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Racial Discrimination and Competition

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Abstract

We provide the first assessment of whether an intensification of product market competition reduces the racial wage gap exactly where taste-based theories predict that competition will reduce labor market discrimination. In economies where employers have strong racial prejudices. We use bank deregulation across the U.S. states to identify an intensification of competition among banks, which in turn lowered entry barriers facing nonfinancial firms, especially firms that depend heavily on bank credit. Consistent with taste-based theories, we find that competition boosted blacks' relative residual wages within the banking industry and bank-dependent industries, but only in states with strong tastes for discrimination.

Keywords: Discrimination, imperfect competition, banks, regulation

JEL Classification Codes: J7, J31, D43, D3, G21, G28

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White Americans receive much higher wages than their black counterparts (Altonji and Blank, 1999; Donohue and Heckman, 1991; Fryer, 2011; Katz and Autor, 1999). Consequently, a large literature examines the nature of the cognitive and noncognitive skill differentials associated with these racial wage disparities.¹ Rather than investigating skills, we focus on the role of racial prejudices in contributing to the black-white wage differential. We assess how racial prejudices interact with changes in product market competition to shape the racial wage gap.

Our research is framed and structured by Becker’s (1957) taste-based theory of discrimination, which suggests that intensified product market competition erodes the manifestation of racial prejudices on labor market outcomes. In Becker’s framework, labor market discrimination—the difference in wages between identically productive black and white workers—is determined by the most racially prejudiced employer of black workers. Therefore, intensified competition, by allowing employers with weaker racial biases to enter the market, reduces labor market discrimination and shrinks the racial wage gap, *but only* in markets where black workers were working for racially biased employers. Although economists have *separately* examined the association between competition and the racial wage gap (e.g., Peoples and Talley, 2001) and that between racial attitudes and the racial wage gap (Charles and Guryan, 2008), we take a different route. Recognizing the *joint* connections among competition, racial attitudes, and labor market discrimination, we provide the first assessment of whether an intensification of competition reduces the racial wage gap in exactly those environments in which the taste-based theory predicts that competition will reduce labor market discrimination—in economies where pre-existing employers have strong racial prejudices. As we show, differentiating economies by racial prejudice is crucial for drawing accurate inferences about the effect of competition on labor market outcomes.

In particular, we evaluate the causal impact of competition on the racial wage gap while differentiating among U.S. state economies with stronger and weaker tastes for discrimination. We use bank deregulation across the U.S. states to identify an exogenous intensification of competition among banks and—more importantly—among manufacturing and other nonfinancial firms. From the mid-1970s to 1994, individual states relaxed restrictions on the entry of banks from other states and the branching of banks within states, boosting the contestability of banking markets. For example, Jayaratne and Strahan (1998) show that deregulation narrowed interest rate spreads

¹See, for example, Austin-Smith and Fryer (2005), Card and Krueger (1992), Carneiro, Heckman, and Masterov (2005), Coate and Loury (1993), Fryer and Levitt (2004), Jencks (1998), Juhn, Murphy, and Pierce (1993), Lang, Manove, and Dickens (2005), Neal (2007), and Neal and Johnson (1996).

and bank profit margins within deregulating states, without harming loan quality.

Moreover, state bank deregulation increased competition within the manufacturing sector and across each state’s entire nonfinancial economy, i.e., bank deregulation affected the overall economy, not just the banking sector. By intensifying competition among banks, state deregulation spurred improvements in bank performance, as measured by lower lending rates and the development and adoption of better techniques for screening borrowers (Jayaratne and Strahan, 1998; Hubbard and Palia, 1995). These improvements, in turn, lowered barriers to the entry of new firms. Black and Strahan (2002) and Kerr and Nanda (2009) demonstrate that state bank deregulation substantively increased new firm entry within deregulating states. And, Cetorelli and Strahan (2006) show that bank deregulation intensified competition more in bank-dependent manufacturing industries—industries where firms rely heavily on bank credit to finance capital expenditures and operations—than in industries that rely less on bank finance.

We differentiate states by racial bias using several measures. From the 1970 U.S. census, we compute the predicted rate of racial intermarriage based on individual and state characteristics. We interpret the difference between the predicted rate of intermarriage and the actual rate as positively related to the taste for discrimination. Although imperfect, this index captures decisions made far before our sample period since the 1970 census contains the accumulated stock of marriages in 1970 and we begin our analyses in 1976. Furthermore, we confirm the results using survey-based measures of racial attitudes from Charles and Guryan (2008).

We conduct three types of analyses to identify the impact of bank deregulation on blacks’ relative residual wages and to shed light on the underlying causal mechanism. We measure blacks’ relative residual wages as the difference between the wages of black and white workers with the same Mincerian characteristics, working in the same state and year, and—in some specifications—employed in the same industry and occupation. We first assess how bank deregulation affected blacks’ relative wages within the banking industry by exploiting the cross-state, cross-time exogenous variation in the timing of bank deregulation and differentiating states by racial bias.

Second, we examine manufacturing industries. Bank deregulation intensified competition more in bank-dependent manufacturing industries than in other manufacturing industries. Thus, we examine whether deregulation boosted blacks’ relative wages more in bank-dependent industries *within* high racial bias states.

Third, we examine each state’s overall economy. With data on new incorporations by state and year, we assess whether bank deregulation reduced a state’s overall racial

wage gap by spurring the entry of new firms, which is the key mechanism suggested by the taste-based theory of discrimination. And, the state-wide analyses provide a unique opportunity to evaluate the impact of deregulation and new firm entry on the racial wage gap in the overall economy, not just in specific industries.

The findings suggest that intensified competition substantially increased the relative demand for black workers but *only* in states with a sufficiently high degree of racial bias. First, bank deregulation induced a sharp increase in blacks' relative wages only in states with a high degree of racial bias. These results hold when allowing blacks' relative wages to vary over time within each state, over time within the banking industry, and across the banking industries of different states.

Second, bank deregulation increased blacks' relative wages more in bank-dependent manufacturing industries, which are those manufacturing industries in which bank deregulation intensified product market competition the most, but only in states with a sufficiently high degree of racial bias. These results hold when allowing blacks' relative wages in manufacturing to vary over time within each state. Moreover, even when allowing the relative wages of black workers within bank-dependent manufacturing industries to vary over time by state, we still find that the racial wage gap within a state's bank-dependent manufacturing industries shrunk relative to the racial wage gap within its other manufacturing industries following bank deregulation—but only in high racial bias states. Thus, the impact of bank deregulation on the racial wage gap is strongest exactly where the taste-based approach suggest it will be most pronounced—in the bank-dependent industries of high racial bias states.

Third, estimates for the overall economy confirm the negative association between competition and the racial wage gap within high racial bias states. Using inter- and intrastate bank deregulation as instrumental variables to identify exogenous shocks to the rate of new incorporations, we find that increases in the rate of new incorporations reduced the racial wage gap in high racial bias states. A ten percent increase in the rate of new incorporations reduced the racial wage gap by 2.5 percentage points.

We also examine segregation. Becker's (1957) theory predicts that when employers are heterogeneous in both quality and taste for discrimination, black workers will be hired by employers with the weakest racial prejudices, creating segregation in the workforce. A lowering of entry barriers that allows new employers with less of a taste for discrimination to enter the market increases the employment opportunities of black workers. Thus, if our findings on blacks' relative wages reflect the causal impact of competition on how racial prejudices affect labor markets, then we should also observe

greater integration following an intensification of competition.

We find that an exogenous increase in new incorporations reduced racial segregation in high racial bias states. To proxy for segregation, we construct measures of the degree to which each industry is disproportionately composed of white workers or run by white managers. In high racial bias states, an increase in new incorporations sparked by bank deregulation induced more blacks to work in historically "white" industries.

This paper's finding—that competition erodes the adverse effects of racial prejudices on the relative demand for black workers, boosting blacks' relative wages and promoting workplace integration—is robust to several potentially confounding influences. First, although deregulation could increase blacks' relative wages by reducing the relative supply of black workers, we find that increases in new incorporations boosted the relative working hours of black workers in high racial bias states. Second, there is no evidence that competition increased blacks' relative wages by disproportionately helping occupations and industries with a comparatively high proportion of blacks. Third, we find no evidence that the results are driven by changes in the skill composition of the labor force through the selection of workers into the labor force, interstate migration, and changes in self-employment. Fourth, bank deregulation improved black workers' location throughout white workers' residual wage distribution, indicating that competition boosted blacks' relative wages in particular, not the relative wages of comparatively low income workers in general.

We are not the first to examine competition and discrimination, though much of this research focuses on the gender wage gap. Oster (1975) compares market concentration and the gender wage gap across industries, obtaining mixed results. Within the banking industry, Black and Strahan (2001) show that bank deregulation reduced the gender income gap. Within manufacturing, Black and Brainerd (2004) find that globalization intensified competition and reduced the gender wage gap. Heywood and Peoples (1994) and Peoples and Talley (2001) find that the deregulation of trucking increased the relative wage rates of black workers. Focusing on integration, Ashenfelter and Hannan (1986) find a negative association between market concentration and the share of female employees across banking markets in Pennsylvania and New Jersey.

Our major contribution is that we provide the first evaluation of whether the impact of an exogenous intensification of competition on both blacks' relative wages and racial integration varies positively with the economy's taste for discrimination. That is, we show that competition increased the relative demand for black workers more in those environments in which the taste-based view of discrimination suggests that impact will

be largest, i.e., in economies with a sufficiently high degree of racial bias and, within those high racial bias economies, in industries that experience the largest intensification of competition.²

Our work complements recent work on racial prejudices. Charles and Guryan (2008) find that a stronger taste for discrimination by the marginal firm reduces blacks' relative wage rates. Bertrand and Mullainathan (2004) show that potential employers are more likely to request interviews based on resumes with "white" sounding names than equivalent resumes with "black" sounding names. Rather than evaluating the relation between racial prejudices and wages or job interviews, we examine the impact of changes in competition on changes in relative wage rates.

Our work also contributes to a growing literature on the broader ramifications of finance (Levine, 2005). Specifically, Beck, Levine, and Levkov (2010) show that deregulation triggered improvements in the banking system that boosted the wages of lower income workers. In this paper, we show that improvements in the banking system exerted a particularly positive impact on the economic opportunities of black workers by reducing the manifestation of racial prejudices in labor markets.

In what follows, Section 1 discusses bank deregulation as an exogenous source of variation in competition. Section 2 describes the data and econometric design. Section 3 presents the results, and Section 4 provides robustness tests. Section 5 concludes.

1 Bank Deregulation and Competition

1.1 A Brief History of Bank Branch Deregulation

The history of geographic restrictions on banking—along with standard econometric evidence—supports a key requirement of our estimation strategy: Namely, that bank deregulation is exogenous to competition and blacks' labor market outcomes. As described by White (1982), geographic restrictions on banking protected local banks from competition for much of the 20th century. By protecting inefficient banks, geographic restrictions created a powerful constituency for maintaining these regulations.

In the last quarter of the 20th century, however, technological, legal, and financial innovations diminished the economic and political power of banks benefiting from ge-

²Furthermore, unlike much of the existing literature, our findings are not subject to Ashenfelter and Hannan's (1986) critique of industry-level studies. If labor is mobile across industries within a state, then relative wages will primarily be established in the state's overall economy, not in separate industries. Under these conditions, it is crucial to examine *both* blacks' relative wages in the state's overall economy and racial segregation.

ographic restrictions. In particular, a series of innovations lowered the costs of using distant banks. This reduced the monopoly power of local banks and weakened their ability and desire to lobby for geographic restrictions. For example, the invention of automatic teller machines (ATMs), in conjunction with court rulings that ATMs are not bank branches, weakened the geographical link between banks and their clientele. Furthermore, the creation of checkable money market mutual funds made banking by mail and telephone easier, thus further weakening the power of local bank monopolies. Finally, the increasing sophistication of credit scoring techniques, improvements in information processing, and the revolution in telecommunications reduced the informational advantages of local bankers, especially with regards to small and new firms.

These national developments interacted with preexisting state characteristics to shape the timing of bank deregulation across the states. As shown by Kroszner and Strahan (1999), deregulation occurred later in states where potential losers from deregulation (small, monopolistic banks) were financially stronger and had a lot of political power. On the other hand, deregulation occurred earlier in states where potential winners of deregulation (small firms) were relatively numerous. Most states deregulated geographic restrictions on banking between the mid-1970s and 1994, when the Riegle-Neal Act effectively eliminated these restrictions.

Research also indicates that the forces driving bank deregulation were exogenous to competition in the non-financial sector and the racial wage gap. The timing of deregulation was not shaped by new firm formation (Black and Strahan, 2002, Kerr and Nanda, 2009), the strength of labor unions (Black and Strahan, 2001), or the degree of earnings inequality (Beck, Levine, and Levkov, 2010). Moreover, we show below that the racial wage gap does not explain the timing of bank deregulation.

1.2 Bank Deregulation and Competition in the Banking and Non-Financial Sectors

Bank deregulation increased competition within the banking sector by making it possible for banks to (a) open branches across markets within a state and (b) open subsidiaries in other states. By increasing competition among banks, deregulation improved banking sector performance. It reduced interest rates on loans, raised them on deposits, lowered overhead costs, and shrunk the proportion of bad loans (Jayaratne and Strahan, 1998). And, by enhancing the contestability of banking markets, deregulation expedited the development of better techniques for evaluating firms (Hubbard

and Palia, 1995).³

In boosting banking sector performance, bank deregulation spurred competition in the nonfinancial sectors. Improvements in banking—lower lending rates, better screening of borrowers, etc.—lowered financial barriers facing new firms, intensifying competition in the overall economy. Black and Strahan (2002) find that deregulation helped entrepreneurs start new businesses, with the rate of new incorporations per capita in a state increasing by six percentage points following deregulation. Kerr and Nanda (2009) find that interstate deregulation increased the number of new start-ups by six percentage points and expanded the number of facilities of existing firms by four percentage points. Kerr and Nanda (2009) also find a dramatic increase in both the entry and exit of firms, suggesting that deregulation increased contestability throughout the economy. And, Cetorelli and Strahan (2006) show that bank deregulation had a particularly big impact on lowering barriers to entry in bank-dependent industries, which are industries in which firms are naturally heavy users of bank finance.

2 Data

In turning toward an assessment of the relationship between bank deregulation, competition, and the racial wage gap, this section first describes the data on the (a) timing of state bank deregulation, (b) competition in each state’s economy, (c) wages of black workers relative to those of white workers with the same skills, and (d) measures of racial attitudes at the state level.

2.1 State-level data on deregulation and competition

We obtain the dates of interstate and intrastate bank deregulation from Kroszner and Strahan (1999) and Amel (2008). Most states removed these geographic restrictions on banking between the mid-1970s and 1994, when they were eliminated by federal legislation. The Annex, which is available on request, provides the deregulation dates for each state.

³Hubbard and Palia (1995) show that (a) the country’s more innovative banks were developing sophisticated credit-scoring techniques that improved the screening of new businesses and (b) deregulation – by enhancing the contestability of banking markets – spurred the spread of these techniques. By improving banking performance, deregulation lowered the barriers to new, nonfinancial firms entering the market. Furthermore, deregulation fostered the formation of larger, geographically diversified banks, potentially easing lending to smaller, more opaque firms, as Berger et al. (1998) show that small business lending increases after small banks are acquired.

Since bank deregulation lowered entry barriers—and hence intensified competition—more in bank-dependent manufacturing industries than in other industries (Cetorelli and Strahan, 2006), we assess whether bank regulation affected the racial wage gap more in bank-dependent industries.

To measure bank-dependence, we follow Cetorelli and Strahan (2006). They note that (a) industries differ in their dependence on bank financing and (b) these differences reflect technological differences associated with the initial scale of the project, the gestation period of the endeavor, the expected pattern of cash-flows from the project, and the likely demands for additional injections of capital. Thus, some industries are heavily bank-dependent, such as Electronics and Chemical Equipment, while other industries are less bank-dependent, such as Leather or Tobacco. In finding that bank deregulation lowers entry barriers more in bank-dependent industries, Cetorelli and Strahan (2006) use the median ratio of bank loans to total firm assets across firms in each industry to measure bank-dependence.⁴

Since the taste-based theory of discrimination focuses on the actual entry of new firms, we use the rate of new incorporations to measure competition. Specifically, we use the log of new business incorporations per capita for each state over the period 1977-1994, where the new incorporations data are from Black and Strahan (2002), who obtain them from Dun and Bradstreet.

2.2 Generating Relative Residual Wages

2.2.1 CPS Samples for the Years 1977 to 2007

Data on wages and worker characteristics are from the Integrated Public Use Microdata Series (IPUMS) from the U.S. Current Population Survey (CPS, March Supplements for the survey years 1977 to 2007). The CPS March Annual Demographic Supplements provide information on earnings, along with weeks and hours worked in the calendar year preceding the March survey so that the 1991 survey provides information on earnings in 1990. We start in Survey year 1977 because that is when the CPS

⁴Cetorelli and Strahan (2006) use data from the 1998 Survey of Small Business Finance. Specifically, manufacturing industries with above the median levels of bank dependence are: (1) furniture and fixtures, (2) stone, clay, glass, and concrete products, (3) printing and publishing, (4) primary metal industries, (5) lumber and wood products, (6) rubber and plastic products, (7) paper and allied products, petroleum and coal products, (8) textile mill products, and (9) chemical and allied products. Those with below the median levels of bank dependence are: (1) leather and leather products, (2) apparel and other textiles, (3) food and kindred products, (4) fabricated metal products, (5) miscellaneous manufacturing, (6) instruments and related products, (7) transportation equipment, (8) industrial machinery and equipment, and (9) electrical and electronic equipment.

reports information on each person’s state of residence. To enhance comparability and connect our analyses to the literature, we restrict our sample to non-Hispanic white and black adult civilian males between the ages of 18 and 65 during the working year, and exclude persons living in group quarters or with missing data on relevant demographics. Our main wage sample further excludes the self-employed, persons in the military, agricultural, or private household sectors, persons with inconsistent reports on earnings, and individuals with allocated earnings.

We classify the adult population into six educational categories: (i) persons with 0–8 years of schooling completed; (ii) high school dropouts; (iii) high school graduates; (iv) some college; (v) college graduate; and (vi) advanced degree. Potential work experience is constructed as the maximum between zero and age (in year of survey) minus years of schooling completed minus seven. Furthermore, in some specifications, we differentiate workers by industry and occupation. We use the three digit industry and occupation codes from the CPS, which means including 408 additional dummy variables each year (144 industries and 262 occupations).

Wage rates are defined as real annual earnings divided by the product of weekly working hours and annual working weeks. We use the Consumer Price Index to deflate earnings to 2000 dollars and set hourly earnings to missing if any of these components is missing or zero. Following Autor, Katz, and Kearney (2008), workers with top coded earnings have their annual earnings set to 1.5 times the annual top-code amount. We trim outliers with hourly wages below the 1st percentile and above the 97th percentile of the year-specific distribution of hourly earnings of full-time, full-year workers. This trimming virtually eliminates individuals with top-coded annual earnings. The results are robust to altering the definition of outliers. Finally, in accord with previous research on bank deregulation, we drop Delaware and South Dakota from our analyses due to large concentration of credit card banks in these states. A detailed Annex, available on request, provides more details on the construction of our sample.

2.2.2 Relative residual wages: Framework

We decompose the black-white wage differential into "explained" and "residual" components. In particular, assume that log hourly wages for a white individual i in state s at time t can be written as:

$$W_{ist}^W = X_{ist}\theta_t^W + R_{ist}^W, \tag{1}$$

and log hourly wages for a black individual i in state s at time t can be written as:

$$W_{ist}^B = X_{ist}\theta_t^B + R_{ist}^B, \quad (2)$$

where X_{ist} represents individual characteristics associated with log hourly wages in state s in year t , including Mincerian characteristics, such as education and experience, and state-year fixed effects. The parameters, θ_t^W and θ_t^B , are defined so that $E(R_{st}^W | X_{st}^W) = 0$ and $E(R_{st}^B | X_{st}^B) = 0$, where X_{st}^W (X_{st}^B) is the mean X_{ist} of white (black) workers in state s in year t , and R_{st}^W (R_{st}^B) is the mean value of R_{ist}^W (R_{ist}^B) across white (black) workers s in year t . Thus, the mean wage across white workers in state s in year t is defined as $W_{st}^W = X_{st}^W\theta_t^W$ and the corresponding value for black workers is $W_{st}^B = X_{st}^B\theta_t^B$.

Thus, we can define the mean black-white wage differential in state s in year t as:

$$W_{st}^B - W_{st}^W = \Delta X_{st}\theta_t^W + X_{st}^B\Delta\theta_t = \Delta X_{st}\theta_t^W + R_{Bst}, \quad (3)$$

where $\Delta X_{st} = X_{st}^B - X_{st}^W$, $\Delta\theta_t = \theta_t^B - \theta_t^W$, and $X_{st}^B\Delta\theta_t = R_{Bst}^W$.

The "explained" component of the black-white wage differential is $\Delta X_{st}\theta_t^W$. It represent the mean wage differential explained by the mean observed "skill" differential between black and white workers ΔX_{st} , where these skill differences are valued or "priced" using the returns that the average white worker gets for these skills (θ_t^W).

The "residual" component, $X_{st}^B\Delta\theta_t$, which we refer to as R_{Bst} for simplicity, is that part of the mean black-white wage differential unaccounted for by mean skill differentials. The residual component represents the average wage gap between black and white workers with identical characteristics that emerges because of racial differences in the returns to these characteristics ($\Delta\theta_t = \theta_t^B - \theta_t^W$). Recall, these characteristics include standard, observable Mincerian traits as well as unobservable differences in the average productive characteristics of black and white workers at the state-year level.

Thus, R_{Bst} captures both the effects of labor market discrimination and unobserved productivity differences between black and white workers. A large literature focuses on identifying the role of these two sources. For example, Neal and Johnson (1996) attribute much of the unexplained gap in wages to differences in cognitive abilities. In this paper we focus on evaluating the effect of competition on labor market discrimination, i.e., the effect of competition on racial differences in the "prices" of skills. We use the differential timing of bank deregulation across states, its differential impact on industries, and differences in the taste for discrimination across states to identify the

effect of competition on labor market discrimination against black workers.

2.2.3 Relative residual wages: Estimation

First we estimate equation (1) separately for each year.⁵ Then, employed with θ_t^W , we compute residual wages (R_{ist}) for all workers, white and black:

$$R_{ist} = W_{ist} - \theta_t^W X_{ist}, \quad (4)$$

By construction, R_{st} for white workers equals zero in each state-year. For black workers the average relative residual wage, R_{Bst} , can differ from zero. Since X_{ist} effectively includes state-year effects (and state-industry-year effects in some specifications), the relative residual wages already account for state-year (or state-year-industry) effects on white workers' wages, including the effect of *banking deregulation* on the wage rates of white workers.

In some of our analyses, we control for additional factors in equation (1) before constructing residual wages in equation (4). Specifically, we estimate the effect of bank deregulation on black workers' relative wages in the banking industry, in manufacturing by the need for external finance, and in the overall economy. In the banking analyses, we introduce bank-year effects into equation (1) that condition out the impact of bank deregulation on white workers' wages in and outside of the banking industry. For the manufacturing level analyses, we include dummy variables for whether the worker is employed in a state's manufacturing industries and whether the worker is employed in a state's bank-dependent industries by year.

By controlling for these wage rate determinants, we account for the impact of bank deregulation on white workers' wages. If bank deregulation affects wages but does not affect labor market discrimination or the unobservable differences in the mean productive characteristics of black and white workers in a state, then we should find no association between deregulation and blacks relative residual wages.

Finally, rather than using this two-step procedure, we could have equivalently run a single wage rate regression that includes sufficient interaction terms based on race, year, state, and demographics to capture the attractive properties mentioned above.

⁵Given changes in the structure of wages in the United States since the mid 1970s (Katz and Autor, 1999), we allow the Mincerian returns to observable skills (θ_t^W) to vary by year. This is crucial because of the the well-documented skill gap between black and white workers. Failure to account for time-varying returns to observables will lead to erroneous estimates of the dynamic pattern of relative wages, potentially biasing our assessments.

Although they yield the same results, the two-step approach is clearer and computationally faster.

2.3 Racial Bias Indexes

Throughout our analyses, we explicitly account for cross-state differences in the taste for discrimination. This is both novel and essential to drawing accurate inferences because competition should have a larger impact on blacks' relative wages in states with a greater taste for discrimination (all other things equal).

We develop two types of racial bias indexes based on the accumulated stock of racial intermarriage in 1970. We use the 1970 Census to construct information on the rate of racial intermarriage in each state (Form 1 State, and Form 2 State one-percent samples). The Census samples are the largest microdata set containing detailed marriage and demographic information. Our primary sample includes married whites and blacks between the ages of 18 and 65, and excludes couples in which at least one person is living in group quarter or has missing data on race, gender, state of residence, marital status and educational attainment.

The "simple" racial bias index equals the difference between the rate of intermarriage that would exist if married people were randomly matched and the actual intermarriage rate. The random rate equals $2P*(1-P)$, where P is the proportion of blacks among the married population. Larger values of the simple racial bias index indicate that intermarriage occurs less in practice than if marriage pairings were random. We interpret larger values as (partially) reflecting racial bias.

In the second type of racial bias index, we account for other factors that might induce the actual rate of intermarriage to deviate from the random rate. Intermarriage depends on the opportunities for interracial social contacts, so that the relative sizes of the black-white populations might independently affect intermarriage (Blau, 1977). Furthermore, since the odds of interethnic unions increase with couples' educational attainment (Massey and Denton, 1987; Qian, 1997; Rubinstein and Brenner, 2009), we also control for education and age. Specifically, based on the 1970s census, we estimate the following equation for all married couples (excluding couples in which either the husband or wife is neither white nor black) in the United States:

$$I_{is} = bH_{is} + cW_{is} + dS_s + \tau_{is}, \tag{5}$$

where I_{is} equals one if couple i in state s is racially mixed and zero otherwise, H_{is} and

W_{is} are vectors of age and education characteristics for the two spouses respectively, S_s are state characteristics, τ_{is} is the unexplained component of intermarriage, while b , c , and d are coefficients. Our benchmark specification conditions on nine categories of education, along with age entered as a quartic. For state characteristics, we include the random intermarriage rate defined above along with the percentage of blacks among married couples. We experimented with numerous specifications, including and excluding the random intermarriage rate and the percentage of blacks, changing the specification of education and age controls, and conditioning on metropolitan and urban locations. These combinations produce the same conclusions.

From equation (5), we compute the intermarriage racial bias index for each state. Let τ_s equal the average value of τ_{is} across couples in state s . Recognizing that $\min\{\tau_s\} < 0$, we compute the racial bias index as $\tilde{T}_s = -\tau_s + \max\{\tau_s\}$, so that \tilde{T}_s equals zero for the state with the largest τ_s . We interpret large values as signaling a stronger taste for discrimination. Appendix Table 1 provides the value of the racial bias index, \tilde{T}_s , for each state and the District of Columbia. Furthermore, $T_s = 1$ if $\tilde{T}_s \geq \text{median}\{\tilde{T}_s\}$, and $T_s = 0$ if $\tilde{T}_s < \text{median}\{\tilde{T}_s\}$.

The intermarriage racial bias index is positively correlated with survey-based measures of racial prejudice. Table 1 (Panel A) shows that the intermarriage racial bias index is positively related to three survey-based measures of racial prejudice used by Charles and Guryan (2008) in their study of relative wages and racial prejudices: (1) the fraction of whites supporting a law against interracial marriage, (2) the fraction of whites that would not vote for a black president, and (3) the fraction of whites supporting the right to segregate neighborhoods by race. Thus, the racial bias index based on intermarriage in 1970 is closely associated with subjective measures of racial attitudes measured over the period 1972 to 2004.

The intermarriage racial bias index is negatively correlated with the relative wage rates of black workers. Panel B of Table 1 shows that the intermarriage racial bias index is strongly, negatively associated with blacks' relative wage rates in the years prior to both inter- and intrastate bank deregulation, suggesting that the racial bias index captures cross-state differences in the relative demand for black workers. The negative relation between the intermarriage racial bias index and blacks' relative wages is robust to controlling for the supply of blacks in the workforce as shown in Panel B of Table 1. Consistent with Becker's (1957) theory, states in which black workers compose ten percent or more of the labor force tend to have lower relative wage rates for black workers than other states, but the racial bias index remains negatively and significantly

associated with blacks' relative wages.

We also use the Charles and Guryan (2008) survey-based estimates of the degree of racial prejudice of the marginal firm in each state to categorize high- and low-racial bias states. As shown in Panel B of Table 1, states with above the median levels of this marginal racial prejudice indicator have significantly lower relative wages of black workers. Nonetheless, the racial bias index based on racial intermarriage remains negatively and significantly associated with blacks' relative wages even when controlling for the marginal racial prejudice indicator and when controlling for both the marginal racial prejudice indicator and the proportion of blacks in the workforce.

For the purposes of this paper, there are advantages to using the intermarriage racial bias index rather than survey-based measures of racial attitudes, though we draw consistent conclusions with either racial bias indicator. The intermarriage racial bias index is based on actual choices made prior to deregulation not survey responses made during the period of deregulation. Moreover, our empirical strategy requires that the measure of racial bias is invariant to bank deregulation and the resultant change in competition. If we differentiate states based on a measure of racial bias that itself reflects the effects of deregulation on the relative demand and supply of black workers, this will confound our strategy of identifying the causal impact of product market competition on the relative demand for black workers. The racial attitude surveys, however, are conducted during the period of bank deregulation. Furthermore, unlike Charles and Guryan (2008), we do not want to measure the racial preferences of the marginal employer. This will incorporate influences of both the relative demand for and supply of black workers. Rather, theory predicts that an intensification of competition will increase the relative demand for black workers and hence boost blacks' relative wages in states with a sufficiently high taste for discrimination, while holding the relative supply of black workers fixed. We test this.

In summary, we evaluate whether an exogenous increase in competition boosts the relative demand for black workers more in states with larger values of the racial bias indices. Measuring racial bias with error will bias the results against finding statistically significant results. We do not require that the racial bias measures are perfect; rather, we simply require that they provide information on racial prejudices across states.

3 Results

3.1 Preliminaries

Our empirical analyses rest on the assumption that the racial wage gap did not shape the cross-state timing of both interstate and intrastate bank deregulation. As discussed above, Kroszner and Strahan's (1999) study of the causes of these banking reforms stresses the interaction between technological innovation and the preexisting structure of banking markets, not attitudes toward race. To assess this timing issue further, we graph the relationship between the racial wage gap and the timing of deregulation.

Figure 1 shows that neither the *level* of the racial wage gap before deregulation (Panels A and B) nor its rate of *change* prior to deregulation (Panels C and D) explains cross-state differences in the timing of interstate or intrastate bank deregulation, where the size of the "bubbles" represents the size of the black workforce in each state. There is no evidence that the racial wage gap influences the timing of bank deregulation.

3.2 Bank Deregulation and Blacks' Relative Wages: Banking

We begin by assessing the impact of bank deregulation on blacks' relative wages within the banking industry, while differentiating between high and low racial bias states. As noted above, bank deregulation intensified competition among banks. Thus, in evaluating the relationship between competition and racial discrimination, it is natural to first test whether bank deregulation boosted the relative wages of black workers within the banking industry.

The dependent variable is the residual wage gap (R_{ist}), which is estimated in the spirit of equation (4). In particular, we estimate a Mincerian wage equation for white workers, separately for each year, while also including state fixed effects and a dummy variable that equals one if the worker is employed within a specific state's banking industry. Since this is estimated separately for each year, we allow for (1) state-year fixed effects, (2) bank-year fixed effects, and (3) the coefficients on the Mincerian characteristics to vary by year. We use the estimated coefficients and the actual characteristics of *all* workers to construct the residual wage gap for white and black workers. By construction, the residual wage gap for white workers sums to zero (a) within a state-year and (b) within *and* outside of the banking industry in each state-year.

To evaluate the impact of bank deregulation on blacks' relative wages, we estimate the following OLS equation:

$$R_{ist} = \beta[D_{st} * BNK_i * B_i] + \delta_{BNK,B,s} + \delta_{BNK,B,t} + \delta_{B,s,t} + \nu_{ist}, \quad (6)$$

where D_{st} equals one when state s in year t has deregulated interstate banking, BNK_i equals one if individual i works in the banking industry, and B_i equals one if individual i is black. The impact of deregulation on competition within the banking industry was immediate, as shown by Jayaratne and Strahan (1998) and Black and Strahan (2001), so we use a simple zero-one dummy variable to indicate before and after deregulation.⁶

Since blacks' relative wages might vary across states, industries, and time for reasons other than bank deregulation, we account for other possible factors. Specifically, we introduce a set of black-state-year fixed effects ($\delta_{B,s,t}$), bank-black-year fixed effects ($\delta_{BNK,B,t}$), and bank-black-state fixed effects ($\delta_{BNK,B,s}$).

The parameter of interest is the coefficient (β) on the triple interaction term ($D_{st} * BNK_i * B_i$). This regression coefficient measures the change in the mean residual wages of black workers in the banking industry *relative to* the change in the mean residual wages of black workers in all other sectors of the economy following bank deregulation.

Table 2 shows the results from estimating equation (6) for the full sample of states, and when distinguishing between high and low racial bias states, i.e., states that have above and below the median levels of the racial bias index. When splitting the sample by racial bias, we allow all of the coefficients, including the black-bank-year effects to differ across these two samples.

When not distinguishing states by measures of racial bias and simply estimating equation (6) for the full sample of states, we do not observe a statistically significant relationship between deregulation and blacks' relative wages. Column (1) provides the results for the full sample of states, imposing a common effect of bank deregulation on the residual wages of black workers in the banking sector across high and low racial bias states. We find a positive, though statistically insignificant effect of 0.16 log points, which is of similar magnitude to that found in Black and Strahan's (2001) study of how women's wages within banking responded to bank deregulation.

By differentiating states by racial bias, we are able to draw sharper inferences:

⁶Black and Strahan (2001) also use a zero-one dummy variable in their examination of deregulation and women's relative wages within banking. Thus, we report our results using their estimation period, which covers the period 1976-1996, though we obtain virtually identical results when using the 1976-2006 period. As emphasized, our work is different. We examine blacks' relative wages, distinguish between high and low racial bias states, use more refined fixed effects, and later examine the ramifications of deregulation—and most importantly competition as measured by new firms entry—on the aggregate economy, not just among the few black workers within the banking industry. Furthermore, our results hold when also including the *Intrastate* deregulation index.

bank deregulation is associated with an increase in blacks' relative wages *only* in high racial bias states. When splitting the sample between states with below and above the median values of the racial bias index (columns (2) and (3)), we find that deregulation boosted blacks' relative wages exactly where the intuition of the taste based theory of discrimination predicts it will: in economies where there is a substantial "taste for discrimination." And, deregulation did not boost blacks' relative wages exactly where the taste based approach suggests it would not: in low racial bias states.⁷ This conclusion is further supported by the results in column (4). There, we include all of the states together and employ a quadruple interaction term involving bank deregulation, a banking industry dummy, a race dummy, and the racial bias dummy. Again, the results indicate that bank deregulation boosted blacks' relative wages within the banking industry, but only within high racial states.

In high racial bias states, the estimated impact of bank deregulation on blacks' relative wages within the banking sector is economically large. Indeed, the point estimate is implausibly large. The estimated coefficient indicates that blacks' relative wages rise 58 percentage points after deregulation in high racial bias states. While acknowledging the large coefficient, it is also worth noting that more plausible effects, such as an 18% increase in blacks' relative wages, are within two standard deviations of the estimated coefficient, β , on the triple interaction term. Furthermore, we have thus far ignored the impact of bank deregulation on the skill composition of black and white workers. If bank deregulation by a state attracts black workers with better unobserved skills into a state's banking industry, then our estimates will overstate the impact of deregulation on blacks' relative wages. Below, when examining the entire economy, we show that such selection biases do not affect the magnitude of our findings.

While illustrative, these banking industry analyses have statistical and conceptual limitations. Statistically, the estimates in Table 2 are based on an exceedingly small number of black workers within the banking industry. For instance, in some years, our sample only includes about 100 black workers in the banking industry. Conceptually, banking is a very unique service sector, in which legal barriers formed an extreme barrier to competition over many decades that created huge rents. Thus, the banking sector is not necessarily "a representative industry" for assessing the impact of competition

⁷To be precise, Becker (1957) explicitly recognized that one could construct joint distributions of the quality and racial prejudices of existing—and shadow—employers such that an increase in competition will not boost blacks' relative wages. We simply observe that the typical articulation of the taste-based model suggests that an intensification of competition reduces the manifestation of racial prejudices in labor markets. And, we empirically assess this prediction.

on racial wage discrimination.

It would be valuable, therefore, to examine the impact of an exogenous intensification of competition on the manifestation of racial prejudices within a broader array of industries. As a further motivation for adopting a broader approach and as emphasized in the Introduction, Ashenfelter and Hannan (1986) question the value of industry-specific studies of the racial wage gap when labor is highly mobile across industries and recommend examining more industries and sectors of the economy.

3.3 Bank Deregulation and Blacks' Relative Wages: Manufacturing

We now examine manufacturing industries. Cetorelli and Strahan (2006) show that bank deregulation had an especially positive effect on competition among firms within those manufacturing industries that are naturally heavy users of bank finance, i.e., bank-dependent industries. Thus, we exploit the differential impact of bank deregulation on competition across manufacturing industries. Using Cetorelli and Strahan's (2006) sample period and measure of bank dependence, we assess whether the impact of deregulation on blacks' relative wages varies positively with the degree of bank-dependence of the worker's manufacturing industry.

We again use a modified version of the residual wage gap (R_{ist}) from equation (4) as the dependent variable. To compute the residual wage gap, we estimate a Mincerian wage equation for white workers, separately for each year, while including (a) state fixed effects, (b) a dummy variable that equals one if the worker is employed within a manufacturing sector, and (c) a dummy variable that equals one if the worker is employed within a bank-dependent manufacturing industry, i.e., a manufacturing industry with above the median bank-dependence. Thus, by construction, the residual wage gap for white workers sums to zero (a) within a state-year, (b) within *and* outside of manufacturing industries by year, and (c) within *and* outside of bank-dependent industries by year.

This means that the residual wage gap already controls for any possible effect of bank deregulation on the wages of white workers across states and time, between manufacturing and nonmanufacturing industries, and between high bank-dependent and low bank-dependent manufacturing industries. This helps in isolating the relationship between bank deregulation and the residual wages of black workers within high and low bank-dependent industries.

Thus, we estimate the impact of bank deregulation on the racial wage gap *only* across manufacturing industries using the following equation:

$$R_{ist} = \beta[D_{st} * BD_i * B_i] + \delta_{BD,B,s} + \delta_{B,s,t} + \nu_{ist}, \quad (7)$$

where D_{st} equals one when state s in year t has deregulated interstate banking, BD_i equals one if individual i works in a bank-dependent manufacturing industry, and B_i equals one if individual i is black. Since several factors besides bank deregulation could influence blacks' relative wages within bank-dependent manufacturing industries, equation (7) introduces an array of fixed effects: $\delta_{BD,B,s}$ is a vector of bank-dependent-black-year fixed effects and $\delta_{B,s,t}$ is a vector of black-state-year fixed effects.

The parameter of interest (β), the coefficient on the triple interaction term ($D_{st} * BD_i * B_i$), measures the change in the mean residual wages of black workers within a state's bank-dependent manufacturing industries relative to changes in the mean residual wages of black workers within other manufacturing industries in that state following bank deregulation. This specification allows (1) the relative wages of black workers within bank dependent manufacturing industries to vary differentially from white workers across states, and (2) the relative wages of black workers within manufacturing to vary differentially from corresponding white workers by state and time.

The Table 3 results show that bank deregulation decreased the racial wage gap in bank-dependent manufacturing industries relative to the racial wage gap in other manufacturing industries but only within high racial bias states. When the sample includes all of the states and we do not differentiate by racial bias (column 1), the triple interaction term enters positively and significantly: blacks' relative wages rise in bank-dependent manufacturing industries after bank regulation. Further inspection, however, demonstrates that this result reflects the impact of competition on blacks' relative wages within high racial bias states.

The analyses again stress the importance of differentiating states by taste for discrimination. Columns (3) and (4) show that bank deregulation only boosted blacks' relative wages in bank-dependent industries within high racial bias states. Within low racial bias states, bank deregulation has no effect on blacks' relative wages, regardless of which manufacturing industry employs the worker. The regression presented in column (2) shows that the results hold when using an interaction term to differentiate among high and low racial bias states instead of splitting the sample.⁸

So far, the analyses have allowed blacks' relative wages within manufacturing to vary by state over time. But, we have not yet allowed blacks' relative wages within

⁸The results are robust to conducting the estimation over the longer sample period, from 1976 - 2006, and to including *Intrastate* deregulation in the regression.

bank dependent manufacturing industries to vary by state over time relative to blacks' relative wages within other manufacturing industries. We do this in column (5) by including bank-dependent-black-year fixed effects and employing a quadruple interaction term based on dummy variables for bank deregulation, bank-dependent manufacturing industries, race, and the state-specific racial bias indicator. As shown, the results hold and there is essentially no change in the estimated coefficient.

Thus, bank deregulation boosted blacks' relative wages only where the taste-based approach to racial discrimination suggests it should: in states where there is a strong taste for discrimination (high racial bias states) and in industries where deregulation had a big effect on competition (bank-dependent industries). And, bank deregulation did not close the racial gap where the taste based approach suggests it would not: in low racial bias states and in manufacturing industries that do not receive much of a spur to competition from bank deregulation.

The estimated impact is large. In high racial bias states, deregulation was associated with an increase in blacks' relative wages of about 10% within bank-dependent manufacturing industries, implying a halving of the racial wage gap.

Differentiating states by measures of racial bias is crucial for drawing accurate inferences. One might argue that bank deregulation increased the relative wages of black workers within bank-dependent industries through a mechanism other than competition. But, it is difficult to explain why bank deregulation increased the relative wages of black workers *only* in bank-dependent industries within high racial bias states, without turning to the intuition underlying the taste based theory of racial discrimination.

3.4 Deregulation and Blacks' Relative Wages: Entire Economy

While these studies of the banking and manufacturing industries provide valuable information, there are at least four reasons for examining the entire economy. First, if there are few barriers to workers moving from the banking industry to the rest of the economy or a high degree of labor mobility among manufacturing industries within a state, then it is worthwhile examining relative wages in the state's overall economy, not simply in a subset of industries.

Second, by examining the entire economy, we can assess whether bank deregulation reduced the racial wage gap by spurring the entry of new firms, as suggested by taste based theories of discrimination. Black and Strahan (2002) and Kerr and Nanda (2009) show that state bank deregulation reduced entry barriers and increased new firm formation. We test whether deregulation boosted blacks' relative wages by accelerating

new firm entry. While the earlier manufacturing level analyses allowed us to compare industries within a state, examining the entire state economy provides information on a possible mechanism—new firm entry—linking bank deregulation and the racial wage gap.⁹

Third and related, we evaluate the dynamic relationships among bank deregulation, new firm entry, and the racial wage gap. This provides an opportunity to distinguish between the effects of deregulation on the racial wage gap and other trends that might affect blacks' relative wages. Furthermore, as we show, the impact of deregulation on new firm entry emerges over time. If deregulation is boosting black's relative wages by lowering entry barriers and fostering the entry of new firms, then we should find that the impact of deregulation on the racial wage gap also emerges over time.

Fourth, we have a unique opportunity to assess the broader ramifications of a better functioning financial system on labor markets. Past work shows that bank deregulation triggered an intensification of competition throughout many segments of the economy, not just in banking and manufacturing. Consequently, we focus the remainder of our analyses on assessing the impact of competition on the relative demand for black workers in the entire economy.

3.4.1 Reduced Form Analyses of Bank Deregulation and Blacks' Relative Wages in the Entire Economy

Our examination of blacks' relative wages in the entire economy proceeds in two parts. In this subsection, we assess the reduced form relationship between bank deregulation and the racial wage gap—both by using the types of regressions employed thus far and by assessing the dynamic relation between deregulation and blacks' relative wages. In the next subsection, we use two-stage least squares to evaluate the impact of new firm entry on the racial wage gap.

We assess the reduced form impact of bank deregulation on the relative wage rates of black workers using three specifications. First, blacks' relative wages are regressed on bank deregulation using the full sample. Second, we add an interaction term of deregulation and the racial bias dummy for each state. Third, we split the sample by the median value of the racial bias index, which allows the coefficients on state and year fixed effects to differ across the subsamples. Throughout, we include state and year fixed effects. We examine each form of deregulation – *Interstate* and *Intrastate*.¹⁰

⁹These data are unavailable at the industry level for our sample period.

¹⁰As noted above, the earlier results hold when including both forms of bank deregulation. We

Table 4 shows that bank deregulation boosted the relative wage rates of black workers only in high racial bias states. By showing that bank deregulation only increased blacks' relative wages in high racial bias states, we reduce concerns that some confounding factor both reduces racial discrimination and just happens to be coincident with the state-specific timing of bank deregulation.

The estimated reduction in the racial wage gap from bank deregulation is economically meaningful. Consider column (4), which provides the regression results for states with above the median value of the racial bias index. Deregulation boosted the wage rates of black workers by 6 percentage points more than their white counterparts after five years ($6 = 0.012 * 5 * 100$). Since the average racial wage gap in these high-bias states was 21 percent in 1976, the results suggest that interstate deregulation eliminates almost one-third of the initial racial wage gap. The results are virtually identical when using *Intrastate*, as shown in column (8).

3.4.2 *Dynamic Analyses of Deregulation and Blacks' Relative Wages: Entire Economy*

We now extend these analyses by examining the dynamic relationship between deregulation and blacks' relative wages. In Figure 2, we trace out the year-by-year relationship between deregulation and the racial wage gap by estimating the following equation:

$$R_{Bst} = \alpha + \beta_1 D_{st}^{-10} + \beta_2 D_{st}^{-9} + \dots + \beta_{25} D_{st}^{+15} + \delta_s + \delta_t + \varepsilon_{st}, \quad (8)$$

where D^{-j} equals one for the j^{th} year before deregulation, and D^{+k} equals one for the k^{th} year after deregulation. These dummy variables equal zero in other years. The year of deregulation is omitted and the regressions include state (δ_s) and year (δ_t) fixed effects. Equation (8) is estimated separately for inter- and intrastate deregulation.¹¹ Panel A includes states with above the median values of the racial bias index and Panel B includes low racial bias states.

There are three crucial messages. First, the impact of both interstate and intrastate bank deregulation on blacks' relative wages is much greater in states where the racial

employ both types in assessing the entire economy to illustrate the robustness of the results, enhance the dynamic analyses, and overidentify the two-stage least squares regressions.

¹¹Before plotting the estimates, we detrend the series as follows. We compute the trend in the coefficients on the dummy variables on bank deregulation prior to deregulation. We then detrend the entire series of estimated coefficients based on the pre-deregulation trend. The resulting figure illustrates the level and trend of blacks' relative wages after bank deregulation relative to the patterns before deregulation.

bias index is above the median than in states with lower values of the racial bias index. Second, there is no evidence that trends or innovations in the wage gap precede either interstate or intrastate bank deregulation. Rather, blacks' relative wages rise after bank deregulation in states with high values of the racial bias index. Third, the impact of deregulation on black's relative wages grows over time.

3.5 Bank Deregulation, New Firm Entry, and Blacks' Relative Wages

While the reduced form and dynamic analyses demonstrate the powerful impact of bank deregulation on the racial wage gap, they do not provide direct evidence on the mechanism linking bank deregulation and blacks' relative wages. We now examine the relationship between the rate of new incorporations and blacks' relative wages to assess whether, and under which conditions, bank deregulation triggers an increase in the rate of new incorporations that in turn reduces the racial wage gap. Thus, we use bank deregulation as an instrument for the rate of new incorporations and assess its impact on blacks' relative wages, distinguishing between high and low racial bias states.

3.5.1 *The Structure of the 2SLS Analyses*

To obtain a consistent estimate of the impact of the rate of new incorporations in each state and year (N_{st}) on relative wages, we need a valid instrumental variable. It is important to instrument for competition because blacks' relative wages could affect the actual entry of firms. For example, firms could enter to exploit the opportunity to hire less expensive labor in states with a large racial wage gap. If this occurs, OLS will underestimate the causal impact of competition on blacks' relative wages.

Thus, we employ the following two-stage least squares (2SLS) structure to capture the causal relationship of interest,

$$R_{Bst} = \beta_0 N_{st} + \delta_s + \delta_t + \epsilon_{ist}, \quad (9)$$

where the predicted value of the rate of new firm entry is obtained from the first stage regression using bank deregulation as an instrument:

$$N_{st} = D'_{st} \gamma_0 + \pi_s + \pi_t + \eta_{st}, \quad (10)$$

where D_{st} is a vector indicating years since bank deregulation, δ_s and π_s are state-specific effects, δ_t and π_t represent year effects, ϵ_{ist} is an error term composed of a person specific idiosyncratic shock to relative wages and any unobserved state-year

fixed effects, and η_{st} is an error term.¹² The standard errors are clustered at the state-year level throughout the analyses. The parameter of interest, β_0 , is the estimated impact of competition on the relative wage rates of black workers.

We assess whether the impact of new firm entry on the racial wage gap depends on the degree to which states have a stronger or weaker taste for discrimination. We do this both by splitting the sample by the racial bias index and by including an additional interaction term (the interaction between the racial bias index and the competition measure (N_{st})) in equation (9).

This estimation strategy allows us to relax the standard 2SLS exclusion restriction that bank deregulation only affects blacks' relative wages through its impact on the rate of new incorporations. By including state and year fixed effects and separately analyzing states with above and below the median value of the racial bias index, we assess the differential impact of an increase in the rate of new incorporations on blacks' relative wages in high and low racial bias states. To obtain a consistent estimate of the differential impact using 2SLS, we simply require that any bias arising from bank deregulation affecting blacks' relative wages beyond its impact through the rate of new incorporations is the same in high and low racial bias states.¹³

3.5.2 Preliminary Analyses of Competition and Blacks' Relative Wages

Our 2SLS strategy requires that bank deregulation increases the rate of new incorporations in the overall economy. Table 5 shows that both interstate bank deregulation and intrastate branch deregulation exert a strong, positive impact on the log of new incorporations per capita over time. In columns (1) – (3), we use simple dummy variables that equal zero before a state deregulates and one afterwards. Interstate deregulation enters significantly and positively, but intrastate does not, which is consistent with the findings in Black and Strahan (2002).

The Table 5 results emphasize that the impact of deregulation on the rate of new in-

¹²The first stage regression is conducted at the individual level, so it is weighted by the proportion of black workers in each state.

¹³To see this explicitly for the 2SLS specification, first consider a simple version of equation (9) for the racial wage gap: $R = \beta N + \varepsilon$, where N (new incorporations) is instrumented with D (bank deregulation). $E\{\beta^{2SLS}\} = \beta$, if the standard exclusion restriction holds, i.e., if $E\{\frac{D'\varepsilon}{D'N}\} = 0$. However, if $E\{\frac{D'\varepsilon}{D'N}\} \neq 0$, 2SLS produces a biased estimate of β , such that $E\{\beta^{2SLS}\} = \beta + E\{\frac{D'\varepsilon}{D'N}\}$.

Next, recognize that we estimate this system separately for high and low racial bias states to compute the differential impact of N on R in high (β_H) and low (β_L) racial bias states respectively.

To compute an unbiased estimate of $\beta_H - \beta_L$, however, we no longer require the standard exclusion restriction that $E\{\frac{D'\varepsilon}{D'N}\} = 0$. Rather, $E\{\beta_H^{2SLS} - \beta_L^{2SLS}\} = \beta_H - \beta_L$ if $\left[E\{\frac{D'\varepsilon}{D'N}\}\right]_H = \left[E\{\frac{D'\varepsilon}{D'N}\}\right]_L$.

corporations grows over time. In columns (4)–(6), we include the number of years since deregulation and its quadratic. *Interstate* and *Intrastate* equal the number of years since interstate and intrastate bank deregulation respectively, and equal zero before deregulation. Both linear terms enter positively and significantly, while the quadratic terms are negative but small. The coefficients in columns (4) and (5) indicate that five years after either inter- or intrastate deregulation the rate of new incorporations is about 10 percent greater than before deregulation. Simultaneously deregulating inter- and intrastate restrictions boosts the rate of new incorporations by 18 percent after five years as shown in column (6). The impact of each form of deregulation on competition grows, reaching a maximum about a decade after interstate deregulation, and over two decades after intrastate deregulation.

Figure 3 more fully illustrates the positive, dynamic impact of both interstate and intrastate deregulation on the rate of new incorporations. We trace out the year-by-year relationship between both interstate and intrastate deregulation and the logarithm of new incorporations. We do this for two samples of states, those with above the median level of the racial bias index and those with below median levels. Specifically, we report estimated coefficients from the following regression:

$$N_{st} = \alpha + \beta_1 Inter_{-9} + \dots + \beta_{18} Inter_{+9} + \gamma_1 Intra_{-9} + \dots + \gamma_{18} Intra_{+9} + \delta_s + \delta_t + \varepsilon_{st}, \quad (11)$$

where $Inter_{-j}$ equals one for the j^{th} year before interstate deregulation, and $Inter_{+k}$ equals one for the k^{th} year after interstate deregulation, while $Intra_{-j}$ equals one for the j^{th} year before intrastate deregulation, and $Intra_{+k}$ equals one for the k^{th} year after intrastate deregulation. These dummy variables equal zero in other years. We present results starting 9 years before each form of bank deregulation and trace out the year-by-year dynamics of the relationship between deregulation and the wage gap until 9 years after each type of bank deregulation. The year of deregulation is omitted and the regressions include state and year fixed effects. Figure 3 illustrates the level and trend of the logarithm of new incorporations following each type of bank deregulation relative to the level and trend before deregulation.¹⁴

Besides illustrating that bank deregulation boosted the rate of new incorporations, Figure 3 provides two insights. First, consistent with the results from Figure 2, the impact of bank deregulation on the rate of new incorporations grows over time. Specifically, if bank deregulation reduced blacks' relative wages by spurring new firm entry,

¹⁴Note, we use the same detrending procedure employed to construct Figure 2.

then we should observe that the dynamic impact of deregulation on new incorporations (Figure 3) should coincide with the dynamic impact of deregulation on the racial wage gap (Figure 2). This is exactly what we find.

Second, the positive impact of bank deregulation on the rate of new incorporations occurs in both high and low racial bias states, though the marginal impact of intrastate deregulation on the rate of new incorporations in low racial bias states is less pronounced than in high racial bias states. Though the impact of bank deregulation on new incorporations does not have to be identical in high and low racial bias states, this part of our analyses focused on the overall economy requires that deregulation boosts the rate of new incorporations in both high and low racial bias states. This is what we find. Moreover, when we restrict our analyses to interstate deregulation, which has the same impact on the rate of new incorporations in high and low racial bias states, we confirm the results reported below.

As a final set of preliminary observations, consider the OLS regressions between new incorporations and the racial wage gap. In columns (1) - (3) of Table 6, the dependent variable is blacks' relative wages and the key regressor is the log of new incorporations per capita. In Panel A, we use the benchmark measure of blacks' relative wages, which conditions on the standard Mincerian characteristics, education and potential work experience. In Panel B, we confirm the findings when also conditioning on each worker's occupation.

There is a strong, positive association between the rate of new incorporations and blacks' relative wages in states with above the median values of the racial bias index (column 3). A ten percent increase in the rate of new incorporations is associated with a 1.4 percent increase in blacks' relative wages in high racial bias states, but not in states with low values of the racial bias index (column 2).

3.5.3 *Competition and Blacks' Relative Wages: 2SLS*

The final six columns of Panel A and Panel B of Table 6 report 2SLS estimates, where we use two different sets of instrumental variables to identify changes in the rate of new incorporations. First, the "linear" instruments include *Interstate* and *Intrastate*. Second, the "Non-Parametric" instruments included dummy variables for each year before and after both types of deregulation. Robustness tests indicate that using *Interstate* and *Intrastate* plus their quadratic terms as instruments produces similar results.

As shown, the instrumental variables pass the validity tests. They significantly

explain new incorporations as shown by the F-test of the excluded instruments. Furthermore, the instruments pass the test of the over-identifying restrictions (OIR test), meaning that the hypothesis that the instruments only affect blacks' relative wages through their effect on new incorporations is not rejected.¹⁵

The exogenous increase in the rate of new incorporations dramatically boosted the wage rates of black workers relative to their white counterparts in states with above the median values of the racial bias index. As reported in both Panels A and B, an acceleration of the rate of new incorporations increased blacks' relative wages only in high racial bias states.

The economic impact is large in states with above the median level of the racial bias index. A ten percent acceleration in the rate of new incorporations increases blacks' relative wages by about 2.5 percent in high racial bias states.¹⁶ Combining these results with those in Figure 3, the results suggest that bank deregulation boosted the rate of new incorporations by over 20% after five years in high racial bias states, which in turn increased blacks' relative wages by about five percent in these same states. By spurring new firm entry, bank deregulation boosted blacks' relative wages by one-quarter of the initial racial wage gap in these states, which equaled, on average, 20 percent in the years before bank deregulation.

3.5.4 Competition and Blacks' Relative Wages: Sensitivity Analyses

The results are robust to using either the Charles and Guryan (2008) measure of racial prejudices (*CG*) or the intermarriage racial bias index (*LLR*) to categorize states as high- or low-racial bias states. Table 7 presents the OLS and 2SLS analyses of the relation between the racial wage gap and the rate of new incorporations. We use the linear instrument set and compute blacks' relative wages conditional on standard Mincerian traits and occupation. We use a common sample of states that is slightly smaller than in Table 6 because the *CG* measure is unavailable for Hawaii, Idaho, Maine, Nebraska, Nevada, and New Mexico. The results hold. In states with above

¹⁵In unreported robustness tests, we also show that the results are not driven by states in which deregulation did not induce an increase in competition, which would run counter to theory and our identification strategy. Thus, we run the first-stage regression while omitting each state one-at-a-time. We then find which states are "flattening" the estimated relationship between competition and deregulation in the first stage. When we eliminate these states, the results strengthen. This robustness test suggests that the effects of deregulation on racial discrimination are driven by states in which the "treatment" is affecting product market competition, not by some spurious channel.

¹⁶The 2SLS parameter estimate is larger than the OLS estimate. This is consistent with the reverse causality argument made above: if firms are attracted to states where blacks' relative wages are low, OLS will underestimate the impact of competition on blacks' relative wages.

the median values of the two racial bias indicators, the log of new incorporations per capita is positively associated with blacks' relative wages.

Figure 4 shows that the results are robust to considering the full range of possible combinations of (1) estimation strategy (OLS and 2SLS), (2) method for computing blacks' relative wages (either conditioning on standard Mincerian controls (R) or also conditioning on occupation (Ro)), (3) categorizing states by taste for discrimination (LLR or CG), and (4) using linear or non-parametric instrumental variables (*Linear* or *Non – param.*). Figure 4 plots each point estimate along with its 95% confidence interval. In terms of the instrumental variable results, there is only one specification in which the rate of new incorporation does not enter positively and significantly at the five percent level, and instead enters with a p-value of (0.10). This exception involves using the CG indicator to define racial attitudes, and we have already discussed the advantages, in the context of our particular study, of using the intermarriage racial bias measure (LLR).

3.6 The Effect of Competition on Segregation

3.6.1 Racial Prejudices, Competition, and Segregation

Besides making predictions regarding relative wages, the taste-based theory of discrimination predicts that when employers are heterogeneous in both productive quality and the "disutility" they receive from employing black workers, there will be segregation as black workers are hired by the least racially biased employers within any particular industry. Indeed, if firms are similar in an industry except for the racial prejudices of employers, segregation will reduce racial wage differentials as workers simply sort into equally productive firms according to the racial preferences of employers. This led Welch (1975) to call the taste-based view a theory of segregation. While racial wage differentials are a fundamental measure of labor market discrimination, segregation offers an additional margin along which to assess the relationship between competition and the racial characteristics of labor markets.

From this perspective, lowering entry barriers will allow new employers with less of a taste for discrimination than existing employers to enter, reducing segregation at the industry-level. If our earlier results on blacks' relative wages reflect the causal impact of intensified competition on how racial prejudices affect labor markets, then we should also observe a reduction in segregation following an intensification of competition.

3.6.2 *The Effect of Competition on Segregation*

Consequently, we evaluate the impact of an intensification of competition on the racial allocation of workers across industries while differentiating states by the degree of racial prejudice. We test whether competition induces black workers to move to historically "white" industries using data at the 3 digit industry level.

We construct and use several measures of the extent to which an industry is particularly "white." First, we calculate the share of white workers by industry. Second, since the racial composition of workers in an industry might simply reflect the human capital needs of the industry in conjunction with the differential racial composition of human capital skills, we also estimate the degree to which the proportion of white workers in an industry is greater than the proportion explained by the underlying characteristics of workers. To do this, we regress (for each year) the proportion of white workers in each of the 144 industries on the characteristics of the white workers in that industry, including education, a quartic in potential experience, as well as occupation and state fixed effects, i.e., the same set of regressors that we employ to generate wage residuals. We collect the average residuals in each industry. These provide crude and residual ("unexplained") measures of the "whiteness" of each industry. Third, motivated by Ashenfelter and Hannan (1986), we calculate the proportion of white managers in each industry and use this proportion as a measure of the degree to which an industry is dominated by whites. Fourth, we also construct the unexplained proportion of white managers, using the same conditioning regressors.

We next estimate the impact of competition on the racial composition of the industry in which each black worker is employed. We use the same specification employed in our relative wage regressions, except the dependent variable is one of the measures of the "whiteness" of the industry in which each black works. Thus, we regress industry whiteness on the log of new incorporations per capita, controlling for state and year fixed effects. We do this using OLS and 2SLS. We divide states by the degree of racial bias, using both the *LLR* and *CG* measures of racial bias to categorize states. Thus, we evaluate whether an exogenous increase in competition induces black workers to move to "white" industries, while differentiating states by racial bias. In the analyses, we obtain the same results whether we use the crude or residual measures of the degree to which an industry is composed of white workers or managers. For simplicity, we present the results for the unexplained proportion of white workers and the crude measure of the fraction of white managers.

Table 8 indicates that an acceleration of the rate of new incorporations in high racial

bias states induced blacks to work in "whiter" industries. These results hold when examining (1) the unexplained proportion of white workers and (2) the proportion of white managers. The results hold when using OLS or 2SLS, and whether we divide states by the *LLR* or the *CG* indicator of racial bias.¹⁷

4 Robustness Checks

We conducted an array of additional robustness tests. In this section, we summarize these briefly and refer interested readers to an extensive Annex with further explanations, tables, and figures that is available on request.

4.1 Mobility between Industries

Perhaps bank deregulation and its impact on the rate of new incorporations induced a shift of black workers to better paying industries, rather than an increase in blacks' relative wages within industries. Thus, we redid the analyses after computing blacks' relative wages by conditioning on education, potential experience, occupation, and industry. We find that the intensification of competition boosted blacks' wages relative to comparable white workers within the same industry and occupation.

4.2 Relative Hours Worked

We were concerned that blacks' relative wages could also rise if deregulation induced the labor supply curve of black males to shift leftward, encouraging the working hours of blacks to fall after deregulation relative to comparable white workers. But, we find that bank deregulation boosted the relative working hours of blacks. The evidence suggests that deregulation increased the relative demand for black workers.

4.3 Selection, Migration, and Self-Employment

We were concerned that changes in the skill composition of black males in deregulating states could affect our evaluation of blacks' relative wages. Consequently, we calculated the projected wage rates for all working age (non-institutionalized) blacks in each state,

¹⁷We do not reject statistical discrimination explanations of racial wage differentials (Arrow, 1972; Phelps, 1972). There is an active literature assessing nature of statistical discrimination (Altonji and Pierret, 2001; Heckman, Stixrud, and Urzua, 2006; Neal and Johnson, 1996). Our work simply shows that the impact of competition on the relative demand for black workers varies positively with an economy's taste for discrimination.

whether they were working or not. We do this using the estimated returns to observable traits from equation (1) and using the actual traits of each black male. In this way, we compute the value of observable traits of all black males. Then, we evaluated the impact of bank deregulation on the composition of skills in the workforce.

We find that deregulation did not have a significant effect on the average value of the traits of black workers. There is no evidence that deregulation substantively affected the skill composition of black workers.

It is also possible that deregulation triggered the selective migration of workers across states. This could potentially confound our interpretation of the findings.

Although we find that the share of black males within a state increased slightly following deregulation, there were only very minor changes in the skill composition of working blacks. There is no indication that migration leads us to overstate the impact of deregulation on the racial wage gap.

Similarly, the boost in blacks' relative wages could attract black males with comparatively low unobserved skills into the labor force, leading us to underestimate the degree to which the rate of new incorporations reduces racial wage differentials.

We find no evidence that selection based on unobservables is causing us to underestimate the true effect of the rate of new incorporations. While the log of new incorporations per capita increases the relative demand for black workers, the number of new black males pulled into the labor force is relatively small, such that median regressions yield virtually identical results to the OLS estimates.

4.4 Swimming Upstream

Although national trends in returns to unobserved skills will not affect our results because we control for year fixed effects, the intensification of competition when a state deregulates could increase returns to unobservable traits. If the average white worker has more of these unobserved traits than the average black worker, we will underestimate the true, positive effect of deregulation on the relative wages of blacks. This is sometimes called “swimming upstream” (Juhn Murphy and Pierce, 1991; Blau and Kahn, 1997; Blau and Kahn, 2000; and Mulligan and Rubinstein, 2008). To assess the importance of swimming upstream, we follow the literature and use quantile regressions. The goal is to compare black and white workers that are more similar in unobserved skills than when using OLS, which compares averages from both groups.

We confirm the existence of swimming upstream. This suggests that we are underestimating the beneficial effects of bank deregulation on blacks' relative wages.

4.5 Race or Poverty?

Since bank deregulation exerted a disproportionately positive impact on the poor and blacks are on average comparatively poor (Beck, Levine, and Levkov, 2010), the current paper’s analyses could reflect this income distributional effect, rather than the impact of competition on the manifestation of racial prejudices in labor markets.

Three observations, however, suggest that this is not the case. First, bank deregulation and the rate of new incorporations boosted blacks’ relative wages only in states with a high degree of racial bias. Second, the results hold when computing relative wages conditional on occupation and industry. Third, we performed a rank analysis to compare the change in blacks’ relative wages with those of comparable whites across the full distribution of relative wage rates. In contrast to the view that deregulation is simply helping the poor, we find that bank deregulation disproportionately helped black workers across the full distribution of wages.

5 Conclusions

Although Becker’s (1957) seminal work has shaped our understanding of discrimination for over fifty years, we provide the first assessment of a central prediction of the taste-based theory of discrimination: An intensification of product market competition will reduce the black-white wage differential in economies where employers have sufficiently strong racial prejudices.

In this paper, we find that an exogenous intensification of competition substantively boosted blacks’ relative wages and reduced racial segregation—*but only* in states with a sufficiently high degree of racial bias. In 2SLS, we use bank deregulation to identify an exogenous intensification of competition. The resultant increase in competition eliminated more than one-fifth of the preexisting black-white wage differential in high racial bias states over a five year period. Consistent with the taste-based theory of racial discrimination, an increase in product market competition reduced the manifestation of racial prejudices in labor markets.

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Table 1

THE RACIAL BIAS INDEX, SURVEY MEASURES OF RACIAL PREJUDICE, AND RELATIVE WAGES

	Fraction whites who support law against interracial marriage	Fraction whites who would not vote for black president	Fraction whites who support right to segregate neighborhoods
Panel A: Correlation Coefficients Between the Different Measures of Taste for Discrimination	(1)	(2)	(3)
Racial bias index	0.36	0.35	0.31
	{0.02}	{0.02}	{0.04}
Observations	43	43	43

Panel B: Taste for Discrimination and Relative Wages of Blacks	Dependent Variable: Relative Wages of Blacks			
	(1)	(2)	(3)	(4)
Racial bias index > median	-.079***		-.072***	-.065***
	(.013)		(.014)	(.012)
Marginal racial prejudice > median		-.058***	-.042***	-.002
		(.015)	(.015)	(.017)
Share of blacks in 1970 \geq 10%				-.082***
				(.013)
Observations	10,076	10,076	10,076	10,076

NOTE – Panel A reports correlation coefficients between (1) The racial bias index, which is based on interracial marriages in 1970, and (2) three recent survey-based indicators of racial prejudice from Charles and Guryan (2008). Panel B reports estimated coefficients from four regressions, where the dependent variable is blacks’ relative wage rates. Relative wages are conditional on five indicators of years of completed education (0-8, 9-11, 12, 13-15, and 16+) and a quartic in potential experience. Estimates are weighted by sampling weights provided by the Current Population Survey. In column (1), the regressor is an indicator which equals one if the racial bias index above the median and zero otherwise. In column (2) the regressor is an indicator which equals one if the marginal racial prejudice above the median and zero otherwise. The marginal racial prejudice index is the p^{th} percentile of the distribution of an aggregate index of racial prejudice, where p is the percentile of workforce that is black. The marginal racial prejudice index is taken from Charles and Guryan (2008). Column (3) includes simultaneously the regressors from columns (1) and (2). In column (4) we also control for an indicator which equals one if the proportion of blacks in the workforce in 1970 is above 10%. The regressions include black workers prior to interstate and intrastate bank deregulation, so that the reported number of observations equals 10,076. All regressions include year fixed effects. We do not include state fixed effects because the regressors are fixed for each state and do not change over time. Standard errors are clustered at the state-year level and appear in parentheses; p-values are in brackets. *, **, and *** indicate significance at the 10%, 5%, and 1% respectively.

Table 2
 THE EFFECT OF INTERSTATE DEREGULATION ON BLACKS' RELATIVE WAGES IN THE BANKING
 INDUSTRY

	All States	Racial Bias Index		All States
		Below Median	Above Median	
	(1)	(2)	(3)	(4)
Deregulation x Bank x Black	.155 (.105)	.031 (.112)	.580*** (.200)	.031 (.112)
Deregulation x Bank x Black x (Racial Bias Index > median)				.549** (.229)
R^2	.040	.035	.044	.041
Observations	43,697	28,571	15,126	43,697

NOTE - Data are taken from the March Supplements to the Current Population Survey (CPS) for the calendar years 1977-1997. The dependent variable in all specifications is the residual wage gap imputed from the difference between actual hourly wage (in logs) and the project hourly wages from by-year OLS Mincerian wage regression of hourly wages (in logs) on education (6 categories) experience (quartic), education-experience interactions, and state-year fixed effects estimated for white males. All specifications control for black-state-year fixed effects, bank-black-year fixed effects, and bank-black-state fixed effects. The last specification also controls for banking-year-black-above-median-racial-bias-index fixed effects. Robust standard errors are clustered by state-year-black level and appear in parentheses. Estimates are weighted by sampling weights provided by the CPS. ** and *** indicate statistical significance at the 5% and 1% respectively.

Table 3

THE EFFECT OF INTERSTATE DEREGULATION ON BLACKS' RELATIVE WAGES ACROSS MANUFACTURING INDUSTRIES

	All States (1)	All States (2)	Racial Bias Index		All States (5)
			Below Median (3)	Above Median (4)	
Deregulation x (Manufacturing Bank Dependence > Median) x Black	.049** (.021)	.007 (.028)	.007 (.028)	.104*** (.032)	-.055 (.062)
(Deregulation x (Manufacturing Bank Dependence > Median) x Black) x (Racial Bias Index > median)		.096** (.043)			.092** (.042)
Fixed Effects: (Manufacturing Bank Dependence > Median)-Black-State-Year					Yes
R^2	.115	.116	.113	.112	.119
Observations	11,403	11,403	6,522	4,881	11,403

NOTE - Data are taken from the March Supplements to the Current Population Survey (CPS) for the calendar years 1977-1997. The dependent variable in all specifications is the residual wage gap imputed from the difference between actual hourly wage (in logs) and the project hourly wages from by-year OLS Mincerian wage regression of hourly wages (in logs) on education (6 categories) experience (quartic), education-experience interactions, and state-year fixed effects estimated for white males. Bank dependence equals the share of assets financed with debt and is taken from Cetorelli and Strahan (2006). All specifications control for black-state-year fixed effects. The last specification controls for (manufacturing bank dependence > median)-black-state-year fixed effects. Robust standard errors are clustered by state-year-black level and appear in parentheses. Estimates are weighted by sampling weights provided by the CPS. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% respectively.

Table 4

BANK DEREGULATION AND RELATIVE WAGE RATES

	Interstate Deregulation				Intrastate Deregulation			
	All States		Racial Bias Index:		All States		Racial Bias Index:	
			Below Median	Above Median			Below Median	Above Median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years since deregulation	.001 (.003)	.004 (.003)	.003 (.003)	.012* (.007)	.005*** (.001)	.005*** (.001)	.004*** (.002)	.011*** (.002)
(Years since deregulation) x (Racial bias index > median)		.003*** (.001)				.004*** (.001)		
Impact after five years	.006 (.015)	.037** (.016)	.013 (.016)	.061* (.034)	.023*** (.006)	.044*** (.007)	.022*** (.007)	.057*** (.012)
Impact after five years as a share of sample's initial wage gap	3%	21%	9%	29%	13%	25%	15%	27%
Observations	73,801	73,801	48,367	25,434	73,801	73,801	48,367	25,434

NOTE - The dependent variable is the relative wage rates of blacks. Relative wages are conditional on five indicators of years of completed education (0-8, 9-11, 12, 13-15, and 16+) and a quartic in potential experience. Estimates are weighted by sampling weights provided by the Current Population Survey. Standard errors are adjusted for state-year clustering and appear in parentheses. All regressions include state and year fixed effects. The reported number of observations is for blacks only. Details about sample construction are in Appendix Table 2. In columns (1)-(4), "years since deregulation" stands for years since interstate deregulation; in columns (5)-(8), "years since deregulation" stands for years since intrastate deregulation. In columns (2) and (6), years since deregulation is interacted with an indicator which equals one if the racial bias index is above the median and zero otherwise. In columns (1), (2), (5), and (6) we include the entire sample. In columns (3) and (7) we include only states with racial bias index below the median. In columns (4) and (8) we include only states with racial bias index above the median. The racial bias index is based on rate of interracial marriages using the 1970 Census of Population. Appendix Table 3 lists the racial bias index for each state. The average initial racial wage gap is 17% for all states, 15% for states with a racial bias index below the median, and 21% for states with a racial bias index above the median. *, **, and *** indicate significance at the 10%, 5%, and 1% respectively.

Table 5

BANK DEREGULATION AND LOG NEW INCORPORATIONS PER CAPITA

	(1)	(2)	(3)	(4)	(5)	(6)
Interstate dummy	.084*** (.031)		.082** (.031)			
Intrastate dummy		.040 (.041)	.038 (.041)			
Interstate				.032** (.015)		.029** (.014)
Interstate squared				-.002 (.001)		-.002 (.001)
Intrastate					.021*** (.008)	.019** (.008)
Intrastate squared					-.0004* (.0002)	-.0004* (.0002)
Observations	882	882	882	882	882	882

NOTE - The table shows the impact of various measures of bank deregulation on log new incorporations per capita. Robust standard errors are adjusted for state-level clustering and appear in parentheses. Intrastate dummy equals one in the years after a state permits branching via mergers and acquisitions and zero otherwise. Interstate dummy equals one in the years after a state permits interstate banking and zero otherwise. *Interstate* is equal to years since interstate deregulation and is equal to zero before interstate deregulation. *Intrastate* is equal to years since intrastate deregulation and is equal to zero before intrastate deregulation. New incorporations are from Dun and Bradstreet. Dates of intrastate and interstate bank deregulations are from Kroszner and Strahan (1999) and Amel (2008). The sample is for the years 1977-1994 and excludes Delaware and South Dakota. All regressions include state and year fixed effects. There are no other covariates. *, **, and *** indicate significance at the 10%, 5%, and 1%, respectively.

Table 6

THE IMPACT OF LOG NEW INCORPORATIONS PER CAPITA ON RELATIVE WAGE RATES: OLS AND 2SLS ESTIMATES

	OLS			2SLS: Linear			2SLS: Non-Parametric		
	All States	Racial Bias Index:		All States	Racial Bias Index:		All States	Racial Bias Index:	
		Below Median	Above Median		Below Median	Above Median		Below Median	Above Median
		(2)	(3)		(5)	(6)		(8)	(9)
(1)			(4)			(7)			
Panel A: Relative Wage Rates are Conditional on Education and Potential Experience									
Log new incorporation per capita	.018 (.022)	-.038 (.023)	.137*** (.038)	.042 (.064)	-.080 (.071)	.267*** (.071)	.019 (.048)	-.122** (.054)	.235*** (.062)
F-test of excluded instruments				21.8	8.2	26.8	3.0	2.3	113.5
OIR test (p-value)				.003	.174	.134	.608	.518	.086
Observations	37,876	24,754	13,122	37,876	24,754	13,122	37,876	24,754	13,122
Panel B: Relative Wage Rates are Conditional on Education, Potential Experience, and Occupation									
Log new incorporation per capita	.016 (.020)	-.029 (.023)	.122*** (.037)	.023 (.064)	-.026 (.071)	.214*** (.068)	.020 (.046)	-.079 (.053)	.198*** (.058)
F-test of excluded instruments				21.8	8.2	26.8	3.0	2.3	113.5
OIR test (p-value)				.046	.358	.123	.845	.683	.082
Observations	37,876	24,754	13,122	37,876	24,754	13,122	37,876	24,754	13,122

NOTE - The dependent variable is the relative wage rates of blacks. In panel A, relative wages are conditional on five indicators of years of completed education (0-8, 9-11, 12, 13-15, and 16+) and a quartic in potential experience. In panel B, relative wages are conditional on five indicators of years of completed education (0-8, 9-11, 12, 13-15, and 16+), a quartic in potential experience, and occupation fixed effects. Estimates are weighted by sampling weights provided by the Current Population Survey. Standard errors are adjusted for state-year clustering and appear in parentheses. All regressions include state and year fixed effects. The reported number of observations is for blacks only. Details about sample construction are in Appendix Table 2. The sample is limited to the years 1977-1994 due to availability of log new incorporations per capita data. In columns (1), (4), and (7) we include the entire sample. In columns (2), (5), and (8) we include only states with racial bias index below the median. In columns (3), (6), and (9) we include only states with racial bias index above the median. The racial bias index is based on rate of interracial marriages using the 1970 Census of Population. Appendix Table 3 lists the racial bias index for each state. Columns (1)-(3) report Ordinary Least Squares estimates of the impact of log new incorporations per capita on the relative wage rates of blacks. Columns (4)-(9) report Two Stage Least Squares estimates of the impact of log new incorporations per capita on the relative wage rates of blacks. In columns (4)-(6), log new incorporations per capita are instrumented by years since interstate deregulation and years since intrastate deregulation. In columns (7)-(9), log new incorporations per capita are instrumented by dummy variables for each year before and after interstate deregulation and dummy variables for each year before and after intrastate deregulation. The F-test of excluded instruments reports the F-statistic from the first-stage. The OIR test reports the p-value of a J-statistic that test over-identifying restrictions. *, **, and *** indicate significance at the 10%, 5%, and 1% respectively.

Table 7

THE IMPACT OF LOG NEW INCORPORATIONS ON THE RELATIVE WAGES OF BLACKS: OLS AND 2SLS ESTIMATES
LEVINE-LEVKOV-RUBINSTEIN AND CHARLES-GURYAN MEASURES OF RACIAL PREJUDICE

	OLS				2SLS			
	Racial Bias Below Median		Racial Bias Above Median		Racial Bias Below Median		Racial Bias Above Median	
	LLR	CG	LLR	CG	LLR	CG	LLR	CG
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log new incorporation per capita	-.029 (.023)	-.029 (.030)	.124*** (.037)	.039 (.028)	-.024 (.071)	-.051 (.057)	.224*** (.068)	.165** (.076)
F-test of excluded instruments					8.1	32.8	26.8	16.2
OIR test (p-value)					.375	.264	.259	.571
Observations	24,272	8,093	12,942	29,121	24,272	8,093	12,942	29,121

NOTE - The dependent variable is the relative wage rates of blacks. Relative wages are conditional on five indicators of years of completed education (0-8, 9-11, 12, 13-15, and 16+), a quartic in potential experience, and occupation fixed effects. Estimates are weighted by sampling weights provided by the Current Population Survey. Standard errors are adjusted for state-year clustering and appear in parentheses. All regressions include state and year fixed effects. The reported number of observations is for blacks only. Details about sample construction are in Appendix Table 2. The sample is limited to the years 1977-1994 due to availability of log new incorporations per capita data. "LLR" stands for the racial bias index and is based on rate of interracial marriages using the 1970 Census of Population. Appendix Table 3 lists the racial bias index for each state. "CG" stands for the marginal racial prejudice which is obtained from Charles and Guryan (2008). In columns (1) and (5) we include only states with racial bias index below the median. In columns (3) and (7) we include only states with racial bias index above the median. In columns (2) and (6) we include only states with marginal racial prejudice below the median. In columns (4) and (8) we include only states with marginal racial prejudice above the median. Columns (1)-(4) report Ordinary Least Squares estimates of the impact of log new incorporations per capita on the relative wage rates of blacks. Columns (5)-(8) report Two Stage Least Squares estimates of the impact of log new incorporations per capita on the relative wage rates of blacks. Log new incorporations per capita are instrumented by dummy variables for each year before and after interstate deregulation and dummy variables for each year before and after intrastate deregulation. The F-test of excluded instruments reports the F-statistic from the first-stage. The OIR test reports the p-value of a J-statistic that test over-identifying restrictions. *, **, and *** indicate significance at the 10%, 5%, and 1% respectively.

Table 8

THE IMPACT OF LOG NEW INCORPORATIONS PER CAPITA ON EMPLOYMENT OF BLACKS IN “WHITE” INDUSTRIES: OLS AND 2SLS ESTIMATES

	Dependent variable: working in a white industry				Dependent variable: working for a white manager			
	OLS		2SLS		OLS		2SLS	
	Racial Prejudice: Below Median (1)	Racial Prejudice: Above Median (2)	Racial Prejudice: Below Median (3)	Racial Prejudice: Above Median (4)	Racial Prejudice: Below Median (5)	Racial Prejudice: Above Median (6)	Racial Prejudice: Below Median (7)	Racial Prejudice: Above Median (8)
Panel A: Levine-Levkov-Rubinstein Measure of Racial Prejudice								
Log new incorporation per capita	.001 (.001)	.004** (.002)	-.001 (.003)	.012*** (.004)	.002 (.002)	.007*** (.002)	-.000 (.004)	.008* (.004)
Observations	24,754	13,122	24,754	13,122	24,687	13,064	24,687	13,064
Panel B: Charles-Guryan Racial Prejudice								
Log new incorporation per capita	.000 (.002)	.004** (.001)	-.001 (.002)	.007** (.003)	.000 (.003)	.009*** (.002)	.000 (.003)	.015*** (.004)
Observations	8,093	29,121	8,093	29,121	8,064	29,034	8,064	29,034

NOTE - The dependent variable in columns (1)-(4) is proportion of “excess” whites in an industry, where the proportion of “excess” whites is the proportion of whites that is unexplained by years of completed education (0-8, 9-11, 12, 13-15, and 16+), a quartic in potential experience, and occupation fixed effects. The dependent variable in columns (5)-(8) is proportion of white managers in an industry. Estimates are weighted by sampling weights provided by the Current Population Survey. Standard errors are adjusted for state-year clustering and appear in parentheses. All regressions include state and year fixed effects. The reported number of observations is for blacks only. Details about sample construction are in Appendix Table 2. The sample is limited to the years 1977-1994 due to availability of log new incorporations per capita data. In columns (1), (3), (5), and (7) we include only states with racial prejudice below the median. In columns (2), (4), (6), and (8) we include only states with racial prejudice above the median. In panel A, racial prejudice is the racial bias index which is based on rate of interracial marriages using the 1970 Census of Population. Appendix Table 3 lists the racial bias index for each state. In panel B, racial prejudice is the marginal racial prejudice which is obtained from Charles and Guryan (2008). Columns (1), (2), (5), and (6) report Ordinary Least Squares estimates, while columns (3), (4), (7), and (8) report Two Stage Least Squares. Log new incorporations per capita are instrumented by dummy variables for each year before and after interstate deregulation and dummy variables for each year before and after intrastate deregulation. *, **, and *** indicate significance at the 10%, 5%, and 1% respectively.

Appendix Table 1

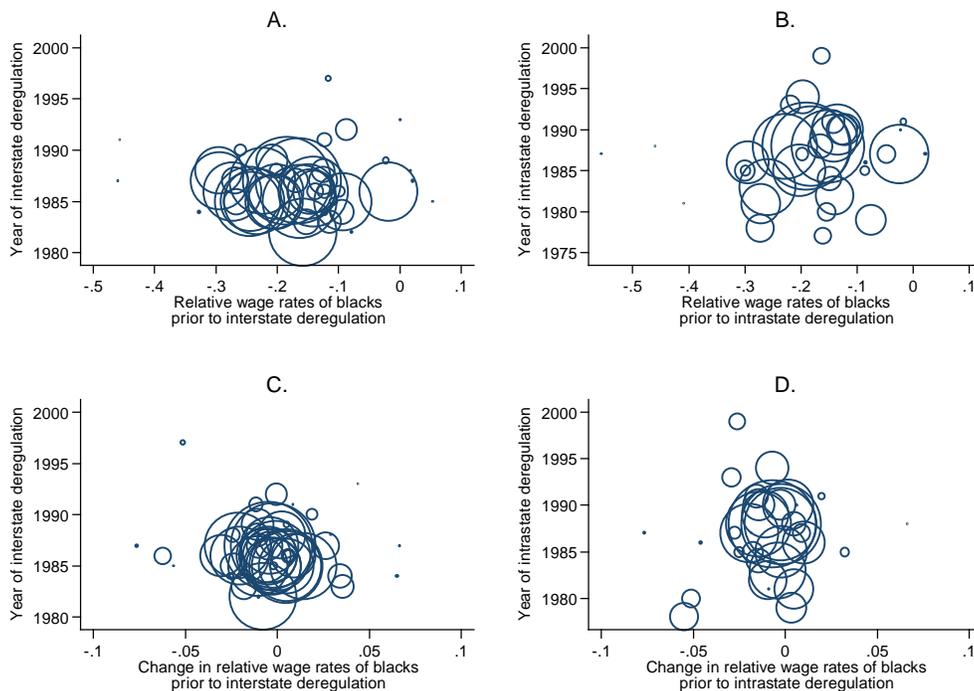
RACIAL BIAS INDEX BY STATES, 1970

States with racial bias index < median		States with racial bias index > median	
State	Racial Bias Index	State	Racial Bias Index
Alaska	0.00	Arkansas	0.30
Hawaii	0.07	Virginia	0.30
Washington	0.10	South Dakota	0.30
New York	0.11	Colorado	0.30
Nevada	0.12	North Carolina	0.32
California	0.15	Texas	0.32
District of Columbia	0.18	Nebraska	0.32
Delaware	0.24	Minnesota	0.32
South Carolina	0.24	Mississippi	0.33
New Jersey	0.25	Oregon	0.33
Pennsylvania	0.25	Louisiana	0.33
Michigan	0.26	Georgia	0.34
Kentucky	0.26	Oklahoma	0.35
Illinois	0.26	Indiana	0.35
Maryland	0.27	Alabama	0.35
Connecticut	0.27	Wisconsin	0.36
Rhode island	0.27	Vermont	0.36
New Mexico	0.27	Utah	0.37
Kansas	0.28	Idaho	0.37
Massachusetts	0.28	Tennessee	0.39
Ohio	0.28	Iowa	0.39
Missouri	0.28	Montana	0.40
Arizona	0.29	North Dakota	0.43
Florida	0.29	West Virginia	0.45
		Maine	0.45
		Wyoming	0.46
		New Hampshire	0.46

NOTE - The racial bias index is based on inter-racial marriage data obtained from the 1970 Census of Population. The sample includes married whites and blacks between that ages of 18 to 65, and excludes couples in which at least one person is living in group quarters or has missing data on race, gender, state of residence, marital status, or educational attainment. The racial bias index is based on the difference between the estimated rate of inter-racial marriage in 1970, where the estimation is based on each state's racial composition along with each individual's education and age characteristics, and the actual rate of inter-racial marriage. Larger values of the racial bias index signify that the actual rate of inter-racial marriage is correspondingly smaller than the estimated rate.

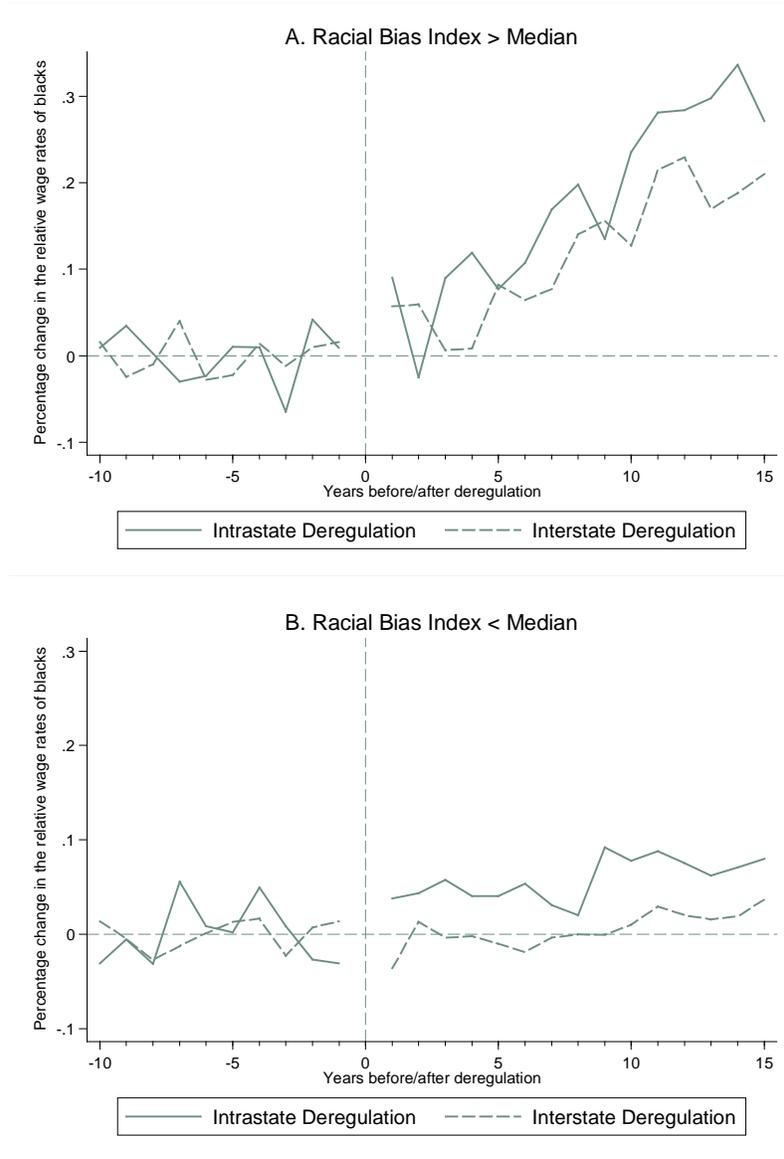
Figure 1

TRENDS AND INNOVATIONS IN THE RELATIVE WAGE RATES OF BLACKS PRIOR TO BANK DEREGULATION



NOTE - Figures A and B plot the year of bank deregulation against the average black-white wage differential prior to deregulation. In Figure A we consider years prior to interstate deregulation. In Figure B we consider years prior to intrastate deregulation. Figures C and D plot the year of bank deregulation against the change in the black-white wage differential prior to deregulation. In Figure C we consider years prior to interstate deregulation. In Figure D we consider years prior to intrastate deregulation. The “bubbles” are proportional to the number of black workers in a state.

Figure 2
THE IMPACT OF DEREGULATION ON THE RELATIVE WAGE RATES OF BLACKS

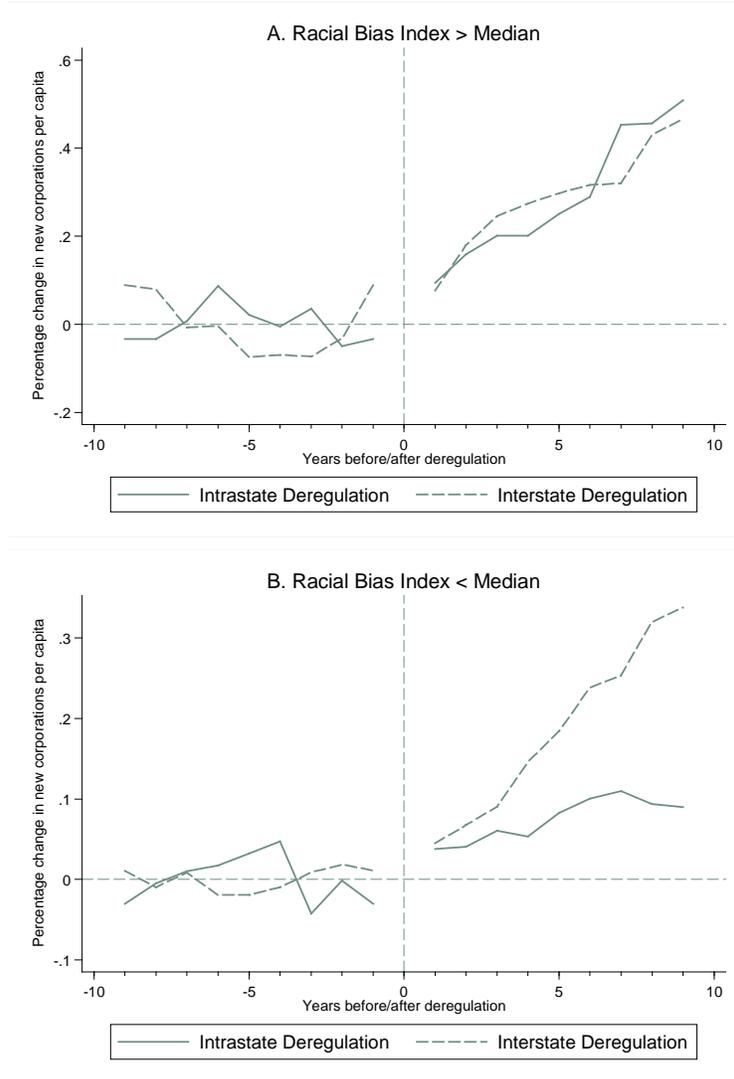


NOTE - The figures plot the impact of interstate and intrastate bank deregulations on the relative wage rates of blacks. The upper figure is for states with racial bias index above the median. The lower figure is for state with racial bias index below the median. We consider a 25 year window, spanning from 10 years before deregulation until 15 years after deregulation. The solid lines represent the impact of intrastate deregulation on the relative wage rates of blacks. The dashed lines represent the impact of interstate deregulation on the relative wage rates of blacks. Specifically, we report estimated coefficients from the following regression:

$$\hat{R}_{ist}^B = \alpha + \beta_1 D_{st}^{-10} + \beta_2 D_{st}^{-9} + \dots + \beta_{25} D_{st}^{+15} + \delta_s + \delta_t + v_{ist}$$

The D 's equal zero, except as follows: D_{-j} equals one for states in the j^{th} year before deregulation, while D_{+j} equals one for states in the j^{th} year after deregulation. We exclude the year of deregulation, thus estimating the dynamic effect of deregulation on the relative wage rates of blacks relative to the year of deregulation. We de-trend the coefficients by prior trends and normalize their average prior to deregulation to be zero. The estimates are weighted by the number of black workers.

Figure 3
THE IMPACT OF DEREGULATION ON ENTRY OF FIRMS



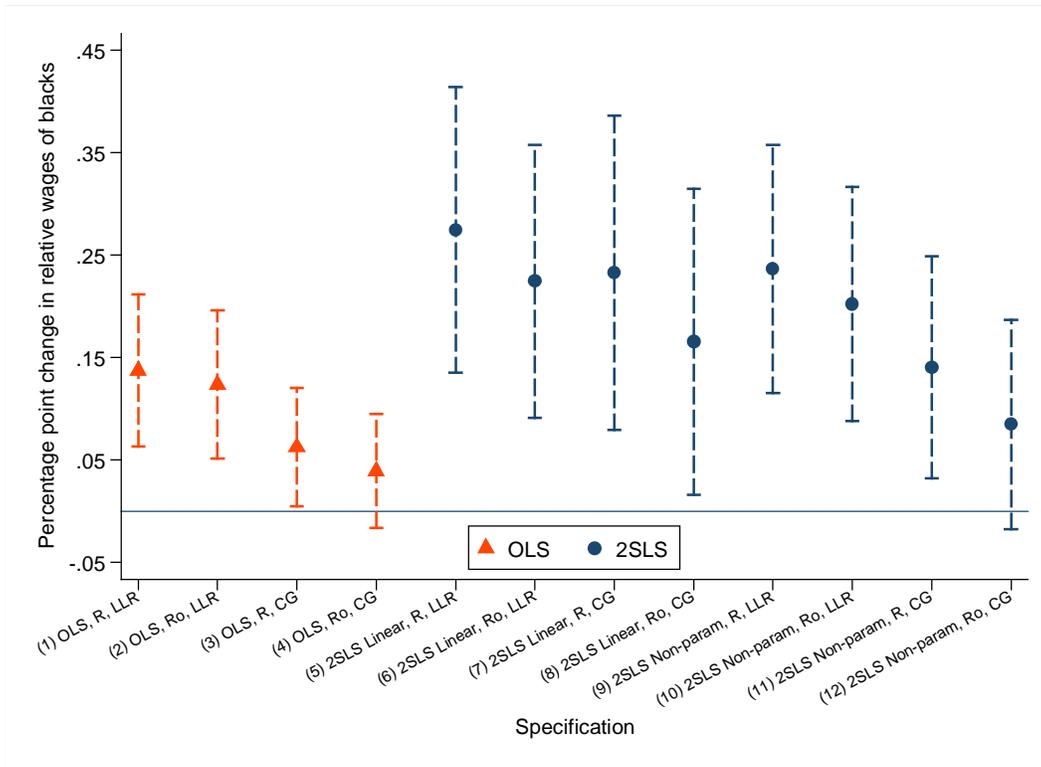
SOURCES - Data on new incorporations per capita are taken from Black and Strahan (2002). Dates of intrastate and interstate deregulations are taken from Kroszner and Strahan (1999).

NOTE - The figures plot the impact of interstate and intrastate bank deregulations on log new incorporations per capita. The upper figure is for states with racial bias index above the median. The lower figure is for state with racial bias index below the median. We consider an 18 years window spanning from 9 years before deregulations until 9 years after deregulations. The solid lines represent the impact of intrastate deregulation on log new incorporations per capita. The dashed lines represent the impact of interstate deregulation on log new incorporations per capita. Specifically, we report estimated coefficients from the following regression:

$$Y_{st} = \alpha + \beta_1 \text{Intra}_{-9} + \gamma_1 \text{Inter}_{-9} + \beta_2 \text{Intra}_{-8} + \gamma_2 \text{Inter}_{-8} + \dots + \beta_{18} \text{Intra}_{+9} + \gamma_{18} \text{Inter}_{+9} + \delta_s + \delta_t + \varepsilon_{st}$$

Y_{st} is log new incorporations per capita in state s and year t . Intra_{-j} equals one for states in the j^{th} year before intrastate deregulation and equals zero otherwise. Intra_{+k} equals one for states in the k^{th} year after intrastate deregulation and equals zero otherwise. Similarly, Inter_{-j} equals one in states in the j^{th} year before interstate deregulation and equals zero otherwise. Inter_{+k} equals one in states in the k^{th} year after interstate deregulation and equals zero otherwise. δ_s and δ_t are state and year fixed effects, respectively. We exclude the year of intrastate and interstate deregulation, thus estimating the dynamic effect of deregulation on log new incorporations per capita relative to the corresponding year of deregulation. We de-trend the coefficients by prior trends and normalize their average prior to deregulation to be zero. The estimates are weighted by the number of black workers.

Figure 4
 THE IMPACT OF LOG NEW INCORPORATIONS PER CAPITA ON THE RELATIVE WAGE RATES OF BLACKS
 DIFFERENT OLS AND 2SLS SPECIFICATIONS
 (DASHED LINES REPRESENT 95% CONFIDENCE INTERVALS)



NOTE - The circles and triangles represent the estimated impact of log new incorporations per capita on the relative wages of blacks. The dashed lines represent the corresponding 95% confidence intervals, adjusted for state and year clustering. The estimated coefficients and the confidence intervals are from twelve different specifications. The notation in the specifications is as follows: OLS - Ordinary Least Squares; 2SLS Linear - Two-Stage Least Squares with Interstate and Intrastate entering linearly; 2SLS Non-param - Two-Stage Least Squares with Interstate and Intrastate entering non-parametrically; R - relative wages of blacks, where the relative wages are conditional on years of completed education and quartic in potential experience; Ro - relative wages of blacks, where the relative wages are conditional on years of completed education, a quartic in potential experience, and occupation fixed effects; LLR - states with racial bias index above the median; CG - states with marginal racial prejudice (From Charles and Guryan, 2008) above the median. The different specifications are: (1) OLS, with relative wages conditional on education and experience, in states above the median racial bias index, (2) OLS, with relative wages conditional on education, experience, and occupation, in states above the median racial bias index, (3) OLS, with relative wages conditional on education and experience, in states above the median marginal racial prejudice, (4) OLS, with relative wages conditional on education, experience, and occupation, in states above the median marginal racial prejudice, (5) 2SLS with linear instruments, relative wages conditional on education and experience, in states above the median racial bias index, (6) 2SLS with linear instruments, relative wages conditional on education, experience, and occupation, in states above the median racial bias index, (7) 2SLS with linear instruments, relative wages conditional on education and experience, in states above the median marginal racial prejudice, (8) 2SLS with linear instruments, relative wages conditional on education, experience, and occupation, in states above the median marginal racial prejudice, (9) 2SLS with non-parametric instruments, relative wages conditional on education and experience, in states above the median racial bias index, (10) 2SLS with non-parametric instruments, relative wages conditional on education, experience, and occupation, in states above the median racial bias index, (11) 2SLS with non-parametric instruments, relative wages conditional on education and experience, in states above the median marginal racial prejudice, (12) 2SLS with non-parametric instruments, relative wages conditional on education, experience, and occupation, in states above the median marginal racial prejudice. Measures of marginal racial prejudice are obtained from Charles and Guryan (2008). All estimates are weighted by sampling weights provided by the Current Population Survey.

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