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Were Americans' Political Attitudes Linked to Objective Threats From COVID-19? An Examination of Data From Project Implicit During Initial Months of the Pandemic

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Abstract
The COVID-19 pandemic has created objectively threatening situations in everyday life (e.g., unemployment, risk of infection), and researchers have begun to ask whether threats from the pandemic are linked to people's political attitudes. However, scholars currently lack a systematic answer to this question. Here, we examined whether objective COVID-19 threats (cases, deaths, and government restrictions) occurring over the initial months of the pandemic (February–June 2020) were associated with seven different assessments of political attitudes among Project Implicit users in the United States ($N = 34{,}581$). We did not consistently observe meaningful associations between COVID-19 threats and political attitudes. The lack of consistent meaningful associations emerged regardless of the level of analysis (country, state, and county) or participant's self-identified ideology. Collectively, these findings failed to find evidence that political attitudes were tied to COVID-19 threats in a meaningful way during the initial months of the pandemic.

Keywords
COVID-19, political attitudes, uncertainty-threat model, ideological affordances

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In January 2020, the United States Centers for Disease Control and Prevention confirmed the first case of the novel coronavirus (COVID-19) in the United States (Harcourt et al., 2020). The COVID-19 pandemic subsequently created objectively threatening situations in everyday life. The virus spread throughout the United States, with a total of nearly 29 million confirmed cases and more than half a million deaths as of March 2021 (Centers for Disease Control and Prevention, 2021). Governments also initially implemented severe restrictions (e.g., stay-at-home orders) that required people to give up individual freedom and face-to-face social interaction, which further contributed to unemployment rates spiking to nearly 15% in April 2020 (U.S. Bureau of Labor Statistics, 2020).

Social and behavioral scientists have debated whether these restrictions and the increasing number of COVID-19 cases were associated with the political attitudes that people adopted (e.g., Jost, 2020; Thomas et al., 2020). Despite this interest, researchers currently lack a systematic understanding of how objective threats from the pandemic corresponded to political attitudes. This question holds theoretical importance for understanding the roots of political attitudes, as well as pragmatic value in pinpointing political stances held during the pandemic.

Here, we conducted an analysis examining whether COVID-19 threats occurring from February to June 2020 were associated with the adoption of particular political attitudes. Specifically, we examined (a) whether COVID-19 threats (cases, deaths, and government restrictions) were associated with people holding more or less conservative attitudes on operational (e.g., policy focused) issues, (b) the degree to which any associations may have varied across general, social, and economic domains, and (c) if associations differed based on whether a person identified as more liberal or conservative. We predicted that exposure to threats brought about by the pandemic would be linked to more operationally conservative attitudes, and that this association

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would be stronger among people who identify as liberal (vs. conservative). We pit this prediction against competing theoretical perspectives that suggest exposure to certain threats would be differentially linked to social and economic views, and that threat should prompt more extreme political attitudes among both liberals and conservatives.

Threat and Politically Conservative Attitudes

Researchers have long been interested in understanding the link between exposure to objectively threatening situations and people's political attitudes. One dominant perspective is the *uncertainty-threat model* (Jost et al., 2007), which proposes that the motivation to manage threat leads people toward more (vs. less) politically conservative attitudes. For example, a recent meta-analysis indicated that exposure to objectively threatening situations (e.g., terrorist attacks) was associated with the adoption of more politically conservative attitudes (Jost et al., 2017). The model's proposal is domain-general, and so it extends to both social (e.g., abortion attitudes) and economic domains (e.g., taxation attitudes), as well as issues that do not exist in a specific domain (e.g., voting). Embracing conservative attitudes is theorized to infuse society with a sense of stability and alleviate psychological distress. For example, some findings indicate that terrorist attacks were associated with people embracing more politically conservative attitudes (Economou & Kollias, 2015), and that the 2014 Ebola outbreak was linked to greater support for conservative politicians in the United States (Beall et al., 2016).

Based on the uncertainty-threat model, we predicted that the COVID-19 pandemic would nudge people toward adopting more operationally conservative views. Some preliminary evidence supports this perspective. For instance, people reported greater endorsement of gender stereotypes when surveyed after (vs. before) the start of the pandemic (Rosenfeld & Tomiyama, 2021), and increasing the salience of the COVID-19 outbreak led to greater support for right-wing candidates in Poland (Karwowski et al., 2020). These preliminary findings suggest that threats from the COVID-19 pandemic are associated with adopting more conservative attitudes. However, these studies examined a limited number of outcomes and operationalizations of pandemic-related threat, and some findings suggest that the COVID-19 pandemic might not have produced mean-level shifts in certain political attitudes (e.g., supporting government intervention to address inequality; Wiwad et al., 2021). Thus, it is unclear at the current time what aspects of the pandemic are linked to people's political attitudes, and which attitudes are tied to pandemic-related threats. As a result, whether threats from the pandemic are consistently associated with more conservative attitudes stands as an open question. Based on the uncertainty-threat model, we predicted that threats from the COVID-19 pandemic would be associated with more politically conservative attitudes across various assessments (e.g., policy stances, voting intentions) and domains (e.g., social, economic).

Distinct Effects on Social and Economic Attitudes

At the same time, some scholars have argued for a perspective of *ideological affordances*, which challenges the uncertainty-threat model and our predictions. This perspective proposes that people adopt distinct attitudes within social and economic domains because such attitudes address different threats (e.g., Eadeh & Chang, 2020). Specifically, more socially conservative attitudes address physical safety threats, whereas less economically conservative (i.e., more liberal) attitudes address economic threats (Brandt et al., 2021). How might COVID-19-related threats be differentially associated with social and economic attitudes? The pandemic has amplified economic threat (e.g., higher unemployment rates; Mann et al., 2020), and some preliminary evidence suggests that people reported greater support for economically liberal policies (e.g., universal basic income) when surveyed after (vs. before) the start of the pandemic (Nettle et al., 2020). The pandemic has also concomitantly produced physical safety threats about disease and illness, which might activate the behavioral immune system and in turn foster socially conservative attitudes (Rosenfeld & Tomiyama, 2021; Terrizzi et al., 2013). Collectively, an approach based on ideological affordances suggests that COVID-19 threats would be simultaneously associated with *less* economically conservative attitudes and *more* socially conservative attitudes. This perspective clashes with our predictions based on the uncertainty-threat model. Here, we examine which (if either) perspective is more strongly supported.

Variation in the Threat-Operational Attitudes Association Based on Symbolic Ideology

Associations between COVID-19 threat and political attitudes might also vary across people. Symbolic ideology concerns people's abstract views and the extent to which they self-identify with political labels (e.g., liberal and conservative) whereas operational ideology describes attitudes that people hold toward specific issues (e.g., attitudes toward abortion; Federico et al., 2012). Although symbolic and operational ideology overlap (e.g., people who identify as liberal on average support legal abortion access), they are distinct constructs. Thus, it is possible that associations between COVID-19 threat and operational political attitudes vary depending on one's symbolic ideology.

The uncertainty-threat model proposes that objective threats correspond to adopting more conservative attitudes, regardless of symbolic ideology (e.g., Jost et al., 2007).

Table 1. Primary (Preregistered) and Competing Predictions of the Association Between COVID-19 Threat and Operationally Conservative Attitudes.

Predicted association between threat and operationally conservative attitudes	Domain-general attitudes	Social attitudes	Economic attitudes	Predictions supported
Main effect predictions				
Uncertainty-threat	Positive association	Positive association	Positive association	None
Ideological affordances	No specific prediction	Positive association	Negative association	None
Interaction effect predictions				
Uncertainty-threat	Positive association stronger among symbolic liberals (vs. conservatives)	Positive association stronger among symbolic liberals (vs. conservatives)	Positive association stronger among symbolic liberals (vs. conservatives)	None
Threat compensation	Positive association among symbolic conservatives & negative association among symbolic liberals	Positive association among symbolic conservatives & negative association among symbolic liberals	Positive association among symbolic conservatives & negative association among symbolic liberals	None

Note. Predicted associations are noted for both domain-general and domain-specific attitudes. "Positive Association" indicates greater threat being associated with more operationally conservative attitudes, and "Negative Association" indicates greater threat being associated with less operationally conservative attitudes.

However, threat is expected to wield a larger impact among symbolic liberals (vs. conservatives) because they are able to experience a larger shift toward operationally conservative attitudes (e.g., Nail et al., 2009). Based on the uncertainty-threat model, we predicted that associations between COVID-19 threat and operationally conservative attitudes would be stronger among symbolic liberals (vs. conservatives).

Importantly, a competing *threat compensation* perspective would arrive at different predictions and would suggest that threats are linked to adopting more extreme political attitudes (e.g., Burke et al., 2013; Proulx et al., 2012). Based on threat compensation models, COVID-19 threat would be associated with more operationally conservative views among symbolic conservatives but more operationally liberal views among symbolic liberals. We examined which (if either) perspective was more strongly supported by testing whether relations between COVID-19 threats and operational political attitudes differed between symbolic liberals and conservatives. Table 1 summarizes the expected main effects and interactions based on the uncertainty-threat, ideological affordances, and threat compensation perspectives for general, social, and economic attitudes.

Present Research

We investigated whether changes in COVID-19 threats corresponded to any shifts in political attitudes. We explored associations with various assessments of COVID-19 threat on the level of the country, state, and county to determine whether particular threats were more strongly linked to political attitudes. In addition, we assessed political attitudes with several measures, including a measure in which social and economic policy views could be clearly distinguished (see Table 2 for classification of measures assessing domain-general, social, or economic political attitudes).

Table 2. Classification of Operational Political Attitude Measures as Assessing Domain-General, Social, or Economic Attitudes.

Outcome	Domain-general	Social	Economic
Voting intentions	X		
Overall policy attitudes	X		
Social policy attitudes		X	
Economic policy attitudes			X
Wilson–Patterson Scale	X		
Explicit attitudes	X		
Implicit attitudes	X		

Method

Participants

Data came from a separate project examining associations between symbolic and operational conservatism (https://osf.io/d9zw6/). No data were collected after analyses were conducted. In total, 34,581 participants (24,039 women, 10,400 men, 142 no gender specified; M_{age} = 35.50 years, standard deviation [*SD*] = 15.22 years, range = 17–91 years) who were American residents completed the study through the Project Implicit research pool between February 21 and June 26, 2020. Several measures contained topics specific to political issues in the United States, so all analyses were limited to current U.S. residents. Participants hailed from all 50 U.S. states, as well as the District of Columbia and Puerto Rico,[1] and from 1,856 counties.

This study was conducted through the Project Implicit research pool: meaning participants decided to complete a study on Project Implicit but were randomly assigned to one of several studies being conducted at that time. Participants could complete the study several times, and analyses were restricted to the first time a participant completed a specific voting or policy attitude measure of operational conservatism. Sample sizes vary across analyses due to missing data. The preregistration plan for the analyses and approach to interpreting results can be found at https://osf.io/nxfks/. We note deviations from the preregistered analyses in Notes 4 to 6. We otherwise report all preregistered analyses in the main text. Deidentified data, measures, analytic code, and the online Supplemental Material can be found at https://osf.io/fxgtc/. We report all measures, manipulations, and exclusions in this study.

Procedure

Each study session consisted of four components, completed in random order: (a) a voting or policy-focused measure of operational conservatism, (b) an explicit attitude questionnaire capturing operational conservatism, (c) an implicit measure capturing operational conservatism, and (d) a symbolic conservatism measure.

Operational conservatism (voting and policy attitude measures). Participants were randomly assigned to complete one of four measures assessing operational conservatism. Measures were scored such that higher values indicated greater operational conservatism.[2]

Anticipated voting behavior. Participants reported what party they anticipated voting for in the 2020 presidential election: *Republican, Democrat, Independent, Libertarian, Candidate of another party, Do not currently know, I do not plan on voting*. The theoretical perspectives that we draw from in the present research all propose conditions under which threat is expected to increase or decrease support for conservative views (e.g., Brandt et al., 2021; Eadeh & Chang, 2020; Jost et al., 2007, 2017). However, not all of these perspectives speak to how threat is expected to align with other beliefs (e.g., more liberal or moderate beliefs) or disengagement from political behavior. To ensure that our analyses could simultaneously test predictions derived from diverse theoretical perspectives, we chose to code the voting intention measure in a manner that would allow us to capture whether COVID-19 threats correspond to increased or decreased interest in the conservative candidate. Thus, we rescored anticipated voting behavior into a binary outcome of voting for the Republican candidate (1) versus all other responses (0).

Policy support. To assess overall policy support, participants completed a 10-item measure from White et al. (2020) using a 1 (*strongly disagree*) to 7 (*strongly agree*) scale. A sample issue includes "Minimal regulations on the free market system." We created a composite ($\alpha = .82$).

Wilson–Patterson C-scale. To assess overall policy support with a separate measure, participants completed a 21-item version of the Wilson–Patterson inventory taken from Smith et al. (2011). Participants indicated attitudes toward various issues using a 0 (*strongly negative*) to 100 (*strongly positive*) scale. A sample issue includes the "Death Penalty." We created a composite ($\alpha = .89$).

Social and economic policy attitudes. To assess distinct attitudes toward social and economic policies, participants completed the 12-item Social and Economic Conservatism Scale (Everett, 2013). Participants used response options that ranged from 0 to 100, with higher numbers indicating greater positivity toward an issue. Seven items assessed attitudes toward social policies, including "Military and National Security." Five items assessed attitudes toward economic policies, including "Welfare benefits" (reverse scored). We created separate composites for social ($\alpha = .87$) and economic ($\alpha = .65$) policy attitudes.

Operational conservatism (explicit attitudes). Participants completed an explicit attitude measure assessing operational conservatism for one of eight topics (e.g., Democrat vs. Republican). For each topic, the explicit attitude measure consisted of five items: (a) a relative preference item (e.g., 1 = "*I strongly prefer Democrats to Republicans*" to 7 = "*I strongly prefer Republicans to Democrats*"), (b) a thermometer item of liking toward one category (e.g., liking of Democrats, with options ranging from 1 = "*strongly dislike*" to 7 = "*strongly like*"), (c) a thermometer item of liking toward the other category (e.g., liking of Republicans, with options ranging from 1 = "*strongly dislike*" to 7 = "*strongly like*"), (d) a slider response of positivity toward one category (e.g., positivity toward Democrats, with anchors of −100 = "*extremely negative*" to 100 = "*extremely positive*"), and (e) a slider response of positivity toward the other category (e.g., positivity toward Republicans, with anchors of −100 = "*extremely negative*" to 100 = "*extremely positive*").

To score explicit measures, we calculated difference scores for items b and c, and d and e. Consistent with past research (e.g., Axt et al., 2020), these two difference scores and the response to Item 1 were then standardized and averaged. Higher values indicate more positive attitudes toward objects that typically receive more positive evaluations from conservatives than liberals (Republicans, Conservatives, Gun rights, Tax reductions, Traditional values, Defense, Straight people, and Management). In other words, higher scores indicated greater explicit operational conservatism. Participants were excluded from implicit attitude analyses if more than 10% of critical trials were faster than 300ms (2.3% of analysis sample; Nosek et al., 2007).

Operational conservatism (implicit attitudes). Participants completed an Implicit Association Test (IAT), a Single-Target IAT

(ST-IAT), or a Single-Category IAT (SC-IAT) assessing operational conservatism for the same topic as the explicit attitude measure. This approach has been previously used to assess operational conservatism (e.g., Hawkins & Nosek, 2012). Implicit measures were scored using the *D* scoring algorithm, with more positive scores indicating more positive evaluations of the object that typically receives more positive evaluations from conservatives than liberals. In other words, higher scores indicated greater implicit operational conservatism.

Symbolic conservatism. Participants were randomly assigned to complete one of six sets of items that we categorized as directly assessing symbolic conservatism or serving as a close proxy measure. Measures were scored such that higher values indicated greater symbolic conservatism.

Single-item conservatism measures. Participants completed a single-item measure of general conservatism (van der Toorn et al., 2017) and single-item measures of social and economic conservatism (Azevedo et al., 2019). For example, participants indicated their general conservatism in response to the question "Where on the following scale of political orientation would you place yourself overall, in general?" using a 1 = "*extremely liberal*" to 11 = "*extremely conservative*") response scale. We created a composite of the three items to generate an overall measure of symbolic conservatism (α = .92).

Social dominance orientation (SDO). Participants completed the eight-item short form of the SDO_7 scale (Ho et al., 2015) using a 1 = "*strongly oppose*" to 7 = "*strongly favor*" response scale. A sample item includes "An ideal society requires some groups to be on top and others to be on the bottom." We created a composite (α = .76).

Right-wing authoritarianism (RWA). Participants completed the 22-item RWA scale (Altemeyer, 1988) using a 1 = "*you very strongly disagree with the statement*" to 9 = "*you very strongly agree with the statement*" response scale. A sample item includes "The 'old-fashioned ways' and the 'old-fashioned values' still show the best way to live." We created a composite (α = .93).

Resistance to change. Participants completed a five-item scale[3] from van der Toorn et al. (2017) using a 1 = "*strongly disagree*" to 7 = "*strongly agree*" response scale. A sample item includes "I think it's best to keep society the way it is, even if it has some flaws." We created a composite (α = .74).

Resistance to change-beliefs scale. Participants completed a 10-item scale from White et al. (2020) using response options that ranged from a 1 = "*strongly disagree*" to 7 = "*strongly agree.*" A sample item includes "Approaches used by people in the past are generally the most effective." We created a composite (α = .83).

Opposition to equality. Participants completed a five-item scale from van der Toorn et al. (2017) using response options that ranged from 1 = "*strongly disagree*" to 7 = "*strongly agree.*" A sample item includes "Laws of nature are responsible for differences in wealth in society." We created a composite (α = .71).

Assessments of COVID-19 threat. We assessed threat derived from the COVID-19 pandemic using approaches that captured threats on the level of the country, state, and county. We focused on three assessments of threat: (a) restrictions on movement (travel recommendations and stay-at-home orders), (b) confirmed number of COVID-19 cases and deaths, and (c) number of COVID-19 cases and deaths relative to population.[4] Each of these operationalizations are related, as they capture salient and objective threats pertinent to physical safety, social interaction, and economic stability (e.g., Mann et al., 2020; Wong, 2020). However, each operationalization also possesses distinct components. We included each operationalization to test for both the robustness and specificity of any effects. That is, if political attitudes are consistently associated with only one operationalization of threat, it would reveal what type of information people were most sensitive to. For instance, if political attitudes were only associated with the *total* number of cases in an area, it would suggest that people were encoding COVID-related threats in terms of the overall number of infections rather than as a relative risk (i.e., relative to the area's population).

It is also important to highlight that the outcome variable we assessed was consistently on the level of the individual, regardless of the level of analysis of the predictor. Some previous research examining the impact of threat on political attitudes has taken an approach in which responses on the outcome variable are averaged to match the predictor's level of analysis (e.g., averaging the attitudes of people within a given location; Beall et al., 2016; Sales, 1972). Although this approach is highly informative for addressing certain questions, it also possesses several limitations for addressing our research questions. First, this approach limits statistical power through focusing on what is often a relatively small number of geographic units (e.g., states). Second, this approach does not allow for an examination of how aspects of individuals (e.g., symbolic ideology) might modulate the relation between factors on higher levels of analysis and individual outcomes. Third, the theoretical models that we draw from most directly make predictions about changes in conservatism on the level of the individual (e.g., Burke et al., 2013; Eadeh & Chang, 2020; Jost et al., 2017). Thus, testing our primary hypotheses and the competing predictions necessitates the usage of models in which the outcome variable is on the individual level.

Based on this reasoning, we take the approach of examining associations between predictors at higher levels of analysis (country, state, and county) with outcomes on the level of the individual, as well as examining the degree to which individual characteristics interact with factors at higher levels of analysis to predict individual-level outcomes. This approach is consistent with recent research examining how features of the environment at a higher level of analysis (e.g., state-level variables) correspond to individual outcomes (e.g., Ofosu et al., 2019; Rentfrow & Jokela, 2016). We examine associations across three levels of analysis, as the same construct (e.g., number of COVID-19 cases) at different levels of analysis can wield different degrees of impact (e.g., Duckitt, 1992; Gully et al., 2002), including geographic levels of analysis (e.g., Ofosu et al., 2019). For example, people's political attitudes might be most sensitive to the degree of threat in their local environment (i.e., their county). While we did not have specific predictions for whether (or how) any patterns of effects might differ across levels of analysis, we viewed it as important to consider the influence of constructs at each of these levels. In doing so, it is also critical to avoid the ecological fallacy, in which inferences at a lower level of analysis are made based on results from a higher level of analysis (e.g., making inferences on the county level based on state-level findings; Selvin, 1958). In other words, the meaningfulness of associations should be interpreted independently within each level of analysis (country vs. state vs. county).

Country-level travel recommendations. We coded whether participants completed the study before (25.4%) or after (74.6%) the U.S. federal government implemented Level 4 recommendations to avoid traveling out of the country on March 19, 2020 (Wong, 2020).

Country-level COVID-19 cases. Each participant received a score indicating the number of COVID-19 cases that had been confirmed in the United States at the time they completed the study. This information was obtained from the New York Times (NYT) COVID-19 database (https://github.com/nytimes/covid-19-data/blob/master/us.csv). Cases were positively skewed, which can lead to biased parameter estimated in models (McClelland, 2014). To address this issue, we natural-log-transformed COVID-19 case variables (Webster et al., 2021).[5]

Country-level COVID-19 deaths[6]. Each participant received a score indicating the number of COVID-19 deaths that had been confirmed in the United States at the time they completed the study. This information was obtained from the NYT COVID-19 database (https://github.com/nytimes/covid-19-data/blob/master/us.csv). Deaths were positively skewed, and so we natural-log-transformed COVID-19 death variables.

State-level stay-at-home orders. We coded whether participants completed the study before (25.6%) or after (24.9%) their state of residence implemented a stay-at-home order (Mervosh et al., 2020; "See Coronavirus Restrictions and Mask Mandates for All 50 States," 2020).[7] Participants completing the study after the date on which their state's stay-at-home order was lifted or participants whose state never implemented a stay-at-home order (49.5%) were excluded from analyses using this variable.

State-level COVID-19 cases. Each participant received a score indicating the number of COVID-19 cases that had been confirmed in their state of residence at the time they completed the study. This information was obtained from the NYT COVID-19 database (https://github.com/nytimes/covid-19-data/blob/master/us-states.csv). We log transformed these scores.

State-level COVID-19 cases (percentage). We calculated the percentage of the population in each participant's state that had a confirmed COVID-19 diagnosis at the time the participant completed the study. To calculate this percentage, we used 2019 census population estimates for each state (https://www.census.gov/data/tables/time-series/demo/popest/2010s-state-total.html). We log transformed these scores.

State-level COVID-19 deaths. Each participant received a score indicating the number of COVID-19 deaths that had been confirmed in their state of residence at the time they completed the study. This information was obtained from the NYT COVID-19 database (https://github.com/nytimes/covid-19-data/blob/master/us-states.csv). We log transformed these scores.

State-level COVID-19 deaths (percentage). We calculated the percentage of the population in each participant's state who had died from COVID-19 at the time the participant completed the study. To calculate this percentage, we used 2019 census population estimates for each state (https://www.census.gov/data/tables/time-series/demo/popest/2010s-state-total.html). We log transformed these scores.

County-level COVID-19 cases. Each participant received a score indicating the number of COVID-19 cases that had been confirmed in their county of residence at the time they completed the study. This information was obtained from the NYT COVID-19 database (https://github.com/nytimes/covid-19-data/blob/master/us-counties.csv). We log transformed these scores.

County-level COVID-19 cases (percentage). We calculated the percentage of the population in each participant's county that had a confirmed COVID-19 diagnosis at the time the participant completed the study. To calculate this percentage, we used 2019 census population estimates for each county (https://www.census.gov/data/datasets/time-series/demo/popest/2010s-counties-total.html). We log transformed these scores.

County-level COVID-19 deaths. Each participant received a score indicating the number of COVID-19 deaths that had been confirmed in their county of residence at the time they completed the study. This information was obtained from the NYT COVID-19 database (https://github.com/nytimes/covid-19-data/blob/master/us-counties.csv). We log transformed these scores.

County-level COVID-19 deaths (percentage). We calculated the percentage of the population in each participant's county who had died from COVID-19 at the time the participant completed the study. To calculate this percentage, we used 2019 census population estimates for each county (https://www.census.gov/data/datasets/time-series/demo/popest/2010s-counties-total.html). We log transformed these scores.

Results

Tables presenting means, standard deviations, and correlations for individual-, state-, and county-level variables can be found in the online Supplemental Material (Tables S3–S5).

Data Analysis

Single and multilevel models. For country-level analyses, we conducted single-level regression analyses. Specifically, we conducted linear regressions for models with a continuous outcome variable and logistic regressions for models with a binary outcome variable (voting intentions).[8] For state- and county-level analyses, we conducted multilevel models to account for nonindependence of responses within locations (Fitzmaurice et al., 2012). We used the MIXED procedure in SPSS for models with continuous outcomes variables and the glmer function in *R* for models with a binary outcome variable (voting intentions). All multilevel models initially included a random intercept and slope of state/county. We trimmed random effects if they failed to converge. If no random effects converged in a multilevel model, we instead conducted a single-level model.

Primary models. We conducted two sets of models to test our predictions (see Table 1) at each level of analysis. First, we conducted main effect models that examined the association between assessments of COVID-19 threat and measures of operational conservatism. These models included the operationalization of COVID-19 threat as a fixed effect predictor. Second, we examined whether the strength of associations between COVID-19 threats and operational conservatism measures varied based on the participant's symbolic ideology. These models included the operationalization of COVID-19 threat, the measure of symbolic ideology the participant was assigned to complete, and their interaction as fixed-effect predictors. We decomposed significant interactions through examining associations among people who were more symbolically liberal (1 *SD* below the ideology mean) and conservative (1 *SD* above the ideology mean; Aiken & West, 1991). We conducted separate models for each assessment of operational political attitudes.

Model covariates. We included a single item assessing symbolic conservatism (i.e., self-identification from −3 = "*very conservative*" to 3 = "*very liberal*") as a covariate in main effect models.[9] All participants completed this measure when registering for the research pool. We included this covariate to rule out the possibility that any observed effects could result from a shift over time in the number of symbolic liberals and conservatives completing the study (e.g., a different number of symbolic liberals completing the study before and after the implementation of stay-at-home orders).

For state- and county-level analyses, we also included preexisting conservatism of the state or county as a covariate because it was likely to correlate with the number of confirmed cases (Gollwitzer et al., 2020). We calculated preexisting conservatism as the percentage of vote that Donald Trump (the Republican presidential candidate) received in 2016. State-level support that Trump received was obtained from the Federal Elections Commission (https://www.fec.gov/resources/cms-content/documents/federalelections2016.pdf), and county-level support was obtained from the MIT Election Lab (https://electionlab.mit.edu/). We included this covariate in both main and interaction effect models.[10]

Coding of variables. To create a common metric for model estimates, categorical predictors (e.g., travel recommendations) and outcomes (i.e., voting intentions) were coded as 0 and 1, and all continuous predictors (e.g., U.S. COVID-19 cases) and outcomes (e.g., policy attitudes) were standardized on the level of the sample.

Interpretation of results. We relied on two criteria to interpret results. First, we used a "cutoff" point of what effect size we considered to be meaningful. Although estimates of effect size benchmarks vary, a r/β of .10 is often considered a "small" effect (e.g., Lakens & Evers, 2014). To this end, we considered any effect size smaller than $r/\beta = .10$ to be trivial. Second, we only considered an assessment of COVID-19 threat to have a consistent meaningful association with operational political attitudes if at least 50% of analyses within that assessment indicated effect sizes of $r/\beta \geq .10$. We possessed greater than 99% power to detect an effect size of $r = .10$ in all analyses. We conducted power analyses for single-level models using G*Power 3.1 (Faul et al., 2009), and an application developed for multilevel models when models were nested (Judd et al., 2017).

Consistent with conventional standards, we also report p values for all analyses. Given the large number of analyses conducted, we adjusted our alpha to .005 for determining statistical significance of an effect (Benjamin et al., 2018).

Table 3. Summary of Main and Interaction Effects for U.S. Travel Recommendations.

Analysis	r/β	LB CI	UB CI	p value
Main effect: travel recommendations				
Voting intentions (N = 8,631)	−.06	−.11	−.01	.02
Overall policy attitudes (N = 9,119)	−.09	−.11	−.07	<.001
Social policy attitudes (N = 9,241)	−.01	−.03	.01	.58
Economic policy attitudes (N = 9,225)	.05	.03	.07	.01
Wilson–Patterson Scale (N = 9,193)	−.04	−.06	−.02	.01
Explicit attitudes (N = 36,898)	−.04	−.05	−.03	<.001
Implicit attitudes (N = 33,109)	−.02	−.03	−.01	.11
Interaction effect: travel recommendations × symbolic ideology				
Voting intentions (N = 8,402)	.03	−.02	.08	.20
Overall policy attitudes (N = 9,125)	.06	.04	.08	.001
Liberals	−.21	−.23	−.19	<.001
Conservatives	−.09	−.11	−.07	<.001
Social policy attitudes (N = 9,183)	.05	.03	.07	.009
Economic policy attitudes (N = 9,176)	.05	.03	.07	.02
Wilson–Patterson Scale (N = 9,154)	.05	.03	.07	.003
Liberals	−.12	−.14	−.10	<.001
Conservatives	−.01	−.03	.01	.60
Explicit attitudes (N = 36,484)	.02	.01	.03	.04
Implicit attitudes (N = 33,917)	.001	−.01	.01	.97

Note. Simple effects are reported separately for symbolic liberals and conservatives when an interaction is significant ($p < .005$). βs are semi-standardized, as the travel recommendation variable is coded 0, 1. LB = Lower Bound; CI = 95% Confidence Interval; UB = Upper Bound.

However, we use the effect size criteria outlined above to determine whether an effect is nontrivial. In other words, some analyses may reach $p < .005$ but have an effect size smaller than the cutoff of $r/\beta = .10$ and so are not interpreted as evidence of a meaningful association.

Country-Level Results

Country-level travel recommendations. We conducted regression models in which we specified whether participants completed the study before or after federal travel recommendations were put in place (coded as 0 = "*before*," 1 = "after") as the measure of COVID-19 threat (Table 3).[11] No observed effect sizes for main or interaction effects reached the threshold for being meaningful. Thus, completing the study after (vs. before) the implementation of travel recommendations was not meaningfully associated with changes in political attitudes.

Country-level COVID-19 cases (log). We conducted regression models in which the number of confirmed COVID-19 cases in the United States at the time the participant completed the study was specified as the measure of threat (Table 4). No observed effect sizes for the main or interaction effects reached the threshold for being meaningful. Thus, a greater number of confirmed U.S. cases was not meaningfully associated with political attitudes.

Country-level COVID-19 deaths (log). We conducted regression models in which the number of confirmed COVID-19 deaths in the United States at the time the participant completed the study was specified as the measure of threat (Table 5). No observed effect sizes for the main or interaction effects reached the threshold for being meaningful. Thus, a greater number of U.S. deaths was not meaningfully associated with political attitudes.

State-Level Results

Stay-at-home orders. We specified whether participants completed the study before or after a stay-at-home order was put in place in their state (coded as 0 = "*before*," 1 = "*after*") as the measure of COVID-19 threat (Table 6). No observed effect sizes for main or interaction effects reached the threshold for being meaningful. Overall, completing the study after (vs. before) the implementation of a stay-at-home order was not meaningfully associated with political attitudes.

State-level COVID-19 cases (log). We specified the number of confirmed COVID-19 cases in the participant's state at the time they completed the study as the measure of threat (Table 7). No observed effect sizes for main or interaction effects reached the threshold for being meaningful. Thus, a greater number of confirmed state-level cases was not meaningfully associated with political attitudes.

State-level COVID-19 cases (log percentage). We specified the percentage of confirmed COVID-19 cases in the population of the participant's state at the time they completed the study as the measure of threat (Table 8). No observed effect sizes for main or interaction effects reached the threshold for being meaningful.

Table 4. Summary of Main and Interaction Effects for Log Transformed U.S. COVID-19 Cases.

Analysis	r/β	LB CI	UB CI	p value
Main effect: U.S. COVID-19 cases log				
Voting intentions (N = 8,631)	−.04	−.06	−.02	<.001
Overall policy attitudes (N = 9,119)	−.05	−.07	−.03	<.001
Social policy attitudes (N = 9,241)	−.01	−.03	.01	.44
Economic policy attitudes (N = 9,225)	.02	.003	.04	.003
Wilson–Patterson Scale (N = 9,193)	−.03	−.05	−.01	<.001
Explicit attitudes (N = 36,898)	−.03	−.04	−.01	<.001
Implicit attitudes (N = 33,109)	−.01	−.02	.0008	.04
Interaction effect: U.S. COVID-19 cases log × symbolic ideology				
Voting intentions (N = 8,402)	.01	−.01	.03	.25
Overall policy attitudes (N = 9,125)	.03	.01	.05	<.001
Liberals	−.11	−.13	−.09	<.001
Conservatives	−.04	−.06	−.02	<.001
Social policy attitudes (N = 9,183)	.03	.01	.05	<.001
Liberals	−.05	−.07	−.03	<.001
Conservatives	.01	−.01	.03	.28
Economic policy attitudes (N = 9,176)	.03	.01	.05	<.001
Liberals	−.03	−.05	−.01	.04
Conservatives	.04	.02	.06	.004
Wilson–Patterson Scale (N = 9,154)	.03	.01	.05	.001
Liberals	−.06	−.08	−.04	<.001
Conservatives	−.01	−.03	.01	.38
Explicit attitudes (N = 36,484)	.01	.001	.02	.01
Implicit attitudes (N = 33,917)	.0001	−.01	.01	.98

Note. Simple effects are reported separately for symbolic liberals and conservatives when an interaction is significant ($p < .005$). LB = Lower Bound; CI = 95% Confidence Interval; UB = Upper Bound.

Table 5. Summary of Main and Interaction Effects for Log Transformed U.S. COVID-19 Deaths.

Analysis	r/β	LB CI	UB CI	p value
Main effect: U.S. COVID-19 deaths log				
Voting intentions (N = 8,631)	−.04	−.07	−.02	<.001
Overall policy attitudes (N = 9,119)	−.05	−.07	−.03	<.001
Social policy attitudes (N = 9,241)	−.01	−.03	.01	.39
Economic policy attitudes (N = 9,225)	.02	.004	.04	.003
Wilson–Patterson Scale (N = 9,193)	−.03	−.05	−.01	<.001
Explicit attitudes (N = 36,898)	−.03	−.04	−.02	<.001
Implicit attitudes (N = 33,109)	−.01	−.02	−.0002	.04
Interaction effect: U.S. COVID-19 deaths log × symbolic ideology				
Voting intentions (N = 8,402)	.01	−.01	.03	.32
Overall policy attitudes (N = 9,125)	.03	.01	.05	<.001
Liberals	−.11	−.13	−.09	<.001
Conservatives	−.04	−.06	−.02	<.001
Social policy attitudes (N = 9,183)	.03	.01	.05	<.001
Liberals	−.05	−.07	−.03	<.001
Conservatives	.01	−.01	.03	.29
Economic policy attitudes (N = 9,176)	.03	.01	.05	<.001
Liberals	−.03	−.05	−.01	.04
Conservatives	.04	.02	.06	.003
Wilson–Patterson Scale (N = 9,154)	.03	.01	.05	.001
Liberals	−.07	−.09	−.04	<.001
Conservatives	−.01	−.03	.01	.38
Explicit attitudes (N = 36,484)	.01	−.0003	.02	.03
Implicit attitudes (N = 33,917)	<.001	−.01	.01	.96

Note. Simple effects are reported separately for symbolic liberals and conservatives when an interaction is significant ($p < .005$). LB = Lower Bound; CI = 95% Confidence Interval; UB = Upper Bound.

Table 6. Summary of Main and Interaction Effects for State Stay-at-Home Orders.

Analysis	r/β	LB CI	UB CI	p value
Main effect: state stay-at-home orders				
Voting intentions (N = 4,344)	.03	−.04	.09	.39
Overall policy attitudes (N_S = 43, N_I = 4,627)	−.01	−.04	.03	.70
Social policy attitudes (N_S = 43, N_I = 4,599)	−.002	−.06	.05	.94
Economic policy attitudes (N_S = 43, N_I = 4,588)	.0001	−.04	.04	.99
Wilson–Patterson Scale (N_S = 43, N_I = 4,661)	.02	−.02	.07	.26
Explicit attitudes (N_S = 43, N_I = 18,595)	.01	−.02	.04	.61
Implicit attitudes (N_S = 43, N_I = 16,483)	.01	−.03	.04	.62
Interaction effect: state stay-at-home orders × symbolic ideology				
Voting intentions (N = 4,201)	−.008	−.07	.05	.78
Overall policy attitudes (N_S = 43, N_I = 4,624)	−.02	−.06	.03	.44
Social policy attitudes (N_S = 43, N_I = 4,567)	.02	−.03	.06	.49
Economic policy attitudes (N_S = 43, N_I = 4,562)	.01	−.04	.06	.61
Wilson–Patterson Scale (N_S = 43, N_I = 4,654)	.04	−.01	.08	.11
Explicit attitudes (N_S = 43, N_I = 18,367)	−.0004	−.03	.02	.98
Implicit attitudes (N_S = 43, N_I = 16,877)	−.001	−.03	.03	.93

Note. βs are semi-standardized, as the stay-at-home order variable is coded 0, 1. No random effects converged in models for voting intentions, and so results reported are from single-level models. LB = Lower Bound; CI = 95% Confidence Interval; UB = Upper Bound; N_S = state sample size; N_I = individual response sample size.

Table 7. Summary of Main and Interaction Effects for Log Transformed State COVID-19 Cases.

Analysis	r/β	LB CI	UB CI	p value
Main effect: state COVID-19 cases log				
Voting intentions (N_S = 51, N_I = 8,302)	−.05	−.07	−.03	<.001
Overall policy attitudes (N_S = 51, N_I = 8,724)	−.05	−.07	−.03	<.001
Social policy attitudes (N_S = 51, N_I = 8,844)	−.01	−.03	.01	.43
Economic policy attitudes (N_S = 51, N_I = 8,829)	.02	.004	.04	.02
Wilson–Patterson Scale (N_S = 51, N_I = 8,822)	−.03	−.05	−.01	.002
Explicit attitudes (N_S = 51, N_I = 35,350)	−.03	−.04	−.02	<.001
Implicit attitudes (N_S = 51, N_I = 31,756)	−.01	−.02	−.002	.02
Interaction effect: state COVID-19 cases log × symbolic ideology				
Voting intentions (N_S = 51, N_I = 8,035)	.01	−.01	.03	.29
Overall policy attitudes (N_S = 51, N_I = 8,665)	.02	.01	.04	.004
Liberals	−.10	−.13	−.07	<.001
Conservatives	−.05	−.08	−.03	<.001
Social policy attitudes (N_S = 51, N_I = 8,724)	.03	.01	.05	.001
Liberals	−.05	−.08	−.02	.002
Conservatives	.01	−.02	.04	.50
Economic policy attitudes (N_S = 51, N_I = 8,718)	.03	.01	.05	.002
Liberals	−.03	−.06	.005	.10
Conservatives	.03	.01	.06	.02
Wilson–Patterson Scale (N_S = 51, N_I = 8,721)	.02	.01	.04	.003
Liberals	−.07	−.09	−.04	<.001
Conservatives	−.02	−.04	.01	.22
Explicit attitudes (N_S = 51, N_I = 34,709)	.004	−.005	.01	.35
Implicit attitudes (N_S = 51, N_I = 32,291)	−.003	−.01	.007	.54

Note. Simple effects are reported separately for symbolic liberals and conservatives when an interaction is significant ($p < .005$). LB = Lower Bound; CI = 95% Confidence Interval; UB = Upper Bound; N_S = state sample size; N_I = individual response sample size.

Table 8. Summary of Main and Interaction Effects for Log Transformed State COVID-19 Cases (Percentage).

Analysis	r/β	LB CI	UB CI	p value
Main effect: state COVID-19 cases log (%)				
Voting intentions ($N_S = 51$, $N_I = 8,302$)	−.09	−.12	−.06	<.001
Overall policy attitudes ($N_S = 51$, $N_I = 8,724$)	−.09	−.12	−.07	<.001
Social policy attitudes ($N_S = 51$, $N_I = 8,844$)	−.02	−.04	−.003	.03
Economic policy attitudes ($N_S = 51$, $N_I = 8,829$)	.02	.005	.04	.01
Wilson–Patterson Scale ($N_S = 51$, $N_I = 8,822$)	−.07	−.09	−.04	<.001
Explicit attitudes ($N_S = 51$, $N_I = 35,350$)	−.05	−.07	−.03	<.001
Implicit attitudes ($N_S = 51$, $N_I = 31,756$)	−.02	−.03	−.008	.001
Interaction effect: state COVID-19 cases log (%) × symbolic ideology				
Voting intentions ($N_S = 51$, $N_I = 8,035$)	.03	.005	.06	.02
Overall policy attitudes ($N_S = 51$, $N_I = 8,665$)	.04	.02	.06	<.001
Liberals	−.13	−.17	−.10	<.001
Conservatives	−.06	−.10	−.02	.001
Social policy attitudes ($N_S = 51$, $N_I = 8,724$)	.03	.01	.05	.002
Liberals	−.04	−.07	−.01	.005
Conservatives	.02	−.01	.05	.21
Economic policy attitudes ($N_S = 51$, $N_I = 8,718$)	.03	.01	.05	<.001
Liberals	−.001	−.03	.03	.94
Conservatives	.07	.04	.10	<.001
Wilson–Patterson Scale ($N_S = 51$, $N_I = 8,721$)	.01	−.003	.03	.096
Explicit attitudes ($N_S = 51$, $N_I = 34,709$)	.01	−.003	.02	.15
Implicit attitudes ($N_S = 51$, $N_I = 32,291$)	.001	−.01	.01	.89

Note. Simple effects are reported separately for symbolic liberals and conservatives when an interaction is significant ($p < .005$). LB = Lower Bound; CI = 95% Confidence Interval; UB = Upper Bound; N_S = state sample size; N_I = individual response sample size.

Thus, a greater percentage of confirmed state-level cases was not meaningfully associated with the political outcomes.

State-level COVID-19 deaths (log). We specified the number of confirmed COVID-19 deaths in the participant's state at the time they completed the study as the measure of threat (Table 9). No observed effect sizes for main or interaction effects reached the threshold for being meaningful. Thus, a greater number of state-level deaths was not meaningfully associated with political attitudes.

State-level COVID-19 deaths (log percentage). We specified the percentage of confirmed COVID-19 deaths in the population of the participant's state at the time they completed the study as the measure of threat (Table 10). A greater percentage of confirmed deaths was meaningfully associated with less conservative overall policy attitudes ($\beta = -.18$) and less conservative attitudes on the Wilson–Patterson Scale ($\beta = -.12$). No other observed effect sizes for main or interaction effects reached the threshold for being meaningful. Thus, a greater percentage of state-level deaths was not consistently associated with political attitudes in a meaningful way.

County-Level Results

County-level COVID-19 cases (log). We specified the number of confirmed COVID-19 cases in the participant's county at the time they completed the study as the measure of threat (Table 11). No observed effect sizes for main or interaction effects reached the threshold for being meaningful. Overall, a greater number of confirmed county-level cases was not meaningfully associated with political attitudes.

County-level COVID-19 cases (log percentage). We specified the percentage of confirmed COVID-19 cases in the population of the participant's county at the time they completed the study as the measure of threat (Table 12). No observed effect sizes for main or interaction effects reached the threshold for being meaningful. Overall, a greater percentage of confirmed county-level cases was not meaningfully associated with the political outcomes.

County-level COVID-19 deaths (log). We specified the number of confirmed COVID-19 deaths in the participant's county at the time they completed the study as the measure of threat (Table 13). No observed effect sizes for main or interaction effects reached the threshold for being meaningful. Overall, a greater number of county-level deaths was not meaningfully associated with political attitudes.

County-level COVID-19 deaths (log percentage). We specified the percentage of confirmed COVID-19 deaths in the population of the participant's county at the time they

Table 9. Summary of Main and Interaction Effects for Log Transformed State COVID-19 Deaths.

Analysis	r/β	LB CI	UB CI	p value
Main effect: state COVID-19 deaths log				
Voting intentions (N_S = 51, N_I = 8,302)	−.06	−.08	−.03	<.001
Overall policy attitudes (N_S = 51, N_I = 8,724)	−.06	−.08	−.04	<.001
Social policy attitudes (N_S = 51, N_I = 8,844)	−.01	−.03	.005	.16
Economic policy attitudes (N_S = 51, N_I = 8,829)	.02	.005	.04	.01
Wilson–Patterson Scale (N_S = 51, N_I = 8,822)	−.04	−.06	−.02	<.001
Explicit attitudes (N_S = 51, N_I = 35,350)	−.03	−.04	−.02	<.001
Implicit attitudes (N_S = 51, N_I = 31,756)	−.02	−.03	−.005	.005
Interaction effect: state COVID-19 deaths log × symbolic ideology				
Voting intentions (N_S = 51, N_I = 8,035)	.02	−.008	.04	.19
Overall policy attitudes (N_S = 51, N_I = 8,665)	.03	.01	.05	<.001
Liberals	−.11	−.14	−.08	<.001
Conservatives	−.05	−.08	−.02	.001
Social policy attitudes (N_S = 51, N_I = 8,724)	.03	.01	.05	<.001
Liberals	−.05	−.08	−.02	<.001
Conservatives	.01	−.01	.04	.28
Economic policy attitudes (N_S = 51, N_I = 8,718)	.03	.02	.05	<.001
Liberals	−.03	−.06	.006	.11
Conservatives	.04	.009	.07	.01
Wilson–Patterson Scale (N_S = 51, N_I = 8,721)	.03	.009	.04	.003
Liberals	−.07	−.10	−.04	<.001
Conservatives	−.02	−.05	.01	.18
Explicit attitudes (N_S = 51, N_I = 34,709)	.004	−.005	.01	.35
Implicit attitudes (N_S = 51, N_I = 32,291)	−.0008	−.01	.01	.88

Note. Simple effects are reported separately for symbolic liberals and conservatives when an interaction is significant ($p < .005$). LB = Lower Bound; CI = 95% Confidence Interval; UB = Upper Bound; N_S = state sample size; N_I = individual response sample size.

Table 10. Summary of Main and Interaction Effects for State COVID-19 Deaths (Percentage).

Analysis	r/β	LB CI	UB CI	p value
Main effect: state COVID-19 deaths log (%)				
Voting intentions (N_S = 51, N_I = 8,302)	−.08	−.12	−.04	<.001
Overall policy attitudes (N_S = 51, N_I = 8,724)	−.18	−.26	−.11	<.001
Social policy attitudes (N_S = 51, N_I = 8,844)	−.03	−.05	−.006	.01
Economic policy attitudes (N_S = 51, N_I = 8,829)	.01	−.006	.03	.18
Wilson–Patterson Scale (N_S = 51, N_I = 8,822)	−.12	−.18	−.06	<.001
Explicit attitudes (N_S = 51, N_I = 35,350)	−.09	−.13	−.05	<.001
Implicit attitudes (N_S = 51, N_I = 31,756)	−.02	−.03	−.008	<.001
Interaction effect: state COVID-19 deaths log (%) × symbolic ideology				
Voting intentions (N_S = 51, N_I = 8,035)	.04	.003	.07	.04
Overall policy attitudes (N_S = 51, N_I = 8,665)	.02	.003	.04	.02
Social policy attitudes (N_S = 51, N_I = 8,724)	.02	−.0004	.04	.055
Economic policy attitudes (N_S = 51, N_I = 8,718)	.02	.001	.04	.04
Wilson–Patterson Scale (N_S = 51, N_I = 8,721)	.0005	−.02	.02	.95
Explicit attitudes (N_S = 51, N_I = 34,709)	.0008	−.01	.01	.87
Implicit attitudes (N_S = 51, N_I = 32,291)	.0003	−.01	.01	.96

Note. LB = Lower Bound; CI = 95% Confidence Interval; UB = Upper Bound; N_S = state sample size; N_I = individual response sample size.

completed the study as the measure of threat (Table 14). A greater percentage of confirmed deaths was associated with less conservative explicit attitudes ($\beta = -.11$). No other observed effect sizes for main or interaction effects reached the threshold for being meaningful. Overall, a greater percentage of county-level deaths was not consistently associated with political attitudes in a meaningful way.

Table 11. Summary of Main and Interaction Effects for Log Transformed County COVID-19 Cases.

Analysis	r/β	LB CI	UB CI	p value
Main effect: county COVID-19 cases log				
Voting intentions (N_C = 1,205, N_I = 8,302)	−.06	−.09	−.04	<.001
Overall policy attitudes (N_C = 1,242, N_I = 8,723)	−.05	−.07	−.04	<.001
Social policy attitudes (N_C = 1,235, N_I = 8,844)	−.01	−.03	.01	.27
Economic policy attitudes (N_C = 1,235, N_I = 8,829)	.02	−.002	.03	.074
Wilson–Patterson Scale (N_C = 1,246, N_I = 8,822)	−.04	−.05	−.02	<.001
Explicit attitudes (N_C = 1,717, N_I = 35,349)	−.03	−.05	−.02	<.001
Implicit attitudes (N_C = 1,673, N_I = 31,755)	−.02	−.03	−.004	.008
Interaction effect: county COVID-19 cases log × symbolic ideology				
Voting intentions (N_C = 1,187, N_I = 8,035)	.02	−.008	.04	.19
Overall policy attitudes (N_C = 1,237, N_I = 8,664)	.03	.01	.05	.002
Liberals	−.10	−.12	−.07	<.001
Conservatives	−.04	−.07	−.01	.003
Social policy attitudes (N_C = 1,242, N_I = 8,724)	.03	.01	.05	.002
Liberals	−.04	−.07	−.01	.006
Conservatives	.02	−.007	.05	.15
Economic policy attitudes (N_C = 1,242, N_I = 8,718)	.03	.01	.05	.002
Liberals	−.02	−.05	.01	.19
Conservatives	.05	.02	.07	.002
Wilson–Patterson Scale (N_C = 1,244, N_I = 8,721)	.03	.01	.04	.002
Liberals	−.07	−.09	−.04	<.001
Conservatives	−.01	−.04	.009	.23
Explicit attitudes (N_C = 1,714, N_I = 34,708)	.01	.0001	.02	.05
Implicit attitudes (N_C = 1,683, N_I = 32,290)	−.0001	−.01	.01	.99

Note. Simple effects are reported separately for symbolic liberals and conservatives when an interaction is significant ($p < .005$). LB = Lower Bound; CI = 95% Confidence Interval; UB = Upper Bound; N_C = county sample size; N_I = individual response sample size.

Table 12. Summary of Main and Interaction Effects for Log Transformed County COVID-19 Cases (Percentage).

Analysis	r/β	LB CI	UB CI	p value
Main effect: county COVID-19 cases log (%)				
Voting intentions (N_C = 1,205, N_I = 8,302)	−.08	−.12	−.04	<.001
Overall policy attitudes (N_C = 1,242, N_I = 8,723)	−.07	−.09	−.04	<.001
Social policy attitudes (N_C = 1,235, N_I = 8,844)	−.02	−.04	−.01	.01
Economic policy attitudes (N_C = 1,235, N_I = 8,829)	.002	−.02	.02	.87
Wilson–Patterson Scale (N_C = 1,246, N_I = 8,822)	−.04	−.06	−.03	<.001
Explicit attitudes (N_C = 1,717, N_I = 35,349)	−.05	−.06	−.03	<.001
Implicit attitudes (N_C = 1,673, N_I = 31,755)	−.01	−.03	−.003	.02
Interaction effect: county COVID-19 cases log (%) × symbolic ideology				
Voting intentions (N_C = 1,187, N_I = 8,035)	.03	−.004	.07	.089
Overall policy attitudes (N_C = 1,237, N_I = 8,664)	.02	.001	.04	.04
Social policy attitudes (N_C = 1,242, N_I = 8,724)	.01	−.01	.03	.26
Economic policy attitudes (N_C = 1,242, N_I = 8,718)	.01	−.01	.03	.21
Wilson–Patterson Scale (N_C = 1,244, N_I = 8,721)	.005	−.01	.02	.58
Explicit attitudes (N_C = 1,714, N_I = 34,708)	.01	−.01	.02	.28
Implicit attitudes (N_C = 1,683, N_I = 32,290)	.005	−.01	.02	.46

Note. LB = Lower Bound; CI = 95% Confidence Interval; UB = Upper Bound; N_C = county sample size; N_I = individual response sample size.

Discussion

We investigated whether associations would emerge across various levels of analysis (country, state, county), domains (general, social, and economic), and people who differed in symbolic ideology (more liberal or conservative). We also examined associations with multiple assessments of political attitudes, including abstract attitudes (policy positions), concrete actions (voting intentions), deliberative responses (explicit attitudes), and reflexive responses

Table 13. Summary of Main and Interaction Effects for Log Transformed County COVID-19 Deaths.

Analysis	r/β	LB CI	UB CI	p value
Main effect: county COVID-19 deaths log				
Voting intentions (N_C = 1,205, N_I = 8,302)	−.07	−.11	−.04	<.001
Overall policy attitudes (N_C = 1,242, N_I = 8,723)	−.05	−.07	−.04	<.001
Social policy attitudes (N_C = 1,235, N_I = 8,844)	−.02	−.04	−.0005	.04
Economic policy attitudes (N_C = 1,235, N_I = 8,829)	.01	−.008	.03	.28
Wilson–Patterson Scale (N_C = 1,246, N_I = 8,822)	−.04	−.06	−.03	<.001
Explicit attitudes (N_C = 1,717, N_I = 35,349)	−.04	−.05	−.02	<.001
Implicit attitudes (N_C = 1,673, N_I = 31,755)	−.02	−.03	−.006	.004
Interaction effect: county COVID-19 deaths log × symbolic ideology				
Voting intentions (N_C = 1,187, N_I = 8,035)	.02	−.01	.05	.20
Overall policy attitudes (N_C = 1,237, N_I = 8,664)	.03	.007	.04	.008
Social policy attitudes (N_C = 1,242, N_I = 8,724)	.03	.009	.05	.005
Liberals	−.04	−.07	−.01	.008
Conservatives	.02	−.01	.05	.23
Economic policy attitudes (N_C = 1,242, N_I = 8,718)	.03	.01	.06	.001
Liberals	−.02	−.05	.01	.20
Conservatives	.05	.02	.08	.002
Wilson–Patterson Scale (N_C = 1,244, N_I = 8,721)	.02	.004	.04	.02
Explicit attitudes (N_C = 1,714, N_I = 34,708)	.008	−.002	.02	.13
Implicit attitudes (N_C = 1,683, N_I = 32,290)	.002	−.01	.01	.69

Note. Simple effects are reported separately for symbolic liberals and conservatives when an interaction is significant (p < .005). LB = Lower Bound; CI = 95% Confidence Interval; UB = Upper Bound; N_C = county sample size; N_I = individual response sample size.

Table 14. Summary of Main and Interaction Effects for Log Transformed County COVID-19 Deaths (Percentage).

Analysis	r/β	LB CI	UB CI	p value
Main effect: county COVID-19 deaths log (%)				
Voting intentions (N_C = 1,205, N_I = 8,302)	−.07	−.14	−.01	.052
Overall policy attitudes (N_C = 1,242, N_I = 8,723)	−.02	−.04	−.005	.01
Social policy attitudes (N_C = 1,235, N_I = 8,844)	−.03	−.05	−.01	.002
Economic policy attitudes (N_C = 1,235, N_I = 8,829)	−.005	−.02	.01	.62
Wilson–Patterson Scale (N_C = 1,246, N_I = 8,822)	−.08	−.13	−.03	.008
Explicit attitudes (N_C = 1,717, N_I = 35,349)	−.11	−.16	−.07	<.001
Implicit attitudes (N_C = 1,673, N_I = 31,755)	−.008	−.02	.004	.19
Interaction effect: county COVID-19 deaths log (%) × symbolic ideology				
Voting intentions (N_C = 1,187, N_I = 8,035)	.07	.001	.15	.075
Overall policy attitudes (N_C = 1,237, N_I = 8,664)	−.002	−.02	.02	.85
Social policy attitudes (N_C = 1,242, N_I = 8,724)	.004	−.02	.03	.73
Economic policy attitudes (N_C = 1,242, N_I = 8,718)	.006	−.02	.03	.62
Wilson–Patterson Scale (N_C = 1,244, N_I = 8,721)	−.005	−.02	.01	.59
Explicit attitudes (N_C = 1,714, N_I = 34,708)	.0001	−.01	.01	.98
Implicit attitudes (N_C = 1,683, N_I = 32,290)	.0005	−.01	.01	.94

Note. LB = Lower Bound; CI = 95% Confidence Interval; UB = Upper Bound; N_C = county sample size; N_I = individual response sample size.

(implicit attitudes). Although we observed three meaningful associations (two state-level and one county-level) between COVID-19 threats and less conservative attitudes, no operationalizations of COVID-19 reached our predetermined threshold for being consistently associated with political attitudes in a meaningful way (i.e., had effect sizes greater than $r/\beta > .10$ occurring for more than half of the outcomes).

The present findings suggest that threats from the COVID-19 pandemic did not systematically correspond to changes in the political attitudes of our sample. These findings potentially hold theoretical implications for understanding the relation between threat and political views. However, they should also be considered in relation to the time, place, and sample in which they occurred. We consider these points below.

Theoretical Implications

These findings highlight the lack of a consistent and meaningful association between COVID-19 threats and the political attitudes of a sample of U.S. residents. Findings were unsupportive of both primary (preregistered) and competing predictions (see Table 1), which were each derived from prominent theoretical perspectives on the association between ideology and threat: the uncertainty-threat model (Jost et al., 2017), ideological affordances (Brandt et al., 2021; Eadeh & Chang, 2020), and threat compensation (Burke et al., 2013; Proulx et al., 2012). As a result, the lack of consistent and meaningful associations that we observed holds theoretical value and suggests that current models might not be sufficient to understand the impact of the COVID-19 pandemic on people's political views. Specifically, we consider two possible, and not mutually exclusive, explanations.

First, these findings might support the idea that some threats are not strongly linked to operational political attitudes in a particular ideological direction. For example, threats to physical safety might most directly correspond to "rally 'round the flag" effects, in that threat increases positive attitudes toward current leaders and national symbols regardless of the attitude's ideological direction (Crawford, 2017; Lambert et al., 2019). Greater support for leaders and symbols potentially allows people to experience collective unity and in turn reduced threat. Some initial evidence indicates increased support for current political leaders after (vs. before) the start of the pandemic, potentially regardless of whether the leader was liberal or conservative (Yam et al., 2020). Thus, threats from the pandemic might have wielded an impact on people's feeling about extant leaders rather than their policy attitudes.

At the same time, our measure of voting intentions might have indirectly gauged people's intent to vote for Trump, as he was the presumptive Republican nominee. Trump was also the U.S. president (i.e., the country's leader) at the time the data were collected, yet we did not observe consistent and meaningful associations between COVID-19 threats and the voting intentions measure. It is possible that attitudes toward Trump were already highly calcified and therefore less responsive to environmental threats given both his incumbent status and the polarized nature of the American electorate during the start of the pandemic (Pennycook et al., in press). Regardless, our findings suggest that further inquiry is needed into whether and when threats, including COVID-19 threats, correspond to support for leaders who espouse various political views.

Second, although no operationalizations of COVID-19 threat reached our predetermined criteria for being considered consistent predictors in a meaningful way, it is nonetheless interesting that patterns of effects across all levels of analysis tended to trend in the direction of greater threat being associated with less conservative attitudes, except on economic policy issues. In addition, findings from exploratory models that excluded covariates (see Note 9) indicated that greater threat was often associated with less conservative political attitudes within all levels of analysis. It is possible that the psychological factors activated through pandemic threats (e.g., anxiety) nudged people toward less conservative policy views. However, the specific patterns we observe do not fully align with expectations from any current perspectives proposing when threats might produce less conservative attitudes (e.g., Brandt et al., 2021; Eadeh & Chang, 2020). Thus, while theoretical avenues considering what types of threats might prompt less conservative attitudes are likely worthwhile, further specification is needed in future research.

Limitations and Alternative Explanations

Here, we conducted multilevel models that accounted for state- and county-level nonindependence in responses. However, a limitation of these models is that they did not account for spatial dependence (e.g., political attitudes being similar among neighboring states), which can lead to biased parameter estimates (Ward & Gleditsch, 2018). Spatial dependence can be accounted for through spatial regression, which consists of creating a spatial lag variable for the outcome variable (e.g., economic attitudes) in the model and then including this lag variable as an additional fixed effect predictor in the model. This approach has been previously used to account for spatial dependence when examining correlates of COVID-19 cases and deaths (e.g., Webster et al., 2021). We created a lag variable for state- and county-level models through averaging scores on the outcome variable that were spatially dependent with a participant's location (i.e., shared a land or water border with another state or county), and then used this variable to conduct spatial regression models. Overall, we did not consistently observe any meaningful relationships between operationalizations of COVID-19 threat and political attitudes when accounting for spatial dependence, indicating that findings aligned with those reported in the main text. A detailed description of these models (pp. S132–S133) and results (Tables S55–S72) can be found in the online Supplemental Material.

It is also important to contextualize the observed findings and consider alternative explanations. First, conservatives view the COVID-19 virus as less severe than do liberals (Calvillo et al., 2020). This is potentially attributable to the highly politicized nature of the pandemic in the United States during 2020, in which many conservative politicians and news pundits downplayed the threat severity of COVID-19 relative to their liberal counterparts (Green et al., 2020; Motta et al., 2020; Pennycook et al., in press). We did not assess subjective aspects of threat, and conservatives might not have experienced the degree of threat necessary to impact political attitudes. However, we also did not observe that associations between COVID-19 threat and political attitudes consistently differed between liberals and conservatives in a meaningful way. In other words, we failed to consistently find any of the proposed effects at a meaningful magnitude even among liberals, who presumably perceived the pandemic as threatening.

In addition, we conducted exploratory analyses in which we examined whether associations with COVID-19 threats varied across participant race, as non-White individuals have been found to be more susceptible to the threats of the pandemic than White individuals (Boulware, 2020; Webster et al., 2021). Here, conclusions were the same as those of analyses in the main text: We did not consistently observe meaningful relationships between operationalizations of COVID-19 threat and political attitudes when accounting for participant race, and race did not consistently interact with threats to predict political attitudes in a meaningful way. A detailed description of these models (p. S192) and results (Tables S73–S96) can be found in the online Supplemental Material.

Overall, it seems unlikely that differential perceptions of threat between liberals and conservatives fully account for our findings. It is also important to highlight that our approach of examining the impact of objective threats without additional assessment of subjective appraisals is consistent with past work exploring the role of various objective threats (e.g., disease threats) on political attitudes (e.g., Inbar et al., 2016; O'Shea et al., in press). Nevertheless, it is possible that differences in the politicization of the pandemic across countries (e.g., the United States compared with Canada) could help account for variation in findings across geographic contexts, and we hope that this idea could be more directly explored in future research.

Second, the lack of meaningful associations between COVID-19 threats and operationally conservative attitudes might have emerged due to differences in the political attitudes of people who completed the study over time. For example, individuals who participated later (vs. earlier) in the study's time frame, when threats were worse (e.g., more confirmed cases), might have held more liberal attitudes prior to the pandemic. That is, if more liberal participants had a greater likelihood of participating later in the study, but this increase in sample liberalism was counteracted by greater conservatism produced through pandemic-related threats, analyses would produce a null result masking an actual effect. We view this explanation as unlikely, as (a) the competing influences of greater sample liberalism and higher conservatism from pandemic-related threats would need to be very comparable in magnitude, and (b) shifts in sample liberalism would have needed to be area-specific and almost perfectly titrated to differential increases in cases across locations.

Third, it is worth noting that these data came exclusively from Project Implicit, meaning the sample was comprised entirely of volunteer participants who were likely interested in issues related to intergroup bias. Although Project Implicit samples have been used productively in the past to identify the individual-level impacts of state- and country-level policy changes (Ofosu et al., 2019), the current sample was not representative of the American public, which may limit the generalizability of these claims. For example, Project Implicit participants are typically younger than members of the general public. If younger participants may have been less concerned about the COVID-19 pandemic, it may have weakened or even nullified any effects of COVID-19 related threats on political attitudes. In general, this work did not support predictions derived from prominent theoretical perspectives on the role of threat in political behavior, and while some prior studies used to support such perspectives also relied on convenience samples, more definitive answers to these questions will come from similar analyses that could be conducted on representative samples.

Conclusion

Overall, the present work contributes to building a systematic understanding of when threats might (or might not) be associated with political attitudes. The findings also provide practical insight into potential solutions for societal problems. For example, our findings suggest that greater threat did not meaningfully correspond to support for economic policies that would ameliorate economic declines observed during the pandemic. As a result, policy makers might face an uphill battle as they work to implement changes that would rectify ongoing societal challenges. We hope that this research motivates further debate about people's political attitudes during the pandemic, as well as more generally about the interplay of threat and politics.

Declaration of Conflicting Interest

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Open Practices

The preregistration plan for the analyses and approach to interpreting results can be found at https://osf.io/nxfks/. Deidentified data, measures, analytic code, and online Supplemental Materials can be found at https://osf.io/fxgtc/.

Supplemental Material

Supplemental material is available online with this article.

Notes

1. Two participants who reported that they were American residents had zip codes indicating that they were in Puerto Rico at the time of completing this study. We included these participants in analyses to maintain consistency with the inclusion criteria of our preregistration plan.
2. Information on the number of responses included in analyses for each measure can be found in the online Supplemental Material (Table S2).
3. Due to a programming error, one item from this measure was administered twice.
4. We also preregistered an analysis to examine county-level information about how far people went outside their home and the amount of time people spent at home. However, upon further reflection we decided that these constructs would be unlikely to capture threat and as a result did not complete these analyses. Because it is unlikely that people knew how frequently people in their community left their residence or how far they traveled when doing so, these measures were poor operationalizations of threat. For this reason, we opted not to analyze county-level information about movement or time at home.
5. Our preregistration plan did not indicate that we would use log transformed variables. In the spirit of transparency, we also report results of models using non-log transformed variables in the online Supplemental Material (Tables S30–S39). Overall conclusions are the same regardless of the approach used.
6. Models including COVID-19 deaths as a predictor were not part of our preregistered analyses. We appreciate the recommendation from an anonymous reviewer and the editor to examine associations with deaths.
7. Information on the percentage of participants in each state who completed the study before and after their state's stay-at-home order was implemented can be found in the online Supplemental Material (Table S1).
8. We also examined voting intentions using linear models. Conclusions are the same as those in the main text (online Supplemental Material Tables S40–S42). Model information and results can be found in the online Supplemental Material.
9. We also conducted main effect models without covariates (online Supplemental Material Tables S43–S54). Within these models, the implementation of country travel recommendations, as well as higher state-level cases (log transformed percentage), state-level deaths (both log transformed raw number and percentage), and county-level cases and deaths (both log transformed raw number and percentage), met the threshold for being consistently associated with less conservative attitudes in a meaningful way. All other conclusions remain the same as those in the main text.
10. In models testing interaction effects, we included both the main effect of the covariate and two-way interactions between the covariate and the constituent variables of the main interaction of interest. Doing so reduces bias in the parameter estimate of the interaction of interest (Yzerbyt et al., 2004). The full set of predictors in all models is outlined in the Supplemental Material tables.
11. Tables presenting full results for all predictors in models can be found in the online Supplemental Material.

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