The Racial Mortality Gap and Unemployment Insurance Cuts during the Covid Pandemic*

John S. Earle† Sungbin Park‡

September 30, 2026

Abstract

We estimate the impact of early discontinuation of pandemic unemployment insurance (UI) across 19 states in 2021 on racial mortality. UI cuts increased COVID-19 mortality for both Blacks and Whites, but twice as much for Blacks. The disparity is driven by a labor supply channel: re-employment rates rose for Black workers at double the rate for Whites. Evidence suggests causality through higher exposure rates rather than racial differences in infection-fatality processes. The higher responsiveness of Blacks to the UI cuts is related to lower wages, higher replacement rates, lower wealth, and higher likelihood of frontline work—factors reflecting structural discrimination.

JEL codes: J15, I18, J65

Keywords: racial disparity, Covid deaths, unemployment benefits, re-employment, community transmission, systemic discrimination, structural discrimination

*We thank participants at presentations at the Society of Labor Economists, American Society of Health Economists, Washington Area Labor Economics Symposium, Society of Government Economists, Stone Center Conference on Economic Inequality, Howard University, Southern Economic Association, and GMU-Schar School for helpful feedback.

†George Mason University, earle@gmu.edu

‡George Mason University, spark224@gmu.edu

1 Introduction

An important but little-understood fact about the Covid pandemic is that age-adjusted mortality rates were substantially higher for Blacks than Whites. The gap might be explained by racial differences in economic status, pre-existing health conditions, or other factors affecting vulnerability to severe infection, but a crucial question is whether public policies mitigated or exacerbated the disparity. This study investigates one such policy: the large reductions in unemployment insurance (UI) by 19 states during early summer 2021, 2-3 months before most of the other states followed. These cuts could have pushed unemployed people back into the work force, creating potential exposure and community transmission, ultimately increasing Covid deaths. If Blacks and Whites responded differently to the UI cuts, and assuming some degree of segregation between racial communities, the UI cuts could lead to racial differences in mortality even though UI is at least formally a race-blind policy.

We test these hypotheses using state-level panel data on Covid deaths by race from the Centers for Disease Control (CDC) and longitudinally linked individual data from the Current Population Survey (CPS). Controlling for state and time fixed effects and potential confounders, we find that the UI cuts raised age-adjusted Covid death rates among Black and White populations by 21.1 and 10.7 per 100,000, respectively, more than doubling the baseline (pre-treatment or control state) rates. Both increases are statistically significant, and estimation by triple-differences shows the rise in the Black rate is statistically higher than that in the White rate.

Examining the hypothesized mechanism for these sharp jumps in mortality, we estimate that the UI benefit cuts increased the probability of unemployment-employment transitions by 15 percentage points among Black workers, and only 7 points for Whites. The marginal impact of the policy-related re-employment increase on Covid deaths, estimated with two-stage least-squares, is similar for Blacks and Whites, however, implying that the larger jump in Black mortality is explained by increased exposure resulting from the policy change rather than by racial differences in community transmission or fatality processes. Finally, we find evidence that the larger re-employment response among Black workers is linked to lower wages, higher UI replacement rates, lower liquid assets, and higher rates of work in occupations involving personal contact.

While there is a large literature on racial gaps along many dimensions, no previous research has investigated the impact of UI policies on racial disparities in outcomes or behavior, as we do in this paper.[1] Research on the relationship between UI and health-related out-

[1]Besides wage gaps, other dimensions of racial disparity that have been investigated include small business lending (Blanchflower et al., 2003), hiring decisions (Bertrand and Mullainathan, 2004), voting wait times (Chen et al., 2022), career awards (Rim et al., 2024), and federal sentencing (Tuttle, 2026), to name a few.

comes has shown that UI can mitigate effects of unemployment, but it does not examine racial differences in these effects (Kuka, 2020; Lindo et al., 2025).[2] Research on racial inequality in UI receipt (Carey et al., 2021; Meyer et al., 2024; Kuka and Stuart, 2025) does not examine the consequences for disparities in outcomes. And while a considerable body of research has examined the effects of UI cuts on job-finding behavior, including a few studies focused on the pandemic period (Holzer et al., 2021; Coombs et al., 2022; Ganong et al., 2022; Parolin and Pignatti, 2025), no previous work has estimated differences by race in the job-finding responses to UI benefits.

Our analysis of racial disparities in mortality resulting from unemployment insurance policy also provides an example of what is variously labeled "systemic" or "structural" discrimination (e.g., Pincus (1996); Lang and Spitzer (2020); Small and Pager (2020); Devakumar et al. (2022); Braveman et al. (2022)). Although these terms are used by scholars in somewhat different ways, a common theme is the presence of interactions among different agents, not all of whom may consciously discriminate. In our paper, racially disparate outcomes (mortality) are produced by interactions between a facially race-neutral policy (UI cuts) and inherited disadvantages (lower wages, wealth, and occupational access) that result from past discrimination (the history of race relations in the US). This definition of "systemic" is closely related to Bohren et al. (2025), who propose a "notion of systemic discrimination that captures how direct discrimination in other decisions leads to differences in relevant attributes for a given decision, which in turn generates disparities in outcomes" (P. 1744-45). In our context, the "other decisions" refer to the history of discrimination against Blacks that lead to present disadvantages. The resulting disparities need not have been intentional, but it should be noted that pandemic UI programs were legislated at the federal level to cover all states and presumably protect all families, while the decisions to cut these programs were taken at the state level, more frequently in states with higher Black populations.[3]

Our research is also related to the longer-term literature on racial differences in mortality (e.g., Bundy et al., 2023; Dwyer-Lindgren et al., 2024) as well as to papers documenting the gap during the pandemic (Alsan et al., 2021; Aburto et al., 2022; Joynt Maddox et al., 2022; Zalla et al., 2022). However, there has been relatively little evidence on the channels, or mechanisms, that might account for higher Covid deaths among Blacks compared to Whites.

[2] In a partial exception, Guo et al. (2026) examine mental health effects of UI cuts and report differences for Whites versus non-Whites as part of their larger heterogeneity analysis.

[3] Papers similar to ours in examining racially disparate outcomes of facially race-neutral policies (or at least which did not have racial disparity as their primary objective) include Derenoncourt and Montialoux (2021) on the 1938 Fair Labor Standards Act, Bartscher et al. (2022) on monetary policy, and Aaronson et al. (2021) on redlining.

Previous research on occupations has documented a higher prevalence of Blacks in fields with higher risk of Covid infections (e.g., Goldman et al., 2021), but no previous studies have estimated effects of UI on Covid cases and deaths, except for Park et al. (2026), which does not consider differences by race. We fill gaps in this previous research by estimating racial differences in the behavioral response to UI, and by connecting those differences to the racial gap in mortality during the pandemic and to measures of underlying economic disadvantage.

2 Data

We analyze a combination of state and individual-level panel data. Use of state-level data is appropriate for our analysis of Covid outcomes because of the nature of our research question and it is also dictated by constraints on data availability. The UI policies we focus on vary by states over time, and the consequences of the policies for Covid deaths concern not only individuals returning to work but also the externality to others through community transmission of infection: workers may bring the virus home to their households and social networks, sometimes resulting in death. To measure these outcomes requires aggregate data, and information on Covid deaths by race is not available on a more disaggregated basis than state-month.

2.1 Measuring Covid Death Rates by Race

Our principal dependent variables are Covid death rates for non-Hispanic Blacks and non-Hispanic Whites, excluding other races and Hispanic ethnicity to focus attention on one potential racial gap. In order to express the number of deaths per capita, we use the April 2020 population data from the Decennial Census for non-Hispanic Blacks and Whites by state.

There has been some debate on whether the CDC under- or over-counts Covid deaths (Hoffman, 2020; Faust et al., 2021). While some degree of measurement error seems likely, our estimates of the racial mortality gap would be affected only if there were a bias in reporting that emerges exactly in the states discontinuing pandemic UI during the intervention period (because there is little change in Covid deaths in the control states during the intervention period). We are unaware of any factors that could lead to such biased reporting. Measurement error could of course reduce precision.

A more important data issue stems from the fact that higher mortality rates among Blacks in the US prior to the pandemic resulted in an age structure substantially different for Black and White populations. Furthermore, Covid death rates increase steeply with age

for both races but beginning at younger ages among Blacks.[4] Together, these facts imply it is crucial to carry out comparisons of mortality across races on an age-adjusted basis.

A problem with constructing age-adjusted mortality rates by race, state, and month arises because the CDC WONDER database (CDC, 2024a,b) suppresses the death count for any cell where the number of deaths is 1-9.[5] Our approach fills in the suppressed data with the following procedure. First, we use the raw CDC 10-year age groups from age 15 (e.g., [15-24), [25-34),...[85-∞)), but we group ages 0-14, for which Covid death counts are negligible. In the 19,440 state-age-month cells thus defined for 2020-2023, there are 5,524 suppressions of Covid deaths for Blacks and 5,882 for Whites in the CDC data. Next, we use unsuppressed counts of all-cause deaths and of deaths from all causes except Covid, subtracting the latter from the former, when possible for each suppressed cell. This reduces the number of remaining suppressions drastically, to 571 cells for Blacks and 98 for Whites. We also checked that in unsuppressed cells this arithmetic holds exactly. Finally, for the remaining suppressed cells, we use unsuppressed counts of Covid deaths for combined non-Hispanic Blacks and Whites and for non-Hispanic Whites, subtracting the latter from the former to obtain the counts for non-Hispanic Black Covid deaths. The final number of suppressions that could not be filled in with these procedures for the 19,440 state-race-age-cells over the analysis period was 17 for Blacks and 67 for Whites, accounting for only 0.4 percent of all cells. We imputed one death for these cases, but also tried imputing five, as a robustness check. Results were essentially identical from alternative imputations.

The age adjustment has a large impact on the racial difference in Covid death rates. Over the whole 2020-2023 period, the crude rates are 6.58 per 100,000 population for Whites and 5.38 for Blacks, implying an 18 percent lower rate for Blacks. But the rate for Blacks adjusted to the age structure of Whites is 9.61 per 100,000, implying a 46 percent higher rate for Blacks. For the period we focus on in this paper, August-September 2021, the Covid mortality rate for Whites is 17.51, the crude Black rate is 19.51, and age-adjusted Black rate is 28.45 per 100,000.

2.2 Pandemic Unemployment Insurance

The intervention we analyze is the early termination of the pandemic UI programs: the Federal Pandemic Unemployment Compensation (FPUC), Pandemic Emergency Unemployment Compensation (PEUC), and Pandemic Unemployment Assistance (PUA). These federal pro-

[4] Appendix Figure A1 shows these patterns. Foster et al. (2024) provides the racial difference in the age gradient for excess mortality from April 2020 to March 2021. Alsan et al. (2021) analyze the race-by-age patterns of excess mortality in 2020.

[5] Lundberg et al. (2023) address the missing data problem by aggregating age categories into three large groups and imputing the missing cells with random draws from an assumed distribution.

grams raised benefits considerably, adding $300 per week to benefits beginning in December 27, 2020, and extended eligibility and duration much longer than the standard duration (usually, 26 weeks) in all states.[6] By law, the programs ended for all recipients by September 4, 2021, but states were allowed to opt out. Based on state government websites, and consistent with previous research (e.g., Park et al. (2026)), we find that the first early terminations, by three states, took place on June 12, 2021. Seven states terminated on June 19, eight on June 26, and one on July 3. We consider these 19 states as treated.[7] Control states are defined as ending PUI by federal law on September 4, 2021.[8]

The differences across states in terminating the PUI programs create variation across geography, racial composition, and time. Figure 1 contains two maps. The first shows states by treated, control, and excluded groups, while the second shows states by Black share, measured as the ratio of the non-Hispanic Black population to the sum of the non-Hispanic White and Black populations. The population-weighted average Black share is 0.21 in treated states and 0.15 in controls, implying that the treatment could affect a larger number of Blacks than if the shares were equal.[9] On an unweighted basis, the average Black shares in the populations of treated and control states are 0.13 and 0.15, respectively, and both groups include states with low and high Black shares.

Concerning time variation, associated with treatment, the slight staggering of PUI discontinuation cannot be addressed in the available monthly data, and we specify the pre-period as the months before discontinuation of PUI, usually from March to June 2021; we start in March because spring 2021 was a relatively stable period after a volatile late fall 2020 and early winter 2021. The post- or intervention period is July and August 2021. Combining geographic and time variation, we define $PUIend_{st}$ as an indicator for states s in months t that equals 1 in July-August 2021 for states discontinuing PUI early, and 0 otherwise.

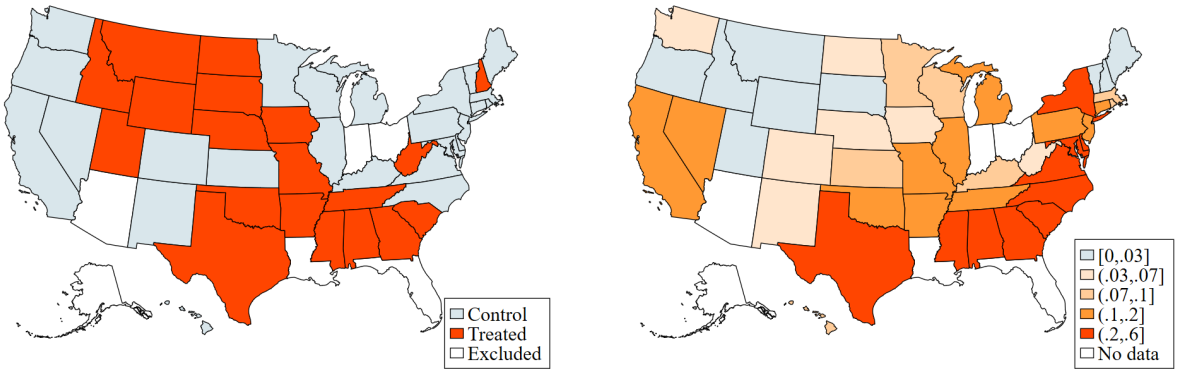
[6]The extra $300 per week is from the FPUC, while PEUC extended benefits an additional 53 weeks and PUA extended eligibility to the self-employed, contractors, and UI exhaustees up to a total of 79 weeks. For more details, see Whittaker and Isaacs (2022).

[7]The treated states are Alabama, Arkansas, Georgia, Idaho, Iowa, Mississippi, Missouri, Montana, Nebraska, New Hampshire, North Dakota, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, West Virginia, and Wyoming. Supplementary Table S1 in Park et al. (2026) contains a list of all states with the date of FPUC, PEUC, and PUA termination and the classification by treated, control, and excluded from our sample.

[8]The control states are California, Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Kansas, Kentucky, Maine, Maryland, Massachusetts, Michigan, Minnesota, Nevada, New Jersey, New Mexico, New York, North Carolina, Oregon, Pennsylvania, Rhode Island, Vermont, Virginia, Washington, and Wisconsin. We exclude six states that terminated programs at different times. Alaska, Arizona, Florida, and Ohio ended FPUC early, but continued PEUC and PUA until September. Indiana's termination in mid-June was reversed by a court ruling. Louisiana ended the programs on July 31.

[9]Appendix Table A1 provides population shares including all races and additionally shows that the Black share among the unemployed is 0.24 in control states and 0.33 in treated states.

Figure 1: Treated and Control States and Black Population Share



(a) Treated and Control States (b) Black Share

Note: Panel (a) shows states with dark orange color (Treated) ended PUI between June 12 and July 3, 2021. States in light blue color (Control) continued the program until September 4, 2021. Panel (b) shows the distribution of the Black share (non-Hispanic Black population/(non-Hispanic White + Black)) across sample states. The Black population share is 0.15 in the control states and 0.21 in the treated states.

2.3 Control Variables

We include several other state-month variables that function as covariates in the Covid death regressions. The Covid vaccination rate is defined cumulatively as the percentage of the total population with at least one dose of a Covid vaccine (CDC, 2023; KFF, 2022). We include the vaccination rate (and its square) for each race separately.[10] To account for differences in weather, which may affect the time spent indoors and thereby the potential spread of disease, we use cooling and heating degree-days, defined respectively as the sums of the deviations of the monthly average temperature above and below 65°F. We replace missing information from this source on Washington D.C. and Hawaii with data from Weather Data Depot (2023). We sum cooling and heating degree days to construct "indoor days" by state-month.

Another set of covariates accounts for differences in the vulnerability of the state population, by race, to Covid infection and death. We measure rates of obesity and cardiovascular disease, the share of the population 65 years and older, and median earnings as of May 2021, and interact each of these variables, by race, with a linear time trend to control for increases in Covid deaths associated with vulnerability. A final control variable is an index of non-pharmaceutical interventions (NPI), based on Raifman et al. (2022).

Summary statistics for all these covariates as of May 2021, before the policy change, are shown in Table 1. Means and standard deviations are shown for control and treated states

[10] KFF (2022) provides vaccination rates by race-state-month for all states except Montana, North Dakota, and Wyoming, and a few others (accounting for only three percent of the US population) have missing values for some months. We impute race-specific vaccination rates for these missing values as the national mean for the corresponding month; using state-level aggregate vaccination rates yields similar results. States are inconsistent in distinguishing ethnicity, so race categories may include both Hispanics and non-Hispanics.

separately, as well as for Blacks and Whites separately, except for indoor degree days and the NPI index, which do not vary by race. For those two variables, the data show lower values on average in treated states: fewer indoor degree days and a lower NPI index.

Table 1: Descriptive Statistics: State-Level Covariates by Race and Treatment, May 2021

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Control			Treated			T-C	
	White	Black	B-W	White	Black	B-W	T(B-W)-C(B-W)	
Vaccination$_{t-1}$ (%)	55.1	33.3	-21.7	42.6	27.5	-15.1	6.7	3.4
	[7.6]	[6.3]	(2.8)	[5.0]	[4.8]	(1.9)	(3.8)	(4.4)
Indoor degree days$_{t-1}$	413.4	374.5	-38.9	305.2	209.7	-95.5	-56.6	-96.9
	[129.0]	[117.7]	(48.7)	[171.2]	[74.3]	(63.1)	(79.1)	(84.0)
Obesity (%)	29.1	40.3	11.2	32.6	42.0	9.3	-1.9	-1.2
	[3.8]	[5.5]	(1.6)	[2.7]	[4.5]	(1.3)	(2.2)	(2.5)
Cardiovascular disease (%)	6.5	5.4	-1.1	7.8	6.1	-1.7	-0.6	-0.2
	[1.0]	[1.3]	(0.4)	[1.5]	[0.9]	(0.6)	(0.7)	(0.9)
Median earning ($1,000)	15.4	11.9	-3.5	12.1	11.6	-0.5	3.0	1.5
	[5.1]	[4.5]	(1.9)	[4.0]	[3.6]	(1.6)	(2.7)	(3.0)
Age 65+ (%)	20.9	13.0	-7.9	19.5	11.4	-8.1	-0.2	-0.7
	[1.6]	[1.7]	(0.6)	[1.7]	[1.5]	(0.7)	(0.9)	(1.0)
NPI index	10.3	10.5	0.2	6.9	6.9	0.0	-0.2	-0.7
	[2.7]	[6.7]	(1.0)	[0.9]	[0.4]	(0.3)	(1.3)	(1.4)
Observations	26	26	52	19	19	38	90	90
Weight	Population							Pop x IPW

Note: Sources and variables are described in text. Period for these statistics: May 2021. Standard deviations are shown in brackets. Observations are weighted by the population of each race by state in columns (1) - (7) and additionally by Inverse Probability Weights (IPW) in column (8). Descriptive statistics for March-June pre-intervention periods are in Appendix Table A3.

For the covariates varying by race, Table 1 shows that vaccination rates are lower among Blacks than Whites during this in both the control and treated states, but the gap is larger in control states: 22.2 versus 14.0 percentage points. Obesity and cardiovascular disease rates are higher among Blacks than Whites, with similar numbers for treated and control states. Median earnings are lower for Blacks, and lower for Whites in treated than control states. The final vulnerability measure, the share of the population 65 years and older, is lower for Blacks than Whites, consistent with our discussion of the differing age structures, above, but the patterns are again similar between control and treated states.

Column (7) of Table 1 shows the treated-control difference for the first two race-invariant variables and the Black-White difference between treated and control states for those that vary by race. These are the relevant differences to examine covariate balance, and the results suggest imbalance in the indoor degree days, NPI index, and vaccination rates, but little difference in the other four variables. To address the imbalance, we construct inverse probability weights (IPW) based on a logit regression of treatment probability on all these variables plus Black population share, unemployment rate, and job opening rate (logit esti-

mates are reported in Appendix Table A2). Applying the IPW reduces all the differences, as shown in column (8), and except for the NPI index none of them are statistically significant. We use IPW-weighted regressions as a robustness check.

2.4 Data on the Labor Mechanism

Our basic data source for studying re-employment, the mechanism through which UI may have affected Covid spread, is the IPUMS version of the Current Population Survey (CPS) from the Bureau of Labor Statistics (Flood et al., 2024). We focus on an extended definition of "unemployed," including two groups: (1) individuals who are currently unemployed by the standard criteria and who have previously worked plus (2) any non-employed (including non-participants) who searched for a job in the past year (based on a direct CPS question). The intention is to capture the individuals most likely to be UI recipients during the pandemic, when search requirements were relaxed and benefits extended to job leavers, re-entrants, and non-searchers for up to 79 weeks. The number of observations in the first group (the "experienced unemployed") is 6731, and including the second group gives 8572 observations total. The CPS does not measure UI receipt, so our estimates can be interpreted as the "intent-to-treat" impacts of the change in UI benefits on individuals in these two groups.[11]

We measure re-employment behavior using the longitudinally linked version of the CPS, which provides consecutive-month pairs. Restricting the sample to individuals who are unemployed in the first month of the pair, we observe whether they have become re-employed in the following month. Individuals may appear more than once (once for each month they are both unemployed and observed in the sample in the next month). We consider the period March-August 2021, excluding September because results show an immediate response to $PUIend$, and the control states discontinue the programs on September 4. We also restrict the sample for this analysis to individuals who are 20 to 64 years old in order to reduce complications associated with schooling and retirement.

We construct an indicator variable, UE_{ist}, equal to 1 for re-employment of individual i in state s, for a transition from unemployment in month *t-1* to employment in month t, and equal to 0 otherwise (including remaining unemployed and exiting from the labor force). When estimating the impact of $PUIend_{st}$ on UE_{ist}, we control for state and month fixed effects and for standard demographic characteristics, including age, sex, ethnicity, race, education, marital status, foreign born, and child under 12 years old. In some specifications,

[11] The UI recipiency rate (ratio of regular state claims to all unemployed, according to the official definition in https://oui.doleta.gov/unemploy/Chartbook/a13.asp) greatly increased during the pandemic, reaching 77 percent for the year 2020. It fell thereafter, but defining the rate as a ratio to job losers with fewer than 27 weeks, those eligible for UI under regular state programs (which we calculate), recipiency remained above 50 percent throughout our study period in 2021 and even into 2022.

we also control for the reason for unemployment (distinguishing job losers from job leavers and reentrants), and an indicator for unemployment duration less than 52 weeks at $t-1$.

State fixed effects control for time-invariant differences across states, and time fixed effects control for nationwide changes in job-finding, but not for idiosyncratic fluctuations at the state level. We therefore include time-varying state-level controls for job openings, UI program performance, and seasonality. We obtain the job openings rate from the Job Openings and Labor Turnover Survey (JOLTS), measuring state-month variation in labor demand (or offer arrival rate) and calculated as the ratio of job openings to total employment plus job openings in the nonfarm sector (BLS, 2022). We also control for UI program performance that could affect UE transitions if payment delays lead to return to work earlier (Navarrete, 2023). We use data from the Department of Labor Employment and Training Administration (DOLETA) on the "continued weeks compensated time lapse" (Employment and Training Administration, 2017), and define UI timeliness as the proportion of payments made within 3 weeks of receiving the claim (Employment and Training Administration, 2022). To account for possible seasonality, we estimate the regular monthly variation in UE for each state separately by race based on unemployed in 2015-2019.

Finally, we measure factors that could account for differences in responsiveness to losing UI benefits: offered wages, replacement rates, liquid assets, and probability of obtaining a job permitting remote work. We predict offered wages using CPS outgoing rotation group data on weekly wages for re-employed workers in 2015-2019, estimating an equation based on detailed characteristics including education, industry, and occupation. We calculate unemployment benefits using these same wage predictions and a state-specific UI benefit calculator from Ganong et al. (2020) for January 2020, adding the FPUC \$300 per week for each person and estimate the replacement rate as a ratio to weekly wages. We construct liquid assets for the CPS sample based on the variable net liquidity in the Survey of Income and Program Participation (following Chetty (2008)) in 2020-2021, again using a prediction equation based on detailed worker characteristics. Finally, we follow a similar procedure to measure the probability that the individual will find employment in a job requiring face-to-face interactions, based on an equation estimated for employed workers on whether they teleworked because of Covid, using a CPS question during 2021. In examining the influence of these factors on responsiveness to $PUIend$, we estimate regressions for separate samples, dividing the distributions of these variables into first and fourth quartiles. We provide summary statistics for these variables when reporting these regression results in Subsection 4.3.

3 Methods

3.1 Estimating the Impact of Unemployment Insurance Cuts on Covid Death Rates by Race

We estimate the following equation for Covid death rates separately by race:

$$D_{rst} = \beta_r \cdot PUIend_{st-1} + \mu_{rs} + \tau_{rt} + W_{rst} \cdot \phi_r + u_{rst}, \tag{1}$$

where D_{rst} represents the Covid death rate per 100,000 of the corresponding population for race r in state s and month t. $PUIend_{st-1}$ is an indicator for treated states in July-August 2021, as described in Subsection 2.2, lagged by one month relative to D_{rst} because of the time delay involved in Covid transmission. The sample runs from March through September 2021, μ_{rs} are state fixed effects, and τ_{rt} are month fixed effects by race. W_{rst} is a vector of time-varying state-level variables, in most cases varying by race, as described in Subsection 2.3. We estimate a baseline specification without W_{rst} and then add cumulative Covid vaccination rates, its square, indoor days, controls for vulnerability measures interacted with time trends (obesity, heart disease, median income, and share of over-65 population), and a state-time-varying index of other non-pharmaceutical interventions. Estimation is weighted by state race population. In a final specification, we add the inverse probability weights (IPW) which addresses covariate balance as discussed in Subsection 2.3. Standard errors are clustered at the state level.

We interpret β_r in Equation (1) as the impact of the policy change $PUIend$ on the Covid death rate for each race. In order to assess the statistical significance of the difference between Blacks and Whites in this impact, we pool the data and estimate a triple-difference (DDD) specification that allows the coefficient on each covariate to differ by race.

The DDD set-up controls for any unobserved state-level shock that affects both Blacks and Whites similarly. DDD identification does not require assumptions on parallel trends either across treatment and control states or across Blacks and Whites (Olden and Møen, 2022), which are differenced away, but only for the triple-difference term. The identification assumption is thus that there was no simultaneous shock that differed between treated and control states and that raised Black mortality disproportionately. We examine event studies of both the treated-control difference for each month by race, separately, and for the Black-White difference in the estimated impact of the intervention.

We also carry out an alternative approach to inference by permuting the assignment of intervention among the states. For this purpose, we randomize intervention across states and re-estimate Equation (1) with 1000 replications. A comparison of our estimate with the distribution of replications provides information on the probability that the estimate could

appear from a random draw of states.

3.2 Estimating the Labor Mechanism by Race

Turning to the mechanism through which the UI cuts could have led to disproportionate increases in Black mortality relative to Whites, we estimate a linear probability model (LPM) for the transition from unemployment to employment (UE) on the sample of unemployed:

$$UE_{irst} = \alpha_r \cdot PUIend_{st} + X_{irst-1} \cdot \theta_r + V_{rst-1} \cdot \eta_r + \mu_{rs} + \tau_{rt} + \epsilon_{irst}, \tag{2}$$

where UE_{irst} is a binary indicator of transition from unemployment$_{t-1}$ to employment$_t$ for individual i of race r in state s at time (month) t, $PUIend_{st}$ is a vector of indicators for the treated states in July and August 2021, separately, and α_r is the race-specific impact of PUI termination on the job-finding rate. Because the UE—$PUIend$ relationship is contemporaneous (shown in Subsection 4.2), $PUIend$ is not lagged, and the estimation period is March—August 2021. X_{irst-1} is a vector of individual characteristics in *t-1*: age, gender, education, marital status, and child under 12, and in some specifications, it also includes a set of state-level covariates V_{rst-1} described in Subsection 2.4: job openings rate, UI timeliness rate, and state-level seasonality. Except for seasonality, we lag these state-level variables in Equation (2), and we also report estimates excluding them; they make little difference to the results.[12] State and month fixed effects (μ_{rs} and τ_{rt}) again vary by race, and standard errors are clustered on states. We estimate Equation (2) separately for Blacks and Whites, pool data for both races and estimate DDD specifications, and carry out event studies.

One potential concern about the estimated racial difference in Covid deaths resulting from the policy change could be that the magnitude reflects other racial differences, namely in either community transmission or fatality rates. The spread of infection could work differently within Black and White social networks—assuming some level of segregation by race (Williams and Collins, 2001; Hellerstein and Neumark, 2008)—if one race is more susceptible to infection or has higher rates of transmission, for instance because of different practices of social distancing. For a given level of workplace exposure resulting from $PUIend$ in the treated states, denoted E_0, there could potentially be differences by race in any of the following factors: the rate at which exposure leads to infection of the re-employed workers, i; the basic reproductive number for secondary infections, R_0; and the number of generations

[12]Note that we do not include measures of Covid outcomes as regressors because they may be simultaneously determined with UE behavior: higher UE may increase Covid, but higher Covid may reduce UE because of safety concerns. Consistent with these opposing effects, if Covid measures are included in Equation (2), the coefficient estimates on these variables are always small and vary in sign depending on the particular Covid variable(s) and other aspects of the specification. Most importantly for current purposes, including these variables has little impact on the estimated coefficient of $PUIend$.

in the chain, n, before the infection rate becomes negligible. Finally, there could be racial differences in the rate δ at which infections lead to deaths.[13] Ideally, if these quantities were known for each race, the race-specific Covid death rates resulting from the policy could be calculated as $E_0 \cdot i \cdot {R_0}^n \cdot \delta \cdot (1/P)$, where P is the relevant population (by race). None of these factors are known with any certainty, nor to our knowledge have they been reliably estimated for each race separately.[14]

We cannot distinguish these factors separately, but we can assess whether they matter collectively by estimating the marginal effect of E_0 on Covid death rates by race. To focus on the policy-induced changes, we use a two-stage least-squares (2SLS) framework. First, we compute state-level UE_{rst}, the transition rate for race r in state s and month t, after netting out differences in individual characteristics X_{irst-1} (as in Equation 2). Next, we estimate first-stage regressions of UE_{rst} on $PUIend$ and obtain fitted values representing the increase in the job-finding rate due to the policy. Finally, we use the fitted values $\widehat{UE}_{rst-1}$ as the regressor of interest in an equation with Covid deaths as the dependent variable for each race separately:

$$D_{rst} = \gamma_r \cdot \widehat{UE}_{rst-1} + \mu_{rs} + \tau_{rt} + W_{rst} \cdot \phi_r + \epsilon_{rst}, \tag{3}$$

where other variables are defined as previously and γ_r represents the impact of a one-unit change in initial exposure from $PUIend$ on Covid deaths by race.

The inclusion of state and time fixed effects varying by race in Equation (3) adjusts for differences across races, states, and time in unemployment-population ratios, allowing γ_r to be interpreted as $\partial D_{st}/\partial E_0 = i \cdot R_0^n \cdot \delta$, in which each component may vary by race r. We estimate the equation separately for Blacks and Whites and pooled as a DDD, in order to assess racial differences. A difference in γ_r would imply that the community transmission mechanism or fatality rates differ for Blacks and Whites. If there is no significant racial difference in the γ_r estimates, this would provide evidence that the relative increase in Black deaths results from greater exposure due to the policy change increasing re-employment.

Finally, we investigate factors that might account for higher responsiveness of Blacks to the UI cuts relative to Whites: wages, UI replacement rate, wealth (net liquidity), and probability of obtaining remote work. We estimate heterogeneous effects by samples divided according to the predicted values of these variables using the proxies and procedures

[13] Stemming from historical distrust (e.g., Alsan and Wanamaker, 2018), the possibly greater reluctance of Blacks to seek health care, such as vaccination or hospitalization, could produce racial differences in i, R_0^n, or δ. Singh and Venkataramani (forthcoming) find longer wait times and higher mortality rates for hospitalized Blacks compared to Whites.

[14] Rentsch et al. (2020) find no difference in the probability of death for Blacks versus Whites who tested positive for Covid. Wei et al. (2024) report a higher rate of deaths per infection among Blacks than Whites but the authors point out that case data do not capture most Covid infections, and if the capture rate is worse for Blacks, then the comparison of Black to White ratio of deaths to infections is upward biased.

described in Subsection 2.4.

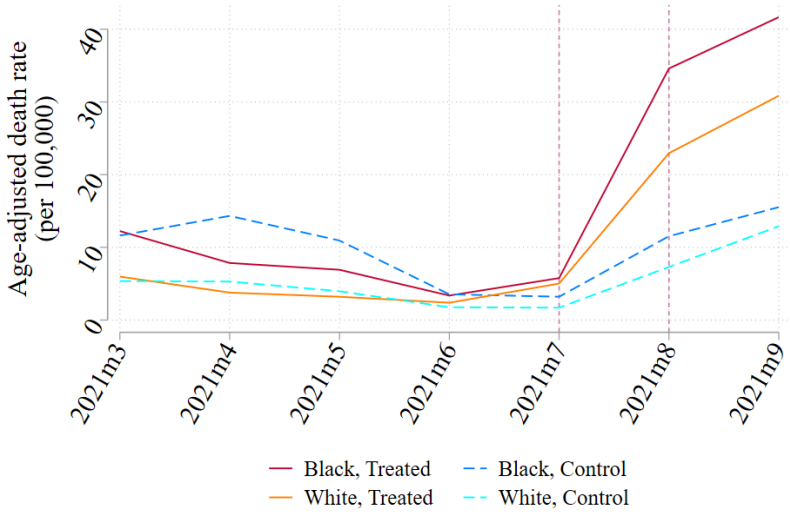
4 Results

4.1 The Impact of UI Cuts on Covid Deaths by Race

Figure 2 shows mean age-adjusted death rates for Blacks and Whites in treated and control states from March to September 2021. The vertical lines highlight July and August 2021, the intervention period during which the UI policy differed drastically across states. We include September because of lags in the Covid disease process, as discussed above, and we do not include months after September because by then all states had discontinued PUI, so there is no longer a control group.

The figure shows that Covid deaths rates by race and treatment status are very similar in March—July 2021, and they tend to drift slightly downwards together. Following the intervention months of July and August, however, the death rates increase sharply for both Blacks and Whites in the treated states relative to the control states. The increase is much greater for Blacks. There is only a slight uptick in treated states in July, but a large jump in August, consistent with lags in the process of infections, sickness, and death.

Figure 2: Monthly Covid Deaths for Blacks and Whites in Treated and Control States



Note: Mean age-adjusted death rates (per 100,000) by race and month for treated and control states, weighted by state-race population. Vertical bar: July and August 2021. The longer time series from April 2020 to April 2022 is in Appendix Figure A2.

Table 2 aggregates the monthly data from Figure 2 into a pre-intervention period (March-June 2021) and post-intervention period (August-September 2021, because of the lag) to derive a simple difference-in-differences estimator of the impact of the policy on Black and White Covid death rates. In the control states, there are slight declines from the pre- to the

post-period in the death rates of both Blacks and Whites. By contrast in the treated states, there is a marked increase for both races. The difference-in-differences estimators imply increases of 27.1 deaths per 100,000 for Blacks and 17.1 for Whites. Relative to the death rates in control states during the post-intervention period, the percentage increase in the Black rate is 100 percent and 69 percent for Whites. The DDD estimate of 10.1 deaths per 100,000 is highly significant and represents a 58 percent difference in the mortality impact of the treatment. The table also shows analogous results using IPW weights, as described in Subsection 2.3, with an even larger DDD estimate of 13.6 deaths per 100,000.

Table 2: Mean Covid Death Rates by Race, Treatment, and Time Period

	Control			Treated				
	Pre	Post	Post-Pre	Pre	Post	Post-Pre	DiD	DiD IPW
Black	10.11	13.53	3.42***	7.60	38.13	30.53***	27.11***	24.75***
	[7.08]	[7.89]	(1.25)	[3.70]	[8.22]	(1.12)	(1.76)	(1.85)
White	4.10	10.10	6.00***	3.85	26.90	23.06***	17.06***	11.11***
	[2.93]	[7.28]	(0.82)	[1.96]	[12.19]	(1.43)	(1.57)	(1.96)
Black-White	6.01***	3.43*	-2.58	3.76***	11.23***	7.47***		
	(0.74)	(1.99)	(1.76)	(0.49)	(3.28)	(2.41)		
DDD							**10.05*** **	
							(2.93)	
Black-White IPW	5.56***	1.64	-3.92*	3.85***	13.58***	9.72***		
	(0.74)	(2.98)	(2.35)	(0.51)	(3.85)	(2.81)		
DDD IPW								**13.64*** **
								(3.69)
Observations	208	104	312	152	76	228	540	540

Note: Mean age-adjusted death rates (per 100,000) by race and control/treated state group. Pre-period includes March-June 2021, and post-period includes August-September 2021, with July 2021 omitted. Standard deviations are in brackets, and standard errors from difference or difference-in-differences are in parentheses. Observations are weighted by race population by state, and IPW results are further weighted by inverse probability weighting. *** p<0.01, ** p<0.05, * p<0.1.

These simple means do not control for any other factors, so we next turn to estimates of Equation (1). In Table 3, three specifications are shown: column (1) contains the results from controlling for only state and month fixed effects; (2) adds controls for vaccination rate, squared vaccination rate, indoor days, the vulnerability variables (median earnings, over-65 population share, obesity rate, and cardiovascular disease rate, each interacted with a linear time trend) and the NPI index; (3) has the same controls but is additionally weighted using IPW. The estimates consistently show a much sharper rise in death rates for Blacks than White. Using the numbers from the second specification relative to the baseline rates for the control states in the intervention period from Table 2, the Black death rate rises about 156 percent, while the White rate rise is about 106 percent. The pooled regressions, including both races in the sample, show that the difference is statistically significant and estimated to be about 10 deaths per 100,000 in specifications (1) and (2) and 12 in specification (3).

Table 3: Estimating the Impact of $PUIend$ on Covid Deaths by Race

	(1)	(2)	(3)
White			
$PUIend_{t-1}$	16.34***	10.69***	6.24*
	(3.03)	(2.98)	(3.14)
Black			
$PUIend_{t-1}$	26.09***	21.08***	17.37***
	(3.16)	(3.51)	(4.09)
Black-White			
$PUIend_{t-1} \times Black$	9.75***	10.39***	11.13***
	(2.66)	(2.43)	(2.82)
State and time FE	Y	Y	Y
Controls		Y	Y
Weight	Population		Pop x IPW

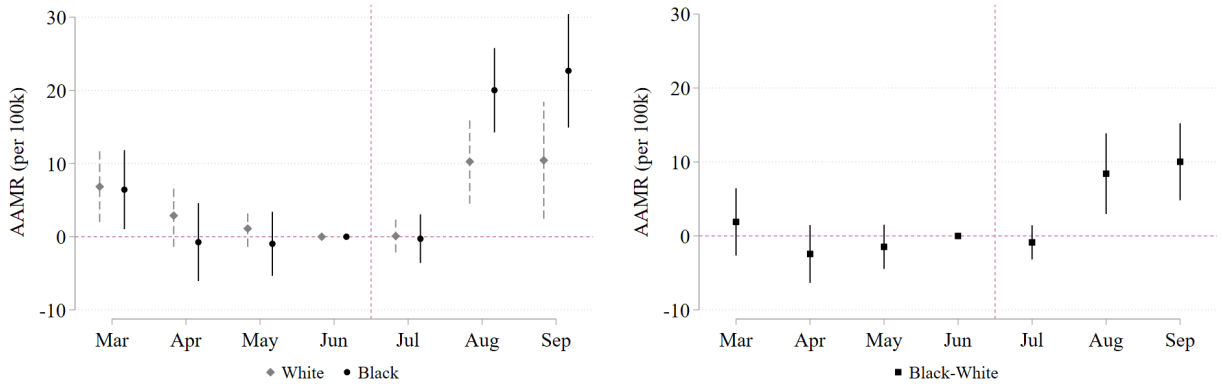
Note: Estimates of Equation (1). Controls = one-month lagged vaccination rate, its square, indoor days; age over 65 population, cardiovascular disease, obesity population shares and median earnings interacted with time trends; and NPI index. The Black-White difference is estimated with DDD. Estimates are weighted by state-level population by race. Standard errors are clustered at state level and are in parentheses. Observations = 630 (45 states, 7 months, 2 races). Coefficients on covariates are in Appendix Table A4. *** p<0.01, ** p<0.05, * p<0.1.

Using specification (2) from Table 3, event studies for each race and for the races pooled are shown in Figure 3. The coefficients (with 95 percent confidence intervals) represent the results of estimating the difference between Covid death rates in treated and control states for each month of the sample (treating June 2021, when all rates were the same, as we observed in Figure 2, as the reference month). The figures show that the differences between treated and control state rates are negligible for both races during late spring and early summer 2021 then jump for both in August and September, resulting from the lag in death rates after $PUIend$. The Black rate jumps much higher than the White rate, and the difference is highly statistically significant.

Permutation tests provide an alternative for assessing the statistical significance of the estimated difference between the effects on Black and White death rates. For this purpose, we randomize the intervention assignment and recompute the estimates in specifications of Equation (3), reported in Columns (1) and (2) of Table 2. For each specification, we carry out this permutation 1000 times. The results in Figure 4 show that the estimated differences of 9.75 and 10.39 from that table are extreme outliers relative to the distribution of outcomes under the permutations, supporting the hypothesis that Black death rates rose much more substantially than those of Whites as a result of the cutting of UI benefits.

From the estimated impact on Covid death rates by race in Table 3, we can estimate the number of deaths resulting from the policy change in the affected states. Multiplying the coefficients in specification (2) by the Black and White population numbers in those states (13,281,206 and 52,521,598 in the 2020 Decennial Census, respectively) and by the

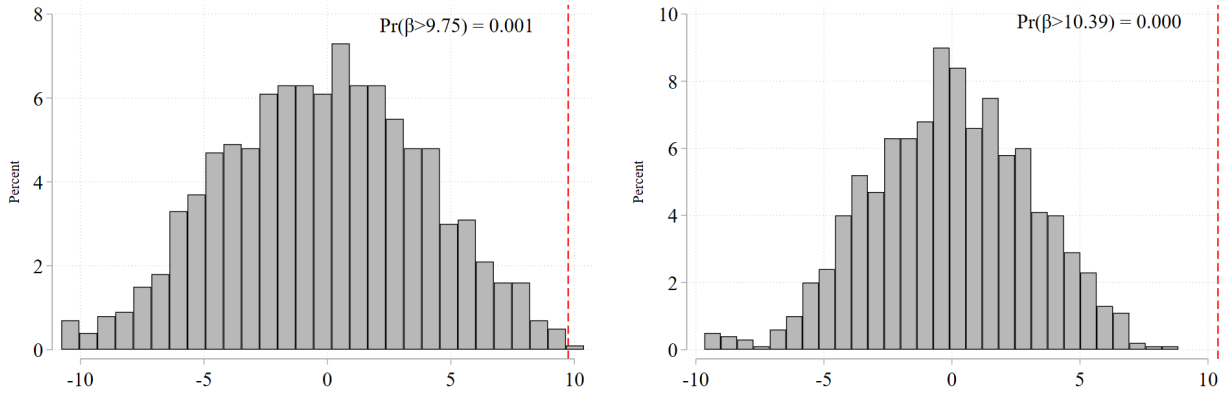
Figure 3: Event Studies for Covid Death Rates



(a) Black and White separately (b) Black-White difference

Note: Graphs (a) and (b) contain event studies by race separately and for the triple-difference specification with races pooled, respectively. Controls are same as in Table 3 column (2). Vertical lines are 95% confidence intervals. Estimates are weighted by race population. Standard errors are clustered at state level. Event studies of other specifications are in Appendix Figures A4 and A5.

Figure 4: Permutation Tests of the DDD Estimates



(1) State, Month FEs (2) + Full Controls

Note: Permutations are conducted for the DDD version of Equation (1) by randomly assigning 19 treated states and calculating $\widehat{\beta_B - \beta_W}$ with 1,000 simulations. Graph (1) uses the specification in Column (1) of Table 3 and Graph (2) uses the specification in Column (2). The vertical dashed lines indicate the DDD estimates reported in that table for each specification, respectively.

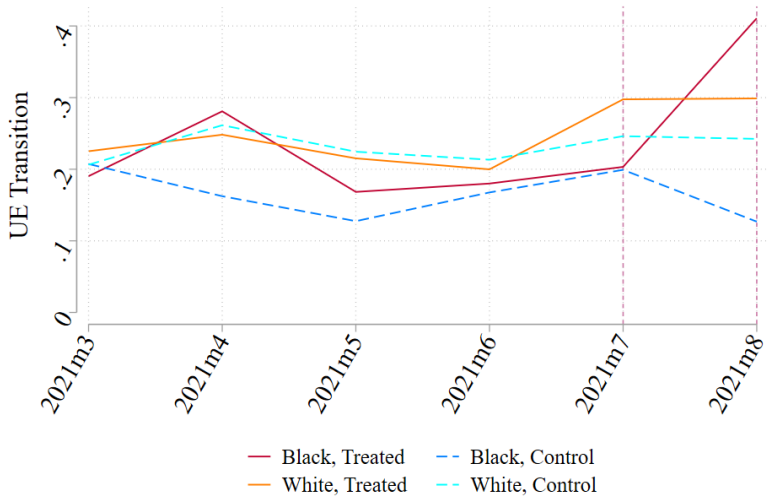
two months of policy difference, we obtain estimates of 5599 (95% CI: 3772, 7427) for Black deaths and 11,229 (5094, 17,365) for White deaths. The point estimate of Black deaths is half that of Whites, but the population ratio is only one-quarter, implying that the mortality rate associated with the policy is twice as high for Blacks as Whites.

4.2 The Re-Employment Mechanism

In this paper, we hypothesize that discontinuation of pandemic UI benefits raised Covid death rates by increasing transitions from unemployment to employment, leading to higher exposure and community spread of illness. If the re-employment response to the loss of UI was greater for Blacks than Whites, their increase in exposure would be greater, and to the extent that social interactions are segregated by race, the subsequent chain of infections would affect Blacks more than Whites. Alternatively, perhaps the disease process works differently for Blacks than Whites, so that for instance infections are transmitted more quickly, or infections more frequently lead to deaths in a Black compared to a White community. We provide evidence on these possibilities in this section.

Figure 5 reports means of UE transitions by month for treated and control states, and for Black and White unemployed individuals. Similar to what we saw for Covid deaths in Figure 2, the UE rates tend to be similar and move together in the pre-treatment period, but UE in the treated states diverges from the controls during the intervention period, with the Black rate rising sharply in August. On average across the two months July-August, the Black UE rate rises twice as much as the White rate.

Figure 5: Monthly UE Transitions, Treated and Control States, Black and White



Note: Mean probability of UE by treated and control states and by race over month, weighted by CPS weight. The vertical bar indicates the intervention period, July and August 2021.

Table 4 shows means of UE for the pre-intervention (March—June 2021) and intervention (July—August) periods. Analogously to Table 2, the results permit a simple difference-in-differences estimation of the policy effect. The racial difference is statistically insignificant in July, but is stark in August, when the Black UE rate is double and the White rate is 25 percent higher, compared to the control states during the same period.

Table 4: Mean UE Transition Rates by Race, Treatment, and Time Period

	Control			Treated			DiD	
	Pre	July	August	Pre	July	August	Jul−Pre	Aug−Pre
Black	0.16	0.20	0.13	0.20	0.20	0.42	-0.04	0.25***
	[0.37]	[0.40]	(0.33)	[0.40]	[0.40]	(0.50)	(0.05)	(0.05)
White	0.23	0.25	0.24	0.22	0.30	0.29	0.06*	0.06*
	[0.42]	[0.43]	(0.43)	[0.42]	[0.46]	(0.46)	(0.03)	(0.03)
Difference	-0.06***	-0.05*	-0.11***	-0.02	-0.10**	0.12***		
	[0.02]	[0.03]	(0.03)	[0.02]	[0.04]	(0.05)		
DDD-Jul							**-0.09**	
							(0.06)	
DDD-Aug								**0.19*** **
								(0.06)
Observations: Black	745	191	188	481	102	93		
Observations: White	2942	711	696	1763	411	392		

Note: Mean unemployment-employment transition rate by race and control/treated state group. Pre-period includes March-June 2021, and post-period is divided into July and August 2021. Standard deviations are in brackets, and standard errors are in parentheses. Observations are weighted by CPS weights. *** p<0.01, ** p<0.05, * p<0.1.

Estimates of the α_r coefficient on $PUIend$ in the linear probability model of Equation (2) are reported in Table 5. Although the estimated coefficients for Whites are larger in July, they are not statistically different from zero; nor is the White-Black differences. Consistently across specifications, the Black coefficient is much larger in August, however, and the racial difference is highly significant that month. The July-August difference could reflect timing differences in the data: the CPS reference period is the week containing the 12th while the CDC mortality data refer to the calendar month. But the jump in the racial difference in UE in August is also consistent with the timing of the racial mortality gap, which emerges only in August, as shown in Figure 3.

An event study of the difference in the Black and White coefficients by month, corresponding to separate estimation of Equation (2) is graphed in Figure 6. For March-June 2021, the data show no significant Black-White differences in coefficients and no indication of a trend, but the difference jumps and becomes statistically significant in August.

Next, we turn to the question of whether the higher Covid death rate among Blacks associated with the UI cuts might be explained by a stronger community spread mechanism, for instance from a higher R_0, or by a higher fatality rate for a given level of infections. In this case, the coefficients on $\widehat{UE}$ in Equation (3) would be higher for Blacks than Whites, as

Table 5: Estimating Impact of $PUIend$ on UE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	White	Black	White	Black	White	Black	White	Black
$PUIend_{Jul}$	0.059	-0.025	0.057	-0.018	0.047	-0.032	0.047	-0.035
	(0.038)	(0.046)	(0.034)	(0.046)	(0.033)	(0.046)	(0.034)	(0.047)
$PUIend_{Aug}$	0.068**	0.240***	0.066**	0.248***	0.058*	0.242***	0.063*	0.237***
	(0.028)	(0.058)	(0.027)	(0.058)	(0.030)	(0.062)	(0.032)	(0.067)
Black-White Difference								
$PUIend_{Jul} \times Black$	-0.084		-0.075		-0.079		-0.081	
	(0.059)		(0.057)		(0.057)		(0.057)	
$PUIend_{Aug} \times Black$	0.172***		0.182***		0.184***		0.174**	
	(0.063)		(0.062)		(0.063)		(0.069)	
State and time FE	Y		Y		Y		Y	
Demog			Y		Y		Y	
Unemp characteristics					Y		Y	
State-level controls							Y	

Note: Estimates of Equation (2). Individual characteristics = age, sex, foreign born, education, marital status, child under 12. State level covariates = job opening rates, UI payment timeliness in $t-1$, and UE seasonality by race. Unemp char = job leaver, reentrant, duration < 52 week indicators. Estimates are weighted by CPS weights. Standard errors clustered on state are in parentheses. Observations = 1770 for Black, 6802 for White from March through August 2021. Coefficients on covariates are in Appendix Table A5. *** p<0.01, ** p<0.05, * p<0.1.

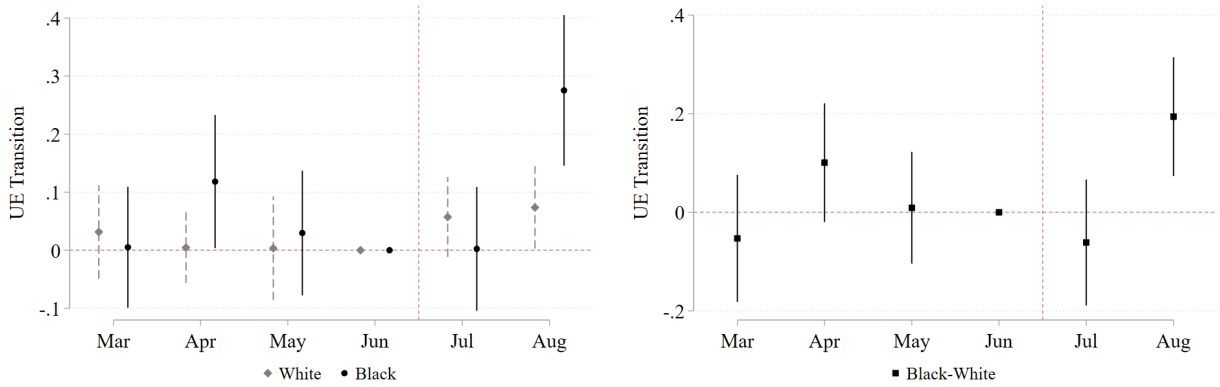
a given initial exposure from re-employment translates into a higher death rate. To assess this possibility, Table 6 contains the results of the 2SLS procedure described in Subsection 3.2. The first-stage results, analogous to those in Table 5 but estimated at the state level, show estimates similar to those in that table: Black workers respond more strongly than Whites to the termination of pandemic UI.

Table 6: Estimated Marginal Effects of Policy-Induced Re-Employment on Mortality

Dependent Variable	First stage UE_{t-1}		2SLS estimates Mortality Rate (%)	
	White	Black	White	Black
$PUIend_{t-1}$	0.065**	0.147***		
	(0.027)	(0.039)		
Black-White difference	0.0814*			
	(0.047)			
$\widehat{UE}_{t-1}$			0.250**	0.181***
			(0.106)	(0.046)
Black-White difference			-0.070	
			(0.114)	
Observations	315	249	315	249
Anderson-Rubin Wald F statistics	28.29	13.58		

Note: Estimates of Equation (6). All specifications include state and month fixed effects. Estimates are weighted by race population and adjusted by standard error from calculating UE_{st}. Some Black-state observations are missing because of small samples. Standard errors clustered at state level are in parentheses. For comparison, the OLS estimate of the Black-White difference in the effect of UE on the mortality rate is 0.002 (0.006). *** p<0.01, ** p<0.05, * p<0.1.

Figure 6: Event Studies for UE Transitions



(a) Black and White separately (b) Black-White difference

Note: Graphs (a) and (b) contain event studies by race separately and for the DDD specification with races pooled, respectively. Controls are same as in Table 5 columns (7-8). Vertical lines are 95% confidence intervals with standard errors clustered on state. Estimates are weighted by CPS weights. Event studies of other specifications are in Appendix Figures A6, A7, and A8.

The estimated coefficients of Equation (3) in Table 6 are similar for Blacks and Whites, however, and the racial difference is not statistically significant. Thus, the marginal impact of a change in UE resulting from the policy is estimated as essentially equal for the two races: the data do not support the hypothesis that the higher death rate of Blacks resulted from a stronger community transmission mechanism, a lower vaccination rate, or a higher fatality rate for a given level of infections.[15] Rather, the result supports the hypothesis that the racial gap in mortality was caused by a greater response Blacks to $PUIend$.

4.3 Factors Affecting Responses to UI Cuts

Finally, we consider factors that might account for the higher average job-finding response of Blacks to the UI cuts relative to Whites: lower wages, higher UI replacement rates, lower liquid assets, and probability of working from home. For each factor, the top panel of Table 7 contains the mean value and the Black share of the population in the first and fourth quartiles of the factor's distribution. The mean values show the substantial variation in the distributions of these variables, with the fourth quartile always much larger than the first. The Black share is much higher in the first quartiles of wages, net liquidity, and working from home, while it is much lower for the replacement ratio.[16]

The bottom panel of Table 7 provides regression estimates analogous to those in Table 5

[15]Concerning vaccinations, which are controlled for separately by race in Equation 1, the racial gap in vaccination rates over our estimation period is actually larger in control states than in treated states, as shown in Appendix Figure A3.

[16]The high replacement ratio in the fourth quartile reflects the FPUC program's $300 topping up of all UI benefits during this period.

Table 7: Estimated Impact of $PUIend$ on UE by Groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log(weekly wage)		Replacement ratio		log(Net Liquidity)		$Pr(WFH)$	
	Q1	Q4	Q1	Q4	Q1	Q4	Q1	Q4
Factor mean (unlogged)	448	1,265	0.372	0.878	-71,741	1,144,182	0.029	0.359
Black share (B/(B+W))	0.419	0.113	0.150	0.375	0.639	0.086	0.332	0.192
Regression Estimates								
$PUIend$	0.146***	0.038	0.029	0.181***	0.090***	-0.003	0.107**	-0.037
	(0.042)	(0.034)	(0.046)	(0.051)	(0.027)	(0.041)	(0.048)	(0.064)
DDD	-0.107**		0.153**		-0.092*		-0.143**	
	(0.042)		(0.062)		(0.047)		(0.066)	
Observations	2,145	2,143	1,829	1,821	2,183	2,175	2,144	2,143

Note: Specifications control for state and time fixed effects and individual characteristics, including age, sex, foreign born, education, marital status, child under 12. Q4 indicates 4th quartile, and Q1 indicates the 1st quartile. Differences are calculated by triple difference. Estimates are weighted by CPS weights. Standard errors clustered at state level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

with the bottom and top quartile samples. For each factor, the results show a large statistical difference in the UE response to $PUIend$ between the first and fourth quartiles. Workers with low wages, high replacement rates, low net liquidity, and low probability of working from home, who are much more likely to be Black than White, display higher responsiveness to $PUIend$ than workers with the opposite characteristics. These heterogeneity results provide further support for, and help explain, the higher job-finding rate of Blacks in response to the loss of UI benefits.

5 Conclusion

Laws and policies that are facially neutral on race may nonetheless have racially disparate effects. This paper focuses on one example: Black-White differences in Covid deaths in relationship to unemployment insurance (UI) cuts. We show that states discontinuing pandemic UI programs in early summer 2021 experienced Covid death rates that more than doubled for both Blacks and Whites, but with a substantially higher increase for Blacks. Our triple-difference estimates are robust to unobserved confounding factors, and there is no indication of a pre-trend in the months leading up to the cuts. Permutation tests show the estimates are large outliers relative to placebos.

On the mechanism through which UI could affect Covid outcomes differentially by race, we provide evidence that the job-finding responsiveness to the UI policy change was greater for Blacks, so that their re-employment rates rise much more than those of Whites. Again, this result is robust to alternative specifications and is not driven by an observable difference in trends. Examining whether Black mortality could have increased disproportionately

because of stronger community transmission or a higher fatality rate conditional on infection, we find no evidence supporting these possibilities: the marginal impact of a rise in re-employment resulting from the policy does not differ by race, implying that the racial gap in Covid deaths is driven by the magnitude of the re-employment response. Finally, we provide evidence that the higher responsiveness of Blacks to the UI cuts may have resulted from their lower wages, higher replacement rates, lower wealth, and smaller remote working possibilities, compared to Whites.

Our estimation results imply deaths of 5599 (95% CI: 3772, 7427) among Blacks and 11,229 (5094, 17,365) among Whites attributable to the policy in the treated states. Relative to their population shares in these states, the estimates imply that the mortality rate associated with the policy is twice as high for Blacks as Whites. The numbers of deaths are substantial particularly given the short two-month time period of the intervention. For a point of comparison, Miller et al. (2021) find that Medicaid expansion reduced mortality by 19,200 over four years in expansion states, which have a larger population than the treated states in our study. If we extrapolate our estimates for the treated states to the controls, following Miller et al. (2021), we can estimate the lives saved in states that continued pandemic UI programs for two additional months. This procedure implies that those states experienced reductions in Black deaths of 8,283 (CI: 5,580-10,987) and 22,912 (CI: 10,393-35,431) in White deaths as a result of maintaining the UI programs through the summer.

There are a number of important caveats to these results. The available data for Covid outcomes by race are limited to state-month-level Covid deaths, requiring the analysis of the main outcome variable to be carried out at this level of aggregation. While this level is the same as that at which the UI policy varies, it would have been preferable to control for more granular types of heterogeneity, as well as to trace out the effects with higher frequency data and for a longer period than the two months of policy difference. The re-employment analysis uses data on unemployed individuals including whether they become re-employed in the following month which allow us to control for many important characteristics, but we do not observe actual UI receipt, and thus our estimates reflect intent-to-treat effects, and are understated relative to average intervention effects; to the extent that the UI take-up rate is lower for Blacks than Whites (Skandalis et al., 2022; Kuka and Stuart, 2025), our estimates of the difference in the policy impact on re-employment are also understated. Moreover, while our 2SLS estimates suggest that the overall community transmission channel from workplace exposure to Covid deaths is similar for Blacks and Whites, the available data do not allow tracking of infections through family and social networks, a process that cannot be observed directly. Finally, the measures of the factors - wages, replacement rates, liquid assets, and probability of working from home - are all imperfect proxies based on prediction

equations estimated on data other than the regression sample.

Despite these caveats, we believe our study offers not only an analysis of the specific effects of UI cuts on Covid deaths during the summer of 2021, but also a useful empirical example of structural or systemic discrimination that complements experimental studies such as Kline et al. (2022) and Bohren et al. (2025). A facially neutral policy produced racially disparate impacts because of inherited disadvantages that resulted in different behavioral responses to the policy. This research therefore carries lessons both on how such disadvantages may be perpetuated by putatively race-neutral programs and on how policymakers could plan for a future pandemic.

References

Aaronson, Daniel, Daniel Hartley, and Bhashkar Mazumder, "The effects of the 1930s HOLC "redlining" maps," *American Economic Journal: Economic Policy*, 2021, *13* (4), 355–392.

Aburto, José Manuel, Andrea M Tilstra, Ginevra Floridi, and Jennifer Beam Dowd, "Significant impacts of the COVID-19 pandemic on race/ethnic differences in US mortality," *Proceedings of the National Academy of Sciences*, 2022, *119* (35), e2205813119.

Alsan, Marcella, Amitabh Chandra, and Kosali Simon, "The great unequalizer: initial health effects of COVID-19 in the United States," *Journal of Economic Perspectives*, 2021, *35* (3), 25–46.

_ **and Marianne Wanamaker**, "Tuskegee and the health of black men," *Quarterly Journal of Economics*, 2018, *133* (1), 407–455.

Bartscher, Alina K, Moritz Schularick, Moritz Kuhn, and Paul Wachtel, "Monetary policy and racial inequality," *Brookings Papers on Economic Activity*, 2022, *2022* (1), 1–63.

Bertrand, Marianne and Sendhil Mullainathan, "Are Emily and Greg more employable than Lakisha and Jamal? A field experiment on labor market discrimination," *American Economic Review*, 2004, *94* (4), 991–1013.

Blanchflower, David G, Phillip B Levine, and David J Zimmerman, "Discrimination in the small-business credit market," *Review of Economics and Statistics*, 2003, *85* (4), 930–943.

BLS, "Job openings levels and rates by industry and region, seasonally adjusted," https://www.bls.gov/news.release/jolts.t01.htm 2022. accessed on 02.13.2023.

Bohren, J Aislinn, Peter Hull, and Alex Imas, "Systemic Discrimination: Theory and Measurement," *Quarterly Journal of Economics*, 2025, *140* (3), 1743–1799.

Braveman, Paula A, Elaine Arkin, Dwayne Proctor, Tina Kauh, and Nicole Holm, "Systemic And Structural Racism: Definitions, Examples, Health Damages, And Approaches To Dismantling," *Health Affairs*, 2022, *41* (2), 171–178.

Bundy, Joshua D, Katherine T Mills, Hua He, Thomas A LaVeist, Keith C Ferdinand, Jing Chen, and Jiang He, "Social determinants of health and premature death among adults in the USA from 1999 to 2018: a national cohort study," *Lancet Public Health*, 2023, *8* (6), e422–e431.

Carey, Patrick, Jeffrey A Groen, Bradley A Jensen, Thomas J Krolik, and Anne E Polivka, "Applying for and receiving unemployment insurance benefits during the coronavirus pandemic," *Monthly Labor Review*, 2021, pp. 1–38.

CDC, "COVID-19 Vaccinations in the United States," https://covid.cdc.gov/covid-data-tracker/#vaccinations_vacc-people-onedose-pop-total 2023. accessed on 05.17.2022.

CDC, "Single-Race Population Estimates 2010-2019," https://wonder.cdc.gov/Single-Race-v2019.HTML 2024. accessed on 05.17.2022.

_ , "Underlying Cause of Death, 2018-2022, Single Race," https://wonder.cdc.gov/ucd-icd10-expanded.html 2024. accessed on 05.17.2022.

Chen, M Keith, Kareem Haggag, Devin G Pope, and Ryne Rohla, "Racial disparities in voting wait times: Evidence from smartphone data," *Review of Economics and Statistics*, 2022, *104* (6), 1341–1350.

Chetty, Raj, "Moral hazard versus liquidity and optimal unemployment insurance," *Journal of Political Economy*, 2008, *116* (2), 173–234.

Coombs, Kyle, Arindrajit Dube, Calvin Jahnke, Raymond Kluender, Suresh Naidu, and Michael Stepner, "Early withdrawal of pandemic unemployment insurance: Effects on employment and earnings," *AEA Papers and Proceedings*, 2022, *112*, 85–90.

Derenoncourt, Ellora and Claire Montialoux, "Minimum wages and racial inequality," *Quarterly Journal of Economics*, 2021, *136* (1), 169–228.

Devakumar, Delan, Sujitha Selvarajah, Ibrahim Abubakar, Seung-Sup Kim, Martin McKee, Nidhi S Sabharwal, Angela Saini, Geordan Shannon, Alexandre IR White, and E Tendayi Achiume, "Racism, xenophobia, discrimination, and the determination of health," *Lancet*, 2022, *400* (10368), 2097–2108.

Dwyer-Lindgren, Laura, Mathew M Baumann, Zhuochen Li, Yekaterina O Kelly, Chris Schmidt, Chloe Searchinger, Wichada La Motte-Kerr, Thomas J Bollyky, Ali H Mokdad, and Christopher JL Murray, "Ten Americas: a systematic analysis of life expectancy disparities in the USA," *Lancet*, 2024, *404* (10469), 2299–2313.

Employment and Training Administration, "Unemployment Insurance 401 Handbook," 2017.

_ , "Benefits timeliness and quality (BTQ) reports of state workforce agencies," https://oui.doleta.gov/unemploy/btq.asp 2022. accessed on 02.08.2023.

Faust, Jeremy Samuel, Harlan M Krumholz, Chengan Du, Katherine Dickerson Mayes, Zhenqiu Lin, Cleavon Gilman, and Rochelle P Walensky, "All-cause excess mortality and COVID-19–related mortality among US adults aged 25-44 years, March-July 2020," *JAMA*, 2021, *325* (8), 785–787.

Flood, Sarah, Miriam King, Renae Rodgers, Steven Ruggles, J. Robert Warren, Daniel Backman, Annie Chen, Grace Cooper, Stephanie Richards, Megan Schouweiler, and Michael Westberry, "Integrated Public Use Microdata Series, Current Population Survey: Version 11.0 [dataset]," 2024. https://doi.org/10.18128/D030.V11.0.

Foster, Thomas B, Leticia Fernandez, Sonya R Porter, and Nikolas Pharris-Ciurej, "Racial and ethnic disparities in excess all-cause mortality in the first year of the COVID-19 pandemic," *Demography*, 2024, *61* (1), 59–85.

Ganong, Peter, Fiona E Greig, Pascal J Noel, Daniel M Sullivan, and Joseph S Vavra, "Spending and job-finding impacts of expanded unemployment benefits: Evidence from administrative micro data," Technical Report, National Bureau of Economic Research 2022. No. w30315.

_ , Pascal Noel, and Joseph Vavra, "US unemployment insurance replacement rates during the pandemic," *Journal of Public Economics*, 2020, *191*, 104273.

Goldman, Noreen, Anne R Pebley, Keunbok Lee, Theresa Andrasfay, and Boriana Pratt, "Racial and ethnic differentials in COVID-19-related job exposures by occupational standing in the US," *PLOS One*, 2021, *16* (9), e0256085.

Guo, Xiaohui, Lizhong Peng, and Xuezhu Shi, "The Impact of Unemployment Insurance Benefit Cuts on Mental Health: Evidence From Early Pandemic Program Expirations," *Health Economics*, 2026.

Hellerstein, Judith K and David Neumark, "Workplace segregation in the United States: Race, ethnicity, and skill," *Review of Economics and Statistics*, 2008, *90* (3), 459–477.

Hoffman, Jason, "Coronavirus death counts for money as cases again hit record levels," *CNN Politics*, 2020.

Holzer, Harry J, R Glenn Hubbard, and Michael R Strain, "Did pandemic unemployment benefits reduce employment? Evidence from early state-level expirations in June 2021," Technical Report, National Bureau of Economic Research 2021. No. w29575.

KFF, "COVID-19 Vaccinations by Race/Ethnicity [data]," 2022. Accessed: 2026-01-10.

Kline, Patrick, Evan K Rose, and Christopher R Walters, "Systemic discrimination among large US employers," *Quarterly Journal of Economics*, 2022, *137* (4), 1963–2036.

Kuka, Elira, "Quantifying the Benefits of Social Insurance: Unemployment Insurance and Health," *The Review of Economics and Statistics*, 07 2020, *102* (3), 490–505.

_ and Bryan A Stuart, "Racial inequality in unemployment insurance receipt," *Journal of Public Economics*, 2025, *247*, 105401.

Lang, Kevin and Ariella Kahn-Lang Spitzer, "Race discrimination: An economic perspective," *Journal of Economic Perspectives*, 2020, *34* (2), 68–89.

Lindo, Jason M, Krishna Regmi, and Isaac D Swensen, "Stable income, stable family," *Review of Economics and Statistics*, 2025, *107* (3), 653–667.

Lundberg, Dielle J, Elizabeth Wrigley-Field, Ahyoung Cho, Rafeya Raquib, Elaine O Nsoesie, Eugenio Paglino, Ruijia Chen, Mathew V Kiang, Alicia R Riley, Yea-Hung Chen et al., "COVID-19 mortality by race and ethnicity in US metropolitan and nonmetropolitan areas, March 2020 to February 2022," *JAMA network open*, 2023, *6* (5), e2311098.

Maddox, Karen E Joynt, Mat Reidhead, Joshua Grotzinger, Timothy McBride, Aaloke Mody, Elna Nagasako, Will Ross, Joseph T Steensma, and Abigail R Barker, "Understanding contributors to racial and ethnic inequities in COVID-19 incidence and mortality rates," *PLOS One*, 2022, *17* (1), e0260262.

Meyer, Bruce, Nikolas Mittag, and Derek Wu, "Race, ethnicity, and measurement error," *University of Chicago, Becker Friedman Institute for Economics Working Paper*, 2024, (2024-105).

Miller, Sarah, Norman Johnson, and Laura R Wherry, "Medicaid and mortality: new evidence from linked survey and administrative data," *Quarterly Journal of Economics*, 2021, *136* (3), 1783–1829.

Navarrete, Michael, "COBOLing together UI benefits: How delays in automatic stabilizers affect aggregate consumption," 2023. Working Paper. Available at SSRN 3928948.

Olden, Andreas and Jarle Møen, "The triple difference estimator," *Econometrics Journal*, 2022, *25* (3), 531–553.

Park, Sungbin, Kyung Min Lee, and John S Earle, "Premature termination of unemployment benefits increased COVID-19 transmission and deaths in the USA," *Nature Human Behaviour*, 2026, pp. 1–15.

Parolin, Zachary and Clemente Pignatti, "Employment and consumption responses to the withdrawal of unemployment benefits," *ILR Review*, 2025, *78* (3), 543–570.

Pincus, Fred L, "Discrimination comes in many forms: Individual, institutional, and structural," *American Behavioral Scientist*, 1996, *40* (2), 186–194.

Raifman, Julia, Katelyn Nocka, Daniel K. Jones et al., "COVID-19 US State Policy Database [dataset]," 2022. Accessed: 2024-01-10.

Rentsch, Christopher T, Farah Kidwai-Khan, Janet P Tate, Lesley S Park, Joseph T King Jr, Melissa Skanderson, Ronald G Hauser, Anna Schultze, Christopher I Jarvis, Mark Holodniy et al., "Patterns of COVID-19 testing and mortality by race and ethnicity among United States veterans: A nationwide cohort study," *PLOS Medicine*, 2020, *17* (9), e1003379.

Rim, Nayoung, Roman Rivera, Andrea Kiss, and Bocar Ba, "The black-white recognition gap in award nominations," *Journal of Labor Economics*, 2024, *42* (1), 1–23.

Singh, Manasvini and Atheendar Venkataramani, "Rationing by race," *American Economic Review*, forthcoming.

Skandalis, Daphné, Ioana Marinescu, and Maxim N Massenkoff, "Racial Inequality in the U.S. Unemployment Insurance System," Working Paper 30252, National Bureau of Economic Research July 2022.

Small, Mario L and Devah Pager, "Sociological perspectives on racial discrimination," *Journal of Economic Perspectives*, 2020, *34* (2), 49–67.

Tuttle, Cody, "Racial disparities in federal sentencing: Evidence from drug mandatory minimums," *Review of Economic Studies*, 2026.

Weather Data Depot, "Monthly degree day comparison," https://weatherdatadepot.com/degree-day-comparison 2023. accessed on 03.06.2022.

Wei, Stanley C, Dane Freeman, Austin Himschoot, Kristie EN Clarke, Miriam E Van Dyke, Jennifer Adjemian, Farida B Ahmad, Tina J Benoit, Kevin Berney, Adi V Gundlapalli et al., "Who Gets Sick From COVID-19? Sociodemographic Correlates of Severe Adult Health Outcomes During Alpha-and Delta-Variant Predominant Periods: September 2020–November 2021," *Journal of Infectious Diseases*, 2024, *229* (1), 122–132.

Whittaker, Julie M and Katelin P Isaacs, "Current status of unemployment insurance (UI) benefits: Permanent-law programs and COVID-19 pandemic response," 2022.

Williams, David R. and Chiquita Collins, "Racial Residential Segregation: A Fundamental Cause of Racial Disparities in Health," *Public Health Reports®*, 2001, *116* (5), 404–416. PMID: 12042604.

Zalla, Lauren C, Grace E Mulholland, Lindsey M Filiatreau, and Jessie K Edwards, "Racial/ethnic and age differences in the direct and indirect effects of the COVID-19 pandemic on US mortality," *American Journal of Public Health*, 2022, *112* (1), 154–164.

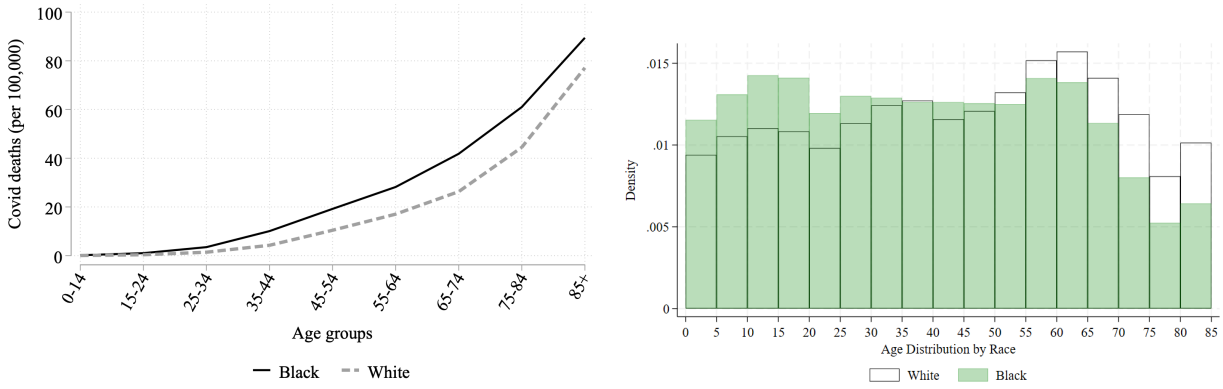
Appendix

Table A1: Racial Composition of Treated and Control States

	Control		Treated	
	Civilian Population	Experienced Unemployed	Civilian Population	Experienced Unemployed
All Races				
Hispanic	0.19	0.26	0.18	0.23
Non-Hispanic White	0.59	0.47	0.61	0.47
Non-Hispanic Black	0.11	0.15	0.15	0.24
Non-Hispanic Asian	0.09	0.08	0.04	0.03
Black and White only				
Non-Hispanic White	0.85	0.75	0.80	0.66
Non-Hispanic Black	0.15	0.24	0.20	0.33

Note: Mean population shares, January-June 2021. The sample is restricted to civilian aged 20-64, month-in-sample 2-4 and 6-8. Experienced unemployed sample further restricts to those who were employed before and were unemployed a month before the survey ($t-1$). Treated states are those terminating pandemic UI programs June 12 - July 3, 2021, and control states are those terminating the programs September 4. Observations are weighted by the CPS weights.

Figure A1: Covid Mortality by Age and Age Distribution, by Race

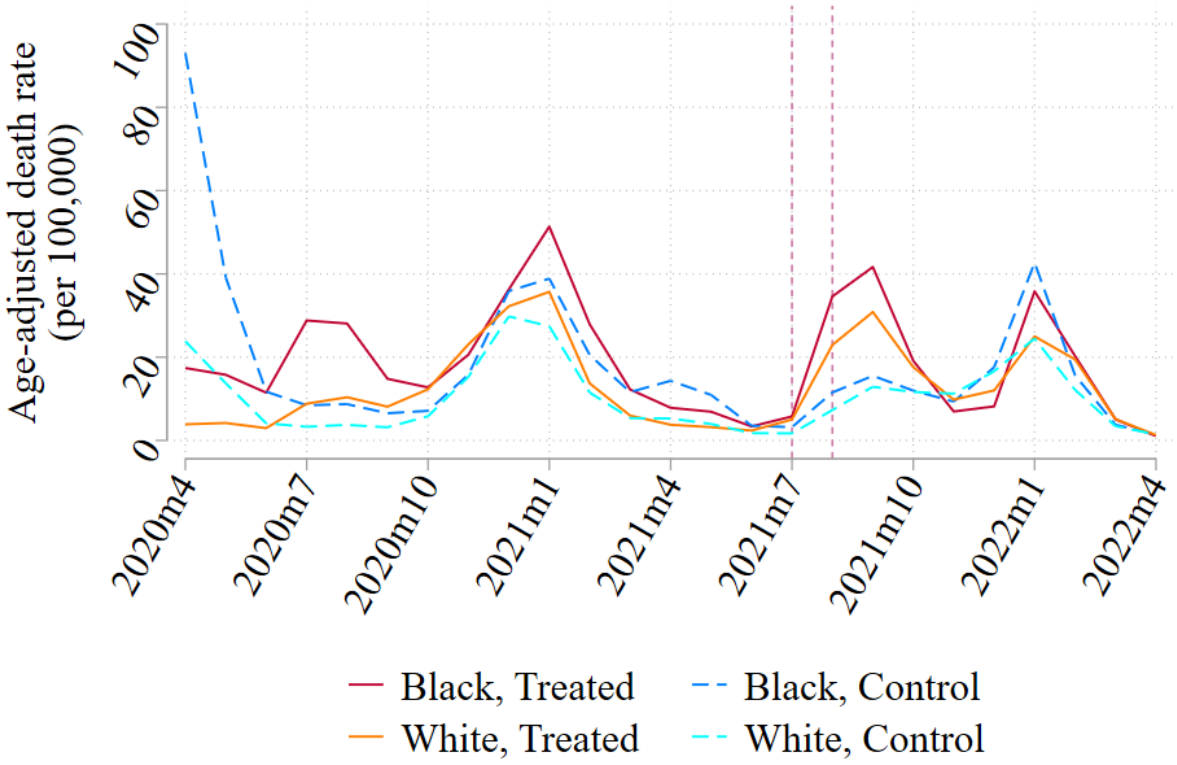


(a) Covid deaths by age

(b) Age distribution

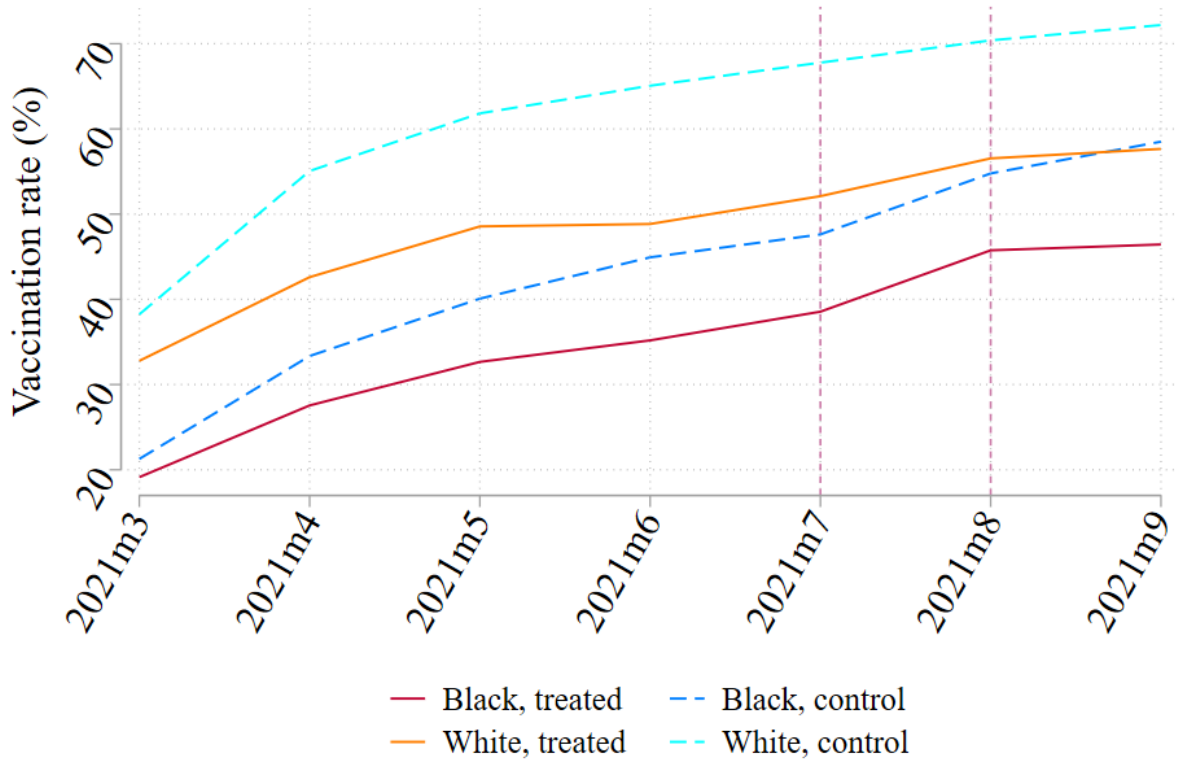
Note: Panel (a) is the monthly average from July to September 2021. Panel (b) is the age distribution by race. The age 80-85 in the right panel includes 85+. Source = CDC WONDER (Panel a), 2019 CPS (Panel b).

Figure A2: Mortality Trends by Race for Treated and Control States: April 2020 - February 2022



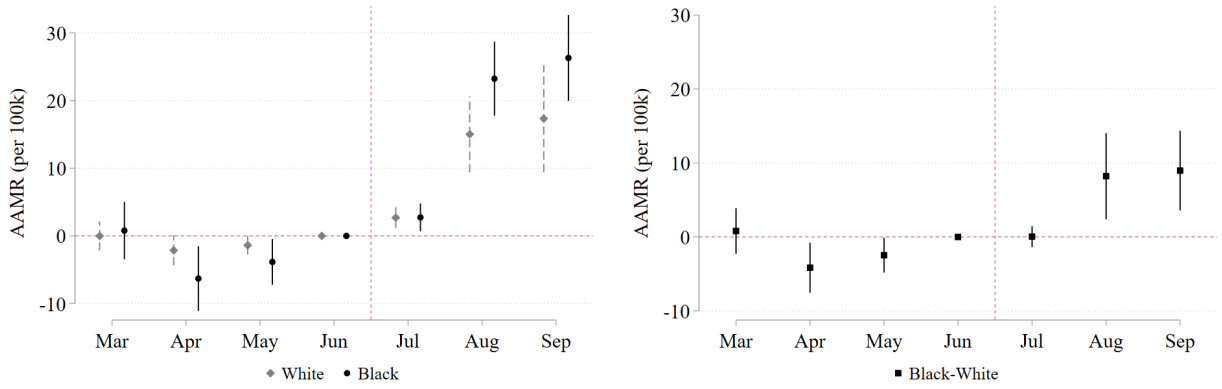
Note: Age-adjusted Covid mortality rates (per 100,000) by race and by treated and control states over the month, weighted by each state's population by race from April 2020 to February 2022. Vertical lines: July-August 2021 discontinuation period.

Figure A3: Vaccination Trends by Race for Treated and Control States: March - September 2021



Note: Vaccination rates (%) by race and by treated and control states over the month, weighted by each state's population by race from March to September 2021. Vertical lines: July-August 2021 discontinuation period.

Figure A4: Event Studies for Covid Death Rates (Specification from Table 3 Col. (1))



(a) Black and White separately (b) Black-White difference

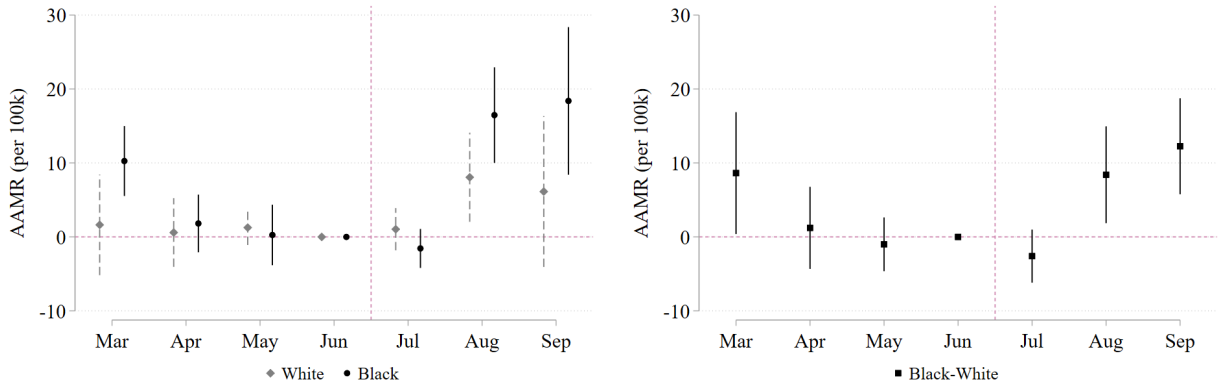
Note: Graphs (a) is estimates of event studies by race separately, and (b) is event studies for the triple-difference specification with races pooled. The estimation includes state and month fixed effects, as in Table 3 column (1). Vertical lines are 95% confidence intervals. Estimates are weighted by race population. Standard errors are clustered at state level.

Table A2: Logit coefficients for inverse probability weights

Vaccination_{t-1}	-8.616***
	(2.995)
$\text{Vaccination}^2_{t-1}$	0.083***
	(0.029)
$\text{Indoor degree days}_{t-1}$	0.015
	(0.013)
Obesity	0.758**
	(0.329)
Cardio disease	-0.368
	(0.448)
log(median earning)	3.410
	(4.277)
Age 65+ share	65.777
	(43.275)
NPI-index	-0.051
	(0.317)
Black share	-16.924
	(16.767)
Unemployment rate	0.092
	(1.189)
Job opening rate	-0.715
	(2.593)

Note: The variables are at state level, not separately calculated by race. Robust standard errors on state are in parentheses. Observations = 45 states. Values are based on May 2021.

Figure A5: Event Studies for Covid Death Rates (Specification from Table 3 Col. (3))



(a) Black and White separately (b) Black-White difference

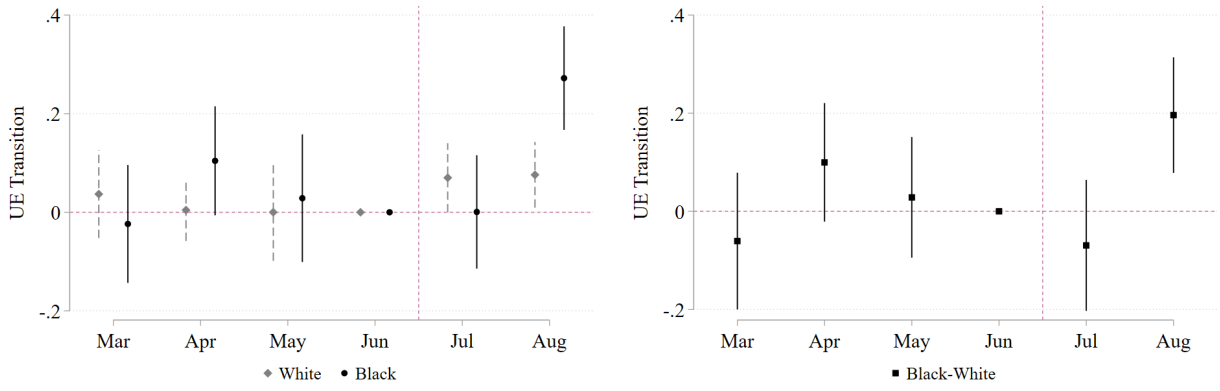
Note: Graphs (a) is estimates of event studies by race separately, and (b) is event studies for the triple-difference specification with races pooled. The estimation includes state and month fixed effects, as in Table 3 column (3). Vertical lines are 95% confidence intervals. Estimates are weighted by race population. Standard errors are clustered at state level.

Table A3: Descriptive Statistics on Covariates, March-June 2021

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Control			Treated			T-C	
	White	Black	B-W	White	Black	B-W	T(B-W)-C(B-W)	
Vaccination$_{t-1}$ (%)	44.1	26.2	-17.9	35.7	22.0	-13.7	4.2	2.1
	[17.1]	[12.8]	(3.2)	[12.1]	[9.4]	(2.3)	(4.3)	(4.4)
Indoor degree days$_{t-1}$	569.9	535.4	-34.6	453.7	341.3	-112.4	-77.8	-117.2
	[329.9]	[317.6]	(62.7)	[316.8]	[212.5]	(60.2)	(91.2)	(97.1)
Obesity (%)	29.1	40.3	11.2	32.6	42.0	9.3	-1.9	-1.2
	[3.8]	[5.4]	(0.8)	[2.7]	[4.4]	(0.6)	(1.1)	(1.2)
Cardiovascular disease (%)	6.5	5.4	-1.1	7.8	6.1	-1.7	-0.6	-0.2
	[1.0]	[1.3]	(0.2)	[1.5]	[0.9]	(0.3)	(0.4)	(0.4)
Median earning ($1,000)	15.4	11.9	-3.5	12.1	11.6	-0.5	3.0	1.5
	[5.0]	[0.4]	(1.0)	[3.9]	[3.5]	(0.8)	(1.3)	(1.5)
Age 65+ (%)	20.9	13.0	-7.9	19.5	11.4	-8.1	-0.2	-0.7
	[1.6]	[1.6]	(0.3)	[1.6]	[1.5]	(0.3)	(0.5)	(0.5)
NPI index	10.3	10.4	0.2	6.8	6.8	0.1	-0.1	-0.6
	[2.7]	[2.6]	(0.5)	[1.0]	[0.5]	(0.2)	(0.6)	(0.7)
Observations	104	104	208	76	76	152	360	360
Weight	Pop weight							Pop x IPW

Note: Sources are described in text. Period for these statistics: March-June 2021. State-race level characteristics (obesity, cardiovascular disease, median earnings, age 65+) are fixed. The top panel includes treated states (those terminating pandemic UI programs June 12 - July 3, 2021) and control states (terminating the programs September 4), as described in text. Standard deviations are shown in brackets. Observations are weighted by the population of each race by state. Indoor days is the sum of deviations of the average temperature above and below 65°F.

Figure A6: Event Studies for UE Transition (Specification from Table 5 Col. (1))



(a) Black and White separately (b) Black-White difference

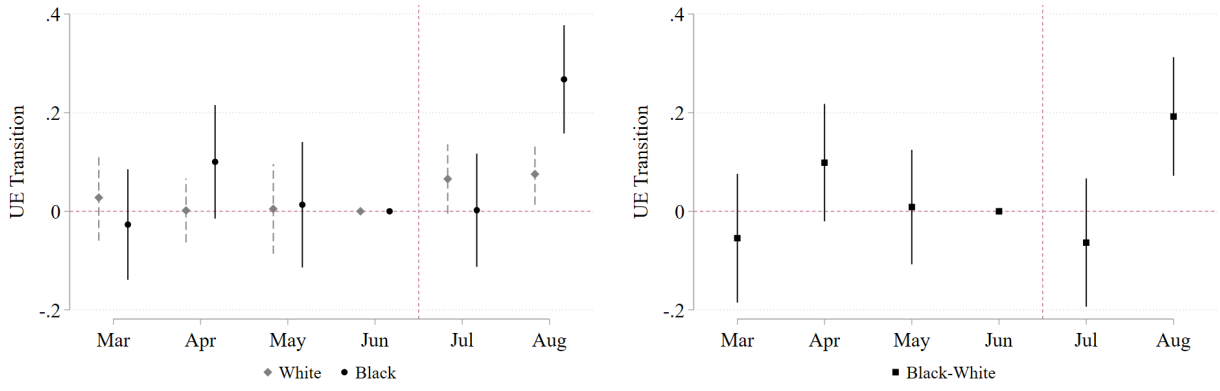
Note: Graphs (a) is estimates of event studies by race separately, and (b) is event studies for the triple-difference specification with races pooled. The estimation includes state and month fixed effects, as in Table 5 column (4). Vertical lines are 95% confidence intervals. Estimates are weighted by CPS weight. Standard errors are clustered at state level.

Table A4: Coefficients on Covariates of Table 3

	(1)	(2)	(3)	(4)	(5)	(6)
	White	Black	White	Black	White	Black
$PUIend_{t-1}$	16.344***	26.095***	10.686***	21.076***	6.241*	17.366***
	(3.025)	(3.161)	(2.982)	(3.506)	(3.144)	(4.087)
Vaccination_{t-1}			0.855***	-0.464	1.355***	0.135
			(0.277)	(0.565)	(0.498)	(0.637)
$\text{Vaccination}^2_{t-1}$			-0.007***	0.003	-0.009***	-0.002
			(0.002)	(0.006)	(0.003)	(0.006)
$\text{Indoor degree days}_{t-1}$			0.005**	0.014***	0.008***	0.014***
			(0.002)	(0.004)	(0.003)	(0.004)
Obesity×time			-0.157	0.032	-0.117	0.044
			(0.113)	(0.071)	(0.117)	(0.079)
Cardio disease×time			0.678	-0.290	0.840*	0.107
			(0.406)	(0.277)	(0.432)	(0.392)
log(Median earning)×time			-0.961	-0.881	-0.764	-0.782
			(1.090)	(0.873)	(1.189)	(1.033)
Age 65^+×time			-0.233	-0.207	-0.409**	-0.223
			(0.158)	(0.152)	(0.168)	(0.168)
NPI-index_t			0.056	0.327	-0.133	0.304
			(0.271)	(0.626)	(0.360)	(0.620)
State and time FE	Y		Y		Y	
Controls			Y		Y	
Weight	Pop				Pop x IPW	

Note: Estimates are weighted by state race population. Clustered standard errors on state are in parentheses. Observations = 315 state-months for separate regressions and 630 for pooled regressions from March through September 2021.

Figure A7: Event Studies for UE Transition (Specification from Table 5 Col. (2))



(a) Black and White separately (b) Black-White difference

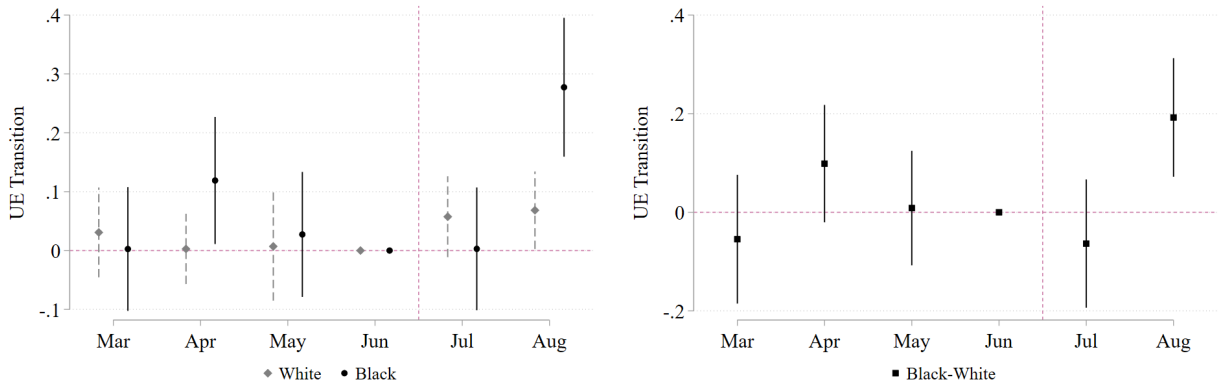
Note: Graphs (a) is estimates of event studies by race separately, and (b) is event studies for the triple-difference specification with races pooled. The estimation includes demographic controls, marital status, education, veteran status, MSA, as well as state and month fixed effects, as in Table 5 column (2). Vertical lines are 95% confidence intervals. Estimates are weighted by CPS weight. Standard errors are clustered at state level.

Table A5: Coefficients on Covariates of Table 5

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	White	Black	White	Black	White	Black	White	Black
$PUIend_{Jul}$	0.059	-0.025	0.057	-0.018	0.047	-0.032	0.047	-0.035
	(0.038)	(0.046)	(0.034)	(0.046)	(0.033)	(0.046)	(0.034)	(0.047)
$PUIend_{Aug}$	0.068**	0.240***	0.066**	0.248***	0.058*	0.242***	0.063*	0.237***
	(0.028)	(0.058)	(0.027)	(0.058)	(0.030)	(0.062)	(0.032)	(0.067)
Age			-0.005	-0.004	-0.006*	-0.006*	-0.006**	-0.006*
			(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Age^2			0.000	0.000	0.000	0.000	0.000	0.000
			(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female			-0.014	-0.045	-0.007	-0.046	-0.007	-0.046
			(0.012)	(0.029)	(0.012)	(0.029)	(0.012)	(0.029)
Foreign born			-0.007	0.009	0.001	-0.002	0.001	-0.002
			(0.022)	(0.035)	(0.022)	(0.037)	(0.022)	(0.037)
Married			0.067***	0.016	0.057***	0.011	0.057***	0.011
			(0.012)	(0.027)	(0.011)	(0.025)	(0.011)	(0.025)
Divorced/separated			0.029	0.037	0.024	0.028	0.024	0.029
			(0.020)	(0.031)	(0.021)	(0.028)	(0.021)	(0.027)
Widowed			0.019	0.001	0.017	0.017	0.017	0.018
			(0.037)	(0.072)	(0.037)	(0.067)	(0.037)	(0.066)
Younges child's age<12			-0.028	-0.005	-0.028	-0.012	-0.028	-0.012
			(0.020)	(0.025)	(0.019)	(0.026)	(0.019)	(0.027)
Veteran			0.040	0.032	0.041	0.029	0.041	0.029
			(0.026)	(0.044)	(0.025)	(0.043)	(0.025)	(0.043)
MSA			-0.001	-0.046	-0.004	-0.042	-0.004	-0.040
			(0.017)	(0.042)	(0.016)	(0.043)	(0.016)	(0.043)
<Highschool			0.021	-0.017	0.029	-0.001	0.029	-0.002
			(0.023)	(0.048)	(0.024)	(0.049)	(0.024)	(0.049)
Some college			0.011	-0.002	0.008	0.006	0.008	0.006
			(0.017)	(0.029)	(0.017)	(0.025)	(0.017)	(0.025)
University			0.004	0.012	-0.000	0.017	-0.000	0.018
			(0.016)	(0.033)	(0.015)	(0.030)	(0.015)	(0.031)
>University			0.042*	-0.012	0.041*	-0.010	0.041*	-0.010
			(0.024)	(0.041)	(0.021)	(0.031)	(0.022)	(0.031)
Job loser					0.061***	0.119***	0.061***	0.118***
					(0.016)	(0.029)	(0.016)	(0.029)
Missing					0.043*	0.083***	0.043*	0.082***
					(0.022)	(0.024)	(0.021)	(0.024)
Job leaver					0.053**	0.084*	0.053**	0.082
					(0.024)	(0.049)	(0.024)	(0.050)
Unemp duration < 52 wks					0.150***	0.104***	0.150***	0.103***
					(0.019)	(0.028)	(0.019)	(0.028)
Job opening rate							0.003	0.010
							(0.014)	(0.025)
Timeliness							-0.001	0.001
							(0.001)	(0.002)
Seasonality							-0.057	0.125
							(0.125)	(0.173)
State and time FE	Y		Y		Y		Y	
Demog			Y		Y		Y	
Unemp char					Y		Y	
State-level controls							Y	

Note: Estimates of Equation (2) and covariates of Table 5. Estimates are weighted by CPS weight. Standard errors clustered at state level are in parentheses. Observations = 1770 for Black, 6802 for White from March through August 2021. *** p<0.01, ** p<0.05, * p<0.1.

Figure A8: Event Studies for UE Transition (Specification from Table 5 Col. (3))



(a) Black and White separately (b) Black-White difference

Note: Graphs (a) is estimates of event studies by race separately, and (b) is event studies for the triple-difference specification with races pooled. The estimation includes demographic controls, marital status, education, veteran status, MSA, unemployment characteristics, as well as state and month fixed effects, as in Table 5 column (4). Vertical lines are 95% confidence intervals. Estimates are weighted by CPS weight. Standard errors are clustered at state level.