The Real Effects of Banking the Poor: Evidence from Brazil*

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March 2022

Abstract

We study how financial development affects economic development and wage inequality. We use a large expansion of government-owned banks into Brazilian cities with low bank branch coverage and combine it with data on the universe of employees from 2000–2014. We find that higher financial development fosters firm growth, higher labor demand, and higher average wages, especially for cities initially in banking deserts. However, these gains are not shared equally. Instead, they increase with workers' productivity, implying a substantial increase in wage inequality. The changes to 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. Our results are inconsistent with alternative explanations such as differential exposure to Brazil's economic boom, an overall increase in government lending, and other government or social welfare programs. These results motivate embedding skill heterogeneity into macro-finance development models in order to capture these distributional consequences.

*We thank Victor Duarte and Chenzi Xu for support and numerous discussions. We also thank Shawn Cole (discussant), Tatyana Deryugina, Pascaline Dupas, Melanie Morten, Chad Jones, Ben Moll, Jacopo Ponticelli, Rebecca de Simone (discussant), Yongseok Shin, and seminar participants at Stanford Economics, Stanford GSB, 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, UGA (Georgia), University of Arizona, and Imperial College for helpful comments. Filipe Correia, Thomás Gleizer Feibert and Peilin Yang provided excellent research assistance.

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1 Introduction

Financial underdevelopment is seen as a major impediment to economic growth from a macroeconomic perspective.[1] The World Bank estimates that about 1.7 billion people, the majority of whom are in developing countries, lack access to financial services (World Bank Findex Database, 2017). This underprovision has motivated policymakers across the world to address this problem through reforms promoting financial inclusion (e.g., China in the 1970s, Thailand in the 1980s and 1990s), but we still know little about their impact. What are the gains in terms of economic growth and how are these gains distributed?

Answering these questions requires understanding the micro-level dynamics underlying the aggregate and distributional effects of financial development, including how financial frictions interact with other friction such as limits to human capital accumulation. Studying these patterns can also shed light on the exact frictions and assumptions needed to build macro-development models capable of explaining how one-time policies can shape economic outcomes in the long-run.

In this paper, we trace out the dynamic effects on both economic development and wage inequality of a government program that lifted Brazilian cities from financial autarky, and we link differences in inequality dynamics to frictions in supply of human capital. We exploit the introduction of the "Banks for All" program (*"Banco para Todos"*) by the Brazilian federal government in 2004, that explicitly targeted underbanked cities by introducing government-owned banks. This policy affected financial development by fostering financial inclusion and increasing the overall amount of credit. It is a unique natural experiment featuring a large exogenous shock to financial access and capital deepening at the level of entire labor markets.

Our empirical analysis combines Brazilian administrative employer-employee data over 2000–2014 covering the universe of formal employees in Brazil with detailed bank branch balance sheets. 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 construct a control group of cities for each treated city where we match on pre-reform population quintile and Gini growth, and we estimate the effect of financial development on employment, entrepreneurship,

1. We provide a review of the literature at the end of the introduction.

firm growth, average wages, and wage inequality.

Brazilian matched employer-employee data contain more socio-demographic information than most similar datasets in other countries and, in particular, contains the precise education level of each worker and a detailed classification of her occupation in the firm. Together with the panel nature of the data, this allows us to track heterogeneous individuals over time to better understand how and why wage inequality evolves. The data also allow us to separate the effect of financial development on wage inequality coming from changes in labor demand from the effect coming from investment in human capital (or lack thereof).

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 showing that it is unlikely to be violated, which we discuss in detail after summarizing our results. Broadly, 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 has a large effect on the financial development of treated cities both on the extensive and intensive margins. The number of bank branches and the overall amount of credit increases substantially after 2004 and do not mean revert in the following decade. Consistent with financial development being driven by our reform, we find that all the gains come from the expansion of government-owned banks. By contrast, the number of branches and credit from private banks stays roughly constant. The limited crowding-out of private banks by government banks explains the overall increases in the number of banks and in credit. At the same time, the absence of an effect on private credit can be seen as a "placebo test," showing that our results are unlikely to be driven by alternative explanations where treated cities are differentially exposed to economy wide shocks experienced by

Brazil during this period. Indeed, higher economic activity fueled by, for instance, differential exposure of treated cities to the 2000s' commodity boom should lead to differential trends in private lending as well.

Our second set of results is about the *average* effect of the reform on economic development. We show that the reform leads to an extensive margin increase of 9.8% in the number of firms, and an intensive margin increase in the average size of establishments existing prior to the reform by 10%. This results in an increase in employment of 10%, which pushes up the average wage per worker by 4.1%. These results are consistent with the loosening of financial constraints allowing both talented but poor entrepreneurs to create firms, and productive but financially constrained firms to expand, resulting in higher labor demand that pushes wages up. We find that the real effects of the reform are gradual and improve at a slower pace than the change in financing frictions, in line with models where relaxing financial frictions creates a gradual reallocation of resources, in particular via the progressive entry of productive but poor entrepreneurs and exit of unproductive ones (e.g., Buera and Shin, 2013).

While the average effects are consistent with most macro-development models, the richness of our data allows us to examine the mechanisms that link financial development and economic development. First, financial expansion could foster growth by increasing aggregate demand since even loans targeting business development are often used for consumption in developing countries.[2] We rule out this local demand channel as the main driver of our results by showing that the economic expansion is, if anything, concentrated in the tradable sector, which is by definition less dependent on local demand.

We then turn to the ways in which financial development would stimulate business investment and labor demand. We contrast the two main classes of models that provide microfoundations for how financial frictions impact business development: models in which the distance between lenders and borrowers affects the cost for financial intermediaries to screen and monitors projects (e.g., Greenwood and Jovanovic, 1990; Townsend and Ueda, 2006; Greenwood, Sanchez, and Wang, 2010), and models in which large non-convex investment costs limit entrepreneurs' ability to save their way out of "poverty traps" over time (e.g., Buera, Kaboski, and Shin, 2011; Midrigan and Xu, 2014). Our results provide clear support for the importance of monitoring costs,

2. See for instance: Kaboski and Townsend, 2012; Devoto et al., 2012; Breza and Kinnan, 2021.

while we find no evidence for an explanation based on non-convex investment costs. In particular, we find that the effect of the policy is concentrated in cities that are in banking deserts. In contrast, when looking within cities and across industries, we find no evidence that industries that operate at larger scale—a common proxy for large fixed costs—grow faster after the reform.

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. 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 reflect 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. This indicates in particular that our results are not driven by workers entering the formal sector after the reform.

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., 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 changes, with firms increasing the share of skilled workers in their workforce. However, when looking at the effect of the policy on the skill composition, 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 start by showing that cities in our setting are characterized by high internal migration costs. Despite a substantial increase in the skill premium of 9% due to the policy, we find a very small increase in skilled workers migrating into a treated city. The migration that occurs is concentrated in the subset of treated cities with the lowest migration costs. 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 different in ways ex-ante that expose them differentially 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 on 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 treated 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 react differently to broader macroeconomic shocks post 2004.

In addition, we show (ii) 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.

In terms of ex-post treated-specific shocks, our setting addresses a wide array of potential shocks because, by construction, control cities already have a government-owned bank. Therefore, any shocks specific to government-owned bank (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. Another main source of ex-post shocks is the possibility of an expansion of welfare programs targeting treated cities. Once again, our design addresses this point directly, as 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 control and treated cities. Other programs might be distributed directly at the state or city level. We show that controlling for state-specific shocks by including state-by-year fixed effects, comparing solely treated and control cities with the same left-leaning affiliation, or directly controlling for the amount of local government expenditures does not affect our results.

We end the paper by discussing how our reduced-form identified coefficients can provide useful causal moments for the macro-finance development literature, and 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 Ji, Teng, and Townsend (2021). We provide causal estimates of how changes in distance can affect credit supply and saving 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 can be useful for macro-development models in which a reduced-form collateral constrain can affect economic growth. The considerably larger effect we find for treated cities in banking deserts points 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 to account 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. First, we contribute to the empirical literature studying how financial frictions affect economic development using natural experiments.[3] This literature has mostly focused on the introduction of specific bank branches to study the consequences of financial outreach in Mexico (Bruhn and Love, 2014) or India (e.g., Burgess and Pande, 2005; Barboni, Field, and Pande, 2021) and finds small, short lived positive effects, or even negative effects (Kochar, 2011).[4]

Other papers have looked at changes in financial depth by studying a targeted lending program in India (Banerjee and Duflo, 2014), Brazil (Bazzi, de Freitas Oliveira, Muendler, and Rauch, 2021), a bankruptcy reform in Brazil (Ponticelli and Alencar, 2016; Fonseca and Doornik, 2021), changes in deposit inflows (e.g., Bustos, Garber, and Ponticelli, 2020) or large government grants in Thai villages (e.g., Kaboski and Townsend, 2011; Kaboski and Townsend, 2012).

A complementary approach exploits randomized control 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."[5] Structural work that incorporates the general equilibrium effects of microcredit generate more ambiguous results, with Breza and Kinnan (2021) finding large positive effects, while Buera, Kaboski, and Shin (2021) concludes that there are limited gains.

Our contribution to this literature is threefold. First, the use of long panel data allows us to track the long-run effect of formal financial policies promoting financial development on economic outcomes. Second, the intervention we study is very large, capable of creating important "local general equilibrium" effects, including on people who do not directly benefit from the bank expansion. This is in contrast with most of the literature that has used randomized control trials or specific bank-shocks and

3. An earlier literature looks at how financial frictions relate to economic development using cross country evidence. This literature is reviewed in Beck, Demirgüç-Kunt, Laeven, and Levine (2008) and Beck and Levine (2018), for instance. See also Xu (2022) and Xu and Yang (2022) and references therein for the importance of financial frictions in cross-country trade and growth, and long-run historical contexts.

4. An important exception is Barboni, Field, and Pande (2021), which studies branch expansions of rural banks in India using a randomized control trial. See also Célerier and Matray (2019) for large positive effects of bank branch expansions on low-income households in the U.S.

5. Works in this literature conduct randomized control trials in Bosnia and Herzegovina (Augsburg, De Haas, Harmgart, and Meghir, 2015), Ethiopia (Tarozzi, Desai, and Johnson, 2015), India (Banerjee, Duflo, Glennerster, and Kinnan, 2015), Mexico (Angelucci, Karlan, and Zinman, 2015), Mongolia (Attanasio et al., 2015), and Morocco (Crépon, Devoto, Duflo, and Parienté, 2015). A notable exception is Karlan and Zinman (2010), who finds large positive effect in the context of consumer credit in South Africa.

mostly focused on borrowers directly affected. The positive effects on non-borrowers 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. Third, we have administrative data that allow us to observe the universe of employment across all formal firms, and in particular the right tail of the firm size distribution that has disproportionate importance on aggregate outcomes, instead of focusing only on a subset of (small) individual firms.

Our finding that most of the effect of the policy comes from cities in banking deserts also contributes to the literature that estimates how the location of bank branches affects financial intermediation costs. In particular, these results provide empirical support to models emphasizing the importance of financial intermediation costs for economic growth, such as Greenwood and Jovanovic (1990), Greenwood, Sanchez, and Wang (2010), and Ji, Teng, and Townsend (2021), which map these costs onto the market's distance from bank branches.

Second, we contribute to the literature studying the effect of financial development on wage inequality. Theoretical work in this literature focuses mostly on wealth inequality or total income inequality (which include capital income) and finds ambiguous effects. 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; Falcao Bergquist et al., 2019; Buera, Kaboski, and Shin, 2021; Besley et al., 2020), 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 increasing wages pushes inequality downward (e.g., Besley et al., 2020; Buera, Kaboski, and Shin, 2021; Ji, Teng, and Townsend, 2021).

An important assumption underlying the results obtained in this literature is 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.[6] We contribute to this field by showing that financial

6. A separate literature has studied how worker heterogeneity interact with labor market frictions and can affect development and labor misallocation, but these models do not incorporate financing frictions, entrepreneurship and firm growth with heterogeneous entrepreneurial talents. See a review of some of these models in Herrendorf, Rogerson, and Valentinyi (2014), or recent examples in

development can increase wage inequality in the presence of worker heterogeneity such as skill differentials and therefore 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.

Third, we contribute to the broad literature that studies how financial frictions affect economic development via its effect on capital and entrepreneurial talent misallocation.[7] More specifically, we relate to the macro-finance development literature that incorporates financial frictions in occupation choice models. Since at least Giné and Townsend (2004), this literature (surveyed in Buera, Kaboski, and Shin (2015) and Buera, Kaboski, and Shin (2021)) models individuals with heterogeneous productivity deciding between working as an employee or becoming an entrepreneur, often assuming that sectors in the economy also differ in productivity and that investment requires paying an upfront fixed cost. These models generally predict that financial frictions affect both firm creation and the ability of existing firms to grow. The detailed nature of our panel data allows us to estimate both the extensive and the intensive margin effect of financial development on firm growth to test these predictions.

More broadly, we relate to the literature studying how financial frictions affect firm labor demand and employment outcomes.[8] We contribute to the specific subset of the literature that studies how financial frictions affect the demand of skilled workers and the skill premium (e.g., Fonseca and Doornik (2021) in Brazil or Quincy (2020) in the US during the Great Depression), as well as how constraints on human capital accumulation shapes development paths (e.g., Jones and Romer (2010); Hsieh, Hurst, Jones, and Klenow (2019); Rossi (2020) for a review, and Porzio, Rossi, and Santangelo (2021) or Jones (2022) for recent examples).

Finally, because the reform we explore relies on the expansion of government-

Lagakos et al. (2017) or Porzio (2017) and references therein.

7. 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 (2020).

8. See among many others: Peek and Rosengren, 2000; Chodorow-Reich, 2014; Duygan-Bump, Levkov, and Montoriol-Garriga, 2015; Dix-Carneiro and Kovak, 2017; Bai, Carvalho, and Phillips, 2018; Berton, Mocetti, Presbitero, and Richiardi, 2018; Hombert and Matray, 2017; Benmelech, Frydman, and Papanikolaou, 2019; Caggese, Cunat, and Metzger, 2019; Bottero, Lenzu, and Mezzanotti, 2020; Greenstone, Mas, and Nguyen, 2020; Bernstein, Colonnelli, Malacrino, and McQuade, 2021; Doornik, Gomes, Schoenherr, and Skrastins, 2021.

owned banks, we relate to the broad literature studying the economic effects of government ownership of banks (e.g., Sapienza, 2004; Dinç, 2005; Cole, 2009; Carvalho, 2014; Delatte, Matray, and Pinardon Touati, 2020; Garber, Mian, Ponticelli, and Sufi, 2021). 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 (e.g., Ferraz and Finan, 2008; Avis, Ferraz, and Finan, 2018). More broadly, we also study how public institutions, in our case banks, shape labor markets in Brazil (e.g., Ferraz, Finan, and Szerman, 2016; Colonnelli, Prem, and Teso, 2020; Colonnelli and Prem, 2021).

2 Institutional Background and Data

2.1 The Banks for All Program

Government-owned banks account for nearly half of bank lending in Brazil but were unevenly distributed geographically prior to 2004, with around 60% of municipalities having no physical presence of government-owned banks. Due to the crucial role that government-owned banks play in reaching underserved communities in Brazil (Mettenheim, 2010), this unequal distribution likely contributed to the fact that nearly 40% of Brazilians were unbanked at the time.[9]

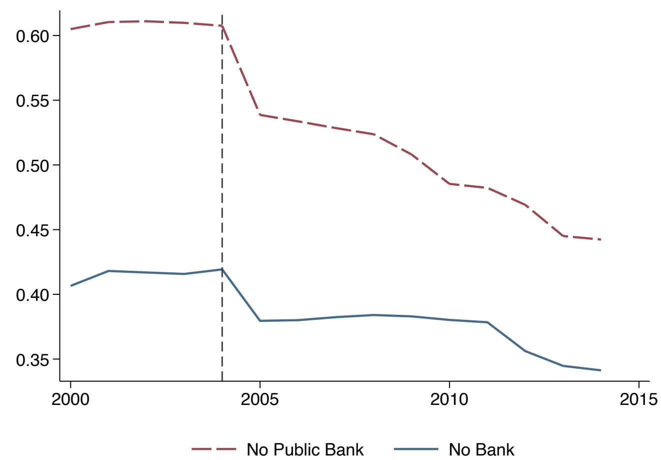
Banks for All (*Banco para Todos*) was a federal government program announced in 2004 as part of the government's 2004–2007 multi-year plan (*Plano Plurianual*). The program was under the purview of the Finance Ministry (*Ministério da Fazenda*) and had the goal of providing Brazil's unbanked population with access to financial services and products.

To achieve this goal, the federal government promoted the physical presence of public banks throughout the country. 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

9. The Central Bank of Brazil estimates that 60.81% of adults had a banking relationship in 2005, the first year for which data is available.

government-owned banks. Figure 1 also shows the share of municipalities without any bank branch (the solid blue line), and shows that expansion of public banks resulted in a drop in the share of cities without any bank branches.

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.

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).[10] 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.

In order to reach the unbanked, 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. Financial services offered by correspondents can include the opening of accounts, deposits and withdrawals, payments, and loan applications.[11] The number of correspondents went from fewer than 50,000 in 2003 to

10. For comparison, there were approximately 16 million individuals residing in the cities that were eligible for the program according to the 2007 population count.

11. In the case of credit card and other loan applications, correspondents collect data from the applicant and forward it to the financial institution for processing (Kumar, Nair, Parsons, and Urdapilleta, 2006).

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).

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 Brazilian 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 worker average gross monthly earnings, occupation and several socio-demographic characteristics such as 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 compared 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 provide bank branch balance sheet information at the city level, which allows us to decompose the number of branches, credit, and deposits between public and private banks. Note that this data does not include correspondent banking outlets, which means that we do not observe the full impact of the program on financial inclusion. We discuss this issue 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). We also obtain cross-sectional data in 2000 from the Census, such as population distribution across years of schooling and share of workers in informality.

3 Empirical strategy

Since the reform promoted financial inclusion by targeting cities with no government-owned banks, 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.[12]

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, 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 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, partially fueled by a commodity boom, that Brazil entered into during our sample period. Figure 2 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.

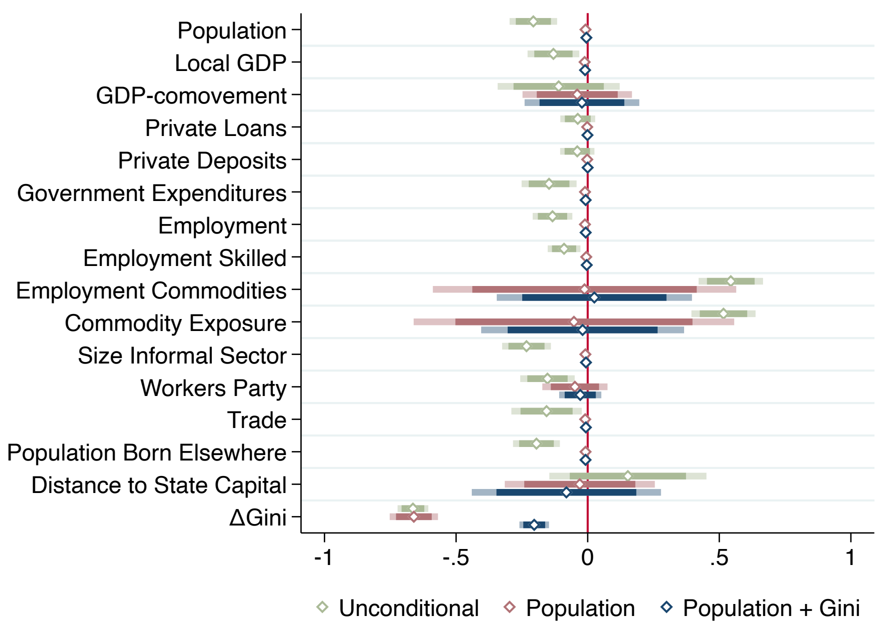
In order to address this challenge and to strengthen our empirical strategy and the plausibility of our identifying assumptions, we use a parsimonious matching approach to construct a control group of untreated cities that is observably similar to treated cities on a wide set of characteristics.

Matching. Our matching strategy first targets city size. We start with all 4,260 cities and compute quintiles of population. We then match with replacement each treated city with all control cities in the same population quintile. Applying this parsimonious approach addresses a large part of the heterogeneity. The red dots in Figure 2 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

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

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 to 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) for almost all variables.

Figure 2: Covariate Balance



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.

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; Helpman, Itskhoki, Muendler, and

Redding, 2017) 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 2 show differences between treated and control 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 6).

After our baseline matching procedure, we are left with 1,415 treated cities and a total of 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 3. Treated and control are spread out across Brazil and do not show geographical clustering.

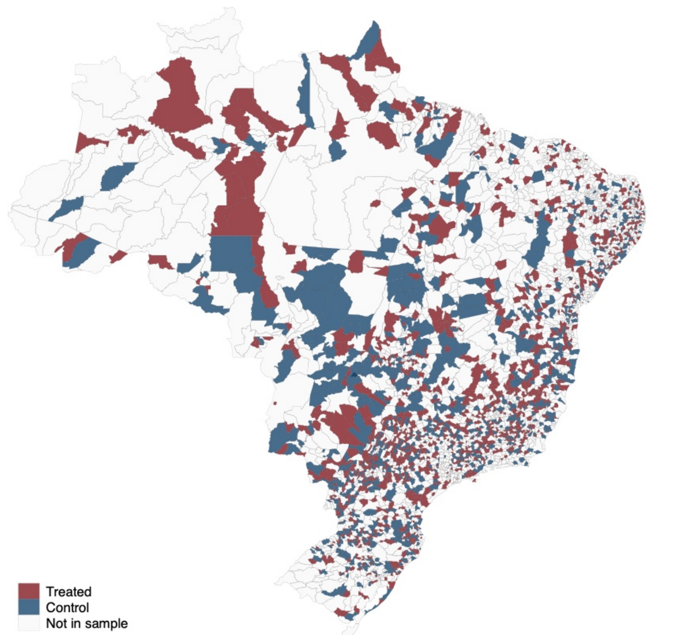
Table 1: Summary Statistics

	Mean	Med.	St. Dev.	N
Loans per Capita	2,178.97	1,318.48	2,471.04	4,713
Public Loans per Capita	1,947.36	1,110.20	2,379.00	4,713
Private Loans per Capita	231.61	53.34	445.12	4,713
Total Branches	1.72	1.00	1.25	4,713
Public Branches	0.92	1.00	0.63	4,713
Private Branches	0.80	1.00	0.91	4,713
Deposits per Capita	1,446.11	1,041.17	1,469.56	4,713
Public Deposits per Capita	1,055.89	733.40	1,232.78	4,713
Private Deposits per Capita	390.21	102.22	629.18	4,713
Wage	913.01	881.31	268.35	4,713
Total Employment	1,023.15	648.00	1,446.76	4,713
Share Skilled	0.09	0.08	0.05	4,713
Skill Premium	2.28	2.14	0.69	4,713
Gini Index	0.31	0.31	0.05	4,713
Population	12,156.20	9,031.00	12,474.92	4,713
GDP per Capita	13,581.36	9,500.44	23,343.73	4,713
Share Manufacturing	0.21	0.14	0.20	4,713
Share Agriculture	0.14	0.09	0.14	4,713

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.

Econometric specification: city level. We analyze the effect of an increase in bank coverage on economic development and inequality by estimating a series of

Figure 3: 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.

matched difference-in-differences 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 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., in 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. We cluster our standard errors at the city 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.

Identifying assumptions and potential threats to identification. This strategy faces two main threats to identification: (i) Even if treated and control cities are perfectly similar ex-ante, unobserved ex-post shocks might only 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 these concerns below.

(i) Treated-specific ex-post shocks. Even with perfect ex-ante balance of covariates 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 would be the case if, for instance, public banks experience idiosyncratic shocks that affect their credit supply *after* 2004 (either due to shocks to their cost of funding or because they are required by the government to extend credit), or if the Lula government expanded welfare programs such as Bolsa Família specifically to places that benefited from the financial inclusion reform.

Our empirical strategy is designed precisely to address this issue as, by construction, public banks are present in control cities prior to the reform. This implies that in our setting, any bank-specific shock post 2004 (such as changes to cost of funding or political pressure) will affect both treated and control cities. For instance, if politicians pressure public banks to expand credit, both treated *and* control cities will benefit from a credit expansion, and our coefficient of interest will not be biased.

This characteristic of our setting also addresses concerns about a potential correlation between financial inclusion policies and other social welfare programs due to a specificity of the Brazilian institutional context, which is that 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 7.1 and find no evidence that it biases our results.

Our setting is therefore conceptually different from ones where no city has a bank and identification is achieved by bank entry in some cities and not others. Instead, our context directly deals with potential ex-post shocks to the public banking sector, as those are common to both treated and control.

(ii) Covariate balance and ex-ante differences. The second main concern is that ex-ante differences lead to a violation of the parallel-trend assumption.

We address this problem in four ways. First, as we show in Figure 2, 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 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 A2 and A3 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.[13]

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:

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

13. 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.

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 5.2.

4 Effect on financial inclusion

We start by testing whether the reform had an effect on financial development or whether the expansion of government-owned banks simply led to a pure substitution between government-owned banks and private banks. We estimate Equation 1 with a dummy variable for whether a city has a bank branch and with new loans per capita as outcome variables, which we then split between government-owned banks and private banks. We define new loans and new deposits, as loans and deposits from branches that were opened after the reform.[14]

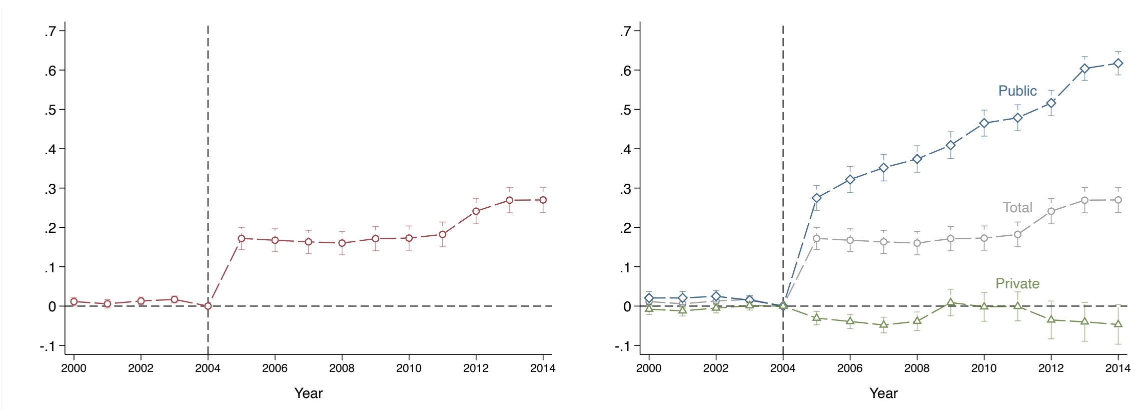
In Figure 4, we report the event study coefficients of our difference-in-differences estimation for the dummy for having a bank branch. Panel (a) shows results for any bank branch, while panel (b) decomposes the total change (the grey circles) into the change coming from public banks (the blue diamonds) and private banks (the green triangles). 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, i.e. were on similar trends even after the reform. Second, the expansion of public banks barely 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.

We also show analogous plots for new loans per capita and new deposits per capita

14. This definition implies that new loans and new deposits per capita will equal zero for both treatment and control units prior to the reform.

in Figure A1 in the Appendix.[15] 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. Panel (b) of Figure A1 reports analogous results for new deposits per capita, and shows that deposits increase sharply in 2005 and continue to rise throughout the post-reform period. Unlike loans, private deposits increase modestly after the reform, implying some "crowding-in" of public bank expansions to deposits in private banks.

Figure 4: 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 a branch of any bank, a public bank, or a private bank, respectively.

We report pooled estimates in Table 2. 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 18.7 p.p. (column 1), new loans per capita increases by BRL 155 (column 4) and new deposits per capita increases by BRL 142 (column 7). These different variables are driven by the expansion of public banks (columns 2, 5, 8). These results confirm that the expansion of public banks increases the overall amount of branches and credit in the city, as public banks do not crowd out private banks. We also find that the policy has a long-lasting effect, as the number of branches and volume of credit do not mean revert after 2004. In

15. By definition, these variables equal zero for both treated and control units prior to the reform. This means that, unlike Figure 4, this exercise should not be interpreted as a test of the parallel trends assumption.

this respect, the policy can be interpreted as a change in the steady state of local financial development, rather than a one time infusion of capital.[16]

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

Dependent Variable:	Has Bank Branch			New Loans per Capita			New Deposits per Capita		
	All	Public	Private	All	Public	Private	All	Public	Private
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated×Post	0.187***	0.425***	-0.022*	155.164***	181.635***	-26.470**	142.325***	118.632***	23.692*
	(0.015)	(0.016)	(0.013)	(28.461)	(24.569)	(11.574)	(25.428)	(19.738)	(12.096)
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. Has Bank Branch variables are dummies that equal one if the city has a branch of any bank, a public bank, or a private banks, respectively. New Loans per capita and New Deposits per capita are, respectively, loans and deposits in 2010 BRL from branches that were opened after the program, divided by population. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

As a robustness check, we report estimates that can be interpreted as percentage changes. Because treated cities have no government-owned banks by construction, which introduces multiple zeros in our dataset, we report results using the inverse hyperbolic sine transformation of the log function defined as: $log[X + (X^2 + 1)^{1/2}]$.[17]

We report event studies for total credit and total deposits using the inverse hyperbolic sine transformation in Figure A2 and pooled estimates in Table A5 in the Appendix. The event studies show that credit and deposits for treated and control units evolve in close parallel prior to the reform. The expansion in credit and deposits is therefore entirely driven by public banks, with minimal crowding out of private banks.

As we discuss in Section 2.2, our data does not include information on correspondent banking outlets, which were also widely used by government-owned banks to promote financial inclusion. Therefore, our estimates understate the true effect of

16. This is an important distinction relative to the literature studying microcredit using randomized control trials or the "Thai Million Baht Village Fund program" experiment analysed in Kaboski and Townsend (2011), Kaboski and Townsend (2012).

17. 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.

the reform on financial development and, for that reason, we focus throughout the paper on the reduced-form effect of the policy on different outcome variables instead of using instrumental-variable methods.

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 reform-induced development of the local financial sector affects the different elements of this causal chain by estimating Equation 1 with the total number of firms, average establishment size, total employment, and average wage in the city 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%, while the size of establishments existing prior to the reform increases by 10.1%. This expansion in the number of firms and in the size of existing firms translates into an increase in the demand for labor, with the number of employees rising by 10% (column 3) and wages increasing on average by 4.1%.

In columns 5 and 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 and reduces the concentration of economic activity. We measure the number of industries with the number of distinct 2-digit industries (column 5) and the concentration of economic activity with the HHI of employment (column 6).[18] Using 3 or 4 digit industries yield quantitatively similar estimates.

We reproduce this analysis in graphical form by estimating the event study version

18. There are 52 distinct industries and the definition is consistent over time.

Table 3: Effect of the Reform on Economic Development

Dependent variable	# Firms	Establishment size	Employment	Wage	# Industries	HHI-Industries
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	0.098***	0.101***	0.100***	0.041***	0.047***	-0.010**
	(0.013)	(0.015)	(0.016)	(0.006)	(0.007)	(0.004)
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 economic development at the city level. All variables in columns 1–6 are in logs. In column 2, the size of the establishment is defined for establishments existing prior to the reform. In column 4, "wage" is the average wage. The number of industries (column 5) is the number of distinct 2-digit industries in the city-year. In column 6, "HHI-Industries" is the industrial concentration of employment across 2-digit industries. Standard errors are clustered at the city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

of Equation 1 in Figure 5. 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.

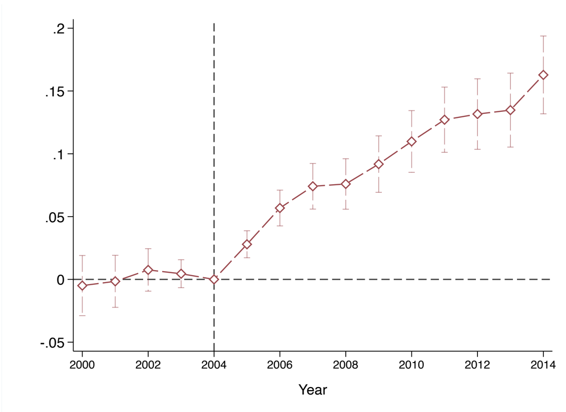
Evolution of sectoral composition. Prior work on economic development has emphasized the role of the manufacturing sector as a key source of productivity gains and changes in economic inequality (e.g., Rodrik, 2012). To analyse if the reform affected the industrial composition of cities, we estimate Equation 1 and use as dependent variables the fraction of employment across 9 sectors: agriculture, manufacturing, construction, retail, food products, transportation, finance and real estate, public administration (including education), and other services.

Table A6 in the Appendix reports the results. Overall, we find limited evidence that the industrial composition changed. In particular, we find no change in manufacturing or agriculture, and a statistically significant albeit small increase in some services like retail and construction.

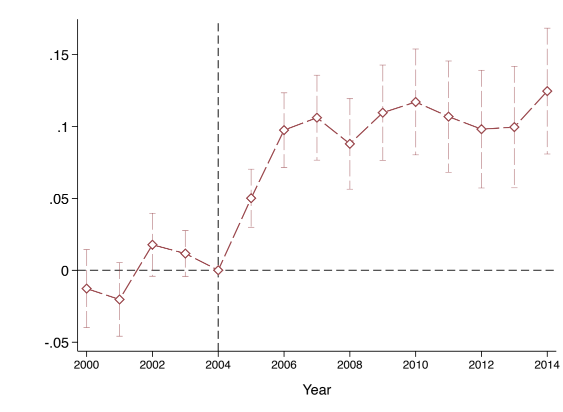
5.2 City-industry level estimation

Even though pre-reform covariates are balanced across treated and control cities (Figure 2) and we show in Appendix Table A3 that directly controlling for these levels do not affect our results, it is always possible that industry-specific shocks post 2004

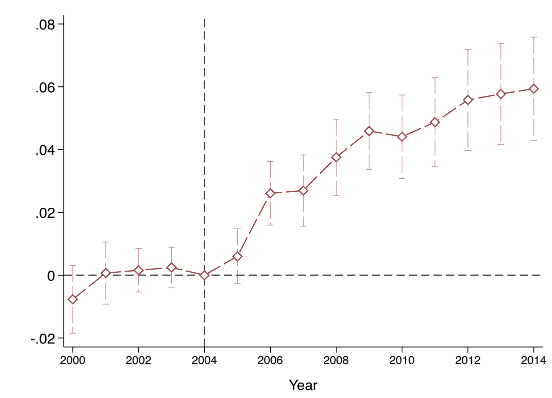
Figure 5: Effect of the Program on Firms, Employment, and Wage



(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.

might affect our results.

We directly address this problem by moving our unit of analysis to the (2-digit) industry-by-city level as specified by equation 2. This 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.

This specification raises one challenge. As column (5) of Table 3 show, the reform had an impact on the entry and exit of industries at the city level. To address the potential bias from entry / exit that makes the panel unbalanced, 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.

Because this data structure creates entry and exit of sectors, the baseline specification of Equation 1 does not guarantee that aggregate results at the city level are preserved when we disaggregate the data at the city-by-industry level. To ensure that property, we modify the specification and collapse the data to 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: $g_{j,c}^X = (X_t - X_{t-1})/[(X_{j,c,t} + X_{j,c,t-1}) \times 0.5]$.

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.

This specification has two appealing properties. First, it handles entry and exit of industries without relying on transformations of the log function, which 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 log function as it is non-linear. 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 $g_{j,c}^X$, and compute the weight as $g_{j,c}^X / (\sum_{j \in c} g_{j,c}^X)$.[19]

In Table 4, we start by reproducing the baseline results at the city-by-industry level. In columns (1), (4) and (7), 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), (5) and (8), we show that the point estimates are *identical* at the city-industry level with the weighting described above. Finally, in columns (3), (6) and (9), 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

19. In our case, because we still want to obtain an effect closer to the aggregate and to remain consistent, we multiply this weight by the population in 2000, which does not affect the aggregation property.

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.

Table 4: Effect on Economic Development: City-Industry Level

Dependent Variable	Firms			Employment			Mean wage		
Unit of analysis	City	City×Industry		City	City×Industry		City	City×Industry	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated×Post	0.104***	0.104***	0.131***	0.094***	0.094***	0.102***	0.025***	0.025***	0.048***
	(0.012)	(0.009)	(0.009)	(0.015)	(0.014)	(0.013)	(0.007)	(0.008)	(0.006)
Match×Year FE	✓	✓	—	✓	✓	—	✓	✓	—
Match×Industry×Year FE	—	—	✓	—	—	✓	—	—	✓
Observations	5,333	153,389	153,389	5,333	155,038	155,038	5,333	155,038	155,038

This table reports the effect of the policy on economic development at the city-by-(2-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-(2 digit) industry-by-year level. Standard errors are clustered at the city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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 Fazenda, 2007), but we unfortunately cannot observe it in the data. Therefore, our estimates under-estimate the true impact of the reform on finance 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 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 during 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; Dix-Carneiro and Kovak, 2017). These slow-moving changes underscores the importance of measuring and taking into account transitional dynamics when estimating the effect of reforms on economic development.

5.3 Mechanisms

There are two main channels through which financial development promotes economic growth in this setting. First, bank expansion can foster *local demand* by relaxing individuals' borrowing constraints and reducing their need for precautionary savings.[20] Since non-tradable industries are more dependent on local demand than tradable industries —since by definition tradable industries produce goods 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. 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.

5.3.1 Consumption channel

To test if most of the effect is coming from a bank expansion-induced increase in demand, we estimate equation 2 and split the regression between tradable and non-tradable. Since Brazil does not report trade data outside manufacturing, there is no obvious way to identify ex-ante tradable industries. We therefore use two methods. First, we classify an industry as "tradable" if it is in the manufacturing sector, and

20. For models where households need to maintain "buffer stocks" in the absence of well functioning insurance markets see Townsend (1994) or Kaboski and Townsend (2011). Cole et al. (2013) provide evidence for limited insurance in developing countries.

"non-tradable" otherwise. Second, we compute the geographical dispersion (HHI) of employment at the industry level, and classify tradable industries as those in first tercile or first quartile of the HHI distribution. The intuition behind this proposed measure is that 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 5. We find that employment results are not driven by non-tradable sectors and, depending on the definition, estimates are larger in tradable industries. This implies that credit-induced demand shocks are an unlikely explanation for our results.

Table 5: Employment in Tradables and Non-Tradables

Dependent variable	Employment					
Tradable Definition	Manufacturing		Tercile HHI		Quartile HHI	
Tradable	Yes	No	1^{st}	3^{rd}	1^{st}	4^{th}
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	0.085***	0.103***	0.103***	0.092***	0.107***	0.033
	(0.033)	(0.013)	(0.020)	(0.015)	(0.017)	(0.021)
Match×Industry×Year FE	✓	✓	✓	✓	✓	✓
Observations	31,480	123,558	45,530	56,586	32,800	40,716

This table reports the effect of the policy on employment at the city-by-(2-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 $g^X_{j,c}/(\sum_{j\in c} g^X_{j,c}) \times pop_{2000}$. In columns 3–6, we define tradable industries based on the geographical HHI of employment of each industry. Low HHI (columns 3 and 5) means that the industry is more concentrated geographically. Standard errors are clustered at the city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

5.3.2 Business development channel

Financial development can also foster economic growth by relaxing credit constraints, allowing poor but talented individuals to create firms and existing productive firms to expand. The two main hypotheses in macro-development models are that financial development relaxes financial frictions either because it reduces monitoring costs for banks (e.g., Greenwood, Sanchez, and Wang, 2010; Ji, Teng, and Townsend, 2021), or because productive industries such as manufacturing are characterized by large fixed costs of investment (e.g., Buera, Kaboski, and Shin (2015), Buera, Kaboski, and Shin (2021) and references therein). Interestingly, these hypotheses lead to very different predictions, offering us a chance to provide rare causal evidence for or against important assumptions in macro-development models.

The main predictions of these two hypotheses are about employment and whether the change in the total number of firms is driven by a change in entry and/or exit of firms. We test these two hypotheses by estimating the triple difference version of equation 3. We create different proxies and interact all explanatory variables and fixed effects with these dummies:[21]

$$\begin{aligned} \Delta Y_{p,c,j,t} = \beta_1 \; Treated_c \times Post_t \times \mathbb{1}Proxy_{c,j} \\ + \beta_2 \; Treated_c \times Post_t + \delta_{p,j,t} \times \mathbb{1}Proxy_{c,j} + \; \varepsilon_{p,c,j,t} \end{aligned} \tag{4}$$

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, such that:

$$\Delta Firms_c = \frac{Firms_{c,2014} - Firms_{c,2000}}{Firms_{c,2000}} = \frac{\sum_{t=2001}^{t=2014} Entry_{c,t} - \sum_{t=2001}^{t=2014} Exit_{c,t}}{Firms_{c,2000}}$$

The two proxies we use to disentangle between the monitoring and fixed costs theories build on Ji, Teng, and Townsend (2021) and Buera, Kaboski, and Shin (2011), respectively. We test for the importance of monitoring costs by comparing cities with a local (private) bank before the reform with cities that did not have a private bank and create a dummy variable *No Private Bank*$_c$ that takes the value one if the city did not have a private bank pre-reform. This proxy relies on the assumption that monitoring costs are larger when banks are farther away. These monitoring costs are potentially even more important in developing countries where most firms primarily produce soft information and are dependent on a banking system that promotes lending relationships (e.g., Rajan and Zingales, 2001, Hombert and Matray, 2017).

As a robustness check, we define a finer proxy of distance within the group of treated cities that did not have a private bank prior to the reform. For each of these cities, we compute the distance to the nearest city with a bank (public or private).

21. Note that interaction $\delta_{p,j,t} \times \mathbb{1}Proxy_{c,j}$ is essential to properly estimate the marginal effect $Treated_c \times Post_t \times \mathbb{1}Proxy_{c,j}$.

We create a dummy variable *High Distance*$_c$ that takes the value one if this measure of distance is above the sample median and reproduce the analysis for this subset of treated cities with no bank. Results are reported in Appendix Table A8 and yield similar conclusions.

For the importance of non-convex investment costs, we follow Buera, Kaboski, and Shin (2011) and use the average establishment size in the industry to create the dummy *High Fixed Costs*$_j$ that equals one if the industry is above the sample median. As before, we sort industries into high and low fixed costs using pre-reform data. The intuition behind this proxy is that in equilibrium, industries in which establishments operate at a larger scale have higher fixed costs of investment. The non-convex investment cost hypothesis therefore predicts that the effect of the reform should be stronger in industries in which establishments are larger on average. This is the exact opposite prediction as that of the monitoring cost hypothesis, as larger establishments produce more hard information and are easier to monitor.

We report results in Table 6. Since we use an interaction term, the coefficient on the variable *Treated*×*Post* shows the result for the sub-sample of cities that are at a below-median distance to the nearest bank (panel A) or industries that have below-median fixed costs (panel B). The total effect for cities farther from a bank or for industries with high fixed costs is obtained by adding the coefficient of *Treated*×*Post* with the marginal interaction term.

In panel A, we test the monitoring hypothesis. We find that employment increases much more in cities that did not have a private bank before the reform (column 1).[22] The results on the number of firms and the dynamics behind it are also consistent with the importance of monitoring costs. The number of firms increases relatively more in cities where ex-ante monitoring costs are higher (+15.2%, column 3), which implies a total effect for this group of (6.9% + 15.2% =) 22.1%. This increase is mostly driven by a marginal higher increase in new firms (+8.3%, column 4), while the number of exiting firms declines slightly relative to cities in low monitoring costs (-6.8%, column 5). We also find that wages rise relatively more in cities where ex-ante monitoring costs are higher (+12.7%, column 6).

Unpacking the effect on firm growth reveals interesting dynamics and shows the

22. The number of observations is not exactly equal between employment growth rate and average establishment size because we require establishment size to be defined both in the pre and post period.

Table 6: Financial Frictions: Monitoring vs. High Fixed Costs

Dependent Variable	Employment	Establishment size	# Firms	Entry	Exit	Wage
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Distance to nearest bank			
Treated×Post	0.060***	0.017	0.069***	0.170***	0.101***	-0.003
	(0.016)	(0.019)	(0.015)	(0.027)	(0.020)	(0.009)
Treated×Post×No Private Bank$_c$	0.116***	0.018	0.152***	0.083**	-0.068***	0.127***
	(0.027)	(0.031)	(0.023)	(0.038)	(0.026)	(0.013)
			Panel B: Fixed costs in investment			
Treated×Post	0.243***	0.058***	0.206***	0.274***	0.068***	0.101***
	(0.019)	(0.015)	(0.016)	(0.026)	(0.019)	(0.011)
Treated×Post×High Fixed Costs$_j$	-0.153***	-0.039	-0.195***	-0.181***	0.014	-0.068***
	(0.020)	(0.024)	(0.016)	(0.025)	(0.022)	(0.012)
Match×Industry×Year FE	✓	✓	✓	✓	✓	✓
Match×Industry×Year× Proxy FE	✓	✓	✓	✓	✓	✓
Observations	154,090	113,112	153,215	153,215	153,215	154,090

This table shows the effect of the expansion of public banks on the growth of employment, establishment growth, number of firms, and wage at the city-by-(2 digit) industry level. Data are collapsed as an average "pre" ($t \leq 2004$) and the average "post" ($t > 2004$) periods, and each dependent variables 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 panel A, *High Distance$_c$* is a dummy equal to one if the distance to the nearest city with a bank is above the sample median. In panel B, *High Fixed Costs$_j$* is a dummy equal to one if the industry is above the sample median of average establishment size. *High Fixed Costs$_j$* is not interacted with the fixed effects Match×Industry×Year because the proxy is defined at the industry level, and by definition already absorbed by the industry fixed effect. Standard errors are clustered at the city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

importance of having panel data relative to cross-sectional data. Indeed, while the average number of firms goes up, the number of new firms goes up by considerably more and the number of firm exits also increases following the reform. This is consistent with macro-development models of occupational choices (e.g., Giné and Townsend, 2004, Buera, Kaboski, and Shin, 2011, Kaboski and Townsend, 2011), in which financial development matters not only because it allows the average existing firm to grow, but also because it allows talented but poor individuals to start a business while untalented but unconstrained entrepreneurs exit. In this respect, our results confirm the importance of misallocation of talent across occupations in explaining economic development.

In Appendix Table A8, we show that we see similar patterns when proxying for monitoring costs with the distance to the nearest city with a bank, for cities that did not have a private bank prior to the reform. In particular, we also see higher employment growth (column 1) and firm growth (column 3) in treated cities that were farther from a bank, relative to treated cities that were closer to a bank.

The results in panel B test the non-convex cost hypothesis and show that we find no support for it. Industries with high fixed costs experience a relatively lower gain in employment (-15.3%, column 1), which is explained by the fact that the average establishment size does not increase (column 2) and the number of firms goes down (-19.5%, column 3). Note that it does *not* mean that the total number of firms or employment goes down in these industries, but just that the *marginal* effect relative to low-fixed cost industries is lower. Employment in high-fixed cost industries goes up by 9% (=24.3%–15.3%) and the number of firms goes up by 1.1% (=20.6%–19.5%). Sectors with low fixed costs display the exact opposite dynamics, with larger employment gains (column 1), an increase in average establishment size (column 2), and an increase in the number of firms (+20.6%, column 3).[23] The increase in the number of firms is driven by an even larger increase in firm entry (+27.4%, column 4), that compensates and potentially causes an increase in firm exit (+6.8%, column 5). Industries with high fixed costs also experience a relatively lower rise in wages (-6.8%, column 6).

While we find no empirical support for the non-convex cost hypothesis in our setting, the pattern displayed in panel B of Table 6 can be reconciled with the presence of fixed costs if public banks extend loans that are sufficiently small. Buera, Kaboski, and Shin (2021) show that an increase in microfinance disproportionately benefits small-scale sectors if loans are too small to finance entry into large-scale sectors. While we find that average loan amount per capita extended by public banks is more than eight times larger than by private banks (Table 1), we are unable to reject this alternative explanation without information on loan amounts per borrower.

6 Effect on inequality

6.1 Aggregate results

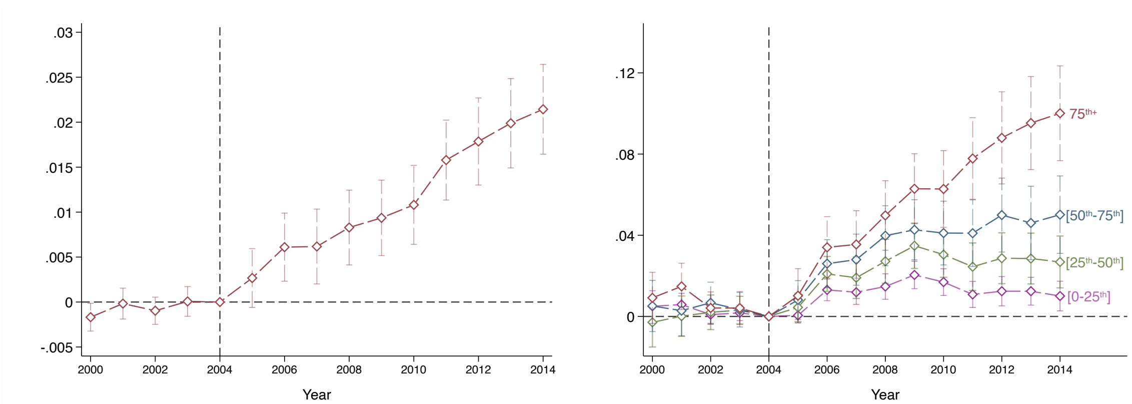
To study how an increase in financial development affects the wage distribution in each local labor market, we estimate Equation 1 using the wage Gini at the city level

23. The effect for low fixed costs is directly reported in the table with the coefficients of the variables *Treated*×*Post*.

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 6. Figure 6a 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. In Figure 6b, we report the evolution of the average wage for each quartile of the city wage distribution. 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. 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.

Figure 6: 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 (panel a). In panel b, the wage distribution is computed every year at the city level.

Table 7 reports estimates of Equation 1. All the results are significant at the 1% level. The point estimates tend to underestimate the effect of the reform on inequality

Table 7: 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)	(0.005)	(0.007)	(0.008)
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 city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

since, as Figure 6b shows, inequality rises steadily over time, while regression results show the average over the whole post-reform period. The Gini increases on average by 1.2 points (column 1) and this is explained 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.

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 (e.g., Herkenhoff, Phillips, and Cohen-Cole, 2019; Bau and Matray, 2020).

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.[24]

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, residualized 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.[25]

24. 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.

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

Table 8 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.

Table 8: 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)	(0.016)	(0.016)	(0.020)
City FE	✓	—	—	—
Match×Year FE	✓	✓	✓	—
Industry×Year FE	—	—	✓	—
Match×Industry×Year FE	—	—	—	✓
City×Industry FE	—	✓	✓	✓
Observations	1,310,489	1,310,489	1,310,489	1,310,489

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 city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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.[26]

In Table 9, 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 wage increase in the top quartile of the distribution (+5.5%, column 5-Table 7) relative to first first quartile (+1%, column 2-Table 7),

26. 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.

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.

Table 9: 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	79,995	79,995	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 logs. Standard errors are clustered at the city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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 10 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 2), and that it has a positive but very small effect when we focus on skilled workers (column 5), as the share of skilled workers coming from other cities increases by 0.7%. 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 10: Worker Migration

Sample:	All workers			Skilled workers		
Dependent variable:	Share local (1)	Share movers (2)	Share new (3)	Share local (4)	Share movers (5)	Share new (6)
Treated×Post	-0.019*** (0.004)	0.000 (0.001)	0.021*** (0.004)	-0.021*** (0.006)	0.007*** (0.002)	0.020*** (0.006)
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 logs. Standard errors are clustered at the city 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 confirm 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 A3 in the Appendix reports the result. 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.[27] 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. This measure 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. 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.

Table 11: 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 city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

We split both measures along the sample median and interact each dummy with all the variables, including the fixed effects. Table 11 reports the results. The increase in Gini (column 1) is entirely explained by the increase in inequality in cities

27. This is the closest proxy we can compute in the Census data to match our definition of skilled workers in the RAIS data.

where the fraction of skilled workers is skilled 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, and 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.

7 Robustness

7.1 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 5. (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 6.

Second and more importantly, the most ambitious programs, such as Bolsa Família, are distributed directly by government-owned banks. Since, by design, our control cities have a branch of a public bank, 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 2 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 A3 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 A4 that results are similar when we include state$\times$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 A7 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.

7.2 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.[28] This allows us to measure wages as the residual of a Mincerian 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.[29] 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.[30] Across all the different level of controls, we find an overall stable effect of the reform, with higher financial development leading to more inequality.

7.3 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 A3 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 2.

We also directly test if exposure to the commodity sector could explain our re-

28. 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.

29. 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.

30. 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***
	(0.003)	(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.

sults by controlling for employment in the commodity sector (column 9 of Table A3), or for the 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).[31] Our results remain quantitatively the same, consistent with the analysis of Table 4, where we non-parametrically control for sector-specific shocks.

7.4 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 A1 in the appendix that results are robust to using different numbers of control cities. In Table A2, 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.

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

In Table A3, 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. 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 A4 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 (2021). In this model, the amount of borrowing depends on the share of wealth 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 (2021) 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 new loans per capita and new deposits per capita 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.[32] In Table 2, we report an increase in new loans per capita of BRL 155.16 (or 1.7% of GDP per capita) and in new deposits per capita of BRL 142.33 (or 1.6% of GDP per capita) for treated cities relative to control cities.[33] 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 new loans per capita of BRL 312.26 (s.d. 31.98) and in new deposits per capita of BRL 219.53 (s.d. 33.68). This corresponds to an increase in new loans of 3.4% of GDP and in new deposits of 2.4% of GDP.

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.[34] 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 the references in the literature review). The results on economic

32. 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.

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

34. 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.

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 a 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 without the ex-ante presence of a private bank (panel A of Table 6.), 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 constrains (e.g., Giné and Townsend, 2004; Buera, Kaboski, and Shin, 2011), 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.[35] 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

35. 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.

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).[36]

9 Conclusion

In this paper, we show that the expansion of financial access and capital deepening promoted by the 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 (potentially older and less productive) 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. This finding also has potential implications for current and future policy as developing countries promote digital banking with the goal of expanding financial access, including 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. Digital banking can increase financial inclusion for retail customers and for small and medium-sized enterprises as it lowers transaction costs, but could be a source of substantial increase in inequality in the future.

36. 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.

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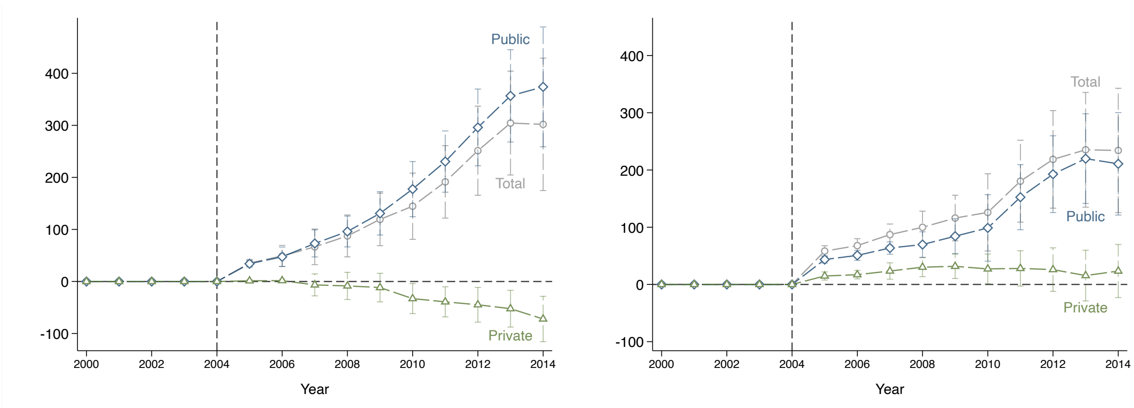
A.1 Appendix Tables and Figures
FOR ONLINE PUBLICATION

Figure A1: Effect of the Program on New Loans and New Deposits

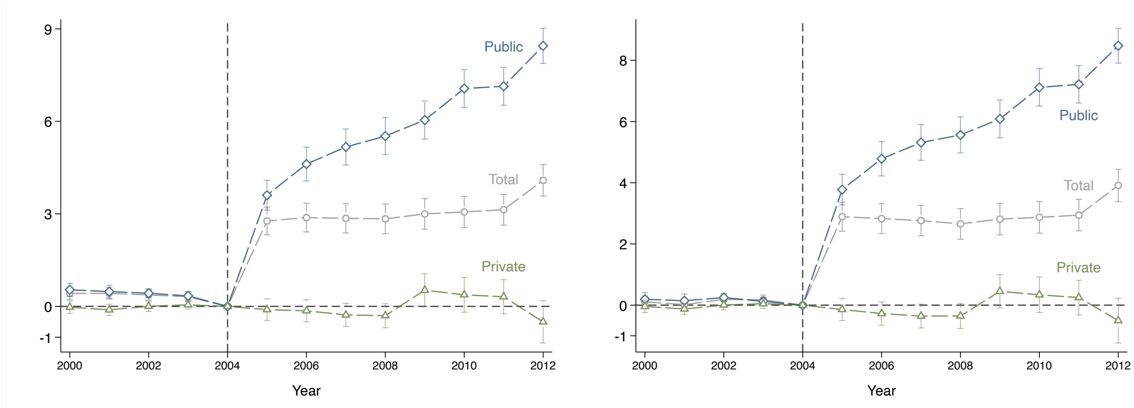


(a) New Loans per Capita

(b) New Deposits per Capita

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 Capita and New Deposits per Capita are, respectively, loans and deposits in 2010 BRL from branches that were opened after the program, divided by population. Note that coefficients prior to 2004 are equal to zero by construction.

Figure A2: Effect on Credit and Deposits in Percentage Changes

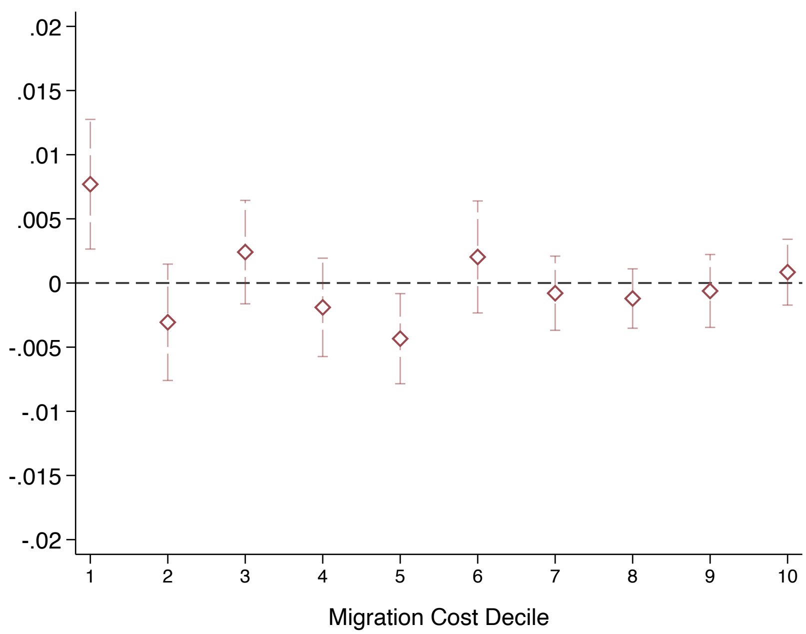


(a) Credit

(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. Dependent variables are all estimated using the inverse hyperbolic sine transformation.

Figure A3: 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: 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***
	(0.015)	(0.014)	(0.017)	(0.006)	(0.002)
City FE	✓	✓	✓	✓	✓
Match-Year FE	✓	✓	✓	✓	✓
Observations	62055	62055	62055	62055	62055
	Panel B: One control city per match				
Treated×Post	0.432***	0.044***	0.064***	0.041***	0.016***
	(0.018)	(0.017)	(0.020)	(0.007)	(0.002)
City FE	✓	✓	✓	✓	✓
Match-Year FE	✓	✓	✓	✓	✓
Observations	42450	42450	42450	42450	42450

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 city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A2: 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***
	(0.016)	(0.013)	(0.016)	(0.006)	(0.002)
			Panel B: Population + Share skill		
Treated×Post	0.437***	0.089***	0.090***	0.039***	0.012***
	(0.015)	(0.013)	(0.017)	(0.006)	(0.002)
			Panel C: Population + Share manufacturing		
Treated×Post	0.425***	0.098***	0.100***	0.041***	0.012***
	(0.016)	(0.013)	(0.016)	(0.006)	(0.002)
			Panel D: Population + Inequality (level)		
Treated×Post	0.4247***	0.0917***	0.0836***	0.0398***	0.0138***
	(0.0151)	(0.0127)	(0.0165)	(0.0062)	(0.0019)
City FE	✓	✓	✓	✓	✓
Match×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 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 city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A3: 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***	0.424***	0.424***	0.424***	0.425***	0.424***	0.424***	0.424***	0.423***	0.424***	0.423***	0.425***	0.425***	0.424***	0.424***	0.417***
	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.015)	(0.016)	(0.016)	(0.015)	(0.016)	(0.015)	(0.016)	(0.016)	(0.015)	(0.016)	(0.014)
Panel B: Dependent variable: Firms																
Treated×Post	0.098***	0.096***	0.097***	0.097***	0.097***	0.099***	0.097***	0.098***	0.099***	0.097***	0.097***	0.099***	0.103***	0.097***	0.098***	0.101***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)
Panel C: Dependent variable: Employment																
Treated×Post	0.103***	0.103***	0.102***	0.101***	0.101***	0.103***	0.101***	0.101***	0.098***	0.099***	0.103***	0.102***	0.106***	0.101***	0.101***	0.112***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.017)
Panel D: Dependent variable: Wage																
Treated×Post	0.040***	0.039***	0.040***	0.040***	0.040***	0.040***	0.041***	0.041***	0.042***	0.041***	0.040***	0.041***	0.040***	0.040***	0.041***	0.037***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Panel E: Dependent variable: Gini																
Treated×Post	0.012***	0.012***	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***	0.012***
	(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)	(0.002)
Fixed Effects																
City	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Match×Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls																
$\text{Population}_{\text{pre}}$	✓	—	—	—	—	—	—	—	—	—	—	—	—	—	—	✓
GDP_{pre}	—	✓	—	—	—	—	—	—	—	—	—	—	—	—	—	✓
$\text{Employment}_{\text{pre}}$	—	—	✓	—	—	—	—	—	—	—	—	—	—	—	—	✓
$\text{Priv. credit}_{\text{pre}}$	—	—	—	✓	—	—	—	—	—	—	—	—	—	—	—	✓
$\text{Priv. deposits}_{\text{pre}}$	—	—	—	—	✓	—	—	—	—	—	—	—	—	—	—	✓
$\text{Informal sector}_{\text{pre}}$	—	—	—	—	—	✓	—	—	—	—	—	—	—	—	—	✓
$\text{Skilled employment}_{\text{pre}}$	—	—	—	—	—	—	✓	—	—	—	—	—	—	—	—	✓
$\text{GDP-comovement}_{\text{pre}}$	—	—	—	—	—	—	—	✓	—	—	—	—	—	—	—	✓
$\text{Employment commodities}_{\text{pre}}$	—	—	—	—	—	—	—	—	✓	—	—	—	—	—	—	✓
$\text{Commodity price boom}_{\text{pre}}$	—	—	—	—	—	—	—	—	—	✓	—	—	—	—	—	✓
$\text{Trade}_{\text{pre}}$	—	—	—	—	—	—	—	—	—	—	✓	—	—	—	—	✓
$\text{Migrants}_{\text{pre}}$	—	—	—	—	—	—	—	—	—	—	—	✓	—	—	—	✓
$\text{Distance state capital}_{\text{pre}}$	—	—	—	—	—	—	—	—	—	—	—	—	✓	—	—	✓
$\text{Gov't expenditures}_{\text{pre}}$	—	—	—	—	—	—	—	—	—	—	—	—	—	✓	—	✓
$\text{Workers' party}_{\text{pre}}$	—	—	—	—	—	—	—	—	—	—	—	—	—	—	✓	✓
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	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. Standard errors are clustered at the city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A4: 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 city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A5: Effect on Bank Branches, Loans and Deposit in Percentage Changes

Dependent Variable:	Bank Branches			Loans			Deposits		
	All	Public	Private	All	Public	Private	All	Public	Private
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated×Post	0.261***	0.331***	0.005	2.864***	5.823***	-0.332*	2.951***	6.132***	-0.369**
	(0.018)	(0.019)	(0.014)	(0.224)	(0.266)	(0.184)	(0.232)	(0.266)	(0.187)
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 financial development at the city level. Dependent variables are all estimated using the inverse hyperbolic sine transformation. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Effect on Industry Composition

Dependent Variable	Share in								
	Agriculture (1)	Manufacturing (2)	Construction (3)	Retail (4)	Food (5)	Transport (6)	FIRE (7)	Administration (8)	Other services (9)
Treated×Post	-0.002	-0.010**	0.005***	0.007***	-0.001*	-0.002**	-0.002	0.009	-0.003
	(0.004)	(0.004)	(0.002)	(0.003)	(0.001)	(0.001)	(0.002)	(0.006)	(0.002)
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 2004 banking reform on industry composition at the city level. Standard errors are clustered at the city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A7: 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***
	(0.027)	(0.020)	(0.026)	(0.011)	(0.003)
Treated×Post×Caixa	-0.152***	-0.103***	-0.033	-0.013	-0.002
	(0.031)	(0.025)	(0.031)	(0.013)	(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 city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A8: Financial Frictions: Distance to Nearest Bank

Dependent Variable	Employment	Establishment size	# Firms	Entry	Exit
	(1)	(2)	(3)	(4)	(5)
Treated×Post	0.108***	-0.018	0.178***	0.220***	0.042
	(0.030)	(0.032)	(0.031)	(0.045)	(0.028)
Treated×Post×High Distance$_c$	0.122***	0.095*	0.078**	0.061	-0.017
	(0.043)	(0.049)	(0.037)	(0.056)	(0.038)
Match×Industry×Year FE	✓	✓	✓	✓	✓
Match×Industry×Year× High Distance FE	✓	✓	✓	✓	✓
Observations	53,634	38,378	53,172	53,172	53,172

This table shows the effect of the expansion of public banks on the growth of employment and firm growth at the city-by-(2 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 $g^X_{j,c}/(\sum_{j \in c} g^X_{j,c}) \times pop_{2000}$. *High Distance$_c$* is a dummy equal to one if the distance to the nearest city with a bank is above the sample median, and is only defined for cities without a private bank in the pre-reform period (2000–2004). Standard errors are clustered at the city 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)	(0.008)	(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 city 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
	(0.002)	(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)
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)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
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***
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Treated×Post×Employment per capita				-0.001										0.010***
				(0.002)										(0.003)
Treated×Post×Share skilled labor force					0.010***									0.009***
					(0.001)									(0.001)
Treated×Post×Employment						0.004**								-0.012***
						(0.002)								(0.004)
Treated×Post×GDP per capita							0.009***							0.006***
							(0.002)							(0.002)
Treated×Post×Population								0.008***						0.018***
								(0.002)						(0.004)
Treated×Post×Number of firms									0.007***					0.004
									(0.002)					(0.003)
Treated×Post×Number of bank branches										0.006***				0.001
										(0.001)				(0.002)
Treated×Post×Total credit											0.006***			0.001
											(0.001)			(0.002)
Treated×Post×Share agriculture												-0.004***		-0.002
												(0.001)		(0.001)
Treated×Post×Share manufacturing													0.000	-0.002
													(0.001)	(0.002)
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 city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.