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

Philip Hanspach*

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Abstract

International public procurement sees low shares of cross-border purchases despite agreements against national preferencing. Introducing a unique dataset of contract awards for medical supplies in 27 European countries 2018 - 2020, we find a large, temporary surge in cross-border awards as the net effect of the Covid-19 pandemic and targeted deregulation. Two difference-in-differences regressions identify effects of crisis urgency and increased buyer discretion on cross-border procurement. The effects are economically large, as deregulation (a one-standard deviation increase in infection rates) increases the share of cross-border awards by 35.7 (19.3) percentage points over a baseline of 1.5 percent.

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

*European University Institute, philip.hanspach@eui.eu. I thank Giacomo Calzolari, David K. Levine, Stephen Davies, Laurens Vandercruysse and Joosua Virtanen for comments, as well as participants at CLEEN 2021, CRESSE 2021 and working groups at EUI. The usual disclaimer applies. Funding by the German Academic Exchange Service is gratefully acknowledged.

1 Introduction

Public sector procurement accounts for a large share of the economy, at 15-20% of GDP in the EU. For this reason, economists worry about misallocation that could 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, the policy goal of completing the "Single Market" remains elusive in spite of strict regulation that aims at promoting competition and a level playing field. Instead, the overwhelming majority of contracts is awarded nationally, essentially fragmenting public procurement markets along national borders. Efforts to promote market integration include regulation on buyer discretion, limiting the ability of buyers to award contracts at will. We study the effect of buyer discretion and emergency on misallocation in the form of home bias under the conditions of the Covid-19 pandemic. This analysis also sheds light on some phenomena in procurement in normal times.

In 2020, the Covid-19 pandemic shocked the global economy on many levels, impacting international supply chains and firm activities. Market conditions for some medical goods, such as personal protective equipment (PPE) or disinfectants, changed rapidly, overturning conventional wisdom of the economics of procurement. As the crisis intensified, rules which prioritize transparency and competition were abandoned. In some markets, 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. However, the results of procurement for medical supplies are now under scrutiny and garner unusual levels of public attention. Anti-corruption activists caution against the risk that vested private interests capture public resources and distort decisions as procurement is highly vulnerable to corruption.[1] Media reports uncover problems ranging from low-quality products in Austria[2] and Switzerland[3], over failures to organize distribution[4] and payment of deliveries[5] in Germany, to even major irregularities in contract awards due to political influence-taking in Germany[6] and Finland[7]. Examples from other European countries abound.

This article makes three main contributions to the study of cross-border procurement: First, we create a novel data set that allows us to study the volume and international dimension of European procurement of medical supplies in the Covid-19 pandemic. 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 home bias. Second, we investigate the separate effects of buyer discretion and crisis urgency with two difference-in-differences analyses. We draw on additional data on Covid-19 infection rates and procurement awards for closely related products from the Common Procurement Vocabulary (CPV) that were unaffected by deregulation. 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 (compared

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

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

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

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

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

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

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

to 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. We also analyze the effect of an EU announcement that lifted restrictions on buyers purchasing certain kinds of Covid-19 related medical supplies, allowing them to purchase goods directly rather than posting lengthy calls for tender. The effect of this suspension of regulation on buyer discretion goes in the same direction: We estimate an increase in the probability of a cross-border award as large as circa 36 percentage points for Covid-19-related tenders (with a lower bound of 16 percentage points) for purchases with lifted restrictions on buyer discretions, relative to a control group with similar supply and demand characteristics. Both effects were only transitory, however. Our third and final contribution is a simple model of monitoring between a government and its purchasing agent that, in contrast with much of the theoretical literature on misallocation in procurement, yields testable predictions that help to interpret the empirical results.

It is unsurprising that cross-border procurement surged in an emergency. What is more surprising is the role that buyer discretion plays in an emergency situation. Our findings show that regulation on buyer discretion impacts cross-border procurement. While buyer discretion is a possible reason for misallocation towards domestic firms, in the context of the pandemic it seems to have facilitated an international emergency response. The empirical results are encouraging that home bias can subside at least temporarily in an emergency. One would expect the opposite: when buyers prefer domestic firms, buyer discretion should facilitate home bias. Our findings then vindicate the crisis response of lifting restrictions as they did not increase home bias. Our model suggests that if home bias is due to misallocation buyer discretion impacts misallocation through the cost of monitoring buyers. In normal times, regulation to limit buyer discretion has the straightforward effect of lowering this monitoring cost, resulting in less misallocation. Deregulation can be interpreted as increasing this cost. However, the above-mentioned scrutiny and public attention on procurement of medical supplies might have worked in the opposite direction by disciplining buyer behavior.

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, if buyers are legally required to treat foreign and domestic firms equally, large variations in cross-border procurement as a result of a change in buyer discretion is inconsistent with a hypothesis of equal treatment. Secondly, consider the possible concurrent explanation that sellers voluntarily specialize in selling to domestic buyers, e.g. because of cost advantages due to linguistic or technical barriers. This may even be consistent with our observation that cross-border contracts are on average larger than domestic purchases. However, 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. Therefore, it seems implausible that a pure cost-based explanation could explain the drastic surge in cross-border procurement from almost nothing pre-pandemic to more than 50 percent in some months of 2020. This is especially true considering that we control for the share of foreign bidders together with the total number of bidders in each lot.

Section 2 positions this paper in the relevant economic literature on misallocation in procurement. The focus of this paper is on Section 3 which presents our data set, our empirical strategy, as well as Section 4 which presents our two difference-in-differences analysis. Section 5 presents a simple model of monitoring and discusses its implications for our empirical analysis. Section 6 concludes with a discussion of policy implications, limitations and extensions.

2 Literature

This paper relates most closely to the recent empirical literature studying various procurement outcomes related to policy goals and economic criteria. Partly fueled by the movement towards open and transparent government data in some countries, access to rich data sets has allowed both descriptive studies and the estimation of causal effects. Jääskeläinen and Tukiainen (2019) present stylized facts about competition, bidding, entry and bidders in Finnish procurement as well as an overview of recent empirical studies in Lithuania, Austria, Italy, Czech Republic, Russia, and several non-European countries based on large public procurement databases. Hyytinen, Lundberg, and Toivanen (2018) finds effects of procurement tender design on participation and cost of procurement for cleaning contracts in Sweden. Buyer discretion has been analyzed with a view to efficiency and rent-seeking in a national context with mixed results: Coviello, Guglielmo, and Spagnolo (2017) study procurement in Italy with 2000-2005 data, using an administrative threshold below which bureaucrat discretion is larger. They 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 firms and favored 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) argues that misallocation in procurement due to political corruption and rent-seeking behavior is prevalent not only in low- and middle income countries, but also in OECD countries. Misallocation need not arise from clandestine corruption, but can also be due to political or other goals attached to procurement, sometimes openly.[8] Such goals include the promotion of small-and-medium-sized firms, jobs, growth, or innovation. Dynamic considerations can, however, sometimes provide an economic justification for home bias (Barbosa and P. C. Boyer, 2012). Our empirical analysis studies the impact of the Covid-19 pandemic on one particular kind of misallocation, home bias, which is favoritism in the allocation of contracts to domestic over foreign firms. Our choice of outcome is motivated by Laffont and Tirole (1991) who 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, at the expense of foreign firms. At least under some conditions in repeated procurement auctions a "law of large numbers" should hold for awards to the group that is potentially disfavored and this might be a useful screen for collusion. A similar bias is also often assumed in the trade literature (Helpman and Krugman, 1989). We study two channels that impact the extent of home bias: emergency, measured through Covid-19 infection rates, may increase the importance of procurement outcomes relative to unrelated goals (such as trading favors with a domestic firm), while an increase in buyer discretion may facilitate allocation of contracts to favored firms (e.g. domestic firms).

Few articles so far have used procurement data published on TED to a similar extent. Prier, Prysmakova, and McCue (2018) describe a consolidated data set published by TED for the years 2009-2015 and advise caution due the large number of missing values and redundant information. On missing values, La Cour and Ølykke (2018) produce descriptive information on the number of

[8]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). For the UK, 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).

bidders and find missing values for this item in the order of 18% of entries.[9] 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 and find that although missing values are common in our data set as well, they are likely of small importance to the estimation of the effects we are interested in. Ours 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 under legal aspects (Lalliot and Yukins, 2020) as well as with a view to trade restrictions (Hoekman et al., 2021) 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 academic responses to the pandemic lay out the policy challenges for the EU. Sanchez-Graells (2020) sees among them a) the timing of future reversal to "normal" procurement rules, b) learning from the crisis to improve preparedness for the future and also to retain tools and methods in procurement that have proven valuable, and c) assuring that the exceptional circumstances of procurement are not abused to further unrelated policy goals (like expanding government spending). By studying empirically the outcomes of procurement for medical supplies during the Covid-19 pandemic, this paper helps to quantify these concerns and draw lessons for the future.

Beyond the narrow context of the pandemic, this paper makes a more general point about economic policy in an emergency situation. The arguments for increased buyer discretion due to the urgency in the Covid-19 pandemic mirror the arguments in the theoretical literature on centralized vs. decentralized procurement in emergencies, summarized by Dimitri, Dini, and Piga (2006). It is also related to the more general argument of Bolton and Farrell (1990) in favor of centralized organization in emergencies: the authors make a spirited argument in favor of centralized organization in emergencies. They assert that when delays are costly, a decentralized process can be less efficient than a centralized one. Their insight rationalizes the command structure of armies and wartime economies, and directly applies to emergency situations: When the inefficiency of a market process (such as supply uncertainty and delays) exceeds the informational inefficiency of a fast decision by a central authority, a centralized response is better. The model which we discuss as the framework for our analysis reflects this informational advantage of a buyer, in our model the bureaucrat, over a principal, who can be thought of as the public or government.

[9] They find that variation at the national level is high: While the number of bidders is missing in only 0.1% of Romanian tenders, this number is as high as 40% in France and Spain, see Table 2 in La Cour and Ølykke (2018).

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

3 Data

In this section, we present our original data set used to analyze the impact of crisis urgency and deregulation of buyer discretion on the awarding of procurement contracts for medical supplies in Europe. We create a new data set using contract award notices published between 2018 and 2020 on "TED: tenders electronic daily, Supplement to the Official Journal of the EU" (TED).[11] We explain the source and data collection as well as potential challenges in Section 3.1, concluding that sample selection and missing values are present, but likely not a major concern for our analysis. We provide descriptive statistics for the regression data set and visualize key data trends in Section 3.2 to describe the net effect of the pandemic and deregulation on procurement. Our main findings are 1) that the value of cross-border awards increases from 1.5% pre-pandemic to more than 50 percent between April and June 2020 and 2) that most tenders in April and May 2020 were direct awards to foreign sellers. The further results of the descriptive analysis are summarized in Section 3.3.

3.1 Data set

We use data from TED to study the procurement of medical supplies during the Covid-19 pandemic. 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." We describe the technical details of how we select, retrieve and clean the data in Appendix A. In summary, we use a total of 9,233 contract award notices for medical supplies and another 31,097 contract award notices for related product groups published in 2018-2020. These documents are complex and rich in features, each describing the results of a tender, giving information on the buyer (name, type and location of the authority), the object (total value of the procurement, product category), and possibly about the 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. The company winning the lot, the contractor, is listed with its location as well as an initial estimate and final reported value of the award. This final reported value can deviate from the initial estimate of the lot value. In addition, the sum of final reported values of the individual lots in the contract award notice deviates significantly in some cases from the initial overall contract value. Moreover, many of the forms are only partially filled out, leading to missing information.

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. We exclude contract awards with contract dates before 2018, but which were only published after 2018, as well as failed procurement tenders. Data on failed procurement tenders is used for a robustness check. We exclude procurement by EU agencies because our outcome variable related to domestic and foreign procurement does not extend to procurement by EU agencies. Thus, we obtain a total of 121,120 observations of individual awards for medical supplies, just over half of which enter into our main regression analysis due to missing values, mainly for contract value. Another 419,451 observations for related product groups are used in the difference-in-difference analysis in Section 4.4. The product groups selected for the control group are summarized in Table 13.

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. 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.

Data limitations: Two main data challenges are missing data and potential sample selection bias. As previous scholars who worked with TED noted (see Section 2), procurement documents are

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

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

sometimes incomplete, leading to missing values. These values are most likely not missing at random. Table 1 presents a least-squares regression of a dummy variable that takes the value 1 if the value of a contract award (our weighting variable) 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. Including as control variables 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.

Dep. var.: Award value is missing		
Domestic award	0.013	0.084***
	(0.019)	(0.019)
controls & dummies	no	yes
adj. R-squared	-0.000	0.566
N	121,120	120,962

Table 1: Missingness of the weighting variable conditional on the main dependent variable

A different data problem might arise from selection bias. 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 can offer two remarks. First, we would 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.[13] 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. Second, we would expect that contracts where misallocation and favoritism play a role were more likely to go unreported. Suppose home bias is present and affected 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.

3.2 Description of the data

This section provides an overview of the data set on Covid-19-related tenders, which is used in all analysis through Section 4.2 to 4.4, as well as the control group of other medical supplies which is used only in Section 4.4 (its selection and composition is described in detail in Appendix A). We describe the major trends of awarding practice for procurement tenders for medical supplies. We proceed to describe the composition of the control group data set used in Section 4.4. Finally, we describe the key outcome and explanatory variables that enter the regression analysis.

Table 2 presents summary statistics for Covid-19-related tenders, while Table 3 presents the same statistics for the control group of other medical supplies. Unsurprisingly, the larger data set has the larger extreme values of the distribution in terms of the total number of bidders and the largest contract value. However, median and mean for both of these are close or identical and given the large

[13]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

	N	Mean	Std. dev.	Minimum	Median	Maximum
Total value of the contract/lot (excluding VAT) in EUR	67,690	137,481.49	3,639,354.66	0.02	2,001.44	420,499,749
Indicator: domestic award	121,120	0.99	0.08	0	1	1
14-day average infection rate per 100 inhabitants	121,120	0.03	0.11	0	0	2.87
Infection rate at seller location	121,120	0.03	0.12	0	0	2.35
Total number of bidders	120,962	5.65	12.27	1	3	456
Share of foreign bidders	120,962	0.00	0.06	0	0	1

Table 2: Descriptive statistics of Covid-19-related tenders data set

standard deviations the means are not statistically different. Variation in the number of observations (column "N") reveals that there is missing data for some variables. We observe contract value in ca. 56% of contracts in Table 2 and ca. 60% of contracts in Table 3. In less than 1% of cases do we encounter missing values for the number of bidders.

	N	Mean	Std. dev.	Minimum	Median	Maximum
Total value of the contract/lot (excluding VAT) in EUR	252,602	162,362.19	7,075,726.22	0.07	1,380.92	1,990,000,000
Indicator: domestic award	419,546	0.99	0.08	0	1	1
14-day average infection rate per 100 inhabitants	419,546	0.02	0.09	0	0	2.35
Infection rate at seller location	419,546	0.02	0.10	0	0	1.87
Total number of bidders	419,451	4.78	11.75	1	3	999
Share of foreign bidders	419,451	0.00	0.06	0	0	1

Table 3: Descriptive statistics of control group of other medical supplies data set

Spending patterns: Overall spending on medical supplies in our data set is highly concentrated with a large range of values. To get a first understanding of cross-border procurement of medical supplies, we analyze the distribution of contract values, total spending by different countries before and after the pandemic, and the largest product groups pre- and post-pandemic. These descriptive statistics already help illustrating the links that will later be summarized through our regression analysis.

Figure 1 shows that the distribution of lot value for Covid-19-related tenders is centered around 1000 and 10,000 EUR in value. The largest individual lot is worth more than 400 million Euros. Figures 2 and 3 compare spending before and during the pandemic for different countries. The red bar refers to the 10 months of pandemic in our data set, the blue bar refers to the preceding 26 months before the start of the pandemic. A comparison based on an equal number of months would be meaningless due to the unique circumstances of the pandemic, so we emphasize the cross-country comparison rather than a per-period comparison. Notice the difference in scale between Figures 2 and 3, as the former shows countries with higher spending in any one period. We record higher spending during the first 10 months of the pandemic than in the (arbitrarily chosen) preceding 26 months in 11 countries (Austria, Belgium, Czech Republic, Denmark, Ireland, Italy, Romania, the UK, Finland, the Netherlands, and Norway), while the converse is true for 16 countries (Spain, France, Hungary, Poland, Sweden, Slovakia, Bulgaria, Cyprus, Germany, Greece, Croatia, Iceland, Lithuania, Latvia, Portugal and Slovenia). Similarly, Figures 4 and 5 show which product groups were purchased in the greatest amount before and during the pandemic.

Table 4 summarizes the total spending on medical supplies during the pandemic (Euro equivalent at prevailing exchange rates without VAT) in our data set. It is computed from the total value of

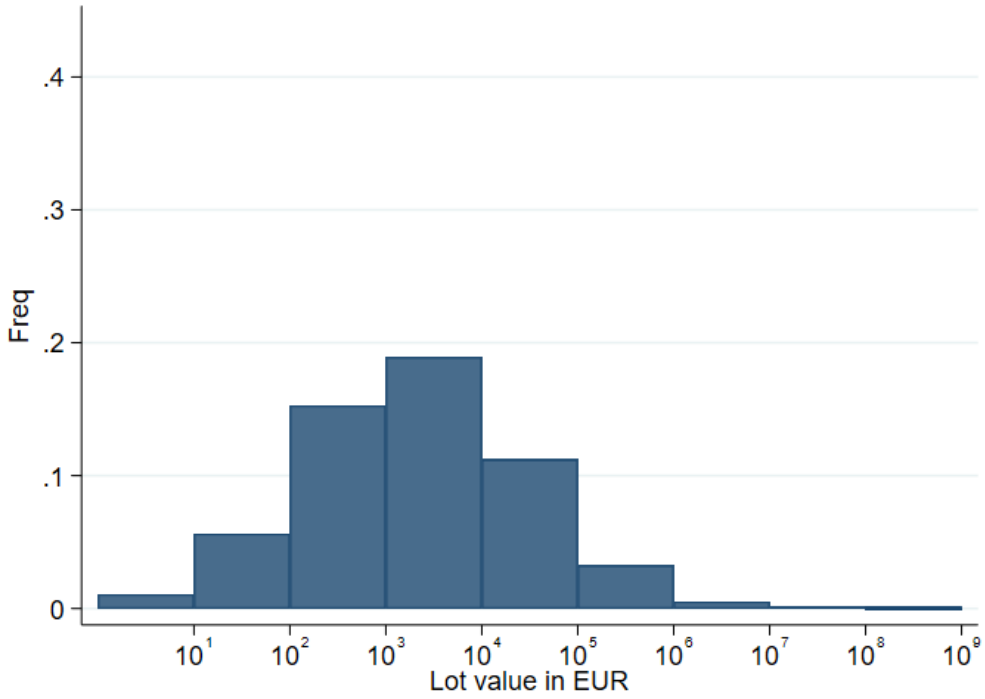

Figure 1: Relative frequency of lot value among contract awards for Covid-19 related tenders in EUR (logarithmic scale)

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. These values give a broad overview of purchases listed on TED by different countries. It is apparent that the amount procured by a country is not just a function of the size of its economy or population. 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. The five largest economies in our data set, Germany, UK, France, Italy and Spain, appear on ranks 3, 1, 7, 8 and 12, respectively.

One reason for this is that this table only includes awards with complete information on contract value. 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 that remain after dropping failed tenders from the original 9,322), 75 percent have buyers from Germany, France, and the UK. Furthermore, there are likely differences in reporting (compare, for example, the varying levels of completeness described by previous researchers in Section 2). 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. Unfortunately, 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) 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). The value and dates used for this table are recorded at the contract level. The values of individual lots (our unit of analysis) used as weights in the regression based on contract dates are reported at a lower level and may be missing even when the total contract value is known. For example, the largest German contract in our data for circa 1.1 billion Euros of protective and safety clothing was split between 49 domestic firms, but no information is given for the individual lot sizes. For a table of contract award values, which translate into different countries' weights in the regression, see Appendix B. Finally, there are purchases completely outside the EU procurement system that do not appear in the list of contract

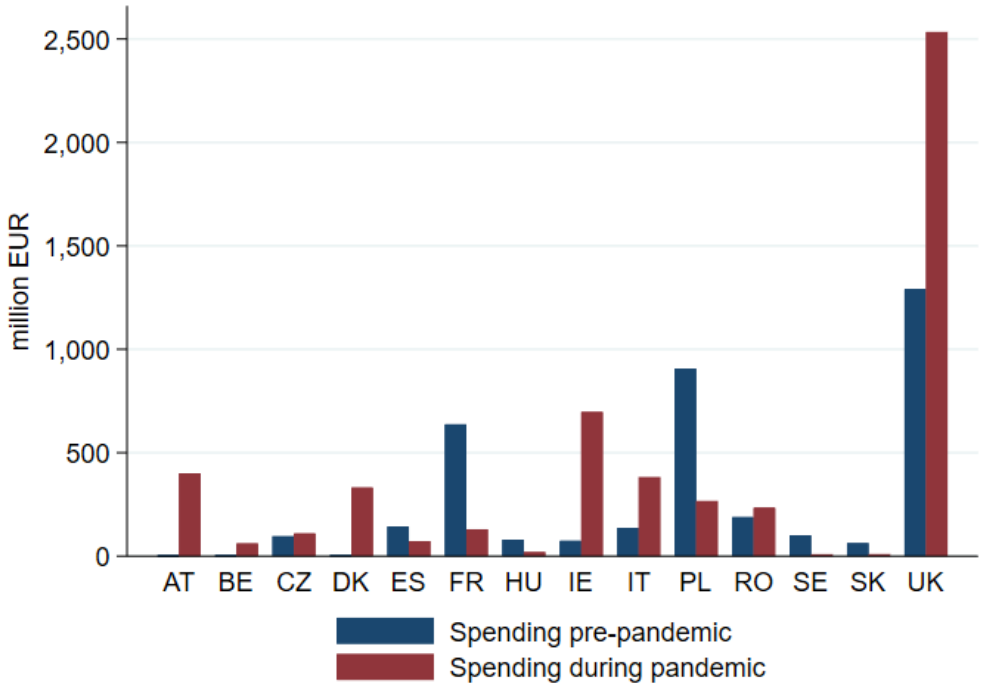

Figure 2: Observed spending on Covid-19 related contracts (in million EUR equivalent) before/during the pandemic by country

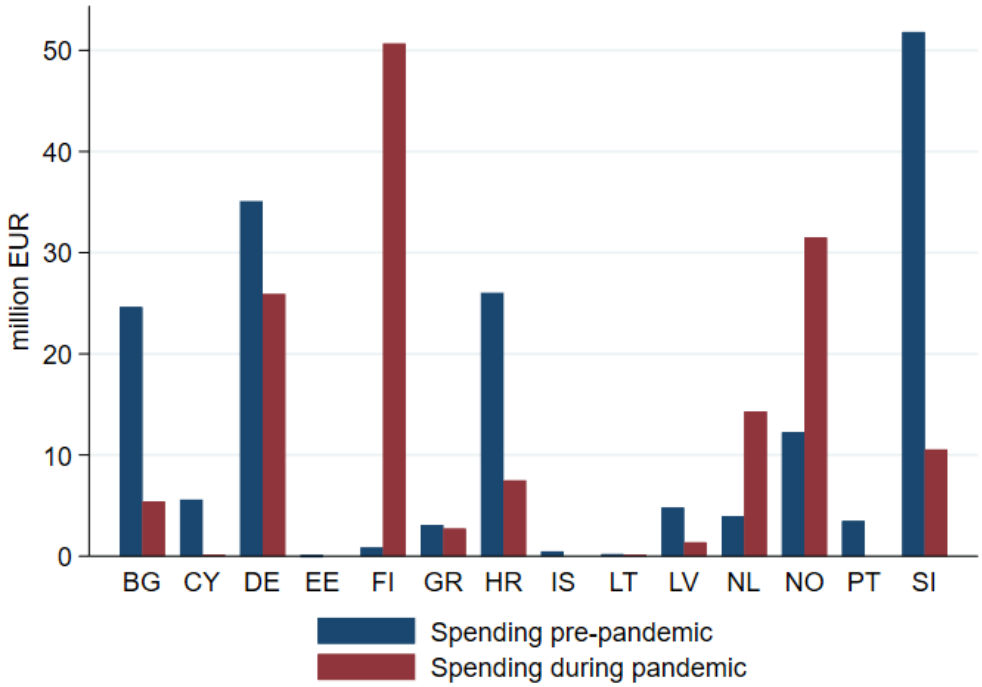

Figure 3: Observed spending on Covid-19 related contracts (in million EUR equivalent) before/during the pandemic by country

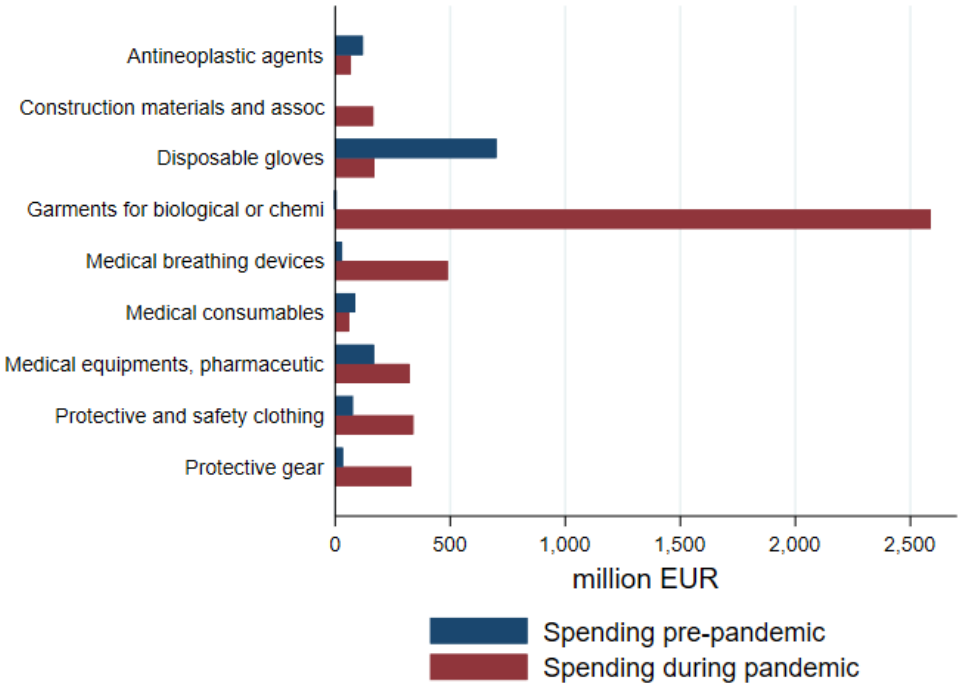

Figure 4: Spending by product category (in million EUR equivalent) before/during the pandemic

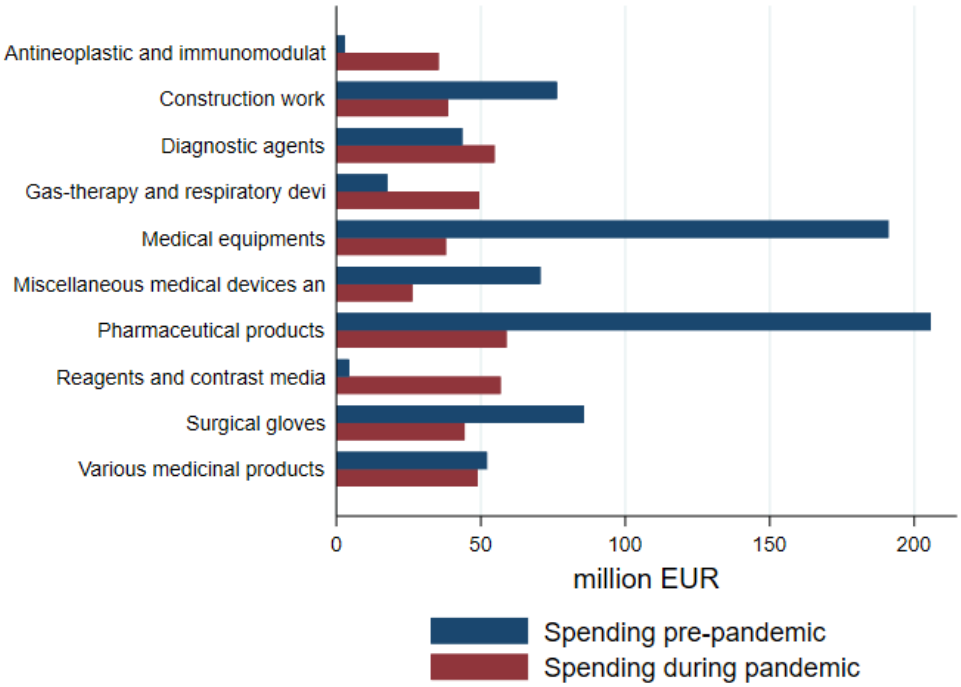

Figure 5: Spending by product category (in million EUR equivalent) before/during the pandemic

award notices on TED. 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.[14] While the contract notice is posted on TED, no contract award notices exist. In contrast with traditional contract awards, all firms that fulfilled certain conditions could deliver goods at the posted price.[15] 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. We take a closer look at the largest individual contracts further below in Table 5.

Table 4: Sum of 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/.

Net change in cross-border awards: We are interested in the share of contracts that were awarded cross-border which we compute from 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 dramatic increase in cross-border awards.[16] Figure 6 describes 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). Before the pandemic, buyers awarded over 90% of total contract value (or "volume") domestically. Over 99% of all contracts are awarded domestically, which immediately implies that cross-border awards tend to be larger, on average.[17] 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. Different purchases in several European countries, including France, the UK, and Italy, contribute to this dip. Our main outcome variable is related to the allocation of contracts to firms based on their location relative to the buyer. We investigate the variation of this outcome variable and distinguish between unweighted and value-weighted awards to demonstrate the importance of weighting contract awards by value, even if this forces us to drop observations for which this information is missing. The outcome variable is an indicator that takes the value 1 if the seller is located in the same country as the buyer and 0 if not. It is necessary to weight contract awards by award value, as there is almost no variation in the simple shares of contract awards domestically and abroad (left panel, Figure 6). Overall, the unweighted shares of domestic contract awards before and

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

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

[16] We do not account for 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, but the necessary data is not available. See e.g. Cernat and Kutlina-Dimitrova (2015).

[17] While this difference might partly be due to higher prices (e.g. due to transportation cost) of foreign goods, the difference between the two panels of Figure 6 is clearly due to the presence of foreign bulk orders and a large number of very small domestic orders, rather than per-unit price differences.

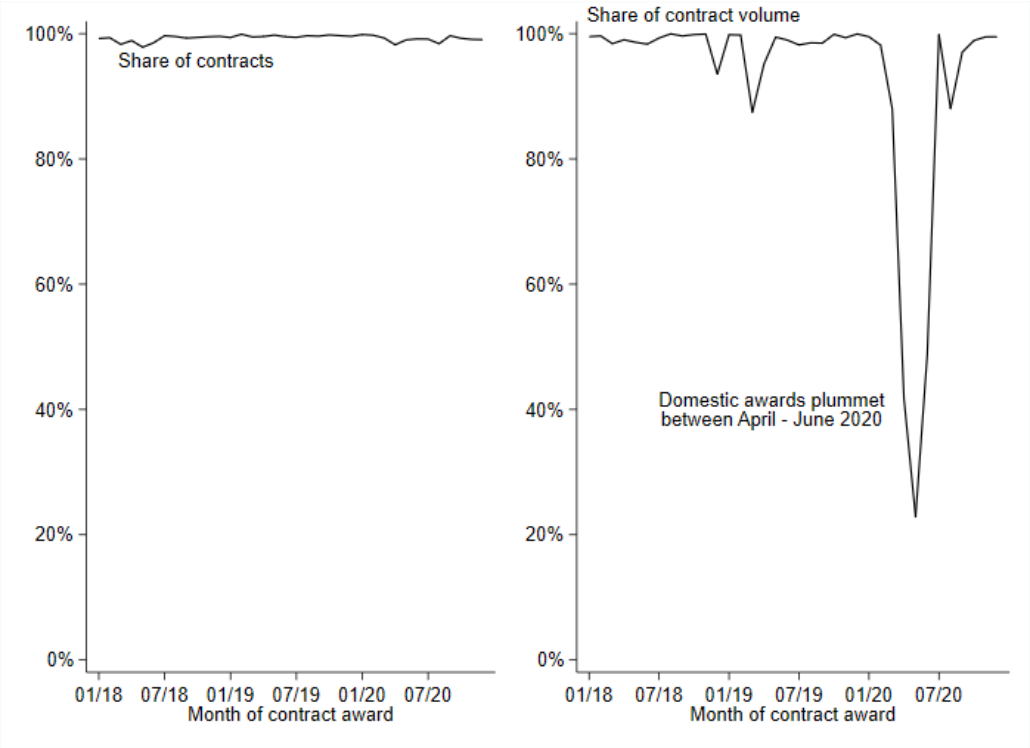

Figure 6: Share of projects awarded to domestic firms (simple share, left, value-weighted, right)

after February 2020 are 99.5% and 99.3% and the value-weighted shares of domestic contract awards are 98.5% and 60.3%.

We look at the largest lots to better understand their impact on the estimation. Table 5 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 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 a global sourcing strategy. 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.

Competition and direct awards: At the start of the pandemic, competition for contract awards was uniquely low. Figure 7 describes the share of the total value that buyers awarded "competitively" in each month. We define competitive contract awards as having several bidders 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 many 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 probably not important drivers of home bias absent the pandemic. To clarify, this does not explain why we observe cross-border awards - be it lack of alternatives or more better offers. It does, however, exclude a possible explanation for a lack of cross-border awards outside the context of the pandemic.

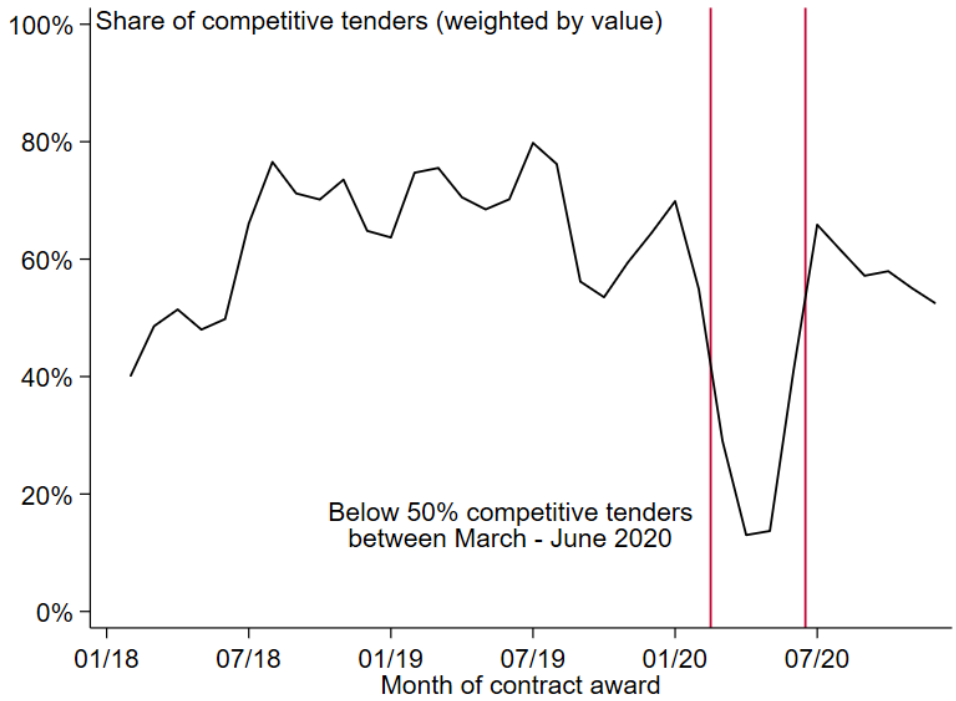

Figure 7: Share of competitive tenders (moving average)

Explanatory variables: One of our main explanatory variables is the average 14-day infection rate per 100 inhabitants (scaled down by a factor of 1000 from the original data for readable regression coefficients). The overall shape of the infection rate in our data set is plotted in Figure 8. We average the infection rate by months, weighted by the total contract value that was awarded in each region in that month. This curve tracks very 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 9. This comparison reassures us that we do not distort our analysis regarding this variable by value-weighting.

The other main explanatory variable is a dummy variable that takes the value 1 for contracts for medical supplies signed after the publication of the European Commission guidelines on procurement from April 1, 2020, and 0 before and in the control group. This represents an increase in 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. This variable is named "Regulation change" in the regression tables.

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

Table 5: Largest cross-border purchases

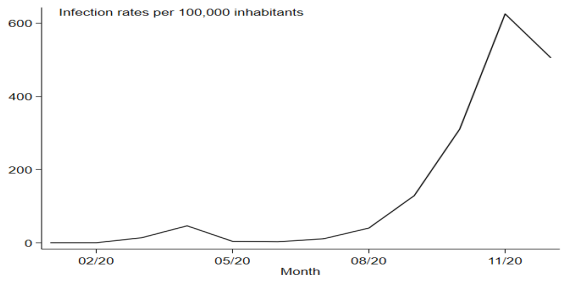

Figure 8: Monthly averages of the 14-day infection rate per 100k inhabitants in 2020 (weighted by purchase value)

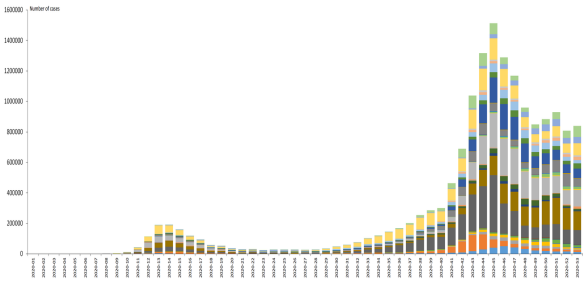

Figure 9: Weekly European infection rate according to ECDC, (c) 2021 European Centre for Disease Prevention and Control

3.3 Summary of the data

We document large heterogeneity in 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. We find that conditional on observables, information about contract size is missing more often for domestic awards post-pandemic. However, the overall distribution of contract size does not shift dramatically over the year and in particular between the periods of high and low cross-border awards, leading us to conclude that sample selection is likely of lesser importance. The largest cross-border contracts include several purchases of PPE from the UK, but also from other European countries, which is why the results of a leave-one-out robustness check without the UK provides a lower bound on effect sizes. 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 increased buyer discretion (measured through a change in regulation) as well as emergency (measured through local infection rates) on the likelihood of cross-border procurement. Our outcome of interest is an indicator for the award of a contract to a domestic firm. It seems plausible that a regulation change that increases buyer discretion by lifting regulations, should result in greater home bias if these regulations are effective. We find the opposite effect, an increase in the likelihood of a cross-border award by almost 36 percentage points following the increase in buyer discretion. This is a large increase compared to the pre-pandemic baseline when only 1.5 percent of the total value was awarded internationally. Also, we find an increase in the likelihood of a cross-border award by 19.3 percentage points for a one-standard deviation increase in infection rates. Even the lower bound effect sizes in a "leave-one-out" analysis remain economically very large, at 16 and 9.3 percentage points, respectively. If buyers have preferences that include both the outcome of any particular purchase (e.g. procuring at a low cost, awarding a contract to the most reliable supplier) and unrelated goals (such as securing favors from or jobs at domestic firms), then we suggest that an increase in Covid-19 infection rates should lead to less home bias as the procurement of medical supplies now becomes more urgent relative to other goals. Both of these motives are rationalized through a model of monitoring presented in Section 5.

4.1 Empirical strategy

We start by conjecturing the ideal experiment to causally identify the effects of emergency and deregulation. Consider a counterfactual Europe with identical infection rates but without the change in regulation. Comparing this to our data would allow a direct causal interpretation of the impact of regulation. Alternatively, if we had complete information on the bidding history of all firms, we could analyze firms' likelihood of winning bids (conditional on their bid and other bids) before and after the regulation change. This data is not available, as individual bids are not released, nor is the identity of non-winning bidders known. Conversely, a deregulation act such as the one we observe absent the pandemic could allow us to identify the effect of the crisis. It is apparent that the ideal experiment does not exist: non-European procurement markets, 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 emergency 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, with our chosen dummies 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 be of importance. 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, requiring buyers to run open and competitive tenders to solicit bids, to post transparent award criteria, and to submit themselves to review by external authorities. Key to our analysis of buyer discretion is a change in procurement rules through an announcement by the European Commission on April 1st, 2020, two months after the start of the pandemic.[18] The announcement clarifies that all common rules on procurement are lifted in face of the emergency. It lists the usual regulations

[18]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.

(publication requirements, time limits, minimum number of candidates) and dismisses them summarily. Instead, buyers could now 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 sometimes mentioned among the general comments of some procurement tender documents. These rules are specific to procurement contracts which require extreme urgency due to the pandemic. Generally, this limits the applicability of these rules to tenders for goods and services that are directly related to the pandemic, not to procurement overall. As a caveat, 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.[19]

We present three distinct regression analyses. In the first step, we analyze the data set of procurement tenders for medical supplies using a naive OLS regression and logit. This serves primarily as an alternative way of summarizing the correlations and trends displayed graphically in Section 3 and secondarily as a benchmark to compare the two remaining analyses to. The effects of deregulation and infection rates admit a causal interpretation only if we assumed that infection rates and regulation were randomly assigned. This is a very strong assumption which would face obvious challenges: infection rates are likely endogenous, as they depend on previous infection rates, infection rates in neighboring regions, and factors that might be correlated with procurement outcomes or cross-border procurement, such as the efficiency of the public sector or existing trade links. Even changes in infection rates might not be random or quasi-random, as they might be influenced, e.g. by the crisis management of national or regional governments which might also 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 closely related product groups (see Appendix A), not just medical supplies. We assume that contracts that are not Covid-19 related fall outside the deregulation and provide a control group. 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.

[19] 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."

4.2 Naive estimation

As explained in the previous section, 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 analyses. 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 which is identified for the location of contract performance for observation i,[20], 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 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. The constant is absorbed in the full set of dummies and therefore not required for unbiased estimation. Estimation is via Weighted-Least-Squares where each observation is weighted by the value of the award. Thus we put greater emphasis on larger awards. 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 6. 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 with standard errors below the regression coefficients in Table 6. The effects are negative and large, both relative to standard errors and economically: after regulation, depending on specification, cross-border awards are 20 to 30 percentage points more likely. This is unsurprising given the large drop in domestic awards in the descriptive data, but as a simple before-after comparison doesn't admit a causal interpretation because we cannot rule out trends or other important contemporary factors. This approach is distinct from the difference-in-differences estimation in Section 4.4 which addresses these issues by means of a comparison with a control group of similar products for which we assume that the evolution of procurement outcomes in the control group provides a counterfactual for developments in the treatment group during the time when the change in regulation was active.

In line with the rest of the literature, and appropriately for a binary outcome variable, we also estimate a logit model with a slightly changed regression equation. Since 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. Its results are reported in Table 7. 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

[20] 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.

	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 squared		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)
regulation effect	-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

** p<0.05, *** p<0.01, robust standard errors in parentheses. 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 6: Linear regression for contract award to domestic companies

numerical stability. This has no effect on the likelihood or estimates of the remaining variables. Most effects go in the same direction as in the linear model. Two differences 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.

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 predict 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 emergency is greater. Note that the model is silent on why or how the government values purchases from one or the other firm. Consistent with the descriptive statistics presented in Section 3.2, it is clear, however, that the effect of infection rates on cross-border procurement is, if anything,

	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

* p<0.10, ** p<0.05, *** p<0.01, robust standard errors of coefficients and z-statistics of marginal effects in parenthesis.

Table 7: Logit regression coefficients and average marginal effects for contract award to domestic companies

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 staggered difference-in-differences analysis (Callaway and Sant'Anna, 2020; Athey and Imbens, 2021). The characteristic of the staggered difference-in-differences approach 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. By contrast, the setup in Section 4.4 is standard, with one treatment and one control group and a treatment dummy that comes into effect for all treatment units in the same period.

4.3.1 Difference-in-differences diagnostics

We start by investigating the standard "parallel trends" assumption and potential spillover in treatment or outcomes. The appropriate diagnostics depend on the shape of our data. Due to the availability of infection rate data on a weekly basis, we create a variable that codes the calendar week to which each contract award belongs. We use this variable to match the contract awards with the applicable infection rates. Geographical groups are formed by the different NUTS-regions of contract performance location. 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.

Practically, 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. We, somewhat arbitrarily, designate regions that first experienced infections in the weeks starting January 20 or 27 as "early" and the rest as "late". Figure 10 compares the evolution of the share of domestic purchases and the average infec-

tion rates. It shows drastic 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 11 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.

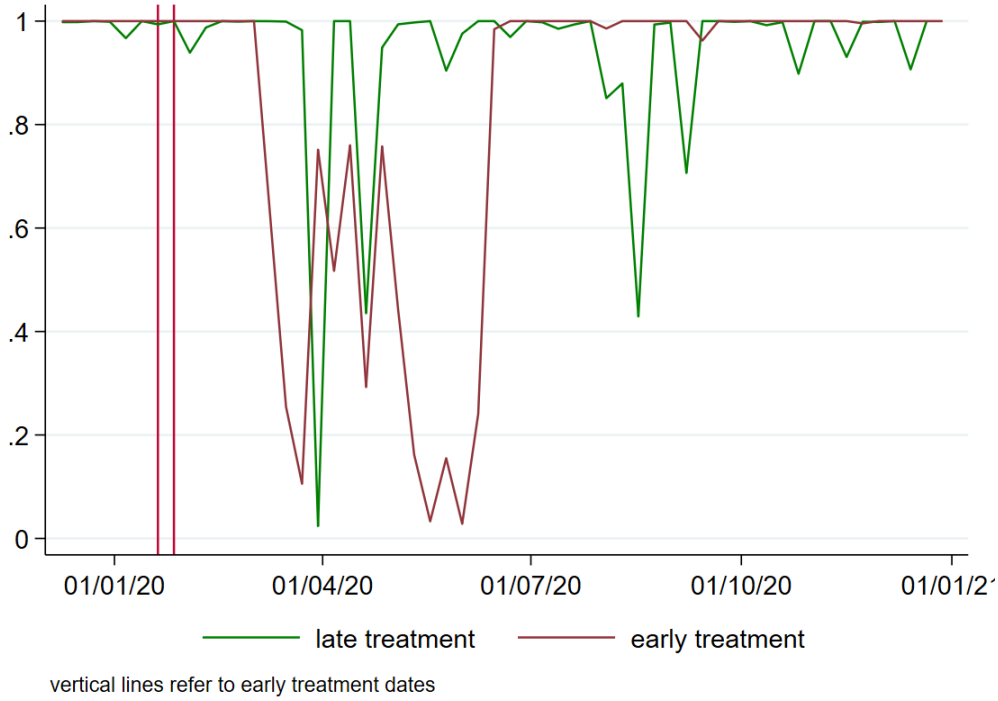

Figure 10: Domestic purchases in regions infected early/late

Spillover might occur if the treatment status of some units influences the treatment of other units (Table 8). 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 independent of whether local infections arise because of movement between regions or infections within a region. A more relevant question to ask then is if buyers are likely influenced by information from other regions when making purchasing decisions for one region. In other words, 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. Allowing for up to 6 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.[21]

We also test for spillover in outcomes (Table 9): 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

[21] We flexibly allow for up to 6 lags and then choose the model based on Bayesian Information Criterion. Allowing for longer or shorter lag structures, e.g. 6 weeks or 12 weeks of past information, yields the same result.

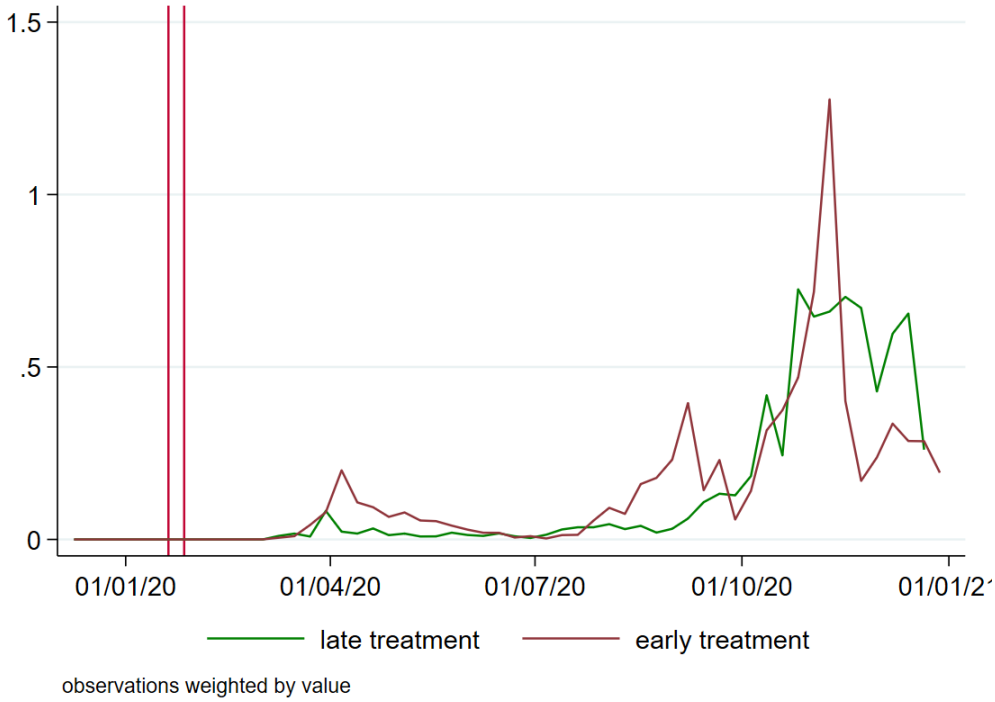

Figure 11: Average infection rates in regions infected early/late

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, nine lags chosen based on Bayesian Information Criterion.

Table 8: Test for Granger-causality of local infection rates through infection rates in the rest of the country

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.

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, which would be of little help as we do not make quantitative claims, for example about welfare, from the share of cross-border procurement.

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.

Table 9: Test for correlation between contract awards and infection rates in other parts of a country when local (national) rates are below (above) the median.

4.3.2 Infection rate: 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 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 10.

	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
adj. R-squared	0.41	0.31
N	67,638	67,387

*** p<0.01, robust standard errors in parentheses. 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 10: Linear regression for contract award to domestic companies

The estimated average treatment effect on the treated (ATET) is for an increase of the 14-day

average infection rate per 100 inhabitants by 1. It is best interpreted relative to observed standard deviations during the pandemic. We compute the standard deviation of the infection rate for observations with non-zero infection rates (otherwise the standard deviation of the infection rate would be artificially diminished through pre-pandemic observations). 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. As we have confirmed 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, given our earlier results, 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 10). Given that cross-border contracts only represent 1.5 percent of value pre-pandemic (see Figure 6), even an increase by almost ten percentage points is economically important.

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, but also as a potential source of efficiency.

In personal communication with the European Union we received confirmation from DG GROW 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 selected a control group from all product groups of the CPV that are at the same hierarchy level as the Covid-related tenders but which were not chosen. 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 Appendix A. For our regression analysis, we assume that products in the control group are similar enough to the products in the treatment group that changes in market conditions and input costs affect products similarly.

4.4.1 Difference-in-differences diagnostics

We construct a control group based on product similarity in the product classification system. 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.[22]

Figure 12 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

[22]See Eurostat, 31.03.2021: https://ec.europa.eu/eurostat/statistics-explained/index.php?title=EU_trade_in_COVID-19_related_products

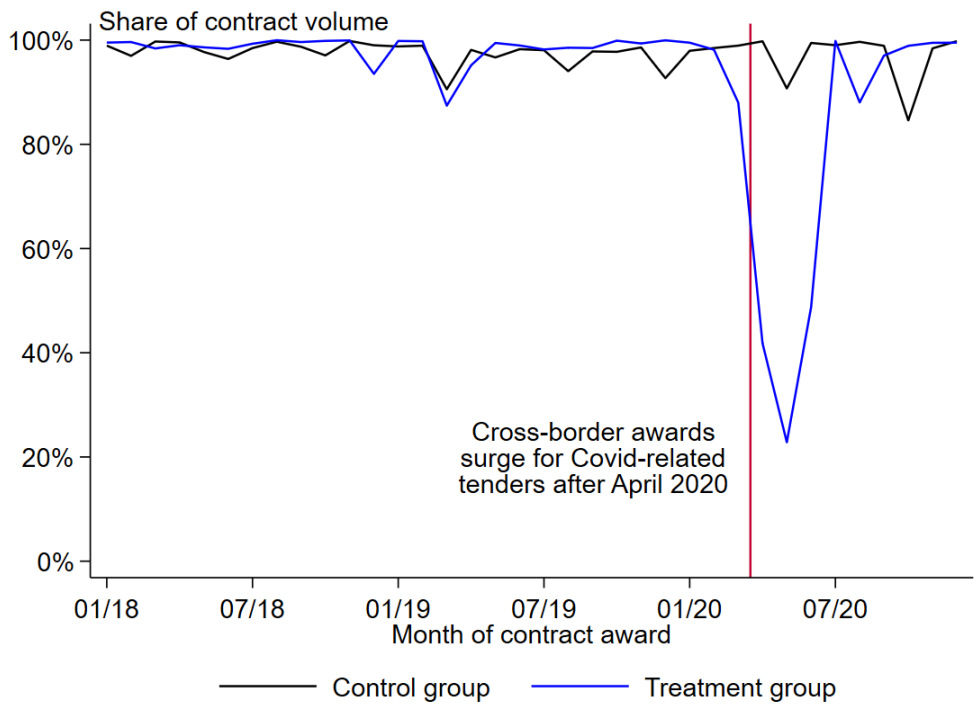

Figure 12: Average share of cross-border awards in the treatment group with greater buyer discretion and the control group

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 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, 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 lower spending during the pandemic. However, the differences are smaller. 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. A note of caution: A possible explanation for variation in average monthly spending could also be that aggregates and miscellaneous categories (such as "various medicinal products") were labelled differently (e.g., they might have been classified more carefully during the pandemic, leading to lower total purchase volumes in these "catch-all groups").

4.4.2 Deregulation: Estimation results

Finally, we estimate the following equation

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

It resembles equation 2 except that τ_i denotes whether a contract awards belongs to the treatment or control group, D_{it} takes the value 1 for observations in the treatment group after the treatment date (April 1st, 2020), and the set of control variables does not include product group dummies (to avoid colinearity with treatment group status). Again, we conduct a leave-one-out analysis by country to investigate if the regression result is driven by 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. The leave-one-out analysis provides a lower bound of 16 percentage points on this effect when the UK is removed, again reflecting large cross-border contracts from the UK. The 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 12.

	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
adj. R-squared	0.33	0.26
N	320,213	319,173

* p<0.10, ** p<0.05, *** p<0.01, robust standard errors in parentheses. 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 11: Linear regression for contract award to domestic companies

4.5 Robustness

We consider a number of robustness checks to address potential challenges to the empirical strategy. We consider ways in which the pandemic impacts 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.

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 can verify 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.

Another argument against a channel from the outcome to infection rates comes from the interpretation of quality differences: Finding that the volume of failed procurement is small and insignificant (see Table 17), any variation in the outcome variable that could potentially affect infection rates would have to be via quality differences between domestically-sourced and foreign-sourced medical supplies. In the European context it makes little sense to generally associate foreign with higher quality and domestic with lower quality as each firm is domestic from the point of view of buyers in the same country and foreign for buyers in all other countries to which it might sell. Such a view might be more relevant when firms from a technologically advanced "foreign" country, such as the US, bid for public sector contracts in a "domestic" country with a less sophisticated industry.

We cannot replicate our results in an unweighted regression on all observations, meaning that rather than weighting observations by their value, all observations have the same weight. 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 16). However, this is unsurprising, as this regression 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%.[23] 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 17). 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

[23] 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.

among public sector buyers, play a role. We re-estimate our difference-in-differences analyses with country-clustered standard errors. Country-clustered standard errors are indeed larger than White-robust standard errors (see Tables 18 and 19), but our estimates remain significant at conventional significance levels. Therefore, we do not change our conclusion regarding the impact of crisis urgency and buyer discretion.

4.6 Summary of the empirical analysis

We find that that the overall surge in cross-border awards is driven by at least two channels: both crisis urgency and deregulation that suspends 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. However, the regression results should not make us overlook what is apparent from the descriptive statistics and charts: 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.

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. 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) argue 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.

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 12 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.

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. If NR is the strictly dominant strategy of the government, the bureaucrat's best response is clearly C to obtain B rather than a payoff of 0. The other condition is $\Delta S > B$, the difference in quality must be greater than

		Bureaucrat	
		C	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

Table 12: Review-and-collusion game between government and bureaucrat

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. We proceed to explain the relation of this model to our application and implications for the empirical analysis.

5.2 Discussion of the results

We argue that this setting, even though it is very simple, is informative to predict the presence of misallocation in procurement. We begin by anticipating some possible concerns before mapping the model to our application.

What justifies the assumption that the quality differential is larger than the bribe? 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. If this condition wasn't true, then collusion would be efficient in our model in the sense that it maximizes total surplus.

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 $\frac{k_g}{\lambda \Delta S}$).

The comparative statics for equation 7 are measurable within our empirical framework as was suggested in Section 4.6. The government's cost of reviewing and punishing collusion k_G 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. Consequently, 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 important. 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, ΔS. In Section 3, we measure urgency through the local Covid-19 infection rate, but also consider alternative urgency measures. From our model, we predict that an increase in crisis urgency should result in less misallocation (and also fewer reviews which we do not observe or study, however).

6 Discussion and conclusions

We analyze the procurement of medical supplies in Europe under the special circumstances of the Covid-19 pandemic using a novel data set of recent procurement tenders. Our results suggest that buyer discretion and crisis urgency are important channels of procurement for medical supplies in the context of the Covid-19 pandemic. In our data set, we describe a temporary surge in cross-border awards as the net effect of suspended rules on buyer discretion and an emergency effect of the pandemic. Two difference-in-differences analyses differentiate between both channels and shows the separate effects of buyer discretion and urgency on cross-border awards and their implications for misallocation. 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 limitations of this study merit follow-up investigation. While our paper investigates the channels that affect home bias, it cannot show directly the effect of misallocation. A follow-up study could zoom in on a more specific product market that is clearly linked to some measurable outcome to investigate the effect of misallocation on (e.g. medical) outcomes. To make Covid-related tenders and the related product groups more comparable, it would be preferable to be able to account for demand as well as domestic capacity constraints for some goods which could push buyers abroad. 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 the buyer-seller relationships in Europe.

We draw two main conclusions from our data. First, in emergencies buyers can rapidly adjust their purchasing behavior. In spite of decades of economic policy wrangling with the low share of cross-border procurement, which especially in the EU stands in the way of the explicit policy goal of a "Single Market", we see a sudden surge at the onset of the Covid-19 pandemic. Both the local extent of emergency, measured through infection rates, and giving buyers more discretion, contributes to this surge. We rationalize our finding through a model in which the extent of misallocation is determined by the costs of monitoring buyers and the extent of their informational advantage. While the former might decrease due to heightened media attention, the latter 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. 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. 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. 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. 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.

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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".[24] 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".[25] 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 18). 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).[26] 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.[27] 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

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

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

[26] "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.

[27] 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 7 to assess the share of non-competitive tenders and Table 4 to describe the total spending by country observed in the data.

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.

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.[28] 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. Their selection is based on using items from the CPV that appear in the same sub-category. The CPV of Covid-19 related tenders are pre-defined by TED. Table 13 shows which CPV were chosen to complement treatment product groups:

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

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

Table 13: Selection of product groups by CPV for treatment and control group

B Sum of values for individual lots

This section reports 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 14 below 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 analyses.

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.[29] A dashed box marks the three months with the highest share of cross-border awards. 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 2, 3, 4 and 5, 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.

Table 14: Sum of 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/.

[29] 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.

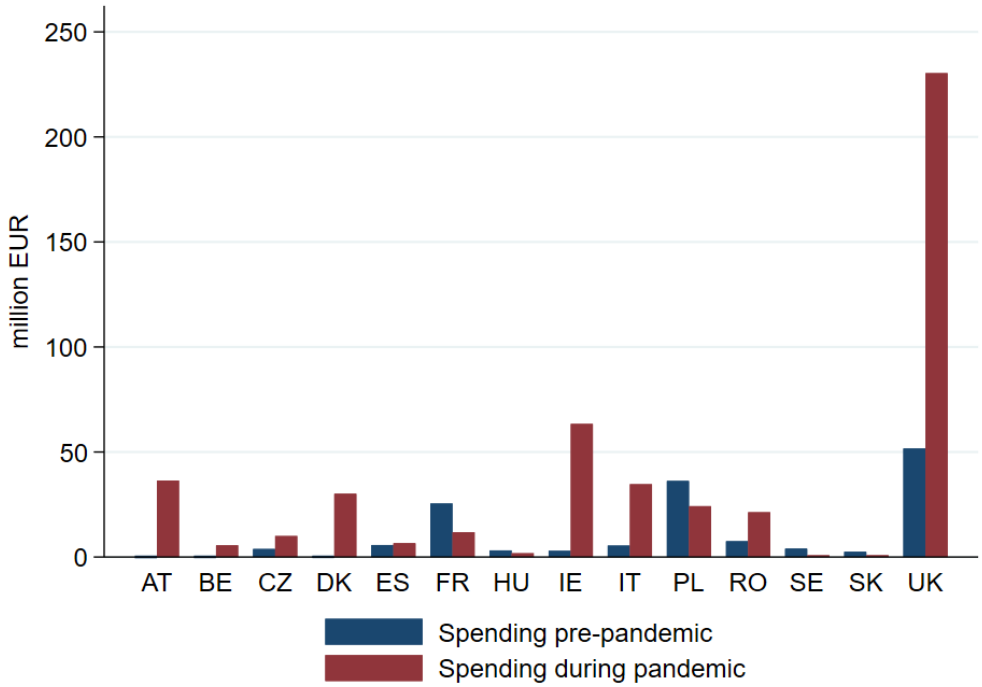

Figure 13: Observed average monthly spending on Covid-19 related contracts (in million EUR equivalent) before/during the pandemic by country

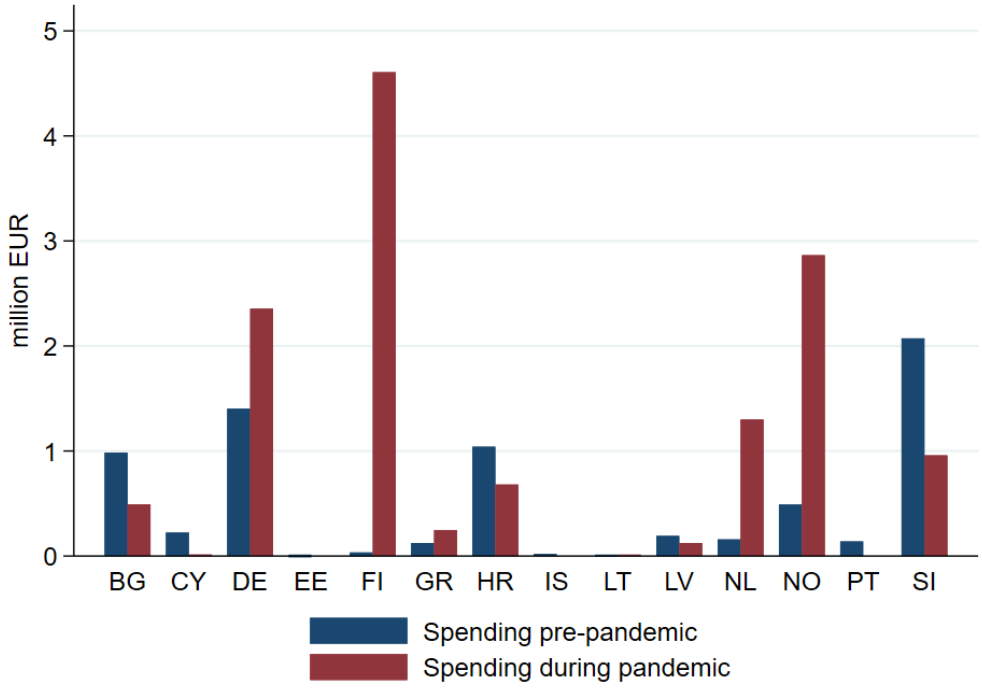

Figure 14: Observed average monthly spending on Covid-19 related contracts (in million EUR equivalent) before/during the pandemic by country

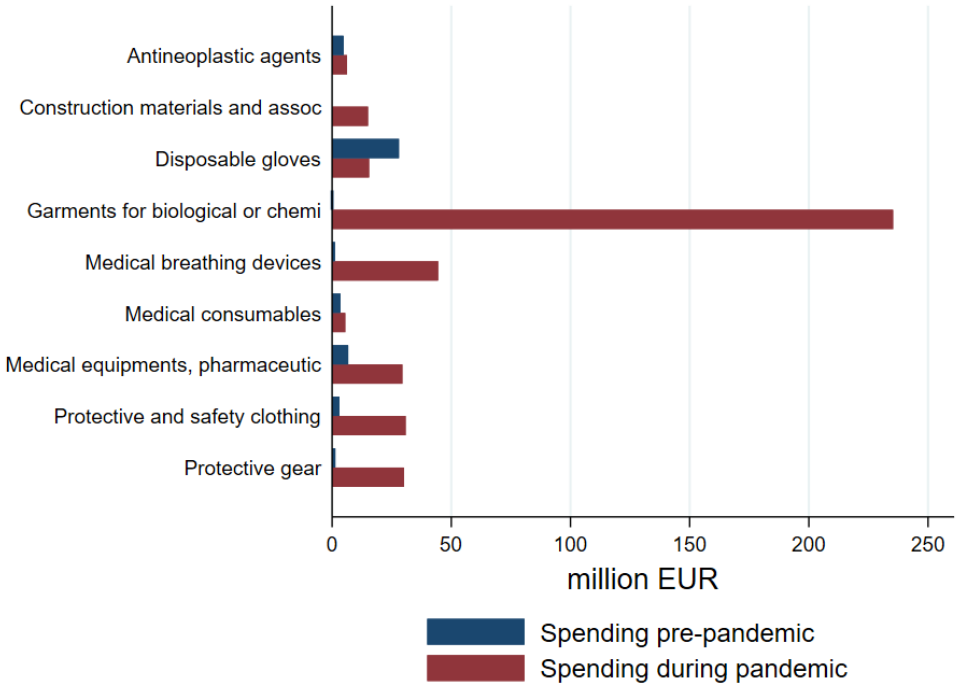

Figure 15: Average monthly spending by product category (in million EUR equivalent) before/during the pandemic

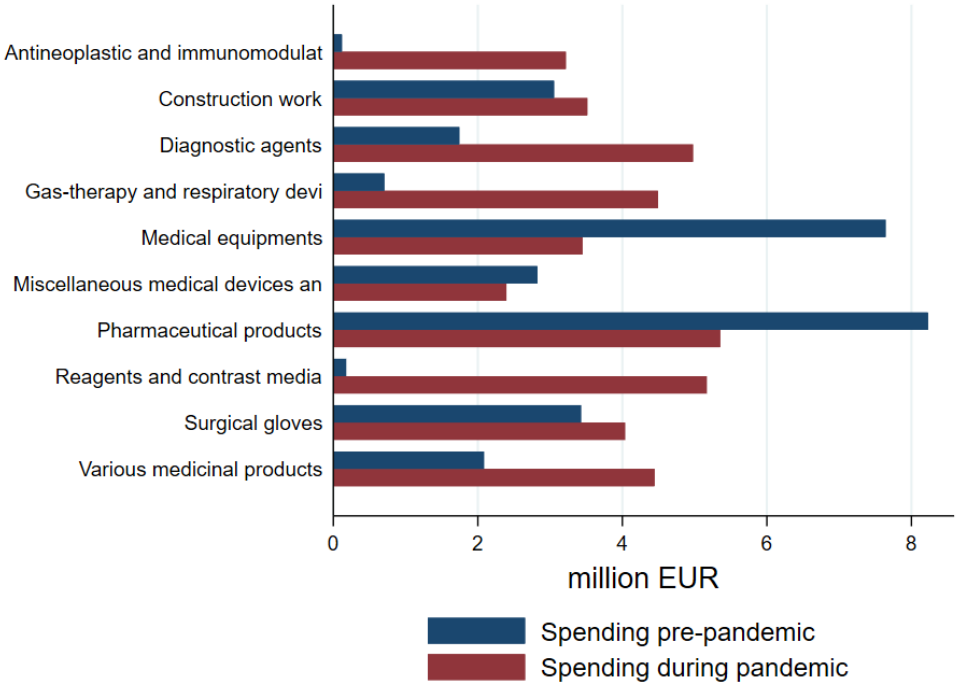

Figure 16: Average monthly spending by product category (in million EUR equivalent) before/during the pandemic

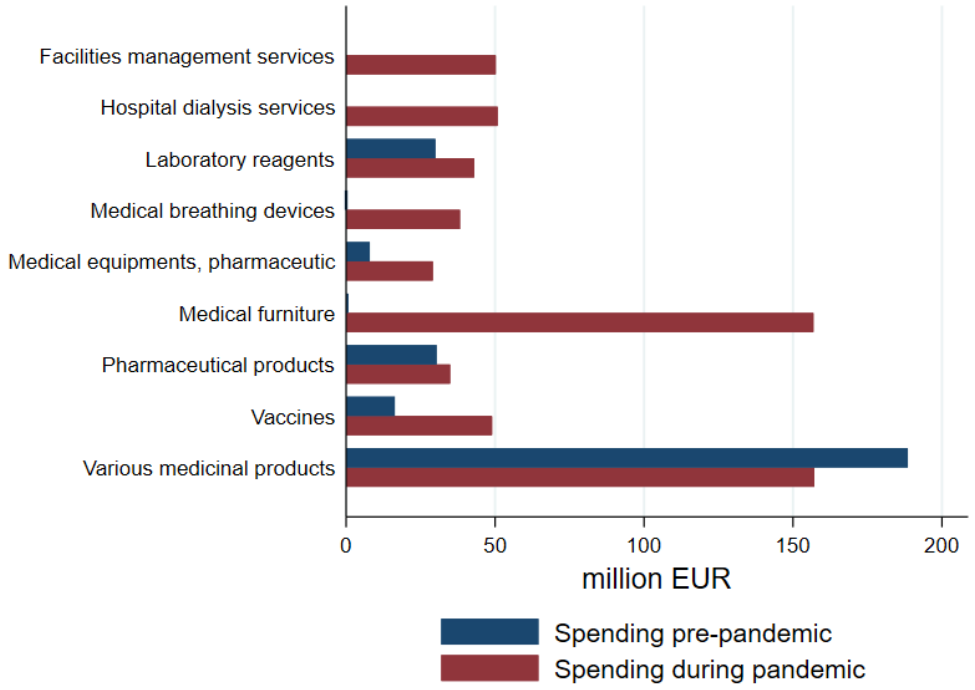

Figure 17: Average monthly spending by product category (in million EUR equivalent) before/during the pandemic (control group products Section 4.4)

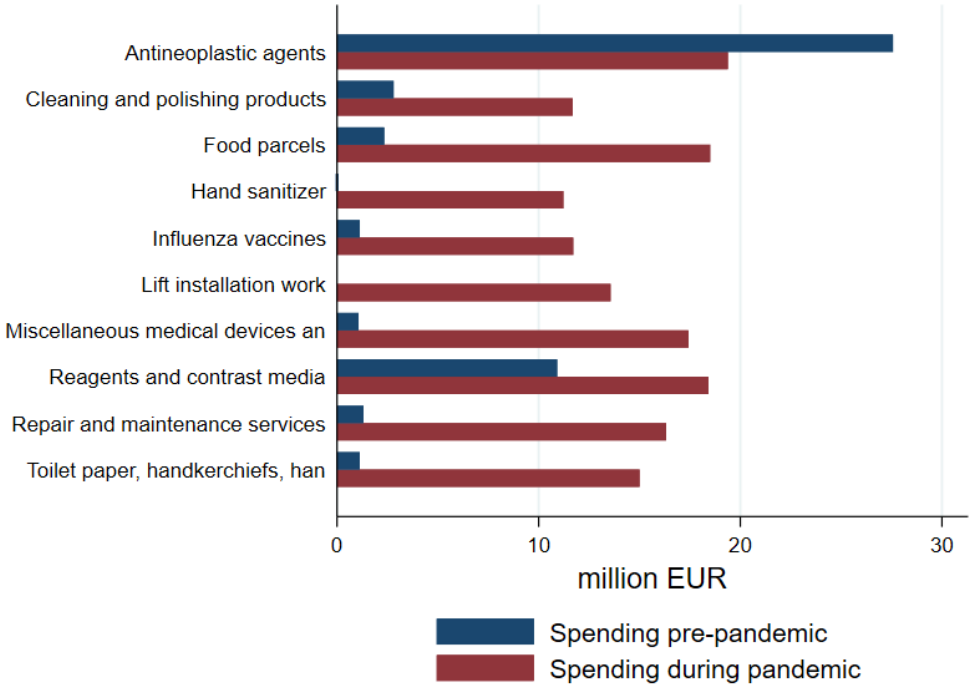

Figure 18: Average monthly spending by product category (in million EUR equivalent) before/during the pandemic (control group products Section 4.4)

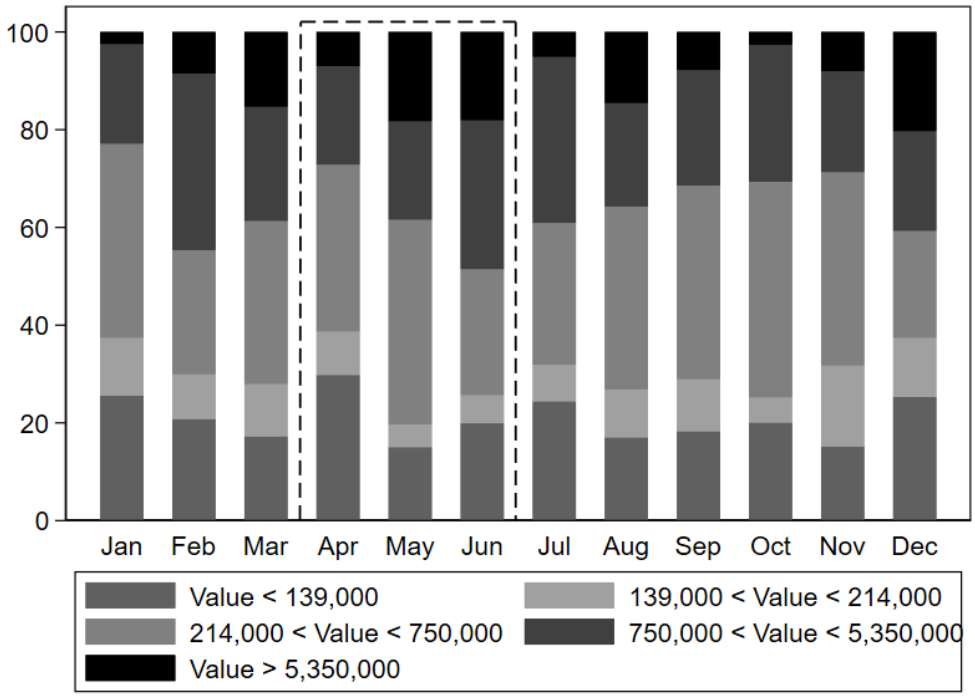

Figure 19: Award sizes in 2020 in EUR

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.

	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
adj. R-squared	0.41	0.31
N	67,638	67,387

* p<0.10, ** p<0.05, *** p<0.01, robust standard errors in parentheses.

Table 15: Replacing the local infection rate with the national infection rate after removing the local infection rate

Unweighted regression

We re-run the first difference-in-difference regression, but do not weight contracts by value. The results are reported in Table 16. 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 3.2), 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.

Analysis of failed procurement contracts

Information about previously failed procurement contracts is potentially useful. The outcome of any individual tender may be uncertain from the point of view of the buyer. 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 6, 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 17. We do not find a statistically significant effect of failed previous procurement, nor does inclusion of this variable change the estimated effect drastically.

	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	
adj. R-squared	0.33	0.25
N	120,922	540,413

* p<0.10, ** p<0.05, *** p<0.01, robust standard errors in parentheses.

Table 16: Unweighted regression for contract award to domestic companies

	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
adj. R-squared	0.34
N	21,111

* p<0.10, ** p<0.05, *** p<0.01, robust standard errors in parentheses.

Table 17: Including the cumulative sum of failed tenders

Inference based on clustered standard errors

One might worry about correlation of errors across countries, which might be driven by 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 10 and 11 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.

	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
adj. R-squared	0.41	0.31
N	67,638	67,387

* p<0.10, ** p<0.05, *** p<0.01, country-clustered standard errors in parentheses. 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 18: Regression from Section 4.3 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)	(0.000)
Share of foreign bidders	-0.030	-0.504***
	(0.135)	(0.146)
dummies	yes	yes
adj. R-squared	0.33	0.26
N	320,213	319,173

* p<0.10, ** p<0.05, *** p<0.01, country-clustered standard errors in parentheses. 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 19: Regression from Section 4.4 with clustered standard errors