



How and when does syntax perpetuate stereotypes? Probing the framing effects of subject-complement statements of equality

Kevin J. Holmes^a, Evan M. Doherty^b and Stephen J. Flusberg^c

^aDepartment of Psychology, Reed College, Portland, Oregon, USA; ^bDepartment of Psychology, Colorado College, Colorado Springs, Colorado, USA; ^cDepartment of Psychology, SUNY Purchase College, Purchase, New York, USA

ABSTRACT


Although subject-complement statements like “girls are as good as boys at math” appear to express gender equality, people infer a gender *difference*: the group in the complement position (boys) is judged superior. We investigated (1) whether this syntactic framing effect generalizes to other socially charged inferences and (2) whether awareness of the bias implied by the syntax mitigates its influence. Across four preregistered experiments ($N = 2,734$), we found reliable framing effects on inferences about both math ability and terrorist behavior, but only for the small subset of participants (~30%) who failed to identify the influence of the subject-complement statements on their judgments. Most participants *did* recognize this influence, and these participants showed reduced or even *reversed* framing effects; they were also more likely to explicitly judge subject-complement syntax as biased. Our findings suggest that this syntax perpetuates stereotypes only when people are oblivious to, or unmotivated to interrogate, its implications.

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“Girls are just as good at math as boys” (Rahhal, 2019), declared an online report publicizing a recent neuroimaging study of children’s mathematical processing (Kersey et al., 2019). This statement appears to express that boys and girls have equal math ability. After all, the girls in the study were “just as good” as the boys. Yet the syntactic structure of the statement insinuates otherwise: the *complement* position, occupied by “boys,” is typically reserved for the reference point—the standard of comparison for the item

CONTACT Kevin J. Holmes  kjholmes@reed.edu  Department of Psychology, Reed College, 3203 SE Woodstock Blvd, Portland, OR, 97202, USA.

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in the *subject* position, occupied by “girls” (Rosch, 1975; Tversky, 1977). By framing boys as the reference point, the statement may subtly perpetuate the very stereotype it promises to refute, as boys are stereotyped as having superior math abilities (Chestnut & Markman, 2018).

An extensive literature has shown that subtle aspects of syntax and other linguistic cues can shape beliefs and attitudes about socially relevant issues (e.g., Bruckmüller et al., 2012; Bruckmüller & Abele, 2010; Fausey & Boroditsky, 2010; Fausey & Matlock, 2011; Gleitman et al., 1996; Hegarty & Bruckmüller, 2013; Matlock et al., 2012; Thibodeau et al., 2017; Thibodeau & Boroditsky, 2011). These findings are grounded in basic principles of discourse processing. As people read a text or engage in conversation, they integrate the linguistic input with prior knowledge to construct a working mental model of the topic under discussion, using social-pragmatic reasoning to draw inferences about the speaker’s communicative intentions (Graesser et al., 1997; Sperber & Wilson, 1986). As a result, even slight differences in sentence structure or word choice can impact how people represent and reason about an issue by activating associated concepts (e.g., Thibodeau et al., 2017; Thibodeau & Boroditsky, 2011) or signaling a particular speaker perspective (e.g., McKenzie & Nelson, 2003).

For example, one line of work has examined how “figure-ground” or “subject-reference” framing shapes how people reason about social groups (Bruckmüller et al., 2012; Bruckmüller & Abele, 2010; Gleitman et al., 1996; Hegarty & Bruckmüller, 2013). Comparative statements like “psychologists use different methods than biologists” situate one group, in this case “biologists,” as the normative ground or reference point against which the figure or subject, “psychologists,” is being compared. Experiments have shown that people tend to infer that the reference group has more power, status, and agency than the subject group (Bruckmüller et al., 2012; Bruckmüller & Abele, 2010).

Recent evidence suggests that subject-complement statements of gender equality bias inferences about the intellectual ability of boys and girls in a similar fashion. In a study by Chestnut and Markman (2018), participants read a summary of scientific evidence for gender equality in math ability much like the report mentioned at the start of this paper. The summary contained several statements with “girls” and “boys” in the subject and complement positions, respectively (e.g., “girls performed as well as boys”), or with the positions of the two genders reversed (“boys performed as well as girls”). When subsequently asked to judge which gender is naturally more skilled at math or which gender must work harder to be good at math, participants consistently attributed superior ability to the gender in the complement position. An analogous syntactic framing effect was observed for judgments of verbal ability, a domain in which girls are

stereotyped as more skilled. These findings suggest that subject-complement statements of gender equality lead people to infer a gender *difference*. In the case of math ability, such statements reinforce the common gender stereotype when boys are framed as the reference point and counteract it when girls are framed as the reference point.

Chestnut and Markman's (2018) findings leave open several key questions, however, that form the focus of the present work. First, do the framing effects of subject-complement syntax generalize beyond judgments of boys' and girls' intellectual ability to inferences about stereotyped social groups in other domains? Subject-complement statements can be used to express that two groups are ostensibly equivalent on a variety of dimensions. Consider the statement "Christians are just as likely as Muslims to be terrorists." This might be uttered with the well-meaning intention of challenging the stereotype that Muslims are prone to committing terrorist acts—a stereotype prevalent in post-9/11 U.S. society (Sides & Gross, 2013) despite being erroneous (START (National Consortium for the Study of Terrorism and Responses to Terrorism), 2018). But because the statement frames Muslims as the reference point for terrorism, it may inadvertently reinforce this stereotype. If subject-complement statements of "equality" in terrorist behavior make people infer that Muslims and Christians *differ* in such behavior, this would provide a powerful demonstration of the potential for this syntax to shape people's thinking about a range of socially charged issues.

Second, if subject-complement syntax shapes judgments in general, how explicit is this influence? Are people who are consciously aware of the bias transmitted by the syntax just as influenced by it as those who are not? Other forms of linguistic framing have been shown to operate outside of conscious awareness (e.g., Robins & Mayer, 2000; Thibodeau & Boroditsky, 2011, 2013). For example, Thibodeau and Boroditsky (2011) found that people's preferred crime mitigation strategies depended on whether crime was framed metaphorically as a "beast" or a "virus." Yet when asked to provide a rationale for their judgments, the vast majority of participants cited seemingly more substantive information like the crime statistics in the report, not the metaphor. In another study, Thibodeau and Boroditsky (2013) found that less than half of participants were able to recall which crime metaphor they had seen minutes earlier, yet those who could not recall the metaphor were just as affected by it as those who could (though other evidence suggests that explicit memory can play a role in metaphor framing; Flusberg et al., 2020).

It is unclear whether the influence of subject-complement syntax is similarly covert. When directly asked whether "Girls do as well as boys at math" is biased against girls, Chestnut and Markman's (2018) participants judged this statement to be relatively egalitarian overall. This suggests that people may be insensitive to the bias implied by the statement. However, such

statements were rated as *more* biased than those that framed neither gender as the reference point for the other (e.g., “Girls and boys do equally well at math”), implying some recognition of the syntax’s implications. Moreover, given that subject-complement statements often communicate important summary information, as in the news headline that began this paper, people may be apt to reflect on their implicit messages. Such conscious reflection, or *elaboration* (Petty & Cacioppo, 1986), could lead people to discern the bias in the statements and avoid affirming it in their judgments. Participants with strongly held beliefs or values that run counter to this bias might be especially motivated to counterargue it by endorsing the group framed more negatively by the syntax (Petty & Cacioppo, 1979). If framing effects are reduced or reversed in people who explicitly recognize the persuasive potential of the syntax, this would suggest that subject-complement statements gain power only when people are not mindful of their implications.

Across four preregistered experiments, we investigated these questions by replicating and extending Chestnut and Markman’s (2018) work. Experiment 1 focused on judgments of math ability. As in Chestnut and Markman’s experiments, participants read a summary of scientific evidence that contained several subject-complement statements and then judged which gender is more skilled at math. To gauge whether participants were explicitly aware of the influence of the statements, we also asked them to indicate which part of the summary was most influential in their judgment, a technique that has been used in metaphor framing studies (Robins & Mayer, 2000; Thibodeau & Boroditsky, 2011, 2013). In Experiment 2, we examined whether the framing effects of subject-complement statements extend to judgments of terrorist behavior. Participants read summaries of actual terrorism data that framed Muslims or Christians as the reference point, judged which religious group is more likely to commit terrorist acts, and indicated which part of the summary was most influential in their judgment. Experiment 3 comprised a high-powered, within-subjects replication of the first two experiments, while Experiment 4 was designed to further explore the mechanisms underlying these syntactic framing effects. Together, these experiments provide novel insights into the consequences of using subject-complement syntax to frame statements of equality about stereotyped groups.

Experiment 1: Math ability

To review, Chestnut and Markman (2018) found that after reading a report of scientific evidence containing subject-complement statements of gender equality in math ability, participants attributed better ability to the gender

in the complement position. Specifically, statements with “boys” in the complement position yielded attributions similar to baseline beliefs, reflecting the stereotype that boys are superior at math. Statements with “girls” in the complement position, on the other hand, reduced the likelihood of attributing better ability to boys, relative to baseline. In addition to replicating these findings, Experiment 1 explored whether the framing effect would be moderated by awareness of the influence of the framing language—operationalized as citing the subject-complement statements as the most influential part of the report. While we anticipated replicating the framing effects observed by Chestnut and Markman, we did not have a specific hypothesis concerning the moderating role of citing the subject-complement syntax as influential.

We preregistered our methods and analysis plans for this and the subsequent experiments on AsPredicted. Our preregistrations, as well as all measures and relevant data, are available through the Open Science Framework at: <https://osf.io/3rgpb/>.

Method

Participants

We recruited 367 English-speaking participants from the United States through Amazon Mechanical Turk (MTurk), using the CloudResearch participant-sourcing platform (formerly TurkPrime; Litman et al., 2017). All participants had a good performance record on MTurk ($\geq 95\%$ rating on at least 100 previous tasks). As an additional quality filter, participants who failed an initial attention check (“... to demonstrate that you are a participant who reads the study instructions carefully and thoroughly, you need to check the option ‘Other’ below and enter the number 8 in the text box of this option ...”; $n = 7$) were prevented from completing the study. An additional 22 participants did not complete all measures, and their data were not analyzed. A power analysis using G*Power 3.1 (Faul et al., 2007) indicated that a sample size of 134 would be needed to replicate the smallest of Chestnut and Markman’s (2018) effects (*Baseline vs. Boys = Girls*) with 99% power. As our experiment had five between-subjects conditions, we sought a total sample size 2.5 times as large. Each participant in our final sample ($N = 338$) received a payment of \$0.30. See Table 1 for participant demographic data for all experiments.

Design, materials, and procedure

The experiment was created using Qualtrics online survey software. In addition to manipulating the position of “boys” and “girls” in subject-complement statements, we investigated whether the effects of these

Table 1. Demographic data for all experiments.

	Experiment 1	Experiment 2	Experiment 3	Experiment 4
<i>N</i> (sampled / analyzed)	367 / 338	378 / 340	835 / 752	1,517 / 1,304
% female / male	45% / 55%	47% / 53%	45% / 54%	43% / 56%
Mean age (<i>SD</i>)	36.4 (10.8)	36.5 (11.0)	37.5 (11.8)	38.1 (13.0)
% by race/ethnicity	72% White, 10% Black, 7% Asian, 6% multiracial, 4% Latinx, 1% other	75% White, 9% Asian, 7% Black, 5% Latinx, 3% multiracial, 1% other	69% White, 11% Black, 10% Asian, 5% Latinx, 5% multiracial	65% White, 11% Black, 11% Asian, 6% Latinx, 4% multiracial, 2% Native American, 1% other
% Democrat / Republican	44% / 25%	45% / 25%	46% / 24%	43% / 31%

statements depend on the kind of claim they express about gender. “Girls are as good as boys at math” is a *generic* claim about boys and girls as a whole. Conversely, “researchers found that girls performed as well as boys in grades two through eleven” is a *non-generic* claim because it describes a specific finding that may or may not generalize beyond the study sample. This property has been referred to as the *genericness* of the claim (Cimpian et al., 2010; Papafragou, 1996). While Chestnut and Markman’s (2018) stimuli included a mix of generic and non-generic statements, previous research suggests that scientific findings are perceived as more important when phrased generically than non-generically (DeJesus et al., 2019). Therefore, we hypothesized that subject-complement statements would differ by genericness, with generic statements eliciting stronger framing effects.

Participants were randomly assigned to one of five conditions: *Baseline* ($n = 72$), *Girls = Boys Generic* ($n = 68$), *Boys = Girls Generic* ($n = 67$), *Girls = Boys Non-Generic* ($n = 64$), or *Boys = Girls Non-Generic* ($n = 67$). In the *Baseline* condition, participants simply judged which gender (girls or boys) is naturally more skilled at math, and then rated how confident they were in their judgment using a sliding scale (0 = not at all confident, 100 = very confident).¹

In the *Girls = Boys Generic* condition, participants were presented with an adapted version of Chestnut and Markman’s (2018) summary of scientific evidence for gender equality in math ability. This report contained three subject-complement statements phrased in generic language, each with “boys” in the complement position (these statements are underlined here, but were not underlined for the participants):

¹Participants in the corresponding condition of Chestnut and Markman’s (2018) Experiment 1 judged which gender is naturally more skilled at math or which gender has to work harder to be good at math. As supplemental analyses suggested that framing effects were stronger for the skill question, we only included this question in our experiments.

Recent Study: Girls Equal Boys at Math

A recent study has shown that girls do just as well as boys at math. At the University of Wisconsin, a team of researchers analyzed scores from standardized tests taken in 2005, 2006, and 2007 by approximately seven million students in ten different states. Overall, they found that girls perform as well as boys in grades two through eleven. A troubling finding from the study, however, is that many tough math questions seem to have been removed from state tests. The researchers worry that teachers, as a result, may start dropping harder math problems from their curriculums.

After 15 seconds had passed, participants were able to click to advance to the next screen. There they judged which gender has better math ability based on the information in the report (“*Based on these findings, who do you think is naturally more skilled at math?*”) and rated their confidence in their judgment on the same 0-100 scale as in the *Baseline* condition. Next, they clicked to advance to another screen where the report was presented again. Participants were asked to indicate which part of the report was most influential in their judgment by copying and pasting into a text box. They were also provided with a second text box to indicate any other information they relied on for their judgment. They were required to enter something in at least the first text box before advancing to the next screen.

The *Girls = Boys Non-Generic* condition was identical to its *Generic* counterpart, except that the three subject-complement statements contained cues that suggested the findings might not be generalizable: past tense, the quantifier “most,” and the article “the” (cf. DeJesus et al., 2019): “Girls Equaled Boys at Math”; “most girls did just as well as most boys at math”; “the girls performed as well as the boys.” The *Boys = Girls Generic* condition and the *Boys = Girls Non-Generic* condition were identical to their *Girls = Boys* counterparts, except that “girls” was in the complement position in the three subject-complement statements.

Finally, participants completed basic demographic questions and were debriefed on the subtle biases communicated by our stimuli. Methods were approved by the Institutional Review Board at Colorado College.

Results

As we found no reliable effects of genericness in Experiments 1 and 2, the analyses reported below collapse across this factor. See the Supplemental Material for separate analyses of the generic and non-generic conditions, which provide converging findings. We did not manipulate genericness in Experiments 3 and 4.

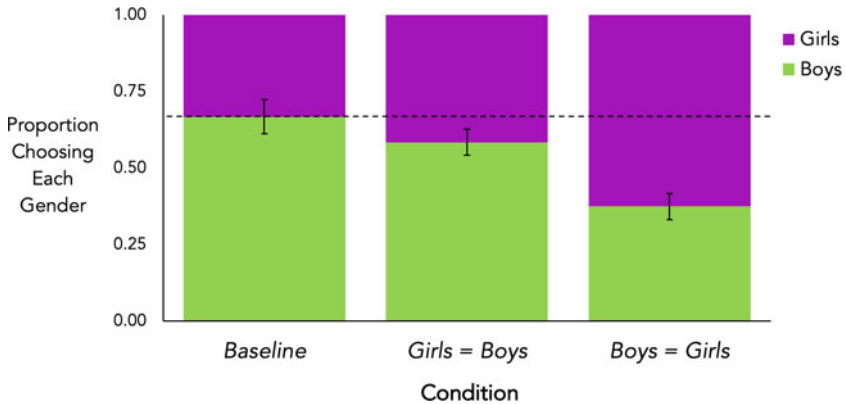


Figure 1. Forced-choice responses in Experiment 1, reflecting the gender to whom participants attributed better math ability. Error bars represent ± 1 SE.

Preregistered analyses

Forced-choice responses. In the *Baseline* condition, the forced-choice responses (girls or boys) reflected the common gender stereotype, with 67% of participants attributing better math ability to boys ($SE = 6\%$). This value was greater than chance (binomial sign test: $p = .006$) and matched Chestnut and Markman's (2018) *Baseline* condition.

Following Chestnut and Markman (2018), we analyzed the forced-choice responses across conditions using logistic regression models with condition as a categorical predictor. Below we report odds ratios (OR) for the Wald tests in these models, which indicate the relative likelihood of selecting "boys" in a given condition. For example, an OR of 0.5 would indicate that participants were half as likely to choose "boys" in the condition of interest compared to the reference condition, while an OR of 3 would indicate that participants were three times as likely to choose "boys."

To test whether judgments of math ability varied by condition, we conducted a series of planned contrasts comparing responses in each of the conditions in which subject-complement statements were presented (henceforth, experimental conditions) to responses in the *Baseline* condition. As shown in Figure 1, the overall results replicated Chestnut and Markman (2018). Whereas participants in the *Girls = Boys* conditions chose boys as having better math ability at rates that did not differ significantly from baseline (58%, $SE = 4\%$, $OR = 0.70$ [95% confidence interval: 0.38–1.28], $p = .24$), participants in the *Boys = Girls* conditions chose boys less often than baseline (37%, $SE = 4\%$, $OR = 0.30$ [0.16–0.54], $p < .001$).

Weighted responses. We also computed weighted responses by multiplying participants' forced-choice responses (with "girls" coded as -1 and "boys" as 1) by their confidence ratings (0–100), as in Chestnut and

Markman (2018). The weighted responses had a range of -100 to 100 , reflecting maximal confidence that girls or boys have better math ability, respectively; 0 indicated that participants believed neither gender has better ability, despite being forced to choose. To analyze the weighted responses, we used linear regression models analogous to the logistic regression models for the forced-choice responses. We report unstandardized beta coefficients for the t -tests in these models.

The weighted responses followed the same pattern as the forced-choice responses. In the *Baseline* condition, participants confidently attributed better math ability to boys ($M = 17.75$, $SE = 5.75$), $t(71) = 3.09$, $p = .003$. Responses in the *Girls = Boys* conditions did not differ significantly from baseline ($M = 7.36$, $SE = 4.90$, $b = -10.39$ [-25.93 – 5.16], $p = .19$), but in the *Boys = Girls* conditions, participants tended to attribute better math ability to girls ($M = -9.55$, $SE = 5.06$, $b = -27.30$ [-43.26 to -11.34], $p < .001$).

Exploratory analyses

In exploratory analyses, we examined whether the patterns shown in Figure 1 were moderated by awareness of the influence of the subject-complement statements. To do this, we coded whether or not the rationales given by participants in the experimental conditions (i.e., the two free response text boxes) contained at least one of the subject-complement statements from the report (designated as “citing the syntax” or not). Across conditions, the majority of participants (76%) cited the syntax.

Forced-choice responses. We used a logistic regression model with condition (*Boys = Girls*, coded as 0; or *Girls = Boys*, coded as 1), whether or not participants cited the syntax (did not cite, coded as 0; or cited, coded as 1), and the interaction of these factors as predictors of the forced-choice responses. The interaction was significant, $OR = 0.13$ [0.03 – 0.58], $p = .008$, indicating that the strength of the framing effect differed between participants who cited the syntax and those who did not cite the syntax. Figure 2 displays the results separately for these two groups, whom we refer to as “citters” and “non-citters.”

To unpack the interaction, we contrast-coded the citing syntax variable (coding each group of interest as 0) to examine the simple effects of condition. For non-citters, there was a significant framing effect: those in the *Girls = Boys* conditions (92%, $SE = 5\%$, $n = 37$) were far more likely to attribute better math ability to boys than those in the *Boys = Girls* conditions (48%, $SE = 10\%$, $n = 27$), $OR = 12.21$ [3.01 – 49.55], $p < .001$. In contrast, for citters, there was no reliable framing effect: the likelihood of choosing boys did not differ significantly between the *Girls = Boys* conditions (45%, $SE = 5\%$, $n = 95$) and the *Boys = Girls* conditions (35%, $SE = 5\%$, $n = 107$), OR

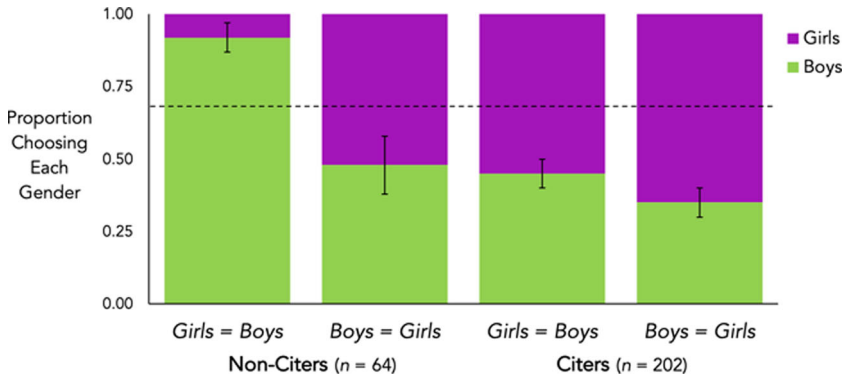


Figure 2. Forced-choice responses in Experiment 1, by condition and whether or not participants cited the subject-complement syntax as influential. Error bars represent ± 1 SE. Dashed line = Baseline condition.

= 1.56 [0.89–2.76], $p = .12$. Thus, the framing effect observed in the full sample was driven by the relatively small subset of participants (24%) who failed to identify the subject-complement statements as influential in their judgments.

Weighted responses. Again, the weighted responses mirrored the forced-choice responses. A linear regression model analogous to the logistic regression model for the forced-choice responses yielded a significant interaction between condition and whether or not participants cited the syntax, $b = -37.32$ [-68.58 to -6.05], $p = .02$. Non-citers chose boys more confidently in the *Girls = Boys* conditions ($M = 46.57$, $SE = 6.21$) than the *Boys = Girls* conditions ($M = 4.15$, $SE = 11.79$), $b = 42.42$ [15.10–69.74], $p = .002$. For citers, however, there was no significant difference between the *Girls = Boys* conditions ($M = -7.91$, $SE = 5.65$) and the *Boys = Girls* conditions ($M = -13.01$, $SE = 5.58$), $b = 5.10$ [-10.11–20.32], $p = .51$.

Discussion

Overall, the results of Experiment 1 replicated Chestnut and Markman (2018). After reading subject-complement statements of gender equality in math ability, participants attributed better math ability to the gender in the complement position. Their judgments thus reinforced the common gender stereotype when boys were framed as the reference point and challenged it when girls were framed as the reference point.

However, our exploratory analyses provide additional insight into the prevalence of these framing effects and the mechanisms driving them. In their rationales, more than three-quarters of our participants cited the subject-complement statements as the most influential part of the report, yet

these participants were no more likely to judge boys as the more skilled gender when boys were framed as the reference point than when girls were. One possible explanation is that citers interpreted the subject-complement statements at face value, as truly expressing gender equality. Another possibility is suggested by citers' increased tendency to choose *girls* as the more skilled gender relative to baseline, even when boys were framed as the reference point (see [Figure 2](#)). These participants may have explicitly recognized the *Girls = Boys* statements as biased against girls and resisted this bias in their judgments. This might be especially likely if the bias detected in the statements ran counter to previously held beliefs or values (Petty & Cacioppo, 1979). We consider these possibilities further in our discussions of the remaining experiments and examine them directly in Experiment 4 and in a cross-experiment analysis.

Notably, the syntactic framing effect documented by Chestnut and Markman (2018) was observed only in the roughly one-quarter of participants who did *not* cite the subject-complement statements as influential, implying that these non-citers drove the overall pattern seen in [Figure 1](#) and in Chestnut and Markman's study. Thus, it appears that the persuasive potential of subject-complement statements must go unrecognized in order for these statements to gain power. When people are aware of the influence of the statements, their effects are eliminated.

Experiment 2: Terrorist behavior

In the next experiment, we investigated whether the framing effects of subject-complement statements extend to an arguably more incendiary domain: the propensity of different religious groups to commit terrorist acts. Here the subject-complement statements, presented as part of a summary of actual terrorism data, ostensibly expressed that Muslims and Christians are equally likely to be terrorists. If the syntactic framing effects observed in Experiment 1 generalize to this novel domain, participants should nevertheless judge the religious group in the complement position as *more* likely to be terrorists. As in Experiment 1, we also explored whether this effect would depend on awareness of the influence of the subject-complement statements and on the genericness of the statements.

Method

Participants

Using the same criteria as in Experiment 1, we recruited 378 new participants through MTurk. Nineteen participants were excluded for failing the initial attention check and another 19 for not completing all measures. Each

participant in our final sample ($N=340$) received a payment of \$0.30. See [Table 1](#) for participant demographics.

Design, materials, and procedure

Participants were randomly assigned to one of five conditions mirroring Experiment 1: *Baseline* ($n=72$), *Christians = Muslims Generic* ($n=67$), *Muslims = Christians Generic* ($n=66$), *Christians = Muslims Non-Generic* ($n=68$), or *Muslims = Christians Non-Generic* ($n=67$). The procedure was analogous to that of Experiment 1. Participants in the four experimental conditions read a summary of a fictional large-scale terrorism study derived from actual terrorism data (START (National Consortium for the Study of Terrorism and Responses to Terrorism), 2018). In the *Christians = Muslims Generic* condition, the report was as follows (the three subject-complement statements are underlined here, but were not underlined for the participants):

Recent Study: Christians Equal Muslims in Terrorist Acts

A recent study has shown that Christians are just as likely as Muslims to commit terrorist acts. At the non-partisan Nation Institute, a team of researchers analyzed religiously motivated acts of violence and intimidation committed by hundreds of people in the United States from 1965 to 2015. Overall, they found that Christians cause as many terror-related civilian deaths as Muslims in major U.S. cities. A troubling finding from the study, however, is that there is no universal agreement on the definition of terrorism. The researchers worry that some government agencies, as a result, may fail to develop effective counterterrorism policies.

The *Christians = Muslims Non-Generic* condition was identical to its *Generic* counterpart, except that the subject-complement statements were “Christians Equaled Muslims in Terrorist Acts,” “some Christians were just as likely as some Muslims to commit terrorist acts,” and “the Christians caused as many terror-related civilian deaths as the Muslims.” The *Muslims = Christians Generic* condition and the *Muslims = Christians Non-Generic* condition were identical to their *Christians = Muslims* counterparts, except that “Muslims” was in the complement position in the three subject-complement statements.

After reading the report, participants advanced to another screen where they judged which religious group (Christians or Muslims) are more likely to be terrorists (“Based on these findings, who do you think are more likely to be terrorists?”) and rated their confidence in their judgment. They then advanced to yet another screen where they were shown the report again and provided a rationale for their judgment as in Experiment 1. In the *Baseline* condition, participants answered only the forced-choice and confidence questions.

Results

Preregistered analyses

Forced-choice responses. In the *Baseline* condition, the forced-choice responses reflected the common U.S. stereotype about terrorism: 76% of participants chose Muslims as more likely to be terrorists ($SE = 5\%$), which was greater than chance (binomial sign test: $p < .001$).

We compared the forced-choice responses across conditions as in Experiment 1. Planned contrasts showed that participants chose Muslims less often in the experimental conditions than in the *Baseline* condition, which was expected for the *Muslims = Christians* conditions (56%, $SE = 4\%$, $OR = 0.40 [0.21-0.76]$, $p = .005$), but not for the *Christians = Muslims* conditions (49%, $SE = 4\%$, $OR = 0.30 [0.16-0.56]$, $p < .001$). As shown in [Figure 3](#), participants chose Muslims at similar rates in the experimental conditions, indicating that there was no framing effect overall.

Weighted responses. The weighted responses, computed as in Experiment 1, followed the same pattern as the forced-choice responses. In the *Baseline* condition, participants confidently judged Muslims as more likely to be terrorists ($M = 42.39$, $SE = 7.58$), $t(71) = 5.59$, $p < .001$. Compared to baseline, participants in the experimental conditions chose Muslims less confidently, which was expected for the *Muslims = Christians* conditions ($M = 6.14$, $SE = 5.45$, $b = -36.25 [-54.52 \text{ to } -17.97]$, $p < .001$), but not for the *Christians = Muslims* conditions ($M = -.31$, $SE = 5.67$, $b = -42.70 [-61.49 \text{ to } -23.91]$, $p < .001$).

Exploratory analyses

In the experimental conditions, the majority of participants (68%) cited one or more of the subject-complement statements from the report in their rationales.

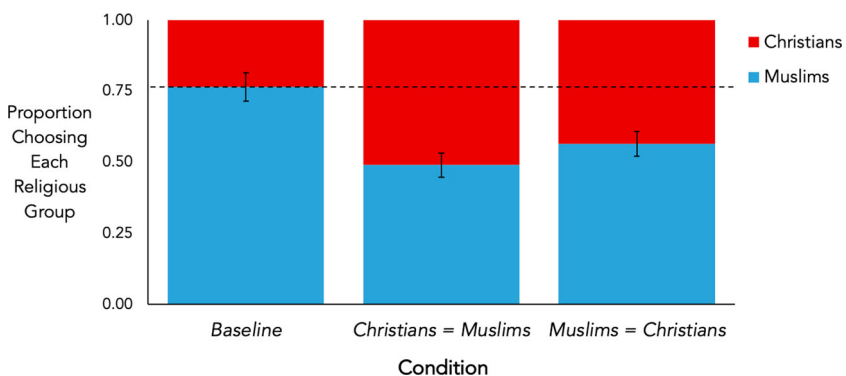


Figure 3. Forced-choice responses in Experiment 2, reflecting the religious group that participants judged as more likely to be terrorists. Error bars represent ± 1 SE.

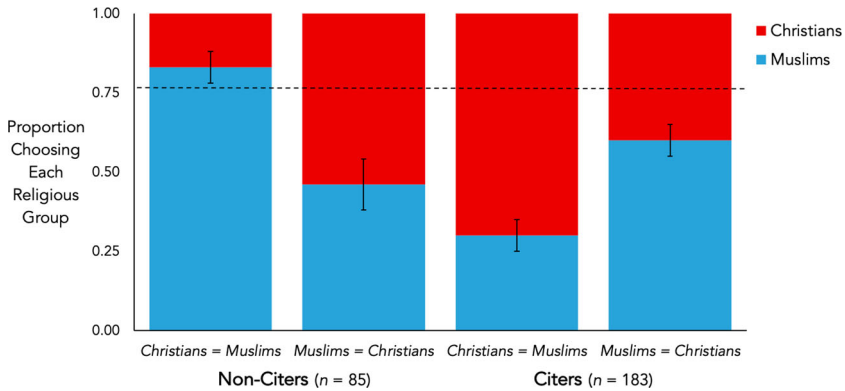


Figure 4. Forced-choice responses in Experiment 2, by condition and whether or not participants cited the subject-complement syntax as influential. Error bars represent ± 1 SE. Dashed line = Baseline condition.

Forced-choice responses. A logistic regression model with condition (*Muslims = Christians*, coded as 0; or *Christians = Muslims*, coded as 1), whether or not participants cited the syntax (non-citers, coded as 0; or citers, coded as 1), and the interaction of these factors as predictors yielded a significant interaction, $OR = 0.05 [0.01-0.15]$, $p < .001$.

As shown in [Figure 4](#), the syntactic framing effect, though not evident overall, depended on whether or not participants identified the subject-complement statements as influential. As in Experiment 1, we unpacked the interaction by contrast-coding the citing syntax variable to examine the simple effects of condition. Non-citers showed the expected framing effect: those in the *Christians = Muslims* conditions (83%, $SE = 5\%$, $n = 48$) were more likely to choose Muslims than those in the *Muslims = Christians* conditions (46%, $SE = 8\%$, $n = 37$), $OR = 5.88 [2.17-15.94]$, $p < .001$. In contrast, citers showed a framing effect in the *opposite* direction: those in the *Muslims = Christians* conditions (60%, $SE = 5\%$, $n = 96$) were about twice as likely to choose Muslims as those in the *Christians = Muslims* conditions (30%, $SE = 5\%$, $n = 87$), $OR = 0.28 [.15-.52]$, $p < .001$. Thus, only the small subset of participants (32%) who did *not* identify the subject-complement statements as influential in their judgments actually judged the group in the complement position as more likely to be terrorists. Those who recognized the statements as influential judged the group in the *subject* position as more likely to be terrorists, contrary to the implications of the syntax.

Weighted responses. Once again, the weighted responses mirrored the forced-choice responses. A linear regression model yielded a significant interaction between condition and whether or not participants cited the

syntax, $b = -87.43$ [-118.78 to -56.08], $p < .001$. Non-citers chose Muslims more confidently in the *Christians = Muslims* conditions ($M = 41.19$, $SE = 7.82$) than the *Muslims = Christians* conditions ($M = -10.49$, $SE = 11.96$), $b = 51.67$ [25.71–77.64], $p < .001$. In contrast, citers chose Muslims more confidently in the *Muslims = Christians* conditions ($M = 12.55$, $SE = 5.89$) than the *Christians = Muslims* conditions ($M = -23.21$, $SE = 6.48$), $b = -35.76$ [-53.33 to -18.19], $p < .001$.

Discussion

Unlike in Experiment 1, there was no syntactic framing effect overall in Experiment 2. Participants judged Muslims and Christians about equally likely to commit terrorist acts, contrary to the baseline belief that Muslims are more likely. Across the full sample, judgments did not differ based on which group was framed as the reference point in the subject-complement statements. However, our exploratory analyses of citing the subject-complement syntax revealed a similar pattern to Experiment 1. The expected framing effect—judging the group in the complement position as more likely to be terrorists—was observed only in the 32% of participants who did *not* cite the subject-complement statements as influential in their judgments. For these participants, framing Muslims as the reference point perpetuated the stereotype that Muslims are more likely to be terrorists, while framing Christians as the reference point counteracted this stereotype relative to baseline.

The other 68% of participants, in contrast, not only failed to show the expected syntactic framing effect but showed a significant effect in the *opposite* direction. This reverse framing effect suggests that explicitly attending to subject-complement statements may lead people to appreciate the bias transmitted by them and consciously resist it, choosing the group in the *subject* position as more likely to be terrorists. Notably, we observed a *reverse* framing effect for citers in the domain of terrorism in Experiment 2, but only a *reduced* framing effect in the domain of math ability in Experiment 1. This may be due to differences in the intensity or salience of the two domains, the strength of the preexisting stereotypes, or the relative sizes of the two groups within each domain in the U.S. population at large. We return to these issues in the General Discussion.

In sum, the results of Experiment 2 show that subject-complement syntax affects socially charged judgments in a markedly different domain than math ability. But as in Experiment 1, this was true *only* for people who failed to recognize the persuasive appeal of the syntax.

Experiment 3: Confirmatory replication

In our first two experiments, evidence that identifying subject-complement statements as influential can reduce or reverse their framing effects came from exploratory analyses. In Experiment 3, we preregistered all analyses, including those to confirm the mitigating effect of citing the subject-complement syntax. We also tested for framing effects on judgments of both math ability and terrorist behavior in a repeated-measures design, and more than doubled our sample size to ensure a high-powered replication. Given the results thus far, we did not have strong predictions about the *overall* effect of subject-complement syntax, but for both domains, we predicted an interaction between syntactic frame (i.e., which group is the reference point) and whether or not participants cited the syntax. Specifically, we expected to replicate the standard framing effect for non-citers, and to find no effect or a reverse framing effect for citers.

Method

Participants

Using our previous criteria, we recruited 835 new participants through MTurk. Thirty-six participants were excluded for failing the initial attention check and 47 for not completing all measures. Each participant in our final sample ($N = 752$) received a payment of \$0.50. See [Table 1](#) for participant demographics.

Design, materials, and procedure

One-third of the participants ($n = 260$) were assigned to the *Baseline* condition for both domains (math ability and terrorist behavior). The other two-thirds ($n = 492$) were assigned to one of the two framing conditions for each domain (math ability: *Girls = Boys*, $n = 244$, or *Boys = Girls*, $n = 248$; terrorist behavior: *Christians = Muslims*, $n = 248$, or *Muslims = Christians*, $n = 244$). Conditions were assigned independently of domain (e.g., a participant assigned to the *Girls = Boys* condition could be assigned to either terrorism condition). Participants viewed all stimuli and answered all questions for one domain before the other. The order of the two domains was counterbalanced. All of the subject-complement statements in each report used generic language. The method was otherwise identical to those of the previous experiments.

Results

Preregistered analyses

Forced-choice responses. The *Baseline* condition replicated the previous experiments: 67% of participants attributed better math ability to boys

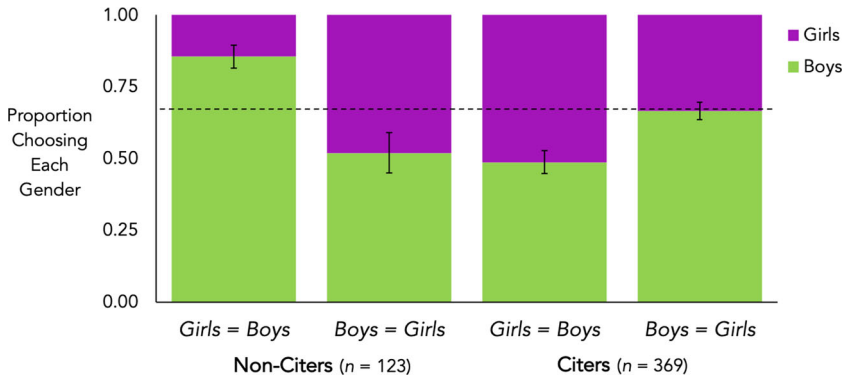


Figure 5. Forced-choice responses for judgments of math ability in Experiment 3, by condition and whether or not participants cited the subject-complement syntax as influential. Error bars represent ± 1 SE. Dashed line = Baseline condition.

($SE = 3\%$), and 73% judged Muslims as more likely to be terrorists ($SE = 3\%$). Both values were greater than chance (binomial sign tests: $ps < .001$).

As in the first two experiments, the majority of participants in the experimental conditions cited subject-complement syntax in their rationales for each domain (math ability: 70%; terrorist behavior: 71%).² Our primary analyses, conducted separately for each domain, mirrored the exploratory analyses of the previous experiments. For planned contrasts comparing each experimental condition against the *Baseline* condition, see the Supplemental Material. These analyses were consistent with the primary analyses and confirmed the mitigating effect of citing the syntax.

Math Ability. For the judgments of math ability, there was a significant interaction between condition and whether or not participants cited the syntax, $OR = 0.09$ [0.03–0.23], $p < .001$, replicating Experiment 1. As shown in Figure 5, non-citers showed the expected framing effect: those in the *Girls = Boys* condition (86%, $SE = 4\%$, $n = 69$) were far more likely to choose boys than those in the *Boys = Girls* condition (52%, $SE = 7\%$, $n = 54$), $OR = 5.48$ [2.33–12.91], $p < .001$. In contrast, citers showed a reverse framing effect: those in the *Boys = Girls* condition (66%, $SE = 3\%$, $n = 194$) were more likely to choose boys than those in the *Girls = Boys* condition (49%, $SE = 4\%$, $n = 175$), $OR = 0.48$ [.31–.72], $p = .001$. Thus, as in Experiment 1, only the small subset of participants (30%) who did not identify the subject-complement statements as influential in their judgments were affected

²Of the 420 participants who cited subject-complement syntax, the majority (71%) did so for both domains; 17% and 12% of participants cited the syntax only for math ability or terrorist behavior, respectively.

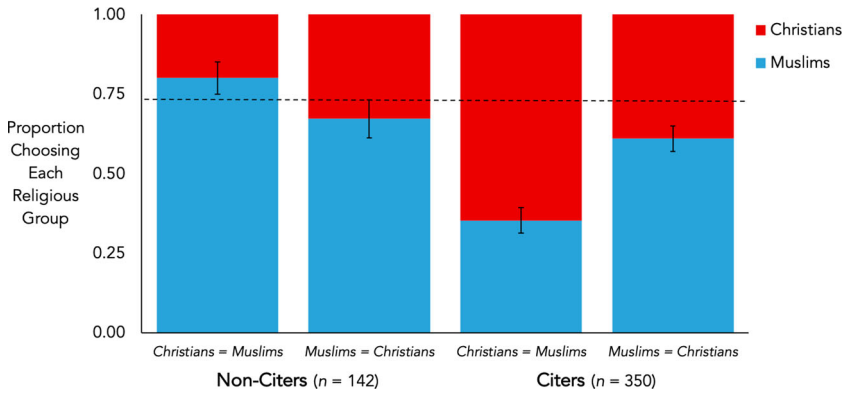


Figure 6. Forced-choice responses for judgments of terrorist behavior in Experiment 3, by condition and whether or not participants cited the subject-complement syntax as influential. Error bars represent ± 1 SE. Dashed line = Baseline condition.

by them as expected. Those who recognized the statements as influential were affected by them in the opposite direction.

Terrorist Behavior. For the judgments of terrorist behavior, there was also a significant interaction, $OR = 0.18$ [0.07–0.43], $p < .001$, replicating Experiment 2. As shown in Figure 6, the framing effect was not significant for non-citers: those in the *Christians = Muslims* condition (80%, $SE = 5%$, $n = 75$) were only descriptively more likely to choose Muslims than those in the *Muslims = Christians* condition (67%, $SE = 6%$, $n = 67$), $OR = 1.96$ [0.91–4.19], $p = .08$. In contrast, citers showed a significant reverse framing effect: those in the *Muslims = Christians* condition (61%, $SE = 4%$, $n = 177$) were more likely to choose Muslims than those in the *Christians = Muslims* condition (35%, $SE = 4%$, $n = 173$), $OR = 0.35$ [0.23–0.54], $p < .001$. These results are similar to Experiment 2: participants who failed to identify the subject-complement statements as influential in their judgments showed a different pattern of responses than those who did. In this case, non-citers showed no significant framing effect, while citers showed a reverse framing effect, mirroring citers in Experiment 2 and in the math domain of the present experiment.

Weighted responses. In the *Baseline* condition, participants confidently attributed better math ability to boys ($M = 42.39$, $SE = 7.58$), $t(71) = 5.59$, $p < .001$, and confidently judged Muslims as more likely to be terrorists ($M = 42.39$, $SE = 7.58$), $t(71) = 5.59$, $p < .001$. Our analyses of the weighted responses in the experimental conditions for each domain were conducted in the same manner as the exploratory analyses in the previous experiments. Planned contrasts comparing each experimental condition against the *Baseline* condition are reported in the Supplemental Material.

Math Ability. Mirroring the forced-choice responses, there was a significant interaction between condition and whether or not participants cited the subject-complement syntax, $b = -64.05$ [-87.84 to -40.27], $p < .001$. Non-citers chose boys more confidently in the *Girls = Boys* condition ($M = 42.51$, $SE = 5.70$) than the *Boys = Girls* condition ($M = -2.93$, $SE = 9.90$), $b = 45.43$ [24.80–66.06], $p < .001$, while citers chose boys more confidently in the *Boys = Girls* condition ($M = 12.44$, $SE = 4.10$) than the *Girls = Boys* condition ($M = -6.18$, $SE = 4.31$), $b = -18.62$ [-30.46 to -6.78], $p = .002$.

Terrorist Behavior. As in the math domain, the interaction was significant, $b = -54.82$ [-78.47 to -31.17], $p < .001$. For non-citers, there was a significant framing effect: those in the *Christians = Muslims* condition ($M = 46.13$, $SE = 6.74$) chose Muslims more confidently than those in the *Muslims = Christians* condition ($M = 25.63$, $SE = 8.07$), $b = 20.51$ [0.55–40.63], $p = .04$. In contrast, citers chose Muslims more confidently in the *Muslims = Christians* condition ($M = 19.97$, $SE = 4.43$) than the *Christians = Muslims* condition ($M = -14.35$, $SE = 4.61$), $b = -34.31$ [-47.01 to -21.62], $p < .001$.

Discussion

The results of Experiment 3 largely replicated the patterns observed in the exploratory analyses of Experiments 1 and 2. Only the roughly 30% of participants who failed to identify the subject-complement statements as influential showed the expected framing effect for math ability and terrorist behavior (at least in their weighted responses, for the latter domain). Participants who recognized the statements as influential, however, showed *reverse* framing effects for both domains. These participants were more likely to judge boys as superior at math and Muslims as more likely to be terrorists when these groups were in the subject position than when they were in the complement position. These reverse framing effects add further weight to the possibility that consciously attending to subject-complement statements can lead people to resist the bias they imply. When people consider the implications of the statement “Christians are just as likely as Muslims to commit terrorist acts,” for example, they may recognize that this statement is biased against Muslims and affirmatively counteract the bias by choosing *Christians* as more likely to be terrorists. Our final experiment tested this possibility directly.

Experiment 4: Perceiving bias in subject-complement syntax

Across the first three experiments, more than 70% of participants justified their judgments of math ability and terrorist behavior by citing statements

containing subject-complement syntax. Their judgments differed markedly from those of participants who did not identify the statements as influential. Rather than choosing the group in the complement position as more skilled at math or more likely to commit terrorist acts as the syntax implied, citers showed either no framing effect (for math ability in Experiment 1) or *reverse* framing effects, choosing the group in the *subject* position (for math ability in Experiment 3 and terrorist behavior in Experiments 2-3). Recognizing the persuasive potential of subject-complement statements but *not* being persuaded by them as expected suggests that citers may have consciously perceived the statements as biased.

In Experiment 4, we investigated this possibility by adapting Chestnut and Markman's (2018) method of asking participants to explicitly judge whether subject-complement statements are biased against one of the groups mentioned. We conducted two versions, Experiments 4a and 4b, that differed only in which group was referenced in the explicit bias question. Although Chestnut and Markman found that subject-complement statements were rated as relatively unbiased overall, we hypothesized that citers would rate them as *more* biased than non-citers.

Method

Participants

Using our previous criteria, we recruited 1,517 new participants through MTurk. Ninety-eight participants were excluded for failing the initial attention check and 115 for not completing all measures. Each participant in our final sample ($N = 1,304$) received a payment of \$0.30 for completing Experiment 4a ($n = 598$) or Experiment 4b ($n = 706$). See Table 1 for participant demographics.

Design, materials, and procedure

To avoid cross-domain carryover effects from the explicit bias ratings, participants in Experiment 4 made judgments for a single domain: math ability or terrorist behavior. Participants were randomly assigned to either the *Girls = Boys* condition ($n = 332$), the *Boys = Girls* condition ($n = 327$), the *Christians = Muslims* condition ($n = 316$), or the *Muslims = Christians* condition ($n = 329$). The *Baseline* condition was not included in this experiment.

Each of the four conditions was identical to its counterpart from Experiment 3, with one exception: after providing a rationale for their judgment, participants advanced to another screen where they were shown one of the subject-complement statements they had read previously (“[Girls/Boys] do just as well as [boys/girls] at math” or “[Christians/Muslims] are just as likely as [Muslims/Christians] to commit terrorist acts”). In

Experiment 4a, participants were asked to rate how biased the statement is against the group in the subject position (“Do you think this sentence is biased against [girls/boys/Christians/Muslims]?”) using a sliding scale (0 = definitely not; 100 = definitely yes). In Experiment 4b, participants in the math ability conditions were also asked to rate how biased the statement is against the group in the subject position, but those in the terrorist behavior conditions were asked to rate how biased the statement is against the group in the complement position. We made this change so that the explicit bias questions for both domains would reference the group framed more negatively by the syntax. Experiments 4a and 4b were otherwise identical.

Results

We preregistered separate analysis plans for Experiments 4a and 4b, but here we report combined analyses because the only difference between the two versions of the experiment was the wording of the explicit bias question, which we address in our analyses below. Separate analyses for each version followed the same pattern as the combined analysis, and are reported in the Supplemental Material.

The majority of participants cited subject-complement syntax in their rationales for each domain (math ability: 71%; terrorist behavior: 68%). We analyzed the forced-choice and weighted responses in the same manner as in the previous experiments. As before, the results were similar for the two sets of responses.

Forced-choice responses

Math Ability. For the judgments of math ability, there was a significant interaction between condition and whether or not participants cited the syntax, $OR = 0.23 [0.11-0.48]$, $p < .001$, replicating Experiments 1 and 3. As shown in Figure 7, non-citers showed the expected framing effect: those in the *Girls = Boys* condition (82%, $SE = 4%$, $n = 103$) were far more likely to choose boys than those in the *Boys = Girls* condition (37%, $SE = 5%$, $n = 90$), $OR = 7.64 [3.96-14.73]$, $p < .001$. Citers showed a reduced but still significant framing effect in the same direction: those in the *Girls = Boys* condition (45%, $SE = 3%$, $n = 229$) were more likely to choose boys than those in the *Boys = Girls* condition (32%, $SE = 3%$, $n = 237$), $OR = 1.73 [1.19-2.52]$, $p = .004$, though participants in both conditions chose girls more often than boys overall. In sum, similar to the previous experiments, subject-complement statements about math ability yielded the expected framing effect, but especially for participants who did *not* identify the subject-complement statements as influential in their judgments.



Figure 7. Forced-choice responses for judgments of math ability in Experiment 4, by condition and whether or not participants cited the subject-complement syntax as influential. Error bars represent ± 1 SE.

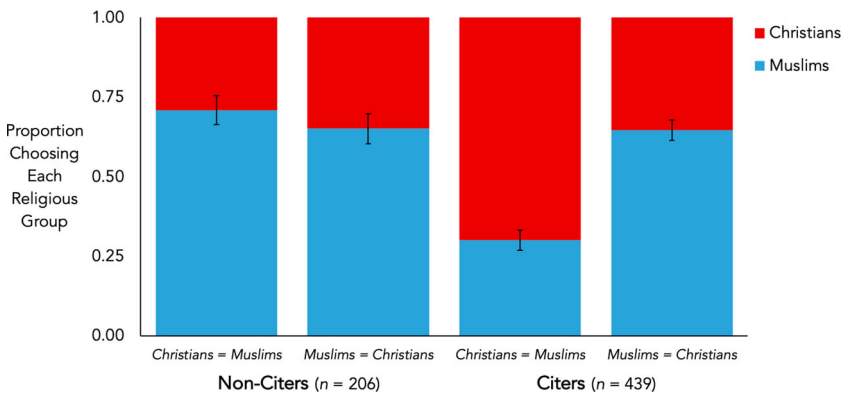


Figure 8. Forced-choice responses for judgments of terrorist behavior in Experiment 4, by condition and whether or not participants cited the subject-complement syntax as influential. Error bars represent ± 1 SE.

Terrorist Behavior. For the judgments of terrorist behavior, there was also a significant interaction, $OR = 0.18$ [0.09–0.37], $p < .001$, replicating Experiments 2 and 3. As shown in Figure 8, there was no reliable framing effect for non-citers: those in the *Christians = Muslims* condition (71%, $SE = 4\%$, $n = 103$) were not significantly more likely to choose Muslims than those in the *Muslims = Christians* condition (65%, $SE = 5\%$, $n = 103$), $OR = 1.31$ [0.73–2.35], $p = .37$. In contrast, citers showed a significant reverse framing effect: those in the *Muslims = Christians* condition (65%, $SE = 3\%$, $n = 226$) were more than twice as likely to choose Muslims than those in the *Christians = Muslims* condition (30%, $SE = 3\%$, $n = 213$), $OR = 0.24$ [0.16–0.35], $p < .001$. Thus, as in Experiments 2 and 3, participants who recognized the subject-complement statements as influential in their

judgments showed a reverse framing effect, judging the group in the *subject* position as more likely to commit terrorist acts. In this experiment, non-citers did not show the expected framing effect.

Weighted responses

Math Ability. There was a significant interaction between condition and whether or not participants cited the subject-complement syntax, $b = -39.09$ [-58.87 to -19.30], $p < .001$. Non-citers chose boys more confidently in the *Girls = Boys* condition ($M = 31.61$, $SE = 4.99$) than the *Boys = Girls* condition ($M = -19.50$, $SE = 6.67$), $b = 51.11$ [34.47–67.76], $p < .001$. For citers, this effect was reduced but still significant, though citers were more confident in choosing girls than boys overall (*Girls = Boys* condition: $M = -6.23$, $SE = 4.04$; *Boys = Girls* condition: $M = -18.26$, $SE = 3.76$), $b = 12.03$ [1.34–22.72], $p = .03$.

Terrorist Behavior. As in the math domain, the interaction was significant, $b = -44.10$ [-64.52 to -23.68], $p < .001$. For non-citers, there was no significant framing effect (*Christians = Muslims* condition: $M = 27.04$, $SE = 5.92$; *Muslims = Christians* condition: $M = 22.50$, $SE = 6.61$), $b = 4.53$ [-12.31–21.38], $p = .60$. In contrast, citers chose Muslims more confidently in the *Muslims = Christians* condition ($M = 17.92$, $SE = 4.20$) than the *Christians = Muslims* condition ($M = -21.65$, $SE = 3.96$), $b = -39.57$ [-51.11 to -28.02], $p < .001$. In fact, citers in the *Christians = Muslims* condition were reasonably confident in choosing Christians, contrary to the implications of the syntax.

Explicit bias judgments. In Experiment 4a, the group mentioned in the explicit bias question was always the group in the subject position. For subject-complement statements about math ability, the implied bias is against this group. For statements about terrorist behavior, however, the implied bias is against the group in the complement position. Therefore, we reverse-scored the explicit bias ratings of participants in the terrorist behavior conditions of Experiment 4a. For example, a rating of 25 for whether “Christians are just as likely as Muslims to commit terrorist acts” is biased against Christians was scored as 75, reflecting the likely inference that the statement is actually biased against Muslims. Though not preregistered, this change was necessary to align the explicit bias ratings with the implied bias in the terrorism statements. In Experiment 4b, the implied bias was against the group mentioned in the explicit bias question for both domains, and the explicit bias ratings were analyzed as is.

To determine whether citers were more likely than non-citers to perceive the subject-complement statements as biased, we conducted a 2 (citers vs.

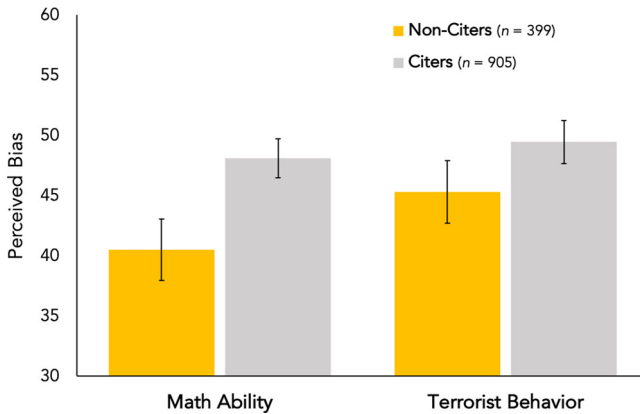


Figure 9. Perceived bias ratings in Experiment 4, by domain and whether or not participants cited the subject-complement syntax as influential. Error bars represent ± 1 SE.

non-citers) $\times 2$ (domain: math ability or terrorist behavior) ANOVA on the explicit bias ratings. Critically, the main effect of citing the syntax was significant, $F(1, 1300) = 7.26, p = .007, \eta_p^2 = .01$. As shown in Figure 9, citers ($M = 48.8, SE = 1.2$) perceived the subject-complement statements as more biased than non-citers ($M = 42.9, SE = 1.8$). There was no interaction, $F(1, 1300) = .63, p = .43$, indicating that the difference between citers and non-citers held across domains.

Discussion

Once again, the framing effects of subject-complement statements depended critically on whether participants cited the statements as influential in their judgments. For math ability, non-citers showed the expected framing effect, while citers showed a reduced framing effect. For terrorist behavior, non-citers showed no significant framing effect, while citers showed a reverse framing effect. Importantly, across both domains, citers explicitly judged the subject-complement statements as more biased than non-citers. These results suggest that the reduced or reversed framing effects exhibited by citers were due in part to consciously resisting the bias they perceived in the statements.

Cross-experiment analysis

For a high-powered test of syntactic framing effects and to provide the most accurate effect size estimates, we pooled the data from all four experiments and conducted combined analyses of the forced-choice and weighted responses in the experimental conditions ($N = 2,330$). Across



Figure 10. Forced-choice responses for judgments of math ability across Experiments 1, 3, and 4, by condition and whether or not participants cited the subject-complement syntax as influential. Error bars represent ± 1 SE. Dashed line = Baseline condition across Experiments 1 and 3.

experiments, 73% of participants cited the syntax in their rationales for math ability, and 69% did so for terrorist behavior.

Forced-choice responses. For both math ability and terrorist behavior, there was a significant interaction between condition and whether or not participants cited the syntax (math ability: OR = 0.15 [0.08–0.25], $p < .001$; terrorist behavior, OR = 0.14 [0.09–0.23], $p < .001$; see Figures 10 and 11).

For both domains, non-citers showed the expected framing effects. They chose boys as having better math ability more often in the *Girls = Boys* condition (85%, $SE = 2\%$, $n = 209$) than the *Boys = Girls* condition (43%, $SE = 4\%$, $n = 171$), OR = 7.25 [4.47–11.75], $p < .001$, and they chose Muslims as more likely to commit terrorist acts more often in the *Christians = Muslims* condition (77%, $SE = 3\%$, $n = 226$) than the *Muslims = Christians* condition (62%, $SE = 3\%$, $n = 207$), OR = 1.97 [1.30–2.99], $p = .001$. In contrast, citers showed reduced or reversed framing effects. For math ability, they chose boys at similar rates in the *Girls = Boys* condition (46%, $SE = 2\%$, $n = 499$) and the *Boys = Girls* condition (45%, $SE = 2\%$, $n = 538$), OR = 1.05 [0.83–1.35], $p = .67$. For terrorist behavior, they showed a reverse framing effect, choosing Muslims more often in the *Muslims = Christians* condition (63%, $SE = 2\%$, $n = 499$) than the *Christians = Muslims* condition (32%, $SE = 2\%$, $n = 473$), OR = 0.28 [0.22–0.37], $p < .001$.

Weighted responses. For both domains, there was a significant interaction between condition and whether or not participants cited the syntax (math ability: $b = -48.78$ [-62.56 to -35.00], $p < .001$; terrorist behavior: $b = -55.64$ [-69.51 to -41.77], $p < .001$). The results followed the same pattern as the

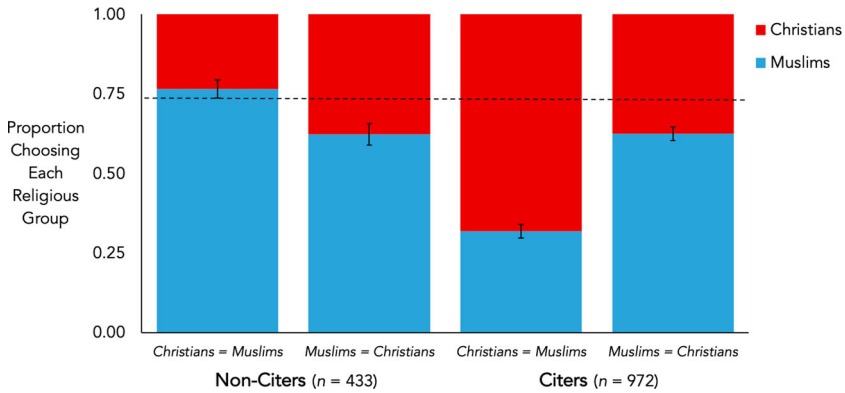


Figure 11. Forced-choice responses for judgments of terrorist behavior across Experiments 2-4, by condition and whether or not participants cited the subject-complement syntax as influential. Error bars represent ± 1 SE. Dashed line = Baseline condition across Experiments 2-3.

forced-choice responses. Non-citers chose boys more confidently in the *Girls = Boys* condition ($M = 37.86$, $SE = 3.30$) than the *Boys = Girls* condition ($M = -10.53$, $SE = 5.08$), $b = 48.39$ [36.59–60.19], $p < .001$, and they chose Muslims more confidently in the *Christians = Muslims* condition ($M = 36.38$, $SE = 3.90$) than the *Muslims = Christians* condition ($M = 17.62$, $SE = 4.78$), $b = 18.76$ [7.23–30.30], $p = .001$. For citers, there was no significant difference between the *Girls = Boys* condition ($M = -6.53$, $SE = 2.62$) and the *Boys = Girls* condition ($M = -6.14$, $SE = 2.55$), $b = -0.39$ [-7.50–6.72], $p = .92$, but citers chose Muslims more confidently in the *Muslims = Christians* condition ($M = 17.61$, $SE = 2.71$) than the *Christians = Muslims* condition ($M = -19.26$, $SE = 2.73$), $b = -36.88$ [-44.57 to -29.18], $p < .001$.

Demographic predictors of citing subject-complement syntax. We also used the pooled data to explore individual differences in recognizing the persuasive potential of subject-complement statements. We reasoned that people who hold prior beliefs that run counter to the biases implied by our statements would be especially likely to recognize the statements as influential, and thus *less* susceptible to framing. To explore this possibility, we considered a host of participant demographic variables as rough indices of anti-bias beliefs, including gender, political ideology, and educational attainment. For example, women, liberals, and more highly educated people are more likely than men, conservatives, and less highly educated people to endorse values such as fairness and egalitarianism (Graham et al., 2009, 2011; Van Leeuwen et al., 2014). These individuals might be more sensitive to, and more apt to resist, the biases implied by the framing language.

To test for demographic predictors of citing the subject-complement statements as influential, we used separate logistic regression models for each domain. We entered the following variables as predictors of our binary cited syntax variable (citors vs. non-citors): participants' age, gender (female, coded as 1; or male, coded as 0; other genders were excluded due to low numbers, $n = 17$), race/ethnicity (five predictors, entered separately: White, Black, Asian, Latinx, or multiracial, each coded as 1; all other groups besides the target group, coded as 0), political ideology (rated from 1 = *very liberal* to 7 = *very conservative*), highest level of education completed (from *some high school*, 1, to *doctorate or professional degree*, 9), and annual household income (from *less than \$25,000*, 1, to *more than \$200,000*, 7).³

For math ability, there were four significant predictors of citing the syntax: gender (OR = 1.72 [1.33–2.21], $p < .001$), political ideology (OR = 0.88 [0.83–0.94], $p < .001$), age (OR = 0.99 [0.98–1.00], $p = .03$), and highest level of education completed (OR = 1.09 [1.02–1.16], $p = .01$). Citors were more likely to be women, and were more liberal, younger, and more highly educated than non-citors. For terrorist behavior, there were two significant predictors: gender (OR = 1.60 [1.26–2.03], $p < .001$) and political ideology (OR = 0.86 [0.80–0.91], $p < .001$). As we found for math ability, citors were more likely to be women and were more liberal than non-citors. These results indicate that participants from certain demographic groups—most of which have been linked to concerns about fairness and egalitarianism—were especially sensitive to the persuasive potential of subject-complement syntax. These participants in turn showed reduced or reversed framing effects across all of our experiments.

General discussion

People often use subject-complement syntax to express an equivalence between groups, as in the phrase “girls are just as good at math as boys.” Contrary to the apparent well-meaning intention of this statement, Chestnut and Markman (2018) showed that observers tend to infer a gender *difference*: the group in the complement position—in this case, “boys”—is judged to have superior ability. This suggests that people treat the complement as the *reference point* against which the subject is compared (Rosch, 1975; Tversky, 1977), and that they are sensitive to the

³“Latinx” is an inclusive, gender-neutral term increasingly used in place of “Latino/a” (de Onís, 2017). In Experiments 2-4, participants were also asked to indicate their religion and degree of religiosity (rated from 0, not at all religious, to 6, very religious). When these variables were included in our regression models (for religion, 2 predictors: Christian or Muslim, each coded as 1; all other religions, coded as 0), none of them uniquely predicted citing the syntax for either domain ($ps > .07$). For demographic variables not included in Table 1, we provide descriptive statistics in the Supplemental Material.

pragmatic implications of this choice of framing (see also McKenzie & Nelson, 2003; Sher & McKenzie, 2006). Expressions about traits like math ability bias observers to judge the complement as more skilled because reference points are usually the more prototypical or exemplary category members (Hegarty & Bruckmüller, 2013). Across four high-powered, preregistered experiments ($N=2,734$), we replicated and extended this work, addressing two key questions: (1) do the framing effects of subject-complement syntax generalize beyond judgments of the intellectual ability of boys and girls to other kinds of inferences about stereotyped social groups?; and (2) how explicit is the influence of this syntax?

Following Chestnut and Markman (2018), our participants read a brief summary of some scientific evidence for gender equality in math ability (Experiments 1, 3, and 4) and/or data about the commensurate tendency for Muslims and Christians to commit terrorist acts (Experiments 2-4). The reports contained several statements with either girls/Christians in the subject position and boys/Muslims in the complement position, or with the positions of the two groups reversed. In our first two experiments, we also manipulated the genericness of the language used to describe the groups, but this did not reliably affect participant responses (see Supplemental Material). After reading each report, participants indicated which group they believed was more skilled at math or more likely to commit terrorist acts, along with how confident they were in this decision. Finally, they were asked to indicate which part of the report was most influential in their judgment by copying and pasting from the report into a text box.

The results revealed syntactic framing effects in both domains. Across experiments, participants tended to view the group in the complement position as more skilled at math or more likely to commit terrorist acts, even when these responses were weighted by the confidence ratings. Critically, however, this was only the case for the relatively small subset of participants (~30%) who did *not* indicate that the statements containing the subject-complement syntax influenced their judgments. This is consistent with findings from the metaphor framing literature, showing that subtle linguistic cues can covertly influence reasoning (e.g., Thibodeau & Boroditsky, 2011, 2013).

That said, the majority of our participants (~70%) *did* cite the subject-complement syntax as influential—probably because this language communicated the key take-home message of the report. These participants showed reduced or even *reversed* framing effects, sometimes viewing the group in the complement position as *less* skilled at math or *less* likely to commit terrorist acts. Citers were also *more* likely to explicitly judge the subject-complement statements as biased (Experiment 4). Together, these

findings suggest that many people are sensitive to the subtle biases implicit in these statements, which they then consciously act to resist.

This conclusion is further supported by our exploratory analysis of the pooled data across experiments. For statements about terrorist behavior, women and ideological liberals were especially likely to cite the syntax as influential. For statements about math ability, these same two groups, as well as younger and more highly educated participants, were especially likely to cite the syntax as influential. Consequently, these participants were *less* likely to show the expected syntactic framing effect in their respective domains.

These results are consistent with the literature on persuasion and motivated reasoning. High involvement with an issue is associated with greater reflection on the content of persuasive messages, reduced persuasion when statements run counter to ideological commitments, and an increased propensity to seek out or generate arguments to reaffirm prior beliefs and attitudes (Epley & Gilovich, 2016; Petty & Cacioppo, 1979, 1986). Research has found that women, ideological liberals, and more highly educated people show greater concern for moral values of fairness and egalitarianism in general (Graham et al., 2009, 2011; Van Leeuwen et al., 2014). Therefore, in a context where these individuals are making judgments relevant to gender inequality in math or Islamophobic stereotypes about terrorism, it makes sense that they would be more likely to interpret statements about these issues as affirming their own counterstereotypical values or positions, even as they show enhanced awareness of the biases communicated by the statements. Taken together, our findings indicate that subject-complement statements perpetuate or counteract stereotypes only when people are oblivious to, or unmotivated to interrogate, their implications.

A closer look at the response patterns of participants who did and did not cite the syntax as influential highlights the generalizability of this form of syntactic framing and the mitigating effect of recognizing its persuasive potential. Across two vastly different domains, citers and non-citers differed primarily *when the syntax implied the stereotype*—that is, when boys or Muslims occupied the complement position (see [Figures 10](#) and [11](#)). In this case, citers were far less likely to affirm the stereotype in their judgments. When the syntax *countered* the stereotype, however—when girls or Christians occupied the complement position—citers and non-citers gave strikingly similar judgments, choosing the group in the complement position more often than baseline. In this case, because participants who recognized the syntax as influential were motivated to affirm—rather than resist—its implications, they responded similarly to participants who were influenced by the syntax without realizing it.

These findings have important real-world implications, as subject-complement syntax is common in everyday discourse about a variety of hot-button issues. For example, consider the case of psychologist Christine Blasey Ford, who publicly accused Judge Brett Kavanaugh of sexual assault while he was under consideration for a U.S. Supreme Court appointment. Republican senator Lindsey Graham defended the judge while reassuring the press that he was sympathetic to Ford's distress, noting that "she is just as much a victim of this [situation] as Brett Kavanaugh" (Loiaconi, 2018). Our results suggest that this statement may have led some observers to view Kavanaugh—who was subsequently confirmed to the Supreme Court—as the more "legitimate" victim (see also Flusberg et al., 2019, 2021, *in press*).

At the same time, our findings reveal some limitations of this form of syntactic framing and suggest that its effects may not be as pervasive or straightforward as previously assumed. First, Chestnut and Markman (2018) argued that because people judge subject-complement statements like "girls are as good as boys at math" as relatively egalitarian, the results of their experiments reveal widespread *implicit* framing effects, which might be used to foster belief change in cases where explicit arguments fail. The results of the present experiments temper this conclusion. While some people show signs of an implicit framing effect from subject-complement statements, most people—especially those from certain demographic groups or who otherwise think carefully about the syntax's implications—consciously recognize that these statements influence their judgments. And these individuals tend to resist the expected framing effects, often by overcompensating in the opposite direction.

Second, we observed subtle differences between the math and terrorism domains that point to additional factors as moderating the effects of subject-complement syntax. Specifically, the terrorism domain was associated with both a comparatively weak framing effect for non-citers and a strong reverse framing effect for citers. For the math domain, there was simply no framing effect for citers (see Figures 10 and 11). These differences could be due to the differing sizes of our comparison groups in the U.S.: whereas boys and girls are roughly equal in numbers, there are far more Christians than Muslims, including in major cities from which the data in our terrorism report were gathered (Pew Research Center, 2019). Therefore, the syntactically counterstereotypical statement "Muslims equal Christians in terrorist acts" could be interpreted as stating that the two groups commit the same absolute number of terrorist acts, and that Muslims are therefore *more* prone to terrorism. "Boys equal girls at math," on the other hand, does not permit this kind of interpretation. Future research could systematically vary the base rates of the comparison groups to examine whether syntactic framing effects differ with the availability of multiple interpretations.

Another explanation for the domain differences we observed is that something about the stereotype itself, like its strength or moral salience, dictates whether subject-complement syntax backfires or is merely nullified in observers with strong counterstereotypical convictions. For example, gender-math stereotypes are somewhat weaker in the United States compared to many other developed countries (Breda et al., 2020). Therefore, even though many participants were motivated to counteract the implication that men are superior at math when boys occupied the complement position, the stereotype may not have been strong enough to trigger an overcorrection in the opposite direction. Replicating the current study in a country with stronger gender-math stereotypes could be a way to test this hypothesis directly.

A related factor is the moral intensity or salience of the stereotyped domain. Gender-based stereotypes about math ability have significant social consequences and may partially explain observed gender gaps in STEM fields (Master & Meltzoff, 2020). However, they are hardly a matter of life and death, and may not be a salient moral issue for many people. Since the 9/11 attacks, on the other hand, fears concerning terrorism have loomed large in American culture, accompanied by a sharp rise in anti-Muslim hate crimes (Kaplan, 2006). For this reason, stereotypes about who is prone to commit terrorist acts are significantly more severe and have more immediate, visceral consequences for the targeted group. The moral intensity of this stereotype may have led participants who strongly oppose it—either sincerely or because they wanted to signal their progressive values—to actively negate the pragmatic implications of the statement “Christians are just as likely as Muslims to commit terrorist acts” by choosing Christians as more likely. One way to test this possibility would be to replicate the current study with additional stereotyped domains that vary systematically in moral intensity or salience (e.g., stereotypes about which ethnic groups are better dancers versus which groups are more violent), and to assess the role of social desirability bias (Grimm, 2010).

There are a number of other related questions that we plan to address in future work. To take one example, do people exhibit syntactic framing effects when there is no prior stereotype about the groups mentioned in a subject-complement statement? For instance, what if the passage described the math abilities of Christians versus Muslims, the basketball skills of Bulgarians versus Colombians, or the brute strength of coffee versus tea lovers? One possibility is that we might see similar or even *stronger* framing effects under these circumstances, as participants would not hold any prior beliefs about such comparisons, give or take the effects of ingroup bias. Therefore, they might be especially susceptible to a subtle linguistic nudge. There is evidence to support this hypothesis. In a study by Bruckmüller and

Abele (2010), participants read about some differences between two fictitious cultural groups living on a South Pacific island. Whichever group occupied the reference position was rated as having higher status and more agency than the other group. Additionally, Chestnut (2017) found that subject-complement statements of gender equality for non-stereotyped actions (e.g., “girls are as good as boys at snapping”) yielded significant framing effects, with the gender occupying the complement position rated as more naturally skilled in the domain. This suggests that the use of subject-complement statements of equality may lead people to internalize new stereotypes as they pick up on the pragmatic implications of the language.

On the other hand, a preexisting stereotype may sometimes be necessary to elicit a syntactic framing effect, as subject-complement statements may have little persuasive appeal when there is no discernible basis for treating one group as the reference point for the other. There is some support for this possibility as well. Bruckmüller and colleagues (2012) found that subject-reference framing was more effective when there was a preexisting stereotype about the domain in question. In their study, some participants read comparative statements about men and women that situated one group as the normative reference in the domain of leadership, where there is a strong pro-male stereotype. For example, the headline “Do women lead differently than men?” positions men as the reference point. Other participants read similar statements on the topic of leisure time, where there are no salient gender-based stereotypes. Participants only showed the expected framing effects in the leadership condition, rating men as having higher power and status than women when men occupied the reference position. In the leisure time condition, on the other hand, participants showed a reverse framing effect, rating women as higher-status than men when men occupied the reference point. This suggests that syntactic framing effects may be highly sensitive to context and depend on a variety of factors, including stereotype strength. As it stands, one limitation of the current work is that we only included two stereotyped domains in our stimulus set, so additional research is needed to fully address the many factors that may moderate the influence of subject-complement syntax.

In sum, this paper asked: how and when does syntax perpetuate stereotypes? Our findings suggest that subject-complement statements of equality gain the power to persuade only when people don’t recognize this power—yet this power is often recognized, especially by those with a vested interest in the issue under discussion. When people are motivated to think carefully about what these statements mean, they may be less susceptible to perpetuating the stereotypes subtly transmitted by them.

Author note

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No potential conflict of interest was reported by the authors.

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