The Racial Penalty in Job Ladder Transitions*

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Abstract: We study the role of job transitions and firm pay policies in the Black-White earnings gap in the US. We use administrative data for the universe of employer-employee matches from 2005-2019 to analyze worker mobility in a general but tractable framework, which allows for firm effects that depend on workers' job history. Using differences in average pay between origin and destination firms as the treatment intensity of a job move, we analyze transitions up and down the job ladder and estimate race-specific passthrough rates of average firm pay into a mover's own earnings. First, we find race-specific asymmetry around the direction of the move, whereby losses experienced in downward transitions are meaningfully larger than gains from upward transitions with a similar treatment intensity. For a \$1 earnings increase in transitions up the job ladder, earnings passthroughs in transitions down the job ladder impose an earnings loss of \$1.25 among White workers and \$1.50 among Black workers. Second, we uncover career setbacks as a novel pathway in the evolution of racial earnings gaps. In transitions down the job ladder, Black workers lose an additional \$0.24 for every \$1 decrease in White workers' earnings, a finding which prevails across sex and age. This "racial penalty" is not driven by differential pay, as it is completely absent when Black and White workers move between the same firm pairs. Instead, the penalty is due to differential sorting following career setbacks, so that Black workers regain employment in "worse" jobs, with strong evidence for racial differences in access to short-run liquidity as a mechanism. Overall, our findings offer a robust and computationally simple framework for modeling earnings determination processes and have implications for safety-net policies in the American labor market.

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1. Introduction

Stark inequalities in labor market outcomes between Black and White Americans are a reality of modern economic life. Indeed, most of the existing Black-White income gap is driven by differences in earnings (Chetty et al. 2020). Two strands of the literature—one on earnings inequality and the other on career trajectories—offer potential new avenues to dissect the persistent nature of these racial gaps. In the context of earnings inequality, the central role of employers has been recently emphasized in a series of influential papers (summarized in Card et al. 2018). Specifically, two-thirds of the rise in earnings inequality in the US since the 1980s comes from the rise in earnings dispersion across firms (Song et al. 2019), and there is clear experimental evidence of discriminatory callback practices that substantially differ across employers (Kline et al. 2022). In the context of career trajectories, job mobility has been long recognized as a critical driver. In their well-known work, Topel and Ward (1992) find that wage gains at job changes account for a third of wage growth during the early career, and recent work verifies that earnings gains frequently follow job transitions at the lower end of the earnings distribution (Clemens and Strain 2023).

In this paper, we connect these two strands of the literature by studying the role of job transitions and firm earnings premia in the Black-White earnings gap in the US. We use population-wide IRS tax records linked with the Census Numident and the Longitudinal Business Database (LBD). The data allow us to observe the universe of employer-employee matches in the American labor market from the years 2005-2019 with employer characteristics (such as size and location) and employee characteristics (such as race, age, and sex). For identification, we employ empirical methods that leverage worker mobility. We use a general job transitions model for earnings determination, which includes individual fixed effects and firm premia that can have race-specific dependence on workers' job history. Using the difference in average firm pay as the "treatment intensity" of a job move, we analyze transitions up and down the job ladder based on the directionality of the move—to focus on "career progression" versus "career setbacks"—and we study how their effects differ by race.

The combination of the model specification and the event-study style estimation strategy allows us to sidestep some key statistical complications identified in prior work and to relax the traditional exogenous mobility assumption in important ways. Specifically, by relying on comparisons across cohorts of movers and avoiding any comparisons between movers and non-movers, we allow for selection into job transitions as well as differential selection into transitions up and down the job ladder. Our framework further allows for rich firm-specific complementarities with employee types based on employment histories. We formalize the requirements for identification and develop various joint tests for our model and its identifying assumptions. The tests rely on pre-move periods to assess comparability across mover cohorts with differential treatment intensity, and on post-move periods to assess any residual sorting on firm-employee complementarities. We find consistently strong support for our design. With this in hand, our key parameters of interest are the race-specific passthrough rates of firms' average pay (as captured by the job move's "treatment intensity") into a worker's own earnings following the move. When the model is well specified, these rates translate to the average share of earnings differentials across firms that are causally attributable to employer effects, i.e., via the firm's earnings premia. We assess our parametric specification of piecewise log-linearity based on sample splits and deviations from predictions, finding that the model tightly fits the observed data for each race.

Estimating our model on the universe of job switchers in the US over a period of 15 years, we provide two sets of new results. First, we find a systematic race-specific asymmetry in the earnings determination process. As captured by the ratio of passthroughs across move directionalities, losses from downward transitions are meaningfully larger than gains from upward transitions of a similar magnitude. The asymmetry factors are such that for a \$1 change in own earnings in upward transitions, earnings changes in downward transitions impose a loss of \$1.25 among White workers and \$1.50 among Black workers. The close fit of our parametric specification to the data uncovers race-specific earnings determination processes that have a clear reference-dependent shape around the directionality of the job move, where the reference point is determined by an individual's work history.

Second, we find a significant racial penalty so that Black workers are "hit twice" in career setbacks. Specifically, we show that Black workers who transition down the job ladder lose an additional \$0.24 for every \$1 decrease in White workers' pay; where transitions up the job ladder have equal passthroughs across race. We term this key finding as the "racial penalty" in job ladder transitions, which, to the best of our knowledge, offers evidence of a novel causal channel in the evolution of racial earnings gaps in the US. We find that the racial penalty is a robust phenomenon: it prevails in similar magnitudes across males and females, across first and second job switches in our analysis period, and across the age distribution.

We next investigate potential explanations for this racial penalty. We show that there is no racial penalty in passthrough rates when focusing on Black and White workers' transitions down the job ladder across identical employer pairs (so moves are exactly comparable across race beyond having similar treatment intensity). This implies there is no differential in pay, but rather that when Black workers suffer a career setback, they sort into the types of firms that penalize pay at a higher rate. A natural question is whether this sorting is driven by supply-side factors (workers) or demand-side factors (employers).

On the supply side of workers, we focus on liquidity constraints that have been shown to play a key role in post-layoff labor market dynamics (Chetty 2008). Liquidity-constrained households face higher pressure to find a job quickly, which potentially pushes laid-off workers to accept worse employment offers. Coyne, Fadlon, and Porzio (2024) show how reliance on withdrawals from retirement accounts can reveal households' valuation of liquidity, and they also show that Black households in the US rely on withdrawals to a higher extent following unemployment, suggesting that differential liquidity constraints could provide an explanation in our setting. To test this hypothesis, we leverage tax-record extracts from Form 1099-R, which covers "Distributions From Pensions, Annuities, Retirement or Profit-Sharing Plans, IRAs, Insurance Contracts, etc." We find strong support for the liquidity hypothesis: for a given level of treatment intensity in transitioning down the job ladder, Black workers are twice as likely to prematurely withdraw funds from their retirement accounts in the year of the adverse event as compared to White workers (with a takeup rate of 10.9 percentage points relative to 5.4 percentage points among White job movers).

On the demand side of employers, we consider a version of the classic conjecture that individuals' adverse employment history may provide a productivity signal to employers. Kroft, Lange, and Notowidigdo (2013) provide evidence that employers use the unemployment spell length as a signal of unobserved productivity while recognizing that this signal is less informative in weak labor markets. Extending this logic to our context, if career setbacks are used as a signal of unobserved productivity and if it is perceived differentially across employees' race, the racial penalty would be mitigated when the signal is less informative in adverse aggregate labor market conditions. To test this conjecture, we study the Great Recession. Using variation in the unemployment shock across commuting zones (as laid out in Yagan 2019), we find no differential impact of the shock by race. This finding suggests differential signaling by race as one of the explanations for the racial penalty in career setbacks: aggregate employment shocks (being less predictive of an individual worker's productivity) affect Black and White workers similarly; but individual downward transitions in typical times are interpreted as a stronger negative signal for Black workers than White workers, which hinders Black workers' reemployment opportunities.

Our results have two sets of implications that contribute to two main strands of the literature. First, our results have clear implications for the empirical modeling of earnings determination processes by offering a computationally-simple framework that we show to be empirically robust, in terms of both identification and parametrization. Our finding of race-specific path-dependence in earnings determination shows a systematic deviation from workhorse models that is consequential for the analysis of employees' job mobility and its effects on race-specific wage dynamics. We specifically deviate from the model developed in the groundbreaking work by Abowd, Kramarz, and Margolis (1999) and followed by a series of influential papers that have studied the role of firms in earnings inequality in different countries (e.g., Gruetter and Lalive 2009, Card, Heining, and Kline 2013, Card et al. 2016, Song et al. 2019, Gerard et al. 2021, Bonhomme et al. 2023). Beyond this work on the role of firms in generating inequality, our findings are instrumental for the modeling of wage dynamics more broadly. Structural empirical work that involves interactions with the labor market, such as in the analysis of labor market policies and government welfare programs, must specify a wage determination process as a modeling fundamental (see, e.g., Low, Meghir, and Pistaferri 2010, Low and Pistaferri 2015). Importantly, while allowing for flexible history dependence has the potential to be highly complex, the robust framework that we offer is notably concise: race-specific earnings determination models with a piecewise-linear reference-dependent shape.

Second, our finding of the racial penalty in job transitions reveals career setbacks as a novel causal source of racial disparities in earnings dynamics. With these results we contribute to the important work on racial disparities in the US, specifically in labor market outcomes and earnings (including, among others, Bayer and Charles 2018, Chetty et al. 2020, Derenoncourt and Montialoux 2021, Kline, Rose, and Walters 2022, Sorkin 2023, Card, Rothstein, and Yi 2024). Moreover, our results on the potential drivers and non-drivers of the racial penalty could help guide effective social protection policies in the labor market. Specifically, our findings pinpoint on a national scale that the penalty is not driven by a potential violation of equal pay laws, and our results further motivate that more generous unemployment benefits could help mitigate the racial penalty by providing greater assistance to households who have a greater need for short-run liquidity upon the event.

The remainder of the paper proceeds as follows. Section 2 describes the data and analysis sample, and Section 3 provides preliminary statistics on the dispersion of earnings gaps and job mobility patterns. Section 4 develops our analytical framework of earnings determination that allows for general patterns of history dependence and interactions with race: it lays out the model, defines our key parameters of interest, and develops identification tests. We then proceed to the core empirical analysis. In Section 5 we provide the empirical evidence on job transitions and race-specific earnings dynamics. In Section 6 we study our key moments of racial inequality, and in Section 7 we study the mechanisms that could drive the racial penalty. Section 8 concludes.

2. Data and Analysis Sample

We leverage a combination of restricted administrative datasets hosted at the US Census Bureau. The data comprise of population-wide information from individual-level IRS tax records linked with the Longitudinal Business Database (LBD). With these data, we observe the universe of employer-employee matches in the American labor market from 2005-2019 with employer characteristics, such as size and location, and employee characteristics, such as race, age, and sex. The data appendix (Appendix B) documents in detail the construction of our analysis sample that we summarize here. It specifically includes the changes in sample size that come from different sample restrictions and describes the exact samples on which each piece of the analysis we provide relies.

We begin by linking individuals in the US Census Bureau Numerical Identification System (Numident) to IRS W-2 Forms. The Census Numident (2023Q1) lists all individuals who have ever received a Social Security Number (SSN). Our race and ethnicity measures are taken from the Numident and are either parent-/guardian-reported or self-reported. We restrict the sample to non-Hispanic Black and non-

Hispanic White individuals, aged 25 to 59 in each year between 2005 and 2019. Throughout the paper, non-Hispanic Black and non-Hispanic White individuals will be referred to as Black and White, respectively.

Using Protected Identification Keys (PIKs), we merge observations in the Census Numident with IRS W-2 Form extracts to obtain taxable earnings reported in Box 1, which lists wages and salary net of pre-tax deductions for health insurance premiums and deferred compensation. Earnings are adjusted to be in 2019 dollars using the CPI for all Urban Consumers (CPI-U). For individuals with multiple W-2s in a year, we select the W-2 with the highest taxable earnings as their "main" employer.

Following the work on firm earnings premia, we restrict the sample based on characteristics that aim to proxy for full-time employees. Specifically, we drop observations with less than $15,000 (in 2019 dollars) in the highest earning W-2. In addition, in each year and for each firm, we count the number of employees who are Black or White, aged 25-59, and earned at least $15,000. Firms with less than 20 employees that meet these restrictions in each year between 2005 and 2019 are flagged, and we exclude workers' full W-2 history if they were ever employed in a flagged firm.

Next, we link person-level data to firm-level data recorded in the Longitudinal Business Database (LBD) using Employer Identification Numbers (EINs). We follow the literature that uses tax data in defining an EIN as our firm/employer unit. For our main analysis of job switchers, we define as the move event an employee's first EIN switch within our data horizon. We note that an EIN can be associated with multiple establishments in a given year, but cross-firm job transitions (as opposed to cross-establishment transitions within a firm) account for over 92 percent of all job changes in the US (Carballo, Mansfield, and Pfander 2024). We exclude individuals ever employed in industries with NAICS codes of 61 ("Educational Services'') or 9211 ("Executive, Legislative, and Other General Government Support") in line with Song et al. (2019). This restriction excludes only 23 percent of workers in Public Administration (NAICS code 92), who themselves composed 1.1 percent of US workforce in 2019.[1] Remaining categories include workers in Health Care and Social Assistance, Postal Services, and more.

Lastly, for geographic location we identify workers' commuting zones (CZs) using a combination of IRS 1040 filings and the LBD matched to the USDA's Economic Research Service's (ERS) list of CZs (using definitions from 2000). We first assign CZs from 1040 filings in cases where the PIK in the 1040 data is unique in a given year. Otherwise, when a single CZ is associated with an EIN in a given year, we use firms' CZs listed in the LBD. Because of data limitations, not all observations are matched to a CZ.

Table 1 provides summary statistics for the overall sample (in panel A) and for our main analysis sample of job movers (in panel B). Our full sample includes 594,400 unique workplaces, as captured by EINs, and 68.5 million unique individuals. About 55 percent of the sample are males, 17 percent are Black,

[1] See "2019 Annual Averages - Household Data - Tables from Employment and Earnings" at: https://www.bls.gov/cps/cps_aa2019.htm.

and the average age is around 43. Among males, the Black-White earnings gap is 42 log points, whereas the racial gap among females is half of that (21 log points). We note that race is also highly relevant for male-female gaps, which are 32 log points among White individuals and a third of that (11 log points) among Black individuals. Panel B that narrows the sample to job movers shows the broad comparability of movers to the entire sample in terms of age (40.35, with movers being somewhat younger), share male (57%), and share Black (15.5%).[2] It also includes a wide coverage of all employers from the full national sample: over 90 percent among White workers and over 70 percent among Black workers (whose union covers 99% of the total).

3. Preliminaries: Dispersion of Earnings Gaps and Job Mobility Patterns

Figure 1 plots the spatial distribution of within-firm racial earnings gaps by sex. To generate these measures, we focus on firms that employ at least one White employee and at least one Black employee of each sex. For each sex, we first calculate the average log earnings by race in a given firm and year, and subtract the firm average log earnings for Black employees from the firm average log earnings for White employees. We then collapse the average annual sex-specific within-firm earnings gaps across all firms within a given state.

Panel A of Figure 1 provides the state-level dispersion of within-firm earnings gaps among males. We see a large spatial dispersion, ranging from 17.5 log points in West Virginia up to 44.3 log points in Washington DC. Using the same color scale, panel B of Figure 1 provides the dispersion for females. With much lower levels, gaps among females are as widely dispersed, with a high correlation of state-level gaps among males and females on the order of 0.90.

Next, we assess the frequency of job mobility by race and sex. To do so, we investigate the number of firms an employee was associated with in our analysis horizon in terms of highest W-2 in a given year. In panel A of Figure 2 we plot the histogram of the number of employers for White individuals by sex, and in panel B we plot the differences in the histogram between Black and White individuals split by sex. We overall find that job mobility is meaningfully more frequent among Black employees, so that the incidence of the job transitions effects that we identify is accordingly more pervasive among them.

4. Analytical Framework

We use a dynamic model of job transitions and earnings determination that allows for general patterns of job-history dependence that can vary by race. We develop the model, lay out the estimation strategy, and define our key parameters of interest.

4.1. Earnings Determination Model and Treatment Effect

[2] These shares are statistically different from those among the full sample given the precision from the sample size.

Earnings Determination Process. Consider worker i from racial subgroup $g \in \{W, B\}$ who works in firm j in year t. We let $J(i,t)$ index the employer of worker i in year t, and we let $\boldsymbol{h} = (J(i,1), J(i,2), \dots, J(i,t))$ denote an employment history vector that indicates the history of firms in which i worked in each year $1, \dots, t$. We can define a set of potential outcomes $y_{g,i,t}(\boldsymbol{h})$, that is, the outcome $y_{g,i,t}$ that would occur under history $\boldsymbol{h}$.

We let the earnings generation process follow the structure:

$$y_{g,i,t}(\boldsymbol{h}) = \alpha_{g,i} + \gamma_{g,t} + x_{g,i,t}\beta_g + \theta_j^g(\boldsymbol{h}) + \varepsilon_{g,i,t}, \tag{1}$$

where $y_{g,i,t}(\boldsymbol{h})$ is log earnings in year t of worker i from group g with employment history $\boldsymbol{h}$. $\alpha_{g,i}$ is a person fixed effect, $\gamma_{g,t}$ is a race-specific calendar year fixed effect, $x_{g,i,t}$ is a third-order polynomial in age, and $\varepsilon_{g,i,t}$ is a time-varying error component. $\theta_j^g(\boldsymbol{h})$ is the firm earnings premium paid by employer $j = J(i,t)$ to a worker in group g with employment history $\boldsymbol{h}$. The dependence of the firm component on a worker's employment history is a key departure from the workhorse two-way fixed effects model, and we also let the earnings determination process vary by race. As we discuss in detail below, this structure allows for flexible transition patterns and meaningfully relaxes the identifying assumptions required in prior work.

Firm Effects in Job Ladder Transitions. We define cohorts of job switchers, indexed by c, according to their origin firm $o(c)$, destination firm $d(c)$, and move year $m(c)$. We let $\Omega_{c,g}$ denote the set of movers from group g in cohort c, where we consider employees who are in their origin firm in periods $t < m$, in their destination firm in periods $t > m$, and may be in either their origin firm or their destination firm in period $t = m$. To focus on event time when studying movers, we let $r(c,t) \equiv t - m(c)$ index periods relative to a cohort's move period. We let $\boldsymbol{h_c^0}$ denote a counterfactual history in which all elements are a cohort's origin firm $o(c)$, and we let $\boldsymbol{h_c}$ denote the observed history.

Next, we rewrite equation (1) for workers from group g in mover cohort c as:

$$y_{g,i,r} = \sigma_{g,i} + \gamma_{g,c,r} + x_{g,i,r}\beta_g + [\theta_j^g(\boldsymbol{h}_c) - \theta_{o(c)}^g(\boldsymbol{h_c^0})]\, \mathbb{I}\{r(i,t) > 0\} + \varepsilon_{g,i,r}, \tag{2}$$

where $\sigma_{g,i} \equiv \alpha_{g,i} + \theta_{o(c)}^g(\boldsymbol{h_c^0})$, and $\mathbb{I}\{\cdot\}$ is the indicator function. We can further generalize the model to allow dynamics in firm effects $\theta_{j,r}^g(\boldsymbol{h}_c)$. We define the treatment effect of a job transition among movers from group g in cohort c as:

$$\tau_c^g \equiv \theta_{d(c)}^g(\boldsymbol{h_c}) - \theta_{o(c)}^g(\boldsymbol{h_c^0}). \tag{3}$$

We denote the difference in average earnings among all workers (of either race) between the destination employer and the origin employer in the year of the move for a given cohort, $m(c)$, by:

$$\Delta_c \equiv E_i[\log y_{g,i,t} \mid J(i, m(c)) = d(c)] - E_i[\log y_{g,i,t} \mid J(i, m(c)) = o(c)].$$

We refer to Δ_c as the "treatment intensity." In calculating Δ_c in practice, we exclude a mover's own observation (that is, we use "leave-out" means) to avoid mechanical correlations in the estimation. Finally, we scale to define our key parameter of interest, λ_c^g, as the share of the earnings gaps across jobs that is causally attributable to employer effects:

$$\lambda_c^g = \frac{\tau_c^g}{\Delta_c}.$$

λ_c^g is a race-specific passthrough rate that governs the earnings dynamics at job transitions.

This parameterization yields our estimating event-study equation:

$$y_{g,i,r} = \sigma_{g,i} + \gamma_{g,c,r} + x_{g,i,r}\beta_g + \lambda_{c,r}^g \mathbb{I}\{r(i,t) > 0\}\Delta_c + \varepsilon_{g,i,r}, \quad (4)$$

which explicitly allows for dynamic treatment effects (since $\lambda_{c,r}^g$ is indexed by r). Our goal is to estimate $\lambda_{c,r}^g$, separately for Black and White workers ($g \in \{W, B\}$). The model in equation (4) is specified with a high degree of flexibility. It lets passthrough rates $\lambda_{c,r}^g$ vary by: i) race (g), our main focus; ii) time relative to the move (r), thus explicitly modeling dynamics in firm effects; and iii) cohort (c), thus allowing for heterogenous treatment effect across cohorts. In practice, we simplify iii) substantially by splitting cohorts based on the directionality of the move, which we classify with the notation $\bar{c} \in \{c^-, c^+\}$. That is, we categorize job transitions into transitions up the job ladder when $\Delta_c \geq 0$, with corresponding race-specific passthroughs $\lambda_{c^+}^g$; and transitions down the job ladder when $\Delta_c < 0$, with corresponding race-specific passthroughs $\lambda_{c^-}^g$. By doing so, we focus on "career progression" versus "career setbacks" and how their effects differ by race. This formulation assumes similar race-specific passthrough rates within moves of the same directionality across the different mover cohorts. As we will show in the empirical analysis, the data closely support this assumption of piecewise log linearity, or, in other words, of approximately constant elasticities within each directionality range. As such, we show that we can reduce the very high level of complexity of equation (1), which allows for firm effects for any history, into to four key parameters that characterize earnings dynamics by the direction of move and race.

4.2. Identification

4.2.1. Estimand and Identifying Assumptions

We define our estimand to be:

$$T_{c',r} \equiv E_{i \in \Omega_{c',g}}\left[y_{g,i,r} - y_{g,i,-1}\right] - E_{i \in \Omega_{c'',g}}\left[y_{g,i,r} - y_{g,i,-1}\right],$$

where c' is a "treatment" cohort and c'' is a "control" cohort with zero treatment intensity, i.e., $\Delta_c = 0$. Our estimating equation in (4) is a log-linear aggregation of the pairwise difference-in-differences comparisons in the form of $T_{c',r}$ within each class of moves (i.e., either up or down the job ladder). We later provide specification tests that strongly support the piecewise log-linear formulation.

It is important to explicitly describe the thought experiment that is at the root of our identification strategy. Identification does not rely on comparisons of movers and non-movers which would require moves to be exogenous. Rather, our approach compares across mover cohorts who differ in the treatment intensity they experience. For a treated cohort in a given directionality (up or down the job ladder), the hypothetical effective control group is a cohort whose treatment intensity converges to 0 (from below in downward transitions and from above in upward transitions). For example, the treatment effect in an upward transition of a mover cohort c' with $\Delta_{c'} > 0$ is recovered by comparisons to mover cohorts c'' with low-intensity upward moves ($\Delta_{c''} \rightarrow 0^{+}$). Equation (4) then aggregates these hypothetical comparisons using a functional form so that identification is coming from the full variation in Δ_c across moves in a given direction.

Next, to clarify the exposition of the identifying assumptions, we explicitly decompose the error term in equation (4) into a conditional match-specific sorting component and a residual:

$$\varepsilon_{g,i,r} = \kappa_{g,i} \times \mathbb{I}\{r(i,t) > 0\} + u_{g,i,r},$$

where $\kappa_{g,i}$ comes from projecting $\varepsilon_{g,i,r}$ onto a constant post-move, so that $E_i(u_{g,i,r}) = 0$. We can then map our estimand to the model parameters as follows:

$$T_{c',r} \equiv \underbrace{\lambda^{g}_{c',r}\Delta_{c'}}_{\substack{\textbf{Treatment}\\ \textbf{Effect}}} + \underbrace{\{\gamma_{g,c',r} - \gamma_{g,c'',r}\}}_{\textbf{Time}} + \underbrace{\left\{E_{i\in\Omega_{c',g}}[\kappa_{g,i}] - E_{i\in\Omega_{c'',g}}[\kappa_{g,i}]\right\}}_{\textbf{Sorting}}.$$

This immediately leads to our identifying assumptions.

Identifying Assumptions: *For mover cohort pairs c' and c'', the following holds:*

A.1: $\gamma_{g,c',r} - \gamma_{g,c'',r} = 0$. ***[Conditional Parallel Time Trends]***

A.2: $E_{i\in\Omega_{c',g}}[\kappa_{g,i}] - E_{i\in\Omega_{c'',g}}[\kappa_{g,i}] = 0$. ***[Non-Differential Conditional Sorting]***

Several points are worth discussing. We note that these assumptions are required to hold conditional on our flexible set of included effects: an individual fixed effect, a job-history-dependent firm effect (that can also be dynamic), calendar year indicators, and indicators for time relative to the job move. As such, the model specification combined with the event-study style estimating strategy relaxes traditional exogenous mobility assumptions in important ways. This is because the richness of the framework requires very particular types of remaining selection not to be present: allowing for a common relative time factor, there should be no differential selection on conditional match-specific components across mover cohorts (with differential treatment intensity Δ_c) within a move directionality (i.e., either up or down the job ladder) within race (g). Accordingly, our model allows for: i) systematic selection across movers and non-movers by racial group, so that moves need not be "exogenous"; ii) rich firm-specific complementarities with employee types as captured in a reduced-form way by employment history vector; and iii) the estimation further allows for differential selection into upward and downward transitions by race group.

4.2.2. Identification Tests

Our model specification and the richness of data allow us to develop various tests that jointly test our model and its identifying assumptions.

Assumption A.1 lends itself to the traditional testable implication of parallel trends in the periods prior to the move. In our setting, the test takes the following form:

Test for Assumption A.1: "Parallel Pre-Trends." *Under Assumption A.1 the following holds for each* $g \in \{W, B\}$*:*

a) Across classes of moves: $\gamma^g_{c^+, r} = \gamma^g_{c^-, r}$ *for all* $r < 0$.

b) Within a class of moves, across different cohorts (Δ_c)*:* $\lambda^g_{c,r} = 0$ *for* $r < 0$.

More interestingly, we can develop an innovative test in our setting that additionally leverages "post-trends" instead. Under the earnings determination model in equation (4), we can devise counterfactuals for the post-move periods to establish testable implications to check the validity of Assumption A.2. Specifically, our model implies that movers from different cohorts would exhibit similar trends in the case of no treatment intensity ($\Delta_c = 0$). Our approach that splits cohorts into moves up the job ladder and down the job ladder lends itself to such a test for counterfactuals across cohort classes. To the best of our knowledge, this simple test that we summarize in the following, offers a tractable new way to assess selection on match-specific components in designs that leverage job mobility as the identifying variation.

Test for Assumption A.2: "Parallel Post-Trends." *Assumption A.2 implies that, for each* $g \in \{W, B\}$*, in the counterfactual of* $\Delta_c = 0$*, it must be that* $\gamma^g_{c^+, r} = \gamma^g_{c^-, r}$ for $r > 0$.

4.3. Key Parameters of Racial Inequality

We define our key parameters of interest as follows.

Definition 1: Race-Specific Asymmetry Factors. *For each racial group* $g \in \{W, B\}$*, the pay asymmetry across transitions down the job ladder and transitions up the job ladder is captured by:*

(5) $$\mu^g_r \equiv \frac{\lambda^g_{c^-,r}}{\lambda^g_{c^+,r}} - 1.$$

Passthrough rates ($\lambda^g_{\bar{c},r}$) are nonnegative and the sign of passthrough dollars in the post-move periods ($\tau^g_{\bar{c},r} = \lambda^g_{\bar{c},r}\Delta_{\bar{c},}$) is governed by the sign of $\Delta_{\bar{c},}$. μ^g_r therefore measures, for each race, the degree to which downward transitions are penalized relative to upward transitions per \$1 of change in earnings.

Definition 2: Relative Earnings Differentials in Race. *For each transition class* $\bar{c} \in \{c^-, c^+\}$*, the racial pay differential is captured by:*

(6) $$\tau_{\bar{c},r} \equiv \frac{\tau^B_{\bar{c},r}}{\tau^W_{\bar{c},r}} - 1 = \frac{\lambda^B_{\bar{c},r}}{\lambda^W_{\bar{c},r}} - 1.$$

*$\tau_{c^+,r}$ is a **racial premium** when transitions are up the job ladder ($\Delta_c \geq 0$), and $\tau_{c^-,r}$ is a **racial penalty** when transitions are down the job ladder ($\Delta_c < 0$).*

The relative earnings differential, $\tau_{\bar{c},r}$, is at the core of our analysis. It means that, for every \$1 of pay change for a White employee, a Black employee experiences an additional change of \$$\tau_{\bar{c},r}$. Specifically, in a transition down the job ladder ($\Delta_c < 0$), $\tau_{c^-,r}$ is a racial penalty in absolute value: it is the additional decline a Black worker experiences for every \$1 earnings decline in a White worker's pay.

4.4. Connection to Frameworks in the Literature

The earnings determination model set forth by Abowd, Kramarz, and Margolis (1999), which has become the workhorse model (commonly referred to as AKM), is a special case of the model in equation (1). It is the case when there is no history dependence, so that $\theta^g_j(\boldsymbol{h}) = \theta^g_j$ and each firm's earnings premium reduces to one scalar across all history vectors $\boldsymbol{h}$. Several points are worth noting. Our approach assesses the impact of firms via passthroughs. As such, we avoid the need for normalizations, which poses an identification problem in standard wage gap decompositions that are based on AKM estimations. Specifically, the contributions of differences in individual fixed effects and differences in firm premia are not separately identifiable, and accordingly they are not invariant to the researcher's choice of reference groups (Oaxaca and Ransom 1999, Card, Cardoso, and Kline 2016, Gerard, Lagos, Severnini, and Card 2021). Our approach is invariant to normalizations for the simple reason that it differences out baseline differentials, as our estimates map to difference-in-differences estimators. Additionally, we focus on first moments rather than second moments, so we do not need to adjust the plug-in estimators to address biases from measurement error due to the well-known "limited mobility bias" (see extensive discussions and analyses in Card et al. 2018 and Bonhomme et al. 2023).

Another well-known limitation of the baseline two-way fixed effect model is the absence of interactions between worker and firm attributes that restricts complementarity patterns in earnings. The standard model is also static, in the sense that worker mobility does not depend on earnings realizations conditional on worker and firm heterogeneity, and that earnings after a job move do not depend on the previous firm. Bonhomme, Lamadon, and Manresa (2019) propose a framework that deals with these issues when the object of analysis, differently from ours, is estimation of earnings distributions and worker composition (i.e., sorting). To identify and estimate our moments of interest, as defined above, in a way that tackles the limitations of the baseline model, we leverage a combination of the tractability of the baseline model with methods developed in the context of households' migration across space that share deep similarities. The "mover design" that leverages household migration has been developed in the healthcare context in a series of influential studies (specifically, Finkelstein, Gentzkow, and Williams 2016; Finkelstein et al. 2022). The core advantage in bringing together the two strands of the literature that mostly evolved in separation stems from event-study like approach to the analysis. It offers a transparent framework with visual clarity and assessment of identifying assumptions, avoids the need for ad-hoc normalizations, and enriches the possible firm-employee interactions and analyses in tractable ways.

5. Empirical Evidence: Job Transitions and Race-Specific Earnings Dynamics

We now turn to our empirical analysis.

5.1. Job Transitions Treatment Intensity

It is useful to begin by characterizing the "treatment intensity" of job transitions, Δ_c, and how it may vary across Black and White workers. Figure 3 summarizes this analysis in several ways.

Panel A shows that the mean and standard deviation are 7.8 and 42.5 log points among Black workers and 6 and 42.7 log points among White workers. Panel B plots the probability density function of Δ_c, separately among Black and White job movers. Finally, panel C compares the two distributions by plotting the decile values of Δ_c for Black job switchers against the decile values of Δ_c for White job switchers. Overall, the distributions of treatment intensity are very similar across race, so that differences in outcomes across race cannot be attributed to differential treatment intensity. Further, our analysis that is race-specific will inherently not be prone to the small differences that remain.

5.2. Dynamic Effects of Job Mobility: Pooled Sample

We first estimate equation (4) on the entire sample of movers, pooling across race and sex. The results are provided in panel A of Figure 4. In this and subsequent figures, the x-axis represents time relative to the job move in years, and on the y-axis we plot estimates for the passthrough rates over time along with their 95-percent confidence intervals. Period 0 is a transition period because the employee is not yet exposed to the treatment for the full year and recall that our earnings measures are annual given the tax data we use. Hence, movers' earnings should have mechanically less systematic correlation with our measure of move intensity in that year.

We can first see that the trends in the pre-period are essentially completely flat, in strong support of the identifying assumption as we discuss in more detail below (in Section 5.4). There is then a clear jump in passthroughs during the first year following the move, which displays a strong persistence over the analysis horizon of 5 years. As laid out in the analytical framework, these findings imply meaningful firm effects in earnings determination. They imply that approximately 20 percent of the cross-job earnings gaps nationwide are causally attributable to firms. Panel B of Figure 4 replicates the estimation on a balanced sample of job switchers that we observe for a complete segment of the analysis horizon, which we narrow to 7 years around the move to limit the decline in sample size. The results are practically the same.

Before we proceed to our key analysis of firm effects by race in job ladder transitions, we investigate heterogeneity in treatment effects across the household income distribution. We are specifically interested in uncovering whether some households are more prone to firm effects, which could help identify pathways for addressing earnings inequality. To do so, we categorize households based on their overall income as measured by "Total Money Income" (TMI) from 1040s.[3] We split the sample of movers into quintiles of log TMI in the year prior to the move, and we estimate a mean version of equation (4) that aggregates periods into pre-move and post-move, separately for each income group. Figure 5 shows that the passthrough rate meaningfully declines in household income, ranging from 0.25 in the bottom quintile to 0.18 in the top quintile. This pattern implies that firm premia have a larger role in the earnings dynamics of lower-income households, which in turn helps deepen our understanding of what shapes earnings inequality.

5.3. Job Ladder Transitions and Race

Next, we estimate specifications of equation (4), where we split transitions by the direction of the move, and we conduct the estimation for each race separately. To draw parallel comparisons across specifications, we zoom in on "stable" job transition episodes that have clear dynamics as set forth in the analytical framework. That is, we look at mover cohorts who, within the analysis horizon of seven years around their move in year m, are in their origin firm in periods $t < m$, in their destination firm in periods $t > m$, and may be in either their origin or their destination firm in $t = m$ (the transition year).[4]

Figure 6 plots the dynamics of the estimates for the passthrough rates $\lambda^{g}_{\bar{c},r}$, separately by race and directionality of move. Panel A provides estimates for White workers who move either up or down the job ladder, and panel B provides estimates for Black workers who move either up or down the job ladder. We first note that parallel pre-trends nicely hold across all specifications. Focusing on White workers, we see that downward job transitions have systematically higher passthrough rates than upward job transitions.

[3] The 1040 extracts provided by the IRS include information on a constructed measure of Total Money Income (TMI). TMI is the sum of taxable wage and salary income, interest (taxable and tax-exempt), dividends, gross Social Security income, unemployment compensation, alimony received, business income or losses (including for partnerships and S-corps), farm income or losses, and net rent, royalty, and estate and trust income. Prior to tax year 2018, TMI also included total pensions and annuities. However, this was removed from TMI due to a change to income reporting on the Form 1040 and the regulations regarding data sharing between IRS and the Census Bureau.

[4] Recall that in the transition period movers' earnings should have mechanically less systematic correlation with our measure of move intensity in that year. This is even more relevant when we split moves by their directionality. Since there are mechanical declines in earnings from the main employer in the transition year, in upward moves (where Δ_c is positive) the regression will load these declines onto the passthrough in that year, $\lambda^{g}_{c^+,0}$, which could take a negative sign to match data. See Figure 6 where such a pattern arises.

This translates to asymmetry in earnings dynamics for moves that are up the job ladder or down the job ladder. Moving to Black workers, we see even more pronounced patterns, which imply a stronger asymmetry. Comparing across race, we see that passthroughs in transitions up the job ladder are quite similar, but that passthroughs in transitions down the job ladder are substantially higher for Black workers. We provide dynamic estimates for these differentials in panel C, which plots the Black-White passthrough gaps for both transitions up and down the job ladder. Indeed, there are no systematic differentials for positive moves, but there is a significant additional bump in passthroughs for negative moves among Black workers. This implies a racial penalty in job ladder transitions that involve a career setback, as we discuss in Section 6.

5.4. Identification Tests

We conclude this section by investigating the validity of our model and design based on the tests we provided in Section 4. The tests rely both on estimates of passthroughs, $\lambda^{g}_{c,r}$, and on estimates of time patterns relative to the move, $\gamma^{g}_{c,r}$. Beginning with the first set, recall that the Test for Assumption A.1 (Parallel Pre-Trends) requires that all estimates of $\lambda^{g}_{c,r}$ in periods $r < 0$ should be 0 within a class of moves. Figures 4 and 6 show that this test is satisfied whether estimated on the whole sample, on a balanced sample, or on the four subsamples of race-directionality combinations.

The estimation for the second set of tests is provided in Figure 7 and considers comparisons across classes of moves within race. Recall that the test for Test for Assumption A.1 (Parallel Pre-Trends) requires that $\gamma^{g}_{c^+,r} = \gamma^{g}_{c^-,r}$ for all $r < 0$, and that the Test for Assumption A.2 (Parallel Post-Trends) similarly requires that $\gamma^{g}_{c^+,r} = \gamma^{g}_{c^-,r}$ but for $r > 0$. Figure 7 plots estimates for $\gamma^{W}_{c^+,r} - \gamma^{W}_{c^-,r}$ and $\gamma^{B}_{c^+,r} - \gamma^{B}_{c^-,r}$ showing that they are all statistically non-distinguishable from 0 for both races and across the entire analysis horizon. These tests overall provide strong support for the validity of our estimations, and we further provide model specification tests in the next section.

6. Racial Equality

We now proceed to estimate and discuss our key parameters of interest: the race-specific asymmetry factors (μ^{g}_{r}) and the racial premium/penalty ($\tau_{c^+,r}, \tau_{c^-,r}$). We provide estimates based on regressions of specification (4) for average effects. We include all job moves and aggregate observations from years -5 to -1 in the pre-period and observations from years 1 to 5 in the post-period (accordingly excluding the transition year 0).

6.1. Earnings Determination Dynamics: Race-Specific Reference Dependence

Panel A of Table 2 reports the average of the passthrough rate $\lambda^{g}_{\bar{c},r}$ for positive and negative transitions, separately by race. Within each race, we see asymmetry in the earnings effects of job mobility in transitions that are up versus down the job ladder. These asymmetry factors are meaningful: 26 percent for White workers and 53 percent for Black workers. The asymmetry around the directionality of the move lends itself to an earnings determination model of job transitions that has a reference-dependent shape around 0. We plot the functions of earnings processes that are implied by the estimates for $\lambda^{g}_{\bar{c},r}$ in Figure 8 (solid lines), and we also provide the counterfactual prediction if earnings determination were symmetric at the rate of the estimated slope for positive moves (dashed lines).

Further, we assess our parametric assumptions of piecewise log-linearity in Δ_c. The specification test assesses average deviations from the model specification by overlaying means of the estimation residuals on top of the linear predictions. Specifically, we estimate equation (4) for each of the four combination cells of $\bar{c} \in \{c^-, c^+\}$ and $g \in \{B, W\}$, and we take the residual from those estimations, which, in particular, include the individual fixed effects. Then, for each cell, we split the sample into ten equal-sized bins based on Δ_c. Within each of the resulting 20 bins per race (with a total of 40 bins), we average the residuals, and we add them onto the prediction in Figure 8 (in circles). We also provide the 95-percent confidence intervals of the averaged residuals, but they are not visible due to the large sample size and tight standard errors. Importantly, all confidence intervals systematically include the 0 point of average deviation.

This exercise shows that the data very closely follow the piecewise linear parametric specification in equation (4).[5] This finding provides new evidence for race-specific earnings determination processes of job transitions that follow a clear reference-dependent pattern. The reference point pivots around the zero point of transition intensity that is determined by an individual's work history. The findings also provide a considerable model simplification compared to the potential complexity of the fully generalized work history model in equation (1), by providing a minimal set of parameters to closely fit the earnings determination process; namely, four passthroughs by job ladder direction and race.

6.2. The Racial Penalty

Panel B of Table 2 reports, within each direction of transitions, the average passthrough rate for White workers and the differential in the passthrough rate for Black workers as compared to White workers. For positive transitions, the implied racial premium is essentially 0 (0.1 percent). That is, the change in earnings in transitions up the job ladder are one for one across race.

[5] It is useful to note that high explanatory power is common when individual fixed effects are included, and that close fits to log-linear relationships are quite common in economics (e.g., Chetty et al. 2016). In our view, it is the piecewise log-linear relationship with a clear reference dependence at zero which is the key innovative feature of this finding.

For negative transitions, however, the implied racial penalty is large and on the order of 24 percent. This means that Black workers who transition down the job ladder lose additional \$0.24 for every \$1 decrease in White workers' earnings. Figure 9 plots this relationship of the racial penalty, which pivots around the directionality of the move. Panel A depicts the racial penalty in terms of total earnings changes, and panel B displays the marginal racial penalty.

In Figure 10 we explore the intensity of the racial penalty across different subsamples. We benchmark these estimates against the baseline estimate of 5.4 pp among the overall sample of downward transitions (reported on the left in panel A). In panel A of Figure 10, we find that the racial penalty prevails across sex, with a larger point estimate among men than among women (6.7 pp as compared to 4.4 pp).[6] Recall that our main sample analyzes first job changes within our analysis horizon. In panel A of Figure 10 we report the racial penalty when we analyze second moves, which provides an estimate of 4.9 pp that is essentially similar in magnitude to that of first moves. Finally, in panel B of Figure 10, we split the sample by age groups, and we plot the passthrough rate in downward transitions over age separately by race. We clearly see that the racial penalty, the vertical gap between the two curves, prevails across the entire age distribution.

These findings, which are at the core of our analysis, provide novel evidence of career setbacks as a causal pathway of racial disparities in earnings trajectories. What mechanisms can explain the racial penalty? We explore several key candidate explanations next.

7. Mechanisms

7.1. Differential Sorting or Differential Pay?

We uncovered that Black workers bear a penalty in transitions down the job ladder as compared to White workers. Perhaps the most immediate question is whether this racial penalty in job transitions comes from unequal pay within the same employers or from downward transitions of similar intensity but across firms of different nature. That is, are the patterns driven by differential pay within firms or by differential sorting from differences in employment opportunities? Naturally, this is a key investigation for Equal Employment Opportunity (EEO) policies.

A direct way to investigate this question is to revert to a finer definition of cohorts, based on specific pairs of origin firm $o(c)$ and destination firm $d(c)$. We focus on origin-destination pairs among which at least one White worker and one Black worker transition during the sample period. We keep only those origin-destination pairs, average observations within each cell of "origin-destination" by "time relative to move" by "race," and run specifications of equation (4) that aggregate over pre-move periods and post-move periods. The extent to which the racial penalty remains is indicative of its sources. If it fully

[6] Appendix Table A.1 provides the full set of key parameters of racial equality from Table 2 split by sex.

disappears, the results would imply that the racial penalty in job transitions is driven by differential sorting in transitions down the job ladder, whereby Black workers transition to firms that extract more rent. If, at the other extreme, the penalty remains to the same extent, the results would point to unequal pay. An important feature of our analysis is that it contains all comparable pairs across the US nationally for over a decade (2005-2019), so that we characterize the universe of such matches, and our conclusions are "representative" by construction (and cannot be explained away by being driven by specific subsamples of the population).

Table 3 summarizes these results. In column 1 we include all moves that fall within this design, and in column 2 we narrow the sample to stable moves. The results provide clear patterns in that the racial penalty completely vanishes when we study racial differentials among the exact same transitions. This finding therefore implies that the racial penalty is driven by differential employment across race. Recall from the analytical framework that all specifications include fixed effects for the origin firm, so that the variation in treatment intensity in attributable to the destination firm. This means that, in career setbacks, Black workers sort into types of firms which penalize pay at a higher rate compared to their White workers counterparts. It is not that the same firms extract more rent from Black workers, but rather that a Black worker who experiences a career setback transitions to firms that extract more rent at a higher rate.

7.2. Characteristics of Firms that Movers Sort Into

With that in mind, we explore several firm characteristics of the employers that Black workers transition to. We study correlations of the racial penalty with characteristics of the destination firm. We estimate specifications of equation (4) which we allow to interact with race and with a given characteristic (one at a time). Panel A of Figure 11 summarizes the analysis. Appendix Figure A.1 also provides the correlations of the passthrough rate in downward transitions among White workers for completeness.

In our context of racial earnings gaps, a natural characteristic to investigate is the share of Black employees in the destination firm. Indeed, we find that sorting into firms with a higher representation of Black employees is associated with a substantial reduction in the racial penalty in career setbacks. We similarly find that sorting to firms with government status is associated with a meaningfully lower racial penalty. In fact, we find that "Public Administration" is the only industry category of the destination firm that displays no racial penalty (in both economic and statistical significance). This is shown in panel B of Figure 11 that provides estimates of the racial penalty by industry.

We see no correlation with wage dispersion within the firm, which is in line with the observation that the penalty is not driven by differential pay. Wage dispersion is a common measure for inequality, and we see that this type of differential pay within a firm does not translate to differential pay by race. Similarly, with respect to firm age, models of discrimination traditionally predict that firms whose pay differentials are not based on productivity are less likely to survive in the presence of market competition; and here we find, if anything, a pattern in the opposite direction. We see no correlations with firm size, in terms of either the number of employees or payroll.

7.3. What Drives Differential Sorting: Demand or Supply?

The differential sorting of Black workers in career setbacks can be driven by supply-side factors, i.e., the workers, and demand-side factors, i.e., the employers. For each side of the market, we study leading potential explanations ingrained in economic modeling that the literature on disparities discusses.

Supply Side (Workers). The work on job loss and unemployment has emphasized liquidity constraints as a major factor that can determine post-layoff labor market outcomes and dynamics. Liquidity constrained households face higher pressure to find a job quickly, which potentially pushes laid-off workers to accept worse employment offers; they have higher short-run consumption needs from their lack of ability to smooth consumption. Indeed, Chetty (2008) finds in the US context that unemployment insurance (UI) benefits affect search behavior predominantly via the mitigation of short-run liquidity needs. As such, upon job loss, tighter liquidity constraints can induce shorter search spells and higher acceptance rates of worse offers. In Coyne, Fadlon, and Porzio (2024) we show how higher reliance on withdrawals from retirement accounts can reveal the households' valuation of liquidity and the lack of access to formal credit options. We also show that Black households rely on retirement account withdrawals to a higher extent than White households following unemployment. This suggests that liquidity constraints could play an important role in explaining the racial penalty that we uncover in this paper.

To test this hypothesis, we leverage extracts from Form 1099-R ("Distributions From Pensions, Annuities, Retirement or Profit-Sharing Plans, IRAs, Insurance Contracts, etc.") provided by the IRS. We merge 1099-R information to our sample of movers to identify the use of withdrawals at the time of a downward transition, and we study the takeup of distributions around the move. We then want to study whether a similar treatment intensity of the career setback, as measured by Δ_c, has differential effect on the takeup of retirement account distributions by race. This maps to a comparison of $\lambda^{W}_{c^-,r}$ and $\lambda^{B}_{c^-,r}$ from equation (4), in which the outcome variable is an indicator for having 1099-R distributions in a given year.

Figure 12 displays the results, providing strong support for the liquidity hypothesis. In panel A, we first plot $\lambda^{W}_{c^-,r}$ to investigate the behavior of White households around a transition down the job ladder. There is a clear strong relationship between the intensity of the setback and takeup of withdrawals: a \$1 increase in treatment intensity in downward transitions (a decrease in Δ_{c^-}), which leads to an average decline of \$0.23 in own earnings, also leads to a 5.4 percentage point (pp) increase in the takeup of withdrawals. This increased takeup is still persistent after 5 years. More importantly for our purposes, we next study in panel B of Figure 12 the differentials in withdrawals takeup across Black and White employees who experience a similar intensity setback, that is, $\lambda^{B}_{c^-,r} - \lambda^{W}_{c^-,r}$. Indeed, in strong support of the hypothesis of differential liquidity constraints, we find clear patterns of intertemporal substitution of available funds that differ by race. The withdrawal rate per unit of treatment is twice as high in the immediate run of the year of the adverse event among Black workers (with an additional 5.5 pp increase). In later periods, the differential falls below that of White workers, further supporting the view that Black workers use withdrawals for short-run liquidity needs (in lieu of future periods as compared to their White counterparts). This evidence is suggestive of short-run liquidity needs creating long-run earnings gaps. In turn, these findings suggest that more generous unemployment benefits (which are race neutral) can help mitigate the racial penalty, as they would have higher effects on households who have a greater need for injection of short-run liquidity.

An additional form of supply-side channel that the literature has considered as a potential mitigator for adverse labor market events is family labor supply responses. In our context, we are interested in investigating whether household labor supply responses may differ by race. We extend our analysis to the household level by studying the spouse's labor supply responses. We provide the analysis in Table 4 using the specifications of equation (4), where we aggregate observations from years -5 to -1 in the pre-period and observations from years 1 to 5 in the post-period. In this analysis, we include married male workers who experienced a downward job move, and we analyze the earnings of both the worker who switched jobs and their spouse. We identify movers who were "married, filing jointly" using 1040s. We then match the spouse to W-2 records to identify their earnings. If the spouse did not have W-2 earnings, their earnings were coded as 0. For the spouse, we study log earnings using $\log(x + 1)$ to account for 0 earnings.

Starting with White workers, we find in Table 4 that spouses meaningfully mitigate the penalty of a downward transition. The negative estimate of their change in earnings implies an increase in wages (since for these households $\Delta_c < 0$), which reduces the downward penalty by 18 percent (=0.04075/0.2292). However, while married households exhibit a large racial penalty of 33 percent (=0.07531/0.2292), the spousal mitigation is not differential by race. This implies that while spousal earnings responses reduce the penalty in downward transitions, they do not further mitigate the racial penalty in these career setbacks.

Demand Side (Employers). A classic conjecture in the labor economics literature in the context of individuals' adverse employment history and subsequent hiring outcomes is signaling to employers. In their large scale resume audit study, Kroft, Lange, and Notowidigdo (2013) study the role of employer behavior in generating the "negative duration dependence" in unemployment, that is, the adverse effect of a longer unemployment spell on future employment. They find that the likelihood of receiving a callback significantly decreases with the length of a worker's unemployment spell, and that this effect is meaningfully mitigated when unemployment rates are higher. As they discuss, this result is consistent with the prediction of a broad class of screening models in which employers use the unemployment spell length as a signal of unobserved productivity, and recognize that this signal is less informative in weak labor markets.

A similar logic extends to our context of career setbacks and race: if career setbacks are used as a signal of unobserved productivity, and if it is perceived differentially across employee race by prospective employers, the racial penalty would be mitigated when the signal is less informative in adverse aggregate labor market conditions. That is, local unemployment shocks that lead to separations may shift the interpretation of the signal away from attribution to the individual's productivity.

To study the effect of a career setback in distressed labor markets, we follow the methodology in Yagan (2019) and utilize spatial variation in the effects of the Great Recession. We study the earnings effects of commuting zone (CZ) level unemployment shocks, separately by race, among our sample. We estimate:

$$y_{g,i,t} = \sigma_{g,i} + \rho_z + x_{g,i,t}\beta_g + \textstyle\sum_{t\neq 2006}\gamma_t^g \times I_t + \sum_{r\neq -1}\lambda_t^g \times I_t \times \Delta_z + \varepsilon_{g,i,t}. \tag{7}$$

I_t are calendar year indicators running from years 2005 to 2019, where year 2006 is taken to be the baseline year and year 2007 is the year of the start of the event. ρ_z are commuting zone fixed effects, and $x_{g,i,t}$ include age fixed effects. Δ_z is the treatment intensity of location z in terms of the unemployment shock. Specifically, we utilize the measures from Yagan (2019), who considers the change in a commuting zone's unemployment rate between the years 2007 and 2009. Standard errors are clustered at the CZ level. Our parameters of interest are λ_t^g, which capture the relative change in log earnings in a locality that is exposed to a 1 pp higher local unemployment shock, and we are particularly interested in examining how the patterns compare across employee race.

For data related reasons, we provide three versions of this analysis in Figure 13. In panel A, we estimate equation (7) on our entire sample of movers and non-movers. In panel B, we focus on workers with downward moves in years 2007-2009. In these two estimations, we use W-2 data as we have done so far. This also has the advantage of including the more fine-grained firm fixed effects instead of location fixed effects, but comes at the expense of only limited pre-event periods, since the earliest available year of W-2s is 2005 in our data. We address this limitation by matching the entire 2000-2019 horizon to 1040s instead. We use wage and salary income from 1040s as the outcome variable, and include CZ fixed effects instead of firm fixed effects since 1040s do not include EINs. We use the full sample of Black workers, but because of processing limitations we restrict the sample of White workers by taking a random 25 percent sample in each year. This version is reported in panel C.

Across all specification in Figure 13, we reach similar conclusions. First, we see larger reductions in log earnings among employees in commuting zones that experience larger unemployment shocks. More importantly for our analysis, unlike "normal" times, the patterns for Black and White employees show no differentials. This pattern is consistent with differential signaling as there is a racial penalty in typical times but there is no penalty in earnings reductions in market-level employment shocks. It provides suggestive evidence that differential signaling by race could hinder the employment opportunities of Black workers in career setbacks as a channel underlying the racial penalty that we find.

We overall conclude that the racial penalty in job transitions does not reflect unequal pay by the same employers, but it rather reflects differential sorting and employment opportunities in downward transitions. The evidence suggests that, in typical career setbacks, Black employees are less likely to find employment in higher paying firms. For explanations of this racial sorting, we find strong support for differential short-run liquidity needs and credit constraints, and we also find suggestive evidence that career setbacks act as productivity signals that employers interpret differentially by the race of the worker.

8. Conclusion

Using population-wide administrative data, we study the role of job transitions and firm earnings premia in the Black-White earnings gap in the US. We provide a robust empirical model for the earnings determination process, which we show to have a reference-dependent shape around the directionality of a job move along with a high racial penalty in downward transitions. We find that Black workers who experience a transition down the job ladder lose an additional \$0.24 for every \$1 decrease in White workers' pay. We explore the sources of this racial penalty, offering insights for effective social protection policies in the American labor market. To mitigate the penalty, our findings point to focusing on hiring processes rather than on potential violations of equal pay laws, and motivate considering more generous unemployment benefits since we find strong support for short-run liquidity needs as an underlying channel.

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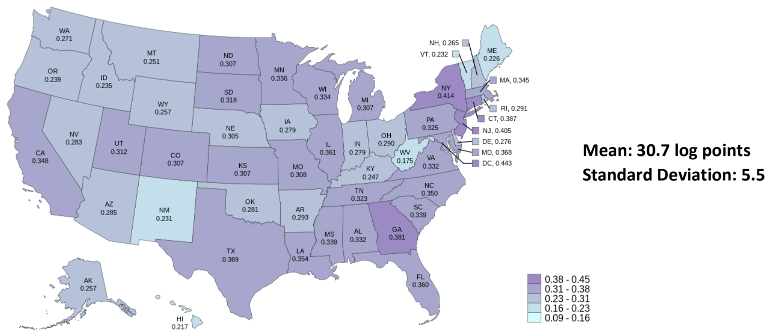
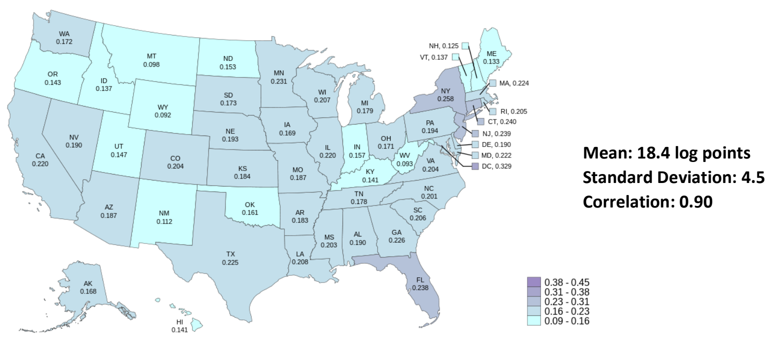
Figure 1: Dispersion of within-Firm Racial Earnings Gaps by State

A. Males

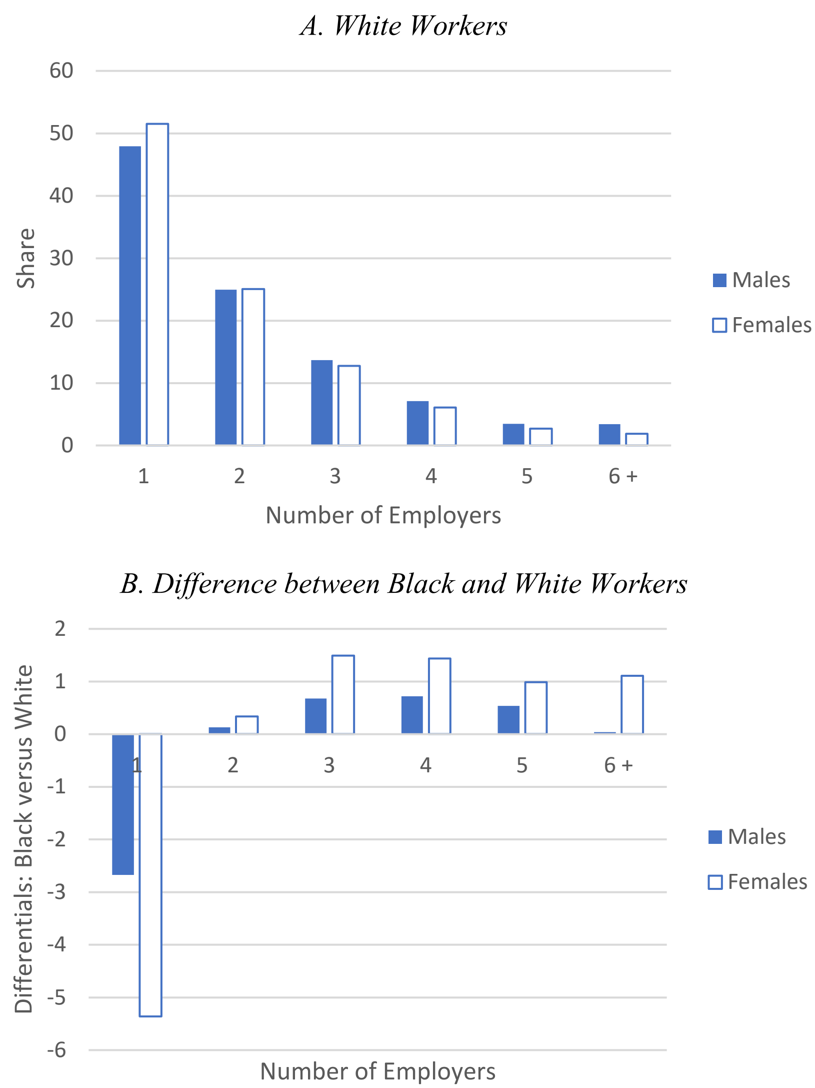


B. Females



Notes: This figure plots average within-firm Black-White earnings gaps by state for the years 2005-2019, separately for males and females.

Figure 2: Frequency of Job Mobility by Race and Sex



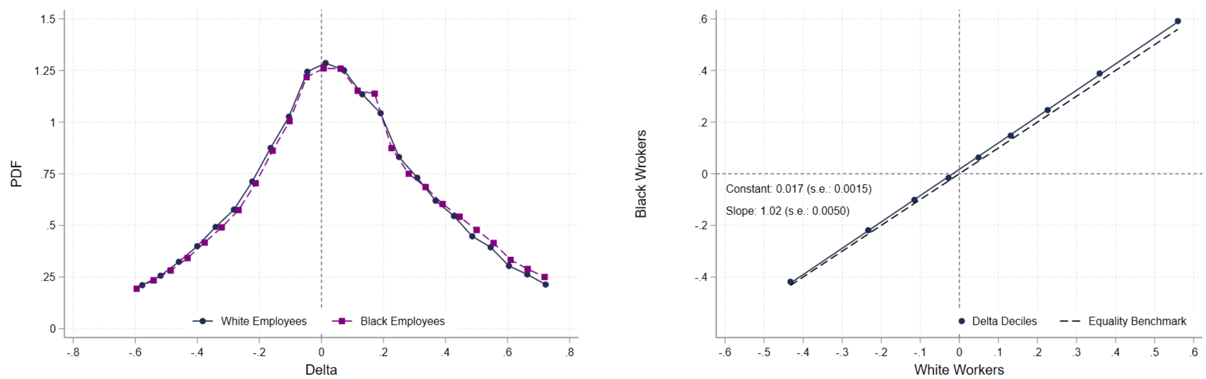
Notes: This figure studies the frequency of job mobility by race and sex. We investigate the number of firms an employee was associated with in our analysis period of years 2005-2019 in terms of highest W-2 in a given year. In panel A we plot the histogram of the number of employers for White individuals by sex, and in panel B we plot the differences in the histogram between Black and White individuals split by sex.

Figure 3: Job Transitions Statistics—Treatment Intensity Δ_c

A. Summary Statistics

	Mean	SD	N
Black workers	0.078	0.425	3,765,000
White workers	0.060	0.428	18,800,000

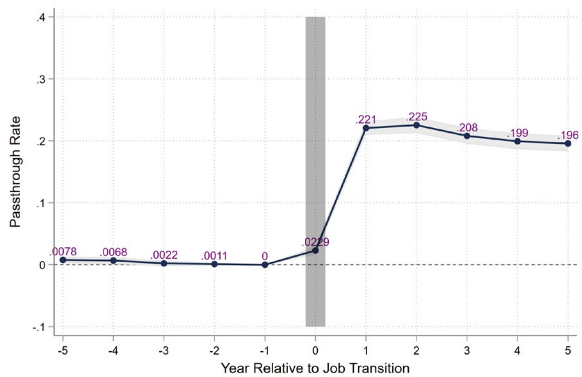
B. PDF by Race *C. Decile Comparison*



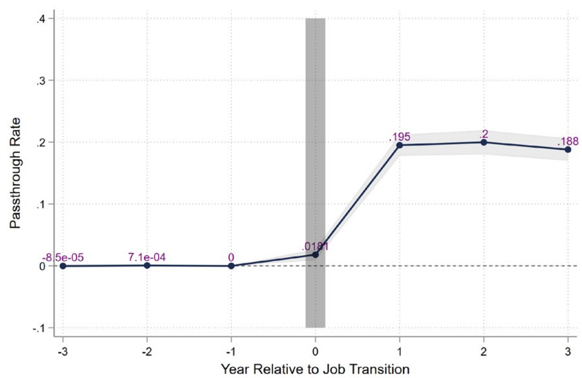
Notes: This figure characterizes the "treatment intensity" of job transitions, Δ_c, and how it varies across Black and White workers. Panel A shows the mean and standard deviation among Black workers and White workers. Panel B plots the probability density function of Δ_c, separately among Black and White job movers. Panel C compares the two distributions by plotting the decile values of Δ_c for Black job switchers against the decile values of Δ_c for White job switchers.

Figure 4: Dynamic Effects of Job Mobility

A. Entire Sample

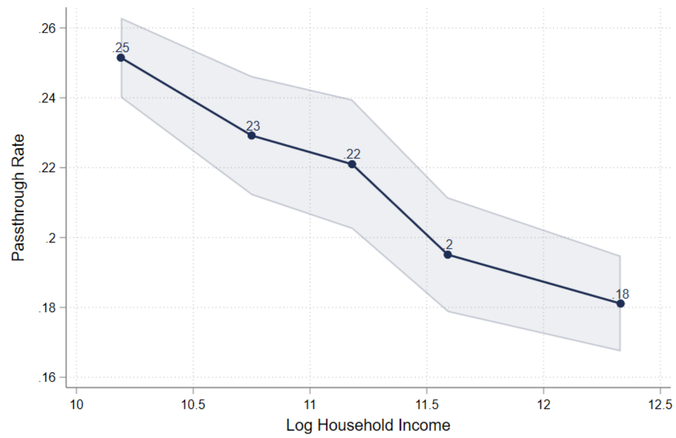


B. Balanced Sample



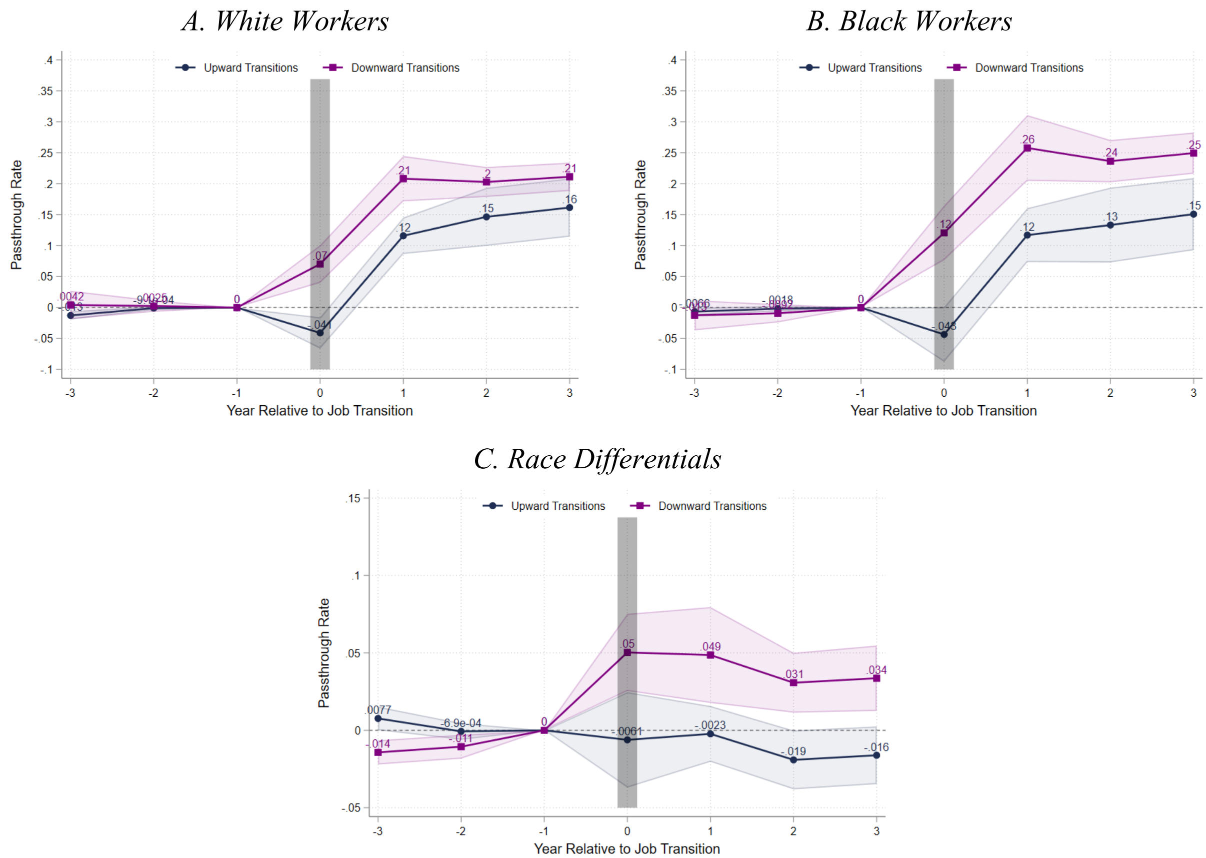
Notes: This figure studies how log earnings evolve following job transitions. The *x*-axis represents time relative to the job move in years, and on the *y*-axis we plot estimates for the passthrough rates from equation (4) over time, $\lambda^{g}_{\bar{c},r}$, along with their 95-percent confidence intervals. Panel A uses the entire sample of movers, pooling across race (and sex). Panel B replicates the estimation on a balanced sample of job movers that we observe for a complete analysis horizon, which we narrow to 3 years before and 3 years after the move.

Figure 5: Firm Effect Passthroughs by Household Income



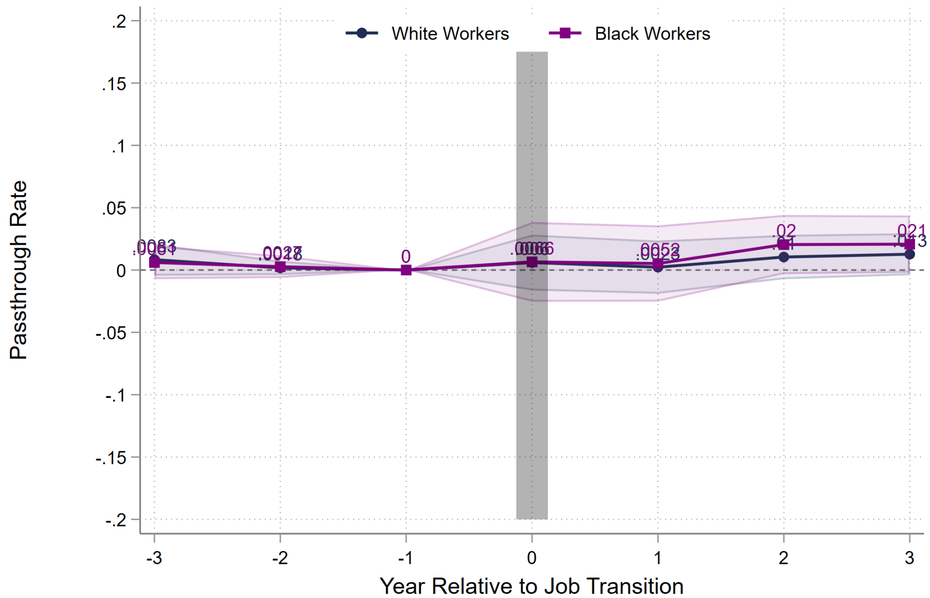
Notes: This figure investigates heterogeneity in treatment effects across the household income distribution. We categorize households based on their overall income as measured by "Total Money Income" (TMI) from 1040s. We split the sample of movers into quintiles of log TMI in the year prior to the move, and we estimate a mean version of equation (4) that aggregates years into pre-move periods (years -5 to -1) and post-move periods (years 1 to 5), separately for each income group. The *x*-axis represents the mean log TMI in each quintile, and on the *y*-axis we plot the average passthrough rate in each quintile along with their 95-percent confidence intervals.

Figure 6: Job Ladder Transitions and Race



Notes: This figure investigates the evolution of log earnings around job ladder transitions by race among stable moves. We study cohorts who, within the analysis horizon of seven years around the move, are in their origin firm from period -3 to -1, in their destination firm from period 1 to 3, and are in either their origin firm or their destination firm in period 0. The x-axis represents time relative to the job move in years. The figure plots estimates of $\lambda^{g}_{\bar{c},r}$ from equation (4) along with their 95-percent confidence intervals, where we split transitions by the direction of the move and conduct the estimation separately by race. Panel A provides estimates for White workers who move either up or down the job ladder, and panel B provides estimates for Black workers who move either up or down the job ladder. We provide dynamic estimates for race differentials in panel C, which plots the Black-White passthrough gap for transitions up the job ladder and the Black-White passthrough gap for transitions down the job ladder.

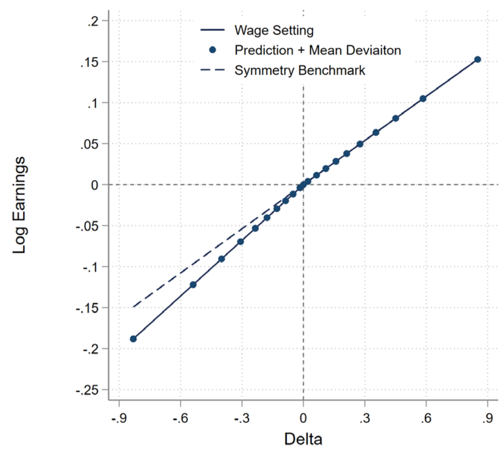
Figure 7: Identification Test



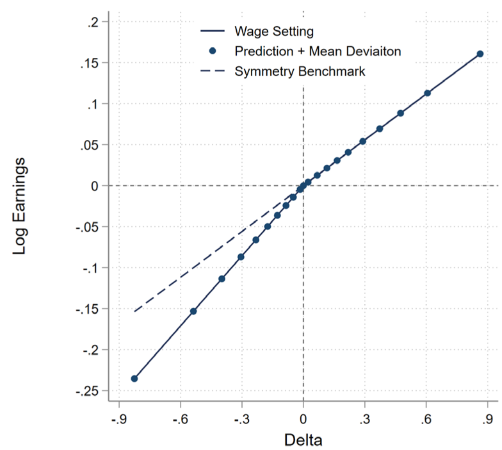
Notes: This figure assesses the underlying evolution of earnings, both pre-move and post-move, comparing across cohorts of workers that are split by whether they move up the job ladder or down the job ladder. This underlying evolution is captured by $\gamma^{g}_{\bar{c},r}$ from equation (4), estimated separately for each combination of $\bar{c} \in \{c^{-}, c^{+}\}$ and $g \in \{B, W\}$, which represents the predicted paths under the counterfactual of no treatment intensity, $\Delta_c = 0$. The figure plots these estimates on the *y*-axis (along with their 95-percent confidence intervals) against the *x*-axis that represents time relative to the job move in years. The estimations include the sample of stable moves of cohorts who, within the analysis horizon of seven years around the move, are in their origin firm from period -3 to -1, in their destination firm from period 1 to 3, and are in either their origin firm or their destination firm in period 0.

Figure 8: Earnings Determination Model

A. White Workers



B. Black Workers

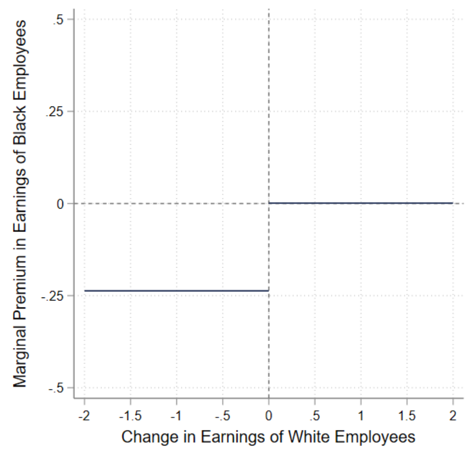


Notes: This figure plots the earnings determination model of job transitions. We plot the functions of earnings processes implied by the estimates for $\lambda^{g}_{\bar{c},r}$ from panel A of Table 2 in the solid line. To assess our parametric assumptions of piecewise linearity in Δ_c, we overlay means of the estimation residuals on top of the linear prediction. Estimates are based on regressions of specification (4) for average effects using all job moves where aggregate observations from years -5 to -1 in the pre-period and observations from years 1 to 5 in the post-period. We estimate this specification for each combination cell of $\bar{c} \in \{c^-, c^+\}$ and $g \in \{B, W\}$, and we take the residual from those estimations. Then, for each cell, we split the sample into ten equal-sized bins based on Δ_c. Within each of the resulting 20 bins per race, we average the residuals, and we add them onto the model prediction in the figure. These are displayed in circles, where we also provide the 95-percent confidence intervals but they are not visible due to tight standard errors. We additionally provide the counterfactual prediction if earnings determination were symmetric at the rate of the estimated slope for positive moves in the dashed line.

Figure 9: Race Differentials

A. Racial Penalty

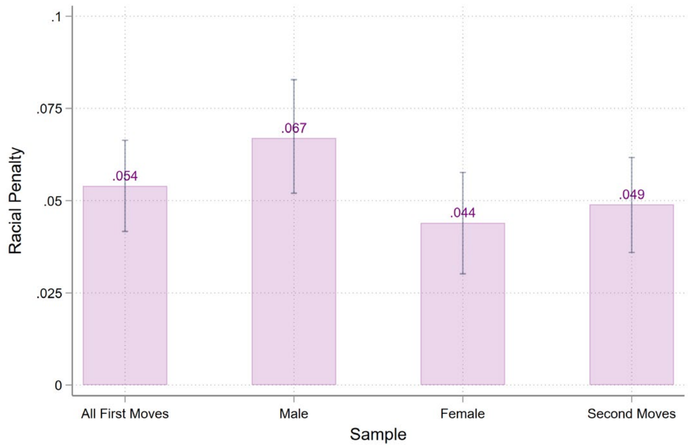
B. Marginal Racial Penalty



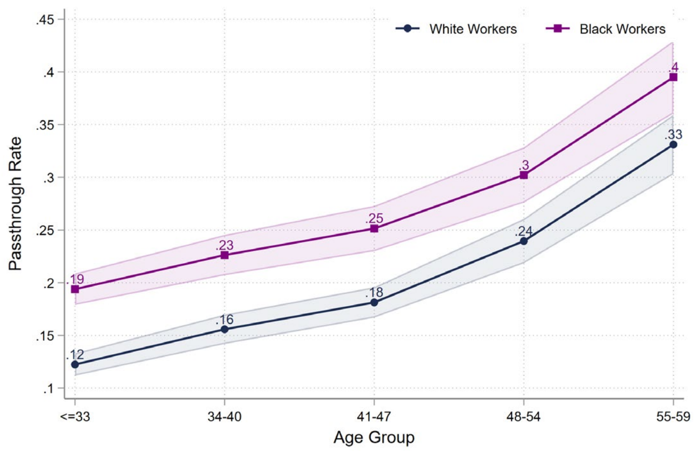
Notes: This figure plots the race differentials in earnings determination at job transitions. We plot functions of earnings processes implied by the estimates for $\lambda^{g}_{\bar{c},r}$ from panel B of Table 2. Panel A depicts the racial penalty in terms of total earnings changes, and panel B displays the marginal racial penalty.

Figure 10: Racial Penalty—Heterogeneity

A. Across Samples



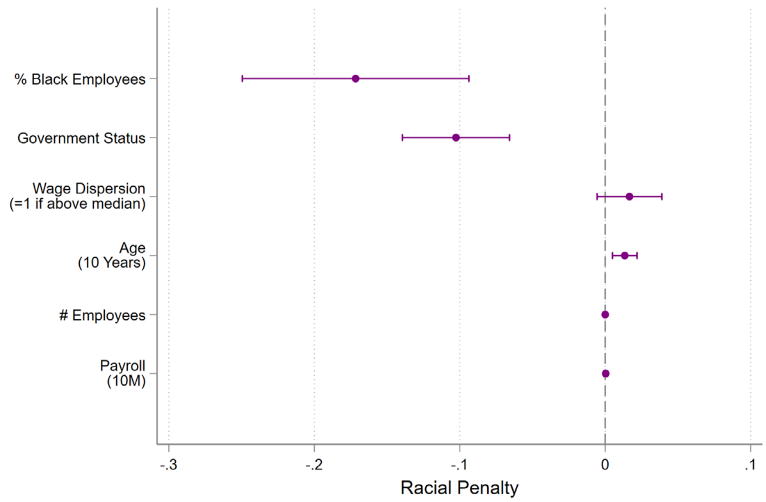
B. By Age



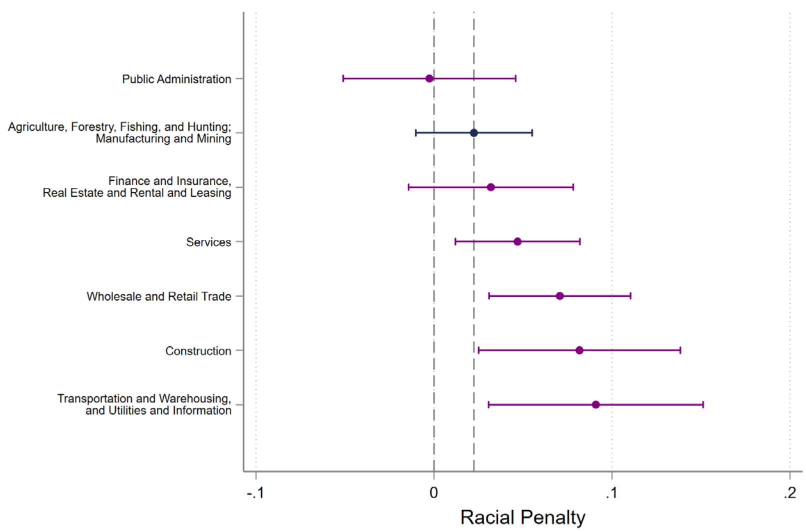
Notes: This figure explores the intensity of the racial penalty across different subsamples of downward transitions. We benchmark these estimates against the baseline estimate of 5.4 pp among the overall sample of downward transitions (reported on the left in panel A). In panel A, we estimate the racial penalty separately by sex, as well as among the sample of second moves within our analysis horizon. In panel B, we split the sample by age group, and we plot the passthrough rate in downward transitions over age separately by race. For each age group, the racial penalty is captured by the vertical gap between the two curves.

Figure 11: Racial Penalty Correlates

A. With Destination Firm Characteristics



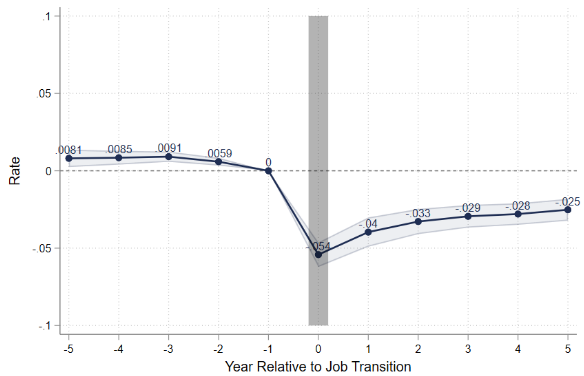
B. With Destination Firm Industry Category



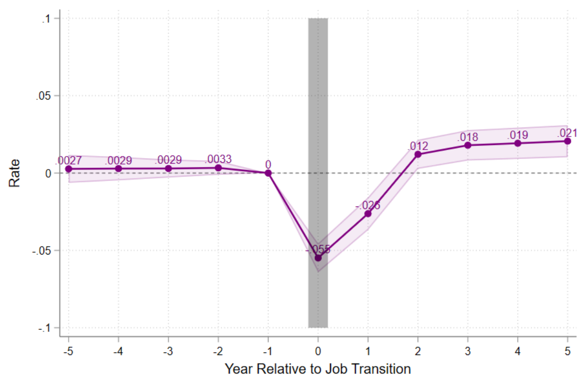
Notes: Panel A summarizes correlations of the racial penalty with characteristics of the destination firm along with 95-percent confidence intervals. Panel B summarizes estimates of the racial penalty by industry groups along with 95-percent confidence intervals, where we use 2017 NAICS categories matched to destination firm (identified using LBD). The estimate for each industry category is relative to "Agriculture, Forestry, Fishing, and Hunting; Manufacturing and Mining" as the baseline category. For that category, standard errors are calculated for the racial penalty, whereas for all other categories, standard errors are calculated for the deviation of the racial penalty from the racial penalty in the baseline category.

Figure 12: Employee Liquidity—Takeup of Retirement Account Distributions in Career Setbacks

A. White Workers



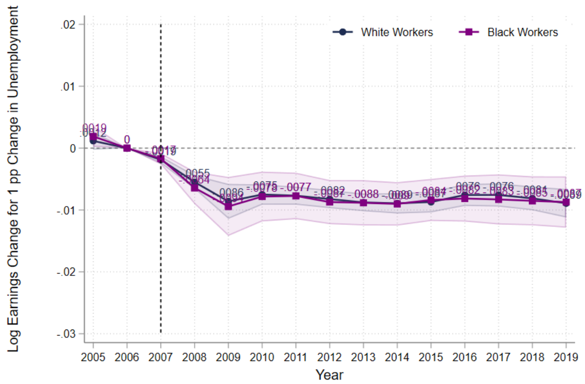
B. Black-White Differential



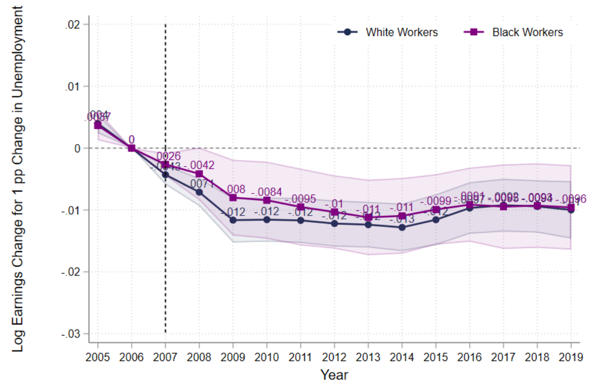
Notes: This figure investigates takeup of withdrawals from retirement accounts in transitions down the job ladder by race. The x-axis represents time relative to the job move in years, and the y-axis displays passthrough estimates. Panel A displays estimates for $\lambda^{W}_{c^-,r}$ from equation (4) estimated among White workers who experience a downward transition. Panel B displays, differentials across Black and White workers who experience a similar intensity setback, $\lambda^{B}_{c^-,r} - \lambda^{W}_{c^-,r}$. The outcome variable is an indicator for any distribution in a given year using extracts from Form 1099-R "Distributions From Pensions, Annuities, Retirement or Profit-Sharing Plans, IRAs, Insurance Contracts, etc." provided by the IRS.

Figure 13: Local Unemployment Shocks and Earnings in the Great Recession

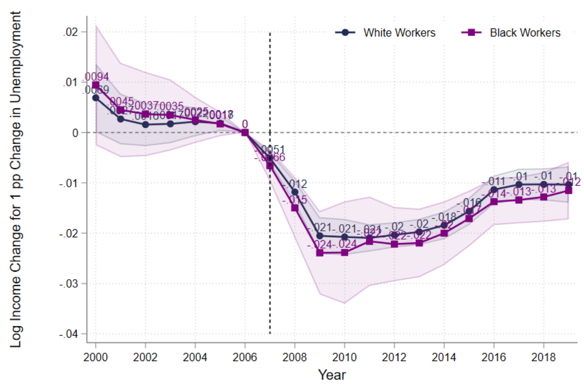
A. All Workers, Form W-2



B. Downward Transitions, Form W-2



C. All Workers, Form 1040



Notes: This figure investigates the evolution of individuals earning around the Great Recession. We plots estimates for λ_t^g from equation (7), separately by race, along with their 95-percent confidence intervals. λ_t^g capture the relative change in individuals' outcomes in a locality that is exposed to a 1 percentage point higher local unemployment shock. We include movers whom we can match to a CZ based on 1990 definitions that are used in Yagan (2019). In panel A, we estimate equation (7) on our entire sample of movers and non-movers. In panel B, we focus on workers with downward moves in years 2007-2009. In these two estimations, we use W-2 data and include firm fixed effects instead of location fixed effects. In panel C, we use wage and salary income from 1040s as the outcome variable, and include CZ fixed effects instead of firm fixed effects since 1040s do not include EINs. Panel C includes the full sample of Black workers, but because of processing limitations we restrict the sample of White workers by taking a random 25% sample in each year.

Table 1: Analysis Sample Summary Statistics

Panel A: All Workers

Variable	Full Sample	Male		Female	
		White	Black	White	Black
	(1)	(2)	(3)	(4)	(5)
Log Earnings	10.89	11.09	10.67	10.77	10.56
Racial Gap		0.42		0.21	
Age	43.20	43.36	41.89	43.53	42.14
% Male	55.13				
% Black	16.78				
Mean Number of Firms Worked at	2.00	2.038	2.131	1.902	2.082
Number of Person-Year Observations	585,800,000	277,100,000	45,880,000	210,400,000	52,390,000
Number of Unique Individuals	68,510,000	30,390,000	6,123,000	25,310,000	6,687,000
Number of Unique Workplaces	594,400	585,500	408,000	580,800	335,500

Panel B: Movers

Variable	Full Sample	Male		Female	
		White	Black	White	Black
	(1)	(2)	(3)	(4)	(5)
Log Earnings	10.91	11.07	10.66	10.81	10.57
Racial Gap		0.41		0.24	
Age	40.35	40.57	39.21	40.57	39.11
% Male	57.06				
% Black	15.54				
Mean Number of Firms Worked at	2.53	2.545	2.591	2.488	2.555
Number of Person-Year Observations	159,300,000	78,870,000	12,020,000	55,660,000	12,730,000
Number of Unique Individuals	22,570,000	10,960,000	1,852,000	7,843,000	1,913,000
Number of Unique Workplaces	588,500	552,900	299,900	513,600	239,400

Notes: This table provides summary statistics for our analysis sample. Panel A includes the entire sample of non-Hispanic Black and non-Hispanic White individuals aged 25 to 59 in years 2005-2019, and panel B narrows the sample to movers. In each panel, column 1 pools observations and columns 2-5 split the sample by sex and race. Earnings are taxable wages and salary reported in Box 1 of the IRS W-2 form. For individuals with multiple W-2s in a year, we select the observation with the highest taxable earnings. Earnings are in 2019 dollars. All numbers are rounded to meet disclosure rules.

Table 2: Key Parameters of Racial Equality

Panel A: Race-Specific Asymmetry

	White		Black	
Direction of Transition:	Upward	Downward	Upward	Downward
Passthrough Rate	0.180	0.227	0.186	0.285
	(0.015)	(0.009)	(0.020)	(0.011)
Difference	0.047		0.099	
	(0.018)		(0.023)	
Asymmetry Factor	26.2%		53%	
Number of Obs.	64,670,000	50,620,000	11,930,000	8,897,000

Panel B: Relative Earnings Differentials

	Direction of Transition:	
	Upward	Downward
Passthrough Rate		
White Workers	0.181	0.227
	(0.015)	(0.009)
Black Workers Differential	0.00015	0.054
	(0.006)	(0.006)
Racial Premium/Penalty	0.1%	23.7%
Number of Obs.	76,600,000	59,520,000

Notes: This table provides estimates for our key parameters of interest, the race-specific asymmetry factors (μ_r^g) and the racial premium/penalty ($\tau_{\bar{c},r}$). Panel A reports the average of the passthrough rate $\lambda_{\bar{c},r}^g$ for positive and negative transitions, separately by race, as well as the resulting asymmetry factors. Panel B reports, within each class of directionality of transitions, the average passthrough rate for White workers and the differential in the passthrough rate for Black workers as compared to White workers. It additionally reports the implied racial premium and penalty.

<u>Table 3:</u> Racial Penalty in Matched Job Moves

	Sample of Downward Transitions	
	All Moves	Stable Moves
Passthrough Rate		
White Workers	0.221	0.212
	(0.003)	(0.008)
Black Workers Differential	-0.002	0.008
	(0.003)	(0.008)
Racial Premium/Penalty	-1%	3.7%
Number of Obs.	3,561,000	431,300

Notes: This table provides estimates for the average passthrough rate for White workers and the differential in the passthrough rate for Black workers as compared to White workers in transitions down the job ladder. We keep only origin-destination pairs that have at least one White worker and one Black worker, average observations within each cell of "origin-destination" by "time relative to move" by "race," and run specifications of equation (4) that aggregate over pre-move periods and post-move periods. Column 1 includes all such moves. Column 2 narrows the sample to stable moves of cohorts who, within an analysis horizon of seven years around the move, are in their origin firm from period -3 to -1, in their destination firm from period 1 to 3, and are in either their origin firm or their destination firm in period 0.

Table 4: Household-Level Analysis of Transitions Down the Job Ladder

	Employee	Spouse
Passthrough Rate		
White Workers	0.2292	-0.04075
	(0.01127)	(0.01498)
Black Workers Differential	0.07531	0.01652
	(0.009442)	(0.03399)
Number of Obs.	20,440,000	20,440,000

Notes: This table provides estimates of the average passthrough rate $\lambda_{\bar{c},r}^{g}$ for various specifications of equation (4), where we aggregate observations from years -5 to -1 in the pre-period and observations from years 1 to 5 in the post-period. We study households as our unit of analysis in the investigation of the racial penalty in downward moves. In this analysis, we include married male workers who experienced a downward job move, and we separately evaluate the effect of the downward transition on the earnings of the worker who switched jobs and the earnings of their spouse. We identify married movers as those who were "married, filing jointly" based on 1040s. We then match the spouse to W-2 records to identify their earnings. If the spouse did not have W-2 earnings, their earnings were coded as 0. In column 1 we study log earnings of the mover in their main employers, and in column 2 we study the log earnings of the spouse in their main employer, using $\log(x + 1)$ to account for 0 earnings. Each column provides the estimate for White households and the estimate for the differential between Black households and White households.

Online Appendix

Appendix A: Tables and Figures

Appendix Table A.1: Key Parameters of Racial Equality by Sex

Panel A: Race-Specific Asymmetry

	Males			
	White		Black	
Direction of Transition:	Upward	Downward	Upward	Downward
Passthrough Rate	0.2042	0.2260	0.2300	0.3001
	(0.02004)	(0.01037)	(0.02728)	(0.01374)
Difference	-0.0218		-0.0701	
	(0.02244)		(0.03065)	
Asymmetry Factor	10.7%		30.5%	
Number of Obs.	37,840,000	29,830,000	5,806,000	4,275,000

	Females			
	White		Black	
Direction of Transition:	Upward	Downward	Upward	Downward
Passthrough Rate	0.1401	0.2221	0.1443	0.2682
	(0.00923)	(0.00721)	(0.01339)	(0.01024)
Difference	-0.0820		-0.1239	
	0.01168		0.01681	
Asymmetry Factor	58.5%		85.9%	
Number of Obs.	26,830,000	20,790,000	6,126,000	4,622,000

Appendix Table A.1: Estimates for Key Parameters by Sex—Continued

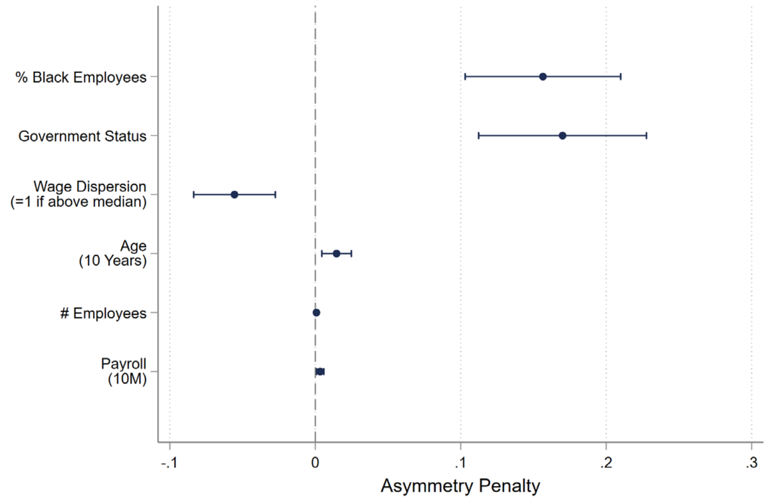
Panel B: Relative Earnings Differentials

	Males	
	Direction of Transition:	
	Upward	Downward
Passthrough Rate		
White Workers	0.2055	0.2263
	(0.02012)	(0.01038)
Black Workers Differential	0.01591	0.06742
	(0.008695)	(0.007852)
Racial Premium/Penalty	7.7%	29.8%
Number of Obs.	43,650,000	34,100,000

	Females	
	Direction of Transition:	
	Upward	Downward
Passthrough Rate		
White Workers	0.1409	0.2218
	(0.009282)	(0.007226)
Black Workers Differential	0.001907	0.0439
	(0.005825)	(0.007013)
Racial Premium/Penalty	1.4%	19.8%
Number of Obs.	32,960,000	25,420,000

Notes: This table provides estimates for our key parameters of interest, the race-specific asymmetry factors (μ_r^g) and the racial premium/penalty ($\tau_{\bar{c},r}$). Panel A reports the average of the passthrough rate $\lambda_{\bar{c},r}^g$ for upward and downward transitions, separately by race and sex, as well as the resulting asymmetry factors. Panel B reports, within each class of directionality of transitions, the average passthrough rate for White workers and the differential in the passthrough rate for Black workers as compared to White workers. It additionally reports the implied racial premium and penalty.

<u>Appendix Figure A.1:</u> Asymmetry Penalty Correlates



Notes: This figure provides the correlations of the passthrough rate with characteristics of the destination firm in downward transitions among White workers.

Appendix B: Data and Analysis Sample

Baseline Sample

We begin by describing the baseline sample, which serves as the starting point for all our analysis.

We start by linking individuals in the US Census Bureau Numerical Identification System (Numident) to IRS W-2 forms. The Census Numident (2023Q1) lists all individuals who have ever received a Social Security Number (SSN), and we use it to restrict the sample to Black, non-Hispanic and White, non-Hispanic individuals, aged 25 to 59 in each year between 2005 and 2019.

Throughout the rest of this description, non-Hispanic Black and White individuals will be referred to as Black and White, respectively.

Using Protected Identification Keys (PIKs), we merge observations in the Census Numident with IRS W-2 Form extracts to obtain taxable earnings reported in Box 1, which lists wages and salary net of pre-tax deductions for health insurance premiums and deferred compensation. For individuals with multiple W-2s in a year, we select the W-2 with the highest taxable earnings. Earnings are adjusted to be in 2019 dollars using the CPI for all Urban Consumers (CPI-U).

We drop observations with less than $15,000 (in 2019 dollars) in the highest earning W-2.

Then, in each year and for each firm, we count the number of employees who are Black or White, of ages 25-59, and earned at least $15,000. Firms with less than 20 employees who meet these restrictions in each year between 2005 and 2019 are flagged, and we exclude workers' full W-2 history if they were ever employed in a flagged firm. Under this restriction, we drop 28,590,000 White workers, 2,990,000 Black workers, and a total of 8,384,000 firms.

Next, we link person-level data to firm-level data recorded in the Longitudinal Business Database (LBD) using Employer Identification Numbers (EINs). From the LBD, we can identify an EIN's four-digit 2017 NAICS code. An EIN can be associated with multiple establishments in a given year, and NAICS codes can vary across those establishments. In these cases, we assign to the EIN the industry code associated with the establishment with the largest employment and annual payroll values. Note that some firms are missing a NAICS value due to missing information on the LBD; workers in these firms remain in our sample. Using the NAICS codes, we identify firms in the Executive, Legislative, and Other General Government Support or the Educational Services industries (NAICS codes in 9211 and 61, respectively). We exclude workers' full W-2 history if they were ever employed at a firm in these industries between 2005 and 2019. Under this restriction, we drop 12,396,000 White workers, 2,467,000 Black workers, and 40,000 firms. An additional 2,823,000 White workers, 409,000 Black workers, and 122,000 firms were dropped due to a combination of the small firm size and NAICS code restriction.

Lastly, we identify workers' commuting zones (CZ) using a combination of IRS 1040 filings and the LBD matched to the USDA's Economic Research Service's (ERS) list of 1990 and 2000 CZs. We first use the 1040 filings to assign a CZ in each year based on the county and state listed for each unique PIK. If no CZ was assigned, we use the LBD's geographic variables to identify a firm's CZ in each year where the EIN is associated with a single CZ. Workers are then assigned a CZ using the EIN for the highest-earnings W-2. Not all observations are matched with a CZ because either (1) values for geographic variables in the administrative data are missing, (2) a PIK in the W-2 data cannot be matched to a PIK in the 1040 filings, (3) an EIN in the W-2 data cannot be matched to an EIN in the LBD, or (4) an EIN in the LBD can be associated with multiple establishments located in different CZs.

To construct our baseline sample of movers, we start with the baseline sample and further restrict to workers who have transitioned to a new EIN at least once between 2005 and 2019. A move is defined as a year-to-year change in the EIN associated with the W-2 with the highest earnings. Note that the years need not be successive; a worker can have years with no W-2. The year in which they re-appear in the W-2 data is classified as a move if the EIN is different from the previous EIN in their work history.

Figure 1

Maps are generated separately for male and female workers. To generate within-firm racial earnings gaps among workers of a given sex, we start with the baseline sample, select firms that employ at least one White employee and at least one Black employee of that sex, and calculate average log earnings by race in a given firm and year. Then, for each firm and year, we subtract the mean log earnings for Black employees from the mean log earnings for White employees of a given sex and average the annual within-firm earnings gaps across all firms within a given state.

Figure 2

For the baseline sample, we calculate the total number of unique main employers each worker has worked at between 2005-2019, where we define the main employer as the EIN at which the employee earned the most in a given year. We then calculate the proportion of workers who worked in a given number of firms within a race by sex subgroup.

Figure 3

The figure uses the baseline movers sample. Panel A shows the mean and standard deviation of Δ_c by race. Panel B plots the probability density function of Δ_c, separately among Black and White job movers. Panel C compares the two distributions by plotting the decile values of Δ_c for Black job switchers against the decile values of Δ_c for White job switchers.

Figure 4

Panel A uses the baseline mover sample, and panel B restricts the analysis to a balanced sample of job movers who have complete W-2 history from period $t = -3$ to $t = 3$.

Figure 5

The 1040 extracts include a constructed measure of Total Money Income (TMI). TMI is the sum of a tax-unit's taxable wage and salary income, taxable and tax-exempt interest income, dividends, gross Social Security income, unemployment compensation, alimony received; business income or losses (including for partnerships and S-corps); farm income or losses, and net rent, royalty, and estate and trust income. Prior to tax year 2018, TMI also included total pensions and annuities. However, this was removed from TMI due to a change to income reporting on the Form 1040 and the regulations regarding data sharing between IRS and the Census Bureau. Observations with negative or zero income are excluded, and not all movers can be matched with the 1040 data. We split the sample into quintiles of log TMI in the year prior to the move and estimate the mean difference-in-differences specification separately for each quintile.

Figures 6-7:

These figures use the balanced sample from panel B of Figure 4, which we further restrict to workers who remained at the destination firm for at least three years. We categorize job transitions into transitions up the job ladder when $\Delta_c \geq 0$, and transitions down the job ladder when $\Delta_c < 0$.

Figures 8-9:

The implied earnings processes (solid lines) and benchmarks (dashed lines) in Figures 8 and 9 use the baseline mover sample in Table 2. For compliance with disclosure requirements, we drop moves in the tails of the distribution of Δ_c when estimating the mean residuals, capturing 98.5% of moves.

Figure 10:

Panel A: The coefficient for all first moves corresponds to the racial penalty coefficient in panel B of Table 2. For the coefficient for second moves, we identify workers who moved at least twice between 2005 and 2019. We then drop observations in the years prior to a worker's first move, so that the effects of the first move are not included in the estimation (we drop the first year of the first move and earlier). We then calculate the treatment intensity for the second job move transition and estimate the racial penalty, using observations from 5 periods before and 5 periods after the second move.

Panel B: We split the movers sample into 5 approximately equally-sized age groups (<=33, 34-40, 41-47, 48-54, 55-59), where age is the worker's age in the year of the first move. We then estimate the passthrough separately for each race by age subgroup for downward transitions.

Figure 11:

All regressions are estimated over downward transitions among the baseline sample of movers. Where noted, there are small sample differences due to additional sample restrictions or data limitations in matching.

% Black Employees: We calculate the average percentage of Black employees at the destination firm between 2005 and 2019 using the full baseline sample of Black and White workers (ages 25-59, earning at least $15,000).

Government, Tax Exempt, Age, Payroll of Destination Firm: Determined using LBD (not all firms in W-2s appear in the LBD).

Government and tax exempt are legal forms of organization. The correlates are indicators for whether the firm was ever categorized as such (between 2005 and 2019). Government Status: A business that taxpayers primarily fund. Tax Exempt: Corporations that are legally incorporated under state laws or other enterprises that are formed by other legal form of organization and are not subject to taxes.

Age: Maximum age of the firm in our sample calculated using the first year of employment or payroll and the last year present in the LBD.

Payroll: The sum of annual payroll across all years a firm is present in the LBD between 2005 and 2019.

Employees: Using the full baseline sample of workers, we count the number of employees at a destination firm who are Black, non-Hispanic or White, non-Hispanic, ages 25-59, and earned at least $15,000. We then convert the counts into a z-score.

Wage Dispersion: First, we calculate standard deviation of log earnings by year and EIN. Then, we take the average of the standard deviation across years. Above Median indicates dispersion above the 50th percentile.

Figure 12:

The figure uses 1099-R extracts provided by the IRS, which includes information on "Distributions From Pensions, Annuities, Retirement or Profit-Sharing Plans, IRAs, Insurance Contracts, etc." We merge the 1099-R extracts to our sample of movers to identify the use of early withdrawals at the time of a downward transition. Conditioning on $\Delta_c < 0$, we estimate a dynamic regression interacting Δ_c with time relative to move, where the outcome is an indicator for having 1099-R distributions in a given year.

Figure 13:

We identify workers' commuting zones (CZ) using a combination of IRS 1040 filings and the LBD matched to the USDA's Economic Research Service's (ERS) list of CZs. We first use the 1040 filings to assign a CZ in each year based on the county and state listed for each unique PIK. If no CZ was assigned, we use the LBD's geography variables to identify a firm's CZ in each year where the EIN is associated with a single CZ. Workers are then assigned a CZ using the EIN for the highest earning W-2. Because of data limitations, not all observations are matched to a CZ. We use the 1990 CZ classification in this analysis.

Panel A: Uses the full baseline sample of Black and White workers (both job movers and non-movers), ages 25 to 59 in each year between 2005 and 2019 who can be matched to a 1990 CZ. The outcome variable is the log of taxable earnings reported in Box 1 of the W-2 Forms.

Panel B: Focuses on workers with downward transitions in years 2007-2009. The outcome variable is the log of taxable earnings reported in Box 1 of the W-2 Forms.

Panel C: To observe a longer history of earnings, we repeat the analyses in panels A and B using the log of wage and salary income from 1040 extracts as the outcome variable. We match the 2000-2019 1040s to the Numident to pull age and race. Like the original W2-Numident sample, we restrict the sample to Black, non-Hispanic and White, non-Hispanic individuals, ages 25 to 59 in each year. We use county and state in the 1040s to identify 1990 CZ. Because 1040s do not include EINs, we use age and CZ fixed effects. We restrict the sample of White workers by taking a random 25% sample in each year for computational reasons.

Table 1:

Panel A uses the baseline sample, and panel B uses the baseline sample of movers.

Table 2

Uses the baseline mover sample. $t = 1$ is the year a worker transitions to a new EIN for the first time within our sample. Job transitions are categorized as transitions up the job ladder when $\Delta_c \geq 0$, and as transitions down the job ladder when $\Delta_c < 0$. The pre-period is defined as t between -5 and -1 and the post-period is defined as t between 1 and 5, and omit the year prior to the first move.

Table 3

Column 1: We begin by using the full sample of workers who transitioned to a new EIN at least once between 2005 and 2019, including those for whom the treatment intensity is missing. Restricting the sample to one observation per worker, we count the number of Black and White workers in each origin-destination firm pair. Second, we residualize log earnings among the sample of movers with a non-missing treatment intensity whose origin and destination firms have positive counts of both Black and White workers. We control for a third-order polynomial in age, worker fixed effects, and year fixed effects. Third, we calculate the average residual and Δ_c for each origin-destination firm pairing by time relative to move and race. Among origin-destination firm pairings where average Δ_c is negative, we then estimate a difference-in-differences specification with average residuals as the outcome.

Column 2: Estimates are generated in the same three steps as in column 1 with additionally restricting the sample to movers who have a complete W-2 history between $t = -3$ to $t = 3$ and who remained in the destination firm for at least three years (including the year of the move).

Table 4

We estimate the difference-in-differences specification over the sample of male movers with $\Delta_c < 0$ and their spouses. We identify spouses for workers who filed their 1040s as "married, filing jointly," and then link the spouses to W-2 records. If the spouse of the mover did not have W-2 earnings, their earnings were coded as 0.

Column 1: The outcome is the mover's log earnings in their main employer (W-2 with the highest earnings in a given year).

Column 2: The outcome is the spouse's earnings in their main employer, using $\log(x + 1)$ to account for 0s.