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

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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, indicating 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 aggregate effect on economic development.[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 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. 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 that caused 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 exploit the introduction of the Banks for All program (*"Banco para Todos"*) by the Brazilian federal government in 2004, which explicitly targeted underbanked cities that were not served by government-owned banks. We trace how this policy 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 to track heterogeneous individuals over time and better understand how and why earnings

1. See 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.

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 income 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 margin. The number of bank branches and the overall amount of credit increases substantially after 2004 and did 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 amount of private banks stay 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 11% 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 9%, which pushes the average wage per worker to rise by 3.5%. 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, education, age, occupation, and sectoral specialization. When decomposing the change in inequality into a between- and a within-firm component, we find that all of the increase happens within firms, while the contribution of the between firm component is null. This finding is different than what has been documented in more developed economies, where most of the increase in wage inequality is driven by the emergence of superstar firms (e.g, Autor et al., 2020) and better assortative matching (e.g., Card, Heining, and Kline, 2013).

We then explore three possible explanations to account for the rise in inequality. First, looser financial constraints for both firms and workers could improve matching in

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

an environment with search frictions. Our results showing that all of the increase in inequality happens within and not between firms reject this possibility. Second, 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 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 are left with a final explanation: economic development increases labor demand across the skill spectrum but the supply of workers is heterogeneous across skills, especially in developing countries. 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 implica-

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.

tions 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 looking 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

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 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.

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 maps these costs onto the market's distance from bank branches.[8]

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

Third, we contribute to the broad literature that studies how financial frictions affect

8. See also Nguyen, 2019 and Celerier Matray (2019) for application in US context.

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)

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), models individuals with heterogeneous intrinsic productivity deciding between staying in the traditional sector, working in the productive sector as an employee, or becoming an entrepreneur. Financial frictions 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. Financial constraints matter in this context because any investment, including the creation of a firm, requires paying an upfront fixed cost. More broadly, we relate to the literature studying how financial frictions affect firm labor demand and employment outcomes.[11]

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 underpriviledged areas. We also more broadly study how public institutions (in our case banks) shape labor markets in Brazil (e.g., **Colonnelli2020;** , **Colonnelli2020;** 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

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).

11. 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.[12]

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 deposit following the introduction of the program.

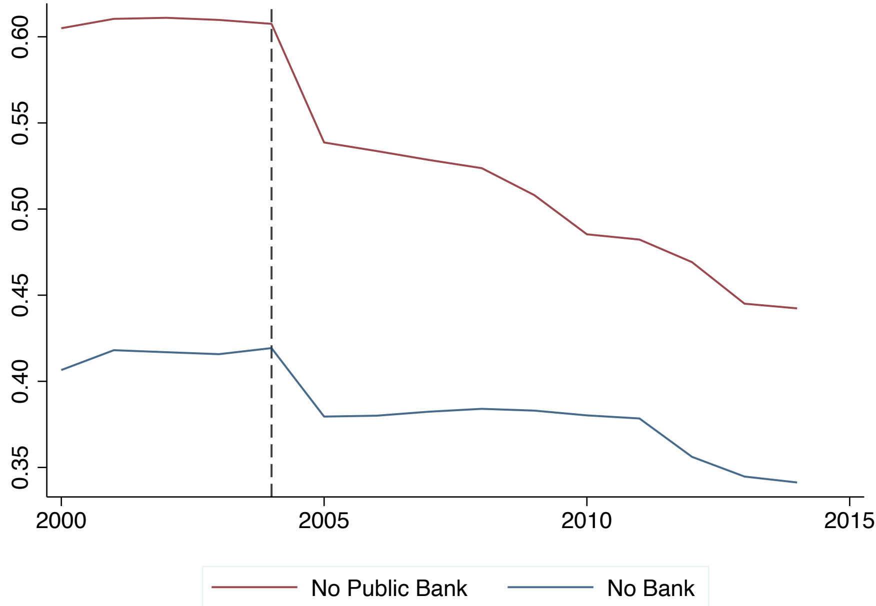
2.2 Data

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

12. 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.

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.

cities from over 5.000 to 4.260. In the rest of the text, we use the term "city" to refer to an AMC.

The number of bank branches, lending activity and deposits come from the ESTBAN database maintained by the Central Bank of Brazil. The data provide bank branch balance sheet information at the city level, which allows us to decompose the number of branches, credit and deposit between public and private banks.

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 so 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 roughly XX% of cities in the initial sample. Identifying the relevant control group is more difficult 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.

To identify the control group, we start with all 4,260 cities and compute quintiles of population and share of skilled workers over the pre-reform period. Throughout the paper, skilled workers are defined as workers with at least some college education. We then match with replacement each treated city with all control cities in the same quintile. We also match treated and control on employment, average wage, and wage inequality measured by the Gini index, and select the three closest control cities.[13] We are left with 1,273 distinct treated cities and a total of 2,092 distinct control cities.

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, employment, wage growth, skill level 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 only condition required for identification, 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.

We report the summary statistics for treated and control cities in our final sample for the pre-reform period in Table 1.

13. Results are robust to using different numbers of controls. Appendix Table A1 shows robustness to using one or two control cities per match.

Table 1: Pre-Reform Characteristics of Treated and Control Cities

	Treated				**Control**			
	Mean	Med.	St. Dev.	N	Mean	Med.	St. Dev.	N
Total Loans	697,920.10	131,712.00	1,853,512.19	6,365	9,504,044.42	6,021,905.00	12,624,795.49	17,825
Public Loans	0.00	0.00	0.00	6,365	7,914,240.55	5,222,742.00	9,679,682.50	17,825
Private Loans	697,920.10	131,712.00	1,853,512.17	6,365	1,589,803.86	389,984.00	5,483,092.64	17,825
Total Branches	0.74	1.00	0.66	6,365	2.01	2.00	1.16	17,825
Public Branches	0.00	0.00	0.00	6,365	1.08	1.00	0.37	17,825
Private Branches	0.74	1.00	0.66	6,365	0.93	1.00	0.99	17,825
Total Deposits	1,687,437.69	574,088.00	3,073,141.47	6,365	7,995,720.10	4,836,353.00	11,394,375.41	17,825
Public Deposits	0.00	0.00	0.00	6,365	5,435,015.95	3,926,170.00	6,585,453.47	17,825
Private Deposits	1,687,437.69	574,088.00	3,073,141.47	6,365	2,560,704.15	491,930.00	6,121,774.24	17,825
Wage	431.49	406.26	144.69	6,365	488.62	467.61	146.58	17,825
Total Employment	645.46	440.00	762.71	6,365	1,176.66	739.00	1,726.87	17,825
Share Skilled	0.07	0.06	0.05	6,365	0.07	0.07	0.04	17,825
Skill Premium	2.36	2.20	0.87	6,318	2.60	2.44	0.78	17,825
Gini Index	0.30	0.30	0.06	6,365	0.33	0.34	0.05	17,825
Population	10,652.31	7,652.00	10,726.66	6,365	11,894.77	9,278.00	11,360.53	17,825
GDP per Capita	5.01	3.73	5.30	6,365	7.98	6.11	7.17	17,825
Share Manufacturing	0.15	0.07	0.18	6,365	0.23	0.17	0.21	17,825
Share Agriculture	0.17	0.13	0.16	6,365	0.15	0.10	0.15	17,825

Notes:

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-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 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.[14] We address this problem by using the pre-reform value of these controls interacted with year fixed effects.

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-owned banks and private banks. We estimate a series of regressions using Eq. 1, with total bank branches and total credit as outcome variables, which we then split between government-owned banks and private banks.

Because treated cities have no government-owned banks by construction, we need to deal with multiple zeros in our dataset. We address this issue by reporting results using the inverse hyperbolic sine transformation of the log function (e.g., Burbidge, Magee, and Robb, 1988; MacKinnon and Lonnie, 1990), defined as: $log[X + (X^2 + 1)^{1/2}]$.[15] 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.[16]

In Figures 2, 3, and 4, we report the event study coefficients of our difference-in-differences estimation for the number of bank branches, credit, and deposits, respectively. Panel (a) shows results for total changes, 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 amount of private credit and private branches of 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 private credit trend prior to the reform.

Second, the expansion of public banks does not crowd out private banks. Instead, it comes in addition to the number of private branches and volume of credit from private banks, resulting in a large increase in *overall* financial development for treated cities. The number of bank branches and the amount of credit increase sharply after 2004, in line with the aggregate pattern reported in Figure 1, and continue to increase progressively

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

15. We obtain similar results if we instead use count data models. Appendix Table A2 shows that our results are robust to using a Poisson regression model with depend variables in levels.

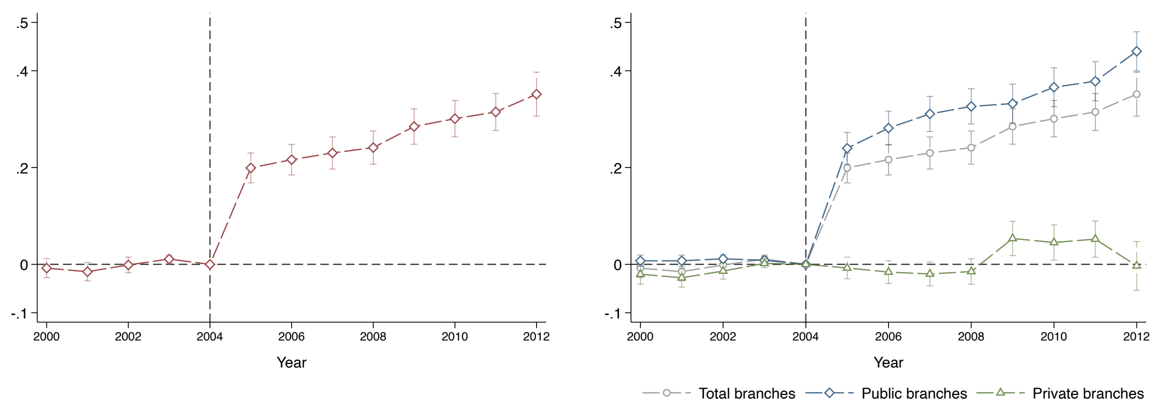
16. An alternative solution is to add a value much smaller than one and use, for instance, $log(X + 0.0025)$. We decide against using this adjustment as our baseline since it does not address the problem that estimation is extremely sensitive to small changes around zero but, as we show in Appendix Table A3, our results are robust to using a $log(X + 0.0025)$ transformation.

throughout the period, with no mean reversion post reform.

We report pooled estimates in Table 2. For all variables, the reform has a strong and significant effect on financial inclusion, entirely driven by government-owned banks. The magnitudes are very large, with the number of bank branches increases by 28.7% (column 1), credit increases by 288% (column 4) and deposits increases by 296% (column 7). Of course, these numbers need to be interpreted with caution as they partially reflect the fact that, by definition, treated cities started with zero public banks. The results are still informative for three reasons. First, they confirm the absence of pre-trends. Second, they show that the expansion public banks increases the overall amount of banks and credit in the city, as public banks do not crowd out private banks. Third, 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 can be interpreted as a change in the steady state of local financial development, rather than a one time infusion of capital.[17]

Nonetheless, given the difficulties in interpreting magnitudes in this setting, we focus throughout the paper on the reduced form effect of the policy on different outcome variables instead of using instrumental-variable methods.

Figure 2: Effect of the Program on Bank Branches



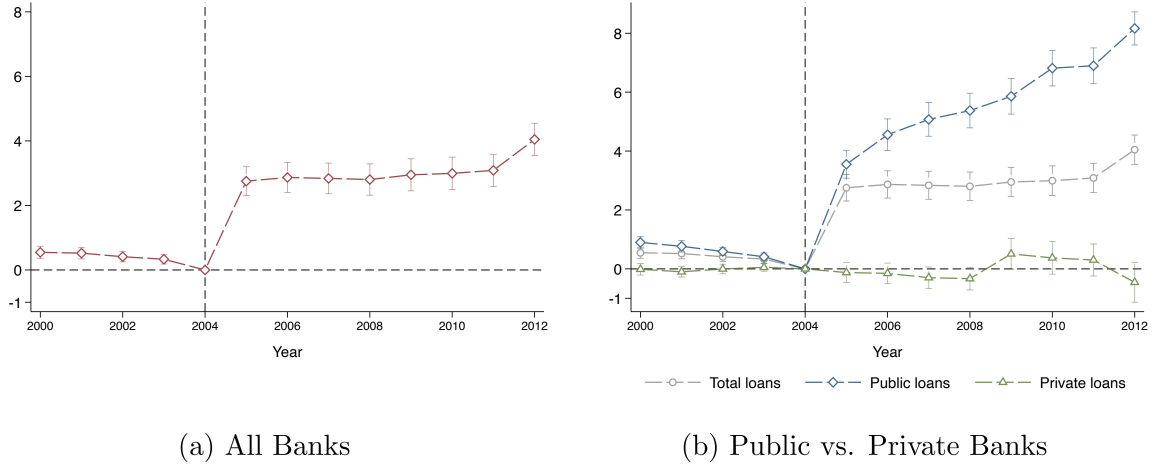
(a) All Banks

(b) Public vs. Private Banks

This figure plots the yearly coefficients and their 95% confidence intervals of the difference-in-differences estimator in Eq. (1) of the 2004 bank reform. Dependent variables are all estimated using the inverse hyperbolic sine transformation

17. 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).

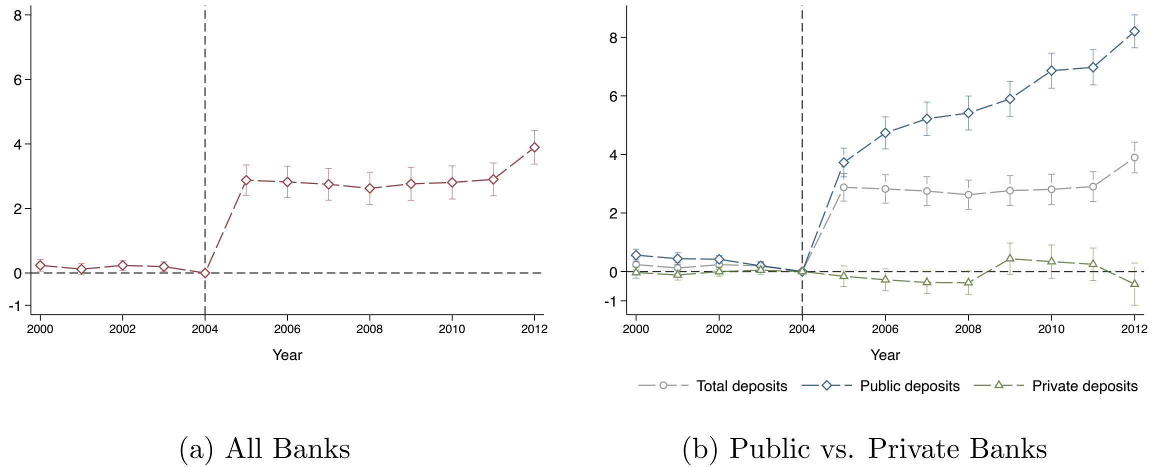
Figure 3: Effect of the Program on Credit



(a) All Banks (b) Public vs. Private Banks

This figure plots the yearly coefficients and their 95% confidence intervals of the difference-in-differences estimator in Eq. (1) of the 2004 bank reform. Dependent variables are all estimated using the inverse hyperbolic sine transformation.

Figure 4: Effect of The Program on Deposits



(a) All Banks (b) Public vs. Private Banks

This figure plots the yearly coefficients and their 95% confidence intervals of the difference-in-differences estimator in Eq. (1) of the 2004 bank reform. Dependent variables are all estimated using the inverse hyperbolic sine transformation.

Table 2: Effect of The Program on Bank Branches Credit and Deposit

Dependent Variable:	Bank Branches			Credit			Deposits		
	All	Public	Private	All	Public	Private	All	Public	Private
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated×Post	0.287***	0.349***	0.020	2.881***	5.729***	-0.096	2.965***	6.021***	-0.125
	(0.018)	(0.017)	(0.016)	(0.230)	(0.265)	(0.211)	(0.238)	(0.263)	(0.214)
City FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Match-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	72,570	72,570	72,570	72,570	72,570	72,570	72,570	72,570	72,570

All columns report estimates of the linear regression model specified in Eq. (1). Dependent variables are all estimated using the inverse hyperbolic sine transformation. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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 talents) 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 Eq. 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 11%, while the size of establishments existing prior to the reform increases by 5.6%. 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 raising by 8.8% (column 2), and higher wages, which increase on average by 3.5%.

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.

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

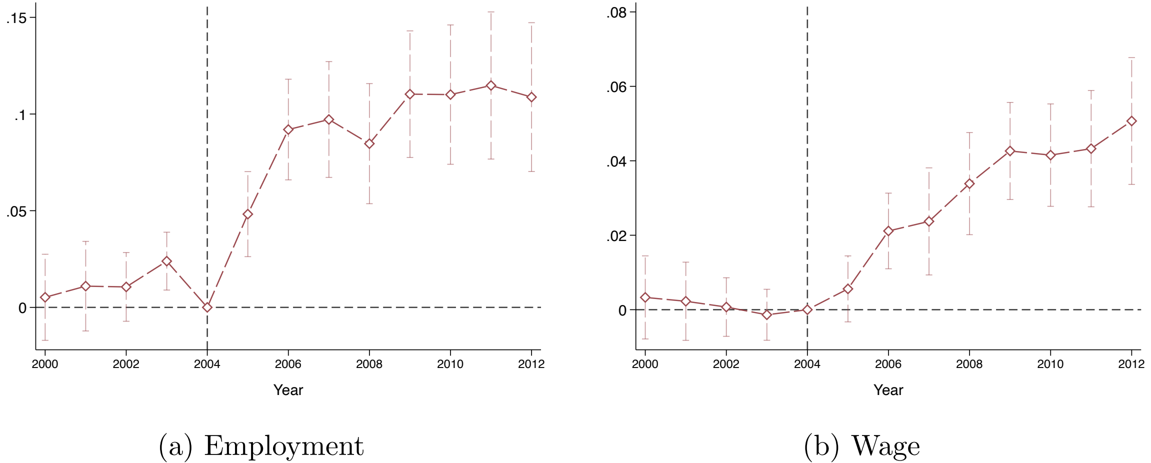
Evolution of sectoral composition. Prior works on economic development have emphasized the role of the manufacturing sector as a key vector of productivity gains and a potential source of changes in economic inequality (e.g., Rodrik, 2012). To analyse if

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	0.112***	0.056***	0.088***	0.035***	0.044***	-0.015***
	(0.012)	(0.015)	(0.016)	(0.006)	(0.007)	(0.005)
City FE	✓	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓	✓
Observations	72,570	72,570	72,570	72,570	72,570	72,570

This table shows the effect of the reform on economic development at the city level. All variables in columns 1–6 are in log. In column 2, the size of the establishment is defined for establishments existing prior to the reform. In column 3, "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.

Figure 5: Effect of the Program on Employment and Wage



(a) Employment

(b) Wage

This figure plots the yearly coefficients and their 95% confidence intervals of the difference-in-differences estimator in Eq. (1) of the 2004 bank reform. In panel (a), the dependent variable is the log of total employment. In panel (b). the dependent variable is the log average wage.

the reform affected the industrial composition of cities, we estimate Eq. 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 A4 in the Appendix reports the results. Overall, we find limited evidence that the industrial composition changed. In particular, we find no change in manufacturing or agriculture, and a statistically significant albeit small increase in some services like retail and construction.

5.2 Mechanisms

There are two main channels through which financial development can promote economic growth. First, bank expansion can create a *demand shock* by relaxing individuals' borrow-

ing constraints and reducing their need for precautionary savings, boosting their demand for non-tradable/local goods. Second, bank expansion can create a *supply shock* by reducing investment frictions, thereby boosting investment of existing firms and facilitating the entry of new firms.

To distinguish between these different hypotheses, we move to the city-2-digit-industry level.[18] We weight each cell by the employment in the industry-city cell, such that results are comparable to the aggregate city level results. Because the reform led to a small change in industry composition, 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.

In Appendix A5, we start by reproducing the baseline results at the city-by-industry level. This allows us to also test the robustness of the results when we control for industry×year fixed effects (columns 4–6), or even tighter industry×group×year fixed effects. 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. Controlling for unobserved time-varying shocks at the industry level increases, if anything, the magnitude of the effects of the reform.[19]

5.2.1 Aggregate 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 use two methods. First, we simply compare firms in agriculture and manufacturing relative to firms in services. Second, we compute the geographical dispersion (HHI) of employment at the industry level, and classify tradable industries if they are 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 4. 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.

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

19. Despite weighting, industry-city level results do not reproduce exactly the results at the city level because the log transformation is not linear.

Table 4: Employment in tradables and non-tradables

Dependent variable	Employment					
Tradable Definition	Agriculture and manufacturing		1st tercile HHI		1st quartile HHI	
Tradable	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	0.104**	0.122**	0.186***	0.225***	0.256***	0.124**
	(0.050)	(0.054)	(0.063)	(0.039)	(0.085)	(0.053)
City×Industry FE	✓	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓	✓
Industry×Year FE	✓	✓	✓	✓	✓	✓
Observations	769,305	1,273,185	678,660	723,705	498,090	513,615

This table reports the effect of the policy on employment at the city-by- (2 digit) industry level. The dependent variable is the inverse hyperbolic sine transformation of the log function defined as: $log[X + (X^2 + 1)^{1/2}]$. In columns 3–6, we define tradable industries based on the geographical HHI of employment of each industry. Low HHI (columns 3 and 5) means that the industry is more concentrated geographically. Standard errors are clustered at the city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

5.2.2 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 development 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. To be able to account precisely for entry and exit and to separate the total change in firms and employment into different margins, we modify our specification and look at the mid-point growth rate between the pre and post periods.[20]

We proceed in two steps. First, we collapse all variables into two periods: pre-reform (2000–2004) and post-reform (2005–2014). Then, we compute the mid-point growth rate

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

and estimate the following equation:

$$\Delta Y_{p,c,j,t} = \beta_1 \; Treated_c \times Post_t \times \mathbb{1}Proxy \\ + \beta_2 \; Treated_c \times Post_t + \delta_{p,j,t} \times \mathbb{1}Proxy + \; \varepsilon_{p,c,j,t} \quad (2)$$

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. An appealing property of using this specification is that we can obtain exactly the same effect of the reform across different levels of aggregation if the regression is scaled by the lag denominator of the dependent variable.[21]

To measure firm entry and exit, we count the number of firms entering or leaving the city each year and set the year 2000 to zero, such that:

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

The two proxies we use to disentangle between the monitoring and fixed costs theories build on 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 $High\ Distance_c$ 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, which is 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 $High\ Fixed\ Costs_j$ 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.[22]

21. In our case, because we still want to obtain an effect closer to aggregate and to remain consistent, we use $[(X_t + X_{t-1}) \times 0.5] \times population_{c,2000}$, which does not affect the aggregation property.

22. In dynamic settings in which productive but poor individuals can accumulate savings over time to

We report results in Table 5. 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 (+17.5%, column 3) and this increase is mostly driven by an increase in new firms (+13.5%, column 4), while the number of exiting firms declines slightly (-4.3%, column 5).

Unpacking the effect on firm growth reveals interesting dynamics and shows the importance 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 and leads untalented but unconstrained entrepreneurs to exit the market. In this respect, our results confirm the importance of misallocation of talents 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 see a relatively lower gain in employment (-6.6%, 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 (-9.6%, column 3). 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 (+17.8%, column 3). The increase in the number of firms is driven by an even larger increase in firm entry (+25.9%, column 4), that compensates and potentially causes an increase in firm exit (+8%, column 5).

save their way out of financial constraints, costs need to be even more non-convex 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).

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.

Table 5: 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	0.089***	0.026**	0.062***	0.204***	0.142***
	(0.019)	(0.011)	(0.014)	(0.025)	(0.019)
Treated×Post×High Distance	0.239***	0.093***	0.175***	0.131***	-0.043*
	(0.025)	(0.017)	(0.019)	(0.033)	(0.026)
		Panel B: Fixed costs in investment			
Treated×Post	0.217***	0.059***	0.178***	0.259***	0.080***
	(0.018)	(0.011)	(0.014)	(0.023)	(0.019)
Treated×Post×High Fixed Costs	-0.066***	0.006	-0.096***	-0.003	0.092***
	(0.020)	(0.018)	(0.016)	(0.026)	(0.022)
Match×Industry×Year FE	✓	✓	✓	✓	✓
Match×Industry×Year× Proxy FE	✓	✓	✓	✓	✓
Observations	136,029	101,704	136,029	136,029	136,029

This table shows the effect of the expansion of public banks on the growth of employment and firm growth. Dependent variables are defined as the growth rate of the average value pre-reform (2000–2004) relative to post-reform (2005–2014). Growth rate is defined as the arc percentage change, computed as $g_t = (X_t - X_{t-1})/[(X_t + X_{t-1}) \times 0.5]$. 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.

6 Effect on inequality

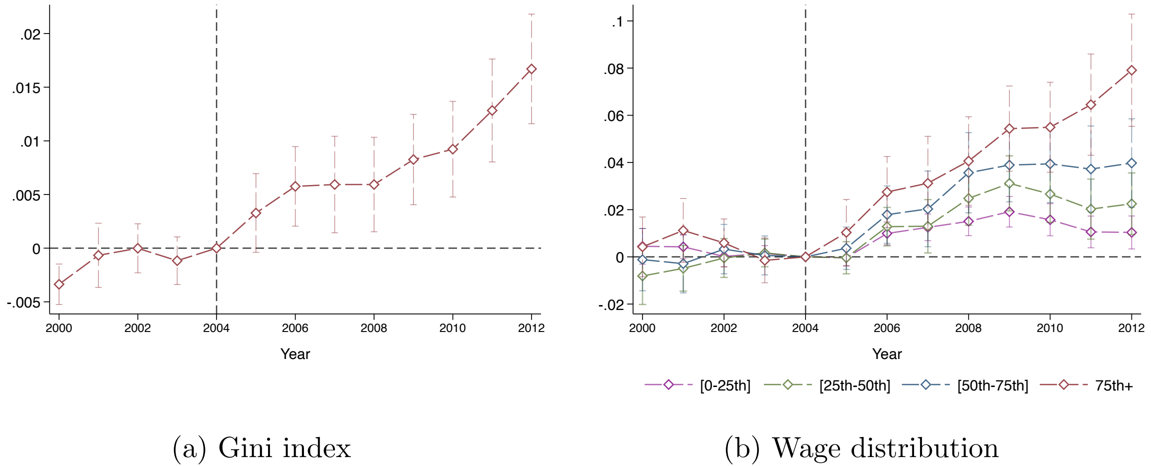
6.1 Aggregate results

To study how an increase in financial development affects the wage distribution in each local labor market, we estimate Eq. 1 using the wage Gini at the city level as an outcome, as well as the average wage per worker in each bin of the city-level wage distribution. We report graphically 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 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 lift 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 position in the wage distribution.

Figure 6: Effect of the Program on Wage Inequality



(a) Gini index

(b) Wage distribution

This figure plots the yearly coefficients and their 95% confidence intervals of the difference-in-differences estimator in Eq. (1) of the 2004 bank reform. In panel B, the wage distribution is computed every year at the city level.

Table 6: Effect of the Program on Wage Inequality

Dependent variable:	Gini	Wage			
		[0–25th]	[25th–50th]	[50th–75th]	[75th+]
	(1)	(2)	(3)	(4)	(5)
Treated×Post	0.011***	0.009***	0.022***	0.031***	0.048***
	(0.002)	(0.003)	(0.005)	(0.007)	(0.009)
City FE	✓	✓	✓	✓	✓
Match-Year FE	✓	✓	✓	✓	✓
Observations	72,570	72,570	72,570	72,570	72,570

This table reports the effect of the policy on income inequality at the city-year 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.

Table 6 reports estimates of Eq. 1. All the results are significant at the 1% 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 1.1 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 4.8% (column 5), an increase five time bigger.

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 7, 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 polynominal on age and fixed effects for sex and seven categories of race.[24] In column 3, we include eleven categories of education.[25] In column 4 we include 2-digit industry fixed effects and in column 5 we include 2-digit industry-by-2 digit occupation fixed effects (4,479 distinct dummies). Finally, in columns 6, we include establishment fixed effects. Across all the different level of controls, we find an overall stable effect of the reform, with higher financial development leading to more inequality.

Table 7: Variance of Wages

Dependent variable	Var[log(wage)]				
Fixed effects	None	Age×Sex ×Race	Education	Industry	Industry ×Occupation
	(1)	(2)	(3)	(4)	(5)
Treated×Post	0.013*** (0.003)	0.014*** (0.003)	0.010*** (0.002)	0.010*** (0.003)	0.010*** (0.002)
City×Industry FE	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓
Observations	72,570	72,570	72,570	72,570	72,570

This table shows the effect of the reform on the change in variance in log(wage) at the city level. From columns 2 to 6, we use as 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 type of race (column 2), added eleven categories for education (column 3), 2-digit industries (column 4), and 2-digit industries× 2 digit occupation (column 5)). Standard errors are clustered at the city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

24. 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.

25. The eleven education categories are: illiterate, primary school dropout, primary school graduate, middle school dropout, middle school graduate, high school dropout, high school graduate, college dropout, college graduate, masters degree, and doctoral degree.

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. 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.[26]

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 disproportionally 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, leading to an increase in the relative demand for skilled workers (Fonseca and Doornik, 2021). A testable implication of either version of the skilled labor demand hypothesis is that, as the relative demand for skilled workers rises, both the relative price and the relative quantity of skilled workers should rise, leading to an increase in the skill premium *and* in the share of skilled workers in treated cities.

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

Better matching. To test if the matching between workers and firms improves following the reform, we do simple variance decompositions of the log(wage) at the industry level and at the firm level.[27] At the industry level, the variance of the log(wage) is:

$$Var(wage_{ct}) = \sum_{j \in c} \frac{N_j}{N_c}(wage_j - wage_c)^2 + \sum_{j \in c} \frac{N_j}{N_c} \sum_{i \in j} \omega_{it}^j (wage_i - wage_j)^2 \qquad (3)$$

where $Var(wage_{ct})$ is the variance of the log wage in a given city c, $wage_j$ is the average log wage in industry j in city c, N_j and N_c are total employment industry j and city c,

26. For direct evidence that financial frictions affect labor misallocation, see Bau and Matray (2020).
27. We detail the decomposition in Appendix A.1.1.

and $wage_i$ is the log wage of individual i that belongs to industry j.[28]

We report results in Table 8. We see that the increase in inequality at the city level is close to evenly split between the between-industry component and the within-industry component (columns 2–3), which likely reflects the very large difference in dynamism post reform between industries more and less dependent on soft information (Table 5). The most important result is the decomposition into the between- and within-firm components (columns 4–5), as it shows that *all* the increase in inequality is coming from a within-firm change in wage variance. This result strongly rejects the hypothesis that inequality rises because of better matching between firms and workers, as this would imply an increase in between-firm inequality.[29]

Table 8: Variance decomposition

Dependent variable	Var[log(Wage)]				
Level of Partition	None	Industry		Firms	
Component		between	within	between	within
	(1)	(2)	(3)	(4)	(5)
Treated×Post×	0.013***	0.008***	0.006***	-0.001	0.015***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
City×Industry FE	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓
Observations	72,570	72,570	72,570	72,570	72,570

This table shows the effect of the reform on the change in variance in log(wage) at the city level. The variance is decomposed using the formula in Eq. 3. Standard errors are clustered at the city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Increase in demand for skilled workers. To test whether a change in the relative demand for skilled workers can explain the rise in wage inequality, we need an ex-ante, time-invariant definition of skill. We leverage the fact that the Brazilian matched employer-employee data allow us to observe education and classify workers as skilled if they have at least some college education and unskilled otherwise.[30]

In Table 9, we start by showing that this measure tracks pretty well the evolution of inequality. In column 1, we show that the skill premium increases by 9% (column 1)

28. Note that whenever we refer to an "industry", we mean an industry×city pair, such that a city is perfectly partitioned into a series of industries.

29. While the fact that all the change in inequality is coming from within-firm changes might seem at odds with the positive between-industry component, this can be explained by high productive entrepreneurs entering disproportionally into certain sectors. In this case, between industries variance would increase, but this would be entirely driven by some specific firms.

30. 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.

and that this increase is driven by a much faster increase in the wage of skilled workers (+12%, column 2) than unskilled workers (+2.7%, column 3). These magnitudes are actually bigger than the wage increase in the top quartile of the distribution (+4.8%) relative to first first quartile (+0.009%), 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). Therefore, the fact that the share of skilled workers does not increase despite a sharp increase in the skill premium suggests that higher relative demand for skilled workers is not the key driver of the rise in wage inequality. In Appendix Table A6, we show that we find similar results when we estimate the effect at the industry-by-city level and control for time-varying industry shocks.

Table 9: Demand for Skilled Workers

Dependent variable	Skill premium	Wage skilled	Wage unskilled	Share skilled
	(1)	(2)	(3)	(4)
Treated×Post	0.090***	0.121***	0.027***	-0.000
	(0.010)	(0.012)	(0.006)	(0.001)
City×Industry FE	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓
Observations	72,492	72,492	72,570	72,570

This table shows the effect of the reform on the variance in log wage at the city-industry level. Standard errors are clustered at the treated-control matched pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Constraints in the supply of skilled workers. To argue that the 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 shock, "outsiders," defined as workers who were living in a different city prior to time t, and "new," who are workers who appear for the first time in labor-market data in a given city and did come from another city.

Table 10 estimates the effect of the reform on the composition of workers across these three groups for all workers (columns 1–3) and skilled workers only (columns 4–6). We find that the reform has no effect on the share of workers coming from other cities in general (column 2), and that it has a positive but very small effect when we focus on skilled workers (column 4), as the share of skilled workers coming from other cities increases by 0.5 %.

Table 10: Worker Migration

Sample:	All workers			Skilled workers		
Dependent variable:	Share local (1)	Share movers (2)	Share new (3)	Share local (4)	Share movers (5)	Share new (6)
Treated×Post	-0.018*** (0.004)	-0.000 (0.001)	0.020*** (0.004)	-0.013** (0.006)	0.005** (0.003)	0.014** (0.006)
City FE	✓	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓	✓
Observations	72,570	72,570	72,570	72,492	72,492	72,492

This table shows the effect of the reform on the share of workers by migration status at the city-year level. Skilled workers are defined as workers with at least a high school degree. "Local" workers are workers that exist in the city the reform. "Movers" are workers that we observe in a different city before year t. "New"' are workers that appear in the city for the first time. All dependent variables are in hyperbolic tangent. Standard errors are clustered at the city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

While the results might be 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 of skilled workers varies as a function of migration cost. We proxy migration cost by the fraction of city outsiders 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 A1 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. 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. For each city-industry-year in the pre-reform period, we compute the skill premium in treated and control cities. We then take the ratio of treated to control skill premium at the industry-by-firm-size-quartile 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 11: Effect on Gini: Heterogeneity in Skill Supply

Dependent variable:	Gini			
	(1)	(2)	(3)	(4)
Treated×Post	0.011*** (0.002)	0.017*** (0.003)	0.016*** (0.003)	0.021*** (0.003)
Treated×Post×High skill gap		-0.012*** (0.003)		-0.011*** (0.003)
Treated×Post×High share skilled population			-0.014*** (0.003)	-0.013*** (0.003)
City×Industry FE	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓
Observations	72,570	72,570	72,570	72,570

This table shows the effect of the reform on wage gini. 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 skill mix in treated cities relative to the national average, and split along the sample median. Standard errors are clustered at the city level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

We split both measures along the sample median and interact each dummy with all the variables, including the fixed effects. Table 11 reports the results. The increase in Gini (column 1) is entirely explained by the increase in 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*×*Post* shows the result for the sub-sample of cities that are below the median of the supply of skilled labor. The total effect for cities with high supply skilled labor is obtained by adding the coefficient of *Treated*×*Post* with the marginal interaction term. Irrespective of the proxy (column 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.

7 Conclusion

In this paper, we show that the expansion of financial access and capital deepening promoted by the government led to an 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 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 Definitions

A.1.1 Variance decomposition

In this section, we detail how we compute the within-firm and between-firm change in inequality that underlie Table 8.

First, we decompose the overall variance in log wage into three components: one between cities (reflecting differences in measurement), one between firms in a given industry-city, and one across workers within a firm and city. We define firms that operate plants in separate industries as different firms, thus biasing the true within-firm component of dispersion downward.

$$
\begin{aligned}
V_t &= \sum_i w_{cjit}(wage_{cjit} - wage_t)^2 \\
&= \underbrace{\sum_c w_{ct}(wage_{ct} - wage_t)^2}_{V_t^C \text{ between-city}} + \underbrace{\sum_c w_{ct} \underbrace{\sum_{j \in c} w_{jt}^c (wage_{jct} - wage_{jt})^2}_{V_{jt}^B \text{between industries within city } c}}_{V_t^B \text{average between-industry}} \\
&+ \underbrace{\sum_c w_{ct} \sum_{j \in c} w_{jt}^c \underbrace{\sum_{i \in j,c} w_{it}^{jc} (wage_{cjit} - wage_{cjt})^2}_{Vcjt^W \text{within firm } i \text{ and industry } j}}_{V_t^W \text{average within-firm}}
\end{aligned} \tag{4}
$$

where i is a firm, j is an industry, c is a city and t is a year. $wage_{ijct}$ is the average wage of a firm belonging to industry j, located in city c at time t, $wage_{jct}$ is the average wage in industry j in city c at time t, and $wage_{ct}$ is the average wage in city c at time t. We ignore the between city component since we care about within city changes in inequality.

A.2 Appendix Tables and Figures

Figure A1: Effect of the Program on Migration by Migration Cost

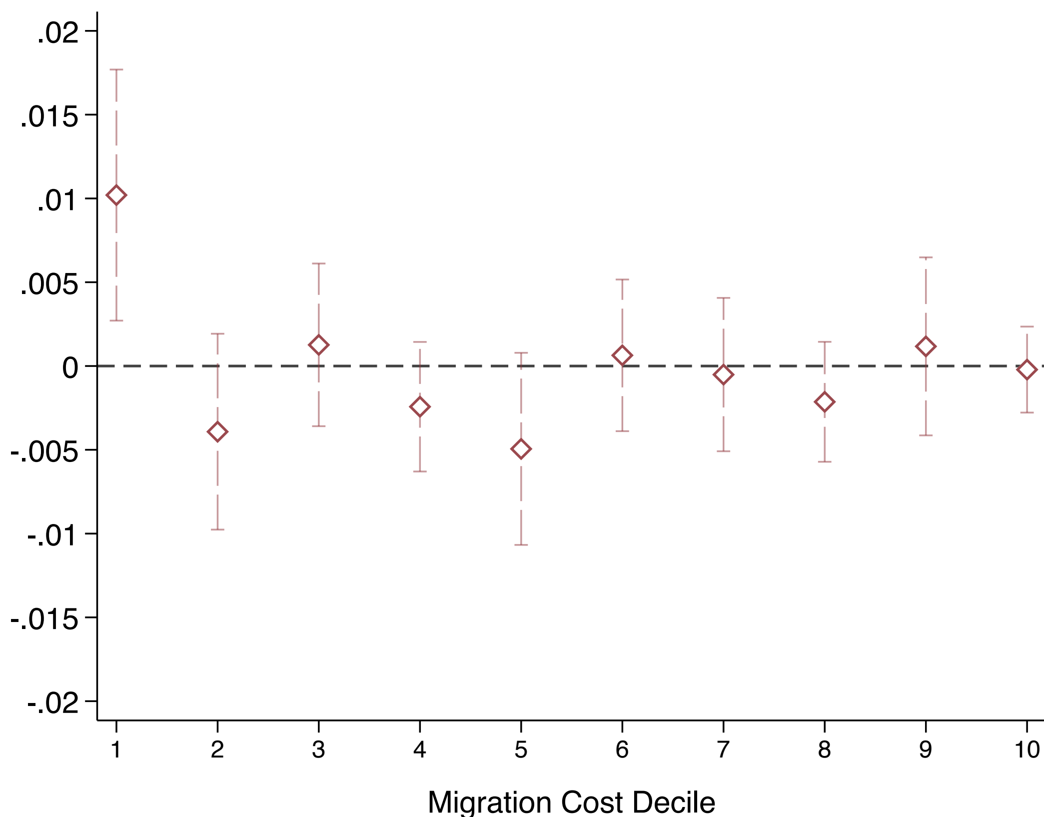

Table A5: Effect on Economic Development: City-Industry

Dependent Variable	Firms			Employment			Wage		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated×Post	0.109*** (0.016)	0.111*** (0.015)	0.111*** (0.017)	0.124*** (0.045)	0.121*** (0.042)	0.116*** (0.047)	0.161*** (0.035)	0.161*** (0.035)	0.163*** (0.036)
City×Industry FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	—	✓	✓	—	✓	✓	—
Industry×Year FE	—	✓	—	—	✓	—	—	✓	—
Match×Industry×Year FE	—	—	✓	—	—	✓	—	—	✓
Observations	2,042,490	2,042,490	2,042,490	2,042,490	2,042,490	2,042,490	2,042,490	2,042,490	2,042,490

Table A1: Robustness to Different Numbers of Matched Controls

Dependent Variable	Credit	Bank Branches	# Firms	Employment	Wage	Gini
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Two control cities per match						
Treated×Post	2.847***	0.290***	0.101***	0.078***	0.039***	0.013***
	(0.233)	(0.018)	(0.013)	(0.017)	(0.007)	(0.002)
City FE	✓	✓	✓	✓	✓	✓
Match-Year FE	✓	✓	✓	✓	✓	✓
Observations	56,085	56,085	56,085	56,085	56,085	56,085
Panel B: One control city per match						
Treated×Post	2.806***	0.289***	0.072***	0.057***	0.044***	0.016***
	(0.283)	(0.021)	(0.015)	(0.019)	(0.008)	(0.002)
City FE	✓	✓	✓	✓	✓	✓
Match-Year FE	✓	✓	✓	✓	✓	✓
Observations	38,190	38,190	38,190	38,190	38,190	38,190

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

Table A2: Robustness to Poisson Model

Dependent variable	Credit	Bank Branches	# Firms	Employment	Wage	Gini
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	0.670***	0.273***	0.087***	0.048***	0.022***	0.015***
	(0.059)	(0.023)	(0.010)	(0.013)	(0.006)	(0.005)
City FE	✓	✓	✓	✓	✓	✓
Match-Year FE	✓	✓	✓	✓	✓	✓
Observations	72,570	72,570	72,570	72,570	72,570	72,570

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

Table A3: Robustness to $log(X + 0.0025)$ Transformation

Dependent variable	Credit	Bank Branches	# Firms	Employment	Wage	Gini
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	3.945***	1.323***	0.108***	0.080***	0.033***	0.032***
	(0.312)	(0.092)	(0.013)	(0.015)	(0.006)	(0.006)
City FE	✓	✓	✓	✓	✓	✓
Match-Year FE	✓	✓	✓	✓	✓	✓
Observations	72,570	72,570	72,570	72,570	72,570	72,570

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

Table A4: 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	-0.000	-0.005	0.003*	0.009***	-0.001	-0.000	0.003	-0.007	-0.001
	(0.004)	(0.005)	(0.001)	(0.003)	(0.001)	(0.001)	(0.003)	(0.006)	(0.002)
City FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	72,570	72,570	72,570	72,570	72,570	72,570	72,570	72,570	72,570

This table shows the effect of the 2004 banking reform on industry composition. Standard errors are clustered at the treated-control matched pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A6: Demand for Skilled Workers

Dependent variable	Skill premium	Wage skilled	Wage unskilled	Share skilled workers
	(1)	(2)	(3)	(4)
Treated×Post	0.055**	0.058***	0.009	-0.006**
	(0.024)	(0.018)	(0.014)	(0.003)
City×Industry FE	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓
Industry×Year FE	✓	✓	✓	✓
Observations	591,327	615,178	1,363,613	1,387,464

This table shows the effect of the reform on the skill premium and the share of skilled workers 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 log. Standard errors are clustered at the treated-control matched pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A7: Skill Supply

Dependent variable:	All workers													
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Treated×Post	0.011***	0.009***	0.009***	0.009***	0.008***	0.007***	0.007***	0.005***	0.006***	0.007***	0.007***	0.008***	0.009***	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Treated×Post×Skill gap	-0.008***		-0.008***	-0.008***	-0.006***	-0.009***	-0.009***	-0.009***	-0.009***	-0.009***	-0.009***	-0.008***	-0.008***	-0.007***
	(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×Share skilled population		-0.008***	-0.007***	-0.008***	-0.009***	-0.008***	-0.012***	-0.005***	-0.010***	-0.009***	-0.009***	-0.007***	-0.008***	-0.010***
		(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×Employment per capita				0.002										0.011***
				(0.002)										(0.004)
Treated×Post×Share skilled labor force					0.010***									0.010***
					(0.002)									(0.002)
Treated×Post×Employment						0.003								-0.012***
						(0.002)								(0.004)
Treated×Post×GDP per capita							0.009***							0.006***
							(0.002)							(0.002)
Treated×Post×Population								0.006***						0.017***
								(0.002)						(0.005)
Treated×Post×Number of firms									0.006***					0.003
									(0.002)					(0.003)
Treated×Post×Number of bank branches										0.005***				-0.001
										(0.002)				(0.002)
Treated×Post×Total credit											0.006***			0.003
											(0.001)			(0.002)
Treated×Post×Share agriculture												-0.003**		0.000
												(0.001)		(0.002)
Treated×Post×Share manufacturing													0.001	-0.001
													(0.001)	(0.002)
City×Industry FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Match×Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	72,510	72,570	72,510	72,510	72,510	72,510	72,510	72,510	72,510	72,510	72,510	72,510	72,510	

This table shows the effect of the reform on the share of workers by migration status. Skilled workers are defined as workers with at least a high school degree. All dependent variables are in hyperbolic tangent. Standard errors are clustered at the treated-control matched pair level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.