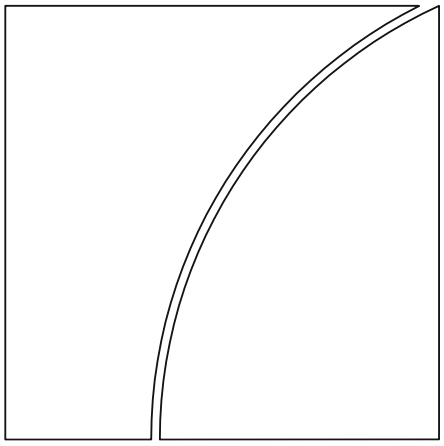

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The financial origins of regional inequality

by Anne Beck and Sebastian Doerr

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JEL classification: G21, R10

Keywords: banking deregulation, credit supply, income inequality, regional inequality

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The financial origins of regional inequality

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Abstract

An increasing number of policies addresses spatial inequality, which is believed to lie at the heart of economic and social cleavages, including entrenched poverty, deaths of despair, and political polarization. Yet little is known about the origins of the gap between prospering urban and "left-behind" rural areas that has emerged since the 1980s. We provide new evidence on the role of banking deregulation in explaining this rural-urban divergence in incomes. In particular, we show that the income gap widened following the removal of geographic restrictions on banking. While deregulation promoted an overall increase in incomes, the increase was significantly larger in urban counties. We show that this is due to increased competition in the banking industry in cities post deregulation. Competition benefited financially constrained small and young firms, thereby boosting employment and incomes in urban areas. Our findings inform the debate on regional inequality and the design of place-based policies.

JEL classification: G21, R10.

Keywords: Banking deregulation, credit supply, income inequality, regional inequality.

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1 Introduction

Over the last decades, spatial inequality in the US has steadily increased as income growth in urban areas has outpaced growth in rural areas (Gaubert et al., 2021). This geographic divergence is believed to have brought economic and social cleavages, including entrenched poverty, deaths of despair, and political polarization (Hendrickson et al., 2018).[1] In response, policymakers are introducing a growing number of place-based policies to foster investment in economically disadvantaged areas, oftentimes by offering firms tax incentives (Shambaugh and Nunn, 2018; Garrett et al., 2020; LaPoint and Sakabe, 2021). This paper provides novel evidence on the importance of the financial sector in shaping spatial inequality in the US. Specifically, we highlight the crucial role that banking deregulation has played in spurring differential income growth in urban vs. rural regions.

Figure 1 illustrates the motivation behind our analysis. It shows the widening dispersion between urban (blue solid line) and rural (black dashed line) incomes: while incomes increased at a similar pace until the early 1980s in urban and rural counties, a gap opened up thereafter. By 2005, the increase in incomes relative to 1980 was around 30 percentage points (pps) higher in urban areas. In the same plot, the grey shaded bars show the cumulative share of counties in states which opened their banking system for interstate banking from a given year onward. The share is zero at the beginning of the sample, as the Douglas Amendment to the Bank Holding Company Act of 1956 effectively prohibited banks to open branches or to acquire banks outside of their headquarters' state, creating a highly geographically fragmented US banking system. The first state lifted the restrictions in 1978. From the early 1980s the share of counties in deregulated states saw a sharp rise – after which urban and rural incomes increasingly diverged. This pattern suggests that deregulation may have triggered regional divergence in incomes, a hypothesis which has not been previously analyzed in the literature.

To empirically investigate the effects of financial deregulation on incomes in urban and rural areas, we exploit the staggered deregulation of the US banking system from the 1970s throughout the 1990s. Our main, novel, finding is that financial deregulation – specifically the geographic integration of banking markets – led to a divergence in income

[1] Regional inequalities also feature prominently in the popular press, see e.g. "One County Thrives. The Next One Over Struggles. Economists Take Note" (The New York Times), "Tackling regional inequality in the rich world" (The Economist), or "The Divide Between America's Prosperous Cities and Struggling Small Towns—in 20 Charts" (Wall Street Journal).

growth across urban and rural regions within a county. While deregulation contributed to higher income growth in general (Jayaratne and Strahan, 1996), it did so significantly more in urban as compared to rural counties. In terms of magnitude, our results suggest that deregulation induced a 0.2 to 0.3 percentage points income growth premium in urban counties (about 10% of the mean). These growth differences ultimately result in a 15% higher income in urban areas.

Our analysis is based on a heterogeneity robust difference-in-differences strategy. We compare income trajectories of rural and urban counties within a cohort of states that deregulated at the same time, before and after the deregulation. This allows us to estimate the differential effect of the deregulation in urban relative to rural areas while accounting for any time constant county characteristics, as well as common time trends. Furthermore, our results are not prone to bias arising from non-constant treatment effects across units or over time – a common issue in difference-in-difference settings with staggered treatment onsets (De Chaisemartin and d'Haultfoeuille, 2022).

While states' timing to deregulate was independent of local bank failure rates, their current business cycle conditions, or potential anticipation of future growth (Strahan, 2003; Kroszner and Strahan, 2014), identifying the causal effects of deregulation on differential income growth in rural and urban areas requires a state's choice to deregulate to be exogenous to previous trends in the development of the urban-rural income gap. In support of our identifying assumption, we show that incomes in urban and rural areas followed a similar trend prior to deregulation. Moreover, we find no discernible relationship between pre-deregulation differences in rural vs. urban income growth within a state and the timing of a state's decision to open its banking markets.

Why would banking deregulation affect income inequality? Previous literature has shown that higher competition in the banking sector can spur economic activity by reducing the cost of credit for firms and households (Jayaratne and Strahan, 1998; Black and Strahan, 2002). Furthermore, the physical proximity to banks, and especially their headquarters (Liberti and Mian, 2008), is still an important factor in facilitating access to credit for small and medium enterprises that rely on soft-information lending (Brevoort and Wolken, 2008; Nguyen, 2019; Kärnä et al., 2020). The local banking market structure hence impacts local economic activity.

Based on these insights, we analyze the differential effect of banking deregulation on the access to banking services and competition in the local banking market, and

investigate whether deregulation benefited financially constrained firms more in urban areas. In line with previous literature, we find evidence for bank deregulation spurring bank activity and competition, but not everywhere at equal rates. Both the number of bank branches and the number of bank headquarters increased more in urban than in rural areas. Moreover, deregulation led to a larger decline in the deposit share of the top-three banks in urban relative to rural counties. Mergers and acquisitions – which were key to the restructuring of the banking industry and promoting local competitive dynamics (Stiroh and Strahan, 2003; Kroszner and Strahan, 2014) – more often included banks in urban than in rural areas. Consistent with the arguments that fiercer competition in the financial sector improves firms' access to finance, we find that smaller firms, younger firms, and firms in industries that depend more on external finance experienced a relative increase in their employment share in urban areas following deregulation.

We complement our findings by studying the potential differential effect of the deregulation on local labor market outcomes in cities and rural areas. Our results suggest that the differential income growth was accompanied by a larger growth in average wages per worker, as well as a more pronounced rise in employment rates in urban when compared to rural areas.

We perform a host of robustness checks. We find near-identical results when we include state*year fixed effects, which control for time-varying observable and unobservable factors that affect all counties within the same state. A large residual effect of the deregulation remains when controlling for alternative drivers of the urban-rural growth gap stemming from differences in a county's initial industry structure. Our results also do not hinge on how we classify urban and rural counties, and remain similar when excluding individual states or deregulation cohorts from the sample.

All in all, our results suggest that financial deregulation has spurred competition in local banking markets mostly in urban areas. Access to finance for firms subsequently improved, which fostered income growth in cities. In consequence, incomes between rural and urban counties increasingly diverged, so that financial deregulation has fuelled regional inequality.

We thereby shed new light on a long-standing debate. Already in the late 18^{th} century, Thomas Jefferson and Andrew Jackson worried about financial deregulation concentrating economic powers in larger cities – at the expense of credit supply to rural America (Baradaran, 2015). Today, an increasing number of place-based policies, often focusing

on tax breaks (Garrett et al., 2020; Freedman et al., 2021; LaPoint and Sakabe, 2021), tries to foster economic development in poorer areas. Our findings help to inform the debates on regional inequality and the design of place-based policies by highlighting the crucial role of credit supply in shaping the geography of economic activity.

Related literature. Our paper contributes to the literature that investigates the effects of financial liberalization and income inequality (Claessens and Perotti, 2007). Predictions from theory can point in both directions: Improved financial development can promote financial inclusion on the extensive margin, thus decreasing income inequality. At the same time, it can enhance financial services for current market participants which has the potential to reinforce existing inequalities (Greenwood and Jovanovic, 1990). Moreover, induced changes to the relative credit allocation across sectors can indirectly affect inequality depending on their impact on the demand for low- relative to high-skilled workers (Demirgüç-Kunt and Levine, 2009). Consequently, empirical studies in the cross-country setting provide mixed evidence and highlight the dependence on the level of financial development (Demirgüç-Kunt and Levine, 2009; De Haan and Sturm, 2017). Beck et al. (2007); Agnello et al. (2012); Delis et al. (2014) show that financial deregulation reduces income inequality, while Jaumotte and Osorio-Buitron (2015) and Jauch and Watzka (2016) find evidence supportive for the opposite.

Beyond cross-country studies, single-country studies suggest that financial deregulation disproportionately benefited lower income groups. For the US, Beck et al. (2010) show that the removal of restrictions on intrastate bank branching has reduced within-state inequality, as it has boosted incomes in the lower part of the income distribution. Célerier and Matray (2019) provide complementary evidence and show that banking deregulation has improved access to finance for low-income households. For India, Burgess and Pande (2005) show that the opening of rural bank branches boosted wages of low-income rural workers. Rather than looking at the effects of banking (de)regulation on inequality along the household income distribution, we add to the literature by studying its implications for spatial inequality.

More broadly, we relate to the discussion on the causes and implications of regional inequality, and policies to mitigate them. Moretti (2012), Gaubert et al. (2021), and Eckert et al. (2022) point out a divergence in wages and incomes across US regions from the 1980s onward – which coincides with the start of banking deregulation. Eckert et al. (2022) point to initial differences in county industry structures and resulting

complementarities with information and communication technology as potential reasons for urban-biased wage growth. In response to the widening regional inequality, policy makers are introducing place-based policies (Garrett et al., 2020; Freedman et al., 2021; LaPoint and Sakabe, 2021). Our paper shows that, while banking deregulation accelerated overall income growth, it did so to a significantly greater extent in urban regions, also when accounting for differential trends based on counties' initial industry structure. The deregulation of the financial sector has thus contributed to an increasing divergence between urban and rural counties, suggesting that Jefferson's concerns were at least partly warranted.

The rest of the paper is structured as follows. Section 2 provides an overview of banking deregulation in the US and describes our data sources and variable constructions. Section 3 details our empirical strategy and presents our main results. It first establishes that income growth post-deregulation was higher in urban counties and then provides evidence on the mechanisms. Section 4 provides robustness tests and Section 5 concludes.

2 Setting and data

This section provides a brief overview of the deregulation of the US banking sector and discusses the data sources and construction of main variables. It then provides summary statistics.

Banking deregulation. To investigate the effects of banking deregulation on rural and urban areas, we exploit the staggered deregulation of states' banking markets as a quasi-natural experimental. Until the 1970s, the Douglas amendment to the Bank Holding Company Act of 1956 effectively banned interstate banking by prohibiting banks to open branches or to acquire banks outside of their headquarters' state. Receiving fees for granting bank charters, many states had an interest to restrict inter- and intrastate branching (Kroszner and Strahan, 1999). In consequence, the US banking system was segregated into fragmented local banking systems. From 1978 onward, more and more states liberalized entry regulations for out-of-state banks. In 1994, the Reigle-Neal Interstate Banking and Branching Efficiency Act stipulated complete interstate banking and branching. Similar to the restrictions on interstate banking, 38 of the 39 states that

initially imposed restrictions on intrastate branching eased these between 1970 and 1994.[2]

Kroszner and Strahan (2014) provide a detailed overview of the underlying sources of the staggered timing of US banking deregulation. In general, technological advances in lending and deposit taking led to a push for deregulation, with a combination of political and technological factors determining the pace. States' timing to deregulate was, however, independent of local bank failure rates, their current business cycle conditions, or potential anticipation of future growth (Kroszner and Strahan, 2014). As we discuss below, it was also uncorrelated with the pre-deregulation income growth differences between urban and rural counties within a state.

The removal of geographical restrictions has led to a substantial change in the structure of the banking industry. Consolidation and expansionary activities across state borders substantially reduced the number of banks, primarily eliminating smaller institutions. Yet the growing market share of large banks covering wider geographic areas did not foster local market concentration on average (Strahan, 2003; Kerr and Nanda, 2009). As a results, the efficiency of the financial sector increased: average operating costs and loan losses fell as better performing banks took over market shares of initially protected but less efficient competitors (Jayaratne and Strahan, 1998; Stiroh and Strahan, 2003). Bank customers benefited through lower loan rates and enhanced financial inclusion (Célerier and Matray, 2019).

We follow Morgan et al. (2004) and define the year of banking deregulation as the year in which a state entered into an interstate banking agreement.[3] In line with the literature, we exclude Delaware and South Dakota, as the development of the banking sector in these states was dominated by the growth of the credit card industry. We then define the dummy *deregulation* that takes on a value of one after a state deregulated interstate banking operations, and zero in the years prior.[4] Figure A1 depicts the number

[2]By 1994, all states but Iowa allowed for intrastate branching via merger and acquisition. A total of 39 states permitted completely unrestricted intrastate branching which gave banks the right to open new branches in new markets rather than expanding into markets by buying existing banks or branches.

[3]In section 4, we discuss alternative strategies to define the deregulation date. We find similar, yet weaker effects when defining deregulation based on the year of intrastate deregulation. The effect of intrastate branching deregulation becomes statistically insignificant when jointly testing for the effect of deregulation across and within state borders. This is in line with a major geographic restructuring of the banking industry and an uptick in large bank mergers and acquisitions being primarily observed after the interstate banking deregulation (Stiroh and Strahan, 2003).

[4]We set the year of deregulation for Hawaii equal to 1994 when the Reigle-Neal Interstate Banking and Branching Efficiency Act enacted full interstate banking and branching. Results are almost unaltered when keeping Hawaii as "untreated" (unreported).

of counties in deregulated states over time by their degree of urbanity and lists the states by deregulation date. While deregulation started in the late 1970s, the majority of counties were subject to a change in regulation between 1983 and 1989.

County-level variables. We collect data on personal income at the county level from the Bureau of Economic Analysis (BEA) regional accounts for 1970 to 2006. Income growth is defined as the log difference in income between two periods.[5] We use a crosswalk to accommodate changes in FIPS county codes over time and build a balanced sample of counties. Information on FIPS code changes is obtained from the U.S. Census Bureau (2021e).

To identify urban counties, we rely on the rural-urban continuum codes provided by the US Department of Agriculture Economic Research Service (USDA). Table A1 provides a detailed description. County's assignments are revised once per decade. We use the 1974 pre-deregulation codes, to avoid classifying counties based on a potential endogenous response of their degree of urbanization to the deregulation. Our baseline measure defines urban counties as counties in metro areas or urbanized non-metro counties with an urban population above 20,000 (urban score equal to or above 0.5). In Section 4 we discuss alternative cut-off values or measures to define urban and rural counties and show the robustness of our results. Using data from the US Census and the BEA regional accounts, we additionally construct the following control variables at the county-year level: the log of the total population, the population share of people of age 65 and above and the share of blacks (U.S. Census Bureau, 2021a,b,c,d). We compute population growth as the log difference of the total population.

In its Summary of Deposits Statistics (SOD), the Federal Deposit Insurance Corporation (FDIC) provides information on banks' branch locations, branch type and deposits as of June 30th in each year (FDIC, 2020, 2021). Data is available from 1981 onward.[6] We calculate the log of the number of branches per capita and the number of bank headquarters per 10,000 inhabitants for each county-year cell. As a measure of local competition

[5]We winsorize county income growth at the 1st and 99th percentile to ensure that our results are not driven by outliers. We show robustness towards using trimmed or non-winsorized income growth in Table A7.

[6]For 1981-1986 we use data prepared by Bouwman which precedes the data available for download from the FDIC web page. For 1987-1993 the data is available from a bulk download that is made accessible under the Freedom of Information Act (FDIC, 2020). We clean the raw data, correcting for instance erroneous FIPS code information by comparing the data entries to the digitized hard copies of the Banks & Branches Databook – Summary of Accounts and Deposits – Operating Banks and Branches.

in the banking sector, we calculate the deposit share of the three largest banks out of total county deposits.

We use two different data sources to construct county employment shares at financially constraint firms for our three approaches to capture financial constraints (age, size, and industry external finance dependency). Information on employment at startups (formed in the year of observation) and older firms in a county is derived from the US Census Bureau's Business Dynamics Statistics (BDS) tabulations by firm age (U.S. Census Bureau, 2018). The BDS tabulations by initial firm size are used to obtain county-level data on employment at small (less than 20 employees) and large firms (20 or more employees).[7] Since the share of suppressed industry-county cells for the BDS tabulations by industry is high, we turn to imputed County Business Patterns data from Eckert et al. (2021) to compute county employment by NAICS 2-digit industry. For each industry we add a proxy capturing its external finance dependency which is derived from US Compustat data (WRDS, 2021).[8]

For additional regressions, we obtain data on wages and salaried employment in a county from the BEA regional economic accounts. We compute counties' log change in the total wage bill, growth in average wage per worker, as well as the employment share (defined as employment over working age population). Information on the working age population is obtained from the U.S. Census Bureau (2021a,b,c,d).

Bank-level variables. We augment our bank data with information on mergers and acquisitions from the Federal Reserve Bank of Chicago. For each merger, the data contains identifiers for acquiring and acquired banks, as well as the date of the merger. We match this information to our county-bank-year level data set constructed from the FDIC's SOD. Specifically, we calculate the share of county deposits that is held at banks targeted for acquisition and the share held at merging banks which were either an acquirer or an acquisition target in a given year. Furthermore, to measure bank profitability, we derive information on annual average quarterly return on equity from banks' Call Reports, made available by Drechsler et al. (2017).

[7]The data starts in 1978. Counties with suppressed employment cells are excluded, leading to a marginally smaller sample which is not fully balanced.

[8]Following Rajan and Zingales (1998) and Cetorelli and Strahan (2006), financial dependence of a NAICS 2-digit industry is approximated with its listed firms' ability to cover capital expenditures with cash flow. Using large, listed firms with access to capital markets, their use of external financing is likely to reflect demand considerations, rather than supply side constraints.

Descriptive statistics Table 1 provides descriptive statistics for our main variables. Our final sample contains 3,027 counties in 49 states for the time period from 1971 to 2006. Personal income grew at 2.9% per year, on average, with a standard deviation of 6.8pp. About 30% of counties in our sample have an urban score greater or equal to 0.5 and are thus classified as urban. The average county in our sample has 4 bank branches and 1.5 bank headquarters per 10,000 inhabitants, with a standard deviation of 2.4 and 2, respectively. Counties' deposit markets tend to be concentrated, with 83% of deposits being held at the three largest banks on average.

3 Bank deregulation and rural-urban inequality

This section lays out the empirical strategy and reports the main results. First, we establish that while banking deregulation has increased incomes in all counties, it did so predominately in urban areas. In a second step, we investigate the underlying mechanisms.

3.1 Empirical strategy and identification

To analyze how deregulation has affected income growth in urban and rural areas, we use an augmented two-way fixed effects regression. A growing literature shows that the standard regression setup with unit and time fixed effects can lead to inconsistent estimates in staggered Difference-in-Difference settings if treatment effects are not constant between units and over time (see De Chaisemartin and d'Haultfoeuille (2022) for a survey). The bias arises from comparing the outcomes of treatment "switchers" between two periods to units that were already treated at both points in time. To avoid such "forbidden comparisons", and since we are interested in estimating the differential effect of the deregulation on urban relative to rural counties, we compare income trajectories of urban and rural areas within states that deregulated at the same point in time (deregulation cohort).[9] In

[9]Transferring our setting to the standard Difference-in-Difference setup, treatment would be defined at the interaction of a county being urban and in a deregulated state, the estimated effect is the "urban growth premium" of the deregulation.

more detail, we estimate the following equation:

$$\Delta income_{c,t} = \underbrace{\beta_1\ deregulation_{s,t} + \beta_2\ urban_c}_{\text{absorbed}} + \beta_3\ deregulation_{s,t} \times urban_c + controls_{c,t-1} + \theta_c + \tau_{s\in g,t} + \epsilon_{c,t}. \tag{1}$$

The dependent variable $\Delta income$ denotes income growth in county c in state s in year t. The dummy *deregulation* takes on a value of one in the years after a state has deregulated its banking system; *urban* takes on a value of one if the urban score in a county is greater or equal to 0.5. We include the following county-level control variables, all lagged by one period: the log of the total population, population growth, the share of population of age 65 and above, and the share of black population.

Each regression includes county fixed effects (θ_c) that absorb time-invariant county characteristics, and thus also the base effect $\beta_2\ urban_c$. Furthermore, we add deregulation cohort g times year fixed effects ($\tau_{s\in g,t}$) that account for common trends and ensure that we do not compare later deregulated to already deregulated counties. In some specifications, we include state*time fixed effects instead of cohort*time fixed effects. These control for time-varying observable and unobservable factors that affect all counties within the same state. In essence, with state*time fixed effects, we compare the differential effect of deregulation on urban vs. rural counties in the same state. Again, this avoids biased results in the presence of heterogeneous effects of the deregulation within the group of urban or rural counties and over time.[10] Standard errors are clustered at the state level, as this is the level at which the policy change is administered.

Our coefficient of interest is β_3, the coefficient on the interaction term that indicates whether urban counties see faster or slower growth in incomes than rural areas post deregulation. In the absence of the interaction term and year fixed effects instead of deregulation cohort specific nonparametric trends, the coefficient β_1 shows whether deregulation has increased income growth in the average (urban or rural) county. Previous literature would suggest that banking deregulation has an overall positive effect on growth and hence $\beta_1 > 0$ (Jayaratne and Strahan, 1996; Levine, 2005).[11] In an initial

[10] When using a non-binary treatment variable, another type of "forbidden comparison" can arise from comparing "more" to "less" treated units if treatment effects are heterogeneous across units, even if treatment occurs at the same time (De Chaisemartin and d'Haultfoeuille, 2022). We thus use an urban dummy variable in our baseline specification to define treatment. Defining treatment based on the continuous urban score in a robustness check yields however a similar result (section 4).

[11] Jayaratne and Strahan (1996) study the effect of intrastate deregulation at the state level, we

check, we apply the robust Difference-in-Difference estimator by Callaway and Sant'Anna (2021) to estimate the average treatment effect of deregulation on deregulated counties.[12]

Identification. For β_3 to causally identify the differential growth effect of the deregulation on urban vs. rural incomes, the income gap between urban and rural counties needs to have followed the same trend absent deregulation. While we cannot observe potential outcomes absent deregulation, we provide evidence that states' timing to deregulate was not systematically correlated with pre-existing rural-urban income differences and show evidence of no pre-trends in the years before the deregulation. In Section 4 we further explore whether the effect of deregulation persists when controlling for other potential drivers of the urban-rural growth gap.

To begin, as emphasized in Strahan (2003) "states did not deregulate their economies in anticipation of future good growth prospects", nor is the timing of deregulation connected to a state's position in the business cycle.[13] Figure 2 provides additional evidence that there was no discernible relationship between differences in rural vs. urban income growth and a state's decision to open its banking markets. Panel (a) depicts a scatter plot with the pre-deregulation difference in income growth between urban and rural counties in a state on the horizontal axis against a state's year of deregulation on the vertical axis. Panel (b) provides the same figure for income growth per capita. Both panels show an insignificant near-zero relationship between pre-deregulation income growth gaps between urban and rural counties and the year of deregulation. This suggests that policy makers at the time did not deregulate in response to differences in income growth across urban and rural counties in their state.

To examine potential differences in pre-trends in more detail, Figure 3, panel (a)

compare the effect of intra- and interstate deregulation in section 4.

[12]Since we cannot compare counties within a deregulation cohort or within a state to estimate the overall effect of the deregulation on county income growth, we use a heterogeneity robust Difference-in-Difference estimator to avoid "forbidden comparisons". The approach by Callaway and Sant'Anna (2021) compares the outcomes of deregulated counties before and after treatment to not-yet deregulated counties in the same periods. No more comparisons can be made once all counties are deregulated. The estimate is robust to arbitrary heterogeneity and dynamic treatment effects.

[13]Previous literature has found that the relative strength of interest groups such as small banks, small firms or rival insurance groups at the state-level determined the pace of deregulation in a state (Kroszner and Strahan, 1999). While we believe that these forces are unlikely to confound our analysis after controlling for county fixed effects, we go one step further, identifying the effect of banking deregulation on the rural-urban divide by using within state variation only: That is, we compare rural and urban counties within one state before and after the deregulation, removing any common state wide time trends, as well as time-constant county characteristics.

plots income in urban and rural counties around the time of a state's deregulation. The horizontal axis plots the time dimension. A value of zero denotes the date at which a state deregulated (the vertical line denotes the year just before deregulation), and the axis ranges from 10 years before to 15 years after deregulation. The vertical axis shows income in urban and rural counties, normalized to one in the year prior to deregulation. Incomes in urban and rural areas followed a similar trend prior to deregulation. Once states deregulated, however, urban incomes increased at a relatively faster pace than those in rural counties.

We also estimate an event study version of Equation 1 and provide the coefficient plot in panel (b) of Figure 3. We replace the dummy *deregulation* with individual dummies for each year in the seven years prior and 15 years after deregulation.[14] The effects are estimated relative to the last year before deregulation. A plot of the coefficient estimates with 95% confidence intervals shows that there were no differential pre-trends in the income gap between urban or rural counties. The coefficient estimates are statistically insignificant in every year prior to deregulation. Following deregulation, urban counties tend to see a significant and economically large increase in incomes.

Figure 2 and Figure 3 thus support the argument that deregulation was unrelated to preexisting growth trajectories in rural and urban incomes and suggest an absence of differential pre-trends.

3.2 Financial deregulation and regional divergence

Table 2 reports results for Equation 1 and shows that financial deregulation leads to a divergence in income growth across urban and rural counties. Column (1) first shows that deregulation leads to significantly higher income growth in general, consistent with the literature. Adding the urban interaction term to the regression equation to analyze the differential effect of deregulation in urban relative to rural counties, column (2) reports the estimates for the standard linear regression design with county and year fixed effects. The result again suggests an overall positive impact of the deregulation on growth in both rural and urban areas. Yet, the positive and significant coefficient on the interaction term indicates the existence of an urban growth premium post deregulation.

[14] To keep a balanced sample of counties, we extend the time window back to seven years before the deregulation took place.

Column (3) applies our preferred augmented two-way fixed effects set up which is robust to potential heterogeneous treatment effects across groups and over time. The coefficient estimate on the interaction term remains almost unchanged, suggesting that "forbidden comparisons" are not driving the result in our comparison of rural vs. urban counties before and after the removal of banking restrictions. In terms of magnitude, the coefficient estimate suggests that deregulation fosters growth in urban areas by an additional 0.35pp per year on average, relative to rural areas. This corresponds to about one third of the overall effect of growth found in column (1).

Our baseline specification in column (3) controls for time-invariant county characteristics through county fixed effects, as well as for trends common to all counties within a deregulation cohort. To strengthen identification, column (4) introduces state*year fixed effects. These fixed effects control for unobservable time-varying factors at the state level that could be correlated with the timing of financial deregulation and also affect income growth. In terms of magnitude, moving from a rural to an urban county within the *same state* implies an increase in income growth by an additional 0.23pp after deregulation. To examine the robustness of our main finding, columns (5) and (6) include time-varying county controls. Results remain nearly identical to columns (3) and (4), reducing the estimated urban growth premium marginally to 0.26pp and 0.21pp, respectively.

Figure 3, panel (b) shows the dynamic effect of deregulation on the rural-urban income gap over time. Pre-deregulation, no statistically significant change in rural vs. urban incomes is found relative to the last year before a state permitted interstate banking. Once a state deregulated, the estimates suggest an increased gap between rural and urban incomes relative to the last pre-deregulation period. The estimated effect is most pronounced in the first five years following deregulation and stabilizes in the medium term.

Taken together, these results suggest that interstate banking deregulation spurred income growth by relatively more in urban areas.

3.3 Examining the mechanisms

We now turn to potential mechanisms driving the divergence in incomes between urban and rural counties following deregulation. We first set the stage by discussing different channels. We then zoom in on the differential effect of deregulation on rural vs. urban

banking markets by analyzing whether deregulation has induced a relative increase in banking presence and competition in urban areas. Previous literature has found that better developed financial markets support economic growth by easing financing constraints for firms (Levine, 2005). We therefore study the differential effect of deregulation on financially constraint firms in rural and urban counties.

3.3.1 Theoretical considerations

Why would bank activity increase more in urban than rural counties following deregulation? One channel could work through banks' desire to diversify their portfolio, combined with the lending opportunities in rural and urban counties.[15]

For simplicity, assume that the local pool of projects requiring financing in rural and urban areas offers the same expected return distribution with idiosyncratic location-specific shocks, but the size of the overall local pool is larger in urban areas.

Consider next risk-neutral banks that maximize their profits (expected portfolio return minus refinancing rate on deposit and wholesale debt) subject to a value-at-risk (VaR) type of constraint, in the spirit of Shin (2012) as well as Hoffmann and Stewen (2020). At the optimum the VaR constraint binds. An increase in banks' (geographic) loan portfolio diversification relaxes their VaR and more diversified banks can sustain a higher leverage ratio, everything else equal. For a given level of equity, this increases the lending capacity of more diversified banks.[16]

Before deregulation, banks are restricted to operating branches within their home state. Their ability to diversify their geographic exposures is limited to lending to rural and urban in-state locations. Assuming that the VaR binds at a point where credit demand exceeds credit supply, some equally profitable projects remain unfunded in the larger urban credit market.

Following deregulation, banks can reduce their geographic portfolio risk by operating branches and lending to locations outside of their home state.[17] The increased diversifica-

[15] Previous work shows that geographic diversification reduces bank risk (Deng and Elyasiani, 2008; Goetz et al., 2016), for example by improving banks' access to funding (Doerr and Schaz, 2021; Levine et al., 2021).

[16] Adrian and Shin (2010) and Adrian and Shin (2008) discuss evidence that US banks increase their leverage to meet the binding value-at-risk constraint via increasing the asset side of their balance sheet, rather than buying back their equity.

[17] As de-novo banking and branching across state borders remained restricted after states allowed

tion translates into an overall increased lending capacity, benefiting both urban and rural areas through increased credit supply.[18] Moreover, rather than lending to rural and urban counties in their home state, banks could now provide credit to urban locations inside and outside their home state to diversify their portfolio. In equilibrium, the marginally financed projects in rural and urban counties should now be equally profitable.

To sum up, under the assumption that urban areas have an overall larger pool of projects requiring external funding, with a similar or better expected return distribution, this predicts a relative larger increase in banks' activities in urban areas post deregulation, as (out-of-state) banks seek to exploit investment opportunities that were previously unfunded due to risk concentration. In the following, we provide empirical evidence for a relative increase in banking activity in cities following deregulation.

3.3.2 Banking markets in rural and urban counties

As local information matters in loan markets (Nguyen, 2019), and in particular for lending to smaller and younger firms (Berger and Udell, 2002), we approximate banks' activity in an area with the presence of branches and headquarters. In particular, to test for a relative increase in banks' activities in cities, we estimate Equation 1 with the log of the number of branches per capita, as well as the number of headquarters per capita as dependent variables.

Table 3 indicates that deregulation has led to a shift in the center of gravity of the banking industry towards urban areas. Columns (1)–(3) show that the log number of branches per capita increased relatively more in cities post deregulation. Compared to a rural county, an urban county experienced a 5.6% larger increase in branches per capita in column (1).[19] The difference remains of similar magnitude and highly statistically significant when including county controls and when accounting for time-varying trends at the state level through state*time fixed effects in columns (2) and (3).

In columns (4)-(6) we repeat the same exercise with bank headquarters per capita as

for entry of out-of-state banks, banks would expand into new markets mainly through mergers and acquisitions (Stiroh and Strahan, 2003; Kroszner and Strahan, 2014).

[18] As discussed above, in addition to banks increased ability to diversify, the deregulation lead to overall efficiency gains, lower average operating costs and loan losses in the financial sector (Jayaratne and Strahan, 1998; Stiroh and Strahan, 2003).

[19] Another complementary channel leading to an increased concentration of banking towards cities might be rational herding. Chang et al. (1997) find evidence for such a behaviour when analysing the location decisions of bank branches in New York.

outcome variable. The presence of banks' headquarters – and thus the accessibility of bank services not available in branches – substantially increased in urban relative to rural counties. The average county in our sample has 1.5 headquarters per 10.000 inhabitants. The coefficient estimate in column (4) hence suggests that the number of headquarters increased by about one fifth of the sample average in an urban relative to a rural county after deregulation. Results are similar in a specification with time varying controls and state*year fixed effects.

The pattern is also visible when we plot branches and headquarters per capita against the share of urban population in the period prior to and after deregulation in Figure 4. Panel a) shows a negative relationship between the urban score of a county and the number of branches per capita - more densely populated areas do not see an over-proportional increase in the number of branches. Post-deregulation however, the number of branches per capita increased particularly in more urban counties. Panel b) shows that urban areas tend to have a smaller number of headquarters per capita compared to more rural counties. Following the deregulation, the number of headquarters dropped however sharply in rural places, while it remained almost unchanged in the most urban areas. Taken together, Figure 4 shows that deregulation is associated with an increase in branches and headquarters per capita in urban relative to rural areas.

Physical proximity to banks, and especially their headquarters (Liberti and Mian, 2008), is an important factor in facilitating access to credit for small and medium enterprises (Brevoort and Wolken, 2008; Nguyen, 2019; Kärnä et al., 2020), even if the recent adoption of information technology in the banking sector mitigates the importance of distance in lending (Ahnert et al., 2022). One way deregulation could have fuelled the urban-rural income divide is thus by eased access to finance for bank dependent firms especially in urban areas. Section 3.3.4 provides complementing evidence by analyzing the differential effect of the deregulation on employment at financially constraint firms.

To continue, previous literature has shown that higher competition in the banking sector can spur economic activity by reducing the cost of credit for firms and households (Jayaratne and Strahan, 1998; Black and Strahan, 2002). To investigate whether a higher number of branches was accompanied by higher competition in the local banking sector, we estimate Equation 1 with the share of deposits accounted for by the three largest banks as outcome variable.[20] Column (7) of Table 3 indicates that deregulation leads

[20]Black and Strahan (2002) show that deregulation has led to tighter competition in local banking markets, increasing the rate of new incorporations in the local economy. We complement their findings

to a significant decline in the deposit share of the top-three banks in urban counties, relative to rural counties. Including state*time fixed effects or additional county controls in columns (8) and (9) leaves this conclusion unaltered. The relative importance of the largest banks in cities declined, suggesting a more diversified and competitive financial sector, with attendant benefits for local firms.

3.3.3 Mergers and acquisitions

Following deregulation, the restructuring of the banking industry occurred mainly through mergers and acquisitions. The reason is that de-novo banking and branching across state borders remained restricted after states allowed for entry of out-of-state banks, so mergers and acquisitions were key to the restructuring of the banking industry and an increase in local competitive dynamics (Stiroh and Strahan, 2003; Kroszner and Strahan, 2014).

We thus provide additional evidence for banks targeting especially urban areas after the removal of interstate banking restrictions by studying mergers and acquisitions and the share of out-of-state banks in local banking markets. In Table 4 we estimate Equation 1 using the deposit share of acquirees and the share of merging banks as dependent variables. The results indicate that banks in urban areas were more often the target of an acquisition post deregulation relative to rural banks. With the inclusion of state*year fixed effects, the deposit share of acquired banks increases by 0.88pp to 0.90pp in an urban relative to a rural county post deregulation (column (2) and (4)) - a sizeable increase relative to the mean deposit share of acquirees which corresponds to 4.2%. The deposit share of merging banks sees a relative increase of 3.15pp to 3.88pp, as indicated by the coefficients in columns (5) to (8).[21]

The consolidation activity in local banking markets post deregulation can also be shown visually in Figure 5. Panel (a) shows how the county-level deposit share of banks not active in a state prior to deregulation evolved after the removal of interstate banking restrictions, splitting the county sample based on the degree of urbanization. The deposit

by looking at the differential effect on competition in rural vs. urban banking markets.

[21]Section B in the Appendix provides additional insights on the relatively larger merger and acquisition activities in cities. It analyzes which characteristics distinguish banks that were the target of an acquisition from banks that were not involved in a merger - before the deregulation when mergers and acquisitions were restricted and after the deregulation when mergers and acquisitions were liberalized. It then compares "desired acquiree characteristics" to the characteristics describing an average bank in rural vs. urban areas before the onset of the banking deregulation.

share of out-of-state banks is plotted against the year since deregulation.[22] Following the removal of interstate banking restrictions, out-of-state banks took over increasing market shares in the local banking industry in all counties. Consolidation was, however, more pronounced in urban counties. For the average county with an urban score below 0.5, the deposit share of banks belonging to a bank holding company that newly entered a state post deregulation reaches 50% 15 years after the deregulation (black dashed line). For counties with an urban population share greater or equal to 0.5, the market share of out-of-state banks jumps up quickly after the removal of interstate banking deregulation and reaches a value of 60% after 15 years.

Before deregulation, restrictions protected under-performing banks from competition (Stiroh and Strahan, 2003). The removal of these restrictions on cross-state ownership lead to the creation of larger, more efficient and better diversified banks (Kroszner and Strahan, 2014). In panel (b) of Figure 5, we analyze differences in portfolio risk diversification of the average rural and urban bank post deregulation. Diversification is approximated with the number of states in which the average bank located in a given county is operating in. Before deregulation banks in almost all counties were only active in the state of the county where they were located – confirming compliance with and limited exceptions from the restrictions. Post deregulation, banks became active in multiple states. Geographic diversification increases, however, sharply in a county's urban score: While the average bank in an entirely rural county maintains branches in 2.5 states, the average bank in an entirely urban county operates in more than five states post deregulation. As detailed in section 3.3.1, banks with a more diversified loan portfolio can work with a higher leverage ratio, especially if they are operating under a Value-at-risk constraint (Hoffmann and Stewen, 2020), suggesting a higher lending capacity in urban areas, everything else equal.[23]

In conclusion, more urban areas likely have benefited from deeper and more competitive financial markets post deregulation. In what follows, we investigate the effects on firms.

[22]Out-of-state banks are defined as banks whose bank holding company was not active in a state before deregulation. By construction, the deposit share of newly entering banks in a state is zero before the banking deregulation.

[23]Employing an IV strategy in the setting of the US interstate banking deregulation, Levine et al. (2021) find that funding costs fall for more geographically diversified bank holding companies, while Goetz et al. (2016) show that overall bank risk decreases.

3.3.4 Financially constrained firms

Fiercer competition in the banking sector benefits financially constrained firms in particular (Cetorelli and Strahan, 2006).[24] Based on our results on banks' presence and competition in local markets in Table 3, we thus expect that financially constrained firms in urban areas benefited disproportionately from deregulation.

To investigate this possibility, we estimate Equation 1 with the county employment share at financially constrained firms as dependent variable. We use three alternative approaches to define financially constrained firms. Following Hadlock and Pierce (2010) who show that age and size are important predictors of firms' credit constraints, we classify startup firms (formed in the year of observation) or small firms (less than 20 employees) as financially constrained.[25] The constraint firm employment share is then computed as county employment at startup firms over total county employment, and county employment at small firms over total county employment, respectively. As a third approach we use the external finance dependency measure originally proposed by Rajan and Zingales (1998). Firms in sectors with an external finance dependency larger than zero are classified as financially constrained. The employment share at constrained firms is defined analogous to before.

Table 5 present the results. Columns (1)-(3) show the results for the county employment share at startups relative to more mature companies. Across the three different specifications, the estimates point towards an increase in the startup employment share by 0.3pp more in urban relative to rural counties post deregulation - a sizable increase relative to the sample average employment share at startups of 3.61%.

The coefficient estimates in columns (4)-(6) indicate a similar increase for the employment share at small firms in cities relative to rural areas following the deregulation. Depending on the set of fixed effects and controls applied, the small firm employment share rose by an additional 1.2 to 1.4pp, relative to a sample average of 31.5%.

Columns (7)-(9) repeat the exercise for the employment share in external finance dependent sectors. The results suggest that employment in industries with higher external

[24] For example, Bertrand et al. (2007) show that bank-dependent sectors benefited in particular from deregulation in France.

[25] In a cross-country setting, Beck et al. (2004) document that small firms are facing more obstacles to obtain financing in highly concentrated banking markets. Petersen and Rajan (1994), Robb and Robinson (2014) and Doerr (2021) provide further evidence that smaller and younger firms are relatively more dependent on banks as a source of financing.

finance dependence increased relatively more in urban areas post deregulation.

Table 5 thus paints a consistent picture: deregulation boosted employment among financially constrained firms in urban areas. This could suggest that a relatively denser branch network and increased competition among banks in urban areas post-deregulation relaxed financial constraints, an interpretation consistent with a relatively larger increase in credit supply in urban areas post deregulation.

4 Robustness and further results

This section investigates alternative drivers of the urban-rural gap, provides evidence on local labor market effects, and presents results from additional robustness tests.

Initial industry structure. While our baseline specification controls for any initial or time constant differences in the local industry structure of rural vs. urban counties via county fixed effects and thus for their time constant effects on income growth, a county's initial industry structure might result in differential growth effects following the banking deregulation. For instance, if manufacturing industries, or tradable industries more general, were to benefit more (less) from deregulation and if manufacturing industries were disproportionally located in urban (rural) areas, the growth premium that we attribute to a county's urbanness might be a story of a boom (decline) in manufacturing. To address these concerns, we augment our baseline specification in Equation 1 by adding interaction terms of initial sector share controls and the deregulation dummy. We compute pre-deregulation initial sector shares from the 1975 County Business Pattern data prepared by Eckert et al. (2021). To begin, average initial manufacturing and tradable industry employment shares do not differ substantially between rural and urban counties as reported in the table notes of Table 6. Turning to the regression analysis, column (1) of Table 6 shows the residual effect of the deregulation in urban vs. rural areas while accounting for a potentially different effect of the deregulation arising from a county's pre-deregulation manufacturing sector share. Column (2) additionally allows for general time trends based on a county's initial manufacturing share. Controlling for potential growth difference stemming from a county's relative strength in manufacturing leaves our baseline finding almost unaltered: deregulation promoted growth more in urban relative to rural areas. Controlling for tradable instead of manufacturing employment shares in

columns (3-4) yields similar effects.

Recent literature has discussed another source of urban biased growth: Higher wage growth in urban areas resulting from a fall in the price for information and communication technology that favor cities with a larger endowment in "skilled scalable services" industries (SSS) that rely on these technologies (Eckert et al., 2020). SSS industries include Information (NAICS 51), Finance and Insurance (NAICS 52), Professional Services (NAICS 54), and Management of Companies (NAICS 55).[26] In columns (5) and (6) of Table 6 we investigate whether our baseline results are primarily driven by expansions in these service industries, using the same setup as above. The coefficient estimate for the urban growth difference post deregulation is marginally reduced and remains highly statistically significant.

To complement the analysis, column (7) jointly controls for interactions with initial employment shares in manufacturing, tradable and SSS industries. Column (8) adds industry specific time trends. The result suggest that the urban growth premium post deregulation does not appear to be rooted in potentially different initial industry structures of rural and urban places and industry specific trends post deregulation. While industry specific trends combined with different comparative advantages in rural and urban areas might have fostered an urban growth bias, a residual effect of the banking deregulation remains.

Local labor market effects. To complement our results on increasing rural-urban income inequality post-deregulation, we study the differential impact of banking deregulation on wages, employment and population in rural vs. urban counties. Table 7 presents the results. Deregulation promoted total wage and salary income growth in an urban county by an additional 0.19pp to 0.37pp compared to a rural county (columns (1-2)). Furthermore, wages per worker tended to grow faster in urban areas post-deregulation (column (3-4)). The results in column (5) and (6) show that urban counties experienced a significantly larger increase in employment relative to working age population following the deregulation. An increase in employment shares, combined with a modest increase in the urban wage premia per worker, seems thus to be associated with the overall increase in wage (and total) income growth. Columns (7)-(10) test whether the growth in

[26]The results are robust to defining "skilled scalable services industries" as business service industries more general (NAICS 5), in line with the updated version of the working paper by Eckert et al. (2020) which emphasizes in addition the complimentary for large business service firms present in cities (Eckert et al., 2022).

employment was accompanied by a differential (working age) population growth post-deregulation. The results in columns (7) and (8) indicate no statistically significant difference in working age population growth rates between urban and rural areas. For total county population growth, the coefficient estimate shows a small positive, marginally significant effect in column (9) which turns insignificant with the inclusion of state*year fixed effects. This suggests that banking deregulation did not induce major migration flows towards urban areas.

Robustness. We conduct several tests to check for the robustness of our results. First, we experiment with different urban score cut-offs to define urban and rural counties. Our baseline measure classifies counties in metropolitan areas and urbanized non-metro counties as urban, designating all other counties as rural. When we use a more "rural" control group and a more "urban" treatment group, while excluding non-metro little urbanized counties (Table A2, columns (1) to (3)), the urban premium effect of deregulation is more pronounced. Dropping non-metro counties that are adjacent to a metro area from the sample and thus limiting potential spillover effects from large neighbouring counties, yields only marginally larger estimates (Table A2, columns (4) to (6)). Using the urban score as a continuous measure of urbanness in columns (7) to (9) of Table A2, confirms the baseline findings.[27] Taken together, the results suggest that the more urban a county, the higher the growth in income following the removal of banking restrictions.

Second, we test for robustness regarding alternative definitions of *urban*. Our preferred measure, the USDA ERS rural-urban continuum codes, take into account the metro vs. non-metro nature and population size of the area that the county is part of, its degree of urbanization, and metro proximity. Alternative measures for the designation of urban counties include a county's population share living inside urban areas, or inside urbanized areas, or a county's population density.[28] As they capture similar underlying characteristics of urban vs. rural counties, they are highly positively correlated with our baseline measure (see table notes of Table A3). Differences stem from counties with overall small population size whose inhabitants primarily reside in small places with little more than 2,500 inhabitants (high urban population share, low urban score) or counties with a

[27]To avoid "forbidden comparisons" between "more" or "less" treated counties in the presence of treatment effect heterogeneity (De Chaisemartin and d'Haultfoeuille, 2022), we prefer the dummy specification for our baseline results.

[28]Urbanized areas consist of one or more places with densely settled surroundings that have a joint minimum population of 50,000. Urban areas are urbanized areas plus places of 2,500 or more inhabitants (U.S. Census Bureau, 1995).

large unpopulated area that contain a major city (low population density, higher urban score). Table A3 presents the results of Equation 1 for the three alternative measures. For the regression specification, we classify counties in the top tercile with respect to their 1970 urban population share (urbanized population share; population density) as being urban.[29] Comparing Table A3 to our baseline results, the coefficient estimates and precision levels obtained are highly comparable. Our baseline results are thus not driven by the choice of urban definition.

Next, the results are similar when analyzing income growth per capita (Table A4). The marginally smaller effects could indicate that part of the effect might be due to a differential increase in population. Directly testing for an effect of deregulation on population growth (Table 7, columns (9) and (10)) yields however a small positive, marginally statistically significant or insignificant effect. The results are also robust to excluding individual states or deregulation cohorts from the estimation (Figure A2).

Using intrastate deregulation or the earlier of inter- or intrastate deregulation to define treatment status for a state, we find similar patterns (Table A5). The effects for intrastate deregulation are however weaker and when jointly estimating the effect of intra- and interstate deregulation in columns (5) and (6), we find a large positive effect of interstate deregulation, while the effect of intrastate deregulation turns insignificant. The increase in the rural-urban gap resulting from banking deregulation is thus mainly rooted in the removal of restrictions on cross-state banking. This is in line with large bank mergers and acquisitions sharply increasing primarily after the interstate banking deregulation, which substantially transformed the US banking system (Stiroh and Strahan, 2003).

5 Conclusion

After moving in tandem during the 1970s, incomes in urban and rural areas diverged since the mid 1980s, as growth in urban areas outpaced income increases in more rural regions. Exploiting the quasi-experimental setting in the history of US banking deregulation, we provide evidence that the removal of restrictions on banks' ability to operate across larger areas contributed to the growing income gap. While we confirm previous findings

[29]Information on the 1970 share of the population that lives inside urban, urbanized and rural areas within each county is obtained from the 1970 Census downloaded via the IPUMS National Historical Geographic Information System (Manson et al. (2021)). Population density is computed by dividing a county's 1970 population (obtained from BEA) by its area (obtained from U.S. Census Bureau).

of banking deregulation promoting economic growth in general, our results suggest that cities benefited from relatively better and potentially cheaper access to finance after banking markets became more integrated. Post-deregulation, urban areas experienced a larger increase in branches and bank headquarters per capita, accompanied by an increase in local banking market competition. This development is reflected in the expansion of firms that are considered to be financially constrained . Furthermore, our results indicate a relatively larger increase in wages per worker and higher employment rates in urban compared to rural counties after the deregulation.

Deregulation has hence led to a relatively larger relaxation of financial constraints in urban areas and lies at the heart of the widening urban-rural gap in incomes. Place-based policies that try to promote growth in disadvantaged areas should take the crucial role of credit supply conditions and competition in the financial sector into account. Our results also suggest that previous findings that financial deregulation tightens the distribution of income come with a caveat: even if inequality goes down overall, it could increases across areas. That being said, it is beyond the scope of this paper to determine whether the allocation of financial resources across regions is economically efficient after the removal of geographic restrictions on banking - despite yielding a potentially undesired outcome from an equality point of view.

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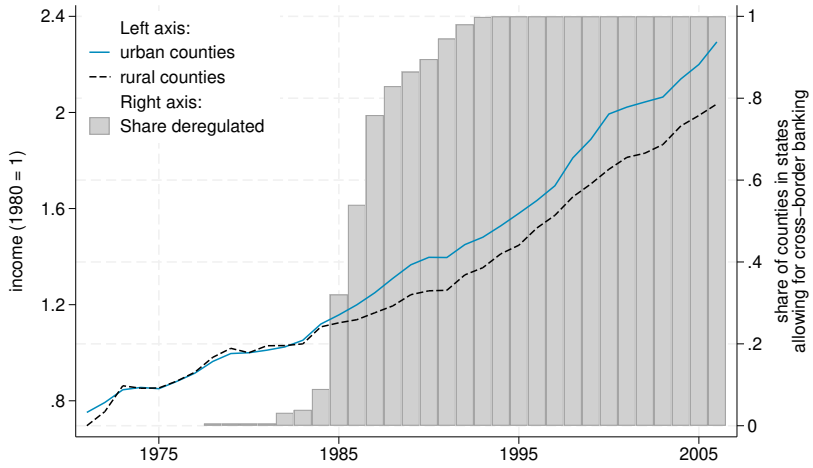
Figures and tables

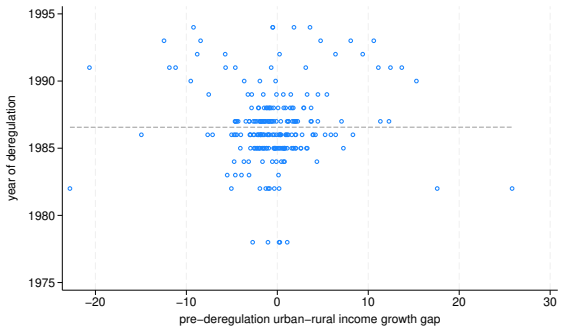
Figure 1: The evolution of income in rural and urban counties



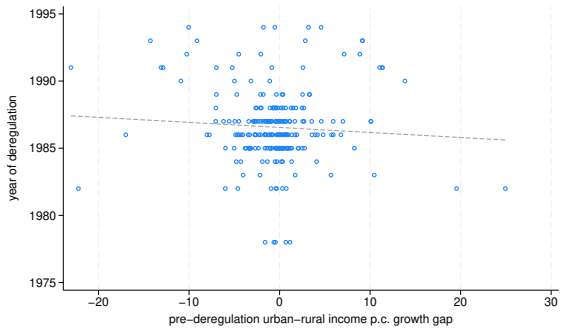
Note: This figure plots average real personal income in urban (blue solid line) and rural (black dashed line) counties from 1971 to 2006 (left y-axis). Both series are normalized to 1 in 1980. Urban areas are defined as counties with an urban score above or equal to 0.5 as detailed in Table A1. The grey shaded bars show the share of counties in states that permit interstate banking (right y-axis).

Figure 2: **Rural vs. urban income growth before deregulation**

(a) Δ income



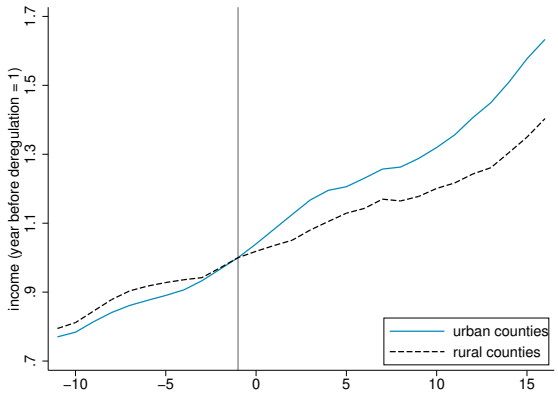
(b) Δ income p.c.



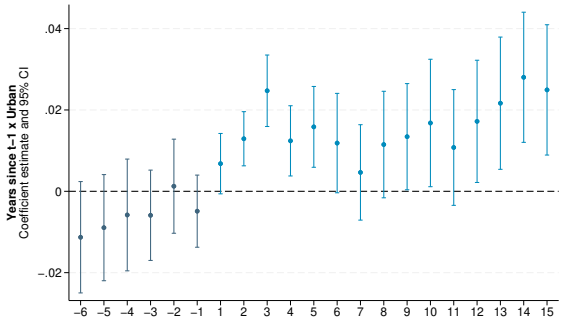
Note: This figure plots the year of deregulation against the pre-deregulation income growth gap between urban and rural areas in a given state (panel (b) depicts the difference in income growth per capita). In more detail, for each year from 1970 to 1975, the difference in urban and rural income growth in a given state is computed. Urban areas are defined as counties with an urban score above or equal to 0.5. The t-statistic for the correlation between the year of deregulation and the average pre-deregulation rural-urban income growth difference is 0.00 and the relationship is thus not statistically significant different from zero. The same holds for pre-existing trends in log income growth per capita, where the t-statistic for the correlation with the year of deregulation is -1.04.

Figure 3: **The dynamic impact of deregulation on incomes**

(a) Raw gap



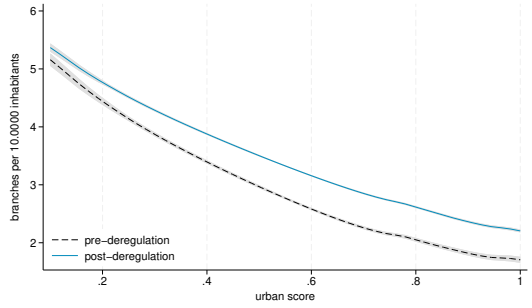
(b) Event study regression



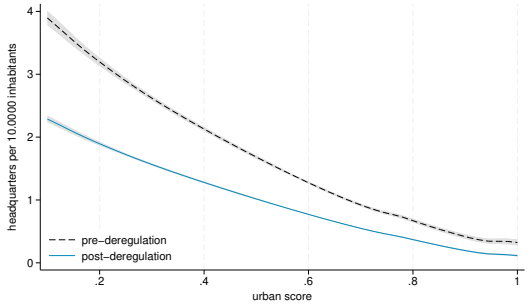
Note: Panel (a) plots average real personal income in urban (blue solid line) and rural (black dashed line) against the year until or since the county's state entered into an interstate banking agreement. The x-axis covers the time window spanning from 10 years before interstate banking deregulation until 15 years after deregulation. Both series are normalized to 1 at $t = -1$. Urban counties are defined as those with at least half of the population living in urban areas. Panel (b) plots the coefficient estimates and 95% confidence intervals of the following event study regression: $log(income)_{c,t} = \sum_{k=-6}^{K=15} \beta_k deregulated_{st}^k * urban_c + controls_{c,t-1} + \theta_c + \tau_{coh,t} + \epsilon_{c,t}$. Coefficient β_k indicates the effect of deregulation on county incomes in urban relative to rural areas k years from the last period before treatment changes. The coefficient of the last year before the deregulation ($k = 0$) is omitted. The x-axis covers the time period 7 years before deregulation (6 years before the last pre-period) to 14 years after states permitted interstate banking. The dark blue lines show the estimates of the pre-treatment period, the bright blue lines show the estimated effect following the deregulation. Standard errors are clustered at the state level.

Figure 4: **Deregulation and local banking presence**

(a) Branches



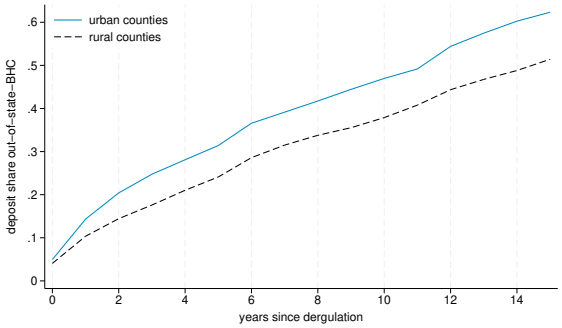
(b) Headquarters



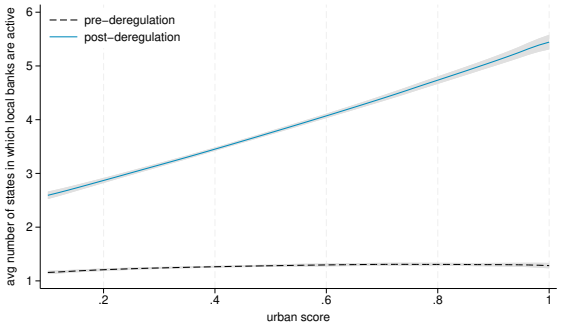
Note: This figure shows a local polynomial of degree one and plots bank branches (panel a)) and bank headquarters (panel b)) per 10.000 inhabitants in a county against a county's urban score, splitting the sample of county-year cells into the pre-deregulation (black dashed line) and post-deregulation (blue solid line) period. Shaded areas denote 99% confidence intervals.

Figure 5: **Deregulation, consolidation, and diversification**

(a) Consolidation in local banking markets



(b) Geographic diversification of local banks



Note: Panel (a) plots the county level deposit share of banks not active in a state before deregulation after the removal of interstate banking restrictions. The x-axis shows the year since deregulation. The black dashed line shows how the share of out-of-state-banks evolved in rural counties with an urban score below 0.5. The blue solid line shows the development for counties with an urban score above or equal to 0.5. By construction, no out-of-state banks where active in a county pre-deregulation. Panel (b) provides a local polynomial of degree one of a county's banks' geographic diversification (proxied by the deposit-weighted average number of states in which local banks are active in) on a county's 1974 urban score, splitting the sample of county-year cells into the pre-deregulation and post-deregulation period. Shaded areas denote 99% confidence intervals.

Table 1: **Descriptive statistics**

	mean	sd	p10	p90	count
Δ income	2.86	6.76	-3.06	8.92	108972
Δ income p.c.	2.04	6.59	-3.49	7.58	108972
urban score	0.40	0.25	0.10	0.80	108972
urban (dummy)	0.31	0.46	0.00	1.00	108972
employment share	54.64	16.52	34.21	75.84	108972
Δ wage	2.43	6.70	-4.23	8.91	108972
Δ wage per worker	0.93	3.52	-2.77	4.52	108972
Δ working age population	1.08	2.47	-1.30	3.71	108972
Δ population	0.83	2.24	-1.31	3.22	108972
population share age 65 and older	13.92	4.28	8.82	19.46	108972
population share black	8.87	14.57	0.04	31.39	108972
branches per 10.000 inhabitants	3.81	2.38	1.74	6.54	78702
HQ per 10.000 inhabitants	1.50	1.96	0.00	3.86	78702
deposit share top 3 banks	0.83	0.17	0.58	1.00	78193

Note: The baseline sample includes 3,027 counties in 49 states from 1971 to 2006 (1981 to 2006 for the banking sector variables).

Table 2: **The impact of deregulation on rural and urban incomes**

	Δ income					
	(1)	(2)	(3)	(4)	(5)	(6)
deregulation	0.973***	0.963***				
	(0.204)	(0.283)				
deregulation × urban		0.394***	0.346***	0.228***	0.285***	0.207***
		(0.092)	(0.077)	(0.063)	(0.067)	(0.059)
Observations	69621	108972	108972	108936	108972	108936
County FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	C*Y	S*Y	C*Y	S*Y
Controls					✓	✓

Notes: The baseline sample includes 3,027 counties in 49 states from 1971 to 2006. Washington, D.C. is omitted with the inclusion of state*year fixed effects reducing the sample to 3,026 counties. The dependent variable is county income growth. *deregulation* is a dummy with value one for all years in which a state permits in-state banking, and zero for all years before. Counties with an urban score above or equal to 0.5 are classified as *urban*. The estimate in column (1) shows the average treatment effect of the deregulation on deregulated counties, applying the robust DiD estimator of Callaway and Sant'Anna (2021) which estimates the effects of a binary treatment with staggered roll out while allowing for arbitrary heterogeneity and dynamic treatment effects. Columns (2) to (6) show the differential growth effect of the deregulation on urban relative to rural counties. All regressions in columns (2)-(6) include county and year, cohort*year or state*year fixed effects, and baseline country controls as indicated. The coefficient on *urban* and *deregulation* (in columns (3)-(6)) is absorbed by the fixed effects. Standard errors clustered on the state-level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: **The impact of deregulation on the banking sector**

	Log branches p.c.			Headquarters p.c.			Deposit share Top 3 banks		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
deregulation × urban	0.056***	0.054**	0.075***	0.278***	0.255***	0.215***	-0.024***	-0.023***	-0.023***
	(0.020)	(0.021)	(0.018)	(0.066)	(0.068)	(0.066)	(0.006)	(0.006)	(0.006)
Observations	78195	78195	78169	78702	78702	78676	78193	78193	78167
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	C*Y	C*Y	S*Y	C*Y	C*Y	S*Y	C*Y	C*Y	S*Y
Controls		✓	✓		✓	✓		✓	✓

Note: The baseline sample includes 3,027 counties in 49 states from 1981 to 2006. Washington, D.C. is omitted with the inclusion of state*year fixed effects reducing the sample to 3,026 counties. Zero branch (deposit) county-year records reduce the sample size in columns (1)-(3) and (7)-(9). The dependent variable is log bank branches per 10.000 inhabitants in columns (1)-(3), bank headquarters per 10,000 inhabitants in columns (4)-(6) and the share of deposits hold at the top-3 largest banks in a county in columns (7)-(9). *deregulation* is a dummy with value one for all years in which a state permits in-state banking, and zero for all years before. Counties with an urban score above or equal to 0.5 are classified as *urban*. All regressions include county and cohort*year or state*year fixed effects, and baseline country controls as indicated. Standard errors are clustered at the state-level. *** p<0.01, ** p<0.05, * p<0.1

Table 4: **Mergers and acquisitions in local banking markets**

	share acquirees				share mergers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
deregulation × urban	0.660*	0.896**	0.661	0.877**	3.152**	3.694**	3.191**	3.876**
	(0.386)	(0.402)	(0.398)	(0.415)	(1.546)	(1.536)	(1.549)	(1.495)
Observations	78193	78167	78193	78167	78193	78167	78193	78167
County FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	C*Y	S*Y	C*Y	S*Y	C*Y	S*Y	C*Y	S*Y
Controls			✓	✓			✓	✓

Note: The sample includes 3,016 counties in 49 states from 1981 to 2006. Washington, D.C. is omitted with the inclusion of state*year fixed effects reducing the sample to 3,015 counties. Zero branch (deposit) county-year records reduce the sample size relative to our baseline sample. The dependent variable in columns (1)-(4) measures the share of county deposits held at banks that were the target of an acquisition in a given year. Columns (5)-(8) look at the deposit share of banks involved in a merger - either as acquisition target or as acquiring bank holding company. *deregulation* is a dummy with value one for all years in which a state permits in-state banking, and zero for all years before. Counties with an urban score above or equal to 0.5 are classified as *urban*. All regressions include county and cohort*year or state*year fixed effects, and baseline country controls as indicated. Standard errors are clustered at the state-level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: **The impact of deregulation on financially constrained firms**

	Employment share								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	startup	startup	startup	small	small	small	efd	efd	efd
deregulation × urban	0.313***	0.292***	0.252***	1.350***	1.361***	1.172***	1.270***	1.545***	1.429***
	(0.058)	(0.050)	(0.050)	(0.387)	(0.374)	(0.340)	(0.357)	(0.338)	(0.303)
Observations	82758	82758	82729	82758	82758	82729	82758	82758	82729
Av. share	3.61	3.61	3.61	31.51	31.51	31.51	37.68	37.68	37.68
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	C*Y	C*Y	S*Y	C*Y	C*Y	S*Y	C*Y	C*Y	S*Y
Controls		✓	✓		✓	✓		✓	✓

Note: The baseline sample includes 2,987 counties in 49 states from 1978 to 2006. Washington, D.C. is omitted with the inclusion of state*year fixed effects reducing the sample to 2,986 counties. The dependent variable is county employment share at financially constraint firms, where constraint firms are defined as startups in columns (1-3), small firms in columns (4-6) and external finance dependent firms in columns (7-9). *deregulation* is a dummy with value one for all years in which a state permits in-state banking, and zero for all years before. Counties with an urban score above or equal to 0.5 are classified as *urban*. All regressions include county and cohort*year or state*year fixed effects, as well as baseline country controls as indicated. Standard errors are clustered at the state-level. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: **Controlling for industry trends**

	Δ income							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Mfg	Mfg	Tradable	Tradable	SSS	SSS	All	All
deregulation × urban	0.347***	0.362***	0.361***	0.368***	0.329***	0.331***	0.265***	0.270***
	(0.075)	(0.072)	(0.077)	(0.077)	(0.073)	(0.074)	(0.072)	(0.068)
Observations	108972	108972	108972	108972	108972	108972	108972	108972
Av. sec share urban	29.44	29.44	31.53	31.53	10.8	10.8		
Av. sec share rural	27.48	27.48	34.08	34.08	8.31	8.31		
County FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	C*Y	C*Y	C*Y	C*Y	C*Y	C*Y	C*Y	C*Y
Deregulation × sec sh.	✓	✓	✓	✓	✓	✓	✓	✓
Year × sec sh.		✓		✓		✓		✓

Note: The baseline sample includes 3,027 counties in 49 states from 1971 to 2006. The dependent variable is county income growth. *deregulation* is a dummy with value one for all years in which a state permits in-state banking, and zero for all years before. Counties with an urban score above or equal to 0.5 are classified as *urban*. All regressions include county and cohort*year fixed effects. Columns (1-2) additionally control for the interaction of a county's 1975 mfg sector employment share and the *deregulation* dummy. Columns (3-4) control for the interaction with the tradable sector employment share, and columns (5-6) for the interaction with the skilled scalable service sector share. Column (7) and (8) jointly control for the interaction with the three sector shares. Columns (2), (4), (6), and (8) additional control for non-parametric trends based on a county's initial employment share in the indicated sector. Standard errors are clustered at the state-level. *** p<0.01, ** p<0.05, * p<0.1.

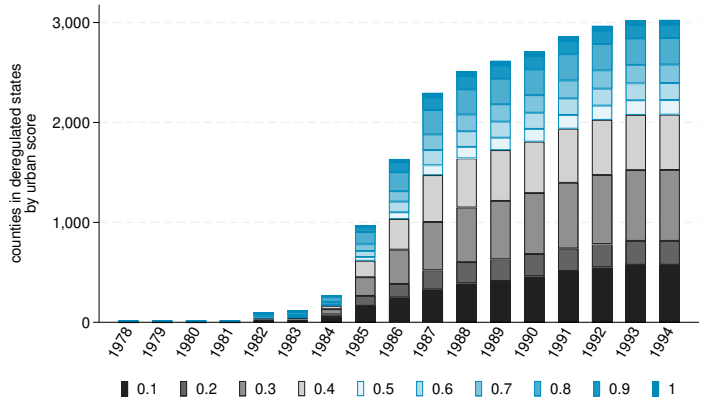
Table 7: **Local labor market outcomes**

	Δ wage		Δ wage p.w.		emp/pop		Δ pop workage		Δ pop	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
dereg × urban	0.365***	0.192*	0.215***	0.127**	3.577***	3.690***	-0.085	-0.106	0.141*	0.100
	(0.121)	(0.099)	(0.077)	(0.061)	(0.493)	(0.472)	(0.083)	(0.076)	(0.080)	(0.072)
Observations	108972	108936	108972	108936	108972	108936	108972	108936	108972	108936
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	C*Y	S*Y	C*Y	S*Y	C*Y	S*Y	C*Y	S*Y	C*Y	S*Y

Note: The baseline sample includes 3,027 counties in 49 states from 1971 to 2006. Washington, D.C. is omitted with the inclusion of state*year fixed effects reducing the sample to 3,026 counties. This table shows the effect of interstate banking deregulation on total wage bill growth (columns (1) and (2)) and wage per worker growth (columns (3) and (4)), as well as on the employment share (computed as wage and salary employment over working age population) (columns (5) and (6)), working age population growth (columns (7) and (8)), and overall population growth (columns (9) and (10)). *deregulation* is a dummy with value one for all years in which a state permits in-state banking, and zero for all years before. Counties with an urban score above or equal to 0.5 are classified as *urban*. All regressions include county and cohort*year or state*year fixed effects. Standard errors are clustered at the state-level. *** p<0.01, ** p<0.05, * p<0.1

A Appendix: Additional figures and tables

Figure A1: **Counties in deregulated states over time**



Note: This graph shows the number of counties by urban score in deregulated states over time. The 49 states in the sample removed interstate banking restrictions in the following years: 1978: ME; 1982: AK, NY; 1983: CT, MA; 1984: KY, RI, UT; 1985: DC, FL, GA, ID, MD, NC, NV, OH, TN, VA; 1986: AZ, IL, IN, MI, MN, MO, NJ, OR, PA, SC; 1987: AL, CA, LA, NH, OK, TX, WA, WI, WY; 1988: CO, MS, VT, WV; 1989: AR, NM; 1990: NE; 1991: IA, ND; 1992: KS; 1993: MT; >1994: HI.

Table A1: **USDA Rural-Urban Continuum Codes**

Score	Code	Name	Description
0.1	9	Rural Not Adjacent	Completely rural or less than 2,500 urban population, not adjacent to a metro area.
0.2	8	Rural Adjacent	Completely rural or less than 2,500 urban population, adjacent to a metro area.
0.3	7	Less Urbanized Not Adjacent	Urban population of 2,500 to 19,999, not adjacent to a metro area.
0.4	6	Less Urbanized Adjacent	Urban population of 2,500 to 19,999, adjacent to a metro area.
0.5	5	Urbanized Not Adjacent	Urban population of 20,000 or more, not adjacent to a metro area.
0.6	4	Urbanized Adjacent	Urban population of 20,000 or more, adjacent to a metro area.
0.7	3	Lesser Metro	Counties in metro areas of fewer than 250,000 population.
0.8	2	Medium Metro	Counties in metro areas of 250,000 to 1 million population.
0.9	1	Greater Metro Fringe	Fringe counties of metro areas of 1 million population or more.
1	0	Greater Metro Core	Central counties of metro areas of 1 million population or more.

Note: This table details the 1974 USDA Rural-Urban Continuum Codes of the Economic Research Service of the U.S. Department of Agriculture. Our baseline set of results defines urban counties as those with an urban score between 0.5 and 1. *Code* refers to the original code published by USDA which we reverse to define the degree of urbanness (*Score*) on a scale from 0.1 to 1, with 1 reflecting the most urban counties. Descriptions are taken from USDA ERS and Hines et al. (1975). All U.S. counties are divided into metropolitan and non-metropolitan counties based on their Office of the Management and Budget classification. The classification is fine-grained according to the population size of the Standard Metropolitan Statistical Area that the county belongs to. For non-metro counties, residents of incorporated and unincorporated towns and cities with more than 2,500 inhabitants are counted as urban population. Adjacency to a metro area is defined based on geographic proximity (contiguity at more than a single corner) and commuter flows of more than 1% of the labor force to the metro core.

Table A2: **Alternative urban dummies**

	Δ income								
	(1) ≥0.6 v. ≤0.2	(2) ≥0.6 v. ≤0.2	(3) ≥0.6 v. ≤0.2	(4) no adjacent	(5) no adjacent	(6) no adjacent	(7) cont.	(8) cont.	(9) cont.
deregulation × urban	0.439***	0.393***	0.289***	0.394***	0.356***	0.262***	0.754***	0.700***	0.516***
	(0.121)	(0.108)	(0.097)	(0.098)	(0.082)	(0.081)	(0.190)	(0.172)	(0.163)
Observations	58176	58176	58140	74340	74340	74304	108972	108972	108936
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	C*Y	C*Y	S*Y	C*Y	C*Y	S*Y	C*Y	C*Y	S*Y
Controls		✓	✓		✓	✓		✓	✓

Note: The baseline sample includes 3,027 counties in 49 states from 1971 to 2006. Washington, D.C. is omitted with the inclusion of state*year fixed effects reducing the sample to 3,026 counties. *deregulation* equals one for all years in which a state permits in-state banking. In columns (1) to (3) the *urban* dummy equals one for counties with an urban score equal or above 0.6. The comparison group are totally rural counties with an urban score below 0.2 In columns (4) to (6), the *urban* dummy follows the baseline definition of an urban score above or equal to 0.5, but excludes counties that are in close proximity to a metro area (urban scores 0.2, 0.4, and 0.6). In columns (7) to (9) *urban* is the continuous urban score, ranging from 0.1 to 1. All regressions include county and cohort*year or state*year fixed effects, and baseline county controls as indicated. Standard errors clustered on the state-level. *** p<0.01, ** p<0.05, * p<0.1.

Table A3: **Alternative measures of urban**

	Δ income								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
deregulation × urban (1970)	0.323***	0.266***	0.289***						
	(0.065)	(0.069)	(0.061)						
deregulation × urbanized (1970)				0.402***	0.289***	0.237***			
				(0.088)	(0.080)	(0.079)			
deregulation × density (1970)							0.334***	0.242***	0.117
							(0.085)	(0.082)	(0.078)
Observations	108972	108972	108936	108972	108972	108936	108972	108972	108936
Correlation w urban score	.713	.713	.713	.622	.622	.622	.723	.723	.723
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	C*Y	C*Y	S*Y	C*Y	C*Y	S*Y	C*Y	C*Y	S*Y
Controls		✓	✓		✓	✓		✓	✓

Note: The baseline sample includes 3,027 counties in 49 states from 1971 to 2006. Washington, D.C. is omitted with the inclusion of state*year fixed effects reducing the sample to 3,026 counties. The dependent variable is county income growth. *deregulation* equals one for all years in which a state permits in-state banking. In columns (1) to (3) the *urban* dummy equals one if a county's population share living inside urban areas as defined by the 1970 census is within the top tercile. In columns (4) to (6) the *urban* dummy equals one if a county's 1970 population share living inside urbanized areas is within the top tercile. In columns (7) to (9) the *urban* dummy marks counties in the top tercile of population density as of 1970. For all three alternative measures, the correlation between the continuous variable and the urban score is shown. All regressions include county and cohort*year or state*year fixed effects, and baseline county controls as indicated. Standard errors clustered on the state-level. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: **The impact of deregulation on rural and urban incomes p.c.**

	Δ income p.c.					
	(1)	(2)	(3)	(4)	(5)	(6)
deregulation	0.766***	0.722**				
	(0.206)	(0.294)				
deregulation × urban		0.222***	0.200***	0.123***	0.164***	0.100**
		(0.062)	(0.058)	(0.044)	(0.056)	(0.042)
Observations	69621	108972	108972	108936	108972	108936
County FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	C*Y	S*Y	C*Y	S*Y
Controls					✓	✓

Notes: The baseline sample includes 3,027 counties in 49 states from 1971 to 2006. Washington, D.C. is omitted with the inclusion of state*year fixed effects reducing the sample to 3,026 counties. The dependent variable is county income growth per capita. *deregulation* is a dummy with value one for all years in which a state permits in-state banking, and zero for all years before. Counties with an urban score above or equal to 0.5 are classified as *urban*. The estimate in column (1) shows the average treatment effect of the deregulation on deregulated counties, applying the robust DiD estimator of Callaway and Sant'Anna (2021) which estimates the effects of a binary treatment with staggered roll out while allowing for arbitrary heterogeneity and dynamic treatment effects. Columns (2) to (6) show the differential growth effect of the deregulation on urban relative to rural counties. All regressions in columns (2)-(6) include county and year, cohort*year or state*year fixed effects, and baseline country controls as indicated. The coefficient on *urban* and *deregulation* (in columns (3)-(6)) is absorbed by the fixed effects. Standard errors clustered on the state-level. *** p<0.01, ** p<0.05, * p<0.1..

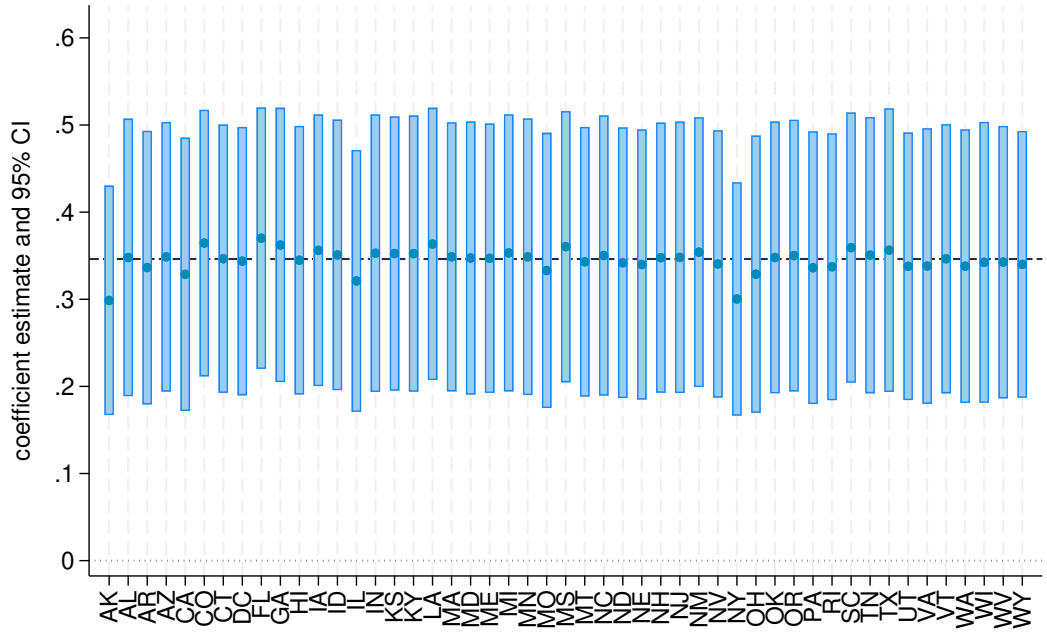
Table A5: **Different definitions of deregulation**

	Δ income								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
deregulation (earlier) × urban	0.303***	0.285***	0.240***						
	(0.085)	(0.079)	(0.074)						
deregulation (intra) × urban				0.195**	0.170**	0.160**	-0.106	-0.098	-0.042
				(0.085)	(0.080)	(0.067)	(0.166)	(0.162)	(0.152)
deregulation (inter) × urban							0.345**	0.303**	0.238*
							(0.142)	(0.139)	(0.136)
Observations	108972	108972	108936	108972	108972	108936	108972	108972	108936
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	C*Y	C*Y	S*Y	C*Y	C*Y	S*Y	C*Y	C*Y	S*Y
Controls		✓	✓		✓	✓		✓	✓

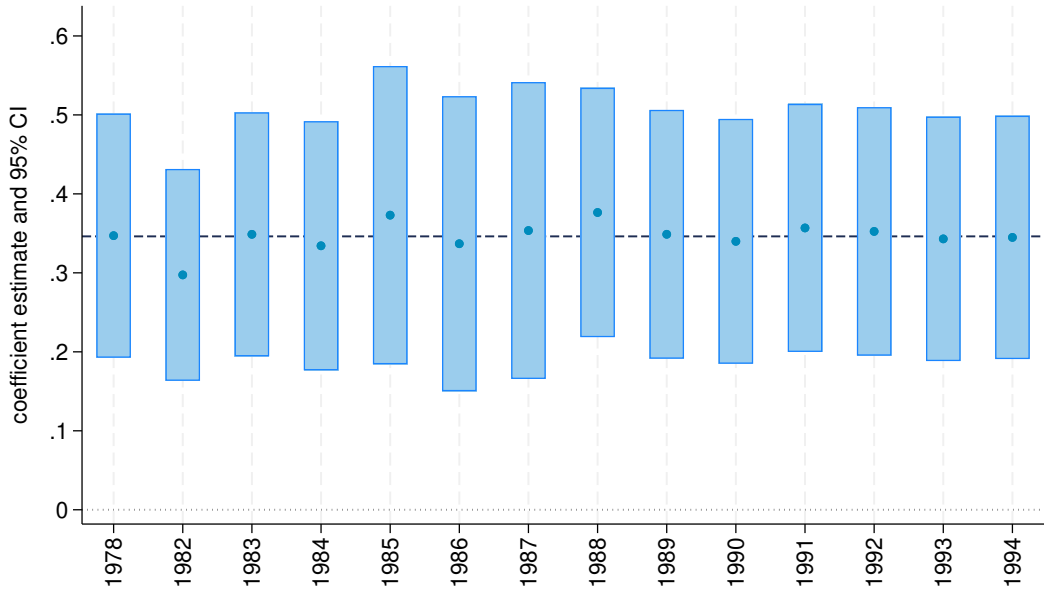
Note: The baseline sample includes 3,027 counties in 49 states from 1971 to 2006. Washington, D.C. is omitted with the inclusion of state*year fixed effects reducing the sample to 3,026 counties. Counties with an urban score above or equal to 0.5 are defined as *urban*. In columns (1) to (3) *deregulation (earlier)* equals one for all years in which a state permits intrastate branching or interstate banking. Cohorts for cohort*year fixed effects are defined based on the earlier date of intra- or interstate banking permission. In (4) and (6) *deregulation (intra)* equals one after a state allows intrastate branching. Cohorts for cohort*year fixed effects are defined based on the date of intrastate banking deregulation. In (7) and (9) dummies for both, intra and interstate banking deregulation are included. Cohort*year fixed effects are included for both, intra- and interstate banking deregulation state cohorts. All regressions include county and cohort*year or state*year fixed effects, and baseline county controls as indicated. Standard errors clustered on the state-level. *** p<0.01, ** p<0.05, * p<0.1.

Figure A2: **Excluding individual states / cohorts**

(a) Excluding states



(b) Excluding deregulation cohorts



Note: Coefficient plot of the baseline regression with county and cohort*year fixed effects when leaving out individual states (panel a)) or groups of states based on their year of deregulation (panel b)). Each bar represents the coefficient estimate and 95% confidence interval of the regression where the respective state (cohort) has been excluded.

Table A6: **The impact of deregulation on rural and urban incomes (Hawaii not deregulated for the entire sample period)**

	Δ income					
	(1)	(2)	(3)	(4)	(5)	(6)
deregulation	1.247***	0.972***				
	(0.245)	(0.281)				
deregulation × urban		0.395***	0.345***	0.226***	0.284***	0.206***
		(0.092)	(0.077)	(0.063)	(0.067)	(0.059)
Observations	108972	108972	108972	108936	108972	108936
County FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	C*Y	S*Y	C*Y	S*Y
Controls					✓	✓

Notes: Notes: The baseline sample includes 3,027 counties in 49 states from 1971 to 2006. Washington, D.C. is omitted with the inclusion of state*year fixed effects reducing the sample to 3,026 counties. The dependent variable is county income growth. *deregulation* is a dummy with value one for all years in which a state permits in-state banking, and zero for all years before. Hawaii is kept as not deregulated for the entire sample period. Counties with an urban score above or equal to 0.5 are classified as *urban*. The estimate in column (1) shows the average treatment effect of the deregulation on deregulated counties, applying the robust DiD estimator of Callaway and Sant'Anna (2021) which estimates the effects of a binary treatment with staggered roll out while allowing for arbitrary heterogeneity and dynamic treatment effects. Columns (2) to (6) show the differential growth effect of the deregulation on urban relative to rural counties. All regressions in columns (2)-(6) include county and year, cohort*year or state*year fixed effects, and baseline country controls as indicated. The coefficient on *urban* and *deregulation* (in columns (3)-(6)) is absorbed by the fixed effects. Standard errors clustered on the state-level. *** p<0.01, ** p<0.05, * p<0.1.

Table A7: **The impact of deregulation on rural and urban incomes p.c.**

	Δ Income (trimmed)				Δ Income (not wins.)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
deregulation × urban	0.355***	0.235***	0.293***	0.214***	0.336***	0.223***	0.274***	0.202***
	(0.076)	(0.061)	(0.065)	(0.057)	(0.077)	(0.065)	(0.068)	(0.061)
Observations	108972	108936	108972	108936	108756	108720	108756	108720
County FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	C*Y	S*Y	C*Y	S*Y	C*Y	S*Y	C*Y	S*Y
Controls			✓	✓			✓	✓

Notes: The baseline sample includes 3,027 counties in 49 states from 1971 to 2006. Washington, D.C. is omitted with the inclusion of state*year fixed effects reducing the sample to 3,026 counties. The dependent variable is county income growth trimmed at the 1st and 99th percentile (columns (1)-(4)) or not winsorized or trimmed (columns (5)-(8)). *deregulation* is a dummy with value one for all years in which a state permits in-state banking, and zero for all years before. Counties with an urban score above or equal to 0.5 are classified as *urban*. All regressions include county and cohort*year or state*year fixed effects, and baseline country controls as indicated. The coefficient on *urban* and *deregulation* is absorbed by the fixed effects. Standard errors clustered on the state-level. *** p<0.01, ** p<0.05, * p<0.1.

B Appendix: Mergers and acquisitions

Section 3.3.3 establishes that mergers and acquisitions were more frequent in cities relative to rural areas following the deregulation. Why were banks in urban areas more often the target of an acquisition? In Table A8, we compare the characteristics of acquired banks to banks not involved in a merger, before and after the deregulation, while accounting for any state and time fixed effects.[30] Pre-deregulation, the set of potential acquisition targets was highly constrained due to the restrictions imposed and the bank types acquired do not necessarily represent the acquirer's optimal choice. Following the liberalization of entry restrictions, the characteristics of acquired banks should more closely reflect what acquiring banks consider to be an attractive target.

The results in Table A8 column (1)-(3) indicate that acquired banks tend to be larger than no-merger banks, especially after the removal of interstate banking restrictions. Post deregulation, the average acquired bank operated five more offices (column (1)), maintained branches in one more county (column (2)) and its deposits were 21% larger relative to no-merger banks. Comparing banks in terms of profitability in column (4), there is no statistically significant difference in the return of equity of acquired and no-merger banks. Hence, bigger banks that maintained an already larger branch network, which grants access to potentially larger markets seem to be a preferred acquisition target, while profitability plays a limited role.[31]

How do these "desired acquiree characteristics" compare to the average bank in rural vs. urban counties at the onset of the removal of interstate banking restrictions? Table A9 reports the results for regressions of bank size and profitability measures on our urban dummy variable. Prior to deregulation, the average bank in cities had more offices (column (1)), the bank holding company was active in more counties (column (2)) and more deposits were held at the bank (column (3)). At the same time, we do not observe a statistically significant and economically meaningful difference in deposit-weighted average

[30] We combine bank-level data on bank size and profitability with the data on mergers and acquisitions. To proxy bank size we compute the number of offices that a bank maintains, the number of counties that a bank's branch network spans and the log bank deposits from the FDIC SOD. Profitability is measured as the annual average of quarterly return on equity which we compute from bank's Call Reports. Acquired banks are the target of an acquisition in a given year. Acquisitions due to bank failure are excluded. Banks that were not involved in a merger were neither acquired nor acquiring during the entire sample period and serve as the control group.

[31] The findings by Stiroh and Strahan (2003) confirm the pattern of many large banks being acquired post interstate banking deregulation.

bank profitability in rural vs. urban counties before the deregulation (column (4)).

Being slightly larger at the onset of the banking deregulation could hence be one additional factor why banks in urban counties constituted potentially preferable acquisitions targets, once restrictions on mergers and acquisitions across state borders were removed.

Table A8: **Characteristics of acquired banks in comparison**

	(1) bank offices	(2) counties active	(3) log deposits	(4) ROE (%)
Post deregulation,	-1.099***	-0.177***	-0.145***	0.290
	(0.214)	(0.044)	(0.022)	(0.293)
acquired	1.515***	0.097	0.361***	-1.024*
	(0.492)	(0.064)	(0.095)	(0.589)
Post deregulation, × acquired	5.212***	0.968***	0.213**	0.492
	(0.725)	(0.123)	(0.084)	(0.416)
Observations	120187	120187	120187	120187
R^2	0.039	0.047	0.231	0.036
State FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Note: This table compares characteristics of acquired banks to banks not involved in a merger, before and after the bank deregulation. The sample includes 16,234 banks in 49 states from 1983 to 2006. *deregulation* is a dummy with value one for all years in which a state permits in-state banking, and zero for all years before. *acquired* is a dummy with value one for banks that were the target of an acquisition in a given year, and zero for banks that were not part of a merger during the entire sample period. Mergers due to bank failure are excluded. 10,236 banks were acquired in the sample. 5,998 banks were neither acquiring nor acquired and serve as the control group. Columns (1)-(3) compare the bank size of acquired vs. non-merging banks before and after the deregulation, as proxied by the number of bank offices that the bank maintains, the number of counties that the bank's branch network spans and its log deposits. Column (4) looks at bank profitability as measured by the average quarterly return on equity. All regressions include state and year fixed effects. Standard errors are clustered at the state-level. *** p<0.01, ** p<0.05, * p<0.1.

Table A9: **Bank characteristics in rural vs. urban counties pre-deregulation**

	(1) bank offices	(2) counties active	(3) ln(dep/bank)	(4) ROE (%)
urban	3.845***	4.841***	0.875***	0.129
	(0.686)	(1.557)	(0.076)	(0.141)
Observations	11229	11229	11229	11229
Year FE	✓	✓	✓	✓

Note: The sample includes 2,893 counties in 44 states in the years before a state permits in-state banking. Data on ROE starts in 1983, hence counties in AK, CT, MA, ME and NY where deregulation occurred before or in 1983 are not included. Counties with an urban score above or equal to 0.5 are classified as *urban*. Columns (1)-(3) look at the relationship between a county's degree of urbanity and the average size of its local banks. Column (1) focuses on the number of bank offices the average bank maintains in the respective county. Column (2) proxies bank size with the average number of counties in which a local bank holding company operates. Column (3) looks at average log deposits per bank. Column (4) studies bank profitability as measured by the deposit-weighted average return on equity of a county's banks. All regressions include year fixed effects. Standard errors clustered on the state-level. *** p<0.01, ** p<0.05, * p<0.1.

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