# Commodity Shocks, Bank Ownership, and the Accumulation of Credit Risk*

Roberto Melis†

**Abstract**

I study how a localized commodity shock transmits through bank balance sheets, exploiting the 2014 oil price collapse and granular bank–municipality data from Colombia. Deposits in oil-producing municipalities fall by roughly 18 percent and continue declining over five years. Local lending in aggregate appears stable, but this masks heterogeneous responses across ownership types: domestic private banks contract consumer lending in the most exposed markets, while the public commercial bank's lending remains stable. Non-performing loan ratios rise gradually and in proportion to local oil dependence, concentrated in consumer and commercial portfolios. The deterioration falls almost entirely on private institutions — foreign banks bear the largest increase, domestic private banks a more moderate one, and the public bank none — despite comparable deposit outflows across all ownership groups. Ownership structure shapes both who maintains credit supply and where credit risk accumulates during regional downturns.

**JEL Classification:** G21, G28, Q33, E44

**Keywords:** commodity price shocks, local banking markets, deposit funding, credit risk, non-performing loans, bank ownership

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†University of Bergamo, Italy, and University of Agder, Norway. Email: roberto.melis@unibg.it

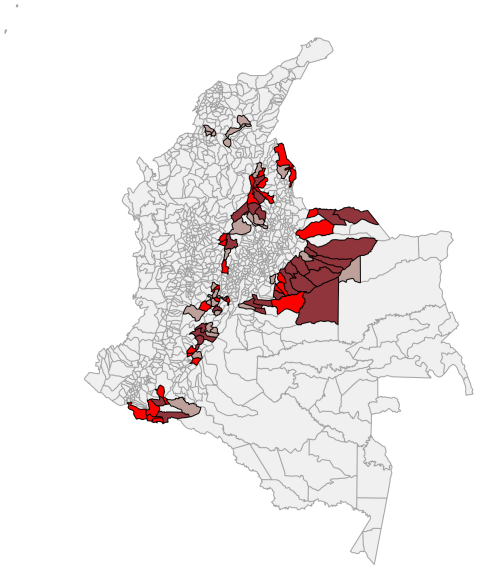
# 1 Introduction

Between mid-2014 and early 2016, the price of Brent crude oil fell from above $110 to below $30 per barrel. For most countries this represented a terms-of-trade shock absorbed gradually through macroeconomic adjustment (International Monetary Fund, 2016). For the municipalities in Colombia where oil extraction accounts for the dominant share of local economic activity, it was something more immediate: the near-collapse of the industry on which local incomes, fiscal revenues, and private-sector cash flows depended. Royalties transferred under the General Royalties System constituted the primary source of subnational fiscal revenue in these areas (Bonet-Morón et al., 2020), and the number of active exploratory wells fell from 56 in April 2014 to just 5 by April 2016 (Inter-American Dialogue, 2016). The shock was large, sudden, and geographically contained — borne disproportionately by a narrow set of municipalities whose economic base had become almost entirely dependent on a single commodity (Dube and Vargas, 2013).

This paper asks what happens to local banks when the economy they serve is hit by a shock of this magnitude. Banks in oil-producing municipalities collect deposits from households and firms whose incomes move with the price of oil, and extend credit to borrowers whose repayment capacity does the same. A contraction of this kind can therefore hit both sides of the balance sheet simultaneously: deposits may fall as local income dries up, loan performance may deteriorate as borrowers come under stress, and banks may respond by cutting credit — amplifying the original shock rather than absorbing it. Answering these questions requires identifying the local balance sheet response of specific institutions to a localized shock, separately from aggregate bank-level adjustments and macroeconomic confounds.

The Colombian setting provides an unusually clean window. Oil production is concentrated in a small number of municipalities distributed across the national territory, as Figure 1 shows, generating sharp geographic variation in exposure that is largely orthogonal to other dimensions of local economic development. Crucially, the banks serving these municipalities are the same large, geographically diversified institutions operating in unexposed areas elsewhere. Exposure to the oil shock therefore varies

Figure 1: Geographic distribution of oil-producing municipalities



*Notes:* The figure maps Colombian municipalities by pre-shock oil exposure. Oil exposure is the average annual crude production over 2010–2012; municipalities with positive production are partitioned into terciles of this distribution (low, medium, high), with darker shades indicating higher exposure. Municipalities without oil production are shown in light gray.

within banks as well as across them, allowing me to compare balance sheet behavior across locations while holding constant any bank-level response to aggregate conditions. Quarterly supervisory data from the Superintendencia Financiera de Colombia provide branch-level deposits, credit volumes by loan category, and non-performing loan ratios for the universe of regulated institutions, enabling me to trace the full balance sheet adjustment within a single unified framework.

I estimate the effects using a difference-in-differences design with bank–municipality and quarter fixed effects, implemented via the doubly-robust estimator of Wooldridge (2025). Identification rests on the parallel trends assumption, supported by clean pre-shock event-study dynamics, placebo timing tests, and randomization inference.

Three findings emerge. First, deposits in oil-producing municipalities fall by approximately 18 percent relative to non-oil areas following the shock and continue declining, with the cumulative effect deepening steadily over the post-treatment window and showing no sign of reversal. The contraction is uniform across the distribution of

oil exposure: even modestly oil-dependent municipalities experience losses of similar magnitude, indicating that the local income channel through which commodity shocks erode deposit bases operates broadly rather than selectively.

Second, the aggregate response of local credit appears muted, with estimated effects on total credit volumes statistically indistinguishable from zero. This aggregate null masks substantial heterogeneity across ownership types: domestic private banks reduce consumer lending by approximately 17 percent in oil-exposed markets, while the public commercial bank's lending does not adjust. The aggregate stability reflects this heterogeneous response across ownership types rather than a uniform absence of credit retrenchment.

Third, the credit risk associated with maintained local lending accumulates within affected markets. Non-performing loan ratios rise gradually, concentrated in consumer and commercial portfolios, and the deterioration strengthens monotonically with oil exposure intensity. The credit risk accumulation falls almost entirely on private institutions: foreign banks bear the largest NPL increase, domestic private banks a more moderate one, and the public commercial bank shows no meaningful response — despite facing deposit outflows of comparable magnitude.

These findings contribute to three strands of literature. The first concerns whether banks buffer or amplify localized real shocks. Standard theory predicts that deposit outflows tighten credit supply (Kashyap and Stein, 2000), a prediction with strong empirical support (Khwaja and Mian, 2008; Paravisini, 2008; Chodorow-Reich, 2014). I document a more nuanced pattern in a banking system dominated by geographically diversified institutions: aggregate local credit is broadly maintained, but this masks heterogeneous adjustment across ownership types. Castellani and Giaretta (2024) shows that Colombian banks reallocate lending toward non-oil municipalities in response to the same shock, and Wang (2021) documents the cross-market propagation of the U.S. shale shock. My contribution is to establish the local counterpart: in the origin markets themselves, private banks contract selectively in their most exposed loan segment while a public bank with a rural mandate does not adjust, leaving aggregate local credit broadly stable. The credit risk associated with maintained local lending accumulates almost

entirely within the institutions that did not retrench.

The second contribution concerns the informational content of liability-side and asset-side adjustments. I show that deposit outflows and NPL deterioration respond to different features of the same shock: the funding channel is broad-based and exposure intensity is immaterial, while credit quality deterioration is concentrated among the most oil-dependent localities. Monitoring deposit dynamics alone therefore understates credit risk in the most exposed areas, while aggregate NPL measures miss the uniformity of the funding pressure. The two margins of balance sheet adjustment are not interchangeable signals of local financial distress.

The third contribution concerns heterogeneous bank behavior during localized shocks. An established literature documents that foreign banks contract credit supply when hit by funding disruptions (Peek and Rosengren, 2000; Cetorelli and Goldberg, 2012) and that public banks play a stabilizing role by maintaining lending during downturns (Micco and Panizza, 2006; Bertay et al., 2015). I document that ownership structure shapes the local response on both balance sheet sides simultaneously: private banks retrench selectively in their most exposed loan segment while the public bank's lending remains stable, and the credit risk that accumulates within affected markets falls almost entirely on the private institutions that maintained their commercial and consumer portfolios. The deposit-outflow channel is uniform across ownership types, so the asymmetric pattern of credit and credit-risk responses cannot be attributed to differential funding sensitivity. It reflects instead differences in portfolio composition, borrower selection, and institutional mandate. The supervisory implication is direct: in systems with a significant public banking presence, aggregate NPL indicators structurally understate the deterioration within private institutions, and aggregate credit indicators understate the role of public-bank lending in stabilizing local credit. Both forms of fragility only become visible when ownership groups are disaggregated.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 outlines the empirical strategy. Section 5 reports the main results. Section 6 presents robustness checks. Section 7 concludes.

# 2 Institutional Background

## The Oil Sector in Colombia

The hydrocarbon sector occupies a central role in the Colombian economy. Crude petroleum is the country's largest export commodity and a primary source of fiscal revenue at both the national and subnational levels (López et al., 2012). During the commodity boom of the 2000s and early 2010s, rising global prices and new field discoveries drove a sustained expansion of oil production and export revenues. At its peak in 2013, crude oil and its derivatives accounted for approximately 55% of Colombia's total merchandise exports and generated revenues equivalent to roughly 5% of GDP and more than a fifth of central government fiscal receipts (Toro et al., 2015). As a consequence, fluctuations in global oil prices transmit swiftly and substantially into national income, government revenues, and aggregate economic activity (Toro et al., 2015; International Monetary Fund, 2016).

Oil production in Colombia is geographically concentrated in a limited number of sedimentary basins. The dominant producing region is the Llanos Orientales Basin in eastern Colombia, which hosts several of the country's largest fields and spans the departments of Meta, Casanare, and Arauca[1]. Casanare alone accounts for approximately one-fifth of national crude output (Ariza et al., 2020). Additional production occurs in the Middle Magdalena Basin, primarily in Santander, as well as in the Upper Magdalena Basin in Huila and Tolima, and in the Putumayo Basin in southern Colombia[1]. This spatial concentration implies that the economic consequences of oil price shocks are distributed unevenly across the national territory, with a small number of producing municipalities absorbing a disproportionate share of the sector's income volatility (Dube and Vargas, 2013; García-Estévez et al., 2024).

The spatial distribution of oil production has important implications for regional fiscal and economic dynamics. Colombia's hydrocarbon revenue-sharing framework, the *Sistema*

[1]U.S. Energy Information Administration, *Colombia: Background Reference*, U.S. Department of Energy. Available at: `https://www.eia.gov/international/analysis/country/COL/background` (accessed May 2026).

*General de Regalías* (SGR), allocates a portion of extraction revenues to subnational governments through royalty transfers linked directly to production activity in their territory (Bonet-Morón et al., 2020). These transfers represent a primary source of financing for public investment in producing regions, funding local infrastructure, health, and education expenditure (Fedesarrollo, 2018; Bonet-Morón et al., 2020). Beyond fiscal transfers, the hydrocarbon sector generates private-sector income through direct employment, service provision, and upstream supply-chain linkages concentrated in producing localities. Because royalties are computed as a function of extraction volumes and prices, any contraction in either variable transmits directly into municipal fiscal capacity, creating a tight link between global commodity markets and local public finances (Bonet-Morón et al., 2020).

The sharp decline in global oil prices beginning in mid-2014 constitutes the most severe shock to the sector in the period under study. Brent crude fell from above $100 per barrel in mid-2014 to below $40 per barrel by early 2016 (Figure A.1). Baumeister and Kilian (2016a) provide a structural decomposition of oil price fluctuations over the longer historical record, documenting that large price swings typically reflect a combination of supply and demand shocks whose relative contributions are difficult to establish in real time. For the 2014 episode specifically, Baumeister and Kilian (2016b) show that a substantial share of the price decline was predictable as of mid-2014, reflecting the cumulative effects of prior demand and supply developments rather than a single discrete event. For oil-producing regions in Colombia, irrespective of the global shock's origin, the consequences were direct and immediate: reduced extraction profitability, a contraction in royalty transfers under the SGR, and a broad deterioration in local economic activity (Fedesarrollo, 2018; International Monetary Fund, 2016).

## The Colombian Banking System

At the time of the oil price shock, Colombia's commercial banking sector was dominated by a small number of large institutions with nationwide branch networks. The five largest banks — Bancolombia, Banco de Bogotá, Davivienda, BBVA Colombia, and Banco

Popular — collectively accounted for the bulk of system assets and operated branches across hundreds of municipalities, ranging from large urban centers to small rural localities (Arvai and Letelier, 2022).

A distinctive feature of the system is its ownership structure. Domestic private banks represent the largest share of system assets, with foreign-owned banks — principally BBVA Colombia — also holding a significant presence. Colombia additionally operates a state-owned commercial bank, Banco Agrario de Colombia, whose statutory mandate is to promote financial access in rural and agricultural regions. Banco Agrario maintains one of the most extensive branch networks among smaller and more remote municipalities, and acts as the primary formal financial intermediary in localities where private banks have limited or no presence Arvai et al. (2023). These three ownership categories — foreign, domestic private, and public — differ systematically in their client base, portfolio composition, and institutional objectives, and are all present, to varying degrees, in oil-producing municipalities.

Because the major banks operate branches across the entire national territory, the same institution is simultaneously present in municipalities that are heavily exposed to the oil sector and in municipalities with no extractive activity. This within-bank geographic dispersion means that oil-related variation in local economic conditions generates differential balance sheet pressure across branches of the same institution — the feature of the Colombian banking system that my empirical strategy exploits.

# 3 Data

My analysis combines supervisory data on local bank balance sheets with municipality-level information on oil production. The unit of observation in the main analysis is a bank-municipality-quarter.

## 3.1 Banking data

The banking data are drawn from the mandatory regulatory filings collected by the Superintendencia Financiera de Colombia (SFC), the country's unified financial supervisor. The SFC requires all regulated institutions to report balance sheet positions at the branch level on a quarterly basis, covering outstanding credit by regulatory loan category (commercial, consumer, microcredit, and mortgage), non-performing loans by category, and total deposits. Because these records originate from supervisory filings rather than surveys, coverage is comprehensive and measurement error in reported positions is limited by regulatory audit requirements.

I retain first-tier institutions that lend directly to the public, comprising commercial banks, financial corporations, and lending cooperatives. Second-tier development banks are excluded because their balance sheets reflect wholesale funding structures and policy mandates that are not comparable to those of retail intermediaries. Within first-tier institutions, I drop bank-municipality-quarter observations recording both zero credit and zero deposits, which correspond to dormant relationships rather than active intermediation. The estimation sample spans 2010q1 to 2019q4. The start date aligns the banking panel with the availability of municipality-level oil production records and provides a six-year pre-period prior to the price collapse that began in mid-2014. The sample ends in 2019q4 to avoid contamination from the COVID-19 pandemic, which generated large concurrent disruptions to both deposit and credit markets.

The resulting dataset contains 109,691 observations corresponding to 3,743 bank-municipality pairs, spanning 870 municipalities and 69 financial institutions over 40 quarters. The panel is unbalanced: 65.8% of bank-municipality pairs are observed in every quarter, while the remaining pairs exhibit gaps concentrated at entry and exit margins.

Table A.1 reports summary statistics. Credit portfolios are dominated by commercial and consumer loans, while mortgage credit is the thinnest category — present in roughly half of all observations — reflecting the limited development of long-term housing finance outside major urban centers. Non-performing loan ratios vary substantially across categories, from 2.8% for mortgage loans, where collateral discipline contains default, to

8.2% for microcredit, where borrowers lack the income stability and credit history typical of the commercial and consumer segments.

The three ownership groups differ in ways that go beyond scale. Domestic private and foreign banks display similar intermediation profiles, with credit volumes closely tracking deposit volumes in both cases. The public bank, Banco Agrario, is structurally different: it collects deposits broadly across rural municipalities but concentrates its lending in small-scale agricultural portfolios, generating a gap of more than three log points between average deposits and average credit outstanding. As a result, its exposure to credit risk from any given local income shock operates through a borrower base that is categorically distinct from that of private institutions — a distinction that proves central to interpreting the ownership heterogeneity documented in Section 5. Table A.2 reports separate summary statistics by ownership group.

## 3.2 Oil Production and Exposure

Municipality-level oil production data are obtained from the Agencia Nacional de Hidrocarburos (ANH), which reports annual crude output and royalty payments by municipality and field. Oil exposure is measured using average municipal production over 2010–2012, three years before the onset of the price collapse, ensuring that treatment status is entirely pre-determined with respect to the shock.

The production distribution has two features that shape the empirical design. First, it is spatially sparse: only 85 of 870 municipalities in the sample record positive output during the pre-period, and these cluster in the eastern plains and the middle Magdalena valley, as shown in Figure 1. Second, it is heavily right-skewed even within this group. The median producing municipality outputs approximately 512,000 barrels per year, while production at the 90th percentile is more than eleven times larger, and the single largest producer accounts for an order of magnitude more than the median. This concentration means that a binary treatment indicator conflates municipalities that are marginally exposed with those whose entire economic base depends on extraction.

I therefore construct two complementary exposure measures. The binary indicator

— equal to one for any municipality with positive pre-period production — defines the treatment group for the baseline difference-in-differences specification and delivers the most conservative estimate of the shock's reach. To trace how responses scale with exposure intensity, I partition treated municipalities into terciles of the pre-period production distribution. The tercile partition is the primary tool for the dose-response analysis in Section 5: it tests whether the deposit and credit quality responses are uniform across oil municipalities or concentrated among the most exposed localities, and whether the two margins of adjustment encode the same information about local shock severity.

# 4 Empirical Strategy

I study how the 2014 collapse in international oil prices affected local banking outcomes by comparing municipalities with pre-existing oil production to those without, before and after the shock. The unit of observation is the bank–municipality–quarter, and standard errors are clustered at the bank–municipality level throughout.

Let $Y_{bmt}$ denote a banking outcome for bank $b$ in municipality $m$ at quarter $t$. The baseline estimating equation is:

$$Y_{bmt} = \beta \left(\text{Oil}_m \times \text{Post}_t\right) + \mathbf{X}_m \times \lambda_t + \mu_{bm} + \lambda_t + \varepsilon_{bmt}, \tag{1}$$

where $\text{Oil}_m$ is an indicator equal to one for municipalities with positive average crude production during 2010–2012, and $\text{Post}_t$ equals one from 2014q3 onward. Bank–municipality fixed effects $\mu_{bm}$ absorb all time-invariant heterogeneity at the unit level; quarter fixed effects $\lambda_t$ control for aggregate shocks common to all units. The vector $\mathbf{X}_m$ contains pre-shock municipal averages of deposits, credit, non-performing loan ratios, and bank branch presence, each interacted with quarter effects to allow municipalities with different initial conditions to follow distinct trends.[2] The coefficient $\beta$ identifies the

[2] I also estimate the full event-study version of equation (1), replacing $\text{Oil}_m \times \text{Post}_t$ with a vector of period-by-period interactions $\sum_{e \neq -1} \tau_e (D_m \cdot \mathbf{1}\{t - t_0 = e\})$, where $e$ indexes quarters relative to the shock onset $t_0 = 2014\text{q3}$ and $e = -1$ is the omitted period. The pre-treatment coefficients $\{\tau_e\}_{e<-1}$ serve as a diagnostic for parallel trends; the post-treatment coefficients $\{\tau_e\}_{e \geq 0}$ trace the dynamic evolution of the treatment effect. I report event-study plots and a joint $F$-test of the pre-treatment coefficients for all

average treatment effect on the treated (ATT) under the assumption that, absent the price collapse, banking outcomes in oil and non-oil municipalities would have evolved in parallel conditional on $\mathbf{X}_m$.

I implement equation (1) using the doubly-robust difference-in-differences estimator of Wooldridge (2025). The estimator combines an outcome regression with inverse probability weighting, delivering consistency provided that at least one of the two models is correctly specified. Relative to standard two-way fixed effects, it avoids the negative weighting problem that arises when treatment effect heterogeneity interacts with the panel structure (Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020), and accommodates pre-shock covariate adjustment through the interactions $\mathbf{X}_m \times \lambda_t$ under a conditional parallel trends assumption. I estimate all specifications with and without these covariate interactions to assess sensitivity to that assumption.

The key identifying assumption is that oil and non-oil municipalities would have followed parallel trends in banking outcomes absent the shock, conditional on pre-shock characteristics. Three features of the setting support this assumption. First, the price collapse originated in global supply and demand conditions — the expansion of U.S. shale production and OPEC's decision to maintain output — entirely unrelated to local banking dynamics in Colombia, which accounts for a negligible share of world supply. Second, oil production is determined by geological endowments and long-run investment decisions, making treatment status pre-determined with respect to any post-2014 banking outcome. Third, the pre-shock covariate interactions $\mathbf{X}_m \times \lambda_t$ relax the parallel trends requirement by allowing trends to differ systematically across observable municipal characteristics, so that identification requires only that unobservable determinants of local banking dynamics are uncorrelated with oil status conditional on pre-shock levels of deposits, credit, loan quality, and bank presence. The event-study estimates, together with placebo timing tests and randomization inference, provide direct empirical support for this assumption.

main outcomes.

# 5 Main Results

## 5.1 Local Funding

Table 1 reports the effect of the oil price collapse on local bank deposits. The aggregate ATT is −0.200 log points in the controlled specification (column 2), implying that deposits in oil-producing municipalities fall by approximately 18 percent relative to non-oil municipalities; the uncontrolled estimate is similar at −0.212 (−19 percent). The estimate is stable across specifications with and without pre-treatment controls, and the full-sample pre-trend test is insignificant in both.

Table 1: Oil price shock and local bank deposits

| | Full sample | | Tercile 1 (low) | | Tercile 2 (medium) | | Tercile 3 (high) | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treat × Post | −0.212*** | −0.200*** | −0.290*** | −0.261*** | −0.124 | −0.113 | −0.245*** | −0.240*** |
| | (0.050) | (0.051) | (0.075) | (0.078) | (0.098) | (0.098) | (0.063) | (0.064) |
| Pre-trend test ($p$-value) | 0.280 | 0.319 | 0.007 | 0.055 | 0.524 | 0.861 | 0.678 | 0.870 |
| Bank–municipality FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | No | Yes | No | Yes | No | Yes | No | Yes |
| Observations | 106,502 | 106,396 | 95,746 | 95,640 | 97,936 | 97,830 | 100,270 | 100,164 |
| Clusters | 3,465 | 3,451 | 3,130 | 3,116 | 3,199 | 3,185 | 3,280 | 3,266 |
| Adj. $R^2$ | 0.875 | 0.875 | 0.872 | 0.872 | 0.871 | 0.871 | 0.875 | 0.876 |
| *Panel B: Pairwise equality tests (controlled specifications)* | | | | | | | | |
| Tercile 1 − Tercile 2 | | | −0.148 | $z = -1.18$ | $p = 0.237$ | | | |
| Tercile 1 − Tercile 3 | | | −0.020 | $z = -0.20$ | $p = 0.841$ | | | |
| Tercile 2 − Tercile 3 | | | 0.127 | $z = 1.09$ | $p = 0.275$ | | | |

*Notes:* The table reports average treatment effects on the treated (ATT) estimated using the doubly-robust difference-in-differences estimator of Wooldridge (2025). The dependent variable is the natural logarithm of total deposits at the bank–municipality level. Columns (1)–(2) use the full sample of oil-producing municipalities as the treated group; columns (3)–(8) split treated municipalities into terciles of average pre-shock oil production (2010–2012), comparing each tercile separately to the never-treated group. Even-numbered columns add pre-treatment municipal controls — average deposits, credit, NPL ratio, and branch count — each interacted with quarter fixed effects. All specifications include bank–municipality and quarter fixed effects. Panel B reports pairwise $z$-tests of equality between tercile-specific ATTs from the controlled specifications (even-numbered columns), with standard errors computed as $\sqrt{\mathrm{SE}(\hat{\tau}_j)^2 + \mathrm{SE}(\hat{\tau}_k)^2}$ under the assumption of independence across subsamples. Standard errors, reported in parentheses, are clustered at the bank–municipality level. The pre-trend $p$-value reports the joint significance of pre-treatment event-time coefficients. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The estimate implies that deposits in oil municipalities contracted by approximately 18 percent on average across the post-shock period, with the cumulative decline deepening over time and reaching roughly 33 percent by the final quarter of the sample (event-time

coefficient of -0.39; Figure 2), with no sign of mean reversion. The mechanism is direct: as international prices collapsed, royalty transfers to oil-producing municipalities fell sharply (Fedesarrollo, 2018), compressing the cash flows of both households and local firms and reducing their capacity to maintain deposit balances. Gilje et al. (2016) document an analogous transmission of local commodity activity through deposits in U.S. counties during the shale boom, indicating that the local income channel through which commodity shocks affect deposit bases operates similarly across institutional settings.

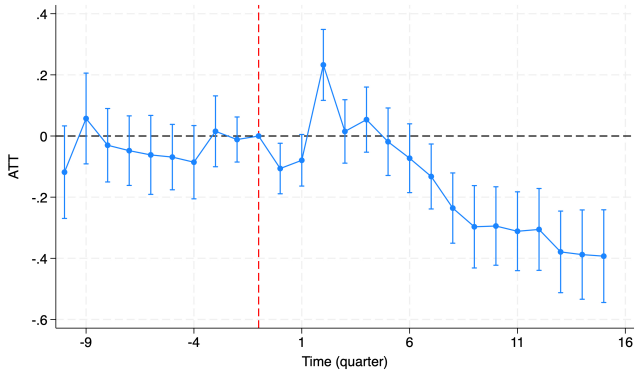
The dose-response estimates in columns (3)–(8) show that the contraction is broad-based rather than concentrated among the most oil-dependent localities. Point estimates are negative and significant for the low- and high-exposure terciles, and pairwise tests fail to reject equality across all three groups (Table 1). Even modest oil dependence is sufficient to generate large and persistent funding pressure. One caveat applies to the lowest-exposure tercile: the unconditional pre-trend test rejects parallel trends. Conditioning on pre-shock municipal characteristics — the specification of Wooldridge (2025) is designed to recover treatment effects under conditional rather than unconditional parallel trends — reduces this to the margin of conventional significance. The Tercile 1 estimate should be read with this caveat in mind; pre-trend tests for the medium- and high-exposure terciles, where the dose-response gradient is identified, are clean throughout.

Figure 2 traces the dynamic adjustment. Pre-treatment coefficients are close to zero and jointly insignificant, consistent with parallel trends and no anticipation of the shock. Following the price collapse, deposits decline gradually and persistently, deepening over the post-treatment window with no sign of stabilization — a pattern that reflects the sustained nature of the oil price decline and the lagged pass-through of reduced royalty transfers to local income and firm cash flows.

Table A.3 shows that the contraction is uniform across ownership types.[3] The ATT is negative and significant for domestic private banks ($-0.269$), foreign banks ($-0.491$), and the public bank ($-0.217$), and pairwise $z$-tests fail to reject equality across all three pairs

[3]See Figure A.2 for the event-study counterpart

Figure 2: Effect of the oil price shock on local bank deposits: event-study estimates




*Notes:* The figure reports event-study estimates of the effect of the 2014 oil price collapse on local bank deposits. Coefficients are obtained using the doubly-robust estimator of Wooldridge (2025). The dependent variable is the natural logarithm of total deposits at the bank–municipality level. Each coefficient corresponds to the ATT at quarter $e$ relative to the shock onset, normalized to zero at $e = -1$. The estimation window spans ten quarters before to fifteen quarters after the shock. The specification includes bank–municipality and quarter fixed effects. The vertical line marks the onset of the oil price collapse (2014q3). Error bars represent 95% confidence intervals based on standard errors clustered at the bank–municipality level.

(Table A.3).[4] When the shock originates locally, ownership structure does not insulate any institution from the funding consequences.

## 5.2 Credit Supply

Standard theories of financial intermediation predict that a contraction in deposit funding tightens credit supply (Kashyap and Stein, 2000), a prediction with strong empirical support in settings where liquidity shocks can be cleanly identified (Khwaja and Mian, 2008). Table 2 reports estimated treatment effects on total credit and on each loan category: all are statistically indistinguishable from zero. The aggregate response would seem to reject the standard prediction. The aggregate result, however, masks substantial heterogeneity across ownership types. I split the sample by bank ownership and re-estimate equation (1) separately for domestic private, foreign, and public institutions.

Table A.4 reports the results: domestic private banks reduce total local credit by

[4]The three subsamples are non-overlapping, so $\text{Var}(\hat{\tau}_j - \hat{\tau}_k) = \text{Var}(\hat{\tau}_j) + \text{Var}(\hat{\tau}_k)$. The larger standard error for foreign banks reflects the smaller subsample size — 535 clusters versus 1,299 and 753.

approximately 17 percent ($exp(-0.186) - 1$); the public bank's lending does not adjust; and the foreign-bank estimate is large in magnitude but imprecisely estimated and should not be interpreted as a credit expansion.[5] The aggregate null reflects a composition effect: private-bank contraction in oil municipalities is not mirrored at the public bank, whose lending remains stable.

Table 2: Oil price shock and local bank credit

| | Total credit | | Commercial | | Consumer | | Microcredit | | Mortgage | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Treat × Post | 0.125 | 0.145 | −0.012 | −0.038 | 0.256 | 0.271 | −0.106 | −0.096 | 0.087 | 0.144 |
| | (0.154) | (0.157) | (0.212) | (0.213) | (0.187) | (0.188) | (0.171) | (0.171) | (0.284) | (0.289) |
| Bank–municipality FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| Observations | 108,808 | 108,699 | 108,808 | 108,699 | 108,808 | 108,699 | 108,808 | 108,699 | 108,808 | 108,699 |
| Clusters | 3,665 | 3,648 | 3,665 | 3,648 | 3,665 | 3,648 | 3,665 | 3,648 | 3,665 | 3,648 |
| Adj. $R^2$ | 0.924 | 0.924 | 0.881 | 0.881 | 0.935 | 0.935 | 0.917 | 0.917 | 0.819 | 0.820 |

*Notes:* The table reports average treatment effects on the treated (ATT) estimated using the doubly-robust difference-in-differences estimator of Wooldridge (2025). The dependent variable in each column pair is the natural logarithm of the indicated credit category at the bank–municipality level. Odd-numbered columns present baseline estimates; even-numbered columns add pre-treatment municipal controls — average deposits, credit, NPL ratio, and branch count — each interacted with quarter fixed effects. All specifications include bank–municipality and quarter fixed effects. Standard errors, reported in parentheses, are clustered at the bank–municipality level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.5 localizes the private-bank contraction within the loan portfolio. The cleanest pattern emerges in consumer credit: domestic private banks cut consumer lending by approximately 17 percent, with a clean pre-trend, while the public bank shows no significant response in the cleanly-identified specifications.[6] Commercial credit displays no significant adjustment for any ownership group. Microcredit and mortgage estimates for domestic private banks fail pre-trend tests at conventional levels and are excluded from the discussion. The consumer segment where private banks contract credit is also where they absorb the largest credit-quality deterioration, alongside the commercial portfolio where credit volumes do not adjust but non-performing loan ratios still rise (Section 5.3).

[5] The pairwise difference between the public bank and domestic private institutions is significant at the 5 percent level.

[6] Several panels of Table A.5 fail pre-trend tests at conventional levels even after conditioning on pre-shock municipal characteristics. The discussion in this section features only those estimates with clean pre-trends; the remaining estimates are reported for completeness.

The headline "no retrenchment" framing therefore requires qualification. Local credit in aggregate is held stable, but the stability reflects a heterogeneous response across ownership types rather than a uniform absence of adjustment: private banks selectively retrench in their most exposed segment, while the public bank's lending remains stable. This pattern is the local-market complement to Castellani and Giaretta (2024), who shows that geographically diversified Colombian banks reallocate lending toward non-oil municipalities in response to the same shock. The two findings together suggest that during localized shocks, private banks combine cross-market reallocation with selective within-market contraction, while a public bank with a rural mandate continues to lend in origin markets. The cost of maintaining aggregate local credit, however, is borne unevenly: as the next section shows, the credit risk that accumulates within affected markets falls almost entirely on the institutions that did not retrench.

## 5.3 Credit quality

Maintaining local credit supply in the face of a persistent funding shock does not preclude deterioration on the asset side. As borrower fundamentals weaken with the contraction in oil revenues, banks that did not fully retrench absorb part of the shock through rising loan distress rather than quantity adjustment. Table 3 confirms this: the aggregate NPL ratio increases by 0.6 percentage points following the shock — a deterioration of roughly 16 percent relative to the pre-shock mean of 3.7 percent. The effect is concentrated in consumer and commercial portfolios, where the estimates are 0.012 and 0.015 respectively, both statistically significant, while microcredit and mortgage portfolios show no detectable response. Pre-trend tests are uniformly clean across all categories.

The dynamic pattern, shown in Figure 3, clarifies the timing. NPL ratios in oil municipalities track those in non-oil municipalities closely in the quarters immediately following the shock, then diverge gradually, with effects emerging after several quarters and stabilizing at roughly 1–1.5 percentage points above the non-oil baseline. This delayed trajectory reflects the mechanics of loan distress: borrower fundamentals deteriorate incrementally as income and employment contract, and loan classifications lag behind the

Table 3: Oil price shock and non-performing loan ratios

| | Total (1) | Commercial (2) | Consumer (3) | Microcredit (4) | Mortgage (5) |
|---|---|---|---|---|---|
| Treat × Post | 0.006** | 0.015* | 0.012** | 0.007 | −0.001 |
| | (0.003) | (0.008) | (0.005) | (0.007) | (0.006) |
| Pre-trend test ($p$-value) | 0.476 | 0.333 | 0.224 | 0.627 | 0.608 |
| Bank–municipality FE | Yes | Yes | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes |
| Observations | 100,684 | 93,621 | 89,812 | 70,484 | 57,007 |
| Clusters | 3,326 | 3,115 | 2,864 | 2,349 | 1,936 |
| Adj. $R^2$ | 0.431 | 0.371 | 0.216 | 0.316 | 0.599 |

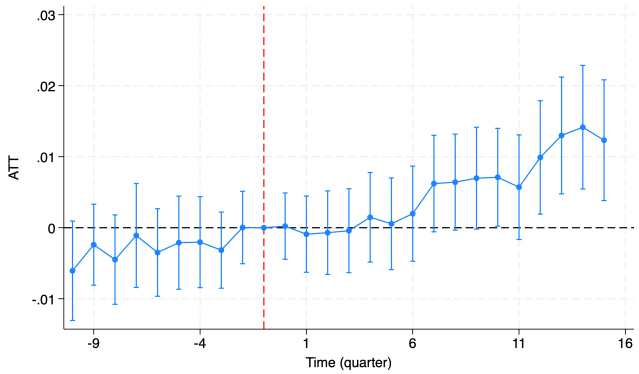
*Notes:* The table reports average treatment effects on the treated (ATT) estimated using the doubly-robust difference-in-differences estimator of Wooldridge (2025). The dependent variable in each column is the non-performing loan ratio for the indicated loan category, defined as the share of non-performing loans in total outstanding loans of that type. All specifications include bank–municipality and quarter fixed effects and pre-treatment municipal controls — average deposits, credit, NPL ratio, and branch count — each interacted with quarter fixed effects. Standard errors, reported in parentheses, are clustered at the bank–municipality level. The pre-trend $p$-value reports the joint significance of pre-treatment event-time coefficients. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. See Figure A.5 for the event-study counterpart

underlying deterioration in cash flows (Bernanke, 1983). The contrast with the deposit response — which is also gradual but begins earlier and continues deepening — suggests that the liability and asset sides of the balance sheet respond to different features of the same shock.

The ownership decomposition sharpens the picture considerably. Table A.6 reveals a monotonic gradient: foreign banks bear the largest NPL increase, with the ratio rising by approximately 2.1 percentage points from a pre-shock mean of 3.7 percent— although their corresponding credit response in Section 5.2 is too imprecise to characterize; domestic private banks show a smaller but meaningful NPL increase of around 0.7 percentage points and are also the ownership group with the cleanest credit-supply contraction; and the public bank, which did not retrench in lending, registers no NPL deterioration.[7] All three pairwise differences in NPL responses are statistically significant. This gradient is not driven by differential funding pressure — deposit outflows are comparable across ownership

[7]See Figure A.4 for the event-study counterpart.

Figure 3: Effect of the oil price shock on the aggregate non-performing loan ratio: event-study estimates




*Notes:* The figure reports event-study estimates of the effect of the 2014 oil price collapse on the aggregate non-performing loan ratio. Coefficients are obtained using the doubly-robust estimator of Wooldridge (2025). The dependent variable is the ratio of non-performing loans to total loans at the bank–municipality level. Each coefficient corresponds to the ATT at quarter $e$ relative to the shock onset, normalized to zero at $e = -1$. The estimation window spans ten quarters before to fifteen quarters after the shock. The specification includes bank–municipality and quarter fixed effects and pre-treatment municipal controls interacted with quarter fixed effects. The vertical line marks the onset of the oil price collapse (2014q3). Error bars represent 95% confidence intervals based on standard errors clustered at the bank–municipality level.

groups, as established above — but instead reflects differences in portfolio composition and borrower selection. Foreign and domestic private banks are more concentrated in commercial and consumer lending to oil-sector clients, while the public bank's agricultural portfolio is less directly tied to hydrocarbon income. The result carries a direct implication for financial supervision: in systems with a significant public banking presence, aggregate NPL indicators structurally understate the credit risk accumulating within private institutions during regional downturns.

# 6 Robustness

## 6.1 Alternative Exposure Measures

My baseline treatment indicator discretizes a continuous underlying variable, which can introduce bias in difference-in-differences settings with heterogeneous treatment intensity (Callaway et al., 2024; Roth et al., 2023). Table A.7 examines sensitivity to alternative

definitions that vary both the partition and the composition of the treated group[8].

The deposit results are stable throughout. A median split (columns 1–2) yields negative and significant effects on both sides, with larger declines among more exposed municipalities. Restricting to the top quartile of production (column 3) reproduces the baseline estimate closely, ruling out upward bias from weakly exposed units. Dropping the lowest-exposure tercile (column 4) confirms the same.

The NPL results tell a different story. Below-median municipalities show no significant deterioration (column 5), while above-median municipalities experience a 1.2 percentage point increase (column 6), strengthening to 1.4 percentage points among the top quartile (column 7). This exposure gradient reinforces the pattern from the dose-response analysis: deposit outflows are broad-based and insensitive to exposure intensity, while credit quality deterioration is concentrated among the most oil-dependent localities. The two margins of balance sheet adjustment therefore respond to different features of the underlying shock — a result that is robust to how treatment intensity is measured.[9]

## 6.2 Alternative Sample Windows

A long estimation window raises two standard concerns: parallel trends may be harder to sustain over extended pre-periods, and long post-periods may absorb dynamics unrelated to the initial shock — most notably the partial recovery in oil prices that began in late 2016. Table A.8 addresses both.

Deposit results are stable across all configurations. Shortening the pre-period to six quarters yields an ATT of $-0.195$, nearly identical to the baseline; truncating the post-period to eight quarters produces a smaller but still significant effect of $-0.096$, consistent with the gradual buildup visible in the event study; and ending the sample in 2017q4 or starting in 2012q1 leaves estimates in the range $-0.158$ to $-0.197$. Pre-trend tests are insignificant throughout.

[8] see Figures A.3 and A.6 for the event-study counterparts

[9] The credit-supply findings, which the main analysis identifies through ownership decomposition rather than exposure intensity, are not subjected to the dose-response robustness checks; the underlying ownership-by-category estimates in Table A.5 report pre-trend tests for each cell.

The NPL results reflect the timing documented in Figure 3. The effect is positive and significant when the window spans a sufficiently long post-treatment horizon (columns 5 and 8), but attenuates toward zero when the post-period is truncated or ends before the deterioration fully materializes (columns 6–7). This is not fragility — it is a direct consequence of the delayed adjustment mechanism: shorter windows simply capture an earlier phase of a process that takes several quarters to become detectable. Taken together, the two outcomes tell a consistent story across all window definitions: deposit contraction is immediate and robust, while credit quality deterioration is gradual and requires a sufficiently long horizon to measure precisely.

## 6.3 Alternative Clustering

My baseline standard errors are clustered at the bank–municipality level, which accounts for serial correlation within the panel unit but may not capture all relevant dimensions of error dependence. Treatment is assigned at the municipality level, and banks operating within the same municipality share common local shocks, while geographically diversified banks may transmit shocks across municipalities through internal capital markets. Following Colin Cameron and Miller (2015), I report results under three alternative clustering schemes.

Table A.10 shows that the deposit ATT of $-0.200$ is precisely estimated under every alternative. Municipality-level clustering (column 2, 841 clusters) yields a slightly tighter standard error of 0.045 relative to the baseline 0.051 — a compression that reflects the within-municipality correlation structure of residuals rather than a mechanical artifact (Colin Cameron and Miller, 2015). Bank-level clustering (column 3) produces comparable standard errors, indicating the result is not driven by a small number of institutions. Department-level clustering (column 4) is the most conservative choice and operates near the lower bound where cluster-robust inference is well-behaved; even so, the estimate remains significant. As Abadie et al. (2023) note, clustering adjustments are most consequential when treatment assignment aligns with cluster boundaries — here treatment varies within departments, so department-level clustering likely over-aggregates

relative to the actual level of treatment variation.

For NPL ratios, point estimates are stable throughout and standard errors vary only modestly. Where significance is lost under the most conservative scheme, this reflects reduced precision under stricter assumptions about within-cluster correlation, not a change in the underlying effect.

## 6.4 Placebo Tests and Randomization Inference

I complement the pre-trend diagnostics with two exercises that test whether the estimated effects are specific to the actual shock date and the actual set of treated municipalities.

**Alternative treatment timing.** Restricting the sample to the pre-treatment period, I re-estimate the baseline specification under four placebo treatment dates: 2011q3, 2012q1, 2012q3, and 2013q1. If the identifying assumption holds, none of these should generate effects resembling the true estimates. Table A.9 confirms this: placebo ATTs for both deposits and NPL ratios are small, statistically insignificant, and show no consistent pattern across dates — in sharp contrast to the pronounced and precisely estimated effects at the true shock date of 2014q3.

**Randomization inference.** I fix the treatment date at 2014q3 and randomly reassign oil municipality status across 500 permutations, each time drawing a treated group of the same size as the actual one and re-estimating the baseline specification. The resulting empirical distribution of placebo ATTs provides a finite-sample benchmark for evaluating whether the observed estimates are unusual relative to random assignment (Young, 2019; Athey and Imbens, 2022).

Figure A.7 shows the results. Both placebo distributions are centered near zero and approximately symmetric. For deposits, the true ATT of $-0.200$ lies deep in the left tail: only a negligible share of the 500 permutations generate effects of comparable magnitude (two-sided RI $p$-value $< 0.01$). For the NPL ratio, the true ATT occupies a similarly extreme position in the right tail, with equally few placebo draws reaching that threshold.

The main findings are not an artifact of which municipalities happen to produce oil.

# 7 Conclusion

This paper traces the transmission of a large localized commodity shock through local bank balance sheets, exploiting the geographic concentration of oil production in Colombia and the 2014–16 price collapse as a source of quasi-experimental variation. Three results emerge. Deposits in oil-producing municipalities contract by approximately 18 percent on average across the post-shock period and continue declining, with no mean reversion over a five-year horizon. Local lending in aggregate is broadly maintained, but this aggregate stability masks a heterogeneous response across ownership types: domestic private banks contract consumer lending in the most exposed markets, while the public commercial bank's lending remains stable. Non-performing loan ratios rise gradually, in proportion to local oil dependence, concentrated in consumer and commercial portfolios.

The ownership decomposition sharpens this picture in ways that carry implications beyond this episode. Foreign banks accumulate the largest increase in credit risk, domestic private banks a more moderate one, and the public commercial bank none — despite facing deposit outflows of comparable magnitude. The credit risk that accumulates within affected markets falls almost entirely on the private institutions that maintained their commercial portfolios while only selectively retrenching in consumer credit; the public bank, which did not retrench, shows no asset-side deterioration. The gradient cannot be attributed to differential funding pressure — deposit outflows are comparable across ownership groups — but reflects deeper differences in portfolio composition, borrower selection, and institutional mandate. The supervisory consequence is concrete: in systems with a significant public banking presence, aggregate non-performing loan indicators mechanically understate the deterioration concentrated within private institutions, and aggregate credit indicators understate the role of public-bank lending in stabilizing local credit. Both forms of fragility only become visible when ownership groups are disaggregated.

The aggregate stability of local credit speaks to the broader role of geographically diversified banks in containing localized shocks. That local credit is broadly maintained at the origin while, as Castellani and Giaretta (2024) documents, the same banks reallocate lending toward non-oil markets, suggests substantial balance sheet flexibility in banking systems dominated by geographically diversified institutions. The aggregate stability is not, however, achieved through uniform balance sheet absorption: private banks contract selectively while a public bank with a rural mandate maintains its lending, and the credit risk associated with maintained local lending accumulates within the institutions that did not retrench. The picture that emerges is one of aggregate stability of local credit sustained by an ownership-differentiated division of labor, at the price of concentrated risk accumulation within private institutions — a pattern that aggregate indicators are structurally ill-equipped to detect, and one that deserves closer attention from supervisors as commodity price cycles continue to generate localized economic distress.

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# A Appendix

Table A.1: Summary statistics

| | Full sample | | | Oil municipalities | | Non-oil municipalities | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | SD | Obs. | Mean | Obs. | Mean | Obs. | Diff. |
| *Panel A: Deposits and credit (log)* | | | | | | | | |
| Log total deposits | 26.77 | 2.67 | 107,362 | 26.90 | 13,637 | 26.75 | 93,725 | $-0.16^{***}$ |
| Log total credit | 26.09 | 3.21 | 101,640 | 26.47 | 13,016 | 26.03 | 88,624 | $-0.43^{***}$ |
| Log commercial credit | 23.90 | 3.81 | 94,532 | 24.54 | 12,225 | 23.80 | 82,307 | $-0.74^{***}$ |
| Log consumer credit | 24.76 | 3.96 | 90,707 | 25.48 | 11,591 | 24.66 | 79,116 | $-0.82^{***}$ |
| Log microcredit | 22.58 | 2.67 | 71,295 | 22.73 | 9,043 | 22.56 | 62,252 | $-0.17^{***}$ |
| Log mortgage credit | 24.99 | 3.38 | 57,412 | 25.17 | 7,911 | 24.96 | 49,501 | $-0.22^{***}$ |
| *Panel B: Non-performing loan ratios* | | | | | | | | |
| NPL ratio (total) | 0.041 | 0.050 | 101,640 | 0.045 | 13,016 | 0.041 | 88,624 | $-0.004^{***}$ |
| NPL ratio (commercial) | 0.072 | 0.132 | 94,532 | 0.072 | 12,225 | 0.072 | 82,307 | $-0.000$ |
| NPL ratio (consumer) | 0.047 | 0.076 | 90,707 | 0.048 | 11,591 | 0.047 | 79,116 | $-0.001^{*}$ |
| NPL ratio (microcredit) | 0.082 | 0.136 | 71,295 | 0.093 | 9,043 | 0.080 | 62,252 | $-0.013^{***}$ |
| NPL ratio (mortgage) | 0.028 | 0.134 | 57,412 | 0.025 | 7,911 | 0.029 | 49,501 | $0.004^{**}$ |
| *Panel C: Oil exposure* | | | | | | | | |
| Oil-producing municipality | 0.127 | 0.333 | 109,691 | 1.000 | 13,977 | 0.000 | 95,714 | — |
| Standardized oil exposure | 0.066 | 0.484 | 109,691 | 0.517 | 13,977 | 0.000 | 95,714 | — |
| Brent crude price (USD/bbl) | 79.46 | 25.88 | 109,691 | 79.22 | 13,977 | 79.49 | 95,714 | — |

*Notes:* The table reports summary statistics at the bank–municipality–quarter level over the sample period 2010q1–2019q4. Log variables are natural logarithms of COP-denominated amounts. NPL ratios are the share of non-performing loans in total outstanding loans by category, winsorized at the 1st and 99th percentiles. *Oil-producing municipality* is an indicator equal to one if the municipality recorded positive average crude output during 2010–2012. *Standardized oil exposure* is average annual crude production over 2010–2012 normalized by the cross-sectional standard deviation among oil-producing municipalities. The *Diff.* column reports the difference in means between oil and non-oil municipalities, with standard errors clustered at the municipality level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.2: Summary statistics by bank ownership

| | Domestic private | | Foreign | | Public | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD |
| Observations | 45,125 | | 15,110 | | 29,072 | |
| *Panel A: Deposits and credit (log)* | | | | | | |
| Log total deposits | 27.73 | 2.17 | 26.99 | 2.91 | 26.85 | 0.93 |
| Log total credit | 27.84 | 2.32 | 27.35 | 3.06 | 23.49 | 1.58 |
| *Panel B: Non-performing loan ratios* | | | | | | |
| NPL ratio (total) | 0.037 | 0.040 | 0.032 | 0.037 | 0.054 | 0.060 |
| NPL ratio (commercial) | 0.058 | 0.115 | 0.046 | 0.115 | 0.112 | 0.151 |
| NPL ratio (consumer) | 0.049 | 0.067 | 0.044 | 0.067 | 0.042 | 0.083 |
| NPL ratio (microcredit) | 0.097 | 0.172 | 0.070 | 0.166 | 0.070 | 0.062 |
| *Panel C: Oil exposure and pre-period controls* | | | | | | |
| Oil-producing municipality | 0.144 | 0.351 | 0.161 | 0.368 | 0.108 | 0.310 |
| Standardized oil exposure | 0.082 | 0.521 | 0.102 | 0.652 | 0.034 | 0.367 |
| Pre-period NPL ratio | 0.035 | 0.018 | 0.037 | 0.017 | 0.050 | 0.054 |

*Notes:* The table reports summary statistics separately by bank ownership category over the sample period 2010q1–2019q4. *Domestic private* refers to Colombian-owned commercial institutions. *Foreign* refers to banks with majority foreign ownership, principally BBVA Colombia. *Public* refers to Banco Agrario de Colombia, a state-owned institution whose statutory mandate to serve rural depositors while concentrating lending in small-scale agricultural portfolios accounts for the gap of more than three log points between its average deposits (26.85) and average credit (23.49). All other variables are as defined in Table A.1.

Table A.3: Oil price shock and local bank deposits by ownership

| | Log total deposits | | |
|---|---|---|---|
| | Domestic private (1) | Foreign (2) | Public (3) |
| Treat × Post | −0.269*** | −0.491*** | −0.217*** |
| | (0.063) | (0.183) | (0.053) |
| Pre-trend test ($p$-value) | 0.256 | 0.449 | 0.394 |
| Bank–municipality FE | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes |
| Observations | 44,295 | 14,336 | 28,964 |
| Clusters | 1,299 | 535 | 753 |
| Adj. $R^2$ | 0.865 | 0.841 | 0.732 |

*Panel B: Pairwise equality tests*

| | | | | |
|---|---|---|---|---|
| Foreign − Domestic private | −0.222 | (0.193) | $z = -1.15$ | $p = 0.252$ |
| Public − Domestic private | 0.052 | (0.082) | $z = 0.63$ | $p = 0.527$ |
| Foreign − Public | −0.274 | (0.191) | $z = -1.44$ | $p = 0.151$ |

*Notes:* The table reports average treatment effects on the treated (ATT) estimated using the doubly-robust difference-in-differences estimator of Wooldridge (2025), separately by bank ownership group. The dependent variable is the natural logarithm of total deposits at the bank–municipality level. All specifications include bank–municipality and quarter fixed effects and pre-treatment municipal controls — average deposits, credit, NPL ratio, and branch count — each interacted with quarter fixed effects. Standard errors, reported in parentheses, are clustered at the bank–municipality level. Panel B reports pairwise $z$-tests of equality between ownership-specific ATTs, with standard errors computed as $\sqrt{\mathrm{SE}(\hat{\tau}_j)^2 + \mathrm{SE}(\hat{\tau}_k)^2}$ under the assumption of independence across subsamples. The pre-trend $p$-value reports the joint significance of pre-treatment event-time coefficients. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.4: Oil price shock and total credit by bank ownership

| | Total credit | | |
|---|---|---|---|
| | Domestic private | Foreign | Public |
| | (1) | (2) | (3) |
| Treat × Post | −0.186* | 0.833 | 0.137 |
| | (0.102) | (0.754) | (0.086) |
| Pre-trend test ($p$-value) | 0.451 | 0.835 | 0.299 |
| Bank–municipality FE | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes |
| Observations | 44,750 | 14,663 | 28,964 |
| Clusters | 1,308 | 539 | 753 |
| Adj. $R^2$ | 0.639 | 0.698 | 0.735 |
| *Panel B: Pairwise equality tests* | | | |
| Foreign − Domestic private | 1.019 | (0.762) | $z = 1.34$ |
| Public − Domestic private | 0.323** | (0.134) | $z = 2.41$ |
| Foreign − Public | 0.696 | (0.759) | $z = 0.92$ |

*Notes:* The table reports average treatment effects on the treated (ATT), estimated using the doubly-robust difference-in-differences estimator of Wooldridge (2025), separately by bank ownership group. The dependent variable is the natural logarithm of total credit at the bank–municipality level. All specifications include bank–municipality and quarter fixed effects and pre-treatment municipal controls — average deposits, credit, NPL ratio, and branch count — each interacted with quarter fixed effects. Standard errors, reported in parentheses, are clustered at the bank–municipality level. Panel B reports pairwise $z$-tests of equality between ownership-specific ATTs, with standard errors computed as $\sqrt{\mathrm{SE}(\hat{\tau}_j)^2 + \mathrm{SE}(\hat{\tau}_k)^2}$ under the assumption of independence across subsamples. The pre-trend $p$-value reports the joint significance of pre-treatment event-time coefficients. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.5: Oil price shock and local bank credit, by credit type and ownership

| | Bank ownership | | |
|---|---|---|---|
| | Domestic private | Foreign | Public |
| | (1) | (2) | (3) |
| *Panel A. Commercial credit* | | | |
| Treat × Post | −0.096 | −0.267 | −0.144 |
| | (0.119) | (0.220) | (0.108) |
| Pre-trend test ($p$-value) | 0.462 | 0.365 | 0.045 |
| Observations | 42,837 | 12,198 | 28,812 |
| Clusters | 1,264 | 493 | 750 |
| Adj. $R^2$ | 0.859 | 0.882 | 0.813 |
| *Panel B. Consumer credit* | | | |
| Treat × Post | −0.186*** | −0.526* | 0.253 |
| | (0.068) | (0.278) | (0.186) |
| Pre-trend test ($p$-value) | 0.990 | 0.058 | 0.011 |
| Observations | 39,657 | 10,736 | 28,909 |
| Clusters | 1,241 | 409 | 752 |
| Adj. $R^2$ | 0.803 | 0.842 | 0.651 |
| *Panel C. Microcredit* | | | |
| Treat × Post | −0.086 | −0.619*** | −0.030 |
| | (0.159) | (0.205) | (0.035) |
| Pre-trend test ($p$-value) | 0.074 | 0.465 | 0.029 |
| Observations | 30,034 | 6,130 | 28,949 |
| Clusters | 1,009 | 288 | 753 |
| Adj. $R^2$ | 0.802 | 0.904 | 0.882 |
| *Panel D. Mortgage credit* | | | |
| Treat × Post | −0.433 | −0.657** | 0.893 |
| | (0.387) | (0.287) | (0.788) |
| Pre-trend test ($p$-value) | 0.012 | 0.374 | 0.245 |
| Observations | 44,750 | 14,663 | 28,964 |
| Clusters | 1,308 | 539 | 753 |
| Adj. $R^2$ | 0.794 | 0.954 | 0.699 |

*Notes:* The table reports average treatment effects on the treated (ATT) estimated using the doubly-robust difference-in-differences estimator of Wooldridge (2025), separately by bank ownership group. The dependent variable in each panel is the natural logarithm of the indicated credit category at the bank–municipality level. Columns split the sample by bank ownership. All specifications include bank–municipality and quarter fixed effects. Standard errors, clustered at the bank–municipality level, are in parentheses. The pre-trend $p$-value reports the joint significance of pre-treatment event-time coefficients. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.6: Oil price shock and non-performing loan ratios by bank ownership

| | NPL ratio | | |
|---|---|---|---|
| | Domestic private | Foreign | Public |
| | (1) | (2) | (3) |
| Treat × Post | 0.007** | 0.021*** | −0.006 |
| | (0.003) | (0.005) | (0.005) |
| Pre-trend test ($p$-value) | 0.108 | 0.140 | 0.159 |
| Bank–municipality FE | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes |
| Observations | 44,421 | 14,506 | 28,952 |
| Clusters | 1,301 | 535 | 753 |
| Adj. $R^2$ | 0.326 | 0.319 | 0.556 |
| *Panel B: Pairwise equality tests* | | | |
| Foreign − Domestic private | 0.013** | (0.006) | $z = 2.33$ |
| Foreign − Public | 0.026*** | (0.007) | $z = 3.90$ |
| Domestic private − Public | 0.013** | (0.006) | $z = 2.16$ |

*Notes:* The table reports average treatment effects on the treated (ATT) estimated using the doubly-robust difference-in-differences estimator of Wooldridge (2025), separately by bank ownership group. The dependent variable is the total non-performing loan ratio, defined as the share of non-performing loans in total outstanding loans, winsorized at the 1st and 99th percentiles. All specifications include bank–municipality and quarter fixed effects and pre-treatment municipal controls — average deposits, credit, NPL ratio, and branch count — each interacted with quarter fixed effects. Standard errors, reported in parentheses, are clustered at the bank–municipality level. Panel B reports pairwise $z$-tests of equality between ownership-specific ATTs, with standard errors computed as $\sqrt{\mathrm{SE}(\hat{\tau}_j)^2 + \mathrm{SE}(\hat{\tau}_k)^2}$ under the assumption of independence across subsamples. The pre-trend $p$-value reports the joint significance of pre-treatment event-time coefficients. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.7: Robustness: alternative exposure measures

| | Log deposits | | | | NPL ratio | | | |
|---|---|---|---|---|---|---|---|---|
| | Below median (1) | Above median (2) | Top quartile (3) | Drop tercile 1 (4) | Below median (5) | Above median (6) | Top quartile (7) | Drop tercile 1 (8) |
| Treat × Post | −0.156** | −0.259*** | −0.239** | −0.189*** | 0.0021 | 0.0118*** | 0.0138** | 0.0060* |
| | (0.067) | (0.068) | (0.094) | (0.057) | (0.0038) | (0.0045) | (0.0066) | (0.0033) |
| Bank–municipality FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 100,700 | 99,315 | 96,888 | 104,375 | 95,385 | 93,831 | 91,596 | 98,746 |
| Clusters | 3,271 | 3,238 | 3,159 | 3,393 | 3,162 | 3,111 | 3,041 | 3,266 |

*Notes:* The table reports average treatment effects on the treated (ATT) estimated using the doubly-robust difference-in-differences estimator of Wooldridge (2025) under alternative definitions of oil exposure. Exposure is measured as the log of average municipal crude production over 2010–2012 (median = 14.08; 75th percentile = 15.30). Columns (1)–(2) and (5)–(6) split treated municipalities at the median of this distribution. Columns (3) and (7) restrict the treated group to the top quartile. Columns (4) and (8) drop the lowest-exposure tercile from the treated group. All specifications include bank–municipality and quarter fixed effects and pre-treatment municipal controls — average deposits, credit, NPL ratio, and branch count — each interacted with quarter fixed effects. Standard errors, reported in parentheses, are clustered at the bank–municipality level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.8: Robustness: alternative sample windows

| | Log deposits | | | | NPL ratio | | | |
|---|---|---|---|---|---|---|---|---|
| | Short pre (1) | Short post (2) | End 2017q4 (3) | Start 2012q1 (4) | Short pre (5) | Short post (6) | End 2017q4 (7) | Start 2012q1 (8) |
| Treat × Post | −0.195*** | −0.096** | −0.158*** | −0.197*** | 0.006** | 0.001 | 0.004 | 0.006** |
| | (0.051) | (0.043) | (0.047) | (0.051) | (0.003) | (0.003) | (0.003) | (0.003) |
| Pre-trend test ($p$-value) | 0.550 | 0.317 | 0.313 | 0.376 | 0.351 | 0.486 | 0.516 | 0.443 |
| Pre-quarters | 6 | 18 | 18 | 10 | 6 | 18 | 18 | 10 |
| Post-quarters | 22 | 9 | 14 | 22 | 22 | 9 | 14 | 22 |
| Bank–municipality FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 76,480 | 71,680 | 84,942 | 87,237 | 72,689 | 66,242 | 79,387 | 82,598 |
| Clusters | 3,319 | 3,336 | 3,362 | 3,340 | 3,176 | 3,148 | 3,180 | 3,212 |

*Notes:* The table reports average treatment effects on the treated (ATT) estimated using the doubly-robust difference-in-differences estimator of Wooldridge (2025) under alternative sample windows. The baseline sample spans 2010q1–2019q4 with the shock onset at 2014q3. Column (1) shortens the pre-treatment period to six quarters (starting 2013q1). Column (2) truncates the post-treatment period to eight quarters (ending 2016q3). Column (3) ends the sample in 2017q4, prior to the partial oil price recovery. Column (4) starts the sample in 2012q1, dropping the early post-financial-crisis quarters. All specifications include bank–municipality and quarter fixed effects and pre-treatment municipal controls — average deposits, credit, NPL ratio, and branch count — each interacted with quarter fixed effects. Standard errors, reported in parentheses, are clustered at the bank–municipality level. The pre-trend $p$-value reports the joint significance of pre-treatment event-time coefficients. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.9: Robustness: placebo treatment dates

| | True shock | Placebo shock dates | | | |
|---|---|---|---|---|---|
| | 2014q3 | 2011q3 | 2012q1 | 2012q3 | 2013q1 |
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A: Log deposits* | | | | | |
| Treat × Post | −0.200*** | 0.024 | 0.036 | −0.069 | 0.006 |
| | (0.051) | (0.063) | (0.063) | (0.062) | (0.042) |
| *Panel B: NPL ratio* | | | | | |
| Treat × Post | 0.006** | 0.002 | 0.000 | 0.000 | −0.000 |
| | (0.003) | (0.003) | (0.002) | (0.002) | (0.003) |
| Sample | Full | Pre-2014q3 | Pre-2014q3 | Pre-2014q3 | Pre-2014q3 |
| Pre-placebo quarters | 18 | 6 | 8 | 10 | 12 |
| Post-placebo quarters | 22 | 12 | 10 | 8 | 6 |
| Bank–municipality FE | Yes | Yes | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes |

*Notes:* Column (1) reproduces the baseline ATT from the full sample at the true shock date (2014q3). Columns (2)–(5) restrict the sample to the pre-treatment period and re-estimate the specification of Wooldridge (2025) assigning alternative shock dates. Each placebo divides the pre-period into pseudo pre- and post-treatment windows; the number of quarters in each window is reported in the table. All specifications include bank–municipality and quarter fixed effects and pre-treatment municipal controls — average deposits, credit, NPL ratio, and branch count — each interacted with quarter fixed effects. Standard errors, reported in parentheses, are clustered at the bank–municipality level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

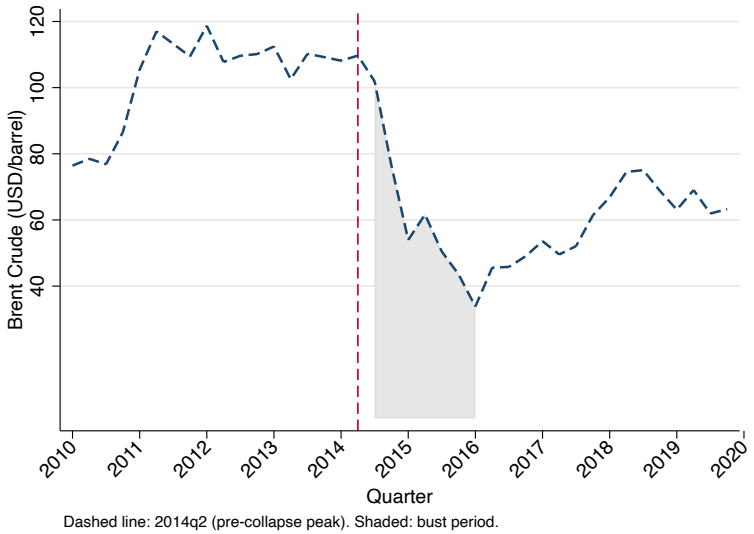
Table A.10: Robustness: alternative clustering

| | Log deposits | | | | NPL ratio | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treat × Post | −0.200*** | −0.200*** | −0.200*** | −0.200*** | 0.006** | 0.006** | 0.006 | 0.006 |
| | (0.051) | (0.045) | (0.047) | (0.044) | (0.003) | (0.003) | (0.005) | (0.004) |
| Bank–municipality FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster level | Bank–mun. | Mun. | Bank | Dept. | Bank–mun. | Mun. | Bank | Dept. |
| Clusters | 3,451 | 841 | 67 | 33 | 3,326 | 854 | 68 | 33 |
| Observations | 106,396 | 106,396 | 106,396 | 106,396 | 100,684 | 100,684 | 100,684 | 100,684 |

*Notes:* The table reports average treatment effects on the treated (ATT) estimated using the doubly-robust difference-in-differences estimator of Wooldridge (2025), varying only the level at which standard errors are clustered. The dependent variable is the natural logarithm of total deposits (columns 1–4) and the total non-performing loan ratio (columns 5–8). All specifications include bank–municipality and quarter fixed effects and pre-treatment municipal controls — average deposits, credit, NPL ratio, and branch count — each interacted with quarter fixed effects. Columns (1) and (5) cluster at the bank–municipality level (baseline); columns (2) and (6) at the municipality level; columns (3) and (7) at the bank level; columns (4) and (8) at the department level. Point estimates are identical across columns by construction. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

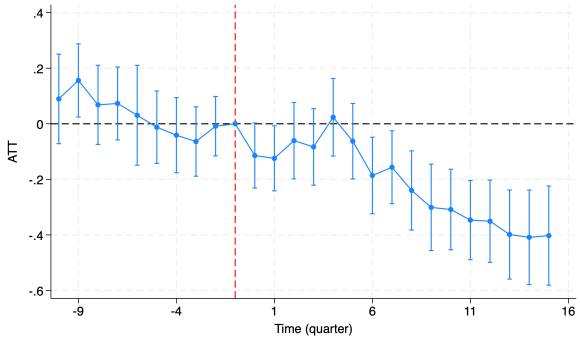
# B Figures

Figure A.1: Brent crude oil price, 2010–2019

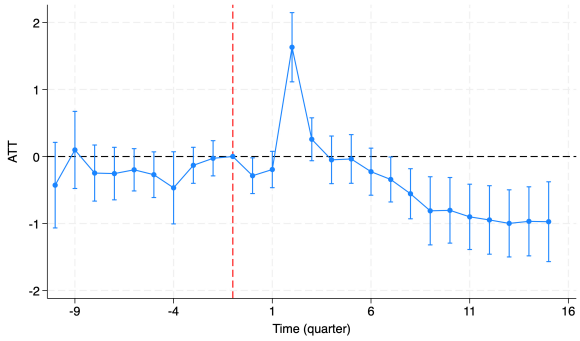



*Notes:* The figure plots the quarterly average Brent crude oil price in USD per barrel. The vertical line marks 2014q3, the onset of the price collapse. The shaded area covers 2014q3–2016q1, the period of sustained price decline.

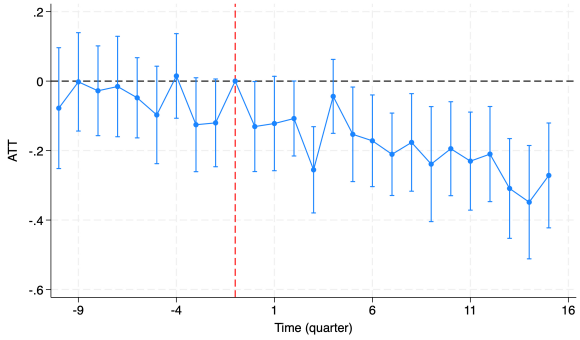
Figure A.2: Effect of the oil price shock on local bank deposits by ownership: event-study estimates




(a) Domestic private banks



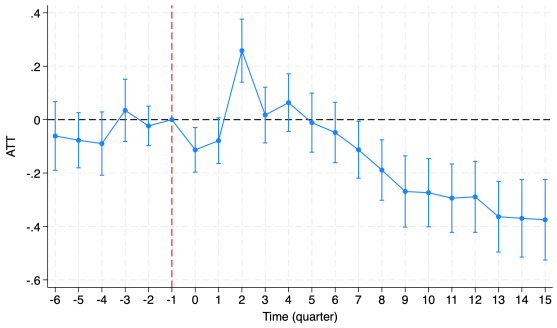

(b) Foreign banks



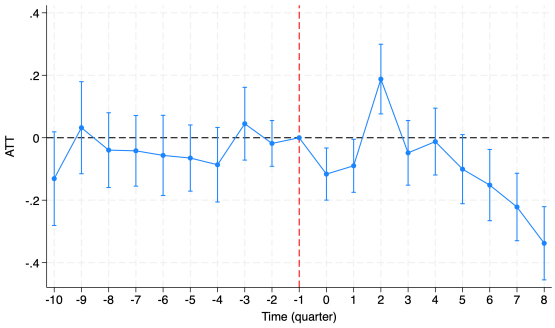

(c) Public bank

*Notes:* The figure reports event-study estimates of the effect of the 2014 oil price collapse on local bank deposits, separately by bank ownership group. Coefficients are obtained using the doubly-robust estimator of Wooldridge (2025). The dependent variable is the natural logarithm of total deposits at the bank–municipality level. Each coefficient corresponds to the ATT at quarter $e$ relative to the shock onset, normalized to zero at $e = -1$. The estimation window spans ten quarters before to fifteen quarters after the shock. All specifications include bank–municipality and quarter fixed effects and pre-treatment municipal controls — average deposits, credit, NPL ratio, and branch count — each interacted with quarter fixed effects. The vertical line marks the onset of the oil price collapse (2014q3). Error bars represent 95% confidence intervals based on standard errors clustered at the bank–municipality level. Aggregate ATT estimates are reported in Table A.3.

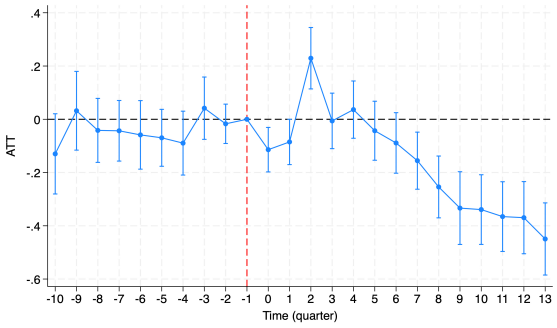
Figure A.3: Robustness: deposit event-study estimates under alternative sample windows



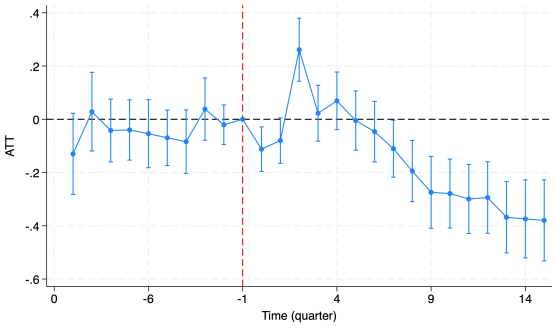

(a) Short pre-period (from 2013q1)




(b) Short post-period (to 2016q3)



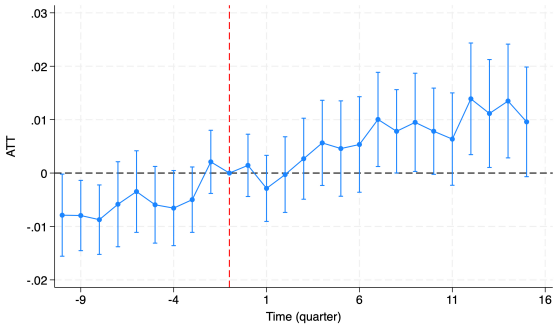

(c) Ending 2017q4



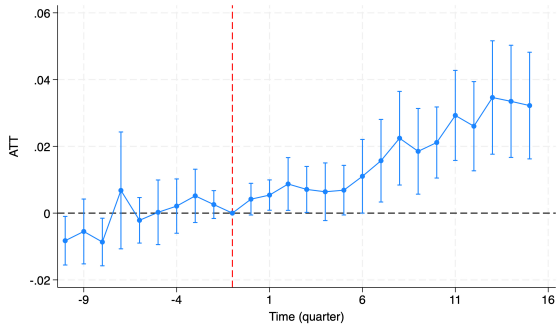

(d) Starting 2012q1

*Notes:* The figure reports event-study estimates of the effect of the 2014 oil price collapse on local bank deposits under alternative sample windows. Coefficients are obtained using the doubly-robust estimator of Wooldridge (2025). The dependent variable is the natural logarithm of total deposits at the bank–municipality level. Each coefficient corresponds to the ATT at quarter $e$ relative to the shock onset, normalized to zero at $e = -1$. All specifications include bank–municipality and quarter fixed effects and pre-treatment municipal controls — average deposits, credit, NPL ratio, and branch count — each interacted with quarter fixed effects. The vertical line marks the onset of the oil price collapse (2014q3). Error bars represent 95% confidence intervals based on standard errors clustered at the bank–municipality level. Panel (a) shortens the pre-treatment period to six quarters (starting 2013q1). Panel (b) truncates the post-treatment period to eight quarters (ending 2016q3). Panel (c) ends the sample in 2017q4, prior to the partial oil price recovery. Panel (d) starts the sample in 2012q1. ATT estimates are reported in Table A.8.

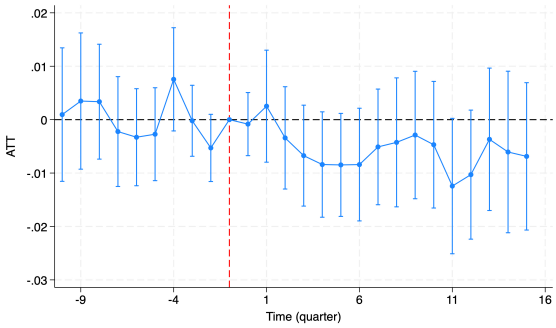
Figure A.4: Effect of the oil price shock on non-performing loan ratios by ownership: event-study estimates




(a) Domestic private banks



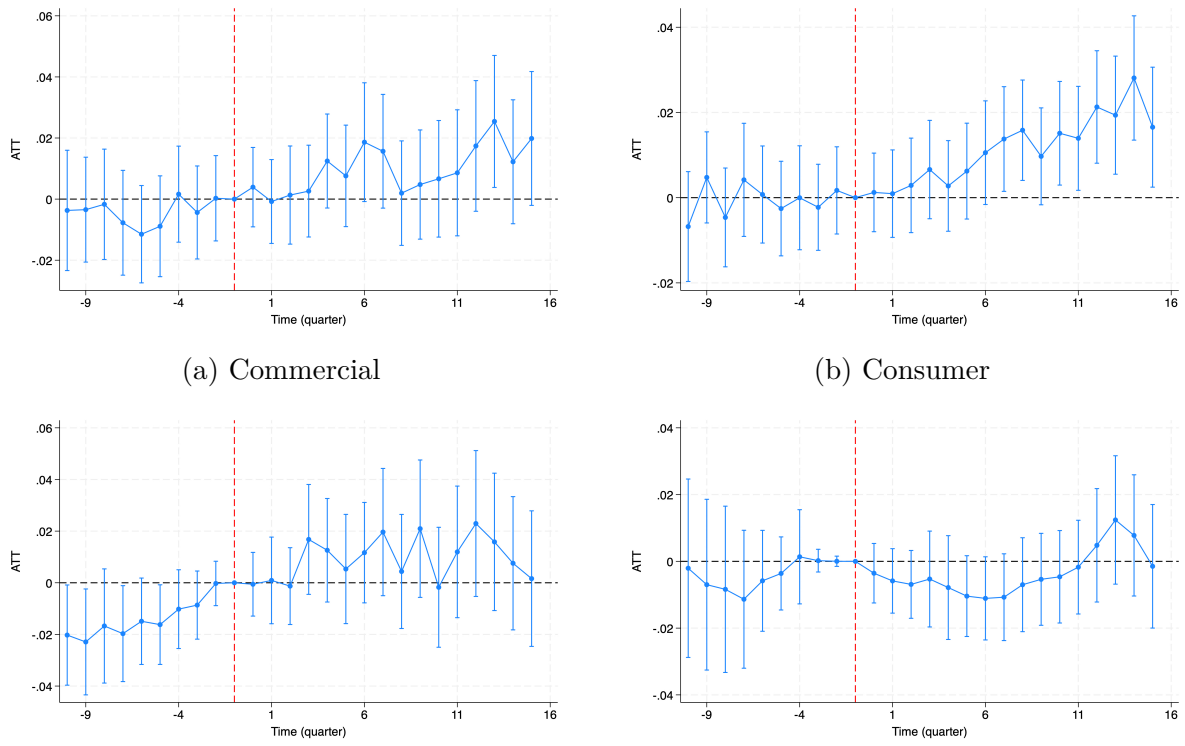

(b) Foreign banks




(c) Public bank

*Notes:* The figure reports event-study estimates of the effect of the 2014 oil price collapse on the total non-performing loan ratio, separately by bank ownership group. Coefficients are obtained using the doubly-robust estimator of Wooldridge (2025). The dependent variable is the ratio of non-performing loans to total outstanding loans at the bank–municipality level, winsorized at the 1st and 99th percentiles. Each coefficient corresponds to the ATT at quarter $e$ relative to the shock onset, normalized to zero at $e = -1$. The estimation window spans ten quarters before to fifteen quarters after the shock. All specifications include bank–municipality and quarter fixed effects and pre-treatment municipal controls — average deposits, credit, NPL ratio, and branch count — each interacted with quarter fixed effects. The vertical line marks the onset of the oil price collapse (2014q3). Error bars represent 95% confidence intervals based on standard errors clustered at the bank–municipality level. ATT estimates are reported in Table A.6.

Figure A.5: Effect of the oil price shock on non-performing loan ratios by credit category: event-study estimates




(a) Commercial

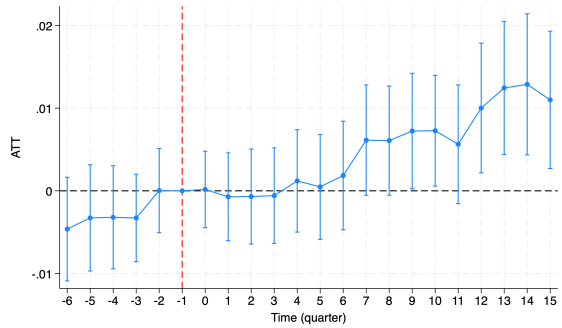
(b) Consumer

(c) Microcredit

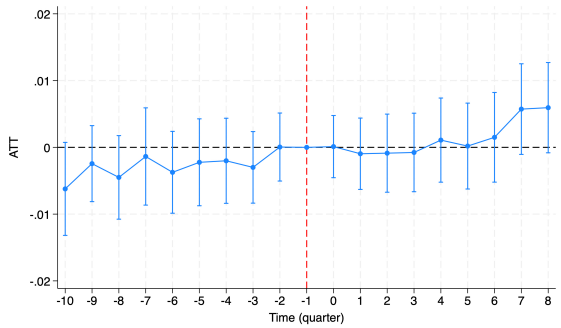
(d) Mortgage

*Notes:* The figure reports event-study estimates of the effect of the 2014 oil price collapse on non-performing loan ratios by credit category. Coefficients are obtained using the doubly-robust estimator of Wooldridge (2025). The dependent variable in each panel is the ratio of non-performing loans to total outstanding loans of the indicated category, winsorized at the 1st and 99th percentiles. Each coefficient corresponds to the ATT at quarter $e$ relative to the shock onset, normalized to zero at $e = -1$. The estimation window spans ten quarters before to fifteen quarters after the shock. All specifications include bank–municipality and quarter fixed effects and pre-treatment municipal controls — average deposits, credit, NPL ratio, and branch count — each interacted with quarter fixed effects. The vertical line marks the onset of the oil price collapse (2014q3). Error bars represent 95% confidence intervals based on standard errors clustered at the bank–municipality level. ATT estimates are reported in Table 3.

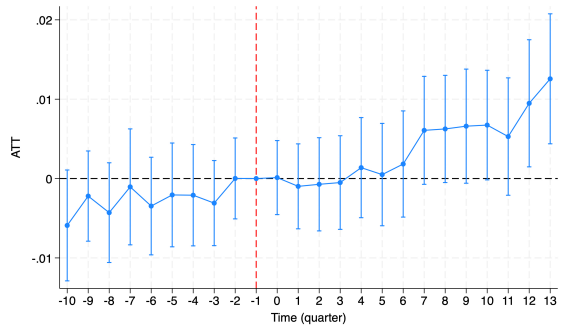
Figure A.6: Robustness: non-performing loan ratio event-study estimates under alternative sample windows



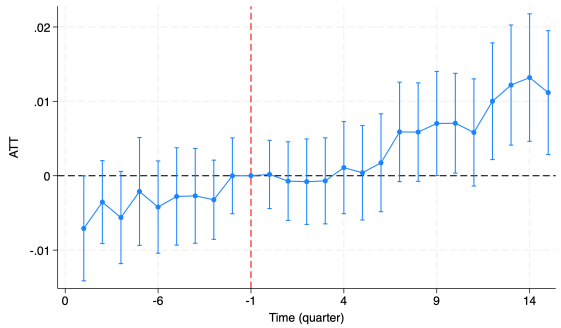

(a) Short pre-period (from 2013q1)




(b) Short post-period (to 2016q3)



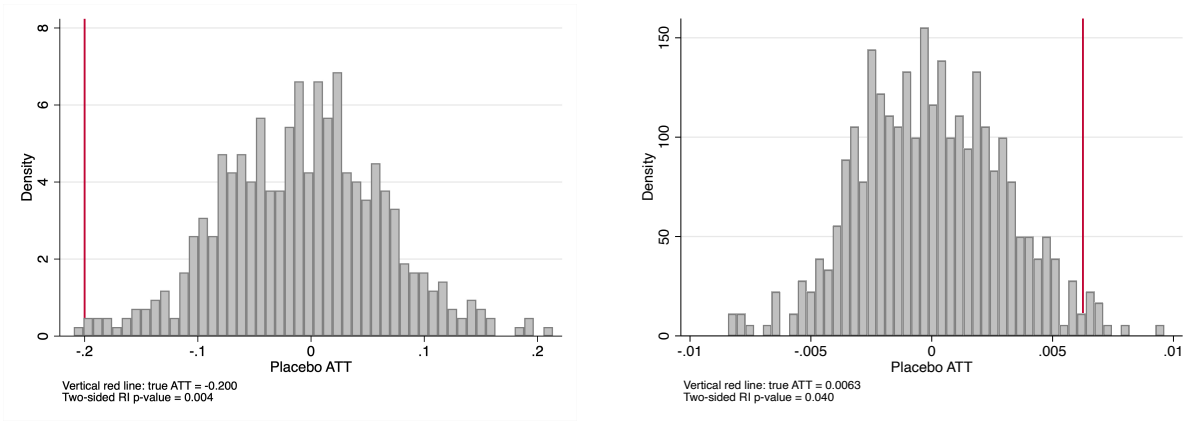

(c) Ending 2017q4




(d) Starting 2012q1

*Notes:* The figure reports event-study estimates of the effect of the 2014 oil price collapse on the total non-performing loan ratio under alternative sample windows. Coefficients are obtained using the doubly-robust estimator of Wooldridge (2025). The dependent variable is the ratio of non-performing loans to total outstanding loans at the bank–municipality level, winsorized at the 1st and 99th percentiles. Each coefficient corresponds to the ATT at quarter $e$ relative to the shock onset, normalized to zero at $e = -1$. All specifications include bank–municipality and quarter fixed effects and pre-treatment municipal controls — average deposits, credit, NPL ratio, and branch count — each interacted with quarter fixed effects. The vertical line marks the onset of the oil price collapse (2014q3). Error bars represent 95% confidence intervals based on standard errors clustered at the bank–municipality level. Panel (a) shortens the pre-treatment period to six quarters (starting 2013q1). Panel (b) truncates the post-treatment period to eight quarters (ending 2016q3). Panel (c) ends the sample in 2017q4, prior to the partial oil price recovery. Panel (d) starts the sample in 2012q1. ATT estimates are reported in Table A.8.

Figure A.7: Randomization inference: distribution of placebo ATTs




(a) Deposits

(b) NPL ratio

*Notes:* Each panel plots the distribution of placebo ATTs from 500 permutations obtained by randomly reassigning treatment status across municipalities, holding the treatment date (2014q3) and the number of treated units fixed, and re-estimating the baseline specification. The vertical line marks the true ATT. The two-sided randomization inference $p$-value is the share of placebo ATTs whose absolute value exceeds that of the true estimate.