The uniformity of stereotype threat
Analyzing the moderating effects of premeasured performance

Andrea H. Stoevenbelt,

Paulette C. Flore

Inga Schwabe

Jelte M. Wicherts

Tilburg University

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Abstract (150 words)

Stereotype threat theory states that female and minority test-takers underperform on cognitive tests because they experience pressure by negative stereotypes about their group's performance. The theory implicates larger effects for test-takers who strongly identify with an academic domain, and for whom the test is most difficult. These moderators could create treatment-by-covariate interactions when authors included premeasured performance (e.g., SAT scores) as covariate, as is common practice in stereotype threat research. In this preregistered Bayesian meta-analysis, we use raw data from 31 stereotype threat studies involving 3357 negatively stereotyped participants to check whether stereotype threat effects are moderated by premeasured performance. Results yield evidence for no moderation. Correlations between the premeasured performance and test scores are similar across conditions, indicating uniformity of stereotype threat with respect to premeasured performance. This suggests that domain identification or test difficulty as both operationalized by premeasured performance fail to moderate stereotype threat effects.

Keywords: Domain identification, test difficulty, stereotype threat, premeasured ability, ANCOVA, Bayesian meta-analysis, treatment-by-covariate interaction

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Statement of relevance

The negative effect of stereotypes on female and ethnic minority test performance can lead to unfairness in academic achievement testing. Stereotype threat is widely studied in experiments, varying the pressures caused by negative performance stereotypes and investigating whether effects are moderated by individual differences. We study two moderators of the stereotype threat effect relating to test-taker's performance on earlier achievement tests to show which tests and test-takers are most susceptible to stereotype threat. We use raw data from 31 experiments and a novel Bayesian meta-analytic approach to document whether stereotype threat is moderated by prior performance, and find evidence for a uniform effect highlighting a lack of moderation. Our approach aids studying other treatment-by-covariate interactions in meta-analyses. Results suggest that moderators related to prior performance as found in many earlier studies are more likely to be chance findings and that stereotype threat is less affected by these moderators as suggested.

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Stereotype threat (ST) is often proffered as an explanation of the observed gap in academic performance between Black and White students (Steele & Aronson, 1995), and the gender gap in quantitative domains (Spencer, Steele, & Quinn, 1999). Many experiments varied the pressure created by negative stereotypes to find performance decrements among test-takers who identify with negatively stereotyped groups. Despite some concerns about potential publication bias (Flore & Wicherts, 2015; Zigerell, 2017), and notwithstanding some failures to replicate the effect in well-controlled studies (Finnigan & Corker, 2016; Flore, Mulder, & Wicherts, 2018; Gibson, Losee, & Vitiello, 2014; Moon & Roeder, 2014), meta-analyses demonstrated that stereotype threat lowers mean test performance in negatively-stereotyped groups (e.g., Doyle & Voyer, 2016; Nguyen & Ryan, 2008).

Stereotype threat theory (Spencer, Logel, & Davies, 2016) states that ST effects are most evident on (and perhaps limited to) tests that are relatively difficult and for test-takers who strongly identify with the relevant academic domain. If indeed test difficulty and domain identification moderate the debilitating effects of stereotype threat, one would expect its effects not to be uniform, and that mean effects present only part of the story. Under effect uniformity, the strength of stereotype threat manipulation would be similar for all respondents, unaffected by moderators (reflected below in Scenario 1). If stereotype threat is moderated by test difficulty and/or domain identification, non-uniformity should be evident in relations between the test used as outcome variable in ST experiments and a measure that is widely used as covariate in such experiments, namely premeasured performance in the same domain. We contend that such previous measures are positively related to both test difficulty and domain identification. Here, we use raw data from 31 experiments to study whether the effects of stereotype threat are non-uniform, and hence possibly moderated by these two variables as reflected in a treatment-by-covariate interaction as implied by stereotype threat theory.

The moderation of stereotype threat by premeasured performance

Stereotype threat theory proposes that participants, who identify highly with the targeted academic domain, and are generally more proficient in that domain (Smith & White, 2001). Domain identification is high when performing well is relevant to the participant's identity, and is thought to heighten susceptibility to stereotype threat (Steele, 1997). Second, stereotype threat is expected to only occur when the test is perceived as difficult (Spencer et al., 1999). Although

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test difficulty is often considered as a test characteristic, in modern psychometrics it is formally defined as representing the test-taker's underlying (latent) ability vis-à-vis the difficulty of a test. Specifically, a given test is less difficult for test-takers higher in the underlying ability than for less proficient test-takers. Because prior ability reflects that latent ability, we can expect later tests - like outcome variables in ST experiments - to be easier for test-takers with higher scores on the pre-measures used as covariate.

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To understand potential moderation of ST effects by test difficulty and domain identification, we consider three scenarios for experiments with two randomized conditions. By randomization, we expect no differences between conditions in means and standard deviations

Figure 1. Potential impact of moderation of stereotype threat by premeasured performance, Scenarios 1-3

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on the premeasured covariate. We assume the absence of floor- or ceiling effects and use linear models throughout.

Scenario 1: Uniform stereotype threat effect. Without moderation, the ST effect is uniform and we expect no interaction between the manipulation and the covariate. Panel 1 of **Error! Reference source not found.** displays a typical result with equal correlations between the covariate and the outcome measure in both stereotype threat and control conditions. Under this scenario, we would also find no systematic differences in variances of the outcome measure between the conditions.

Scenario 2: Larger manipulation effect for high-achieving participants. If indeed higher prior ability reflects higher domain identification, and, hence, stronger ST effects, the stereotype manipulation is not equally effective across the range of the premeasured performance. This leads to a manipulation-covariate interaction (see Figure 1, Panel 2), where highly able participants are more affected. The correlation between the outcome measure and the covariate will be smaller in the stereotype threat condition than in the control condition. The test scores at the high-end of the distribution are suppressed in the stereotype condition, resulting in diminished variance on the outcome measure. If correlations or variances differ across conditions, regression weights across these groups, by definition, differ, leading to violation of the homogeneity of regression slopes, yielding biased mean adjustments in analysis of covariance (ANCOVA; Cochran, 1957).

Scenario 3: Stereotype threat effect for low-achieving participants. If test difficulty moderates the effect at the individual level, we expect tests to be most challenging for those with lower ability. We assume a test of average difficulty, where test difficulty is always relative to the test-taker's ability, reflected in the literature by Nguyen and Ryan (2008) who classify 41% of their studies to contain easy-to-moderately difficult tests, and Flore and Wicherts (2015) who report that their participants answered, on average, 51% of the items correctly. Under a manipulation-covariate interaction (see Figure 1, Panel 3), The scores of the lower achieving participants are affected, which leads to a *stronger* correlation between the covariate and the outcome measure in the stereotype threat condition compared to the control condition (see also Wicherts & Zand Scholten, 2010). The scores on the low-end of the distribution are lower in the experimental condition, and the variance of this condition will be larger than the variance of the

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control group. The implications for the ANCOVA are similar as under Scenario 2, albeit in the opposite direction.

The current study

We study the difference in correlations and variances between the experimental and the control group for the stereotyped participants using raw data from 31 stereotype threat experiments. This is also relevant for the commonly used ANCOVA analyses, which faced scrutiny within the ST literature (Stoet & Geary, 2012; Wicherts, 2005; Yzerbyt, Muller, & Judd, 2004). We opted for a meta-analytic approach using raw data from ST experiments, to circumvent low power to detect treatment-by-covariate interactions in individual studies, and because premeasured performance is an individual difference variable that cannot be adequately captured by averaging over all included participants of studies as has been done in previous (study-level) meta-analyses. The scenarios present three conflicting hypotheses, which we formulated for the negatively stereotyped groups only, as positively stereotyped groups should not be affected by stereotype threat. We preregistered our hypotheses and analyses (see https://osf.io/23t7m?view_only=d197ac7cef0f4654a93bdd535203b6d5), and stated not having a priori preferences for any of the scenarios and estimate Bayesian models that can provide evidence for either of the hypotheses.

Methods

Data collection

We extracted stereotype threat experiments from published meta-analyses on stereotype threat (Doyle & Voyer, 2016; Flore & Wicherts, 2015; Nadler & Clark, 2011; Nguyen & Ryan, 2008; Picho, Rodriguez, & Finnie, 2013; Shewach, Sackett, & Quint, 2019; Stoet & Geary, 2012; Walton & Cohen, 2003) and requested the raw data from the studies that met our inclusion criteria. Details on our data-collection are available in our preregistration (https://osf.io/23t7m?view_only=d197ac7cef0f4654a93bdd535203b6d5); minor deviations are included in the SOM-R materials. We used the following inclusion criteria: 1) the study included a covariate that was a measure of previous academic performance. Outcome measures could span a wide variety of academic tests, such as GRE items, TIMSS items, or GMAT items. Covariates were measures of previous performance based on self-report, SAT scores, FCAT scores, ACT scores, GPA, or a score on another academic test that was administrated at least one day before

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original data collection. 2) The study included a randomized, experimental design. 3) The study concerned either the gender stereotype or a comparison of White and Black students. 4) If the study included multiple factors group membership (e.g., Gonzales, Blanton, & Williams, 2002), we include gender as natural group when the test entailed mathematics, and minority status if the test entailed verbal items. 5) If the study includes multiple tests, we chose one for a particular group (matching the stereotyped group or including the longest test). We identified 63 eligible studies, from 47 articles. In total, the data of 18 studies were already available to us before data collection started (see *knowledge of data* in the preregistration) as they had been requested from authors by JMW over the years. The corresponding authors of the remaining 45 included studies were contacted by email, from whom we received 18 additional studies, resulting in 36 studies. After preregistration but before data analysis, we excluded four studies that did not meet the inclusion criteria. In the analysis stage, one study was excluded, as there were only two stereotyped participants in the stereotype threat condition, who had both an outcome measure and a covariate observation, resulting in a final sample of 31 studies. An exploratory power-analysis demonstrated 66.94% power, assuming 40 studies with 40 participants included in each study, and a small Cohen's q and heterogeneity (see SOM-R for details). The full set of 63 studies, coming from 47 articles, is cited, but individual studies are not identified in the results section below for reasons of privacy. As a study using anonymized secondary data, our study was exempt from ethical review by the Ethical Review Board at Tilburg University (EC-2018.EX204).

Data handling

We created subsets of the data based on stereotype threat condition. The stereotype threat condition was defined as the condition where the performance of the stereotyped group is expected to be reduced compared to some safe condition. This category included both manipulation where stereotype threat is introduced and manipulations where it is not actively removed. In the control condition, threat was taken away or not introduced at all, allowing for normal performance in the stereotyped groups. Note that these two definitions overlap, and we follow the description of the stereotype threat condition as reported in the original study. Control conditions were collapsed for studies that included more than one control condition. If a dataset contained a three-way interaction, we split the dataset also according to those corresponding levels, resulting in, in total, 41 data subsets, from 31 studies. Outcome measures and covariates

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were standardized per study. Lastly, we included an indicator of the randomization technique employed by the original researchers, and an indicator of whether the scores on prior performance (covariate) were gathered (typically through self-report) either before or after the manipulation.

We excluded all participants who were excluded in the original studies in the confirmatory models. If the original article was unclear on exclusions or when we received a dataset in which participants were already excluded, we excluded no (additional) participants. All descriptives and effect sizes were estimated, using default case-wise deletion. After standardizing, we estimated the effect sizes (Cohen's d , Cohen's q and variances ratio T), and the corresponding variances. All data handling, and analyses were conducted in R (R Core Team, 2021), and standardized descriptive data and effect sizes, and analysis scripts are available on the Open Science Framework

(https://osf.io/6rfvh/?view_only=d197ac7cef0f4654a93bdd535203b6d5;

https://osf.io/9smp2/?view_only=d197ac7cef0f4654a93bdd535203b6d5).

Confirmatory analyses:

We assessed potential moderation by premeasured performance on the correlations between the covariate and the outcome measure. We computed the Pearson correlation coefficient within each experimental cell for each included study. Then, we computed the differences between the correlations based on Cohen's q (e.g., Wicherts & Zand Scholten, 2010), which was defined as the difference between Fisher-transformed standardized correlations of the stereotype threat- and the control group. The Fisher transformation is defined as:

$$r_{jz} = \frac{1}{2} \ln \frac{(1 + r_j)}{(1 - r_j)} \quad (1)$$

, where r_{jz} represents the transformed correlation within an experimental cell, $\ln$ the natural logarithm, and r_j the correlation within an experimental cell (see Borenstein, Hedges, Higgins, & Rothstein, 2011, p. 264). Consequently, Cohen's q per experiment can be defined as:

$$q = r_{stz} - r_{cz} \quad (2)$$

, where r_{stz} is defined as the standardized correlation in the stereotype threat condition, and r_{cz} as the standardized correlation in the control condition. The variance of q is defined as:

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$$Var(q) = \frac{1}{n_{st} - 3} + \frac{1}{n_c - 3} \quad (3)$$

, where n_i are the sample sizes of the stereotype threat- and the control group, respectively (only participants who both had a covariate and an outcome measure score). Cohen's q is positive when the correlation in the stereotype threat condition is higher than in the control condition (Scenario 3), and negative when the correlation in the stereotype threat condition is lower than in the control condition (Scenario 2). Cohen's q is zero when there are no differences across conditions in terms of correlations (Scenario 1), and there is thus no impact of premeasured performance, or when Scenarios 2 and 3 counteract each other.

Next, we fitted a meta-analysis over the log-transformed ratios of the variances of test score of the conditions of the included studies (Hedges & Friedman, 1993; Katzman & Alliger, 1992). Under the null hypotheses, variances across groups are equal, and log-transformed ratios should be equal to zero. The variance in the stereotype threat condition could be smaller (ratio < 0, Scenario 2) due to the influence of the domain identification moderator, as the scores of the high-ability women are suppressed downwards. Alternatively, the variance in the stereotype threat condition could be larger (ratio > 0, Scenario 3) than the variance in the other conditions as the lower achieving test-takers' scores are extended downwards. We computed the natural logarithm of the ratio between the variances on the outcome measure in the stereotype threat condition and the control condition.

$$T = \ln\left(\frac{sd_{ST}^2}{sd_c^2}\right) \quad (4)$$

, with variance

$$Var(T) = \frac{2}{(n_{st} - 1)} + \frac{2}{(n_c - 1)} \quad (5)$$

, where T is the effect size; sd^2 the variance in either the stereotype threat or the control group; and n_i the sample size in either the stereotype threat or the control group (Hedges & Friedman, 1993, p. 102). Exploratory analyses, except a meta-analysis on the average ST effect,

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can be found on the Open Science Framework

(https://osf.io/xuc52/?view_only=d197ac7cef0f4654a93bdd535203b6d5).

Statistical models

We estimated meta-analytic models using the metaBMA-package (Heck, Gronau, & Wagenmakers, 2019) and metafor and (Viechtbauer, 2010). We estimated a fixed-effect model with a zero effect size; a fixed-effect model with a non-zero effect size; a random-effects model with a zero effect size; and a random-effects model with a non-zero effect size. Models with a non-zero effect size reflect the alternative hypothesis and, thus, differences in correlations and variance ratios over groups. Next, differences in correlations between experimental conditions itself could differ over studies for reasons related to, for example, sampling, type of academic test, the strength of the manipulation, or setting. Meta-analysis deals with this by estimating variance of the effect sizes (e.g., Cohen's d , Cohen's q or the variance ratio T , indicated in the priors by μ) across studies; a parameter denoted by τ^2 . Model 4 reflects the hypothesis that the variance of the effect size estimate is non-zero and we observe heterogeneity. We consider support for one of the four particular models as support for an estimate being (non-) zero (for an example, see Gronau et al., 2017). Support for a model estimated by means of posterior probabilities, denoted as $P(\theta|X)$, where θ reflects the model and X the data. metaBMA only provides estimates of τ rather than τ^2 . For consistency and comparability with the results of metafor, we also report squared estimates next to estimates of τ . Note that standard deviations, and highest probability-density intervals (HPDI) provided by metaBMA relate to τ , whereas standard errors and confidence intervals (CI) provided by metafor relate to τ^2 .

Results

Mean effect of stereotype threat among negatively stereotyped groups.

We exploratively compared the standardized mean differences on the outcome measure between the experimental- and control groups among negatively stereotyped groups by means of Cohen's d ($k = 41$, unadjusted for the covariate, for formulas see Borenstein et al., 2011, p. 27). This represents the standard effect size related to the mean effect and would replicate previous meta-analyses that focused on the negative mean (standardized) effect of stereotype threat on test performance. We estimated the four possible models: A null model where Cohen's d is equal to zero and there is no heterogeneity (Fixed, H_0); a fixed effect model where Cohen's d is non-zero

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(Fixed, H1); a random effects model with a zero effect size estimate for Cohen's d , but non-zero heterogeneity (Random, H0); and a random effects model with both a non-zero estimate for Cohen's d and for heterogeneity (Random, H1).

Figure 2. Forest plot of the included effect sizes (Cohen's d) including a Bayesian model-averaged (averaged), fixed-effect, and random-effects meta-analysis. Estimates are based on the Cauchy prior for μ , and the inverse gamma prior for τ .

In the forest plot (see Figure 2), we show both the results of the standardized mean difference in the outcome measure corrected by model uncertainty (in terms of Cohen's d ; grey lines) and the corresponding regular estimates of Cohen's d (black lines). Open circles or

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triangles reflect zero effect sizes, closed symbols reflect estimates that differ from zero. We estimate three average effect sizes: one averaged over all four potential models, one from the fixed-effect meta-analysis, and one from the random-effects meta-analysis. We report model-averaged estimates in text.

Table 1

Posterior probabilities for the meta-analytic models of the standardized mean effect of ST among negatively stereotyped groups (in terms of Cohen's d)

	Model	Model prior probability	Cauchy	Standard normal	Normal (SD = 100)	Normal (SD = 10)
Mean effect (μ)	Fixed, H0	0.25	0.000	0.000	0.000	0.000
	Fixed, H1	0.25	0.000	0.000	0.000	0.000
	Random, H0	0.25	0.014	0.014	0.561	0.113
	Random, H1	0.25	0.986	0.986	0.439	0.887

Posterior probabilities for all four models per prior distribution can be found in Table 1. These probabilities reflect the support for a model given the data under one of the four priors distributions (the Cauchy prior, and the three normal priors that reflect the sensitivity analysis). Thus, a posterior probability of, say, $P(\theta|X) = 0.887$ indicates that the chance is 0.887 that random-effects model with a non-zero effect size is the best-fitting model, given the data. The Bayesian hypothesis tests support a random-effects model with a non-zero stereotype threat standardized mean effect on test performance ($P(\theta|X)'s > 0.887$). Under this model, the means across the experimental groups differ, on average, and there is a non-zero variance among these effect sizes. Only the results under the least informative prior for μ , show similar levels of support for both random-effects models ($P(\theta_{H1}|X) = 0.439$). Thus, although a non-zero ST effect is considered most likely, this conclusion may depend on the prior choice. The negative estimate of Cohen's d is small-to-medium: $\hat{d} = -0.35$, $SD = 0.10$ (HPDI95% [-0.55; -0.17]), for the Cauchy prior (with frequentist estimates: $\hat{d} = -0.37$, $SE = 0.10$, 95%CI [-0.56; -0.18]). Hence, in line with the theory, stereotyped participants in the experimental conditions underperformed

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on average compared to the stereotyped participants in the control conditions. The Bayesian random-effects meta-analysis estimates $\hat{\tau} = 0.52$ ($\hat{\tau}^2 = 0.27$, $SD = 0.10$, HPDI95% [0.34; 0.71]; frequentist estimates $\hat{\tau}^2 = 0.28$, $SD = 0.09$), suggesting variation over studies in the size of the stereotype threat standardized mean effect on test performance. Stereotype threat effects differ across studies, ranging from $d = -2.86$ to $d = 1.06$. The spread among the true effect sizes may potentially be caused by our sample of studies. We included both studies on ethnicity- and gender differences, and included participants of all ages. During data-analysis, we noticed that Cohen's d for Study 6A is exceptionally large (Cohen's $d = -2.86$). We post-hoc reran the models without this study included and retrieved an estimate of $\hat{d} = -0.30$ ($SD = 0.09$, HPDI95% [-0.47; -0.14]). Thus, the mean difference is lower without this outlier included, but the mean ST effect remains small-to-medium. Similarly, estimates for τ remained similar (0.42, $SD = 0.09$, HPDI95% [0.26; 0.59]).

Confirmatory results: does premeasured performance moderate stereotype threat?

Cohen's q . To investigate the influence of premeasured performance on ST effects, we conducted a Bayesian meta-analysis on the differences in Fisher-transformed correlations between the control- and the stereotype threat condition in the stereotyped groups, as estimated by Cohen's q . Cohen's q is zero when there are no differences, negative when the correlation is higher in the control group (Scenario 2), and positive with the correlation is smaller in the control group (Scenario 3).

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Figure 3. Forest plot of the included effect sizes (Cohen's q) including a Bayesian model-averaged (averaged), fixed-effect, and random-effects meta-analysis. Estimates are based on the Cauchy prior for μ , and the inverse gamma prior for τ .

Table 2

Posterior probabilities for the meta-analytic models of the difference in Fisher-transformed correlations between experimental groups (Cohen's q)

Model	Model prior probability	Cauchy	Standard normal	Normal (SD = 100)	Normal (SD = 10)
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Mean effect (mu)	Fixed, H0	0.25	0.809	0.813	0.851	0.847
	Fixed, H1	0.25	0.042	0.037	0.000	0.004
	Random, H0	0.25	0.142	0.142	0.149	0.148
	Random, H1	0.25	0.008	0.007	0.000	0.001

We estimate the same four meta-analytic models as for the standardized mean difference (see Figure 3). In Table 2, we report the posterior probabilities for each of these models across all included priors for Cohen's q . Under all priors, the null model is considered to most likely ($P(\theta_{null}|X) \geq 0.809$), indicating that the Cohen's q is close to zero and that correlations do not differ across experimental conditions. Cohen's q is estimated as $\hat{q} = 0.02$, with $SD = 0.04$ (and HPDI95% reaching from -0.06 to 0.10; with frequentist estimates $\hat{q} = 0.02$, $SE = 0.04$, $CI95\%$ [-0.06; 0.10]).

The estimate for the between-study variance $\hat{\tau}$ for the inverse gamma prior of τ is close to zero, $\hat{\tau} = 0.08$ ($\hat{\tau}^2 = 0.01$, $SD = 0.04$, and HDP95 [0.02; 0.16]; with frequentist estimates $\hat{\tau}^2 = 0.00$, $SE = 0.01$). Thus, there is negligible between-study variation, and Cohen's q is estimated to be relatively consistent across studies. Based on these results, we found most evidence that there is no difference in correlation between the stereotype threat condition and the control condition, and hence that the ST effect is uniform with respect to the moderator premeasured ability. We also found little evidence that this uniformity differed over included studies.

Variance ratio. We also investigated the potential moderation of premeasured performance of the ST effect by investigating the variances of the outcome-measure test scores across experimental groups. We expected that the variances of the test scores that were included as outcome measure across the groups would differ between conditions. Note that all variance ratios are log-transformed, and, that variance ratios of zero reflect no differences across conditions. Again, we fit the same four meta-analytic models as above.

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Figure 4. Forest plot of the included effect sizes (variance ratio T) including a Bayesian model-averaged (averaged), fixed-effect, and random-effects meta-analysis. Estimates are based on the Cauchy prior for μ , and the inverse gamma prior for τ .

Table 3

Posterior probabilities for the meta-analytic models of the ratio of the variance of the outcome measure score across experimental conditions (variance ratio T)

Model	Model prior probability	Cauchy	Standard normal	Normal (SD = 100)	Normal (SD = 10)
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Mean effect (mu)	Fixed, H0	0.25	0.634	0.638	0.677	0.673
	Fixed, H1	0.25	0.044	0.039	0.000	0.004
	Random, H0	0.25	0.302	0.304	0.322	0.320
	Random, H1	0.25	0.021	0.019	0.000	0.002

We find most evidence for a null model (See Table 3 and Figure 4), where the variance ratio is equal to zero and there is no heterogeneity across studies in this variance ratio. These results are reflected in the average estimate for the variance ratio. The model-averaged estimate for $\hat{\tau} = -0.03$ (SD = 0.05, HPDI95% [-0.13; 0.08]; with frequentist estimates $\hat{\tau} = -0.03$, SE = 0.05, CI95% [-0.13; 0.08]). Therefore, there are no clear differences in the variances across conditions. For the estimates of τ , we find no between-study variance. Posterior probabilities range from 0.590 to 0.803, for the fixed-effect model with a zero effect size. Notably, the least-informative prior finds a high posterior probability for the null model. This suggests that the evidence for a potential model including heterogeneity is dependent on our prior choice. τ is estimated $\hat{\tau} = 0.11$ ($\hat{\tau}^2 = 0.01$, SD= 0.06, HDPI95[0.02; 0.23]; with frequentist estimates $\hat{\tau}^2 = 0.01$, SE = 0.02) under the inverse gamma prior. Thus, there is no difference across conditions in variance on the test used in stereotype threat experiments, and there is little heterogeneity across studies.

Sensitivity analyses

As preregistered, we also investigated whether the results were dependent on our knowledge of the data or experimental characteristics (see https://osf.io/xuc52/?view_only=d197ac7cef0f4654a93bdd535203b6d5). We excluded studies of which we already had the data prior to preregistration, and assessed whether randomization strategy and time point at which the covariate was measured affected the results. Results were similar over all included subsets. Since there was evidence that results were dependent on prior choice, we exploratively refitted the confirmatory models with three non-informative priors for tau. Results show clear evidence for the null models, in contrast to the preregistered analyses (all

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$P(\theta|X) > 0.920$), whereas estimates for tau were stable over prior choice, indicating lack of heterogeneity among the effect sizes.

In summary, our meta-analyses provide evidence for no moderation by premeasured performance on stereotype threat results, and hence support the notion that stereotype threat is uniform over different levels of premeasured performance.

Discussion

We analyzed the findings of 31 studies to investigate whether the stereotype threat is moderated by premeasured ability. We found support for a significant mean stereotype threat effect ($d = -0.35$), but note that the current sample is highly selective and not reflective of the stereotype threat literature at large. The estimate for Cohen's d was not corrected for publication bias. To study the uniformity of ST effects, we estimated Bayesian and frequentist meta-analytic models for Cohen's q (the difference in correlations between the outcome measure and the covariate) and the variance ratio T (the difference in variances on the outcome measure). If Cohen's q or the variance ratio T is non-zero, the covariate may have moderated the strength of the manipulation. In the confirmatory analyses, we found no support for moderation and heterogeneity in the Bayesian analysis for both effect sizes, and hence our Scenario 1 is clearly supported by the data.

The strength of the stereotype threat manipulation did not depend on premeasured performance, resulting in effect uniformity in the current sample. As premeasured performance is related to both domain identification and test difficulty, these moderators may not affect the strength of the manipulation. This is unexpected, as more-able participants are expected to be more susceptible to stereotype threat, and only tests perceived as being difficult are expected to elicit ST experiences (Keller, 2007). We may have selected published studies with a relatively strong, and, thus, uniform effect, where potentially only these studies are available under publication bias (Zigerell, 2017). We note that (actual) heterogeneity in the stereotype threat effects would typically lower the power to detect these effects, and we do not preclude the possibility that studies with more heterogeneity are less well represented in our sample, and that results may differ when more unpublished studies with less pronounced effects would be included.

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We argued that the moderation of premeasured performance may cause ANCOVA assumption violations (Wicherts, 2005). As ST effects do not seem moderated by premeasured ability, our concerns are not supported. However, when both stereotyped and non-stereotyped participants are included in a study, mean differences on the covariate may still exist, and form a well-known debate (Lord, 1967; Miller & Chapman, 2001).

Correlations between the covariate and the outcome measures were relatively low, where we expected higher correlations when the outcome measure reflects the covariate's domain. We found an average Pearson's correlation of $r = 0.34$ within the stereotyped group (see https://osf.io/xuc52/?view_only=d197ac7cef0f4654a93bdd535203b6d5, predictive validity) suggesting an unreliable outcome measure and/or some restriction of range caused by the selection of participants in the studies. Low reliability suppresses correlations (e.g., Muchinsky, 1996). Reliability estimates are not commonly reported in ST research but appear relatively low (Flore, 2018), arguably because tests are often short. Restriction of range may also affect correlation strength, due to the preselection of participants based previous performance (Spencer et al., 1999). Post-hoc, we compared the standard deviation on the actual SAT-M in 2009 ($SD = 112$, in 2009; Ball, Cribbie, & Steele, 2013) to the standard deviations of some SAT-M covariates. The standard deviations on the covariate in our sample are lower: $SD = 86.26$ (Johns, Schmader, & Martens, 2005); $SD = 56.54$ (study 1, Chalabaev, Major, Sarrazin, & Cury, 2012); $SD = 65.08$ (Lesko & Corpus, 2006); $SD = 51.16$ (Schmader, 2002). There may be some restriction of range on the possible covariate values; lowering the degree to which the covariate could affect the strength of the manipulation.

We assumed premeasured performance to be a proxy for domain identification and test difficulty. Domain identification as measured by the Domain Identification Measure is in part assessed by performance, but also by motivation and interest which are not directly captured in an SAT or GPA score (Smith & White, 2001). Moreover, prior performance does not directly capture participants' latent ability in a domain. Utilizing direct measures of domain identification or test difficulty may change conclusions. In addition, the opposing moderation in Scenarios 2 and 3 may counteract each other, on average. We deem this unlikely and failed to find heterogeneity across the studies to support this idea.

Prior distribution choices may have steered the results from preregistered analyses, providing mixed support for fixed-effect and random-effects models, even though the

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heterogeneity estimates and the frequentist tests showed consistent small estimates, and null results. The least informative preregistered prior and the explorative uninformative priors found stronger support for a fixed-effect model. Mixed results could be due to prior choice rather than effect size heterogeneity, illustrating the importance of proper prior specifications and sensitivity analyses.

Summarizing, we recovered a stereotype threat effect in the selected datasets with significant heterogeneity of the stereotype threat effect across studies. However, we found that this ST effect not to be moderated by premeasured performance. Therefore, the moderating effect of domain identification or test difficulty, defined by premeasured performance, appears not as strong as assumed. We note that our method can be applied more widely in meta-analyses of treatment-by-covariate interactions. A rigorous assessment of the robustness of the ST effect, the theoretical meaning of moderation of the ST effect, and the key question of generalizability of the ST effect would benefit from more rigorous (preferably large and preregistered), openly reported studies.

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Appendix A: Additional methods (to SOM-R)

Power

We conducted an exploratory power analysis in which we assessed the power of our frequentist random-effects meta-analytic models for Cohen's q under various conditions (see https://osf.io/hvzfn/?view_only=d197ac7cef0f4654a93bdd535203b6d5). We assumed 40 studies, each containing 40 or 80 participants in two conditions, a Cohen's q of 0.15 or 0.25, and a ratio between the between-study- and within-study variance of 0.33, 0.667 or 1, reflecting small, medium and large heterogeneity (Borenstein et al., 2011, p. 270). Throughout, we assumed a conventional alpha level of 5%. Power to detect the mean effect size estimate ranged from 49%-99%, only displaying power levels under 66% for meta-analytic models with a large amount of heterogeneity. The power for the test of homogeneity only depends on the number of studies, and ranged between 38% to detect a small amount of heterogeneity, and 92% for a large amount of heterogeneity.

Deviations from preregistration

Included datasets. The list of the studies in the preregistration (<https://osf.io/2cp37/>, pages 24 to 26) erroneously contains 60 studies instead of 63. Schmader (2002) is included twice. The preregistration correctly specifies the included datasets under 'Prior Knowledge current dataset' and we did not obtain the data of this study before the study was preregistered. Similarly, four additional studies were not included in the overview of included studies in the preregistration, but are correctly included in the sample on page 7 of the preregistration, resulting in a maximum sample size of 63 studies (see https://osf.io/w8brd/?view_only=d197ac7cef0f4654a93bdd535203b6d5 for an updated complete list). We already had 18 datasets available to us; this number was incorrectly identified as 14 in the preregistration, one of which was incorrectly identified on list of unavailable studies and for which we erroneously contacted the author. We excluded (Steele & Aronson, 1995; study 3, before data collection) and Schmader and Johns (2003; studies 1 and 2, during data collection), as these studies did not include an academic outcome measure. We excluded Marx and Goff (2005) and Marx and Ko (2012) during data collection as these studies did not include an academic covariate.

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Data analysis. Opposed to list-wise deletion as we specified in the preregistration, to avoid the unnecessary exclusion of participants. We did not run any of the analyses using list-wise deletion. We deviated from our preregistered models by utilizing a different analysis package and model-test method. However, as intentions for the current project were explorative, and we did not have explicit hypotheses on the level of heterogeneity, we do not believe that the change in analysis method affected the results. Lastly, we estimated the variances of the effect sizes on the basis of the number of participants with an available score on either the outcome measure or covariate rather than all participants who were included in the data. This modification to the variances was not explicated in the preregistration.

Priors

Priors are briefly depicted below, and explicated in our preregistration (https://osf.io/2cp37/?view_only%3Dd197ac7cef0f4654a93bdd535203b6d5/). Prior names reflect the prior-specific specified distribution shape. We specify a main prior for parameters μ (Cauchy prior) and τ (inverse gamma prior), and include, respectively, three or four additional priors as a sensitivity analysis. For the measure of effect size (μ), we defined a Cauchy-distributed prior, with $\sim C(0, \frac{\sqrt{2}}{2})$ (Gronau et al., 2017; Morey & Rouder, 2018). For the heterogeneity measure τ , we defined an Inverse Gamma-distributed prior $\sim IG(\alpha = 1, \beta = 0.15)$,

Figure 6. Preregistered priors for μ

Figure 5. Preregistered priors for τ

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using the estimates of τ from generally found in psychology (Gronau et al., 2017; Van Erp, Verhagen, Grasman, & Wagenmakers, 2017). We reported the estimated coefficients for the mean effect (random-effects model) or sole effect (fixed-effect model) parameter μ and the τ parameter for the between-study variance (random-effects models only) for all prior choices on the Open Science Framework

(https://osf.io/kpjnc/?view_only=d197ac7cef0f4654a93bdd535203b6d5). As another check on the sensitivity of results to the choice of prior, we estimated the same meta-analysis under a frequentist framework, using the metafor-package and a REML-estimator. We report the results of our main Bayesian analyses and only report results of other priors when they differ from the results under the main priors (see

https://osf.io/kpjnc/?view_only=d197ac7cef0f4654a93bdd535203b6d5).

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Studies from which the data was not available to us prior to preregistration

1. Ben-Zeev, Fein, and Inzlicht (2005)
2. R. P. Brown and Josephs (1999), study 1
3. R. P. Brown and Josephs (1999), study 2
4. R. P. Brown and Josephs (1999), study 3
5. Chalabaev et al. (2012), study 1
6. Chalabaev et al. (2012), study 2
7. Cherney and Campbell (2011)
8. Clark, Eno, and Guadagno (2010) , study 4
9. Elizaga and Markman (2008)
10. Finnigan and Corker (2016)
11. Grand, Ryan, Schmitt, and Hmurovic (2010)
12. Huguet and Régner (2007), study 1
13. Huguet and Régner (2009), study 2
14. Inzlicht and Ben-Zeev (2000), study 1
15. Inzlicht and Ben-Zeev (2000), study 2
16. Inzlicht and Ben-Zeev (2003)
17. Inzlicht, Aronson, Good, and McKay (2006)
18. Jones (2011), study 1
19. Jones (2011), study 2
20. Josephs, Newman, Brown, and Beer (2003), study 1
21. Keller (2002)
22. Lesko and Corpus (2006)
23. Marchand and Taasobshirazi (2012)
24. Marx and Goff (2005)
25. Marx and Ko (2012)
26. Marx and Roman (2002), study 1
27. Marx and Roman (2002), study 2
28. Marx and Roman (2002), study 3
29. McFarland, Lev-Arey, and Ziegert (2009)
30. Nguyen, O'Neal, and Ryan (2003)
31. Oswald and Harvey (2000)
32. Rivardo, Rhodes, and Klein (2008)
33. Schmader (2002)
34. Schmader and Johns (2003), study 2 (Excluded, no academic test as dependent variable)
35. Schmader, Johns, and Barquissau (2004), study 2
36. Shaffer, Marx, and Prislin (2013)
37. Smeding, Dumas, Loose, and Régner (2013)
38. Smeding et al. (2013)
39. Steele and Aronson (1995), study 3 (Excluded prior to data collection)
40. Tomasetto, Matteucci, and Pansu (2012)
41. Walsh, Hickey, and Duffy (1999)
42. Wen, Zuo, Wu, Dong, and Wang (2016), study 1
43. Wen et al. (2016) , study 2
44. Wout, Danso, Jackson, and Spencer (2008) , study 1
45. Wout et al. (2008) , study 2

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Studies from which the data was available to us prior to preregistration

46. Inzlicht et al. (2006), study 2 (Erroneously identified as not available in the preregistration)
47. Broadnax, Crocker, and Spencer (1997)
48. J. L. Brown (2000), study 1
49. J. L. Brown (2000), study 2
50. R. P. Brown and Pinel (2003)
51. Eriksson and Lindholm (2007)
52. Gonzales et al. (2002)
53. Johns et al. (2005)
54. Keller and Dauenheimer (2003)
55. Keller (2007)
56. Schmader and Johns (2003), study 1 (Excluded, no academic test as dependent variable)
57. Steele and Aronson (1995), study 1
58. Steele and Aronson (1995), study 2
59. Steele and Aronson (1995), study 4
60. Ho and Siduanus (2009)
61. Logel, Iserman, Davies, Quinn, and Spencer (2009), study 4
62. Martens, Johns, Greenberg, and Schimel (2006), study 1
63. Spencer et al. (1999), study 3