The importance of digitalization in COVID-19 and Competitive Edge in Credit Markets

Abstract

Which banks did COVID-19 affect the most in credit markets: traditional or more digitalized? We answer this question by analyzing prices, marginal costs, and market power at the bank branch level using microdata from Brazil. We found no effect on prices. Marginal costs increased at branches in localities most affected by COVID-19, reducing market power. These branches experienced lending reductions and could not adjust costs in the short term. However, branches of more digitalized banks mitigated these effects due to their cost and lending flexibility: they could expand credit to other locations less affected by COVID-19, improving their market power.

Keywords: COVID-19, market power, digitalization, information technology, Lerner index.

JEL Classification: C58, D22, D40, G21, I19, O31.

1 Introduction

The COVID-19 pandemic affected the global economy, causing recessions, business failures, and increased unemployment. However, the impact was heterogeneous among economic agents, sectors, and countries (Muggenthaler et al., 2021).[1] Depending on banks' location and sectoral exposure, the pandemic may have also had heterogeneous effects on their costs, prices, and profit margins, ultimately altering their market power differently. We test this hypothesis using granular data and show that banks' market power changed in nontrivial ways during the COVID-19 outbreak. Market power has implications for the economy, affecting the cost and availability of credit and the effectiveness of monetary policy passthrough. This feature becomes even more important during a global economic downturn, such as the one caused by the COVID-19 crisis.

Innovation is one key factor influencing how COVID-19 could have affected market power.[2] Like other pandemics, COVID-19 transformed and accelerated certain trends,[3]including digitalization (Ceylan et al., 2020). Before the COVID-19 outbreak, financial systems were undergoing a heavy process of digitalization.[4] With the introduction of public health measures that discouraged person-to-person contacts, this process accelerated in both the financial and real sectors (OECD, 2020). Aghion et al. (2005) show that competition encourages leaders to invest in innovation while discouraging laggard firms from doing the same. These changes may have widened the gap between leading banks and followers, increasing the participation and market power of banks that were better prepared to face the adverse effects of the pandemic. This financial digitalization could alleviate cost pressures from the pandemic shock, especially through online banking services that were not sensitive to social distancing measures. Therefore, more digitalized financial institutions

[1]For instance, the entertainment, restaurants, and tourism sectors, which require face-to-face interaction, suffered severe losses. In contrast, information, communication, and delivery experienced substantial growth (Rio-Chanona et al., 2020). The economic consequences of COVID-19 also varied significantly between countries, depending on their economy's pre-pandemic conditions, the extent of public containment measures, and the quality of institutional settings (Muggenthaler et al., 2021).

[2]For instance, retail giants with platforms and digital technologies expanded their activities during the first year of the pandemic while numerous small offline firms experienced huge losses or went bankrupt (Bloom et al., 2021).

[3]We highlight the following structural changes in previous pandemics. The Black Death destroyed a large portion of the world's workforce and resources, contributing to the shift from labor-based to capital-based production and significantly increasing rural-urban migration (Clark, 2016). As of 2014, the Spanish flu was the fourth largest economic shock to income and consumption after WWII, WWI, and the Great Depression (Barro and Ursúa, 2008).

[4]Philippon (2015) examines data up to the global financial crisis in 2008 and finds an interesting puzzle. Despite significant advances and investments in computer and communications technologies, the unit cost of financial intermediation has remained close to 200 basis points for over a century. Philippon (2020) reruns the model with data after the global financial crisis and finds that the unit cost of financial intermediation declined from 2010 to 2020. He attributes the structural change to the rapid growth of fintechs, which have benefited from digital innovations that can disrupt industry structures.

would be more resilient to the effects of the pandemic. Our paper adds to this literature by investigating whether more digitalized banks improved their positioning in terms of market power relative to traditional banks during the pandemic.

We address these empirical questions by employing a difference-in-differences approach with several matched datasets from Brazil, which has interesting features that allow us to assess the pandemic's impact on banks' market power. First, Brazil is a strongly bank-oriented economy: SMEs mostly rely on bank credit for external funding, and credit for households is primarily through the banking system. Second, bank credit in Brazil increased substantially during COVID-19 due to government programs designed to combat the economic effects of the pandemic. Despite pervasive across all Brazilian regions, credit growth rates were heterogeneous across localities in 2020. Third, Brazil has a considerable variety of economic development, climate, and demographics across localities, leading to very distinct settings of market power in local credit markets. Fourth, during COVID-19, the federal government took the majority of economic measures to combat the pandemic, such as providing financial support to families and small businesses, deferring loan payments, modifying banking system requirements to increase credit capacity, and easing monetary policy. The diversity of characteristics of the localities and the different ways the pandemic affected them led to a great diversity of local economic developments, even in the context of a similar set of government measures. This diversity provides the conditions for examining COVID-19's impact on local credit market power. In addition, Brazil has rich datasets that make these analyses feasible.

Our identification exploits the fact that localities in Brazil faced substantially different levels of COVID-19 severity. Brazil experienced a spread pattern of COVID-19 similar to the United States.[5] First, the virus hit heavily state capitals, which are the most populated areas and host all core airports, before spreading significantly to inland municipalities. Similar to Levine et al. (2021), we use this cross-sectional variation to identify local COVID-19 shocks. We measure the *local COVID-19 intensity* as the number of COVID-19 cases as a share of the local population.[6] In terms of

[5]The country's dimensions and the initial COVID-19 occurrence are critical factors in determining the COVID-19 spreading pattern. For instance, China was the initial COVID-19 epicenter, spreading the virus primarily from the Hubei province to its closest neighbors. Therefore, the COVID-19 spread correlated exclusively with the geographic distance from Hubei (Kang et al., 2020). In Italy, COVID-19 contagion was not uniform. Following the outbreak in the province of Lodi (northwestern region of the country), health authorities registered cases in three additional northern regions: Lombardy, Emilia-Romagna, and Veneto, shortly after (Giuliani et al., 2020). In countries with continental dimensions, the virus targeted larger urban areas first—more populated and closer to core airports—before moving on to inland municipalities (Paul et al., 2020; Wang et al., 2020).

[6]A natural measure to assess the COVID-19 spread is mobility. However, the available databases for Brazil either have a higher level of aggregation than is required for analyzing the local credit markets, or they do not provide broad geographic coverage. The Google (Community Mobility Reports) is only available at the state level, the São Paulo

identification, one potential concern is that the timing and intensity of local COVID-19 are likely not exogenous to localities' economic and geographic conditions, such as population, economic development, distance to state capitals, and economic structure. The local economy's structure may affect COVID-19 transmission rates as agriculture-related activities are geographically sparser than services and industrial activities. We show that once we compare localities within the same macrolocality and with similar wealth levels, the local COVID-19 spread becomes unrelated to many locality-specific correlates. This fairly exogenous variation of local COVID-19 intensity across localities within the same macrolocality and with similar wealth levels is essential to support the causal interpretation of the results.[7]

In this setup, we also require a measure of banks' market power within a locality, enabling us to capture a geographical component in our estimates. The Lerner index and Hirschman-Herfindahl Index (HHI) have this feature and are among the literature's most widely used market power measures. The scarcity of microdata encourages using concentration indicators, such as the HHI, which are less data-intensive. In contrast, they are more disputed (Shaffer and Spierdijk, 2020), while the Lerner index is the standard measure of market power among economists (Blair and Sokol, 2014). The Lerner index is also bank-specific, while the HHI is evaluated at the market level. Therefore, in this paper, we adopt the Lerner index as a measure of market power and a proxy for competition. The main advantage of the Lerner index is that it tells us about the channels through which market power changes: either through the *effective price channel* or the *marginal cost channel*. All else equal, increases in the first (second) lead to higher (lower) market power. This interpretability is important to rationalize our results and understand the mechanisms through which COVID-19 can affect market power.

Since we aim to measure a bank's market power within a locality, we define a *local market* as a set of "local banks" in a delimited geographical locality, lending in a specific modality. The term "locality" refers to the physical location of the bank branch that extends the credit. Borrowers can be located anywhere. This approach accommodates bank branches' lending both to local and remote borrowers, which is important given the increasing bank digitalization and adoption of online

Government Intelligent Monitoring System includes data only for 104 cities in the state of São Paulo with more than 70 thousand inhabitants, and the BID/WAZE has data for large cities only.

[7]This empirical strategy also mitigates several non-observable and region-specific concerns. For instance, the under-notification of COVID-19 cases was a serious concern at the beginning of the outbreak (Cintra and Fontinele, 2020). By comparing adjacent localities with similar wealth levels, local health institutions and authorities are likely to be more similar, and we should not expect systematic differences in the under-notification levels across localities.

banking. A "local bank" is the sum of all branches of a specific bank that operate in the locality (the representative bank branch in the locality). Our definition of local markets is sensitive to the credit modality because these have unique prices and marginal costs, leading to distinct market power levels. Since the traditional Lerner index is evaluated at the bank level, we use a Lerner index calculated in a decentralized way, enabling us to evaluate market power for each local bank and credit modality.[8]

We resort to a within-bank and across-locality empirical strategy to analyze how COVID-19 affected market power and its components in local Brazilian credit markets. This strategy enables us to isolate bank-specific temporal changes in credit supply while allowing variations in COVID-19 intensity across localities. Our variation comes from the *same* bank operating in *different* but *similar* localities experiencing *distinct* COVID-19 intensity levels. To further alleviate any issues with differences in the bank's credit composition portfolio in different localities, we also compare the same bank operating in the *same* credit modality market across similar localities. In this empirical setup, we can view the COVID-19 shock as a *local demand shifter*, as broad credit supply is controlled for in a within-bank analysis. Another empirical challenge is the existence of numerous concurrent confounding variables during the pandemic, such as the introduction of government programs designed to combat the economic effects of COVID-19. Most of these measures can influence credit-taking decisions. We also account for these issues by introducing controls. Our analysis focuses on the pandemic's first year to allow more precise identification of the effect of COVID-19 on banks. In 2020, it was not clear the scale of the crisis and economic agents had not yet adjusted to the new economic conditions.

We find that COVID-19 did not impact effective prices in local credit markets. However, we find substantial changes in the effective price components: credit income and granted credit declined significantly in localities more affected by COVID-19 during 2020. Therefore, the significant decrease in credit income was offset by a corresponding reduction in credit concessions in the locality, resulting in unchanged effective prices. Using data on firm income from credit and debit card transactions, invoices, wire transfers, and exports, we show that localities with higher COVID-19 intensity had lower local economic activity than similar localities with lower levels of COVID-19 prevalence. This decreased economic activity rationalizes the decrease in credit income and concessions in

[8] As a robustness test, we also used the HHI and less saturated models to analyze the changes in banks' market power resulting from the pandemic.

localities more affected by COVID-19.

In contrast, we find that COVID-19 significantly affected marginal costs. A one-standard-deviation increase in the local COVID-19 prevalence (4%) increased banks' marginal costs by 0.5 cents during the first year of the pandemic. This value is expressive as the marginal cost's sample mean is 4 cents. We show that the increase in marginal costs occurred because bank branches could adjust their total costs in the short term due to the stickiness of cost factors arising from economic rigidities and legal and financial frictions. Combining these findings, we find evidence that the COVID-19 pandemic reduced the market power of Brazilian banks mainly through the marginal cost channel.

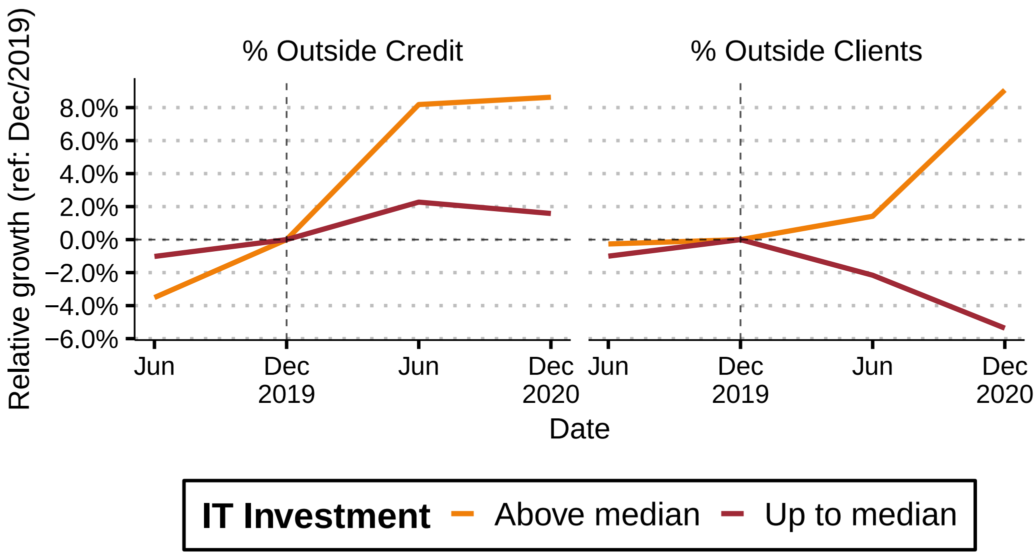


Figure 1. *Relative growth rate (reference: December 2019) of the volume of credit concessions (left) and clients (right) residing in areas outside the bank branch's locality in the aftermath of the COVID-19 outbreak (2019–2020) for banks with high IT spending (above the sample median) and low IT spending (up to the sample median) before the COVID-19 crisis.*

The COVID-19 pandemic highlighted the importance of IT development in the banking sector. In this paper, we proxy digitalization as the bank-specific IT spending as a share of its total costs in the pre-pandemic period. We find that more digitalized banks enjoyed advantages over traditional banks during the pandemic. First, we show that bank digitalization provided better cost flexibility. More digitalized banks experienced fewer frictions in adjusting their cost structure in the short term, especially funding costs. Digitalization also provided higher lending flexibility. Precisely banks that spent more on IT before COVID-19 are likely to have more developed and trustworthy online banking systems, enabling these remote transactions to a more considerable extent. Figure 1 displays the growth rate (relative to December 2019) of the share of credit concessions and distinct clients external to the bank branch's locality for more (orange curve) and less (red curve) digitalized banks. After the COVID-19 outbreak, more digitalized banks expanded credit and engaged with new clients living outside the bank branches' physical locality. We corroborate this raw evidence with an econometric exercise. We find that more digitalized banks complemented the decrease in credit concessions in localities more affected by COVID-19 by extending credit to borrowers in remote localities less affected by COVID-19. Given the same level of COVID-19 intensity, bank branches expanded more in remote localities that were less wealthy, more populated, and where the bank branch had higher market power, all relative to the bank branch's locality.

We also show that digitalization was particularly important for credit modalities that were not traditionally settled over online systems, requiring more advanced IT systems. Digitalization did not play a role in credit modalities related to pre-approved lines, which were already largely automated among banks before the pandemic. Our results highlight the role of digitalization in allowing bank branches to be less sensitive to local borrowers' conditions. Consequently, more digitalized bank branches mitigated the increase in marginal costs in localities more affected by COVID-19, improving their market power over traditional banks.

It is worth noting that, while IT can warrant market power during the COVID-19 pandemic, it may be a double-edged sword for financial stability. On the one hand, IT allows banks to rearrange their operations to accommodate their sticky cost factors. This feature increases efficiency from which clients can, in principle, benefit, yields higher market power, and leverages bank profitability. This feature is beneficial for financial stability. On the other hand, banks over-reliant on IT are likely to overemphasize hard information to the detriment of soft information because of the standardization of IT systems. This is especially important in lending to remote borrowers. Local competence becomes secondary in highly standardized banking processes, eliminating the soft information component from lending decisions. Since soft information becomes critically important in times of distress (D'Aurizio et al., 2015), this feature may undermine financial stability.

Our paper connects to the empirical literature examining the impact of COVID-19 on banks. Regarding financial stability, Duan et al. (2021) analyze whether and how the COVID-19 crisis affects systemic risk in a cross-country setting. They also assess the banking sector's resilience and the role of bank features in alleviating the pandemic's effects. They find that systemic risk increases during the health crisis. However, deposit insurance and foreign and public banks help mitigate this risk. Igan et al. (2022) analyze the effectiveness of macroprudential policies implemented before the COVID-19 outbreak in mitigating bank risk during the pandemic crisis. They find that macroprudential policy strengthened bank resilience as assessed by stock market investors. Levine et al. (2021) examine the deposit market at the bank branch level during the pandemic using cross-county variation in the US. They find that higher local COVID-19 infection rates are associated with increased local bank deposits and lower local deposit interest rates, mainly due to customers' precautionary behavior. Özlem Dursun-de Neef and Schandlbauer (2021) examine the lending responses of European banks to the COVID-19 outbreak. They find that higher exposure to COVID-19 leads to a relative increase in lending by poorly capitalized banks. Better-capitalized peers reduced their lending and experienced an increase in delinquent and restructured loans.

We believe this is the first study on how COVID-19 affected market power using comprehensive data for an important emerging market country. Dadoukis et al. (2021) examine the effects of pre-pandemic IT investments on banks' performance during the COVID-19 crisis. They find that higher pre-pandemic IT spending is associated with better performance during the first quarter of 2020 in terms of stock price, credit supply, and credit renegotiation. Our results complement these findings by documenting that IT spending was crucial in securing higher market power in local credit markets.

2 Data

Neighboring Brazilian municipalities have strong social, economic, and financial interconnections. These interconnections form urban networks, typically with a large urban center and small towns surrounding it. We believe these urban networks are more suitable than municipal boundaries for measuring market power in local credit markets. Based on this concept of urban networks, the Brazilian Institute of Geography and Statistics (IBGE) defines the geographical unit known as the Immediate Geographic Region. We refer to it as *locality* in this paper. Branches of the same bank within this locality are aggregated into a representative bank branch (the local bank). In 2021, IBGE grouped the 5,570 Brazilian municipalities into 510 immediate geographic regions. Our sample comprises 508 localities, as there are no bank branches in two of them.

We also require a time frame to estimate a bank's market power, which is determined by analyzing credit operations that originated during that period. Too limited periods may lead to unreliable estimates, while broad periods may not capture current competition accurately. We select a semi-annual period based on this trade-off and the available data. Therefore, our baseline analysis goes from 2019 to 2020 semiannually. We focus on the first year of the pandemic as it is more informative from an empirical point of view. At the beginning of the pandemic, economic agents could not clearly anticipate the scale of the crisis and did not have enough time to adapt. These characteristics make identifying the impact of the first year of COVID-19 clearer from an empirical point of view.

We compile and merge many public and proprietary datasets to run our empirical specifications. We extract accounting banking data from the Accounting Plan of the Institutions of the National Financial System (Cosif). We collect information about credit operations from the Credit Information System (SCR). Cosif and SCR are proprietary datasets maintained by the Central Bank of Brazil (BCB). While Cosif has aggregated accounting bank information, SCR allows us to identify the branch bank creditor, the credit borrower, and credit conditions, such as the amount granted, interest rates, credit maturity, and credit modality. To combine the data from these two datasets, we employ two additional proprietary datasets: Information on Entities of Interest to the Central Bank (Unicad) and the Brazilian Federal Revenue Service (RFB). The Unicad, maintained by the BCB, has information about the bank's identification, ownership, and prudential segment. The RFB dataset comprises meta-information about individuals and firms. Additionally to the locality *per se*, we extract from the IBGE information regarding the locality's wealth (*per capita* GDP) and geographical data. These five datasets allow us to connect information about bank branches, individuals, firms, credit operations, and credit market measures.

We extract bank branches' market power measures from Silva et al. (2022). The evaluation of market power in a decentralized way is data-intensive. In addition to the previous datasets, it requires the following datasets: Monthly Banking Statistics by Municipality (ESTBAN), a public dataset maintained by the BCB, and matched formal employment relationships from the Annual List of Social Information (RAIS) and the General Register of Employed and Unemployed (Caged), both proprietary data maintained by the Ministry of Economy. Although we do not calculate the market power measures in this paper, we mention these datasets in Section 3, where we briefly explain the market power methodology.

We also compile COVID-19 epidemiological bulletins from the Ministry of Health (public data) to construct the locality-specific COVID-19 intensity. These bulletins contain the number of COVID-19 cases per municipality daily. Finally, we extract cash transfers received for each beneficiary and their location from the Ministry of Economy's Emergency Aid Beneficiaries database (public data).

The federal government transferred this amount to combat the economic impact of the pandemic on individuals.

3 Measuring market power

We measure market power in a decentralized way using a data-intensive version of the Lerner index. This methodology assesses each bank branch's degree of market power in each locality and credit modality. We should note that it is important to have locality-specific measures of bank market power to identify COVID-19 shocks across credit markets in different localities.

The term "locality" refers to the physical location of the bank branch that extends the credit. Borrowers can be anywhere. This approach is coherent with production and cost functions because costs are allocated to the branch that originated the credit, not branches in the borrower's locality. "Local banks" refer to the sum of all branches of a specific bank operating in the locality (the representative bank branch in the locality).[9] In addition, the bank branch's location perspective enables us to capture electronic transactions originating from a particular bank branch regardless of the borrower's location. Such a feature is important because online banking operations have increased substantially, as shown in Figure 2.

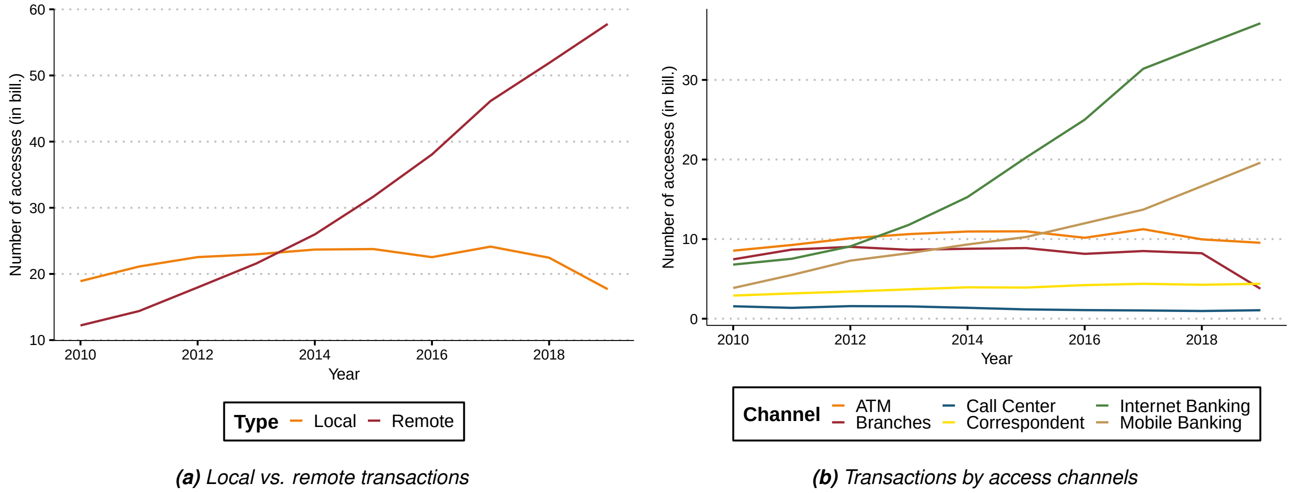


Figure 2. *Number of transactions in Brazil from 2010 to 2019. (a) Local vs. remote transactions. (b) Transactions for each access channel (ATM, bank branch, call center, banking correspondents, internet banking, mobile banking). Transactions include invoice payment, deposits, transfers, loans, withdraw, financial statement queries, other financial and non-financial transactions. Data is public and comes from the Central Bank of Brazil (access here > Financial Inclusion > Relationship with the NFS > Transaction > ["Number of transactions by type" (a) and "Type of transactions per access channel" (b)].*

[9] The term "local bank" refers to a bank's physical presence in a specific locality. It does not relate to the scope of its business. For instance, universal banks that operate nationwide and internationally are considered local banks in each locality, where they maintain at least one branch.

Unlike structural measures, the Lerner index enables us to understand the channels through which market power is changing: either through price or marginal costs. Theoretical and computational details of the Lerner index in local credit markets can be found in Silva et al. (2022). Here, we streamline the main rationale of the method and the outputs from this methodology that will be critical for our empirical strategy in the following sections. Mathematically, we evaluate the Lerner index $L_{blt}^{(m)}$ for bank b in locality l at time t for the credit modality m as follows:

$$L_{blt}^{(m)} = \frac{p_{blt}^{(m)} - MC_{blt}^{(m)}}{p_{blt}^{(m)}}, \qquad p_{blt}^{(m)} = \frac{\text{Credit Income}_{blt}^{(m)}}{\text{Credit Concessions}_{blt}^{(m)}}, \tag{1}$$

in which $p_{blt}^{(m)}$ and $MC_{blt}^{(m)}$ are the bank b's effective price and marginal cost at locality l during time t (semiannually) for product m. Effective prices include the contractual rate, taxes, and other fees and are net of losses. We evaluate effective prices by dividing the credit income in time t by the average outstanding credit among the concessions in t, for a specific credit modality m and a bank b in a specific locality. We estimate the marginal costs using a translog function that models total costs at the bank branch level rather than the usual bank level, enabling us to estimate marginal costs for each bank branch.

One of the main empirical challenges in estimating total costs at the bank branch level is the lack of data. The Central Bank of Brazil has detailed consolidated financial statements of banks at the national level but not at the bank branch level. We resort to a strategy to distribute these aggregate cost factors across branches of the same bank, as depicted in Figure 3. In the translog function, banks have four types of costs: funding, tax, labor, and other administrative costs. Our strategy is to distribute these bank-level aggregates across branches by combining Cosif (national bank-level data) with other datasets that include geographic information, such as the SCR, ESTBAN, and RAIS. Our allocation strategies are cost-specific. For example, we use RAIS, which contains detailed payroll and the number of employees of each branch, to distribute national bank-level labor costs to each branch proportionally to its local workforce size and salary.

The literature typically uses credit, bonds and securities, and other assets as outputs of the translog function. One significant distinction in our approach is with the credit product. We consider each credit modality as an individual product instead of combining all credit into a single product. We believe this segregation is important because credit modalities serve different financial inter-

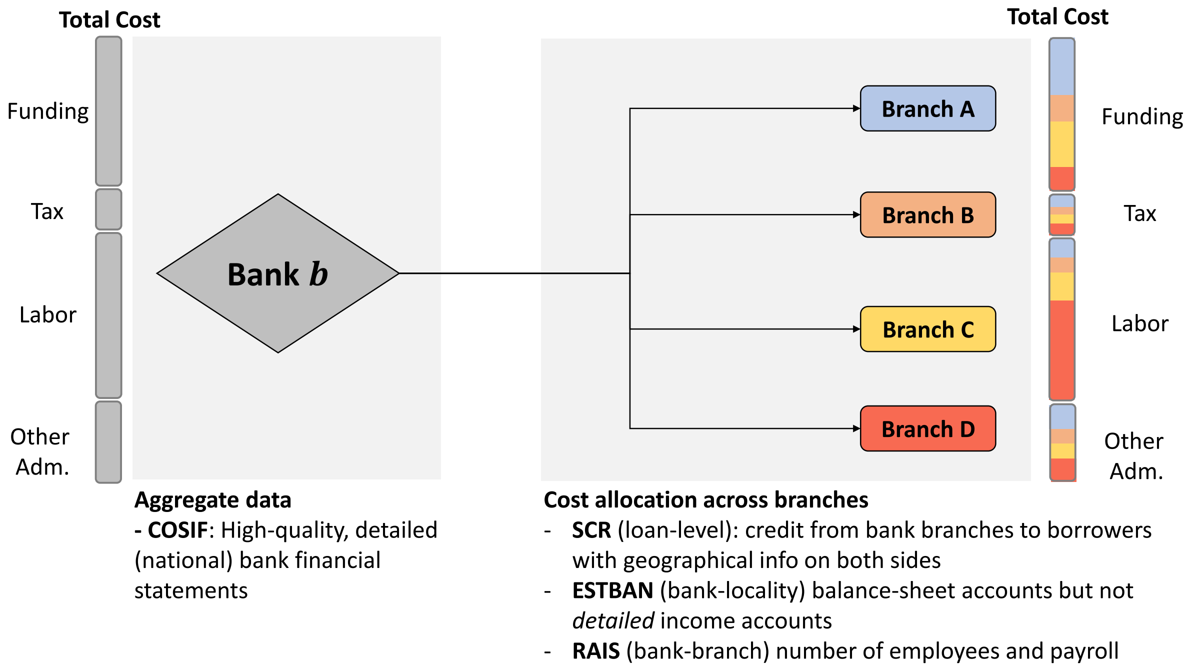


Figure 3. *Schematic of the allocation of costs factors for the same bank across its branches in different localities. We have aggregated national-level information on funding, tax, labor, and other administrative costs. We allocate these costs across bank branches in different localities of the same bank, such that the sum of all bank branches' costs matches the national bank-level data. For that, we combine Cosif, which contains detailed bank-level cost components, with other datasets incorporating geographic information, such as the SCR, ESTBAN, and RAIS.*

mediation purposes.[10] Additionally, we separate credit granted in the current half-year from prior half-years for each credit modality. The objective is to separate older credit concessions from newer ones in the total cost function, enabling us to estimate their marginal costs separately. We then focus on credit operations within the current half-year when evaluating effective prices and marginal costs of a particular credit modality for each bank in each locality. This approach allows us to measure the current local market's competitive conditions more accurately.

Each bank branch in our approach produces a total of 30 banking products in total: (i) 14 credit products (modalities) granted within the half-year of analysis; (ii) 14 credit products corresponding to those granted before the half-year; (iii) 1 for operations with bonds and securities; and (iv) 1 for operations with other assets. Six credit modalities are available to individuals: payroll-deducted personal credit, non-payroll-deducted personal credit, real estate financing, rural credit, vehicle financing, and other credit. Credit modalities for non-financial firms (8 modalities) encompass working capital, revolving working capital, infrastructure financing, real estate financing, investment credit, account receivables, agribusiness, and other credit.[11]

[10]Credit modalities have different characteristics that influence price and cost, including collateralization (vehicle financing), non-collateralization (non-payroll-deducted personal credit), long-term maturity (real estate financing), short-term maturity (revolving working capital credit), and earmarked credit (infrastructure financing).

[11]We refer the reader to Silva et al. (2022) to get a sense of the variation of our estimates across localities and credit modalities.

4 Measuring local COVID-19 intensity

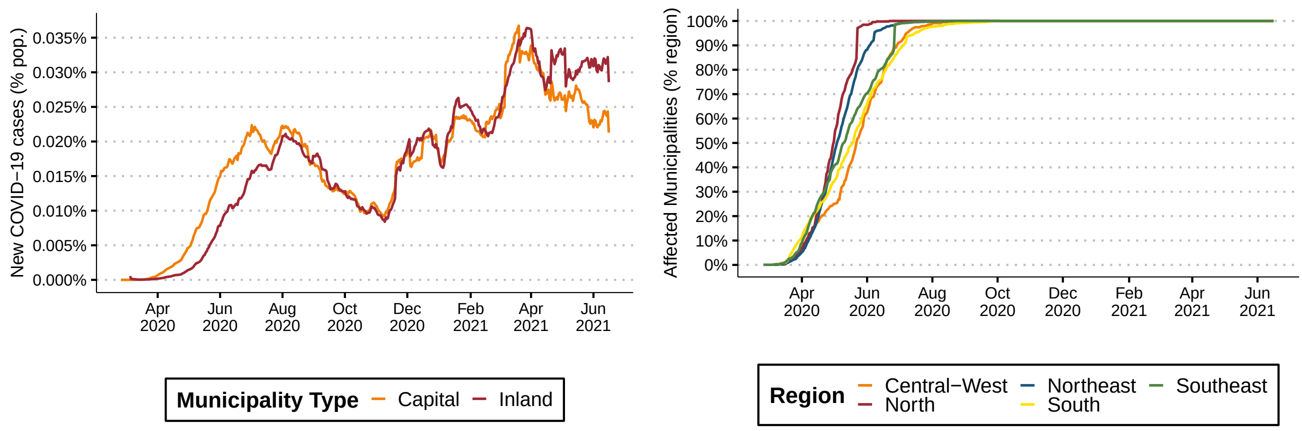
This section defines our measure of *local COVID-19 intensity*. We measure the local COVID-19 intensity with the locality's number of COVID-19 confirmed cases as a share of its population. We collected daily data on the number of COVID-19 cases per municipality in Brazil using COVID-19 epidemiological bulletins from all 27 State Health Departments from the first reported COVID-19 case in Brazil on February 25, 2020, São Paulo (SP), to June 16, 2021.[12] Each Brazilian State Health Department compiles local reports from municipalities within their geographical circumscription and submits them daily to the Federal Ministry of Health for consolidation. We end up with 2,238,003 municipality-time epidemiological bulletins.

Figure 4a shows the total number of new COVID-19 cases (incidence) as a share of the local population in capital (27) and inland (5,543) municipalities. There are two waves, with the first escalating from April 2020 to August 2020 and the second soaring from December 2020 to April 2021. In the first wave, there is an offset in the dynamics of capital and inland municipalities, mainly driven by the timing that COVID-19 hit these localities. In the second wave, both areas evolved similarly. This fact occurred because, by October 2020, every municipality in Brazil had registered at least one COVID-19 case, as Figure 4b reveals. Before that, inter-municipality contagion was a significant catalyst of COVID-19 cases in inland municipalities, with epidemic dynamics in neighboring or economically connected capitals serving as the primary driver. Following that, the primary factor was intra-municipality contagion.

Figure 5 displays the spatial COVID-19 prevalence (accumulated cases) in Brazilian municipalities as a share of the local population three, six, and ten months after the first case was reported in São Paulo in February 2020. At the end of three months, 4,255 (76.4%) municipalities had already registered at least one case of COVID-19, showing the spread was very quick. After six months, 5,558 (99.8%) municipalities had reported at least one COVID-19 case. There is a large variation in the share of the local affected population, even across adjacent municipalities.

To run our econometric exercises, we first aggregate the municipality-level number of COVID-19 cases to the locality level by summing all cases within the same locality. We then divide by the corresponding locality's population at the end of 2020 to obtain the share of the population affected

[12] This data is scattered around a large number of state government sites. The bulletins are generally not standardized across different states or even adjacent municipalities. We use the compiled dataset from Brasil.io for this task.



***(a)** Incidence of COVID-19 cases (% local population)*

***(b)** Share of municipalities with at least one COVID-19 case*

***Figure 4.** Evolution of COVID-19 across Brazilian municipalities from the first case (February 25, 2020) to June 16, 2021. (a) Number of new COVID-19 cases (incidence) as a share of the local population in capital and inland municipalities with at least one COVID-19 case. (b) Number of municipalities with local bulletins registering at least one COVID-19 case as a share of the total number of municipalities within the region.*

by COVID-19 for each month-year.[13] Finally, we take the average of this share over January to December 2020, obtaining our local COVID-19 Intensity$_l$, as follows:

$$\text{COVID-19 Intensity}_l = \frac{1}{12}\sum_t \frac{\text{COVID-19 Cases}_{lt}}{\text{Population}_l}, \tag{2}$$

in which t are months from January to December 2020, and l are localities. In terms of identification, one potential concern is that the timing and intensity of local COVID-19 intensity are likely not exogenous to localities' economic and geographic conditions, such as population, economic development, distance to state capitals, and economic structure. The local economy's structure may affect COVID-19 transmission rates as agriculture-related activities are geographically sparser than services and industrial activities. We run a cross-section regression to correlate the share of the population affected by COVID-19, our measure of local COVID-19 intensity in Equation (2), with the following *ex-ante* locality-level determinants (fixed with values in December 2019): distance to the locality's state capital, *per capita* GDP, population, and the preponderant activities (agriculture and industry). We include variables capturing the locality's demographics, which may impact the transmission of COVID-19 and the local economy. By examining the predominant local activity, we account for the industrial composition of the localities. During the pandemic, hospitality and tourism (services) industries were hit particularly hard. This empirical exercise is important to understand

[13]We aggregate from municipality to locality to make compatible the geographical units of the COVID-19 dataset and the locality-level variables on effective prices, marginal costs, and Lerner indices for each bank-modality.

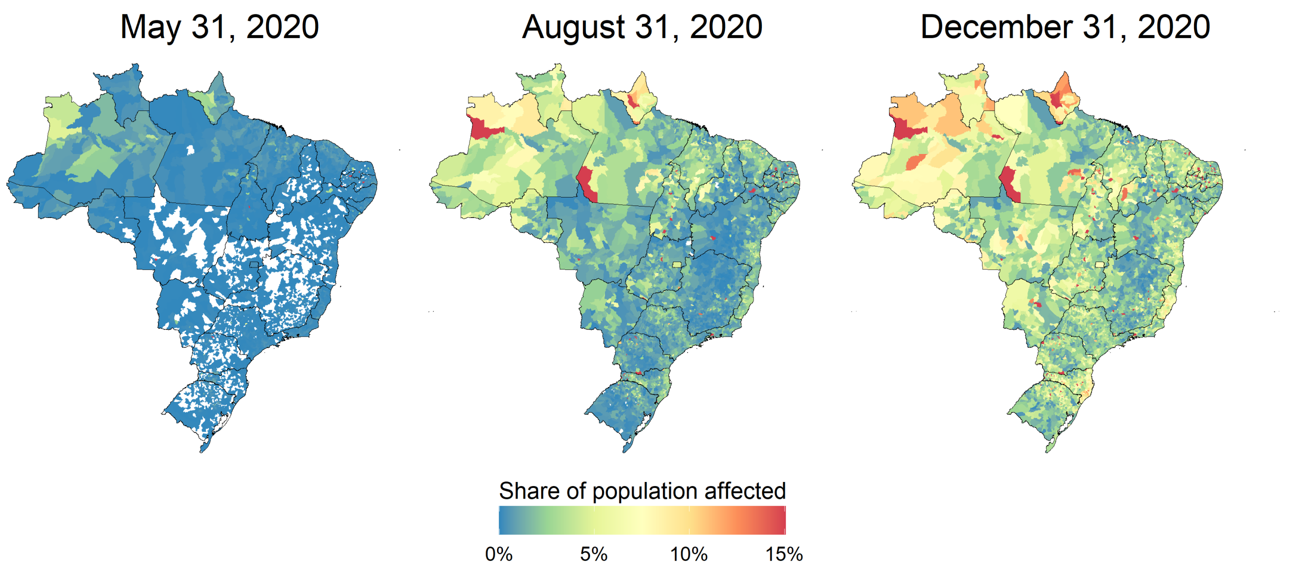


Figure 5. *Spatial COVID-19 prevalence in Brazilian municipalities. Total number of local COVID-19 cases as a share of the local population on May 31, 2020 (left); August 31, 2020 (center); December 31, 2020 (right). Colors from cold to warm represent increasing local COVID-19 prevalence. Blank localities represent places without occurrences of COVID-19 at that time. Shares were winsorized for better visualization.*

if observable locality-specific characteristics correlate with our local COVID-19 intensity. These systematic differences, if present, could drive our results and invalidate our causal interpretations.

Table I shows the results of the cross-section estimation. We compare across localities with increasing saturated specifications: localities all over the country (Spec. I), within the same region (Spec. II), state (Spec. III), macrolocality or Intermediate Geographical Region[14] (Spec. IV), and within the same macrolocality *and* localities of similar *per capita* GDP.

We use within-macrolocality comparisons of localities with similar *per capita* GDP in the following sections because our local COVID-19 intensity measure becomes unrelated to the observable locality-level characteristics (see Spec. IV in Table I). This empirical strategy also mitigates several non-observable macrolocality-level concerns. For instance, the under-notification of COVID-19 cases was a serious concern at the beginning of the outbreak (Cintra and Fontinele, 2020). By comparing adjacent localities with similar wealth levels, local health institutions and authorities are likely to be more similar, and we should not expect systematic differences in the under-notification

[14]The Intermediate Geographical Region encompasses contiguous and economically dependent Immediate Geographical Regions (our unity of locality in this paper). All municipalities within the same Intermediate Geographical Region belong to the same state. Therefore, we have the following geographical hierarchy in Brazil: municipalities (5,570 municipalities in 2021) < Immediate Geographical Region (510 units) < Intermediate Geographical Region (133 units) < state (27 states) < region (5 regions) < country (Brazil).

levels across localities. In addition, authorities intervened in the economy with several programs to mitigate the effects of the pandemic, such as temporary direct cash transfers for individuals, repayment postponement, and subsidized loans. It is more likely that nearby localities with similar *per capita* GDP will be exposed to these government programs more evenly.

TABLE I. *Correlates of the measure "COVID-19 Intensity$_l$," our proxy for local COVID-19 intensity*

Dependent Variable:	COVID-19 Intensity$_l$				
Model:	(I)	(II)	(III)	(IV)	(V)
Variables					
Distance to Capital$_l$	0.0399	-0.0315	-0.0722	0.1608	0.2112
	(0.0429)	(0.0469)	(0.0680)	(0.1559)	(0.2215)
Per Capita GDP$_l$	0.2296***	0.2599***	0.2498***	0.2377***	0.1338
	(0.0540)	(0.0587)	(0.0771)	(0.0812)	(0.0870)
Population$_l$	-0.1587***	-0.1239**	-0.0745**	-0.0365	-0.0476
	(0.0583)	(0.0492)	(0.0306)	(0.0445)	(0.0374)
Has Capital$_l$ (dummy)	0.8025***	0.5607***	0.3779*	0.0720	0.2681
	(0.2274)	(0.2077)	(0.2055)	(0.3800)	(0.2994)
Agriculture as Preponderant Activity$_l$ (dummy)	-0.3963***	-0.5405***	-0.5461***	-0.4735	-0.7117
	(0.1050)	(0.1191)	(0.1669)	(0.2942)	(0.4938)
Industry (Extractive and Manufacturing) as Preponderant Activity$_l$ (dummy)	-0.0357	-0.1071	-0.1648	-0.2432	-0.3220
	(0.1698)	(0.1696)	(0.1916)	(0.2335)	(0.3011)
(Intercept)	-0.0289				
	(0.0517)				
Fixed effects					
Region	—	Yes	—	—	—
State	—	—	Yes	—	—
Macrolocality	—	—	—	Yes	—
Macrolocality · *Per capita* GDP(3)	—	—	—	—	Yes
Fit statistics					
Observations	508	508	508	506	425
R^2	0.0643	0.0983	0.2506	0.3789	0.4613

Note: This table reports coefficient estimates of the cross-section regression COVID-19 Intensity$_l = \alpha_{g(l)} + \beta$Local Covariates$_l + \epsilon_l$, in which l is the locality. The dependent variable is the monthly average share of affected population by COVID-19 during 2020. We use the following local covariates (fixed with the last available information *ex-ante* to the pandemic values): distance to capital, *per capita* GDP, population, dummy variable that equals one if the locality contains the state capital, and dummy variables that equal one if agriculture or industry (extractive and manufacturing) activity is the local preponderant activity. The preponderant activity dummy is relative to localities where services (including construction) are the dominant activity. We follow Silva et al. (2021) and define the local preponderant activity as the one that contributes the most to local GDP. The term $\alpha_{g(l)}$ represents geographical fixed effects that permit us to perform comparisons across localities within the same geographical circumscription g: all over the country (Spec. I), within the same region (Spec. II), state (Spec. III), within the same macrolocality or Intermediate Geographical Region (Spec. IV), and within the same macrolocality *and* localities of similar *per capita GDP* (discretized in terciles). We standardize numerical variables. One-way (locality) standard errors in parentheses. *, **, *** denote statistical significance of 10%, 5%, and 1%, respectively.

5 Empirical results

This section presents the main empirical findings of the paper. We first examine how COVID-19 affected bank branches' market power through changes in the effective price and marginal cost channels. We then analyze the role of digitalization in market power during COVID-19. Table II reports the summary statistics of the dependent and independent variables used in the empirical specifications in this paper.

TABLE II. *Summary statistics of the dependent and independent variables employed in this paper.*

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
A. Dependent Variables (Variation: Bank-Modality-Locality-Time)								
Effective Price (%, semiannual rate)	79,256	13.319	12.043	0.363	5.293	9.007	17.049	68.241
Marginal Cost (R$ / +1 R$ credit)	79,256	0.043	0.272	−0.657	0.003	0.015	0.044	1.073
Lerner	79,256	0.466	1.489	−6.778	0.502	0.846	0.971	3.299
Credit Income (in mill. R$)	78,847	5.591	111.339	0.000	0.145	0.598	2.294	23,616.480
Granted Credit (in mill. R$)	79,256	15.896	9.665	0.000	0.224	1.239	5.533	17,231.310
Provision/Credit (%)	79,256	7.921	15.215	0.000	0.932	2.385	6.937	100.000
Contractual Price (%, annual rate)	78,783	51.023	70.063	0.000	13.567	24.155	54.533	1,060.101
Credit Market Share	79,195	16.789	20.003	0.000	2.652	10.108	22.536	100.000
B. Dependent Variables (Variation: Bank-Locality-Time)								
Local Cost (in bill. R$)								
Total	7,788	0.030	0.050	0.002	0.005	0.011	0.027	0.231
Funding	7,788	0.015	0.029	−0.0002	0.002	0.005	0.014	0.142
Tax	7,788	0.001	0.002	0.00003	0.0002	0.0004	0.001	0.010
Labor	7,788	0.009	0.016	0.001	0.002	0.003	0.007	0.080
Other Administrative	7,788	0.005	0.009	0.0001	0.001	0.002	0.004	0.044
% Credit Outside Locality	7,788	18.716	11.790	0.000	11.264	15.291	21.863	98.031
% Clients Outside Locality	7,788	16.908	8.293	7.633	11.444	14.435	19.539	47.043
C. Dependent Variables (Variation: Modality-Locality-Time)								
Credit HHI	43,164	0.261	0.221	0.017	0.087	0.185	0.377	0.811
D. Dependent Variables (Variation: Bank-Bank Locality-Borrower Locality-Time)								
Outside Granted credit (in mill.)	1,185,267	0.209	0.683	0.00002	0.002	0.013	0.079	4.567
Outside #Clients	1,185,267	78.570	3,251.422	1	1	2	6	1,488,752
E. Dependent Variables (Variation: Locality-Time)								
Firm Income (in mill. R$)								
Total	13,514	1,445.088	10,918.330	2.812	64.333	202.191	586.779	275,666.300
Credit and Debit Cards	13,514	254.653	2,151.801	0.405	15.487	38.962	108.770	60,210.090
Invoice	13,514	582.612	4,507.953	0.018	13.927	63.814	207.533	110,986.700
Exports	9,360	45.931	364.829	0.0003	0.519	3.064	14.032	12,770.330
Wire Transfers	13,514	575.757	4,097.172	1.086	25.475	83.001	249.648	102,043.900
Firms w/o Branches Income (in mill. R$)								
Total	13,514	412.327	2,522.329	0.930	32.535	84.329	236.356	65,044.750
Credit and Debit Cards	13,514	94.325	528.756	0.203	8.454	21.275	57.698	13,579.820
Invoice	13,514	138.314	907.829	0.003	6.315	24.303	70.798	24,080.300
Exports	6,193	2.317	9.956	0.00002	0.099	0.475	1.935	209.104
Wire Transfers	13,514	178.566	1,087.497	0.633	14.782	36.370	102.539	28,034.600
F. COVID-19 intensity measurement (Variation: Locality)								
COVID-19 intensity	509	4.887	3.963	0.514	2.732	4.034	5.932	40.898
G. Time-invariant, pre-pandemic covariate (Variation: Bank-Modality-Locality)								
Market Share (%)	25,095	15.374	20.070	0.000	2.255	8.808	19.491	100.000
Provisions / Total Credit (%)	26,689	5.863	13.687	0.000	0.000	1.185	4.643	100.000
Avg. Maturity (in months)	22,995	71.275	80.784	0.000	31.796	47.553	73.871	426.133
Avg. Local Ticket (in thous. R$)	22,995	2.138	2.424	0.000	0.954	1.427	2.216	12.784
H. Time-invariant, pre-pandemic covariate (Variation: Bank-Locality)								
Local Cost Factor (% Local Total Cost)								
Funding	1,983	48.007	11.496	0.561	41.162	47.812	55.813	88.867
Tax	1,983	3.691	1.399	0.006	2.723	3.756	4.520	10.132
Labor	1,983	32.901	12.873	1.779	24.600	32.067	39.170	99.411
Other Administrative	1,983	15.400	6.560	0.021	11.113	14.053	19.327	49.591
I. Time-invariant, pre-pandemic covariate (Variation: Bank)								
IT Cost (% total cost)	74	5.502	8.738	0.063	2.047	3.203	4.468	61.691
Bank Capitalization (capital / total assets, %)	74	17.620	16.012	2.568	9.946	13.675	18.052	93.925
Bank Liquidity (LCR)	74	4.288	8.403	0.308	1.469	2.314	3.309	67.308
J. Time-invariant, pre-pandemic covariate (Variation: Bank-Bank Locality-Borrower Locality)								
ΔCOVID-19 Intensity	1,185,267	0.005	0.876	−5.921	−0.227	−0.002	0.223	5.921
ΔLerner	1,185,267	−0.073	0.132	−0.933	−0.161	−0.075	0.013	0.723
ΔCredit Market Share (%)	1,185,267	−0.676	15.247	−93.537	−9.293	−0.631	7.979	93.537
Δ*Per capita* GDP (in thous.)	1,185,267	−1.591	23.076	−85.415	−15.572	−1.497	12.063	85.600
ΔPopulation (in mill.)	1,185,267	−0.010	3.108	−21.538	−0.339	−0.003	0.313	21.538
ΔLocality's IT Readiness	1,185,267	−0.988	1.951	−9.169	−2.314	−1.002	0.337	7.122
K. Time-invariant controls (Variation: Locality)								
Emergency Aid Volume / GDP (%)	509	14.947	10.628	2.159	6.345	10.944	22.569	49.586
SMEs (% total firms in locality)	509	64.523	4.980	46.271	61.212	64.732	67.698	80.403
Distance to Capital (in km)	508	402.950	207.590	49.936	251.691	364.315	509.751	1,485.773
Per Capita GDP (in thous. R$)	509	25.730	15.098	6.525	13.186	23.291	34.522	92.171
Population (in mill.)	509	0.410	1.219	0.033	0.120	0.192	0.327	21.571
Agriculture as Preponderant Activity	509	0.057	–	–	–	–	–	–
Industry as Preponderant Activity	509	0.067	–	–	–	–	–	–
Locality's IT Readiness	509	9.425	3.379	1.194	8.875	9.556	13.422	16.792

Note: Panels A–D: data ranges from January 2019 to December 2020, semiannually. Panel D: variables refer to the volume of credit and number of clients of a bank in a locality different from the bank branch's physical locality. Panel E: data ranges from January 2019 to June 2021, monthly. Panel F: "COVID-19 Intensity" is the average number of local infectious persons per month of 2020 divided by the local population at the end of 2020, as in Equation (2). Panels G and H: data is from December 2019. Panel I: Bank's IT spending as a share of its total cost from 2015 to 2019. Panel J: the difference is taken between the borrower's physical location and the bank branch's physical location. "Credit Market Share" is the bank's credit share with respect to each locality-level credit. "Locality's IT Readiness" is the average IT spending share relative to the total cost across banks in the locality, weighted by their credit volume. Panel K: The variable "Emergency Aid Volume / GDP" is the total volume of emergency aid received by residents during 2020 in a locality as a share of that locality's GDP. The variable "% SMEs" is the percentage of firms in a locality with up to three formal employees as of 2019. The terms *Per capita* GDP, the dummy variables "Agriculture as Preponderant Activity" and "Industry as Preponderant Activity" refer to 2018 (end-of-year, latest information available at the time of writing this paper). The remaining variables are from 2019.

5.1 COVID-19 and market power

There are two channels through which market power can be affected: (i) the effective price and (ii) the marginal cost channels. Figure 6 illustrates the empirical setup designed to estimate the effect of COVID-19 on bank branches' market power across localities. Below, we discuss the challenges and concerns that our empirical strategy addresses.

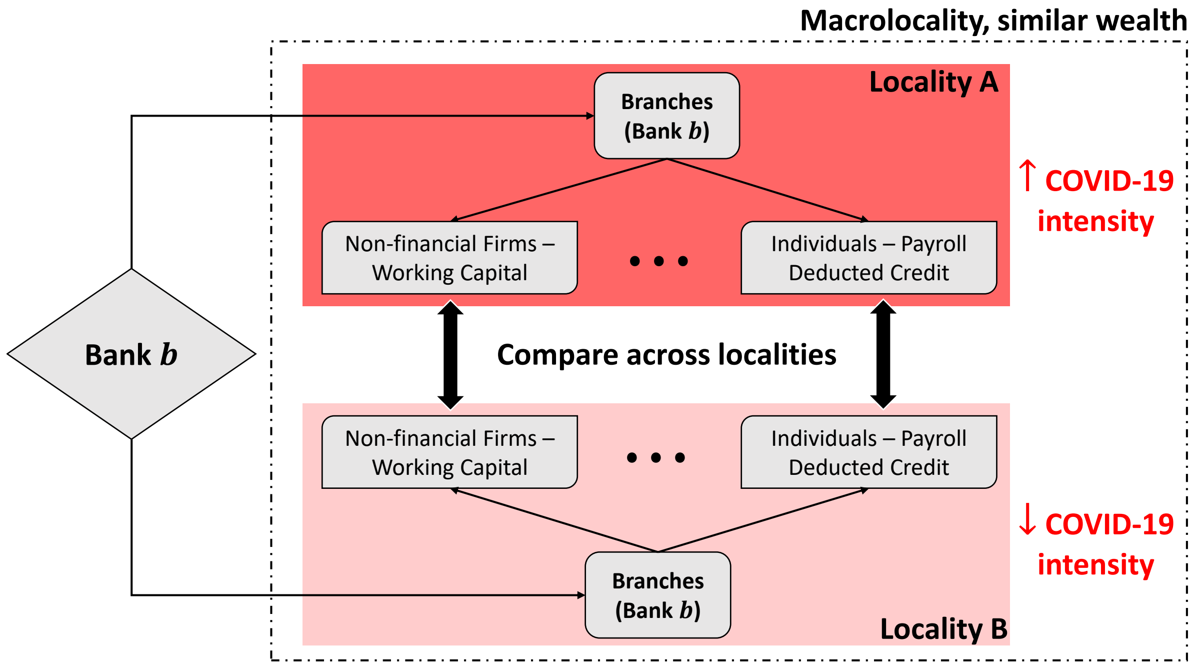


Figure 6. *Empirical setup: viewing COVID-19 as a local demand shock. Data is at the bank-locality-modality-time level.*

The first empirical challenge is in isolating bank-specific credit supply variations across time. Since our goal is to quantify the effect of COVID-19 on bank branches' market power, we need to control for changes in the bank's credit supply while allowing variations in locality-specific conditions. This locality-specific variation should be due only to differences in COVID-19 intensity if we desire to attribute our findings to the pandemic. To this end, we identify COVID-19 shocks across localities by differences in their local COVID-19 intensity (see Equation (2)). Empirically, we compare the *same* bank operating in *different* but *similar* localities experiencing *distinct* COVID-19 intensity levels (within-bank and across-locality approach). We compare across localities residing in the same macrolocality and with similar *per capita* GDP levels. As with this approach, differences in COVID-19 intensity across localities are likely to be exogenous, as discussed in the previous section, which is important to interpret our estimates causally. Additionally, localities that are part of the same macrolocality tend to be similar, mitigating concerns about potential omitted variables. Since credit modalities tend to have specific effective prices and marginal costs, we also compare within the *same* credit modality. This strategy also alleviates concerns about differences in a bank branch's credit composition portfolio across localities.[15] In this empirical setup, we can view the COVID-19 shock as a *local demand shock*, as broad credit supply is controlled for in a within-bank analysis.

Another empirical challenge in identifying the effect of COVID-19 on market power is the existence of numerous concurrent confounders during the pandemic, such as the introduction of government programs designed to combat the economic effects of COVID-19 on households, firms, and banks. Most of these measures can influence the decision of credit-taking. We carry out the following treatments for recipients of government policies during the COVID-19 outbreak:

1. **Households:** received financial support through direct cash transfers and incentives for credit renegotiation and restructuring.

   Treatment: introduce the control "emergency aid volume received by residents in a locality over the local GDP."

2. **Firms:** received financial assistance via incentives for banks to renegotiate and extend credit to the corporate sector, as well as special credit line programs for SMEs.

   Treatment: introduce the control "number of SMEs in each location."

3. **Banks:** experienced changes in the regulatory framework aimed at fostering credit concessions, such as reductions in reserve requirements and in the Countercyclical Capital Buffer.

   Treatment: since we compare branches of the same bank across different localities, changes are netted out (within-bank analysis).

4. **Macroeconomics:** changes in monetary and exchange policies

   Treatment: changes are also netted out due to the difference-in-differences analysis.

The following DiD specification operationalizes the empirical strategy outlined in Figure 6:[16]

[15] To illustrate the rationale underlying our identification strategy, we compare the same bank (e.g., Banco do Brasil) operating in two distinct but comparable localities within the same macrolocality and with similar *per capita* GDP (e.g., localities Limeira – SP and Rio Claro – SP, both in the Campinas – SP macrolocality) in a specific credit market (e.g., working capital for firms).

[16] As a general note, we include lower-order terms for all interactions introduced in empirical specifications. For example, if a covariate represents the interaction of three terms, then all pairs of these terms and the marginal terms will also be covariates in the regression (provided they are not collinear with fixed effects). To maintain clarity, we do not explicitly display all of these lower-order interactions in the written specifications nor show them in the regression tables. The only exception is when a particular lower-order term is a coefficient of interest. In any case, the notes in the regression tables are complete and will explicitly state each regression's specificities.

$$y_{b,m,l,t} = \alpha_{b,m,g(l),t} + \alpha_l + \beta \cdot \text{COVID-19}_t \cdot \text{COVID-19 Intensity}_l + \\ + \gamma^T \cdot \text{COVID-19}_t \cdot \text{Controls}_{b,m,l} + \epsilon_{b,m,l,t}, \tag{3}$$

in which b, m, l, t index the bank, credit modality, localities (508 Immediate Geographical Regions), and time (semiannually from 2019 to 2020). We examine the following dependent variables $y_{b,m,l,t}$: average effective price (credit income/granted credit), marginal cost, Lerner index, credit income, granted credit, provisions as a share of the outstanding credit, and contractual prices (interest rates), all of which are specific to each bank b and credit modality m at locality l during the semiannual period t. The dummy variable COVID-19$_t$ is a step variable that equals one when the year is 2020 and zero otherwise. The continuous treatment variable COVID-19 Intensity$_l$ follows Equation (2). The vector Controls$_{b,m,l}$ has two groups of variables. The first group controls for government policies introduced to combat the economic effects of COVID-19: the total emergency aid volume received by residents in the locality during 2020 as a share of the local GDP (contemporaneous variables, new policy in 2020) and the share of SMEs in the locality before COVID-19. The second group includes *ex-ante* bank-modality-locality controls (fixed with December 2019 values): local market share, credit provisions as a share of the total credit, average maturity, and average ticket.[17] We standardize all numeric variables. $\epsilon_{b,m,l,t}$ is the error term. Following Abadie et al. (2020), we cluster errors at the locality level, which coincides with the level of variation of our COVID-19 intensity measure.

The introduction of the time-varying fixed effects $\alpha_{b,m,g(l),t}$ enables us to interpret our estimates in terms of the *same* bank b operating in a set of similar localities $g(l)$—i.e., localities with similar wealth levels and in the same macrolocality—for the *same* credit modality m. We further introduce locality fixed effects α_l to absorb time-invariant and locality-specific non-observable factors.

Our coefficient of interest is β in (3). It captures the *relative effect* of a one-standard-deviation increase in a locality's COVID-19 intensity on the outcome variable compared to similar localities with a COVID-19 intensity corresponding to the sample's mean. Table III shows our coefficient estimates for (3). We also rerun the specification in (3) but (i) substituting semiannual pulse dummies

[17] Joaquim et al. (2019) show how changes in credit concentration affect banks' local behavior in credit markets. Therefore, we introduce the covariate "local market share" to control for differences in the local credit concentration in a specific local credit market. Following Schnabl (2012), we also include the controls "credit provisions as a share of the total credit," "average maturity," and "average ticket" to account for differences in the riskiness profiles and locality-specific idiosyncrasies that may still exist across nearby and comparable localities within the same bank-modality.

for the step variable COVID-19$_t$ and (ii) expanding the temporal window from the beginning of 2017 to the end of 2020 to examine the parallel trends assumption. Figure 7 displays the estimated β coefficients for our three main variables: effective prices, marginal costs, and Lerner indices. The dynamics of these three variables are statistically equivalent in localities with different levels of COVID-19 intensity in the pre-pandemic period. Only after the pandemic outbreak did the dynamics diverge.

TABLE III. *How does COVID-19 affect market power components and lending behavior in localities?*

Dependent Variables:	Effective Price$_{bmlt}$	Marginal Cost$_{bmlt}$	Lerner$_{bmlt}$	Credit Income$_{bmlt}$	Granted Credit$_{bmlt}$	$\frac{\text{Provision}_{bmlt}}{\text{Credit}_{bmlt}}$	Contractual Price$_{bmlt}$
Model:	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Variable of interest							
COVID-19$_t$ · COVID-19 Intensity$_l$	-0.0032	0.0187***	-0.0221***	-0.0058**	-0.0231***	0.0126*	0.0057**
	(0.0037)	(0.0062)	(0.0076)	(0.0028)	(0.0062)	(0.0072)	(0.0024)
Controls							
COVID-19$_t$ · Emergency Aid Volume/GDP$_l$	-0.0106	-0.0302*	0.0131	0.0032	0.0121	-0.0214	-0.0088
	(0.0110)	(0.0169)	(0.0196)	(0.0069)	(0.0096)	(0.0164)	(0.0055)
COVID-19$_t$ · % SMEs$_l$	0.0033	0.0260***	-0.0239***	-0.0023	-0.0356***	-0.0001	0.0009
	(0.0041)	(0.0077)	(0.0090)	(0.0032)	(0.0062)	(0.0069)	(0.0022)
COVID-19$_t$ · Distance to Capital$_l$	0.0011	-0.0050	0.0056	-0.0018	-0.0027	0.0058	0.0006
	(0.0063)	(0.0104)	(0.0107)	(0.0050)	(0.0086)	(0.0098)	(0.0032)
COVID-19$_t$ · Population$_l$	0.0017	0.0135**	-0.0022	-0.0055	0.0083	0.0001	0.0030***
	(0.0021)	(0.0063)	(0.0068)	(0.0051)	(0.0156)	(0.0038)	(0.0010)
COVID-19$_t$ · Market Share$_{bl}$	0.0222***	0.0407***	-0.0463***	-0.0180***	-0.0112	0.0085	0.0051
	(0.0069)	(0.0074)	(0.0097)	(0.0065)	(0.0069)	(0.0136)	(0.0046)
COVID-19$_t$ · Provision/Credit$_{bl}$	-0.0035	-0.0234**	0.0102	-0.0055	-0.0094**	-0.3297***	0.0103**
	(0.0104)	(0.0099)	(0.0100)	(0.0035)	(0.0044)	(0.0229)	(0.0049)
COVID-19$_t$ · Credit Maturity$_{bl}$	-0.0256	0.0428*	-0.0204	-0.0222	0.0377*	0.0157	0.0709***
	(0.0200)	(0.0257)	(0.0415)	(0.0146)	(0.0192)	(0.0329)	(0.0087)
COVID-19$_t$ · Credit Ticket$_{bl}$	-0.0016	0.0038	-0.0201	-0.0197*	-0.0404	-0.0014	-0.0014
	(0.0024)	(0.0061)	(0.0181)	(0.0111)	(0.0479)	(0.0025)	(0.0011)
Fixed effects and controls							
Locality	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time · Bank · Modality · · Macrolocality · *Per capita* GDP(3)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Observations	78,535	78,535	78,535	78,535	78,535	78,535	78,362
R^2	0.9242	0.7674	0.7691	0.7367	0.7494	0.9094	0.9731

Note: This table reports coefficient estimates for the specification in Equation (3) using semiannual data from 2019 to 2020 at the bank-modality-locality-time level. We use the following dependent variables: effective price (Spec. I), marginal cost (Spec. II), Lerner (Spec. III), credit income of within-half-year granted credit (Spec. IV), within-half-year granted credit (Spec. V), provisions as a share of the outstanding credit (Spec. VI), and contractual price or interest rate (Spec. VII). The dummy variable COVID-19$_t$ is a step variable that equals one when the year is 2020 and zero otherwise. The continuous treatment variable COVID-19 Intensity$_l$ follows Equation (2). All specifications have two sets of control variables. The first set comprises controls for government policies introduced to combat the economic effects of COVID-19: the total emergency aid volume received by residents in the locality during 2020 as a share of the local GDP and the share of SMEs in the locality (firms with up to three employees) by the end of 2019. The second set encompasses *ex-ante* bank-modality-locality controls (fixed with December 2019 values) encompassing: local market share (bank outstanding credit as a share of the locality-level outstanding credit), provisions as a share of the outstanding credit, average maturity, and average ticket. We add all low-order interactions and marginal terms for covariates appearing in a higher-order interaction that are not collinear with the fixed effects. We also introduce locality and time-bank-modality-macrolocality-*per capita* GDP (discretized in terciles) fixed effects. Coefficients represent the change in the dependent variable in terms of standard deviations for a one-standard-deviation increase in the independent variable. One-way (locality) standard errors in parentheses. *, **, *** denote statistical significance of 10%, 5%, and 1%, respectively.

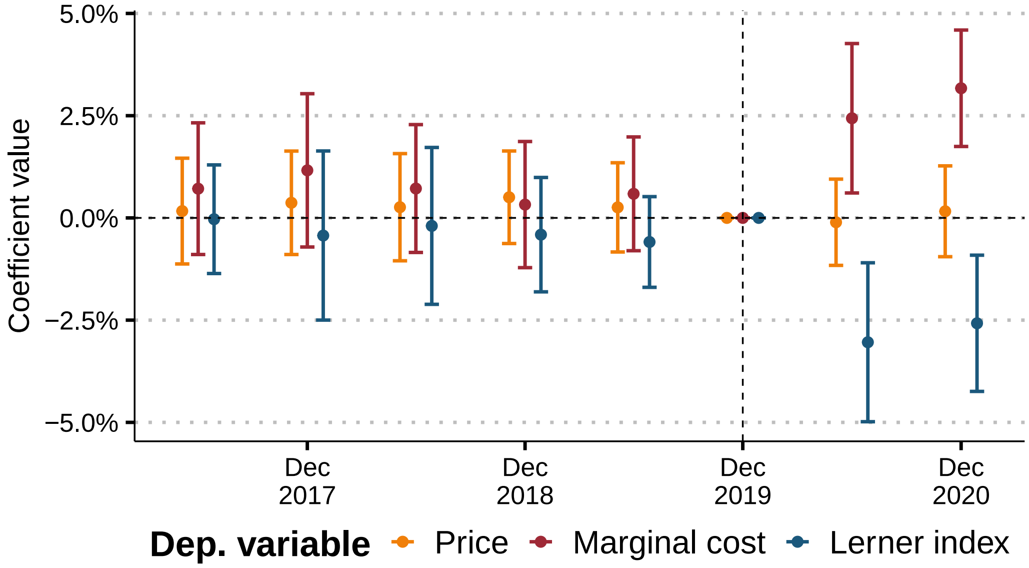


Figure 7. *Parallel trends check. We run specification in (3) but (i) changing the step variable COVID-19$_t$ with semiannual pulse dummies and (ii) widening the temporal window from the beginning of 2017 to the end of 2020. The figure displays the estimated β coefficients for each half-year. Vertical bars denote the 95% confidence interval.*

Effective price channel: we find that the local COVID-19 intensity did not alter effective prices in the first year of the pandemic (Spec. I). However, substantial changes occurred in the effective price components: credit income (Spec. IV) and granted credit (Spec. V). A one-standard-deviation increase in the local COVID-19 prevalence (4%) reduced credit income by 0.0058 standard deviation, corresponding to $0.0058 \cdot$ R\$ 111.339 million = R\$ 0.65 million or 12% of the sample average. Simultaneously, granted credit decreased by 0.231 standard deviation, equivalent to $0.0231 \cdot$ R\$ 9.66 million = R\$ 2.23 million or 14% of the sample average.[18] The decline in credit income and granted credit in 2020 was economically relevant. Section 6.1 provides a rationalization of this result, showing that localities more affected by COVID-19 experienced decreased economic activity, leading to a deterioration of financial conditions.[19] Therefore, the substantial decrease in credit income was offset by a corresponding decrease in credit concessions in the locality, resulting in an unchanged effective price. We conclude that the effective price channel was not a substantial component that affected banks' market power during the first year of the pandemic.

Marginal cost channel: A one-standard-deviation increase in the local COVID-19 prevalence (4%) caused a 0.0187-standard-deviation increase in banks' marginal costs, corresponding to a $0.0187 \cdot$

[18]While the bank-time fixed effects in (3) capture the bank balance-sheet channel, we cannot cleanly identify the borrower balance-sheet channel (credit demand) because our data is not at the loan level.

[19]Additionally, a higher COVID-19 prevalence increased (i) borrowers' aggregate riskiness measured in terms of credit provisions as a share of the total credit (Spec. VI of Table III) and (ii) contractual prices or interest rates (Spec. VII of Table III), indicating that banks perceived borrowers as more financially constrained in areas more severely affected by COVID-19 during the first year of the pandemic.

$0.272 \approx 0.5$ cents more expensive marginal cost for a sample mean of 4 cents (12% of the sample mean). We have seen that granted credit decreases in localities with more COVID-19 prevalence (Spec. V, Table III). The increase in marginal costs suggests banks were unable to reduce their total costs in the first year of the pandemic, mostly because cost factors are sticky in the short term due to economic rigidities and legal and financial frictions.

We test this hypothesis empirically by examining whether bank branches' total cost $TC_{b,l,t}$—which is the sum of local funding, tax, labor, and other administrative costs—reduced in localities more affected by COVID-19 during 2020. We run the following econometric specification:

$$\begin{aligned} TC_{b,l,t} = \alpha_{b,g(l),t} + \alpha_l + \beta \cdot \text{COVID-19}_t \cdot \text{COVID-19 Intensity}_l + \\ + \gamma^T \cdot \text{COVID-19}_t \cdot \text{Controls}_{b,l} + \epsilon_{b,l,t}, \end{aligned} \tag{4}$$

in which b, l, and t index bank, locality, and time, respectively. The fixed effects $\alpha_{b,g(l),t}$ allow us to interpret the results in terms of the *same* bank operating in *different* but *similar* localities $g(l)$ (within the same macrolocality and comparable *per capita* GDP levels) over time. Our coefficient of interest is β in (4). It quantifies the effect of a one-standard-deviation increase in a locality's COVID-19 intensity on the bank branch's total cost *vis-à-vis* another branch of the same bank in similar localities with average levels of COVID-19 intensity. All the remainder setup is identical to (3) but at the bank-locality-time instead of bank-modality-locality-time level. Specification I of Table IV shows the estimated coefficients of (4). We confirm our hypothesis that bank branches in localities more affected by COVID-19 could not adjust their local total costs in response to the relative reduction in credit concessions in 2020. This finding reflects the short-term stickiness of the bank branch's cost factors.

Market power: COVID-19 did not change effective prices in more affected localities. However, bank branches' marginal costs increased substantially in more affected localities by COVID-19 due to a combination of (i) reduced credit concessions and (ii) the stickiness of bank branches' cost factors in the short term. In net terms, the COVID-19 pandemic reduced the market power of Brazilian banks through the marginal cost channel during 2020. One advantage of performance measures—such as our Lerner index—is the possibility of understanding the sources that lead to changes in competition. Structural measures, such as the HHI, do not provide this information despite being less data-intensive. We revisit our baseline regressions but substitute the HHI for the Lerner index

TABLE IV. *How does COVID-19 affect bank branches' total costs? Does IT development provide cost flexibility?*

Dependent Variable:	Local Total Cost$_{blt}$					
Model:	(I)	(II)	(III)	(IV)	(V)	(VI)
Variables of interest						
COVID-19$_t$ · COVID-19 Intensity$_l$	0.0029	0.0031	-0.0011	0.0034	-0.0012	0.0087
	(0.0053)	(0.0057)	(0.0069)	(0.0055)	(0.0060)	(0.0062)
COVID-19$_t$ · COVID-19 Intensity$_l$ · % Cost Factor$_{bl}$		0.0072	0.0025	-0.0006	-0.0144	-0.0116**
		(0.0323)	(0.0274)	(0.0095)	(0.0090)	(0.0053)
Controls						
COVID-19$_t$ · % Cost Factor$_{bl}$		0.0396	0.0416	0.0201*	0.0493**	-0.0357***
		(0.0347)	(0.0273)	(0.0107)	(0.0214)	(0.0125)
COVID-19$_t$ · Emergency Aid Volume/GDP$_l$	-0.0285	-0.0245	-0.0242	-0.0280	-0.0281	-0.0263
	(0.0221)	(0.0217)	(0.0214)	(0.0217)	(0.0220)	(0.0219)
COVID-19$_t$ · % SMEs$_l$	0.0507***	0.0466***	0.0451***	0.0479***	0.0505***	0.0503***
	(0.0140)	(0.0139)	(0.0139)	(0.0141)	(0.0141)	(0.0139)
COVID-19$_t$ · Distance to Capital$_l$	-0.0080	-0.0081	-0.0085	-0.0086	-0.0063	-0.0078
	(0.0116)	(0.0112)	(0.0112)	(0.0114)	(0.0115)	(0.0114)
COVID-19$_t$ · Population$_l$	0.0101	-0.0133	-0.0182	-0.0025	0.0008	0.0116
	(0.0144)	(0.0166)	(0.0153)	(0.0155)	(0.0161)	(0.0139)
COVID-19$_t$ · Average Market Share$_{bl}$	0.0075	0.0015	0.0045	0.0109	-0.0031	0.0083
	(0.0104)	(0.0111)	(0.0104)	(0.0111)	(0.0117)	(0.0103)
COVID-19$_t$ · Average Provision/Credit$_{bl}$	0.0022	0.0034	0.0029	0.0022	0.0024	0.0029
	(0.0057)	(0.0058)	(0.0057)	(0.0057)	(0.0057)	(0.0057)
COVID-19$_t$ · Average Credit Maturity$_{bl}$	-0.0990***	-0.0950***	-0.0953***	-0.0992***	-0.0954***	-0.0998***
	(0.0287)	(0.0285)	(0.0284)	(0.0286)	(0.0286)	(0.0287)
COVID-19$_t$ · Average Credit Ticket$_{bl}$	0.0133***	0.0133***	0.0133***	0.0139***	0.0118***	0.0134***
	(0.0044)	(0.0043)	(0.0043)	(0.0044)	(0.0044)	(0.0044)
Cost Factor	—	Funding	Tax	Labor	Other Adm.	IT
Variation of the Cost Factor	—	Local	Local	Local	Local	National
Fixed effects and controls						
Locality	Yes	Yes	Yes	Yes	Yes	Yes
Time · Bank · Macrolocality · *Per capita* GDP(3)	Yes	Yes	Yes	Yes	Yes	Yes
Other controls and interactions?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,788	7,788	7,788	7,788	7,788	7,788
R^2	0.9196	0.9209	0.9199	0.9202	0.9201	0.9201

Note: This table reports coefficient estimates for the specifications in Equations (4) (Spec. I) and (5) (Specs. II-VI) using semiannual data from 2019 to 2020 at the bank-locality-time level. The dependent variable is the bank branch's total cost, which is the sum of the local funding, tax, labor, and other administrative costs. The dummy variable COVID-19$_t$ is a step variable that equals one when the year is 2020 and zero otherwise. The continuous treatment variable COVID-19 Intensity$_l$ follows Equation (2). We make triple interactions of our local COVID-19 intensity measure, the step variable COVID-19, and the following cost factors (fixed with December 2019): funding (Spec. II), tax (Spec. III), labor (Spec. IV), other administrative costs (Spec. V), and IT (Spec. VI). The first four cost factors vary at the local level (bank-branch-specific) while the last one varies at the national level (bank-specific). Cost factors are normalized by the bank branch's local total cost in Specs. II–V and by the bank's total cost in Spec. VI. We add municipality-level controls for the local intensity of government programs to combat the economic effects of COVID-19 (Emergency Aid Volume / GDP$_l$ and Share of SMEs$_l$) and the locality's distance and population. We also introduce ex-ante controls for the bank-branch-specific market share, provision/credit, credit maturity, and credit ticket by taking their averages across credit modalities weighted by the credit volume in the pre-pandemic period. We add all low-order interactions and marginal terms for covariates appearing in a higher-order interaction that are not collinear with the fixed effects. We add locality and time-bank-macrolocality-*per capita* GDP (discretized in terciles) fixed effects. Coefficients represent the change in the dependent variable in terms of standard deviations for a one-standard-deviation increase in the independent variable. One-way (locality) standard errors in parentheses. *, **, *** denote statistical significance of 10%, 5%, and 1%, respectively.

in Section 6.2. We also check the robustness of our results with alternative specifications that do not employ controls or use a less comprehensive set of fixed effects in Section 6.3.

5.2 The role of bank digitalization during the pandemic

This section examines how *bank digitalization*—measured as the (national) bank's IT spending as a share of its total costs in the pre-pandemic period—influenced market power in the credit market during 2020. We first investigate the flexibility that digitalization brought to banks regarding costs and lending during the first year of the pandemic. We then discuss the role of digitalization in warranting higher market power during COVID-19.

5.2.1 Correlates of pre-pandemic bank digitalization

Table V reports the correlates of pre-pandemic bank digitalization for our entire sample (Spec. I), sub-sample of banks with IT spending below (Spec. II), and above (Spec. III) the median. We use bank-specific variables and variables that are averaged across localities with at least one branch of the bank. Pre-pandemic digitalization does not correlate with bank control or size. Additionally, there were no significant differences between digitalized and non-digitized banks in capitalization and liquidity prior to COVID-19. More importantly, bank digitalization does not correlate with locality-specific features where bank branches operate.[20]

5.2.2 Advantages of bank digitalization

IT and cost flexibility: the previous section showed that bank branches could not adjust their local total costs in the short term in the first year of the pandemic. We refine this analysis by examining whether bank branches more reliant on a specific cost factor gained more flexibility in their cost structure, i.e., whether there exist cost factors that are less (or even more) sticky. We use two different cost factors: local factors (funding, tax, labor, and other administrative) as a share of the branch's local total costs and a national factor (IT) as a share of the bank's total costs. For each specification, we triple interact our measure of local COVID-19 intensity, the step variable COVID-19, and the cost factor (fixed with December 2019 values) as follows:

$$\begin{aligned} TC_{b,l,t} = \alpha_{b,g(l),t} + \alpha_l + \beta \cdot \text{COVID-19}_t \cdot \text{Intensity}_l + \lambda \cdot \text{COVID-19}_t \cdot \text{Intensity}_l \cdot \text{Cost Factor}_{bl} + \\ + \gamma^T \cdot \text{COVID-19}_t \cdot \text{Controls}_{b,l} + \epsilon_{b,l,t}, \end{aligned} \tag{5}$$

[20] The release of the Pix in November 2020 is also a significant concern. As the adoption of Pix by banks is likely correlated with their degree of digitalization, our estimates for the second semester of 2020 may be inaccurate. For robustness, we re-estimated all empirical specifications in this section using data only until the end of the first semester of 2020. Our results remained qualitatively unchanged.

TABLE V. *Correlates of pre-pandemic bank digitalization (IT spending in terms of total costs during 2015–2019).*

Dependent Variables:	% IT Share$_b$		
Model:	(I)	(II)	(III)
Bank-specific variables			
Public Bank$_b$ (dummy)	0.2279	0.0112	0.5600
	(0.3107)	(0.0838)	(0.8097)
Medium-Sized Bank$_b$ (dummy)	0.0372	-0.0897	-0.0761
	(0.2435)	(0.0758)	(0.6240)
Micro-Sized Bank$_b$ (dummy)	0.4620*	-0.0169	0.3102
	(0.2407)	(0.0952)	(0.3971)
Bank Capitalization$_b$	-0.2191	0.1021	-0.4723
	(0.2019)	(0.0703)	(0.3410)
Bank Liquidity$_b$	0.1154	-0.1160	0.2406
	(0.1326)	(0.0918)	(0.2278)
Funding Cost/ATA$_b$	-0.1532	0.0278	-0.1715
	(0.1277)	(0.0452)	(0.1753)
Tax Cost/ATA$_b$	0.3062	-0.1622**	0.2477
	(0.3373)	(0.0611)	(0.3886)
Labor Cost/ATA$_b$	0.3922	-0.3055	0.6085*
	(0.2589)	(0.1839)	(0.3024)
Variables averaged across bank branches' localities			
Average *Per Capita* GDP$_b$	0.0447	0.0172	0.0177
	(0.1327)	(0.0274)	(0.1722)
Average Population$_b$	-0.1680	-0.0344	-0.1941
	(0.1346)	(0.0304)	(0.2094)
Average Distance to Capital$_b$	-0.0142	-0.0056	-0.1747
	(0.1443)	(0.0335)	(0.2522)
Average % Credit Outside Bank Branch's Locality$_b$	0.0104	-0.0479	0.2348
	(0.2262)	(0.0507)	(0.3649)
Average % Clients Outside Bank Branch's Locality$_b$	0.1996	0.0339	0.1827
	(0.1871)	(0.0392)	(0.3224)
(Intercept)	-0.2633	-0.4820***	0.1109
	(0.2382)	(0.0712)	(0.4401)
Sample	Full	Less Digitalized	More Digitalized
Observations	74	37	37
R^2	0.3949	0.3454	0.4347

Note: This table reports coefficient estimates of the cross-section regression % IT Share$_b$ = βBank Covariates$_b$ + ϵ_b, in which b is the (national) bank. The dependent variable is the total spending on IT as a share of bank's total costs from 2015 to 2019. Results are reported for the full sample of banks (Spec. I), banks less digitalized or with % IT Share lower than the median (Spec. II), and banks more digitalized or with %IT Share higher than the median (Spec. III). We employ two types of bank covariates: bank-specific variables and variables averaged across bank branch's localities (weighted by the volume of credit concessions in each bank branch's locality). The dummy "Public bank" is relative to the observed values for private banks. The dummies "Medium-Sized Bank" and "Micro-Sized Bank" are relative to values observed for large banks. "Bank capitalization" is the bank capital as a share of its total assets. "Bank Liquidity" is the Liquidity Coverage Ratio (LCR). We divide funding, tax, and labor costs by the pre-pandemic bank's total assets to prevent a mechanical relationship with the dependent variable, which is in terms of the bank's total costs. Data on bank control and size comes from the Registry of Financial Institutions (Unicad). Bank capitalization and liquidity comes from Cosif. "Average Exposure to Emergency Aid Volume/GDP" is locality-specific Emergency Aid Volume/GDP$_l$ averaged across localities in which the bank has at least one branch. The other variables that are averaged across bank branch's localities follow the same rationale. We standardize numerical variables, including the dependent variable. One-way (bank) standard errors in parentheses. *, **, *** denote statistical significance of 10%, 5%, and 1%, respectively.

in which $TC_{b,l,t}$ is the bank branch's local total costs and Cost Factor$_{bl}$ is one of the five cost factors discussed above. For convenience, the variable Intensity$_l$ is a shorthand for COVID-19 Intensity$_l$. Our coefficient of interest is λ in (5). If banks can adjust a specific cost factor more quickly than the average following the COVID-19 outbreak, this should be loaded in this coefficient. Specifications II–VI of Table IV show the coefficient estimates of (5) for each cost factor, one at a time. Local spending patterns during the pre-pandemic period on funding, tax, labor, and other administrative costs are unrelated to the bank branch's ability to adjust more easily its local total costs during the first year of the pandemic, suggesting a strong stickiness of these factors.

In contrast, banks with more IT spending relative to their total costs can reduce local costs. For each one-standard-deviation increase in the bank's IT cost share (5.5%) *ex-ante* the COVID-19 outbreak, local total costs reduce by 0.0116 standard deviation, or $0.0116 \cdot$ R\$ 0.05 billion $\approx$ R\$ 0.58 million (1.9% of the sample average). This finding highlights the critical nature of IT development and the cost flexibility it provides during times of distress.

We now analyze through which channels digitalization enabled reduced costs in 2020. We examine how each of the four local cost factors—funding, tax, labor, and other administrative costs—behaved during the first year of the pandemic for banks with different levels of digitalization. We employ a similar empirical setup as in (5), except that we (i) change the dependent variable for each of the four specific local cost factors above ($C_{b,l,t}$) and (ii) triple interact the IT cost share with the local COVID-19 intensity, and the step variable COVID-19, yielding:

$$\begin{aligned} C_{b,l,t} = \alpha_{b,g(l),t} + \alpha_l + \beta \cdot \text{COVID-19}_t \cdot \text{Intensity}_l + \lambda \cdot \text{COVID-19}_t \cdot \text{Intensity}_l \cdot \text{\% IT Cost}_b + \\ + \gamma^T \cdot \text{COVID-19}_t \cdot \text{Controls}_{b,l} + \epsilon_{b,l,t}, \end{aligned} \tag{6}$$

Table VI reports the coefficient estimates of (6) for the following dependent variables: local funding costs (Specs. I–II), local tax costs (Specs. III–IV), local labor costs (Specs. V–VI), and local other administrative costs (Specs. VII-VIII). To establish a baseline on how COVID-19 affected specific cost factors, we run (6) with (even-numbered specs) and without (odd-numbered specs) the interactions with the IT cost share.

Consistent with our previous findings regarding local total costs, each cost factor also did not respond to different levels of local COVID-19 intensity. However, we observed a shift in the cost structure for more digitalized banks during the first year of the COVID-19 outbreak: they reduced funding costs (Spec. II) at the expense of increased labor (Spec. VI) and other administrative costs (Spec. VIII). Given that more digitalized banks could reduce total costs compared to less digitalized banks (Spec. VI of Table IV), the decrease in funding costs dominated the increase in the other two cost components. One potential explanation for lower funding costs may arise from the convenience provided by digital services. Customers who cannot access digital services easily may perceive the need to carry physical money as important. In contrast, if they can pay and manage their

TABLE VI. *Benefits of IT (cost flexibility): through which cost factor does digitalization provide cost flexibility?*

Dependent Variables:	Funding Costs		Tax Costs		Labor Costs		Other Adm. Costs	
Model:	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Variables of interest								
COVID-19$_t$ · COVID-19 Intensity$_l$	0.0124	0.0241	0.0085	0.0096	-0.0044	-0.0035	-0.0049	-0.0066
	(0.0145)	(0.0156)	(0.0071)	(0.0081)	(0.0044)	(0.0055)	(0.0056)	(0.0072)
COVID-19$_t$ · COVID-19 Intensity$_l$		-0.0390***		-0.0055		0.0138***		0.0173***
· % IT Cost$_b$		(0.0082)		(0.0057)		(0.0053)		(0.0061)
Controls								
COVID-19$_t$ · Emergency Aid Volume/GDP$_l$	-0.0515	-0.0443	-0.0319	-0.0310	-0.0176	-0.0200	-0.0085	-0.0115
	(0.0430)	(0.0424)	(0.0245)	(0.0242)	(0.0183)	(0.0182)	(0.0192)	(0.0191)
COVID-19$_t$ · % SMEs$_l$	0.1015***	0.0997***	0.0610***	0.0607***	0.0305**	0.0313***	0.0146	0.0156
	(0.0293)	(0.0291)	(0.0157)	(0.0157)	(0.0119)	(0.0118)	(0.0128)	(0.0127)
COVID-19$_t$ · Distance to Capital$_l$	0.0062	0.0072	-0.0030	-0.0029	-0.0010	-0.0015	-0.0001	-0.0006
	(0.0249)	(0.0248)	(0.0133)	(0.0133)	(0.0099)	(0.0099)	(0.0124)	(0.0126)
COVID-19$_t$ · Population$_l$	-0.0514	-0.0476	0.0206	0.0210	-0.0070	-0.0073	0.0038	0.0028
	(0.0642)	(0.0630)	(0.0228)	(0.0229)	(0.0097)	(0.0098)	(0.0162)	(0.0167)
COVID-19$_t$ · Average Market Share$_{bl}$	0.0030	0.0057	-0.0117	-0.0114	0.0269***	0.0261***	0.0293***	0.0282***
	(0.0163)	(0.0164)	(0.0109)	(0.0109)	(0.0078)	(0.0077)	(0.0101)	(0.0101)
COVID-19$_t$ · Average Provision/Credit$_{bl}$	-0.0051	-0.0043	0.0012	0.0012	0.0042	0.0050	-0.0045	-0.0041
	(0.0116)	(0.0117)	(0.0052)	(0.0053)	(0.0037)	(0.0040)	(0.0047)	(0.0046)
COVID-19$_t$ · Average Credit Maturity$_{bl}$	-0.2047***	-0.2072***	0.0210	0.0207	-0.0285	-0.0278	0.0257	0.0268
	(0.0436)	(0.0435)	(0.0257)	(0.0258)	(0.0223)	(0.0224)	(0.0237)	(0.0237)
COVID-19$_t$ · Average Credit Ticket$_{bl}$	0.0082	0.0084	0.0216***	0.0217***	0.0116	0.0116	0.0327***	0.0326***
	(0.0076)	(0.0077)	(0.0082)	(0.0082)	(0.0091)	(0.0091)	(0.0087)	(0.0087)
Fixed effects and controls								
Locality	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time · Bank · Macrolocality · *Per capita* GDP(3)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls and interactions?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,788	7,788	7,788	7,788	7,788	7,788	7,788	7,788
R^2	0.8700	0.8710	0.9138	0.9138	0.9132	0.9136	0.9217	0.9218

Note: This table reports coefficient estimates for variations of the specification in Equation (6) using semiannual data from 2019 to 2020 at the bank-locality-time level. We use the following bank-branch-specific cost factors: funding costs (Specs. I–II), which include costs from deposits, securities and bonds; tax costs (Specs. III–IV); labor costs (Specs. V–VI); and other administrative costs (Specs. VII–VIII), which is the remaining administrative costs after deducting labor costs. The dummy variable COVID-19$_t$ is a step variable that equals one when the year is 2020 and zero otherwise. The continuous treatment variable COVID-19 Intensity$_l$ follows Equation (2). Even-numbered (odd-numbered) specifications do (do not) include terms with the IT share cost variable. In even-numbered specifications, we make triple interactions of our local COVID-19 intensity measure, the step variable COVID-19, and the national bank's IT spending as a share of its total cost before the COVID-19 outbreak (% IT Cost$_b$). We add controls for the local intensity of government programs to combat the economic effects of COVID-19 (Emergency Aid Volume / GDP$_l$ and Share of SMEs$_l$) and the locality's distance and population. We add all low-order interactions and marginal terms for covariates appearing in a higher-order interaction that are not collinear with the fixed effects. We also introduce ex-ante controls for the bank-branch-specific market share, provision/credit, credit maturity, and credit ticket by taking their averages across credit modalities weighted by the credit volume in the pre-pandemic period. We add locality and time-bank-macrolocality-*per capita* GDP (discretized in terciles) fixed effects. Coefficients represent the change in the dependent variable in terms of standard deviations for a one-standard-deviation increase in the independent variable. One-way (locality) standard errors in parentheses. *, **, *** denote statistical significance of 10%, 5%, and 1%, respectively.

financial needs electronically via internet banking or cell phones, they may hold money in banking accounts, lowering the bank's funding costs. One possible reason for increased labor costs for more digitalized banks may arise from their specific distribution of job roles compared to traditional banks. More digitalized banks are more likely to have job roles more prone to teleworking (e.g., IT-related), which were less affected by public policies to combat the COVID-19 economic effects.

IT and lending flexibility: IT development enables financial transactions, including credit, electronically with borrowers regardless of their locality. Precisely banks that spent more on IT before COVID-19 are likely to have more developed and trustworthy online banking systems, enabling these remote transactions to a greater extent. If that is the case, we should expect that banks with more developed IT systems could more easily replace or complement borrowers in localities more affected by COVID-19 with other remote borrowers. Digitalization could provide higher lending flexibility, which may have been especially useful during the COVID-19 pandemic. Since credit concessions decreased in more affected localities (Spec. V of Table III), more digitalized banks in localities heavily affected by COVID-19 could rearrange their credit portfolio, potentially away from local borrowers.

If our hypothesis sustains, we should observe a shift in the borrower's locality concentration: the share of borrowers outside (inside) the bank branch's physical locality should increase (decrease) for more digitalized bank branches in localities more affected by COVID-19. We test this hypothesis by running through loan-level data in the SCR, inspecting each credit operation's bank branch's and borrower's locations. SCR contains the bank branch's CEP (or ZIP code). We can determine the borrower's location by matching its tax identifier with the *Receita Federal do Brasil* dataset.

We test the shift in the borrower's locality concentration of bank branches using the same specification as in (6) but changing the dependent variables. We also report our results without interactions with the level of IT spending as a share of total costs to establish a baseline. Table VII reports the coefficient estimates for the following dependent variables: share of credit to borrowers (Specs. I–II) and share of distinct borrowers (Specs. III–IV) living outside the bank branch's physical locality. Bank branches in localities more affected by COVID-19 reduced credit concessions to non-local borrowers (Spec. I) relative to branches of the same bank in less affected but similar localities. The number of non-local clients was insensitive to the local COVID-19 intensity (Spec. III). However, more digitalized bank branches increased the share of clients living outside the bank branch's locality after the COVID-19 outbreak, suggesting IT development facilitated credit reallocation between local and remote borrowers. Our results highlight the role of digitalization in allowing bank branches to be less sensitive to local borrowers' conditions.

5.2.3 Bank digitalization and market power during COVID-19

The advantages of digitalization may have led to an increase in market power. We test this hypothesis by augmenting our baseline specification at the bank-modality-locality-time level in Equa-

TABLE VII. *Benefits of IT (lending flexibility): digitalized banks become less sensitive to local borrowers' conditions.*

Dependent Variables:	% Credit Outside Locality		% Clients Outside Locality	
Model:	(I)	(II)	(III)	(IV)
Variables of interest				
COVID-19$_t$ · COVID-19 Intensity$_l$	-0.0350**	-0.0385***	-0.0121	-0.0149
	(0.0137)	(0.0146)	(0.0087)	(0.0096)
COVID-19$_t$ · COVID-19 Intensity$_l$ · % IT Cost$_b$		0.0373***		0.0198**
		(0.0132)		(0.0093)
Controls				
COVID-19$_t$ · Emergency Aid Volume/GDP$_l$	-0.0045	-0.0116	-0.0092	-0.0130
	(0.0313)	(0.0308)	(0.0340)	(0.0341)
COVID-19$_t$ · % SMEs$_l$	-0.0062	-0.0044	0.0053	0.0062
	(0.0143)	(0.0143)	(0.0134)	(0.0135)
COVID-19$_t$ · Distance to Capital$_l$	-0.0177	-0.0188	-0.0078	-0.0085
	(0.0241)	(0.0240)	(0.0174)	(0.0177)
COVID-19$_t$ · Population$_l$	0.0108	0.0082	0.0019	0.0003
	(0.0086)	(0.0086)	(0.0044)	(0.0051)
COVID-19$_t$ · Average Market Share$_{bl}$	-0.0216	-0.0245	-0.0182	-0.0200
	(0.0275)	(0.0275)	(0.0200)	(0.0200)
COVID-19$_t$ · Average Provision/Credit$_{bl}$	-0.0124	-0.0126	0.0086	0.0082
	(0.0191)	(0.0190)	(0.0104)	(0.0108)
COVID-19$_t$ · Average Credit Maturity$_{bl}$	0.0784**	0.0802**	0.0020	0.0033
	(0.0391)	(0.0391)	(0.0238)	(0.0238)
COVID-19$_t$ · Average Credit Ticket$_{bl}$	0.0056	0.0057	-0.0052	-0.0054
	(0.0242)	(0.0243)	(0.0084)	(0.0083)
Fixed effects and controls				
Locality	Yes	Yes	Yes	Yes
Time · Bank · Macrolocality · *Per capita* GDP(3)	Yes	Yes	Yes	Yes
Other controls and interactions?	Yes	Yes	Yes	Yes
Observations	7,788	7,788	7,788	7,788
R^2	0.6559	0.6568	0.7123	0.7124

Note: This table reports coefficient estimates for variations of the specification in Equation (6) using semiannual data from 2019 to 2020 at the bank-locality-time level. We use the following bank-branch-specific variables: share of credit to borrowers (Specs. I–II) and share of distinct borrowers (Specs. III–IV) living outside the bank branch's physical locality. The dummy variable COVID-19$_t$ is a step variable that equals one when the year is 2020 and zero otherwise. The continuous treatment variable COVID-19 Intensity$_l$ follows Equation (2). Even-numbered (odd-numbered) specifications do (do not) include terms with the IT share cost variable. In even-numbered specifications, we make triple interactions of our local COVID-19 intensity measure, the step variable COVID-19, and the national bank's IT spending as a share of its total cost before the COVID-19 outbreak (% IT Cost$_b$). We add controls for the local intensity of government programs to combat the economic effects of COVID-19 (Emergency Aid Volume / GDP$_l$ and Share of SMEs$_l$) and the locality's distance and population. We also introduce ex-ante controls for the bank-branch-specific market share, provision/credit, credit maturity, and credit ticket by taking their averages across credit modalities weighted by the credit volume in the pre-pandemic period. We add all low-order interactions and marginal terms for covariates appearing in a higher-order interaction that are not collinear with the fixed effects. We add locality and time-bank-macrolocality-*per capita* GDP (discretized in terciles) fixed effects. Coefficients represent the change in the dependent variable in terms of standard deviations for a one-standard-deviation increase in the independent variable. One-way (locality) standard errors in parentheses. *, **, *** denote statistical significance of 10%, 5%, and 1%, respectively.

tion (3) with the bank-level IT spending as a share of total costs in the pre-pandemic period as follows:

$$\begin{aligned} y_{b,m,l,t} = \alpha_{b,m,g(l),t} + \alpha_l + \beta \cdot \text{COVID-19}_t \cdot \text{Intensity}_l + \lambda \cdot \text{COVID-19}_t \cdot \text{Intensity}_l \cdot \text{\% IT Cost}_b + \\ + \gamma^T \cdot \text{COVID-19}_t \cdot \text{Controls}_{b,m,l} + \epsilon_{b,m,l,t}. \end{aligned} \tag{7}$$

Table VIII reports the coefficient estimates of (7) using the following dependent variables: average effective price (Spec. I), marginal cost (Spec. II), Lerner index (Spec. III), credit income (Spec. IV), the volume of credit concessions within the half-year (Spec. V), provisions as a share of the outstanding credit (Spec. VI), and contractual prices (Spec. VII). Compared to traditional banks, more digitalized banks increased lending (Spec. V) and collected more credit income (Spec. IV) in localities more affected by COVID-19 in 2020 than the same bank operating in other similar but less affected localities. The increase in credit concessions, combined with the fact that digitalized banks expanded their credit concessions to borrowers outside the bank branch's physical location, suggests that more digitalized banks *complemented* rather than *substituted* their local borrower *clientele* with remote borrowers.

Effective prices of more digitalized banks were similar to traditional banks (Spec. I) when comparing their values for the same credit modality in more affected and less affected localities during the first year of the pandemic. This finding suggests that the increase in credit income was a mechanical consequence of increased credit concessions and not higher interest rates (Spec. VII) or changes in borrowers' riskiness profiles (Spec. VI).

More digitalized banks could mitigate the increase in marginal costs in localities more affected by COVID-19 when compared to traditional banks (Spec. II). This finding is consistent with the fact that digitalization enabled increased credit concessions and provided cost and lending flexibility during the first year of the pandemic, reducing the frictions of adjusting the bank branch's cost structure in the short term. As a result, the adverse effects of COVID-19 on marginal costs were attenuated. Therefore, digitalized banks experienced an attenuated loss (or even an increase for high-tech banks) in their market power relative to traditional banks. Digitalization enabled banks to adjust their credit portfolio quickly and re-balance credit across localities facing distinct levels of COVID-19 intensity.

5.2.4 Which credit modalities benefit the most from high levels of bank digitalization?

It is reasonable to assume that some credit modalities were relatively automated in the banking sector even before the pandemic. Examples include working capital and revolving working capital for non-financial firms and non-payroll-deducted credit (CDC) for individuals. These credit modalities do not require collateral or are related to pre-approved credit lines. Other credit modalities require more sophisticated online banking systems, and only more digital banks have prepared systems to conduct such transactions electronically. Examples include real estate, rural or agribusi-

TABLE VIII. *How does COVID-19 affect local market power and lending behavior for more digitalized bank branches?*

Dependent Variables:	Effective Price$_{bmlt}$	Marginal Cost$_{bmlt}$	Lerner$_{bmlt}$	Credit Income$_{bmlt}$	Granted Credit$_{bmlt}$	$\frac{\text{Provision}_{bmlt}}{\text{Credit}_{bmlt}}$	Contractual Price$_{bmlt}$
Model:	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Variable of interest							
COVID-19$_t$ · COVID-19 Intensity$_l$	-0.0055	0.0221***	-0.0251***	-0.0075**	-0.0259***	0.0133*	0.0067***
	(0.0043)	(0.0066)	(0.0077)	(0.0033)	(0.0060)	(0.0078)	(0.0026)
COVID-19$_t$ · COVID-19 Intensity$_l$ · % IT Cost$_b$	0.0049	-0.0171***	0.0146**	0.0090**	0.0119**	-0.0025	-0.0022
	(0.0044)	(0.0061)	(0.0071)	(0.0043)	(0.0057)	(0.0068)	(0.0027)
Controls							
COVID-19$_t$ · Emergency Aid Volume/GDP$_l$	-0.0114	-0.0279*	0.0112	-0.0012	0.0105	-0.0210	-0.0084
	(0.0110)	(0.0169)	(0.0194)	(0.0059)	(0.0095)	(0.0165)	(0.0056)
COVID-19$_t$ · % SMEs$_l$	0.0035	0.0254***	-0.0233***	0.0070*	-0.0352***	-0.0002	0.0008
	(0.0041)	(0.0077)	(0.0090)	(0.0039)	(0.0061)	(0.0069)	(0.0022)
COVID-19$_t$ · Distance to Capital$_l$	0.0013	-0.0050	0.0056	0.0041	-0.0027	0.0058	0.0005
	(0.0063)	(0.0104)	(0.0107)	(0.0048)	(0.0085)	(0.0098)	(0.0032)
COVID-19$_t$ · Population$_l$	0.0011	0.0147**	-0.0033	-0.0159***	0.0074	0.0002	0.0033***
	(0.0019)	(0.0059)	(0.0065)	(0.0043)	(0.0155)	(0.0039)	(0.0011)
COVID-19$_t$ · Market Share$_{bl}$	0.0220***	0.0410***	-0.0465***	-0.0193***	-0.0114*	0.0085	0.0052
	(0.0069)	(0.0074)	(0.0097)	(0.0063)	(0.0069)	(0.0136)	(0.0046)
COVID-19$_t$ · Provision/Credit$_{bl}$	-0.0036	-0.0234**	0.0102	-0.0027	-0.0094**	-0.3297***	0.0104**
	(0.0104)	(0.0099)	(0.0100)	(0.0047)	(0.0044)	(0.0229)	(0.0049)
COVID-19$_t$ · Credit Maturity$_{bl}$	-0.0254	0.0428*	-0.0203	0.0337*	0.0377**	0.0157	0.0708***
	(0.0200)	(0.0257)	(0.0415)	(0.0189)	(0.0191)	(0.0329)	(0.0087)
COVID-19$_t$ · Credit Ticket$_{bl}$	-0.0017	0.0037	-0.0200	-0.0396*	-0.0403	-0.0014	-0.0013
	(0.0025)	(0.0061)	(0.0181)	(0.0231)	(0.0479)	(0.0025)	(0.0011)
Fixed effects and controls							
Locality	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time · Bank · Modality · · Macrolocality · *Per capita* GDP(3)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls and interactions?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Observations	78,535	78,535	78,535	78,535	78,535	78,535	78,362
R^2	0.9242	0.7674	0.7691	0.7999	0.7495	0.9094	0.9731

Note: This table reports coefficient estimates for the specification in Equation (7) using semiannual data from 2019 to 2020 at the bank-modality-locality-time level. We use the following dependent variables: effective price (Spec. I), marginal cost (Spec. II), Lerner (Spec. III), credit income of within-half-year granted credit (Spec. IV), within-half-year granted credit (Spec. V), provisions as a share of the outstanding credit (Spec. VI), and contractual price or interest rate (Spec. VII). The dummy variable COVID-19$_t$ is a step variable that equals one when the year is 2020 and zero otherwise. The continuous treatment variable COVID-19 Intensity$_l$ follows Equation (2). All specifications have two sets of control variables. The first set comprises controls for government policies introduced to combat the economic effects of COVID-19: the total emergency aid volume received by residents in the locality during 2020 as a share of the local GDP and the share of SMEs in the locality (firms with up to three employees) by the end of 2019. The second set encompasses *ex-ante* bank-modality-locality controls (fixed with December 2019 values) encompassing: local market share (bank outstanding credit as a share of the locality-level outstanding credit), provisions as a share of the outstanding credit, average maturity, and average ticket. We add all low-order interactions and marginal terms for covariates appearing in a higher-order interaction that are not collinear with the fixed effects. We also introduce locality and time-bank-modality-macrolocality-*per capita* GDP (discretized in terciles) fixed effects. Coefficients represent the change in the dependent variable in terms of standard deviations for a one-standard-deviation increase in the independent variable. One-way (locality) standard errors in parentheses. *, **, *** denote statistical significance of 10%, 5%, and 1%, respectively.

ness credit, and vehicle financing for individuals and non-financial firms.

Therefore, we should expect increased digitalization to provide more gains precisely in credit modalities that were not traditionally settled over online systems. Table IX tests this hypothesis by rerunning the same specification as in Equation (7) but dividing the sample into credit modalities that are traditionally non-digital (Specs. I–IV) and digital (Specs. V–VIII). Dependent variables are the effective price (Specs. I and V), marginal cost (Specs. II and VI), Lerner index (Specs. III and VII), and granted credit (Specs. IV and VIII). Our baseline results remained unchanged for both samples: a higher local COVID-19 intensity increased marginal costs and did not change effective prices, reducing banks' market power. However, more digitalized banks could mitigate the marginal cost increase because of their cost and lending flexibility. This effect originated solely from credit modalities that are traditionally non-digital, in which digitalized banks enjoy a clear comparative advantage, confirming our empirical hypothesis.

TABLE IX. *Which credit modalities benefit the most from high levels of bank digitalization?*

Credit Modality:	Traditionally non-digital				Traditionally digital			
Dependent Variables:	Effective Price$_{bmlt}$	Marginal Cost$_{bmlt}$	Lerner$_{bmlt}$	Granted Credit$_{bmlt}$	Effective Price$_{bmlt}$	Marginal Cost$_{bmlt}$	Lerner$_{bmlt}$	Granted Credit$_{bmlt}$
Model:	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Variables								
COVID-19$_t$								
× COVID-19 Intensity$_l$	-0.0014	0.0250**	-0.0338***	-0.0231***	-0.0088	0.0206**	-0.0143*	-0.0271**
	(0.0049)	(0.0098)	(0.0126)	(0.0073)	(0.0091)	(0.0081)	(0.0077)	(0.0105)
× COVID-19 Intensity$_l$ · % Local IT Cost$_{bl}$	0.0004	-0.0213**	0.0199*	0.0144**	0.0131	-0.0122	0.0102	0.0017
	(0.0049)	(0.0091)	(0.0104)	(0.0066)	(0.0091)	(0.0083)	(0.0082)	(0.0061)
Fixed effects and controls								
Other Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Locality	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time · Bank · Modality · · Macrolocality · *Per capita* GDP(3)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics								
Observations	48,678	48,678	48,678	48,678	29,857	29,857	29,857	29,857
R^2	0.9303	0.7524	0.7652	0.7248	0.8996	0.7980	0.8020	0.8304

Note: This table reports coefficient estimates for the specification in Equation (7) using semiannual data from 2019 to 2020 at the bank-modality-locality-time level. We divide our sample as follows. The first sub-sample comprises modalities that are traditionally contracted digitally either because they do not need collateral or are related to pre-approved credit lines: non-financial firms (working capital, revolving working capital) and individuals (non-payroll-deducted credit). The second sub-sample is composed of modalities that require more advanced online banking systems and are more common among more digital banks: non-financial firms (account receivables, infrastructure, agribusiness, real estate, investment, other credit) and individuals (payroll-deducted, real estate, rural, vehicle financing, other credit). We use the following dependent variables: effective price (Specs. I and V), marginal cost (Specs. II and VI), Lerner (Specs. III and VII), and granted credit (Specs. IV and VIII). The dummy variable COVID-19$_t$ is a step variable that equals one when the year is 2020 and zero otherwise. The continuous treatment variable COVID-19 Intensity$_l$ follows Equation (2). All specifications have two sets of control variables. The first set comprises controls for government policies introduced to combat the economic effects of COVID-19: the total emergency aid volume received by residents in the locality during 2020 as a share of the local GDP and the share of SMEs in the locality (firms with up to three employees) by the end of 2019. The second set encompasses *ex-ante* bank-modality-locality controls (fixed with December 2019 values) encompassing: local market share (bank outstanding credit as a share of the locality-level outstanding credit), provisions as a share of the outstanding credit, average maturity and average ticket. We add all low-order interactions and marginal terms for covariates appearing in a higher-order interaction that are not collinear with the fixed effects. We also introduce locality and time-bank-modality-macrolocality-*per capita* GDP (discretized in terciles) fixed effects. Coefficients represent the change in the dependent variable in terms of standard deviations for a one-standard-deviation increase in the independent variable. One-way (locality) standard errors in parentheses. *, **, *** denote statistical significance of 10%, 5%, and 1%, respectively.

5.2.5 Where are more digitalized bank branches expanding during COVID-19?

Our previous results showed that more digitalized bank branches could expand lending outside their physical locations in terms of volume and number of clients during COVID-19. This feature gave these bank branches an upper hand in maintaining their operational activities, reflecting lower marginal costs and improved market power compared to other local bank branches. This section analyzes where these more digitalized banks expanded in the first year of the pandemic.

The original empirical setup employs microdata on the bank's financial outcomes over a semiannual period in a specific locality for a credit modality. It does not discriminate against the credit destination, i.e., the borrower's locality. We also need information on the borrower's location to examine geographically the expansion of digitalized banks during COVID-19. Therefore, we go back to the SCR dataset and reconstruct the dataset but now segregate the bank's financial outcomes over a semiannual period for both the bank branch's and borrower's locations. We only have information on the volume of credit concessions and the number of distinct clients. We end up with bank-borrower-specific lending networks, where vertices are localities. An edge connecting two localities represents a credit concession from the bank branch in the edge's originating locality to a borrower in the edge's ending locality. We only keep edges connecting different localities, i.e., remove self-loops in the network. Additionally, we remove singleton vertices that are localities where none of the bank branches lend outside the locality.

Our identification strategy relies on the exogenous variation of COVID-19 intensity across similar borrowers' locations, i.e., within the same macrolocality and with similar *per capita* GDP (discretized in terciles). The data in network format enables us to compare lending across borrowers' locations for the *same* bank branch (within-bank-branch analysis), thereby allowing us to isolate any time-variant supply shocks at the bank branch level. For instance, this approach accounts for different local COVID-19 intensities on the lender side. Differences in local COVID-19 intensity can vary only on the borrower's side. Similar to our baseline setup, we interpret COVID-19 intensity in this configuration as a local credit demand shock in the borrower's locality.

To gain interpretability, we use the *difference* rather than the level of local COVID-19 intensity in the borrower's locality concerning that in the bank branch's locality (ΔIntensity). Therefore, positive (negative) values in this difference indicate that COVID-19 intensity is stronger (weaker) in the borrower's location than in the bank branch's location. We operationalize this empirical strategy with the following specification:

$$y_{b,l,d,t} = \alpha_{b,l,g(d),t} + \alpha_d + \lambda \cdot \text{COVID-19}_t \cdot \Delta\text{Intensity}_{ld} \cdot \%\ \text{IT Cost}_b + \gamma^T \cdot \text{COVID-19}_t \cdot \text{Controls}_{b,d} + \epsilon_{b,l,d,t}, \tag{8}$$

in which b indexes the bank, l is the bank branch's locality (credit origination), d is the borrower's locality (credit destination), and t is the period (semiannual from 2019 to 2020). Table X reports coefficient estimates of (8) for the following dependent variables: the bank branch's number of distinct clients (Spec. I) and the volume of credit concessions in a specific locality d (Spec. II). To save space, we only show the coefficient λ, which indicates how bank digitalization affects remote lending in localities more affected by COVID-19 than other less affected but similar localities. More digitalized bank branches reduced the number of distinct clients (Spec. I) and the volume of credit concessions (Spec. II) in remote localities relatively more affected by COVID-19 than traditional bank branches. Our findings indicate that digitalized bank branches extended more credit to borrowers in remote areas less affected by COVID-19 than the lending branch's own locality.

TABLE X. *Where are more digitalized bank branches expanding during COVID-19?*

Dependent Variables:	#Clients$_{bldt}$	Granted Credit$_{bldt}$					
Transmission Factor:	—	—	ΔLerner$_{bld}$	ΔMark. Share$_{bld}$	Δ*Per cap.* GDP$_{ld}$	ΔPopulation$_{ld}$	ΔIT Readiness$_{ld}$
Model:	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Variables							
COVID-19$_t$ × Δ COVID-19 Intensity$_{ld}$	-0.0089***	-0.0188***	-0.0178***	-0.0183***	-0.0204***	-0.0138***	-0.0193***
×% Local IT Cost$_b$	(0.0030)	(0.0032)	(0.0032)	(0.0033)	(0.0032)	(0.0033)	(0.0032)
COVID-19$_t$ × Δ COVID-19 Intensity$_{ld}$			0.0042**	0.0009	-0.0049**	0.0065***	0.0007
×% Local IT Cost$_b$ × Transmission Factor$_{bld}$			(0.0017)	(0.0022)	(0.0020)	(0.0022)	(0.0025)
Fixed effects and controls							
Borrower's Locality	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time · Bank · Bank's Locality ·	Yes	Yes	Yes	Yes	Yes	Yes	Yes
· Borrower's Macrolocality ·							
· Borrower's *Per capita* GDP(3)							
Other controls and interactions?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Observations	1,177,031	1,177,031	1,177,031	1,177,031	1,177,031	1,177,031	1,177,031
R^2	0.8688	0.8179	0.8181	0.8179	0.8183	0.8185	0.8183

Note: This table reports coefficient estimates for the specification in Equation (8) using semiannual data from 2019 to 2020 at the bank-bank branch's locality-borrower's locality-time level. We use the following dependent variables: the bank branch's number of distinct clients (Spec. I) and volume of credit concessions in a specific locality d (Specs. II–VII). To save space, we only report the coefficient λ in (8) and also the quadruple interactions in Specs. III to VII. The dummy variable COVID-19$_t$ is a step variable that equals one when the year is 2020 and zero otherwise. The continuous variables in differences Δ COVID-19 Intensity$_{ld}$ and the transmission factors ΔLerner$_{bld}$, ΔMark. Share$_{bld}$, Δ*Per cap.* GDP$_{ld}$, ΔPopulation$_{ld}$, and ΔIT Readiness$_{ld}$ are taken from observed values in borrower's locality minus that from the bank branch's locality. We add controls for the local intensity of government programs to combat the economic effects of COVID-19 (Emergency Aid Volume / GDP$_l$ and Share of SMEs$_l$) and the locality's distance and population, all with respect to the borrower's location. We add all low-order interactions and marginal terms for covariates appearing in a higher-order interaction that are not collinear with the fixed effects. We add borrower's locality and time-bank-bank branch's locality-borrower's macrolocality-borrower's-*per capita* GDP (discretized in terciles) fixed effects. Coefficients represent the change in the dependent variable in terms of standard deviations for a one-standard-deviation increase in the independent variable. One-way (borrower's locality) standard errors in parentheses. *, **, *** denote statistical significance of 10%, 5%, and 1%, respectively.

We now analyze to which types of remote localities bank branches expanded more. For that, we augment (8) by adding a quadruple interaction (and all other lower-order interactions) between the terms COVID-19$_t$, Δ COVID-19 Intensity$_{ld}$, % Local IT Cost$_b$, and the difference between an observable transmission factor of the borrower's and the bank branch's locality (Transmission Factor$_{bld}$).

By interacting the Transmission Factor$_{bld}$ with the three covariates, we can understand how bank branches choose among remote localities with the *same* relative level of local COVID-19 intensity. This feature is important because our baseline results show that digitalized bank branches moved away from remote localities more affected by COVID-19 than their own localities. We use the following relative transmission factors (difference between observables of the borrower's and bank branch's localities): (i) bank-branch-specific differences, such as Lerner indices and market shares; and (ii) locality-specific differences, such as *per capita* GDP, population, and IT readiness. Bank branches expanded more to remote localities in which they had relatively higher market power (Spec. III) and to remote localities that were less wealthy (Spec. V) and more populated (Spec. VI). Relative differences in market share (Spec. IV) and IT readiness (Spec. VII) did not drive lending decisions to remote localities.

6 Additional empirical evidence

6.1 COVID-19 and local economic activity

Localities with a higher COVID-19 prevalence are more likely to implement public health measures such as horizontal social distancing, lockdown, and quarantines. Such measures may impact various economic activities, notably those relying on in-site labor and consumption. This section shows that an increase in local COVID-19 prevalence caused a decrease in local economic activity. This piece of evidence rationalizes many of the empirical findings discussed in the main empirical section, such as reduced credit concessions and income in more affected localities.

The first challenge is the lack of recent data on economic activity at the local level. The natural candidate in Brazil is data from the IBGE, which contains estimates of the Brazilian municipalities' local GDP. However, this data has a lag of three to four years. Therefore, we resort to payment transactions received by firms in Brazil. We use firm-specific inflows as a proxy for firm income. In Brazil, firms can receive payments in several ways. We attempt to encompass many of these income streams in our empirical exercises by analyzing electronic transactions from the following confidential datasets:[21]

[21] We exclude funds received from financial institutions or investment funds. We also eliminate transactions within the same firm's economic group, a typical operation among firms with multiple plants for internal liquidity management purposes. We also remove transactions from the public sector; international representative bodies, such as embassies; and sectors that deal with water supply and sanitation activities, which are likely to be financed by local governments.

- Transactions from credit and debit cards from open-array operations reported by accreditors.[22] In Brazil, individuals, and firms typically settle small-valued transactions using credit and debit cards. Therefore, this is an important income source for sectors in the retail market. In 2019 and 2020, 3.5 million firms received debit or credit card funds. In 2019 (2020), these firms were recipients of 1.67 (1.68) billion operations with an aggregate value of R$ 1.51 (1.62) trillion, equating to 20% of Brazil's 2019 GDP (22% of Brazil's 2020 GDP).

- Invoices or "*Boletos*" from the *Câmara Interbancária de Pagamentos* (CIP), which is a not-for-profit association that is part of the Brazilian Payments System (SPB). Invoices are a document widely used in Brazil as a payment instrument for a product or service. In 2019 and 2020, 1.80 million firms received funds from invoices. In 2019 (2020), these firms were recipients of 2.58 (2.81) billion operations with an aggregate value of R$ 3.79 (3.75) trillion, equating to 51% of Brazil's 2019 GDP (50% of Brazil's 2020 GDP).

- Wire transfers or *Transferência Eletrônica Disponível* (TED) from the *Sistema de Transferência de Reservas* (STR) and the *Sistema de Transferência de Fundos* (CIP-Sitraf), both part of the SPB and maintained by the BCB.[23] We end up with 6.74 million firms in our sample during 2019 and 2020. In 2019 (2020), there were 192.78 (258.73) million firm-to-firm operations with an aggregate value of R$ 3.88 (4.38) trillion, equating to 52% of Brazil's 2019 GDP (59% of Brazil's 2020 GDP).

- Exports. We proxy the firm's exports using the Foreign Exchange System or *Sistema de Câmbio* maintained by the BCB. The system captures foreign exchange market operations in

Even though we use these data sources as a proxy for firm income, we should acknowledge that there may be some potential mismatches between our proxy and the *de facto* firm income. First, our data is based on a cash basis of accounting, whereas firm income accounting is formally based on an accrual basis of accounting. Second, we do not have transactions settled with cash, checks, direct deposits into accounts, or automatic debit transactions (typical for paying water and electricity, telephone, TV, and Internet in Brazil). Third, we lack data on businesses where the buyer uses the good as part of payment, such as automobile dealerships and agriculture. Fourth, we do not have registries for foreign operations' income. Last, we do not include Pix (an instant payment scheme that enables users to send or receive payment transfers in a few seconds at any time, including non-business days) operations because they only began operating in November 2020 for the corporate sector.

[22] The dataset does not include private label and flagged private label cards because these operations run through the merchant's network. In addition, we do not have operations from other closed arrangements, such as meal vouchers and single-ticket sales.

[23] STR and CIP-Sitraf are real-time gross settlement payment systems that record electronic interbank transactions between two economic agents in Brazil. These high-frequency datasets provide information on the transaction's exact time, the identification and location of the payer and receiver of the money, and the transaction's purpose, among others. Our analysis removes payments among branches of the same firm conglomerate, as they are likely to increase when a firm branch is experiencing liquidity issues.

Brazil at the transaction level with high frequency.[24] In 2019 and 2020, over 25 thousand firms received funds from exports. In 2019 (2020), these firms were recipients of 19.86 (20.39) thousand operations with an aggregate value of R$ 0.20 (0.19) trillion, equating to 3% of Brazil's 2019 GDP (3% of Brazil's 2020 GDP).

We aggregate these firm-specific income streams to the locality level using information from the firm's geographical position from the *Receita Federal do Brasil*, the Brazilian IRS. We run the following locality-time econometric specification:

$$\text{Income}_{l,t} = \alpha_l + \alpha_{g(l),t} + \beta \cdot \text{COVID-19}_t \cdot \text{COVID-19 Intensity}_l + \epsilon_{l,t}, \tag{9}$$

in which l and t index locality and time (January 2019 to June 2021, monthly). We use the following locality-level income of firms as the dependent variable: (i) aggregate of all streams mentioned above, (ii) credit and debit cards, (iii) invoices, (iv) exports, and (v) wire transfers. The variable COVID-19 Intensity$_l$ follows (2), and COVID-19$_t$ is a dummy variable that assumes the value of one when the year is 2020 or 2021, and zero otherwise. The term α_l represents locality fixed effects that absorb any time-invariant, non-observable, and locality-specific characteristic. We use the time-variant fixed effects $\alpha_{g(l),t}$ to make within-comparisons of localities within the same macrolocality *and* with similar *per capita* GDP (discretized in terciles) levels as in our baseline regressions. We cluster errors at the locality level, which coincides with the level of variation of our COVID-19 intensity measure. We apply a standardization procedure to all numeric variables. The variable $\epsilon_{l,t}$ is the error term.

Our coefficient of interest is β in (9). We interpret the coefficient as the *relative effect* of a one-standard-deviation increase in the COVID-19 intensity on the locality-level income compared to other localities within the same macrolocality and similar *per capita* GDP levels.

Specifications I–V of Table XI report the coefficient estimates of (9). A one-standard-deviation increase in COVID-19 intensity (4%) caused a 0.0248 standard deviation reduction in the locality's

[24] We exclude inter-company operations as they are more related to liquidity or investment opportunities managed by the firm conglomerate. The *Sistema de Câmbio* can only capture operations in which there are inflows of funds from abroad to Brazil. Therefore, we do not observe financial resources maintained (paid by the importer) abroad in international accounts. Since we do not have information about the money destination, we may also include receipts unrelated to purchases, such as capitalization of companies by partners or third parties, sales of non-operational assets, and marketplace operations.

income when we consider all income streams (Spec. I). This effect is economically significant, corresponding to a decrease of $0.0248 \cdot$ R$ $10,918$ million $\approx$ R$ 272 million or around 19% of the sample average. Even though a higher COVID-19 intensity negatively affected all firm income channels, its effect was statistically significant for credit and debit cards and invoices only, which have widespread use by households to settle purchases with businesses. While common in the corporate segment, wire transfers are used by households only for relatively large-valued operations. Exports are frequently limited to firms.

TABLE XI. *How does COVID-19 affect local economic activity?*

Sample:	All Firms					Firms without Branches				
Dependent Variables (Inflow):	All	Cred/Deb Cards	Invoices	Exports	Wire Transfers	All	Cred/Deb Cards	Invoices	Exports	Wire Transfers
Model:	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)
Variables										
COVID-19$_t$	-0.0248***	-0.0092***	-0.0098***	-0.0083	-0.0059	-0.0215***	-0.0161***	-0.0123***	-0.0220	-0.0074
× COVID-19 Intensity$_l$	(0.0058)	(0.0034)	(0.0032)	(0.0172)	(0.0038)	(0.0055)	(0.0049)	(0.0045)	(0.0281)	(0.0058)
Fixed effects and controls										
Locality	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time · Macrolocality · *Per capita* GDP(3)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics										
Observations	13,514	13,514	13,514	9,359	13,514	13,514	13,514	13,514	6,184	13,514
R^2	0.9920	0.9971	0.9982	0.9147	0.9929	0.9934	0.9973	0.9975	0.9229	0.9945

Note: This table reports coefficient estimates of the specification in (9) using monthly data from January 2019 to June 2021 at the locality-time level. The dependent variable takes the aggregate firm income considering the following inflow streams: (i) all streams (Specs. I and VI), (ii) credit and debit cards (Specs. II and VII), (iii) invoices (Specs. III and VIII), (iv) exports (Specs. IV and IX), and (v) wire transfers (Specs. V and X). We report results when we aggregate all firms within the locality (Specs. I–V) and only firms without branches (Specs. VI–X). The variable COVID-19 Intensity$_l$ is the average number of COVID-19 infectious residents as a share of the locality's population in 2020, and COVID-19$_t$ is a dummy variable that assumes the value of one when the year is equal to 2020 and zero, otherwise. We use locality and time-macrolocality-discretized *per capita* GDP fixed effects in all specifications. Coefficients represent the change in the dependent variable in terms of standard deviations for a one-standard-deviation increase in the independent variable. One-way (locality) standard errors in parentheses. *, **, *** denote statistical significance of 10%, 5%, and 1%, respectively.

To examine the parallel trends assumption, we also provide a case study of the time-varying COVID-19 effect on the locality's income. For that, we replace the step variable COVID-19$_t$ in (9) with monthly pulse dummies. Figure 8 depicts the β coefficient for each month from January 2019 to June 2021. Overall, the differences were statistically insignificant before 2020, corroborating the similarities between the compared groups. There is a strong break in the trend of credit and debit cards in March–April 2020, coinciding with the implementation of social distancing and quarantine health measures by local authorities to mitigate the COVID-19 spreading.

We assign the income to the locality of the firm's headquarters. However, it is reasonable to assume that large firms could centralize income in a specific plant to enjoy gains of scale while offering products and services in a decentralized way. Ideally, we would like to assign the income to

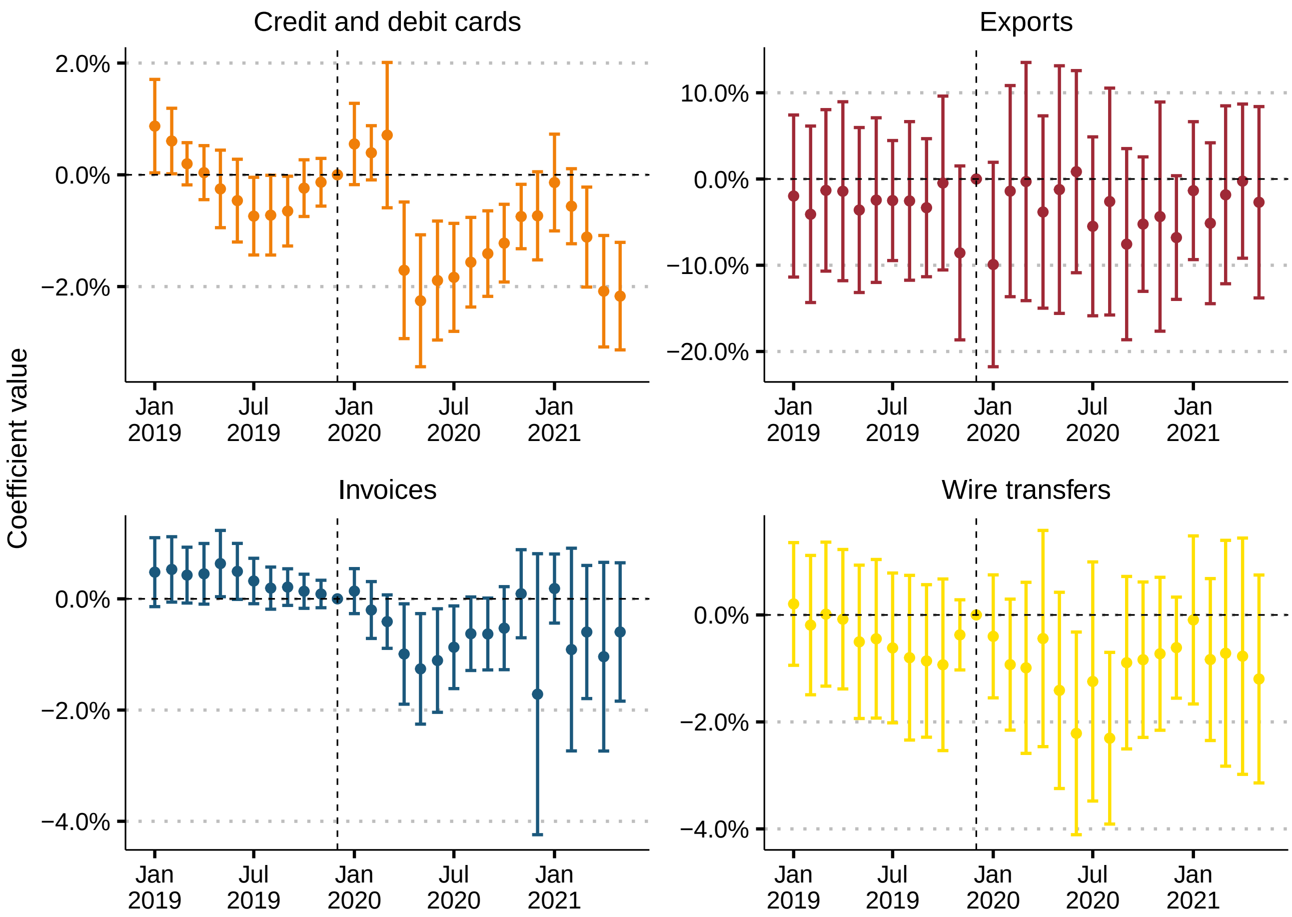


Figure 8. *Time-varying effects of COVID-19 prevalence in Brazilian localities for credit and debit cards (upper left), invoices (bottom left), exports (upper right), and wire transfers (bottom right). We run the specification in (9) but changing the step variable COVID-19$_t$ for monthly pulse dummies. Each point represents the β coefficient for a specific month from January 2019 to May 2021. Vertical bars denote the 95% confidence interval.*

the locality where the firm produced the goods or services. While we lack this type of information, we can mitigate this concern by running regressions only with firms without branches. We again use information from the *Receita Federal do Brasil* to identify the number of branches for each active Brazilian non-financial firm. Then, we only retain firms without branches before aggregating income at the locality level. Specifications VI–X rerun (9) using only firms without branches. The results remained unchanged. The reduction in credit and debit cards and invoices was even stronger for these firms, which mainly deal with final consumers who settle their purchases with such payment media.

6.2 Other market power measures as alternatives to the Lerner index

This paper uses a version of the Lerner index for local credit markets, which is very data-intensive and requires some assumptions about the functional form of the bank branches' local total cost function. This section revisits our baseline regressions reported in Specs. III of Tables III (without IT covariates) and VIII (with IT covariates) but using alternative measures that are less data-intensive and require fewer assumptions. This analysis aims to give a sense of the marginal value of using the Lerner index over other simpler approaches. Additionally, it enables us to understand the implications of using easier to compute but conceptually less appropriate measures. We hold our sample constant as in the baseline results to enable comparability of both approaches.

Table XII reports coefficient estimates for two alternative measures for market power as dependent variables: the bank branch's local market share in a specific credit modality (Specs. I–II) and the HHI of these local market shares across bank branches in a specific locality (Specs. III–IV). The higher the local market share and HHI (more concentrated), the higher the local market power tends to be. We apply the same empirical specifications as in Spec. III of Tables III and VIII for the first measure, because it also varies at the bank-locality-modality-time level. The second measure varies at the modality-locality-time level because we aggregate across bank branches. Therefore, we create a locality-specific measure of IT readiness by using the average IT spending as a share of the total costs of banks operating in that locality, weighted by their local volume of credit. Additionally, we exchange the fixed effects time-bank-macrolocality-*per capita* GDP for time-modality-macrolocality-*per capital* GDP (terciles).

We find that the bank branch's local market shares decreased in localities more affected by COVID-19 when compared to the same bank operating in other but similar localities in the same credit modality market (Specs. I and II) in the first year of the pandemic. Credit became more diversified (less concentrated) in localities more affected by COVID-19 than other similar localities (Specs. III and IV). Both results suggest a decrease in the local market power in localities more affected by COVID-19, which is consistent with our baseline results. However, these more straightforward measures do not tell us the channels through which market power can change. This feature is a substantial advantage of the Lerner index over these measures, which one may have to ponder when deciding on using complex or more straightforward measures to evaluate market power. On the one hand, the Lerner index provides insightful information on the main drivers of local market power changes, which may be critical for policymaking. On the other hand, it requires microdata and assumptions over the bank branch's production functions.

TABLE XII. *Robustness tests: using alternative measures of market power to the local Lerner index.*

Dependent Variables:	Credit Market Share$_{bmlt}$		Credit HHI$_{mlt}$	
Model:	(I)	(II)	(III)	(IV)
Variables				
COVID-19$_t$				
× COVID-19 Intensity$_l$	-0.0080**	-0.0112***	-0.0141**	-0.0118*
	(0.0040)	(0.0042)	(0.0072)	(0.0064)
× COVID-19 Intensity$_l$ · % IT Cost$_b$		0.0102*		
		(0.0061)		
× COVID-19 Intensity$_l$ · Locality's IT Readiness$_l$				-0.0057
				(0.0072)
Fixed effects and controls				
Locality	Yes	Yes	Yes	Yes
Time · Bank · Modality · Macrolocality · *Per capita* GDP(3)	Yes	Yes	No	No
Time · Modality · Macrolocality · *Per capita* GDP(3)	No	No	Yes	Yes
Other controls and interactions?	Yes	Yes	Yes	Yes
Fit statistics				
Observations	78,500	78,500	43,164	43,164
R^2	0.8968	0.8968	0.8127	0.8127

Note: This table reports coefficient estimates for variations of the specifications in Equation (3) (Spec. I and III) and (7) (Specs. II and IV) using semiannual data from 2019 to 2020. We use the following dependent variables: the bank branch's local market share in a specific credit modality (Specs. I–II) and the HHI of these local market shares across bank branches in a specific locality (Specs. III–IV). All controls (coefficients not shown) for the empirical setup in Specs. I and III (II and IV) follow Spec. III of Table III (Table VIII). Locality's IT Readiness is the average IT spending as a share of the total costs of banks operating in that locality weighted by their local volume of credit. Fixed effects of Specs. I and II follow Spec. III of Table III. Fixed effects of Specs. III and IV are similar except that time-bank-macrolocality-*per capita* GDP is changed for time-modality-macrolocality-*per capital* GDP (terciles). Coefficients represent the change in the dependent variable in terms of standard deviations for a one-standard-deviation increase in the independent variable. One-way (locality) standard errors in parentheses. *, **, *** denote statistical significance of 10%, 5%, and 1%, respectively.

6.3 Alternative less saturated specifications

This section revisits our baseline regressions reported in Specs. I–III of Tables III (without IT) and VIII (with IT) but dropping controls or employing less saturated regressions. We hold our sample constant as in the baseline results to enable comparability of both approaches. Table XIII reports coefficient estimates for these baseline regressions when we remove the original controls (Specs. I–VI) and use a less saturated set of fixed effects (Specs. VII–XII). In the second case, we retain only time-bank fixed effects, such that we still examine how the *same* bank changes its financial outcomes across localities. Our results remained qualitatively the same, implying they are not dependent on controls, which can be noisy, or a comprehensive set of fixed effects.

7 Conclusions

This paper investigated how the COVID-19 pandemic and bank digitalization—measured in terms of the share of IT spending in terms of the total costs in the pre-pandemic period—affected market power in Brazilian credit markets during the first year of the pandemic. We use a multi-

TABLE XIII. *Robustness tests: how does COVID-19 affect local market power and its components? How does digitalization change these results?*

Robustness Test Type:	No controls, same fixed effects						Same controls, less fixed effects					
Dependent Variables:	Price_{bmlt}		$\text{Marginal Cost}_{bmlt}$		Lerner_{bmlt}		Price_{bmlt}		$\text{Marginal Cost}_{bmlt}$		Lerner_{bmlt}	
Model:	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)	(XI)	(XII)
Variables												
COVID-19_t												
$\times$ $\text{COVID-19 Intensity}_l$	-0.0028	-0.0049	0.0225***	0.0261***	-0.0293***	-0.0327***	-0.0026	-0.0002	0.0152***	0.0170***	-0.0241***	-0.0240***
	(0.0036)	(0.0041)	(0.0070)	(0.0073)	(0.0086)	(0.0087)	(0.0037)	(0.0045)	(0.0051)	(0.0051)	(0.0063)	(0.0061)
$\times$ $\text{COVID-19 Intensity}_l \cdot \%\ \text{Local IT Cost}_{bl}$		0.0040		-0.0216***		0.0192**		-0.0034		-0.0156***		0.0112*
		(0.0042)		(0.0066)		(0.0081)		(0.0033)		(0.0049)		(0.0062)
Fixed effects and controls												
Other Controls?	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Time · Bank	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Locality	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No	No
Time · Bank · Modality · · Macrolocality · *Per capita* GDP(3)	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No	No
Fit statistics												
Observations	79,782	79,782	79,782	79,782	79,782	79,782	79,052	79,052	79,052	79,052	79,052	79,052
R^2	0.9237	0.9238	0.7645	0.7646	0.7681	0.7681	0.1481	0.1482	0.0494	0.0496	0.0711	0.0714

Note: This table reports coefficient estimates for robustness tests in which we remove controls (Specs. I–VI) and use only time-bank fixed effects (Specs. VII–XII) from the baseline regressions in Specs. I–III of Tables III (without IT, odd-numbered specifications in this table) and VIII (with IT, even-numbered specifications in this table). All the remaining empirical setup follows those tables. Coefficients represent the change in the dependent variable in terms of standard deviations for a one-standard-deviation increase in the independent variable. One-way (locality) standard errors in parentheses. *, **, *** denote statistical significance of 10%, 5%, and 1%, respectively.

product Lerner index evaluated in a decentralized way (branch level), allowing us to identify the effects of COVID-19 shocks across localities. The Lerner index allows us to understand changes in market power through variations in effective prices and marginal costs, providing better interpretability of the empirical results.

Our identification exploited the different intensities and timings in which Brazilian localities were affected by COVID-19 during the first year of the pandemic. We resorted to a within-bank and across-locality empirical strategy for the same credit modality market. Our empirical setup views COVID-19 as a local demand shifter. We also control for government programs designed to combat the economic effects of the pandemic since they can influence credit-taking decisions.

We found that COVID-19 did not affect effective prices in the first year of the pandemic. However, there were substantial changes in the effective price components: the volume of credit income and concessions. Our results showed that the reduction in credit concessions offset the corresponding decrease in credit income, resulting in unaltered effective prices. We also found that the pandemic increased banks' marginal costs. The decline in the volume of credit concessions and the increase in marginal costs in 2020 suggest banks could not reduce their total costs in the short term, highlighting the stickiness of banks' total cost structure due to economic stickiness and legal and financial frictions. Combining these results, we conclude that the pandemic reduced the market power of Brazilian banks through the marginal cost channel in the first year of the pandemic. We also performed robustness tests with alternative, less data-intensive (and informative) market power measures.

We also examined the advantages of bank digitalization and its impact on market power during the pandemic. Despite the rigidity in total costs for the average bank, more digitalized banks had greater cost flexibility during the first year of the pandemic, primarily through lower funding costs. Digitalization also provided lending flexibility. More digitalized bank branches increased the volume of credit concessions and the percentage of clients living outside the bank branches' locality, particularly in remote localities less affected by COVID-19. Given the same level of COVID-19 intensity, bank branches expanded more in remote localities that were less wealthy, more populated, and where the bank branch had higher market power, all relative to the bank branch's locality. Finally, our findings revealed that the competitive advantage provided by digitalization was mainly based on credit modalities that were not traditionally settled online and required more advanced computational systems that more digitalized banks could support at the beginning of the COVID-19 outbreak. Our results highlight the role of digitalization in allowing bank branches to be less sensitive to local borrowers' conditions.

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