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Clarifying the relationship between randomness dismissal and conspiracist ideation: A preregistered replication and meta-analysis☆

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ABSTRACT

A large body of research has found mixed evidence that people who are quick to dismiss randomness as a potential cause for an event are also more likely to believe conspiracy theories. To clarify the relationship between randomness dismissal and conspiracist ideation, we conducted a high-powered preregistered replication of an influential study in the United States ($n = 521$) and Switzerland ($n = 293$), and a meta-analysis of the literature ($N = 55$ effect sizes). Both our study ($0.03 < r$'s < 0.15) and meta-analysis ($r = 0.16$) found small, but positive and robust relationships between randomness dismissal and conspiracist ideation. Our replication investigated differences in statistical power, culture, and education as potential explanations for the conflicting findings in the literature. None of these factors could fully account for the mixed findings, although culture had an unexpected moderating role. Our study suggests that the relationship between randomness dismissal and conspiracist ideation is small and contextually sensitive.

1. Introduction

Amid the first wave of the COVID-19 pandemic in spring 2020, the United Kingdom recorded over 70 cases of burned down mobile towers (Reichert, 2020). It soon turned out that people intentionally vandalized mobile towers, motivated by the belief that the COVID-19 pandemic was a cover up for the harmful effects of 5G technology. Supporters of the '5G hoax' conspiracy theory referred to an (inaccurate) map that allegedly showed an overlap between COVID-19 cases and the adoption of 5G technology (Waterson & Hern, 2020). In reality, the spread of COVID-19 was unrelated to 5G technology (World Health Organization, 2020) and the "pattern" that some people saw was entirely random. The current paper examines whether people's perception of randomness is related to conspiracy theory beliefs.

Conspiracy theories explain events as the result of a malicious and secret plot carried out by a group of people (Douglas, Sutton, & Cichocka, 2017). Although conspiracy theories are often equated with wrong and irrational narratives, some are perfectly plausible and, in fact, eventually turn out to be true (e.g., role of Purdue Pharma in the U. S. opioid epidemic; see Grimes, 2016, 2021 and Dentith, 2018 on the veracity of conspiracy theories). True or false, all conspiracy theories involve "connecting the dots" and dismissing randomness as the cause for an event. They are often guided by the idea that "nothing happens by accident" (Dieguez, Wagner-Egger, & Gauvrit, 2015, p. 1762) and some involve a tendency to overexplain events and underappreciate the possibility of chance and ordinary causes (Brotherton & French, 2014; Douglas, Sutton, Callan, Dawtry, & Harvey, 2016; Leman & Cinnirella, 2007; van Prooijen & van Dijk, 2014).

Several studies have found that people who are predisposed or motivated to dismiss randomness as a possible cause for an event are more likely to believe in conspiracy theories (Dieguez et al., 2015; van Prooijen, Douglas, & De Inocencio, 2018). Though this general idea is highly influential in the literature on conspiracy theories (e.g., Barkun, 2003; Brotherton, 2019; Sunstein & Vermeule, 2009; Taleb, 2005; van Prooijen, Klein, Milosevic Dordevic, Knight, & Butter, 2020; Whitson & Galinsky, 2008), the empirical evidence is actually mixed with several null effects (e.g., Adam-Troian, Caroti, Arciszewski, & Ståhl, 2019; Dieguez et al., 2015; Ståhl & Van Prooijen, 2018; van der Wal, Sutton, Lange, & Braga, 2018; van Prooijen et al., 2018; Wagner-Egger, Delouvée, Gauvrit, & Dieguez, 2018; see also Melley, 2020). To better understand the relationship between these two variables, the current paper examines the robustness and generality of this idea in a

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preregistered replication and meta-analysis.

Several papers have found evidence that randomness dismissal[1] and conspiracist ideation are positively related (e.g., Adam-Troian et al., 2019; Caroti, Adam-Troian, & Arciszewski, 2021; Ståhl & Van Prooijen, 2018; van der Wal et al., 2018; van Prooijen et al., 2018). For instance, one paper measured people's tendency to see patterns in random coin tosses and their belief in real and fictitious conspiracy theories (van Prooijen et al., 2018). The authors found robust relationships between (illusory) pattern perception and belief in conspiracy theories.[2] Later studies obtained similar results using various measures of randomness dismissal (e.g., visual stimuli; van der Wal et al., 2018 Study 1, van Prooijen et al., 2018 Study 3) and conspiracist ideation (e.g., Adam-Troian et al., 2019).

In contrast, other studies have found mixed evidence for a relationship between randomness dismissal and conspiracist ideation (e.g., Dieguez et al., 2015; Wagner-Egger et al., 2018; van der Wal et al., 2018 Study 1). One line of research had people evaluate the randomness of binary letter-strings that varied in their normative, "true", randomness which constitutes the probability that a process is random, given the output of the process (see Dieguez et al., 2015). Afterward, people completed four different measures of conspiracist ideation. Across three studies, there was no association between peoples' tendency to dismiss the possibility that the letter strings were produced by a random process and their conspiracist ideation. Another paper conducted a similar study and found a moderate relationship between randomness dismissal and conspiracist ideation (Wagner-Egger et al., 2018 Study 3). However, this relationship became non-significant when the authors statistically adjusted for other relevant variables such as cognitive reflection. In other studies, the relationship between randomness dismissal and conspiracist ideation was very robust, even when statistically adjusting for cognitive reflection and analytical thinking (Betsch, Aßmann, & Glöckner, 2020, Ståhl & Van Prooijen, 2018). Together, these results suggest that randomness dismissal may not be related to conspiracist ideation or may be contextually sensitive and hinge on hidden moderators (see Van Bavel, Mende-Siedlecki, Brady, & Reinero, 2016).

In the current research, we examined at least three explanations for the inconsistent results.[3] First, we examined the role of statistical power due to small effect sizes. Most of the studies that did not find a relationship between randomness dismissal and conspiracist ideation had small sample sizes. In contrast, studies that found a positive relationship between the two variables tended to include sample sizes that were sufficiently large to detect small to moderate correlations (see Table 1). For instance, we conducted a post hoc power analysis using Dieguez et al.'s (2015) analysis parameters (correlation analysis; $\alpha = 0.05$; one-tailed testing) and found that Study 1 ($n = 107$) had only 47% power, Study 2 ($n = 123$) had 51% power, and Study 3 ($n = 217$) had 72% power to detect a small to moderate correlation between randomness dismissal and conspiracist ideation ($r = 0.15$). In contrast, van Prooijen et al.'s (2018) studies–which found robust positive correlations between randomness dismissal and conspiracist ideation–all had over 70% statistical power to detect this effect size (Study 1: $n = 264$, statistical power = 79%; Study 2: $n = 223$, statistical power = 73%, Study 3: $n = 214$, statistical power = 71%). As such, the null effects might reflect a lack of statistical power. Indeed, a recent analysis of nearly 200 replication studies suggests that statistical power is one of the most powerful predictors of replication success (Reinero, Dikker, & Van Bavel, 2020).

Second, results might diverge because of differences in the social context in which the studies were completed, which can account for replication differences (see Van Bavel et al., 2016). Most studies that obtained null effects recruited samples from the French-speaking part of Switzerland. On the other hand, studies that found significant relationships between randomness dismissal and conspiracist ideation were primarily conducted in the U.S. As such, the relationship between conspiracist ideation and randomness dismissal might be sensitive to cultural differences (see Schweinsberg et al., 2016 for a similar example of cultural effects on replication success). This dovetails with findings of cultural differences related to pattern perception and paranormal beliefs (e.g., horoscope interpretations; Wang, Whitson, & Menon, 2012), which are closely related to conspiracy theory beliefs (Imhoff & Lamberty, 2018; Suthaharan et al., 2021). Moreover, vertical individualism is associated with the belief in COVID-19 conspiracy theories, while other forms of cultural orientation, including horizontal individualism, are not (Biddlestone, Cichocka, Žeželj, & Bilewicz, 2020). Vertical individualism describes the construal of the self as autonomous with an acceptance of inequality, whereas horizontal individualism describes the construal of the self as autonomous with an opposition to inequality (Triandis, 1995). The U.S. is a vertical individualistic country and Switzerland is a horizontal individualistic country (Györkös et al., 2013; Singelis, Triandis, Bhawuk, & Gelfand, 1995; Sivadas, Bruvold, & Nelson, 2008), which could account for different findings. Further, interpersonal and institutional distrust can be critical for the formation processes of conspiracy theory beliefs (Goertzel, 1994; Miller, Saunders, & Farhart, 2016; Meuer & Imhoff, 2021; Saunders, 2017). Americans score much lower in interpersonal trust and confidence in their government than Swiss people do (Haerpfer et al., 2022). Together, these or other cultural differences may account for differences in the relationship between randomness dismissal and conspiracist ideation.

Third, divergent study results might stem from differences in sample characteristics such as education level. Most studies that obtained null effects recruited college students, while most studies that found a relationship between randomness dismissal and conspiracist ideation used more diverse samples (see Table 1). This difference could be important because education negatively correlates with conspiracy theory beliefs (van Prooijen, 2017) and effect sizes in conspiracy theory research tend to be lower amongst college student samples (Biddlestone, Green, Cichocka, Douglas, & Sutton, 2022). In addition, education moderates the relationship between randomness dismissal and paranoid ideation. Specifically, researchers found a significant relationship between the two variables only amongst people with lower education (e.g., Bressan, 2002). The relationship between randomness dismissal and conspiracist ideation may only occur for people with relatively low educational attainment.

To reconcile these different findings and better understand the

[1] Studies differ in how randomness dismissal —and randomness perception more generally—is conceptualized. We therefore use the broad term "randomness dismissal" for simplicity. For a detailed discussion of randomness perception and its different facets, see William and Griffiths (2013) and our General Discussion

[2] Note that van Prooijen et al. (2018) Studies 4 & 5 experimentally tested whether exposure to conspiracy theories (vs. other types of worldviews) increases illusory pattern perception and are therefore not included in our overview.

[3] Note that some researchers (e.g., Dieguez et al., 2015; Wagner-Egger et al., 2018) presented letter strings that covered a broad range of possible normative randomness. Other researchers (e.g., van Prooijen et al., 2018; Ståhl & van Prooijen, 2018) used letter strings that were created so that the generated distribution aligned with the expected distribution of a random process. Although, this constitutes a systematic difference between these studies, we find it to be an unlikely *main* source of the conflicting findings. van Prooijen et al. (2018) conceptually replicated the findings using various measurements of randomness dismissal. On the other hand, Wagner-Egger et al. (2018, Study 1) found *no* relationship between the two variables when they measured randomness dismissal as pattern perception in meaningless/−full pictures. The studies also varied in the presentation order of the measurements. We find differences in presentation order also an unlikely explanation for different study findings. In fact, van Prooijen et al. (2018) found significant, positive relationships between randomness dismissal and conspiracist ideation regardless of whether they measured conspiracist ideation or randomness dismissal first.

Table 1
Overview of key studies on the relationship between randomness dismissal and conspiracist ideation.

Paper	Study	Place of Recruitment	Education Level	Study Results [Correlation randomness dismissal & conspiracist ideation]	Sample Size	Post hoc Power r = 0.15 α = 0.05 one-tailed	Post hoc Power $r = 0.15$ α = 0.05 two-tailed
Dieguez et al. (2015)	Study 1	French-speaking Swiss	College students	ns	107	47%	34%
	Study 2	French-speaking Swiss	College students	ns	123	51%	38%
	Study 3	Not reported[a]	Diverse	ns	217	72%	60%
Dagnall, Denovan, Drinkwater, Parker, and Clough (2017)		U.K.	Diverse	n.s.	246	77%	66%
Wagner-Egger et al. (2018)	Study 1	French-speaking Swiss	College students	ns	157	60%	47%
	Study 3	French-speaking Swiss & France	College students & Professionals	$p < .01$ (*ns when adjusted for covariates*)	733	99%	98%
van Prooijen et al. (2018)	Study 1	U.S.	Diverse	p's < 0.001	264	79%	69%
	Study 2	U.S.	Diverse	p's $\leqq 0.002$	223	73%	62%
	Study 3	U.S.	Diverse	p's $\leqq 0.002$	214	71%	60%
Ståhl and Van Prooijen (2018)	Study 1	U.S.	Diverse	p's < 0.001	343	88%	80%
van der Wal et al. (2018)	Study 1	U.S.	Diverse	$p < .001$	195	68%	56%
	Study 2	U.S.	Diverse	$p < .010$ $p < .001$	216	72%	60%
	Study 3	U.S.	Diverse	$p < .001$	214	71%	60%
	Study 4	U.S.	Diverse	$p < .001$	211	71%	59%
Adam-Troian et al. (2019)		France	Teachers	p's < 0.001	730	99%	98%
Betsch et al. (2020)		Germany	College students	p's < 0.05	599	98%	96%
Caroti et al. (2021)		France	Teachers	$p < .05$	130	53%	40%
Gligorić et al. (2021)		Primarily Europe	Diverse	p's < 0.05 conspiracy mentality & specific conspiracy theories beliefs; n.s. COVID-19 conspiracy theories	354	89%	81%

Note. Most studies administered several measures of conspiracist ideation.
[a] Dieguez et al.'s Study 3 reported that a sample of French-speaking participants was recruited through email and online social networks. Participants' place of residence was not reported

psychology of conspiracy theory beliefs, we conducted a high-powered, cross-cultural replication study of Dieguez et al.'s (2015) Study 1. We recruited samples from the French-speaking parts of Switzerland and the U.S. that were sufficiently large to provide precise effect size estimates and to detect differences in culture and education. We chose Study 1 because it intersects most with the other papers and would thus render our findings maximally informative. In addition, we meta-analyzed the literature on randomness dismissal and conspiracist ideation to refine effect size estimates and to assess potential moderators. Together, we employed best practices for evaluating theoretical claims (i.e., high-powered preregistered replication, tests of generalization, and meta-analysis of the literature) to clarify the relationship between randomness dismissal and conspiracist ideation.

2. Study overview

The study was approved by the New York University Ethical Review Board. We conducted a high-powered direct replication with several study extensions. We extended the original study (Dieguez et al., 2015; Study 1) in four meaningful ways. First, we recruited high-powered samples from both the French-speaking parts of Switzerland and the U.S. to explore whether cultural differences may account for discrepant findings in the literature. Second, we explored whether education level moderated the relationship between randomness dismissal and conspiracist ideation. Third, we statistically isolated conspiracist ideation from paranoid ideation since the two constructs are closely related (Imhoff & Lamberty, 2018; Suthaharan et al., 2021). Fourth, we added a measurement of belief in COVID-19 conspiracy theories given the urgency of identifying psychological processes that are associated with these dangerous beliefs (Biddlestone, Green, & Douglas, 2020; Sternisko, Cichocka, Cislak, & Van Bavel, 2021).[4]

First, we examined whether differences in statistical power may account for conflicting findings in the literature on randomness dismissal and conspiracist ideation. We calculated correlations between randomness dismissal and the four original conspiracist ideation measures using the full sample. The statistical significance of the correlations was tested in accordance with Dieguez et al.'s (2015) approach ($p < .05$, one-tailed testing). Next, we investigated cultural differences as a potential explanation for the conflicting findings. We statistically predicted conspiracist ideation from randomness dismissal, culture (between-subject factor: Switzerland, U.S.), and the interaction between the two variables. We then probed differences in sample education as a possible explanation. We statistically predicted conspiracist ideation from randomness dismissal, education, and the interaction between the two variables. We examined how any of these conclusions changed when we isolated the relationships from paranoid thinking. Next, we re-ran all analyses, predicting belief in COVID-19 conspiracy theories. We then conducted a meta-analysis to generate more precise effect size estimates and interpret our replication results (see Greenwald, 1975 and Waller,

[4] We initially also planned to include analytical thinking as an additional covariate. We dropped this measure due to logistical constraints. Past work found that the relationship between randomness dismissal and conspiracist ideation is robust against adjusting for various cognitive skills, including analytical thinking (Betsch et al., 2021; cf. Wagner-Egger et al., 2018). We thus do not consider analytical thinking a decisive covariate.

2004 on null-result replications). Our analysis included all published and unpublished research findings on randomness dismissal and conspiracist ideation that we could locate through thorough database scanning and public outreach.

Preregistration, data, analysis code, and study materials are publicly available on OSF (https://osf.io/aj64k/) . (https://osf.io/aj64k/)

3. Replication study

3.1. Methods

3.1.1. Participants

We conducted an a priori power analysis to determine a sample size that provides sufficient power to probe our three potential explanations for the inconsistent findings in the literature: low statistical power, culture, education. We used G*Power 3.1 to determine a sample size that provides high power to find potential interaction effects between randomness dismissal and culture/education. In G*Power 3.1., we specified the model as "Linear multiple regression: Fixed model, R^2 deviation from zero", error probability = .01, statistical power = 0.90, and number of tested predictors as one. We chose a small $f^2 = 0.015$ for our interaction effect. Our decision of this effect size was informed by recent simulation studies on statistical power for interaction effects (Baranger, 2019) which recommends specifying the anticipated interaction effect size as at least half the size of the expected (or previously observed) main effect. The majority of observed zero-order correlations between randomness dismissal and conspiracist ideation fell within the range of 0.1 to 0.4 (with a few exceptions). Considering what constitutes a meaningful effect, we set the anticipated main effect of randomness dismissal to $r = 0.24$. Consequently, we powered an interaction effect of $r = 0.12$ which corresponds to $f^2 = 0.015$.

The analysis revealed that a total of 996 participants were necessary to have 90% power to detect such an interaction effect. Anticipating data attrition, we planned to collect a total sample of 1200 participants, 600 participants in each country.[5] Note that without any data attrition ($N = 1200$) the study would have 90% power to detect an effect size of $f^2 = 0.012$. We initially planned to recruit participants via the survey platform 'PollFish'. However, the data obtained by PollFish was of unacceptably low quality,[6] so we were forced to use the service of 'Lucid Marketplace' to recruit our sample. Lucid slightly oversampled ($N_{Switzerland} = 634$, $N_{US} = 643$, $N_{Total} = 1277$). Unfortunately, a significant number of participants entered the survey but were not qualified to participate. We excluded 286 participants who entered the Swiss survey but were not from the French-speaking part of Switzerland and 41 participants who entered the U.S. survey but were not from the U.S.[7] Furthermore, we deleted the data from 49 participants who did not give us permission to analyze and publish their data ($N_{Switzerland} = 21$, $N_{US} = 28$).

As preregistered, we excluded participants who failed the attention check at the end of the survey ($N = 87$) from our main analyses. With a final sample size of 814 ($N_{Switzerland} = 293$, $N_{US} = 521$), the analysis had 95% power to detect a correlation of $r = 0.11$ (assuming error probability = .05, one-tailed testing in accordance with Dieguez et al., 2015) and an interaction effect of $f^2 = 0.022$ (power = 95%, error probability = .01, two-tailed in accordance with our more stringent criteria, one predictor). When we apply the conventional parameters of error probability = .05 and two-tailed testing, the analysis had 80% power to detect a correlation of $r = 0.097$ and an interaction effect of $f^2 = 0.009$. We examined whether the dataset included statistical outliers defined as scoring + − 3 standard deviations beyond the mean for any of the dependent or independent variables. We identified 20 statistical outliers, all of which were observed for the randomness dismissal measure. We indicate if results diverged when we excluded these outliers from our analyses throughout the manuscript, otherwise all conclusions were robust to the inclusion of these participants. Results based on the entire sample (including participants who failed the attention check) are reported in the Supplement. Demographic information of our final sample is reported in Table 2.

Table 2
Sample demographics.

	Unites States (N = 521)		**Switzerland** (N = 293)		**Total** (N = 814)	
	n	*percent*	*n*	*percent*	*n*	*percent*
Gender						
Male	199	38.2%	167	57%	366	45.0%
Female	318	61.1%	126	43%	444	54.5%
Other/Choose not to answer	4	0.8%	0	0%	4	0.4%%
Education						
Less than High School	6	1.2%	22	7.5%	28	3.4%
Some High School, ~ 2 years career-based training	13	2.5%	73	24.9%	86	10.6%
High School	177	34%	54	18.4%	231	28.4%
Associates Degree, advanced career-based training	115	22.1%	34	11.6%	149	18.3%
Bachelor's degree	150	28.8%	50	17.1%	200	24.6%
Master's degree	49	9.4%	50	17.1%	99	12.2%
PhD / Professional Degree	10	1.9%	10	3.4%	20	2.5%
Missing Value	1		0		1	
Age	M = 48.22	SD = 17.43	M = 40.28	SD = 14.11	M = 45.35	SD = 16.74

Note. Participants who failed the attention check were excluded.

3.1.2. Measures

We used the original material of Dieguez et al.'s (2015) Study 1 and administered two additional scales to measure paranoid ideation and belief in conspiracy theories related to the COVID-19 pandemic. French and English versions for all administered scales were validated (Barreto Carvalho et al., 2017; Bortolon, Capdevielle, Dubreucq, & Raffard, 2020; Van Bavel et al., 2022; Wagner-Egger et al., 2018).[8] Detailed descriptions of stimuli and scales are available on our OSF page.

[5] Due to the increased mobility during the pandemic and the small population size in the French-speaking part of Switzerland, we included participants who both currently reside in the French-speaking part of Switzerland or have been residing there in the past. Length of residency did not moderate the results.

[6] In the US sample, only 68% of participants (or potentially bots) passed the simple attention check. Of those participants (or potentially bots), 10% wrote nonsensical comments at the end of the survey and 50% were flagged as speeders. In the Swiss sample, only 63% of (or potentially bots) passed the simple attention check. Of those participants (or potentially bots), 35% were flagged as speeders.

[7] We cannot verify how and why some participants were able to bypass recruitment conditions and enter the survey. One possibility is that they used a VPN client which bypassed Lucid's screening method. Another possibility is that participants were inattentive and falsely reported to reside outside the U.S. and Switzerland. Either way, exclusion of these participants is warranted and in line with our preregistration.

[8] For two of the scales (randomness dismissal, belief in classic conspiracy theories), we disagreed with the previous translations. Using forward-backward translation method, we created revised versions of the scales. We validated our translations with a sample of 46 Prolific participants that was stratified over education level. Both scales showed excellent internal consistency, α's > 0.80 (data and analysis results are available on OSF https://osf.io/aj64k/).

3.1.2.1. Randomness dismissal. Participants saw forty random 12-character strings of X's and O's taken from the original experiment. For each letter string, participants evaluated its randomness on a 6-point scale from 1 = *certainly random* to 6 = *certainly not random*. Following Dieguez et al.'s (2015) methods, we later performed a linear transformation. The new scale constituted a 6-point scale proceeding in increments of 0.2 from 0 = *certainly random* to 1 = *certainly not random*. Ratings of each letter string were averaged to a randomness dismissal score ($\alpha = 0.94$, $M = 0.46$, $SD = 0.15$).

3.2. Conspiracist ideation

Participants' conspiracist ideation was assessed using Dieguez et al.'s (2015) original four measurements and our exploratory measurement of COVID-19 conspiracy theory beliefs.

3.2.1. Measurement (a): Truth evaluation of classic conspiracy theories (Wagner-Egger & Bangerter, 2007)

Participants read four classic conspiracy theories (e.g., the Apollo 11 moon landing was faked). For each conspiracy theory, participants evaluated its likelihood to be true on a 7-point scale from −3 = *not at all* to 3 = *absolutely*. We performed a linear transformation to the scale so that *not at all* was represented as 1 and *absolutely* was represented as 7. Responses to all items were averaged to a classic conspiracy theory belief score ($\alpha = 0.78$, $M = 3.60$, $SD = 1.48$).

3.2.2. Measurement (b): Generic conspiracist beliefs scale (Brotherton, French, & Pickering, 2013)

Participants read fifteen statements about conspiracy theories in general (e.g., *The government permits or perpetrates acts of terrorism on its own soil, disguising its involvement*). For each statement, participants evaluated its likelihood to be true on a scale from 1 = *definitely not true* to 5 = *definitely true*. Responses to all items were averaged to a generic conspiracist beliefs score ($\alpha = 0.93$, $M = 2.88$, $SD = 0.87$).

3.2.3. Measurement (c): Fictitious workplace scenario (Whitson & Galinsky, 2008)

Participants imagined an ambiguous workplace scenario in which their promotion was unexpectedly denied. They then rated their agreement with three different interpretations of the scenario on a 7-point scale from −3 = *not at all* to 3 = *perfectly sure*. One of the three interpretations suggested a conspiracy *(To what extent do you think your co-worker may be connected to you not getting the promotion?*), while the other two interpretations proposed bad luck and personal behavior as the cause of the denied promotion, respectively. We performed a linear transformation to the scale so that *not at all* was represented as 1 and *perfectly sure* was represented as 7. Agreements with the two non-conspiratorial interpretations were then reverse-coded. The Cronbach alpha of the scale was unacceptably low in the total sample and in the sub-samples (α's < 0.33) and the correlations between the three items were weak to moderate ($0.008 < |r\text{'s}| < 0.27$). The correlations between the item proposing a conspiratorial explanation correlated and the items proposing a non-conspiratorial interpretation were particularly low ($0.008 < |r\text{'s}| < 0.18$). This suggests that the items did not capture the same construct. We, therefore, deviated from our preregistration and only included the item that directly measured belief in conspiracy theories in our analysis ($M = 4.75$, $SD = 1.53$). This item correlated only weakly to moderately with other measures of conspiracist ideation ($0.18 < |r\text{'s}| < 0.30$). Conclusions based on this measure should be treated with caution.

3.2.4. Measurement (d): Fictitious political scenario (Leman & Cinnirella, 2007)

Participants read a fictitious and ambiguous scenario about the assassination of a president. They then rated their agreement with three different interpretations of the scenario on a 7-point scale from −3 = *not at all* to 3 = *perfectly sure*. One of the three interpretation suggested a conspiracy theory *(To what extent do you think the gunman was part of an organized group that planned to assassinate the President?*), while the other two suggested that the assassination was a solo effort. We performed a linear transformation to the scale so that *not at all* was represented as 1 and *perfectly sure* was represented as 7. The two non-conspiratorial items were then reverse-coded. In the total and sub-samples, the Cronbach alpha of the scale was unacceptably low (α's < 0.43) and the absolute correlations between the three items were low to moderate ($0.02 < |r\text{'s}| < 0.37$). Taken together, this suggests that the items did not clearly capture the same construct. We deviated from our preregistration and only included the item that directly measured belief in a conspiracy in our analysis ($M = 4.78$, $SD = 1.45$). Similar to the workplace measure, the conspiratorial item correlated only weakly to moderately with other measures of conspiracist ideation ($0.23 < |r\text{'s}| < 0.43$). Conclusions based on this measure should be treated with caution.

3.2.5. Belief in COVID-19 conspiracy theories (Exploratory)

We administered Van Bavel et al. (2022) COVID-19 conspiracy theory belief scale. Participants read four conspiracy theories related to the COVID-19 pandemic (e.g., *The coronavirus (COVID-19) is a hoax invented by interest groups for financial gains*). For each conspiracy theory, participants evaluated its likelihood to be true on a 11-point scale from 0 = *not at all* to 10 = *very much.* We later performed a linear transformation to the scale so that *not at all* was represented as 1 and *very much* was represented as 11. Responses to all items were averaged to a COVID-19 conspiracy theory belief score ($\alpha = 0.92$, $M = 4.50$, $SD = 3.01$).

3.2.6. Education

Education was measured on a 7-point scale.[9] Participants chose the highest level of education they had completed from a list of different education levels: (1) Less than High School [Ecole obligatoire] (2) Some High School, ~ 2 years career-based training [Attestation fédérale de formation professionnelle AFP, Certificat fédéral de capacité CFC, Certificat de culture Générale] (3) High School [Maturité professionnelle, Maturité spécialisée, Maturité gymnasiale] (4) Associates Degree, Advanced Career-based Training [Brevet fédéral BF, Diplôme fédéral DF, Diplôme ES] (5) Bachelor (6) Master (7) PhD [Doctorat]. Participants could also choose "other" and specify their education level in a comment box. Education levels reported in the comment box were matched with one of the listed education levels and received the corresponding numerical value for the statistical analyses.

3.2.7. Paranoid ideation

We administered a subscale of the General Paranoia Scale, a subclinical measure to study paranoid ideation in the general population (Fenigstein & Vanable, 1992). We administered the subscale that captures self-referential persecutory ideas (Barreto Carvalho et al., 2017; Bortolon et al., 2020), measured on a 5-point scale from 1 = *not at all applicable to me* to 5 = *extremely applicable to me*. Sample item: *Someone has it in for me.* Responses to all items were averaged to a paranoid ideation score ($\alpha = 0.86$, $M = 2.46$, $SD = 0.91$).

3.2.8. Attention checks

As an attention check, participants read the following: "People vary in the amount they pay attention to these kinds of surveys. Some take them seriously and read each question, whereas others go very quickly and barely read the questions at all. If you have read this question carefully, please write the word yes in the blank box below labelled other. There is no need for you to respond to the scale below." Below this instruction, participants saw an 8-point numerical scale in which the last option was labelled as "other". Failure to type "yes" in the blank box was coded as "attention check failed".

[9] We slightly modified the preregistered scale to better capture the complexities of the Swiss education system

Because we observed several bots and speeders when we first soft-launched the survey, we later added a second attention check at the beginning of the survey. We used a modified version of a rank-order screener developed and validated by Berinsky, Margolis, Sances, and Warshaw (2019). Before entering the survey, participants saw a list of physical activities and read: *"There are many ways to spend your free time. Many people enjoy doing sports. We also want to know if you are paying attention. Please ignore the following question and put "Swimming" in the top position and "Baseball" in the bottom position. Leave the rest of the issues in the same order. Please rank the following activities from 1 (most enjoyable) to 7 (least enjoyable). You can change your rankings by dragging and dropping different activities."* Only participants who passed this additional attention check were able to proceed.

3.3. Procedure

We conducted the study online using the Qualtrics software, Version 2021. After giving informed consent, participants first evaluated the randomness of the forty letter-strings and responded to the four original measurements of conspiracist ideation. Following Dieguez et al.'s (2015) methods, the presentation order of the original conspiracist ideation scales was counterbalanced and items within each scale were not randomized. Afterward, participants responded to the Paranoid Ideation Scale and the COVID-19 Conspiracy Theory Belief Scale. The presentation order of these two additional measures were randomized. This semi-random presentation order of scales ensured that the items of the two additional scales did not affect responses to the original items. The presentation order of items within the additional scales was also randomized. Afterward, participants were asked to report various demographic information, including their education level. Last, we presented the attention check and the debriefing form. Although Lucid uses screeners to exclude people who do not reside in the U.S. and Switzerland from participation, we additionally asked participants to report their place of residence (Country and State/Canton) for cross-validation. In the Swiss sample, they also indicated whether they are or have ever been living in the French-speaking part of Switzerland.

3.4. Results

3.4.1. Data exclusion

We report analysis results based on a sample that excluded participants who failed the attention check or self-reported to reside outside of the U.S. or Switzerland. For the Swiss sample, we additionally excluded participants who had never resided in the French-speaking part of Switzerland. Some results slightly changed when we additionally excluded participants whose responses to the dependent variables exceeded +/− 3 Standard Deviations ("statistical outliers"). These differences are indicated with asterisks throughout the paper. For 67 Swiss participants, one point on the COVID-19 conspiracy theory scale was missing. We excluded these participants in all analyses related to belief in COVID-19 conspiracy theories.

3.4.2. Direct replication

We calculated zero-order correlations between randomness dismissal and the original conspiracist ideation measures using the full sample. The statistical significance of the correlations was tested in accordance with Dieguez et al.'s (2015) approach ($p < .05$, one-tailed testing). We found that randomness dismissal was significantly positively associated with belief in classic conspiracy theories ($r = 0.12$) and generic conspiracist beliefs ($r = 0.12$). It was also very weakly associated with the tendency to believe in conspiratorial explanations for the workplace scenario ($r = 0.04$) and the political scenario ($r = 0.05$). However, neither of these relationships reached statistical significance. Further, randomness dismissal was significantly positively associated with belief in COVID-19 conspiracy theories ($r = 0.14$). Taken together, results based on our total sample lent support for the notion that randomness dismissal and conspiracist ideation are modestly related. Results are displayed in Table 3.

3.4.3. Model extensions

Preparing for the possibility that we could find statistically significant relationships, we preregistered to reduce the critical p-value to 0.01 for these analyses to adjust for alpha inflation due to multiple testing (Abdi, 2007). Since the measures varied in their scales, we used standardized scores for these analyses. We ran separate stepwise multiple linear regression analyses predicting each of the five conspiracist ideation measurements.

3.4.4. Culture

For each conspiracist ideation measurement, we first entered randomness dismissal, culture (dummy-coded; U.S. = 0, Switzerland = 1), and the interaction between randomness dismissal and culture as predictors into the model. As preregistered, we applied our stringent significance criteria ($p < .01$, two-tailed testing). The interaction between randomness dismissal and culture was significant for belief in classic conspiracy theories and generic conspiracist beliefs and marginally significant for belief in COVID-19 conspiracy theories. The interaction was not significant for the two scenario measures (see Table 4). Conclusions remained the same when we adjusted for paranoid ideation, although the interaction was now only marginally significant for belief in classic conspiracy theories ($\beta = 0.16$, $p = .014$) and trending for COVID-19 conspiracy theory beliefs ($\beta = 0.12$, $p = .097$).

3.4.4.1. Decomposing interaction. We then analyzed each country individually. First, we examined participants from the French-speaking part of Switzerland, where the original study by Dieguez et al. (2015) was conducted. In our Swiss sample, randomness dismissal was significantly positively associated with belief in classic conspiracy theories, generic conspiracist beliefs, and belief in COVID-19 conspiracy theories. These results contradict Dieguez et al.'s (2015) original null findings. Randomness dismissal was not associated with a tendency to endorse conspiratorial explanations for the workplace scenario and the political scenario, respectively (see Table 5). This is in line with Dieguez et al.'s (2015) null findings, although psychometric concerns of these measures limit this conclusion. Taken together, we found stronger evidence for a positive relationship between randomness dismissal and conspiracist ideation in our Swiss sample.

In the U.S. we found no significant relationship between randomness dismissal and any of the conspiracist ideation measures (see Table 5). This result is conceptually in line with Dieguez et al.'s (2015) finding but contradicts a larger body of work that found positive relationships between randomness dismissal and conspiracist ideation in the U.S.

In sum, we found evidence that culture influences the relationship between randomness dismissal and conspiracist ideation. However, the pattern that emerged in our study is the *opposite* of what previous work has found (see interactions in Fig. 1). Culture can thus not fully reconcile the contradicting evidence in the literature.

3.4.4.2. Education. We conducted separate multiple linear regression analyses predicting each of the conspiracist ideation measurements, respectively. As preregistered, we applied our stringent significance criteria ($p < .01$, two-tailed testing). We first entered randomness dismissal, education level, and the interaction between randomness dismissal and education level as predictors into the model. None of the interactions between education and randomness dismissal reached significance ($0.05 < p < .88$), although the interaction was marginally significant for the political scenario measure ($\beta = 0.07$, $p = .059$). The conclusion remained the same when we adjusted for paranoid ideation,

Table 3
Descriptive statistics and correlations of key variables for total sample.

Variable	*α*	*M*	*SD*		*1*	*2*	*3*	*4*	*5*	*6*	*7*
1. Randomness dismissal	0.94	0.46	0.15								
2. Classic conspiracy theories	0.78	3.60	1.48	***r***	**0.12****						
				CI_{95}	[0.05, 0.18]						
				df	805						
3. Generic conspiracist beliefs	0.93	2.88	0.87	***r***	**0.12****	**0.63****					
				CI_{95}	[0.05, 0.19]	[0.58, 0.67]					
				df	805	810					
4. Workplace conspiracy theory	–	4.75	1.53	***r***	**0.04**‡	**0.22****	**0.26****				
				CI_{95}	[−0.03, 0.11]	[0.15, 0.29]	[0.20, 0.33]				
				df	806	810	810				
5. Political conspiracy theory	–	4.78	1.45	***r***	**0.05**∔ / ❁	**0.38****	**0.34****	**0.25****			
				CI_{95}	[−0.02, 0.12]	[0.32, 0.44]	[0.28, 0.40]	[0.19, 0.32]			
				df	805	809	809	810			
6. Covid-19 conspiracy theories	0.92	4.50	3.01	***r***	**0.14****	**0.62****	**0.68****	**0.22****	**0.27****		
				CI_{95}	[0.07, 0.21]	[0.58, 0.67]	[0.64, 0.72]	[0.15, 0.28]	[0.20, 0.33]		
				df	740	743	743	743	742		
7. Paranoia	0.86	2.46	0.91	***r***	**0.09****	**0.45****	**0.38****	**0.24****	**0.24****	**0.40****	
				CI_{95}	[0.02, 0.16]	[0.39, 0.50]	[0.31, 0.43]	[0.18, 0.30]	[0.18, 0.31]	[0.34, 0.46]	
				df	805	810	810	810	809	743	
8. Education	–	3.96	1.40	***r***	**−0.08***	**−0.12****	**−0.14****	**−0.01**	**−0.09***	**−0.10****	**−0.04**
				CI_{95}	[−0.15, −0.01]	[−0.18, −0.05]	[−0.20, −0.07]	[−0.08, 0.05]	[−0.15, −0.02]	[−0.17, −0.03]	[−0.11, 0.03]
				df	805	810	810	810	809	743	810

Note. If not indicated otherwise, significance levels remain the same when conducting one-tailed or two-tailed testing. Values in square brackets indicate the 95% confidence intervals for each correlation (two-sided). * $p < .05$, ** $p < .01$. ∔Significant when statistical outliers were additionally excluded. ‡ Trending when statistical outliers were additionally excluded ❁ trending when one-tailed test.

although some of the interactions became marginally significant and trending (see Supplement). Conclusions also remained the same when we excluded statistical outliers (see Supplement).

3.4.5. *Exploratory analyses paranoid ideation*

Last, we conducted several exploratory analyses to investigate further the role of paranoid ideation in the relationship between randomness dismissal and conspiracist ideation. We conducted multiple linear regressions, regressing each of the conspiracist ideation measures on randomness dismissal and paranoid ideation (see Supplement). Conclusions for the relationships between randomness dismissal and each of the conspiracist ideation measures remained the same, although randomness dismissal became only a marginally significant predictor of belief in classic conspiracy theories ($\beta = 0.07$, $p = .018$).

4. Meta-analysis

The results of our pre-registered replication provided modest evidence in support of the hypothesis that randomness dismissal is related to conspiracy theory beliefs. However, this effect was moderated by culture in a direction that was contrary to the prior research. We conducted a meta-analysis of the entire literature (both published and unpublished) to further clarify the relationship between randomness dismissal and conspiracy theory beliefs and explored potential moderators.

4.1. *Methods*

We synthesized empirical findings on randomness dismissal and conspiracist ideation in a meta-analysis to complement our replication results. We sourced from the databases PsychInfo, JSTOR, and the Conspiracy Theory Research Base to identify published academic papers on randomness dismissal and conspiracist ideation. The Conspiracy Theory Research Base is a database of academic literature on conspiracy theories and other closely related topics (Douglas, 2021; Douglas et al., 2016). In addition, we emailed researchers in the field of conspiracy theory beliefs and posted on social media to further locate relevant papers. We called for both published and unpublished studies on this topic. To further locate unpublished work, we reviewed unpublished articles uploaded on PsyArXiv, Open Access Thesis and Dissertations, and ProQuest Dissertations.

For each unpublished paper, we evaluated whether the quality of its methods and analyses was sufficiently high to justify inclusion into the meta-analysis (one study did not meet this criterion and was excluded, see Supplement). Our meta-analysis included studies that were reasonably similar to Dieguez et al.'s (2015) study (Ioannidis, Patsopoulos, & Evangelou, 2007). This means, for instance, studies that did not directly measure randomness dismissal (e.g., Whitson & Galinsky, 2008) or studies that manipulated conspiracist ideation (e.g., van Prooijen et al., 2018, Study 5) were not included in the meta-analysis. Last, we examined data provided from a meta-analysis on motivational processes linked to conspiracy theories (Biddlestone et al., 2022). We identified 24 studies from 11 papers that met all our criteria, including our Swiss and U.S. surveys respectively. Most studies included multiple measures of conspiracist ideation, so we conducted a random effects meta-analysis to account for dependencies of effect sizes and differences in sample sizes (Borenstein, Hedges, Higgins, & Rothstein, 2009; Hedges, Tipton, & Johnson, 2010). For that, we implemented robust variance estimation (RVE) using the R package 'robumeta' (Fisher, Tipton and Zhipeng, 2017; Tanner-Smith, Tipton, & Polanin, 2016). All located papers and exclusion decisions are available on OSF (https://osf.io/aj64k/).

Table 4
Multiple linear regression analysis of conspiracist ideation measures on randomness dismissal and culture.

	Classic conspiracy theories				Generic conspiracist beliefs				Workplace conspiracy theory				Political conspiracy theory				COVID-19 conspiracy theories			
Predictors	β	SE	95% CI	*p*	β	SE	95% CI	*p*	β	SE	95% CI	*p*	β	SE	95% CI	*p*	β	SE	95% CI	*p*
(Intercept)	0.04	0.04	[−0.04 – 0.13]	0.340	0.06	0.04	[−0.03 – 0.14]	0.170	0.13	0.04	[0.04 – 0.21]	**0.003**	−0.06	0.04	[−0.15 – 0.02]	0.153	0.11	0.04	[0.03 – 0.19]	**0.010**
Randomness dismissal	0.03	0.04	[−0.05 – 0.12]	0.434	0.01	0.04	[−0.08 – 0.10]	0.842	0.01	0.04	[−0.08 – 0.09]	0.894	0.06	0.04	[−0.03 – 0.14]	0.208	0.07	0.04	[−0.01 – 0.16]	0.094
Culture	−0.09	0.07	[−0.24 – 0.05]	0.194	−0.14	0.07	[−0.28 – 0.00]	0.053	−0.33	0.07	[−0.48 – -0.19]	**<0.001**	0.17	0.07	[0.02 – 0.31]	**0.023**	−0.35	0.08	[−0.50 – -0.20]	**<0.001**
Interaction	0.20	0.07	[0.06 – 0.34]	**0.004**°	0.28	0.07	[0.14–0.42]	**<0.001**	0.08	0.07	[−0.06 – 0.22]	0.279	0.00	0.07	[−0.14 – 0.14]	0.996	0.19	0.08	[0.04 – 0.34]	**0.014**✢ / ¶
Observations	807				807				808				807				742			
R^2 / R^2 adjusted	0.026 / 0.022				0.038 / 0.034				0.029 / 0.026				0.009 / 0.006				0.054 / 0.051			

Note. Estimates based on standardized scored of randomness dismissal and dependent variables are reported. The dummy-coded predictor "Culture" was not standardized. U.S. coded as 0 and Switzerland coded as 1. ✢ Significant when statistical outliers were excluded. ° Marginally significant when statistical outliers were excluded ($p = .022$). ¶ Variable had a considerable positive skew. When we transformed the variable (square-rooting) to mitigate the skew, the interaction became significant ($p = .008$).

4.2. Results

Our final sample comprised 55 effect sizes that were transformed into Fisher's z correlation coefficients.[10] We fitted an intercept-only meta-regression model with correlated effect weights and found a significant, meta-analytical effect size of $z = 0.16$ ($SE = 0.03$, $t(22.7) = 4.66$; $CI_{95} = [0.09, 0.22]$, $p < .001$) which translates into a meta-analytical effect size of $r = 0.16$ (see Fig. 2). This effect size was very similar to the results of our pre-registered replication (except for our two scenario measures). We also found considerable effect size heterogeneity $I^2 = 87.12\%$ (Higgins & Thompson, 2002, Higgins, Thompson, Deeks, & Altman, 2003) which is consistent with the notion that the relationship between randomness dismissal and conspiracist ideation is sensitive to other factors.

Correlated effects models in RVE require an estimate for within-study correlations which we specified as $\rho = 0.08$. As a robustness analysis, we examined whether the average effect size would meaningfully change under different ρ estimates. A sensitivity analysis found that the meta-analytical effect size was robust against different values of ρ (see Supplement).

In light of the cultural differences observed in our preregistered replication, we explored whether culture would appear as moderator in our meta-analysis as well. Note that this analysis was not preregistered. Due to the small number of studies conducted in the French-speaking part of Switzerland, we grouped studies conducted in Europe and studies conducted in the U.S. We fitted a meta-analysis model with correlated effect weights, again implementing robust variance estimation (RVE), where we entered culture (here: U.S. = 0, Europe = 1) as a binary predictor. The main effect of culture was significant, $z = -0.20$, $SE = 0.06$, $t(20.79) = -3.51$, $CI_{95} = [-0.32, -0.08]$, $p < .001$) and in the opposite direction of our findings. The meta-analytic effect size was positive, moderate, and significant in the U.S. group ($z = 0.27$, $SE = 0.05$, $t(9.93) = 5.72$, $CI_{95} = [0.16, 0.37]$, $p < .001$, 22 effect sizes), but was positive, very small, and only marginally in the European group ($z = 0.07$, $SE = 0.03$, $t(11.5) = 2.06$; $CI_{95} = [-0.00, 0.14]$, $p < .010$, 33 effect sizes).

We then examined the possibility that publication bias (here: favoring findings of a significant positive relationship between randomness dismissal and conspiracist ideation) might have considerably biased our meta-analytic effect size.

First, we assessed how sensitive our meta-analysis is to potential publication bias using the R package 'Publicationbias' (Mathur & VanderWeele, 2020). Specifically, we identified the severity of publication bias required to attenuate our meta-analytic effect size estimate to zero. Severity of publication bias is quantified as the ratio by which affirmative studies (here: significant positive associations) are more likely to be published than non-affirmative studies (here: non-significant or significant negative associations). Fig. 3 displays corrected meta-analytical effect size estimates and confidence intervals as a function of different severities of publication bias. The meta-analytic effect size remained positive, and the lower confidence interval remained greater than zero even under severe publication bias. We should note, however, that the meta-analytic effect size estimate quickly approximated small effect sizes. For instance, the meta-analytic effect size estimate would be $r = 0.10$ *if* significant positive associations would be roughly twice as likely to get published as non-significant and significant negative associations. These results suggest that although the positive relationship between randomness perception and conspiracist ideation seem to be relatively robust against publication bias, it might well be small in the case of a moderate publication bias.

Next, we assessed how likely it is that such a publication bias in the

[10] Some studies used experimental designs. We added effects sizes obtained within condition separately. Conclusions did not change when we collapsed effect sizes over conditions.

Table 5
Correlations and descriptive statistics of key variables for U.S. and Swiss samples.

			U.S. Sample		1. Randomness	2. Classic	3. Generic	4. Workplace	5. Political	6. COVID-19	7. Paranoia	8. Education
			α		0.94	0.80	0.94	–	–	0.91	0.88	–
			M		0.47	3.66	2.93	4.93	4.69	4.83	2.53	4.11
Swiss Sample	*α*	*M*	*SD*		0.15	1.52	0.90	1.43	1.48	3.05	0.98	1.17
1. Randomness dismissal	0.95	0.45	0.16	*r*		0.03	0.01	0.01	0.05✢	0.07▽	0.04	-0.05
				CI_{95}		[−0.05, 0.12]	[−0.08, 0.09]	[−0.08, 0.09]	[−0.03, 0.14]	[−0.02, 0.16]	[−0.04, 0.13]	[−0.13, 0.04]
				df		516	516	517	516	516	516	516
2. Classic conspiracy theories	0.75	3.50	1.40	*r*	0.26**		0.61**	0.19**	0.42**	0.61**	0.52**	−0.09*
				CI_{95}	[0.15, 0.37]		[0.55, 0.66]	[0.11, 0.27]	[0.34, 0.49]	[0.56, 0.66]	[0.45, 0.58]	[−0.17, −0.00]
				df	287		517	517	516	517	517	517
3. Generic conspiracist beliefs	0.92	2.79	0.82	*r*	32**	0.66**		0.28**	0.39**	0.68**	0.40**	−0.14**
				CI_{95}	[0.21, 0.42]	[0.59, 0.72]		[0.20, 0.36]	[0.31, 0.46]	[0.63, 0.72]	[0.33, 0.47]	[−0.23, −0.06]
				df	287	291		517	516	517	517	517
4. Workplace conspiracy theory	–	4.41	1.64	*r*	0.08❁	0.26**	0.21**		0.24**	0.21**	0.23**	−0.01
				CI_{95}	[−0.03, 0.19]	[0.15, 0.36]	[0.10, 0.32]		[0.16, 0.32]	[0.13, 0.29]	[0.15, 0.31]	[−0.10, 0.08]
				df	287	291	291		517	517	517	517
5. Political conspiracy theory	–	4.93	1.40	*r*	0.06✢	0.33**	0.26**	0.32**		0.35**	0.31**	−0.01
				CI_{95}	[−0.05, 0.18]	[0.22, 0.43]	[0.15, 0.37]	[0.21, 0.42]		[0.27, 0.42]	[0.23, 0.39]	[−0.09, 0.08]
				df	287	291	291	291		516	516	516
6. COVID-19 conspiracy theories	0.92	3.75	2.76	*r*	0.29**	0.66**	0.68**	0.17*	0.11		0.44**	−0.10*
				CI_{95}	[0.17, 0.41]	[0.57, 0.72]	[0.60, 0.74]	[0.04, 0.29]	[−0.02, 0.24]		[0.36, 0.50]	[−0.19, −0.02]
				df	222	224	224	224	224		517	517
7. Paranoia	0.82	2.35	0.77	*r*	0.19**	0.27**	0.30**	0.24**	0.11▽	0.27**		−0.04
				CI_{95}	[0.07, 0.30]	[0.16, 0.37]	[0.19, 0.40]	[0.12, 0.34]	[−0.00, 0.22]	[0.14, 0.38]		[−0.13, 0.05]
				df	287	291	291	291	291	224		517
8. Education	–	3.71	1.71	*r*	−0.14*	−0.18**	−0.17**	−0.07	−0.17**	−0.14**	−0.09	
				CI_{95}	[−0.25, −0.03]	[−0.29, −0.06]	[−0.28, −0.05]	[−0.18, 0.05]	[−0.28, −0.06]	[−0.27, −0.01]	[−0.20, 0.03]	
				df	287	291	291	291	291	224	291	

Note. If not indicated otherwise, significance levels remain the same when conducting one-tailed or two-tailed testing. Values in square brackets indicate the 95% confidence intervals for each correlation (two-sided). * $p < .05$, ** $p < .01$ ▽Only significant when one-tailed testing. ✢Significant when statistical outliers were additionally excluded. ‡ Trending when statistical outliers were additionally excluded ❁ Trending but only when one-tailed testing.

literature on randomness dismissal and conspiracist ideation exists. Specifically, we plotted and examined a standard contour-enhanced funnel plot (Peters, Sutton, Jones, Abrams, & Rushton, 2008) and a significance funnel plot (Mathur & VanderWeele, 2020). Both plots did not show strong indicators of publication bias (see Fig. 4). Egger's regression test for funnel plot asymmetry was also not significant ($z = -0.35$, $p = .727$). In sum, there was not clear evidence of publication bias, and the meta-analytic effect size was very similar to the results of our well powered pre-registered replication.

Taken together, it seems unlikely that publication bias in the literature on randomness dismissal and conspiracist ideation substantially impacted our meta-analytic effect size. The positive relationship between randomness dismissal and conspiracist ideation appears to be relatively robust against potential publication bias and we also found no strong evidence for the presence of such publication bias.

5. General discussion

We conducted a high-powered preregistered cross-cultural replication ($N = 814$) and meta-analysis of the literature ($N_{Effectsizes} = 55$) to clarify the relationship between randomness dismissal and conspiracist ideation. Using demographically diverse samples collected in Switzerland and the U.S., our study examined three potential explanations for inconsistencies in previous findings: culture, education, and statistical power. We found that randomness dismissal was positively related to conspiracist ideation in our study and our meta-analysis. Moreover, the effect sizes were similar across the multiple approaches ($0.12 < r_{significantStudies} < 0.15$, $r_{MetaAnalysis} = 0.16$). The association between randomness dismissal and conspiracist ideation was also distinguishable from paranoid ideation (study) and relatively robust against publication bias (meta-analysis). However, our findings also suggest that the relationship between randomness dismissal and conspiracist ideation is heterogenous and may be sensitive to other factors like social context.

We found that culture moderated the relationship between randomness dismissal and conspiracist ideation. This finding is consistent with a growing body of research suggesting that culture influences the relationships between conspiracy theory beliefs and various psychological processes (e.g., Biddlestone, Green and Douglas, 2020; van Prooijen & Song, 2021; Biddlestone et al., 2022). Yet, the cultural differences that emerged in our study were somewhat surprising. So far, most studies that did not find a relationship between randomness dismissal and conspiracist ideation were conducted in the French-speaking part of Switzerland, whereas most studies that found such a relationship were conducted in the U.S. This was also evident in our exploratory meta-analysis where we found that the relationship between randomness dismissal and conspiracist ideation was statistically significant for U.S. samples but not for European samples. Surprisingly, our preregistered study found the opposite pattern. Randomness dismissal and conspiracist ideation were mostly positively related in the Swiss

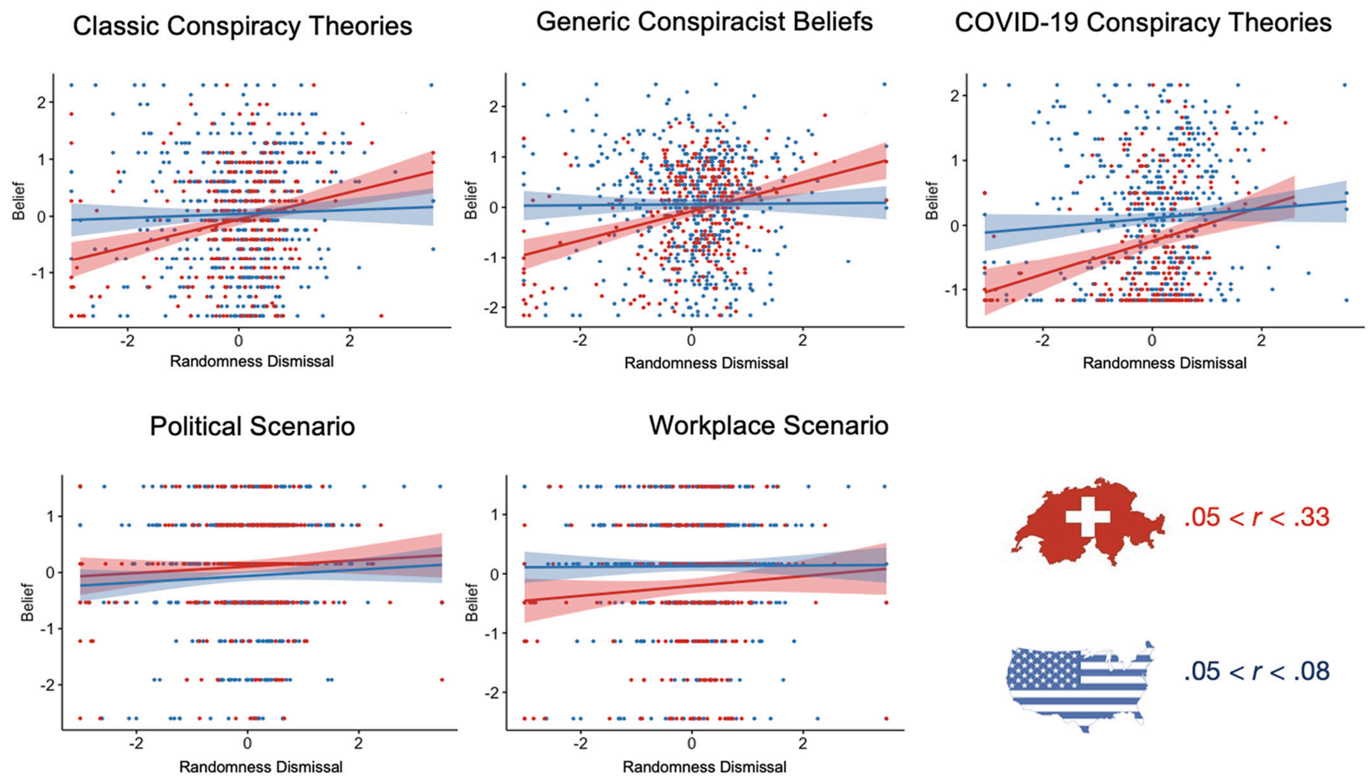

Fig. 1. Culture moderates the relationship between randomness dismissal and conspiracist ideation.

sample (contradicting the null findings of Dieguez et al. (2015) Study 1 that we attempted to replicate) but were not related in the U.S. sample (contradicting previous U.S. studies). Although culture impacts the relationship between randomness dismissal and conspiracist ideation, differences in cultural context cannot fully account for the inconsistent findings in the literature.

Education, on the other hand, did not moderate the relationship between randomness dismissal and conspiracist ideation. Differences in the samples' education level are therefore unlikely to be the cause of inconsistencies in research findings.

Our sample sizes ($N_{\text{Total}} = 814$, $N_{\text{Switzerland}} = 293$, $N_{\text{US}} = 521$) were large enough to provide stable correlation estimates (Schönbrodt & Perugini, 2013) and to detect even small correlations and interaction effects. The effect sizes that we found for the relationship between randomness dismissal and conspiracist ideation were generally very small. Some of the previous studies, therefore, might have obtained null results due to insufficient statistical power (see Table 1).

One pressing question is whether randomness dismissal is a cognitive or motivational process – or both. People may be more prone to dismiss randomness as a possible cause of an event because they are less skilled to correctly discern random processes from non-random processes and outcomes, and/or because they are motivated to believe that the world is ordered and predictable. Disentangling these psychological processes will be critical to grasp the complex relationship between randomness dismissal and conspiracist ideation. We found that education did not impact the relationship between randomness dismissal and conspiracist ideation in our study. Since education is highly correlated with cognitive skills (Ritchie & Tucker-Drob, 2018), this might be tentative evidence that examining cognitive processes alone is not sufficient to understand the relationship between randomness dismissal and conspiracist ideation. Indeed, people are very good at grasping randomness, regardless of whether they are more prone to believe in conspiracy theories or not (Dieguez et al., 2015). The link between randomness dismissal and conspiracist ideation might therefore be modulated by other motivational states (Biddlestone et al., 2022; Douglas et al., 2017). For example, research suggests that randomness dismissal and conspiracist ideation are influenced by people's need for control (e.g., Whitson & Galinsky, 2008; van Prooijen & Acker, 2015; cf. Stojanov & Halberstadt, 2020).

Looking at randomness dismissal and conspiracist ideation through a motivational lens might help us understand the cultural differences in our study. We speculate that the relationship between randomness dismissal and conspiracist ideation is contingent on other motives such as existential motives. In one set of studies, researchers found that the link between the need for cognitive closure and conspiracy theory beliefs was conditional on people's sense of uncertainty (Marchlewska, Cichocka, & Kossowska, 2017). At the time we conducted our study, COVID-19 cases in Switzerland skyrocketed. The relative number of daily new COVID-19 cases in Switzerland was more than three times higher than in the U.S. Societal crises increase feelings of uncertainty and conspiratorial thinking (van Prooijen & Douglas, 2017), and might also influence the relationship between randomness dismissal and conspiracist ideation.

In addition, research found that valuing epistemic rationality moderates the relationship between analytical skills and conspiracy theory beliefs. More specifically, people with strong (versus lower) analytical skills are only less likely to believe conspiracy theories if they are also motivated to come to rational and logical conclusions (Ståhl & Van Prooijen, 2018). It could be that similar dynamics occur for the relationship between randomness dismissal and conspiracist ideation, which may explain the cultural differences in our study. It could be that our U. S. participants were not sufficiently motivated to be "right" for the relationship between randomness dismissal and conspiracist ideation to emerge, maybe because other motives like the desire to align one's beliefs with one's political ingroup took priority (Sternisko et al., 2021; Sternisko, Cichocka, & Van Bavel, 2020; Van Bavel & Pereira, 2018). Indeed, some of the conspiracy theories we presented are highly politicized in the U.S. (e.g., COVID-19).

It is important to note that randomness dismissal, is an umbrella term that encompasses different psychological processes. The dismissal that

Forest Plot for Meta-Analysis

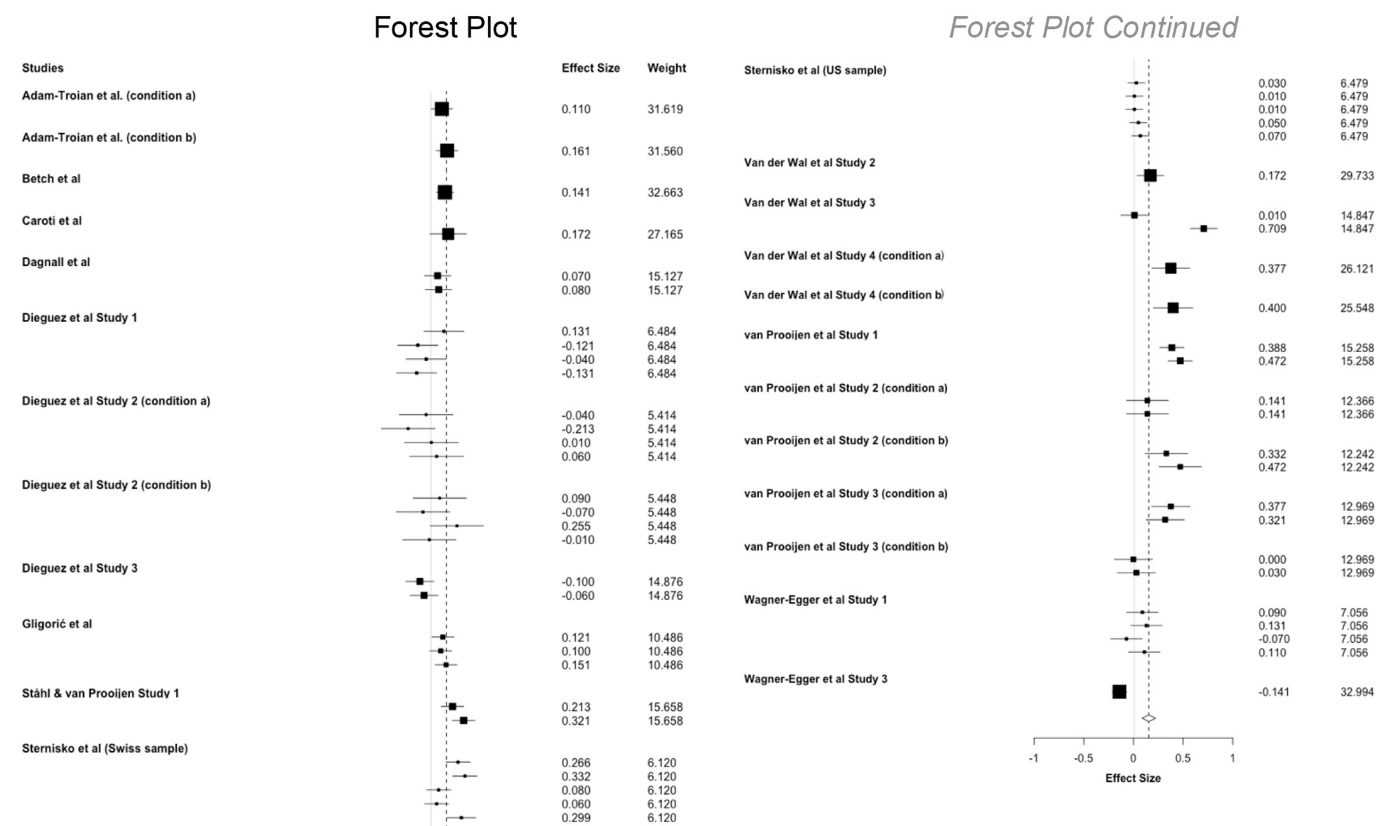

Fig. 2. Forest plot for meta-analysis.
Note. Boxes indicate point estimates of individual effect sizes with lines indicating the 95% confidence intervals. Box sizes are proportional to the weight assigned to the respective effect size. The overall, meta-analytic effect size is represented by the dashed line and the diamond with its width reflecting the 95% confidence interval.

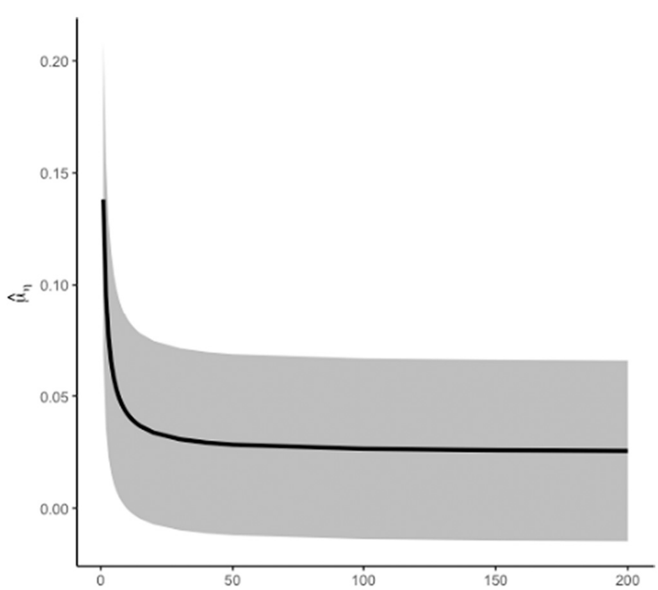

Fig. 3. Publication sensitivity.
Note. Corrected point estimates and 95% confidence intervals (shaded area) as a function of publication bias severity.

the *process* that created the letter strings is random (captured in our study) is not psychologically identical to the perception of *patterns* in random letter strings. Further, we should distinguish between patterns, true randomness, and illusory randomness (see Dieguez et al., 2015; Oskarsson, Van Boven, McClelland, & Hastie, 2009; Williams & Griffiths, 2013 for discussions). Conflicting findings in the literature on randomness dismissal and conspiracist ideation could be the result of conceptual and measurement differences between studies (see Brotherton, 2019; van Prooijen et al., 2018). Although our study was not designed to examine this possibility, our results offer important insights on that matter. We administered the same scale to assess randomness dismissal in the U.S. and Switzerland. Nevertheless, we found differences in how randomness dismissal relates to conspiracist ideation. Moreover, our Swiss findings diverge from Dieguez et al.'s (2015) Swiss findings even though we used the same measures. While distinguishing between different facets of randomness dismissal is theoretically important and urgently demands future research, our findings suggest that those differences cannot fully account for current inconsistencies in the literature either.

We would like to highlight that we do not claim any epistemological authority over the truth of the conspiracy theories that we presented. In fact, while most conspiracy theories were false or at least extremely implausible (e.g., moon landing was faked, COVID-19 is a hoax, see Supplement), some were tenable (e.g., information is deliberately concealed from the public out of self-interest, see Supplement). We agree with other researchers that the veracity and plausibility of conspiracy theories is difficult to assess and often secondary to the research

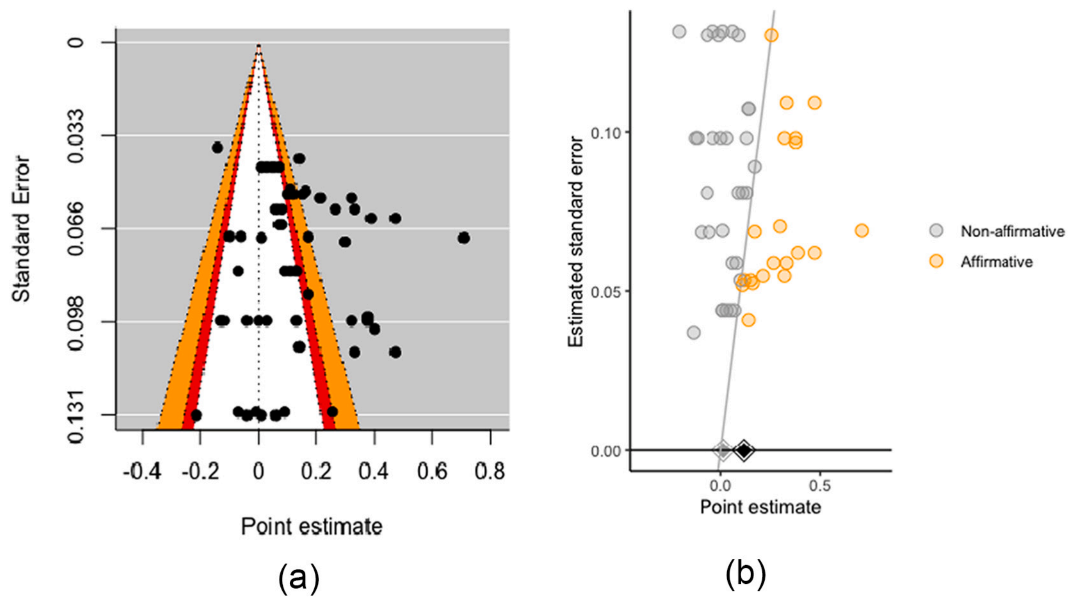

Fig. 4. Assessing publication bias.
[a] Contour-enhances funnel plot. Each black dot denotes an individual study finding. The white area marks p-values greater than 0.10, the red area marks p-values between 0.10 and 0.05, the orange area marks p-values between 0.05 and 0.01, and the grey area marks p-values below 0.01. [b] Significance funnel plot. Orange dots represent affirmative findings (i.e., a significant positive relationship between randomness dismissal and conspiracist ideation). Grey dots represent non-affirmative findings. The diagonal line denotes p = .05. The black diamond represents the meta-analytic effect size estimate. The light grey diamond indicates the meta-analytic effect size estimate when only non-affirmative findings were included in the analysis.

questions (e.g., Dentith, 2018; Uscinski, 2018), including our own. We examined the generalizability of the notion that randomness dismissal and conspiracist ideation are related. This notion rests on the rationale that people who are quick to dismiss randomness as a possible cause of an event are also more ready to believe narratives that negate the idea that an event happened by accident. All conspiracy theories – true or false – provide such narratives. Naturally, conspiracy theories vary in the rigidity and expansiveness of such proposed processes and patterns, which may have implications for their relationship with randomness dismissal. Future research may explore this possibility. Identifying psychological processes that are linked to conspiracy theory beliefs more generally is an important steppingstone for researchers who are interested in the belief of specific types of conspiracy theories like irrational ones.

One of the most important messages to take away from our paper is that the relationship between randomness dismissal and conspiracist ideation is sensitive to contextual factors. Assessing findings more holistically via meta-analytical strategies is therefore critical. Our meta-analytical findings deserve particular attention. In our meta-analysis we synthesized 55 effect sizes obtained from 11 papers and found a significant positive relationship between randomness dismissal and conspiracist ideation. Yet, this association was small ($r = 0.16$), suggesting that –on average– randomness dismissal itself plays only a moderate role in conspiracist ideation.

5.1. Limitations and future directions

We found robust, positive relationships between randomness dismissal and three of the five conspiracist measurements in the total sample as well as the Swiss sample. The relationship did not reach significance for the two scenario measures. One interpretation could be that randomness dismissal is only associated with certain facets of conspiracist ideation. This is consistent with a growing body of research suggesting psychological differences between conspiracy theory mindset and belief in specific conspiracy theories (Biddlestone et al., 2022; Frenken & Imhoff, 2021; Enders et al., 2021; Stojanov & Halberstadt, 2020). In addition, the scenarios were short and there were not many "random dots to connect". In such cases, it makes sense that randomness dismissal does not correlate with people's belief in conspiratorial narratives.

Another conceivable explanation for the scenarios' distinct patterns concerns measure validity. If the scenario measures would in fact capture a different facet of conspiracist ideation, we would expect that they correlate highly with each other and at least moderately with other measures of conspiracist ideation (Frenken & Imhoff, 2021; Enders et al., 2021). This was not the case. The correlation between the two scenario measures was only small to moderate, and their internal consistencies were also unacceptably low.[11] The correlations between conspiracy theory ideation and both scenario measures were also slightly lower than what one would generally expect from previous work (Imhoff & Lamberty, 2018). Measurement validity might be particularly challenging for the workplace scenario. Here, participants imagined getting a promotion denied and being themselves the target of a conspiracy. The item may therefore also capture people's self-serving biases (Miller & Ross, 1975) and a general distrust of people in one's immediate environment. Taken together, there is reason to believe that, in our study, the two scenario scales were imprecise measures of conspiracist ideation. Future research may further investigate these possibilities.

We recruited large samples, diverse in their gender, age, and educational attainment. This increases the generalizability of our findings. Yet, both samples were recruited in W.E.I.R.D (Wester, Educated, Industrialized, Rich, Democratic) countries and online. This recruitment strategy was most appropriate for our paper's aim to reconcile previous findings in the literature, which to our knowledge were all obtained in W.E.I.R.D. countries. At the same time, it limits the conclusions that we can draw about the nature of the relationship between randomness dismissal and conspiracist ideation more broadly (Biddlestone et al., 2022; Henrich, Heine, & Norenzayan, 2010; Roberts, Bareket-Shavit, Dollins, Goldie, & Mortenson, 2020). We urge future research to recruit more diverse samples to understand the complexities of

[11] Reliability issues have emerged for the workplace measure before (e.g., Kumareswaran, 2014).

randomness dismissal and conspiracist ideation. This is particularly important given that our findings suggest that these processes are sensitive to social context.

In our meta-analysis, we conducted a comprehensive search to identify studies on randomness dismissal and conspiracist ideation. Yet, it is possible that we did not locate all data on that topic. Furthermore, all studies were conducted in W.E.I.R.D countries. Therefore, our meta-analysis offers only a first glimpse into the relationship between randomness dismissal and conspiracist ideation.

5.2. Conclusion

In April 2020, conspiracy theory supporter David Icke broadcasted an interview on YouTube claiming that COVID-19 was linked to 5G technology. Though his theory was baseless, 65,000 people livestreamed the interview (Kelion, 2020). Conspiracy theories are not fringe ideas, and understanding their psychological processes is pressing and timely.

Although anecdotal evidence such as the 5G conspiracy theory suggests that the tendency to dismiss chance and randomness may be linked to conspiracy theory beliefs, empirical work paints a more complicated picture. We used complementing methods –direct replication, theoretical extension, and meta-analysis– to better understand the nature of the relationship between randomness dismissal and conspiracist ideation. We found that differences in culture, education, and measurement could not fully account for such inconsistent findings. Together, our results highlight the complexity and context-sensitivity of the relationship between randomness dismissal and conspiracist ideation.

Disclosure statement

In the present manuscript we report all measures, manipulations, and exclusions in the proposed study. We also report the method of determining the final sample size.

Conflict of interest

Nothing declared.

Author contributions

A.S. & J.V·B authors designed the pre-registered study. S.D. gave feedback on study design and translated relevant materials, A.S. drafted the manuscript. J.V.B. and S.D. edited the manuscript.

OPEN PRACTICES STATEMENT

All experimental materials, methodological approach (including sample size determination and data exclusion criteria), and analysis plan, were preregistered on OSF (https://osf.io/aj64k/). All deviations from the preregistration are reported in this manuscript. Data, analysis code, and study materials are publicly available on the same OSF page.

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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.2022.104357.

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