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Abstract

We use a large expansion of government-owned banks in cities with extremely low bank branch coverage and data on the universe of formal-sector employees in Brazil over 2000-2014 to study how financial development affects economic development and wage inequality. We find that higher financial development fosters firm creation and firm expansion, which increases labor demand and leads to higher average wages, especially for cities initially located in banking deserts. The gains produced by higher financial development are not shared equally, but instead monotonically increase with workers' productivity, which leads to a substantial increase in inequality. This increase is concentrated in cities where the initial supply of skilled workers is low, showing that talent scarcity is an important driver of how financial development affects inequality.

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1 Introduction

Financial underdevelopment has been long identified as a crucial impediment to economic development both because it reduces the level of aggregate investment and because it distorts the allocation of capital across firms and talented entrepreneurs. As a result, many developed and developing countries have implemented policies to promote access to finance in lagging regions over the past forty years.[1] Such policies are important in practice; the World Bank estimates that about 1.7 billion people, the majority of whom are in poor countries, lack access to financial services (World Bank Findex Database, 2017).

While there is now a consensus that finance matters for development, less is known about the micro-level dynamics underlying the positive aggregate effects.[2] These are key to understanding the distributional effects of financial development, and whether financial development amplifies or reduces economic inequality. Studying these patterns can also shed light on the exact frictions and assumptions needed to build macro-development models and to conduct counterfactual analyses of different policies.

In this paper, we make progress on both fronts. We study how a government program that lifted cities from financial autarky in Brazil affected earnings inequality between 2000 and 2014. We exploit the introduction of the Banks for All program (*"Banco para Todos"*) by the Brazilian federal government in 2004, which explicitly targeted under-banked cities that were not served by government-owned banks. This policy affected financial development on both the extensive and intensive margin. It promoted financial inclusion by causing a large expansion in the density of bank branches and led to financial deepening by expanding the overall amount of credit. This offers us a unique natural experiment with a large, exogenous shock to financial access and capital deepening at the level of whole labor markets.

Our empirical analysis combines Brazilian administrative matched employer-employee data over 2000–2014, covering the universe of formal employees in Brazil, with detailed bank branch balance sheets and income statements. We trace how the policy in 2004 affected the reallocation of capital and labor, and provide causal evidence on the impact of the program on wage inequality.

Our setting provides us with several appealing institutional features for understanding how financial inclusion and financial deepening affect both the level and the distribution of earnings across workers. First, matched employer-employee data in Brazil contain more socio-demographic information than most similar datasets in other countries and, in particular, contain 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

1. See for instance Thailand in the 1980s and 1990s, China in the 1970s, or OECD, 2016.
2. We provide a review of the literature at the end of the introduction.

to track heterogeneous individuals over time to better understand how and why earnings inequalities change. Second, it allows us to separate the effect of financial development on inequality coming from changes in labor demand from the effect coming from investment in human capital. Third, the impact of the program on treated cities is important enough to generate quantitatively large infusions of credit across a vast number of local labor markets. Combined with the fact that treated cities have limited economic integration due the extreme spatial dispersion of cities in Brazil produced by the country's size, we can plausibly treat cities like a collection of small independent economies and interpret our estimates as "local general equilibrium effects."

We estimate the effect of financial development on earnings inequality using a matched difference-in-differences research design that compares the evolution of earnings inequality in cities benefiting from this policy relative to unaffected cities. Our identification strategy relies on ex-ante differences in the presence of government-owned banks across cities, but it does *not* require either the initial presence of government-owned banks to be random or a common support in the *level* of covariates across cities. It only requires that outcomes of treated and control cities would have evolved similarly to each other absent the reform.

We use two methods to ensure that our estimates are well-identified. First, we provide visual evidence of the evolution of key city outcomes such as employment, wages, credit and inequality around the year of the reform by estimating difference-in-differences event studies. The graphs confirm that control and treated cities evolve in parallel in the years leading to the reform and only start to diverge after 2004. Second, we saturate our difference-in-differences specification with high-dimensional fixed effects to remove as much unobserved time-varying heterogeneity as possible. This is possible because control and treated cities share significant overlap in size, skill mix, and industrial composition. In our preferred specification, we compare treated and control cities in the same quintile of population size and share of skilled workers pre-reform.

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 does not mean revert in the long-run. Consistent with this development being driven by our reform, we find that all the increase comes from the expansion of government-owned banks. By contrast, the number of branches and credit from private banks stays constant. The lack of crowding-out of private banks by government banks explains why the overall number of banks and credit increases.

Our second set of results is about the *average* effect of the reform on economic development. We show that the reform leads to a large increase of 9.8% in the number of firms, and an increase in the average size of establishments existing prior to the reform. This results in an increase in employment of 10%, which pushes the average wage per worker to rise by 4.1%. These results are consistent with the loosening of financial constraints allowing both talented but poor entrepreneurs to create firms (extensive margin) and productive but financially constrained firms to expand (intensive margin), leading to higher demand for labor that pushes wages higher.

While the average effects are consistent with most macro-development models, the richness of our data allows us to dig deeper into the mechanisms that link financial development and economic development. Financial expansion could foster growth by increasing aggregate demand since even loans targeting business development are often used as consumption loans in developing countries.[3] We rule out this local demand channel as the main driver of our results by showing that the economic expansion is not driven by non-tradable sectors.

We then turn to the reason why financial development would stimulate business investment and labor demand. We contrast the two main classes of models that provide microfoundations for why financial frictions impact business development: models in which the development of the financial sector 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 affect who can create a firm and which firms can expand (e.g., Buera, Kaboski, and Shin, 2011; Midrigan and Xu, 2014). Our results provide clear support for the importance of monitoring costs and reject an explanation based on non-convex costs. In particular, we find that the effect of the policy is concentrated in cities that are in banking deserts, while cities that are closer to other cities with bank presence gain less. 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 effect 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 really reflects an increase in wages holding fixed individuals' sex, age, occupation, and sectoral specialization.

We then explore two explanations that can account for the rise in inequality. First, financial development could increase the relative demand for skilled labor, either because of a large fixed component to the cost of skilled labor (e.g., Benmelech, Bergman, and Seru, 2021) or because the 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 predict that the equilibrium skill mix changes, with firms increasing

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

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.

We find support for another explanation: economic development increases labor demand across the skill spectrum but the supply of workers is heterogeneous across skills, especially in developing countries. While the supply of low skilled workers in the formal labor market is high, the supply of high skilled workers is more inelastic 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 coming from out of town, and this increase is concentrated in a subset of treated cities with lower 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.

Literature Our paper contributes to three strands of literature. First, we contribute to the empirical literature studying how financial frictions affect economic development using natural experiments to obtain within-country variation in financial depth or financial outreach.[4] 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) and finds small, short lived positive effects, or even negative effects (Kochar, 2011).[5]

Other papers have looked at changes in financial depth by studying a targeted lending program in India (Banerjee and Duflo, 2014), 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."[6] The literature look-

4. 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.

5. 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.

6. 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

ing at the introduction of saving products surveyed in Karlan, Ratan, and Zinman (2014) also concludes that microcredit has modest effects on savings and in developmental outcomes such as consumption, schooling, and health (Dupas, Karlan, Robinson, and Ubfal, 2018).[7]

More recent works, however, suggest that the initially modest impacts of microfinance persist and grow over time, especially for incumbent businesses (Banerjee, Breza, Duflo, and Kinnan, 2019; Beaman, Karlan, Thuysbaert, and Udry, 2020). Structural works that incorporate the general equilibrium effects of microcredit generate more ambiguous results. While Breza and Kinnan (2021) finds large effects of microcredit based on a natural experiment in India, Buera, Kaboski, and Shin (2021) uses existing estimates and concludes that general-equilibrium effects substantially dampen the short-term partial equilibrium effect of microfinance on income and productivity by creating upward pressure on wages and interest rates.

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 benefit from the bank expansion. This is in stark contrast with most of the literature using randomized control trials or natural experiments to study the transmission of banks shocks, which, by construction, cannot study the effects on non-borrowers—potentially a key driver of multiplier effects. This has the potential to explain why we find large positive effects on economic development while most papers find limited positive effects. Third, because we have administrative data that allow us to observe the universe of formal employment, we can study in detail the reallocation effect of the reform, instead of only focusing on individual firms or households. This also allows us to measure the effect of financial development on the right tail of the firm size distribution, which has disproportionate importance on aggregate outcomes and misallocation.

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.[8]

Second, we contribute to the literature studying the effect of financial development

is Karlan and Zinman (2010), who finds large positive effect in the context of consumer credit in South Africa.

7. For specific outcomes, see Dupas and Robinson (2013) on health, Dupas and Robinson (2013) on microenterprise development, Prina (2015) on education.

8. See also Célerier and Matray (2019) for an application in US context.

on inequality. Theoretical works in this literature focus mostly on wealth inequality and find 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 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). Models that combine both functions of financial sectors—offering credit, which boosts business development and increases labor demand, and mobilizing deposits, which yields higher returns—generally conclude that capital income pushes inequality upward, as it benefit mostly 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. Therefore, higher labor demand in more-productive sectors will benefit lower-paid workers who reallocate away from less-productive sectors. We contribute to this field by showing that financial development can increase wage inequality in the presence of worker heterogeneity such as skill differentials.

On the empirical side, the literature surveyed in Demirguc-Kunt, Klapper, and Singer (2017) mostly concludes that improvements in financial markets tighten the distribution of income. This literature however, mostly relies on cross country analyses or focuses on the U.S. context (e.g. Black and Strahan, 2001; Beck, Levine, and Levkov, 2010) with few exceptions looking at the skill premium (e.g., Fonseca and Doornik (2021) in Brazil).[9]

We contribute to this literature in two ways. First, we analyse a policy that created variation within a country, allowing us to hold fix country-level (and in some cases state-level) institutions. Second, we show that financial development benefits low income households (consistent with the literature on finance and outcomes of low-income households), but at the same time increases earnings inequality.

Third, we contribute to the broad literature that studies how financial frictions affect economic development via its effect on capital and talent misallocation.[10] More specifically, we relate to the macro-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), mod-

9. Somewhat related is the recent literature studying the distributional effects of monetary policy or financial shocks on wages and employment in developed countries such as Caggese, Cuñat, and Metzger (2019); Moser, Saidi, Wirth, and Wolter (2020); Bergman, Matsa, and Weber (2021); Broer, Kramer, and Mitman (2021)

10. See, among many others: Giné and Townsend (2004); Townsend and Ueda (2006); Banerjee and Moll (2010); Buera, Kaboski, and Shin (2011); Kaboski and Townsend (2011); Buera and Shin (2013); Midrigan and Xu (2014); Cheremukhin, Golosov, Guriev, and Tsyvinski (2017); Moll, Townsend, and Zhorin (2017); Bau and Matray (2020).

els individuals with heterogeneous intrinsic productivity deciding between working as an employee or becoming an entrepreneur, often assuming that sectors in the economy also differ in productivity. Financial frictions can affect both the size of the productive sector, the talent pool of of entrepreneurs in this sector, and the ability of existing firms to grow. One way financial constraints can matter in this context is if any investment, including the creation of a firm, requires paying an upfront fixed cost.[11] More broadly, we relate to the literature studying how financial frictions affect firm labor demand and employment outcomes.[12]

Finally, because the reform we explore relies on the expansion of government-owned banks, we relate to the broad literature studying the economic effects of government ownership of banks (e.g., Sapienza, 2004; Khwaja and Mian, 2005; Dinç, 2005; Cole, 2009; Carvalho, 2014; Delatte, Matray, and Pinardon Touati, 2020). Most of this literature emphasizes the risk of political capture and the creation of politically motivated credit cycles. We show that such form 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

11. In dynamic settings in which productive but poor individuals can accumulate savings over time to save their way out of financial constraints, fixed costs may need to be even higher for financial frictions to matter (e.g., Buera, Kaboski, and Shin, 2011), particularly if productivity shocks are not transitory (Moll, 2014). Intuitively, forward looking individuals can anticipate the return to entrepreneurship and will save to self-finance their investment. One force limiting the need for high non-convex costs is the need for households to maintain "buffer stocks" in the absence of well functioning insurance markets, which would prevent them from making illiquid physical investments (see Townsend (1994) or Kaboski and Townsend (2011) for such models, and Cole et al. (2013) for evidence on limited insurance in developing countries).

12. 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; 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.

Brazilians were unbanked at the time.[13]

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 solid 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, 44% of municipalities have no 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.

Evidence suggests the program succeeded in reaching unbanked 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). 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.[14] The number of correspondents went from fewer than 50,000 in 2003 to over 150,000 in 2010 (Loureiro, Abreu Madeira, and Bader, 2016) and, taking into account partnerships with correspondents, government-owned banks were present in 100% of municipalities by 2007 (Ministério da Fazenda, 2007).

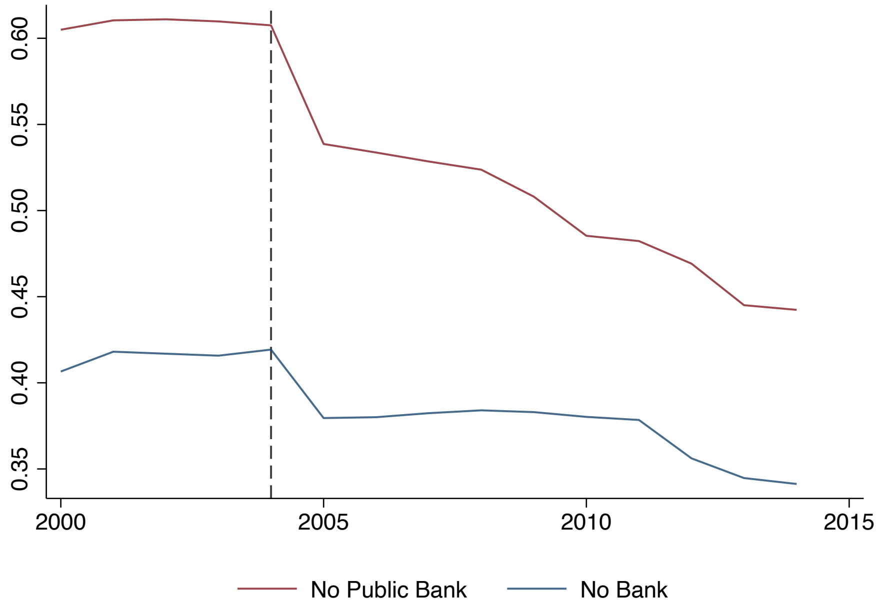
2.2 Data

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

13. 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.

14. 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).

Figure 1: Share of Municipalities without Bank Branches



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

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 observe data not only on average gross monthly earnings and the average number of hours worked, but also on worker characteristics such as education, occupation, 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 tof 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 deposit 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. These include local GDP per capita, population size, the share of manufacturing and of services in local value added, local tax revenues, and local government expenditures from the Brazilian Institute of Geography and Statistics (*Instituto Brasileiro de Geografia e Estatística*, or IBGE). We also use data on city-level population by years of schooling from the 2000 Census, which we use to construct the share of skilled workers in the population using years of schooling as a proxy for skill.

3 Empirical strategy

Selecting matched city groups. We define cities as being treated if they did not have a public bank prior to 2004, which represents 43% of cities in the initial sample. Identifying the relevant control group is challenging for several reasons. First, the reform targeted poor, underdeveloped, and relatively small cities that are not representative of the average Brazilian city during this period. Second, Brazil was about to enter a period of sustained growth, partially fulled by a commodity boom. This general context implies that many cities experienced extreme fluctuations in employment and aggregate output and make the use of a matching estimator a necessity. This can be seen clearly in Figure 2, which plots a "balance covariate test." The green dots show the difference between treated and control cities when we use the universe of cities.

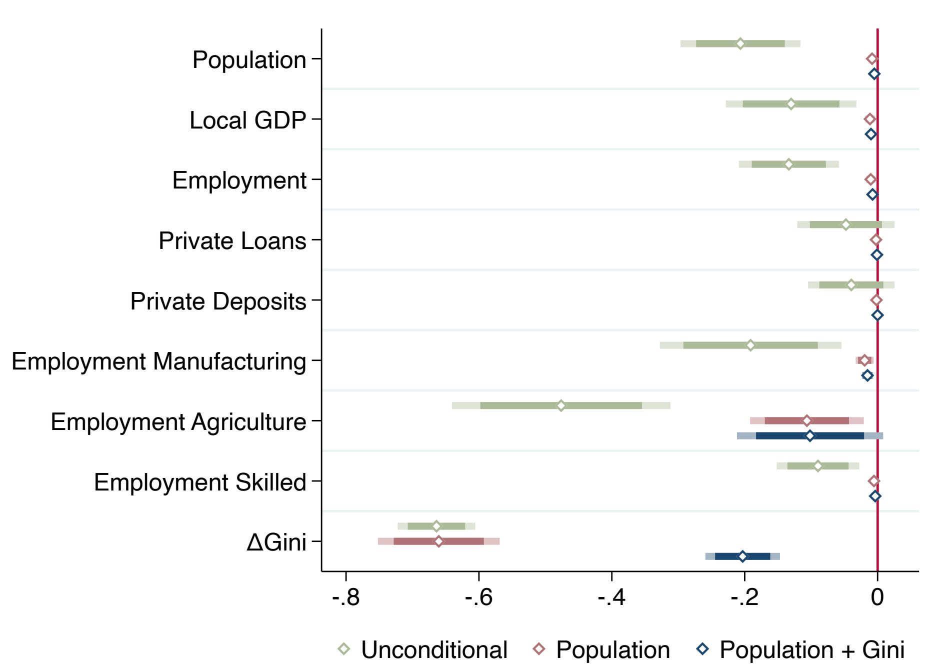
To identify the control group, we start with all 4,260 cities and compute quintiles of population. We then match with replacement each treated city with all control cities in the same population quintile. This parsimonious approach addresses a large part of the heterogeneity, as showed by the red dots in Figure 2. The figure shows that the treatment and control groups are similar over a rich array of city characteristics that were not included in the matching process, such as private deposits and loans, employment in manufacturing and agriculture, and skilled employment. In addition, while some of the point estimates are not exactly zero, the standardized difference between both groups remains below the threshold of 0.20 suggested by Imbens and Rubin (2015) for almost all variables. Although the validity of our difference-in-differences design does not require that treatment and control units are similar in levels, such similarity increases the likelihood that they follow similar trends before the treatment, making the common-trend assumption more plausible.

After exact matching on population quintile, a remaining large and statistically significant difference between treatment and control units 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 (Lopez and Perry, 2008), we also match with replacement on Gini growth in the pre-period. We use a nearest neighbor algorithm and select the three closest control cities. The blue dots show differences between treated and control when we add this matching criterion. While there some remaining differences, we show in Figure 6 that the

Gini index of treatment and control units evolved in close parallel prior to the reform, and there is no evidence of pre-trends.

Results are robust to using different numbers of control cities (Appendix Table A1) and to different matching procedures (Appendix Table A2). In particular, Table A2 in the Appendix shows that results are robust to matching on the share of manufacturing and the share of skilled workers. 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 clear geographical patterns.

Figure 2: Covariate Balance

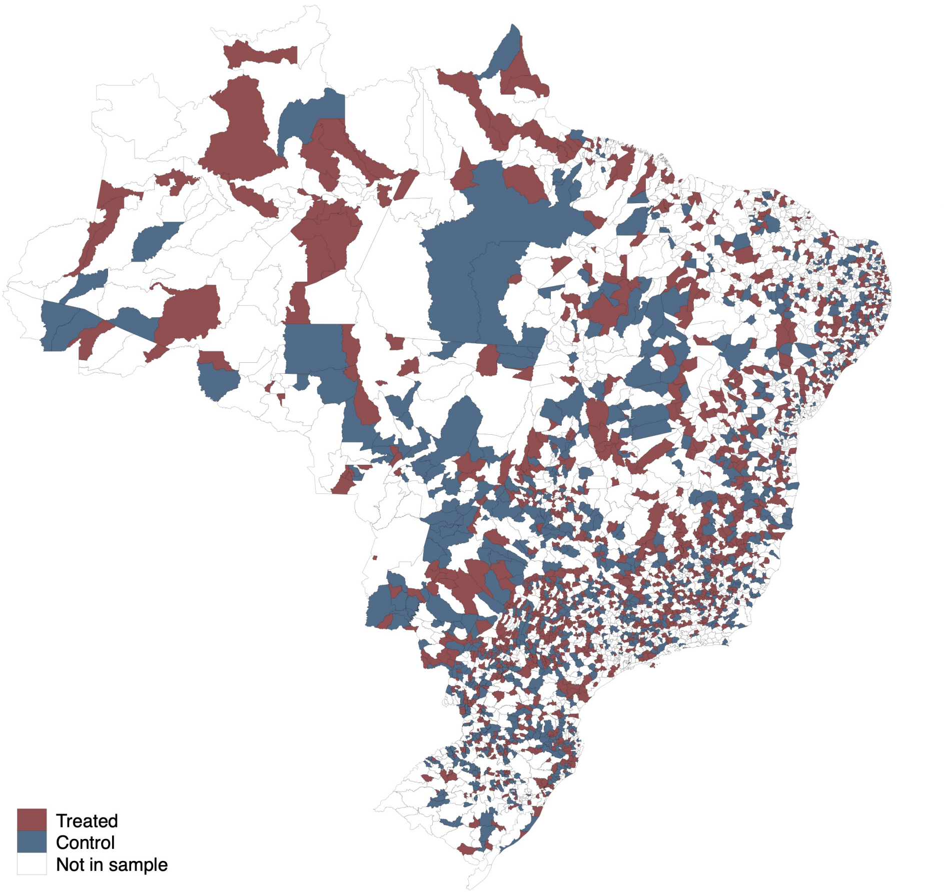


This figure shows coefficient estimates and 95% error bands of the difference between treated and control cities along different variables. All variables are normalized to have a mean of zero and a standard deviation of one in the full sample.

The use of a difference-in-differences matching estimator allows us to include group-by-year fixed effects, which ensures that the effect of the reform is identified by comparing cities that are exposed to similar size and Gini-growth specific time-varying shocks, even if they differ in the level of some outcomes. Similar *trends* before the reform between the two types of firms is the key identification assumption, which we demonstrate graphically in Sections 4, 5, and 6 by plotting event studies. Specifically, identification does not require the reform to be random, nor for treated and control groups to share similar levels of covariates.

Econometric specification. In order to analyze the effect of an increase in bank coverage on economic development and inequality, we estimate a series of matched difference-

Figure 3: Geographical Distribution



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.

in-differences specifications of the form:

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

where $Y_{c,g,t}$ are various city outcomes for city c at year t that belongs to a group of treated-controls g. θ_c are city fixed effects that remove time-invariant heterogeneity across cities, and $\delta_{g,t}$ are group-by-year fixed effects that controls for time-varying unobserved heterogeneity across groups. Because we select our groups using pre-reform population size, skill composition, formal employment, average wage and inequality, the inclusion of group-by-year fixed effect implies that we are filtering out unobserved correlated shocks that might exist between all these characteristics and the reform. The use of group-by-year fixed effects forces the parameter of interest β to be identified solely by comparing cities within the *same* group. 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.

In robustness tests, we include a collection of additional city-level controls denoted

Table 1: Summary Statistics

	Mean	Med.	St. Dev.	N
Loans per Capita	2,178.97	1,318.48	2,471.04	79,99
Public Loans per Capita	1,947.36	1,110.20	2,379.00	79,99
Private Loans per Capita	231.61	53.34	445.12	79,99
Total Branches	1.72	1.00	1.25	79,99
Public Branches	0.92	1.00	0.63	79,99
Private Branches	0.80	1.00	0.91	79,99
Deposits per Capita	1,446.11	1,041.17	1,469.56	79,99
Public Deposits per Capita	1,055.89	733.40	1,232.78	79,99
Private Deposits per Capita	390.21	102.22	629.18	79,99
Wage	913.01	881.31	268.35	79,99
Total Employment	1,023.15	648.00	1,446.76	79,99
Share Skilled	0.09	0.08	0.05	79,99
Skill Premium	2.28	2.14	0.69	79,99
Gini Index	0.31	0.31	0.05	79,99
Population	12,156.20	9,031.00	12,474.92	79,99
GDP per Capita	13,581.36	9,500.44	23,343.73	79,99
Share Manufacturing	0.21	0.14	0.20	79,99
Share Agriculture	0.14	0.09	0.14	79,99

This table reports summary statistics of 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.

by $X_{i,t}$: share of agriculture and share of manufacturing in local value added, GDP per capita, and population. Given that the reform may have a direct impact on many city characteristics, using time-varying controls would potentially bias our coefficients of interest.[15] We address this problem by using the pre-reform value of these controls interacted with year fixed effects. In Appendix Table A3, we also show that results are robust to adding state-by-year fixed effects to control for time-varying unobserved variation across regions of Brazil.

4 The Banks for All program and financial inclusion

We start by testing whether the reform had an effect on financial development or whether the expansion of government-owned banks led to a pure substitution between government-

15. This is commonly referred to as the problem of "bad controls" (e.g., Angrist and Pischke, 2008).

owned banks and private banks. We estimate Equation 1 with a dummy 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, respectively, as loans and deposits from branches that were opened after the reform.[16]

In Figure 4, we report the event study coefficients of our difference-in-differences estimation for the dummy for having a bank branches. Panel (a) shows results for any bank branch, while panel (b) decomposes the total change (the grey rounds) 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 is particularly reassuring given the large credit boom that Brazil experienced during this period and validates our design, as both treated and control cities are on the same trend prior to the reform. Second, the expansion of public banks only modestly crowds out private banks, resulting in a large increase in *overall* financial development for treated cities. The probability of having a public bank branch or any bank branch increase sharply after 2004, in line with the aggregate pattern reported in Figure 1, and continue 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 in Figure A1 in the Appendix.[17] 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, consistent with positive spillovers from public banking to private banking.

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 capital increases by BRL 155.16 (column 4) and new deposits per capita increases by BRL 142.33 (column 7). 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 this respect, the policy

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

17. Note that, by definition, these variables exactly 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.

can be interpreted as a change in the steady state of local financial development, rather than a one time infusion of capital.[18]

Since all other outcome variables are defined in logs, we report estimates that can be interpreted as percentage changes as a robustness check. 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 (e.g., Burbidge, Magee, and Robb, 1988; MacKinnon and Lonnie, 1990), defined as: $log[X + (X^2 + 1)^{1/2}]$. Except for very small values of X, the inverse sine is approximately equal to $log(2X)$ or $log(2) + log(X)$, and so it can be interpreted in exactly the same way as a standard logarithmic dependent variable. But unlike a log variable, the inverse hyperbolic sine is defined at zero and is less sensitive to jumps around zero than the more widely used $log(X + 1)$ transformation.

We report event studies for total credit and total deposits using the inverse hyperbolic sine transformation in Figure A2 and pooled estimates in Table A4 in the Appendix. As before, the event studies show that credit and deposits for treated and control units evolve in close parallel prior to the reform. These results also confirm that the expansion in credit and deposits is entirely driven by public banks and that there is minimal crowding out of private banks.

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

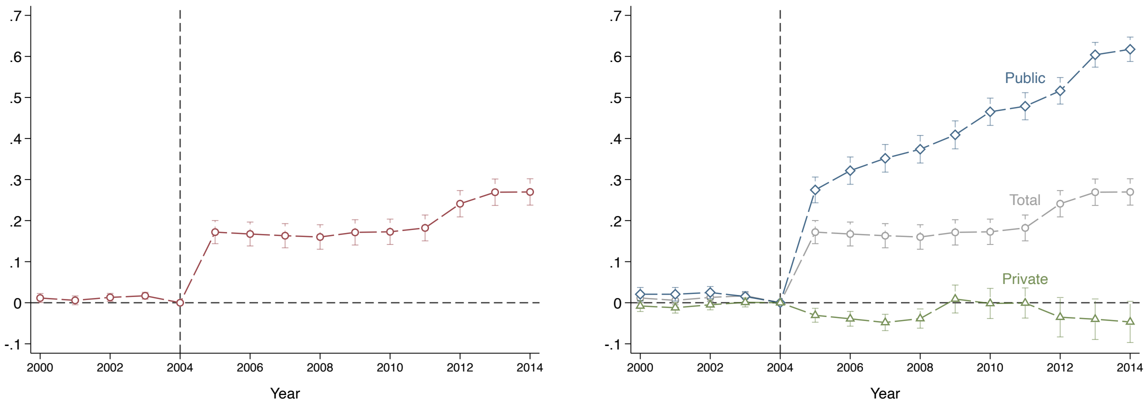
Table 2: Effect of the Program on Bank Branches, Credit, and Deposits

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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

18. 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).

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 their 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 banks, respectively.

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-development emphasize that financial frictions hamper economic development because talented 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 in 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 higher wages, which increase 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 by counting the number of distinct 2-digit industries (column 5) and obtain similar results, albeit stronger, when we use instead 3 or 4 digit industries. The concentration of economic activity is measured using the HHI of employment across 2-digit industries (column 6). Again, using HHI across 3 or 4 digit only increases the magnitude of the estimates.

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.

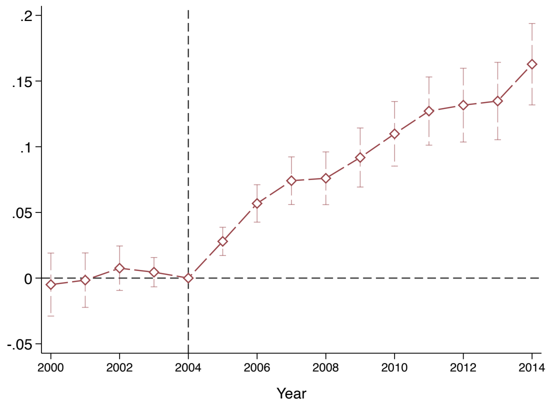
We reproduce this analysis in graphical form by estimating the event study version of equation 1 in Figure 5. In all cases, we find that treated cities display no pre-trend relative to control cities. We also find that each outcome increases progressively over time after the reform and stabilizes at 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 works on economic development have emphasized the role of the manufacturing sector as a key vector of productivity gains and a potential source of 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.

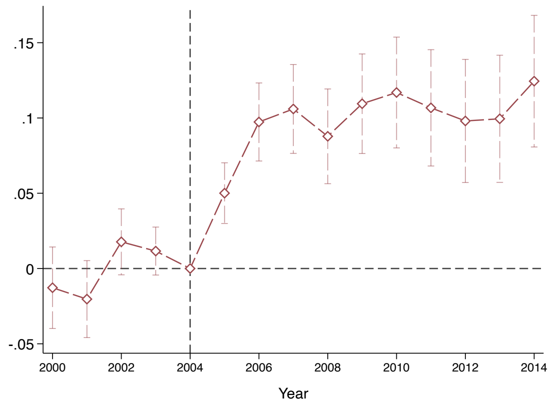
Table A5 in the Appendix reports the results. Overall, we find limited evidence that the industrial composition changed. In particular, we find no change in manufacturing or agriculture, and a statistically significant albeit small increase in some services like retail and construction.

Robustness: government programs. One potential concern is that government and social welfare programs are often disbursed through government banks. That means that it is possible that our results are driven by increased access to government programs,

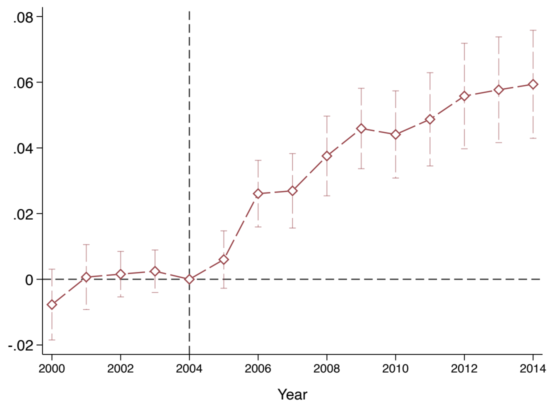
Figure 5: Effect of the Program on Employment, Wage and Firms



(a) Firms



(b) Employment



(c) Mean wage

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 log of total number of firms, total employment, and average wage in panel (a), (b) and (c) respectively.

rather than by financial inclusion. Of special concern is the far reaching cash transfer program "Bolsa Família," which was introduced one year before our reform, in 2003.

To determine if our results are driven by government programs, we exploit the fact some of the largest government programs, like Bolsa Família, are disbursed by the same government bank: Caixa. We estimate how our results vary depending on whether or not cities in the control group have a branch from Caixa prior to the reform. Intuitively, if results are driven by access to government programs, treated cities should experience stronger effects if compared to control cities that did not have a branch of the bank responsible for disbursing these programs. We report results of this exercise in Table A6 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.

5.2 Mechanisms

There are two main channels through which financial development promotes economic growth in this setting. First, bank expansion can foster *aggregate demand* by relaxing individuals' borrowing constraints and reducing their need for precautionary savings. This in turn would boost their demand, which should primarily affect non-tradable industries in treated cities. Second, bank expansion can foster *supply* by reducing investment frictions, thereby boosting investment of existing firms and facilitating the entry of new firms, which will affect firms both in tradable and non-tradable industries.

To distinguish between these different hypotheses, we move to the city-2-digit-industry level.[19] We create a balanced panel in which we assume that each industry we observe during our sample period in a given city is present during the whole period and fill observations where no firm exists 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" ($t \leq 2004$) and the average "post" ($t > 2004$). We then compute the mid-point growth rate for all our different outcomes.[20]

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

$$\Delta Y_{c,j,t} = \beta_1 \; Treated_c \times Post_t + \delta_{j,t} \times + \varepsilon_{c,j,t} \tag{2}$$

Since $\Delta Y_{p,c,j,t}$ is already the change between the pre and post period, we do not need to include city×industry fixed effects as they are already differentiated 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. To be precise, for each industry j in city c, we define the mid-point growth rate for a variable X as $g^X_{j,c} = (X_t - X_{t-1})/[(X_{j,c,t} + X_{j,c,t-1}) \times 0.5]$ and compute the weight as $g^X_{j,c}/(\sum_{j \in c} g^X_{j,c})$.[21]

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

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

20. The mid-point growth rate, also known as the arc percentage change is computed as $g_t = (X_t - X_{t-1})/[(X_t + X_{t-1}) \times 0.5]$.

21. 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.

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 the same industry within a given group of treated-control cities. These additional fixed effects ensure that our baseline effects are not driven by industry shocks that might correlate with the reform and the sectoral composition of treated cities.

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

Dependent Variable	Firms			Employment			Wage		
	(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" ($t \leq 2004$) 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 $g^X_{j,c}/(\sum_{j\in c} g^X_{j,c}) \times pop_{2000}$. Standard errors are clustered at the city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

5.2.1 Consumption channel

To test if most of the effect is coming from a bank expansion-induced increase in demand, we split the sample between tradable and non-tradable goods. 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 simply compare firms in manufacturing relative to firms in other sectors. 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 results of this exercise 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.

5.2.2 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 de-

Table 5: Employment in Tradables and Non-Tradables

Dependent variable	Employment					
Tradable Definition	Manufacturing		1^{st} tercile HHI		1^{st} quartile HHI	
Tradable	Yes	No	Yes	No	Yes	No
	(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.

velopment relaxes financial frictions either because it reduces monitoring costs for banks (e.g., Greenwood, Sanchez, and Wang, 2010), or because productive industries such as manufacturing are characterized by large fixed costs of investment (see 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 . We create different proxies and interact all explanatory variables and fixed effects with these dummies:[22]

$$\begin{aligned} \Delta Y_{p,c,j,t} = {} & \beta_1 \; Treated_c \times Post_t \times \mathbb{1}Proxy \\ & + \beta_2 \; Treated_c \times Post_t + \delta_{p,j,t} \times \mathbb{1}Proxy + \; \varepsilon_{p,c,j,t} \end{aligned} \tag{3}$$

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

22. Note that interaction $\delta_{p,j,t} \times \mathbb{1}Proxy$ is essential to properly estimate the marginal effect of the proxy.

build on Greenwood, Sanchez, and Wang (2010) and Buera, Kaboski, and Shin (2011), respectively. We proxy for the importance of monitoring costs by computing the distance between each city and the nearest city with a bank (public or private) and create a dummy variable $\textit{High Distance}_c$ that takes the value one if the distance to the nearest city with a bank is above the sample median. We sort cities into high and low distance using pre-reform (2000–2004) data and hold this classification fixed. 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).

We follow Buera, Kaboski, and Shin (2011) and proxy for the importance of non-convex investment costs using the average establishment size in the industry, and create the dummy $\textit{High Fixed Costs}_j$ 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 predicts that the effect of the reform should be stronger in industries in which establishments are larger on average. Interestingly, 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 are farther away from the nearest bank (column 1) and that this expansion is partially driven by the fact that existing firms become bigger (column 2).[23] 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).

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

23. 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.

portance of having panel data instead of 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.

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).[24] The increase in the number of firms is driven by an even larger increase in firm entry (+26.1%, column 4), that compensates and potentially causes an increase in firm exit (+8%, column 5).

Note that 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 as an outcome, as well as the average wage per worker in each bin of the city-level wage

24. The effect for low fixed costs is directly reported in the table with the coefficients of the variables *Treated*$\times$*Post*.

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

Dependent Variable	Employment	Establishment size	# Firms	Entry	Exit
	(1)	(2)	(3)	(4)	(5)
		Panel A: Distance to nearest bank			
Treated×Post	0.060***	0.017	0.069***	0.170***	0.101***
	(0.016)	(0.019)	(0.015)	(0.027)	(0.020)
Treated×Post×High Distance$_c$	0.116***	0.018	0.152***	0.083**	-0.068***
	(0.027)	(0.031)	(0.023)	(0.038)	(0.026)
		Panel B: Fixed costs in investment			
Treated×Post	0.243***	0.058***	0.206***	0.274***	0.068***
	(0.019)	(0.015)	(0.016)	(0.026)	(0.019)
Treated×Post×High Fixed Costs$_j$	-0.153***	-0.039	-0.195***	-0.181***	0.014
	(0.020)	(0.024)	(0.016)	(0.025)	(0.022)
Match×Industry×Year FE	✓	✓	✓	✓	✓
Match×Industry×Year× Proxy FE	✓	✓	✓	✓	✓
Observations	154,090	113,112	153,215	153,215	153,215

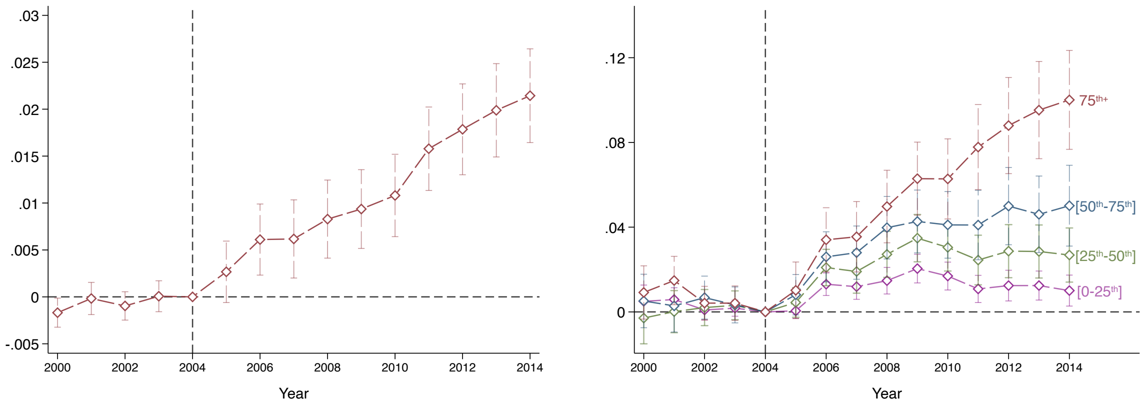
This table shows the effect of the expansion of public banks on the growth of employment and firm growth at the city-by-(2 digit) industry level. Data are collapsed as an average "pre" ($t \leq 2004$) and the average "post" ($t > 2004$) periods, and each dependent variables are the midpoint growth rate $g^X_{j,c} = [(X_{j,c,t} + X_{j,c,t-1}) \times 0.5]$. Each cell is weighted by $g^X_{j,c}/(\sum_{j \in c} g^X_{j,c}) \times pop_{2000}$. 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 mean 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.

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.

Table 7 reports estimates of equation 1. All the results are significant at the 1%

Figure 6: Effect of the Program on Wage Inequality



(a) Gini index

(b) Wage distribution

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. In panel B, the wage distribution is computed every year at the city level.

level. The point estimates tend to underestimate the effect of the reform on inequality 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), an increase five time larger.

Robustness: sample composition. So far, the results on changes in inequality might be partially driven by a change in the sample composition in treated cities. We investigate this possibility in Table 8, in which we measure inequality using the city-level variance of log wage. 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.[25] 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.[26] Across

25. 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.

26. Results are similar when we impose for 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

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 column 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.

all the different level of controls, we find an overall stable effect of the reform, with higher financial development leading to more inequality.

Robustness: informality. Note that columns 5 and 6 of Table 8 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 share of employment in the informal sector from the 2000 Census. We split the sample into quartiles of the size of the informal sector and show in Table A7 in the Appendix that results are robust to controlling for informality-quartile×match×year fixed effects. This once again confirms that our results are not driven by the informal sector.

Other robustness. 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. In panel C, we exact match on quintiles of the share of manufacturing and, in panel D, we exact match on quintiles of the level of inequality. In all cases, the point estimates of all the outcomes are quantitatively very similar.

6.2 Mechanisms

Three channels can account for the increase in inequality following a reduction in financial frictions. First, financial development might lead to better employer-employee matching.

affects inequality.

Table 8: 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.

This could happen either because looser financial constraints on individuals allow them to search longer and find a better 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 (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.

Second, 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 works that give a structural interpretation to Abowd, Kramarz, and Margolis (1999) methodology. This method consists of studying sorting, i.e. whether productive workers are matched with productive firms, by analyzing the correlation between firm and worker fixed effects from wage regressions, which are usually found to be close to zero or negative.[27]

Eeckhout and Kircher (2011) and Lopes de Melo (2018) give a structural interpretation to the firm fixed effects in Abowd, Kramarz, and Margolis (1999) regressions and show that the relationship between them and the true firm type is theoretically ambiguous, which means that the correlation between firm and worker fixed effects will not generally measure the strength of sorting. Accordingly, we test whether matching improves in our setting by relying on another prediction that arises from these frameworks: that better matching reduces the dispersion of worker ability within the firm (Eeckhout and Kircher, 2011; Bombardini, Orefice, and Tito, 2019).

We proxy for worker type with the average log wage over all job spells, which can be shown to be monotonically increasing in worker type in the framework of Eeckhout and Kircher (2011) (Bombardini, Orefice, and Tito, 2019). We then compute the standard deviation of worker types at the firm-year level and residualize this variable from firm fixed effects in order to control for cross-sectional differences in the dispersion in worker types. Finally, we collapse the residualized dispersion in worker types to the 2-digit-industry-by-city level for each year and test whether the average dispersion declines as a consequence of the reform.

Table 9 shows results of this exercise. Across all specifications, we find that, if anything, the within-firm dispersion in worker type increases (by a small amount relative to the pre-reform average of 0.34). This is the opposite of what we would expect from an improvement in employer-employee matching, which should lead to lower within-firm dispersion in worker types.

Increase in demand for skilled workers. To test whether a change in the relative demand for skilled workers can explain the rise in wage inequality, we need an ex-ante, time-invariant definition of skill. We leverage the fact that the Brazilian matched employer-employee data allow us to observe education and classify workers as skilled if they have at least some college education and unskilled otherwise.[28]

In Table 10, we start by showing that this measure tracks the evolution of inequality

27. For applications of the Abowd, Kramarz, and Margolis (1999) methodology, see, for instance, Abowd, Creecy, Kramarz, and Census (2002), Abowd, Kramarz, and Woodcock (2008), and Card, Heining, and Kline (2013). See Lopes de Melo (2018) for a comparison of the correlation between worker and firm fixed effects across France, Germany, Italy, Denmark, and Brazil.

28. 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. But using at least some college education as a measure classifies only 7% of workers in a city as skilled, on average.

Table 9: Dispersion in Worker Type

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

pretty 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), 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 our results. In Appendix Table A8, we show that we find similar results at the industry-by-city level controlling for time-varying industry shocks.

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

Table 11 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

Table 10: Demand for Skilled Workers

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

This table shows the effect of the reform on the skill premium (column 1), the average wage of skilled and unskilled workers (columns 2 and 3), and the share of workers that are skilled (column 4) at the city level. Skilled workers are defined as workers with at least a high school degree. 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.

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%.

Table 11: 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***	0.000	0.021***	-0.021***	0.007***	0.020***
	(0.004)	(0.001)	(0.004)	(0.006)	(0.002)	(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 with at least a high school degree. "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 results might seem surprising given the large increase in skill premium following the reform, they 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 large costs to 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, according to data from the 2000 Demographic Census.[29] 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. 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.

Table 12: 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 12 reports the results. The increase in Gini

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

(column 1) is entirely explained by the increase in cities where the fraction of population which is skilled is low (column 2). Since we use an interaction term, the coefficient on the variable *Treated*$\times$*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*$\times$*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 essentially zero. In Appendix Table A9, 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 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 by an important process of "creative destruction," whereby the entry of new entrepreneurs led to the exit of older and less productive entrepreneurs.

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 implement simultaneous 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.

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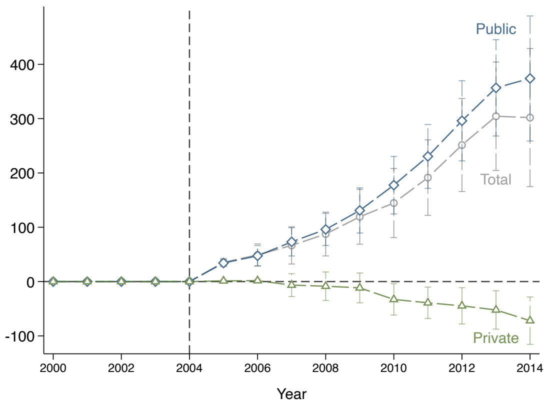
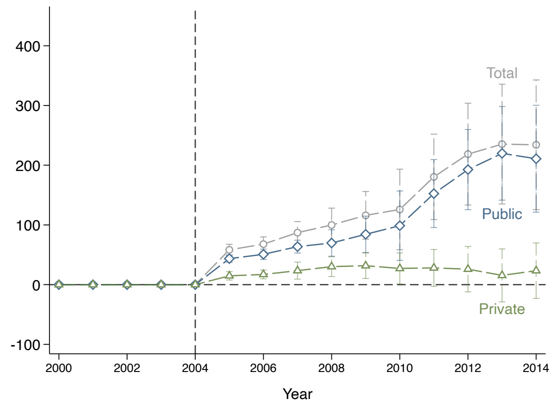
A.1 Appendix Tables and Figures

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



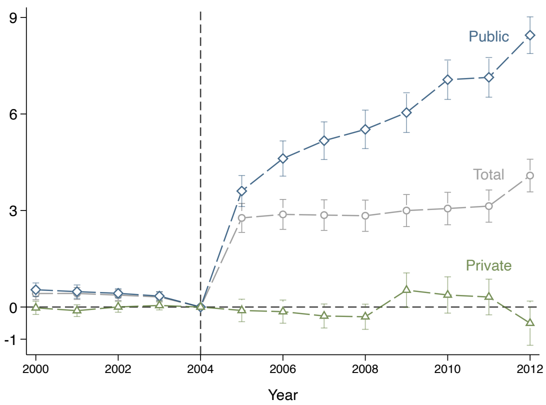
(a) New Loans per Capita



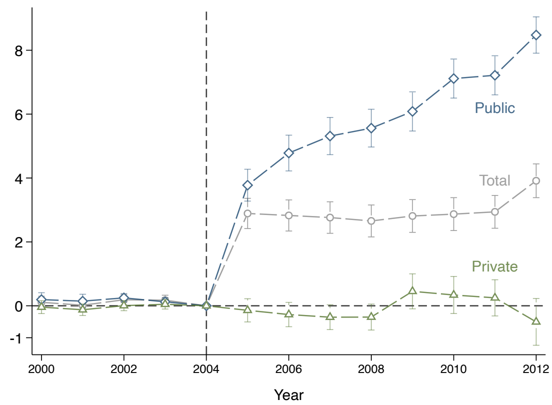
(b) New Deposits per Capita

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



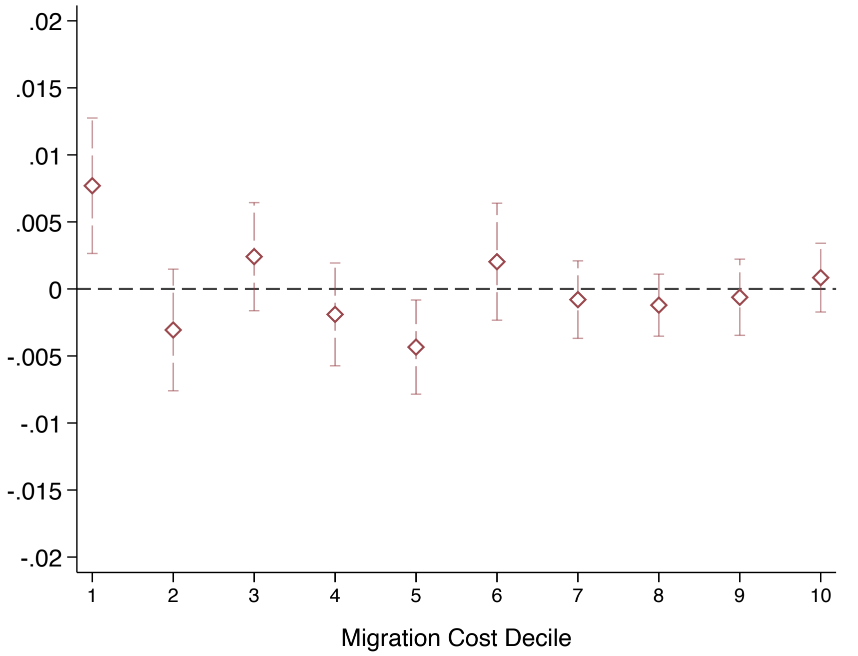
(a) Credit



(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	81,390	81,390	81,390	81,390	81,390

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

Table A3: 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 A4: 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 A5: 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 A6: 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 A7: Robustness to Controlling for Informality

Dependent variable	Has Public Branch	# Firms	Employment	Wage	Gini
	(1)	(2)	(3)	(4)	(5)
Treated×Post	0.425***	0.098***	0.100***	0.041***	0.012***
	(0.016)	(0.013)	(0.016)	(0.006)	(0.002)
City FE	✓	✓	✓	✓	✓
Informality-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 with the inclusion of informality-quartile-year fixed effects. We measure informality as the city-level share of employment in the informal sector from the 2000 Census, and split this variable into quartiles. 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: 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 A9: 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.