Access to Financing and Racial Pay Gap Inside Firms*

Janet Gao
Georgetown University
janet.gao@georgetown.edu

Wenting Ma
UMass Amherst
wentingma@umass.edu

Qiping Xu
UIUC
qipingxu@illinois.edu

Abstract

We examine how access to financing affects the racial pay gap inside U.S. firms using data from the Census Bureau and worker resumes. Exploiting exogenous shocks to firms' debt capacity, we find that better access to financing significantly narrows the earnings gap between minority and white workers, both in dollar amount and relative pay rank. The effect is stronger for mid- and high-skill workers and for firms with worse diversity practices ex ante. Following the shock, minority workers are more often promoted and reassigned to technology-oriented occupations. Taken together, access to finance makes 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 many 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). While the racial pay gap can be partially explained by worker-firm sorting (i.e., between-firm pay gap), a large fraction originates from firm-specific hiring and pay policies for different race groups (i.e., within-firm pay gap) (Altonji and Blank 1999; Heywood and Parent 2012; Gerard et al. 2021). According to academic and anecdotal evidence, various factors contribute to racial pay gap inside firms, including occupational segregation and stratification, lack of mentorship, promotion biases, etc.[1] All of these mechanisms suggest that firms generally under-utilize and under-invest in minority workers' human capital.

In discussing ways to address racial inequality, recent developments often center on firms' diversity and inclusion efforts while abstracting from the role of financial markets and firms' access to financing. Access to financing is shown to generate profound impacts on firms and workers. It not only affects total employment, but also leads to labor reallocation and shifts in wage distribution (Bai et al. 2018; Hombert and Matray 2017; Moser et al. 2018; Popov and Rocholl 2018). As firms have an easier time raising capital, they can expand the scope of production, pursue new investment opportunities, and adopt technologies, all of which lead to increased demand for human capital. In the phase of expansion and growth, firms may find it costlier to under-invest in minority workers and in response, allocate minority workers to more productive tasks and provide them with better career opportunities. At the same time, as firms raise more capital, the additional resources may be disproportionately captured by the incumbent majority, leading to an exacerbated pay gap.[2] Ultimately, the impact of financial

[1]Many studies, such as Elvira and Town (2001), Castilla (2008), Lahey and Oxley (2018), Miller and Schmutte (2021), Tripp and Fadlon (2020),Kline et al. (2022) explore within-firm frictions that contribute to racial income gaps, including biased performance evaluation, different promotion rates, social connections among workers and firms, or simply homophily. Other studies also document that substantial segregation and occupational stratification at the workplace, e.g., employers assign non-white workers to lower-paid or rank occupations, contributing to the racial pay gap inside firms (Penner, 2008; Giuliano et al., 2009; Åslund et al., 2014). Practitioner reports often cite worker-task sorting and the lack of mentorship as reasons for the unequal progression between White and non-White workers (Dobbin and Kalev, 2016; Smith, 2022). Ferreira and Pikulina (2022) build a theoretical framework that predicts the association between firm productivity, discrimination, and the incentives to invest in human capital by employees of different demographic groups.

[2]Such an effect has been documented related to the gender wage gap in the banking sector (Black and Strahan, 2001).

access on the racial pay gap remains an empirical question.

We answer this question by exploiting an exogenous shock that improved firms' access to debt financing and examining its impact on the racial income gap inside firms. Using the U.S. Census Bureau administrative data on worker earnings and a proprietary resume database on individual workers' career paths, we find that better access to financing reduces the racial pay gap inside firms. Importantly, following the shock, minority workers are more likely to be promoted to higher-paid positions and be reallocated to technology-oriented occupations.

Our granular datasets allow us to track workers' employment histories across employers along with their earnings and positions over time. The data also provide detailed information on individual demographics and work locations. With this information, we first document the presence of the earnings gap between white and non-white workers in U.S. public firms. Over our sample period, this earnings gap has risen from 11.6% to 19.72% and persists even when we control for important 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. Fixing the occupations, we observe that non-white workers are less likely to occupy a senior, high-paying position in the firm and less likely to be promoted to such positions than white workers. These differences could have contributed to the observed racial earnings gap.

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 were adopted in a staggered fashion in multiple U.S. states, and affected firms incorporated in those states while their workers often operate outside their states of incorporation.[3] Our method compares individuals with similar characteristics working in the same state whose employers face similar local labor market conditions but are incorporated in treated and control states. This allows us to isolate idiosyncratic, firm-level changes that affect

[3] 71% of publicly listed firms within Compustat operate their businesses outside their incorporation states during 1990-2017.

the racial pay gap and labor allocation inside firms.

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 base specifications include worker-by-firm fixed effects, which help rule out the possibility that the documented changes in the 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 incorporation states. These controls remove the confounding effects of local economic conditions, making it unlikely that our findings could be driven by differential changes in state labor laws or the labor supply between minority and white workers. In the strictest specification, we additionally control for firm-by-year 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.

Our results suggest that, following the passage of anti-recharacterization laws, the 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. Moreover, 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 gap 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 pay gap between white and minority workers does not change before the adoption of ARLs 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 of the pay gap.

Through what mechanisms does better access to financing reduce 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.[4] As workers accumulate task-specific human capital, skill-task matching affects compensation and could contribute to the pay gap (Acemoglu and Autor, 2011).

There are at least two channels through which the financing shocks we study could increase minority workers' pay. First, the shocks may increase the return to minority-dominated job categories, which tend to be lower-skill, lower-paid jobs. This mechanism suggests that, among minority workers, lower-skill individuals should be more affected by firms' access to financing. Second, the shocks may make firms better utilize and develop minority workers' human capital. Prior research documents that the adoption of ARLs leads firms to expand, adopt new technology, and innovate more (Ersahin, 2020; Mann, 2018). Affected firms may thus face an expansion of tasks and a higher demand for skilled labor. With a limited supply of skilled, experienced workers, firms may assign these tasks to minority employees, whose human capital was under-utilized. For example, firms can provide more mentorship and training to develop minority workers' human capital, promote them to more senior positions, or allocate them to other positions that better align their skills to tasks. Ultimately, these changes could contribute to an increase in minority workers' compensation. This mechanism suggests that skilled minority workers should be affected more by the shocks.

To distinguish these two mechanisms, 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, the racial pay gap decreases by 4% among high-skill workers but remains barely changed for low-skill ones. This evidence is consistent with the latter channel, which we now label as the "human capital utilization channel."

To further substantiate the human capital utilization channel, 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 de-

[4]The prior literature documents in many settings that non-white workers tend to occupy lower ranked positions compared to white workers, even if they possess similar skills. Golan et al. (2019) find that black workers are assigned to less complex tasks than white workers in their early careers, leading 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.

tailed description of a worker's career path, including her employer's name, location, job title, and estimated salary for each position. 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 they change 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 switches to a higher-paying position either through a promotion to a senior position or moving to other better-paid occupations. Third, we focus on the incidences in which a worker is promoted to higher-paying and more senior positions within the same occupation category. Finally, we investigate the likelihood that a worker is assigned to tech-orientated occupations, such as software developers and engineers, that are complementary to new technology and innovation. Specifically, we examine the job switches from non-tech to technology-oriented 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 tech-orientated occupations. However, these differences diminish 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 transition to a tech-oriented occupation. In sum, 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 pre-existing biases or frictions inside the firms prevent minority workers from matching to best-suited tasks. We verify this condition by examining the heterogeneity of our effects across firms that exhibited a higher or lower pay gap prior to the shocks. We find the reduction in the racial pay gap to be more pronounced at 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. [5]

[5] For example, based on Becker's theory, managers, including board members, tend to select workers based on their taste and discriminate against other races (Becker, 1971; Giuliano et al., 2009). When

Finally, we document that our effects become stronger in commuting zones with a lower employment share of white workers, where employers potentially face a shortage of white workers. In these labor markets, firms may have to rely more on minority workers when they have an elevated labor demand, especially for high-skill workers.

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. On average, workers are less likely to leave the company after the ARLs, suggesting that access to the debt market helps firms retain workers, but the changes in separation rates are not statistically different between white and non-white workers. We do, however, find a reduction in the pay gap between newly hired white and non-white workers post the adoption of ARLs by 6.8%. The effect on new-hire earnings indicates that, under the pressure of production and labor demand expansion, firms seek to attract more workers, including minority workers, by offering more equal pay.

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. At least two reasons could explain the persistent effects. First, the affected minority workers may be given better job opportunities or training in the affected firms, which permanently improves

managerial positions are predominately occupied by white workers, white workers are more likely to be hired or promoted than minority workers. Consistently, Bernile et al. (2018) shows that firms with more diverse boards are associated with a more diverse workforce.

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. and other economies (see Altonji and Blank (1999) for a review). Most 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) show that banking deregulation is followed by reduced racial inequality in a state.[6] Beck and Levkov (2010) document 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. Unlike 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

[6] The mechanisms documented in Levine et al. (2012) differ significantly from the ones in our study. Levine et al. (2012) find that banking deregulation leads to more firm entry, which strengthens local labor market competition and raises minority pay. Instead, we show that better access to financing improves the promotion opportunities and the skill-position matching for minority workers.

[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.

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 an 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 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.

However, 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 debtor's ownership 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 recharacterizating the asset sales between sponsor firms to their SPVs as loans. The adoption of those laws resulted from the lobby efforts from the financial industry (Kettering, 2010). 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

We compile our sample from various sources, including the Longitudinal Employment-Household Dynamics (LEHD) database, the Longitudinal Business Database (LBD), Revelio Labs and Compustat. We describe the key points of our data cleaning and sampling procedures, leaving more details to Appendix C.

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 (except for Maryland, which starts in 1985).

Within the LEHD program, we use data from the Employment History Files (EHF) to track workers' quarterly earnings, locations, and industries across employers, and use the National Individual Characteristics File (ICF) to identify worker demographic characteristics. Each worker is categorized 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, the

ICF also reports workers' birth years, gender, and education levels, which we use as controls.

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. 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. If a worker worked at different firms within that year, we keep the highest average earning (i.e., the best-paid job). 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

To identify the state of incorporation for workers' employers, we link the worker-year data constructed from LEHD files with the Compustat firm identifiers through two steps. In the first step, we link LEHD data with firm identifiers in the Census Bureau's LBD through the Business Register Bridge (BRB). Second, we link the LEHD-LBD matched sample with Compustat using the Compustat-SSEL Bridge (CSB) to obtain employers' gvkeys and their financial data. For each gvkey-year, we merge in their historical incorporation states obtained from the SEC Analytics Suite by WRDS.

We define several firm characteristics using the LBD data. First, firm age is defined as the current year minus the first year of observation from the oldest establishment with positive employment numbers owned by the firm in the LBD (Haltiwanger et al., 2014). Second, we measure firm size by summing up the employment counts from all of their establishments. Finally, 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.

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 title, O*NET occupation codes, and estimated salary. Revelio imputes salary based on job title, company, location, years of experience, and seniority using a statistical model. Revelio provides each position's start and end dates. 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.

Revelio predicts the probabilities of a worker belonging to each racial group, White, Black, Asian and Pacific Islander, Hispanic, Native, or multiple races. We define a worker as a minority if her probability of being a non-white worker exceeds 50%. We also use the gender and education background information provided by Revelio.

In the resume data, the 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, and match employers to Compustat's public firm identifier. 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 careers. First, we define *New Position* to be a binary variable that equals one if a worker is assigned to a different job position in the next year, and zero otherwise. This is an indicator of 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* as one if a worker changes to a higher-paying position within the same firm and the same three-digit SOC in the next year, and zero otherwise. Finally, we code *Change to Tech-oriented* as an indicator for whether a worker changes his (her) job code from a non-tech-oriented category to a tech-oriented category within a firm.[8] All indicators are multiplied by 100, so our coefficients indicate job transition and promotion rates in percentage points.

[8]The classification of tech-oriented occupations follows Hecker (2005) and refers 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.

3.4 Sample Construction

We consider a firm to be affected by the ARL 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 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 an 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 sector (NAICS2) 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 potent 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. 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). Our sample created using the Census data includes the employment histories of 453,600 unique workers and 498 unique firms.[9] The resume-

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

based sample includes 2.67 million unique workers and about 3.26 million job spans.[10] Both samples span the years of 1990 to 2012. This gives us sufficient time series before the first adoption of the anti-recharacterization laws (1997). The samples end in 2012 because the matching quality between LEHD and LBD worsens in 2013 and 2014.

3.5 Descriptive Evidence

3.5.1 Summary Statistics

Table 1 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. On average, workers in our sample are 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.[11]

TABLE 1 ABOUT HERE

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.

[10] Revelio data covers workers across all states, whereas our Census project only has access to workers in 25 participating states. This could contribute to the difference between samples created using Census and Revelio data.

[11] These numbers are comparable to the data published by the Bureau of Labor Statistics. For example, in 2022, among full-time employed individuals aged above 16 years or above, 6.9% are Asian, 6.4% are Black or African-American and 56.6% are male. See more details at here.

Consistent with the literature, white workers persistently earn more than their non-white peers, with the racial earnings gaps rising 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 2 reports the results from four specifications. In the first column, we do not impose any control. We then add event-by-firm fixed effects 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. An "event" refers to one of the three ARL events that we study, representing a match cohort in our sample. 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 2 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, and an individual's job history and age. Jobs are categorized into seven levels: entry, junior, associate/analyst, manager, vice president, director, and C-suite.[12] 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

[12]Examples of these categories include: 1. Entry level (e.g., 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.

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.

3.6 Effects of ARLs on Debt Capacity

In the last step of our initial analysis, we verify the argument that the anti-recharacterization laws facilitate firms' access to debt markets and allow firms to expand production and adopt technologies. While such evidence has been well-established in the prior literature, we repeat the analysis for our sample. Specifically, we examine the changes in firms' total debt, leverage ratios, total employment, and technology-related fixed assets (equipment and machinery) around the adoption of the ARLs.

Table C.1 shows that following the law adoption, treated firms issue more debt and experience a significant increase in leverage by about 4 percentage points, similar to estimates obtained in Li et al. 2016. They also substantially expand employment and acquire more equipment and machinery. Overall, our evidence is consistent with the argument that firms expand production and adopt technology following the laws (Ersahin 2020), which ultimately lead to an increased demand for skilled labor.

4 Empirical Framework

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

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

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.[13]

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 terms 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 (NAICS3) dynamics that could influence the racial earnings gap inside firms within the matched group. 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 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 across treat-

[13]LEHD reports race and ethnicity (i.e., Non-Hispanic and Hispanic or Latino) as two separate items. Hispanic may be of any race. In addition, the ethnicity question in the Census survey was one of the most unanswered questions in surveys conducted before 2003 because it was asked after the race question and people thought they have answered the ethnicity question by answering the race question. This issue remained until the Revisions to the Standards for the Classification of Federal Data on Race and Ethnicity in 1997. For these reasons, in our testing sample created using Census data, we do not differentiate Hispanics from other races, and define $Minority_i$ solely based on race.

ment and control firms. Standard errors are clustered by the state where individual i works.[14]

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 Tech-oriented*. 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 3 presents the main findings of our study. We estimate Equation (1) 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-

[14] In Table 13, we show that our baseline results stay unchanged 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 3 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 (1). Table 4 reports the results from the same set of specifications as in our baseline analysis (Table 3). 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 4 ABOUT HERE

5.2 Dynamic Effects

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

We estimate the dynamic effects of the ARLs over a [-2, +5]-year event window. The length of the window is limited by the length of workers' job span (on average 6 years). We keep a longer post-event window to trace the long-run effect of the shock on worker earnings.

Our estimation proceeds in two steps. First, we estimate coefficients from the triple difference design as follows:

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

$$\mu_{2e,s(f),t} + X_{e,i,f,t} + \epsilon_{i,e,f,t}, \quad (3)$$

where k represents each year during 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. As described in Equation 1. $\alpha_{e,i,f}$ represents worker-by-firm fixed effects. $\mu_{1e,j(f),t}$ represents event-industry-year fixed effects. $\mu_{2e,s(f),t}$ represents event-state-year fixed effects. Other controls ($X_{e,i,f,t}$) are the same as in column (5) of Table 3. $Treated_f \times Minority_i$ is absorbed by worker-by-firm fixed effects. The interactions of $Minority_i$ and event time dummies are absorbed by event-minority-year fixed effects.

Second, we separately examine how earnings evolve for minority and white workers around the passage of ARLs. This helps ease the interpretation of the triple-interaction coefficients and validate that the reduction in the earnings gap is not driven by a reduction in White workers' earnings. We thus estimate Equation 3 for each of the subsamples:

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

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. 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. $Treated_f$ is absorbed by worker-by-firm fixed effects. Controls are the same as in column (5) of Table 3.

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 non-white 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 1 by substituting the dependent variable to be *Pay Rank*, the percentile ranking (1–100) of an individual's annual earnings relative to their peers 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 5 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 2. 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 5 ABOUT HERE

Overall, 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/skill 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 a "human capital utilization" channel. Due to biases or labor frictions inside firms, minority workers are matched to lower-paying positions, or worse-fit tasks for their skills compared to white workers. The laws relax firms' financing frictions, leading to an expansion of higher-paying/skill 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, the human capital utilization channel does not predict a stronger effect for low-skill 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 channel 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, lack of training and mentorship for minority workers, or social or cognitive limitations of managers. We conjecture that our effects should be stronger in cultural environments that tolerate greater inequality.

To investigate these explanations, we examine the role of worker skills and local 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 averaged quarterly earnings 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:

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

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 3, including controls for worker tenure, event-firm-worker fixed effects as well as interactions of year fixed effects with other worker characteristics and firm 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 suggest that 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/skill but minority dominated positions. Instead, they are consistent with the human capital utilization channel, i.e., minority workers have access to more job opportunities and 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 non-white workers in our sample firms. Table 6 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 2, and in addition event-occupation-year interactive fixed effects. We find that minority workers are around 0.5 percentage points less likely to switch to a new job position than white workers, 0.34 percentage points less likely to be promoted to a higher-pay position, and 0.17 percentage points 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, which represents a 10.6% (= 0.019/0.18) difference relative to the sample mean reported in Table 1

TABLE 6 ABOUT HERE

In Table 7, we 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 compared to White workers. 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 averaged gap reported in Table 6 (= 0.196/0.486). In Panel B, as compared to white workers, 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 experience a greater increase in the likelihood of switching to a technology-heavy occupation compared to white workers after the adoption of ARLs.

TABLE 7 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 further support the human capital utilization channel, i.e., better access to financing improves the utilization and development of minority workers' human capital.

6.3 The Role of White Worker Supply

As firms expand and increase the demand for skilled jobs, predominately occupied by white workers, they may allocate more job opportunities to minority workers, particularly in regions where the labor market for white workers is comparatively tighter than in other areas. 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 inside the firm to a greater extent.

6.4 The Role of Pre-Existing Inequality

The human capital utilization channel suggests that biases or frictions inside the firms prevent 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 measure the demographic diversity among the board of directors following Bernile et al. (2018) and Genin et al. (2023). 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) negative Herfindahl–Hirschman index (HHI) in director ethnicity.[15] 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 $Treated \times Post \times Minority$ in a quadruple difference-in-difference framework. Table 8 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 the pay gap 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 worse diversity practices. These patterns highlight the important role of corporate finance in alleviating the labor market frictions faced by minority workers.

Table 8 About Here

[15]HHI in directory ethnicity is constructed as the sum of the squares of director ethnicity shares within the board of a given firm-year. A higher HHI indicates a greater concentration of board members' races within a single group, so we employ a negative HHI to measure diversity.

7 Extensive Margins and Long-Run Effects

In the analysis so far, we have focused on the income growth of existing workers who 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 who leave their employers eventually.

7.1 Separation

In Table 9, we examine the differential job separation likelihood between white and minority workers using the employer-employee-matched LEHD-LBD sample. Panel A follows the setup in Table 2 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 9 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 changes in separate rates are not significantly different between white and minority workers. This means that, while increased debt financing reduces the racial pay gap, minority workers are not more likely to be retained by the firm than white workers.

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 2. Panel A of Table 10 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 where income inequality tends to be smaller than that at senior positions (Fox, 2009).

<u>Table 10 About Here</u>

In Panel B, we estimate Equation (1) 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* $\times$ *Post* $\times$ *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. The magnitude of this effect is more pronounced for new hires than for incumbent workers. There are at least two explanations. First, compared to new hires, incumbent workers may be willing to take a smaller raise, because exploring outside options may lead to losing their firm-specific human capital (Burdett, 1978; Black, 1981). Second, to employ minority workers for skilled positions, firms may directly search for qualified workers outside the firm or invest in incumbent workers through training. The additional investment in incumbent workers could contribute to the differential pay raise.

7.3 Long-Run Effects

Our evidence suggests that when employers have better access to financing, minority workers receive higher earnings, leading to a narrower racial pay gap inside affected firms. 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, as minority workers accumulate more human capital in the affected firms, they may become more valuable and productive for future employers. As a result, their income level may be raised permanently. In addition, their current earning levels can serve as a benchmark for future wage bargaining, raising their compensation 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 zero 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 1. Coefficients on *Treated* × *Post (Same Firm)* × *Minority* represent the direct effect of the laws on minority workers through the affected employers, while coefficients on *Treated* × *Post (Different Firm)* × *Minority* compares workers' income after their job switch to the pre-event levels, thus capturing the long-run effects. Since 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 11. This table follows the same specifications as our baseline analysis (Table 3). We find that *Treated* × *Post (Same Firm)* × *Minority* and *Treated* × *Post (Different Firm)* × *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* × *Post (Same Firm)* × *Minority* and *Treated* × *Post (Different Firm)* × *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 11 About Here

8 Additional Analyses

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

To start, we validate the importance of SPVs as a 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 raise off-balance sheet financing, we expect the effects to become stronger for firms that have outstanding SPVs, who can more directly 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 12, we find that the effect of the ARLs on the within-firm earnings gap is driven by 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 12 About Here

We then consider 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 some 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 11 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 to zero after 2003 (Li et al. 2016; Ersahin 2020). We label this new indicator as *Treat (on-off)*. The coefficient for *Treat (on-off)* $\times$ *Post* $\times$ *Minority* captures the effect of the laws before the federal court ruling in 2003. Panel A of Table 13 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.

<u>Table 13 About Here</u>

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 results that are consistent with 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 the gender pay gap, instead of racial inequality. We test this argument in Table D.1. In particular, we estimate the coefficient of a quadruple interaction term between *Treated*, *Post*, *Minority*, and *Female*. The coefficient seems large in economic magnitude, suggesting that minority female workers enjoy a 5% greater pay raise than minority male workers; however, it is statistically insignificant. Interestingly, the coefficient of $Treat \times Post \times Female$ is negative and significant, indicating that the adoptions of ARLs, on average, do not reduce the gender earnings gap inside firms. These findings may be explained by other labor market frictions that are not reduced by better access to finance. For example, women may prefer flexible work hours (Goldin, 2014), which is less likely to be offered when firms expand and increase labor demand.

9 Conclusion

This paper investigates the role of access to financing in shaping the racial pay gap within firms. Using employer-employee matched data administrated by the U.S. Census Bureau, we show that with better access to financing, firms substantially reduce the racial earnings gap.

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. The granularity of the administrative data 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) or dynamic worker-firm matching. We also rule out the explanation that low-skill tasks (potentially disproportionately more by minority workers) are better compensated after the passage of the anti-recharacterization laws.

We find that following positive financing shocks, the increase in earnings is concentrated among middle- or high-skill minority workers. Minority workers are also more likely to switch to higher-pay, higher-skill positions inside firms. These findings indicate that firms better 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.

This study is the first to provide causal evidence that the ability to raise debt financing narrows the racial income gap inside firms. We not only improve the identification strategy by utilizing idiosyncratic shocks to financing, but also shed 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 plots the quarterly earnings in 2018 dollars averaged over each period for non-white and white workers in our Census sample over 1990-2012.

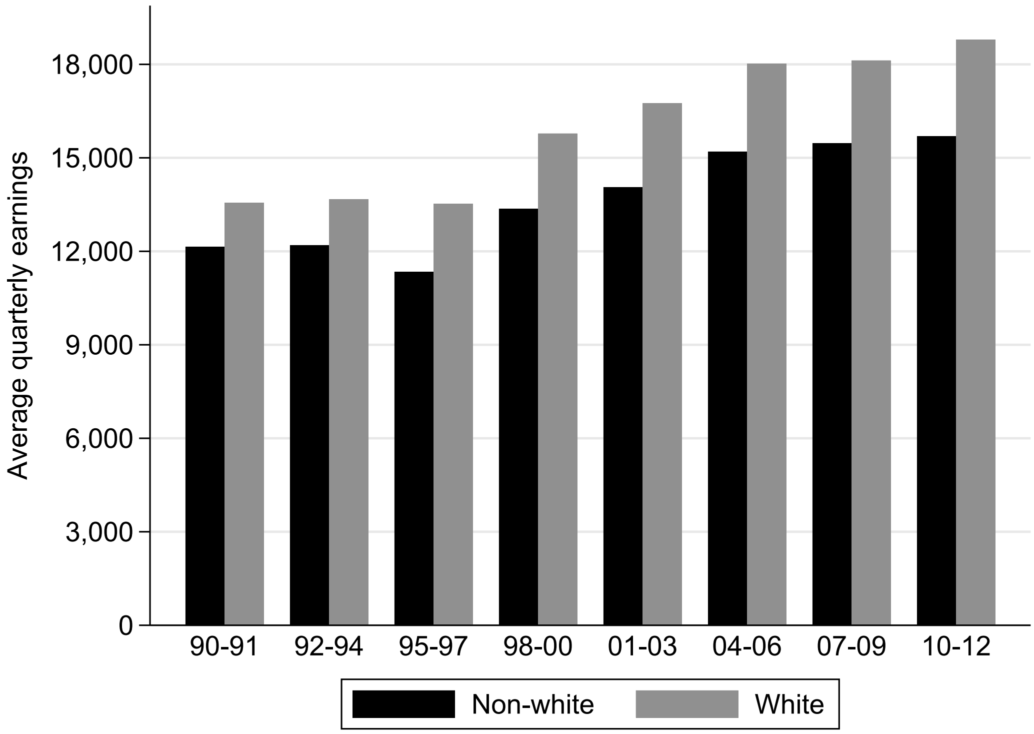


Figure 2. Minority Worker Share by Job Seniority
The figure plots the percentage of workers that are non-white at each job seniority in our sample. Data come from Revelio Labs.

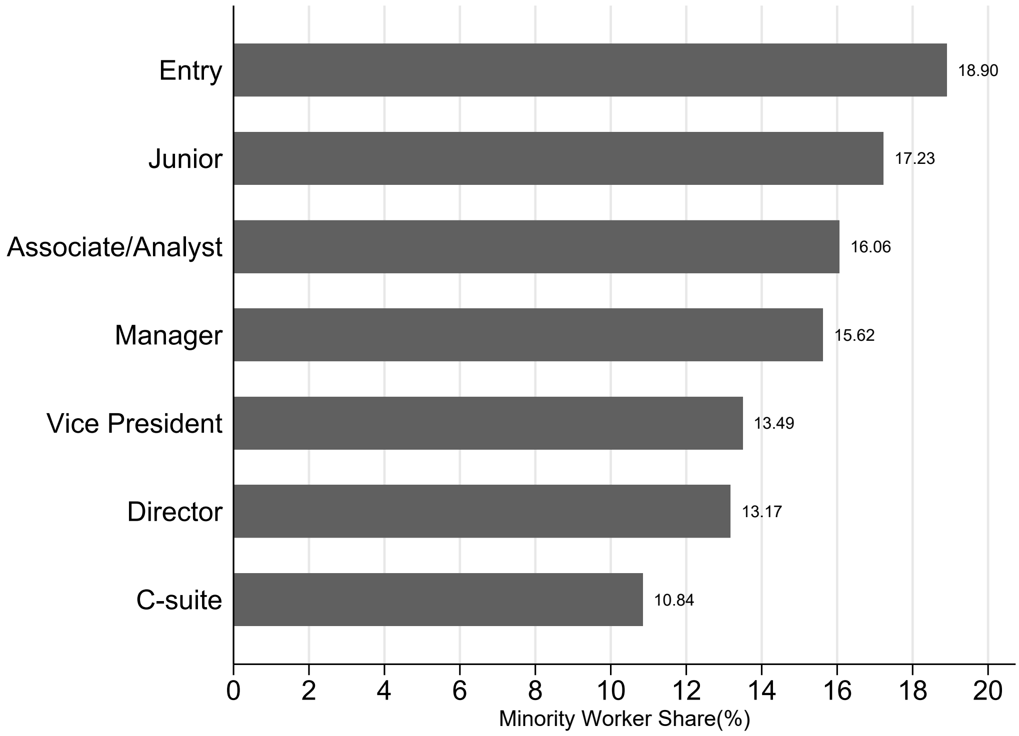
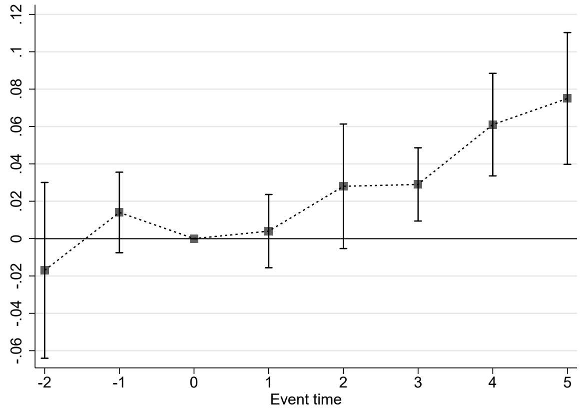
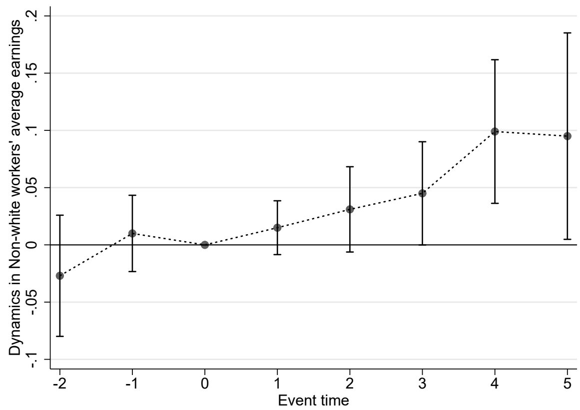


Figure 3. Dynamic Effects of ARLs on Worker Earnings

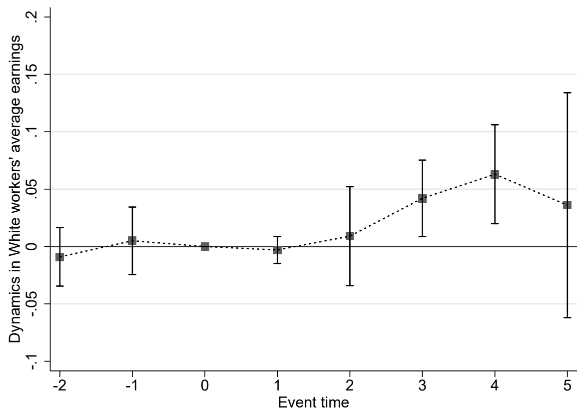
The figure plots the dynamic effects of the ARLs on the earnings gap between minority and white workers within firms. The dependent variable is workers' average quarterly earnings in a year. Panel A presents dynamic coefficients of the triple difference terms ($Treat \times 1_{t=e_t+k} \times$ $Minority$ in Equation 3) indicating earnings gap, Panel B reports dynamic difference-in-difference coefficients ($Treat \times 1_{t=e_t+k}$ in Equation 4) for minority workers, indicating the changes in earnings for minority workers around the event relative to the event year. Panel C reports the corresponding difference-in-difference coefficients for white workers. All panels include the same controls and fixed effects as in column (5) of Table 3 Standard errors are clustered by workers' state.



Panel A: Earnings Gap (Triple Interaction Coefficients)

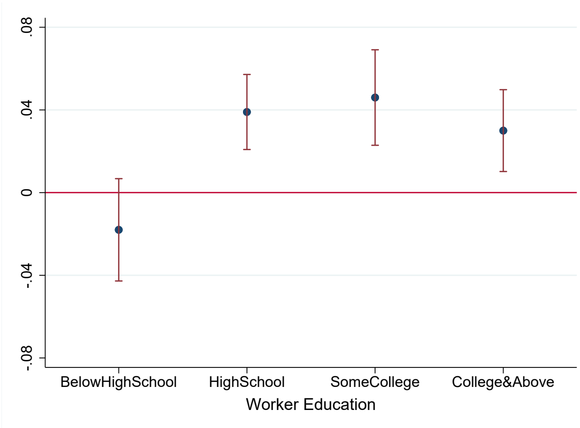


Panel B: Minority Worker Earnings

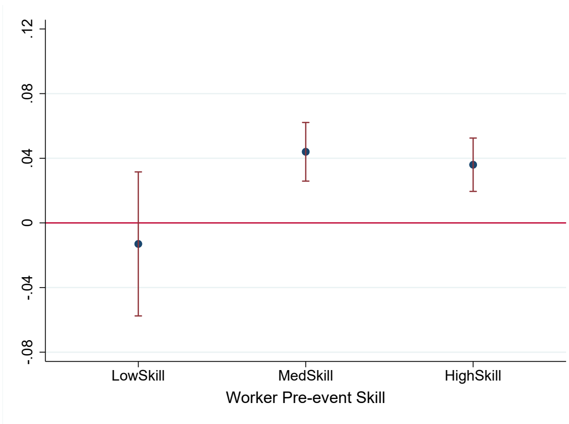


Panel C: White Worker Earnings

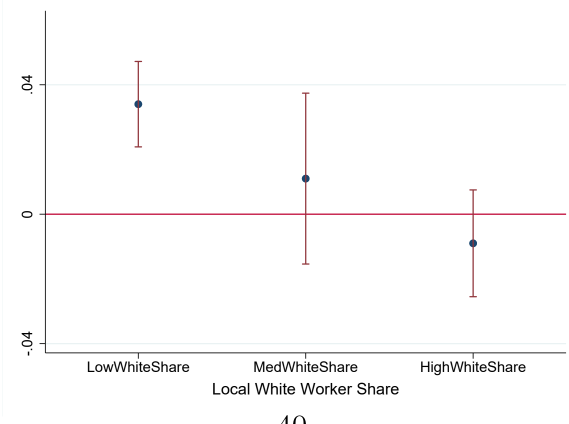
Figure 4. Heterogeneity of Effects: Worker Skill and the White Worker Share
The figure plots the differential effects of the ARLs on the earnings gap between minority and white workers within firms by measures of worker skill levels and by white worker labor market tightness measured by white worker share. 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 terciles of the average earnings over the five years prior to the event. Panel C provides the interactive coefficient estimates related to local white worker share, i.e., terciles of the percentage of white workers in the commuting zone. 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. The coefficient estimates indicate the differential change in earnings for minority workers relative to white workers in a skill and location category. All regressions include the same controls as Column (6) of Table 3, including controls for worker tenure, event-firm-worker fixed effects as well as interactions of year fixed effects with other worker characteristics and firm fixed effects.



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
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 the 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.
Earnings ($)	14670	16850
Log(Earnings)	9.35	0.68
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 Tech-oriented (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 2
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 firm's age 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 3
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 4
Access to Debt and Earnings Changes Across Races

This table reports the change in the minority earnings gap using a triple difference-in-difference 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 5
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 the racial pay rank gap changes around the adoption of the anti-recharacterization laws. *Minority* is a dummy variable for all non-white workers. *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 6
Racial Gap in Worker Positions

This table reports the differences in job positions between minority and white workers. We examine the following variables: an indicator for whether a worker obtains a new position in the following year, i.e., *New Position*; an indicator for whether a worker changing to a new position with a higher salary next year, i.e., *Promotion*; whether a worker is promoted to a new position in the same three-digit ONET code, i.e., *Promotion Within Occ*; and whether a worker changes to a tech-oriented occupation, i.e., *Change to Tech-oriented*. *Minority* indicates 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 3. 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 Tech-oriented*	
	(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
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 7
Changes in Worker Career

This table reports the differential changes in the careers of minority and white workers, including having a new position, promotion, promotion within the occupation category, and changing to tech-oriented positions, following the passage of anti-recharacterization laws. The sample is an event-worker-year panel. 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 tech-oriented occupation, i.e., *Change to Tech-oriented*. 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* indicates 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 3. 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)
$Treat \times Post \times Minority$	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)
$Treat \times Post$	-0.38**	-0.451***	-0.262*	-0.264**	-0.243*	-0.252**	
	(0.151)	(0.166)	(0.139)	(0.12)	(0.134)	(0.118)	
$Treat \times Minority$	-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)
$Minority \times Post$	-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)
$Treat \times Post \times Minority$	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)
$Treat \times Post$	-0.397***	-0.396***	-0.268***	-0.219***	-0.239***	-0.195***	
	(0.081)	(0.09)	(0.077)	(0.058)	(0.064)	(0.05)	
$Treat \times Minority$	-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)
$Minority \times Post$	-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 Tech-Oriented Positions

Dep. Var.: *Change to Tech-oriented* (%)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat×*Post*×*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×*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×*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×*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 8
Heterogeneity of Effects: Pre-event Inequality
This table reports the change in the earnings gap between minority and white workers by firms' pre-event diversity practices. 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 gap 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 3. 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 9
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 10
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 11
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 difference-in-difference model. 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. The coefficients on these *Post* variables are estimated but not reported. *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 12
Firms With and Without SPVs
This table reports the change in the 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 3. 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 13
Robustness Checks
This table reports results from robustness checks regarding our empirical specification. In Panel A, we re-estimate Equation (1) 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

- *Minority*: An indicator equal to one for non-white workers, and zero otherwise. Source: LEHD /Revelio
- *Earnings*: The quarterly earnings (in 2018Q3 dollars) averaged across quarters within a given year for a given worker. 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-LBD/Revelio 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-LBD/Revelio and Compustat
- *Log(Earnings)*: The log of the average quarterly earnings (in 2018Q3 dollars) across quarters within a year for a given worker. Source: LEHD
- *Black*: An indicator equal to one if a worker's reported race is Black or African-American, and zero otherwise. Source: LEHD
- *Asian*: An indicator equal to one if a worker's reported race is Asian or Native Hawaiian, Other Pacific Islander, and zero otherwise. Source: LEHD
- *Other Minority*: An indicator equal to one if a worker's reported race is American Indian, Alaska Native, or workers with two or more race groups, and zero otherwise. Source: LEHD
- *Worker Tenure*: The number of years a worker has worked for their employee. Source: LEHD-LBD
- *Male*: An indicator equal to one for male workers, and zero otherwise. 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-year divided by the number of employees of a given firm-year). Source: LEHD-LBD
- *Skill*: Measured by the average quarterly earnings (*Log(Earnings)*) of a worker during the year prior to the event. Source: LEHD-LBD
- *High/Mid/Low Skill*: An indicator equal to one if a worker's *Log(Earnings)* in the year prior to the event year is in the top/middle/bottom tercile of the sample distribution. Source: LEHD-LBD
- *New Position*: An indicator that equals one if a worker obtains a new position within the same firm in the following year, and zero otherwise. This indicator is multiplied by 100 in regressions. Source: Revelio
- *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 in regressions. Source: Revelio
- *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 in regressions. Source: Revelio
- *Change to Tech-oriented*: An indicator that equals one if a worker changes to a tech-oriented occupation next year, and zero otherwise. This indicator is multiplied by 100 in regressions. Following Hecker (2005) and Ma et al. (2022), tech-oriented occupations

include scientific, engineering, and technician 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. Source: Revelio

- Low/Mid/High Pre-event Earnings Gap:An indicator equal to one for employers whose racial earnings gap is ranked in the bottom/middle/top tercile during the year prior to the event year, and zero otherwise. Source: LEHD-LBD
- Board Diversity For a given firm-year, following Bernile et al. (2018), this index is calculated as Std. Age + Share of female directors -Ethnicity HHI, where each factor is normalized to have zero mean and standard deviation of one. Std. Age is the standard deviation of the ages of the board directors at a given firm-year. Ethnicity HHI is equal to 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. Source: BoardEx and RiskMetrics
- Low/Mid/High Board Diversity An indicator equal to one for employers whose board diversity is ranked in the bottom/middle/top tercile during the year prior to the event year, and zero otherwise. Source: LEHD-LBD and BoardEx and RiskMetrics
- *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-LBD
- *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-LBD and Compustat
- *No/Has SPV*: An indicator for whether a firm discloses at least one subsidiary (no subsidiary) in its 10-Ks in a given year. Source: SEC 10-Ks
- *Log(Earnings) (Highest)*: The log of the highest quarterly earnings (in 2018Q3 dollars) for a given worker-year. 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 ARL and Firm-level Outcomes

Table C.1
ARL and Firm-level Outcomes
This table reports the effect of ARL on firm-level debt financing and real operations. The dependent variables in Panel A include *Log(Debt)*, the log of total book debt, *Log(Dltt)*, the log of total long-term debt, and *Leverage*; in Panel B include *Log(Emp)*, the log of total employment (Compustat), *Log(Capex)*, the log of total capital expenditure, and *Log(FAT)*, the log of total costs of machinery and equipment (including purchases and leasing). *Treat* is an indicator for workers working for parent companies that incorporated in LA (1997), TX (1997), and AL (2001). The control group includes firms that never experienced ARL, and in the same sector (NAICS2) and quintile of employment size bin with the treated firms. In Panel A, control variables include *Log(Sales)*, *Profitability*, *Tobin's Q*, and *Tangibility*, consistent with the prior literature (Li et al. (2016),Ersahin (2020)). In Panel B, control variables include *Log(Sales)*, *Profitability*, and *Tobin's Q*. Standard errors are clustered by firms. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Regressions are estimated based on the following:

$$Y_{e,f,t} = \beta Treat_f \times Post_{e,t} + \alpha_{e,f} + \mu_{e,t} + X_{e,f,t} + \epsilon_{e,f,t} \tag{6}$$

where 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.

Panel A: Firm Debt Issuance

Dep. Var.:	Log(Debt)		Log(Dltt)		Leverage	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	0.480***	0.288***	0.486***	0.270**	0.044**	0.037**
	(0.129)	(0.107)	(0.146)	(0.121)	(0.018)	(0.017)
Event-firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls		Yes		Yes		Yes
Observations	98125	84843	87353	76709	105861	101706
R-squared	0.813	0.844	0.801	0.830	0.646	0.675

Panel B: Firm Real Operations

Dep. Var.:	Log(Emp)		Log(Capex)		Log(FAT)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	0.184***	0.117**	0.369***	0.258***	0.245**	0.161**
	(0.071)	(0.047)	(0.114)	(0.091)	(0.103)	(0.064)
Event-firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls		Yes		Yes		Yes
Observations	123350	101428	112621	98670	70620	63102
R-squared	0.936	0.966	0.840	0.882	0.938	0.965

Appendix C Additional Analyses

Table D.1
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 3. 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

Access to Financing and Racial Pay Gap Inside Firms

Janet Gao, Wenting Ma and Qiping Xu

INTERNET APPENDIX

Internet Appendix: Data Description

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, which starts in 1985), while other states' coverage begins later. Appendix Table IA.1 lists covered states and years available.

Within the covered states, the Employment History Files (EHF) of the LEHD program track workers' quarterly earnings, locations, and industries across employers. 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. Workers' earnings include all forms of immediately taxable compensation, 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. If a person's race response is missing but at least one other member of the household reports a valid race, then the allocation is retained. However, this rule was not blindly applied. The quality of the allocation was confirmed using bestrace from the Numident. The correspondence with bestrace must be relatively high for the allocation to be retained. No hot (cold) deck allocations were retained (LEHD data manual, page 5-1 and 5-5). We define all non-White workers as minority workers.

Besides race, ICF also reports workers' birth years, gender, and education levels. Demographic characteristics are 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). 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 dataset allows us to observe quarterly earnings, it does not provide information on the number of weeks worked. 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.[1] 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.[2] 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

[1]The federal minimum quarterly earning threshold=federal hourly minimum wage in a given year × 40 hours per day × 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`.

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

average across quarterly earnings earned at each firm within a given year. If a worker worked at different firms within that year, we keep the highest average earning (i.e., the best-paid job). 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.

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

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 title, O*NET occupation codes, and estimated salary. Revelio imputes salary based on job title, company, location, years of experience, and seniority using a statistical model. Revelio provides each position's start and end dates. 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.

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.[3] 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 exceeds 50%. Similarly, Reverlio predicts a worker's gender using their first name. We also have information regarding their educational background from worker resumes.

In the resume data, the 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 sample is matching employers to Compustat's public firm identifier.[4] To do so, we start with

[3]The algorithm is a Bayesian Inference Model drawing data based on first and last names and locations. See more details about gender and racial group predictions at here.

[4]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.

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 careers. First, we define *New Position* to be a binary variable that equals one if a worker is assigned to a different job position in the next year, and zero otherwise. This is an indicator of 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* as one if a worker changes to a higher-paying position within the same firm and the same three-digit SOC in the next year, and zero otherwise. Finally, we code *Change to Tech-oriented* as an indicator for whether a worker changes his (her) job code from a non-tech-oriented category to a tech-oriented category within a firm. The classification of tech-oriented occupations follows Hecker (2005) and refers 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. All indicators are multiplied by 100, so our coefficients indicate job transition and promotion rates in percentage points.

Table IA.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