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Revisiting Feeling of Threat and Agency Detection: A Preregistered Virtual Reality Study

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While agency detection has been used to explain religious phenomena, there are two competing theories regarding the neurocognitive mechanism underlying it: hyperactive agency detection device (HADD) and the predictive processing-based model. Those two theories differ in their predictions regarding the impact of feeling of threat on agency detection. The present study aimed to put this hypothesized effect to a new test in virtual reality. One hundred ninety-nine participants explored an agentless, foggy virtual forest, instructed to press a button whenever they perceive a being. While all participants were primed to expect a stable number of agents, we manipulated the description of beings, introducing them as either hostile or neutral, depending on the group. We also measured participants' heart rate and their reported anxiety during the task. To test our hypotheses, we performed a Wilcoxon's test for our main analysis. Two linear regressions models have been run to examine the moderating effect of arousal and whether anxiety mediates the relationship between priming and agency detection. We also performed a test of statistical equivalence and exploratory analyses. The study did not confirm that people are more likely to falsely detect agents when they expect them to be dangerous, which casts doubt on HADD conceptualized as a threat-triggered mechanism. However, we found several patterns in both quantitative and qualitative data which call for further studies on the topic of agency detection as a building block of religious and spiritual phenomena.

Keywords: feeling of threat, agency detection, hyperactive agency detection device, predictive processing, religion

As the everyday experiences of many humans indicate, we easily fall under the illusion that someone is present in our surroundings: we take cracking branches to be the sound of footsteps, and we see monsters in piles of clothes at night. These experiences are often accompanied by feelings of uncertainty, anxiety, or fear. But do these emotions emerge in response to the illusory presence of strange agents, or could it be the other way around? Are we more prone to detect illusory agents around us *when* we are afraid of them?

While this is an interesting question as such, the answer could tell us much about the mechanisms underpinning religious and spiritual phenomena. Previous research has suggested that agency detection—a concept that encompasses different abilities related to

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All additional online materials can be found at the Open Science Framework repository (https://osf.io/4mgbf/). The materials are divided into two files. The first file includes all materials relevant for the preregistration: documents of board approval, Stage-1 manuscript, study materials, and study protocol. The second file includes the preregistered plan of data analysis, description of the pilot study, raw and preprocessed data, analyses, and the appendix to the Stage-2 manuscript featuring instructions and questionnaires. The Stage-1 manuscript is also available at the PsyArXiv repository as a previous version of the uploaded preprint (https://osf.io/preprints/psyarxiv/dc4g5).

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detecting agents and their actions in the environment—is linked to supernatural beliefs and extraordinary experiences involving supernatural agents. Most famously, it has been suggested that humans are inclined toward beliefs in supernatural agents due to innate anthropomorphizing tendencies (Guthrie, 1993), or because we are equipped with an adaptive, hyperactive cognitive module specialized in agency detection called the hyperactive agency detection device (HADD; see Barrett & Lanman, 2008). Crucially for religion, it was theorized that HADD underlies experiences of a supernatural presence, facilitating the acquisition of supernatural beliefs (Barrett, 2000). However, experimental studies yielded inconclusive evidence of whether humans truly possess HADD (see, e.g., Van Leeuwen & van Elk, 2019). Importantly, M. Andersen (2019) has recently proposed a new approach to agency detection based on the theoretical framework of predictive processing. On his approach, our brains do *not* possess an innate specialized module for agency detection; agency detection is rather the result of domain-general predictive processing mechanisms; enhanced agency detection, including illusory agent detection, can result from unreliable sensory data interacting with expectations about the distribution of agents in the environment.

Still, some recent studies (embedded within the original modular concept of agency detection) have found that people are more inclined toward illusory agent perceptions when they feel threatened (Nieuwboer et al., 2015; van Elk, 2021). While these findings align with the idea of HADD and could explain some encounters with supernatural beings (especially frightening ones, e.g., ghosts), they are quite puzzling from the predictive processing perspective. Why should the feeling of threat increase the number of false agency perceptions if the expected number of agents is stable and we do not possess any specialized agent-related bias causing such an increase? In a predictive processing account, the higher the perceived prior probability of encountering an agent, the higher the chance of experiencing illusory agency. Conditions of threat (i.e., a qualitative change in expectations) should not, by default, increase the subjective prior probability, so the increase in agency detection (i.e., a quantitative change in the number of illusory perceptions) found in some studies calls for further investigation. Therefore, we decided to address the question of whether participants will detect more illusory agents while they feel potentially threatened by them. The answer might move us toward a more thorough understanding of experiences involving supernatural agents believed to be hostile, from ghosts and phantoms to monsters and aliens.

We used virtual reality (VR) as a previously proven way to simulate real-life scenarios without bringing up serious ethical issues. Virtual environment can successfully induce illusory agency detection, as evidenced by M. Andersen et al. (2019) and Tratner et al. (2020). Other agency detection studies are following this trend (Maij et al., 2019; van Elk, 2021), with virtual reality being generally considered an ecologically valid tool for the study of agency detection (M. Andersen et al., 2019; Maij & van Elk, 2019).

Thus, we designed the present study to empirically tackle the problem left open by previous studies: Before entering a sensorially unreliable, agentless virtual environment, participants were instructed to expect a stable number of agents across groups but received differing descriptions of these agents as either being hostile (experimental group) or neutral (control group). This way, we revisited a hypothetical causal effect of the feeling of threat on the number of false-positive agency detections. Our goal was not to confirm or refute the predictive processing approach (which we consider attractive), and the null results of our study would not contradict its basic predictions. But, finding an effect of manipulation on the number of false-positive detections—that is, the number of detections would be significantly higher in the experimental group—would suggest a more sophisticated picture of the agency detection phenomenon, possibly involving some form of evolutionarily driven bias after all (which might be, but not necessarily is realized by HADD). Thus, our aim was to take a first step toward investigating which model of neurocognitive realization of agency detection would be promising to pursue in further research. Consequently, the study is also relevant to assessing the strength of prominent theories that aim to explain the etiology of beliefs about supernatural agents. Overall, the results might help us learn more about the origins of both religious experiences and supernatural beliefs that the mechanisms of agency detection were said to facilitate.

Agency Detection and Feeling of Threat

When it comes to the idea of agency detection as a source of religious beliefs, its origins are usually linked to the work of Stewart Guthrie (1993), who proposed that in an inherently ambiguous world, it is adaptive for humans to interpret the data around them using a maximally meaningful pattern, namely, the pattern of human behavior. According to Guthrie, humans are naturally inclined to anthropomorphize their surroundings (e.g., objects, phenomena, and nonhuman animals), and religious beliefs originate from this inclination. Following the ideas of Guthrie, Barrett and Lanman (2008) proposed that humans are evolutionarily equipped with a specialized HADD module that makes us biased toward over-detecting intentional agents and agency cues in our surroundings. According to the logic behind HADD, it is adaptive for us to falsely assume that an (possibly dangerous) agent is present when the sensory data are ambiguous, because assuming otherwise could create the risk of becoming the unseen agent's prey. From this logic, it was derived that while feeling threatened, humans should detect more illusory agents (Maij et al., 2019). However, unlike Guthrie's anthropomorphism, HADD was not regarded as the single most important driver of religious belief. Instead, it was supposed to be a mechanism that makes humans prone to acquire and transfer supernatural beliefs (which may already exist in the culture). As Barrett & Lanman proposed, HADD could facilitate experiences that can be accounted for—only or most easily—using supernatural agent concepts, making individuals more prone to believe in these concepts and engage in their further transfer.

To this day, many studies have been conducted to test whether there exists a relationship between supernatural beliefs and the tendency to overdetect agents (e.g., Tratner et al., 2020; van Elk, 2013, 2015) and under what conditions agency detection is more active (e.g., Maij et al., 2019; Nieuwboer et al., 2015). However, these studies have yielded limited support for the HADD model (see M. Andersen, 2019; Van Leeuwen & van Elk, 2019). This fact, combined with objections based on neuroscientific evidence (Lisdorf, 2007), led to the turn in paradigm toward a novel model embedded within the predictive processing framework.

The predictive processing framework, which is increasingly popular in cognitive neuroscience (see Friston, 2018), has already been applied to many problems of cognitive science and cognitive and evolutionary science of religion (CESR). Crucially, it was also used by M. Andersen (2019) in what we will call the predictive processing model of agency detection (PPAD). In the PPAD, agency detection is not seen as a function of a built-in, hardwired module, as the HADD model proposed. Instead, since predictive processing assumes that our brain is a domain-general predictive machine that calculates probabilities of outside occurrences, humans should detect agency only if the calculated probability of an agent's presence is sufficiently high. Specifically, the brain estimates the so-called posterior probability of a given hypothesis (such as "an agent is present") based on the perceived prior probability of that hypothesis (i.e., the probability of an agent's presence estimated independently of incoming sensory data) and on the likelihood of that hypothesis (i.e., how well an agent's presence explains the incoming sensory data; see, e.g., Hohwy, 2020). Hence, as M. Andersen (2019) argued, there are no reasons why humans should, *by default,* be prone to perceiving illusory agents.[1] According to the PPAD, we falsely perceive agents only when the individual prior probability estimate of their presence is high and when the environmental data are unreliable. This is because our brain depends primarily on its prior expectations when the likelihood of different predictions is approximately equal (M. Andersen, 2019). This central prediction of PPAD already found some evidence in a recent experimental study, where participants who were primed to expect more agents in an agentless virtual reality environment reported more illusory agent detections than participants who were primed to expect fewer agents. Increasing the sensorial unreliability also increased illusory agency detection (M. Andersen et al., 2019).

Crucially, PPAD has been proposed as a robust explanation of extraordinary experiences involving an encounter with a supernatural agent. As M. Andersen (2019) argued, religions provide us with approximate distributions of supernatural agents in our surroundings (e.g., they inform us where to meet spirits or in what conditions one can hear God), shaping our prior expectations about the probability of agents' nearby presence. Combined with sensorially unreliable or scarce data, often found in religiously meaningful environments (e.g., dim lightning, smoke, and darkness), these expectations can trigger illusory supernatural agency detections. In other words, expecting to encounter a ghost in a dark forest, where discriminatory capacities of the predictive mind are impaired, might result in an illusory perception of such a ghost. Furthermore, personal experience of a ghost sighting could reinforce already possessed expectations and could evoke these expectations in other people via cultural transfer (M. Andersen, 2019; also suggested by Barrett & Lanman, 2008, in the context of HADD). This idea could explain some of the "third-kind" encounters found in religious contexts, including not only prototypical events (such as, e.g., Saul hearing the voice of God, Muhammad meeting the archangel Gabriel, etc.) but also the phenomena that happen on a common basis; for example, according to Bader et al. (2010), as much as fifth of Americans claim to have witnessed a ghost and 10% believe that they have experienced a sighting of an unidentified flying object.

In summary, while agency detection has been linked to supernatural beliefs and experiences, there are at least two competing views on the neurocognitive realization of its cognitive function: the HADD view and the PPAD. These two models vary regarding their consideration of the evolutionary basis of agency detection. While in the domain-general PPAD, this basis is largely downplayed in favor of sociocultural learning, the central assumption of HADD is that such specialized module evolved as a safety mechanism governed by the logic that it is better to detect illusory agents than assume safety and fall prey to real agents (Barrett, 2000; for a discussion of HADD's possible maladaptiveness, see Lisdorf, 2007). Based on this idea, Maij et al. (2019) predicted that, assuming the existence of HADD, the feeling of threat should increase illusory agency detection. In PPAD, on the other hand, no such hypothesis is easily derived from the theory, as the feeling of threat does not necessarily alter the perceived probability of encountering an agent. Hence, we should not, by default, expect the feeling of threat to increase the number of illusory agency detections.

According to our best knowledge, results from three sets of studies on this matter seem quite puzzling. In their project, Maij et al. (2019) found no effect of the feeling of threat on agency detection across five studies. However, the authors recognize a major methodological limitation of their experimental settings. Namely, the feeling of threat was not induced as directly related to suggested agents; instead, participants were simply exposed to threatening stimuli before the agency detection task. Therefore, threat manipulation and agency detection tasks were arguably unrelated. On the other hand, other disclosed (Nieuwboer et al., 2015) and nondisclosed (van Elk, 2021) studies reported that the feeling of threat increases agency detection. However, these experiments were also conducted with threat priming "detached" from the agency detection task. We believe that these studies left an opening that we now address with a revised methodology. Thus, in the experimental study, our aim was to address the unresolved question of whether the feeling of threat, directly related to suggested agents, increases agency detection.

The Present Study

We followed the paradigm exploited by M. Andersen et al. (2019) and measured illusory agency detection in an agentless virtual reality environment. While participants in the experimental group were primed with information about the hostile nature of the agents inhabiting the virtual environment, for the control group, agents were introduced as neutral. Crucially, we kept the expected number of agents stable across groups, with the assumption being that our priming and control instructions would induce priors of approximately the same precision. In other words, the likelihood of the same data under these two instructions should remain roughly the same.

Hence, in our study, we intended to create a link between threat priming and the agency detection task, directly addressing the limitations of previous studies. Straight threat priming allowed us to determine whether there exists a bias toward agency detection when one receives information that the potential agents themselves are dangerous (as opposed to priming participants with no specified threatening stimuli). As sensorial unreliability has been found to increase agency detection (M. Andersen et al., 2019), we kept it a

[1] On a side note, there is one study that seemingly shows that people detect agency in nonagentic objects without being primed to do so. In the classic Heider and Simmel's (1944) experiment, participants detected agency in moving triangles without any corresponding priming. However, Radvansky and Zacks (2014) noted that Heider and Simmel (1944) were quite clear that they have constructed the triangle animation so that it conveys a story and agency cues. Thus, the study does not show that we are prone to detect agency by default, but rather that we easily interpret nonhumanoid objects as agents when they show signs of intentionality.

constant condition across all participants to induce false-positive detections.

The implications of our study are relevant to the development of the PPAD model and, through it, to our understanding of religious experiences, the model is said to underlie. In the context of PPAD, there are no reasons to expect that priming subjects with the threatening nature of agents should lead to an increase in false-positive agency detections. We believe that if such a result were found, a reconsideration of the possible evolutionary aspects of PPAD would be called for. Besides bringing back a modular approach to agency detection, this could involve assuming that there might be agency detection biases existing in the predictive architecture of the mind (e.g., "evolved priors" or "hyperpriors" that increase the perceived prior probability of encountering an agent given that the agent can be dangerous, see Asprem, 2019; Maij & van Elk, 2019). Since many studies have suggested that the idea of HADD is questionable, positive results of our study could motivate a development of the hyperprior account.

On the other hand, finding no effect of experimental manipulation would mean that the study failed to provide evidence favoring the idea that a hypothetical HADD is triggered by a feeling of threat. In general, our study contributes to resolving some doubts about the mixed results of previous studies on the feeling of threat and agency detection. However, it is not designed to rule out one of the two distinct neurocognitive models of agency detection, but rather to constrain the theoretical space for further studies to come. Hence, we believe that the present study makes for a first step toward a "strong inference" (Platt, 1964) research in the field of agency detection. Instead of focusing on testing one particular prediction of a given theory—which is also valuable—our experiment points out which of the competing models is more worthy of further pursuit.

We present and discuss the possible impact of our study in Figure 1, which contains a hypothetical development of the PPAD model involving hyperpriors, feasible for interpreting the positive results of our study (for a general discussion of possible implementation of modular insights into the predictive processing framework, see, e.g., Asprem, 2019; Drayson, 2017). Regardless of the results, we believe that our study adds to the debate on agency detection and consequently—considering that agency detection is understood as a building block underlying religious beliefs and experiences—contributes to our understanding of religious experience and belief.

Since we designed the study to provide novel empirical data and aim to explore a theoretical shift in the understanding of neurocognitive realizations of agency detection (where both the null and positive results would be important), we saw the preregistration of our study as essential. The format of the registered report ensures publication of our data regardless of the nature of the results and increases the probability that the study will motivate further discussion in the field of agency detection.

Method

Study Rationale

In the present study, we aimed to induce the feeling of threat by using direct priming embedded within the instructions for participants, describing the hostile versus neutral nature (for the experimental and control group, respectively) of the agents supposedly inhabiting a sensorially unreliable virtual environment. The instructions acted as a proxy for culturally induced expectations, analogous to the study by M. Andersen et al. (2019), but instead of altering the expectations about the probability of encountering an agent, our instruction for the experimental group was intended to alter the expectations about the nature of agents, while keeping the expected number of agents stable across both groups. After receiving the instructions, the participants explored a foggy, virtual reality forest (with fog as a proxy of sensorial unreliability) and were asked to press a button whenever they feel they have detected an agent. There were no actual agents in the environment, so all reported detections were false positives. The main hypothesis was that participants primed with threatening information about the hostile nature of agents would report more agency detections during the exploration of the environment. A manipulation check with the use of an anxiety questionnaire was used to help decide whether our priming was effective.

We also administered a heart rate (HR) measurement, which was previously used as an indicator of fear (e.g., M. M. Andersen et al., 2020). Nevertheless, we acknowledge that the heart rate can also measure other affective states, such as stress related to a gaminglike challenge (e.g., Trotman et al., 2018). Considering this, in our study, the heart rate levels acts as a continuous variable reflecting the general arousal of participants, followed by a check of whether arousal has a differential impact on agency detection in participants from the experimental group as compared to controls. Finding such an effect would indicate that our priming has induced specific arousal in the experimental group (most likely related to feeling threatened by the agents). Hence, our secondary hypothesis was that priming would moderate the relationship between heart rate and agency detection. Specifically, we expected that this relationship would be stronger for participants from the experimental group.

Finally, we administered a questionnaire measure of anxiety (apart from being used as a manipulation check), which we expected to act as a mediator of the effect of priming on agency detection. We hoped that both tests would bring us closer to ascertaining not only whether our priming induced anxiety but also whether it was the anxiety that made the participants detect more agents in the experimental group. Hence, our third hypothesis was that the effect of manipulation on agency detection would be mediated by anxiety, with higher anxiety in the experimental group leading to increased agency detection.

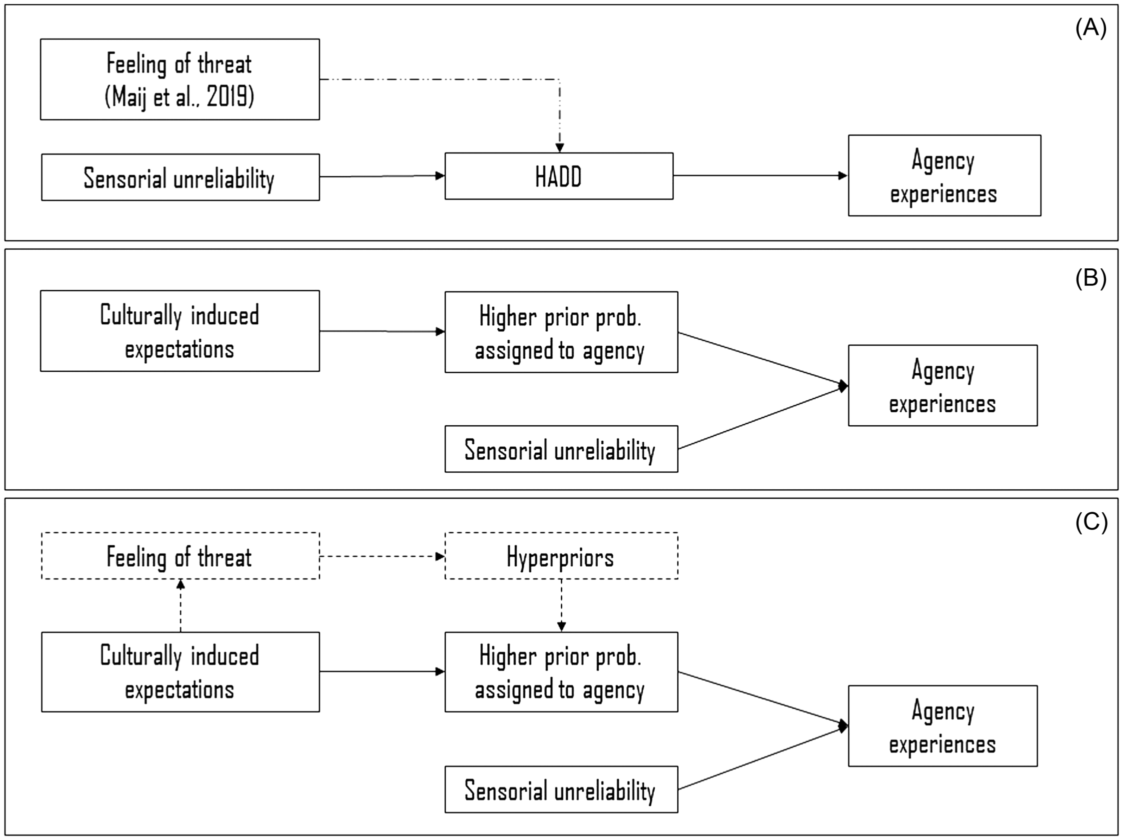
Hypotheses

In the present study, we experimentally tested three directional hypotheses:

> *Hypothesis 1:* In a sensorially unreliable virtual environment, the feeling of threat induced in the experimental group by priming about the hostile nature of suggested agents will significantly increase false-positive agency detections compared to the control group.

> *Hypothesis 2:* The difference in agency detection between participants with low and high arousal levels will be greater for participants in the experimental group compared to the control group.

Figure 1
Possible Impact of Our Study on the Development of Agency Detection Models



Note. (A) A classic HADD model is depicted: The HADD module, activated by sensorially unreliable stimuli (originally referred to as "ambiguous data"), produces agency experiences and can be also facilitated by the feeling of threat (see Maij et al., 2019). A predicts positive results for our study. (B) shows a PPAD account, with expectations leading to an increase of the prior probability assigned to the agency, which, combined with unreliable stimuli, leads to agency detection. B, does not predict the positive results for our study. (C), presents a hypothetical development of the PPAD model, where induced expectations lead to a feeling of threat, under which a hyperprior increases the prior probability assigned to the agency. C is feasible for interpreting positive results of the study instead of returning to the HADD model. HADD = hyperactive agency detection device; PPAD = predictive processing model of agency detection.

Hypothesis 3: Participants from the experimental group will exhibit higher anxiety levels than the controls, leading to increased agency detection.

The Masaryk University Research Ethics Committee approved the design of the study.

Sample

We collected data from 227 healthy Czech and Slovak adults who signed informed consent to participate in the study. We used convenience sampling. A G*Power (Faul et al., 2007) analysis and a Shiny App simulation (https://shiny.ieis.tue.nl/anova_power) were performed, based on which we established that the sufficiently powered sample size would be 219 (see the full power analysis in the additional online materials; https://osf.io/4mgbf/; Szymanek, Nenadalová, & Van Leeuwen, 2024). During the data collection, we excluded from the final sample 17 participants, following our preregistered rationale (substantial issues occurring during the procedure) and ran further trials to reach the maximum number of 219 participants who completed the trial. However, due to a large number of volunteers at the final days of trials and our ethics concerns regarding canceling the invitation to the lab, the final number increased to 227.

We did not invite participants with (not corrected) vision or hearing problems, heart diseases, psychological and psychiatric disorders, previous experience of virtual reality side effects, or high baseline anxiety levels found in an administered prestudy scale. The participant reward was 150 Czech crowns (approx. $6).

Materials

Virtual Reality Equipment and Environment

Participants entered a virtual, dark forest covered in thick fog, proxying conditions of perceptual unreliability (see Figure 2).

Figure 2
Screenshot From the Virtual Forest

Note. See the online article for the color version of this figure.

The environment featured a faint forest ambience and the sound of the avatar's footsteps. There was a winding path through the woods which participants were to follow. Crucially for our study, no agents or agency cues were implemented in the environment. Participants used an HTC Vive virtual reality gear. The environment was created using the Unreal Engine and run on a computer with the following technical specifications: Intel Core i7-5820 K 3.30 GHz, 64 GB RAM, 64-bit Windows 10 Pro, and GeForce GTX 980.

Heart Rate Measurement

To measure the participants' arousal-related heart rate, we used Zephyr Echo BioHarness equipment. The equipment works on a belt placed on the participant's chest. The heart rate measurement from the 5-min task was analyzed against a previously measured 5-min calm baseline. We subtracted the baseline for each participant from the average heart rate during the task. That way, every subject was assigned a parameter reflecting their relative arousal during the task.

Questionnaires

Following Maij et al. (2019), we extracted the Fear subscale from the Positive and Negative Affect Schedule–X (PANAS-X) scale (Watson & Clark, 1999), which consists of six items. To these items, we added three more: "anxious" (following Maij and collaborators), "threatened," and "tense." On a scale from 1 to 5, participants indicated to what extent they had felt as specified during the exploration of the virtual forest. We planned that the reliability of the scale must reach at least $\alpha = .80$.

Additionally, for exploratory purposes, participants filled out a short questionnaire at the final stage of the trial, asking (a) what the beings they perceived looked like, (b) whether they had any peculiar experiences around any of the button presses, (c) what they thought the real purpose of the study was, (d) whether this was their first time using virtual reality, (e) whether they regretted pressing the button at certain times, and (f) about their religious and/or spiritual background. The answer to the third question was one of the criteria for data exclusion, and answers regarding religious or spiritual affiliation were planned to be used for additional data analysis. All questionnaires can be found in the appendix uploaded as additional online materials (https://osf.io/4mgbf/).

Procedure

Eligible candidates (see the Sample section) were invited to the lab and informed they would participate in a study on perception in virtual reality. They put on a heart measurement tool, which collected baseline data for 5 min while they read the informed consent form.

Participants then put on virtual reality gear, received usage instructions, and underwent training in a neutral virtual scene. Next, they were randomly and double-blindly assigned to either the experimental or control group. Both groups received detailed instructions via an automated recording before entering the virtual forest. They were to explore the forest for 5 min and click a button whenever they felt they detected one of the "beings" that were supposed to inhabit the forest. While the experimental group's instructions introduced the beings as hostile, the control group was told the beings were neutral. Complete English versions of instructions can be found in the appendix (see the additional online materials; https://osf.io/4mgbf/).

All participants entered the same virtual forest. Their heart rates were measured during the task. After the 5-min exploration, participants removed the gear and filled out an affect scale and final questionnaire (see the Materials section). They were then debriefed and received monetary compensation. The procedure was tested in a pilot study ($N = 11$), detailed in the additional online materials (https://osf.io/4mgbf/).

Data Analysis

The data from the study were analyzed using SPSS, Jamovi, and R software. All data in raw and preprocessed form, as well as all changelogs and analysis scripts, have been uploaded to an open science repository (see additional online materials; https://osf.io/4mgbf/). We also placed there a detailed, preregistered plan of the analysis. Please note that any deviations from the plan are marked in the relevant point in the Results section.

Results

Data Preprocessing and Descriptives

After reaching the number of 218 participants and excluding 17 of them on the basis of the methodological criteria, we continued trials, reaching the number of 227. Another 28 participants were excluded on the basis of criteria related to guessing the purpose of the study. This left us with a final sample of 199 valid participants (four missing responses on gender and age; 127 F, 66 M, two nonbinary; age $M = 23.70$, $SD = 5.73$). The reliability of the affect scale satisfied our preregistered criteria ($\alpha = .9$, 95% CI [0.88, 0.92]). Ten participants were additionally excluded from relevant analyses due to missing group information, affect scores or first time in VR response. We present the means and standard deviations for heart rate increase, anxiety, and agency detection separately for groups in Table 1. Table 2 presents correlation matrix of those variables together with age.

Analysis 1a: Manipulation Check

To test whether our manipulation was successful, we conducted a linear regression with group as the predictor of affect scale scores,

Table 1
Means and Standard Deviations of Relative Increase in Heart Rate, Anxiety, and Agency Detection by Group

	Experimental		Control		All	
Group	*M*	*SD*	*M*	*SD*	*M*	*SD*
Heart rate increase	10.89	28.87	8.36	26.81	9.74	27.90
Anxiety	18.75	6.75	16.54	5.77	17.74	6.40
Agency detection	4.89	8.20	3.08	4.27	4.07	6.75

Note. Scores for heart rate increase were not trimmed of ±3 *SD* outliers (see the Analysis 2 section).

adding first time in VR as a control predictor. After transforming the affect scores with inverse tangent, assumptions for linear regression were met: The data did not differ from normal (Q–Q plot), and we did not observe deviation from homoscedasticity of residuals (eyeball test).

The model was significant, $F(2, 190) = 5.13$; $p = .007$; adjusted $R^2 = .04$, and the group predicted PANAS-X scores, with the experimental group scoring higher than controls ($\beta = 0.11$; β 95% CI [0.02, 0.21]; $t = 2.28$; $p = .024$). The PANAS-X data by group are presented in Figure 3.

There was no statistically significant difference in affect scores between participant who used VR for the first time and those more experienced with the technology ($\beta = -0.10$; β 95% CI [−0.20, 0.01]; $t = -1.90$; $p = .058$). After trimming the PANAS-X data (±3 *SD* outliers), the model did not change significantly.

This result suggests that our manipulation was successful: Participants in experimental group reported higher anxiety than controls. Thus, we performed the subsequent analyses as planned.

Analysis 1b: Main Effect

We excluded six participants with missing first time in VR responses from the analysis. After fitting our data to a linear regression model to test whether group (and first time in VR as a control) predicts number of agency detections, we found the assumptions for the test not satisfied, even after different transformations of the data. Thus, we switched to an independent-samples *t* test, but the assumption of normality—as indicated by a Q–Q plot—was also not met. We therefore resorted to a Mann–Whitney *U* test, which did not reveal significant evidence for a difference between groups in the number of agency detections ($W = 3{,}915$, $p = .060$). We conducted another test with group as a predictor of trimmed (± 3 *SD*) agency detection data (which excluded one participant scoring 61 button presses), but it did not reveal any significant change. A separate Mann–Whitney *U* test of first time in VR as predictor of agency detection also did not provide any evidence for an effect ($W = 4{,}363$, $p = .511$). See Figure 4 for a histogram of agency detection across groups.

Therefore, the analysis did not provide evidence supporting our first hypothesis, namely, that there will be more false-positive agency detections in the experimental group as compared to the control group.

Analysis 2: Moderation

After excluding 20 participants who had at least 10% of their heart rate data missing and combining the heart rate data with agency detection data, which revealed another 27 missing observations, we tested the assumption for linear regression. Despite transforming the data with logarithmic function, we found both the Q–Q plot and plot of residuals ambiguous. No strong collinearity was found (variance inflation factor = 1.00). The scatterplot did not suggest a nonlinear relationship between heart rate and agency detection. However, overall, it is not clear whether all assumptions for the analysis were met (which would also be true for analysis of variance analysis with interaction), so the results need to be interpreted carefully.

Next, we fitted a PROCESS model of moderation (Model 1), with the relative increase of heart rate as predictor, group as moderator, and the number of button presses as the dependent variable. The model was not statistically significant, $F(3, 166) = 1.47$; $p = .220$; $R^2 = .03$.

It must be noted, though, that due to unknown reasons, the validity of the heart rate data was strongly compromised. Fifteen participants had a heart rate of over 200 either during the rest time or during the task, and, in some cases, the relative change of heart rate reached the absolute value of over 100. We investigated the plots of heart rate related to 15 such participants (our criteria: 150 < heart rate < 50, absolute value of relative increase >100) and discovered that the measurement confidence in those measurements was scarcely reaching higher than 50% (compared to participants randomly sampled from the rest of the group, where the confidence was often 100% and had a higher variability).

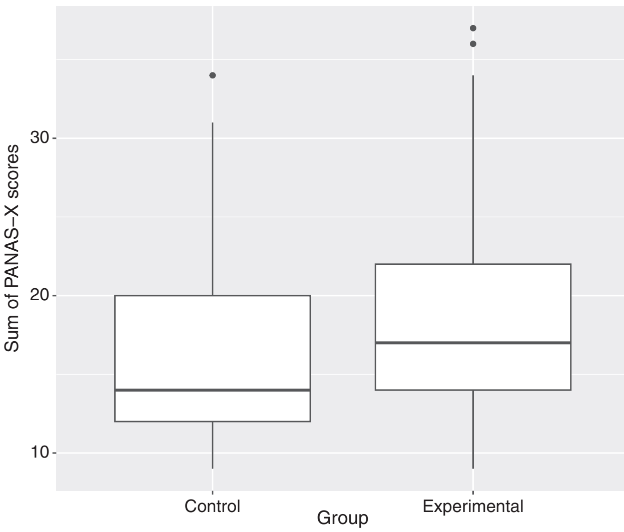
To avoid arbitrary decisions, we decided to trim the data of outliers twice and see if there is any significant change in the regression model. First trimming rendered the model significant, $F(3, 153) = 3.61$; $p = .015$; $R^2 = .07$, and revealed that relative increase in heart rate predicted agency detection ($\beta = 0.17$; β 95% CI [0.005, 0.03]; $t = 2.91$; $p = .004$), though there was no effect of group ($\beta = 0.2$; β 95% CI [−1.12, 0.52]; $t = 1.25$; $p = .213$) and no interaction ($\beta = -0.009$; β 95% CI [−0.03, 0.08]; $t = -1.02$; $p = .312$). We performed a second cut, after which the whole model turned out not significant, $F(3, 144) = 1.35$; $p = .262$; $R^2 = .03$. We also checked if the trimming excluded the 15 participants with criteria specified above—since it did, we believe the results of the additional two analyses are more representative of the effects in the sample.

Table 2
Correlation Matrix for Variables Age, Relative Increase in Heart Rate, Anxiety, and Agency Detection

Variable	1	2	3	4
1. Age	—			
2. Heart rate increase	$r = -0.14$ ($p = .067$)	—		
3. Anxiety	$r = -0.12$ ($p = .119$)	$r = 0.09$ ($p = .238$)	—	
4. Agency detection	$r = -0.20^{**}$ ($p = .009$)	$r = -0.02$ ($p = .833$)	$r = 0.24^{**}$ ($p = .002$)	—

Note. Scores for heart rate increase were not trimmed of ±3 *SD* outliers (see the Analysis 2 section).
** $p < .01$.

Figure 3
Boxplot of Sum of PANAS-X Scores Separately for Control and Experimental Group



Note. PANAS-X = Positive and Negative Affect Schedule–X.

Overall, as conditions specified in the Methods section were not met, the results of analysis do not support our second hypothesis; we found no evidence for a moderation effect of group on relationship between heart rate and agency detection.

Analysis 3: Mediation

To test the third hypothesis, we fitted a PROCESS mediation model (Model 4; bootstrapping method = 5,000) with agency detection as dependent variable, group as predictor and PANAS-X scores as a mediator. Assumptions of normality and homogeneity of residuals were met (eyeball tests), and there was no evidence for strong collinearity (variance inflation factor = 1.04).

In line with Analysis 1a, the first model, $F(1, 195) = 7.54$; $p = .007$; $R^2 = .04$, revealed a simple effect between group and affect scale score (β = 2.43; β 95% CI [0.68, 4.18]; $t = 2.75$; $p = .007$), further corroborating that our manipulation was successful. Within the second simple-effects model, $F(2, 194) = 4.28$; $p = .015$; $R^2 = .04$, we found an effect of anxiety on agency detection (β = 0.03; β 95% CI [0.005, 0.049]; $t = 2.43$; $p = .016$) but no direct effect of group on agency detection (β = 0.16; β 95% CI [−0.12, 0.43]; $t = 1.13$; $p = .262$). As for the total effect model, $F(1, 195) = 2.58$; $p = .110$; $R^2 = .01$, the effect of group on agency detection was also not statistically significant (β = 0.22; β 95% CI [−0.05, 0.49]; $t = 1.61$; $p = .110$), in line with our null finding from Analysis 1b. However, the bootstrapping method revealed a positive indirect effect of group on agency detection through the PANAS-X scores (β = 0.07; 95% Boot CI [0.003, 0.15]).

As the model did not bring any new evidence for an effect of group on agency detection, the conditions specified in the Methods section for the third hypothesis to be corroborated were not met. However, within the model (a) agency detection was predicted by PANAS-X scores and (b) subtracting the indirect path decreased the total effect, even though the total effect was not significant. We conducted another mediation to see if significance could be driven by ±3 *SD* outliers. The effect of group on affect score was stable ($p = .013$), which was also true for effect of anxiety on agency detection ($p = .020$), and similarly to the previous model, group had no direct effect on agency detection ($p = .320$). There was also no total effect ($p = .156$), and the indirect effect reported previously disappeared (β = 0.06; 95% boot CI [−0.001, 0.14]).

Analysis 4: Equivalence Testing

Since the test of our main hypothesis turned out insignificant, to investigate the validity of our null results, we conducted an equivalence testing with the use of two one-sided *t* tests procedure (we estimated the smallest effect size of interest at $d = 0.15$; α level = .05). The equivalence bounds were calculated to be −0.92 and 0.92. The one-side *t* test with higher *p* value did not reach the α level, $t(163) = −0.98$; $p = .836$, and 90% CI [−3.19, −0.34]. Thus, we cannot assert equivalence: The procedure did not bring evidence that the effect of group on agency detection is smaller than the smallest effect size of interest.

Exploratory Analyses

Poisson Regression

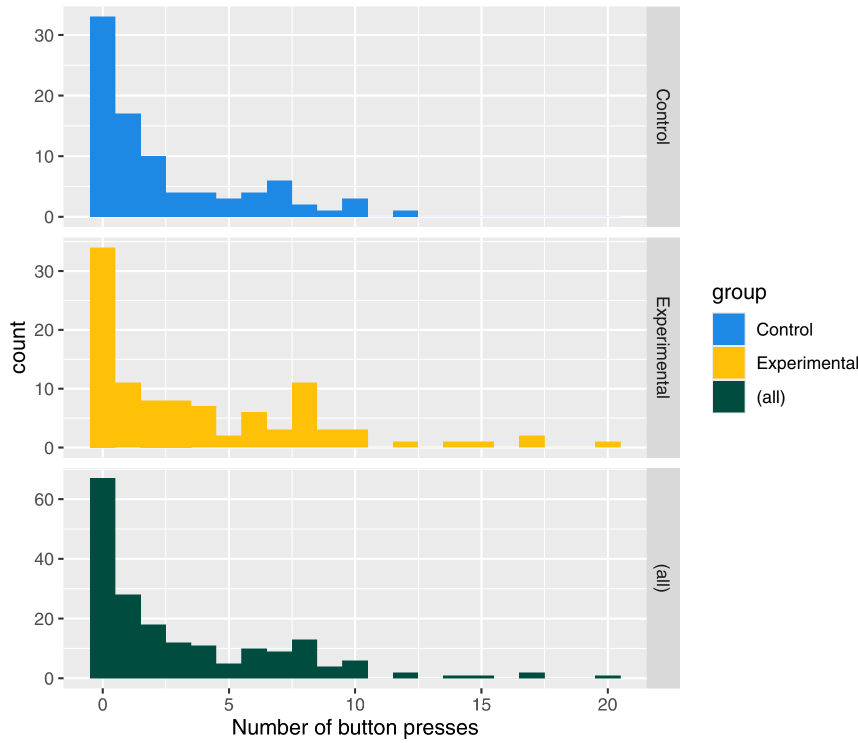
The results of our planned analysis raised some doubts regarding their robustness. First, it should be noted that due to the assumptions not being met in Analysis 1b, we were forced to switch to Mann–Whitney *U* test, which tends to be more conservative (i.e., there is a higher probability of a type I error) than *t* test. Second, an inspection of the data revealed that another, more suitable analysis could be performed, namely, a generalized linear model that assumes a Poisson-like distribution of the dependent variable (used also by M. Andersen et al., 2019, for the same kind of agency detection data). Indeed, our count numbers of agency detection approximated a negative binomial distribution and met the assumption of overdispersion. Thus, using this kind of analysis would allow for a more representative results than Mann–Whitney *U* test. Finally, we were quite surprised to find a stable relationship between anxiety and agency detection, revealed by correlations (Table 2) and mediation analysis; the latter also provided some evidence for an indirect effect of group on agency detection.

Based on those considerations, we decided to fit a negative binomial model with group as the predictor for agency detection. We trimmed the dependent variable to make sure that the outlier with a score of 61 would not drive the results. Within the sample, both absolute and conditional variance was higher than absolute and conditional means, which indicated an overdispersed distribution. The model revealed a result in line with our main analysis and the same *p* value just above the level of significance; we thus did not find a statistically significant effect of group on agency detection (β = 0.38; $p = .060$).

Religiosity/Spirituality and Agency Detection

To test the long-discussed relationship between supernatural beliefs and agency detection, we decided to run correlations between the latter variable and participants' responses to questions about their religiosity, spirituality, and belief in supernatural agents (responses

Figure 4
Counts of Button Presses Numbers in Groups and Across the Whole Sample



Note. The plotted data have been trimmed of ±3 *SD* outliers. See the online article for the color version of this figure.

were provided on an yes/no/hard to tell or 1–7 Likert scale). All "hard to tell" responses were treated as missing. All responses were correlated ($.30 < r < .88$, p values $< .001$). However, we observed no correlations between any of these responses and agency detection counts (see Table 3).

Qualitative Analysis of Participants' Subjective Experience

To explore how agency detection in the virtual forest was experienced subjectively, we conducted a qualitative analysis of data obtained from the final questionnaire. Using a standard open coding method (i.e., inductive derivation of categories based on participants' answers; Saldaña, 2009), we analyzed responses to three open questions: (a) what the beings the participant perceived looked like, (b) whether they had any peculiar experiences around any of the button presses, and (c) whether they regretted pressing the button at certain times.

Following the contingency table summarizing analysis results (see it in the additional online materials; https://osf.io/4mgbf/), we found that the numbers of illusory perceptions were only slightly higher in the experimental than control group (see Table 4 for the total numbers in each category). Agency detection experiences were mostly based on auditory cues from the forest environment (forest sounds, noises in the proximity of participants, footsteps, etc.; 161 in total). Of all those auditory cases, 33 were reports of feeling that one is being observed or closely followed by a being which made the respective noise. However, we also observed that some participants explicitly mentioned misattribution of a sound (like crackling branches under their virtual legs or their footsteps, as designed in the game) to a presence of another agents.

Some participants also note that they vividly imagined an appearance of expected beings based on felt presence, emotions, or sounds (23 cases). Visual perceptions (57 cases in total) were often in a form of silhouettes, shapes, or shadows in the mist (27 cases), sometimes later recognized as trees, stones, and so on. Only in three cases did participants report actually seeing respective beings or figures. Interestingly, as evident from the description of perceived or imagined beings, participants' imagination sometimes followed the priming instructions. In the experimental group, participants tended to label these beings as "slenderman," "quite horrible and frightening," or "predatory animals," while participants in control group imagined "forest fairies" and "shy and harmless," "rather friendly" beings (see Table 5 for examples).

Analyses of button press regrets show that some participants indeed regretted pressing the button (29 in the experimental and 17 in the control group) for reasons like double-click, mistake, or subsequent understanding that the perceived sound was made by them and not by beings, as well as that the spotted shape was a tree, stone, and so on. However, there were also cases where participants regretted not pressing the button (11 cases in the experimental and eight in the control group). They mostly indicated being too

Table 3

Correlations Between Agency Detection and Responses to Questions Related to Religiosity, Spirituality, and Supernatural Beliefs

Variable	Religiously affiliated*	Follows a form of spirituality*	Religious*	Spiritual*	Engages in religious/ spiritual activity*	Religious	Importance of religion	Spiritual	Importance of spirituality	Belief in supernatural agents*	Belief in merciful agents*	Belief in malicious agents*
Agency detection	r = 0.01 (p = .909)	r = −0.05 (p = .500)	r = 0.06 (p = .450)	r = −0.04 (p = .605)	r = −0.03 (p = .727)	r = 0.03 (p = .709)	r = −0.02 (p = .833)	r = 0.00 (p = .983)	r = −0.01 (p = .902)	r = −0.04 (p = .641)	r = −0.03 (p = .754)	r = 0.03 (p = .718)

Note. "Hard to tell" responses were treated as missing. Variables marked with asterisks are dichotomous, the rest of the variables were measured on a scale. See the appendix in the additional online materials (https://osf.io/4mgbf/) for exact wording of all respective questions.

focused on visual detection of expected beings, and thus did not click if they only perceived a sound or felt some presence without actually seeing an agent.

Discussion

Our study aimed to revisit the problem of whether feeling of threat increases agency detection. While we found evidence that our manipulation worked, we were not able to corroborate any of our hypotheses. First, there was no statistically significant effect of our manipulation on agency detection in either the preregistered or exploratory analysis, though it bears noting that both produced a *p* value just above the threshold for significance with the difference between groups tending in the hypothesized direction. Second, we did not find that the effect of the manipulation moderated the relationship between arousal and agency detection. Third, our data did not allow us to corroborate the hypothesis that there is an indirect effect of our manipulation on agency detection through participants' felt anxiety as a mediator (though see below for complications). To sum up, our study did not provide compelling evidence that humans are more prone to detect illusory agents when they feel threatened by them. However, there are several remaining points that should be discussed to draw the full picture of what our study means for the field of agency detection, and more generally, to the cognitive and evolutionary study of religion.

Neurocognitive Mechanism of Agency Detection

Looking back at the theoretical space we presented in the Introduction section, we would like to start with discussing the implications for the claim that conditions of threat activate the HADD, leading to increased numbers of false-positive agency detections. First, it needs to be noted that while our results do not support the existence of HADD as conceptualized in the study by Maij et al. (2019), they do not speak against a more general idea that humans are naturally biased toward detecting the agents that are not really there, be it in a modular or predictive model of the mind. The idea put forward by Maij et al. (2019) was based on the reasons that are usually used to justify the existence of hyperactive agency detection, namely, that HADD evolved as a safety valve that kept us safe from costly errors. However, this does not necessarily mean that HADD should activate in conditions of threat; its function could be to decrease predatory threat precisely by detecting agency in ambiguous data independently of other factors, which would resonate with the idea of modular encapsulation.

However, for the sake of the second point, let us assume that there exists a relationship between feeling of threat and agency detection large enough to be practically interesting. On one hand, given our sample size, we should be able to find this relationship, especially considering that our study seems to be "methodologically strong"—we administered a manipulation that directly influenced participants' expectations about agents, and the use of virtual reality likely rendered our test ecologically valid. On the other hand, we were obliged to exclude a high number of participants from the analysis (which reduced the statistical power), and our data revealed quite interesting patterns: First, in one of our models, anxiety was mediating the effect of the manipulation on agency detection (even though the latter did not show up in our analyses); and second, we

Table 4

Summary of Illusory Perceptions Based on Involved Senses With Respective Type of Experience and Accompanying Feelings, Emotions, and Imagination

Group	Experimental	Control	Experimental	Control	Experimental	Control	Experimental	Control	Experimental	Control
Visual reports	Respective figures		Silhouettes, shadows, mist		Shapes mistaken for beings		Movements		Unclear	
	2	1	15	12	6	8	4	4	2	3
Auditory reports	Forest sounds		Nearby sounds/noise		Footsteps		Voices		Unclear	
	8	6	42	35	39	27	1	0	1	2
Tactile reports and other	Touch		Uncertainty, nervousness, anxiety, fear		Feeling of being watched and followed		Percepts- and emotion-based imagination		Strong expectations, curiosity	
	0	1	12	14	17	16	13	10	2	9

Note. Grey represents experimental, while white represents control. Reported experiences were mainly auditory in a form of nearby sounds, noises, and footsteps (sometimes described as "other than my own"). Among visual perceptions, strange shadows, silhouettes, or shapes in the mist were among the most common ones. Still, number of felt presences of beings without any actual visual perception was slightly higher than that. Participants also tended to vividly imagine how the beings looked on the basis of (mainly auditory) perception and feelings of presence.

found a stable correlation between levels of anxiety and agency detection. Additionally, our test of equivalence did not provide evidence that the effect size of manipulation is smaller than the smallest effect size of interest. It is then possible that the true effect size landed in a kind of a "sweet spot" between the borderline of practical significance and the smallest value detectable by our tests. Hence, we speculate that our results could actually underestimate the effect of threat, and further studies need to be conducted to provide a more comprehensive picture.

These considerations aside, predictive processing-based ways of interpreting the results do not suggest that feelings of threat increase the prior probability assigned to agent-related predictions in the predictive mind—thus, we do not see sufficient reasons to develop the idea of an evolutionary built-in hyperprior that affects detections of agents in conditions of possible danger. Still, as mentioned earlier, a general bias toward agency detection could exist, even with the assumption that our perception works in a predictive way. On the one hand, we could be hardwired to overestimate the probability that an agent is present nearby if the sensory data are unreliable and we are generally uncertain or anxious—as suggested by the robust relationship of anxiety and agency detection found across our two groups (see also Nenadalová & Řezníček, 2024). On the other hand, we live surrounded by other agents and it is likely that during our development, we start to overfit the sensory data with the model of an agent (see Van Eyghen, 2020) or simply learn that in most cases, the probability of encountering another human or animal is indeed quite high.

Finally, the results of our study do not reveal any inconsistencies with basic predictions of the PPAD. In M. Andersen's (2019) model, false detections of agents are driven by two basic factors: expectations (prior probability assigned to the prediction of agent) and sensory unreliability (which leads to reduced precision and thus decreases the weight of prediction error). In our study, 65% of all participants pressed the button at least once, which might reflect that their expectations regarding the possibility of encountering an agent combined with conditions of low sensory unreliability successfully induced false perceptions of agency. This would align with previous research that found agency detection to be driven by expectations

Table 5

Quotation Examples From the Experimental and Control Group

Quotation example
Experimental group
Humanoid with long limbs and could crawl well. I had the feeling I was being followed the whole time, but when I turned around, they were just hiding behind a bush. I think they might have looked a bit like Slenderman.
I had only heard the creatures, but I imagined them as some modified version of a human being. Something dark standing and walking nearby, something that's always on guard.
Unfortunately, I only heard the creatures, however two of them were rather shy, with one of them being defensive—growling and snarling to defend its territory.
Animal-like appearance, I was a little scared to see if they'd jump out at me.
Control group
If I saw anything out of the corner of my eye, it was more like shadows. They were very loud, though, and I could often hear footsteps behind me, but I couldn't see any particular source of the sounds. The footsteps were quite frightening. At the same time, I was afraid of a rock that appeared halfway down my path. There was not a good atmosphere.
Cute and human maybe furry but smaller.
I felt their presence, they came around me and touched me.
They were shy and harmless, about the size of a 10-year-old child; as there were no visible footprints, food remains, disturbed soil, or similar things in the forest; they are apparently intelligent.

combined with noise (M. Andersen et al., 2019; Szymanek, Homan, et al., 2024). However, as mentioned above, it could be said that the result speak also to a general bias in the predictive architecture (spontaneous detections triggered by uncertainty; see Nenadalová & Řezníček, 2024) or the HADD account. A way to conduct a "strong inference" test regarding whether agency detection is driven by a general bias or expectations would be to check not only false positives, but also false negatives, as within this approach, one can evaluate if differing expectations can also *decrease* agency detection in the same, unreliable stimuli (see Szymanek, Homan, et al., 2024). Furthermore, as noted by one of our anonymous reviewers, another way to create a more robust experimental design is to create a "super-control" condition with minimal content (e.g., "you might see some animals"), which could render the priors more random, and thus provide a kind of "base rate" to compare other groups with.

However, the results of our qualitative analysis inspire us to think about a more qualitative approach to the problem; namely, it could be tested whether *qualitatively* different expectations lead to *qualitatively* different perceptions of agents. In the case of our study, participants in the experimental group tended to report imagining more "grim" and threatening agents than controls, but there were not enough visual perceptions to see if the percepts themselves differed between groups. Further studies should be conducted to test if indeed expecting different kind of agents leads to different perceptual experiences, which would be very much in line with the core idea of PPAD but has not been predicted by any iteration of the HADD.

Agency Detection, Religion, and Spirituality

At the beginnings of CESR, our propensity for illusory agency detection was said to be an underlying cause of creation, acquisition, and transmission of beliefs in supernatural agents. Since then, multiple studies have provided rather mixed evidence regarding this relationship (e.g., Tratner et al., 2020; van Elk, 2013, 2015; see also Van Leeuwen & van Elk, 2019). In our study, false agency detections did not correlate with any variables related to participants' religiousness and spirituality. However, a natural limitation to this null finding is that our sample was relatively nonreligious and nonspiritual: histograms of respective variables revealed that responses were strongly skewed toward no religious affiliation, no belief in supernatural agents, and low levels of religiosity and spirituality. We cannot rule out that a sample with higher variability would reveal a covariance of agency detection and said variables.

Furthermore, since our results do not contradict a general agency detection bias (see above), the possibility that agency detection underlies supernatural beliefs is still on the table. However, assuming the PPAD model, detection of false agents can also be driven by expectations and—as M. Andersen (2019) argued—these expectations might be influenced by information about the "distribution" of supernatural agents in a given environment. If providing information such as "that house is haunted" changes estimated prior probability of seeing a ghost and leads to experience of a ghost sighting, supernatural beliefs can be said to "piggyback" our mind's natural propensity to rely on its best guess when faced with unreliable sensory data. In our study, we provided participants with a proxy of information that can be found in the world of religious, spiritual, and paranormal beliefs, and although we do not know if perceptual experiences responded to this information, the manipulation seemed to affect participants' retrospective imagery- and emotions-infused representation of the agents. This would be in line with Van Leeuwen and van Elk's (2019; see also Barrett & Lanman, 2008) suggestion that agency detection experiences are later interpreted in light of prior (religious) beliefs.

A final note is that some of our participants reported an experience that could be considered the "feeling of presence"—an experience of closeness of another agent, found in laboratory, religious, and many other contexts (Alderson-Day, 2023; Barnby et al., 2023; Brugger et al., 1996) which can be directly related to one's cultural and religious background (Granqvist & Larsson, 2006; Luhrmann et al., 2021; Vehar et al., 2022). In previous studies, feelings of presence have been found to covary with participants' uncertainty induced by sensory deprivation (Nenadalová & Řezníček, 2024), as well as virtual reality immersion combined with active imagination (Erickson-Davis et al., 2021). In the present study, the fog covering the virtual forest inhibited capacities of sensory discrimination, likely putting participants in a comparable state of uncertainty (as evidenced by their reports). Regardless of our prior considerations, we speculate that conditions of sensory unreliability found in various religious contexts might trigger uneasy feelings of being observed or followed, which could be due to a general agency detection bias reacting solely to uncertainty. If that is correct, religiously meaningful feelings of presence could be thought of as more spontaneous and independent of one's prior expectations than other agency detection experiences.

Limitations and Outstanding Questions

Our study has several important limitations. First, as mentioned earlier, due to preregistration of our analyses, we needed to exclude many participants from our sample, which reduced the power of statistical tests. In effect, the sample could be underpowered when it comes to finding a practically significant effect of our manipulation on agency detection.

A related, problematic matter is that despite finding no main effect, we found a statistically significant relationship between anxiety and agency detection. If we assumed that a feeling of threat indeed increases agency detection to an interesting extent, the effect of anxiety would not be surprising—in that case, the plausible explanation would be that our manipulation did not induce a feeling of threat *enough* to create a detectable difference between groups.

Third, as noted in the Results section, during our analyses, we discovered that the validity of our heart rate data was compromised. However, we were unable to determine what caused the problem and if it affected data from large numbers of participants. A two-round trimming of outliers likely increased the robustness of results from the moderation analysis, but they are still loaded with low confidence.

Coming to a close, we would like to make two final notes. First, the analysis of qualitative data revealed that participants detected agents mostly based on auditory cues (e.g., footsteps) and some of them regretted not pressing the button at times because they thought that merely hearing a sound does not count as a proper "detection." While the latter hints that our instructions could be too suggestive of visual kind of detection, the large number of auditory detections might imply that the unreliability of visual data in our virtual environment was too low to induce high number of visual misperceptions (although some participants saw "silhouettes" or "shadows") or, alternatively, that agency detection is strongly driven by auditory cues. The last implication aligns with the idea that agency detection has evolutionary roots, as it would be adaptive for humans to rely on the 3-D modality of audition, which depends on attention to a lesser extent than vision and can be of use in low-visibility conditions.

Apart from exploring this matter in more detail and our recommendations provided in previous subsections, we would also like to point out that some of participants reported misattribution of sounds made by their own avatar to the presence of other agents. In the predictive processing view, our perceptual experiences stem from an on-going effort of predicting all incoming signals, including bodily ones. As our participants were not really walking, there could have been a mismatch between their visual and auditory input and proprioceptive data; in conditions like this, people can experience a decrease of felt ownership over the avatar (see, e.g., Tsakiris, 2017) and hence mistake the avatar-produced sounds to be not "their own." Further studies could investigate this potentially interesting cause of agency detection (self-generated "other" agent detection) and its place within CESR.

Conclusions

In this preregistered, experimental study, we revisited whether people are more prone to falsely detect an agent in their surroundings when they expect the agent to be dangerous. Despite not corroborating our hypotheses, we found several patterns in both quantitative and qualitative data which call for further studies on the topic of agency detection as a building block of religious and spiritual phenomena.

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