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

Nick D. Fox[1]

Matt N. Williams[1,2]

Stephen R. Hill[1]

[1]Massey University, New Zealand.

[2]Corresponding author. Email: M.N.Williams@massey.ac.nz

Abstract

Previous research has found positive correlations between manifestations of psychological distress such as anxiety, depression, stress, and belief in conspiracy theories. However, it remains unclear whether these relationships represent causal effects. We therefore tested whether anxiety, depression, and stress affect (and are affected by) belief in conspiracy theories in a preregistered longitudinal study. We sampled participants from Australia, New Zealand, and the United Kingdom ($N = 970$), with seven monthly waves (October 2022 to 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 cross-lagged effect of anxiety on belief in conspiracy theories. We also found no evidence of belief in conspiracy theories itself provoking psychological distress. Our findings provide grounds for scepticism of the notion that beliefs in conspiracy theories are often motivated by psychological distress or feelings of "existential threat."

Introduction

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). While conspiracies do happen, a non-trivial minority of the public express belief in conspiracy theories that are unwarranted or even strongly contradicted by evidence (for reviews see Douglas et al., 2017, 2019). For example, Marques et al. (2022) found that 7% of a demographically representative sample of Australians and New Zealanders agreed that "Vapor trails left by aircraft are actually chemical agents deliberately sprayed in a clandestine program directed by government officials" (p. 11). Such trails, of course, are simply *contrails*: frozen water vapour (Shearer et al., 2016).

Belief in Conspiracy Theories and Psychological Distress

In recent years, a number of studies have been dedicated to understanding the correlates, causes, and consequences of belief in conspiracy theories (for a review see Uscinski et al., 2022). One well-replicated observation is that belief in conspiracy theories is positively correlated with manifestations of psychological distress such as stress, anxiety, and depression. A meta-analysis by Bowes et al. (2023) found a mean correlation of $\bar{r} = .19$ (17 studies) between anxiety and belief in conspiracy theories. Another meta-analysis by Biddlestone et al. (2022) found a mean correlation with conspiracy theories of $\bar{r} = .16$ for anxiety (18 studies), and $\bar{r}$ = .08 for stress (two studies). A preregistered study by Williams et al. (2022) subsequently replicated a positive correlation between stress and belief in conspiracy theories (r = .20) Positive relationships between depression and belief in conspiracy theories have likewise been reported by de Coninck et al. (2021) and Green et al. (2023). Why might manifestations of psychological distress and belief in conspiracy theories be positively related?

Theoretical Background: Existential Motives

A popular perspective argues that three core psychological *motives* underlie belief in conspiracy theories (Douglas et al., 2017). The first of these motives is epistemic: The need to understand the world around us, and avoid uncertainty. The second is social: the desire to maintain a positive image of the self or groups one belongs to. The third is existential: The desire to feel safe and in control, especially in the face of threat. It is this third motive that is most salient to explaining a relationship between belief in conspiracy theories and psychological distress.

The notion of existential motives driving belief in conspiracy theories was elaborated in the existential threat model (van Prooijen, 2020). van Prooijen reasons that the perception of existential threat will make people more attentive to their environments, and increase "mental sense-making processes"—i.e., attempts to understand the causes of events, and especially whatever events or stimuli are causing them to experience existential threat. He suggests that this process in turn may lead to belief in conspiracy theories—but only in the existence of an antagonistic outgroup that appears to be salient when people are making sense of distressing events.

Existential Threat and Psychological Distress

van Prooijen (2020) defines existential threat in a broad fashion as "feelings of anxiety or uncertainty, often because of distressing events" (p. 1). He also interprets empirical findings involving anxiety, negative emotions, powerlessness, and death-related anxiety as bearing on the validity of his existential threat model. He states that "feelings of anxiety and uncertainty that emerge due to [anxiety-provoking societal events] often stimulate belief in conspiracy theories" (p. 10). We therefore interpret van Prooijen's theorising as implying that psychological distress should have a positive causal effect on belief in conspiracy theories, at

least assuming the presence of a salient antagonistic outgroup. This in turn provides an explanation for the observed relationship between psychological distress and belief in conspiracy theories: The former causes the latter. But does it?

Empirical Evidence for the Effects of Psychological Distress on Belief in Conspiracy Theories

While we know of no prior experimental studies testing for effects of depression or stress on belief in conspiracy theories, two experiments testing the effects of anxiety have been conducted. Grzesiak-Feldman (2013) found that assigning participants to complete a measure of belief in conspiracy prior to an exam (a high-anxiety condition) resulted in more conspiracy thinking about Jews than did a control condition. However, it is unclear whether Grzesiak-Feldman randomly assigned participants to conditions, raising the possibility that this observation could be attributed to pre-existing differences. Radnitz and Underwood (2017) found that assigning participants to an anxiety-provoking prime (writing about how they had been affected by the US financial crisis) increased belief in conspiratorial explanations for events described in a fictional vignette. However, the estimated effect was small, and the vignette approach may have limited external validity.

While these experimental studies are creative and informative, their findings leave a great deal of uncertainty. The most credible experiment design would involve a substantial manipulation of feelings of existential threat, with random assignment to conditions, and a measure of belief in conspiracy theories about the real world. None of the prior studies we are aware of unambiguously meet these standards, and to do so would present non-trivial practical and ethical difficulties.

An alternative source of evidence is longitudinal research, which can rule out some threats to internal validity. For example, a cross-lagged analysis can rule out the possibility

that an apparent relationship between predictor and outcome is actually due to an effect in the opposite direction to that hypothesised. This is a particularly important consideration for this topic: It is entirely plausible that beliefs involving powerful figures secretly seeking to do harm might provoke distress (as we will discuss further below). Some analysis methods for longitudinal data can also rule out all *time-invariant* (i.e., stable) confounding variables. In particular, this is true of the random intercept cross-lagged panel model (Hamaker et al., 2015). This capacity to rule out some alternative explanations for apparent relationships can mean that longitudinal studies can warrant credible, albeit tentative, causal inferences, even if they cannot rule out all alternative explanations (c.f., time-variant confounding variables; see Rohrer & Murayama, 2021). Beyond their capacity for supporting causal inferences, longitudinal studies also permit a focus on *within-person* variance—not just asking why some people believe conspiracy theories while others don't, but why people change their minds.

A small number of longitudinal studies estimating the effects of various forms of psychological distress on belief in conspiracy theories have been conducted. Leibovitz et al. (2021) found no significant relationship between anxiety and belief in conspiracy theories relating to COVID-19 in a panel study with two waves. In another study with two waves, Heiss (2021) found some evidence that threat perceptions led to increased belief in conspiracy theories, albeit not general conspiracy thinking. However, both of these studies used cross-lagged designs without random intercepts, meaning that their estimates could have been confounded by stable individual differences.

In contrast, Chan et al. (2023) used a RI-CLPM, providing better protection against confounding by stable individual differences. They estimated reciprocal effects between psychological distress (measured using the Depression, Anxiety and Stress Scale – 21) and belief in COVID-19 conspiracy theories in a study with five waves. They found no

significant cross-lagged effects. However, they used a RI-CLPM *without* multiple indicators (i.e., not fully accounting for measurement error), which could have biased their estimates.

A particularly rigorous longitudinal study was conducted by Liekefett et al. (2023). Liekefett et al. used the RI-CLPM model with multiple indicators, explicitly accounting for the effects of measurement error. They conducted two studies: One with four waves over two months (N = 405), and one with four waves over 18 months (N = 1012). In neither study did they find any evidence of cross-lagged effects of anxiety or existential threat on belief in conspiracy theories. That said, their studies each involved just four waves, and the power of the RI-CLPM can be very sensitive to the number of waves (see Mulder, 2021), so it is important to subject these ideas to further tests.

Effects *of* Belief in Conspiracy Theories on Psychological Distress

Whether or not psychological distress affects belief in conspiracy theories, it is entirely possible that the converse is true: that developing beliefs in conspiracy theories can cause distress. Indeed, if a person genuinely believes that the world is beset with evil plots by secretive and powerful agents, it seems reasonable to believe that this might make them perceive the world as more stressful and threatening. This effect might in turn explain observed relationships between psychological distress and belief in conspiracy theories.

The empirical studies discussed above (and particularly those using longitudinal designs and the RI-CLPM) provide some useful evidence bearing on this question. However, Chan et al. (2023) found no evidence of cross-lagged effects of distress on belief in COVID-19 conspiracy theories. Liekefett et al. (2023) found evidence of effects of conspiracy beliefs on anxiety and existential threat in Study 1, but not in Study 2. Overall, these findings still leave uncertainty about the effects of belief in conspiracy theories on psychological distress.

Knowledge about such effects is important, because it speaks to the general question of the risks posed by belief in conspiracy theories (see Douglas, 2021).

The Current Study

Considered in combination, the extant evidence suggests that various manifestations of psychological distress are positively correlated with belief in conspiracy theories. However, the evidence for a causal effect of distress on belief in conspiracy theories is much more tentative. Similarly, there is uncertainty about the degree to which belief in conspiracy theories itself causes psychological distress. We therefore designed a longitudinal study to answer these causal questions. We preregistered this study in detail, so as to permit readers confidence that the hypotheses were subject to severe tests (see Lakens, 2019).

We focus specifically on stress, depression, and anxiety as these are archetypal forms of psychological distress, and because all three have been subject to prior investigations in terms of their relationship with belief in conspiracy theories. With regard to stress, we study both perceived stress as well as an additional measure of stressful life events (as did Swami et al., 2016).

We hypothesised that depression (**H1**), perceived stress (**H2**), stressful life events (**H3**), and anxiety (**H4**) would have positive cross-lagged effects on belief in conspiracy theories. We also hypothesised most of the converse effects: That belief in conspiracy theories would have positive cross-lagged effects on depression (**H5**), perceived stress (**H6**), and anxiety (**H7**). We did not make any hypothesis about whether belief in conspiracy theories would lead to more stressful life events (e.g., suffering a financial crisis, being a victim of crime). It is possible that increased belief in conspiracy theories could bring about such events, but we expected that if any such effect held it would be too small relative to the other causes of such events to be detectable.

We also specified a hypothesis that specifically tests a key proposition of the existential threat model of belief in conspiracy theories: i.e., that the effect of existential threats (or distress) on belief in conspiracy theories depends on the salience of antagonistic outgroups. To our knowledge, no prior study has specifically tested this implied interaction effect. We therefore hypothesised that the cross-lagged effect of perceived stress on belief in a specific conspiracy theory (COVID-19 is a bioweapon created by China) will be more positive amongst people who perceive the outgroup implied in the conspiracy theory (the Chinese government) as threatening (**H8**).

Hypotheses about conspiracy mentality

Beliefs in conspiracy theories can be conceptualised literally as beliefs in specific theories, but they can also be conceptualised as indicators of a predisposition to conspiratorial explanations: A "conspiracy mentality" (see Imhoff & Bruder, 2014). Conspiracy mentality is conceptualised as a more stable individual difference than belief in conspiracy theories (Imhoff et al., 2022). As such, given the longitudinal context of this study we used belief in specific conspiracy theories as the key variable in our primary hypotheses (H1-H8) above. However, whether conspiracy mentality is *in reality* more stable than belief in specific conspiracy theories remains an open question, and specifying hypotheses about conspiracy mentality allowed us to test our key claims using an alternative well-validated measure (see Bruder et al., 2013).

We therefore hypothesised that depression (**H9**), perceived stress (**H10**), stressful life events (**H11**), and anxiety (**H12**) would have positive cross-lagged effects on general conspiracy mentality, and that general conspiracy mentality would have positive cross-lagged effects on depression (**H13**), perceived stress (**H14**), and anxiety (**H15**).

Transparency and Openness

Preregistration

The design and analysis plan was preregistered prior to data collection at https://osf.io/5k4yb/?view_only=d01066eb6c974fe48fd07c677fe09967

Data, Materials, Code, and Online Resources

Data, analysis code and materials are openly accessible at https://osf.io/365qr/?view_only=3c9afa31770d4e808ae3e4fceb815b8d

Reporting

We report how we determined our sample size, all data exclusions, all manipulations, and all measures in the study. This manuscript arises from a longitudinal project with multiple preregistered components; as such, a number of measures were collected. These are described briefly below; for full copies of the questionnaires, see our OSF project.

Ethical Approval

This study was approved by the Massey University Human Ethics Committee (Southern A, application SOA 22/42). It was carried out in accordance with the provisions of the Declaration of Helsinki (World Medical Association, 2013).

Method

Sample Size Determination

It is possible to conduct power analyses for the RI-CLPM via simulation, but the complexity of the models we specify below means that fitting a single model to a dataset can take several hours. Power analysis by simulation for the preregistered models was thus

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

First, power analysis for the single-indicator RI-CLPM (powRICLPM; Mulder, 2022) suggested that an estimated sample of 500 participants over seven waves would have 84% power to detect a relatively 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 et al. (2024).

Second, Liekefett et al. (2023, Study 2) conducted a four-wave study over one year focused on similar hypotheses and using the multiple-indicator RI-CLPM. Their sample size at wave 1 was 1,012, declining to 437 at wave 4. Despite this substantial attrition and using fewer waves than we did in this study, the standard error for cross-lagged effects 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).

Together, these considerations suggested that a sample size of approximately 1,000 and seven waves would allow the ability to estimate cross-lagged effects precisely, even with substantial attrition. Our target sample size for wave 1 was therefore 1,000.

Participants and Procedure

Participants were recruited from the crowdsourcing platform Prolific. The sample was drawn from participants aged 18 and over living in the United Kingdom, Australia, and New Zealand. These three countries were selected together to permit a sufficiently large sampling frame. All three countries are English-speaking Commonwealth countries with connected histories, and it was thus feasible to select conspiracy theory items that were relevant in all

three countries. A screening survey was applied to identify participants who wished to take part in the full longitudinal study; see the Supplemental Material for more details.

The assumed target population for inferences was the population of adults residing in the United Kingdom, Australia, and New Zealand. That said, our use of convenience sampling mean that substantial uncertainty surrounds our inferences about this population.

Wave 1 survey

The first wave of the survey was released on 3 October 2022. It was advertised to recruit 450 participants from the UK, 400 from Australia and 150 from New Zealand, for a total of 1,000 participants. The wave 1 survey included 89 questions in total, and participants were paid GBP1.65. Seven days were allocated for wave one data collection. However, all quotas for each country were filled within three days.

At wave 1, 1003 responses were received. The preregistered exclusion criteria were then applied. Participants were excluded if they indicated a country of residence other than the UK, Australia or NZ; if they failed or did not answer either of two attention checks; if their study duration was less than the number of items (89) multiplied by 2 (see Huang et al., 2012); if they did not reach the end point of the survey, or their Prolific submission timed out; or if they returned their Prolific submission (i.e., revoking consent). The remaining participants (N = 995) were then invited for all subsequent waves.

After wave 1, participants were invited to complete surveys every month for six months (seven waves total). 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 (with the exact number varying due to some conditionally displayed questions). For waves 2 to 7 participants were paid GBP1.30 each. Each survey in wave 2 to 8 was available for seven days, from the 3rd to the 10th of each month.

Exclusion Criteria

Exclusion criteria applicable at wave 1 are described above; participants who met these criteria were excluded from the study in its entirety. The exclusion criteria listed below were set at the wave level: If a participant met any one of these criteria at a given wave their responses from that wave were excluded, but their response at other waves were retained and they continued to receive invites for subsequent waves.

1. Failing or not responding to any one of the two attention checks at each wave
2. A study duration in seconds of less than the total number of items (65) multiplied by two (see Huang et al., 2012)
3. "Returning" their submission on Prolific (i.e., revoking consent)
4. Duplicate response, as detected via Prolific ID.
5. A Qualtrics status other than 0 (normal response – i.e., excluding previews and suspected duplicates).

In addition to these wave-level criteria, participants who took part only in wave 1 but no subsequent waves were excluded from analysis. This resulted in a final sample size of 970.

Attrition

Attrition was low (see Table 1). The median number of participants per wave after exclusions was 819 (82% of the wave 1 sample size), and 764 participants were still taking part at wave 7. Within completed surveys there was little missing data: Just ten missing data points across all 6,020 survey responses.

Table 1

Dates of Survey Waves

Wave	Survey open dates	*n*	% of final sample
1	3-5 October 2022*	970	100
2	3-10 November 2022	884	91
3	3-10 December 2022	851	88
4	3-10 January 2023	851	88
5	3-10 February 2023	844	87
6	3-10 March 2023	829	85
7	3-10 April 2023	798	82

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, necessitating the full seven days

Demographic Characteristics

Of the final sample, 38.9% resided in Australia, 15.1% in New Zealand, and 46.1% in the United Kingdom. 44.4% described themselves as men, 54.4% as women, and 0.9% as non-binary. Participants varied in age from 18 to 85 years (M = 40.3, SD = 13.1). A large majority (76.5%) held a university or other tertiary qualification. On a political orientation scale where 0 = extreme left and 10 = extreme right, the median response was 4 (i.e., slightly left of centre). Most participants were of European ethnicities; see Table 2. For more information about participants' demographics characteristics, see the Supplemental Material.

Table 2

Participant Ethnicities

Ethnicity	Frequency	Percent
United Kingdom (*n* = 447)		
White	397	88.8%
Asian / Asian British	24	5.4%
Black / Black British	20	4.5%
Other	10	2.2%
Australia (*n* = 377)		
European / White	266	70.6%
Asian (including South Asian)	103	27.3%
Aboriginal or Torres Strait Islander	4	1.1%
Other	19	5.0%
New Zealand (*n* = 146)		
European or Pākehā	101	69.2%
NZ Māori	10	6.8%
Asian (including South Asian)	39	26.7%
Pacific peoples	3	2.0%
Other	10	6.8%

Note. The percentages within each country sum to more than 100% because participants could select multiple ethnicities.

Measures

The following measures were used in all waves. Full copies of all questionnaires can be found on our OSF project.

Beliefs in specific conspiracy theories.

Belief in conspiracy theories was measured with items pertaining to 11 specific conspiracy theories. The items are found in Table 3. This measure was adapted from a measure constructed by Williams et al. (2024), which in turn drew items from a range of sources. A small number of revisions were made for the current study to enhance the contemporary relevance of items. 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. Cronbach's alpha at time 1 was .92.

Table 3

Items in the Belief in Conspiracy Theories Measure

	Item	Original 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 (2014)
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. These items were prefaced with the instructions "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.

Conspiracy mentality was measured using the 5-item Conspiracy Mentality Questionnaire (CMQ: Bruder et al., 2013). Each question was 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 responded to each item on an 11-point scale with options from 0 (0% - certainly not) to 10 (100% - certain). Cronbach's alpha at time 1 was .86.

Depression

Depression was measured using the 8-item Patient Health Questionnaire (PHQ-8), a measure of depression severity in the general population (Kroenke et al., 2009). The PHQ-8 differs from the better-known PHQ-9 by excluding an item about thoughts of suicide. It 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. Cronbach's alpha at time 1 was .90.

Anxiety.

Anxiety was measured using a brief 7-item Generalised Anxiety Disorder questionnaire (GAD-7; Spitzer et al., 2006). The measure asks participants about the degree to which they feel 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. Cronbach's alpha at time 1 was .92.

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 measurement, we changed the wording to refer to "the past two weeks". Participants respond to each item on a 5-point rating scale with options of never, almost never, sometimes, fairly often, and very often. Items 4, 5, 7 and 8 were reverse coded. Cronbach's alpha at time 1 was .92.

Stressful life events.

Although we regarded the perceived stress scale as our primary measure of stress, we also included the stressful life events scale (Lin et al., 2020). 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.

Cronbach's alpha at time 1 was low, $\alpha = .55$, presumably because the items in this scale are not indicators of some single underlying construct. Rather, this scale can be considered a *formative* measure (see Borsboom, 2008): The experiences probed in the items cause stress rather than the other way round. Given the formative status of the model we did not treat it as reflective in latent variable models, but instead created stressful life event score by summing each participants' responses to the 12 items at each wave. We then used this score as a single indicator of stressful life events.

Salience of an antagonistic outgroup (intergroup threat).

The salience of an antagonistic outgroup plays a key role as a moderator in van Prooijen's (2020) existential threat model. However, we were unable to identify any existing measure of this specific construct. We therefore chose to draw on the more established concept of intergroup threat perception (see Stephan et al., 2016). We reasoned that if a person perceives another group as threatening, it implies that they perceive this group as antagonistic to at least some degree.

We opted to select one specific theory in our measure of belief in specific conspiracy theories to link to a salient antagonistic outgroup. We selected the item "COVID-19 is a biological weapon intentionally created and released by China". This item was useful in this context given that it clearly implicates a specific group. As a measure of intergroup threat

(and consequently salience of an antagonistic outgroup), we presented the item, “I feel threatened by the Chinese Government.”

We provided four response options: strongly disagree, somewhat disagree, somewhat agree, and strongly agree. This allowed us to split participants into a low intergroup threat group (strongly & somewhat disagree) and a high intergroup threat group (strongly & somewhat agree). Doing so allowed us to treat intergroup threat as a dichotomous grouping variable, permitting us in turn to test for moderation in a multigroup RI-CLPM. More information can be found below under Data Analyses.

Attention checks.

Two types of attention check questions were used at each wave: a nonsensical item and an instructional manipulation check (IMC). A complete list of attention checks can be found in the Supplemental Material.

Additional measures.

This study forms part of an overarching longitudinal project incorporating several preregistered studies. Consequently, several other measures were included in the surveys that were not used in the data analyses reported here. These items included a 4-item measure of trust (Marques et al., 2021), 3-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 also asked whether they had heard of the specific made by each conspiracy theory in Table 3. The surveys in second and subsequent waves also prompted participants with an open-ended question if a substantial change in their agreement with a specific conspiracy theory was detected in comparison with the previous

time they had responded. More information can be found in the full copies of the surveys on our OSF project.

Data Analyses

All data analysis was completed using RStudio (version: 12.0+343) and the R programming language (version: 4.0.2) (R Core Team, 2021). We relied heavily on the packages lavaan (Rosseel, 2012), the tidyverse (Wickham et al., 2019), ICC (Wolak et al., 2012), psych (Revelle, 2022), here (Mueller, 2017), and likert (Sarstedt et al., 2020).

The majority of hypotheses were tested using multiple indicator RI-CLPMs (Mulder & Hamaker, 2021). The exceptions were the models used to test H3, H8, and H11. The models for H3 and H11 incorporated the stressful life events variable, which cannot be considered as a reflective construct (as discussed above), while the model for H8 included just a single conspiracy theory. For these analyses, the single-indicator RI-CLPM (Hamaker et al., 2015) was used. For H3 and H11, this was a deviation from the preregistration; we discovered during analysis that models including both multiple-indicator variables (belief in conspiracy theories, conspiracist mentality) and variables with single indicators (stressful life event score) in the same RI-CLPM could not converge.

We tested relationships between pairs of constructs (e.g., depression and belief in conspiracy theories) in separate models, rather than including all variables in one extremely large model. Doing so assisted against the risk of convergence failure in overly complex models.

We estimated models using full information maximum likelihood (FIML). This allowed us to account for missing data (see Enders & Bandalos, 2001) which was a substantial concern given the use of a longitudinal design. While our analyses assumed multivariate normality, we report robustness checks in the Supplemental Material using

"MLR" estimation (maximum likelihood with robust standard errors). These analyses leave our substantive conclusions largely unchanged.

Measurement invariance.

In multiple indicator RI-CLPMs, reported below, we assumed strong factorial invariance (i.e., loadings, intercepts, and regression coefficients were held constant across time). Doing so had two main advantages. First, it reduced the number of free parameters and therefore the risk of convergence failures. Second, for cross-lagged coefficients in specific, this facilitated clear inferential criteria (whereas if cross-lagged coefficients were permitted to vary over time, each hypothesis would involve six different cross-lagged coefficients). We report tests of measurement invariance in the Supplemental Material.

Results

Descriptive Statistics

To describe inter-individual variation in responses, we calculated each participant's mean response to all six measures at each time point (see Table 4). The mean conspiracy theory scores indicated that participants typically disagreed with each conspiracy theory. Participants' mean depression, anxiety and perceived stress scores were consistent with the presence of mild symptoms. Means were relatively stable over time for most variables.

Table 4

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

Notes. Means calculated using full information maximum likelihood.

Confirmatory Analyses

Each of the RI-CLPMs we estimate contain many "nuisance" parameters (e.g., variances, factor loadings, autoregressive effects, etc.) For the sake of brevity, we therefore focus our reporting below on the cross-lagged coefficients from the models. It is these coefficients that provide credible estimates of causal effects, and that we used to test our hypotheses via our preregistered inferential criteria.

Cross-lagged coefficients for models 1 to 4 (the models involving belief in specific conspiracy theories) can be found in

Table 5. Of the seven hypotheses tested via the output in this table, only one was supported: The estimated effect of anxiety on belief in conspiracy theories was positive and barely significant.

Cross-lagged coefficients for models 6 to 9 (the models involving conspiracy mentality) can be found in Table 6. Of the seven hypotheses tested via the output in this table, none were supported. The effect of conspiracy mentality on stressful life events, about which we had made no hypothesis, was positive and barely significant.

Table 5

Cross-lagged Coefficients: RI-CLPMs including Belief in Specific Conspiracy Theories

Model	Estimated effect	*b* [95% CI]	*p*	Estimated effect	*b* [95% CI]	*p*
1	Depression -> belief in conspiracy theories (H1)	.03 [-.01, .06]	.111	Belief in conspiracy theories -> depression (H5)	.05 [-.03, .12]	.237
2	Perceived stress -> belief in conspiracy theories (H2)	.02 [-.01, .05]	.118	Belief in conspiracy theories -> perceived stress (H6)	.06 [-.03, .15]	.193
3	Stressful life events -> belief in conspiracy theories (H3)	-.01 [-.02, .01]	.330	Belief in conspiracy theories -> stressful life events (no hypothesis)	-.02 [-.12, .08]	.722
4	Anxiety -> belief in conspiracy theories (H4)	.03 [.00, .05]	.042*	Belief in conspiracy theories -> anxiety (H7)	.07 [-.02, .15]	.130

Notes. *$p < .05$.

Table 6

Cross-lagged Coefficients: RI-CLPMs including Conspiracy Mentality

Model	Estimated effect	*b* [95% CI]	*p*	Estimated effect	*b* [95% CI]	*p*
6	Depression -> Conspiracy mentality (H9)	-.09 [-.26, .08]	.301	Conspiracy mentality -> Depression (H13)	.00, [-.02, .03]	.840
7	Perceived stress -> Conspiracy mentality (H10)	-.03 [-.13, .08]	.625	Conspiracy mentality -> Perceived stress (H14)	.01 [-.01, .03]	.412
8	Stressful life events -> Conspiracy mentality (H11)	-.02 [-.05, .02]	.310	Conspiracy mentality -> Stressful life events (no hypothesis)	.03 [.00, .06]	.027*
9	Anxiety -> Conspiracy mentality (H12)	-.12 [-.25, .01]	.061	Conspiracy mentality -> Anxiety (H15)	.00 [-.03, .03]	.847

Salience of antagonistic outgroups.

To test if the effect of perceived stress (summed score) on belief in a specific conspiracy theory ("COVID-19 is a bioweapon created by China") was the same for those with high levels of intergroup threat versus 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 (M5a) with no constraints across groups. Next, we fitted a model where all parameters could vary across groups, except for the cross-lagged parameter of perceived stress on belief in a specific conspiracy theory (M5b). Within model M5a, the estimated effect of perceived stress on belief in the theory within participants who perceived the Chinese government as threatening was b = .00, 95% CI [-.01, .01], p = .818. For participants who did not perceive the Chinese government as threatening, this estimated effect was nearly identical, b = .00, 95% CI [-.00, .01], p = .339. The chi-square difference test of models M5a and M5b was $\Delta\chi^2(1) = 0.154$, p = .695, implying no evidence to reject a null hypothesis that the cross-lagged parameter is identical in these two populations. Therefore, hypothesis 8 was not supported.

For fit statistics of the estimated models, see the Supplemental Material.

Discussion

Overall, we found almost no evidence for any effects of anxiety, depression, perceived stress, or stressful life events on belief in conspiracy theories or conspiracy mentality. Just one of the 15 preregistered hypotheses was supported, with a significant positive cross-lagged effect of anxiety on belief in conspiracy theories. That said, even this coefficient was only barely significant (p = .042), and did not remain significant when applying robust standard errors (see the Supplemental Material). Set against the number of

hypotheses we tested we thus consider this only very tentative evidence of an effect of anxiety on belief in conspiracy theories.

Our findings cohere with the closely-related longitudinal work of Liekefett et al. (2023) who found no evidence that anxiety, uncertainty aversion or existential threat produced increased belief in conspiracy theories. Considered in conjunction with Liekefett et al.'s findings, our findings provide reason for scepticism about the notion that psychological distress or existential threats (as suggested by van Prooijen, 2020) play a substantial role in motivating belief in conspiracy theories.

That said, there is a potential critique of the degree to which our results test the existential threat model of van Prooijen (2020). This model suggests that existential threat will lead to belief in conspiracy theories only when an antagonistic outgroup is salient. For most of the theories in our survey, we have no way of knowing whether our participants did perceive some antagonistic outgroup to be salient to the events described. That said, in model 5 (hypothesis 8) we specifically tested this feature of van Prooijen's model. We found that the estimated effect of stress on belief in a conspiracy theory implicating the Chinese government was tiny and non-significant both in people who perceived the Chinese government as threatening (i.e., antagonistic) and those who did not, with no evidence of interaction. Overall, our findings are not consistent with van Prooijen's existential threat model, albeit they do not refute it with certainty. Future investigations of this model could specifically measure existential threat (as opposed to distress broadly), and further test the implied interaction between existential threat, salience of an antagonistic outgroup, and belief in conspiracy theories.

Why might depression, stress, and anxiety, or threat *not* have substantially affected our participants' beliefs in conspiracy theories in our study? One explanation is simply that these variables hold no obvious information value in respect to such theories, and therefore

there is no reason participants *should* take them into account when considering the merits of various theories. Obviously, feeling depressed, stressed, or anxious about some event might lead one to seek explanations for that event, but people tend to enthusiastically seek explanations of events around them regardless: Humans are curious creatures (see Kidd & Hayden, 2015).

Effects of Conspiracy Beliefs *on* Psychological Distress

Considering the content of the conspiracy theories we canvassed, it is surprising that we also found no evidence of effects *of* belief in conspiracy theories (or conspiracy mentality) on anxiety, perceived stress, or depression. After all, the theories in our scales referred to powerful people acting in a furtive and malevolent fashion. We expected that accepting such theories should cause people some degree of emotional turmoil. That said, people's own mental health may often have much more important drivers (in their genetics, their biology, their personal lives, their economic circumstances) than abstract beliefs about large-scale societal events. This could mean that the effects of belief in conspiracy theories on mental health, if it exists, could be readily drowned out by statistical noise.

Interestingly, one of only two significant cross-lagged effects in our study was one about which we had made no hypothesis: A positive effect of conspiracy mentality on stressful life events. It is *possible* that such an effect holds in reality: For example, perhaps increased conspiracy mentality might strain relationships at home or at work (Toribio-Flórez et al., 2023; van Prooijen et al., 2022). That said, the *p* value for this coefficient was only barely significant, and occurred in the context of non-significant effects of conspiracy mentality on anxiety, perceived stress and depression. It should therefore be regarded as very tentative until replicated.

Limitations

By using the RI-CLPM with longitudinal data we were able to rule out biases due to stable confounding variables or from effects in the opposite directions to those hypothesized. However, our estimates could still be biased by confounds that vary over time (see Rohrer & Murayama, 2021). In addition, our estimates might be biased if causal effects truly are present, but occur slower (or faster) than implicitly assumed by the 1-month interval between waves.

We used convenience sampling, via Prolific. As such, we cannot be confident that our findings generalise to the wider populations of New Zealand, Australia, and the United Kingdom.

Although our sample size (in terms of both participants and waves) was large, it was not based on an *a priori* statistical power analysis. It is possible that effects were present in reality, but our study lacked sufficient power to detect them. This said, the confidence intervals surrounding our estimates were fairly narrow. For example, the 95% confidence interval for the effect of depression on belief in conspiracy theories ranged from -.01 to just .06. As such, while we cannot confidently rule out the presence of any effects whatsoever, our findings allow us to credibly rule out the presence of *large* effects (for most of the effects examined).

One critique of our study could be that its focus on within-person effects may have hidden the presence of substantial between-person effects. In other words, perhaps stable individual differences in anxiety, stress, or depression might have causal effects on belief in conspiracy theories, which were in turn ignored in our analysis. This is possible but not especially plausible, considering that even the sheer between-person correlation between these variables and belief in conspiracy theories tend to be small. Indeed, in two preregistered cross-sectional studies by Fox and Williams (2023), with controls for plausible confounds,

the authors found no significant effect of anxiety in either study, and only a small significant positive effect of perceived stress in one of the studies.

Future Directions

We found little evidence that manifestations of psychological distress (stress, depression, or anxiety) increased belief in conspiracy theories or conspiracy mentality. There is, however, a narrower symptom of psychopathology that might plausibly affect belief in conspiracy theories: rumination. Liekefett et al. (2024) theorise that ruminating about distressing events could narrow a person's attention to a view of the world that is negatively biased, which might in turn make conspiracy theories seem more plausible. Indeed, Liekefett et al. found that experimentally inducing brooding (a form of rumination) produced an increase in conspiracy beliefs that was statistically significant, $d = .19$, albeit small. While rumination can induce negative affect (Blanke et al., 2022), it may not be negative affect or distress *per se* that affect belief in conspiracy theories, but rather rumination as a cognitive process. It may be useful to subject this idea to further testing in other contexts.

Clinical Implications

Several authors have advocated interventions aimed at addressing psychological distress (especially stress-reduction interventions) as a strategy to reduce belief in conspiracy theories (e.g., Fournier & Varet, 2023; Pfeffer et al., 2022; Scheffer et al., 2022). However, the findings from this study—and other longitudinal studies—suggest that if reductions in psychological distress reduce belief in conspiracy theories at all, this effect is likely to be small. Furthermore, the effects of stress-reduction interventions on anxiety and stress levels are themselves small (see Fischer et al., 2020 for meta-analysis). Consequently, stress-reduction interventions (and other treatments for psychological distress)—while undoubtedly

useful and justifiable for other reasons—seem unlikely to be effective for addressing belief in conspiracy theories.

Author Contributions

Conceptualization: N. D. Fox, M. N. Williams, S. R. Hill; Data curation: N. D. Fox; Investigation: N. D. Fox, M. N. Williams; Funding acquisition: M. N. Williams; Methodology: N. D. Fox, M. N. Williams; Project Administration: M. N. Williams; Supervision: M. N. Williams, S. R. Hill; Writing – original draft: N. D. Fox; Writing – review and editing: M. N. Williams, S. R. Hill.

Conflicts of Interest

The author(s) declare that there were no conflicts of interest with respect to the authorship or the publication of this article.

Funding

This study was supported by the Marsden Fund Council from Government funding, managed by Royal Society Te Apārangi. Mr Fox was supported by the Massey University Doctoral Scholarship.

Supplemental Material

Supplemental Material is available on our OSF project at https://osf.io/dg6nq?view_only=3c9afa31770d4e808ae3e4fceb815b8d

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