Received: 28 January 2022 | Revised: 20 June 2022 | Accepted: 30 June 2022

DOI: 10.1111/jopy.12755

ORIGINAL ARTICLE

WILEY

Dispositional forgiveness buffers paranoia following interpersonal transgression

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Abstract

Objective: To test a novel proposition that dispositional forgiveness has the unrecognized benefit of buffering feelings of paranoia following negative interpersonal experiences and interpersonal transgressions.

Methods: In Study 1 ($N = 128$), we used an experimental paradigm, the Prisoner's Dilemma Game (PDG), to test the premise that an interpersonal transgression increases state paranoia. Study 2 ($N = 180$) used a longitudinal design to test the central proposition that dispositional forgiveness buffers state paranoia following naturally occurring difficult (vs pleasant) interpersonal events. Study 3 ($N = 102$) used a novel experimental paradigm to determine the causal effect of manipulating forgiveness on paranoia.

Results: In Study 1, interpersonal transgressions in the PDG increased paranoia. In Study 2, paranoia was higher following difficult (rather than pleasant) events, and higher levels of dispositional forgiveness moderated the negative effect of difficult events on paranoia. In Study 3, there was a causal effect of forgiveness on (reduced) paranoia.

Conclusions: This is the first evidence that (1) interpersonal transgressions increase paranoia, (2) high dispositional forgiveness moderates the deleterious effect of interpersonal transgression on paranoia, and (3) dispositional forgiveness is causally related to less paranoia.

KEYWORDS

forgiveness, interpersonal transgression, paranoia, personality, prisoner's dilemma game

1 | INTRODUCTION

In a landmark paper, Strauss (1969) proposed that paranoia, which had hitherto been viewed as a symptom of psychiatric disorder, lay on a continuum with ordinary human behavior. Social psychologists, beginning with Fenigstein and Vanable (1992), have subsequently conceptualized paranoia as a personality trait distributed within the general population, and subsequent empirical research has supported this view (Bebbington et al., 2013; Ellett et al., 2003; Freeman et al., 2005, 2019). This framing of paranoia is consistent with conceptualisations of other personality traits, such as narcissism, which have both nonclinical (general population) and clinical (narcissistic

personality disorder; Weiss & Miller, 2018) counterparts (Hepper et al., 2021).

At the heart of paranoia is a perception that another person or group are intentionally trying to cause one harm (Freeman & Garety, 2000). Paranoia can be further understood as a response to and way of making sense of (negative) *interpersonal* experiences (Chadwick & Lowe, 1994; Maher, 1974). A body of evidence provides empirical support for the centrality of interpersonal experiences in paranoia: (1) Adverse childhood experiences involving negative interpersonal interactions with others, including bullying (Bentall et al., 2012), physical abuse, and neglect (Sitko et al., 2014), are associated with increased paranoia in adulthood (Sitko et al., 2014). (2) A UK survey on individual experiences of nonclinical paranoia showed that paranoid experiences were often interpersonal, involving unexpected victimization and exclusion by others (Ellett et al., 2003). (3) Paranoia is characterized by a distinctive profile of interpersonal schematic beliefs about self and others (Chadwick & Trower, 1997; Kesting & Lincoln, 2013; Paget & Ellett, 2014). (4) Research using the Prisoner's Dilemma Game (PDG) showed that individuals experienced higher state paranoia when they believed they were playing another person rather than when playing a computer (Ellett et al., 2013). Collectively, this body of research supports the conceptualisation of paranoia as a cognitive and emotional response to interpersonal experiences where the intention of the other person(s) is believed to be malevolent.

One way to research this type of subjectively negative interpersonal experience is to examine the effects of an interpersonal transgression within an experimental context. However, to date, there has been no experimental research examining whether interpersonal transgression leads to increases in paranoia. To address this lacuna, an experimental paradigm simulating an interpersonal transgression is needed. One such paradigm is the PDG, which can be used to examine concurrently the impact of an unexpected interpersonal transgression on in-the-moment or state paranoia (Ellett et al., 2013).

In the PDG paradigm, one makes a forced choice to either cooperate or compete with another person. The game is ideally suited to examine paranoia because it is interpersonal, it involves threat and uncertainty concerning others' intentions towards the self, and it is necessarily ambiguous (Ellett et al., 2013). Further, the PDG is suitable for simulating an interpersonal transgression (McCullough & Witvliet, 2002). This is accomplished by creating a situation in which the real participant interacts with a programmed opponent who ostensibly agrees to cooperate but then breaks this agreement and competes. Therefore, our first key objective was to test the hypothesis that an interpersonal transgression (operationalized as breaking an agreement to cooperate in the PDG), would increase state paranoia (H1; Study 1).

Prior research has proposed a range of emotional responses to interpersonal mistreatment and transgression, including anger, low self-esteem, loss of hope, anxiety, and depression (e.g., see Berry et al., 2005), though rarely paranoia. This is surprising, given the assertion that interpersonal transgression is at the heart of paranoia. If interpersonal transgressions lead to increases in paranoia, it then becomes imperative to identify and test psychological resources that might buffer, or moderate, this effect. In previous research, increasing accessibility of positive self-cognitions (Ellett & Chadwick, 2007) and self-affirmation (Kingston & Ellett, 2014) have been shown to buffer experimental inductions of paranoia. Another potential buffer, which has not yet been examined in relation to paranoia, is dispositional forgiveness, that is, forgiveness as an enduring personality trait (see Berry et al., 2005). Beyond general associations between forgiveness and mental health (see Griffin et al., 2015 for a review), there are specific reasons to explore the relation between forgiveness and paranoia. Forgiveness and paranoia are both personality traits that share key characteristics – for example, both involve interactions with another person or group characterized by actual or perceived interpersonal transgression; both represent ways in which an individual might respond to an interpersonal transgression; and both have been understood from an evolutionary perspective (Billingsley et al., 2019; Ellett et al., 2003, 2013). Additionally, existing questionnaire measures focus on theoretically-relevant aspects of forgiveness that could enhance our understanding of paranoia. For example, the Transgression-Related Interpersonal Motivations Inventory (McCullough et al., 1998) measures avoidance and revenge motivations that might map onto paranoia, and the Rye Forgiveness Scale (Rye et al., 2001) assesses cognitive, emotional, and behavioral responses which are dimensions that are central to understanding paranoia, particularly in clinical therapeutic contexts (Chadwick et al., 1996). Furthermore, there is a long-established causal connection between agents learning to forgive those perceived to have treated them unjustly and an improvement in their own psychological health (Helb & Enright, 1993). Given that perceived intended mistreatment by others is central to paranoia, this points to the intriguing possibility that high dispositional forgiveness might buffer the negative effect of interpersonal transgression on paranoia. Accordingly, our second key objective was to test the hypothesis that high (compared to low) dispositional forgiveness would attenuate the deleterious effects of negative interpersonal experiences on paranoia (H2; Study 2). Finally, in Study 3, we developed an experimental forgiveness induction and implemented it to test if forgiveness causally reduces paranoia (H3).

2 | STUDY 1

In Study 1, we addressed our first key objective—to test the effect of one type of negative interpersonal experience, an interpersonal transgression, on paranoia. If paranoia is a response to negative interpersonal experiences, we would expect it to be higher following exposure to an interpersonal transgression compared to a no-transgression control (H1).

3 | METHOD

3.1 | Participants and design

We recruited 128 participants from a student participant pool, which afforded power of 0.80 to detect an effect size $d = 0.50$ (two-tailed $\alpha = 0.05$). The sample consisted of 103 women and 22 men ($n = 2$ identified as gender fluid and $n = 1$ preferred not to say) residing in the UK ($M_{age} = 19.54$ years, SD = 1.72). We randomly assigned participants to either the transgression condition ($n = 66$) or no-transgression condition ($n = 62$).

3.2 | Materials and procedure

Participants accessed the experiment online by following a web link. After participants provided informed consent and completed sociodemographic information, they received detailed instructions on the PDG. Consistent with previous research (Ellett et al., 2013), we informed them that they would be playing between one and six rounds of the PDG. In reality, they played one round only. We created the impression that there could be multiple rounds to avoid "end-gaming," that is, an increase in competition when participants believe they are nearing the end of an interaction (Axelrod, 1984). We informed participants that they would be interacting with another participant online but, in reality, they interacted with a simulated opponent. This programmed opponent always selected the competitive choice. We showed participants the PDG matrix and provided a detailed outline of possible choices within the game and the associated payoffs. Participants were not given any information about their ostensible opponent or any guidance on potential game strategy. Throughout the study, the cooperative and competitive PDG choices were labeled as X and Y, respectively. Participants completed a test to ensure they fully understood the PDG prior to starting the task.

In the transgression condition, participants saw a message from the other player stating "I think we should both choose X," indicating a desire to cooperate. Participants had to click "OK" to acknowledge receipt of the message and were then asked to select the X or Y choice. Participants were then shown the other player's choice, which was always the competitive Y choice. Participants in the no-transgression condition made their PDG choice immediately, with no message from the other player. Participants in both conditions then completed the State Paranoia Scale (SPS; Ellett et al., 2013), a validated 4-item scale assessing how participants perceived the other player in the PDG using 7-point scales with two opposing poles (e.g., 1 = w*ants to help me*, 7 = *wants to harm me*). Each item contains a negative pole that explicitly relates to a perception that the other player intended harm to the participant—a key element of persecutory thinking (Freeman & Garety, 2000). We averaged the four items to create a state paranoia index ($\alpha = 0.90$, $M = 5.54$, SD = 1.73). Finally, we debriefed participants and thanked them for their time.

4 | RESULTS AND DISCUSSION

Supporting H1, individuals who experienced an interpersonal transgression ($M = 5.89$, SD = 1.95) reported higher levels of state paranoia than those in the no-transgression ($M = 5.15$, SD = 1.38) condition, $t(126) = 2.49$, $p = .015$, $d = 0.44$. This is the first empirical evidence to demonstrate that interpersonal transgressions cause increased state paranoia.

5 | STUDY 2

Having demonstrated in Study 1 that a particular type of negative interpersonal experience, interpersonal transgressions, increase state paranoia, Study 2 sought to both conceptually replicate this effect and address our second key objective—to examine if dispositional forgiveness moderates or buffers the impact of negative interpersonal experiences on paranoia. In Study 1, the PDG allowed for an experimental manipulation of interpersonal transgression, but it could be argued that laboratory-based manipulations lack ecological validity. We address this issue in Study 2 by adopting a naturalistic approach to capture negative interpersonal experiences, to examine their effects on paranoia.

We therefore examined the relation between dispositional forgiveness and paranoia following naturally-occurring pleasant or difficult interpersonal events. Consistent with previous research (Hepper et al., 2021), we used a longitudinal, naturalistic design in which participants completed measures at three time points (T). At T1, participants completed the Heartland Forgiveness Scale (HFS; Thompson

et al., 2005) to assess dispositional forgiveness. Three days later, at T2, they identified (in counterbalanced order) a pleasant and a difficult interpersonal interaction they had experienced within the past three days and rated their state paranoia in relation to each experience. A week later, at T3, they again rated their state paranoia following a pleasant and a difficult interpersonal interaction, this time ones they had experienced in the previous week. We expected that dispositional forgiveness would be negatively associated with state paranoia, and that state paranoia would be higher following a difficult interpersonal event compared to a pleasant interpersonal event. More importantly, we expected that these two main effects would be qualified by an interaction effect, such that dispositional forgiveness would buffer the deleterious effect of difficult (vs. pleasant) events on state paranoia (H2).

6 | METHOD

6.1 | Participants

Our focal effect of interest was the association of dispositional forgiveness with the within-subjects difference between paranoia in response to difficult events versus pleasant events. The test of this association is mathematically equivalent to a correlation coefficient and, hence, we expressed the anticipated effect size in this metric. This is the first study to test whether dispositional forgiveness buffers the deleterious effect of difficult (vs. pleasant) events on state paranoia. In the absence of prior estimates, we based our the effect-size estimate on Study 1 findings ($d = 0.44$), and specified an equivalent effect size ($r = .22$).[1] Power analysis revealed a requisite sample size $N = 159$ to achieve power of 0.80 (two-tailed $\alpha = 0.05$), which we exceeded to guard against attrition. Two hundred twenty-seven individuals initially consented to participation (198 women, 28 men, 1 who did not report their gender), recruited through a student participation pool and online through Facebook. We excluded two participants who did not complete the HFS at T1. We excluded a further 45 participants who did not complete any state paranoia assessments at either T2 or T3. The final sample comprised 180 participants (158 women, 21 men, 1 who did not report their gender). Age group distribution was: 18–24 years = 130; 25–34 years = 38; 35–44 years = 4; 45–54 years = 4; 55–64 years = 3; 65+ years = 1.

6.2 | Materials and procedure

The study received ethical approval and was conducted entirely online. At T1, participants provided demographic information and completed the HFS. Participants rated HFS items (e.g., "I continue to punish a person who has done something that I think is wrong") on a 7-point scale (1 = *almost always false of me*, 7 = *almost always true of me*; $\alpha = 0.89$, $M = 4.45$, SD = 0.89). Three days later, participants completed Time 2 measures. Consistent with previous research (Hepper et al., 2021), participants identified both a pleasant and difficult interpersonal experience from within the past three days, and rated state paranoia in relation to each experience. We assessed state paranoia with the 10-item Persecution subscale of the State Social Paranoia Scale (SSPS; Freeman et al., 2007). This scale measures state paranoia specifically in relation to reported interpersonal interactions (e.g., "Someone had bad intentions towards me"; 1 = *do not agree*, 5 = *totally agree*). We averaged the items to create indices of state paranoia pertaining to the pleasant ($\alpha = 0.89$) and difficult ($\alpha = 0.91$) interpersonal experience. Seven days later, participants received an email reminder to complete the T3 measures. At T3, participants identified different pleasant and difficult interpersonal experiences which had occurred within the intervening seven days, and again rated state paranoia in relation to the pleasant ($\alpha = 0.92$) and difficult ($\alpha = 0.92$) experience. The order of pleasant and difficult interpersonal events was independently counterbalanced at T2 and T3. As a manipulation check, participants rated how stressful each event was on a 9-point scale (1 = *not stressful at all*, 9 = *extremely stressful*).

6.3 | Analysis strategy

Not all participants completed ratings of state paranoia for both a pleasant and difficult event at both T2 and T3. Of the final sample of 180 participants, 165 completed all four ratings (i.e., both ratings at both time points), seven completed three ratings, a further seven completed two ratings, and one completed a single rating only. Our main analyses are based on those participants who completed all four ratings.[2] We ran mixed ANOVAs, with forgiveness as a continuous between-subjects variable, and time (T2 vs. T3) and event type (pleasant vs. difficult) as categorical within-subjects variables. For main effects and interactions involving the between-subjects variable, forgiveness, we used the familiar effect-size metric *r*. We indexed effects of the within-subjects variables with the classical Cohen's (1988) $d = \left(\frac{M1-M2}{\sigma}\right)$ (for an overview, see: Westfall, 2016). We present descriptive statistics for our measures at each time point in Table 1.

7 | RESULTS AND DISCUSSION

Participants reported a range of difficult interpersonal events, including the following examples: "I found out

TABLE 1 Means (standard deviations) for state paranoia and stress at T2 and T3 as a function of type of experience

		T2		T3	
	N	Pleasant experience	Difficult experience	Pleasant experience	Difficult experience
State paranoia	165	1.07 (0.26)	1.63 (0.79)	1.09 (0.32)	1.61 (0.81)
Stress	156	1.84 (1.47)	6.06 (2.00)	1.78 (1.38)	6.06 (2.04)

my friends had a Zoom party without inviting me and I felt upset and left out. I confronted my friend about it and she implied that she didn't really like me which made me angry"; "At work, clashing with a colleague in a fast paced environment, who does not put effort into work and has problems with others"; "I had an argument with my dad as he was in a bad mood"; "I went to the shop to buy some stationary and the woman at the till was rude and rushed all the customers out of the shop because she wanted to take a break from her shift." Difficult events occurred with either a friend (32%), family member (25%), colleague (15%), an unknown person encountered in daily life (13%), a partner (7%), or it was unclear who the event occurred with (8%).

7.1 | Manipulation check: Stress

One hundred and fifty-eight participants completed all four stress ratings. A Forgiveness × Time × Event Type mixed ANOVA revealed, as intended, that stress ratings were significantly higher for difficult ($M = 6.06$, SD = 1.70) than pleasant ($M = 1.81$, SD = 1.10) interpersonal events, $F(1, 156) = 849.51$, $p < .001$, $d = 3.06$. Neither the time main effect, $F(1, 156) = 0.07$, $p = .785$, $d = 0.02$, nor the Time × Event Type interaction, $F(1, 156) = 0.05$, $p = .816$, $d = 0.02$, were significant. Thus, the event-type manipulation was successful. Results further revealed a significant negative association between forgiveness and stress, $F(1, 156) = 4.42$, $p = .037$, $r = -.17$. This forgiveness main effect was not qualified by interactions involving time or event type, indicating that hsigh-forgiveness (compared to low-forgiveness) individuals generally regarded interpersonal interactions as less stressful.

7.2 | State paranoia

A Forgiveness × Time × Event Type mixed ANOVA revealed that dispositional forgiveness was significantly negatively associated with state paranoia, $F(1, 163) = 26.39$, $p < .001$, $r = -.37$. Additionally, difficult interpersonal experiences ($M = 1.62$, SD = 0.68) were associated with more paranoia than pleasant ones ($M = 1.08$, SD = 0.26), $F(1, 163) = 144.09$, $p < .001$, $d = 1.15$. Crucially, results revealed a significant Forgiveness × Event Type interaction, $F(1, 163) = 14.43$, $p < .001$, $r = -.29$. We display this interaction in Figure 1.

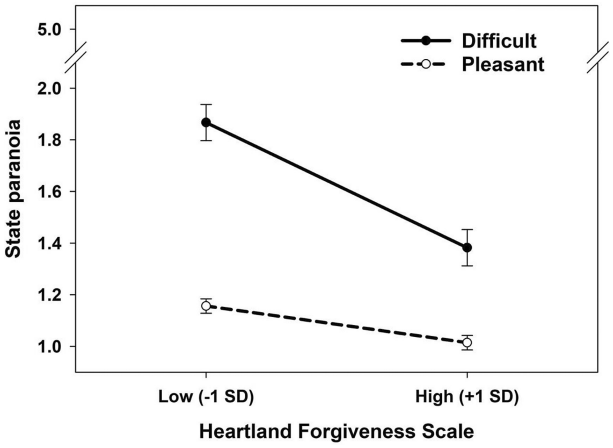

FIGURE 1 Interaction effect of dispositional forgiveness (heartland forgiveness scale) and event type (pleasant vs. difficult) on state paranoia. Plotted values are predicted means, conditioned at −1 SD and +1 SD on dispositional forgiveness. Error bars represent standard errors.

We first probed the interaction by testing the simple effect of event type at low (−1 SD) and high (+1 SD) levels of dispositional forgiveness. For low-forgiveness participants, paranoia was significantly higher in difficult than pleasant interpersonal interactions, $F(1, 163) = 124.77$, $p < .001$, $d = 1.12$. For high-forgiveness participants, paranoia was also significantly higher in difficult than pleasant interactions, $F(1, 163) = 33.41$, $p < .001$, $d = 0.58$, but this event-type effect was significantly smaller (as indicated by the significant interaction) than for low-forgiveness participants. Looked at from a different angle, forgiveness was significantly negatively associated with paranoia in difficult interpersonal interactions, $F(1, 163) = 23.62$, $p < .001$, $r = -.36$, and with paranoia in pleasant interactions, $F(1, 163) = 12.96$, $p < .001$, $r = -.27$, but this negative association was weaker in the latter case. High (compared to low) dispositional forgiveness attenuated the deleterious effect of difficult (vs. pleasant) events on state paranoia, supporting H2.[3]

8 | STUDY 3

Study 2 demonstrated a negative association between dispositional forgiveness and state paranoia, particularly in

the context of a difficult interpersonal experience. These findings were correlational, however, and cannot identify the causal direction of the link between forgiveness and paranoia. Our key objective in Study 3, then, was to experimentally manipulate forgiveness and test its causal effect on state paranoia. We predicted that high (compared to low) forgiveness would reduce state paranoia. To our knowledge, there are no forgiveness manipulations to draw on. We therefore modeled our manipulation on a validated loneliness induction (Wildschut et al., 2006). To be precise, we provided participants with false feedback from a questionnaire that ostensibly assessed their level of forgiveness. We hypothesized that participants in the high-forgiveness condition would report lower paranoia than those in the low-forgiveness condition (H3).

9 | METHOD

9.1 | Design and participants

We based our anticipated effect size on the association between forgiveness and paranoia in Study 2: $r = -.37$ (equivalent to $d = 0.80$). Accordingly, the requisite sample size to achieve power of 0.80 (two-tailed $\alpha = 0.05$) was $N = 55$. We exceeded this and recruited 102 participants from a student participant pool (77 women, 24 men, 1 who did not report their gender; $M_{age} = 21.25$ years, SD = 4.29). We randomly assigned participants to the high-forgiveness ($n = 47$) or low-forgiveness ($n = 55$) condition.

9.2 | Materials and procedure

Participants first completed a questionnaire labeled the "University of London Forgiveness Scale." They indicated whether they agreed or disagreed with each of 10 items that we selected from three existing measures of forgiveness: the Trait Forgiveness Scale (Berry et al., 2005), the Tendency to Forgive Scale (Brown, 2003), and the forgiveness measure developed by Toussaint et al. (2012). The items in the high-forgiveness condition were prefaced with the words "I sometimes" (e.g., "I sometimes forgive those who hurt me"). This phrasing aimed to elicit agreement with the statements. The items in the low forgiveness condition were prefaced with the words "I always" (e.g., "I always forgive those who hurt me"). This phrasing aimed to elicit disagreement with the statements. We then told participants that their scores were being calculated and, after 20 s, we gave them the following feedback:

> The University of London Forgiveness Scale has been administered to a large number of university students over the last five years. Based on the responses of over twelve hundred students, we have developed a way of scoring your answers. This allows us to provide you with valid and detailed feedback regarding your level of forgiveness.

We informed participants in the high-forgiveness condition that their responses indicated that they were in the 62nd percentile of the distribution of forgiveness, and that compared to other students they were "above average on forgiveness." We informed participants in the low-forgiveness condition that they were in the 12th percentile of the distribution of forgiveness, and that compared to other students they scored "very low on forgiveness." To strengthen the forgiveness manipulation, we then instructed participants to give a written explanation for their forgiveness score. Next, as a manipulation check, participants completed both the HFS (Thompson et al., 2005; 1 = *almost always false of me*, 7 = *almost always true of me*; $\alpha = 0.70$, $M = 4.55$, SD = 0.69) and a single, face-valid item ("Right now, I feel that I am a forgiving person"; 1 = *strongly disagree*, 5 = *strongly agree*; $M = 3.52$, SD = 1.00). We then measured paranoia using the 20-item Paranoia Scale (PS; Fenigstein & Vanable, 1992). Participants rated the PS items (e.g., "someone has it in for me") on a 5-point scale (1 = *not at all applicable to me*, 5 = *extremely applicable to me*; $\alpha = 0.90$, $M = 2.28$, SD = 0.68). Participants were fully debriefed at the end of the study.

10 | RESULTS AND DISCUSSION

Participants in the high-forgiveness condition ($M = 3.81$, SD = 0.92) scored higher than those in the low-forgiveness condition ($M = 3.27$, SD = 1.01) on the single forgiveness item, $t(100) = 2.78$, $p = .007$, $d = 0.56$. Further, HFS scores were higher in the high-forgiveness condition ($M = 4.71$, SD = 0.68) than the low-forgiveness condition ($M = 4.42$, SD = 0.67), $t(100) = 2.21$, $p = .029$, $d = 0.44$. The forgiveness manipulation was successful.

As hypothesized, forgiveness reduced paranoia (H3). Participants in the high-forgiveness condition ($M = 2.13$, SD = 0.61) scored significantly lower on paranoia than those in the low-forgiveness condition ($M = 2.41$, SD = 0.71), $t(100) = -2.19$, $p = .039$, $d = -0.44$. These findings provide the first evidence for a causal effect of forgiveness on (reduced) paranoia.

11 | GENERAL DISCUSSION

Our series of studies addressed three key objectives: (1) to determine if one type of negative interpersonal experience,

that is an interpersonal transgression, leads to increases in paranoia (H1), (2) to examine if dispositional forgiveness moderates or buffers the effect of negative interpersonal experience on paranoia (H2), and (3) to test the causal effect of forgiveness on paranoia (H3). Our findings make a number of unique and important contributions. As hypothesized, state paranoia was significantly higher following a negative interpersonal experience, an effect that was demonstrated both experimentally (breaking an agreement to cooperate in the PDG in Study 1) and following self-report of naturally-occurring difficult interpersonal interactions (Study 2). This is the first empirical evidence to demonstrate dynamically and in action that paranoia (clinical or nonclinical) is heightened in response specifically to an interpersonal transgression, and supports the conceptualisation of paranoia as a response to negative interpersonal experiences (Chadwick & Trower, 1997; Ellett et al., 2003, 2013; Kesting & Lincoln, 2013; Paget & Ellett, 2014).

We also found that high levels of dispositional forgiveness buffered the negative effect of difficult (compared to pleasant) interpersonal experiences on state paranoia (Study 2). Specifically, when dispositional forgiveness was low, individuals were more likely to experience an increase in state paranoia after a difficult (compared to pleasant) interpersonal experience. However, when dispositional forgiveness was high, this effect was attenuated and individuals evinced a smaller negative effect of difficult interpersonal interactions on state paranoia. These findings add dispositional forgiveness to the range of psychological resources that buffer or attenuate paranoia (Ellett & Chadwick, 2007; Kingston & Ellett, 2014). The absence of forgiveness could also help to explain why, once activated, paranoia persists (Allen-Crooks & Ellett, 2014; Ellett & Chadwick, 2007). This is a fruitful direction for future research.

Study 2 revealed a strong negative association between dispositional forgiveness and state paranoia, particularly in the context of a difficult interpersonal experience. Importantly, in Study 3, we conceptually replicated and extended these findings by demonstrating, for the first time, that forgiveness exerts a causal effect on (reduced) paranoia. Our finding that forgiveness is a key psychological resource that buffers paranoia also has important clinical implications; helping individuals to increase forgiveness may be a means to reduce clinical paranoia. An advantage of this approach is the potential to target paranoia without a need to work directly with the individual's specific (paranoid) belief, which can provoke psychological reactance (Chadwick et al., 1996). This is another promising avenue for future research.

The present research raises wider conceptual issues concerning the definition and conceptualisation of paranoia. Firstly, within traditional psychiatry, paranoia is framed as a psychiatric symptom that is by definition a false perception of actual or planned harm by others. However, from a continuum perspective (Strauss, 1969), paranoia may alternatively be framed as a personality trait that is distributed across the general population. Personal safety is clearly an ecologically important concern and, from an evolutionary perspective, it is plausible that paranoia was selected because it had survival value in ancestral environments (Ellett et al., 2003; Ellett & Chadwick, 2007). When detecting threat, the evolutionary maxim "better safe than sorry" would dictate vigilance regarding intended harm by others. At the same time, falsely perceiving threat and danger of intended harm where none exists (i.e., false alarm) is unlikely to be adaptive. Thus, from the perspective of personality dynamics, paranoia may be conceptualized as a trait distributed within the general population that occurs in response to specific conditions, including negative interpersonal experiences and interpersonal transgression, and is moderated by forgiveness.

This evolutionary perspective points to a second central conceptual issue for paranoia research. Whereas there is consensus both that paranoia is interpersonal and that intention is central to its phenomenology (in the form of an attribution of malevolent intent to the other person), there is a related dimension of paranoia that is less clear cut—that is, does paranoia always have to be unfounded? Certainly, the notion that paranoia is by definition unfounded was enshrined in traditional psychiatric definitions of paranoia as a "false belief." An alternative position, one that is perhaps more aligned with personality dynamics and with a stress-vulnerability model of health, is to propose that the degree to which paranoia is founded is likely to reflect both environmental and individual factors. For example, in our first study, there is a clear transgression, and a paranoid interpretation is arguably therefore more founded—though even here, there remains an element of choosing to attribute a strong malevolent intent to the other player that goes beyond the action (scale items include "has it in for me," "wants to harm me," and "is hostile towards me"). In interpersonal contexts where another's actions are less overtly unpleasant or hostile, it would seem likely that paranoia would be driven more by personality characteristics and history of interpersonal trauma, and would be more strongly correlated with trait paranoia measures.

There are a number of limitations to consider. The samples across all studies were predominately female and white British, which reduces generalisability of the findings. Future studies aiming to replicate and advance this line of research should recruit samples representing a broader mix of ethnicities and gender. We also relied solely on self-report measures,

which could have introduced bias. Future research might usefully adopt a range of paranoia measures, including interviewer ratings and behavioral indices. Additionally, in Study 2, state paranoia scores were low overall. A possible explanation is the measure of state paranoia—the Persecution subscale of the SSPS—tapped explicit persecutory beliefs (e.g., "Someone would have harmed me in some way if they could," "Someone had it in for me"). Perhaps not surprisingly, few participants in our non-clinical sample strongly endorsed these statements. Yet, despite this restriction of range, results supported the buffering role of forgiveness in the context of difficult (compared to pleasant) interpersonal interactions. Future research might usefully implement more subtle, implicit measures of state paranoia and examine the role of forgiveness in clinical populations. Further, it is possible that the single-item manipulation check used in Study 3 may have introduced a demand characteristic, although the HFS offered an additional (successful) manipulation check. Additionally, we focused on the broader concept of difficult interpersonal experiences in Study 2, rather than on interpersonal transgressions per se. Nonetheless, we noted from participant descriptions that some clearly contained elements of transgression to varying degrees. We were not able to formally assess degree of transgression as participants did not provide enough information in their descriptions of difficult interpersonal events. It will be important to explicitly measure transgression in future research. Finally, we did not measure state forgiveness or other psychological constructs that are known to be associated with paranoia, such as negative schematic beliefs, which should be measured in future research.

Overall, our studies show that negative interpersonal experiences increase paranoia (Study 1), that dispositional forgiveness buffers these deleterious effects (Study 2), and that experimentally induced high (compared to low) forgiveness causally reduces paranoia (Study 3). Our findings demonstrate that experiments play a valuable role in personality research (Revelle, 2007). Yet, they also raise two important questions for future research. First, is induced forgiveness isomorphic with dispositional forgiveness; that is, do findings from one domain generalize to the other? Second, although we hypothesized and found that forgiveness decreased paranoia, could there also be a causal path in the opposite direction, whereby paranoia reduces forgiveness? Both issues could be fruitfully addressed in future research by examining the causal ordering of dispositional forgiveness and paranoia using intensive longitudinal designs.

AUTHOR CONTRIBUTIONS

Ellett, Wildschut and Chadwick all contributed to conceptualisation of ideas and study design.Foxall contributed to data collection.Ellett, Wildschut and Chadwick all contributed to data analysis, interpretation, and writing of the manuscript.

CONFLICT OF INTEREST

The authors declare that there are no conflicts of interest to report.

ETHICS STATEMENT

The studies included in this manuscript were not preregistered. Studies were reviewed and approved by the relevant University Ethics Committees.

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ENDNOTES

[1] Blake and Gangestad (2020) cautioned against using the magnitude of a main effect within an "effect present" condition to calculate sample size for detecting an interaction effect in a design including both "effect present" and "effect absent" conditions. For our purposes, low dispositional forgiveness represents the "effect present" condition and high dispositional forgiveness represents the "effect absent" condition. The main effect of interpersonal transgression documented in Study 1 ($d = 0.44$), then, represents the average effect across both these "conditions," because the Study 1 sample presumably included both low- and high-forgiveness individuals. Accordingly, using the Study 1 effect size to determine sample size in Study 2 implies the following simplifying assumptions: (1) that the effect of pleasant (vs. difficult) experiences on state paranoia for high-forgiveness individuals (i.e., "effect absent" condition) is zero, and (2) that said effect for low-forgiveness individuals (i.e., "effect present" condition) is twice the size of the main effect obtained in Study 1 ($d = 0.44 \times 2 = 0.88$). That is, for the sake of simplicity and in the absence of prior evidence, we assumed that the effect in Study 1 was entirely "driven" by low-forgiveness individuals.

[2] In supplementary analyses, we used hierarchical linear modeling (HLM). This allowed us to use available data from participants with one or more missing responses. Specifically, we treated the four ratings as level-1 units of analysis and participants as level-2 units of analysis. We modeled dependence among ratings by the same participant by including a random intercept for participants. We mean-centered dispositional forgiveness scores and effects-coded the categorical independent variables, time (T2 = −1, T3 = 1) and event type (pleasant = −1, difficult = 1). Results were essentially identical to the ANOVA results reported in the text. We prioritized the ANOVA approach because, in comparison to HLM, it facilitates a simple power analysis and provides intuitive effect-size measures.

[3] The distribution of state paranoia scores displayed a marked right skew, with the center of the distribution close to the lower end of the rating scale. Overall, explicit persecutory beliefs were clearly low in this non-clinical sample. Thus, it is unlikely that the ratings follow a multivariate normal distribution in the population. We therefore applied a log10 transformation to the state paranoia scores, which reduced skewness and kurtosis. We then repeated the analyses with the transformed paranoia score. Results were essentially identical. The assumption of sphericity was met because the within-subject variables (time and event type) each had only two levels. The assumption of independent observations was met because each participant provided only one set of four ratings and did so independently of other participants.

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How to cite this article: Ellett, L., Foxall, A., Wildschut, T., & Chadwick, P. (2022). Dispositional forgiveness buffers paranoia following interpersonal transgression. *Journal of Personality*, *00*, 1–10. https://doi.org/10.1111/jopy.12755