



# Highly public anti-Black violence is associated with poor mental health days for Black Americans

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**Highly public anti-Black violence in the United States may cause widely experienced distress for Black Americans. This study identifies 49 publicized incidents of racial violence and quantifies national interest based on Google searches; incidents include police killings of Black individuals, decisions not to indict or convict the officer involved, and hate crime murders. Weekly time series of population mental health are produced for 2012 through 2017 using two sources: 1) Google Trends as national search volume for psychological distress terms and 2) the Behavioral Risk Factor Surveillance System (BRFSS) as average poor mental health days in the past 30 d among Black respondents (mean weekly sample size of 696). Autoregressive moving average (ARMA) models accounted for autocorrelation, monthly unemployment, season and year effects, 52-wk lags, news-related searches for suicide (for Google Trends), and depression prevalence and percent female (for BRFSS). National search interest varied more than 100-fold between racial violence incidents. Black BRFSS respondents reported 0.26 more poor mental health days during weeks with two or more racial incidents relative to none, and 0.13 more days with each log<sub>10</sub> increase in national interest. Estimates were robust to sensitivity tests, including controlling for monthly number of Black homicide victims and weekly search interest in riots. As expected, racial incidents did not predict average poor mental health days among White BRFSS respondents. Results with national psychological distress from Google Trends were mixed but generally unsupportive of hypotheses. Reducing anti-Black violence may benefit Black Americans' mental health nationally.**

racism | mental health | population health | disparities | big data

Recent police killings of Black Americans (e.g., George Floyd, Michael Brown) were societal flashpoints that generated massive protests and intense public attention to police misconduct within the United States (1). Aided by video footage, widespread discussion via social media, and coverage by traditional news organizations, numerous incidents of police use of deadly force became major national stories with potential wide-reaching effects on public perceptions and even well-being. For instance, documenting acts of anti-Black violence helped generate and broaden public support for the Black Lives Matter movement (2, 3), but such news may also be a source of stress for Black Americans (4). In particular, because police and state-sanctioned violence against Black persons are both contemporary and historic in the United States (5), recognition of persistent injustices following highly public police killings may evoke anger, threat, and grief (3, 6, 7). Thus, incidents of racial violence have potential to become major events that serve as a window into the structure of society and elicit strong emotional responses among observers. However, population health effects of highly public racial violence are not understood (4, 8, 9).

Discrimination is an established risk factor for poor mental health, whether personally experienced or observed against one's ingroup (10). That racial discrimination and racism can be vicariously experienced and act as a health determinant via stress-mediated

pathways is increasingly being documented (11–15). Racialized public incidents may therefore have measurable spillover effects on the mental and physical health of members of the targeted racial group. For instance, following a large immigration raid in Postville, Iowa, low birth weight risk increased among United States-born and immigrant Latinas in the state but not Whites, postulated to be due to elevated racial stress and terror (16). Similarly, Arabic-named women had poorer birth outcomes during a period of heightened anti-Arab sentiment following the September 11th terrorist attack (17). Moreover, evidence from major disasters suggests that mental health and stress effects are likely to extend past direct victims and their networks to include nonacquaintances (18, 19). Thus, unjust and destabilizing racial and sociopolitical events may lead to widely experienced psychological distress and accompanying adverse health effects, particularly for persons who identify with victims.

Robust evidence for societal events acting as widespread stressors comes from large cross-sectional samples where data were being collected prior to and after focal events (4, 18–20). Yet, the necessary reliance on natural experiment designs has led to key limitations in the extant literature. In particular, studies have generally not quantitatively described public interest, awareness, or distress in response to specific events, limiting comparisons between presumed societal stressors (i.e., treating events of varying intensity as equivalent) and not adequately demonstrating the plausibility of widespread effects (4, 19). For example, a germinal paper identifying spillover effects on mental health used the number of state-level police killings of unarmed Black persons in

## Significance

**Violent acts that are widely publicized and perceived as anti-Black may harm the mental health of observers, particularly Black Americans. We identified 49 incidents of racial violence in the United States, occurring between 2013 and 2017 and receiving widely varying levels of search interest. We show that Black but not White Americans reported poorer mental health in weeks when two incidents of anti-Black violence occurred and when national interest was higher. Reducing racial violence, including police killings of Black individuals, is likely to benefit the mental health of Black Americans nationally.**

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the prior 3 mo as the exposure variable, thus assuming equivalence across eligible killings despite likely meaningful differences in public awareness. Also influenced by a lack of measurement of the exposure, the extent to which effects are targeted by geography or social group is poorly understood, although larger effects with closer proximity (e.g., geographic, identity) are presumed (4, 16). In addition, a key limiting factor for this research is data scarcity for pre- and postevent periods from unbiased, random samples. Relatedly, there is often a substantial time lag from when a distressing societal event occurs until the release of data from large continuously administered surveys and health records (e.g., survey data from the Behavioral Risk Factor Surveillance System [BRFSS] are not released until ~8 mo after the calendar year ends), such that real-time surveillance of population health during periods of societal crises is substantially limited.

One potential solution to these challenges is the use of large-scale public data sources released in near real-time that allow for operationalization of public interest in societal events and modeling of mental or physical health sequelae. As an example, Google Trends—a tool that utilizes Google Search and other Google products to reveal aggregated user search behavior—has been tested as a real-time measure of aggregate suicide risk (21–23). Early research was encouraging in that associations were found between search volume for suicide-related terms and suicide incidence using time-series and aggregate methods in the United States and elsewhere (21–23). However, subsequent systematic data analysis showed these associations to be inconsistent across search terms and countries (24). Google Trends has been used in other forms of health surveillance and to document changes in psychological health and mood (25–27). Overall, existing evidence supports the notion that Google Trends may serve as a real-time albeit weak indicator of health and well-being, yet further research is needed (25).

The aim of the current study was to describe incidents of racial violence and test whether they acted as societal stressors by harming the mental health of Black Americans. We identified as potential stressors a subset of high publicity police killings and hate crime murders of Black persons in the United States. These included 28 and 10 police killings of unarmed and armed Black individuals, respectively; 9 decisions not to indict or convict the officer involved; and 2 white supremacist murders of Black persons. We expected substantial variation in news coverage and national search interest for these incidents, demonstrating potential heterogeneity in spillover effects. National interest was indexed using the weekly relative volume of Google searches that included the victim's name following the incident and was combined across incidents to proxy for weekly variation in national anti-Black stress. We used two data sources to measure weekly population mental health: 1) Google Trends as national search volume for terms with potential to indicate psychological distress (e.g., depression; how to suicide; see *SI Appendix, Table S1*) and 2) the BRFSS as the average number of poor mental health days in the past 30 d among Black respondents. Our hypothesis was that national psychological distress would be higher in weeks when racial violence incidents occurred and with higher national interest in racial incidents. BRFSS allowed for consideration of respondent race, and we hypothesized poorer mental health for Black but not White respondents for weeks with racial incidents and higher national interest.

## Results

A total of 49 high publicity incidents of racial violence were identified representing the years 2013 through 2017, with 219 wk (84.2%) having no incident, 34 wk (13.1%) with one incident, and 7 wk (2.7%) with two or more incidents. Descriptive statistics for racial incidents by type and search interest terciles are shown in Table 1. Values are reported on a relative scale (in percentage points) where 100 represents the week when Michael

Brown received the highest search interest. We summed interest in each incident over 9 wk to allow flexibility in timing, and individual incidents received a total of less than 1 to 173% of Michael Brown's highest week. Fig. 1 depicts relative search interest by week separately for low-, medium-, and high-interest racial incidents with four reference events included. We include these references to show how diverse events may be quantified on a comparable scale. Notably, reference events that were acts of mass violence (i.e., the Boston marathon bombing and Las Vegas mass shooting) received 374 and 443% of the search interest as Michael Brown's highest week. Additional descriptive statistics for the full list of racial incidents are reported in *SI Appendix, Table S2*. Overall, these findings demonstrate potential for misspecified effects when treating police killings equivalently in statistical models.

Spanning the period from January 1, 2012, to December 30, 2017, we created 312 weekly observations from BRFSS 2012 through 2017 (an additional year was included due to the use of 52 wk lags). For Black respondents, the mean weekly sample size was 696 (SD = 180; range: 220 to 1,205), and weekly samples, on average, were 64.2% female (SD = 3.1) and 15.5% reporting a depressive disorder (SD = 1.6). For Whites, mean weekly samples had 6,707 respondents (SD = 1,795), of which 57.6% were female (SD = 2.1), and 19.3% reported depression (SD = 1.0). We considered whether characteristics of weekly Black BRFSS samples varied with racial incidents to test for potential sample selection bias. The number of racial incidents was not a significant predictor of the weekly number of Black respondents [ $F(2,257) = 0.11, P = 0.90$ ], percent of all respondents who identified as Black [ $F(2,257) = 0.20, P = 0.82$ ], percent of Black respondents who were female [ $F(2,257) = 2.28, P = 0.10$ ], or percent of Black respondents reporting a depressive disorder [ $F(2,257) = 0.20, P = 0.82$ ]. Similarly, interest in racial incidents was not correlated with the number of Black respondents ( $r = -0.09, P = 0.16$ ), percent of Black respondents in the full sample ( $r = -0.04, P = 0.57$ ), or percent with a depressive disorder ( $r = -0.05, P = 0.38$ ). However, Black BRFSS samples had a lower percentage of female respondents in weeks when interest in racial incidents was higher ( $r = -0.13, P = 0.032$ ).

Correlations between key study variables are included in *SI Appendix, Table S3*. Number of racial violence incidents and affiliated search interest were not correlated with national psychological distress but had weak, nonsignificant correlations with poor mental health days among Black BRFSS respondents ( $r = 0.08, P = 0.17$ ;  $r = 0.08, P = 0.20$ , respectively).

To examine whether our Google Trends measure serves as a valid indicator of national psychological distress, we used weekly averages of poor mental health days in the past 30 d from the full BRFSS sample as a validity check in regression models. Adjusting for news-related searches including suicide and first order autoregressive (AR[1]) and moving average (MA[1]) disturbances, we regressed national psychological distress on poor mental health days for the national BRFSS sample in the same week and following 5 wk. Inclusion of subsequent weeks was motivated by the 30 d reference period for the BRFSS item, with observations from 4 and 5 wk later included as falsification tests. National psychological distress was predicted by the average number of poor mental health days measured in the same week ( $P = 0.080$ ) and following 3 wk ( $P < 0.05$ ), further described in *SI Appendix*.

**Hypothesis Test Using National Weekly Time Series.** To test whether high-publicity racial violence influenced mental health, we considered both national psychological distress and average poor mental health days among Black BRFSS respondents as outcomes. Regression results are shown in Table 2 (full models are presented in *SI Appendix, Table S4*). National psychological distress was 0.20 SD units higher in weeks when one high publicity racial incident occurred relative to none (95% CI: 0.01, 0.38; see Model 1a). However, the estimate was of comparable magnitude,

**Table 1. Descriptive statistics for 49 incidents of racial violence by type and search interest terciles**

Type of incident	Cumulative search interest	
	Median (Q <sub>1</sub> , Q <sub>3</sub> )	Range
Police killing of unarmed Black person ( <i>n</i> = 28)	2.9 (1.0, 6.7)	0.1–172.7
Police killing of armed Black person ( <i>n</i> = 10)	17.5 (1.0, 77.0)	0.9–161.9
No indictment of officer ( <i>n</i> = 9)	16.5 (4.5, 43.1)	3.4–157.7
Hate crime murders of Black persons ( <i>n</i> = 2)	N/A	0.6–53.1
Search interest terciles		
Low interest ( <i>n</i> = 17)	0.9 (0.3, 1.1)	0.1–2.4
Medium interest ( <i>n</i> = 16)	4.4 (3.6, 6.5)	2.7–10.8
High interest ( <i>n</i> = 16)	39.0 (22.9, 136.5)	13.8–172.7

Q<sub>1</sub> = first quartile. Q<sub>3</sub> = third quartile. Nine-week cumulative search interest for each incident is on relative scale, in percentage points, where 100 indicates the week when Michael Brown received the highest search interest.

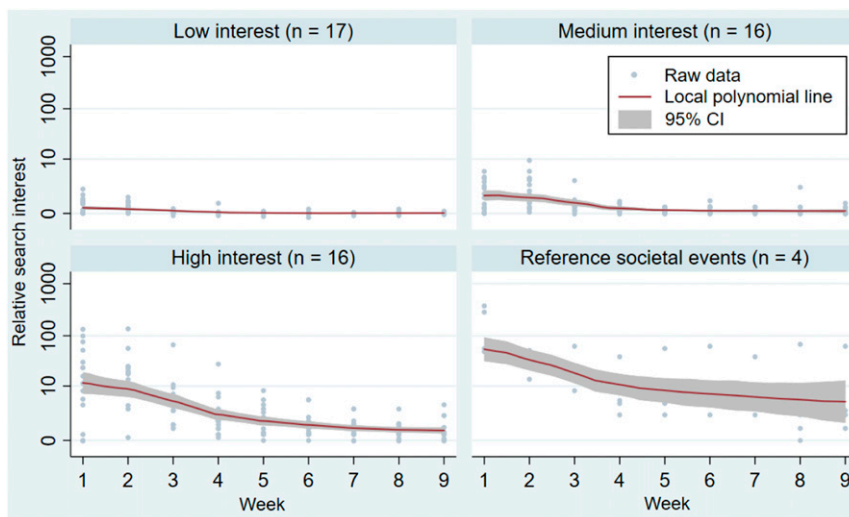
albeit nonsignificant, and in the opposite direction for weeks with two or more incidents. Shown in Model 1b, weekly national search interest was not associated with national psychological distress. In addition, to explore differences between indicators, we separately modeled the four psychological distress indicators as outcomes (*SI Appendix, Table S5*). Estimates for higher distress in weeks when one racial incident occurred were in the same direction but of varying magnitude (depression:  $B = 0.19$ , 95% CI:  $-0.06, 0.45$ ; anxiety:  $B = 0.07$ , 95% CI:  $-0.01, 0.15$ ; suicide-related terms [how to suicide]:  $B = 0.25$ , 95% CI:  $-0.04, 0.54$ ; [kill myself]:  $B = 0.06$ ; 95% CI:  $-0.10, 0.22$ ).

Results for poor mental health days among Black respondents are shown in Table 2, Models 2a through 2d. Consistent with hypotheses, the average number of poor mental health days for Black respondents was higher in weeks when two high publicity racial violence incidents occurred and when search interest was higher. In particular, relative to weeks with no incident, Black BRFSS respondents reported an average of 0.31 more poor mental health days in the past 30 d during weeks with two or more racial incidents, equivalent to an effect size of 0.84 SD units. Each log<sub>10</sub> unit increase in national weekly interest was associated with 0.13 more average poor mental health days; scaled differently, a 1 SD increase in interest predicted 0.17 higher SD units in the

outcome. To account for differences in weekly samples, particularly if varying systematically with publicized racial violence, percent of female respondents and prevalence of depressive disorders among weekly Black BRFSS samples were added in Models 2c and 2d. The estimate for two or more racial incidents was attenuated by ~15% but remained significant ( $B = 0.26$ , 95% CI: 0.10, 0.43), whereas the estimate for national interest was of comparable magnitude across models. Unexpectedly, Black poor mental health days were not higher in weeks with one racial incident in either Models 2a or 2c.

Due to the possibility that adverse effects may be delayed, lags up to 3 wk were tested for racial incidents and search interest for both mental health outcomes (*SI Appendix, Table S6*). Weeks with one racial incident were followed by lower national psychological distress (from Google Trends) 1 and 3 wk later ( $B = -0.18$ , 95% CI:  $-0.36, -0.01$ ;  $B = -0.22$ , 95% CI:  $-0.38, -0.06$ , respectively). No other lags were significant, including when modeling poor mental health days among Black BRFSS respondents.

**Sensitivity Tests.** Because we expected effects to be primarily limited to Black Americans, we fit models using White BRFSS respondents as falsification tests (*SI Appendix, Table S7*). Results indicated nonsignificant associations between weekly racial incidents



**Fig. 1.** Relative search interest by week for high-publicity incidents of racial violence and reference events, with local polynomial smoothed lines. Racial incidents are split into terciles based on cumulative search interest in the first 9 wk. Reference events include the Las Vegas mass shooting, the Boston Marathon bombing, and the 2016 Hillary Clinton and Donald Trump presidential campaign announcements.



**Table 2. Time series models testing associations between high publicity racial violence and mental health outcomes, 2012–2017**

Predictor variables	National psychological distress		Black poor mental health days			
	Model 1a*		Model 2a		Model 2c <sup>†</sup>	
	B	[95% CI]	B	[95% CI]	B	[95% CI]
Racial violence incidents (ref.=none)						
One incident	0.20	[0.01, 0.38]	0.02	[-0.14, 0.18]	0.03	[-0.10, 0.15]
Two+ incidents	-0.19	[-0.57, 0.20]	0.31	[0.08, 0.54]	0.26	[0.10, 0.43]
Autocorrelation parameters						
AR (1)	0.56	[0.21, 0.92]	0.07	[-0.07, 0.21]	0.04	[-0.12, 0.20]
MA (1)	-0.25	[-0.60, 0.10]				
52-wk lag	0.56	[0.32, 0.79]	0.16	[0.03, 0.29]	0.06	[-0.06, 0.19]
Log <sub>10</sub> (national interest in racial incidents)	0.04	[-0.21, 0.29]	0.13	[0.05, 0.22]	0.13	[0.05, 0.21]
Autocorrelation parameters						
AR (1)	0.60	[0.23, 0.97]	0.04	[-0.09, 0.17]	0.01	[-0.14, 0.16]
MA (1)	-0.29	[-0.65, 0.08]				
52-wk lag	0.54	[0.29, 0.78]	0.16	[0.03, 0.29]	0.07	[-0.05, 0.18]

Estimates are unstandardized, and robust 95% CIs are shown. All models adjust for monthly unemployment (first differenced) and season and year fixed effects.

\*Includes news-related search volume for suicide.

<sup>†</sup>Includes percent of female respondents and prevalence of depressive disorders.

(for 1 and 2+ relative to none) and national interest with average poor mental health days among White BRFSS respondents.

Inferences with the Black poor mental health days outcome were robust to a series of sensitivity tests. First, to consider non-linear effects, we created four categories of weekly search interest (representing no search interest and remaining weeks split into terciles). In a model otherwise comparable to Model 2d, Black Americans had more poor mental health days in high interest weeks relative to no interest ( $B = 0.20$ , 95% CI: 0.09, 0.32). Estimates were in the expected direction but nonsignificant for moderate and low interest weeks (i.e.,  $B = 0.08$ , 95% CI: -0.02, 0.18;  $B = 0.05$ , 95% CI: -0.05, 0.16, respectively). In addition, we refit models controlling for monthly number of homicides including Black victims, and separately search interest in riots (proxying for actual riots and related news coverage), but adjustment for these variables did not appreciably alter estimates and inferences for the racial violence variables (*SI Appendix, Table S8*). Finally, the four types of racial incidents were modeled individually using combined search interest within respective incident types (*SI Appendix, Table S9*). Associations with national psychological distress were inconsistent in direction between types, and the only significant estimate was in the unexpected direction for police killings of armed Black persons. In contrast, when predicting Black poor mental health days, estimates were in the same direction and indicated poorer mental health with greater national interest in racial incidents, yet only decisions not to indict or convict the officer involved was a significant predictor.

We used weekly panel data for states to examine whether associations between racial violence and psychological distress were larger in states where the incident occurred and in states with a higher share of Black residents. Due to prior results suggesting our Google Trends measure is a weak indicator of psychological distress and the lack of consistent findings for hypothesis tests with national psychological distress, we reported findings as exploratory tests in *SI Appendix*. In brief, results provided inconclusive evidence that effects were larger in the state of occurrence, and associations did not vary based on state population share of Black residents.

## Discussion

When documented by news organizations and widely shared via social media, violent interactions seemingly motivated by racism or prejudice have potential to become major societal events (28). Incidents may be especially prone to elicit public outrage when understood as a product of systemic and historical injustices (3, 28) and could evoke experiences of distress among individuals sharing a racial identity with the victim. Accordingly, we find that Black Americans reported a higher number of poor mental health days in weeks when two or more high-publicity racial violence incidents occurred and when national interest was higher. Aggregated online search behavior likely operates as a proxy for timing and intensity of public attention (29), allowing us to quantify national exposure to racial incidents. That anti-Black violence and ensuing national interest predicted poor mental health days for Black but not White Americans is consistent with causal effects. Further strengthening the case for causal inference, we accounted for expected poor mental health days by controlling for prevalence of depressive disorders in large weekly samples, by implementing robust time series methods (i.e., autoregressive moving average [ARMA] disturbances, annual lags and trends), and by considering several sensitivity analyses. Our study findings are broadly consistent with prior research detailing a higher number of poor mental health days for Black Americans exposed to police killings of unarmed Black persons within their state (4). We extended this research by examining heterogeneity in interest in police killings and other racial incidents and demonstrated that adverse mental health effects following major racial incidents may be national in scale yet primarily experienced by Black Americans. Our findings are specific to a subset of racial violence that received considerable news coverage. Incidents receiving little news or social media coverage are unlikely to widely influence mental health (e.g., have state or national spillover effects) but may have adverse health and social effects within the neighborhoods where incidents occur (9, 30).

Nonsignificance of lagged effects suggests that declines in Black Americans' mental health may primarily be in weeks when interest remains elevated. As an example, based on national interest in the killings of Akai Gurley (11/20/2014) and Tamir Rice (11/22/2014), our estimates (from Model 2d) indicate that Black Americans

nationally had an average of 0.13 more poor mental health days in the week following the incidents relative to the counterfactual of no incidents, and, in the next 7 wk, 0.10, 0.09, 0.08, 0.05, 0.03, 0.04, and 0.02 more poor mental health days. We omit the initial incident week in these estimates because number of incidents per week and national interest were not simultaneously modeled, but Model 2c estimates suggest 0.26 additional poor mental health days in the incident week. The temporary nature of population-level effects is consistent with prior research on societal stressors (e.g., terrorist acts, controversial political events), although the identified harmful effects on mental health have spanned periods of weeks to a few months (4, 18, 31). Integrating measures of the timing of public interest or distress may advance this research on the duration and scale of spillover effects from societal stressors. In addition, individual-level vulnerability, cumulative, and moderating processes warrant further research. For instance, some individuals may experience traumatic symptoms and adverse health effects years after exposure (32). Moreover, while we identified short-term impacts on psychosocial well-being, the accumulation of exposure to racial societal stress over time may act as a chronic stressor that contributes to an accelerated physiologic dysregulation or aging process (33). Exposure to societal incidents of racial violence may also amplify personal experiences of acute and chronic racial stress, with potential for cumulative, longer-term health impacts that cannot be captured with this study design.

Another conclusion from this study, given our mixed findings with the Google Trends derived outcome, is that aggregated online search behaviors may have limited value for understanding population mental health effects of racial incidents. In particular, our results indicate that psychological distress increased in weeks when one racial incident occurred relative to none but may have decreased for weeks with two incidents. State-level analyses showed some parallel findings, demonstrating that psychological distress may be higher following medium-interest racial incidents but lower after high-interest incidents. Moreover, national distress decreased in the weeks following a high publicity racial incident (lags 1 and 3). The reasons for such mixed findings are unclear but merit further attention. One major measurement limitation is that only ~13.4% of Americans identify as Black or African American, such that elevated distress for Black Google users could be overwhelmed by variations among other users. However, if the primary weakness of Google Trends data in the present study is the small share of users constituting the likely exposed group, then Google Trends may still be a valid tool when examining the effects of societal events expected to generate more widely experienced distress.

Additional reasons for mixed findings are 1) emotional responses to unjust societal events may vary according to collective and judicial-political actions following incidents, and 2) the use of Google Search may change during periods of intense societal stress but in ways our measurement strategy did not capture. More specifically, highly public racial incidents may spur protests and social movements as well as political and judicial actions. If such actions are perceived as progress and supportive of racial justice efforts, psychological distress may even subside (e.g., due to higher optimism, enhanced social solidarity). The counter example of limited social mobilization and insufficient political-judicial responses could lead to elevated distress and despair, particularly among persons expecting meaningful actions, or, for instance, among Black individuals who believe their fate is linked with that of their racial group (34). Thus, although prior literature suggests protests and other collective actions are associated with greater traumatic symptoms and depression (35), the political response to social movements may be a key factor in determining population mental health effects and individual responses. Alternatively, newsworthy events, particularly when emotionally evocative, may serve as competing demands for attention and search behavior, such that the validity of Google Trends measures of psychological distress could be systematically biased in a way that

would confound our analyses. For instance, individuals may become preoccupied with details of the incident or the surrounding public conversation rather than contemplate their own mental health, or the higher share of searches relating to major events may decrease the total share but not absolute volume of searches due to psychological distress. Additionally, the terms selected for Google Trends may primarily indicate severe psychological distress to a greater degree than is broadly experienced following racial incidents. Alternative search terms may be identified in future research to capture less severe distress, or even stress coping strategies more commonly used by Black Americans (e.g., religious coping) (36). Understanding how racial societal stressors influence the use of internet search platforms is important for efforts to use Google Trends in forecasting and surveillance (29).

Our findings do not address the specific processes underlying the link between racial incidents and poorer mental health. Notably, of the incident types, only news of legal decisions not to indict or convict the officer involved had a significant relationship with poor mental health days. If replicated, the unique importance of legal proceedings to influence spillover effects suggests that distress may stem from perceptions of injustice or grief rather than heightened threat or vigilance. Analysis of social media content following high-publicity police killings revealed that injustice was a common theme of posts (3, 28). Rapid, honest, and transparent communication of evidence and investigative processes may therefore be critical in reducing adverse mental health effects of highly public incidents of racial violence. In addition, structural and historical characteristics specific to an area likely influence perceptions of racial incidents (6). Tellingly, two of the three most highly searched incidents (i.e., Michael Brown and Freddie Gray) occurred in cities where the Department of Justice subsequently released extensive reports detailing racist systemic policing actions. Thus, active efforts to promote collaborative and just relationships between police departments and Black communities and social policies aimed at redressing existing racial structural inequities, such as reducing residential segregation and the wealth gap, could help attenuate perceptions of injustice following police killings.

Given the potential for far-reaching effects on the mental health of Black Americans, it is imperative that concrete steps are taken to reduce the frequency of racial violence incidents. Much current scientific inquiry and public debate is centered around police reform initiatives (37, 38), and our findings suggest that successful efforts to reduce the number of police killings and ensure accountability for police misconduct may benefit the mental health of Black Americans. Our results also raise questions, however, on how the tone and content of public discussion and journalism following potentially divisive racial incidents may act as societal stressors. Prior research indicates that exposure to traditional and social media coverage of natural disasters and terrorist events is associated with acute stress and traumatic symptoms (15, 32, 39, 40). In the case of documenting racial violence, any adverse spillover effects must be weighed against the potential for heightened public awareness, reduced anti-Black bias, and the spawning of sociopolitical movements that lead to reform actions and preventive measures (2, 3, 41). Indeed, information dissemination remains a critical responsibility of news organizations, a process that has been increasingly democratized through social media (28), and thus it is crucial that accurate and timely information is communicated to the public in a way that minimizes additional harm.

Several limitations in our research design exist. First, the poor mental health days item was self-reported and could be systematically biased by the social environment (e.g., responding in a way to signal frustration with society or per expectations of current norms for one's social group). Thus, consideration of objective outcomes is warranted. Second, use of aggregate data did not allow us to examine personal mediating and moderating factors, such as identification of distressing content (e.g., graphic videos or images, racist rhetoric); mediating cognitive, behavioral, and physiological

processes (11, 42, 43); or moderating factors (e.g., age, depression, racial identity or linked fate). Next, we did not consider within group heterogeneity (e.g., Caribbean relative to African Americans) in responses to anti-Black violence nor did we test whether other ethnoracial groups were adversely affected. Similarly, political beliefs and affiliation, independent of racial identity, may moderate psychosocial responses to news of racial violence (7). In addition, following exposure to police killings, racial justice protests, and changes in the social climate, law enforcement officers may also experience declines in mental health (40, 44). A final limitation is that we did not include an exhaustive list of racial incidents nor could we account for the numerous other sociopolitical events that may influence psychological distress levels. Although beyond the scope of this research, these topics represent important avenues for future research.

## Conclusion

Highly public incidents of racial violence, such as police killings of Black persons and decisions to not prosecute involved officers, may harm the mental health of Black Americans. Our findings thus highlight one way that racism is a public health issue. Despite limitations in making causal inference from ecological studies, our findings are consistent with research suggesting racist acts can impact community- and population-level mental health. Person-level analyses are needed to confirm our findings and to examine social and cognitive processes underlying the link between incidents of racial violence and Black Americans' mental health. Identification of systemic and structural conditions that influence the level of anti-Black violence and the processes through which spillover effects occur is essential to improving the health of Black Americans.

## Materials and Methods

**Data and Measures.** Sample data came from two sources: Google Trends and the BRFSS, each covering the period from January 1, 2012, to December 30, 2017. Google Trends is a web-based tool that provides aggregated search activity over time and by region (45). Search interest is based on a representative but nondisclosed sample of all Google searches, with our sample representing users of Google Search in the United States from 2012 to 2017. Because of widespread use (e.g., ~83% of all online searches during this period were conducted with Google) (46), even a portion of searches is sufficient to produce reliable trends in search interest for large areas (47).

BRFSS is a nationally representative surveillance system of health-related risk behaviors, which includes more than 400,000 interviews per year (48). Telephone surveys are continuously administered throughout the year by state health departments for all 50 states and the District of Columbia with assistance from the Centers for Disease Control and Prevention. To coincide with the timing of Google Trends data, we selected respondents with interview dates between January 1, 2012, to December 30, 2017. BRFSS data were used to validate the Google Trends-derived outcome and for more robust hypothesis tests where the race of the respondent was known. Restricted to the 98.3% of participants with valid responses to our outcome variable, sample sizes were as follows: full BRFSS sample for validation tests,  $n = 2,702,239$ ; non-Hispanic Black respondents for tests of hypothesized stress effects,  $n = 217,171$ ; non-Hispanic White respondents for falsification tests,  $n = 2,092,683$ . We aggregated data based on interview week within the respective samples, similar to prior research (18). Other data sources were also integrated, as described below, and include Mapping Police Violence, Proquest's US Newsstream, Anti-Defamation League H.E.A.T. Map, Bureau of Labor Statistics Unemployment Rate, FBI Supplementary Homicide Reports, and the American Community Survey. Study data, preregistration details, and other materials are available at <https://osf.io/mnqjp/>.

**Exposure Variables.** Identification and measurement of racial violence incidents was a multistep procedure that included identifying high publicity incidents based on news coverage; quantifying weekly national interest in individual incidents according to a common scale; and summarizing the number of incidents and search interest by week. Fatal incidents with Black victims were prioritized as we assumed such incidents would have higher potential to evoke psychosocial responses than nonfatal incidents (3, 28). We focused on four types: police killings of unarmed Black persons; police killings of armed Black persons; decisions not to indict or convict the officer

involved in these killings; and hate crime murders of Black persons perpetrated by white supremacists.

**Police Killings of Unarmed Black Persons.** We used a list of unarmed Black persons killed by law enforcement in 2013 through 2017 from the Mapping Police Violence (MPV) data set, including fatal police shootings and non-firearm killings (49). We selected all unarmed, named Black persons in the MPV data set ( $n = 278$ ), which, despite likely missing a few cases, should include all high publicity incidents. Although determinations of unarmed are ambiguous and controversial (4, 50), we assumed police killings where the victim was reportedly unarmed were more likely to be perceived as unjust.

News coverage for police killings was operationalized as the total number of stories including the victim's name within 60 d of the incident date and was collected using US Newsstream. Due to the potential for irrelevant search retrievals (e.g., due to a shared name), we used the following search string: (variants of "victim's name") AND ("city" OR "state") AND ("police" OR "cop" OR "officer" OR "deputy" OR "detective"). In cases where incident location and location of the law enforcement agency involved were different, either location was allowed. News coverage included a variety of types, but the large majority of search retrievals were traditional news stories. Duplicate stories were excluded. Most police killings of unarmed Black persons had little news coverage (e.g., 64% had five or fewer search hits). For high publicity police killings, we selected incidents receiving the highest decile of coverage. News coverage for these incidents ranged from 44 to 8,210 stories (mean = 658; median = 224). We considered an alternative window of 120 d for news coverage, and agreement of high-publicity cases was high (93%).

**Other incidents of racial violence.** Exclusion of police killings where the victim was allegedly armed likely omits well-known incidents with potential for spillover effects, particularly when widely reported as police misconduct (e.g., killings of Freddie Gray and Philando Castille). Thus, we also identified possible high-publicity incidents where the victim was allegedly armed or armed status was unclear using manual review of news stories and based on related queries when searching other incidents. From a list of 30 police killings of armed Black persons, we assessed news coverage in the 60 d postincident. A total of 10 incidents met our threshold for high publicity (i.e., 44 stories).

For police killings perceived as unjust, legal proceedings not resulting in indictment or conviction for the officer involved may receive substantial media attention and public interest, even comparable to the original incident (3). We identified potential high-publicity legal decisions based on second spikes in search interest from Google Trends (described further below) and confirmed that searches aligned with news of legal decisions (i.e., grand jury decisions not to indict or when a mistrial or not guilty verdict resulted). A total of nine incidents were identified, for which we assessed news coverage in the 60 d following the legal decision. Each met the threshold of 44 stories and was treated as an incident separate from the initial killing.

Hate crime murders where the perpetrator was linked with white supremacist ideology came from the Anti-Defamation League (51). We selected murders with at least one Black victim and coded news coverage following similar procedures to high-publicity killings. Because coverage of these incidents tended to focus on the perpetrator rather than victims, we used the perpetrator's name in the search string and included terms pertaining to racist motivations. In particular, we used the following search string: (variants of "perpetrator's name") AND ("city" OR "state") AND ("racist" OR "racial" OR "racism" OR "white supremacist" OR "white supremacy" OR "extremist" OR "hate crime"). We retained two white supremacist hate crime murders that were mentioned in at least 44 news stories.

**Interest in high-publicity racial incidents.** We used Google Trends to measure national interest in individual incidents by week. Search strings were defined as the victim's first and last name or the perpetrator's name for hate crime murders. We downloaded estimates for weekly relative volume of United States-based Google searches, including the specified string, with up to five names included in each request. Weekly values were on a relative scale from 0 to 100 where 100 represents the week when one of the names received the highest share of all Google searches, and other values refer to the relative percentage of this maximum (i.e., 10 indicates 10% of the relative share of searches of the 100 value). To ensure comparability across incidents, we used common references across data requests. In particular, as Michael Brown (MB) received the highest relative search volume among unarmed victims (with a value of 100 during the peak interest week), his name was used as a common reference for all incidents. However, some incidents had nearly all weekly values reported as "<1" due to relatively low search interest. To capture variance for these lower interest incidents, we used a different reference when downloading data (namely, Terence Crutcher



whose maximum interest was 10.67 relative to MB's 100), and then transformed values from these downloads to be on the MB scale (multiplying values by 10.67/100). For instance, Terence Crutcher's maximum value of 100 would be 10.67 after the transformation. In contrast, maximum weekly search interest was higher for a few armed police killings relative to MB, with Freddie Gray receiving the maximum interest in a week, and so we applied a similar transformation (in this case, based on MB's maximum value of 73.33 relative to Freddie Gray's 100, multiplying weekly values where Freddie Gray was the reference by [100/73.33]). Our approach was necessary to measure national interest in incidents on a comparable scale while including variance in lower interest incidents. In addition, we corrected for preincident search interest differences between names by averaging values for the 20 wk preincident period and subtracting this average from postincident values.

Reliability issues for Google Trends have been documented (24), a problem especially for infrequently searched terms and areas with relatively small populations (e.g., states). Google Trends estimates come from samples of Google searches, cached every day (52), such that averaging values collected on separate days improves reliability. Although some prior research averaged values from 10 series (24), weekly estimates for national relative search volume for racial incidents had very high consistency across downloads. Intraclass correlation coefficients [ICC] were >0.99 for all incidents and 0.98 for the 10 incidents with the lowest interest, indicating nearly all variance was between weeks rather than between estimates for the same week, such that averaging three series downloaded on separate days was deemed sufficient.

We manually reviewed weekly search interest for all high publicity police killings to identify cases of second spikes in search interest. We selected the highest spike that occurred at least 3 mo after the initial incident, requiring a value of at least 1 on the MB scale, and then verified the date for the spike in interest corresponded to news of legal decisions not to indict or convict an officer involved in the killing. For one incident (i.e., Freddie Gray), there were multiple eligible spikes of comparable volume, and so we selected the incident date that would include the highest interest over the 9 wk period (the selected incident date and following weeks captured three acquittals relative to an earlier single-week spike due to news of a mistrial). For news of legal decisions, the correction for preincident search interest was based on the 20 wk prior to the initial killing rather than the period preceding legal decisions.

Based on the data collected, we computed number of racial incidents occurring in each week (coded as dummy variables representing 1 and 2+ incidents relative to none) and total national search interest across incidents by week. To allow flexibility in the timing of interest, we included 9 wk of search interest for each incident. This variable had substantial skew, and so we applied a  $\log_{10}$  transformation to weekly search interest (1 was added to all values to prevent 0 values from being undefined).

### Outcome Variables.

**National psychological distress.** We used Google Trends to collect weekly search interest for terms expected to indicate psychological distress (i.e., suicidality, depression, and anxiety), spanning the period of 2012 through 2017. There is a long list of potential terms that could be included (24), but we prioritized those from prior research and with lower perceived ambiguity (e.g., searches of "suicide" capture the substantial but irrelevant search interest in the movie *Suicide Squad*) (21, 22, 24). The related queries tool of Google was used to further refine our terms, as some preliminary search strings brought up a considerable number of irrelevant related queries unlikely to indicate distress. Four psychological distress search terms were selected and are further described in *SI Appendix, Table S1*.

Weekly estimates of national search interest for depression and anxiety terms showed high reliability (ICC of 0.99 and 0.98, respectively), such that we required only three values to compute pooled averages. For suicidality terms, estimates had only moderate reliability (ICC of 0.65 and 0.73), and so we required at least 10 values for weekly averages (24). To gather weekly data across the full period we used two intervals (January 1, 2012, to March 4, 2017, and November 4, 2012, to December 30, 2017), as the time unit reported by Google Trends changes from week to month when the date range exceeds 270 wk. When the maximum value was not shared across the two intervals, we averaged the 10 series for each interval and adjusted one series so the mean value from overlapping weeks was identical. Our procedures ensured that weekly time series had high reliability (e.g., ICC for averaged suicidality terms were 0.97 and 0.98) and a common scale across the full period. Next, we standardized the averaged time series for each distress search term and averaged the four distress indicators to create a composite representing national psychological distress (which was standardized once again). Indicators for the composite demonstrated adequate internal consistency (Cronbach's  $\alpha = 0.81$ ).

In addition, weekly search interest for anxiety and depression terms was collected for individual states. Suicidality terms were omitted due to high missing data and low reliability. The procedures and reliability are further described in *SI Appendix*.

**Black poor mental health days.** As a validation of our measure of national psychological distress and a more robust test of spillover effects, we used data from the BRFSS item, "Now thinking about your mental health, which includes stress, depression, and problems with emotions, for how many days during the past 30 d was your mental health not good?". This established item has high validity and holds strong potential for measuring population mental health (53). We aggregated data by week to represent the average number of national poor mental health days among the full sample and separately for Black and White respondents.

**Covariates.** All models included the seasonally adjusted monthly unemployment rate, collected from the Bureau of Labor Statistics (54). Due to a strong decreasing trend across the study period, we first-differenced monthly unemployment to represent recent macroeconomic changes that may influence both crime rates and psychological distress.

When modeling the Google Trends measure of national psychological distress, we controlled for news-related search volume of the term "suicide" (22). This control was justified as some changes in search behavior may be due to public events or news rather than individual distress levels (e.g., interest in the suicide of public figures or news about increasing suicide rates). We used the News category option in Google Trends, downloaded data on separate days, and averaged across three time series (ICC = 0.96 for individual series).

BRFSS data on respondent's sex and history of depressive disorders were used to compute the share of female participants and prevalence of depression specific to the samples included in each weekly panel (e.g., among Black respondents). These covariates were included only when modeling poor mental health days with BRFSS data.

**Analysis Plan.** We had two overarching aims with our analyses. The first was descriptive, and we reported search interest for racial incidents. To illustrate differences in the volume and duration of heightened interest, we plotted search interest by week for low, medium, and high search interest terciles. As a reference, we included search volume for four other major contemporary events—the 2017 *Las Vegas mass shooting*, the *Boston Marathon bombing*, and the 2016 *Hillary Clinton and Donald Trump* presidential campaign announcements (search terms in italics). Search interest associated with these events was computed by subtracting average interest in the 20 wk pre-event period.

Our second aim was to examine high-publicity racial violence incidents and national interest in incidents as predictors of aggregate mental health outcomes. We used ARMA techniques to eliminate autocorrelation in weekly outcome variables. In particular, we visualized the correlogram and fit a series of models to determine ARMA parameters to include in hypothesis tests, using Akaike information criterion and Bayesian information criterion statistics as selection criteria (*SI Appendix, Table S11*). Additionally, after fitting the models presented in Table 2, we verified that residuals did not show autocorrelation patterns for up to 60 lags or white noise based on Box-Pierce Portmanteau tests (55). Models are fit in Stata 15.1 using the ARIMA command with robust confidence intervals (56).

Weekly outcome variables included 1) national psychological distress derived from Google Trends and 2) average poor mental health days among Black BRFSS respondents. Whereas the Google Trends outcome offers a test of spillover effects, the BRFSS outcome is a more rigorous test of expected stress effects due to the use of an established and validated item and the ability to restrict the sample to Black respondents. We separately modeled number of racial incidents occurring each week (using dummy variables representing one racial incident and two or more racial incidents in a week) and national search interest combined across incidents by week (on a  $\log_{10}$  scale). All models were adjusted for unemployment, year and season fixed effects, 52 wk lags, and ARMA disturbances (based on initial diagnostic tests). Season and year fixed effects are included due to reliable trends in psychological distress and to account for longer-term variations in distress and internet search behaviors. Additional model-specific controls are described in *Results*.

We considered a few sensitivity and supplemental tests. First, as falsification tests, we used average poor mental health days among White BRFSS respondents. Second, to test for nonlinear effects, we modeled search interest in racial incidents using four categories, representing no, low, medium, and high interest weeks (the latter three defined based on terciles). Third, as increases in violent crime may correlate with police killings and could influence mental health among Black Americans (e.g., through news coverage), we examined whether findings are sensitive to controlling for monthly homicide rates. Using the FBI Supplemental Homicide Reports, the monthly

number of homicides or nonnegligent manslaughters with Black victims was computed and added as a control (57). Fourth, because adverse mental health effects could stem from civil unrest and riots rather than attention to unjust racial incidents (35), we added search interest in “riots” as an additional control. Fifth, we disaggregated search interest in racial incidents to examine associations between different types of racial incidents and mental health. Finally, we used weekly panel data on state-specific psychological distress that largely parallel national time series but allowed us to examine whether associations were larger for states where the incident occurred and states with a higher share of Black residents (further described in *SI Appendix*).

**Data Availability.** Anonymized, aggregate data files and syntax have been deposited in the publicly accessible Open Science Framework (<https://osf.io/mnqpi/>).

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