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The Effects of Lending Quotas

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Abstract*

We study the effects of sectoral lending quotas in Bolivia, which required lenders to allocate a minimum share of their portfolios to priority sectors. Exploiting the timing of the reform and variation in pre-policy compliance at the lender and locality levels, we estimate impacts on credit markets and economic activity. To meet the quotas, lenders compressed their financial margins and expanded their branch networks. The resulting credit expansion outpaced deposit growth, raising loan-to-deposit ratios persistently, leaving lenders more exposed to liquidity shocks. At the local level, the credit expansion raised household income by 7.8%, driven largely by self-employment in non-priority sectors—the very sectors the policy did not target. Our findings illustrate how sectoral policies can generate important trade-offs and equilibrium consequences that extend well beyond their intended objectives.

JEL codes: G21, G28, O16, R11, D21, O25
Keywords: Lending quotas, Credit markets, Industrial policy

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I. Introduction

Industrial and sectoral policies are ubiquitous: virtually every government targets at least one industry for support, and low- and middle-income countries are among the most intensive users (Fernandes and Reed, 2026). A growing empirical literature evaluates the impacts of these policies (Juhász, Lane and Rodrik, 2023), but less is known about how they are implemented. One common implementation approach is to establish sectoral lending quotas—regulations requiring lenders to direct credit toward priority sectors or groups.[1] Complying with such mandates may lead intermediaries to adjust their portfolios and operations, and those adjustments can reach targeted and non-targeted sectors alike. Evaluating these policies therefore requires tracing effects on lenders, on targeted sectors, and on those left outside. Yet most empirical studies of lending quotas focus narrowly on direct effects, either on lenders or on targeted beneficiaries (Banerjee and Duflo, 2014; Agarwal et al., 2012; Kim, 2023). Without an economy-wide view, it is difficult to assess whether the sectors these policies target are the ones that benefit, or whether the incidence falls elsewhere.

This paper fills this gap by studying the impact of minimum lending quotas for priority sectors on lenders, overall economic activity, and spillovers across sectors.

Empirically assessing these questions is challenging for two reasons. First, governments do not select priority sectors randomly. Second, lending quotas affect not only borrowers but also non-borrowers and the financial institutions responsible for implementing them. Thus, analyzing the effects of lending quotas requires plausibly exogenous variation in exposure at both the lender and economy levels.

We leverage a 2014 reform in Bolivia that established lending quotas to be met by financial intermediaries over a four-year period. The stated goal of the policy was to redirect financing to priority sectors of the economy including manufac-

[1] Notable examples include the Community Reinvestment Act in the United States, the Priority Sector Lending program in India, targeted re-lending facilities in China, rural lending quotas in Brazil, and the Agri-Agra Credit Reform in the Philippines. More broadly, financial institutions that are beneficiaries of sovereign guarantees or thematic bonds are required to meet lending targets (Feldhütter, Halskov and Krebbers, 2024).

turing, construction, agriculture, among others, and excluding retail and services. The policy required lenders to meet targets for the share of lending allocated to priority sectors, which before the reform accounted for approximately one-third of total lending in Bolivia. Financial authorities assessed compliance based on the stated intended use of the loans. We combine the exogenous timing of the policy change with variation in pre-policy compliance at the lender level to examine adherence to the law and the costs associated with it. We then combine cross-municipality variation in the physical presence of more and less exposed financial institutions with the timing of the policy to estimate impacts on local economic activity and study the mechanisms behind those effects. We further distinguish between priority and non-priority sectors within localities to analyze spillovers.

First, we analyze compliance. Relative to the pre-period, more exposed lenders—those further from meeting the quota at baseline—increased lending to prioritized sectors by 69% and increased their share of prioritized lending by 15 percentage points relative to less exposed lenders. By December 2018, virtually all financial institutions were compliant. We also find that lenders who were more exposed were able to meet the targets without substantially reducing non-prioritized lending, perhaps to preserve valuable relationships with existing clients in the non-priority sector (Petersen and Rajan, 1994). Indeed, we observe a significant increase in total lending of 31%.

Second, we analyze how exposure to the policy affected lenders' financial margins and exposure to risk. We document a reduction in average interest rates of 96.4 basis points. This decline coincides with an average reduction in interest rate spreads of 141 basis points, suggesting that lenders lowered active interest rates to meet the targets while also increasing deposit rates to attract deposits. This decline in spreads persisted after the deadline to comply with the quotas passed. We find no impacts on delinquency rates but find that the growth in lending outpaced the growth in deposits in the longer term, leading to an increase in

lenders' exposure to demand-deposit shocks. Five years after the introduction of quotas, and a year after the compliance deadline, the loan-to-deposit ratio (LDR) increased by 12 percentage points. Taken together, the results reveal a trade-off between credit expansion and liquidity risks.

Third, we analyze the impacts of the quotas on local credit markets and economic activity. Our empirical strategy compares outcomes in localities with a stronger physical presence of exposed lenders to those with a weaker physical presence, before and after the policy change. Using administrative records, we find that total lending increased by 34% in more exposed municipalities, driven by growth in lending to the priority sector. We also find that this credit expansion coincided with an 18% increase in the number of branches of exposed financial institutions in more exposed municipalities, suggesting that lenders invested in new branches to expand their client base. This is consistent with evidence from Brazil showing that physical branches are important tools for improving borrower screening and expanding credit to microenterprises (Fang and Vlaicu, 2024).

Using survey data, we find substantial impacts on economic activity. Specifically, we document a 7.8% increase in total personal income due to the policy, corroborated by evidence from nighttime lights at the municipality level. The income gains are not explained by increases in employment or hours worked; instead, they appear to be driven by wage increases. These wage effects point to equilibrium responses to the lending quotas and complement existing evidence on the equilibrium effects of credit supply disruptions in other settings (Breza and Kinnan, 2021).

We also find that the impacts on income are largely driven by self-employment income rather than wages from salaried work. The impacts on wages for the self-employed—measured as income divided by hours worked—can therefore be interpreted as increases in labor productivity among the self-employed. We find suggestive evidence that the policy did not increase the probability of being self-employed but did increase small-business investment by 13%, suggesting that it

enabled incumbent microentrepreneurs to improve their existing businesses.

Fourth, we unpack the impacts on income by sector. We find neither significant nor substantial impacts on income from activities in priority sectors. Instead, the positive income effects are driven by activities in *non-priority* sectors, in which income increased by 9% because of the policy.

Fifth, we analyze the mechanisms behind the asymmetric impacts across sectors. On the supply side, lenders' branch expansion in more exposed localities appears to have increased lending to both priority and non-priority sectors. On the implementation side, compliance was monitored on the stated use of each loan rather than the sector of the borrowing firm, so credit access appears to have expanded for non-priority firms as well. Consistent with this, the rise in non-priority self-employment income suggests that businesses in that sector borrowed to finance high-return purchases from the priority sector. On the borrower side, the stated purpose of a loan need not have coincided with its actual use. We find that some borrowers appear to have used priority loans to pivot their businesses toward non-priority activities or to finance a spouse's business in the non-priority sector. We also rule out alternative mechanisms, including demand spillovers from increased consumption among priority-sector workers and savings effects from the expansion of financial services.

Our paper contributes to the empirical literature on sectoral and industrial policies in two ways. First, the financial system has been central to the implementation of many prominent sectoral policies, which typically combine lending with tax incentives, tariffs, and public investment (Goldberg et al., 2024; Juhász et al., 2025). Yet the adjustments this implementation requires of lenders are rarely measured alongside real effects of the policy. Indeed, prior studies on lending quotas focus either on banks' responses to these mandates (Agarwal et al., 2012; Cespedes et al., 2024) or on impacts on targeted sectors and beneficiaries (Banerjee and Duflo, 2014; Kim, 2023; Calomiris and Himmelberg, 1993), but not both. We contribute with evidence tracing how private lenders adjust to a credit

mandate and how those adjustments, together with the way the government monitors compliance, shape downstream outcomes. We find that the gains ultimately land disproportionately in the sector the policy did not target. This finding challenges a key premise of sectoral/industrial policies: that priority sectors are the natural locus of gains from directed-credit programs.

Second, the empirical literature on sectoral policy draws largely on historical episodes (Lane, 2025; Juhász, 2018) or on contemporary programs in advanced economies (Erten, Stiglitz and Verhoogen, 2026), among others. However, it is unclear whether these lessons can be applied to a contemporaneous developing-economy context. We provide evidence on a policy currently in force in a developing economy, where microenterprises and self-employment account for a large share of economic activity (Hsieh and Olken, 2014) and where the constraints on firm growth may differ substantially from those in the settings previously studied in this literature.

Finally, our analysis complements a large literature on the equilibrium effects of credit supply shocks (Kaboski and Townsend, 2012; Breza and Kinnan, 2021; Bazzi et al., 2023; Chodorow-Reich, 2013; Alfaro, García-Santana and Moral-Benito, 2021; Gutierrez, Jaume and Tobal, 2023; Cai and Szeidl, 2024). These studies typically take the nature of the shock as given, but policy reactions entail understanding the nature and source of the shock. Our analysis makes this link explicit by tracing the full chain from regulatory compliance to economy-wide outcomes. Like Kaboski and Townsend (2012) and Breza and Kinnan (2021), we find wage effects consistent with equilibrium responses to credit supply changes, but unlike in settings in which credit affects aggregate demand or employment (Gutierrez, Jaume and Tobal, 2023), the mechanism here operates through productivity-enhancing investment by incumbent microentrepreneurs.

II. Context and Data

A. Credit Markets in Bolivia

The Bolivian financial system is characterized by a diverse set of intermediaries that differ in size, specialization, and market share. Table 1 reports summary statistics on the Bolivian credit market using data for 2013. It shows that traditional commercial banks (*Banca Múltiple*) dominated the market, accounting for approximately 80% of total lending. Microfinance institutions (*Bancos PYME*), typically lending to micro, small, and medium enterprises (MSMEs), represented 7.5% of lending, while savings and credit associations (*Mutuales*), which specialize in mortgages, and savings and credit cooperatives (*Cooperativas*) accounted for 3.9% and 5.4%, respectively. This composition reflects a system in which large banks coexist with specialized institutions targeting niche segments such as microenterprises and housing finance.

Most borrowers are individuals rather than firms: In 2013, 99.1% of borrowers were individuals, representing 60.3% of total lending volume. Microcredit and MSME loans constituted the largest share of lending volume (51.7%). Corporate loans represented 16%, mortgages 19%, and consumer loans 12%. The predominance of microcredit and MSME lending reflects the central role of small businesses and self-employment in Bolivia's economy. These patterns in the credit market are largely reflected in labor markets. In 2013, before the policy was implemented, 43% of working adults were self-employed (according to household survey data), with average monthly earnings that account for 72% of the average earnings for salaried workers (see Appendix Table A1).

B. Policy Change: Lending Quotas in Bolivia

Bolivia's National Development Plan identified limited access to formal credit among small and community enterprises as a key obstacle to inclusive economic development, and established the explicit policy goal of redirecting financial sys-

tem resources toward the priority sector (Ministerio de Planificación del Desarrollo, 2007). The enabling legislation, Law 393, which regulates financial services and was passed in 2013, mandated that financial services fulfill the social function of contributing to development and poverty reduction, and authorized the executive to set minimum portfolio shares to prioritize lending to targeted sectors (Asamblea Legislativa Plurinacional de Bolivia, 2013).

Following the passage of the law on December 18, 2013, the government enacted a regulatory reform establishing mandatory lending quotas (Estado Plurinacional de Bolivia, 2013), which was implemented by executive order on July 10, 2014. The policy required financial institutions to allocate a minimum share of their portfolio to priority sectors and social housing, with quotas varying by type of lender: commercial banks at 60% (including at least 25% of loans directed to priority sectors), microfinance banks at 50%, and housing finance institutions at 50% for social housing. Cooperatives and other smaller intermediaries were exempt. A summary of the mandated lending quotas is reported in column 1 of Appendix Table A2.

The prioritized sectors were defined as "productive sectors" by the authorities and included activities such as manufacturing and agriculture (see Appendix Table A3 for a list of prioritized sectors). Loans outside this category—such as consumption credit and lending to non-prioritized sectors like retail and services—were classified as non-priority. For social housing and loans to prioritized sectors, the reform also introduced maximum interest rates, which varied based on loan size and the type of stated productive activities declared upon loan application.

Compliance with the quotas was required within five years for commercial banks, with the deadline being December 2018, and within four years for microfinance banks, with annual intermediate targets set by the Ministry of Economy and Finance.[2] After the deadline, financial institutions were mandated to maintain the quotas. Enforcement was delegated to the financial authority—the

[2]Newly created lenders had a three-year window to comply with the mandated quotas.

Financial System Supervisory Authority (ASFI), which could impose fines and administrative sanctions for non-compliance. For compliance purposes, the financial authorities considered the stated intended use of the loan as opposed to whether the borrower was a firm or a worker in a prioritized sector. For example, if a borrower obtained a loan to expand the current facility of their retail business (a non-priority sector), the loan would count toward compliance because expanding a facility is classified as construction (a priority sector). Thus, the policy directly expanded loans to priority sectors while stimulating demand for goods produced by those sectors.

Similar policies have been implemented in other developing countries. For example, India's Priority Sector Lending program mandates that banks allocate a fixed share of their credit portfolio to "priority sectors" such as agriculture, MSMEs, housing, and renewable energy. Brazil enforces mandatory rural credit quotas linked to demand deposits, requiring banks to allocate a portion of these funds to agriculture. Finally, China uses targeted re-lending facilities managed by the People's Bank of China to direct credit toward rural areas and micro or small businesses, relying on central bank instruments. In the United States, the Community Reinvestment Act tasks regulators with inducing lenders to meet the needs of low-income communities.

C. Data

Our analysis combines multiple data sources that allow us to measure total lending, local economic activity, and lender behavior over time.

First, we use administrative records on lending, aggregated at the municipality-lender level and reported quarterly from 2012 to 2018. These data provide detailed information on loan volumes distinguishing between prioritized and non-prioritized loans, average interest rates, number of loans to individuals and firms, and delinquent loans. This enables us to track quota compliance and other credit market outcomes. We use these data primarily to study compliance with the

lending quotas before the compliance deadline (2018). We also complement these data with data on the number of physical branches per lender and municipality for a similar period.

Second, we draw on household survey data covering the period 2006–16 collected by the national bureau of statistics (Instituto Nacional de Estadisticas, or INE). These surveys include information on income, employment, and demographic characteristics, which we use to assess the policy's effects on income and labor market outcomes. We build a pooled cross-section dataset for the period 2006–16 because that is the period for which municipality identifiers are publicly available, which are critical to our locality-level empirical design (see Section III.B). Our sample includes adults aged 18 to 65 years old to capture effects on individuals with stronger attachment to labor markets.

Third, we incorporate satellite-based nighttime light data at the municipality level from 2006 to 2021. However, we drop observations from 2020 and 2021 because of the COVID-19 pandemic. Specifically, we use the harmonized global nighttime light dataset constructed by Li et al. (2020*b*).[3] Nighttime light intensity, measured as the monthly mean satellite-observed lighting in each municipality weighted by the quality of monthly radiance (cloud-free observations), serves as a proxy for local economic activity and allows us to validate income-based measures using an alternative proxy for economic activity.

Finally, we compile publicly available information on balance sheets from financial institutions, reported annually to the financial authority between 2010 and 2019, and lender-level data on interest rate spreads, which are available for all lenders in the financial authority's annual reports since 2011. These records provide information on revenues, costs, and profitability, which we use to evaluate

[3]This dataset integrates observations from two satellite systems: the Defense Meteorological Satellite Program's Operational Linescan System (DMSP/OLS version 4) and the Visible Infrared Imaging Radiometer Suite (VIIRS version 1) aboard the Suomi National Polar-orbiting Partnership satellite. The harmonization process across the DMSP/OLS data, available from 1992 to 2013, and the VIIRS data, available from 2012 onward, is described in detail by Li et al. (2020*a*). They also show that the harmonized data produce a temporally consistent and spatially comparable time series of nighttime light intensity.

the policy's implications for lender performance. These data enable to analyze impacts on lender-level outcomes in the longer term, after the 2018 deadline to comply with the mandated quotas.

III. Empirical Strategy

We exploit Bolivia's 2014 reform as a quasi-natural experiment to identify the impacts of establishing lending quotas. Identification combines two sources of variation: (i) the unexpected timing of the law and (ii) cross-sectional heterogeneity in pre-policy compliance. By 2013, the share of priority lending (loans to priority sectors or social housing) was below the required quota for a large fraction of lenders (see Table A2). Financial institutions that were already compliant (62%) faced minimal incentives to adjust, while those further from the target were more likely to respond.[4]

A. Lender-Level Design

To analyze impacts on compliance, lending, and other lender-specific outcomes, we utilize a lender-level difference-in-differences design. Let i index lenders and t index years. Define *Distance to Quota*$_i$ as the gap between the mandated share (60% or 50%, depending on the type of lender; see Appendix Table A2) and the lender's pre-policy share of priority lending measured in the last quarter of 2013. For lenders exempt from quotas and for lenders who were already compliant, this measure equals 0. Specifically, we compute *Distance to Quota*$_i$ = (*Mandated Share*$_i$ − *Observed Share*$_i$) × $\mathbf{I}$[*Mandated Share*$_i$ > *Observed Share*$_i$]. Our baseline specification is:

$$Y_{i,t} = \alpha_i + \delta_t + \beta\big(\text{Post}_t \times \text{High Distance to Quota}_i\big) + \epsilon_{i,t} \tag{1}$$

[4] Appendix Table A4 shows correlations between pre-policy distance to the mandated quota and lender characteristics. More profitable lenders and larger lenders appear to be further away from meeting the targets before the policy.

To assess dynamics, we estimate:

$$Y_{i,t} = \alpha_i + \delta_t + \sum_{j \neq -1} \beta_j \big(\mathbf{I}[\tau_t = j] \times \text{High Distance to Quota}_i\big) + \epsilon_{i,t} \tag{2}$$

Here, $Y_{i,t}$ denotes the outcome for lender i in year t. The terms α_i and δ_t are lender and year fixed effects, respectively. $Post_t$ is an indicator equal to 1 for post-law periods ($t \geq 2014$) and 0 otherwise. *High Distance to Quota*$_i$ is an indicator that measures whether the difference between the mandated quota and lender i's share of priority lending in 2013Q4 (set equal to 0 for exempt lenders) is above the median. We focus on a discrete measure of the treatment to make interpretation of the coefficients easier, but we also report results using a continuous measure of treatment. $\mathbf{I}[\tau_t = j]$ is an indicator for event time j, normalized so that $j = -1$ corresponds to the last pre-policy period. Finally, $\epsilon_{i,t}$ is an error term. In both equations, the standard errors are clustered at the lender level.

In equation (1), the parameter of interest is β. It captures changes in outcomes before and after the reform for lenders with more distance to the quota relative to lenders with less distance to the quota. When we use a continuous measure of distance to the quotas, we multiply β with the average lender-level distance to quota to provide an average treatment effect interpretation.

This lender-level design relies on the assumption that absent the law, outcomes for lenders with different distances to the quota would have evolved similarly. We evaluate this assumption using event-study plots. We also report a number of robustness checks like excluding cooperatives and other financial institutions that were not required to comply with quotas, controlling for relevant covariates, and estimating the impacts using a synthetic difference-in-differences approach.

One concern arises because the government also established interest rate caps on priority loans. These caps, however, apply to all lenders, regardless of whether they comply with the quotas. Thus, one might think that the caps affected all lenders equally and that their potential impacts are accounted for by the time

fixed effects. However, to relax that assumption and control for their potential impacts across lenders, we report robustness checks controlling for pre-law average interest rates interacted with $Post_t$. The intuition is that lenders that charged lower interest rates before the policy were less likely to be affected by the caps, while those charging higher interest rates were more affected by them.

Another concern arises because other regulations imposed on financial institutions are correlated with exposure to lending quotas. However, the financial authorities in Bolivia impose traditional capital adequacy requirements. While some of these prudential regulations became more stringent in our analysis period, these changes are applied to the entire financial system without generating differential lender-level exposure based on the sectoral composition of a lender's portfolio.

B. Locality-Level Design

To analyze the impacts of the law on economic activity, we rely on cross-municipality variation in exposure to the law.

Our measure of local exposure relies on the idea that loan applications required in-person visits to branches.[5] Thus, a lender's ability to respond in the short term was constrained by its preexisting branch network. We compute municipality exposure as

$$\text{Exposure}_m = \sum_{i=1}^{N} \textit{branch share}_{i,m} \cdot \text{Distance to Quota}_i.$$

$Exposure_m$ measures the average non-compliance gap at baseline, weighted by the share of physical branches for each lender operating in municipality m. $Exposure_m$ is defined for only 145 out of the 337 municipalities in Bolivia (43%) with physical branches at baseline. However, based on the 2012 Population Cen-

[5]By 2014, no FinTech lenders operated in Bolivia.

sus, these municipalities captured 84% of the country's population. Appendix Figure A1 shows that there is substantial geographic variation in exposure to the policy change.

Appendix Table A5 shows that this measure of exposure is uncorrelated with municipality size, average pre-policy interest rates, overall employment, and its composition. Importantly, treatment exposure is not correlated with the percentage of workers in the priority sector. In contrast, exposure appears correlated with total lending and whether a municipality is one of the three largest regional capitals in the country.[6] We offer robustness checks to ensure that the results are not driven by these three municipalities.

One concern about our locality-level measure of exposure is that it does not account for potential cross-municipality flows of credit. While we cannot directly rule out this concern, we argue that it may not be empirically relevant. Cross-municipality credit flows are more likely to be directed to municipalities without physical access to lenders, as borrowers in these municipalities may have no choice but to apply for loans somewhere else. By construction, our measure of exposure excludes municipalities without physical branches—those that may benefit from additional loans even if they are not exposed to the policy. For a robustness check, we also report results including observations from municipalities without branches at baseline, for which *Exposure* is coded as 0.

Our main specification is:

$$Y_{m,t} = \alpha_m + \delta_t + \gamma\big(\text{Post}_t \times \text{High Exposure}_m\big) + \epsilon_{m,t} \tag{3}$$

The event-study version is:

$$Y_{m,t} = \alpha_m + \delta_t + \sum_{j \neq -1} \gamma_j \big(\mathbf{I}[\tau_t = j] \times \text{High Exposure}_m\big) + \epsilon_{m,t} \tag{4}$$

[6]These are La Paz, Cochabamba, and Santa Cruz de la Sierra.

Here, $Y_{m,t}$ denotes the outcome for municipality m in year t, and α_m and δ_t are municipality and year fixed effects, respectively. $Post_t$ is an indicator equal to 1 for post-law periods. $\mathbf{I}[\tau_t = j]$ is an indicator for event time j, normalized so that $j = -1$ corresponds to the last pre-policy period. Finally, $\epsilon_{m,t}$ is an error term. In both equations, standard errors are clustered at the municipality level.

Like in our lender-level design, we focus on a discrete version of exposure (whether the municipality's exposure is above the median). We also report results using a continuous exposure measure. In this case, we multiply γ with the average locality-level exposure to provide an average treatment effect interpretation.

This design assumes that, conditional on fixed effects, municipalities with different exposures would have followed parallel trends absent the law. We test this using pre-policy dynamics. Like in our lender-level design, a central concern is the presence of interest rate caps applied to priority loans. To ensure that differential exposure to interest rate caps across municipalities is not driving our results, we report results after controlling for the average municipality-level interest rates before the policy change interacted with the $Post_t$ indicator. In addition, we report results from a battery of robustness checks including the exclusion of the three largest urban centers (by population) in the country and the inclusion of municipalities without branches at baseline. for which *Exposure* is coded as 0. In the case of survey data, our main specification includes sampling weights, but we also report results without survey weights. In addition we report robustness after winsorizing continuous variables with respect to the 95th percentile.

One concern about our approach is that locality-level exposure to the policy change may be correlated with the relative size of the prioritized sector. If so, underlying sectoral trends could confound our results. Reassuringly, Appendix Figure A2 shows that locality-level exposure to the policy change is not correlated with the relative size of the priority sector at baseline, measured using data from 2012's population census.[7]

[7]This concern also justifies our choice of relying on locality-level exposure as opposed to sectoral

IV. Effects on Compliance and Total Lending

A. Compliance

Figure 1 documents suggestive evidence of strong compliance. It plots the evolution of the share of prioritized loans for three groups of lenders: those that were noncompliant before the policy, those already compliant, and those exempt from quotas. Prior to 2014, the share of prioritized lending was relatively stable across groups. Following the introduction of quotas, there is a clear trend break for noncompliant lenders, whose share of prioritized loans rose sharply. By the fourth quarter of 2017, the gap between noncompliant and compliant lenders had largely closed, indicating convergence toward the mandated targets.

Appendix Figure A3 shows changes in the composition of lending within prioritized and non-prioritized sectors. Among prioritized sectors, the largest share gains occurred in construction, manufacturing, and agriculture. In contrast, within non-prioritized sectors, the share of loans for wholesale and retail commerce declined modestly, suggesting that portfolio adjustments were concentrated in prioritized activities.

Next, we turn to estimates from our lender-level difference-in-differences design. Panel A in Table 2 reports treatment effects for key outcomes, following the specification in equation (1). Column 1 examines impacts on compliance, measured by the share of total lending volume allocated to prioritized loans. The estimated effect is positive (13 percentage points) and implies a 30% increase with respect to the baseline mean (0.43). We find similar results using our continuous measure of baseline distance to the quotas in Panel B. Figure 1b plots impacts over time and shows that the impacts on compliance grow. The figure also shows that there were neither substantial nor statistically significant pre-trends, providing empirical support to the parallel trends assumption.

exposure in our analysis, as governments may direct resources to priority sectors if they foresee growth opportunities or foresee the need to prevent a slowdown of such sectors. This concern is common in the context of the analysis of industrial and sectoral policies (Juhász, Lane and Rodrik, 2023).

B. Effects on Lending

We next analyze impacts on lending volumes. Lenders may adjust by expanding total priority lending (increasing the numerator of the target outcome) or by reducing non-priority loans (reducing the denominator of the target outcome). Columns 2 to 4 in Panel A of Table 2 indicate that lending to prioritized sectors increased significantly by 69%, while lending to non-prioritized sectors did not decline (column 3). As a result, total lending expanded by 31% for more exposed lenders relative to less exposed ones (column 4). We find similar results when using our continuous exposure measure in Panel B. Appendix Figure A4 corroborates these findings and shows that as priority lending increased sharply and non-priority lending moderately declined, total lending grew substantially.

These patterns suggest that compliance was achieved primarily through portfolio expansion rather than crowding out of non-priority loans. This result is consistent with canonical models of relationship lending (Petersen and Rajan, 1994): Lenders may not want to let their older clients go, because they invested substantial resources in building a relationship with them. Appendix Table A6 reports results on additional sources of loan financing. We do not find reductions in cash on hand (column 2) or increases in interbank borrowing (column 3); thus lenders did not finance additional loans by reducing their liquidity or borrowing from other banks (including the central bank). Instead, they appear to have made efforts to increase deposits (see column 1).

Together, the results point to strong compliance without credit rationing. Between 2013 and 2019, total lending more than doubled from $83.7 billion to $183.7 billion (a 119% increase).[8] They also suggest that affected lenders were induced to collect more deposits from the public to finance additional lending. These adjustments may have carried additional costs, which we study below.

[8]Calculations based on the Financial System Supervisory Authority's data from `https://www.asfi.gob.bo/sites/default/files/2026-06/PRINCIPALES%20VARIABLES%20MAYO%202026.pdf`, consulted on July 14, 2026.

Effects on financial margins and risk. — In this section, we analyze the extent to which the strategies that lenders followed to comply with the quotas generated unintended consequences for their financial margins and risk.

We first analyze impacts on financial margins. Our previous results suggest that to comply with the regulation, lenders increased their deposits from the public. Thus, they may have had to incur additional intermediation costs to attract deposits. We can test for this directly by focusing on interest rates (using quarterly data from administrative records) and interest rate spreads (based on annual reports published by the regulator). Consistent with the observed dynamics about compliance, Figure 2a shows a gradual decline in average interest rates,[9] which coincides with a decline in average interest rate spreads (Figure 2b). The decline in spreads persisted even after the deadline to comply with the lending quotas (period 5 onward). Column 2 in Panel A of Table 3 reports a decline in average interest rates of 96 basis points. Column 3 shows a decline in the interest rate spread of 141 basis points—a 16% decline relative to the pre-period mean. That the point estimate in column 3 is somewhat larger than that in column 2 suggests that financial institutions reduced interest rates on loans to comply with the quota while increasing interest rates on deposits to finance additional lending. Column 4 shows no impacts on overall profits (net operative results over assets).

We next analyze the impacts of the policy in two dimensions of risk: repayment and liquidity. Column 1 in Panel A of Table 3 shows that there is no evidence of a significant change in delinquency rates, defined as the share of past-due loans (arrears of 30 days or more), indicating that credit quality remained stable despite rapid portfolio adjustments.

We find a much different pattern when we analyze liquidity risk. While lenders increased total deposits (see Appendix Table A6), in the longer term, the increase in deposits was not enough to offset the increase in lending. Column 5 in Table 3

[9]Specifically we compute the average interest rates by lender across different loan types weighted by share of total lending corresponding to each type of loan.

shows no average impacts on the LDR, though the point estimates suggest a 10 percentage point increase. Panel C in Figure 2 shows that lenders' liquidity risk increased over time. Five years after the start of the lending quotas, more exposed banks experienced a statistically significant increase in their LDRs (12 percentage points).[10] The results suggest that the policy may have placed lenders in a vulnerable liquidity position in the longer term.

Discussion. — The results suggests an empirical trade-off for lenders: Expanding credit to prioritized sectors required a reduction of markups and an increase in exposure to liquidity shocks in the longer term. To attract more deposits (see column 1 in Appendix Table A6), lenders compressed their financial margins and took on higher liquidity risk. Over time, the smaller spreads and higher risk may have induced lenders to shift the structure of their lending portfolio just enough to comply with the quotas. Indeed, the reduction of spreads and liquidity is consistent with descriptive longer-term aggregate patterns in the financial market. Figure A6 plots the share of total lending to priority sectors for commercial banks. It shows that the initial sustained growth in the share of loans to priority sectors stagnated right at the mandated levels after meeting the quotas. In contrast, the LDR for the banking system went from 0.78 in 2013 to 1.02 in 2019, putting lenders in a vulnerable position.[11]

C. Robustness

Our results are robust to several alternative specifications. Our main specification includes cooperatives and other financial institutions that were not subject to quotas, whose distance to quota thus equals 0. Panel A in Appendix Tables A7 and A8 shows that the results do not substantially change when excluding these

[10] Column 6 in Table A6 shows decreases in the deposit-to-loan ratio, and Appendix Figure A5 shows the corresponding event-study estimate. In both cases, the impacts are aligned with our results using the LDRs. However, these are estimated with more precision. Overall, both sets of estimates suggests that liquidity risk increased over time because of the policy.

[11] An increase in the LDR signals greater dependence on more volatile funding sources and heightened exposure to liquidity shocks (Fund, 2006).

lenders from the analysis. One concern is that because some financial institutions entered or exited the market during our analysis period, our results may reflect selection as opposed to portfolio adjustments. Panel B shows, however, that our results are robust to only focusing on a balanced panel.

A central concern arises because, as part of the reform, the financial authority introduced interest rate caps on priority loans. Even though the caps did not depend on quota compliance, in Panel C, we report results after controlling for the average interest rate (at the lender level) during 2013 interacted with the $Post_t$ indicator. The intuition is that lenders that offer loans at higher interest rates are more likely to be affected by the caps. The results are robust to including this control.

Finally, to assuage concerns about potential violations of the parallel trends assumption, Panel D reports results from a synthetic difference-in-differences model that gives higher weights to lenders with similar trends during the pre-period and also controls for relevant covariates (Arkhangelsky et al., 2021).[12]

V. Effects on Local Financial Markets and Economic Activity

We next examine whether the reform translated into measurable changes in economic activity.

A. Effects on Local Financial Markets

Using our locality-level design, we first replicate the main results on lending. Appendix Table A9 shows that municipalities with above-median physical presence of noncompliant lenders at baseline experienced significant increases in the share of prioritized lending, increases in overall lending after the policy change, and

[12]The controls are selected based on a Lasso model for each outcome. The initial set of controls includes the total number of branches, total assets, deposits over short-term liabilities, coefficient of leverage, capital adequacy ratio, returns over assets, revenues/assets, administrative expenses/assets, share of loans with 30 days or more of past-due payments, average interest rates, and total net lending (logs).

declines in interest rates, though this last result is not estimated with precision.[13] The point estimates in columns 2 and 4 suggest an increase in priority and total credit of 35% and 34%, respectively. Both point estimates and dynamic patterns are consistent with those observed in the lender-level analysis (see Appendix Figure A7), confirming that compliance translated into local credit expansion. In addition, Appendix Figure A7 shows no evidence of differential pre-trends between municipalities with different levels of exposure.

We do not observe interest rate spreads at the locality level, but we can proxy for intermediation costs by analyzing impacts on the locality-level supply of financial services. Figure 3 shows that the number of branches per 1,000 adults in a municipality increased significantly in more exposed municipalities after the reform, indicating that lenders expanded their physical presence to meet quotas.[14] Appendix Table A10 reports point estimates following equation (3), by type of branch. It shows that the overall impact on total branches is driven by permanent branches as opposed to pop-ups. Column 3 suggests that the reform led to an average increase of 0.05 physical branches per 1,000 adults, which represents 17% of the pre-period mean. This adjustment underscores the importance of physical proximity for micro- and small-firm lending (Fang and Vlaicu, 2024; Fonseca and Matray, 2024) and suggests that at the local level, the policy induced a broad expansion of financial services.

B. Effects on Local Economic Activity

We next turn to household survey data covering the period 2006–17 and focus on adult respondents. Figure 4a presents an event-study plot showing that average total income rises following the implementation of the reform in more exposed municipalities relative to less exposed ones, with no evidence of pre-trends.

[13] Unlike in the case of our lender-level design, we do not report impacts on deposits and spreads, as these variables are measured using balance sheet data, which is only available at the lender level.

[14] These include permanent and pop-up agencies, branches, offices, financial and nonfinancial correspondent service points, and other customer service offices.

Panel A of Table 4 reports estimates using the municipality-level difference-in-differences specification in equation 3. Column 1 reports effects on income: The point estimate is positive and significant. It also suggests an average increase of $176 billion, or a 7.8 % increase relative to the pre-period mean.

Columns 2 and 3 suggest that the income gains are not explained by changes in employment or hours worked, which show no statistically significant change. Instead, they are driven by increases in hourly wages (i.e., personal income divided by total hours worked per month). Figure 4b illustrates that the increase in income coincides with an increase in wages using an event-study specification. Column 4 in Panel A shows a statistically significant coefficient, which implies a 7% increase relative to the pre-period.

We validate these findings using an alternative proxy for economic activity: satellite-based nighttime light data. Column 5 of Panel A in Table 4 shows that nighttime light intensity increased significantly in more exposed municipalities after the reform. The consistency between survey-based measures and nighttime light measures reinforces the conclusion that the policy stimulated overall local economic activity. Panel B shows that across outcomes, we obtain similar results if we use a continuous measure of exposure to treatment.

Putting the results together, the large increase in local credit supply induced by the quotas appears to deliver important general equilibrium effects, which manifest in increases in wages. These results are consistent with evidence from India and Thailand showing that changes in local credit supply can affect wages (Breza and Kinnan, 2021; Kaboski and Townsend, 2012). However, unlike in India and Thailand, the changes in the supply of credit were not a consequence of restrictions affecting an entire industry or of inflows of public funds to develop local credit markets. They were the outcome of sectoral lending quotas applied to a well-established financial system.

C. *Unpacking the Equilibrium Effects of Sectoral Lending Quotas*

We next explore the mechanisms behind the observed income and wage effects. We explore two hypotheses. One hypothesis is that firms obtaining loans increased their productivity and that this increase is reflected in higher labor demand and wages—the aggregate demand channel. Another hypothesis is that self-employed workers used the additional credit to improve their businesses, thus increasing their labor productivity—the business-investment channel.

We test these hypotheses by analyzing whether the main impacts on income are explained by increases on income from salaried work as employees or from self-employment income. Column 1 in Panel C of Table 4 shows evidence of significant income gains from salaried work.[15] Impacts on wages for salaried workers are positive, though imprecisely estimated (see column 2 in Panel C). These impacts are lower than the effects on income and wages from self-employment (see columns 3 and 4),[16] though the difference is not statistically significant. The impacts on income from salaried work account for 6% of the baseline mean, while the impacts on income from self-employment account for 10% of the baseline mean. Panel D shows that these patterns are accentuated when we use the continuous measure of exposure.[17] Overall, the impacts on income for salaried workers account for 45% of the total impact on income (column 1 in Panel A of Table 4), while the impacts on income from self-employment account for 51%.[18]

We provide supporting evidence for the business-investment channel in Table 5, which shows significant increases in spending in self-employed activities (column 2) and increases in gross revenues (column 1).[19] Columns 3 to 6 report impacts by type of business expenditure and show that most of the impacts

[15] This includes paid workers (workers, employees, workers in cooperatives, and household workers) and paid business owners.

[16] This includes self-employed workers and unpaid business owners.

[17] Column 5 shows that the increased in income from self-employment is not driven by individuals switching from salaried work to self-employment.

[18] The magnitudes do not add up to 100 because total income also includes income from apprenticeships and from non-labor sources.

[19] We include revenues and expenses corresponding to businesses that constitute a household's primary or secondary activity.

on total costs are driven by impacts on inputs (e.g., raw materials, supplies, or merchandise), which account for 77% of the observed impact on total business spending, using the discrete measure of exposure in Panel A. There do not seem to be substantial impacts on wages, rent and utilities, and taxes.[20] That the increase in the supply of credit increased investments in inputs and revenues among microenterprises is consistent with evidence from microcredit programs (Banerjee et al., 2019*a*,*b*) and evidence on how local credit supply shocks can generate equilibrium effects through business investments (Breza and Kinnan, 2021).

Put together, the results suggest that while there is evidence on both the aggregate demand channel and the business-investment channel, the observed increases in wages (e.g., column 4 of Panel A in Table 4) reflect improvements in labor productivity——higher profits per hour worked——more than changes in labor demand. The results are consistent with the prominent role of self-employment in developing economies (Feng, Lagakos and Rauch, 2023). In our setting, 43% of workers were self-employed before the policy. The results are also consistent with the structure of the credit market, in which the vast majority of loans are granted to individuals rather than firms and in which microcredit and loans to micro, small and medium enterprises accounted for 51.7% of total lending volume and 64% of total loans (see Table 1).

Robustness. — Appendix Tables A11-A16 report results from alternative specifications, using our locality-level design. For outcomes computed using survey data, Panel B in Appendix Tables A12-A15 shows that our results are robust to not using sampling weights while Panel E in the same tables shows that our results are robust to winsorizing continuous variables with respect to the 95th percentile.[21]

[20]The impact on taxes is statistically significant in our main specification but only represents a 16% increase in a category that accounts for roughly 1% of total business spending.

[21]In Panel E of Appendix Table A15, there is no variation in expenses in rent and utilities, wages, and taxes after winsorizing the continuous variables with respect to the 95th percentile. That we observed impacts on rent and utilities in our main specification is consistent with the idea that the credit expansion

Panel C in Appendix Tables A11-A15 shows that our results are robust to controlling for average pre-program interest rates (measured at the municipality level), suggesting that the impacts that we find are not driven by the caps on interest rates. Finally, our measure of locality-level exposure excludes municipalities without branches at baseline. Panel D in Appendix Tables A11-A15 shows that the effects are similar if we include individuals living in municipalities without branches at baseline by coding $Exposure = 0$ in such cases.

Finally, in most cases, our results are robust to excluding the largest three municipalities (see Panel A in Appendix Tables A11- A15), but impacts on income and wages are somewhat diluted in this specification (see Panel A in Appendix Table A12). That the impacts using nighttime light data are still significant gives us confidence in our results (see column 5 in Appendix Table A12). Moreover, when we replicate the analysis using our continuous measure of exposure (see Appendix Table A14), we still find impacts that are remarkably similar to those presented in Panels B and D of Table 4.

VI. Effects by Sector

The motivation for establishing lending quotas was to stimulate the priority sector. Thus, a natural question is whether the lending quotas were able to improve the economic performance of the priority sector relative to the non-priority sector. This is not empirically obvious. The priority sector could benefit from increased access to credit. However, there are different mechanisms that can generate substantial impacts on the non-priority sector. First, although the motivation for the quotas was primarily to benefit the priority sector, the enforcement of the quotas was based on the stated use of the loans. This allowed businesses in the non-priority sector to finance productivity-enhancing projects involving goods and services produced by the priority sector. Second, loans whose intended use was

due to the reform was used to expand the operations of these small businesses. It is also consistent with a general pattern in the literature on credit and microenterprises: that the returns to credit tend to be larger among ex ante more profitable firms (Meager, 2022).

to finance activities in or purchases from the priority sector could have been, in part, diverted to non-priority activities. Third, as lenders expanded their physical presence, they may also have expanded the financial possibilities for businesses in the non-priority sector.

Our setting enables us to shed light on this question by analyzing impacts on income related to activities in the priority and non-priority sectors.[22] Columns 2 to 4 of Table 6 report treatment effects on income from activities in priority and non-priority sectors. We find no evidence of substantial or significant impacts on income in the priority sector. For example, in column 2 of Panel A, the point estimate of the impact on total income from activities in the priority sector ($36.09 billion) accounts for just 20% of the impact of the quotas on total income ($175.85 billion; see column 1 of Panel A of Table 4).

In contrast, we find evidence of substantial effects on income from activities in *non-priority* sectors. Column 6 of Panel A of Table 6 shows that the increase in total income following the reform is driven primarily by activities in non-priority sectors. The impacts on income from activities in the non-priority sector are statistically significant, are 3.6 times larger than those found for the priority sector (column 2), and represent 9% of the baseline mean and 77% of the overall impact on income (column 1 of Panel A of Table 4).[23] Appendix Table A19 shows that our results are robust to a battery of alternative specifications discussed in Section V.C. These results are also consistent with macroeconomic sectoral trends. Appendix Figure A8 plots the trajectories of the priority and non-priority sectors over time, using country-level sectoral data obtained from Bolivia's National In-

[22]Recall that priority loans include loans whose stated purpose is to finance activities related to priority sectors, but also include loans for social housing. However, social housing is expected to increase activity mostly in the construction and manufacturing sectors, which are already included in the definition of the priority sector (see Appendix Table A3). Likewise, non-priority loans include consumption loans, which are expected to affect primarily activities in the retail and service sectors, which are included in the government's definition of non-priority sectors.

[23]Consistent with our previous results on self-employment, column 7 of Table 6 shows that income from self-employment activities in non-priority sectors increased significantly, while column 2 suggests this was not the case for activities in the priority sector. Columns 4 and 8 show less contrast when we focus on income from salaried work, particularly when using our continuous measure of exposure (see Panel B).

stitute of Statistics. A clear pattern emerges: The priority sector grows slower than the non-priority sector following the establishment of the quotas.

The goal of establishing lending quotas was to ease credit constraints and stimulate the growth of the priority sector, but the results suggest that the intended beneficiaries may not have been particularly credit constrained. The insights of Banerjee and Duflo (2014), who analyze the Priority Sector Lending program in India, are useful in clarifying this point: They postulate that credit-constrained firms will use additional credit to expand production, while unconstrained firms may either not borrow or use additional credit to substitute away from more expensive forms of financing. By this logic, our results suggest that firms in the priority sector were not particularly constrained. As a result, the main beneficiaries of the quotas appear to have been businesses in the non-priority sector.

This asymmetric impacts by sector provide a novel result in the empirical literature on sectoral policies. A policy that targeted one specific sector ends up generating growth for the non-targeted sector. We argue that this result is a product of implementation issues and the reactions of financial intermediaries to lending quotas.We study several potential mechanisms below.

A. Unpacking the Sectoral Effects of Lending Quotas

In this section we discuss how lender's adjustments to lending quotas affect downstream sectoral outcomes. We focus mainly on two implementation issues: supply side responses and enforcement.

Supply-side responses. — The quotas enabled businesses in the non-priority sector to finance high-return purchases from the priority sector. This direct effect is consistent with our finding that impacts for the non-priority sector appear to have been largely driven by self-employment activities is consistent with this interpretation (see column 7 of Table 6). Indirect effects are also possible. In localities with greater exposure to the policy change, financial institutions expanded not

only credit but also the number of branches. This may have broadened the overall supply of credit and, more generally, the supply of other financial services. Fonseca and Matray (2024) provide evidence from Brazil showing that an increase in bank branches can lead to increases in employment. If this were the case in our setting, we should observe an increase in lending to the non-priority sector in localities more exposed to the policy. Column 3 of Appendix Table A9 reports impacts on total lending to non-priority sectors using our locality-level design. While we find no evidence of significant impacts on lending to the non-priority sector under our main specification, the point estimates are positive, suggesting a 21% increase in non-priority lending. Indeed, in specifications with greater statistical power—such as those in Panels C and D of Appendix Table A11, which include additional controls and expand the control group, respectively—we are able to detect significant increases. Thus, one possible unintended consequence of the policy was the broad expansion of credit to non-priority activities.

We also provide evidence against an increase in savings as a driving force. Cramer (2025) finds that increases in bank branches lead to increases in saving accounts and long-term investments. Thus, in our setting, households could have increased their savings and used those savings to self-finance businesses in the non-priority sectors. This would be consistent with theoretical work from Moll (2014). If this mechanism holds in our setting, we should observe positive impacts on savings among households in the non-priority sector.

Appendix Table A17 suggests that this is not the case. Column 4 reports impacts on savings, computed as the difference between total household income and total household expenses. We find no evidence of increases in savings in the overall sample. Columns 5 and 6 report impacts for households that are likely to work in or run businesses in the priority sector and those who are likely to be part of the non-priority sector, based on a random forest classification model trained with baseline data,[24] respectively. Column 6 shows statistically nonsignificant

[24]We follow this approach because sector of employment is potentially endogenous to the policy, and,

impacts. Together, these results suggest that the savings channel was not a particularly relevant mechanism explaining the different results by sector.

Enforcement. — Recall that regulators monitored compliance based on the stated use of a loan rather than the borrower's sector of activity. Beyond allowing borrowers in non-priority activities to access credit directly, this left room for diversion: the stated purpose of a loan need not have coincided with its actual use. This idea is consistent with previous evidence of borrowers misreporting their debt (Karlan and Zinman, 2008) and evidence suggesting that loans are used in activities that differ from the stated loan purpose.[25] In our setting, the lending quotas encouraged lenders to expand the supply of credit to priority sectors and enforcement considered the stated purpose of the loan. Accordingly, individuals may have utilized priority loans to fund higher-return activities in the non-priority sector.

We test for this mechanism in two ways. One hypothesis is that individuals working in priority sectors used the loans to pivot to a different sector. Column 5 in Panel A of Table 6 provides suggestive evidence on this hypothesis. It suggests that employment in the non-priority sector increased in localities with higher exposure to the lending quotas.

Another explanation is that there was *within*-household reallocation of credit from relatively less to more constrained family businesses. To test this hypothesis, we rely on the insights from Bernhardt et al. (2019), who find that household members obtaining credit or capital grants do not always invest in their own

without panel data, we cannot condition on predetermined sector of employment. Our approach is as follows. We take the data from the pre-period to estimate a random forest model in order to predict probability of working in the priority sector, based on individual-level demographic characteristics and locality-level characteristics. We then use the model to predict probabilities for the post period. We then take the average of these predicted probabilities across the working-age adults for each household to get a household-level probability. We then classify households as likely to be in the priority or non-priority sector if the household-level probabilities are above or below the sample median, respectively.

[25] For example, loans from a microcredit program aiming to generate income in Thailand increased consumption without having any impacts on productive activities (Kaboski and Townsend, 2012). Likewise, microloans to finance investments in water sanitation in Cambodia were not always used to finance such investments (Ben Yishay et al., 2017), and loans obtained by women entrepreneurs end up financing the businesses of their spouses (Bernhardt et al., 2019).

businesses but may invest in those owned by their partners. We apply this idea to the data by focusing on the subsample of household heads and spouses/partners that cohabit. We then estimate our locality-level difference-in-differences model for individuals whose spouse works in the priority sector (Panel A of Table 7) and for individuals whose spouse works in a non-priority sector (Panel B).

We find evidence of significant impacts on self-employment activities in non-priority sectors among individuals whose partner worked in the priority sector (column 2 in Panel A). In contrast, we do not find evidence of significant impacts for those whose partner did not work in a priority sector (column 2 in Panel B). The magnitudes of the coefficients also differ substantially, although they are not statistically significant. The effects on income from activities in the non-priority sector for individuals whose partner worked in the priority sector are almost twice as large as those observed for individuals whose partner did not work in the priority sector. Reassuringly, we do not find impacts on self-employment income in priority sectors in either sample. In addition, we find similar patterns when we use a continuous measure of locality exposure (see Appendix Table A18) and when we implement the alternative specifications described in Section V.C (see Appendix Table A20).

Consumption. — The credit directed to the mostly microentrepreneurs in the priority sector may have freed up resources to finance household consumption spending as opposed to business spending. Because many businesses in the non-priority sector provide services, and are related to retail and commerce, one might expect that this potential increase in consumption by households in the priority sector generated an increase in income in the non-priority sector. For this to have happened, we would have to observe positive impacts on consumption, particularly among households in the priority sector.

We do not find evidence supporting this channel. Appendix Table A17 reports results based on our main specification in equation (3) using household-level data.

Column 1 shows no evidence of statistically significant impacts on per capita consumption for the entire sample. Based on a random forest classification model trained with baseline data, columns 2 and 3 report impacts for households that are likely to work or run businesses in the priority sector and those who are likely to be part of the non-priority sector, respectively. We do not find substantial impacts for either group.

B. Discussion

Put together, the results suggest that even though the loans were directed to the priority sector, the overall impacts on economic activity were driven by the non-priority sector.

Our analysis of mechanisms rules out the general equilibrium effect in which increased consumption by households in the priority sector stimulated the businesses of households in the non-priority sector. Likewise, we do not find evidence of savings as a driving force of our results by sector.

Instead, our results suggest that the policy enabled businesses in the non-priority sector to invest in priority-sector activities to improve their businesses. One example of this is construction—the category with the largest increase in its share of lending between 2014 and 2018 (see Appendix Figure A3). Further, the expansion of bank branches in response to the policy may have increased the supply of credit to the non-priority sector. In addition, loans meant to finance activities in the priority sector ended up being redirected to finance activities in the non-priority sector; private arrangements within the household made this reallocation possible.

Overall, our results suggest that implementation can shape the downstream outcomes of sectoral policies in ways that run contrary to a policy's stated objective.

There is evidence on directed-credit programs delivered through state-owned banks, where inefficient targeting and low-quality intermediation may limit the

impact of increased credit supply (Ru, 2018; Garber et al., 2024; Cole, 2009). Less is known about delivery through private intermediaries. In our setting, the gap between objective and outcome arises from how private lenders responded to the mandate and from the enforcement rule the regulator applied.

VII. Conclusion

This paper documents the equilibrium consequences of sectoral lending quotas introduced in Bolivia in 2014, tracing their effects from lender balance sheets to local economic activity and across sectors.

This paper documents the impacts of a nationwide regulation along multiple dimensions, illustrating important policy trade-offs. First, lenders reduced their financial margins both to attract more deposits and to draw in more borrowers. On net, the increase in deposits was outpaced by the increase in lending, increasing lender's balance sheet exposure to liquidity shocks. Second, the policy increased economic activity, but, contrary to its primary objective, the impacts were largely on non-priority sectors. Our analysis underscores that directed-credit policies are likely to spill over into other areas of the economy. Third, while economic activity increased as a result of the policy, similar policies are unlikely to generate sustained economic impacts: As markups and liquidity levels decline, lenders may become less willing to continue expanding credit without making more drastic and potentially risky and distortive adjustments. This last point is particularly relevant for other emerging economies with histories of bank runs, where lenders may be especially averse to assuming vulnerability to liquidity shocks.

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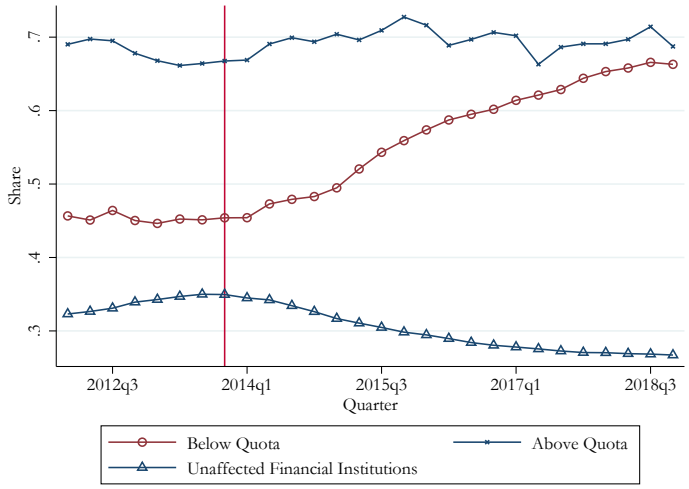
VIII. Figures and Tables

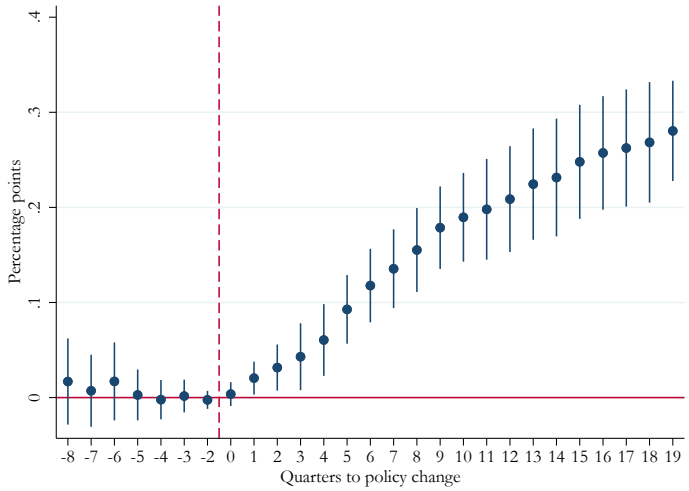
Table 1—: Shares of Total Lending and Number of Loans by Lender and Credit Characteristics, Baseline Year (2013)

	(1) Amount	(2) Number
Type of lender (%)		
Commercial banks (Banca Multiple)	.8	.776
Microfinance institutions (Banca PYME)	.075	.094
Cooperatives	.054	.072
Savings and credit associations (Mutuales)	.039	.016
Other intermediaries	.032	.042
Credit type (%)		
Consumption	.124	.305
Business	.166	.003
SME	.231	.043
Mortgages	.193	.068
Microcredit	.286	.581
Type of borrower (%)		
Individuals	.603	.991
Firms	.397	.009

Note: The table presents distribution of total portfolio by type of lender, credit type, and type of borrower. Data correspond to the year preceding the policy change (2013).



(a) Share of priority loans by pre-policy quota compliance



(b) Effects on share of priority loans

Figure 1. : Effects of Lending Quotas on Compliance - Lender-Level Design

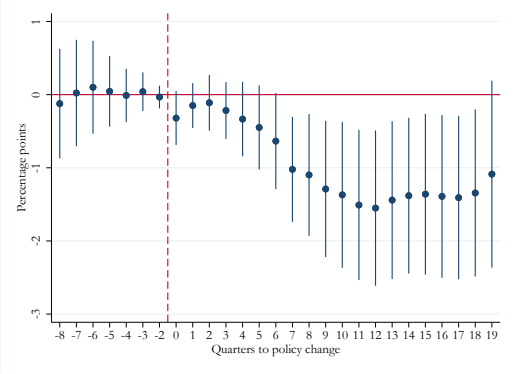
Note: Panel A presents the average share of lending allocated to priority sectors across financial institutions over time by pre-policy compliance. Each line corresponds to a different group: lenders whose share was below the target quota, lenders whose share was above the target quota, and lenders that were not affected by the lending quota policy. Panel B plots the event-study coefficients corresponding to the specification in equation (2). The outcome is the share of priority loans. The dotted line marks the timing of the lending quota's introduction, 2013Q4, which is the omitted category in our models. 95% confidence interval bars are based on clustered standard errors at the lender level.

Table 2—: Effects on Compliance and Lending - Lender-Level Design

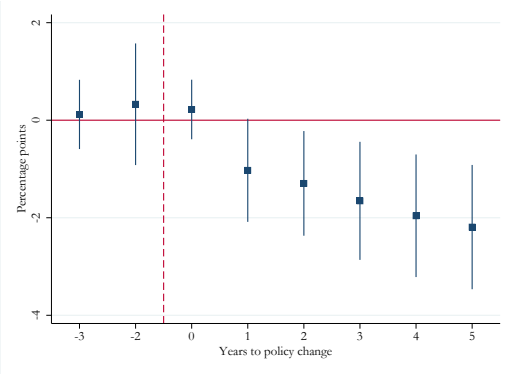
	(1)	(2)	(3)	(4)
	Share	Lending (Logs)		
	(Priority)	Priority	Non-priority	Total
Panel A: Discrete exposure				
Post × High distance to quota	0.15***	0.69***	-0.01	0.31***
	(0.03)	(0.09)	(0.10)	(0.07)
Observations	1396	1396	1396	1396
Panel B: Continuous exposure				
Post × Distance to quota	0.79***	3.88***	0.33	1.74***
	(0.13)	(0.70)	(0.37)	(0.47)
Observations	1396	1396	1396	1396
Average Effect	0.03	0.17	0.01	0.08
Mean (Baseline)	0.43	16.84	17.17	17.76

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

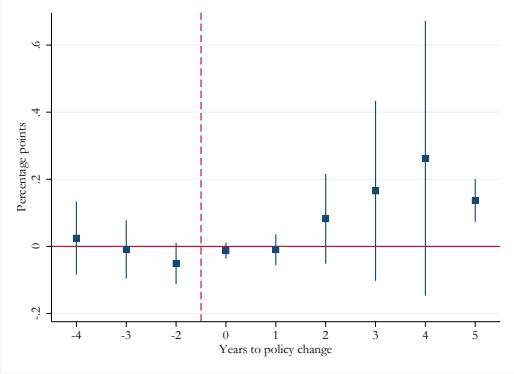
Note: The table reports estimates of the effects of lending quotas on key lending outcomes using the specification in equation (1). Panel A reports outcomes using a discrete measure of exposure, and Panel B reports outcomes using a continuous measure of exposure. The average treatment effect in Panel B is computed by multiplying the average exposure (0.04) by the corresponding point estimate. Standard errors are clustered at the lender level and reported in parentheses.



(a) Interest rates



(b) Spreads



(c) Loan-to-deposit ratio

Figure 2. : Dynamic Effects on Interest Rates, Spreads, and Liquidity - Lender-level design

Note: The figure plots event-study coefficients corresponding to the specification in equation (2). Panel A plots impacts on the lender-level average interest rate, across loans. The dotted line marks the timing of the lending quota's introduction, 2013Q4, which is the omitted category in our models. The estimates include quarter and lender fixed effects. The 95% confidence interval bars are obtained from clustering standard errors at the lender level. Panel B plots impacts on the lender's effective interest rate spread using annual data from ASFI's reports. Panel C plots impacts on the ratio of total lending portfolio to total deposits using annual balance sheet data. The dotted line marks the timing of the lending quota's introduction, in 2013, which is also the reference category. The estimates include year and lender fixed effects.

Table 3—: Effects on Risk, Interest Rates, Spreads, Returns, and Operative Results - Lender-Level Design

	(1) Delinquency Rate	(2) Interest Rate %	(3) Spreads	(4) Operative result	(5) Loans to deposits ratio	(6) Deposits to loans ratio
Panel A: Discrete exposure						
Post × High distance to quota	-0.000	-0.964*	-1.412***	0.003	0.106	-0.155
	(0.003)	(0.482)	(0.509)	(0.003)	(0.092)	(0.104)
Observations	1396	1396	440	492	492	492
Panel B: Continuous exposure						
Post × Distance to quota	0.000	-10.121***	-11.533***	0.026**	0.365	-0.399
	(0.014)	(3.207)	(3.601)	(0.012)	(0.379)	(0.443)
Observations	1396	1396	440	492	492	492
Average Effect	0.000	-0.442	-0.514	0.001	0.016	-0.017
Mean (Baseline)	0.027	14.425	8.771	0.013	0.947	1.093

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

Note: The table reports estimates of the effects of lending quotas on lenders outcomes using the specification in equation (1). Panel A reports outcomes using a discrete measure of exposure, and Panel B reports outcomes using a continuous measure of exposure. Column (1) reports impacts on the share of loans with $>$ 30 days of past-due payments over total number of loans. Column (2) reports impacts on the average interest rate across all loans, for a given lender. Column (4) reports impacts on a lender's operative result, defined as the ratio of the operative result (financial and nonfinancial revenues net of financial costs, operative costs, and administrative expenses). Column (5) reports impacts on the ratio total portfolio over deposits. Columns 1 and 2 use quarterly administrative records. Columns 3, 4, and 5 use annual data from the financial authority's annual reports. Spreads are only measured starting in 2011. In Panel B, the average treatment effect is computed by multiplying the average exposure (0.04) by the corresponding point estimate. Standard errors are clustered at the lender level and reported in parentheses.

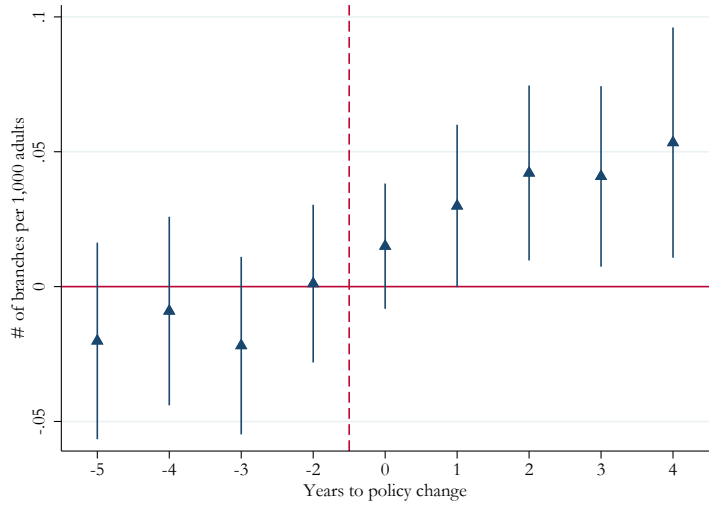
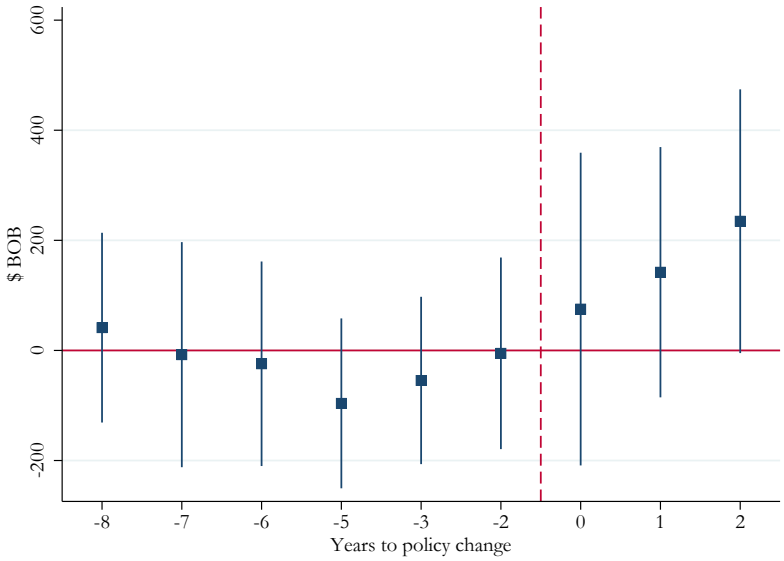
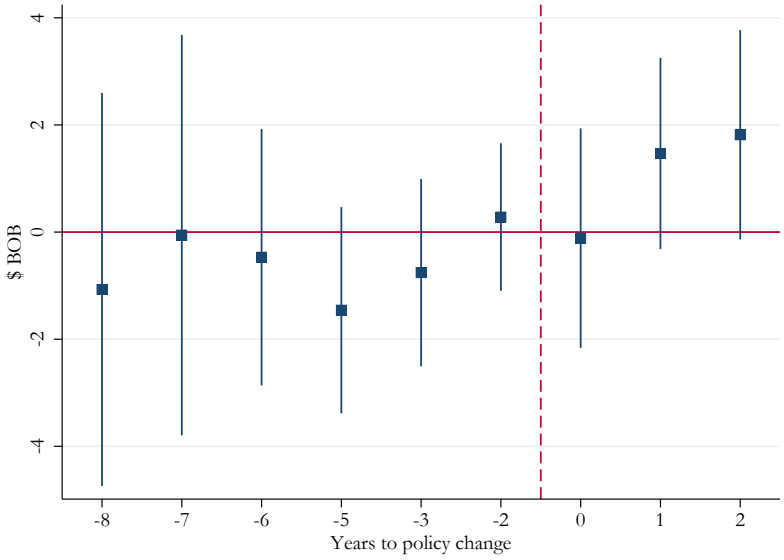


Figure 3. : Dynamic Effects on Physical Branches - Locality-Level Design

Note: The figure plots event-study coefficients corresponding to the impacts of lending quotas on the total number of physical branches per 1,000 adults using the specification in equation (4). The dotted line marks the year before the implementation of the quotas, 2013, which is also the reference category in our models. The 95% confidence interval bars are based on clustered standard errors at the municipality level.



(a) Personal income



(b) Wages

Figure 4. : Dynamic Effects on Income and Wages - Locality-Level Design

Note: The figure plots the event-study coefficients corresponding to the specification in equation (4). Panel A plots effects on real personal income ($BOB deflated with respect to 2016 prices) while Panel B plots effects on real hourly wages (income $BOB deflated with respect to 2016 prices and divided by hours worked). Both outcomes are winsorized with respect to the 99th percentile. The dotted line marks the year before the policy change, 2013, which is also the reference category. The 95% confidence interval bars are based on clustered standard errors at the municipality level.

Table 4—: Effects on Economic Activity and Labor Market Outcomes - Locality-Level Design

Panel A: Effects on employment, income and economic activity (Discrete exposure)					
	(1) Personal income	(2) Works	(3) Hours worked	(4) Hourly wage	(5) Night lights
Post × High exposure	175.85**	0.01	0.21	1.57**	1.23***
	(77.88)	(0.01)	(0.54)	(0.79)	(0.39)
Observations	134,734	134,734	134,734	87,126	2,030
Panel B: Effects on employment, income and economic activity (Continuous exposure)					
Post × Exposure	2093.55***	-0.12	-6.48	23.76***	4.83**
	(729.24)	(0.11)	(5.69)	(8.96)	(2.18)
Observations	134,734	134,734	134,734	87,126	2,030
Average Effect	299.46	-0.02	-0.93	3.40	0.62
Mean (Baseline)	2232.51	0.72	33.36	22.61	3.00
Panel C: Effects on income and hourly wages (Discrete exposure)					
	Wage income	Hourly Wage salaried	Self emp. income	Hourly Wage Self emp.	Self emp. worker
Post × High exposure	79.00*	1.26	90.19**	1.96**	-0.00
	(45.45)	(0.91)	(45.10)	(0.88)	(0.01)
Observations	134,734	45,954	134,734	40,853	96,970
Panel D: Effects on income and hourly wages (Continuous exposure)					
Post × Exposure	832.84	25.25**	1182.63**	20.54**	-0.01
	(572.42)	(11.69)	(536.34)	(9.62)	(0.10)
Observations	134,734	45,954	134,734	40,853	96,970
Average Effect	119.13	3.61	169.17	2.94	-0.00
Mean (Baseline)	1257.73	24.97	882.87	19.99	0.42

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

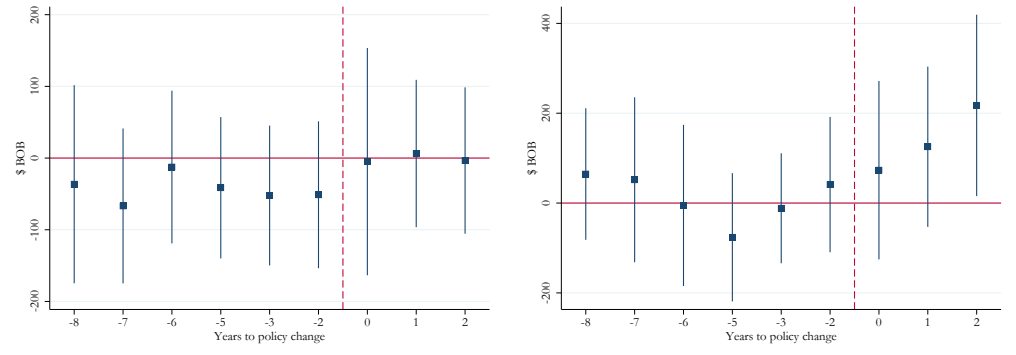
Note: The table reports estimates of the effects of lending quotas on economic activity and labor market outcomes using the specification in equation (3). Panels A and C report results using a discrete measure of exposure, and Panels B and D report outcomes using a continuous measure of exposure. All monetary outcomes are deflated using a consumer price index with 2016 as base (2016 = 100) and winsorized at the 99th percentile. In Panel B, the average treatment effect is computed by multiplying the average exposure (0.143 in regressions using survey data and 0.13 in regressions using night time lights) by the corresponding point estimate. Column 4 in Panels A and B reports results conditioning on being employed. Columns 2 and 4 in Panels C and D report results conditioning on being a salaried employee and on being self-employed, respectively. Column 5 in Panel A reports results using municipality-level data. Standard errors are clustered at the municipality level and reported in parentheses.

Table 5—: Effects on Self-employment Revenues and Costs - Locality-Level Design

	(1) Gross Income	(2) Total Costs	(3) Inputs	(4) Wages	(5) Rent and Utilities	(6) Taxes
Panel A: Discrete exposure						
Post × High exposure	171.48**	81.29*	63.07**	2.62	0.66	0.96**
	(81.00)	(41.63)	(30.82)	(6.01)	(2.20)	(0.43)
Observations	134,734	134,734	134,734	134,734	134,734	134,734
Panel B: Continuous exposure						
Post × Exposure	2657.97***	1475.34***	1065.87***	65.47	45.09**	5.28
	(877.40)	(455.61)	(367.79)	(67.84)	(22.78)	(4.52)
Observations	134,734	134,734	134,734	134,734	134,734	134,734
Average Effect	380.20	211.04	152.46	9.37	6.45	0.75
Mean (Baseline)	1547.62	664.75	470.09	57.89	34.07	6.10

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

Note: The table reports estimates of the effects of lending quotas on businesses outcomes using the specification in equation (3). Panel A reports outcomes using a discrete measure of exposure and Panel B reports outcomes using a continuous measure of exposure. Gross income is defined as revenues of self-employed workers before subtracting operation costs. Inputs include the purchase of raw materials, supplies, or merchandise. Wages include the payment of salaries, wages, pension's contribution and other benefits to employees. Rent and utilities include the rent payment for the premises and utilities (water, electricity, telephone, security). Taxes include payments for taxes, duties, and dues to unions and associations. All outcomes were deflated using the consumer price index with 2016 as a base year (2016 = 100) and were winsorized at the 99th percentile. In Panel B, the average treatment effect is computed by multiplying the average exposure (0.14) by the corresponding point estimate. Standard errors are clustered at the municipality level and reported in parentheses.



(a) Priority (b) Non-priority

Figure 5. : Dynamic Effects on Incomes by Economic Sector - Locality-Level Design

Note: The figure plots the event-study coefficients corresponding to the specification in equation (4). Panel A plots effects on income from prioritized sectors while Panel B plots effects on income from non-prioritized sectors (both in $BOB deflated with respect to 2016 prices). See Appendix Table A3 for a description of each of the sectors. Both outcomes are winsorized with respect to the 99th percentile. The dotted line marks the year preceding the policy change, 2013, which is also the reference category in our models. The 95% confidence interval bars are based on clustered standard errors at the municipality level.

Table 6—: Effects on Income by Economic Sector

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Priority				Non-priority			
		Income				Income		
	Prob.	Total	Self emp.	Salaried	Prob.	Total	Self emp.	Salaried
Panel A: Discrete exposure								
Post × High exposure	-0.01	36.09	20.00	16.09	0.02**	133.86**	70.22**	63.63*
	(0.01)	(39.31)	(23.85)	(23.54)	(0.01)	(56.31)	(31.73)	(38.11)
Observations	134,734	134,734	134,734	134,734	134,734	134,734	134,734	134,734
Panel B: Continuous exposure								
Post × Exposure	-0.34	694.97	88.54	606.43	0.22	1319.97**	1095.13***	224.84
	(0.22)	(425.84)	(317.25)	(380.63)	(0.17)	(664.93)	(375.28)	(433.33)
Observations	134,734	134,734	134,734	134,734	134,734	134,734	134,734	134,734
Average Effect	-0.05	99.41	12.66	86.74	0.03	188.81	156.65	32.16
Mean (Baseline)	0.28	705.44	310.21	395.24	0.44	1434.01	572.33	861.69

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

Note: The table reports estimates of the effects of lending quotas on the likelihood of working, income, and wages using the specification in equation (3) by type of activity. Panel A reports outcomes using a discrete measure of exposure, and Panel B reports outcomes using a continuous measure of exposure. See Appendix Table A3 for a description of each of the sectors. The outcomes in columns (1) and (5) take the value of 1 if an individual works in the respective sector; 0 otherwise. The labor income outcomes in column (2) and (6) includes wage and self-employment incomes. All income outcomes are deflated using a consumer price index with 2016 as baseline year (2016 = 100) and winsorized at the 99th percentile. In Panel B, the average treatment effect is computed by multiplying the average exposure (0.14) by the corresponding point estimate. Standard errors are clustered at the municipality level and reported in parentheses.

Table 7—: Effects on Self-Employment Income by Type of Sector and Partner's Occupation Sector - Discrete Exposure

	(1) Total	(2) Non-priority	(3) Priority
Panel A: Partner works in the priority sector			
Post × High exposure	171.29***	139.11***	32.18
	(63.71)	(51.20)	(36.80)
Observations	23,642	23,642	23,642
P-value Diff. (2)-(3)			0.087
Panel B: Partner works in the Non-priority sector			
Post × High exposure	125.01	72.46	51.29
	(95.44)	(73.42)	(39.85)
Observations	32,035	32,035	32,035
P-value Diff. (2)-(3)			0.759
P-value Diff. Panel A-B (2)			0.345
P-value Diff. Panel A-B (3)			0.705

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

Note: The table reports the effects of lending quotas on self-employment income using the specification in equation (3) for individuals that cohabit with their spouses or partners. Panel A reports results for individuals whose partner works in the priority sector. Panel B reports results for individuals whose partner works in the non-priority sector. The table reports p-values corresponding to a test of differences between columns 2 and 3 within each panel and to a test of differences across panels within each column. Standard errors are clustered at the municipality level and reported in parentheses.

Online Appendix: Appendix Figures and Tables.

Table A1—: Labor Markets in Bolivia (Household Survey, 2013)

Variable	Value
Panel A: Employment	
% of adults who work	74
% of adults working as salaried workers	32
% of workers working for a salary	44
% of self-employed adults	32
% of self-employed workers	43
Panel B: Average Monthly Income and Wages	
Household income	6003.5
Per-capita household income	1478.1
Personal earnings	2097.7
Personal earnings from salaried work (unconditional)	1163.8
Personal earnings from salaried work (conditional on working)	1572.9
Personal earnings from self-employment (unconditional)	844.9000000000001
Personal earnings from self-employment (conditional on working)	1141.9
Hourly wage	21.5
Hourly wage - salaried workers	24.9
Hourly wage - self-employed workers	18.2

Note: This table presents descriptive statistics of Bolivian labor markets for the baseline year (2013). Panel A summarizes key employment indicators for the adult population aged 18 to 65. Panel B reports monthly income and earnings metrics at three levels: household, individuals, and employed workers. Income and earnings figures are expressed in real terms, deflated using a consumer price index with year 2016 as base (2016 = 100).

Table A2—: Lending quotas by type of lender, baseline quarter (2013-Q4)

Type of lender	(1) Mandated lending quota (%) portfolio	(2) Complying lenders (%)	(3) Average distance to quota (percentage points)
Commercial Banks (Banca Multiple)	60	7.7	.1
Microfinance institutions (Banca PYME)	50	0	.1
Cooperatives	0	100	-.3
Savings and Credit Associations (Mutuales)	50	62.5	-.1
Other Intermediaries	60	0	.2
Total		62.7	-.2

Note: The table presents mandated quotas and baseline comliance by type of lender. Column (1) reports the stated minimum portfolio share by type of lender. Column (2) displays the percentage of lenders who are already compliant at baseline by 2013Q4. Column (3) provides the average distance to the mandated quota; a negative value indicates that, on average, lenders fall below the target quota.

Table A3—: Description of prioritized sectors

Priority sectors	**Non-priority sectors**
- Agriculture and livestock	- Wholesale and retail sales
- Hunting, forestry and fishing	- Hotels and restaurants
- Extraction of hydrocarbons	- Transportation, storage and communications
- Mining	- Real estate and business ser vices
- Manufacturing	- Public administration ser vices
- Production and distribution of electricity, gas and water	- Social, communal and per sonal services
- Construction	- Other services

Note: The table reports the portfolio classification by credit destiny according to the lending quota policy.

Table A4—: Correlates of lender-level exposure and lender characteristics.

	(1) Mean	(2) Coefficient
Distance to quota	0.089	
	(0.019)	
Share of loans	0.040	-0.626
	(0.008)	(0.692)
Share of clients	0.040	0.299
	(0.012)	(0.422)
Microfinance institutions (Banca PYME)	0.120	-0.156***
	(0.066)	(0.044)
Savings and Credit Associations (Mutuales)	0.320	-0.156***
	(0.095)	(0.038)
Other Intermediaries	0.040	-0.018
	(0.040)	(0.060)
Delinquency rate	0.030	-0.759
	(0.004)	(0.732)
Deposits over assets	0.689	0.245**
	(0.031)	(0.105)
Financial return	0.061	1.548**
	(0.006)	(0.714)
Interbank borrowing	0.053	0.379
	(0.010)	(0.431)

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

Note: The table reports means (column 1) and coefficients from a regression of lender-level *Distance to quota* on multiple covariates measured in 2013 (column 2). Heteroskedasticity-robust starndard errors are reported in parentheses.

Table A5—: Correlates of municipality-level exposure and municipality characteristics.

	(1) Mean	(2) Coefficient
Exposure	0.131	
	(0.007)	
Population (logs)	3.151	0.007
	(0.088)	(0.011)
Top three largest municipalities	0.021	-0.057**
	(0.012)	(0.023)
% of adults who work	72.032	0.002
	(0.580)	(0.002)
% of self-employed workers	51.606	0.001
	(1.089)	(0.001)
% of workers in priority sectors	60.632	-0.000
	(1.475)	(0.001)
Interest rate	18.462	0.005
	(0.203)	(0.004)
Total net lending (logs)	17.979	0.013***
	(0.281)	(0.005)

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

Note: The table reports means (column 1) and coefficients from a regression of locality-level *Exposure* on multiple covariates measured in 2013 (column 2). Standard errors are clustered at the municipal level and reported in parentheses.

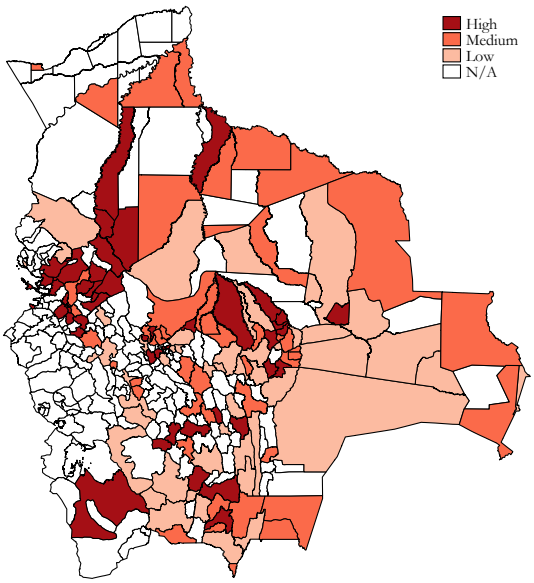


Figure A1. : Geographical distribution of municipality-level exposure

Note: The figure displays the distribution, across municipalities, of our measure of exposure to the lending quotas. It is divided in three levels of intensity according to the distribution of the exposure variable: high, medium and low. There are 339 municipalities in Bolivia. We show municipalities with no presence of financial institutions branches in white.

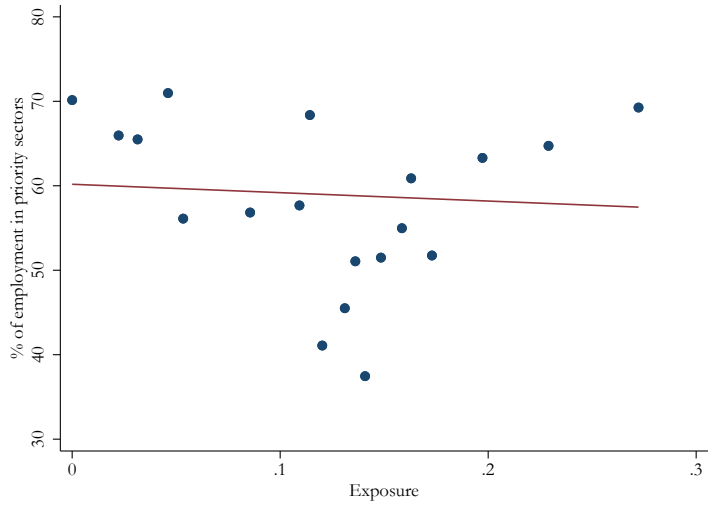
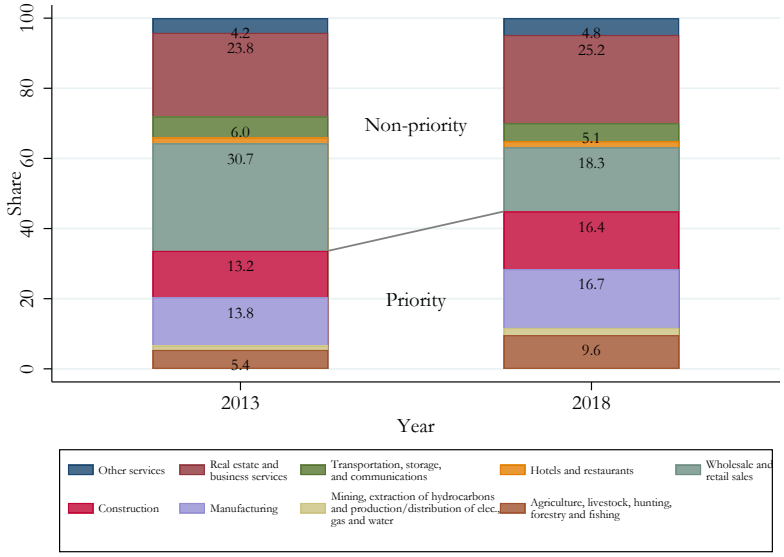
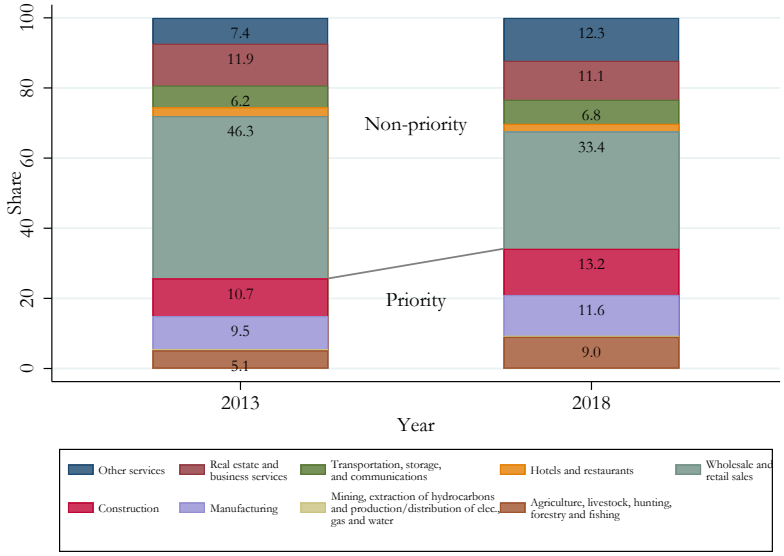


Figure A2. : Baseline share of employment in priority sectors and exposure (municipality-level)

Note: The figure plots the municipality-level share of employment in the priority sector by quantiles (20) of exposure. The shares of employment in the priority sector are computed using data from the 2012 population census.



(a) Total credit



(b) Number of loans

Figure A3. : Total lending portfolio by stated sector before (2013) and after the policy change (2018)

Note: The figure plots the distribution of total lending (panel a) and loans (panel b) by sectors for the years 2013 and 2018 respectively.

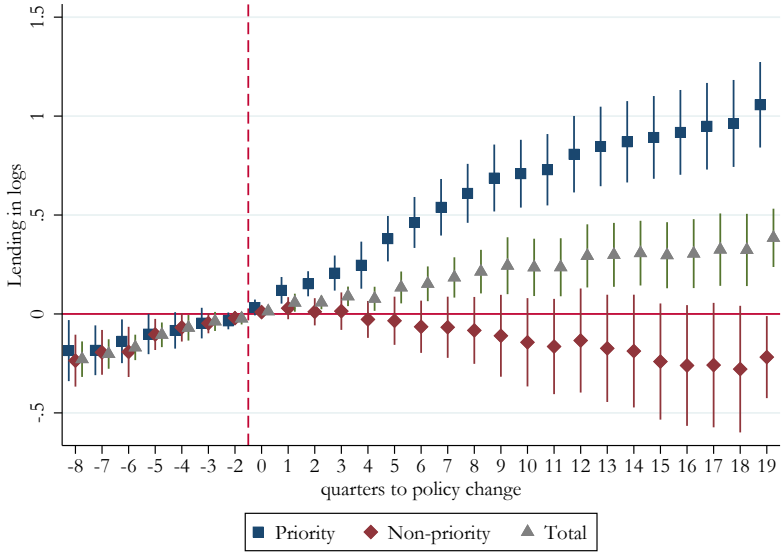


Figure A4. : Effects of lending quotas on lending - Lender level

Note: The figure plots event-study coefficients corresponding to the specification in equation (2). Priority loans include credit given to priority sectors or for social housing. Non-priority loans include credit given to other non-priority sectors and consumption loans. See Appendix Table A3 for details on priority and non-priority sectors. The dotted line marks the quarter before the policy change, 2013Q4, which is also the reference category. 95% confidence interval bars are based on clustered standard errors at the lender level.

Table A6—: Effects on Deposits, Cash on hand, and Interbank Borrowing - Lender-level design

	(1) Deposits (logs)	(2) Cash on hand (logs)	(3) Interbank borrowing (logs)
Panel A: Discrete exposure			
Post × High distance to quota	0.336***	-0.013	0.204
	(0.104)	(0.127)	(0.399)
Observations	492	492	401
Panel B: Continuous exposure			
Post × Distance to quota	2.175***	-0.606	0.410
	(0.611)	(0.881)	(1.565)
Observations	492	492	401
Average Effect	0.095	-0.026	0.018
Mean (Baseline)	10.89	8.77	7.51

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

Note: The table reports estimates of the effects of lending quotas on key lending outcomes using the specification in equation (1). Panel A reports outcomes using a discrete measure of exposure and Panel B reports outcomes using a continuous measure of exposure. Column (1) reports impacts on the sum of all lender's obligations with the public (demand, term saving accounts and restricted deposits and accrued payable charges) in logs. Column (2) reports impacts on the sum of all lender's availabilities that lenders have (in cash, at the Central Bank, local banks, head office and branches, banks abroad, documents for immediate collection, accrued income receivable and provision of available funds) in logs. Column (3) reports impacts on the lender's total obligations with Banks and Financial Entities in logs. We lose some observations as not all lenders borrow from other financial entities at each point in time. Outcomes in columns (1)-(3) are based on Balance Sheets and are only available at the annual level. Columns (1)-(3) include year and lender fixed effects. The average treatment effect in panel B is computed by multiplying the average exposure (0.04) by the corresponding point estimate. Standard errors are clustered at the lender level and reported in parentheses.

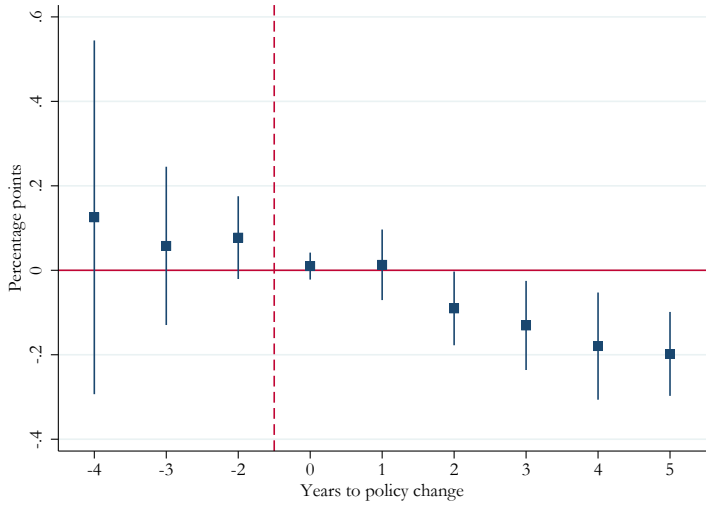


Figure A5. : Deposits-to-loans ratio

Note: The figure plots event-study coefficients corresponding to the impacts of lending quotas on the deposit-to-loans ratio using the specification in equation (4). The dotted line marks the timing of the lending quota's introduction, at the year 2013, which is also the reference category in our models. The estimates include year and lender fixed effects. 95% confidence interval bars are based on clustered standard errors at the lender level.

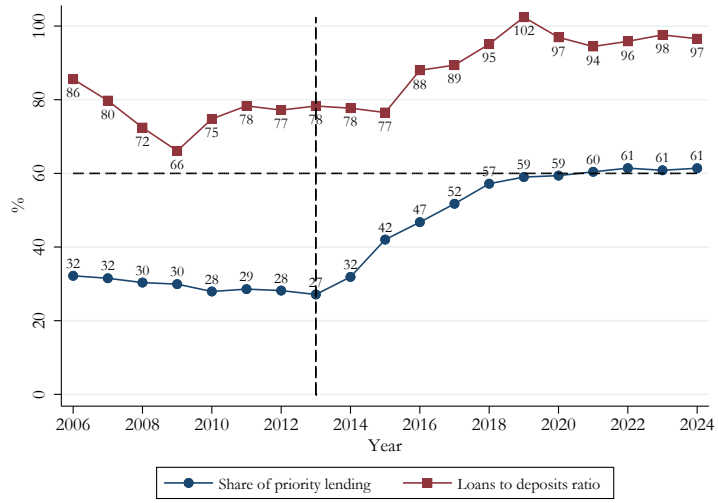


Figure A6. : Portfolio structure and liquidity over time

Note: The figure plots the evolution of the share of priority sectors over total portfolio, the share of total portfolio over deposits as well as the amount of each of the prioritized sectors. The blue dotted line represents the minimum quota imposed on lenders. The dotted black line marks the quarter preceding the policy change (2013 Q4).

Table A7—: Effects on Compliance and Lending - Alternative Specifications - Lender level

	(1)	(2)	(3)	(4)
	Share	Lending (Logs)		
	(Priority)	Priority	Non-priority	Total
Panel A: Excluding unaffected lenders				
Post × High distance to quota	0.09**	0.52***	0.13	0.33***
	(0.04)	(0.13)	(0.15)	(0.11)
Observations	671	671	671	671
Panel B: Balanced panel				
Post × High distance to quota	0.14***	0.74***	0.11*	0.38***
	(0.03)	(0.10)	(0.07)	(0.06)
Observations	1232	1232	1232	1232
Panel C: Including Post × Pre-policy Avg. interest rate				
Post × High distance to quota	0.15***	0.69***	-0.01	0.31***
	(0.03)	(0.09)	(0.10)	(0.07)
Observations	1396	1396	1396	1396
Panel D: Synthetic Diff-in-diff (including controls)				
Average effect	0.16***	0.60***	-0.06	0.23***
	(0.02)	(0.05)	(0.07)	(0.04)
Observations	1232	1232	1232	1232

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

Note: The table reports results using alternative specifications to those reported in Table 2. Panel A drops untreated lenders. Panel B restricts the sample to a balanced panel, retaining only lenders with observations in every year of the study period. Panel C introduces as a control the interaction between the post-policy period and the average interest rate in the pre-policy year (2013). Panel D employs a synthetic difference-in-differences estimator, using baseline lender characteristics as controls. Standard errors are clustered at the lender level and reported in parentheses.

Table A8—: Effects on Delinquency Rates, Interest Rates, Spreads, Returns and Operative Results - Alternative Specifications (Lender-level design)

	(1) Delinquency Rate	(2) Interest Rate %	(3) Spreads	(4) Operative result	(5) Loans to deposits ratio	(6) Deposits to loans ratio
Panel A: Excluding unaffected lenders						
Post × High distance to quota	0.001	-1.627**	-1.526***	0.000	-0.001	-0.042
	(0.004)	(0.687)	(0.510)	(0.003)	(0.087)	(0.112)
Observations	671	671	209	234	234	234
Panel B: Balanced panel						
Post × High distance to quota	-0.000	-0.778	-1.298**	0.005*	0.125	-0.183
	(0.004)	(0.553)	(0.575)	(0.003)	(0.105)	(0.119)
Observations	1232	1232	387	440	440	440
Panel C: Including Post × Pre-policy Avg. interest rate						
Post × High distance to quota	0.000	-1.026***	-1.435***	0.003	0.095	-0.142*
	(0.003)	(0.359)	(0.485)	(0.002)	(0.071)	(0.079)
Observations	1396	1396	440	492	492	492
Panel D: Synthetic Diff-in-diff (including controls)						
Average effect	-0.003	-0.821***	-1.490***	0.005*	0.150**	-0.122**
	(0.004)	(0.271)	(0.412)	(0.002)	(0.068)	(0.059)
Observations	1232	1232	387	440	440	440

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

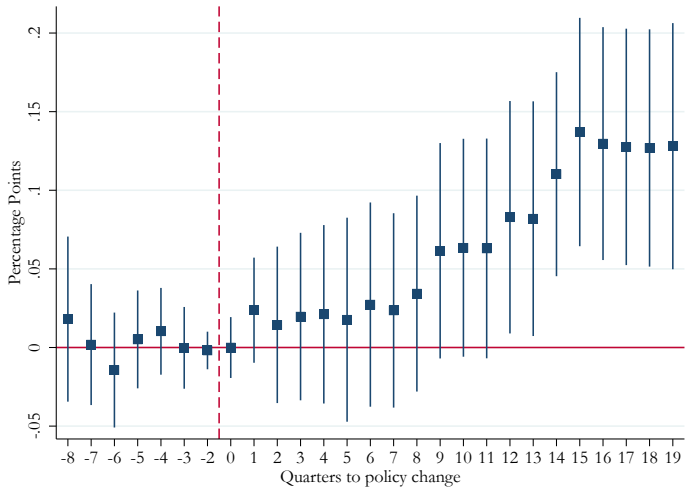
Note: The table reports results from alternative specifications to those in Table 3. Panel A drops untreated lenders. Panel B restricts the sample to a balanced panel, retaining only lenders with observations in every year of the study period. Panel C introduces as a control the interaction between the post-policy period and the average interest rate in the pre-policy year (2013). Panel D employs a synthetic difference-in-differences estimator, using baseline lender characteristics as controls. Standard errors are clustered at the lender level and reported in parentheses.

Table A9—: Effects on Lending and Interest Rates - Locality-level Design

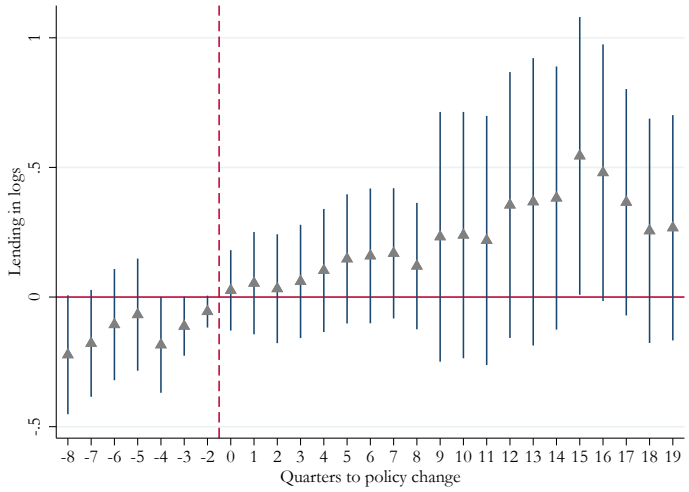
	(1)	(2)	(3)	(4)	(5)	(6)
	Share	Lending (Logs)			Interest	Delinquency
	(Priority)	Priority	Non-priority	Total	Rate %	Rate
Panel A: Discrete exposure						
Post × High exposure	0.06**	0.35***	0.21	0.34**	-0.14	0.00
	(0.02)	(0.11)	(0.13)	(0.13)	(0.28)	(0.01)
Observations	3327	3214	3289	3327	3327	3328
Panel B: Continuous exposure						
Post × Exposure	0.51**	2.51***	1.04	2.12**	-2.68	0.03
	(0.20)	(0.87)	(0.96)	(0.87)	(2.13)	(0.06)
Observations	3327	3214	3289	3327	3327	3328
Average Effect	0.07	0.33	0.14	0.28	-0.35	0.00
Mean (Baseline)	0.45	14.72	14.82	15.46	17.95	0.02

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

Note: The table reports estimates of the effects of lending quotas on key compliance and lending outcomes using the specification in equation (3). Panel A reports results using a discrete measure of exposure and Panel B reports results using a continuous measure of exposure. Column (6) reports impacts on the ratio of loans with > 30 days of past-due payments divided by the total number of loans in municipality m. In Panel B, The average treatment effect is computed by multiplying the average exposure (0.13) by the corresponding point estimate. Standard errors are clustered at the municipality level and reported in parentheses.



(a) Compliance (share of priority lending)



(b) Total lending (in logs)

Figure A7. : Effects of lending quotas and total lending - Locality-level design

Note: The figure plots event-study coefficients corresponding to the specification in equation (4). Panel A plots the share of priority loans while Panel B plots total lending in logs. The dotted line marks the quarter preceding the policy change, 2013Q4, which is also the reference category in our models. The estimates include quarter and municipality fixed effects. 95% confidence interval bars are based on clustered standard errors at the municipality level.

Table A10—: Effects on Physical Branches per 1000 Adults By Type of Branch - Locality-level Design

	(1) Permanent	(2) Pop-ups	(3) All
Panel A: Discrete exposure			
Post × High exposure	0.04***	0.01	0.05***
	(0.01)	(0.01)	(0.02)
Observations	1450	1450	1450
Panel B: Continuous exposure			
Post × Exposure	0.18*	0.06	0.24**
	(0.11)	(0.04)	(0.12)
Observations	1450	1450	1450
Average Effect	0.02	0.01	0.03
Mean (Baseline)	0.25	0.03	0.28

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

Note: The table reports estimates of the effects of lending quotas on the number of physical branches using the specification in equation (3). Panel A reports outcomes using a discrete measure of exposure and Panel B reports outcomes using a continuous measure of exposure. Column (1) reports impacts on permanent branches which includes permanent agencies, offices, financial correspondent service points and branches. Column (2) reports impacts on pop-ups which include mobile agencies, nonfinancial correspondent service points and other costumer service attention points. All outcomes are expressed in branches per 1,000 adults. In Panel B, the average treatment effect is computed by multiplying the average exposure (0.13) by the corresponding point estimate. Standard errors are clustered at the municipality level and reported in parentheses.

Table A11—: Effects on Compliance, Lending and Interest Rates - Alternative Specifications (Locality-level design)

	(1)	(2)	(3)	(4)	(5)	(6)
	Share	Lending (Logs)			Interest	Delinquency
	(Priority)	Priority	Non-priority	Total	Rate %	Rate
Panel A: Excluding the three largest urban centers						
Post × High exposure	0.06**	0.36***	0.21	0.35**	-0.17	0.00
	(0.02)	(0.11)	(0.14)	(0.14)	(0.28)	(0.01)
Observations	3243	3130	3205	3243	3243	3244
Panel B: Balanced panel						
Post × High exposure	0.09***	0.34***	0.12	0.35**	-0.21	-0.00
	(0.02)	(0.10)	(0.14)	(0.14)	(0.29)	(0.00)
Observations	2800	2768	2790	2800	2800	2800
Panel C: Including Post × Pre-policy Avg. interest rate						
Post × High exposure	0.06**	0.35***	0.22*	0.34***	-0.31	0.00
	(0.02)	(0.11)	(0.12)	(0.12)	(0.20)	(0.01)
Observations	3171	3128	3133	3171	3171	3172
Panel D: Including municipalities with no branches						
Post × High exposure	0.04*	0.57***	0.32**	0.54***	-0.05	0.00
	(0.02)	(0.12)	(0.13)	(0.14)	(0.26)	(0.01)
Observations	3780	3565	3673	3780	3780	3782
Panel E: Synthetic Diff-in-diff (including controls)						
Average effect	0.07***	0.71***	0.15	0.35***	-0.28	-0.00
	(0.02)	(0.18)	(0.10)	(0.12)	(0.31)	(0.01)
Observations	2800	2800	2800	2800	2800	2800

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

Note: The table reports results from alternative specifications to those reported in Table A9, using our discrete measure of exposure. Panel A excludes observations from the three largest urban centers (by population); La Paz, Cochabamba, and Santa Cruz. Panel B excludes sample weights from the estimations. Panel C controls for the interaction between the post-policy period and the average interest rate in the pre-policy year (2013). Panel D includes municipalities without physical bank branches, assigning them an exposure value ($High\ exposure_m$) of zero. Panel E employs a synthetic difference-in-differences estimator, using baseline municipal characteristics as controls. Standard errors are clustered at the municipal level and reported in parentheses.

Table A12—: Effects on Economic Activity and Labor Market Outcomes - Alternative Specifications (Locality-level design)

	(1) Personal income	(2) Works	(3) Hours worked	(4) Hourly wage	(5) Night lights
Panel A: Excluding the three largest urban centers					
Post × High exposure	74.94	-0.00	-0.36	1.08	1.26***
	(74.66)	(0.01)	(0.58)	(0.95)	(0.38)
Observations	82,286	82,286	82,286	52,930	1,988
Panel B: Without sample weights					
Post × High exposure	164.19*	0.01	-0.15	1.70**	
	(84.33)	(0.01)	(0.45)	(0.79)	
Observations	134,734	134,734	134,734	87,126	
Panel C: Including Post × Pre-policy Avg. interest rate					
Post × High exposure	170.94***	0.01	0.11	1.58**	1.09**
	(60.38)	(0.01)	(0.53)	(0.73)	(0.42)
Observations	129,687	129,687	129,687	84,040	1,652
Panel D: Including municipalities with no branches					
Post × High exposure	168.56**	0.01	0.49	1.22	1.17***
	(72.42)	(0.01)	(0.47)	(0.80)	(0.38)
Observations	152,220	152,220	152,220	97,469	2,198
Panel E: Winsorizing at the 95th percentile					
Post × High exposure	129.57**		0.21	1.25**	0.55*
	(52.52)		(0.50)	(0.52)	(0.30)
Observations	134,734		134,728	87,126	2,030

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

Note: The table reports results from alternative specifications to those reported in Panel A of Table 4, using our discrete measure of exposure. Panel A excludes observations from the three largest urban centers (by population); La Paz, Cochabamba, and Santa Cruz. Panel B excludes sample weights from the estimations. Panel C controls for the interaction between the post-policy period and the average interest rate in the pre-policy year (2013). Panel D includes municipalities without physical bank branches, assigning them an exposure value ($High\ exposure_m$) of zero. Panel E winsorizes each continuous outcome with respect to the 95th percentile. Standard errors are clustered at the municipality level and reported in parentheses.

Table A13—: Effects on Labor Market Outcomes by Type of Employment - Alternative Specifications (Locality-level design)

	(1) Wage income	(2) Hourly Wage salaried	(3) Self emp. income	(4) Hourly Wage Self emp.	(5) Self emp. worker
Panel A: Excluding the three largest urban centers					
Post × High exposure	-9.91	0.90	91.91	1.48	0.01
	(41.42)	(1.03)	(56.72)	(1.12)	(0.01)
Observations	82,286	25,540	82,286	27,104	60,636
Panel B: Without sample weights					
Post × High exposure	76.89	1.25	79.65	2.28***	-0.01
	(47.20)	(0.87)	(53.90)	(0.78)	(0.01)
Observations	134,734	45,954	134,734	40,853	96,970
Panel C: Including Post × Pre-policy Avg. interest rate					
Post × High exposure	78.87***	1.10	87.24*	2.27**	-0.00
	(28.48)	(0.69)	(46.79)	(0.88)	(0.01)
Observations	129,687	44,842	129,687	38,911	92,979
Panel D: Including municipalities with no branches					
Post × High exposure	74.76*	1.27	89.59**	1.40	-0.01
	(41.03)	(0.89)	(42.83)	(0.86)	(0.01)
Observations	152,220	48,794	152,220	48,192	111,781
Panel E: Winsorizing at the 95th percentile					
Post × High exposure	61.89*	1.12**	66.70*	1.45**	
	(35.04)	(0.54)	(34.72)	(0.63)	
Observations	134,734	45,954	134,734	40,853	

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

Note: The table reports results from alternative specifications to those reported in Panel C of Table 4, using our discrete measure of exposure. Panel A excludes observations from the three largest urban centers (by population); La Paz, Cochabamba, and Santa Cruz. Panel B excludes sample weights from the estimations. Panel C controls for the interaction between the post-policy period and the average interest rate in the pre-policy year (2013). Panel D includes municipalities without physical bank branches, assigning them an exposure value ($High\ exposure_m$) of zero. Panel E winsorizes continuous outcomes with respect to the 95th percentile. Standard errors are clustered at the municipality level and reported in parentheses.

Table A14—: Effects on Economic Activity and Labor Market Outcomes - Excluding the three largest urban centers (Locality-level design)

Panel A: Effects on employment, income and economic activity (Continuous exposure)					
	(1) Personal income	(2) Works	(3) Hours worked	(4) Hourly wage	(5) Night lights
Post × Exposure	1803.53***	-0.15	-6.46	19.99**	5.07**
	(616.35)	(0.11)	(5.92)	(7.95)	(2.33)
Observations	82,286	82,286	82,286	52,930	2,272
Average Effect	262.67	-0.02	-0.94	2.91	0.65
Mean (Baseline)	1982.75	0.73	33.31	20.64	2.58
Panel B: Effects on income and hourly wages (Continuous exposure)					
	Wage income	Hourly Wage salaried	Self emp. income	Hourly Wage Self emp.	Self emp. worker
Post × Exposure	562.21	20.61*	1185.81**	17.63*	0.02
	(555.97)	(10.79)	(501.62)	(8.95)	(0.10)
Observations	82,286	25,540	82,286	27,104	60,636
Average Effect	81.88	3.00	172.70	2.57	0.00
Mean (Baseline)	999.62	23.29	901.80	18.42	0.46

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

Note: The table reports estimates of the impact of the quotas using our continuous measure of exposure to estimate the specification on equation (3) over a subsample that excludes the municipalities of the three largest urban centers in the country (by population); La Paz, Cochabamba, and Santa Cruz de la Sierra. The average treatment effect is computed by multiplying the average exposure (0.15) by the corresponding point estimate. Standard errors are clustered at the municipality level and reported in parentheses.

Table A15—: Effects on Self-employment Revenues and Costs - Alternative specifications (Locality-level design)

	(1) Gross Income	(2) Total Costs	(3) Inputs	(4) Wages	(5) Rent and Utilities	(6) Taxes
Panel A: Excluding the three largest urban centers						
Post × High exposure	166.14*	74.23	68.88*	-4.27	-0.49	0.59
	(93.17)	(46.69)	(37.80)	(5.38)	(2.84)	(0.55)
Observations	82,286	82,286	82,286	82,286	82,286	82,286
Panel B: Without sample weights						
Post × High exposure	142.55	62.90	51.09	1.05	1.27	0.41
	(93.58)	(45.12)	(31.20)	(6.40)	(1.69)	(0.47)
Observations	134,734	134,734	134,734	134,734	134,734	134,734
Panel C: Including Post × Pre-policy Avg. interest rate						
Post × High exposure	169.03**	81.80**	63.45**	3.00	0.87	0.91***
	(77.63)	(35.40)	(27.83)	(4.53)	(2.26)	(0.34)
Observations	129,687	129,687	129,687	129,687	129,687	129,687
Panel D: Including municipalities with no branches						
Post × High exposure	151.06*	61.47	45.19	1.08	0.95	0.41
	(78.77)	(42.10)	(31.45)	(5.71)	(1.90)	(0.50)
Observations	152,220	152,220	152,220	152,220	152,220	152,220
Panel E: Winsorizing at the 95th percentile						
Post × High exposure	91.78*	25.08	12.23	0.00	0.00	0.00
	(47.85)	(16.69)	(11.79)	(.)	(.)	(.)
Observations	134,734	134,734	134,734	134,734	134,734	134,734

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

Note: The table reports results from alternative specifications to those reported in Table 5, using our discrete measure of exposure. Panel A excludes observations from the three largest urban centers (by population); La Paz, Cochabamba, and Santa Cruz. Panel B excludes sample weights from the estimations. Panel C controls for the interaction between the post-policy period and the average interest rate in the pre-policy year (2013). Panel D includes municipalities without physical bank branches, assigning them an exposure value ($High\ exposure_m$) of zero. Panel E winsorizes each outcome with respect to the 95th percentile. Standard errors are clustered at the municipality level and reported in parentheses.

Table A16—: Effects on Branches - Alternative Specifications (Locality-level design)

	(1) Permanent	(2) Pop-ups	(3) All
Panel A: Excluding the three largest urban centers			
Post × High exposure	0.04***	0.01	0.05***
	(0.01)	(0.01)	(0.02)
Observations	1420	1420	1420
Panel B: Including Post × Pre-policy Avg. interest rate			
Post × High exposure	0.03*	0.01	0.04**
	(0.01)	(0.01)	(0.02)
Observations	1180	1180	1180
Panel C: Including municipalities with no branches			
Post × High exposure	0.08***	0.02***	0.10***
	(0.01)	(0.00)	(0.01)
Observations	3390	3390	3390

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

Note: The table reports results from alternative specifications to those reported in Table A10, using our discrete measure of exposure. Panel A excludes observations from the three largest urban centers (by population); La Paz, Cochabamba, and Santa Cruz. Panel B controls for the interaction between the post-policy period and the average interest rate in the pre-policy year (2013). Panel C includes municipalities without physical bank branches, assigning them an exposure value ($High\ exposure_m$) of zero. Standard errors are clustered at the municipality level and reported in parentheses.

Table A17—: Effects on Consumption and Savings - Locality-level design.

	(1)	(2)	(3)	(4)	(5)	(6)
	Consumption			Savings		
		Priority sector (likelihood)			Priority sector (likelihood)	
	Total	High	Low	Total	High	Low
Panel A: Discrete exposure						
Post × High exposure	63.84	12.71	101.57	95.38	117.57	64.45
	(50.90)	(58.98)	(71.10)	(91.78)	(102.88)	(83.10)
Observations	50,645	21,511	29,124	50,645	21,511	29,124
Panel B: Continuous exposure						
Post × Exposure	-125.56	-174.14	-423.17	1683.75**	1784.48***	1570.95
	(592.00)	(484.61)	(1762.86)	(797.81)	(587.39)	(2358.66)
Observations	50,645	21,511	29,124	50,645	21,511	29,124
Average Effect	-17.96	-24.91	-60.53	240.85	255.25	224.71
Mean (Baseline)	2153.19	1793.02	2394.93	436.82	373.58	477.89

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

Note: The table reports estimates of the effects of lending quotas on per-capita consumption and savings using the specification in equation (3) by the likelihood of working in the priority sector. Panel A reports outcomes using a discrete measure of exposure and Panel B reports outcomes using a continuous measure of exposure. The outcomes in columns (1) and (4) report the effects on consumption and savings respectively using one observation per household. We classify households by their likelihood of engaging in the priority sector as follows. First, we use a random forest algorithm using time-invariant individual and municipality characteristics to classify individuals as employed in the priority sector. We set the pre-policy period as the training sample and then predict the probability using the trained model for all the observations including the post-policy years. We then take an average of the predicted probabilities within each household. Finally, we classify households as being of high and low likelihood of working in the priority sector based on whether their predicted probability was above or below the median. All outcomes were deflated using a consumer price index with 2016 as baseline year (2016 = 100) and were winsorized at the 99th percentile. In Panel B, the average treatment effect is computed by multiplying the average exposure (0.14) by the corresponding point estimate. Standard errors are clustered at the municipal level and reported in parentheses.

Table A18—: Effects on Self-employment Income by Type of Sector and Partner's Occupation Sector - Continuous exposure

	(1) Total	(2) Non-priority	(3) Priority
Panel A: Partner works in a priority sector			
Post × Exposure	1032.00	1096.23**	-64.23
	(671.94)	(430.88)	(417.63)
Observations	23,642	23,642	23,642
Average Effect	147.62	156.81	-9.19
Mean (Baseline)	832.00	334.57	497.43
P-value Diff. (2)-(3)			0.025
Panel B: Partner works in a Non-priority sector			
Post × Exposure	604.04	342.29	257.80
	(1229.46)	(1079.47)	(569.28)
Observations	32,035	32,035	32,035
Average Effect	86.40	48.96	36.88
Mean (Baseline)	1200.24	937.90	260.89
P-value Diff. (2)-(3)			0.944
P-value Diff. Panel A-B (2)			0.458
P-value Diff. Panel A-B (3)			0.594

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

Note: The table the effects of lending quotas on self-employment income using the specification in equation (3) for individuals that cohabit with their spouses or partners, using our continuous measure of exposure. Panel A reports results for individuals whose partner works in the priority sector. Panel B reports results for individuals whose partner works in the non-priority sector. The table reports p-values corresponding to a test of differences between columns 2 and 3 within each panel and to a test of differences across panels within each column. The average treatment effect is computed by multiplying the average exposure (0.14) by the corresponding point estimate. Standard errors are clustered at the municipal level and reported in parentheses.

Table A19—: Effects on Income by Economic Sectors - Alternative Specifications (Locality-level design)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Priority				Non-priority			
		Income				Income		
	Prob.	Total	Self emp.	Salaried	Prob.	Total	Self emp.	Salaried
Panel A: Excluding the three largest urban centers								
Post × High exposure	-0.01	48.50	22.12	26.38	0.01	33.38	70.23	-36.85
	(0.02)	(38.27)	(26.26)	(31.21)	(0.02)	(69.75)	(47.55)	(33.24)
Observations	82,286	82,286	82,286	82,286	82,286	82,286	82,286	82,286
Panel B: Without sample weights								
Post × High exposure	-0.02	11.86	2.41	9.45	0.02**	144.76**	77.17**	67.59
	(0.01)	(41.34)	(27.45)	(21.58)	(0.01)	(62.59)	(34.92)	(41.88)
Observations	134,734	134,734	134,734	134,734	134,734	134,734	134,734	134,734
Panel C: Including Post × Pre-policy Avg. interest rate								
Post × High exposure	-0.01	34.84	17.88	16.96	0.02**	132.07**	69.38**	62.69*
	(0.01)	(30.81)	(22.89)	(18.76)	(0.01)	(53.14)	(33.46)	(33.08)
Observations	129,687	129,687	129,687	129,687	129,687	129,687	129,687	129,687
Panel D: Including municipalities with no branches								
Post × High exposure	-0.01	32.16	10.25	21.91	0.02*	132.85**	79.41**	53.44
	(0.01)	(36.89)	(23.27)	(21.81)	(0.01)	(52.04)	(30.78)	(35.64)
Observations	152,220	152,220	152,220	152,220	152,220	152,220	152,220	152,220
Panel E: Winsorizing at the 95th percentile								
Post × High exposure		24.65	15.16	9.50		104.46**	51.59**	52.87*
		(30.34)	(19.80)	(18.53)		(40.54)	(21.59)	(31.65)
Observations		134,734	134,734	134,734		134,734	134,734	134,734

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

Note: The table reports results from alternative specifications to those reported in Table 6, using our discrete measure of exposure. Panel A excludes observations from the three largest urban centers (by population); La Paz, Cochabamba, and Santa Cruz. Panel B excludes sample weights from the estimations. Panel C controls for the interaction between the post-policy period and the average interest rate in the pre-policy year (2013). Panel D includes municipalities without physical bank branches, assigning them an exposure value ($High\ exposure_m$) of zero. Panel E winsorizes continuous outcomes with respect to the 95th percentile. Standard errors are clustered at the municipality level and reported in parentheses.

Table A20—: Effects on Self-employment Income by Type of Sector and Partner's Occupation Sector - Discrete exposure - Alternative Specifications (Locality-level design)

	(1)	(2)	(3)	(4)	(5)	(6)
	Partner works in the priority sector			Partner works in the Non-priority sector		
	Total	Non-priority	Priority	Total	Non-priority	Priority
Panel A: Excluding the three largest urban centers						
Post × High exposure	146.39**	135.07**	11.32	84.36	32.06	52.30
	(72.22)	(58.60)	(47.94)	(81.86)	(86.62)	(39.19)
Observations	17,920	17,920	17,920	18,147	18,147	18,147
Panel B: Without sample weights						
Post × High exposure	152.06**	148.25***	3.81	137.25	89.31	46.05
	(67.41)	(56.25)	(30.91)	(98.13)	(69.90)	(39.58)
Observations	23,642	23,642	23,642	32,035	32,035	32,035
Panel C: Including Post × Pre-policy Avg. interest rate						
Post × High exposure	164.61**	135.39**	29.23	125.77	78.44	46.03
	(66.19)	(54.72)	(35.97)	(91.14)	(74.81)	(31.87)
Observations	21,862	21,862	21,862	31,399	31,399	31,399
Panel D: Including municipalities with no branches						
Post × High exposure	159.51***	135.04***	24.47	120.66	75.63	44.66
	(59.13)	(49.19)	(32.23)	(93.24)	(70.70)	(40.75)
Observations	31,448	31,448	31,448	33,539	33,539	33,539
Panel E: Winsorizing at the 95th percentile						
Post × High exposure	120.28***	99.67***	20.61	98.92	54.90	43.04
	(41.75)	(32.41)	(28.78)	(70.78)	(50.40)	(30.27)
Observations	23,642	23,642	23,642	32,035	32,035	32,035

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

Note: The table reports results from alternative specifications to those reported in Table 7. Panel A excludes observations from the three largest urban centers (by population); La Paz, Cochabamba, and Santa Cruz. Panel B excludes sample weights from the estimations. Panel C controls for the interaction between the post-policy period and the average interest rate in the pre-policy year (2013). Panel D includes municipalities without physical bank branches, assigning them an exposure value ($High\ exposure_m$) of zero. Panel E winsorizes continuous outcomes with respect to the 95th percentile. Standard errors are clustered at the municipality level and reported in parentheses.

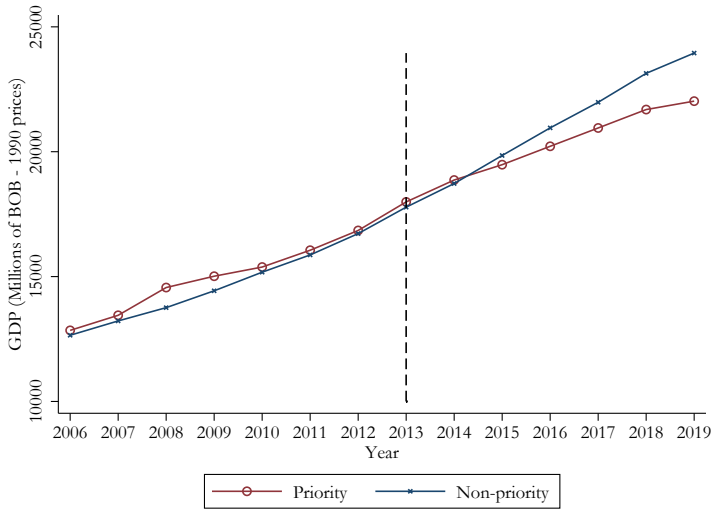


Figure A8. : Sectoral GDP over time

Note: The figure plots the evolution of sectoral GDP (1990 constant prices) of the priority and non-priority sectors, using national accounts data. The dotted black line marks the year preceding the policy change (2013).