression analysis (Table 15), we continue to find large and statistically significant effects, for the regulation change. They are of similar quantitative size both for the full sample and the leave-one-out analysis where again leaving out the UK provides a lower bound of the effect size: In the full sample (sample excluding UK), a one-standard-deviation increase in national infections excluding the local region, results in a 25.1 (10.5) percentage points higher share of cross-border procurement.

Domestic capacity constraints

To verify that our results are robust even when accounting for buyers that had to turn to foreign sellers because of domestic capacity constraints, we conduct an additional robustness check. For this robustness check, we select a subsample of contracts for which the greatest number of domestic bidders for a contract (across all lots) is greater than the number of distinct domestic winners for lots on that contract. Thus, we identify at least the extensive margin of domestic spare capacity for these contracts. As discussed in Section 4.5, our method is likely conservative. The results are reported in Tables 16 and 17.

The effect of a one standard deviation in the average infection rate at the buyer's location is approximately the same, at circa 20 percentage points. The effect of regulation is mitigated to circa 9 percentage points, compared to 35 percentage points in the baseline regression. Still, the effect size and statistical significance of our main variables remains high. Domestic capacity constraints were certainly important during the pandemic (affecting up to almost two-thirds of contracts in our sample) but do not suffice to explain the movements in cross-border procurement in the pandemic.

Table 15: National infection rate (excluding local infections) as alternative explanatory variable

	All countries	Excluding UK
Dep. var.: Contract awarded to domestic company		
ATET		
Infection rate other regions same country	-1.793***	-0.527***
	(0.405)	(0.141)
Controls		
Infection rate at seller location	2.050***	0.427**
	(0.398)	(0.168)
Total number of bidders	0.002***	0.000
	(0.001)	(0.000)
Share of foreign bidders	-0.361***	-0.428***
	(0.099)	(0.061)
dummies	yes	yes
standard error of infection rate	0.14	0.20
1-std.dev. increase	-0.251	-0.105
N	67,638	67,387

Robust standard errors in parentheses, ** p<0.05, *** p<0.01.

Table 16: Main regression Section 4.3 on a subsample with domestic spare capacity

	All countries	Excluding UK
Dep. var.: Contract awarded to domestic company		
ATET		
14-day average infection rate per 100	-0.896***	-0.908***
	(0.252)	(0.258)
Controls		
Infection rate at seller location	1.212***	1.222***
	(0.405)	(0.408)
Total number of bidders	0.001**	0.001**
	(0.000)	(0.000)
Share of foreign bidders	0.057	0.051
	(0.107)	(0.110)
dummies	yes	yes
standard error of infection rate	0.22	0.23
1-std. dev. increase	-0.197	-0.209
N	23,443	23,294

Robust standard errors in parentheses, ** p<0.05, *** p<0.01.

Table 17: Main regression Section 4.4 on a subsample with domestic spare capacity

	All countries	Excluding UK
Dep. var.: Contract awarded to domestic company		
ATET		
Regulation change	-0.088**	-0.089*
	(0.043)	(0.052)
Controls		
Infection rate at seller location	0.214	0.033
	(0.137)	(0.020)
Total number of bidders	-0.000	0.001***
	(0.000)	(0.000)
Share of foreign bidders	-0.147***	-0.545***
	(0.027)	(0.079)
dummies	yes	yes
N	276,018	275,080

Robust standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01.

Unweighted regression

We re-run the first difference-in-difference regression, but do not weight contracts by value. The results are reported in Table 18. We also use contracts for which no value is available, leading to a greater number of observations. However, a contract split into multiple small lots (observations) now carries much greater weight than a single, large contract compared to the main regression. As there is no significant variation of the outcome variable at this level (documented in Section 3.6), we do not expect to find any correlation with our explanatory variables.

Indeed, due to the great differences in award values, as well as the more significant variation of the outcome variables, the results are null. Although ignoring contract size allows us to use more observations, no meaningful analysis with regards to our outcome of interest is possible. Effects disappear (or become economically insignificant in the second column) when we attempt to estimate this regression without taking into account the differences in lot size through their monetary value.

Table 18: Unweighted regression for contract award to domestic companies

	Infection rate	Regulation change
Dep. var.: Contract awarded to domestic company		
ATET		
Infection rate	-0.012	
	(0.011)	
Regulation change		-.004***
		(0.000)
Controls		
Infection rate at seller location	0.029***	0.006**
	(0.011)	(0.003)
Total number of bidders	0.000**	0.000***
	(0.000)	(0.000)
Share of foreign bidders	-0.505***	-0.597***
	(0.023)	(0.011)
dummies	yes	yes
standard error of infection rate	0.22	
1-std.dev. increase	-0.003	
N	120,922	540,413

Robust standard errors in parentheses, ** p<0.05, *** p<0.01.

Analysis of failed procurement contracts

Information about previously failed procurement contracts is potentially useful. For example, repeatedly failing to purchase desired goods or services may induce a buyer that discriminates against foreign sellers to reduce the extent of discrimination to avoid failed tenders in the future.

We analyze the total contract value of tenders which we code as failed (i.e., where no contractor and contract date are given) and create a weekly time series of the cumulative failed value of procurement for every country, starting in February 2020, thus restricting this analysis only to the time period that is indeed related to the Covid-19 pandemic. We estimate a similar specification as in Table 3, but also add the Euro-equivalent value of total failed procurement at the time of each award and a linear time-trend. The results are reported in Table 19. We do not find a statistically significant effect of failed previous procurement, nor does inclusion of this variable change the estimated effect drastically.

Table 19: Including the cumulative sum of failed tenders

	All countries
Dep. var.: Contract awarded to domestic company	
ATET	
Infection rate	-1.207***
	(0.300)
Controls	
Cumulative sum of failed tenders	0.000
	(0.000)
Infection rate at seller location	2.346***
	(0.432)
Total number of bidders	0.003***
	(0.001)
Share of foreign bidders	-0.614***
	(0.073)
standard error of infection rate	0.21
1-std. dev. increase	-0.254
N	21,111

Robust standard errors in parentheses, *** p<0.01.

Inference based on clustered standard errors

Errors may be correlated across countries due to similarity in regulatory and legal environments and similarities in training, doctrine, and perspective among public sector buyers within each country. We re-estimate the difference-in-difference regressions in Tables 4 and 5 using clustered standard errors with clustering at the country-level. Standard errors are indeed larger with clustering, although only moderately. Only for the full-sample regression regarding infection rates are the results no longer statistically significant at the 5%-level. In the remaining regressions, the precision of the estimates remains high relative to the point estimates.

Table 20: Difference-in-difference analysis for infection rates with clustered standard errors

	All countries	Excluding UK
Dep. var.: Contract awarded to domestic company		
ATET		
14-day average infection rate per 100	-1.376*	-0.463***
	(0.718)	(0.166)
Controls		
Infection rate at seller location	2.242	0.529
	(1.398)	(0.312)
Total number of bidders	0.002	0.000
	(0.001)	(0.001)
Share of foreign bidders	-0.363**	-0.428**
	(0.136)	(0.189)
dummies	yes	yes
N	67,638	67,387

Country-clustered standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01. Infection rates are 14-day moving average per 100 inhabitants at the NUTS-region reported as performance location (buyer infection rate) or as location of the contractor (seller infection rate). Share of foreign bidders computed as number of foreign bidders from EU and non-EU countries divided by total number of bidders.

Table 21: Difference-in-difference analysis for the effect of deregulation with clustered standard errors

	All countries	Excluding UK
Dep. var.: Contract awarded to domestic company		
ATET		
Regulation change	-0.357***	-0.160**
	(0.062)	(0.074)
Controls		
Infection rate at seller location	0.394	0.058
	(0.319)	(0.060)
Total number of bidders	0.001*	0.001*
	(0.000)	(0.001)
Share of foreign bidders	-0.030	-0.504***
	(0.135)	(0.146)
dummies	yes	yes
N	320,213	319,173

Country-clustered standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01. Infection rates are 14-day moving average per 100 inhabitants at the NUTS-region reported as performance location (buyer infection rate) or as location of the contractor (seller infection rate). Share of foreign bidders computed as number of foreign bidders from EU and non-EU countries divided by total number of bidders.