Financial Inclusion, Economic Development, and Inequality: Evidence from Brazil

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July 2023

Abstract

We study a financial inclusion policy targeting Brazilian cities with low bank branch coverage using data on the universe of employees from 2000–2014. The policy leads to bank entry and to similar increases in both deposits and lending. It also fosters entrepreneurship, employment, and wage growth, especially for cities initially in banking deserts. These gains are not shared equally and instead increase with workers' education, implying a substantial increase in wage inequality. The changes in inequality are concentrated in cities where the initial supply of skilled workers is low, indicating that talent scarcity can drive how financial development affects inequality.

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‡We thank Victor Duarte and Chenzi Xu for their unfailing support and numerous discussions. We also thank Daniel Carvalho (discussant), Shawn Cole (discussant), Tatyana Deryugina, Pascaline Dupas, Melanie Morten, Chad Jones, Dean Karlan (discussant), Ben Moll, Tarun Ramadorai, Rebecca de Simone (discussant), Amit Seru, Yongseok Shin, Tracy Wang (discussant), and seminar participants at Stanford Economics, Stanford GSB, SITE Financial Regulation, the Finance-Organization-and-Markets Conference at Dartmouth, WashU Olin, Boston College, Queen Mary University, University of Pittsburgh, FGV-EPGE, Insper, University of São Paulo, HEC-Paris, the Brazilian Econometric Society Seminar, Cheung Kong University GSB, the Bank of Lithuania, University of Georgia, University of Arizona, Imperial College, Australian National University, the Women in Applied Microeconomics conference, the Wabash River Finance conference, the Central Bank of Brazil, NBER-SI, Central Bank of Italy, Tuck Dartmouth, McDonough-Georgetown, FED-board, John Hopkins Carey business school and Berkeley University for helpful comments. Filipe Correia, Thomás, Gleizer Feibert and Pablo Enrique Rodriguez provided superbe research assistance.

1 Introduction

The presence of bank branches at fine geographical levels has long been considered a key determinant of financial inclusion and an important driver of economic growth. Bank branch proximity mitigates the transaction costs of mobilizing savings from many agents, which can increase the capital available to entrepreneurs. Branch proximity also lowers the cost to banks of screening and monitoring entrepreneurs, and of providing access to liquidity services. However, setting up bank branches is not only costly but also risky, as demand for deposits and loans can only be observed after the creation of the branch, which can lead to an under-provision of financial services.

For this reason, policymakers across the world have sought to promote financial inclusion by implementing large-scale reforms to expand the physical networks of bank branches.[1] The popularity of these financial inclusion policies raises multiple questions: do they succeed in promoting financial and economic development and, if so, how? And what are the distributional consequences of such policies?

In this paper, we trace out the dynamic effects on both economic development and wage inequality of a government program that improved access to mainstream financial services. We use the introduction of the "Banks for All" program (*"Banco para Todos"*) by the Brazilian federal government in 2004, which explicitly targeted underbanked cities by introducing branches of government-owned banks. This policy constitutes a unique natural experiment featuring a large, plausibly exogenous shock to financial access and capital deepening at the level of entire labor markets.

Our empirical analysis combines Brazilian administrative employer-employee data covering the universe of formal employees in Brazil with detailed bank branch balance sheets from 2000–2014. In a difference-in-differences research design, we compare the evolution of various outcomes in cities benefiting from this policy (those with no government-owned banks prior to the reform) relative to unaffected cities. We use a parsimonious matching procedure to select control cities for each treated city, where we match on the pre-reform population quintile and Gini growth, and we estimate the effect of financial development on employment, entrepreneurship, firm growth, average wage, and wage inequality.

Our identification strategy exploits ex-ante differences in the presence of government-owned banks across cities, but it does *not* require the initial presence of government-owned banks to be random. It only requires that outcomes of treated and control cities would have evolved similarly absent the reform. While, by definition, this identifying assumption is untestable, we provide a battery of tests that are supportive of it, which we discuss in detail

1. Examples include China in the 1970s, India in the 1980s, Thailand in the 1980s and 1990s.

after summarizing our results. Specifically, we: (i) show evidence of pre-reform parallel trends for our key city-level outcomes, (ii) show that our matching procedure leads to covariate balance across a wide array of city-level characteristics not targeted by the matching, (iii) directly control for city characteristics pre-reform, (iv) employ a city-by-industry difference-in-differences estimator to control for sector-specific shocks that could differentially impact cities exposed to the policy. The stability of point estimates across the different strategies implies our results cannot be explained by differential exposure to aggregate or city-specific shocks.

We start by showing that the reform leads to financial inclusion and financial development. After 2004, the number of bank branches increases in treated cities, leading to an inflow of local deposits and an increase in credit supply of similar magnitude. The similar expansion in local deposits and credit points to an increase in the amount of capital available to local firms, rather than a reallocation of capital to treated cities from other regions of the country.[2]

The increases in total bank branches, deposits, and credit do not mean revert but instead shift to a new, higher steady state. This implies better access to external funding and liquidity services, both of which can foster economic development.[3] Consistent with the policy driving this shift in the steady state of financial development, these increases are driven by government-owned banks, whose expansion only modestly crowds out private banks. The absence of an effect on private credit can be seen as a placebo test, showing that our results are not driven by differential exposure of treated cities to economy-wide shocks experienced by Brazil during this period.

Our second set of results is about the average effect of the reform on economic development. We show that the reform leads to an increase in employment by 10%, mostly driven by an expansion of smaller firms. Increased labor demand pushes up the average wage per worker by 4.1%. Looking at firm dynamics, we find that the reform-induced bank branch expansion fosters entrepreneurship, as the number of firms increases by roughly 10%. This increase masks an even higher acceleration of underlying firm dynamism, as both firm entry and firm exit rate increase. While these results only reflect patterns in the formal sector, entrepreneurial activity and firm growth in the formal sector are of first-order importance for economic development, as the transition from subsistence to transformational entrepreneurs and the integration of workers in the formal sector foster economic growth (e.g., Dix-Carneiro, Goldberg, Meghir, and Ulyssea, 2021). Nonetheless, we show in robustness checks that our results are not driven by firms and workers moving from the informal sector to the formal sector.

2. As additional evidence that capital is not flowing from control cities to treated cities, we show that credit growth does not slow down in control cities around the time of the reform.

3. See Yang and Xu (2022) for an example of how access to liquidity services thanks to bank branch expansion promoted development in 19th-century US.

The richness of our data allows us to examine the mechanisms that link financial inclusion and economic development. Financial inclusion could foster growth by increasing aggregate demand by improving households' access to credit and by allowing households to better smooth their consumption, thereby reducing the need for precautionary saving. We rule out this local demand channel as the main driver of our results by showing that employment growth is mostly driven by firms in the tradable sector, which are by definition less dependent on local demand.

So why would financial inclusion promote entrepreneurship and firm growth? We show that the policy-induced increase in financial inclusion works by reducing the physical distance between banks and entrepreneurs. Indeed, the positive effect of the reform increases proportionally with the distance between treated cities and the closest city with a bank branch prior to the reform and is larger for smaller firms, which likely face bigger informational frictions. These gains are the same whether the nearest bank is private or public, suggesting that our results are not driven by particulars of how public banks operate (e.g., by access to subsidized or politically-motivated loans). Instead, these results are consistent with the distance to bank branches affecting access to liquidity, and with models in which the distance between borrowers and lenders affects the cost of credit either because it reduces screening and monitoring costs (e.g., Greenwood and Jovanovic, 1990; Townsend and Ueda, 2006), in particular when soft information is prevalent (e.g., Petersen and Rajan, 1994,; Hombert and Matray, 2017).

Our third set of results is about the distributional effects of the reform. We find that the policy leads to a sizable increase in wage inequality within treated cities. This is explained by the fact that, although all workers are better off after the reform, the magnitude of wage gains rises monotonically with the position of workers in the wage distribution. Our detailed panel data of workers allow us to show that this increase is not driven by a change in the sample composition, but instead reflects an increase in wages holding fixed individuals' sex, age, education, occupation, and sectoral specialization. We also show that our results are quantitatively unchanged when we restrict our sample to workers that we observe throughout the sample period and to firms already in the data prior to the reform. These sample restrictions allow us to show that our results are not driven by workers entering the formal sector after the reform or more general changes in the sample composition of worker characteristics.

We then explore two explanations that can account for the rise in inequality. First, financial development could increase the relative demand for skilled labor, either because of a large fixed component to the cost of skilled labor (e.g., Benmelech, Bergman, and Seru, 2021; Schoefer, 2021) or because the relative productivity of skilled workers increases with financial development (Fonseca and Doornik, 2021). Models that assume that financial development increases the relative productivity of skilled workers or loosens constraints on the demand for skilled workers generally predict that the equilibrium skill mix should change, with firms increasing the share of skilled workers in their workforce. However, when looking at the effect of the policy on the average skill composition of firms, we find that the share of skilled workers does not increase in treated cities.

Instead, we find support for another explanation: skills are scarce, especially in developing countries, which means that the supply of skilled workers is more inelastic than that of unskilled workers in the short run. We show that cities in our setting are characterized by high internal migration costs and that the reform does not induce worker migration to treated cities. This lack of inter-city mobility implies that an increase in labor demand can only be served by the supply of local workers. Consistent with skilled workers being in short supply, we find that all the increase in inequality is concentrated in cities where a lower fraction of the population is educated prior to the reform.

We consider a wide range of robustness checks. We start by showing that our results are quantitatively unchanged when we use different matching procedures. We then discuss threats to identification. Our strategy faces two key threats. First, even in the absence of pre-trends, treated cities may be ex-ante different in ways that differentially expose them to aggregate shocks post-2004. That would be the case if, for instance, treated cities are ex-ante more exposed to the commodity boom of the mid-2000s. Second, our policy might have coincided with shocks that specifically affected treated cities, such as idiosyncratic shocks to banks entering treated cities or a targeted expansion of welfare programs.

We address the threat produced by ex-ante differences in three ways. (i) We show that our matched treatment and control groups are similar over a rich array of city characteristics that were not included in the matching process, including exposure to the commodity sector, skilled employment, political affiliation, size of the informal sector, or the co-movement of local GDP with aggregate fluctuations. While common support in *levels* is not required for differences-in-differences designs, such similarity makes the common-trend assumption more plausible, as these similarities in the level of characteristics make it less likely that they reacted differently to broader macroeconomic shocks post-2004.

In addition, (ii) we show that our results are quantitatively unchanged after directly controlling for a wide range of pre-reform controls interacted with year fixed effects. Estimating all possible combinations of pre-reform controls across the hundreds of different specifications yields very similar point estimates. Finally (iii), we exploit the granularity of our data to build a city-by-industry difference-in-differences estimator. This allows us to include industry-by-year fixed effects and non-parametrically control for any unobserved time-varying sector-specific shocks (e.g., commodity booms or trade shocks). Our coefficients of interest are estimated in this case by comparing the *same* sector across treated and control cities, and therefore this strategy does not require that treated and control cities are similarly exposed to sector-specific shocks. We show that point estimates at the city-industry level are quantitatively similar to city-level estimates.

Our setting also addresses a wide array of potential ex-post treated-specific shocks because, by construction, control cities already have a government-owned bank. Therefore, any shocks specific to government-owned banks (such as an overall increase in lending by public banks) will affect both treated *and* control cities at the same time and will be absorbed by our difference-in-differences specification. This design also addresses the possibility that welfare program expansions might differentially benefit treated cities, since some of the largest welfare programs are distributed by one of the public banks already present in control cities (including the largest at the time, *Bolsa Família*). Therefore, an expansion of these programs would also affect both treated and control cities. In additional robustness tests, we show that results are robust to including state-by-year, which controls for state-level shocks such as differences in state-administered welfare programs. At the municipality level, our effects are unchanged when we compare treated and control cities with the same political affiliation or directly control for the observed changes in expenditures.

We end the paper by discussing how our reduced-form identified coefficients can provide useful causal moments for the macro-finance development literature and speak to potentially important frictions or sources of heterogeneity that future models could incorporate. Our paper shows the importance of explicitly linking distance to the nearest bank to the cost and availability of credit as in the structural model of Ji, Teng, and Townsend (2023). We provide causal estimates of how changes in distance can affect credit supply and savings in interest-bearing products, as well as their impact on employment, firm growth, and firm entry. We also provide moments linking changes in the supply of credit and real outcomes, which relate to key parameters in macro-development models in which a reduced-form collateral constraint affects economic growth. The considerably larger effects we find for treated cities in banking deserts point toward the existence of a non-linearity around very low levels of external finance, something that is usually not explicitly modeled and could help to reconcile different results in the literature. Finally, our paper highlights the importance of worker heterogeneity and constraints on the supply of human capital in accounting for the dynamics of wage inequality as a result of financial development in macro-finance models.

Literature Our paper contributes to several strands of literature. The closest one is on the role of financial inclusion on financial intermediation costs and economic development, which plays a central role in macro-development models, but for which we have limited causally estimates elasticities that can be tightly linked with model parameters.[4]

Our setting allows us to better understand why financial inclusion fosters economic growth and provide new insights on the distributional effects of such policies. First, our high-quality administrative data allow us to study many new margins through which financial inclusion policies can foster development. Because we can observe the universe of formal economic activity and track workers and firms over time, we can study a host of new outcomes: the process of creative destruction (firm entry and exit rate), the evolution of firm size distribution and the differential benefit for small vs. large firms measured pre-reform, as well as the evolution of sectoral composition (and in particular the relative growth of tradable vs. non-tradable sectors). Because we observe the universe of workers and can track them over time, we can also shed light on the evolution of the skill premium and wage inequality, accounting for changes in sample composition, and the interaction between financial inclusion policies and constraints on the accumulation of human capital.

The literature on the effect of bank branch presence has so far mostly focused on developed economies and studied financial outcomes such as credit or wealth accumulation. The literature on developing countries has mostly focused on the introduction of specific bank branches in localized markets and used short-run cross-sectional survey data to study real outcomes and find small, short-lived positive effects or even negative effects.[5] By contrast, our administrative panel data allow us to track the long-run effect of the policy on the *full causal chain* linking financial inclusion to financial development (credit supply, access to liquidity services); real outcomes (employment, sectoral growth, average wages, entry and exit rates of firms, firm size distribution) and the distributional consequences of this financial-inclusion driven economic growth.

Second, the policy-driven expansion of bank branches happens through profitable, publicly owned commercial banks rather than development-focused state banks with specific sectoral and poverty reduction mandates as in the Indian experiment (Burgess and Pande, 2005; Cole, 2009). The policy also induced large variation at a sufficiently large geographical level to estimate "local general equilibrium effects," but sufficiently small to provide precise estimates.[6]

4. See for instance Greenwood and Jovanovic (1990); Greenwood, Sanchez, and Wang (2010); Ji, Teng, and Townsend (2023), where geographical distance to a bank branch governs financial intermediation costs and the returns to saving. We provide a clear discussion of the connection between our estimates and the macro-development models in Section 8

5. See Bruhn and Love (2014); Burgess and Pande (2005); Fe Cramer (2022) for positive effects and Kochar (2011) for negative effects. A complementary approach exploits randomized controlled trials to study the implications of access to microcredit and savings products in developing countries. The literature on microcredit is surveyed in Banerjee, Karlan, and Zinman (2015), which concludes that microcredit has "modestly positive, but not transformative, effects".

6. By contrast Bruhn and Love (2014) studies the opening of outlets of a bank specialized for low-income in supermarkets, while Burgess and Pande (2005) uses state-level variations across Indian states. More generally, most of the literature on developing countries has used randomized controlled trial and shocks to specific banks

This level of analysis coupled with data on the universe of bank branch networks in Brazil allows us to provide the first causal estimate of the role of distance to financial services, a key parameter in macro-development models that analyze the role of banks, and to highlight that financial inclusion is not a dichotomous concept but should instead be thought of as continuous.

Taken together, our results have several policy implications. First, financial inclusion policies can generate large effects on economic development, even while banks operate profitable branches. Second, these policies can have large distributive effects, in particular as they interact with constraints on human capital accumulation. Third, the initial distance to existing bank branches matters for the expected gains in terms of economic development. Therefore, financial inclusion policies can reap larger benefits at lower costs by carefully taking into account the initial network of bank branches, which is important given the large number of policies around the world attempting to foster economic development by promoting financial inclusion.

This paper also contributes to the empirical literature using natural experiments to show how financial frictions, broadly defined, affect economic development.[7] Most of the evidence for developing countries studies short-run capital injections that originate outside the city and focuses on changes in credit, *holding fixed the network of banks.*[8] Our paper highlights a fundamentally different mechanism and our object of interest is the promotion of financial inclusion by *expanding the network of bank branches.* This branch expansion fosters the mobilization and pooling of local savings to start a virtuous circle between increased deposits, higher credit, and economic development, originating from within the city as the distance between depositors, lenders, and borrowers is reduced. Therefore, our results imply that *how* credit is distributed across places can matter as much as *how much* credit is distributed.

Third, we contribute to the literature that studies the effect of financial development on wage inequality. Theoretical work in this literature focuses mostly on wealth inequality or total

with a focus on directly affected bank clients. The positive effects on non-clients are potentially a key driver of multiplier effects, which can account for why we find large positive effects on economic development while most papers find limited effects.

7. An earlier literature looks at how financial frictions relate to economic development using cross-country evidence. This literature is for instance reviewed in Beck and Levine (2018). See Buera, Kaboski, and Shin (2011), Buera and Shin (2013), or Midrigan and Xu (2014) for macro-models linking financial frictions and development and the survey in Buera, Kaboski, and Shin (2015). See also Xu (2022) and **Xu2022** (**Xu2022**) and references therein for the importance of financial frictions in cross-country trade and growth, and long-run historical contexts.

8. For instance shocks to the liquidity of lenders coming from targeted lending programs in India (Banerjee and Duflo, 2014) and Brazil (Bazzi, de Freitas Oliveira, Muendler, and Rauch, 2021), deposit volatility (Choudhary and Limodio, 2022), large government grants in Thai villages (e.g., Kaboski and Townsend, 2011; Kaboski and Townsend, 2012), or broader financial market reforms such as bankruptcy reforms (e.g., Fonseca and Doornik, 2021 for Brazil), collateral laws (e.g., Vig, 2013), or financial liberalization (e.g., Crescenzi and Limodio (2021), Bau and Matray (2023) and references therein).

income inequality (which includes capital income) and derives ambiguous predictions. The effect of financial development depends on whether that development is concentrated on the intensive or the extensive margin (e.g., Greenwood and Jovanovic, 1990, Townsend and Ueda, 2006; Greenwood, Sanchez, and Wang, 2010), how it alters the aggregate demand of workers and investment returns (e.g., Giné and Townsend, 2004; Buera, Kaboski, and Shin, 2021), and whether individuals can accumulate human capital (e.g., Mestieri, Schauer, and Townsend, 2017). These models generally conclude that capital income pushes inequality upward, as it mostly benefits the wealthy and entrepreneurs, while rising wages push inequality downward (e.g., Buera, Kaboski, and Shin, 2021; Ji, Teng, and Townsend, 2023). These theoretical and quantitative results that wage inequality should go down as financial development increases rely on the assumption that labor is a *homogeneous* input to production. Therefore, higher labor demand in more-productive sectors will benefit more lower-paid workers who reallocate away from less-productive sectors.

Our contribution to this literature is twofold. First, we provide rare empirical evidence on the effect of financial inclusion on wage inequality, as empirical evidence focuses on developed countries and studies credit rather than financial inclusion.[9] Second, we show that financial inclusion leads to higher wage inequality in our setting due to skill differentials. Therefore, we show that taking into account labor heterogeneity and limits to human capital accumulation in macro-development models is crucial to better understanding and predicting how policies promoting financial development will affect inequality.

Fourth, this paper contributes to our understanding of how financial frictions impact capital and entrepreneurial talent misallocation and thereby economic development.[10] More broadly, we relate to the literature studying how financial frictions affect firm labor demand and employment outcomes.[11] We contribute to the specific subset of the literature that studies how financial frictions affect the demand for skilled workers and the skill premium in developing countries (Fonseca and Doornik, 2021).[12]

Finally, because the reform we explore relies on the expansion of government-owned banks, we relate to the broad literature studying the economic effects of government ownership of

9. See Beck, Levine, and Levkov (2010) for evidence from the U.S., and a discussion of their results once accounting for staggered D-i-D corrections by Baker, Larcker, and Wang (2022).

10. See, among many others: Giné and Townsend (2004); Townsend and Ueda (2006); Banerjee and Moll (2010); Buera, Kaboski, and Shin (2011); Kaboski and Townsend (2011); Buera and Shin (2013); Midrigan and Xu (2014); Moll, Townsend, and Zhorin (2017); Bau and Matray (2023).

11. See among many others: Peek and Rosengren, 2000; Chodorow-Reich, 2014; Hombert and Matray, 2017; Bai, Carvalho, and Phillips, 2018; Berton, Mocetti, Presbitero, and Richiardi, 2018; Caggese, Cunat, and Metzger, 2019; Greenstone, Mas, and Nguyen, 2020; Baghai, Silva, Thell, and Vig, 2021; Doornik, Gomes, Schoenherr, and Skrastins, 2021.

12. For recent works on financial frictions and the demand for skills in developed countries, see Quincy (2020) and Jasova et al. (2021).

banks (e.g., Sapienza, 2004; Dinç, 2005; Cole, 2009; Carvalho, 2014; Delatte, Matray, and Pinardon Touati, 2020). Most of this literature emphasizes the risk of political capture and the creation of politically motivated credit cycles. We show that such forms of ownership can have positive effects on economic development when the private sector is unable or unwilling to serve underprivileged areas, even in countries where corruption can be high.

2 Institutional background and data

2.1 The Brazilian banking landscape

2.1.1 Situation pre-reform

Brazil has three types of public banks: government-owned banks controlled by the federal government (Banco do Brazil, Caixa Economica Federal, Banco do Nordeste, and Banco da Amazonia), government-owned banks controlled by state governments, and a national development bank (BNDES).

Government-owned banks, in particular the ones controlled by the federal government, differ from most public banks in developing countries and are better described as "government-owned commercial banks." They are profitable and their performance is comparable to that of both foreign and domestic private banks (Mettenheim, 2010).

BNDES, the national development bank, differs substantially from federal and state-owned banks and is much closer to public banks in other developing countries that have been studied previously such as India (e.g., Burgess and Pande, 2005). This bank provides subsidized loans to targeted sectors or even "grants," in the form of loans that are often not reimbursed. Two things are important to highlight about BNDES. First, this is the only bank studied by previous papers that have documented the existence of political influence in banks' behavior in Brazil (e.g., Carvalho, 2014). Second, this bank, as well as state-controlled banks, was not part of the policy and not involved in the branch expansion we study, and its lending was not specifically targeted at treated or control cities. Therefore, in the rest of the paper, we use the term "public banks" to refer to federally-owned banks, which excludes BNDES.

Public banks differ from private banks in some dimensions, such as in having a legal mandate to provide earmarked credit.[13] However in practice, public banks are similar to private banks in their lending practices and the sectors to which they lend. For instance, public and private banks have similar portfolio compositions across credit products and borrowers, charge similar interest rates and face similar delinquency rates (Coelho, Mello, and Rezende,

13. There are also incentives for private banks to provide earmarked credit and, in fact, nearly 40% of outstanding indirect earmarked loans to firms in 2016 were originated by private banks (Ornelas, Pedraza, Ruiz-Ortega, and Silva, 2021).

2013). These banks are also similar along standard balance sheet and income statement measures. As we show in Figure 2, there are no significant differences between branches of public and private banks across a wide range of covariates, including measures of lending, profitability, loan performance, and size.

2.1.2 The Banks for All program

Banks for All (*Banco para Todos*) was a federal government program announced in 2004 as part of the government's 2004–2007 multi-year plan (*Plano Plurianual*). The program was under the purview of the Finance Ministry (*Ministério da Fazenda*) and aimed to provide Brazil's unbanked population with access to financial services and products through the actions of federal government banks, particularly Caixa Econômica Federal and Banco do Brasil.

To achieve the goal of reaching underserved communities, the federal government promoted the physical presence of public banks throughout the country, focusing on cities with no presence of government banks. Figure 1 plots the evolution of municipalities without a public bank branch since 2000 (the dashed red line). Consistent with the effect of the reform, this share is stable until 2004 at 60%, then drops abruptly in 2005 and keeps declining such that in 2014, only 44% of municipalities have no government-owned banks. Figure 1 also reports the share of municipalities without any bank branch (the solid blue line), and shows that the expansion of public banks resulted in a drop in the share of cities without any bank branches.[14]

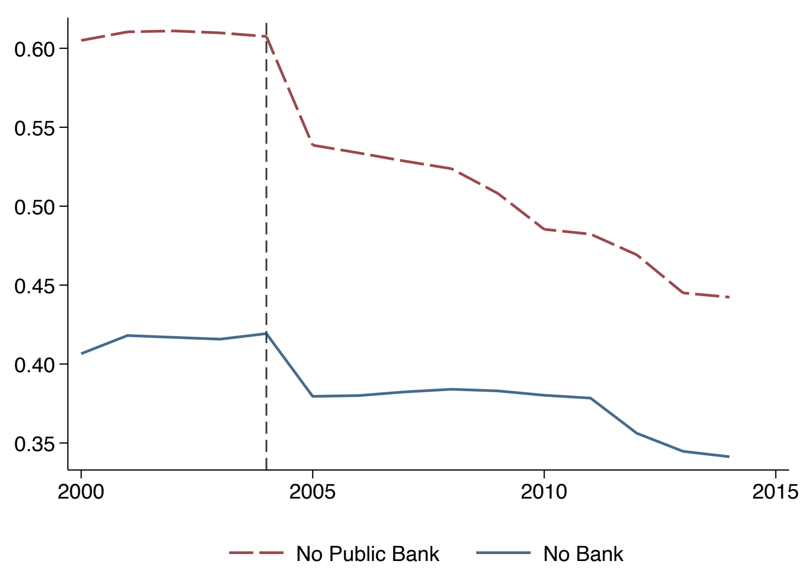
The program succeeded in reaching unbanked cities and underbanked populations. According to an evaluation of the program by the federal government, public banks opened 7.8 million accounts and banked 1.46 million low-income, previously unbanked individuals between 2004–2007 (Ministério da Fazenda, 2007).[15] In Section 4, we formally show that cities without public bank branches prior to 2004 saw a sharp increase in credit and deposits following the introduction of the program.

Banking correspondents. In order to reach unbanked households, the program also relied on correspondent banking outlets. These arrangements consist of banks hiring commercial entities—typically lottery retailers, post offices, pharmacies, and other retailers—to serve as distribution outlets for financial services. Since 2003, financial services offered by correspondents include the opening of accounts, deposits and withdrawals, payments, and loan

14. Between 2004 and 2014 (the end of our sample period), 1,262 new bank branches were open in cities eligible to the reform.

15. For comparison, in 2007, there were approximately 16 million individuals residing in the cities that compose our treatment group.

Figure 1: Share of Municipalities without Bank Branches



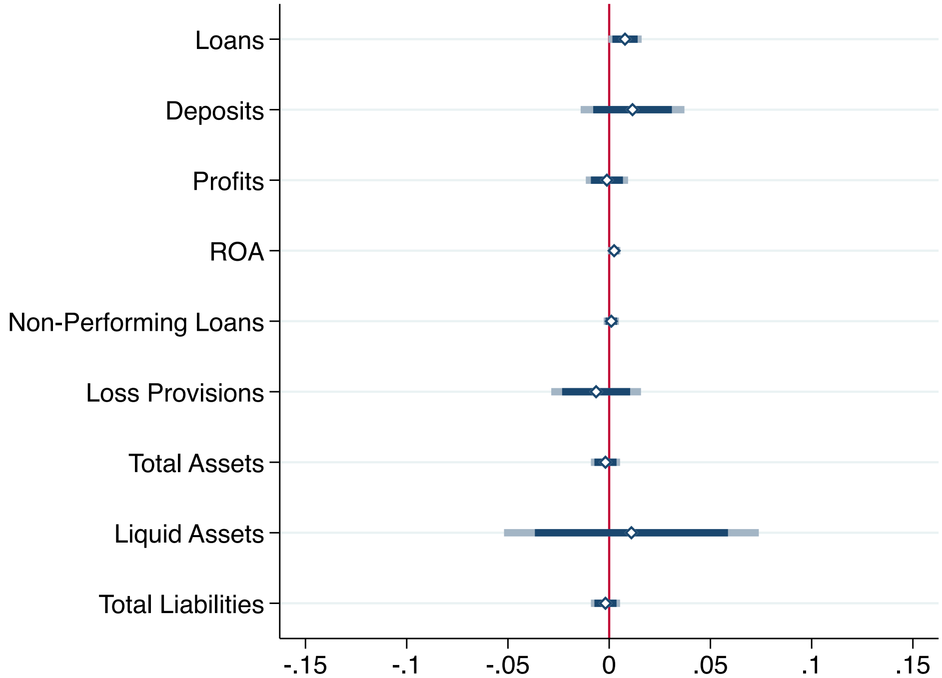
This figure plots the evolution of the share of municipalities without at least one government-owned bank branch in red and the share of municipalities without any bank branches in blue.

applications.[16] The number of correspondents went from approximately 20,000 in 2000 to over 150,000 in 2010 (Loureiro, Abreu Madeira, and Bader, 2016) and, taking into account partnerships with correspondents, government-owned banks were present in 100% of municipalities by 2007 (Ministério da Fazenda, 2007).

While we do not observe the precise location of correspondents, we know that, prior to the reform, they were concentrated in areas already served by bank branches (Loureiro, Abreu Madeira, and Bader, 2016). One notable network of correspondents was that of Banco Postal, which emerged from a 2001 partnership between Bradesco and the Brazilian Post and Telegraph Company to provide financial services at post offices. However, despite its goal of reaching all unbanked municipalities, it was only present in roughly half of unbanked municipalities prior to the reform (Gual and Ansón, 2008). Moreover, prior to 2003, correspondents were not allowed to provide most of the services they offer today. Thus, to the extent that they were available, they mostly provided bill payment services and were not a meaningful substitute for financial institutions and were particularly ill-suited for the needs of firms (Bittencourt, Magalhães, and Abramovay, 2005). Even after 2003, the vast majority of Brazilian households and an even larger fraction of Brazilian businesses did not use correspondent banking outlets for deposits, withdrawals, account openings, or borrowing, and did not view correspondents as safe or trustworthy according to survey evidence (Sanford, 2013).

16. CMN Resolution 3,110 of July 31, 2003.

Figure 2: Public vs. Private Bank Covariate Balance



This figure shows coefficient estimates and 95% error bands of the difference between public and private banks along different variables, using ESTBAN data at the branch-year level between 2000 and 2004. All variables are normalized to have a mean of zero and a standard deviation of one. Standard errors are clustered at the bank level.

Taken together, these findings suggest that banking correspondents were present in many unbanked municipalities prior to the reform but mostly provided bill payment services. While having some access to financial services is certainly better than no access at all, the fact that these services were limited implies that the expansion of branch networks we study led to financial inclusion by improving the quality and scope of financial products and services to which households and businesses have access.

2.2 Data

We use data from four distinct sources. Matched employer-employee data come from the *Relação Anual de Informações* (RAIS), a mandatory annual survey containing information on the universe of tax-registered firms in Brazil. There are severe penalties associated with incomplete or late information, which leads to a high degree of compliance and essentially complete coverage of all employees in the formal sector. RAIS contains time-invariant identifiers for workers and firms, as well as information on where the firm is located. We also observe data on workers average gross monthly earnings, occupation and several socio-demographic characteristics such as their education, race, age, and gender.

Using geographical information on firms, we build a city-level panel from 2000 to 2014 with information on average wages, wage inequality, employment, and skill-specific wages. Because municipality borders have changed over time, we use as our level of aggregation minimum comparable areas (*Área Mínima Comparável*, or AMC), which can be consistently tracked throughout our sample period. This reduces the number of cities from over 5,000 to 4,260. In the rest of the text, we use the term "city" to refer to an AMC.

The number of bank branches, lending activity, and deposits come from the ESTBAN database maintained by the Central Bank of Brazil. The data provides branch-level balance sheet information that we aggregate to the city level, which allows us to decompose the number of branches, credit, and deposits between public and private banks. Note that these data do not include correspondent banking outlets (such as the outlets of Banco Postal), which means that we do not observe the full impact of the program on financial inclusion. We discuss this point further in Section 4.

Finally, we use city-level aggregate data. We obtain time-varying outcomes from the Brazilian Institute of Geography and Statistics (*Instituto Brasileiro de Geografia e Estatística*, or IBGE), and cross-sectional demographic and economic characteristics in 2000 from the Census, such as population distribution across years of schooling and share of workers in informality.

3 Empirical strategy

The reform promoted financial inclusion by targeting cities with no government-owned banks, so we identify treated cities as those that did not have a public bank prior to 2004. This implies that all control cities had a public bank prior to the reform.[17] We can identify the effect of the financial inclusion reform by comparing the evolution of multiple economic outcomes for treated and control cities, before and after the reform, in a difference-in-differences setting. The key identifying assumption is that absent the reform, treated and control cities would have evolved in close parallel. While this identification strategy does not require that treated and control cities be similar in levels prior to the reform, any such similarity makes the common-trend assumption more plausible.

This strategy raises a natural challenge: the average treated city in Brazil does not look like the average untreated city. Since the reform targeted unbanked cities, these tended to be smaller and less developed, and it is possible that they evolved and grew in different ways after the reform relative to other untreated cities for reasons not directly tied to the reform. For instance, they could have disproportionally benefited from the period of sustained growth,

17. Cities with no public bank prior to 2004 represents 43% of Brazilian cities.

partially fueled by a commodity boom, that Brazil entered into during our sample period. Figure 3 plots a covariate balance test and shows that the unconditional difference in levels between treated and untreated cities (green coefficients) is large and significant for most city characteristics.

In order to address this challenge and to strengthen our empirical strategy, we use a parsimonious matching approach to construct a control group of untreated cities that is observably similar to treated cities on a wide set of characteristics.

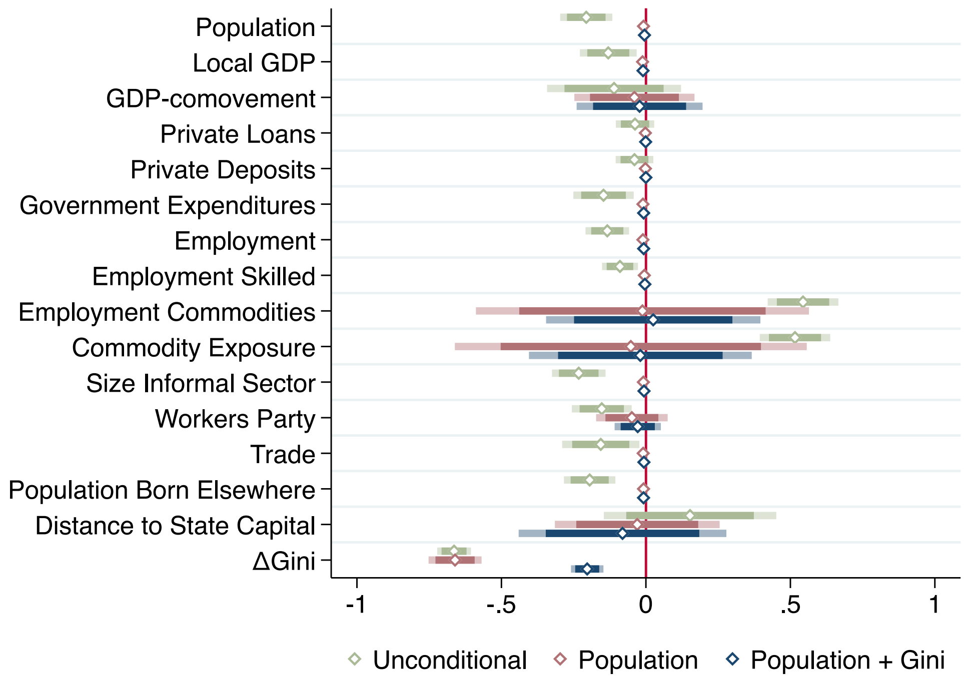
Matching. Our matching strategy first targets city size. We start with all 4,260 cities and compute quintiles of population. We then match each treated city with all control cities in the same population quintile with replacement.[18]. This parsimonious approach addresses a large part of the heterogeneity. The red dots in Figure 3 show that the treatment and control groups are now similar over a rich array of city characteristics constructed pre-reform that were not targeted in the matching process. These characteristics include proxies for economic development (GDP, employment, skilled employment, size of informal sector); propensity to receive social transfers (local government expenditures, political affiliation of the mayor); economic integration (distance to the state capital, share of population born elsewhere, exports and imports made by local firms); exposure to aggregate shocks and the commodity sector (local GDP co-movement with aggregate GDP, total employment in commodities, exposure to commodity prices post-reform); and development of the private banking sector (private loans and private deposits). In addition, while some of the point estimates are not exactly zero, the standardized difference between both groups remains well below the threshold of 0.20 suggested by Imbens and Rubin (2015).

After matching on population quintile, the only remaining large and statistically significant difference between treated and control cities is the change in the Gini index during the pre-period. Since we are interested in understanding how financial development affects inequality, and because Brazil experienced large changes in inequality during this period (e.g., Lopez and Perry, 2008) we also match on changes in inequality pre-reform. We do so by selecting the three control cities in the same population quintile with the closest pre-reform Gini growth. The blue dots in Figure 3 show differences between treated and control cities after we further restrict our matches to this criterion. The difference in Gini growth becomes much closer to the 0.2 threshold and later in the paper, we show that the Gini index of treatment and control units evolved in close parallel prior to the reform, and that there is no evidence of pre-trends (Figure 8).

After our baseline matching procedure, we are left with 1,415 treated cities and a total of

18. Because the same city can be used multiple times as a control, we follow Chaisemartin and Ramirez-Cuellar (2021) and adjust the standard errors by clustering at the matching pair level

Figure 3: Balance Covariate



This figure shows coefficient estimates and 95% error bands of the difference between treated and control cities along different variables. All variables are normalized to have a mean of zero and a standard deviation of one in the full sample. "Unconditional" refers to the sample where we compare treated cities to all untreated cities. "Population" refers to the sample where we match treated cities with untreated cities in the same population quintile pre-reform. "Population + Gini" refers to the sample where we select the three control cities in the same population quintile with the closest pre-reform Gini growth.

3,918 control cities. We report the summary statistics of our final sample in Table 1, and we display the spatial distribution of treated and control cities in Figure 4. Treated and control cities are spread out across Brazil and do not show geographical clustering.

Econometric specification: city level. We analyze the effect of an increase in bank coverage on economic development and inequality by estimating a series of matched difference-in-differences (D-i-D) specifications of the form:

$$Y_{c,g,t} = \beta \; Treated_c \times Post_{t \geq 2004} + X_{c,t} + \; \theta_c + \delta_{g,t} + \; \varepsilon_{g,c,t} \tag{1}$$

where $Y_{c,g,t}$ are various city outcomes for city c at year t that belongs to a matched treated-control group g, and $Treated_c$ is a dummy variable that takes the value one if city c had no government-owned banks prior to 2004. θ_c are city fixed effects that remove time-invariant heterogeneity across cities, and $\delta_{g,t}$ are matched group-by-year fixed effects that controls for time-varying unobserved heterogeneity across groups. Because we select our groups using pre-reform population size and inequality growth, the inclusion of matched group-by-year fixed effect implies that we are absorbing unobserved correlated shocks that might exist between

Table 1: Summary Statistics

	Mean	Med.	St. Dev.	N
Loans / GDP	0.18	0.14	0.17	79,995
Public Loans / GDP	0.16	0.11	0.16	79,995
Private Loans / GDP	0.02	0.01	0.04	79,995
Total branches	1.77	1.00	4.60	79,995
Public branches	0.93	1.00	1.29	79,995
Private branches	0.84	1.00	3.43	79,995
Deposits / GDP	0.13	0.10	0.11	79,995
Public deposits / GDP	0.09	0.07	0.09	79,995
Private deposits / GDP	0.04	0.01	0.06	79,995
Wage	926.41	893.98	273.93	79,995
Total employment	1,056.30	620.00	5,653.74	79,995
Share skilled	0.09	0.09	0.05	79,995
Skill premium	2.28	2.14	0.69	79,901
Gini index	0.31	0.31	0.06	79,995
Population	12,347.19	8,635.00	24,640.18	79,995
GDP per capita	13,478.91	9,630.87	16,685.64	79,995
Share manufacturing	0.20	0.12	0.20	79,995
Share agriculture	0.14	0.09	0.14	79,995

This table reports summary statistics of average city-level characteristics our final sample. Monetary values are in 2010 BRL. Number of bank branches, lending activity and deposits are from the ESTBAN database. Wage, employment, and other labor market variables are from the RAIS database. Local GDP per capita, population, and the share of manufacturing and agriculture in local value added are from the Brazilian Institute of Geography and Statistics.

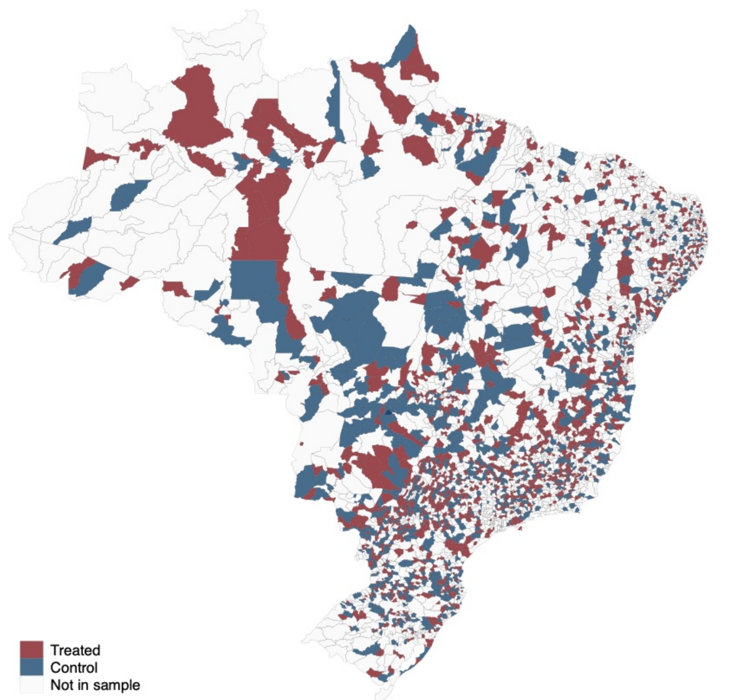
these characteristics and the reform.

For example, concerns that smaller cities may have grown for reasons unrelated to the reform will be addressed because the parameter of interest β is identified solely by comparing cities within the *same* group, i.e., within the same size quintile. Similarly, unobserved shocks to places with larger changes in their Gini prior to 2004 will also be differenced out by these fixed effects. $X_{c,t}$ is a collection of city-level controls that we include in the robustness analysis.[19] We cluster our standard errors at the matching-pair level to account for serial correlation and weight the regression by population size at the beginning of the period to estimate the aggregate effect of the reform on inequality and economic development.

Intent-to-treat estimator. Because our identification strategy assumes that all cities without a public bank branch are treated immediately after 2004, our estimator can be interpreted as an intent-to-treat, where the eligibility rule is whether a city has a public bank branch and the treatment year is 2004. We choose this approach rather than using the actual date when

19. We use the value pre-reform and interact with year fixed effects to avoid the classic problem of “bad controls.”

Figure 4: Geographical Distribution of Treated and Control Cities



This figure shows the geographical distribution of treated and control cities. “Not in sample” refers to cities that are neither treated nor part of the matched control group, and thus not in our final sample.

a public bank branch opens because the latter would imply defining treated and control units based on the ex-post *realization* of the policy, rather than an ex-ante *assignment* rule. Doing so will bias the estimator if the opening of a bank branch after the policy is not random or accidental, but instead the result of systematic differences in behavior or characteristics among cities eligible to the reform.[20]

In this case, it is possible to recover an unbiased estimate of the treatment by ignoring the actual receipt of the treatment and focusing on the causal effect of assignment to the treatment, in an intention-to-treat analysis (Fisher et al., 1990; Imbens and Rubin, 2015). In our setting, we can assume that the policy (the assignment) is exogenous based on the ex-ante characteristic of not having a government bank prior to 2004. But whether or not a government bank branch opens in these a priori treated cities *after* the policy is adopted (i.e., whether the assigned unit takes the treatment) is potentially endogenous to their economic outcomes.[21]

20. This is because the exogeneity of the *assignment* does not bear on the exogeneity of the *receipt of treatment* if the receipt of treatment is different from the assignment to treatment (e.g., Athey and Imbens, 2017).

21. To be able to use a staggered treatment, there would have to exist an official staggered assignment rule but none of the official program documentation we examined suggests this was the case. Therefore, the only unbiased estimator we can implement requires us to consider as treated all cities that were eligible to receive a public bank branch after 2004, in an intent-to-treat analysis.

Identifying assumptions and potential threats to identification. Our identification strategy faces two main threats: (i) Even if treated and control cities are perfectly similar ex-ante, unobserved ex-post shocks might specifically affect the cities that are treated by our financial inclusion policy. (ii) Despite the use of a matching procedure, the variable we use to sort cities into treatment and control groups—the presence of a government-controlled bank—might still be correlated with other city-level characteristics that make treated units more sensitive to aggregate shocks post 2004. We discuss how we address both of these concerns below.

(i) Treated-specific ex-post shocks. Even with perfect ex-ante covariate balance between treated and control cities, the estimated effect of promoting financial inclusion on city-level outcomes could be biased if this policy correlates with other unobserved shocks that specifically affect cities that received the treatment. This is a concern in a setting where no city has a bank and identification is achieved by bank entry in some cities and not others. However, it is important to emphasize that our setting is conceptually different. By construction, public banks are present in all control cities prior to the reform. Therefore, any bank-specific shock after 2004 will affect both treated and control cities.

This setting therefore directly addresses two standard concerns about empirical designs featuring bank entry. First, if public banks experience idiosyncratic shocks that affect their credit supply after 2004 (either due to shocks to their cost of funding or because they face political pressure to extend credit), both treated and control cities will benefit from a credit expansion, and our coefficient of interest will not be biased.[22]

Second, potential correlations between financial inclusion policies and the expansion of other social welfare programs after 2004 are also addressed due to a specificity of the Brazilian institutional context. Most of the large-scale welfare programs, and in particular *Bolsa Família*, are distributed via public banks. Therefore, all cities (including cities in the control group) would benefit from the creation or expansion of such programs. We also conduct more detailed tests about this specific concern in Section 7.2 and find no evidence that it biases our results.

(ii) Covariate balance and ex-ante differences. The second main concern is that ex-ante differences lead treated cities to respond differentially to aggregate shocks.

We address this problem in four ways. First, as we show in Figure 3, using a parsimonious matching estimator allows us to obtain covariate balance across a wide range of proxies for exposure to commodity-driven aggregate growth, economic integration of the city, and

22. To be precise, the coefficient is not biased under the assumption that this bank-specific shock affects all branches of the bank in the same way. This would not be the case if this bank shock affects the bank's branches differentially, for instance, due to age differences across branches.

exposure to welfare programs promoted by left-leaning governments. Second, we show that treated and control cities were on a similar trend before the reform for a host of outcomes (credit, employment, number of firms, inequalities) in Sections 4, 5, and 6. The parallel-trends pre-reform indicate that any remaining unobserved differences that could drive the estimated effects would need to have not mattered before 2004 and only mattered afterward.

Third, we directly control for a collection of additional city-level characteristics. We show in Appendix Tables A4 and A5 that point estimates are very stable to the inclusion of controls such as GDP per capita, employment in the commodity sector, skilled employment, political affiliation of the mayor, trade, distance to the state capital, and the co-movement of local GDP with aggregate fluctuations, as well as to all the different combinations of such controls.[23]

Finally, we exploit the granularity of our data and adapt equation (1) into a D-i-D estimator at the city-by-industry level, which allows us to relax the assumptions needed to identify the effect of the reform. Because we can now include industry-by-year fixed effects and therefore non-parametrically control for time-varying unobserved industry shocks, the effect of the reform remains unbiased even if treated and control cities are unbalanced in their exposure to sector-specific shocks (for instance because treated cities have more employment in the commodity sector).

Specifically, we estimate the regression:

$$Y_{i,c,g,t} = \beta \; Treated_c \times Post_{t \geq 2004} + X_{i,c,t} + \; \gamma_{i,c} + \delta_{i,g,t} + \; \varepsilon_{g,c,t} \tag{2}$$

The key difference in equation (2) relative to our city-level D-i-D is that we can include $\delta_{i,g,t}$, i.e., matched group-by-industry-year fixed effects. These fixed effects mean that β is estimated by comparing the *same* industry across treated and control cities that belong to the same matched group. This implies in particular, that sector-specific level shocks post 2004, such as commodity booms or productivity shocks specific to certain sectors, cannot bias the estimation of β. We report the results and details of the estimation in Section 7.1.

4 Effect on financial inclusion

In this section, we start by describing how the policy affected financial inclusion by fostering bank entry and increased deposits and lending in similar proportion. We then discuss the possible frictions that can explain the patterns we observe in the data and how the reform

23. Given that the reform may have a direct impact on many city characteristics, using time-varying controls would potentially bias our coefficients of interest. This is commonly referred to as the problem of "bad controls" (e.g., Angrist and Pischke, 2008). We address this problem by using the pre-reform value of these controls interacted with year fixed effects.

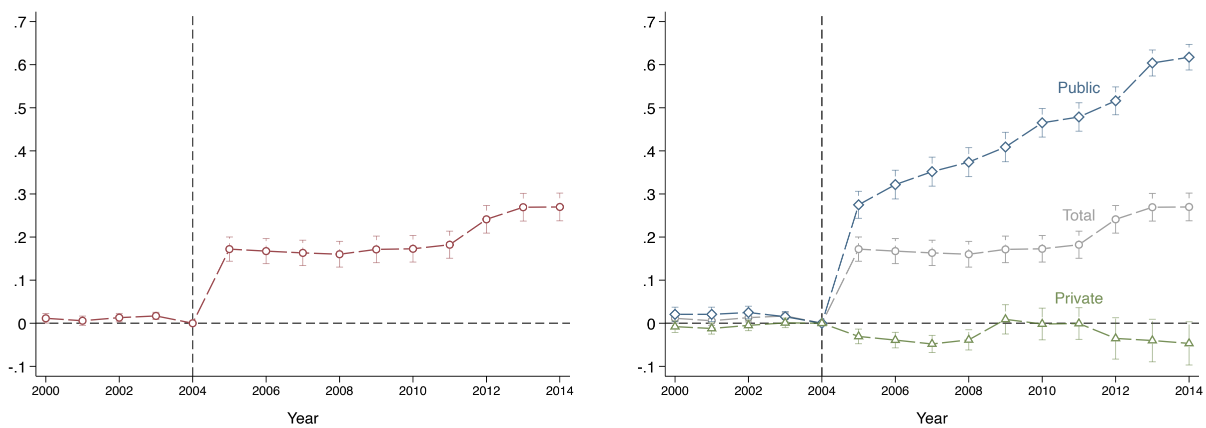
could affect real outcomes.

4.1 Higher access to bank branches

We start by showing that the reform increased access to bank branches, as the entry of government-owned banks did not crowd out private banks. To do so, we use as the LHS variable in equation (1) a dummy that equals one if the city has any bank branch (private of public), which allows us to estimate the change in likelihood for treated cities to have access to a bank branch.

We also decompose this dummy between a dummy that equals one if the city has a public bank branch, and a dummy that equals one if the city has a private bank branch. Figure 5 reports the event study coefficients of our difference-in-differences estimation. Panel (a) shows results when the LHS is a dummy that equals one if the city has any bank branch (public or private), while panel (b) decomposes the change in likelihood to have a bank branch (the grey circles) into the change coming from access to a public bank (the blue diamonds) and access to a private bank (the green triangles).

Figure 5: Effect of the Program on Having a Bank Branch



(a) Any Branch (b) Public vs. Private Banks

This figure plots the yearly coefficients and 95% confidence intervals of the difference-in-differences estimator in equation (1) of the 2004 bank reform. Dependent variables are dummies that equal one if the city has at least a branch of any bank, a public bank, or a private bank, respectively. Standard errors are clustered at the matching-pair level.

Two facts are noteworthy. First, the probability of having a branch from a private bank in treated and control cities evolve in close parallel prior to the reform. This result indicates that private banks in treated and control cities evolved in the same way during the large credit boom that Brazil experienced prior to the reform, and remain on similar trends even after the reform.

Second, the expansion of public banks only modestly crowds out private banks, resulting in a large increase in *overall* financial development for treated cities. The probability of having a public bank branch or any bank branch increases sharply after 2004, in line with the aggregate pattern reported in Figure 1, and it continues to increase progressively throughout the period with no mean reversion post reform.

4.2 Increased deposits and credit

We then study how this higher presence of bank branches affects deposits and credit supply. In order to measure financial development net of any mechanical wealth effects, we compute total credit and total deposits scaled by city GDP. Because treated cities often have zero credit and deposits before the reform (since many have no bank branch) and, by construction, have zero credit and deposits from government-owned banks, we estimate the effect of the policy using the inverse hyperbolic sine transformation of the log function.[24]

We show the event study coefficients of our difference-in-differences estimation for total deposits and total credit in Figure A1. The event studies show that credit and deposits for treated and control units evolve in close parallel prior to the reform and that the expansion in credit and deposits is entirely driven by public banks, with minimal crowding out of private banks. Panel (a) of Figure A1 shows that the initial increase in credit after the reform continues throughout the period and is driven entirely by public credit. There is a modest decline in private credit after 2010, but the total amount of credit still rises substantially after the reform, implying that overall, treated cities benefit from an increase in credit. Panel (b) of Figure A1 reports analogous results for deposits, and shows that deposits increase sharply in 2005 and continue to rise throughout the post-reform period.

We also conduct robustness checks by using the level of credit and deposits for branches that open in both treated and control cities after the reform and we find similar post-reform dynamics, in particular a similar increase in deposits and credit (Appendix Figure A2).[25]

We report pooled estimates in Table 2 and confirm the results of Figure A1. For all variables, the reform has a strong and significant effect on financial inclusion, driven by government-owned banks. The probability of having a bank branch increases by 18.6 p.p. (column 1), which fosters the accumulation of total deposits and total credit at the city level (columns 4 and 7).

24. The inverse hyperbolic sine transformation of the log function is defined as $log[X + (X^2 + 1)^{1/2}]$. Except for very small values of X, the inverse sine is approximately equal to $log(2X)$ or $log(2) + log(X)$, and so it can be interpreted in exactly the same way as a standard logarithmic dependent variable. But unlike a log variable, the inverse hyperbolic sine is defined at zero and is less sensitive to jumps around zero than the more widely used $log(X + 1)$ transformation.

25. By definition, these variables equal zero for both treated and control units prior to the reform. This means that, unlike Figure A1, this exercise should not be interpreted as a test of the parallel trends assumption.

Table 2: Effect of the Program on Bank Branches, Credit, and Deposits

Dependent Variable:	Has Bank Branch			Deposits			Credit		
	All	Public	Private	All	Public	Private	All	Public	Private
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated×Post	0.186***	0.425***	-0.022**	0.880***	1.295***	-0.098***	0.935***	1.557***	-0.154***
	(0.016)	(0.017)	(0.009)	(0.070)	(0.074)	(0.037)	(0.072)	(0.071)	(0.036)
City FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	79,995	79,995	79,995	79,995	79,995	79,995	79,995	79,995	79,995

This table shows the effect of the reform on financial development outcomes at the city level using the difference-in-differences estimator in equation (1). Has Bank Branch variables are dummies that equal one if the city has a branch of any bank, a public bank, or a private bank, respectively. Credit and deposits are both scaled by city GDP, and are in arcsin logs (inverse hyperbolic sine transformation of the log function). Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.

These results confirm the idea that financial inclusion policies can be successful at starting a virtuous circle between savings and credit, since the average expansion of credit in a city matches the average increase in deposits flowing into the branches of the same city. This similar increase in credit and deposits implies that bank branches in treated cities are able to increase their credit supply without having to use capital coming from other cities and that treated cities are not net capital receivers, but instead are able to engage in a self-sustained expansion of credit and deposits.[26]

5 Effect on economic development

5.1 Average effect

We start by estimating the effect of the reform on aggregate outcomes at the city level. Standard models of macro-finance development emphasize that financial frictions hamper economic development because talented but poor individuals are unable to start a firm (misallocation of talent) and existing productive but cash-poor firms are unable to expand their business (misallocation of capital). As financial development progresses, more firms are created and existing firms grow, generating higher demand for labor that translates into higher wages.

We test how the financial inclusion policy affects the different elements of this causal chain by estimating equation (1) with the total number of firms, total employment, employment growth at small firms (less than 20 employees), large firms, average wage and the number of

26. While the SUTVA assumption is, by definition, untestable, we also show in Appendix Figure A3 the evolution of total credit separately for treated and control cities. While the growth of credit in treated cities accelerates after 2004, it does not appear to be at the expense of a slowing down of credit in control cities.

industries as outcomes. Table 3 reports the results of these different regressions. In column 1, we show that the number of firms increases by 9.8% and that total employment increases by 10%. This increase is concentrated at small firms (less than twenty employees), which expands twice as fast as large firms (column 3 vs. 4). This increase in the demand for labor explains why average wage increases by 4.1%.

In column 6, we study how the reform affected industry dynamics. Consistent with models emphasizing that economic development requires countries to diversify their industrial base and explore their comparative advantage (e.g., Hausmann and Rodrik, 2003; Imbs and Wacziarg, 2003), we find that financial development increases the number of industries, which we measure as the number of distinct 4-digit industries (column 6).[27]

Table 3: Effect of the Reform on Economic Development

Dependent variable	# Firms	Employment all	Employment small firms	Employment large firms	Average wage	# Industries
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	0.098***	0.100***	0.214***	0.116***	0.041***	0.088***
	(0.011)	(0.015)	(0.023)	(0.018)	(0.005)	(0.008)
City FE	✓	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓	✓
Observations	79,995	79,995	79,947	79,995	79,995	79,995

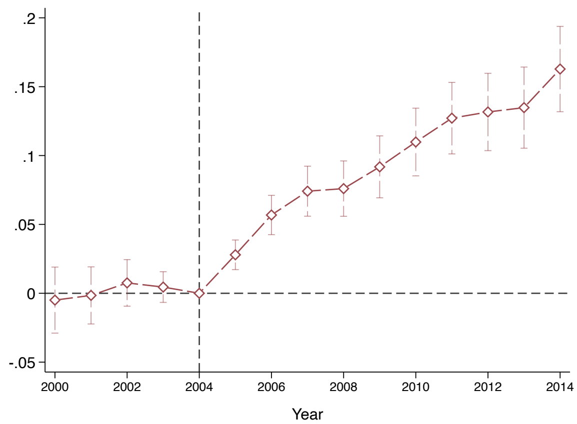
This table shows the effect of the reform on economic development at the city level using the difference-in-differences estimator in equation (1). All variables in columns 1–6 are in logs. In columns 3 and 4, employment is decomposed between firms with less than 20 employees ("small firms") and more than 20 employees ("large firms"). The number of industries (column 6) is the number of distinct 4-digit industries in the city-year. Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

We reproduce this analysis in graphical form by estimating the event study version of equation (1) in Figure 6. In all cases, we find that treated cities display no pre-trend relative to control cities. We also find that each outcome increases progressively over time after the reform and stabilizes at a new high after five years, consistent with the notion that the reform relaxed financial constraints and allowed the local economy to reach a new steady state with a higher level of development.

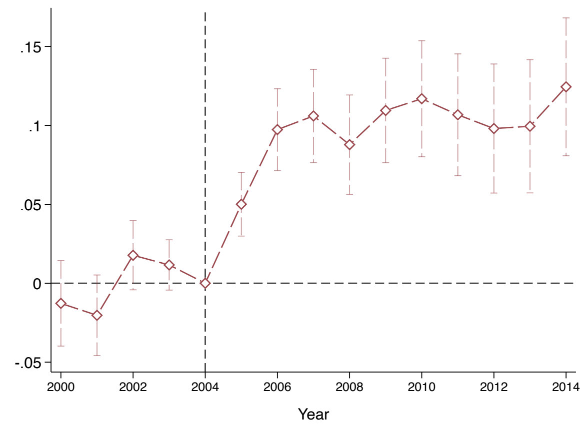
Discussion of magnitudes. While we show in Section 4 that the reform led to financial development, we do not observe the entire effect of the reform and therefore cannot use these results as a "first stage." In particular, *all* treated cities that did not experience the entry of a bank branch after the reform obtained at least a banking correspondent (Ministério da

27. There are 614 distinct industries and the definition is consistent over time.

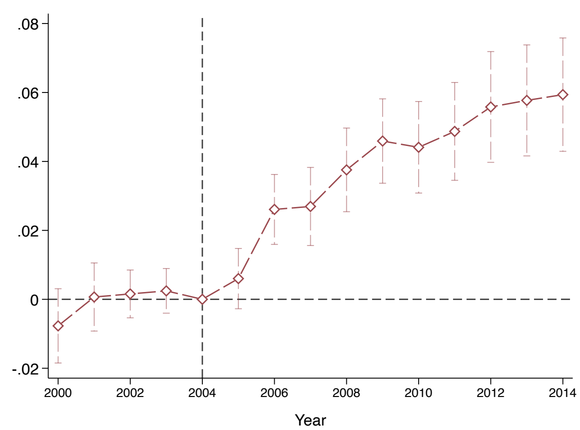
Figure 6: Effect of the Program on Firms, Employment, and Wage



(a) Firms



(b) Employment



(c) Mean wage

This figure plots the yearly coefficients and 95% confidence intervals of the difference-in-differences estimator in equation (1) of the 2004 bank reform. Dependent variables are logs of total number of firms, total employment, and average wage in panels (a), (b) and (c) respectively. Standard errors are clustered at the matching-pair level.

Fazenda, 2007), but we unfortunately cannot observe it in the data.[28] And while the evidence presented in Section 2.1 indicates that correspondents do not provide the quality and scope of financial services as bank branches, having a correspondent almost certainly provides more access than having neither a correspondent nor a branch, suggesting that all cities receive some level of treatment. Therefore, our estimates on financial outcomes under-estimate the true impact of the reform on financial development, and rescaling the coefficients on the economic development outcomes shown in Table 3 by the point estimates in Table 2—as in a standard 2SLS approach—would inflate the true magnitude of the elasticities.

Due to this caveat, we think the more natural approach is to directly interpret the point

28. As explained in Section 2.2, data on bank branches does not keep track of banking correspondents.

estimates in Table 3 as the elasticity of economic development outcomes with respect to the introduction of formal financial services. In this context, the two closest experiments to our setting are Barboni, Field, and Pande (2021), which looks at the entry of bank branches in Indian villages, and Bruhn and Love (2014), which looks at the opening of bank branches in stores of a large retailer of consumer goods focused on underserved and low-income clients.

Barboni, Field, and Pande (2021) finds that a new bank branch leads to an 8% reduction in poverty and a 6% increase in average income. The latter is comparable to the 4.1% increase in average wages that we estimate in our setting. Bruhn and Love (2014) finds similar estimates, with income increasing by 7%, employment by 1.4%, and informal businesses by 7.6%, although formal business is unaffected. Our larger effects on employment and business creation can be explained by the fact that our experiment improved financial development at the city level, and therefore is more likely to have positive "local GE effects." The longer time period over which we can trace out the effect of the reform can also partly explain the difference since resources reallocate slowly, particularly in developing countries (e.g., Buera and Shin, 2013). These slow-moving changes underscore the importance of measuring and taking into account transitional dynamics when estimating the effect of reforms on economic development.

5.2 Mechanisms

There are two main channels through which financial development promotes economic growth in this setting. First, bank expansion can foster *local demand* either because it relaxes individuals' borrowing constraints and reduces their need for precautionary savings, or because the opening of a bank branch functioned as fiscal stimulus.[29]

Second, bank expansion can foster *supply* by reducing investment frictions, thereby boosting investment of existing firms and facilitating the entry of new firms. In this case, the differential dependence on local demand should not matter and we expect both tradable and non-tradable industries to benefit from the reform.

5.2.1 Consumption vs. business development channel

To test if most of the effect is coming from a bank-expansion-induced increase in demand, we decompose growth in aggregate employment at the city level by firms in non-tradable vs. tradable industries. Indeed, since non-tradable industries are more dependent on local demand than tradable industries—because, by definition, tradable industries produce goods

29. Financial inclusion will reduce the need of precautionary savings for instance because of limited insurance in developing countries.

that can be sold across the whole country, if not worldwide—an increase in local demand driven by the reform should benefit non-tradable industries relatively more.

To do so, we estimate equation (2) and split the regression between tradable and non-tradable, which requires us to work at the city-industry level. This requires a slight modification to our specification since the reform had an impact on the entry and exit of industries at the city level (Table 3–column 6), implying that the baseline specification of equation (1) at the city-by-industry level will not match the aggregate results at the city level.

We explain in detail how we account for this adjustment in Section 7.1. Briefly, we create a balanced panel and compute the mid-point growth rate between the average pre period (before to 2004) and post period (after 2004). We show in Table 11 that this specification preserves the aggregate city level results and report that, for all variables, it produces very similar point estimates as the ones obtained with our baseline log panel specification.

Table 4: Employment in Tradables and Non-Tradables

Dependent variable	Employment					
Tradable Definition	Manufacturing		Value of Traded Goods		Geographic Concentration	
Tradable	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	0.200***	0.123***	0.190***	0.124***	0.146***	0.126***
	(0.037)	(0.015)	(0.042)	(0.015)	(0.025)	(0.015)
Match×Industry×Year FE	✓	✓	✓	✓	✓	✓
Observations	106,574	414,795	84,772	436,597	129,728	391,641

This table reports the effect of the policy on employment at the city-by-(4-digit) industry level. Data are collapsed as an average "pre" ($t \leq 2004$) and the average "post" ($t > 2004$) periods, and each dependent variables are the midpoint growth rate $g^X_{j,c} = [(X_{j,c,t} + X_{j,c,t-1}) \times 0.5]$. Each cell is weighted by $g^X_{j,c}/(\sum_{j\in c} g^X_{j,c}) \times pop_{2000}$. See Section 7.1 for a detailed explanation of the construction. In columns 1–2, tradable is defined as firms in the manufacturing sector. In columns 3–4, we define tradable industries based on the value of exports and imports in the custom data aggregated at the sector level. In columns 5–6, tradable is defined using the geographical HHI of employment of each industry. "Tradable" (column 5) corresponds to an HHI in the top quartile (i.e., high level of geographic concentration). Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

We use three methods to identify industries that produce tradable goods and are therefore not dependent on local demand. First, we classify an industry as tradable if it is in the manufacturing sector, and non-tradable otherwise. Second, we flag industries as tradable if the imports plus exports equal to at least \$10,000 per worker, or if total exports plus imports for the industry four-digit industry exceeds \$500M.[30] Third, we compute the geographical dispersion (HHI) of employment at the industry level and classify tradable industries as those in the top quartile of the HHI distribution. The intuition behind this proposed measure is that,

30. Numbers are in 2006 values. We manually build a crosswalk between US NAICS codes and the Brazilian industry classification.

since non-tradable industries have to be consumed locally, they should be less geographically concentrated.

We report the results of the effect of the reform on tradable and non-tradable industries in Table 4. We find that employment growth is almost two times bigger in tradable industries depending on the exact definition (e.g., column 1 vs. 2). This implies that while the entry of bank branches in the cities might have had a direct effect on demand, a substantial part of the increase in economic development induced by the financial inclusion policy is coming from a relaxation of financing constraints for entrepreneurs.

The fact that firms in tradable industries benefited more from the reform than firms in non-tradable industries is also consistent with another channel through which financial inclusion policies can promote economic development, namely the "bank liquidity channel."[31] Indeed, firms and households need a place to safely store their liquidity and transact with others. This is particularly true for firms in tradable industries that need to pay suppliers and receive customer payments from firms that are physically far away. Banks provide such services, which facilitate trade and business transactions (see Xu and Yang (2022) for an example in 19^{th}-century US).

5.2.2 Why do local branches matter? The role of distance

Next, we investigate why the presence of local banks appears to relax firms' financial constraints. A classic assumption in the macro-development and finance literature is that geographical proximity reduces banks' monitoring and screening costs (e.g., Greenwood, Sanchez, and Wang, 2010; Ji, Teng, and Townsend, 2023) in particular in the presence of soft information (e.g., Rajan and Zingales, 2001; Hombert and Matray, 2017), which is prevalent in developing countries.

The main prediction of these models is that the effect of the policy should increase with the ex-ante distance to the nearest bank. We test this prediction in two ways. First, we compare cities with a local (private) bank before the reform with cities that did not have a private bank by interacting the $\textit{Treated} \times \textit{Post}$ variable with a dummy variable $\textit{No Private Bank}_c$ that takes the value one if the city did not have a private bank pre-reform. Second, *within* the set of treated cities that have no bank branches prior to the reform, we compute the distance to the nearest city with a bank (public or private). We estimate the conditional effect of distance in the full panel by interacting the main $\textit{Treated} \times \textit{Post}$ variable with a dummy equal to one if the distance is zero, and then with a continuous variable *Distance to the nearest bank* that is the (log) distance to the nearest bank.

31. For a survey of the bank liquidity channel, see Drechsler, Savov, and Schnabl (2018) and Dreschler, Savov, and Schnabl (2018). We would like to thank our editor for suggesting this channel.

This analysis also allows us to test whether our results are specific to the presence of government-controlled banks—for instance, because these banks extend subsidized credit. To do so, we separately interact our main *Treated*×*Post* variable with the distance to the nearest public bank and with the distance to the nearest private bank. Intuitively, if our results hinge on access to *subsidized* credit, the effect of the reform should be stronger for cities that were far from a government-controlled bank than for those that were far from a private bank.

We report these results in Table 5. In panel A, we show that our results are much stronger in cities with no bank presence prior to the reform. Cities without a local private bank before the reform experience a larger increase in the number of firms (10.4%, column 1), in employment (13.9%, column 2), and in average wages (7%, column 5). In panel B, we show that, conditional on not having a local private bank pre-reform, the real effects of financial inclusion increase with the distance to the nearest bank.

Both sets of results are consistent with the reform promoting economic development by reducing the distance between borrowers and lenders, which lowers the monitoring and screening costs of financial intermediaries. This idea is also consistent with the result that small firms (fewer than 20 employees) benefit more from the reduction in distance than large firms (column 3 vs. 4), in line with the idea that small firms are more opaque and more intensive in soft information.

The fact that the effect of the reform on economic development increases with the initial distance of treated cities to existing bank branches is also consistent with the "bank liquidity channel." Indeed, a reduction in distance to a branch may matter simply because it makes it less costly to access liquidity services.

In panels C and D, we report similar results for the distance to the nearest public and private bank, respectively. Across all variables, we find that the conditional effect of distance is quantitatively similar whether we measure distance to public banks (panel C) or private banks (panel D). These findings suggest that our results are not specific to services provided by government-controlled banks, such as access to subsidized credit. Indeed, if access to government-owned banks mattered in itself, we should find a larger effect of distance to a public bank relative to the distance to a private bank.

5.3 Discussion of the reform

Interpretation: improved access to financial services. The permanent shift in financial development, the similar increase in deposits and credit, the sustained economic development principally driven by the growth of the tradable sector, and the fact that these effects increase with the ex-ante distance to a bank branch imply that the financial policy we study fostered access to mainstream financing services for firms.

Table 5: Financial Frictions, the Role of Distance

Dependent variable	# Firms	Employment all	Employment small firms	Employment large firms	Average wage
	(1)	(2)	(3)	(4)	(5)
	Panel A: Private bank before the reform				
Treated×Post	0.054***	0.042***	0.111***	0.050***	0.012*
	(0.014)	(0.016)	(0.026)	(0.020)	(0.006)
Treated×Post×No private bank	0.104***	0.139***	0.246***	0.158***	0.070***
	(0.023)	(0.031)	(0.048)	(0.038)	(0.011)
	Panel B: Distance to nearest bank				
Treated×Post	0.054***	0.042***	0.111***	0.050***	0.012*
	(0.014)	(0.016)	(0.026)	(0.020)	(0.006)
Treated×Post×Distance to nearest bank	0.068***	0.108***	0.164***	0.131***	0.022***
	(0.012)	(0.013)	(0.034)	(0.022)	(0.007)
	Panel C: Distance to nearest public bank				
Treated×Post	0.054***	0.042**	0.111***	0.050**	0.012*
	(0.015)	(0.017)	(0.028)	(0.021)	(0.007)
Treated×Post×Distance nearest public bank	0.069***	0.110***	0.166***	0.132***	0.024***
	(0.012)	(0.013)	(0.030)	(0.020)	(0.006)
	Panel D: Distance to nearest private bank				
Treated×Post	0.054***	0.042**	0.111***	0.050**	0.012*
	(0.015)	(0.017)	(0.028)	(0.021)	(0.007)
Treated×Post×Distance nearest private bank	0.070***	0.113***	0.174***	0.136***	0.026***
	(0.011)	(0.013)	(0.027)	(0.019)	(0.007)
City FE	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓
Observations	79,995	79,995	79,947	79,995	79,995

This table shows the effect of the policy on multiple outcomes interacted with various measures of ex-ante distance between treated cities and existing banks using the difference-in-differences estimator in equation (1). In Panel A, *No private bank* is a dummy equal to one if the treated cities did not have a private bank prior to the reform. In Panel B, *Distance to the nearest bank* is the distance to the nearest city with a bank, for treated cities with no bank prior to the reform. Panel C and D compute this distance for the nearest private and the nearest public bank. Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

This policy operates through a distinct mechanism that is inconsistent with two alternative interpretations: (i) the reform was a one-time capital infusion coming from outside the city that potentially generated excessive borrowing, and (ii) the policy was a generic government spending policy.

On (i), the permanent change in the steady state of financial development, together with the sustained economic development over ten-year with no sign of mean reversion, rules out the concern that the policy was fueled by bad loans, as this should trigger a boom-bust cycle.

On (ii), the long-lived effect on economic development is also inconsistent with a short-term stimulus. In addition, the fact that employment in the tradable sector expands more than employment in the non-tradable sector (Table 4) and that the effect increases proportionally with the distance to the nearest bank are also inconsistent with these results being driven by higher spending in treated cities overall.

Is the reform beneficial in net? While the reform was successful in terms of fostering substantial economic development, it does not necessarily mean that this success is unambiguous. Indeed, the cost of operating bank branches in these unbanked cities might be substantial. Doing a clear cost-benefit analysis of the program is beyond the scope of this paper and would require access to data on the total costs of setting up and operating local bank branches, which do not exist to the best of our knowledge.

Nonetheless, ESTBAN contains information about expenses and profitability at the branch level. This allows us to look at the yearly average expense-to-revenue ratio and ROA for the public branches created in treated cities after the reform, and to compare them to the average for all private branches in control cities and for new private branches in control cities.

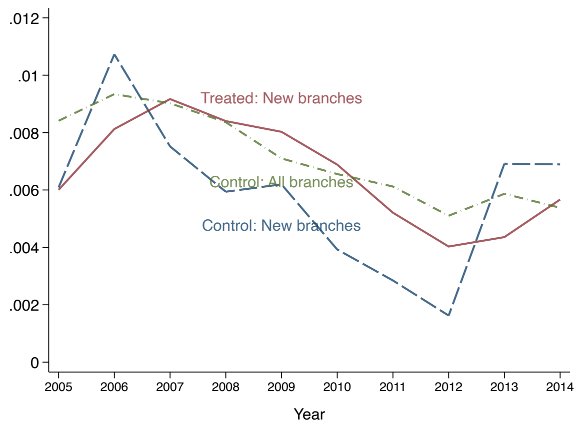
Figure 7 plots the evolution between 2005 and 2014, and shows that branches opened in treated cities are not less profitable or more expensive to run than those opened in control cities. Panel (a) displays that the average ROA of new branches in treated cities is, if anything, higher than that of new branches in control cities and comparable to the average ROA of all branches, new and existing, in control cities. Panel (b) displays that the average expense-to-revenue ratio of new branches in treated and control cities are similar, and only slightly higher than that of all branches in control cities.

Of course, it is still possible that the fixed costs associated with setting up a bank branch are much higher in treated cities. But, conditional on paying this fixed cost, there is no evidence that the bank branches of public banks in treated cities are particularly subsidized.

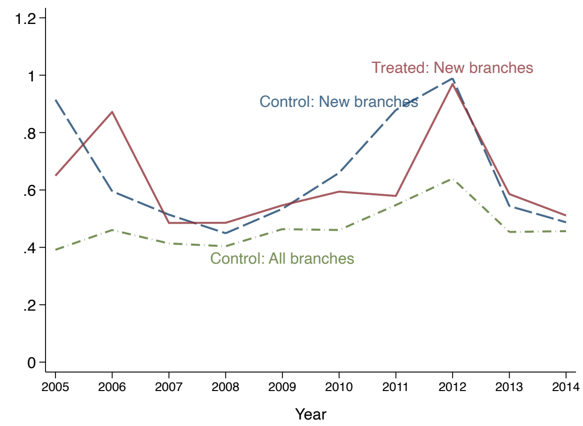
Why do banking deserts exist in the first place? The permanent shift in deposits and credit, together with the sustained effect on economic activity that the financial inclusion policy triggered and the similarity in bank branch ROA in treated and control cities, imply that treated cities that received a bank branch were profitable markets. Then why did banks not serve these markets prior to the reform? This question is beyond the scope of our paper, but we discuss possible explanations in Appendix A.2. Briefly, the lack of coverage can be explained by banks being in a situation of monopolistic competition and facing downward-sloping demand curves, which implies that banks can maximize their markups and hence their profits by restricting quantities, i.e., limiting their branch expansion.

On the cost side, banks might be able to minimize the cost of their branch network by

Figure 7: Average ROA and Expense-to-Revenue Ratio



(a) ROA



(b) Expense-to-Revenue Ratio

This figure shows the evolution of ROA (panel a) and total operating expenses divided by total operating revenues (panel b) for new branches in treated and control cities, defined as branches that were opened after the reform, as well as for all branches in control cities.

extending their network in a capillary way and only up to a certain limit. In addition, the existence of sunk costs in setting up a branch might imply that, while branches are profitable once they operate, the total cost (variable plus fixed cost to set up) might still be too high, particularly if set-up costs are larger in more remote cities. As such, a possible rationale for why bank branches are not present in treated cities prior to the reform, but their entry has such a strong positive effect on economic development, is that using public banks to open up branches in unbanked cities is akin to the government subsidizing the set-up cost of a bank branch.

6 Effects on inequality

6.1 Aggregate results

To study how the aggregate economic gains produced by the financial inclusion policy are distributed in each local labor market, we estimate equation (1) using the wage Gini at the city level as an outcome, as well as the average wage per worker in each bin of the city-level wage distribution. We graphically report the result for the evolution of Gini and the change in average wage for each quartile of the wage distribution in Figure 8. Figure 8a shows the effect of the reform on the Gini coefficient. As before, treated cities display no differential pre-trend prior to the reform. Following the reform, we find a continuous increase in Gini, implying an increase in wage inequality. The magnitude is substantial, with treated cities having a Gini index that is two points higher ten years after the reform relative to control cities, which represents an increase of 7% relative to the pre-reform mean.

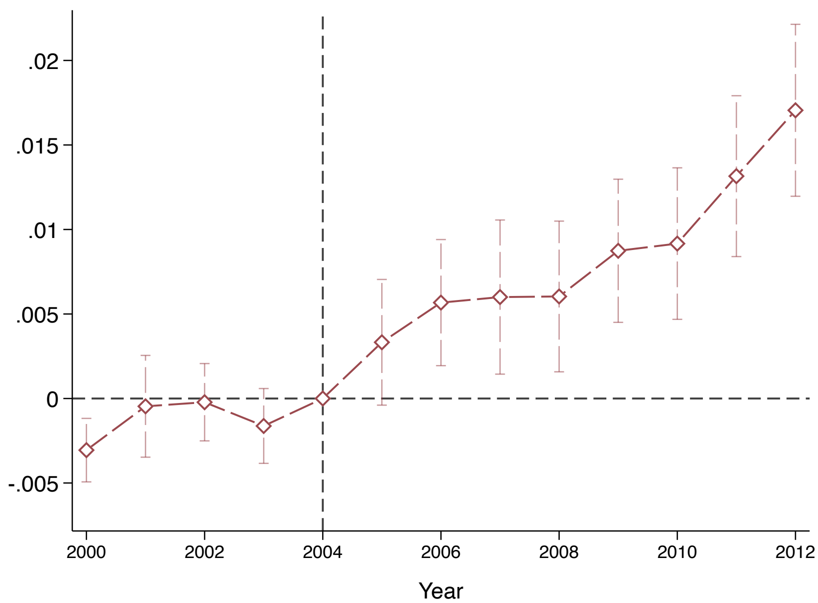
While this result shows that higher financial development leads to higher inequality, it does not tell us why the Gini is increasing in treated cities.

We therefore unpack the evolution of Gini by computing the average wage for each quartile of the city wage distribution to better understand the source of the overall change in inequality. To do so, we estimate the distribution of wage within each city-year cell, split the sample into quartiles, and take the mean wage in each cell.

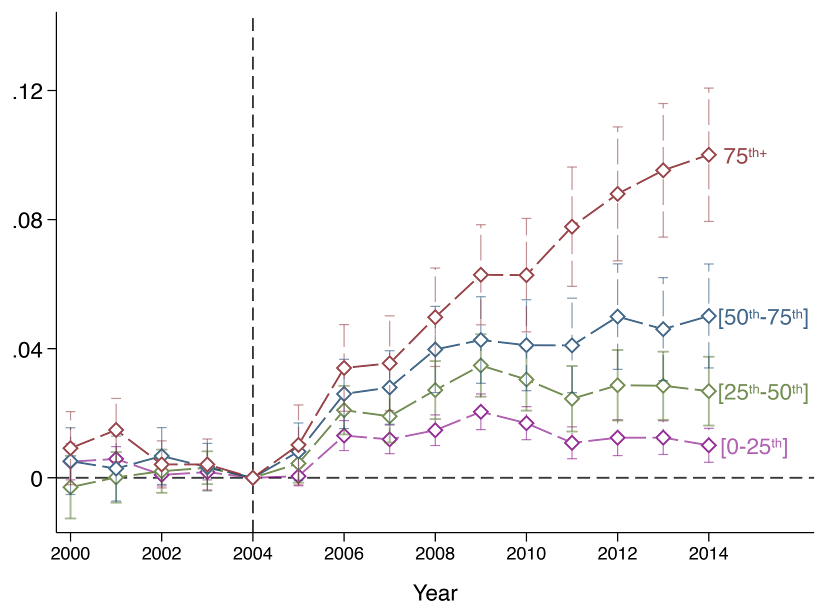
Figure 8b reports the evolution of each wage quartile. Consistent with the idea that economic development is a "tide that lifts all boats," we find that all workers benefit from the reform. However, workers in the first quartile of the distribution (the purple line) gain far less than workers in the last quartile (the red line), and wage gains increase monotonically with the initial position in the wage distribution.

Table 6 reports estimates of equation (1). The point estimates tend to underestimate the effect of the reform on inequality since, as Figure 8b shows, inequality rises steadily over time, while these regression results show the average over the whole post-reform period. The Gini increases on average by 1.2 points (column 1), which is driven by larger wages gains at the top of the income distribution. Individuals in the bottom quartile of the wage distribution experience an increase in their average wage of 1% (column 2), while individuals in the top quartile see their wages increase by 5.5% (column 5), five time more.

Figure 8: Effect of the Program on Wage Inequality



(a) Gini index



(b) Wage distribution

This figure plots the yearly coefficients and 95% confidence intervals of the difference-in-differences estimator in equation (1) of the 2004 bank reform on city-level wage Gini (Figure 8a). In Figure 8b, the wage distribution is computed every year at the city level. Standard errors are clustered at the matching-pair level.

Table 6: Effect of the Program on Wage Inequality

Dependent variable:	Gini	Wage			
		[0–25th]	[25th–50th]	[50th–75th]	[75th+]
	(1)	(2)	(3)	(4)	(5)
Treated×Post	0.012***	0.010***	0.024***	0.034***	0.055***
	(0.002)	(0.003)	(0.004)	(0.006)	(0.007)
City FE	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓
Observations	79,995	79,995	79,995	79,995	79,995

This table reports the effect of the policy on earnings inequality at the city level. In columns 2–4, the dependent variable is the (log) average wage for each bin of the wage distribution in a city-year cell. Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

6.2 Mechanisms

We explore three channels that can account for the increase in inequality following a reduction in financial frictions: better matching, skilled-labor demand and constrained skilled-labor supply. First, financial development might lead to better employer-employee matching. This could happen either because looser financial constraints on individuals allow them to search longer and to find a better job match, or because less severe financial frictions can allow productive firms to front-load wages and attract more productive workers, resulting in a reduction in labor misallocation and higher wages at the top of the distribution.

Second, financial development can foster higher labor demand for skilled workers relative to unskilled workers. Financial frictions can directly impact labor demand if there is a mismatch between payments to labor and the generation of cash-flows or if labor has a fixed-cost component due to hiring and firing costs (Schoefer, 2021; Benmelech, Bergman, and Seru, 2021). Since skilled workers require higher wages and are arguably more expensive to recruit and train, financial frictions disproportionately constrain the demand for skilled labor and, when lessened by the reform, lead to an increase in the demand for skilled labor relative to unskilled labor.

Alternatively, if capital and skilled labor are relative complements, looser financial constraints can increase capital investment and, consequently, increase the marginal productivity of skilled workers relative to unskilled workers, also leading to an increase in the relative demand for skilled workers (Fonseca and Doornik, 2021). A testable implication of either version of the skilled labor demand hypothesis is that, as the relative demand for skilled workers rises, both the relative price and the relative quantity of skilled workers should rise, leading to an increase in the skill premium *and* in the share of skilled workers in treated cities.

Third, labor demand might go up uniformly across the skill distribution, but the supply of unskilled workers could be more elastic than the supply of skilled workers. In this case, the skill composition of firms remains stable, but the price of skilled workers goes up, particularly so in cities facing higher shortages of skilled workers.

Better matching. To test if the matching between workers and firms improves following the reform, we build on Eeckhout and Kircher (2011) and Lopes de Melo (2018), which give a structural interpretation to the firm fixed effects in Abowd, Kramarz, and Margolis (1999) regressions and show that better matching should reduce the dispersion of worker ability within the firm.[32]

We proxy for worker type with the average log wage over all job spells. We compute the standard deviation of worker types at the firm-year level, residualize the variable from firm fixed effects to account for changes in industry-city composition over time, and take the mean of the residualized dispersion in worker types at the 2-digit-industry-by-city level for each year. We can then test whether the average dispersion declines as a consequence of the reform.[33]

Table 7 shows results of this exercise. Across all specifications, we find that if anything, the within-firm dispersion in worker type increases (by a small amount relative to the pre-reform average of 0.34). This is the opposite of what we would expect from an improvement in employer-employee matching, which should lead to lower within-firm dispersion in worker types.

Increase in demand for skilled workers. To test whether a change in the relative demand for skilled workers can explain the rise in wage inequality, we need an ex-ante, time-invariant definition of skill. We leverage the fact that the Brazilian matched employer-employee data allow us to observe education and classify workers as skilled if they have at least some college education and unskilled otherwise.[34]

In Table 8, we start by showing that this measure tracks the evolution of inequality well. In column 1, we show that the skill premium increases by 8.3% (column 1) and that this increase is driven by a much faster increase in the wage of skilled workers (+11.8%, column 2) than unskilled workers (+2.8%, column 3). These magnitudes are actually bigger than the

32. Another potential way of testing for sorting would be to study the correlation between firm and worker fixed effects, but, as Eeckhout and Kircher (2011) and Lopes de Melo (2018) show, this correlation does not measure the strength of sorting in a general setting.

33. See Bombardini, Orefice, and Tito (2019) for an application of this method in a trade context.

34. This is a less stringent definition than studies looking at developed countries who use college education as a proxy, since we include college dropouts in our definition of skilled.

Table 7: Dispersion in Worker Type

Dependent variable:	Std. Dev. Worker Type			
	(1)	(2)	(3)	(4)
Treated×Post	0.026*	0.027∼	0.026*	0.027
	(0.015)	(0.017)	(0.016)	(0.020)
City FE	✓	—	—	—
City× Industry FE	—	✓	✓	✓
Match×Year FE	✓	✓	✓	—
Industry×Year FE	—	—	✓	—
Match×Industry×Year FE	—	—	—	✓
Observations	1,286,478	1,286,478	1,286,478	1,286,478

This table shows the effect of the reform on the change in the average within-firm standard deviation of worker type at the city-by-(2 digit) industry level. Worker type is measured as the average log wage over all job spells of a given worker. We then compute the standard deviation of worker types at the firm-year level and residualize this variable from firm fixed effects. Standard errors are clustered at the matching-pair level. ***, **, *, ∼ indicate statistical significance at the 1%, 5%, 10% and 11% levels, respectively.

wage increase in the top quartile of the distribution (+5.5%, column 5-Table 6) relative to first quartile (+1%, column 2-Table 6), which suggests that the increase in inequality reflects an increase in the returns to skill.

Absent labor supply constraints or other frictions, a credit-fueled rise in the relative demand for skilled labor increases the relative quantity of skilled labor (e.g., Fonseca and Doornik, 2021). While the coefficient for the share of skilled workers is positive and significant at 10%, the magnitude (+0.2%) is very small compared to the 8% increase in the skill premium. This suggests that other frictions, such as labor supply constraints, are necessary in order to explain the bulk of our results. In Appendix Table A9, we show that we find similar results at the industry-by-city level controlling for time-varying industry shocks.

Constraints in the supply of skilled workers. To argue that a city's own supply of skilled workers is a driver of higher wage inequality, we first need to establish that worker mobility across cities is limited. To do so, we exploit the panel dimension of our data to decompose the number of workers in a given city-year into "local," defined as workers who are already in the city prior to the reform, "movers," defined as workers who were living in a different city prior to the reform, and "new," defined as workers who appear for the first time in labor-market data in a given city and did not come from another city.

Table 9 estimates the effect of the reform on the composition of workers across these three groups for all workers (columns 1–3) and skilled workers only (columns 4–6). We find that the reform has no effect on the share of workers coming from other cities in general (column

Table 8: Demand for Skilled Workers

Dependent variable	Skill premium	Wage skilled	Wage unskilled	Share skilled
	(1)	(2)	(3)	(4)
Treated×Post	0.083***	0.118***	0.028***	0.002*
	(0.010)	(0.012)	(0.006)	(0.001)
City FE	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓
Observations	79,901	79,901	79,995	79,995

This table shows the effect of the reform on the skill premium (column 1), the average wage of skilled and unskilled workers (columns 2 and 3), and the share of workers that are skilled (column 4) at the city level. Skilled workers are defined as workers with at least some college education. All dependent variables are in arcsin-logs. Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

2), and that it has a positive but very small effect (+0.7%) when we focus on skilled workers (column 5). This implies that the reform had a limited effect on domestic migration and that cities that benefited from the financial inclusion policy did not experience an important inflow of skilled workers.

Table 9: Worker Migration

Sample:	All workers			Skilled workers		
Dependent variable:	Share local	Share movers	Share new	Share local	Share movers	Share new
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	-0.019***	0.000	0.021***	-0.021***	0.007***	0.020***
	(0.004)	(0.001)	(0.004)	(0.006)	(0.002)	(0.005)
City FE	✓	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓	✓
Observations	79,995	79,995	79,995	79,901	79,901	79,901

This table shows the effect of the reform on the share of workers by migration status at the city level. Skilled workers are defined as workers some college education. "Local" workers are workers observed in the city before the reform. "Movers" are workers that we observe in a different city before the reform. "New"' are workers that appear in the city for the first time. All dependent variables are in arcsin-logs. Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

While the low domestic migration of skilled workers following the reform might seem surprising given the skill premium increase in treated cities, this can be explained by the existence of very large migration costs in Brazil, particularly for residents of poor cities (e.g., Porcher, 2020). We provide evidence for this hypothesis by estimating how the migration response varies as a function of migration cost. We proxy for migration cost using the share of movers during the pre-reform period and split the data into deciles of migration cost. We then estimate the effect of the reform on the share of within-country migrants for each decile of the migration cost distribution. Figure A4 in the Appendix reports the results. Consistent with outsiders being attracted by a higher skill premium when migration costs are low, we find an increase in the share of migrant workers in the first decile of migration cost, with an increase of 1%. However, this effect sharply drops to zero at the second decile and remains around zero afterwards.

Given the low rate of internal migration, an increase in the demand for labor (skilled and unskilled) can only be met by local workers. To proxy for the potential supply of skilled labor, we use the share of the local population with 11 years or more of education from the 2000 Demographic Census. The intuition behind this measure is that if a treated city faces a shortage of skilled workers, we should observe an abnormally large skill premium. In order to determine what is abnormally large, we compare the skill premium in treated cities with the skill premium in the same industry-by-firm-size category in control cities.

Our measure of the relative supply of skilled labor is based on the population census, which has the advantage of neither being affected by the fraction of workers in the informal sector, nor reflecting the equilibrium outcomes in the formal labor market. As a robustness check, we supplement this measure by computing a measure of the "skill gap" at the city level to construct that measure. We split firms into employment size quartiles according to the city-year distribution and, for each year in the pre-reform period, we compute the skill premium in each city-industry-firm-size cell for both treated and control cities. We then take the ratio of treated to control skill premium at the industry-firm-size level and define the skill gap as the city-level mean of all industry-firm-size ratios in a given city.

We split both measures along the sample median and interact each dummy with all the variables, including the fixed effects. Table 10 reports the results. The increase in Gini (column 1) is entirely explained by the increase in inequality in cities where the fraction of skilled workers is low (column 2). Since we use an interaction term, the coefficient on the variable *Treated*×*Post* shows the result for the sub-sample of cities that are below the median of the supply of skilled labor. The total effect for cities with high supply skilled labor is obtained by adding the coefficient of *Treated*×*Post* with the marginal interaction term. Irrespective of the proxy (columns 2 and 3), we find that the total effect of the policy on inequality for cities with a high supply of skilled workers is much smaller (column 2), and it is close to zero when we measure the supply of skilled workers with the share of population with some college education (column 3). In Appendix Table A10, we show that these results are robust to using continuous versions of these skill supply measures and adding a wide range of control variables.

Table 10: Effect on Gini, Heterogeneity in Skill Supply

Dependent variable:	Gini			
	(1)	(2)	(3)	(4)
Treated×Post	0.012***	0.016***	0.018***	0.020***
	(0.002)	(0.002)	(0.002)	(0.003)
Treated×Post×Low skill gap		-0.008***		-0.006**
		(0.003)		(0.003)
Treated×Post×High share skilled population			-0.014***	-0.013***
			(0.003)	(0.003)
City×Industry FE	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓
Observations	79,995	79,995	79,995	79,995

This table shows the effect of the reform the Gini index at the city-by-(2 digit) industry level. In column 2, we split treated cities based on whether their fraction of population with at least 11 years of education is above or below the median of the sample distribution. In column 3, we estimate the ratio of skilled workers in treated cities relative to the national average, and split along the sample median. Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

7 Robustness

7.1 City-industry level estimation

Controlling for industry-dynamics. Even though pre-reform covariates are balanced across treated and control cities (Figure 3) and we show in Appendix Table A5 that our results are robust to directly controlling for these levels, it is possible that industry-specific shocks post 2004 might affect our results. Alleviating these concerns requires us to work at the city-by-industry level. However, since we observe entry and exit of industries at the city level, the baseline specification of equation (1) does not guarantee that aggregate results at the city level (which capture the extensive margins by construction) are preserved when we disaggregate the data at the city-by-industry level.

We are able to provide an alternative estimation that does ensure this aggregation property. We modify our baseline specification in two ways. First, we create a balanced panel by assuming that each industry we observe at any point in a given city is present during the whole sample period, and we fill observations without firms in an industry with zero. Second, we collapse the data into two periods: the average "pre" ($t < 2004$) and the average "post" ($t \geq 2004$). We then compute the mid-point growth rate for all our different outcomes, that we define for a variable X as: $g^X_{j,c} = (X_t - X_{t-1})/[(X_{j,c,t} + X_{j,c,t-1}) \times 0.5]$.

Specifically, we estimate the following equation at the city c, industry j, period t level:

$$\Delta Y_{c,j,t} = \beta_1 \; Treated_c \times Post_t + \delta_{j,t} + \varepsilon_{c,j,t} \tag{3}$$

Since $\Delta Y_{p,c,j,t}$ is the change between the pre and post period, we do not need to include city×industry fixed effects as they are already differenced out, but we do include industry-by-pair-by-time fixed effects $\delta_{j,t}$.

This specification has two appealing properties. First, it handles entry and exit of industries without relying on transformations of the log function (such as "x+1"), that are always sensitive to small variations around zero. Second, it ensures that the coefficient at the city-industry level aggregates exactly to the coefficient at the city level when using the correct weights, which is not possible with the non-linear log function. The weights are defined as the share of the denominator in the total city-period cell. For each industry j in city c, we use the mid-point growth rate for a variable X in city c and industry j $g^X_{j,c}$, and compute the weight as $g^X_{j,c}/(\sum_{j\in c} g^X_{j,c})$.[35]

This specification allows us to include industry-by-year fixed effects (or even industry-by-match-by-year fixed effects), which ensures that the effect of the reform is now estimated by comparing the *same* industry across treated and control cities in the same matched pair. This implies, for instance, that even if treated cities are more dependent on the commodity sector in the midst of a commodity boom, these industry-specific dynamics will not bias our estimates.

In Table 11, we start by reproducing the baseline results at the city-by-industry level. In columns (1) and (4), we report results at the city level and show that they are very close to the baseline city-level results of Table 3. In columns (2) and (5), we show that the point estimates are *identical* at the city-industry level with the weighting described above. Finally, in columns (3) and (6), we show that the inclusion of match×year×industry fixed effects yield, if anything, larger point estimates. In this case, the identification relies solely on comparing outcomes in the same industry within a given group of treated-control cities. These additional fixed effects ensure that our baseline effects are not driven by industry shocks that might correlate with the reform and the sectoral composition of treated cities.

Estimating the underlying change in firm dynamics. An additional advantage of this industry-level specification that both accommodates zeros and uses a linear estimator is that we can exactly decompose the change in the number of firms in the cross section of cities into the evolution of entry and exit. To measure firm entry and exit, we count the number of firms entering or leaving the city each year and set the year 2000 to zero. This allows us to

35. In our case, we multiply this weight by the population in 2000 in order to be able to exactly reproduce the city level results, which does not affect the aggregation property.

decompose the change in the number of firms in a industry-city dell c, j as:

$$\Delta Firms_{c,j} = \frac{Firms_{c,j,2014} - Firms_{c,j,2000}}{Firms_{c,j,2000}} = \frac{\sum\limits_{t=2001}^{t=2014} Entry_{c,j,t} - \sum\limits_{t=2001}^{t=2014} Exit_{c,j,t}}{Firms_{c,j,2000}}$$

We report the results in columns 7 and 8 of Table 11. We find that the reform leads to a substantial increase in both firm entry *and* exit, supporting the view that financial development fosters a process of creative destruction. In terms of magnitude, the increase in the number of new firms created is almost two time bigger than the increase in number of firms (column 7 vs. column 6), which highlights the importance of having panel data rather than cross-sectional data in order to fully grasp changes in firm dynamics as positive local shocks accelerate churn.

Table 11: Effect on Economic Development, City-Industry Level

Dependent Variable	Employment			# Firms			Entry	Exit
Unit of analysis	City	City×Ind.		City	City×Ind.		City×Ind.	City×Ind.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated×Post	0.098***	0.098***	0.126***	0.104***	0.104***	0.167***	0.303***	0.136***
	(0.015)	(0.013)	(0.016)	(0.012)	(0.011)	(0.014)	(0.028)	(0.020)
Match×Year FE	✓	✓	—	✓	✓	—	—	—
Match×Industry×Year FE	—	—	✓	—	—	✓	✓	✓
Observations	5,333	521,369	521,369	5,333	511,058	511,058	511,058	511,058

This table reports the effect of the policy on economic development at the city-by-(4-digit) industry level. Data are collapsed as an average "pre" ($t < 2004$) and the average "post" ($t \geq 2004$) periods, and each dependent variable is the midpoint growth rate $g^X_{j,c} = [(X_{j,c,t} + X_{j,c,t-1}) \times 0.5]$. Each cell is weighted by $g^X_{j,c}/(\sum_{j\in c} g^X_{j,c}) \times pop_{2000}$. In columns (1)-(4)-(7), the sample is at the city-by-year level. In all other columns, the sample is at the city-by-(4 digit) industry-by-year level. Standard errors are clustered at the city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

7.2 Government programs

One potential concern is that the expansion of government and social welfare programs might be correlated with the entry of government-owned banks in treated cities after 2004. Of special concern is the far-reaching cash transfer program Bolsa Família, which was introduced one year before our reform, in 2003. We think that this concern is unlikely to explain our results for four reasons.

First, this mechanism is inconsistent with some of our results: (i) additional income from government programs could serve as a positive income shock, fostering growth by driving up local demand. This would imply that non-tradable sectors grow faster than tradable sectors, which is the exact opposite of what we find in Table 4. (ii) While government transfers can affect *income* inequality, there is no reason it should affect *wage* inequality a priori. A possible connection would be that higher government transfers increase the reservation wage of workers, but this would imply an increase in workers' bargaining power that should mostly benefit workers at the bottom of the wage distribution, thereby predicting a *reduction* in wage inequality rather than the *increase* we find in Figure 8.

Second, the most ambitious programs, such as Bolsa Família, are distributed directly by government-owned banks. Since our control cities have a branch of a public bank by design, this implies that control cities have the same access as treated cities to government programs disbursed through government-controlled banks.

Third, we show in Figure 3 that treated and control cities are similar in their government expenditures, as well as in the likelihood that the mayor is affiliated with Lula's party (the Worker's Party). Therefore, even if, post 2004, the Lula government decided to expand social transfers particularly to places with more left-leaning voters, both treated and control cities would benefit from such an expansion in the same way.

Fourth, we test if the point estimates for our main outcomes are affected when we directly control for total local government expenditures or the political affiliation of the mayor. We show in Appendix Table A5 that our results remain quantitatively the same when controlling only for local government expenditures (column 14), whether the mayor is affiliated to Lula's party (column 15) or both at the same time (column 16) . The inclusion of these controls imply that the effect of the reform is estimated by comparing cities that have similar political inclinations and welfare spending. We also show in Appendix Table A6 that results are similar when we include state×year fixed effects, implying that differences in state-level welfare programs or differences in political incentives at the state level cannot explain our results.

Finally, we provide additional evidence that our results are not driven by government programs by exploiting the fact that some of the largest government programs, like Bolsa Família, are distributed by a specific government bank: Caixa Econômica Federal. If our results were driven by Bolsa Família or other welfare programs, they would be strongest when treated cities are compared with control cities that did *not* have a branch of Caixa, as, in this case, treated cities would benefit from the welfare program expansion and control cities would not since, by construction, control cities do not have access to the distributor of the program. We report results of this exercise in Table A8 in the Appendix. Unlike what we would expect if results were driven by government programs, we find that, if anything, results are weaker when no Caixa branches were present in control cities prior to the reform.

7.3 Sample composition

Our results on changes in inequality might be partially driven by a change in the worker composition in treated cities. Inequalities might increase for instance because following the reform, more low productivity workers enter the sample, pushing the mean wage of low-skill workers downward. We investigate this possibility in Table 12, in which we measure inequality using the city-level variance of log wage.[36] This allows us to measure wages as the residual of a Mincer equation including different worker characteristics. The inclusion of these characteristics is equivalent to holding fixed the sample composition along these dimensions.

In column 1, we report the result when we use the raw wage. In column 2, we add a third-order polynomial on age and fixed effects for sex and seven categories of race.[37] In column 3 we include 2-digit industry fixed effects and in column 4 we include 2-digit industry-by-2 digit occupation fixed effects (4,479 distinct dummies). Finally, in columns 5 and 6, we use the unfiltered wage, but restrict to the sample of workers present from 2004 to 2014 (column 5) and to firms present prior to the reform (column 6) to estimate whether our effect are driven by a change in the entry / exit of workers or firms.[38] Across all the different level of controls, we find an overall stable effect of the reform, with higher financial development leading to more inequality.

7.4 Informality and exposure to commodity sector

Note that columns 5 and 6 of Table 12 show that our results are robust to restricting to workers and firms already in the formal sector, and thus suggest our findings are not driven by workers and firms moving into or out of the informal sector. We complement these results by controlling for the city-level employment in the informal sector from the 2000 Census, which we include as one of controls in column (6) of Table A5 in the Appendix. This confirms once again that our results are not driven by the informal sector. These results are in line with the fact that treated and control cities have the same level of informality prior to the shock, as shown in the covariate balance test of Figure 3.

We also directly test if exposure to the commodity sector could explain our results by controlling for employment in the commodity sector (column 9 of Table A5), or for the

36. We use the variance instead of the Gini here because the Gini requires only positive values, but residualizing wages leads to potential negative values. By contrast, the variance is always well defined.

37. There are six race categories in RAIS: Indigenous, White, Black, Asian, multiracial, and not reported. We also include missing race values as a seventh category so as not to exclude those observations from this analysis.

38. Results are similar when we require firms to be present throughout the period. We only condition on firms exiting pre-reform because the increase in firm exit post reform and workers losing their firm-specific human capital or firm-specific shared rent could be a channel through which financial development affects inequality.

Table 12: Variance of Wages

Dependent variable	Var[log(Wage)]					
Fixed effects	None	Age×Sex ×Race	Industry	Industry ×Occupation	Workers 2004–2014	Firms 2004
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	0.015***	0.014***	0.011***	0.010***	0.021***	0.013***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
City FE	✓	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓	✓
Observations	79,995	79,995	79,995	79,995	79,980	79,995

This table shows the effect of the reform on the change in the variance of log(wage) at the city level. From columns 2 to 5, we use as the wage the residual of a Mincerian regression, after we have filtered a polynomial of age (age, age-square, age-cube) and fixed effects for gender and seven race categories (column 2), added 2-digit industry fixed effects (column 3), and 2-digit industries× 2 digit occupation fixed effects (column 4)). In columns (5) and (6), we use the unfiltered wage, but restrict to the sample of workers present from 2004 to 2014 (column 5) and to firms present prior to the reform (column 6). Standard errors are clustered at the city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

change in commodity prices post reform. We construct this variable as the weighted sum of prices across the fourteen main commodities in Brazil, similar to the measure developed by Benguria, Saffie, and Urzúa (2018).[39] Our results remain quantitatively the same, consistent with the analysis of Table 11, where we non-parametrically control for sector-specific shocks.

7.5 Other robustness checks

As we discuss in Section 3, we conduct a number of other robustness checks relating to our matching procedure and empirical specification.

We show in Table A3 in the appendix that results are robust to using different numbers of control cities. In Table A4, we show results are not sensitive to the matching procedure. In panel A we replicate our results in the baseline sample. In panel B, we additionally exact match on quintiles of the share of skilled workers pre-reform. In panel C, we exact match on quintiles of the share of manufacturing pre-reform and, in panel D, we exact match on quintiles of the level of inequality pre-reform. In all cases, the point estimates of all the outcomes are quantitatively very similar.

In Table A5, we include a collection of additional city-level controls, such as GDP, employment, skilled employment, political affiliation of the mayor, trade, distance to the state capital, and the comovement of local GDP with aggregate fluctuations. These results help rule out the possibility that our results are driven by differential exposure of treated cities to aggregate shocks or by political connections. In particular, including for state-by-year fixed effects implies that we control for any state-level political cycles, and controlling for the po-

39. We would like to thank the authors for generously sharing their measure with us.

litical affiliation of the mayor implies that we estimate our effects by comparing cities where mayors have the same political affiliation.

Given that the reform may have a direct impact on many city characteristics, we control for the pre-reform value of these characteristics interacted with year fixed effects. Finally, we also show in Table A6 in the Appendix that results are robust to adding state-by-year fixed effects to control for time-varying unobserved variation across regions of Brazil.

8 Relation to macro-development models

This paper presents causal evidence on the effect of financial inclusion on economic development and inequality, which can provide moments to inform and discipline macro-development models that study these relationships. Our paper also highlights margins not present in existing models, which could help future models better match the dynamics of financial development and wage inequality. In this section, we discuss how our estimates relate to macro-development models and summarize key moments that these models may target.

Role of access to financial intermediaries. Our results highlight the importance of bank entry and financial inclusion on economic development. One recent model that explicitly considers how the distance to the nearest bank affects the cost and availability of both credit and savings instruments, such as deposits, is Ji, Teng, and Townsend (2023). In this model, the amount of borrowing depends on the share of wealth that entrepreneurs can pledge (i.e., the tightness of the borrowing constraint), which is itself a function of cash savings, deposit savings and an upfront market-specific credit entry cost. Both the upfront credit entry cost and the cost of adjusting balances on interest-earning savings products depend linearly on the distance to the nearest bank. Table 1 of Ji, Teng, and Townsend (2023) reports calibrating the slope of the credit entry cost to the sensitivity of loan access to bank distance and of the portfolio adjustment cost to the sensitivity of the deposit-to-cash ratio to bank distance, with both sensitivities being obtained from empirical correlations.

Our estimates of the effect of financial inclusion on credit and deposits can provide causal moments to help identify the slope of the relationship between bank distance and the credit entry cost and the portfolio adjustment cost, respectively. As we discuss in Section 5, our results are best interpreted as the effect of bank entry or, alternatively, as a reduction in the distance to the nearest bank to zero.[40]

To map more closely the parameters used in macro-models, we re-estimate equation (1)

40. The pre-reform average distance to the nearest bank for treated cities is 8.66 kilometers. Restricting attention to treated cities initially without a private bank, the pre-reform average distance to the nearest bank is 23.59 kilometers.

using credit and deposits among new branches scaled by the city GDP. In Table A1, we report an increase in credit of 2.3% of the city GDP and in deposits of 2.5% of the city GDP for treated cities relative to control cities.[41] Note that, since we do not observe banking correspondents, these estimates likely represent a lower bound, as they do not capture the expansion in credit in cities that only received a banking correspondent and not a bank branch. Conditioning on treated cities that did receive a branch of a public bank, we estimate an increase in loans of 3.4% of GDP and in deposits of 2.4%.

Finally, the results on economic development that we present in Table 3 can also be used to evaluate model predictions about employment, firm growth, and wages as a result of this expansion in access to banks, thus providing additional identified moments that can be used to discipline macro-finance development models.

External financing and economic development. A broader class of models does not explicitly consider the extensive margin of access to credit but includes credit market frictions that, when severe enough, will imply that the economy operates close to autarky.[42] Our results are most supportive of frictions relating to monitoring costs, such as credit entry costs or intermediation costs (e.g., Greenwood and Jovanovic, 1990; Greenwood, Sanchez, and Wang, 2010; Dabla-Norris, Ji, Townsend, and Unsal, 2021) but might also help inform a large literature that models credit frictions as a reduced-form collateral constraint, which can represent a wide range of credit-market imperfections (see references in the literature review). The results on economic development that we present in Table 3 can be used to evaluate model predictions as a response to a reduction in financial frictions that leads to an increase in new loans corresponding to 1.7% (lower bound) to 3.2% (upper bound) of GDP. However, we highlight that our results most likely speak to the effect of financial inclusion and not simply a credit expansion where credit is already available. The large effect on economic development that we identify, which is considerably larger for treated cities that are farther away from a nearby bank (Table 5), points toward the existence of a non-linearity around very low levels of external finance. In that case, this evaluation would only be appropriate in an environment where the baseline level of financial frictions is severe enough to imply that there is initially little to no credit.

Importance of worker heterogeneity. In the workhorse model of occupational choice with financial constraints (e.g., Giné and Townsend, 2004; Buera, Kaboski, and Shin, 2011),

41. The average GDP per capita of treated cities between 2000 and 2014 is BRL 9,212.05.

42. The literature so far has mostly modeled three types of financial constraints: credit entry costs, which capture fixed transaction costs to access credit; a collateral constraint, which limits loan amounts by pledgeable assets; and intermediation costs, modeled as a wedge between the interest rate charged on loans and the deposit rate. A recent paper by Dabla-Norris, Ji, Townsend, and Unsal (2021) offers a framework to include the three frictions directly in a standard macro-development model.

agents are heterogeneous in wealth and entrepreneurial productivity, and can choose to be workers or entrepreneurs. If they decide to be a worker, they are paid the equilibrium wage.[43] Therefore, while the *average* wage is affected by who becomes an entrepreneur—as this affects aggregate labor demand and supply—the gains accrue equally to all workers, as individuals are homogeneous in their labor productivity.

The type of inequality that this class of models is able to study is therefore wealth inequality, or *income* inequality, i.e., the sum of capital income (which is the business income of entrepreneurs and the interest income households obtain from interest-bearing saving products) and labor income. In this case, capital income is usually a force that pushes income inequality up with financial development, and labor income is a force that pushes income inequality down.

Our paper focuses on labor income inequality (i.e., *wage* inequality). It shows that wage inequality increases with financial development when workers are heterogeneous, suggesting that incorporating skill heterogeneity in macro-finance development models is important to fully explain how finance affects inequality. Indeed, even in a country like Brazil, the vast majority of the population are workers and not entrepreneurs. This is important because, while income inequality is likely larger than wage inequality, wage inequality potentially matters a great deal for aggregate inequality. According to the 2000 Census, we find that the Gini *wage* inequality index among employees is 0.54, barely smaller than the *total income* inequality index of 0.60 among the total population (employees plus entrepreneurs).[44]

9 Conclusion

In this paper, we show that the expansion of financial access and capital deepening promoted by the Brazilian government led to a permanent increase in economic development, driven both by an expansion of existing businesses and an increase in firm creation, which accelerated the exit of existing firms. These effects materialize over time, underlying the need to study a long-enough period to capture the true effect of one-time reforms on long-run development.

This important economic development triggered a substantial rise in wage inequality, which is mostly explained by the limited supply of skilled labor in some cities. This result raises the question of whether governments should also implement simultaneous labor-oriented policies in order to reap the full benefit of formal financial market policies.

43. A notable exception is Cagetti and De Nardi (2006) who allows individuals to have different productivity as workers and entrepreneurs, but focus on wealth inequality.

44. The fact that a large part of the Gini index of total income is explained by wages in the aggregate is not that surprising since, in Brazil, over 72% of the population are employees and not entrepreneurs. This high fraction confirms that understanding the drivers of wage inequality among employees is an important avenue for better understanding how financing frictions affect aggregate inequality.

The importance of financial inclusion also has potential implications for current and future policy on digital banking. Such policies are already underway in some developing countries with the goal of expanding financial access, including in Brazil with the launch of an instant payment platform (Pix) and its mandatory use by all financial institutions and payment institutions that are licensed by the Central Bank of Brazil. This initiative, by improving deposit provisions and lowering costs of access to liquidity services, should boost development and firm growth, particularly in tradable industries. However, whether this initiative alone will achieve its intended goals or if, instead, it would be complementary with the expansion of physical bank branches remains an open debate. It might well be the case that the expected gains from Pix will materialize mostly in places where households and businesses are already banked if the initial connection to the formal finance sector requires the physical presence of a bank branch.

Our results also imply that policies such as digital banking that increase financial inclusion for retail customers and for small and medium-sized enterprises could be a source of substantial increase in inequality in the future if they interact with other frictions, such as the limited supply of human capital, that are prevalent in many developing countries.

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A.1 Appendix
FOR ONLINE PUBLICATION

A.2 Why do banking deserts exist in developing countries?

The permanent shift in deposits and credit, together with the sustained effect on economic activity that the financial inclusion policy triggered, implies that treated cities that received a bank branch were profitable markets. Then why did banks not serve these markets prior to the reform? Two reasons can explain this pattern.

First, on the revenue side, monopolistic competition and downward-sloping demand curves imply that banks can maximize their markups and hence their profits by restricting quantities, which will restrict bank expansion.[45] The limit on bank network expansion will be amplified when the uncertainty about the profitability of possible markets is high (e.g., Hellmann, Murdock, and Stiglitz, 1997), which is likely the case for many smaller cities in a developing country like Brazil.[46]

Second, on the cost side, the existence of fixed costs to set up a branch and the need to minimize the distance across branches to minimize costs implies that banks do not decide on branch locations in an unconstrained way, but instead will extend their network in a capillary way and up to a certain limit, particularly in a country as geographically large as Brazil. Therefore, cities where banks would be profitable if historical branch networks were different may remain unbanked.

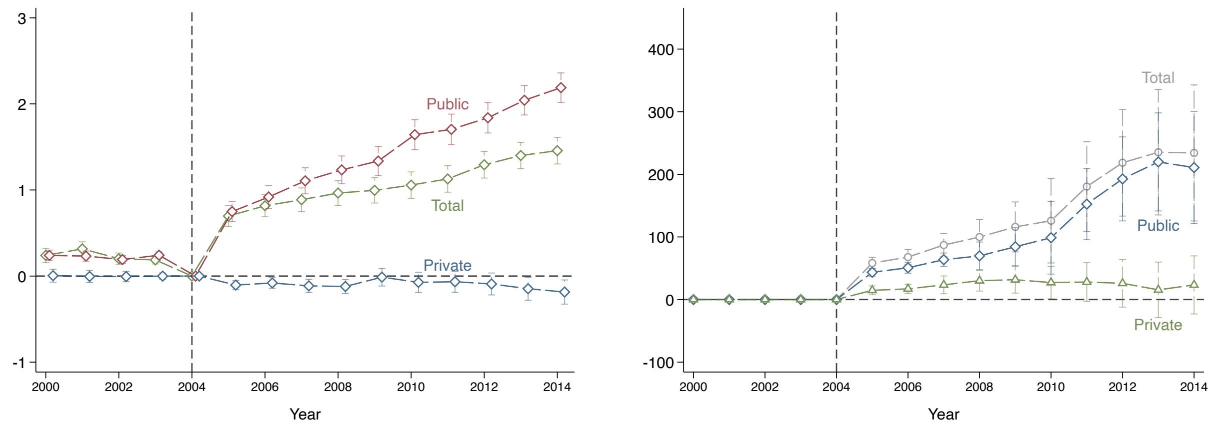
The revenue and cost reasons that explain why banks did not serve these cities before the reform can also explain why the entry of public banks did not crowd in private banks, despite evidence that these cities have solvent demand.[47]

45. Maximizing bank mark-up will result in cities not having a branch under the hypothesis that the geographical market served by a branch can be larger than the administrative unit "city." While our data does not allow us to provide direct evidence, this idea is consistent with our results on distance to bank branches across cities in Section 5.2.2 and in line with the findings in Assuncao, Mityakov, and Townsend (2020).

46. This puzzling feature of limited bank expansion to lower margin but profitable markets is actually a feature that has been found in many countries, including developed countries with an active financial sector like the US. See Célerier and Matray (2019) for evidence and references on this phenomenon.

47. A way to model why profit-maximizing private banks would not enter following the entry of public banks is to have a model where public banks maximize profits while, at the same time, caring about total financial access. Assuncao, Mityakov, and Townsend (2020) shows that such a model can explain well the geographical expansion pattern of government-controlled banks vs. private banks in Thailand.

Figure A1: Effect of the Program on Total Credit and Deposits

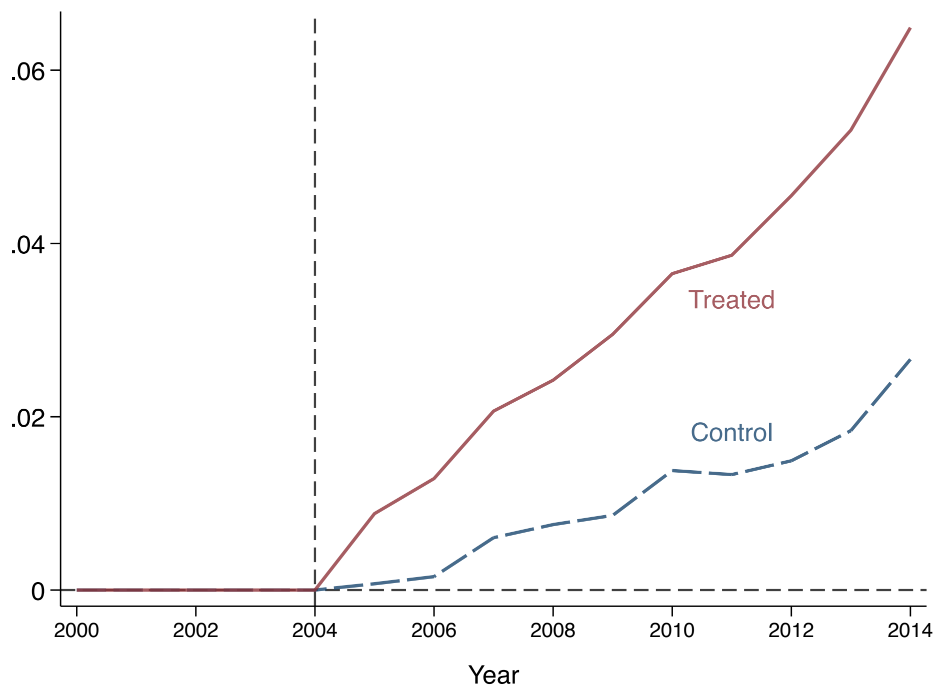


(a) Total credit

(b) Total deposits

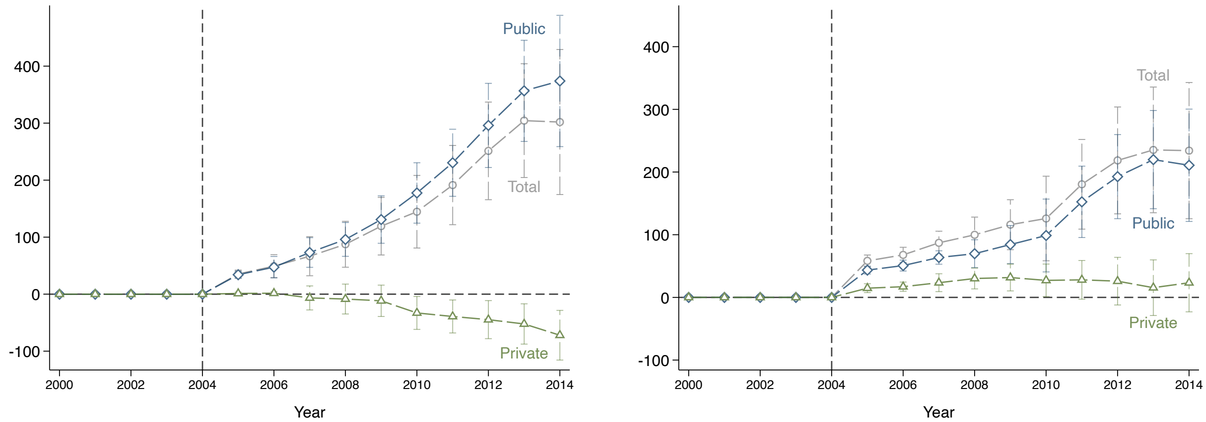
This figure plots the yearly coefficients and their 95% confidence intervals of the difference-in-differences estimator in equation (1) of the 2004 bank reform. Total credit and total deposits are scaled by the city GDP. Each LHS variable is the inverse hyperbolic sine transformation of the log function, defined as $log[X + (X^2 + 1)^{1/2}]$. Standard errors are clustered at the matching-pair level.

Figure A3: Evolution of Total Credit among New Branches



This figure shows the evolution of loans granted by branches that open after 2004, scaled by city GDP in treated and control cities.

Figure A2: Effect of the Program on Loans and Deposits at new bank branches

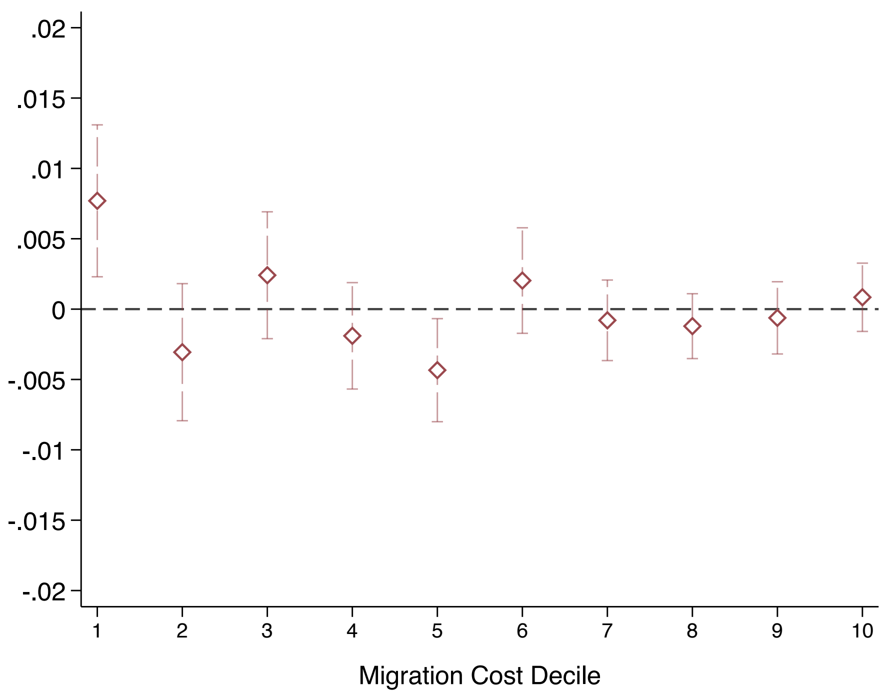


(a) Credit

(b) Deposits

This figure plots the yearly coefficients and their 95% confidence intervals of the difference-in-differences estimator in equation (1) of the 2004 bank reform. New Loans per GDP and New Deposits per GDP are, respectively, loans and deposits in 2010 BRL from branches that were opened after the program, divided by the city population. Note that coefficients prior to 2004 are equal to zero by construction. Standard errors are clustered at the matching-pair level.

Figure A4: Effect of the Program on Migration by Migration Cost



This figure shows the effect of the reform (along with 95% confident intervals) on the share of movers at the city level, split by deciles of migration cost. Movers are workers that we observe in a different city before the reform. We proxy for migration cost with the share of movers during the pre-reform period.

Table A1: Effect of the Program on Credit and Deposits among New Branches

Dependent Variable:	Deposits			Credit		
	All	Public	Private	All	Public	Private
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	0.025***	0.019***	0.006***	0.023***	0.023***	-0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)
City FE	✓	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓	✓
Observations	79,995	79,995	79,995	79,995	79,995	79,995

This table shows the effect of the reform on financial development outcomes at the city level. Credit and deposits are both scaled by city GDP and are computed for branches created after 2004 onward in both treated and control cities. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.

Table A3: Robustness to Different Numbers of Matched Controls

Dependent variable:	Has Public Branch	# Firms	Employment	Wage	Gini
	(1)	(2)	(3)	(4)	(5)
	Panel A: Two control cities per match				
Treated×Post	0.428***	0.091***	0.096***	0.045***	0.015***
	(0.016)	(0.012)	(0.015)	(0.005)	(0.002)
	Panel B: One control city per match				
Treated×Post	0.432***	0.044***	0.064***	0.041***	0.016***
	(0.016)	(0.014)	(0.017)	(0.006)	(0.002)
City FE	✓	✓	✓	✓	✓
Match-Year FE	✓	✓	✓	✓	✓
Observations	42,450	42,450	42,450	42,450	42,450

This table shows the effect of the reform on our key outcome variables at the city level using different numbers of control cities. Has Public Branch variables is a dummy that equal one if the city has a branch of a public bank. Dependent variables in columns 2–4 are in logs. Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A2: Robustness, Adding Controls Progressively

Dependent variable	# Firms	Employment all	Employment small firms	Employment large firms	Average wage	# Industries
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: City - Year fixed effects			
Treated×Post	0.133***	0.132***	0.252***	0.158***	0.050***	0.060***
	(0.017)	(0.016)	(0.026)	(0.024)	(0.010)	(0.007)
City FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
			Panel B: City - Population quintile×year fixed effects			
Treated×Post	0.143***	0.141***	0.269***	0.163***	0.050***	0.064***
	(0.017)	(0.016)	(0.025)	(0.025)	(0.009)	(0.007)
City FE	✓	✓	✓	✓	✓	✓
Population Quintile×Year FE	✓	✓	✓	✓	✓	✓
			Panel C: City - Match cell×year fixed effects			
Treated×Post	0.098***	0.100***	0.214***	0.116***	0.041***	0.047***
	(0.011)	(0.015)	(0.023)	(0.018)	(0.005)	(0.006)
City FE	✓	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓	✓
Observations	79,995	79,995	79,947	79,995	79,995	79,995

This table shows the effect of the reform on our key outcome variables at the city level using . Standard errors are clustered at the matching pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A4: Robustness to Alternative Matching Procedures

Dependent variable:	Has Public Branch	# Firms	Employment	Wage	Gini
	(1)	(2)	(3)	(4)	(5)
			Panel A: Baseline		
Treated×Post	0.425***	0.098***	0.100***	0.041***	0.012***
	(0.017)	(0.011)	(0.015)	(0.005)	(0.002)
			Panel B: Population + Share skill		
Treated×Post	0.437***	0.089***	0.090***	0.039***	0.012***
	(0.017)	(0.010)	(0.015)	(0.006)	(0.002)
			Panel C: Population + Share manufacturing		
Treated×Post	0.425***	0.098***	0.100***	0.041***	0.012***
	(0.017)	(0.011)	(0.015)	(0.005)	(0.002)
			Panel D: Population + Inequality (level)		
Treated×Post	0.425***	0.092***	0.084***	0.040***	0.014***
	(0.016)	(0.011)	(0.015)	(0.005)	(0.002)
City FE	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓
Observations	81,390	81,390	81,390	81,390	81,390

This table shows the effect of the reform on our key outcome variables at the city level under different matching procedures. Has Public Branch variables is a dummy that equal one if the city has a branch of a public bank. Dependent variables in columns 2–4 are in logs. Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A5: Robustness to Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Panel A: Dependent variable: Has Public Branch																
Treated×Post	0.424***	0.423***	0.424***	0.424***	0.424***	0.425***	0.424***	0.424***	0.426***	0.425***	0.426***	0.425***	0.426***	0.424***	0.424***	
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.017)	(0.016)	(0.016)	(0.017)	(0.017)	(0.016)	(0.017)	(0.017)	(0.016)	(0.016)	
Panel B: Dependent variable: # Firms																
Treated×Post	0.097***	0.096***	0.096***	0.097***	0.097***	0.098***	0.097***	0.098***	0.099***	0.100***	0.095***	0.101***	0.104***	0.096***	0.097***	
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	
Panel C: Dependent variable: Employment																
Treated×Post	0.103***	0.102***	0.101***	0.101***	0.101***	0.100***	0.101***	0.101***	0.101***	0.101***	0.101***	0.104***	0.107***	0.101***	0.101***	
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	
Panel D: Dependent variable: Wage																
Treated×Post	0.040***	0.039***	0.040***	0.040***	0.040***	0.041***	0.040***	0.041***	0.042***	0.042***	0.042***	0.041***	0.040***	0.040***	0.041***	
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
Panel E: Dependent variable: Gini																
Treated×Post	0.012***	0.013***	0.012***	0.012***	0.012***	0.012***	0.012***	0.012***	0.012***	0.012***	0.013***	0.012***	0.012***	0.012***	0.012***	
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
Fixed Effects																
City	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	
Match×Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	
Fixed Effects																
$\text{Population}_{\text{pre}}$	✓	—	—	—	—	—	—	—	—	—	—	—	—	—	—	
GDP_{pre}	—	✓	—	—	—	—	—	—	—	—	—	—	—	—	—	
$\text{Employment}_{\text{pre}}$	—	—	✓	—	—	—	—	—	—	—	—	—	—	—	—	
$\text{Priv. credit}_{\text{pre}}$	—	—	—	✓	—	—	—	—	—	—	—	—	—	—	—	
$\text{Priv. deposits}_{\text{pre}}$	—	—	—	—	✓	—	—	—	—	—	—	—	—	—	—	
$\text{Informal sector}_{\text{pre}}$	—	—	—	—	—	✓	—	—	—	—	—	—	—	—	—	
$\text{Skilled employment}_{\text{pre}}$	—	—	—	—	—	—	✓	—	—	—	—	—	—	—	—	
$\text{GDP-comovement}_{\text{pre}}$	—	—	—	—	—	—	—	✓	—	—	—	—	—	—	—	
$\text{Employment commodities}_{\text{pre}}$	—	—	—	—	—	—	—	—	✓	—	—	—	—	—	—	
$\text{Commodities price boom}_{\text{pre}}$	—	—	—	—	—	—	—	—	—	✓	—	—	—	—	—	
$\text{Trade}_{\text{pre}}$	—	—	—	—	—	—	—	—	—	—	✓	—	—	—	—	
$\text{Migrants}_{\text{pre}}$	—	—	—	—	—	—	—	—	—	—	—	✓	—	—	—	
$\text{Distance state capital}_{\text{pre}}$	—	—	—	—	—	—	—	—	—	—	—	—	✓	—	—	
$\text{Gov't expenditures}_{\text{pre}}$	—	—	—	—	—	—	—	—	—	—	—	—	—	✓	—	
$\text{Workers' party}_{\text{pre}}$	—	—	—	—	—	—	—	—	—	—	—	—	—	—	✓	
Observations	79,995	79,995	79,995	79,995	79,995	79,995	79,995	79,995	79,995	79,995	79,995	79,995	79,995	79,995	79,995	

This table shows the effect of the reform on our key outcome variables at the city level controlling for a wide range of city characteristics. We use the pre-reform value of these controls interacted with year fixed effects. Has Public Branch variables is a dummy that equal one if the city has a branch of a public bank. Dependent variables in Panels B–E are in logs. All the controls are defined using their value in 2000 interacted with a year fixed-effect. Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A6: Robustness to State Fixed Effects

Dependent variable:	Has Public Branch	# Firms	Employment	Wage	Gini
	(1)	(2)	(3)	(4)	(5)
Treated×Post	0.436***	0.060***	0.054***	0.031***	0.014***
	(0.016)	(0.013)	(0.016)	(0.006)	(0.002)
City FE	✓	✓	✓	✓	✓
Match-Year FE	✓	✓	✓	✓	✓
State-Year FE	✓	✓	✓	✓	✓
Observations	79,995	79,995	79,995	79,995	79,995

This table shows the effect of the reform on our key outcome variables at the city level with the inclusion of state-year fixed effects. Has Public Branch variables is a dummy that equal one if the city has a branch of a public bank. Dependent variables in columns 2–4 are in logs. Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A7: Main Results, No Weighting

Dependent variable	# Firms	Employment all	Employment small firms	Employment large firms	Average wage	Gini	Skill premium
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated×Post	0.073***	0.074***	0.184***	0.085***	0.028***	0.007***	0.054***
	(0.007)	(0.010)	(0.015)	(0.012)	(0.003)	(0.001)	(0.006)
City FE	✓	✓	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓	✓	✓
Observations	79,995	79,995	79,947	79,995	79,995	79,995	79,901

This table reports the effect of the policy on Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A8: Robustness to Government Program Disbursement

Dependent variable:	Has Public Branch	# Firms	Employment	Wage	Gini
	(1)	(2)	(3)	(4)	(5)
Treated×Post	0.521***	0.163***	0.121***	0.049***	0.014***
	(0.030)	(0.019)	(0.026)	(0.011)	(0.002)
Treated×Post×Caixa	-0.152***	-0.103***	-0.033	-0.013	-0.002
	(0.035)	(0.023)	(0.031)	(0.012)	(0.003)
City FE	✓	✓	✓	✓	✓
Match-Caixa-Year FE	✓	✓	✓	✓	✓
Observations	79,995	79,995	79,995	79,995	79,995

This table shows robustness to whether public branches belong to Caixa, the official bank of most government programs, or other government owned banks. Caixa is a dummy that equals one if no cities in the control group had a branch from Caixa before the reform. Has Public Branch variables is a dummy that equal one if the city has a branch of a public bank. Dependent variables in columns 2–4 are in logs. Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A9: Demand for Skilled Workers

Dependent variable:	Skill premium	Wage skilled	Wage unskilled	Share skilled workers
	(1)	(2)	(3)	(4)
Treated×Post	0.019***	0.014*	-0.003	-0.001
	(0.007)	(0.009)	(0.003)	(0.001)
City×Industry FE	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓
Industry×Year FE	✓	✓	✓	✓
Observations	692,606	716,875	1,566,588	2,325,570

This table shows the effect of the reform on the skill premium (column 1), the average wage of skilled and unskilled workers (columns 2 and 3), and the share of workers that are skilled (column 4) at the city-(2 digit) industry level. Skilled workers are defined as workers with at least a high school degree. All dependent variables are in logs. All dependent variables are in log. Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A10: Effect on Gini: Heterogeneity in Skill Supply with Continuous Measures

Dependent variable:	All workers													
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Treated×Post	0.013***	0.010***	0.011***	0.011***	0.010***	0.009***	0.009***	0.006***	0.007***	0.008***	0.009***	0.010***	0.011***	0.003***
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treated×Post×Skill gap	-0.008***		-0.008***	-0.008***	-0.006***	-0.009***	-0.009***	-0.008***	-0.009***	-0.009***	-0.008***	-0.007***	-0.008***	-0.007***
	(0.001)		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treated×Post×Share skilled population		-0.007***	-0.008***	-0.007***	-0.009***	-0.008***	-0.012***	-0.004***	-0.011***	-0.010***	-0.009***	-0.007***	-0.008***	-0.009***
		(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treated×Post×Employment per capita				-0.001										0.010***
				(0.001)										(0.002)
Treated×Post×Share skilled labor force					0.010***									0.009***
					(0.001)									(0.001)
Treated×Post×Employment						0.004***								-0.012***
						(0.001)								(0.002)
Treated×Post×GDP per capita							0.009***							0.006***
							(0.001)							(0.001)
Treated×Post×Population								0.008***						0.018***
								(0.001)						(0.002)
Treated×Post×Number of firms									0.007***					0.004***
									(0.001)					(0.001)
Treated×Post×Number of bank branches										0.006***				0.001
										(0.001)				(0.001)
Treated×Post×Total credit											0.006***			0.001
											(0.001)			(0.001)
Treated×Post×Share agriculture												-0.004***		-0.002**
												(0.001)		(0.001)
Treated×Post×Share manufacturing													0.000	-0.002*
													(0.001)	(0.001)
City FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	72,015	79,995	72,015	72,015	72,015	72,015	72,015	72,015	72,015	72,015	72,015	72,015	72,015	

This table shows the effect of the reform on the Gini index at the city level. In column 2, we interact Treated×Post with fraction of population with at least 11 years of education. In column 3, we estimate the ratio of skilled workers in treated cities relative to the national average, and interact this ratio with Treated×Post. Standard errors are clustered at the matching-pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.