NBER WORKING PAPER SERIES

BIG BAD BANKS? THE IMPACT OF U.S. BRANCH DEREGULATION ON INCOME DISTRIBUTION

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Working Paper 13299
http://www.nber.org/papers/w13299

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2007

Martin Goetz and Carlos Espina provided exceptional research assistance. We especially thank Phil Strahan for providing insightful comments and sharing his data and Yona Rubinstein, whose suggestions substantively improved the quality of the research. We received many helpful comments at the World Bank conference, "Access to Finance," and seminars 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, University of Frankfurt, University of Lausanne, University of Mannheim, and the University of Virginia. This paper's findings, interpretations, and conclusions are entirely those of the authors and do not necessarily represent the views of the World Bank, its Executive Directors, the countries they represent, or the National Bureau of Economic Research.

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NBER Working Paper No. 13299
August 2007, Revised June 2009
JEL No. D31,G28,O16

ABSTRACT

By studying intrastate branch banking reform in the United States, this paper provides evidence that financial markets substantively influence the distribution of income. From the 1970s through the 1990s, most states removed restrictions on intrastate branching, which intensified bank competition and improved efficiency. Exploiting the cross-state, cross-time variation in the timing of bank deregulation, we evaluate the impact of liberalizing intrastate branching restrictions on the distribution of income. We find that branch deregulation significantly reduced income inequality by boosting the incomes of lower income workers. The reduction in income inequality is fully accounted for by a reduction in earnings inequality among salaried workers.

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1. Introduction

This paper assesses how financial markets affect the distribution of income by studying intrastate branch banking reform 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). While researchers have examined the impact of these reforms on aggregate economic activity (e.g., Jayaratne and Strahan, 1996; Black and Strahan, 2002; Huang, 2008; and Kerr and Nanda, 2009), we provide the first evaluation of how branch deregulation altered the distribution of income. We test whether removing these restrictions intensified, ameliorated, or had no effect on income inequality and also study particular channels linking bank deregulation and income distribution.

Policy and theoretical debates motivate our analysis (Allen and Gale, 2000). Since Thomas Jefferson first opposed the creation of the Bank of the United States, U.S. policymakers have expressed concerns that big banks would primarily help the wealthy and widen the distribution of income (Hammond, 1957). If banking is a natural monopoly, then unregulated, monopolistic banks may earn rents through high fixed fees that disproportionately curtail the economic opportunities of the poor (Greenwood and Jovanovic, 1990; Banerjee and Newman, 1993; and Galor and Zeira, 1993). Based on this argument, politicians in many U.S. states implemented and maintained restrictions on bank branching for much of the 20th century (Southworth, 1928; White, 1982). Furthermore, most countries regulate bank mergers and acquisitions, with the advertised goals of constraining the expansion of powerful banks and expanding access to credit (Barth et al, 2006).

Countervailing arguments, however, challenge the view that regulations on bank expansion help the poor. These regulations could curtail competition and raise fees that disproportionately hurt the poor. Indeed, Flannery (1984) and Jayaratne and Strahan (1998) show that U.S. branching restrictions created and protected local banking monopolies, which allowed banks to maintain higher fees and interest rate margins. From this perspective, intrastate branch deregulation will operate on the extensive margin, disproportionately expanding economic opportunities for the poor.

The deregulation of intrastate branching provides a natural setting for identifying and assessing the impact of bank regulatory reform on the distribution of income. As shown by Kroszner and Strahan (1999), national technological innovations triggered branch deregulation at the state level. Specifically, (1) 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; (2) checkable money market mutual funds facilitated banking by mail and telephone, which weakened local bank monopolies; and, (3) improvements in communications technology lowered the costs of using distant banks. 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 preexisting 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 helps 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 helping lower income workers. 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 conditioning on 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. Seven years after deregulation, the Gini coefficient of income inequality is about four percent lower than before deregulation after conditioning on state and year fixed effects. Put differently, deregulation explains about 60% of the de-trended variation of inequality relative to state and year averages.

Removing restrictions on intrastate bank branching reduced inequality by boosting the incomes of the relatively poor, not by hurting higher income workers. Deregulation increased the average incomes of those in the bottom quarter of the income distribution by more than five percent, but deregulation did not significantly affect the incomes of those in 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 by affecting 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, 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, 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 of the labor market 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 datasets. 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-income workers. More specifically, deregulation dramatically increased the rate of new incorporations (Black and Strahan, 2002) and the rates of entry and exit of non-incorporated firms (Kerr and Nanda, 2009). However, we find that 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 10% of the working age population, and this percentage did not change significantly after 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 earnings of low-education workers relative to workers with more education.

This paper relates to several strands of research and to current policy debates. 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. Burgess and Pande (2007) 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 business income of the poor. Second, following the onset of the current financial crisis, many stress the potential dangers of financial deregulation. Though our work does not address the causes of the crisis, the results do indicate that regulations that impeded competition among banks during the 20th century were disproportionately harmful to lower income individuals. Thus, reforms to bank regulations could substantively affect the distribution of income. Finally, our work complements recent cross-country analyses of finance and the distribution of income. Beck, Demirguc-Kunt and Levine (2007) find that an overall index of banking sector development is associated with lower income inequality across countries. We improve on this work by analyzing the impact of a specific, exogenous policy change rather than a broad index of financial development and by using a differences-in-differences methodology rather than simple cross-country comparison that combine to yield sharper inferences about a policy change and reduce concerns about endogeneity bias.

This paper also relates to a substantive body of work on the effects of branch deregulation. Besides the investigations discussed above, researchers have examined the impact of branch deregulation on output volatility (Demyanyk, Ostergaard, and Sorensen, 2007; Acharya, Imbs and Sturgess, 2008), the wage rate gap between men and women bank executives (Black and Strahan, 2001), and the income growth of proprietors differentiated by race and gender (Demyanyk, 2007).

In this paper, we examine the impact of branch deregulation on the distribution of income in the overall economy and help resolve a debate about bank regulation that extends over two centuries.

The remainder of the paper proceeds as follows. Section 2 describes the data and econometric methodology. Section 3 provides the core results, while Section 4 provides further evidence on how deregulation influences labor market conditions. Section 5 concludes.

2. 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.

2.1. Branch deregulation

Historically, most U.S. states had restrictions on branching within and across state borders. With regards to intrastate branching restrictions, most states allowed bank hold 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. Appendix Table 1 presents 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 were 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 branch deregulation, we find that only intrastate deregulation enters significantly. Thus, we focus on intrastate rather than interstate deregulation throughout the remainder of this paper.

2.2. Income distribution data

Information on the distribution of income is 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 through 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.

We measure the distribution of income for each state and year over the period 1976-2006 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 frequently use the logarithm of the Gini coefficient in the regression analyses. 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 it into inequality accounted for by differences in income between groups in the sample and inequality accounted for by differences among those within each group of the sample. 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)). Appendix Table 2 provides more detailed information on the construction of these income distribution indicators.

Consistent with studies of the U.S. labor market, our main sample (a) includes prime-age (25-54) civilians that have non-negative personal income, (b) excludes individuals with missing observations on key variables (education, demographics, etc), (c) excludes the richest 1% of individuals, (d) excludes people living in group quarters, (e) excludes individuals who receive zero income and live in households with zero or negative income from all sources of income, and (f) excludes a few individuals for which the CPS assigns a zero (or missing) sampling weight. Appendix Table 3 provides details on the construction of the sample.

There are 1,859,411 individuals in our sample. Table 1A provides summary statistics on the sample of individuals, while Table 1B gives summary statistics on the income inequality measures. 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 Table 1B, we present basic descriptive statistics on the four 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. These standard deviations help in assessing the economic magnitude of the impact of bank branch deregulation on the distribution of the income that we report below.

2.3. Control variables

To control for time-varying changes in a state's economy, we use the 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 socio-demographic 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, each of which we measure in the year before deregulation. 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, Wisconsin, West Virginia, 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 twenty 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.

2.4. 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:

$$Y_{s,t} = A_s + B_t + \beta D_{s,t} + \delta X_{s,t} + \varepsilon_{s,t}, \qquad s = 1, \ldots, 49; \quad t = 1976, \ldots, 2006. \tag{1}$$

In equation (1), $Y_{s,t}$ is a measure of income distribution in state *s* in year *t*, A_s and B_t are vectors of state and year dummy variables that account for state and year fixed-effects, $X_{s,t}$ is a set of time-varying, state-level variables and $\varepsilon_{s,t}$ is the error term. The variable of interest is $D_{s,t}$, a dummy variable that equals one in the years after state s deregulates and equals 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, i.e. allowing for correlation in the error terms over time within states.

3. Branch deregulation and income distribution

3.1. 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 the rate of change of the Gini coefficient in the years before deregulation, the t-statistic on the inequality indicators are 0.20 and -1.16 respectively. Furthermore, in unreported robustness tests, we find that changes in the state-specific labor protection laws examined by Autor, Donohue, and Schwab (2006) do not predict the timing of branch deregulation.

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 2 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 (1999) dataset, our sample period ends in 1994, when there were three states that had not deregulated yet – Arkansas, Iowa and Minnesota.

Table 2 indicates that the timing of bank branch deregulation does not vary with the degree of income inequality. Column 1 reports the results of a regression with only the Gini coefficient of income inequality, while columns 2 – 5 provide regression results controlling for numerous state-level control variables, including those state characteristics employed by Kroszner and Strahan (1999). As in Kroszner and Strahan (1999), we find that states with a larger share of small banks and better capitalized small banks deregulate later, while states with a higher share of small firms deregulate earlier. As shown, the Gini coefficient does not enter significantly in any of the Table 2 regressions.

3.2. Deregulation and the distribution of income

In Table 3, we assess the impact of branch deregulation on income inequality using four 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 gross state product, the proportion of blacks in the population, the proportion of high-school dropouts in the population, the proportion of female-headed households in the population, and the unemployment rate.

The Table 3 results indicate that bank branch deregulation substantially reduced income inequality. The branch deregulation dummy enters negatively and significantly at the 5% level in all eight regressions. For example, consider the Gini coefficient. The column 1 results suggest that deregulation induced a 2.2% reduction in the Gini coefficient, which is economically large. To gauge the economic effect of this result, we compare the coefficient estimate to the standard deviation of the logarithm of the Gini coefficient after accounting for state and year fixed effects. This standard deviation is 0.037 as shown in Table 1, suggesting that branching deregulation explains about 60% of the variation of income inequality relative to state and year averages.

The Table 3 results indicate that deregulation tightened the distribution of income even when conditioning on time-varying state-level factors. Higher unemployment is associated with higher income inequality, though the other state characteristics do not enter independently significantly. This does not imply that these other characteristics, such as per capita economic growth, an economy's socio-demographic traits, or educational attainment, are unrelated to income inequality. Rather, it suggests that after conditioning on state and year fixed effects, unemployment, and branch deregulation, there is not a significant, independent link between each of these traits and the various measures of income inequality. Most importantly for the purposes of this paper, the results on deregulation are robust to conditioning on these factors.

Additional robustness tests confirm these findings. Controlling for the size of each state's aggregate economy, the level of real per capita income in each state, or lagged values of each state's Gini coefficient yields similar results. We were 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. In unreported regressions, we regress the share of immigrants per state-year on the branch deregulation dummy, while controlling for year and state-fixed effects. We did not find any significant effects of branch deregulation on migration flows. We also controlled for migration flows directly in the Table 3 regressions and obtain the same conclusions. Thus, interstate labor migration does not seem to be driving this paper's results. Finally, the results hold when examining household income, rather than individual income.

3.3. 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 one the logarithm of the Gini coefficient:

$$\text{Log (Gini)}_{st} = \alpha + \beta_1 D^{-10}_{st} + \beta_2 D^{-9}_{st} + \ldots + \beta_{25} D^{+15}_{st} + \mathbf{A}_s + \mathbf{B}_t + \varepsilon_{st,} \qquad (2)$$

where the deregulation dummy variables, the "D's," equal zero, except as follows: D^{-j} equals one for states in the j^{th} year before deregulation, while D^{+j} equals one for states in the j^{th} year after deregulation. We exclude the year of deregulation, thus estimating the dynamic effect of deregulation on income distribution relative to the year of deregulation. $\mathbf{A}_s$ and $\mathbf{B}_t$ are vectors of state and year dummy variables, respectively. At the end points, D^{-10}_{st} equals one for all year that are ten or more years before deregulation, while D^{+15}_{st} equals one for all years that are fifteen or more years after deregulation. Thus, there is much greater variance for these end points and the estimates may be measured with less precision. Figure 2 plots the results and the 95% confidence intervals, centering the estimates around year 0, the year of deregulation.

Figure 2 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 seven years after deregulation and then the effect levels off, indicating a steady-state drop in the Gini coefficient of inequality of about 3.5%. In sum, changes in inequality do not precede deregulation and deregulation has a level effect on inequality, but does not have a trend effect.

3.4. Deregulation and income for different income groups

Although the results demonstrate that income inequality fell after intrastate branch deregulation, the analyses do not yet provide information on whether the distribution of income tightened because the rich got poorer, or because deregulation disproportionately helped the poor. We now address this issue by examining the impact of branch deregulation on the incomes 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, $Y(i)_{s,t}$. We do this for i equal to 5, 10, 15, …, 90, 95. We then run 19 regressions of the form:

$$Y(i)_{st} = \alpha + \gamma D_{st} + \mathbf{A}_s + \mathbf{B}_t + \varepsilon_{st}, \tag{3}$$

where the regressions are run for each ith percentile of the income distribution. Figure 3 depicts the estimated coefficient, γ, from each of these 19 regressions and also indicates whether the estimates are significant at the 5% level.

Figure 3 shows that intrastate branch deregulation tightened the distribution of income by disproportionately helping relatively low income individuals. Deregulation boosted the incomes of those with incomes below the 40^{th} percentile. Deregulation did not have a significant impact on others. Rather than reducing the incomes of relatively high income individuals, deregulation reduced income inequality by increasing the incomes of the comparatively poor.

3.5. 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 provides greater confidence in the conclusions, sheds empirical light on the mechanisms through which deregulation influences the distribution of income, and also reduces concerns about reverse causality.

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. 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 will tend to benefit disproportionately from 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. Thus, we 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 will 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 had adopted unit banking before deregulation tended to have a higher share of small banks and firms and 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 will be largest.

The results in Table 4 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 4, branch deregulation reduced income inequality more in states that had (a) unit banking (column 1), (b) a more dispersed population (column 2), (c) a higher share of small banks (column 3), and (d) 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 non-unit banking states. In terms of population dispersion, the share of small banks, and the share of small firms, the results indicate that branch deregulation exerted an economically large and statistically significant impact on income inequality in those states with above the 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.

4. Labor market channels

4.1. Theories of how financial markets affect labor markets and 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 (i) branch deregulation improving the ability of the poor to access banking services directly and (ii) 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: (1) the poor have comparatively little collateral and (2) the fixed costs of borrowing are relatively high for the poor. From this perspective, branch deregulation that improves credit markets will 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 (1) substitute capital for labor and (2) 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.

4.2. Evidence on banks, inequality, and labor markets

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 within the self-employed and within 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. This decomposition is done 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 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 5, 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: (1) the self-employed account for only 9% of the sample, (2) the proportion self-employed individuals did not increase following branch deregulation, and (3) 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.

In Panel B of Table 5, 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 showed that all of the reduction in income inequality is accounted for by 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 fall in inequality among low-educated workers. From Panel B of Table 5, 73% (0.0074/0.0102) 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: (1) bank deregulation eased credit constraints and induced lower-income individuals to increase their investment in education, thereby reducing income inequality and (2) bank deregulation increased the demand for workers in the lower-education group, reducing between group inequality.

To help distinguish among possible explanations for these findings, Table 6 presents two additional analyses. First, we test whether bank deregulation lowers earnings inequality among workers of different ages. Besides examining workers between the ages of 25 and 54 as above, we also assess the 30-54 and 53-54 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 is reducing earnings inequality by increasing education we should obverse this primarily among the relatively young workers, not those who are older than 30 or 35. If we find the relation between deregulation and earnings inequality across the different age groups, this suggests that increased educational attainment is not the primary channel 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 have computed 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. Then, we 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 measure, this suggests that deregulation is not reducing earnings inequality only by its affect on educational attainment. In particular, we first regress log earnings on six dummy variables corresponding to the number of years of educational attainment (0-8, 9-11, 12, 13-15, 16, and more than 15 and year fixed effects. We then collect the residuals to calculate the conditional earnings inequality measures. In unreported robustness tests, we also condition on gender and ethnicity, and obtain the same results.

As shown in Table 6, 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 four earnings inequality indicators, we obtain very similar results when using different age samples of workers as reported in Panels A-C. The easing of credit constraints in 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 will 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 D, the estimated impact of deregulation on earnings inequality holds for unconditional and conditional earnings. These findings imply that deregulation is not reducing earnings inequality only through its affect on educational attainment. Rather, though needing additional research, the findings are more consistent with the view that bank branch deregulation reduced earnings inequality by boosting the relative demand for low-income workers.

5. Conclusions

Policymakers and economists disagree sharply about the impact of bank regulations on the distribution of income. 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 widen the distribution of income. 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 Claessens and Perotti (2007) and Haber and Perotti (2008).

We find that liberalizing restrictions on intrastate branching across the states of the U.S. tightened the distribution of income. 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. Critically, deregulation tightened the distribution of income by disproportionately helping the poor, not by hurting the rich. 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.

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Table 1A

SUMMARY STATISTICS ON INDIVIDUALS

	Mean	Min	Max
Age	38.4	25	54
Female	0.49	0	1
White, non-Hispanic	0.75	0	1
Black, non-Hispanic	0.11	0	1
Hispanic	0.09	0	1
High-school dropout	0.13	0	1
High-school grad	0.36	0	1
Some college education	0.24	0	1
College grad or advanced degree	0.27	0	1
Wage or salary earner	0.80	0	1
Entrepreneur	0.09	0	1
Total personal income ($2000)	32,369	0	385,961

NOTE – The table provides summary statistics for the sample of respondents to March Current Population Surveys in the years 1977-2007. The sample is subject to restrictions described in Appendix Table 3. The number of observations in the sample is 1,859,411. The mean values in the first column are weighted by CPS sampling weights. Total personal income is adjusted to constant 2000 dollars using the Consumer Price Index.

Table 1B

DESCRIPTIVE STATISTICS ON INCOME INEQUALITY

	N	Mean	Min	Max	Standard deviation of logs Cross-states	Within-states	Within state-years
Gini coefficient	1,519	0.431	0.334	0.532	0.045	0.047	0.037
Theil index	1,519	0.326	0.187	0.506	0.105	0.098	0.080
Log(90/10) ratio	1,519	2.772	1.653	10.797	0.636	0.380	0.329
Log(75/25) ratio	1,519	1.218	0.747	2.637	0.146	0.127	0.094

NOTE – The table provides descriptive statistics for the following measures of income inequality: [1] Gini coefficient, [2] Theil index, [3] log ratio of the 90^{th} and 10^{th} percentiles of the income distribution, and [4] log ration of the 75^{th} and 25^{th} percentiles of the income distribution. Each measure of inequality is based on total personal income of respondents to March Current Population Surveys described in Table 1A. We use sampling weights in all calculations of inequality measures. Inequality measures are discussed in more details in Appendix Table 2. The number of observations in the table corresponds to 49 states (we exclude Delaware and South Dakota) times 31 years between 1976 and 2006. For each measure of inequality we report the mean, the minimum and the maximum values, as well as the standard deviation of log of each measure (except log(90/10) and log(75/25) which are already in logs). We report three types of standard deviations: cross-state, within-state, and within state-year. These standard deviations are useful in calculating the economic magnitude of the impact of bank deregulation on income inequality.

Table 2

TIMING OF BANK DEREGULATION AND PRE-EXISTING INCOME INEQUALITY: THE DURATION MODEL

	(1)	(2)	(3)	(4)	(5)
Gini coefficient	.02	.02	.03	.03	.01
	(.03)	(.05)	(.02)	(.03)	(.03)
Growth rate of per capita GDP ($2000)		-1.50		.41	.41
		(1.41)		(1.15)	(1.05)
Proportion blacks		-1.28**		-1.08	-.82
		(.64)		(1.01)	(.95)
Proportion high-school dropouts		-.73		-3.14**	-3.16***
		(1.27)		(1.44)	(1.09)
Proportion female-headed households		-.64		2.40	2.62
		(2.41)		(1.84)	(1.87)
Unemployment rate		.02		.04	.03
		(.04)		(.04)	(.03)
Small bank asset share of all banking			6.62***	7.53***	8.17***
assets in the state			(2.36)	(2.06)	(1.70)
Capital ratio of small banks relative			12.00**	9.00**	10.00**
to large in the state			(5.14)	(3.69)	(4.20)
Relative size of insurance in states			3.76	1.99	.90
where banks may sell insurance, 0 otherwise			(2.38)	(2.44)	(2.08)
Indicator is 1 if banks may sell insurance			-2.12**	-1.06	-.50
in the state			(.99)	(1.02)	(.87)
Relative size of insurance in states where			-1.95***	-1.17**	-.52
banks may not sell insurance, 0 otherwise			(.62)	(.54)	(.54)
Small firm share of the number of firms			-11.27***	-12.50***	-16.28***
in the state			(3.20)	(3.31)	(4.23)
Share of state government controlled by			.25	.41**	.12
Democrats			(.20)	(.20)	(.17)
Indicator is 1 if state controlled by			.04	.06	.18
one party			(.12)	(.10)	(.16)
Average yield on bank loans in the state			-2.44	-5.35	-6.23
minus Fed funds rate			(6.65)	(6.33)	(4.57)
Indicator is 1 if state has unit			.29**	.24*	.23
banking law			(.13)	(.13)	(.14)
Indicator is 1 if state changes bank			.00	-.03	-.13
insurance powers			(.20)	(.15)	(.19)
Regional indicators	No	No	No	No	Yes
Observations	408	408	408	408	408

NOTE - 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. Sample period is 1976 to 1994 and the sample comprises 37 states that deregulated after 1977. States drop from the sample once they deregulate. Data on per capita Gross State Product are from the Bureau of Economic Analysis. Proportion blacks, high-school dropouts, and female-headed households are calculated from the March Supplements to the Current Population Surveys. Data on unemployment rate are obtained from the Bureau of Labor Statistics. All other control variables are taken from Kroszner and Strahan (1999). Standard errors are adjusted for state-level clustering and appear in parentheses. *, **, and *** indicate significance levels at 10%, 5%, and 1%, respectively.

Table 3

THE IMPACT OF DEREGULATION ON INCOME INEQUALITY

	Gini Coefficient (1)	Theil Index (2)	(90/10) Ratio (3)	(75/25) Ratio (4)
PANEL A: NO CONTROLS				
Bank deregulation	-0.022***	-0.041**	-0.135**	-0.077***
	(0.008)	(0.016)	(0.058)	(0.020)
R^2	0.35	0.43	0.74	0.60
Observations	1,519	1,519	1,519	1,519
PANEL B: WITH CONTROLS				
Bank deregulation	-0.018***	-0.032**	-0.101**	-0.066***
	(0.006)	(0.014)	(0.050)	(0.017)
Growth rate of per capita GDP ($2000)	-0.028	-0.050	-0.140	-0.114
	(0.041)	(0.081)	(0.229)	(0.119)
Proportion blacks	-0.218	-0.462	-0.826	-0.231
	(0.154)	(0.320)	(1.451)	(0.473)
Proportion high-school dropouts	0.140*	0.219	0.432	-0.072
	(0.071)	(0.147)	(0.635)	(0.155)
Proportion female-headed households	0.017	0.028	0.226	0.102
	(0.058)	(0.125)	(0.501)	(0.153)
Unemployment rate	0.006***	0.013***	0.069***	0.023***
	(0.001)	(0.003)	(0.014)	(0.003)
R^2	0.39	0.46	0.75	0.63
Observations	1,519	1,519	1,519	1,519

NOTE - The table shows the impact of bank deregulation on the natural logarithm of the different measures of income inequality. 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 models control for state and year fixed effects. Standard errors are clustered at the state-level and appear in parentheses. *, **, and *** indicate significance levels at 10%, 5%, and 1%, respectively.

Table 4

THE IMPACT OF DEREGULATION ON LOG GINI COEFFICIENT OF INCOME INEQUALITY AS A FUNCTION OF INITIAL STATE CHARACTERISTICS

	(1)	(2)	(3)	(4)
Bank deregulation	-.012	-.014*	.019	.577**
	(.008)	(.008)	(.013)	(.253)
Deregulation x (unit banking)	-.020*			
	(.010)			
Deregulation x (initial population dispersion)		-.180**		
		(.079)		
Deregulation x (initial share of small banks)			-.326***	
			(.111)	
Deregulation x (initial share of small firms)				-.674**
				(.287)
Linear combination	-.032***			
	(.010)			
Evaluated at the 25th percentile		-.016**	-.003	-.009
		(.007)	(.008)	(.008)
Evaluated at the 50th percentile		-.017**	-.014*	-.016**
		(.007)	(.007)	(.007)
Evaluated at the 75th percentile		-.021***	-.027***	-.026***
		(.007)	(.008)	(.008)
Observations	1,519	1,519	1,209	1,209

NOTE – The table presents estimates of the impact of bank deregulation on income inequality as a function of initial state characteristics. The dependent variable is the natural logarithm of the Gini coefficient. 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. Data on population dispersion are from the Census Bureau. Data on the share of small banks and small firms is obtained from Kroszner and Strahan (1999). These data exclude 10 states that deregulated in 1960 and therefore have 310 fewer state-year observations. Standard errors are adjusted for state-level clustering and appear in parentheses. *, **, and *** indicate significance levels at 10%, 5%, and 1%, respectively.

Table 5

DECOMPOSING THE IMPACT OF DEREGULATION ON INCOME INEQUALITY TO BETWEEN- AND WITHIN-GROUPS

PANEL A: ALL WORKERS	Total	Between Groups	Within Groups	Employment Groups: Self Employed	Salaried
Bank deregulation	-.0103**	.0002	-.0105**	-.0077	-.0102**
	(.0043)	(.0003)	(.0042)	(.0074)	(.0042)

PANEL B: SALARIED WORKERS	Total	Between Groups	Within Groups	Education Groups: High School or Less	Some College or More
Bank deregulation	-.0102**	-.0028**	-.0074**	-.0086*	-.0039
	(.0042)	(.0011)	(.0035)	(.0043)	(.0038)

NOTE - The table reports the impact of intrastate deregulation on the Theil index of income inequality. 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. In panel A we divide the sample into two mutually exclusive groups: (a) those who are self-employed, and (b) those who work for wages. In panel B we divide the sample of *wage workers* into two mutually exclusive groups: (a) those with twelve or less years of completed education, and (b) those with thirteen or more years of completed education. In the first column in both panels we estimate the overall impact of intrastate 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, whereas 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 significance levels at 10%, 5%, and 1%, respectively.

Table 6

THE IMPACT OF DEREGULATION ON EARNINGS INEQUALITY

	Gini Coefficient (1)	Theil Index (2)	(90/10) Ratio (3)	(75/25) Ratio (4)
PANEL A: AGES 25-54, UNCONDITIONAL EARNINGS				
Bank deregulation	-0.022**	-0.042**	-0.094***	-0.051***
	(0.008)	(0.017)	(0.030)	(0.017)
R^2	0.13	0.26	0.47	0.33
Observations	1,519	1,519	1,519	1,519
PANEL B: AGES 30-54, UNCONDITIONAL EARNINGS				
Bank deregulation	-0.023***	-0.045**	-0.111***	-0.058***
	(0.009)	(0.018)	(0.033)	(0.017)
R^2	0.11	0.24	0.46	0.35
Observations	1,519	1,519	1,519	1,519
PANEL C: AGES 35-54, UNCONDITIONAL EARNINGS				
Bank deregulation	-0.019**	-0.036**	-0.072**	-0.055***
	(0.008)	(0.018)	(0.031)	(0.016)
R^2	0.11	0.22	0.42	0.33
Observations	1,519	1,519	1,519	1,519
PANEL D: AGES 25-54, EARNINGS CONDITIONAL ON EDUCATION				
Bank deregulation	-0.037***	-0.073***	-0.091***	-0.038***
	(0.012)	(0.027)	(0.029)	(0.013)
R^2	0.58	0.51	0.59	0.50
Observations	1,519	1,519	1,519	1,519

NOTE – The table shows the impact of bank deregulation on the natural logarithm of different measures of earnings inequality. Standard errors are adjusted for state level clustering and appear in parentheses. All specifications control for state and year fixed effects and do not include other control variables. In panels A-C, the inequality measures are based on real annual earnings as reported by CPS respondents. Panels A-C differ only in the ages of the respondents. In panel D, in contrast, we first regress log real annual earnings on six educational categories corresponding to years of completed education (0-8, 9-11, 12, 13-15, 16, and 16+) and then calculate measures of inequality based on the residuals.

Appendix Table 1

TIMING OF INTRASTATE BANK DEREGULATION

State	Postal code	Year of deregulation	State	Postal code	Year of deregulation
Alabama	AL	1981	Montana	MT	1990
Alaska	AK	1960	Nebraska	NE	1985
Arizona	AZ	1960	Nevada	NV	1960
Arkansas	AR	1994	New Hampshire	NH	1987
California	CA	1960	New Jersey	NJ	1977
Colorado	CO	1991	New Mexico	NM	1991
Connecticut	CT	1980	New York	NY	1976
Delaware	DE	1960	North Carolina	NC	1960
District of Columbia	DC	1960	North Dakota	ND	1987
Florida	FL	1988	Ohio	OH	1979
Georgia	GA	1983	Oklahoma	OK	1988
Hawaii	HI	1986	Oregon	OR	1985
Idaho	ID	1960	Pennsylvania	PA	1982
Illinois	IL	1988	Rhode Island	RI	1960
Indiana	IN	1989	South Carolina	SC	1960
Iowa	IA	1999	South Dakota	SD	1960
Kansas	KS	1987	Tennessee	TN	1985
Kentucky	KY	1990	Texas	TX	1988
Louisiana	LA	1988	Utah	UT	1981
Maine	ME	1975	Vermont	VT	1970
Maryland	MD	1960	Virginia	VA	1978
Massachusetts	MA	1984	Washington	WA	1985
Michigan	MI	1987	West Virginia	WV	1987
Minnesota	MN	1993	Wisconsin	WI	1990
Mississippi	MS	1986	Wyoming	WY	1988
Missouri	MO	1990			

NOTE – The table shows the year of branch deregulation for each state. Source: Kroszner and Strahan (1999).

Appendix Table 2

DIFFERENT MEASURES OF INCOME INEQUALITY

Measure	Mathematical Expression	Interpretation	Advantages	Disadvantages
Gini coefficient	$1 - 2\int L(x)dx$, where L() is the Lorenz curve showing the relation between the percentage of income recipients and the percentage of income they earn.	The Gini coefficient is equal to 0 in the case of perfect equality when exactly *s* percent of total income is held by bottom *s* individuals (*s*=1,...,100). The Gini coefficient is equal to 1 if all the income is held by one individual.	[1] Very intuitive and widely used. [2] Makes use of all information about the distribution.	[1] Sensitive to changes in the middle of the distribution. [2] Not easily decomposable to between- and within-group inequality.
Theil index	$n^{-1}\sum_i\{(y_i/\mu)\ln(y_i/\mu)\}$, where i indexes individuals (i=1,…,n), y is personal income, and μ is the mean value of y. The first term inside the sum is individual's share of total income and the second term is that individual's income relative to the mean.	If all individuals have the same (i.e., mean) income, then the Theil index is 0. If one individual has all the income, then the index is ln(n).	Easily decomposable to between- and within-group inequality.	Hard to interpret.
Log(75/25)	$\ln(y_{75}) - \ln(y_{25})$, where y_{75} and y_{25} are the 75th and the 25th percentiles of personal income distribution (y), respectively.	The ratio is equal to 0 if the 75th and the 25th percentiles of the distribution are equal. There is no upper bound to the ratio.	[1] Intuitive measure of the percentage difference between the third and the first quartiles of a distribution. [2] Robust to extreme values.	Does not measure the entire distribution.
Log(90/10)	$\ln(y_{90}) - \ln(y_{10})$, where y_{75} and y_{25} are the 90th and the 10th percentiles of personal income distribution (y), respectively.	The ratio is equal to 0 if the 90th and the 10th percentiles of the distribution are equal. There is no upper bound to the ratio.	[1] Intuitive measure of the percentage difference between the top and the bottom deciles of a distribution. [2] Robust to extreme values.	Does not measure the entire distribution.

Appendix Table 3

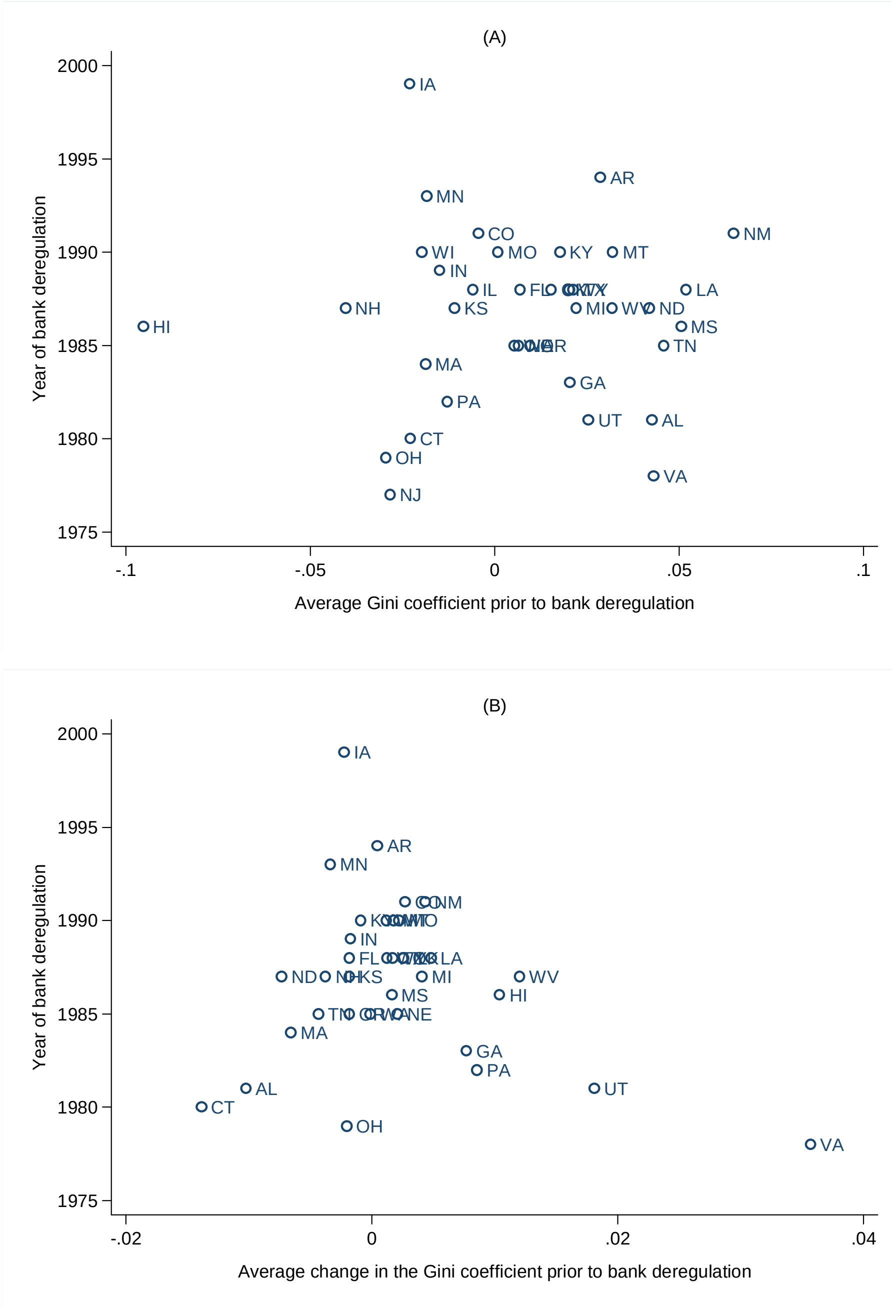
SAMPLE CONSTRUCTION

Total number of observations in the March Current Population Surveys in the years 1977-2007:	5,085,135
Sample restrictions (observations deleted):	
1. Persons between the ages of 25 and 54 with non-negative personal income below the 99th percentile	(3,154,652)
2. Non-missing years of completed education and demographic characteristics	(21,786)
3. Not residing in group quarters	(2,142)
4. Not residing in Delaware or South Dakota	(45,780)
5. With positive total household income	(1,276)
6. Positive and non-missing sampling weights	(88)
Total number of observations that satisfy sample restrictions above:	1,859,411

NOTE – March Current Population Surveys (CPS) are available at <http://cps.ipums.org/cps/>. We start with the 1977 survey because exact state of residence is not available prior to 1977. We follow the literature and exclude Delaware and South Dakota because of large concentration of credit card banks in these states. From 1977 to 1982, group quarters included housing units containing five or more people unrelated to the person in charge. As of 1983, group quarters were defined in the CPS as non-institutional living arrangements for groups not living in conventional housing units or groups living in housing units containing ten or more unrelated people or nine or more people unrelated to the person in charge. Because we use sampling weights to construct measures of income inequality, we exclude persons with missing or zero sampling weights.

Figure 1

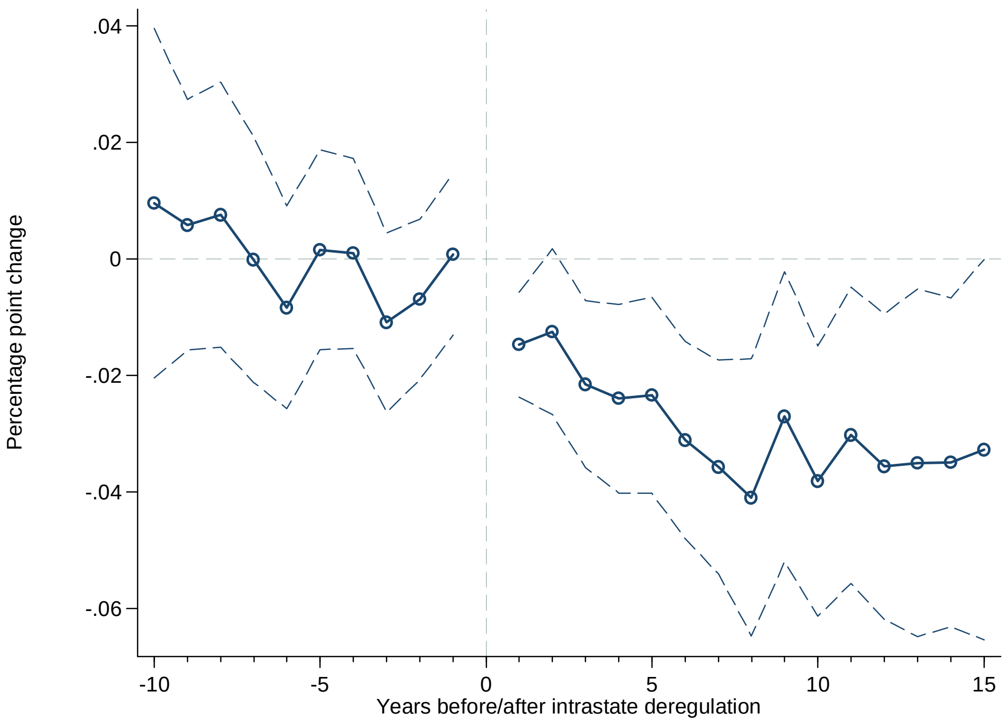
TIMING OF BANK DEREGULATION AND PRE-EXISTING INCOME INEQUALITY: GRAPHICAL ANALYSIS



NOTE – 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. The t-statistics for the correlations in figures (A) and (B) are 0.20 and -1.16, respectively.

Figure 2

THE DYNAMIC IMPACT OF DEREGULATION ON LOG GINI COEFFICIENT



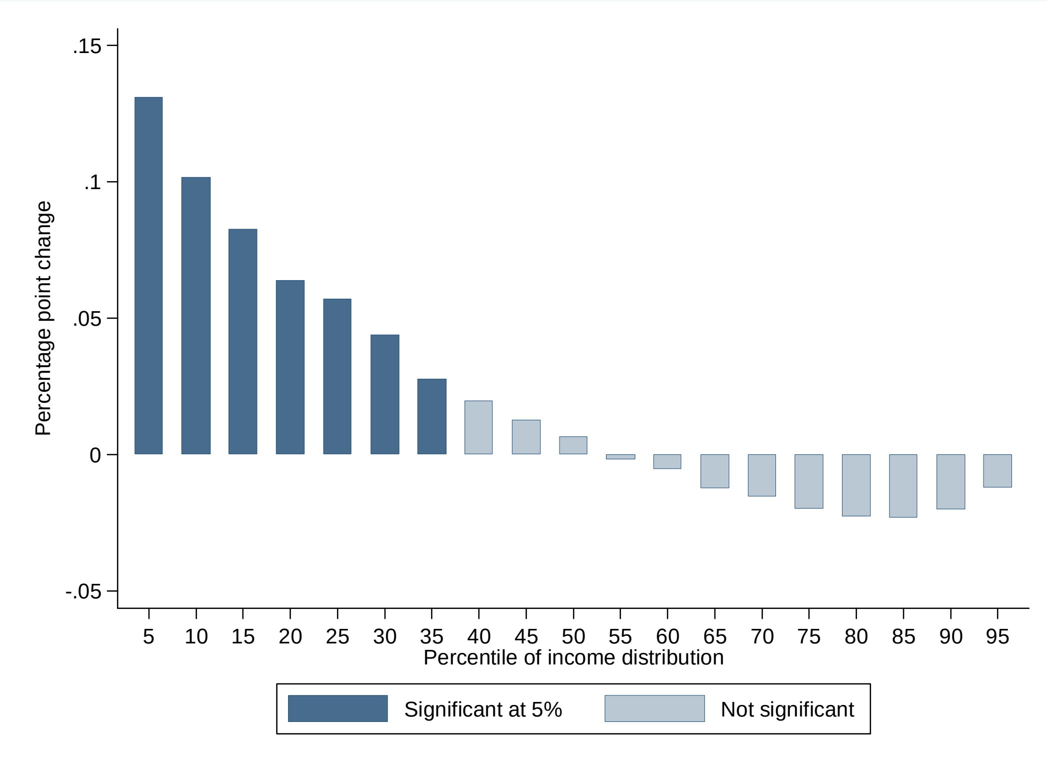
NOTE – The figure plots the impact of intrastate bank deregulation on the natural logarithm of the Gini coefficient of income inequality. We consider a 25 year window, spanning from 10 years before deregulation until 15 years after deregulation. The dashed lines represent 95% confidence intervals, adjusted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

$$\log(\text{Gini})_{st} = \alpha + \beta_1 D^{-10}_{st} + \beta_2 D^{-9}_{st} + \ldots + \beta_{25} D^{+15}_{st} + \mathbf{A}_s + \mathbf{B}_t + \varepsilon_{st}$$

The D's equal zero, except as follows: D^{-j} equals one for states in the j^{th} year before deregulation, while D^{+j} equals one for states in the j^{th} 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. $\mathbf{A}_s$ and $\mathbf{B}_t$ are vectors of state and year dummy variables that account for state and year fixed effects, respectively.

Figure 3

THE IMPACT OF DEREGULATION ON DIFFERENT PERCENTILES OF INCOME DISTRIBUTION



NOTE – Each bar in the figure represents the estimated impact of bank deregulation on a natural logarithm of a specific percentile of income distribution. Dark bars represent estimates significant at 5% 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:

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

where $Y(i)_{st}$ is the natural logarithm of *i*th percentile of income distribution in state *s* and year *t*. D_{st} is a dummy variable which equals to zero prior to bank deregulation and equals to one afterwards. $\mathbf{A}_s$ and $\mathbf{B}_t$ 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.