The Effect of Branching Deregulation on Finance Wage Premium*

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Abstract

How does financial deregulation contribute to the rising finance wage premium in the US? This study uses the Interstate Banking and Branching Efficiency Act of 1994 as an exogenous shift to local banking markets and investigates the effect of branching deregulation on relative wages in finance. We find that the finance wage premium increased significantly in deregulated states, driven by direct effects on wages in commercial banking and spillover effects on the broader financial industry. Our estimates suggest that the deregulation explains approximately a quarter of the increase in the finance wage premium between 1994 and 2008.

JEL classification: G21, G28, J24, J31, J33, M52

Keywords: Banking deregulation; Finance wage premium; Competition; Rent sharing; Skill-biased technological change

1 Introduction

The financial industry has grown dramatically since the 1980s. This was accompanied by a sharp increase in compensation in finance, in particular for skilled labor (e.g., Philippon and Reshef (2012), Bell and Van Reenen (2014), Boustanifar et al. (2018)). This increase in the finance wage premium coincided with two equally dramatic global trends: financial deregulation and advances in information technologies. Philippon and Reshef (2012) show that deregulation was followed by increases in education, job complexity, and wages in the financial industry. Kaplan and Rauh (2010) observe a dramatic increase in the amount of capital per employee in US securities firms over the last decades. Célérier and Vallée (2019) demonstrate that advances in information technology allowed skilled individuals to handle larger value projects.

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Previous studies suggest an interplay of financial deregulation and advances in information technology giving rise to the finance wage premium. However, the evidence presented is largely correlational. In contrast, we propose an identification strategy to isolate the effect of deregulation on the finance wage premium from other confounding trends. We do this by exploiting the US interstate branching deregulation as an exogenous shift to local banking. More specifically, we study the effect of the Interstate Banking and Branching Efficiency Act (IBBEA) on relative wages in banking and finance. We show that branching deregulation increased wages in the financial industry compared to the rest of the economy. The first and foremost beneficiary of this increase were commercial banks: interstate branching expansion provided direct efficiency gains for them. Relative employment in the financial industry, on the other hand, experienced a decline after deregulation.

The IBBEA was adopted in 1994 and made interstate branching legal. However, states retained the right to erect several barriers to prevent out-of-state banks from opening or acquiring branches. States chose to restrict or allow interstate branching at different degrees and in different years leading to a staggered and varying adoption of deregulation.[1] We use the time-varying index created by Rice and Strahan (2010) which tracks this staggered implementation of barrier removals across states. To estimate the effect of deregulation on relative wages and employment in the financial industry, we implement an imputation strategy suggested by Borusyak et al. (2024). This estimator ensures "correct" comparisons of treatment and control groups in the case of staggered event studies. We use average payroll and employment data from County Business Patterns between the years 1990 and 2008.

We find that the increase in the finance wage premium (the wage differential between the financial industry and the rest of the economy) is 5.5 percentage points higher in deregulating states than in non-deregulating states. This increase comes from two sources: i) a direct effect of interstate branching deregulation on wages in commercial banking and, ii) spillover effects of deregulation on the rest of the financial industry. The effect of deregulation is especially strong for commercial banks. For those, we also observe a relative decline in employment compared to the rest of the economy. We find that the finance wage premium increases after deregulation when we conduct the analysis both at the county and at the state level. The result is also robust

[1] We refer to deregulation also as treatment.

to different sampling choices. Our estimation using individual data qualitatively confirms these findings.

In addition, we explore various dimensions of heterogeneity that may influence how deregulation affects relative wages and employment in the financial industry. Our findings indicate that counties with a high share of employment in the finance sector in 1990 experienced stronger growth in the finance wage premium following deregulation. The effect is also stronger in counties with a higher concentration of skilled individuals within the financial industry and in counties that experience higher house price increases after deregulation.

We evaluate these findings through the lens of a stylized Cournot model of product and labor market competition with heterogeneous productivity. Our framework highlights three channels i) labor market competition, which predicts an increase in wages and employment, ii) product market competition, which predicts an increase in employment but may either raise or reduce wages based on the wage-setting regime; and iii) productivity channel, which predicts expansion of larger, more efficient banks increase wages and may decrease employment.

Our results are mostly consistent with the productivity channel: interstate branching deregulation enabled large and productive commercial banks to exploit benefits of geographic diversification and to share these benefits with their employees in terms of higher wages. Skill biased technological change further amplified these effects for skilled individuals in finance. Rent sharing and product market competition in the financial industry created additional spillover gains: employees in the rest of the financial sector enjoyed wage increases stemming from mortgage-driven house price increases and from competition/innovation in financial products such as securitization.

Our paper closely relates to the literature on the causes and consequences of the rising finance wage premium in developed economies. Philippon and Reshef (2012) investigate the long-term trends in wages and human capital in the US finance industry. They find that the finance wage premium increased significantly since the beginning of the 1980s and that this was accompanied by equivalently dramatic changes in financial deregulation. Boustanifar et al. (2018) provide evidence on the positive relationship between the finance wage premium and

financial deregulation at the international level. The subsequent literature tries to explain the rising finance wage premium by changes in the return to talent (Célérier and Vallée, 2019), higher rents (Böhm et al., 2023), and high ICT capital and skill complementarity (Bertay et al., 2022) in the financial industry. We complement these papers by using the IBBEA as a quasi-natural experiment to investigate the effect of deregulation on relative wages in banking and the financial industry. Our findings provide a clean identification of how the growth in the finance wage premium was driven by a combination of banks' efficiency gains through branch and credit expansion across states, and the spillover effects of this to the rest of the financial industry.

Our estimates should be interpreted as evidence on the relative price and allocation of finance labor after interstate branching deregulation. The IBBEA reshaped local banking competition and branch supply, with documented effects on credit access, innovation, household financial inclusion, housing markets, and income distribution. We complement our baseline estimates with first-order banking-market effects and situate our findings within the broader literature on the consequences of bank deregulation.

The existing evidence on the real effects of inter-state banking deregulation is more nuanced than the broader branching-deregulation literature. Aggregate state-level regressions do not show robust income-growth effects for interstate banking reform (Strahan et al., 2003; Taşkın and Yaman, 2023), but other studies document meaningful microeconomic and distributional consequences, including greater financial inclusion (Célerier and Matray, 2019), stronger mortgage credit and house-price responses (Favara and Imbs, 2015), higher productivity among financially constrained firms (Krishnan et al., 2015), and wider spatial divergence between urban and rural areas (Beck and Doerr, 2023). Our contribution is to show how one important part of this broader adjustment occurred through the relative compensation of finance workers.

Our paper further relates to the growing literature that investigates how employer concentration in labor markets affects wages and employment. Azar et al. (2022), Benmelech et al. (2022) and Qiu and Sojourner (2023) find negative effects of local labor market concentration on worker compensations. Prager and Schmitt (2021) document a reduction in employee wages after hospital mergers if the change in concentration is significant and only for workers with industry-specific skills. Arnold (2021) exploits merger-induced changes in concentration in the

US and finds negative effects of increased labor market concentration on employment overall and on earnings, but only when the increase in concentration is large. We use this literature to distinguish the mechanism through which interstate branching deregulation affects wages and employment in the banking and financial industry.

Finally, this paper contributes to the topic of the relationship between finance, growth (Jayaratne and Strahan, 1996) and more specifically inequality (Demirgüç-Kunt and Levine, 2009). Previous studies investigated the branching deregulation wave starting in the 1970s and analyzed the effect on income inequality within states (Beck et al., 2010), on the skill premium (Jerzmanowski and Nabar, 2013), and on gender (Black and Strahan, 2001) and race (Levine et al., 2014) inequality. While our study does not specifically study the effects of the IBBEA on wage inequality, our finding of an increasing finance wage premium would have direct implications for rising income inequality between industries and locations.

The rest of the paper is organized as follows. Section 2 provides a brief history of branching deregulation that led to the IBBEA of 1994 and the literature that discusses its effects. Section 3 explains theoretical channels through which interstate branching deregulation could affect wages and employment in the banking and financial industry. Sections 4 and 5 introduce the data used in our analysis and our identification strategy that addresses the concerns related to staggered event studies. Section 6 presents results of our estimations with county, state and individual level data. Section 7 discusses the role of different channels in explaining the effect of deregulation on the finance wage premium. Finally, Section 8 concludes.

2 Financial Deregulation in the USA

Restrictions on bank branching in the United States can be traced back to the McFadden Act of 1927, which granted states the authority to regulate the branching activities of national banks. These regulatory powers were further reinforced by the Bank Holding Company Act of 1956, which restricted banks from acquiring other banks or branches located outside their home state unless the state of the targeted bank permitted such acquisitions. These restrictions were supported by smaller and less efficient banks as they effectively shielded them from competition.

Prior to the 1970s, most states imposed stringent limitations on branching within and across state borders. However, over the subsequent decades, states gradually began to ease these restrictions on both intrastate and interstate banking. While the expansion of interstate banking commenced on a reciprocal basis, it wasn't until 1994 that banks were allowed to establish branches across state borders.[2]

The wave of branching deregulation finally culminated in federal legislation with the Riegle-Neal Interstate Banking and Branching Efficiency Act (IBBEA) of 1994, which effectively permitted all types of interstate branching expansion. While IBBEA lifted restrictions on bank expansion across state limits, it nonetheless gave states some discretion to limit the expansion of out-of-state banks. The act allowed for mainly four provisions to limit bank expansion: 1) setting a minimum age of the target institution for bank acquisitions, 2) prohibiting de-novo branching by out-of-state banks, 3) setting a cap on the state-wide deposit concentration that a merger with an out-of-state bank would create, and 4) prohibiting acquisition of bank branches by an out-of-state bank. The IBBEA stipulated a trigger date of 1997 until when the states had to legislate whether to on opt into any of these regulatory provisions. However, states also retained the right to lift or modify the aforementioned provisions after 1997.

While the IBBEA created a federal framework for interstate branching, states retained and exercised substantial discretion through the four aforementioned barriers. These barriers largely reflected pre-existing state banking rules and the long-run political economy of entry protection. The timing of branching deregulation was shaped largely by institutional and political factors rather than by short-run growth prospects: Kroszner and Strahan (1999) show that adoption reflects the relative strength of likely winners such as large national banks and losers such as small protected banks. We therefore view IBBEA implementation as a policy-induced shift in banking competition whose timing predated, and was plausibly orthogonal to, the finance labor-market outcomes we study.

Subsequently, states exercised and later removed these restrictions over time which gave rise to waves of interstate branching deregulation. Deregulation after the IBBEA thus took the form

[2]Intrastate banking refers to the ability of banks to open statewide branches. Interstate banking refers to the practice of bank holding companies to operate across state borders. Interstate branching means that a single bank entity may operate branches across several states without further corporate structure.

of staggered implementation across states and years, and deregulating states could choose to relax any combination of the above restrictions. The details of this process are described in Rice and Strahan (2010) who construct a simple index ranging from 0 to 4, measuring the number of restrictions that a state has in place in a given year to suppress banking competition. Table 1 reports the evolution of interstate branching deregulation across states and years.[3] Before the IBBEA came into effect no state had deregulated interstate branching. However, between 1994 and 2005, 43 states took steps to deregulate their interstate branching laws, out of which 39 of them already implemented a deregulation by 1997.

Previous articles investigating the effect of the IBBEA find that deregulated states experienced an entry of interstate bank branches that increased competition in local banking markets (Johnson and Rice (2008), Célerier and Matray (2019)). This competition increased credit supply leading to lower interest rates (Rice and Strahan, 2010), higher bank-to-firm lending (Keil and Müller, 2020), and more access to finance for low income households (Célerier and Matray, 2019). The IBBEA also encouraged the creation of geographically diverse banking institutions. Favara and Imbs (2015) show that commercial banks that operate across states increased mortgage supply in deregulated states while mortgage companies and credit unions did not. This increase in credit supply led to higher house prices especially in locations with inelastic housing supply. The economic size of the effect of deregulation is large: their estimates suggest that IBBEA deregulation alone explains more than half of the increase in mortgage loans and more than a third of the increase in house prices.

The increase in loan activity across state borders has changed the nature of competition in local banking markets: McGowan et al. (2024) point out that deregulation induced competition fosters securitization especially for mortgage loans. Chu (2018) demonstrates that after deregulation local community banks and non-bank lenders increase credit supply in commercial real estate markets. Finally, the changes in local mortgage lending and house prices have secondary effects on the banking industry: Flannery et al. (2022) find that banks operating in areas that experience housing booms become larger. What emerges from the literature on the deregulation of inter-state branching is that large commercial banks were the immediate beneficiaries. The rest of the financial sector benefited by extension, including through new financial instruments

[3] As in Célerier and Matray (2019) 0 denotes no deregulation and 4 denotes full deregulation.

Table 1: Deregulation index over time by state

	1994	1997	2005
Alabama	0	1	1
Alaska	2	2	2
Arizona	0	1	2
Arkansas	0	0	0
California	0	1	1
Colorado	0	0	0
Connecticut	0	3	3
Delaware	0	1	1
District of Columbia	0	4	4
Florida	0	1	1
Georgia	0	1	1
Hawaii	0	1	4
Idaho	0	1	1
Illinois	0	1	4
Indiana	0	4	3
Iowa	0	0	0
Kansas	0	0	0
Kentucky	0	0	1
Louisiana	0	1	1
Maine	0	4	4
Maryland	0	4	4
Massachusetts	0	3	3
Michigan 26	0	4	4
Minnesota 27	0	1	1
Mississippi 28	0	0	0
Missouri	0	0	0
Montana 30	0	0	0
Nebraska 31	0	0	0
Nevada 32	0	1	1
New Hamphsire 33	0	0	4
New Jersey 34	0	3	3
New Mexico 35	0	1	1
New York 36	0	2	2
North Carolina 37	0	4	4
North Dakota 38	0	1	3
Ohio 39	0	4	4
Oklahoma 40	0	0	3
Oregon 41	0	1	1
Pennsylvania 42	0	4	4
Rhode Island	0	4	4
South Carolina	0	1	1
South Dakota	0	1	1
Tennessee	0	1	3
Texas	0	0	3
Utah	0	2	3
Vermont	0	2	4
Virginia	0	4	4
Washington	0	1	3
West Virginia	0	3	3
Wisconsin	0	1	1
Wyoming	0	1	1

Source: Johnson and Rice (2008) and Rice and Strahan (2010).

such as securitization and through house price growth.

3 Theoretical Background

How does deregulation of interstate branching affect wages and employment in banking and finance? We outline a static Cournot model of competition in labor and product markets, similar in spirit to Arnold (2021) and Thoresson (2024), and relate the impact of deregulation induced changes in competition and productivity to labor market effects.

3.1 Stylized Model

There are N firms hiring in the market, indexed by $i = 1, \ldots, N$. Total employment, L, consists of variable labor $V = \sum_i v_i$ and fixed labor $F = \sum_i f_i$, where v_i and f_i are firm i's variable and fixed employment, respectively.[4] Firms choose variable labor through the process of profit maximization and take fixed labor as a decreasing function of productivity A_i.[5] For a given productivity A_i, firms maximize the following profit function:

$$\pi_i = P(Q)\, q_i(A_i, v_i) - w_V(V) v_i - w_F f_i,$$

where $Q = \sum_i q_i$ is total loan output, $P(Q)$ is price. q_i increasing in both productivity and variable labor:

$$\frac{\partial q_i}{\partial A_i} > 0, \quad \frac{\partial q_i}{\partial v_i} > 0.$$

Firms take labor supply as upward sloping and anticipate competitors' choices in both labor and product markets. The wage for variable labor is characterized by:

$$w_V \;=\; \frac{\varepsilon_V}{HHI_V + \varepsilon_V}\, \Omega_V, \tag{1}$$

where ε_V is the labor supply elasticity (of variable labor), HHI_V the Herfindahl index of employer concentration, and Ω_V the employment-weighted average marginal revenue product of variable labor (MRPL). The first term, $\frac{\varepsilon_V}{HHI_V + \varepsilon_V}$, captures the markdown of wages from imperfect competition in labor markets. The second term includes i) marginal revenue of the firm including product-market markup determined by HHI_p and demand elasticity, and ii)

[4] Fixed labor can be thought as overhead labor necessary for a firm's operation but not directly productive. Variable labor corresponds to productive employees who generate revenue.

[5] This captures the idea that firms with high productivity may pool resources more efficiently and do not need overhead labor as much.

marginal product of labor that also contains productivity A_i.[6]

This structure allows us to link three distinct channels of deregulation induced branch expansion with wages and employment in finance. These are 1) labor market competition, 2) product market competition and 3) productivity channels. Moreover, we also discuss the role of skill biased technological change (SBTC) and rent sharing channels in combination with deregulation.

3.2 Channels of Deregulation

Labor market competition The labor market power (summarized by (HHI_V)) enables employers to impose a markdown to competitive wages (See evidence in Benmelech et al. (2022) and Thoresson (2024)). A fall in HHI_V reduces this markdown, **raising wages and employment**. In the context of IBBEA deregulation, this would require an increase in the number of banks and branches operating in deregulated locations which diminish banks' local labor market power.

Product market competition Firms with product market power impose a markup on prices and restrict output. Equation 1 implies that a decrease in product market concentration (keeping production technology,A_i, fixed) results in an increase in employment and (under wage posting) in wages as they do in the case of a decline in labor market concentration. If wages are determined through bargaining (see below), under fixed productivity, higher product competition may lower profits per worker, which results in lower wages. Henceforth, absent productivity channel, product market competition predicts **an increase in employment** and it does not have a clear prediction on wages. The literature emphasizes that the IBBEA promoted competition in credit markets: deregulated states experienced an increase in the amount of credit and lower interest rates.[7] This increased credit activity promotes further competition in the rest of the financial industry through linkages of commercial banks with investment banks and insurance companies. Therefore, the product market competition channel also predicts **spillover effects** to the rest of the financial industry.

[6]More formal treatment of the role of product and labor market competition on wages may be found in Deb et al. (2024).

[7]See evidence in Rice and Strahan (2010), Keil and Müller (2020), Favara and Imbs (2015), Chu (2018), Célerier and Matray (2019).

Productivity The model includes a labor-augmenting productivity parameter A_i for variable labor. An increase in productivity would yield higher profitability per unit of labor and higher wages. Variable labor increases as it does in competition channels and fixed labor decreases. If the decline in fixed labor dominates, then higher productivity would predict an increase in wages and a decrease in employment. In the case of IBBEA, deregulation allowed large and productive banks to operate across state lines. There is evidence that geographic expansion could foster diversification of services (Goetz et al. (2016)) and increase bank stability and profitability (Boyd and De Nicolo (2005) and Goetz (2018)). Moreover, Favara and Imbs (2015) argue that the IBBEA led to an increase in mortgage lending through out-of-state lenders that diversified geographically. Thus, the productivity channel predicts **rising wages** but potentially **lower total employment**.

Skill biased technological change While our model does not explicitly model skilled vs. unskilled labor, it still offers insights through which deregulation and skill biased technological change (Katz and Murphy (1992), Acemoglu and Autor (2011)) could interact.[8] In our model, firms choose variable labor for a given productivity. If SBTC raises the productivity of variable (skilled) labor, deregulation induced product-market competition would further yield an increase in wages and employment (of variable-skilled labor).

Rent sharing While our baseline model relies on posted wages, it is likely that (part of the) employees bargain their wages due to presence of *rent-sharing* in the financial industry.[9] In this case, wages would still rise following an increase in productivity. If higher product market competition yields lower profits per employee, this implies a decline in wages. Therefore, rent sharing provides important refinements to the wage effects of product market competition and productivity. In the case of the IBBEA, we could test the direct effect of rent sharing within the banking industry and indirect effect through the rest of the financial industry. The direct effect comes from the fact that commercial banks directly benefited from the removal of interstate

[8]SBTC provides an alternative channel that explains rising wage premium in finance. According to this view, advances in information and communications technology (ICT) disproportionately increased the productivity of skilled workers compared to unskilled workers. This is in line with the observation that the products offered by the financial industry and the tasks performed by financial workers became increasingly complex over time (Célérier and Vallée (2019)). Bertay et al. (2022) argue that ICT capital-skill complementarity explains most of the finance wage premium in the Netherlands.

[9]Financial industry is characterized by tight entry regulations (Black and Strahan (2001)), government guarantees (Coskun et al. (2025)) and opaque tasks (Biais and Landier (2020), Bolton et al. (2016)) that generate large agency rents. Literature finds that rent sharing in finance explains finance wage premium (Böhm et al. (2023)) and finance gender wage gap (Coskun et al. (2025)).

entry barriers compared to local community banks.[10] The entry of new commercial banks to deregulated states would decrease the rents enjoyed by local community banks, predicting **an increase in wages and employment** for commercial banks compared to credit unions and savings banks. In addition, interstate branching expansion due to deregulation promoted an expansion of mortgage credit which increased housing prices and spurred further financial activity such as securitization (Favara and Imbs (2015)). This indirect effect would create spillover benefits to the rest of the financial industry: employees in the financial industry would enjoy **higher compensation** due to increased rents in deregulated states.

4 Data

4.1 Wages and employment

Our main outcome of interest are wages and how they compare between the financial and non-financial sectors. We classify finance, insurance and real estate as the finance sector and all other industry classifications as the non-finance sector. To compute wages we use the County Business Patterns (CBP). The data cover the near-universe of establishments in the USA and are sourced from administrative records. As such the data are not subject to sampling error.In the data, an establishment is a single physical location where business activity is conducted. For each geography-industry cell, CBP reports the number of establishments, employment during the week of 12 March, first-quarter payroll, and annual payroll. Geography is assigned using physical location when available, although county assignment can be distorted in special cases such as employee-leasing arrangements or central payroll offices.[11] Because such measurement issues are primarily within-state geography issues, they are a more important concern for county-level than for state-level estimates. We retrieve payroll and employment variables for the years 1990 to 2008, separately for the finance and non-finance sectors. We restrict our attention to 2,039 counties which have available payroll and employment information throughout the sampling period. We define wage as total payroll divided by employment. The finance wage premium (or gap) is defined as the log wage paid by the finance sector less the log wage paid by the non-finance sectors, multiplied by 100 to scale to percentages.

[10] Favara and Imbs (2015) find that savings banks and credit unions do not experience an increase in mortgage lending in deregulated states.

[11] https://census.gov/programs-surveys/cbp/technical-documentation/methodology.html

While these calculations are transparent, they are not exact measures of our targets. The CBP are compiled from establishment data, and any establishment is assigned to one industry classification. A cleaner employed by a bank would thus also count as a finance employee. It is possible that deregulation for a sector affects wages of all employees regardless their function within a sector, but ideally we would want to categorize wages and employment by both sector and function (e.g., occupation). Unfortunately, the CBP do not permit this. While there would be non-finance employees within the finance sector, the reverse is less likely. We also note that if deregulation benefits only finance workers (but not non-finance workers within the finance sector), then we are likely to underestimate the effect of deregulation on this more narrowly defined finance wage premium.

4.2 Deregulation

Our main independent variable of interest is deregulation. We treat each of the four deregulation measures described in section 2 as binary. The deregulation index is the sum of those variables and ranges from 0 to 4. Since deregulation is determined at the state level, conducting the analysis at the state level would seem the natural choice. However, county-level analysis has some advantages. First, in exploring the channels and heterogeneity of deregulation effects, we can draw on a larger sample and differences across counties (e.g., the initial size of the finance sector) within a state. Second, with 51 states it is difficult to find a comparable control state for any deregulating state. On the other hand, a sample of more than 2,000 counties allows us to match a county in a deregulating state to a county with similar characteristics in a state which does not deregulate at the same time. We pursue such a matching-based strategy as a complement to our main analysis (see estimation strategy section).

Table 2 provides an overview of the wage premium and related measures over time and by counties which are never treated, not yet treated, or treated. In 1993 no state has yet deregulated (Alaska is the first state to deregulate in 1994), and most states which eventually deregulated had already done so by 1997. We observe that the share of employees working in finance increased from 6.9% to 7.4% in never treated counties, but stayed fairly stable in the remaining counties around 7.5%. The wage premium on the other hand increased from 1993

to 2005: by 12 log points in counties which never deregulated, and by close to 23 log points in counties which deregulated at some point. Our regression analysis outlined below estimates to what extent this growing differential in the finance wage premium can be attributed to deregulation.

Table 2: Employment and finance wage premium by treatment status

		1993	1997	2005
Number of counties	Never treated	412	412	412
	Not yet treated	1,627	279	0
	Treated	0	1,348	1,627
Share employed in finance (percent)	Never treated	6.9	6.6	7.4
	Not yet treated	7.5	6.3	n.a.
	Treated	n.a.	7.3	7.6
Finance wage premium (log difference)	Never treated	23.4	26.8	35.6
	Not yet treated	22.6	28.5	n.a.
	Treated	n.a.	32.4	45.4

Note: The table shows the number of counties which are never treated, not yet treated, and treated, by year. It also shows the share of employees in finance, and the finance wage premium for those counties (weighted by number of employees).

4.3 Banks, branches and deposits

We compute the number of banks, branches and total deposits in a county using the Summary of Deposits (SOD). The SOD are annual data where each row is a bank branch of an FDIC insured institution. All institutions with branch offices are required to submit deposit information at the branch level. The data also identify the location of the bank headquarters of a branch, allowing us to classify a bank branch as being in-state or out-state. In particular, we generate the following variables at the county-year and the in-state or out-state level: The number of distinct banks, the number of branches and the total holdings of deposits. The SOD are publicly available from 1994 onward. Our analysis of branches and deposits therefore draws on a smaller set of not / not-yet treated observations (see section 5).

4.4 Additional variables

We supplement the above information with county level population growth and per capita income, both obtained from the Bureau of Economic Analysis (BEA). We further add the following data for exploring treatment heterogeneity and the channels by which deregulation might have

affected the finance wage premium: 1) the share of the finance sector in total county payroll in 1990, 2) housing supply elasticity estimates for Metropolitan Statistical Areas (MSA) measured by Saiz (2010), 3) the 1990 share of college educated employees in the finance sector, and 4) the 1990 share of IT employees in the finance sector. The last two variables are computed from the 5% sample of the 1990 census which identifies unique counties only if their population is at least 100,000. The housing supply elasticity is available only for MSAs. Consequently, the analyses using these data include only counties for which this information is available.

4.5 Individual level data

For the individual-level analysis we use the March Supplements from the Current Population Survey (CPS) from 1991 to 2008. The data contain information on annual income, employment status, number of weeks worked and industry/occupation. Households report this information about their labor market activity for the previous year. The data also include detailed demographic characteristics such as education, age, race, and gender. This enables us to control for individual changes. We use individual wage and employment information. We focus on full time civilian employees between ages 20 and 65. We restrict our sample to individuals who have worked at least 48 weeks over the previous year with at least 30 hours per week. We also drop self employed individuals and workers without pay. We compute the weekly wage as annual earned income divided by the number of weeks worked.

A practical limitation of the CPS is topcoding, which matters in our context because wage gains due to deregulation are likely concentrated among high earners. Over our sample period the Census Bureau changed its procedure during our sampling period and neither procedure conditions on the state of residence (Larrimore et al. (2008)). In addition, wage salary earnings variable in CPS is comprised of two components with separate topcoding. Because our identification compares wages in finance and non-finance across states, top-coding can distort the right tail (especially important for the college educated group) both across states and over time. We provide suggestive tests in the form an indicator for high wages to attenuate this problem.

5 Estimation Strategy

We exploit the staggered adoption of deregulation across states and over time to arrive at an average treatment on the treated (ATT) effect. That is, we provide an answer to the question: By how much does the wage of someone working in finance increase (or decrease) compared to someone not working in finance as a result of banking deregulation k years ago? Our target estimand is thus

$$ATT(k) = E(Y(1,k) - Y(0))$$

We adopt the potential outcome notation: $Y(1,k)$ is the outcome (the finance wage premium) k years after deregulation, and $Y(0)$ is the outcome if the county had never deregulated. $Y(0)$ is of course not observed for counties which deregulate. We are also interested in an aggregate treatment measure given by

$$ATT = \frac{\sum_{k=0}^{7} ATT(k)}{8}$$

Traditionally, staggered adoption cases have been modeled by two-way fixed effects (TWFE) models regressing the outcome on unit and time fixed effects as well as a treatment variable. To recover dynamic effects in event studies, lags and leads of the treatment can also be included in the empirical model. A recent literature shows that these models deliver a weighted average of pair-wise ATTs, but that the weights do not deliver a sensible average ATT unless treatment effects are homogeneous across both units and time.[12] The TWFE model estimates the treatment effect by comparing observations which switch from non-treatment to treatment status to observations whose treatment status remains unchanged over the same period - this includes observations which remain untreated, but also observations which started treatment at an earlier time and *remain treated*. Borusyak et al. (2024) refer to these latter comparisons as "forbidden" comparisons: they induce bias in the estimation of dynamic treatment effects. For example, De Chaisemartin and d'Haultfoeuille (2024) demonstrate that the dynamic effects of deregulation on house prices are much stronger than the TWFE model by Favara and Imbs (2015) suggest.

[12] See for example Goodman-Bacon (2021), de Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021), and Sun and Abraham (2021). de Chaisemartin and d'Haultfoeuille (2023) survey this literature in detail.

The cited literature suggests an intuitive solution: Estimate all ATTs separately by difference-in-differences estimation comparing a treated group to all not-yet-treated or never-treated control units, and then aggregate these ATTs to arrive at an average ATT. The pre-treatment period for treated observations is the period just before treatment. The researcher (rather than the estimator) chooses the weights at the averaging stage to compute the ATT of interest. While the details vary, most cited papers propose a variation on this solution as an unbiased treatment effect estimator.

Borusyak et al. (2024) suggest an alternative method. They propose to estimate unit and time fixed effects using only observations not (yet) under treatment, and then to infer what the outcome for treated observations would have been in the absence of treatment. In contrast to alternative methods, *all* untreated observations are used in the computation of unit and time fixed effects and of $Y(0)$. We thus choose this estimator on efficiency grounds. The difference between the observed outcome under treatment $Y(1)$ and the inferred no-treatment outcome $Y(0)$ is then the estimated treatment effect. We base our estimation on this alternative imputation approach, but in the appendix we also report results from TWFE event studies for comparison purposes. We make two assumptions:

Assumption 1 (No anticipation)**.**

$$Y_{it} = Y_{it}(0) \quad \textit{for all} \quad it \in \Omega_0$$

where $Y_{it}(0)$ is the outcome for county i in year t if it is never treated, and Ω_0 is the set of all observations not under treatment.

The assumption rules out that any (yet) untreated county has an outcome different from a scenario in which it is never treated. In particular, the untreated outcome is in no way affected by future treatment.

Assumption 2 (Parallel trends)**.**

$$E[Y_{it}(0)] = \alpha_i + \lambda_t + \beta X_{it}$$

where α_i is a fixed effect for county i, λ_t is a fixed effect for year t, and X_{it} is a vector of control

variables.

For any county this implies that its never-treatment outcome changes by $(\Delta\lambda_t + \beta\Delta X_{it})$. In particular, the never-treatment outcome changes by this amount, regardless of whether a county is treated. For example, this assumption rules out that states who saw a fast expansion of labor demand in finance were the first to deregulate. Under these two assumptions, we can estimate $ATT(k)$ as follows:

We estimate

$$Y_{it} = \alpha_i + \lambda_t + \beta X_{it} + u_{it}$$

via linear regression using only untreated observations to uncover β and, more crucially, α and λ. Our main outcome of interest is the finance wage premium, though we also estimate models with the finance employment premium as outcome.[13] We weight the estimation by total employment in a county so that counties affect the estimates of λ and β according to their size. We then compute the predicted outcome $(\hat{Y}_{it})$, both in-sample (for untreated observations) and out-of-sample (for treated observations). These predicted outcomes are estimates for $Y_{it}(0)$. The difference between observed outcomes Y_{it} and the predicted no treatment outcomes $(\hat{Y}_{it})$ is then our estimate of the treatment effect. Call this difference ATT_{it}. The average ATT_{it}, weighted by county i's employment in year t, among counties k years into their treatment is our estimated $ATT(k)$. We also obtain treatment effects for years before any treatment occurs which allows us to inspect potential violations of the no anticipation and the parallel trends assumption.

Borusyak et al. (2024) show that this estimator is consistent — even if the county fixed effects are not consistent due to the short time dimension. Finally, ATT is given by the weighted sum of $ATT(k)$ over an 8 year time horizon (k ranging from 0 to 7).[14]

States can have different deregulation 'intensities': they can choose any combination of the

[13] To be consistent with our terminology we define the finance employment premium in county i in year t as $100 * (\ln E_{it,finance} - \ln E_{it,non-finance})$.

[14] To avoid confusion, we note that weighting occurs twice. In the computation of county and year fixed effects and the effects of control variables we weight counties by their total employment. This ensures that the year fixed effects and the effects of control variables are simple averages across all employees. After obtaining treatment effects ATT_{it}, we aggregate them again by weighting by the counties' total employment to obtain the average treatment effect across all employees.

four deregulation measures described in section 2.[15] Exploratory analysis showed that treatment effects are unlikely to be additive. We therefore estimate the treatment effects for counties with different numbers of deregulations separately, and then aggregate the treatment effects to give us estimates of the effect *per treatment unit*. A deregulation index of two is thus not assumed to have twice the effect as a deregulation index of one.[16] We follow the procedure described in Borusyak et al. (2024) and implement a pre-treatment estimation strategy using leads of the deregulation index for untreated observations and recover the anticipation effect as per index point estimate. Finally, in cases in which a state deregulates twice (e.g., increasing or reversing a previous deregulation) we exclude the year of the second deregulation and all subsequent years for that state.

We compute bootstrapped standard errors clustered at the state level to reflect the fact that treatment is assigned at the state level. As a result of the small number of clusters (51) we expect confidence intervals to be wide. However, Abadie et al. (2020) discuss inference when uncertainty around estimation is not due to sampling, but results from imputing unknown quantities such as the potential never-treated outcome. They show that conventional standard errors will be too high. Our standard errors are thus conservative and likely to overstate the true uncertainty over our estimates.

We follow the procedure described in Borusyak et al. (2024) and implement a pre-treatment estimation strategy using leads of the deregulation index for untreated observations and recover the anticipation effect as per index point estimate. for the presence of anticipation or pre-existing trend differentials between treated and not (yet) treated observations.

We further support our pre-existing trend differentials test by preceding the estimation with a matching approach: Each deregulating county is matched to the closest county which never deregulates. We estimate the propensity to deregulate based on the growth in the finance wage premium and the growth in the finance employment premium over the five years before the first treatment or from 1990 to the year before first treatment (whichever is shorter).

[15] A fully flexible model of deregulation would view each combination as a different treatment, resulting in principle in $2^4 - 1$ varieties of treatment (even though only a subset of these treatments occur in reality). A fully restrictive model would assume that each treatment has the same effect for a given county and that the treatment effects are simply additive in the number of treatments.

[16] While our procedure assumes separate effects for each number of deregulation, we do not further distinguish between the four *types* of deregulation. For example, allowing de-novo branching and not setting a minimum age for bank acquisitions are assumed to have the same effect on outcomes.

By construction, the growth trend in the finance wage and the finance employment premia in deregulating states follows the growth trend of untreated counties to which they are matched. A never-treated county can be matched to several deregulating counties, and we weight these never-treated counties according to the number of times that they serve as a matched control observation.

Our regressions based on the CBP data control for counties' per capita income and for population growth. We also interact the share of the finance sector in total county payroll in 1990 with an annual trend. This controls for baseline trend differences across counties with different sizes of the finance sector before the passing of the IBBEA.

We follow a similar estimation strategy for our individual-level regressions based on the CPS. In the first stage we use observations in states which are not (yet) treated to estimate:

$$Y_{ist} = \alpha_s + \lambda_t X_i + \beta X_{st} + \epsilon_{ist} \tag{2}$$

Here, the dependent variable is either weekly log wage of individual i or an indicator of working in finance, in state s and year t. We control for a rich set of demographic factors that vary over time: gender-year and race-year fixed effects, triple interaction of four education and five age categories with year fixed effects (all captured by the interaction of year fixed effects with individual characteristics X_i). Finally, we control for state fixed effects, α_s and state level controls X_{st} such as population growth, per capita income, yearly average unemployment rate and 1990 payroll share of finance at the state level interacted with an annual trend. Based on our estimated coefficients we impute the counterfactual no-treatment outcomes for all observations and compute treatment effects as the difference between the actual and the counterfactual outcome.

6 Results

In this section we present our estimation results. The next section discusses the results in the framework of our theoretical discussion above and explores the mechanisms through which deregulation has affected labor markets.

6.1 Main Results

Figure 1 summarizes our main findings on the effect of interstate branching deregulation on the finance wage premium (left panel) and on the employment premium (right panel). The post-deregulation coefficients give the per-index-point effect (based on the Rice and Strahan (2010) index), while the pre-deregulation coefficients are estimated by fitting six annual leads of the deregulation index to the untreated sample.[17] Based on this construction, we find that, before the introduction of branching deregulation, states that took deregulation steps exhibit trends similar to those of states that did not. The graph suggests that after deregulation, counties in deregulated states begin to experience an increase in the finance wage premium relative to counties in states that do not deregulate. The estimated effect of deregulation rises to 3 percentage points per index point three years after deregulation and remains there in subsequent years. This means that, on average, the finance wage premium in a county is 3 percentage points higher per index point than in a scenario without any deregulation. The appendix contains the event-study graph based on the dynamic TWFE estimator, which produces comparable average effects of deregulation but suggests different dynamics: a decreasing finance wage premium before deregulation, and a reversal of this trend after deregulation (Figure A.3).

Table 3 reports the average effect of deregulation over the first seven years (average of years 0 to 7). Columns 1 and 2 of Panel A show that average wages in finance increase by close to 2 percent per index point after deregulation, while wages in the rest of the economy decline slightly (both statistically insignificant). Consequently, Column 3 of Panel A reports that the average effect of interstate deregulation on the finance wage premium amounts to 2.5 percentage points over the first seven years after deregulation.[18] Between 1994 and 2008, the average value of the deregulation index is 2.2. A simple back-of-the-envelope calculation based on this suggests that interstate branching deregulation increased the finance wage premium by 5.5 percentage points. This amounts to 25% of the increase in the finance wage premium between 1994 and 2008.[19]

In Figure 1, we also see that employment in finance relative to employment in the rest of the

[17] This departs from a conventional event study that normalizes the coefficient at t=-1 to zero. As Borusyak et al. (2024) and Roth (2026) point out, this construction interprets the leads as a direct test of parallel trends against the untreated counterfactual.

[18] Figure 1 shows that the effect of deregulation on the finance wage premium becomes insignificant after the fourth year; the smaller number of treated states observed at longer horizons contributes to this loss of precision.

[19] During the same period, the finance wage premium rose by 22 log points.

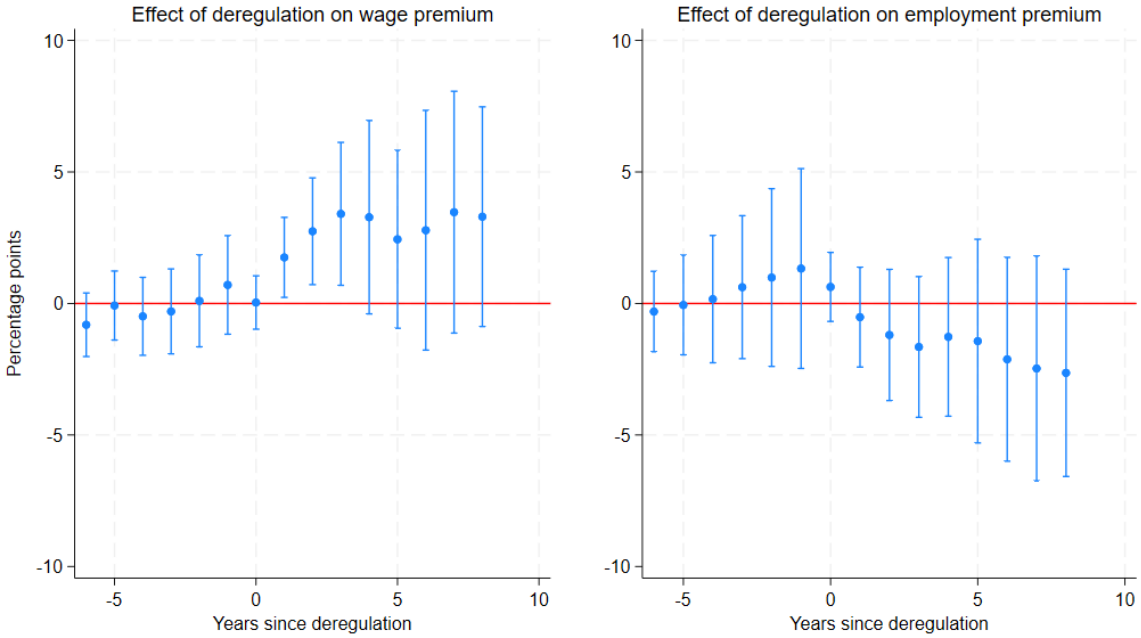

Figure 1: The figure plots event-study estimates of a relaxation of interstate branching by one index point where the event is the first time the state has ever lifted a restriction. The wage (employment) premium is the log ratio of wages (employment) in finance to average wages (employment) in the non-financial sector (multiplied by 100). All regressions include county and time fixed effects, county level population growth, per capita income and interaction of 1990 finance payroll share in that county with a time trend.

economy declines gradually, albeit at a slower pace and with a much larger standard error. This reflects a larger decline in finance employment than in non-finance employment in deregulated states (Table 3, Panel B). The finance employment premium falls by 1.2 percentage points per index point, but the effect is not statistically significant, following graphical evidence in Figure 1.

To understand what drives these wage and employment patterns, we first turn to the direct effects of deregulation on the structure of commercial banking, which is the immediate target of the IBBEA. Table 4 reports these effects at the state level. We find that while the total number of banks rises, though not significantly (Panel A, Column 1), the number of commercial bank branches experiences a strong and significant decline (Panel A, Column 2). Moreover, Panel B demonstrates that these changes mask a sharp compositional shift: the share of out-of-state banks increases by 2.1pp and the share of out-of-state branches by 6.6pp. The share of deposits held by out-of-state banks increases by 6.9pp. Thus, it appears that deregulation resulted in out-of-state banks replacing in-state banks and bank branches. In Panel C, we complement these results using state level establishment and employment information. The number of com-

Table 3: The effect of deregulation on relative wages and employment in finance

Panel A: Wages

	Finance	Non-Finance	Wage Premium
	(1)	(2)	(3)
Deregulation index	1.957	-0.524	2.481*
	(1.379)	(0.487)	(1.313)
Number of Counties	2,039	2,039	2,039

Panel B: Employment

	Finance	Non-Finance	Employment Premium	Employment Share
	(1)	(2)	(3)	(4)
Deregulation index	-2.284	-1.041	-1.243	-0.083
	(1.921)	(1.252)	(1.391)	(0.123)
Number of Counties	2,039	2,039	2,039	2,039

Notes: The coefficients report the estimated average effect of a relaxation of interstate banking by one index point from the year of deregulation through seventh year. Employment is calculated as 100 * log (county level employment). Wage is calculated as 100 * log (average wage). Wage (Employment) premium is calculated as wage (employment) in finance minus wage (employment) in the non-financial sector. Employment Share is calculated by 100 * (finance employment)/(total employment) All regressions include county and time fixed effects, county level population growth, per capita income and interaction of 1990 finance payroll share in that county with a time trend. We calculate bootstrapped standard errors clustered at the state level (reported in parentheses). *** p-value<0.01, ** p-value<0.05, * p-value<0.1.

mercial bank establishments declines by 10.4 percent per index point and employment by 5.7 percent.[20] This implies an increase in employment per establishment of 4.6 percent (though imprecisely estimated). These findings suggest that, following the IBBEA, commercial banking consolidates into fewer and larger establishments.

Following the first order effects of interstate branching deregulation, we now trace how relative wages and employment respond across commercial banking and the rest of the financial sector. We again rely on state level data, which offer two advantages over the county level analysis. First, it allows us to provide estimates for a nearly complete population, whereas data availability forced us to exclude some counties at the county level. Second, State Business Patterns provide more detailed information on industry classification with finer subcategories. Column 1 of Table 5 reports state level regressions on the finance wage premium (Panel A) and the on finance employment premium (Panel B). The effect of deregulation is almost identical to the county level analysis for wages which confirms that interstate deregulation increased the finance wage premium. Relative employment in finance is negative but insignificant as in the

[20] Compared to finance employment as demonstrated in Table 3, this highlights that deregulation first and foremost affected employment in commercial banking.

Table 4: The effect of deregulation on commercial banking and branching activity

Panel A: Banks, Branches, Deposits			
	Banks	Branches	Deposits
	(1)	(2)	(3)
Deregulation index	3.676	-6.627***	2.319
	(2.284)	(2.137)	(2.473)
Number of states	51	51	51

Panel B: Share of out-of-state banks in			
	Banks	Branches	Deposits
	(1)	(2)	(3)
Deregulation index	2.057***	6.589**	6.925**
	(0.672)	(3.325)	(3.390)
Number of states	51	51	51

Panel C: Establishments and Employment			
	Establishments	Employment	Employment per Establishment
	(1)	(2)	(3)
Deregulation index	-10.363*	-5.706*	4.643
	(5.865)	(3.428)	(3.278)
Number of states	51	51	51

Notes: The coefficients report the estimated average effect of a relaxation of interstate banking by one index point from the year of deregulation through seventh year. Banks (Branches) are measured 100 * log (number of banks (branches)) per state over time. Deposits are measured as 100 * log(sum of all deposits). Shares are in percent. Establishments (Employment) are calculated as 100 * log (state level establishments (employment)). Employment per establishment is 100 * log (employment / establishments) All regressions include state and time fixed effects, state level population growth, per capita income and interaction of 1990 finance payroll share in that state with a time trend. We calculate bootstrapped standard errors clustered at the state level (reported in parentheses). *** p-value<0.01, ** p-value<0.05, * p-value<0.1.

case of county level analysis. Panel C indicates that the relative size of establishments (log employment per establishment) in finance increases after deregulation, suggesting that employment in finance is reallocated toward larger establishments.

We next focus on commercial banking, the direct target and primary beneficiary of interstate deregulation. As expected, the wage premium in commercial banking relative to the non-financial sector is much higher after deregulation (Panel A, Column 3). Banking employment also declines relative to the non-financial sector, though this effect is not statistically significant (Panel B, Column 3). Commercial banks experience a notable increase in wages

relative to the rest of the financial industry, such as investment banks and insurance companies (Column 5); the increase relative to other credit institutions such as credit unions and savings banks (Column 4) is larger in magnitude but statistically insignificant. These wage gains are accompanied by consolidation in establishment size (Panel C). Commercial banks' employment per establishment rises by 5.4 percentage points relative to the non-financial sector (Column 3) and by 11.8 percentage points relative to other credit institutions (Column 4). The latter result suggests that commercial banking consolidates and replaces the local credit institutions as they expand across state borders.

6.2 Individual Analysis

Our main analysis with CBP does not allow us to control for individual characteristics that affect decisions to take a job in the financial industry or not. For instance, it is possible that in deregulated states the composition of the labor force between industries may have changed. More specifically, the share of skilled workers in the financial industry might have grown, together with a secular rise in the skill wage premium. We test (skilled) employment developments in finance and control for the contribution of compositional factors on finance wage premium using March Supplements of the Current Population Survey (CPS).

We allow that wages in finance and wages in the rest of the economy could get exposed to different impacts of individual controls and other state level factors. We estimate the equation 2 separately for the financial and non-financial industry and report the effect of deregulation on relative wages in finance in Table 6. The first column in Panel A reports the effect of deregulation on finance wage premium using a limited set of controls available in county and state level regressions reported in Table 5. We find that the effect is positive though the coefficient is insignificant. In the next column, we account for compositional effects by controlling for individual factors such as sex-year and race-year fixed effects together with detailed age-by-education-by-year cells. In this case, we find the effect of deregulation on finance wage premium to be 2.3 percentage points which is significant at the 10 percent level and close to the effect that we obtain using county and state level aggregate data. This suggests that the premium reflects higher pay for workers similar in skill, age and demographic characteristics.

Table 5: State level analysis

Panel A: Wage Premium					
	Finance vs.		Banking vs.		
	Non-finance	Non-finance (Ex. Banking)	Non-finance	Other credit inst.	Other finance
	(1)	(2)	(3)	(4)	(5)
Deregulation index	2.521*	1.677	7.005***	6.293	4.222**
	(1.341)	(1.243)	(2.626)	(4.088)	(1.981)
Number of states	51	51	51	51	51
Panel B: Employment Premium					
	Finance vs.		Banking vs.		
	Non-finance	Non-finance (Ex. Banking)	Non-finance	Other credit inst.	Other finance
	(1)	(2)	(3)	(4)	(5)
Deregulation index	-1.004	-0.377	-4.248	5.304	-5.385*
	(1.375)	(1.654)	(2.791)	(4.914)	(2.870)
Number of states	51	51	51	51	51
Panel C: Employment per Establishment Premium					
	Finance vs.		Banking vs.		
	Non-finance	Non-finance (Ex. Banking)	Non-finance	Other credit inst.	Other finance
	(1)	(2)	(3)	(4)	(5)
Deregulation index	2.087*	2.169	5.355*	11.782**	1.856
	(1.186)	(1.418)	(3.028)	(4.605)	(2.630)
Number of states	51	51	51	51	51

Notes: The coefficients report the estimated average effect of a relaxation of interstate banking by one index point from the year of deregulation through seventh year. All regressions include state and time fixed effects, state level population growth, per capita income, unemployment rate and interaction of 1990 finance payroll share in that state with a time trend. The first columns report estimates for finance wage (Panel A), finance employment premium (Panel B) and finance employment per establishment premium (Panel C) using state level data. The second columns compute the premia excluding commercial banking from the financial sector. The third, fourth and fifth column report estimates for relative wages and employment in commercial banking compared to the non-financial sector, other credit institutions excluding commercial banks and other financial institutions respectively. See notes to Table 3 for further information.

Next, we split the sample based on college education and repeat the same exercise for individuals that have at least a college degree vs. individuals who do not. Here, the coefficient of interstate deregulation is positive for college and non-college educated individuals. However, we fail to obtain significant results in either case. This could be related to top coding procedure that truncates distribution of income in the right tail. This becomes especially relevant for high **skilled individuals working in finance whose wages are much more likely to be above the cut-off value. Moreover, CPS has experienced major data revisions over time with changes in top coding procedures. This makes wage and (to an extent) employment comparisons over time difficult.**

We demonstrate the importance of this issue using a indicator for very high wages: for each worker we assign the value to be one if he/she earns above the (CPI-adjusted) 95th percentile of the 1991 wage distribution.[21] We then estimate the relative likelihood of a financial sector employee to earn high wages based on this threshold. Panel B shows that deregulation raises the probability that a finance worker is such a high earner by 1.6 percentage points per index point after controlling for state-level and individual level factors (Column 2). The wage gains thus concentrate at the top of the finance distribution.[22] Splitting the sample by college education yields positive point estimates for both groups (larger for college group as expected), but neither is statistically significant (Columns 3–6). With smaller cells, the CPS lacks the power to separate the premium cleanly by education, so we read these splits as suggestive. Controlling additionally for two-digit occupation leaves the full-sample estimates essentially unchanged: 2.4 percentage points for the wage premium and 1.3 percentage points for the probability of being a high earner (Appendix Table A.1, column 1). This indicates that the increase in wage premium is not due to relative occupational changes between finance and non-finance.[23]

Finally, we report the effect of interstate deregulation on relative employment in finance (Panel C). The estimates of this analysis are directly comparable to a finance employment share regression as documented in Table 3, Panel B, Column 4. As in county level analysis, we find negative (but insignificant) effects of deregulation on the individual likelihood to work in

[21] This corresponds to an income of 60,000 as of 1991, which is significantly below the top coding threshold.

[22] The results are not sensitive to the selection of high wage indicator: the estimates are very similar if we use a tighter threshold closer to the top coding value.

[23] We note that the point estimates are less precise under this specification, since two-digit occupation absorbs much of the identifying variation especially for the college educated sample.

finance: following deregulation finance employment likelihood declines close to 0.1 percentage point. For college educated individuals, this decline is larger as expected. We conclude that interstate deregulation did not draw further (skilled) employment to the financial industry from the rest of the economy. Therefore, our finance wage premium results do not stem from compositional changes in the financial sector.

6.3 Robustness

The previous section confirms that there is an economically sizable and statistically significant effect of financial deregulation on the finance wage premium. Here, we check the robustness of this finding through several sampling restrictions.

We first check that the result does not depend on estimating the effect of deregulation as an index. For this purpose, we re-estimate the event study using an indicator equal to one once a state has deregulated. Figure 2 plots the result. We find the same pattern as in the baseline analysis: the finance wage premium is small and insignificant before deregulation and rises afterward, reaching around 5 percentage points by the 4th year. Average effect of deregulation until the 7th year is 4.2 percentage points which is slightly lower than the 5.5 percentage points aggregate effect that we obtain in the baseline analysis. Relative employment in finance, as before, declines after deregulation but the effect is insignificant.

One possible critique is that counties in deregulated states are inherently different with regards to the size of the financial industry and its development over time leading to the violation of the parallel trends assumption. We address this concern by matching treated to never-treated counties before the estimation, as described in the Estimation Strategy. This, arguably, reduces concerns with regards to trend differences between treatment and control groups. Figure A.1 in the appendix shows the resulting distribution of propensity scores of treated (above the x-axis) and matched (below the x-axis) counties and shows that the matching procedure achieves very close similarity of treated and untreated counties. Figure A.2 in the appendix demonstrates that we also achieve the desired balance in terms of wage and employment premium growth before treatment. This procedure yields very similar findings on the effect of deregulation both for the finance wage premium and relative employment in finance (Table 7 Columns 1).

Table 6: Individual Analysis

Panel A: Wage Premium

	Full Sample	Full Sample	College	College	Non-College	Non-College
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulation index	0.790	2.305*	1.410	1.268	1.002	2.056
	(1.362)	(1.253)	(2.631)	(2.892)	(1.937)	(1.729)
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	No	Yes	No	Yes
Observations	657,839	657,839	192,850	192,850	464,989	464,989

Panel B: Probability of High Wages in Finance

	Full Sample	Full Sample	College	College	Non-College	Non-College
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulation index	0.990	1.594***	2.157	2.305	0.496	0.697
	(0.640)	(0.595)	(1.788)	(1.691)	(0.422)	(0.548)
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	No	Yes	No	Yes
Observations	657,839	657,839	192,850	192,850	464,989	464,989

Panel C: Probability of Employment in Finance

	Full Sample	Full Sample	College	College	Non-College	Non-College
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulation index	-0.121	-0.086	-0.322	-0.348	-0.048	-0.024
	(0.190)	(0.192)	(0.367)	(0.393)	(0.232)	(0.253)
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	No	Yes	No	Yes
Observations	657,839	657,839	192,850	192,850	464,989	464,989

Notes: The coefficients report the estimated average effect of a relaxation of interstate banking by one index point from the year of deregulation through seventh year. All reported estimates are percentage point effects. Basic controls include state and time fixed effects, state level population growth, per capita income, unemployment and interaction of 1990 finance payroll share in that state with a time trend. Individual controls include indicators for sex, race and education interacted with year fixed effects and triple interaction of five age categories, four education categories and year fixed effects. We calculate bootstrapped standard errors clustered at the state level (reported in parentheses). *** p-value<0.01, ** p-value<0.05, * p-value<0.1.

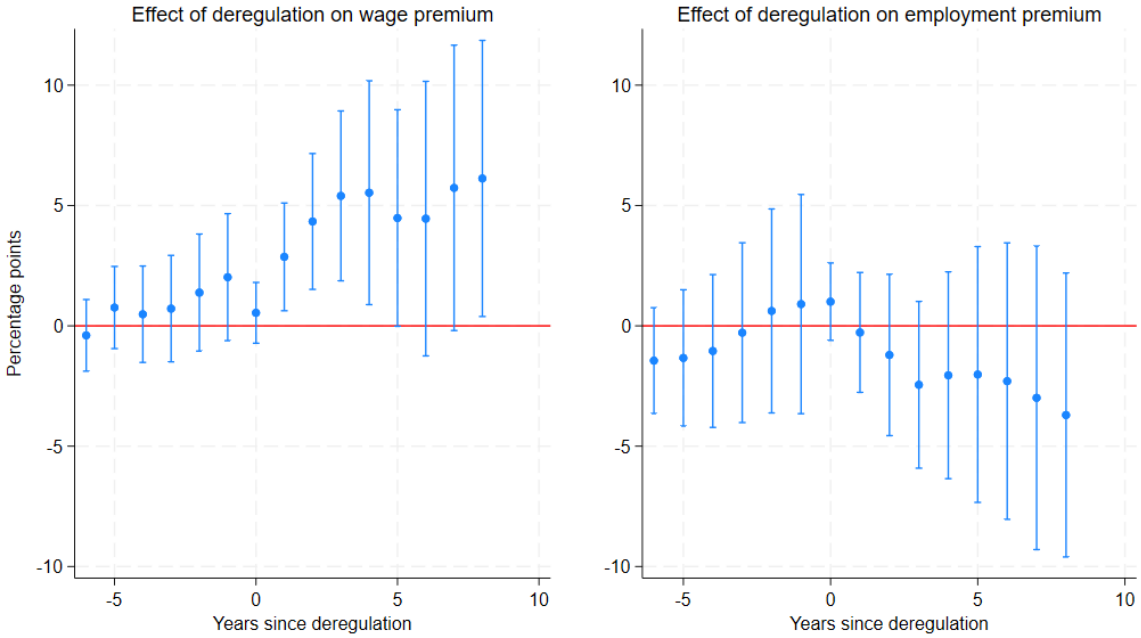

Figure 2: The figure plots event-study estimates of a relaxation of interstate branching (binary) where the event is the first time the state has ever lifted a restriction. The wage (employment) premium is the log ratio of wages (employment) in finance to average wages (employment) in the non-financial sector (multiplied by 100). All regressions include county and time fixed effects, county level population growth, per capita income and interaction of 1990 finance payroll share in that county with a time trend.

A similar critique relates to the comparability of finance to all other sectors. To mitigate this, we estimate the wage and employment premium in finance relative to other *service* sectors (column 2). Compared to other services, we find that the wage premium in finance increases after deregulation, and the relative employment in finance is negative but insignificant.

For the main analysis, we include real estate as part of the financial industry. We motivate this based on the observation that interstate deregulation after 1994 increased house prices (Favara and Imbs (2015)). Moreover, Popov (2022) argues that this increased economic rents for the real estate industry which has consequences on labor market outcomes in the real estate industry. Nonetheless, we check the robustness of our findings by removing the real estate industry from the estimation. Column 3 in Table 7 suggests that the effect of deregulation on the finance wage premium and relative employment in finance is similar after we remove real estate from the financial industry.

Table 7: Robustness checks

Panel A: Wage Premium					
	PSM	Only services	Ex. Real Estate	Ex. Finance Centers	Interior counties
	(1)	(2)	(3)	(4)	(5)
Deregulation index	2.822**	3.129**	2.822*	2.288*	2.699**
	(1.400)	(1.484)	(1.624)	(1.228)	(1.224)
Number of counties	2019	2007	2039	2019	1538
Panel B: Employment Premium					
	PSM	Only services	Ex. Real Estate	Ex. Finance Centers	Interior counties
	(1)	(2)	(3)	(4)	(5)
Deregulation index	-0.879	-1.499	-1.287	-0.719	-1.300
	(1.438)	(1.865)	(1.713)	(1.428)	(1.592)
Number of counties	2019	2007	2039	2019	1538

Notes: Propensity Score Matching (PSM) estimation in the first column we match counties based on their pre-deregulation wage and employment growth. In the second column we compare wage and employment developments relative to service industries. In the third column we create wage (employment) premium for the financial industry excluding real estate. In the fourth column we drop those counties whose payroll share of financial industry in 1990 are in the top 99th percentile. In the fifth column we focus on interior counties that are not part of a cross-state commuting zone. See notes to Table 3 for further information.

Another concern is the possibility that most of the benefits of interstate deregulation accrue to states that already have a large and productive financial sector. For instance, New York and Connecticut which host centers of finance have deregulated early after the IBBEA was passed. It is possible that those counties take the lion's share of the increase in the finance wage premium while the remaining deregulating states experience comparable wage changes in both finance and non-finance. To mitigate this problem we remove the counties in the top one percentile in terms of the share of finance in the county's total payroll. The discussed possibility is borne out by the results in column 4 in Table 7: the effect of deregulation on the finance wage premium declines once we remove the top 1 percent finance payroll share counties. For relative employment in finance, the coefficient is also lower and becomes insignificant.

A final concern, specific to the staggered design across states, is that control counties close to deregulating states may be contaminated by cross-border spillovers in credit and labor markets. To assess this, we restrict the sample to interior counties (for those counties that are not part of any cross-state commuting zone) which are insulated from such cross-border spillovers (Column 5). The finance wage premium in these counties is 2.7 percentage points, very close

to the full-sample estimate, and the employment premium is comparable. We conclude that cross-border spillovers do not drive our results.

6.4 Heterogeneity Analysis

In the previous section we presented an average effect of interstate deregulation on the finance wage premium. However, there are good reasons to believe that some locations are affected more than others. For instance, we find smaller effects on the finance wage premium after we remove financial centers such as New York county (which hosts Wall Street). Here, we look at how the effect of deregulation varies by the size of the finance sector (as measured by the share of finance in total payroll) in 1990. More specifically, we recover the per index point effect of deregulation for each county and regress this on the size of the finance sector in 1990. Figure 3 (top left panel) shows that the effect of deregulation is indeed higher for counties with large initial financial industries.[24]

Next, we investigate the effect of local house price developments on differential compensation in the financial industry. The prevalence of securitization in the mortgage industry means that an increase in house prices would inflate profits of firms (commercial and investment banks, insurance companies etc.) which engage in securitization activities. Employees in these firms could benefit from the rents associated with rapid house price growth. Since house price growth is endogenous to the local income developments, one needs a proxy that provides exogenous variation on this. Saiz (2010) estimates housing supply elasticity measures for a large group of MSAs based on land availability and local regulations. This measure, as the long list of work argues, is independent from time varying local developments in real and financial industries. Moreover, locations with inelastic housing supply are found to experience higher increases in house prices after relaxation of credit conditions (Mian and Sufi, 2011; Adelino et al., 2015; Zevelev, 2021). More specifically to our analysis, Favara and Imbs (2015) show that in counties with inelastic housing supply, an increase in credit supply due to the IBBEA deregulation yields a higher increase in house prices. Relying on these findings, we henceforth use the interaction of housing supply elasticity with deregulation as a proxy for house price growth. We follow

[24]For straightforward visualisation, we deviate from our regression models and impose additivity in the effect of deregulation on outcomes. Our heterogeneity results are not sensitive to this. Results for the non-additive models are available upon request.

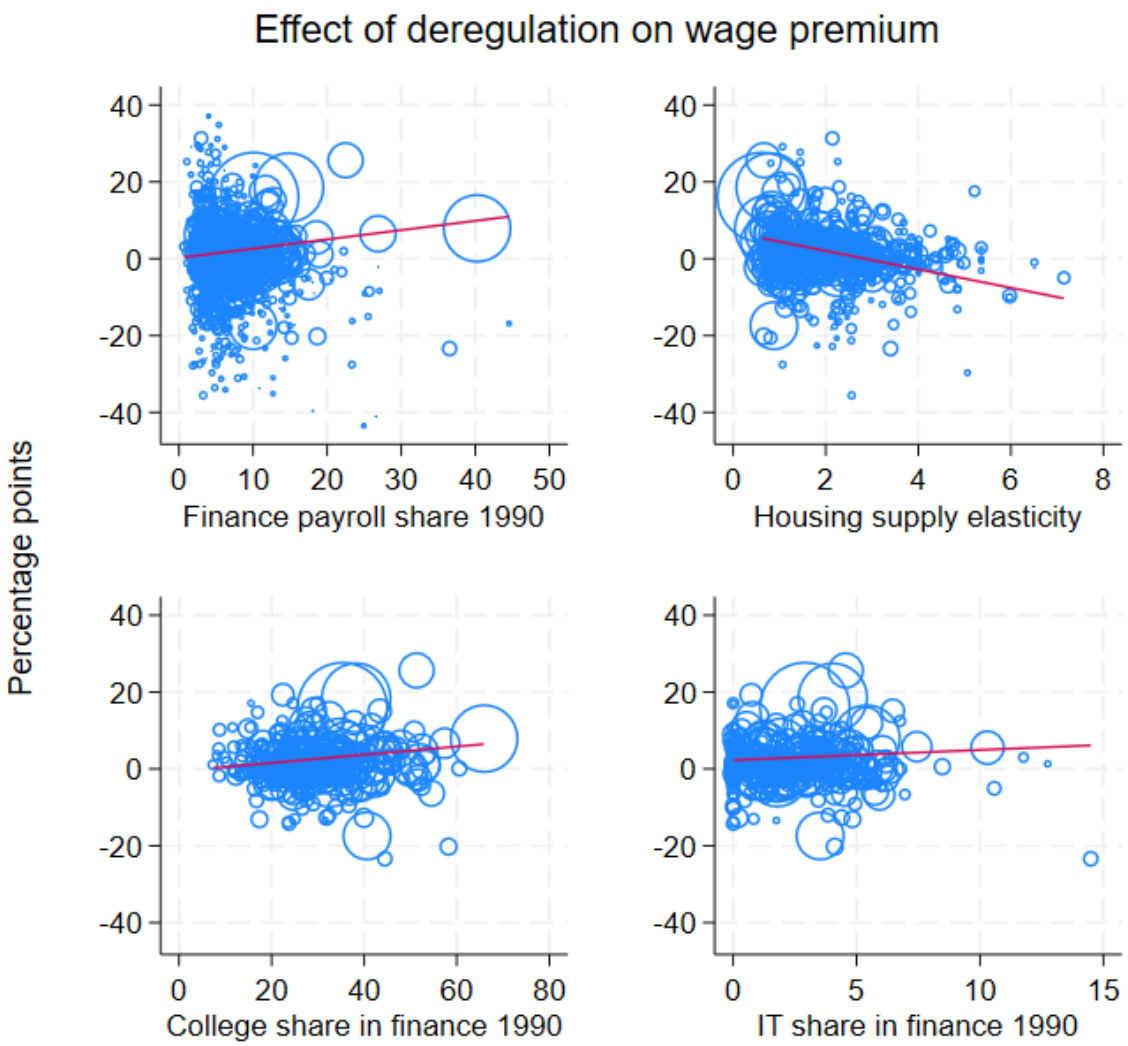

Figure 3: Heterogeneity of deregulation. Each circle represents a county, and the size of the county represents total employment in that county. The red line is the best linear fit in an employment-weighted regression. The top left figure relates the effect of deregulation to the size of the finance sector in the county in 1990 (slope = 0.24, $p < 0.01$)). The top right figure relates the effect of deregulation to the housing supply elasticity for counties within MSAs (slope = -2.34, $p < 0.01$). The bottom left figure relates the effect of deregulation to the share of college educated employees in the finance sector in the county in 1990 (slope = 0.11, $p < 0.01$). The bottom right figure relates the effect of deregulation to the share of IT professionals in the finance sector in the county in 1990 (slope = 0.26, $p > 0.1$).

the same strategy as above and report differential effect of house price growth on finance wage premium. We find that counties with inelastic supply experience higher increases in relative wages in finance (Figure 3 top right panel).

Finally, we investigate the relationship between skill biased technological change and the effects of financial deregulation. More specifically, we explore the idea that skilled individuals in the financial industry may experience higher wage increases following deregulation. While the county level data does not discriminate by skill level, we show how the effect of deregulation on the finance wage premium varies with the proportion of skilled professionals within the financial sector in 1990 - as we did above with the size of the financial sector. We obtain individual level data from the 1990 Census and compute the share of college educated workers and of IT professionals within the financial industry for each county. We assume that the proportion of college-educated / IT professionals in finance has remained relatively stable over time.[25] A positive slope would then indicate that counties with a higher initial share of skilled finance workers experience an additional increase in the finance wage premium. We find a clear, statistically significant relationship for the college share: the finance wage premium rises more in counties where college-educated workers made up a larger part of the financial sector in 1990 (Figure 3, bottom left). This is consistent with our individual results: the full sample effect on the probability of earning high wages is positive and significant, and the point estimates are larger (albeit insignificant) for college educated workers. This suggests that financial deregulation could facilitate and accelerate the impact of skill biased technological change on the additional compensation received by finance workers. The analogous relationship for the share of IT occupations is also positive but not statistically significant (bottom right).

7 Discussion of Channels

We show that interstate branching deregulation induces an increase in average pay in the financial industry compared to the rest of the economy. The effect is especially strong for workers in commercial banking with spillover effects to the rest of the financial industry. Moreover, financial industry employees in counties with a high share of employment in the financial industry in

[25] Correlation of 1990 share measures with 2000 share measures is close to 90 percent.

1990 benefit from this deregulation more. The effect is also strong for those locations that had a higher share of skilled individuals in the financial industry and those locations that experience higher house price increases after deregulation. Which of the channels discussed in section 3 do these observations support? In this section, we evaluate the role of these channels and their likely contribution to the growth in the finance wage premium.

7.1 Competition

Our results do not support the labor market competition channel. Theory predicts that an increase in labor market competition (lower concentration, i.e. $HHI_V \downarrow$) should raise both wages and employment. While we find strong wage increases in the banking and financial industry across our estimations, relative employment does not increase after deregulation. In fact, employment in the banking industry declines compared to both the broader financial sector and the non-financial sector (Tables 3 and 5, Panel B). Evidence on commercial bank branching and employment (Table 4) also corroborates this: following deregulation, commercial banks experience a decline in total number of branches, number of establishments and employment. Thus, deregulation did not expand the total number of employers but instead enabled out-of-state banks to replace incumbents through mergers and acquisitions. Therefore, we rule out that the labor market competition channel explains our findings.

Product market competition within the banking industry leads to an increase in credit supply and lower interest rates. Absent changes in productivity, greater credit activity would raise employment and its effect on wages depends on how they are determined and on the impact of competition on profitability. In models with wage posting, stronger product market competition predicts an increase in wages, whereas under wage bargaining, wages may fall if profits per employee decline. Our main findings—higher wages and lower employment in banking—are not consistent with the predictions of this channel in isolation. Although the literature documents increased loan activity and lower interest rates after deregulation (Rice and Strahan (2010),Favara and Imbs (2015)), the fact that employment in the financial industry does not expand indicates that product market competition alone cannot explain the observed outcomes. Nonetheless, higher product market competition may still foster labor demand in the rest of the financial industry through increased activity, proxied by rising house prices. This

mechanism may explain the additional increase in the finance wage premium in locations with high house price increases (Figure 3) and the spillover wage gains observed after deregulation.

7.2 Productivity

The productivity channel predicts that, following deregulation, commercial banks—the primary targets of the IBBEA—became more efficient.[26] Theory predicts that higher productivity ($A_i \uparrow$) raises the marginal product of variable labor, which increases wages. At the same time it allows firms to reduce overhead labor (F). Total employment would fall if the decline in overhead labor dominates.

Our state-level analysis documented in Table 4 and in Table 5 supports these predictions. We find that although employment in commercial banks declines strongly after deregulation, relative wages experience a dramatic increase (Table 5, column 3). Establishments in banking and (to an extent) in finance also grow larger after deregulation (Table 5, panel C), consistent with more productive banks acquiring market share. In addition, the heterogeneity analysis shows that counties with larger finance payroll shares in 1990 experience greater increases in the finance wage premium (Figure 3). We interpret that these counties have a higher concentration of the financial industry because they were already productive. Consequently, their initial productivity generates a multiplier effect for them after deregulation. Taken together, these findings are consistent with the productivity channel: after deregulation, banking (finance) wage premium increases and relative employment in banking decreases.

7.3 Skill Biased Technological Change and Rent Sharing

The SBTC channel posits that skilled workers in certain industries benefit disproportionately from advances in information technology, and finance is one of these industries. In the context of interstate deregulation, SBTC provides an amplification mechanism for wages and employment of skilled individuals in finance. Accordingly, if there is an increase in the productivity of skilled labor, this predicts an increase in the skill wage premium. At the same time, higher productivity allows firms to substitute skilled (variable) labor for unskilled (overhead) labor. Our heterogeneity analysis supports this view: wage effects are stronger in counties with higher

[26] Efficiency gains may arise either because deregulation increased competition in affected states or because more productive banks replaced incumbent, less efficient banks. Favara and Imbs (2015) argue that commercial banks improved efficiency through geographic diversification.

shares of college-educated finance workers (Figure 3). We conclude that SBTC interacts with deregulation to widen finance wage premium.

Rent sharing provides further refinement to the interpretation of our findings. If the pay in financial industry is largely driven by rents, theory predicts that interstate deregulation would lower relative wages of incumbent institutions such as credit unions and savings banks relative to commercial banks. Results in Table 5 (column 4) qualitatively confirm this prediction. Moreover, deregulation could further boost rents in the banking and financial industry by stimulating additional activity, such as securitization.[27] Consistent with this mechanism, we find that locations that experience higher house price increases have an additional increase in the finance wage premium (Figure 3).

Taken together, our findings are mostly in line with the productivity channel. It is the only channel that predicts an increase in relative wages and a decrease in relative employment. The skill biased technological change amplifies the wage effects in skill-intensive markets, while rent-sharing determines how rents from deregulation and rising house prices are distributed within the financial industry. On the other hand, results are in contrast with labor market competition channel which predicts higher employment in the banking and the financial industry. Product market competition may play a secondary role through division of deregulation induced rents to the rest of the financial industry.

8 Conclusion

We use the Interstate Banking and Branching Efficiency Act (IBBEA) as a quasi-natural experiment, exploiting the staggered adoption of deregulation across states, to estimate the impact of interstate branching deregulation on relative wages and employment in banking and finance. We find that the finance wage premium increases significantly in counties located in deregulated states. This increase reflects both direct effects of deregulation on commercial banking wages and spillover effects to the rest of the financial industry. By contrast, employment in commercial banking decreases after deregulation with consolidation of banking into fewer and

[27] Favara and Imbs (2015) show that the deregulation induced increase in mortgage lending raises house prices. Flannery et al. (2022) find that banks operating in areas that experience housing booms become larger.

larger establishments.

Further examination reveals that counties with a higher pre-existing payroll share in finance, a greater concentration of skilled finance workers, and larger post-deregulation house price growth experience stronger increase in the finance wage premium. These findings suggest that employees in large and productive banks are direct beneficiaries of the rising finance wage premium.

We interpret these findings through a stylized Cournot model of product and labor market competition with heterogeneous productivity. The model highlights three mechanisms through which deregulation could affect labor market outcomes: labor market competition, product market competition, and productivity gains from consolidation. Our results are mostly in line with the productivity channel that predicts an increase in wages and a decrease in (overhead) labor. SBTC further amplifies finance wage premium in skill-intensive locations after deregulation. We also find that rent sharing in the financial industry facilitates spillover gains stemming from mortgage-driven house price increases across the broader finance sector.

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A Propensity Score Matching

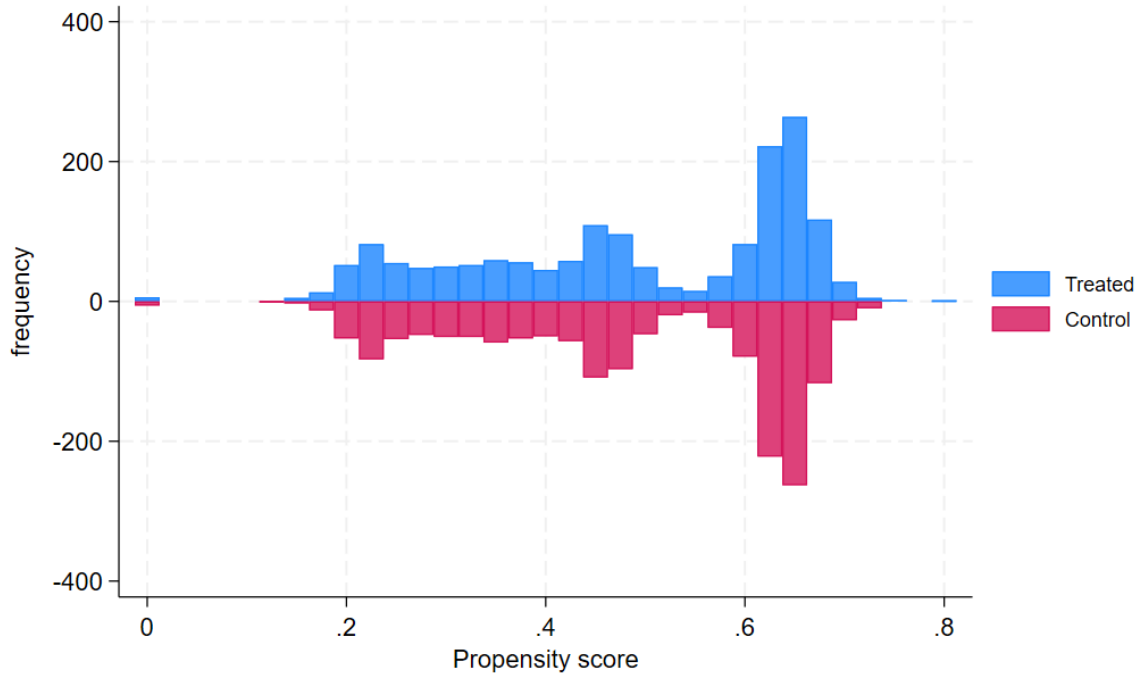

Figure A.1: Distribution of propensity scores after nearest-neighbor propensity score matching.

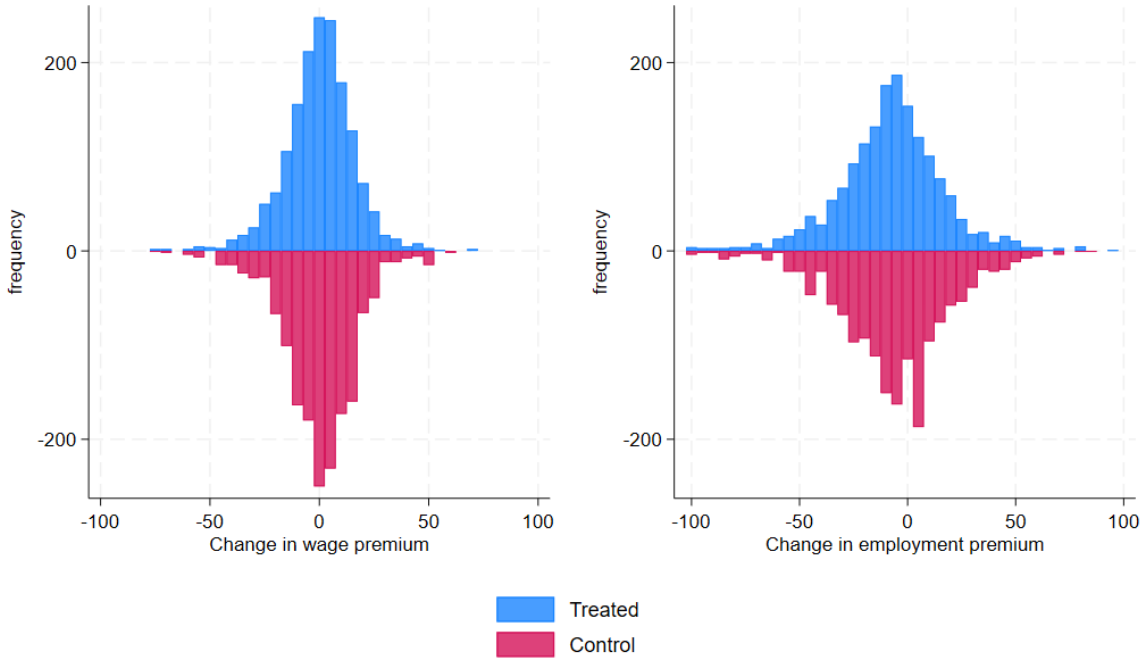

Figure A.2: Distribution of wage premium and employment premium growth prior to deregulation.

B Two way fixed effects event study

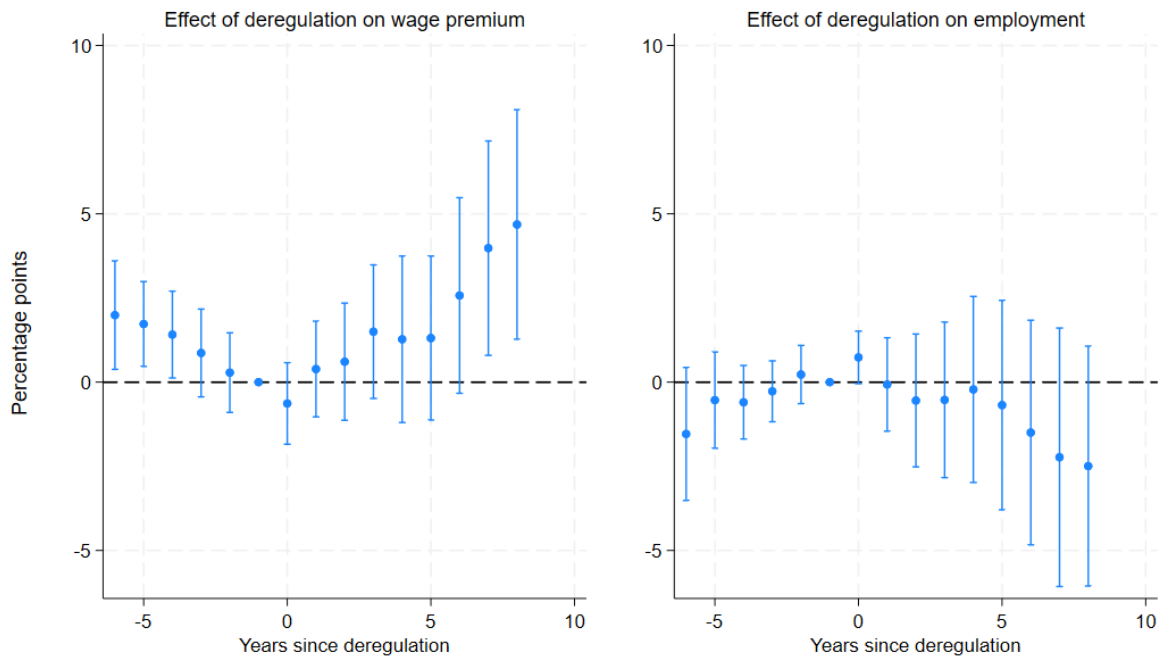

Figure A.3: The figure plots event-study estimates from a dynamic TWFE model. See notes to Figure 1 for interpretation.

Table A.1: Individual Analysis: Occupation Controls

Panel A: Wage Premium			
	All	College	Non-College
	(1)	(2)	(3)
Deregulation index	2.364	0.261	2.586
	(1.892)	(2.686)	(2.008)
Basic controls	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
Occupation controls	Yes	Yes	Yes
Observations	657,839	192,850	464,989

Panel B: Probability of High Wages in Finance			
	All	College	Non-College
	(1)	(2)	(3)
Deregulation index	1.292*	1.373	0.504
	(0.762)	(1.616)	(0.602)
Basic controls	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
Occupation controls	Yes	Yes	Yes
Observations	657,839	192,850	464,989

Notes: Basic controls include state and time fixed effects, state level population growth, per capita income, unemployment and interaction of 1990 finance payroll share in that state with a time trend. Individual controls include indicators for sex, race and education interacted with year fixed effects and triple interaction of five age categories, four education categories and year fixed effects. Occupation controls include two-digit occupation codes with year interactions. See notes to Table 6 for further information.

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