



Protest movements involving limited violence can sometimes be effective: Evidence from the 2020 BlackLivesMatter protests

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The murder of George Floyd ignited one of the largest mass mobilizations in US history, including both nonviolent and violent BlackLivesMatter (BLM) protests in the summer of 2020. Many have since asked: Did the violence within the largely nonviolent movement help or hurt its goals? To answer this question, we used data [R. Kishi, et al., (Report, Armed Conflict Location & Event Data Project, 2021)] about the location of all BLM protests during the summer of 2020 to identify US counties that featured no protests, only nonviolent protests, or both nonviolent and violent protests. We then combined these data with survey data ($n = 494$; study 1), data from the Congressional Cooperative Election Study ($n = 43,924$; study 2A), and data from Project Implicit ($n = 180,480$; study 2B), in order to examine how exposure to (i.e., living in a county with) different types of protest affected both support for the key policy goals of the movement and prejudice toward Black Americans. We found that the 2020 BLM protests were not associated with reduced prejudice among either liberals or conservatives. However, when containing a mix of nonviolence and violence, these protests predicted greater support for BLM's key policy goals among conservatives living in relatively liberal areas. As such, this research suggests that violent, disruptive actions within a broader nonviolent movement may affect those likely to be resistant to the movement. We connect these findings to the notion of disruptive action, which explains why these effects do not materialize in reducing prejudice, but in generating support for important policy goals of the movement.

collective action | nonviolent protest | violent protest | prejudice

During the summer of 2020, the murder of George Floyd at the hands of the police set off one of the largest mass mobilizations in US history, as hundreds of thousands of people protested against systematic racism under the banner of BlackLivesMatter (BLM). While this movement was overwhelmingly nonviolent (1), a small minority of violent protests garnered significant attention and set off discussions both within and outside of the movement about the effectiveness of protests, particularly of violent protests within the larger (nonviolent) movement. This raises a key question at the core of the current paper: Does violent protest hurt or help the broader movement in terms of advancing its goals? Both public discourse (2–4) and the current literature (5–11) offer mixed answers to this question. We aim to shed additional light on this debate by combining survey data with a publicly available dataset of all BLM protest events in the summer of 2020 (1). Thus, we expand on the literature by examining 1) the effect of violence within in a larger nonviolent movement for equality by a minority group (i.e., the effects of local variation in protest tactics within the movement), and 2) the effects on support for the movement's key policy goals along with prejudice toward the broader group.

Effectiveness of Protests in Terms of Policy Support or Prejudice?

The question of whether violence aids or hinders disadvantaged minority groups in their struggle for equality is one that is currently of great interest and debate in psychology (11, 12), sociology (13, 14), and political science (5, 6, 15, 16). As the United States has recently experienced a surge in protests by minority groups calling for greater equality, most notably the BLM movement, many scholars have continued to examine this question specifically in the context of this movement (17–19). The answer to the question of whether violence makes protests more or less effective likely depends largely on what outcome is used to evaluate effectiveness. A number of outcomes are potentially relevant to evaluating movement success, including prejudice toward the minority group, public awareness of the protested inequality, support for the movement's key policy aims, and even systematic structural change. However, so far, many of the large-

Significance

The effects of recent protests for racial equality, particularly when they included violence, are currently of public and academic interest. To better understand these effects, we combine a dataset of all 2020 BlackLivesMatter protests with survey data containing measures of prejudice and support for police reform. Protests were not associated with reductions in prejudice, but were associated with increases in support for police reform. Specifically, a mix of nonviolent and violent protests was associated with an increase in police-reform support among conservatives living in liberal areas. This study highlights the importance of considering multiple measures of protest effectiveness and suggests that mass protest (including when it mixes nonviolence and violence) can be effective at advancing the movement's goals.

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scale studies that examined effects of BLM protests in an ecologically valid context (18, 19) have mainly focused on one outcome: Whites' prejudice and negative attitudes toward African Americans. Perhaps as a result, these studies found that any effects of protests are either very small in size (18, 19) or limited to those already sympathetic to the movement [e.g., liberals (17)]. Little to no effects were found on the prejudice of conservatives, who tend to be more resistant to the goals of BLM (18, 19). This reflects an (at least implicit) assumption that in order to be effective, protests for racial equality need to succeed in reducing prejudice or, put differently, in making the advantaged like the disadvantaged more (20).

Drawing from social psychological understandings of collective action and prejudice reduction, we question this assumption. Research has shown that prejudice reduction is often a result of positive contact with the relevant outgroup or other interventions focused on social harmony and commonality (20, 21), whereas social protests are fundamentally an expression of social conflict that emphasize the differences (namely, inequalities that exist) between the two groups (22). This is why some argue that prejudice reduction and collective action rely on fundamentally different psychological processes (20). Thus, we may not expect protests to be very effective in prejudice reduction, but this may not mean that protests are not effective in other ways. And, indeed, protestors may have other motives and goals for their protests (such as changing policy, raising awareness, building the movement, and expressing their values. [For reviews of protest motives, see Hornsey et al., 2006 (23) and Postmes and Brunsting, 2002 (24).])

Based on sociological and social-psychological theorizing and research (13, 25–27), disruptive protests may be able to generate support for making the policy changes demanded by the movement. Indeed, some theories on large-scale mass protests argue that they derive their power from the ability to disrupt normally cooperative relations and the broader social order. Such disruption can thus incentivize even those who may not agree with the movement goals to make specific policy concessions.

In other words, when protests are disruptive and occur nearby [see Andrews et al., 2016 (28)], they can become something that directly affects the normal life and relations of other people in society [see Shuman et al., 2020 (27)]. For example, Shuman et al. (2021) (27) have shown that being randomly assigned to read about disruptive protests, especially protest that involves nonnormative means (e.g., blocking roads) vs. normative means (e.g., peaceful rallies), created a sense of disruption. This, in turn, explained greater support for the protesters' goals, particularly among those less sympathetic to the protest. Thus, disruptive protests, especially in the area where one lives, can impact the lives of those who may not agree with the movement goals, putting pressure on them to bring the disruption to an end, which could be achieved via conceding to some of their key policy demands [see Chow et al., 2013 (29)]. As such, a key consequence of the occurrence of protests may be their effect on the public's support for specific policies, rather than prejudice reduction toward the group as a whole. Thus, we will assess two outcomes in the current work: support for the policy goals of the movement and prejudice. This enables us to look beyond prejudice and examine whether the BLM protests affected public opinion about specific policies.

How Effective Is Violence?

Expanding the focus of the outcomes of protest from reducing prejudice to increasing public support for the policy goals of

the movement is particularly relevant to the question of the effectiveness of violence, as many of the studies that have shown that violent protests can be effective have focused on policy support or related outcomes, although in other contexts than BLM (5–8, 30). While much contemporary scholarship has emphasized the effectiveness of nonviolence over violence in increasing people's identification with the movement, as people identify more with nonviolent protesters (10) and thus nonviolent movements are more likely to mobilize large numbers of their sympathizers (9), the literature is still divided. Recent work that reanalyzed some of the key data in this scholarship found that the link between nonviolence and movement success was nonexistent for minority groups (16). In addition, there are notable examples that indicate that violence can increase support for a minority group's policy goals and help to achieve specific policy gains (5–8). Indeed, Gould and Klor (2010) (7) found that exposure to moderate (but not high) levels of Palestinian violence increased support for concessions, particularly among right-wing Israelis. Of particular relevance is literature on the positive effects of a small amount of violence as a part of a larger nonviolent movement (sometimes known as a "radical flank") on policy-related outcomes. For example, Tompkins (2015) (8) found that the presence of a violent radical flank was linked to short-term progress in achieving the movement's goals and winning concessions from the government. However, this work was based on a large dataset of many different movements and did not specifically consider protests by minority groups.

Importantly, such potentially positive effects of violent protests within the larger (nonviolent) movement may apply particularly to those resistant to the protest's goals. This is because 1) those who are already sympathetic to the movement are likely already high on support for its policy goals; and 2) the logic of disruptive action is that it produces pressure leading to concessions among those resistant. As indicated earlier, Shuman et al. (2021) (27) experimentally found that disruptive protest was particularly effective for those resistant, as long as the protestors were perceived as having constructive intentions (thus producing "constructive disruption"). Consistent with this, a larger nonviolent movement such as BLM may help balance the disruption produced by the violence with more positive perceptions of the protesting group, as well as provide a more moderate alternative with which to negotiate and make concessions to (31). Thus, we suggest that violent protests within the larger (nonviolent) BLM movement may increase support for specific policies among those resistant to the movement goals (without necessarily reducing prejudice).

The Current Research

The aim of the current research was to examine the psychological impact of BLM protests that occurred in the wake of the murder of George Floyd and the role of violence within them on both 1) increasing support for key policy demands of the movement and 2) reducing prejudice. In particular, we were interested in whether local variations in protest occurrence and tactics (i.e., no protests, nonviolent protests, and nonviolent and violent protests) affected these outcomes. Drawing on the logic of disruptive action (13, 25–27), the amount and type of protests occurring in the area where one lives should more directly affect the amount of disruption people experience, thus affecting their responses to the protest in the context of the broader movement. While these and other outcome measures (e.g., prejudice, support for the movement, support for policy

changes, etc.) have often been used interchangeably as indicators of effectiveness based on the implicit assumption that they would move together, we argue that different effects of protest may be observed on different outcomes. Specifically, we argue that while protests are unlikely to reduce prejudice, they may increase public support for the movement's policy goals and that some amount of violence may even increase this effect.

Furthermore, based on work showing that disruptive protests can be particularly relevant for those more resistant to the movement, we expect to find these effects mainly among conservatives, who are generally resistant to BLM (17). Thus, we hypothesize that for conservatives, violence occurring alongside nonviolent protests implies greater disruption, which should increase support for the movement's policy goals (but not reduce prejudice). Against this backdrop, we do not expect to find clear effects of such protests on liberals. This is because we assume liberals to already be lower in prejudice toward African Americans (32) and generally already supportive of and sympathetic with the cause (17), and hence likely high in support for the movement's policy goals. As a result, they have less room to move on our dependent variables both conceptually and statistically. We therefore focus our results and discussion on conservatives, as this is where we anticipate effects, but we include and present all participants in our analyses.

Overview of Methods, Studies, and Data-Analysis Strategy

Data. In all studies, we combined survey data with a publicly available dataset [see ACLED 2020 (33)] of all BLM protest events in the summer of 2020 based on location. This dataset included an entry for each protest and variables describing that protest, including location (what city it occurred in) and whether it was nonviolent or violent, as coded by Armed Conflict Location & Event Data (ACLED). Peaceful protests were coded as when “demonstrators are engaged in a protest while not engaging in violence or other forms of rioting behaviour,” and violent protests were coded as “violent events where demonstrators or mobs engage in disruptive acts, including but not limited to rock throwing, property destruction, etc. They may target other individuals, property, businesses, other rioting groups” [for more detail, see *SI Appendix* and the ACLED codebook (34)].

By combining these data with survey data, where participants reported the zip code or county in which they lived, we were able to characterize the types of protests that occurred in the area where participants lived. Study 1 used survey data collected by the researchers during the George Floyd protests as an initial test of our hypotheses. However, given this study's small sample and our use of location-based data, we felt it was important to replicate these results using large-scale, nationally representative datasets. Therefore, we sought out large, publicly available datasets that measured support for the BLM movement's policy goals, prejudice toward Black Americans, or both. While we were not able to find a single dataset that measured both of these variables, we found two datasets that included at least one of our key variables. First, the 2020 version of the Cooperative Congressional Election Survey (CCES) measured support for the policy goals of the BLM movement (35) and was conducted between September 29 and November 2, 2020, thus allowing us to measure the impact of the protest movement. In addition, Project Implicit (36) posted their data collected for 2020, which included a feeling thermometer measure of prejudice. Both datasets also included location data per participant,

allowing us to examine the effect of exposure to different types of protests on our dependent variables. Thus, we tested our hypotheses about policy support (study 2A) and prejudice (study 2B) in these two datasets.

Data-Analysis Strategy. We adopted a quasiexperimental design and compared three groups: people living in areas featuring 1) no protest events, 2) only nonviolent protest events, and 3) both nonviolent and violent protest events. As there were only five counties in the entire country that experienced only violent protests, and we had very few participants from these counties (study 1, $n = 0$; study 2A, $n = 41$; study 2B, $n = 79$), we excluded them from analyses (we present analyses including them in *SI Appendix*). We use categorical groups rather than a continuous measure of the amount of violence, as this variable was extremely skewed (*SI Appendix*), and there was no clear value to assign to areas with no protests on a continuous measure. In other words, while the proportion of protests that were violent would be a continuous measure, the value on such a measure for areas with no protests would be undefined (i.e., 0/0), and if we assign a value of zero to these areas, then they would have the same value as areas with only nonviolent protests, despite the qualitative difference between them. However, analyses using this continuous measure (which exclude areas with no protest) are presented in *SI Appendix*.

Given the quasiexperimental design, which lacked random assignment, we adopted a two-pronged approach to estimate the causal effects of protest type. First, we used regression analyses and controlled for potential confounding variables. Second, we used propensity score matching to better estimate the causal effect where possible. Propensity score matching, which balances covariates across treatment groups through creating matched pairs or subgroups, rather than controlling in regression, is generally considered the gold standard for estimating causal effects from observational data (37, 38) [for a detailed discussion of the advantages of propensity scores vs. regression models with covariates, see Schafer and Kang, 2008 (39)]. One meaningful strength is that propensity scores are particularly useful when the distribution of covariates across the treatment groups is very dissimilar, as is the case in our data (*SI Appendix*, Figs. 3–29, 34–52, and 61–78). However, propensity score matching requires additional assumptions to be met (e.g., adequate sample size to conduct matching, region of common support, etc.), which limits its applicability. As a result of lack of common support and sample-size issues (for more details, see *SI Appendix*), we could only use propensity score matching to estimate the causal effect of the nonviolent-only protests vs. violent and nonviolent protests comparison in studies 2A and 2B.

For both approaches, the selection of appropriate controls (i.e., potential confounds) is important. However, the variables measured in each dataset varied: Some studies included additional variables that could affect responses to the George Floyd protests (e.g., exposure to COVID-19, perceptions of the BLM movement, etc.), whereas others only had demographics. In addition, we combined our data with data from the nonprofit Fatal Encounters about the amount of police killings in a given area, as this might explain both the nature of protests and reactions to those protests. Therefore, for the sake of consistency, we present analyses including individual-level and geographic-level demographics (including the number of police killings) in the main text of the article and analyses that include additional relevant covariates in *SI Appendix*.

Study 1 Results

Data and code for analyses can be found at <https://osf.io/pz9c2/>. We first examined the differences on all covariates between areas featuring 1) no protest events ($n = 188$), 2) only nonviolent protest events ($n = 162$), and 3) both nonviolent and violent protest events ($n = 144$); for details, see *SI Appendix*. To test our hypotheses, we examined the interactions between this categorical variable and political ideology on support for the movement's policy goals and on prejudice. Since the independent variable was categorical, it was coded into two dummy variables with the group who lived in areas with violence as the reference category.

Effects on Support for Policies. We ran a linear regression testing the interaction between type of protest and political ideology on support for policies, controlling for the variables described above. There was no interaction between D1 (violent protests vs. no protest) and political ideology; however, the interaction between D2 (violent protests vs. nonviolent protest only) and political ideology was marginally significant (Table 1). Simple slopes analysis revealed that among conservatives, support for the movement's policy goals was significantly stronger where violent protests were present (alongside nonviolent protests), compared to where there were only nonviolent protests ($b = -0.56$, $SE = 0.22$, $t = -2.59$, $P = 0.01$; Fig. 1). Among liberals, there were no such difference ($P = 0.88$). Overall, the simple slope results are in line with our hypotheses, but we treat the overall pattern as suggestive for now, as the overall interaction effect was marginally significant. Although we note that when additional relevant control variables (which were only measured in this study) are added to the model,

the interaction effect is statistically significant (*SI Appendix*). We also note that these additional covariates include measures of news exposure and perceptions of the national BLM movement, so controlling for these variables helps to ensure that the effects observed are due to the exposure to the local protests above and beyond the effects of the general national movement.

No Effects on Prejudice. We ran the same model predicting prejudice as measured by the feeling thermometer toward Black Americans. There were no significant interactions between either D1 (violent protests vs. no protest) or D2 (violent protests vs. nonviolent protest only) and political ideology (*SI Appendix, Tables S1 and S2*), which aligned with our hypothesis that protests would be less effective at reducing prejudice. This held true regardless of whether we used the feeling thermometer toward Black Americans as the outcome or difference scores between feeling thermometer ratings between Black and White Americans and regardless of whether a categorical or continuous measure of violent protest was used (for analysis details, see *SI Appendix*).

Study 1 Discussion

Study 1 found that conservatives who lived in areas where both violent and nonviolent protests occurred were more supportive of the movement's policy goals than those who lived in areas where only nonviolent protests occurred. There was no effect of protest type on conservatives' or liberals' prejudice toward African Americans. This is in line with our reasoning that the disruption produced by some level of violence can pressure conservatives to be more supportive of the movement's policy goals, rather than reducing their prejudice.

Table 1. Study 1 model predicting policy support

Predictors	Policy support		
	Estimates	CI	<i>P</i>
(Intercept)	5.21	4.97 to 5.45	<0.001
Gender D1 (men vs. women)	-0.19	-0.42 to 0.04	0.111
Gender D2 (men vs. other)	1.01	-0.82 to 2.85	0.279
Participant age	-0.14	-0.25 to -0.02	0.019
Participant education	0.21	0.09 to 0.33	0.001
Participant employment	-0.32	-0.80 to 0.17	0.202
ZCTA population	-0.06	-0.20 to 0.08	0.413
ZCTA median age	-0.05	-0.19 to 0.09	0.477
ZCTA median income	-0.10	-0.25 to 0.06	0.219
ZCTA percent female	0.07	-0.05 to 0.20	0.229
ZCTA percent White	0.13	-0.29 to 0.55	0.549
ZCTA percent Black	0.16	-0.18 to 0.50	0.364
ZCTA percent Asian American	0.13	-0.12 to 0.38	0.306
ZCTA percent Hispanic	0.04	-0.15 to 0.23	0.694
ZCTA percent race other	0.04	-0.18 to 0.25	0.739
ZCTA Trump vote share	-0.02	-0.18 to 0.14	0.792
ZCTA per capita police killings	-0.03	-0.16 to 0.09	0.597
ZCTA per capita police killings of African Americans	0.04	-0.11 to 0.19	0.596
Protest type D1: Violent vs. no protests	-0.10	-0.43 to 0.23	0.540
Protest type D2: Violent vs. nonviolent only	-0.32	-0.63 to 0.00	0.048
Political ideology	0.56	0.35 to 0.77	<0.001
Protest type D1 × political ideology	0.06	-0.23 to 0.34	0.696
Protest type D2 × political ideology	0.26	-0.02 to 0.55	0.073
Observations	494		
R^2/R^2 adjusted	0.278/0.245		

Bold effects indicate statistical significance at $P < .05$.

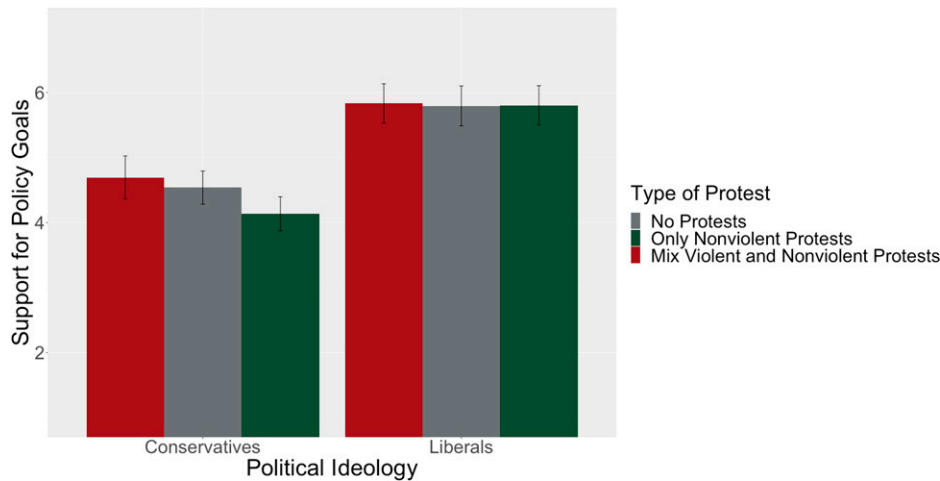


Fig. 1. Interaction between protest type and ideology on support for policy goals. *Note.* Error bars represent 95% CIs.

An important limitation of the study is that almost all of the counties that experienced both nonviolent and violent protests were generally liberal counties (i.e., with low Trump voting; *SI Appendix, Fig. S12*). Although we statistically controlled for this variable, it could present a boundary condition to the interpretation of the findings, especially given that a previous study (28) of exposure to disruptive civil rights protests using a similar methodology found that effects were limited to Whites who lived in liberal areas. So perhaps disruptive protests that include violence only increase policy support among conservatives who live in relatively liberal areas. There are a number of reasons for why this might be the case: First, according to the logic of disruptive action (25, 26), policy concessions reflect a way of pacifying the protestors and ending the disruption. This may only seem a feasible option in liberal areas, where there is some chance of concessions being made to the protestors. Alternatively, in areas where conservatives are in the majority, conservatives may feel less pressure to make concessions to minority group protests, even if these protests are more extreme, as they might feel less threat or pressure from the disruption. Finally, it might be that conservatives living in more liberal areas are more sympathetic to begin with, perhaps because they are less prejudiced in general. We consider these possibilities in the following study by 1) examining the local political context (i.e., vote share for Trump in 2016) as an additional moderator (in studies 2A and 2B), and 2) comparing levels of prejudice among conservatives living in different areas (in study 2B).

Study 2A Results

Data and code for analyses can be found at <https://osf.io/pz9c2/>. We followed the same analysis plan as in study 1. Specifically, we compared three groups: people living in counties where there were 1) no protest events ($n = 5,401$), 2) only nonviolent protest events ($n = 21,270$), and 3) both nonviolent and violent protest events, ($n = 17,253$). We examined the interactions between this categorical variable and political ideology, and county 2016 vote share for Trump, on support for the movement's policy goals.

Regression Analysis. To test our hypothesis, we ran a linear mixed model to account for the nesting of participants within counties and tested the interaction between type of protest,

political ideology, and political context, controlling for all personal and county demographics (for models with additional controls, see *SI Appendix*). The interaction between D1 (violent protests vs. no protest), political ideology, and Trump vote share was significant, and the interaction between D2 (violent protests vs. nonviolent protest only), political ideology, and Trump vote share was significant as well (Table 2). Simple slopes analysis revealed that among conservatives, policy support was significantly higher where violent and nonviolent protests were present compared to where there were only nonviolent protests ($b = -0.14$, $SE = 0.04$, $t = -3.12$, $P < 0.01$) and was also higher compared to where there were no protests ($b = -0.29$, $SE = 0.10$, $t = -2.91$, $P < 0.01$; Fig. 2), but only in relatively liberal areas (low Trump vote share counties). When Trump vote share was high, policy support was significantly lower where violent and nonviolent protests were present compared to where there were only nonviolent protests ($b = 0.14$, $SE = 0.06$, $t = 2.54$, $P = 0.01$), but not compared to where there were no protests ($b = 0.08$, $SE = 0.06$, $t = 1.27$, $P = 0.20$). Among liberals, there were no differences on policy support between nonviolent and violent protest ($P > 0.06$). These findings replicate the findings of study 1. Moreover, the large sample size of study 2A enabled us to use propensity score analysis, which can reduce uncertainty about the presumed causality of the effect.

Propensity Score Analysis. We present detailed information of how we conducted the propensity score matching in *SI Appendix*. Given that some covariates were not perfectly matched after matching (standardized mean differences [SMDs] greater than 0.2), we included these confounders as covariates in the weighted outcome multilevel regression model to further control for them. However, results remain the same whether these or no covariates are included in the final model (*SI Appendix*). Results from the outcome model for estimating the effects of a mix of violent and nonviolent protests (vs. only nonviolent protests) replicated the three-way interaction found in the regression analyses (Table 3). Similar to the earlier analyses, the presence of violent and nonviolent protests increased support for policy concessions relative to only nonviolent protests among conservatives in areas where Trump received low support ($b = 0.13$, $SE = 0.04$, $t = 3.19$, $P < 0.01$; Fig. 3). However, the negative effect of the presence of violent and nonviolent protests was no longer significant when using

Table 2. Study 2A model predicting policy support

Predictors	Policy support		
	Estimates	CI	P
(Intercept)	5.34	5.30 to 5.39	< 0.001
County population	-0.01	-0.05 to 0.03	0.559
County median age	-0.04	-0.06 to -0.02	< 0.001
County median income	-0.01	-0.03 to 0.02	0.571
County percent female	-0.02	-0.04 to -0.00	0.018
County percent White	-0.04	-0.13 to 0.05	0.429
County percent Black	-0.08	-0.15 to -0.01	0.024
County percent Asian American	-0.02	-0.07 to 0.03	0.385
County percent race other	-0.06	-0.11 to -0.02	0.004
County percent Hispanic	-0.01	-0.04 to 0.02	0.448
County per capita police killings	0.02	0.00 to 0.04	0.035
County per capita police killings of African Americans	-0.01	-0.03 to 0.00	0.112
Gender	-0.00	-0.02 to 0.01	0.847
Age	-0.21	-0.23 to -0.19	< 0.001
Education	0.15	0.14 to -0.17	< 0.001
Employment	-0.10	-0.12 to -0.08	< 0.001
Protest type D1 (violent and nonviolent protests vs. no protests)	-0.10	-0.21 to 0.00	0.059
Protest type D2 (violent and nonviolent protests vs. nonviolent)	-0.01	-0.07 to 0.04	0.621
Trump vote share	-0.12	-0.18 to -0.07	< 0.001
Political ideology	-1.36	-1.40 to -1.33	< 0.001
Protest type d1 x Trump vote share	0.06	-0.02 to 0.15	0.144
Protest type d2 x Trump vote share	0.06	0.00 to 0.12	0.046
Protest type d1 x political ideology	-0.01	-0.10 to 0.09	0.908
Protest type d2 x political ideology	0.02	-0.02 to 0.06	0.343
Trump vote share x political ideology	-0.04	-0.08 to -0.00	0.026
Protest type d1 x Trump vote share x political ideology	0.14	0.06 to 0.22	< 0.001
Protest type d2 x Trump vote share x political ideology	0.10	0.05 to 0.14	< 0.001
Random effects			
σ^2	2.87		
T_{00} countyfips	0.01		
ICC	0.01		
$N_{\text{countyfips}}$	2,541		
Observations	43,891		
Marginal R^2 /conditional R^2	0.413/0.416		

Bold effects indicate statistical significance at $P < .05$

propensity score matching ($b = -0.12$, $SE = 0.06$, $t = -1.83$, $P = 0.07$). There were no effects of violent protests on liberals ($P > 0.10$). In addition, we note that when additional relevant control variables (which were only measured in this study, including various types of news exposure) are added, these results remain

significant. This corroborates that these effects are driven by the exposure to the local protests above and beyond effects of the general national movement.

In sum, these results supported our hypotheses. Exposure to a combination of both nonviolent and violent protests was

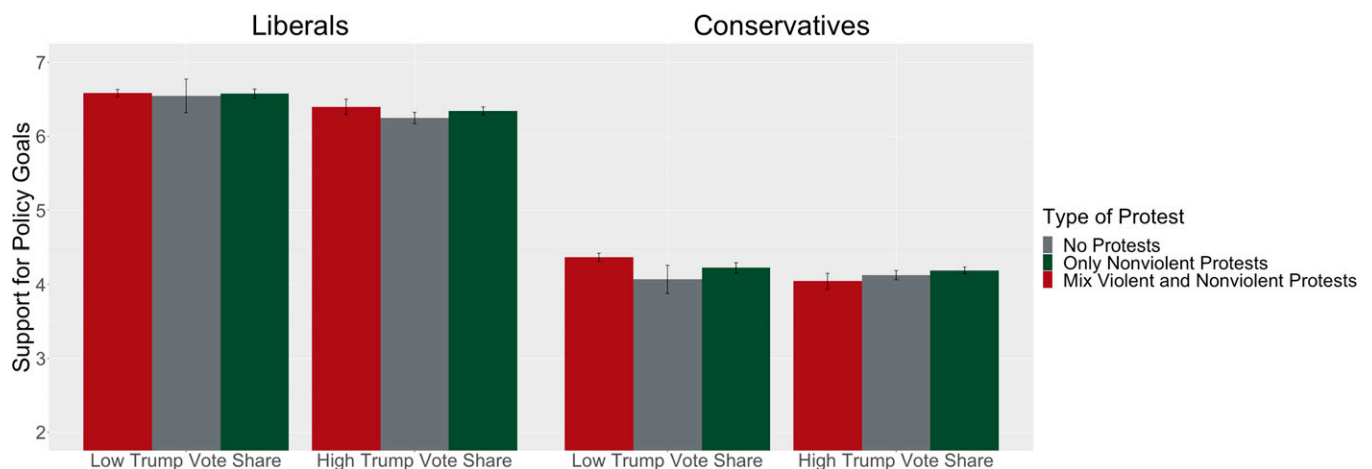


Fig. 2. Interaction between protest type, ideology, and political context on support for policy goals. Note: Error bars represent 95% CIs.

associated with an increase in conservatives' support for policy concessions to the BLM movement compared to exposure to only nonviolent protests or no protests at all, but only in relatively liberal areas. Thus, this study also reveals an important boundary condition of this effect: that it only occurs in generally liberal areas. Rather, in highly conservative areas, there is evidence that violence led to a decrease in support relative to purely nonviolent protests, although the lack of this effect in the propensity score analysis calls into question its causal nature. A potential explanation for this is that conservatives in generally liberal areas are simply less prejudiced and thus more open to the goals of the BLM movement. We examine this possibility and test our hypotheses regarding the effects of different types of protest on prejudice in the next study.

Study 2B Results

All data and code for analyses can be found at <https://osf.io/pz9c2/>. We followed the same analysis plan as in the previous study: We compared three groups: people living in counties where there were 1) no protest events ($n = 12,152$), 2) only nonviolent protest events ($n = 82,842$), and 3) both nonviolent and violent protest events ($n = 85,486$).

Regression Analysis. We ran a linear mixed model to account for the nesting of participants within counties and tested the interaction between type of protest, political ideology, and political context, controlling for all personal and county demographics (for models with additional controls, see *SI Appendix*).

Because we hypothesized that if any type of protest would reduce prejudice, it would be nonviolent protest, nonviolent protest was coded as the reference category in the dummy variables. There was a significant interaction between D1 (nonviolent protests vs. no protest), political ideology, and Trump vote share; and between D2 (violent protests vs. nonviolent protest only), political ideology, and Trump vote share (Table 4). Simple slopes analysis revealed that among conservatives, prejudice was significantly lower where nonviolent protests were present compared to where there were no protests ($b = 0.35$, $SE = 0.07$, $t = 5.04$, $P < 0.01$) and was also lower compared to where there were violent and nonviolent protests ($b = 0.10$, $SE = 0.03$, $t = 3.86$, $P < 0.01$; Fig. 4), but only in relatively liberal areas (low Trump vote share counties). Where Trump vote share was high, there was no effect of protest type on conservatives' levels of prejudice. Among liberals, there was no effect of type of protest on prejudice ($P > 0.43$).

Propensity Score Analysis. We present detailed information of how we conducted the propensity score matching in *SI Appendix*. Given that some covariates were not perfectly matched after matching (SMDs greater than 0.2), we included these confounders as covariates in the weighted outcome regression model to further control for them. However, results remain the same whether these or no covariates are included in the final model (*SI Appendix*). Importantly, and unlike the results of the regression, results from the outcome model estimating the effects of a mix of violent and nonviolent protests on prejudice did not find any effects of protest type or

Table 3. Study 2A propensity score balance model comparing only nonviolent and both nonviolent and violent protests

Predictors	Policy support		
	Estimates	CI	P
(Intercept)	5.42	5.38 to 5.47	<0.001
County total population	-0.01	-0.05 to 0.04	0.728
County median income	-0.00	-0.04 to 0.03	0.884
County percent female	-0.10	-0.13 to -0.06	<0.001
County percent White	0.06	-0.01 to 0.13	0.099
County percent African American	0.03	-0.02 to 0.09	0.245
County percent Asian American	0.02	-0.03 to 0.07	0.420
County percent Hispanic	-0.09	-0.13 to -0.05	<0.001
County per capita police killings	0.03	0.00 to 0.06	0.026
County per capita police killings of African Americans	-0.02	-0.03 to -0.00	0.010
Gender	-0.00	-0.02 to 0.02	0.997
Employment	-0.02	-0.03 to 0.00	0.059
Education	0.17	0.15 to 0.19	<0.001
Protest type: Violent vs. nonviolent only	-0.03	-0.09 to 0.03	0.397
Trump vote share	-0.12	-0.18 to -0.07	<0.001
Political ideology	-1.40	-1.43 to -1.37	<0.001
Protest type × Trump vote share	0.02	-0.04 to 0.08	0.467
Protest type × political ideology	0.00	-0.04 to 0.04	0.940
Trump vote share × political ideology	-0.05	-0.08 to -0.01	0.003
Protest type × Trump vote share × political ideology	0.11	0.07 to 0.15	<0.001
Random effects			
σ^2	2.40		
T_{00} countyfips	0.02		
ICC	0.01		
$N_{\text{countyfips}}$	1,325		
Observations	38,520		
Marginal R^2 /conditional R^2	0.456/0.462		

Bold effects indicate statistical significance at $P < .05$

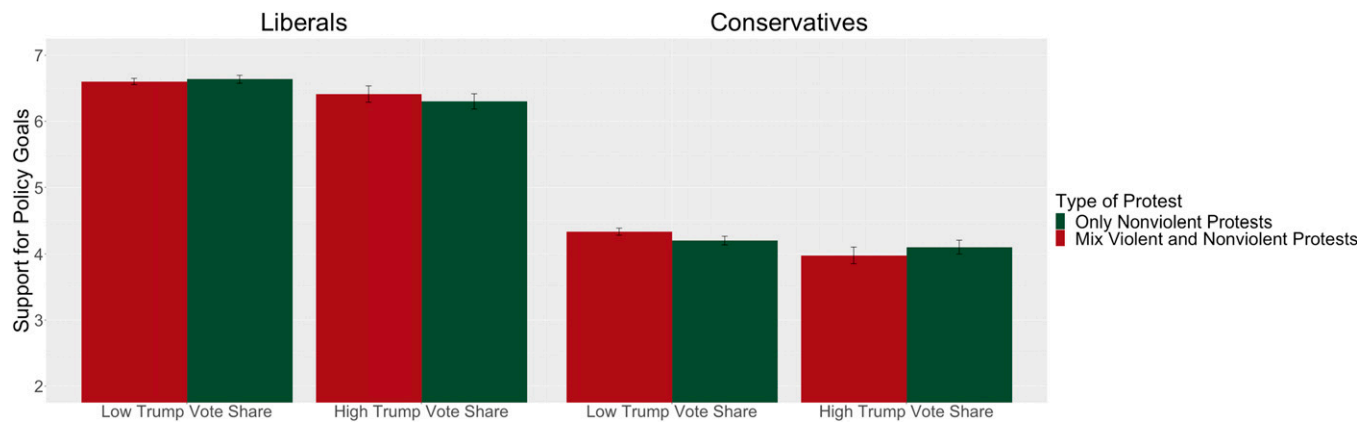


Fig. 3. Interaction between protest type and ideology on support for policy goals using balanced sample. Note: Error bars represent 95% CIs.

interactions. Given the enhanced ability of this approach to estimate the causal effect, this finding suggests that the small difference observed in the regression analyses may not reflect a true causal effect, and hence we do not interpret it as such. Thus, in sum, the only robust effect was that conservatives living in liberal areas with no protests were higher on prejudice than conservatives living in all other types of areas.

General Discussion

Did violent protests within the larger (nonviolent) movement hurt or help the BLM movement during the 2020 George Floyd protests? One answer to this question is that it seemed to have helped by increasing conservatives' (in liberal areas) support for the policies advocated for by the BLM movement. Another answer, however, is that it did not help—as there was no evidence of prejudice reduction and some evidence of a decrease in policy support for conservatives living in conservative areas, although this evidence is weaker, as it was not born out by the propensity score analysis. In this sense, our findings offer a nuanced view on the general tendency in the literature to depict violent protest as an ineffective, if not problematic, strategy for social movements (9, 10, 15) and raises the question of whether local violent protests within a larger (nonviolent) movement can sometimes help movements to successfully earn concessions for specific policy goals. Most of all, our findings—uniquely combining subjective and objective data—suggest that we need to better understand not only whether, but more importantly when and how, violent protests might positively (or negatively) influence those who are not sympathizers.

Specifically, we found across three studies that exposure to violent protests within the larger (nonviolent) movement was associated with increases in conservatives' support for policy concessions to the BLM movement (compared to exposure to only nonviolent protests or no protests), but only in relatively liberal areas. These findings fit nicely with sociological and experimental psychological work (7, 8, 25), suggesting that the disruption produced by protests against the backdrop of nonviolent protests can create pressure for those resistant (in this case, conservatives) to support policy concessions to the movement's goals. Importantly, these findings offer a more contextualized view on the question of whether violence can be an effective social movement tactic, suggesting that it is pivotal to consider both local context and level of exposure to protests. Indeed, conservatives' policy concessions can be viewed as a response to end the disruption caused by the protests in the

area where they live, where there is some chance to make such concessions and appease the protestors. Yet, in areas where conservatives are in the majority, they may feel less threatened by the protests or that they have other means to end the disruption. Alternatively, conservatives in liberal areas may simply have more exposure to liberals, perhaps even liberals who participated in the protest, making them slightly more egalitarian and likely to respond in a conciliatory manner. Future research should further study these psychological mechanisms that can explain when and why conservatives are willing to make concessions [Shuman et al., 2021 (27)]. Indeed, we want to emphasize that our findings should first be better understood scientifically before using them for practical applications.

We note that our findings regarding policy support acquire even more relevance given that we did not find similar effects of protest types on prejudice. This suggests that when we think or talk about the “effectiveness” or “effects” of a protest, it is absolutely pivotal to specify on which outcome variable(s) we are expecting to find such effects, and why. We interpret our findings as suggesting that these two different outcomes reflect different ways to think about how social movements can have psychological effects on public opinion. Reducing prejudice, although important in its own right, may be less likely to occur through conflict- or disruption-based actions and perhaps more likely to occur through well-designed social harmony interventions (such as intergroup contact) than through forms of social conflict (20, 40).

This diversity in effects also suggests a potential framework for making sense of potentially conflicting findings in the literature regarding the effectiveness of nonviolence vs. violence. Many of the studies showing that nonviolence is more effective focus on more attitudinal outcomes, such as identification with the protestors, attitudinal support, and mobilization in support of the movement (10, 41, 42), whereas many of the studies that have found positive effects of violence have used more policy-related outcomes (5–8, 27). Thus, it may be that different types of protest are effective for different goals. Future research should explore these differential effects and how they interact to determine a movement's overall impact. Such general considerations about what goal to target through social protests may, of course, be of prime importance for social movement organizers and practitioners. We need more research that can examine when and how which goals might be realistically achieved through which type of protest.

The studies reported have a number of strengths, but also some limitations. This set of studies combines subjective psychological

Table 4. Study 2B model predicting prejudice

Predictors	Prejudice		
	Estimates	CI	P
(Intercept)	4.47	4.36 to 4.57	< 0.001
County population	0.01	-0.01 to 0.03	0.257
County median age	0.02	-0.01 to 0.03	< 0.001
County median income	0.01	-0.00 to 0.02	0.244
County percent female	-0.02	-0.02 to -0.01	0.001
County percent White	0.12	0.06 to 0.17	< 0.001
County percent Black	0.07	0.03 to 0.11	0.001
County percent Asian American	0.05	0.03 to 0.08	< 0.001
County percent race other	0.03	0.01 to 0.05	0.001
County percent Hispanic	-0.06	-0.08 to -0.05	< 0.001
County per capita police killings	0.01	-0.00 to 0.02	0.212
County per capita police killings of African Americans	-0.00	-0.01 to 0.00	0.183
Gender D1: Male vs. female	0.33	0.28 to 0.38	< 0.001
Gender D1: Male vs. other	-0.50	-0.55 to -0.45	< 0.001
Age	0.04	0.03 to 0.05	< 0.001
Education	0.05	0.04 to 0.05	< 0.001
Employment	-0.00	-0.01 to 0.01	0.943
Feeling thermometer White	-1.32	-1.32 to -1.31	< 0.001
Protest type D1 (nonviolent vs. no protests)	0.88	0.53 to 1.23	< 0.001
Protest type D2 (nonviolent vs. violent and nonviolent protests)	0.28	0.14 to 0.43	< 0.001
Trump vote share	-0.01	-0.21 to 0.18	0.903
Political ideology	-0.13	-0.15 to -0.11	< 0.001
Protest type D1 × Trump vote share	-1.24	-1.79 to -0.70	< 0.001
Protest type D2 × Trump vote share	-0.52	-0.84 to -0.20	0.001
Protest type D1 × political ideology	-0.16	-0.24 to -0.09	< 0.001
Protest type D2 × political ideology	-0.04	-0.07 to -0.02	0.001
Trump vote share × political ideology	-0.02	-0.06 to 0.01	0.200
Protest type D1 × Trump vote share × political ideology	0.24	0.12 to 0.35	< 0.001
Protest type D2 × Trump vote share × political ideology	0.08	0.03 to 0.14	0.005
Random effects			
σ^2	1.66		
T_{00} countyfips	0.00		
ICC	0.00		
$N_{\text{countyfips}}$	2,448		
Observations	180,480		
Marginal R^2 /conditional R^2	0.516/0.517		

Bold effects indicate statistical significance at $P < .05$

data and objective data about the location and type of protests to examine the effects of the BLM movement. While our data had extremely high ecological validity, as they included exposure to

protests in the midst of a large-scale nationwide movement, their observational nature makes it impossible to draw unequivocal causal conclusions. However, we make a strong case for causal effects by

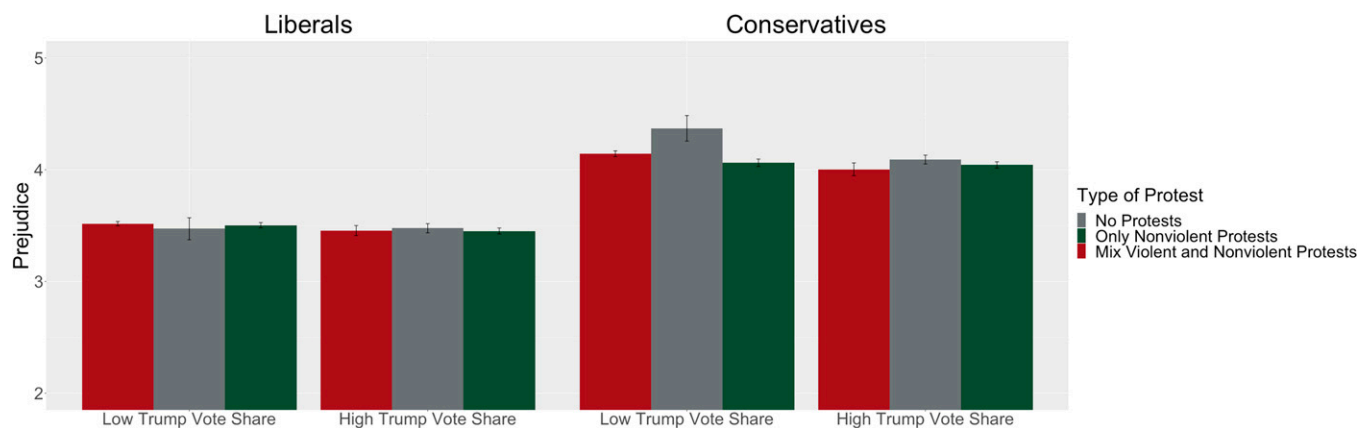


Fig. 4. Interaction between protest type and ideology on prejudice in study 2B. Note: Error bars represent 95% CIs.

controlling for all available confounds in regression analyses and conducting propensity score matching. Although these methods cannot eliminate potential reverse causality (i.e., protestors are aware of conservative support and thus feel licensed to use violence), it is very unlikely theoretically, as past research on violent action suggests that it is driven by strong negative emotions and low efficacy (43, 44). In addition, although our results point to the important role that local political context plays in moderating the effects of disruptive collective action (particularly violence), all these results came from the same protest movement and political context in the United States. Thus, research should also examine these processes in other movements and national contexts. Finally, while we considered both prejudice and policy support and assumed that change in policy would be a desired aim of the protests, there are other potentially relevant outcomes of protest (e.g., public awareness or government reprisals) that might be important to study as well in future studies.

To conclude, this research revealed that the BLM movement can have positive effects even on majority group members who are more resistant to its goals. Following the logic of disruptive action, these positive effects are not in reducing prejudice toward the minority group, but rather in generating support for important policy goals of the movement. In this context, it seems that some amount of violence mixed with a largely non-violent protest movement can, in some cases, increase the effectiveness of its protests by increasing the disruption and thus pressure for policy concessions.

Study 1: Method

Participants and Procedures. Participants were 567 White Americans who completed a survey on Amazon's Mechanical Turk; sample size was determined by budgetary considerations. Sixteen participants were excluded because they failed a question designed to ensure that they were not bots, and 21 participants were excluded because they failed both attention-check measures (these exclusion criteria were preregistered; see https://osf.io/f3h25/?view_only=4bc996743a924be187d832a6f612d617). Additionally, because we had a small percentage of missing cases, we excluded participants with missing data on the variables in our model ($n = 27$) on a listwise basis (45). And an additional 9 participants were excluded because they provided invalid zip codes—preventing us from conducting the location matching. This left a final sample of 494 (291 male, 201 female, and 2 other; $M_{\text{age}} = 39.5$; 47% conservative, 15% moderate, and 38% liberal).

Participants were invited to take part in a study on current events; this study was approved by the Hebrew University Ethics Committee. After completing the informed-consent form (all participants provided consent), participants were told, “Following the killing of George Floyd by a Minneapolis police officer, crowds have filled the streets in cities across the U.S., protesting against police brutality and systemic racism. We are interested in your general perceptions of and reactions to these protests.” They then completed a survey including following measures: We display the measures here organized according to their role in the model; however, they are displayed in the order viewed by the participants in *SI Appendix*:*

*This project was part of a larger project designed to test a number of research questions related to the George Floyd protests. For a full list of measures and a preregistration of the various research questions included in this survey, see https://osf.io/pz9c2/?view_only=6d86d237ece548b9818869b55852d7e9. We note that these hypotheses were not preregistered as at the time the study was conducted; we did not know that we would have access to such detailed data on the location and type of real-world protests.

Measures.

Independent variables. *Protest type in participants' location.* Participants were asked to report the zip codes of the area where they lived, and this was then cross-referenced with a publicly available dataset published by the ACLED project, which included a list of all the protest events that happened during the summer of 2020, with variables coding location (on the town/city level), organizations protesting, violence/nonviolence, date, and a news source reference (<https://acleddata.com/2020/08/31-us-crisis-monitor-releases-full-data-for-summer-2020/>). We first cleaned the dataset so that it only included protests related to George Floyd/BLM. Then, based on these data, we calculated the following variables per city: number of protest events, number of nonviolent protest events, and number of violent protest events and used them to categorize cities as experiencing no protests, nonviolent protests, or both nonviolent and violent protests. We then linked these data to participants by connecting participants with the relevant city using participant-provided zip codes.

Political ideology. Political ideology was measured with one item, “Please describe your political ideology using the following scale” with a scale of 1, very conservative; 2, conservative; 3, somewhat conservative; 4, moderate; 5, somewhat liberal; 6, liberal; and 7, very liberal.

Outcome variables. *Support for the movement's policy goals.* Five items measured participants' support for policies advocated for by the protesters based on statements by protesters, movement leaders, and policy outlines (e.g., Campaign Zero): “There should be community oversight of police (i.e., local non-police government committees should decide when there has been police misconduct and determine the consequences)”; “Funding to the police should be reduced”; Funds that used to be directed to the police should be used to invest in social services (e.g., for mental health, domestic violence and homelessness, etc.) for disadvantaged communities”; “There should be large scale criminal justice reform to address racial inequalities in the justice system”; and “The government needs to invest more in closing racial gaps in education, employment, housing, and other areas” ($\alpha = 0.84$).

Prejudice. We measured prejudice with a simple feeling thermometer, which asked participants to “Please rate your feelings towards people in the following groups from 0 (cold) to 100 (warm)”; the list of groups included White and Black Americans. We used participants' ratings of Black Americans (while controlling for their ratings of White Americans) as a measure of prejudice. The measure was reverse-scored, so that higher scores reflected more prejudice (rather than warmer feelings).

Control variables. *Person-level demographics.* Participants completed a brief demographic questionnaire. Items included gender, age, education, employment, and political ideology.

Area-level control variables. Given that the demographics of a certain area may affect the number of protests and the reactions of people who live there to such protests, we wanted to control for area-level demographics in addition to the individual demographics of our participants. Therefore, we downloaded demographic data from the 2019 American Communities Survey using the *tidycensus* package in R. We used this variable to calculate the total population, percent female, percent White, percent African American, percent Hispanic, percent Asian American, percent other race, median age, and median income for the zip code of each participant. In addition, we obtained election data from the Harvard Dataverse [MIT Election Data and Science Lab, 2018 (46)] and used these to determine the vote share for Trump in each zip code where our participants

lived. Finally, we used publicly available data collected by the nonprofit Fatal Encounters on police killings to calculate the per capita number of police killings and per capita police killings of African Americans per zip code.

Study 2A: Method

Participants and Procedures. The 2020 CCES involved 60 teams, yielding a Common Content sample of 61,000 participants. Each research team purchased a 1,000-person national sample survey, conducted by YouGov of Redwood City, CA. The questionnaire was in the field from September 29 to November 2. For each survey of 1,000 persons, half of the questionnaire was developed and controlled entirely by each individual research team, and half of the questionnaire was devoted to Common Content. The Common Content consists of the questions common to all team modules and has a sample size equal to the total sample size of all team modules combined.

Because we were interested in protests' effects on the advantaged group, we selected from this dataset to include only White participants; this left a final sample of 43,924 (43.8% men and 56.1% women; $M_{\text{age}} = 51.3$; 31.3% conservative, 36.3% moderate/don't know, and 32.4% liberal).

Measures.

Independent variables. *Protest type in participant's location.* We used the same ACLED dataset; however, in this dataset, participants did not report their zip codes, but rather only their county Federal Information Processing System (FIPS) codes. So, we recoded the ACLED dataset to be at the county level; all analyses are now performed at the county level.[†]

Political ideology. Political ideology was measured with one item: "In general, how would you describe your own political viewpoint?" with a scale of 1, very liberal; 2, liberal; 3, moderate; 4, conservative; 5, very conservative; and 6, not sure. Rather, than exclude those who answered "not sure," we combined them with the category of moderate. Results showed that 31.56% were liberal (1 or 2), 34.67% were moderate, and 33.77% were conservative (4 or 5). In all analyses, we used the continuous measure and simply evaluated the simple slopes at 2 (liberal) and 4 (conservative).

Political context. Political context was operationalized as the vote share for Trump in 2016 per county; data were collected from the Harvard Dataverse (46).

Outcome variable. *Support for the movement's policy goals.* Eight items measured participants' support for policies advocated for by the movement, e.g., "Eliminate mandatory minimum sentences for non-violent drug offenders"; "Decrease the number of police on the street by 10 percent, and increase funding for other public services"; and "Ban the use of choke holds by police." Participants were asked to indicate whether or not they supported each proposal on a binary scale. Thus, we calculated total number of the movement's policy goals that they supported as a measure of movement policy support.

Control variables. *Person-level demographics.* Participants reported both their gender and age. Education was measured with the item "What is the highest level of education you have completed?" on the scale 1, did not graduate from high school; 2, high school graduate; 3, some college, but no degree (yet); 4, 2-y college degree; 5, 4-y college degree; and 6, postgraduate degree (MA, MBA, MD, JD, PhD, etc.). Employment was

measured with one item with many response options (*SI Appendix*); for ease of analysis, it was recoded into a variable that indicated if a person was employed or not.

Area-level control variables. We controlled for the exact same variables, but just at the county level rather than the zip code tabulation area (ZCTA) level.

Study 2B: Method

Participants and Procedures. Participants were visitors to the Project Implicit website (<https://implicit.harvard.edu>) who chose to complete the "Race Implicit Association Test (IAT)" between August 29, 2020 (after the last protest in the ACLED dataset), and December 31, 2020. We did not include participants who completed the IAT during the protests, as the type of protests that occurred in the county where they lived was changing on a day-to-day basis as the protests unfolded. We also did not have data from this period in the other datasets, so we wanted to make this comparable to those data. Participants completed the IAT, explicit attitude measures, and a few demographic questions. Only White participants who lived in the United States and who had location data were included in the final sample, which included 180,480 participants (34% men and 65% women, 1% other; $M_{\text{age}} = 34.5$; 24.1% conservative, 18.1% moderate, and 57.8% liberal).

Independent variables. *Protest type in participant's location.* For protest type in participant's location, we used the same ACLED dataset; however, in the Project Implicit dataset, participants' location was also coded according to their county FIPS codes, so all protest exposure type was determined at the county level.

Political ideology. Political Ideology was measured with one item, "Please describe your political ideology using the following scale," with a scale of 1, very conservative; 2, conservative; 3, somewhat conservative; 4, moderate; 5, somewhat liberal; 6, liberal; and 7, very liberal.

Political context. Political context was operationalized as the vote share for Trump in 2016; data were collected from the Harvard Dataverse (46).

Outcome variables. *Prejudice.* Prejudice was measured with a simple feeling thermometer, which asked participants to "Please rate your feelings towards people in the following groups from 0 (very cold) to 10 (very warm)"; the list of groups included White and Black Americans. We used participants' ratings of Black Americans (while controlling for their ratings of White Americans) as a measure of prejudice. We reverse-scored this item so that higher scores reflected more prejudice.

Control variables. *Person-level demographics.* In addition, we aimed to control for all potentially relevant variables that might explain the differences between the groups; since participants were not randomly assigned, we also tried to control for as similar variables as possible to the prior studies, so difference in control variables could not explain differences in results. Specifically, participants reported both their gender and age. Education was measured with the item "What is the highest level of education you have completed?" on a scale ranging between elementary school and PhD. Employment was measured with one item with many response options (*SI Appendix*); for ease of analysis, it was recoded into a variable that indicated if a person was employed or not. Since these data were collected over a long period following the wave of the protests, we also controlled for time in days.

Area-level control variables. We controlled for the exact same county level variables as in study 2A.

Data Availability. Original data for study 1 have been deposited in the Open Science Framework (<https://osf.io/pz9c2/>) (47). The data regarding the protests, study 2A, and study 2B do not belong to us and were not collected by us. All are

[†]If we go back to the pilot study and reanalyze it at the county level, all the patterns remain the same; however, the interactions were not significant. This is likely due to increased noise, as counties are less specific locations than zip codes.

publicly available, and we have included links in our repository to where they can be downloaded from their original owners, but we could not include them in our own repository.

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