

Made in Brazil: Job Creation and Firm Performance Under a Local Content Requirement*

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Abstract

Local content requirements (LCRs) are a widely used industrial policy to promote domestic firm growth and job creation, but empirical evidence of their efficacy is thin. We use establishment-level microdata from Brazil to estimate direct and spillover effects of an LCR that required oil companies to source inputs locally. We find that demand shocks channeled to local suppliers by the LCR raise employment, wages, revenues, and profits for these suppliers in the short run and generate positive employment spillovers onto neighboring establishments. Over the long run, local content suppliers exhibit higher employment and wage growth and increase their share of educated and innovative workers relative to controls, but show no improvement in investment, labor productivity, TFP, or export propensity. Local suppliers are also more likely to donate to the political party most supportive of the LCR. Our findings suggest that while LCRs can generate jobs and skill-upgrading, they are less effective at promoting firm performance and may strengthen special interest groups.

JEL Codes: L52, L25, Q32, L71, J24

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1 Introduction

Trade restrictions and industrial policies are back on the policy menu, but measuring their efficacy remains challenging (Fernandes and Reed, 2026; Juhász et al., 2023; Lane, 2020). Interest in industrial policies is especially acute in countries dependent on natural resources, where diversification away from raw commodity exports is a policy priority (UNCTAD, 2022; Gelb, 2010; African Union, 2009). Countries ranging from Indonesia to Norway have introduced resource-based industrial policies to internalize value-addition along commodity chains and stimulate development of upstream and downstream industries (Bosker et al., 2025; Yan Ing and Grossman, 2024; Guadagno, 2015).

One common industrial policy is a local content requirement (LCR), which mandates that firms undertake key operations (e.g., input sourcing, output processing) in-country (Stone et al., 2015). The goal of such policies is to provide demand-stimulus to domestic firms and workers, generating learning-by-doing and scale economies. However, LCRs are not costless. These policies may deter foreign direct investment, create deadweight loss, and reduce competition – potentially enabling rent-seeking and protecting inefficient firms (Hansen and Therkildsen, 2016).

In this paper, we analyze the establishment-level impacts of an LCR imposed on Brazil’s oil and gas industry. This policy was created in 1997 but strengthened significantly in 2003 by requiring oil companies to source a minimum percentage of their inputs from Brazil-based firms, with the goal of promoting domestic firm growth and job creation. While the LCR channeled demand for oil and gas inputs to local firms, it also led to complaints from oil companies about high costs, low quality, and long wait-times for inputs, eventually leading to a significant reduction in required local content percentages in 2017 (Clavijo Vitto, 2016; Chagas and Craide, 2017). This tension between the policy’s intent to build domestic industrial capacity and the costs it imposed on regulated firms raises a policy-relevant question: did the LCR succeed in creating durable local employment and upgrading firm capabilities, or did it protect inefficient firms without lasting gains?

We address this question by linking longitudinal employer-employee administrative records, manufacturing census data, and a registry of local establishments certified to sell inputs

through the LCR (referred to throughout the text as “LC establishments”) to measure job creation, wage growth, skill and innovation intensity, and firm performance between 2000 and 2019. We implement three complementary empirical strategies to assess the impacts of Brazil’s LCR across this rich set of outcomes. First, within the sample of LC establishments, we leverage data on product-specific annual demand from oil companies for locally-sourced oil inputs (referred to throughout the text as “LC demand”) to measure how establishments respond to plausibly exogenous demand shocks. Second, following [Borusyak et al. \(2025\)](#), we implement a shift-share approach at the municipality-year level, interacting each municipality’s baseline LC employment share with annual LC demand shocks to measure employment spillovers onto neighboring, non-LC establishments. Third, we measure dynamic long-term impacts of the policy by estimating event studies that compare outcome trends between LC establishments and closely-matched non-LC establishments before and after the strengthening of Brazil’s LCR in 2003.

First, comparing within the sample of LC establishments, we find that 1% growth in product-specific LC demand is associated with contemporaneous growth in average monthly wages (+0.1%), total employment (+0.03%), and high-education employment (+0.03%), as well as a marginally higher likelihood of remaining active (+0.01 percentage points), in establishments producing that product, relative to LC establishments producing other products. Using firm-level data from Brazil’s Manufacturing Census, we additionally find that 1% growth in product-specific LC demand is associated with contemporaneous growth in firm-level profits (+0.45%) and revenues (+0.03%), along with small increases in exported share of output and likelihood of exporting. These results indicate that LC establishments are responsive in the short-term to demand channeled toward them by the LCR.

Second, using the shift-share exposure design, we estimate employment spillovers of LC demand onto neighboring (i.e., same municipality) non-LC establishments. A one-standard-deviation increase in LC demand raises employment in LC establishments themselves by 11.5% (roughly 165 workers at the baseline mean), and generates positive spillovers onto neighboring establishments in oil-linked sectors: employment rises by 4.9% among establishments downstream of oil and gas extraction (roughly 161 workers) and by 8.6% among upstream establishments (roughly 319 workers). All-sector non-LC employment remains

essentially unchanged (+0.6%, not statistically significant). We find no evidence that LC demand crowds out local employment on net, with positive spillovers concentrated among establishments linked to the oil and gas supply chain.

Third, based on event study estimates around the strengthening of the LCR in 2003, we find that the policy significantly increased wages and the education intensity of employment among participating LC establishments. Average earnings increase by 32% (+BRL\$363), the high-education share of employment by 4.3 p.p., and the innovation-related share of employment by 2.0 p.p. by 2015. The average LC establishment added approximately 100 more jobs than matched controls by the 2012 peak, though we interpret this descriptively since LC establishments were already adding jobs significantly faster than controls before 2003, violating parallel pre-trends for this outcome. Establishments' skill upgrading was driven by significant increases in the share of professional and technical employees, while the share of managerial employees remained flat – suggesting genuine increases in technical capacity rather than additional hiring to manage LCR-related administrative compliance. Event studies for measures of performance from Brazil's Manufacturing Census (e.g., profits, revenues, investment, export propensity) show no difference between LC firms and matched controls, suggesting the policy did not have measurable long-term effects on firm performance beyond gains in earnings and skill intensity.

Importantly, these employment and wage gains erode following Brazil's 2014 oil bust and a 2017 reduction in mandated minimum local content shares from 37-55% to 18-25%. This policy change was followed by a rapid reduction in oil companies' local content commitments to the mandated floor, suggesting that firms viewed minimum local content requirements as binding and sought contractual flexibility once these were lowered. Local content as a share of realized investment did not immediately fall – and in most input categories continued rising through 2019 — because commitments made prior to 2017 still bind for the life of each field. The sharp drop in commitments after 2017 nonetheless suggests that 14 years of LC demand stimulus and accumulated firm relationships were not sufficient to make local suppliers internationally competitive once LCR protections were lifted.

Finally, we explore two extensions that address central topics of interest among industrial policymakers: competition and special interest formation. Competition is often highlighted

as a key determinant of industrial policy success, as it disciplines beneficiary firms and reduces the need for bureaucrats to evaluate firm performance (Aghion et al., 2015; Cherif and Hasanov, 2019). By excluding international competition, Brazil’s LCR guaranteed market share for some sectors that were domestically competitive and others that were domestically concentrated. We exploit this variation to conduct heterogeneity analyses comparing LCR impacts on establishments in competitive versus concentrated product categories, based on product-specific Herfindahl-Hirschman Indexes. Results show that establishments in *concentrated* product markets were more responsive to LC demand shocks – potentially because LC demand for products from competitive sectors was diluted among many competing establishments, reducing per-establishment exposure. Event study heterogeneity by market concentration reveals that employment growth was significantly higher among LC establishments in concentrated sectors. These findings suggest that LC demand must be felt by participating establishments at sufficient scale to produce a response, and this scale is more likely achieved in concentrated markets where only a few firms are eligible to capture LC demand. This result highlights a tension in how competition shapes industrial policy impacts: while competitive pressure is often credited with disciplining beneficiary firms, it can simultaneously dilute the demand stimulus each individual firm receives.

Second, based on concerns that LCRs create concentrated interest groups that may engage in rent seeking or prolong sectoral protections (Hansen and Therkildsen, 2016), we connect establishments with a registry of political campaign donations to assess the behavior of local content suppliers as a special interest group. We find that LC establishments are 38% more likely to make a campaign donation relative to matched non-LC establishments. Conditional on donating, LC establishments donate 50% larger amounts and direct a 70% larger share of their donations to the Workers’ Party, which was the party most supportive of the LCR during this period, indicating that LC establishments were politically active and disproportionately supportive of the party that backed their sectoral interests. While these results are descriptive, they are consistent with concerns that sector-specific industrial policies can create constituencies invested in their own continuation.

Taken together, our main findings speak to the direct effects of Brazil’s LCR on targeted upstream establishments and their neighbors. A full accounting of the policy’s net impact

additionally requires quantifying its indirect costs, such as foregone oil investments and delayed or reduced oil production relative to a counterfactual scenario where oil companies were not required to comply with the LCR.

To provide suggestive evidence on these indirect costs, we exploit variation in LC% commitments that oil companies submitted for blocks in Brazil’s concession rounds 9–13 (2007-2015). In these rounds, companies committed to minimum LC% levels for the exploration and development phases of each block. These commitments were contractually binding and varied substantially across blocks, even within the same bidding round. We combine LC% commitments with data on block-year exploration, development, and production activity and estimate panel regression and survival models to compare outcomes between blocks with higher and lower LC% commitments, conditional on a rich set of bid and block characteristics and round, year, and operator fixed effects. We find no evidence that higher commitments are associated with delayed or reduced oil exploration, development, or production. Compliance audits further show that, among inspected blocks, most LC% commitments were met or exceeded, suggesting they were not a binding constraint on oil companies. This evidence is correlational and only covers blocks licensed in concession rounds 9-13, missing ultra-deepwater pre-salt blocks licensed under production sharing, where the LCR may have imposed higher costs due to more complex technical requirements for inputs (Clavijo et al., 2019).

1.1 Related Literature and Contributions

Our use of establishment-level panel data from Brazil allows us to contribute nuanced insights into the impacts, spillovers, and mechanisms underlying an increasingly popular industrial policy – the local content requirement.

Rigorous firm-level evaluations of LCRs are scarce: Probst et al. (2020) evaluate an LCR for solar panels in India and find that it raised costs and did not substantially expand domestic manufacturing. Münch and Scheifele (2023) evaluate the same policy and find that it did not significantly increase Indian firms’ innovation activity. These authors cite reduced competition, insufficient scale, and weak performance requirements to explain this outcome. Bai et al. (2024) study a Chinese policy that required foreign automakers to set

up joint ventures with local firms in exchange for market access, finding that this policy generated knowledge spillovers and quality improvements in domestic automakers. [Chang \(2025\)](#) studies an LCR for the mining sector in Ghana and finds that the policy increased local employment, procurement, and economic activity around mines. Most closely related to our study, [Rocha \(2018\)](#) measures innovation-related employment in Brazilian LC establishments relative to matched controls between 2009-2011 and finds a small positive effect.

We contribute to this LCR-specific literature with rich establishment-level panel data measuring both the human capital and operational dimensions of firm performance, allowing us to assess employment spillovers, heterogeneity in LCR impacts by market concentration, and long-term impacts across a 20-year window. The period covered by our data (2000-2019) is particularly useful, given that it allows us to observe 14 years post-policy strengthening – sufficient time for effects to plausibly emerge or fail to emerge – as well as 3 years post-policy weakening, enabling assessment of whether LC establishments developed sufficiently during their period of protection to weather exposure to international competition.

We also contribute to the literature on spatial and supply-chain spillovers from industrial policies or other demand shocks ([Cerqua and Pellegrini, 2017](#); [Du et al., 2014](#); [Greenstone et al., 2010](#)). [Lane \(2025\)](#) shows that South Korea’s promotion of heavy industry in the 1970s generated positive effects on downstream sectors and negative effects on upstream sectors, while [Machado Parente et al. \(2025\)](#) similarly find that protective industrial policies targeting upstream sectors tend to improve downstream firm outcomes, whereas policies targeting downstream sectors are associated with weaker upstream performance. [Toews and Vézina \(2022\)](#) document positive local employment multipliers from FDI-driven job creation in Mozambique, and [Cust et al. \(2019\)](#) show that oil booms raise both wages and employment in nearby manufacturing firms in Indonesia. Our findings show a related but distinct pattern: Brazil’s LCR, which targeted upstream oil and gas inputs, generated positive employment spillovers onto neighboring establishments in both upstream and downstream oil-linked sectors. We find no evidence of net crowding-out of local employment overall, suggesting that demand-linkage and agglomeration channels dominated any labor-competition channel of the kind emphasized in the classic Dutch Disease framework ([Corden and Neary, 1982](#)).

Most broadly, our findings contribute to the growing empirical literature on industrial

policies, including [Barwick et al. \(2026\)](#), [Allcott et al. \(2024\)](#), [Barwick et al. \(2023\)](#), [Gebrewolde and Rockey \(2022\)](#), and [Criscuolo et al. \(2019\)](#). Relative to existing evidence, we provide novel insights on the tradeoffs involved with implementing a natural resource-based local sourcing requirement, which is relevant for resource-dependent countries seeking to promote development by harnessing an extractive sector.

2 Policy Context

Globally, local content requirements are a widespread and resurgent policy tool. The OECD documented the introduction of 145 new LCRs between 2008 and 2015, with large numbers concentrated in Brazil (17 policies), Indonesia (18 policies) and the United States (23 policies) ([Stone et al., 2015](#)). The 2022 *Inflation Reduction Act* in the United States provided substantial tax credits for steel, iron, and manufactured products that were domestically produced, with renewable energy and battery products eligible for bonus credits of up to 10% if they met domestic wage and apprenticeship requirements ([RSM US, 2023](#)). China’s *Made in China 2025* policy initiative emphasizes internalizing supply chains and production capacity in key technology areas ([Branstetter and Li, 2022](#)). In 2025, Mexico announced *Plan México*, a portfolio of US\$277 billion in investments focused on achieving 50% domestic production in key manufacturing sectors and stipulating that local governments procure at least 50% of their acquisitions from domestic sources ([Quintana, 2025](#)). Policies requiring solar or wind energy developers to source minimum shares of employment or inputs locally have been implemented in at least 21 countries since 2009 ([OECD, 2015](#)).

There are compelling economic arguments both for and against LCRs. Models of agglomeration externalities, which allow for knowledge spillovers, learning-by-doing, and economies of scale, suggest that channeling demand from the oil sector to domestic firms could help entrants overcome fixed costs and realize dynamic within-firm productivity gains ([Duranton and Puga, 2004](#)). [Greenstone et al. \(2010\)](#) finds that demand stimulus provided by the opening of large plants leads to significant increases in TFP in neighboring firms, especially those that participate in similar factor markets. [Liu \(2019\)](#) demonstrates that targeting subsidies to upstream sectors may generate positive aggregate economic effects in the presence

of market imperfections.

While LCRs can provide demand stimulus for domestic firms, they also shelter those firms from international competition – reducing trade and productivity (Yan Ing and Grossman, 2024). LCRs may also impede the formation of regional supply chains (Cust and Zeufack, 2023). Particularly for small and medium countries, achieving sufficient economies of scale domestically may be difficult, highlighting the importance of region-level policy coordination (Korineck and de Sa, 2024). Likewise, Hansen and Therkildsen (2016) argue that local content policies cannot be considered in purely economic terms, as their underlying motivation is often to build and sustain political coalitions and patronage networks – creating opportunities for rent seeking and corruption (Kinyondo and Villanger, 2017). Nwapi (2015) argue that local content rules often prioritize national ownership and sourcing, but neglect truly *local* participation, i.e. community-level input and benefits. Finally, Kolstad and Kinyondo (2017) note that local content requirements may reduce multinationals’ production, and hence resource tax revenues, reducing governments’ capacity to spend on public goods provision. They argue that this opportunity cost of local content requirements must be weighed against any potential benefits.

Local content requirements are particularly prominent in extractive industries, where developing countries seek to diversify away from raw material exports and resource dependence by harnessing extractive sectors to create jobs, stimulate and upgrade domestic firms, and generate spillovers to upstream and downstream industries (Cosbey and Ramdoo, 2018; Ovadia, 2016; Ramdoo, 2015). The World Bank identified 21 developing countries implementing hydrocarbon-oriented local content initiatives as of 2023 (12 in Africa, 5 in Asia, and 4 in Latin America), and 16 countries implementing mineral and metals policies (10 in Africa, 3 in Asia, and 3 in Latin America) (ELLEED, 2023). Active policies include requirements for domestic asset ownership, employment, technology transfer, input sourcing, and downstream processing. Domestic processing requirements have grown particularly rapidly in recent years, including Indonesia’s ban on raw nickel and bauxite exports in 2014 (Bosker et al., 2025). In Sub-Saharan Africa, the share of countries imposing raw mineral or metal export bans increased from 26% in 2009 to 42% in 2020 (Cust and Zeufack, 2023).

Nevertheless, implementation of these policies has often failed to deliver transformative

development gains. [Adedeji et al. \(2016\)](#) find that a local ownership policy for Nigeria’s oil sector made limited contributions to local employment and value-addition, though the analysis relies on a small cross-sectional survey. [Rexer \(2025\)](#) assesses the same policy and finds that shifting oil fields to local ownership increased output and reduced violent conflict, but also resulted in more oil spills. [Ovadia \(2012\)](#) notes that a policy of “Angolization” of the oil industry in Angola likely contributed to economic growth, but also concentrated wealth and power among local elites. Furthermore, extractives-focused LCRs could lock countries into sectors that are at risk of long-term declines associated with climate change mitigation and energy transition, resulting in stranded assets ([van der Ploeg and Rezai, 2020](#)).

2.1 Brazil’s LCR for the Oil and Gas Industry

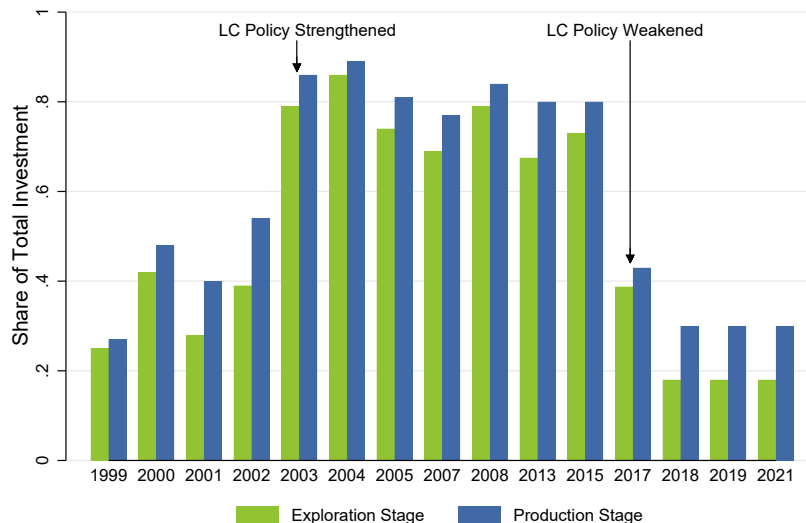
Brazil’s LCR for oil and gas companies began with reform of the sector in 1997. Petroleum Law N^o 9.478/1997 required oil and gas companies to give preference to local inputs (e.g., ships, drilling rigs, pipes), when these were available and competitive with international equivalents, but did not set minimum local content levels. In 2003, the National Oil Agency (ANP) began to require minimum local content commitments in bids submitted by oil companies participating in auctions for rights to offshore fields ([de Negri et al., 2010](#)). As shown in Figure 1, the 2003 rule change resulted in a significant increase in local content commitments, which bind for the duration of production and exploration activities within a field ([ANP, 2024](#)).¹ The stated goals of Brazil’s LCR policy were to promote national industrial development, technological innovation, export competitiveness, and job creation across all upstream segments of the oil and gas sector ([de Almeida et al., 2016](#)).² The ANP enforced

¹In 2005, the ANP strengthened the LCR further by shifting from an average, or “global” local content requirement *across* all product categories, to minimum local content requirements *within* each product category. Regulators imposed product-level requirements to encourage companies to source sophisticated inputs from Brazilian firms, as they had previously been satisfying the requirement by sourcing only less-sophisticated inputs locally ([Rocha, 2018](#)). From oil and gas companies’ perspective, product-specific requirements restricted their ability to allocate procurement between local and international suppliers in accordance with the feasibility and costs of production within each product category in Brazil ([Clavijo Vitto, 2016](#)).

²Along with its strengthening of the LCR in 2003, the Brazilian government introduced sectoral policies to support capacity-building among input-supplying firms. Initiated in 2003, the Program for Mobilization of the Oil and Gas Industry (PROMINP) promoted sector-specific training courses and knowledge-exchange between local and foreign firms and universities. Beginning in 2012, Inova Petro financed R&D for local content suppliers through Brazil’s National Development Bank (BNDES) in partnership with Petrobras, the national oil company ([Leão, 2017](#); [de Negri et al., 2010](#)).

local content commitments through the application of fines. By 2016, the agency had assessed 110 fines to oil and gas companies totaling R\$570 million, of which 62% were paid by Petrobras, the national oil company (de Almeida et al., 2016).

Figure 1: Average Local Content Commitment in Winning Bids by Oil Companies



Note: Average local content commitments in winning bids for rights to oil and gas fields, from ANP (2024), expressed as the share of total planned procurement that oil companies committed to source from Brazil-based suppliers at the time of the bid. Commitments apply to newly auctioned fields and bind for the duration of exploration, development, and production within the field. Data are not available for every year because field auctions were only held periodically.

Oil and gas companies protested consistently about the requirement for up-front local content commitments before conditions of fields were known, as well as about local input suppliers being slower, higher cost, and lower quality than international alternatives (Guimarães, 2012). A study by the National Organization of the Petroleum Industry (ONIP) found that costs of local inputs exceeded international alternatives by 70% in some product categories and that only 24% of Brazilian input suppliers were internationally competitive (ONIP, 2010).³ These complaints eventually led to the weakening of the LCR in 2017. In that year, minimum local content percentages were reduced from 37% to 18% for offshore exploration and from 55% to 25% for offshore production (ANP, 2024). The result of these reduced requirements is evident in Figure 1: local content commitments in winning bids for offshore

³Upstream suppliers also criticized the administrative complexity of Brazil's LCR, which requires them to obtain certification of the local content embodied in each sub-component of their final products (Guimarães, 2012). These requirements favor large firms over smaller ones, potentially explaining the presence of disproportionately large firms among the pool of LC-certified suppliers.

fields fell from approximately 75% between 2003 and 2015 to 40% in 2017, and then to approximately 25% in subsequent bidding rounds.

Several qualitative studies have discussed the impacts of Brazil’s LCR for the oil and gas industry. [Clavijo Vitto \(2016\)](#) assesses the evolution of the policy up to 2014, concluding that it suffered from institutional fragmentation in governance and piecemeal formulation through trial and error, resulting in lack of formal mechanisms for impact evaluation and course-correction. However, [Clavijo Vitto \(2016\)](#) also concludes that the policy was effective in encouraging economies of scale and R&D investments in some product categories. Likewise, a report by the Brazilian Petroleum Institute found that the policy’s focus was overly broad, covering all upstream segments rather than focusing on areas that were more competitive, strategic, or likely to deliver local development benefits ([IBP, 2015](#)). In sum, these qualitative assessments point to limited gains among input-supplying firms in some upstream segments, but also to high costs and lack of overall transformation of the sector.

3 Data

We draw on three establishment-level datasets to conduct empirical analyses. The first is an identified registry of 3,459 Brazil-based establishments that received certifications to sell inputs to the oil and gas industry through the National Oil Agency’s Local Content program ([ANP, 2024](#)). To obtain local content certification, an establishment must provide documentation of the percentage of goods, services, and labor inputs of national origin embodied in the final product or service ([ANP, 2023](#)). Certifications report the year of certificate emission and expiration, the local content percentage of the product, product category (scope), and establishment ID. Certified local content suppliers range from multi-product establishments with over 2,000 product certifications across more than a dozen scopes, to 771 establishments with a single product certification.

Second, we use employer-employee linked administrative records from the *Relação Anual de Informações Sociais* (RAIS) from 2000 to 2019 ([MTE, 2020](#)) to measure employment-related outcomes in LC establishments and matched control establishments. We achieve an exact match between LC establishments and RAIS using establishment IDs (CNPJs). Based

on employer-employee records, we construct establishment-year measures of total employment (disaggregated by education level and occupation type), wages, establishment entry and exit, task-intensity, and innovation.⁴

Third, we construct a panel of firm profits, revenues, investments, inventories, export-orientation, labor productivity, and total factor productivity (TFP) from Brazil’s Manufacturing Census (*Pesquisa Industrial Anual-Empresa*, PIA-Empresa), for 2000-2019 (IBGE, 2022). Variable definitions for this dataset are reported in Appendix 1. Manufacturing Census data are only reported at the firm-level, aggregating across all the firm’s establishments. We exactly connect data from the Manufacturing Census with LC establishments using firm-roots of establishment ID numbers within a restricted data facility at the *Instituto Brasileiro de Geografia e Estatística*. The Manufacturing Census is an annual survey of all firms in manufacturing and extractive industries with 30 or more employees, as well as a stratified representative sample of firms with fewer than 30 employees (IBGE, 2024). Because nearly all LC establishments are large (mean employment in 2003 = 125), this restriction does not significantly reduce our observed sample.

We draw on several supplementary data sources. To measure product-level demand shocks generated by oil companies sourcing inputs from Brazil-based establishments (“LC demand”), we use the reported monetary value of oil and gas companies’ certified local input purchases, which are available from 2006-2019 (ANP, 2024). Oil companies are required to report these data each trimester to the National Oil Agency. To explore political involvement, we connect each LC establishment and control establishment with data on campaign donations from Brazil’s *Tribunal Superior Eleitoral* (2024) using establishment IDs. To assess impacts of the LCR on oil company exploration, development, and production activity, we use data from the ANP (2026) open data portal.

⁴We construct a novel measure of establishment-level innovation intensity by combining employment data from RAIS with occupation-specific descriptions from the *Quadro Brasileiro de Qualificações* (QBQ) (Ministério do Trabalho, 2022). The QBQ provides detailed text descriptions as well as knowledge, ability, and soft-skill requirements for 1,901 occupations specified in the *Classificação Brasileira de Ocupações* (CBO), Brazil’s equivalent of O*NET. We perform text-analyses of occupation descriptions in the QBQ to identify the frequency of innovation-related language, and collapse these measures to the establishment-year level. Innovation-related keywords are reported in Appendix Table A1. Bustos et al. (2022) use a similar approach but rely on less-detailed occupation descriptions reported for around 700 aggregated occupation families described in the *Classificação Brasileira de Ocupações* (CBO).

3.1 Descriptive Evidence

Demand for inputs into the Brazilian oil and gas sector comes primarily from offshore production, which experienced a major expansion during the study period as a result of giant offshore discoveries in “pre-salt” formations under the seabed. Appendix Figure A1 plots the total value of locally sourced inputs purchased by oil and gas exploration and production sectors, relative to cumulative offshore discoveries. Following major discoveries from 2006 onward, demand for exploration inputs ramped up first, followed with a delay of several years by a ramp up in development inputs as companies began shifting from exploration to production.⁵ Within these aggregate trends, there was significant year-on-year variation in product-specific LC demand, as shown in Appendix Figure A2.

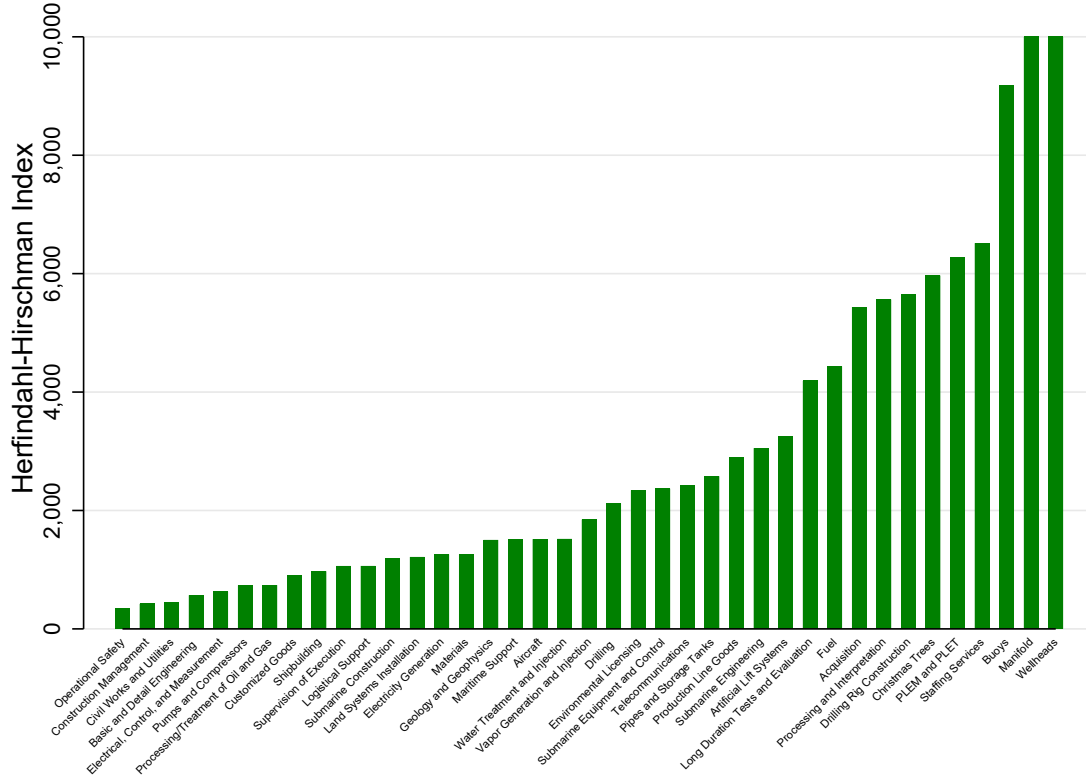
To explore the mediating effects of competition on LCR efficacy, we compute baseline (2002) Herfindahl-Hirschman Indexes (HHI) by product scope. We document large variation in product-specific market concentration in Figure 2. HHIs for products covered by Brazil’s LCR range from relatively competitive ($\text{HHI} < 1000$) for scopes including operational safety, construction management, basic and detail engineering, pumps and compressors, and ship-building, to highly concentrated ($\text{HHI} > 5000$) for data processing and interpretation, drilling rig construction, and specialized well equipment.

The composition of the LC establishment sample varies over time, as shown in Appendix Figure A4. Net entry of LC establishments between 2000 and 2019 followed broader economic trends but with exaggerated pro-cyclicality with oil prices, turning negative after the 2014 oil price collapse. LC establishments are also more stable than their non-LC counterparts: those present in 2000 are more likely to remain active through 2019, and those present in 2019 are more likely to have also been active in prior years, relative to non-LC establishments in the same municipality and 5-digit sector (CNAE activity code).

Human capital characteristics of LC establishments (2000-2019) are reported in Figure

⁵The spatial distribution of field-specific local content commitments and LC establishments is mapped in Appendix Figure A3. As shown on the map, demand for local oil and gas inputs between 2000 and 2020 came primarily from offshore fields, which range along the entirety of Brazil’s Atlantic coastline – with many clustered in the Southeast region near Rio de Janeiro and São Paulo. LC-certified establishments are also clustered in the major industrial centers of the Southeast, as well as around major shipyards, which serve as final assembly points for inputs into offshore oil and gas production. Because offshore fields are relatively accessible from any well-equipped port, we are not able to exploit spatial variation in local content investments by field as a source of identifying variation.

Figure 2: Herfindahl-Hirschman Index by LC Product Category (2002)

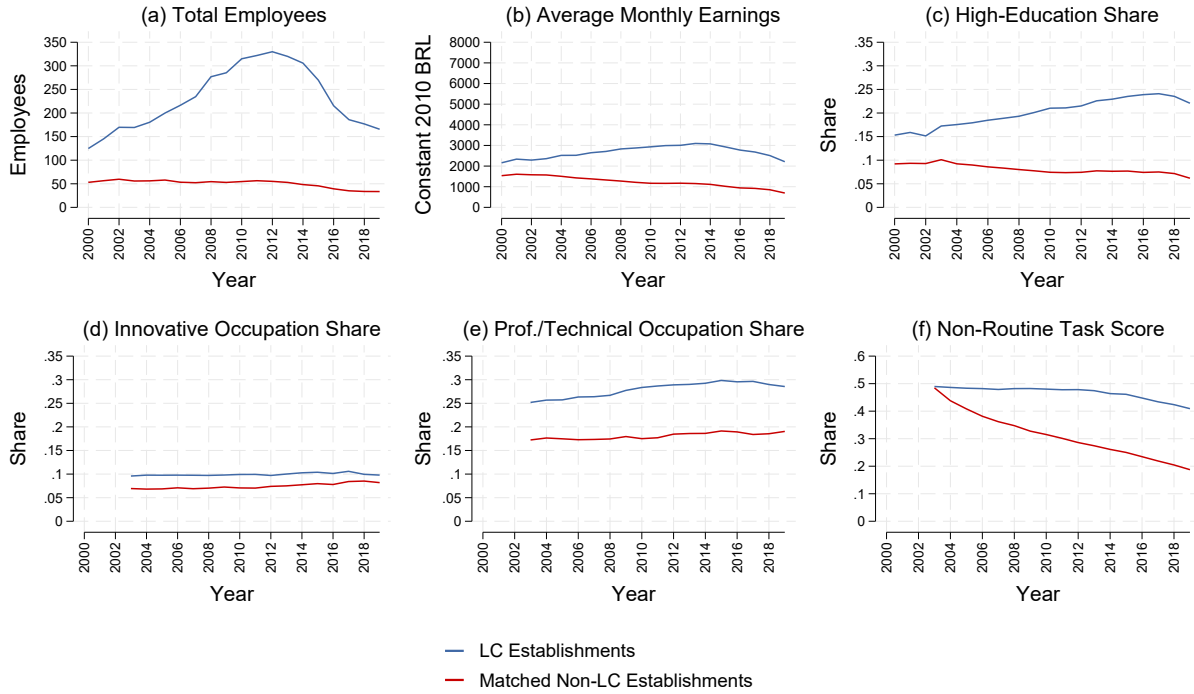


Note: HHIs are based on total revenues at the firm level, using data from PIA-Empresa. Because oil and gas production in Brazil occurs primarily offshore, it is feasible to produce and assemble inputs in many locations before transporting them to extraction sites. We thus compute product-specific HHIs for a single national market, rather than regional markets.

3. We use a fully balanced sample – imputing zeros for inactive firms where appropriate – to avoid selection effects due to entry and exit. LC establishments tend to be larger, pay higher wages, and employ a more skill-intensive workforce relative to non-LC establishments in the same municipalities and 5-digit sectors, including higher shares of high-education, innovative, professional/technical, and non-routine task employees. Total employment and earnings grew substantially in LC establishments between 2000-2014, and then declined following the 2014 oil bust. Higher-education intensity also grew steadily until 2017, in contrast to a gradual decline among non-LC establishments. Innovation intensity remained mostly flat, while the share of professional/technical workers gradually increased. Finally, the share of LC establishments’ workers performing non-routine tasks remained roughly stable despite falling sharply among non-LC establishments, potentially reflecting stronger demand for complex tasks among LC establishments relative to the broader economy.⁶

⁶Figure 3 compares LC establishments to a simple reference group sharing a municipality and 5-digit

Figure 3: Human Capital Characteristics of LC Establishments



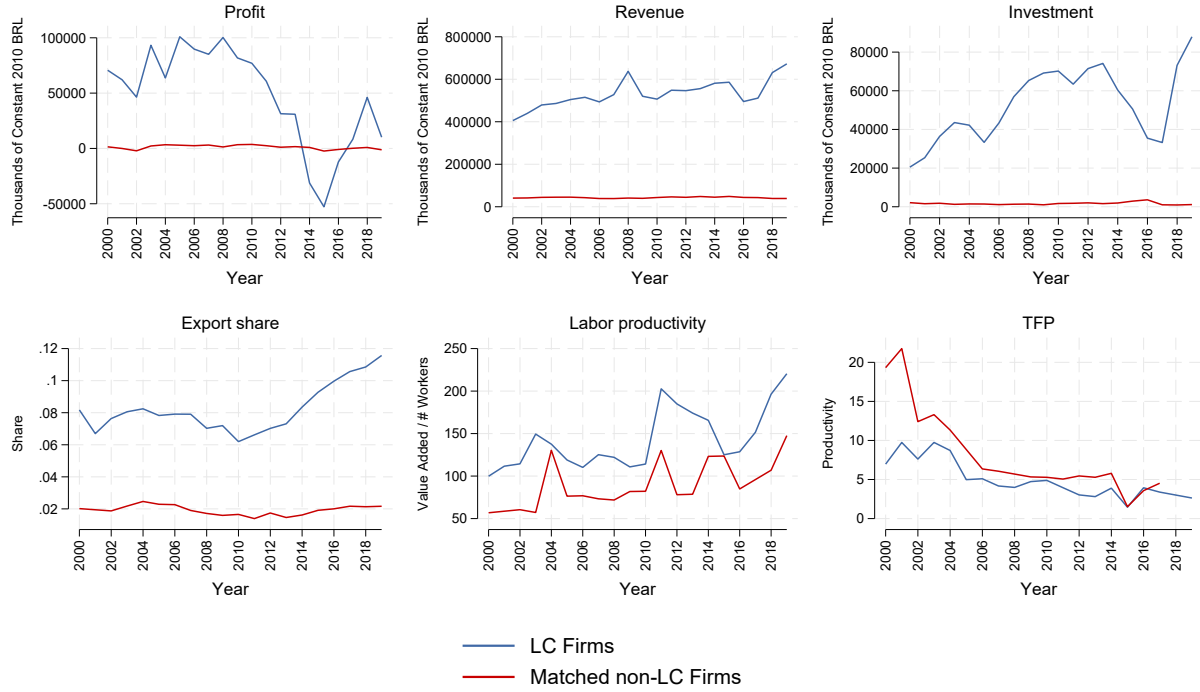
Note: Mean values are computed from RAIS. LC establishments are exactly matched with RAIS using unique establishment IDs (CNPJs). Monetary values are deflated to constant 2010 BRL. “High-Education” workers are defined as those with more than secondary education. Innovation intensity is defined as the share of an establishment’s workers employed in innovation-related occupations, as defined using text analysis of the *Quadro Brasileiro de Qualificações* (QBQ). Non-routine tasks are also derived from the QBQ, and are plotted on a different y-axis scale. Panels (d)-(f) omit data from 2000-2002 because RAIS reports occupation codes using a different classification system for these years, making harmonization difficult. Matched non-LC establishments, presented for comparison, are establishments registered in RAIS with at least one employee that share a municipality and 5-digit CNAE activity code with LC establishments. Note that this is a much looser matching than what is used to estimate event studies in Section 5.3 below.

Figure 4 reports firm performance characteristics for LC firms and comparison firms in the same municipalities and 5-digit sector codes between 2000-2019.⁷ LC firms exhibit substantially higher profits (except for the oil crash period of 2014-2016), revenues, and investment, and export a substantially higher share of their output to international markets.

CNAE activity code with LC establishments. This is not the matched comparison group used in our event study specifications (Sections 4.3 and 5.3). Those specifications additionally match on deciles of pre-2003 average wage, total employment, and total high-education employment. Appendix Figure A6 plots the same human capital characteristics against this more closely matched sample, providing a descriptive analog to the event studies.

⁷For variables from the Manufacturing Census (PIA-Empresa), we use an unbalanced sample because the census design omits firms in years they do not have at least 30 employees, meaning we cannot accurately impute zero values in years when firms are missing from the panel. Interpretation of these descriptives should thus be made in light of the fact that they could either reflect meaningful changes within LC firms, or changes in sample composition over time. However, because LC firms tend to be large, the 30 employee cutoff does not typically cause them to be excluded. Due to data use restrictions, we are unable to connect RAIS and PIA in the restricted data room. We thus run analyses on each dataset separately.

Figure 4: Firm Performance Characteristics of LC Firms



Note: Means are computed from PIA-Empresa. LC establishments are exactly matched with RAIS using unique firm root IDs. Monetary values are deflated to constant 2010 BRL and expressed in thousands. Non-LC Firms, presented for comparison, are firms present in PIA with at least 30 employees that share a municipality and 5-digit CNAE activity code with one or more LC-certified firms.

LC establishments also exhibit higher labor productivity (likely reflecting higher capital intensity) and slightly lower TFP. Focusing on trends over time, LC firm profits are procyclical with the oil and gas industry's boom-bust cycle. Revenues and investment grow steadily throughout the period except for a dip during the 2014-2016 oil bust. Export share of output remained flat until 2010, and then began increasing steadily among LC firms – without a similar increase among non-LC firms. This increase may reflect increased export orientation or competitiveness of LC firms, but could also reflect disproportionate exit of less export-oriented LC firms as a result of the oil boom. Labor productivity in LC establishments increased noisily but substantially over the period – more than doubling by 2019 – while TFP fell. Notably, revenues, investments, export share, and labor productivity in LC firms rose after 2017, despite the weakening of the LCR policy in this year. This likely reflects ongoing local sourcing by oil companies in compliance with local content commitments made in prior years, which bind for the lifetime of each oil field.

4 Empirical Strategies

We implement three complementary empirical strategies to assess (i) short-term direct impacts, (ii) spillover employment effects, and (iii) long-term dynamic impacts of Brazil’s LCR for the oil and gas industry.

4.1 LC Demand Shocks Generated by the Oil Industry

First, restricting the sample to LC establishments, we leverage annual variation in product-specific demand by oil companies for domestically-sourced inputs to estimate establishments’ short-term responsiveness to demand shocks. Specifically, for establishment i in municipality m that produces product basket p in year t ,⁸ we regress outcome of interest Y_{ipmt} on establishment and municipality-year fixed effects (δ_i and γ_{mt} , respectively) and the aggregate value of oil and gas companies’ purchases of Brazil-sourced inputs from product basket p in year t , D_{pt} :

$$Y_{ipmt} = \delta_i + \gamma_{mt} + \beta D_{pt} + \epsilon_{ipmt} \quad (1)$$

We transform both outcomes Y_{ipmt} and LC demand D_{pt} using the inverse hyperbolic sine function to estimate demand elasticities and cluster standard errors at the level of treatment – the scope-group-year level – where a scope group is an establishment’s unique combination of certified product scopes (Abadie et al., 2022).⁹ To assess non-linear effects of demand, we also de-mean D_{pt} and include this value and its quadratic term in an extended version of Equation 1.

In Equation 1, coefficient β identifies the effect of annual product-specific demand on outcomes in LC establishments producing those products, under the identifying assumption that LC demand D_{pt} is exogenous to establishment-level conditions. We argue that this assumption is reasonable for three reasons. First, overall quantities of inputs demanded by oil and gas companies operating in Brazil are mostly determined by exogenous global oil prices and unpredictable offshore discoveries. This pattern is illustrated in Appendix Figure

⁸Due to the nature of our data on establishments’ product-specific certifications, we are unable to treat establishments’ product basket p as time-variant.

⁹We opt for the inverse hyperbolic sine transformation to accommodate zero-value observations, following Bellemare and Wichman (2020). Results are very similar when using $\ln(D_{pt} + 1)$.

A1, which shows how the oil industry’s demand for inputs follows sequentially (first exploration, then development) after major discoveries. Second, due to Brazil’s LCR, companies’ otherwise endogenous choice of whether to source inputs locally versus internationally was constrained, forcing them to source inputs from Brazil-based firms. Third, unique features of Brazil’s LCR further reduce concerns over reverse causality. The requirement that oil and gas companies commit to minimum local content percentages in their initial bids for offshore fields – which are then binding for the duration of exploration and production in that field – locked companies into local input purchases regardless of subsequent conditions in local industries. Likewise, product-specific local content requirements constrained companies’ ability to respond to conditions within any specific product segment by satisfying local content minimums in other segments. In sum, exogenous global prices and discoveries drove demand for inputs, which companies were forced to source locally due to the LCR – regardless of conditions in local establishments.

Furthermore, by restricting the sample to only LC establishments and exploiting product-specific shocks, this specification avoids concerns over self-selection into LC certification. Establishment fixed effects absorb time-invariant establishment characteristics, including unobservables. Municipality-year fixed effects absorb shocks affecting all establishments in specific municipality-years (e.g., oil prices, which may exert differential effects depending on a municipality’s proximity to shipyards and offshore fields). While this empirical strategy does not directly compare outcomes in LC versus non-LC establishments, it tests the first-order question of whether (and along which dimensions) LC establishments respond to demand stimulus generated by the oil industry.

4.2 Shift-Share Exposure to LC Demand Shocks

Next, following [Borusyak et al. \(2025\)](#), we compute a municipality-year shift-share variable to estimate the spillover effects of LC demand shocks on employment in neighboring non-LC establishments. The direction of these spillovers is theoretically ambiguous. The LCR may crowd out employment in nearby firms if LC establishments hire workers away from them or raise wages in local labor markets. It may also raise employment in neighboring firms through demand linkages or agglomeration externalities. We calculate municipality

m 's baseline exposure to LC demand in product category p using 2002 employment:

$$BaselineExposure_{mp} = \frac{LCEmployment_{mp}^{2002}}{Employment_m^{2002}} \quad (2)$$

where $LCEmployment_{mp}^{2002}$ is the number of formal employees in establishments on the LC supplier list for product category p in municipality m in 2002, and $Employment_m^{2002}$ is total formal employment in municipality m in 2002. We then interact $BaselineExposure_{mp}$ with national LC demand for product category p in year t ($LocalDemand_{pt}$), normalized by total 2002 employment in LC establishments nationwide for that product category ($LCEmployment_p^{2002}$). Aggregating these interactions across product categories gives the municipality-year shift-share exposure:

$$Z_{mt} = \sum_p (BaselineExposure_{mp} \times \frac{LocalDemand_{pt}}{LCEmployment_p^{2002}}) \quad (3)$$

We include this shift-share exposure in the following municipality-year regression:

$$Y_{mt} = \psi_m + \omega_{st} + \beta Z_{mt} + \phi \Omega_{mt} + \theta Royalties_{mt} + \epsilon_{mt} \quad (4)$$

where Y_{mt} denotes municipality-year employment outcomes, measured separately for (i) LC establishments, (ii) all non-LC establishments, (iii) LC-CNAE controls (i.e., establishments in the same 5-digit sector code as LC establishments, but which are not themselves LC establishments), and sectors upstream and downstream of the oil industry. All outcome variables are log transformed.

The term $\Omega_{mt} = S_m \times \delta_t$ (where $S_m = \sum_p BaselineExposure_{mp}$ is municipality m 's total baseline LC exposure) interacts this exposure with year fixed effects.¹⁰ The inclusion of Ω_{mt} restrict identification to within year variation between scopes. $Royalties_{mt}$ denotes oil and gas royalties received by municipality m in year t – a control for differential exposure to the oil and gas cycle, which we discuss in more detail below. ψ_m and ω_{st} are municipality and

¹⁰Because exposure shares in Equation 2 are defined relative to total municipal employment, they do not sum to one across product categories. Instead, their sum S_m measures municipality m 's overall baseline intensity in LC-listed activities. The instrument therefore has the “incomplete shares” interpretation in [Borusyak et al. \(2022\)](#): Z_{mt} combines product-year LC demand shocks with municipalities' pre-determined exposure to each product category, while allowing the total degree of baseline exposure to differ across municipalities. To avoid confounding the effect of changes in the composition of LC demand with differential trends in overall LC intensity, we control for $S_m \times \delta_t$. This absorbs year-specific shocks to municipalities that were more exposed to LC-listed activities at baseline, regardless of which product category generated the exposure.

state-year fixed effects, respectively. We estimate this regression by weighted least squares, weighting municipalities by their 2006 population. Upstream and downstream oil industry linkages are from [Katovich et al. \(2025\)](#).

Following [Borusyak et al. \(2022\)](#), we estimate this model using its equivalent shock-level representation. Operationally, we first partial out the municipality fixed effects, state-year fixed effects, baseline-exposure-by-year controls in Ω_{mt} , and oil and gas royalties from the municipality-year outcomes and the shift-share exposure. We then aggregate residualized variables to the product-year level using baseline exposure shares $BaselineExposure_{mp}$, such that each observation corresponds to a shock cell (p, t) . At the shock level we estimate the instrumental-variables regression:

$$\bar{Y}_{pt} = \delta_t + \beta \bar{Z}_{pt} + \bar{\epsilon}_{pt}, \quad (5)$$

where bars denote exposure-weighted residualized variables, the aggregated exposure $\bar{Z}_{pt}$ is instrumented by the scaled demand shift $LocalDemand_{pt}/LCEmployment_p^{2002}$, and each shock cell is weighted by municipalities' average baseline exposure to product category p , as in the Borusyak–Hull–Jaravel aggregation procedure. This IV estimator delivers a coefficient numerically identical to the weighted least squares estimate of the municipality-year specification above, but makes explicit that identification comes from variation across normalized LC product-year demand shocks, not from the number of municipalities exposed to them. We cluster standard errors at the product-year (shock) level.

The identifying assumption underlying this strategy is that, conditional on municipality fixed effects, state-year fixed effects, baseline-exposure-by-year controls, and oil and gas royalties, changes in national LC demand for product category p are unrelated to unobserved municipality-level shocks to non-LC employment. This assumption is plausible because $LocalDemand_{pt}$ varies at the national product-year level and is driven by exogenous oil-sector demand, offshore discoveries, and the timing of LCR policy implementation, rather than by labor-market conditions in any individual municipality. We measure exposure using 2002 employment, prior to LCR strengthening in 2003, so the shares are predetermined with respect to subsequent LC demand shocks. State-year fixed effects absorb state-specific trends

and policy changes, while royalties control for direct exposure to the oil and gas cycle.¹¹

4.3 Event Studies Around 2003 Policy Strengthening

Our third empirical strategy leverages the significant strengthening in 2003 of Brazil’s LCR (as illustrated in Figure 1) to estimate dynamic treatment effects of this policy on LC establishments relative to closely matched non-LC establishments through 2019. For establishment i in year t , let $K_{it} = t - 2003$ be the number of years before or after the policy was strengthened, and let LC_i be an indicator for establishments on the LC suppliers list. We regress outcome Y_{it} on relative-year indicators $\mathbb{1}(K_{it} = k)$ interacted with LC_i , controlling for establishment and municipality-year fixed effects (δ_i and γ_{mt} , respectively):

$$Y_{it} = \delta_i + \gamma_{mt} + \sum_{k \neq -1} \beta_k \mathbb{1}(K_{it} = k) \times LC_i + \epsilon_{it} \quad (6)$$

Coefficients β_k identify the dynamic average treatment effect on the treated (ATT) k years after the 2003 strengthening of the LCR, relative to the omitted base year 2002 ($k = -1$), under the conditional parallel trends assumption. We cluster standard errors at the establishment level and use untransformed outcomes.

There are three primary threats to identification in this specification. First, establishments self-select into LC certification. To minimize bias from this selection, we implement a pre-matching procedure that restricts non-LC control establishments to those exactly matching LC establishments on municipality and 5-digit industry code, combined with coarsened exact matching on deciles of pre-treatment characteristics in each of the years 2000, 2001, and 2002. For RAIS establishment-level outcomes, these pre-treatment characteristics are number of employees, share of high-education employees, and average wage; for PIA firm-level outcomes, they are wage bill and capital stock. Matching weights are included in the regression estimation for both samples. Additionally, inclusion of establishment (or firm) fixed effects absorbs time-invariant characteristics at that level and identifies effects off within-unit variation. Second, LC establishments may be disproportionately exposed

¹¹Oil and gas royalties proxy for direct exposure to the oil and gas industry because their allocation formula depends on municipalities’ spatial alignment with offshore fields and the presence of oil and gas infrastructure, including pipelines, refineries, and off-loading terminals (Katovich, 2024).

to the oil boom and bust that occurred in Brazil during the 2000s and 2010s, confounding our identification of LCR effects. Municipality-year fixed effects reduce bias from this confounder by absorbing factors that vary spatially and over time – including exposure to the oil boom and bust, which was strongest in municipalities near shipyards and offshore fields. Third, spillovers between treatment and control groups could violate the stable unit treatment value assumption (SUTVA). Indeed, we document spillovers between LC establishments and neighboring non-LC establishments with our shift-share design. To reduce bias from potential within-municipality, within-sector spillovers between treated LC establishments and matched controls, we estimate a robustness check wherein we flip the exact match on municipality in our matching procedure, thus restricting control establishments to those that match with treated establishments on all other criteria but are located in municipalities with no LC establishments at baseline, eliminating the possibility of local spillovers. Because treated and control establishments then never share a municipality, this specification replaces municipality-year fixed effects with year (or state-year) fixed effects. Appendix 6 reports the results, together with fixed-effects and functional-form robustness checks.

5 Results

5.1 Short-Term Responsiveness to LC Demand Shocks

Results showing direct effects of product-specific LC demand shocks on LC establishments are presented in Table 1. A 1% increase in product-specific LC demand in a given year is associated with LC establishments that produce that product increasing their employees' monthly earnings by 0.099%, total number of employees by 0.033%, and number of high-education employees by 0.029%. Low-education employment increases by 0.027%, though this effect is not statistically significant, as is the case for innovation-related employment (+0.004%). Positive demand shocks also make LC establishments significantly more likely to remain active in the market in that year (+0.010 percentage points).

Turning to outcomes from PIA, a 1% increase in product-specific LC demand is associated with 0.454% growth in firm profits and 0.030% growth in revenues. Effects on exporting

are positive and statistically significant, but economically small: a 1% demand increase raises the probability of exporting by +0.00002 percentage points (p.p.) and their exported share of output by 0.00057 p.p. We find no significant effects of short-term demand shocks on investment (which shows a negative but statistically insignificant coefficient estimate of -0.213) or TFP (+0.019). Appendix Table A2 reports additional PIA outcomes, including inventory stock, labor productivity, labor stock, and capital stock. We do not find any statistically significant effects on these outcomes, with the exception of labor stock, for which we find a marginally significant positive effect, consistent with the results from RAIS.

Table 1: Short-Term Local Demand Effects – RAIS and PIA

Panel A: RAIS	<i>Average Earnings</i>	<i>Employees (Total)</i>	<i>Employees (High-Ed)</i>	<i>Employees (Low-Ed)</i>	<i>Employees (Innov.)</i>	<i>Estab. Active</i>
Local Demand	0.099*** (0.034)	0.033* (0.020)	0.029** (0.012)	0.027 (0.019)	0.004 (0.009)	0.010*** (0.004)
Establishment FEs	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	38,324	38,324	38,324	38,324	38,324	38,324
Adjusted R-squared	0.441	0.678	0.746	0.695	0.753	0.435
Baseline DV Mean (asinh)	5.088	2.866	1.330	2.493	1.064	—
Baseline DV Mean	1645.5	142.7	18.2	87.4	8.9	0.617
Panel B: PIA	<i>Profit</i>	<i>Revenue</i>	<i>Investment</i>	<i>TFP</i>	<i>Export %</i>	<i>Export (0/1)</i>
Local Demand	0.454* (0.243)	0.0299* (0.0178)	-0.213 (0.168)	0.0192 (0.0166)	0.00057** (0.00023)	0.00002*** (0.000007)
Establishment FEs	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8412	8412	8412	8412	8412	8412
Adjusted R^2	0.308	0.917	0.225	0.393	0.709	0.731
Baseline DV Mean (asinh)	18.824	20.529	18.097	2.153	—	—
Baseline DV Mean	74,861k	411,599k	36,168k	4.25	7.91	0.52

Note: Standard errors clustered at the scope#year level for RAIS outcomes and scope level for PIA outcomes. The panel is fully balanced for employment outcomes measured from RAIS, and unbalanced for PIA due to the nature of PIA data collection. All variables in Panel A and B are transformed using the inverse hyperbolic sine function with the exception of Export Share and Export indicator, where level-level specifications are used.

In Appendix Table A3, we report analogous estimates including a quadratic term to assess non-linear effects of LC demand shocks. In RAIS, this quadratic term is statistically insignificant for all outcomes except high-education employment, where it is positive and significant, indicating that hiring of educated workers accelerates disproportionately in response to larger demand shocks. In PIA, the quadratic term is also significant and positive for TFP, suggesting that larger shocks may generate productivity gains.

5.2 Employment Spillovers

Results from our shift-share strategy are reported in Table 2. A one-standard-deviation increase in local content demand is associated with an approximately 11.5% increase in employment within LC establishments themselves (column 1), equivalent to roughly 165 workers evaluated at the baseline mean.¹² Turning to spillovers onto non-LC establishments, we find no evidence of net crowding-out: total non-LC employment is essentially unchanged (+0.6%, column 2, not statistically significant).¹³ Employment among establishments operating in the same 5-digit sector as LC establishments rises by 9.4% (column 3), but this coefficient is imprecisely estimated.

While spillover effects of LC demand on overall employment are null, effects on non-LC employment in oil-linked sectors are positive and significant. Establishments downstream of oil and gas extraction gain 4.9% (column 4), equivalent to roughly 161 workers at baseline, and establishments upstream gain 8.6% (column 5), equivalent to roughly 319 workers. These positive spillovers are consistent with demand-linkage and agglomeration benefits for establishments connected to the oil and gas supply chain.

5.3 Event Studies Around 2003 Policy Strengthening

We plot results from event studies around the 2003 strengthening of the LCR in Figure 5. Focusing first on employment, results indicate that LC establishments were on an upward employment trajectory relative to controls prior to 2003 – which violates the parallel pre-trends assumption and suggests that significant post-2003 differences in employment growth between LC establishments and matched control establishments are likely not fully attributable to the LCR. Nevertheless, these results can be interpreted descriptively, indicating that employment in LC establishments grew substantially more than in control establishments after 2003, reaching a peak of approximately 100 additional workers per establishment in 2012 before falling from 2014 onward.

Turning to intensive margin measures of skill and innovation intensity (average earnings,

¹²Coefficients are log points and are interpreted throughout as approximate percent changes. The exact transformation $100(e^\beta - 1)$ gives 12.2% for the direct LC effect, 5.0% downstream, and 8.9% upstream.

¹³Implied effects on number of workers (e.g., +165 workers in directly treated LC firms) are based on widely varying baseline employment magnitudes, and are not directly comparable.

Table 2: Shift-Share LC Demand Exposure Effects on Employment

Dependent Variables:	LC (log) (1)	Total (excl. LC) (log) (2)	LC CNAE Control (log) (3)	Downstream (log) (4)	Upstream (log) (5)
Model:					
LC demand (1 SD)	0.1151*** (0.0286)	0.0064 (0.0077)	0.0944 (0.0633)	0.0487** (0.0203)	0.0856*** (0.0276)
Baseline Mean (log)	5.82	10.24	5.90	6.85	6.97
Baseline Mean (count)	1,428.87	98,255.16	9,144.65	3,303.41	3,726.04
Fixed Effects: Year	Yes	Yes	Yes	Yes	Yes
Observations	740	740	740	740	740

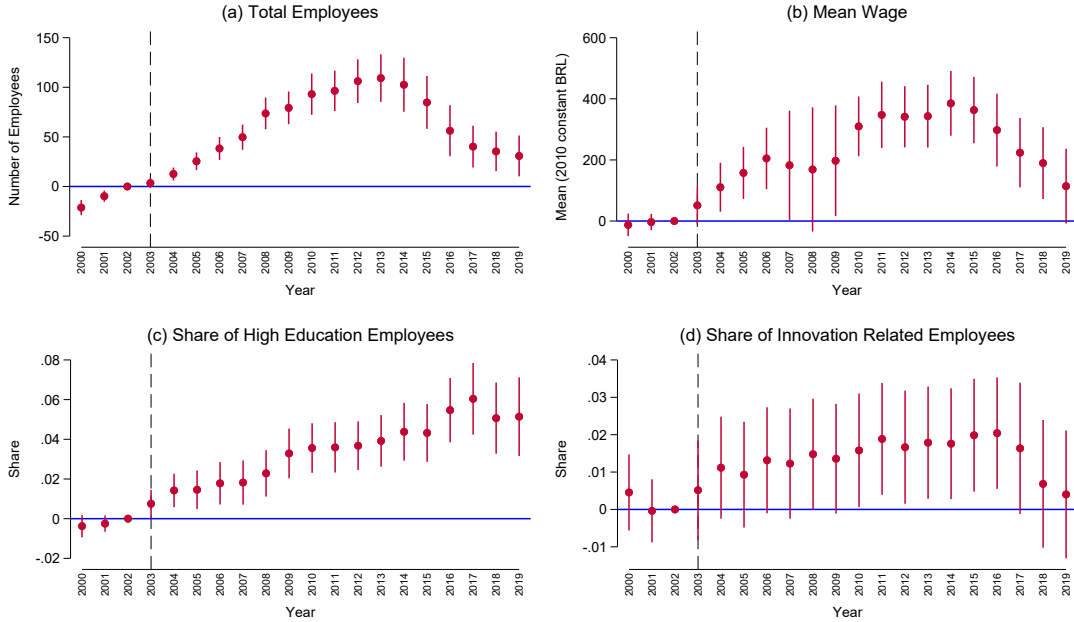
Notes: Regressions are estimated at the shock (scope×year) level following [Borusyak et al. \(2022\)](#). We residualize the original municipal-year level data controlling for municipality fixed effects, year-by-state fixed effects, the interaction of the time-invariant sum of municipal shares with year fixed effects, and a control for oil and gas royalties received by the municipality each year. The outcome is log-transformed, and coefficients are interpretable as percent changes in employment per one-standard-deviation increase in the local content demand shift-share. Standard errors clustered at the scope×year level are reported in parentheses. Column 1 restricts the sample to formal establishments registered as LC suppliers (direct effect); column 2 measures total municipal employment excluding LC establishments; column 3 measures employment in non-LC establishments operating in the same CNAE as treated LC establishments (potential competitors); columns 4 and 5 measure employment (excluding direct LC employment) in establishments downstream and upstream of the oil and gas extraction sector, respectively. Baseline Mean (log) is the unweighted average of $\log(Y)$ across municipalities in 2006; Baseline Mean (count) is the unweighted average of Y across municipalities in 2006. Appendix 5 reports the sensitivity of these estimates to alternative specifications and to a leave-one-scope-out exercise.

Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

high-education share, and innovation-intensive share), we find that pre-trends are parallel prior to 2003. After this year, LC establishments significantly increased their employees' average earnings (+32%, or +BRL\$363, by 2015), their high-education share of employment (+4.3 p.p. by 2015), and their innovation-intensive share of employment (+2.0 p.p. by 2015) relative to matched controls. Appendix Figure A5 further decomposes establishments' employment by occupational category, showing that LC establishments significantly increased their share of professional and technical employees after 2003, while their share of managerial employees remained flat – suggesting that observed skill upgrading reflects genuine increases in technical capacity rather than simply hiring additional managers to navigate the LCR's administrative requirements. All employment measures decline among LC establishments following the 2014 oil bust and subsequent 2017 LCR policy weakening, though they remain elevated relative to controls.

Turning to firm-level outcomes from PIA, we do not measure any systematic differences between participating LC firms and closely matched non-LC firms before or after the 2003 LCR strengthening. A handful of individual year-coefficients reach significance – profits fall in 2015 and 2017 (10% level), investment rises in 2008 and 2014, and export share rises weakly in 2018 and 2019 – but these are isolated and inconsistent in sign and timing, consistent with

Figure 5: Event Studies around 2003 LCR Strengthening (RAIS)



Note: Figure reports coefficient estimates with 95% confidence intervals. Standard errors are clustered at the establishment level. 2002 (the year preceding LCR strengthening) is the omitted reference year. Total Employees is estimated on a balanced panel, with zeros imputed for years in which an establishment is inactive. Mean Wage, Share of High Education Employees, and Share of Innovation Related Employees are estimated on an unbalanced panel restricted to years in which the establishment is active, since these outcomes are not well-defined otherwise. Results are from a TWFE regression with municipality-year fixed effects, exact matching on municipality and economic activity sector, and coarsened exact matching based on deciles of pre-treatment average wage, total employment, and total high-education employment.

the overall pattern of flat, insignificant effects. This suggests LC participation had a greater effect on employment-related variables than on other measures of firm performance such as profits, revenues, investment, TFP, or export participation.¹⁴

5.4 Robustness

In this section, we test the robustness of our results to alternative specifications, estimators, and variable transformations, with full results reported in Appendices 4, 5, and 6.

Product-specific demand shock estimates are broadly stable and robust across alternative

¹⁴The firm-level PIA sample is considerably smaller than our establishment-level RAIS sample, and LC status is measured with some noise at the firm level, since certification applies at the establishment level and we classify a firm as LC if any of its establishments are certified. These factors limit our ability to detect treatment effects if they exist. However, this is not a case of plausible treatment effects obscured by wide confidence intervals: post-2003 point estimates remain mostly flat around zero, rather than showing economically meaningful patterns obscured by imprecise bounds.

Figure 6: Event Studies around 2003 LCR Strengthening (PIA)

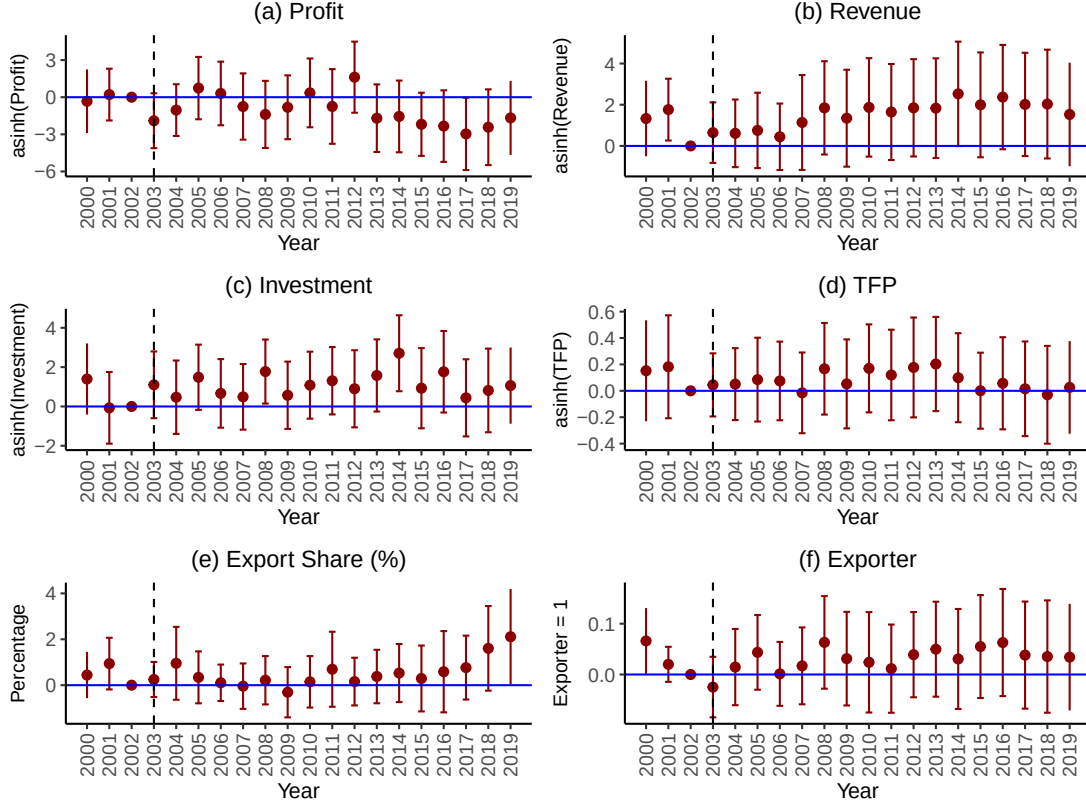


Figure reports coefficient estimates with 95% confidence intervals. Standard errors are clustered at the establishment level. All outcomes are estimated on an unbalanced panel restricted to years in which the establishment appears in PIA. Results are from a TWFE regression with municipality-year fixed effects, exact matching on municipality and economic activity sector, and coarsened exact matching based on deciles of pre-treatment wage bill and capital stock.

functional forms, fixed effects, and clustering choices. For RAIS outcomes, including a control for municipal oil royalties, clustering by establishment instead of scope-group-year, and alternative fixed effects leave conclusions essentially unchanged, though an untransformed level-level specification is noisier and loses significance for some outcomes (Appendix Table A10). PIA outcomes are similarly stable under alternative fixed effects, with the exception of the revenue response, which becomes insignificant, and the profit response, which strengthens (Appendix Table A11).

For the shift-share design, the leading concerns are whether the demand shifts are as good as random and how concentrated the identifying variation is. Appendix 5 examines both, following [Borusyak et al. \(2025\)](#). Balance tests show that of five predetermined scope characteristics, only scope size predicts the demand shifts, an association attributable to

the construction of the scaled shift. The only significant municipality-level pre-trend, for upstream employment, runs opposite in sign to the estimated positive effect. Because the effective number of product scopes is 17, and roughly 57% of identifying variation originates in 2017 and 2018, we re-estimate the model dropping one scope at a time, confirming that no significant coefficient changes sign under any omission of any one scope.

Event study results are insensitive to inclusion of alternative fixed effects, and the employment result grows more convincingly causal, not less, as we probe it further. Using the Poisson pseudo-maximum likelihood estimator rather than untransformed levels – which is likely more appropriate for a non-negative count like employment – the employment pre-trend disappears entirely. A related concern is that same-municipality controls are themselves exposed to the local spillovers we document in the shift-share analysis, which would bias our estimates toward zero. Appendix 6 addresses this directly by re-matching LC establishments to controls in municipalities with no LC presence at all. This spillover-free comparison again strengthens a causal interpretation of the employment result: the employment pre-trend disappears, and the post-2003 employment effect grows larger, consistent with positive employment spillovers onto neighboring control establishments attenuating our main estimates. Wage and high-education effects survive this spillover-free exercise as well, with only a modest pre-period wage gradient emerging. Innovation share becomes statistically insignificant, but remains positive and of roughly similar magnitude.

5.5 How Much of the LC Demand Shock Is Policy-Driven?

The direct, spillover, and long-run effects estimated above capture establishments’ responsiveness to *total* oil industry demand for their products, only part of which is attributable to the LCR itself. Oil companies would likely have sourced some inputs locally even absent the policy. Local content commitments in winning bids (Figure 1) provide the cleanest available benchmark for this counterfactual. Committed local shares averaged roughly 25% before the strengthening of the LCR in 2003, roughly 75% while high minimum requirements were binding, and returned to roughly 25% in the first auctions held after requirements were reduced in 2017. This immediate reversion, together with extensive qualitative evidence that oil companies protested the policy and would have preferred to source internationally

(Guimarães, 2012), indicates that a 25% local share reflects firms’ unconstrained sourcing preferences. For fields auctioned under the strengthened LCR, the additional 50 percentage points of committed local sourcing, or roughly two-thirds of the committed share, are therefore attributable to the policy.¹⁵ As a result, roughly two-thirds of our estimated effects for these fields plausibly reflect the policy itself, with the remainder attributable to local sourcing that would have occurred regardless.

5.6 Heterogeneity by Market Structure

The effect of competition on firm performance under Brazil’s LCR is an empirical question. On one hand, firms exposed to competitive domestic markets may fare better after LCR protections are lifted, while firms with local market power may struggle when suddenly faced with international competition. On the other hand, domestic competition could dilute the impacts of LC demand shocks among many firms, making it difficult for any individual firm to undertake major investments or achieve economies of scale.

To explore this question empirically, we compute baseline Herfindahl-Hirschman Indexes (HHI) for each product market included in the LCR. Specifically, for product category p , where $share_i$ is the market share (of total revenue) of establishment i in baseline year 2002, we calculate the HHI as follows:¹⁶

$$HHI_p^{2002} = \sum_{i=1}^N (share_i^{2002})^2 \quad (7)$$

¹⁵Because fields require exploration and development investments for years after their auction round (when local content commitments are fixed), the realized LC demand we observe in any given calendar year reflects a mix of commitment vintages rather than the commitment level in force at that moment. In the years just after 2003, aggregate demand still partly reflects fields auctioned before the LCR strengthening, whose lower ($\approx 25\%$) commitments continue to generate local and foreign procurement as those fields are developed, pulling the effective policy-attributable share below what the 75% commitment level for newly auctioned fields would suggest. Likewise, in the years after 2017, aggregate demand is still propped up by fields auctioned before the LCR weakening, whose higher ($\approx 75\%$) commitments remain in force for the life of the field. This keeps realized LC demand elevated even as new auctions revert to local content commitments around 25%. As a result, the policy-attributable share of aggregate demand does not track the prevailing commitment rule cleanly at either transition, but instead phases in and out gradually as older vintages of fields are replaced by newer ones.

¹⁶HHIs range from near 0 in the case of very competitive markets to 10,000 in the case of a pure monopoly. Antitrust regulators typically consider markets with an HHI greater than 1,800 to be concentrated (US Antitrust Division, 2024).

Next, we re-estimate Equations 1 and 6 interacting treatment with an indicator for high market concentration, defined as $\text{HHI} \geq 1800$ – a standard benchmark that also sits near the median HHI in our data.

Appendix Table A7 reports the short-term effects of LC demand shocks on LC establishments, interacted with an indicator for product-specific market concentration. Results indicate that effects of LC demand shocks are significantly stronger in concentrated markets. A 1% increase in demand for domestically-sourced products by the oil industry is associated with a 0.09% increase in average employee earnings among treated establishments, and an additional 0.23% increase if those treated establishments are in a concentrated market. Similarly, a 1% increase in LC demand is associated with a statistically insignificant 0.03% increase in employment for establishments in competitive markets, and an additional, statistically significant 0.11% increase for establishments in concentrated markets. This effect may be mechanical, given that total LC demand is dispersed among many establishments in competitive sectors and concentrated among just a few firms in concentrated sectors. Disproportionate responsiveness to LC demand shocks among firms in concentrated sectors suggests highly competitive markets may dilute the impacts of an LCR, preventing establishments from overcoming high fixed costs or achieving economies of scale. Appendix Table A8 reports analogous results for PIA outcomes, which again indicate that responsiveness to LC demand shocks was most concentrated among firms in concentrated markets.

Finally, we re-estimate event studies around the 2003 LCR strengthening separately for establishments in competitive versus concentrated product markets. Results are reported in Appendix Figure A7. LCR effects on wages, high-education, and innovation share are roughly similar for LC establishments in competitive versus concentrated product markets. In contrast, LC establishments in concentrated markets add significantly more employment after 2003 than do LC establishments in competitive product markets (both add significantly more jobs than do closely matched non-LC establishments). These results complement findings from Tables A7 and A8, suggesting establishments that faced less domestic competition may have benefited from greater shares of LC demand directed towards them by the LCR.

5.7 Political Engagement

Sector-specific industrial policies such as Brazil’s LCR can generate rents and lead to formation of special interest groups with strong incentives to prolong or deepen sectoral protections, whether or not these measures benefit society as a whole (Hansen and Therkildsen, 2016). This is an example of concentrated benefits and dispersed costs, which can distort public resource allocation and create deadweight loss (Knight, 2004).¹⁷

To assess LC establishments’ political engagement, we exactly match establishments with the universe of campaign donations made to Brazilian political parties and candidates between 2002 and 2014.¹⁸ Using these data, we regress donation-related outcomes Y_{it} in establishment i in year t on state and year fixed effects (δ_s and γ_t , respectively), an indicator for whether i is LC-certified (LC_i), and a vector of establishment covariates X_{it} , including number of total employees, share of high education employees, share of female employees, and average wage:

$$Y_{it} = \delta_s + \gamma_t + \beta LC_i + \mathbf{X}_{it}'\mu + \epsilon_{ist} \quad (8)$$

Within the sample of all formally registered establishments with at least one employee, we first assess whether LC establishments are more likely to make a campaign donation. Next, conditional on donating, we assess whether LC establishments donate larger values, and whether a larger share of their donations is directed to the Workers’ Party (PT), which was the political party most associated with support for the LCR (Schutte, 2021). We conduct the analysis on two samples: (i) a matched sample including LC establishments and all other establishments that exactly matched on municipality, 5-digit industry code, and quintiles of baseline number of employees, average wage, and average high-education share of employment; (ii) all establishments.

Results are reported in Appendix Table A9. Conditional on state and year fixed effects

¹⁷In Brazil, the National Syndicate of Construction and Repair of Ships and Offshore Equipment (SINAVAL), an association of upstream producers for the oil and gas industry, has been a vocal advocate for higher local content percentages, inclusion of additional products in LC requirements, and continuation of local content protections (Oliveira, 2023).

¹⁸Campaign donation data are available from 2002 onward. However, Brazil’s Supreme Court banned corporate campaign donations in 2015, preventing analysis of establishment-linked donations in subsequent years. The donation dataset thus runs from 2002-2014.

and establishment-level controls, LC establishments are 37.9% more likely to make a campaign donation relative to comparable (matched) non-LC establishments. Conditional on making a donation, LC establishments donate 50.1% higher real values and donate a 70.3% higher share of their donations to the Workers' Party. These findings indicate that establishments benefiting from the LCR are politically active and provide disproportionate backing to the political party that supports their sectoral policy.¹⁹

6 Potential Costs: LCR Impacts on the Oil Industry

The three empirical strategies reported above identify direct and spillover effects of the LCR on domestic establishments, but they do not speak to potential deadweight loss the LCR may have imposed. Mandating local sourcing when domestic suppliers were not the lowest-cost or highest-quality option may have delayed offshore oil and gas projects and displaced FDI – as oil companies operating in Brazil consistently argued (Guimarães, 2012). Delayed or reduced oil and gas production would in turn imply reduced royalty revenues, which fund public investments and provision of public goods like health and education.

Quantifying these indirect costs is difficult: pre/post comparisons around LCR strengthening or weakening are confounded by sector-wide trends, and LC percentage requirements generally apply across the board, leaving no plausible control group. However, in Concession Rounds 9 through 13 (2007–2015) of Brazil's oil and gas licensing, oil companies submitted LC% commitments for the exploration and development phases of each block-level contract as part of their bids, which were contractually binding. These commitments varied substantially across blocks, even within the same bidding round.²⁰ Comparing exploration, development, and production outcomes between blocks with low and high LC% commitments, conditional on a rich set of bid, round, and block characteristics, allows us to assess whether higher LC commitments are associated with delayed or reduced oil production. Full

¹⁹These comparisons are descriptive, not causal. LC certification is not randomly assigned and we do not exploit changes in political support over time. However, the pattern is consistent with concerns in the literature that sector-specific industrial policies can foster political engagement by beneficiaries seeking to deepen or prolong the policy benefiting them.

²⁰As shown in Appendix Figure A13, block-level exploration LC commitments for Rounds 9-13 ranged from 37-80% with a mean of 69%, while development LC commitments ranged from 55-85%, with a mean of 79%.

details on data sources (ANP, 2026), dataset construction, and empirical specifications are reported in Appendix 7.

We begin with OLS panel regressions of block-year outcomes on LC% commitment (Appendix Table A15). Coefficients measure the effect of a one percentage point increase in LC% commitment. For exploration, measured by exploratory wells drilled per year, the effect is indistinguishable from zero in the bivariate specification, as well as after inclusion of round, year, basin, depth category, and sector (onshore/offshore) fixed effects with bid-level controls (cash bonus, auction score, committed exploration program size, and block area), and becomes positive and significant with operator fixed effects (0.006, $p < 0.01$). For development, using the development-phase LC% commitment, the number of discovery assessment plans reported for a block is positively associated with the LC% commitment, with estimates of 0.0004 ($p < 0.10$) once round fixed effects are included, 0.0017 ($p < 0.05$) with full controls, and 0.0017 ($p < 0.01$) with operator fixed effects. LC% has a positive association with the number of development wells reaching first production in every specification. The effect of LC% on production, measured as the inverse hyperbolic sine of oil production, is positive and statistically insignificant across all specifications except that with the operator fixed effect, where it becomes significant (0.018, $p < 0.10$). Poisson models tell a similar story: incidence-rate ratios for exploration and production exceed 1 and are statistically insignificant, while development estimates are positive but imprecise given sparse outcomes (Appendix Table A16). Overall, we find no evidence that higher LC% commitments are associated with reduced oil exploration, development, or production.

To study delays directly, we estimate Cox Proportional Hazards models of time to discovery, commerciality, and first production, where hazard ratios above one indicate faster progress (Appendix Table A17). Higher exploration-phase LC% commitments are not associated with delays at any milestone, while higher development-phase commitments are associated with a significantly *faster* time to first production (hazard ratio 1.17, $p < 0.01$). These models rely on only a few events (85 discoveries, 42 commerciality declarations, and 43 first-production events), resulting in imprecise estimates. We interpret them as showing no statistically significant evidence of delays associated with higher LC% commitments.

Finally, data on LC% compliance inspections suggest that most minimum LC% commit-

ments did not bind. Among the 160 Round 9–13 blocks with a completed exploration-phase inspection, realized local content averaged 87%, relative to an average bid-stage commitment of 73%. Eighty six percent of blocks met or surpassed their original bid-stage commitment, with 21 blocks achieving 100% local sourcing (Appendix Figure A15). Only 22 blocks (14%) were fined for not meeting their commitments within specific sub-categories, even though most of these exceeded their overall LC% commitment.

We interpret these results as suggestive, correlational evidence that higher LC% commitments did not measurably reduce or delay exploration, development, or production, despite frequent complaints from oil companies to the contrary. We acknowledge several limitations. First, LC% commitments are endogenous and could reflect unobserved block or supplier characteristics. We mitigate this omitted variable bias by including block-level covariates and round, year, and operator fixed effects. Commitments are binding and set in the initial bid, prior to exploratory drilling, so they cannot reflect subsequent discoveries or detailed geological assessments. Second, later-stage outcomes such as commerciality and production are observed only for blocks that progress to those stages, which could introduce survivor bias. We address this by using a balanced panel and imputing missing outcomes as zero where appropriate. LC% compliance is observed only for blocks that completed exploration, so we treat it as descriptive. Third, our sample only covers ANP concession bidding rounds 9-13. Several earlier and later rounds were also subject to the LCR, but their LC% levels were fixed by round and thus offer no variation to exploit. Results from concession rounds 9-13 may therefore not generalize. These rounds were dominated by onshore and shallow-water blocks, and none of their deep and ultra-deep blocks had produced by 2023. They also exclude the large pre-salt blocks licensed under ANP’s production-sharing rounds 1-6, which account for much of Brazil’s recent offshore oil production. If these large ultra-deepwater projects require more sophisticated or larger-scale inputs, the LCR could have a larger impact than in rounds 9-13. Consistent with this, [Clavijo et al. \(2019\)](#) simulate cash-flow for a representative pre-salt project under production sharing and find that high LC% commitments can compromise project profitability when oil prices are low.

7 Discussion

We contribute policy-relevant empirical evidence on the direct and spillover effects of a local content requirement for Brazil’s oil and gas sector. Overall, the LCR succeeded in generating employment and skill upgrading, at least as long as minimum local content shares remained high, but did less to foster sustained improvements in firm performance. In the short run, demand shocks attributable to the LCR raised employment, wages, revenues, and profits among participating establishments. Over the long run, employment, wages, and the share of educated and innovative workers all grew relative to matched controls, while profits, revenue, investment, TFP, labor productivity, and export propensity did not. The policy’s reach also extended beyond the establishments it targeted: LC demand raised employment among neighboring establishments both upstream and downstream of the oil industry, with no evidence of net crowding-out. Furthermore, we find no evidence that higher LC% commitments by oil companies were associated with delayed or reduced oil exploration, development, or production, though this finding is correlational and encompasses only the subset of blocks licensed under the concession regime between 2007-2015.

Ultimately, the trajectory of local content commitments points to the same conclusion: Brazil’s LCR created skilled jobs but did not durably improve local suppliers’ competitiveness. LC% commitments in winning bids rose from roughly 25% of total sourcing before 2003 to roughly 75% while high minimum requirements were binding, then reverted to roughly 25% after requirements were eased in 2017. Had 14 years of policy protection built local suppliers into globally competitive firms or fostered supplier relationships and investments durable enough to survive on their own, oil companies would have had little reason to revert to their pre-policy sourcing commitments the moment they were free to choose. This is consistent with our finding of no measurable gains in investment or productivity among LC firms over this period. Our findings have several implications for policymakers:

Job creation and firm upgrading. First, LCRs can contribute meaningfully to job creation – including high-education and innovative jobs – which is a policy priority for developing countries ([World Bank, 2012](#)). But achieving firm upgrading along non-employment dimensions appears to require more than mandating local content alone. Governments may

need to actively support upgrading through training, facilitated technology transfer, or joint ventures with foreign firms, alongside clear and transparent criteria for continued participation or phase-out tied to performance targets such as R&D, capital expenditure, technology adoption, export success, or on-time, cost-competitive deliveries.

Sector selection. Our finding of positive employment spillovers onto upstream and downstream establishments suggests that LCR-driven demand can diffuse through regional supply chains. This is encouraging evidence relative to common concerns about Dutch Disease, suggesting upstream-focused LCRs may counteract crowding out of tradeable sectors during a resource boom. Furthermore, sector selection is likely to determine policy impact: LCRs may be more likely to succeed where (i) investment opportunities are sufficiently attractive or investors have sufficiently limited outside options to simply shift their investments to another country (Bosker et al., 2025), and (ii) demand for locally-sourced inputs can plausibly generate learning-by-doing or scale economies. Policymakers should also weigh sectors' long-run growth potential: the oil industry generated substantial demand for local inputs during our study period, but faces stranded-asset risk in the future as the energy transition proceeds (van der Ploeg and Rezai, 2020).

Implementation, administration, and evaluation. Where Brazil's LCR was most successful – skilled job creation – it was accompanied by supporting policies such as PROMINP, which expanded technical training in oil-related skills and ensured a domestic workforce was available as firms scaled up. At the same time, Brazil's approach to certifying and regulating local content was administratively intensive, requiring substantial state capacity and effectively limiting participation to larger, more established firms. Streamlined certification procedures could lower program costs and broaden participation to smaller suppliers. Finally, governments should build in mechanisms for cost-benefit evaluation from the outset. This need not impose additional reporting burdens on firms: as our own analysis shows, linking certification records to existing administrative data can enable real-time tracking, improving decisions about whether to continue, adjust, or phase out the policy.

Impacts on FDI. A common concern is that LCRs may deter foreign investment. In Brazil's concession rounds 9-13, which covered mostly onshore and shallow-water blocks, we find no evidence that higher LC% commitments reduced oil exploration, development, or

production. In contrast, [Clavijo et al. \(2019\)](#) use cash-flow simulations to suggest that high LC% commitments may compromise the profitability of ultra-deepwater projects, where required investments and inputs were larger and more technologically advanced. These contrasting findings suggest that the degree to which LCRs deter FDI depends on mismatch between local supplier capabilities and industry requirements. LCRs contribute little where domestic suppliers are already fully capable, and impose prohibitive costs on investors where domestic suppliers lack any capacity to meet demand. These policies are likely most effective when targeted toward suppliers at the margin, where demand stimulus can feasibly enable domestic firms to scale and upgrade without deterring FDI.

Overall, Brazil’s local sourcing requirement is one variant of an LCR – a policy family that can also include requirements for local downstream processing, asset ownership, employment, or technology transfer. The proliferation of these policies across low-, middle-, and high-income countries alike reflects widely shared goals of promoting local industry and employment. However, as our results show, LCRs are complex instruments with effects that depend on how, where, and how long they are applied, underscoring the need for rigorous empirical evidence to guide their design.

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Online Appendix

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1 PIA Variable Description

In this section, we describe variables derived from Brazil’s Pesquisa Industrial Anual - Empresa (PIA-Empresa), an annual firm-level survey conducted by the Instituto Brasileiro de Geografia e Estatística (IBGE). PIA-Empresa covers all industrial establishments with 30 or more employees and representative samples of smaller firms. We construct a firm-level panel from PIA-Empresa covering 2000-2019. Each firm is identified by its unique firm ID (CNPJ), which is encrypted by IBGE staff to preserve anonymity. The panel is unbalanced due to the nature of PIA data collection: firms enter when they cross the 30-employee threshold and exit when they fall below it or cease operations.

Profit

$$\text{Profit}_{it} = \text{Revenue}_{it} - \text{Total Costs}_{it} \quad (9)$$

where i and t are firm and year subscripts, respectively. Revenue corresponds to PIA variable X13 (total net revenue from industrial activity, including sales of industrial products and industrial services provided to third parties, net of taxes, returns, and discounts) and Total Costs corresponds to X33 (total industrial costs, including labor, materials, energy, and other operating expenses).

Value Added (directly reported in PIA as X78)

$$\text{Value Added}_{it} = \text{Gross Output}_{it} - \text{Intermediate Consumption}_{it} \quad (10)$$

Investment (gross fixed capital formation)

$$\text{Investment}_{it} = \text{Acquisitions}_{it} + \text{Own Construction}_{it} + \text{Improvements}_{it} - \text{Sales}_{it} \quad (11)$$

where the components are:

- VC085: Acquisitions of machinery, equipment, and vehicles
- VC090: Own construction of buildings and structures
- VC096: Improvements to existing fixed assets
- VC102: Sales and disposals of fixed assets

Export Orientation We construct two measures of export orientation:

Export Share: The proportion of revenue derived from exports, calculated as:

$$\text{Export Share}_{it} = \frac{\text{Direct Exports}_{it} + \text{Indirect Exports}_{it}}{\text{Total Revenue}_{it}} \quad (12)$$

where Direct Exports (VC024) are sales directly to foreign markets and Indirect Exports (VC025) are sales to domestic trading companies for export.

Export Indicator: A binary variable equal to 1 if the establishment has any export activity (Export Share > 0) and 0 otherwise.

Inventory Stock

$$\text{Inventory}_{it} = \text{Raw Materials}_{it} + \text{Work in Progress}_{it} + \text{Finished Goods}_{it} \quad (13)$$

using variables VS036, VS037, and VS038, respectively. All values are measured at year-end book value.

Total Factor Productivity (TFP) We estimate firm-level TFP following the methodology developed by [Mendonça et al. \(2020\)](#) for PIA-Empresa. The calculation assumes a Cobb-Douglas production function and proceeds in three steps: constructing labor stock, capital stock, and computing TFP.

Labor Stock Labor stock L_{ft} is defined as the total value of salaries and benefits paid to establishment f 's workers in year t :

$$L_{ft} = \text{Total Labor Costs}_{ft} \quad (14)$$

This is constructed as:

$$L_{ft} = \text{Salaries}_{ft} + \text{Benefits}_{ft} \quad (15)$$

where Salaries (VC034) includes wages paid to production workers and Benefits (VC039) includes wages paid to administrative, technical, and managerial workers, plus all social contributions, benefits, and other labor-related expenses.

Capital Stock Capital stock is constructed using the perpetual inventory method (PIM). For establishments present in the base year 2000, or for establishments entering the sample after 2000, initial capital stock K_{f0} is defined as:

$$K_{f0} = \text{Fixed Assets}_{f0} + \text{Inventory Stock}_{f0} \quad (16)$$

where:

- **Fixed Assets (VC103):** Book value of tangible fixed assets, including machinery, equipment, vehicles, buildings, land, and other physical capital
- **Inventory Stock:** Sum of raw materials (VS033), work-in-progress (VS034), and finished goods (VS035)

For establishments appearing after 2000, we use the first year of appearance in PIA as the baseline year t_0 .

Capital stock in subsequent years evolves according to:

$$K_{ft} = (1 - \delta)K_{f,t-1} + I_{ft} \quad (17)$$

where $\delta = 0.10$ is the (assumed) annual depreciation rate and I_{ft} is investment.

TFP Calculation We calculate establishment f 's TFP in year t as:

$$TFP_{ft} = \frac{VA_{ft}}{K_{ft}^{\alpha} L_{ft}^{1-\alpha}} \quad (18)$$

where VA_{ft} is value added (X78), and $\alpha = 0.4$ is the (assumed) capital share, implying a labor share of $1 - \alpha = 0.6$ (Mendonça et al., 2020).

Derived Variables We construct several ratio variables for robustness analyses:

- **Profit Rate:** $\text{Profit}_{it}/\text{Revenue}_{it}$
- **Inventory Rate:** $\text{Inventory}_{it}/\text{Revenue}_{it}$
- **Investment Rate:** $\text{Investment}_{it}/\text{Revenue}_{it}$
- **Labor Productivity:** $\text{Value Added}_{it}/\text{Labor Stock}_{it}$

Data Availability PIA-Empresa microdata contain potentially sensitive confidential information about firms. Access to the restricted microdata is available through IBGE's Sala de Acesso a Dados Restritos (Restricted Data Room), located at IBGE headquarters in Rio de Janeiro, Brazil. To access PIA-Empresa microdata, researchers must complete:

1. **Project Submission:** Submit a research proposal to IBGE detailing the study's objectives, methodology, specific data requirements, and justification for using restricted microdata.
2. **Technical Review:** The proposal undergoes evaluation by IBGE specialists who assess its scientific relevance, feasibility, and compliance with ethical and legal standards for data confidentiality.
3. **Confidentiality Agreement:** Upon approval, researchers must sign a confidentiality agreement committing to: (i) use data strictly for the approved research purpose, (ii) follow IBGE's security and privacy policies, and (iii) make no attempts to re-identify individuals or organizations.
4. **Controlled Access:** Research is conducted exclusively within IBGE's Restricted Data Room under staff supervision. The environment has no internet access and prohibits external storage devices. Statistical software (Stata, R) and required packages must be requested in advance and are installed by IBGE staff.
5. **Output Review:** All statistical results, tables, and figures must undergo IBGE's disclosure review before extraction. IBGE verifies that outputs contain no individually identifiable information and maintain data confidentiality standards.
6. **Publication Clearance:** Researchers may publicly release findings only after receiving final approval from IBGE's disclosure review.

There are no restrictions based on researcher nationality. Access costs vary by project duration and characteristics but typically range from US\$200-500. Additional information and application materials are available at: <https://www.gov.br/pt-br/servicos/solicitacao-de-acesso-a-sala-de-dados-restritos>

IBGE provides a synthetic version of PIA-Empresa at <https://www.ibge.gov.br/acesso-informacao/sala-de-acesso-a-dados-restritos.html>, which allows researchers to understand available variables and develop analysis code in advance, minimizing time required in the Restricted Data Room.

All results presented in this paper were produced in accordance with IBGE's confidentiality protocols and have received clearance through IBGE's disclosure review process.

2 Supplementary Tables

Table A1: Keywords for Identifying Innovation-Related Occupations

English	Portuguese
Research and development	Pesquisa e desenvolvimento
Innovation	Inovação
R&D	P&D
Product development	Desenvolvimento de produtos
Process development	Desenvolvimento de processos
Researcher	Pesquisador
New technologies	Novas tecnologias
Prototypes	Protótipos
Technological research	Pesquisas tecnológicas
Process automation	Automação de processos
Develop products	Desenvolvem produtos
Develop research	Desenvolvem pesquisas
Develop equipment	Desenvolvem equipamentos
Develop processes	Desenvolvem processos
Develop devices	Desenvolvem dispositivos
Optimize methods	Otimizam métodos
Optimize means	Otimizam os meios
Improve systems	Aperfeiçoam sistemas
Improve processes	Aperfeiçoam processos
Improve products	Aperfeiçoam produtos
Improve devices	Aperfeiçoam dispositivos
Implement automation devices	Implementam dispositivos de automação
Develop, test and supervise systems	Desenvolvem, testam e supervisionam
Processes and production methods	Sistemas, processos e métodos produtivos
New product	Produto novo / Novo produto
New products	Produtos novos / Novos produtos
Improved product	Produto aprimorado
Improved products	Produtos aprimorados
Product innovation	Inovação de produto
Product improvement	Aperfeiçoamento de produto
New process	Processo novo / Novo processo
New processes	Processos novos / Novos processos
Improved process	Processo aprimorado
Improved processes	Processos aprimorados
Process innovation	Inovação de processo
Process improvement	Aperfeiçoamento de processo

Notes: This table lists keywords used to identify innovation-related occupations. We perform text analysis on detailed occupation descriptions from Brazil’s *Quadro Brasileiro de Qualificações* (QBQ; Brazilian Qualifications Framework), which provides comprehensive text descriptions and skill requirements for 1,901 occupations specified in the *Classificação Brasileira de Ocupações* (CBO; Brazilian Classification of Occupations), Brazil’s equivalent of O*NET. Keywords are searched (case-insensitive, with accents removed) in three text fields for each occupation: (1) *Descrição Sumária* (summary description), (2) *Condições Gerais de Exercício* (general conditions of practice), and (3) *Formação e Experiência* (training and experience requirements). An occupation is classified as innovation-related if any of these keywords appear in its CBO description. We combine these occupation-level classifications with establishment-level employment data from RAIS to construct establishment-year measures of innovation intensity. This approach improves upon the methodology described in [Bustos et al. \(2022\)](#), who use less-detailed descriptions for approximately 700 aggregated occupation families in the CBO.

Table A2: Additional Short-Term Local Demand Effects — PIA

	<i>Inventory Stock</i>	<i>Labor Productivity</i>	<i>Labor Stock</i>	<i>Capital Stock</i>
Local Demand	−0.00914 (0.0209)	0.0722 (0.0895)	0.0299* (0.0163)	−0.0491 (0.112)
Establishment FEs	Yes	Yes	Yes	Yes
Municipality-Year FEs	Yes	Yes	Yes	Yes
Observations	8,412	8,412	8,412	8,412
Adjusted R^2	0.835	0.484	0.986	0.716
Baseline DV Mean (asinh)	10.601	12.122	17.863	19.247
Baseline DV Mean	20.1k	91.9k	28,626k	114,237k

Note: The table reports firm-level PIA estimates of the effect of contemporaneous product-specific local demand. Local Demand and all dependent variables are transformed using the inverse hyperbolic sine function. All specifications include establishment and municipality-year fixed effects. Standard errors, clustered at the scope-year level, are reported in parentheses. The PIA panel is unbalanced because firms enter and exit the survey sample. Baseline dependent-variable means refer to treated firms in 2006; “k” denotes thousands of constant 2010 BRL. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Local Demand Effects, Quadratic Specification — RAIS and PIA

Panel A: RAIS	<i>Average Earnings</i>	<i>Employees (Total)</i>	<i>Employees (High-Ed)</i>	<i>Employees (Low-Ed)</i>	<i>Employees (Innov.)</i>	<i>Estab. Active</i>
Local Demand (Centered)	0.078* (0.041)	0.046** (0.023)	0.052*** (0.014)	0.034 (0.021)	0.009 (0.011)	0.007 (0.005)
Local Demand (Centered ²)	−0.005 (0.005)	0.003 (0.003)	0.006*** (0.002)	0.002 (0.003)	0.001 (0.001)	−0.001 (0.001)
Establishment FEs	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	38,324	38,324	38,324	38,324	38,324	38,324
Adjusted R-squared	0.441	0.678	0.746	0.695	0.753	0.435
Panel B: PIA	<i>Profit</i>	<i>Revenue</i>	<i>Investment</i>	<i>TFP</i>	<i>Export %</i>	<i>Export (0/1)</i>
Local Demand (Centered)	0.374 (0.253)	0.0233* (0.0116)	−0.289* (0.168)	0.0307** (0.0154)	0.000788*** (0.000205)	0.0000210*** (0.00000557)
Local Demand (Centered ²)	−0.0291 (0.0359)	−0.00241 (0.00165)	−0.0275 (0.0238)	0.00417* (0.00218)	−0.0000000203 (0.0000000147)	0.000000000109 (0.000000000400)
Establishment FEs	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8412	8412	8412	8412	8412	8412
Adjusted R^2	0.311	0.917	0.228	0.396	0.711	0.732

Note: Standard errors clustered at the scope#year level for RAIS outcomes and scope level for PIA outcomes. The panel is fully balanced for employment outcomes measured from RAIS, and unbalanced for PIA due to the nature of PIA data collection. All variables in Panel A and B are transformed using the inverse hyperbolic sine function with the exception of Export Share and Export indicator, where level-level specifications are used.

Table A4: Event-Study Estimates Around the 2003 LCR Strengthening (RAIS)

	(1) <i>Total Em- ployees</i>	(2) <i>Mean Wage</i>	(3) <i>Share High-Ed</i>	(4) <i>Share Innov.</i>
2000	-21.235*** (3.914)	-12.656 (18.699)	-0.004 (0.003)	0.005 (0.005)
2001	-9.771*** (2.868)	-3.370 (13.737)	-0.002 (0.002)	-0.000 (0.004)
2002	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
2003	3.465 (2.577)	51.159 (31.795)	0.008** (0.004)	0.005 (0.007)
2004	12.627*** (3.319)	110.580*** (40.786)	0.014*** (0.004)	0.011 (0.007)
2005	25.473*** (4.558)	157.545*** (43.380)	0.015*** (0.005)	0.009 (0.007)
2006	38.318*** (5.885)	204.980*** (51.256)	0.018*** (0.005)	0.013* (0.007)
2007	49.723*** (6.466)	182.536** (91.194)	0.018*** (0.006)	0.012 (0.008)
2008	73.655*** (8.163)	168.774 (103.560)	0.023*** (0.006)	0.015* (0.008)
2009	79.286*** (8.382)	197.247** (92.351)	0.033*** (0.006)	0.014* (0.007)
2010	93.092*** (10.614)	309.753*** (49.924)	0.036*** (0.006)	0.016** (0.008)
2011	96.457*** (10.395)	347.462*** (55.383)	0.036*** (0.006)	0.019** (0.008)
2012	106.178*** (11.222)	341.194*** (51.034)	0.037*** (0.006)	0.017** (0.008)
2013	109.286*** (12.226)	343.165*** (52.412)	0.039*** (0.007)	0.018** (0.008)
2014	102.586*** (13.895)	385.153*** (54.057)	0.044*** (0.007)	0.018** (0.008)
2015	84.758*** (13.578)	363.252*** (55.341)	0.043*** (0.007)	0.020*** (0.008)
2016	56.254*** (13.120)	297.803*** (60.761)	0.055*** (0.008)	0.020*** (0.008)
2017	40.204*** (10.735)	223.726*** (57.946)	0.060*** (0.009)	0.016* (0.009)
2018	35.391*** (10.138)	189.629*** (59.947)	0.051*** (0.009)	0.007 (0.009)
2019	30.842*** (10.512)	114.126* (62.449)	0.051*** (0.010)	0.004 (0.009)
Baseline dep. var. mean	34.402	1151.542	0.048	0.090
Observations	867715	588872	588872	588872
Adj. R-squared	0.696	0.801	0.739	0.620
Within R-squared	0.009	0.001	0.001	0.000

Note: Table reports the coefficients underlying the event-study figure in Figure 5, estimated from Equation 6 with establishment and municipality-year fixed effects and CEM weights. The omitted base year is 2002; standard errors, clustered by establishment, are in parentheses. The matched sample pairs LC establishments with controls in the same municipality and 5-digit sector, within deciles of 2000–2002 average wage, employment, and high-education share. Total Employees is estimated on a balanced panel with zeros imputed for missing establishment-years; the remaining outcomes are estimated on the sample of active establishments. Baseline dep. var. mean is the CEM-weighted 2002 mean of the matched sample. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: Event-Study Estimates — Occupational Shares (RAIS)

	(1) <i>Share Profes- sional</i>	(2) <i>Share Manager</i>	(3) <i>Share Techni- cian</i>	(4) <i>Share Worker</i>
2000	-0.005 (0.004)	0.000 (0.006)	-0.003 (0.008)	0.010 (0.009)
2001	-0.000 (0.003)	-0.001 (0.004)	-0.008 (0.006)	0.012* (0.007)
2002	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
2003	-0.001 (0.005)	-0.007 (0.007)	0.021** (0.010)	-0.015 (0.012)
2004	0.002 (0.005)	-0.006 (0.007)	0.023** (0.010)	-0.020* (0.012)
2005	0.004 (0.005)	-0.007 (0.007)	0.021* (0.010)	-0.018 (0.012)
2006	0.008 (0.006)	-0.007 (0.007)	0.028*** (0.011)	-0.030** (0.012)
2007	0.009* (0.005)	-0.008 (0.008)	0.028** (0.011)	-0.029** (0.013)
2008	0.011** (0.005)	-0.010 (0.007)	0.031*** (0.011)	-0.034*** (0.013)
2009	0.014** (0.006)	-0.008 (0.007)	0.034*** (0.011)	-0.042*** (0.013)
2010	0.017*** (0.006)	-0.012* (0.007)	0.042*** (0.011)	-0.048*** (0.013)
2011	0.018*** (0.006)	-0.011 (0.007)	0.043*** (0.011)	-0.051*** (0.013)
2012	0.018*** (0.006)	-0.008 (0.007)	0.046*** (0.011)	-0.056*** (0.013)
2013	0.018*** (0.006)	-0.004 (0.007)	0.049*** (0.011)	-0.063*** (0.013)
2014	0.018*** (0.006)	-0.002 (0.007)	0.052*** (0.011)	-0.067*** (0.013)
2015	0.019*** (0.007)	-0.001 (0.007)	0.060*** (0.011)	-0.078*** (0.014)
2016	0.023*** (0.007)	-0.002 (0.007)	0.057*** (0.011)	-0.078*** (0.014)
2017	0.020*** (0.008)	-0.002 (0.008)	0.054*** (0.012)	-0.072*** (0.015)
2018	0.013* (0.007)	-0.004 (0.008)	0.050*** (0.012)	-0.060*** (0.014)
2019	0.010 (0.008)	-0.005 (0.008)	0.039*** (0.013)	-0.045*** (0.015)
Baseline dep. var. mean	0.047	0.079	0.198	0.649
Observations	588872	588872	588872	588872
Adj. R-squared	0.604	0.468	0.611	0.645
Within R-squared	0.000	0.000	0.000	0.001

Notes: Table reports the coefficients underlying the event-study figure in Figure A5, estimated from Equation 6 with establishment and municipality-year fixed effects and CEM weights. The omitted base year is 2002; standard errors, clustered by establishment, are in parentheses. The matched sample pairs LC establishments with controls in the same municipality and 5-digit sector, within deciles of 2000–2002 average wage, employment, and high-education share. All four outcomes — the shares of professional, manager, technician, and worker occupations in establishment employment — are estimated on the sample of active establishments. Baseline dep. var. mean is the CEM-weighted 2002 mean of the matched sample. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: Event-Study Estimates Around the 2003 LCR Strengthening — PIA Outcomes

	(1) Profit	(2) Revenue	(3) Investment	(4) TFP	(5) Export Share (%)	(6) Exporter
2000	-0.328 (1.314)	1.331 (0.935)	1.397 (0.923)	0.152 (0.195)	0.443 (0.514)	0.066** (0.033)
2001	0.212 (1.067)	1.763** (0.765)	-0.070 (0.930)	0.182 (0.199)	0.940 (0.574)	0.020 (0.018)
2002	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
2003	-1.901* (1.132)	0.646 (0.749)	1.101 (0.866)	0.045 (0.122)	0.248 (0.390)	-0.025 (0.030)
2004	-1.042 (1.066)	0.612 (0.837)	0.471 (0.952)	0.051 (0.139)	0.953 (0.810)	0.015 (0.038)
2005	0.737 (1.285)	0.754 (0.933)	1.486* (0.846)	0.085 (0.162)	0.342 (0.577)	0.044 (0.037)
2006	0.303 (1.311)	0.445 (0.823)	0.665 (0.890)	0.075 (0.152)	0.105 (0.405)	0.001 (0.032)
2007	-0.753 (1.366)	1.139 (1.175)	0.490 (0.852)	-0.015 (0.156)	-0.043 (0.505)	0.017 (0.039)
2008	-1.392 (1.384)	1.848 (1.155)	1.775** (0.831)	0.167 (0.177)	0.215 (0.537)	0.063 (0.046)
2009	-0.814 (1.316)	1.346 (1.200)	0.570 (0.873)	0.053 (0.172)	-0.306 (0.559)	0.031 (0.047)
2010	0.350 (1.417)	1.871 (1.219)	1.082 (0.869)	0.170 (0.170)	0.146 (0.572)	0.024 (0.050)
2011	-0.747 (1.539)	1.647 (1.189)	1.308 (0.874)	0.120 (0.175)	0.695 (0.834)	0.011 (0.044)
2012	1.618 (1.466)	1.851 (1.205)	0.899 (1.001)	0.177 (0.193)	0.157 (0.530)	0.039 (0.043)
2013	-1.696 (1.393)	1.834 (1.234)	1.577* (0.936)	0.203 (0.182)	0.378 (0.594)	0.050 (0.048)
2014	-1.553 (1.478)	2.529* (1.293)	2.706*** (0.985)	0.099 (0.172)	0.528 (0.645)	0.031 (0.050)
2015	-2.187* (1.301)	1.995 (1.298)	0.929 (1.040)	0.001 (0.147)	0.292 (0.732)	0.055 (0.052)
2016	-2.334 (1.475)	2.372* (1.292)	1.765* (1.058)	0.057 (0.178)	0.587 (0.903)	0.063 (0.054)
2017	-2.964** (1.489)	2.015 (1.279)	0.439 (1.001)	0.015 (0.183)	0.766 (0.710)	0.038 (0.054)
2018	-2.425 (1.561)	2.033 (1.347)	0.815 (1.085)	-0.030 (0.189)	1.605* (0.941)	0.035 (0.056)
2019	-1.671 (1.525)	1.528 (1.281)	1.062 (0.989)	0.026 (0.179)	2.112** (1.061)	0.034 (0.053)
Observations	15351	15351	15351	15351	15351	15351
Adj. R-squared	0.236	0.508	0.352	0.387	0.568	0.609

Note: Table reports the coefficients underlying the event-study figure in Figure 6, estimated on the firm-level PIA panel with establishment and municipality-year fixed effects and CEM weights (exact match on municipality and sector, coarsened exact matching on deciles of pre-treatment wage bill and capital stock). The omitted base year is 2002; standard errors, clustered by establishment, are in parentheses. Profit, Revenue, Investment, and TFP are transformed with the inverse hyperbolic sine; Export Share and Exporter are estimated in levels, in percentage points and as a binary indicator, respectively. Estimates were computed in IBGE's restricted-access data room; significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A7: Local Demand Shocks – HHI Interaction (RAIS)

	<i>Average Earnings</i>	<i>Employees (Total)</i>	<i>Employees (High-Ed)</i>	<i>Employees (Low-Ed)</i>	<i>Employees (Innov.)</i>	<i>Estab. Active</i>
Local Demand	0.088** (0.034)	0.028 (0.020)	0.026** (0.012)	0.024 (0.019)	0.001 (0.009)	0.009** (0.004)
Local Demand × Concentrated	0.233*** (0.051)	0.111*** (0.029)	0.073*** (0.022)	0.072** (0.028)	0.070*** (0.017)	0.027*** (0.006)
Observations	38,324	38,324	38,324	38,324	38,324	38,324
Establishment FEs	YES	YES	YES	YES	YES	YES
Municipality-Year FEs	YES	YES	YES	YES	YES	YES

Note: Concentrated is a dummy that equals 1 for establishments certified in a scope-group with an HHI greater than or equal to 1800. Standard errors (reported in parentheses) are clustered at the scope-group#year level. All variables are IHS transformed except for Estab. Active, which is a 0/1 indicator for whether the establishment is present in RAIS in a given year.

Table A8: LC Demand Shocks – HHI Interaction (PIA)

	<i>Profit</i>	<i>Revenue</i>	<i>Investment</i>	<i>TFP</i>	<i>Export Share (%)</i>	<i>Exporter (0/1)</i>
Panel A: Below p50						
LC Demand	0.268 (0.229)	0.0816 (0.168)	-0.200 (0.160)	-0.00235 (0.0225)	-0.0000716 (0.000255)	0.00000684 (0.00000749)
Establishment FEs	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4206	4206	4206	4206	4206	4206
Adjusted R2	0.280	0.594	0.340	0.451	0.637	0.723
Panel B: Above p50						
LC Demand	0.392 (0.279)	0.450 (0.294)	0.277 (0.172)	0.0821*** (0.0310)	0.000744** (0.000323)	0.0000168** (0.00000722)
Establishment FEs	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4206	4206	4206	4206	4206	4206
Adjusted R2	0.225	0.526	0.284	0.390	0.518	0.661

Note: LC demand shock regressions estimated as in Equation 1, separately for establishments above and below the median product-level HHI. Standard errors, reported in parentheses, are clustered at the scope-group level. All variables are IHS-transformed except Export Share (%) and Exporter (0/1), which are estimated in levels. Unlike Table A7, which estimates an interaction with an indicator for concentration, PIA results are estimated separately for firms below and above median HHI, reflecting how these estimates were run within IBGE's restricted-access data room.

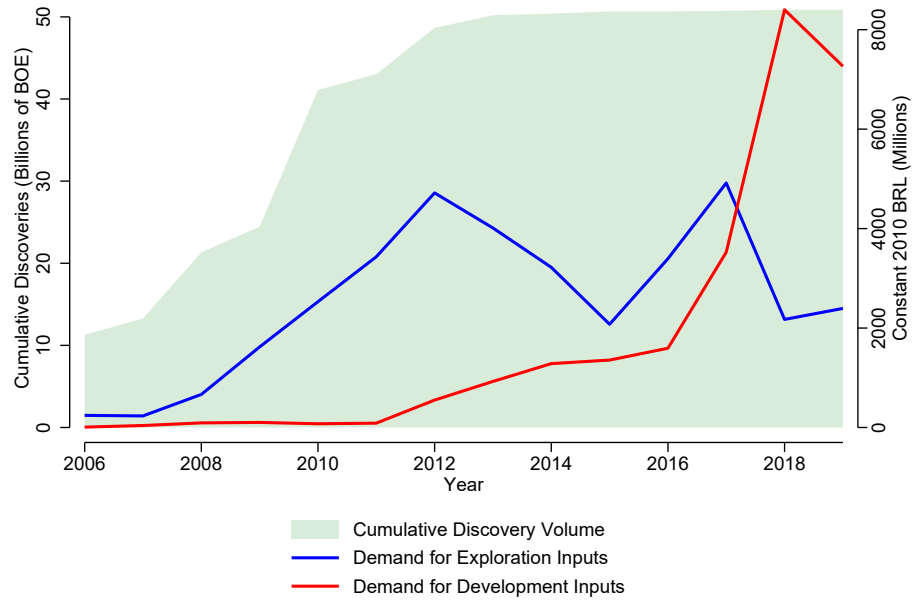
Table A9: Political Involvement of LC Establishments (2002-2014)

	Among All Establishments	
	<i>Made a Campaign Donation (0/1)</i>	
	Matched Firms	All Firms
<i>LC-Beneficiary = 1</i>	0.0118*** (0.0008)	0.0118*** (0.0008)
DV Control Mean n=	0.0311 13,587,715	0.0450 14,282,797
	Among Donor Establishments	
	<i>Donation Value (asinh)</i>	
	Matched Firms	All Firms
<i>LC-Beneficiary = 1</i>	0.4064*** (0.0671)	0.4150*** (0.0671)
DV Control Mean (asinh) n=	13.273 38,934	12.398 41,136
	<i>Share of Donation Value to PT</i>	
	Matched Firms	All Firms
	Matched Firms	All Firms
<i>LC-Beneficiary = 1</i>	0.0622*** (0.0134)	0.0637*** (0.0134)
DV Control Mean n=	0.088 38,934	0.131 41,136

Note: Table reports coefficient estimates and standard errors in parentheses for regression of outcomes on LC-beneficiary dummy, a vector of controls, and state and year fixed effects. Outcomes are defined as: (i) an indicator for whether an establishment made any campaign donation between 2002-2014; (ii) the inverse hyperbolic sine of the value of the establishment's campaign donation, for the sub-sample of establishments that donated; and (iii) the share of total donation value over the 2002-2014 period that went to the Workers' Party (PT), for the sub-sample of establishments that donated. Controls include number of total employees, share of high education employees, share of female employees, and average wage. Share of donations to the PT is chosen since this was the party most associated with support for the LC policy. Column 1 reports results for the matched sample, which includes LC-beneficiary establishments and all other establishments that exactly matched on municipality, 5-digit industry code, and quintiles of baseline number of total employees, average wage, and average high-education share of employment. Column 2 reports results from the full sample of establishments with at least one employee.

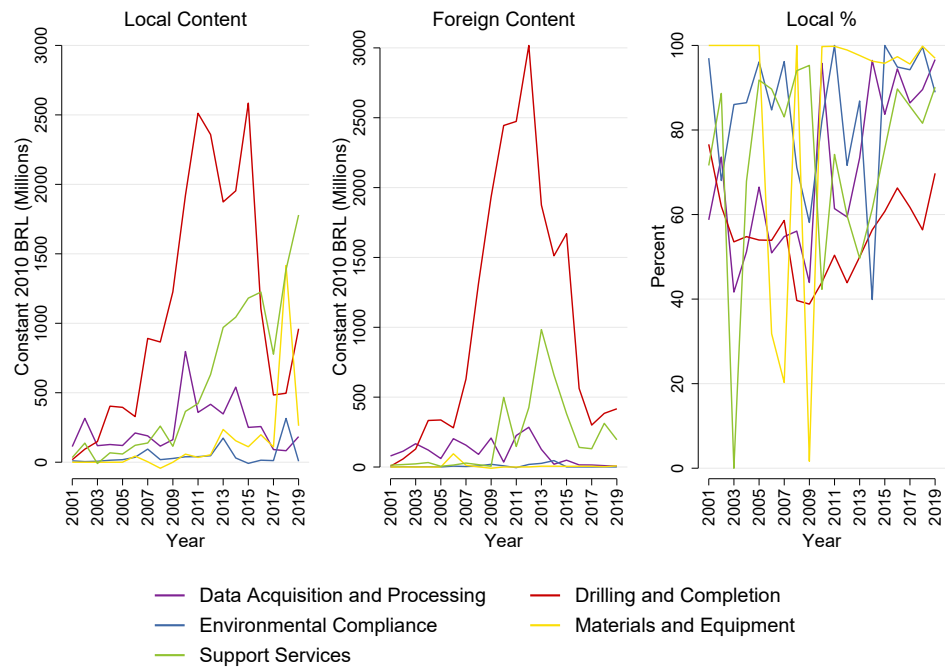
3 Supplementary Figures

Figure A1: Offshore Discoveries and Demand for Local Oil and Gas Inputs



Note: Data on cumulative offshore discoveries are drawn from [Katovich \(2024\)](#). Data on local content investments are aggregated from product-specific investment reports submitted to the ANP ([ANP, 2024](#)). Monetary values are deflated to constant 2010 BRL.

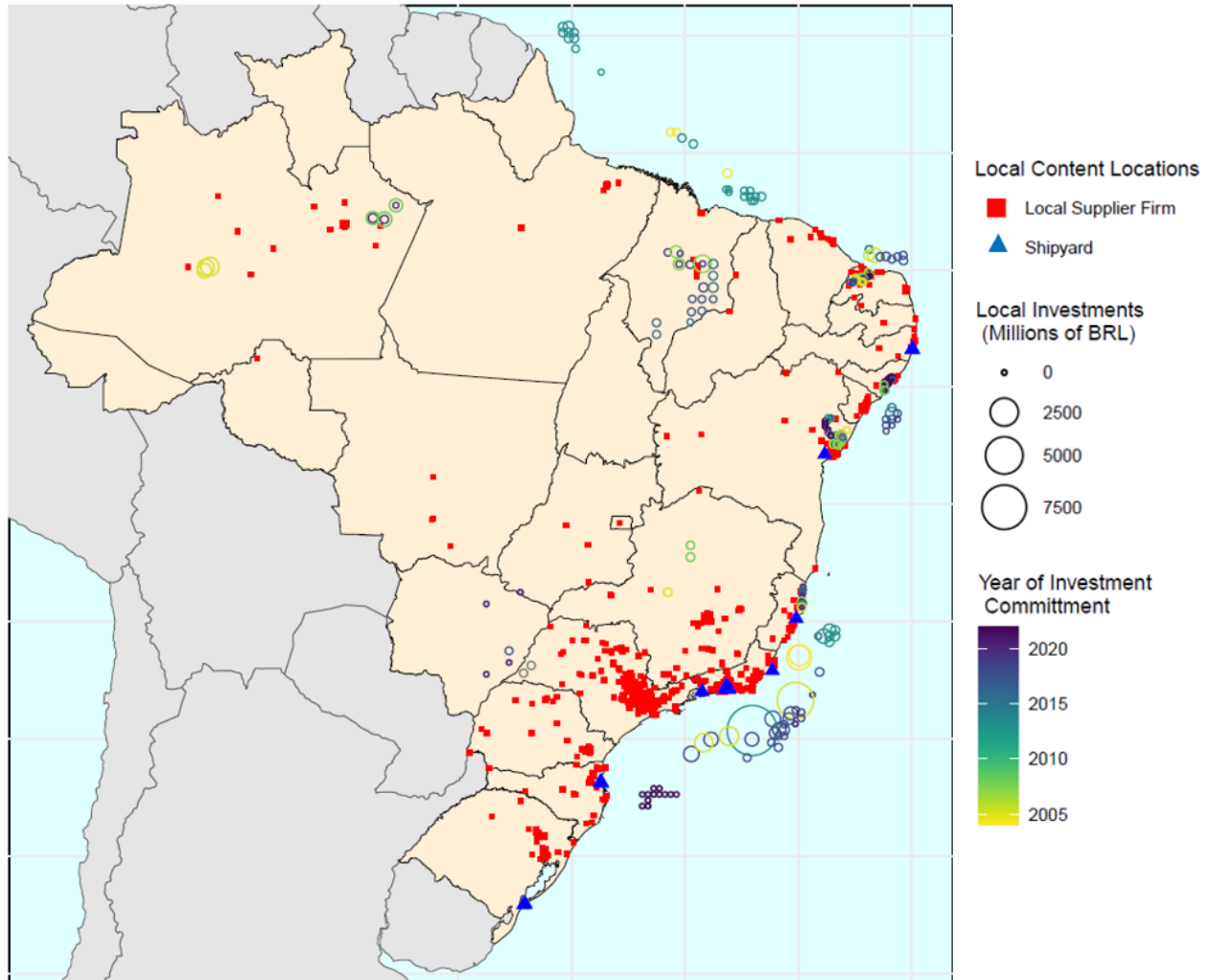
Figure A2: Oil and Gas Investments (2001-2019)



Note: Trimesterly investments in local and foreign inputs are drawn from [ANP \(2024\)](#). Monetary values are deflated to constant 2010 BRL.

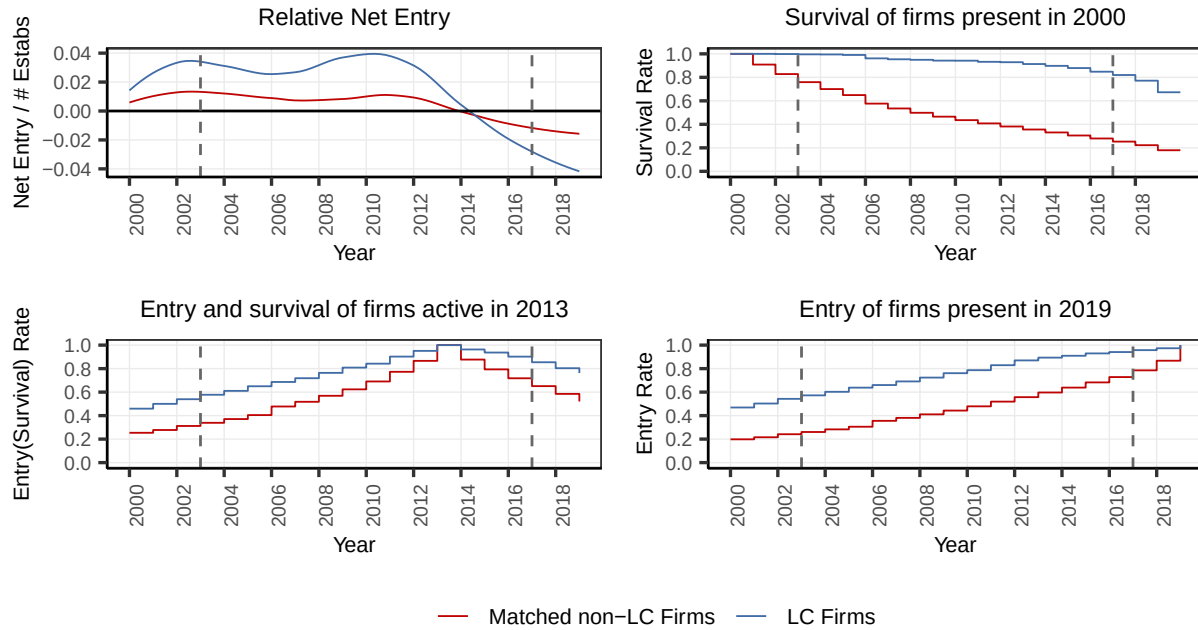
Figure A3: LC-Certified Input-Suppliers and Local Content Investments

Local Oil Industry Suppliers and Investments



Note: Investments in inputs and firm locations are drawn from [ANP \(2024\)](#). Monetary values are deflated to constant 2010 BRL.

Figure A4: LC Establishment Entry and Exit



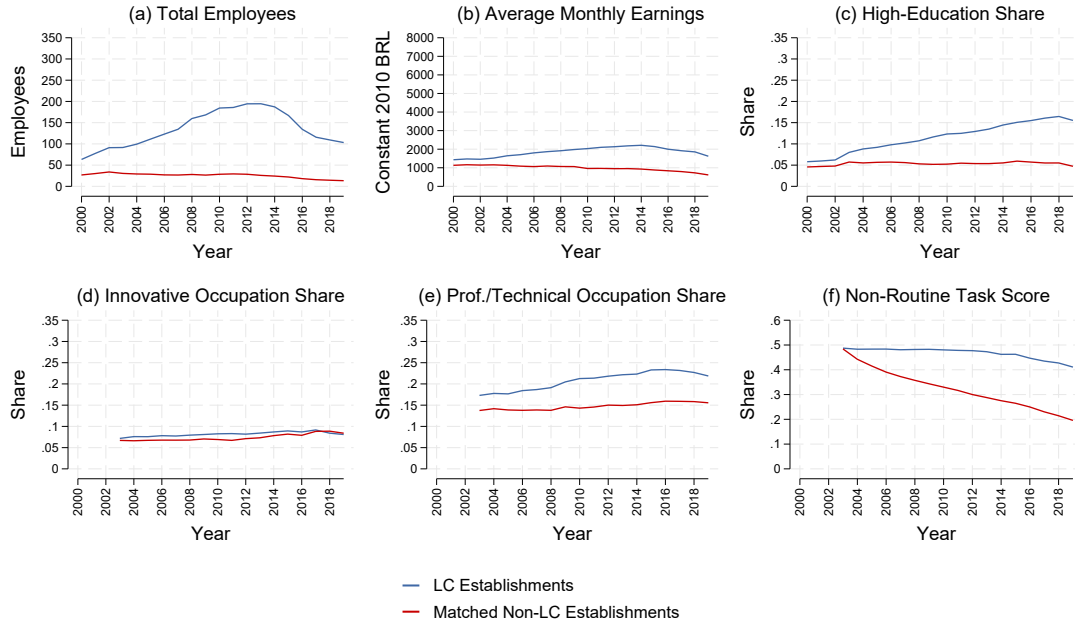
Note: The first vertical dashed line indicates the year the LCR was strengthened (2003), while the second marks the year it was weakened (2017). In the Relative Net Entry graph, locally weighted regression (LOESS) smoothing is applied using a span of 0.5. Non-LC establishments, presented for comparison, are establishments registered in RAIS with at least one employee that share a municipality and 5-digit CNAE activity code with one or more LC establishments.

Figure A5: Event Studies for Employee Category Shares (RAIS)



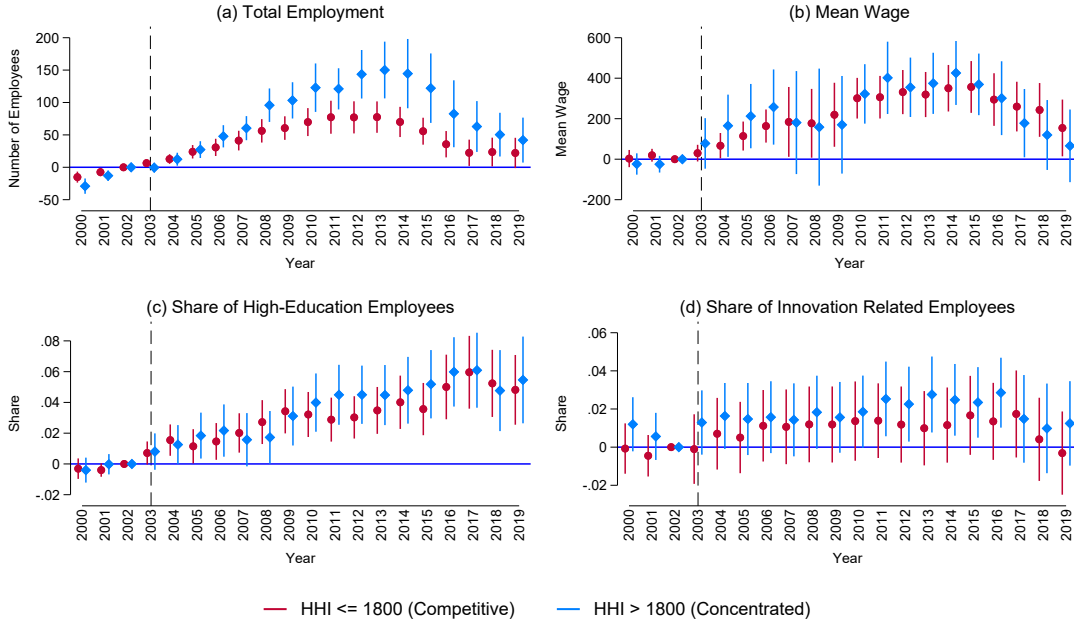
Note: Figure reports coefficient estimates with 95% confidence intervals; standard errors are clustered at the establishment level. All four outcomes are estimated on an unbalanced panel restricted to years in which the establishment is active. Results are from a TWFE regression with establishment and municipality-year fixed effects, exact matching on municipality and economic activity sector, and coarsened exact matching based on deciles of pre-treatment average wage, total employment, and total high-education employment.

Figure A6: Human Capital Characteristics of LC Establishments (Event Study Matching)



Note: Means are computed from RAIS. LC-certified establishments are exactly matched with RAIS using unique establishment IDs. Monetary values are deflated to constant 2010 BRL. High-Education workers are defined as those with more than secondary education. Innovation intensity is defined as the share of an establishment's workers employed in innovation-related occupations, as defined using text analysis of the *Quadro Brasileiro de Qualificações* (QBQ). Matched Non-LC Establishments, presented for comparison, are constructed using the full matching procedure employed in our event studies: exact matching on municipality and 5-digit CNAE activity code (i.e., sector), combined with coarsened exact matching on deciles of pre-treatment average wage, total employment, and total high-education employment. This contrasts with the simpler municipality-and-sector match shown in Figure 3 in the main text. The figures shown here therefore serve as the raw-data analog to our event studies, depicting the treated and matched control groups that underlie those specifications.

Figure A7: Dynamic DiD for selected outcomes by Concentration of market



Note: Standard errors clustered at establishment levels. Results from a TWFE regression with municipality-year fixed effects, exact matching on municipality and economic activity sector, coarsened exact matching based on deciles of pre-treatment levels of average wage, total employment and total high education employment. The two graphs on the left consider the intensive margin only while the two on the right consider both intensive and extensive margin. The variable concentrated is a dummy that equals 1 for establishments in scope-groups where the mean HHI in 2002 exceeded 1800, and 0 otherwise.

4 Robustness: Local Demand Shocks

Table A10 re-estimates the headline RAIS specification under alternative transformations of the outcome and demand variables, clustering levels, and fixed effects. Replacing inverse hyperbolic sine with $\ln(x + 1)$ leaves every coefficient essentially unchanged. In the untransformed level-level specification – more exposed to the skewness of the outcome distributions – the high-education employment response remains positive and significant, though other outcomes lose significance. Clustering by establishment instead of the headline scope-group-year level leaves the conclusions unchanged. Furthermore, fixed-effects structure matters little: replacing municipality-year fixed effects with year fixed effects produces similar estimates, with the low-education employment response becoming marginally significant. Adding a control for municipality-year oil and gas royalties changes magnitudes only slightly and has no effect on significance levels, suggesting that differential exposure to the oil cycle does not confound the results.

Table A11 reports analogous sensitivity exercises for the PIA outcomes of Table 1, Panel B. Replacing municipality-year fixed effects with year fixed effects strengthens the profit response, which becomes significant at the one percent level, and leaves both export margins positive and significant, while the revenue coefficient becomes smaller and insignificant and the negative investment effect becomes marginally significant. In the level-level specification the monetary outcomes are insignificant, with the large and scale-dependent coefficients

Table A10: Short-Term Local Demand Effects – RAIS: Alternative Transformations, Clustering, and Fixed Effects

	<i>Average Earnings</i>	<i>Employees (Total)</i>	<i>Employees (High-Ed)</i>	<i>Employees (Low-Ed)</i>	<i>Employees (Innov.)</i>	<i>Estab. Active</i>	Obs.
1. Headline: $\text{asinh}(y)$, $\text{asinh}(D)$	0.099*** (0.034)	0.033* (0.020)	0.029** (0.012)	0.027 (0.019)	0.004 (0.009)	0.010*** (0.004)	38,324
2. $\ln(y + 1)$, $\ln(D + 1)$	0.095*** (0.033)	0.032* (0.018)	0.028** (0.011)	0.025 (0.017)	0.003 (0.008)	0.011** (0.004)	38,324
3. Untransformed y and D	0.033 (0.023)	0.0009 (0.0018)	0.0026*** (0.0006)	−0.0017 (0.0015)	0.0001 (0.0002)	−0.6 (0.4)	38,324
4. Clustering: establishment	0.099*** (0.037)	0.033* (0.019)	0.029** (0.014)	0.027 (0.018)	0.004 (0.012)	0.010** (0.004)	38,324
5. Establishment + year FEs	0.097** (0.038)	0.045* (0.024)	0.031** (0.013)	0.041* (0.023)	0.006 (0.011)	0.011** (0.005)	40,614
6. Estab. + year FEs, royalties	0.096** (0.038)	0.044* (0.024)	0.031** (0.013)	0.040* (0.023)	0.006 (0.011)	0.011** (0.005)	40,614

Notes: Each cell reports the coefficient on local demand from a separate regression of the column outcome, following the specification of the row. Row 1 reproduces the headline specification of Table 1, Panel A: outcome and demand transformed with the inverse hyperbolic sine, establishment and municipality-year fixed effects, standard errors clustered at the scope-group-year level (10,178 clusters). Row 2 replaces the inverse hyperbolic sine with $\ln(x + 1)$ for both outcome and demand. Row 3 uses untransformed outcomes and demand in levels; the Estab. Active coefficient and standard error are scaled by 10^5 . Row 4 re-estimates the preferred specification, clustering standard errors by establishment. Rows 5 and 6 replace municipality-year fixed effects with year fixed effects; row 6 adds the inverse hyperbolic sine of municipal oil and gas royalties as a control. Observations increase in rows 5–6 because fewer singleton groups are dropped. The Estab. Active outcome is a binary indicator in all rows. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

typical of highly skewed variables estimated in levels, whereas the export outcomes, which are level-level in the headline as well, are unchanged. Additional robustness exercises are not reported, as they require re-estimation within IBGE’s restricted-access data room.

Table A11: Short-Term Local Demand Effects – PIA: Specification and Functional Form Sensitivity

	<i>Profit</i>	<i>Revenue</i>	<i>Investment</i>	<i>TFP</i>	<i>Export %</i>	<i>Export (0/1)</i>	Obs.
1. Headline (as published)	0.454* (0.243)	0.0299* (0.0178)	−0.213 (0.168)	0.0192 (0.0166)	0.00057** (0.00023)	0.00002*** (0.000007)	8,412
2. Establishment + year FEs	0.557*** (0.208)	0.0120 (0.0128)	−0.214* (0.127)	0.0126 (0.0166)	0.00047*** (0.00014)	0.00001*** (0.000005)	9,958
3. Untransformed y and D	−108,651 (114,827)	664,310 (670,672)	81,425 (85,349)	−0.0002 (0.0003)	0.00057** (0.00023)	0.00002*** (0.000007)	8,412

Notes: Each cell reports the coefficient on local demand from a separate firm-level PIA regression of the column outcome, following the specification of the row. Row 1 reproduces the headline specification of Table 1, Panel B: establishment and municipality-year fixed effects, standard errors clustered at the scope-year level, outcome and demand transformed with the inverse hyperbolic sine except Export % and Export (0/1), which are level-level. Row 2 keeps the headline transformation of each outcome and replaces municipality-year fixed effects with year fixed effects. Row 3 uses untransformed outcomes and demand in levels for all columns, such that the Export % and Export (0/1) cells coincide with row 1. Observations increase in row 2 because fewer singleton groups are dropped. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Estimates are computed in IBGE's restricted data room.

5 Robustness: Shift-Share

This appendix reports specification, inference, and design diagnostics for the shift-share estimates of Table 2, following the practical recommendations in [Borusyak et al. \(2025\)](#) for shift-share designs identified by exogenous shifts. Our preferred specification is the shock-level instrumental-variables estimator of [Borusyak et al. \(2022\)](#), where we residualize outcomes and the shift-share exposure Z_{mt} on controls, aggregate to the product-year (scope $\times$ year) level, and instrument the aggregated exposure with the underlying national scope-year demand shift. This delivers a coefficient numerically equal to the municipality-year weighted least squares estimate with exposure-robust inference. We cluster at the scope-year level, consistent with the product-specific demand design.

5.1 Specification Robustness

Table A12 re-estimates results under alternative specifications. Row 1 reproduces the headline. Row 2 omits the incomplete-shares control (the interaction of baseline total LC exposure with year fixed effects), which [Borusyak et al. \(2022\)](#) require when exposure shares do not sum to one. Estimates are sensitive to this omission: without it, the direct LC employment effect falls from +11.5% to +3.9%, upstream employment from +8.6% to an insignificant -0.2% , and LC-CNAE controls from +9.4% to +1.5%.

Rows 3 and 4 probe two potential timing concerns. Row 3 addresses the fact that our local demand data begin in 2006, which may not behave like later years in the panel. Excluding it leaves the results essentially unchanged (LC +7.1%, downstream +4.9%, upstream +10.2%, all significant; total non-LC still null). Row 4 addresses a concern that local demand shocks are serially correlated, so that estimates attributed to the contemporaneous shock could partly reflect a lagged response. We add the one-year lag of the exposure, instrumenting both the contemporaneous and lagged terms by their own national demand shifts, and report the contemporaneous coefficient. The upstream estimate is broadly consistent with the headline result (+6.8%, though no longer significant), and its loss of significance is consistent with the collinearity introduced by the added lag rather than a decline in the underlying effect. The LC and downstream point estimates fall more substantially (to +6.1% and +0.1%, respectively) and are also no longer significant. For these two exposures, we thus cannot rule out that part of the contemporaneous effect in our headline specification reflects a dynamic response to past demand shocks.

5.2 Balance Tests

We assess exogeneity at two levels, following the quasi-experimental shift-share framework of [Borusyak et al. \(2022\)](#), in which identification relies on the demand shifts being as-good-as-randomly assigned across scopes, conditional on scope and year fixed effects. At the shift level, Table A13 tests this assumption directly by regressing the standardized demand shift on five scope characteristics that should, under exogeneity, have no predictive power: three strictly predetermined as of 2002 (scope size, number of exposed municipalities, geographic concentration) and two measured in the first available shock year, 2006 (skill intensity and oil exposure). Column 1 weights observations by the importance weight s_n used in the

Table A12: Shift-Share Estimates Under Alternative Specifications

	LC (1)	Total (excl. LC) (2)	LC CNAE Control (3)	Downstream (4)	Upstream (5)
Panel A: Employment (log, per 1 SD)					
1. Headline (scope-year clustered)	0.1151*** (0.0286)	0.0064 (0.0077)	0.0944 (0.0633)	0.0487** (0.0203)	0.0856*** (0.0276)
2. Without $S_m \times \delta_t$	0.0391*** (0.0145)	-0.0035 (0.0035)	0.0146 (0.0242)	0.0272*** (0.0097)	-0.0018 (0.0157)
3. Excluding 2006 (no lag)	0.0710** (0.0337)	0.0094 (0.0092)	0.0686 (0.0748)	0.0493** (0.0242)	0.1024*** (0.0331)
4. Contemporaneous + lagged Z	0.0610 (0.0474)	0.0104 (0.0122)	0.1566* (0.0802)	0.0012 (0.0289)	0.0682 (0.0446)

Notes: Each cell reports the coefficient on the standardized shift-share from a separate shock-level IV regression following [Borusyak et al. \(2022\)](#), weighted by the exposure weight (740 scope-year cells), with shock-level year fixed effects and standard errors clustered at the scope-year level. Row 2 omits the $S_m \times \delta_t$ incomplete-shares control. Row 3 re-estimates the headline on the 2007 onward sample (dropping the first year, no lag). Row 4 adds the one-year lag of the exposure, with both endogenous terms instrumented, and reports the contemporaneous coefficient; rows 3 and 4 standardize the exposure on the full estimation sample, so their magnitudes are comparable to row 1.

Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

shock-level estimator itself, so this specification reflects the balance actually relevant to our estimates; column 2 reports the unweighted analogue as a robustness check. In the importance-weighted specification, only scope size significantly predicts the shift, with a negative coefficient. Because scope size is a time-invariant scope characteristic, this level relationship is absorbed by the scope fixed effects in our main estimator and does not threaten identification. It also reflects, mechanically, how the scaled shift is constructed by normalizing demand by baseline scope employment, rather than any behavioral relationship between scope size and demand timing. No other characteristics are significant at the 5% level in either specification. Taken together, these results are consistent with the shift-level exogeneity assumption underlying our design.

Table A13: Shock-Level Balance: Demand Shifts and Baseline Scope Characteristics

Dependent variable:	Standardized scaled demand shift	
	Importance-weighted (1)	Unweighted (2)
Log scope employment, 2002	-0.0766** (0.0372)	-0.2481 (0.1561)
Log number of exposed municipalities	0.0733 (0.0609)	0.0861 (0.2538)
Geographic concentration (HHI)	0.2364 (0.2307)	-0.2510 (1.015)
Workforce skill intensity (std.)	-0.0029 (0.0170)	0.3038* (0.1736)
Oil exposure (std.)	0.0612 (0.0431)	0.0363 (0.1544)
Year fixed effects	Yes	Yes
Observations	740	740

Notes: Scope-year regressions of the standardized scaled demand shift on five predetermined scope characteristics jointly, with year fixed effects and standard errors clustered by scope (53 clusters). Column 1 weights by the importance weight s_n used in the shock-level estimator, while column 2 is unweighted. Skill intensity is the exposure-weighted high-education share of LC employment in a scope's municipalities and oil exposure is the exposure-weighted oil and gas royalties of those municipalities, both measured in the initial shock year (2006) rather than strictly pre-shock, and both standardized. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

At the exposure level, Table A14 tests whether a municipality’s exposure intensity predicts its outcome trend over 2002–2005, controlling for state fixed effects, the incomplete-shares control, and baseline royalties, reported both population-weighted (the headline weight) and unweighted. Because our demand-shift measure is unavailable before 2006, this period predates our shock series rather than the LCR itself, which was already in effect following its 2003 strengthening. The test thus checks for differential trends prior to the start of the shock series rather than prior to the policy. Under population weighting, one of the five employment pre-trends is significant at five percent (upstream employment, with a negative sign that runs against a simple continuation-of-trends explanation for the positive upstream estimate). The same coefficient is also the only one that rejects when unweighted. Taken together with the shift-level tests, we read the balance evidence as broadly supportive of the exogeneity assumption underlying our regressions.

Table A14: Municipality-Level Pre-Trend Balance: Exposure Intensity and 2002–2005 Outcome Trends

Pre-trend outcome (2002–2005)	Population-weighted		Unweighted	
	Coef.	SE	Coef.	SE
Panel A: Employment				
LC	0.113	(0.084)	0.064	(0.067)
Total (excl. LC)	−0.034	(0.021)	−0.028	(0.015)
LC CNAE control	−0.227	(0.193)	−0.046	(0.156)
Downstream	−0.084	(0.075)	−0.028	(0.048)
Upstream	−0.197***	(0.058)	−0.194***	(0.065)

Notes: Each row is a cross-sectional regression of a municipality’s 2002–2005 log outcome change on its mean standardized shift-share exposure (per one standard deviation), controlling for state fixed effects, the incomplete-shares control S_m , and baseline royalties. Heteroskedasticity-robust standard errors are reported in parentheses. The left panel weights by 2006 population, while the right panel is unweighted. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

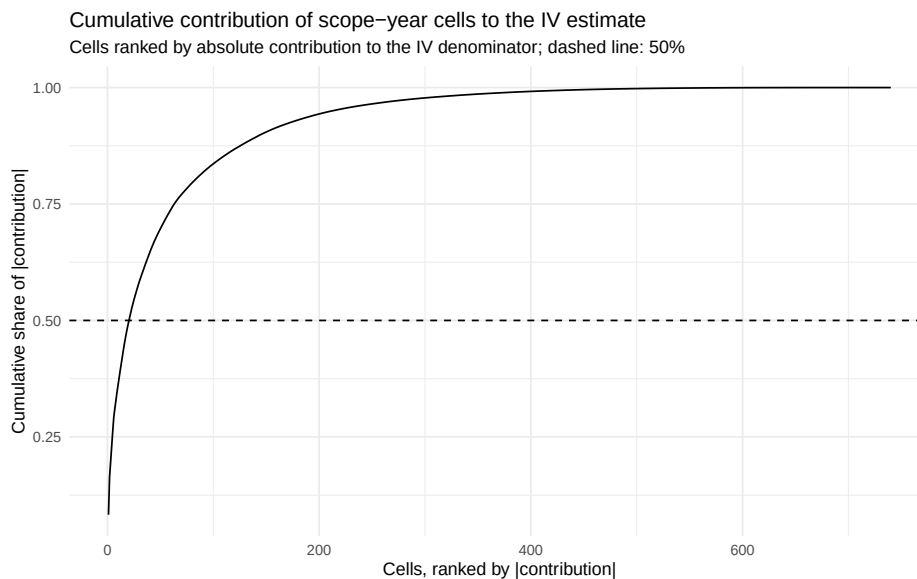
5.3 Concentration of Identifying Variation and Leave-One-Scope-Out

Following [Borusyak et al. \(2025\)](#), we characterize which shifts drive the estimates. The effective number of shifts — the inverse Herfindahl index of the importance weights — is 236 out of 740 scope-year cells, or 17 out of 53 product scopes once weights are aggregated to the scope level, reflecting the fact that some cells carry more identifying weight than others, as is typical in shift-share designs. To characterize this further, we compute each scope-year cell’s contribution to the IV estimator’s denominator (which can be positive or negative) and rank cells by the absolute size of this contribution. Figure A8 plots the resulting cumulative distribution, which shows a modest share of cells accounting for a disproportionate share of the total contribution. The five largest scopes account for roughly 54% of the total, and the years 2017 and 2018 alone – around the weakening of the policy – account for roughly 57%. We probe the influence of this variation with the leave-one-scope-out check below.

We re-estimate the headline specification, dropping one scope at a time. No coefficient that is statistically significant in the headline specification changes sign under any single-scope omission. The direct LC effect ranges from 0.091 to 0.142 across these re-estimations

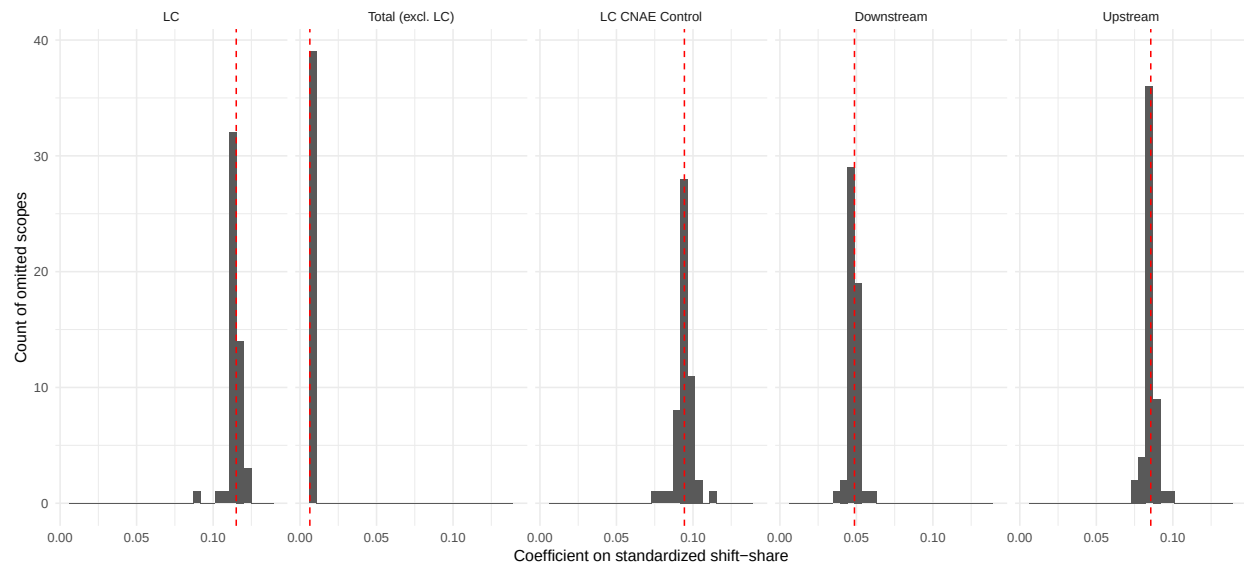
(full-sample estimate: 0.115), with the largest single-scope movement representing a 23% change from the full-sample estimate (Figure A9). While the sign of every significant result is robust to dropping any individual scope, the range of estimates still reflects economically meaningful heterogeneity in effect magnitudes across scopes.

Figure A8: Concentration of Identifying Variation Across Scope-Year Cells



Note: Scope-year cells are ranked by the absolute size of their contribution to the IV estimator's denominator (importance weight $\times$ residualized shift $\times$ residualized exposure). The curve plots the cumulative share of total absolute contribution as cells are added in this order. Dashed line: 50%.

Figure A9: Leave-One-Scope-Out Coefficient Distributions



Note: Histograms of coefficients from re-estimating the headline 53 times, dropping one product scope each time. Dashed red lines mark the full-sample estimates.

6 Robustness: Event Studies

This appendix reports robustness checks for the matched event studies of Section 4.3: alternative fixed-effects structures, Poisson estimation of the main outcomes, and an alternative matched sample in which control establishments are drawn from municipalities with no LC establishment at baseline, eliminating local spillovers onto the control group.

6.1 Fixed-Effects Structure and Poisson Estimation

Figure A10 re-estimates the four main outcomes replacing the municipality-year fixed effects of the preferred specification with (i) year fixed effects and (ii) state-year fixed effects. The three sets of estimates are nearly indistinguishable (for example, the 2012 employment coefficient is 106.2 with municipality-year, 108.7 with year, and 108.6 with state-year fixed effects).

Figure A11 re-estimates the four main outcomes by Poisson pseudo-maximum likelihood. PPML is a natural choice for employment, which is a non-negative count outcome, and unlike our levels specification, it is not dominated by the small number of establishments with very large employment counts, whose idiosyncratic pre-trends can skew level estimates even under matching. Consistent with this, the employment pre-trend is flat and close to zero under PPML, in contrast to the modest upward pre-trend visible in levels. This supports a causal interpretation of the post-2003 employment effect: rather than reflecting a continuation of pre-existing growth among a subset of large establishments, positive effects emerge only after the LCR strengthening. Post-2003 dynamics are otherwise similar to the preferred specification, and pre-2003 coefficients are insignificant across all four outcomes.

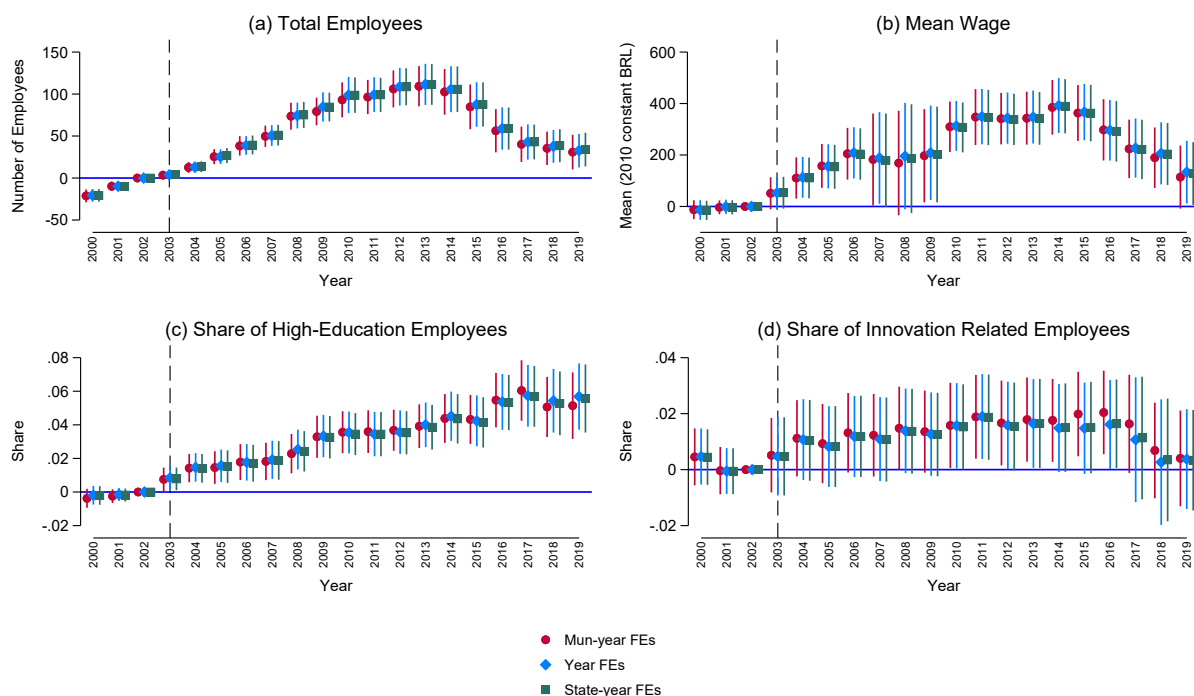
6.2 Flipped-Municipality Matching

The matching procedure of Section 4.3 pairs LC establishments with controls in the same municipality, which guards against local confounders but exposes the controls to local spillovers documented by the shift-share design. We therefore re-match keeping every criterion except the municipality condition, which is flipped: control candidates must operate in the same 5-digit sector, lie in the same deciles of baseline wage, employment, and high-education share, and be located in a municipality with *no* LC establishment at baseline. Of the 1,116 treated establishments, 726 retain a match under this stricter geography, matched to 181,374 controls outside the 243 municipalities with baseline LC activity. Because treated and control establishments never share a municipality, municipality-year fixed effects would absorb the treatment-control contrast. We thus include year and state-year fixed effects, which the previous subsection shows to be innocuous on the preferred sample.

Figure A12 reports the results, which strengthen our main conclusions. The employment pre-trend that violates parallel trends in the headline sample disappears and the post-2003 employment effect is larger than in the headline, reaching +168 employees in 2012 against a headline peak near +100. This pattern is to be expected if positive local spillovers raise employment among same-municipality controls and attenuate the headline comparison. Wage and high-education responses also survive with spillover-free controls, though a modest pre-2003 wage gradient appears. The innovation-intensive share result is the most sensitive to

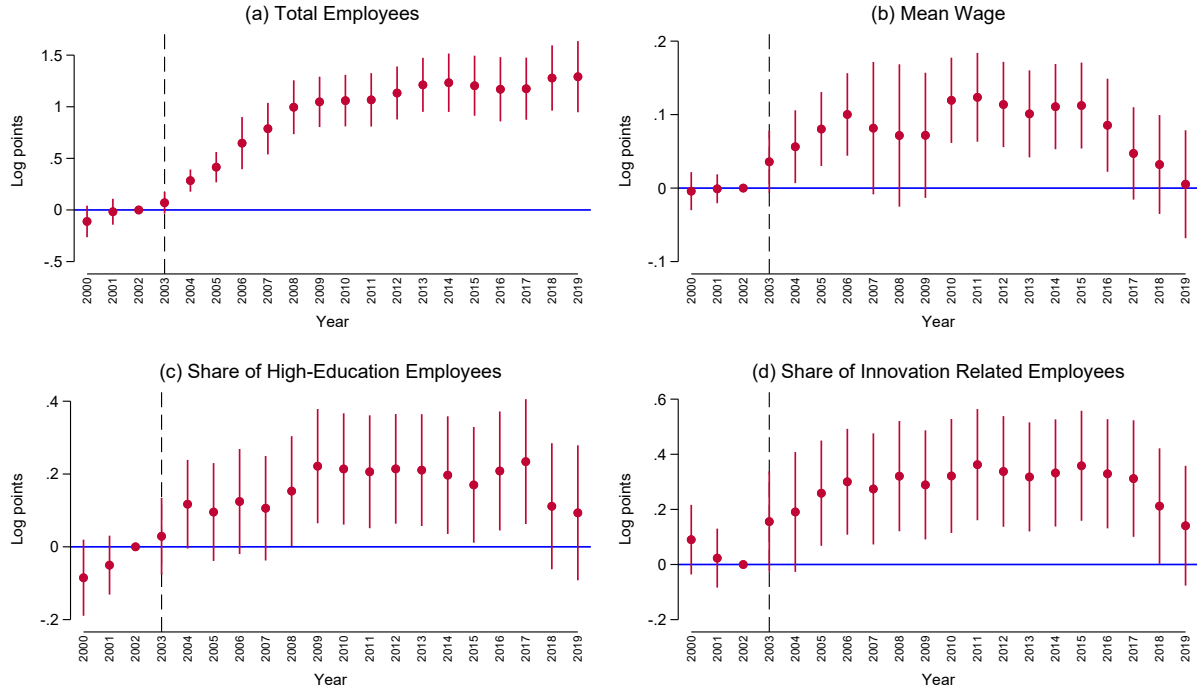
this specification: coefficients remain positive but are smaller and not significant at the five percent level, compared to +2.0 percentage points in our main specification. Year and state-year fixed effects give nearly identical estimates throughout.

Figure A10: Event-Study Robustness: Fixed-Effects Structure



Note: Figure reports coefficient estimates with 95% confidence intervals from the Poisson pseudo-maximum likelihood estimator (`ppmlhdfc`), reported in log points. Estimates use the headline matched sample (matched on municipality, sector, and baseline deciles) with CEM weights, establishment and municipality-year fixed effects, base year 2002, and standard errors clustered by establishment. Panel (a) is estimated on the balanced panel with zeros imputed for missing establishment-years, while panels (b)–(d) are estimated on the sample of active establishments. Establishments with no variation in the outcome are dropped by the estimator. The corresponding levels specification appears in the main text.

Figure A11: Event-Study Robustness: Poisson Estimation



Note: Figure reports coefficient estimates with 95% confidence intervals for estimates from the Poisson pseudo-maximum likelihood (ppmlhdf) estimator, reported in log points. Headline matched sample (match on municipality, sector, and baseline deciles) with CEM weights, establishment and municipality-year fixed effects, base year 2002, standard errors clustered by establishment. Panel (a) is estimated on the filled panel including exits; panels (b) to (d) on active establishments. Establishments with no variation in the outcome are dropped by the estimator. The levels version appears in the main text.

Figure A12: Event Studies with Flipped-Municipality Controls

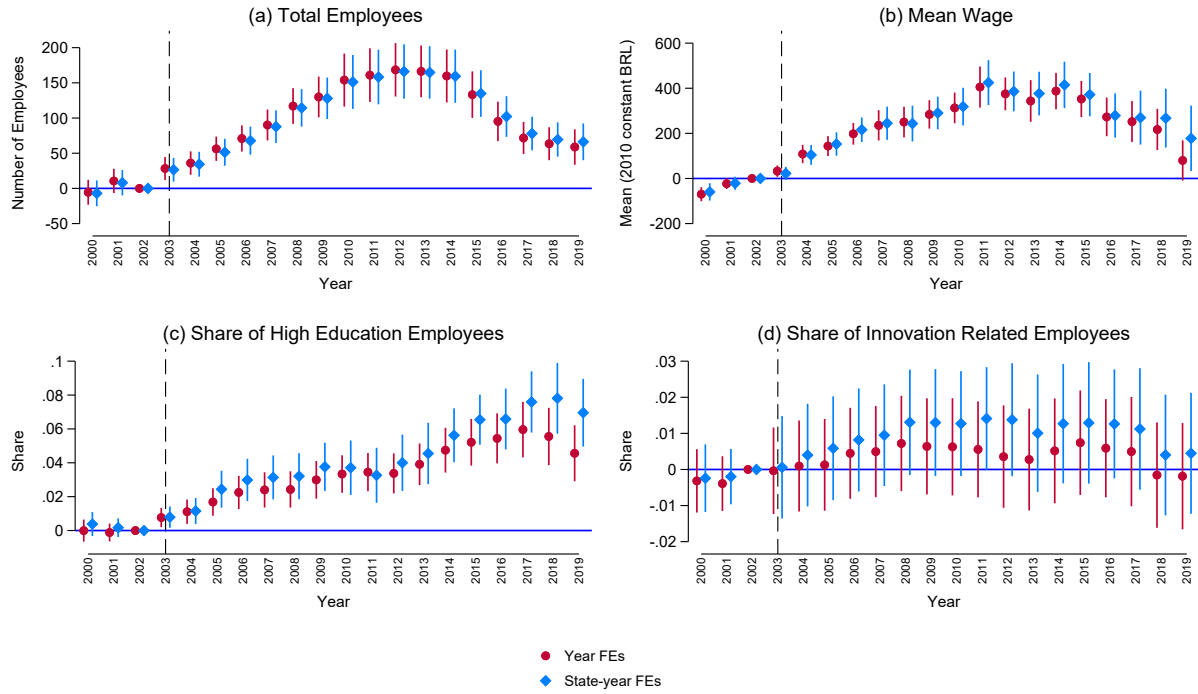


Figure reports coefficient estimates with 95% confidence intervals for the specification with control establishments restricted to municipalities containing no LC establishment. Each panel overlays two sets of estimates: (i) establishment and year fixed effects, and (ii) establishment and state-year fixed effects. Establishments are CEM-matched on 5-digit sector and deciles of 2000–2002 wage, employment, and high-education share, with CEM weights, base year 2002, and standard errors clustered by establishment. Panel (a) is estimated on the balanced panel with zeros imputed for missing establishment-years, while panels (b)–(d) are estimated on the sample of active establishments.

7 LCR Impacts on the Oil Industry

7.1 Data

Sources. All data are published on the [ANP \(2026\)](#) open-data portal. Four types of data underlie the analysis. For each block, *Rodadas de Licitações* (bidding rounds) provide the round, basin, sub-basin/area code, cash bonus bid, auction score, committed exploration program size, and – the treatment variables – the local content percentage committed separately for exploration and development phases. *Fase de Exploração* supplies everything that happens to a block after licensing: discovery assessment plans (PADs), commerciality declarations, and exploratory wells drilled since 1998. *Produção por Poço* supplies monthly oil production by well between 2005-2023. Spatial well-point and block-boundary shapefiles for Rounds 9–17 are used to assign wells to blocks and to compute each block’s area. Separately, ANP’s *Fiscalização de Conteúdo Local* open-data series reports, for every block whose exploration-phase compliance inspection is complete (162 of 479 Round 9-17 blocks), the committed and realized LC%, together with fines assessed for shortfall. ANP does not use a consistent block-naming convention across its data products, so wells and production cannot be linked directly to auction-era blocks. Instead, each well in the well-point shapefile is assigned to the block polygon it intersects with via a point-in-polygon spatial join.

Panel construction and sample. We expand each winning bid in Rounds 9-17 into a balanced block-year panel through 2023, filling block-years with no recorded activity with zeros. We measure development investment using (i) the number of discovery assessment plans (PADs), a regulatory filing, and (ii) the count of development wells reaching first recorded production. Our analysis sample is Rounds 9-13 (406 blocks, 5,369 block-years). We exclude Rounds 14-17 (roughly 76 blocks), where LC% was set at a flat regulatory minimum in each round’s bidding rules and varied only by onshore/offshore location, leaving no bid-level variation to exploit.

7.2 Estimation

Panel regressions. For each outcome family Y (production, exploration investment, development investment), we estimate

$$Y_{bt} = \beta \text{LC\%}_b + X'_{bt} \gamma + \delta_{r(b)} + \lambda_t + \varepsilon_{bt}$$

by OLS on the full block-year panel, where b indexes blocks, t indexes calendar year, $\delta_{r(b)}$ is a fixed effect for block b ’s licensing round, and λ_t is a calendar-year fixed effect. X_{bt} is built up across four columns: (i) a bivariate specification, (ii) adding $\delta_{r(b)}$, (iii) adding λ_t , and (iv) a full-control specification adding basin, water-depth-category, and sector fixed effects together with $\log(\text{cash bonus bid})$, the auction score, $\log(1 + \text{committed exploration program size})$, and $\log(\text{block area})$. A block fixed effect cannot be used, since LC% is fixed at the bid stage and never varies within a block. Standard errors are clustered at the block level. Treatment is the exploration-phase committed LC% for production and exploration outcomes, and the development-phase committed LC% for development outcomes. Because production and

investment measures are non-negative outcomes with many zeros, we also estimate Poisson regressions with the full set of controls as a robustness check. A remaining concern is that unobserved firm-level heterogeneity (capability, risk tolerance, financing access) may be correlated with the LC% a firm chooses to bid. We address this concern by adding operator fixed effects in place of the basin/water-depth/sector fixed effects, alongside round, year, and continuous controls.

Survival analysis. For time to discovery, commerciality declaration, and first production, we build a corrected-risk-set version of the panel (i.e., each block-year after a block’s own first occurrence of that event is dropped) and estimate the Cox proportional-hazards model

$$h(t | X_b) = h_0(t) \exp \left(\beta \cdot \text{LC\%}_b + X'_b \gamma + \delta_{r(b)} + \lambda_t + [\text{basin, depth, sector FE}] \right),$$

reported as hazard ratios, with robust standard errors.

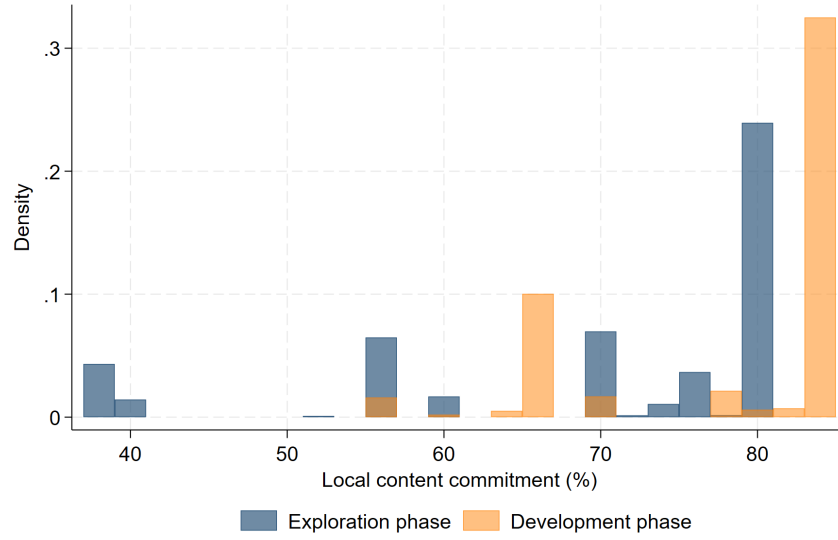
7.3 Descriptive Results

Figure A13 plots the distribution of LC% commitments across the 406 winning bids in concession Rounds 9-13. Commitments vary substantially. Exploration-phase commitments range from 37% to 80% (median 75%) and development-phase commitments from 55% to 85% (median 85%). Commitments are concentrated near the top of each range, with at least a quarter of blocks at the maximum, but a sizable share committed to considerably lower levels: approximately a quarter of blocks committed to 60% local content or less for exploration and 70% or less for development. Figure A14 shows how far the 479 winning bids in Rounds 9-17 had progressed by 2023. Attrition is considerable: only 21% of blocks yield a discovery, 11% reach a commerciality declaration, and 10% reach production. Because most blocks never advance, restricting our sample to those that do would introduce survivor bias. We avoid this by retaining each winning bid in a fully balanced block-year panel, imputing zeros where appropriate.

7.4 Panel Regression Results

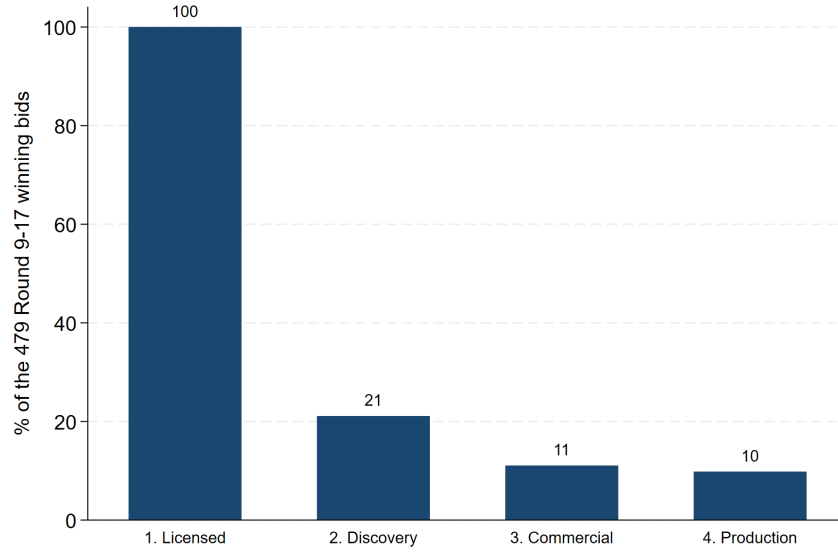
Table A15 reports OLS regressions of each block-year outcome on the committed LC%. Coefficients indicate the change in the outcome associated with a one-percentage-point higher commitment. Columns progressively add controls and fixed effects. For exploration (Panel B), the estimate is small and insignificant in the bivariate specification and after adding round and year fixed effects and full controls (0.0006, standard error 0.0017), but becomes positive and significant with operator fixed effects (0.006, $p < 0.01$; sample mean 0.08 wells per block-year). For development (Panels C and D), estimates are positive throughout. PADs increase significantly with LC% commitment once round fixed effects are included (0.0017 with full controls, $p < 0.05$, and with operator fixed effects, $p < 0.01$). New development wells are significant at the 10 percent level in the first three columns (0.0010 to 0.0015), not significant with full controls (0.0026), and again significant with operator fixed effects (0.004). For production (Panel A), estimates are near zero and insignificant in every pooled

Figure A13: Distribution of Local Content Commitments, Rounds 9–13



Notes: Bid-level LC% commitment for exploration and development phases of winning bids in concession Rounds 9-13.

Figure A14: Milestone Attrition Across Round 9–17 Winning Bids



Notes: Share of all 479 Round 9–17 winning bids reaching each successive progress milestone by 2023.

specification (0.0009 to 0.0028) and positive with operator fixed effects (0.018, $p < 0.10$). No specification for any outcome results in a negative estimate.

Because the outcomes are non-negative with many zeros, Table A16 reports Poisson regressions with the full set of controls. Incidence-rate ratios indicate the proportional change in the outcome per percentage point of committed LC%, such that values above one indicate more activity. Estimates for exploration (1.01) and production (1.08) are above one but not

statistically significant. Estimates for the two development outcomes (1.41 for PADs and 27.7 for development wells) are significant but implausibly large, reflecting how rarely these outcomes occur.

Table A15: Local Content Commitments and Subsequent Block Activity

	(1) Bivariate	(2) +Round FE	(3) +Round, Year FE	(4) +Full controls	(5) +Operator FE
Panel A: Production, inverse hyperbolic sine of oil					
Local content %, exploration phase	0.0028 (0.0028)	0.0009 (0.0030)	0.0009 (0.0030)	0.0021 (0.0104)	0.0178* (0.0091)
Round FE	No	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes
Basin/Depth/Sector FE	No	No	No	Yes	No
Bonus/Score/PEM/Area Controls	No	No	No	Yes	Yes
Operator FE	No	No	No	No	Yes
Observations	5,369	5,369	5,369	5,369	5,369
Panel B: Exploration investment, wells drilled per year					
Local content %, exploration phase	0.0006 (0.0006)	0.0003 (0.0007)	0.0003 (0.0007)	0.0006 (0.0017)	0.0060*** (0.0018)
Round FE	No	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes
Basin/Depth/Sector FE	No	No	No	Yes	No
Bonus/Score/PEM/Area Controls	No	No	No	Yes	Yes
Operator FE	No	No	No	No	Yes
Observations	5,369	5,369	5,369	5,369	5,369
Panel C: Development investment, PADs started per year					
Local content %, development phase	0.0003 (0.0002)	0.0004* (0.0002)	0.0004* (0.0002)	0.0017** (0.0009)	0.0017*** (0.0004)
Round FE	No	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes
Basin/Depth/Sector FE	No	No	No	Yes	No
Bonus/Score/PEM/Area Controls	No	No	No	Yes	Yes
Operator FE	No	No	No	No	Yes
Observations	5,369	5,369	5,369	5,369	5,369
Panel D: Development investment, new development wells online per year					
Local content %, development phase	0.0010* (0.0006)	0.0015* (0.0009)	0.0015* (0.0009)	0.0026 (0.0023)	0.0042* (0.0025)
Round FE	No	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes
Basin/Depth/Sector FE	No	No	No	Yes	No
Bonus/Score/PEM/Area Controls	No	No	No	Yes	Yes
Operator FE	No	No	No	No	Yes
Observations	5,369	5,369	5,369	5,369	5,369

Notes: Sample is restricted to blocks won in Rounds 9-13, where the local content (LC%) commitment has cross-block variation. Sample is a fully balanced block-year panel. Columns (1)-(5) are estimated by OLS and add controls cumulatively: round FEs, year FEs, then basin, water-depth-category, and sector FEs together with log(cash bonus bid), the auction score (points), log(1+committed exploration program size), and log(block area). Column (5) instead adds operator FE to round, year FE, and the continuous controls, without the basin/depth/sector FE of column (4). Panel A reports production as the inverse hyperbolic sine of oil production, which accommodates many zero-valued block-years. Panels C and D report two development-investment measures: PADs (discovery assessment plans, ANP's post-discovery evaluation filings) and new development wells reaching first production. Standard errors, clustered by block, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

7.5 Survival Results

Table A17 reports Cox proportional-hazards models of time to discovery, commerciality, and first production, where hazard ratios above one indicate faster progress per percentage point of committed LC%. Higher exploration-phase commitments are not associated with

Table A16: Local Content Commitments and Block Activity: Poisson

	(1) Production (oil, bbl)	(2) Exploration (wells drilled)	(3) Development (PADs started)	(4) Development (wells online)
Local content %, exploration phase	1.0792 (0.1646)	1.0141 (0.0229)		
Local content %, development phase			1.4142*** (0.1633)	27.6809*** (31.1772)
Full controls	Yes	Yes	Yes	Yes
Observations	5,369	5,369	5,369	5,369

Notes: Poisson regressions of block-year outcomes on committed LC% for winning bids in concession Rounds 9-13. Column (1) is oil production rounded to the nearest whole barrel (bbl), column (2) is wells drilled, column (3) is PADs started, and column (4) is new development wells reaching first production. All columns include the full controls of Table A15, column (4). Standard errors, clustered by block, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

measurably slower progress at any milestone: the hazard ratios are 1.00 for discovery, 1.10 for commerciality, and 0.97 for production, with none significantly different from one. Higher development-phase commitments are associated with significantly faster time to production (1.17, $p < 0.01$), and the exploration-phase estimate for production rises to 1.10 ($p < 0.01$) once operator fixed effects are included. These models rely on few events (85 discoveries, 42 commerciality declarations, and 43 first-production events), so estimates are imprecise.

Table A17: LC Commitments and Time to Discovery, Commerciality, and Production

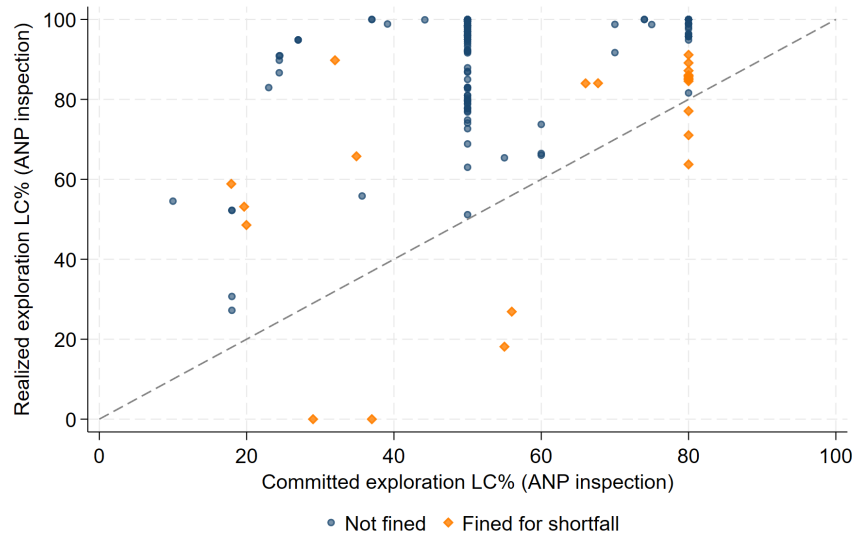
	Discovery	Commerciality	Production	Production	Production
Local content %, exploration phase	1.0007 (0.0602)	1.1018 (0.0747)	0.9745 (0.1003)		1.0953*** (0.0342)
Local content %, development phase				1.1736*** (0.0394)	
Controls	Full	Full	Full	Reduced*	Operator FE
Failures (events)	85	42	43	43	43
Block-years at risk	4,550	5,066	5,090	5,090	5,090

Notes: Table reports hazard ratios from Cox Proportional Hazard models per percentage point of committed LC% (values > 1 indicate faster progress to the milestone). Columns (1)-(3) and (5) use the exploration-phase commitment, and column (4) uses the development-phase commitment. Each block-year after a block's first occurrence of the milestone is dropped, separately for each outcome. Full controls match Table A15, column (4). Column (4) uses reduced controls (round fixed effects (FE) and the continuous controls only), because the full set failed to converge. Column (5) replaces the basin, water-depth, and sector FE with operator FE, as in Table A15, column (5). Robust standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

7.6 LC Compliance

Figure A15 plots realized LC% against committed exploration-phase LC% for the 160 concession Round 9-13 blocks with a completed ANP compliance inspection. Most blocks lie above the 45-degree line: 153 (96%) delivered at least the committed minimum LC percentage, and 21 delivered 100%, indicating that most commitments did not bind. Only 22 blocks (14%) were fined for shortfall. Most of these (15) were fined despite meeting their aggregate commitment because ANP assesses compliance separately across sixteen spending sub-categories. There were seven blocks below the line, including two that realized none of their committed local content.

Figure A15: Committed vs. Realized Exploration-Phase Local Content



Notes: Rounds 9–13 blocks with a completed ANP exploration-phase compliance inspection (160 of 406). Committed LC% is the commitment ANP inspected against, which is below the original bid-stage commitment for 107 of the 160 blocks. Dashed line is full compliance (realized = committed); points above it over-delivered relative to their commitment, points below it fell short, and orange diamonds mark blocks fined for shortfall. A block can be fined despite sitting above the line: ANP assesses compliance separately across sixteen spending sub-categories, so a block can over-deliver in aggregate while still falling short, and being fined, on one specific sub-category (e.g. block REC-T-256 committed to an aggregate 80.00%, realized 84.57%, and was fined R\$192,613.92 because its drill-bit sub-commitment of 25.00% was realized at 0.00%).