Access to Financing and Racial Pay Gap Inside Firms*

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Abstract

How does access to financing influence racial pay inequality inside firms? We answer this question using the employer-employee matched data administered by the U.S. Census Bureau and detailed resume data recording workers' career trajectories. Exploiting exogenous shocks to firms' debt capacity, we find that better access to debt financing significantly narrows the earnings gap between minority and white workers. Minority workers experience a persistent increase in earnings and also a rise in the pay rank relative to white workers in the same firm. The effect is more pronounced among mid- and high-skill minority workers, in areas where white workers are in shorter supply, and for firms with ex-ante less diverse boards and greater pre-existing racial inequality. With better access to financing, minority workers are also more likely to be promoted or be reassigned to technology-oriented occupations compared to white workers. Our evidence is consistent with access to financing making firms better utilize minority workers' human capital.

Keywords: Racial Inequality, Diversity, Financing Friction, Access to Debt.

JEL classification: J31, J71, G3.

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1 Introduction

The pay gap between white and non-white workers is a prevalent and persistent phenomenon in the U.S. and other economies. As of 2021, the average white worker in the U.S. receives around 14% higher annual earnings than the average non-white worker and 32% higher than the average black worker (McKinney et al., 2022).[1] Such inequality differs substantially across firms, suggesting that firm policies, as well as inherent biases inside firms, play an important role in shaping racial inequality (Altonji and Blank 1999; Bertrand and Mullainathan 2004; Heywood and Parent 2012; Miller and Schmutte 2021; Gerard et al. 2021; Kline et al. 2022).[2]

Recent years have seen heated discussions regarding how corporations can help combat racial biases and reduce the inequality between white and non-white workers.[3] While advocates believe promoting racial equity could benefit firms in the long run, this endeavor could require substantial financial resources in the short term. For example, to narrow the racial earnings gap, firms may need to create job opportunities for minority workers, remunerate them at a higher level, and under some circumstances, overcome discrimination from customers (Holzer and Ihlanfeldt, 1998).[4] For firms facing limited financial resources, it can be difficult to break the status quo. Despite the relevance of financing capacity, there is little academic research regarding how access to financing affects racial pay inequality inside firms.[5]

We investigate this question by examining how access to debt financing affects the racial income gap inside firms. Better access to debt markets brings additional financial resources to the firm. Yet, its effect on the racial pay gap is not ex-ante obvious. On the one hand, the infusion of financial resources can help firms invest in under-utilized minority employees, improving their compensation as well as career opportunities. On the other hand, the additional resources may be disproportionately captured by the incumbent majority, leading to exacerbated pay inequality. Such an effect has been documented related to the gender wage gap in the

[1] We also validate these numbers using data from the Current Population Survey (CPS).

[2] These studies explore within-firm frictions that contribute to racial income gaps, including biased beliefs by managers, pre-labor market selection, social connections among workers and firms, or simply homophily.

[3] See, for example, "U.S. businesses must take meaningful action against racism," *Harvard Business Review* 2020. "Closing the racial and gender wealth gap through compensation equity," Glassdoor's testimony at the congressional hearing, 2021.

[4] According to the 2022 Workplace DEI Report, the majority of companies lack the financial resources to conduct DEI initiatives.

[5] Prior studies document the role of banking deregulation in affecting local gender and racial wage gaps (e.g., Avenancio-León and Shen 2021; Levine et al. 2012). Yet, those studies do not separate the role of the employers from labor supply effects.

banking sector (Black and Strahan, 2001). Using the U.S. Census Bureau administrative data and detailed resume data on individual workers' career paths, we find evidence consistent with the first prediction, i.e., better access to debt markets reduces the racial pay gap inside firms.

Our granular datasets allow us to track workers' employment histories across employers along with their earnings and positions over time. The data also provides detailed information on individual demographics and work locations. With this information, we first document a significant and economically important difference between the earnings of white and non-white workers in U.S. public firms. Over our sample period, the earning gap between white and non-white workers rises from 11.6% to 19.72%. Non-white workers are also less likely to occupy a senior, high-paying position in the firm, and less likely to be promoted to such positions compared to white workers. These gaps persist even when we keep fixed other determinants of earnings such as education, gender, and tenure, and when we compare across workers within the same firm and year, suggesting that these differences are not explained by observable worker characteristics or worker-firm sorting.

We seek causal inferences regarding the effect of access to financing using the staggered introduction of anti-recharacterization laws across several states from the late 1990s to the early 2000s. The anti-recharacterization laws strengthened the protection of creditors' rights by facilitating their seizure of collateral assets during bankruptcy proceedings. Anticipating improved protection, creditors are more willing to lend to firms outside of bankruptcy. As a result, the laws expanded firms' debt capacity, allowing them to raise more debt, potentially from different markets and at lower costs (Li et al. 2016; Ersahin 2020; Favara et al. 2021). Anti-recharacterization laws have been adopted in a staggered fashion in multiple U.S. states and affected firms incorporated in those states. This represents a good setting for studying firm-specific shocks, as we can analyze individuals working in the same area but employed by firms incorporated in different states. We adopt a triple-difference-in-difference design in a stacked event sample, comparing the real earnings of white and minority workers in matched treated and control firms around the passage of the laws. Our results suggest that, following the passage of anti-recharacterization laws, within-firm racial pay gap shrinks by around 3–5% in affected firms relative to workers in control firms operating in the same industry, location, and time.

Our base specifications include worker-by-firm fixed effects, which help rule out the possibility that the documented changes in income gap could result from dynamic worker-firm sorting.

We also include state-by-year fixed effects to narrow the comparison to individuals working in the same location but hired by firms with different states of incorporation. These controls remove the confounding effects of local economic conditions, making it unlikely that our findings could be driven by differential changes in the labor supply between minority and white workers. In addition, our findings survive firm-by-year interactive fixed effects, which purge away the influence of any firm-level characteristics and only contrast the earnings of minority and white individuals working in the same firm. Finally, we find an increase in the relative pay rank of minority workers inside the firm, suggesting that minority workers may be climbing up the job rank. Importantly, this also means that our effects are not driven by the specific measurement of pay gaps or by rising pay inequality between job ranks (Bayer and Charles, 2018).

Separating minorities into Black, Asian, and other races, we find that our effects are present for Black as well as Asian workers, with similar magnitudes across these races. In a dynamic setting, we show that the real earnings gap between minority and white workers does not change prior to the law adoption, but declines substantially after the adoption. Separately looking at minority and white workers, we find the earnings of both groups remain unchanged in pre-event years and only increase after the event. The earnings of minority workers increase more than those of white workers, which explains the narrowing pay gap.

Through what mechanisms does better access to financing narrow the racial pay gap within firms? We consider several possibilities. We start with the observation that minority workers are disproportionately matched to less senior, lower-paying positions inside firms. This is consistent with both academic and anecdotal evidence that minority workers are assigned lower-ranked positions or worse-fitting tasks compared to white workers due to frictions such as biased beliefs, social connections between management and workers, psychological dissonance, etc.[6] As workers accumulate task-specific human capital, skill-task matching affects compensation and could contribute to pay inequality (Acemoglu and Autor, 2011).

There are at least two ways through which the financing shocks we study could increase mi-

[6]The prior literature documents in many settings that nonwhite workers tend to occupy lower ranked positions even with similar skills. Golan et al. (2019) find that black workers are assigned to less complex tasks than white workers at the early stages of their careers, which leads to persistent income differentials. Gui (2021) finds that minority staffers are more likely to be assigned to lower-ranked positions with fewer promotion chances. Social skills and referral relationships could also lead to different chances of promotion between white and minority workers (Fadlon, 2022). Through randomized field experiments, Cohen et al. (2006) find that the racial achievement gap could be partially explained by the psychological threat of confirming a negative stereotype when seeing certain racial groups make achievements.

nority workers' pay. First, the shocks may increase the value of lower-skill jobs to affected firms and thus reduce income inequality between high- and low-skill workers inside the firms (Levine, 2021). This mechanism suggests that, among minority workers, lower-skill individuals should be more affected by firms' access to financing. Second, the adoption of ARLs may improve the matching of minority workers and job positions, allowing them to pursue more senior and skilled jobs and better aligning their human capital to jobs. This is plausible in our setting because prior research documents that ARL adoption leads firms to adopt new technology and innovate (Ersahin, 2020; Mann, 2018). Affected firms may thus experience an expansion of tasks and an increasing demand for skilled human capital. Facing a limited supply of skilled, experienced workers, firms may assign these tasks to minority employees who are under-utilized, consequently raising their compensation. This mechanism suggests that high-skill workers should be affected more by the shocks.

To distinguish the two explanations, we investigate the heterogeneous effects across worker skill levels. Partitioning workers based on their education and pre-event earnings, we find that the effects of anti-recharacterization laws are more pronounced for mid- and high-skill workers. Following the enactment of the laws, racial pay inequality reduces by 4% among high-skill workers but remains barely changed for low-skill ones. This evidence is consistent with the human capital utilization channel.

To further substantiate this mechanism, we take advantage of a large, unique dataset containing individual worker resume data to shed light on how financing shocks influence the career trajectories of minority and white workers. Our resume data provide a detailed description of a worker's career path, including the place of employment, the name of the employer, job title, occupation category, and indicators for their seniority inside the firm at each point in time. Moreover, we can observe worker demographics and backgrounds such as gender, race and education levels. The dataset covers approximately 33 million unique workers across all states and 70 million job records at U.S. public companies. It allows us to estimate the progression of individuals' career paths and how it changes around the financing shocks.

We examine several dimensions of workers' career progression within firms. First, we look at whether a worker switches his/her job positions after the shocks, which indicates within-firm job mobility. Second, we examine whether a worker receives a promotion to a higher-pay position. Third, we focus on the incidences that a worker is promoted to higher-paying positions within the same occupation category. Finally, we investigate the likelihood that a worker is assigned to higher-skill tasks that are complementary to new technology and innovation. Specifically, we examine the job switches from non-tech to technology-orientated (high-tech) occupations. We first show that minority workers on average have lower within-firm job mobility and lower promotion rates compared to white workers. They are also less likely to take up jobs in high-tech occupations. However, these differences start to narrow when firms have better access to debt markets. Following the adoption of the ARLs, minority workers experience a significantly higher increase in within-firm job mobility and promotion rates by about 40% and 60%, respectively, compared to white workers. They are also more likely to be matched to an occupation in the high-tech category. Taken together, these results provide textured evidence regarding *how* access to financing promotes the career of skilled minority workers.

The argument that financial shocks help firms better utilize minority workers' human capital implies that there are pre-existing biases or frictions inside the firm preventing minority workers from matching to best-suited tasks. We verify this condition by examining the heterogeneity of our effects across firms that exhibited higher or lower inequality prior to the shocks. We find the reduction in the racial pay gap to be more pronounced in firms where white workers earned higher premiums over minority workers prior to the shocks, and among firms with less diverse boards of directors. These represent cases where minority workers likely have faced greater obstacles in moving up the job ladder and pursuing the most suitable career opportunities.

Moreover, we document that our effects become stronger in commuting zones with a smaller fraction of white workers, where employers potentially face a shortage of white worker supply. In these labor markets, firms may have to rely more on minority workers when they have an elevated demand for labor skills.

Finally, we examine the role of labor market competition to evaluate the argument that the laws affected the outside options of non-white workers in the local labor market, raising their equilibrium earnings. We design two analyses. First, we check how our estimates vary with the inclusion of local labor market-by-year fixed effects, and find our estimates to remain unchanged. Second, we compare our results in more and less competitive labor markets, and do not find differential effects. These results suggest that changes in local labor market tightness are unlikely to be the main driver of our findings.

Collectively, findings from the above analysis speak to the economic mechanisms underlying our central results. The evidence suggests that our effects are unlikely to be driven by white and minority workers having different skills or education, or by time-varying premiums for low-skill tasks. Instead, they are consistent with the idea that reducing external financing frictions helps alleviate existing labor frictions inside firms and allows firms to better allocate minority workers to productive tasks.

Our main analysis focuses on the "incumbent" workers who remain in the firm. We next turn to the extensive margin and examine whether the expansion of firms' debt capacity affects the turnover rates of existing workers and the earnings of newly hired ones. We do not find the separation rates of white and non-white workers to change differentially after the laws, and do find a lower pay gap between newly hired white and non-white workers. Our estimates suggest that after the adoption of anti-recharacterization laws, new-entrant minority workers experience around 6% higher growth in income compared to new white workers.

How do minority workers fare in the long run? When minority workers switch jobs, does the labor market attenuate their earnings gain from firm-specific financial shocks? We track workers affected by the laws throughout their entire employment histories observed within the U.S. Census Longitudinal Employment-Household Dynamics (LEHD) database and test whether the minority-white earnings gap reverts to the pre-event level after job changes. We do not find that to be the case. Instead, non-white workers, whose earnings have increased after the laws, continue to enjoy elevated income levels even after they switch to other employers. In other words, the external labor market does not undo the earnings growth of minority workers. The persistence of effects could be explained by at least two reasons. First, the affected minority workers may be given better job opportunities or training in the affected firms, which permanently improves their human capital. Second, minority workers may have received raises or been promoted by the affected firms, so outside employers need to provide higher salaries to attract those workers. Overall, our analysis suggests that the relaxation of financing frictions increases the earnings of minority workers in the long run.

Our study contributes to several strands of literature. First, it adds to the research on racial wage gaps. A large body of literature documents the existence, trends, and determinants of the racial wage gap in the U.S. as well as other economies (see Altonji and Blank (1999) for a review). The vast majority of the literature focuses on labor market frictions and, relatedly, the sorting of workers to firms, skills, and tasks. Recent work suggests that firm policies and characteristics play an important role in shaping racial inequality (e.g., Carrington and Troske 1998; Miller and Schmutte 2021; Gerard et al. 2021). Yet, there remains to be little evidence on how financial frictions faced by employers affect the racial pay gap. Using granular employee-employer matched data, we add to this literature by providing evidence that firms' access to debt markets significantly reduces the earnings gap between white and non-white workers inside firms.

Relatedly, our findings complement two studies examining the effect of financial shocks on income inequality in a locality. Levine et al. (2012) shows that banking deregulation is followed by reduced racial inequality in a state. Beck and Levkov (2010) documents that bank deregulation tightened the income distribution by increasing the relative wage rates of unskilled workers. Avenancio-León and Shen (2021) find that credit expansion associated with banking deregulation is associated with a reduced gender pay gap in certain industries. Different from these studies, we do not look at aggregate shocks at the state level, but instead focus on idiosyncratic, firm-specific shocks and compare affected firms to unaffected ones operating in the same state and industry. This approach allows us to isolate the role of firms in moderating the racial pay gap, and purge away potential confounding effects related to labor supply or economic conditions at the local level.[7]

Our study also contributes to the growing literature on the effects of financial markets on corporate ESG performance (Xu and Kim 2022; Houston and Shan 2022). Studies in this literature document that access to financing helps improve firms' environmental policies and ESG ratings. We add to this literature by showing that better access to debt financing helps improve racial equity. More importantly, we provide evidence shedding light on the mechanisms leading to this effect.

2 Background: Anti-Recharacterization Laws

The U.S. bankruptcy codes impose automatic stay on collateralized assets belonging to firms that file for bankruptcy. The automatic stay can significantly delay creditors' seizure of collateral till the resolution of the bankruptcy. In the process, collateralized assets may lose value, leading to creditor losses. In anticipation of these legal frictions, firms can structure special purpose vehicles (SPVs) and conduct off-balance sheet financing. Specifically, the sponsor firm sells

[7]Our results are also related to Howell and Brown (2022), who document those incumbent and new-hire workers inside small, private firms benefit differently from a cash windfall.

assets to the SPVs, which in turn issue loans backed by those assets. Proceeds from the loans are transferred to the sponsor firm in exchange for the asset sale.

The SPVs are generally bankruptcy remote, which means that if the sponsor firm files for bankruptcy, creditors of the SPVs can directly seize collateral assets without going through the automatic stay. This way, SPV financing protects creditors' rights by isolating them from bankruptcy costs (Gorton and Souleles, 2007). Having the option to finance through an SPV thus reduces the cost of debt financing for firms.

Yet, judges could recharacterize the asset transfer from the sponsor firm to the SPV as a loan instead of a true sale. This means that the collateralized assets are again under the ownership of the debtor and subject to the automatic stay. In other words, recharacterization revokes the benefits of SPV financing.

Since the early 1990s, seven states in the U.S. have passed anti-recharacterization laws (ARLs), which prevent judges from recharacterization the asset sales between sponsor firms to their SPVs as loans. These states include Louisiana and Texas in 1997, Alabama in 2001, Delaware in 2002, South Dakota in 2003, Virginia in 2004, and Nevada in 2005. The anti-recharacterization laws reinstate creditor rights protection and increase the option value of SPV financing for firms incorporated in those states. Consequently, the laws allow firms to tap into different debt markets and expand their debt capacity. Recent academic evidence suggests that firms affected by the ARLs increase borrowing, adopt new technology, and innovate more (Li et al. 2016; Mann 2018; Ersahin 2020).

In 2003, the federal court overruled in favor of recharacterization in the case of *Reaves Brokerage Company, v. Sunbelt Fruit & Vegetable Company.* This ruling suggests that the federal court could overrule state-level statutes, leading to uncertain prospects regarding the effectiveness of anti-recharacterization laws at the state level (Janger, 2003; Kettering, 2010). Following Ersahin (2020), we consider as our treatment the three ARLs passed prior to 2003, including Louisiana in 1997, Texas in 1997, and Alabama in 2001. Firms incorporated in these three states are classified as affected by anti-recharacterization laws. Firms incorporated outside of all seven adoption states form the initial control group.

3 Data sources and sample selections

3.1 Longitudinal Employment-Household Dynamics (LEHD)

We use the employer-employee matched microdata maintained by the U.S. Census Bureau in their LEHD program to identify workers' races and track workers' earnings at their employers over time. The LEHD program is constructed from administrative unemployment insurance (UI) records of states participating in the program and contains every worker who is ever employed in any participating state (Abowd et al., 2009; Vilhuber et al., 2018). We have access to LEHD for 25 participating U.S. states from 1990 to 2014. Data coverage starts in 1990 for most states (except for Maryland starts in 1985), while other states' coverage begins later. Table 1 lists covered states and years available.

TABLE 2 ABOUT HERE

Within the covered states, the Employment History Files (EHF) of the LEHD program track workers' quarterly earnings, locations, and industries across employers.[8] Workers' earnings include all forms of compensation that are immediately taxable, including gross wages and salaries, bonuses, exercised stock options, tips, and other gratuities. Within the LEHD program, the National Individual Characteristics File (ICF) reports worker-level demographic characteristics and categorizes each worker into one of the following six racial groups: White, Black or African American, American Indian or Alaska Native, Asian, Native Hawaiian or Other Pacific Islander, or multi-race group. We define all non-White workers as minority workers. Besides race, ICF also reports workers' birth years, gender, and education levels.[9] Worker demographic characteristics from ICF can be linked to workers in EHF through the Census administrative worker identifiers.

To construct our baseline sample, we start with all workers between 18 and 64 years old observed in the accessible states. We retrieve these workers' entire work histories in the EHF and adjust earnings for inflation to 2018 constant dollars. While the EHF data allow us to observe quarterly earnings, it does not provide information on the number of weeks worked.

[8]See Abowd et al. (2009) and Vilhuber et al. (2018) for more detailed descriptions of the LEHD program and the underlying datasets that it generates.

[9]Information on demographic characteristics is imputed by the LEHD program using a hierarchical approach when missing. See more details about the imputation process in Section 5.1.1.2 of Vilhuber et al. (2018).

The quarterly earnings may generate noise in our analysis if workers only worked part of the time within a quarter. Following conventions in the literature (Babina 2019; Ouimet and Zarutskie 2014; Philippon and Reshef 2012), we take the following three steps to eliminate those cases. First, we keep the observations with the highest earnings for each worker-quarter-year combination. Second, we drop observations with earnings below 50% of the federal minimum quarterly earnings.[10] Third, since worker transitions between jobs not occurring at the exact start of a new quarter would lead to a downward bias in earnings around a job change, we drop observations that do not have the same employee-employer pair in both the preceding and the subsequent quarter.[11] Lastly, to minimize the computational requirements of a large sample size, we reduce the data frequency from worker-quarter-year to worker-year by taking an average across quarterly earnings earned at each firm within a given year.[12] From the LEHD data, we define our variable of interest *Log(Earnings)* as the natural logarithm of the average quarterly earnings that a worker receives from a firm during a year.

3.2 Longitudinal Business Database (LBD) and Compustat

Our analysis requires us to reliably identify the state of incorporation for firms over our sample period. Information on incorporate states is available for publicly listed companies in Compustat. To this end, we link worker-year data constructed from LEHD files with firm identifiers in the Census Bureau's LBD through the Business Register Bridge (BRB). The LBD tracks the universe of U.S. business establishments with at least one paid employee annually (Jarmin and Miranda 2002; Melissa et al. 2021). The longitudinal nature of the LBD allows us to define firm age using the oldest establishment with positive employment numbers that the firm owns in the first year the firm is observed in LBD (Haltiwanger et al., 2014). The full geographic coverage of the LBD allows us to measure firms' size by summing up their establishments' employment. Following the Statistics of U.S. Businesses program, we classify firms into 4-digit NAICS industries in which they paid the largest share of their payroll based on their establishment-level payroll data in LBD. We start our sample in 1990, which provides

[10] The federal minimum quarterly earning threshold=federal hourly minimum wage in a given year $\times$ 40 hours per day $\times$ 52 weeks /4 quarters. The U.S. federal minimum wage time series was downloaded from https://www.dol.gov/agencies/whd/minimum-wage/history/chart.

[11] A potential limitation of this adjustment is that we undersample workers who switch jobs twice in two subsequent quarters.

[12] We keep the highest average earning (i.e., the best-paid job) if a worker worked at different firms within that year.

us with sufficient time series before the first adoption of the anti-recharacterization laws (1997). We end our sample in 2012 because the matching quality between LEHD and LBD worsens after 2012. We then link the LEHD-LBD matched sample with Compustat using the Compustat-SSEL Bridge (CSB) to obtain employers' gvkeys and their financial data. Lastly, for each gvkey-year, we merge in their historical incorporation states obtained from the SEC Analytics Suite by WRDS.[13]

3.3 Resume Data

We obtain proprietary resume data from Revelio Labs. Revelio gathers publicly available profiles from various sources, and unifies employer names to create a unique set of company IDs. Their individual position data covers the names and unique identifiers of the employer, employee, job position, and occupation categories. Revelio provides the start and end dates of each position. To correct potential lags in updating resumes, they also adopt a nowcasting model to provide a more timely estimate of the inflows and outflows of employees. In our analysis, we rely on salary differentials across positions to infer promotion. In Revelio, salary is imputed based on job title, company, location, years of experience, and seniority using a statistical model. We consider a worker to be promoted if he/she moves to more highly paid positions.

At the worker level, Revelio predicts the probabilities of a worker belonging to each racial group, White, Black, Asian and Pacific Islander, Hispanic, Native, or multiple races. The algorithm is a Bayesian Inference Model drawing data based on first, last names and locations.[14] For a given worker, the sum of probabilities of belonging each group is equal to 100%. We define a worker as a minority if her probability of being a non-white worker exceed 50%. Similarly, Reverlio predicts a worker's gender using their first name. We also have information regarding educational background from their resume. We classify individuals based on the highest education attained, into four categories analogously to the classification in the Census Bureau. These categories include below high school, high school, some college, and college and above.

In the resume data, each unit of observation is a worker's job span. From this dataset, we remove non-U.S. jobs and part-time jobs. We expand the remaining observations into a worker-year panel parallel to the one built from the Census data. A key procedure in constructing this

[13]The state of incorporation may change over time, but the information provided in Compustat/CRSP only represents the most recent state of incorporation.

[14]See more details about gender and racial group predictions at here.

sample is to match employers to the public firm identifier in Compustat.[15] To do so, we start with the Factset ID provided by Revelio, which links employer names to Factset establishments. We then retrieve the historical corporate hierarchy from Factset and connect them to gvkey identifiers in Compustat back in time. This requires us to use linking tables from the identifiers in Factset to those in CRSP, and from CRSP to Compustat, both of which are provided by WRDS. This procedure gives us a worker-firm-year panel that allows us to track the progression of individual workers' career paths over time. After matching to public firms, our initial resume sample contains approximately 33 million unique workers and 70 million jobs.

We create several variables indicating changes in worker career. First, we define *New Position* that equals one if a worker is assigned to a different job position in the next year, and zero otherwise. This is an indicator for within-firm job mobility. Second, we define *Promotion* as an indicator for whether a worker changes his/her position and the new position offers a higher salary than the current one in the following year. Third, we define *Promotion Within Occ* to be one if a worker changes to a higher-paid position within the same firm and the same three-digit SOC in the next year, and zero otherwise. Finally, we code *Change to HiTech* as an indicator for whether a worker changes his (her) job code from a non-high-tech category to a high-tech category within a firm.[16] All indicators are multiplied by 100 so our coefficients indicate job transition and promotion rates in percentage points.

3.4 Sample Construction

We consider a firm to be treated if its state of incorporation is Louisiana, Texas, or Alabama, which enacted the anti-recharacterization laws prior to 2003. As previously discussed, four other states also adopted the laws around or after 2003, including Delaware in 2002, South Dakota

[15]While Revelio Labs provides a firm ID-gvkey mapping, the mapping is based on the most recent corporate structure and does not account for mergers and acquisitions over time.

[16]Following Hecker (2005), high-tech occupations refer to scientific, engineering, and technician occupations, which include the following occupational groups and detailed occupations: computer and mathematical scientists, Standard Occupational Classification (SOC) 15-0000; engineers, SOC 17–2000; drafters, engineering, and mapping technicians, SOC 17–3000; life scientists, SOC 19–1000; physical scientists, SOC 19–2000; life, physical, and social science technicians, SOC 19–4000; computer and information systems managers, SOC 11–3020; engineering managers, SOC 11–9040; and natural sciences managers, SOC 11–9120. Workers in these occupations need an in-depth knowledge of the theories and principles of science, engineering, and mathematics underlying technology, a knowledge generally acquired through specialized post-high school education in some field of technology leading up to an award ranging from a vocational certificate or an associate's degree to a doctorate. Individuals employed in these occupations are collectively referred to as technology-oriented workers.

in 2003, Virginia in 2004, and Nevada in 2005. Following Li et al. (2016), we only consider the three early-adoption states because the 2003 federal court ruling introduced uncertainty to the implementation of later state laws. To create a clear separation of firms affected and unaffected by the laws, we exclude firms incorporated in the late-adoption states from our sample, and only retain firms incorporated in LA (1997), TX (1997), and AL (2001)(i.e., "affected" firms) as well as firms in never-adoption states. Among the remaining publicly traded firms, we further exclude financial firms (NAICS 53-54), regulated utilities (NAICS 22), and public administration (NAICS 92). This creates our initial sample of publicly traded employers.

Using the worker-public firm linkage, we retain all workers that have been employed by our initial sample of firms. We classify workers to be "treated" if they were employed by an affected firm during the year prior to the passage of the laws. To account for the changes in earnings due to job transitions, we focus on workers with at least one year of work record both before and after the event with the affected firm and obtain their entire employment histories at that firm.

To construct the control group of workers, we start with those employed by firms incorporated in never-adoption states and match them to treated workers in "comparable" firms. Specifically, we require treated and control workers to be employed by firms in the same industry (2-digit NAICS) and belong to the same employment size quintile across all firms in the initial sample. Size is measured in the year before law adoption. Recent research documents that firm characteristics are powerful determinants of income inequality (e.g., Song et al. 2019; Mueller et al. 2017). Our matching based on employer characteristics helps us compare racial pay gaps in similar firms, instead of comparing across different types of firms.

After matching each group of treated workers with their control group, we stack the matched groups together to form a stacked panel. Performing analysis on this stacked panel helps address concerns related to the generalized difference-in-difference regressions highlighted in contemporary work (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021). Using the census data, our testing sample includes the employment histories of 453,600 unique workers and 498 unique firms.[17] Using the resume data, our testing sample includes 2.67 million unique workers and about 3.26 million job spans in total.[18]

[17] All observation counts and estimates are rounded according to Census disclosure policies.

[18] Revelio data covers workers across all states, whereas our Census project only have access to workers in 25 participating states. This could contribute to the difference between our resume sample and the Census sample.

3.5 Descriptive Evidence

3.5.1 Summary Statistics

Table 2 presents the summary statistics of the key variables used in our study. Within the LEHD-LBD sample, the average worker in our sample makes around $14,670 per quarter, which translates to annual earnings of $58,680 in 2018 dollars. Workers in our sample are, on average, 40 years old and have 6 years of experience working in a firm. Around 15% of workers are non-white minorities, consisting of 4.4% Asian, 8.2% Black or African-American, and 2.1% other races. 56.6% of workers are male.

Based on the resume data, workers have a 3.5% likelihood of switching to a new job position within the firm. They also face about the same likelihood of being promoted to a higher-pay position, and about 1.5% of being promoted to a position in the same three-digit SOC category. Finally, we note that the likelihood of transitioning to a high-tech position is generally low, around 0.2%. Workers in the resume sample, on average, have 6 years of experience working in a firm. Around 17% of workers are non-white minorities and 64% of workers are male.

3.5.2 Racial Earnings Gaps in Public Firms

As a starting point of our analysis, we examine the difference in earnings between white and non-white workers in our sample of U.S. public firms. Figure 1 presents the differences in average quarterly earnings between minority and white workers in our Census administrative sample. Consistent with the literature, white workers persistently earn more than their non-white peers, with the racial earnings gaps rise from 12% to 20% over our sample period.

We next regress the log of earnings on an indicator *Minority* for whether an individual is a non-white worker or not. Table 3 reports the results from four specifications. In the first column, we do not impose any control. We then add event-by-firm fixed effects so as to track workers in the same firm over time, event-by-year fixed effects to remove common trends around an event, and control for firm characteristics. In column (3), we control for worker characteristics, including worker tenure as well as dynamic fixed effects related to worker gender and education levels. In columns (4) and (5), we add event-state-year and event-industry-year fixed effects to control for local economic conditions and industry dynamics. In column (6), we further include event-firm-year fixed effects to absorb any firm-level dynamics that might affect earnings across white and non-white workers. Across all specifications, we find a significant, negative coefficient for *Minority*, which indicates that minority workers have lower earnings compared to white workers with the same education, gender, and work experience. The coefficient estimates remain stable across all specifications, suggesting around a 10% earnings gap between minority and white workers with similar characteristics in our Census sample.

TABLE 3 ABOUT HERE

Figure 2 presents the shares of minority workers across different seniority levels inside firms in our sample. To evaluate the job hierarchies, Revelio creates a seniority index using an ensemble model, based on information regarding the title, company, industry, as well as an individual's job history and age. Jobs are categorized into seven levels: entry, junior, associate/analyst, manager, vice president, director, and C-suite.[19] At the lowest level of seniority ("Entry"), minority workers account for 19% of total workers. As we move up the job ladder to the vice president or director level, minority workers decline to 13%. Less than 11% of C-suite jobs are occupied by minority workers. These patterns suggest that minority workers potentially face frictions preventing them from moving up the job ladder, and the lack of vertical movement could contribute to the racial pay gap inside firms.

4 Empirical Framework

We employ a triple-difference-in-difference framework, examining the differential effect of the anti-recharacterization laws on white and nonwhite workers. Our main regression approach using the Census sample is the following:

$$\begin{aligned} Log(Earnings)_{i,e,f,t} = {} & \beta Treat_f \times Post_{e,t} \times Minority_i + \theta Treat_f \times Post_{e,t} \\ & + \gamma Minority_i \times Post_{e,t} + \alpha_{e,i,f} + \mu_{e,j(f),t} + X_{e,i,f,t} + \epsilon_{i,e,f,t}, \quad (1) \end{aligned}$$

[19] Examples of these categories include: 1. Entry level (e.g., Accounting Intern, Software Engineer Trainee, Paralegal); 2. Junior Level (e.g., Account Receivable Bookkeeper, Junior Software QA Engineer, Legal Adviser); 3. Associate/Analyst Level (e.g., Senior Tax Accountant; Lead Electrical Engineer; Attorney); 4. Manager Level (e.g., Account Manager; Superintendent Engineer; Lead Lawyer); 5. Vice President Level (e.g., Chief of Accountants; VP Network Engineering; Head of Legal); 6. Director Level (e.g., Managing Director, Treasury; Director of Engineering, Backend Systems; Attorney, Partner); 7. C-suite Level (Ex. CFO; COO; CEO). See more details at here. This index, while well suited for comparing worker seniority within each firm, can be a coarse metric of workers' career progression. We thus rely on salary information to infer promotions.

where i is an individual, f represents a firm, and t is a year. e indicates an event, which includes all observations related to a matched group of treated and control firm observations. $Treated_f$ equals one if firm f is incorporated in any of the three states that passed an anti-recharacterization law prior to 2003. $Post_{e,t}$ turns to one for years after the inception of the laws under event e. $Minority_i$ equals one if individual i's race is Black, African American, American Indian Native, Alaska Native, Asian, Native Hawaiian or Other Pacific Islander, or multi-race, and zero if individual i's race is White.

Our estimation controls for a rigorous set of fixed effects to remove potential confounding effects. To start, we include event-worker-firm fixed effects ($\alpha_{e,i,f}$), which allow us to track a worker's earnings inside a firm over time, and eliminate effects related to worker-firm matching around a specific event. This set of fixed effects absorbs the standalone term of $Treated$ and $Minority$ as well as their interaction. We also control for event-state-year fixed effects to purge away the effects of local economic conditions that may jointly affect a treated firm and its matched control firms. Similarly, event-industry-year fixed effects help remove industry dynamics that could influence the racial earnings gap inside firms within the matched group. Industry is defined at the 3-digit NAICS level. These dynamic fixed effects absorb the standalone term $Post$. In more rigorous specifications, we also impose firm-by-year interactive fixed effects, which control for any changes at the firm level or broader than the firm (such as local conditions, including labor supply) and narrow down the comparison to only white and minority workers within the same firm.

We additionally control for a range of firm and worker characteristics. Firm characteristics include firm size (log of total assets), firm age (years since a firm first appears with positive employment in LBD), ROA (net income over total assets), and market-to-book ratio (the ratio between market and book value of firm assets). Worker characteristics include workers' tenure (in years), event-worker education category-year fixed effects, as well as event-worker gender-year fixed effects, and event-minority-year fixed effects. These fixed effects remove effects from all common trends that affect skill premium, gender pay differentials, and racial earnings gap. Note that event-minority-year fixed effects absorb the coefficient from $Minority \times Post$. Given that they are interacted with each event (or match group), they also account for the differential evolution of the earnings gap among worker types across matched groups.

From this estimation, we are interested in β, which captures the differential effects of the anti-recharacterization laws on the earnings of minority relative to white workers. Standard errors are clustered by the state where individual i works.[20]

Using the resume data, we estimate workers' promotion probability using a similar regression framework:

$$Y_{i,e,f,t} = \beta Treat_f \times Post_{e,t} \times Minority_i + \theta Treat_f \times Post_{e,t} + \gamma Minority_i \times Post_{e,t} + \eta Treat_f \times Minority_i + \alpha_{e,f} + \tau_{e,t} + \epsilon_{i,e,f,t}, \quad (2)$$

where Y includes *New Position*, *Promotion*, *Promotion Within Occ*, and *Change to HighTech*. Our estimation includes event-firm and event-year fixed effects. In this panel, given that individual workers face few promotions, we follow Benson et al. (2022) and do not impose worker fixed effects in the regression. But, in some specifications, we additionally control for a set of worker characteristics, including workers' tenure (in years), event-worker education category-year fixed effects, event-worker gender-year fixed effects, event-minority-year fixed effects, as well as event-occupation-year fixed effects. Standard errors are clustered by state. Again, we are interested in β, which informs us whether access to financing leads to greater increases in promotion rates and job category changes for minority workers, compared to white workers.

5 Main Findings

5.1 Baseline Results

Table 4 presents the main findings of our study. We estimate Equation (2) using the matched event-worker-year panel. We report six specifications. In column (1), we include event-firm-individual fixed effects and event-year fixed effects. In column (2), we add firm characteristics. In the first two columns, we estimate *Minority*×*Post* but do not report the coefficients. In column (3), we switch event-year fixed effects to dynamic fixed effects related to worker characteristics, including event-worker gender-year, event-worker-education year, and event-minority-year fixed effects. We also control for worker tenure. In column (4), we additionally impose controls for local economic conditions using event-state-year fixed effects. In column (5), we add event-industry-year fixed effects to remove changes in industry conditions. Finally, in col-

[20]Our baseline results are robust when standard errors are clustered by firms' headquarter states or incorporate states.

umn (6), we impose event-firm-year fixed effects to focus on the differential changes in earnings between minority and white workers around the event.

Table 4 About Here

Across all specifications, *Treated*×*Post*×*Minority* generates a positive and statistically significant coefficient, suggesting that after the adoption of the anti-recharacterization laws, minority workers experience a higher increase in earnings than white workers in affected firms. The estimates suggest that in treated firms, minority workers observe a 3%–4% greater increase in earnings compared to white workers in the same firm and time, with the same gender and education levels. This effect is substantial compared to the overall racial earnings gap in our sample firms of around 10%.

What race drives this effect? We next decompose *Minority* into three groups, including *Black*, *Asian*, and *Other* and re-estimate Equation (2). Table 5 reports the results from the same set of specifications as in our baseline analysis (Table 4). We find that both Black and Asian workers experience substantially higher earnings increases than white workers after the laws. Specifically, the triple interaction coefficient is 4%–6% for both Asian and Black workers. There is virtually no effect from other groups.

Table 5 About Here

5.2 Dynamic Effects

We examine how access to debt markets dynamically influences the racial earnings gap by investigating the evolution of worker earnings every year within the event window. This also allows us to verify the parallel trend assumption for difference-in-difference settings.

We estimate the dynamic effects of the ARLs in two ways. First, we estimate coefficients from the triple difference design for every year of the event window as follows:

$$Log(Earnings)_{i,e,f,t} = \sum_{k=-2}^{5} \phi_k 1_{t=e_t+k} \times Treated_f \times Minority_i + \alpha_{e,i,f} + \psi_{e,f,t} + X_{e,i,f,t} + \epsilon_{i,e,f,t}, \tag{3}$$

Where k represents years around the [-2, +5]-year window around the adoption of the laws. e_t is the year of the adoption. $1_{t=e_t+k}$ is an indicator that equals one if the current year is k

years past the year of the event. $\psi_{e,f,t}$ represents event-firm-year fixed effects, which absorb the interactions between *Treated* and event time dummies.

To ease the interpretation of our estimates, we also separately examine how earnings evolve for minority and white workers around the passage of ARLs. We thus estimate the following regression for each of the subsamples:

$$Log(Earnings)_{i,e,f,t} = \sum_{k=-2}^{5} \phi_k 1_{t=e_t+k} \times Treated_f + \alpha_{e,i,f} + \mu_{e,j(f),t} + X_{e,i,f,t} + \epsilon_{i,e,f,t}, \quad (4)$$

Given that this model is estimated on subsamples of minority and white workers separately, we no longer include the triple interaction of *Minority*, *Treat*, and event time indicators. Instead, we are interested in ϕ_k, which informs us when the effect of financing shocks takes place, and how strong the effect becomes at each point of the event horizon. Controls are the same as in column (5) of Table 4.

Figure 3 reports the results from this analysis. Year 0 (i.e. the year of the law adoption) is absorbed as the benchmark year, so all coefficients reflect the changes in worker earnings relative to their Year-0 levels. Panel A reports the coefficients from the triple difference designs, based on Equation (3). Panels B and C report estimates for minority and white workers, respectively, based on Equation (4). From both specifications, we do not observe any significant changes in the earnings of minority or white workers prior to the passage of the ARLs. After the passage of the laws, earnings of both minority and white workers start to increase and become statistically significantly higher than their event-year levels at Year 3. However, the earnings growth for white workers is smaller than that of minority workers. As a result, the pay gap between white and nonwhite workers is significantly reduced starting in Year 3, and the gap continues to close in later years after the event.

Overall, findings from this analysis confirm that workers' earnings do not change prior to the law adoption, but increase significantly after the events. They also show that the narrowing of the racial earnings gap is driven by the most substantial increase in earnings for minority workers but not the decline in white worker earnings.

5.3 Within-firm Pay Rank

Research on racial inequality suggests that it is important to look at both the level earnings gap and the earnings rank gap between white and non-white workers (Bayer and Charles, 2018). To the extent that non-white workers may take different positions in the firm compared to white workers, the changes in earnings gap could be driven by workers changing their job/pay rank, or by the changes in the earnings inequality among ranks inside the firm. To investigate these possibilities, we re-estimate Equation 2 by substituting the dependent variable to be *Pay Rank*, the percentile ranking (1–100) of an individual's annual earnings relative to all workers inside the firm during the year before the event. This test helps shed light on whether the changes in the racial earnings gap are purely driven by changes in the pay between high- and low-rank employees inside the firm, or the changes in the relative job rankings for minority and white workers.

Table 6 presents the results from this analysis. Similar to the baseline results, we add controls and fixed effects in stages. In Panel A, we look into the average differences in the pay rank between minority and white workers in our sample. We follow the same set of specifications as in Table 3. Our results indicate that minority workers receive compensations that are ranked around 2 to 3-percentile lower than white workers. In Panel B, we find that better access to debt markets improves the relative pay rank of minority workers, narrowing the gap by around 2 percentiles. This estimate is stable and consistent across all columns, suggesting that our effects are unlikely to be explained by worker or firm characteristics.

TABLE 6 ABOUT HERE

Taken together, our results consistently suggest that better access to debt financing leads to a narrowing of the within-firm racial earnings gap. This effect is not explained by dynamic worker-firm matching or worker characteristics. It is driven at least partially by minority workers moving up the pay rank relative to white workers. We investigate the underlying mechanisms in greater detail next.

6 Mechanisms

In this section, we explore potential mechanisms underlying our main findings. Specifically, we consider the following possibilities. First, the financial shocks may have increased the returns to low-paying positions. This, in turn, raises the income of minority workers who are disproportionately matched to those positions. According to this explanation, low-skill, low-income minority workers should be most affected by the shocks.

Another explanation is that, due to biases or labor frictions inside firms, minority workers are matched to lower-paid positions, or worse-fit tasks for their skills compared to white workers. The laws relax firms' financing frictions, leading to an expansion of tasks that need to be filled by workers. To the extent that white workers are relatively well-matched to their current positions, firms are more likely to open up job opportunities to minority workers inside the firm. Consequently, minority workers are matched to higher-paying or better-fit positions. In contrast to the previous explanation, this argument does not predict a stronger effect for low-income minority workers. Instead, results should be stronger for skilled workers, who possess task-specific or firm-specific human capital that makes them difficult to replace with candidates outside the firm. We should also observe more frequent job switches by minority workers, especially to higher paid, higher skilled positions inside firms. In addition, our effects may also become stronger in areas where white workers are in relatively short supply.

Note that this human-capital-utilization explanation also requires there to be pre-existing frictions in the firm that prevent minority workers from being matched to the best-suited job opportunities. Such frictions could arise from discriminatory practices, biased beliefs, or social or cognitive limitations of managers. We conjecture that our effects should be stronger in cultural environments that tolerate greater inequality.

The third possibility is that financial shocks tightened the labor market for minority workers more than the market for white workers. Employers, in turn, raise compensation to retain minority workers. This argument suggests that results should be stronger in labor markets with lower concentration.

To investigate these explanations, we examine the role of worker skills and labor market tightness in moderating our effects.

6.1 Differential Effects Across Worker Skill

We examine whether financial shocks affect higher-skill or lower-skill workers differently. While worker skills are not directly observable, we rely on two proxies. First, we examine worker education provided by the LEHD data and define four categories of education attainment: below high school, high school, some college, college and above. Second, we use workers' prior income as a proxy for skill. This measure is motivated by the persistent skill premium in the U.S., whereby an important portion of the variation in worker pay is linked to labor skills (Juhn et al., 1993; Acemoglu and Autor, 2011; Guvenen et al., 2014). We sort workers into low-, middle- and high-skill groups using their annual income during the year prior to the adoption of the anti-recharacterization laws.

We examine the differential earnings growth between minority and white workers at different skill levels using a quadruple difference-in-difference framework. Formally, we estimate the following model:

$$\begin{aligned} Log(Earnings)_{i,e,f,t} = & \sum_s \beta_s Treated_f \times Post_{e,t} \times Minority_i \times 1^s_{e,i,t} \\ & + \sum_s \gamma_s Treated_f \times Post_{e,t} \times 1^s_{e,i,t} + \alpha_{e,i,f} + \mu_{e,j(f),t} + X_{e,i,f,t} + \epsilon_{i,e,f,t}, \quad (5) \end{aligned}$$

where 1^s indicates worker skill type s, and β_s reveals the effect of the financing shock on the racial earnings gap within skill s. We follow the specification in Column (6) of Table 4, including controls for event-firm-worker fixed effects as well as event-firm-year fixed effects.

Figure 4 reports the results. For simplicity, we only plot the coefficient estimates for $\{\beta_s\}$ and not those of control variables. Panel A provides results related to worker skill defined by education, while Panel B reports results where worker skill is defined by pre-event earnings. Across both measures of labor skill, we find that better access to debt markets does not reduce the racial pay gap among low-skill workers, but substantially narrows the pay gap for medium- and high-skill workers. The estimates the racial pay gap between minority and white workers reduce by about 4% following the passage of ARLs for workers with a high-school or college degree, and for workers with medium or high pre-event income.

These results are at odds with the argument that the increases in minority workers' earnings are driven by a higher return to low-paying positions. Instead, they are consistent with the explanation that minority workers are granted more, or better-fit tasks (positions) as their employers can better access external debt markets.

6.2 How Financing Changes Worker Careers: Evidence from Resume Data

We use the resume database to further validate the human capital utilization channel. To start, we provide descriptive evidence regarding the job mobility and promotion patterns for white and nonwhite workers in our sample firms. Table 7 provides this information. Each career outcome variable is described in two columns, with the first one containing no control variables, and the second one representing the most stringent specification. In it, we control for all the fixed effects and control variables in Table 3, and in addition event-occupation-year interactive fixed effects. We find that minority workers are around 0.5 percentage point less likely to switch to a new job position than white workers, 0.34 percentage point less likely to be promoted to a higher-pay position, and 0.17 percentage point less likely to be promoted within the same three-digit job category. Finally, minority workers also have a 2 bps lower likelihood to switch to a technology-oriented position.

Table 7 About Here

In Table 8, we further investigate whether better access to financing affects the careers of minority and white workers differently. Each panel presents results regarding a career outcome variable, with controls and fixed effects added in stages. In Panel A, we find that minority workers are more likely to obtain a new position following the adoption of anti-recharacterization laws. The coefficient from the most stringent specification (Column (7)) suggests that the job mobility gap between minority and white workers reduces by 0.2 percentage points, about 40% of the sample average (= 0.196/0.486). In Panel B, we find that minority workers are more likely to be promoted, defined as a worker moving to a new position with a higher salary. This effect also generates sizable economic magnitudes, around 57% of the sample mean (= 0.195/0.34). Panel C further shows that minority workers are more likely to face promotion within the same three-digit occupation code. Finally, in Panel D, we find that minority workers face a greater increase in the likelihood of switching to a technology-heavy occupation compared to white workers after the adoption of ARLs.

Table 8 About Here

These results are in line with the previous evidence suggesting that the financing shocks mostly influenced skilled minority workers. Consistently, we observe minority workers to be more likely to switch technology-oriented occupations and be promoted to more senior, higher-pay positions. These findings lend further support to the human capital utilization channel, i.e., better access to financing improves the matching of minority workers' human capital to their tasks.

6.3 The Role of White Worker Supply

As firms go through expansion and increase the demand for skilled workers, they may allocate more job opportunities to minority workers if white workers are in relatively short supply. We thus test whether the narrowing of the racial pay gap is more pronounced in areas with a lower fraction of white workers. We measure white worker share as the percentage of workers in a commuting zone during the year before the adoption of the law. Data on aggregate worker count by race come from the LEHD. We create tercile indicators of white worker share and interact $Treated \times Post \times Minority$ with each of these indicators, following the specification in Equation (5).

Panel C of Figure 4 provides the results from this analysis. Consistent with our conjecture, the narrowing of the earnings gap is most pronounced in areas with a low share of white workers, potentially implying a shortage of such workers. In these areas, firms seeking to increase the usage of skilled workers may rely on minority workers to a greater extent.

6.4 The Role of Pre-Existing Inequality

The task-based explanation suggests that there are biases or frictions inside the firm preventing them from fully utilizing the human capital of minority workers. We gauge the relevance of such frictions by examining whether pre-existing racial inequality or norms inside the firm can moderate our effects. We design two analyses along this line. First, we compute the racial pay gap inside firms during the year prior to the financing shock, which sheds light on the pre-existing inequality inside firms. Second, we follow Bernile et al. (2018) and measure the demographic diversity among the board of directors. Specifically, board diversity is a linear combination of the standardized values of (1) the share of female directors, (2) the standard deviation of director ages, and (3) the reversed Herfindahl–Hirschman index (HHI) in director ethnicity.[21] Cai et al. (2022) find that greater board diversity is associated with more diverse workforce hiring and more inclusive corporate cultures.

Similar to the way we categorize worker skills, we create tercile indicators for pre-event pay gap and board diversity, and interact each of the tercile indicators with *Treated*×*Post*×*Minority* in a quadruple difference-in-difference framework. Table 9 shows the results. Columns (1) through (3) provide results for the pre-event racial pay gap, and Columns (4) through (6) present results for board diversity. Note that the higher values of the pay gap represent more severe inequality, while higher values of diversity indicate lower diversity-related frictions inside the firm. Our evidence suggests that the reduction in pay inequality is concentrated among firms with the highest racial pay gap prior to the event and firms with the lowest level of board diversity. In other words, better access to debt increases minority pay in workplaces with severe racial inequality and less tolerance of diversity. These patterns highlight the important role of corporate finance in alleviating the labor market frictions faced by minority workers.

TABLE 9 ABOUT HERE

6.5 The Role of Labor Market Concentration

We next discuss an alternative explanation, which is, the financial shocks we study reshape the structure of labor markets, which in turn affects worker pay. To predict a differential change in white and minority worker pay, this argument needs to assume that the shocks changed the labor market concentration for the two groups of workers differently. This seems unlikely, given that our baseline analysis already includes industry-year fixed effects, and state-year fixed effects. Still, one may argue that the dynamics of sub-labor markets at a smaller locality could be driving our results.

To evaluate this explanation, we compute the labor market concentration at the commuting zone level both for all workers and separately for white and minority workers. Labor market concentration is the Herfindahl-Hirschman index (HHI) based on the percentage of workers hired by each employer in a given commuting zone. Lower values indicate more competitive markets. Table B.1 reports the quadruple interactive effect related to local labor market concentration.

[21] We collect the demographic characteristics of board directors from BoardEx and RiskMetrics. HHI in directory ethnicity is constructed as the sum of the squares of director ethnicity shares within the board of a given firm-year. Ethnic categories, as defined in RiskMetrics, include Asian, African-American, Caucasian, Hispanic, and Native American.

We do not see a clear effect from labor market concentration as there is no difference in the effects of the laws between the most competitive and the most concentrated labor markets.

Taken together, our results so far suggest that firms with better access to debt markets increase the compensation for minority workers, especially minority workers with higher education and labor skill. Minority pay increases more in areas with a lower supply of white workers and in firms where minority workers likely faced greater obstacles in the workplace. These patterns are consistent with the conjecture that better access to debt markets allows firms to expand production and allocate minority workers to better-suited positions. Our results also seem at odds with alternative explanations, such as financial shocks mainly affecting low-skill, low-income minority workers, or that they affect worker pay by altering the structure of local labor markets.

7 Extensive Margins and Long-Run Effects

In the analysis so far, we have focused on the income growth of existing workers that remain in the firm. We now shift our lens to the extensive margin and study the effect of debt capacity on the racial retention gap as well as the racial earnings gap for new-hire workers. We also track the long-term earnings of workers that leave their employers eventually.

7.1 Separation

In Table 10, we examine the differential job separation likelihood between white and minority workers using the employmer-employee matched LEHD-LBD sample. Panel A follows the setup in Table 3 and reports the average difference in job separation rates between white and minority workers. On average, we observe a significant racial gap in job separation: Minority workers are 2 percentage points more likely to separate from their employers next year compared to white workers with the same gender, tenure, and education. Results also survive the strict control for industry-year and state-year fixed effects.

TABLE 10 ABOUT HERE

In Panel B, we examine whether the adoption of anti-recharacterization laws is associated with a change in the racial gap in separation, but do not find an effect. Specifically, workers, on average, become less likely to separate from their employers after the treatment, but the effects are not significantly different between white and minority workers. This means that, while increased debt financing increases the compensation for minority workers, those workers are not more likely to be retained by the firm.

7.2 New-hire Earnings

We next focus on the earnings of newly hired workers. New hires are defined as workers whose tenure is 0 or 1, indicating that they have just joined the employer. We analyze whether there is a racial income gap among new hires by regressing their income on the indicator for *Minority* with the same set of controls as those in Table 3. Panel A of Table 11 shows that among new joiners, minority workers receive 7% lower income compared to white workers. This gap is slightly smaller than the average income gap of all workers inside the firm (around 10%), likely because new hires occupy relatively junior positions and previous research documents that income inequality is amplified among senior positions inside firms (Fox, 2009).

TABLE 11 ABOUT HERE

In Panel B, we estimate Equation (2) to examine whether the racial income gap for new entrants narrows when the employer faces better access to debt markets. In this analysis, we drop individual fixed effects because an individual only appears once as a new hire for a firm. Thus, our test does not track the same worker over time, but instead compares the different cohorts of new hires of the same firm. We find a significant, positive coefficient for *Treated* × *Post* × *Minority*. Our estimates suggest that after the laws, newly hired minority workers experience around 6% higher growth in income compared to newly hired white workers. This is a large magnitude than the one for incumbent workers.

7.3 Long-Run Effects

Our results so far suggest that when employers have better access to financing, minority workers receive higher earnings, leading to a narrower earnings gap relative to white workers. This effect holds not only for incumbent workers inside the firm, but also for new entrants. What happens when workers leave the firm? When minority workers depart the treated firms to join another, does the labor market undo the earnings gain? One could expect this to be the case if the majority of potential employers are not affected by the laws. Those new employers may reverse the earnings gain, bringing the income levels of minority workers back to the pre-event level. On the other hand, the increased earning levels may serve as the benchmark for future wage bargaining, thus permanently raising minority workers' income in the future. Relatedly, if minority workers have been exposed to better-fit tasks and new opportunities in the original firms, they can accumulate more valuable human capital and become more desirable in the external labor market.

To answer this question, we decompose *Post* into two indicators: *Post (Same Firm)* equals one after the passage of the anti-recharacterization laws and before a worker leaves his employer at the time of the event. This indicator turns to one when the worker departs the original employer. *Post (Different Firm)* turns to one once the worker joins another employer. This variable equals zero before the job switch. We interact both indicators with *Treated* and *Minority* in the triple-difference-in-difference framework described in Equation 2. Coefficients on *Treated* $\times$ *Post (Same Firm)* $\times$ *Minority* represent the direct effect of the laws on minority workers through the affected employers, while coefficients on *Treated* $\times$ *Post (Different Firm)* $\times$ *Minority* compares workers' income after their job switch to the pre-event levels, thus capturing the long-run effects. Given that we allow workers to switch employers, we no longer control for interactive effects related to firm and industry, such as event-firm-year and event-industry-year fixed effects.

Results are shown in Table 12. This table follows the same specifications as our baseline analysis (Table 4). We find that *Treated* $\times$ *Post (Same Firm)* $\times$ *Minority* and *Treated* $\times$ *Post (Different Firm)* $\times$ *Minority* both generate a positive, statistically significant coefficient with similar magnitudes. This means that minority workers receive an increase in earnings from the original employer after the law's adoption, and this earnings gain persists even if the workers join a new employer. Across all columns, the equality tests between the coefficient estimates on *Treated* $\times$ *Post (Same Firm)* $\times$ *Minority* and *Treated* $\times$ *Post (Different Firm)* $\times$ *Minority* fail to reject the null hypothesis that earnings increases are the same across the original employer and new employer, with P-value all above 0.80. In other words, the labor market does not undo the effect of the financial shock, either because the affected workers have accumulated more human capital, or because the elevated earnings serve as a benchmark for future pay negotiations.

TABLE 12 ABOUT HERE

8 Additional Analyses

We design several analyses to test the robustness of our baseline results to alternative empirical choices.

8.1 SPV Usage

We verify a key mechanism through which the anti-recharacterization laws affect firms' ability to raise debt financing. Under these laws, firms have an expanded debt capacity because they can tap into various debt markets and raise off-balance sheet debt through SPVs. If our results indeed stem from firms' ability to finance off-balance sheet, we expect the effects to become stronger for firms that have SPVs outstanding, who are more likely to enjoy the benefit of the anti-recharacterization laws.

We follow Feng et al. (2009) and collect firms' disclosure of SPV-like subsidiaries. Specifically, we proxy for a firm's usage of SPVs using the existence of limited partnerships, limited liability partnerships, limited liability companies, and trusts among the firm's subsidiaries and affiliates that are disclosed in Exhibit 21 of the SEC Form 10-K. Accordingly, we create an indicator for *Has SPV* that equals one if a firm discloses at least one SPV-like entity in Exhibit 21 in a year, and zero otherwise. Analogously, *No SPV* indicates that the firm has no SPV outstanding. In Table 13, we find that the effect of the ARLs on the within-firm earnings gap is concentrated among firms that likely have an established SPV. This result helps validate the channel that ARLs affected work compensation by increasing firms' ability to raise debt through SPVs.

TABLE 13 ABOUT HERE

We start by considering the role of the federal court ruling for Reaves Brokerage Company, Inc v. Sunbelt Fruit & Vegetable Company, Inc. (336 F.3d 410, 413 (5th Cir. 2003)). In this bankruptcy case, the federal court overruled the anti-recharacterization law statute in Texas and re-characterized the transfer of assets from the debtor to its SPV as a loan. While this ruling does not nullify the existing and future anti-recharacterization laws at the state level, it does introduce uncertainty to the effectiveness of those state laws. In our baseline framework, we already take this ruling into account and exclude from our sample the state laws passed after 2003. Yet, for the early-adoption state (LA, TX, and AL), we continue to assign *Post* to be one after 2003. This is to account for the stickiness of salaries in the labor markets. In other words, minority workers who were affected by the earlier laws may continue to enjoy a raised income level even after those laws become less effective. Indeed, we confirm in Table 12 that minority workers observe a persistent earnings increase, even when they leave the treated firms.

We now gauge the direct effect of the laws passed prior to 2003 on minority workers by turning the indicator for law adoption (*Post*) to zero after 2003 (Li et al. 2016; Ersahin 2020). Under this definition, the coefficient for *Treat* × *Post* × *Minority* captures the effect of the laws before the federal court ruling in 2003. Panel A of Table 14 presents the results. The layout of this table strictly follows the baseline setup. We continue to find a significant, positive coefficient on the triple interaction term, suggesting that the earlier-adopted laws increased minority workers' earnings by around 1.6 to 3 percent more than white workers' earnings. This effect is slightly smaller than the baseline estimate, likely because we no longer account for the persistence of earnings in the long run.

TABLE 14 ABOUT HERE

In Panel B, we use an alternative method to cluster our standard errors. Our base results cluster standard errors by workers' state of employment to account for the fact that worker earnings are correlated within a local labor market. Now we cluster standard errors by firms' state of incorporation. Our results stay unchanged.

In Panel C, we switch the dependent variable from the average earnings of a worker across all four quarters in a year to the highest quarterly earnings in a year. This test helps to address the concern that anti-recharacterization laws may benefit white workers if they are disproportionately matched with performance-pay jobs where a main source of compensation comes from bonuses (Heywood and Parent, 2012). To the extent that average quarterly earnings smooth out the changes in bonuses, the effects on bonuses could be disguised. Our results continue to hold, suggesting that our findings are unlikely to be unduly driven by the matching of white workers to bonus-heavy jobs.

Finally, in Panel D, we use Poisson regressions for worker earnings without log transformation. This helps address the concern that the log transformation may lead to biased estimate (Cohn et al., 2022). We find that the Poisson regression generates consistent results as the OLS regressions. The estimates from Poisson regressions are even higher than those from the OLS results. Overall, our evidence suggests that the OLS results are unlikely to suggest inflated estimates for the effect of debt financing on the racial earnings gap.

In the last step of our robustness checks, we examine whether our results could be driven by changes in gender pay inequality, instead of racial inequality. We test this argument in Table B.2. In particular, we estimate the coefficient of a quadruple interaction term between *Treated*, *Post*, *Minority*, and *Female*. We do not find a statistically significant coefficient on this interaction term, suggesting no significant differential patterns exist between the pay of minority male and minority female workers. The interactive coefficient, however, seems large in economic magnitude, suggesting that minority female workers enjoy a 5% greater pay raise than minority male workers.

9 Conclusion

This paper investigates the role of access to financing in shaping racial pay inequality within firms. Using administrative data on individual employment records from the U.S. Census Bureau, we show that with better access to financing, firms substantially reduce the earnings differential between minority and white workers.

Our empirical strategy takes advantage of the staggered passage of anti-recharacterization laws across states and compares affected and unaffected individuals working in the same labor market and industry. We also benefit from the granularity of the administrative data, which allows us to contrast minority and white workers in the same firm, with the same gender and educational attainment. These empirical design choices help rule out multiple alternative explanations. For example, our results are unlikely to be driven by intrinsic differences across workers (aside from their race), time-varying compensation for skill by employers, labor market tightness, or dynamic worker-firm matching. We also rule out the explanation that low-skill tasks (potentially performed disproportionately more by minority workers) are better compensated after the passage of the anti-recharacterization laws.

We find that following the positive financing shocks, the increase in earnings is concentrated among middle- or high-skill minority workers. Minority workers also are more likely to switch to higher-pay, higher-skill positions inside firms. These findings indicate that firms are better able to utilize the human capital of minority workers when they have higher debt capacity. As documented in prior literature, anti-recharacterization laws allow firms to invest in innovation and new technology. These mechanisms could lead to more career opportunities for employees, especially minority workers whose human capital may have been under-utilized prior to the shocks.

We push forward the literature in several dimensions. This study is the first to provide causal evidence that the ability to raise debt financing narrows the racial income gap inside firms. We improve on the identification strategy used in the literature by utilizing idiosyncratic shocks to financing. We also shed new light on the underlying mechanisms. Importantly, our results inform the current debate regarding corporate social responsibility, showing that firms' ability to combat racial inequality critically depends on their access to financial resources.

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Figure 1. Racial Earnings Gap
The figure presents the average quarterly earnings in 2018 dollars for non-white vs white workers in our Census sample.

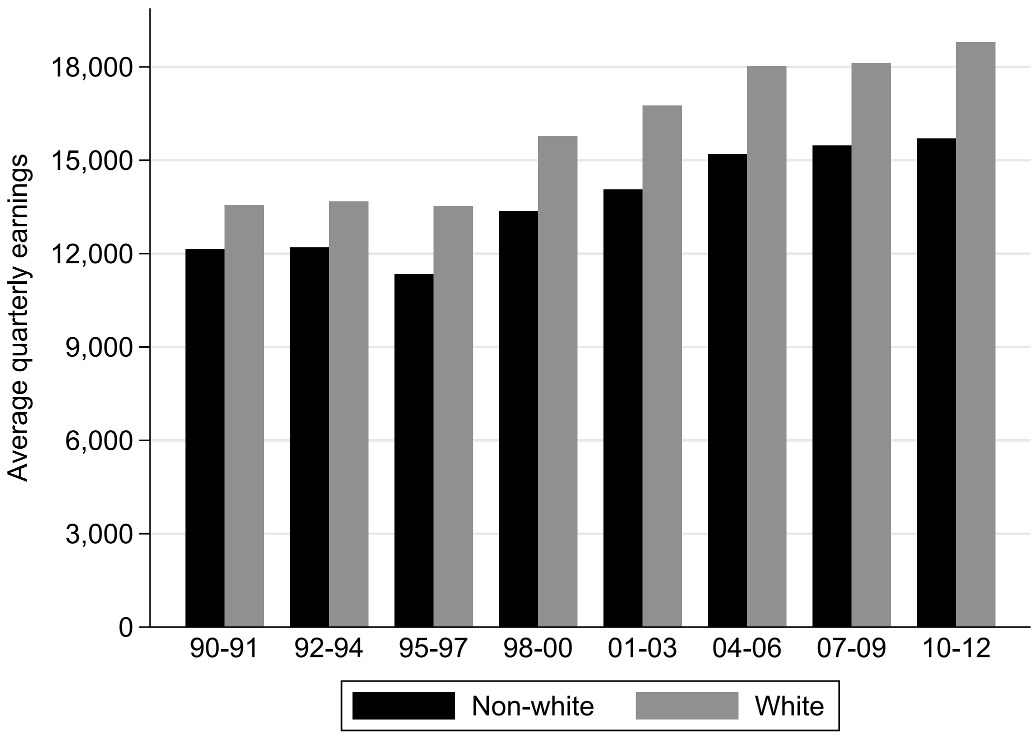


Figure 2. Minority Worker Share For Each Seniority
The figure plots the percentage of workers that are non-white at each job seniority in our sample. Data come from Revelio Labs.

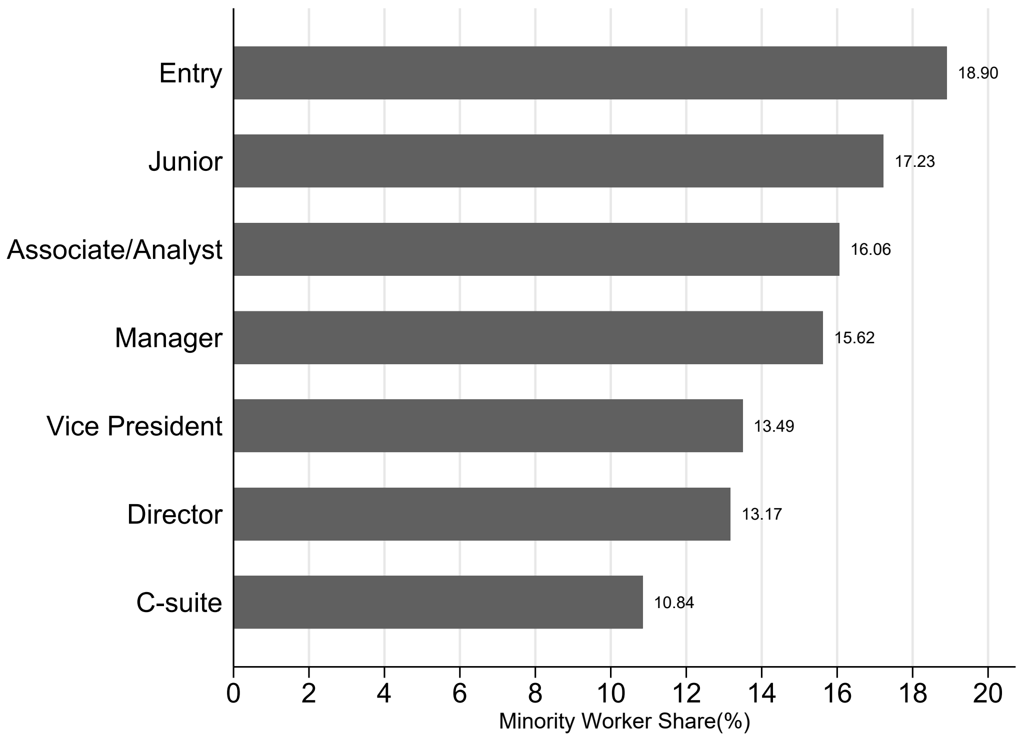
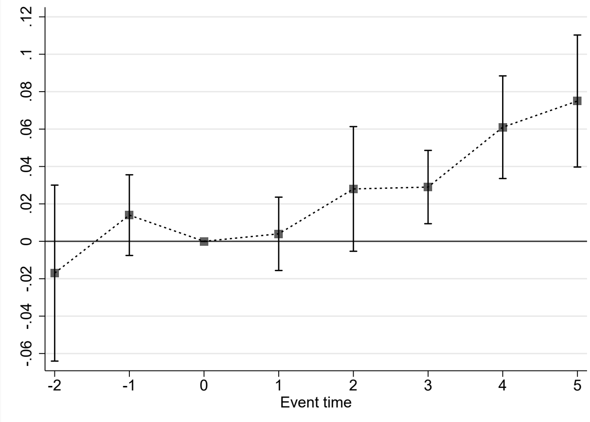
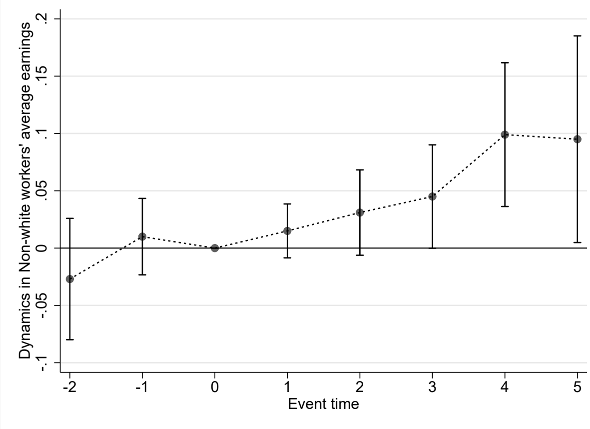


Figure 3. Dynamic Effects of ARLs on Worker Earnings

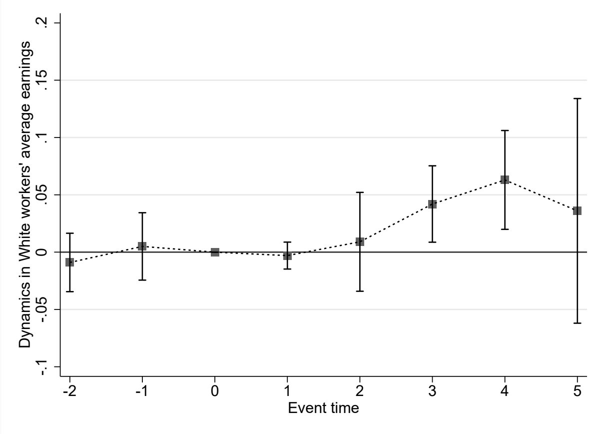
The figure plots the dynamic effects of the ARLs on the earnings gap between minority and white workers within firms. The dependent variable is workers' average quarterly earnings in a year. Panel A presents dynamic coefficients of the triple difference terms indicating earnings gap, Panel B reports changes in earnings for minority workers, and Panel C reports changes in earnings for white workers. In Panel B (C), the y-axis represents the estimated differences in average quarterly earnings ($log(Earnings)$) of treated and control minority (white) workers. Standard errors are clustered by workers' state.



Panel A: Earnings Gap

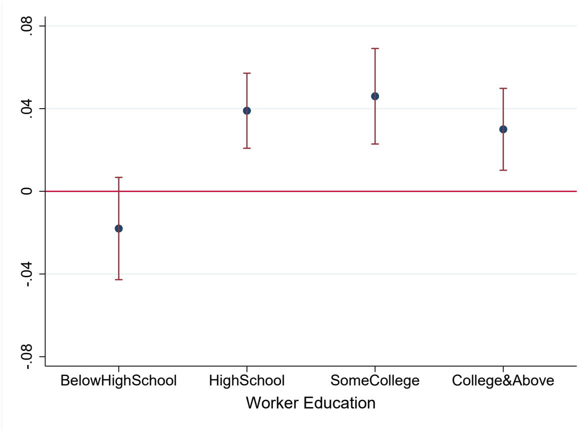


Panel B: Minority Worker Earnings

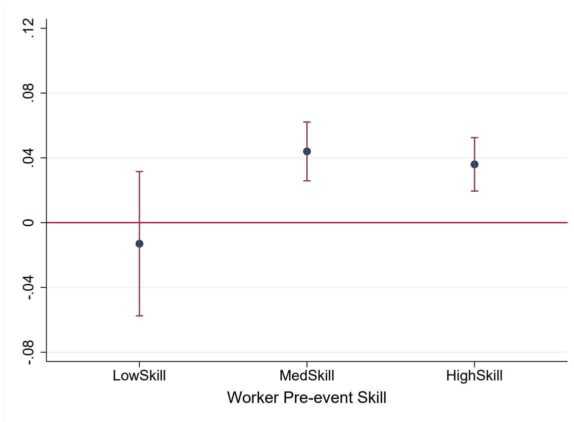


Panel C: White Worker Earnings

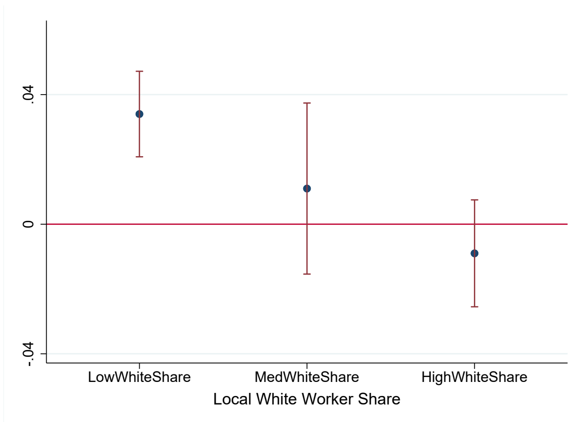
Figure 4. Heterogeneity of Effects: Worker Skill and the Supply of White Workers
The figure plots the differential effects of the ARLs on the earnings gap between minority and white workers within firms across various worker skill measures and local white worker supply. The dependent variable is workers' average quarterly earnings in a year ($log(Earnings)$). Panel A presents coefficient estimates for $Treated \times Post \times Minority$ interacted with different levels of worker education, categorized by below high school, high school, some college, and college and above. Panel B reports the interactive coefficients with worker skill, measured based on the five years of average earnings prior to the event. Panel C provides the interactive coefficient estimates related to local white worker share, i.e., the percentage of white workers in the commuting zone. Each interactive variable is partitioned into terciles, and the dots in each panel indicate the point estimate of $Treated \times Post \times Minority$ interacted with each tercile indicator. The vertical lines represent the associated 90% confidence intervals. All regressions include the same controls as Column (6) of Table 4, including event-firm-worker fixed effects and event-firm-year fixed effects. Standard errors are clustered by workers' state.



Panel A: Differential Effects Across Worker Education



Panel B: Differential Effects Across Worker Pre-event Earnings



Panel C: Differential Effects Across White Worker Supply

Table 1
LEHD Sample Coverage
This table presents the accessible states and years in the Employment History File (EHF) maintained by the U.S. Census LEHD program. See Vilhuber et al. (2018) for details of the LEHD program.

State	First year	Last year
Arkansas	2002	2014
Arizona	1992	2014
California	1991	2014
Colorado	1990	2014
D.C.	2002	2014
Delaware	1998	2014
Hawaii	1995	2014
Idaho	1990	2014
Illinois	1990	2014
Indiana	1990	2014
Iowa	1998	2014
Kansas	1990	2014
Maine	1996	2014
Maryland	1985	2014
Missouri	1994	2014
Nevada	1998	2014
New Mexico	1995	2014
New York	1995	2014
North Dakota	1998	2014
Ohio	2000	2014
Oklahoma	2000	2014
Pennsylvania	1991	2014
Tennessee	1998	2014
Virginia	1998	2014

Table 2
Summary Statistics
This table reports the summary statistics for the main variables used in our study. Panel A presents average quarterly earnings, tenure and worker demographics in the sample constructed using Census LEHD-LBD data. Panel B reports summary statistics of job position changes, tenure and worker demographics in the resume sample constructed using Revelio data. Both samples span the period from 1990 through 2012. The LEHD-LBD sample includes 3,669,000 worker-year observations. The resume sample includes 36 million worker-year observations. All estimates and observation counts are rounded according to Census disclosure rules. Detailed variable definitions are provided in Appendix A.

Panel A: Census LEHD-LBD Sample

Variable	Mean	St. Dev.
Log(Earnings)	9.35	0.68
Earnings ($)	14670	16850
Worker Age	40.49	10.98
Worker Tenure (in years)	5.94	4.56
Minority (in %)	14.8	35.5
Asian (in %)	4.43	20.6
Black (in %)	8.20	27.4
Other Minority (in %)	2.13	14.4
Male (in %)	56.6	49.6

Panel B: Revelio Resume Sample

Variable	Mean	St. Dev.
New Position (in %)	3.47	18.3
Promotion (in %)	3.49	18.3
Promotion Within Occ (in %)	1.45	12.0
Change to HiTech (in %)	0.18	4.20
Worker Tenure (in years)	5.83	6.77
Minority (in %)	16.9	37.5
Male (in %)	63.8	48.1

Table 3
Earnings Gap within Firms
This table examines the minority earnings gap using OLS model with different time-varying controls and fixed effects, where the dependent variable is *Log(Earnings)*, the log of quarterly earnings (in $2018) averaged within a given year for each worker. *Minority* is a dummy variable for all workers that are non-white. The sample is an individual-year panel, spanning from the period of 1992 through 2012. It covers workers in treated and control firms. Treated firms refer to companies incorporated in LA (1997), TX (1997), and AL (2001). Control firms are those that are in the same sector (NAICS2) and quintile of employment size bin as a treated firm, but are incorporated in states that never passed the laws. *Event* represents an indicator for a state passing the ARL. *Firm Age* is the age of the firm defined based on the first year observing positive employment in the Census LBD data. *Worker Tenure* is a worker's total work tenure with a given employer. *Firm ROA*, *Firm Market/Book*, and *Firm Size* are the return on assets, market-to-book ratio, and log of total assets of the employer, respectively. Firm characteristics are computed from Compustat. Standard errors are clustered by workers' state and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Dep. Var.: *Log(Earnings)*	(1)	(2)	(3)	(4)	(5)	(6)
Minority	-0.108***	-0.122***	-0.095***	-0.101***	-0.099***	-0.099***
	(0.028)	(0.019)	(0.011)	(0.010)	(0.010)	(0.010)
Firm Age		-0.012**	0.003	0.005	0.007**	
		(0.005)	(0.005)	(0.004)	(0.003)	
Firm ROA		0.190***	0.165***	0.175***	0.138***	
		(0.032)	(0.030)	(0.034)	(0.034)	
Firm Market/Book		0.010*	0.007*	0.006	-0.001	
		(0.005)	(0.004)	(0.004)	(0.004)	
Firm Size		0.02	0.032	0.054***	0.032***	
		(0.017)	(0.020)	(0.014)	(0.009)	
Worker Tenure			0.052***	0.060***	0.062***	0.065***
			(0.005)	(0.003)	(0.003)	(0.003)
Event-Firm FE		Yes	Yes	Yes	Yes	
Event-Year FE		Yes				
Event-Education-Year FE			Yes	Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes	Yes
Event-Minority-Year FE			Yes	Yes	Yes	Yes
Event-State-Year FE				Yes	Yes	Yes
Event-Industry-Year FE					Yes	
Event-Firm-Year FE						Yes
Observations	3669000	3669000	3669000	3669000	3669000	3669000
R-squared	0.003	0.301	0.429	0.439	0.446	0.451

Table 4
Access to Debt and Racial Earnings Gap Inside Firms
This table reports the change in the minority earnings gap post-treatment using a triple Diff-in-Diff model with different time-varying controls and fixed effects, where the dependent variable is *Log(Earnings)*, the log of quarterly earnings (in $2018) averaged within a given year for each worker. *Minority* is a dummy variable for all workers that are non-white. *Treat* is an indicator for workers working for parent companies incorporated in LA (1997), TX (1997), and AL (2001). The control group includes employees working for firms that are in the same sector (NAICS2) and quintile of employment size bin as a treated firm, but are incorporated in states that never passed the laws. *Post* is an indicator for years after the passage of the anti-recharacterization laws. *Treat* and *Treat×Minority* are absorbed by eventid-gvkey-pik fixed effects. Coefficients on *Post×Minority* are estimated but not reported for brevity in specifications without Event-Minority-Year fixed effects, and are absorbed when Event-Minority-Year fixed effects are included. *Firm Age* is the age of the firm defined based on the first year observing positive employment in the Census LBD data. *Tenure* is a worker's total work tenure with a given employer. *Firm ROA*, *Firm Market/Book*, and *Firm Size* are the return on assets, market-to-book ratio, and log of total assets of the employer, respectively. Firm characteristics are computed from Compustat. Standard errors are clustered by workers' state and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Dep. Var.: *Log(Earnings)*	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	0.038	0.017	0.013	0.011	0.033*	
	(0.023)	(0.023)	(0.025)	(0.019)	(0.017)	
Treat×Post×Minority	0.052***	0.048***	0.045***	0.039***	0.042***	0.032***
	(0.011)	(0.011)	(0.013)	(0.011)	(0.011)	(0.009)
Firm Age		-0.008**	-0.004	-0.004	-0.006*	
		(0.004)	(0.003)	(0.003)	(0.003)	
Firm ROA		0.122***	0.120***	0.117***	0.110***	
		(0.033)	(0.034)	(0.031)	(0.020)	
Firm Market/Book		0.001	0.001	0.001	0.005**	
		(0.003)	(0.002)	(0.003)	(0.002)	
Firm Size		0.045***	0.044***	0.050***	0.034***	
		(0.008)	(0.008)	(0.005)	(0.004)	
Worker Tenure			0.010**	0.010***	0.011***	0.017***
			(0.004)	(0.003)	(0.003)	(0.004)
Event-Firm-Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-Year FE	Yes	Yes				
Event-Education-Year FE			Yes	Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes	Yes
Event-Minority-Year FE			Yes	Yes	Yes	Yes
Event-State-Year FE				Yes	Yes	Yes
Event-Industry-Year FE					Yes	
Event-Firm-Year FE						Yes
Observations	3669000	3669000	3669000	3669000	3669000	3669000
R-squared	0.91	0.911	0.911	0.913	0.915	0.917

Table 5
Access to Debt and Earnings Changes Across Races
This table reports the change in the minority earnings gap using a triple Diff-in-Diff model with different time-varying controls and fixed effects, where the dependent variable is *Log(Earnings)*, the log of quarterly earnings (in $2018) averaged within a given year for each worker. *Asian* is an indicator for Asian, *Black* is an indicator for African American workers, and *Other Minority* includes all other minority groups. *Treat* is an indicator for workers working for parent companies incorporated in LA (1997), TX (1997), and AL (2001). The control group includes employees working for parent companies that never experienced ARL, and in the same sector (NAICS2) and quintile of employment size bin with the parent companies of the treated workers. *Post* is an indicator for periods post the ARL. *Treat* and its interactions with *Asian*, *Black*, *Other Minority* are absorbed by Event-Firm-Worker fixed effects. *Post×Asian*, *Post×Black*, and *Post×Other Minority* are estimated but not reported for brevity. *Firm Char* include *Firm Age*, *Firm ROA*, *Firm Market/Book*, and *Firm Size*. Standard errors are clustered by workers' state and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Dep. Var.: *Log(Earnings)*	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	0.038	0.017	0.013	0.011	0.034*	
	(0.023)	(0.023)	(0.025)	(0.019)	(0.016)	
Treat×Post×Asian	0.061***	0.054***	0.051***	0.044***	0.044***	0.025***
	(0.014)	(0.015)	(0.015)	(0.013)	(0.009)	(0.008)
Treat×Post×Black	0.059***	0.055***	0.051***	0.047***	0.051***	0.043***
	(0.012)	(0.012)	(0.015)	(0.014)	(0.014)	(0.011)
Treat×Post×Other Minority	0.029**	0.027*	0.024*	0.014	0.015	0.009
	(0.013)	(0.013)	(0.013)	(0.011)	(0.011)	(0.010)
Event-Firm-Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-Year FE	Yes	Yes				
Firm Char		Yes	Yes	Yes	Yes	
Worker Tenure			Yes	Yes	Yes	Yes
Event-Education-Year FE			Yes	Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes	Yes
Event-Minority-Year FE			Yes	Yes	Yes	Yes
Event-State-Year FE				Yes	Yes	Yes
Event-Industry-Year FE					Yes	
Event-Firm-Year FE						Yes
Observations	3669000	3669000	3669000	3669000	3669000	3669000
R-squared	0.91	0.911	0.911	0.913	0.915	0.917

Table 6
Access to Debt and Minority Workers' Pay Rank
This table examines changes in the pay rank gap between non-white and white workers within their employers following the adoption of anti-recharacterization laws. The dependent variable is *Pay Rank*, defined as 100*the rank of employee's average quarterly earnings within a given firm-year divided by the number of employees of a given firm-year observed in the sample. Panel A reports the results on the average difference between the pay rank of minority and white workers. Panel B provides results on how racial pay rank gap changes around the adoption of the anti-recharacterization laws. *Minority* is a dummy variable for all workers that are non-white. *Worker Tenure* is a worker's total work tenure with a given employer. *Firm Char* include *Firm Age*, *Firm ROA*, *Firm Market/Book*, and *Firm Size*. Standard errors are clustered by workers' state and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Racial Pay Rank Gap Inside Firms

Dep. Var.: *Pay Rank*	(1)	(2)	(3)	(4)	(5)	(6)
Minority	-3.357***	-3.359***	-2.621***	-2.305***	-2.330***	-2.331***
	(0.837)	(0.838)	(0.723)	(0.450)	(0.451)	(0.433)
Event-Firm FE		Yes	Yes	Yes	Yes	
Event-Year FE		Yes				
Event-Education-Year FE			Yes	Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes	Yes
Event-Minority-Year FE			Yes	Yes	Yes	Yes
Event-State-Year FE				Yes	Yes	Yes
Event-Industry-Year FE					Yes	
Event-Firm-Year FE						Yes
Observations	3669000	3669000	3669000	3669000	3669000	3669000
R-squared	0.321	0.321	0.391	0.426	0.436	0.456

Panel B: Changes in Racial Pay Rank Gap Around ARL

Dep. Var.: *Pay Rank*	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	-2.529	-2.485	-2.728	-0.903	0.713	
	(1.491)	(1.705)	(1.690)	(1.305)	(1.020)	
Treat×Post×Minority	1.962**	2.028**	2.640***	2.236**	1.946***	1.453**
	(0.795)	(0.780)	(0.935)	(0.836)	(0.586)	(0.524)
Event-Firm-Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-Year FE	Yes	Yes				
Event-Education-Year FE			Yes	Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes	Yes
Event-Minority-Year FE			Yes	Yes	Yes	Yes
Event-State-Year FE				Yes	Yes	Yes
Event-Industry-Year FE					Yes	
Event-Firm-Year FE						Yes
Observations	3669000	3669000	3669000	3669000	3669000	3669000
R-squared	0.862	0.862	0.863	0.878	0.886	0.903

Table 7

Racial Gap in Worker Positions

This table reports the differences in job positions between minority and white workers. We examine the following variables: the likelihood that a worker obtains a new position in the following year, i.e., *New Position*; the likelihood that a worker is promoted the next year, i.e., *Promotion*, which is defined as an indicator for whether a worker changing to a new position with a higher salary; whether a worker is promoted to a new position in the same three-digit ONET code, i.e., *Promotion Within Occ*; and the likelihood that a worker changes to a high-technology occupation, i.e., *Change to HiTech*. *Minority* is an indicator for whether a worker is a non-white ethnicity. Occupation is defined based on three-digit ONET code. Control variables are defined in the same way as in Table 4. Standard errors are clustered by workers' state and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Dep. Var.:	*New Position*		*Promotion*		*Promotion Within Occ*		*Change to HiTech*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Minority	-0.526***	-0.486***	-0.321***	-0.340***	-0.192***	-0.170***	-0.029***	-0.019***
	(0.058)	(0.044)	(0.048)	(0.038)	(0.025)	(0.021)	(0.006)	(0.003)
Event-Education-Year FE		Yes		Yes		Yes		Yes
Event-Gender-Year FE		Yes		Yes		Yes		Yes
Event-Minority-Year FE		Yes		Yes		Yes		Yes
Event-Occupation-Year FE		Yes		Yes		Yes		Yes
Event-State-Year FE		Yes		Yes		Yes		Yes
Event-Firm-Year FE		Yes		Yes		Yes		Yes
Firm Char		Yes		Yes		Yes		Yes
Worker Tenure		Yes		Yes		Yes		Yes
Observations	34,122,764	33,895,029	34,122,764	33,895,029	34,122,764	33,895,029	34,122,764	33,895,029
R-squared	0.0116	0.0220	0.0092	0.0187	0.0065	0.0154	0.0010	0.0062

Table 8

Changes in Worker Career

This table reports the differential changes in workers' career between minority and white workers, including having a new position, promotion, promotion within the occupation category, and changing to high-tech positions, following the passage of anti-recharacterization laws. In Panel A, we examine the likelihood that a worker obtains a new position in the following year, i.e., *New Position*. In Panel B, we examine promotion rates, i.e., *Promotion*, defined as a worker changing to a new position with a higher salary. In Panel C, we further examine whether a worker is promoted to a new position in the same firm and with the same three-digit ONET code, i.e., *Promotion Within Occ*. Finally, in Panel D, we examine the likelihood that a worker changes to a high-technology occupation, i.e., *Change to HighTech*. All outcome variables are multiplied by 100. *Treat* is an indicator for workers working for parent companies that incorporated in LA (1997), TX (1997), and AL (2001),. The control group includes employees working for parent companies that never experienced ARL, and in the same sector (NAICS2) and quintile of employment size bin with the parent companies of the treated workers. *Post* is an indicator for periods post the ARL. *Minority* is an indicator for whether a worker is a non-white ethnicity. Occupation is defined based on three-digit ONET code. Control variables are defined in the same way as in Table 4. Standard errors are clustered by workers' state and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Having a New Position

Dep. Var.: *New Position* (%)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat×*Post*×*Minority*	0.658***	0.66***	0.276**	0.246**	0.245**	0.222**	0.196*
	(0.144)	(0.14)	(0.112)	(0.104)	(0.104)	(0.098)	(0.102)
Treat×*Post*	-0.574***	-0.603**	-0.35*	-0.372**	-0.294	-0.324*	
	(0.208)	(0.231)	(0.207)	(0.183)	(0.199)	(0.181)	
Treat×*Minority*	-0.276**	-0.278**	0.156	0.109	0.137	0.086	0.108
	(0.129)	(0.128)	(0.104)	(0.107)	(0.102)	(0.104)	(0.103)
Minority×*Post*	-0.550***	-0.555***					
	(0.058)	(0.058)					
Event-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	
Event-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	
Event-Education-Year FE			Yes	Yes	Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes	Yes	Yes
Event-Minority-Year FE			Yes	Yes	Yes	Yes	Yes
Event-Occupation-Year FE				Yes		Yes	Yes
Event-State-Year FE					Yes	Yes	Yes
Event-Firm-Year FE							Yes
Firm Char	Yes	Yes	Yes	Yes	Yes		
Worker Tenure		Yes	Yes	Yes	Yes	Yes	Yes
Observations	34,122,764	33,895,321	33,895,321	33,895,306	33,895,314	33,895,299	33,895,029
R-squared	0.0116	0.0116	0.0165	0.0189	0.0176	0.0199	0.0220

Panel B: Promotion

Dep. Var.: *Promotion* (%)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat×*Post*×*Minority*	0.539***	0.564***	0.306***	0.271**	0.254**	0.226**	0.195*
	(0.148)	(0.147)	(0.11)	(0.108)	(0.106)	(0.103)	(0.107)
Treat×*Post*	-0.38**	-0.451***	-0.262*	-0.264**	-0.243*	-0.252**	
	(0.151)	(0.166)	(0.139)	(0.12)	(0.134)	(0.118)	
Treat×*Minority*	-0.287**	-0.31**	0.01	-0.032	0.019	-0.026	0.000
	(0.125)	(0.128)	(0.105)	(0.108)	(0.103)	(0.107)	(0.11)
Minority×*Post*	-0.337***	-0.34***					
	(0.049)	(0.049)					
Observations	34,122,764	33,895,321	33,895,321	33,895,306	33,895,314	33,895,299	33,895,029
R-squared	0.0092	0.0092	0.0138	0.0155	0.0147	0.0163	0.0187

Panel C: Promotion Within Occupation Category

Dep. Var.: *Promotion Within Occ* (%)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat×*Post*×*Minority*	0.309***	0.321***	0.195***	0.166***	0.177***	0.153**	0.153**
	(0.065)	(0.065)	(0.067)	(0.061)	(0.065)	(0.061)	(0.066)
Treat×*Post*	-0.397***	-0.396***	-0.268***	-0.219***	-0.239***	-0.195***	
	(0.081)	(0.09)	(0.077)	(0.058)	(0.064)	(0.05)	
Treat×*Minority*	-0.145***	-0.155***	-0.008	-0.021	-0.014	-0.031	-0.025
	(0.052)	(0.052)	(0.06)	(0.063)	(0.057)	(0.06)	(0.061)
Minority×*Post*	-0.201***	-0.203***					
	(0.026)	(0.026)					
Observations	34,122,764	33,895,321	33,895,321	33,895,306	33,895,314	33,895,299	33,895,029
R-squared	0.0065	0.0065	0.0097	0.0134	0.0103	0.0140	0.0154
Event-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	
Event-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	
Event-Education-Year FE			Yes	Yes	Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes	Yes	Yes
Event-Minority-Year FE			Yes	Yes	Yes	Yes	Yes
Event-Occupation-Year FE				Yes		Yes	Yes
Event-State-Year FE					Yes	Yes	Yes
Event-Firm-Year FE							Yes
Firm Char	Yes	Yes	Yes	Yes	Yes		
Worker Tenure		Yes	Yes	Yes	Yes	Yes	Yes

Panel D: Change to High-Tech Positions

Dep. Var.: *Change to HighTech* (%)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Treat \times Post \times Minority$	0.069***	0.062***	0.052**	0.049**	0.049**	0.047**	0.040*
	(0.021)	(0.021)	(0.02)	(0.021)	(0.019)	(0.02)	(0.02)
$Treat \times Post$	-0.029*	-0.024	-0.014	-0.047**	-0.013	-0.047**	
	(0.016)	(0.017)	(0.017)	(0.021)	(0.014)	(0.018)	
$Treat \times Minority$	-0.022	-0.016	-0.004	0.001	-0.007	-0.002	0.004
	(0.018)	(0.018)	(0.02)	(0.019)	(0.02)	(0.019)	(0.02)
$Minority \times Post$	-0.032***	-0.033***					
	(0.006)	(0.006)					
Event-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	
Event-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	
Event-Education-Year FE			Yes	Yes	Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes	Yes	Yes
Event-Minority-Year FE			Yes	Yes	Yes	Yes	Yes
Event-Occupation-Year FE				Yes		Yes	Yes
Event-State-Year FE					Yes	Yes	Yes
Event-Firm-Year FE							Yes
Firm Char	Yes	Yes	Yes	Yes	Yes		
Worker Tenure		Yes	Yes	Yes	Yes	Yes	Yes
Observations	34,122,764	33,895,321	33,895,321	33,895,306	33,895,314	33,895,299	33,895,029
R-squared	0.0010	0.0010	0.0013	0.0046	0.0015	0.0048	0.0062

Table 9
Heterogeneity of Effects: Pre-event Inequality
This table reports the change in the earnings gap between minority and white workers by pre-existing inequality at the firm level. The dependent variable is *Log(Earnings)*, the log of quarterly earnings (in $2018) averaged within a given year for each worker. In Columns (1) through (3), we partition the sample based on the pre-event earnings gap between white and minority workers. It is computed as the average earnings by white workers minus the average earnings of minority workers during the year prior to the event. Higher values indicate larger inequality. In Columns (4) through (6), we partition the sample based on board diversity, with higher values indicating more diverse boards. Board diversity is measured based on the demographics of board of directors in the year prior to the event. It is a linear combination of standardized value of share of female directors, standard deviation of director ages, and the reversed HHI in ethnicity. *High*, *Medium*, and *Low* represent tercile indicators of pre-event pay inequality or board diversity. *Treat* is an indicator for workers working for parent companies incorporated in LA (1997), TX (1997), and AL (2001). The control group includes employees working for parent companies that never experienced ARL, and in the same sector (NAICS2) and quintile of employment size bin with the parent companies of the treated workers. *Post* is an indicator for periods post the ARL. Control variables are defined in the same way as in Table 4. Standard errors are clustered by workers' state and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Partitioning Var:	Pre-event Earnings Gap			Board Diversity		
Dep. Var.: *Log(Earnings)*	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	0.024	0.011		0.126***	0.083**	
	(0.030)	(0.032)		(0.024)	(0.031)	
Treat×Post×Low	0.023	0.027	0.029	-0.080*	-0.044	
	(0.035)	(0.028)	(0.017)	(0.042)	(0.054)	
Treat×Post×Medium	0.042	0.048	0.052**	-0.154***	-0.110***	
	(0.031)	(0.032)	(0.021)	(0.031)	(0.030)	
Treat×Post×Minority×Low	0.019	0.007	-0.006	0.059***	0.054***	0.045***
	(0.038)	(0.026)	(0.031)	(0.019)	(0.017)	(0.014)
Treat×Post×Minority×Medium	0.012	-0.01	-0.004	0.038***	0.024*	0.019*
	(0.026)	(0.022)	(0.019)	(0.010)	(0.013)	(0.010)
Treat×Post×Minority×High	0.063***	0.060***	0.051***	-0.045	-0.047	-0.046
	(0.020)	(0.018)	(0.012)	(0.029)	(0.029)	(0.030)
Event-Firm-Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-Year FE	Yes			Yes		
Firm Controls		Yes			Yes	
Worker Tenure		Yes	Yes		Yes	Yes
Event-Education-Year FE		Yes	Yes		Yes	Yes
Event-Gender-Year FE		Yes	Yes		Yes	Yes
Event-Minority-Year FE		Yes	Yes		Yes	Yes
Event-State-Year FE		Yes	Yes		Yes	Yes
Event-Industry-Year FE		Yes	Yes		Yes	
Event-Firm-Year FE			Yes			Yes
Observations	3669000	3669000	3669000	3669000	3669000	3669000
R-squared	0.909	0.914	0.916	0.909	0.914	0.916

Table 10
Access to Debt and Job Separation Rates between Minority and White Workers
This table reports the differences in the separation rates between non-white and white workers and how the racial separation gap changes around the adoption of anti-recharacterization laws. The dependent variable is *Separation*, an indicator equal to one for worker-years if in the next year the worker is separated from the current employer. Panel A presents results for the average difference in the separation rates between minority and white workers, while Panel B presents results for how the racial separate rate gap changes around anti-recharacterization laws. *Minority* is a dummy variable for all workers that are non-white, 0 otherwise. *Worker Tenure* is a worker's total work tenure with a given employer. *Firm Char* include *Firm Age*, *Firm ROA*, *Firm Market/Book*, and *Firm Size*. Standard errors are clustered by workers' state and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Racial Gap in Separation Rates

Dep. Var.: *Separation*	(1)	(2)	(3)	(4)	(5)	(6)
Minority	0.031***	0.031***	0.021***	0.022***	0.022***	0.022***
	(0.007)	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)
Event-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-Year FE	Yes	Yes				
Firm Char		Yes	Yes	Yes	Yes	
Worker Tenure			Yes	Yes	Yes	Yes
Event-Education-Year FE			Yes	Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes	Yes
Event-State-Year FE				Yes	Yes	Yes
Event-Industry-Year FE					Yes	
Event-Firm-Year FE						Yes
Observations	15770000	15770000	15770000	15770000	15770000	15770000
R-squared	0.057	0.058	0.058	0.089	0.112	0.144

Panel B: Changes in Separation Rate Racial Gap Around ARLs

Dep. Var.: *Separation*	(1)	(2)	(3)	(4)	(5)	(6)
Treat× Post	-0.179***	-0.165***	-0.179***	-0.184***	-0.031	
	(0.060)	(0.058)	(0.058)	(0.030)	(0.037)	
Treat× Post× Minority	-0.019	-0.014	-0.024	-0.032	-0.013	0.002
	(0.027)	(0.026)	(0.030)	(0.018)	(0.021)	(0.008)
Event-Firm-Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Char		Yes	Yes	Yes	Yes	
Worker Tenure			Yes	Yes	Yes	Yes
Event-Education-Year FE			Yes	Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes	Yes
Event-Minority-Year FE			Yes	Yes	Yes	Yes
Event-State-Year FE				Yes	Yes	Yes
Event-Industry-Year FE					Yes	
Event-Firm-Year FE						Yes
Observations	15770000	15770000	15770000	15770000	15770000	15770000
R-squared	0.516	0.517	0.524	0.536	0.559	0.588

Table 11
Access to Debt and New Hire Pay Gap between Minority and White Workers
This table reports the differences in the new hire workers' earnings between white and non-white workers and how such earnings gap varies around the adoption of anti-recharacterization laws. The dependent variable is *Log(Earnings)*, the average quarterly earnings in a year (in $2018) for newly hired workers. The sample includes only new hires, defined as workers whose tenure is one. Panel A presents results for the average difference in the new hire earnings between minority and white workers, while Panel B presents results for how the racial earnings gap for newly hired workers changes around anti-recharacterization laws. *Minority* is an indicator variable for all non-white workers. *Firm Char* include *Firm Age*, *Firm ROA*, *Firm Market/Book*, and *Firm Size*. Standard errors are clustered by workers' state and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Racial Gap in New Hire Earnings

Dep. Var.: *Log(Earnings)*	(1)	(2)	(3)	(4)	(5)
Minority	-0.074***	-0.074***	-0.075***	-0.074***	-0.068***
	(0.011)	(0.011)	(0.007)	(0.006)	(0.006)
Event-Firm FE	Yes	Yes	Yes	Yes	
Event-Year FE	Yes	Yes	Yes	Yes	
Firm Char		Yes	Yes	Yes	
Event-Education-Year FE			Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes
Event-State-Year FE			Yes	Yes	Yes
Event-Industry-Year FE				Yes	
Event-Firm-Year FE					Yes
Observations	4777000	4777000	4777000	4777000	4777000
R-squared	0.398	0.398	0.448	0.458	0.475

Panel B: Changes in New Hire Earnings Gap Around ARLs

Dep. Var.: *Log(Earnings)*	(1)	(2)	(3)	(4)	(5)
Treat×Post	-0.091	-0.092	-0.078	-0.031	
	(0.066)	(0.067)	(0.052)	(0.049)	
Treat×Post×Minority	0.083**	0.083**	0.062**	0.063***	0.062**
	(0.040)	(0.039)	(0.028)	(0.022)	(0.023)
Event-Firm FE	Yes	Yes	Yes	Yes	
Event-Year FE	Yes	Yes	Yes	Yes	
Firm Char		Yes	Yes	Yes	
Event-Education-Year FE			Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes
Event-Minority-Year FE			Yes	Yes	Yes
Event-State-Year FE			Yes	Yes	Yes
Event-Industry-Year FE			Yes	Yes	
Event-Firm-Year FE					Yes
Observations	4777000	4777000	4777000	4777000	4777000
R-squared	0.398	0.399	0.448	0.459	0.475

Table 12
Long Run Effects of Access to Debt on Racial Earnings Gap
This table reports the change in the minority earnings gap post-treatment using the triple Diff-in-Diff model with different time-varying controls and fixed effects. The dependent variable is *Log(Earnings)*, the average quarterly earnings in a year (in \$2018) for workers. The sample includes workers' full employment records, including years at the treated and control companies as well as years at other employers afterwards. *Minority* is an indicator variable for all non-white workers. *Treat* is an indicator for workers working for parent companies incorporated in LA (1997), TX (1997), and AL (2001). The control group includes employees working for parent companies that never experienced ARL, and in the same sector (NAICS2) and quintile of employment size bin with the parent companies of the treated workers. *Post (Same Firm)* is an indicator for periods post the ARL but before the worker switched to another employer. *Post (Different Firm)* is an indicator for periods that the worker has moved to a new employer post the ARL. *Firm Char* include *Firm Age*, *Firm ROA*, *Firm Market/Book*, and *Firm Size*. Standard errors are clustered by workers' state and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Dep. Var.: *Log(Earnings)*	(1)	(2)	(3)	(4)
Treat×Post (Same Firm)	0.028	0.026	-0.006	0.002
	(0.017)	(0.018)	(0.023)	(0.012)
Treat×Post (Different Firm)	-0.048**	-0.044**	-0.057**	-0.062***
	(0.021)	(0.021)	(0.021)	(0.014)
Treat×Post (Same Firm)×Minority	0.048***	0.049***	0.039***	0.030**
	(0.008)	(0.009)	(0.010)	(0.012)
Treat×Post (Different Firm)×Minority	0.043**	0.045**	0.034*	0.032*
	(0.019)	(0.019)	(0.018)	(0.018)
Firm-Worker FE	Yes	Yes	Yes	Yes
Event-Year FE	Yes	Yes		
Firm Char		Yes	Yes	Yes
Worker Tenure			Yes	Yes
Event-Education-Year FE			Yes	Yes
Event-Gender-Year FE			Yes	Yes
Event-Minority-Year FE			Yes	Yes
Event-State-Year FE				Yes
P-value (Same $\neq$ Different Firm)	0.80	0.83	0.80	0.93
Observations	6538000	6538000	6538000	6538000
R-squared	0.79	0.791	0.796	0.798

Table 13
Firms With and Without SPVs
This table reports the change in earnings gap between minority and white workers for firms with and without SPVs. The dependent variable is *Log(Earnings)*, the log of quarterly earnings (in $2018) averaged within a given year for each worker. *Has SPV* (*No SPV*) is an indicator for whether a firm discloses at least one subsidiary (no subsidiary) in its 10-Ks in a given year. *Treat* is an indicator for workers working for parent companies incorporated in LA (1997), TX (1997), and AL (2001). The control group includes employees working for parent companies that never experienced ARL, and in the same sector (NAICS2) and quintile of employment size bin with the parent companies of the treated workers. *Post* is an indicator for periods post the ARL. Control variables are defined in the same way as in Table 4. Standard errors are clustered by workers' state and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Dep. Var.: *Log(Earnings)*	(1)	(2)	(3)
Treat×*Post*	0.057**	0.053**	
	(0.025)	(0.019)	
Treat×*Post*×*No SPV*	-0.084**	-0.048*	
	(0.036)	(0.026)	
Treat×*Post*×*Minority*×*No SPV*	0.016	-0.009	-0.004
	(0.018)	(0.014)	(0.015)
Treat×*Post*×*Minority*×*Has SPV*	0.055***	0.045***	0.033***
	(0.012)	(0.012)	(0.010)
Event-Firm-Worker FE	Yes	Yes	Yes
Event-Year FE	Yes		
Firm Controls		Yes	
Worker Tenure		Yes	Yes
Event-Education-Year FE		Yes	Yes
Event-Gender-Year FE		Yes	Yes
Event-Minority-Year FE		Yes	Yes
Event-State-Year FE		Yes	Yes
Event-Industry-Year FE		Yes	Yes
Event-Firm-Year FE			Yes
Observations	3668504	3668504	3668504
R-squared	0.909	0.914	0.916

Table 14
Robustness Checks
This table reports results from robustness checks regarding our empirical specification. In Panel A, we re-estimate Equation (2) by assigning treated firms to be un-treated after 2003. *Treat (on-off)* is an indicator that turns to one for individuals working in companies that are incorporated in LA (1997), TX (1997), and AL (2001), after those states passed the anti-recharacterization laws, but no later than 2003. This indicator turns to zero for treated individuals in years after 2003, and also for control observations. In Panel B, we cluster standard errors by firms' state of incorporation. In Panel C, we use as dependent variable the highest quarterly earnings that a worker makes in a given year. In Panel D, we report results from Poisson regressions without transforming the dependent variable in log terms. For all regressions, the control group includes employees working for parent companies that never experienced ARL, and in the same sector (NAICS2) and quintile of employment size bin with the parent companies of the treated workers. Post is an indicator for periods post the ARL. The dependent variable is *Log(Earnings)* in Panels A through C, and is the level of earnings without log transformation in Panel D. *Minority* is an indicator variable for all non-white workers. *Post×Minority* is estimated but not reported for brevity in specifications without Event-Minority-Year fixed effects, and is absorbed when Event-Minority-Year fixed effects are included. *Firm Char* include *Firm Age*, *Firm ROA*, *Firm Market/Book*, and *Firm Size*. Standard errors are clustered by workers' state and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Excluding Post-2003 Years

Dep. Var.: *Log(Earnings)*	(1)	(2)	(3)	(4)	(5)	(6)
Treat (on-off)	-0.173***	-0.148***	-0.141***	-0.141***	-0.087**	
	(0.033)	(0.034)	(0.035)	(0.021)	(0.034)	
Treat (on-off)×Post	-0.016	-0.032	-0.033	-0.026	0.027*	
	(0.028)	(0.026)	(0.028)	(0.021)	(0.015)	
Treat (on-off)×Minority	-0.100***	-0.096***	-0.099***	-0.091***	-0.092***	-0.074***
	(0.023)	(0.023)	(0.023)	(0.016)	(0.021)	(0.023)
Treat (on-off)×Post×Minority	0.034***	0.030**	0.026*	0.018	0.023**	0.016*
	(0.012)	(0.012)	(0.013)	(0.012)	(0.011)	(0.009)
Event-Firm-Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-Year FE	Yes	Yes				
Firm Char		Yes	Yes	Yes	Yes	
Worker Tenure			Yes	Yes	Yes	Yes
Event-Education-Year FE			Yes	Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes	Yes
Event-Minority-Year FE			Yes	Yes	Yes	Yes
Event-State-Year FE				Yes	Yes	Yes
Event-Industry-Year FE					Yes	
Event-Firm-Year FE						Yes
Observations	3669000	3669000	3669000	3669000	3669000	3669000
R-squared	0.911	0.911	0.912	0.913	0.915	0.918

Panel B: Clustering by States of Incorporation

Dep. Var.: *Log(Earnings)*	(1)	(2)	(3)	(4)	(5)	(6)
Treat× Post	0.038***	0.017	0.013	0.011	0.033**	
	(0.010)	(0.017)	(0.017)	(0.014)	(0.014)	
Treat× Post× Minority	0.052***	0.048***	0.045***	0.039***	0.042***	0.032***
	(0.010)	(0.011)	(0.011)	(0.010)	(0.011)	(0.010)
Event-Firm-Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-Year FE	Yes	Yes				
Firm Char		Yes	Yes	Yes	Yes	
Worker Tenure			Yes	Yes	Yes	Yes
Event-Education-Year FE			Yes	Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes	Yes
Event-Minority-Year FE			Yes	Yes	Yes	Yes
Event-State-Year FE				Yes	Yes	Yes
Event-Industry-Year FE					Yes	
Event-Firm-Year FE						Yes
Observations	3669000	3669000	3669000	3669000	3669000	3669000
R-squared	0.91	0.911	0.911	0.913	0.915	0.917

Panel C: Using Highest Quarter Earnings

Dep. Var: *Log(Earnings)* (Highest)	(1)	(2)	(3)	(4)	(5)	(6)
Treat× Post	0.065***	0.040*	0.035	0.023	0.033*	
	(0.018)	(0.021)	(0.021)	(0.017)	(0.018)	
Treat× Post× Minority	0.037***	0.031**	0.032**	0.029**	0.032**	0.021**
	(0.012)	(0.012)	(0.013)	(0.012)	(0.012)	(0.010)
Event-Firm-Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-Year FE	Yes	Yes				
Firm Char		Yes	Yes	Yes	Yes	
Worker Tenure			Yes	Yes	Yes	Yes
Event-Education-Year FE			Yes	Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes	Yes
Event-Minority-Year FE			Yes	Yes	Yes	Yes
Event-State-Year FE				Yes	Yes	Yes
Event-Industry-Year FE					Yes	
Event-Firm-Year FE						Yes
Observations	3669000	3669000	3669000	3669000	3669000	3669000
R-squared	0.9	0.9	0.901	0.903	0.906	0.91

Panel D: Poisson Regressions Without Log Transformation

Dep. Var.: *Earnings*	(1)	(2)	(3)	(4)	(5)	(6)
Treat× Post	0.058***	0.033*	0.033	0.028*	0.043**	
	(0.018)	(0.020)	(0.021)	(0.015)	(0.019)	
Treat× Post× Minority	0.066***	0.061***	0.059***	0.054***	0.059***	0.046***
	(0.005)	(0.006)	(0.007)	(0.006)	(0.007)	(0.007)
Event-Firm-Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-Year FE	Yes	Yes				
Firm Char		Yes	Yes	Yes	Yes	
Worker Tenure			Yes	Yes	Yes	Yes
Event-Education-Year FE			Yes	Yes	Yes	Yes
Event-Gender-Year FE			Yes	Yes	Yes	Yes
Event-Minority-Year FE			Yes	Yes	Yes	Yes
Event-State-Year FE				Yes	Yes	Yes
Event-Industry-Year FE					Yes	
Event-Firm-Year FE						Yes
Observations	3669000	3669000	3669000	3669000	3669000	3669000

Appendix A Variable Definitions

- *Log(Earnings)*: The log of the average quarterly earnings (in 2018Q3 dollars) across quarters within a year for a given worker. Source: LEHD
- *Log(Earnings) (Highest)*: The log of the highest quarterly earnings (in 2018Q3 dollars) for a given worker-year. Source: LEHD
- *Earnings*: The quarterly earnings (in 2018Q3 dollars) averaged across quarters within a given year for a given worker. Source: LEHD
- *Pay Rank*: The percentile of a worker's average quarterly earnings ranked within a firm-year. It equals 100×(the rank of employee's *Log(Earnings)* within a given firm (SEIN)-year divided by the number of employees of a given firm-year). Source: LEHD
- *Separation*: An indicator equal to one for worker-years if the worker separates with the current employer in the following year, and zero otherwise. Source: LEHD
- *New Position*: An indicator that equals one if a worker obtains a new position in the following year, and zero otherwise. This indicator is multiplied by 100.
- *Promotion*: An indicator that equals one if a worker changes to a new position with a higher salary next year, and zero otherwise. This indicator is multiplied by 100.
- *Promotion Within Occ*: An indicator that equals one if a worker changes to a new position with a higher salary in the same firm and the same three-digit ONET code next year, and zero otherwise. This indicator is multiplied by 100.
- *Change to HiTech*: An indicator that equals one if a worker changes to a high-technology occupation next year, and zero otherwise. This indicator is multiplied by 100.
- *Minority*: An indicator equal to one for non-white workers, and zero otherwise. Source: LEHD
- *Black*: An indicator equal to one if a worker's reported ethnicity is Black or African-American, and zero otherwise. Source: LEHD
- *Asian*: An indicator equal to one if a worker's reported ethnicity is Asian, and zero otherwise. Source: LEHD
- *Other Minority*: An indicator equal to one if a worker's reported ethnicity is American Indian, Alaska Native, Native Hawaiian, Other Pacific Islander workers, or workers with two or more race groups, and zero otherwise. Source: LEHD
- *Treat*: An indicator equal to one for workers working for parent companies incorporated in LA (1997), TX (1997), and AL (2001), and zero for individuals working in control firms. Source: LEHD and Compustat
- *Post*: An indicator equal to one for periods post the treatment (passage of the ARL in a given incorporation state), 0 otherwise. Source: LEHD and Compustat
- *Treat (on-off)*: An indicator equal to one for employment years before (including) 2003 of individuals working for parent companies that incorporated in AL (2001), TX (1997), and LA (1997), and zero for all other observations of those workers as well as for workers in the control group. Source: LEHD and Compustat
- *Skill*: Measured by the average quarterly earnings (*Log(Earnings)*) of a worker during the year prior to the event.
- *High Skill*: An indicator equal to one if a worker's *Log(Earnings)* in the year prior to the

event year is in the top tercile of the sample distribution.

- *Mid Skill*: An indicator equal to one if a worker's *Log(Earnings)* in the year prior to the event year is in the middle tercile of the sample distribution.
- *Labor HHI*: Labor market concentration defined by Herfindahl index using employers' (SEIN) labor shares within a given commuting zone. Source: LEHD
- *Low Labor HHI*: An indicator equal to one for workers located in commuting zones where the labor market concentration is ranked in the bottom tercile during the year prior to the event year, and zero otherwise.
- *Mid Labor HHI*: An indicator equal to one for workers located in commuting zones where the labor market concentration is ranked in the middle tercile during the year prior to the event year, and zero otherwise.
- *Male*: An indicator equal to one for male workers, and zero otherwise. Source: LEHD
- *Worker Tenure*: The number of years of a given worker has worked for a firm. Source: LEHD
- *Firm Age*: Age of a firm, defined as the difference between the current year and the first year the firm is observed in LBD with positive employment. Source: LBD
- *Firm ROA*: Return on asset, defined as net income scaled by total assets. Source: Compustat
- *Firm Market/Book*: Source: Compustat
- *Firm Size*: The log of total assets. Source: Compustat

Appendix B Additional Analyses

Table B.1
The Role of Labor Market Competition
This table reports the change in the earnings gap between minority and white workers by the degree of labor market concentration. The dependent variable is *Log(Earnings)*, the log of quarterly earnings (in $2018) averaged within a given year for each worker. In Columns (1) and (2), labor market concentration is defined by the Herfindahl index across all workers in a commuting zone. In Columns (3) and (4), labor market concentration is defined by the Herfindahl index across only white workers in a commuting zone, and in Columns (5) and (6), concentration is defined according to minority workers. *High*, *Medium*, and *Low* represents tercile indicators of labor market concentration index. *Treat* is an indicator for workers working for parent companies that incorporated in LA (1997), TX (1997), and AL (2001). The control group includes employees working for parent companies that never experienced ARL, and in the same sector (NAICS2) and quintile of employment size bin with the parent companies of the treated workers. *Post* is an indicator for periods post the ARL. Control variables are defined in the same way as in Table 4. Standard errors are clustered by workers' state and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Partitioning Var:	HHI (All Worker)		HHI (White)		HHI (Minority)	
Dep. Var.: *Log(Earnings)*	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	0.033		0.038		0.033	
	(0.029)		(0.033)		(0.029)	
Treat×Post×Low	0.027	0.018	0.022	0.018	0.027	0.019
	(0.037)	(0.018)	(0.041)	(0.018)	(0.037)	(0.017)
Treat×Post×Medium	0.007	0	0	0	0.005	0.003
	(0.039)	(0.016)	(0.039)	(0.015)	(0.039)	(0.016)
Treat×Post×Minority×Low	-0.001	-0.009	-0.001	-0.009	-0.001	-0.009
	(0.009)	(0.017)	(0.009)	(0.017)	(0.009)	(0.017)
Treat×Post×Minority×Medium	0.071***	0.052***	0.066***	0.051***	0.067***	0.050***
	(0.015)	(0.011)	(0.015)	(0.010)	(0.014)	(0.010)
Treat×Post×Minority×High	0.034	-0.001	0.045*	0.001	0.043**	0.006
	(0.020)	(0.013)	(0.023)	(0.014)	(0.019)	(0.013)
Event-Firm-Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-Year FE	Yes		Yes		Yes	
Worker Tenure		Yes		Yes		Yes
Event-Education-Year FE		Yes		Yes		Yes
Event-Gender-Year FE		Yes		Yes		Yes
Event-Minority-Year FE		Yes		Yes		Yes
Event-State-Year FE		Yes		Yes		Yes
Event-Industry-Year FE		Yes		Yes		Yes
Event-Firm-Year FE		Yes		Yes		Yes
Observations	3669000	3669000	3669000	3669000	3669000	3669000
R-squared	0.909	0.916	0.909	0.916	0.909	0.916

Table B.2
Access to Debt, Minority Wage Gap, and Gender
This table reports the differential change in the earnings gap between minority and white workers by gender. The dependent variable is *Log(Earnings)*, the log of quarterly earnings (in $2018) averaged within a given year for each worker. *Treat* is an indicator for workers working for parent companies incorporated in LA (1997), TX (1997), and AL (2001). The control group includes employees working for parent companies that never experienced ARL, and in the same sector (NAICS2) and quintile of employment size bin with the parent companies of the treated workers. *Post* is an indicator for periods post the ARL. Control variables are defined in the same way as in Table 4. Standard errors are clustered by workers' state and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Dep. Var.: *Log(Earnings)*	(1)	(2)	(3)
Treat×Post×Female	-0.158***	-0.146***	-0.152***
	(0.047)	(0.044)	(0.042)
Treat×Post×Minority	0.028*	0.020*	0.007
	(0.015)	(0.011)	(0.012)
Treat×Post×Minority×Female	0.051	0.05	0.053
	(0.043)	(0.040)	(0.038)
Event-Firm-Worker FE	Yes	Yes	Yes
Event-Year FE	Yes		
Firm Controls		Yes	
Worker Tenure		Yes	Yes
Event-Education-Year FE		Yes	Yes
Event-Gender-Year FE		Yes	Yes
Event-Minority-Year FE		Yes	Yes
Event-State-Year FE		Yes	Yes
Event-Industry-Year FE		Yes	
Event-Firm-Year FE			Yes
Observations	3669000	3669000	3669000
R-squared	0.911	0.915	0.918