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The Effect of Branching Deregulation on Finance Wage Premium

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Abstract

What is the role of financial deregulation on rising finance wage premium in the US? This study makes use of the Interstate Banking and Branching Efficiency Act of 1994 as an exogenous shift to local banking markets and investigates the effect of deregulation induced competition on relative wages in finance. We find that the finance wage premium increased significantly in deregulated states. Our estimates suggests that the deregulation explains about a quarter of the increase in finance wage premium between 1994 and 2008.

1 Introduction

The financial industry has experienced dramatic growth since the 1980s which 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 allow skilled individuals to handle larger value projects.

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 sector. More specifically, we study the effect of the Interstate Banking and Branching Efficiency Act (IBBEA) on

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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 rights to put up several barriers to preclude out-of-state banks to open or acquire 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. (2023). 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 growth differential between the financial industry and the rest of the economy) is 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 to different sampling choices. Our estimation using individual data qualitatively confirm 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 experience stronger

[1] We refer to deregulation also as the treatment.

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.

Taken together, these findings suggest that 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. Employees in the rest of the financial sector enjoys these benefits due to spill-over, effects stemming from innovations in financial products such as securitization and increased competition within the financial industry. Finally, deregulation also implied a further increase in the finance wage premium for skilled workers.

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 U.S. finance industry. They find that the finance wage premium increased significantly since the beginning of 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 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

U.S. and finds negative effects of increased labor market concentration on employment overall and on earnings only when the change 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 investigate the branching deregulation wave starting in the 1970s and analyze 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 findings of 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 leads up 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 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 legislature 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 have to legislate its choice on opt in and out of the regulatory provisions. However, states also retained the right to lift or modify the aforementioned provisions after 1997.

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 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. We also observe that most states implement – and complete – deregulation by 1997, but there are also states which deregulate after this date.

[2]Intrastate banking refers 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.

[3]As in Célerier and Matray (2019) 0 denotes non-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).

Previous papers that investigate the effect of the IBBEA find that deregulating states expe-

rienced an entry of interstate bank branches which increased competition in the 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 one-half of the increase in mortgage loans and more than one-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. (2022) 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. 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 such as securitization and through house price growth.

3 Theoretical Background

How would deregulation of interstate branching affect wages in banking and finance? The literature suggests four mutually non-exclusive theories that link regulation induced branch expansion and employee compensation in finance. These are 1) the competition, 2) productivity, 3) rent sharing and 4) the skill biased technological change channels. While the first two channels directly work through the availability and the extent of interstate branching in deregulated states, the latter two channels affect wages in combination with deregulation.offer further refinements by examining their interactions with deregulation.

The first channel relates to the direct effect of deregulation induced competition on banks' market power in local credit and labor markets. This competition channel relies on standard economic theory that suggests that increased competition leads to higher quantities in the product market (more credit supply) and higher prices in the factor market (higher wages). In this framework, the effect of competition on wages could be decomposed into three components: labor market power effects, product market power effects and productivity effects (Arnold (2021)). The labor market power effect refers to the ability of employers to depress wages below competitive levels (See evidence in Benmelech et al. (2022) and Thoresson (Thoresson). Evidently, interstate branching deregulation enabled the expansion of new (out-of-state) banks into new locations (Taşkın and Yaman (2023)). More competition increases the number of banks and branches operating in deregulated locations and diminishes banks' local labor market power. This would predict **an increase in both wages and employment**.

Increased product market competition fosters an expansion of quantity (credit) in deregulated states. Assuming fixed productivity, this would induce an increase in employment. The effect on wages is ambiguous: if labor demand effects dominate, then wages would increase. If profit sharing dominates, then wages could also decrease (assuming fixed productivity) (Black and Strahan, 2001). 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 in increase in the amount of credit and lower prices in corporate lending (Rice and Strahan (2010), Keil and Müller (2020)), household and commercial mortgage loans (Favara and Imbs (2015),Chu (2018),Célerier and Matray (2019)). This increased credit activity promotes product 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.

The second channel suggests that deregulation could foster productivity in the local banking industry which results in higher profitability and consequently higher wages. This may either stem from increased competition which makes the local banking industry more efficient or gains through diversification where larger and more productive banks are able to operate across state borders. 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), Goetz (2018)). Moreover, Favara and Imbs (2015) argue that the IBBEA led to an increase in mortgage lending through out-of-state lenders diversifying geographically. Higher productivity, either through competition or diversification, would predict **an increase in wages**. To the extent that more productive banks consolidate through mergers and acquisitions, it would predict **a decrease in employment**.

The third theory relates wage growth in finance to the literature of skill biased technological change (Katz and Murphy (1992), Acemoglu and Autor (2011)). According to this view, advances in information and communications technology (ICT) could disproportionately raise the productivity of skilled workers compared to unskilled workers. Recent findings suggest that the products offered by the financial industry and the tasks performed by financial workers became increasingly complex. This development particularly favors skilled labor, increasing returns to skill in finance. Bertay et al. (2022), for instance, claims that ICT capital-skill complementarity explains most of the finance wage premium in Netherlands. In the case of interstate branching deregulation, as argued above, securitization activity grew strongly in deregulated states. This, together with product level competition, would increase profits per skilled worker in finance. The skill biased technological change channel hence argues that skilled employees in the financial industry would experience **higher increases in wages** compared to unskilled employees after deregulation.

The last theory suggests that state-level restrictions on banks' ability to expand across local markets allowed incumbent banks to enjoy rents.[4] Black and Strahan (2001) test this idea using intrastate and (bilateral) interstate banking deregulations in the US for a period of 20 years starting in 1970s. They find that wages in the banking industry declined after states allowed branching and this effect was stronger for states that had tighter ex ante restrictions on bank expansion. In the case of interstate branching deregulation that took place after 1994, Favara and Imbs (2015) find that thrifts and credit unions do not experience an increase in mortgage lending in deregulated states. According to the rent-sharing channel, after a state deregulates,

[4]This is due to the (past) nature of banking where informational asymmetries foster a long-term relationship between local banks and borrowers (Sharpe (1990), Petersen and Rajan (1995)). In contrast, large commercial banks are more likely to rely on hard information (Stein (2002)) and adopt a transaction based approach to lending (Cole et al. (2004)). Absent an entry threat, local banks are able to extract rents from borrowers and distribute those rents according to their own preferences.

competition by entry of new commercial banks would decrease the rents enjoyed by local community banks. This would predict **an increase in wages and employment** for commercial banks compared to credit unions and savings banks.

Rent-sharing in banking and finance does not exclusively pertain to rents associated with relationship lending. Theoretical work of Biais and Landier (2020) and Bolton et al. (2016) argue that more complex and opaque tasks generate larger agency rents. Philippon and Reshef (2012) and Célérier and Vallée (2019) demonstrate evidence that tasks in finance become exceedingly complex over time. One such complex product extensively used in the financial industry is Mortgage Backed Securities (MBS). To the extent that interstate branching deregulation of 1994 promoted securitization of mortgage loans, the financial industry would be able to extract rents. This 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

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. CBP record for each county and year the total first-quarter payroll and employment as of the 12th of March. We retrieve these variables for the years 1990 to 2008, separately for the finance and non-finance sectors. We restrict our attention to 2,028 counties which have available payroll and employment information throughout the entire 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.

While these calculations are transparent, they might not exactly measure our object of interest. 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.

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. Deregulation thus 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 conveys 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 50 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 1994 to 2005: by 11 log points in counties which never deregulated, and by 20 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.

We supplement the above information with county level population growth and per capita

Table 2: Employment and finance wage premium by treatment status

		1993	1997	2005
Number of counties	Never treated	409	409	409
	Not yet treated	1,619	278	0
	Treated	0	1,341	1,619
Share employed in finance	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	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).

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) from the 5% sample of the 1990 census the share of college educated employees in the finance sector, and 3) housing supply elasticity estimates for Metropolitan Statistical Areas (MSA) measured from Saiz (2010). 2) only identifies single counties with a population of at least 100,000, and 3) provides elasticity estimates for MSAs only. Consequently, the analyses using these data include only counties for which this information is available.

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.

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

$$ATT(k) = E(Y(1,k) – Y(0) \quad | \quad \text{deregulated k years ago})$$

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 been under deregulation. $Y(0)$ is of course not observed for counties which deregulate. We are also interested in an aggregate treatment measure given by

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

Traditionally, staggered adoption data structures have been modelled 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, burgeoning 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 homogenous across both units and time.[5] 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. (2023) 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 (2022) demonstrate that the dynamic effects of deregulation on house prices are much stronger than Favara and Imbs (2015) suggest.

[5]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. (2023) 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 inferred no-treatment outcome $Y(0)$ and the observed outcome under treatment $Y(1)$ 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. 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.[6] 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. (2023) show that this is 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).[7]

States can have different deregulation 'intensities': they can choose any combination of the four deregulation measures described in section 2. 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

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

[7] 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.

effects are simply additive in the number of treatments. We choose a model somewhere between these extremes. We do not 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. On the other hand, after some exploratory analysis, we decided to relax the assumption of additivity. A deregulation index of two is thus not assumed to have twice the effect as a deregulation index of one. We 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*. 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. 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 inspect event study plots based on the above estimator to detect any potential violations of the identifying assumptions. While the plots do not suggest a violation of the assumptions, we also complement our analysis 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). 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:

$$Y_{ist} = \alpha_s + \lambda_t X_i + \beta X_{st} + \epsilon_{ist} \tag{1}$$

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 year-specific age effects (we interact age and age squared with year dummies), year-specific differences between men and women, between the college educated and those without college degrees, and between white, black, and people of other races (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 Empirical Findings

6.1 Main Results

Figure 1 summarizes our main findings of the effect of interstate branching deregulation on the finance wage premium. We find that states that took deregulation steps exhibit similar trends compared to states that did not before the introduction of branching deregulation. The graph suggests that after deregulation, counties in deregulated states start to experience an increase in the finance wage premium compared to counties in states which do not deregulate. The coefficient on the effect of deregulation rises to 3 percentage points per index point 3 years after deregulation and stays there in the following years. This means that on average the finance wage premium in a county is 3% higher per deregulation point compared to 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 dy-

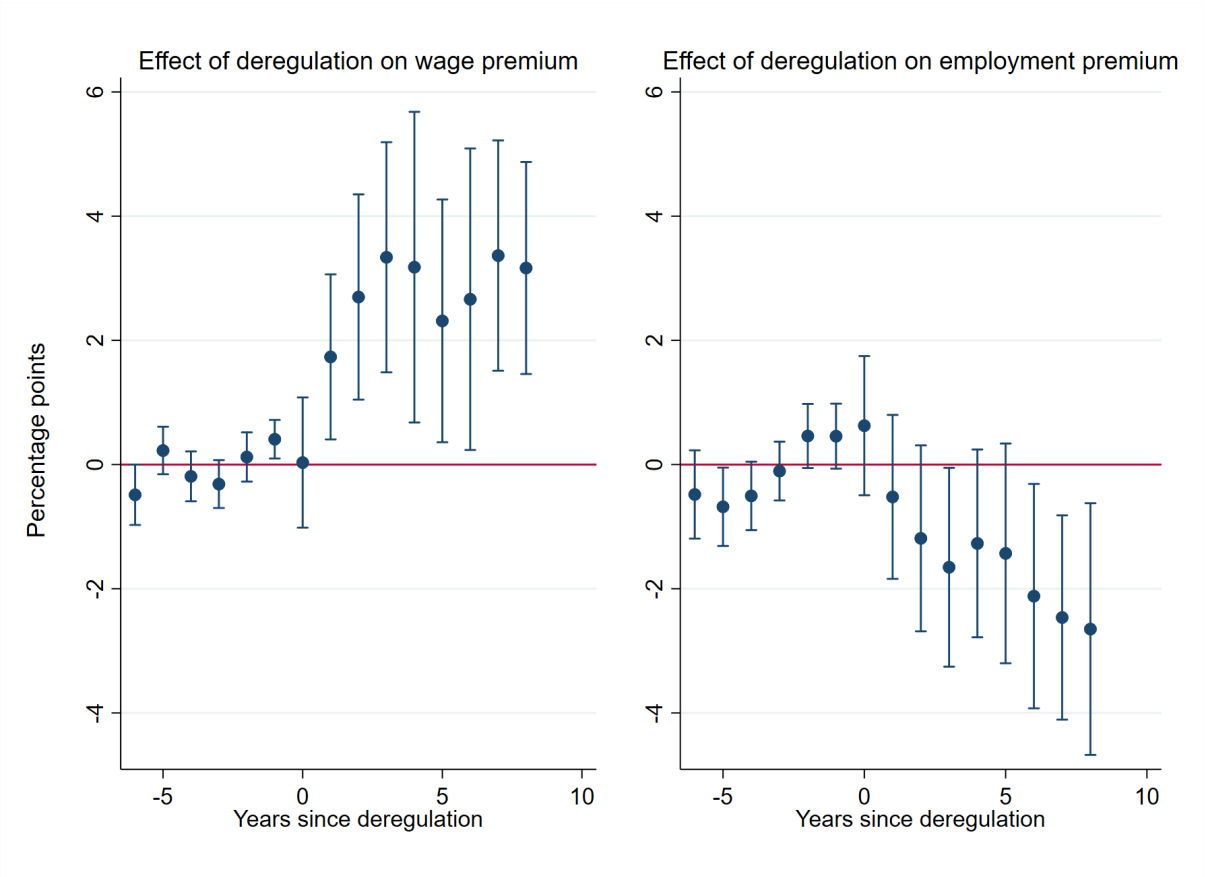

Figure 1: The figure plots event-study estimates of a relaxation of interstate branching by one index point where the event is the first time the state has ever lifted a restriction. The wage (employment) premium is the log ratio of wages (employment) in finance to average wages (employment) in the non-financial sector. 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.

namics: A decreasing finance wage premium before deregulation, and a reversal of this trend after deregulation.

In Table 3 we show that this is due to the fact that wages in finance respond strongly to deregulation compared to wages in the rest of the economy. Table 3, Column 3 reports that the average effect of interstate deregulation on the finance wage premium amounts to 2.4 percentage points over the first eight years after deregulation. Between 1994 and 2008, average number of interstate branching restrictions removed amounted to 2.2. A simple back of the envelope calculation based on this suggests that interstate branching deregulation explains about 5 percentage points of the increase in finance wage premium. This alone is responsible about a quarter of the increase in finance wage premium between 1994 and 2008.[8]

[8]During the same period, finance wage premium has risen by 22 log points.

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	2.935***	0.527***	2.407***
	(0.448)	(0.202)	(0.424)
Number of Counties	2028	2028	2028

Panel B: Employment			
	Finance	Non-Finance	Employment Premium
	(1)	(2)	(3)
Deregulation index	-2.828***	-1.587***	-1.241***
	(0.508)	(0.256)	(0.436)
Number of Counties	2028	2028	2028

Notes: The coefficients report the estimated average effect of a relaxation of interstate banking by one index point over the first 7 years after deregulation. Employment is calculated as log ratio of county level employment to local population. Wage is calculated as log ratio of average wage to local per capita income. Wage (Employment) premium is the log ratio of wages (employment) in finance to average wages (employment) in the non-financial sector. 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. Standard errors are clustered by state-years and reported in parentheses. *** p-value<0.01, ** p-value<0.05, * p-value<0.1.

In Figure 1, we also see that employment in finance relative to employment in the rest of the economy declines gradually, albeit with a slower pace. This comes from a fast decline in finance employment in deregulated states compared to non-finance employment. Table 3, Column 3 reports an average effect of interstate deregulation on relative employment of -1.2 percentage points by the 7th year since the beginning of deregulation.

We support our county level analysis using state level data. This has two advantages: First, it allows us to provide estimates for a nearly complete population, while we had to exclude some counties due to data availability in the county level analysis. Second, state business patterns provide more detailed information on industry distribution, including lower subcategories and their size distribution. Table 4, column 1 presents coefficients from state level regressions on the finance wage premium and relative employment in finance. We find that the effect of deregulation is almost identical to the county level analysis for the former and of a similar magnitude (albeit statistically insignificant) for the latter.

Next, we focus exclusively on commercial banking as the direct and primary beneficiary of interstate deregulation. As expected, the wage premium in the commercial banking industry, relative to the non-financial sector is much higher after deregulation (Column 3). This is also true for the relative decline in banking employment. Commercial banks experience a notable increase in wages relative to other credit institutions such as credit unions, savings banks and mortgage financing institutions (Column 4) and the rest of the financial industry such as investment banks and insurance companies (Column 5). However, it is worth noting that the effect of interstate deregulation extends beyond commercial banks: the rest of the financial industry also experiences an increase in relative wages (Column 2). Relative employment in the financial industry excluding commercial banks, on the other hand, does not respond to deregulation. This suggests that wage increases resulting from branching deregulation have positive spillover gains to the rest of the financial industry, while employment effects are primarily concentrated within the banking and credit institutions. Interestingly, we observe that the relative employment of commercial banks compared to other credit institutions does not decline after deregulation. This hints that commercial banks operating across state borders are replacing other local banks through the process of deregulation.

6.2 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.

One common critique that comes to mind is that counties in deregulated states are inherently different with regards to the size of 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 above. This, arguably, reduces concerns with regards to trend differences between treatment and control groups. This procedure yields very similar findings on the effect of deregulation both for the finance wage premium and relative employment in finance (Table 5 Columns 1).

Table 4: 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***
	(0.689)	(0.648)	(1.300)	(1.784)	(1.226)
Number of States	51	51	51	51	51

Panel A: 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***
	(0.648)	(0.706)	(1.421)	(2.876)	(1.661)
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 over the first 7 years after deregulation. 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 and finance employment premium using state level data. The second columns computes 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 non-financial sector, other credit institutions excluding commercial banks and other financial institutions respectively. See notes to Table 3 for further information.

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 2 in Table 5 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.

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

Table 5: Robustness checks

Panel A: Wage Premium			
	PSM	Ex. Real Estate	Ex. Finance Centers
	(1)	(2)	(3)
Deregulation index	2.824***	2.816***	2.225***
	(0.484)	(0.426)	(0.436)
Number of Counties	2028	1904	2008

Panel B: Employment Premium			
	PSM	Ex. Real Estate	Ex. Finance Centers
	(1)	(2)	(3)
Deregulation index	-1.217**	-1.279**	-0.716
	(0.515)	(0.515)	(0.438)
Number of Counties	2028	1904	2008

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 create wage (employment) premium for the financial industry excluding real estate. In the third column we drop those counties whose payroll share of financial industry in 1990 are in the top 99th percentile. See notes to Table 3 for further information.

premium while the remaining deregulating states experience comparable wage changes in both finance and non-finance. To circumvent 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 3 in Table 5: the effect of deregulation on the finance wage premium declines once we remove the top 1 percent finance payroll share counties.[9] For relative employment in finance, the coefficient is also lower and becomes insignificant.

6.3 Heterogeneity Analysis

In the previous section we presented an average effect of interstate deregulation on the finance wage premium. However, there are strong 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 2 shows that the effect of deregulation is indeed higher for counties with large initial financial

[9]The coefficient falls even further if we exclude top 5 percent. We return to this issue in the next section.

industries.

Next, we investigate the effect of local house price developments on differential compensation in 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 the same strategy as above and report differential effect of house price growth on finance wage premium. We find that counties with low elastic supply experiences higher increases in relative wages in finance.

Finally, we investigate the potential 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 our 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 within the financial industry for each county. We assume that the proportion of college-educated finance professionals has remained relatively stable over time. Therefore, a positive slope would indicate that counties that have a higher share of college educated individuals in the finance sector experience an additional increase in the finance wage premium. Our results indicate that the

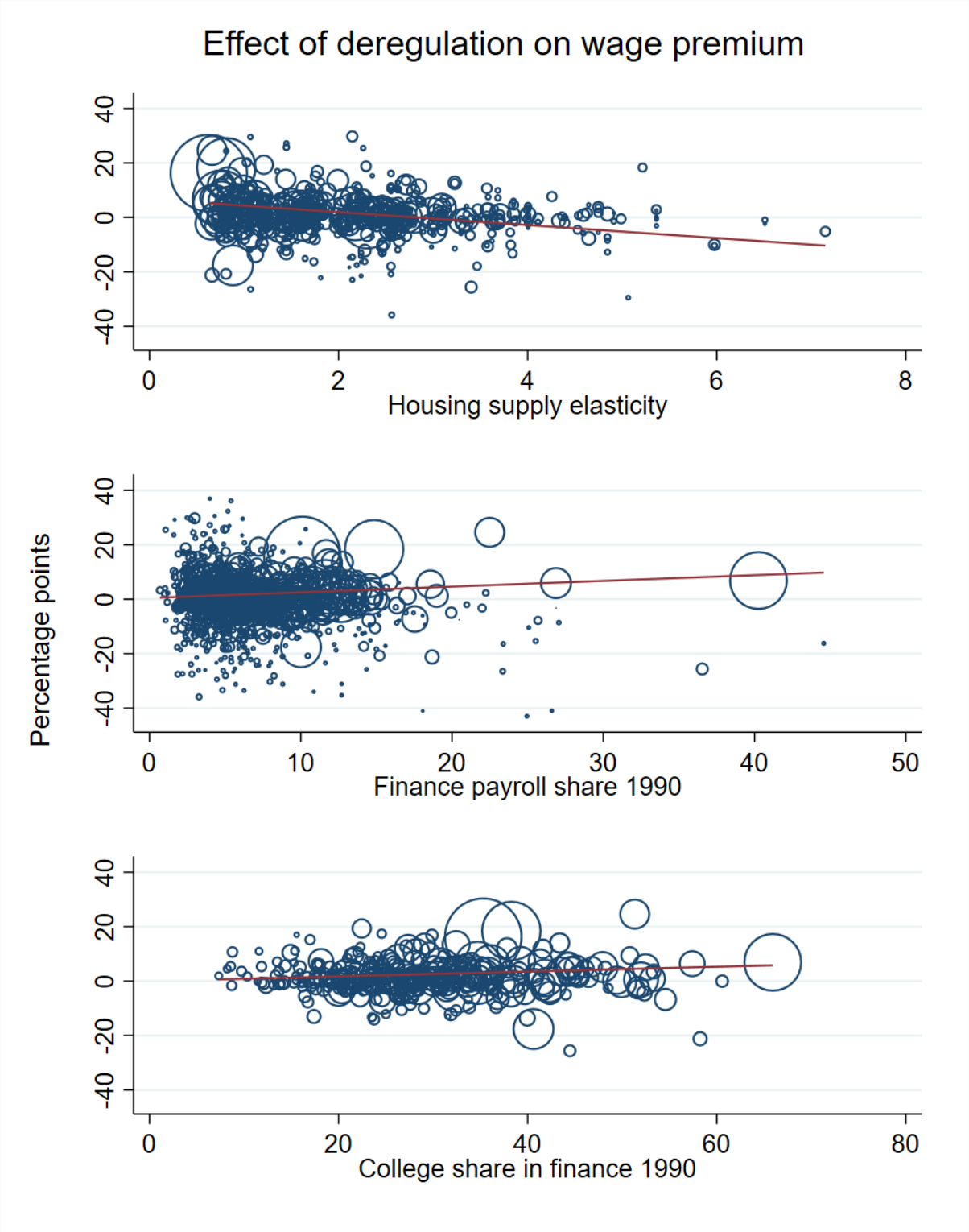

Figure 2: Heterogeneity of deregulation. Each circle represents a county, and the size of the county represents total employment in that county. The red line is the best linear fit in an employment-weighted regression. The top figure relates the effect of deregulation to the housing supply elasticity for counties within MSAs. Slope coefficient b = -2.39 (standard error: 0.30). The middle figure relates the effect of deregulation to the size of the finance sector in the county in 1990. b = 0.21 (0.03). The bottom figure relates the effect of deregulation to share of college educated employees in the finance sector in the county in 1990. b = 0.09 (0.03).

Table 6: Heterogeneity Analysis

Panel A: Wage Premium			
	Finance Payroll Share	Housing Supply Ela.	College Finance Share
	(1)	(2)	(3)
Deregulation index	-1.521	4.016***	0.546
	(1.703)	(1.221)	(0.741)
Deregulation index x	0.093*	-1.617***	0.197***
Interaction	(0.054)	(0.455)	(0.076)
Number of Counties	2028	696	379

Panel B: Employment Premium			
	Finance Payroll Share	Housing Supply Ela.	College Finance Share
	(1)	(2)	(3)
Deregulation index	0.333	-6.628***	1.268
	(0.831)	(1.214)	(1.913)
Deregulation index x	-0.166*	2.959***	-0.097*
Interaction	(0.094)	(0.419)	(0.057)
Number of Counties	2028	696	379

Notes: The coefficients report the outcome of regressing county level per index point estimates against an interaction variable. County level estimates are recovered using the original estimation. Finance payroll share is the payroll share of the financial industry for each county as of 1990. Housing supply elasticity is from Saiz (2010). College finance share is the share of college educated individuals in the financial industry of each county as of 1990. Each original estimation that is used to predict county level estimates includes the interaction variable with a time trend. See notes to Table 3 for further information.

finance wage premium is especially strong in areas with skilled financial professionals. This suggests that financial deregulation could facilitate and accelerate the impact of skill biased technological change on the additional compensation received by finance workers.

6.4 Individual Analysis

Our main analysis 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 finance sector might have grown, at the same time as the wage premium for skilled labor in general. To control for these industry specific demographic factors we need individual data over time. For that purpose we use the 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 1 separately for financial and non-financial industry and report the effect of deregulation on relative wages in finance in Table 7. 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 3 and Table 5. We find that the effect is positive though the coefficient is insignificant. In the next column, we include individual controls to account for compositional effects. In this case we find results very similar in size to the estimates obtained using County Business Patterns data. This confirms that interstate deregulation increased relative wages of an average individual in finance.

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. This is similar in spirit to the heterogeneity analysis using college share in finance above. 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 topcoding procedures. This makes wage and (to an extent) employment comparisons over time difficult. For these reasons, we take these findings with caution.

Finally, we report the effect of interstate deregulation on relative employment in finance. Here, the dependent variable is 1 if individual i works in finance. As in county level analysis, we find negative effects of deregulation on the individual likelihood to work in finance. This is the case for the general sample and for both college educated and non-college educated individuals. We conclude that interstate deregulation did not draw further employment to financial industry from the rest of the economy.

Table 7: 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	2.259*	1.410	1.053	1.002	1.929
	(1.342)	(1.204)	(2.542)	(2.039)	(1.463)	(1.401)
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	No	Yes	No	Yes

Panel B: 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	-0.082	-0.322	-0.346	-0.048	-0.030
	(0.142)	(0.157)	(0.305)	(0.325)	(0.142)	(0.163)
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	No	Yes	No	Yes

Notes: The coefficients report the estimated average effect of a relaxation of interstate banking by one index point within the first 7 years after deregulation. 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 age, age squared, indicators for sex, race and education interacted with year fixed effects. Standard errors are clustered by state-years and reported in parentheses. *** p-value<0.01, ** p-value<0.05, * p-value<0.1.

7 Discussion of Channels

We showed 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 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

The labor market competition channel predicts that new banks enter into deregulated states which increases the number of banks and branches creating a surge in labor demand. This

leads to an increase in wages and employment in the banking and financial industry. While the previous literature has found that the share of out-of-state bank branches increased in deregulating states, this has not translated into higher number of banks or branches in those locations (Favara and Imbs (2015), Taşkın and Yaman (2023). This suggests that some incumbent bank branches are replaced by their competitive bank branches after branching deregulation allow for mergers and acquisitions. This is supported by our observation that relative employment in banking and the financial industry fails to increase after deregulation. As a matter of fact, employment in the banking industry declines compared to the rest of the financial and non-financial sector. Therefore, we rule out that the labor market competition channel explains our findings.

Product market competition within the banking industry produces an increase in credit and a decline in interest rates. Absent changes in productivity, increased credit activity would boost employment with ambiguous effects on wages. This is, however, difficult to observe in our event since interstate branching deregulation of 1994 has changed the nature of competition within and across states. On the one hand, the previous literature emphasized the role of deregulation induced competition on credit growth and interest rates. On the other hand, the extent to which competition or productivity caused an increase in mortgage lending after IBBEA is a matter of debate. Therefore, we fail to observe labor market outcomes directly implied by product market competition in the banking industry. However, increased product market competition could foster labor demand in the rest of the financial industry and explain observed spillover wage gains after deregulation.

7.2 Productivity

The productivity channel predicts that after deregulation, commercial banks (which are the primary target of deregulation) become more efficient. This could work through an increased competition in deregulated states or more productive banks' replacing the incumbent inefficient banks. In either scenario, this implies that relative wages of the banking industry would go up and relative employment therein would go down. Our state level analysis offers insight to these predictions: there we see that although employment in commercial banks declines strongly after deregulation, relative wages experience a dramatic increase. Moreover, we also show in

the heterogeneity analysis that counties with high finance shares experience an additional increase in the finance wage premium. We argue that these counties have a higher concentration of the financial industry because it was already productive. Consequently, their initial productivity generates a multiplier effect for them. Henceforth, productivity channel explains the increase in banking (finance) wage premium and the decrease in relative employment in banking.

7.3 Skill Biased Technological Change and Rent Sharing

The skill biased technological change channel claims that skilled individuals in certain industries gain from changes in information technology. Finance is one of these industries. Since we are comparing relative wages in the financial industry across states and over time, our identification already controls for this channel. However, deregulation induced competition and productivity could interact with advances in information technology. Therefore, to the extent that deregulation complements technology for workers in the financial industry, this would predict an increase in relative wages and decrease in relative employment in the financial industry. Our interaction of the college share in the financial industry aims to account for this: we see that finance wage premium has increased further after deregulation in counties with higher share of college educated individuals in their workforce. The reverse is true for employment.

The rent sharing channel argues that incumbent banks such as credit unions and other savings institutions would lose their rents to entering out-of-state commercial banks. In our state level analysis we find support for this channel: After deregulation, wages in commercial banking increase much more than wages in the rest of the credit institutions. Moreover, deregulation could further boost rents in the banking and financial industry in the form of larger balance sheets. While we do not have a direct way of testing this hypothesis we find that locations that experience higher house price increases have additional increase on finance wage premium.

Overall, 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 and rent sharing channels could offer further refinements to the increase in finance wage premium in the form multiplier effects and comparison within 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 increased significantly in counties with deregulated states. This increase stems from both direct effects on commercial banking wages and spillover effects to rest of the financial sector. Relative employment in finance, on the other hand, experiences decline after deregulation. This comes from the strong decline in employment in commercial banking.

Further examination reveals that counties with a higher proportion of payroll in the financial industry in 1990, a greater concentration of skilled individuals in finance, and increased house prices after deregulation experience a stronger growth in the finance wage premium. These findings suggest that employees in large and productive banks are direct beneficiaries of rising finance wage premium.

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A Two way fixed effects event study

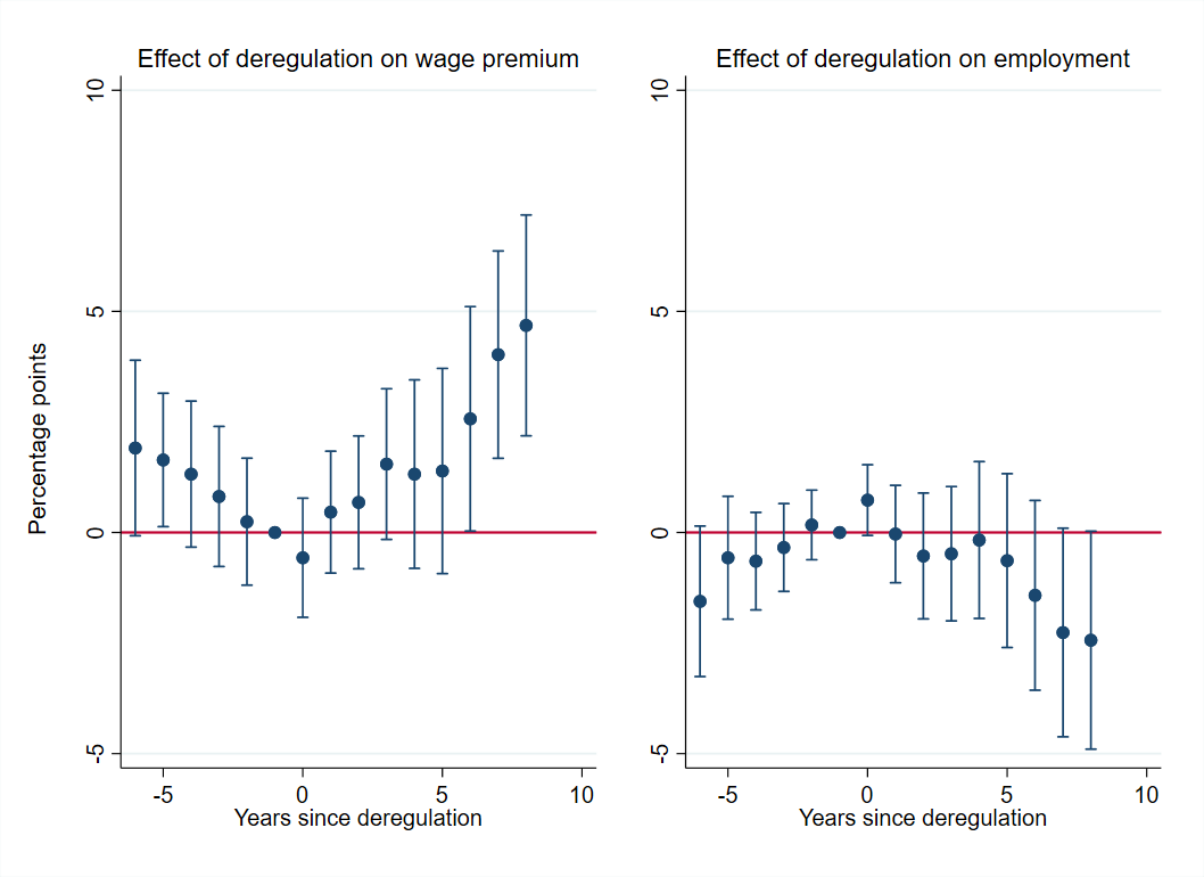

Figure 3: The figure plots event-study estimates from a dynamic TWFE model. See notes to 1 for interpretation.