Reasons to Believe: A Systematic Review and Meta-Analytic Synthesis of the Motives Associated with Conspiracy Beliefs

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Data Availability Statement

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Abstract

Belief in conspiracy theories has been linked to harmful consequences for individuals and societies. In an effort to understand and mitigate these effects, researchers have sought to explain the psychological appeal of conspiracy theories. This article presents a wide-ranging systematic review and meta-analysis of the literature on conspiracy beliefs. We analyzed 971 effect sizes from 279 independent studies ($N_{participants}$ = 137,406) to examine the relationships between psychological motives and conspiracy beliefs. Results indicated that these relationships were significant for all three analyzed classes of motivation: epistemic (k = 114, r = .14), existential (k = 121, r = .16), and social motivations related to the individual, relational, and collective selves (k = 100, r = .16). For all motives examined, we observed considerable heterogeneity. Moderation analyses suggest that the relationships were weaker, albeit still significant, when experimental (vs. correlational) designs were used, and differed depending on the conspiracy measure used. We statistically compare the absolute meta-analytic effect size magnitudes against each other and discuss limitations and future avenues for research, including interventions to reduce susceptibility to conspiracy theories.

Keywords: meta-analysis, systematic review, conspiracy beliefs, conspiracy theories, motives

Public Significance Statement

Conspiracy theories pose a threat to individuals, groups, and societies, and it is therefore important to understand why people endorse them. Our meta-analysis of 279 psychological studies shows that people are more likely to endorse conspiracy theories when their psychological needs are frustrated. These include epistemic needs (to feel informed), existential needs (to feel secure), and social needs (to feel valued). People may find conspiracy theories less appealing if they have alternative ways to meet their psychological needs, or if their needs are not frustrated in the first place.

Reasons to believe: A systematic review and meta-analytic synthesis of the motives associated with conspiracy beliefs

Research suggests that belief in conspiracy theories is a threat to democratic processes and social cohesion. Conspiracy theories have been linked to extremist ideology (Enders & Uscinski, 2021; Imhoff, Zimmer et al., 2022), political violence (Rottweiler & Gill, 2020), prejudice (Jolley et al., 2019), and problematic civic disengagement, including unwillingness to reduce one's carbon footprint (Biddlestone et al., 2022; Jolley & Douglas, 2014a), to follow COVID-19 pandemic guidelines (e.g., Biddlestone et al., 2020), or to accept COVID-19 vaccines (e.g., Bertin et al., 2020). To better understand the social and psychological implications of conspiracy theories, researchers have sought to explain their psychological appeal. Douglas and colleagues' (2017, 2019) reviews of the literature suggest that conspiracy theories are especially attractive to people who are seeking to satisfy basic unmet psychological motives—that is, psychological states that guide individuals toward their desired goals through directed thoughts and actions (see Ryan & Deci, 2000).

Although recent meta-analytic efforts have made progress in identifying the role of personality and motivational variables related to conspiracy beliefs (e.g., Bowes et al., 2023;[1] Stasielowicz, 2022a; Sternisko et al., 2022; Stojanov & Halberstadt, 2020; Yelbuz et al., 2023), they have not directly compared the absolute strengths of the different motives. A nuanced understanding of these motives is important in guiding efforts to protect individuals and society from the adverse effects of conspiracy beliefs. In the meta-analysis presented here, we aim to provide a comprehensive synthesis of the motives associated with conspiracy beliefs. We quantify and compare the absolute meta-analytic effect sizes of different

[1] Note that Bowes and colleagues' (2023) meta-analysis was partially conducted as a replication of the current findings (which was previously posted as a white paper) and published in parallel.

motivational factors. Our aim is to advance efforts to understand the ranked importance and shared variance of the many motivational factors that are linked to belief in conspiracy theories, and how those motives may be structured hierarchically.

To achieve this objective, we systematically test a theoretical model by Douglas and colleagues (2017) who linked conspiracy beliefs to the motives to achieve certainty (that is, epistemic motives; e.g., automatic thinking styles), security (that is, existential motives; e.g., perceived external threats), and maintaining a positive image of oneself and the groups that one belongs to (that is, social motives; e.g., low self-esteem). Our primary goal was to compare the absolute meta-analytic effect size magnitudes of the three motive types, their subtypes, and the specific motives themselves. We also examined the extent to which the associations between motives and conspiracy beliefs might depend on socio-political or cultural contexts. Finally, to evaluate the strength of the causal evidence for the motivational account, we compare effect sizes that are based on experimental versus correlational evidence.

In our review, we first outline contemporary theoretical accounts of belief in conspiracy theories, alongside important applied and theoretical issues. Second, we explain how and why motivations are a particularly useful lens through which to view the reasons people adopt conspiracy beliefs. Third, we highlight the various approaches to their measurement. Finally, we explain the importance of the current meta-analysis, identifying key questions it can answer.

Belief in Conspiracy Theories: Definition and Unanswered Questions

Conspiracy theories allege that two or more individuals have conspired—that is, have colluded in secret to bring about a nefarious outcome—in a way that is of public interest but not (yet) public knowledge (e.g., Douglas et al., 2019; Douglas & Sutton, 2023). In the context of the COVID-19 pandemic for example, conspiracy theories targeted the Chinese

government (Lee, 2020), pharmaceutical companies (Grimes, 2021), and public figures like Bill Gates and his associates (Goodman & Carmichael, 2020a) among others (e.g., Goodman & Carmichael, 2020b; Ramzy & Chien, 2021). Conspiracy theories therefore blame negative events and circumstances on a wide variety of individuals and groups, crediting them with power and malevolent intentions (Nera et al., 2022).

Despite considerable recent advancements in understanding the impact of beliefs in conspiracy theories, several pertinent questions remain unanswered. The evidence reviewed by Douglas and colleagues (2017) suggests that conspiracy beliefs are associated with epistemic, existential, and social motives. Yet, less is known about the nature of these relationships. Unanswered questions include whether some motives play a more central role than others (van Prooijen, 2020) or whether there is causal evidence for a frustrated motives model (Stojanov & Halberstadt, 2020). It also remains unclear whether these beliefs actually alleviate the unmet motives that drive their endorsement in the first place (but see Liekefett et al., 2021 for longitudinal evidence), whether they serve to challenge or defend the status quo (e.g., Jolley et al., 2018), are harmful or constructive to personal well-being (Cichocka, 2020; Douglas et al., 2015), and whether they should be seen as threatening, bolstering, or a manifestation of the processes that are inherent to democratic societies (Douglas et al., 2019).

Many researchers tend to focus on the motivational account of conspiracy beliefs, whereas others extend their investigations into other potential factors, such as societal circumstances (e.g., structural inequalities; van Prooijen et al., 2018) and personality factors (e.g., Stasielowicz, 2022a; see Dweck, 2017). We argue that to answer some of the outstanding questions in the conspiracy beliefs literature, we must first understand why people are drawn to conspiracy theories. In other words, a comprehensive theoretical picture of the different types of motives that predict conspiracy beliefs will inform efforts to

understand and intervene upon their consequences. We also seek to work backwards to examine the factors that might moderate the impact of these motives.

Motives and Conspiracy Beliefs

Decades of psychological research have led to widespread support for the notion that people's motivated reasoning—forming beliefs and attitudes based on desired outcomes (Kunda, 1990) and collective goals (Tetlock, 2002) rather than evidence—shapes the way in which they perceive the world. This work has uncovered how motivated thinking strategies are contingent on goals (Fiske, 1992) that direct the construction and biased justification of beliefs (Kunda, 1990) in attempts to cope with adaptive challenges (Tetlock, 2002). These mechanisms—reflecting attempts to address epistemic, existential, and social motives—are often associated with maladaptive coping strategies, such as adopting motive substitutes (i.e., attempts to satisfy thwarted motives vicariously) or psychological rigidity (i.e., an unwillingness to alter one's beliefs in the face of contrary evidence; see Deci & Ryan, 2000). As a result, motivated cognition can differentially account for ideological beliefs and attitudes (e.g., Jost et al., 2003), often regardless of, or in direct conflict with, strong and abundant evidence.

Douglas and colleagues (2017, 2019) argued that conspiracy theories can be particularly appealing to people experiencing feelings of uncertainty, through the promise of neat and comprehensive explanations for complex phenomena. This response reflects the adoption of conspiracy beliefs as a result of *epistemic motives*, such as intolerance of uncertainty. Furthermore, the authors outlined how threats to feelings of security might turn individuals towards conspiracy theories in attempts to understand and regain a sense of autonomy over *existential threats* that may otherwise appear random and largely uncontrollable. Finally, Douglas and colleagues argued that conspiracy beliefs may appeal because of the *social motives* people have to maintain and defend the image of oneself and

the groups that one belongs to. For example, conspiracy theories might be adopted to deflect from personal shortcomings or position the ingroup in a morally superior victim role. Douglas and colleagues (2017) therefore provided a comprehensive framework in which to place a meta-analytic synthesis of the variables associated with conspiracy beliefs. Nevertheless, more specific theoretical perspectives are required to categorize the variables that fall into these three broad motivational domains (see Figure 1).

Epistemic Motives

We conceptualize epistemic motives as variables that foster the desire for knowledge states (e.g., ambiguity, accuracy, certainty, confidence), and to employ preferred cognitive processes to attain those states (e.g., deliberation, intuition, critical thinking). Epistemic motives inherently involve metacognitive appraisal of the knowledge one has and the processes one has followed to obtain it (Goupil & Proust, 2023; Richter & Schmid, 2010). External stimuli can trigger epistemic motives. For example, political unrest (e.g., Kofta et al., 2020) and global crises (see van Prooijen & Douglas, 2017) typically create aversive levels of uncertainty, by impeding people's ability to predict and anticipate threatening circumstances (Kramer, 1998; Park, 2010; van den Bos, 2009). This motivates sense-making processes that may include intuitive or reflective thinking (Feeney et al., 2017; Kramer, 1998; Park, 2010; van den Bos, 2009; Tversky & Kahneman, 1983). In contrast to subjective uncertainty, states of overconfidence may reduce people's willingness to employ deliberative thinking appropriately in order to evaluate their beliefs (Pennycook et al., 2022). More widely, the desire for structure and cognitive closure can affect the way people pursue, avoid, and think about information (Webster & Kruglanski, 1994; Kruglanski, 2011; Kossowska & Bukowski, 2015; Kruglanski & Webster, 1996; Bhushan & Amal, 1986; Zhao et al., 2014).

Variables such as the need for closure, or intuitive and deliberative thinking styles, can be described as *cognitive styles*. Cognitive styles are goal-directed and have been

described as “microstrategies for cognitive operations” (Stanovich & Stanovich, 2010; see also Gjoneska, 2021). Small-to-medium relationships have been found between cognitive styles and conspiracy beliefs in the meta-analyses conducted by both Yelbuz and colleagues (2023), r = .19, and Bowes and colleagues (2023), rs = .15 to .21.

To achieve a comprehensive understanding of epistemic motives in conspiracy belief, we must also consider the role played by cognitive abilities. C*ognitive abilities* determine the capacity to build one’s knowledge (e.g., through information search and inference), and to evaluate and update one’s knowledge effectively. Relevant indicators of cognitive ability, including lower intelligence (Swami et al., 2011) or other measures of poor reasoning ability such as numeracy or vocabulary (e.g., Swami & Furnham, 2012; Rizeq et al., 2020; Adam-Troian et al., 2019; Stieger et al., 2013), are likely to be associated with conspiracy beliefs as a result of an inability to apply critical reasoning (Gjoneska, 2021). When the cognitive demands of a situation or a preferred cognitive style exceed people’s cognitive abilities, they are likely to experience frustration of their epistemic goals and may select less optimal strategies for the pursuit of knowledge. Under these circumstances, conspiracy narratives are especially appealing and especially difficult to resist, because the cognitive abilities needed to critically appraise these narratives are lacking (Douglas et al., 2017; 2019). In keeping with this perspective, a small-to-medium relationship between low cognitive abilities and conspiracy beliefs was found in the meta-analyses presented by Stasielowicz (2022a), r = .13, and Bowes and colleagues (2023), r = .16. Therefore, in the current meta-analysis, we aim to further establish the absolute meta-analytic effect sizes of cognitive abilities as well as cognitive styles and compare their absolute effect size magnitudes.

Existential Motives

Our approach to existential motives relies on the work by Onraet and colleagues (2013), who distinguished between *internal* and *external* existential threats. Measures of

internal threats concern those that capture ongoing mental distress, such as anxieties (e.g., Grzesiak-Feldman, 2007) and feelings of low personal control (e.g., Imhoff & Lamberty, 2018). In contrast, measures of external threats capture heightened perceptions of situational threats from the world around us, such as the perceived threat of certain groups (e.g., Swami et al., 2017) and feelings of low socio-political control (e.g., Bruder et al., 2013). Conspiracy theories may be adopted to attempt to manage both types of threats. In our meta-analysis, we adopted this theoretical distinction and sought to determine and compare the absolute effect size magnitudes for internal versus external threats. Since past reviews have indicated strong absolute effect sizes for specific external threats (e.g., dangerous worldview, $r = .39$; Bowes et al., 2023), we expected to find stronger relationships between conspiracy beliefs and external threats than with internal threats. From a theoretical perspective, concerns about the dangers that other individuals and groups might pose may be especially strongly linked to conspiracy theories as they readily point to supposedly dubious and threatening actors that can be accused of engaging in devious plots (see Douglas & Sutton, 2023).

Social Motives

Adopting Brewer and Gardner's (1996) distinction between the three levels of self-definition, Biddlestone and colleagues (2021) argued that frustrated motives concerning the individual self might lead people to adopt conspiracy beliefs in an attempt to enhance their self-image (through processes such as demonstrating their personal uniqueness; e.g., Lantian et al., 2017) and defend their self-image (through processes such as the social rivalry expressed by narcissists; e.g., Cichocka, Marchlewska, & Golec de Zavala, 2016; see also Back et al., 2013). Furthermore, Biddlestone and colleagues argued that unmet motives concerning the relational self (e.g., social exclusion; Poon et al., 2020) might lead people to endorse conspiracy theories in attempts to gain social support and provide a basis on which to share epistemic and ideological concerns (see also Ren et al., 2023). Finally, Biddlestone and

colleagues explained how collective self motives might lead people to adopt conspiracy beliefs in an attempt to defend their group image (e.g., through collective narcissism—a defensive form of ingroup identity characterised by the need for in-group image validation; e.g., Cichocka, Marchlewska, Golec de Zavala, et al., 2016; or by placing the ingroup in a morally superior victim role; e.g., Bilewicz et al., 2013).

Previous meta-analyses have explored some of the self-related motives, for instance linking conspiracy beliefs to individual (*r*s = .22 to .28; Bowes et al., 2023; Stasielowicz, 2022a) and collective (Golec de Zavala et al., 2022, Bowes et al., 2023; both *r*s = .34) narcissism. In our meta-analysis, we go beyond these analyses and systematically test the framework proposed by Biddlestone and colleagues (2021), covering a broader set of social motives, including relational motives not included in previous meta-analyses. Considering relational motives is important because conspiracy beliefs might be shaped by people's connections and relationships with others. For example, there is increasing evidence that if people feel lonely or isolated, engagement with conspiracy theory communities can be viewed as a source of social support (e.g., Bierwiaczonek et al., 2024; Poon et al., 2020). Examining the effects for relational motives also allows us to compare the absolute effect size magnitudes for the individual, relational, and collective selves as the three subtypes identified in the original theorizing on the social self by Brewer and Gardner (1996).

Why Motives?

It is important to note that many psychological variables other than motives have been linked to conspiracy beliefs. These include personality traits (see meta-analyses by Bowes et al., 2023; Goreis & Voracek, 2019; Stasielowicz, 2022a), values (e.g., Jolley et al., 2018), more clinical and subclinical measures of mental health concerns (e.g., Bowes et al., 2023; March & Springer, 2019; Stasielowicz, 2022a), as well as the consequences of conspiracy beliefs (e.g., Jolley & Douglas, 2014a, 2014b; for meta-analyses on the role of conspiracy

beliefs in COVID-19 prevention, see Bierwiaczonek et al., 2021; Stasielowicz, 2022b; for a meta-analysis on the consequences of belief in climate change conspiracy theories, see Biddlestone et al., 2022). In this article, we focus on motives, because other variables, such as normal-range personality traits, play only a negligible role in predicting the endorsement of conspiracy theories (Bowes et al., 2023; Goreis & Voracek, 2019; Stasielowicz, 2022a), and because there are many lines of theory and evidence suggesting that motivated social cognition is a fundamental source of conspiracy beliefs. Aside from the conceptual understanding this perspective can provide us with, it also promises the development of interventions that can reduce the psychological appeal of conspiracy theories by equipping people with the tools and structural circumstances to satisfy their motives more effectively (Cichocka, 2020; Douglas et al., 2015).

Some of the most pervasive and influential factors that shape human perceptions and behavior on a grand scale are those stemming from experiences of deprivation that occur as a consequence of structural inequality. That is, chronic experiences of long-term stress (e.g., Cheadle et al., 2020; Ryan et al., 2021; see also Pat-Horenczyk et al., 2013) and existential frustrations associated with being socially disadvantaged (see Douglas et al., 2017) can negatively impact mental and physical health (e.g., German & Latkin, 2012) and heighten feelings of powerlessness and perceptions of threat (e.g., Ross et al., 2001). The subsequent frustration of these basic needs for security can result in maladaptive psychological responses (Deci & Ryan, 2000; see also Ryan & Deci, 2000). Thus, motivations are largely determined by pervasive systemic threats that prompt individuals to make sense of their circumstances, and this sense-making often takes the form of conspiracy beliefs (see Crocker et al., 1999; Marchlewska et al., 2021; van Prooijen et al., 2018).

Types of Conspiracy Beliefs

Like previous meta-analyses on similar topics (Bowes et al., 2023; Golec de Zavala et al., 2022; Goreiz & Voracek, 2019; Stasielowicz, 2022a; Stojanov & Halberstadt, 2020), we also took into account the possibility that effect size magnitudes may depend on the type of conspiracy beliefs being studied. For example, researchers often measure belief in well-known specific conspiracy theories, such as those surrounding the deaths of President John F. Kennedy or Princess Diana (e.g., Douglas & Sutton, 2008; Swami et al., 2011). Other scales have been constructed to capture belief in more general notions of conspiracies. These measures do not rely on specific events or popular conspiracy theories but rather describe generic beliefs often found in conspiracy theorizing (e.g., secret actions of government agencies; Brotherton et al., 2013). Beliefs that some groups have traits that make them especially likely to conspire (e.g., low warmth and high competence or socio-economic agency; see Fiske et al., 2002; Koch et al., 2016) can also be measured through conspiracy stereotypes (e.g., Kofta et al., 2020). Finally, researchers sometimes measure conspiracy mentality, which can either be viewed as a proxy for the propensity to believe conspiracy theories, regardless of their content or context (Bruder et al., 2013; Frenken & Imhoff, 2021; Imhoff & Bruder, 2014), or a worldview capturing the tendency to interpret events in conspiracist terms (see Imhoff, Bertlich et al., 2022).

Going beyond existing meta-analyses (e.g., Bowes et al., 2023), we argue that it is especially important to differentiate between conspiracy mentality and specific conspiracy beliefs scales. While conspiracy mentality scales focus on a more general rule of conspiracy theorizing (i.e., questioning political authority), and make no problematic claims about specific facts, other scales focus on belief in specific conspiracy theories including those that make epistemically risky claims. The psychological correlates of conspiracy mentality and belief in specific conspiracy theories may therefore sometimes differ (Sutton & Douglas, 2020), even if they are strongly correlated and appear to be roughly equally suited to

profiling individuals as strong versus weak conspiracy adherents (Frenken & Imhoff, 2021). Recent theoretical contributions have thus argued that we need to further clarify the distinction between conspiracy mentality and belief in specific conspiracy theories to tease apart the distinct propositions and political attitudes that underlie them (see Nera, 2024; Trella et al., 2024; Sutton et al., 2024). Some evidence provides support for this view. For example, Stojanov and Halberstadt (2020) found that a lack of control had a positive significant meta-analytic effect on conspiracy beliefs only when belief in specific conspiracy theories (rather than conspiracy mentality) was measured.

Beliefs in specific conspiracy theories can be distinguished not only from conspiracy mentality, but from each other. Sternisko and colleagues (2020), for example, argued that different conspiracy theories might appeal to different social motives. Because conspiracy theories claim that the important events have occurred that are not of public knowledge (Douglas & Sutton, 2023), most, if not all conspiracy theories appear to offer the prospect of possessing secret knowledge. This element provides a particular allure for those seeking to demonstrate their personal uniqueness from others (e.g., Imhoff & Lamberty, 2017; Lantian et al., 2017). However, only some conspiracy theories target specific outgroups. Such theories—best illustrated by conspiracy stereotypes—may be particularly alluring to those seeking to maintain positive group identities (e.g., Cichocka, Marchlewska, Golec de Zavala et al., 2016). For example, Golec de Zavala and colleagues (2022) found that collective narcissism only had a strong significant meta-analytic relationship with conspiracy beliefs about groups. With these considerations in mind, we aimed to synthesize the broad literature to uncover the motives that might drive these different forms of conspiracy beliefs.

The Need to Synthesize Theoretical Claims

The research we have reviewed so far suggests that conspiracy theorizing is attractive to people whose basic psychological motives are frustrated, and more specifically, that the

desire to engage in conspiracy theorizing is strongest when people's epistemic, existential, or social motives are unmet (Douglas et al., 2017; see also Bowes et al., 2023; Douglas et al., 2019; Biddlestone et al., 2021). Although this framework has proven popular in investigating the motivations associated with conspiracy beliefs, to date it has not been analyzed systematically. In this article, we go beyond other meta-analytic reviews and address three broad and interrelated questions.

First, we go beyond identifying a list of the motivational variables associated with conspiracy beliefs by examining which of these *appear to be most important*. To understand this, we start by asking: do the findings support the unmet psychological motives perspective (Douglas et al., 2017) by suggesting that epistemic, existential, and social motives robustly predict conspiracy beliefs? Then, we explore whether the strengths of the absolute meta-analytic effect size magnitudes for these associations are similar across their categorizations. Furthermore, we compare meta-analytic relationships that account for the shared variance between motives versus those that do not at the study level to explore the robustness of the predictive power of each motive.

Second, we examine *what factors strengthen or weaken the relationships* between motivational variables and conspiracy beliefs. We focus especially on the role of sample characteristics, such as the socio-political context in which the data were collected (operationalized as an index of how Western, Educated, Industrialized, Rich, and Democratic, that is WEIRD, the samples were; Henrich et al., 2010). We examine whether relationships are stronger or weaker in experimental (versus correlational) studies. Similarly to previous meta-analyses, we also consider whether the strength of the associations between the motivational variables and conspiracy beliefs depend on the way conspiracy beliefs were measured. Unlike past analyses, we specifically compare conspiracy mentality to other types of conspiracy beliefs.

Third, we consider how robust our theoretical framework is and the strength of the evidence linking each variable to conspiracy beliefs. What is its evidential value: in other words, are there any indications of systematic bias that might reduce confidence in our findings, and how consistent or uniform are these relationships? By addressing these questions, we aim to significantly extend existing work (e.g., Bowes et al., 2023; Stasielowicz, 2022a), generate new avenues for further theory and research, provide evidence-based recommendations for future research methodology, and identify the variables that could be used to most effectively intervene on the formation and fallout of conspiracy beliefs.

Method

Transparency and Openness

We adhered to the PRISMA 2020 guidelines for systematic reviews (Page et al., 2021). All data and research materials (including our coding scheme and details of our search strategy) were pre-registered and are available at the project online repository: https://osf.io/5aw7m/?view_only=360474b4bd404983939f14d716c1b2d8. All data and materials can also be found in the OSF repository. Details on where we deviated from the pre-registered analyses can be found in relevant sections of the analytic strategy.

Eligibility Criteria

We considered studies that (a) included any measure of conspiracy beliefs, and (b) manipulated or measured any type of epistemic, existential, or social motive. Below, we discuss these criteria in detail.

Types of Studies

We included quantitative studies of all designs if they fulfilled the criteria outlined above. We considered study design features (i.e., correlational vs. experimental) as moderators of study effect sizes.

Types of Participants

We included any participants from studies that measured belief in conspiracy theories. As moderators, we considered mean age, sample nationality (to calculate WEIRD scores), and whether the sample consisted exclusively of students.

Selection of Variables

We were interested in the relationships between conspiracy beliefs and epistemic, existential, and social motives. Therefore, we excluded from our sample studies that only dealt with ideological rationalizations, psychological measures of personality types, pathology, cultural orientation, values, and the consequences of conspiracy beliefs. Furthermore, we excluded any studies that only measured susceptibility to misinformation, fake news, or measures of conspiracy beliefs that did not fall under our definition of belief in conspiracy theories.[2]

Search Strategy

We started our search by extracting all empirical articles from the conspiracy theories research database funded by the Centre for Research and Evidence on Security Threats (CREST; Douglas, Sutton, Cichocka et al., 2016). At the time, this was complete until May 2017. We scanned them for potential relevance, obtaining 266 articles (see Figure 2). Next, we screened the meta-data of these articles, excluding any that explicitly did not fit our criteria (e.g., included only qualitative methods). Then, we read the full-texts and coded their inclusion/exclusion criteria, moderators, sample sizes, and effect sizes. Once we checked each other's coding and resolved our disagreements, we proceeded to obtain records from other sources starting from 2017. To do this, we searched the online *PsycINFO, Scopus,*

[2] One example is the belief in aliens, monsters, and conspiracies scale that does not treat belief in conspiracy theories as a distinct construct (Stone et al., 2018).

ProQuest, and *ProQuest Dissertations and Theses*[3] databases, returning 198 additional articles using the broad search term "conspir*." We did not constrain our search with language restrictions. Then, we carried out the same process as we had for the articles obtained from the CREST database. During our systematic review, a meta-analysis was published investigating the effects of a lack of control on conspiracy beliefs (Stojanov & Halberstadt, 2020). Therefore, we looked through the data included in this article, as well as papers cited in our database to check for any additional articles. In total, these searches resulted in 116 potentially relevant articles, but upon more detailed examination, we further excluded nine of them (for justifications of these decisions, please refer to the online repository).

Furthermore, the COVID-19 pandemic began during our systematic review, so we decided to extend our pre-registered deadline to the end of July 2020, enabling us to include articles that were published during the influx of research at the beginning of the pandemic. After carrying out the same process again, we obtained 11 additional pre-prints to be included in our meta-analysis (see Figure 2). Finally, we made a call for papers, contacting the corresponding authors of the papers with unreported data, and included messages in selected journal announcements. Upon completion of this process, we were left with a total of 153 articles, pre-prints or unpublished data, with 279 independent studies and 971 effect sizes to be included in the meta-analysis.

Categorization of Studies

To view our full classifications of independent and dependent variables, please refer to the Supplementary Materials 1 that can be found in the online repository.

[3] We did not carry out a search using Google Scholar or Web of Science, but it was conducted in the creation of the CREST database.

Classification of Different Motives

All predictors were coded into the three categories of motives and their subtypes (see Figure 1). Two authors acted as coders for all studies, but coding decisions were discussed with all authors. Discussions involved refinement of the variable categorizations based on Douglas and colleagues' (2017, 2019) framework. Inter-coder reliability was not calculated because all coding was conducted between the two main coders and any discrepancies were discussed in person.

Potential Moderators

We considered several moderators that could affect the pattern of associations.

Conspiracy Beliefs Measure. The studies relied on several measures capturing different facets of conspiracy beliefs, often differing widely in their conceptual approach. Therefore, although conspiracy beliefs were our overarching dependent variable, we coded the way conspiracy beliefs were measured as one of our pre-registered moderating variables. The most common approach was measuring belief in the shared general characteristics of conspiracy narratives, captured most frequently by the Generic Conspiracist Beliefs Scale (e.g., "New and advanced technology which would harm current industry is being suppressed"; Brotherton et al., 2013). A similar measure capturing general belief in conspiracy theories was the Single Item Conspiracy Belief Scale (i.e., "I think that the official version of the events given by the authorities very often hides the truth"; Lantian et al., 2016). Therefore, when either of these measures were used, the type of belief was coded as "General."

Another broad but distinct measure was the Conspiracy Mentality Questionnaire (CMQ; e.g., "Those at the top do whatever they want"; Bruder et al., 2013; Imhoff & Bruder, 2014). Instead of presenting general characteristics of different types of conspiracy theories, this measure presents items intended to capture the defining components of conspiracist

thinking, theoretically treated as a distinct conspiracist political attitude. Whenever a version of this measure was implemented, data was coded as “Mentality.”

Another common approach to measuring conspiracy beliefs was through the presentation of specific conspiracy theories. Scales used to capture this type of belief were more varied, spanning from the commonly used Belief in Conspiracy Theories Inventory (e.g., “Government agencies in the UK are involved in the distribution of illegal drugs to ethnic minorities”; Swami et al., 2011, 2017) or Conspiracist Belief Scale (Douglas, Sutton, Callan et al., 2016)—both measuring belief in well-known conspiracy theories—to the more idiosyncratic measurement of belief in conspiracy theories regarding specific events, such as the Smoleńsk plane crash (e.g., “Polish and Russian authorities jointly conceal the truth about the catastrophe”; Bilewicz et al., 2019; Cichocka, Marchlewska, Golec de Zavala et al., 2016) or the COVID-19 pandemic (e.g., “The implementation of 5G technology is a means of deliberately spreading Coronavirus”; Biddlestone et al., 2020). Whenever one of these measures was used, they were coded as “Specific conspiracy theories.”

Some studies also measured conspiracy stereotypes of specific groups (e.g., “Do you agree or disagree with the opinion that Jews aspire to dominate the world?”; Kofta & Sedek, 2005), and these were coded as “Stereotypes.” Finally, several studies included combinations of these measures (e.g., assessing “General,” “Specific conspiracy theories,” etc. simultaneously; Imhoff & Lamberty, 2018), in which case they were classified as “Mixed.” We analyzed whether the relationships depended on the type of conspiracy beliefs measure used: General, Specific conspiracy theories, Mentality, Stereotype, or Mixed measures.

Relevance to the COVID-19 Pandemic. In line with recommendations from reviewers, we deviated from our pre-registered moderation analyses by coding for whether the variables included were relevant to the COVID-19 pandemic (vs. not). An example of a relevant independent variable was found in Biddlestone and colleagues’ (2020) study, that

adapted Jolley and Douglas' (2014a) measure of powerlessness around climate change to reference feelings of powerlessness around the COVID-19 pandemic (e.g., "I feel that the Coronavirus is too big for my actions to have an impact"). A relevant dependent variable was found in the study by Imhoff and Lamberty (2020) that measured belief in COVID-19 conspiracy theories (e.g., "The [COVID-19] virus is intentionally presented as dangerous in order to mislead the public").

Study Design. As we included data from both correlational and experimental designs, we checked whether effect sizes varied significantly depending on the design used, as outlined in our pre-registration document. All experimental designs included at least one experimental condition versus the control group, which was either a filler task or no stimulus. No studies used a pre-post experimental design.

Mean Age. Analysis of mean sample age would not necessarily enable us to explore whether age itself plays a role in the relationships analyzed here, but our pre-registered moderator of sample age allowed us to control for whether effect sizes varied based on older or younger samples. For example, the cognitive decline and knowledge accumulation associated with aging may play differential roles in conspiracy belief formation (see Brashier & Schacter, 2020).

WEIRD Index. The vast majority of conspiracy beliefs research has been conducted on samples exclusively comprising participants from WEIRD nations (see Henrich et al., 2010). As a more informative substitution for our pre-registered moderator of nationality, we coded for whether countries were Western or not by following Wojcik and colleagues' (2021) categorization between Western and Eastern Europe as the dividing line between East (0) and West (1). Then, following the approach of Klein and colleagues (2018), we used the United Nations Educational, Scientific and Cultural Organization's (e.g., UNESCO, 2018) education index as our metric for education, the United Nations Industrial Development Organization's

(e.g., UNIDO, 2018) competitive industrial performance index as our metric for industrialization, the United Nations World Economic Situation and Prospects (e.g., WESP, 2018) as our metric for whether nations were economically developed (1) or not (0), and the Economist Intelligence Unit's (e.g., EIU, 2018) democracy index as our democratic metric to create a mean composite WEIRD index for each nation. For published data, these metrics were calculated at year of publication due to practical difficulties associated with identifying the exact year that data was collected, while for unpublished data we used the information on year of data collection provided by the authors. WEIRD metrics for mixed nationality samples were calculated as the mean overall WEIRD index for all nations included in the meta-analyses, and WEIRD indices for known mixtures of nationalities (e.g., UK and EU, or Switzerland and France) were calculated using averaged WEIRD metrics of the relevant nations.

Student Sample. The social psychology literature as a whole relies heavily on the use of student samples. Therefore, we tested whether the use of student samples alters the strength of relationships investigated here by including it as a pre-registered moderator. For example, it may be the case that students are more familiar with the measures used in psychological research, or that they are a particularly homogeneous sample pool.

Publication Status. We pre-registered analyses of possible publication bias, but we also included publication status as a pre-registered moderator to investigate whether there was any preliminary evidence for the file drawer problem.

Overview of Corpus of Studies

The articles collected in the systematic review were published between 1999 and 2020 (note that for unpublished data, we considered the year of collection). Most (69%) were published between 2016 to 2020. Of the total articles, 71% were published in scientific journals, 22% comprised unpublished data, and 7% were student theses. Among the journals

in which the articles were published, researchers most frequently published in the *European Journal of Social Psychology*, which accounted for 7% of the total articles. The journal *Personality and Individual Differences* also represented a substantial share, comprising 5% of the articles.

Analytic Strategy

We adhered to the MARS guidelines for meta-analytic reporting (Appelbaum et al., 2018).

Calculation of Effect Sizes

Fisher's standardized *Z* coefficient was calculated as the main effect size metric to be used in our analyses because its standard error is determined solely by the sample size. This avoids issues associated with comparing the metrics of the correlational Pearsons's *r* and experimental Cohen's *d* and does not risk larger effect sizes appearing more precise due to their standard errors being a function of the magnitude of the effect size (see Harrer et al., 2021). Cohen's *d* effect size coefficients were first converted into Pearson's *r* correlation coefficients with the following formula:

$$\text{ì سي} \frac{\text{Ὼ}}{\text{ИΪΩ· طى τ}}$$

Next, a Fisher's *Z* transformation was performed with the FISHER excel function, and the respective variances of Fisher's *Z* were calculated using the following formula:

$$\text{ώ ώ ώ سي} \frac{\rho}{\text{ϋ طي σ}}$$

When the Fisher's *Z* meta-analytic effect sizes and respective variances were obtained, we back transformed them into their equivalent Pearson's *r* equivalents using the FISHERINV excel function for ease of interpretability. Regarding benchmarks for effect size magnitude, $r = .05$ was treated as very small, $r = .10$ was treated as small, $r = .20$ was treated

as medium, $r = .30$ was treated as large, and $r = .40$ was treated as very large (see Funder & Ozer, 2019).

In some cases, conspiracy beliefs were treated as a median split measure, requiring the conversion of Odds Ratio (*OR*) effect sizes. In these cases, the *OR* was converted into Cohen's *d* with the following formula:

$$d = \frac{\ln(OR)\sqrt{3}}{\pi}$$

In some experimental studies, Cohen's *d* was not reported. In this case, we input the relevant sample sizes, means and standard deviations into Wilson's (n.d.) online calculator (see also Lipsey & Wilson, 2001). If information required for this analysis—such as the sample size of each experimental condition—could not be obtained from the paper or corresponding authors, equal groups were assumed.[4] We also calculated the standard error of each Fisher's *Z* with the following formula:

$$SE = \frac{1}{\sqrt{N - 3}}$$

The standard error of *Z* was also used to calculate the lower and upper bounds of the respective 95% confidence intervals using the following formula:

$$CI_{95\%} = Z \pm 1.96 \times SE$$

[4] While discrepancies in sample sizes between conditions can lead to insensitive estimation of variances, we were able to calculate the average percentage discrepancy in sample sizes between conditions for 18 of the 48 experimental studies included in the meta-analysis (we were unable to calculate the rest due to unreported sample sizes for conditions). The average discrepancy was 3% of the respective total sample size and the largest discrepancy was 10% of the respective total sample size. This suggests that our method of converting Cohen's *d* to Pearson's *r* without taking the specific sample size of each condition into account is unlikely to have significantly influenced the results of our analyses.

When positive meta-analytic effect sizes were obtained, this would indicate that as the respective motivation strengthens (e.g., stronger need for uniqueness), this is accompanied by stronger conspiracy beliefs. A negative meta-analytic effect size in this case would indicate that conspiracy beliefs were higher among participants displaying lower levels of certain motives. We also included separate syntheses of correlational and experimental meta-analytic effect sizes (see ***Main Analyses*** sub-section). For the experimental meta-analytic effect sizes, a positive effect size would indicate that when the respective motive is induced (e.g., heightened perceptions of external threats relative to a condition that reduces perceptions of external threats or a neutral control group), this would cause an increase in susceptibility to conspiracy theories. Negative experimental meta-analytic effects would, in contrast, indicate that as respective motives are triggered, this reduces susceptibility to conspiracy theories relative to a condition that either reduces experiences of this motive or serves as a control group. Importantly, strong evidence for the causal relationships between motives and conspiracy beliefs can only be inferred using more robust designs such as those implemented in experimental methods.

Main Analyses

In our pre-registered analyses, we planned to carry out our main analyses with multiple Robust Variance Estimation (RVE) models using the *robumeta* package (Fisher et al., 2017) in R (R Core Team, 2021). This was because it is common in the conspiracy beliefs literature to include multiple measures of conspiracy beliefs in one sample (resulting in non-independent effect sizes), and RVE accounts for this possibility of multiple effect sizes being dependent on the same sample (*k*; see Fisher & Tipton, 2015).[5] During the development of

[5] Note that this does not suggest that multiple dependent variables were included in our models. Instead, conspiracy beliefs were treated as a single conceptual dependent variable, and—in line with previous meta-analyses on conspiracy beliefs (e.g., Bowes et al., 2023; Goreis & Voracek, 2019; Stasielowicz, 2022a; Stojanov

our manuscript, tools were released for the *metafor* package in R, enabling us to streamline the analyses. Therefore, for each analysis, we created a main multilevel model with individual effect sizes nested within studies as random effects, and we conducted RVE with an imputed covariance matrix for the data.

Unlike most forms of multivariate analysis, analysis of dependent effect sizes requires estimation of the within-study correlations, which we assumed to be similar and consistent (i.e., roughly the same correlation across effect sizes in the same study). Hedges and colleagues (2010) recommend conducting sensitivity analyses to confirm that the approximate conservative $\jmath$ estimate ($\jmath$ = .80) is appropriate. Therefore, when conducting the RVE analyses, within-study correlations of $\jmath$ = .80 were assumed, but sensitivity analyses were conducted by inspecting the variability of effect size magnitude when weaker within-study correlations were tested. If these sensitivity analyses revealed that effect size magnitudes varied depending on the different strengths of within-study correlations, different strengths of within-study correlations were selected (e.g., $\jmath$ = 1). For the sake of brevity, these results will only be presented here if varied effect size magnitudes were detected by the sensitivity analyses.[6]

We deviated from our pre-registered analyses and additionally calculated meta-analytic effect sizes for all psychological motives combined, as well as the main epistemic, existential, and social motives categories by removing the intercept in the main multilevel RVE model and adding each respective level of (a) all psychological motives combined and

& Halberstadt, 2020) and in recent meta-analyses on other topics (Körner et al., 2022)—we later moderated for the type of conspiracy beliefs measured (see the ***Moderators*** sub-section).

[6] Variations between effect size magnitudes were negligible regardless of the level of $\jmath$ assumed in nearly all cases. The one exception was for low self-esteem, which indicated a meta-analytic effect size that became significant when $\jmath$ = .60, r = .10, 95% CI [.01, .19] (main analysis r = .10, 95% CI [-.01, .20]).

(b) the main motive types as predictors. We then computed separate meta-analytic effect sizes for the main motive subtypes in each of the main motive categories (e.g., the internal vs. external threat subtypes in the existential motives category) by removing the intercept in the main multilevel RVE model and including (c) the motive subtypes as predictors, and then for (d) each of the specific motives within each of these subtypes (e.g., anxiety in the internal threats subtype) by adding this level as a predictor. We pre-registered running individual models for each effect size, but we opted to include all data in a single model as this enabled us to control for the variation in all other respective meta-analytic effect sizes in the meta-regression models. Still, individual models for each effect size were constructed to obtain the respective heterogeneity statistics (see ***Heterogeneity and Data Distribution*** section for details) and to compare meta-analytic effect sizes when the shared variance with other motives was not controlled for.[7]

After the raw meta-analytic effect sizes were calculated, we deviated from our pre-registration and included the intercept in the main multilevel RVE model, adding predictors corresponding to pairwise comparisons of (a) each of the main motive type variables, (b) each of the subtype variables, and (c) each of the specific motives to compare the absolute strengths of meta-analytic effect size magnitudes at each level of analysis. Specifically, positive and negative significant predictors would indicate that certain measures predicted conspiracy beliefs more or less strongly than the respective reference category.

Heterogeneity and Data Distribution

In line with our pre-registered analyses, we analyzed heterogeneity statistics. We deviated slightly from our original approach to improve the readability and interpretability of

[7] Note that variables with fewer than 10 effect sizes were grouped together as a general undefined variable due to insufficient power to compare their strengths alone. This approach was also followed in our moderation analyses (see ***Moderators*** sub-section; Supplementary Materials 2).

these findings. Specifically, we reported the variance at the effect size level ($\hat{\tau}^2_{(1)}$) and the study level ($\hat{\tau}^2_{(2)}$). We used these to calculate the standard deviation, $\hat{\tau}$ of the respective meta-analytic effect size on the Fisher's *Z* scale with the following formula:

$$\sqrt{\hat{\tau}^2_{(1)} + \hat{\tau}^2_{(2)}}$$

If $\hat{\tau}$ were greater than the magnitude of their respective Fisher's *Z* meta-analytic effect sizes, we treated this as an indication that the respective meta-analytic effect size magnitude did not accurately represent all individual effect sizes included in the analysis. In these cases, we implemented the residual-based approach to detecting outliers recommended by Negeri and colleagues (2019). Specifically, we calculated the respective *Z* score residuals for each individual Fisher's *Z* effect size included in the respective analysis, and then removed all *Z* score residuals outside of the 95th and 99th percentile of the residual distribution range for comparative re-analysis. We reported any changes in the $\hat{\tau}$ relative to the respective meta-analytic effect sizes or notable differences in the re-analyzed meta-analytic effect sizes compared to the main analysis (e.g., non-significant effect sizes becoming significant).

To obtain a conservative estimate of the potential range of effect sizes in future studies, we calculated 95% critical *t* prediction intervals for each of the respective meta-analytic effect sizes (see IntHout et al., 2016). The specific details for these prediction intervals are reported in the heterogeneity sections of the main analysis tables. Following IntHout and colleagues (2016), we calculated the two-tailed critical *t* with the online *Critical value calculator* (2024), where we input the relevant degrees of freedom (*k* -1) for each meta-analytic effect size and the recommended probability level ($p = .025$; see IntHout et al., 2016). We calculated SD_{pi} using the following formula:

$$\sqrt{SE^2 + \hat{\tau}^2_{(1)} + \hat{\tau}^2_{(2)}}$$

where t^2 is the total variance of the respective Fisher's *Z* meta-analytic effect size, and SE^2 is the squared standard error of the respective Fisher's *Z* meta-analytic effect size. Next, we calculated the lower and upper bounds of the 95% prediction intervals with the following formula:

$$M \pm 1.96 \sqrt{t^2 + SE^2}$$

where *M* is the respective Fisher's *Z* meta-analytic effect size. Finally, we converted the respective upper and lower bounds of the prediction intervals into Pearson's *r* effect size equivalents using the FISHERINV function in Microsoft Excel.

Considering the importance of quantifying and interpreting heterogeneity with regard to the theoretical framework applied, we also deviated from our pre-registered analyses by additionally reporting the Cochrane's *Q* statistic to determine whether significant heterogeneity was present overall (i.e., the total between- and within-study heterogeneity). From this, we also calculated H^2 as a complementary indicator of the presence of heterogeneity comparing the ratio of observed variation with the expected variance due to sampling error that is not impacted by the number of studies (although note it is influenced by the sample sizes of primary studies). In line with recommendations from Higgins and Thompson (2002), $H^2 > 1.5$ was treated as an indication of the presence of heterogeneity.

In our moderation models and multilevel meta-regression models comparing the absolute meta-analytic effect size magnitudes, the Q_E statistic of residual heterogeneity and *F* omnibus statistic of heterogeneity explained by the covariates were used to determine whether respective covariates or moderators could fully account for the presence of heterogeneity, and whether heterogeneity could be fully explained by the lack of empirical validity of the theoretical framework applied (i.e., whether the categorization of motive types and subtypes is in itself a source of significant heterogeneity). Significant *F* statistics would indicate that our categorization of motive types and subtypes may be a source of

heterogeneity, but simultaneously significant Q_E statistics would indicate that controlling for this factor does not fully explain the presence of heterogeneity (i.e., other confounding factors may still be at play).

Moderators

For effect sizes that appear to show substantial or considerable heterogeneity, Wiernik and colleagues (2017) recommend investigating potential moderation effects. Importantly, to ensure appropriate power, moderation analysis and comparison of the absolute meta-analytic effects size magnitudes were only conducted on variables that contained a minimum of 10 effect sizes.[8]

In line with our pre-registered analyses, we tested for several potential moderating effects by adding our moderator variables as simultaneous predictors of the Fisher's *Z* effect size alongside the intercept in respective multilevel meta-regression RVE models with effect sizes nested within studies using the *metafor* package. To analyze our planned moderator variables, mean age of the sample (as pre-registered) and the mean-centered WEIRD index of each nation included in the meta-analysis relative to the WEIRD index for all nations we were able to obtain complete data for (which served as our pre-registered moderator for nationality) were included as continuous variables. Four additional variables, each with two levels, were entered as simultaneous categorical predictors of the effect size: (a) study design

[8] For variables that contained some levels with sufficient power and other levels that did not (e.g., > 10 effect sizes measuring conspiracy mentality but < 10 effect sizes measuring conspiracy stereotypes), categories were combined if this provided sufficient power at the respective level. Otherwise, the whole variable was excluded from these analyses if sufficient power could not be reached by combining the respective levels. Analysis of conspiracy stereotypes and mixed measures were combined in our analysis of the existential motive subtype of external threats, and analysis of belief in general notions of conspiracies, conspiracy mentality, and conspiracy stereotypes were combined in our analysis of the specific social motive of low ingroup identification. Neither of these had significant moderating effects.

(as pre-registered; experimental vs. correlational), (b) whether the sample was comprised exclusively of students (as pre-registered; non-student vs. student), (c) the publication status of the study (note that this substituted our pre-registered moderator of journal: unpublished vs. published), and (d) in line with recommendations from reviewers, we added whether variables were relevant (vs. not relevant) to the COVID-19 pandemic.

Furthermore, the type of conspiracy measure applied (e.g., Specific conspiracy theories, General, Mentality, Stereotypes, and Mixed variants) often contained more than two levels. As previous meta-analyses have not explored whether the relationship between psychological motives and conspiracy beliefs differs for conspiracy mentality versus other measures, we entered conspiracy measure as an additional categorical predictor to the respective multilevel meta-regression models with conspiracy mentality as the reference category. When effect sizes for conspiracy mentality were not available, we used a mix of measures, or alternatively general notions of conspiracies as the reference categories.

To account for the possibility that some of the moderating effects could be an artifact of multicollinearity, analyses of any significant moderation effects were repeated with the significant moderator as a single predictor in its own respective meta-regression model.[9] We only interpreted those significant moderation effects that remained in both the simultaneous and single moderator models. In these instances, we calculated separate meta-analytic effect sizes for each category to provide future researchers with a reference value of the raw magnitude of effect sizes for each relevant measure, excluding all other moderators from the model.

[9] In line with recommendations from reviewers, we also explored whether significant moderation effects of WEIRD index remained similar when WEIRD scores from mixed nationality samples were removed (see Supplementary Materials 2).

Publication Bias

To gain an initial understanding of the potential for presence of publication bias, we analyzed whether publication status moderated the strength of effect sizes in the respective meta-regression moderation models, in line with our pre-registered analyses. This approach is less problematic than other options under instances of high heterogeneity (see Johnson, 2021).

Next, we followed reviewer recommendations and deviated from our pre-registered analyses by additionally using precision-effect testing and precision-effect estimate with standard errors (PET-PEESE). PET was calculated by running comparable multilevel meta-regression RVE models in *metafor* to our main analyses with the standard error of Fisher's *Z* as a predictor, and PEESE was calculated by running similar models with the variance of Fisher's *Z* as a predictor. Evidence of publication bias was detected in these analyses when the PET coefficients were significant. In this case, the publication bias corrected meta-analytic effect size included in the PEESE model is reported. The robustness of PET-PEESE in multilevel meta-analysis has not yet been documented, and it can produce insensitivities under both particularly heterogeneous relationships ($I^2 > 80\%$) and when a small number of studies are included in a given meta-analysis ($k < 10$). Nevertheless, its usage is still recommended alongside other comparable publication bias analyses with differing strengths and weaknesses (see Carter et al., 2019; Stanley, 2017). We report these findings in the online repository (see Supplementary Materials 3).

We also produced contour-enhanced funnel plots that distinguished between published and unpublished effect sizes to investigate potential funnel plot asymmetry using the *metafor* package in R. Following recommendations made by Fernández-Castilla and colleagues (2020) for dependent effect sizes, we produced two sets of contour-enhanced funnel plots for each analysis: one that included all effect sizes, and the other that included

dependent effect sizes that were averaged across their respective samples. These can also be found in the Supplementary Materials 3.

Finally, we deviated from our pre-registered plan and assessed publication bias through *Z* curves (see Brunner & Schimmack, 2016), which provide an estimate of the systematic publication of significant findings and have been known to perform well under instances of high heterogeneity (see Brunner & Schimmack, 2020). These *Z* curves were produced using the *zcurve* package in R with clusters at the study level. *Z* curves indicate potential publication bias when there is a steep drop in effect sizes from the marginally significant to marginally non-significant areas (i.e., either side of the $Z = 1.96$ region).

Under high levels of heterogeneity, analyses of publication bias can be fraught with uncertainty (see Johnson, 2021). Therefore, considering the high levels of heterogeneity we expected to detect throughout the meta-analyses conducted here, the presence of publication bias was only assumed when two or more of our publication bias analyses indicated its presence. Details of these analyses were only reported here when potential publication bias was detected.

Evidential Value

In line with recommendations from reviewers but deviating from our pre-registered analyses, we analyzed the evidential value of our results through the inspection of our *Z* curves. These *Z* curves also provide estimates of the expected replicability rate (see Brunner & Schimmack, 2016, 2020). The *Z* curve approach was originally created to estimate the reproducibility of the work by certain individual scholars, but we used it here to analyze the evidential value of data collected by a collection of researchers, classified instead by the theoretical framework we employ. Most importantly, *Z* curve has an advantage over the *P* curve method (our original pre-registered analysis of evidential value) due to the inclusion of non-significant values. Only Z-Curves indicating problematic metrics (i.e., expected

replicability rates under 80 or higher discovery rates than the respective estimated discovery rates) were interpreted here, the rest are presented in the Supplementary Materials 3.

Results

Sample Characteristics

The average sample age was 32.81 years, min. = 17.80, max. = 52.44, median = 32. There were 41 distinct nations included in the meta-analysis for which WEIRD indices were calculated, $M = 0.71$, $SD = 0.16$ (see Table 1).

Analysis of bivariate correlations between the moderators of mean age of the sample, study design, relevance to the COVID-19 pandemic, student (vs. non-student) samples, and publication status indicated significant correlations between these variables (see Table 2). Specifically, mean age of the sample was significantly higher among correlational (vs. experimental) designs and studies relevant (vs. not relevant) to the COVID-19 pandemic, whereas mean age of the sample was significantly lower among student (vs. non-student) samples and published (vs. unpublished) studies. Furthermore, experimental (vs. correlational) designs were significantly more frequent among student (vs. non-student) samples, and studies that were relevant (vs. not relevant) to the COVID-19 pandemic were significantly more frequent among unpublished (vs. published) studies.

All Psychological Motives Combined and Three Main Motive Types

Main Analyses

In the first series of models, we analyzed the associations between conspiracy beliefs and all psychological motives combined as well as epistemic, existential, and social motives specifically. There was a small-to-medium meta-analytic relationship between motives and conspiracy beliefs overall (see Table 3). Similarly, there were small-to-medium meta-analytic relationships between each of the three main motive types and conspiracy beliefs (see Table 3).

Heterogeneity and Data Distributions

High heterogeneity was present overall and for all main motive types (see Table 3). More specifically, the $\hat{\tau}^2$s were greater than the meta-analytic effect sizes for the overall model and for the main motive types, indicating that the meta-analytic effect sizes did not accurately represent all respective individual effect sizes included in their analyses. Re-analysis of the meta-analytic relationships between conspiracy beliefs and motives overall as well as the main motive types excluding outliers revealed effect sizes of similar magnitude, with $\hat{\tau}^2$s slightly weaker than the respective meta-analytic effect sizes (see Supplementary Materials 4).

Distinguishing between all main motive types alongside the intercept did not explain a significant amount of heterogeneity, $F(2, 68.01) = 0.68$, $p = .510$, suggesting that it would not be appropriate to infer that there are significant differences in the absolute strengths of meta-analytic effect sizes across motive types (see Supplementary Materials 5). Similarly, distinguishing between all motive subtypes alongside the intercept did not explain a significant amount of heterogeneity, $F(6, 43.17) = 1.68$, $p = .149$, also suggesting that it would be inappropriate to infer significant differences between the strengths of these absolute meta-analytic effect sizes (see Supplementary Materials 5). In contrast, distinguishing between all specific motives alongside the intercept explained a significant amount of heterogeneity, $F(26, 15.77) = 4.68$, $p = .001$ (see Supplementary Materials 5). Therefore, although significant heterogeneity remained, $Q_E(944) = 27{,}871.73$, $p < .001$, we interpreted significant differences in the absolute strengths of effect size magnitudes for specific motives throughout the meta-analyses.

Moderators

All Psychological Motives Combined. Full details of the descriptive statistics for each moderator variable in the overall moderation analyses and for the motive types are presented in Table 4.

When moderators were added as predictors alongside the intercept of the overall RVE model, this explained a significant amount of heterogeneity, $F(10, 54.52) = 4.43$, $p < .001$, but significant residual heterogeneity remained, $Q_E(813) = 23879.20$, $p < .001$. In this model, effect size magnitudes were significantly weaker for studies employing experimental (vs. correlational) designs (see Table 5). Nevertheless, effect sizes remained significant when both correlational and experimental meta-analytic effect sizes were calculated separately, with a small-to-medium meta-analytic relationship between psychological motives and conspiracy beliefs for correlational designs and a very small meta-analytic effect for experimental designs (see Table 5; Figure 3).

There was also a significant moderation effect of conspiracy measure in this model, such that effect size magnitudes for conspiracy mentality were significantly weaker than both belief in general notions of conspiracies and conspiracy stereotypes (see Table 5). Nevertheless, the meta-analytic relationships remained significant for all conspiracy measures when calculated separately, with small-to-medium effect sizes for mixed measures, conspiracy mentality, and belief in specific conspiracy theories, a medium effect size for belief in general notions of conspiracies, and a medium-to-large effect size for conspiracy stereotypes (see Table 5; Figure 4).

There was also a significant moderation effect of studies that were relevant (vs. not relevant) to the COVID-19 pandemic, such that effect size magnitudes were significantly weaker among studies that were relevant (vs. not relevant) to the COVID-19 pandemic (see Table 5). Nevertheless, the meta-analytic relationships remained significant for both studies

relevant and not relevant to the COVID-19 pandemic when calculated separately, with a small effect size for studies relevant to the COVID-19 pandemic, and a small-to-medium effect size for studies that were not relevant to the COVID-19 pandemic (see Figure 5).

Finally, there was a significant moderation effect of student (vs. non-student) samples in this model, such that effect size magnitudes were significantly weaker among student (vs. non-student) samples (see Table 5). Nevertheless, meta-analytic effect sizes both remained significant when calculated separately, with a small meta-analytic relationship for student samples, and a small-to-medium meta-analytic relationship for non-student samples (see Figure 6).

There was no significant moderation effect of WEIRD index, but the bubble plot of individual effect sizes with a fitted regression line suggested a negative trend in the strength of effect sizes, with a limited number of effect sizes at the low end of the WEIRD index (see Figure 7). Separate calculations of the meta-analytic effect sizes at the 0 to 33rd, 33rd to 67th, and 67th to 100th percentiles for WEIRD index indicated a small upward trend towards the middle, and a more pronounced downward trend again at the upper end (see Table 5).

The Three Motives. When moderation analysis was conducted on the epistemic motives, inclusion of the moderators explained a significant amount of heterogeneity, $F(10, 14.76) = 4.75$, $p = .004$, but significant heterogeneity remained, $Q_E(310) = 6{,}378.41$, $p < .001$. In this model, effect size magnitudes were significantly weaker among student (vs. non-student) samples (see Table 5). Nevertheless, meta-analytic effect sizes both remained significant when calculated separately, with a small meta-analytic relationship for student samples, and a small-to-medium meta-analytic relationship for non-student samples (see Table 5; Figure 6).

In the model for existential motives, the inclusion of moderators explained a significant amount of heterogeneity, $F(10, 25.03) = 4.15$, $p = .002$, but significant

heterogeneity remained, $Q_E(334) = 9{,}786.61$, $p < .001$. In this model, effect size magnitudes were significantly weaker for experimental (vs. correlational) designs (see Table 5). Nevertheless, both meta-analytic effect sizes remained significant when calculated separately, with a very small meta-analytic effect for experimental designs, and a small-to-medium meta-analytic relationship for correlational designs (see Table 5; Figure 3).

Furthermore, effect size magnitudes were significantly weaker among studies relevant (vs. not relevant) to the COVID-19 pandemic (see Table 5). When meta-analytic effect sizes were calculated separately, the meta-analytic effect size for studies that were not relevant to COVID-19 was small-to-medium and significant, whereas the meta-analytic effect size for studies relevant to COVID-19 was non-significant (see Table 5; Figure 5).[10]

Finally, effect size magnitudes were significantly stronger when conspiracy stereotypes were measured compared to when conspiracy mentality was measured (see Table 5). Nevertheless, meta-analytic effect sizes for all conspiracy measures remained significant when calculated separately, with a very small meta-analytic effect size for a mix of measures, small-to-medium meta-analytic effect sizes for specific conspiracy theories and conspiracy mentality, and medium-to-large meta-analytic effect sizes for general notions of conspiracies and conspiracy stereotypes (see Table 5; Figure 4).

[10] Effect size magnitudes were also significantly weaker among student (vs. non-student) samples in this model (see Table 5). Both meta-analytic effect sizes remained significant when calculated separately, with a small effect size for student samples, and a small-to-medium effect size for non-student samples (see Figure 6). Furthermore, effect size magnitudes were significantly weaker for published (vs. unpublished) studies in this model (see Table 5). Nevertheless, both meta-analytic effect sizes remained significant when calculated separately, with small-to-medium effect sizes for both (see Figure 8). However, when entered as single moderators in their own respective meta-regression models, these moderation effects were no longer significant (see Supplementary Materials 2).

Inclusion of the moderators (i.e., mean age of the sample, study design, conspiracy measure, WEIRD index, student vs. non-student samples, and publication status) as covariates alongside the intercept did not explain a significant amount of heterogeneity for social motives, $F(8, 14.04) = 1.43$, $p = .265$, and significant heterogeneity remained, $Q_E(149) = 3205.88$, $p < .001$.

Publication Bias

There was no consistent evidence of publication bias in the overall model or the main motive types. Observed discovery rates were higher than expected discovery rates on the *Z* curves for all cases, indicating potential systematic publication of significant effect sizes (see Supplementary Materials 3).

Evidential Value

Sufficient power was obtained to detect the respective meta-analytic effect sizes for the overall model and for each motive type. *Z* curve plots indicated that the overall expected replicability rate was below 80, as well as for epistemic motives, existential motives, and social motives (see Supplementary Materials 3).

All Psychological Motives ï Interim Summary

In the first set of models, it was clear that the meta-analytic relationships between all three motives and conspiracy beliefs were small-to-medium, all with similar meta-analytic effect size magnitudes.[11] Nonetheless, the average meta-analytic effect size magnitudes were

[11] In line with a reviewer's suggestion, we identified a total of nine studies and 126 effect sizes in the entire meta-analysis that measured all three main motive types at once, $N_{participants} = 3{,}108$. Meta-analysis of these findings with comparable models to those in our main analyses revealed that descriptively, the strongest effect size was indicated for existential threats, $r = .22$, 95% CI [.15, .28], followed by epistemic, $r = .15$, 95% CI [.09, .22], and social motives, $r = .12$, 95% CI [.02, .22]. Furthermore, comparison of the absolute meta-analytic effect size magnitudes revealed that the effect size for existential motives was significantly stronger than

not precisely representative of the individual effect sizes included in their analyses. All prediction intervals also showed that effect sizes collected in the future could range from negative to positive (see Table 3).

Moderation analyses for all motives combined indicated that the magnitude of effect sizes for correlational designs were significantly stronger than effect sizes for experimental designs. Although subsequent analyses showed a significant moderating effect of design for existential motives only, across all motive types, the magnitudes of experimental effect sizes were roughly half the strength of the correlational effect sizes (in fact, for social motives, we only observed a significant meta-analytic effect size for correlational studies). Thus, weaker average effects sizes across the literature might have been observed if correlational (vs. experimental) findings were not so overrepresented.

Furthermore, it appeared that conspiracy mentality was a significantly weaker correlate of psychological motives, particularly existential motives, than measures of general notions of conspiracies or conspiracy stereotypes. Conspiracy stereotypes indicated particularly strong meta-analytic effect sizes compared to the other conspiracy measures consistently in all models. The meta-analytic effect size for existential motives was only significant when variables were unrelated (vs. related) to COVID-19. Effect sizes for the overall model, epistemic motives, and existential motives were weaker among student (vs. non-student) populations. Moderation effects were also detected for relevance to the COVID-19 pandemic in the overall model.

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epistemic motives, B = -0.07, SE = 0.03, 95% CI [-0.13, -0.01], and marginally stronger than social motives, B = -0.10, SE = 0.04, 95% CI [-0.21, 0.01]. However, these ad hoc findings should be treated with caution due to the very small number of studies included in the analysis, difficulty in comparing the three motives included within each study, and a complete reliance on correlational findings.

Crucially, heterogeneity was high in all models analyzed so far, warranting caution when interpreting how representative of the individual effect size magnitudes each meta-analytic effect size was. Average meta-analytic effect sizes became more representative of the individual effect sizes upon removal of just a few outliers (*k*s = 1 to 6. This uncertainty may also be reduced by more closely examining the motives in their own theoretical domains (see also Bowes et al., 2023). With this in mind, we now turn our investigation deeper, analyzing the meta-analytic relationships between conspiracy beliefs and each of the motive subtypes and the specific motives of which they are comprised. An overview of the effect sizes included in these analyses is presented in Figure 9 (for motive subtypes) and Figure 10 (for specific motives).

Epistemic Motives: Cognitive Styles and Abilities

In the next series of models, we analyzed the meta-analytic associations between conspiracy beliefs and the epistemic motives of both cognitive styles and abilities (see Table 6). Indices of cognitive ability do not strictly reflect epistemic *motives*, but they were included here due to their close overlap with cognitive styles and presumed reflection of reduced motivation to implement reflective thinking styles (see Gjoneska, 2021). We aimed to determine whether belief in conspiracy theories can be partially understood as a reliance on cognitive styles including (a) automatic thinking styles, (b) a need to maintain structure and closure, and (c) compensation for feelings of epistemic uncertainty. Furthermore, we examined whether low cognitive abilities were linked to conspiracy beliefs due to an inability or unwillingness to interpret and comprehend complex information through (a) poor reasoning ability (i.e., poor vocabulary ability, poor numeracy ability), (b) poor memory performance, (c) low general intelligence (i.e., low verbal reasoning or self-reported intelligence), (d) low emotional intelligence, and (e) poor scientific literacy. To view our

detailed classification of measures for the epistemic concerns, see Supplementary Materials 1.

Main Analyses

Small-to-medium positive meta-analytic relationships were indicated for both cognitive styles and low cognitive abilities (see Table 6). Comparison of the absolute meta-analytic effect size magnitudes between the specific cognitive styles and abilities indicated that the effect size for poor reasoning ability was significantly stronger than the effect size for the need for structure and closure (see Supplementary Materials 5).

Specific Cognitive Styles. A non-significant meta-analytic association was indicated for the need for structure and closure, but small-to-medium significant meta-analytic relationships were indicated for automatic thinking styles and epistemic uncertainty (see Table 6).[12] There were no significant differences in the absolute strengths of effect sizes within specific cognitive styles (see Supplement Materials 5).

Specific Cognitive Abilities[13]. A medium-to-large significant meta-analytic association was indicated for poor reasoning ability, and a small-to-medium significant effect size was obtained for low general intelligence. None of the other specific measures of low cognitive abilities had significant meta-analytic relationships with conspiracy beliefs (see Table 6). Comparison of the absolute meta-analytic effect size magnitudes within specific cognitive abilities was not possible due to insufficient power in all indices of cognitive abilities except for poor reasoning ability.

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[12] When the need for structure and closure was analyzed as a single variable in its own model, the meta-analytic effect size was small but significant, r = .09, 95% CI [.04, .15].

[13] The following specific variables were excluded from the subsequent analyses because there were not enough samples: ontological confusion and poor theory of mind.

Heterogeneity and Data Distributions

The presence of high heterogeneity was indicated for both cognitive styles and cognitive abilities (see Table 6). The $\hat{\tau}$ for cognitive styles was equal to the magnitude of the meta-analytic effect size. Re-analysis of the meta-analytic relationship between cognitive styles and conspiracy beliefs with outliers removed revealed an effect size of similar magnitude and a $\hat{\tau}$ that was now slightly weaker than the meta-analytic effect size (see Supplementary Materials 4).

Specific Cognitive Styles. The presence of high heterogeneity was indicated for all three specific cognitive style motives (see Table 6). More specifically, the $\hat{\tau}^2$s were greater than the magnitude of the respective meta-analytic effect sizes for both epistemic uncertainty and the need for structure and closure. Re-analysis of the meta-analytic relationships between conspiracy beliefs and both epistemic uncertainty and the need for structure and closure did not reduce the $\hat{\tau}^2$s relative to their respective meta-analytic effect sizes (see Supplementary Materials 4). Nevertheless, re-analysis of the meta-analytic relationship between conspiracy beliefs and the need for structure and closure with outliers removed revealed that this effect size became small and significant, r = .09, 95% CI [.04, .13], but $\hat{\tau}$ remained greater than the magnitude of the effect size (see Supplementary Materials 4).

Specific Cognitive Abilities. The presence of significantly high heterogeneity was indicated for all of the specific low cognitive abilities except for low emotional intelligence. Furthermore, the respective $\hat{\tau}^2$s were greater than the meta-analytic effect sizes for poor memory performance and low emotional intelligence (see Table 6). No outliers were detected for these variables.

Moderators

Inclusion of moderators alongside the intercept did not explain significant heterogeneity for cognitive styles, $F(9, 18.10) = 2.25$, $p = .068$,[14] or low cognitive abilities, $F(6, 1.01) = 0.64$, $p = .743$,[15] and significant residual heterogeneity remained for both cognitive styles, $Q_E(273) = 5647.59$, $p < .001$, and low cognitive abilities, $Q_E(31) = 515.62$, $p < .001$.

Specific Cognitive Styles. Inclusion of moderators alongside the intercepts for specific cognitive styles did not explain a significant amount of heterogeneity or there was insufficient power to conduct moderation analyses on these variables (see Supplementary Materials 2).

Specific Cognitive Abilities. There was insufficient power to conduct moderation analysis on any of the specific cognitive abilities (see Supplementary Materials 2).

Publication Bias

No consistent evidence of publication bias was detected for the epistemic motive subtypes.

Specific Cognitive Styles. Despite a relatively symmetrical contour-enhanced funnel plot and a non-significant PET statistic, the PEESE statistic for automatic thinking styles was significant and the observed discovery rate was higher than the expected discovery rate in its *Z* curve (see Supplementary Materials 3). The PEESE publication bias corrected effect size

[14] As there was only one effect size measuring conspiracy stereotypes in this analysis (see Table 7), it was collapsed into the mix of measures category.

[15] As there were less than ten effect sizes measuring belief in general notions of conspiracy theories, conspiracy mentality, and a mix of measures (see Table 7), these were collapsed into the mix of measures category. Note that in particular, analysis of the experimental meta-analytic effect of cognitive abilities was not possible as there was only one experimental study in this subtype.

for automatic thinking styles remained significant, r = .15, 95% CI [.12, .18], but was slightly weaker than the one obtained in the main analysis (r = .19, 95% CI [.15, .23]). Furthermore, despite a relatively symmetrical contour-enhanced funnel plot and an observed discovery rate that was lower than the estimated discovery rate in the need for structure and closure Z-Curve, the PET statistic was significant (see Supplementary Materials 3). The PEESE statistic for the need for structure and closure was non-significant (see Supplementary Materials 3), but the publication bias corrected effect size became significant, r = .14, 95% CI [.06, .21]; for comparison, in the main analysis, r = .05, 95% CI [-.07, .17].

Specific Cognitive Abilities. The PET and PEESE statistics for low emotional intelligence were significant and the contour-enhanced funnel plot was asymmetrical (see Supplementary Materials 3). Furthermore, the publication bias corrected effect size for low emotional intelligence became negative and significant, r = -.80, 95% CI [-.80, -.80], and was notably stronger than the one obtained in the main analysis (r = .04, 95% CI [-.66, .70]). Still, these findings should be treated with caution as the Z-Curve did not converge due to the small number of studies included in the analysis, k = 2 (see Supplementary Materials 3 and 6).

Evidential Value

Sufficient power was achieved to detect the meta-analytic effect sizes for both cognitive styles and cognitive abilities, but Z-Curve plots indicated that the expected replicability rate was below 80 for both (see Supplementary Materials 3).

Specific Cognitive Styles. Sufficient power was achieved to detect the meta-analytic effect sizes for all specific cognitive styles. Z-Curve plots indicated that the expected replicability rates were below 80 for automatic thinking styles and epistemic uncertainty (see Supplementary Materials 3).

Specific Cognitive Abilities. Post-hoc power analysis indicated that insufficient power of .41 was achieved to detect the meta-analytic effect size for poor memory performance, and insufficient power of .11 was achieved to detect the meta-analytic effect size for low emotional intelligence.

Epistemic Motives ï Interim Summary

In this section, we analyzed the association of epistemic motives with conspiracy beliefs. We detected average meta-analytic relationships that were of similar magnitude for both cognitive styles and cognitive abilities overall. These effect sizes were small-to-medium and heterogeneity was high. All prediction intervals also indicated that future effect sizes could range from negative to positive (see Table 6). When the specific epistemic motives were analyzed separately, relatively precise representation of the individual effect sizes by the respective average meta-analytic effect sizes could only be obtained for the automatic thinking styles, poor reasoning ability, and low general intelligence. Furthermore, there was no evidence for moderation by study design, which suggested no significant differences for the effect sizes of both correlational and experimental designs for the cognitive styles subtype (but note that cognitive abilities as well as for the specific cognitive style motive of the need for structure and closure, there was no data with experimental evidence).

Despite significant effect sizes for automatic thinking styles and epistemic uncertainty from the cognitive styles, and both poor reasoning ability and low general intelligence from the cognitive ability measures, particularly unrepresentative average meta-analytic effect sizes of the individual effect sizes were indicated for the need for structure and closure and epistemic uncertainty from the cognitive styles, and poor memory performance and low emotional intelligence from the cognitive abilities. This may be explained by insufficient power to detect the meta-analytic effect sizes for poor memory performance and low emotional intelligence. Some inconsistent evidence of publication bias was also detected for

automatic thinking styles and the need for structure and closure. Thus, we urge caution when interpreting the accuracy of our findings for all epistemic motives.

Interestingly, the meta-analytic relationship for the need for structure and closure became significant when outliers were removed and when publication bias was corrected for, suggesting that the meta-analytic effect size in the main analysis may have been non-significant due to the systematic publication of non-significant effect sizes. Evidence of publication bias was also indicated for automatic thinking styles and low emotional intelligence, and the evidential value for the meta-analytic effect sizes of both epistemic motive subtypes and the specific motives of automatic thinking styles, epistemic uncertainty, poor memory performance, and low emotional intelligence was unclear.

Existential Motives: Internal and External Threats

In the next series of models, we analyzed the meta-analytic associations of existential motives with conspiracy beliefs (see Table 8). To create subtypes of existential threats, we drew from Onraet and colleagues' (2013) distinction between *internal* (i.e., ongoing mental distress) or *external* (i.e., situational threats, or subjective perceptions of external threat) sources of existential threat. Specifically, we used this distinction to determine whether endorsement of conspiracy theories may be related to attempts to boost feelings of security about the self as a result of internal threats such as (a) state-trait anxiety, (b) attachment anxieties, (c) attachment avoidance, (d) cognitive looming, (e) low personal control, (f) insecurity, (g) stress, or (h) vulnerability (see Douglas et al., 2017, 2019; van Prooijen, 2020). Additionally, we used this perspective to examine whether conspiracy beliefs are related to attempts to increase feelings of security in response to perceived external threats, characterized by (a) political alienation, (b) anomie/anomia, (c) competitive worldview, (d) dangerous worldview, (e) cynicism, (f) deprivation, (g) disillusionment, (h) low external control, (i) powerlessness, (j) external threat perceptions, (k) perceived risk, (l) system

justification, or (m) existential uncertainty. It is important to note that in this section we looked at political alienation and anomie/anomia in the context of socio-political systems. In this, we conceptualize political alienation as a sense of estrangement from society and confusion about existing values and norms (e.g., Citrin et al., 1975). It is captured for instance by Dean's (1961) normlessness sub-scale of this political alienation measure (while the powerlessness subscale was considered an indication of powerlessness). We considered anomie/anomia as perceptions of a breakdown of society as a whole (measured for example with Srole's 1956 or Goertzel's 1994 scales).[16] To view additional details of our classification of measures for the existential threats, see Supplementary Materials 1.

Main Analyses

A small significant positive meta-analytic relationship was indicated for internal threats, and a medium significant positive meta-analytic relationship was indicated for external threats (see Table 8). Comparison of the absolute meta-analytic effect size magnitudes across specific internal and external existential motives indicated that the effect sizes for the specific external threats of political alienation, dangerous worldview, anomie/anomia, and powerlessness were significantly stronger than the effect sizes for the specific internal threats of anxiety, avoidant attachment, and low personal control (see Supplementary Materials 5). Furthermore, the meta-analytic effect sizes for the specific external threats of deprivation, external threat perceptions, and system justification were significantly weaker than the effect size for the specific internal threat of disillusionment, the meta-analytic effect size for the specific external threat of low external control was significantly stronger than the effect sizes for the specific internal threats of avoidant

[16] Note that scales or subscales measuring political alienation and anomie/anomia which explicitly referred to isolation from other people were considered under the relational self in the social motives section.

attachment, disillusionment, and low personal control, and the meta-analytic effect size for the specific external threat of perceived risk was significantly weaker than the effect sizes for the specific internal threats of anxiety, anxious attachment, and disillusionment (see Supplementary Materials 5).

Specific Internal Threats.[17] Medium-to-large significant positive meta-analytic relationships were indicated for vulnerability and anxious attachment, a small-to-medium meta-analytic association was indicated for anxiety, and a small positive meta-analytic relationship for avoidant attachment (see Table 8). There were no significant meta-analytic relationships for insecurity, cognitive looming, low personal control, or stress.[18] Comparison of the absolute meta-analytic effect size magnitudes revealed that the meta-analytic effect size for anxiety was significantly weaker than the effect size for disillusionment, and significantly stronger than the effect sizes for avoidant attachment and low personal control (see Supplementary Materials 5). Furthermore, the meta-analytic effect size for low personal control was significantly weaker than the effect size for anxious attachment, the meta-analytic effect size for anxious attachment was significantly stronger than the effect size for avoidant attachment, and the meta-analytic effect size for avoidant attachment was significantly weaker than the effect size for disillusionment (see Supplementary Materials 5).

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[17] Some specific motives were excluded from the subsequent analyses because there were not enough samples. These were: daily hassles, egocentric threat bias, escapism, externality of attributions, hopelessness, life purpose, loneliness, low optimism, pain catastrophizing, stress catastrophizing, low sense of power, and certain measures of insecure attachment.

[18] When low personal control was entered as a single variable in its own model, the meta-analytic relationship was very small but significant, r = .06, 95% CI [.03, .08] (main analysis r = .04, 95% CI [-.03, .11]). Furthermore, meta-analysis of cognitive looming without the implementation of RVE revealed a significant meta-analytic effect size which was considerably stronger than the one obtained in the main analysis, r = 28, 95% CI [.03, .51] (main analysis r = .15, 95% CI [-.14, .42]).

Specific External Threats.[19] A very large significant meta-analytic relationship was indicated for dangerous worldview, a large-to-very large effect size was indicated for anomie/anomia, medium-to-large effect sizes were indicated for disillusionment, political alienation, and powerlessness, a medium effect size was indicated for low external control, and small-to-medium effect sizes were indicated for cynicism, external threat perceptions, and deprivation (see Table 8). In contrast, we detected no significant meta-analytic associations for competitive worldview, existential uncertainty, system justification, or perceived risk. Comparison of the absolute meta-analytic effect size magnitudes indicated that the meta-analytic effect size for political alienation was significantly stronger than the effect sizes for deprivation, perceived risk, and system justification (see Supplementary Materials 5). Furthermore, the meta-analytic effect size for anomie/anomia was significantly stronger than the effect sizes for deprivation, external threat perceptions, low external control, perceived risk, powerlessness, and system justification (see Supplementary Materials 5). The meta-analytic effect size for dangerous worldview was also significantly stronger than the effect sizes for deprivation, external threat perceptions, low external control, perceived risk, and system justification (see Supplementary Materials 5). Finally, the meta-analytic effect size for powerlessness was significantly stronger than the effect sizes for deprivation, perceived risk, and system justification, and the meta-analytic effect size for low external control was significantly stronger than the effect size for perceived risk (see Supplementary Materials 5).

[19] Some specific motives were excluded from the subsequent analyses because there were not enough samples. These were: external anxiety, belief in an unjust world, low benevolence, negative life events, sense-making, and negative economic sentiment.

Heterogeneity and Data Distributions

High significant heterogeneity was indicated for both internal and external threats (see Table 8). More specifically, the τ^2s were equal to or greater than the magnitude of effect sizes for both internal and external threats, indicating that the average meta-analytic effect size magnitudes were not particularly representative of the respective individual effect sizes included in their analyses. Re-analysis of the meta-analytic relationship between external threats and conspiracy beliefs with outliers removed revealed a τ^2 weaker than the magnitude of the effect size, which remained similar to the one obtained in the main analysis (see Supplementary Materials 4). Re-analysis of the meta-analytic relationship between internal threats and conspiracy beliefs without outliers only reduced the τ^2 relative to the magnitude of the effect size when outliers above the more parsimonious 95th percentile were removed, with a meta-analytic effect size similar to the one obtained in the main analysis (see Supplementary Materials 4).

Specific Internal Threats. High significant heterogeneity was indicated for all of the specific internal threats except for vulnerability and stress (see Table 8). Furthermore, the τ^2s for cognitive looming and low personal control were greater than the magnitudes of their respective meta-analytic effect sizes. There were no outliers detected for cognitive looming, and re-analysis of the meta-analytic relationship between low personal control and conspiracy beliefs without outliers did not reduce the τ^2 relative to the magnitude of its effect size (see Supplementary Materials 4). Nevertheless, the effect size for low personal control without outliers became very small but significant, r = .06, 95% CI [.03, .09] (see Supplementary Materials 4).

Specific External Threats. High significant heterogeneity was indicated for all specific external threats except for competitive worldview and cynicism (see Table 8). More specifically, τ^2s were greater than the magnitudes of the respective meta-analytic effect sizes

for existential uncertainty, external threat perceptions, system justification, and perceived risk. There were no outliers for existential uncertainty, and re-analysis of the relationships between conspiracy beliefs and external threat perceptions, system justification, and perceived risk with the outliers removed did not reduce their ῦ relative to the magnitudes of their respective meta-analytic effect sizes (see Supplementary Materials 4).

Moderators

Full details of the descriptive statistics for each moderator variable in the moderation analyses for internal and external threats are presented in Table 9.

Inclusion of moderators alongside the intercept for internal threats did not explain a significant amount of heterogeneity, $F(9, 9.67) = 1.24$, $p = .370$, and significant residual heterogeneity remained, $Q_E(122) = 2834.59$, $p < .001$. In contrast, inclusion of moderators alongside the intercept for external threats explained a significant amount of heterogeneity, $F(9, 7.97) = 4.10$, $p = .030$, but significant heterogeneity remained, $Q_E(203) = 5189.99$, $p < .001$.[20] In this model, effect size magnitudes were significantly weaker for studies that were relevant (vs. not relevant) to the COVID-19 pandemic, $B = -0.24$, $SE = 0.04$, 95% CI [-0.33, -0.15]. When calculated separately, the meta-analytic effect size for studies that were relevant to the COVID-19 pandemic was non-significant, $r = .02$, 95% CI [-.06, .11], and the effect size for studies not relevant to the COVID-19 pandemic was medium and significant, $r = .20$, 95% CI [.14, .25] (see Figure 11).

Specific Internal Threats. There was not enough power to conduct moderation analysis on any of the specific internal threats relationships (see Supplementary Materials 2).

Specific External Threats. Inclusion of moderators alongside any of the intercepts for the specific external threats either did not explain a significant amount of heterogeneity or

[20] As there were fewer than 10 effect sizes for conspiracy stereotypes and a mix of measures in this model (see Table 9), these two variables were collapsed into a mix of measures variable.

there was insufficient power to conduct moderation analysis on these variables (see Supplementary Materials 2).

Publication Bias

No consistent evidence of publication bias was detected for the existential motive subtypes of internal or external threats. Observed discovery rates were higher than the expected discovery rates in their respective Z-Curves (see Supplementary Materials 3), indicating potential systematic publication of significant findings.

Internal Threats. No consistent evidence of publication bias was detected for any of the specific internal threats. The Z-Curves for anxiety and low personal control indicated observed discovery rates that were higher than their expected discovery rates (see Supplementary Materials 3), suggesting potential systematic publication of significant findings.

External Threats. Evidence of publication bias was indicated for disillusionment through significant PET and PEESE statistics (see Supplementary Materials 3), with a publication bias corrected effect size that remained significant, r = .17, 95% CI [.17, .17], but became notably weaker than the one obtained in the main analysis (r = .29, 95% CI [.19, .40]). Furthermore, the Z-Curve for disillusionment indicated a higher observed discovery rate than the expected discovery rate, and the contour-enhanced funnel plots displayed an underrepresentation of effect sizes in the non-significant region (see Supplementary Materials 3). Still, these findings should be treated with caution due to the small number of studies included in the analysis, k = 2.

The *Z* curve for deprivation also indicated an observed discovery rate that was higher than the expected discovery rate, and the contour-enhanced funnel plot with dependent effect sizes collapsed across their respective studies revealed an underrepresentation of effect sizes in the non-significant region (see Supplementary Materials 3), suggesting potential

systematic publication of significant findings. Finally, the Z curve for system justification indicated an observed discovery rate that was higher than the expected discovery rate, and the contour-enhanced funnel plots revealed that despite many significant effect sizes in both the positive and negative regions, there was an underrepresentation of effect sizes in the non-significant region (see Supplementary Materials 3). Therefore, potential systematic publication of significant findings was also indicated for system justification.

Evidential Value

Sufficient power was achieved to detect the meta-analytic effect sizes for both internal and external threats, but Z curve plots indicated that the expected replicability rate was below 80 for internal threats (see Supplementary Materials 3).

Specific Internal Threats. Post-hoc power analysis indicated that insufficient power of .23 was achieved to detect the meta-analytic effect size for stress. Furthermore, Z curve plots indicated that the expected replicability rates were below 80 for anxiety and low personal control (see Supplementary Materials 3).

Specific External Threats. Post-hoc power analysis indicated that insufficient power of .54 was achieved to detect the meta-analytic effect size for perceived risk. Furthermore, the Z curve indicated that the expected replicability rate was below 80 for perceived risk (see Supplementary Materials 3).

Existential Motives ï Interim Summary

In the second motive subtypes section, we analyzed the internal and external existential threats associated with conspiracy beliefs. The average meta-analytic effect size we observed was small for internal threats, and medium for external threats. High heterogeneity was indicated for both internal and external threats, indicating that these average effect sizes may not represent the respective individual effect sizes included in their analyses well. Comparison of the absolute effect size magnitudes indicated that the effect

sizes for specific external threats were typically stronger than those for specific internal threats.

A more representative average meta-analytic effect size of the individual effect sizes was achieved for internal threats when outliers were removed. Unlike all other prediction intervals for internal threats—which ranged from negative to positive—the prediction interval for vulnerability indicated that future effect sizes were *only* likely to be positive. Also, the average meta-analytic effect size for low personal control became significant when it was entered as a single variable in its own model. This suggests that the meta-analytic relationship between low personal control and conspiracy beliefs may share an overlap in variance with other psychological motives. Interestingly, the effect size for low external control was significantly stronger than the effect size for low personal control. Therefore, it may be a lack of control over one's socio-political reality, rather than everyday life, that is associated with susceptibility to conspiracy theories. Interestingly, we observed a medium-to-large effect size for anxious attachment, with its absolute meta-analytic effect size being significantly stronger than the effect size for avoidant attachment.

With respect to external threats, we observed a very large average meta-analytic effect size for dangerous worldview, and large average meta-analytic effect sizes for political alienation, anomie/anomia, and disillusionment. Unlike all other prediction intervals for external threats—which ranged from negative to positive—the prediction intervals for political alienation, anomie/anomia, and low external control indicated that future effect sizes were *only* likely to be positive (see Table 8). Comparison of the absolute meta-analytic effect size magnitudes revealed that dangerous worldview, anomie/anomia, and political alienation are significantly stronger correlates of conspiracy beliefs than many of the other external threats (see also Bowes et al., 2023). Evidence of publication bias was also discovered for the specific external threats of disillusionment, deprivation, and system justification.

External threats only appeared to be associated with conspiracy beliefs in studies measuring variables unrelated (vs. related) to COVID-19. Inspection of specific effect sizes indicated that they were limited to studies by Imhoff and Lamberty (2020) who measured perceptions of COVID-19 as threatening and found that it was generally negatively or not significantly associated with belief in conspiracy theories claiming that the pandemic was a hoax or that the virus was deliberately manufactured. This work may not have captured the nuanced threat responses to COVID-19 that contrastingly lead to both behavioural inhibition or more active distal threat responses (see Jutzi et al., 2020). Finally, experimental designs were only included in a very small amount of the specific motives included here. Therefore, we warrant caution when interpreting the results for internal and external threats as evidence of their causal effect on conspiracy beliefs.

Social Motives: Individual, Relational, and Collective Selves

In the final series of models, we analyzed the meta-analytic associations between variables related to the individual, relational, and collective selves with conspiracy beliefs (see Table 10). To categorize the subtypes of self-related motives, we used Biddlestone and colleagues' (2021) framework based on Brewer and Gardner's (1996) distinction between the three levels of self-definition with reference to conspiracy beliefs. For the *individual* self, we aimed to determine whether conspiracy theories are endorsed in attempts to enhance the self-image (captured by the need for uniqueness, narcissistic grandiosity, or responses to low self-esteem), or defend the self-image (captured by narcissistic defensiveness and psychological reactance). Thus, we examined separate relationships for (a) low self-esteem, (b) individual narcissism, (c)need for uniqueness, and (d) reactance. For the *relational* self, we examined whether conspiracy beliefs can be adopted to compensate for the negative experience of social exclusion. Finally, for the *collective* self, we analyzed whether conspiracy beliefs can be a response to a negative view of the ingroup through (a) low ingroup identification, or the

need to defend the group image and position, expressed through a (b) defensive ingroup identity (e.g., collective narcissism), or (c) perceived ingroup victimhood. To view our classification of measures for the three selves, see Supplementary Materials 1.

Main Analyses

A medium positive meta-analytic relationship was indicated for the individual self motives, and small-to-medium positive meta-analytic associations were indicated for the relational and collective selves motives (see Table 10). Comparison of the absolute meta-analytic effect size magnitudes across the three social motives indicated that the meta-analytic effect size for low ingroup identification was significantly weaker than the effect sizes for low self-esteem, individual narcissism, the need for uniqueness, reactance, and social exclusion (see Supplementary Materials 5).

Specific Individual Self Motives.[21] Medium-to-large positive meta-analytic relationships were indicated for reactance and individual narcissism, a small-to-medium effect size was indicated for the need for uniqueness, and a non-significant meta-analytic association was indicated for low self-esteem (see Table 10).[22] Comparison of the absolute meta-analytic effect size magnitudes within the individual self motives indicated that the effect size for low self-esteem was significantly weaker than individual narcissism and reactance (see Supplementary Materials 5).

[21] Dominance, knowledge overestimation, and prestige were excluded from the subsequent analyses because there were not enough samples.

[22] The meta-analytic effect size for low self-esteem became significant in a model where it was entered as a single predictor, r = .08, 95% CI [.02, .13].

Specific Relational Self Motives.[23] A non-significant meta-analytic relationship was indicated for social exclusion.

Specific Collective Self Motives.[24] A medium meta-analytic relationship was indicated for defensive ingroup identity (see Table 10). The meta-analytic effect sizes for both perceived ingroup victimhood and low ingroup identification were not significant. Comparison of the absolute meta-analytic effect size magnitudes within the collective self motives revealed that the effect size for low ingroup identification was significantly weaker than the effect size for defensive ingroup identity (see Supplementary Materials 5).

Heterogeneity and Data Distributions

High significant heterogeneity was indicated for the individual, relational, and collective selves (see Table 10). Furthermore, $\acute{U}$s were greater than the magnitude of the respective meta-analytic effect sizes for both the relational and collective selves. Re-analysis of the meta-analytic relationship between the collective self and conspiracy beliefs with outliers removed did not notably alter the $\acute{U}$relative to the magnitude of the meta-analytic effect size (see Supplementary Materials 4). Re-analysis of the meta-analytic relationship for the relational self with outliers removed at the more parsimonious 95th percentile revealed a weaker $\acute{U}$relative to the magnitude of its meta-analytic effect size (see Supplementary Materials 4). Furthermore, the effect size for the relational self with outliers removed remained significant and became slightly stronger than the one obtained in the main analysis, r = .17, 95% CI [.07, .27] (main analysis r = .16, 95% CI [-.02, .33]).

[23] Social value refutation and leadership were excluded from the subsequent analyses because there were not enough samples.

[24] Low humanity-esteem, low perceived ingroup/outgroup overlap, social competition, and social devaluation were excluded from the subsequent analyses because there were not enough samples.

Specific Individual Self Motives. Significant heterogeneity was indicated for all four of the specific individual self motives (see Table 10). Yet, $\hat{U}$s were all weaker than the respective meta-analytic effect sizes, and in the cases of individual narcissism and reactance, the $\hat{U}$s were considerably weaker than their respective meta-analytic effect sizes.

Specific Relational Self Motives. High significant heterogeneity was indicated for social exclusion (see Table 10), with a $\hat{U}$greater than the magnitude of its meta-analytic effect size. Re-analysis of the meta-analytic relationship between conspiracy beliefs and social exclusion with outliers removed at the more parsimonious 95th percentile level revealed that the $\hat{U}$was no longer greater than the magnitude of its meta-analytic effect size, which became significant and medium in magnitude, r = .21, 95% CI [.11, .32] (see Supplementary Materials 4).

Specific Collective Self Motives. High significant heterogeneity was indicated for all three of the specific collective self motives (see Table 10). Furthermore, the $\hat{U}$was greater than the magnitude of the meta-analytic effect size for low ingroup identification. This remained the case when this meta-analytic relationship was re-analyzed with outliers removed (see Supplementary Materials 4).

Moderators

Inclusion of moderators alongside the intercept either did not explain a significant amount of heterogeneity or resulted in insufficient power to conduct moderation analyses (see Supplementary Materials 2). In fact, some of the moderation analyses were impossible due to no data available in certain cells (see Table 11).

Publication Bias

There was no consistent evidence of publication bias for any of the three social motive subtypes (see Supplementary Materials 3). The observed discovery rates were higher

than the expected discovery rates in each of the respective Z curves (see Supplementary Materials 3), indicating potential systematic publication of significant findings.

Specific Individual Self Motives. Evidence of publication bias was indicated for the need for uniqueness through a significant PET statistic and an absence of published effect sizes in the non-significant region of the contour-enhanced funnel plot when dependent effect sizes were averaged across their respective samples (see Supplementary Materials 3). Furthermore, the observed discovery rate was higher than the expected discovery rate in the need for uniqueness Z curve (see Supplementary Materials 3), and the publication bias corrected effect size became non-significant, r = .01, 95% CI [-.15, .17], which, descriptively, was notably weaker than the one obtained in the main analysis (r = .18, 95% CI [.06, .29]). Furthermore, the Z curves for individual narcissism and low self-esteem indicated observed discovery rates that were higher than the expected discovery rates (see Supplementary Materials 3), suggesting potential systematic publication of significant findings.

Specific Relational Self Motives. There was no consistent evidence of publication bias for social exclusion. The observed discovery rate was higher than the expected discovery rate in the Z curve (see Supplementary Materials 3), indicating potential systematic publication of significant findings.

Specific Collective Self Motives. Evidence of publication bias was detected for perceived ingroup victimhood through significant PET and PEESE statistics (see Supplementary Materials 3). Nevertheless, the corrected publication bias effect size remained non-significant, r = .01, 95% CI [-.49, .50], but was descriptively notably weaker than the one obtained in the main analysis (r = .27, 95% CI [-.41, .76]). Still, this finding should be treated with caution due to the small number of studies included in the analysis, k=3. Finally, the Z curves for low ingroup identification and defensive ingroup identity indicated observed

discovery rates that were higher than the expected discovery rates (see Supplementary Materials 3).

Evidential Value

Sufficient power was achieved to detect the meta-analytic effect sizes for the individual, relational, and collective selves. Z curves indicated that the expected replicability rate was below 80 for both the relational and collective selves (see Supplementary Materials 3).

Individual Self. Sufficient power was achieved to detect the meta-analytic effect sizes for all individual self motives. Z curve plots indicated that the replicability rate was below 80 for low self-esteem (see Supplementary Materials 3).

Relational Self. Sufficient power was achieved to detect the meta-analytic effect size for social exclusion, but the Z curve indicated that the replicability rate was below 80 (see Supplementary Materials 3).

Collective Self. Sufficient power was achieved to detect the meta-analytic effect sizes for all specific collective self motives. The Z curves indicated that the expected replicability rates were below 80 for both defensive ingroup identity and low ingroup identification (see Supplementary Materials 3).

Social Motives ï Interim Summary

Analysis of social motives indicated significant medium or small-to-medium average meta-analytic effect sizes for all three levels of self-definition: individual, relational, and collective. Heterogeneity was especially high for the latter two. The absolute meta-analytic effect sizes for low self-esteem (an individual self variable) and low ingroup identification (a collective self variable) were both non-significant and the weakest social motives associated with conspiracy beliefs (see also Cichocka, Marchlewska, & Golec de Zavala, 2016; Cichocka, Marchlewska, Golec de Zavala et al., 2016). The relationships were much clearer

for individual narcissism, reactance, and defensive ingroup identity. Unlike all other prediction intervals for social motives—which ranged from negative to positive—the prediction interval for reactance indicated that future effect sizes were *only* likely to be positive (see Table 10). No moderation effects were detected, this was partially due to low power, with many of the moderation tests lacking relevant data. For example, in our sample, there were no experimental studies for the collective self motives, and no studies in the individual or relational self motives examined conspiracy stereotypes. This points to gaps in the literature, which we hope can be addressed in the future.

The effect size for social exclusion was non-significant, but it became medium and significant when removing outliers. We suggest that the small number of studies included in this analysis, coupled with results from interaction studies included from the literature (van Prooijen, 2016) may have contributed to particularly high heterogeneity for social exclusion. Similarly, the average meta-analytic effect size became more representative of the individual effect sizes when outliers were removed for the relational self motives subtype more generally. Finally, the evidential value for defensive ingroup identity was unclear, and evidence of publication bias was detected for the need for uniqueness and perceived ingroup victimhood. In both cases of publication bias, the publication bias corrected effect sizes became or remained non-significant. Therefore, we urge researchers to publish more findings on the links between social motives and conspiracy beliefs.

Discussion

In this article, we set out to synthesize observed associations between conspiracy beliefs and unsatisfied psychological motives. Our specific focus was on the epistemic, existential, and social motives that have been associated empirically and theoretically with conspiracy beliefs (Douglas et al., 2017). We aimed to extend our understanding of the comparative strengths of the absolute meta-analytic effect size magnitudes for the

relationships between conspiracy beliefs and unsatisfied motives, uncover the methodological and theoretical parameters that affect the strengths of these relationships, and evaluate the evidential value of the relevant findings in the literature thus far. Our findings were largely consistent with the central tenets of Douglas and colleagues' (2017, 2019) framework of conspiracy beliefs as attempts to compensate for thwarted psychological motives.

Overall, our findings demonstrated that psychological motives have a small but significant meta-analytic relationship with conspiracy beliefs. The magnitudes of effect sizes were similar for epistemic, existential, and social motives. With respect to key moderators, we found overall that the effect sizes of correlational studies were stronger than those of experiments. This was especially clear in the analyses of existential motives. We found little overall evidence for effect sizes varying with the socio-cultural context, with no moderating effects of sample "WEIRDness." Finally, we also found general evidence for somewhat weaker effect sizes when conspiracy mentality was used as an indicator of conspiracy beliefs compared to conspiracy stereotypes or general notions of conspiracies. However, the absolute meta-analytic effect size magnitude of conspiracy mentality was comparable to that of the effect size magnitude for belief in specific conspiracy theories (see Frenken & Imhoff, 2021; Nera, 2024; Sutton & Douglas, 2020; Sutton et al., 2024; Trella et al., 2024). It is important to note that high heterogeneity was indicated for all motives. We now zoom in on the specific motive types.

Epistemic Motives

We found conspiracy beliefs to be overall weakly but significantly associated with epistemic motives, but evidential value was uncertain around the majority of specific epistemic motives. The most conclusive evidence was found for a reliance on automatic thinking styles of the cognitive styles, and poor reasoning ability from the low cognitive

abilities subtype. This corroborates past work pointing to comparable meta-analytic associations between conspiracy beliefs and less reflective or intuitive thinking (see Bowes et al., 2023; Yelbuz et al., 2023). Less conclusive evidence was provided for the link between conspiracy beliefs and the other cognitive styles of epistemic uncertainty and motives to achieve structure and closure. Our meta-analysis of epistemic uncertainty is the first in the literature and points to a need to collect more data on this association. The statistical uncertainty around the role of the need for structure and closure in conspiracist reasoning echoes Bowes and colleagues' (2023) results of a similarly small meta-analytic effect size. Specifically, it appears that this relationship is weak and significant when the shared variance with other motives is controlled for. Furthermore, high heterogeneity for need for closure is consistent with other evidence suggesting that the needs for structure and closure only make conspiracy explanations appealing under certain specific circumstances (e.g., Marchlewska et al., 2017). Overall, our findings support the notion that a reliance on automatic thinking styles over the careful deliberation of evidence are particularly important epistemic correlates of conspiracist reasoning (see Gjoneska, 2021).

We also confirmed that conspiracy beliefs are linked to low cognitive abilities. However, fairly clear meta-analytic relationships were only uncovered between conspiracy beliefs and both poor reasoning ability and low general intelligence (see also Bowes et al., 2023; Gjoneska, 2021). These findings resonate with the argument that the inherent epistemic riskiness of conspiracy theories (Douglas & Sutton, 2023) makes them seem more plausible to people who have poor cognitive capacity to evaluate their plausibility.

Existential Motives

We also explored the degree to which conspiracy beliefs are linked to experiences of existential threat and insecurity. We found a medium effect size for external threats and a slightly weaker effect size for internal threats. Furthermore, meta-analytic effect sizes for

many of the specific external threats were significantly stronger than many of those for internal threats. These findings offer support for the hypothesis that conspiracy theories may thrive in times of societal crisis (see van Prooijen & Douglas, 2017). Nevertheless, we found that the meta-analytic relationship between external threats and conspiracy beliefs was only significant for correlational, but not experimental studies. We argue that more evidence may be required to clarify the experimental effect of external threats on conspiracy beliefs. Furthermore, these findings may resonate with work suggesting that the relationship between certain existential motives and conspiracy beliefs is bidirectional (see Liekefett et al., 2021). Our correlational findings do not allow for inference of the causal direction of relationships, meaning that the effects could run in both directions between motives and conspiracy beliefs. Thus, it is at least plausible that some of the effects we observed might be due to conspiracy beliefs increasing perceptions of external threats.

Importantly, the raw effect sizes for the external threats of dangerous worldview, anomie/anomia, disillusionment, political alienation, and powerlessness were especially strong. Weaker, medium strength relationships were obtained for low external control, and small-to-medium effect sizes were observed for cynicism, external threat perceptions, and deprivation. Overall, these findings showed that abstract feelings of malicious plots and motives related to finding a secure place in society (captured by closely related constructs of political alienation, anomie and powerlessness; Seeman, 1959; see also Kofta et al., 2020 as well as dangerous worldviews) are the clearest and most consistent predictors for the endorsement of conspiracy theories.

The findings for political alienation diverge from the meta-analysis of Bowes and colleagues (2023) who did not find a significant relationship for what they termed as alienation (although the effect size estimate was very similar to the one we observed) . Importantly, Bowes and colleagues (2023) did not differentiate between the different ways in

which alienation can be operationalized (e.g., Seeman, 1959). In contrast to our analysis, their analysis did not differentiate a sense of estrangement from society and confusion about existing values and norms (which we considered to be the core of alienation and, thus, categorized it as political alienation specifically under external existential motives), from alienation understood as isolation from significant others specifically (which we considered under relational motives; see also Dean, 1961).

There were clear indications that the general meta-analytic associations for perceived risk and system justification might not be reliable. Findings regarding the relationship between conspiracy beliefs and system justification have been especially inconsistent, and synthesized here for the first time, indicate that the average overall meta-analytic effect size was close to zero (e.g., Crocker et al., 1999; Davis et al., 2018; Jolley et al., 2018; Kofta & Soral, 2020; Pellegrini et al., 2019). Our results show that conspiracy beliefs should not be seen in gross, unqualified terms as being motivated by satisfaction or dissatisfaction with the status quo. Furthermore, high heterogeneity observed for system justification suggests that the average meta-analytic effect size does not accurately represent the individual effect sizes included in the analysis. It is possible that relationships for system justification may depend interactively on the contents of specific conspiracy theories in certain sociopolitical contexts. For example, conspiracy theories about NATO orchestrating the 2022 Russian invasion of Ukraine may be positively associated with justification of the status quo in Russia, but negatively associated with system justification in Ukraine. Research testing such hypotheses is relatively scarce (see Federico, 2022).

Our analysis revealed robust medium-to-large relationships between conspiracy beliefs and certain internal threats: vulnerability and anxious attachment. In contrast, the effect size for anxiety was relatively consistent but considerably weaker, and effect sizes for cognitive looming, low personal control, and stress were non-significant. These findings may

demonstrate that when observing the ongoing personal distress factors that predispose individuals to conspiracy beliefs, experiences of long-term relational and situational anxieties might play a stronger role than immediate personal stressors and concerns. Our discovery of a significantly stronger effect size for anxious attachment compared to avoidant attachment also helps to settle discrepancies in the literature, which diverged in pointing to anxious (e.g., Green & Douglas, 2018) or avoidant attachment (e.g., Leone et al., 2018) as the better predictor of conspiracy beliefs. Nevertheless, the heterogeneity observed for both attachment styles suggest their effects might be qualified by other factors.

Furthermore, we found no support for the idea that conspiracy beliefs were linked to low personal control. This finding diverges from the findings of Bowes and colleagues (2023) who reported significant, though small, effect sizes for control. This could be because previous meta-analyses (see Bowes et al., 2023; Stojanov & Halberstadt, 2020) did not differentiate low *external* control from low *personal* control. In fact, it was only when low personal control was analysed as a single predictor in its own model that the meta-analytic association was significant (yet very small). Nevertheless, our analyses comparing the absolute strengths of different effect size magnitudes indicated that the meta-analytic effect size for low external control was significantly stronger than for low personal control, suggesting that it may be the lack of control over one's socio-political reality, rather than everyday life, that is linked to finding conspiracy narratives appealing (see also Kofta et al., 2020).

Social Motives

In our final set of models, we were able to go beyond other meta-analyses and test the full framework of self-related motives proposed by Biddlestone and colleagues (2021), which differentiated individual, relational and collective self motives. We confirmed that conspiracy beliefs are associated with all three self-related motives. The effect sizes for the individual

self were generally strongest, especially for reactance and individual narcissism (but not self-esteem), corroborating past theoretical accounts (Biddlestone et al., 2021; Cichocka, Marchlewska, & Golec de Zavala, 2016) as well as previous meta-analyses from Bowes et al. (2023) and Stasielowicz (2022a). The relationship between the need for uniqueness and conspiracy beliefs was small-to-medium and significant, but correction for evidence of publication bias indicated that this relationship may in fact be non-significant. This appears unlikely considering recent evidence that publicly endorsing conspiracy theories does in fact make one appear more unique to others (Green et al., 2023). Nevertheless, it may be the case that the link between need for uniqueness and conspiracy beliefs is conditional on, or partially explained by, other variables such as individual narcissism (see Cichocka et al., 2022; Kay, 2021). With respect to self-esteem, we found that its meta-analytic effect size became significant when entered as a single predictor in its own model. This is consistent with existing evidence showing that it is the defensive self-esteem component of individual narcissism sharing variance with low self-esteem that predicts conspiracy beliefs rather than low self-esteem itself (see Cichocka, Marchlewska, & Golec de Zavala, 2016).

Although the overall meta-analytic effect size for the relational self was significant, social exclusion had a non-significant meta-analytic relationship with conspiracy beliefs. This meta-analytical relationship became significant and notably larger when outliers were removed, suggesting that the effect size magnitude in the main analysis may be underestimated. The outlier removed corresponded to van Prooijen's (2016) measurement of the interaction between social exclusion and uncertainty, implying that the straightforward relationship between social exclusion and conspiracy beliefs is indeed medium and significant once this interaction effect is excluded. Thus, we present the first meta-analytic evidence supporting the notion that conspiracy beliefs are linked to needs to manage one's relationships with others (see Biddlestone et al., 2021).

For collective self motives, analyses indicated that there was a medium relationship for defensive ingroup identity, but less evidence for the role of perceived ingroup victimhood or ingroup identification (e.g., Cichocka, Marchlewska, Golec de Zavala et al., 2016). These results are consistent with meta-analytic evidence suggesting that collective narcissism, a typical operationalisation of defensive identity, is linked to various forms of belief in conspiracy theories (Golec de Zavala et al., 2022, Bowes et al., 2023). It appears that viewing one's group as chronically undermined and deserving of privileged treatment seems to be robustly associated with conspiracy beliefs, although the evidential value of this predictor was unclear. More work is needed to examine to the role of collective self-motives, especially with the implementation of experimental methods. For example, a recent set of experiments by Bertin (2024) suggests that some of the associations observed between conspiracy beliefs and collective victimhood might be explained by intergroup conflicts (rather than conspiracy beliefs per se) increasing collective victimhood beliefs.

Evaluation of Effect Sizes

Evidential Value

Analysis of evidential value through the inspection of Z curves revealed that the expected replicability rates were below 80 for eight of the 37 motives analyzed, calling the evidential value of the relevant findings into question. Importantly, this may have been due to our aggregation of higher order categories between the motives in some cases, with only low expected replicability rates for automatic thinking styles, epistemic uncertainty, anxiety, low personal control, perceived risk, low self-esteem, social exclusion, and low ingroup identification from the specific motives. Furthermore, a sufficient power of .80 was achieved to detect the meta-analytic effect sizes in most cases, but insufficient power was achieved to detect the meta-analytic effect sizes for poor memory performance, low emotional intelligence, stress, and perceived risk. Therefore, to gain a clearer understanding of their

evidential value, we encourage researchers to conduct more studies with larger sample sizes examining the relationships between these variables and conspiracy beliefs.

Heterogeneity

There was generally high effect size heterogeneity throughout the meta-analysis, but the most concern should be raised with regard to the high τ^2 standard deviations of meta-analytic effect sizes. Although this was improved by the removal of outliers in many instances, we recommend caution when analyzing these findings to avoid overinterpreting potentially "fictitious" averages (see Greenland & O'Rourke, 2008; Light et al., 1984). Distinguishing between the specific motives (but not motive types or subtypes; see Supplementary Materials 5) often explained a significant amount of the heterogeneity, but residual heterogeneity remained for many motives. Around 90% of the prediction intervals also indicated that future effect sizes could range from negative to positive. Exceptions indicating only positive effect sizes were found for the internal existential threat of vulnerability, external existential threats of political alienation, anomie/anomia, and low external control, and the social individual self motive of reactance. This suggests that the presence of many average meta-analytic effect sizes that were not precisely representative of the individual effect sizes included in their analyses was likely influenced by other confounding or moderating factors that have yet to be considered in existing meta-analyses.

In our correlational analysis between moderator variables, mean age of the sample appeared to correlate with many of the other moderators. Therefore, future work could focus on exploring the potential confounding effects of age on the relationships outlined here. Another potential unexplored variable may be the proportion of voters for certain political candidates and parties in each sample, which could determine the strengths of some of the meta-analytic relationships presented here. For example, evidence suggests that certain conspiracy theories are appealing depending on the political ideology and partisanship of

participants (e.g., Enders et al., 2022), with conspiracy beliefs more pronounced among right-wingers and those holding extreme ideologies (Imhoff, Zimmer et al., 2022). Imhoff, Zimmer and colleagues' (2022) work on the interaction between ideology and political control deprivation suggests that the meta-analytic relationship between external existential threats and conspiracy beliefs may be stronger among right-wing extremists than those holding other ideologies. Still, many of the meta-analytic effect sizes obtained with high heterogeneity were comparable in magnitude to those reported in other syntheses (e.g., Bowes et al., 2023; Stasielowicz, 2022a).

We found some evidence that the effect sizes for studies conducted on beliefs in COVID-19 conspiracy theories were reliably different from the ones observed for other types of conspiracy theories. Nevertheless, our meta-analysis only included studies from the first few months of the pandemic and more work is needed to systematically analyze the new literature.

Study Design

With regard to study design, there was a distinct scarcity of experimental designs throughout the meta-analysis (for instance, none for the collective self; see Table 11). Furthermore, we detected significantly weaker meta-analytic effect sizes for studies employing experimental (vs. correlational) designs when analyzing the overall model (see also Stojanov & Halberstadt, 2020). Despite the experimental meta-analytic effect remaining significant, caution is warranted when interpreting our findings as evidence that psychological motives *increase* conspiracy beliefs, and instead our findings more decisively confirm associations between these constructs. Our investigation provides clear evidence that conspiracy beliefs are linked to unsatisfied motives, but more experimental work is needed to clarify the causal direction of these processes. Examining confounding variables might also clarify the nature of these correlational relationships.

It is also plausible that certain motives have bidirectional associations with conspiracy beliefs. Such relationships can be studied with longitudinal designs. For example, Liekefett and colleagues (2021) provided longitudinal evidence that conspiracy beliefs not only stem from anxiety, uncertainty aversion, and experiences of existential threat, but that conspiracy beliefs in turn heightened these same experiences over time (in line with the theorizing by Douglas et al., 2017). Overall, more research with longitudinal designs is needed in this field (see Bierwiaczonek et al., 2020; Jolley et al., 2021 for examples).

Publication Bias

Strong evidence of publication bias was rare throughout the meta-analysis. However, some of the methods used to detect these potential biases (e.g., PET-PEESE) have been criticized when heterogeneity is high, such as in our analyses (see Johnson, 2021). Despite over a tenth of the studies (and a third of the effect sizes) analyzed here coming from unpublished work, we again recommend a push for further publication of the grey literature analyzing the relationships between psychological motives and conspiracy beliefs, especially studies that rely on internally and externally valid methods, including experimental and longitudinal designs which remain scarce in this field.

Implications, Limitations, and Future Research

The rapid spread of COVID-19 misinformation—dubbed the infodemic (see Bradd, 2020)—galvanized work on the potential consequences of conspiracy theories. These studies paint a concerning picture, showing, for example, that conspiracy beliefs predict support for violence (Jolley & Paterson, 2020), and lower willingness to engage in prosocial behaviors such as reducing one's carbon footprint or accepting the COVID-19 vaccines (see Biddlestone et al., 2020, 2022; Bertin et al., 2020; see Douglas, 2021 for a review). We should note that these findings are mostly correlational and do not imply causality, nor do they account for other potentially important factors. Longitudinal research also points to the

potentially harmful consequences of conspiracy theorizing during the COVID-19 pandemic (Bierwiaczonek et al., 2020). So, it appears that in order to mitigate the fallout of issues like the infodemic, particular emphasis could be placed on improving the conditions of citizens to reduce widespread abstract suspicions of malicious plots and feelings of societal estrangement and confusion.

Threats from the world around us are likely to be particularly poignant to people experiencing anxieties about society and their role within it. This notion is in line with theorizing offered by van Prooijen (2020), who argued that these existential threats trigger sense-making processes, possibly characterized by a reliance on automatic thinking styles, which we found were linked to conspiracy beliefs. Furthermore, van Prooijen (2020) posits that these risk factors are most likely to take the form of conspiracy beliefs when social processes guide the selection of an antagonistic outgroup. This theorizing is also consistent with our results showing the relatively strong effect sizes for conspiracy stereotypes overall, and that conspiracy beliefs are particularly strongly related to a need to defend the image of oneself and the groups that one belongs to.

Kruglanski and colleagues (2022) have further specified the ways in which the various motives directed people towards the endorsement of conspiracy theories. They explain the process of the three Ns: unmet *Needs* turn individuals towards certain social *Networks* that define the routes through which said needs can supposedly be met by supporting a given *Narrative***.** In this way, conspiracy theories are used to defend individuals' sense of significance and circulated among networks, culminating in a "community of radicalization" (see Vegetti & Littvay, 2021). They suggest that this process can be countered by offering alternative, pro-social narratives and networks that are equally if not more able to meet people's desire for significance.

The foregoing discussion highlights the possible value of the motives perspective in pointing to remedies: it suggests that satisfying people's unmet needs may not only help them directly but also help counter problematic conspiracy narratives. The challenge is that this might require policymakers to consider how political and economic structures affect people's psychological functioning and address societal issues in a way that considers psychological motives (e.g,, Cichocka, 2020). For example, there is evidence that high (vs. low) income inequality enhances feelings of anomie, which in turn increases the appeal of conspiracy narratives (Salvador-Casara et al., 2022).

Another potential way to reduce conspiracy beliefs is to equip people with psychological buffers that protect individuals against the appeal of conspiracy narratives. One promising example of this approach was presented by Poon and colleagues (2020), who showed that strengthening the self-image through induced feelings of competence and morality (self-affirmation; e.g., Sherman & Cohen, 2006) mitigated the effect of social exclusion on conspiracy beliefs. Other work has demonstrated how psychological inoculation can be used to confer resistance against feelings of anxiety (Jackson et al., 2017) and certain cognitive biases (Biddlestone et al., 2022; Vraga et al., 2019).

The current meta-analysis tested the robustness of the motivational framework of conspiracy beliefs (see Douglas et al., 2017, 2019). It explored a broader set of psychological motives than previous meta-analyses, which focused on fewer motivational variables but included other predictors such as ideological attitudes (e.g., right-wing authoritarianism and social dominance orientation), personality traits, or psychopathological characteristics (Bowes et al., 2023; Stasielowicz, 2022a). The relationships we observed in the current meta-analysis show that the motives account provides a better explanation for conspiracy beliefs than focusing on normal range personality traits, which tend to have generally weak

associations with conspiracy beliefs (Bowes et al., 2023; Goreis & Voracek, 2019).[25] There seems to be more evidence for the importance of psychopathological characteristics, with effect sizes for traits such as schizotypy, psychoticism, and paranoia showing strong effect sizes (Bowes et al., 2023; Stasielowicz, 2022a) comparable to those we observed for certain external existential threats.

Our meta-analysis also benefits from the inclusion of a greater number of cognitive abilities than previous meta-analyses (e.g., Bowes et al.'s, 2023 inclusion of intelligence; Stasielowicz's, 2022a inclusion of cognitive ability) within the epistemic motives framework. This allows us to more accurately compare the importance of these abilities with cognitive styles and other factors that motivate goal-directed epistemic activity. Like others (e.g., Douglas et al., 2017; Gjoneska, 2021), we have argued that a comprehensive understanding of how epistemic motives relate to conspiracy beliefs requires the consideration of cognitive abilities, which may constrain and frustrate the pursuit of epistemic goals. It is important to acknowledge that cognitive abilities do not necessarily provide the same direct motivational impetus as other motives such as aversion to uncertainty. Thus, we include some cognitive abilities in our analysis that are seldom conceptualized as a motive (e.g., verbal reasoning), although others are often conceptualized in more motivational terms (e.g., low emotional intelligence: see Joseph & Newman, 2010).[26] Since the absolute meta-analytic effect sizes of

[25] Although note that there might be some crossover between variables that measure personality traits and motives included this meta-analysis (e.g., vulnerability as a component of neuroticism; Costa & McCrae, 1992), reflecting difficulty in reliably differentiating traits from needs and goals (see for example Dennisen & Penke, 2008).

[26] Competing perspectives conceptualize emotional intelligence as either overlapping with cognitive abilities (e.g., Mayer et al., 2000) or as a series of subdimensions with a cascading set of motivational processes (e.g., Joseph & Newman, 2010). In the current meta-analysis, we decided to categorize this variable as a single

cognitive abilities are of similar magnitude to cognitive styles, their inclusion in the present meta-analysis is unlikely to have inflated the overall meta-analytic estimates of the importance of epistemic motives. As Gjoneska (2021) has argued, future research would profit from examining the causal interplay between cognitive abilities, subjective frustration of epistemic motives, and conspiracy belief.

Future research is also required to clarify an emerging debate on the relative importance of deliberation versus the motives that classically feature in the social and personality psychology literature (e.g., self-enhancement and cognitive consistency motivations). Pennycook (2023) has argued that epistemically unwarranted beliefs, including conspiracy beliefs, could be parsimoniously explained by sheer lack of deliberation. Still, there are questions about the construct validity of measures of deliberation (Patel et al., 2019), and findings are mixed. For example, deliberation has been found to be less effective in predicting susceptibility to misinformation than other constructs (Roozenbeek et al., 2023). In a meta-analysis, partisanship was also found to nullify interventions designed to improve deliberation (Rathje et al., 2022; cf: Martel et al., 2024). In the present study, less deliberative thinking styles were relatively robust predictors of conspiracy beliefs. Importantly, Pennycook and colleagues (2022) demonstrated how variables such as overconfidence may relate distally to epistemically unwarranted beliefs by reducing the degree to which people engage in deliberation. This logic points the way to obtaining a stronger evidence base to

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cognitive ability due to its treatment as a single factor in the studies included and the majority of scale items referring to abilities and skills rather than motivational processes (see Petrides, 2009; Wong & Law, 2002). Others may argue that distinguishing between the subdimensions of emotional intelligence (e.g., emotional stability, emotional regulation) may have been more appropriate, and we urge future meta-analytic work to further examine these nuances.

determine whether deliberation interacts with, mediates, or out-competes other epistemic motives as a predictor of conspiracy belief.

Future work could also incorporate meta-analytic factor analyses to determine the most appropriate categorization schemes for the variables we considered. It might also consider testing interactions between variables measured in separate studies. It could be the case that some motives are linked to conspiracy beliefs only under specific circumstances. For example, Marchlewska and colleagues (2017) provided evidence that the epistemic need for cognitive closure is only linked to conspiracy beliefs under circumstances of uncertainty, making conspiracy explanations more cognitively accessible (see also van Prooijen, 2016). Therefore, future research should focus on uncovering interaction effects between the different motives analyzed here, particularly the ones with weak effect sizes and high heterogeneity.

Additionally, the majority of participants resided in countries that were high—rather than low—in WEIRD scores. Although we observed no evidence for the moderating role of WEIRDness, more data is needed to determine the universality of the relationships observed here, particularly with regard to non-WEIRD populations. Furthermore, due to constraints in the availability of continuous measures for all WEIRD indices, our metric of Richness (i.e., developed vs. developing nations) was ultimately a dichotomous variable. We also used a dichotomous categorization of Western vs. non-Western countries. This introduces some insensitivities when capturing the nuances between different levels of these variables. That is, our analyses might have underestimated the influence of WEIRD scores on the strengths of relationships due to a lack of specificity in capturing these two dichotomous indices.

Finally, our statistical comparison of the absolute meta-analytic effect size magnitudes provided an initial platform for future work to uncover a hierarchy of motivations. Still, there are statistical limitations to the approach we used. Most importantly,

meta-analysis of study-level absolute effect sizes does not allow for a fine-grained understanding of the participant-level shared variance between the specific variables in question. Therefore, future efforts could focus on implementing more robust methods, such as meta-analytic structural equation modelling, to more closely estimate the true relative strengths of relationships between certain motives and conspiracy beliefs (see Cheung, 2015).

Conclusions

The meta-analytical findings presented here largely support the argument that conspiracy beliefs might be an attempt to cope with unsatisfied epistemic, existential, and social motives (Douglas et al., 2017; 2019). Specifically, we found that conspiracy beliefs were associated with cognitive styles that rely on intuition, poor reasoning ability, existential threats (particularly from the world around us and in society), alongside efforts to defend the self-image and the image of the ingroup. Researchers and policymakers could therefore focus on satisfying these motives when designing interventions aimed at tackling problematic conspiracy beliefs.

As argued by Robert K. Merton, “The very same society that produces this sense of alienation and estrangement generates in many a craving for reassurance, an acute need to believe, a flight into faith” (1946; p. 143). Our findings from the comprehensive meta-analytic synthesis presented here suggest that similar motivational processes likely occur in the adoption of conspiracy beliefs. Specifically, unequal or systematically oppressive societal conditions are likely to thwart basic psychological needs for security, certainty, and social belonging. Indications of these psychological experiences may take the form of feelings of political alienation, poorer reasoning abilities, or a lack of social support. In attempts to compensate for these unmet needs, people may engage in epistemic processes that are either conducive or detrimental to managing these needs. One such detrimental process is the “flight into faith” required in the adoption of conspiracy beliefs, which may occur directly or

through a version of the motivational processes outlined here. We hope this work can serve as a basis on which scholars can develop tools to better protect us against complex societal challenges.

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Asterisks indicate reports with at least one qualifying study for the meta-analysis.

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