Discretionary Procurement Flexibilization, Efficiency, and Rent-Seeking: Evidence from Chile During COVID-19*

Edgar Castro Mendez†
Anastasiya Yarygina‡
Matias Villalba Ortega§

December 16, 2025

Abstract: This paper examines the effects of increased bureaucratic discretion in public procurement during the COVID-19 pandemic, when the Chilean government relaxed restrictions on direct deals and expanded discretionary authority for small contracts. While greater discretion can lower administrative costs and accelerate purchasing, it may also create opportunities for rent-seeking and inefficiency. Using contract-level procurement data from 2019 to 2021, we study how these regulatory changes affected purchasing outcomes. We find that discretion substantially reduced processing times and improved reporting quality. However, it also increased unit prices in larger contracts, whereas prices for smaller contracts remained stable. We further document significant bunching just below the threshold for non-competitive direct deals, consistent with strategic contract sizing and rent-seeking. Using these estimates, we show that savings from small contracts were outweighed by higher costs in larger ones, resulting in a net fiscal loss of approximately USD 99 million. Overall, discretion appears beneficial for low-value purchases but generates inefficiencies as contract size increases.

Keywords: Public Procurement; Bureaucratic Discretion; Rent-Seeking; Administrative Efficiency; Bunching; Chile.

JEL Codes: H11, H57, D73, D72, H83

*We thank Monica Mogollon, Cesar Martinelli, Thomas Stratmann, Hugo Diaz, and the LACEA 2025 seminar participants for their helpful comments. We are especially grateful to Ana Cristina Calderon, Karen Astudillo, and Carola Pessino for their invaluable contributions to the development of this study. We also thank Philip Keefer, Andrea Lopez-Luzuriaga, Alejandro Rasteletti, and the anonymous peer reviewer for reviewing the manuscript and providing valuable suggestions. This study received financial support from the IDB Technical Cooperations RG-T3694 and RG-T4120. The authors declare that they have no financial or material interests in the results of this study. A previous version of this paper appears as Chapter 3 of Castro Mendez's doctoral dissertation at George Mason University (2023).

†ecastrom@tec.mx - Tecnologico de Monterrey, corresponding author.
‡anastasiyay@iadb.org - Inter-American Development Bank
§matias.villalba.ortega@gmail.com

1 Introduction

Increasing bureaucratic discretion in government purchases can have ambiguous effects. On the one hand, it can lead to rent-seeking and malpractice due to reduced oversight. On the other hand, it can enhance efficiency by lowering administrative costs. The net outcome is likely to depend on several factors, such as the complexity of existing procurement rules, the technical competence of the bureaucracy, the integrity of government officials, and the extent to which social norms encourage law-abiding behavior.

During the COVID-19 pandemic, Chile, along with many other countries, modified its procurement regulations to respond more quickly and effectively to the health crisis, increasing the level of discretion granted to government officials. This paper investigates the impact of such a relaxation of procurement rules on Chile's purchasing efficiency during the COVID-19 pandemic.

Chile stands out in Latin America for its relatively low levels of perceived corruption, stronger governance, and higher income compared to other countries in the region. Although its governance indicators have declined in recent years—for example, Chile ranked 21st in the Control of Corruption index and 39th in Government Effectiveness in 2014, but fell to 41st and 66th, respectively, by 2022 (World Bank, 2024)—it remains one of the best-rated countries in the region.

Government expenditure in Chile represented about 27% of GDP in 2023, with a quarter of this amount allocated to government purchases.[1] This makes procurement efficiency a crucial policy objective. This paper provides direct evidence on how changes in procurement discretion affect both efficiency and the scope for rent-seeking in a middle-income setting with relatively strong institutions.

Auctions are the gold standard for government procurement, and a substantial body of literature supports the benefits of open, structured, and transparent auctions in improving procurement outcomes. On one hand, open auctions reduce bureaucratic discretion, on the other hand, they increase efficiency by promoting fair competition. Additionally, they aim to mitigate the risk of corruption by enhancing transparency. This perspective aligns with the OECD's recommendation that competitive procedures should be the standard method of procurement (OECD, 2018). In contrast, the frequent use of highly discretionary procedures is often associated with a greater risk of corruption and is commonly regarded by scholars as an indicator of procurement malpractice (Fazekas and Kocsis, 2017; Gallego et al., 2021; Decarolis et al., 2023).

[1]For comparison, OECD countries spent an average of 12% of their GDP on public procurement, accounting for about 28.4% of total government expenditure (OECD, 2017).

Yet, procurement rules can be complex, and conducting an open auction is often a slow and cumbersome process, affecting both contractors and governments (Gerardino et al., 2017). Complexity can partially offset the potential benefits of transparency. For instance, complex procurement rules may deter potential bidders and favor local firms (Baldi et al., 2016). They also lead to deadweight losses in the form of active and passive waste (Bandiera et al., 2009). Discretion not only reduces administrative costs but also allows bureaucrats to apply their expertise and knowledge more effectively, potentially leading to efficiency gains—for example, by enabling them to select the best candidates. Similar arguments have been advanced by other authors (Bajari and Tadelis, 2001; Klein, 1991; Kelman, 2005; Banfield, 1975; Bandiera et al., 2009; Albano and Sparro, 2010).

Recent literature has found evidence of the benefits of such flexibility and bureaucratic discretion. Coviello et al. (2018) finds that increased discretion can favor project outcomes in Italy by improving the quality of bidders and decreasing the total duration of the works without affecting cost overruns. Bandiera et al. (2021) finds that granting more autonomy to purchasing officers can reduce prices by 9% without compromising quality in Pakistan. Similarly, Fazio (2022) finds that discretion significantly reduces mortality rates in Brazilian hospitals and clinics. Due to the dialectic nature of discretion, there is also evidence that bureaucratic discretion increases the likelihood of political favoritism (Baltrunaite et al., 2021; Szucs, 2023; Best et al., 2017). However, the latter papers often do not find negative impacts on procurement outcomes (Lisa Chever and Yvrande-Billon, 2017).

In early 2020, during the COVID-19 pandemic, the Chilean government introduced two changes to its public procurement rules. The first expanded the use of the *direct deal* procedure (*trato directo* in Spanish), which allows agencies to contract directly with suppliers without a competitive bidding process. Under normal circumstances, Chilean procurement law restricts this procedure to exceptional cases.[2] The reform relaxed these restrictions, enabling agencies to use direct deals with minimal justification. Initially, the change applied only to healthcare agencies, but within one month it was extended to all public entities.

Another key policy change was the introduction of *agile purchase* (*compra ágil* in Spanish), which facilitated direct deals for contracts valued under 30 UTM (approximately $1,750 USD in 2020).[3] [4] These policy changes played a crucial role in the government's emergency

[2]For more information, see `https://www.chilecompra.cl/trato-directo-comprador/`.

[3]We report values in UTM (Unidad Tributaria Mensual), a tax unit indexed to inflation and used to define procurement thresholds, including eligibility for *agile purchases*. This preserves both real values and the institutional interpretation of contract size. For reference, in 2020 one UTM was approximately $50,000 CLP (about $58 USD).

[4]Prior to the pandemic, a procedure known as *micro purchase* (*microcompra*) applied only to purchases of less than 10 UTM (approximately $580 USD). The introduction of *agile purchase* extended similar conditions to larger contracts valued at up to 30 UTM.

response, reducing processing times by an average of 10 days (see Section 4.2)[5].

To interpret how agencies responded to these changes, we introduce a simple model in Section 3. When the cost of using discretionary procedures falls, officials have more incentive to use them, particularly on larger purchases where potential gains are higher. Because the *agile purchase* rule applies only to contracts below 30 UTM, the model predicts that some contracts will be adjusted to stay just under this threshold. Therefore, a concentration of contracts just below 30 UTM after the reform is consistent with contract-size manipulation for discretionary purposes.

For this study, we collected open-source data published by *Chilecompra*, the Chilean Public Procurement Office, for all procurement processes between 2019 and 2021. We first analyze the evolution of certain contract characteristics, identifying the trend changes that occurred during the pandemic period. Then, we employ a panel data model to estimate the causal impact of increased discretion on efficiency and transparency indicators. In particular, we look at the causal effect of the increased discretion on the unit price, the probability of price paid in excess (measured as a unit price exceeding the mean plus one standard deviation), and the probability of the procurement process to report all relevant data information in the procurement information system, as a measurement of transparency.[6]

Our results show that one of the key changes during the pandemic was a rise in the use of discretionary procurement procedures, both in total value and number of contracts—particularly below the 30 UTM threshold. This bunching pattern is consistent with the predictions of our model and provides evidence of contract size manipulation associated with rent-seeking behavior. Moreover, we find evidence that the manipulation stemmed primarily from contract splitting. At the same time, we observe efficiency gains: faster processing of emergency contracts, no evidence of increased unit prices for smaller purchases, and a statistically significant, yet small, increase in the probability of overpayment. However, we also find suggestive evidence of discretion being used to increase unit prices in larger contracts.

Overall, increased discretion facilitated a more agile procurement response during the public health emergency. Our estimates suggest that discretion enhances efficiency and reporting quality for smaller contracts, but leads to inefficiencies for larger ones. Using our estimates, we find that while the "agile purchase" reform for small contracts generated savings of roughly USD 64 million, the expanded use of direct deals for larger contracts led

[5]The practice of simplifying small purchases is common. Many European Union (EU) member states, including Austria, Italy, Denmark, and Spain, frequently use restricted auctions for small contracts below EU thresholds. These contracts, regulated by national laws, are typically straightforward and rarely renegotiated. For details, see `https://single-market-economy.ec.europa.eu/single-market/public-procurement/legal-rules-and-implementation/thresholds`.

[6]See Fazekas and Kocsis (2017)

to additional costs of about USD 163 million, resulting in a net fiscal loss of approximately USD 99 million. These findings support the view that discretion in procurement is beneficial for smaller contracts but detrimental for larger ones (Auriol, 2006), likely because higher contract values create greater opportunities for rent-seeking (Martinelli, 2022).

These patterns are not uniform across agencies. For small contracts, the impact on unit prices is similar for health and non-health purchasing agencies. For large contracts using direct deals, non-health agencies show larger price increases than health-related agencies. This suggests that agencies directly involved in the emergency response used the additional discretion more cautiously, possibly because the nature of the emergency created stronger intrinsic motivations to act in the public interest or heightened expectations of accountability. We also find that contracts explicitly justified on emergency grounds, when the product was unrelated to the pandemic, were more likely to experience unit price increases.

The rest of the paper is organized as follows. Section 2 describes the institutions in Chile and details policy changes. Section 3 presents our conceptual framework and model. Section 4 presents the data and the empirical strategy. Section 5 describes the results. Finally, Section 6 concludes.

2 Institutional Framework

2.1 Public procurement in Chile

In this section, we summarize Chilean procurement rules and describe the main policy changes during the COVID pandemic. A detailed description of each purchasing mechanism is available in the Online Appendix table A.1. Procurement procedures in Chile can be grouped into three categories.

i. **Auctions:** These are divided into two types: open auctions (*Licitación Pública*) and restricted auctions (*Licitación Privada*). The key difference is that, in restricted auctions, bureaucrats have the discretion to select participants. However, this type of auction is rarely used in Chile, accounting for less than 5% of all purchases in our data.

   Auctions are both costly and complex for government agencies and suppliers. A typical purchasing process through an auction can last several weeks and involve substantial transactional costs, such as hiring lawyers and attending meetings or information sessions throughout the process. The complexity increases with the size of the contract, which is reflected in the required waiting periods. For contracts valued at less than 100 UTM, there is a mandatory 5-day waiting period. For transactions between 100

UTM and 1,000 UTM, the waiting period increases to 10 days. In these two cases, formal contracts are not required; and a simple purchase order suffices. For this reason, throughout this paper, we use "contracts" and "purchasing orders" interchangeably. For larger purchases, ranging from 1,000 UTM to 5,000 UTM, a 20-day waiting period is required. For contracts exceeding 5,000 UTM, the waiting period extends to 30 days.

Auctions also provide more safeguards for participants, and it is not uncommon for participants to resort to lawsuits or other legal measures to ensure fairness in the competition process. Defining the competition parameters is crucial, given the concerns they may raise if perceived as favoring certain bidders.

ii. **Framework Agreements:** (*Acuerdos Marco*) Purchases through this method are performed on the centralized online marketplace (tienda electronica), similar to Amazon, where pre-approved suppliers are connected with government agencies to facilitate the purchase of standard products. Suppliers are vetted by Chilecompra through a competitive process to be listed on the platform. The platform is user-friendly and fast, allowing agencies to efficiently procure goods and services from a pre-selected set of supplies.

Procurement officers must first seek products on this platform before considering alternative procedures, making it the preferred legal option for purchasing standard products. However, agencies are limited to the suppliers available on the platform, which reduces their discretion.

For purchases above 1000 UTM, agencies must notify all catalog suppliers and allow at least 10 days for bid submissions, unless there is an emergency. As noted by (Celhay et al., 2022), the platform's product standardization promotes competition. However, OECD (2022) points out that most contracts are awarded without a second-stage competition, which is mostly limited to the largest purchases.

In addition, in recent years, the Chilean government has implemented policies to encourage small businesses to register and offer their services. Despite this, according to (OECD, 2017), 10% of suppliers account for 90% of the contracts and their value. This concentration suggests that a small number of high-quality suppliers may be capturing most of the contracts, which could indicate that many other suppliers are not as competitive. This could limit the platform's diversity and overall efficiency for lacking of real competition.

iii. **Direct Deals:** These procedures are non-competitive and highly discretionary. Under Chilean public procurement regulation, direct deals, or direct purchases, require at

least three different suppliers to provide viable quotations[7], along with a public declaration and a resolution citing a valid reason for using this procedure. Valid reasons include having a unique supplier, emergencies, contracts of an indispensable nature, confidential contracts authorized by supreme decree, or as a last resort when auctions have failed. While only three quotations are required in this process, public procurement officers responsible for the purchase typically have access to the broader market to select candidates. This can be advantageous as it does not require pre-registration, unlike framework agreements.

During the pandemic, three categories of procedures were used: "Micro-purchase" (*Microcompra*) for contracts under 10 UTM (discontinued in July 2021),"agile purchases" (*Compra Ágil*) for contracts under 30 UTM (introduced in April 2020), and regular direct deals (*Trato Directo*) for larger contracts. The key difference between smaller and larger contracts is that regular direct deals require formal disclosure by the purchasing agency head, while agile purchases do not. Importantly, disclosure requirements can be waived during emergencies.

2.2 Policy changes during the COVID-19 pandemic

As mentioned in the introduction, Chile implemented two significant policy changes during the COVID-19 emergency. First, a temporary provision authorized agencies to use direct deal procedures for emergency response. Second, a permanent change introduced "agile purchases" for transactions below the 30 UTM threshold.

These changes were initiated with the declaration of a COVID emergency on February 8, 2020, through Decree 4 of 2020. The first policy granted key healthcare agencies, including the Health Subsecretary, Assistance Networks Subsecretary, Regional Health Secretaries, Public Health Institute, National Health Fund, Health Superintendency, CENABAST, and public hospitals and clinics, the authority to directly acquire goods for the health emergency and exercise hiring discretion. Additionally, some of these agencies were given broader discretion to regulate the health market, including direct intervention in the supply chain, price controls, rationing quotas, and managing the distribution of hospital supplies.

On March 18, 2020, the Ministry of the Interior and Public Safety declared a state of constitutional exception due to public calamity through Supreme Decree No. 104. This measure allowed all public buyers and agencies to make large purchases (greater than 30 UTM) via restricted auctions and direct deals, requiring only the approval of the respective

[7]Quotations refer to the price offers or estimates provided by different suppliers for a specific product or service.

agency head.

In April 2020, agile purchases were introduced for transactions below the 30 UTM threshold. This procedure allowed direct deals, facilitating the acquisition of goods with only a purchase order accepted by a supplier and without formal disclosure.

3 Conceptual Framework

Consider a bureaucrat faced with a finite number of purchase needs, denoted by N. For each purchase need $i \in \{1, 2, ..., N\}$, the bureaucrat must decide on two key variables: the type of procurement procedure and the size of the contract. The variable k_i represents the type of procedure chosen, where $k_i = 1$ indicates a discretionary procedure, such as a direct deal or agile purchase, and $k_i = 0$ represents a non-discretionary procedure, typically involving an auction. The second variable, y_i, represents the size of the contract for purchase need i, which reflects the monetary value associated with the transaction.

The bureaucrat's utility function is influenced by the characteristics of the product to be purchased, denoted by x_i, and the market conditions, represented by the number of potential suppliers q_i. The utility also depends on the choice of procedure k_i and the size of the contract y_i. Formally, the utility function can be written as:

$$u(k_i, y_i, x_i, q_i) = f(k_i, x_i, q_i) + r(y_i, k_i) - c(k_i, y_i),$$

where $f(k_i, x_i, q_i, y_i)$ captures the bureaucrat's utility from satifying a procurement need and depends on product characteristics and market conditions, while $r(y_i, k_i)$ represents the rents that the bureaucrat can extract from the transaction. These rents may arise from reduced effort in managing the transaction or direct bribes from suppliers. In accordance with the literature, it is reasonable to assume that the bureaucrat's ability to extract rents increases with the size of the contract, implying that the partial derivative of rents with respect to y_i is positive: $r'(y_i, k_i) > 0$ (Auriol, 2006; Martinelli, 2022). The ability to extract rents also depends on the choice of procurement procedures; we assume that $r(y_i, 1) > r(y_i, 0)$, since discretionary procedures provide fewer oversight mechanisms and greater room for opportunistic behavior compared to less discretionary methods.

The bureaucrat incurs effort costs depending on the procedure type k_i and contract size y_i. Discretionary procedures, such as direct deals, typically require less effort due to their simplicity, whereas non-discretionary procedures, such as auctions, are more costly. Thus, we assume $c(1, y_i) < c(0, y_i)$ for a contract of any size. Additionally, administrative costs increase and are convex in y_i, as larger contracts require more stringent procedures and

take longer to finalize, as discussed in Section 2. We assume continuity and convexity for simplicity.

So far, the bureaucrat's payoff depends on satisfying a procurement need, effort, and capturing rents. However, the problem would be incomplete without considering the likelihood of an audit and the potential punishment if these rents are discovered. We introduce an additional term, $A(r(y_i, k_i))$, which represents the expected value of punishment. We assume that the expected punishment increases with the size of the rent, so $A'(r(y_i, k_i)) > 0$, because enforcement agencies have limited resources and tend to prioritize high-stakes cases, making large contracts more susceptible to detection and sanctioning. Moreover, large rents are arguably more noticeable and provide stronger incentives for whistleblowers.

The bureaucrat's objective is to maximize utility net of administrative costs and the expected punishment for each purchase. The decision problem can thus be written as:

$$\max_{k_i, y_i} \left[u(k_i, y_i, x_i, q_i) - A(r(y_i, k_i)) \right].$$

This formulation captures the trade-offs faced by the bureaucrat: maximizing utility through contract size and procedure choice while balancing the risk of potential audits. We assume that utility increases with the size of the contract, y_i, since larger contracts provide greater opportunities for rent-seeking. Discretionary procedures, though cheaper, may facilitate rent-seeking behavior more effectively.

We simplify the problem by assuming that the agent chooses the contract size rather than the level of rents per purchase. This assumption is justified because rent-seeking is typically constrained by external factors such as market conditions and contract size, the latter being the only one that we model explicitly. Since our focus is on the bureaucrat's choice of procurement procedures, introducing an explicit rent choice would add complexity without significantly altering the model's main implications. Instead, we assume rents arise from the interaction between contract size and discretion rather than being directly controlled by the agent.

It is important to note that we do not impose specific assumptions on the marginal effects of the product characteristics, x_i, and the number of potential suppliers, q_i, in the utility function. Depending on the nature of the product, it may be optimal for the bureaucrat to purchase in either smaller or larger quantities, reflecting differing scales of efficiency and procurement needs. For example, consider a product like staples (without loss of generality). Arguably, it does not make sense to purchase a large supply of staples, and their optimal contract size should be small.

With respect to the number of suppliers, q_i, the general belief is that having a larger pool

of suppliers tends to increase competition among bidders and reduce search costs by making it easier and faster to find a willing supplier. However, some literature suggests that restricted competition (fewer suppliers) may improve efficiency in complex transactions. For instance, Bajari et al. (2009) and Heijboer and Telgen (2002) argue that in such cases, buyers may intentionally limit the number of competitors or negotiate directly with a single candidate. This approach is often justified using an "incomplete contracts" framework, where buyers prefer to negotiate rather than face potential inefficiencies when dealing with technically complex or hard-to-specify goods and services.

To solve the bureaucrat's maximization problem, we assume that both the benefits (through rent-seeking) and the costs (through increased scrutiny) depend on the size of the contract y_i. The bureaucrat's utility, $u(k_i, y_i, x_i, q_i)$, increases with y_i due to greater opportunities for rent-seeking, but the likelihood of the contract drawing attention and being audited also increases as y_i grows.

The first-order condition with respect to the continuous variable y_i is now:

$$r'(y_i, k_i) - A'(r(y_i, k_i))r'(y_i, k_i) - c'(k_i, y_i) = 0, \tag{1}$$

which implies that the bureaucrat will continue to increase the contract size until the marginal utility from rent-seeking, $r'(y_i, k_i)$, is offset by both the marginal punishment $A'(r(y_i, k_i))r'(y_i)$ and the marginal administrative cost, which captures the total marginal cost.

For the discrete decision k_i, the bureaucrat compares the total utility from choosing a discretionary procedure $k_i = 1$ with the utility from a non-discretionary procedure $k_i = 0$. The bureaucrat will choose $k_i = 1$ if:

$$u(1, y_i, x_i, q_i) - A(r(y_i, 1)) - c(1, y_i) > u(0, y_i, x_i, q_i) - A(r(y_i, 0)) - c(0, y_i). \tag{2}$$

This condition highlights the trade-off between the potential for greater rents under $k_i = 1$, offset by the higher administrative costs and the increased likelihood of audits associated with larger contracts. If this condition does not hold, the bureaucrat will opt for the non-discretionary procedure $k_i = 0$, which may involve lower rents but also reduced risk.

After determining the optimal choice of k_i, it is important to note that non-discretionary procedures may be unfeasible for small purchases due to high administrative costs. Assuming this cost is constant across products and contract sizes, it is only justified for contracts exceeding a certain threshold. Consider the staples example mentioned earlier. Given the low value of the purchase, the only logical choice is a small contract with a discretionary procedure. In such cases, the costs of a non-discretionary procedure outweigh its benefits,

making it an inefficient option for small contracts.

Impact of COVID policies on the model

The first policy implemented during the COVID pandemic initially authorized only healthcare agencies to use discretionary purchases, but this authorization was soon extended to all public buyers. For the purposes of this paper, we treat all agencies equally in the model. The primary impact of this policy is a **notch** in the expected audit cost $A(r)$, which results from the government's explicit authorization of discretionary procedures. This authorization reduces the likelihood or severity of audits, effectively lowering the bureaucrat's expected punishment from rent-seeking across all contract sizes.

The second policy change, which simplified purchases below 30 UTM through the "Agile Purchases" procedure, further affects $A(r)$ by reducing expected audit costs even more for contracts below the 30 UTM threshold. This creates a discontinuity in $A(r)$ at the threshold, incentivizing greater use of discretionary procedures and smaller contracts compared to an otherwise monotonically increasing $A(r)$.

The impact of these policy changes is represented graphically in Figure 1. The dotted black line in the left panel denotes the expected audit cost before the pandemic, which is assumed to be monotonic and increasing with contract size. The solid red line represents the changes in $A(r)$ described above. In the right panel, the dotted red line represents rents, which, for simplicity, are also assumed to be monotonically increasing with contract size. The solid blue line represents expected rents net of audit costs, showing higher net rents just below the 30 UTM threshold and lower net rents for contracts slightly above this threshold.

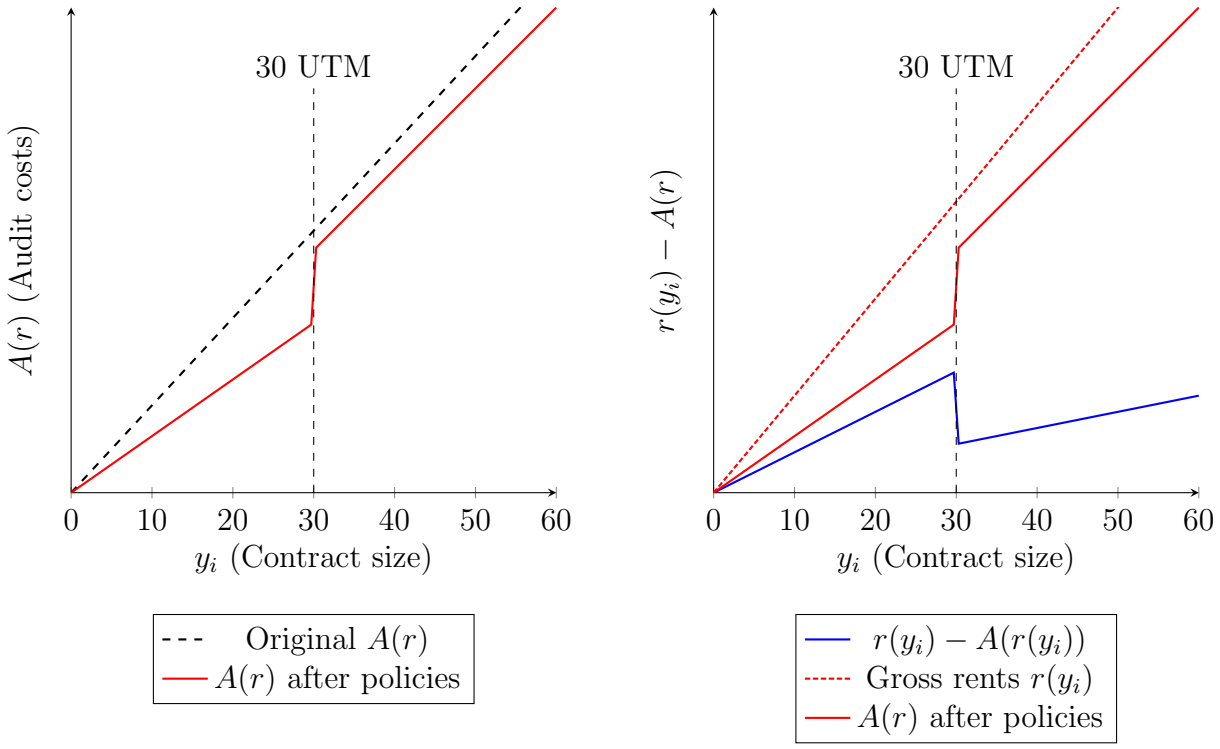


Figure 1: Expected impacts of policy changes

The problem is conceptually similar to that in Kleven (2016). Our model predicts bunching behavior in response to the policy changes described above. To illustrate this, we use the same type of graphs as Kleven to depict notch-induced bunching. The left panel of Figure 2 presents this behavior through a budget set diagram and the right panel presents it with a density distribution.

While the first policy increases the expected net rents across all contract sizes, the Agile Purchases policy generates a notch point at y^* by introducing a discrete change in the net rent-seeking rate only for contracts smaller than the $y*$ threshold. This causes individuals to adjust their choices and, for some, to bunch at the notch point. We define the individual whose optimal contract size before both policy changes was y'' as the "marginal buncher", as they are completely indifferent between choosing $y*$ and y'' after the reforms. That is, individuals with contract sizes above y'' before the reforms do not bunch at the notch point, but instead reduce their optimal contract size slightly, proportionally to the increase in rate from the first policy. In the interval (y', y'') individuals will bunch at the notch point.

In the right panel, this is reflected in the contract size density, where contracts originally between y^* and y'' are displaced to the notch point y^*. For contract sizes larger than y', the density must be lower because the contracts that would have been in this range have either been shifted downward to bunch at y^* or removed entirely, reducing the overall mass

of contracts for larger sizes.

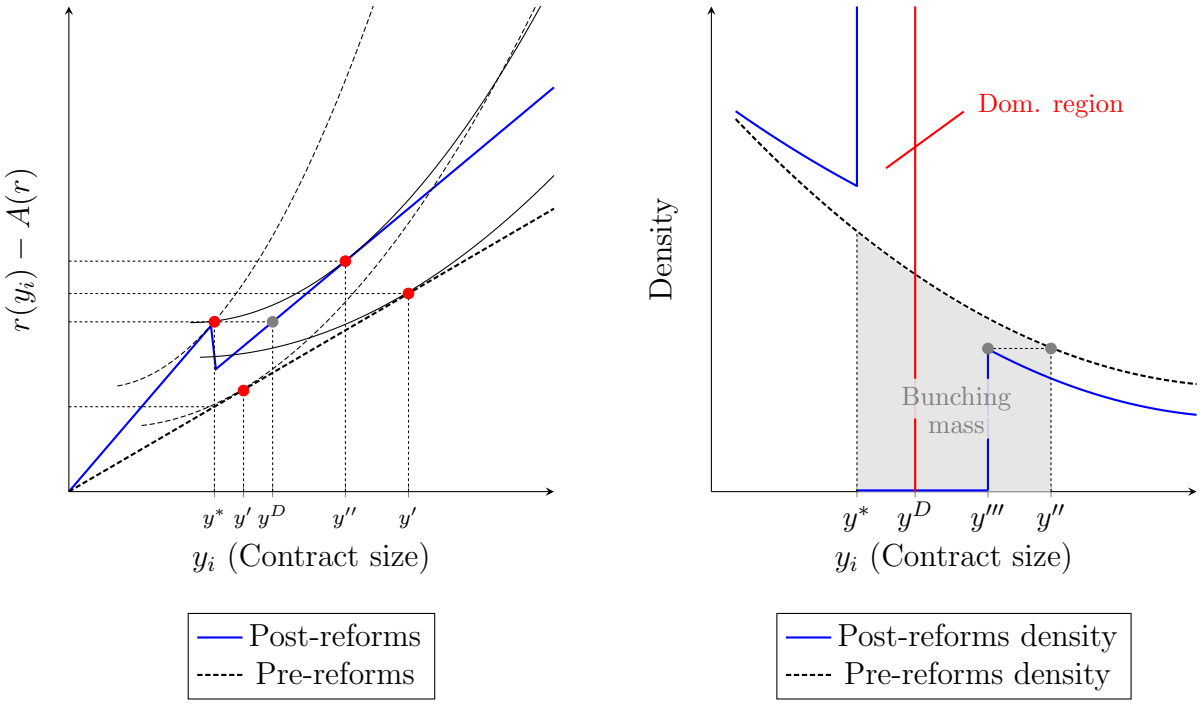


Figure 2: Expected bunching from policy changes

3.1 Efficiency and rents

We measure efficiency in public procurement using the unit price of purchased products. While our model primarily focuses on the rents that bureaucrats can extract from transactions, these rents have a direct relationship with unit prices: greater rents typically lead to higher unit prices. This assumption follows from the idea that when bureaucrats extract rents, whether through reduced effort or bribes, the cost of procurement increases, resulting in a higher unit price.

However, policy changes may influence procurement outcomes through multiple channels beyond rent-seeking. In particular, they can affect both the level of scrutiny and the degree of competition. Chilecompra enforces a vetting system through framework agreements, which restricts the pool of eligible suppliers and, in turn, reduces competition within each procurement process. While this system may improve oversight and simplify procedures, it can also limit access to lower-cost suppliers. Empirical evidence suggests that the degree of competition plays a central role in shaping procurement outcomes. Carril et al. (2022) shows that competition significantly reduces unit prices in U.S. defense contracts, while Bandiera et al. (2009) finds that procurement officers obtain better prices when they can source from a broader supplier market outside framework agreements. Similarly, in Chile, discretionary

procedures enable procurement officers to select suppliers from a larger pool, potentially increasing competition and lowering prices.

At the same time, changes in procurement rules can affect bureaucratic incentives to seek better deals. When regulations impose strict procedural constraints, bureaucrats may prioritize compliance over cost-effectiveness, potentially leading to inefficient purchases (Kelman, 1990). Greater discretion, however, may allow procurement officers to identify better suppliers and negotiate lower prices, provided they act in the public interest rather than pursue rent-seeking (Laffont and Tirole, 1993; Bandiera et al., 2009).

In summary, multiple opposing forces are at play. On one hand, reduced audit costs may encourage rent-seeking behavior, driving prices up. On the other hand, increased competition, information efficiency gains, and reductions in waste caused by procedural complexity could push prices down. The net effect of these forces is ultimately an empirical question, which we investigate in the following chapters.

4 Empirical Strategy

4.1 Data description

The data for this study comes from the *ChileCompra Open Data Portal*[8], which provides comprehensive information on government procurement. Our dataset includes over 14 million purchases made between 2019 and 2021, covering all government agencies and various procurement methods. Table 1 summarizes key statistics, including the number of purchase orders, tenders, procurement methods, and average contract values by contract characteristics.

[Table 1 about here]

We restrict the analysis to products that allow comparison before and after the policy change. First, we require that each product be purchased by at least one agency in both the pre- and post-reform periods. Second, we group procurement procedures into three types: (i) direct deals, (ii) auctions, and (iii) framework agreements, and we require that each product appear under all three types of procedure in both periods. The final list of products is therefore the set of products observed in both time periods and across all procedure types. These conditions ensure that identification comes from variation in procedure type and timing within the same products, rather than from changes in the product mix.

[8]Visit `https://datos-abiertos.chilecompra.cl/descargas/ordenes-y-licitaciones`

In addition, we follow a systematic product-level filtering approach inspired by Bandiera et al. (2009). While they manually selected a small set of standardized goods based on comparability, diffusion, and relevance, we apply these criteria at scale to refine the sample while retaining a large product set. This allows the analysis to remain representative of the overall procurement landscape.

- **Relevance**: We keep products above the median of pre-treatment total spending, so the sample focuses on goods with substantial fiscal importance.
- **Diffusion**: We keep products above the median of pre-treatment purchase counts, ensuring the sample includes goods commonly purchased across agencies.
- **Comparability**: We exclude the top 1% of products with the highest pre-pandemic price volatility, retaining products with more stable and comparable prices.

Although these filters remove about 58% of products (604 items), they retain 85% of pre-treatment spending and 90% of pre-treatment observations. This shows that the filtering mainly removes products with highly volatile prices, limited diffusion, or minimal fiscal relevance, while preserving the core structure of Chile's procurement market.

Finally, we exclude products on ChileCompra's COVID product list (e.g., medical supplies and hospital equipment). These items were subject to emergency provisions under Supreme Decree 4 of 2020, which allowed direct government intervention in prices and supply. As a result, price variation in these products is likely driven by market regulation rather than by the procurement flexibilization.

The selected list covers both *high-discretion procedures* (e.g., micro-purchases, agile purchases, and direct deals) and *low-discretion procedures* (e.g., auctions and framework agreements). After applying our selection criteria, we retain approximately 4.8 million observations. Table 2 provides further details, including the distribution of purchases across procurement procedures, agency types, suppliers, contract amounts, unit prices, product categories, and relevant process dates.

[Table 2 about here]

4.2 Stylized facts

4.2.1 Response in the use of discretionary procedures

The most noticeable impact of the pandemic has been the increased use of discretionary procedures. Figure 3 shows the share of each type of procurement procedure. In the chart,

the proportion of contracts awarded through direct deal procedures increased during the post-pandemic period, even though the average contract value for these deals declined. This shift is largely due to an increase in the use of smaller contract sizes in direct deals, which now make up a larger portion of total purchases. This trend is evident in Figure 4, which shows a marked increase in the number of direct deal contracts, particularly for contracts with a size below the newly imposed threshold, skewing the average size downward.

We can argue that restricted auctions and direct deals are both high-discretion procedures, while open auctions and framework agreements are low-discretion procedures. In Figure 5, we apply this classification and observe a clear increase in the proportion of discretionary procedures after the start of the pandemic, both in terms of total value share and the share of contracts.

These changes in the contract size distribution may enhance efficiency, aligning with the theory that discretionary procedures pose less risk of corruption for smaller contracts, whereas costly non-discretionary procedures are preferable for larger contracts (Auriol, 2006; Zhipeng Cui et al., 2024; Søreide, 2002). Therefore, an increase in the use of direct deals exclusively for smaller contracts may be optimal.

[Figure 5 about here]

4.2.2 Response in processing times

Before the pandemic, the trend in processing times between the two types of procedures remained relatively stable. This observation supports the hypothesis of parallel trends prior to the pandemic.

Figures 6 and 7 present the changes in processing times for each quarter from 2019 to 2021. The trend appears largely stable, with only minor fluctuations throughout the year. Notably, contracts directly related to the emergency were processed significantly faster. Furthermore, the data indicates that direct deals have substantially shorter processing times compared to other procedures. Overall, the increased reliance on direct deals enabled the government to accelerate its response to the emergency effectively.

[Figures 6 and 7 about here]

Moreover, Figure 8 reveals that, prior to the reform, processing times for purchase orders were balanced around the threshold. Contracts just below the threshold required essentially the same processing time as contracts just above it. However, after the reform, a clear disparity emerged: contracts just below the threshold were processed significantly faster than those just above it. This highlights the contract size manipulation implications discussed earlier.

[Figure 8 about here]

4.2.3 Response in completeness of reporting

A similar pattern is observed regarding the completeness of reporting. Figure 9 indicates that, after the reform, incomplete reporting —defined as missing information on dates, total value, product price, quantity, or the presence of errors in pricing— became less prevalent overall. However, it is particularly noteworthy that contracts just below the 30 UTM threshold were significantly less likely to exhibit incomplete reporting compared to those just above it.

[Figure 9 about here]

4.2.4 Changes in product composition around the threshold

Figure 10 demonstrates that, after the reform, the composition of products purchased just above and below the 30 UTM threshold differs significantly, whereas the placebo thresholds at 20 and 40 UTM show no apparent differences. This pattern is even more pronounced within a basket of 30 standard goods, as shown on the right-hand side of the figure.

[Figure 10 about here]

Additionally, in Figure 11, we employ kernel-weighted local polynomial smoothers and 0.25 UTM bins to plot changes in the share of each product category around the threshold. Before the reform, the share of contracts buying each product category just below the 30 UTM mark was not statistically different than to the ones just above it, for any category. However, after the reform, a statistically significant discontinuity emerged for nearly every category, further suggesting the presence of manipulation.

[Figure 11 about here]

4.3 Identification strategy

As described in Section 2.2, during the pandemic, the Chilean government implemented two key policy changes that increased discretion in public procurement contracts. First, the "agile purchase" procedure was introduced, applying to contracts valued at less than 30 UTM. Second, the emergency declaration allowed the heads of all purchasing agencies to authorize direct deals for larger contracts. These changes substantially altered procurement dynamics, particularly near the newly established 30 UTM threshold.

To assess the implications of these policy changes, we imkplement two empirical exercises. The first exercise aims to identify rent-seeking behavior through evidence of manipulation in contract sizes. The second exercise evaluates the impact of this manipulation on procurement efficiency, measured by unit prices, transparency, and overpricing.

4.3.1 Detecting rent-seeking

A key implication of increased discretion, as hypothesized in the conceptual framework in Section 3, is that public agencies may strategically adjust contract values to remain below the 30 UTM threshold, thereby avoiding more competitive procurement procedures. If this behavior is widespread, it constitutes direct evidence of rent-seeking, as discussed in Tas (2023).

To test for manipulation, we examine whether the distribution of contract values shows discontinuities at the 30 UTM threshold. If contracts were assigned independently of the policy change, their distribution should be smooth. However, if bureaucrats are strategically adjusting contract values, we should observe an accumulation of contracts just below the threshold.

We apply the density test proposed by McCrary (2008) to detect discontinuities in the distribution of contract sizes. As shown in Figure 12, there is a statistically significant excess mass of contracts just below the 30 UTM threshold. To quantify the extent of this manipulation, we estimate the degree of bunching using the method developed by Kleven and Waseem (2013), presented in Figure 13. The results confirm that this pattern only emerges after the introduction of the new procurement rule. Prior to 2020, the distribution of contract values was smooth around the threshold. The estimated excess mass corresponds to a bunching factor of 7.3, indicating a substantial concentration of contracts just below the regulatory cutoff.

These findings indicate that contract values were adjusted to remain within discretionary limits, reinforcing the hypothesis that increased discretion facilitated rent-seeking. The observed bunching alone is evidence of strategic behavior, suggesting that some contracts were structured to avoid procedural scrutiny and reduce bureaucratic effort —both of which can enhance the utility of the officials involved.

[Figures 12 and 13 about here]

4.3.2 Measuring the impact on procurement efficiency

Having established the presence of rent-seeking through bunching in the previous section, we now examine how this behavior affects procurement outcomes. Specifically, we estimate

the impact of increased discretion on three key outcomes: (i) unit prices and (ii) overpricing, defined as contracts that exceed the mean price plus one standard deviation for the same product, as measurements of *procurement efficiency*; and (iii) the dummy variable indicating whether the procurement process failed to report all required information in the system, as a measure of *lack of transparency* as proposed in Fazekas and Kocsis (2017).

We employ a generalized fixed effects model with time dummies to estimate a difference-in-differences (DiD) estimator, comparing procurement outcomes between discretionary and non-discretionary procedures, both before and after the policy change. [9]

Formally, our estimates are derived from the panel data model that includes agency and product fixed effects, as defined in equation 3.

$$\begin{aligned} Y_{ipjvt} =& \beta_0 + \beta_1 flex_t + \beta_2 DD_i + \beta_3 flex_t \times DD_i \\ &+ \beta_4 H_i + \beta_5 H_i \times flex_t + FE_p + FE_j + FE_v + month_t + \varepsilon_{ipjvt} \end{aligned} \tag{3}$$

In the equation, we analyze three outcome variables, represented by Y_{ipjvt} for the contract i purchasing product p signed by the agency j and the vendor v with a signature date of t. In particular, we use three measurements of contract performance that we mentioned above.

The equation includes two policy variables: DD_i and H_i. The variable DD_i is an indicator, assigned a value of 1 when a large contract, valued above 30 UTM, is procured via a direct deal method and 0 otherwise. Similarly, H_i indicates contracts valued at less than 30 UTM, potentially qualifying them for "agile purchase". Another dummy variable, $flex_t$, is set to 1 during the pandemic period and 0 otherwise. We also include product code fixed effects (FE_p), agency fixed effects (FE_j), supplier fixed effects (FE_v), and time fixed effects ($month_t$). Finally, ε_{ipjvt} represents the error term.

Two key pieces of evidence support this strategy. First, Figures 3 and 5 show stable parallel trends in key contract characteristics before the pandemic, validating a critical assumption for DiD. Second, the decision to authorize direct deals is made at the agency level, allowing us to control for decision-maker fixed effects by including agency dummies.

Identification challenges arise from the fact that the pandemic itself influenced key parameters in the strategic decision-making of bureaucrats, such as the time required to complete a process. For instance, the pandemic forced officers to operate remotely, likely affecting their ability to carry out regular operations. Additionally, it disrupted global supply chains

[9] Due to manipulation near the threshold, RD is not a credible strategy for causal inference. Instead, we prefer a difference-in-differences (DiD) approach, as it better accounts for the simultaneous policy changes affecting direct deals on both sides of the threshold. Szucs (2023) suggests structural estimation to address bunching, but this would not fully resolve the identification issues in our setting due to the two simultaneous policy changes.

and caused shortages of many crucial products, further complicating procurement processes.

Our main analysis refines the product sample and excludes goods directly affected by the pandemic. In the Online Appendix, we also replicate Bandiera et al. (2009) and construct a basket of 30 standardized goods that strictly satisfy comparability, diffusion, and relevance. The results using this reduced basket are very similar to those from the main sample.[10]

Moreover, we conduct an event study to assess the parallel trends assumption. Following Caires et al. (2024), we compare the probability of using direct procedures for contracts in the bunching region (defined as contracts between 27 and 30 UTM) with that for contracts outside this region. As shown in Figure 14, before the reform there is no significant difference in the likelihood of being awarded through a direct deal across the two groups, which is consistent with parallel trends. After the reform, however, the probability of using direct procedures increases sharply within the bunching region. These results are robust whether the comparison group is all contracts or only those eligible for the agile purchases procedure.

[Figure 14 about here]

5 Results

Our results suggest that increased discretion does not necessarily reduce procurement efficiency. Discretionary procedures did not result in higher unit prices for smaller contracts. For larger contracts, we find evidence of price increases. We assess the robustness of these findings in Section 5.1. Transparency improved under discretionary procedures, although the probability of overpayment rose slightly (by 1 percentage point).

Table 3 presents the regression results. We estimate two specifications: the first includes agency, product, and time fixed effects, while the second also incorporates supplier fixed effects. The variable F identifies purchases under the 30 UTM threshold, while H indicates direct deals above this threshold. Our coefficients of interest are the interaction terms between H and F, and these variables with the post-pandemic period T, when the policy changes took effect.

The interaction between F and T, which captures the effect of "agile purchases", is negative but not statistically significant. We estimate a decrease of 0.623 UTM per unit (about 36 USD in 2020) for contracts under 30 UTM. For larger contracts, where direct deals were allowed, unit prices increased by 9.263 UTM (about 137 USD in 2020). This suggests that while flexibility did not increase costs for smaller purchases, it led to higher prices for larger ones.

[10] In the Online Appendix, Table A.2 provides the list of selected products and Table A.3 shows the results.

Notably, transparency improved for small contracts, with the probability of incomplete reporting falling by 1.3 percentage points for purchases below the threshold, while for large direct deals it increased by 3.7 percentage points. By contrast, the probability of paying an excessive price increased for both contract sizes, by 1 percentage point for smaller contracts and by 1.8 percentage points for large ones. This suggests greater price dispersion and is consistent with some degree of rent-seeking in a subset of transactions. Overall, these results indicate that discretionary procurement did not systematically worsen pricing or transparency for small contracts, despite concerns about reduced oversight.

Finally, we approximate the fiscal impact of the policies using the estimated changes in unit prices. For each contract category, we scale the unit price coefficient to the total value purchased in the post-pandemic period. Table 4 summarizes the results. For small contracts—those eligible for the "agile purchase" modality—the reduction of 0.623 UTM per unit translates into estimated savings of about 1.11 million UTM (equivalent to roughly 64.5 million USD, or 2.8% of total post-pandemic procurement expenditure). In 2020 specifically, these savings amount to approximately 0.32 million UTM (about 18.5 million USD, or 1.5% of total procurement spending in that period), while in 2021 they reach roughly 0.79 million UTM (around 46 million USD, or 4.3% of that period's spending). In contrast, for larger direct-deal contracts, the increase of 9.263 UTM per unit corresponds to an additional fiscal cost of about 2.82 million UTM (approximately 163 million USD, or 7.2% of total post-pandemic procurement expenditure). For 2020, this cost amounts to approximately 1.93 million UTM (about 111.9 million USD, or 9.2% procurement spending in that period), while in 2021 it reaches roughly 0.89 million UTM (around 51.5 million USD, or 4.9% of that period's procurement spending) Therefore, the net impact is estimated to be an overall increase in cost of 99 million USD due to the use of discretionary procedures.

It is particularly insightful to observe the evolution of these impacts over time. While in 2020 the additional costs from large direct-deal contracts overwhelmingly exceeded the savings generated by small contracts, the pattern changes notably in 2021. The rise in fiscal savings from small contracts together with the diminished impact of large contracts resulted in a much smaller net effect for that year, with the two components largely offsetting one another.

Overall, these results indicate that, while flexibility for small contracts through the "agile purchases" modality generated significant savings, the regulation flexibilization policy that affected larger contracts generated considerable spending inefficiencies.

One concern is whether the results differ for agencies directly involved in the emergency response. Table 5 presents an alternative specification including an additional term, G, which

equals one for health sector agencies[11]. The results of the heterogeneity analysis show that, for small contracts, the difference in effects between health and non-health agencies was not statistically significant. Among non-health agencies, unit prices decreased by 0.821 UTM per unit (about 48 USD in 2020), while health organizations experienced a similar decrease of 0.124 UTM per unit (about 7 USD in 2020), a non-significant difference of 0.696 UTM. For larger contracts, however, the difference in effects between agency types was statistically significant. While non-health organizations saw an increase of 23.06 UTM per unit (about 1,338 USD in 2020), health organizations saw an increase of 3.39 UTM per unit (about 197 USD). These results suggest that, although both types of agencies experienced increases in unit prices for large contracts, health agencies were less affected overall, possibly because they exercised their additional discretion more responsibly—whether due to altruistic intent to support the emergency response, increased public attention to health sector procurement, or a combination of both—yielding a net effect lower than agencies less directly involved in the pandemic response.

The heterogeneity analysis shows clear differences between health and non-health agencies in reporting quality. For small contracts, non-health agencies reduce underreporting by about 0.7 percentage points, while health agencies show a much larger improvement of 4.1 percentage points. For large contracts, underreporting rises for both groups, but the increase is greater for non-health agencies (3.7 percentage points) than for health agencies (2.4 percentage points). These results are consistent with the unit price estimates and indicate that health agencies made more effective use of the additional discretion.

Overall, procurement flexibilization improved efficiency for smaller contracts, especially within health sector organizations, but led to higher costs for larger purchases.

We additionally re-estimate the effects using the COVID-related product basket that was excluded from the main sample. The results, presented in Table 6, closely mirror the main findings: efficiency gains for small contracts and losses for large ones.

Finally, we examine the subset of post-pandemic purchases explicitly justified on COVID-related grounds, defined as transactions in which the justification field contains a reference to COVID.[12] These contracts purchase products that are not part of the official COVID product list but whose descriptions include pandemic-related keywords such as *covid19*, *sars-cov*, or *emergencia sanitaria*. In other words, they involve products unrelated to the pandemic that agencies nevertheless justified using COVID-related language. The results are reported in Table 7. As a complementary analysis, in Table 8 presents the results for the main sample

[11]Health sector agencies are healthcare providers in ChileCompra data. There are 224 health sector agencies in the refined sample.

[12]All tenders must include a brief justification, which is recorded in the dataset.

(estimations in Table 3) excluding COVID-justified contracts.[13]

For the contracts justified on COVID-related grounds, the coefficient estimates show the signs in the direction opposite to the desired one: unit prices increased for both small and large contracts, and the likelihood of incomplete reporting also rose. This pattern suggests that these purchases were particularly vulnerable to opportunistic behavior, with some agencies potentially using COVID-related justifications to relax oversight. In constrast, the estimates in the sample excluding these contracts show results similar to those reported in Table 3.

5.1 Robustness Checks and Limitations

We conduct two additional exercises to test the robustness of our results. The first is a placebo test for the policy change threshold. In Figure 15, we apply the same McCrary test as before, using arbitrary thresholds of 25 UTM and 45 UTM. The results show no evidence of bunching behavior, reinforcing that the observed manipulation is specific to the policy change threshold of 30 UTM.

[Figure 15 about here]

As a second robustness test, we re-estimate our main specification in an event-study framework with the data aggregated by three-months periods (quarters). Figure 16 plots the corresponding point estimates. The results provide further support for the parallel trends assumption, as all pre-reform coefficients are statistically indistinguishable from zero. After the reform, the two treatment groups evolve in line with our main findings: large contracts show positive and statistically significant effects in most quarters, whereas small contracts exhibit estimates that are generally negative and statistically insignificant throughout the post-reform period.

[Figure 16 about here]

As a third robustness exercise, we re-estimate the main specification at the product family (UNSPSC) level to examine heterogeneity in the effects and assess the consistency of the fiscal impact estimates. Figure 17 presents a forest plot of the estimated coefficients by product family, and Table 9 summarizes these results by reporting the number of statistically significant positive, negative, and non-significant estimates, together with their implied

[13]COVID-related justifications are only present in the post-pandemic period, so all pre-pandemic observations are included in both regressions. The two specifications differ solely in the post-pandemic period: one retains contracts justified on COVID grounds, while the other excludes them.

fiscal impacts. The overall pattern is consistent with our main findings, although there is substantial heterogeneity across product families in both the magnitude and direction of the estimated effects.

[Figure 17 about here]

When focusing on the statistically significant estimates, the results remain aligned with our main findings. For small contracts, positive and negative effects largely offset one another, yielding an overall fiscal saving of around 1 million UTM. For large contracts, by contrast, most significant effects are positive, resulting in approximately 1.5 million UTM in additional fiscal costs. While estimating at the product-family level introduces noise and reduces statistical precision due to smaller sample sizes, the qualitative pattern of the results continues to support our main conclusions.

Taken together, the robustness exercises confirm that our main findings are not driven by sample composition, product selection, or specification choices. The direction and relative magnitude of the effects remain consistent across placebo tests, event-study specifications, and product-family disaggregation. While estimates for small contracts exhibit some variability across product groupings, the differences are minor. By contrast, the evidence of higher prices for large discretionary contracts is stable and statistically robust. Overall, the results reinforce the conclusion that increased discretion improves performance in small purchases, but generates spending inefficiencies and weaker oversight for larger ones.

5.1.1 Limitations

While our difference-in-differences strategy is supported by parallel trends and agency-level fixed effects, some limitations remain. One key limitation is that we do not observe the identity of the bureaucrat responsible for each procurement process. Although agency heads authorize large direct deals, smaller contracts can be approved at lower levels, introducing unobserved discretion that we cannot fully capture. While this may introduce some heterogeneity in decision-making, the agency-level fixed effects included in the estimations help mitigate this concern.

Second, the pandemic altered procurement conditions more broadly—through supply chain disruptions, shifting administrative priorities, and heightened public scrutiny. We address this by excluding COVID-specific products from the main sample, limiting the confounding role of emergency purchasing. Moreover, any supply constraints would have affected both discretionary and non-discretionary procedures similarly, reducing their potential to bias relative effects.

However, the crisis may also have changed bureaucratic incentives in several ways. Health sector agencies, in particular, operated under greater oversight, media attention, and public expectations, which could have encouraged more cautious and accountable use of discretion. Thus, the more favorable outcomes observed for small contracts—and especially purchases done by health purchasing agencies—may reflect either the efficiency advantages of simplified procedures or changes in bureaucratic behavior under crisis conditions. We cannot disentangle these mechanisms, and our results should be interpreted with this caveat in mind.

Finally, although the completeness of the report improved, we cannot verify the accuracy of the report. Having information on the accuracy of the reported data (for example, the number of adjustments made) would have been helpful in exploring this dimension of the reporting quality.

5.2 Mechanisms

5.2.1 Contract Splitting vs. Contract Shifting

Given the evidence of bunching below the 30 UTM threshold, it is valuable to assess how this manipulation occurs. Manipulation can arise from either contract splitting or contract shifting. The former involves dividing large contracts into multiple smaller ones, while the latter entails altering the price of a given project, shifting it below the threshold. Both manipulations can result in the application of discretionary non-competitive procedures such as direct deals.

While manipulation itself does not have a clear impact on post-reform efficiency metrics—since the effects depend on the motives behind the manipulation—each form of manipulation does have distinct implications for the post-reform contract size distribution. Contract shifting is more feasible for contracts that are initially near the threshold, which would lead to a nearly symmetric window in which the excess and missing masses are present around the contract size threshold. In contrast, contract splitting could affect a wider range of sizes, meaning the excess mass below the threshold would not necessarily correspond to the missing mass above it.

If contract shifting were the primary form of manipulation, most manipulated contracts would experience a reduction in contract size, leading to a decrease in the total value contracted for each product, while the number of contracts would remain relatively unchanged. On the other hand, contract splitting would result in a non-negative change in the total value purchased for each product, a significant increase in the number of contracts, and a sharp decrease in the average contract value.

We test these theoretical hypotheses using the framework presented by Caires et al.

(2024). We define a Procurement Need (PN) as the combination of the complete product code and the code of the government agency purchasing the product, and compute six yearly measures for each PN: The total value purchased (TV), the total number of contracts (NC), the average contract size (AC), the number of sellers (NS), the number of contracts in the bunching region (NBR), and the share of contracts in the bunching region (SBR). Using these metrics, we estimate the following model:

$$Y_{it}^{j} = \delta_i + \gamma^j T + \varepsilon_{it} \tag{4}$$

Where Y_{it}^{j} corresponds to metric j (as defined above) for PN i in year t, δ_i represents PN fixed effects, and T is the post-reform indicator. This model employs the within estimator at the PN level, allowing us to interpret the estimates as the average change in the purchasing composition of each PN.

Unlike in Caires et al. (2024), the distributional changes that the estimates may capture are not only due to the introduction of the threshold. They are also shaped by the complexities of the pandemic. Since we're trying to understand the nature of the manipulation behind the bunching—whether it's mostly due to contract shifting or splitting—other changes in the distribution that aren't related to these behaviors could distort our analysis. To deal with this, we focus on PNs whose contracts were mostly (over 50% of them) above the threshold prior to the pandemic and did not include any contracts below the bunching region (UTM $<$ 27). This restriction allows us to focus on the PNs most likely to have benefited from some form of bunching following the reform.

The two hypotheses discussed earlier, concerning the implications of shifting versus splitting, are formalized in Table 10. If contract shifting were the predominant response to the reform, we should observe a negative change in the yearly total value purchased ($\hat{\gamma}^{TV} < 0$), no significant change in the number of contracts ($\hat{\gamma}^{NC} = 0$), and a modest decline in average contract value ($\hat{\gamma}^{AV} < 0, \text{small}$). In contrast, if contract splitting were more prevalent, we expect a non-negative change in total value ($\hat{\gamma}^{TV} \geq 0$), a strong increase in the number of contracts ($\hat{\gamma}^{NC} > 0$), and a much sharper drop in average contract value ($\hat{\gamma}^{AV} << 0$). The results of this analysis are presented in Table 11.

[Table 10 about here]

The results clearly indicate that, on average, the total value contracted for each PN did not decrease; on the contrary, it rose by approximately 74 UTM, although this increase is not statistically significant at the 5% level. In contrast, the number of contracts increased significantly, with an average rise of about 3 contracts per product. At the same time, the average

contract value declined sharply, decreasing by approximately 57 UTM per product. Taken together, these three findings provide evidence that contract splitting was the predominant manipulation mechanism.

In addition, there was a marked increase in the concentration of contracts in the bunching region—both in absolute number and in relative share—further confirming the presence of strategic bunching behavior among the affected PNs in the post-reform period.

[Table 11 about here]

6 Conclusions

This paper evaluates the policy impacts of increasing bureaucratic discretion in public procurement in Chile during the COVID-19 pandemic. In particular, we analyze two key changes: the authorization of direct deals for emergency-related purchases, and the introduction of the Agile Purchase mechanism, which allowed streamlined procurement for contracts below a 30 UTM threshold.

To interpret the impacts of these reforms, we developed a conceptual framework that explicitly incorporates the potential for rent-seeking behavior when bureaucrats are granted more discretion. The model highlights how changes in oversight and enforcement can generate incentives to manipulate contract sizes, particularly around the 30 UTM threshold.

Consistent with the model's predictions, we find strong evidence of rent-seeking behavior. Specifically, we document clear bunching in contract size distribution just below the 30 UTM threshold after the reform, which was absent prior to the policy change. This finding signals that contract size manipulation was used to exploit the relaxed procedural requirements. Moreover, the evidence suggests that manipulation occurred mainly through contract splitting, as agencies fragmented purchases into smaller contracts to remain under the threshold.

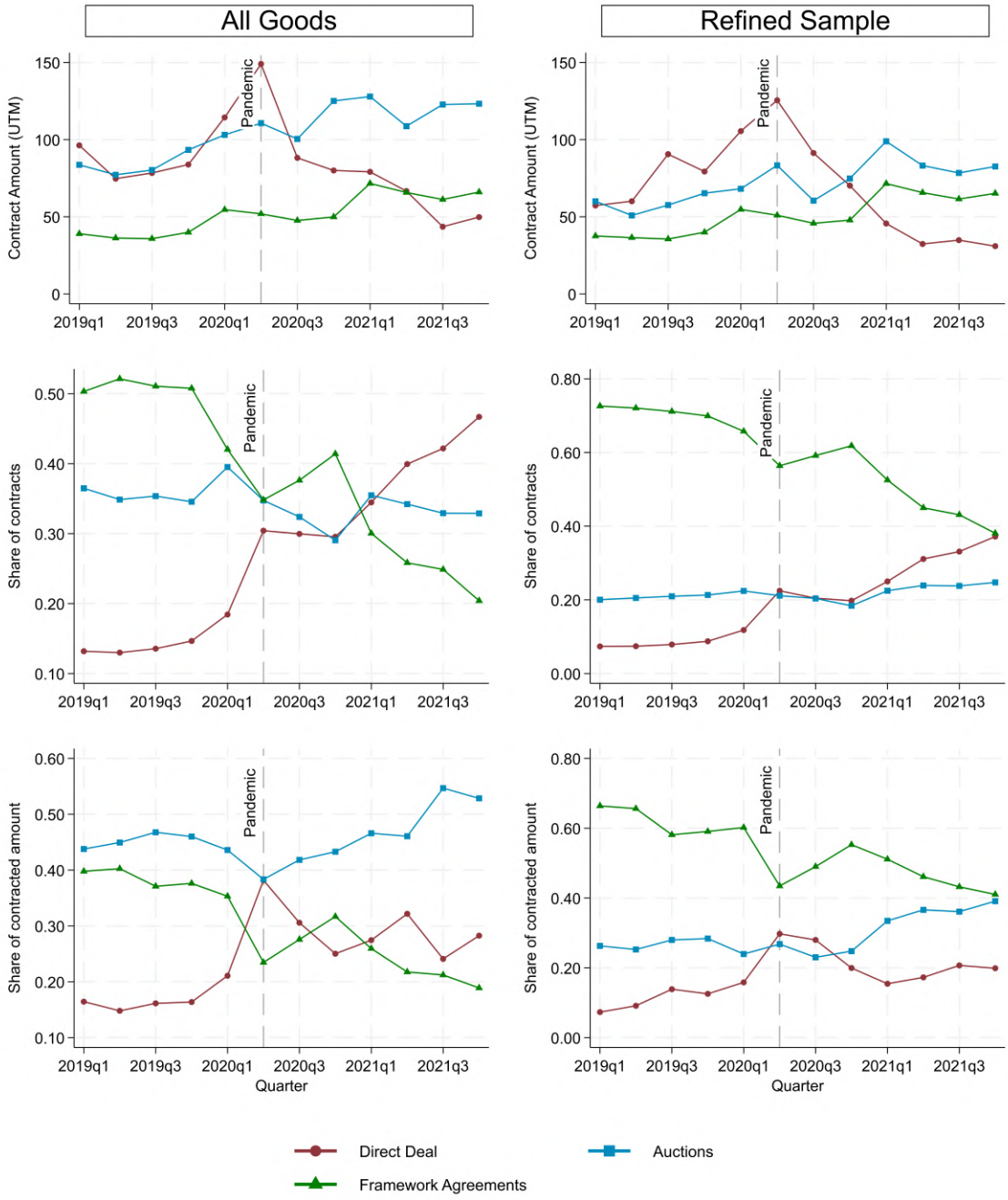
However, while rent-seeking behavior increased, the impact of the policy on the procurement outcomes is not straightforward. On the one hand, greater discretion can increase the potential for opportunistic behavior. On the other hand, the rigid nature of traditional procurement rules may impose significant administrative burdens and limit competition, particularly for small-value purchases.

Our empirical results suggest that discretion led to efficiency losses for larger contracts. We observe increases in unit prices, lower reporting completeness, and a higher probability of overpricing. These results are robust to different model specifications and consistent with prior findings in the literature.

By contrast, the impact of discretion on smaller contracts is not consistently negative. Unit prices decrease on average—though with some heterogeneity—and this change is not statistically significant. This suggests that, for small purchases, the gains in administrative efficiency from expanded discretion may outweigh the risks of rent-seeking. These patterns are not uniform across agencies: health-sector organizations, which faced stronger public scrutiny during the emergency, used discretion more cautiously and showed weaker signs of rent-seeking relative to non-health agencies.

Overall, our findings align with recent empirical literature showing that bureaucratic discretion can yield efficiency gains under certain conditions, particularly in contexts involving smaller transactions or when rigid rules impose high administrative costs (Bandiera et al., 2021; Fazio, 2022). The results emphasize that rent-seeking and efficiency effects coexist, and that the net impact of procurement flexibilization depends critically on contract size and product characteristics.

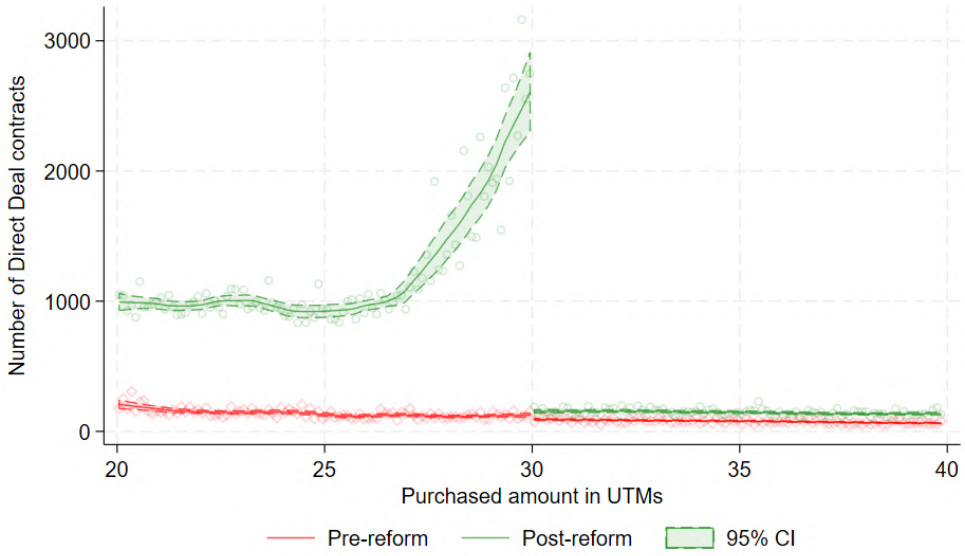
7 Tables and Graphs



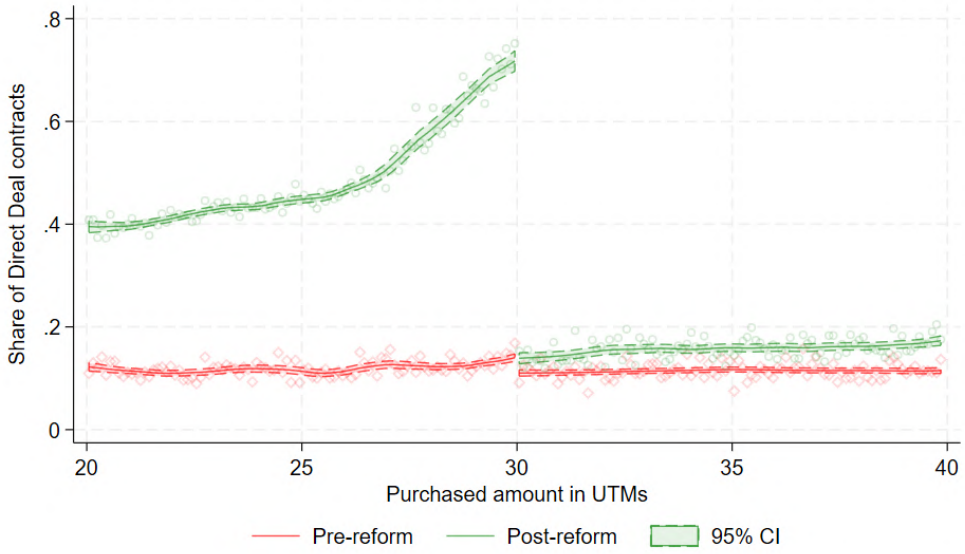
Notes: Each panel shows the evolution of procurement procedures before and after the onset of the pandemic. The left column includes all goods; the right column focuses on our refined sample of goods.

Figure 3: Trends in procurement by procedure type

(a) Number of Contracts (within bin)

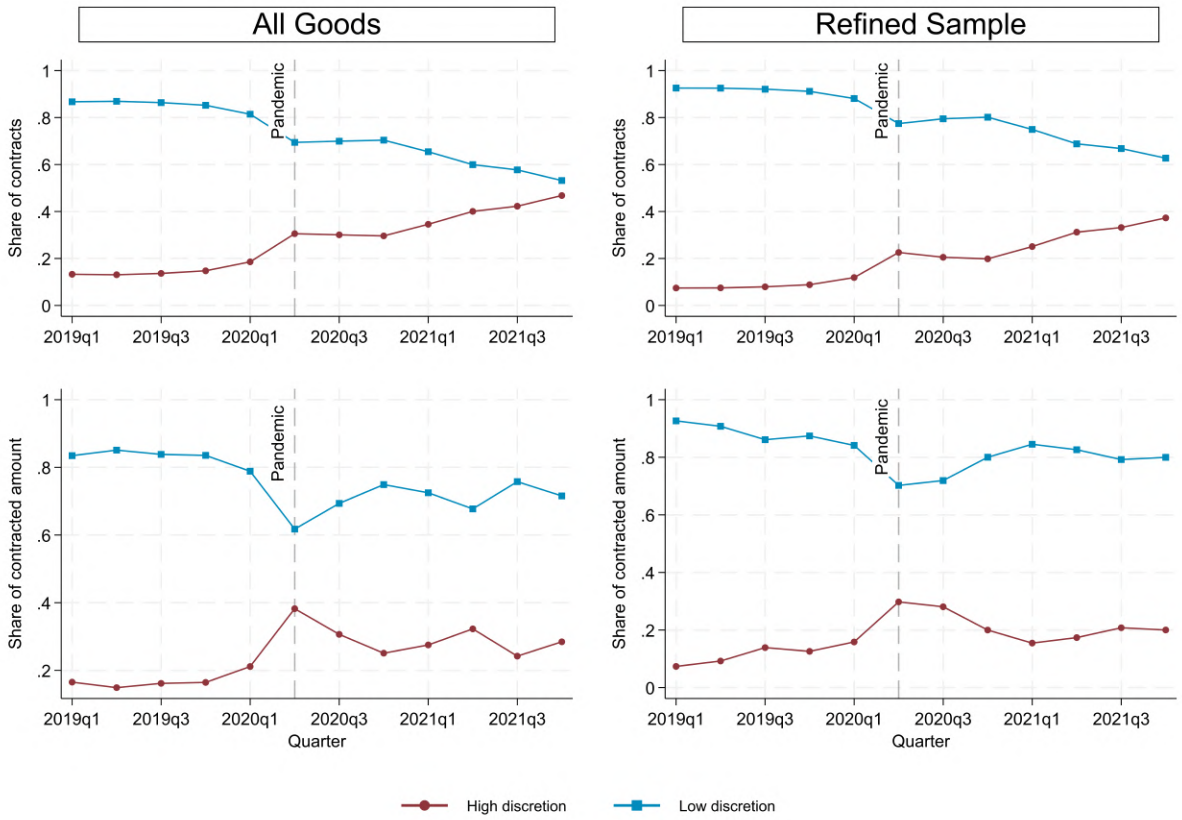


(b) Share of Contracts (within bin)



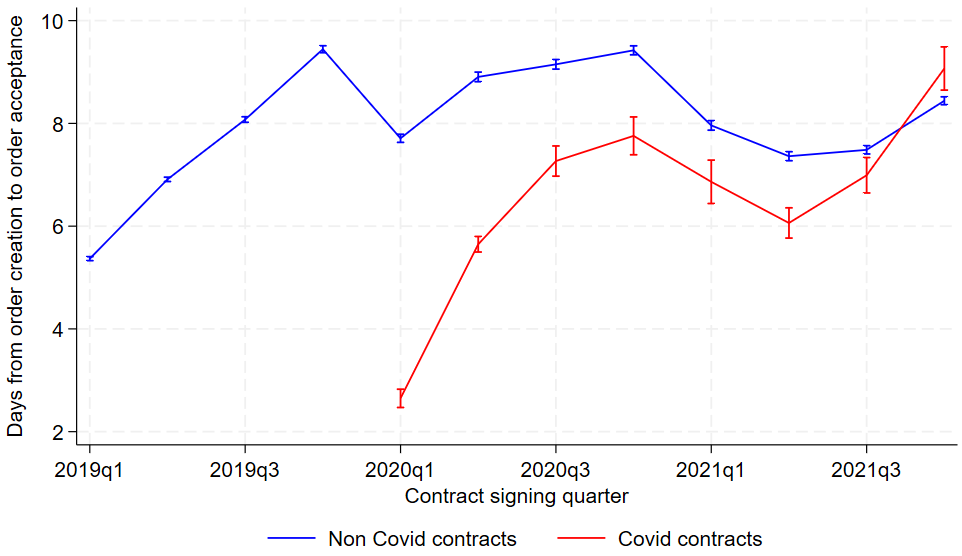
Notes: 'Only goods' orders and auctions were included in the graph. Bins of 0.25 UTM were used for the kernel-weighted local polynomial smoothers. Pre-pandemic period is from January 1, 2019, to February 28, 2020, and the pandemic period goes from March 1, 2020, until December 31, 2021.

Figure 4: Presence of Direct Deal around threshold



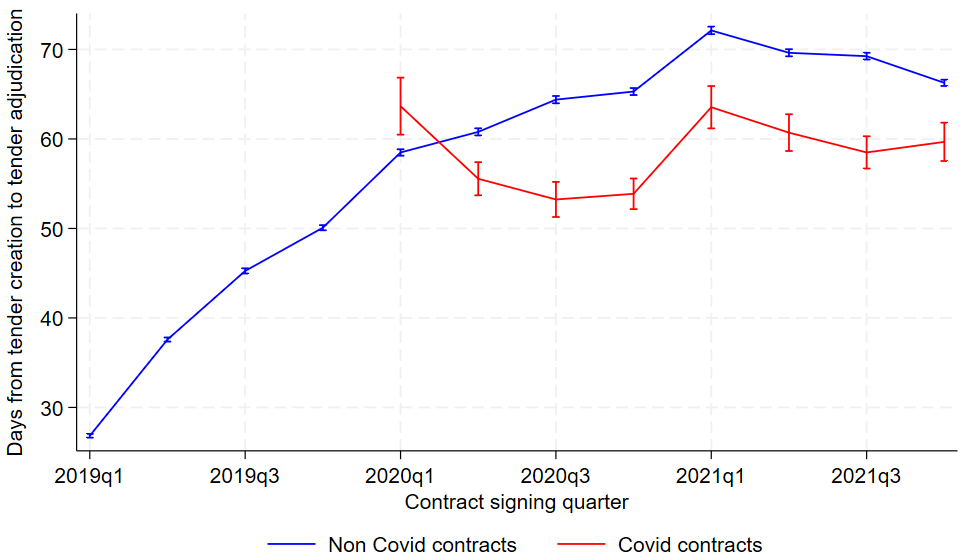
Notes: Each panel shows the evolution of high-discretion and low-discretion procedure categories around the onset of the pandemic. The left column includes all goods; the right column focuses on our refined sample of goods.

Figure 5: Trends in procurement by discretion type



Notes: Days between purchase order creation and contractor's acceptance of the purchase order. Quarterly average for 'Only good' order purchases. A contract is classified as covid if its description contains aislamiento intel, aislamiento obli, aislamiento prev, aislamiento soc, corona vi, coronavir, cov 19, cov2, covbid19, covd, covi 19, covi-1, covi19, covic, covid, covit 19, covud 19, emergencia sani, pandem, sars co, sars-c, sarscov, social y sanitaria decretada, virus-19', oxígeno med*, mascarillas, oximetro, aire medicinal, camas hospitalarias, canulas, entubacion, personal medico, enfermer, personal asistencial, medico, equipos de proteccion personal, dispositivos medicos, bioseguridad, emergencia nacional or virus 19.

Figure 6: Days to complete a purchase order



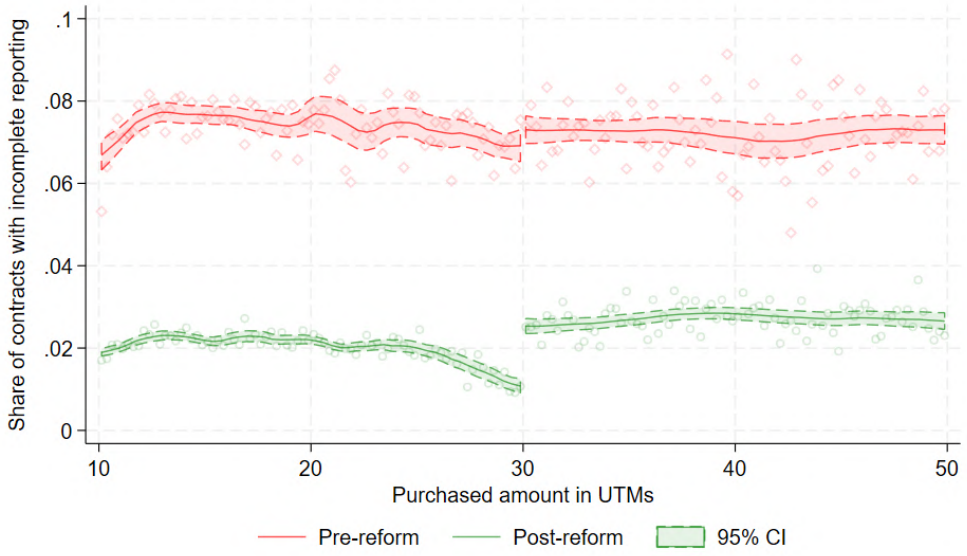
Notes: Days between auction creation and the auction award date. Quarterly average for 'Only good' order purchases. A contract is classified as covid if its description contains aislamiento intel, aislamiento obli, aislamiento prev, aislamiento soc, corona vi, coronavir, cov 19, cov2, covbid19, covd, covi 19, covi-1, covi19, covic, covid, covit 19, covud 19, emergencia sani, pandem, sars co, sars-c, sarscov, social y sanitaria decretada, virus-19', oxígeno med*, mascarillas, oximetro, aire medicinal, camas hospitalarias, canulas, entubacion, personal medico, enfermer, personal asistencial, medico, equipos de proteccion personal, dispositivos medicos, bioseguridad, emergencia nacional or virus 19.

Figure 7: Days to complete an auction



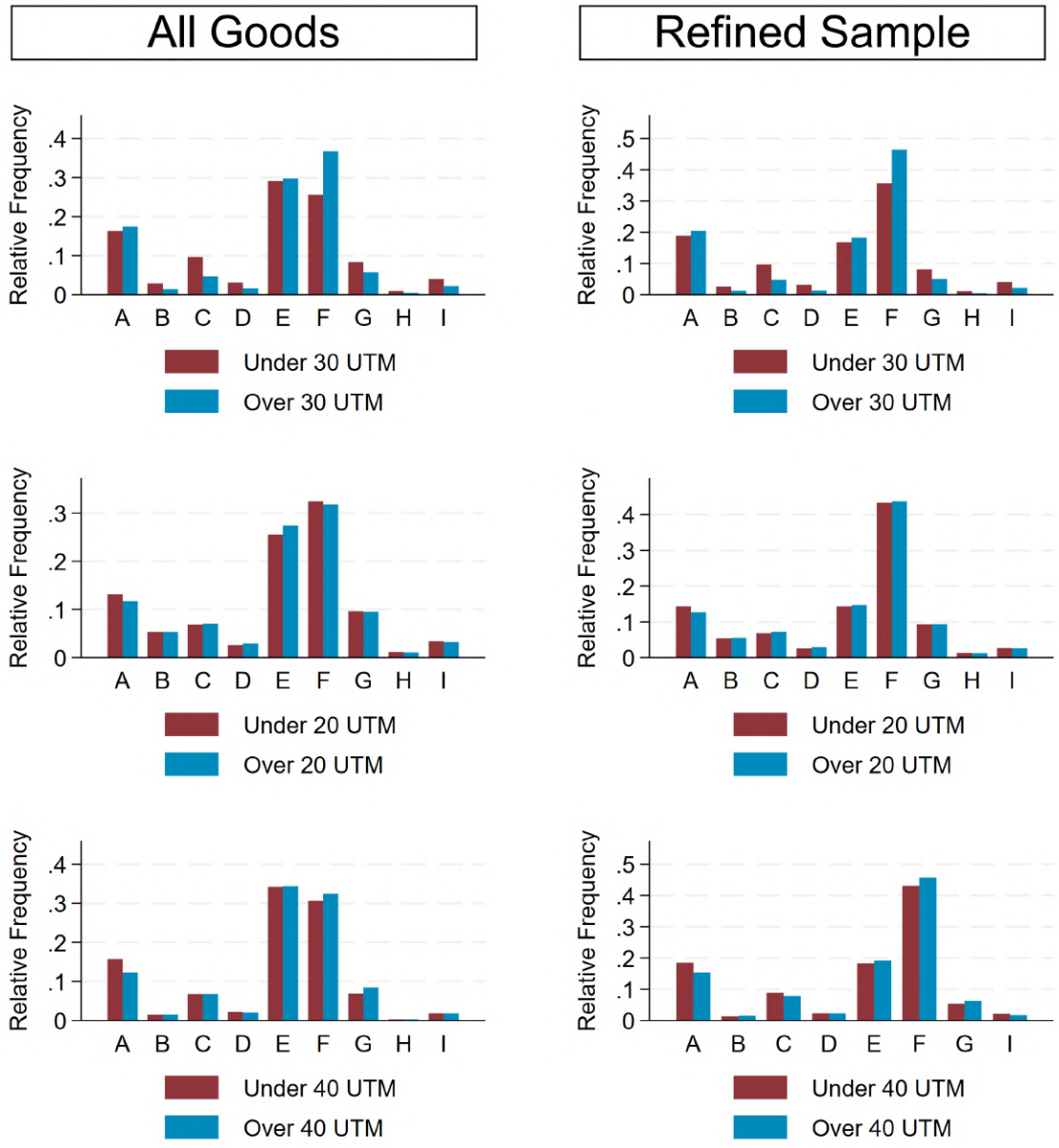
Notes: Days between purchase order creation and contractor's acceptance of the purchase order. We use kernel-weighted local polynomial smoothers and 0.25 UTM bandwidths on either side of the threshold to plot the figure.

Figure 8: Days to complete a purchase order around threshold



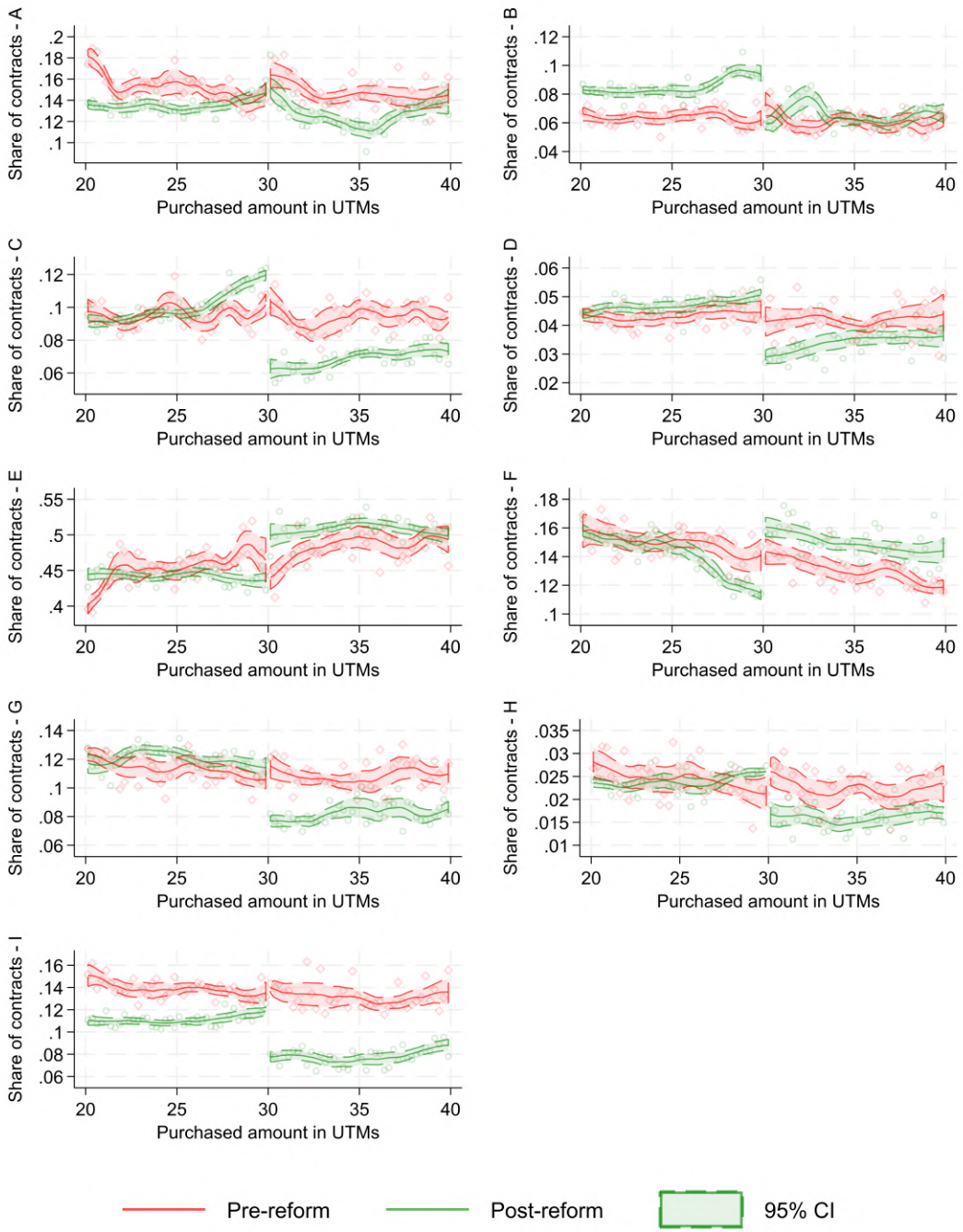
Notes: We use kernel-weighted local polynomial smoothers and 0.25 UTM bandwidths on either side of the threshold to plot the figure.

Figure 9: Incomplete reporting around threshold



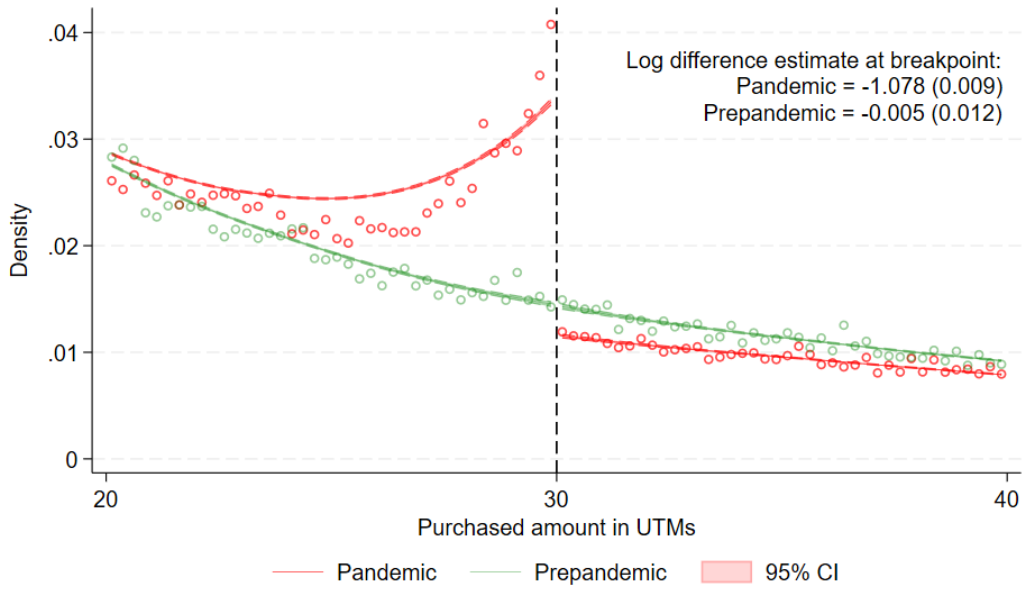
Note: This figure presents the distribution of contracts and purchase orders across nine product categories in the neighborhood based on three thresholds. The true threshold is 30 UTM. The other thresholds - 20 UTM and 40 UTM - serve as placebo tests. We used a 2 UTM window below and above each threshold. The categories are based on the broadest classification of products according to the UNSPSC (**A**: Raw Materials, Chemicals, Paper, Fuel, **B**: Industrial Equipment & Tools, **C**: Components & Supplies, **D**: Construction, Transportation & Facility Equipment & Supplies, **E**: Medical, Laboratory & Test Equipment & Supplies & Pharmaceuticals, **F**: Food, Cleaning & Service Industry Equipment & Supplies, **G**: Business, Communication & Technology Equipment & Supplies, **H**: Defense, Security & Safety Equipment & Supplies, **I**: Personal, Domestic & Consumer Equipment & Supplies).

Figure 10: Product composition around thresholds



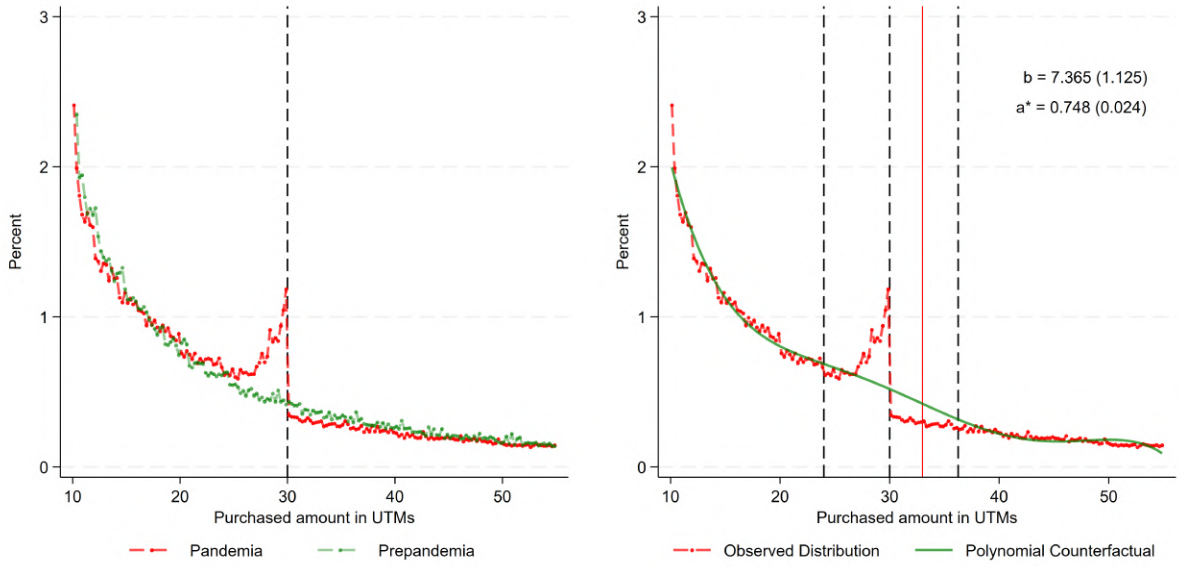
Note: This figure shows the pre- and post-reform distributions of share (within each bin) of contracts and purchase orders buying nine product categories in the neighborhood of the 30 UTM threshold. The categories are based on the broadest classification of products according to the UNSPSC (**A**: Raw Materials, Chemicals, Paper, Fuel, **B**: Industrial Equipment & Tools, **C**: Components & Supplies, **D**: Construction, Transportation & Facility Equipment & Supplies, **E**: Medical, Laboratory & Test Equipment & Supplies & Pharmaceuticals, **F**: Food, Cleaning & Service Industry Equipment & Supplies, **G**: Business, Communication & Technology Equipment & Supplies, **H**: Defense, Security & Safety Equipment & Supplies, **I**: Personal, Domestic & Consumer Equipment & Supplies). We use kernel-weighted local polynomial smoothers and 0.25 UTM bandwidths on either side of the threshold to plot the figure.

Figure 11: Share of product category around the threshold



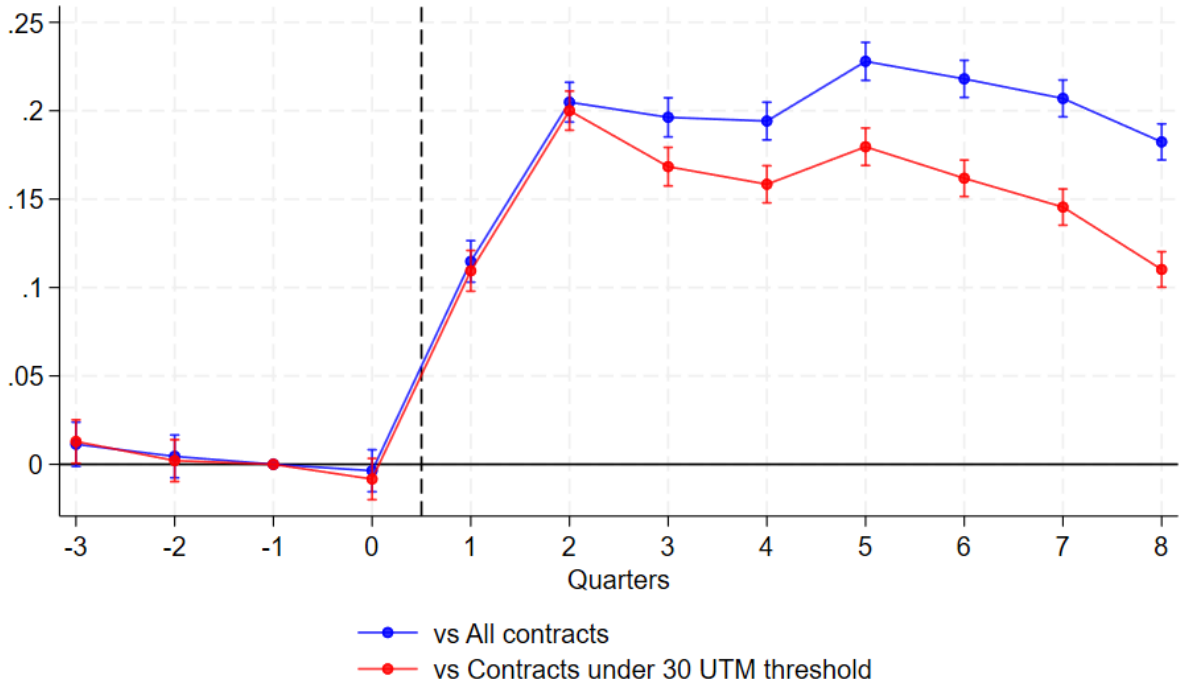
Notes: The figure above shows the McCrary density test around the 30 UTM threshold, with separate estimations for the pre-pandemic and pandemic periods. The log difference in density at the threshold and standard errors are reported. Shaded area indicates 95% confidence interval for the pandemic estimate.

Figure 12: McCrary test around threshold



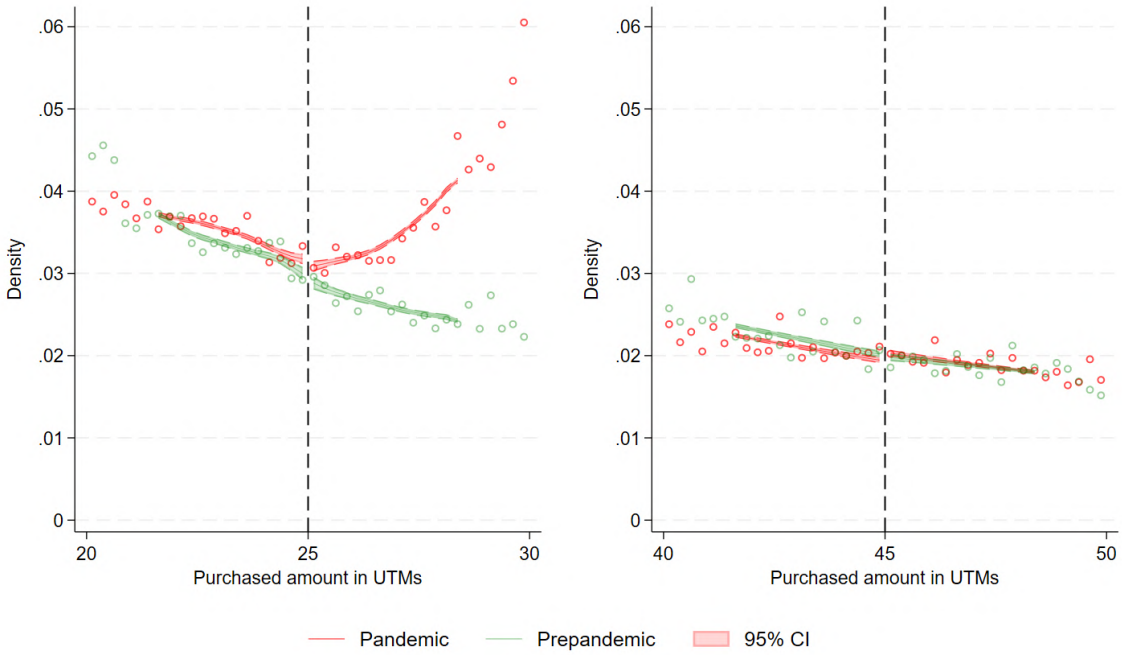
Notes: The left panel shows the distribution of purchases below 100 UTM across pre-pandemic and pandemic periods. The right panel presents the Kleven-Waseem estimator, comparing the observed pandemic distribution to a polynomial counterfactual. 'Only goods' orders and auctions under 100UTM were included in the graph. Pre-pandemic period is from January 1, 2019, to February 28, 2020, and the pandemic period goes from March 1, 2020, until December 31, 2021.

Figure 13: Kleven-Waseem bunching estimator



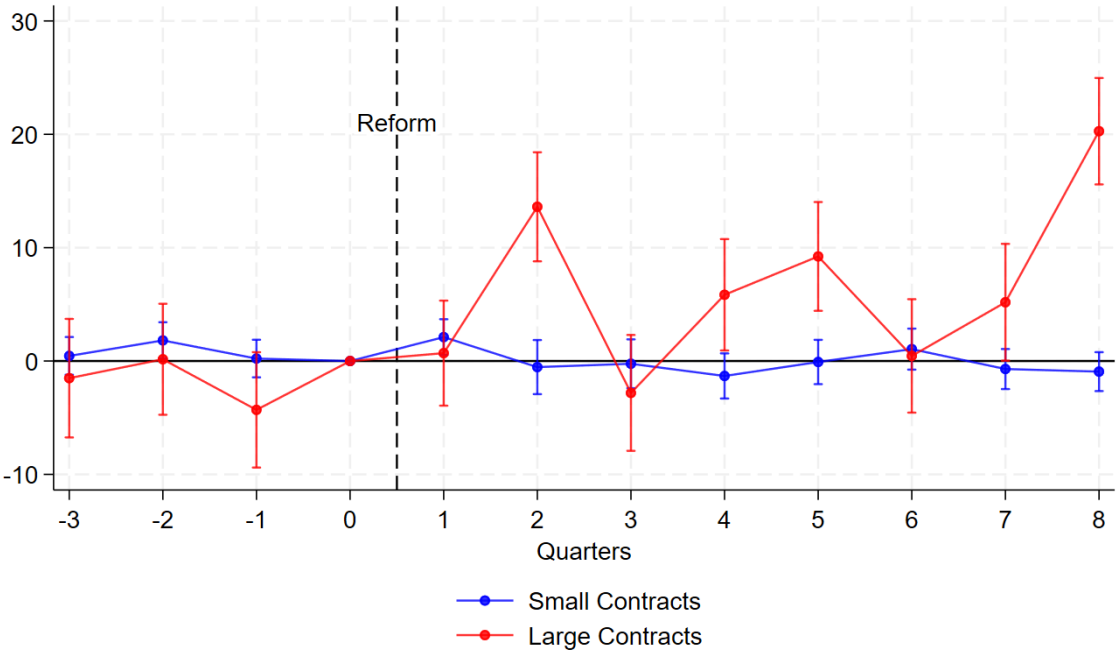
Note: This figure plots the difference in the probability of using direct deals for contracts in the bunching region (contracts with sizes between 27 and 30 UTM), relative to all other contracts (blue) and to other contracts below the 30 UTM threshold (red), across quarters before and after the reform. Vertical line marks the reform quarter. Error bars represent 95% confidence intervals.

Figure 14: Direct Deal presence in the bunching region



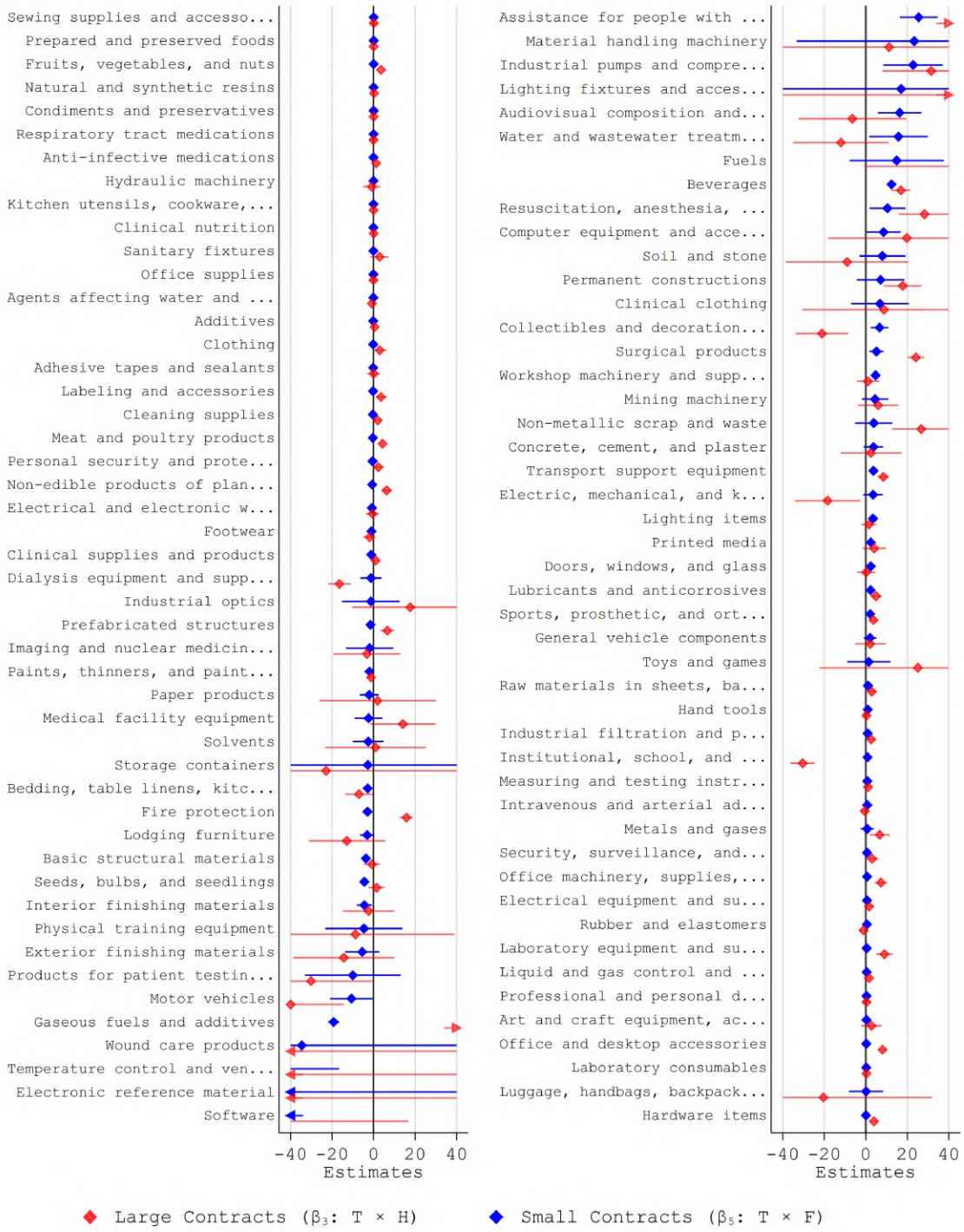
Note: This figure displays placebo McCrary density tests centered at 25 and 45 UTM thresholds, using pre-pandemic and pandemic samples. Shaded areas represent 95% confidence intervals for the estimates.

Figure 15: Placebo McCrary tests for policy change threshold



Note: This figure presents the event study estimates of the effect of the emergency procurement flexibility across quarters before and after the reform. The vertical dashed line marks the implementation of the policy. Each point represents the estimated coefficient for a given quarter relative to the reform, with 95% confidence intervals shown. Blue markers correspond to small contracts ($\leq$ 30 UTM) and red markers to large direct deals ($>$ 30 UTM).

Figure 16: Main regression event study (Quarters)



Note: This figure presents the estimated effects of the emergency procurement measures across product families (UNSPSC four-digit level). Each point represents the estimated coefficient of the interaction between the pandemic period and the contract type—T×F for small contracts (blue) and T×H for large contracts (red)—with 95% confidence intervals shown. The estimates are obtained from separate regressions by product family, following the specification used in the main analysis. In 64 out of the 95 regressions, the impact for large contracts was higher than for small contracts.

Figure 17: Main Regression at the Family Code Level (UNSPSC)

Table 1:
Summary statistics (all dataset)

	Number of purchase orders and tenders	Number of purchases	Avg. contract value (in millions CLP)
Justified by covid*			
No	5,089,505	14,088,107	6.17
Yes	115,506	235,757	7.59
Total	5,205,011	14,323,864	6.20
Type of agency			
National goverment	960,385	2,647,418	7.53
Local goverment	1,814,450	5,556,417	3.82
Public infraestructure	104,815	189,774	26.89
Health	1,834,049	4,312,092	6.16
Military	464,085	1,546,128	3.53
Other	27,222	72,030	85.92
Missing	5	5	0.06
Procurement procedure			
Under Framework Agreement	1,984,903	6,206,498	2.28
Public tender	1,822,727	4,742,382	11.52
Private tender	7,964	14,924	17.12
Direct contract	750,471	1,671,193	8.14
Fast track	497,625	1,345,391	0.59
Microcompra	118,288	302,334	0.27
Order for information purposes	22,989	41,008	7.01
Missing	44	134	1.91
Type of product			
Only goods	3,731,976	12,084,361	3.81
Only services	1,451,202	2,150,518	12.35
Services and goods	17,173	84,321	3.48
Missing	4,660	4,664	14.98
Signing date			
Prepandemic	2,347,404	6,616,007	4.19
Pandemic	2,857,607	7,707,857	7.85

Notes: All dataset was included for the statistics in the table, except for the purchase order 2698-843-SE19. This purchase order amounts for 4 billion CLP and was contracted by the local government of Cartagena in 2019q4. Prepandemic period is from January 1, 2019 to February 28, 2020 and pandemic period goes from March 1, 2020 until December 31, 2021. 'Only goods' category accounts for purchase orders or tenders in which 70% or more of the total contract amount was due to goods purchases. 'Only services' category accounts for purchase orders or tenders in which 70% or more of the total contract amount was due to servicespurchases. Cells in second column indicate the number of purchase orders or tenders found in the databased and cells in the third column indicate the number of different products purchased on the purchase orders or tenders. Fourth colum contains the average contracted amount in millions of CLP. *A contract is classified as covid if its description contains aislamiento intel, aislamiento obli, aislamiento prev, aislamiento soc, corona vi, coronavir, cov 19, cov2, covbid19, covd, covi 19, covi-1, covi19, covic, covid, covit 19, covud 19, emergencia sani, pandem, sars co, sars-c, sarscov, social y sanitaria decretada, virus-19', ox?geno med*, mascarillas, oximetro, aire medicinal, camas hospitalarias, canulas, entubacion, personal medico, enfermer, personal asistencial, medico, equipos de proteccion personal, dispositivos medicos, bioseguridad, emergencia nacional or virus 19.

Table 2:
Summary statistics (refined sample)

	Number of purchase orders and tenders	Number of purchases	Avg. contract value (in millions CLP) Mean	Min	Max
Justified by covid*					
No	1,330,549	4,726,054	2.41	0.02	187,293
Yes	21,967	43,951	4.57	0.02	5,296
Total	1,352,516	4,770,005	2.44	0.02	187,293
Type of agency					
National goverment	250,047	996,575	3.02	0.02	10,259
Local goverment	590,800	2,042,221	1.56	0.02	8,985
Public infraestructure	21,751	56,982	4.82	0.02	983
Health	332,788	1,082,115	2.95	0.02	12,880
Military	150,714	569,405	3.49	0.02	187,293
Other	6,416	22,707	2.54	0.02	759
Procurement procedure					
Under Framework Agreement	662,641	2,712,009	1.83	0.02	9,997
Public tender	370,308	1,192,971	3.63	0.02	187,293
Private tender	1,452	3,091	3.18	0.02	245
Direct contract	133,387	357,528	4.70	0.02	12,772
Fast track	151,756	429,373	0.57	0.02	2
Microcompra	27,994	66,101	0.26	0.02	5
Order for information purposes	4,975	8,923	4.44	0.02	3,987
Missing	3	9	0.26	0.02	1
Signing date					
Prepandemic	637,931	2,287,943	1.74	0.02	7,635
Pandemic	714,585	2,482,062	3.08	0.02	187,293

Notes: Prepandemic period is from January 1, 2019 to February 28, 2020 and pandemic period goes from March 1, 2020 until December 31, 2021 *Category Others include several fundations, technical formation centers, public enterprises, the Santiago subway and local education services. *A contract is classified as covid if its description contains aislamiento intel, aislamiento obli, aislamiento prev, aislamiento soc, corona vi, coronavir, cov 19, cov2, covbid19, covd, covi 19, covi-1, covi19, covic, covid, covit 19, covud 19, emergencia sani, pandem, sars co, sars-c, sarscov, social y sanitaria decretada, virus-19', ox?geno med*, mascarillas, oximetro, aire medicinal, camas hospitalarias, canulas, entubacion, personal medico, enfermer, personal asistencial, medico, equipos de proteccion personal, dispositivos medicos, bioseguridad, emergencia nacional or virus 19.

Table 3: Impact estimation results

	Unit price (in UTM) [1]	Unit price (in UTM) [2]	1(incomplete report) [3]	1(incomplete report) [4]	1(excesive price) [5]	1(excesive price) [6]
T: Pandemic	-3.234***	-1.759***	-0.036***	-0.039***	-0.020***	-0.018***
	(0.333)	(0.296)	(0.000)	(0.000)	(0.000)	(0.000)
F: 1(contract value $\leq$ 30 UTM)	-6.514***	-3.882***	-0.044***	-0.050***	-0.026***	-0.021***
	(0.336)	(0.298)	(0.000)	(0.000)	(0.000)	(0.000)
H: 1(direct deal & contract value > 30 UTM)	5.750***	-0.195	-0.060***	-0.056***	0.029***	0.017***
	(1.027)	(0.928)	(0.001)	(0.001)	(0.001)	(0.001)
T x F	0.513	-0.623	-0.015***	-0.013***	0.016***	0.010***
	(0.470)	(0.440)	(0.001)	(0.001)	(0.000)	(0.000)
T x H	11.358***	9.263***	0.034***	0.037***	0.021***	0.018***
	(1.230)	(1.099)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	8.138***	6.225***	0.107***	0.112***	0.042***	0.040***
	(0.293)	(0.260)	(0.000)	(0.000)	(0.000)	(0.000)
Controls						
Organization fixed effects	Y	Y	Y	Y	Y	Y
Product code fixed effects	Y	Y	Y	Y	Y	Y
Supplier fixed effects	N	Y	N	Y	N	Y
Number of observations	4,756,837	4,746,552	4,770,002	4,759,675	4,770,002	4,759,675
Adjusted R^2	0.76	11.50	14.82	20.81	3.54	11.69

Notes: This table reports the main estimation results of the effect of emergency procurement flexibility on prices and procurement outcomes. The dependent variables are unit price (columns [1]–[2]), an indicator for incomplete reporting (columns [3]–[4]), and an indicator for excessive prices (columns [5]–[6]). The variable F equals one for contracts valued at or below 30 UTM, and H equals one for direct deals above this threshold. T equals one for contracts signed during the pandemic period, when the flexibility measures were in effect. The coefficients on the interaction terms T×F and T×H capture the differential impact of the policies on small and large contracts, respectively. All regressions include organization and product code fixed effects; even-numbered columns additionally include supplier fixed effects. The sample includes all products meeting the panel conditions, excluding those in ChileCompra's COVID product list. ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

Table 4:
Estimated Fiscal Impact

	Contracts Subject to Flexibilization (Small Contracts)	Other Direct Deals (Large Contracts)	Total
A. Estimated effect on contract unit price	-0.623	9.263	–
B. Total purchased value, post-pandemic (UTM)			
2020 (Mar - Dec)	542,961	5,927,713	6,470,674
2021	1,356,082	2,726,164	4,082,246
Total	1,899,043	8,653,877	10,552,920
C. Estimated fiscal impact (UTM)			
2020 (Mar - Dec)	-317,703	1,928,269	1,610,566
2021	-793,485	886,814	93,329
Total	-1,111,188	2,815,083	1,703,895
D. Estimated Fiscal Impact (2020 USD)			
2020 (Mar - Dec)	-18,426,788	111,839,609	93,412,821
2021	-46,022,126	51,435,201	5,413,075
Total	-64,448,911	163,274,814	98,825,903

Notes: This table reports the estimated fiscal impact implied by the point estimates from the main regression. The reported values are obtained by scaling the estimated effects on unit prices by a factor proportional to the total purchased value of each contract category, restricted to the post-pandemic period. This factor is constructed under the assumption that total expenditure is linearly related to unit prices by construction. Figures for 2020 correspond to the period from March to December, reflecting the introduction of the policies in March.

Table 5: Heterogeneous Effects - Health Agencies

	Unit price (in UTM) [1]	Unit price (in UTM) [2]	1(incomplete report) [3]	1(incomplete report) [4]	1(excesive price) [5]	1(excesive price) [6]
T: Pandemic	-3.430***	-1.663***	-0.040***	-0.044***	-0.018***	-0.015***
	(0.387)	(0.343)	(0.000)	(0.000)	(0.000)	(0.000)
T x G: 1(Health sector organization)	0.920	-0.234	0.019***	0.020***	-0.008***	-0.010***
	(0.760)	(0.670)	(0.001)	(0.001)	(0.001)	(0.001)
F: 1(contract value $\leq$ 30 UTM)	-6.935***	-3.917***	-0.050***	-0.056***	-0.025***	-0.019***
	(0.383)	(0.338)	(0.000)	(0.000)	(0.000)	(0.000)
F x G	2.243***	0.273	0.027***	0.027***	-0.006***	-0.008***
	(0.803)	(0.711)	(0.001)	(0.001)	(0.001)	(0.001)
H: 1(direct deal & contract value > 30 UTM)	6.321***	-8.129***	-0.066***	-0.062***	0.051***	0.032***
	(1.719)	(1.611)	(0.002)	(0.002)	(0.001)	(0.001)
H x G	-1.123	10.938***	0.020***	0.021***	-0.039***	-0.028***
	(2.166)	(1.990)	(0.002)	(0.003)	(0.002)	(0.002)
T x F	0.554	-0.821*	-0.009***	-0.007***	0.015***	0.008***
	(0.531)	(0.494)	(0.001)	(0.001)	(0.000)	(0.000)
T x F x G	-0.165	0.696	-0.032***	-0.036***	0.003***	0.006***
	(1.142)	(1.015)	(0.001)	(0.001)	(0.001)	(0.001)
T x H	21.636***	23.069***	0.037***	0.037***	0.014***	0.011***
	(2.116)	(1.956)	(0.002)	(0.002)	(0.002)	(0.002)
T x H x G	-15.636***	-19.680***	-0.013***	-0.012***	0.017***	0.019***
	(2.674)	(2.425)	(0.003)	(0.003)	(0.002)	(0.002)
Constant	8.141***	6.198***	0.108***	0.113***	0.042***	0.039***
	(0.294)	(0.261)	(0.000)	(0.000)	(0.000)	(0.000)
Controls						
Organization fixed effects	Y	Y	Y	Y	Y	Y
Product code fixed effects	Y	Y	Y	Y	Y	Y
Supplier fixed effects	N	Y	N	Y	N	Y
Health sector organization effects						
(T x F) + (T x F x G)	0.389	-0.124	-0.041***	-0.042***	0.018***	0.014***
	(1.024)	(0.913)	(0.001)	(0.001)	(0.001)	(0.001)
(T x H) + (T x H x G)	5.999***	3.389**	0.024***	0.026***	0.031***	0.029***
	(1.636)	(1.436)	(0.002)	(0.002)	(0.001)	(0.001)
Number of observations	4,712,970	4,702,841	4,726,051	4,715,881	4,726,051	4,715,881
Adjusted R^2	0.76	11.68	14.86	20.82	3.58	11.71

Notes: This table reports heterogeneous effects of the emergency procurement measures by agency type. The variable G equals one for health sector agencies. Coefficients for interaction terms (T×F×G and T×H×G) capture differences in the effects between non-health and health organizations for small and large contracts, respectively. Columns [1]–[2] present results for unit prices (in UTM), [3]–[4] for incomplete reporting, and [5]–[6] for excessive prices. All regressions include organization and product code fixed effects; even-numbered columns also include supplier fixed effects. The bottom panel reports implied effects for health agencies combining base and interaction terms. The sample includes all products meeting the panel conditions, excluding those in ChileCompra's COVID product list. ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

Table 6: Model estimates on COVID basket

	Unit price (in UTM) [1]	Unit price (in UTM) [2]	1(incomplete report) [3]	1(incomplete report) [4]	1(excesive price) [5]	1(excesive price) [6]
T: Pandemic	-0.096*	-0.176***	-0.071***	-0.058***	-0.010***	-0.010***
	(0.051)	(0.048)	(0.000)	(0.000)	(0.000)	(0.000)
F: 1(contract value $\leq$ 30 UTM)	-0.693***	-0.573***	0.002***	-0.008***	-0.014***	-0.012***
	(0.053)	(0.049)	(0.000)	(0.000)	(0.000)	(0.000)
H: 1(direct deal & contract value > 30 UTM)	3.201***	1.814***	-0.091***	-0.075***	0.066***	0.028***
	(0.295)	(0.286)	(0.003)	(0.003)	(0.002)	(0.002)
T x F	-0.109	-0.312***	-0.135***	-0.119***	0.015***	0.007***
	(0.079)	(0.083)	(0.001)	(0.001)	(0.001)	(0.001)
T x H	4.270***	3.744***	0.027***	0.014***	-0.021***	-0.010***
	(0.330)	(0.322)	(0.003)	(0.003)	(0.003)	(0.003)
Constant	1.036***	1.048***	0.120***	0.116***	0.045***	0.045***
	(0.046)	(0.042)	(0.000)	(0.000)	(0.000)	(0.000)
Controls						
Organization fixed effects	Y	Y	Y	Y	Y	Y
Product code fixed effects	Y	Y	Y	Y	Y	Y
Supplier fixed effects	N	Y	N	Y	N	Y
Number of observations	2,585,039	2,581,152	2,593,033	2,589,147	2,593,033	2,589,147
Adjusted R^2	1.18	16.49	35.75	45.26	5.29	13.29

Notes: This table reports the estimated effects of the emergency procurement measures for products included in ChileCompra's official COVID basket. The dependent variables are unit price (columns [1]–[2]), an indicator for incomplete reporting (columns [3]–[4]), and an indicator for excessive prices (columns [5]–[6]). The variable F equals one for contracts valued at or below 30 UTM, and H equals one for direct deals above this threshold. T equals one for contracts signed during the pandemic period, when the flexibility measures were in effect. The coefficients on the interaction terms T×F and T×H capture the differential impact of the policy on small and large contracts, respectively. All regressions include organization and product code fixed effects; even-numbered columns additionally include supplier fixed effects. ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

Table 7: Model estimates on COVID justified contracts

	Unit price (in UTM) [1]	Unit price (in UTM) [2]	1(incomplete report) [3]	1(incomplete report) [4]	1(excesive price) [5]	1(excesive price) [6]
T: Pandemic	-3.817***	-3.701***	-0.045***	-0.049***	-0.013***	-0.016***
	(1.125)	(0.963)	(0.002)	(0.002)	(0.001)	(0.001)
F: 1(contract value $\leq$ 30 UTM)	-6.691***	-4.366***	-0.048***	-0.056***	-0.027***	-0.022***
	(0.258)	(0.218)	(0.000)	(0.000)	(0.000)	(0.000)
H: 1(direct deal & contract value > 30 UTM)	6.554***	1.029	-0.048***	-0.043***	0.035***	0.021***
	(0.810)	(0.710)	(0.001)	(0.001)	(0.001)	(0.001)
T x F	3.520**	2.139	0.024***	0.038***	0.014***	0.009***
	(1.700)	(1.539)	(0.003)	(0.003)	(0.002)	(0.002)
T x H	4.298**	3.665*	0.028***	0.044***	-0.005**	-0.000
	(2.170)	(1.963)	(0.004)	(0.004)	(0.002)	(0.002)
Constant	7.817***	6.017***	0.122***	0.129***	0.044***	0.041***
	(0.222)	(0.186)	(0.000)	(0.000)	(0.000)	(0.000)
Controls						
Organization fixed effects	Y	Y	Y	Y	Y	Y
Product code fixed effects	Y	Y	Y	Y	Y	Y
Supplier fixed effects	N	Y	N	Y	N	Y
Number of observations	2,327,355	2,321,815	2,334,691	2,329,146	2,334,691	2,329,146
Adjusted R^2	1.22	13.10	16.28	22.66	4.46	13.37

Notes: This table reports the estimated effects of the emergency procurement measures on contracts whose descriptions contain COVID-related terms—such as "COVID-19", "SARS-CoV", "emergencia sanitaria", or similar expressions—but that do not belong to ChileCompra's official COVID product list. These contracts are therefore classified as COVID-justified. All pre-pandemic observations from our refined sample are retained, while for the post-pandemic period only COVID-justified contracts from that sample are included. The dependent variables are unit price (columns [1]–[2]), an indicator for incomplete reporting (columns [3]–[4]), and an indicator for excessive prices (columns [5]–[6]). The variables F and H identify contracts below and above the 30 UTM threshold, respectively, while T equals one for purchases made during the pandemic period. All regressions include organization and product code fixed effects; even-numbered columns additionally include supplier fixed effects. ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$*

Table 8: Model estimates on contracts non-COVID justified

	Unit price (in UTM) [1]	Unit price (in UTM) [2]	1(incomplete report) [3]	1(incomplete report) [4]	1(excesive price) [5]	1(excesive price) [6]
T: Pandemic	-3.226***	-1.734***	-0.036***	-0.039***	-0.020***	-0.018***
	(0.335)	(0.297)	(0.000)	(0.000)	(0.000)	(0.000)
F: 1(contract value $\leq$ 30 UTM)	-6.518***	-3.876***	-0.044***	-0.051***	-0.026***	-0.021***
	(0.337)	(0.299)	(0.000)	(0.000)	(0.000)	(0.000)
H: 1(direct deal & contract value > 30 UTM)	5.722***	-0.345	-0.060***	-0.056***	0.029***	0.017***
	(1.032)	(0.933)	(0.001)	(0.001)	(0.001)	(0.001)
T x F	0.455	-0.681	-0.016***	-0.014***	0.016***	0.010***
	(0.473)	(0.443)	(0.001)	(0.001)	(0.000)	(0.000)
T x H	12.232***	9.591***	0.036***	0.038***	0.023***	0.020***
	(1.259)	(1.124)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	8.132***	6.204***	0.108***	0.112***	0.042***	0.040***
	(0.294)	(0.260)	(0.000)	(0.000)	(0.000)	(0.000)
Controls						
Organization fixed effects	Y	Y	Y	Y	Y	Y
Product code fixed effects	Y	Y	Y	Y	Y	Y
Supplier fixed effects	N	Y	N	Y	N	Y
Number of observations	4,712,970	4,702,841	4,726,051	4,715,881	4,726,051	4,715,881
Adjusted R^2	0.76	11.68	14.85	20.80	3.55	11.70

Notes: This table reports the estimated effects of the emergency procurement measures on contracts whose descriptions do not contain COVID-related terms—such as "COVID-19", "SARS-CoV", or "emergencia sanitaria". All pre-pandemic observations from our refined sample are retained, while for the post-pandemic period only non COVID-justified contracts from that sample are included. The dependent variables are unit price (columns [1]–[2]), an indicator for incomplete reporting (columns [3]–[4]), and an indicator for excessive prices (columns [5]–[6]). The variables F and H identify contracts below and above the 30 UTM threshold, respectively, while T equals one for purchases made during the pandemic period. All regressions include organization and product code fixed effects; even-numbered columns additionally include supplier fixed effects. ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$*

Table 9:
Coefficient and Fiscal Impact Summary

		Positive Estimates (+)	Non-significant Estimates (0)	Negative Estimates (-)
Small Contracts	# of products	23	58	14
	Fiscal Impact	931,939 UTM	-2,235,584 UTM	-2,713,425 UTM
Large Contracts	# of products	29	58	8
	Fiscal Impact	1,807,648 UTM	-4,502,686 UTM	-286,260 UTM

Notes: This table summarizes the estimated coefficients and their implied fiscal impacts from regressions run separately at the product family (UNSPSC) level. Each regression replicates the main specification, with coefficients classified as positive, non-significant, or negative based on their sign and statistical significance. The corresponding fiscal impacts are computed by scaling each point estimate using the same linear mapping described in Table 4.

Table 10: Hypothesis for Shifting vs. Splitting Test

		Mechanism	
		Contract Shifting	**Contract Splitting**
Δ Total Value Purchased	($\hat{\gamma}^{TV}$)	< 0	≥ 0
Δ Number of Contracts	($\hat{\gamma}^{NC}$)	$= 0$	> 0
Δ Average Contract Value	($\hat{\gamma}^{AV}$)	< 0, *small*	$<< 0$

Notes: $\hat{\gamma}$ are the estimated coefficients from each outcome variable used in equation (4). Table extracted from Caires et al. (2024).

Table 11: Contract Shifting vs. Splitting

	Total Value (in UTM) [1]	Number of Contracts [2]	Average Value (in UTM) [3]	Number of Sellers [4]	No. of Contracts in B. Region [5]	% of Contracts in B. Region [6]
Pandemic	74.418	2.953***	-56.505**	1.481***	1.438***	0.392***
	(107.575)	(0.365)	(27.857)	(0.219)	(0.153)	(0.049)
Constant	497.914***	2.616***	129.923***	1.369***	0.073	0.017
	(71.717)	(0.243)	(18.571)	(0.146)	(0.102)	(0.033)
Controls						
PN fixed effects	Y	Y	Y	Y	Y	Y
Observations	1,470	1,470	1,470	1,470	1,470	1,470
Adjusted R^2	0.09	12.26	0.79	13.35	6.21	3.68

Notes: Standard Errors are clustered at the UNSPSC level. Unit of observation is PN - year. The sample for the calculation of the metrics includes all products meeting the panel conditions, excluding those in ChileCompra's COVID product list.

References

Albano, G. L. and Sparro, M. (2010). Flexible strategies for centralized public procurement. *Review of Economics and Institutions*, 1(2).

Auriol, E. (2006). Corruption in procurement and public purchase. *International Journal of Industrial Organization*, 24(5):867–885.

Bajari, P., McMillan, R., and Tadelis, S. (2009). Auctions versus negotiations in procurement: An empirical analysis. *Journal of Law, Economics, and Organization*, 25(2):372–399.

Bajari, P. and Tadelis, S. (2001). Incentives versus transaction costs: A theory of procurement contracts. *The RAND Journal of Economics*, 32(3):387–407.

Baldi, S., Bottasso, A., Conti, M., and Piccardo, C. (2016). To bid or not to bid: That is the question: Public procurement, project complexity and corruption. *European Journal of Political Economy*, 43:89–106.

Baltrunaite, A., Giorgiantonio, C., Mocetti, S., and Orlando, T. (2021). Discretion and supplier selection in public procurement. *The Journal of Law, Economics, and Organization*, 37:134–166.

Bandiera, O., Best, M. C., Khan, A. Q., and Prat, A. (2021). The Allocation of Authority in Organizations: A Field Experiment with Bureaucrats. *The Quarterly Journal of Economics*, 136(4):2195–2242.

Bandiera, O., Prat, A., and Valletti, T. (2009). Active and Passive Waste in Government Spending: Evidence from a Policy Experiment. *American Economic Review*, 99(4):1278–1308.

Banfield, E. C. (1975). Corruption as a feature of governmental organization. *The Journal of Law & Economics*, 18(3):587–605.

Best, M. C., Hjort, J., and Szakonyi, D. (2017). Individuals and Organizations as Sources of State Effectiveness.

Caires, F. B., Peralta, S., and Mendes, D. (2024). Contract splitting in public procurement. *SSRN Electronic Journal*.

Carril, R., Gonzalez-Lira, A., and Walker, M. S. (2022). Competition under incomplete contracts and the design of procurement policies. Economics Working Papers 1824, Department of Economics and Business, Universitat Pompeu Fabra.

Celhay, P. A., Gertler, P. J., Olivares, M., and Undurraga, R. (2022). Monitoring, organizational culture, and the efficiency of public procurement. Technical report.

Coviello, D., Guglielmo, A., and Spagnolo, G. (2018). The effect of discretion on procurement performance. *Management Science*, 64(2):715–738.

Decarolis, F., Fisman, R., Pinotti, P., and Vannutelli, S. (2023). Rules, discretion, and corruption in procurement: Evidence from italian government contracting. Technical Report 28209, National Bureau of Economic Research, 1050 Massachusetts Avenue, Cambridge, MA 02138.

Fazekas, M. and Kocsis, G. (2017). Uncovering High-Level Corruption: Cross-National Objective Corruption Risk Indicators Using Public Procurement Data. *British Journal of Political Science*, 50(1):155–164.

Fazio, D. (2022). The two sides of discretion: Assessing efficiency and quality in government purchases. *SSRN Electronic Journal*.

Gallego, J. A., Prem, M., and Vargas, J. F. (2021). Inefficient Procurement in Times of Pandemia. *SSRN Electronic Journal*.

Gerardino, M. P., Litschig, S., and Pomeranz, D. (2017). Distortion by audit: Evidence from public procurement. Working Paper 23978, National Bureau of Economic Research.

Heijboer, G. and Telgen, J. (2002). Choosing the open or restricted procedure: A bif deal or a big deal? *Journal of Public Procurement*, 2(2):187–215.

Kelman, S. (1990). Procurement and public management: The fear of discretion and the quality of government performance. *AEI Press*.

Kelman, S. (2005). Unleashing change : a study of organizational renewal in government. page 308.

Klein, R. (1991). Steven kelman, procurement and public management: The fear of discretion and the quality of government performance, washington, dc: The aei press, 1990. 213 pp. isbn 0-8447-3712-7. *Journal of Public Policy*, 11(3):345–346.

Kleven, H. J. (2016). Bunching. *Annual Review of Economics*, 8:435–464.

Kleven, H. J. and Waseem, M. (2013). Using notches to uncover optimization frictions and structural elasticities: Theory and evidence from pakistan *. *The Quarterly Journal of Economics*, 128(2):669–723.

Laffont, J.-J. and Tirole, J. (1993). *A Theory of Incentives in Procurement and Regulation*. MIT Press, Cambridge, MA.

Lisa Chever, S. S. and Yvrande-Billon, A. (2017). The law of small numbers: investigating the benefits of restricted auctions for public procurement. *Applied Economics*, 49(42):4241–4260.

Martinelli, C. (2022). Accountability and grand corruption. *American Economic Journal: Microeconomics*, 14(4):645–679.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, 142(2):698–714. The regression discontinuity design: Theory and applications.

OECD (2017). *Public Procurement in Chile: Policy Options for Efficient and Inclusive Framework Agreements*. OECD Public Governance Reviews. OECD Publishing, Paris.

OECD (2018). *SMEs in Public Procurement: Practices and Strategies for Shared Benefits*. OECD Public Governance Reviews. OECD Publishing, Paris.

OECD (2022). Public Procurement - OECD.

Szucs, F. (2023). Discretion and Favoritism in Public Procurement. *Journal of the European Economic Association*, page jvad017.

Søreide, T. (2002). Corruption in public procurement. causes, consequences and cures. *Report - Chr. Michelsen Institute*.

Tas, B. (2023). Bunching below thresholds to manipulate public procurement. *Empirical Economics*, 64:303–319.

World Bank (2024). Worldwide governance indicators dataset. Accessed: January 21, 2025.

Zhipeng Cui, Dan Wang, Carter B. Casady, Hang Yin, and Anglong Dong (2024). Assessing the institutional- and project-level determinants of corruption in infrastructure projects: Evidence from world bank–funded projects. *ASCE OPEN: Multidisciplinary Journal of Civil Engineering*, 2(1). S2ID: e95cf6266d722e7161d37adbb39ba6338e1fe047.