Indirect Savings from Public Procurement Centralization

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Centralization of public procurement can lower prices for the government's direct purchase of goods and services. This paper focuses on indirect savings. Public administrations that do not procure directly through a central procurement agency might benefit from the availability of centrally-procured goods. We exploit the introduction of a central purchasing agency in Italy and find that prices came down by 22% among administrations that bought autonomously. These indirect effects appear to be driven by informational externalities, especially for less competent public buyers purchasing technologically more complex goods. Accounting for indirect savings increases the estimate of direct ones.

JEL: D44, H11, H57, H83, L38, L88.

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Public procurement, the governments' purchase of goods and services from private contractors, accounts for around 15% of GDP in most economies (WTO, 2018), up to 20% for developing economies, and is on the rise. The efficiency of the procurement process directly influences the availability and quality of government-

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provided goods and services that are often crucial to social welfare and growth. Improving this efficiency is important, and there seem to exist large margins in light of the wide performance heterogeneity across and within countries documented by recent research.[1] One organizational lever that can be used for such a purpose is centralization.[2]

The COVID-19 pandemic brought centralization back to the center of the public debate (e.g., American Medical Association, 2020). Specifically, the devastating experience of the outbreak was accompanied by a crisis in the public procurement of health-related supplies. At various points, different levels of government and public agencies within a country bid against each other, and widespread abuses of increased discretion were allowed by certain emergency regulations (Bandiera, Bosio and Spagnolo, 2021).

In contrast, procurement centralization has the potential to enable speedy and transparent acquisitions without excessive discretion, simultaneously limiting public spending through buyer power and economies of scale.[3]

Centralization also entails important costs, including difficulties in satisfying or adapting to specific local needs, loss of relationships with local suppliers, possible barriers to entry for small and medium-sized enterprises, and a lack of control over non-contractible quality.[4] In light of these potential costs and benefits, it is essential to quantify the effects of centralization empirically.

This paper focuses on *indirect savings* in public spending caused by procurement centralization. In addition to direct savings from public administrations that buy through a central procurement agency documented by Bandiera, Prat and Valletti (2009), this study identifies sizable indirect savings from public administrations that do not purchase through the central agency but are still affected by its entry, something that until now has been overlooked, at least to our knowledge.

[1] See e.g. Best, Hjort and Szakonyi (2017); Bosio et al. (2020); Decarolis et al. (2020); Bandiera et al. (2021).

[2] Another is electronic procurement (e-procurement). See e.g. Lewis-Faupel et al. (2016).

[3] See U.S. House of Representatives (2006); Bandiera, Prat and Valletti (2009).

[4] See e.g. Dimitri, Dini and Piga (2006); Alonso, Dessein and Matouschek (2008).

We build on Bandiera, Prat and Valletti (2009) (henceforth BPV), who are the first to exploit the establishment of the Italian central public procurement agency (Consip) as a quasi-experiment to identify the sources and levels of waste of public funds in Italy. Besides these contributions, BPV also provide the first causal estimate of the direct effects of centralization on public savings. The authors find that public bodies that purchase through Consip save 28%, on average, on the price of goods and services.[5] To reach this result, BPV employ, as a control group, all the administrations that did not not purchase via Consip.

In this paper, we implement a difference-in-differences research design, exploiting the fact that Consip's entry into different markets took place at different points in time. We estimate the indirect savings from centralization in terms of price reductions obtained by public administrations that did not buy from Consip but whose purchases followed its entry in the specific market. Our first main finding is that, when controlling for the characteristics of goods, these indirect effects reduce the price of non-centralized purchases by 22% on average.

We then perform an event study analysis which suggests that there are no pre-trends in prices and no anticipatory effects from Consips entry in the market.

We investigate the origin of the estimated indirect effects, exploring two candidate mechanisms: information externalities and increased buyers' outside options. Information externalities stem from the publicly observable lower prices that Consip obtains when auctioning framework agreements for a large fraction of Italian public demand (about 40% at the time). This allows public administrations purchasing outside Consip to learn and benchmark their reserve prices against those obtained by Consip.[6] A publicly observable benchmarking price from a well-informed central buyer may also discourage or limit corruption, as prices can no longer be easily inflated without raising suspicion about the purchase.

The improved outside option, on the other hand, would be more directly linked

[5]The estimate is 20% in the baseline specification, but it rises to 28% when the characteristics of goods are also controlled for.

[6]Grennan and Swanson (2020) analyze a related informational effect for US hospitals that subscribe to a web-based benchmarking database that provides information on other hospitals' previous purchases.

to the availability of an active Consip agreement from which public bodies can purchase at that moment. In this case, if public bodies fail to reach favorable offers in their decentralized acquisitions, they have the option to switch to the centralized agreement. This option should pressure suppliers to reduce decentralized prices.

To disentangle the role of these two plausible mechanisms, we exploit the fact that information externalities only depend on Consip having previously entered a specific market. Contrary to the improved outside option, they do not require an active central agreement. Our analysis suggests that the indirect effects we estimate are mainly linked to information externalities: the prices of non-centralized purchases do not seem to fluctuate based upon the presence or the expiration of a centralized agreement, which would create or remove the outside option.

We then explore the heterogeneity of these indirect effects across different types of purchases and buyers. We find that the indirect savings result mainly stems from complex goods such as laptops, projectors, and fax machines. This is consistent with the effects being driven by informational externalities that – contrary to the improved outside option – are less relevant for simple goods such as paper, desks, or copper cables.

The second heterogeneity dimension we investigate connects our paper to a recent and growing literature on buyers' characteristics as determinants of public procurement outcomes.[7] In line with related studies, we find that heterogeneity among public authorities in terms of competence has important implications: most of the indirect savings from centralization are obtained by *less* competent public buyers. Our preferred interpretation is that the most competent administrations are already able to figure out plausible prices for complex goods in the absence of a central purchasing body, and therefore learn little from the price they could obtain from Consip. This would be consistent with Bucciol, Camboni and

[7]In addition to BPV, see Best, Hjort and Szakonyi (2017); Castellani, Decarolis and Rovigatti (2018); Bucciol, Camboni and Valbonesi (2020); Chiappinelli (2020); Decarolis et al. (2020, 2021).

Valbonesi (2020), who find that the introduction of reference prices by a supervisor in the Italian medical devices market reduces prices only for less competent public buyers.

Finally, we attempt to shed some further light on the role played by Consip. All the public bodies in our dataset purchase outside of Consip's framework agreements. But some of these public bodies did actually purchase through Consip in the past, while some did not. We find that, having had the chance to purchase initially through Consip, increases the savings that an administration can obtain when purchasing on its own. In other words, having had some "Consip experience" seems to help a more autonomous procurement process.

The price of non-centralized purchases is the benchmark that BPV use to estimate the 28% direct savings from buying from the central agency. Our results imply that this benchmark was deflated by the indirect effects, leading to an underestimation of the direct effects. We show that accounting for the indirect effects lifts the estimate of the direct savings up to 37%. Adding brands to BPV's controls for quality pushes in the opposite direction, leading to a final revised estimate of direct savings of 20%, the same order of magnitude as indirect savings.

In addition to BPV, a number of recent studies have tried to assess the direct effects of public procurement centralization on savings. For example, Clark, Coviello and De Leverano (2021) and Ferraresi, Gucciardi and Rizzo (2021) examine the impact of procurement centralization in the Italian health system.[8] Dubois, Lefouili and Straub (2021) assess the effect of centralized procurement on drug prices in seven low- and middle-income countries.[9] These studies do not consider possible indirect effects but are consistent with our finding that the effect of centralization varies considerably across types of goods and services.

[8]Leveraging the staggered implementation of statutory centralization for different medical devices, Clark, Coviello and De Leverano (2021) document a 15% reduction in prices and a 20% increase in delivery times for centralized purchases relative to non-centralized ones. Similarly, using local health authorities' balance sheet data, Ferraresi, Gucciardi and Rizzo (2021) show that the presence of a regional purchasing body is associated with a 2-8% reduction in expenditures but no change in the provision of health services. Moreover, the authors find that savings come from areas that suffer from poor institutional quality.

[9]They find that centralized public procurement leads to 15% lower prices on average. The reduction is smaller when the supply side is more concentrated.

The rest of the paper is organized as follows. Section I describes the institutional framework and data. Section II discusses the empirical strategy. Section III presents the main results regarding indirect savings. Section IV explores dimensions of heterogeneity. Section V investigates the mechanisms underlying indirect savings. Section VI and VII present the revised estimates for overall savings and conclude, respectively.

I. Institutional Framework and Data

Institutional framework. The Italian central procurement agency, Consip, was established in 2000. It procures goods and services via framework agreements, i.e., general contracts committing the centrally selected supplier to deliver goods or services within a certain time frame at specified prices and conditions to any public body requesting them. The use of Consip framework agreements was mandatory for the central administration from 2000 to 2002, while other public bodies were free to join. The mandatory regime was briefly extended to all public bodies in 2003, then replaced by the optional use of framework agreements for all public bodies in 2004 and 2005. In practice, even when there was a mandatory regime in place, public bodies could justify external purchases by declaring that Consip products did not meet their specific requirements (BPV, 2009). Such discretion implies that there was no need for public bodies to manipulate the timing or size of purchases in order to maintain their autonomy.[10]

In this paper, we concentrate the analysis on purchases that were not made via Consip, and that we call *out-of-Consip* purchases throughout the text.

Data. Our data consists of procurement purchases of generic goods made by a sample of Italian public bodies (PBs) during the period 1999 - 2005. It is the result of a survey designed and implemented by the Italian statistical agency, ISTAT, and was made publicly available by BPV after publication.[11] The survey was

[10]One concern is that public bodies may strategically alter the characteristics of the goods they purchase in order to avoid having to buy from Consip. However, BPV show that there is no evidence of such manipulation.

[11]The data and code used by the authors can be found on the American Economic Association website

distributed to 500 public bodies, of which 447 were selected by cutoff sampling on expenditures, accounting for 80% of the expenditure on goods and services by the Italian public sector as a whole. The survey response rate was over 70%. Respondents and non-respondents did not differ on observable characteristics such as location, annual expenditure, and the type of public body. It contains 6,068 observations on purchases by 208 PBs for 21 goods (e.g., stationery and office furniture, desktop computers, and utilities).

Given the nature of the survey data, retrospective bias would be a concern in case of self-reported information. However, in the present context, we do not believe this would be a major concern for a variety of reasons. First, PBs are requested to keep records of their purchases. Filling the questionnaires would imply accessing their records, not relying purely on self-reports. Second, the whole survey was run by the ISTAT jointly with the Italian Treasury, with considerable resources put into the exercise.[12]

The final dataset contains anonymous PB identifiers, together with PB characteristics such as region and governance type. For each contract, we observe price, quantity, and goods' characteristics such as brand, model, delivery, and maintenance conditions. In addition, we observe the date of the purchase and, for each type of good, the date of Consip's entry into the market, and whether there was an active agreement in place at the time of the purchase by the PB. In Table 1, we present summary statistics of our data, including PB and good type.

For our analysis, we restrict this sample to goods that i) are purchased both pre- and post- Consip's entry in the market, and ii) are strictly purchased out-of-Consip. Our final dataset contains the purchases of 13 generic goods by 208 unique PBs. The resulting sample contains 3,794 observations on purchases of 13 (out of the original 21) types of goods by the same 208 PBs. We emphasize that this dataset is a subset of the dataset of BPV. We drop from the original sample all purchases made through Consip.

at the following link: `https://www.aeaweb.org/articles?id=10.1257/aer.99.4.1278`.

[12]We provide more information on the procedure in Section C in the Appendix.

TABLE 1—DESCRIPTIVE STATISTICS

Panel A. Sample characteristics by public body category									
	N. of total observations	N. of PBs	N. of different goods purchased	Average yearly expenditure	Share of yearly expenditure	Post-Consip purchases	Out-of-Consip purchases	Purchases while deal active	Out-of-Consip while deal active
Ministries and government	454	12	13	425.71	0.16	0.83	0.47	0.64	0.29
Social security	36	3	11	100.03	0.07	0.94	0.69	0.58	0.33
Regional councils	171	11	13	132.95	0.06	0.88	0.79	0.52	0.43
Province and town councils	952	71	13	678.53	0.31	0.91	0.73	0.56	0.37
Health centers	1,151	81	13	543.20	0.25	0.88	0.69	0.57	0.39
Mountain village councils	102	11	12	2.19	0	0.85	0.67	0.62	0.43
University	516	13	13	165.54	0.10	0.91	0.80	0.52	0.41
Other	412	6	13	77.64	0.04	0.88	0.78	0.56	0.46
Panel B. Sample characteristics by good type									
	N. of total purchases	N. of different PBs	Post-Consip purchases	Out-of-Consip purchases	Purchases while deal active	Out-of-Consip while deal active	Average quantity per order	Average price	Coefficient of variation (price)
Laptop	752	190	0.99	0.86	0.40	0.26	5.88	1,209.02	0.36
Office desk	245	111	0.60	0.93	0.58	0.51	11.95	232.12	0.73
Office chair	280	122	0.70	0.96	0.70	0.66	30.40	96.61	0.54
Landline contracts	143	89	0.97	0.13	0.92	0.06	125,272.10	1.90	0.31
Projector	191	103	0.89	0.84	0.36	0.20	1.82	1,437.94	0.41
Local network: switch	215	99	0.65	0.95	0.65	0.60	164.36	138.75	1.86
Local network: cable	101	52	0.67	0.82	0.67	0.50	8,704.67	3.35	1.44
Lunch vouchers	231	131	0.97	0.50	0.97	0.47	665,895.50	70.05	0.05
Paper	755	195	0.96	0.96	0.42	0.38	6,546.51	2.40	0.38
Fax	249	148	0.85	0.71	0.69	0.41	6.89	338.16	0.44
Mobile phone contracts	183	121	0.92	0.54	0.79	0.32	1,244,620.00	0.04	2.07
Office software	155	119	0.95	0.82	0.91	0.73	151.07	233.17	0.39
Printer	294	148	0.96	0.82	0.38	0.20	22.58	483.95	1.15

*Notes:*Panel A shows: the number of total observations; the number of PBs; the number of different goods purchased; the average yearly expenditure in millions of euros; the average share of expenditure in a year over total expenditure in a year; the fraction of post-Consip purchases on total purchases; the fraction of post-*out-of-Consip* purchases; the fraction of purchases while a Consip deal is active; the fraction of out-of-Consip purchases while a deal is active. Panel B shows: the number of total purchases; the number of different PBs purchasing the good; the fraction of post-Consip purchases on total purchases; the fraction of out-of-Consip purchases; the fraction of purchases while a Consip deal is active; the fraction of out-of-Consip purchases while a deal is active; the average quantity per order (quantity equals the number of items in a single purchase, except cables, where quantity is measured in meters, and landline, mobile, and lunch vouchers, where quantity is measured as total yearly outlay); the average price (price equals the cost of one unit; for landline contracts, price equals the per-minute charge for national calls (in eurocents); for mobile contracts, price equals the per-minute charge for calls to landlines); the coefficient of variation of price within good-year across PBs. Each price is normalized according to the monthly consumer price index.

Panel A of Table 1 summarizes the data for the different PB categories. Spending per year varies dramatically across PB type, ranging from an average of €2 million for mountain village councils to €678 million for province and town councils. PBs in all classes buy the 13 different types of goods with the exception of social security administrations and mountain village councils, which buy, respectively, 11 and 12 types of goods. Although most of the purchases by PBs took place after Consip entered the market, many of the purchases were made externally. The share of out-of-Consip purchases ranges between 0.47 for ministries and 0.80 for universities. This share decreases if we consider only the out-of-Consip purchases made while a Consip deal was available (*active*), but it continues to be heterogeneous among PB classes: in line with BPV, we notice that central PBs are more likely to buy from Consip than local PBs and semi-autonomous bodies. This finding could be due to heterogeneous preferences or to the institutional context, and in particular, may result from how central PBs have been subject to the compulsory centralized purchasing regime longer than other PBs (albeit with the possibility of circumventing this obligation).

Panel B of Table 1 summarizes the data for the 13 goods categories, documenting a substantial dispersion in price: the average coefficient of variation within good-year across PBs is 0.78, but this ranges from a minimum of 0.05 for lunch vouchers to a maximum of 2.07 for mobile phone contracts. All the goods in the sample are purchased both from Consip and out-of-Consip, but with considerable variation, potentially reflecting a variation in the relative attractiveness of the Consip deal.[13]

II. Empirical Strategy

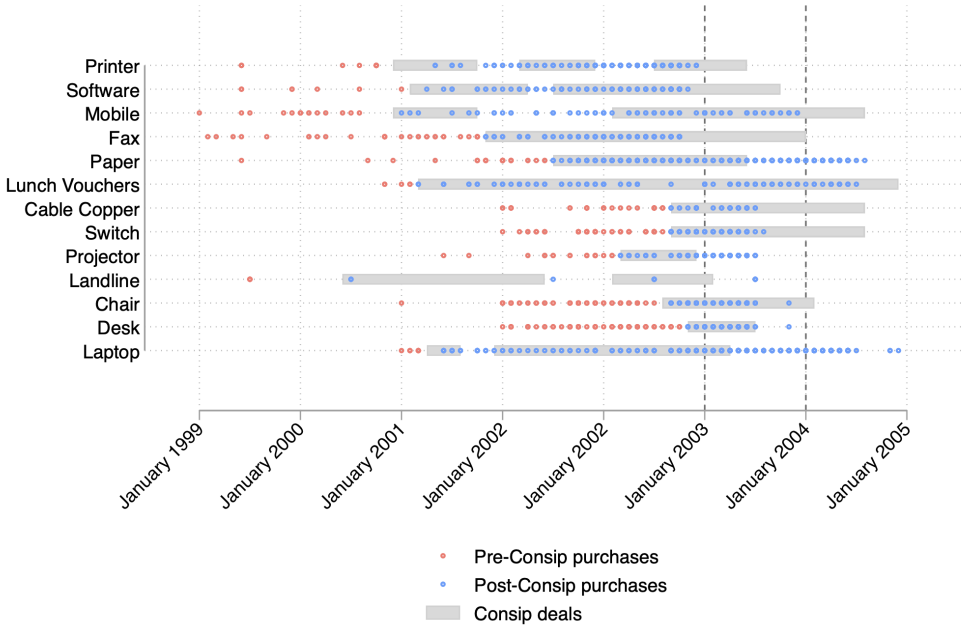
To understand the spillover effects of procurement centralization, we exploit Consip's entry into the relevant market and construct the treatment accordingly.

[13]In Appendix Table A1, we list the number of days a deal has been active for each good. In most cases, Consip has negotiated one deal in the sample period. However, for a few goods, there is also a second deal and, for two goods, a third deal.

We compare prices that PBs pay before and after Consip's entry in the specific market when running their own procurement procedures.

Figure 1 illustrates the construction of our treatment variable and the variation in the data. We plot out-of-Consip purchases throughout the time period in our dataset. We depict the purchases before Consip enters a market (pre-Consip) in red circles; in blue circles we plot purchases in the post-Consip period. The grey areas denote the presence of an active Consip deal in the specific market.

FIGURE 1. GOOD DEALS AND OUT-OF-CONSIP PURCHASES



Notes: The plot depicts unweighted out-of-Consip purchases for each good in the dataset, collapsed to the monthly level. The red circles show pre-Consip purchases, the blue circles post-Consip purchases, and the gray bars the periods when a deal was active from the Consip catalog. The generalized mandatory regime, subjecting in principle all PBs to purchase through Consip, is comprised between the dashed vertical lines.

The data allows us to observe the prices paid by PBs for each good purchased before and after Consip's entry into the specific market. All goods considered for

the analysis were available through an active Consip framework agreement. Since Consip's entry into different markets took place at different points in time, we leverage this variation for identification purposes.

We implement a difference-in-differences research design, by exploiting the time and good variation generated by the Consip experiment. The regression model is shown in Equation 1:

$$\ln p_{igt} = \alpha + \beta Post\ Consip_{gt} + X_{igt}\gamma + \rho_g Q_{igt} + \theta_g + w_i + \eta_t + \epsilon_{igt}, \tag{1}$$

where p_{igt} denotes the price paid by PB i for good g at time t. $Post\ Consip$ is an indicator variable taking value one after Consip enters the specific market g at time t. X_{igt} denotes a vector of good specific characteristics, and Q_{igt} is a vector of quantities allowed to be different for each type of good. We denote goods fixed effects, PBs fixed effects, and month-year fixed effects with θ_g, w_i, and η_t, respectively, while ϵ_{igt} is the error term.

Our parameter of interest, β, captures the indirect savings generated by procurement centralization, namely Consip's entry in the market, for PBs that do not buy via Consip.

Including PB fixed effects allows us to compare the PB with itself before and after Consip's entry in the market. PB fixed effects control for time-invariant unobserved individual characteristics that may be relevant in determining the choice to buy externally. Indeed, the purchasing manager's choice to buy out of Consip, provided that there is an active Consip agreement in the market, may be motivated by specific characteristics or preferences, such as a preference for higher quality, or a preference for corrupt practices. It is also important to stress that the usual simultaneity issue of prices affecting quantities and viceversa, is not a relevant concern here. PBs must state upfront the quantity they are purchasing when running a procurement auction, hence they cannot alter their quantities based on the price they obtain.

Finally, we need to deal with a large number of potential controls. For this purpose, we select the characteristics to be included in X_{igt} using the post-double-selection (PDS) Lasso procedure introduced in Belloni, Chernozhukov and Hansen (2014).[14] This machine learning method implements the Lasso estimator to select the controls in the presence of a large set of potential control variables in a consistent manner that does not lead to wrong estimates of the standard errors. Specifically, the Lasso methodology is used twice. The first step predicts the dependent variable based on all potential controls. This helps select variables that are good predictors of the dependent variable and therefore obtain robust and consistent estimates and increase the power. The second step performs the Lasso to predict the treatment variable based on all potential controls. The final choice of controls to be included in the regression model is the union of the variables selected in these two steps.

III. Indirect Savings

A. Difference-in-Differences

Estimates of indirect savings are shown in Table 2. Each column is a variation of Equation 1. In column (1), we control for quantity purchased, PB fixed effects, good fixed effects, and time fixed effects (interacting non-parametrically month and year-fixed effects). In column (2), we interact good fixed effects with PB fixed effects. This specification is the most restrictive as it drops many observations due to some goods being purchased only once by a PB. In column (3), we control for good specific non-parametric time-trends interacting good fixed effects with time fixed effects. In column (4), we present our preferred specification in which we control for good fixed effects, PB fixed effects, time fixed effects, and a vector of good characteristics selected using the PDS lasso methodology. In the remainder, we refer to the latter as our "baseline" specification.

[14] E.g., for laptops: processor type, RAM size, hard drive size, screen size, included CD reader/DVD reader/CD writer/floppy disk/software, delivery and maintenance conditions. We assign the sample mean (or the mode in case of categorical or indicator variables) to missing goods characteristics.

The coefficient of interest remains negative and statistically significant across all specifications, indicating that the entry of Consip generates indirect savings: public bodies that purchase out-of-Consip pay lower prices for the same goods – with comparable characteristics – than they were paying before Consip's entry in the market. These savings are economically large as they range between 18 and 24%, with the baseline resulting in savings of 22%.[15]

To further argue on the robustness of our main results, in Section D of thr Appendix, we present a battery of robustness checks. First, we drop one buyer at a time. Second, and similar in spirit, we drop one good type at a time. Third, we drop one PB-type at a time. The coefficient estimates resulting from these specifications remain close and statistically indistinguishable from our main estimate. Finally, we use the methodology proposed by De Chaisemartin and d'Haultfoeuille (2020) to discuss the robustness of our estimator.

Table 2—Indirect Savings from Centralization

	(1)	(2)	(3)	(4)
Post Consip	-0.275	-0.205	-0.201	-0.253
	(0.069)	(0.067)	(0.115)	(0.067)
Observations	3091	2299	2984	3091
PB fixed effects	Yes	Yes	Yes	Yes
Good fixed effects	Yes	Yes	Yes	Yes
Year-Month fixed effects	Yes	Yes	Yes	Yes
Good x PB fixed effects	No	Yes	No	No
Good x Year-Month fixed effects	No	No	Yes	No
Controls	No	No	No	Yes

Notes: The dependent variable is the logarithm of the price paid by the public bodies for the specific good. Post Consip is an indicator variable that takes values one if the good is purchased after Consip enters the market and zero otherwise. The set of controls in column (4) corresponds to Lasso selected controls. Standard errors, clustered at the PB level, are shown in parentheses.

[15]Note that our estimation consists of an outcome variable in the log form and our treatment variable is an indicator, so the coefficient estimate must be interpreted as $exp(\beta) - 1$. Our estimated savings are larger that those estimated, e.g., by Grennan and Swanson (2020) for hospitals. This is probably expected as they study the prices of fairly similar hospitals and goods, while the Consip experiment was quite disruptive and run at a much larger (national) scale.

B. Event study analysis

To observe directly and in greater depth the evolution of out-of-Consip prices over time, we use an event-study analysis. The focus is on the first entry in the specific market as Consip made available a second or a third deal only for some goods during the study period. We aggregate the data at the quarterly level and include leads and lags preceeding and following Consip's entry in the market at quarter t_0. The estimating regression becomes the following:

$$\ln p_{igt} = \alpha + \sum_{t=t_0-n}^{t_0+N} \beta_{t-t_0} PostConsip_{gt} + X_{igt}\gamma + \rho_g Q_{igt} + \theta_g + w_i + \epsilon_{igt}. \quad (2)$$

As baseline we use the quarter before entry, and we normalize it to zero. The coefficients β_{t-t_0} capture changes in prices with respect to the baseline category.

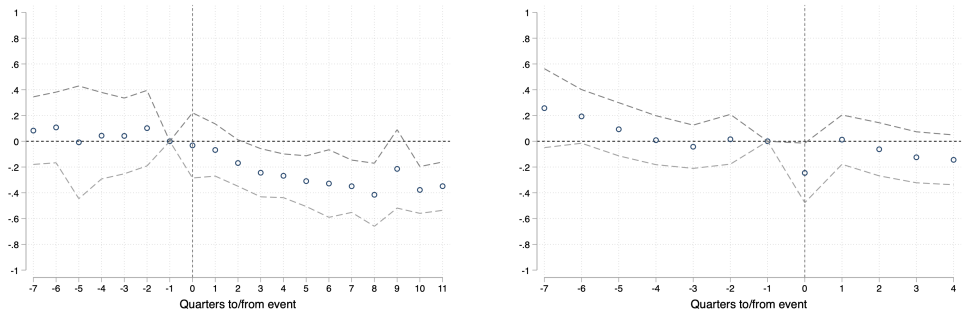
In Figure 2, we present the estimated coefficients and the respective 95% confidence intervals. The coefficient estimates confirm that, after Consip's entry in the market, PBs pay less. The estimated coefficients are negative exactly at the time of entry and continue to decrease over time after the event.

Moreover, the event study design documents two main findings. First, it documents that there are no pre-trends in prices and no anticipatory effects from Consip's entry, as all the coefficient estimates preceding the entry of Consip are close to zero and not statistically significant.[16] These findings lend more credibility to our estimation strategy. Second, it shows that Consip causes prices paid by PBs to be persistently lower for, at least, 11 quarters after the first entry. Note that the duration of a deal is, on average, 5 quarters, hence PBs continue to generate savings even after Consip's deal is over.

In panel (b) of Figure 2, we perform a second event study design centering our analysis around the end of the availability of the first Consip deal. For symmetry, the baseline category is the quarter before the end of the Consip deal, and we

[16] In Appendix Table F1 we report coefficient estimates and standard errors for the event study analysis.

FIGURE 2. EVENT STUDY DESIGN



(a) Deal Start (b) Deal End

Notes: The plots show coefficient estimates and respective confidence intervals for our event study approach. In Panel (a), we center our analysis around Consip's first entry – activation of the first deal– in the market. In Panel (b), we instead center our analysis around the end of the first deal.

normalize it to zero. The end of the deal does not seem to diferentially affect out-of-Consip prices. All leads and lags are close to zero in magnitude and statistically insignificant.

Finally, in order to show that our findings are not driven by compositional effects of buyers strategically altering the timing of their purchases, we plot the timing of purchases. The plots, shown Appendix Figure B1 and B2, show no evidence that PBs adjust their purchases just before Consip deals start or end.

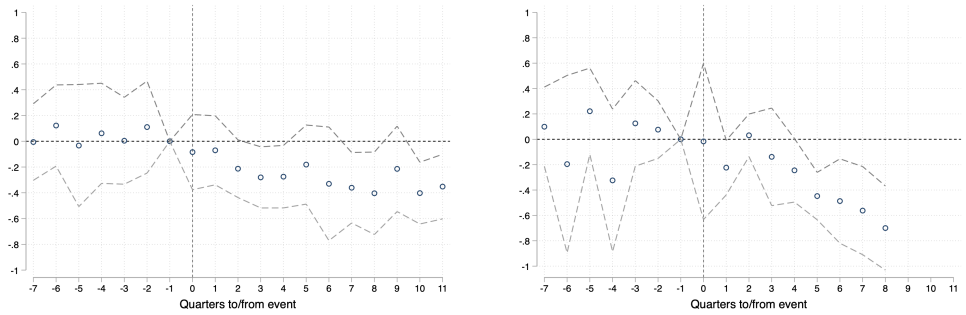
IV. Heterogeneity Analysis

We extend our regression model to allow for heterogeneity in the treatment effect. The first heterogeneity dimension we explore relates to the type of good: complex versus simple. We define as complex goods those that are technologically more composite, such as laptops, projectors, or printers. Examples of simple goods instead include paper, chairs, or desks.

The purpose of the analysis is twofold. First, it helps us understand if the Consip experiment facilitated lower prices for a specific category of goods. It is arguably easier to compare prices and procure simple goods. Second, it allows

us to further investigate if prices exhibit anticipatory effects. The anticipatory effect of Consip's entry could lead public administrations to strategically alter prices in the pre-Consip period.[17] These results are shown in Figure 3 and in the corresponding Table F1 reported in the Appendix.

FIGURE 3. EVENT STUDY BY TYPE OF GOOD



(a) Simple Goods (b) Complex Goods

Notes: The plots show the event-study coefficient estimates by good type. In Panel (a), we restrict the sample to simple goods, and in Panel (b), we focus on complex goods.

This analysis reveals interesting findings. First, it suggests that complex goods generate higher savings than simple goods. Second, the lack of pre-trends for each good category suggests that our findings cannot be attributed to anticipatory price effects. Similar to our event study design, for each category, the coefficient estimates in the quarters preceding the entry of Consip in the market are statistically insignificant and close to zero in magnitude.

The second dimension of heterogeneity we attempt to explore is the competence level of the public body. On the one hand, the most competent PBs might be better able to exploit the potential indirect gains of centralization and thus generate savings. On the other hand, the presence of a central agency's offer might benefit more the least efficient PBs, which were previously unable to buy

[17]We would expect prices to go down in anticipation of Consip entering the market. However, one might argue that this is not necessarily the case, as in an attempt to generate possible bribes, PBs may collude with suppliers and pay higher prices prior to Consip's entry.

at competitive prices.

A way to capture the effects of the competence of PBs is to look at each PB's place in the distribution of fixed effects for different products, before Consip's entry into the market. We proceed in a similar way to BPV and first estimate the average price paid by each PB for all goods purchased, and obtain the PB-fixed effect in a regression model that controls for the characteristics and quantity of the goods, restricting the sample to the pre-Consip period. The equation is:

$$\ln p_{igt} = \alpha + X_{igt}\gamma + \rho_g Q_{igt} + \theta_g + w_i + \eta_{gt} + \epsilon_{igt}, \tag{3}$$

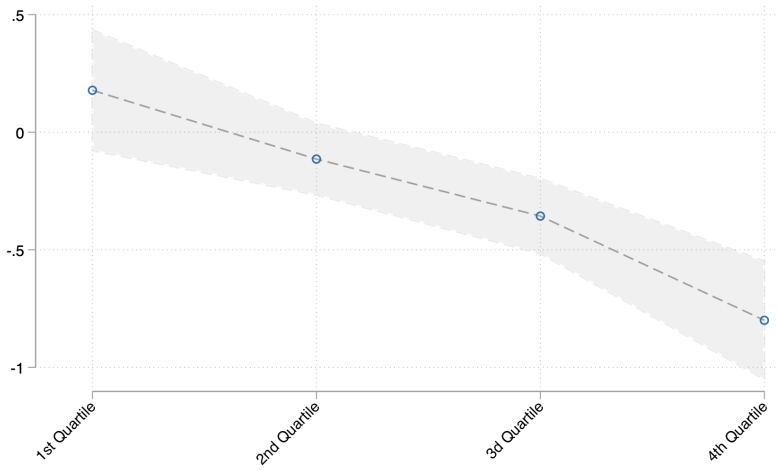
where our coefficients of interest are the estimated PB-fixed effects $\hat{w}_i$. We mark each PB relative to the quartiles of the PB fixed effects distribution, where higher quartiles indicate less efficient public bodies.

We identify the heterogeneous indirect effects of centralization depending on the level of competence of each PB by estimating an extension of the model of Equation 1, in which we interact the $Post\,Consip$ indicator with indicator variables that take value one for each pre-Consip PB-fixed effect quartile.

Results are reported in Figure 4 and show that indirect effects are statistically different from zero and increasing in magnitude only in the two upper quartiles of the distribution. This evidence supports the hypothesis that indirect savings come from the least efficient public bodies, while the more efficient ones already knew how to procure their goods.[18]

[18]In Appendix Table E4, we present the table counterpart to Figure 4, with different variations of the estimating equation. Moreover, in Appendix Table E3, we present an alternative heterogeneity analysis with respect to the distribution of quantities purchased pre-Consip. We find that the indirect effects are statistically different from zero in the lower quartiles of this distribution, reinforcing our interpretation that savings emerge where PBs purchase less and thus have less individual purchasing experience.

FIGURE 4. SPILLOVER EFFECTS: PREVIOUS EXPERIENCE



Notes: The figure plots coefficient estimates, and 95% confidence interval, for each quartile of the PB fixed effect distribution. Higher quartiles correspond to less efficient PBs. Each quartile is interacted with the post-Consip treatment and controls are added as those in our baseline specification. Standard errors are clustered at the PB level.

V. Mechanisms Behind Indirect Savings

A. Information acquisition or bargaining power?

Our results show that public procurement centralization leads to lower prices paid by public bodies when purchasing on their own. In this section, we assess and investigate two natural mechanisms that might be behind these indirect effects, namely information externalities and the improved outside option. The first relates to the fact that PBs acquire potentially useful information from observing similar goods provided by Consip. The second instead captures the fact that a Consip deal represents a credible alternative, that is, a threat point, when the PB is purchasing on its own and contracting over various terms with a supplier.

While these two mechanisms are somewhat linked, we argue that we can still

hope to distinguish between them in our data, exploiting some differences in the procurement regime. Information externalities depend exclusively on Consip having entered the market, but the outside option effect requires the presence of an active deal through Consip. Hence, in periods when there is no active deal present, we expect only information spillovers to be at work. In periods when an active deal through Consip is present on the market, public bodies may also find themselves in a situation with higher bargaining power.

For this, we explore the heterogeneous effect of the indicator *Post Consip* between periods when a deal through Consip is active and not active. Periods with active deals are those depicted in grey bars in Figure 1. Specifically, we interact our treatment with two indicator variables that take value one when a deal is active or not active, respectively. In other words, we focus on the post-Consip period, and split it into two sub-periods, with and without presence of a deal. The coefficients estimates are to be interpreted with respect to the pre-Consip baseline.

Results are reported in Table 3. In column (1), we estimate the same specification as in column (1) of Table 2. In column (2), we add our vector of Lasso-selected controls, as in the baseline. We find little support for the bargaining power mechanism: in fact, savings increase during periods when Consip deals are not available at the same time. A formal test rejects the null of the equality of the coefficients at any conventional level. This is shown by the F-statistic on $\beta_1 = \beta_2$, and the respective p-value, where β_1 and β_2 are the coefficient estimates on Post Consip x Active Deal and Post Consip x No Active Deal, respectively.

The evidence that prices do not move after the end of the deal, as shown in the event study, reported in Panel (b) of Figure 2, corroborates the finding that bargaining power is not the main channel behind our findings. Indeed, if bargaining power were to increase during periods in which Consip deals are active, the end of a deal would remove that option for the PBs, and we would observe prices to go up afterwards, ceteris paribus. This is not what we find in

the data.

Taken together, our findings so far suggest that the most plausible mechanism behind indirect savings is related to informational externalities. PBs acquire information through Consip, and seem to be able to run better procurement auctions by themselves. In particular, PBs learn how to procure better technologically complex goods.

TABLE 3—DIFFERENT REGIMES

	(1)	(2)
Post Consip x Active Deal (β_1)	-0.315	-0.266
	(0.070)	(0.066)
Post Consip x No Active Deal (β_2)	-0.702	-0.557
	(0.108)	(0.106)
F-stat $\beta_1 = \beta_2$	41.044	21.030
p-value $\beta_1 = \beta_2$	0.000	0.000
Observations	3091	3091
PB fixed effects	Yes	Yes
Good fixed effects	Yes	Yes
Year-Month fixed effects	Yes	Yes
Controls	No	Yes

Notes: The dependent variable is the logarithm of the price paid by the public bodies for the specific good. Post Consip is an indicator variable that takes value one if the good is purchased after Consip enters the market and zero otherwise. Active Deal takes value one if a deal is being negotiated from Consip. No Active Deal takes value one if there is no Consip deal active. β_1 and β_2 are coefficient estimates on the Post Consip x Active Deal and Post Consip x No Active Deal, respectively. Standard errors are clustered at the PB level and are shown in parentheses.

B. Information spillovers from the "Consip experience"

To dive deeper on the mechanisms, we attempt next to understand how the informational spillovers arise. A natural hypothesis is that it is the mere entry of Consip in the market that matters, with its lower prices. Still, many PBs had previously procured via Consip, prior to their out-of-Consip purchases, and this could have led to some additional learning in the subtle art of procurement. We denote this other potential channel as the *Consip experience*. This might capture several features beyond prices and observable characteristics, such as supplier

contacts, how to structure a procurement contract, how to deal with post-sales assistance, and so forth.

We estimate the effect of Consip's entry in a specific market, distinguishing between the PBs that had previously purchased through Consip, that is, they had a Consip experience, and those who had not.[19] To estimate experience spillovers, we also use heterogeneities due to different purchasing regimes. We create two overlapping indicator variables that take value one if the Consip experience occurred during a mandatory regime or during an optional regime, respectively.

We estimate a version of Equation 1, augmented by the PB-specific Consip experience during these two purchasing regimes, mandatory and optional. Results are shown in Table 4. The only difference between columns (1) and (2) is the addition of Lasso-selected set of controls in the latter. The breakdown presents interesting findings. Looking at the interaction terms, when compared to PBs without previous Consip experience, those that have previously purchased through Consip manage to generate additional savings. These additional savings arise especially from PBs who have previously experienced Consip goods during an optional regime.

To visualize the additional savings from experiencing each regime, we plot the coefficient estimates in Figure 5. The first coefficient plotted on the left is the baseline result and corresponds to column (4) of Table 2. It includes all PBs that purchase out-of-Consip, both those who had a previous Consip experience and those who had not. The second and the third plotted coefficients show the spillovers accounting for the additional Consip experience, by regime, and are derived from column (2) in Table 4.

The results in Figure 5 suggest that Consip's entry in the market generates not only informational spillovers, but also experience spillovers. The additional gains are large in magnitude and statistically and economically significant for PBs who had a previous experience during an optional regime.

[19] These spillovers are in the spirit of Huber and Steinmayr (2021).

We note that results suggest compositional effects.[20] In particular, PBs with previous Consip experience from a market typically pay more for their out-of-Consip purchases in other markets where Consip did not yet enter. But once a Consip deal becomes available in these other markets, then the same PBs pay much lower prices for their out-of-Consip purchases also in these markets. However, with the data at hand we cannot disentangle this particular finding, as we would need more detail on PBs and their suppliers.

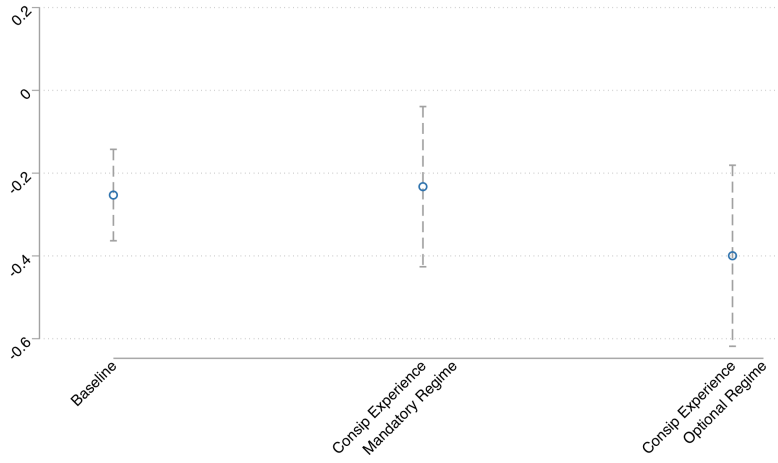
Table 4—Experience Spillovers

	(1)	(2)
Post Consip (β_1)	-0.173	-0.156
	(0.084)	(0.087)
Consip Experience from Mandatory Regime	-0.017	-0.035
	(0.128)	(0.109)
Consip Experience from Optional Regime	0.336	0.311
	(0.145)	(0.120)
Post Consip x Consip Experience from Mandatory Regime (β_M)	-0.036	-0.076
	(0.133)	(0.119)
Post Consip x Consip Experience from Optional Regime (β_O)	-0.311	-0.243
	(0.154)	(0.126)
Estimate $\beta_1 + \beta_M$	-0.209	-0.233
SE $\beta_1 + \beta_M$	0.127	0.117
Estimate $\beta_1 + \beta_O$	-0.484	-0.400
SE $\beta_1 + \beta_O$	0.158	0.132
Observations	3091	3091
PB fixed effects	Yes	Yes
Good fixed effects	Yes	Yes
Year-Month fixed effects	Yes	Yes
Controls	No	Yes

Notes: The dependent variable is the logarithm of the price paid by the public bodies for the specific good. Post Consip is an indicator variable that takes value one if the good is purchased after Consip enters the market and zero otherwise. Controls are selected using the PDS Lasso methodology. Consip Experience from Mandatory or Optional are indicator variables taking value one if the public body has at least one previous Consip purchase done while a mandatory or optional regime was in place. Standard errors, clustered at the PB level, are shown in parentheses.

[20] We hypothesise that this is possibly due to the category of goods purchased and to the types of public bodies. In Section A, Table A2, in the Appendix we document what types of goods and which PBs purchase through Consip during each regime.

FIGURE 5. SPILLOVER EFFECTS: PREVIOUS CONSIP EXPERIENCE



Notes: The figure plots coefficient estimates, and their respective 90% confidence interval, from different regressions. The baseline coefficient estimate is shown in column (4) of Table 2. The second and third coefficient estimates and standard errors are estimates of spillovers generated by having experienced first Consip goods during a Mandatory or Optional regime, respectively. These coefficient estimates and confidence intervals correspond to $\beta_1+\beta_M$ and $\beta_1+\beta_O$ in column (2) of Table 4, respectively.

VI. Overall savings from procurement centralization

We find significant positive spillovers of centralization on PBs that purchase autonomously. What do our findings say about the overall effects of centralization? To answer this question, we revisit BPV. The authors estimate that PBs purchasing through Consip save on average 28% of the price. To derive this result, they focus on PBs that buy a good from Consip when feasible, keeping in the control group not only purchases made before Consip's entry into the market but also purchases made on the open market *post-Consip*. In light of our findings, not accounting for indirect effects underestimates the direct savings from centralization. Here, we provide a revised estimate of direct savings in our subsample of contracts accounting for the indirect effects. Note that we cannot compare our

results directly with BPV since, as discussed in Section I, our sample is different from theirs: we restrict their sample to goods that are purchased both before and after Consip's entry in the specific market.

We evaluate both direct and indirect savings in a model where we interact our treatment variable $Post\ Consip_{gt}$ with an indicator for Consip purchases, as well as an indicator for out-of-Consip purchases. For this exercise, purchases via Consip are also added to our original sample.

Results are reported in Table 5, where we consider versions of controls analogous to those discussed in Section III for the estimation of indirect effects. Across specifications, the estimates are consistently negative and statistically significant at conventional levels for both savings components. The (baseline) specification in column (4), suggests that, after accounting for indirect savings, direct savings from purchasing from Consip increase to 37%. In our sample, our estimate is larger than that of BPV. Such large direct savings could be plausible due to the economies of scale associated with the nationwide size of centralized purchases, accompanied by extensive disintermediation.[21] Estimates of indirect savings also increase in this larger sample, though not statistically different from those shown in Table 2.

An additional source of bias could be present and counteract the first: if Consip purchased lower quality goods, the prices in the control group would be higher reflecting their better quality and, consequently, the direct savings would be over-estimated. In our previous estimates we followed BPV in controlling for quality through goods' characteristics, but not through their brand (e.g., Apple vs Asus laptops). Brands contain useful information, observable and unobservable characteristics. In column (5), we also consider this hypothesis and control for brand fixed effects. The estimate of direct savings falls from 37% to 20% of the purchase price, highlighting that part of the direct savings generated by centralized purchases could stem from a tendency of Consip to buy lower-value brands, an

[21] Discounts on the order of 40-50% of the already tight auction reserve price were common.

issue that had not been previously pointed out.

Two notes of caution should be mentioned. First, the specification in column (5) is fairly restrictive and we cannot also include good characteristics on top of brand fixed effects. Second, the sample size in column (5) is smaller, due to many singleton brands, than the sample used in the specification shown in column (4).[22]

Importantly, the estimate of indirect savings in column (5) remains close to earlier estimates from our analysis in Section III, in the range of 20 percent of the purchase price. In other words, these findings suggest that Consip may purchase, in an efficient way, low and cheap brands, reducing the magnitude of its direct savings. Instead, PBs that purchase autonomously benefit from informational spillovers from Consip and are also free to choose the brand that most suits their needs.

Table 5—Direct and Indirect Savings from Centralization

	(1)	(2)	(3)	(4)	(5)
Post Consip x Consip	-0.538	-0.419	-0.478	-0.464	-0.226
	(0.082)	(0.100)	(0.106)	(0.074)	(0.112)
Post Consip x Out-of-Consip	-0.347	-0.324	-0.284	-0.341	-0.177
	(0.072)	(0.079)	(0.090)	(0.070)	(0.097)
Observations	3783	2783	3690	3783	3163
PB fixed effects	Yes	Yes	Yes	Yes	Yes
Good fixed effects	Yes	Yes	Yes	Yes	Yes
Year-Month fixed effects	Yes	Yes	Yes	Yes	Yes
Good x PB fixed effects	No	Yes	No	No	No
Good x Year-Month fixed effects	No	No	Yes	No	No
Controls	No	No	No	Yes	No
Brand Fixed Effects	No	No	No	No	Yes

Notes: The dependent variable is the logarithm of the price paid by the public bodies for the specific good. Post Consip is an indicator variable that take values one if the good is purchased after Consip enters the market and zero otherwise. Standard errors, clustered at the PB level, are shown in parentheses.

[22] When we rerun the same specification as in column (4) using the sample as in column (5), the point estimate for the coefficient on Post Consip x Consip is -.366 with standard error of .0777. Instead the coefficient estimate on Post Consip x Out-of-Consip is -.262 with standard error of .078.

VII. Conclusions

This paper shows that public procurement centralization generates large indirect savings for contracting authorities that do not buy centrally, in addition to the direct savings for those that do. Our analysis suggests that the indirect savings from the introduction of Consip, the Italian central purchasing agency, are 22% on average.

These indirect effects mainly result from information externalities, rather than from an improved outside option for buyers. When we explore the heterogeneity of these effects, we find that they stem primarily from less competent public buyers purchasing more complex goods. Moreover, we also document that indirect savings for public bodies that purchase out-of-Consip are of similar magnitude to the direct savings generated by the centralized procurer that leverages large economies of scale, but purchases lower quality, and more standardized, goods.

While these results have clear and important policy implications, we must stress that we are only looking at the monetary benefits of centralization. We do not measure its many possible costs such as, for example, standardization and the resultant mismatch with heterogeneous buyers' preferences, a lack of control over non-contractible quality through local relationships, or barriers to entry for small and medium-sized firms. Centralization may also generate other benefits that we are unable to quantify, including reduced litigation, administrative costs, and corruption. To obtain a complete picture of the effects of public procurement centralization, future studies should address these other important aspects.

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Appendix: Indirect Savings from Public Procurement Centralization

By Clarissa Lotti, Arieda Muço, Giancarlo Spagnolo, and Tommaso Valletti

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This Appendix is structured as follows. Section A discusses additional features of the Consip framework program. Section B presents evidence on the lack of strategic timing of purchases. Section C provides details about the survey and data work. We conduct further robustness checks in Section D. Section E and Section F report tables for the heterogeneities mentioned in the paper and for the event study analysis, respectively.

A. More on the Consip Experiment

The treatment considered in the paper is Consip's entry into the relevant market, that is, PBs' ability to access centrally-negotiated framework agreements for the purchase of a specific good or service, and the possibility to observe the centrally-negotiated price for that good or service.

Table A1 presents the number of days a Consip agreement was active for each good type. We show the first, second and third deal negotiated by Consip in columns (1), (2) and (3), respectively. Goods that had no second or third Consip deal are assigned a zero.[1]

TABLE A1—DEAL DURATION

Number of days a Consip deal is active

	First deal	Second deal	Third deal
Laptop	120	865	0
Desk	222	0	0
Chair	549	0	0
Landline	729	364	0
Projector	287	0	0
Switch	730	0	0
Cable Copper	730	0	0
Lunch Vouchers	729	1,009	0
Paper	691	0	0
Fax	1,158	0	0
Mobile	319	918	0
Software	406	365	456
Printer	304	271	358

Notes: Each column shows the number of days a Consip agreement has been active for each type of good. The number of days corresponding to the first, second and third deal are shown in columns (1), (2) and (3), respectively. Note that not all goods had a second or a third deal.

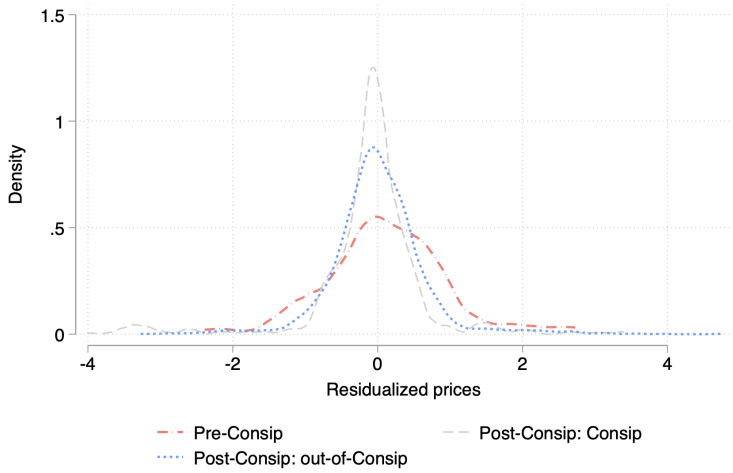
[1]The data summarized in Table A1 correspond to those shown in Figure 1 of the paper. At first sight, there seems to be an inconsistency for the goods Software and Lunch vouchers because the table reports more deals than what visually emerges from the figure. However, the underlying reason is simply the granularity of the data, as the figure cannot distinguish between deals that occur within a few days of each other (in the case of Software, the second deal ended on 17 July 2003, and the third deal began on 25 July 2003; in the case of Lunch vouchers, the first deal ended on 19 March 2003, and the second deal began on 24 March 2003).

Figure A1 displays the distribution of residualized prices, distinguishing between pre-Consip, Consip, and (post) out-of-Consip purchases. In panel (a), we compute residualized prices by controlling for PB, good, and month-year fixed effects; in panel (b), we additionally control for good characteristics.

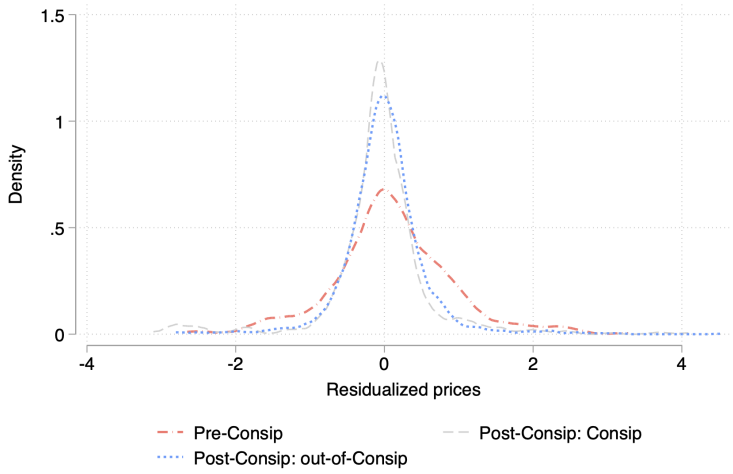
We show the distribution of residualized pre-Consip prices in red, whereas post Consip residualized prices are depicted in blue and gray, for out-of-Consip and Consip purchases, respectively.

The pre-Consip purchases are characterized by a higher mean and dispersion in both panels (a) and (b), respectively. The post-Consip distributions are characterized by a lower dispersion and mean. The latter two distributions seem to converge when we control for good characteristics. A formal Kolmogorov-Smirnov test, however, rejects the null of the equality of the distributions (although only at the 10 percent level).

FIGURE A1. PRE- AND POST-CONSIP RESIDUALIZED PRICES



(a) Not controlling for good characteristics



(b) Controlling for good characteristics

Notes: The plot depicts distributions of residualized prices obtained after regressing the logarithm of prices on a vector of quantities, PB fixed effects, good fixed effects, month-year fixed effects, and, only in panel (b), good characteristics. In red, we depict the distribution of pre-Consip prices, in blue that of (post) out-of-Consip prices, and in gray we show Consip prices.

A1. Who purchases during mandatory and optional regimes?

To understand which PBs buy and what types of goods are purchased from Consip during each regime, mandatory and optional, we present a balance test in Table A2. Columns (1) and (2) report the means for each group, while column (3) shows a formal mean comparison, namely a t-test, adjusted for group size.

As can be seen from the table, PBs that buy from Consip during mandatory or optional regimes are similar; only Universities seem to be more likely to purchase through Consip during a mandatory regime. Moreover, technologically complex goods such as Laptop, Fax, Software and Printer are more likely to be purchased from Consip while a mandatory regime is in place. The opposite is true for simple goods, which are more likely to be purchased from Consip during an optional regime.

TABLE A2—MEAN COMPARISON OF CONSIP PURCHASES DURING DIFFERENT REGIMES

	Consip		
	Mandatory (1)	Optional (2)	*t-stat* (3)
Type of Public Body			
Ministries and government	0.25	0.20	1.59
Social security	0.01	0.02	-0.59
Regional councils	0.02	0.03	-0.25
Province and town councils	0.25	0.26	-0.29
Health centers	0.30	0.33	-0.66
Mountain village councils	0.02	0.04	-1.51
University	0.09	0.05	1.90
Other	0.05	0.08	-1.23
Type of Good			
Laptop	0.22	0.03	8.89
Desk	0.00	0.07	-4.13
Chair	0.00	0.04	-3.22
Landline	0.20	0.13	2.37
Projector	0.07	0.00	5.66
Switch	0.01	0.03	-1.96
Cabble Copper	0.00	0.07	-3.79
Lunch Vouchers	0.06	0.38	-9.60
Paper	0.06	0.01	3.82
Fax	0.13	0.05	3.76
Mobile	0.09	0.18	-2.87
Software	0.05	0.02	2.15
Printer	0.11	0.00	7.02
Observations	454	239	

Notes: The table shows mean comparisons of Consip purchases during mandatory and optional regimes across types of public bodies and goods.

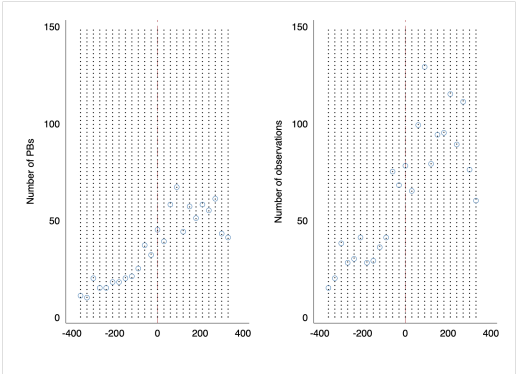
B. Strategic timing

We investigate whether PBs strategically alter the timing of their purchases to avoid delegating them to Consip. Managers who strategically alter their timing would purchase just before the start or just after the end of a Consip deal.

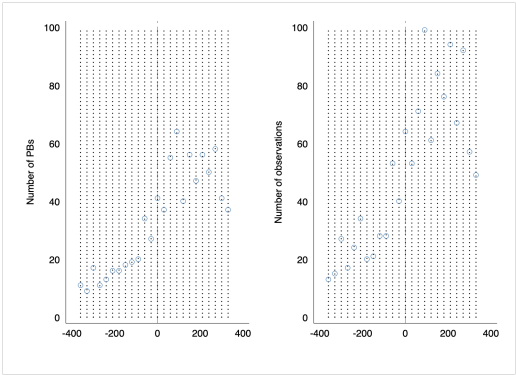
We analyze the distribution of PBs and their purchases around both the start and the end of the Consip deals. We recenter PBs and their purchases around the relevant event (i.e., start or end of a deal), accounting for all the Consip deals available in our sample for the different goods.

As shown in the following figures, reassuringly, we find no evidence of strategic timing behavior, even when separating between PBs that buy from Consip and those that do not. Indeed, we see no concentration of PBs or purchases before the start or after the end of Consip deals.

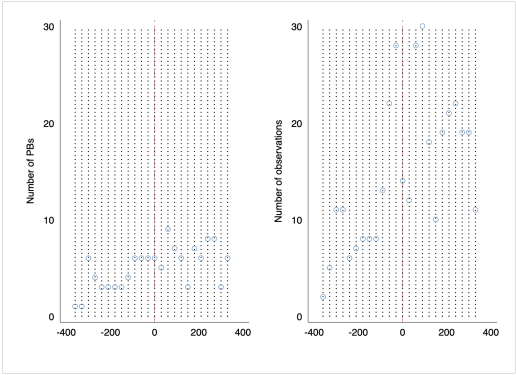
FIGURE B1. NUMBER OF PBS AND OF PURCHASES AROUND THE START OF A CONSIP DEAL



(a) All sample



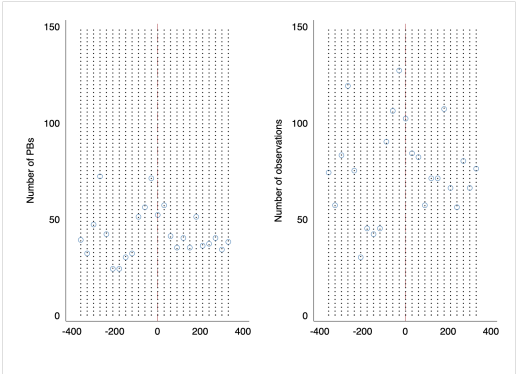
(b) PBs that do not buy from Consip



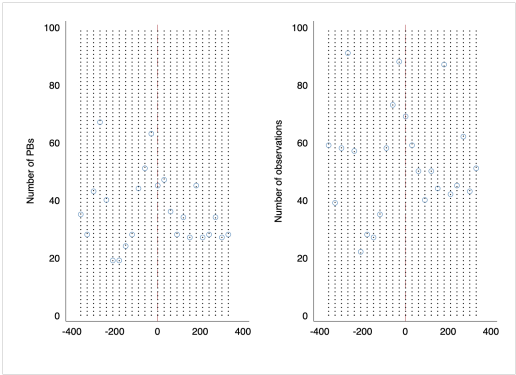
(c) PBs that do buy from Consip

Notes: The figure shows the number of PBs and the total number of purchases around the start of a Consip deal in 30-day intervals. In Panel (a) we plot the full sample, in panel (b) we plot PBs that do not buy from Consip, and in panel (c) PBs that do buy from Consip.

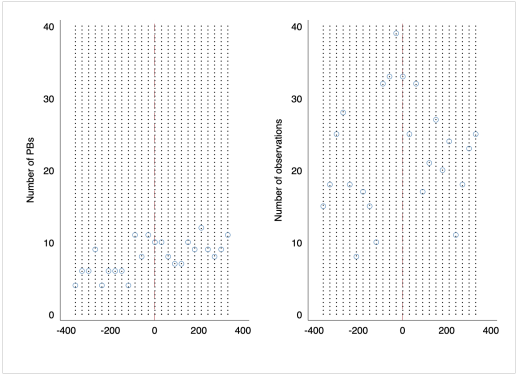
FIGURE B2. NUMBER OF PBS AND OF PURCHASES AROUND THE END OF A CONSIP DEAL



(a) All sample



(b) PBs that do not buy from Consip



(c) PBs that do buy from Consip

Notes: The figure shows the number of PBs and the total number of purchases around the end of a Consip deal in 30-day intervals. In Panel (a) we plot the full sample, in panel (b) we plot PBs that do not buy from Consip, and in panel (c) PBs that do buy from Consip.

C. Data Source and Data Handling

C1. Bias due to retrospective survey

Generally, retrospective bias is a potential concern because of the self-reported nature of the data. However, in the present context, we do not believe this is a major issue for a variety of reasons.

First, PBs are requested to keep records of their purchases. Filling the questionnaires would imply accessing their records, not relying purely on self-reports. Managers had to report previous purchases covering around two years (because multiple surveys were run, one can see purchases for longer time periods in the dataset).

Second, the whole survey was run by the Italian National Institute of Statistics (ISTAT) jointly with the Italian Treasury (Ministero dell'Economia e delle Finanze, MEF), with considerable resources put into the exercise. For each public body, there was a particular person in charge of answering the questionnaire, typically the person signing procurement contracts. For all these people there are personal IDs, emails, and phone numbers in our dataset. Each person had assigned a counterpart in ISTAT ("referente ISTAT"), whose job was to check the progress until the survey was responded to properly. There was a dedicated call center and dedicated e-mail service for queries, all with the purpose of supporting filling the questionnaire. This was run by Consip and handled thousands of queries: possibly, as a consequence of this set up, the response rates were very high for this type of exercise (around 80%). Also, random samples of contracts had to be supplied to MEF. The questionnaires themselves were quite rigorous, nothing like "what do you remember about" or the like. They all had a friendly web interface to facilitate filling out the survey.

C2. Data handling

There is a small difference between our Figure 1 in the paper and a plot similar in spirit included in Appendix T&F in **?**: the presence of Consip deals in the market for Fax machines. During our data work, we noticed a small inaccuracy in how the deal for Fax machines was recorded in **?**, and we corrected it to be consistent with the coding for other deals. This correction is included in the replication package. This coding difference, however, does not alter the original BPV findings.

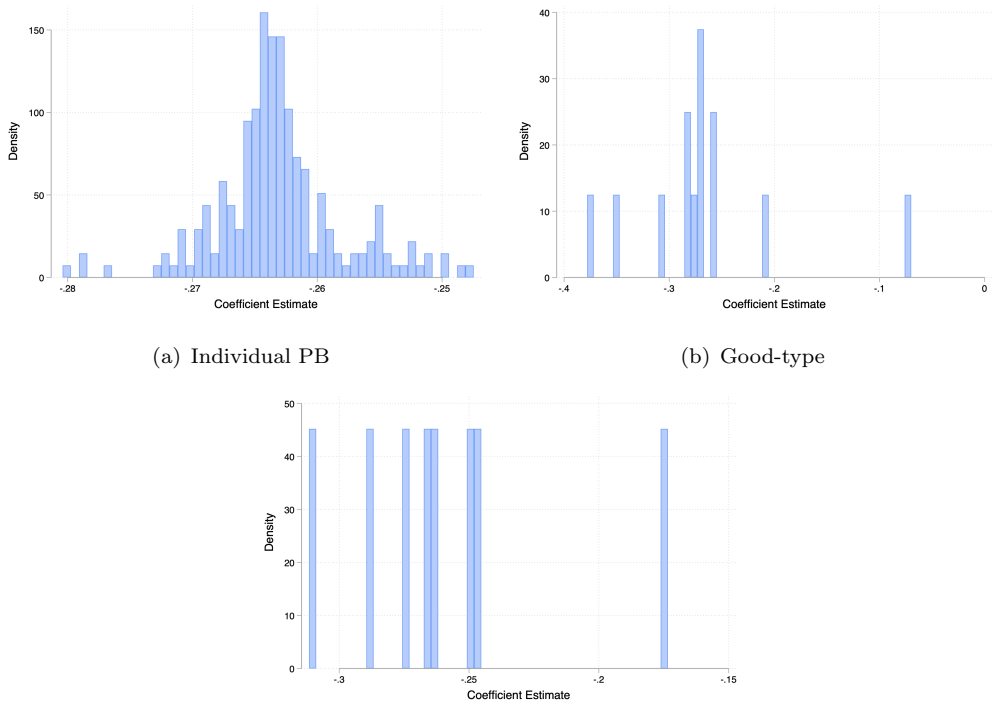
D. Robustness

To check for the robustness of our results, we run a battery of tests.

D1. Dropping one group at a time

First, to confirm the robustness of our main parameter of interest, as an outlier detection exercise, we drop one PB at a time, one good at a time, and one type of PB at a time. Our results are shown in Figure D1. The coefficient estimate remains, in 99 percent of the cases, statistically significant and comparable in magnitude to our main estimate.

FIGURE D1. OUT-OF-CONSIP PURCHASES: ROBUSTNESS



(a) Individual PB

(b) Good-type

(c) PB-type

Notes: The plots show out-of-Consip coefficient estimates resulting from an outlier detection exercise. In Panel (a), we drop one buyer at a time. In Panel (b), we drop one good at a time. In Panel (c), we drop one PB-type at a time.

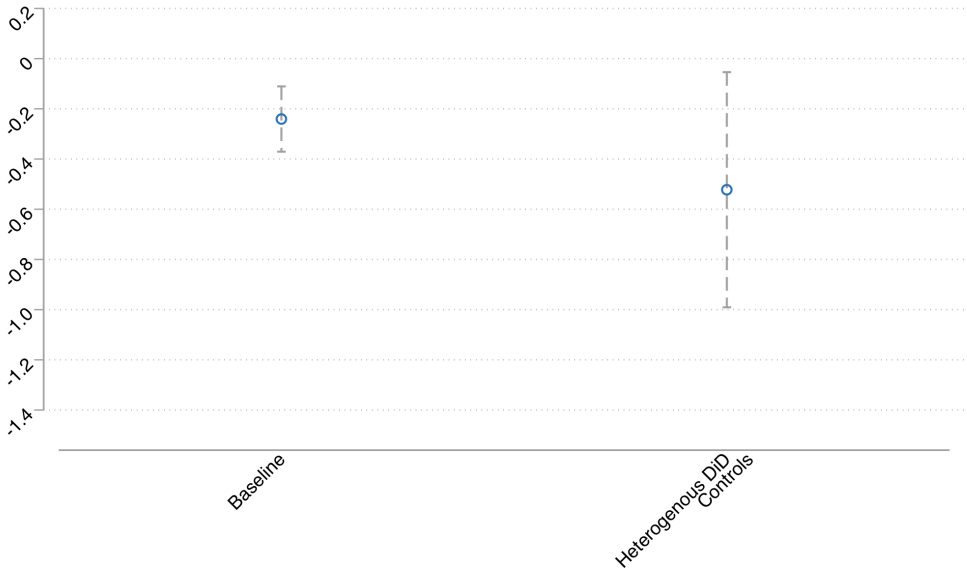
D2. Heterogeneous Differences-in-Differences

Moreover, we implement heterogeneous difference-in-differences as proposed by De Chaisemartin and d'Haultfoeuille (2020). Our setting, differently from what is considered in the framework of De Chaisemartin and d'Haultfoeuille (2020), has multiple treatment points in time for the same group, different groups (group heterogeneity), and an extremely large number of controls (which we pre-select via PDS Lasso).

To handle this, we first residualize prices accounting for the distinct goods purchased by the PB and good-specific non-parametric time trends, including the previously selected set of Lasso controls. We then use the Stata command *did_multiplegt* by De Chaisemartin, D'Haultfoeuille and Guyonvarch (2019) that implements, in Stata, De Chaisemartin and d'Haultfoeuille (2020), using as our outcome variable the residualized prices previously derived.

The results from implementing this strategy are shown in Figure D2. Results are compared to our 'baseline', namely our preferred estimate derived in the paper. Albeit the methods of De Chaisemartin and d'Haultfoeuille (2020) yield a higher coefficient estimate, the confidence interval contains the baseline.

FIGURE D2. HETEROGENEOUS DID



Notes: The figure presents coefficients estimates from our baseline regression and the heterogeneous DiD estimates following De Chaisemartin and d'Haultfoeuille (2020), and the respective 95% confidence intervals.

D3. Robustness Consip Experience

We conduct some robustness on the results on spillovers from the Consip experience (Section V.B of the paper). We check if our results are driven by the type of PB. The governance of central PBs is different from the governance of more autonomous PBs such as universities. Hence we first drop from our sample central PBs, and then we drop autonomous PBs.

Results are reported in Table D1. In column (1), we consider the full sample, as in the paper. In column (2), we drop central PBs, whereas in column (3) we drop autonomous PBs such as universities and health centers. As can be seen from the table, our findings are robust to these sample restrictions.

Table D1—Consip Experience Spillovers (Robustness)

	Full sample	No Central PBs	No Autonomous
	(1)	(2)	(3)
Post Consip (β_1)	-0.173	-0.195	-0.135
	(0.084)	(0.088)	(0.127)
Consip Experience from Mandatory Regime	-0.017	0.211	-0.061
	(0.128)	(0.168)	(0.129)
Consip Experience from Optional Regime	0.336	0.225	0.434
	(0.145)	(0.163)	(0.205)
Post Consip x Consip Experience from Mandatory Regime (β_M)	-0.036	-0.248	0.039
	(0.133)	(0.168)	(0.155)
Post Consip x Consip Experience from Optional Regime (β_O)	-0.311	-0.209	-0.288
	(0.154)	(0.170)	(0.207)
Estimate $\beta_1 + \beta_M$	-0.209	-0.443	-0.097
SE $\beta_1 + \beta_M$	0.127	0.186	0.137
Estimate $\beta_1 + \beta_O$	-0.484	-0.404	-0.423
SE $\beta_1 + \beta_O$	0.158	0.160	0.217
Observations	3091	2771	1695
PB fixed effects	Yes	Yes	Yes
Good fixed effects	Yes	Yes	Yes
Year-Month fixed effects	Yes	Yes	Yes
Controls	No	No	No

Notes: The dependent variable is the logarithm of the price paid by the public bodies for the specific good. Post Consip is an indicator variable that takes value one if the good is purchased after Consip enters the market and zero otherwise. Controls are selected using the PDS Lasso methodology. Consip Experience from Mandatory or Optional are indicator variables taking value one if the public body has at least one previous Consip purchase done while a mandatory or optional regime was in place. Standard errors, clustered at the PB level, are shown in parentheses.

E. Heterogeneities by good, institutions and size

We extend our regression model to allow for various types of heterogeneity in the treatment effect depending on the type of good, complex or simple, and type of institutional class the public body belongs to.

To identify heterogeneous effects by market, we interact the $Post\,Consip$ indicator with indicator variables for each good. Table E1 reports the results and highlights heterogeneity. We observe that indirect savings emerge mainly in markets with technologically complex goods, such as laptops, projectors, and fax machines. A possible interpretation is that simpler goods are more easily comparable, and leave less room for price heterogeneity or differentiation. Therefore, simpler goods could already be rather competitive before centralization takes place. If a market is more transparent and competitive (which is captured by good fixed effects), it is plausible that Consip's entry in the market does not generate strong information externalities.

The second dimension of heterogeneity we investigate relates to the type of PB. In Table E2, we interact the $Post\,Consip$ indicator with an indicator variable for each PB institutional class.[2] We follow BPV and classify PBs into: i) Napoleonic bodies, i.e., central administrations whose operations tend to be controlled by civil servants; ii) local governments, whose CEOs are elected directly and have broad powers; and iii) semi-autonomous bodies, such as health authorities and universities, who enjoy substantial budgetary and administrative autonomy. We find that only some classes of PBs generate significant indirect savings: semi-autonomous bodies and, to a lesser extent, local bodies. Savings for central administrations, are not statistically different from zero.

Table E3 considers heterogeneity analysis with respect to pre-Consip distribution of purchased quantities. We rank PBs in quartiles based on their pre-Consip average quantities purchased, from lowest (1st Quantity Quartile) to highest (4th

[2]The model specification is different from that considered so far because we cannot include PB fixed effects but only PB type fixed effects.

Quantity Quartile). Then, interact the $Post\,Consip$ indicator with indicator variables for each pre-Consip PB quantity quartile. We find that the indirect effects are statistically different from zero in the lower quartiles. Savings emerge when PBs purchase less, and thus are less likely to have individual purchasing experience before Consip's entrance. This is in line with our interpretation in Section IV of the paper, and Figure 4 in particular: savings emerge when PBs have less experience and are more inefficient, while more efficient PBs already know how to procure their goods.

Finally, in Table E4 we show the table equivalent of Figure 4 included in Section IV of the paper. Namely, we explore the heterogeneity in indirect savings among PBs based on their competence levels. Results indicate that indirect effects are statistically different from zero and increasing in magnitude only in the two upper quartiles of the distribution.

TABLE E1—HETEROGENEOUS EFFECTS BY MARKET

	(1)	(2)
Post-Consip × Laptop	-0.747	-0.830
	(0.186)	(0.192)
Post-Consip × Desk	-0.310	-0.233
	(0.121)	(0.107)
Post-Consip × Chair	-0.149	-0.154
	(0.112)	(0.107)
Post-Consip × Landline	-1.570	-0.715
	(0.790)	(0.780)
Post-Consip × Projector	-0.499	-0.515
	(0.083)	(0.086)
Post-Consip × Switch	-0.363	-0.293
	(0.211)	(0.212)
Post-Consip × Cable Copper	-0.004	-0.025
	(0.329)	(0.299)
Post-Consip × Lunch Vouchers	-0.556	-0.632
	(0.159)	(0.189)
Post-Consip × Paper	-0.079	-0.023
	(0.119)	(0.122)
Post-Consip × Fax	-0.485	-0.551
	(0.134)	(0.171)
Post-Consip × Mobile	0.104	0.611
	(0.753)	(0.737)
Post-Consip × Software	-0.527	-0.959
	(0.504)	(0.517)
Post-Consip × Printer	-0.307	-0.583
	(0.739)	(0.643)
Observations	3091	3091
PB fixed effects	Yes	Yes
Good fixed effects	Yes	Yes
Month-year fixed effects	Yes	Yes
Controls	No	Yes

Notes: The dependent variable is the logarithm of price. Each specification controls for the vector of quantities purchased by each PB. Standard errors are clustered at the PB level.

TABLE E2—HETEROGENEOUS EFFECTS BY PB INSTITUTIONAL CLASS

	(1)	(2)
Post-Consip × Napoleonic bodies	-0.124	-0.121
	(0.115)	(0.104)
Post-Consip × Local governments	-0.376	-0.271
	(0.151)	(0.127)
Post-Consip × Semi-autonomous bodies	-0.283	-0.290
	(0.084)	(0.077)
Observations	3091	3091
PB type fixed effects	Yes	Yes
Good fixed effects	Yes	Yes
Month-year fixed effects	Yes	Yes
Controls	No	Yes

Notes: The dependent variable is the logarithm of price. Each specification controls for the vector of quantities purchased by each PB. Standard errors are clustered at the PB level.

TABLE E3—HETEROGENEOUS EFFECTS BY PRE-CONSIP QUANTITIES

	(1)	(2)
Post-Consip × 1st Quantity Quartile	-0.251	-0.196
	(0.080)	(0.077)
Post-Consip × 2nd Quantity Quartile	-0.409	-0.337
	(0.153)	(0.155)
Post-Consip × 3rd Quantity Quartile	-0.107	-0.118
	(0.113)	(0.109)
Post-Consip × 4th Quantity Quartile	-0.072	-0.070
	(0.154)	(0.129)
Observations	2136	2136
PB fixed effects	Yes	Yes
Good fixed effects	Yes	Yes
Month-year fixed effects	Yes	Yes
Controls	No	Yes

Notes: The dependent variable is the logarithm of price. Each specification controls for the vector of quantities purchased by each PB. Standard errors are clustered at the PB level.

TABLE E4—HETEROGENEOUS EFFECTS BY PRE-CONSIP PRICES

	(1)	(2)
Post Consip x 1st Quartile	0.167	0.178
	(0.139)	(0.131)
Post Consip x 2nd Quartile	-0.189	-0.114
	(0.072)	(0.078)
Post Consip x 3rd Quartile	-0.339	-0.357
	(0.081)	(0.081)
Post Consip x 4th Quartile	-0.918	-0.800
	(0.132)	(0.128)
Observations	2550	2550
PB fixed effects	Yes	Yes
Good fixed effects	Yes	Yes
Month-Year fixed effects	Yes	Yes
Controls	No	Yes

Notes: The dependent variable is the logarithm of price. Each specification controls for the vector of quantities purchased by each PB. Standard errors are clustered at the PB level.

F. Event study coefficients

In Table F1 we present the event study coefficients, and their standard errors, for each event study plot included in the paper. Columns (1) and (2) present our main results (illustrated in the paper in Figure 2), columns (3) and (4) instead show the results by good-type: complex and simple (illustrated in the paper in Figure 3).

TABLE F1—EVENT STUDIES AROUND START AND END OF FIRST DEAL

	Main results		Heterogeneity by good-type	
	(1) Deal Start	(2) Deal End	(3) Complex	(4) Simple
Quarter -7	0.178	0.288	0.079	0.167
	(0.130)	(0.145)	(0.087)	(0.172)
Quarter -6	0.099	0.158	-0.009	0.080
	(0.136)	(0.104)	(0.243)	(0.151)
Quarter -5	0.021	0.130	0.013	0.002
	(0.220)	(0.090)	(0.165)	(0.238)
Quarter -4	-0.087	0.085	-0.189	-0.116
	(0.155)	(0.079)	(0.339)	(0.188)
Quarter -3	-0.025	-0.004	0.119	-0.062
	(0.145)	(0.080)	(0.176)	(0.166)
Quarter -2	0.042	0.019	0.084	0.023
	(0.138)	(0.087)	(0.124)	(0.167)
Quarter -1	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
Quarter 0	-0.112	-0.205	-0.107	-0.160
	(0.118)	(0.144)	(0.282)	(0.132)
Quarter +1	-0.087	0.006	-0.249	-0.089
	(0.101)	(0.086)	(0.102)	(0.129)
Quarter +2	-0.154	-0.053	-0.016	-0.192
	(0.091)	(0.095)	(0.120)	(0.107)
Quarter +3	-0.254	-0.087	-0.266	-0.283
	(0.095)	(0.106)	(0.141)	(0.113)
Quarter +4	-0.242	-0.049	-0.303	-0.197
	(0.092)	(0.094)	(0.176)	(0.118)
Quarter +5	-0.312		-0.485	-0.224
	(0.100)		(0.120)	(0.148)
Quarter +6	-0.349		-0.504	-0.331
	(0.130)		(0.192)	(0.209)
Quarter +7	-0.373		-0.689	-0.342
	(0.118)		(0.221)	(0.158)
Quarter +8	-0.446		-0.725	-0.411
	(0.120)		(0.199)	(0.157)
Quarter +9	-0.204			-0.182
	(0.173)			(0.190)
Quarter +10	-0.371			-0.364
	(0.107)			(0.134)
Quarter +11	-0.314			-0.289
	(0.106)			(0.138)
Constant	7.450	7.288	7.404	-2.390
	(0.104)	(0.097)	(0.150)	(1.637)
Observations	2024	1419	386	1638
PB fixed effects	Yes	Yes	Yes	Yes
Good fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: We show coefficient estimates and standards errors for each event study plot in the paper.

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