How Americans Evaluate Redistributive vs. Symbolic Racial Justice Policies

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Abstract

Recent debates over how to address racial injustice in the United States often center on two types of policies: redistributive measures that redress material inequities between groups and symbolic reforms that challenge dominant racial narratives. How do citizens evaluate these differing approaches to advancing racial justice? How do recent removals of Confederate symbols shape support for each of these policy types? In a survey of American adults, we find that support for redistributive and symbolic policies is positively correlated across partisan, racial, and regional lines. However, when pressed, respondents express a stronger preference for redistributive measures, often viewing symbolic reforms as insufficient or distracting. In an experimental framework, we find that informing respondents about recent Confederate statue removals does not significantly alter support for either policy type. Looking at qualitative reactions to the treatment, we identify a plausible explanation for this null finding: most respondents see the removals as a fight over history and less directly relevant a broader racial justice policy agenda.

Word count: 4,902

Introduction

In 2015, nine Black members of the Emanuel African Methodist Episcopal Church attending a Bible study were shot and killed in a hate crime in Charleston, South Carolina. In response to public pressure, state lawmakers agreed shortly thereafter to remove the Confederate Flag from the grounds of the legislature. In the years since, the City of Charleston has passed a resolution formally apologizing for the city's role in the slave trade, created a special commission aimed at studying local policies to address systemic racism, and conducted a racial bias audit of the local police department (SCEIRC, 2021; Kayanja, 2023).

Over the past decade, debates have emerged about whether and how to address racial injustice in the United States (Drakulich and Denver, 2022; Eligon and Burch, 2020). The events in South Carolina illustrate that receptive responses to demands for action generally fall into two categories. On the one hand, *symbolic* changes attempt to redefine how historical narratives and racial identities are reflected in the public domain, with the goals of advancing racial inclusivity. For example, the Confederate Flag at the capitol, as well as statues, building and street names associated with various legacies of racism have been removed or replaced. On the other hand, *redistributive* policy reforms have aimed to address inequities in opportunities and outcomes across racial groups. Governments at multiple levels have sought to implement police and criminal justice reforms, address disparities in education, and expand affirmative action programs. These two approaches are neither mutually exclusive nor always co-occurring: institutions have in some cases implemented either symbolic or redistributive reforms, and in others, both. Moreover, while redistributive action may be perceived as more costly, symbolic policies are not synonymous with costless or "easy" solutions.

In this research note, we investigate how the public views these two differing approaches to advancing racial justice. Do they see symbolic and redistributive policies as complementary or substitutable? How do recent Confederate statue removals shape support for further racial

justice reform? In an original survey, we asked nearly 1,000 American adults about their support for a number of different symbolic and redistributive racial justice policies.

We find that support for each policy type is strongly and positively correlated, even within racial and partisan subgroups. Indeed, there is not a large group of citizens who strictly want to see redistributive reforms or who will only tolerate symbolic changes. But when we force respondents to choose between the two policy types, there is a definite preference for redistributive reforms and many believe symbolic changes are a distraction from larger problems.

We then use an experimental intervention to assess how policy preferences change when respondents are informed about the recent wave of Confederate statue and placename removals across the United States. While most respondents were unaware of the scale of these symbolic changes, we observe no significant effect of exposure to this information on preferences for other racial justice policies. There are also no detectable heterogeneous responses across racial or partisan subgroups. Using respondents' qualitative reactions to the treatment, we highlight a plausible explanation for the null results: few citizens see the symbolic changes as connected to a broader racial justice project and many more view them as unnecessary or contentious. For most respondents, Confederate symbols are associated with a debate over history and the past, not contemporary racial justice. Taken together, our findings suggest that information about symbolic reforms alone may have limited influence on public support for more substantive policy change going forward.

How Citizens Evaluate Racial Justice Policies

After the murder of George Floyd in 2020 and the large-scale removal of symbols with racist legacies around the country, headlines in some prominent national newspapers posed questions like "statues vs. systemic change: how much of a difference does tearing down monuments really make?" (Moore, 2020). Others, by contrast, have argued that symbolic

changes are a necessary step toward advancing racial equality (Leary and Moore, 2021). These different framings suggest a trade-off between symbolic and redistributive reforms, with some people arguing that only one type of reform has meaningful impact and others arguing that both are needed. What are the opinions of everyday citizens on these debates? How does the public evaluate these two different types of racial justice policies?

On the one hand, citizens may support or oppose symbolic and redistributive racial justice policies in tandem, with the two types of policies being viewed in complementary terms. Rahnama (2025), for example, finds that Confederate statue removals in the aftermath of the Charleston massacre in 2015 triggered an increase in support for redistributive policies like affirmative action in areas nearby the symbol removals. One interpretation of these results is that citizens see them as jointly necessary to advance racial justice.

On the other hand, citizens may differentially oppose and support symbolic and redistributive policies, viewing them as substitutes or alternatives to one another. For example, some citizens may believe that symbolic changes do not make a difference, or may even serve as a distraction, when compared to more structural, redistributive racial justice reforms. This concern informs a view of the two policy types as involving a trade-off. For example, Joshua Mannery, a student activist who campaigned to remove a Confederate statue at the University of Mississippi, argues that "in a perfect world, you could do both ... you could address the symbolic representations of the worst parts of our history, and address the fundamental inequities ... for me, I would sacrifice one or two symbolic wins if I got six to eight fundamental wins" (Adams, 2020).

Existing research has examined the factors that explain when each type of policy change occurs, as well as their attitudinal consequences (Benjamin et al., 2020; Johnson et al., 2019; Rigby and Griffie, 2024; Christiani et al., 2024; Tallent et al., 2024), but this literature largely looks at each policy type separately. No existing survey research has systematically evaluated public opinion toward the two types of racial justice policy in tandem (see Appendix A for a review). By contrast, our study uses a novel survey instrument to assess citizen preferences

for a range of policy items and views on how each policy type contributes to a broader racial justice project.

Data on Racial Justice Policy Preferences

In August 2023, we fielded an online survey with 985 participants through Centiment. The sample includes individuals who are (i) 18 years of age or older and (ii) are citizens and residents of the United States. It is broadly representative of the American population in terms of gender, age, race, region, education, and partisanship (see Appendix B.1).

After a demographic questionnaire, we asked respondents about their support for a number of different policies on a zero to ten scale, ranging from "really dislike" to "really like." These policies include three symbolic and three redistributive policy options. The set of policies, shown in Table 1, were chosen based on a pre-test survey we ran on Amazon MTurk (see Appendix C.8 for details). Within each policy category, we take respondents' average rating as an indication of their support for symbolic and redistributive policies. If a respondent did not provide a response for a given item, we took the average of the non-missing items.

Note that the policies in Table 1 do not reference Confederate statue removals. This approach allows us to focus on symbolic racial justice reforms that (i) have been less salient in popular discourse and (ii) are *additive* in nature, rather than about removing something. However, we did separately ask about support for banning Confederate symbol removals, which we return to below.

Table 1: Descriptive statistics on symbolic and redistributive policy items

Policy category	**Specific policy**	**Avg. Support (0 to 10)**	**% who rated the policy ≥8/10**	**% who rated the policy ≤2/10**	**% who most preferred the policy within category**
Symbolic	Install a national memorial recognizing victims of lynchings.	5.6	32%	23%	15%
	Organize an annual Juneteenth celebration to commemorate the emancipation of enslaved African Americans.	5.5	31%	22%	16%
	Establish a grant program for public school field trips to the African American History Museum in Washington, D.C.	5.9	34%	17%	23%
Redistributive	Update and expand the government's Diversity, Equity, and Inclusion (DEI) Initiative for hiring underrepresented racial groups in government employment.	5.7	33%	19%	19%
	Direct the government planning agency to review and recommend new zoning rules to address racial segregation in housing.	5.8	33%	17%	22%
	Reallocate police budget funds to support services for minority communities that have traditionally been targets of police violence.	5.4	30%	23%	17%

We also asked respondents about their broader views on racial justice in America, including how much progress they believe has been made, whether they see redistributive or symbolic changes as more pressing, and whether symbolic reforms are a distraction. Finally, the survey included an experimental component before these attitudinal and policy support items which we discuss in detail below. For the descriptive analyses in the next section, we focus on responses in the control group, which did not receive any stimuli before offering their policy preferences.

Support for Different Types of Racial Justice Policies

In this section, we investigate how citizens evaluate the different symbolic and redistributive policies in our survey and the extent to which they perceive these policies as complements or substitutes. In Figure 1, we begin by taking the average score across policy categories and plot them against each other, distinguishing by respondents' partisanship, race, and racial resentment (see Appendices C.5 and C.9 for details on these measures). Prior research emphasizes stark differences in support for policies to advance racial justice across these variables (Britt et al., 2020; Cooper et al., 2021; Doherty et al., 2021).

Overall, preferences for redistributive and symbolic racial justice policies are tightly correlated, at around $r = 0.79$. Those who support (oppose) policies that seek to address material inequities across racial groups also support (oppose) policies that would reimagine how race and history are represented in American society. Despite differences in *levels* of support for racial justice policies, these patterns are consistent across partisan and racial groups. The top row shows, for example, that Democrats tend to support both policy types more than Republicans. Yet for both of these partisan groups, as well as Independents, most respondents fall into the lower left or upper right quadrants of each plot. That is, very few respondents strictly prefer one policy type over the other when asked to rate them individually. In fact, just 14 percent of respondents have an average score above 5 on one

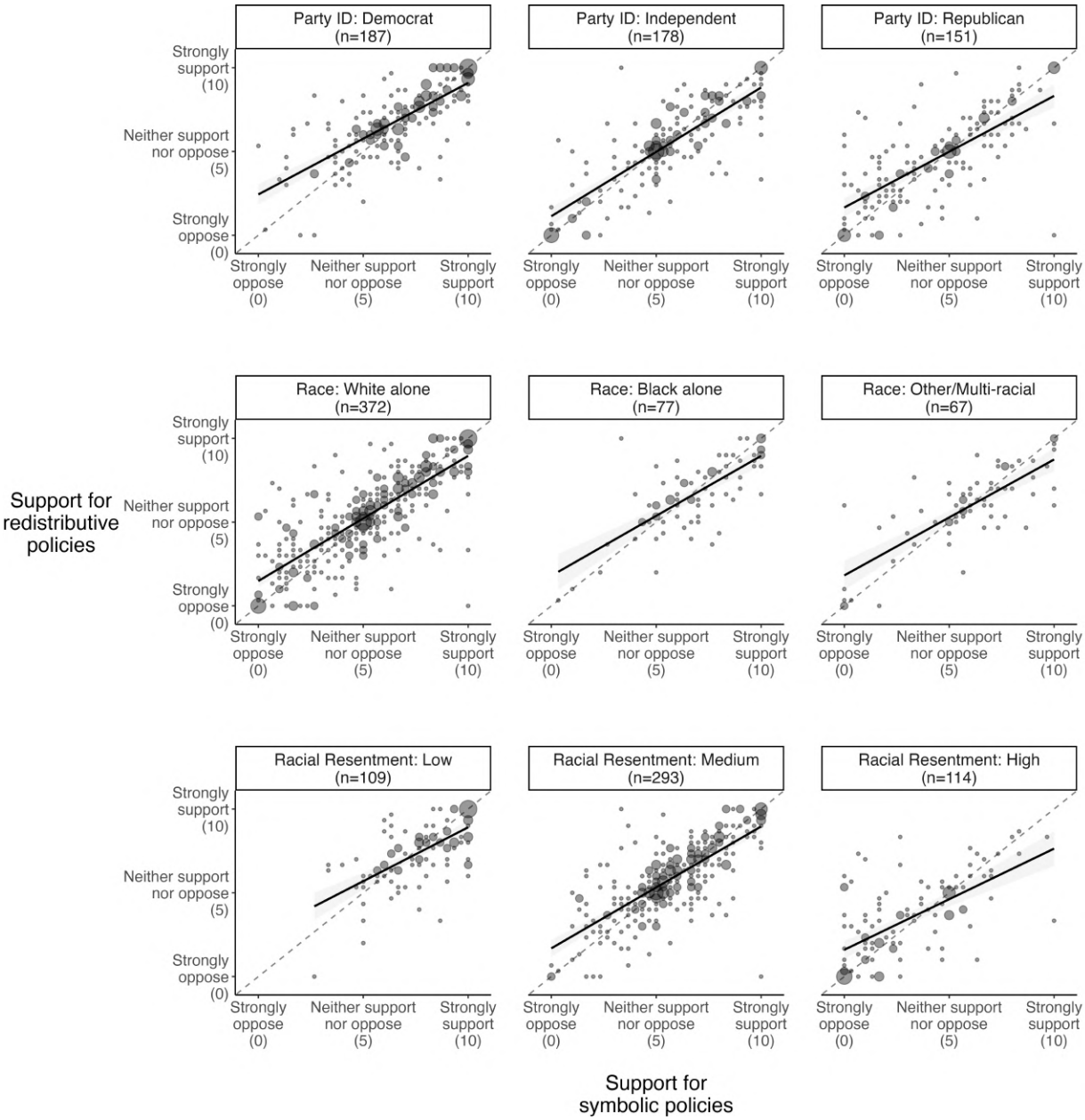

Figure 1: Support for symbolic and redistributive racial justice policies by partisanship, race, and racial resentment

Plots show the relationship between support for symbolic and redistributive racial justice policies based on respondents' average score across three policy proposals in each category on a 0 to 10 scale. Points are scaled by the number of respondents located at each coordinate.

policy type (i.e. "support" that policy) while also having an average score below 5 on the other policy type (i.e. "oppose" it). These results also hold when comparing White and Black Americans, or looking at those with higher versus lower levels of resentment toward Black Americans. These results clarify that there is not a large group of Democrats or Black Americans who strictly want to see redistributive reforms, or conversely a set of Republicans or racially resentful citizens who will only tolerate symbolic changes.

The correlations in Figure 1 show that citizens who support redistributive policies also support symbolic policies, and vice versa. Yet these patterns do not necessarily imply that respondents value symbolic and redistributive reforms equally. In Figure 2, we report on whether respondents prefer symbolic, redistributive or neither type of reform when they are forced to only choose one to address racial injustice (upper panel). In the lower panel, we summarize the extent to which respondents believe that Confederate statue removals are distracting attention away from larger issues of racial justice. For this analysis, we split the sample into those who believe that there is "still a lot more to do" versus "enough" or "too much" has been done on racial justice.

The patterns in this plot present a more nuanced picture. First, among those who believe there is still more to do on racial justice, there is a clear preference for redistributive reforms. Over half prefer policies like affirmative action, compared with less than a quarter who prefer symbolic acts like new holidays. Among those who do not believe there is more progress to be made on this issue, the vast majority do not support either policy type.

Second, many respondents, regardless of their views on the need for further progress on racial justice, express concern that statue removals are a distraction. Across both groups in Figure 1, a majority of respondents agreed with this position. Yet even among those that are especially supportive of action on racial justice, less than one in five pushed back against the idea that these changes were a distraction.

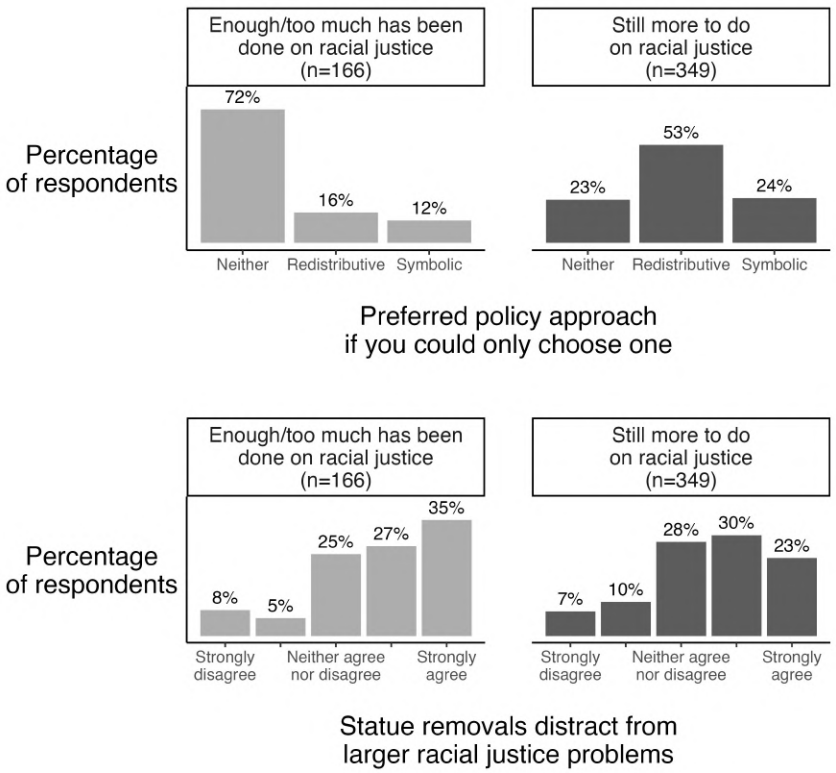

Figure 2: Forced choice policy preferences and perceptions of statue removals as a distraction, by perceived progress on racial justice

Plots summarize the percentage of respondents that would choose symbolic, redistributive, or neither policy type "if you could only choose one [way] ... to address racial injustice in the United States" (upper panel) and that agree or disagree that "removing statues is distracting Americans from larger racial justice problems" (bottom panel). Respondents are divided in columns based on whether they believe there is "still more to do on racial justice" or if "enough" or "too much" has been done on this issue.

Can Symbolic Changes Motivate Support for Additional Reform?

The preceding analyses indicate that measures of support for the two types of racial justice policies are closely related, but there is a relatively higher preference for redistributive reforms and many respondents are concerned that symbolic reforms divert attention from larger goals. Given these competing considerations, what happens when individuals are informed about recent Confederate symbol removals? To test whether and how actual symbolic change may influence support for and opposition to additional reforms, we conducted an experiment. In our survey, half of the respondents were randomly assigned within partisan groups to treated

and control conditions with equal probabilities.

Our intervention informs respondents about recent Confederate symbol removals across the United States. Treated respondents read a multi-page, newspaper article-style vignette describing the number of statues and place names that have been removed since 2015 (see Appendix C.7 for full treatment text). On one page, respondents also engaged with an interactive map created by the authors showing all the locations where Confederate iconography has been removed in recent years. After reading the vignettes and interacting with the map, respondents were asked to offer their opinions on the symbol removals in an open-ended text box. Respondents in the control condition did not receive any information.

After the vignette, respondents in the treatment condition answered the same questions about their support for racial justice policies that were used in the analyses above. If the information mobilized support for additional action (Grose and Peterson, 2020; Rahnama, 2025), we should see higher support for symbolic and redistributive policy preferences in the treated group. If instead it triggered backlash or a sense that the "problem is solved" (Tesler and Sears, 2010), then there should be lower levels of support for racial justice policies among the treated group.

Table A5 reports the average treatment effect (ATE) estimates on each policy support index (see Appendix D.3 for ATE estimates separated by item).[1] These estimates come from an OLS model in which support for each policy type is regressed on a treatment indicator and a series of pre-treatment covariates specified in our pre-analysis plan (see notes to Table A5).

Effect sizes are scaled in terms of control group standard deviations (s.d.) such that, for example, the first estimate implies 4 percent of a s.d. stronger support for symbolic policies in the treated group. The effect on support for redistributive policies is of a similar magnitude in the opposite direction. Even if we consider the most extreme values in the confidence intervals around each estimate ($\sim$0.15 s.d.), these effects do not suggest a particularly large or meaningful change in attitudes. On the original 0 to 10 scale, the coefficients imply a

Table 2: Average Treatment Effects in Informational Experiment

	Policy Support	
	Symbolic Policies	Redistributive Policies
Informational intervention	0.042	−0.037
	(0.050)	(0.053)
Observations	968	971
R^2	0.407	0.374
Controls	✓	✓

Robust standard errors in parentheses. Coefficients are expressed in terms of control group standard deviations. The following covariates are included but not reported: age, region, gender, race and its interaction with racial identity attachment, left-right self-placement, political interest, political knowledge, party identification, American pride, racial resentment, education and household income.

change of just 0.1 points (± 0.3).

In sum, the treatment produced no statistically or substantively significant average change in either symbolic or redistributive policy preferences. In Appendices B.4 and D.2, we provide evidence that the null results are unlikely to be driven by a weak treatment or lack of attentiveness. Indeed, many respondents underestimated the true scale of these removals and thus found the information about Confederate symbol removals surprising.

Another explanation for the null estimates is that the treatment was polarizing. Some respondents may have positively updated on the need for additional policy action, while others updated in a negative direction, thus producing an average effect close to zero. In Appendix D.4, we estimate conditional average treatment effects (CATEs) within subgroups that theoretically might exhibit differing responses to the intervention. We do not identify substantially different effects among Democrats versus Republicans, or White versus Black Americans, or those that were more versus less racially resentful at baseline. To be transparent, our ability to reliably detect meaningful differences in effect sizes across partisan, racial, and other relevant sub-groups is constrained by our sample size and statistical power.

Future research could use more well-powered designs to capture these quantities, although in the next section we highlight qualitative reactions to our intervention that contextualize why there may have been minimal effects, even across partisan or racial groups.

Do Citizens See Confederate Symbol Removals as Relevant to Racial Justice?

In this section, we investigate how the treatment group felt about the Confederate statue and place name removals using their open-ended, written reactions that were collected after the intervention. A qualitative coding of these responses reveals that, overall, 54 percent of respondents who answered the open-ended question were opposed to the removals (see Appendix D.5 for details on the coding scheme). Two consistent themes emerged in the reactions: respondents either (i) do not see the symbol removals as related to broader racial justice issues; or (ii) see them as a distraction from those larger issues.

On the first theme, very few respondents drew a connection between the symbolic changes and debates over contemporary racial justice policy. Among those who opposed the removals, almost two-thirds highlighted concerns about forgetting or "erasing" history. Among the 29 percent of respondents who expressed support for the statue removals, there was less of a common understanding about why these changes were a positive development (see Appendix Table A6). However, even supporters largely saw the removals as a fight over how to remember the past.

To illustrate this focus on history, we ran a quantitative analysis of the text, identifying in Figure 3 the most distinctive words used by supporters versus opponents of the Confederate statue removals. Among opponents, there is a clear concern about history and whether it is being "erased," or "learned" from, or destined be "repeated." This led many to express confusion about the rationale for the removals. One White Southerner said, "I honestly think it's silly to remove our history. Why change the names?" A Republican similarly argued, "those statues are part of history ... why in the world are we taking them down? Do these

people not have anything better to do?" For supporters, the predominant concerns identified in our qualtiative coding and in Figure 3 are around how these symbols "honor," "celebrate" and "represent" a past tied to the Confederacy and slavery.

From this analysis, it is clear that the debates around statues are largely backward-looking, with neither side making strong connections to a contemporary racial justice policy agenda. It is not surprising then, that in our survey data, we find that respondents' support for banning Confederate statue removals is essentially uncorrelated with the other current symbolic and redistributive racial justice policies used in the earlier analyses ($r \approx 0.1$; see Appendix Figure A1).

A second theme in the open-ended responses is that many respondents view statue and place name removals as distracting from larger issues of racial justice. This concern is evident in Figure 2, but it also came through in respondents' own words. For example, a White Republican noted, "the more we talk and virtue signal about a problem [like removing statues], the less we are actually doing to rectify it." A Hispanic Democrat felt that "a lot of unnecessary stuff was changed ... there's bigger problems than this in America and we [are] focusing on the wrong things which is why nothing ever [gets] fixed." In some cases, these concerns bleed into cynicism, even among those who express support for both symbolic and redistributive policies. One White Independent argued that "tearing down and removing Confederate statues will not change anything." A Black Independent similarly said, "I think it's a nice gesture to take the statues down. Do I think it helps anything? No."

In sum, many respondents failed to make a connection between the information about Confederate symbol removals and broader debates over racial justice policy. Either they saw them as a distraction from those debates or as a contentious attack on history, rather than a part of a broader policy agenda. This suggests one plausible explanation for the null results in the previous section: if recent symbol removals are broadly perceived as irrelevant to racial justice, merely informing people about these removals may be insufficient for them to update preferences for related symbolic and redistributive policies.

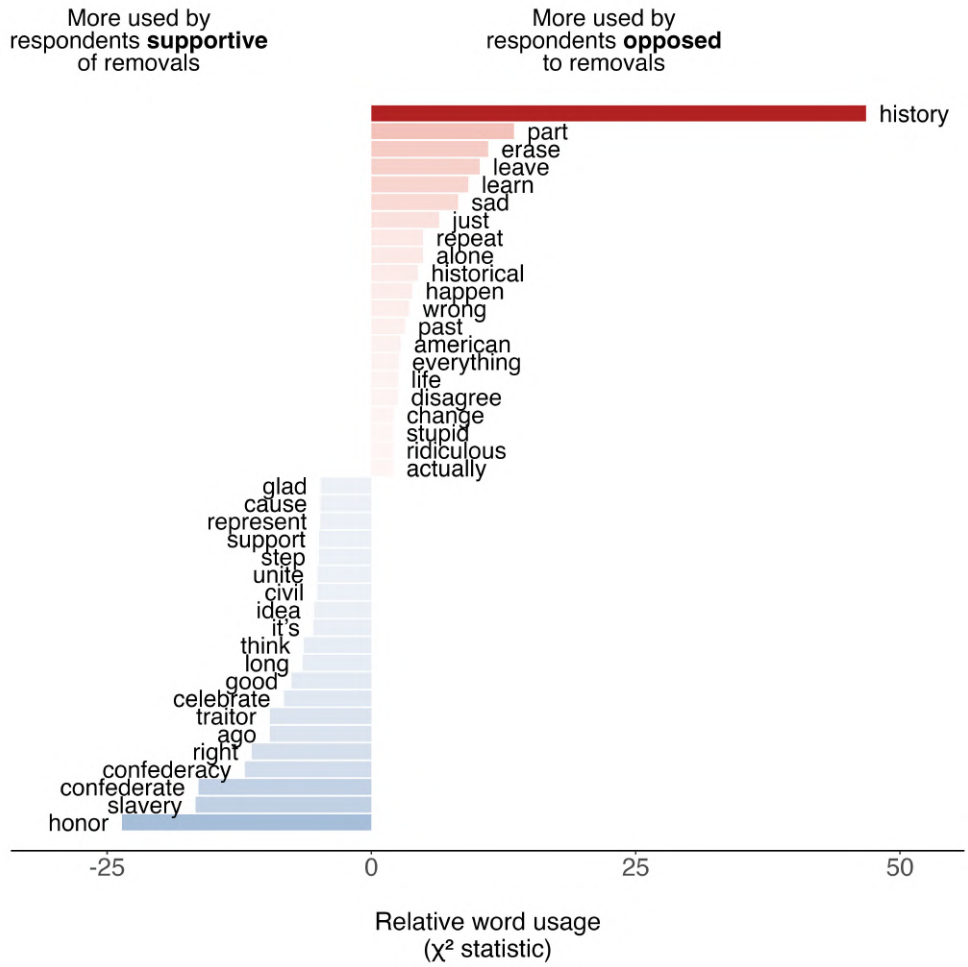

Figure 3: Relative word usage among supporters and opponents of Confederate statue removals

Plot presents keyness scores, which quantify relative differences in word usage across groups, for terms that were most distinctive to those who expressed support (to the left of the plot) or opposition (to the right of the plot) to the Confederate monument removals, based on the hand-coding scheme described in Appendix D.5. Stopwords are removed and all terms are lemmatized.($n = 345$).

Discussion

In this research note, we asked two questions: How do everyday citizens see the relationship between symbolic and redistributive racial justice reforms? How does the recent reckoning with historical symbols influence support for broader racial justice reform in America? We found that respondents often express support for or opposition to symbolic and redistributive

policies in tandem, regardless of their race or partisanship. However, in an experimental setting, exposure to information about actual symbol removals did not shift support for additional racial justice policies. Many citizens feel that symbolic acts are distractions that on their own will not translate into material change. This concern is evident even among those who support express support for advancing racial justice. In respondents' own qualitative reactions to the experimental intervention, we found that many citizens simply do not see a strong connection between the past – at least as it is reflected in historical symbols – and contemporary debates around racial justice.

These findings suggest that information about symbolic changes on its own is unlikely to generate citizen support and momentum for a broader racial justice policy agenda. Yet symbolic changes hold real, intrinsic value for marginalized communities (Doherty et al., 2021; PRRI, 2023). A priority for future research is therefore to design interventions that could overcome the cynicism and contention that often accompany these changes. How can the rationale for symbolic reforms be better articulated to a skeptical public? Can redistributive and symbolic reforms be made more appealing when paired together?

Finally, this paper highlights opportunities for future research on measuring policy preferences. We used rating scales and forced choice questions to gauge respondents' relative preferences for policies of different types. Given the ample discussion in popular discourse about trade-offs between symbolic and redistributive action, ranking, willingness-to-pay, and fixed-budget allocation mechanisms could offer an insightful new ways to quantify citizen attitudes toward specific policy proposals.

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Supplementary Materials

Contents

A. Additional Literature

A.1 Review of symbolic and redistributive questions on other surveys

The 2020 and 2022 Cooperative Election Study (CES) Common Core includes a few attitudinal questions on non-symbolic racial justice policies like policing policies, but nothing on symbolic policies. The 2021 and 2022 General Social Survey (GSS) also includes questions on attitudes toward the police and what respondents would prioritize in terms of criminal justice reform but does not include questions on symbolic reforms or attitudes toward racial symbols. The 2020 American National Election Survey (ANES) includes a feeling thermometer question toward the police and toward Black Lives Matter (as a movement that has symbolic elements) but nothing further. There are some questions in the PRRI between 2020 and 2020 that include questions about what respondents think Confederate symbols mean and what they think statements like "defund the police" mean, as well as questions on evaluating the police. However, these are again rather limited in their scope of symbolic and redistributive racial justice policies and are more about evaluations of institutions and public actors (like "the police") and symbolic meaning than about reforms and specific policies. Ultimately, the 2020 Collaborative Multiracial Post-election Survey (CMPS) does include questions on how individuals would rank as high and low priority difference policies and reforms for the community (including addressing police brutality, stopping voter suppression, increasing funding for public schools, reducing mass incarceration, and increasing the minimum wage) and attitudes toward affirmative action policies. It also includes one symbolically oriented question on whether state governments should officially recognize a heritage month, where white respondents were shown "Confederate History Month" and Black respondents were shown "Black History Month." This format is restrictive since the symbolic question changes by racial/ethnic sample.

B. Data

B.1 Sample representativeness

Table A1 compares our sample's characteristics with those of the general American population. Overall, the sample is fairly representative of the population, although it skews slightly toward White respondents and non-Independent partisans.

Table A1: Sample versus general population characteristics

	Averages	
	Sample	General pop.
Man	0.47	0.49
Age: 18 to 24	0.12	0.11
Age: 25 to 34	0.16	0.18
Age: 35 to 44	0.18	0.16
Age: 45 to 64	0.31	0.32
Age: Over 65	0.22	0.23
Bachelor's degree	0.38	0.38
Region: Midwest	0.19	0.21
Region: Northeast	0.20	0.17
Region: South	0.39	0.38
Region: West	0.22	0.24
Race: White	0.72	0.58
Race: Black	0.15	0.12
Race: Other	0.14	0.30
Party ID: Democrat	0.35	0.27
Party ID: Independent	0.35	0.43
Party ID: Republican	0.30	0.27

General population data from 2020 Census and 2023 Gallup Poll. Note that the percentage in each age category is based on the population aged 18 or older (the sampling frame for the study).

B.2 Missing data

Respondents could skip any question in the survey without repercussions (except for the screening and block randomization questions). The following rates of non-response were observed across pre-treatment covariates:

- Age: 0%
- State: 0.1%
- Gender: 0.1%
- Race: 0%
- Importance of racial identity: 5%
- Ideology: 0.7%
- Political interest: 0.7%

- Political knowledge: 0%
- Party ID: 0%
- American pride: 0.3%
- Racial resentment: 0%
- Education: 0%
- Income: 0%

Since no pre-treatment covariates saw more than 10% of their values missing, we recode all missing entries to the mean (mode for categorical variables) of that variable among non-missing observations in the same randomization block. This imputation only applies to the experimental analyses; in the descriptive analyses, observations are deleted pairwise depending on the variables used.

B.3 Attrition

We test for differential attrition (i.e. missingness on outcomes) across treatment arms using two models. First, we run a linear regression of an attrition indicator (missing on more than one of the main outcome measures) on the treatment indicator, baseline covariates, and randomization block fixed effects. Second, we run the same model again, adding an interaction between the treatment indicator and pre-treatment variables. In the first case, the two-tailed p-value on the treatment indicator is 0.96 ($\beta < 0.001$). In the second case, we use a Wald test on the hypothesis that all the interaction coefficients are zero, which results in a χ^2 p-value of 0.62. Since there is therefore no convincing evidence of treatment-induced attrition, we conduct our analyses by dropping observations with missing outcome measures.

B.4 Respondent Quality

We use several checks on respondent quality. First, 5/990 (0.5%) of respondents completed the survey in less than one-third of the median response time.[2] Another 8 respondents also self-reported that they were not paying close attention to the survey (see SM C.9). Finally, none of the responses were flagged as potential bots by the Qualtrics software. These indicators of quality do not differ in any statistically significant way between treatment groups. In our analyses, we exclude speeder respondents from the analysis, per our pre-analysis plan.

B.5 Properties of Policy Support Indices

The analyses in the main text are based on indices of support for policies corresponding to symbolic and redistributive forms of racial justice. Appendix C.8 provides the full text for each policy proposal. In that section from our pre-analysis plan, we also describe a third category of policies related to "backlash." We do not analyze these policies in the main text, partly because it is unclear whether they constitute a reliable measure of respondents' preferences for policies that push back against racial justice.

In Figure A1, we plot the bivariate correlations between support for each policy item. Within the symbolic and redistributive scales, the items are all fairly strongly correlated with one another at between $r = 0.5$ and 0.7. For the backlash items, the correlations fall between just 0.3 and 0.4. Cronbach's α measures for each scale confirm these differences in internal consistency. For the symbolic and redistributive scales, $\alpha = 0.85$ and 0.84, respectively. For the backlash scale, $\alpha = 0.64$, well below standard thresholds for reliability.

Preferences for backlash policies are also not strongly related to preferences for symbolic and redistributive policies (see three right-most columns in Figure A1). In fact, we would expect these policies to be negatively correlated with pro-racial justice proposals, but the observed correlations are all weakly positive.

One explanation for these patterns is that the wording of the backlash items was more complex and sometimes included double-negatives, which could have introduced measurement error. To simplify the exposition of results in the main text, we focus on the redistributive and symbolic policy types, but, in line with our pre-analysis plan, we report all experimental analyses for the full set of policy support indices (see Appendix D).

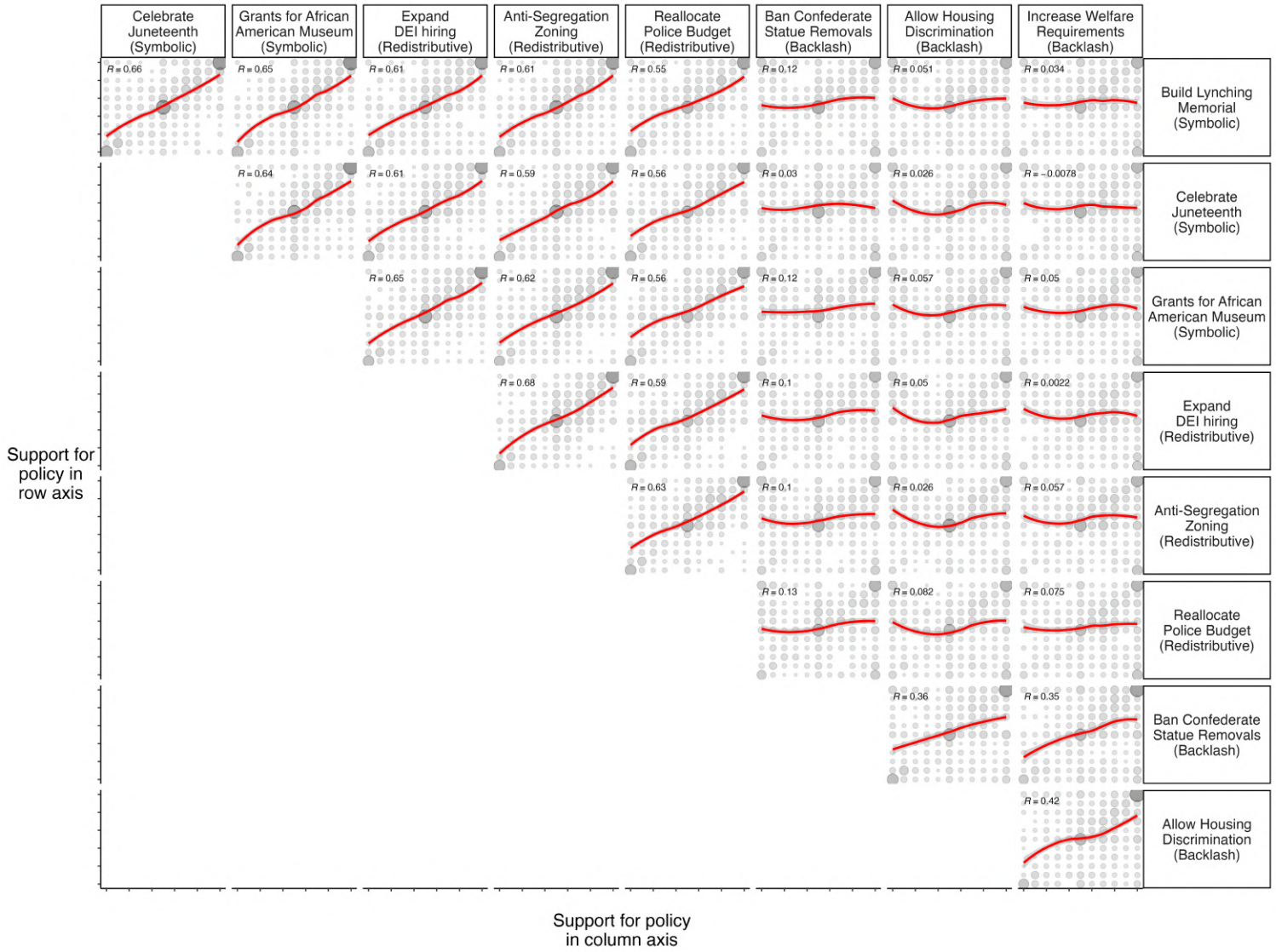


Figure A1: Correlation matrix for racial justice policy support items

C. Experiment Pre-Analysis Plan

The pre-analysis plan for the experimental component of this study was registered with OSF at **REDACTED**. We reproduce that document here.

C.1 Motivation

In recent years, voters in the United States have increasingly called for action to address racial injustice (Drakulich and Denver, 2022; Eligon and Burch, 2020). Sympathetic responses to these demands have primarily fallen into two categories. The first includes *redistributive* policy reforms that aim to address inequitable opportunities and outcomes for racial minorities. For example, governments at multiple levels have sought to implement police and criminal justice reforms, address inequities in education, and expand affirmative action programs. Second, *symbolic* changes have attempted to redefine how historical narratives and racial identities are reflected in the public domain. For example, monuments with legacies rooted in white supremacy have been removed, buildings and streets renamed, and flags taken down. Do these kinds of symbolic reforms mobilize further action on redistributive policies, crowd out support among voters, or evoke a backlash toward further reforms?

C.2 Overview of study design

This project uses a survey experiment to study the effects of information about symbol removals on policy preferences related to racial justice. We focus on Confederate statues in the U.S. South, the removals of which have garnered widespread local and national attention in recent years (Gunter et al., 2016). Respondents in the treatment group will read a brief set of vignettes about Confederate symbol removals in the United States. In the control condition, respondents do not read a vignette. All respondents are then asked to rate, one-by-one, a series of symbolic, redistributive, and backlash policies capturing respondents' absolute preferences across these policy categories. We will also ask respondents a series of questions probing the treatment's effect on theoretical mechanisms of interest.

C.3 Theoretical framework

Our theoretical expectations are premised on the idea that symbolic changes, like statue removals, act as signals to voters about the political environment (Rahnama, 2025). In particular, these events provide information about social norms, political opportunity, outsider perceptions, and group esteem.

First, information about actual removals can shift perceptions of community social norms (Tankard and Paluck, 2016, 2017; Paluck and Shepherd, 2012). Rahnama (2025) finds that localities with Confederate symbol removals experienced a positive shift in racial attitudes toward Blacks relative to those with symbols that were not removed. Publicly successful symbolic changes offer information about how others in a community feel toward intergroup relations, as preferences are revealed in public debate and in the aftermath of the change. After gaining this information, one's own attitudes towards further symbolic and redistributive policy reforms in support of racial justice can also update through a desire to conform.

Political events, like monument removals, also have the potential to provide citizens with knowledge about the viability of pursuing other reforms. A literature on social movements highlights the importance of political opportunities and moments in which social groups can advance their agenda (McAdam et al., 2003). In the context of symbolic changes at the local level, these events could signal the capacity and interest of activist groups, politicians, and bureaucrats to implement other racial justice reforms. For voters who are already sympathetic to this cause, symbolic change can signal that the time is right to pursue goals for additional symbolic and redistributive reforms more aggressively.

Symbolic changes can also provide information about how one's community is viewed by outsiders. Grose and Peterson (2020) show that local elected officials become more supportive of removing Confederate symbols in their community when they are primed to think about how these symbols affect their ability to attract business from other areas. For this reason, voters may infer that symbolic changes occur at least in part to improve their community's reputation to others. If voters do not want their community to be perceived as racist, and understand that symbolic changes are part of that project, they may come to support additional racial justice reforms after symbolic changes occur. Ultimately, information about symbolic change can also activate voters' intrinsic motivations. In particular, for voters who may have an underlying interest or inclination toward racial justice, salient political events like statue removals may heighten racial justice policy priorities to the forefront of their minds and further mobilize support for additional actions.

The preceding arguments suggest that symbolic changes can signal the desirability, viability, and reputational benefits of racial justice reforms. Indeed, these arguments point to the possibility of a *crowding-in* phenomena, whereby mobilization and change on the symbolic dimension of racial justice policies can spill over and mobilize reform on the redistributive dimension. Of course, it is also plausible that there may be a *crowding-out* effect among a subset of the population. In this scenario, while individuals receive and positively interpret the same informational signals, they may also conclude that the "problem is solved" or enough has be done to address racial justice issues with the extant symbolic changes. A similar reasoning took place among individuals who perceived the 2008 election of Barack Obama as a turning point to a "post-racial" America (Tesler and Sears, 2010). If the same logic is driving interpretations of recent symbolic changes, it is likely that voters will not update their policy preferences related to racial justice or only support further reforms on the symbolic dimension.

More concerning is the possibility of a negative reaction or backlash to symbolic reforms. Groups may be protective over their symbolic capital (Manekin et al., 2019) and perceive a threat to their group's esteem as a result of changes that present their group in a negative light (e.g. see Rozenas and Vlasenko (2022); Villamil and Balcells (2021)). Social Identity Theory suggests this is because the value that individuals attach to their group identity tends to be largely symbolic rather than material (Huddy, 2003; Tajfel, 1981). When individuals perceive a threat to their identity, it may increase the salience of their own group attachments or worsen attitudes towards out-groups that are thought to benefit from the symbolic reform. This, in turn, can reduce support for policy preferences aimed at addressing intergroup inequality and increase support for policies that roll back existing reforms.

Finally, a literature on intergroup attitudes suggests that "principled conservatism" may also drive opposition to racial justice reforms (Feldman and Huddy (2005); Sidanius et al.

(1996)). If conservative voters perceive symbolic changes as an excessive government intervention into social issues, they may react negatively toward additional concessions to minorities because of their desire for a limited government. Under this account, opposition to racial justice is driven not by signals about one's own group identity, but about the nature of government involvement in society.

Hypotheses As the preceding discussion highlights, voters may vary in their receptiveness to signals provided by political events. In the context of American debates over racialized symbols, we argue that partisanship represents one of the most important determinants of how voters will interpret symbolic change. In America, racial issues are a highly salient political cleavage and there are significant baseline differences in support for both the removal of Confederate symbols and racial justice reforms between Democrats, Republicans, and Independents (Cooper et al., 2021). We make predictions about the effects of symbolic changes across partisan groups because these identities capture both prior racial attitudes and past exposure to elite cues on racial issues. In Section C.9, we discuss supplementary analyses based on the effects of symbolic changes across varying levels of attitudes towards Blacks, racial identity, and region.

Our first prediction is that, while all voters may perceive shifts in social norms, political opportunity, and outsider perceptions, supporters of the Republican Party, which is more stridently opposed to racial justice initiatives, will be more likely to *also* feel that their identity is being devalued by the removal of symbols or that symbolic changes are an excessive government intervention into societal debates. As a result, being informed about symbolic change will lead these voters to maintain their opposition to any redistributive or symbolic racial justice reforms. Because support for these kinds of policies is already so low among Republicans, we do not expect symbolic changes to affect preferences for moves toward increased racial justice. However, this group is likely to increase in its support for actions that weaken existing racial justice initiatives:

H_1: Priming symbolic changes in America will increase Republican support for backlash policies.

Conversely, Democrats will not perceive the same identity threat and will instead be mobilized into supporting redistributive racial justice reforms because of the other signals that symbolic changes communicate to voters (crowding-in effect).

H_2: Priming symbolic changes in America will increase Democrat support for symbolic policies.

H_3: Priming symbolic changes in America will increase Democrat support for redistributive policies.

We do not make strong predictions about the relative magnitude of these two treatment effects. Because Democrats also are already strongly opposed to backlash policies, we also do not expect any change in support on this policy dimension.

The third group of theoretical interest, political independents, is likely to include a mix of voters (Drakulich and Denver, 2022) who either (a) positively update their policy preferences based on perceptions of social norms and shifting opportunity structures to support additional racial justice reforms or (b) perceive that existing efforts toward racial justice are sufficient or that redistributive reforms are a step too far. The racial policy preferences of

this group of moderates have been under-theorized and we view our project as offering a first look into the extent to which a crowding-in or crowding-out effect dominates their views on these issues. As such, we remain agnostic in our hypotheses regarding the effects of symbolic changes among this group of voters, but note that the following observed results would be consistent with one of three theoretical accounts:

- **Crowding-in**:
  - Priming symbolic changes will increase Independent support for symbolic policies.
  - Priming symbolic changes will increase Independent support for redistributive policies.
- **Weak crowding-out**:
  - Priming symbolic changes will increase Independent support for symbolic policies.
  - Priming symbolic changes will not affect Independent support for redistributive policies.
- **Strong crowding-out**:
  - Priming symbolic changes will not affect Independent support for symbolic policies.
  - Priming symbolic changes will not affect Independent support for redistributive policies.

We do not expect the treatment to affect support for backlash policies among Independents because identity threat considerations are likely less salient for this group. With regards to the two crowding-out effect hypotheses, we remain agnostic about whether a weak or strong effect will dominate.

Finally, we do not offer predictions for the effect of information about symbolic changes among the general population. The heterogeneous relationships hypothesized here will aggregate to average treatment effects in ways that will depend upon the pre-treatment distribution of partisanship in the sample and the treatment effect sizes within each subgroup. That being said, we will report estimates of overall effects (see Section C.9).

C.4 Sample and Recruitment

To participate in the survey, respondents must be (1) 18 or older, (2) reside in the United States, (3) be citizens of the United States, and (4) be proficient in English. We are working with a survey firm, Centiment, which has pre-recruited members of their panel through a number of approaches, including via platforms like LinkedIn and social media marketing. In our survey, Centiment only engages respondents who have been recruited to become members of their survey panel: respondents receive a notification in their portal informing them that a survey is available to them that they qualify for (based on inclusion criteria / target demographic information we have provided the firm). Prior to entering the Qualtrics survey, respondents receive two pieces of information: namely, the estimated time to complete the survey and their expected reward if they do complete it.

C.5 Pre-treatment covariates

Prior to treatment, we collect information on the following covariates:

1. Age [Continuous]
2. State [Categorical: Alabama, Arkansas, ...]
3. Gender [Categorical: Man; Woman or non-binary]
4. Race [Categorical: Asian; Black or African American; Hispanic or Latino; Native American or Alaska Native; Native Hawaiian or Other Pacific Islander; White; Multi-racial; Other]
5. Importance of racial/ethnic identity [Continuous]
6. Left-right self-placement [Continuous]
7. Political interest [Continuous]
8. Political knowledge [Continuous based on average score on non-missing answers to two factual questions]
9. Partisanship [Categorical: Democrat; Republican; Independent or other]
10. American pride [Continuous]
11. Racial resentment index (from (Kinder et al., 1996)) [Continuous based on average score on non-missing agreement with four items]
12. Highest level of education completed [Continuous]
13. Household income [Continuous; n.b. if respondent answers only to income categories version of question, use category mid-point (see survey text)]

All of the covariates described in this section will be used in the balance checks described in Section C.9 and the estimation procedures in Section C.9. When the covariates are used to adjust regression estimates, items (4) and (5) will be interacted with one another.

C.6 Randomization procedure

In order to ensure an efficient estimation of heterogeneous treatment effects, we employ block randomization. Specifically, we sort respondents into one of three categories based on their pre-treatment partisan identification: Democrats, Republicans and Independents/Other. This question is asked at the beginning of the survey after the screening questions and is mandatory. Within each of the three partisan blocks, we randomly assign respondents to the treated and control conditions with equal probabilities.

C.7 Interventions

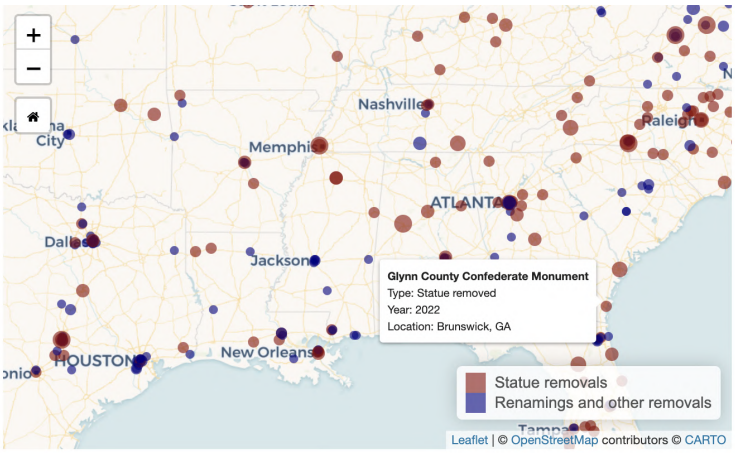
The survey experiment involves one informational treatment that mimics a national news report, presented over a series of pages to respondents. Respondents assigned to the treatment condition will read the following series of texts, accompanied by images (with hyperlinks to their sources), and will be required to stay on each page for a minimum amount of time to ensure they have read and engaged with the content. (We also record how long the participant spends on each page). The first page says the following:

> **America is currently seeing a wave of changes to the symbols that tell its history**

> A statue of Confederate General Alfred Mouton is removed on Saturday, July 17, 2021 in Lafayette, Louisiana. The statue was moved from the front of city hall, where it stood for 99 years.

On the second page, respondents read the following text and will be able to engage with an interactive map of Confederate statue removals:

> Since 2015, nearly **200 statues** honoring Confederate leaders have been removed across the United States.
>
> The interactive map below summarizes the removal of Confederate symbols, including statues, in America. Please zoom in, click on the points, and explore the map to learn more!



To ensure that the interactive map worked, we also ask respondents whether they encountered any problems using the map or if it worked fine. The third page presents the following vignette and images:

> These statue removals are part of a broader process in which commemorations of the Confederacy are being removed from public spaces.

A lawn sign in support of renaming Lee High School in Jacksonville, Florida.

> Over **200** schools, public buildings, roads and other landmarks honoring men like Robert E. Lee and Jefferson Davis have been renamed in the past seven years.

Street names that have been targeted for removal in Concord, North Carolina.

In the final page, respondents engage with the following text and image:

> Just this year, the Department of Defense began the process of renaming all of its Army bases whose names honor Confederate leaders.

U.S. Army bases named after Confederate military leaders that have been or will be renamed this year.

After reading the vignettes, to strengthen the treatment, respondents will be asked in an open-ended text box: "How did you feel reading the story on the previous pages? In 1-3 sentences, please tell us your opinion on the removals of Confederate statues and place names in America." To assess whether the treatment manipulated particular subjective concepts (e.g. identity threat, social norms, etc.), we may engage in an inductive hand-coding exercise of these responses. Afterwards, respondents in the treatment group will also be asked (in a multiple choice 3-point scale) "To what extent did you find the number of statue removals and renamings in the past few years surprising?" Finally, respondents in the treatment group will also be asked "How many statues have been removed since 2015?" This will serve as both an attention check and another means to heighten the salience of the information in the vignette.

C.8 Outcomes

To create our set of policy preferences items, we fielded a pre-test survey with 25 academic and non-academic colleagues familiar with the topic of the study. Respondents were presented with a subset of policy options drawn from a longer list of policies and presented in a randomized order. Focusing specifically on which policies maximized differentiation between types, we selected 3 policies within the symbolic, redistributive, and backlash categories, and one placebo non-racial policy.

In our main survey, after the informational vignette treatment is administered to respondents, we ask a series of questions to measure our primary outcomes on policy preferences (based on the policies identified in the pre-test above) as well as operative mechanisms (secondary outcomes).

Primary outcomes To measure whether exposure to information about symbolic changes mobilizes or deters support for additional racial justice reforms, we will ask respondents their level of support for 10 different policy proposals. These policies include 3 Symbolic-Racial policies, 3 Redistributive-Racial policies, and 3 Backlash-Racial policy options, and 1 Non-racial policy as a placebo check. The first three sets will be summarized into indices by averaging responses within each category. Notably, the backlash policies contain a mix of policies that are symbolic, redistributive, and racially coded in nature.

The set of policies were chosen based on a pre-test survey with a small sample of respondents, where individuals were asked to categorize a wider range of policy proposals into each of the three policy types (details from this pre-test are discussed in the Appendix). We selected the items that were most strongly associated with their respective type and could be easily differentiated from other types. This first set of main outcome variables capture respondents' absolute preferences over these sets of policy options. We begin by explaining the task to respondents:

> In the following questions, we would like you to tell us how much you support or do not support different possible policy ideas.
>
> For each policy idea, set the slider to a number between 0 and 10, where 0 means you really dislike the policy idea and 10 means you really like the idea. The number 5 indicates that you don't particularly like or dislike the policy idea.

On the following pages, respondents will be presented with the 10 policy options in a random ordering:

1. **Symbolic**:

   Policy 1: Install a national memorial recognizing victims of lynchings.

   Policy 2: Organize an annual Juneteenth celebration to commemorate the emancipation of enslaved African Americans.

   Policy 3: Establish a grant program for public school field trips to the African American History Museum in Washington, D.C.

2. **Redistributive**:

Policy 1: Expand affirmative action programs for hiring underrepresented racial groups in government employment.

Policy 2: Direct government planning agencies to review and recommend new zoning rules to address racial segregation in housing.

Policy 3: Reallocate police budget funds to support services for minority communities that have traditionally been targets of police brutality.

3. **Backlash**:

   Policy 1: Pass a resolution that prevents public schools and streets that are currently named after Confederate leaders from being renamed.

   Policy 2: Revoke the government's anti-discrimination protections related to housing and allow landlords to rent whomever they deem fit, regardless of their race or ethnicity.

   Policy 3: Increase work requirements for families receiving welfare through programs like Temporary Aid to Needy Families (TANF).

We also include the following non-racial policy to be used as a supplementary placebo test: "Require elected officials to reside in their district for at least 8 months out of the year." To reduce problems of multiple comparisons, we convert the policy preferences above into respondent-level indices through the following algorithm:

1. Calculate a z-score of support for each policy item by subtracting the control group's average support from the observed support value and then divide by the control group standard deviation.

2. Calculate the respondent-level average of the z-scores within each policy category.

3. Re-standardize the policy support indices again using the control group means and standard deviations.

In the event that a respondent does not provide a response for a given item, we will take the average of the non-missing items (although see Section B.2 for more details on how we address outcome missingness).

We also measure relative preferences for the three types of policies through the following constrained-choice question: "If you could only choose one, which of the following actions do you personally support the most to address racial injustice in the United States?":

- Redistributive reforms, like expanding affirmative action, to improve opportunities for underrepresented minorities.

- Symbolic acts, like removing Confederate statues and flags, to confront our difficult history.

- Neither of the above.

We will then convert this outcome into a linear measure of commitment to costly racial justice reforms and model it as a function of treatment assignment.

Secondary outcomes Our theoretical expectations are based on a number of plausible mechanisms by which learning about symbolic change can trigger changes in policy support:

shifts in perceptions of social norms, political opportunity, outsider views, and intrinsic motivation, as well as group identity threat.

To measure social norms, political opportunity, outsider perceptions, and intrinsic motivations – each of which relate in some form to the *crowding-in* mechanism – we ask respondents how strongly they agree on a five-point scale with the following statements:

- Racial justice is an important priority for me.
- Racial justice is an important priority for my fellow Americans.
- America is currently experiencing an opportune moment to pursue racial justice.
- America needs to pursue racial justice to improve its reputation among other countries.

If our hypothesized mechanisms are driving differences in policy support, we expect that treated (compared to untreated) Democrats and Independents should report stronger agreement with each of these statements.

To measure perceptions about the need for racial justice reforms, which relates to the *crowding-out* mechanism, we ask respondents the following:

- Of the following three views, which comes closest to your own opinion of racial justice reforms in the past few years?
    1. There is still a lot more to do on racial justice reform.
    2. Enough has been done on racial justice reform.
    3. Too much has been done on racial justice reform.

We will convert this measure to a three-point continuous outcome. If we observe that treatment does not increase Independents' support for racial justice policies, this question will help adjudicate whether this is driven by beliefs that sufficient progress has been made or that further reforms are a step too far. Crowding-out implies respondents will perceive there is not as much still to do on racial justice.

The main mechanisms driving H_1, our expectations regarding preferences for backlash policies, are identity threat and principled conservatism. In the existing literature, identity threat refers to various types of threat (Xiao and Van Bavel, 2012), but in this study we focus on the loss of symbolic and cultural capital *to the value of one's social identity*, or a feeling that the values of one's group is being undermined (Branscombe et al., 1999; Stephan et al., 2005). To capture whether these specific feelings of threat are driving changes in the in- and out-group attitudes described in the previous paragraph, we field the following 5-point agree-disagree item:

- I feel that my cultural heritage and history are under attack in America.

To measure principled conservatism, we ask for agreement with the following (reversed) item (based on Feldman and Huddy (2005)):

- The government should be doing more to solve societal problems in America.

In line with our theoretical framework, we should only observe treatment increasing agreement with these two items among Republicans.

Finally, we measure whether respondents see symbolic politics as distracting from the larger challenges of racial justice reform. Specifically we ask about agreement with the following statement:

- Removing statues is distracting Americans from larger racial justice problems.

This mechanism check will be especially informative in the event that we observe positive effects on preferences for redistributive policies but no change, or a negative effect, on symbolic policy preferences among Democrats or Independents. Were that to be the case, greater agreement with this item would clarify that these changes in policy preferences are driven by concerns about distraction from more pressing challenges.

C.9 Estimation

Covariate balance check By virtue of randomization, respondents in the different treatment conditions should be balanced with respect to their individual-level pre-treatment covariates (conditional on their randomization block). To test for whether any observed imbalances are larger than would be expected by chance, we estimate a regression of treatment assignment on the survey-based pre-treatment covariates listed in Section C.5 and the randomization block fixed effects. Using a robust Wald statistic, we will test the null hypothesis that all coefficients on the covariates are equal to zero.

If $p < 0.01$, we will review the random assignment procedure and investigate any possible issues with data cleaning and management. If we can find no errors, we will report the covariate imbalance test described here and proceed under the assumption that the imbalance is due to chance.

ATE estimation Our main analyses focus on preferences for distinct policy types. Section C.8 explained the main dependent variable in our models: the average support for a given policy type (e.g. symbolic). We model this average support outcome using the following OLS equation, estimated separately for each of the three policy categories:

$$y_{ib} = \beta D_i + \mathbf{X}_i\gamma + \text{PartyID}_b + \varepsilon_{ib}$$

where y_i is the measure of how much respondent i likes the particular policy type; D_i is an indicator for whether a respondent was assigned to the informational treatment; $\mathbf{X}_i$ is a vector of pre-treatment covariates described in Section C.5; and PartyID_b is the set of randomization block fixed effects. In the above specification, the average treatment effect is given by β, capturing the effect of treatment on how much respondents like a given policy type.

Effect heterogeneity As outlined in Section C.3, we expect that the experimental intervention will vary in its effects depending on participants' pre-treatment partisanship. We model the policy preferences outcomes using interactions between the treatment indicator and the categorical Party ID variable (which also serves as the randomization block variable)

in the following OLS equation:

$$y_{ib} = \beta_1 D_i + \beta_2 D_i \mathbb{1}\{\text{PartyID}_b = \text{“Democrat”}\} + \beta_3 D_i \mathbb{1}\{\text{PartyID}_b = \text{“Republican”}\} + \mathbf{X}_i\gamma + \text{PartyID}_b + \varepsilon_{ib}$$

where again y_i is the policy feeling thermometer average; D_i is an indicator for whether a respondent was assigned to the informational treatment; PartyID_b is the randomization block category based on pre-treatment partisanship; and $\mathbf{X}_i$ is the vector of pre-treatment covariates described in Section C.5.

This specification produces three estimates of interest:

1. $\beta_1 + \beta_2$: The effect of treatment among Democrats.
2. β_1: The effect of treatment among Independents.
3. $\beta_1 + \beta_3$: The effect of treatment among Republicans.

Our hypotheses imply the following predictions about these estimates:

H_1: $\beta_1 + \beta_3 > 0$ when the dependent variable is support for backlash policies.

H_2: $\beta_1 + \beta_2 > 0$ when the dependent variable is support for symbolic policies.

H_3: $\beta_1 + \beta_2 > 0$ when the dependent variable is support for redistributive policies.

H_4: $\beta_1 > 0$ when the dependent variable is support for symbolic policies (if crowding-in effect dominates; see Section C.3)

H_5: $\beta_1 > 0$ when the dependent variable is support for redistributive policies (if crowding-in effect dominates; see Section C.3).

We will evaluate these hypotheses against null hypotheses of no effect using two-tailed tests with $\alpha = 0.05$.

Supplementary heterogeneity analyses Our main investigation of differential responses to the treatment focuses on effect heterogeneity across partisan identities, which we argue proxy for racial attitudes and prior elite priming. For completeness, we will also investigate effects by prior attitudes toward racial minorities. Specifically, we calculate each respondent's average racial resentment score (ranging from 1 to 5) and then group them into one of three categories:

$$\text{ResentmentIndicator}_i = \begin{cases} \text{Low}, & \text{if AvgResentment}_i \leq 2 \\ \text{Medium}, & \text{if AvgResentment}_i > 2 \text{ and AvgResentment}_i < 4 \\ \text{High}, & \text{otherwise} \end{cases}$$

These specific thresholds were chosen so that “low” resentment individuals somewhat or strongly disagree with racially resentful items on average, whereas “high” resentment individuals express some degree of agreement on average and “medium” scores represent intermediate attitudes.

We then model the policy preferences outcomes using the following equation:

$$\begin{aligned} y_{ib} = &\beta_1 D_i + \beta_2 D_i \mathbb{1}\{\text{ResentmentIndicator}_i = \text{“Low”}\} + \\ &\beta_3 D_i \mathbb{1}\{\text{ResentmentIndicator}_i = \text{“High”}\} + \\ &\mathbf{X}_i\gamma + \text{ResentmentIndicator}_i + \text{PartyID}_b + \varepsilon_{ib} \end{aligned}$$

The estimates from this model will be reported in a similar way as in the analyses from Section C.9. In a related set of analyses, we will also investigate effects by race, region and political interest. We will look at differential responses to treatment among Blacks, whites and other racial groups (i.e. three categories), among those that currently reside in the South versus versus those that do not, and among those with political interest above and below the sample median. We will model the policy preference outcomes using a formula analogous to the one for the racial resentment variable, interacting the treatment indicator with the categorical race and region variables, respectively. These tests are exploratory, so we do not make specific predictions about the patterns we are likely to observe, but we will make sure to note this in any published findings.

Exploratory analyses To assess whether the hypothesized mechanisms are being triggered by the experimental intervention, we specified a number of secondary outcomes in Section C.8. We will model these variables using the same estimating equations and hypothesis testing procedures described in Sections C.9 and C.9. Our exploratory hypotheses for the estimates are as follows:

- $\beta_1 + \beta_2 > 0$ and $\beta_1 > 0$ when the dependent variable is beliefs in other Americans' racial justice priority level.
- $\beta_1 + \beta_2 > 0$ and $\beta_1 > 0$ when the dependent variable is beliefs in the fortuitousness of the current moment for racial justice action.
- $\beta_1 + \beta_2 > 0$ and $\beta_1 > 0$ when the dependent variable is beliefs in the need for racial justice to improve America's reputation.
- $\beta_1 + \beta_2 > 0$ when the dependent variable is the amount still to be done on racial justice.
- $\beta_1 < 0$ when the dependent variable is the amount still to be done on racial justice (if crowding-in effect dominates).
- $\beta_1 + \beta_3 > 0$ when the dependent variable is feelings of identity threat.
- $\beta_1 + \beta_3 < 0$ when the dependent variable is whether the government should be doing more to solve societal problems.
- $\beta_1 + \beta 2 > 0$ and $\beta_1 > 0$ when the dependent variable is whether statue removals are distractions (if the main analyses show $\beta_1 + \beta_2 < 0$ for symbolic policy support).

Missing data, attrition and outliers Respondents are free to skip any question in the survey without repercussions (except for the screening and block randomization questions). If less than 10% of a missing pre-treatment covariate's values are missing, we will recode

those values to the mean (mode for categorical variables) of that covariate among non-missing observations in the same randomization block. If missingness exceeds 10%, we will include an indicator variable for observations that are missing on that covariate and recode the missing values to that variable's minimum value minus one.

We will also test for differential attrition (i.e. missingness on outcomes) across treatment arms using two models. First, we will run a linear regression of an attrition indicator (missing on more than one of the main outcome measures) on the treatment indicator, baseline covariates and randomization block fixed effects. Second, we will run the same model again, adding an interaction between the treatment indicator and pre-treatment variables. In the first case, we will report a two-tailed p-value and in the second, our p-value will come from a Wald test on the hypothesis that all the interaction coefficients are zero. If $p < 0.05$ in either case, we will report extreme value (Manski-type) bounds. Regardless of this condition, we will also report the analyses with missing observations dropped from any applicable analyses.

With regard to outliers, we will not delete or edit especially large or small values.

Attention checks and low-quality responses Our design requires that respondents are attentive in their completion of the survey and reading of the informational treatment. To boost attention, we include one question immediately before the treatment is administered which attempts to strategically foster attention in the subsequent questions, regardless of what the respondents answer (based on Meade and Craig (2012) and Alesina et al. (2022); see Stantcheva 2022):

> Before proceeding to the next part of the survey, we want to ask for your feedback about the responses you have provided so far. It is vital to our study that we only include responses from people who devoted their full attention to this study. Otherwise, years of effort (the researchers' time and the time of other participants) could be wasted.
>
> Your response to this question will not affect in any way the compensation you will receive for taking this survey.
>
> In your honest opinion, should we use your responses, or should we discard your responses since you did not devote your full attention to the questions so far?
>
> - Yes, I have devoted full attention to the questions so far and I think you should use my responses for your study.
> - No, I have not devoted full attention to the questions so far and I think you should not use my responses for your study.

Alesina et al. (2022) report that 99.5% of respondents report "yes," so this item mainly serves to improve answer quality and treatment uptake in the rest of the survey, although we will also re-run our main analyses excluding respondents who answer "no."

Beyond this proactive measure, we also attempt to measure attentiveness to the treatment itself. First, we include a factual manipulation check immediately after treatment that assesses whether treated respondents can recall how many statues have been removed based on the information in the vignette. We also ask control respondents this same item (based on their best guess), except the question is asked after all of the outcome measures to avoid incidental priming. Because this indicator is measured post-treatment, we only use this check to establish whether treated respondents internalized the information. We will check for this

by regressing an indicator for whether respondents gave the correct response on treatment assignment, pre-treatment covariates and randomization block fixed effects. This test, as well as the overall percentage of treated respondents that gave the correct response, will offer one indication for whether treatment was received.

Finally, our survey partner, Centiment, has a general policy of removing low-quality responses based on whether respondents complete the survey in less than a third of the median response time.

C.10 Power analysis

To assess the statistical power of our study, we conduct a simulation-based analysis under the following assumptions:

1. The randomization block fixed effects and observed covariates described in Section 3 will explain 30% of the variability in the untreated potential outcomes. This assumption is generally in line with literature on the determinants of public opinion related to Confederate monuments: Cooper et al (2021) estimate a multinomial logistic regression of individual support for monument removals on many of the same covariates we will adjust for and report a pseudo-R^2 of 0.26; Benjamin et al (Benjamin et al. (2020)) and Evans and Sims (2021) conduct a similar analysis across geographic units and find pseudo-R^2 values between 0.2 and 0.4.

2. The distribution of partisan identification in the sample will be the same as in the 2022 American National Election Study (ANES) Pilot Survey (35% Democrat, 36% Independent, 29% Republican).

3. Responses to the three main outcome measures will be correlated at 0.65. For reference, the four racial resentment items were correlated with one another at the same value on average in the 2020 ANES.

Under these conditions, we simulate potential outcomes data and block randomize respondents into treatment arms within each simulation (see Appendix for the code used in the data generating process and estimation procedure). We investigate power to reject each of the main hypotheses H_1 to H_5 independently as well as simultaneously given a specified effect size (in terms of control group standard deviations). For each effect size, we conduct 5,000 simulations.

Figure A2 summarizes the results. Each cell plots the expected power to reject a given hypothesis and, in the case of the bottom right cell, to reject all of the five hypotheses together. Power varies only slightly across hypotheses because of differences in the expected sample proportions of partisan sub-groups. In general, we are well-powered to detect effects greater than 0.3 standard deviations for each hypothesis by itself. Our power to reject all five hypotheses simultaneously is lower, reaching the standard threshold of 80% when all effect sizes are greater than 0.4.

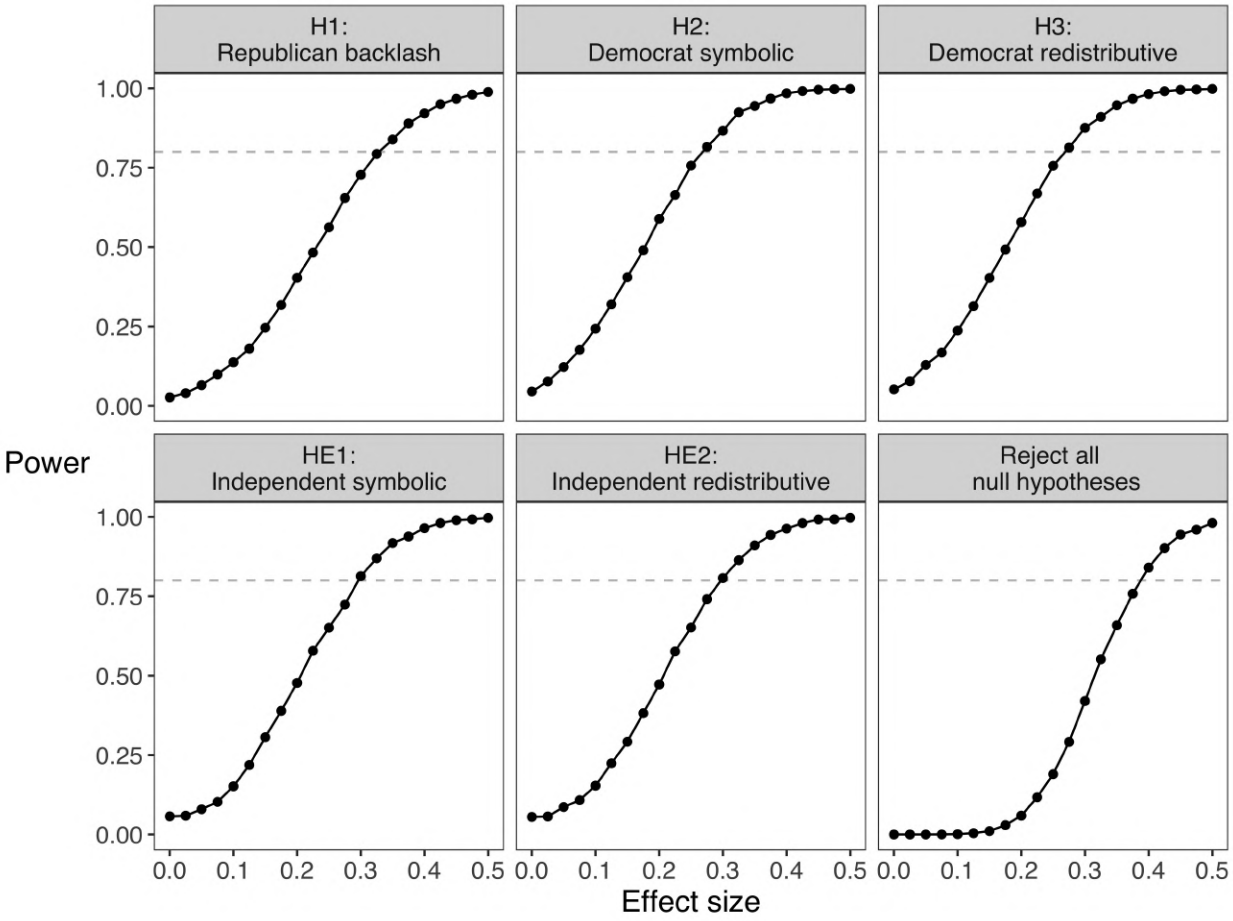


Figure A2: Power analysis

C.11 Deviations from the pre-analysis plan

1. The theoretical framework and hypotheses in our PAP stress the importance of conditional effects among partisan groups. In the main text, we opted to devote less attention to this framing and instead only report the average treatment effects. This choice was made because we are underpowered to detect the relevant conditional treatment effects. We note this in the main text and report the relevant estimates in Appendix D.4.

2. We originally specified to include state of residence as a pre-treatment covariate in our regression models, but the sparsity of this variable created problems estimating HC2 variance-covariance matrices. Instead, we re-code this variable to a four-level region variable (Northeast, Midwest, West, South).

3. We originally specified to include self-reported race/ethnicity as a pre-treatment covariate, but several of our categories received vary few respondents, which again created problems for robust standard error estimation. We instead group respondents into three categories: Black, White and an Other category, including Asian, Hispanic/Latino, Na-

tive American or Alaska Native, Native Hawaiian or Other Pacific Islander, Multi-racial or Other (open specification).

4. We specified one outcome measure based on respondents' preferred type of racial justice policy if they could only choose one type (see Appendix C.8). We originally planned to convert this categorical outcome to a linear measure of commitment to costly racial justice reforms, but in Appendix D.3, we instead retain its categorical structure and model this outcome as a function of treatment assignment using a mulitnomial logistic regression. If we instead estimate the model using OLS and the linear outcome, the ATE is 0.03 ($p = 0.57$).

5. We originally specified to test for heterogeneous treatment effects using separate OLS models in which potential moderators were individually interacted with the treatment indicator (i.e. one model interacting partisanship with treatment, a separate model interacting race with treatment, etc. See Appendix C.9). Instead, we estimate one model interacting the treatment indicator with all pre-treatment covariates to address potential confounders systematically. The results, reported in Appendix D.4, are substantively similar to the estimates from the models specified in the PAP.

D. Experimental Analyses

D.1 Balance check

To confirm successful randomization, we estimate a regression of treatment assignment on pre-treatment covariates and randomization block fixed effects. Figure A3 summarizes the coefficient estimates from this model. None of the estimates are statistically significant and the p-value from a Wald test on the null hypothesis that all coefficients on the covariates are equal to zero is 0.89.

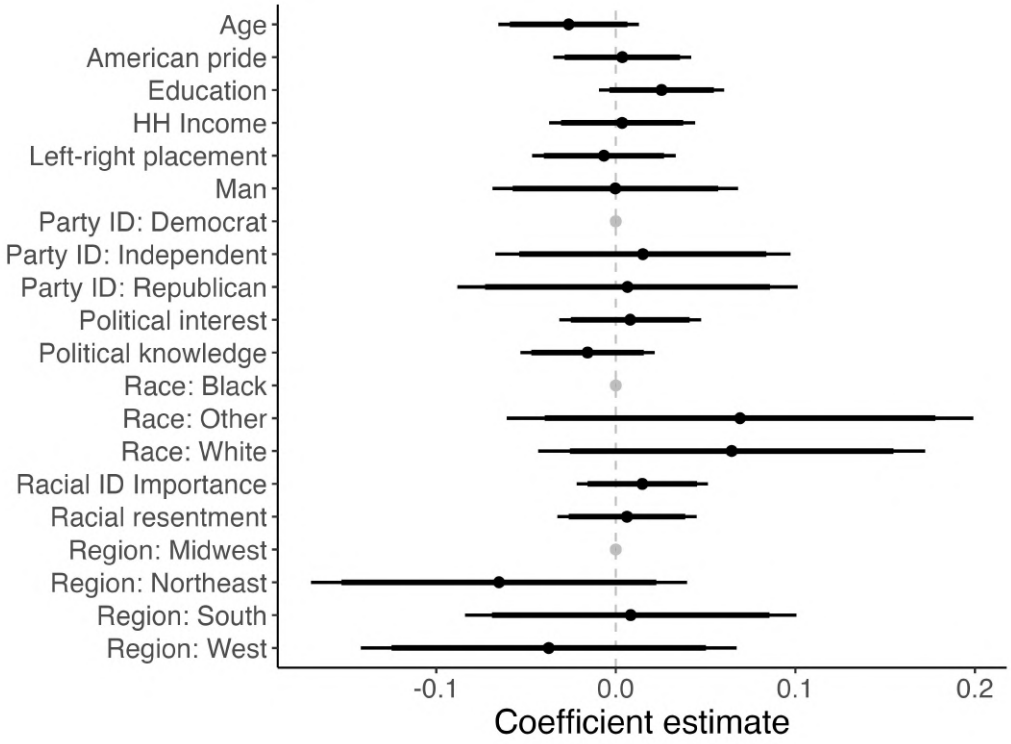


Figure A3: Balance check

Plot presents coefficient estimates from an OLS model regression treatment status on each of the coefficients listed on the y-axis. ($n = 922$)

D.2 Manipulation check

In the treatment vignette, respondents are told that "since 2015, nearly 200 statues honoring Confederate leaders have been removed across the United States." To gauge whether respondents internalized this information, we ask a factual manipulation check question on exactly this number after treatment is administered.

Figure A4 displays the distribution of estimates across the treatment and control conditions. The plot reveals that baseline beliefs − as captured in the control group − are below the true number of removals. Over 44% of control respondents believed that only 50 or 100 statues had been removed in the preceding years and just 11% correctly estimated the actual number of removals. Over a third also opted not to provide a response to this question.

By contrast, in the treated group, just under 60% of respondents were able to correctly recall the number of removals from the vignette. Although this is a relatively low recall rate, the proportions in Figure A4, confirmed by a supplementary regression model, indicate that the treatment did increase the probability of correctly identifying the number of removals by roughly 48 percentage points.

We also asked treated respondents whether they found the information in the vignette surprising. The distribution of their responses was as follows, indicating that many found it novel:

- Not at all surprising: 27%

- A little surprising: 37%
- Very surprising: 36%

Finally, we recorded how long the participant spends on each page. Respondents typically spent about 1.5 to 2 minutes engaging with the treatment content, and 50-60 seconds on the interactive map, in particular.

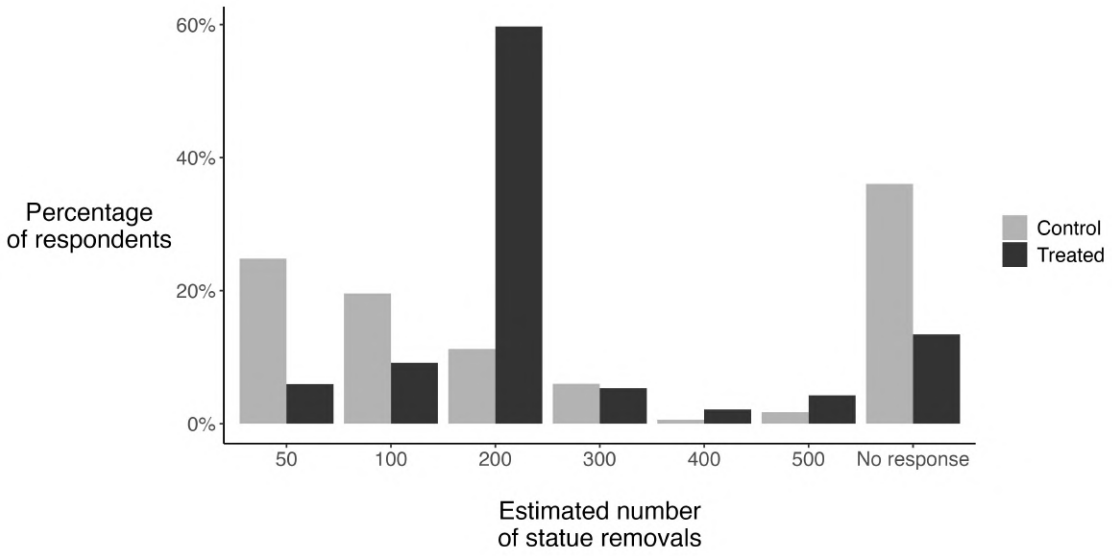


Figure A4: Factual manipulation check

Plot presents the distribution of the estimated number of Confederate statue removals by treatment condition. ($n = 985$)

Table A2: Average Treatment Effects on Policy Support Indices

	Policy Support					
	Symbolic Policies		Redistributive Policies		Backlash Policies	
Informational intervention	0.062	0.042	−0.024	−0.037	0.031	0.021
	(0.064)	(0.050)	(0.066)	(0.053)	(0.067)	(0.059)
Controls	No	Yes	No	Yes	No	Yes
Observations	968	968	971	971	967	967
R^2	0.001	0.407	0.0001	0.374	0.0002	0.260

Robust standard errors in parentheses. Coefficients are expressed in terms of control group standard deviations. The following covariates are included but not reported: age, region, gender, race and its interaction with racial identity attachment, left-right self-placement, political interest, political knowledge, party identification, American pride, racial resentment, education and household income.

D.3 ATE estimates

Table A3: Average Treatment Effects on Forced Choice Policy Support (Multinomial Logistic Regression)

	Preferred policy type (Reference category: Neither)	
	Redistributive	Symbolic
Informational Intervention	0.118	0.338
	(0.167)	(0.197)
Observations	985	
Akaike Inf. Crit.	1,793.18	

Robust standard errors in parentheses. Coefficients come from a multinomial logistic regression model of a dependent variable in which respondents indicate whether they would most support redistributive, symbolic or neither policy type to address racial injustice. In the above model, "neither" is the reference category. The following covariates are included but not reported: age, region, gender, race and its interaction with racial identity attachment, left-right self-placement, political interest, political knowledge, party identification, American pride, racial resentment, education and household income.

Table A4: Average Treatment Effects on Individual Policy Items

	Support for Symbolic Policies		
	Lynching Memorial	Juneteenth Celebration	Museum Field Trips
Informational intervention	0.108*	−0.038	0.042
	(0.053)	(0.053)	(0.056)
Observations	973	975	973
R^2	0.318	0.349	0.283
Controls	✓	✓	✓
	Support for Redistributive Policies		
	Affirmative Action	Anti-segregation Zoning	Redirect Police Budget
Informational intervention	−0.091	−0.029	0.036
	(0.055)	(0.056)	(0.056)
Observations	979	979	975
R^2	0.312	0.276	0.277
Controls	✓	✓	✓
	Support for Backlash Policies		
	Protect Confederate Symbols	Permit housing Discrimination	Stricter Welfare Rules
Informational intervention	−0.010	0.019	0.035
	(0.062)	(0.060)	(0.060)
Observations	976	974	976
R^2	0.128	0.169	0.201
Controls	✓	✓	✓

Robust standard errors in parentheses. Coefficients are expressed in terms of control group standard deviations. The following covariates are included but not reported: age, region, gender, race and its interaction with racial identity attachment, left-right self-placement, political interest, political knowledge, party identification, American pride, racial resentment, education and household income.
*$p < 0.05$

Table A5: Average Treatment Effects on Mechanism Checks

	Racial justice is a priority for me	Racial justice is a priority for others	Now is the moment to pursue racial justice
Informational intervention	−0.089	0.025	−0.096
	(0.055)	(0.064)	(0.060)
Observations	922	922	922
R^2	0.313	0.083	0.198
Controls	✓	✓	✓
	Racial injustice hurts America's reputation	My heritage is under attack	Statue removals are a distraction
Informational intervention	−0.085	−0.083	−0.122
	(0.056)	(0.061)	(0.064)
Observations	922	922	922
R^2	0.310	0.177	0.096
Controls	✓	✓	✓
	Government should do more on social problems	There is more to do on racial justice	
Informational intervention	−0.088	0.068	
	(0.061)	(0.056)	
Observations	922	921	
R^2	0.198	0.312	
Controls	✓	✓	

Robust standard errors in parentheses. Coefficients are expressed in terms of control group standard deviations. The following covariates are included but not reported: age, region, gender, race and its interaction with racial identity attachment, left-right self-placement, political interest, political knowledge, party identification, American pride, racial resentment, education and household income.
$^{*}p < 0.05$

D.4 CATE estimates

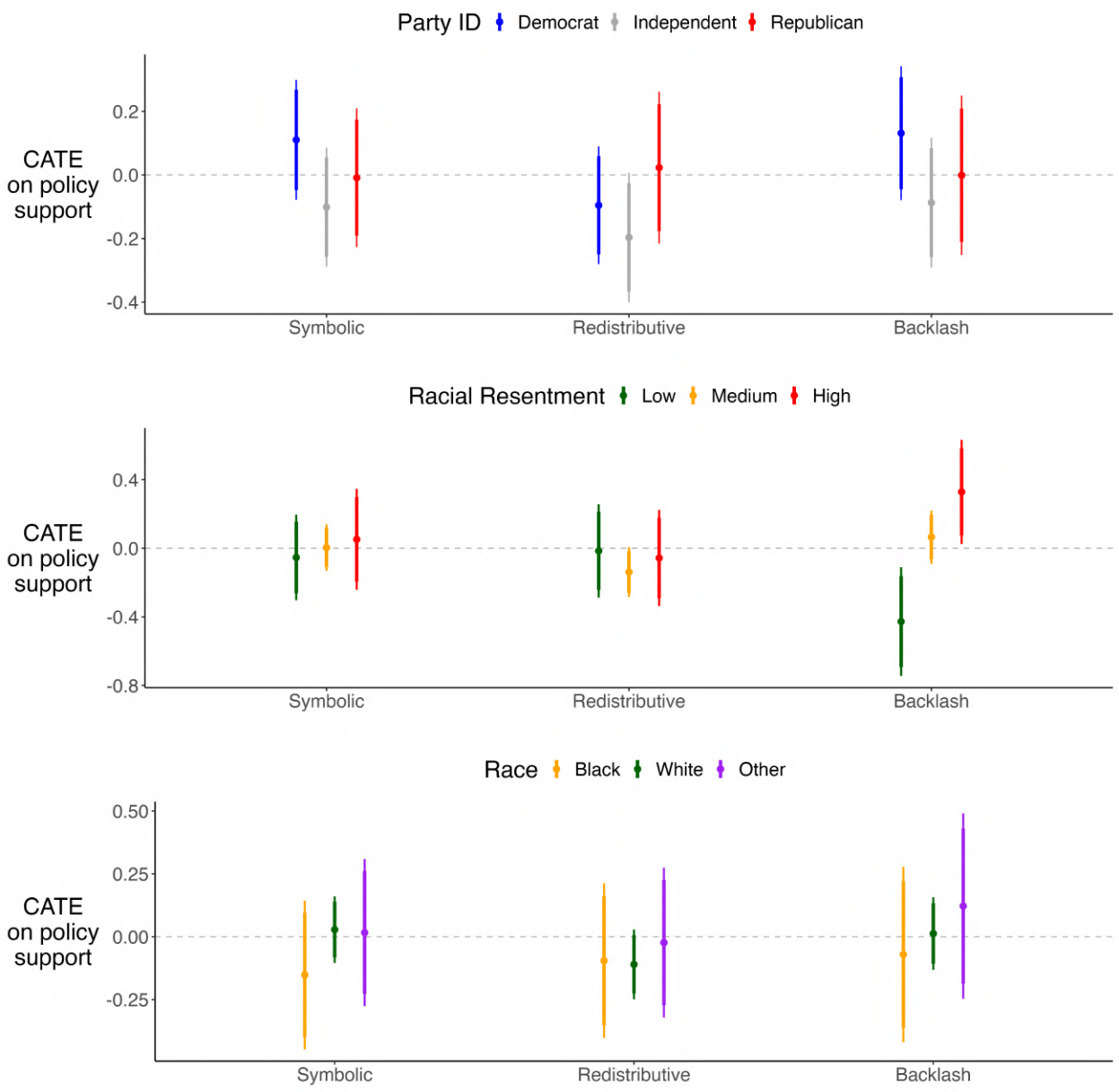


Figure A5: Conditional average treatment effect on policy support by partisanship, race, and racial resentment

Plot summarises three models regressing support for each policy type listed on the x-axis on a treatment indicator interacted with the following pre-treatment covariates: age, region, gender, race, left-right self-placement, political interest, political knowledge, partisanship, American pride, racial resentment, education and household income (only the estimates for the variables shown in the three panels are reported). See Appendix C.9 for details on the categorization of racial resentment. ($n \approx 905$)

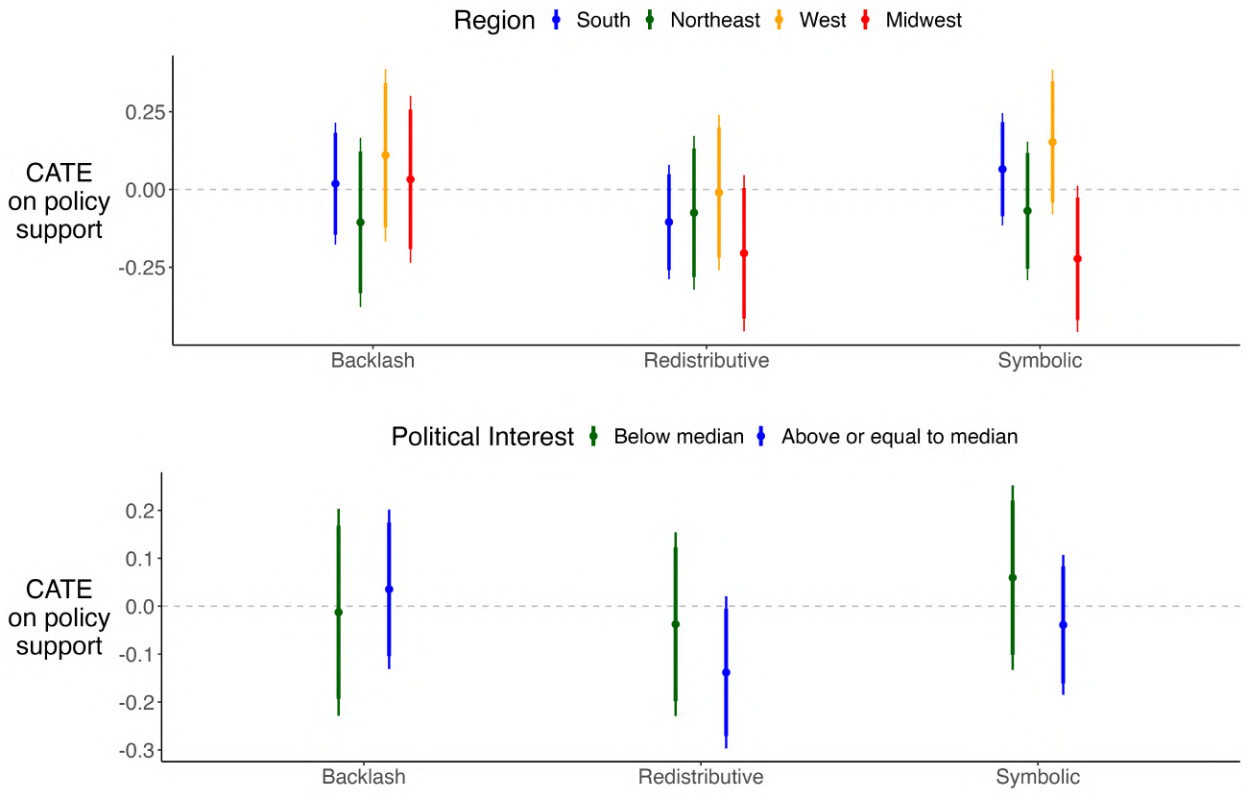


Figure A6: Conditional average treatment effect on policy support by region and political interest

Plot summarizes three models regressing support for each policy type listed on the x-axis on a treatment indicator interacted with the following pre-treatment covariates: age, region, gender, race, left-right self-placement, political interest, political knowledge, partisanship, American pride, racial resentment, education and household income (only the estimates for the variables shown in the two panels are reported). The models reported here are the same as those in Figure A5. ($n \approx 905$)

D.5 Qualitative Coding of Open-Ended Responses

After the informational intervention, respondents in the treatment condition were asked the following open-ended question: "How did you feel reading the information on the previous pages? In 1-3 sentences, please tell us your opinion on the removals of Confederate statues and place names in America." We developed a two-tier coding scheme for hand-coding the responses. In the first tier, respondents were assigned to 5 possible values with regards to whether their answers expressed some sort of support or opposition to Confederate symbol removals: 1 for "No Response", 2 for "Indifferent," 3 for "Oppose," 4 for "Support" and 5 for "Other." Once we drop the individuals who did not leave a response to the open-ended questions, we are left with 435 respondents.

We then developed a more detailed second-tier system to classify the specific content of these open-ended responses (across the indifferent, oppose, support, and other first-tier categories). The details of this detailed classification scheme that were used to hand-code the responses are provided in Table A6 below. This table shows the two-tier hand-coding scheme

for the open-ended text responses and the percentage of the 545 total respondents who fell within each of the categories. Note that these percentages do not add up to 100 percent because there are some respondents whose open-ended responses fell into multiple categories; thus, these percentages reflect whether these themes are mentioned by a respondent at all.

Figure A7 shows the distribution of the Tier 2 responses among the respondents who did give a response, amounting to a total of 435 respondents. By far, the modal response was "cannot forget/erase history" - which was referenced in about 36 percent of respondents' open-ended answers. When we subset this to those who opposed Confederate symbol removals (based on the Tier 1 classification), 66 percent of those who oppose removals mention "cannot forget/erase history" in their answers.

Table A6: Detailed Classification Scheme for Open-Ended Responses

Code	Description	Tier 1 %	Tier 2 %
1	**No response**	**20.18**	11.19
1a	Don't know		2.39
1b	Prefer not to comment		0.55
1c	Unrelated comment		6.06
2	**Indifferent**	**7.16**	
2a	Don't care		2.75
2b	Doesn't change anything		3.12
2c	Statues and names are unimportant		1.65
3	**Oppose**	**43.12**	
3a	Oppose-Non-specific		6.61
3b	Material concerns		2.20
3c	Status quo is fine		2.02
3d	Status threat		0.55
3e	Blaming people in the past for today's problems is wrong		0.73
3f	Removals are non-democratic		0.37
3g	Removals are creating divisions/polarization		0.73
3h	No country/person is perfect		0.55
3i	Slippery slope		1.47
3j	These statues are not meant to be disrespectful		0.55
3k	Whataboutism		0.92
3l	Removals represent discrimination against whites		0.37
3m	These demands are coming from radicals		4.04
3n	Expression of prejudice		2.20
3o	Cannot forget/erase history		28.62
4	**Support**	**24.77**	
4a	Support-Non-specific		5.87
4b	Statues represent bad things that should not be celebrated		6.97
4c	Should be replaced with better things		0.73
4d	Signals we are a good country		0.00
4e	Honoring Confederacy is inconsistent with today's values		4.95
4f	Keeping statues up obscures bad things from the past		0.18
4g	Removing statues helps some groups feel included		2.94
4h	This is a necessary step forward		6.24
5	**Other codes**	**23.67**	
5a	Other specific mentions		0.73
5b	Idea that statues have been up so long		0.73
5c	Teach history without honoring		4.59
5d	Other: References to Confederacy		2.02
5e	References to treatment uptake		4.22
5f	Emotions		11.01

Note that these percentages do not add up to 100 percent because there are some respondents whose open-ended responses fell into multiple categories; thus, these percentages reflect whether these themes are mentioned by a respondent at all.

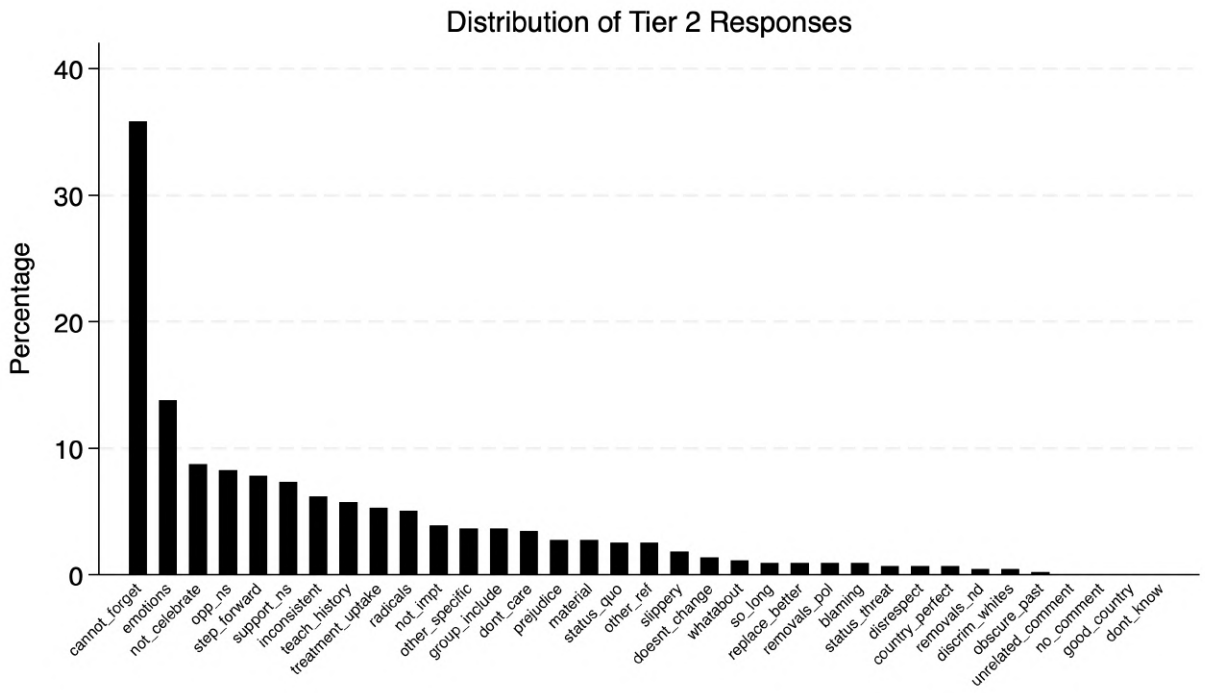


Figure A7: Distribution of open-ended responses based on tier-2 classification

Plot shows the distribution of the open-ended responses according to the second tier classification scheme for the hand-coding. The five most common responses were cannot forget/erase history, emotions, statues represent bad things that should not be celebrated, oppose non-specific, and this is a necessary step forward. ($n = 435$).

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