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No evidence that priming analytic thinking reduces belief in conspiracy theories: A Registered Report of high-powered direct replications of Study 2 and Study 4 from Swami, Voracek, Stieger, Tran, and Furnham (2014)☆

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ABSTRACT

Analytic thinking is reliably associated with lower belief in conspiracy theories. However, evidence for whether increasing analytic thinking can reduce belief in conspiracies is sparse. As an exception to this, Swami et al. (2014) showed that priming analytical thinking through a verbal fluency task (i.e., scrambled sentence task) or a processing fluency manipulation (i.e., difficult-to-read fonts) reduced belief in conspiracy theories. To probe the robustness of these effects, in this Registered Report, we present two highly powered (i.e., 95%) direct replications of two of the original studies (i.e., Studies 2 and 4). We found no evidence that priming analytic thinking through the scrambled sentence task ($N = 302$), nor the difficult-to-read fonts ($N = 488$) elicited more analytic thinking, nor reduced belief in conspiracy theories. This work highlights the need for further research to identify effective ways of inducing analytic thinking in order to gauge its potential causal impact on belief in conspiracies.

The rapidly expanding psychological literature on conspiracy beliefs reflects the growing academic and societal awareness of their prevalence and consequentiality. Conspiracy beliefs, i.e., beliefs that certain significant social and political events and circumstances are a result of secret plots by groups of powerful people (Douglas et al., 2019), have been repeatedly linked to negative outcomes, both on an individual and societal level (Jolley, Marques, & Cookson, 2022). For instance, belief in conspiracy theories is related to lower support for pro-environmental policies (Biddlestone, Azevedo, & van der Linden, 2022) and less adherence to medical advice (e.g., Oliver & Wood, 2014). This was particularly striking during the COVID-19 pandemic—conspiracy beliefs were repeatedly linked to less adherence to recommended COVID-19 protective measures, and in particular, to increased COVID-19 vaccine hesitancy (Ruggeri et al., 2022; van Mulukom et al., 2022). Furthermore, conspiracy beliefs can lower institutional trust (Pummerer et al., 2022), elicit fear of social exclusion (Lantian et al., 2018), and increase support for violent political action (Imhoff, Dieterle, & Lamberty, 2021).

Although correlational, cross-sectional research has generated important insights into the structure of conspiracy beliefs, as well as their epistemic, existential and social motivation correlates (Douglas et al., 2019; Douglas, Sutton, & Cichocka, 2017; van Prooijen & Douglas, 2018), experimental work on the causes and consequences on conspiracy beliefs is still limited (Sassenberg, Bertin, Douglas, & Hornsey, 2023). Even more difficult to come by are studies on potential interventions to reduce conspiracy beliefs (Krekó, 2020; but see Banas & Miller, 2013; Jolley & Douglas, 2017; Orosz et al., 2016). Among exceptions to this is the highly cited finding that priming analytic thinking reduces belief in conspiracy theories (Swami et al., 2014).

Indeed, one of the most studied cognitive factors thought to play a role in conspiracy beliefs is (lower) analytic thinking—a mode of information processing characterized as rational, logic-based, intentional, slow, and primarily verbal (Epstein, 1991). A negative correlation between analytical thinking and conspiracy beliefs is well established (Georgiou, Delfabbro, & Balzan, 2021; Gligorić et al., 2021; Gligorić, Većkalov, & Žeželj, 2018; Šrol, 2022; see Yelbuz, Madan, & Alper, 2022 for a meta-analysis). Furthermore, recent data from 45 countries suggests that this relationship is also involved in important behavioral outcomes: analytical thinking was related to higher adherence to COVID-19 public health guidelines through reduced conspiracy beliefs (Kantorowicz-Reznichenko, Folmer, & Kantorowicz, 2022).

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An analytical approach to information processing could function as a buffer for belief in conspiracies given that conspiracy theories can be considered oversimplified, straightforward explanations for otherwise complex events (Hofstadter, 1966; van Prooijen, 2017). In other words, analytic thinking can arguably override the intuitive appeal of conspiracy theories (Ståhl & van Prooijen, 2018) and make their oftentimes faulty logic more apparent. Eliciting analytical thinking could then lead to lower endorsement of conspiracy theories. However, despite a large evidence base for the negative correlation between analytic thinking and conspiracy beliefs, the causal relationship is often implied in the literature but supported by only one set of studies.

1. The Swami et al. (2014) studies

Swami et al. (2014) conducted four studies and found that priming analytical thinking reduces belief in well-known conspiracy theories. In Study 1, the authors found the often-reported negative correlation between analytical thinking style and conspiracy beliefs. Studies 2–4 experimentally tested this relationship and found that priming analytical thinking reduces conspiracy beliefs. More specifically, Study 2 found that individuals primed with words related to analytic thinking (e.g., *logic*, *analyze*) in the scrambled sentence verbal fluency task (Gervais & Norenzayan, 2012) reported lower conspiracy beliefs, in comparison to individuals primed with neutral words. Studies 3 and 4 employed the disfluent font manipulation of analytic thinking: individuals presented with a questionnaire in a difficult-to-read font (argued to promote effortful, analytical System 2 processing; Alter, Oppenheimer, Epley, & Eyre, 2007; Song & Schwarz, 2008), reported lower conspiracy beliefs compared to individuals presented with the same questionnaires in an easy-to-read font. This sole work is often the only one cited as proof that analytical thinking directly reduces conspiratorial beliefs (Hornsey, Bierwiaczonek, Sassenberg, & Douglas, 2023, p.3; Roberts & Risen, 2022, p.3; Rottweiler & Gill, 2022, p.14; van der Linden, 2022, p.462), and is, according to Web of Science,[1] the most cited experimental research on reducing belief in conspiracies (787 Google Scholar citations at the time of writing).

Despite its prominence in the literature, this set of findings has, to the best of our knowledge, never been directly replicated in the published literature. Besides being frequently cited, there are other compelling reasons to further examine these effects. First, similar findings obtained via (analytical thinking) priming procedures have failed to replicate. Reducing religious belief by implicitly priming analytical thinking did not replicate (Camerer et al., 2018; Sanchez, Sundermeier, Gray, & Calin-Jageman, 2017). Second, using difficult-to-read fonts to manipulate processing fluency and analytic thinking (used in two out of three studies in the original paper) has also been difficult to replicate (Klein et al., 2018; Meyer et al., 2015; Sirota, Theodoropoulou, & Juanchich, 2021; Yılmaz & Saribay, 2016). Third, using the scrambled sentence task to prime religious belief (Shariff & Norenzayan, 2007) failed to produce previously reported effects in a high-powered replication study (Gomes & McCullough, 2015). Finally, some of the original studies reported in Swami et al. (2014) were underpowered to detect the reported effects. This is suggested by the wide confidence intervals for the reported effects (Study 2: $d = 0.46$, 95% CI [0.08–0.83], Study 3: $d = 0.49$, 95% CI [0.20–0.78], Study 4: $d = 0.42$, 95% CI [0.09–0.76] and $d = 0.34$, 95% CI [0.01–0.67]). To further test this assumption, we ran sensitivity power analyses. The results indicate that the smallest effect size Study 2 could have detected, given its sample size ($N = 112$, scrambled sentence task), and with $\beta = 90\%$, is $d = 0.62$. The smallest effect size that Study 3 could have detected with the same power ($N = 189$, disfluent fonts manipulation) is $d = 0.47$. Finally, the smallest effect size that Study 4 ($N = 140$, disfluent fonts manipulation) could have detected with $\beta = 90\%$ is $d = 0.55$. This indicates that two out of the three experimental studies (i.e., Study 2 and Study 4) were insufficiently powered to detect the reported effects reliably (see Appendix A for details of the power analyses).

Taken together, these points make the studies by Swami et al. (2014) excellent candidates for a valuable direct replication (Nosek, Spies, & Motyl, 2012; Simons, 2014). According to Isager et al. (2021), when considering the replication value of a study, researchers should take into account (1) the value of having knowledge about the research claim, (2) the uncertainty of the present state of knowledge about the claim, as well as (3) the ability of the replication to reduce uncertainty (i.e., replication quality). Given the grave consequences of conspiratorial beliefs (Jolley et al., 2022), there is a need to develop interventions to reduce them. However, the overall experimental evidence for such interventions is scarce, with the work of Swami et al. (2014) being a prominent exception. As it stands, the current literature seemingly suggests that reducing conspiratorial beliefs via analytic priming is the most effective way to tackle them. Therefore, having knowledge about the main claims from Swami et al. (2014) is highly valuable. Second, the questioned effectiveness of the techniques used to elicit analytic thinking, low power in the original studies, and the lack of replications of this effect amount to high uncertainty about the conclusion that inducing an analytic mindset reduces belief in conspiracy theories. Finally, to ensure that the replication effort indeed has the potential to reduce uncertainty about the effect of priming analytical thinking on conspiracy beliefs, highly powered and high-quality samples are needed. Thus, our aim is to closely replicate the studies on the effects of priming analytical thinking on conspiratorial beliefs using two highly powered, preregistered studies (replication of Studies 2 and 4 from the original paper).

2. Study choice

Out of the four studies in the original paper, three studies (Studies 2–4) were experimental. Therefore, these three studies were candidates for replication. Study 2 used the scrambled sentence task as a manipulation of analytic thinking, whereas Studies 3 and 4 used the disfluent font paradigm. We aimed to test both manipulations. Given that Studies 3 and 4 used the same experimental procedure to induce analytical thinking, we selected one of them (i.e., Study 4) for two reasons. First, unlike Study 3, it was conducted on a more diverse (non-student) sample. The second reason is practical—Study 3 had a two-wave data collection, while Study 4 had only one wave, making it less costly to replicate whilst still testing the same manipulation of analytic thinking (i.e., disfluent font) and its effect on conspiracy beliefs. Therefore, we decided to replicate Studies 2 and 4 from the original paper.

3. Methodological comparisons between original and replication studies

Differences between the original and replication studies are reported in Table 1. The main difference is mode of data collection—on site (lab) data collection in the original studies, as opposed to online samples in the replications. However, replication outcomes depend substantially more on the effect being studied, as opposed to minor procedural deviations, including lab versus online samples (e.g., Klein et al., 2018; Kochari, 2019). Accordingly, following a replication classification

[1] This is supported by Web of Science search results for the following string: ((AB = (conspiracy)) AND AB = (reduc*)) OR ((AB = (conspiracy)) AND AB = (chang*)), in which AB = Abstract.

Table 1
Original and replication studies feature comparisons.

		Replication Study 1 (original Study 2)		Replication Study 2 (original Study 4)	
		Original	Replication	Original	Replication
Sample	Size	112	302	140	488
	Type	UK, student	UK, Prolific	UK, community	UK, Prolific
	Attrition	5.1% ($n = 6$)	15.5%	N/A	N/A
	Remuneration	No	Yes	Yes	
	Setting	On-site, private computer cubicle	Online (no mobile devices)	On-site, paper and pencil	Online (no mobile devices)
Study	Design	Repeated (5-week gap)		Between-subjects	
	Priming manipulation	Scrambled sentence task		Disfluent fonts	
	Manipulation check	Moses Illusion Task		Moses Illusion Task	
	DVs	BCTI		GCBS, 7/7 conspiracy theories	
	Distractors	Unknown	BFI-10 & B-SRES	Unknown	BFI-10 & B-SRES

Note. BCTI = Belief in Conspiracy Theories Inventory (Swami et al., 2011).
GCBS = Generic Conspiracist Beliefs Scale (Brotherton, French, & Pickering, 2013).
BFI-10 = 10-item Big Factor Inventory (Rammstedt & John, 2007).
B-SRES = Brief Rosenberg Self-Esteem Scale (Monteiro et al., 2022).

developed by LeBel, McCarthy, Earp, Elson, and Vanpaemel (2018), our studies can be considered "very close"/"close" replications.[2]

Both studies were approved by the University of Amsterdam Ethics Review Board (protocol FMG-1135). For both studies, we report all measures, manipulations and exclusions. All data, materials, and code for power and data analysis are available at: https://osf.io/keahr/.

4. Replication Study 1—Replication of Study 2 from Swami et al., 2014

4.1. Method

4.1.1. Participants

4.1.1.1. Sample planning. The target sample size was preregistered and based on a priori power analyses. The original study included 112 participants and detected the effect size of $d = 0.46$ (analytic vs. control group post-test difference in conspiracy beliefs). We considered two approaches when determining sample size. One of them is to assume H0 is true (i.e., priming analytical thinking does not reduce belief in conspiracy theories). Under this assumption, we would follow Simonsohn's (2015) recommendation that replication *N* should be 2.5 times higher than the original study *N*. This amounts to a needed sample size of $N = 280$ (i.e., 2.5×112). The second approach is to assume that H1 is true, for which we conducted a power analysis for an independent *t*-test ($d = 0.46$, $\beta = 95\%$, $\alpha = 0.05$). This returned the required sample size of $N = 248$. Given that we did not make assumptions about which hypothesis is true, we based our target replication *N* on the option that returned the higher sample size (i.e., $N = 280$). To account for attrition (from T1 to T2) and data exclusions (see section 4.1.1.2) , which we assumed could be up to 30%, we recruited 400 participants (400–30% = 280) at Time 1. Participants who failed the attention check question were automatically excluded and did not add up to the total sample size. Participants were remunerated in line with the estimated fair wages on the Prolific platform.

4.1.1.2. Data exclusions. To ensure high data quality, we preregistered and applied several exclusion criteria. Only participants who did not exceed a maximum time (determined by Prolific), had an approval rate of 95% or higher on Prolific, passed the attention check, and were not flagged by Qualtrics as likely bots (Q_RecaptchaScore ≥. 50) or fraudulent responses (Q_RelevantIDDuplicateScore ≥75, Q_RelevantIDFraudScore ≥30)[3] were used in the final sample. We also planned to exclude speeders, i.e., participants who completed either of the two surveys extremely fast (three standard deviations below the mean duration, as per Prolific guidelines), but there were none in the sample.

4.1.1.3. Sample. A total of 316 UK Prolific participants completed both waves of the study. After removing responses that did not pass preregistered quality checks (see below) in one of the two waves ($n = 14$), we were left with $N = 302$ participants (147 in the experimental and 155 in the control condition). The sample was balanced in terms of gender with 49% females, 49% males, while 2% indicated a different identification ($M_{age} = 41.68$, $SD_{age} = 14.34$). Most participants had a university bachelor's degree or higher (57.3%). Most participants identified as White (89.1%).

4.1.2. Design

The study followed the same design as the original study, with a 2 (Experimental manipulation: analytic prime vs control prime; between-subjects factor) x 2 (Time: Pretest vs Posttest; within-subjects factor) mixed factorial design. At Time 1, all participants filled out baseline conspiracy belief questionnaires and basic demographics. At Time 2, participants were randomly assigned to either the analytic or the control prime condition.

4.1.3. Procedure

The surveys were hosted on Qualtrics. To increase experimental control, the study was available to participants only from a desktop or laptop computer. We prevented people from participating from mobile devices (i.e., smartphones and tablets) through a Qualtrics filter.

We closely followed the procedure that was used in the original study. Both groups of participants first filled out the Belief in Conspiracy Theories Inventory (BCTI; Swami et al., 2011). As in the original study, the participants also filled out filler scales unrelated to the study. All scales were presented in a randomized order. Finally, we also collected sociodemographic data. Participants were invited to participate in the

[2] The difference between the two is whether there are differences in IV and/or DV stimuli (LeBel et al., 2018). In our case, we have kept all DV stimuli identical, as well as IV stimuli to the extent possible given that the corresponding author of the original publication was not able to retrieve exact wording of IV stimuli in Replication Study 1. Target prime words in the analytic condition were identical. Filler words differed, due to the original materials not being retrievable. We used the same stimuli as Gervais and Norenzayan (2012), as this is what Swami et al. based their materials on. More specifically, Swami et al. (2014) used the same target primes and only changed the additional words in the task.

[3] More information on these filters can be found here: https://www.qualtrics.com/support/survey-platform/survey-module/survey-checker/fraud-detection/

post-test part of the study after five weeks, as in the original study. Participants were told that they would be participating in several unrelated 'mini studies' in a random order to minimize hypothesis guessing. Both groups then completed the priming scrambled-sentence verbal fluency task. Immediately after, they answered the manipulation check, and then filled out the BCTI, along with two filler scales. To keep the procedure consistent with the original study, the BCTI and the filler scales were displayed to participants in a randomized order. Finally, the participants were debriefed and asked what they believed was the purpose of the study, as in the original research.

4.1.4. Experimental manipulation

The experimental manipulation consisted of the priming scrambled-sentence verbal fluency task (Gervais & Norenzayan, 2012). Following the original study procedure, the participants were told that they are taking part in a verbal fluency task. They were then presented with ten sets of five-word scrambled sentences (Appendix B). For each set, they were instructed to form a coherent sentence by dropping one of the five words and ordering the rest of the words accordingly. In the analytic condition, half of the scrambled word lists contained a target prime word that is related to analytic reasoning (*think, analyze, reason, ponder, rational*). The remaining five-word lists in the analytic condition and all ten in the control condition did not contain any analytic primes (e.g., *paper, hammer, shoes, holiday, sky*). Target prime words in the analytic condition were identical to Swami et al. (2014). However, filler words differed, due to the original materials not being retrievable. We therefore used the same stimuli as Gervais and Norenzayan (2012), as Swami et al. (2014) based their manipulation on these materials. More specifically, Swami et al. (2014) used the same target primes and only changed the additional words in the task.

4.1.5. Materials

4.1.5.1. Manipulation check. As in the original study, we used the one-item Moses Illusion task ("How many of each kind of animal did Moses take on the Ark?"; Erickson & Mattson, 1981) as a manipulation check, to test whether the priming task indeed induced analytical thinking. Scores were manually recoded so that the intuitive responses (e.g., "two") are coded as incorrect, and the analytical responses (e.g., "none") as correct. Responses that did not fit any of the two categories (e.g., a number other than two) were also coded as incorrect, as was the single "don't know" response in the dataset.

4.1.5.2. Belief in conspiracy theories. Belief in conspiracy theories was assessed with the Belief in conspiracy theories inventory (BCTI; Swami et al., 2011), consisting of 15 items tapping into belief in specific conspiracy theories (e.g., "The assassination of John F. Kennedy was not committed by the lone gunman, Lee Harvey Oswald, but was rather a detailed, organized conspiracy to kill the President"). The participants indicated the truthfulness of the items on a 9-point scale (1 = *Completely false*; 9 = *Completely true*). The BCTI showed high reliability in both Wave 1 ($\alpha = 0.92$) and 2 ($\alpha = 0.93$).

4.1.5.3. Distractor scales. We used two distractors scales—the short Big Five inventory (BFI-10; Rammstedt & John, 2007) and the Brief Rosenberg Self-Esteem Scale (B-RSES; Monteiro, de Coelho, Hanel, de Medeiros, & da Silva, 2022). Because it was not possible to obtain distractor scales from the original studies, these were chosen as they measure general, stable traits that are unlikely to be affected by the manipulation, and have null or negligible correlations with conspiracy belief scale(s) (Goreis & Voracek, 2019; Stasielowicz, 2022).

The BFI-10 consists of 10 items that measure the five major personality traits (e.g., "I see myself as someone who tends to be lazy"). Each dimension is represented by two items, one of which is reverse scored. The participants indicated their agreement with the items on a 5-point scale (1 = *Disagree strongly*; 5 = *Agree strongly*). The attention check question was included with the items of the BFI-10 ("This is an attention check question. Please select the 'disagree strongly'.").

The B-SRES is the short version of the Rosenberg Self-Esteem Scale, which consists of five items (e.g., "On the whole, I am satisfied with myself."). Participants answered using a 7-point scale (1 = *Strongly Disagree*; 7 = *Strongly Agree*).

4.1.5.4. Sociodemographics. Sociodemographic questions included gender, age, education, ethnicity and socioeconomic status.

4.2. Results

Data analyses were performed using R (R-4.2.2; R Core Team, 2022) . All reported tests were pre-registered in the Stage 1 Registered Report (https://osf.io/uvmhy).

To check if the scrambled sentence task induced analytical thinking, we tested the effect of the experimental condition on answers to the Moses Illusion task. Contrary to the original study, in which significantly more participants answered correctly in the analytic condition (i.e., 31%), compared to the control condition (i.e., 8%), $\chi2(1) = 5.75$, $p = .016$, we found no evidence of a difference in correct answers between the experimental (17) and the control group (18), $\chi^2(1) = 0$, $p = 1$.

Next, we ran a mixed 2 × 2 ANOVA with the testing session (pre-test vs. post-test) as the within-subject factor and the condition (control vs. analytic prime) as the between-subject factor. As opposed to the original study, a 2 × 2 ANOVA did not show a significant interaction, $F(1,300) = 1.18$, $p = .278$, $\eta^2 = 0.00$, see Fig. 1. There was also no main effect of either the testing session $F(1,300) = 0.43$, $p = .514$, $\eta^2 = 0.00$, nor of the experimental condition, $F(1,300) = 0.699$, $p = .404$, $\eta^2 = 0.00$. In the pre-test, there was no evidence of difference in belief in conspiracy theories between the analytic ($M = 3.30$; $SD = 1.54$) and control condition ($M = 3.39$; $SD = 1.53$), $t(300) = 0.52$, $p = .603$, $d = 0.06$, 95% CI [−0.16, 0.28]. Contrary to the original paper's findings, there was also no evidence of difference in belief in conspiracy theories in the post-test between the analytical prime ($M = 3.28$; $SD = 1.58$) and the control condition ($M = 3.48$; $SD = 1.62$), $t(300) = 1.08$, $p = .282$, $d = 0.12$, 95% CI [−0.09, 0.37]. Similarly, a Bayesian *t*-test (Cauchy distribution prior, $r = 1/\sqrt{2}$) showed moderate evidence for the null hypothesis over the alternative, $BF_{01} = 4.54$.

To test if belief in conspiracy theories for participants in the analytic condition reduced after post-test, we also conducted a paired *t*-test comparing pre-test ($M = 3.30$) and post-test ($M = 3.28$) scores for participants in the analytic condition which showed no significant effect, $t(146) = 0.32$, $p = .752$, $d_z = 0.026$, 95% CI [−0.14, 0.19]. A Bayesian *t*-test showed strong evidence in favor of the null hypothesis, $BF_{01} = 10.36$.

To test the robustness of these findings, we aimed to run the *t*-tests after excluding univariate outliers whose mean score on the BCTI was more than three times the Median absolute deviation (3*MAD) away from the median scores (Leys, Ley, Klein, Bernard, & Licata, 2013). We only excluded one participant based on these criteria in the pre-test for the control condition. There was still no significant difference between the conditions in the post-test after this exclusion, $t(299) = 0.91$, $p = .362$, $BF_{01} = 5.29$, $d = 0.11$, 95% CI [−0.12, 0.34].[4]

Overall, we found no evidence that the analytical prime group decreased in belief in conspiracy theories. On the contrary, we found

[4] In addition to the pre-registered robustness analyses, we also tested if the pattern of results remains the same after excluding participants who were close to guessing what the hypothesis of the study was (e.g., making a connection between belief in conspiracy theories and literacy, intelligence, verbal reasoning etc.). There was still no significant difference between the conditions in the post-test after excluding 16 such participants, $t(284) = 1.366$, $p = .189$, $BF_{01} = 3.37$, $d = 0.16$, 95% CI [−0.09, 0.42].

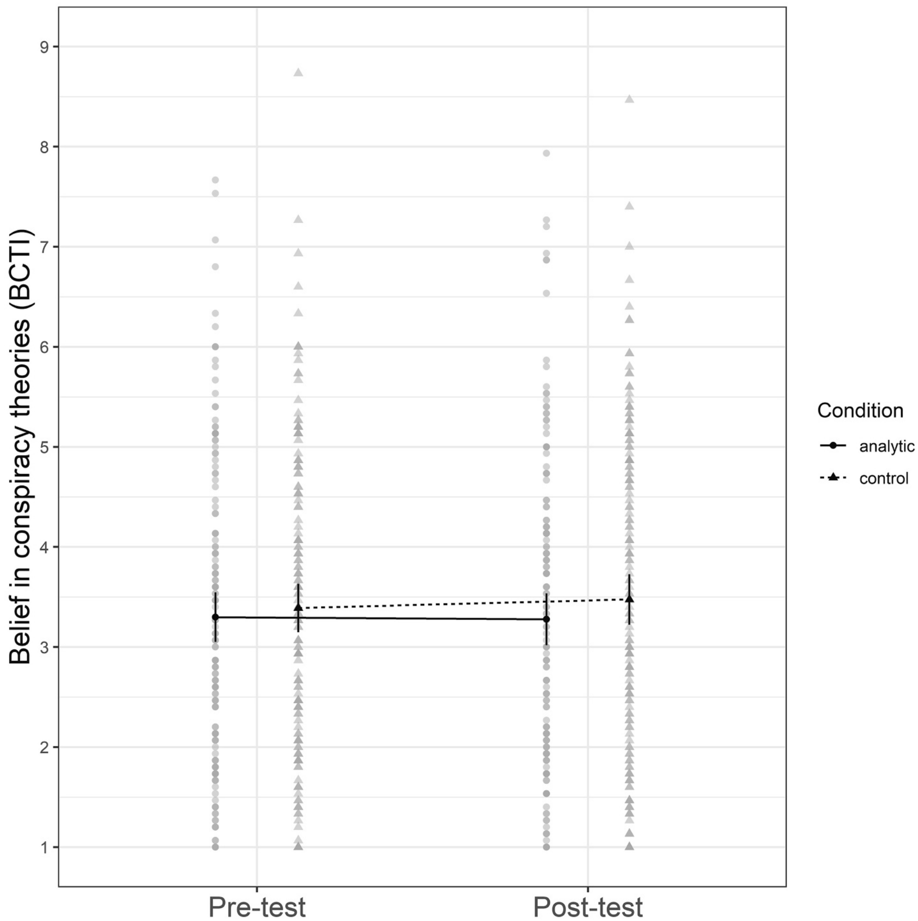

Fig. 1. BCTI scores in the pre- and post-test, per condition in Replication Study 1.
Note. Dots represent means, lines around means represent 95% confidence intervals.
BCTI = Belief in Conspiracy Theories Inventory.

evidence for the null hypothesis, i.e., that the scrambled sentence analytical priming manipulation did not influence belief in conspiracy theories.

5. Replication Study 2—Replication of Study 4 from Swami et al., 2014

5.1. Method

5.1.1. Participants

5.1.1.1. Sample planning. The target sample size was based on a priori power considerations. The original study included 140 participants and the smaller of the two main effect sizes detected was $d = 0.34$ (analytic vs. control group difference in belief in conspiracy theories about the 7/7 London bombings; see Materials). As in Replication Study 1, we relied on two approaches to determine the sample size, and opted for the one that returned a higher estimate. Assuming that H0 is true (i.e., that priming analytical thinking does not reduce belief in conspiracy theories), the sample size of the replication should be 2.5 times higher than the original study (Simonsohn, 2015). This returned sample size of $N = 350$ (2.5×140). Assuming that H1 is true, a power analysis for an independent *t*-test ($d = 0.34$, $\beta = 95\%$, $\alpha = 0.05$) returned the required sample size of $N = 452$. Therefore, we decided to follow the higher estimate and set the target sample size of $N = 452$. We oversampled to account for data exclusions (estimated at 10%, see the Stage 1 Registered Report: https://osf.io/uvmhy).

5.1.1.2. Data exclusions. We used the same criteria for data exclusions as in Replication Study 1, and therefore excluded participants with missing data and those who failed the attention check question. We also excluded likely bots and fraudulent responses (using the same criteria variables as in Replication Study 1), and similarly to Replication Study 1, did not detect any speeders.

5.1.1.3. Sample. In total, 507 UK Prolific participants[5] completed the study. After removing responses that did not pass quality checks ($n = 19$), 488 participants were left in the final sample. The sample was balanced in terms of gender with 49% females, 50% males, and 1% indicating a third option ($M_{age} = 41.40$, $SD_{age} = 14.42$). Most participants had a university bachelor's degree or higher (59.2%). Most participants identified as White (86.9%). As in Study 1, to ensure high data quality, only participants who did not exceed a maximum time (determined by Prolific), had an approval rate of 95% or higher on Prolific, passed the attention check, and were not flagged by Qualtrics as likely bots or fraudulent responses were used in the final sample.

5.1.2. Design

We employed the same design as the original study (Study 4; Swami et al., 2014), with a two-level between-subjects factor (Experimental manipulation: Analytic prime vs Control). Participants were randomly

[5] To ensure independent samples across replication studies, participants who took part in Replication Study 2 were prevented from participating in Replication Study 2.

assigned to either the Analytic or the Control prime condition.

5.1.3. Experimental manipulation and procedure

The survey was hosted on Qualtrics. As in Replication Study 1, the study was available to participants only from a desktop or laptop computer in order to increase experimental control. People attempting to participate from mobile devices (i.e., smartphones and tablets) were not able to access the survey due to a Qualtrics filter.

All participants first provided electronic informed consent and read task instructions in an easy-to-read font (i.e., black Arial, font size 12). Participants in the experimental group then completed the survey in a difficult-to-read font (gray Brush Script MT, font size 12), while the control group completed the survey in the same easy-to-read font. Following the Swami et al. (2014) design, and similarly to Replication Study 1, we included several filler scales to minimize the possibility of hypothesis guessing. The order of the conspiracy and the distractor scales was randomized, with the manipulation check shown after the scales. Finally, participants provided the same demographic data as in Replication Study 1. Upon survey completion, participants were debriefed and paid.

5.1.4. Materials

5.1.4.1. Generic Conspiracist Beliefs Scale (GCBS; Brotherton et al., 2013). This 15-item scale taps into the tendency to engage in conspiracist ideation (e.g., "A small, secret group of people is responsible for making all major world decisions, such as going to war."). Items were rated on a 5-point scale (1 = *Definitely not true*, 5 = *Definitely true*) and an overall score was computed as the mean of all items. The GCBS showed high reliability (α = 0.94).

5.1.4.2. Belief in 7/7 conspiracy theories. To measure beliefs about a specific (and perhaps less prominent) conspiracy theory, a 12-item measure of conspiracist beliefs about the July 7, 2005, bombings in London was used (Swami et al., 2011; sample item: "The fact that the UK government is withholding information about the 7/7 bombings is evidence of a cover-up"). Responses were provided on a 9-point scale (1 = *Completely false*, 9 = *Completely true*). The measure showed good reliability (α = 0.94).

5.1.4.3. Distractor scales. We used the same two distractors scales as in Replication Study 1—the BFI-10 (Rammstedt & John, 2007) and the B-SRES (Monteiro et al., 2022).

5.1.4.4. Manipulation check. As in Replication Study 1, we included the one-item Moses Illusion task (Erickson & Mattson, 1981) as a manipulation check. Again, like in Replication Study 1, only one participant gave a "don't know" response.

5.2. Results

First, to test if the disfluent font manipulation induced analytical thinking, we tested the effect of experimental condition on answers to the Moses Illusion task. Contrary to the original study 35% of participants in the experimental condition gave a correct answer, compared with 12% in the control condition, $\chi^2(1) = 10.05$, $p = .002$), and same as in Replication Study 1, we found no evidence of a difference in correct answers between the experimental (34) and the control group (41), $\chi^2(1) = 0.27$, $p = .602$.

Next, we conducted two independent sample *t*-tests to compare scores on the two conspiracy belief scales (i.e., GCBS & Belief in 7/7 conspiracy theories) between the two groups (see Fig. 2). Contrary to the original study, we found no evidence of difference between the analytic ($M = 2.66$; $SD = 0.90$) and control condition ($M = 2.61$; $SD = 0.89$) on the GCBS, $t(486) = 0.63$, $p = .529$, $d = 0.06$, 95% CI [−0.12, 0.25].

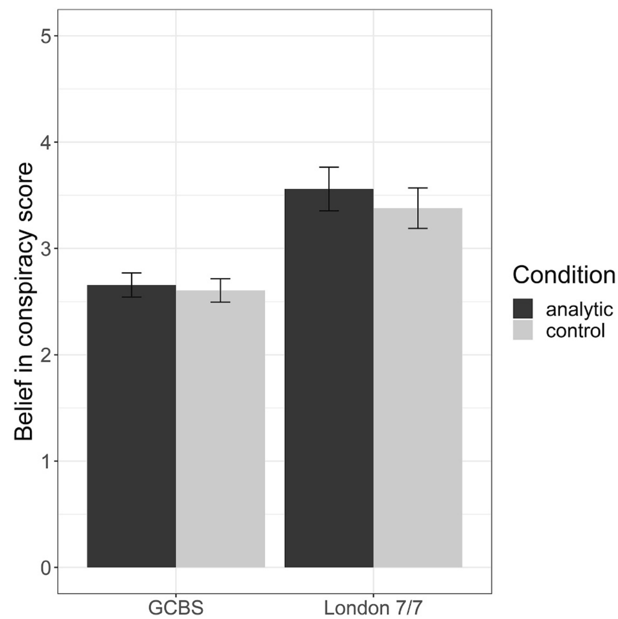

Fig. 2. Belief in conspiracy theories (GCBS and London 7/7) per condition in Replication Study 2.

Similarly, we found no evidence of difference between analytic ($M = 3.56$; $SD = 1.62$) and control condition ($M = 3.38$; $SD = 1.54$) on the Belief in 7/7 conspiracy theories, $t(486) = 1.27$, $p = .206$, $d = 0.11$, 95% CI [−0.07, 0.30]. Bayesian *t*-tests (Cauchy distribution prior, $r = 1/\sqrt{2}$) showed moderate evidence for the null hypothesis over the alternative for both the GCBS, $BF_{01} = 8.20$, and Belief in 7/7 conspiracy theories, $BF_{01} = 4.57$.

Although we preregistered an outlier exclusion as a robustness check, there were no outliers based on the 3*MAD from median criterion for either of the dependent variables. Therefore, we did not re-run the main analyses.

Overall, similarly to Replication Study 1, the original study failed to replicate. We found no evidence that the analytical thinking primes lowered belief in conspiracy theories. On the contrary, we found moderate-to-strong evidence for the null hypothesis, i.e., that the disfluent font manipulation did not influence belief in conspiracy theories.

6. General discussion

This Registered Report aimed to replicate two experimental studies from Swami et al. (2014) which found that priming analytical thinking reduces belief in conspiracy theories. In both highly powered close replications, we found no evidence for the hypothesis that priming analytical thinking through either the scrambled sentence task (original Study 2, Replication Study 1) or the use of difficult-to-read fonts (original Study 4, Replication Study 2) reduced such beliefs. On the contrary, Bayesian analyses showed moderate-to-strong evidence for the null hypotheses in both studies. Furthermore, we found no evidence that analytic primes increased correct responses to the Moses Illusion Task (suggested to measure analytic thinking; Song & Schwarz, 2008). Our studies therefore contribute to the growing evidence base suggesting that difficult-to-read fonts (Janouskova et al., 2022; Sirota et al., 2021; Yilmaz & Saribay, 2016) and scrambled sentence priming (Deppe et al., 2015) are not effective at inducing analytic thinking, and subsequently at eliciting hypothesized downstream effects of analytic thinking, such as reduced religious belief (Camerer et al., 2018; Sanchez et al., 2017; Villanueva, Chen, & Huang, 2022) or political conservatism (Deppe et al., 2015).

Based on our results and the accumulating validity concerns of analytical thinking priming manipulations, we conclude that the Swami et al. (2014) studies do not provide strong support for the claim that analytic thinking reduces conspiracy beliefs. This is especially important in light of the scarcity of research on reducing belief in conspiracy theories (Sassenberg et al., 2023; van der Linden, Thompson, & Roozenbeek, 2023), with most of the interventions proving ineffective at reducing conspiracy beliefs (O'Mahony, Brassil, Murphy, & Linehan, 2023). In fact, the Swami et al. (2014) experimental findings have been highlighted as one of the most effective strategies for conspiracy belief reduction in a recent evidence synthesis, with independent replication of these effects explicitly called for (O'Mahony et al., 2023).

This implies that the current literature lacks evidence for the causal relationship between an analytic mindset and belief in conspiracies. Given the high consequentiality of belief in conspiracies and the urgent need for viable reduction strategies, our results call for developing and utilizing valid procedures of inducing analytic thinking in order to more reliably document its potential causal impact on belief in conspiracies, and therefore gauge its usefulness in reducing them.

From the standpoint of intervention design and content-specificity, the causality of this relationship has important implications. While some previous studies reduced conspiracy beliefs by using content-specific rational or scientific arguments in debunking attempts *after* exposure to conspiratorial content (e.g., by providing rational counterarguments as in Orosz et al., 2016 or scientific counterarguments as in Georgiou, Delfabbro, & Balzan, 2023), the Swami et al. (2014) studies are the only ones that found a direct effect of eliciting a more content-independent analytical mindset on subsequent belief in conspiracies. Therefore, the debunking findings imply tailoring each intervention to the specific arguments used in each conspiracy theory is required, while crafting a more general analytical mindset intervention aimed at reducing belief in conspiracies would be potentially applicable across different conspiracy theories and other epistemically unwarranted beliefs.

On that note, Isler and Yilmaz (2022) identified debiasing training as the most effective among ten tested procedures for inducing analytic thinking (as measured by the Cognitive Performance Test, a five-item extension of the original Cognitive Reflection Test; Frederick, 2005; Sirota & Juanchich, 2018). This procedure aims to increase awareness of three common cognitive biases (i.e., the semantic illusion, the base rate fallacy, and the availability bias) by asking riddle-like questions demonstrating each of them and providing feedback on the correct answers, accompanied by an explanation of each of the biases. This technique was shown to affect contextualized political attitudes (Yilmaz & Saribay, 2017) and reduce superstitious beliefs (Tosyali & Aktas, 2021), but its effectiveness for reducing conspiracy beliefs has yet to be investigated.

In addition, albeit this does not stem directly from our results given that the analytic thinking manipulations in our replication studies likely did not activate an analytic mindset as intended, it is important to consider the possibility that the robust negative correlation between analytic thinking and belief in conspiracies (Binnendyk & Pennycook, 2022; Yelbuz et al., 2022) does not straightforwardly translate into a causal effect of analytic thinking on conspiracy beliefs. On that note, Bago, Rand, and Pennycook (2022) found that experimentally increasing reliance on intuition by increasing time pressure and cognitive load (compared to an unrestricted response allowing for more deliberation) increases belief in conspiracies only for less known conspiracy theories and for people on the extreme ends of the conspiracy mentality spectrum. Furthermore, Stall and Petrocelli (2023) found that providing people with training to avoid the conjunction fallacy (by asking riddle-like questions and providing correct responses and their explanations; similar to the debiasing training from Isler & Yilmaz, 2022) reduced belief in a novel conspiracy theory, but only when participants were also tasked with listing arguments against said conspiracy theory. These findings provide preliminary support for the notion that prior beliefs and motivation to think analytically about the conspiratorial material presented in the studies likely play a moderating role in the potential causal impact of analytic thinking on belief in conspiracies (Binnendyk & Pennycook, 2022). It would be useful to further build upon these findings to fully gauge the relationship between analytic thinking and belief in conspiracy theories.

6.1. Potential limitations and considerations

Several considerations regarding our studies should be noted. First, in order to keep the procedure as identical as possible to the original study, we utilized a one-item, culturally specific measure (i.e., the Moses Illusion Task; Erickson & Mattson, 1981) as the analytic thinking manipulation check. Even though this measure was culturally congruent with our sample, future studies should use a more extensive and validated measure to gauge the validity of analytic thinking manipulations (e.g., the CRT; Frederick, 2005).

Second, while both original studies were conducted on site—Study 2 from Swami et al. (2014) on a computer in a private cubicle, and Study 4 as a paper-and-pencil survey—our replications were conducted online, via a crowdsourcing platform. While this could raise concerns about a lower level of attention to the tasks and stimuli being the reason for our null findings, there is little to no evidence that data quality for cognitive tasks is lower in online studies, when appropriate measures (e.g., attention checks) are taken to ensure data quality (e.g., Bartneck, Duenser, Moltchanova, & Zawieska, 2015; Casler, Bickel, & Hackett, 2013; Moseley et al., 2021). We therefore posit it is highly unlikely that the null findings are attributable to this difference in testing modality.

Of note is also that the study materials used in Replication Study 1 were not entirely identical to the original study (Study 2 from Swami et al., 2014) due to the original materials not being retrievable from the original first author. More specifically, while analytic word primes were identical, other words in the verbal fluency task were not. However, given that we relied on the materials and procedure that Swami et al. (2014) based their own on, and that the target prime words were identical, these minor changes are unlikely to have had any meaningful impact on the results and the replication outcome. We argue that our results, alongside previous failed replications of priming on analytical thinking, instead highlight the need to move beyond findings based on these procedures and search for other avenues for inducing analytical thinking and examining its downstream consequences.

7. Conclusion

In the present Registered Report, we conducted two high-powered direct replications of the findings that analytic thinking primes reduce belief in conspiracies (Swami et al., 2014). We found no evidence for the claims that priming analytic thinking through a scrambled sentence task, nor a disfluent-font paradigm, reduces belief in conspiracies. We also failed to find evidence for the validity of analytic thinking priming manipulations. Our results call into question the reliance on priming procedures to induce analytic thinking, and to reduce belief in conspiracies. Therefore, further research on the causal relationship between analytic thinking and belief in conspiracies is needed using more valid methods to increase analytic thinking.

Declaration of Competing Interest

The authors declare no competing interests.

Data availability

All data is available on the OSF project page: https://osf.io/keahr/

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jesp.2023.104549.

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