Journal of Experimental Psychology: General

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Online First Publication, January 12, 2017. http://dx.doi.org/10.1037/xge0000261

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0096-3445/17/$12.00 http://dx.doi.org/10.1037/xge0000261

Know Thy Enemy: Education About Terrorism Improves Social Attitudes Toward Terrorists

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Hatred of terrorists is an obstacle to the implementation of effective counterterrorism policies—it invites indiscriminate retaliation, whereas many of the greatest successes in counterterrorism have come from understanding terrorists' personal and political motivations. Drawing from psychological research, traditional prejudice reduction strategies are generally not well suited to the task of reducing hatred of terrorists. Instead, in 2 studies, we explored education's potential ability to reduce extreme negative attitudes toward terrorists. Study 1 compared students in a college course on terrorism (treatment) with wait-listed students, measuring prosocial attitudes toward a hypothetical terrorist. Initially, all students reported extremely negative attitudes; however, at the end of the semester, treatment students' attitudes were significantly improved. Study 2 replicated the effect within a sample of treatment and control classes drawn from universities across the United States. The present work was part of an ongoing research project, focusing on foreign policy and the perceived threat of terrorism; thus classes did not explicitly aim to reduce prejudice, making the effect of treatment somewhat surprising. One possibility is that learning about terrorists "crowds out" the initial pejorative associations—that is, the label *terrorism* may ultimately call more information to mind, diluting its initial negative associative links. Alternatively, students may learn to challenge how the label *terrorist* is being applied. In either case, learning about terrorism can decrease the extreme negative reactions it evokes, which is desirable if one wishes to implement effective counterterrorism policies.

Keywords: prejudice, education, terrorism, attitudes

Supplemental materials: http://dx.doi.org/10.1037/xge0000261.supp

The most prominent and prolific terrorist groups today—from ISIS to Al-Qaeda to Boko Haram—welcome our hatred as a key part of their strategy, inviting indiscriminate retaliation that polarizes communities and drives up support for their extreme ideologies and tactics (Kydd & Walter, 2006; Lake, 2002). States driven to pursue these indiscriminate policies have either failed to eliminate terrorist attacks, or have even increased them (Cronin, 2009). By contrast, most successful counterterrorism and counterinsurgency campaigns have involved breakthroughs in understanding the motivations, organization, and strategies of terrorist and insurgent groups. For instance, in the late 1990s, India's government and police were able to fragment and beat back Islamist militants in Kashmir by recognizing cleavages in the insurgency and flipping their former adversaries to fight on their side (Staniland, 2012). In 2006, the United States located and killed the former leader of Al-Qaeda in Iraq (now ISIS)—Abu Musab Al-Zarqawi—after American intelligence officers interviewed suspects and members of the community to understand the motivations and social ties of the terrorist network (Alexander & Bruning, 2011). The most dramatic example may be Northern Ireland, where centuries of sectarian violence were effectively ended—not through the extermination of all insurgents, but through the inclusion of Sinn Féin (the political wing of the Irish Republican Army [IRA]) in negotiations and the political process, culminating in the Good Friday Agreement of 1998. To effectively combat terrorism, states must understand their adversary as a rational actor who is sustained by recruits, funding, and sanctuary, and who is motivated by political objectives; hatred of terrorists, in either policymakers or the citizens that elect them, is an obstacle to this aim, and may lead to policies that are the exact opposite of what effective counterterrorism strategy demands. Ironically then, to combat terrorism, we must find ways to reduce prejudice against terrorists.

Techniques for Reducing Prejudice

Prejudice reduction is one of the most prolific areas of research in social psychology; yet, applying it to terrorists or, for that matter, many real-world contexts is difficult for a number of

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This work was funded by a Research Across Departments and Schools (RADS) Grant from the Boston College Provost's Office.

We thank Philip Collins, Katie Frake, Larisa Heiphetz, Joshua Kertzer, Peiyan Liu, Damian Mencini, Kevin Miranda, Natalia Peña, Todd Rogers, James Russell, Benjamin Seo, Joseph Young, Yael Ziera, and the National Consortium for the Study of Terrorism and Responses to Terrorism at the University of Maryland, in addition to all participating students and professors.

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reasons (Paluck, 2016; Paluck & Green, 2009). "Implicit attitudes" paradigms have revolutionized our understanding of prejudice, showing that even those who reject explicit prejudice continue to show a measurable bias toward outgroups (Greenwald, McGhee, & Schwartz, 1998; Lai et al., 2014). But this research typically focuses on prejudice against marginalized groups (e.g., African Americans, disabled people), where public displays of prejudice are already socially unacceptable. Researchers have rarely turned their attention to the "fundamental challenge" of "[discovering] ways of changing 'hard-core' [or extreme] prejudiced beliefs" (Monteith, Zuwerink, & Devine, 1994), such as explicit racist beliefs or the extreme prejudice instilled by the label *terrorist*. A similar problem is present in the "minimal-groups" paradigm, where researchers instill and then attempt to reduce prejudice between "teams" that were formed on the basis of irrelevant traits (and in actuality were randomly assigned; Tajfel, 1970). This method provides tight experimental control, but the prejudice being studied lacks a real world historical context; it occurs in the absence of any competing or complicating influences. Furthermore, prejudice in the context of minimal groups is typically defined as preference for one's own team, rather than outright intergroup hostility (Paluck & Green, 2009). Finally, classic approaches to prejudice reduction suggest that prejudice can be reduced by facilitating contact with the outgroup under ideal conditions (Allport, 1954; Pettigrew, & Tropp, 2006; Sherif, Harvey, White, Hood, & Sherif, 1961). Unfortunately, for prejudice against terrorists, this approach is practically and politically unfeasible—most citizens will never interact with a terrorist personally; yet, their attitude toward them remains politically important.

Education, as a technique for prejudice reduction, has the potential to overcome the limitations of the approaches earlier. It is uniquely positioned to reduce prejudice where explicit antipathy is present, and where parties cannot be physically brought together—this includes (but is not limited to) the case of prejudice against terrorists. People can learn about, and change their attitudes toward, people that they may never encounter. For instance, an education approach was implemented in the context of the Israeli–Palestinian conflict: a class of Israeli students studied the history of conflict in other countries (Lustig, 2002; Salomon, 2004). End of term essays, written by treatment students, were more equitable to both sides of the Israeli–Palestinian conflict and more likely to be written from the first-person perspective of Palestinians (although the treatment had no effect on students' explicit prejudice against Palestinians). This, and other work (Gurin, Peng, Lopez, & Nagda, 1999; Schaller, Asp, Rosell, & Heim, 1996), gives some reason to believe that education-based prejudice reduction can be effective.

Another advantage of an education-based approach is that prejudice reduction is both tested and implemented in the same context—the classroom. In such field-based research, statistically significant effects are difficult to identify, but their ecological validity can generally be trusted, as they must emerge from an environment full of noise and competing influences. Many educational-based interventions are field studies, yet few use well-controlled designs—including control groups, or ideally as-if randomization—that allow for inferences about the causal effect of treatment. According to a recent review, fewer than 12 of 207 quasi-experimental studies had designs that licensed causal inferences (Paluck & Green, 2009; but see Broockman & Kalla, 2016). Given the lack of field-based, experimental prejudice reduction research, conclusions drawn from the present work may also have implications for prejudice reduction more generally. Reducing antipathy toward terrorists (for the purposes of counterterrorism) may be taken as a case study in reducing extreme and explicit antipathy, and it may be that our findings can be applied to other cases (such as explicit racism, or sectarian hatred).

Present Work

The present work was performed in the context of a larger ongoing research project, exploring the impact of education on attitudes concerning terrorism and foreign policy (thus, the majority of the survey was not focused on attitudes toward terrorists, and classes generally focused on counterterrorism, as opposed to tolerance). In this context, we had the opportunity to explore education's potential role in reducing prejudice toward terrorists. In Study 1, we performed an as-if randomized study, taking advantage of randomized course registration times at our university. Study 2 replicated the effect in a more representative sample, comparing treatment and control classes at 11 universities across the United States.

Because our study was performed in the context of a larger ongoing project, the courses had no explicit antiprejudice aim. Main themes of the course used in Study 1 were (a) the individual and group level causes and objectives of terrorism; (b) the methods and mechanisms of terrorism; (c) discussion of recent and ongoing conflicts, such as conflict with Al-Qaeda and the insurgencies in Iraq and Syria; and (d) counterterrorism and counterinsurgency strategy (for a complete list of course readings in Study 1, see the Appendix in online supplemental materials). Prior work has focused on teaching tolerance—for example, teaching white students about the positive role of intergroup conflict in democratic society, and then tracking their attitudes toward students of color across their university tenure (Gurin et al., 1999)—however, we had no intention of teaching students to tolerate terrorists. Students were simply taught about terrorism, and completed surveys at the beginning and end of the class, allowing us to track any changes in their attitude. It is possible then, that students, or even professors, might show a confirmation bias (Haidt, 2001; Kuhn, 1991; Kunda, 1990; Wason, 1960)—students might only learn, or professors might only teach, information that is consistent with their initial view of terrorists (for an example in the context of the Israeli–Palestinian conflict, see Gvirsman et al., 2016). For instance, political conservatives, who are generally more threat-sensitive (for review see Jost & Amodio, 2012), may attend to the most threatening information taught and resist any positive effect of treatment. Likewise, professors may lead their students to adopt their personal viewpoint by consciously or unconsciously presenting selective information about terrorists. In both studies, we explore these possibilities, examining biases based on political orientations, self-reported willingness to learn, students' initial attitudes, and even the views of the teaching professors.

Study 1

Method

Participants. Fifty-eight students (M_{Age} = 21.3, SD_{Age} = 0.9, 34 female, 2 unspecified; Table 1) were given preclass and

Table 1
Study 1 Classes, Response Rates, and Demographics

School	Professor	Class Name	Semester	Enrollment	Responses	Gender	Age	Political orientation
Boston College	P. Krause	Terrorism, Insurgency, and Political Violence	Fall, 2013	17	17	9 female 8 male	$M = 21.1$ $SD = .8$	$M = 3.7$ $SD = 1.4$
			Spring, 2015	18	18	15 female 3 male	$M = 21.6$ $SD = .5$	$M = 2.7$ $SD = 1.3$
		Wait-list	Fall, 2013	11	8	3 female 5 male	$M = 20.6$ $SD = 1.3$	$M = 3.1$ $SD = 1.8$
			Spring, 2015	15	15	7 female 2 unspecified 6 male	$M = 21.1$ $SD = 1.1$	$M = 2.5$ $SD = 1.2$

Note. Political orientation was measured on a 7-point scale (1 = *very liberal*, 7 = *very conservative*). For control samples, enrollment is the number of students who completed pre-class surveys, responses are the number of students who completed both pre- and post-class surveys.

postclass questionnaires at the beginning and end of the semester (Qualtrics software). Thirty-five students completed coauthor Peter Krause's class "Terrorism, Insurgency, and Political Violence" at Boston College (Fall, 2013, $n = 17$; Spring, 2015, $n = 18$); 23 students who were wait-listed for the same class (Fall, 2013, $n = 8$; Spring, 2015, $n = 15$) formed an as-if randomized control group. Wait-listed students had been randomized by the university to receive a later course registration time and had e-mailed coauthor Peter Krause to enroll in the class after it had been filled. The class was filled by 1:55 p.m. (Fall, 2013), and 9:32 a.m. (Spring, 2015) on the first of 8 days of registration, making it unlikely that student interest drove their allotment to the treatment or control group; put another way, it was reasonable to assume that wait-listed students would be in a treatment class if they had not been randomly assigned a late course registration time. The preclass questionnaire was completed on the first day of class, and the postclass questionnaire was completed 3 months later. Students were included if they completed both the preclass and postclass survey (response rate: 95.1%). In treatment classes, after both the preclass and postclass survey, 5 participating students were randomly awarded $10 Amazon.com gift cards. Students in the control group who completed both surveys received $20 Amazon.com gift cards. The Boston College Institutional Review Board approved the study, and informed consent was obtained from all participants.

Procedure and measures. Preclass and postclass surveys were identical. We collected responses for dependent measures, covariates of interest, and other items that were of interest for additional studies (see the online supplemental materials for a complete description). Questions related to social affiliation made up a small percentage of the total survey (one of six blocks, plus demographics), meaning that any attention drawn to terrorists' humanity was most likely diluted among questions about the threat, motives, and effectiveness of terrorists, as well as the effectiveness of counterterrorism policies. Relationships between social affiliation and the measures collected in the remaining blocks were not examined, to avoid introducing unnecessary comparisons in our analysis. Furthermore, although demand characteristics are always a concern, coauthor Peter Krause, who taught the course, was not responsible for the inclusion of the social affiliation measures and personally had no strong hypotheses about the direction of the effect (social affiliation measures were proposed by coauthor Liane Young). Despite this, we take a more direct approach to combatting demand characteristics in Study 2, testing whether results depend on the inclusion of data from Peter Krause's classes.

Dependent measures. Questions related to social affiliation were asked on a single page. Students read a brief introduction: "Suppose you met someone belonging to a group that had carried out at least one terrorist attack," and were then asked: "How much would you like this person?" ["liking"]; "How similar would you be to this person?" ["similarity"]; "How much would you get along with this person?" ["getting along"]; and "How much would you like to interact with this person?" ["interaction"] (1 = *not at all*, 7 = *very much*). This set of four questions formed our measure of social affiliation, provided that the questions were not differentially affected by treatment.

These questions showed good reliability ($\alpha_{Pre} = .78$; $\alpha_{Post} = .77$); however, we opted not to combine them into a scale in our analysis of treatment below. Our data were hierarchical (e.g., multiple observations from each student; students were clustered within classes), meaning that there was no simple way to model each observation as independent from all others. Instead, we adopted a mixed effects approach, which allowed us to respect this hierarchical design while also allowing that relationships among variables may not be uniform across levels of the design. In particular, because students provided four responses (at pretreatment and at posttreatment), we could allow that preclass social affiliation may predict postclass social affiliation differently for each student (see the statistical methods and random effects structure presented subsequently for more detail).

Covariates of interest. Students were asked to rate (a) their knowledge and (b) interest regarding terrorism (e.g., 1 = *I have no knowledge of/interest in the topic*, 7 = *I have a tremendous amount of knowledge about/interest in the topic*); (c) the likelihood that they would change their opinions on terrorism (1 = *very unlikely*, 7 = *very likely*); and (d) the confidence they had in their opinions (1 = *not confident at all*, 7 = *extremely confident*). At the end of the survey students completed a brief demographics form.

Statistical methods. Not all samples collected were independent. Data were collected across two semesters (Fall, 2013; Spring, 2015), meaning that groups of students could be subject to cohort effects; likewise, each student provided multiple measures of social affiliation. To address this, most analyses in this article uses linear mixed effects analyses (Baayen, Davidson, & Bates, 2008; Bates, Kliegl, Vasishth, & Baayen, 2015; Judd, Westfall, & Kenny, 2012), also commonly referred to as *hierarchical linear modeling*. This technique allows us to model and test the significance of dependencies within our sample—such as cohort effects, or the nonindependence of multiple data points from each student—and control for them when necessary. When dependencies were a nonsignificant source of variance, they were removed from the model to avoid overfitting, as per recent recommendations (Bates et al., 2015). We began with a full factorial model of our random effects structure and winnowed it to a parsimonious model using log-likelihood ratio tests, before testing for fixed effects of interest. The parsimonious model is reported subsequently, and necessary tests to derive it are reported in the online supplemental materials (see Table S1 in the online supplemental materials). We performed mixed effects analyses using R (R Core Team, 2015) and the lme4 package (Bates, Maechler, Bolker, & Walker, 2015) and obtained p values for fixed effects using the Kenward-Roger approximation of degrees of freedom, implemented in lmerTest (Kuznetsova, Brockhoff, & Christensen, 2015) and pbkrtest packages (Halekoh & Højsgaard, 2014). Following recent recommendations (Cumming, 2014), for key results, we report bootstrapped 95% confidence intervals (5,000 resamples) in square brackets using the bias corrected and accelerated method (BCa; Efron, 1987). We also use Welch's unequal variance *t* tests, in lieu of traditional student's *t* tests, to avoid imposing the assumption that variance is perfectly equal between groups (Moser & Stevens, 1992). Note that our results report noninteger degrees of freedom; for mixed effects analyses this reflects corrections for the nonindependence of observations, and for Welch's *t* tests this reflects corrections for unequal variance between groups.

Results

Pretest scores. As-if randomization placed students into treatment and wait-list (control) groups; however, it remained possible that the groups may differ on preclass measures. The groups did not differ on any attitudinal measures: "liking," $t(30.9) = 1.33$, $p = .194$; "similarity," $t(42.8) = 0.20$, $p = .843$; "getting along," $t(39.4) = 0.60$, $p = .550$; "interaction," $t(41.3) = 0.46$, $p = -.646$. Scores were generally low for all preclass attitudinal measures—$M_{\text{Preliking}} = 1.79$; $M_{\text{Presimilarity}} = 2.32$; $M_{\text{Pregetting along}} = 1.93$; $M_{\text{Preinteraction}} = 2.79$—and were all significantly below the scale midpoint: "liking," $t(56) = 15.93$, $p < .001$; "similarity," $t(56) = 9.06$, $p < .001$; "getting along," $t(56) = 14.21$, $p < .001$; "interaction," $t(56) = 4.42$, $p < .001$. Thus, at the beginning of the semester, attitudes were low, and equal between treatment and control groups. We also conducted combined placebo tests, using the random effects structure described subsequently for "effect of treatment." There was no interaction between treatment and question, $F(3, 165.0) = 0.24$, $p = .868$, so the parameter was removed from our model. In the resulting model, treatment and control groups did not differ on the combined measure of preclass social affiliation, $b = -0.23$, $t(54.9) = 0.73$, $p = .467$.

We compared treatment and wait-list groups on a number of additional covariates: openness to change, $t(51.6) = 0.37$, $p = .711$; interest, $t(40.5) = 0.66$, $p = .514$; confidence, $t(40.7) = 1.03$, $p = .307$; and knowledge, $t(49.0) = 0.78$, $p = .437$. We also compared treatment and wait-list groups on demographic measures: political orientation (1 = *very liberal*, 7= *very conservative*), $t(41.9) = 1.10$, $p = .277$; gender (male = 0; female = 1), $t(39.5) = 1.53$, $p = .135$; and age, $t(25.8) = 1.86$, $p = .075$. The marginal difference in age uncovered one potential limitation of our as-if randomization procedure—although course registration times are randomized within each student year, they are not randomized across them; college seniors are given priority above juniors, sophomores and freshman in registration, meaning that our treatment group is biased to contain more senior students ($M_{\text{Age:Treatment}} = 21.49$; $SD_{\text{Age:Treatment}} = 0.66$; $M_{\text{Age:Wait-list}} = 21.0$; $SD_{\text{Age:Wait-list}} = 1.19$). Given this, we report whether key results below are affected by the inclusion of student year as a covariate.

Random effects structure. Before testing fixed effects (e.g., treatment), we created a random effects structure, also commonly called a hierarchical linear model (Baayen et al., 2008). Each data point was nested within several levels—for example, student, semester—and by modeling each, when necessary, we could produce accurate estimates of effects that also generalize to a sampled population (e.g., to a population of university students). Effects may also vary across these levels; for instance, preclass attitudes may predict postclass attitudes better for some students more than for others. Working backward from a maximal model (Bates et al., 2015; Table S1), we arrived at the following parsimonious model:

$$\text{Attitude}_{\text{Post}} = 1 + (0 + \text{Attitude}_{\text{Pre}}|\text{Semester}) + (1 + \text{Attitude}_{\text{Pre}}|\text{Student})$$

Within our sample, there was significant variability in: (a) the by-semester relationship between preclass and postclass social affiliation, (Attitude $_{\text{Pre}}$ | Semester), $\chi^2(1) = 18.56$, $p < .001$; (b) the by-student relationship between preclass and postclass social affiliation, (Attitude $_{\text{Pre}}$ | Student), $\chi^2(1) = 7.62$, $p = .006$; and (c) by-student mean postclass social affiliation, (1 | Student), $\chi^2(1) = 27.57$, $p < .001$. Thus, our model allows that the relationship between preclass and postclass social affiliation differs for each semester and student, and that mean postclass social affiliation differs for each student.

Effect of treatment. We added fixed effects of interest to the random effects structure described above. First, we examined whether treatment differentially affected our four measures of postclass social affiliation (i.e., liking, getting along, similarity, interaction); the interaction between treatment and question was nonsignificant, $F(3, 154.0) = 1.37$, $p = .254$, and so social affiliation was defined as the combination of the four attitudinal measures. With the interaction term removed, there was a main effect of question, $F(3, 157.6) = 4.38$, $p = .005$, where some questions received higher ratings than others; however, critically, there was a main effect of treatment, where treatment students reported higher postclass social affiliation toward terrorists than toward wait-listed students, $F(1, 52.5) = 7.59$, $p = .008$, $b = .70$, [0.21, 1.19] (Figure 1; see Table S2 in the online supplemental materials). The main effect of treatment remained significant after controlling for student year, $F(1, 52.8) = 5.31$, $p = .025$, $b = .76$, [0.13, 1.43]. Thus, treatment students, relative to wait-listed students, reported having less extreme negative attitudes toward terrorists at the end of the semester.

Note that this effect of treatment did not depend on the specification of our random effects structure. In an ordinary least squares regression, predicting the average of our four postclass social affiliation measures ($\alpha = .77$), and including average preclass social affiliation ($\alpha = .78$), and student class year (freshman/

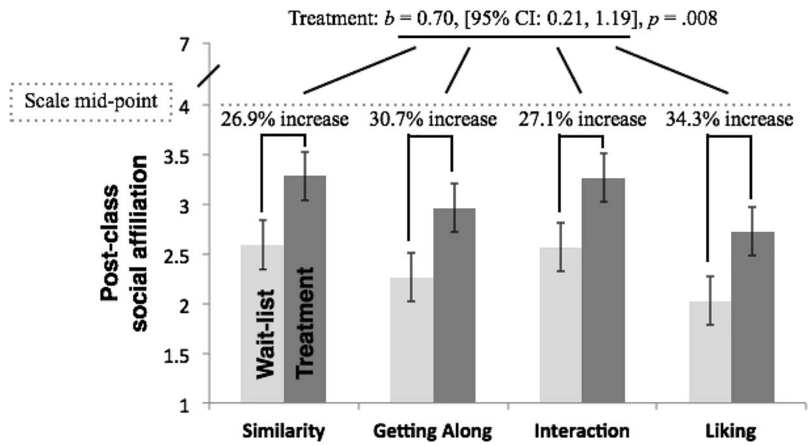

Figure 1. Study 1 main effect of treatment. 95% confidence interval computed using BCa method (Efron, 1987; 5,000 resamples). The scale midpoint for postclass social affiliation is marked with a dotted line. Percent increase represents the estimate for the treatment group relative to the wait-list group. Error bars represent standard error of the treatment coefficient.

sophomore/junior/senior/graduate) as covariates, the effect of treatment remained significant, $t(49) = 2.83$, $p = .007$, $b = .87$, [0.25, 1.49].

Potential moderators. It was possible that treatment might affect some students more strongly than others. As we had collected several measures of individual differences, we explored the interaction between treatment and preclass measures of (a) knowledge, (b) interest, and (c) opinion confidence. In no case was the interaction with treatment significant, ps $> .350$ (see Table S3 in the online supplemental materials). Thus, there were no obvious individual differences accounting for the effect of treatment—at the end of the semester, students who completed a course on terrorism, compared with those who were wait-listed, reported having less extreme negative attitudes toward terrorists.

Potential confirmation bias. Several covariates were of additional interest because they may reflect confirmation bias on the part of students. Treatment may be less effective for students who (a) initially reported extreme hostility toward terrorists (i.e., students with low preclass social affiliation), (b) initially reported being unwilling to change their minds about terrorists (i.e., low preclass openness to change), or (c) were more politically conservative. None of these covariates interacted with treatment (ps $>$ .280; see Table S4 in the online supplemental materials). Thus, in our sample, there was no evidence that treatment was affected by confirmation biases.

Discussion

Study 1 provided causal evidence (through as-if randomization) that participation in a course on terrorism improved students' initial (strongly negative) attitudes toward terrorists. This was surprising, as the course was not intended to teach students tolerance—the survey itself was part of an ongoing project to study politically relevant attitudes surrounding terrorism, and given this, coauthor Peter Krause had no explicit aim to reduce prejudice (for an example of the course readings in Study 1, see Appendix in the online supplemental materials). To ensure that the observed effects were not specific to Peter Krause's class, Study 2 aimed to replicate the effect within a larger sample, spanning professors, classes, and universities. Collecting a larger and more diverse sample also provided an opportunity to revisit the potential moderation of treatment by individual differences.

Study 2

Study 2 surveyed students in 31 classes, taught by 16 professors, at 11 universities across the United States. As-if randomization was not possible in this case; instead, treatment classes (classes teaching about terrorism; e.g., "Causes of Terrorism and Political Violence"; "Chemical, Biological, Radiological, and Nuclear Threats to the Homeland") were compared with control classes (classes covering topics only indirectly related to terrorism; e.g., "Causes of War"; "Theories of Peace and Conflict"). We were interested in whether the main effect of treatment would replicate within this more diverse sample.

Method

Participants. ($M_{Age} = 22.0$, $SD_{Age} = 4.9$; 189 female, 12 unspecified, 176 male; for full crosstabs see Table 2 and Table 3) completed a preclass and postclass survey, as described in Study 1. Students were recruited from classes across the United States over a period of 2 years ($N_{Classes} = 31$; $N_{Professors} = 16$; $N_{Universities} = 11$; $N_{Semesters} = 4$). We compared classes teaching about terrorism (treatment; $N_{Students:\ Treatment} = 249$; $N_{Classes:\ Treatment} = 20$; see Table 2), with classes covering topics only indirectly related to terrorism (control; $N_{Students:\ Control} = 128$; $N_{Classes:\ Control} = 11$; see Table 3).

To collect as many treatment and control classes as possible, we solicited professors to participate and included all who responded, categorizing each as treatment or control on the basis of syllabus content. Coauthor Peter Krause and three other professors taught classes at Boston College. Other classes were taught by professors who responded to a request for participants, circulated through the Study of Terrorism and the Prevention of Terrorism (START) professional listserver. Some of these professors were currently teaching courses related to terrorism, and some were currently

Table 2
Study 2 Treatment Classes, Response Rates, and Demographics

School	Professor	Class Name	Semester	Enrollment	Responses	Gender	Age	Political orientation
American University	Prof A	Causes of Terrorism and Political Violence	Fall, 2013	25	17	6 female 11 male	M = 20.3 SD = .9	M = 4.1 SD = 1.5
	Prof B	Psychology of Political Violence and Terrorism	Fall, 2014	23	7	2 female 5 male	M = 23.3 SD = 1.8	M = 3.9 SD = 1.2
American Military University	Prof C	Chemical, Biological, Radiological, and Nuclear Threats to the Homeland	Fall, 2013	22	2	1 female 1 male	M = 27.5 SD = .7	M = 5 SD = 1.4
		Senior Seminar in Homeland Security	Fall, 2013	13	3	3 male	M = 38 SD = 11.3	M = 5.3 SD = 1.2
		Introduction to Homeland Security and Defense	Fall, 2014	13	1	Omitted to protect anonymity.		
Boston College	Prof D	The History of Terrorism	Fall, 2014	75	56	29 female 26 male 1 unspecified	M = 20.5 SD = .7	M = 3.3 SD = 1.3
	Prof E	Terror and the American Century	Spring, 2014	28	12	4 female 8 male	M = 20.8 SD = .8	M = 4.1 SD = 1.4
	P. Krause	Terrorism, Insurgency, and Political Violence	Fall, 2013	17	17	9 female 8 male	M = 21.1 SD = .8	M = 3.7 SD = 1.4
			Spring, 2014	13	11	6 female 5 male	M = 21.4 SD = 3.3	M = 2.8 SD = 1.3
			Spring, 2015	18	18 (including 4 repeats)	11 female 3 male	M = 21.6 SD = .5	M = 2.9 SD = 1.4
		Introduction to International Studies	Spring, 2014	32	15	5 female 10 male	M = 19.3 SD = .5	M = 3.5 SD = 1.7
		International Studies Senior Seminar	Spring, 2014	12	12 (including 2 repeats)	6 female 4 male	M = 21.1 SD = .6	M = 2.9 SD = 1.4
Excelsior College	Prof F	International Terrorism	Fall, 2013	7	2	0 female 2 male	M = 42 SD = 2.8	M = 5.5 SD = .7
Georgia Institute of Technology	Prof G	The Challenges of Terrorism	Fall, 2014	23	12	4 female 7 male	M = 21.7 SD = 2.3	M = 3.1 SD = 1.2
Northeastern University	Prof H	Terrorism, Violence, and Politics	Spring, 2014	22	11	6 female 5 male	M = 29.1 SD = 12.6	M = 3.9 SD – .9
University of Denver	Prof I	International Terrorism	Spring, 2014	15	13	6 female 1 unspecified 6 male	M = 27.3 SD = 4.6	M = 4.2 SD = 1.5

(*table continues*)

Table 2 (*continued*)

School	Professor	Class Name	Semester	Enrollment	Responses	Gender	Age	Political orientation
University of Maryland	Prof J	Asymmetric Warfare	Fall, 2013	58	28	6 female 2 unspecified 20 male	$M = 22.4$ $SD = 3.9$	$M = 3.7$ $SD = 1.6$
	Prof K	Motivations and Intents of Terrorists and Terrorist Groups	Fall, 2014	10	6	1 female 5 male	$M = 29.2$ $SD = 7.0$	$M = 2.5$ $SD = 1.0$
Westwood College	Prof L	Terrorism (Class 1)	Spring, 2014	23	5	0 female 4 unspecified 1 male	$M = 22.8$ $SD = 2.8$	$M = 2.8$ $SD = 1.3$
		Terrorism (Class 2)	Spring, 2014	23	7	4 female 3 male	$M = 23.6$ $SD = 5.4$	$M = 3.1$ $SD = 1.2$

Note. Political orientation was measured on a 7-point scale (1 = *very liberal*, 7 = *very conservative*).

teaching other courses, creating a natural control group of professors who were knowledgeable about terrorism but not currently teaching it. Control classes at Boston College were selected to cover material in related subfields that excluded terrorism (i.e., international relations, security). Classes in which terrorism was studied for over three weeks were coded as treatment; otherwise classes were coded as control.

All classes were taught within political science, history, and international studies departments. No classes were taught within psychology departments or by psychology professors, and only one class included psychological readings related to prejudice reduction ("Psychology of Political Violence and Terrorism"; 7 students, comprising 1.9% of our total sample). Classes were generally small (28 of 30 classes had fewer than 36 students), and conducted as lecture-discussions. Treatment classes focused on topics like the causes, strategies, and effects of terrorism, whereas control classes focused on topics like the causes of war, crisis communication, and the politics of intelligence.

Thirteen students completed the survey in more than one class; entries beyond their first were excluded, and if two entries occurred within the same semester then only the treatment entry was retained. Within each class, after the preclass and postclass survey, 5 participating students were randomly awarded $10 Amazon.com gift cards—except at Georgia Tech, where a state ban prohibits gambling via random incentives, so all students received $20 gift cards for completing both surveys. Professors (with the exception of coauthor Peter Krause) who completed the survey received a $50 Amazon.com gift card. Institutional review board approval was obtained from each school, and informed consent was obtained from all participants.

The average response rate in Study 2 (52.4%) was lower than that reported in Study 1 (95.1%). This was not unexpected, as students would almost certainly be less motivated to complete a survey for a professor they did not know personally. Detailed comparisons between full and drop-off respondents were precluded, as informed consent was collected with the postclass survey (to avoid alerting students to the purpose of the survey). It was critical for us, however, that there were no differences in response rate across classes between the treatment and control group—Welch's unequal variance t test, $t(25.8) = 0.26$, $p = .796$. Likewise, there were no differences in class size between the treatment and control group—Welch's unequal variance t test, $t(28.8) = 0.40$, $p = .691$. Thus, although a higher response rate would be desirable, our treatment and control groups were well matched.

Procedure and measures. Preclass and postclass surveys were identical to those described in Study 1. Once again, measures of social affiliation (liking, similarity, getting along, and interaction) showed good reliability ($\alpha_{Pre} = .78$; $\alpha_{Post} = .79$), and again, we opted to avoid combing them into a scale in most analyses, favoring a mixed effects approach to estimate by-subject random slopes and intercepts (see statistical methods and random effects structure in the following text for more detail).

Statistical analysis. As in Study 1, not all samples were independent: data could potentially be clustered within students, professors, classes, universities, and semesters. Linear mixed effects analyses allowed us to examine effects while controlling for this variability when necessary (Baayen et al., 2008; Bates et al., 2015; Judd et al., 2012). Once again, we began with a full factorial

Table 3
Study 2 Control Classes, Response Rates, and Demographics

School	Professor	Class Name	Semester	Enrollment	Responses	Gender	Age	Political orientation
American University	Prof A	U.S. National Security and Civil Wars	Fall, 2014	13	6	2 female 4 male	M = 28 SD = 4.0	M = 2.5 SD = 1.0
Boston College	Prof M	Causes of War	Fall, 2013	35	11	5 female 6 male	M = 20.6 SD = .8	M = 3.6 SD = 1.8
			Fall, 2014	29	6	3 female 3 male	M = 20.8 SD = 1.0	M = 2.3 SD = .8
		Intelligence and International Security	Fall, 2013	36	14 (including 2 repeats)	9 female 3 male	M = 21.2 SD = .4	M = 3.6 SD = 1.7
		Modern Classics of International Relations	Fall, 2014	9	3	1 female 2 male	M = 20.3 SD = .6	M = 3.3 SD = 2.3
		United Nations and International Security	Fall, 2014	36	14 (including 1 repeat)	6 female 1 unspecified 6 male	M = 21.1 SD = .9	M = 3.2 SD = .8
	P. Krause	Research Methods and National Movements	Spring, 2014	16	14 (including 4 repeats)	6 female 4 male	M = 20.5 SD = 1.3	M = 3.4 SD = 1.6
University at Albany, SUNY	Prof N	Honors Course on Political Violence	Fall, 2013	23	17	13 female 2 unspecified 2 male	M = 18.6 SD = .8	M = 3.2 SD = 1.4
		Honors Course on Political Violence	Fall, 2014	25	21	16 female 5 male	M = 20.9 SD = 7.0	M = 3.3 SD = 1.5
University of Maryland	Prof O	Crisis Communication	Fall, 2013	33	13	11 female 2 male	M = 21.2 SD = 1.3	M = 3.2 SD = 1.7
Washington College	Prof J	Theories of Peace and Conflict	Fall, 2014	25	16	10 female 6 male	M = 21.1 SD = 3.6	M = 3.4 SD = 1.4

Note. Political orientation was measured on a 7-point scale (1 = *very liberal*, 7 = *very conservative*).

model of our random effects structure, and winnowed it to a parsimonious model before testing fixed effects of interest (see Table S5 in the online supplemental materials). For key results, we also report bootstrapped 95% confidence intervals in square brackets (5,000 resamples [BCa]; Efron, 1987). Noninteger degrees of freedom reflect corrections for the nonindependence of observations in mixed effects analyses, and for corrections based on unequal variance between groups in Welch's *t* tests.

Results

Pretest scores. Although we aimed to collect a representative control group, it remained possible that it might differ from treatment on preclass measures. There were no group differences for measures of social affiliation: liking, $t(201.7) = 1.54$, $p = .125$; similarity, $t(224.8) = 0.60$, $p = .547$; getting along, $t(227.7) = 0.95$, $p = .343$; interaction, $t(244.8) = 0.63$, $p = .527$. As in Study 1, preclass attitudes were low for all measures ($M_{\text{Preliking}} = 1.71$; $M_{\text{Presimilarity}} = 2.17$; $M_{\text{Pregetting along}} = 1.90$; $M_{\text{Preinteraction}} = 2.50$) and were all significantly below the scale midpoint: liking, $t(360) = 43.68$, $p < .001$; similarity, $t(361) = 27.58$, $p < .001$; getting along, $t(358) = 35.41$, $p < .001$; interaction, $t(361) = 15.6$, $p < .001$. We also conducted combined placebo tests, using the random effects structure described subsequently for effect of treatment. There was no interaction between treatment and question, $F(3, 1070.8) = 0.15$, $p = .929$, so the parameter was removed from our model. In the resulting model, treatment and control groups did not differ on the combined measure of preclass social affiliation, $b = -0.15$, $t(326.0) = 1.21$, $p = .226$. Thus, both treatment and control groups began the semester with the same strong negative attitudes toward terrorists.

We compared treatment and control groups on the remaining pretest covariates. Groups did not differ in openness to change, $t(236.8) = 1.00$, $p = .319$, knowledge, $t(216.6) = 0.94$, $p = .348$, or confidence, $t(246.2) = 1.02$, $p = .308$. Across groups, there were significant differences in interest, $t(266.1) = 4.86$, $p < .001$, which was expected given that treatment students chose to be in the course on terrorism. There were also differences in gender, $t(261.2) = 3.93$, $p < .001$, and age, $t(339.3) = 3.45$, $p < .001$, and a marginal difference in political orientation, $t(248.2) = 1.78$, $p = .076$, such that treatment students were more likely to be younger, female, and (marginally more) liberal. Given this, in the following text we report final models that include age, gender, political orientation, and "interest" as covariates, to ensure that key effects remain significant after controlling for these preexisting differences.

Random effects structure. As in Study 1, we created our random effects structure by beginning with a maximal model and working backward to remove nonsignificant random-effects components (Bates et al., 2015; see Table S5 in the online supplemental materials). We arrived at the following model:

$$\text{Attitude}_{\text{Post}} = 1 + (0 + \text{Attitude}_{\text{Pre}}|\text{Semester}) + (1 + \text{Attitude}_{\text{Pre}}|\text{Student}) + (1 \mid \text{Professor})$$

Within our sample, there was significant variability in (a) the by-semester relationship between preclass and postclass social affiliation (Attitude $_{\text{Pre}}$ | Semester), $\chi^2(1) = 133.7$, $p < .001$; (b) the by-student relationship between preclass and postclass social affiliation, (Attitude $_{\text{Pre}}$ | Student), $|^2(1) = 57.8$, $p < .001$; (c) by-student mean postclass social affiliation, $\chi^2(1) = 166.6$, $p < .001$; and (d) by-professor mean postclass social affiliation, (1 | Professor), $\chi^2(1) = 14.5$, $p < .001$. Thus, our model allows that the relationship between preclass and postclass social affiliation differs for each semester and student, and that mean postclass social affiliation differs for each student, and group of students taught by a professor.

Sources of variability in this model were the same as in Study 1, with the addition of the final term—(d) by-professor random-intercepts, which was not strictly necessary for our purposes. The aim of Study 2 was to replicate Study 1 in a more diverse sample, that is, it tested the claim that: treatment is generalizable beyond a sample of students, within a novel sample of professors. Controlling for by-professor random-intercepts actually tested an even stronger claim: that treatment is generalizable beyond a sample of students and beyond a sample of professors. Although the prospect of this outcome was exciting, it was also unlikely that our sample of only 16 professors would allow for this level of generalization. Thus, analyses of treatment including by-professor random intercepts are reported in online supplemental materials (see Table S6), and the following random effects structure was used in the following analyses.

$$\text{Attitude}_{\text{Post}} = 1 + (0 + \text{Attitude}_{\text{Pre}}|\text{Semester}) + (1 + \text{Attitude}_{\text{Pre}}|\text{Student})$$

Effect of treatment. We added fixed effects of interest to the random effects structure described above. Fixed effects included treatment, question (liking, similiarity, getting along, interaction), and preclass social affiliation (to control for any relationship not already captured by random effects). There was no interaction between treatment and question, $F(3, 972.3) = 1.75$, $p = .154$. With the interaction term removed, there was a main effect of question, $F(3, 972.1) = 10.9$, $p < .001$, and critically, a main effect of treatment, $F(1, 328.8) = 9.00$, $p = .003$, $b = 0.34$, [0.12, 0.55] (see Figure 2). To ensure that these results did not depend on the inclusion of coauthor Peter Krause, we removed his students from the sample (77 students, 20.4% of the total sample); the effect of treatment remained significant, $F(1, 251.11) = 4.89$, $p = .028$, $b = 0.27$, [0.03, 0.50]. Still excluding students taught by Peter Krause, treatment remained significant after controlling for age, gender, political orientation, and preclass interest, $F(1, 239.7) = 3.92$, $p = .049$, $b = 0.26$, [0.01, 0.52]. Thus, the effect of treatment observed in Study 1 was successfully replicated in a novel sample of professors.

Once again, our effect of treatment did not depend on the specification of our random effects structure. In an ordinary least squares regression, predicting the average of our four postclass social affiliation measures ($\alpha = .79$) and including treatment and preclass social affiliation ($\alpha = .78$) as predictors, the effect of treatment was significant, $b = 0.32$, $t(334) = 2.93$, $p = .004$. When school was added as a fixed effect (to control for differences in the probability of assignment to treatment/control), treatment remained significant, $b = 0.29$, $t(324) = 2.11$, $p = .036$. Thus, the effect of treatment was not dependent on our specifying a random effects model.

Potential moderators. As in Study 1, we explored whether individual differences moderated the effect of treatment, making it more or less effective. We explored interactions between treatment and preclass measures of (a) knowledge and (b) opinion confi-

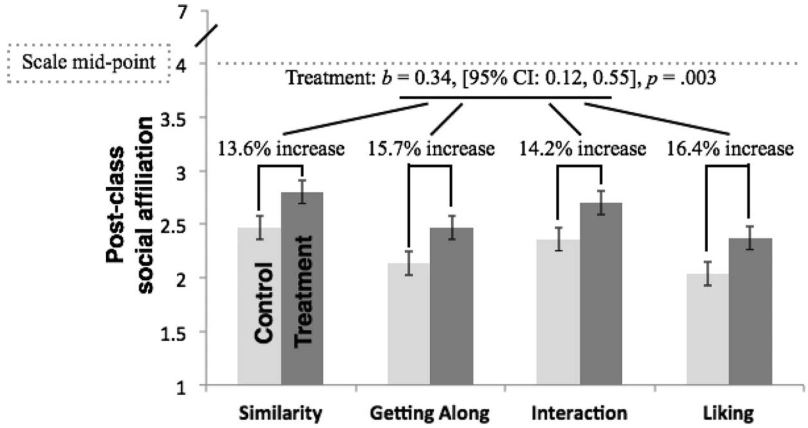

Figure 2. Study 2 main effect of treatment. Treatment remained significant after excluding students taught by coauthor Peter Krause (20.4% of total sample; $b = 0.27$, [0.03, 0.50], $p = .028$). The scale midpoint for postclass social affiliation is marked with a dotted line. 95% confidence interval computed using the BCa method (Efron, 1987; 5,000 resamples). Percentage increase represents the treatment rating relative to the wait-list group. Error bars represent standard error of the treatment coefficient.

dence. Interactions were nonsignificant ($ps > .71$; see Table S7 in the online supplemental materials).

Potential confirmation bias. As in Study 1, we expected that some individual differences might reduce our treatment's effectiveness on the basis of confirmation bias: (a) students' preclass openness to change, (b) students' and professors' preclass social affiliation, and (c) students' and professors' political conservatism. None of these covariates interacted with treatment ($ps > .245$; see Table S8 in the online supplemental materials). Thus, as in Study 1, confirmation biases neither interfered with, nor accounted for, the effectiveness of treatment.

General Discussion

Some of the most prominent terrorist groups today welcome hatred from opposing states and citizens as a means of provoking indiscriminate retaliation against their own communities (Kydd & Walter, 2006; Lake, 2002). This indiscriminate retaliation is at best ineffective, and at worst counterproductive (Cronin, 2009); it runs counter to the most effective counterterrorism policies, which stem from understanding terrorists as rational agents, acting in pursuit of political goals. Hatred of terrorists, in either policymakers or the citizens that elect them, is an obstacle for effective counterterrorism strategies. Education, as a prejudice reduction technique is well suited to reducing this hatred in this context. The present work tested whether students' initial extreme negative attitudes toward terrorists became less negative after they completed a college course on the topic (treatment). Studies of education-based methods for prejudice reduction rarely allow for causal inference (Paluck & Green, 2009), making the use of as-if randomization in Study 1 a particular strength of the present work. Study 1 demonstrated that education about terrorists increased students' social affiliation toward them: students became more willing to say they would "like," "get along with," were "similar to," and would "interact with," "someone belonging to a group that had carried out at least one terrorist attack" (see Figure 1). Study 2 replicated the effect within a sample of treatment and control classes drawn from 31 classes, taught by 16 professors, at 11 United States universities (see Figure 2). Students' attitudes did not become positive in either study (see Figures 1 and 2, means and error bars are nowhere near the scale midpoint); we consider this ideal—after treatment, students do not think positively of terrorists, but critically, they no longer hate them as they once did.

People are known to have a confirmation bias; they selectively attend to and remember information that reinforces their existing beliefs (Gvirsman et al., 2016; Haidt, 2001; Kuhn, 1991; Kunda, 1990; Wason, 1960). We initially hypothesized that treatment would be influenced by the confirmation biases of either students (as measured by their initial attitudes, their political orientations, or their self-reported willingness to learn) or professors (as measured by professors' political orientations or social affiliation toward terrorists). However, we found no evidence that confirmation biases affected treatment.

But, presumably, both students and professors do have confirmation biases—they are a well-established effect in social psychology (Haidt, 2001; Kuhn, 1991; Kunda, 1990; Wason, 1960). It is reasonable to assume that if confirmation biases could have exerted an influence then they would have, and their absence may assist speculation about the psychological mechanisms responsible for reducing prejudice. One possibility is that the effectiveness of treatment stems from general, rather than specific, knowledge; that is, if there were some specific piece of knowledge, some silver bullet, that could have changed a student's mind about terrorists, then he or she could have chosen to ignore it, or the professor could have neglected to teach it. By contrast, if the effectiveness of treatment depends on general knowledge, then it should be more difficult for confirmation biases to exert an effect—there is no specific piece of information that students (or professors) could either ignore or latch on to. Consistent with this, all our measures of social affiliation (e.g., "liking") asked students about a generic terrorist, as opposed to an individual from a particular group (e.g., ISIS or the IRA). If we had asked about a particular group then treatment might depend on specific knowledge about that group, such as the historical or social circumstances that motivated their attack.

But how exactly did education increase students' social affiliation toward terrorists? While prior work has reduced prejudice by providing positive examples of stigmatized outgroups or of intergroup interactions (e.g., Gurin et al., 1999), given that classes (particularly Study 1; see the Appendix in online supplemental materials) focused on counterterrorism and the causes, objectives, and methods of terrorism, it is less likely that positive information about terrorists was responsible for our effect. Professors taught their students about terrorism—they were not explicitly interested in fostering students' prosocial attitudes. Given that most students did not receive positive information about terrorists, is it possible that neutral information alone could dilute a strong initial prejudice?

Associative research provides a psychological framework that could account for this effect (Greenwald et al., 2002). In this framework, activating one concept calls associated concepts to mind, which are (to varying extents) positively or negatively valenced. At the beginning of the semester, students knew relatively little about terrorism; that is, the concept "terrorism" only possessed a small set of associative links to (mostly negative) related concepts (e.g. Al Qaeda, Osama bin Laden, ISIS, foreigners; Tuman, 2010). Thus, when *terrorism* was called to mind, only these few negative associations came to mind with it. Treatment classes, in our study, did not attempt to remove these initial negative associations, but they may have flooded the concept "terrorism" with new associative links (e.g., IRA, Weather Underground, specific political objectives of terrorist groups). Through learning about terrorism, students may come to associate it with so much that its strong pejorative connotations—the initial links—are diluted among the new associations they have learned.

This mechanism, if confirmed in future work, would be promising for other antiprejudice interventions, particularly as an alternative to methods that focus exclusively on positive counterexamples, where treatment can suffer from issues related to subtyping: positive counterexamples are represented as distinct from the more general category, and thus fail to reduce prejudice (Greenwald et al., 2002; Weber & Crocker, 1983). Theoretically, our proposed mechanism—diluting pejorative links among new associations—should be less likely to risk subtyping, as the central concept is not pressured by opposite positively and negatively valenced associations. Instead, the intervention may avoid putting pressure on the concept *terrorism* to split, and it may do this by using new associations that do not have a strong valence themselves (i.e., general knowledge about terrorism). This finding is consistent with Salomon's (2004) interpretation of the intervention in Lustig (2002), where Israeli students studied external conflicts, as opposed to their own. As in their intervention, the present work may allow students to learn about the nature of terrorism in less immediate and emotionally charged contexts. Consistent with this, Salomon notes that learning in this way could circumvent defenses, such as entrenchment in existing beliefs—an outcome we also observed in the present work.

Limitations and Extensions

Although we favor this psychological explanation, we allow that other mechanisms may also account for the effect of treatment. One possibility is that students learn to challenge whether the label *terrorist* is properly applied, treating the term as no more or less pejorative, but questioning whether its use is justified, or what its use actually tells them about the targeted group. *Terrorist* is a nebulous term, and although its public usage carries a clear negative connotation, its professional use is vigorously debated, to the point that the formal definition varies even across government departments within the United States (Hoffman, 2006). At the beginning of the semester, when students were told that an individual's group had committed a terrorist attack, they may have seen very little ambiguity in the statement; at the end of the semester they might ask, "What was the attack (e.g., what did it target, what were the aims), and who declared it a terrorist attack?" In its public usage, to apply the label *terrorist* is to implicitly make a moral judgment (Jenkins, 1985). In its professional usage, with which students may have become familiar, it becomes valid to ask whether the label is being properly applied—does this group fit the objective features that define terrorism? As students are exposed to a broader academic understanding of terrorism, they may become less likely to blindly accept that all usage of the term is appropriate.[1]

Although the implications of this explanation may be more specific to terrorism, rather than to antiprejudice research more generally, its importance should not be understated. Politicians have, at times, silenced meaningful debate by labeling their opponents *terrorists*, and students may now see through this rhetorical strategy. Leaders in Syria and Egypt today apply the label to much of their political opposition as a means of justifying increased executive powers and repressive policies (Black, 2012; "Egypt's Muslim Brotherhood Declared 'Terrorist Group'," 2013); in the post-9/11 United States, accused foreign terrorists can be held indefinitely without trial, (de Nies, 2011), while extreme environmentalists who committed arson can be labeled terrorists and sent to maximum security prisons for years (PBS News Hour, 2011). Indeed, the label of *terrorist* is one of the most powerful rhetorical tools in policy today, as invoking it can shift the treatment of suspects and prisoners, the focus of the media, and government funding and policies from a crime model to a war model (Miller & Gordon, 2014). Thus, shifting students' understanding and interpretation of the label could have serious political ramifications. Effectively, learning about terrorism may neuter it as a rhetorical tool to inspire hatred.

We must also acknowledge that our discussion is framed in terms of *hatred*, yet we did not explicitly measure hatred of

[1] Note that this explanation speaks to students' knowledge of the term *terrorism* and their avoiding a blind acceptance of it as necessarily pejorative. Alternatively, the effect could depend on which terrorist group students assumed to stand in for "a group that had carried out at least one terrorist attack," in our measures of social affiliation. For instance, knowing few terrorist groups initially, students may think the question must refer to ISIS or Al Qaeda, only to realize postclass that it could refer to many more groups. If this were the case, then students who can name more terrorist groups (particularly Western groups, such as environmental activists) should report more positive attitudes. Although not analyzed in preceding text, students were asked to name up to 10 terrorist groups pre- and postclass. Across our full Study 2 sample, the postclass number of correct groups was correlated with postclass social affiliation; however, the number of specifically Western groups was not. Furthermore, the number of correct groups did not eliminate the effect of treatment when modeled as a covariate (see the Results section in the online supplemental materials). Thus, though this explanation may describe a small component of treatment, it cannot completely account for it.

terrorists. The questions that were asked were less emotionally charged: specifically, whether students would "like," "get along with," "interact with," or were "similar to" someone who belongs to a terrorist group. Our concern was that asking students about hatred might introduce demand characteristics and prompt students to signal that they have the "correct" attitudes (which might be to denigrate terrorists or to renounce hate; in either case, students would report extremes on our scale and variance would be reduced). Although our measures do not specifically ask about hatred, they are reliable and collectively they can be used to measure positive and negative attitudes. Even after treatment, students' attitudes were significantly below the scale midpoint, suggesting that they maintained their initial negative attitudes, but that these attitudes were also less negative than they were before. Thus, students were hardly ever willing to say that they liked terrorists, but they varied in the strength of their objection to this prompt.

Finally, when we assert that the most effective counterterrorism strategies are based around understanding the enemy, one might object: Couldn't it be just as effective to brutally repress a population, at least until it is incapable of engaging in terrorist attacks? For instance, a major military effort defeated the Tamil Tigers in Sri Lanka; likewise, Russia has faced several insurgencies and has successfully repressed the majority of them, often through harsh measures such as mass deportation (Engelhardt, 1992). Whereas authoritarian tactics can be effective in some cases, they also carry a number of costs—beyond their moral repugnance—that make them less effective than democratic methods (Byman, 2016). Furthermore, these authoritarian methods are generally successful in counterinsurgency campaigns, where insurgents are organized, have strong support from the local community, and are geographically confined. By contrast, counterterrorism efforts must contend with loosely organized and geographically dispersed groups—Paris cannot be bulldozed or put under martial law until the threat of terrorism passes. The most proven counterterrorism methods rely on human interaction and communication (Lyall & Wilson, 2009), and we believe that the use of these methods will find more support when terrorists are less hated by the general population.

Conclusion

The threat posed by terrorism is one to be taken seriously. However, the greatest successes in counterterrorism have stemmed from an understanding of terrorists' personal and political motivations. Given this, hatred toward terrorists is an obstacle; it is actively counterproductive and may even lead to policies that increase attacks (Cronin, 2009). We found that learning about terrorism can decrease the extreme negative reactions it evokes. This suggests that knowing our enemies is an effective step toward defeating them.

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Received April 11, 2016
Revision received October 14, 2016
Accepted November 8, 2016 ■