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January 22, 2026

**Abstract**

We study how procurement-driven fiscal contractions propagate through firm-to-firm production networks. The 2014 oil price collapse triggered a sharp fall in public procurement in Ecuador, generating an externally driven fiscal demand shock. Using administrative data covering the formal-firm universe from 2012 to 2019, we construct exposure measures that trace the shock from government contractors to their trading partners. Aggregating the causal estimates implies a network-adjusted fiscal multiplier of 1.95: a one-dollar procurement reduction lowers aggregate value added by US$1.95, compared with US$1.18 when network propagation is ignored. Production linkages therefore account for about 39 percent of the total response.

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# 1 Introduction

In economies where the state is a large buyer, fiscal contractions often affect private firms through public procurement. Cuts in government purchasing withdraw demand from public contractors and can propagate to other firms through supplier-buyer linkages. This paper asks a simple question: when a procurement-driven fiscal contraction occurs, how much of the resulting decline in real economic activity reflects the direct effects on firms that lose public demand, and how much reflects second-order spillovers transmitted through production networks?

We study these effects by exploiting a large contraction in public procurement triggered by the 2014 collapse in international oil prices. Our setting is Ecuador, a resource-dependent, dollarized economy in which the state is the largest buyer of goods and services. Public procurement accounted for an average of 10.6 percent of GDP from 2010 to 2014, but only 6.2 percent from 2015 to 2024, reflecting a persistent procurement-driven fiscal contraction following the oil price collapse.

The origin of the shock was global and orthogonal to firm-level conditions and when combined with Ecuador's institutional environment makes public procurement a natural transmission channel (Zanoni and Pedemonte, 2025). While Ecuador provides a useful laboratory, the mechanism is broader: commodity-price volatility and sizable public procurement are common features of many middle-income economies (OECD, 2019).

To answer the research question, we assemble and link administrative data covering the universe of formal firms and government entities from 2012 to 2019. The data include firms' balance sheets, transaction-level procurement records, and detailed buyer–supplier relationships. In addition to firm–state transactions, we also observe firm–to–firm transactions. These features allow us to construct firm-level exposure measures that map the procurement contraction into the production network.

Addressing our research question raises two central challenges from a causal inference perspective. First, firms that sell to the state may differ systematically from those that do not, so naïve comparisons of their performance would conflate selection into public procurement with the effects of the shock. Second, standard causal inference typically relies on the Stable Unit Treatment Value Assumption (SUTVA), which in our case translates into the requirement that a firm's outcome depends only on its own exposure and not on the exposure of other firms. A procurement contraction violates this assumption by construction: one firm's exposure can affect its suppliers, customers, and competitors, which is precisely the propagation mechanism emphasized in the production networks literature (Acemoglu et al., 2012; Carvalho et al., 2020; Boehm, 2020).

We address selection with a weighted difference-in-differences design that balances preshock observables and trends between exposed and nonexposed firms. We address interference by using an exposure-mapping approach: we first identify state suppliers (direct exposure), then identify suppliers of state suppliers (indirect exposure), and more generally define exposure at successive layers of network proximity, allowing us to separate first-order effects from second-order spillovers. This approach allows outcomes to depend on multiple dimensions of exposure rather than assuming away spillovers.

We find that the procurement contraction propagated beyond government contractors. Directly exposed firms experienced moderate but persistent declines in revenues (3.5–8.6 percent), labor costs (4.2–8.6 percent), and investment (up to 5.7 percent). Network structure is associated with substantial amplification of these effects: firms embedded in dense procurement clusters suffered additional revenue declines of 20–26 percent and labor cost reductions of 11–14 percent. Spillovers were also economically meaningful: firms with no public contracts but that sold to state suppliers experienced revenue losses of up to 7.3 percent and labor reductions of nearly 6 percent. Across all margins, micro and small firms were disproportionately affected.

Our microlevel estimates also deliver a macroeconomic object. Leveraging the richness of the administrative data and the causal identification of first- and second-order effects, we construct a network-adjusted fiscal multiplier directly from microevidence without imposing functional-form restrictions or structural modeling assumptions. We estimate this multiplier to be 1.95. In contrast, a calculation that ignores network propagation yields a multiplier of 1.19 in the same setting. This gap implies that standard approaches capture only about 61 percent of the total output response, while the remaining 39 percent reflects propagation through production networks.

## Relation to the literature

This paper speaks to three related literatures. First, the production networks literature emphasizes that interfirm links can transmit and amplify shocks (Acemoglu et al., 2012; Carvalho, 2014). More-recent research has shown the relevance of nonlinearities, sector heterogeneity, input specificity, and input-output linkages (Baqaee and Farhi, 2019; Barrot and Sauvagnat, 2016; Boehm et al., 2019; Carvalho et al., 2020; Barattieri et al., 2025). Yet the empirical causal evidence mapping first- and second-order responses using firm-level data in the context of fiscal shocks remains scarce, particularly for developing economies.

A closely related gap exists in quantitative macromodels of fiscal policy, which often rely on representative firms or highly aggregated sectors and treat supplier-buyer relationships with reduced-form parameters (Baqaee and Farhi, 2019). Although this preserves tractability and enables counterfactual analysis, it leaves key propagation mechanisms largely untested, a concern emphasized by recent work calling for microevidence to discipline network assumptions (Barnichon et al., 2022). Our exposure-mapping design provides a direct way to separate first-order contraction effects from second-order network spillovers and to quantify the contribution of propagation to the total response.

The second literature to which we make a contribution is that of public procurement, as we provide causal microevidence on how fiscal retrenchment transmits through procurement in a commodity-dependent economy (Coviello et al., 2021). This matters most in settings where procurement is quantitatively important (OECD, 2019) and where contract allocation may deviate from efficiency criteria, creating scope for misallocation (Brugués et al., 2024; Coviello et al., 2021). In such environments, expansions may reallocate demand in ways unrelated to productivity, and contractions may transmit and amplify demand shocks through supplier networks. Distinguishing whether procurement shocks remain localized among contractors or cascade across their trading partners is therefore central for policy design and for assessing the propagation mechanisms embedded in network-based macroframeworks (Acemoglu et al., 2012; Baqaee and Farhi, 2019; Boehm, 2020; Carvalho et al., 2020).

The interpretation of aggregate fiscal responses is the third strand of the literature for which our results are relevant. Fiscal multiplier estimates have made substantial progress in identification and measurement (Auerbach and Gorodnichenko, 2012; Ilzetzki et al., 2013; Nakamura and Steinsson, 2018; Ramey, 2019), yet without causal evidence on the firm-level channels through which spending shocks affect output, employment, and investment it is difficult to know what component of the general-equilibrium response those aggregate estimates are capturing. Existing firm-level evidence remains scarce and concentrated in the United States (Bouakez et al., 2025; Cohen et al., 2011; Chodorow-Reich, 2019). By quantifying both direct contractor effects and spillovers to indirectly connected firms, we provide a microfoundation for interpreting aggregate fiscal responses in environments where procurement is the main transmission channel, and we connect reduced-form multiplier evidence to the propagation mechanisms highlighted in the networks literature (Acemoglu et al., 2012; Baqaee and Farhi, 2019; Carvalho et al., 2020; Boehm, 2020). Specifically, while the classic fiscal multiplier literature tells us how much output responds, our approach sheds light on how and why those responses materialize, emphasizing network spillovers that are usually invisible in aggregate data.

The rest of the paper is organized as follows: section 2 describes the institutional setting and the procurement channel. Section 3 presents the administrative data and key construction steps. Section 4 lays out the exposure measures and the weighted difference-in-differences design. Section 5 reports the first- and second-order firm-level effects, heterogeneity, and the implied network-adjusted multiplier (Table 2). Section 6 presents robustness exercises. Section 7 concludes. The appendix reports additional descriptive statistics, complementary results and coefficient tables, and additional robustness checks.

## 2 Institutional context

Public procurement is a central component of current fiscal expenditure in Ecuador. According to the Ministry of Economics and Finance (MEF), purchases of goods and services by the central government accounted for approximately 46.0 percent of total current expenditure between 2012 Q1 and 2014 Q4. From 2015 Q1 onward, this share fell to 32.8 percent, reflecting a sharp adjustment in public purchases due to the collapse of international oil prices.

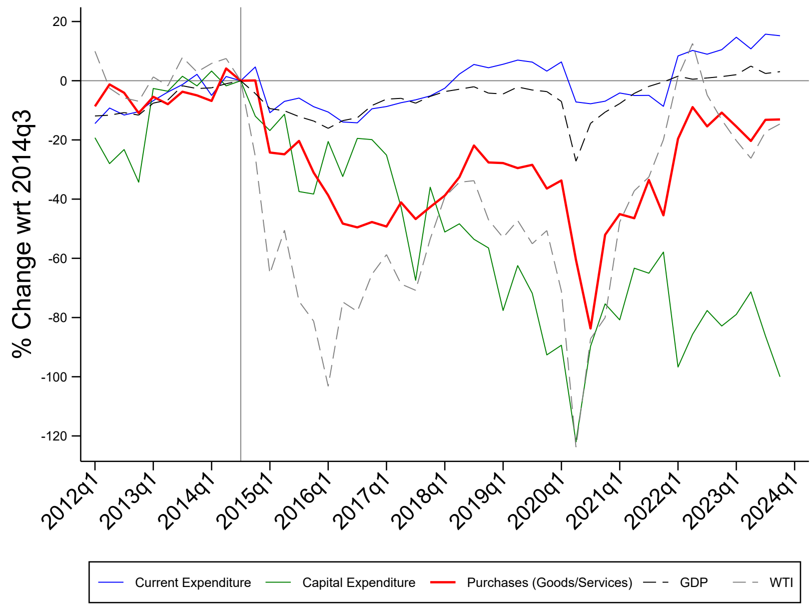



Figure 1: **Public procurement and capital expenditure around the oil price collapse** *Note:* The figure plots central government purchases of goods and services and capital expenditure as shares of total current expenditure for the nonfinancial public sector. The vertical line marks the onset of the oil price collapse in 2015.

Figure 1 shows the evolution of purchases of goods and services, current and capital expenditure, oil prices (measured by the WTI basket), and GDP. Both capital expenditure and purchases correlate strongly with the oil shock and drive the observed contraction.

The regulatory framework in Ecuador imposes a strict link between the composition of spending and the composition of revenues. Article 286 of the Ecuadorian Constitution states that public finances must be managed in a sustainable and responsible way and that permanent expenditures must be financed with permanent revenues, mainly tax collections and other predictable sources.[1] The Ministerio de Economía y Finanzas defines permanent and nonpermanent revenues and expenditures in its official glossaries and fiscal programming documents. Nonpermanent revenues include oil revenues, proceeds from asset sales, and borrowing, and are meant to finance nonpermanent expenditures associated with capital formation and investment. Permanent expenditures correspond to recurrent outlays that sustain the ongoing provision of public services.

Within this framework, not all public procurement is classified as permanent expenditure. A

[1] Article 286 of the Ecuadorian Constitution; see also, for example, the legal basis summarized in the *Proforma Presupuestaria 2025* issued by the Ministerio de Economía y Finanzas.

significant share corresponds to purchases linked to public investment projects and is therefore recorded as nonpermanent expenditure, alongside broader capital spending. This institutional distinction matters, because it determines which items are adjusted when nonpermanent revenues fall short.

Oil-related income is a key nonpermanent revenue source. The budget documents distinguish petroleum revenues that finance the Presupuesto General del Estado (PGE) from gross oil receipts. Net budgetary oil income is computed by subtracting operating costs, investment by public oil companies, contractual payments to private operators, and legally mandated earmarks from the gross value of crude and derivative exports. The remaining balance accrues to the PGE as oil revenue.[2] These flows are channeled through public oil companies and the Banco Central del Ecuador and then transferred to the Ecuadorian Treasury as fiscal revenue.

When international oil prices fall, the gross value of exports declines while many costs and earmarked transfers remain fixed in the short run, so the residual oil income available to the PGE contracts disproportionately. This mechanism generates an immediate liquidity constraint for the central government and forces rapid cuts in nonpermanent spending, particularly in public investment and procurement contracts.

Importantly, this fiscal transmission mechanism is not limited to the central government. A substantial share of subnational governments' revenues in Ecuador is also directly linked to oil revenues, implying that procurement adjustments at the local level are likewise driven by oil price fluctuations rather than by independent policy choices. This institutional feature reinforces the interpretation of the oil price collapse as a common fiscal shock affecting both central and subnational procurement decisions (Sánchez-Aragón et al., 2026).

The microdata show that the state is also a central node in the production network. In 2014, roughly 40 percent of formal private firms sold directly to at least one public-sector entity, so they depended on public demand for part of their revenues. Figure 2 illustrates the network of state suppliers by economic sector, while figure 3 shows that the public sector ranks among the most connected sectors even after aggregating firms to the one-digit ISIC level. These patterns reinforce the idea that a contraction in public procurement can propagate widely through the

[2] See, for example, the *Justificativo de Ingresos y Gastos* of the PGE, which details the origin of oil revenues and the deductions applied before resources are transferred to the budget.

production network.

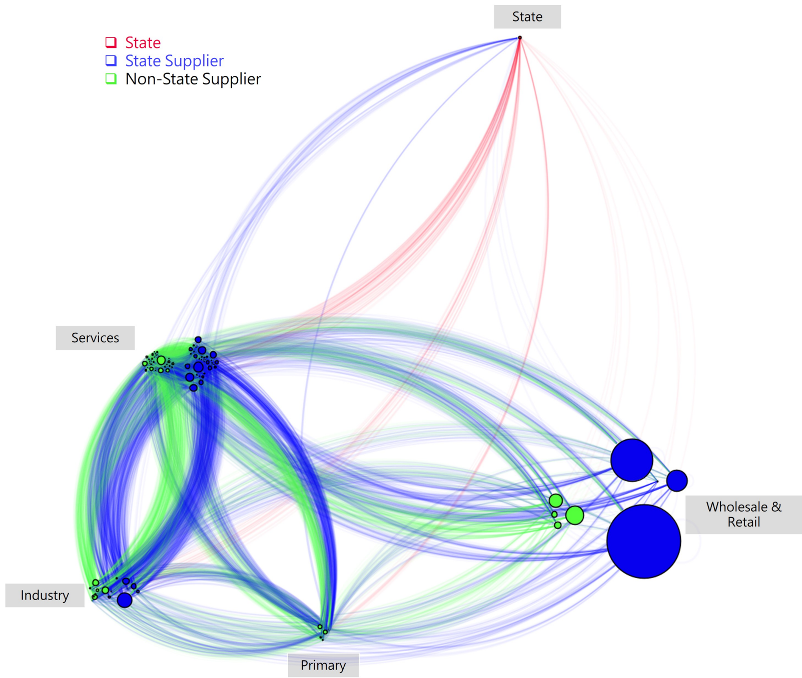



Figure 2: **Network of state suppliers by sector in Ecuador, 2014** *Note:* The figure depicts sales flows across broad economic sectors in Ecuador, with node size proportional to firm counts and edge opacity reflecting transaction volumes. Edge color identifies the selling sector. Approximately 40 percent of formal private firms are state suppliers in this baseline year.

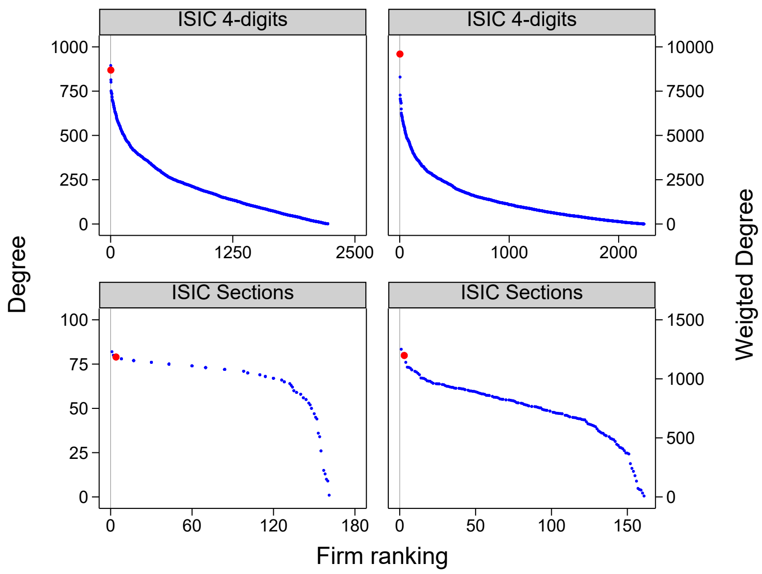



Figure 3: **Centrality of the public sector in the production network in Ecuador** *Note:* The figure reports degree and revenue-weighted degree centrality measures by one-digit ISIC sector. The red dot corresponds to the state, which ranks among the top sectors in both metrics, underscoring its central role as a buyer in the domestic production network.

# 3 Data

The analysis combines administrative data sets covering the universe of formal firms in Ecuador from 2012 to 2019. This period spans both the expansion preceding the 2014 oil-price collapse and the fiscal contraction that followed. We treat 2012–2014 as the pretreatment baseline, using 2014 to define firm classifications and reference values.

Balance sheet data come from the *Formulario 101*, the annual corporate tax declaration filed with the Ecuadorian Internal Revenue Service (Servicio de Rentas Internas, SRI). When multiple amended declarations exist for a given firm–year, we retain the most recent filing; in the rare cases where multiple filings share the same submission timestamp, we prioritize non-zero declarations, following the SRI's filing framework for amended and zero-return submissions (Servicio de Rentas Internas, 2016a, 2018). This source provides total and domestic revenue, input purchases, labor costs, investment, liabilities, and export share.

Production networks are identified using the *Anexo Transaccional* (ATS), which records all transactions between buyers and sellers reported for tax purposes from 2012 to 2019. By aggregating buyer-reported purchases, we reconstruct seller-level revenue and identify (1) firms selling to the public sector (state suppliers), (2) firms buying from the public sector (state buyers), and (3) second-order links, such as suppliers of state suppliers. While reporting obligations under the ATS changed during the period, particularly for electronic sales documents, our network construction relies on buyer-reported purchases, whose definitions and coding remain stable over time (Servicio de Rentas Internas, 2019, 2016b). The *Catastro RUC*, updated to July 2025, complements this information with firm registries and detailed ISIC codes, allowing us to restrict the analysis to private firms.

To ensure comparability, we keep active firms with positive values for labor costs and investment, and drop observations with nonpositive revenue, liabilities, or input purchases in the pretreatment years. We trim the bottom 1 percent of the distribution of revenue and value added, but do not winsorize the upper tail. All nominal variables are deflated using the CPI (base year 2014).

After these restrictions are made, the data set contains roughly 20,000 firms per year—over 160,000 firm–year observations in total. Table 1 summarizes the annual distribution of firms by

| | Number of firms | |
|---|---|---|
| **Year** | **State supplier** | **Nonstate supplier** |
| 2012 | 9,041 | 12,756 |
| 2013 | 9,926 | 14,663 |
| 2014 | 10,968 | 17,866 |
| 2015 | 10,153 | 16,097 |
| 2016 | 9,549 | 14,731 |
| 2017 | 9,012 | 13,640 |
| 2018 | 8,626 | 12,804 |
| 2019 | 8,256 | 12,112 |

Table 1: **Annual distribution of state supplier status in Ecuador, 2012–2019** *Note:* Firms are classified according to their supplier status in 2014. Values correspond to the total number of active private firms reporting positive labor costs and investment each year.

their 2014 supplier status. State suppliers account for about 40 percent of formal firms, peaking in 2014 with 10,968 suppliers out of 28,834 active firms. The subsequent decline mirrors the contraction in public procurement following the oil-price collapse.

Selection into public procurement is widespread across industries. The public sector's degree centrality—the number of firms directly connected to it—is the highest among all ISIC 4-digit industries and ranks among the top five ISIC sections, underscoring its systemic role as a buyer in the economy. Still, procurement is more concentrated in specific sectors, particularly Wholesale & Retail, Manufacturing, Construction, and Professional Services (see figure A1 in appendix A).

Figure 4 compares the size distributions of state and nonstate suppliers across industries using kernel density estimates of log revenue. State suppliers consistently occupy the upper tail of the distribution, indicating that public procurement is concentrated among larger, more dominant firms. This pattern is especially pronounced in Mining, Financial Services, Real Estate, and Health sectors.

This size asymmetry is central to the propagation mechanism: if state suppliers are systematically larger, a decline in state demand affects not only them, but their trading partners as well, amplifying the aggregate effect through production networks.

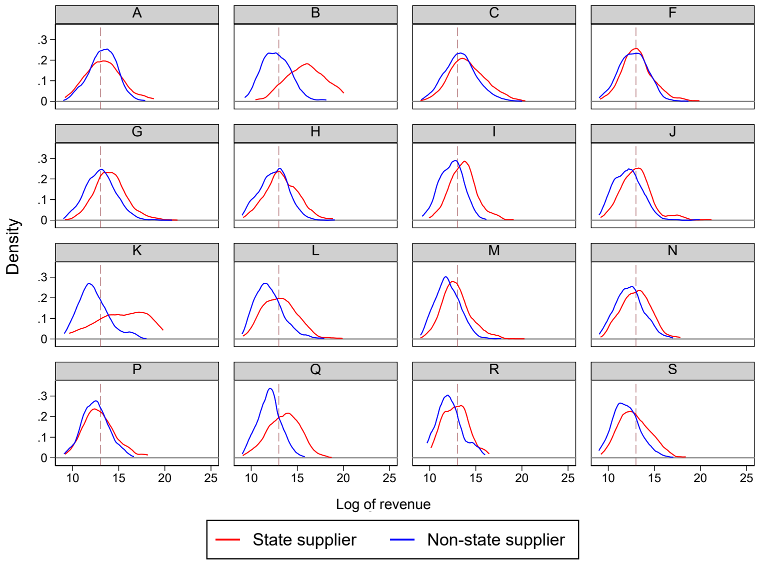



Figure 4: **Size distributions by supplier status in Ecuador** *Note:* Each panel presents kernel density estimates of log revenue for state and nonstate suppliers within 16 ISIC sections. Red lines correspond to state suppliers and blue lines to nonstate suppliers. State suppliers tend to be larger firms within their industries, implying a higher concentration of public procurement among dominant firms.

# 4 Empirical strategy

This section describes the identification strategy used to estimate how the fiscal contraction triggered by the collapse in oil prices propagated through firms and their production networks. We begin by defining direct and indirect exposure to government demand. We then address the endogeneity of selection into public procurement using inverse probability weighting (IPW). Next, we model amplification and spillovers through network linkages. Finally, we use these components to construct network-adjusted fiscal multipliers.

## 4.1 Direct and indirect exposure to public demand

We first identify firms directly connected to the state before the shock. A firm $i$ is a *state supplier* if it sold to a public-sector entity in the baseline year (2014). Let $\mathcal{G}$ denote the set of government entities. We define firm $i$'s direct exposure as

$$e_i^{\rightarrow G} = \frac{\sum_{j \in \mathcal{G}} y_{i,j}}{\sum_j y_{i,j}}, \tag{1}$$

where $y_{i,j}$ denotes sales from firm $i$ to buyer $j$. We define the treatment indicator:

$$S_i^{\to G} = \mathbf{1}[e_i^{\to G} > 0]. \tag{2}$$

Indirect exposure is constructed using 2014 network linkages. Let $\mathcal{S}^{\to G}$ denote the set of state suppliers. We define

$$e_i^{\to S} = \frac{\sum_{j\in\mathcal{S}^{\to G}} y_{i,j}}{\sum_j y_{i,j}}, \qquad F_i^{\to S} = \mathbf{1}[e_i^{\to S} > 0]. \tag{3}$$

We also compute buyer-side exposure to (1) the government ($e_i^{\leftarrow G}$), (2) state suppliers ($e_i^{\leftarrow S}$), and (3) government buyers ($e_i^{\leftarrow B}$). These measures map the five layers of the network observed in the data: direct state suppliers, upstream suppliers to the state, downstream suppliers, state buyers, and downstream of state buyers. All exposure variables are computed in the baseline year to preserve exogeneity.

**Baseline network and endogenous reconfiguration.** All exposure measures are constructed using the production network observed in the baseline year (2014), which we treat as predetermined with respect to the subsequent fiscal contraction. This choice avoids simultaneity and reflection problems that would arise if contemporaneous network links were used. From an economic perspective, a large fiscal shock may induce endogenous reconfiguration of the network through the exit of suppliers, substitution of inputs, or the termination of commercial relationships. Our estimator therefore identifies a reduced-form effect of the procurement contraction conditional on the initial network structure. Any endogenous adjustment of links after 2014 is implicitly incorporated into the estimated responses, rather than separately identified. The estimates should thus be interpreted as the causal effect of the shock given the pre-shock network, not as structural effects holding network connections fixed.

## 4.2 Selection into public procurement: Inverse probability Weighting

Firms do not enter public procurement randomly. Selection depends on observable characteristics such as size, leverage, sector, and market position—all correlated with firms' outcomes.

Moreover, in a networked economy, treatment spillovers may violate the standard Stable Unit Treatment Value Assumption (SUTVA), because potential outcomes may depend on the treatment of neighbors.

To address both issues, we construct weights following Abadie (2005), Hirano et al. (2003), and Imbens and Wooldridge (2009). Let $p(X_i)$ denote the probability of being a state supplier, estimated using baseline covariates $X_i$. The weights are

$$w_i = \begin{cases} 1, & \text{if } S_i^{\to G} = 1, \\ \dfrac{p(X_i)}{1 - p(X_i)}, & \text{if } S_i^{\to G} = 0. \end{cases} \tag{4}$$

Applying these weights in a difference-in-differences (DiD) framework identifies the average treatment effect on the treated (ATT) under conditional parallel trends.

The weighted DiD specification is

$$\Delta y_{it} = \alpha_t + \beta_t S_i^{\to G} + \varepsilon_{it}, \tag{5}$$

where $\Delta y_{it}$ is the cumulative log-change of outcome $y$ between the baseline and year $t$.

**Identification diagnostics.** The credibility of the IPW-DiD design rests on two testable implications: (i) that inverse probability weighting achieves balance in pretreatment covariates between treated and control firms, and (ii) that treated and control firms display parallel trends prior to the 2014 shock. We explicitly verify both conditions. First, we assess covariate balance by comparing standardized mean differences before and after weighting; IPW substantially reduces initial imbalances in firm size, investment intensity, leverage, and sectoral composition, bringing all covariates within conventional balance thresholds (Appendix C, Figure C4). Second, we conduct placebo tests using pseudo-treatment years in the pretreatment period. Re-estimating the IPW-DiD with placebo shocks in 2012 and 2013 yields no significant effects across all outcomes, supporting the conditional parallel-trends assumption (Appendix C, Table C1).

## 4.3 Estimating the propensity score

Propensity-score estimation plays a central role for two reasons. First, it corrects the selection bias arising from firms' self-selection into public procurement. Second, it helps isolate the effects of the fiscal demand shock from concurrent shocks, particularly the recession triggered by the oil-price collapse and the safeguard tariffs introduced between 2015 and 2017. We estimate

$$p(X_i) = \Pr(S_i^{\to G} = 1 \mid X_i) = \text{logit}^{-1}(\beta_0 + X_i\beta') \,. \tag{6}$$

**Confounders related to public demand linkages.** To account for potential SUTVA violations through unmodeled links to the public sector, we include baseline measures of public-demand exposure: purchases from government entities, state suppliers, and buyers connected to the state.

**Confounders related to the macroeconomic recession.** The oil-price collapse generated a nationwide downturn. Firms' ability to absorb this shock varied with size, leverage, investment, diversification, and export orientation. We therefore include baseline firm-level controls: log workers, investment, liabilities, export share, and the revenue Herfindahl index.

**Alternative macro-financial channels.** The collapse in international oil prices may affect firms through channels other than public procurement, including tighter financial conditions, higher sovereign risk premia, and reduced access to credit. In Ecuador, the oil-price shock was accompanied by a sharp increase in the EMBI spread and a generalized deterioration of financing conditions. These macro-financial channels could, in principle, depress investment and employment even among firms with no direct exposure to the state. Our identification strategy relies on the assumption that, conditional on baseline firm characteristics, sector, and network position, these aggregate shocks affect treated and control firms symmetrically. We address this concern in three ways. First, the IPW design balances treated and control firms on size, leverage, investment, export orientation, and sectoral composition, which are key determinants of financial vulnerability. Second, all specifications include year fixed effects that absorb aggregate macro-financial shocks common to all firms. Third, by comparing firms differentially exposed to public demand within the same macroeconomic environment, the DiD design differences out economy-wide financial tightening. Under these conditions, remaining differences can be interpreted as arising from procurement exposure and network propagation rather than from alternative macro channels.

**Confounders from trade policy.** Safeguard tariffs (2015–2017) affected importers and propagated through the value chain (Uribe-Terán et al., 2025a). These shocks were stronger for smaller firms (Uribe-Terán et al., 2025b). If firms more exposed to international markets are also more involved in public procurement, estimates could confound fiscal and trade shocks.

We control for this channel using industry-level safeguard exposure (output and input exposure), constructed by mapping the full tariff schedule to prepolicy import patterns in 2014. These measures are predetermined and exogenous.

## 4.4 Amplification through network interactions

Network externalities may amplify the effects of the fiscal contraction if suppliers to the state also transact with other state suppliers. To capture this channel, we extend the baseline DiD to allow treatment intensity to vary with second-order exposure:

$$\Delta y_{it} = \alpha_t + \beta_t S_i^{\to G} + \gamma_t S_i^{\to G} \cdot e_i^{\to S} + \varepsilon_{it}. \tag{7}$$

The interaction term identifies whether firms embedded in clusters of state suppliers experience larger contractions. A positive $\gamma_t$ would indicate reinforcement through network linkages.

## 4.5 Spillovers to indirect suppliers

**Interference and exposure mapping.** Our identification strategy adopts a partial-interference framework in which firms' potential outcomes are allowed to depend on a limited set of observed exposure measures derived from the baseline production network. Specifically, we allow outcomes to depend on (i) direct exposure to public demand and (ii) second-order exposure to state suppliers, as captured by $S_i^{\to G}$, $e_i^{\to S}$, and $F_i^{\to S}$. Following the exposure-mapping approach in Hudgens and Halloran (2008a); Manski (2013), and Aronow and Samii (2017a), we assume that, conditional on these first- and second-order exposure variables, there are no additional relevant channels of interference operating through more distant network paths. In other words, once direct and second-order connections to the state are accounted for, higher-order network links do not generate further spillovers that systematically affect firms' outcomes.

Firms that do not supply the state directly may still be affected if they sell to state suppliers. To isolate this channel, we restrict the sample to firms with $S_i^{\to G} = 0$ and estimate

$$\Delta y_{it} = \alpha_t + \theta_t F_i^{\to S} + \varepsilon_{it}, \tag{8}$$

using inverse probability weighting with propensity scores recalculated for this restricted sample. The coefficient $\theta_t$ captures second-order spillovers, effects transmitted through private-sector intermediaries rather than through direct procurement links.

This specification aligns with recent approaches to partial interference and exposure mapping (Hudgens and Halloran, 2008b; Manski, 2013; Aronow and Samii, 2017b). Potential outcomes are allowed to depend on firms' direct exposure and their second-order exposure, while ruling out unobserved higher-order interference pathways beyond the observed network structure.

### 4.6 Network-adjusted fiscal multipliers

The oil-price collapse produced a strong, aggregate contraction in public procurement. This variation allows us to estimate fiscal multipliers at the firm level. Let $\Delta G_t$ denote the change in aggregate public procurement relative to 2014. We define

$$\omega_i^{\to G} = \frac{g_i}{g}, \quad z_{it}^{\to G} = \omega_i^{\to G} \Delta G_t,$$

where $g_i$ is firm $i$'s sales to the state and $g$ is total baseline procurement. Because $\omega_i^{\to G}$ is predetermined and $\Delta G_t$ is an aggregate shock, $z_{it}^{\to G}$ is exogenous.

Similarly, we define $\omega_i^{\to S}$ as the baseline share of private-sector sales made to state suppliers.

We estimate

$$\Delta VA_{it} = \lambda_t + \mu^d z_{it}^{\to G} + \mu^a z_{it}^{\to G} e_i^{\to S} + \mu^s (\Delta G_t) \omega_i^{\to S} + X_i \delta' + \varepsilon_{it}. \tag{9}$$

Because $\sum_i z_{it}^{\to G} = \sum_i (\Delta G_t)\omega_i^{\to S} = \Delta G_t$, each coefficient in the regression has a direct interpretation as a fiscal multiplier. The parameter $\mu^d$ captures the direct effect of the fiscal contraction on state suppliers. The parameter $\mu^a$ measures the amplification of this direct effect through second-order exposure to other state suppliers. The parameter $\mu^s$ captures the spillover effect on firms that do not sell to the state but do sell to state suppliers.

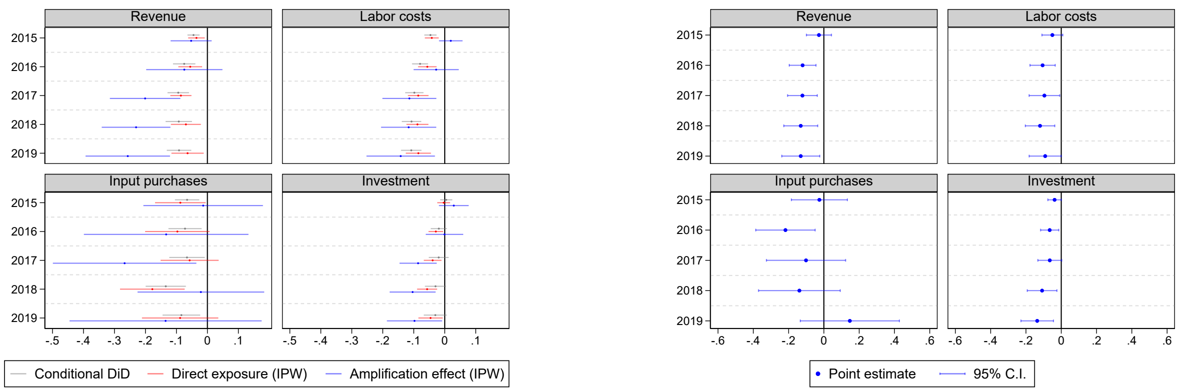
The network-adjusted fiscal multiplier is therefore

$$\mu = \mu^d + \mu^a \cdot \overline{e}^{\to S} + \mu^s, \tag{10}$$

where $\overline{e}^{\to S} = 1$ represents the maximum level of network exposure. Setting $\mu^a = \mu^s = 0$ provides a counterfactual multiplier that disregards all network propagation.

# 5 Results

We estimate average treatment effects on treated firms (ATT) using inverse probability weighting (IPW) in the DiD framework described in section 4. The outcomes follow a production-function logic: revenue, labor costs, input purchases, and investment.




(a) Direct and amplification effects

(b) Spillovers effects

Figure 5: **Main results and propagation** *Note:* The table presents the coefficients from IPW–DiD regressions of log changes relative to 2014. Panel a reports ATT estimates for state suppliers and amplification effects by exposure to other state suppliers ($e^{\to S} \in [0, 1]$). Panel b shows spillovers on firms selling to state suppliers. Horizontal bars denote 95 percent confidence intervals.

**Direct effects on state suppliers.** Panel a of figure 5 reports the estimated direct and amplification effects for state suppliers. Revenue exhibits large and persistent contractions over 2015–2019, with growth losses ranging from -3.5 percent in 2015 ($p = 0.010$) to -8.6 percent in 2017 ($p < 0.001$). These declines capture both the immediate reduction in state demand and the erosion of state-linked client networks.

Labor costs fall even more sharply and remain depressed throughout the period. Estimated effects are -4.2 percent in 2015 ($p < 0.001$), deepening to roughly -8.6 percent from 2017 onward. The pattern indicates that firms first adjust flexible labor margins—mainly temporary or noncore contracts—while retaining permanent employees. This strategy is consistent with Ecuador's rigid employment regulations and precautionary labor hoarding.

Investment declines with a lag. Effects become significant from 2016, reaching -5.7 percent in 2018 ($p < 0.001$) and -4.6 percent in 2019 ($p = 0.022$). Firms likely delayed investment adjustments to assess whether the fiscal retrenchment would persist. Once it became clear that public spending cuts were structural, they reduced or postponed investment expenditures. This slow response matches the higher irreversibility of investment relative to labor.

Input purchases, by contrast, display little systematic adjustment. Apart from declines in 2015 (-8.7 percent, $p = 0.036$) and 2018 (-17.8 percent, $p < 0.001$), coefficients are small and statistically insignificant. This suggests that state suppliers maintained material spending to complete ongoing contracts or fulfill minimum delivery requirements despite scaling back labor and investment.

**Amplification within clusters of state suppliers.** The interaction estimates highlight strong propagation within clusters of state-connected firms. The coefficients represent the change in outcomes when baseline exposure to other state suppliers increases from zero to one—that is, when a firm moves from complete isolation to full embeddedness within the procurement network.

For revenue, the amplification effect grows from -20.1 percent in 2017 to -25.7 percent in 2019 ($p < 0.001$). Labor costs show similar patterns: -11.4 percent (2017), -11.6 percent (2018), and -14.2 percent (2019), all significant at the 1 percent level. Investment also exhibits significant amplification: -8.6 percent (2017), -10.4 percent (2018), and -9.8 percent (2019).

These results confirm that exposure to other state suppliers amplifies the contraction, with firms embedded in dense procurement clusters facing larger and more-persistent declines. No robust amplification arises for input purchases, consistent with the rigidity of this input category.

**Spillovers to upstream nonstate suppliers.** Panel b in figure 5 shows the second-order (spillover) effects on firms supplying state suppliers. The propagation pattern is clear. Upstream firms experience significant revenue losses beginning in 2016 (-2.9 percent, $p = 0.048$), which deepen through 2019 (-7.3 percent, $p < 0.001$). Labor costs follow a similar trajectory, declining from -2.2 percent in 2016 ($p = 0.035$) to about -6.1 percent in 2019 ($p < 0.001$). Investment also contracts persistently, with effects ranging between -1.8 and -4.5 percent over 2016–2019.

Input purchases again stand out: although overall spending by state suppliers remains broadly stable, their upstream partners experience sustained revenue contractions. This divergence suggests that, while total input purchases remain steady, demand shifts away from specific suppliers–likely those producing specialized or higher-value intermediate goods–toward more-generic inputs or service providers outside our supplier set. The fiscal contraction thus propagates through network reallocation rather than aggregate input collapse.

**Discussion.** Two empirical facts stand out. First, directly exposed state suppliers cut labor and investment sharply, while maintaining near-constant material purchases. Second, upstream nonstate suppliers nonetheless suffer persistent revenue losses. Together, these patterns point to reallocation and propagation mechanisms beyond simple demand contraction.

*Rebalancing across input categories.* Facing fiscal retrenchment, state suppliers prioritize the completion of ongoing contracts, preserving material inputs needed to meet procurement obligations while reducing flexible expenditures such as temporary labor and investment. Moreover, material inputs may include both generic and specialized goods. Firms might substitute away from specialized intermediates—often produced by smaller nonstate suppliers—toward cheaper or standardized materials. As a result, total material spending remains stable, but composition changes, reducing demand for upstream firms in specialized segments.

*Extensive versus intensive margin adjustments.* Public procurement networks feature long-term supplier relationships but limited flexibility in contract design. When budgets tighten, state

| | Estimated multipliers | | |
|---|---|---|---|
| | **Coefficient** | **Std. error** | **Share of total (%)** |
| *Panel A: Aggregate multipliers* | | | |
| Network-adj. multiplier | $1.951^{**}$ | (0.298) | 100.0 |
| Simple multiplier | $1.188^{**}$ | (0.269) | 60.9 |
| Network uplift | 0.828 | – | 42.4 |
| *Panel B: Network-adjusted components* | | | |
| Direct multiplier | 1.123 | (0.586) | 57.6 |
| Amplification multiplier | 0.095 | (0.509) | 4.9 |
| Spillover multiplier | $0.733^{**}$ | (0.163) | 37.6 |

Table 2: **Network-adjusted multipliers** *Note:* Panel A compares the aggregate multiplier with and without network propagation and panel B decomposes the network-adjusted multiplier into direct, amplification, and spillover effects; reported shares indicate each component's contribution to the total response.

suppliers may drop some upstream providers altogether (extensive margin) while increasing order size with a few incumbents (intensive margin). If the survivors fall outside the nonstate supplier sample (or represent only a small share) the aggregate material cost of state suppliers appears unchanged even though many upstream firms lose clients entirely. This mechanism explains why upstream revenue contracts despite flat material expenditures downstream.

*Network propagation.* Amplification results show that the contraction deepens for firms embedded in densely connected procurement clusters. Direct exposure to public demand thus sets off a cascade of second-order effects: suppliers to state firms reduce their own orders, transmitting the shock upstream and across network layers. These reinforcing linkages transform a targeted fiscal contraction into a broader network adjustment.

**Network-adjusted multipliers.** Table 2 reports the estimated fiscal multipliers once we account for firms' network positions. The network-adjusted multiplier is 1.95, while the simple multiplier that ignores production links is 1.19. The difference, a network uplift of 0.83, shows that standard estimates capture only 60.9 percent of firms' total output response, with the remaining 42.4 percent reflecting propagation through network connections.

Decomposing the network-adjusted multiplier shows that the direct effect of fiscal spending on firms is 1.12, accounting for 57.6 percent of the total. This direct channel explains most of the aggregate elasticity.

Amplification through purchases among state suppliers is small. When exposure to other state suppliers increases from 0 to 1, the implied amplification multiplier is 0.10, or 4.9 percent of the total. This indicates that interactions among state suppliers contribute little beyond the direct link between firms and the government.

Spillovers to firms upstream of state suppliers are more important. The spillover multiplier equals 0.73 and represents 37.6 percent of the total network-adjusted response. Most of the network uplift therefore arises from the transmission of the fiscal shock to connected firms through upstream production links.

# 6 Robustness

This section assesses the robustness of our baseline inverse probability-weighted difference-in-differences (IPW-DiD) estimates. We examine concerns related to model misspecification, extreme weights, the role of network exposure in estimating propensity scores, and the potential endogeneity of time-varying exposure to public demand. Appendix C presents additional tests addressing violations of conditional parallel trends, sensitivity to the threshold defining state suppliers, and postweighting balance diagnostics.

**Attrition and sample selection.** The severity and persistence of the fiscal contraction raise the possibility that some firms exited the market after 2014. If exit were systematically correlated with procurement exposure, attrition could in principle, affect the composition of the estimation sample. In our setting, firm exit is largely driven by observable characteristics such as size, leverage, sector, and pre-shock performance, all of which enter the propensity score and are balanced by inverse probability weighting. The IPW-DiD design therefore, identifies the average treatment effect on surviving firms, conditional on observables. To the extent that attrition is a function of these predetermined characteristics, it does not bias the estimated ATT. We view the estimated effects as describing the causal impact of the fiscal contraction on continuing firms, which is the relevant population for assessing propagation through production networks.

## 6.1 Model misspecification and doubly robust estimation

Although IPW balances treated and control firms in expectation, the method can be sensitive to misspecification of the propensity score model and to extreme weights. To address these issues we implement the doubly robust DiD estimator of Callaway and Sant'Anna (2021). Because all treated firms become state suppliers in the same year, our design simplifies to a single treatment cohort with multiple postperiods. In this case, the estimator reduces to the panel-data doubly robust DiD of Sant'Anna and Zhao (2020), applied separately for each posttreatment year.

For any postperiod $t > 0$, we define the outcome change $\Delta Y_{it} = Y_{it} - Y_{i0}$ and let $D_i = S_i^{\to G}$ denote the treatment indicator. Let $\hat{p}(X_i)$ be the estimated propensity score and let $\hat{m}_{0,\Delta,t}(X_i)$ denote the fitted conditional mean of $\Delta Y_{it}$ for control firms. The doubly robust estimator of the ATT is

$$\widehat{ATT}_t^{DR} = \frac{1}{n} \sum_{i=1}^{n} (\hat{w}_{1i} - \hat{w}_{0i}) \left(\Delta Y_{it} - \hat{m}_{0,\Delta,t}(X_i)\right), \tag{11}$$

where

$$\hat{w}_{1i} = \frac{D_i}{\widehat{E}_n[D_i]}, \qquad \hat{w}_{0i} = \frac{\frac{\hat{p}(X_i)(1-D_i)}{1-\hat{p}(X_i)}}{\widehat{E}_n\left[\frac{\hat{p}(X)(1-D)}{1-\hat{p}(X)}\right]}, \tag{12}$$

and $\widehat{E}_n[\cdot]$ denotes the sample average. The regression $\hat{m}_{0,\Delta,t}(X_i)$ is estimated using control firms only. The estimator is consistent if either the propensity score model or the outcome regression is correctly specified.

## 6.2 Sensitivity to extreme weights: Kernel-based matching

If overlap between treated and untreated firms is limited, IPW may generate excessively large weights for a small number of controls. To evaluate whether such weights influence our conclusions, we reestimate treatment effects using propensity score matching with kernel weights. Each treated firm is matched to a weighted average of nontreated firms, with weights decreasing in the distance between estimated propensity scores. We use the Epanechnikov kernel and a bandwidth chosen to balance bias and variance.

We then estimate

$$\Delta y_{it} = \alpha_t + \beta_t S_i^{\to G} + \varepsilon_{it} \tag{13}$$

on the matched sample. Because kernel matching constructs a control group in a fundamentally different way than IPW, the stability of $\beta_t$ across both estimators suggests that our results are not driven by extreme weights or limited support overlap.

## 6.3 The role of network exposure in treatment assignment

Identification under network interference requires that interference occurs only through observed exposure variables and that, conditional on these variables, no further interference remains. To assess the extent to which network exposure variables matter empirically, we reestimate the IPW-DiD excluding all network exposure measures from the propensity score model. If network-adjusted exposure captures the relevant channels of interference, removing these variables should affect the balance or modify the estimated treatment effects.

## 6.4 Instrumental variables approach with shift-share design

As an additional robustness exercise, we implement an instrumental variables (IV) strategy using a shift-share design. We redefine treatment as the contemporaneous share of firm $i$'s sales directed to the state, denoted as $e_{it}^{\to G}$. Because this quantity may respond to unobserved shocks or strategic adjustments, we estimate

$$\Delta y_{it} = \alpha_t + \beta_t e_{it}^{\to G} + \theta_t' X_i + \varepsilon_{it}, \tag{14}$$

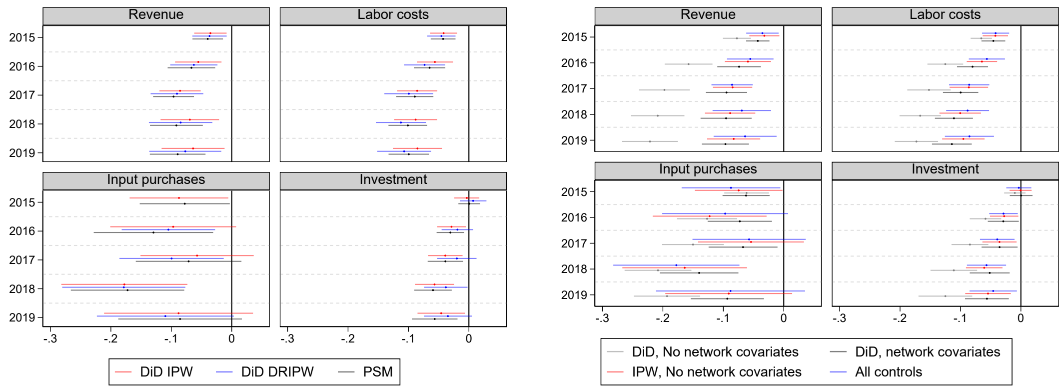
where $X_i$ contains predetermined covariates. We address the potential endogeneity of $e_{it}^{\to G}$ by constructing the shift-share instrument

$$z_{it} = e_i^{\to G} \cdot \Delta \log p_t, \tag{15}$$

where $e_i^{\to G}$ is baseline exposure in 2014 and $\Delta \log p_t$ is the change in the international price of oil. The first term captures the firm-specific share, while the second term represents the aggregate fiscal shock. Variation in $p_t$ affects firm-level demand from the state only through public expenditure, ensuring both relevance and exogeneity. Ecuador's negligible role in global oil supply reinforces the exogeneity of the shift.

The coefficient $\beta_t$ captures the effect of marginal changes in public-sector exposure on firm performance. For comparability with the binary ATT estimates, we scale the IV coefficient using the average baseline exposure among treated firms. The similarity of the magnitudes across both approaches supports the view that our findings are not driven by endogenous time variation in exposure.

Taken together, the doubly robust estimator, kernel-based matching, the exclusion of network exposure from the propensity score, and the shift-share IV design confirm that our results are not sensitive to functional form assumptions, extreme weights, or alternative sources of variation in public demand. The fiscal contraction consistently produced substantial declines in firm performance across all robustness checks.




(a) Alternative DiD estimators

(b) Accounting for network exposure

Figure 6: **Robustness of ATT estimates** *Note:* Panel a compares the benchmark IPW-DiD estimator with alternative DiD specifications and panel b illustrates how controlling for firms' network exposure mitigates bias. All results confirm that estimated treatment effects are robust to model specification and weighting methods. Points denote coefficient estimates, and horizontal bars show 95 percent confidence intervals. Standard errors are clustered at the three-digit ISIC industry level.

**Results.** Panel a of figure 6 shows that ATT estimates are stable across alternative DiD specifications. This suggests that our findings are not driven by specific identifying assumptions. Panel b highlights the importance of accounting for firms' network positions: controlling for network exposure substantially reduces the bias present in the naïve DiD estimates, while further adjustments through IPW yield only marginal improvements.

The IV results in table 3 confirm the direction and significance of our baseline findings. Firms

| | Change in outcomes relative to 2014 | | | | |
|---|---|---|---|---|---|
| | **2015** | **2016** | **2017** | **2018** | **2019** |
| *Panel A: Revenue* | | | | | |
| Marginal effect | −0.237 ** | −0.423 ** | −0.573 ** | −0.703 ** | −0.852 ** |
| | (0.045) | (0.081) | (0.101) | (0.127) | (0.150) |
| *Panel B: Labor costs* | | | | | |
| Marginal effect | −0.074 * | −0.220 ** | −0.309 ** | −0.358 ** | −0.478 ** |
| | (0.034) | (0.053) | (0.068) | (0.091) | (0.106) |
| *Panel C: Input purchases* | | | | | |
| Marginal effect | −0.576 ** | −0.718 ** | −0.696 ** | −1.156 ** | −1.146 ** |
| | (0.099) | (0.141) | (0.211) | (0.214) | (0.239) |
| *Panel D: Investment* | | | | | |
| Marginal effect | −0.035 | −0.098 * | −0.168 ** | −0.127 | −0.214 * |
| | (0.031) | (0.049) | (0.063) | (0.079) | (0.088) |
| First Stage | −1.168 ** | −0.865 ** | −0.965 ** | −1.455 ** | −0.998 ** |
| | (0.021) | (0.023) | (0.037) | (0.064) | (0.052) |
| F-stat | 394.6 | 199.4 | 102.4 | 76.1 | 56.8 |

Table 3: **Instrumental variables estimates by year** *Note:* Each coefficient corresponds to the marginal effect from the IV regression in Equation (14). Robust standard errors are in parentheses. First-stage coefficients and $F$-statistics are also reported. Stars indicate significance levels: $^{*}p < 0.05$, $^{**}p < 0.01$.

more exposed to public demand experience larger declines in revenue, costs, and investment following the fiscal contraction. While magnitudes differ due to differences in identification (ATT vs. LATE), both sets of estimates are consistent in sign and economic interpretation. The DiD results are, if anything, more conservative, reinforcing the robustness of our conclusions.

# 7 Discussion and conclusions

The evidence supports a propagation mechanism with three layers: (1) state suppliers absorb the initial demand shock by cutting labor and postponing investment while maintaining core input purchases, (2) nonstate upstream suppliers experience reduced orders as buyers reallocate toward generic inputs or fewer vendors, and (3) network clustering magnifies these effects, turning localized procurement cuts into economy-wide slowdowns. This interpretation aligns with recent evidence on fiscal multipliers in production networks, emphasizing the importance of supplier concentration and input substitutability in shaping transmission dynamics.

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# A Additional descriptive statistics

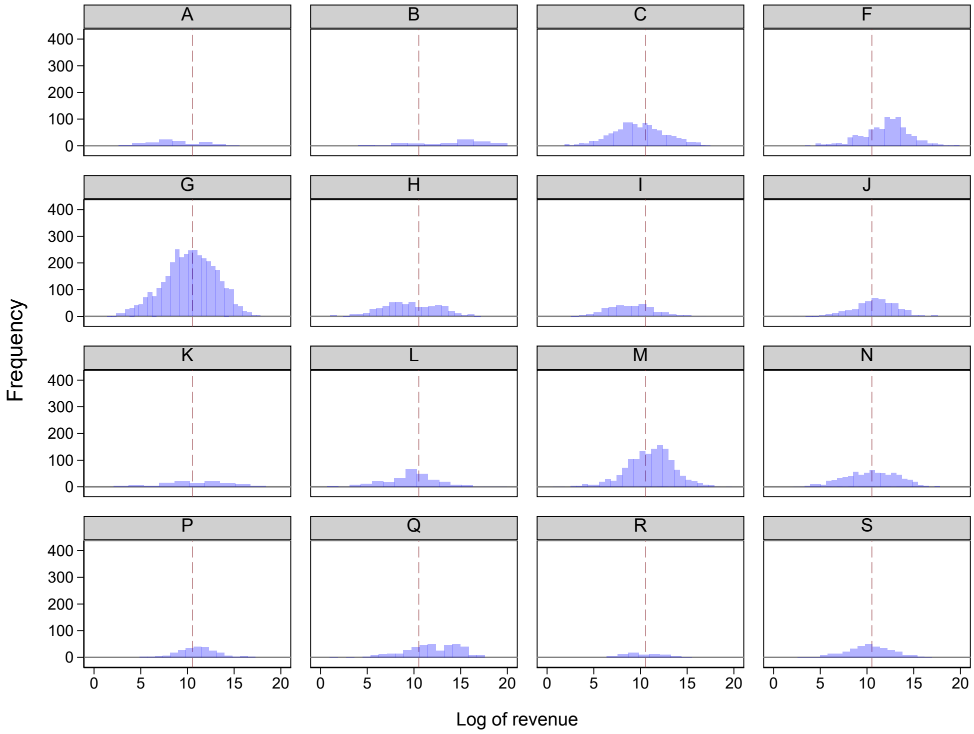


Figure A1: **Histogram of firms' revenue from the Ecuadorian state, 2014** *Note:* The figure displays the distribution of public procurement revenues and the number of firms engaged in procurement by ISIC section. It illustrates that participation in state demand is widespread across sectors but concentrated in a limited set of industries.

Figure A1 reports the number of state suppliers by ISIC section in the baseline year (2014). State suppliers operate in all major sections, with prevalence varying across sectors. The largest counts are in Wholesale and Retail (G), Professional, Scientific and Technical Activities (M), Manufacturing (C), and Construction (F). In contrast, the largest firms by sales are concentrated in Mining and Quarrying (B), Financial and Insurance Activities (K), and Human Health and Social Work Activities (Q), consistent with the upper-tail patterns in figure 4.

These sectoral patterns are informative for identification. The presence of suppliers and non-suppliers within most sections supports overlap for inverse probability weighting. Conditional on sector and baseline firm characteristics (size, leverage, investment, export orientation, and network controls), treated and untreated firms share common support, allowing IPW to reweight nonsuppliers to match suppliers in the weighted DiD design.

# B Additional results

In this section, we present a set of complementary results. We first examine the effects of the fiscal contraction on formal employment among state suppliers. Although the lack of valid placebos limits causal interpretation, the estimates remain informative about the adjustment margin. We then document heterogeneity in the direct effects by firm size and economic sector. Finally, we report the full set of coefficient estimates underlying the main results in the paper.

## B.1 Effects on formal labor of state suppliers

Table B1 shows no significant changes in payrolls, indicating that firms adjusted mainly through temporary labor. Faced with falling demand, state suppliers appear to have cut flexible jobs while retaining core staff, consistent with labor hoarding and limited employment flexibility under Ecuador's rigid regulations.

| | **Change between $t$ and 2014** | | | | |
|---|---|---|---|---|---|
| | **2015** | **2016** | **2017** | **2018** | **2019** |
| *Panel A: Direct effect* | | | | | |
| Marginal effect | 0.007 | 0.004 | −0.025 | 0.009 | −0.014 |
| | (0.014) | (0.020) | (0.017) | (0.021) | (0.025) |
| *Panel B: Amplification effect* | | | | | |
| Marginal effect | −0.029 | 0.037 | −0.050 | −0.058 | −0.050 |
| | (0.025) | (0.030) | (0.043) | (0.051) | (0.054) |
| *Panel C: Second-order effect* | | | | | |
| Marginal effect | −0.033 | −0.033 | −0.018 | −0.009 | −0.005 |
| | (0.026) | (0.034) | (0.034) | (0.038) | (0.038) |

Table B1: **IPW-DiD estimates on formal employment** *Note:* Each entry is the year-specific ATT from IPW-DiD regressions. Robust standard errors in parentheses. Stars indicate significance levels: $^{*}p < 0.05$, $^{**}p < 0.01$.

## B.2 Heterogeneous first-order effects

This appendix examines whether the direct effects of the fiscal contraction differ across firm characteristics. We study heterogeneity by firm size (figure B2) and by economic sector (figure B3), focusing on revenue, labor costs, material costs, and investment.

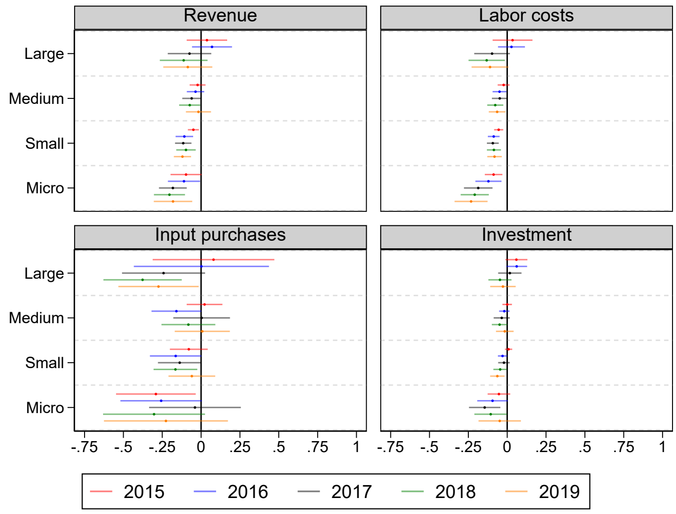


Figure B2: **Effects by firm size** *Note:* Direct effects for state suppliers by CAN size category. Points denote coefficient estimates and horizontal bars show 95 percent confidence intervals. IPW estimates with trimmed scores; 2014 is the baseline year.

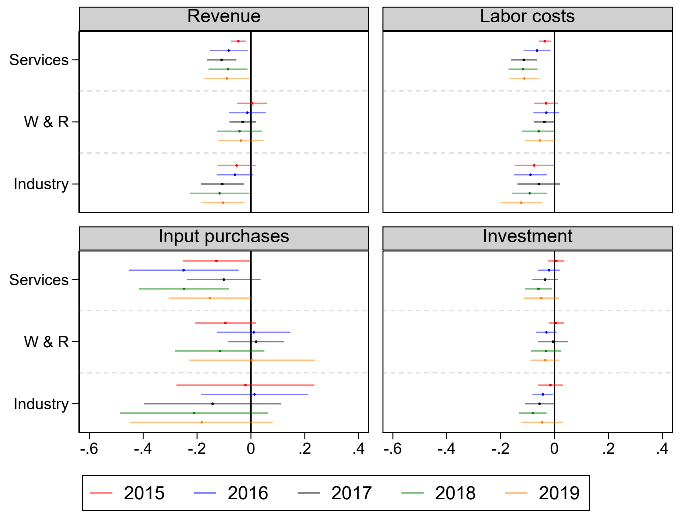


Figure B3: **Effects by economic sector** *Note:* Direct effects for state suppliers by ISIC sector. Points denote coefficient estimates and horizontal bars show 95 percent confidence intervals. IPW estimates with trimmed scores; 2014 is the baseline year.

**Firm size.** The contraction has larger effects for micro and small firms relative to medium and large firms. These firms exhibit persistent reductions in revenue and labor costs, consistent with higher liquidity risk and more limited access to credit. Contractions in material costs and investment indicate that smaller firms adjust across multiple margins when public demand falls. Effects for medium and large firms are limited and concentrated in specific years.

**Economic sector.** Sectoral differences are present but not systematic. Reductions in revenue and labor costs occur across primary, industrial, and service sectors, with the primary sector exhibiting the greatest volatility. Contractions in material costs are sizable but heterogeneous across sectors, with effects concentrated in the primary sector. Declines in investment are observed mainly in the industrial sector, consistent with its higher capital intensity and greater reliance on external financing.

Size-based heterogeneity aligns more closely with the mechanisms highlighted in the main text: firms with weaker financial buffers are consistently more exposed to a fiscal demand contraction. Sectoral differences, while informative, display more variation across outcomes and years.

## B.3 Main effects coefficients

Table B2 presents the exact coefficients corresponding to the results in panel a of figure 5 in the main text.

| | Change in outcomes between $t$ and 2014 | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **Direct effect** | | | | | **Amplification effect** | | | | | **Second-order effect** | | | | |
| | **2015** | **2016** | **2017** | **2018** | **2019** | **2015** | **2016** | **2017** | **2018** | **2019** | **2015** | **2016** | **2017** | **2018** | **2019** |
| *Panel A: Revenue* | | | | | | | | | | | | | | | |
| Marginal effect | $-0.035^{**}$ | $-0.055^{**}$ | $-0.086^{**}$ | $-0.069^{**}$ | $-0.064^{*}$ | $-0.052$ | $-0.075$ | $-0.201^{**}$ | $-0.230^{**}$ | $-0.257^{**}$ | $-0.028$ | $-0.121^{**}$ | $-0.122^{**}$ | $-0.131^{**}$ | $-0.131^{*}$ |
| | (0.014) | (0.020) | (0.017) | (0.025) | (0.026) | (0.034) | (0.062) | (0.058) | (0.056) | (0.069) | (0.036) | (0.038) | (0.043) | (0.049) | (0.055) |
| Mean | −0.068 | −0.202 | −0.147 | −0.117 | −0.135 | −0.068 | −0.202 | −0.147 | −0.117 | −0.135 | −0.068 | −0.202 | −0.147 | −0.117 | −0.135 |
| Observations | 16077 | 14717 | 13626 | 12791 | 12101 | 16077 | 14717 | 13626 | 12791 | 12101 | 16077 | 14717 | 13626 | 12791 | 12101 |
| *Panel B: Labor costs* | | | | | | | | | | | | | | | |
| Marginal effect | $-0.042^{**}$ | $-0.056^{**}$ | $-0.086^{**}$ | $-0.088^{**}$ | $-0.085^{**}$ | $0.019$ | $-0.028$ | $-0.114^{*}$ | $-0.116^{*}$ | $-0.142^{*}$ | $-0.050$ | $-0.106^{**}$ | $-0.095^{*}$ | $-0.120^{**}$ | $-0.091^{*}$ |
| | (0.011) | (0.015) | (0.017) | (0.018) | (0.021) | (0.019) | (0.037) | (0.044) | (0.045) | (0.056) | (0.030) | (0.036) | (0.044) | (0.042) | (0.046) |
| Mean | 0.059 | −0.009 | 0.001 | 0.042 | 0.040 | 0.059 | −0.009 | 0.001 | 0.042 | 0.040 | 0.059 | −0.009 | 0.001 | 0.042 | 0.040 |
| Observations | 16077 | 14717 | 13626 | 12791 | 12101 | 16077 | 14717 | 13626 | 12791 | 12101 | 16077 | 14717 | 13626 | 12791 | 12101 |
| *Panel C: Input purchases* | | | | | | | | | | | | | | | |
| Marginal effect | $-0.087^{*}$ | $-0.097$ | $-0.057$ | $-0.178^{**}$ | $-0.088$ | $-0.014$ | $-0.133$ | $-0.267^{*}$ | $-0.021$ | $-0.135$ | $-0.026$ | $-0.218^{*}$ | $-0.101$ | $-0.139$ | $0.147$ |
| | (0.041) | (0.053) | (0.048) | (0.053) | (0.063) | (0.098) | (0.135) | (0.117) | (0.104) | (0.157) | (0.081) | (0.085) | (0.114) | (0.117) | (0.142) |
| Mean | −0.681 | −0.901 | −0.913 | −0.887 | −0.966 | −0.681 | −0.901 | −0.913 | −0.887 | −0.966 | −0.681 | −0.901 | −0.913 | −0.887 | −0.966 |
| Observations | 16077 | 14717 | 13626 | 12791 | 12101 | 16077 | 14717 | 13626 | 12791 | 12101 | 16077 | 14717 | 13626 | 12791 | 12101 |
| *Panel D: Investment* | | | | | | | | | | | | | | | |
| Marginal effect | $-0.003$ | $-0.029^{*}$ | $-0.039^{**}$ | $-0.057^{**}$ | $-0.046^{*}$ | $0.029$ | $-0.001$ | $-0.086^{**}$ | $-0.104^{**}$ | $-0.098^{*}$ | $-0.038$ | $-0.065^{*}$ | $-0.065$ | $-0.109^{*}$ | $-0.136^{**}$ |
| | (0.011) | (0.012) | (0.014) | (0.016) | (0.020) | (0.024) | (0.031) | (0.030) | (0.038) | (0.045) | (0.019) | (0.026) | (0.034) | (0.043) | (0.047) |
| Mean | −0.023 | −0.017 | 0.056 | 0.140 | 0.184 | −0.023 | −0.017 | 0.056 | 0.140 | 0.184 | −0.023 | −0.017 | 0.056 | 0.140 | 0.184 |
| Observations | 16077 | 14717 | 13626 | 12791 | 12101 | 16077 | 14717 | 13626 | 12791 | 12101 | 16077 | 14717 | 13626 | 12791 | 12101 |

Table B2: **Direct, amplification, and second-order effects by year and outcome** *Note:* Each cell reports the marginal effect from separate regressions, with heteroskedasticity-robust standard errors in parentheses. Stars indicate significance levels: $^{*}p < 0.05$, $^{**}p < 0.01$.

# C Additional robustness checks

This appendix presents three additional tests that reinforce the credibility of our baseline results: (1) covariate balance after applying inverse probability weights (IPW), (2) placebo tests for pretrend validation, and (3) robustness to alternative definitions of treatment intensity using an instrumental variables (IV) approach.

**Balance achieved by IPW.** The validity of IPW-DiD estimates depends on achieving balance in pretreatment characteristics between treated and untreated firms. To verify this, we test for covariate balance by comparing weighted means across groups before treatment. Figure C4 displays the standardized differences in means before and after weighting.

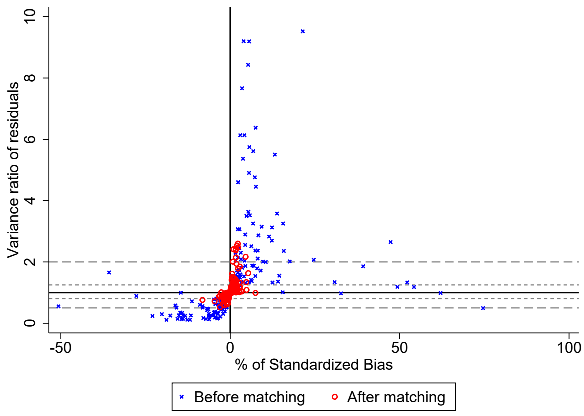


Figure C4: **Covariate balance before and after IPW** *Note:* The figure shows standardized mean differences between treated and control firms for all pretreatment covariates before and after weighting. The dashed lines mark the conventional balance threshold ($|SD| < 0.1$). IPW considerably reduces initial imbalances in both means and variances, indicating that treatment and control groups are well balanced.

The results confirm that weighting achieves substantial covariate balance. Prior to IPW, differences across groups were sizable, especially for firm size, investment intensity, and sectoral composition. After weighting, standardized mean differences shrink well within conventional limits, supporting the validity of the conditional parallel-trends assumption in the weighted sample.

**Parallel trends in IPW-DiD.** We next assess whether our estimates could be driven by preexisting trends or spurious correlations. To do so, we perform placebo tests using pseudo-treatment years prior to 2014, when no fiscal contraction occurred. We reestimate our IPW-DiD specification using these placebo treatments. If identification is valid, we should observe no significant effects in these preshock years.

As expected, all placebo coefficients are small and statistically insignificant across outcome

| | Change in outcomes relative to 2014 | | | |
|---|---|---|---|---|
| | **Direct effect** | | **Second-order effect** | |
| | 2012 | 2013 | 2012 | 2013 |
| *Panel A: Revenue* | | | | |
| Marginal effect | −0.004 | 0.017 | 0.070 | 0.049 |
| | (0.015) | (0.011) | (0.050) | (0.026) |
| *Panel B: Labor costs* | | | | |
| Marginal effect | −0.021 | 0.006 | −0.017 | −0.010 |
| | (0.014) | (0.010) | (0.042) | (0.028) |
| *Panel C: Input purchases* | | | | |
| Marginal effect | −0.037 | −0.015 | 0.057 | −0.014 |
| | (0.025) | (0.027) | (0.086) | (0.049) |
| *Panel D: Investment* | | | | |
| Marginal effect | −0.004 | 0.020 * | 0.005 | −0.021 |
| | (0.013) | (0.009) | (0.030) | (0.025) |

Table C3: **Placebo tests for pretreatment periods** *Note:* Each coefficient corresponds to the estimated ATT for pretreatment years (2012 and 2013) using the IPW-DiD specification. Standard errors in parentheses. None of the coefficients are statistically significant under the revised thresholds: $^{*}p < 0.05$, $^{**}p < 0.01$.

variables. This supports the validity of the parallel-trends assumption and indicates that post-2014 effects are not driven by preexisting differences between treated and control firms.