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Big Bad Banks? The Winners and Losers from Bank Deregulation in the United States

THORSTEN BECK, ROSS LEVINE, and ALEXEY LEVKOV*

ABSTRACT

We assess the impact of bank deregulation on the distribution of income in the United States. From the 1970s through the 1990s, most states removed restrictions on intrastate branching, which intensified bank competition and improved bank performance. Exploiting the cross-state, cross-time variation in the timing of branch deregulation, we find that deregulation materially tightened the distribution of income by boosting incomes in the lower part of the income distribution while having little impact on incomes above the median. Bank deregulation tightened the distribution of income by increasing the relative wage rates and working hours of unskilled workers.

INCOME DISTRIBUTIONAL CONSIDERATIONS have played a central—if not the central—role in shaping the policies that govern financial markets. For instance, Thomas Jefferson's fears that concentrated banking power would help wealthy industrialists at the expense of others spurred him to fight against the Bank of the United States, and similar anxieties fueled Andrew Jackson's veto of the re-chartering of the Second Bank of the United States (Hammond (1957), Bodenhorn (2003)). During the 20th century, politicians in many U.S. states implemented and maintained restrictions on bank branching, arguing that the formation of large banks would disproportionately curtail the economic opportunities of the poor (Southworth (1928), White (1982), Kroszner and Strahan (1999)). And today, in the midst of a financial crisis, unease about centralized economic power and growing income inequality have fueled debates about Gramm-Leach-Bliley and the desirability of stiffer regulations on financial conglomerates.[1] However, while beliefs about the influence of financial regulations

*Beck is from CentER, Department of Economics and EBC, Tilburg University; Levine is with the Brown University and NBER; Levkov is with The Federal Reserve Bank of Boston. Martin Goetz and Carlos Espina provided exceptional research assistance. We thank an anonymous referee, the Editors, Phil Strahan, and Yona Rubinstein for detailed comments on earlier drafts. We received many helpful comments at the Bank of Israel, Board of Governors of the Federal Reserve System, Brown University, Boston University, International Monetary Fund, European Central Bank, Georgia State University, New York University, Tilburg University, Vanderbilt University, University of Frankfurt, University of Lausanne, University of Mannheim, University of Virginia, and the World Bank. We are grateful to the Charles G. Koch Charitable Foundation for providing financial support. The views expressed herein are those of the authors and not necessarily those of the Federal Reserve Bank of Boston or the Federal Reserve System.

[1]On compensation in the financial sector and income inequality in the overall economy, see Philippon and Reshef (2009) and Kaplan and Rauh (2010). Many have recently suggested that

on the distribution of income affect policies, researchers have devoted few resources toward evaluating the actual impact of financial regulations on the distribution of income. In this paper we econometrically evaluate the causal impact of bank regulations on income distribution.

Theoretical debates and welfare concerns further motivate our analyses of the distributional effects of bank regulation. If banking is a natural monopoly, then unregulated, monopolistic banks may earn rents through high fixed fees that disproportionately hurt the poor as developed in models by Greenwood and Jovanovic (1990), Banerjee and Newman (1993), and Galor and Zeira (1993). Countervailing arguments, however, challenge the view that restricting the consolidation and expansion of banks will help the poor. Regulatory restrictions on bank mergers, acquisitions, and branching could create and protect local banking monopolies, curtailing competition and raising fees that primarily hurt the poor. In light of this debate, we evaluate whether regulatory restrictions on bank expansion increased, decreased, or had no effect on income inequality. Furthermore, bank regulations could directly affect social welfare by altering the distribution of income. As summarized by Kahneman and Krueger (2006) and Clark, Frijters, and Shields (2008), people care about relative income, as well as absolute income and risk. Thus, understanding the impact of financial regulations on the distribution of income provides additional information on the welfare implications of finance.

More specifically, this paper assesses how branch deregulation altered the distribution of income in the United States. From the 1970s through the 1990s, most states removed restrictions on intrastate branching, which intensified bank competition and improved bank efficiency and performance (Flannery (1984), Jayaratne and Strahan (1998)). Researchers have examined the impact of these reforms on economic growth (Jayaratne and Strahan (1996), Huang (2008)), entrepreneurship (Black and Strahan (2002), Kerr and Nanda (2009)), economic volatility (Morgan, Rime, and Strahan (2004), Demyanyk, Ostergaard, and Sørensen (2007)), and the wage gap between male and female bank executives (Black and Strahan (2001)). In this paper, we provide the first evaluation of the impact of branch deregulation on the distribution of income in the overall economy and help resolve a debate about bank deregulation that extends across two centuries.

The removal of branching restrictions by states provides a natural setting for identifying and assessing who won and lost from bank deregulation. As shown by Kroszner and Strahan (1999), national technological innovations triggered branch deregulation at the state level. Specifically, (i) the invention of automatic teller machines (ATMs), in conjunction with court rulings that ATMs are not bank branches, weakened the geographical bond between customers and banks; (ii) checkable money market mutual funds facilitated banking by mail and telephone, which weakened local bank monopolies; and (iii) improvements in communications technology lowered the costs of using distant banks.

financial deregulation, including bank branch deregulation, the Gramm-Leach-Bliley Act, and the accommodating supervisory approach at the Federal Reserve, contributed both to the financial crisis and to growing income inequality; see, for example, Moss (2009).

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These innovations reduced the monopoly power of local banks, weakening their ability and desire to fight against deregulation. Kroszner and Strahan (1999) further show that cross-state variation in the timing of deregulation reflects the interactions of these national technological innovations with pre-existing state-specific conditions. For example, deregulation occurred later in states where politically powerful groups viewed large, multiple-branch banks as potentially serious competitors. Moreover, as we demonstrate below, neither the level nor rate of change in the distribution of income before deregulation help predict when a state removed restrictions on bank branching, suggesting that the timing of branch deregulation at the state level is exogenous to the state's distribution of income. Consequently, we employ a difference-in-differences estimation methodology that exploits the exogenous cross-state, cross-year variation in the timing of branch deregulation to assess the causal impact of bank deregulation on the distribution of income.

The paper's major finding is that deregulation of branching restrictions substantively tightened the distribution of income by disproportionately raising incomes in the lower half of the income distribution. While income inequality widened in the overall U.S. economy during the sample period, branch deregulation lowered inequality relative to this national trend. This finding is robust to using different measures of income inequality, controlling for time-varying state characteristics, and controlling for both state and year fixed effects. We find no evidence that reverse causality or prior trends in the distribution of income account for these findings. Furthermore, the economic magnitude is consequential. Eight years after deregulation, the Gini coefficient of income inequality is about 4% lower than before deregulation after controlling for state and year fixed effects. Put differently, deregulation explains about 60% of the variation of inequality after controlling for state and year fixed effects.

Removing restrictions on intrastate bank branching reduced inequality by boosting incomes in the lower part of the income distribution, not by shrinking incomes above the median. Deregulation increased the average incomes of the bottom quarter of the income distribution by more than 5%, but deregulation did not significantly affect the upper half of the distribution of income. These results are consistent with the view that the removal of intrastate branching restrictions triggered changes in banking behavior that had disproportionately positive repercussions on lower income individuals.

To provide additional evidence that bank deregulation tightened the distribution of income via changes in bank performance and not through some other mechanism, we show that the impact of deregulation on the distribution of income varied across states in a theoretically predictable manner. In particular, if branch deregulation tightened the distribution of income by improving the operation of banks, then deregulation should have had a more pronounced effect on the distribution of income in those states where branching restrictions were particularly harmful to bank operations before deregulation. Based on Kroszner and Strahan (1999), we use four indicators of the degree to which intrastate branching restrictions hurt bank performance prior to deregulation. For example, in states with a more geographically diffuse population,

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branching restrictions were particularly effective at creating local banking monopolies that hindered bank performance. After deregulating, therefore, we should observe a bigger effect on bank performance in states with more diffuse populations. This is what we find. Across the four indicators of the cross-state severity of branching restrictions and their impact, we find that deregulation reduced income inequality more in those states where these branching restrictions had been particularly harmful to bank operations. These findings increase confidence in the interpretation that deregulation reduced income inequality by enhancing bank performance.

We finish by conducting a preliminary exploration of three possible explanations for the channels underlying these findings. We view this component of the analysis as a preliminary exploration because each of these explanations warrants independent investigation with individual-level longitudinal data sets. Nevertheless, we provide this extension to further motivate and guide future research on the channels linking bank performance and the distribution of income.

The first two explanations stress the ability of the poor to access banking services directly. In Galor and Zeira (1993), for example, credit market imperfections prevent the poor from borrowing to invest more in education, which hinders their access to higher paying jobs. Deregulation that eases these credit constraints, therefore, allows lower income individuals to invest more in education, reducing inequality. A second explanation focuses on the ability of the poor to become entrepreneurs. In Banerjee and Newman (1993), financial imperfections are particularly binding on the poor because they lack collateral and because their incomes are relatively low compared to the fixed costs of obtaining bank loans. Thus, deregulation that improves bank performance by lowering collateral requirements and borrowing costs will disproportionately benefit the poor by expanding their access to bank credit.

A third explanation highlights the response of firms to the lower interest rates triggered by deregulation, rather than stressing increased access to credit by lower income individuals. While the drop in the cost of capital encourages firms to substitute capital for labor, the cost reduction also increases production, boosting the demand for capital and labor. On net, if the drop in the cost of capital increases the demand for labor and this increase falls disproportionately on lower income workers, then deregulation could reduce inequality by affecting firms' demand for labor.

Although branch deregulation stimulated entrepreneurship and increased education, our results suggest that deregulation reduced income inequality primarily by boosting the relative demand for low-skilled workers. More specifically, deregulation dramatically increased the rate of new incorporations (Black and Strahan (2002)) and the rates of entry and exit of nonincorporated firms (Kerr and Nanda (2009)). However, the reduction in total income inequality is fully accounted for by a reduction in earnings inequality among salaried employees, not by a movement of lower income workers into higher paying self-employed activities or by a change in income differentials among the self-employed. Furthermore, the self-employed account for only about 9% of the working age population, and this percentage did not change significantly after

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deregulation. On education, Levine and Rubinstein (2009) find that the fall in interest rates caused by bank deregulation reduced high school dropout rates in lower income households. Yet, changes in educational attainment do not account for the reduction in income inequality triggered by branch deregulation during our sample period. Rather, consistent with the view that bank deregulation increased the relative demand for low-income workers, we find that deregulation increased the relative wages and relative working hours of unskilled workers, thus accounting for a tightening of income distribution. Beyond the increase in the relative wages and working hours of low-income workers, bank deregulation also lowered the unemployment rate, further emphasizing the labor demand channel.

This paper relates to policy debates concerning the current financial crisis and the role of financial markets in promoting economic development. First, the international policy community increasingly emphasizes the benefits of providing the poor with greater access to financial services as a vehicle for fighting poverty and reducing inequality. In a broad cross-section of countries, Beck, Demirguc-Kunt, and Levine (2007) find that an overall index of banking sector development is associated with a reduction in income inequality across countries, but they do not analyze the impact of a specific, exogenous policy change. Burgess and Pande (2005) find that when India reformed its banking laws to provide the poor with greater access to financial services, this policy change reduced poverty by boosting wages in rural areas. Our findings also suggest that financial development might help the poor primarily by intensifying competition and boosting wage earnings, not by increasing the income of the poor from self-employment.

Second, given the severity of the global financial crisis, many governments are re-evaluating their approaches to bank regulation. Many economists and policymakers stress the potential dangers of financial deregulation. Though our work does not examine the current crisis, the results do indicate that regulations that impeded competition among banks during the 20th century were disproportionately harmful to lower income individuals, which should not be ignored as countries rethink and redesign their regulatory systems.

The remainder of the paper proceeds as follows. Section I describes the data and econometric methodology. Section II provides the core results, while Section III provides further evidence on how deregulation influences labor market conditions. Section IV concludes.

I. Data and Methodology

To assess the effect of branch deregulation on income distribution, we gather data on the timing of deregulation, income distribution, and other banking sector and state-level characteristics. This section presents the data and describes the econometric methods.

A. Branch Deregulation

Historically, most U.S. states had restrictions on branching within and across state borders. With regards to intrastate branching restrictions, most states

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allowed bank holding companies to own separately capitalized and licensed banks throughout a state. Other states were "unit banking states," in which each bank was typically permitted to operate only one office.

Beginning in the early 1970s, states started relaxing these restrictions, allowing bank holding companies to consolidate subsidiaries into branches and permitting de novo branching throughout the state. This deregulation led to significant entry into local banking markets (Amel and Liang (1992)), consolidation of smaller banks into large bank holding companies (Savage (1993), Calem (1994)), and conversion of existing bank subsidiaries into branches (McLaughin (1995)). This relaxation, however, came gradually, with the last states lifting restrictions following the 1994 passage of the Riegle-Neal Interstate Banking and Branching Efficiency Act.

Consistent with Jayaratne and Strahan (1996) and others, we choose the date of deregulation as the date on which a state permitted branching via mergers and acquisitions (M&As) through the holding company structure, which was the first step in the deregulation process, followed by de novo branching. In the Internet Appendix (an Internet Appendix for this article is available online in the "Supplements and Datasets" section at http://www.afajof.org/supplements.asp) we present the deregulation dates. Twelve states deregulated before the start of our sample period in 1976. Arkansas, Iowa, and Minnesota were the last states to deregulate, only after the passage of the Riegle-Neal Act in 1994. We have data for 50 states and the District of Columbia. Consistent with the literature on branch deregulation, we drop Delaware and South Dakota because the structure of their banking systems was heavily affected by laws that made them centers for the credit card industry.

Over this period, states also deregulated restrictions on interstate banking by allowing bank holding companies to expand across state borders. We confirm this paper's results using the date of interstate deregulation instead of the date of intrastate deregulation. However, when we simultaneously control for inter- and intrastate bank deregulation, we find that only intrastate deregulation enters significantly. Thus, we focus on intrastate rather than interstate deregulation throughout the remainder of this paper.

B. Income Distribution Data

Information on the distribution of income comes from the March Supplement of the Current Population Survey (CPS), which is an annual survey of about 60,000 households across the United States. The CPS is a repeated, representative sampling of the population, but it does not trace individuals over time. The CPS provides information on total personal income, wage and salary income (earnings), proprietor income, income from other sources, and a wide array of demographic characteristics in the year prior to the survey. Most importantly for our study, we start with the 1977 survey because the exact state of residence is unavailable prior to this survey. Each individual in the CPS is assigned a probability sampling weight corresponding to his or her representativeness in the population. We use sampling weights in all our analyses.

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We measure the distribution of income for each state and year over the period 1976 to 2006 in four ways. First, the Gini coefficient of income distribution is derived from the Lorenz curve. Larger values of the Gini coefficient imply greater income inequality. The Gini coefficient equals zero if everyone receives the same income, and equals one if a single individual receives all of the economy's income. We present results with both the natural logarithm of the Gini coefficient as well as the logistic transformation of the Gini coefficient (logistic Gini) in the regression analyses. While using the logistic Gini does bound the minimum value at zero, using the log of Gini allows one to interpret the regression coefficient as a percentage change. Our second measure of income distribution is the Theil index, which is also increasing in the degree of income inequality. If all individuals receive the same income, the Theil index equals zero, while the Theil index equals Ln(N) if one individual receives all of the economy's income, where N equals the number of individuals. An advantage of the Theil index is that it is computationally easy to decompose the index into that part of inequality accounted for by differences in income between groups in the sample and that part of inequality accounted for by differences within each group. Third, we examine the difference between the natural logarithm of incomes of those at the 90th percentile and those at the 10th percentile (Log (90/10)). Finally, we use the difference between the natural logarithm of incomes of those at the 75th percentile and those at the 25th percentile ((Log (75/25)). The Internet Appendix provides more detailed information on the construction of these income distribution indicators.

Consistent with studies of the U.S. labor market, our main sample includes prime-age (age 25 to 54) civilians that have nonnegative personal income, and excludes (i) individuals with missing observations on key variables (education, demographics, etc.), (ii) individuals with total personal income below the 1st or above the 99th percentile of the distribution of income, (iii) people living in group quarters, (iv) individuals who receive zero income and live in households with zero or negative income from all sources of income, and (v) a few individuals for whom the CPS assigns a zero (or missing) sampling weight. As discussed below, the results are robust to relaxing these standard definitions of the relevant labor market. The Internet Appendix provides details on the construction of the sample.

There are 1,859,411 individuals in our sample. The average age in the sample is 38 years, 49% are female, and 75% are white, non-Hispanic individuals. In the sample, 49% have a high school degree or less, while 27% graduated from college. Only 9% of the individuals report being self-employed (entrepreneurs).

In the Internet Appendix, we present basic descriptive statistics on the five measures of income inequality, which are measured at the state-year level. In particular, we have data for the 31 years between 1976 and 2006 and for 48 states plus the District of Columbia. Thus, there are 1,519 state-year observations. Besides providing information on the means of the inequality indicators and their minimum and maximum values, we also present three types of standard deviations of the natural logarithms of the inequality indexes: cross-state, within-state, and within state-year. The cross-state standard deviation of Y is

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the standard deviation of $(Y_{st} - \tilde{Y}_s)$, where $\tilde{Y}_s$ is the average value of Y in state s over the sample period. The within-state standard deviation of Y is the standard deviation of $(Y_{st} - \tilde{Y}_t)$, where $\tilde{Y}_t$ is the average value of Y in year t. The within state-year standard deviation of Y is the standard deviation of $(Y_{st} - \tilde{Y}_s - \tilde{Y}_t)$, where $\tilde{Y}_s$ is the average value of Y in state s and $\tilde{Y}_t$ is the average value of Y in year t. These standard deviations help in assessing the economic magnitude of the impact of bank branch deregulation on the distribution of income.

C. Control Variables

To control for time-varying changes in a state's economy, we use U.S. Department of Commerce data to calculate the growth rate of per capita Gross State Product (GSP). We also control for the unemployment rate, obtained from the Bureau of Labor Statistics, and a number of state-specific, time-varying sociodemographic characteristics, including the percentage of high school dropouts, the proportion of blacks, and the proportion of female-headed households.

We also test whether the impact of deregulation on income inequality varies in a predictable way with different state characteristics at the time of deregulation. As we discuss below, we control for the interaction of branch deregulation with a unit banking indicator, the small bank share, the small firm share, and population dispersion. The unit banking indicator equals one if the state had unit banking restrictions prior to deregulation and zero otherwise. The following states had unit banking before deregulation: Arizona, Colorado, Florida, Illinois, Iowa, Kansas, Minnesota, Missouri, Montana, Nebraska, North Dakota, Oklahoma, Texas, West Virginia, Wisconsin, and Wyoming. The small bank share equals the fraction of banking assets in the state that are held by banks with assets below the median size bank of each state, while the small firm share equals the proportion of all establishments operating in a state with fewer than 20 employees. Data on the small firm share and small bank share are from Kroszner and Strahan (1999). Population dispersion equals one divided by population per square mile, which is obtained from the U.S. Census Bureau.

D. Methodology

We use a difference-in-differences specification to assess the relation between branch deregulation and income distribution, based on the following regression set-up:

$$Y_{st} = \alpha + \beta D_{st} + \delta X_{st} + \boldsymbol{A}_s + \boldsymbol{B}_t + \varepsilon_{st}, \quad s = 1, \ldots, 49;\ t = 1976, \ldots, 2006. \tag{1}$$

In equation (1), Y_{st} is a measure of income distribution in state s in year t, $\boldsymbol{A}_s$ and $\boldsymbol{B}_t$ are vectors of state and year dummy variables that account for state and year fixed effects, X_{st} is a set of time-varying state-level variables, and ε_{st}

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is the error term. The variable of interest is D_{st}, a dummy variable that equals one in the years after state s deregulates and zero otherwise. The coefficient, β, therefore indicates the impact of branch deregulation on income distribution. A positive and significant β suggests that deregulation exerts a positive effect on the degree of income inequality, while a negative and significant β indicates that deregulation pushed income inequality lower. In total, we have data for 48 states plus the District of Columbia, over 31 years, so the 1,519 state-year observations serve as the basis for much of our analysis.

The difference-in-differences estimation technique allows us to control for omitted variables. We include year-specific dummy variables to control for nation-wide shocks and trends that shape income distribution over time, such as business cycles, national changes in regulations and laws, long-term trends in income distribution, and changes in female labor force participation. We include state-specific dummy variables to control for time-invariant, unobserved state characteristics that shape income distribution across states. We estimate equation (1) allowing for state-level clustering of the errors, that is allowing for correlation in the error terms over time within states.[2]

II. Branch Deregulation and Income Distribution

A. Preliminary Results

Our empirical analysis rests on the assumption that the cross-state timing of bank branch deregulation was unaffected by the distribution of income. Figure 1 shows that neither the level of the Gini coefficient before deregulation nor its rate of change prior to deregulation explains the timing of branch deregulation. In a regression of the year of deregulation on the average Gini coefficient before deregulation or on the rate of change of the Gini coefficient in the years before deregulation, the t-statistic on the inequality indicators is 0.20 and –1.16, respectively.

Additional evidence that income inequality did not affect the timing of branch deregulation emerges from a hazard model study of deregulation. Following Kroszner and Strahan (1999), Table I reports tests of whether the Gini coefficient of income inequality influences the likelihood that a state deregulates in a specific year given that it has not deregulated yet. While the Kroszner and Strahan (1999) sample period starts in 1970, we do not have Gini data available before 1976. Also, since we use the original Kroszner and Strahan

[2]In robustness tests, reported in the Internet Appendix, we confirm the results using both bootstrapped standard errors and seemingly unrelated regression (SUR) standard errors. Bootstrapped standard errors are calculated using the following procedure: First, we take a random sample of 1,519 state-year observations from our data and calculate the impact of deregulation on income inequality while accounting for state and year fixed effects. The sample size is done with replacement such that a certain state in a certain year may appear several times. We take 500 such samples and estimate the impact of deregulation on income inequality 500 times. The standard deviation of the resulting estimates is the bootstrapped standard error. Second, following Bekaert, Harvey, and Lundblad (2005), we estimate SUR standard errors, restricting the off-diagonal elements of the weighting matrix to be identical.

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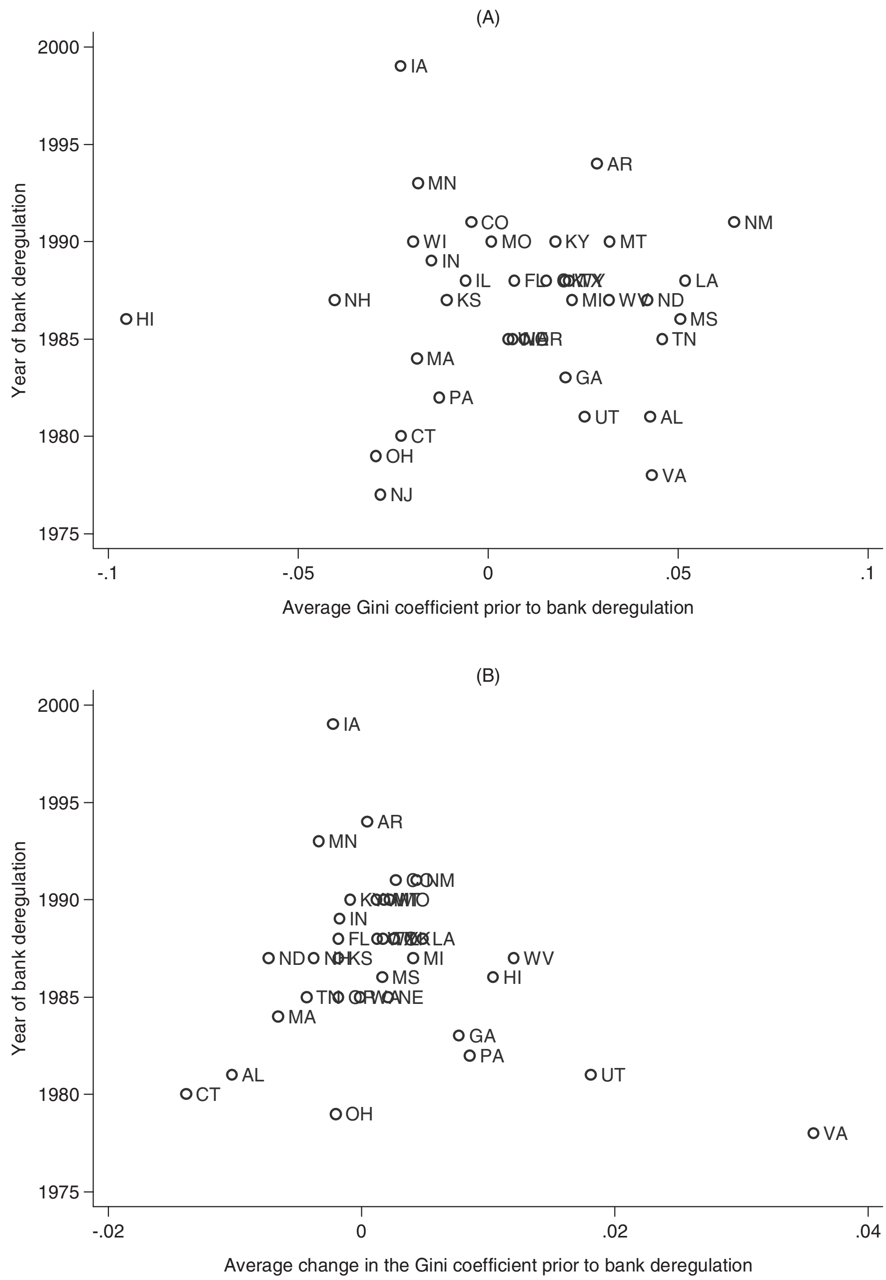

Figure 1. Timing of bank deregulation and pre-existing income inequality: Graphical analysis. Figure (A) shows a scatter plot of the average Gini coefficient of income inequality prior to bank deregulation and the year of bank deregulation. Figure (B) shows a scatter plot of the average *change* in the Gini coefficient of income inequality prior to bank deregulation and the year of bank deregulation. A larger value of the Gini coefficient means greater income inequality. The t-statistics for the correlations in Figures (A) and (B) are 0.20 and –1.16, respectively.

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Table I

Timing of Bank Deregulation and Pre-existing Income Inequality: The Duration Model

The model is a Weibul hazard model where the dependent variable is the log expected time to bank branch deregulation. All the right-hand-side variables are included in levels. The sample period is 1976 to 1994 and the sample comprises 37 states that deregulated after 1977. States drop from the sample once they deregulate. The Gini coefficient of income inequality is calculated from total personal income from the March CPS. Control variables include real per capita GDP, proportion blacks, proportion high school dropouts, proportion female-headed households, and the unemployment rate in a state. Data on real per capita GDP are from the Bureau of Economic Analysis. Proportion blacks, high school dropouts, and female-headed households are calculated from the CPS. Data on the unemployment rate are obtained from the Bureau of Labor Statistics. The political economy factors are from Kroszner and Strahan (1999). These factors include: (1) small bank share of all banking assets, (2) capital ratio of small banks relative to large, (3) relative size of insurance in states where banks may sell insurance, (4) an indicator that takes a value of one if banks may sell insurance, (5) the relative size of insurance in states where banks may not sell insurance, (6) small firm share, (7) share of state government controlled by Democrats, (8) an indicator that takes a value of one if the state is controlled by one party, (9) average yield on bank loans minus Fed funds rate, (10) an indicator that takes a value of one if the state has unit banking laws, and (11) an indicator that takes a value of one if the state changes bank insurance powers. Standard errors are adjusted for state-level clustering and appear in parentheses.

	(1)	(2)	(3)	(4)	(5)
Gini coefficient of income inequality	0.02	0.02	0.03	0.03	0.01
	(0.03)	(0.05)	(0.02)	(0.03)	(0.03)
Controls	No	Yes	No	Yes	Yes
Political-economy factors	No	No	Yes	Yes	Yes
Regional indicators	No	No	No	No	Yes
Observations	408	408	408	408	408

(1999) data set, our sample period ends in 1994, when three states had not yet deregulated—Arkansas, Iowa, and Minnesota.

Table I indicates that the timing of bank branch deregulation does not vary with the degree of pre-existing income inequality. Column 1 reports the results of a regression with only the Gini coefficient of income inequality, while columns 2 through 5 provide regression results controlling for numerous state-level control variables, including those state characteristics employed by Kroszner and Strahan (1999). As shown, the Gini coefficient does not enter significantly in any of the Table I regressions.

B. Deregulation and the Distribution of Income

In Table II, we assess the impact of branch deregulation on income inequality using five indicators of income inequality and two regression specifications. In Panel A, the regressions simply condition on state and year fixed effects, which are not reported. Panel B also includes numerous time-varying, state-specific characteristics: the growth rate of per capita GSP, the proportion of blacks in the population, the proportion of high school dropouts in the

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Table II

The Impact of Deregulation on Income Inequality

The table shows estimates of the impact of bank branch deregulation on the different measures of income inequality. The bank deregulation indicator equals one during all years in which a state permits in-state branching and equals zero otherwise. The measures of income inequality are: (1) logistic transformation of the Gini coefficient, (2) natural logarithm of the Gini coefficient, (3) natural logarithm of the Theil index, (4) natural logarithm of the ratio of the 90th and 10th percentiles, and (5) natural logarithm of the ratio of the 75th and 25th percentiles. The number of observations in each regression corresponds to 49 states (we exclude Delaware and South Dakota) times 31 years between 1976 and 2006. All regressions control for state and year fixed effects. There are no other control variables in Panel A. In Panel B, we control for the growth rate of real per capita GDP, proportion of blacks, proportion of high school dropouts, proportion of female-headed households, and the unemployment rate. Standard errors are clustered at the state level and appear in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Logistic Gini (1)	Log Gini (2)	Log Theil (3)	Log 90/10 (4)	Log 75/25 (5)
Panel A: No Controls					
Bank deregulation	−0.039	−0.022	−0.041	−0.135	−0.077
	(0.013)***	(0.008)***	(0.016)**	(0.058)**	(0.020)***
R^2	0.36	0.35	0.43	0.74	0.60
Observations	1,519	1,519	1,519	1,519	1,519
Panel B: With Controls					
Bank deregulation	−0.031	−0.018	−0.032	−0.101	−0.066
	(0.011)***	(0.006)***	(0.014)**	(0.050)**	(0.017)***
Growth rate of per capita GDP (2000 dollars)	−0.053	−0.028	−0.050	−0.140	−0.114
	(0.072)	(0.041)	(0.081)	(0.229)	(0.119)
Proportion blacks	−0.390	−0.218	−0.462	−0.826	−0.231
	(0.265)	(0.154)	(0.320)	(1.451)	(0.473)
Proportion high-school dropouts	0.256	0.140	0.219	0.432	−0.072
	(0.124)**	(0.071)*	(0.147)	(0.635)	(0.155)
Proportion female-headed households	0.030	0.017	0.028	0.226	0.102
	(0.100)	(0.058)	(0.125)	(0.501)	(0.153)
Unemployment rate	0.011	0.006	0.013	0.069	0.023
	(0.002)***	(0.001)***	(0.003)***	(0.014)***	(0.003)***
R^2	0.40	0.39	0.46	0.75	0.63
Observations	1,519	1,519	1,519	1,519	1,519

population, the proportion of female-headed households in the population, and the unemployment rate.

The Table II results indicate that bank branch deregulation substantially reduced income inequality. The branch deregulation dummy enters negatively and significantly at the 5% level in all 10 regressions. For example, consider the logistic Gini. The column 1 results suggest that deregulation induced a 3.9% reduction in the logistic Gini, which is economically large. To gauge the economic effect of this result, we compare the coefficient estimate to the

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standard deviation of the logistic Gini coefficient after accounting for state and year fixed effects. This standard deviation is 6.5% as shown in the Internet Appendix, suggesting that branching deregulation explains about 60% of the variation of income inequality after controlling for fluctuations in inequality accounted for by state and year effects. That said, state and year fixed effects explain much more of the total variation in inequality than branch deregulation. The R^2 in the logistic Gini regression (column 1) of Table II is 0.36, but branch deregulation explains, on average, only two percentage points of this R^2.

Table II results indicate that deregulation tightened the distribution of income even when controlling for several time-varying state-level factors. Higher unemployment is associated with higher income inequality, though the other state characteristics do not enter independently significantly across the five inequality measures. Given that unemployment is highly correlated over time within a state, we also run regressions including up to five lags of unemployment. This does not change the statistical or economic significance of the coefficient on bank deregulation as shown in the Internet Appendix. Most importantly for the purposes of this paper, the results on deregulation are robust to controlling for unemployment, per capita economic growth, an economy's socio-demographic traits, and educational attainment.

Numerous robustness tests, which are reported in the Internet Appendix, confirm these findings. First, we are concerned that some other time-varying, state-specific characteristic could be both highly correlated with the timing of each state's branch deregulation and powerfully linked to changes in income inequality. Consequently, we also control for the state-specific timing of different labor protection laws, which were constructed by Autor, Donohue, and Schwab (2006). We find that the timing of these labor reforms is not correlated with branch deregulation and the labor market laws do not explain changes in the distribution of income. Thus, bank deregulation is not simply proxying for labor market reforms that underlie the resultant tightening of the distribution of income. Furthermore, we control for an array of time-varying, state-specific traits, including the size of each state's aggregate economy, the level of real per capita income in each state, and lagged values of each state's Gini coefficient. Adding these regressors does not alter the results. Second, we are also concerned that the migration of labor across state lines could affect the results. If deregulation induces interstate labor reallocations that tighten the distribution of income, we want to identify and understand these dynamics. Thus, we regress the share of immigrants per state-year on the branch deregulation dummy, while controlling for year and state fixed effects. We do not find any significant effects of branch deregulation on migration flows. We also control for migration flows directly in the Table II regressions and obtain the same conclusions. Third, although we use the standard sample of prime-aged workers (ages 25 to 54), we conduct a number of robustness tests regarding sample selection. In particular, the results hold when using different age groups, such as 18–64, 18–54, 25–64, 25–35, 36–45, and 46–54. Fourth, since the inclusion or exclusion of outliers could affect the results, we re-run the analyses and confirm

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the findings when (i) including all observations and (ii) excluding individuals with incomes below the 1st and above the 99th percentiles of the year-specific income distribution. Fifth, Figure 1 seems to suggest that Hawaii, Utah, and Virginia might be outliers and we therefore re-run all of the analyses without these states. All of the results hold. Sixth, since Iowa was the last state to deregulate in 1999, we re-run the regressions for the period 1976 to 1999, thus dropping the last seven years of our sample period. All the findings are confirmed. Finally, the results hold when examining household income rather than individual income.

C. Deregulation and Income for Different Income Groups

Although the results in Table II demonstrate that income inequality fell after intrastate branch deregulation, the analyses do not yet provide information on whether the distribution of income tightens because the rich get poorer, or because deregulation disproportionately helps the poor.

We now address this issue by examining the impact of branch deregulation on the income of individuals across the full distribution of incomes. More specifically, we compute the logarithm of income for the ith percentile of the distribution of income in each state s and year t, $Y(i)_{st}$. We do this for i equal to 5, 10, 15, …, 90, and 95. We then run 19 regressions of the form

$$Y(i)_{st} = \alpha + \gamma D_{st} + \boldsymbol{A}_s + \boldsymbol{B}_t + \varepsilon_{st}, \qquad (2)$$

where the regressions are run for each ith percentile of the income distribution. Figure 2 depicts the estimated coefficient, γ, from each of these 19 regressions and also indicates whether the estimates are significant at the 5% level.

Figure 2 shows that intrastate branch deregulation tightened the distribution of income by disproportionately raising incomes in the lower part of the income distribution, not by lowering the incomes of the rich. Specifically, deregulation boosted incomes below the 40th percentile of the distribution of income. Deregulation did not have a significant impact on other parts of the income distribution. Rather than reducing incomes above the median income level, deregulation reduced income inequality by increasing incomes at the lower end of the income distribution.

D. Dynamics of Deregulation and the Distribution of Income

We next examine the dynamics of the relation between deregulation and inequality. We do this by including a series of dummy variables in the standard regression to trace out the year-by-year effects of intrastate deregulation on the logarithm of the Gini coefficient:

$$\mathrm{Log}(Gini)_{st} = \alpha + \beta_1 D_{st}^{-10} + \beta_2 D_{st}^{-9} + \cdots + \beta_{25} D_{st}^{+15} + \boldsymbol{A}_s + \boldsymbol{B}_t + \varepsilon_{st}, \qquad (3)$$

where the deregulation dummy variables, the "D's," equal zero, except as follows: D^{-j} equals one for states in the jth year before deregulation, while D^{+j}

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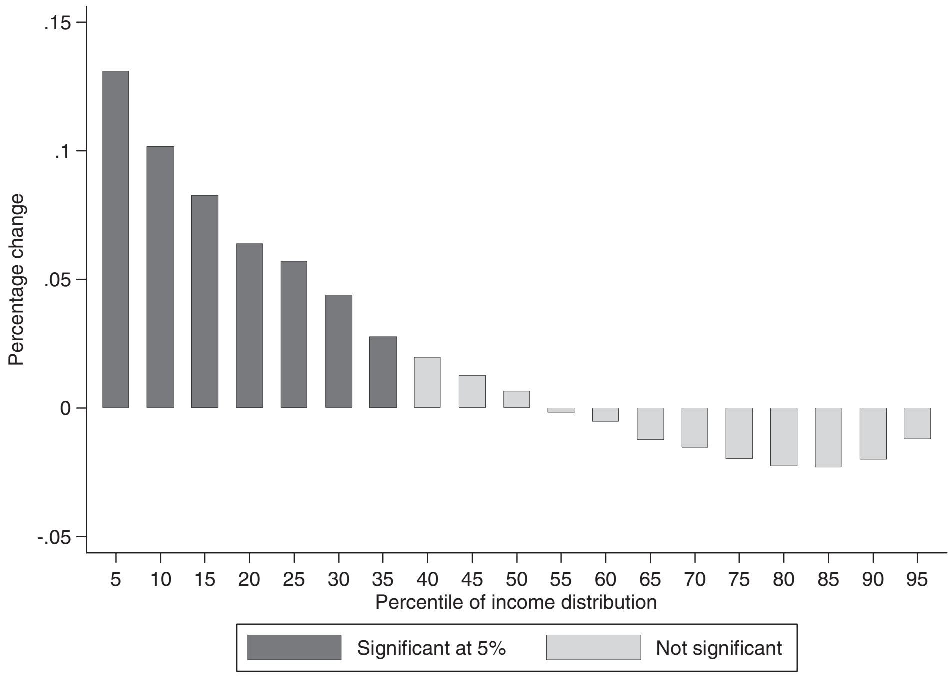

Figure 2. The impact of deregulation on different percentiles of the income distribution. Each bar in the figure represents the estimated impact of bank deregulation on the natural logarithm of a specific percentile of the income distribution. Dark bars represent estimates significant at the 5% levels after adjusting the standard errors for clustering. Light bars represent statistically insignificant estimates. Specifically, we report the estimates of γ from 19 separate regressions of the following form:

$$Y(i)_{st} = \alpha + \gamma D_{st} + \boldsymbol{A}_s + \boldsymbol{B}_t + \varepsilon_{st},$$

where $Y(i)_{st}$ is the natural logarithm of ith percentile of income distribution in state s and year t. D_{st} is a dummy variable that equals zero prior to bank deregulation and one afterwards. $\boldsymbol{A}_s$ and $\boldsymbol{B}_t$ are vectors of state and year dummy variables that account for state and year fixed effects, respectively. Each of the 19 regressions has 1,519 observations corresponding to 49 states (we exclude Delaware and South Dakota) times 31 years between 1976 and 2006.

equals one for states in the jth year after deregulation. We exclude the year of deregulation, thus estimating the dynamic effect of deregulation on the income distribution relative to the year of deregulation. The vectors $\boldsymbol{A}_s$ and $\boldsymbol{B}_t$ are vectors of state and year dummy variables, respectively. At the end points, D_{st}^{-10} equals one for all years that are 10 or more years before deregulation, while D_{st}^{+15} equals one for all years that are 15 or more years after deregulation. Thus, there is much greater variance for these end-points and the estimates may be measured with less precision. After de-trending and centering the estimates on the year of deregulation (year 0), Figure 3 plots the results and the 95% confidence intervals, which are adjusted for state-level clustering.

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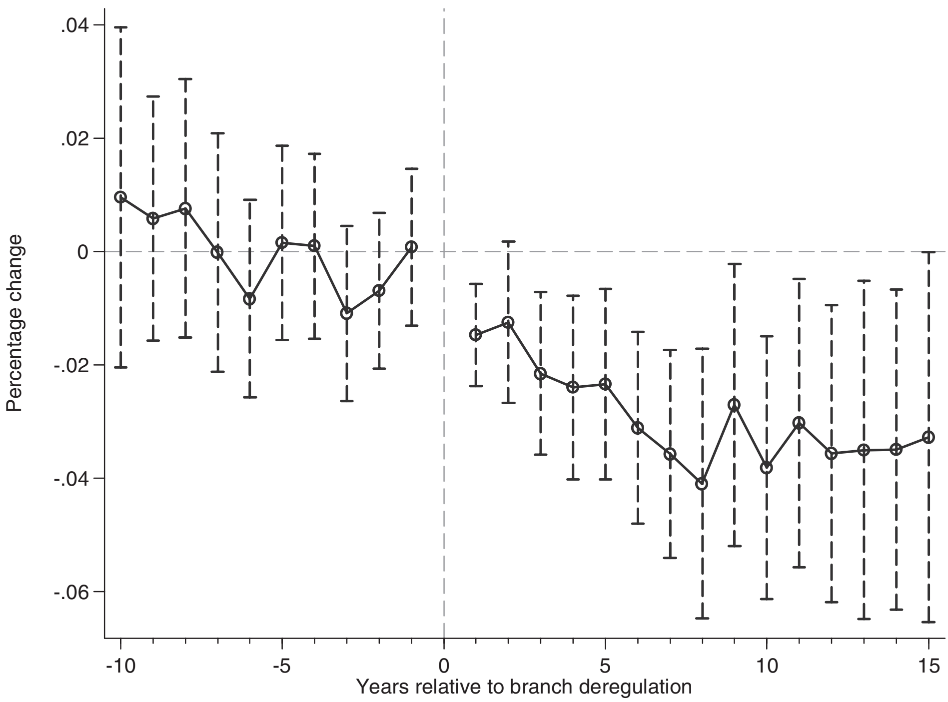

Figure 3. The dynamic impact of deregulation on the Gini coefficient of income inequality. The figure plots the impact of intrastate bank deregulation on the natural logarithm of the Gini coefficient of income inequality. We consider a 25-year window, spanning from 10 years before deregulation until 15 years after deregulation. The dashed lines represent 95% confidence intervals, adjusted for state-level clustering. Specifically, we report estimated coefficients from the following regression:

$$\log(Gini)_{st} = \alpha + \beta_1 D_{st}^{-10} + \beta_2 D_{st}^{-9} + \cdots + \beta_{25} D_{st}^{+15} + \boldsymbol{A}_s + \boldsymbol{B}_t + \varepsilon_{st}.$$

The D's equal zero, except as follows: D^{-j} equals one for states in the jth year before deregulation, while D^{+j} equals one for states in the jth year after deregulation. We exclude the year of deregulation, thus estimating the dynamic effect of deregulation on the different percentiles of income distribution relative to the year of deregulation. $\boldsymbol{A}_s$ and $\boldsymbol{B}_t$ are vectors of state and year dummy variables that account for state and year fixed effects, respectively.

Figure 3 illustrates two key points: innovations in the distribution of income did not precede deregulation, and the impact of deregulation on inequality materializes very quickly. As shown, the coefficients on the deregulation dummy variables are insignificantly different from zero for all years before deregulation, with no trends in inequality prior to branch deregulation. Next, note that inequality falls immediately after deregulation, such that D^{+1} is negative and significant at the 5% level. Thus, the particular mechanisms and channels connecting bank deregulation with the distribution of income must be fast acting. The impact of deregulation on inequality grows for about eight years after deregulation and then the effect levels off, indicating a steady-state drop

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in the Gini coefficient of inequality of about 4%. In sum, changes in inequality do not precede deregulation and deregulation has a level effect on inequality, but does not have a trend effect.

E. Mechanism: Impact of Deregulation as a Function of Initial Conditions

We next assess whether the impact of deregulation on the distribution of income varies in predictable ways across states with different initial conditions. If the impact of deregulation on income inequality varies in a theoretically predictable manner, this would provide greater confidence in the conclusions, shed empirical light on the mechanisms through which deregulation influences the distribution of income, and also reduce concerns about reverse causality.

Specifically, if bank deregulation reduced income inequality by boosting bank performance, then the impact of bank deregulation should be stronger in states where branch regulation had a more harmful effect on bank performance prior to deregulation. Following Kroszner and Strahan (1999), we consider four initial conditions that reflect the harmful effects of branch regulation before deregulation. To proxy for the initial conditions, we use data from 1976, though the results are robust to using values measured in the year before each state deregulated. First, unit banking—where states typically restricted banks to having one office—was the most extreme form of branching restriction and exerted the biggest effect on bank performance before deregulation. Thus, we expect that deregulation exerted a particularly large impact on income inequality in states that had unit banks before they deregulated. Second, states with a high share of small banks should benefit disproportionately from eliminating branching restrictions that protect small banks from competition. Thus, we expect that deregulation had an especially large impact on inequality in states with a comparatively high ratio of small banks at the time of deregulation. Third, small firms tend to face greater barriers to obtaining credit from distant banks than larger firms, suggesting that local branching restrictions that protect local banking monopolies were particularly harmful in states dominated by small firms. We thus expect that deregulation had a bigger impact in states with a large proportion of small firms prior to deregulation. Finally, we examine population dispersion. Local banking monopolies are likely to be particularly well protected if the population is diffuse, so that other banks tend to be far away. This suggests that deregulation would have a bigger effect on inequality in states with high initial population dispersion. These four initial conditions are not independent. States that adopted unit banking before deregulation tended to have a higher share of small banks and firms, and to have more dispersed populations. The correlations between the four characteristics are far from perfect, however, the highest pair-wise correlation coefficient is 0.53. Since we do not have strong reasons to favor one indicator over another, we provide the results on each in our assessment of whether intrastate branch deregulation has a particularly large effect on the distribution of income in those economies where theory suggests the impact should be largest.

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Table III
The Impact of Deregulation on Income Inequality as a Function of Initial State Characteristics

The table presents estimates of the impact of bank deregulation on the logistic transformation of the Gini coefficient of income inequality as a function of initial state characteristics. Bank deregulation equals one during all years in which a state permits in-state branching and equals zero otherwise. All models control for state and year fixed effects. Since we control for state fixed effects the initial state characteristics are dropped from the regressions. Unit banking states are: CO, AR, FL, IL, IA, KS, MN, MO, MT, NE, ND, OK, TX, WI, WV, and WY. Population dispersion equals one divided by population per square mile, which we obtain from the 1960 estimates of the U.S. Census Bureau. We use the 1976 values of the share of small banks and small firms in a state, which we obtain from Kroszner and Strahan (1999). These data exclude 12 states that deregulated before 1976. Standard errors are adjusted for state level clustering and appear in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Bank deregulation	−0.022	−0.026	0.026	1.797
	(0.014)	(0.013)*	(0.025)	(0.524)***
Deregulation × (unit banking)	−0.033			
	(0.017)*			
Deregulation × (initial population dispersion)		−0.313		
		(0.138)**		
Deregulation × (initial share of small banks)			−0.503	
			(0.189)**	
Deregulation × (initial share of small firms)				−2.062
				(0.590)***
Linear combination	−0.055			
	(0.017)***			
Evaluated at the 25th percentile		−0.029	−0.011	−0.011
		(0.013)**	(0.016)	(0.016)
Evaluated at the 50th percentile		−0.030	−0.029	−0.028
		(0.013)**	(0.015)*	(0.015)*
Evaluated at the 75th percentile		−0.037	−0.043	−0.046
		(0.013)***	(0.016)***	(0.015)***
Observations	1,519	1,519	1,147	1,147

The results in Table III indicate that the impact of branch deregulation on income inequality was stronger in states where branching restrictions had been especially harmful to bank activities before deregulation. As shown in Table III, branch deregulation reduced income inequality more in states that had (i) unit banking (column 1), (ii) a more dispersed population (column 2), (iii) a higher share of small banks (column 3), and (iv) a larger share of small firms (column 4). More specifically, deregulation exerted a strong, negative effect on inequality in unit banking states, while this effect was weaker, both economically and statistically, in nonunit banking states. In terms of population dispersion, the effect of deregulation on the logistic Gini holds across the 25th, 50th, and 75th percentile of the distribution of population dispersion, but is stronger for states with initially more dispersed population. In terms of the share of small banks and the share of small firms, the results indicate

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that branch deregulation exerted an economically large and statistically significant impact on income inequality in those states with above-median values of these pre-deregulation characteristics. Branch deregulation reduced inequality more in states where branching restrictions had been particularly harmful to the operation of the banking system before liberalization, suggesting that branch deregulation tightened the distribution of income by enhancing bank performance.

III. Channels

A. Theories of How Financial Markets Affect the Distribution of Income

Having found that branch deregulation decreased income inequality by affecting bank performance, we now explore three potential channels underlying these findings. The first two explanations rely on branch deregulation improving the ability of the poor to access banking services directly, and the poor using this improved access to either purchase more education or become entrepreneurs. The third explanation focuses on firms' demand for labor, not on the poor directly using financial services. These explanations are not mutually exclusive.

In terms of entrepreneurship, financial imperfections represent particularly severe impediments to poor individuals opening their own businesses for two key reasons: (i) the poor have comparatively little collateral and (ii) the fixed costs of borrowing are relatively high for the poor. From this perspective, branch deregulation that improves credit markets should lower the barriers to entrepreneurship disproportionately for poor individuals (Banerjee and Newman (1993)).

In terms of human capital accumulation, financial imperfections in conjunction with the high cost of schooling represent particularly pronounced barriers to the poor purchasing education, perpetuating income inequality (Galor and Zeira (1993)). In this context, financial reforms that ease financial market imperfections will reduce income inequality by allowing talented, but poor, individuals to borrow and purchase education.

Textbook price theory provides a third channel through which bank deregulation affects income inequality that does not involve the poor directly increasing their use of financial services. Jayaratne and Strahan (1998) show that branch deregulation reduced the cost of capital. Reductions in the cost of capital induce firms to (i) substitute capital for labor and (ii) expand output, which increases demand for capital and labor. On net, if the output effect dominates, the reduction in the cost of capital will increase the demand for labor. Even under these conditions, however, the impact of deregulation on inequality is ambiguous because we do not know if the increased demand for labor falls primarily on higher- or lower-income workers. If deregulation disproportionately increases the demand for lower-income workers, then branch deregulation could tighten the distribution of income by affecting firms' demand for labor, not by directly increasing the use of financial services by relatively low-income individuals.

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B. Evidence on the Entrepreneurship Channel

To provide an initial assessment of the entrepreneurship channel, we decompose the impact of bank branch deregulation on income inequality into that part accounted for by a reduction in the income gap between the self-employed and wage earners and that part accounted for by a reduction in income inequality among the self-employed and among wage earners. We conduct this decomposition in two steps. First, using the Theil index, we decompose income inequality into the "between" component, which measures income inequality between the self-employed and wage earners, and the "within" component, which is composed of inequality among the self-employed and inequality among wage earners. As detailed in the Internet Appendix, the Theil index is easily decomposable into between- and within-group components. Thus, we now examine the Theil index (rather than its log) in decomposing income inequality for each state and year. We then estimate the impact of deregulation on each of these components, controlling for state and year fixed effects. This yields that part of the estimated change in income inequality from deregulation that is accounted for by a reduction in inequality between the self-employed and wage earners and that part accounted for by a reduction in inequality within the two groups.

Enhanced entrepreneurship does not directly account for the impact of deregulation on the distribution of income. As shown in Panel A of Table IV, none of the change in income inequality is accounted for by a reduction in between-group inequality. All of the reduction in income inequality from deregulation is accounted for by a reduction in income inequality among salaried workers. The change in between-group inequality is actually positive, but insignificant. These results are unsurprising in light of the following observations: (i) the self-employed account for only 9% of the sample, (ii) the proportion of self-employed individuals did not increase following branch deregulation, and (iii) the self-employed do not, on average, have higher incomes than salaried employees after accounting for educational differences (Hamilton (2000)). These results do not suggest that the relation between branch deregulation and entrepreneurship is unimportant. Bank deregulation boosted the rate of entry and exit of firms (Black and Strahan (2002), Kerr and Nanda (2009)). Nonetheless, the decomposition findings indicate that direct changes in entrepreneurial income and the movement of lower-income salaried workers into higher-income entrepreneurial activities do not account for the tightening of the distribution of income following deregulation.

C. Evidence on the Education Channel

In Panel B of Table IV, we conduct a similar decomposition but focus on education groups. We divide the sample into those with some education beyond a high school degree (about 51% of the sample) and those with educational attainment of a high school degree or less (about 49% of the sample). Since Panel A shows that all of the reduction in income inequality is accounted for by

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Table IV

Decomposing the Impact of Deregulation on Income Inequality into Between- and Within-Groups

The table reports the impact of bank branch deregulation on the Theil index of income inequality. The bank deregulation indicator equals one during all years in which a state permits in-state branching and equals zero otherwise. The number of observations in each decomposition is 1,519, corresponding to 49 states (we exclude Delaware and South Dakota) times 31 years between 1976 and 2006. All decompositions control for state and year fixed effects. In Panel A, we divide the sample into two mutually exclusive groups: (i) those who are self-employed and (ii) those who work for wages. In Panel B we divide the sample of *wage workers* into two mutually exclusive groups: (i) those with 12 or less years of completed education and (ii) those with 13 or more years of completed education. In the first column, in both panels, we estimate the overall impact of branch deregulation on the Theil index of inequality using all groups. In the next column, we estimate the impact of deregulation on inequality *between* the different groups. In the third column, we estimate the impact of deregulation on inequality *within* the different groups combined. The second and the third columns add up to the first column. In the next columns we estimate the impact of deregulation on income inequality separately within *each* of the groups. Standard errors are adjusted for state-level clustering and appear in parentheses. * and ** indicate statistical significance at the 10% and 5% levels, respectively.

Panel A: All Workers

				Employment Groups	
	Total	Between Groups	Within Groups	Self Employed	Salaried
Bank deregulation	−0.0103	0.0002	−0.0105	−0.0077	−0.0102
	(0.0043)**	(0.0003)	(0.0042)**	(0.0074)	(0.0042)**

Panel B: Salaried Workers

				Education Groups	
	Total	Between Groups	Within Groups	High School or Less	Some College or More
Bank deregulation	−0.0102	−0.0028	−0.0074	−0.0086	−0.0039
	(0.0042)**	(0.0011)**	(0.0035)**	(0.0043)*	(0.0038)

a reduction in inequality among wage earners, we focus only on wage earners in conducting the decomposition by educational attainment.

The reduction in income inequality triggered by branch deregulation is accounted for by both a closing of the gap between low- and high-educated workers and by a decline in inequality among low-educated workers. From Panel B of Table IV, 73% (0.0074/0.0102) of overall income inequality is accounted for by a reduction in inequality within the two education categories, and the bulk of this reduction arises because of a tightening of the distribution of income among the less educated group. Furthermore, 27% (0.0028/0.0102) of the reduction in income inequality explained by bank deregulation is accounted for by a reduction in the income gap between education groups. The between-group results are consistent with at least two possible explanations: (i) bank

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deregulation eased credit constraints and induced lower-income individuals to increase their investment in education, thereby reducing income inequality, and (ii) bank deregulation increased the demand for workers in the lower-education group, reducing between-group inequality.[3]

To evaluate whether an increase in relative educational attainment by low-skilled workers following bank deregulation accounts for the reduction in income inequality, Table V presents two additional analyses. First, we test whether bank deregulation lowers earnings inequality among workers of different ages. Specifically, we assess whether there is a differential effect of branch deregulation on income inequality for the 25–35, 36–45, and 46–54 age groups. Since Figure 3 shows that the impact of deregulation on income inequality is almost immediate and Levine and Rubinstein (2009) find that the main impact of deregulation on education involves a reduction in high school dropout rates, then, if deregulation reduces earnings inequality by increasing education, we should observe this primarily among relatively young workers, not those who are older than 35. If we find the same relation between deregulation and earnings inequality across the different age groups, this would suggest that increased educational attainment is not the primary channel through which bank deregulation reduces income inequality during our estimation period.

Second, we more directly control for education by eliminating the educational attainment component of wage earnings. Specifically, in the analyses thus far, we compute measures of earnings inequality based on the unconditional wage earnings of individuals. We now condition each individual's earnings on educational attainment. That is, we compute that part of an individual's earnings that are unexplained by years of education. We then assess the impact of branch deregulation on measures of earnings inequality that are computed based on conditional earnings. If branch deregulation also reduces these conditional earnings inequality measures, this suggests that deregulation is not reducing earnings inequality only by its effect on educational attainment. In particular, we first regress log earnings on five dummy variables corresponding to the number of years of educational attainment (0–8, 9–11, 12, 13–15, and 16+) and year fixed effects. We then collect the residuals to calculate the conditional earnings inequality measures. In robustness tests, reported in the Internet Appendix, we also control for gender and ethnicity, and obtain the same results.

[3]We also examine whether bank deregulation reduced income inequality by affecting the income gap between black and white individuals or the gap between women and men. First, when splitting the sample between black and white workers, we find that only 20% of the reduction in income inequality is accounted for by a tightening of the income gap between blacks and whites, while 80% of the reduction in total income inequality is accounted for by a tightening of income inequality within the group of whites. Second, when splitting the sample between women and men, we find that the reduction in income inequality is accounted for by a tightening of income inequality among women and among men, but not a reduction in income inequality between women and men. Also, see Demyanyk (2008), who examines the impact of bank deregulation on proprietors differentiated by race and gender.

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Table V

The Impact of Deregulation on Earnings Inequality

The table shows estimates of the impact of bank branch deregulation on the different measures of earnings inequality. The bank deregulation indicator equals one during all years in which a state permits in-state branching and equals zero otherwise. The measures of earnings inequality are: (1) logistic transformation of the Gini coefficient, (2) natural logarithm of the Gini coefficient, (3) natural logarithm of the Theil index, (4) natural logarithm of the ratio of the 90th and 10th percentiles, and (5) natural logarithm of the ratio of the 75th and 25th percentiles. The number of observations in each regression corresponds to 49 states (we exclude Delaware and South Dakota) times 31 years between 1976 and 2006 times three age groups (25–35, 36–45, and 46–54). All regressions control for state and year fixed effects, age fixed effects, age-specific state fixed effects, and age-specific year fixed effects. In Panel A, we use total annual earnings of wage and salary workers. In Panel B, we use total annual earnings of wage and salary workers, which are conditional on years of completed education. Specifically, we first regress log real annual earnings on five dummies of years of completed education (0–8, 9–11, 12, 13–15, and 16+) and year fixed effects and then calculate measures of inequality based on the residuals. When calculating the residuals we use sampling weights provided by the CPS. Standard errors are clustered at the state level and appear in parentheses. ** and *** indicate statistical significance at the 5% and 1% levels, respectively.

	Logistic Gini (1)	Log Gini (2)	Log Theil (3)	Log 90/10 (4)	Log 75/25 (5)
Panel A: Unconditional Earnings					
Bank deregulation	−0.041	−0.025	−0.053	−0.108	−0.054
	(0.018)**	(0.011)**	(0.023)**	(0.038)***	(0.020)***
(Bank deregulation) ×	0.006	0.004	0.013	0.034	−0.005
(Ages 36–45)	(0.015)	(0.010)	(0.020)	(0.036)	(0.015)
(Bank deregulation) ×	0.011	0.007	0.020	0.034	−0.010
(Ages 46–54)	(0.018)	(0.012)	(0.024)	(0.042)	(0.018)
R^2	0.26	0.26	0.26	0.40	0.39
Observations	4,557	4,557	4,557	4,557	4,557
Panel B: Earnings Conditional on Education					
Bank deregulation	−0.040	−0.039	−0.083	−0.005	−0.002
	(0.015)**	(0.015)**	(0.031)**	(0.002)***	(0.001)**
(Bank deregulation) ×	0.004	0.004	0.019	0.001	−0.000
(Ages 36–45)	(0.014)	(0.014)	(0.033)	(0.002)	(0.001)
(Bank deregulation) ×	0.005	0.005	0.016	0.002	−0.001
(Ages 46–54)	(0.015)	(0.015)	(0.034)	(0.002)	(0.001)
R^2	0.50	0.50	0.40	0.48	0.46
Observations	4,557	4,557	4,557	4,557	4,557

As shown in Table V, education does not account for the impact of bank deregulation on earnings inequality, suggesting that branch deregulation reduced earnings inequality primarily by boosting firms' relative demand for low-income workers. First, across the five earnings inequality indicators, we do not find any differential effect of branch deregulation on income inequality among the 25–35, 36–45, and 46–54 age groups. The easing of credit constraints in

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response to bank deregulation is most likely to affect the educational choices of individuals in school, or just out of school. It seems unlikely that branch deregulation would cause a sufficiently large and rapid increase in the educational attainment of workers above the age of 35, such that the resulting increase in earnings would tighten economy-wide measures of earnings inequality in the year after deregulation. Second, bank deregulation reduces conditional earnings inequality, where the conditioning is done based on educational attainment. As shown in Panel B, the estimated impact of deregulation on earnings inequality holds for conditional earnings and there is no differential impact on the 25–35, 36–45, and 46–54 age groups. These findings imply that deregulation is not reducing earnings inequality only through its effect on educational attainment.

D. Evidence on the Labor Demand Channel

We now conduct a more focused examination to determine whether branch deregulation reduced income inequality by increasing the relative demand for unskilled workers. Specifically, we assess the impact of branch deregulation on the relative wages and relative working hours of unskilled vis-à-vis skilled workers, where unskilled workers are those with 12 or fewer years of completed education and skilled workers are those with 13 or more years of education. Our goal is to abstract from differences in experience, race, and gender between unskilled and skilled workers, and control for potentially time-varying returns to experience, race, and gender so as to focus only on differences in wage rates and hours worked between unskilled and skilled workers.

We follow a two-step procedure to compute the relative wage rates and relative working hours of unskilled workers while controlling for differences in experience, race, and gender between unskilled and skilled workers and accounting for time-varying returns to these characteristics. In this examination of relative wages and working hours, we exclude unemployed individuals and instead directly focus on the impact of bank deregulation on unemployment.[4] For simplicity, we describe the procedure for wage rates and simply note that we follow the same two-step procedure for relative working hours. We first estimate the following log hourly wage equation using the sample of skilled workers:

$$w^s_{ist} = X_{ist}\theta^s_t + \varepsilon_{ist}, \tag{4}$$

[4]For these analyses, we use May CPS files for the years 1977 to 1982 and Outgoing Rotation Groups CPS files for the years 1983 to 2006, which, unlike the March CPS files, provide both reported relative wage rates and working hours. Besides the sample selection criteria discussed above, we focus only on non-agricultural wage and salary workers who are either working or with a job but currently not at work. When analyzing wage rates, we further restrict the sample to individuals with positive weekly working hours and hourly earnings above one-half of the real minimum wage in 1982. These restrictions are standard in the literature on wage rates and working hours.

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where w^s_{ist} is the log real hourly wage of skilled worker i in state s during time t and X_{ist} is a vector of person-specific observable characteristics that includes the level, square, cubic and quartic in potential experience, gender and race indicators, and interaction terms between potential experience and gender and race. Equation (4) is estimated separately for every year between 1976 and 2006. This yields time-varying returns to observable characteristics, that is, θ^s_t. This is important given the changes in the structure of wages in the United States since the mid-1970s (Katz and Autor (1999)). Critically, equation (4) also contains a constant term in X_{ist}, so that estimating equation (4) separately in each year provides an estimate of the conditional mean skilled wage rate in each year as part of θ^s_t.

In the second step, we generate the estimated relative wage rate of each unskilled worker i in state s during time t as the worker's actual log real wage rate (w^u_{ist}) minus the estimated wage rate that a skilled worker with the same characteristics would earn:

$$r_{ist} = w^u_{ist} - X^u_{ist}\theta^s_t, \tag{5}$$

where $X^u_{ist}\theta^s_t$ is computed based on the condition that each unskilled worker's observable characteristics (X^u_{ist}) are rewarded at the same time-varying estimated prices (θ^s_t) as his skilled counterpart. In this way, we abstract from potential time-varying differences in the valuation of race, gender, and experience across unskilled and skilled workers in the labor market and focus on relative wage rates and working hours. Furthermore, in computing the relative wage rates of unskilled workers in equation (5), we subtract the estimated time-varying constant term from equation (4), that is, we subtract the conditional mean skilled wage rate in each year in calculating the relative wage rate of unskilled workers. We calculate the relative working hours in exactly the same manner as above, but use weekly working hours instead of wages. We then run regressions similar to those underlying Figure 3. Specifically,

$$r(w)_{ist} = \alpha + \beta_1 D^{-10}_{st} + \beta_2 D^{-9}_{st} + \cdots + \beta_{25} D^{+15}_{st} + \boldsymbol{A}_s + \boldsymbol{B}_t + \varepsilon_{ist}, \tag{6}$$

where $r(w)_{ist}$ is the log real relative wage of unskilled worker i, who resides in state s in year t, and the "D's" equal zero, except as follows: D^{-j} equals one for states in the jth year before deregulation, while D^{+j} equals one for states in the jth year after deregulation. We use an analogous procedure for relative weekly working hours.

Together, Figures 4 and 5 indicate that bank deregulation boosted both the relative wage rates and relative working hours of unskilled workers in comparison to skilled workers. Figure 4 shows that the relative wages of unskilled workers display a significant increase 3 years after branch deregulation, a trend that continues thereafter, with an overall increase of almost 9% 15 years after branch deregulation. Figure 5 shows an immediate impact of branch deregulation on the relative hours worked of unskilled vis-à-vis skilled workers, a trend that continues for the following 15 years, with an overall effect of 1.5 hours per week.

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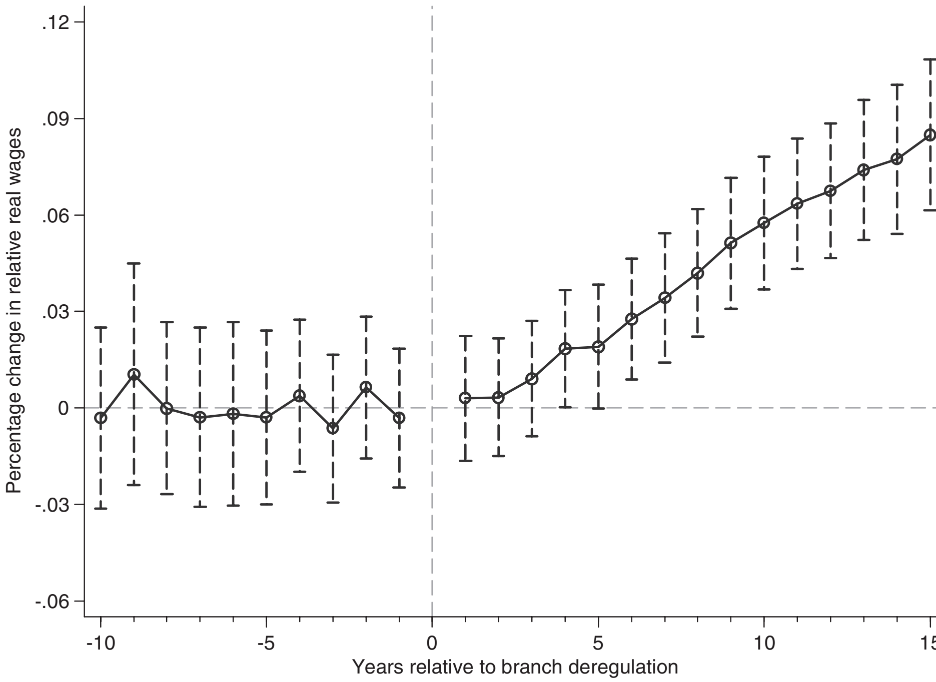

Figure 4. The impact of deregulation on the relative wages of unskilled workers. The figure shows the dynamic impact of branch deregulation on log real wages of unskilled workers relative to skilled workers. Unskilled workers are those with 12 or less years of completed education. Skilled workers are those with 13 or more years of completed education. The impact of deregulation on relative wages is represented by connected circles; dashed "arms" around the circles represent 95% confidence intervals, adjusted for state-level clustering. All estimates are relative to the year of deregulation. Specifically, we report estimated coefficients from the following regression:

$$r(w)_{ist} = \alpha + \beta_1 D_{st}^{-10} + \beta_2 D_{st}^{-9} + \cdots + \beta_{25} D_{st}^{+15} + \boldsymbol{A}_s + \boldsymbol{B}_t + \varepsilon_{st},$$

where $r(w)_{ist}$ is log real relative wages of *unskilled* worker i, who resides in state s in year t. Relative wages are obtained as explained in equations (4) and (5). The D's equal zero, except as follows: D^{-j} equals one for states in the jth year before deregulation, while D^{+j} equals one for states in the jth year after deregulation. We exclude the year of deregulation, thus estimating the dynamic effect of deregulation on the relative wages relative to the year of deregulation. $\boldsymbol{A}_s$ and $\boldsymbol{B}_t$ are vectors of state and year dummy variables that account for state and year fixed effects, respectively.

Figure 6 provides additional evidence on how branch deregulation affects labor demand. Recall that, when examining relative wages and relative hours worked, we examine only those in the labor force and exclude the unemployed. We now focus only on the relation between bank deregulation and the unemployment rate. Specifically, we examine the dynamic effect of branch deregulation on unemployment by running the following regression:

$$\text{Log}\,(Unemployment)_{st} = \alpha + \beta_1 D_{st}^{-10} + \beta_2 D_{st}^{-9} + \cdots + \beta_{25} D_{st}^{+15} + \boldsymbol{A}_s + \boldsymbol{B}_t + \varepsilon_{st}. \tag{7}$$

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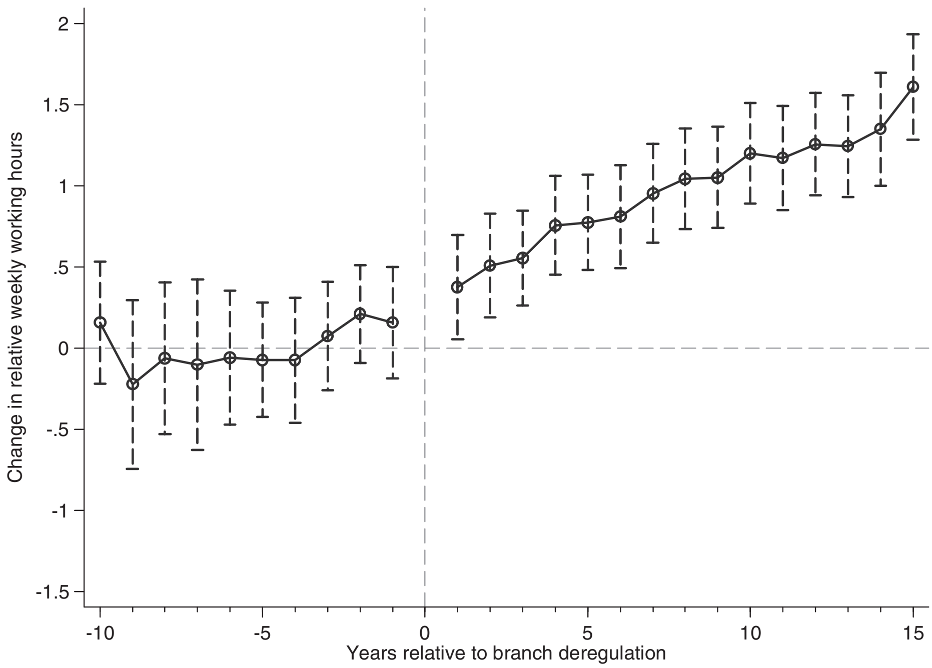

Figure 5. The impact of deregulation on the relative working hours of unskilled workers. The figure shows the dynamic impact of branch deregulation on weekly working hours of unskilled workers relative to skilled workers. Unskilled workers are those with 12 or less years of completed education. Skilled workers are those with 13 or more years of completed education. The impact of deregulation on relative working hours is represented by connected circles; dashed "arms" around the circles represent 95% confidence intervals, adjusted for state-level clustering. All estimates are relative to the year of deregulation. Specifically, we report estimated coefficients from the following regression:

$$r(h)_{ist} = \alpha + \beta_1 D_{st}^{-10} + \beta_2 D_{st}^{-9} + \cdots + \beta_{25} D_{st}^{+15} + \boldsymbol{A}_s + \boldsymbol{B}_t + \varepsilon_{st},$$

where $r(h)_{ist}$ is relative weekly working hours of *unskilled* worker i, who resides in state s in year t. Relative working hours are obtained as explained in equations (4) and (5). The D's equal zero, except as follows: D^{-j} equals one for states in the jth year before deregulation, while D^{+j} equals one for states in the jth year after deregulation. We exclude the year of deregulation, thus estimating the dynamic effect of deregulation on the relative working hours relative to the year of deregulation. $\boldsymbol{A}_s$ and $\boldsymbol{B}_t$ are vectors of state and year dummy variables that account for state and year fixed effects, respectively.

Figure 6 shows that bank deregulation was associated with a significant drop in the unemployment rate starting 2 years after deregulation, with a cumulative effect of more than two percentage points after 15 years. Beyond bank deregulation's positive effect on both the relative wage rates and working hours of unskilled workers, branch deregulation also reduced the unemployment rate.[5]

[5]We extend the analysis of bank deregulation and unemployment along two dimensions. First, the paper's core results in Table II hold when (1) excluding the unemployed from the sample

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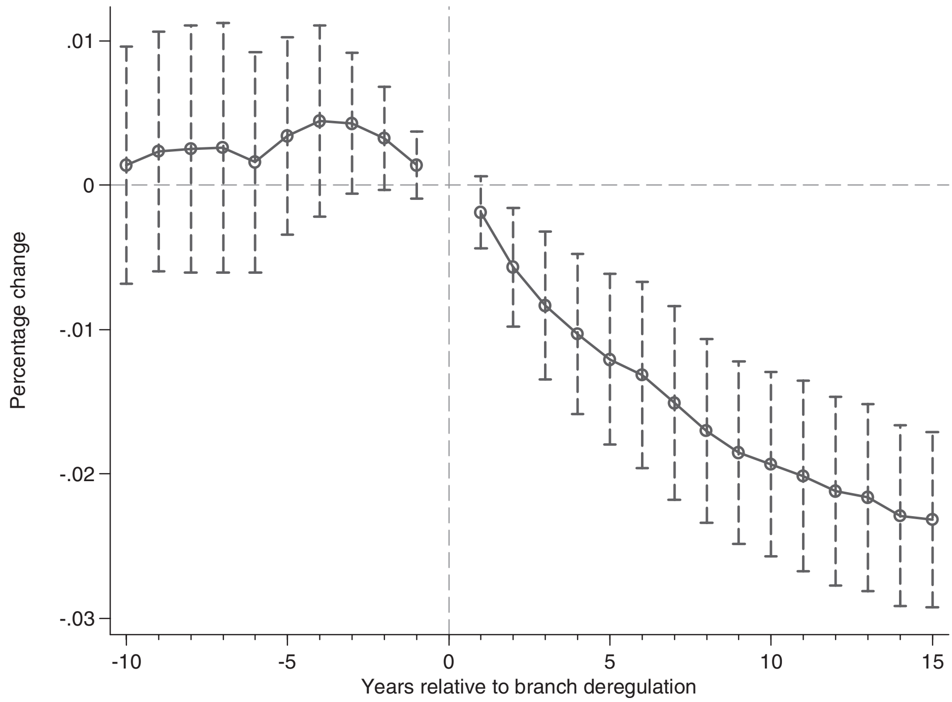

Figure 6. The impact of deregulation on unemployment rate. The figure shows the dynamic impact of branch deregulation on the unemployment rate. The impact of deregulation on the unemployment rate is represented by connected circles; dashed "arms" around the circles represent 95% confidence intervals, adjusted for state-level clustering. All estimates are relative to the year of deregulation. Specifically, we report estimated coefficients from the following regression:

$$\log(unemployment)_{st} = \alpha + \beta_1 D_{st}^{-10} + \beta_2 D_{st}^{-9} + \cdots + \beta_{25} D_{st}^{+15} + \boldsymbol{A}_s + \boldsymbol{B}_t + \varepsilon_{st},$$

where $unemployment_{st}$ is the unemployment rate in state s in year t. The D's equal zero, except as follows: D^{-j} equals one for states in the jth year before deregulation, while D^{+j} equals one for states in the jth year after deregulation. We exclude the year of deregulation, thus estimating the dynamic effect of deregulation on unemployment rate relative to the year of deregulation. $\boldsymbol{A}_s$ and $\boldsymbol{B}_t$ are vectors of state and year dummy variables that account for state and year fixed effects, respectively.

or (2) when controlling for contemporaneous and numerous lagged values of the unemployment rate. These results suggest that the relation between branch deregulation and income inequality is not completely accounted for by a reduction in unemployment following deregulation. Second, when assessing the impact of branch deregulation on income inequality for different levels of initial unemployment rates, we find that states with initially higher levels of unemployment also experience a significantly greater reduction in income inequality after branch deregulation, while states with an initial unemployment rate below the median level across states experience a weaker or even insignificant reduction in income inequality after branch deregulation. As noted, however, bank deregulation is associated with a tightening of the distribution of income even when excluding unemployed individuals from the sample. These results are reported in the Internet Appendix.

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IV. Conclusions

Policymakers and economists disagree sharply about who wins and who loses from bank regulations. While some argue that the unregulated expansion of large banks will increase banking fees and reduce the economic opportunities of the poor, others hold that regulations restrict competition, protect monopolistic banks, and disproportionately help the rich. More generally, an influential political economy literature stresses that income distributional considerations, rather than efficiency considerations, frequently exert the dominant influence on bank regulations as discussed in Barth, Caprio, and Levine (2006), Claessens and Perotti (2007) and Haber and Perotti (2008).

We find that removing restrictions on intrastate branching tightened the distribution of income by increasing incomes in the lower part of the income distribution while having little impact on incomes above the median. This finding is robust to an array of sensitivity analyses. We find no evidence that reverse causality drives the results. Moreover, the impact of deregulation on income distribution varies in a theoretically predictable manner across states with distinct economic, financial, and demographic characteristics at the time of deregulation. These findings support the view that branch regulation in the United States restricted competition, protected local banking monopolies, and impeded the economic opportunities of the relatively poor.

We also present evidence that the impact of branch deregulation on income inequality is an indirect one. There is no evidence that branch deregulation reduces inequality by boosting incomes of the self-employed or by increasing educational attainment. Rather, the effect of branch deregulation on income inequality is driven by a reduction in inequality between skilled and unskilled workers and a reduction in income inequality among unskilled workers. In addition, we show that the relative wages and the relative working hours of unskilled vis-à-vis skilled workers increased significantly after branch deregulation. This is consistent with branch deregulation leading to a greater demand for labor that falls disproportionally on lower-skilled workers who therefore see both their working hours and their wage rates increase.

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