Do stress, depression and anxiety lead to beliefs in conspiracy theories over time?

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Abstract

Prior research has found positive correlations between various indicators of psychological distress such as anxiety, depression and stress, and belief in conspiracy theories. However, whether these relationships reflect causal effects remains unclear. In this preregistered longitudinal study, we tested whether anxiety, depression, and stress affect – and are affected by – belief in unwarranted conspiracy theories. Participants (N = 970) from Australia, New Zealand, and the United Kingdom completed seven monthly online surveys between October 2022 and March 2023. Using a multiple indicator random intercept cross-lagged panel model (RI-CLPM), we found support for only one of 15 preregistered hypotheses: a small within-person cross-lagged effect of anxiety increasing belief in conspiracy theories. Conversely, we found no evidence that belief in conspiracy theories increases psychological distress over time. These findings align with other longitudinal studies, suggesting that any reciprocal relationship between psychological distress and conspiracy beliefs is relatively small. This raises questions about the role of distress and existential threat as primary contributors to belief in conspiracy theories.

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Introduction

The goal of the current research is to better understand how anxiety, stress, and depression both affect, and are affected by, conspiracy beliefs. The following review provides an overview of important existing research and theory relevant to this goal. This introductory chapter consists of five sections. First, it begins by defining conspiracy theories, explaining the conceptual differences between neutral and loaded definitions, and clarifying the focus on unwarranted conspiracy theories. Second, it discusses approaches for measuring conspiracy beliefs, contrasting belief in specific conspiracy theories with general conspiracy mentality. Third, the chapter briefly reviews negative outcomes linked with conspiracy beliefs. Fourth, it introduces psychological explanations for conspiracy beliefs, summarising Douglas et al.'s (2017) social, epistemic, and existential motives, and presents van Prooijen's (2020) existential threat model as a key theoretical framework. Finally, the chapter reviews empirical evidence on the relationship between anxiety, depression, stress, and conspiracy beliefs, identifying the gaps that the present longitudinal study aims to address.

Conspiracy Theories and Conspiracy Theory Beliefs

What is a Conspiracy Theory?

A conspiracy theory is an explanation of an event or observation as the result of a conspiracy – multiple actors secretly plotting to do something harmful or unlawful (Swami et al., 2016). The key features of a conspiracy theory include multiple actors working together (Swami & Furnham, 2014), having malevolent intentions (i.e., to cause harm; van Prooijen, 2018), and the protagonists working in secret (Keeley, 1999).

While this definition is one of several, it is preferred because it does not imply that all conspiracy theories are inherently irrational or false. By highlighting core features, such as multiple actors, malevolent intent, and secrecy (Keeley, 1999; Swami & Furnham, 2014; van Prooijen, 2018), it includes essential aspects common across conspiracy theories. An important strength of this definition is that it does not presume the plausibility or irrationality of conspiracy theories, consistent with philosophical positions that advocate for careful consideration of the content of each conspiracy (Dentith, 2018; Pigden, 1995).

Definitions of conspiracy theories vary considerably within the academic field, particularly regarding whether the definition itself implies irrationality or implausibility (see Coady, 2018). Some researchers prefer a loaded definition, implying that all conspiracy theories are epistemically problematic and irrational by definition (Cassam, 2023). Advocates of a loaded definition argue that it aligns more closely with the common, everyday use of the term ‘conspiracy theory’ which typically has a pejorative connotation and public scepticism toward such beliefs (Duetz, 2023; Napolitano & Reuter, 2021). In contrast, the neutral definition adopted here avoids assuming that conspiracy theories must be false by definition. Historically, conspiracies have occurred, and the definition recognises that while conspiracy theories can involve unwarranted claims, questioning authority or seeking alternative explanations can be a rational response in certain circumstances (Dentith, 2018). For example, the Watergate scandal revealed a conspiracy theory that was later substantiated, leading to the impeachment of President Nixon (Woodward & Bernstein, 2007).

Carefully evaluating the rationality of and evidence for each conspiracy theory (Buenting & Taylor, 2010; Pigden, 1995) is particularly important for this study, which is specifically interested in beliefs in unwarranted conspiracy theories – those empirically unsupported and clearly contradicted by evidence (Keeley, 1999).

Why Focus on Unwarranted Conspiracy Theories?

Although conspiracy theories are not inherently false, what we, as psychological researchers, are specifically interested in understanding are unwarranted conspiracy theories. Unlike warranted conspiracy theories (e.g., Watergate), which require no special psychological explanation, unwarranted conspiracy theories appear to persist, despite overwhelming evidence against their validity. These beliefs suggest that psychological factors, other than rational evaluation, may strongly influence these beliefs (Douglas et al., 2017).

While focusing specifically on unwarranted conspiracy theories is useful, it introduces some challenges. Disentangling unwarranted conspiracy theories from those that are warranted requires careful consideration of the available empirical support, a task that inevitably involves a degree of judgement (see Hagen, 2022). However, what this does allow is a focus on what we are particularly interested in – understanding why people believe in conspiracy theories that appear to be overwhelmingly unsupported by empirical evidence. That said, restricting the research to conspiracy theories that lack empirical support allows for the findings to relate to the psychological process that might underpin such beliefs. This conceptual decision also guided the selection of psychometric measures used in this study (discussed further in the Methods section).

Measures of Conspiracy Belief

Two general types of conspiracy measures are commonly used: belief in specific conspiracy theories, and a general conspiracy mentality (Douglas & Sutton, 2023). Although these are often used interchangeably (Imhoff et al., 2022), there is ongoing debate in the literature about whether these two measurement approaches represent distinct psychological constructs or simply different measurement approaches for essentially the same underlying concept.

Measures of belief in specific conspiracy theories are made up of individual conspiracy theories, with questions relating to the extent of their participants' beliefs in a specific conspiracy theory (e.g., COVID-19 was released by the Chinese government as a form of biological warfare). The benefit of this approach is that each specific conspiracy theory can be linked to the concept of unwarranted conspiracy theories, where each conspiracy theory can be evaluated by researchers regarding its empirical validity. One issue with this approach is that the content of the belief in conspiracy is argued to be more ideologically contaminated due to some conspiracy theories being inherently left- or right-wing (Imhoff et al., 2022).

Measurement of a general conspiracy mentality uses broader non-specific items to assess the general tendency to think a conspiracy is at play (Bruder et al., 2013), and is conceptualised as a set of beliefs or worldviews, rather than agreement with particular narratives or claims (Imhoff & Bruder, 2014; Sutton et al., 2024). These include questions that ask a more general set of questions about conspiracy beliefs (e.g., I think that there are secret organisations that greatly influence political decisions; Bruder et al., 2013). While it has been argued that conspiracy mentality is independent of conspiracy narratives or specific ideological biases (Imhoff, 2024; Pummerer, 2024), empirical evidence to support this claim remains limited. An issue relating to the measurement of conspiracy mentality, is that, given the broadness of the concept, it does not necessarily reflect specific problematic beliefs linked to harmful behaviours.

Given these distinctions, using both measurement approaches can provide complementary but possibly distinct (see Sassenberg et al., 2023) insights relevant to understanding belief in conspiracy theories. Beliefs in specific conspiracy theories allow for direct testing of unwarranted and empirically unsupported claims, whereas measures of conspiracy mentality provide an approach to testing broader predisposition toward conspiracy thinking. While more detailed methodological justifications will be provided in later sections, the use of both measures of conspiracy beliefs supports the aim of addressing the key research questions.

Consequences of Belief in Conspiracy Theories

Belief in unwarranted conspiracy theories can have negative consequences. A growing body of experimental studies has demonstrated how exposure to conspiracy theory content causally affects various outcomes. Experimental research has found that exposure to vaccine-related conspiracy theories impacted anti-vaccination attitudes (Jolley & Douglas, 2014b); exposure to health-related conspiracy theories resulted in decreased health-seeking intentions (Natoli & Marques, 2021); exposure to government-related conspiracy theories reduced intention to engage in politics (Jolley & Douglas, 2014a); exposure to immigrant conspiracy theories increased prejudice toward immigrants (Jolley et al., 2020); and exposure to political conspiracy theories increased aggressive behaviour intentions (Poon et al., 2024).

Given the negative consequences, an important next step is to understand why people may come to believe in unwarranted conspiracy theories that can result in these harmful outcomes. Identifying underlying psychological motives that might draw people to conspiracy beliefs could help explain why such beliefs persist, despite their negative consequences.

Why Do People Believe Conspiracy Theories? Psychological Motives

Considerable psychological research has been focused on understanding why individuals come to believe in conspiracy theories, particularly those that are contrary to the widely accepted or mainstream explanations. One influential framework suggests that belief in conspiracy theories may be related to underlying psychological needs not being met by the official explanation, resulting in an increased tendency to believe in conspiracy theories to satisfy these needs (Douglas et al., 2017). These psychological motives are broadly categorised into three overarching motives, which include social (e.g., the need to maintain a positive image of one’s self and one’s group), epistemic (e.g., the need for understanding and certainty), and existential (e.g., the need to feel in control in the face of threat). According to this framework, when these psychological motives are unmet by official or mundane explanations, individuals may become more susceptible to endorsing conspiracy theories.

Social Motives

The social motive refers to a desire to belong, feel good about oneself, and maintain a positive image of oneself or a group (Douglas et al., 2017). Conspiracy theories inherently have an us-versus-them narrative, blaming outgroups or powerful "others" for negative outcomes. Protecting the perceived image of one's ingroup is posited as a key psychological motive in leading to belief in conspiracy theories. Douglas et al. argue that when an ingroup's image is threatened, particularly by a group they perceive as the outgroup, there will be an increased belief in conspiracy theories, particularly about the outgroup. An example is that often, people who are on the losing side of a particular political party endorse conspiracy theories about the winning political party, accusing them of foul play (Uscinski & Parent, 2014), but are less likely to endorse conspiracy theories about the losing political party when their political party wins. Douglas et al. explain that from a social motives perspective, belief in conspiracy theories is an attempt to reinforce a superior image of their ingroup, and the blame for negative outcomes is instead attributed to a specific outgroup (Douglas & Sutton, 2023).

Empirical evidence strongly supports this theoretical position. Collective narcissism – a defensive form of ingroup identity characterised by the belief that the ingroup's exaggerated greatness is not sufficiently recognised by others – is found to have a robust association with belief in conspiracy theories (Cichocka et al., 2016; Golec de Zavala & Federico, 2018). This association is strongest ($r = .42$) when the content of the conspiracy theories pertains to specific outgroups perceived as hostile or threatening to the ingroup (Golec de Zavala et al., 2022). Additionally, experimental studies have demonstrated that manipulating threats to group identity leads to stronger endorsement of conspiracy theories about competing outgroups (Jolley et al., 2020; Mashuri & Zaduqisti, 2015).

Therefore, according to the social motives perspective proposed by Douglas et al. (2017), when members of an ingroup perceive their group image as being challenged or insufficiently recognised as superior, their unmet social needs may lead them to turn to conspiracy theories to fulfil those needs.

Epistemic Motives

The epistemic motive refers to the human need to comprehend events and obtain accurate, certain knowledge to ensure a reliable and stable view of the world (Douglas et al., 2017; Douglas & Sutton, 2023). Conspiracy theory explanations often provide a hidden truth or pattern behind confusing or random events, potentially appealing to those with frustrated epistemic needs. Douglas et al. cite experimental research by van Prooijen et al. (2018) as evidence that illusory pattern perceptions leads to belief in conspiracy theories. For example, experimentally manipulating belief in a specific conspiracy theory influenced how participants perceived patterns in world events (van Prooijen et al., 2018). Additionally, cross-sectional research has found that factors such as perceiving patterns, agency, and meaning where they do not exist are associated with an increased tendency to believe in conspiracy theories (Brotherton, 2019; van der Wal et al., 2018; Whitson & Galinsky, 2008). These findings suggest that attempts to make sense of ambiguity or confusion could indirectly lead to belief in conspiracy theories through heightened pattern seeking.

A key aspect of epistemic motivation is the need for definitive answers and an intolerance for uncertainty. Uncertainty has been found to be exacerbated during political unrest (Kofta et al., 2020) and elevated during crises (see van Prooijen & Douglas, 2017). Researchers typically measure uncertainty with self-report measures of participants' general intolerance of uncertainty (e.g., Intolerance of Uncertainty Scale; Carleton et al., 2007). Studies using these measures often find that intolerance of uncertainty is associated with endorsement of belief in conspiracy theories (Kofta et al., 2020; Moulding et al., 2016; van den Bos, 2009; van Prooijen & Jostmann, 2013).

Overall, epistemic motives appear to have a large role in conspiracy thinking. When events seem ambiguous, or official explanations are experienced as unsatisfying, individuals may feel motivated to 'do epistemic work' to protect their worldview or belief systems.

Existential Motives

The existential motive refers to the human desire to feel safe and in control, particularly in the face of threat (Douglas et al., 2017; Douglas & Sutton, 2023). While epistemic motives are concerned with cognitive certainty and understanding, existential motives are distinct in that they primarily involve emotional and motivational responses to perceived threats or dangers.

According to Douglas et al. (2017), conspiracy theories may appeal to existential motives because they offer clear, structured explanations for otherwise uncontrollable and distressing events. Major societal crises are examples of distressing events, which are often termed existential threats – defined by Douglas and Sutton (2023) as "a threat to the very existence of a person or group" (p. 4). Historical interpretations of societal crises (e.g., the great fire of Rome in the year 64 AD) are often used as evidence of existential threats resulting in an increased belief in conspiracy theories (van Prooijen & Douglas, 2017). An example of this is that Emperor Nero had deliberately caused the fire, with rumours stating that he was singing as he watched the city burn (Brotherton, 2015). Having a powerful and malevolent outgroup (e.g., Nero) provided a clear focus for the sensemaking process. During these times of distress, individuals may compensate with conspiracy theories in an attempt to cope, by regaining a sense of safety and control over the distressing event (Douglas & Sutton, 2023).

Empirical support for existential motives primarily comes from studies linking increased feelings of anxiety, insecurity, and threat to personal safety to elevated conspiracy theory endorsement (Green & Douglas, 2018; Radnitz & Underwood, 2017; Stojanov & Halberstadt, 2019). However, findings regarding the specific role of perceived control – often posited as a key part of the existential motive – are somewhat mixed. Stojanov and Halberstadt (2020) found no evidence for a significant association between lack of control and belief in conspiracy theories in their meta-analysis, suggesting that other feelings associated with existential threats, such as elevated anxiety and distress levels, may have a more direct role.

Psychological Motives: Meta-analyses

The psychological motives framework encompasses a broad range of psychological correlates of belief in conspiracy theories, amounting to a substantial body of work. Two comprehensive meta-analyses have attempted to synthesise this research base. Biddlestone et al. (2025) reviewed 335 studies and Bowes et al. (2023) reviewed 170 studies. Both meta-analyses support the notion that belief in conspiracy theories appeals to certain psychological motives but highlight considerable variability in the strength and nature of these relationships.

Bowes et al. (2023) found that the strongest motivational correlates of conspiratorial ideation included perceiving danger and threat, reliance on intuition, and antagonism or feelings of superiority. Existential motives, particularly perceptions of external threats, also showed moderate associations with conspiracy beliefs. Bowes et al. emphasise the importance of integrating social, epistemic, and existential motives, proposing van Prooijen's (2020) existential threat model as a possible framework for generating testable hypotheses about how these motives might collectively contribute to conspiracy beliefs.

Similarly, Biddlestone et al. (2025) found some support for the three motivational categories proposed by Douglas et al. (2017). However, Biddlestone et al. have also noted considerable heterogeneity within these categories. For example, existential motives such as feelings of threat or anxiety had small-to-moderate correlations with conspiracy beliefs, indicating that not all forms of existential threat may lead to conspiracy endorsement.

Importantly, both meta-analyses caution against overinterpreting cross-sectional correlational findings, which constituted a substantial portion of the studies included in their meta-analyses (approximately 90% in Bowes et al., 2023, and 75% in Biddlestone et al., 2022). Biddlestone et al. highlighted that correlational effects were notably larger than experimental effects, implying the need for caution when interpreting cross-sectional data alone.

Overall, while the psychological motives framework provides a helpful framework of broad psychological correlates, the meta-analyses highlight the need to refine these frameworks through more rigorous methods and clearer specification of causal pathways, such as those proposed by the existential threat model.

The Existential Threat Model

The existential threat model of conspiracy theories proposed by van Prooijen (2020) provides a framework explaining why some people may believe conspiracy theories (see Figure 1). It states that times of existential threat (e.g., major societal crises) trigger a psychological sensemaking process aimed at understanding and managing the threat. A central claim of the existential threat model is the critical condition that this sensemaking process must occur in the context of a salient antagonistic outgroup, perceived as responsible for the distressing event. Without this antagonistic outgroup, van Prooijen argues that belief in conspiracy theories will not emerge. Instead, individuals will likely turn to alternative explanatory frames, such as religion, spirituality, or political ideologies (van Prooijen, 2020).

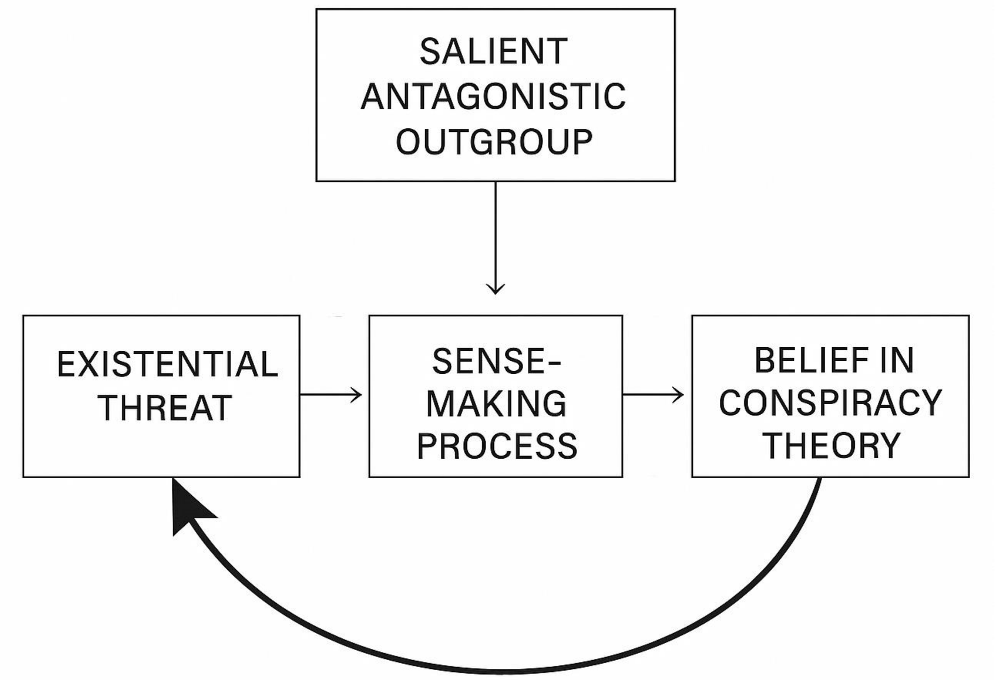
van Prooijen (2020) defines an existential threat as “feelings of anxiety or uncertainty, often because of distressing events” (p. 1). In a subsequent article, van Prooijen (2022) defines an existential threat as a “composite term for the feelings of anxiety and uncertainty that people experience when they perceive distressing events as threatening to one’s values, one’s way of life, or one’s existence” (p. 92). Both definitions highlight that the individual’s subjective perception of threat, rather than the objective presence of threat itself, is important for initiating the sensemaking process, leading to increased belief in conspiracy theories.

The existential threat model integrates epistemic, existential, and social psychological motives into a single framework. While van Prooijen (2020) does not directly mention the influence of the psychological motives posited by Douglas et al. (2017), similarities exist. For example, existential threat acts as the initiating cause of belief in conspiracy theories but also includes epistemic motives as a mediating factor (e.g., sensemaking processes), and social motives are evident throughout the condition of requiring an antagonistic outgroup.

A distinctive contribution of the existential threat model is its explicit theorising of potential feedback loops between conspiracy beliefs and psychological distress. Specifically, while individuals initially adopt conspiracy theories to manage feelings of distress, the belief itself may subsequently exacerbate distress. Believing that a malevolent group exists that aims to cause harm is likely to create further threat perceptions, thus potentially forming a vicious cycle (van Prooijen, 2020). Empirical evidence surrounding this claim is scarce, with psychological antecedents of belief in conspiracy theories being the main focus.

Figure 1

The Existential Threat Model of Conspiracy Theories



Note. The salience of antagonistic outgroups moderates the sensemaking process, leading to increased belief in conspiracy theories. Adapted from "An existential threat model of conspiracy theories," by J-W. van Prooijen, 2020, *European Psychologist, 25* (1), p. 17.

The Salience of an Antagonistic Outgroup

The existential threat model identifies the salience of an antagonistic outgroup as a key aspect for conspiracy beliefs to emerge during an existential threat. An antagonistic outgroup, such as governments, corporations, or minority groups, is perceived as intentionally causing harm. For example, the conspiracy theory that COVID-19 is a biological weapon developed and intentionally released by a weapons program in China (see Nie, 2020) implies that the Chinese government is the specified outgroup in this conspiracy theory. Blaming the outgroup (i.e., the Chinese government) provides a convenient scapegoat to make sense of a societal crisis, where the whole situation was caused by "them".

While initially described by van Prooijen (2020) as a moderator, the antagonistic outgroup might more accurately represent a necessary condition. However, this conceptualisation remains theoretical, as no empirical study has formally tested the moderating effect of antagonistic outgroup salience. For example, experimental research by Mashuri and Zaduqisti (2015), cited as indirect support by van Prooijen, demonstrated increased conspiracy beliefs under outgroup threat manipulation but lacked direct measures of existential threat or psychological distress. Thus, empirical evidence supporting the moderating role or necessary condition of an antagonistic outgroup remains unclear, highlighting that further research, which specifically tests for a moderating effect, is necessary.

Negative Affect Associated with Existential Threat

Central to the existential threat model is that subjective threat feelings, rather than objective threats themselves, influence conspiracy beliefs. Subjective threat perceptions typically involve negative affective experiences like anxiety and stress. Several empirical studies indirectly support this link, demonstrating associations between anxiety, stress, and existential threats. For example, existential threats have been found to provoke anxious arousal (see Tabri et al., 2020), and the perceived threat of COVID-19 (as a proxy measure for existential threat) was found to have a large positive association with anxiety ($\beta = .51$, $p < .001$), in a sample of 1,758 US participants (Vail et al., 2023). Similarly, elevated stress levels are associated with experiences of existential threats, such as distressing life events (Park, 2010).

In contrast, depression is less explicitly accounted for in the frameworks provided by van Prooijen (2020) and Douglas et al. (2017), with empirical evidence to support this link being more limited. However, some experimental research has found that when reflecting on existential threats, participants reported more depressive symptoms than a control group (Poppelaars et al., 2020). In addition, perceived threat during warzone deployment has been positively associated with depressive symptoms within a sample of US military participants (Lancaster et al., 2016). Therefore, depression was primarily included in the current study based on empirical observations in the literature (discussed further below), deeming it appropriate to theorise alongside anxiety and stress.

Limitations of the Existential Threat Model

The existential threat model, while valuable, has several conceptual ambiguities. First, the definitions of existential threat and antagonistic outgroup remain broad and underspecified. For example, the model offers high-level explanations without clearly testable boundary conditions. In addition, the sensemaking process itself remains largely theoretical, serving more as a conceptual placeholder than a clearly articulated cognitive or affective mechanism. That said, it is important to test the existential threat model empirically to understand the plausibility of the argued predictions.

Anxiety, Stress, Depression and Belief in Conspiracy Theories: Empirical Evidence

The existential threat model implies that psychological distress influences belief in conspiracy theories. The following sections focus on three prominent forms of psychological distress – anxiety, stress, and depression. While these constructs have conceptual overlap, they are broadly grouped into separate sections, informed by the type of measure used in the specific study. Therefore, the following subsections summarise the relevant empirical evidence of each construct with belief in conspiracy theories, and outline the limitations of the established research.

Anxiety and Belief in Conspiracy Theories

Anxiety is a broad term used to describe a range of disorders (e.g., specific phobia, social anxiety, and panic disorder), often characterised by the anticipation of future danger or threat (American Psychiatric Association, 2022). Previous research that considered anxiety and belief in conspiracy theories often linked anxiety to a preoccupation with future threatening situations (Grzesiak-Feldman, 2013; Heiss et al., 2021; Radnitz & Underwood, 2017; Šrol, Cavojova, et al., 2021). Therefore, anxiety is, by definition, inherently connected to the existential threat model. Numerous researchers have explored the relationship between anxiety and belief in conspiracy theories and have used a range of different constructs to measure anxiety (see Table 1). Most studies have used cross-sectional designs, with fewer longitudinal (e.g., Liekefett et al., 2023) and experimental studies (e.g., Grzesiak-Feldman, 2013).

Several cross-sectional studies report consistent evidence for a positive correlation between anxiety and belief in conspiracy theories. The studies summarised in Table 1 report small to moderate positive associations between anxiety and belief in conspiracy theories ranging from $r = .09$ to $r = .27$ (Bowes et al., 2023; Fox & Williams, 2023; Green & Douglas, 2018; Grzesiak-Feldman, 2013; Heiss et al., 2021; Hettich et al., 2022; Krüppel et al., 2023; Peitz et al., 2021; Simione et al., 2021; Šrol, Cavojova, et al., 2021; Swami et al., 2017). Only a few cross-sectional studies have failed to find this association (De Coninck et al., 2021; Grzesiak-Feldman, 2007). In addition, two recent meta-analyses summarising cross-sectional findings found a small positive association between anxiety and belief in conspiracy theories

(Biddlestone et al., 2025; Bowes et al., 2023). These findings could suggest that anxiety has, at least on average, some relationship with belief in conspiracy theories. However, the existing cross-sectional research does not allow us to conclude that there is a causal relationship between anxiety and belief in conspiracy theories (see Rohrer, 2018).

Longitudinal studies allow for an understanding of the causal relationship that holds between anxiety and belief in conspiracy theories by establishing temporal order. However, the current longitudinal research results provide ambiguous results regarding causality. Leibovitz et al. (2021), in a two-wave panel study, found no significant cross-lagged effect between anxiety and belief in COVID-19-related conspiracy theories. Similarly, Heiss et al. (2021), in a panel study with two time points, found that anxiety (operationalised as threat perceptions) predicted a change in believing conspiracy claims but had no effect on change in conspiracy thinking at the later time point. However, both studies use cross-lagged panel models (CLPMs), which cannot rule out stable confounding variables, meaning that stable individual differences could bias the reported effects. As such, providing limited details for understanding of individual-level changes in anxiety or conspiracy beliefs over time.

Liekefett et al. (2023) overcame this limitation by using a random intercept cross-lagged panel model (RI-CLPM), which distinguishes between-person associations and within-person associations (Hamaker et al., 2015). Liekefett et al., in their first study with four time points over two months, found no within-person cross-lagged effect of anxiety increasing belief in COVID conspiracy theories over time (i.e., increased anxiety at time t-1 affecting belief in conspiracy theories at time t). In addition, their second study, with four time points over 18 months, found no within-person cross-lagged effect of anxiety increasing belief in COVID conspiracy theories over time. As RI-CLPMs allow for testing of bidirectional relationships, Liekefett et al. found evidence for the opposite direction – a significant within-person effect of increased belief in COVID-19 conspiracy theories predicting subsequent increases in anxiety levels in Study 1. This effect was not supported in Study 2. Therefore, Liekefett et al. (2023) found tentative evidence that belief in conspiracy theories may increase anxiety levels, but no evidence to support a within-person effect of increased anxiety affecting belief in conspiracy theories.

Longitudinal findings by Liekefett et al. (2023) contradict experimental research (Grzesiak-Feldman, 2013; Radnitz & Underwood, 2017). Grzesiak-Feldman (2013) conducted two experimental studies, manipulating anxiety by having participants wait to take an exam and comparing them to participants waiting to attend a lecture (control condition). As participants were waiting, they were provided with measures of conspiracy thinking. In the first experiment ($N = 46$), they found that the high-anxiety condition had increased conspiracy thinking about Jews in comparison to the neutral condition. In the second experiment ($N = 67$), they found evidence that the anxiety condition situation increased conspiracy thinking about Jews, but not about Germans or Arabs. In the article, Grzesiak-Feldman does not explicitly state whether participants were randomly assigned to the pre-exam or pre-lecture conditions, raising concerns about the rigor of the experimental design. In addition, it is challenging to see how students could be randomly assigned to have an exam immediately after measuring the dependent variable (conspiracy thinking), versus attending a lecture. For example, the coordination of organising exam schedules and ensuring genuine random assignment into exam and no exam conditions are problematic. These issues make it difficult to rule out pre-existing differences between participants, weakening the causal claims of this experimental research.

In a large-scale survey experiment of 1997 US-based participants, Radnitz and Underwood (2017) concluded that manipulating worry levels subsequently increased belief in conspiracy theories. Radnitz and Underwood (2017) used a manipulation of anxiety that involved a priming vignette that asked half of the participants how “worried” they were after reading a description of the financial crisis and subsequently asked how it impacted them personally. All participants were then provided with a fictional ‘conspiracy vignette’ about a mysterious illness afflicting a small midwestern town, with a biochemical plant as the possible source of the illness. Participants were then asked questions regarding their perception of conspiratorial explanations in relation to the fictional vignette. Radnitz and Underwood found that participants who were primed to feel anxious were more likely to perceive a conspiracy in the vignette. The apparent effect found was small ($d = 0.205$). It is unclear whether these effects would generalise beyond the context of a fictional vignette. In addition, the anxiety priming manipulation was not compared with a control condition, only between participants in the worry condition. Therefore, the differences in conspiracy scores cannot be confidently causally attributed to the manipulation of anxiety.

Most recently, Adamus et al. (2025) conducted two studies exploring economic anxiety and belief in COVID-19 conspiracy theories. Their initial cross-sectional study involving 21,306 participants across 17 European countries found a significant positive correlation between economic anxiety and conspiracy beliefs ($\beta = .17$), controlling for various demographic and psychological covariates. In Study 2 a longitudinal cross-lagged panel model (CLPM) over three waves with 925 Slovak participants found that COVID-19 conspiracy beliefs significantly predicted increases in economic anxiety with medium to large effect sizes ($\beta = .17$ from T1 to T2 and $\beta = .10$ from T2 to T3, $p < .005$), but economic anxiety did not predict increased conspiracy beliefs. These findings suggest that conspiracy beliefs may exacerbate perceptions of economic distress over time rather than being a response to economic anxiety.

In summary, current research on anxiety and conspiracy beliefs has found consistent cross-sectional associations. Experimental studies suggest an effect of anxiety on conspiracy beliefs but face methodological limitations. Longitudinal evidence remains mixed and largely inconclusive. Table 1 below is a summary of all studies that have examined anxiety or anxiety-related indicators, with belief in conspiracy theories.

Table 1

Summary of Studies Involving Anxiety and Conspiracy Beliefs

Authors	Country	*N*	Research Design	Statistical Method	Conspiracy Measure	Anxiety Measure	Key Results	Pre-registered?
Adamus et al. (2025)	Slovakia	925	Longitudinal (3 waves)	CLPM: CT -> threat	COVID-19-related conspiracy beliefs	Modified financial threat scale	β = .17*** (T1 to T2) β = .10*** (T2 to T3)	No
Adamus et al. (2025)	Slovakia	925	Longitudinal (3 waves)	CLPM: threat -> CT	COVID-19-related conspiracy beliefs	Modified financial threat scale	β = .-03 (T1 to T2) β = .03 (T2 to T3)	No
Adamus et al. (2025)	17 European Countries	21,306	Cross-sectional	Mixed Model	COVID-19-related conspiracy beliefs	Modified financial threat scale	β = . 17	No
Bowes et al. (2020)	US	327	Cross-sectional	Bivariate correlation	BCTI	PROMIS	*r* = .16***	No
De Coninck et al. (2021)	Eight Countries	8,806	Cross-sectional	Structural equation model	Original Belief in Conspiracy Theories Scale	GAD-7	β = .04	No
Fox and Williams (2023) Study 1	NZ and AU	502	Cross-sectional	Structural equation model	aBCTI	GAD-7	β = .07	Yes
Fox and Williams (2023) Study 2	USA	1020	Cross-sectional	Structural equation model	aBCTI	GAD-7	β = -.05	Yes

Authors	Country	N	Research Design	Statistical Method	Conspiracy Measure	Anxiety Measure	Key Results	Pre-registered?
Green and Douglas (2018) Study 1	Majority US	246	Cross-sectional	Bivariate correlation	GCB	ECR-R	$r = .24^{***}$	No
Green and Douglas (2018) Study 2	Majority UK	230	Cross-sectional	Multiple regression	SCP	ECR-R	$\beta = .18^{*}$	No
Grzesiak-Feldman (2007)	Poland	118	Cross-sectional	Bivariate correlation	Conspiracy Beliefs Scale	STAI - Polish adaptation	State; $r = .05$ Trait; $r = .02$	No
Grzesiak-Feldman (2013) Study 1	Poland	87	Cross-sectional	Multiple regression	Conspiracy Thinking about Jewish People	STAI - Polish adaptation	State; $r = .27^{*}$ Trait; $r = .20$	No
Grzesiak-Feldman (2013) Study 2	Poland	46	Experiment/ survey	One way ANOVA	Conspiracy Thinking about Jewish People	Anxiety Prime (pre-exam)	$F(1, 44) = 4.08^{*}$	No
Grzesiak-Feldman (2013) Study 3	Poland	67	Experiment/ survey	Least Significant Difference Test	Conspiracy Thinking about Jewish People	Anxiety Prime (pre-exam)	$M_{\text{exam}} = 55.92$ vs. $M_{\text{control}} = 49.24^{*}$	No
Heiss et al. (2021)	Austria	1,024	Cross-sectional	Multiple regression	Original Belief in COVID-19 Conspiracy Theories	Threat Perception	$\beta = .16^{***}$	No
Heiss et al. (2021)	Austria	632	Longitudinal (two-time points)	Panel analysis	Original Belief in COVID-19 Conspiracy Theories Scale	Threat perception	$\beta = .05^{**}$	No

Authors	Country	N	Research Design	Statistical Method	Conspiracy Measure	Anxiety Measure	Key Results	Pre-registered?
Hettich et al. (2022)	Germany	2,503	Cross-sectional	Multiple regression	Conspiracy mentality	HADS-6 (anxiety)	β = .12***	No
Krüppel et al. (2023)	Germany	589	Cross-sectional	Bivariate correlation	Generic Conspiracist Beliefs Scale (German)	STAI	State; r = ..09* Trait; r = .10*	Yes
Leibovitz et al. (2021)	US	395	Longitudinal (two-time points)	Cross-lagged panel correlation	FICS	GAD-7	r = .07	No
Leone (2018) Study 2	Italy (students)	321	Cross-sectional	Bivariate	GCB	STAI	r = .048	No
Liekefett et al. (2023) Study 1	Germany	405	Longitudinal (four-time points)	Cross-lagged panel correlation	CMQ	GAD-7 (German)	β = .05	No
Liekefett et al. (2023) Study 2	Germany	1,012	Longitudinal (four-time points)	Cross-lagged panel correlation	CMQ	GAD-7 (German)	β = .02	No
Peitz et al. (2021)	UK	1,579	Cross-sectional	Structural equation Model	COVID-19 Conspiracy Theories (1-item)	1-item Anxiety	β = .17***	Yes
Radnitz and Underwood (2017)	US	1,997	Experiment/ survey	Cohen's d	Conspiracy vignette	MAP	d = 0.21	No

Authors	Country	*N*	Research Design	Statistical Method	Conspiracy Measure	Anxiety Measure	Key Results	Pre-registered?
Simione et al. (2021)	Italy	374	Cross-sectional	Bivariate correlation	BOC-19	STAI	r = .14**	No
Srol et al. (2021)	Slovak	783	Cross-sectional	Bivariate	COVID-19 conspiracy beliefs	COVID-19 Anxiety (ad hoc)	r = .19***	No
Srol et al. (2021)	Slovak	783	Cross-sectional	Multiple regression	COVID-19 conspiracy beliefs	COVID-19 Anxiety (ad hoc)	β = .03	No
Swami et al. (2016)	USA	420	Cross-sectional	Multiple regression	BCTI	STAI	Trait; r = .10* State r = .06	No

Note. BOC-19 = Beliefs on COVID-19 Conspiracy sub-scale; CMQ = Conspiracy Mentality Questionnaire; ECR-R = Experiences in Close Relationships-Revised (ECR-R) scale; Anxious + Avoidance scale; FICS = Flexible Inventory of Conspiracy Suspicions; GAD-7 = General Anxiety Disorder 7-Item; GCB = Generic Conspiracist Beliefs Scale (Brotherton et al., 2013); MAP = Macro-economic anxiety prime; PROMIS = Patient-Reported Outcomes Measurement; SCP = Specific Conspiracy Beliefs (Douglas et al 2016); STAI = . The different studies used a variety of effect sizes measures, including correlations (*r*), unstandardised regression weights (B), standardised regression weights (β), analysis of variance (*F*), unstandardised means (*M*), and Mann-Whitney U test (M-W).

*p < .05, **p < .01 ***p < .001

Stress and Belief in Conspiracy Theories

Stress is best defined in the context of belief in conspiracy theories as the subjective appraisal of stressful situations in one's life (Cohen et al., 1983). However, the definition of "stress" itself remains contentious, with no universally agreed-upon description. Definitions vary widely depending on whether the emphasis is placed on external stressors or individuals' psychological and physiological responses, and whether stress is defined primarily in biological or psychological terms (Fink, 2016). Despite this definitional complexity, most empirical research examining stress and belief in conspiracy theories has consistently operationalised stress using subjective appraisals, most frequently using the Perceived Stress Scale (PSS; Cohen et al., 1983; Blondé et al., 2020; Braud et al., 2021; Ferreira et al., 2020; Fox & Williams, 2023; Georgiou et al., 2020; Pfeffer et al., 2022; Simione et al., 2021; Swami et al., 2017; Williams, Anderson, et al., 2022). Fewer studies have relied on unvalidated, self-constructed measures of COVID-19 stress (Constantinou et al., 2021; Dyrendal & Hestad, 2021) or single-item measures of momentary subjective stress (Kuhn et al., 2021). The widespread use of the PSS allows for more consistent comparisons across studies.

Swami et al. (2016) first established the association in a cross-sectional study of stress and belief in conspiracy theories. Using two measures of stress, they found significant partial correlations – perceived stress with belief in conspiracy theories ($r = .15$) and stressful life events with belief in conspiracy theories ($r = .29$) after controlling for subjective socio-economic status and age. From these findings, Swami et al. concluded that subjective stress and adverse life events could lead to belief in conspiracy theories. Following this research, 11 cross-sectional studies established positive correlations ranging from $r = .08$ to .26, with samples from Switzerland, Greece, Norway, Australia, New Zealand, Germany, Italy, and the USA (Blondé et al., 2020; Braud et al., 2021; Constantinou et al., 2021; Dyrendal & Hestad, 2021; Ferreira et al., 2020; Fox & Williams, 2023; Georgiou et al., 2020; Kuhn et al., 2021; Pfeffer et al., 2022; Simione et al., 2021; Williams, Anderson, et al., 2022). Not all research has found a relationship between stress and belief in conspiracy theories. For example, studies by Georgiou et al. (2020) and Ferreira et al. (2020) failed to find a significant bivariate correlation. In general, there appears to be strong evidence for a positive correlation between stress and belief in conspiracy theories.

The significant bivariate correlations between stress and belief in conspiracy theories sometimes become negligible when plausible confounding variables are controlled for. For example, my Master's thesis research indicated a similar pattern in a cross-sectional study of 502 Australian and New Zealand participants (see Fox & Williams, 2023, Study 1). The study found a positive bivariate correlation (r = .17) between perceived stress and belief in conspiracy theories. However, this bivariate correlation became non-significant after controlling for plausible confounding variables of age, subjective socio-economic status, education level and political orientation. That said, a follow-up cross-sectional study in the US with a larger sample (N = 1020) did find a significant effect of β = .12 (see Fox & Williams, 2023, Study 2), suggesting that studies may need to be sufficiently powered to detect this plausibly small effect after controlling for the same plausible confounding variables.

Overall, there is evidence for a positive correlation between stress and belief in conspiracy theories, but inferences beyond a positive correlation are unclear. Despite only having correlational evidence to support such a claim, stress reduction has been suggested as a possible intervention in reducing belief in conspiracy theories (e.g., Giotakos, 2022; Pfeffer et al., 2022; Swami et al., 2016). However, given the lack of clarity regarding causality, the possibility remains that belief in conspiracy theories might also increase stress levels, potentially creating a bidirectional relationship. If such bidirectional effects exist, intervention focused solely on reducing stress might have limited effectiveness in decreasing conspiracy beliefs.

Table 2

Summary of Studies Involving Stress and Conspiracy Beliefs

Author	Country	*N*	Design	Statistical Method	Conspiracy Measure	Stress Measure	Key Results	Pre-registered?
Blondé et al. (2020)	Switzerland	376	Cross-sectional	Bivariate correlation	1-item measure of Conspiracy Belief	PSS (French)	$r = .16^{**}$	No
Braud et al. (2021)	No country specified	237	Cross-sectional	Multiple regression	Combined Conspiracy Beliefs	PSS	B = .83** (Table 10)	Yes
Constantinou et al. (2021)	Cyprus & Greece	1,001	Cross-sectional	Bivariate correlation	Original COVID-19 Conspiracy Beliefs	COVID-19 Distress	$r = .13^{***}$	No
Dyrendal & Hestad (2021)	Norway	1,225	Cross-sectional	Bivariate	COVID-19 beliefs	COVID-19 Stress	$r = .08^{***}$	No
Dyrendal & Hestad (2021)	Norway	1,225	Cross-sectional	Multiple regression	COVID conspiracy beliefs	COVID Stress	*B = .00*	No
Ferreira et al. (2020)	Not specified	438	Cross-sectional	Bivariate correlation	Original COVID-19 Conspiracy Beliefs	PSS	$r = .01$	No
Fox & Williams (2023) Study 1	Australia & NZ	502	Cross-sectional	SEM	aBCTI	PSS	$\beta = .06$	Yes
Fox & Williams (2023) Study 2	USA	1020	Cross-sectional	SEM	aBCTI	PSS	$\beta = .12^{*}$	Yes
Georgiou et al. (2020)	UK, US, Europe	640	Cross-sectional	Bivariate correlation	BCTI	PSS	$r = .07$	No

Author	Country	*N*	Design	Statistical Method	Conspiracy Measure	Stress Measure	Key Results	Pre-registered?
Kuhn et al. (2021)	Switzerland	1,684	Cross-sectional	Bivariate correlation	Specific COVID-19 conspiracy beliefs.	MSS	$r = .19$***	No
Pfeffer et al. (2022)	Germany	1,354	Cross-sectional	Bivariate	Covid-related Conspiracy Beliefs	PSS	$r = .26$***	No
Simione et al. (2021)	Italy	374	Cross-sectional	Bivariate correlation	BOC-19	PSS	$r = .20$**	No
Swami et al. (2016)	USA	420	Cross-sectional	Multiple regression	BCTI	PSS	$r = .15$*	No
Williams et al. (2022)	New Zealand, Australia	372	Cross-sectional	Bivariate correlation	Original Belief in Conspiracy Theory Scale	PSS	$r = .20$***	Yes

Note. BOC-19 = Beliefs on COVID-19 Conspiracy sub-scale; BCTI = Belief in Conspiracy Theories Inventory; Combined Conspiracy Beliefs = Belief in Conspiracy Theories Inventory, Generic Conspiracist Beliefs and COVID-19 Conspiracy Beliefs; MSS = Momentary Subjective Stress; PSS = Perceived Stress Scale; PSS-French = Perceived Stress Scale (French version). All studies used cross-sectional survey designs. The different studies used a variety of effect sizes measures, including correlations (r), unstandardised regression weights (B), and standardised regression weights (β),

*$p < .05$, **$p < .01$, ***$p < .001$

Depression and Belief in Conspiracy Theories

Depression is a broad term used to describe a range of depressive disorders, characterised by depressive symptoms that include sadness, hopelessness, emptiness, and irritability, which accompany changes that significantly affect an individual's capacity to function in their day-to-day life (American Psychiatric Association, 2022). Depression is not explicitly theorised in the existential threat model to affect belief in conspiracy theories (van Prooijen, 2020), and rather consists of our own theorising. For example, depression is often associated with a generally bleak and pessimistic outlook on the future (World Health Organization., 2019), which could be the key characteristic that links depression to the existential threat model. A bleak future outlook could be a pathway into believing conspiratorial explanations for major societal crises, but also, believing such a conspiratorial explanation could subsequently worsen depression symptoms.

Empirical research aimed at understanding the relationship between depression and belief in conspiracy theories has primarily emerged following the onset of the COVID-19 pandemic in 2020. Table 3 highlights all current empirical research testing the relationship between depression and belief in conspiracy theories, which are predominantly cross-sectional designs with a range of different measures of depression. Overall, most studies report a positive cross-sectional correlation between depression and belief in conspiracy theories ranging from $r = .11$ to $r = .20$ (Bowes et al., 2020; De Coninck et al., 2021; Dębski et al., 2022; Kosarkova et al., 2022; Leone et al., 2018; Panfil et al., 2022; Simione et al., 2021). When plausible confounding variables (e.g., age and education level) are statistically controlled for, this standardised effect is smaller but still statistically significant, with Dębski et al. (2022) finding $B = .06$ and Zwar et al. (2022) finding $B = .10$.

Recent longitudinal research by Green et al. (2023) offers some insight into the relationship between depression and belief in conspiracy theories. Using three waves of data collected over seven months in 2021 from 18,427 US participants, the study found evidence for a positive association between depression (using the Patient Health Questionnaire-9) and belief in COVID-19 conspiracy theories at the between-person level. The data were analysed using two approaches: logistic regression and a machine learning approach. First, Green et al. used logistic regression, dichotomising depression and conspiracy beliefs at the person level at each wave. They classified depression as either "not depressed" or "at least moderate depressive symptoms" and conspiracy beliefs as either "holding a belief" or "no evidence of such a belief." Due to the poor reliability of their belief in conspiracy theories measure [1] (alphas for each wave = 53, .49, and .55), they analysed each conspiracy theory item individually at the *same* wave. The output effectively provided cross-sectional evidence to support a positive correlation between depression and belief in each conspiracy theory item. However, this approach provides limited causal insights and offers no real information about changes over time.

The second analysis by Green et al. (2023) used a machine learning approach, specifically the single-learner algorithm (Künzel et al., 2019). This model included a wide range of covariates (e.g., income, ethnicity, education level, social support) and, similar to the previous analysis, examined each conspiracy belief item individually at each wave. They consistently found a positive association between depression and belief in conspiracy items. However, there are considerable limitations to this approach to analysing their data. For example, data-driven prediction models are often mistakenly used in attempts to find causal effects (Prosperi et al., 2020). Despite this, Green et al. appeared to perceive their findings as causal, concluding that their results "provide crucial insight into interventions" (p. 345). While their research offers a descriptive understanding of the relationship between depression and conspiracy beliefs, it provides limited insight into the causal mechanisms underlying this association.

While there is some evidence for a positive association between depression and belief in conspiracy theories, whether depression causally affects belief in conspiracy theories remains uncertain. Fountoulakis et al. (2021) highlighted this ambiguity in their cross-sectional study, concluding that conspiracy theories could either cause depression or result from it, or both could be influenced by another factor entirely. Experimental or longitudinal studies with a focus on causal inference are needed to gain further insight into this relationship.

[1] Their items were the following: "Coronavirus was created as a weapon in a Chinese lab", "the COVID-19 vaccines contain the lung tissue of aborted foetuses", and the "COVID-19 vaccines contain microchips that could track people."

Table 3

Summary of Studies Involving Depression and Conspiracy Beliefs

Authors	Country	*N*	Research Design	Statistical Method	Conspiracy Measure	Depression Measure	Key Results	Pre-registered?
Bowes et al. (2020)	US	327	Cross-sectional	Bivariate correlation	BCTI	PROMIS	*r* = .16***	No
De Coninck et al. (2021)	Eight Countries	8,806	Cross-sectional	Structural equation model	Original Belief in Conspiracy Theories Scale	PHQ-9	*r* = .10***	No
Dębski et al. (2022)	Poland	700	Cross-sectional	Bivariate correlation	COVID-19 CBS, GCBS	HADS-D	*r* = .09* *r* = .13***	No
Dębski et al. (2022)	Poland	700	Cross-sectional	Multiple regression	COVID-19 CBS, GCBS	HADS-D	*b* = .06*** *b* = .06***	No
Elek et al. (2022)	Hungary	763	Cross-sectional	Linear regression	CTBS	CES-D (Hungarian)	b = .000	No
Fountoulakis et al. (2021)	Greece	3,399	Cross-sectional	Chi-square tests	COVID-19 Conspiracy Scale (ad hoc)	CES-D	No relationship	No
Green et al. (2023)	USA	18,427	Longitudinal	Logistic regression	COVID-19 conspiracy theories	PHQ-9	*r*’s from .18 to .82	No

Authors	Country	*N*	Research Design	Statistical Method	Conspiracy Measure	Depression Measure	Key Results	Pre-registered?
Kosarkova et al. (2022)	Czech	1,273	Cross-sectional	Logistic regression	Religious conspiracy theories	ODSIS	$r = .20^{***}$	No
Leone (2018) Study 2	Italy	321	Cross-sectional	Bivariate	GCB	BDI-III	$r = .12^{*}$	No
Panfil et al. (2022)	Romania	1,446	Cross-sectional	Bivariate	COVID-19 conspiracies (ad hoc)	CES-D	$r = .12^{***}$	No
Simione et al. (2021)	Italy	374	Cross-sectional	Bivariate correlation	BOC-19	GHQ	$r = .19^{***}$	No
Zwar et al. (2022)	Germany	489	Cross-sectional	Multiple regression	CMQ	PHQ-9	$B = 0.10^{***}$	No

Note. BCTI = Belief in Conspiracy Theories Inventory; PROMIS = Patient-Reported Outcomes Measurement Information System – Depression Subscale; PHQ-9 = Patient Health Questionnaire – 9-item Depression Scale; GCBS = Generic Conspiracist Beliefs Scale; COVID-19 CBS = COVID-19 Conspiracy Beliefs Scale; HADS-D = Hospital Anxiety and Depression Scale – Depression Subscale; CTBS = Conspiracy Theory Belief Scale; CES-D = Center for Epidemiologic Studies Depression Scale; BOC-19 = Belief in COVID-19 Conspiracies Scale; GHQ = General Health Questionnaire; CMQ = Conspiracy Mentality Questionnaire; ODSIS = Overall Depression Severity and Impairment Scale; GCB = Generic Conspiracist Beliefs Scale; BDI-III = Beck Depression Inventory – Third Edition. b = unstandardised regression coefficient. $^{*}p < .05$, $^{**}p < .01$ $^{***}p < .001$

Distress and Belief in Conspiracy Theories

Some researchers have used combined measures of psychological distress, which intend to measure symptoms of anxiety, depression, and stress. A summary of those studies can be found in Table 4. Ballová Mikušková and Teličá (2024) tested the bidirectional effects of unfounded COVID-19 conspiracy beliefs on psychological distress at three time points over 18 months in a sample of 925 Slovakian participants, and did not find any cross-lagged effects over time. Another longitudinal study by Chan et al. (2023) estimated reciprocal effects between psychological distress (measured using the total score of the Depression, Anxiety and Stress Scale – 21) and belief in COVID-19 conspiracy theories, finding no significant cross-lagged effects. However, this study used an RI-CLPM *without* multiple indicators (i.e., not fully accounting for measurement error), and its findings pertain to the overarching construct of psychological distress rather than stress or anxiety *per se*.

Recent longitudinal research by Samayoa et al. (2025) explored the relationships between well-being, economic turmoil, conspiracist ideation, and COVID-19 conspiracy beliefs across two studies – one cross-sectional and one employing a two-wave design with a cross-lagged panel model (CLPM). Study 1 found that belief in COVID-19 conspiracy theories predicted lower stress (measured via a single item) when accounting for economic turmoil and conspiracist ideation. The second study found that when economic distress and conspiracist ideation were statistically controlled, initial belief in COVID-19 conspiracy theories predicted increased feelings of contentment over time. However, this relationship diminished over time. Samayoa et al. thus implied that belief in conspiracy theories may temporarily benefit psychological well-being by enhancing positive affect and perceived capability to cope with threat, but these benefits dissipate over time.

Controlling for conspiracist ideation (or conspiracy mentality) when examining the effects of specific conspiracy beliefs presents significant interpretive challenges, as the causal direction between general conspiracy ideation and specific conspiracy beliefs remains uncertain (Nera, 2024; Sutton et al., 2024). If specific conspiracy beliefs enhance general conspiracist ideation, rather than vice versa, statistically controlling for ideation may inadvertently obscure true relationships or introduce spurious results. Consequently, findings from studies like that of Samayoa et al. (2025), where positive effects of conspiracy beliefs emerge only after accounting for conspiracist ideation, must be interpreted cautiously.

In summary, existing longitudinal research on psychological distress and conspiracy beliefs indicates limited or no reciprocal effects, with methodological differences and measurement issues complicating interpretation. Thus, questions remain regarding whether psychological distress affects belief in conspiracy theories.

Table 4

Summary of Studies Involving Distress and Conspiracy Beliefs

Author	Country	*N*	Design	Statistical Method	Conspiracy Scale	Distress Scale	Key Result	Pre-registered?
Chan et al. (2023)	USA	818	Longitudinal	RI-CLPM	COVID-19 Conspiracy Beliefs	DASS-21	*b* = -.01 to .07	No
Ballová Mikušková & Teličá (2024)	Slovakia	925	Longitudinal	CLPM	COVID-19 Unfounded Beliefs Scale	Symptom Checklist-10	β = -.07 to .05	No
Samayoa et al. (2025) Study 1	USA	441	Cross-sectional	Multiple regression	Belief in COVID-19 conspiracy theories	General stress	β = -0.24*	No
Samayoa et al. (2025) Study 2	UK	712	Longitudinal (4 waves)	Multiple regression	Belief in COVID-19 conspiracy theories	Subjective well-being	Multiple analyses reported (varied results)	No

Note. DASS-21 = Depression Anxiety Stress Scales (21-item). General stress was measured using a single item by Samayoa et al. (2025). Samayoa et al. (2025) Study 2 used multiple measures of subjective well-being (Subjective Happiness Scale, Brief Measure of Hopelessness Scale, PHQ-9, GAD-7, and life satisfaction). Due to the variety of analyses and outcomes, individual results are not reported here. *$p < .001$. Unstandardised regression weights (*b*) and standardised regression weights (β).

The Current Study

Existing evidence supports a positive correlation between various forms of psychological distress and belief in conspiracy theories. Longitudinal and experimental studies provide tentative evidence suggesting that psychological distress may causally affect belief in conspiracy theories, but the reverse relationship – whether belief in conspiracy theories *causes* psychological distress – remains largely unexplored.

This study addresses these directional causal questions through a longitudinal design, focusing on anxiety, depression, and stress. Psychological distress is operationalised through these commonly studied indicators, as they have established associations with conspiracy beliefs, and are conceptually aligned with the notion of existential threat.

Additionally, the existential threat model (van Prooijen, 2020), theorises that heightened psychological distress (i.e., existential threat) increases belief in conspiracy theories, but depends on the salience of antagonist outgroups, implying an interaction effect. This study aimed to empirically test this interaction effect, contributing the first empirical test of this core prediction of the existential threat model.

Hypotheses

The following were hypothesised:

Depression (**H1**), perceived stress (**H2**), stressful life events (**H3**), and anxiety (**H4**) would have positive cross-lagged effects on belief in conspiracy theories.

Belief in conspiracy theories would have positive cross-lagged effects on depression (**H5**), perceived stress (**H6**), and anxiety (**H7**).

Consistent with the existential threat model, the cross-lagged effect of perceived stress on belief in a specific conspiracy theory (that COVID-19 is a bioweapon created by China) would be more positive amongst people who perceive the outgroup implied in the conspiracy theory (i.e., the Chinese government) as threatening (**H8**).

It was not hypothesised that belief in conspiracy theories would lead to increases in stressful life events (e.g., financial crises, victimisation). While increased belief in conspiracy could potentially increase exposure to such life events, this effect was expected to be small relative to the more substantial, established causes of these events, making it difficult to detect within this study.

Conspiracy Mentality

Few studies have considered the difference between measures of belief in specific conspiracy theories and general conspiracy mentality (Imhoff et al., 2022; Nera, 2024). Therefore, ambiguity remains regarding the optimal measure for accurately measuring an individuals belief in conspiracy theories. Belief in conspiracy theories can be understood as belief in *specific* theories, or as reflecting a broader predisposition toward conspiratorial thinking, often referred to as "conspiracy mentality" (Imhoff & Bruder, 2014).

A conspiracy mentality has also been argued to be a more stable individual trait, compared to belief in conspiracy theories (Imhoff et al., 2022). However, whether conspiracy mentality is truly *more* stable than belief in conspiracy theories remains uncertain. Consequently, hypotheses H9–H15 focus on conspiracy mentality as an alternative, validated measure (Bruder et al., 2013) to test the robustness of the main hypotheses.

The following were also hypothesised:

Depression (**H9**), perceived stress (**H10**), stressful life events (**H11**), and anxiety (**H12**) would have positive cross-lagged effects on general conspiracy mentality.

General conspiracy mentality would have positive cross-lagged effects on depression (**H13**), perceived stress (**H14**), and anxiety (**H15**).

Method

Preregistration

This study was preregistered prior to data collection on the Open Science Framework, following best practice guidelines to ensure transparency in what was planned (Lakens, 2019). Any deviations to the preregistration plan are detailed, and a rationale for why they occurred is provided (Lakens, 2024). A full copy of the preregistration can be found here: https://osf.io/5k4yb.

Design

The current research had a longitudinal design using an online survey. The same participants were invited to complete seven surveys from October 2022 to April 2023. A longitudinal design allows for stronger inferences about causality (compared to a cross-sectional design) by establishing temporal precedence, although it is not as strong as an experimental design. For example, longitudinal research can establish temporal precedence but cannot rule out time-variant confounding variables (see Murayama & Gfrörer, 2022). However, a key aspect of experimental designs is the need to experimentally manipulate anxiety, depression, or stress. While experimentally manipulating levels of anxiety, depression, or stress is possible (e.g., Brouwer & Hogervorst, 2014; Buhr & Dugas, 2009), it is ethically problematic to induce these states at levels sufficient to plausibly affect belief in conspiracy theories.

An assumption with experimental research (e.g., Grzesiak-Feldman, 2013; Radnitz & Underwood, 2017) is that only a temporary intensification of stress or anxiety is needed to affect belief in conspiracy theories, indicating a relatively fast effect on belief in conspiracy theories. While this temporary intensification may increase belief in conspiracy theories, an increased belief in conspiracy theories resulting from short-duration, experimentally induced distress may not be sustained over longer periods of time, as found with longitudinal research indicating that belief in conspiracy theories is quite stable (Williams, Ling, et al., 2022). Therefore, sustained exposure to elevated levels of anxiety, depression, and stress is likely required before an individual might change their beliefs. A longitudinal design allows for measuring the cause (anxiety, depression, and stress) as it occurs naturally over a long period of time. This avoids the assumption that these effects occur quickly, from brief exposure to elevated levels of anxiety, depression, and stress.

Longitudinal research is scarce in the literature dealing with belief in conspiracy theories. Pilch et al. (2023) found in a systematic review of all conspiracy belief research from 2018 to 2021 that longitudinal designs comprised only 5.5% (23 out of 396) of all studies. Only a handful of longitudinal studies (see Table 5) have considered the relationship between belief in conspiracy theories and anxiety, depression, and stress (Chan et al., 2023; Green et al., 2023; Heiss et al., 2021; Leibovitz et al., 2021; Liekefett et al., 2023), and the current study has the largest number of measurement occasions. The scarcity of longitudinal research is a crucial limitation in research on belief in conspiracy theories (see Douglas & Sutton, 2023; Prooijen & Imhoff, 2022) and longitudinal designs could provide valuable insights into cross-validating experimental research (Sassenberg et al., 2023).

Table 5

Longitudinal Studies using Measures of Anxiety, Depression, Stress, and Conspiracy Beliefs

Study	**Time Interval (between waves)**	**Waves (timepoints)**	**Time Span (total time)**
Adamus et al. (2025)	~ 9 months	3	18 months
Ballová Mikušková & Teličák (2024)	~ 9 months	3	18 months
Chan et al. (2023)	2 weeks	5	2.5 months
Green et al. (2023)	3 months*	3	7 months
Heiss et al. (2021)	2 months	2	2 months
Leibovitz et al., (2021)	1 month	2	1 month
Liekefett et al. (2023) Study 1	2 weeks	4	2 months
Liekefett et al. (2023) Study 2	4 months	4	18 months
Samayoa et al. (2025)	Variable**	4	~ 13 months
Current Study	1 month	7	7 months

Note. *Green et al. (2023) had inconsistent intervals: Wave 1: June–July; Wave 2: August–September; Wave 3: December–January. Samayoa et al. (2025) collected relevant data across four specific waves: Wave 1 (March 23–28, 2020), Wave 2 (April 22–May 1, 2020), Wave 3 (July 9–23, 2020), and Wave 5 (March 24–April 20, 2021), resulting in variable intervals between waves.

Marsden Project

The following study forms part of an overarching 2-year longitudinal project led by Dr Matt Williams and supported by a Marsden Fund Fast-Start Grant. The availability of external funding (via the Marsden) allowed for a longitudinal design of this size, which is not typically financially viable for Doctoral students.

Procedure

All participants were recruited from the crowdsourcing platform Prolific. While commercial research platforms are available in New Zealand (e.g., Dynata), they are significantly more expensive, whereas Prolific offered more affordable pricing. Despite being more affordable, Prolific has been found to have low attrition rates in previous longitudinal studies (Kothe & Ling, 2019; Williams, Ling, et al., 2022). Prolific as a platform is also grounded in providing an ethical platform for participants by providing fair pay and working conditions. All participants consented after reading an information sheet outlining the study (see our OSF page for full copies of the survey).

Recruitment and Inclusion Criteria

The sample was drawn from participants aged 18 and over, living in the United Kingdom, Australia, and New Zealand. These countries were selected for three main reasons. First, sampling participants from New Zealand alone through Prolific would not have provided a sufficient sample size. Adding participants from Australia and the United Kingdom allowed a sufficiently large sampling frame. Second, the wider team working on the overarching Marsden project during the planning phase were based in all three countries, which assisted in making the survey linguistically and culturally relevant to all participants. Lastly, all three countries are English-speaking Commonwealth countries with closely connected histories, and the content of conspiracy theories is likely to be relevant in all three countries.

Screening Procedure

All of the following were used as pre-screening criteria during the *screening procedure* to determine who the study was advertised to, and only applied during the screening phase. In Prolific, this specific pre-screening procedure is set by the researcher and filters the available participants. For all three countries, the study was only advertised to participants with an approval rate of 95% (Note: Prolific operationalises the approval rate as the upper limit of the 95% confidence interval based on a participants prior submissions).

Two additional pre-screening criteria were applied specifically to the UK sample. These additional UK prescreening criteria were only applied to the UK sample due to a larger available sampling pool, with 3,000 participants in Australia and New Zealand active on the Prolific platform over the past 90 days versus 40,000 participants for the UK. Therefore, applying additional strict criteria to the Australian and New Zealand samples would have made our target samples less likely to be fulfilled. Firstly, UK participants were required to have completed at least 20 prior studies on Prolific. These criteria were designed to ensure participants had a sufficient level of engagement with the platform. Previous longitudinal research using Prolific by Williams et al. (2022) demonstrated empirically that participants with a greater number of prior completions were less likely to drop out of the study. Secondly, gender-balancing was applied to the recruitment of UK-based participants (e.g., 50% of available spots were advertised for women).

The screening procedure was advertised on Prolific on 13 September 2022 with two questions. The first question[2] measured belief in conspiracy theories (Lantian et al., 2016), and the second asked whether potential participants would be interested in completing future longitudinal surveys. The screening survey was advertised separately on Prolific to participants currently living in the UK (target $N = 1500$), Australia (target $N = 1500$) and New Zealand (target $N = 350$). These numbers were different between countries due to the available sampling pool accessible on Prolific.

We initially screened potential participants intending to use this information to uniformly sample across the range of beliefs in conspiracies (from participants' responses to the Lantian et al., 2016 item). Using this item in the screening process was to help ensure we obtained an adequate sample of people with relatively high levels of belief in conspiracy theories. However, the distribution of responses consisted of more participants who agreed with the item than those who disagreed. For example, in the screening survey, 11% of participants strongly disagreed, 33% moderately disagreed, 42% moderately agreed, and 14% strongly agreed. Upon completing the screening step, we decided not to sample participants based on responses to this item, as this would reduce the proportion of participants who agreed with the item.

[2] The question was from Lantian et al. (2016), "I think that the official version of the events given by the authorities very often hides the truth."

After excluding participants who responded with “no” or “unsure” when asked if they wanted to participate in future longitudinal surveys ($N = 124$), the final screening procedure identified 1471 potential participants in the UK, 707 participants in Australia, and 210 in New Zealand who were eligible to receive invitations to the main study (for further information regarding the screening procedure, see the preregistration here: https://osf.io/5k4yb).

Sampling Procedure

Wave one: The first wave of the survey was released on 3 October 2022 (see Table 6). The aim was to recruit 450 participants from the UK, 400 from Australia and 150 from New Zealand, for a total of 1,000 participants. The smaller target sample size for New Zealand was informed by previous experience (e.g., Fox & Williams, 2023; Williams, Anderson, et al., 2022; Williams, Ling, et al., 2022) with recruiting participants and monitoring of the realistic available sample pool of Prolific participants in these countries. Eligibility for the study was determined via custom-allow lists that included the Prolific IDs of all participants identified in the screening step who met the designated criteria (i.e., country) and were interested in participating in future longitudinal surveys. These IDs are unique identifiers of participants who complete studies on Prolific and were obtained via responses in the screening survey.

The survey for wave one was advertised on Prolific, and participants were directed from Prolific to the survey hosted on Qualtrics. A brief information sheet provided details about the study, including that there would be 89 questions in total, taking approximately nine minutes, and that participants would be paid a small reward of GBP1.65 (~3.28NZD) to compensate for their time. Upon completing the survey, participants were debriefed and provided links to relevant mental health services within their country. Seven days were allocated for wave one data collection. However, all quotas for each country were filled within 3 days (see Table 6).

Subsequent waves: After the completion of the first wave, we applied the exclusion criteria to check which participants would be invited back for subsequent surveys. The survey from waves two to six was slightly shorter, as the demographic items were removed for a total survey of approximately 65-75 questions. The survey was estimated to take around seven minutes to complete, and participants were paid GBP1.30 (~2.58NZD). The survey's median completion times at each wave were all less than 7 minutes (except for wave 2: 7:19

minutes). All measures were randomised for participants at the block level (i.e., entire measures), and items within each block were randomised (i.e., the questions within the measure). Items for the belief in conspiracy theory measure were manually randomised at the start of each wave due to issues with complexity in Qualtrics related to open-ended questions (attached to another related Marsden project using the same survey).

For each subsequent wave, the survey was made available for 7 days, from the 3rd to the 10th of each month (see Table 6). A reminder message was sent to participants who had not completed the survey (within three days of closing). Once each survey was complete, preliminary checks were made to identify whether participants had failed an attention check or completed the survey too quickly. Those who met the exclusion criteria were sent a message via the Prolific platform to gently remind them to pay careful attention in the next wave, but all participants who completed the survey were still compensated. Participants were still invited to participate in subsequent waves if they missed a wave.

Table 6

Dates of Survey Waves

Wave	Survey open date	Survey close date
1	3 October 2022	5 October 2022*
2	3 November 2022	10 November 2022
3	3 December 2022	10 December 2022
4	3 January 2023	10 January 2023
5	3 February 2023	10 February 2023
6	3 March 2023	10 March 2023
7	3 April 2023	10 April 2023

Note. *The survey at wave one met its quota much more quickly than the remaining waves due to being open to a wide sampling frame. Subsequent surveys were open only to those who had completed a survey at wave one, needing the full 7 days.

Participants

Sample Size Determination

The main analyses used multiple indicator random intercept cross-lagged panel models (RI-CLPMs). Estimating power for this model is difficult given that no closed-form formulae approach has been developed in the statistical literature. One option is to use simulations to estimate power, but several obstacles to this approach limit its feasibility. First, the complexity of the multiple indicator RI-CLPM means that each model would take at least a minute to run. To develop a reliable power estimate, thousands of simulations would have been necessary, taking days of uninterrupted computer time. Second, the number of different models exacerbated the computational issues involved in estimating power. Lastly, there are an extensive number of nuisance parameters in every multiple indicator RI-CLPM (e.g., factor loadings, autoregressions, covariances, variances). Not all of these can be estimated, and the results of simulated power analyses are likely to depend heavily on arbitrary inputs for these parameters.

Therefore, two key considerations were used to determine the sample size and time points needed for appropriate power:

First, power analysis for a single-indicator RI-CLPM (powRICLPM; Mulder, 2022) suggested that an estimated sample of 500 participants over seven waves would have 84% power to detect an estimated small cross-lagged effect of $B = .20$. This is assuming an intraclass correlation coefficient (ICC) of 0.9 along with large autoregressive effects ($B = .70$). This ICC and autoregressive effect is similar to that found in a descriptive longitudinal study over six months of belief in conspiracy theories by Williams, Ling et al. (2022). Therefore, we set the target sample size as 1,000 to ensure the study would be sufficiently powered, even when accounting for substantial participant attrition.

Second, Liekefett et al. (2023, Study 2) conducted a four-wave study over one year, estimating the reciprocal cross-lagged effects (using a latent RI-CLPM; see Figure 3 in Mulder and Hamaker, 2021) of anxiety, uncertainty aversion, and existential threat on belief in conspiracy theories. Their sample size at wave one was 1,012, declining to 437 at wave 4. Despite this relatively high attrition, the standard error for cross-lagged effects in the single indicator RI-CLPMs was consistently relatively small in their models (e.g., SE of 0.04 and 95% CI of [-0.07, 0.11] for the standardised cross-lagged effect of anxiety on belief in conspiracy theories). These findings suggested that a sample size of approximately 1,000, with substantial attrition, would still allow estimation of cross-lagged effects reasonably precisely, even with four waves. We used seven waves, which provided additional assurance that we had the appropriate power to test our hypotheses.

Exclusion Criteria

Exclusions at the Study Level: Participants who met any of the following criteria were excluded entirely from the study (i.e., they were not invited to participate in subsequent waves).

1. Participants who indicated a country other than Australia, New Zealand, or the UK at wave one were excluded. (Note: residence was not checked in subsequent waves, allowing participants to move throughout the longitudinal study.)
2. Participants who failed either attention check at wave one were excluded and not invited back for subsequent waves. Participants were excluded to ensure the main sample to be invited throughout was relatively attentive.
3. Participants with a study duration of less than 178 seconds (total number of items (89) multiplied by two) in wave one were excluded. This was informed by experimental evidence from Huang et al. (2012) to detect unreliable responses. Participants were considered unlikely to have read each question accurately if they had responded in less than two seconds per question.
4. Participants who did not reach the endpoint or timed out of the survey at wave one were excluded.
5. Participants who reached the end of the wave one survey but did not reach the endpoint of any of the remaining waves were excluded. Participants who did not complete any subsequent wave effectively provide no relevant information for data analysis.
6. Participants who "returned" their wave one submission (i.e., revoking consent) were excluded.

Exclusions at the Wave Level: Participants who met any of the following criteria at a specific wave were excluded from that wave only (i.e., their data was treated as missing) but were invited back for subsequent waves.

1. Participants who failed any of the attention checks in each wave were excluded.
2. Participants with a survey duration of less than the total number of items (65) multiplied by two (130 seconds) were excluded.
3. Participants who returned their submission on Prolific, indicating they revoked consent, were excluded.

4. If duplicate submissions from the same participant to the same wave were detected by their Prolific ID, the most complete response was retained. If both were fully complete, the most recent was retained.
5. Participants with a Qualtrics status other than 0 (normal response) were excluded. These excluded responses primarily related to survey tests and previews.

Sample after the Exclusion Process

Due to discrepancies between Prolific and Qualtrics' counting of incomplete surveys, the sample size at wave one before the exclusion process was 1,003. The exclusion criteria included two steps: exclusions at the study level (e.g., removed from the entire study) and at the wave level (e.g., removed from a specific wave). At wave one, eight participants were removed at the study level (leaving 995), and an additional 25 participants did not complete any surveys past wave one, resulting in 970 participants completing two or more waves. The remaining exclusions at the wave level included eight removals at wave two, 17 at wave three, 14 at wave four, 27 at wave five (a minor technical issue with Qualtrics, which resulted in a large number of blank submissions), eight at wave six and 16 at wave seven. After the exclusion process, 970 participants completed at least two survey waves, and 654 completed all seven waves, exceeding the target implied by the power analysis ($N = 500$). The number of participants who completed each wave is presented in Table 7.

Table 7

Summary of Participants Returning at Each Wave

Number of waves responded to	*n of participants*	Percent of final sample ($N = 970$)
7	654	67.4
6 or more	773	79.7
5 or more	842	86.8
4 or more	889	91.6
3 or more	929	95.8
2 or more	970	100

Note. Total sample size after exclusions.

Demographic Characteristics of Participants

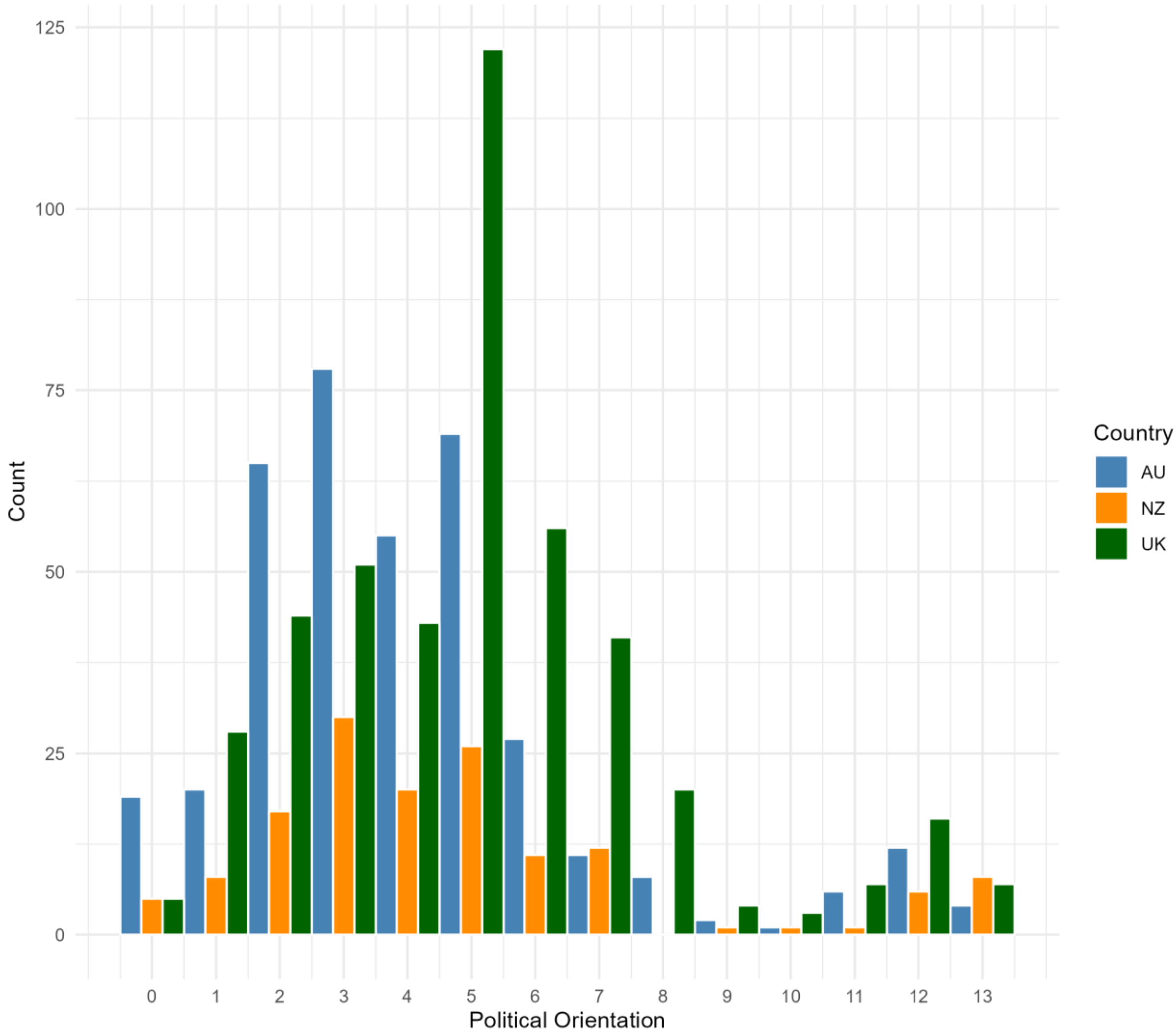
Demographic information is provided in Table 8 for the overall sample and per country. Across the total sample, 54.4% of participants identified as women, 44.4% as men, 0.9% as non-binary (indicated via free text response), and two participants preferred not to respond. By country, the New Zealand sample had slightly more women (60.3%) compared to the Australian sample (55.7%) and the UK sample (51.5%). The UK sample had a gender split applied during the screening recruitment process, which explains the relatively equal gender proportion. Overall, participants were generally highly educated: 41% had at least an undergraduate degree. The overall mean age was 40.3 years (SD = 13.1). Per country, the mean ages were similar for Australia (M_{age} = 37.5, SD = 13.2) and New Zealand (M_{age} = 37.2, SD = 12.7) and slightly older in the UK (M_{age} = 43.8, SD = 12.3). The median employment status of the last three months was employed in full-time work, with median incomes for each subsample[3] being \$60,000 - \$79,999 (AUD), \$40,000 - \$59,999 (NZD), and £20,000 - £29,999 (GBP).

Regarding political orientation, participants were asked, "In politics, people sometimes talk about the 'left' and the 'right'. Where would you place yourself on a scale from 0 to 10, where 0 means the extreme left and 10 means the extreme right?" Responses indicated a slight left-leaning orientation: the median score was 4, with 50.4% of participants selecting values from 0 to 4. 39.7% selected scores from 5 to 10, and 9.9% chose 'don't know' options (e.g., unsure or unfamiliar with the terms). Political orientation varied by country: Participants from Australia and New Zealand tended to respond similarly to the left. However, the UK participants tended to be more evenly distributed (see Figure 2).

[3] Participants were shown region specific currency after selecting their country.

Figure 2

Endorsement of Political Orientation Across Countries



Note. 0 = extreme left, 10 = extreme right, 11 = don't know, 12 = haven't thought much about this, 13 = I do not know the meaning of the terms “left” and “right”.

Table 8

Demographic Characteristics of Participants (Time 1)

	Australia (*n* = 377)		New Zealand (*n* = 146)		United Kingdom (*n* = 447)		Total (*N* = 970)	
	n	%	*n*	%	*n*	%	*n*	%
Gender								
Man	161	42.7	57	39.0	213	47.7	431	44.4
Woman	210	55.7	88	60.3	230	51.5	528	54.4
Non-binary	5	1.3	1	0.7	3	0.7	9	0.9
Prefer not to say	1	0.3	0	0.0	1	0.2	2	0.2
Age								
18-24	57	15.1	27	18.5	15	3.4	99	10.2
25-34	125	33.2	40	27.4	102	22.8	267	27.5
35-44	96	25.5	39	26.7	142	31.8	277	28.6
45-54	54	14.3	24	16.4	90	20.1	168	17.3
55-65	28	7.4	11	7.5	71	15.9	110	11.3
65-74	13	3.4	4	2.7	25	5.6	42	4.3
75+	4	1.1	1	0.7	2	0.4	7	0.7

	Australia (n = 377)		New Zealand (n = 146)		United Kingdom (n = 447)		Total (N = 970)	
	n	%	n	%	n	%	n	%
Highest level of completed education								
Doctoral degree (e.g., PhD, PsyD, MD)	25	6.6	5	3.4	9	2.0	39	4.0
Postgraduate degree (e.g., Masterate)	73	19.4	25	17.1	68	15.2	166	17.1
Undergraduate degree (e.g., Bachelor's)	146	38.7	69	47.3	183	40.9	398	41.0
Other tertiary qualification	65	17.2	19	13.0	55	12.3	139	14.3
Completed high school	59	15.6	25	17.1	121	27.1	205	21.1
Some high school (without completing)	9	2.4	3	2.1	10	2.2	22	2.3
No high school	0	0	0	0	1	0.2	1	0.1
Employment status								
Working full-time	185	49.1	76	52.1	267	59.7	528	54.4
Working part-time	86	22.8	30	20.5	97	21.7	213	22.0
Unemployed and looking for work	25	6.6	7	4.8	9	2.0	41	4.2

	Australia (*n* = 377)		New Zealand (*n* = 146)		United Kingdom (*n* = 447)		Total (*N* = 970)	
	n	%	*n*	%	*n*	%	*n*	%
A homemaker or stay-at-home parent	21	5.6	5	3.4	22	4.9	48	4.9
Student	33	8.8	17	11.6	8	1.8	58	6.0
Retired	14	3.7	4	2.7	34	7.6	52	5.4
Other (employment)	13	3.4	7	4.8	10	2.2	30	3.1
Marital status								
Married	109	29.0	51	34.9	232	51.9	392	40.4
Living with a partner	68	18.1	36	24.7	92	20.6	196	20.2
Widowed	4	1.1	1	0.7	5	1.1	10	1.0
Divorced/separated	36	9.6	7	4.8	37	8.3	80	8.2
Never married	159	42.3	51	34.9	81	18.1	291	30.0

Note. All demographic questions can be found in the full Qualtrics survey in the OSF project.

Measures

The following measures were used in all survey waves. As longitudinal research requires participants to complete each survey numerous times, a primary criterion for selecting each measure was selecting brief measures with sufficient psychometric evidence to accurately measure each construct at different time points. Full copies of all measures used in the current study, as well as the full Qualtrics survey, can be found in the supplementary folder on the Open Science Framework.

Conspiracy Beliefs and Mentality

Two measures of conspiracy beliefs were used – a measure of belief in specific conspiracy theories and a measure of a conspiracist mentality. Both measures were used for several reasons. First, some researchers argue that belief in conspiracy theories and conspiracy mentality are different constructs (e.g., Imhoff et al., 2022). Second, measuring belief in specific conspiracy theories allows for specific insight into unwarranted conspiracy theories, aligning with how we defined the type of conspiracy theory we were most interested in. This is compared to the conspiracist mentality, which is more of a measure of the general tendency to believe in conspiracy theories, with no distinction between unwarranted or warranted conspiracy theories. The two measures can be considered slightly different in that one measures the tendency to believe unwarranted conspiracy theories, and the other measures the tendency to believe conspiracy theories in general.

Belief in Conspiracy Theories

Belief in conspiracy theories (BCT) was measured with 11 specific conspiracy theories. The items are found in Table 9, and all were intended to address *unwarranted* conspiracy theories unsupported by empirical evidence. While established measures with some psychometric evidence for belief in specific conspiracy theories currently exist (e.g., Belief in Conspiracy Theory Inventory; Swami et al., 2017), they contain relatively outdated conspiracy theories (e.g., The assassination of Martin Luther King, Jr., was the result of an organised conspiracy by US government agencies such as the CIA and FBI). Therefore, we used a measure that focuses on contemporary conspiracy theories.

The belief in conspiracy theories measure was based on a 10-item measure of unwarranted conspiracy theories constructed initially by Williams et al. (2022) to meet the following requirements:

1. Each item must align and describe critical components of a conspiracy theory (e.g., a claim involving multiple actors, secrecy, and malevolence). This was to ensure the content validity of items.
2. The conspiracy theory is well known. Specifically, the theory had to be at least mentioned in a mainstream media article or have its own Wikipedia page. This reduced the chance of participants being unfamiliar with theories.
3. The claimed event has occurred in the last 20 years or is ongoing (e.g., New World Order; Spark, 2000). This helped to ensure that items have contemporary relevance.
4. The item needs to be clear and easy to read.

Williams et al. (2022) selected each item in an iterative process, where items were revised and reviewed until each team member was satisfied that every item met each of these requirements. All items were included to refer to unwarranted conspiracy theories, unsupported by current empirical evidence. The Williams et al. (2022) version showed good internal consistency reliability ($\alpha = .86$) and strong test-retest correlations across seven waves ($r = .86 – .96$).

Prior to data collection for the current study, a second round of revisions was conducted to ensure contemporary relevance, as the original item selection was finalised in early 2021. Since then, the salience of certain conspiracy theories has declined, and updates were necessary to maintain content relevance.

First, the item “COVID-19 vaccines contain microchips to monitor and control people” was removed, as while this item may have been of contemporary relevance in early 2021, given the enforcement of vaccine mandates, by late 2022, this was less of an issue. Second, the item “The collapse of the World Trade Centre on Sept 11, 2001 was caused by controlled demolitions arranged by US government insiders” was removed due to not having occurred in the last 20 years. The emphasis on contemporary relevance was aimed at increasing the plausibility of seeing fluctuation in these beliefs over time. The two removed items were replaced with three new items (see items 4, 7, and 11 in Table 9) and were perceived to meet the four requirements initially set by Williams et al. (2022).

Participants responded to each item on a five-point scale, with options of strongly disagree = 1, somewhat disagree = 2, neither agree nor disagree = 3, somewhat agree = 4 and strongly agree = 5. Using a conventional agree–disagree response scale was intentional to ensure a balanced response scale for each item and not inflate the level of agreement to conspiracy theories (for benefits of this approach, see Sutton & Douglas, 2020). A conspiracy belief score was created by calculating the mean of participants' responses to all items (possible range 1 to 5). Cronbach's alpha at time 1 was .92.

Table 9

Items in the Belief in Conspiracy Theories Measure

	Item	Source
1.	COVID-19 is a biological weapon intentionally created and released by China.	Adapted from Miller (2020)
2.	A powerful and secretive group, known as the New World Order, are planning to rule the world.	Adapted from Swami et al. (2011)
3.	Telecommunication companies are covering up the health risks of the 5G cellular network.	Adapted from Marques et al. (2021)
4.	I think that the government wants to limit the rights and freedoms of citizens using the pretext of fighting the COVID-19 pandemic.	Adapted from Oleksy et al. (2021)
5.	The trails left behind airplanes are toxic chemicals released as part of a secret government programme.	Adapted from Oliver and Wood (2014)
6.	Fluoride is added to the water supply by governments to make people less intelligent and easier to control.	Adapted from Marques et al. (2021)
7.	The claim that the climate is changing due to emissions from fossil fuels is a hoax perpetrated by corrupt scientists who want to spend more taxpayer money on climate research.	Adapted from Lewandowsky et al. (2013)
8.	Vaccines are harmful, and this fact is covered up by governments and pharmaceutical companies.	Adapted from Jolley and Douglas (2014b)
9.	Democrats stole the 2020 US Presidential election from Donald Trump by creating fraudulent ballots.	Paraphrased from the Wikipedia page (*Attempts to Overturn the 2020 United States Presidential Election*, 2023)
10.	Pharmaceutical companies ("Big Pharma") know of a cure for cancer, but they are keeping it secret to protect their profits.	Constructed for this study
11.	Governments and agricultural businesses are hiding evidence that genetically modified organisms (GMOs) harm human health.	Constructed for this study

Note. Participants were asked, "Please indicate the extent to which you agree with each of the statements below. Please answer carefully and honestly; we're interested in what you really think."

Conspiracy Mentality

A general conspiracy mentality was measured using the 5-item Conspiracy Mentality Questionnaire (CMQ; Bruder et al., 2013) to assess the general tendency to believe in conspiracy theories. It is a widely used measure of conspiracy beliefs and has established psychometric evidence (Bruder et al., 2013; Ćirović & Pedović, 2025). Bruder et al. provided evidence for the convergent, discriminant, and predictive validity of the CMQ upon development. Additionally, longitudinal research with four time points over two months by Liekefett et al. (2023) found a large $r = .80$ intraclass correlation coefficient, indicating it is a reliable measure of the construct.

The CMQ measures belief in conspiracy theories indirectly by asking questions that capture the general propensity of participants to attribute the outcome of certain events to conspiracies (Bruder et al., 2013). Each question is prefaced with "I think that", and an example question is, "Events which superficially seem to lack a connection are often the result of secret activities." Participants respond to each item on an 11-point scale with options from 0 (0% = certainly not) to 10 (100% = certain). A conspiracy mentality score was created by calculating the mean of participants' responses to all items (possible range 0 to 11). Cronbach's alpha at time 1 was .86.

Depression and Anxiety

The measures of depression and anxiety were selected because of their clinical relevance and also to build on recent developments to establish common measures in mental health. The Common Measures in Mental Health Science (CMMHS) Initiative undertook extensive consultation with research funders (e.g., National Institute of Mental Health and Wellcome Trust), medical journals (The Lancet Psychiatry and JAMA Psychiatry), measurement experts, and international researchers, with the aim of building a shared framework for measurement in mental health research (see Farber et al., 2023). Following an extensive consultation process, two key measures were identified for depression and anxiety, the Patient Health Questionnaire-9 (PHQ-9) and General Anxiety Disorder-7 (GAD-7) – aligning with other extensive working group recommendations (Obbarius et al., 2017). These measures have key features of being relatively brief, freely available, commonly used by researchers, and demonstrating the capability to measure the intended construct while being relevant and meaningful in mental health contexts.

While the PHQ-9 and GAD-7 were identified by the CMMHS as common measures, we do not view them as capturing the full range of experiences of a given condition, and it does not mean they are superior measures, as, in general, more needs to be done to improve the measurement of depression and anxiety (see Flake & Fried, 2020). Instead, it is the first step in adopting standardised measures, which are the most widely used.

Depression

Depression was measured using the 8-item Patient Health Questionnaire (PHQ-8), a useful measure of depression severity in the general population (Kroenke et al., 2009). The PHQ-8 differs from the PHQ-9 by excluding an item about thoughts of suicide. The item regarding suicide in the PHQ-9 was omitted due to not being able to provide appropriate and timely support by a mental healthcare worker to a participant if the item was endorsed, posing an ethical risk. However, the PHQ-8 remains appropriate for depression screening in community samples (Shin et al., 2019). The PHQ has been found to have temporal measurement invariance across time, identifying it as a useful measure for longitudinal research (Stochl et al., 2022). The measure asks about participants' depressive symptoms over the previous two weeks, and they respond to each item on a 4-point rating scale with options ranging from not at all, several days, more than half the days, and nearly every day. A depression score was calculated by summing participants' responses to all items (possible range 0 to 24). Cronbach's alpha at time 1 was .90.

Anxiety

Anxiety was measured using the brief 7-item Generalised Anxiety Disorder questionnaire (GAD-7; Spitzer et al., 2006). The GAD-7 has been found to be unidimensional (Shevlin et al., 2022) and demonstrated temporal measurement invariance, identifying it as a useful measure to compare anxiety levels across time (Stochl et al., 2022). The measure asks participants about the degree to which they have felt anxiety symptoms (e.g., worrying, nervous, on edge, and restless) over the previous two weeks. The response format is the same as the PHQ-8. An anxiety score was calculated by summing participants' responses to all items (possible range of 0 to 21). Cronbach's alpha at time 1 was .92.

Stress

Two measures of stress were used. Perceived stress was selected as the primary stress measure used in the current literature, which has tested the relationship of stress with belief in conspiracy theories. Additionally, while most conspiracy belief researchers use subjective measures of stress, we also opted to use an additional objective measure of stressful life events.

Perceived Stress

Perceived stress was measured using the 10-item Perceived Stress Scale (PSS; Cohen et al., 1983). The measure asks questions regarding the subject's thoughts and feelings over the past month. However, to align with the GAD-7 and PHQ-8 and to ensure the recall period did not overlap across measurements, we changed the wording to “past two weeks”. Participants responded to each item on a 5-point rating scale with options of never, almost never, sometimes, fairly often, and very often. A perceived stress score was calculated by summing participants' responses to all items (possible range of 0 to 40). Items 4, 5, 7 and 8 required reverse coding. Cronbach’s alpha at time 1 was .92.

Stressful Life Events

Objective stress was measured using 12 questions about stressful life events (SLE). The National Epidemiologic Survey on Alcohol and Related Conditions (NESARC; Lin et al., 2020) initially constructed the measure with items from the List of Threatening Experiences (Brugha et al., 1985) and the Social Readjustment Rating Scale (Holmes & Rahe, 1967). An example item is “In the last month, were you unemployed and looking for a job?” Participants respond on a dichotomous scale with the response options yes = 1 and no = 0.

A stressful life event score was calculated by summing the total responses to the items in the measure (possible range of 0 to 12). The summed score was used for all stressful life event analyses. Cronbach’s alpha at time 1 was .55. The items in this scale are not intended to be indicators of an underlying latent construct that affects all items. Instead, it intends to be a measure that represents the different types of stressful life events (e.g., formative measure; Borsboom, 2008) that the participant has experienced in the last month. To illustrate this, the item “Did any of your immediate family or close friends die?” is unlikely to be highly related to the item “Were you or an immediate family member the victim of any type of crime?” Thus, the low Cronbach’s alpha was expected. Further details regarding the analytic implications of the SLE being a formative measure can be found in the data analysis subsection.

Intergroup Threat

An item was created to measure the salience of an antagonistic outgroup (a key moderator in the existential threat model by van Prooijen, 2020). An established measure of the salience of an antagonistic outgroup does not appear to exist currently, and ambiguities in the way a salient antagonistic outgroup is defined have created a disconnect in how to measure this construct. In addressing these issues, a measure of intergroup threat perception was used to operationalise a salient antagonistic outgroup. The concept of intergroup threat appears closely related to that of a salient antagonistic outgroup (Stephan et al., 2016), given that an antagonistic outgroup would likely be inherently threatening. Additionally, the salience of an antagonistic outgroup relates to an ingroup (those who believe in the claims made by the conspiracy theory) who perceive the specific outgroup who are implied in a conspiracy theory as threatening. Therefore, a specific conspiracy was used to test this effect.

The COVID-19 pandemic is often associated with feelings of existential threat, which may lead some people to believe that COVID-19 is a bioweapon developed by China and intentionally released as an act of biological warfare (see Nie, 2020). A specific outgroup is often implicated in this conspiracy theory – the Chinese government. Therefore, we used item one of the conspiracy theories listed in Table 9 to ensure that a clear outgroup was associated with the conspiracy theory ("COVID-19 is a biological weapon intentionally created and released by China"). This provided an approach to directly test whether high intergroup threat perception moderates the association between perceived stress and belief in a specific conspiracy theory.

The intergroup threat measure consisted of one item, "I feel threatened by the Chinese Government", with a four-point response format ranging from strongly disagree, somewhat disagree, somewhat agree, and strongly agree. The rationale for a four-point response format was to split participants into a low intergroup threat group (strongly and somewhat disagree) and a high intergroup threat group (strongly and somewhat agree). The following data analysis section will explain the rationale for dichotomising the responses.

Attention Checks

Participant inattention and careless responses are possible sources of invalidity in survey-based research (Huang et al., 2012, 2015). One approach to detecting careless responses by participants is to include attention check questions, which act as indicators to identify possible inattentive responses (Curran & Hauser, 2019; Oppenheimer et al., 2009).

Two types of attention check questions were used at each wave – a nonsensical item and an instructional manipulation check (IMC). Two unique items were created for each wave, and these attention checks were created following Prolific's attention check guidelines (Prolific Team, 2022). A complete list of items and participant responses can be found in Appendix A.

Nonsensical

The nonsensical items were created with the intention that an exact response could be justified as objectively correct, and each item did not assume prior knowledge. An example item was "In the last ten years, I have left Earth to visit the moon". In this example, the objectively correct answer was either "strongly disagree" or "moderately disagree," given that no human has set foot on the moon since the Apollo 17 mission in 1972 (NASA, 2024). All nonsensical items used a four-point Likert scale response, with strongly disagree, disagree, agree, and strongly agree, with no neutral response option.

Instructional Manipulation Check

The instructional manipulation checks (IMC) were created to have one correct answer, which participants were explicitly asked to select (e.g., click 'strongly disagree' for this question). For example, an IMC asked participants, "When asked for the last day of the traditional work week, you must select 'Friday'. This is an attention check." Participants were then given five options from Monday to Friday, with Friday being the correct option. The same general structure and response format were applied for each IMC item.

Demographic Details

Participants' demographic details were collected only during wave one for descriptive purposes. These included age, country, gender, level of education, political orientation, income, employment status, current marital status, and ethnicity (see ethics subsection). In addition, participants' anonymised Prolific ID numbers were collected at every wave to ensure we could link responses over time.

Additional Measures in the Survey

As the survey was part of a wider project, there were four other measures included in the survey that were not used in the data analysis. Four additional measures were included in all seven waves – a 4-item measure of trust (Marques et al., 2021), three items from the Socio-Political Control subscale (Paulhus & Christie, 1981), a single item probing sympathy for violent protests (Bhui et al., 2014), and a single item measuring belief in modern medicine (Pennycook et al., 2020). In the first wave only, participants were asked whether they had heard of the specific claims made by each conspiracy theory in Table 9. The second and subsequent waves included an open-ended question if a change in response to each conspiracy theory in Table 9 was detected. The listed measures and text responses were for other research outputs and were not used in the analyses for this study.

Ethical Considerations

A full ethics application was completed to cover the wider Marsden project, of which this formed part. It was submitted in June 2022 and was approved with minor amendments in September 2022 (*Approved by the Massey University Human Ethics Committee: Southern A, Application 22/42)*. The ethics application was drafted with cultural consultation from Associate Professor Natasha Tassell-Matamua, an ongoing cultural advisor for the overarching Marsden project. Associate Professor Matt Williams was the primary investigator for the ethics application, with Associate Professor Stephen Hill and me listed as co-applicants. The wider Marsden team, including all listed investigators, considered and discussed several ethical concerns during the application process. My role in the ethics application included co-drafting the ethics application with Dr Matt Williams, providing input with subsequent edits incorporating feedback, and drafting the amendments needed to approve the application.

Avoidance of Harm

The primary consideration was the possibility of the study pathologising and marginalising people who believed in conspiracy theories. The ramifications of this could lead to stigmatising of those who question authorities and also question whether conspiracies are happening, which are healthy and normal considerations. This research aimed to respect those who believe in conspiracy theories and avoid reinforcing negative stereotypes. Additionally, we were careful to be selective of where the research would be disseminated (e.g., working with reputable journalists) to ensure it was not distributed in a way that would further stigmatise those who believe in conspiracy theories.

Another aspect relates to the sensitive questions relating to mental health, as we asked participants about stress, stressful life events, depression and anxiety, which had the potential to provoke some aversive emotions. However, recent research found that self-report questionnaires are unlikely to cause distress to participants (McMurtrie, 2022; Paing et al., 2023). In the unlikely chance that these questions did create distress, we provided participants with links to mental health services in all three countries in a debrief section at the end of the survey.

Justice

The principle of justice was addressed in that participants were fairly compensated for the burden of their time spent completing the survey. At wave one, participants were paid GBP1.65 (~3.28NZD) for completing a slightly longer survey, which took approximately 9 minutes. For subsequent waves, participants were paid GBP1.30 (~2.66NZD) for surveys that took around 7 minutes of their time. These payments are considered fair and above the amount recommended as a "good" payment on Prolific, and aligned with the 2022 minimum wage in NZ (0.38NZD per minute = 22.80NZD per hour).

Autonomy

All participants were anonymous, consenting adults. However, lengthy information sheets are often not read by participants, which could be a risk to participants' autonomy. In addressing this, we created concise information sheets, and we reiterated some of the most important points in the advertisement to ensure potential participants made an informed decision before providing consent (See OSF page for full survey; link when possible).

Cultural Appropriateness of the Present Study

The present study considers the Te Tiriti obligations and principles specified in Te Ara Tika and the Massey ethics code. While all four principles were addressed, the following are the most relevant to discuss.

In addressing Tika, the purpose of the study was to apply rigorous research methods (longitudinal design with sophisticated data analysis). These methods have been thoughtfully selected to maximise the credibility of future findings. In using Prolific for data collection, we could not guarantee equitable access for Māori participation in our study, given the small pool of participants to recruit from in New Zealand. Using larger companies in New Zealand for data collection could have made this possible, but it was financially unaffordable. However, the purposefulness of the study may indirectly benefit Māori by identifying the risks associated with belief in unwarranted conspiracy theories.

In relation to Mana (justice and equity), we addressed Māori data sovereignty. While we collected ethnicity data, Associate Professor Natasha Tassell-Matamua was the only person with access to the ethnicity variable to run ethnicity-based analyses for New Zealand participants. Additionally, all data uploaded to the Open Science Framework did not include the ethnicity variable. This approach minimised the risk of inappropriate statistical inferences being made about Māori from subsequent analyses, and ensured that data pertaining to Māori remained under appropriate governance, in alignment with the principles of Māori data sovereignty.

Data Analysis

All data analysis was completed using RStudio (version: 12.0+343) and the R programming language (version: 4.0.2) (R Core Team, 2021). All confirmatory analyses and exclusion criteria were preregistered prior to data collection. A range of packages was used in data preparation and analysis. All main analyses used the lavaan package (Rosseel, 2012). Additional packages used for data management, processing, description, and visualisation included tidyverse (Wickham et al., 2019), ICC (Wolak et al., 2012), psych (Revelle, 2022), here (Mueller, 2017), likert (Sarstedt et al., 2020), and bain (Gu et al., 2019).

The data analysis procedure consisted of three steps. First, the raw data was deidentified, and all subsequent analyses used a deidentified data set. Second, the exclusion criteria were applied, along with preparing the data for the main analyses. Lastly, the main analyses were completed. All deidentified data and scripts for the last two steps can be found in the OSF project folder.

All hypotheses were tested using a type of longitudinal structural equation model known as the random intercept cross-lagged panel model (RI-CLPM), which is an extension of the traditional cross-lagged panel model (CLPM). The following is a brief introduction to the CLPM and RI-CLPM before outlining the nine preregistered models to the 15 hypotheses.

Cross-lagged Panel Model

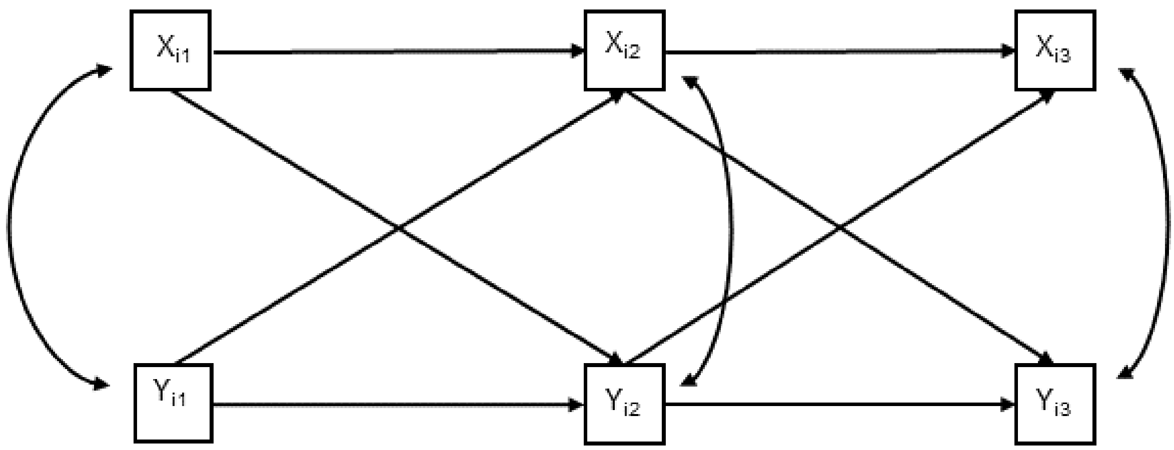
The cross-lagged panel model (CLPM) is a popular longitudinal modelling approach to understand how variables interact with each other over time. An example of a traditional CLPM is illustrated in Figure 3, which is a model with three time points that test for cross-lagged effects (e.g., $X_{i1} \rightarrow Y_{i2}$ and $Y_{i1} \rightarrow X_{i2}$). The cross-lagged effects are estimated by controlling for the other variable. For example, the cross-lagged effect of X on Y is estimated after controlling for baseline levels of X at time 1 (see Kline, 2015). A key benefit of this modelling approach is the ability to establish temporal precedence, establishing cause before effect.

There are important limitations to the CLPM, particularly in its ability to make appropriate causal inferences (see Hamaker et al., 2015; Lucas, 2023). A key aspect of this critique is the inability to differentiate *between-person effects,* which can be described as trait-like stable between-person differences, and *within-person effects,* which can be described as state-like differences that may fluctuate over time. The CLPM cross-lagged effect can be thought of as a combination of both between and within-person effects.

Numerous researchers have outlined a strong argument for why it is crucial to disentangle between- and within-person effects in longitudinal modelling (see Curran & Bauer, 2011; Hamaker, 2023b; Rohrer & Murayama, 2021; Schuurman, 2023). For example, Hamaker et al. (2015) demonstrated that when using a CLPM, not differentiating between-person and within-person effects can produce biased parameter estimates. This is problematic not only when researchers are interested in within-person processes (e.g., how an individual changes over time) but also when the goal is simply to estimate the effect of one variable on another. If variables have at least some degree of stable between-person variance, then the CLPM will confound these sources of variance, potentially distorting the interpretation of causal direction and magnitude (Hamaker, 2023a; Schuurman, 2023). In response to the limitations of the CLPM, Hamaker et al. (2015) developed the random-intercept cross-lagged panel model, which is described below.

Figure 3

Traditional Cross-Lagged Panel Model



Note. A cross-lagged panel model with two variables, X and Y, and three time points. $_i$ = observed variable, and the second subscript indicates the time of measurement.

Random Intercept Cross-Lagged Panel Model

The RI-CLPM is an extension of the CLPM (see Figure 4) developed by Hamaker et al. (2015). It addresses key limitations of the CLPM by including between and within components. The between-person components of the model (e.g., blue highlighted area) are the random intercepts, and these refer to participants' stable trait-like effects (over the study period). The within-person component (e.g., red highlighted area) refers to individual fluctuations in response to the measured variable at each time point.

The addition of the between- and within-person components is important, as the within-person cross-lagged parameters often do not align with those found in a traditional CLPM (see Hamaker, 2023b; Hamaker et al., 2015). In addition, the between-person and within-person components found in an RI-CLPM can be of opposite value (e.g., a positive between-person effect and a negative within-person effect). Therefore, when the research hypotheses of interest pertain to individual changes over time, the within-person cross-lagged effects are more informative in the context of the current research. The within-person component indicates whether different-from-usual scores for one variable (e.g., depression) will be associated with different-from-usual scores for other variables (e.g., belief in conspiracy theories) at the next time point after controlling for the first variable at the first time point (e.g., belief in conspiracy theories). In comparison, the between-person component provides an understanding of how a participant, on average, compares to other participants.

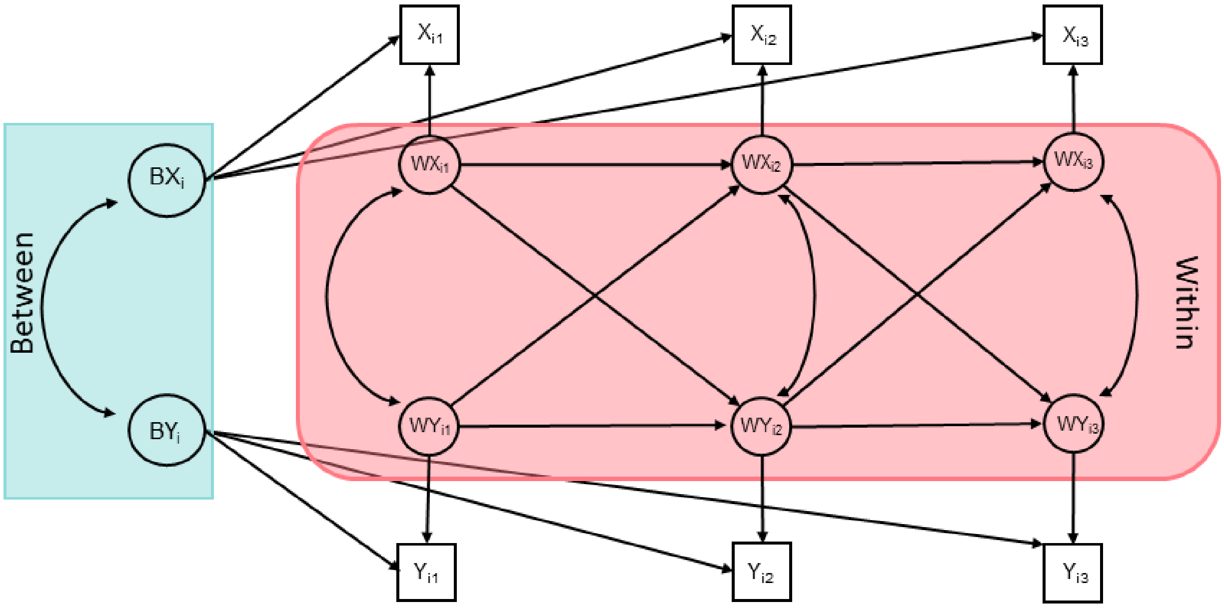
The RI-CLPM has unique benefits in terms of its ability to make causal inferences. Using cross-lagged coefficients enables estimates of the effect of X on Y that are not distorted by a possible effect of Y on X. Additionally, controlling for stable individual difference factors using random intercepts (the squares in

provides the ability to rule out all stable individual difference confounding variables. However, the possibility of time-varying confounding variables cannot be ruled out.

The model in Figure 4 is an example of a single indicator RI-CLPM, with each indicator (e.g., X_{i1}) typically an average or sum score of a measured variable. The limitation of this approach is that it assumes that each variable is measured without error. This measurement assumption is rarely met in psychology. An extension of the single indicator RI-CLPM was developed by Mulder and Hamaker (2021) to address this limitation.

Figure 4

Single Indicator Random Intercept Cross-Lagged Panel Model



Note. $_i$ = observed variable, and number indicates the time occasion.

Multiple Indicator RI-CLPM

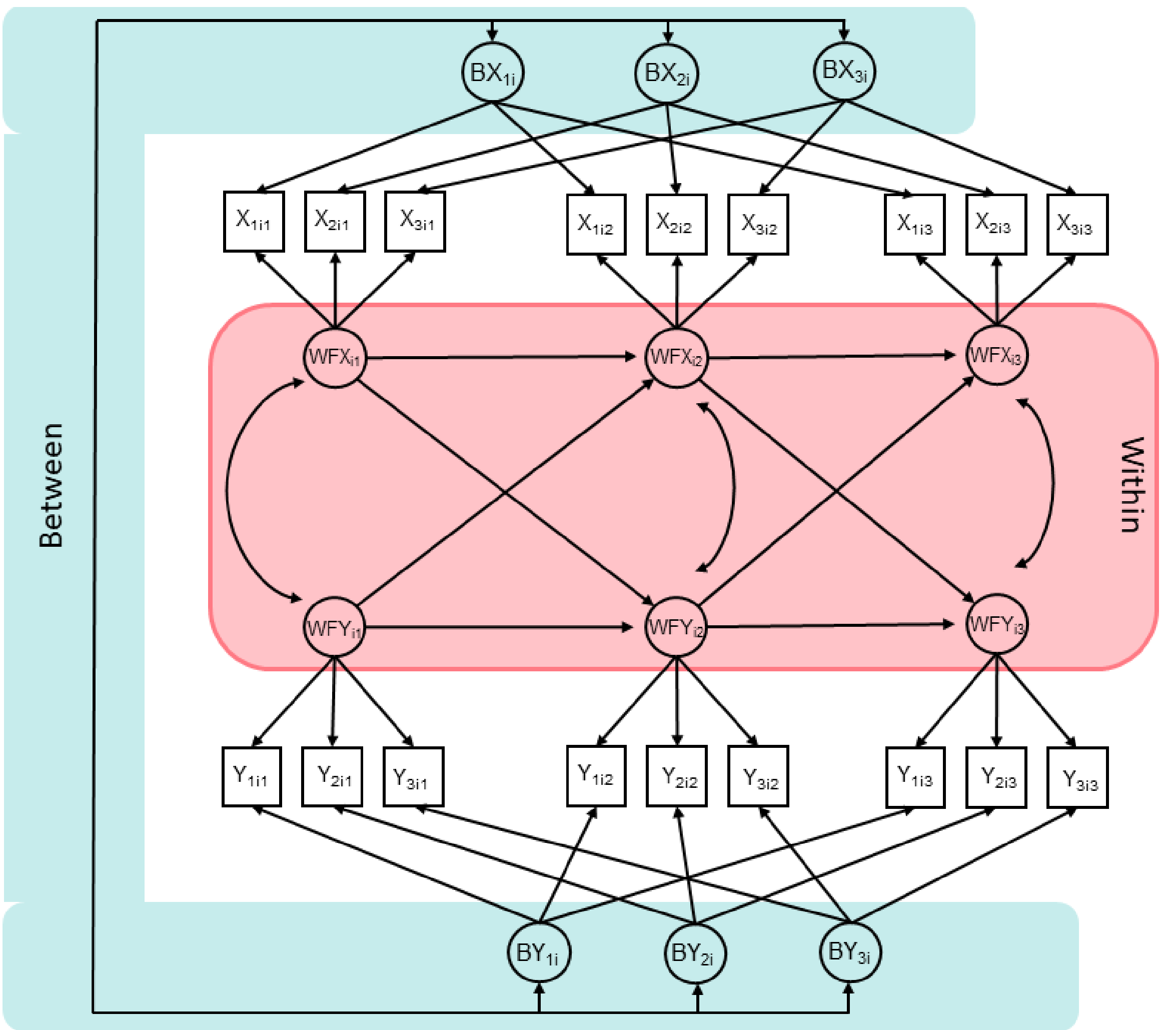
The multiple indicator RI-CLPM, developed by Mulder and Hamaker (2021) and Figure 5 demonstrate features of an example model that includes three time points and two variables. The key difference from the previous model is the addition of multiple indicators (i.e., items – X_{1i1}), and each indicator has its own random intercept (e.g., BX_{1i}). Therefore, in this model, with three items to measure X and three items to measure Y, this results in six random intercepts in total. In addition, the three indicators have a common factor (e.g., WFX_{i1}), which represents the within-person component, resulting in six within-person variables (across X and Y). Therefore, we end up with two parts – a between-person component, which is stable, and a within-person component, accounting for within-person variability over time.

The key benefit of using multiple indicators in an RI-CLPM is the ability to account for measurement error (Mulder & Hamaker, 2021). This eliminates the need to compute the sum or mean scores, as in a single indicator model. Accounting for measurement error is valuable, as it provides greater power to detect an effect and reduce Type I error rates (Sardeshmukh & Vandenberg, 2017; Westfall & Yarkoni, 2016). Failing to do so can bias lagged-parameter estimates (Mulder & Hamaker, 2021).

With this in mind, the majority of hypotheses were tested using multiple indicator RI-CLPMs. Each model tested pairs of constructs in separate models (e.g., depression and belief in conspiracy theories in model one, for H1 and H5). Using separate models instead of one combined RI-CLPM helped to avoid the risk of convergence failure in overly complex models. In total, nine models were preregistered.

Figure 5

Multiple Indicator Random Intercept Cross-Lagged Panel Model



Note. $_{i}$ = observed variable, and number indicates the time occasion.

Multiple Group RI-CLPM

Testing for moderating effects (H8) within a structural equation modelling (SEM) framework using an RI-CLPM has a range of challenges, primarily due to the complexity of the statistical analysis techniques needed to test for these effects appropriately. The solution we found was to use a recent extension of the RI-CLPM, using a multiple-group approach

(Mulder & Hamaker, 2021). This allows testing of group differences in lagged regression coefficients (e.g., the differences in the effect of stress on belief in a specific conspiracy theory in a low intergroup threat group and a high intergroup threat group).

- As illustrated in Figure 6, the multiple-group RI-CLPM is made up of two single indicator RI-CLPMs, essentially the same as the RI-CLPM illustrated in Figure 4.
- The multiple-group RI-CLPM allows for the comparison of group differences in cross-lagged regression coefficients, which is described by Mulder and Hamaker (2021) as a test of moderation. The key difference is that the multiple group RI-CLPM requires a grouping variable, and two distinct groups are used to test the group differences in lagged regression coefficients. While a limitation of this dichotomising approach results in a loss of information (see Altman & Royston, 2006), it was the most viable option for testing moderation within a longitudinal model.

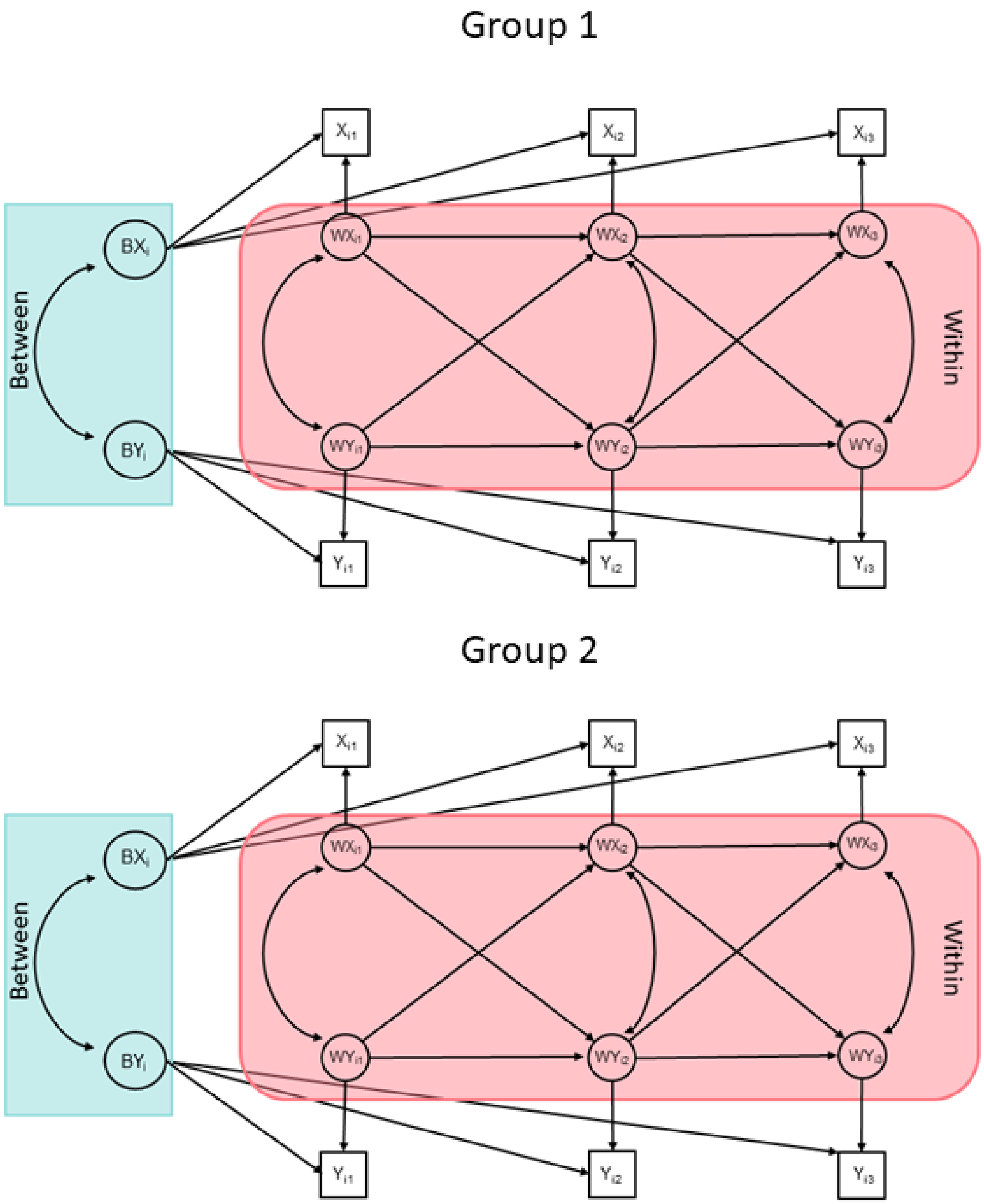
Model 5 was preregistered as a multiple-group RI-CLPM using single indicators to measure intergroup threat differences. The single indicators included two observed variables – perceived stress (summed score) and belief in a single conspiracy theory[4]. Two sub-models were preregistered to test whether intergroup threat perception moderates the effect of perceived stress on belief in a specific conspiracy theory:

- Model 5a: An unconstrained model where all parameters could vary across groups.
- Model 5b: A partially constrained model where all parameters varied across groups, except for the cross-lagged parameter of perceived stress on belief in a specific conspiracy theory.

[4] COVID-19 is a biological weapon intentionally created and released by China.

Figure 6

Multiple Group RI-CLPM



Formative Models

Two models used single indicator RI-CLPMs. These were models three and eight, which tested stressful life events with belief in conspiracy theories and conspiracy mentality. We had no conceptual basis for assuming a latent variable (i.e., reflective construct) underlies “stressful life events”, and interpreted it as a formative construct (see Figure 7). A formative construct means that the indicators (i.e., the items relating to specific stressful life events) are likely the cause of the latent variable of stressful life events (Bollen & Lennox, 1991;

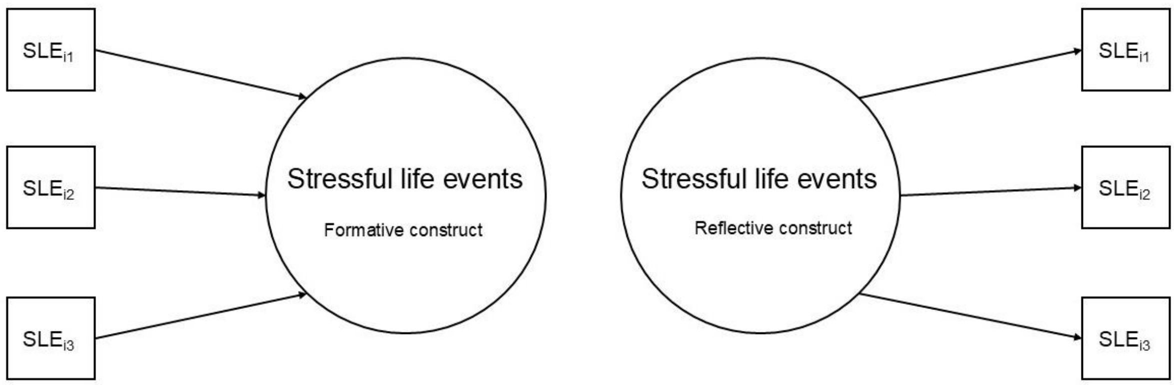
Hanafiah, 2020). In contrast, if it were a reflective construct, the specific stressful life event items are affected by a latent variable, “stressful life events” (Schmittmann et al., 2013).

To illustrate this, the two items “Did any of your immediate family or close friends die?” and “Have you had trouble with your boss or a coworker?” are unlikely to be closely related and represented as a latent variable, but both are important contributors to the overarching construct of recent life stress. As Borsboom (2008) argued, in formative models, the construct does not exist independently of its indicators: it is defined by them. Therefore, it seems more likely that it is a formative construct that can be represented as the sum score of all the stressful life event items.

The preregistered model was to input the stressful life event variable as a formative construct (i.e., a single indicator) and belief in conspiracy theories as a multiple indicator latent variable, as a half single, half multiple indicator RI-CLPM. However, despite repeated attempts, the model could not be estimated using the lavaan package due to limitations in handling this specific combination of formative and reflective constructs with a random intercept structure. As a result, a single-indicator model (similar to Figure 4) was used for both stressful life event models. The same limitations apply to this model, as outlined in the single-indicator model subsection.

Figure 7

Differences Between Formative and Reflective Stressful Life Event Constructs



Estimation Method

All of the models mentioned above were estimated using full information maximum likelihood (FIML) using the lavaan package. The use of FIML estimation prioritises accounting for missing data (see Enders & Bandalos, 2001 for a complete description), which was a substantial concern given the use of a longitudinal design (e.g., participants who missed an entire survey wave but completed other waves). In contrast, other available estimators can only function with complete data (e.g., weighted least squares). The use of a maximum likelihood estimator does assume a multivariate normal distribution for the data, an assumption that is likely violated given the use of an ordinal rating scale (Li, 2016). Estimators that account for missing data and multivariate non-normal data are limited. A plausible option was maximum likelihood with Huber-White standard errors and a scaled test statistic (MLR). From previous experience in running RI-CLPMs using MLR, it is an estimation method that is prone to error and warning messages. Therefore, we used FIML as the primary preregistered estimation method, and all reported parameter estimates used FIML estimators. However, as a robustness check, we ran all models with MLR and reported the outcomes of each model (see Appendix D).

Fit Indices

The following fit statistics, based on the maximum likelihood (ML) estimation method, were reported as per the preregistration*:*

- Root Mean Square Error of Approximation (RMSEA) and associated 90% confidence intervals
- Standardised Root Mean Square Residual (SRMR)
- Comparative Fit Index (CFI)
- Chi-squared test and associated p-value

Model fit was considered supported if all the following criteria are met (as suggested by Hu & Bentler, 1999):

- RMSEA is less than or equal to 0.06
- SRMR is less than or equal to 0.08
- CFI is equal to or greater than 0.95

Models were judged to have a good fit if all criteria were met, a poor fit if none were met, and an ambiguous fit if only some criteria were met. However, the fit statistics were not used to determine whether the hypotheses were supported. They are best regarded as reflecting on auxiliary measurement assumptions.

Inferential Criteria

Hypotheses were considered supported if the cross-lagged coefficient for the specified effect was positive with a p-value less than 0.05. Two-tailed p values were used in all cases (except the chi-squared difference test for H8; chi-squared tests are implicitly one-sided).

For the moderation hypothesis (H8), support required a significant chi-squared test of group difference (with a p-value less than 0.05) – indicating a difference between low and high intergroup threat groups – and the cross-lagged coefficient needed to be *more* positive in the high intergroup threat group.

More detailed inferential criteria for each model can be found in the preregistration here: https://osf.io/5k4yb.

Effect Size Interpretation

The interpretation of within-person cross-lagged coefficients was guided by empirical benchmarks proposed by Orth et al. (2024). They used a data set of studies from four subfields of psychology (developmental, social-personality, clinical and organisational), which contained 302 effect sizes for the RI-CLPM. In establishing these empirical benchmarks, they calculated the 25^{th}, 50^{th}, and 75^{th} percentiles, corresponding to cross-lagged effect sizes described as small, medium and large. Thus, Orth et al. proposed the following thresholds for interpreting standardised cross-lagged effects:

- .03 as a small effect
- .05 as a medium effect
- .11 as a large effect

The cross-lagged coefficients reported in the current study were *unstandardised* and, therefore, not directly comparable to standardised benchmarks. That said, standardised and unstandardised coefficients tended to be very similar in this study. However, we do acknowledge that the benchmarks proposed by Orth et al. (2024) were not preregistered, but found them useful as benchmarks to describe the magnitude of the cross-lagged effects.

Results

The focus of this study is on the reciprocal within-person effects of belief in conspiracy theories and a general conspiracy mentality on anxiety, depression, perceived stress, and stressful life events. In this case, the within-person effects have been identified as the best approach to making credible causal inferences. Therefore, only within-person effects are reported[5], given that we only identified a causal identification strategy for the within-person part of the RI-CLPM. We do not make inferences using statistics that are not credible estimates of between-person effects (e.g., correlations between random intercepts), given that they have not been identified in our causal identification strategy.

Descriptive Analyses

Mean Scores at Each Wave

To describe inter-individual variation in responses, we calculated each participant's mean response to all six measures at each time point (see Table 10). The mean of belief in conspiracy theories across all waves indicated that participants typically disagreed with each conspiracy theory. Participants' mean responses to the conspiracy mentality questionnaire indicate that participants typically responded undecided to each conspiratorial statement. Participants' mean depression responses indicated mild depressive symptoms (Kroenke et al., 2001). Participants' mean general anxiety responses indicated mild anxiety symptoms. The perceived stress mean was 15.35 (out of 40), and participants typically reported at least one stressful life event. The means across time for conspiracy mentality, depression, anxiety, perceived stress, and stressful life events decreased slightly over time, compared to wave one. The mean across time for belief in conspiracy theories remained relatively stable.

To calculate the mean score at each wave, it was necessary to account for missing data (i.e., participants not returning to a wave). Rather than using listwise deletion (see Hayes & Enders, 2023), we used lavaan to estimate the means at each wave, using full information maximum likelihood (FIML) estimation, and accounted for missing data using maximum likelihood (ML). Missing data within completed survey responses was minimal, with only 10 item-level omissions, across the 6,020 total submissions, and therefore was not considered an issue.

[5] Full parameter estimates are available via our open data and analysis scripts on the Open Science Framework: https://osf.io/365q

Table 10

Means for Main Variables over Time

	T1	T2	T3	T4	T5	T6	T7	Range
Belief in conspiracy theories	1.81	1.79	1.81	1.85	1.81	1.87	1.79	1 – 5
Conspiracy mentality	5.38	5.23	5.21	5.19	5.12	5.13	5.16	0 – 10
Depression	6.48	6.37	6.37	6.13	6.07	6.08	5.98	0 – 24
Anxiety	5.60	5.56	5.42	5.05	5.33	5.36	5.21	0 – 21
Perceived stress	15.35	15.37	15.21	14.30	14.83	14.91	14.60	0 – 40
Stressful life events	0.79	0.62	0.61	0.55	0.57	0.57	0.50	0 – 12

Change in Mean Agreement Over Time

In the next analysis, we examined changes over time at the level of the overall sample. The mean level of agreement with each specific conspiracy theory and general conspiratorial statement is displayed in Figure 8 and Figure 9. As can be seen in the figures, there was little change in average levels of belief in specific conspiracy theories and conspiracy mentality over time. One exception is a slight increase in the endorsement of the COVID-19 bioweapon item at wave six, likely due to a news announcement by the US Energy Department on 26 February 2023, which concluded with low confidence that the COVID-19 pandemic most likely originated from a laboratory leak in China (see Barnes, 2023), five days before we started data collection for wave six (see Figure 8). There was relatively little change to the responses to items of the GAD-7, PHQ-8, PSS-10, and outgroup threat measures, with the exception of a slight decrease in perceived stress and anxiety levels at wave four, which was collected at the start of January 2023 (see Appendix B for agreement levels for all measures).

Figure 8

Changes in Mean Agreement with Belief in Conspiracy Theories Over Time

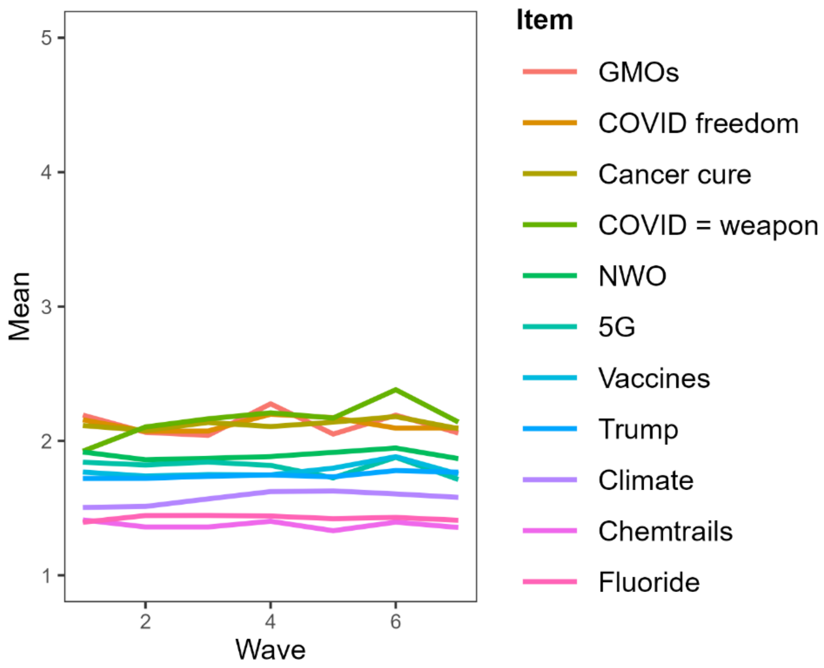
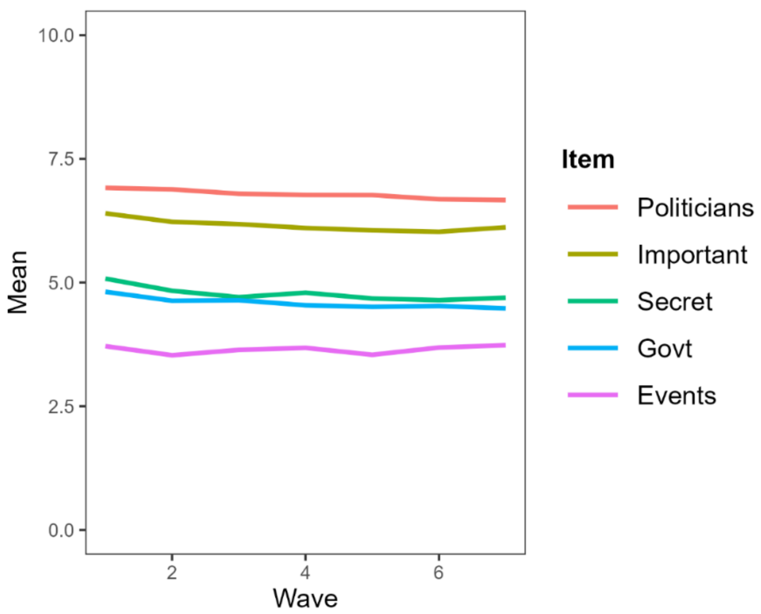


Figure 9

Changes in Mean Agreement with Conspiracy Mentality Over Time



Intraclass Correlation Coefficients

The following provides information regarding the intraclass correlation coefficient (ICC), referring to correlations within repeated measures of the measured variables, providing an understanding of individual variability. ICCs were calculated using participants' overall scores for each measure at each time point (see Table 11 for details). The ICC for belief in conspiracy theories was $r = .92$, indicating that across the seven waves, most of the observed variance was due to stable, between-person differences, with relatively minimal within-person changes. The ICC for conspiracy mentality was slightly lower ($r = .84$). The remaining measures ranged from $r = .84$ to $r = .79$, with one exception for stressful life events ($r = .40$). The ICC for stressful life events is understandably low, given that it is a questionnaire about *events* in the last month, rather than stable individual differences.

Table 11

Intraclass Correlation Coefficients for Main Analysis Variables

Measure	ICC	95% CI
Belief in conspiracy theories	.92	.91 – .93
Conspiracy mentality	.84	.83 – .86
Depression	.82	.81 – .84
Anxiety	.79	.77 – .81
Perceived Stress	.79	.78 – .81
Stressful life events	.40	.37 – .43
Intergroup threat	.70	.68 – .72

Note. ICCs were calculated using the ICC package in R (Wolak et al., 2012).

Confirmatory Analyses: RI-CLPM

The previous descriptive statistics identified that there was an average pattern of stability. However, it is the within-person level that we identified as being of importance for making credible causal inferences.

Each model tested hypotheses using pairs of constructs (e.g., depression and belief in conspiracy theories for model one). For each hypothesis to be supported, the cross-lagged regression path needed to be positive and statistically significant ($p < .05$). Detailed inferential criteria can be found in the preregistration. All reported regression coefficients are unstandardised effects unless otherwise specified. Two deviations from the preregistration were made for models three and eight, where the model could not converge, and we instead switched to a single-indicator RI-CLPM for each model.

Random Intercept Correlations

While the primary focus is on within-person effects, it is important to note the random intercepts, which capture between-person variance in the models. These are a necessary component of the model but are difficult to report when using multiple-indicator RI-CLPMs, given that each indicator (i.e., item) is treated as a latent variable, thus resulting in approximately 400 random intercept correlations across the nine models. For more information, see Appendix G.

Measurement Invariance and Model Fit

Measurement invariance testing was completed to ensure the comparability of latent constructs across time. The process consisted of estimating a sequence of nested models with increasing constraints, consistent with procedures outlined by Mulder and Hamaker (2021). The first model tested configural invariance with no constraints on the factor loadings of intercepts. The second model tested weak factorial invariance by constraining factor loading to be equal across time. The final model tested strong factorial invariance, which further constrained the intercepts of the observed indicators to equality over time. In addition, the final model also constrained the autoregressive and cross-lagged regression coefficients to equality over time.

Measurement invariance was tested using common measures of model fit (Putnick & Bornstein, 2016). The chi-square test assesses whether the model reproduces the data perfectly (though it is highly sensitive to sample size); the CFI evaluates model fit by comparing it to a baseline model, with values closer to 1 indicating better fit; the RMSEA estimates how well the model approximates the population covariance matrix per degree of freedom, with lower values indicating better fit; and the SRMR reflects the average discrepancy between observed and predicted correlations. In comparing the nested models, we used Chen's (2007) recommended changes in goodness of fit indices: ΔCFI $\leq .01$, ΔRMSEA $\leq .015$, and ΔSRMR $\leq .03$ ($\leq .01$ for strong invariance). Given the limitations of the chi-square test in large samples (Brown, 2006; Chen, 2007; Cheung & Rensvold, 2002), this was reported but not used in determining model fit.

Across all multiple-indicator models, changes in fit indices remained within acceptable bounds, supporting the assumption of strong factorial invariance. However, models involving stressful life events (M3 and M8) did not meet the criteria for at least weak factorial invariance. A comparison of changes in goodness of fit indices for all models can be found in Appendix C.

The next step was evaluating whether it was tenable to constrain the cross-lagged parameters to equality across time. In this step, we compared the models of strong factorial invariance with one model where the cross-lagged parameters were unconstrained over time. Following the recommended cut-off points proposed by Chen (2007), all multiple-indicator models remained within acceptable bounds, suggesting that constraining the cross-lagged parameters over time was tenable (See Appendix C).

Lastly, all models of strong factorial invariance were evaluated against the preregistered fit indices. Most models met these criteria, suggesting a good overall fit. However, model 2 (perceived stress – BCT) had a CFI value that missed this cut-off. In addition, Models 3 and 8 (stressful life events) had RMSEA values that both missed this cut-off. These discrepancies introduce additional uncertainty regarding measurement assumptions, according to the preregistered criteria, implying additional ambiguity around the precision and interpretation of the direct effect estimates from these particular models. Nevertheless, acknowledging these uncertainties, all models reported below are presented as models of strong factorial invariance. Specific fit indices are reported below.

Belief in Specific Conspiracy Theories

The following four models used a measure of belief in *specific* conspiracy theories (BCT) and were tested in four models with depression (Model 1), perceived stress (Model 2), stressful life events (Model 3), and anxiety (Model 4). Model five used a single conspiracy theory item from the BCT measure.

Depression and belief in conspiracy theories

The first model (M1) tested two hypotheses. First, when controlling for belief in conspiracy theories at time t-1, depression was hypothesised to have a positive cross-lagged effect on belief in conspiracy theories at time t (H1). Second, when controlling for depression at time t-1, belief in conspiracy theories was hypothesised to have a positive cross-lagged effect on depression at time t (H5). The overall model fit was good, with RMSEA = .025 [.025, .026], CFI = .96, and SRMR = .03, all meeting preregistered thresholds. The chi-square test was significant, $\chi^2(8,648) = 14,091.80$, $p < .001$.

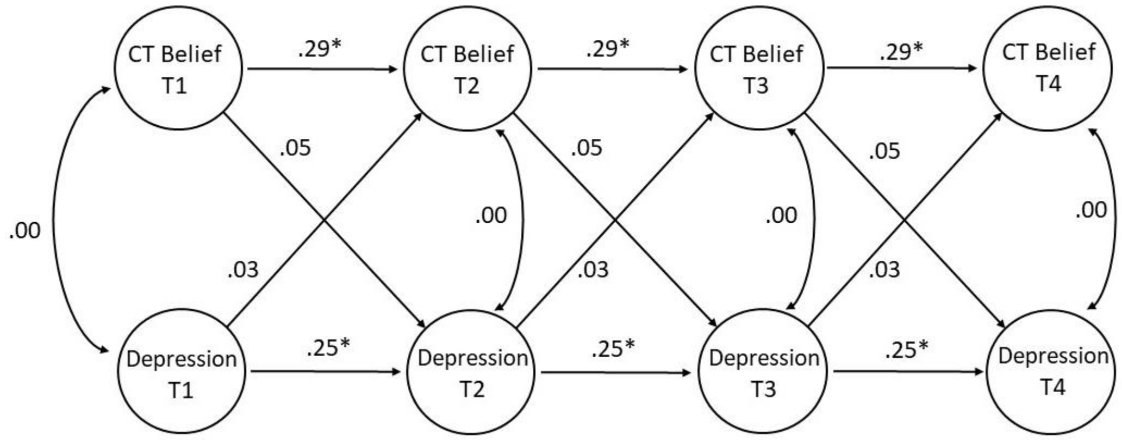
Neither hypothesis was supported. The cross-lagged effect of depression on belief in conspiracy theories was positive but not significant (*b* = .03, 95% CI = [-.01, .06], *p* = .111). The cross-lagged effect of belief in conspiracy theories on depression was also positive but not significant (*b* = .05, 95% CI [-.03, .12], *p* = .237).

There was a positive autoregressive effect for depression, indicating that increases in depression predicted further increases in depression at the next measured time point (*b* = .25, 95% CI = [.20, .30], *p* < .001). There was also a positive autoregressive effect for belief in conspiracy theories, indicating that increases in belief in conspiracy theories predicted further increases in belief in conspiracy theories at the next measured time point (*b* = .29, 95% CI = [.23, .35], *p* < .001).

The standardised within-person correlations were generally small (ranging from *r* = -0.03 to 0.11), and no correlations were statistically significant. In RI-CLPMs, these correlations represent the extent to which within-person fluctuations in one variable are associated with fluctuations in another variable at the same time point, after accounting for stable between-person differences. In the first wave, this correlation reflects associations between individuals' deviations from their average score. From wave two onward, the correlations represent associations between residuals (i.e., the remaining within-person variance between both variables after accounting for autoregressive and cross-lagged effects). See Table 10 for a simplified diagram.

Figure 10

Simplified Model 1 RI-CLPM



Note. * = $p < .001$. Only within-person regression coefficients and correlations are displayed due to the complexity of visualising the full multiple indicators for RI-CLPM. Only four waves are displayed out of the seven. However, as the regression coefficients were constrained over time, these remained the same for the three waves that were excluded in the figure. All output reflects unstandardised estimates.

Perceived stress and belief in conspiracy theories

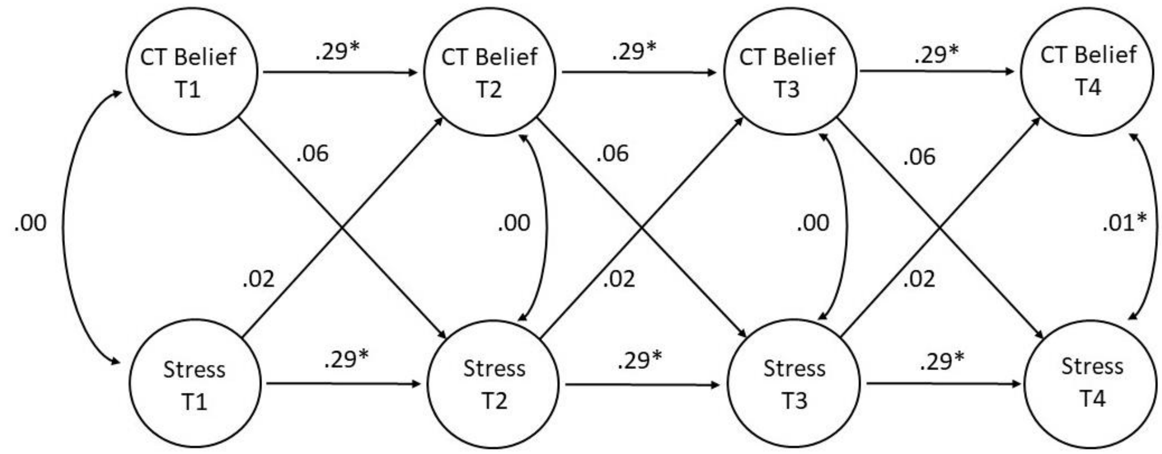
The second model (M2) tested two hypotheses. First, when controlling for belief in conspiracy theories at time t-1, perceived stress was hypothesised to have a positive cross-lagged effect on belief in conspiracy theories at time t (H2). Second, when controlling for perceived stress at time t-1, belief in conspiracy theories was hypothesised to have a positive cross-lagged effect on depression at time t (H6). The overall model fit was ambiguous, with RMSEA = .027, [.026, .028], CFI = .946, and SRMR = .03; the CFI was slightly below the preregistered thresholds. The chi-square test was significant, $\chi^2(10{,}570) = 17{,}998.53$, $p < .001$.

Neither hypothesis was supported (see Figure 11). The cross-lagged effect of perceived stress on belief in conspiracy theories was positive but not significant ($b = .02$, 95% CI [-.01, .05], $p = .118$). The cross-lagged effect of belief in conspiracy theories on perceived stress was also positive but not significant ($b = .06$, 95% CI = [-.03, .15], $p = .193$).

There was a positive autoregressive effect for perceived stress (b = .29, 95% CI = [.23, .34], p < .001) and also a positive autoregressive effect for belief in conspiracy theories (b = .29, 95% CI = [.23, .35], p < .001). The standardised within-person correlations ranged between -.08 and 0.11 and were generally non-significant, with the exception of wave seven (r = .11, p = .040).

Figure 11

Simplified Model 2 RI-CLPM



Note. * = p < .001. All output reflects unstandardised estimates.

Stressful life events and belief in conspiracy theories

The third model (M3) tested one hypothesis. When controlling for belief in conspiracy theories at time t-1, stressful life events (summed score) were hypothesised to have a positive cross-lagged effect on belief in conspiracy theories at time t (H3). We had preregistered a model that included one construct measured with multiple indicators (using the items of the BCT scale), and one construct measured as a single indicator (using the SLE summed score), in a mixed indicator RI-CLPM. However, the complexity of the lavaan syntax resulted in the model not converging. As preregistered, when this issue was encountered, we switched the model to a single-indicator RI-CLPM by using a single mean score of responses to belief in conspiracy theories rather than multiple indicators. The model fit was ambiguous: RMSEA = .069 [.063, .074], CFI = .966, and SRMR = .058; the RMSEA exceeded the preregistered threshold. The chi-square test was significant, $\chi^2(99) = 550.33$, p < .001.

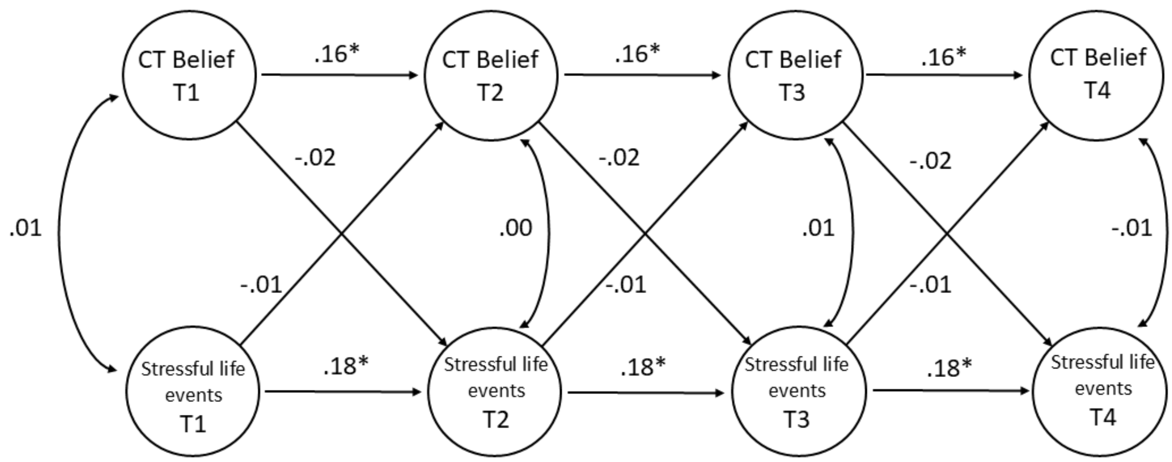
Hypothesis three was not supported. The cross-lagged effect of stressful life events on belief in conspiracy theories was negative and not significant (b = -.01, 95% CI [-.02, .00], p

= .191). While not formally hypothesised, the cross-lagged effect of belief in conspiracy theories on stressful life events was negative and not significant (*b* = -.04, 95% CI [-.14, .04], *p* = .456).

There was a positive autoregressive effect for stressful life events (*b* = .18, 95% CI [.14, .21], *p* < .001) and a positive autoregressive effect for belief in conspiracy theories (*b* = .16, 95% CI = [.12, .19], *p* < .001). The standardised within-person correlations ranged between -.01 and .18, and were significant at waves four (*r* = .13, *p* = .001), six (*r* = .18, *p* = .004), and seven (*r* = .17, *p* = .009).

Figure 12

Simplified Model 3 RI-CLPM



Note. * = *p* < .001. All output reflects unstandardised estimates.

Anxiety and belief in conspiracy theories

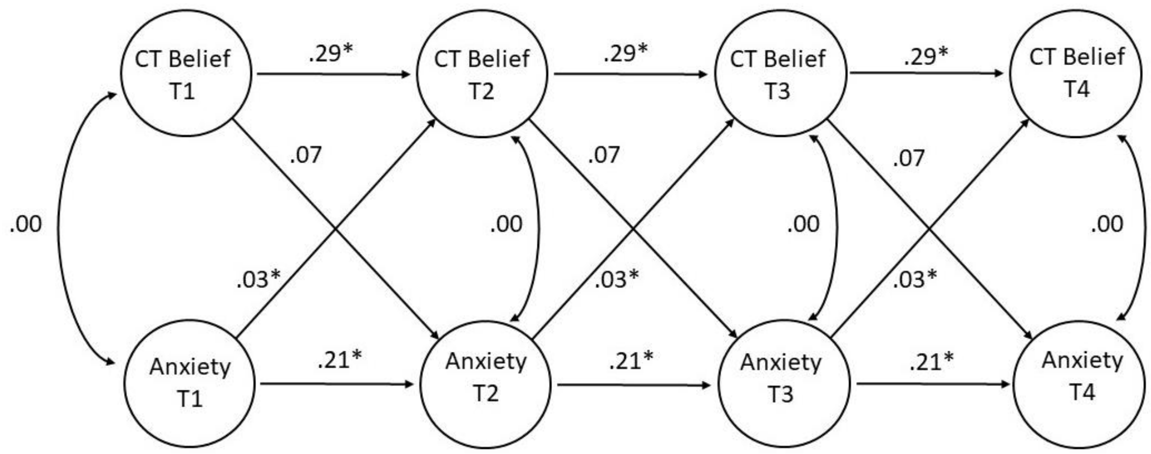
The fourth model (M4) tested two hypotheses. First, when controlling for belief in conspiracy theories at time t-1, anxiety was hypothesised to have a positive cross-lagged effect on belief in conspiracy theories at time t (H4). Second, when controlling for anxiety at time t-1, belief in conspiracy theories was hypothesised to have a positive cross-lagged effect on anxiety at time t (H7). The model fit was good, with the RMSEA = .026 [.026, .027], CFI = .960, and SRMR = .028, all meeting preregistered thresholds. The chi-square test was significant, $\chi^2(7{,}759) = 12{,}995$, *p* < .001.

Hypothesis four was supported: the cross-lagged effect of anxiety on belief in conspiracy theories was small but positive and statistically significant (*b* = .03, 95% CI [.00, .05], *p* = .042), implying that increases in anxiety predicted subsequent increases in belief in conspiracy theories. Hypothesis seven was not supported: the cross-lagged effect of belief in conspiracy theories on anxiety was positive but not significant ($b = .07$, 95% CI = [-.02, .15], $p = .130$).

There was a positive autoregressive effect for anxiety ($b = .21$, 95% CI = [.16, .26], $p < .001$) and also a positive autoregressive effect for belief in conspiracy theories ($b = .29$, 95% CI = [.23, .35], $p < .001$). The standardised within-person correlations were generally small (between -.07 and .18) and were only significant at wave six ($r = .18$, $p = .002$).

Figure 13

Simplified Model 4 RI-CLPM



Note. * = $p < .001$. All output reflects unstandardised estimates.

Intergroup threat differences

To test whether the effect of perceived stress (summed score) on belief in a specific conspiracy theory was the same for those with high levels of intergroup threat and those low in intergroup threat, we performed a multiple group RI-CLPM as a test of moderation (see Mulder & Hamaker, 2021). A detailed analysis plan and inferential criteria can be found in the preregistration.

First, we fitted a model (Model 5a) with all structural parameters constrained across time but freely estimated across groups and found $\chi^2(198) = 643.99$, $p < .001$. Within model 5a, the estimated effect of perceived stress on belief in a specific conspiracy theory[6] for participants who received the Chinese government as threatening was $b = .00$, 95% CI [-.01,

[6] The item used to assess belief in a specific conspiracy theory was: “COVID-19 is a biological weapon intentionally created and released by China.”

.01], $p = .781$. For participants who did not perceive the Chinese government as threatening, the estimated effect was similar $b = .00$, 95% CI [-.00, .01], $p = .494$.

Next, we compared this to a model where only the cross-lagged path from perceived stress to belief in a specific conspiracy theory was constrained to equality across groups (Model 5b) and found $\chi^2(199) = 644.02$, $p < .001$. The chi-square difference test of these two nested models was not significant, $\Delta\chi^2$ (1) = 0.04, $p = 0.849$, implying there was no evidence to reject the null hypothesis that the cross-lagged parameter is identical in these two populations. Therefore, hypothesis eight was not supported.

In addition, in line with recommendations by Chen (2007), changes in fit indices were minimal (ΔCFI = .002, ΔRMSEA = -.002, ΔSRMR = .003), all being below suggested thresholds for meaningful change (i.e., ΔCFI > .01, ΔRMSEA > .015, ΔSRMR > .01). These results further support the conclusion that constraining cross-lagged effects across groups had a limited impact on model fit.

Conspiracy Mentality

The following models used a measure of conspiracy mentality (CMQ) and were tested in four models, which included depression (Model 6), perceived stress (Model 7), stressful life events (Model 8), and anxiety (Model 9). As discussed in the method section, the reason for using a measure of conspiracy mentality is that it is argued to measure a slightly different construct (e.g., Imhoff et al., 2022).

Depression and conspiracy mentality

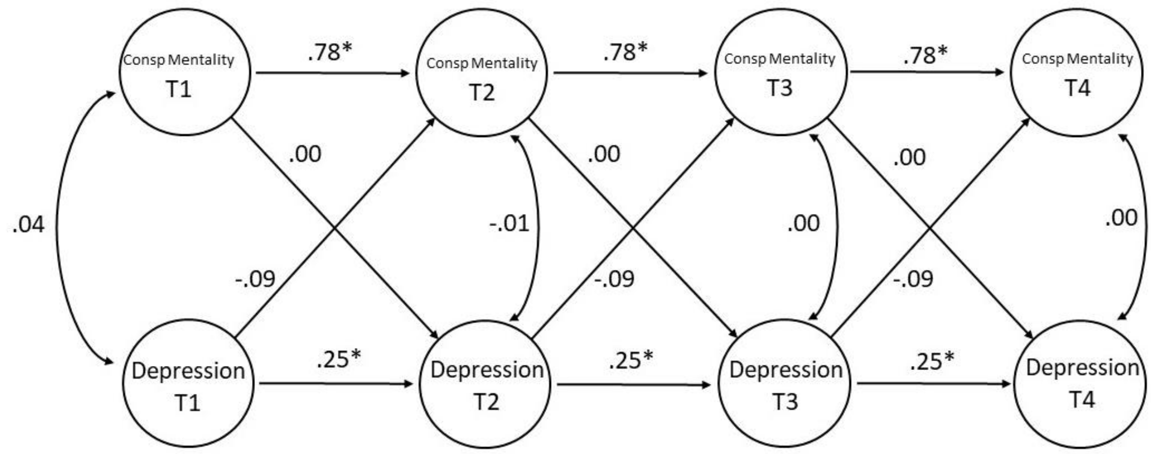
The sixth model (M6) tested two hypotheses. First, when controlling for conspiracy mentality at time t-1, depression was hypothesised to have a positive cross-lagged effect on conspiracy mentality at time t (H9). Second, when controlling for depression at time t-1, conspiracy mentality was hypothesised to have a positive cross-lagged effect on depression at time t (H13). The model fit was good, with RMSEA = .022 [.021, .024], CFI = .975, and SRMR = .028, all meeting preregistered thresholds. The chi-square test was significant, $\chi^2(4{,}034) = 5{,}986$, $p < .001$.

Neither hypothesis was supported. The cross-lagged effect of depression on conspiracy mentality was negative and not significant ($b = -.09$, 95% CI [-.26, .08], $p = .301$). The cross-lagged effect of conspiracy mentality on depression was also not significant ($b = .00$, 95% CI = [-.02, .03], $p = .840$).

There was a positive autoregressive effect for depression (b = .25, 95% CI = [.20, .31], p < .001). There was also a positive autoregressive effect for conspiracy mentality (b = .78, 95% CI = [.71, .85], p < .001). The standardised within-person correlations for waves two to seven were generally small (between r = -.05 and .03). At wave one, the correlation was moderately sized (r = .27, p = .058). However, no other wave had any significant correlations.

Figure 14

Simplified Model 6 RI-CLPM



Note. * = p < .001. All output reflects unstandardised estimates.

Perceived stress and conspiracy mentality

The seventh model (M7) tested two hypotheses. First, when controlling for conspiracy mentality at time t-1, perceived stress was hypothesised to have a positive cross-lagged effect on conspiracy mentality at time t (H10). Second, when controlling for perceived stress at time t-1, conspiracy mentality was hypothesised to have a positive cross-lagged effect on perceived stress at time t (H14). The model fit was good, with the RMSEA = .025 [.024, .026], CFI = .961, and SRMR = .036, all meeting preregistered thresholds. The chi-square test was significant, $\chi^2(5{,}380) = 8602$, p < .001.

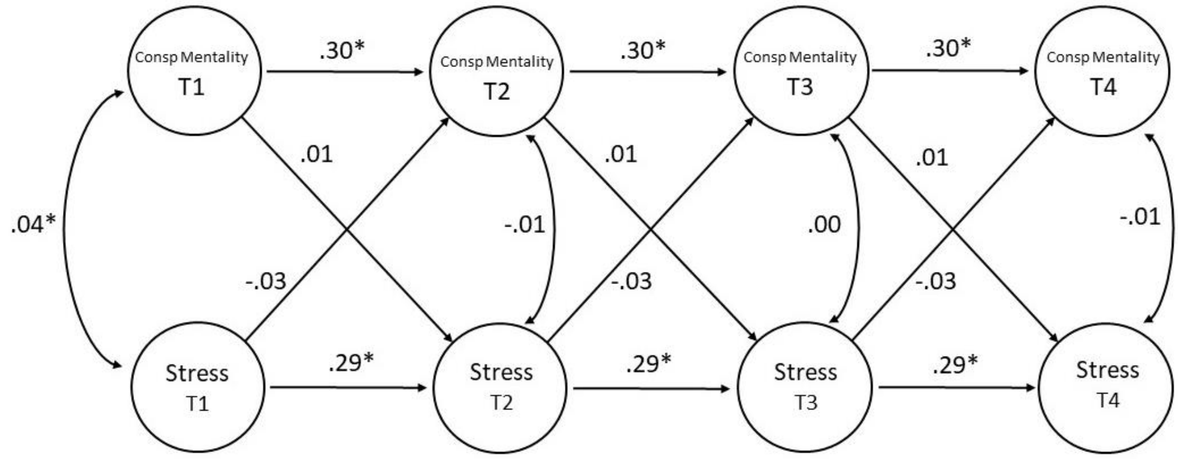
Neither hypothesis was supported. The cross-lagged effect of perceived stress on conspiracy mentality was negative and not significant (b = -.03, 95% CI [-.13, .08], p = .625). The cross-lagged effect of conspiracy mentality on perceived stress was positive but not significant (b = .01, 95% CI = [-.01, .03], p = .412).

There was a positive autoregressive effect for perceived stress (b = .29, 95% CI = [.24, .34], p < .001) and also a positive autoregressive effect for conspiracy mentality (b =

.30, 95% CI = [.23, .36], $p < .001$). The standardised within-person correlation for wave one was significant ($r = .12$, $p = .039$). However, the correlations at waves two to seven were generally small (between $r = -.05$ and .08), and none were significant.

Figure 15

Simplified Model 7 RI-CLPM



Note. * = $p < .001$. All output reflects unstandardised estimates.

Stressful life events and conspiracy mentality

The eighth model (M8) tested one hypothesis. When controlling for conspiracy mentality at time t-1, stressful life events (summed score) were hypothesised to have a positive cross-lagged effect on belief in conspiracy theories at time t (H11). We faced similar issues to model three when running a half multiple indicator and half single indicator RI-CLPM. We changed the model to a single-indicator RI-CLPM, using a single mean score of responses to the conspiracy mentality questionnaire rather than multiple indicators.

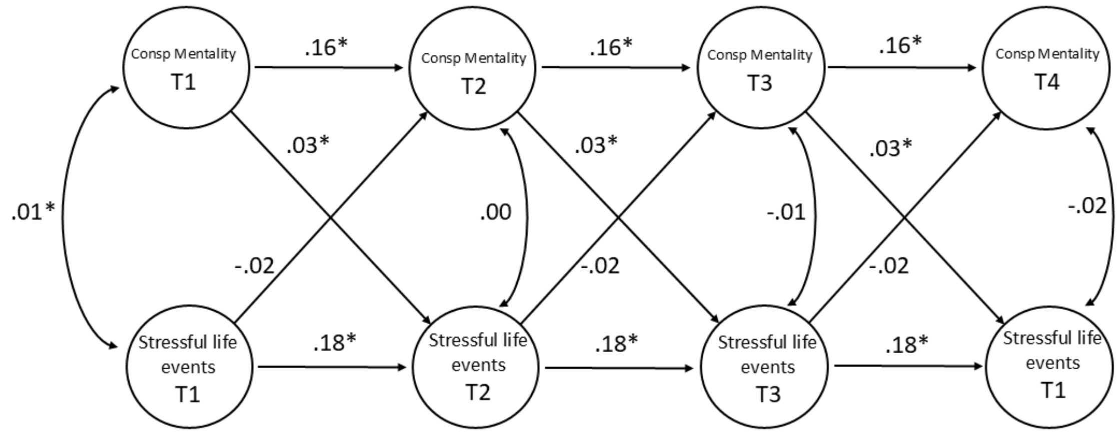
The model fit was ambiguous, with the RMSEA = .067 [.062, .073], CFI = .956, and SRMR = .068; the RMSEA exceeded the preregistered threshold. The chi-square test was significant, $\chi^2(99) = 536.35$, $p < .001$.

Hypothesis 11 was not supported. The cross-lagged effect of stressful life events on conspiracy mentality was negative and not significant ($b = -.02$, 95% CI [-.06, .01], $p = .210$). While not hypothesised, the cross-lagged effect of conspiracy mentality on stressful life events was positive and statistically significant ($b = .03$, 95% CI [.00, .06], p = .050).

There was a positive autoregressive effect for stressful life events ($b = .18$, 95% CI = [.14, .21], p < .001) and a positive autoregressive effect for conspiracy mentality ($b = .16$, 95% CI = [.13, .20], $p < .001$). The standardised within-person correlations ranged between -.03 and .10), and were significant at wave one (r = 09, $p = .019$), five (r = 08, $p = .050$), and seven ($r = .10$, $p = .050$)

Figure 16

Simplified Model 8 RI-CLPM



Note. * = $p < .001$. All output reflects unstandardised estimates.

Anxiety and conspiracy mentality

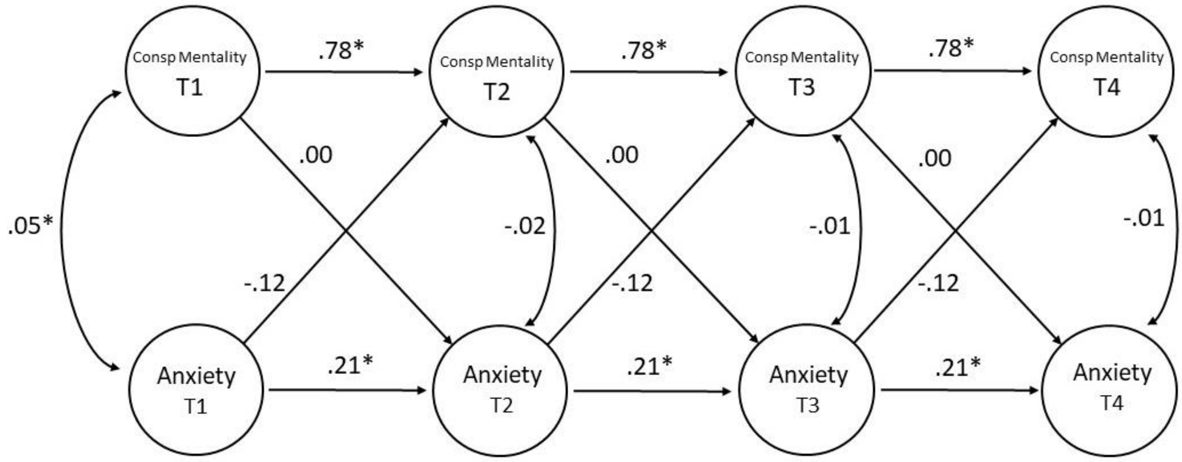
The last model (M9) tested two hypotheses. First, when controlling for conspiracy mentality at time t-1, anxiety was hypothesised to have a positive cross-lagged effect on conspiracy mentality at time t (H12). Second, when controlling for anxiety at time t-1, conspiracy mentality was hypothesised to have a positive cross-lagged effect on anxiety at time t (H15). The model fit was good, with RMSEA = .022 [.021, .024], CFI = .979, SRMR = .029, all meeting preregistered thresholds. The chi-square test was significant, $\chi 2(3,433) = 5{,}097$, $p < .001$.

Neither hypothesis was supported. The cross-lagged effect of anxiety on conspiracy mentality was negative and not significant (b = -.12, 95% CI [-.25, .01], $p = .061$). The cross-lagged effect of conspiracy mentality on anxiety was not significant (b = .00, 95% CI = [-.03, .03], $p = .847$).

There was a positive autoregressive effect for anxiety (b = .21, 95% CI = [.16, .26], $p < .001$) and also a positive autoregressive effect for conspiracy mentality (b = .78, 95% CI = [.70, .85], $p < .001$). The standardised within-person correlation for wave one was statistically significant (r = .29, $p = .028$). However, the correlations at waves two to seven were generally small and not statistically significant (between r = -.06 and .09).

Figure 17

Simplified Model 9 RI-CLPM



Note. * = $p < .001$. All output reflect unstandardised estimates.

Exploratory Analyses

Measurement Invariance Across Countries

We had preregistered that we would conduct a test of measurement invariance of the strong invariance models across the three countries with a multiple group RI-CLPM using multiple indicators RI-CLPM for each country. However, the estimation algorithm was unable to reach convergence for the model, likely due to the complexity of the number of estimated parameters in the multiple-group structure.

Non-normality Estimated RI-CLPMs

ML estimation assumes multivariate normality, an assumption likely breached by using ordinal rating scale data. An estimation method that does not assume multivariate normality is robust maximum likelihood estimation (MLR), which uses Huber-White standard errors and a Yuan-Bentler scaled test statistic (Li, 2016). This estimation method was used to assess the robustness of the model under conditions of non-normality (Brosseau-Liard et al., 2012; Brosseau-Liard & Savalei, 2014). MLR is prone to error and warning messages when estimating RI-CLPMs; therefore, it was not the primary preregistered estimation method. That said, all models in this study converged successfully when re-estimated using MLR.

Across all models, the within-person effects of interest remained consistent between ML and MLR estimates, with a full comparison of outputs provided in Appendix D. Model fit indices using robust statistics from MLR were all mostly within preregistered thresholds. The exception was stressful life event models, where the fit statistics exceeded the cut-offs for RMSEA and SRMR, but there was a consistent pattern across both ML and MLR estimation, indicating fit issues were likely model-specific. Overall, the use of an MLR estimator provided additional confidence in the robustness of the findings from the ML-estimated RI-CLPMs.

Bayes Factors for Estimated Effects

Given that most hypotheses resulted in null effects, a further exploratory analysis of the cross-lagged effect was conducted. Fractional Bayes factors using the *bain* package in R were used to determine the evidence in favour of each hypothesis (Hoijtink et al., 2019). Although these analyses were conducted with the preregistered RI-CLPM models, this additional inferential approach was not preregistered. For each hypothesised effect, we compared two models: A null model (effect = 0) and an alternate model where the effect was permitted to be non-zero.

Across all cross-lagged paths, the Bayes factors consistently suggested stronger support for the null hypothesis, compared to the alternative model. Most models had Bayes factors exceeding 3, with many in the 20-30 range, indicating “positive” to “strong” evidence in favour of the null hypotheses according to the suggested thresholds by Kass and Raftery (1995). Full output and further detail can be found in Appendix H.

Unconstrained Average Effects

As an exploratory analysis, preregistered hypotheses for multiple-indicator RI-CLPMs were re-tested using models that allowed for cross-lagged effects to vary across time without assuming the equality of effects across time. An average of the unconstrained cross-lagged effects over time was estimated for each direction, providing a time-variant approach to testing the preregistered directional hypotheses. This method permits hypothesis testing even when cross-lagged effects vary across time.

Overall, the results were broadly consistent with the constrained models. Fit indices remained good across the models, with all meeting preregistered thresholds, except for Model 2 (perceived stress and belief in conspiracy theories). Three models showed statistically significant average cross-lagged effects (see Tables 12 and 13).

The within-person effect of anxiety on belief in conspiracy theories (H4) was significant in both approaches: b = .03, [.00, .05], p = .042 (constrained) vs. b = .109, 95% CI

[.009, .209], *p* = .032 (averaged unconstrained). While the effect was stronger in the unconstrained model, the confidence interval was narrower in the constrained model, suggesting more precision under the equality constraint.

In contrast, two effects were only significant in the unconstrained averages: Model 1, depression and belief in conspiracy theories *b* = .091, 95% CI [.003, .179], *p* = .044, and Model 9, conspiracy mentality and anxiety *b* = -.114, 95% CI [-.217, -.011], *p* = .030. These findings suggest that the constrained models may have underestimated time-varying effects by forcing them into a single parameter across time.

However, given the strong alignment between constrained and unconstrained estimates in direction, magnitude, and model fit, the assumption of time-invariant cross-lagged effects appears to have been reasonable.

Table 12

Average Unconstrained Cross-lagged Coefficients: Belief in Conspiracy Theories

Model	Estimated effect	*b* [95% CI]	*p*	Estimated effect	*b* [95% CI]	*p*
1	Depression -> belief in conspiracy theories (H1)	.091 [.003, .179]	.044*	Belief in conspiracy theories -> depression (H5)	.032 [-.003, .066]	.069
2	Perceived stress -> belief in conspiracy theories (H2)	.103 [-.002, .208]	.054	Belief in conspiracy theories -> perceived stress (H6)	.016 [-.012, .044]	.275
3	Stressful life events -> belief in conspiracy theories (H3)	-.067 [-.181, .047]	.248	Belief in conspiracy theories -> stressful life events (no hypothesis)	-.009 [-.021, .002]	.118
4	Anxiety -> belief in conspiracy theories (H4)	.109 [.009, .209]	.032*	Belief in conspiracy theories -> anxiety (H7)	.022 [-.004, .049]	.101

Note. * = $p < .001$.

Table 13

Average Unconstrained Cross-lagged Coefficients: Conspiracy Mentality

Model	Estimated effect	*b* [95% CI]	*p*	Estimated effect	*b* [95% CI]	*p*
6	Depression -> Conspiracy mentality (H9)	.004 [-.022, .029]	.773	Conspiracy mentality -> Depression (H13)	-.007 [-.147, .134]	.925
7	Perceived stress -> Conspiracy mentality (H10)	-.009 [-.039, .021]	.548	Conspiracy mentality -> Perceived stress (H14)	-.066 [-.177, .045]	.245
8	Stressful life events -> Conspiracy mentality (H11)	.029 [-.004, .062]	.085	Conspiracy mentality -> Stressful life events (no hypothesis)	-.030 [-.069, .009]	.139
9	Anxiety -> Conspiracy mentality (H12)	-.001 [-.030, .027]	.920	Conspiracy mentality -> Anxiety (H15)	-.114 [-.217, -.011]	.030*

Note. * = $p < .001$.

Discussion

This study aimed to examine the causal effect between psychological distress and conspiracy beliefs, in addition to the opposite effect: whether conspiracy beliefs *increase* feelings of psychological distress.

Effect of Psychological Distress *on* Conspiracy Beliefs

Overall, we found minimal evidence for an effect of anxiety, depression, perceived stress, or stressful life events on belief in conspiracy theories or conspiracy mentality. Of the 15 preregistered hypotheses, only one was supported: a significant within-person cross-lagged effect of anxiety increasing belief in specific conspiracy theories. That said, this coefficient did not remain significant when applying robust standard errors (see Appendix D).

These findings are consistent with previous longitudinal studies. Both Chan et al. (2023) and Liekefett et al. (2023), using single indicator RI-CLPMs, also found no evidence for a significant within-person effect of psychological distress (including anxiety) on conspiracy beliefs across multiple time points ranging from two to 18 months. Similarly, other longitudinal studies using cross-lagged panel models consistently provide limited evidence for distress predicting belief in conspiracy theories (e.g., Adamus et al., 2025; Ballová Mikušková & Teličák, 2024; Heiss et al., 2021; Leibovitz et al., 2021). Taken together, these findings suggest that if psychological distress increases belief in conspiracy theories, the effect is likely to be small. In our models, 95% confidence intervals were consistently narrow and centred around zero. For example, for the unstandardised within-person effect of depression on belief in conspiracy theories, the 95% confidence interval ranged from b = -.01 to just b = .06. While this does not entirely rule out effects, the precision of the confidence intervals allows us to credibly rule out at least the presence of *large* within-person effects.

In contrast to the minimal longitudinal evidence, cross-sectional studies frequently report small to moderate positive correlations between psychological distress and belief in conspiracy theories (see meta-analyses by Biddlestone et al., 2025; Bowes et al., 2023). While these correlations might initially appear to suggest a causal relationship, they remain vulnerable to being influenced by unmeasured confounding variables. For example, Fox and Williams (2023) demonstrated no significant effect after controlling for plausible confounding variables in a sample of New Zealand and Australian participants; however, reported a small, significant effect of perceived stress on increasing belief in conspiracy theories in a US-based sample.

Finally, experimental studies, which attempt direct causal inference through short-term manipulations, have reported stronger effects of anxiety on conspiracy beliefs. For example, Grzesiak-Feldman (2013) and Radnitz and Underwood (2017) both found that anxiety primes increased scores on conspiracy belief measures in comparison to control conditions. In contrast, the effects found in this study provide more cautious evidence. Although we found evidence for a small within-person effect of anxiety increasing belief in conspiracy theories over time, it was not replicated in analyses of conspiracy mentality or sustained when using robust estimation methods. Therefore, despite the apparent similarity between our longitudinal results and experimental findings, caution remains warranted when interpreting such effects as strong evidence of causality.

Effects of Conspiracy Beliefs *on* Psychological Distress

It seemed plausible to expect that endorsing conspiracy beliefs would contribute to psychological distress, given the inherently distressing nature of the conspiracy theories examined, many of which involved secretive, malevolent actions by powerful individuals or groups. However, our findings showed no significant within-person effects of belief in conspiracy theories or conspiracy mentality on anxiety, depression, or stress.

Previous longitudinal research using similar statistical methods has produced mixed results regarding whether conspiracy beliefs increase psychological distress. For instance, Liekefett et al. (2023) found evidence of a significant within-person effect of conspiracy beliefs increasing anxiety (b = .37) over two months but did not replicate this effect in a second study over 18 months. Similarly, Chan et al. (2023), consistent with our findings, reported no significant within-person effect of conspiracy beliefs on increasing psychological distress over three months. Our results, collected over six months, align and reinforce this inconclusive evidence.

Although most longitudinal research does not support this effect of conspiracy beliefs increasing psychological distress, one recent exception by Samayoa et al. (2025) suggested a potential short-term benefit. Samayoa et al. found that belief in conspiracy theories temporarily increased well-being one month later but was not sustained in the long term. While crucial methodological limitations – such as statistically controlling for conspiracist ideation – weaken the strength of these claims, the theorised possibility that conspiracy beliefs may temporarily reduce distress in the short term cannot be ruled out. It is plausible that our study, with a one-month lag between data collection, might have captured a transitional point, after any initial short-term reduction in distress but before longer-term distress emerged, potentially explaining the lack of significant within-person effects. This possibility is explored further in the limitations section.

Testing the Existential Threat Model

We also attempted to test the interaction effect implied in van Prooijen's (2020) existential threat model. Specifically, we examined whether high outgroup threat (i.e., feeling threatened by the Chinese government) would moderate the within-person effect of perceived stress on belief in the conspiracy theory that China deliberately released COVID-19 as a bioweapon. According to the model, feelings of existential threat (e.g., psychological distress) are more likely to increase belief in conspiracy theories when an antagonistic outgroup is salient. However, the results from this test (model 5, hypothesis 8) showed no significant interaction. The estimated effect of stress on this conspiracy belief was small and non-significant, regardless of whether participants perceived high or low outgroup threat.

To our knowledge, this study provides the first empirical test of the interaction effect proposed by the existential threat model. While recognising certain methodological limitations (discussed further below), we interpret these findings as inconsistent with the model's key prediction. However, our operationalisation may not fully capture the theoretical assumptions of the model, particularly the role of a clearly defined existential threat and an active sensemaking process. That said, the lack of a significant within-person effect between perceived stress and belief in conspiracy theories under conditions of high outgroup threat poses a direct challenge to the model's predictive validity.

Overall, these findings challenge key assumptions embedded in existing theoretical frameworks that suggest a cyclical or escalating relationship between conspiracy beliefs and distress. Both Douglas et al. (2017) and van Prooijen (2020) theorise that conspiracy beliefs initially serve as forms of coping mechanisms, with the intention of alleviating distress or uncertainty, which may provide short-term relief, but subsequently worsen distress over time. The lack of significant effects found in this study, alongside that of other longitudinal research, suggests that this proposed escalation in distress over time might not consistently occur, or at least not within the time intervals examined by our design.

Explaining the Limited Evidence for Effects: Methodological and Conceptual Factors

The discrepancy between the effects found in longitudinal (including the current study), cross-sectional, and experimental research, likely reflects methodological differences and conceptual nuances in how relationships are examined. Longitudinal studies, especially those aiming to distinguish within- and between-person effects, focus on *within-person* changes over time (the ups and downs within the same person). In contrast, cross-sectional research typically examines differences between individuals at a single time point, which may reflect either direct causal effects or shared underlying factors (i.e., confounding). As a result, previous cross-sectional findings linking distress and conspiracy beliefs might reflect stable individual differences rather than dynamic, causal within-person processes (Rohrer & Murayama, 2021). Similarly, experimental studies, although valuable for establishing causality, often rely on artificial manipulations of anxiety or stress, which may not generalise to naturally occurring distress. Our longitudinal design attempted to measure naturally occurring variations in psychological distress and conspiracy beliefs, providing one plausible explanation for why the effects observed in experimental studies were not consistently replicated here.

Advances in longitudinal modelling offer further insight into these discrepancies. Both Chan et al. (2023) and Liekefett et al. (2023) reported significant between-person effects[7], suggesting that the correlation of distress and conspiracy belief may exist on average across individuals. We found a similar pattern of positive between-person correlations in the random intercepts (see Appendix F). Significant between-person effects were found in almost all models for conspiracy mentality models, but fewer than half of the between-person effects were significant in the belief in conspiracy theory models. Thus, it is possible that the observed association between distress and conspiracy beliefs could primarily reflect stable between-person differences, rather than a within-person causal process. This fits with our finding that within-person effects were generally small or non-significant. In other words, some people may differ from each other in their levels of distress and conspiracy beliefs. However, individual fluctuations over time might not directly cause changes in beliefs. This pattern is consistent with broader evidence demonstrating that within-person effects often do not align with between-person correlations (Hamaker et al., 2015; Schuurman, 2023).

[7] Both Chan et al. (2023) and Liekefett et al. (2023) used latent RI-CLPMs, which allowed for a single random intercept correlation, rather than a multiple indicator RI-CLPM which is used in the current study, resulting in a large number of random intercept correlations.

Although significant between-person correlations emerged, these correlations alone cannot rule out confounding influences, highlighting the need for caution when interpreting these relationships as causal (Rohrer & Murayama, 2021).

Beyond methodological differences, another key issue is conceptual. Specifically, why did our results contradict the predictions of the existential threat model and psychological motives framework? Both theories argue that psychological distress should *increase* belief in conspiracy theories. However, they do not clearly specify the time course over which these effects unfold. For example, van Prooijen (2020) and Douglas and Sutton (2017) argue that belief in conspiracy theories may initially reduce distress by offering a sense of meaning or control, providing short-term relief. This initial reduction in distress could act as a form of negative reinforcement, strengthening the belief early on and allowing it to persist when distress subsequently increases. Therefore, conspiracy beliefs might initially flourish because they alleviate immediate distress, but later remain fixed despite contributing to longer-term distress.

This temporal complexity – an initial short-term reduction followed by long-term exacerbation of distress – may not have been clearly captured in the current study's methodological design, which assumed stable, linear, within-person effects over the seven-month study period. Our models therefore primarily tested whether average within-person changes in distress predicted changes in conspiracy beliefs (and vice versa), possibly missing nuanced effects occurring at distinct stages of belief formation and maintenance.

If belief in conspiracy theories contributes to worsening distress over time, why would individuals continue to endorse them? What maintains these beliefs once they stop providing relief? For conspiracy beliefs to persist despite increasing distress, something must shift in the longer term to account for the maintenance of the belief once it no longer serves a distress-*reducing* function. It is possible that the mechanism driving belief in conspiracy theory adoption in the short term may not be the same mechanism that maintains it in the long term. This potential shift in the underlying mechanisms challenges the straightforward notion that psychological distress alone maintains belief in conspiracy theories over extended periods.

Strengths of the Research

First, a longitudinal design allowed for stronger causal inferences by establishing temporal precedence between variables (e.g., anxiety, depression, stress, and belief in conspiracy theories). In addition, using the RI-CLPM with longitudinal data, stable confounding variables could be ruled out, although its use could not rule out time-variant confounding variables (see Murayama & Gfrörer, 2022). This approach addresses gaps in the literature, as longitudinal studies on psychological distress and conspiracy beliefs are scarce (Pilch et al., 2023).

Second, using seven measurement points enabled us to detect within-person variability over time more accurately. Rast and Hofer (2014) demonstrated that increasing the number of waves in a longitudinal design substantially improves its power to detect individual differences in change. This design may have increased the likelihood of capturing small within-person fluctuations in psychological distress and conspiracy beliefs, surpassing the four waves reported by Liekefett et al. (2023) and the five waves reported by Chan et al. (2023). However, despite these additional waves, our results aligned with similar research by Liekefett et al. and Chan et al., suggesting that within-person variability in anxiety, depression, stress, and belief in conspiracy theories remains relatively small over both six months and 18 months (Study 2; Liekefett et al., 2023).

Third, our study achieved a high retention rate of 82% at the final wave, which is notably higher than retention rates reported in similar studies. For comparison, two recent studies by Liekefett et al. (2023) reported retention rates of 57% and 43% at their final time point, and Chan et al. (2023) reported a retention rate of 51%. The strong retention rate allowed us to maintain a sufficiently large sample size, making it possible to conduct complex statistical analyses without relying on missing data imputation.

Fourth, the consistency of results across two distinct measures – specific conspiracy beliefs and general conspiracy mentality – strengthens the reliability of our findings. We found that no hypotheses were supported when using a measure of general conspiracy mentality, despite this measure demonstrating higher within-person variability (in all models). Greater variability would typically make it more likely to detect a within-person effect (discussed further in the Limitations section).

Lastly, our study is the first to incorporate preregistration within a longitudinal design in research that examines psychological distress and conspiracy beliefs, building on prior preregistered cross-sectional studies (see Braud et al., 2021; Fox & Williams, 2023; Krüppel et al., 2023; Peitz et al., 2021; Williams, Anderson, et al., 2022). The combination of preregistration and a longitudinal design allowed for greater transparency about the severity of the test of our hypotheses, alongside stronger causal inferences (e.g., Granger causality) with fewer assumptions than those made in cross-sectional studies (e.g., Fox & Williams, 2023). While preregistration does not inherently make the study superior to non-preregistered research, it provides a clear framework for evaluating the severity of hypotheses and ensuring that a study can appropriately falsify a prediction (Lakens, 2019). This strengthened the reliability of our findings by offering severe tests of all 15 hypotheses.

Limitations

Convenience Sampling

The current study used convenience sampling through Prolific. Like other crowdsourcing platforms, Prolific relies on nonprobability sampling, which includes limitations such as self-selection bias (Bethlehem, 2010). Although this approach provides easy and affordable access to participants and has been found to provide high-quality data regarding meaningful responses to the researcher's questions (Douglas et al., 2023; Peer et al., 2022), it limits the external validity of the findings. Specifically, caution is needed in generalising these findings to the broader populations of New Zealand, Australia, and the United Kingdom.

Measures

The measures used in the current study were selected based on whether they had established validity evidence, except for the measure of belief in specific conspiracy theories. The measure was selected over measures of specific conspiracy theories that have documented validity evidence (e.g., BCTI; Swami et al., 2017) due to the items in the BCTI not being of contemporary relevance (e.g., references to the SARS (Severe Acute Respiratory Syndrome) outbreak). The current measure, while not formally validated, draws on prior work where its development was documented in previous longitudinal studies (Williams, Ling, et al., 2022), providing some evidence to support, at least, the reliability of the measure. In addition, with documented psychometric evidence (Bruder et al., 2013; Ćirović & Pedović, 2025), the conspiracy mentality questionnaire yielded similar reliability and stability over time to the unvalidated measure, which provides initial support that it might capture the construct of interest consistently. Despite this, the lack of validity evidence still limits confidence in whether the measure accurately captures the intended construct, potentially weakening the validity of the study's conclusions (Flake & Fried, 2020).

Time

No theory clearly specifies how long the effects of psychological distress on belief in conspiracy theories are expected to take to emerge. The one-month interval was informed by previous longitudinal research that focused on anxiety and specific beliefs in conspiracy theories (Liekefett et al., 2023). These studies had time lags ranging from two weeks to four months, and it was decided that with a time frame of six months (necessitated by the constraints of a doctoral research project), one-monthly intervals were considered reasonable to allow enough time to detect meaningful individual differences over time.

However, it is possible that the selected time lags may have been unsuitable for detecting a causal effect between psychological distress and conspiracy beliefs. Our estimates could be biased if causal effects occurred at either a slower or faster rate than the one-month interval designed to capture them. For example, Liekefett et al. (2023) found differing effects depending on interval duration – effects emerged over two weeks but not over four months. If causal effects operate on a shorter timescale (e.g., psychological distress immediately affecting conspiracy beliefs), these effects should be reflected as strong within-person correlations of psychological distress and conspiracy beliefs at the same time point. In the current study, the within-person correlations were generally small and non-significant. However, we observed a small number of significant within-person correlations, such as r = .29 between anxiety and conspiracy mentality at wave one, and correlations ranging from r = .13 to .18 in Model 2 at waves four, five, and seven. While this inconsistency challenged the short-term hypothesis, we cannot rule it out entirely, as such effects could be too brief or context-specific to detect in monthly assessments.

Additionally, as noted earlier, our study may have overlapped with a transitional point described in theoretical frameworks – between short-term distress relief and subsequent longer-term exacerbation (e.g., Douglas et al., 2017; van Prooijen, 2020). Some longitudinal evidence supports this temporal complexity, as effects appeared at shorter but not at longer intervals (Liekefett et al., 2023). If the trajectories of belief effects are non-linear, our one-month intervals across six months may have coincided with a period between short- and long-term effects – a period in which no clear effect would be detectable. This could potentially explain the lack of significant within-person effects in either direction.

Within-person Variance

The high level of stability for belief in conspiracy theories, conspiracy mentality, and measures of psychological distress suggests limited within-person variance. The small within-person variances could be a plausible reason why only a small amount of significant within-person effects were detected. For example, average unstandardised within-person variances for belief in conspiracy theories across all three multiple indicator models (models 1, 2, and 4) ranged from 0.02 to 0.06, and were slightly larger[8] for conspiracy mentality models (models 6, 7, and 9), which ranged from 0.48 to 0.50. Similarly, within-person variance for distress measures (PHQ-8, PSS-10, GAD-7) averaged between 0.09 and 0.12. With small amounts of within-person variance, detecting the causes of within-person variance becomes difficult. All within-person variances (except one) were statistically significant at $p < .05$, indicating that there was not quite *zero* variance. Although our sample size was large, the low within-person variance has implications for statistical power in the current research and our ability to rule out very small but real effects.

Challenges in Testing the Existential Threat Model

We aimed to test van Prooijen's (2020) existential threat model by examining whether the salience of an antagonistic outgroup would moderate the relationship between existential threat and belief in conspiracy theories. Attempting to test the implied interaction effect had challenges, particularly the ambiguity in how the model defines a salient antagonistic outgroup. This ambiguity led to specific interpretative decisions that may or may not be accurate depictions of the model's theoretical assumption, limiting our ability to thoroughly test this hypothesis.

First, we used a single item that focused on a specific conspiracy theory – the Chinese government's role in the COVID-19 pandemic, therefore limiting generalisability to other types of conspiracy beliefs. Second, dichotomising outgroup threats into high and low categories simplified the analysis but reduced the richness of the data, potentially losing important information and power (Altman & Royston, 2006). Lastly, we assumed that the outgroup threat was time-invariant based on participants' wave-one responses. However, the ICC value ($r = .70$) indicated only moderate stability, suggesting that perceptions of outgroup threat may fluctuate over time (Mulder & Hamaker, 2021). Despite these limitations, given

[8] The CMQ was measured with a 11-point scale, compared to a five-point scale for the BCT, therefore having a wider possible range, which increased the unstandardised within-person variance.

the lack of prior empirical studies directly testing this hypothesis, our study represents a valuable preliminary test of the existential threat model.

Future Directions

One key lesson from our study is that psychological theories about belief in conspiracy theories would benefit from clearer temporal specifications. Most existing theories, including the existential threat model, do not explicitly explain how these beliefs and their relationship with psychological factors such as distress unfold over time. For example, the existential threat model implies that conspiracy beliefs can evolve into a more rigid worldview, but it does not clarify whether this happens over days, weeks, months or years after distress is experienced. The lack of a temporal component in psychological theories is a common issue when designing longitudinal studies, where data collection intervals are often determined by practical or logical constraints (Hopwood et al., 2022). Because of this, it is unclear how long to expect an increase in belief in conspiracy theories after experiencing psychological distress.

Future research could focus more directly on whether acute, real-world stressors predict short-term increases in belief in conspiracy theories and whether this coincides with temporary reductions in distress. Theoretical frameworks (e.g., Douglas et al., 2017; van Prooijen, 2020) argue that beliefs in conspiracy theories initially develop as a coping response to an existential threat, potentially offering temporary relief from distress. However, these frameworks do not specify *how long* this short-term effect is expected to last. If belief formation occurs quickly (e.g., hours to days), then standard longitudinal study intervals (e.g., weeks to months) would likely be too long to detect.

Exploring whether these fluctuating short-term effects exist would clarify whether the time intervals are the current difficulty, with not finding an effect in most longitudinal studies. Intensive longitudinal methods might be better suited to identify the underlying temporal processes of short-term effects. For instance, ecological momentary assessment (EMA; Shiffman et al., 2008) could better track short-term fluctuations in psychological distress and belief formation, especially immediately following a major stressor.

This short-term dynamic also raises theoretical questions regarding how conspiracy beliefs are maintained. While existing theories tend to emphasise the triggering role of distress and existential threat, they tend to articulate less clearly how a belief is maintained over time. If belief in conspiracy theories fails to reduce distress over time, or actively worsens it, it is unlikely that relief from distress alone will sustain these beliefs. Given that conspiracy beliefs appear to be relatively stable and chronic, it is plausible that trait-like cognitive or personality factors rather than transient emotional states have an important role in maintaining them.

One possibility is brooding – a maladaptive form of rumination characterised by repetitive, negative thinking about distressing events (Blanke et al., 2022). Liekefett et al. (2024) found that experimentally inducing brooding led to a statistically significant increase in conspiracy beliefs (d = .19). They argue that brooding may increase conspiracy beliefs by narrowing a person’s focus to negative interpretations of the world, suggesting it could gradually reinforce and entrench such beliefs over time. Future research should explore whether brooding serves as a self-perpetuating cognitive process that supports conspiracy beliefs through recurrent negative thought patterns. As these effects might be slow to develop, and brooding does not appear to produce immediate, large shifts in conspiracy belief (Liekefett et al., 2024), longitudinal studies should test the cumulative effects of extended periods (i.e., six months to years), to understand whether such thoughts patterns contribute to how belief in conspiracy theories may become entrenched.

In addition, future research could benefit from refining theoretical frameworks, particularly the existential threat model, to more clearly address different types of threat and social meaning attached to outgroups. It is possible that our measure of outgroup threat was too narrow, or that more nuanced distinctions –such as between symbolic and realistic threats – might be needed (see Rios et al., 2018). The model could be improved by accounting for how outgroups are perceived as threatening. For example, it is possible that in the case of the China-based conspiracy belief, the outgroup might have been seen as a realistic threat when perceived to undermine the economic welfare of the ingroup. In contrast, a symbolic threat might be more relevant in a context like the US, where the outgroup might be viewed as threatening the national identity or symbolic power. The existential threat model does not currently make this specific distinction. Instead, distressing social events are perceived as threats, leading to a general suspicion of the targeted outgroup, increasing the tendency toward belief in conspiracy theories. Integrating ideas from intergroup threat theory (Stephan et al., 2016) could enhance the existential threat model and guide future research on more accurately testing the implied interaction effect.

Practical Implications

Several authors have argued that reducing psychological distress, particularly through stress-reduction interventions, could indirectly decrease belief in conspiracy theories (e.g., Fournier & Varet, 2023; Pfeffer et al., 2022; Scheffer et al., 2022; Swami et al., 2011). For example, Fournier and Varet (2023) proposed that "emotional interventions could be tested and implemented… then it would offer another path to counteract conspiracy beliefs in healthcare" (p. 15). Similarly, Pfeffer et al. (2022) highlighted the need for targeted stress management during times of crisis to reduce conspiracy beliefs. However, many of these recommendations are tentative, with authors emphasising the need for further research (Fournier & Varet, 2023; Swami et al., 2016).

The findings from this study, in combination with other longitudinal studies (e.g., Chan et al., 2023; Liekefett et al., 2023), suggest that if psychological distress reduction does influence belief in conspiracy theories, the resulting effect is likely to be small. Stress-reduction interventions have been found to have only small effects on anxiety and stress levels in a recent meta-analysis (Fischer et al., 2020). Since the effect of distress on conspiracy beliefs is also small, any indirect effect would likely be very small. This is because an indirect effect depends on multiplying the effect of stress-reduction on distress with the effect of distress on conspiracy beliefs. Since both of these effects are small, the combined indirect effect is even smaller. Therefore, while stress-reduction interventions still have value for other reasons, their efficacy in reducing belief in conspiracy theories is likely to be limited. That said, it is possible that certain individuals or specific contexts could show a stronger relationship between distress and conspiracy beliefs, and current research designs might not effectively capture such nuanced effects.

Conclusion

In conclusion, this study aimed to understand the causal effects between anxiety, depression, stress, and conspiracy beliefs using a preregistered longitudinal design. A small but significant within-person effect was found, with anxiety increasing belief in conspiracy theories. No significant within-person effects were observed for the remaining 14 hypotheses. Additionally, the confidence intervals of most within-person effects were relatively narrow, suggesting the estimated effects were relatively precise. These results indicate that if psychological distress does causally affect belief in conspiracy theories, the effect is likely to be small. This study contributed to the growing body of longitudinal research that has found limited evidence for causal effects between psychological distress and conspiracy beliefs in either direction. While cross-sectional studies frequently document associations between anxiety, depression, stress, and conspiracy beliefs, the current findings highlight that such associations do not necessarily represent causal effects.

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Appendix A

Attention Checks

The following attention checks were included in the survey at each wave. The number of participants who failed or missed an attention check is provided in Table A1. The way in which each participant was determined to have failed an attention check is outlined below. Each attention check was created to meet Prolific's attention check guidelines (see Prolific Team, 2022)

Table A1

Number of participants who failed or missed an attention check.

Wave	Nonsensical Failed	Nonsensical Missed	IMC Failed	IMC Missed	Total Excluded
1	1	4	1	2	7
2	3	3	1	1	7
3	1	6	6	6	14
4	7	4	2	5	14
5	2	21	1	23	25
6	4	2	1	3	8
7	3	11	0	10	14

Note. Some participants failed or missed both attention checks – for example, at time one, 8 people in total failed or missed an attention check, but only 7 were excluded, due to one participant failing or missing both attention checks. At time five, there was an issue with Qualtrics, and it recorded blank incomplete submissions, resulting in 21 submissions that missed nonsensical items, and 23 that missed the IMC. All of these participants returned to complete the survey in full. The majority of participants across all waves missed an attention check due to incomplete submissions being recorded in Qualtrics.

Table A2

Instructional Manipulation Check (IMC) Items

Wave	Item	Answer Choices	Correct Answer
1	Instructional Manipulation Check: The city test you are about to take part in is very simple, when asked for your favourite city you must select ‘Pawnee'. This is an attention check.	Auckland, Pawnee, Scranton, Eagleton, Perth	Pawnee (2) considered correct.
2	Instructional Manipulation Check: The weekday test you are about to take part in is very simple, when asked for the last day of the traditional work week, you must select ‘Friday’. This is an attention check.	Monday, Tuesday, Wednesday, Thursday, Friday	Friday (5) considered correct.
3	Instructional Manipulation Check: To ensure data quality, please choose 'Strongly disagree' for this item.	Strongly disagree, Moderately disagree, Neither agree nor disagree, Moderately agree, Strongly agree	Strongly disagree (1) considered correct.
4	Instructional Manipulation Check: The giraffe test you are about to take part in is very simple, when asked for the task a giraffe (a large African mammal) cannot do, you must select ‘Perform open heart surgery’. This is an attention check.	Eat leaves off trees, Eat grass, Perform open heart surgery, Run fast over short distances	Perform open heart surgery (3) considered correct.
5	Instructional Manipulation Check: The name test you are about to take part in is very simple, when asked to select a name, you must select ‘Harper’.	Tanya, Valentina, Daphne, Harper, Ethan	Harper (4) considered correct.
6	Instructional Manipulation Check: The name test you are about to take part in is very simple, when asked to select a name, you must select ‘Joel’.	Ellie, Bill, Marlene, Joel, Tess	Joel (4) considered correct.
7	Instructional Manipulation Check: The name test you are about to take part in is straightforward. When asked to select a name, you must select ‘Ted’.	Zava, Will, Coach Beard, Ted, Rebecca	Ted (4) considered correct.

Table A3

Nonsensical Items

Wave	Item
1	Every morning I run up Mount Everest to eat breakfast.
2	I have personally visited the planet Neptune.
3	I have never used a computer.
4	Every day I crash my car into a brick wall at high speeds because it is fun.
5	I own a pet stingray and take it to work every day.
6	In the last ten years, I have left earth to visit the moon.
7	I own a submarine and use it to fly amongst the clouds.

Note. A 4-point Likert response scale was used for all nonsensical items. Responses of "somewhat disagree" or "strongly disagree" were scored as correct for all 7 waves.

Appendix B

Mean Responses to Measures Over Time

The following indicates the level of endorsement of each item of the GAD-7, PHQ-8, PSS-10, and outgroup threat over time.

Figure B1

Mean responses over time to GAD-7 items

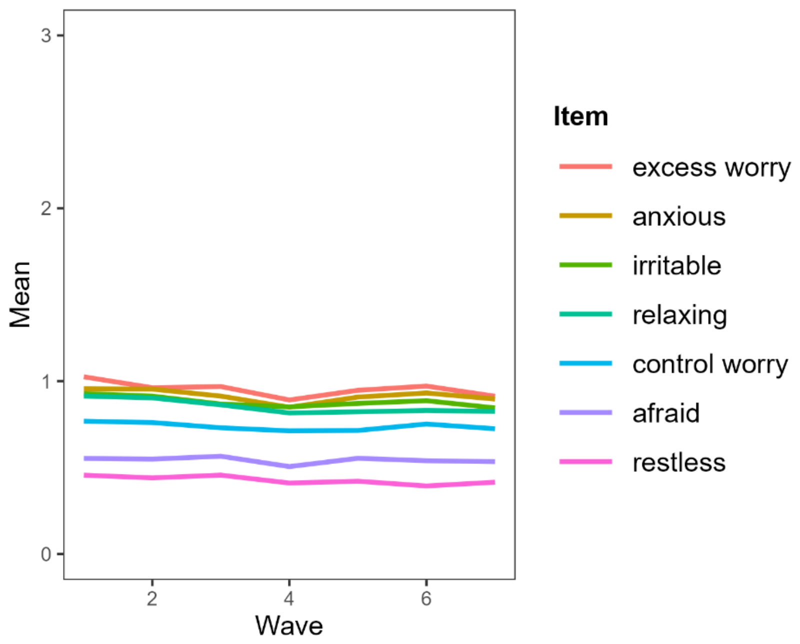


Figure B2

Mean responses over time to PHQ items

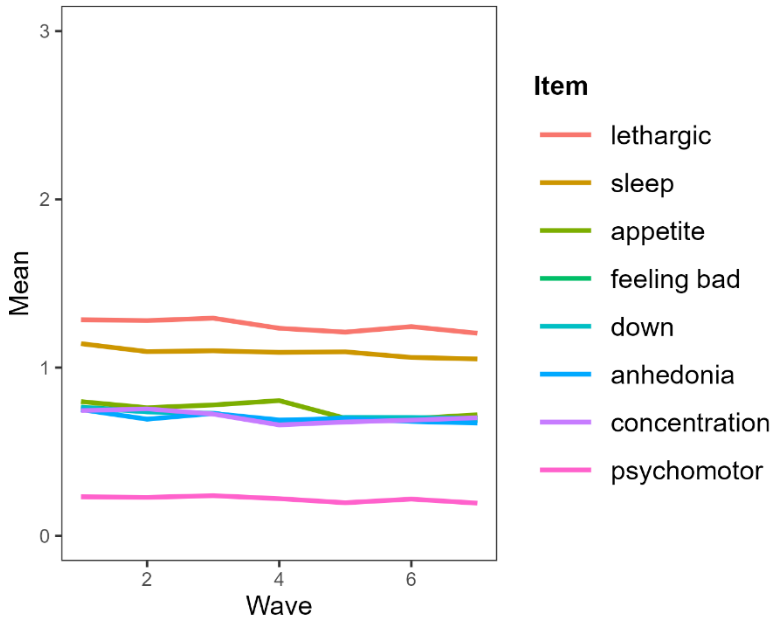


Figure B3

Mean responses over time to PSS items

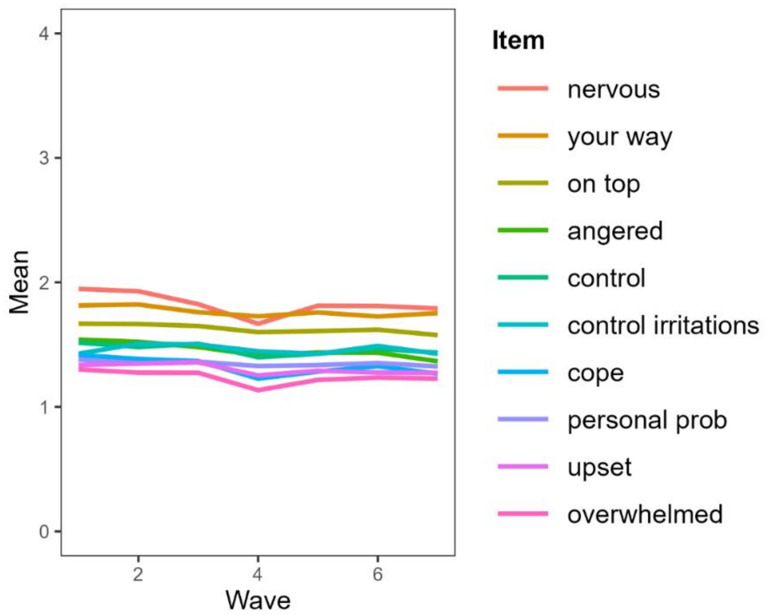
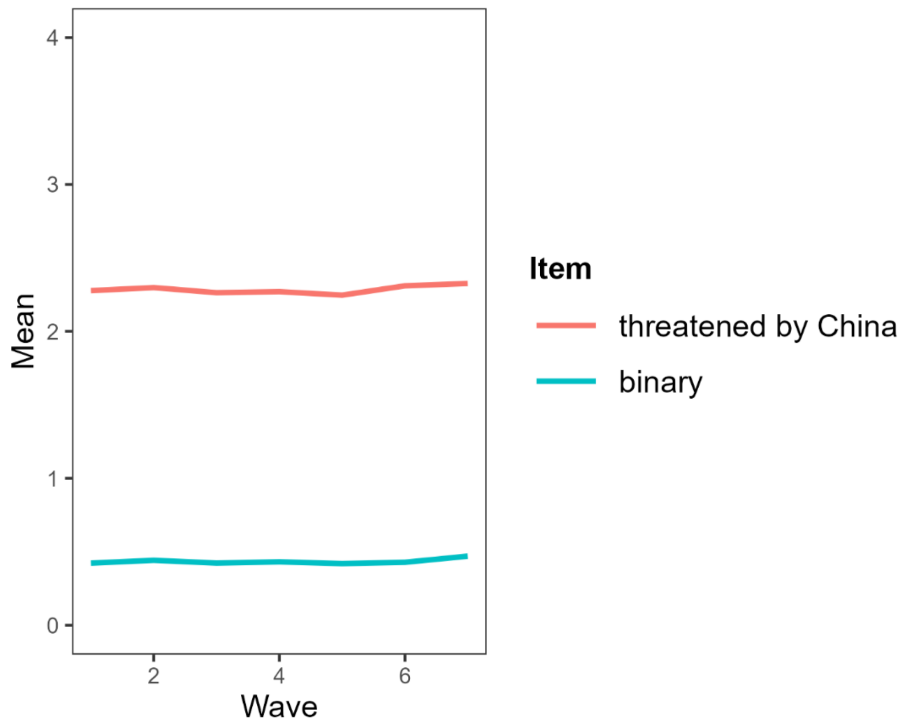


Figure B4

Mean responses over time to outgroup threat item



The figure B4 indicates the mean response to outgroup threat over time. Minimal changes were found on average across all waves.

Appendix C

Measurement Invariance Testing Across Waves

This appendix provides detailed results for measurement invariance testing across waves for all RI-CLPMs. Measurement invariance ensures that latent constructs (e.g., belief in conspiracy theories, depression) maintain consistent measurement over time, a key assumption for interpreting longitudinal data.

As described in the main text, the reported multiple RI-CLPMs assume strong factorial invariance, consistent with preregistration. This involved constraining factor loadings, intercept, and regression coefficients to equality across time points. A series of nested models was tested to assess this assumption. This included configural invariance (no constraints), weak factorial invariance (factor loading constrained), strong factorial invariance (factor loadings, intercepts, and autoregressive parameters constrained), and strong factorial invariance with additional constraints (cross-lagged parameters constrained). For tests of measurement invariance, see Tables C1, C2, C4, C6, C7, and C9.

In assessing the tenability of these constraints over time, we followed the recommendations by Chen (2007), who suggested thresholds of ΔCFI $\leq$ .01, ΔRMSEA $\leq$ .015, and ΔSRMR $\leq$ .03 ($\leq$ .01 for strong invariance). Comparing changes in RMSEA, CFI, and SRMR with increased constraints is also informative as to the degree to which these constraints affected model fit. In interpreting these changes, across the multiple-indicator models (M1, M2, M4, M6, M7, and M9), changes in these fit indices remained within these thresholds when applying configural, weak, and strong invariance, providing support for strong factorial invariance.

In contrast, models involving stressful life events (M3 and M8) and the multiple group model (M5) were estimated using single-indicator RI-CLPMs, and it was not possible to test measurement invariance. Instead, the tables below (Tables C3, C5, and C8) present results indicating whether constraining the cross-lagged and autoregressive parameters across time resulted in reduced model fit.

Table C1

Model 1 Fit indices of nested models for measurement invariance testing of belief in conspiracy theories and depression

Model No.	Model	χ^2	df	CFI	RMSEA [90% CI]	SRMR	$\Delta\chi^2$	Δdf	p ($\Delta\chi^2$ test)	ΔCFI	ΔRMSEA	ΔSRMR
M1_1	Configural invariance	13247*	8424	.963	.024 [.024, .025]	.026	—	—	—	—	—	—
M1_2	Weak factorial invariance	13438*	8526	.963	.024 [.024, .025]	.027	190.89	102	< .001	.000	.000	.001
M1_2a	Strong invariance (no CL constraints)	14040*	8628	.959	.025 [.025, .026]	.027	601.80	102	< .001	-.004	.001	.000
M1_3	Strong factorial invariance	14092*	8648	.959	.025 [.025, .026]	.027	653.66	122	< .001	.000	.000	.000

Notes. $^*p < .001$; $N = 970$; χ^2 = chi-square; df = degrees of freedom; CFI = comparative fit index; RMSEA = root mean squared error of approximation; SRMR = standardised root mean squared residual; $\Delta\chi^2$ = chi-square difference test; Δdf = degrees of freedom in chi-square difference test; ΔCFI, ΔRMSEA, and ΔSRMR represent changes from the previous model.

Table C2

Model 2 Fit indices of nested models for measurement invariance testing of belief in conspiracy theories and anxiety

Model 2 Fit Indices of Nested Models for Measurement Invariance Testing of Belief in Conspiracy Theories and Anxiety

Model No.	Model	χ^2	df	CFI	RMSEA [90% CI]	SRMR	$\Delta\chi^2$	Δdf	*p* ($\Delta\chi^2$ test)	ΔCFI	ΔRMSEA	ΔSRMR
M2_1	Configural invariance	17084*	10322	.950	.026 [.025, .027]	.030	—	—	—	—	—	—
M2_2	Weak factorial invariance	17291*	10436	.950	.026 [.025, .027]	.030	206.41	114	< .001	0.000	0.000	0.000
M2_2a	Strong invariance (no CL constraints)	17928*	10550	.946	.027 [.026, .028]	.031	637.00	114	< .001	-.004	0.001	0.001
M2_3	Strong factorial invariance	17999*	10570	.946	.027 [.026, .028]	.031	707.92	134	< .001	0.000	0.000	0.000

Note. $^*p < .001$.

Table C3

Model 3 Fit indices of nested models for measurement invariance testing of belief in conspiracy theories and stressful life events

Model No.	Model	χ^2	df	CFI	RMSEA [90% CI]	SRMR	$\Delta\chi^2$	Δdf	p ($\Delta\chi^2$ test)	ΔCFI	ΔRMSEA	ΔSRMR
M3_1	No constraints	120.97*	57	.995	.034 [.026, .042]	.040	—	—	—	—	—	—
M3_2	Constraints over time	550.33*	99	.966	.069 [.063, .074]	.058	439.36	42	< .001	-0.029	0.035	0.018

Note. $^{*}p < .001$. The difference between these two models is that in the second model, the autoregressive effects, cross-lagged effects, and residual (co)variances of within-person variables were constrained to be constant over time/waves.

Table C4

Model 4 Fit indices of nested models for measurement invariance testing of belief in conspiracy theories and anxiety

Model No.	Model	χ^2	df	CFI	RMSEA [90% CI]	SRMR	$\Delta\chi^2$	Δdf	p ($\Delta\chi^2$ test)	ΔCFI	ΔRMSEA	ΔSRMR
M4_1	Configural invariance	12176*	7547	.965	.025 [.024, .026]	.027	—	—	—	—	—	—
M4_2	Weak factorial invariance	12331*	7643	.964	.025 [.024, .026]	.028	155.15	96	< .001	-0.001	0.000	0.001
M4_2a	Strong invariance (no CL constraints)	12923*	7739	.961	.026 [.025, .027]	.028	592.00	96	< .001	-.003	0.001	0.000
M4_3	Strong factorial invariance	12995*	7759	.960	.026 [.026, .027]	.028	663.16	116	< .001	-.001	0.000	0.000

Note. $^{*}p < .001$.

Table C5

Model 5 Fit indices for Multiple Group Testing

Model No.	Model	χ^2	df	CFI	RMSEA [90% CI]	SRMR	$\Delta\chi^2$	Δdf	p ($\Delta\chi^2$ test)	ΔCFI	ΔRMSEA	ΔSRMR
MG_1	No constraints	164.84*	114	.996	.030 [.019, .040]	.039	—	—	—	—	—	—
MG_2 ("5a")	Constraints over time	653.06*	198	.965	.068 [.062, .074]	.056	488.23	94	<.001	-0.031	0.036	0.017

Note. $^{*}p < .001$. The difference between these two models is that in the second model, the autoregressive effects, cross-lagged effects, and residual (co)variances of within-person variables were constrained to be constant over time/waves. In the main text, the latter model was then compared to one in which the cross-lagged effect of stress on belief in the conspiracy theory was held constant across groups, allowing us to test hypothesis 8.

Table C6

Model 6 Fit indices of nested models for measurement invariance testing of conspiracy mentality and depression

Model No.	Model	χ^2	df	CFI	RMSEA [90% CI]	SRMR	$\Delta\chi^2$	Δdf	p ($\Delta\chi^2$ test)	ΔCFI	ΔRMSEA	ΔSRMR
M6_1	Configural invariance	5604*	3882	.978	.021 [.020, .023]	.031	—	—	—	—	—	—
M6_2	Weak factorial invariance	5718*	3948	.978	.022 [.020, .023]	.032	114.09	66	< .001	0.000	0.001	0.001
M6_2a	Strong invariance (no CL constraints)	5859*	4014	.977	.022 [.021, .023]	.032	141.00	66	< .001	-.001	0.000	0.000
M6_3	Strong factorial invariance	5986*	4034	.975	.022 [.021, .024]	.028	268.19	86	< .001	-.002	0.000	-.004

Note. $^{*}p < .001$.

Table C7

Model 7 Fit indices of nested models for measurement invariance testing of conspiracy mentality and perceived stress

Model No.	Model	χ^2	df	CFI	RMSEA [90% CI]	SRMR	$\Delta\chi^2$	Δdf	p ($\Delta\chi^2$ test)	ΔCFI	ΔRMSEA	ΔSRMR
M7_1	Configural invariance	8220*	5204	.964	.024 [.023, .025]	.034	—	—	—	—	—	—
M7_2	Weak factorial invariance	8339*	5284	.963	.024 [.023, .025]	.034	119.48	78	< .001	-0.001	0.000	0.000
M7_2a	Strong invariance (no CL constraints)	8516*	5360	.962	.025 [.024, .026]	.034	177.00	76	< .001	-.001	0.001	0.000
M7_3	Strong factorial invariance	8602*	5380	.961	.025 [.024, .026]	.036	262.68	98	< .001	-.001	0.000	0.002

Note. $^*p < .001$.

Table C8

Model 8 Fit indices of nested models for measurement invariance testing of conspiracy mentality and stressful life events

Model No.	Model	χ^2	df	CFI	RMSEA [90% CI]	SRMR	$\Delta\chi^2$	Δdf	p ($\Delta\chi^2$ test)	ΔCFI	ΔRMSEA	ΔSRMR
M8_1	Basic model	185.42*	57	.987	.048 [.041, .056]	.052	—	—	—	—	—	—
M8_2	Constraints over time	536.35*	99	.956	.067 [.062, .073]	.068	350.93	42	< .001	-0.031	0.019	0.016

Note. * $p < .001$.

Table C9

Model 9 Fit indices of nested models for measurement invariance testing of conspiracy mentality and anxiety

Model No.	Model	χ^2	df	CFI	RMSEA [90% CI]	SRMR	$\Delta\chi^2$	Δdf	p ($\Delta\chi^2$ test)	ΔCFI	ΔRMSEA	ΔSRMR
M9_1	Configural invariance	4753.20*	3293	.982	.021 [.020, .023]	.032	—	—	—	—	—	—
M9_2	Weak factorial invariance	4822.78*	3353	.981	.021 [.020, .023]	.033	69.58	60	.186	-0.001	0.000	0.001
M9_2a	Strong invariance (no CL constraints)	4953.20*	3413	.981	.022 [.021, .023]	.033	130.42	60	< .001	0.000	0.001	0.000
M9_3	Strong factorial invariance	5096.68*	3433	.979	.022 [.021, .024]	.029	273.91	80	< .001	-.002	0.000	-.004

Note. * $p < .001$.

Appendix D

MLR Estimated Models

All models were estimated using MLR and converged without warnings. The following tables indicate the output of MLR regression coefficients compared to the ML estimator. Cross-lagged effects remained similar using both MLR and ML estimators for all models. The robust fit statistics of the MLR strong factorial invariance model are reported for all models, and most models had a slightly better fit using an MLR estimator. In addition, the sample-corrected robust outputs have been demonstrated to be superior for non-normal data (Brosseau-Liard et al., 2012; Brosseau-Liard & Savalei, 2014).

Belief in Specific Conspiracy Theories

Depression and belief in conspiracy theories

Table D1

Regression coefficients with MLR vs ML estimator

	MLR				ML			
	B	SE	*p*	95 CI	*B*	SE	*p*	95 CI
$PHQ_{(t-1)} \rightarrow BCT_{(t)}$	.03	.02	.209	[-.01, .06]	.03	.02	.111	[-.01, .06]
$BCT_{(t-1)} \rightarrow PHQ_{(t)}$	.05	.05	.381	[-.06, .15]	.05	.04	.237	[-.03, .12]
$PHQ_{(t-1)} \rightarrow PHQ_{(t)}$	.25	.04	.000	[.17, .34]	.25	.03	.000	[.20, .30]
$BCT_{(t-1)} \rightarrow BCT_{(t)}$	.29	.06	.000	[.18, .41]	.29	.03	.000	[.23, .35]

In terms of fit statistics, the Satorra-Bentler chi-square statistic ($SB\ \chi^2$ (8648) = 11,912.47, $p < .001$) was significant, but the CFI = .970, RMSEA .021 [.021, .022], and SRMR = .027 were within an acceptable range.

Perceived stress and belief in conspiracy theories

Table D2

Regression coefficients with MLR vs ML estimator

	MLR				ML			
	B	SE	*p*	95 CI	*B*	SE	*p*	95 CI
$PSS_{(t-1)} \rightarrow BCT_{(t)}$	.02	.02	.189	[-.01, .05]	.02	.01	.118	[-.01, .05]
$BCT_{(t-1)} \rightarrow PSS_{(t)}$	.06	.05	.237	[-.04, .16]	.06	.06	.193	[-.03, .15]
$PSS_{(t-1)} \rightarrow PSS_{(t)}$	.29	.04	.000	[.21, .36]	.29	.03	.000	[.23, .34]
$BCT_{(t-1)} \rightarrow BCT_{(t)}$	.29	.06	.000	[.17, .40]	.29	.03	.000	[.23, .35]

In terms of fit statistics, the Satorra-Bentler chi-square statistic (*SB* χ^2 (10,570) = 15,959.50, $p < .001$) was significant, but the CFI = .955, RMSEA .024 [.024, .025], and SRMR = .031 were within an acceptable range.

Stressful life events and belief in conspiracy theories

Table D3

Regression coefficients with MLR vs ML estimator

	MLR				ML			
	B	SE	*p*	95 CI	*B*	SE	*p*	95 CI
$SLE_{(t-1)} \rightarrow BCT_{(t)}$	-.01	.01	.154	[-.02, .00]	-.01	.01	.330	[-.02, .01]
$BCT_{(t-1)} \rightarrow SLE_{(t)}$	-.02	.05	.742	[-.11, .08]	-.02	.05	.722	[-.12, .08]
$SLE_{(t-1)} \rightarrow SLE_{(t)}$	.20	.04	.000	[.13, .27]	.18	.02	.000	[.14 .21]
$BCT_{(t-1)} \rightarrow BCT_{(t)}$	.17	.03	.000	[.11, .23]	.16	.02	.000	[.12, .19]

In terms of fit statistics, the Satorra-Bentler chi-square statistic (*SB* χ^2 (99) = 310.28, $p < .001$) was significant, and the RMSEA .070 [.061, .079], and SRMR = .063 were both outside the preregistered cut-offs. The CFI = .970 was in an acceptable range

Anxiety and belief in conspiracy theories

Table D4

Regression coefficients with MLR vs ML estimator

	MLR				ML			
	B	SE	*p*	95 CI	*B*	SE	*p*	95 CI
$GAD_{(t-1)} \rightarrow BCT_{(t)}$	.03	.02	.131	[-.01, .06]	.03	.01	.042	[.00, .05]
$BCT_{(t-1)} \rightarrow GAD_{(t)}$	.07	.06	.237	[-.04, .18]	.07	.04	.130	[-.02, .15]
$GAD_{(t-1)} \rightarrow GAD_{(t)}$	.21	.04	.000	[.14, .28]	.21	.03	.000	[.16, .26]
$BCT_{(t-1)} \rightarrow BCT_{(t)}$	.29	.06	.000	[.18, .41]	.29	.03	.000	[.23, .35],

In terms of fit statistics, the Satorra-Bentler chi-square statistic ($SB\ \chi^2$ (7759) = 10,898.35, $p < .001$) was significant, but the CFI = .971, RMSEA .022 [.021, .023], and SRMR = .028 were within an acceptable range.

Intergroup threat differences

MLR estimator was used for model 5, the multiple group RI-CLPM, and the results were similar to the ML estimator. The regression coefficients from perceived stress to belief in a specific conspiracy theory were not significant in either low threat or high threat groups. Additionally, the chi-square difference test was $\Delta\chi^2$ (1) = 0.025, p = 0.874, implying that cross-lagged effects for individuals with different levels of intergroup threat were not significantly different.

Conspiracy Mentality

Depression and conspiracy mentality

Table D5

Regression coefficients with MLR vs ML estimator

	MLR				ML			
	B	SE	*p*	95 CI	*B*	SE	*p*	95 CI
$PHQ_{(t-1)} \rightarrow CMQ_{(t)}$	-.09	.12	.459	[-.32, .15]	-.09	.09	.301	[-.26, .08]
$CMQ_{(t-1)} \rightarrow PHQ_{(t)}$	.00	.02	.884	[-.03, .04]	.00	.01	.849	[-.02, .03]
$PHQ_{(t-1)} \rightarrow PHQ_{(t)}$	.25	.04	.000	[.17, .34]	.25	.03	.000	[.20, .31]
$CMQ_{(t-1)} \rightarrow CMQ_{(t)}$	.78	.08	.000	[.62, .94]	.78	.04	.000	[.71, .85]

In terms of fit statistics, the Satorra-Bentler chi-square statistic (*SB* χ^2 (4034) = 5239.02, $p <$ .001) was significant, but the CFI = .983, RMSEA .019 [.017, .020], and SRMR = .028 were within an acceptable range.

Perceived stress and conspiracy mentality

Table D6

Regression coefficients with MLR vs ML estimator

	MLR				ML			
	B	SE	*p*	95 CI	*B*	SE	*p*	95 CI
$PSS_{(t-1)} \rightarrow CMQ_{(t)}$	-.03	.06	.670	[-.14, .09]	-.03	.05	.625	[-.13, .08]
$CMQ_{(t-1)} \rightarrow PSS_{(t)}$	.01	.01	.432	[-02, .04]	.01	.01	.412	[-.01 .03]
$PSS_{(t-1)} \rightarrow PSS_{(t)}$	.29	.04	.000	[.21, .37]	.29	.03	.000	[.24, .34]
$CMQ_{(t-1)} \rightarrow CMQ_{(t)}$	.30	.06	.000	[.19, .41]	.30	.03	.000	[.23, .36]

In terms of fit statistics, the Satorra-Bentler chi-square statistic (*SB* χ^2 (5380) = 7971.03, $p <$ .001) was significant, but the CFI = .966, RMSEA .023 [.033, .024], and SRMR = .036 were within an acceptable range.

Stressful life events and conspiracy mentality

Table D7

Regression coefficients with MLR vs ML estimator

	MLR				ML			
	B	SE	*p*	95 CI	*B*	SE	*p*	95 CI
$SLE_{(t-1)} \rightarrow CMQ_{(t)}$	-.02	.02	.366	[-.06, .02]	-.02	.03	.310	[-.05, .02]
$CMQ_{(t-1)} \rightarrow SLE_{(t)}$	.03	.01	.025	[.00, .06]	.03	.02	.027	[.00, .06]
$SLE_{(t-1)} \rightarrow SLE_{(t)}$	.18	.04	.000	[.11, .25]	.18	.02	.000	[.14 .21]
$CMQ_{(t-1)} \rightarrow CMQ_{(t)}$	.16	.03	.000	[.11, .22]	.16	.02	.000	[.13, .20]

In terms of fit statistics, the Satorra-Bentler chi-square statistic (*SB* $\chi^2(99) = 330.49$, $p < .001$) was significant, and the RMSEA .063 [.055, .070], and SRMR = .068 were outside the preregistered cut-offs. The CFI = .962 was within an acceptable range.

Anxiety and conspiracy mentality

Table D8

Regression coefficients with MLR vs ML estimator

	MLR				ML			
	B	SE	*p*	95 CI	*B*	SE	*p*	95 CI
$GAD_{(t-1)} \rightarrow CMQ_{(t)}$	-.12	.09	.166	[-.30, .05]	-.12	.07	.061	[-.25, .01]
$CMQ_{(t-1)} \rightarrow GAD_{(t)}$	.00	.02	.896	[-.04, .05]	.00	.03	.847	[-.03, .03]
$GAD_{(t-1)} \rightarrow GAD_{(t)}$	.21	.04	.000	[.14, .28]	.21	.03	.000	[.16, .26]
$CMQ_{(t-1)} \rightarrow CMQ_{(t)}$	.78	.10	.000	[.59, .96]	.78	.04	.000	[.70, .85]

In terms of fit statistics, the Satorra-Bentler chi-square statistic (*SB* χ^2 (3433) = 4508.71 $p < .001$) was significant, but the CFI = .985, RMSEA .019 [.018, .021], and SRMR = .029 were within an acceptable range.

Appendix E

Journal Article Based on Thesis

A manuscript has been prepared based on the doctoral thesis. This was submitted to the journal *Clinical Psychological Science* on 05/08/2024. The article underwent peer-review and has been revised and resubmitted on 01/05/2025, and was accepted for publication.

Appendix F

Survey Questionnaires

The questionnaire and specific wording of measures used in the data collection for this research can be found in the Open Science Framework repository here: https://osf.io/365q

Appendix G

Correlations between Random-Intercepts

In a multiple-indicator RI-CLPM, each item has its own random intercept. This brings complexity to reporting succinctly due to creating a large number of correlations. For example, in model 1, there are 171 correlations between random intercepts. Table G1 reports the standardised random intercept correlations between pairs of items across different constructs (e.g., in model 1, PHQ items correlated with BCT items). These correlations reflect between-person effects. For the sake of brevity, no random intercepts of items of the same construct were reported (e.g., item one of BCT with item two of BCT),

In the conspiracy mentality models, the random intercepts were almost all positive and significant. However, the random intercepts for belief in conspiracy theory models were more mixed, ranging from $r = -.11$ to $r = .15$, and approximately less than half were significant.

In contrast to the multiple indicator models, the single indicator models provide one clear cross-construct random intercept correlation. For stressful life events and belief in conspiracy theories, the standardised correlation was $r = .09$ ($p = .017$), and for stressful life events and conspiracy mentality, it was $r = .12$ ($p = .001$).

Table G1

Summary of Random Intercept Correlations for Multiple Indicator Models

Model	Variable Pairs	Correlation Range (r)	Median Correlation	Number of Pairs	Significant Correlations
Model 1	BCT (11 items) vs PHQ-8 (8 items)	-0.10 to 0.15	0.04	88	30 significant (34%)
Model 2	BCT (11 items) vs PSS-10 (10 items)	-0.10 to 0.23	0.06	110	51 significant (46%)
Model 4	BCT (11 items) vs GAD-7 (7 items)	-0.11 to 0.13	0.04	77	25 significant (32%)
Model 6	CMQ (5 items) vs PHQ-8 (8 items)	0.13 to 0.18	0.13	40	38 significant (95%)
Model 7	CMQ (5 items) vs PSS-10 (10 items)	0.11 to 0.29	0.15	50	50 significant (100%)
Model 9	CMQ (5 items) vs GAD-7 (7 items)	0.07 to 0.16	0.13	35	33 significant (94%)

Note. Significant at p = .05. All correlations standardised.

Appendix H

Bayes Factors for Estimated Effects

Bayesian hypothesis testing was conducted using the *bain* package in R (Hoijtink et al., 2019). Each hypothesised effect contrasted a null model (cross-lagged effect constrained to zero) with an alternative model (cross-lagged effect freely estimated). The prior variance was automatically set by the *bain* package using a fractional Bayes approach. This approach used a default prior derived from a minimal fraction of the likelihood function, providing a wide but data-informed prior distribution. This approach provides a conservative approach, favouring the alternative hypothesis only if substantial evidence is present.

The results are presented in Tables H1 and H2. The Bayes factors suggested positive to strong support in favour of the null hypotheses across all models. Evidence strength was interpreted according to the suggested thresholds by Kass and Raftery (1995) in Table H3.

Table H1

Bayes Factors for Cross-Lagged Coefficients: Effects of Depression, Stress and Anxiety on Belief in Conspiracy Theories and Conspiracy Mentality

Model	Effect Direction	Bayes factor	Direction	Interpretation	PMPb
1	Depression -> BCT	8.78	In favor of H0	Positive	0.898
2	Perceived stress -> BCT	9.20	In favor of H0	Positive	0.902
3	Stressful life events -> BCT	13.25	In favor of H0	Positive	0.930
4	Anxiety -> BCT	3.94	In favor of H0	Positive	0.797
6	Depression -> CMQ	18.24	In favor of H0	Positive	0.948
7	Perceived stress -> CMQ	27.64	In favor of H0	Strong	0.965
8	Stressful life events -> CMQ	14.15	In favor of H0	Positive	0.934
9	Anxiety -> CMQ	5.35	In favor of H0	Positive	0.843

Notes. The Bayes factor is calculated using a null/constrained model in the numerator. PMPb = posterior probability of constrained/null model with cross-lagged effect fixed to zero. Calculated by assuming equal prior odds for constrained/null and alternate models.

Table H2

Bayes Factors for Cross-Lagged Coefficients: Effects of Belief in Conspiracy Theories and Conspiracy Mentality on Depression, Stress and Anxiety

Model	Effect Direction	Bayes factor	Direction	Interpretation	PMPb
1	BCT -> depression	15.48	In favor of H0	Positive	0.939
2	BCT -> perceived stress	13.35	In favor of H0	Positive	0.930
3	BCT -> stressful life event	23.59	In favor of H0	Strong	0.959
4	BCT -> anxiety	9.89	In favor of H0	Positive	0.908
6	CMQ -> depression	30.52	In favor of H0	Strong	0.968
7	CMQ -> perceived stress	22.25	In favor of H0	Strong	0.957
8	CMQ -> stressful life event	4.55	In favor of H0	Positive	0.820
9	CMQ -> anxiety	30.57	In favor of H0	Strong	0.968

Notes. The Bayes factor is calculated using a null/constrained model in the numerator. PMPb = posterior probability of constrained/null model with cross-lagged effect fixed to zero. Calculated by assuming equal prior odds for constrained/null and alternate models.

Table H3

Interpretation of Bayes Factors (Kass & Raftery, 1995)

Bayes Factor (BF)	Interpretation
< 1	Evidence against H_0
1 – 3	Weak evidence for H_0
3 – 20	Positive evidence for H_0
20 – 150	Strong evidence for H_0
> 150	Very strong evidence for H_0

Appendix I

Please note: This research case study was completed as a requirement for the Doctor of Clinical Psychology programme (DClinPsych Part C: 175.993) at Massey University and received a passing grade in December 2024. It includes reflections on my role as an Intern Psychologist during 2024. The case study is presented here as a programme requirement and is separate from the doctoral thesis. It has been examined separately as part of the DClinPsych clinical internship examination process. As such, doctoral thesis examiners are not expected to read it.

Research Case Study

As a clinical psychologist, integrating research with clinical practice is an important aspect of the scientist-practitioner model (Jones & Mehr, 2007). My experience in completing doctoral research assisted me in understanding what rigorous quantitative research might look like. My clinical internship, based in acute inpatient mental health services, required me to apply this research knowledge to applied practice. My understanding of how to evaluate literature benefited the subsequent application of translating the conceptual challenges of academic knowledge into applied practice.

Integrating Research with Clinical Practice

In my experience of integrating my research learnings into clinical practice, three general areas appeared to be particularly important: engaging with open science practices, defining and distinguishing conspiracy theories in clinical settings, and navigating the complexities of working with conspiracy theories.

Open Science Practices

One of the distinctive aspects of my research was its alignment with the open science movement (e.g., Nosek et al., 2019). Open science practices encourage greater transparency, reproducibility, and accessibility of research. This includes practices like preregistering studies, sharing data and materials, and publishing research through open access. The goal is to improve scientific integrity, reduce biases, and ensure that research findings are more reliable and widely available (Haeffel, 2022).

Despite open science practices growing within psychology, clinical psychology has been slow to adopt them. As Howard et al. (2024) point out, clinical psychology has been a fringe participant in the open science movement. This has limited its ability to benefit from increased transparency and reproducibility, particularly when it comes to evaluating and delivering therapeutic interventions.

As someone training under the scientist-practitioner model (Jones & Mehr, 2007), I found that open science practices offered a lens to evaluate research critically. This supported me in thinking more carefully about whether the research would be appropriate to apply to my practice and the limitations of the subsequent application if I did integrate the research. In doing so, I became more aware of the methodological weaknesses in some clinical psychology research that make it difficult to distinguish between an unhelpful treatment and a clinically relevant one – a known limitation in the field (Schleider, 2022). Applying weak or poorly supported evidence in clinical settings must be cautiously approached, carefully evaluating the possible harms it could cause. Because of this, I took a careful and critical approach when integrating scientific literature into my clinical practice. Engaging with open science for my doctoral research and clinical practice ultimately strengthened the rigour of my research and assisted me in assessing the quality of evidence in my clinical practice.

I also encountered some practical challenges in translating evidence-based interventions into clinical practice. For example, many randomised controlled trials (RCTs) in acute inpatient settings fail to provide accessible intervention materials. Schleider (2022) found that only 7% (2 out of 27) of mental health interventions tested in low to middle-class populations included the necessary materials to implement them. Even when the RCTs may provide evidence supporting the interventions' efficacy, the lack of transparency around what the content involved and how it was delivered makes it difficult to put that evidence into practice. I experienced this firsthand while designing a psychology group for an inpatient ward. While a handful of RCTs have attempted to validate such a group (e.g., Jacobsen et al., 2020), their methods were described in broad terms, with no specific materials openly shared. Attempts at contacting the research authors and requesting the materials were largely unsuccessful. As a result, I had to rely on my research abilities to piece together session plans based on broad descriptions in the article's methods section. This experience highlighted how open access to research materials can make a meaningful difference for clinicians implementing evidence-based care.

Defining and Distinguishing Conspiracy Theories in Clinical Practice

A key aspect of researching conspiracy theories is the debate of what a conspiracy theory is within psychology and adjacent fields such as philosophy and psychiatry. I adopted a neutral definition: a conspiracy theory is an explanation of an event or observation resulting from a conspiracy – multiple actors secretly plotting to do something harmful or unlawful (Swami et al., 2016). This definition of a conspiracy acknowledges that not all conspiracy theories are irrational, implausible, and unlikely to be true. It captures those conspiracies, such as the Watergate scandal (Woodward & Bernstein, 2007), that happened. The provided definition implies that careful exploration of the rationality of each conspiracy theory is needed before making a judgment of whether they are plausible.

While some conspiracies are real, a minority of the public believes in conspiracy theories that are unwarranted or strongly contradicted by evidence. For example, Marques et al. (2022) found that 7% of a demographically representative sample of Australians and New Zealanders believed in the "chemtrails" theory[9], which is disproven (Shearer et al., 2016). Such beliefs can blur the lines between unwarranted conspiracy theories and paranoid beliefs.

In my work with clients who had persecutory delusions, a severe form of paranoid belief, I reflected on the challenge of distinguishing between unwarranted conspiracy beliefs and paranoid beliefs. Initially, I considered these two constructs relatively distinct, as much of the literature suggests (see Imhoff & Lamberty, 2018). The underlying rationale for this distinction was that conspiracy beliefs are typically characterised by multiple actors secretly plotting harm against a group of people (e.g., the vapour trails conspiracy theory is not directly targeted at the individual) (van Prooijen, 2018). In contrast, paranoid beliefs are more characterised by the belief that others may intend to harm the individual (e.g., "I was convinced there was a conspiracy against me") (Alsuhibani et al., 2022). However, in clinical practice, this distinction is not always clear.

The DSM-5-TR describes persecutory delusions as involving beliefs that the individual is being conspired against, cheated, spied on, followed, poisoned or drugged, maliciously maligned, harassed, or obstructed in the pursuit of long-term goals (American Psychiatric Association, 2022). Many unwarranted conspiracy theories resonate with at least one of those themes. Recent research by Aminot et al. (2024) highlights the difficulty mental health clinicians have in distinguishing delusional beliefs and conspiratorial beliefs. In an experimental study involving 198 forensic mental health clinicians in Canada and the US, 54% of clinicians reported confidence in distinguishing between psychotic disorders and

[9] Vapour trails left by aircraft are actually chemical agents deliberately sprayed in a clandestine program directed by government officials.

conspiracy theories. However, most clinicians also indicated they lacked sufficient training to effectively make this differentiation.

The DSM-5-TR also acknowledges the challenges of distinguishing a delusion and a strongly held idea (American Psychiatric Association, 2022; p. 208), suggesting that this depends on the degree of conviction with which the belief is held, even in the face of clear or reasonable contradictory evidence. The dichotomous evaluation of whether a belief is delusional requires judgment to determine whether a belief is false, a process similar to evaluating unwarranted conspiracy theories. However, even unwarranted conspiracy beliefs can be held with strong conviction, as belief in conspiracy theories tends to be relatively stable (Williams et al., 2022). Therefore, conviction alone is insufficient to distinguish between a delusion and a strongly held idea.

I found the distinction challenging in clinical practice, especially when unwarranted conspiracy and paranoid beliefs were present. To explore this challenge, I researched how leading paranoia researchers would approach such situations (e.g., Alsuhibani et al., 2022). One solution was to focus on conviction in a belief, alongside the level of distress the belief may be causing the person. For example, Freeman and Garety (2000) argue that diagnostic criteria for persecutory delusions should include not only the belief in harm but also the intention behind it and the distress it causes. Indeed, in the research by Aminot et al. (2024), clinicians were more likely to accurately differentiate between delusional and conspiratorial beliefs when the level of distress was considered.

The presence and level of distress caused by a belief may be a helpful way to distinguish between an unwarranted belief in a conspiracy theory and a paranoid belief. My doctoral research focused on whether beliefs in unfounded conspiracy theories cause psychological distress. The findings suggest that, for the most part, belief in unwarranted conspiracy theories does not cause significant psychological distress. In contrast, clinical levels of paranoia do cause significant distress, pointing to a distinction between the two types of beliefs. This distinction is important because it helps identify whether a client is expressing conspiracy beliefs, paranoid beliefs, or both, and it provides a framework for understanding these beliefs without pathologising them unnecessarily.

During my internship, I worked with a number of clients experiencing psychosis, many of whom also presented with symptoms of paranoia. This clinical experience sparked my interest in the potential overlap between unwarranted conspiracy and paranoid beliefs. I noticed that some unwarranted beliefs in conspiracy theories included paranoid features, while some paranoid presentations had overt conspiratorial themes. This overlap got me thinking about where – and how – we draw the line between the two. Such a distinction is not always obvious, and a substantial degree of subjectivity is involved. It highlighted the importance of careful clinical judgment in making these distinctions.

Navigating Conspiracy Beliefs in Clinical Practice

Working with possible unwarranted belief in conspiracy theories in clinical settings made me realise how challenging it can be to translate academic literature to real-world practice. In my research, I took a neutral stance on conspiracy theories, recognising that they are not always inherently wrong and should be judged based on the evidence of each claim (Buenting & Taylor, 2010; Pigden, 1995). However, I also noticed that both the general public and some academics tend to view conspiracy theories from a more negative light, often assuming that they are false by definition (e.g., generalists; Cassam, 2016).

As scientist-practitioners, applying academic literature about conspiracy theories to clinical practice has a number of challenges. A key challenge is deciding whether an unwarranted conspiracy belief is more clinically significant, like a paranoid belief resembling a persecutory delusion. This distinction relies heavily on clinical judgment. Understanding how plausible a belief is can have serious diagnostic implications, especially when considering whether such a belief is symptomatic of a possible mental health-related issue. This assessment demands careful thought, not just about the client's belief but also our assumptions, such as a tendency toward a more pejorative view of conspiracy theories. I found that being aware and consistently reflecting on my beliefs and possible biases helped me stay open-minded and better positioned to make thoughtful, balanced decisions.

That awareness also shaped how I worked with clients. I found that treating an individual's beliefs respectfully and without judgment helped build trust. Many clients with paranoid beliefs had previously felt dismissed by clinicians, family, and friends. For them, their beliefs were tied to genuine distress and providing these individuals with space to express their beliefs without judgment allowed me to validate their experiences. This, in turn, helped strengthen the therapeutic relationship and encouraged clients to open up about their belief systems. Once I gained an understanding of their belief system, it became easier to appreciate why they held these beliefs, evaluate their plausibility, and determine whether they might be part of a broader clinical picture.

Of course, this approach takes time – something that's not always abundant in an inpatient setting. Thoroughly understanding each client's specific belief system can be a lengthy process. But even within those constraints, taking the time to understand a client's beliefs remains an important aspect of clinical practice and is often worth the effort.

Concluding Remarks

Before starting this internship, I didn't realise how much my doctoral research would shape my thinking and work as a clinician. One of the biggest areas of growth has been learning to evaluate quantitative research critically, particularly how to assess the methodology and the feasibility of the conclusions drawn by the researchers. This skill has been at the core of my development as a scientist-practitioner and proved especially useful during my internship, where I often needed to sift through research to determine which research findings would be helpful for the clients I was working with.

My research on belief in conspiracy theories also pushed me to carefully distinguish between unwarranted conspiracy beliefs and paranoid beliefs that might be clinically relevant. Being forced to explore these grey areas required me to think carefully about the underlying assumptions I might be making. However, it ultimately helped me approach my clinical work with more nuance and empathy. It is this which I believe has helped me both in research and practice.

Finally, working closely with clients in acute mental health services, especially those experiencing severe mental illness, has been an incredibly rewarding and humbling part of my internship year.

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