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

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Abstract

We link unemployment insurance (UI) to covid death rates among Whites and Blacks during the pandemic. Exploiting cross-state differences in UI benefit cuts in summer 2021, we estimate that ending pandemic UI programs led to higher mortality for both Blacks and Whites, but the increase was 30-50 percent more for Blacks. Examining the mechanism underlying this racial mortality gap, we find that the UI cuts raised re-employment rates for both races but again with a 30-50 percent larger increase for Blacks. These results are qualitatively robust to alternative specifications, and falsification tests support causal interpretations. Preliminary evidence suggests that the higher responsiveness of Blacks to the UI cuts is related to lower wealth and savings, higher UI replacement rates (because of lower wages), and higher rates of work in occupations involving personal contact.

JEL codes: J15, J65, I18

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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. Part of the explanation might lie with racial differences in average pre-existing health conditions, income and wealth, or other factors affecting vulnerability to severe infection, but an important question is whether policies may have affected races differentially. This study investigates one such policy: the large cuts in unemployment insurance (UI) in 19 states during summer 2021, 2-3 months before most of the other states followed. These cuts pushed unemployed people back into the work force, creating potential exposure and community spread of disease, ultimately increasing covid deaths. To the extent that this process differed by race, 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.

In this paper, we develop this argument using both state-level panel data on covid deaths by race from the Centers for Disease Control (CDC) and individual data from the Current Population Survey (CPS). The CPS data has a short longitudinal dimension that allows us to track individual labor force transitions at a monthly frequency, while identifying race and many other relevant characteristics. It also contains information on work time losses due to illness, another type of evidence on the health consequences of the pandemic UI cuts.

We find that the UI cuts raised age-adjusted covid death rates among Black and White populations by 0.023 and 0.012 percentage points, respectively. Both increases are statistically significant, and the rise in the Black rate is statistically higher than that in the White rate. Examining the hypothesized mechanism for these sharp increases in mortality, our analysis of CPS data finds that the UI benefit cuts increase the probability of unemployment-employment transitions among Blacks by 13-15 percentage points, about a 70 percent increase compared to the pre-policy mean of 20 percent among Blacks in the early terminating states. Whites experience a rise in the re-employment probability by 9-11 percentage points, about 40 percent compared to the mean of 25 percent. Nevertheless, the relationship between covid deaths and re-employment behavior, estimated through ordinary least squares and instrumental variable methods, is similar for Blacks and Whites, implying that the community spread mechanism works similarly for the two groups and that the higher Black mortality is explained by higher exposure resulting from re-employment. We also find a higher increase in illness-related losses in work time among Blacks, providing corroboration from a different angle of the health effects of re-employment during a pandemic.

Previous studies have documented the higher rate of covid deaths among Blacks compared with Whites in the U.S. (Aburto et al., 2022; Elo et al., 2022; Joynt Maddox et al., 2022;

Lundberg et al., 2023; Zalla et al., 2022). However, there has been relatively little attention to the channels, or mechanisms, that might account for this racial gap. In addition, previous research on UI and health analyzes pre-pandemic health consequences of UI receipt (e.g., Kuka (2020)) and studies of covid deaths have not estimated effects of UI (Bollyky et al., 2023). The extent of racial segregation in workplaces has been documented by Hellerstein and Neumark (2008) and the relationship of residential segregation to health is discussed in Williams and Collins (2001). 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 (Coombs et al., 2022; Ganong et al., 2022; Holzer et al., 2021). But no previous paper estimates different effects of UI by race during the pandemic, and none connects those differences to the racial gap in mortality.

2 Data

We analyze a combination of state and individual-level panel data. We use state-level data for some of the analysis both because of the nature of our research question and because of constraints on data availability. The unemployment insurance 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, resulting sometimes in death. To measure these outcomes requires aggregate data. Information on covid deaths by race is available only by state-month, which is then the unit of analysis for much of our research.

Our principal dependent variables are covid deaths for non-Hispanic Blacks and non-Hispanic Whites, excluding other races and Hispanic ethnicity to focus attention on one potential racial gap. There has been some debate on whether the CDC under- or over-counts covid deaths (Hoffman, 2020; Faust et al., 2021). Measurement error seems likely, but for it to affect our results there would have to be a bias in reporting that emerges exactly in the treatment period in the treated states (because there is little change in covid deaths in the control states during the treatment period). We are unaware of any factors that could lead to such biased reporting. Because the age structure differs substantially for Blacks and Whites, we follow the literature (e.g., Lundberg et al. (2023)) in calculating age-adjusted covid deaths by race for each state-month. Some observations on covid deaths by age group and race are suppressed due to small numbers, so we use two alternative approaches and sources. The first combines separate data for deaths by race and population by 10-year age category from the CDC WONDER database (CDC, 2024a,b). In order to express the

number of deaths per capita, we use the April 2020 population from the Decennial Census for Blacks and Whites by state.

The treatment 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 programs raised benefits considerably, adding $300 per week to benefits beginning in December 27, 2020 in all states, and extended eligibility and duration much longer than the 26 weeks normally standard.[1] The first 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.[2] Control states are defined as ending PUI by federal law on September 4, 2021.[3]

The differences across states in terminating the PUI programs create both geographic and time variation. Figure 1 contains a map of the states by treated, control, and excluded category. The pre-period is specified as the period before the early ending of PUI, usually from February to June 2021. The treatment period is July and August 2021. We define $PUIend_{st}$ as an indicator for states s in months t that equals 1 in July-August 2021 for states ending PUI early, and 0 otherwise.

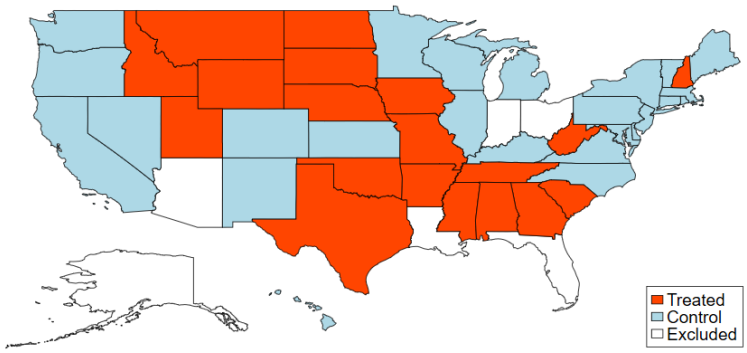
We include several other state-month variables that function as controls in the covid death regressions. We include the the covid vaccination rate, which is defined as the percentage of the total population with at least one dose of a covid vaccine (CDC, 2023). To account for differences in weather, which may affect the amount of time spent indoors and thereby the potential spread of disease, we use cooling and heating degree-days from the National Center for Environmental Information [need ref]. Cooling and heating degree-days are 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 the cooling and heating days to

[1]The extra $300 per week is from the FPUC, while PEUC extended benefits an additional 49 weeks and PUA extended eligibility to the self-employed, contractors, and UI exhaustees up to a total of 75 weeks. All these programs ended for all recipients by September 4, 2021. For more details, see Whittaker and Isaacs (2022).

[2]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. Appendix Table A1 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.

[3]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.

Figure 1: Treated and Control States



Note: States with dark orange color (Treated) ended the PUI by between mid June and early July 2021. States in light blue color (Control) continued the program until early September 2021.

construct the number of "indoor days" by state-month.

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 the "experienced unemployed": unemployed individuals who have previously worked (based on a direct CPS question). The CPS does not measure unemployment insurance receipt, so our estimates can be interpreted as the "intent-to-treat" impacts of the change in UI benefits on individuals classified by the CPS as unemployed (not working, available for work, and actively searching or expecting recall) rather than on UI recipients.[4]

We measure re-employment behavior using the longitudinally linked version of the CPS, based on its "4-8-4" rotation panel design (Feng, 2001; Rivera Drew et al., 2014). The data structure for this analysis consists of two-consecutive-month pairs, for individuals observed in both months. 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. The main period for the re-employment analysis is January 2021 through August 2021, but we also consider other periods for robustness and to investigate behavior after the policy differences end in September 2021. Individuals may appear more than once (once for each month they are both unemployed and observed in the sample in the next month). We also restrict the sample to individuals who are 20 to 64 years old in order to reduce complications associated

[4]The UI recipiency rate (the ratio of regular state UI claims to all unemployed, according to the official definition in https://oui.doleta.gov/unemploy/Chartbook/a13.asp, which also provides annual figures) 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.

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 standard demographic characteristics, including age, sex, ethnicity, race, education, marital status, foreign born, and child under 6 and 12 years old. In some specifications, 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 $t-1$. We control for some time-varying state-level characteristics: job openings, state UI program performance, and seasonality. The job openings rate obtained from the Job Openings and Labor Turnover Survey (JOLTS) represents state-month variation in labor demand (or offer arrival rate) for unemployed workers. It is calculated as the ratio of job openings to total employment plus job openings in the nonfarm sector (BLS, 2022). UI program performance may affect UE transitions if payment delays lead the discouraged unemployed to return to work earlier than they might otherwise have (Navarrete, 2023). For this measure, we draw upon data from the Department of Labor Employment and Training Administration (DOLETA), a Benefits Timeliness and Quality (BTQ) standard, the "continued weeks compensated time lapse" (Employment and Training Administration, 2017). UI timeliness is defined as the proportion of payments made within 3 weeks of receiving the claim, based on the performance standards of payment within 14-21 days.(Employment and Training Administration, 2022).

Time fixed effects, which we include in our regression specifications, control for nationwide seasonal fluctuations in job-finding, but not for systematic state-level variation across different months. Whether seasonality is an important consideration during the volatile period we study is uncertain, but to account for the possibility we derive a state-month varying control variable by estimating the regular monthly variation in UE for each state separately by race based on experienced unemployed in 2015-2019. This variable controls for differences across states not only in weather but also factors such as school year start dates that might affect incentives for re-employment.

Summary statistics for all variables during the period January-June 2021, before the policy change, are shown in Table 1. Means (and standard deviations for continuous variables) are shown for all states and for control and treated states separately, as well as for Blacks and Whites separately, where possible. The age-adjusted covid death rate is consistently higher among Blacks than Whites, while the UE (transition from unemployment to employment) rate is lower; the difference-in-differences estimation will of course take these baselines into account. Vaccination rates and median earnings are higher in the control than treated states,

Table 1: Descriptive Statistics, January-June 2021

	All states		Control		Treated	
	Black	White	Black	White	Black	White
State-level						
Covid death rate (age-adjusted)	0.0158	0.0094	0.0149	0.0089	0.0171	0.0104
	(0.0166)	(0.0113)	(0.0152)	(0.0102)	(0.0184)	(0.0133)
Vaccination rate (%, May 2021)	52.22		57.43		45.09	
	(8.82)		(6.75)		(5.83)	
Indoor days (/100)	5.69		6.58		4.38	
	(3.19)		(3.36)		(2.39)	
Age>65 (proportion, 2020)	0.18		0.18		0.17	
	(0.02)		(0.02)		(0.02)	
Cardiovascular disease (%, 2019)	10.53		9.62		11.77	
	(2.68)		(2.33)		(2.66)	
Obesity (%, 2020)	31.84		30.04		34.32	
	(4.12)		(3.69)		(3.35)	
Median earnings ($, 2020)	36676		38885		33652	
	(5383)		(5812)		(2623)	
Job opening rate	5.34		5.22		5.59	
	(1.04)		(1.10)		(1.03)	
UI Timeliness (%)	82.92		82.16		84.11	
	(8.62)		(10.13)		(5.93)	
Unemployed individual-level						
UE (unemployment-employment)	0.17	0.24	0.17	0.23	0.18	0.25
Age	37.14	40.28	36.56	40.51	38.18	39.64
	(12.05)	(13.32)	(11.83)	(13.37)	(12.37)	(13.17)
Female	0.47	0.43	0.48	0.43	0.44	0.43
Foreign born	0.13	0.07	0.16	0.08	0.08	0.04
< High school	0.09	0.06	0.08	0.05	0.11	0.08
High school	0.38	0.32	0.34	0.31	0.45	0.35
Some college	0.33	0.30	0.35	0.30	0.29	0.31
University	0.15	0.24	0.16	0.25	0.12	0.19
> University	0.06	0.08	0.07	0.09	0.03	0.06
Never married	0.66	0.46	0.69	0.48	0.61	0.40
Married	0.20	0.38	0.18	0.38	0.22	0.41
Divorced/Separated	0.13	0.14	0.11	0.13	0.17	0.17
Widowed	0.01	0.01	0.01	0.01	0.00	0.02
Child's age < 6	0.04	0.04	0.04	0.04	0.03	0.03
Child's age < 12	0.20	0.20	0.21	0.20	0.17	0.20
Job loser	0.68	0.71	0.69	0.74	0.64	0.64
Job leaver	0.08	0.09	0.07	0.08	0.11	0.14
Reentrant	0.24	0.19	0.24	0.18	0.25	0.22
Unemployment duration < 52w	0.80	0.81	0.76	0.81	0.85	0.82
Observations	1264	4884	769	3050	495	1834

Note: Sources are described in text. Period for these statistics: January-June 2021. 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. The CPS sample in the bottom panel includes all experienced unemployed aged 20-64. Standard deviations are shown for continuous variables in parentheses. 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. Observations = 270, 114, and 156 for state-level variables in the three columns of the top panel, respectively.

while health indicators (cardiovascular disease and obesity rates) are lower, and the over-65 population, job opening rates, and UI timeliness are similar. Among individual characteristics of the unemployed, control states have higher rates of foreign-born and higher education and lower rates of divorce. The empirical analysis controls for all these factors.

3 Methods

In order to adjust mortality rates to account for differences in the age structure by race, we use two procedures. Ideally, we would have a full set of covid deaths by race for each age group in each state and month. However, the CDC (2024b) suppresses nearly half of all such cells where the number of deaths is less than ten. In a first procedure, we calculate age-adjusted covid mortality of Black $\tilde{D}^B_{st}$, following Equation (1) for relative comparisons.

$$\tilde{D}^B_{st} = \sum_a \{(N^W_{as}/N^W_s) \times (D^B_{ast}/N^B_{as})\} \tag{1a}$$

$$\approx \sum_a \left\{(N^W_{as}/N^W_s) \times \frac{D_{ast}}{N_{as}} \times \frac{(D^B_{st}/N^B_s)}{(D^W_{st}/N^W_s)}\right\}, \tag{1b}$$

where N^W_{as} is the number of people in the population whose race is White W in state s by 10-year age category a,[5] N^W_s is the total number of White W people by state s. With a lack of reliable data for death counts by race and age category, we multiply the death rate D_{ast}/N_{as} by each age group a in state s in time t by the death rate of Black (D^B_{st}/N^B_s), then normalize by the rate of White (D^W_{st}/N^W_s). The total age-adjusted number is then summed over the age categories in Equation (1b).

In a second procedure as in Equation (1a), we aggregate the age categories to three groups ($a \in \{(0,55),[55,75),[75,\infty)\}$), following Lundberg et al. (2023), which reduces the number of suppressed cells. For the remaining suppressed cells, we then impute the state-month age-race cell in the data on Census divisions, each of which corresponds to between three and nine states. For remaining suppressions, mostly occurring in small states and/or states with small Black populations, we impute one death. Results from alternative imputations produce similar results.

Having constructed the dependent variable of age-adjusted covid death rates by race and

[5] $a \in \{(0,1),[1,5),[5,15),[15,25),[25,35),[35,45),[45,55),[55,65),[65,75),[75,85),[85,\infty)\}$

state-month, we estimate the following equation for Blacks and Whites separately:

$$D_{st} = \sum_{k=0}^{2} \beta_k PUIend_{st-k} + \mu_s + \tau_t + W_{st} \cdot \phi + u_{st} \tag{2}$$

where D_{st} represents the covid death rate in state s and month t and, as described above, $PUIend$ is an indicator for treatment, in other words for treated states from July-August 2021. The sample runs through October and the specification takes into account contemporaneous and two lags of $PUIend$ because we found that longer lags have very small and statistically insignificant estimated effects. μ_s are state fixed effects, and τ_t are month fixed effects. W_{st} is a vector of time-varying state-level variables. In the main specification, W_{st} includes cumulative covid vaccination rates and its square, and indoor days, while in other specifications and for robustness, it includes controls for vulnerability measures interacted with time trends: obesity, heart disease, median income, and share of over-65 population. Estimation is weighted by state race population, and standard errors are clustered at the state level.

Equation (2) is estimated separately by race, and we calculate $\sum_{k=0}^{2} \beta_k$ as the cumulative impact of the policy change $PUIend$ on the covid death rate for each race. To assess the statistical significance of the difference between Blacks and Whites in this impact, we pool the data and estimate a fully interacted specification that allows every coefficient to differ by race.

The identifying assumption in this difference-in-differences analysis is parallel trends: in the absence of the intervention, death rates in the population subgroups would have moved similarly, followed parallel trends. The subgroups we investigate include both different states (treated and control) and different races (Black and White), so we investigate parallel trends by examining both the treated-control difference by race for each month of the sample as well as 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 treatment among the states. For this purpose, we randomize treatment across states and re-estimate Equation (2) 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.

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 for the transition from unemployment to employment (UE) from the sample of individual unemployed described in the previous section:

$$UE_{ist} = \beta \cdot PUIend_{st} + X_{ist-1} \cdot \gamma + V_{st-1} \cdot \eta + \mu_s + \tau_t + \epsilon_{ist}, \tag{3}$$

where UE_{ist} is the transition from unemployment$_{t-1}$ to employment$_t$ for individual i in state s at time (month) t, $PUIend_{st}$ is defined as before as an indicator for the treated states in July and August 2021, and X_{ist-1} is a vector of individual characteristics in *t-1*: age, gender, education, marital status, and youngest child's age under 6 and 12. In some specifications, we also control for a set of state-level covariates V_{st-1} described above: job openings rate, UI timeliness rate, and state-level seasonality. Except for seasonality, we lag these variables in Equation (3), and we also report estimates excluding them; we find they make little difference to the results.[6] Finally, we control for state (μ_s) and month (τ_t) fixed effects, and we cluster standard errors on states.

We estimate Equation (3) separately for Blacks and Whites and calculate the statistical significance of the difference similarly to our analysis of covid deaths. We also carry out similar event studies with respect to UE.

Finally, in order to understand the magnitude of the Black-White difference in covid deaths estimated to result from the policy change, we consider the possibility that community spread works differently within Black and White communities. For instance, for a given level of initial workplace exposure resulting from $PUIend$ in the treated states, denoted E_0, there could potentially be differences in the rate at which exposure leads to infection of the re-employed workers, r, and two aspects of the subsequent chain of transmissions (through families and social and work networks), the basic reproductive number for secondary infections, R_0, and the number of generations in the chain, n, before the infection rate becomes negligible. Finally, there could be differences in the rate δ at which infections lead to deaths. Ideally, if these quantities were known and place- and time-invariant (or using appropriate averages over all places for the whole time-period), the total covid deaths resulting from the policy could be calculated as $E_0 \cdot r \cdot {R_0}^n \cdot \delta$. None of these factors are known with any certainty, nor to our knowledge have they been estimated separately by race. Wei et al. (2024) report a higher rate of deaths per infection among Blacks than Whites but the authors admit 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. Our method here is to assess whether any of the factors

[6]Note that we do not include measures of covid outcomes as regressors because they may be simultaneously determined with UE behavior: higher covid may reduce UE, but higher UE may increase covid. Consistent with these opposing effects, if covid measures are included in Equation (3), 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 $PUIend$.

beyond E_0 matter using an instrumental variables (IV) framework in which we ask whether the mortality outcomes of the UI cuts differ by race for the same level of E_0.

In this framework, we first obtain the fitted values of UE from a regression on $PUIend$ and controls, and then use $\widehat{UE}$ as the regressor of interest in an equation with covid deaths as the dependent variable:

$$D_{st} = \sum_{k=0}^{2} \beta_k \cdot \widehat{UE_{st-k}} + \mu_s + \tau_t + W_{st} \cdot \phi + \epsilon_{st} \tag{4}$$

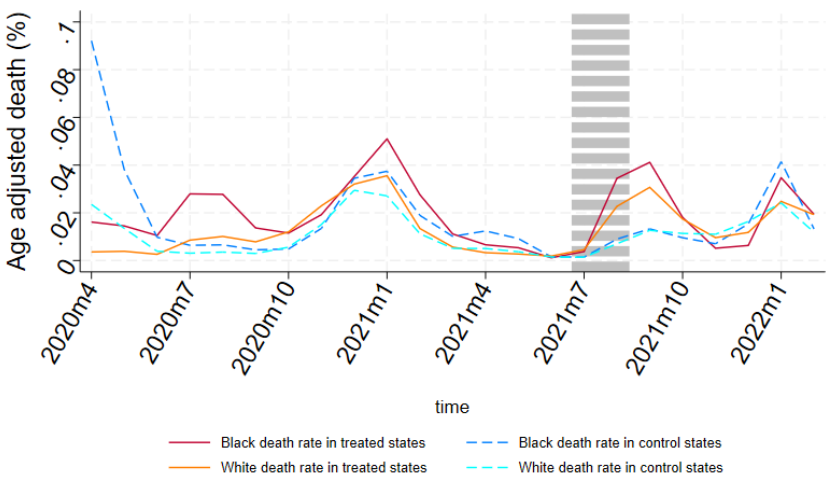
where UE_{st-k} is the transition rate for state s in month t-k, from which we have netted out differences in individual characteristics, and other variables are defined as previously. In order to capture delayed effects of UE transitions on covid outcomes, similar to the reduced form Equation (2), this equation includes contemporaneous and two monthly lagged effects. To estimate the β_k resulting from the policy change $PUIend$, we instrument UE and its lags with $PUIend$ and its lags, estimating two-stage least squares (2SLS) regressions. If the community transmission mechanism is similar for Blacks and Whites, then there should be no racial difference in the IV estimates, which would provide evidence that the relative increase in Black deaths results from greater exposure resulting from a larger increase in re-employment.

4 Results: The Impact of Unemployment Insurance on Covid Deaths by Race

Figure 2 shows mean age-adjusted death rates for Blacks and Whites in treated and control states from the beginning of the pandemic in April 2020 through early 2022. The shaded area highlights July and August 2021, the treatment period during which the UI policy differed drastically across states. The four average rates tend to move together, and they are nearly identical in late spring - early summer 2021. During the treatment months, however, the death rates increase much more for both Blacks and Whites in the treated states, but the increase is much greater for Blacks. After October 2021, the rates reconverge and move in tandem.

Table 2 aggregates the monthly data from Figure 2 into a pretreatment period (January-June 2021) and post treatment period (July-October 2021) 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, but in the treated states, there is a marked increase for both

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



Note: Mean age-adjusted death rates (%) by race and by treated and control states over month, weighted by each state's population by race. Shaded area: July-August 2021.

races. The difference-in-differences estimators imply increases of 0.0138 percent for Blacks and 0.0093 percent for Whites.

Table 2: Mean Covid Death Rates

Age-adjusted Black Death Rate				
	Jan-Jun (Pre)	Jul-Oct (Post)	Post-Pre	Obs
Control	0.0149	0.0083	-0.0066	208
Treated	0.0171	0.0244	0.0072	152
Treated-Control	0.0022	0.0161	**0.0138**	
White Death Rate				
	Jan-Jun (Pre)	Jul-Oct (Post)	Post-Pre	Obs
Control	0.0089	0.0081	-0.0008	208
Treated	0.0104	0.0188	0.0084	152
Treated-Control	0.0015	0.0107	**0.0093**	

Note: Mean age-adjusted death rates (%) by race and state for the periods shown, weighted by each state's population by race.

These simple means do not control for any other factors, so we next turn to estimates of Equation (2), the reduced form relationship of $PUIend$ with Black and White covid death rates. Three specifications are shown: columns (1) and (2) contain the results from controlling for only state and month fixed effects; (3) and (4) add controls for vaccination rate, squared vaccination rate, and indoor days; and (5) and (6) additional control for the vulnerability variables (median earnings, over-65 population share, obesity rate, and cardiovascular disease rate) each interacted with a linear time trend. The odd-numbered columns

have the age-adjusted Black covid death rate (in percent) as the dependent variable, and the even-numbered columns contain results for the White covid death rate. Consistently across specifications, the contemporaneous effect of $PUIend$ is tiny and statistically insignificant for both Blacks and White, but all coefficients jump in the one-month lag, especially for Blacks, and become highly significant, before falling to lower levels in the two-month lag. The sum of the coefficients, $\Sigma_0^2\beta_{t-k}$, consistently shows a much sharper rise in death rates for Blacks than White, a difference that is statistically significant and estimated to be about 0.01 percentage points. Using the numbers from the third specification relative to the baseline pre-treatment rates from Table 2, the Black death rate rises about 135 percent, while the White rate rise is about 120 percent.

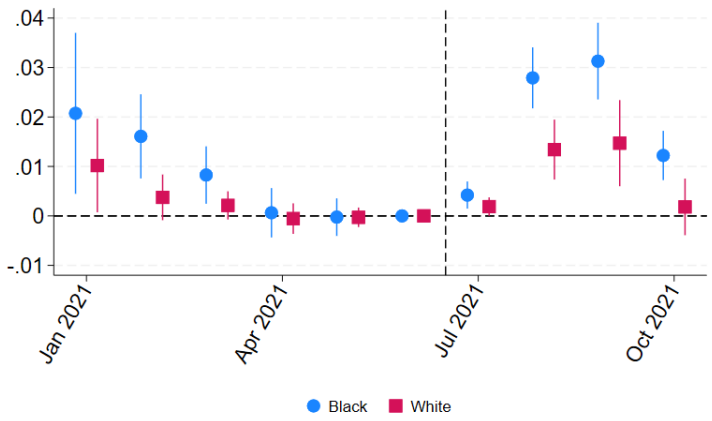
Table 3: Estimating Impact of $PUIend$ on Covid Deaths

	1	2	3	4	5	6
	Black	White	Black	White	Black	White
$PUIend_t$	0.0011	0.0018	0.0008	-0.0001	0.0008	0.0000
	(0.0018)	(0.0017)	(0.0025)	(0.0015)	(0.0026)	(0.0015)
$PUIend_{t-1}$	0.0213	0.0123	0.021	0.0121	0.0210	0.0121
	(0.0022)	(0.0028)	(0.0022)	(0.0028)	(0.0022)	(0.0028)
$PUIend_{t-2}$	0.0054	0.0044	0.001	0.0005	0.0009	0.0000
	(0.0020)	(0.0021)	(0.0039)	(0.0025)	(0.0037)	(0.0026)
$\Sigma_0^2\beta_{t-k}$	0.0277	0.0185	0.0227	0.0125	0.0226	0.0121
	(0.0034)	(0.0029)	(0.0061)	(0.0038)	(0.0066)	(0.0041)
Diff B-W	0.0092		0.0102		0.0105	
	(0.0024)		(0.0044)		(0.0047)	
State, Month FE	Y	Y	Y	Y	Y	Y
Controls	N	N	Y	Y	Y	Y
Vulnerability trend	N	N	N	N	Y	Y

Note: Estimates of Equation (2). Black rate is adjusted using Equation (1). Observations = 450 (45 states, 10 months). Controls = vaccination rate and it square, indoor days. Vulnerability trend = age over 65 population, cardiovascular disease, obesity population share and median earnings interacted with time trend. Results with age-adjustment from Equation (1a) are in Appendix Table A1. Difference in Black and White in sum of coefficients is calculated using full-interaction triple difference. Estimates are weighted by race population. Standard errors clustered at state level are in parentheses.

An event-study for each race is 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 figure shows that the differences between treated and control state rates are negligible for both races during late spring and early summer 2021 but jump for both in August and September before falling back in October, with the Black rate showing a much higher jump.

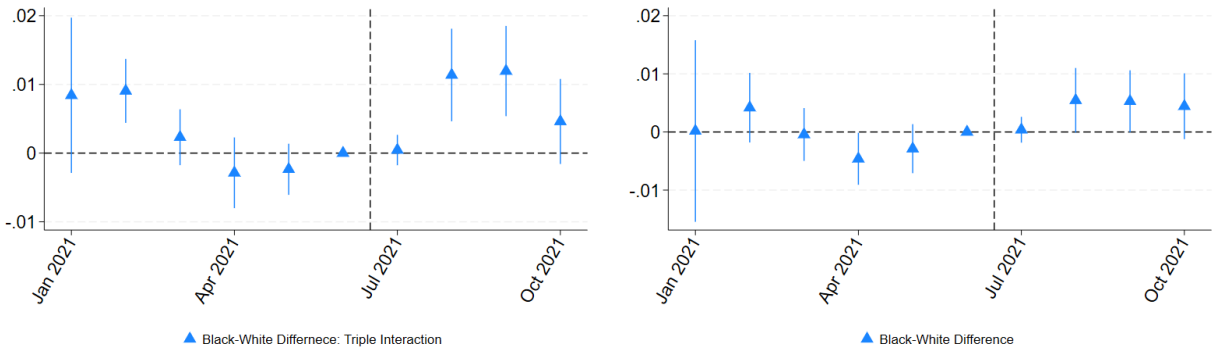
Figure 3: Treated-Control Difference in Covid Death Rates



Note: Specification includes state and month fixed effects, vaccination rate, squared vaccination rate, indoor days, and vulnerability variables interacted with trends as in Table 3 columns (5-6). Vertical lines are 95% confidence interval. Difference is calculated using triple difference. Estimates are weighted by race population. Standard errors clustered at state level. The results from the alternative way of age-adjusting deaths are in Appendix Figure A1.

Figure 4 contains calculations of the estimated differences, and confidence intervals around the differences, between the Black and White coefficients in Figure 3. In one calculation, we include a full set of state-month interactions, but qualitatively the results are similar, showing the largest Black-White mortality gap emerging in August-September 2021.

Figure 4: Treated-Control Difference by Race in Covid Death Rates



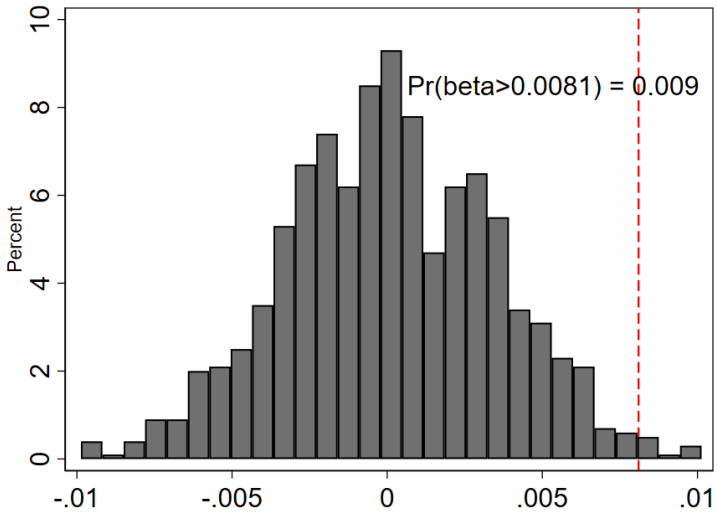
(a) Triple Difference (b) Full Interaction

Note: Specification includes state and month fixed effects, vaccination rate, squared vaccination rate, number of indoor days, and vulnerability variables interacted with trends as in Table 3 columns (5-6). Vertical lines are 95% confidence interval. Difference is calculated using triple difference. Estimates are weighted by race population. Standard errors clustered at state level. Panel (a) is calculated by interacting every term with race indicator, and panel (b) spans the interaction with state by month indicators. The results from the alternative way of age-adjusting deaths of panel (a) is in Appendix Figure A1.

The results of a permutation test provide an alternative way of assessing the statistical significance of the estimated difference between the effects on Black and White death rates.

For this purpose, we randomize the treatment assignment and recompute the estimates in the full specification of Equation (3), reported in columns (5) and (6) of Table 3. We carry out this permutation for 1000 replications. The results show that the estimated difference of 0.0105 from that table is an extreme outlier relative to the distribution of outcomes under the replications, supporting the hypothesis that Black death rates rose much more substantially than those of Whites as a result of the cutting of UI benefits.

Figure 5: Permutation Test



Note: Specification includes state and month fixed effects, vaccination rate, squared vaccination rate, indoor days, and vulnerability variables interacted with trends. Vertical line indicates the estimated difference between Black and White from Table 3 columns (5-6). The results from the alternative way of age-adjusting deaths are in Appendix Figure A2.

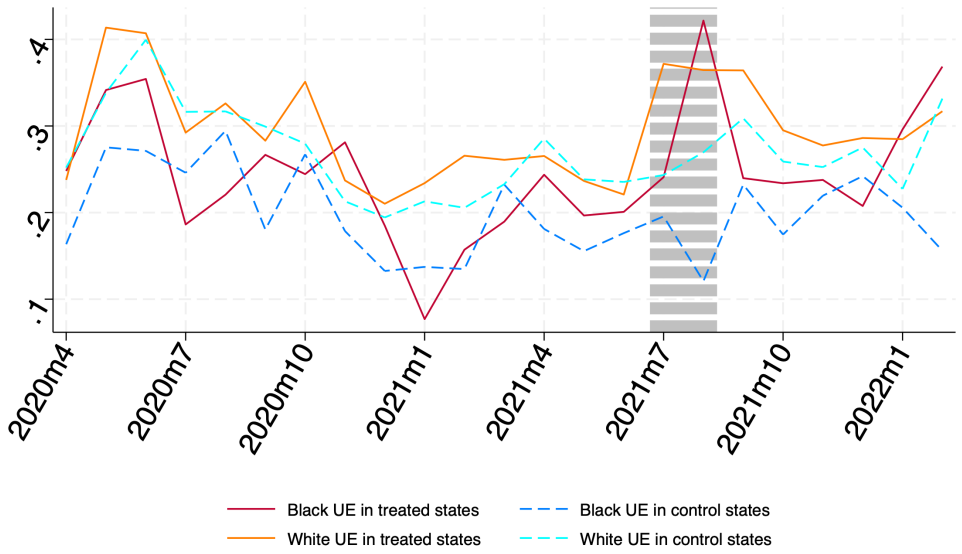
5 Results: The Re-Employment Mechanism

How did cuts to UI benefits raise covid death rates more for Blacks than Whites? Our hypothesis is that the cuts raised transitions from unemployment to employment, increasing exposure and leading to 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. Alternatively, perhaps the community spread mechanism works differently for Blacks than Whites, so that for instance exposure is more likely to lead to infection or infections are transmitted more quickly through a community. We provide evidence on both of these possibilities in this section.

Figure 6 reports means of UE transitions by month for treated and control states, and for Black and White unemployed individuals. Similar to Figure 2, the rates tend to be

similar and move together, but UE in the treated states diverges from the controls during the treatment period, and the Black rate rises particularly sharply in August.

Figure 6: 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. Shaded area highlights the treatment period, July-August 2021.

Table 4 shows means of UE for the pretreatment (January-June 2021) and treatment (July-August) periods; unlike covid deaths, the re-employment response is nearly immediate, so there is no need for a lagged specification and additional months of data after August. Analogously to Table 2, the results permit a simple difference-in-differences estimation of the policy effect, with a rise of 15.4 percentage points for Blacks and 9.8 points for Whites, representing increases of 97 percent and 38 percent, respectively, relative to the control states during the same period.

Estimates by race of the β coefficient on $PUIend$ in the linear probability model of Equation (3) are reported in Table 5. We allow the coefficient to vary between July and August, because the response in August is rather different from July. The White coefficient is larger in July, although the White-Black difference is not statistically different from zero, but the Black coefficient is much larger in August, and the racial difference is highly significant that month.

An event study of the difference in the Black and White coefficients by month, corresponding to separate estimation by race of UE Equation (3) is graphed in Figure 7. The data show no statistically significant Black-White differences in coefficients and no indication

Table 4: Mean UE Transition Rates

Black				
	Jan-Jun (Pre)	Jul-August (Post)	Post-Pre	Obs
Control	0.170	0.159	-0.011	1017
Treated	0.178	0.321	0.143	631
Treated-Control	0.008	0.162	**0.154**	
White				
	Jan-Jun (Pre)	Jul-Oct (Post)	Post-Pre	Obs
Control	0.235	0.257	0.022	3974
Treated	0.248	0.368	0.120	2336
Treated-Control	0.014	0.111	**0.098**	

Note: Mean UE transition rate by race and by treated and control states over the period shown, weighted by CPS weight. Sample includes experienced unemployed in $t-1$, aged 20-64 civilian.

Table 5: Estimating Impact of $PUIend$ on UE

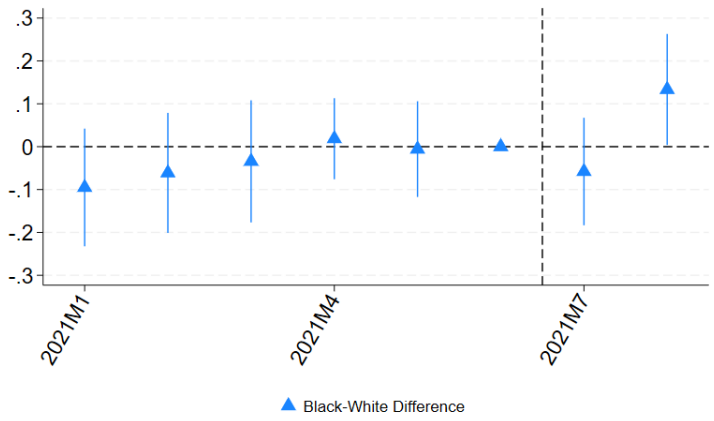
	1	2	3	4	5	6	7	8
	Black	White	Black	White	Black	White	Black	White
$PUIend_{Jul}$	0.049	0.119	0.053	0.116	0.044	0.109	0.028	0.106
	(0.052)	(0.049)	(0.051)	(0.047)	(0.052)	(0.054)	(0.054)	(0.054)
$PUIend_{Aug}$	0.278	0.091	0.284	0.088	0.27	0.072	0.252	0.067
	(0.090)	(0.033)	(0.088)	(0.032)	(0.087)	(0.035)	(0.083)	(0.036)
Diff B-W$_{Jul}$	-0.070		-0.063		-0.065		-0.076	
	(0.078)		(0.076)		(0.077)		(0.078)	
Diff B-W$_{Aug}$	0.187		0.196		0.193		0.187	
	(0.094)		(0.092)		(0.097)		(0.093)	
State, Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Individual Charac.	N	N	Y	Y	Y	Y	Y	Y
State-level covariates	N	N	N	N	Y	Y	Y	Y
Unemployment Charac.	N	N	N	N	N	N	Y	Y

Note: Estimates of Equation (3). Observations = 1648 for Black, 6310 for White from January through August 2021. Individual characteristics = age, sex, foreign born, education, marital status, youngest child's age under 6 and 12. State level covariates= job opening rates, UI payment timeliness in $t-1$, and UE seasonality by race. Unemp char = job leaver, reentrant, duration $<$ 52w indicators. Difference in Black and White is calculated using full-interaction triple difference. Estimates are weighted by CPS weight. Standard errors clustered at state level are in parentheses.

of a trend from January until July 2021, but the difference jumps and becomes statistically significant in August.

Finally, 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. In this case, the coefficients on $\widehat{UE}$ in Equation (4) should be higher for Blacks than Whites, as the initial exposure from re-employment translates into a higher death rate. To assess this possibility, Table 6 contains estimates of Equation (4) separately by race. The estimated coefficients are similar for Blacks and Whites, with a weak contemporaneous effect, a strong one-month lag effect, and a weak (usually negative

Figure 7: Treated-Control Difference by Race in UE Rates



Note: Specification includes state and month fixed effects, vaccination rate, squared vaccination rate, indoor days, and vulnerability variables interacted with trends as in Table 5 columns (7-8). Vertical lines are 95% confidence interval. Difference is calculated using triple difference. Estimates are weighted by race population. Standard errors clustered at state level.

but small and insignificant) two-month lag effect. The sum of coefficients is sometimes larger for Blacks, sometimes larger for Whites, and the different is never statistically significant. Thus, the data do not support the hypothesis that the higher death rate of Blacks resulted from a stronger community transmission mechanism or a lower vaccination rate. Rather, the racial gap in mortality results from the greater exposure of Blacks to infection in the workplace as a consequence of their higher rate of re-employment.

6 Conclusion

Although the worst of the covid pandemic is behind us, and many would prefer to have it vanish even from their rear-view mirrors, there is still much to learn from the covid experience that can inform policy makers about responses to future outbreaks of infectious disease. The dimension we focus on in this paper is Black-White differences in covid deaths and how those relate to unemployment insurance (UI) that encourages social distancing. We focus on the large cuts in UI benefits in summer 2021 because the variation across states in the timing of the cuts provides leverage for identifying the causal effects on both covid deaths and re-employment behavior.

We first provide evidence of a relationship between UI cuts and covid death rates, showing that the rates more than double for both Blacks and Whites, but with a higher increase for Blacks, in states cutting UI earlier. The relationship is robust to different specifications and control variables, and there is no indication of a pre-trend in the months leading up to the

Table 6: Estimated Effects of $PUIend$ on Covid Deaths: 2SLS

	1	2	3	4	5	6
	Black	White	Black	White	Black	White
$\widehat{UE}_t$	0.0202	0.0480	0.0369	-0.0147	0.0428	-0.0172
	(0.0333)	(0.0444)	(0.0460)	(0.0453)	(0.0502)	(0.0450)
$\widehat{UE}_{t-1}$	0.1303	0.1330	0.1100	0.1310	0.0967	0.129
	(0.0607)	(0.0473)	(0.0443)	(0.0527)	(0.0448)	(0.0534)
$\widehat{UE}_{t-2}$	-0.0016	0.0130	-0.0151	-0.0193	-0.0322	-0.0298
	(0.0429)	(0.0375)	(0.0755)	(0.0480)	(0.0979)	(0.0460)
$\Sigma_{k=0}^{2}\alpha_{t-k}$	0.1489	0.1938	0.1321	0.0973	0.1073	0.0819
	(0.0694)	(0.0610)	(0.0971)	(0.0585)	(0.1252)	(0.0569)
Diff B-W	-0.0449		0.0348		0.0254	
	(3.6236)		(42.6700)		(8.1176)	
Controls	N	N	Y	Y	Y	Y
Vulnerability Trend	N	N	N	N	Y	Y

Note: Estimates of Equation (4). Black rate is adjusted using Equation (1). Observations = 450 (45 states, 10 months). Controls = vaccination rate and it square, indoor days. Controls = vaccination and its square, and indoor days. Vulnerability trend includes age over 65 population share, cardiovascular disease rate, obesity rate, and median earning, interacted with time trend. First stage results are in Appendix Tables A2, A3, and A4 for columns (1-2), (3-4), and (5-6), respectively. Difference in Black and White in sum of coefficients is calculated using full-interaction triple difference. Estimates are weighted by race population. Standard errors clustered at state level are in parentheses.

cuts. Permutation tests show estimates are large outliers relative to placebos. Turning to the mechanism through which UI could affect covid outcomes, we provide evidence at the 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, for instance because of differences in vaccination or social distancing, we find no evidence supporting this possibility: the impact of a one-unit rise in re-employment resulting from the policy shows no racial difference, implying that the racial gap in covid deaths is driven by the magnitude of the re-employment rate difference.

There are a number of important caveats to these results. To start with, 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. This level is the same as that at which the UI policy varies, so it is appropriate in some ways, but it would have been better if it were possible to control for more granular types of heterogeneity. Also, because the age structure of the Black and White populations are so different, it is necessary to construct consistent death rates by age structure, yet there are frequent

suppressions in the available data for race by age by state by month. We have employed alternative methods of age-adjusted death rates, each with advantages and disadvantages, and we have found similar results across the different methods, but it must be recognized that there is no perfect way of handling this missing data problem. The re-employment analysis uses data on unemployed individuals including whether they become re-employed in the following month and allows us to control for many important characteristics, but we do not observe whether the unemployed individual is actually a UI recipient, and thus if the UI cut is a treatment intended to affect all unemployed then our estimates reflect intent-to-treat effects, and are understated relative to average treatment effects; to the extent that the UI recipiency rate is lower for Blacks than Whites (Kuka and Stuart, 2021; Skandalis et al., 2022), our estimates of the difference in the policy impact on re-employment are also understated. Finally, while our instrumental variable 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. Nevertheless, we believe our study offers some lessons with potential value for future research and policy, both on the possibly divergent effects by race of a race-neutral program such as UI and on planning the policy response to a future pandemic.

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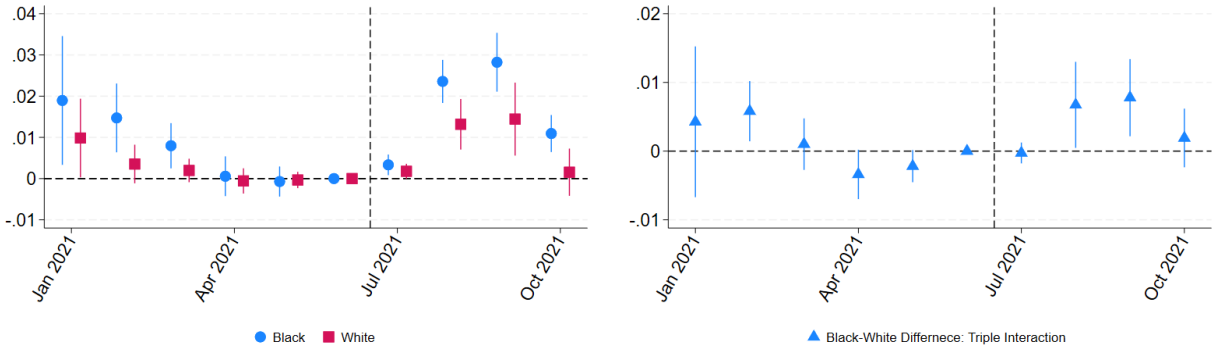
Appendix

Table A1: Estimating Impact of $PUIend$ on Covid Deaths: Simple Age-Adjustment

	1	2	3	4	5	6
	Black	White	Black	White	Black	White
$PUIend_t$	0.0012	0.0018	0.0006	-0.0001	0.0003	0.0000
	(0.0017)	(0.0017)	(0.0024)	(0.0015)	(0.0026)	(0.0015)
$PUIend_{t-1}$	0.0186	0.0122	0.0184	0.0121	0.0184	0.0120
	(0.0020)	(0.0028)	(0.0020)	(0.0028)	(0.0020)	(0.0028)
$PUIend_{t-2}$	0.0055	0.0043	0.0016	0.0004	0.0015	-0.0002
	(0.0018)	(0.0020)	(0.0037)	(0.0025)	(0.0036)	(0.0027)
$\Sigma_0^2 \beta_{t-k}$	0.0253	0.0183	0.0206	0.0123	0.0202	0.0119
	(0.0033)	(0.0029)	(0.0056)	(0.0038)	(0.0063)	(0.0041)
Diff B-W	0.0070		0.0083		0.0083	
	(0.0023)		(0.0042)		(0.0046)	
State, Month FE	Y	Y	Y	Y	Y	Y
Controls	N	N	Y	Y	Y	Y
Vulnerability trend	N	N	N	N	Y	Y

Note: Estimates of Equation (3). Black rate is adjusted using Equation (1a). Observations = 450 (45 states, 10 months). Controls = vaccination rate, it square, indoor days at $t-1$. Vulnerability trend × trend. Difference in Black and White in sum of coefficients is calculated using full-interaction triple difference. Estimates are weighted by race population. Standard errors clustered at state level are in parentheses.

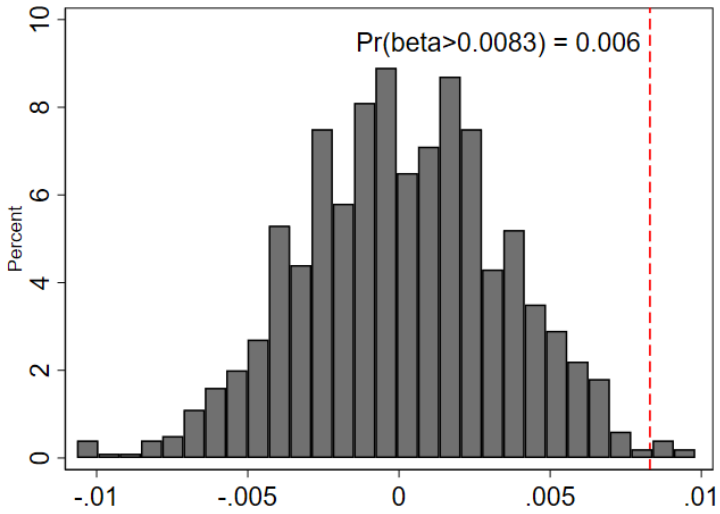
Figure A1: Treated-Control Difference in Covid Death Rates



Treated-Control Difference by Race | Treated-Control, Black-White Difference

Note: Estimates of Equation (3). Specification includes state and month fixed effects, vaccination rate, squared vaccination rate, indoor days, and vulnerability variables interacted with trends as in Table A1 columns (5-6). Vertical lines are 95% confidence interval. Difference is calculated using triple difference. Estimates are weighted by race population. Standard errors clustered at state level.

Figure A2: Permutation Test



Note: Estimates of Equation (3). Vertical line is the estimated Black and White difference from Table A1 columns(5-6). Black rate is adjusted using Equation (1a). Specification includes state and month fixed effects, vaccination rate, squared vaccination rate, indoor days, and vulnerability variables interacted with trends.

Table A2: Estimated Effects of *PUIend* on Covid Deaths: First Stage Base Specification

	1	2	3	4	5	6
	Black UE_t	Black UE_{t-1}	Black UE_{t-2}	White UE_t	White UE_{t-1}	White UE_{t-2}
$PUIend_t$	0.131	-0.00288	0.0645	0.107	-0.0261	-0.0229
	(0.0488)	(0.0392)	(0.0757)	(0.0491)	(0.0324)	(0.0486)
$PUIend_{t-1}$	0.0839	0.144	-0.0762	0.00866	0.114	-0.0208
	(0.0945)	(0.0531)	(0.0822)	(0.0385)	(0.0449)	(0.0477)
$PUIend_{t-2}$	-0.0324	0.0631	0.185	0.027	0.00983	0.118
	(0.0466)	(0.0847)	(0.103)	(0.0349)	(0.0311)	(0.0347)
Controls	N	N	N	N	N	N
Vulnerability	N	N	N	N	N	N

Note: Estimates of Equation (6), first stage with *PUIend* and its lags as instrumental variables. Black rate is adjusted using Equation (1). Observations = 450 (45 states, 10 months). Controls = vaccination rate and it square, indoor days. Controls = vaccination and its square, and indoor days. Vulnerability trend includes age over 65 population share, cardiovascular disease rate, obesity rate, and median earning, interacted with time trend. Corresponding estimates are in Table 6, columns (1-2). Standard errors clustered at state level are in parentheses.

Table A3: Estimated Effects of $PUIend$ on Covid Deaths: First Stage Controlled Specification

	1	2	3	4	5	6
	Black UE_t	Black UE_{t-1}	Black UE_{t-2}	White UE_t	White UE_{t-1}	White UE_{t-2}
$PUIend_t$	0.106	-0.023	0.0471	0.132	0.00903	-0.0195
	(0.0624)	(0.0557)	(0.0891)	(0.0508)	(0.0375)	(0.0585)
$PUIend_{t-1}$	0.0831	0.145	-0.0812	0.00818	0.113	-0.0208
	(0.0944)	(0.0544)	(0.0826)	(0.0382)	(0.0450)	(0.0486)
$PUIend_{t-2}$	-0.0626	0.0558	0.121	0.046	0.0287	0.116
	(0.0690)	(0.0558)	(0.124)	(0.0392)	(0.0420)	(0.0411)
Controls	Y	Y	Y	Y	Y	Y
Vulnerability	N	N	N	N	N	N

Note: Estimates of Equation (6), first stage with $PUIend$ and its lags as instrumental variables. Black rate is adjusted using Equation (1). Observations = 450 (45 states, 10 months). Controls = vaccination rate and it square, indoor days. Controls = vaccination and its square, and indoor days. Vulnerability trend includes age over 65 population share, cardiovascular disease rate, obesity rate, and median earning, interacted with time trend. Corresponding estimates are in Table 6, columns (3-4). Standard errors clustered at state level are in parentheses.

Table A4: Estimated Effects of $PUIend$ on Covid Deaths: First Stage Final Specification

	1	2	3	4	5	6
	Black UE_t	Black UE_{t-1}	Black UE_{t-2}	White UE_t	White UE_{t-1}	White UE_{t-2}
$PUIend_t$	0.0971	-0.0260	0.0353	0.140	0.0146	-0.0114
	(0.0643)	(0.0547)	(0.0897)	(0.0526)	(0.0382)	(0.0578)
$PUIend_{t-1}$	0.0848	0.146	-0.0798	0.00762	0.112	-0.0205
	(0.0953)	(0.0546)	(0.0828)	(0.0386)	(0.0452)	(0.0487)
$PUIend_{t-2}$	-0.0705	0.0828	0.106	0.0532	0.0333	0.137
	(0.0789)	(0.0630)	(0.113)	(0.0400)	(0.0463)	(0.0438)
Controls	Y	Y	Y	Y	Y	Y
Vulnerability	Y	Y	Y	Y	Y	Y

Estimates of Equation (6), first stage with $PUIend$ and its lags as instrumental variables. Black rate is adjusted using Equation (1). Observations = 450 (45 states, 10 months). Controls = vaccination rate and it square, indoor days. Controls = vaccination and its square, and indoor days. Vulnerability trend includes age over 65 population share, cardiovascular disease rate, obesity rate, and median earning, interacted with time trend. Corresponding estimates are in Table 6, columns (5-6). Standard errors clustered at state level are in parentheses.