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Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

16th Jan 24

Dear Mr McGovern,

We apologize for the delay in reaching a decision on your manuscript. As explained via email, two reviewers had withdrawn from the paper which delayed the evaluation. Thank you for your patience!

Your manuscript titled "The power of insight: Psychedelics and the emergence of false beliefs" has now been seen by 3 reviewers, and I include their comments at the end of this message.

The reviewers are in principle enthusiastic about your work. However, they also mention several concerns. We are very interested in the possibility of publishing your manuscript in Communications Psychology, but would like to consider your response to these concerns in the form of a revised manuscript before we decide on publication. To support you in these revisions, I have attached an annotated version of your manuscript.

Review and Perspective papers must speak to a broad audience, including experts from different subfields, for whom it serves as an entry point into your line of research. At the same time, the piece needs to offer some new insights for specialists in the field. We ask that in the revisions, you implement the necessary changes to serve both of these aims.

Importantly, Reviewers #2 and #3 mention that the first part of your manuscript can be revised to aid the reader in grasping the essence of your argument more quickly. Please do not remove specialist literature, however, please ensure the technical arguments are easy to follow and the non-specialist reader understands why they are presented with these details.

At the same time, please revise the piece in response to the technical queries that Reviewers #1 and #3 raise regarding your framework.

In sum, we invite you to revise your manuscript taking into account all reviewer and editor comments.

EDITORIAL POLICIES AND FORMATTING

You will find a complete list of formatting requirements following this link:

<https://www.nature.com/documents/commsj-style-formatting-checklist-review-perspective.pdf>

Please use the checklist to prepare your manuscript for resubmission.

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If you have any questions about any of our policies or formatting, please don't hesitate to contact me.

Please use the following link to submit your revised manuscript and a point-by-point response to the referees' comments (which should be in a separate document to any cover letter):

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** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within 12 weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Antonia Eisenkoeck

Senior Editor

Communications Psychology

REVIEWERS' EXPERTISE:

Reviewer #1: predictive processing

Reviewer #2: false memories/beliefs

Reviewer #3: psychedelics

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I enjoyed reading this timely manuscript, which offered a convincing account of the experience of insight associated with psychedelic experiences. I felt this was informative and well cited, including much of the relevant literature from both computational neuroscience and neuropharmacology. The conclusion regarding false insights is an important message with potentially very significant clinical implications.

There are very few comments I can make to improve this. However, there was one section that I felt conceptually weaker than the others and one other point the authors could think about expanding on.

The weakest point here is the story told about the Eureka Heuristic on page 17:

- "...results in a novel model, it solicits a prediction error..." - if the novel reduced model leads to greater prediction error, Bayesian model reduction would not have let us select this model in the first place. In other words, Bayesian model reduction is just a model comparison technique and almost by definition will end up selecting models with lower absolute prediction errors as these will be the best models to account for data.

- "...the ideas that have high expected precision..." - precision of what? I assume this high precision would come from the large difference in model evidence between the novel (reduced) model, and the full model it is derived from (i.e., the definitive model comparison)? If so, it would be useful to say so. However, if prediction error magnitude is a proxy for negative model evidence, then this implies less prediction error when we develop precise beliefs during model reduction and seems to contradict the earlier point as outlined above.

- "...the feeling of insight - the precise prediction error..." - the association of the feeling of insight and precise prediction errors needs further unpacking. Intuitively, I would have thought the precise posterior belief we arrive at - and not the error that allows us to reach it - a more obvious analogue for the feeling of insight.

The other point that could possibly be improved is on page 18, which brings in an interesting dimension with the comments about dopamine.

There is potentially a missed opportunity here. Under active inference, dopamine is sometimes (e.g., doi:10.1162/NECO_a_00912, doi:10.1093/cercor/bhu159) seen as a measure of changes in precision in beliefs about the course of action we are following i.e., if we become more confident in what we are doing, there is a spike in dopamine (reward being a common example of something that convinces us we are on the right track).

Arguably, this could also relate to mental actions such as the comparisons between alternative hypotheses in Bayesian model reduction (i.e., 'choosing' between models). In addition to reward, information gain can be an important sign that we are on the right track, so can also increase our confidence in our choices, leading to a dopamine spike. If insight is read as a change in beliefs following the choice of a model that leads to a precise model comparison and belief update, there is a clear mechanistic link between dopamine and belief-updating which links directly to the idea of developing a very precise new belief.

I hope the above is helpful in updating this, already very strong, manuscript.

Reviewer #2 (Remarks to the Author):

Review of “The power of insight: Psychedelics and the emergence of false beliefs” by H. T. McGovern et al., submitted to Nature Communications (Psychology)

General impression

This is a review paper that aims to add to existing theory towards understanding the impact of psychedelic drugs (used in psychotherapy settings) on beliefs, with a particular focus on the risk of creating false beliefs in the process. The paper draws mostly on the neuropsychological literature, which is not my area, so I see my role here mostly as that of a non-expert reader wishing to learn more about this topic, and I will try to judge the paper on the basis of its merits for a wider audience. Here are my core points:

(1) The topic of this paper and the research reviewed are certainly interesting, and the paper is generally competently written and informative, but my feeling is that it is too narrowly aimed at an ‘insider’ neuropsychological audience (for a general journal such as Nature Communications). Much of the literature review that underpins the theoretical ideas put forward just refers to specialist literature without much description of the relevant findings, and as a result it is difficult to judge the merits of the proposed arguments without detailed knowledge of this literature. Put differently, I would have liked more actual explanation and development of the arguments (vs. overly referring to the specialist literature).

(2) While the general argument (as far as I understand it) seems intuitive – i.e. (necessarily simplifying here) that psychedelic drugs (a) weaken people’s a priori beliefs, (b) strengthen, partly through insight experiences, the role of concurrent sensory input and thereby (c) facilitate belief change (whether for better or worse) – it seems also very (and perhaps too) broad and general, and I’m not sure if at this level of generality it would actually be falsifiable (certainly I would have liked to hear a lot more about typical or possible operationalisations of the core constructs, specific hypotheses that can be derived from the proposed framework, or concrete examples of beliefs that could be changed in specific directions under specific circumstances, etc.).

(3) Related to the point above about the paper relying (too) much on the neuropsychological literature, there are other theories that are clearly relevant for some of the arguments and theoretical assumptions made in the paper, and which might be useful sources for the further development of the ideas put forward. Specifically, the idea that low-level ‘prediction errors’ are referred to higher levels for possible resolution is very reminiscent of a long tradition of dual processing theories in cognition (e.g. heuristic vs. systematic processing, System 1 vs. System 2; see Evans, 2008, Ann. Rev. Psych.). Also, the idea that prediction errors may be ‘resolved’ by doubting the sensory input strongly reminds of Cognitive Dissonance Theory (Festinger, 1957) and similar cognitive consistency theories.

(4) Perhaps because of my lack of familiarity with this specific research area, I can’t really say what the contribution of the paper above and beyond existing theory is – it feels more like an extension of

the REBUS model the authors draw on in many parts of the paper. While this may be valuable, I'm not sure if it is the scope of contribution Nature Communications are looking for.

In summary, while covering interesting territory, this paper is a somewhat unsatisfying mix between a review and a theory development paper – for a review, it provides too little detail and depth, and as a theoretical paper the contribution doesn't seem substantial enough (certainly for this journal), both in terms of background covered (i.e., mainly limited to neuropsychology) and the further development of existing theory. With some regret, therefore, I won't recommend publication in Nature Communications.

Hartmut Blank

(I sign my reviews)

A minor terminological point

- Intro, definition of insight: it seems odd to say that e.g. “a proportion of insight moments are false” – in my understanding, the insight moment itself seems to be neither correct or false but a subjective phenomenological experience; the CONTENT of the insight (i.e. the idea or view arrived at) can be correct or false; i.e. there are two separate aspects of the meaning of ‘insight’ that are best kept separate?

Reviewer #3 (Remarks to the Author):

The authors discuss models on false insights and how these models might be applied to false beliefs induced by psychedelics. I appreciate the authors' contribution. The topic is certainly of interest and has been mostly neglected in the current literature. I'm not aware of any other work that connects existing models to these phenomena.

I'm not an expert in the models discussed but, as far as I can tell, the model proposed by the authors is comprehensible. Clinical implications are mentioned. However, most points raised are already common practice in psychedelic-assisted therapy. Therefore, the paper does not add much new in this regard.

I don't have any substantial concerns. However, I think that the manuscript could be improved by addressing the following points:

- General: A large portion of the manuscript reviews prior work on insights, memories, prediction etc. For me, this section was a bit hard to read. The first part of the manuscript was a bit confusing because many details of the models are discussed and it was unclear to me, how the model of the authors fits into all these details. Therefore, I think it would be beneficial if the authors would sketch their model in more detail at the beginning of the manuscript.

Compared with the rest of the manuscript, the section in which the authors discuss their model is relatively short (pages 19-20). I was wondering if all the concepts introduced in the first part of the manuscript are really necessary for the statements in this section. This section might benefit from more elaboration, e.g. from an example that illustrates the proposed formation of false beliefs.

- Abstract: The authors state that "the acute plasticity associated with psychedelics..." It seems unclear, what "plasticity" means in this context.

- Page 3: The sentence "However, restructuring hinges on prior information and beliefs, which are not always well-founded" seems to be incomplete

- Page 3: The statements of the last section seem to be already mentioned in the first section of the same page. I think the paper would be easier to read without this repetition.

- Page 7: The authors state that "positive feelings produced by insight problem-solving can solicit feelings of plausibility regarding content presented alongside insights". It seems clear, that psychedelics can also induce insights that are associated with negative feelings (maybe these insights might be particularly maladaptive). How does this fit into the model?

- Page 11: "We also describe another pathway to psychedelic-facilitated belief changes that does not rely on the assumptions of the REBUS model": I think it would be useful to state more clearly to which particular model the authors are referring. Related to this: on pages 13-15, it was not clear to me if this section refers to this particular model or the REBUS model. "In contrast, this memory systems account predicts..." It was unclear to me to which section this sentence is referring. Overall, it would be beneficial to make a clearer distinction between the models discussed.

- Page 12: I don't think that the statement "Recent work suggests psychedelics change metaphysical beliefs" is an argument for the REBUS model because there are no specific

connections between this observation and the REBUS model (i.e., the same statement could be used in the context of other models). This is also true for the statement "studies from clinical trials suggest that revision of self-related beliefs..."

- Page 19: What exactly are the clear implications for future studies? Is this referring to testability? If yes, how could the model be tested?

- Page 22: It is unclear to me how exactly sociocultural factors might fit into the model. Could you explain?

- Page 26: I do not understand how the framework of the mentioned "gentle touch" fits into the model. Could you explain?

REVIEWERS' EXPERTISE:

Reviewer #1: predictive processing

Reviewer #2: false memories/beliefs

Reviewer #3: psychedelics

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I enjoyed reading this timely manuscript, which offered a convincing account of the experience of insight associated with psychedelic experiences. I felt this was informative and well cited, including much of the relevant literature from both computational neuroscience and neuropharmacology. The conclusion regarding false insights is an important message with potentially very significant clinical implications.

There are very few comments I can make to improve this. However, there was one section that I felt conceptually weaker than the others and one other point the authors could think about expanding on.

Many thanks to the reviewer for taking the time to review our manuscript. We have outlined changes and revisions in non-bolded font.

The weakest point here is the story told about the Eureka Heuristic on page 17:

- "...results in a novel model, it solicits a prediction error..." - if the novel reduced model leads to greater prediction error, Bayesian model reduction would not have let us select this model in the first place. In other words, Bayesian model reduction is just a model comparison technique and almost by definition will end up selecting models with lower absolute prediction errors as these will be the best models to account for data.

Apologies this was not clear from what was written. What was meant here was that arriving at a novel model necessarily entails the realization that the previous model (that one subscribed to) was wrong. It is important to notice here that any update of any model could entail this implicit realization – we are often surprised by our own discoveries. Put differently, while implicit processes have arrived at a new understanding that *reduces* prediction error, at higher-order conscious levels of processing this new model is still a surprise (solicited by ascending prediction errors, consistent with neural evidence of insight emergence).

To be even more precise, in Laukkonen et al. (2023, page 5), Karl Friston advised us as follows: *"In Bayesian model reduction, there is no concurrent data to worry about and therefore the imperative for selecting novel models is to reduce complexity and provide a simpler account of the sensorium; in other words, 'join the dots', 'see common themes', etc. This imperative underwrites the ability to generalise to new data and precludes what statisticians call 'overfitting'. However, it also entails the loss of accuracy,*

in relation to previously observed data. It is this loss of accuracy that engenders a change in prediction errors that can be registered at high levels of hierarchical processing."

However, you are right that Bayesian reduction reduces absolute predictions errors across the system, and we should make this clearer. Thank you for pointing that out. We have tried to make this clear on page 20:

"The Eureka Heuristic proposes that when the implicit process of Bayesian reduction results in a novel (i.e., updated) model, it necessarily solicits a prediction error at a higher-order conscious level of abstraction."

and page 19-20

"A curious fact about insight is therefore that we seem to be able to implicitly arrive at a new perspective but not yet know it. The new perspective needs to make its way into conscious awareness and when it does, we may find it surprising (Webb et al., 2018). Insight therefore inherently demonstrates a kind of 'split' between unconscious and conscious processing, wherein one part of the system can surprise the other. This transfer of information needs to be accounted for in making sense of the emergence of insight."

"In other words, while Bayesian reduction inherently reduces global prediction errors across the system, a certain amount of time is required to share this information via ascending prediction errors."

- "...the ideas that have high expected precision..." - precision of what? I assume this high precision would come from the large difference in model evidence between the novel (reduced) model, and the full model it is derived from (i.e., the definitive model comparison)? If so, it would be useful to say so. However, if prediction error magnitude is a proxy for negative model evidence, then this implies less prediction error when we develop precise beliefs during model reduction and seems to contradict the earlier point as outlined above.

Apologies this was not made clear. We trust that the changes above also address this point by considering that there are prediction-error minimization processes happening in parallel at different levels of the processing hierarchy. New models at lower-levels that reduce PE (even global PE) still need to ascend the hierarchy as a PE with a certain expected precision (assigned by higher levels). In other words, insights arrived at implicitly (i.e., the *content* of the insight) need to make their way into conscious levels of processing via ascending PEs where they are assigned expected precision (i.e., the *feeling* of insight).

On page 21:

"Crucially, since many ideas can appear in the mind, it is only the ideas that have high expected precision (i.e., subjective confidence in the ideas)—thus feeling insightful—which are selected and meaningfully impact beliefs."

- "...the feeling of insight - the precise prediction error..." - the association of the feeling of

insight and precise prediction errors needs further unpacking. Intuitively, I would have thought the precise posterior belief we arrive at - and not the error that allows us to reach it - a more obvious analogue for the feeling of insight.

Thank you, this is an interesting point that may need to be unpacked further in future work. Note that that a prediction error (content/idea) assigned high precision (confidence/insight) does imply (via Bayesian inference) that some changes to posterior beliefs are likely to occur. But it's also likely that global posterior belief changes require a more prolonged process of negotiation throughout the mind. In classical insight literature this is known as the 'confirmation' phase of the creative process, which follows the discovery. That is, we may have an insight that feels true (like a scientific Eureka) but not change our beliefs until we've tested the idea. There may also be some gaps here that simply don't fit the active inference model perfectly, since it's not clear how an "idea"—as an entity we accept or reject—ought to be treated. Simulations are likely the next important step but is outside the scope of this project, unfortunately. Thank you for all the food for thought.

The other point that could possibly be improved is on page 18, which brings in an interesting dimension with the comments about dopamine.

There is potentially a missed opportunity here. Under active inference, dopamine is sometimes (e.g., doi:10.1162/NECO_a_00912, doi:10.1093/cercor/bhu159) seen as a measure of changes in precision in beliefs about the course of action we are following i.e., if we become more confident in what we are doing, there is a spike in dopamine (reward being a common example of something that convinces us we are on the right track).

Arguably, this could also relate to mental actions such as the comparisons between alternative hypotheses in Bayesian model reduction (i.e., 'choosing' between models). In addition to reward, information gain can be an important sign that we are on the right track, so can also increase our confidence in our choices, leading to a dopamine spike. If insight is read as a change in beliefs following the choice of a model that leads to a precise model comparison and belief update, there is a clear mechanistic link between dopamine and belief-updating which links directly to the idea of developing a very precise new belief.

I hope the above is helpful in updating this, already very strong, manuscript.

It's an insightful idea to think about mental actions in an analogous way to action policies in the context of dopamine and precision! In Shwartenbeck et al. (2014) they note "In other words, subjects have to infer both what they should do as well as how confident they are in their choices, where confidence may be encoded by dopaminergic firing." This is similar to what we are suggesting, that subjects have to infer what the idea or perspective is (the content) and how confident they are in that idea or perspective (the insight), and the latter is similarly driven by dopaminergic activity.

We have briefly tried to build this in on page 21, but will definitely think about building on it in more detail in future work:

“Crucially, since many ideas can appear in the mind, it is only the ideas that have high expected precision (i.e., subjective confidence in the ideas)—thus feeling insightful—which are selected and meaningfully impact beliefs. This is analogous to the way that an organism must infer both an action policy and (dopaminergic) confidence in it (Shwartenbeck et al., 2014; Friston et al., 2017). Similarly, organisms infer both the content of ideas as well as their (dopaminergic) insightfulness.”

We have now made more explicit efforts in highlighting testable hypotheses arising from our paper, and ways they might be tested:

On pages 30 and 31 we now say:

“Our model lends itself to novel empirical predictions. First, our model predicts that subjective veracity and perceived number of insights under psychedelics should linearly increase alongside dopaminergic release during the acute psychedelic phase. This could be tested by leveraging tools such as Positron Emission Tomography (PET) (see Hou, Tian, & Zhang, 2012), or blood sampling in the acute psychedelic phase. In the post-acute phase, researchers could ask how frequently and strongly participants experienced these insight moments during the acute phase. A second prediction here is that psychedelics should induce more false insights than during ordinary cognition, as psychedelics can increase the perceived (but not necessarily objective novelty or accuracy) of new ideas (Gandy et al., 2022; Mason et al., 2021; Tulver et al., 2023). To test this, future researchers could have participants perform tasks whilst under varied doses of psychedelics and see whether the number insights preceding a solution increase in number, and whether they are less accurate than during ordinary cognition. To test for strength of belief accuracy, researchers could administer a (for example) Brown Assessment of Beliefs Scale (Eisen et al., 1998) post acutely. The implication is that these beliefs should be stronger but less accurate when there was higher dopamine, and more (false) insights, arising during the psychedelic experience.”

Reviewer #2 (Remarks to the Author):

Review of “The power of insight: Psychedelics and the emergence of false beliefs” by H. T. McGovern et al., submitted to Nature Communications (Psychology)

General impression

This is a review paper that aims to add to existing theory towards understanding the impact of psychedelic drugs (used in psychotherapy settings) on beliefs, with a particular focus on the risk of creating false beliefs in the process. The paper draws mostly on the neuropsychological literature, which is not my area, so I see my role here mostly as that of a non-expert reader wishing to learn more about this topic, and I will try to judge the paper on the basis of its merits for a wider audience. Here are my core points:

We wish to thank the reviewer for their thoughtful comments. We have responded to each point in turn below, which we hope alleviates your (fair) concerns.

(1) The topic of this paper and the research reviewed certainly interesting, and the paper

is generally competently written and informative, but my feeling is that it is too narrowly aimed at an ‘insider’ neuropsychological audience (for a general journal such as Nature Communications). Much of the literature review that underpins the theoretical ideas put forward just refers to specialist literature without much description of the relevant findings, and as a result it is difficult to judge the merits of the proposed arguments without detailed knowledge of this literature. Put differently, I would have liked more actual explanation and development of the arguments (vs. overly referring to the specialist literature).

Thank you for the suggestion. In this piece we have attempted to cater to a general audience, whilst providing enough detail so that the basis for our more general arguments is appropriately justified to a more specialist audience. As such, the goal is to provide a mechanistic-level description of how psychedelically derived insights can solicit beliefs changes. We provide several intuitive examples to enable the reader to understand how such mechanisms solicit these proposed changes (see pages 5, 7, 9, 11, 12, 15, 18, 27, 28, 29). We have also provided a figure which outlines the process by which these belief changes takes place, which has not been proposed in the literature before. We suggest that the combination of technical detail and the providing of several examples, and a figure which outlines this process, is a strength of the paper, rather than a weakness. Nevertheless, we do understand your perspective here and hope that the fact that other reviewers have similar expertise in these specialist fields is a reassuring fact.

(2) While the general argument (as far as I understand it) seems intuitive – i.e. (necessarily simplifying here) that psychedelic drugs (a) weaken people’s a priori beliefs, (b) strengthen, partly through insight experiences, the role of concurrent sensory input and thereby (c) facilitate belief change (whether for better or worse) – it seems also very (and perhaps too) broad and general, and I’m not sure if at this level of generality it would actually be falsifiable (certainly I would have liked to hear a lot more about typical or possible operationalisations of the core constructs, specific hypotheses that can be derived from the proposed framework, or concrete examples of beliefs that could be changed in specific directions under specific circumstances, etc.).

As we note above, this paper has attempted to maximise accessibility to a general audience, whilst also providing enough technical detail so a reader will understand the basis for our mechanistic explanation (please see examples alluded to above). This may help address the point about being too general here.

We appreciate the suggestion of adding more specificity to our predictions. To amend this, we have now added a section on pages 30-31 outlining testable predictions and how they might be tested:

“Our model lends itself to novel empirical predictions. First, our model predicts that subjective veracity and perceived number of insights under psychedelics should linearly increase alongside dopaminergic release during the acute psychedelic phase. This could be tested by leveraging tools such as Positron Emission Tomography (PET) (see Hou, Tian, & Zhang, 2012), or blood sampling in the acute psychedelic phase. In the post-acute phase, researchers could ask how frequently and strongly participants experienced these insight

moments during the acute phase. A second prediction here is that psychedelics should induce more false insights than during ordinary cognition, as psychedelics can increase the perceived (but not necessarily objective novelty or accuracy) of new ideas (Gandy et al., 2022; Mason et al., 2021; Tulver et al., 2023). To test this, future researchers could have participants perform tasks whilst under varied doses of psychedelics and see whether the number insights preceding a solution increase in number, and whether they are less accurate than during ordinary cognition. To test for strength of belief accuracy, researchers could administer a (for example) Brown Assessment of Beliefs Scale (Eisen et al., 1998) post acutely. The implication is that these beliefs should be stronger but less accurate when there was higher dopamine, and more (false) insights, arising during the psychedelic experience.”

(3) Related to the point above about the paper relying (too) much on the neuropsychological literature, there are other theories that are clearly relevant for some of the arguments and theoretical assumptions made in the paper, and which might be useful sources for the further development of the ideas put forward. Specifically, the idea that low-level ‘prediction errors’ are referred to higher levels for possible resolution is very reminiscent of a long tradition of dual processing theories in cognition (e.g. heuristic vs. systematic processing, System 1 vs. System 2; see Evans, 2008, Ann. Rev. Psych.). Also, the idea that prediction errors may be ‘resolved’ by doubting the sensory input strongly reminds of Cognitive Dissonance Theory (Festinger, 1957) and similar cognitive consistency theories.

Thanks for pointing this out. We have provided a footnote on page 19 acknowledging the existence of system 1 and system 2 theories, noting that these ideas at least partially aligned with the framework suggested in our paper. Notably, one of the primary limitations of S1 and S2 in the context of our paper is that this model cannot account for insight moments (central to our model). Insights are on the one hand spontaneous (like S1) but they are also more often accurate (like S2). We also briefly explain why we adopt the predictive processing framework (page 20).

“We note that dual process theories provide a framework for understanding cognition as a binary of ‘conscious, deliberate, effortful’ and ‘unconscious, rapid, and largely involuntary’ thought (Evans, 2008). However, more recent work has expanded upon this concept and empirical findings (such as the phenomenology of insight occurring in traditionally analytic problems; Macchi & Bagassi, 2012) and identified the need for a more comprehensive view of cognition as occurring more as a hierarchy, with “system 1” and “system 2” effectively existing on opposite ends of a continuum encompassing all of conscious thought, unconscious judgement and decision-making, as well as even ‘lower-order processes’ such as perception and emotion” (Tulver et al., 2023). One of the key deviations that PP takes from these earlier theories is it emphasises these lower-level processes as occurring mostly prior to (outside of) conscious awareness. This is also a key component of our argument, as predictive processing can be applied to phenomena that have until now been thought of as completely “conscious” such as belief, insight, and even cognitive dissonance theory.”

We have additionally edited the section to make clear that it explicitly deals with the implicit/explicit processing distinction (page 19-20):

“It is well established that insight moments follow implicit processing (Hedne et al., 2016; Laukkonen et al., 2021; Laukkonen & Tangen, 2017; Metcalfe & Wiebe, 1987; Ovington et al., 2018; Bowden, 1997, also refer to Hattori, Sloman, & Orita, 2013; Grant & Spivey, 2003). A curious fact about insight is therefore that we seem to be able to implicitly arrive at a new perspective but not yet know it. The new perspective needs to make its way into conscious awareness and when it does, we may find it surprising (Webb et al., 2018). Insight therefore inherently demonstrates a kind of ‘split’ between unconscious and conscious processing, wherein one part of the system can surprise the other. This transfer of information needs to be accounted for in making sense of the emergence of insight.”

We also agree that our language could likely be adjusted to make these arguments more accessible to a wider audience. We believe that our revisions have achieved this goal, without compromising the scope of the current paper.

(4) Perhaps because of my lack of familiarity with this specific research area, I can’t really say what the contribution of the paper above and beyond existing theory is – it feels more like an extension of the REBUS model the authors draw on in many parts of the paper. While this may be valuable, I’m not sure if it is the scope of contribution Nature Communications are looking for.

Thank you for the feedback. You are correct that Nature Communications is aimed at a more generalist audience. However, this newer journal “Communications Psychology” is within the Nature portfolio but publishes from a wide range sub-disciplines on issues of importance to all psychologists.

In summary, while covering interesting territory, this paper is a somewhat unsatisfying mix between a review and a theory development paper – for a review, it provides too little detail and depth, and as a theoretical paper the contribution doesn’t seem substantial enough (certainly for this journal), both in terms of background covered (i.e., mainly limited to neuropsychology) and the further development of existing theory. With some regret, therefore, I won’t recommend publication in Nature Communications.

**Hartmut Blank
(I sign my reviews)**

A minor terminological point

- **Intro, definition of insight: it seems odd to say that e.g. “a proportion of insight moments are false” – in my understanding, the insight moment itself seems to be neither correct or false but a subjective phenomenological experience; the CONTENT of the insight (i.e. the idea or view arrived at) can be correct or false; i.e. there are two separate aspects of the meaning of ‘insight’ that are best kept separate?**

This is what was meant – the actual content of the insight being false. We have amended this as follows:

“While often correct, a proportion of insight moments are false (i.e., not objectively true – see end of manuscript)”

Reviewer #3 (Remarks to the Author):

The authors discuss models on false insights and how these models might be applied to false beliefs induced by psychedelics. I appreciate the authors' contribution. The topic is certainly of interest and has been mostly neglected in the current literature. I'm not aware of any other work that connects existing models to these phenomena.

I'm not an expert in the models discussed but, as far as I can tell, the model proposed by the authors is comprehensible. Clinical implications are mentioned. However, most points raised are already common practice in psychedelic-assisted therapy. Therefore, the paper does not add much new in this regard.

We would like to thank the reviewer for their helpful suggestions. We hope we can amend your concerns and feedback in what follows.

I don't have any substantial concerns. However, I think that the manuscript could be improved by addressing the following points:

- General: A large portion of the manuscript reviews prior work on insights, memories, prediction etc. For me, this section was a bit hard to read. The first part of the manuscript was a bit confusing because many details of the models are discussed and it was unclear to me, how the model of the authors fits into all these details. Therefore, I think it would be beneficial if the authors would sketch their model in more detail at the beginning of the manuscript.

Compared with the rest of the manuscript, the section in which the authors discuss their model is relatively short (pages 19-20). I was wondering if all the concepts introduced in the first part of the manuscript are really necessary for the statements in this section. This section might benefit from more elaboration, e.g. from an example that illustrates the proposed formation of false beliefs.

Thank you for pointing this out. We agree the manuscript would be easier to follow if the model was sketched out earlier in the piece to tie in the various strands discussed in the piece. On page 4 – we have offered a summary of the proposed model which sets the scene for subsequent sections:

“Our proposal is as follows: Psychedelics imbue a decreased ability to make sense of sensory data, leading to an increased number of insight moments and noetic feelings. Following the experience, the person may be left with a lack of detailed memory, but an increased noetic confidence in the insight moments encountered during the experience. Crucially, the increased quantity of insights and acute malleability may leave one vulnerable to empirically false, misleading, or maladaptive insights, alongside the prospect of obtaining valuable new

perspectives. This points to the importance of developing tools for integrating psychedelic-induced insights into beliefs conferring psychological benefit. We discuss the bases for each of these tenants below."

- Abstract: The authors state that "the acute plasticity associated with psychedelics..." It seems unclear, what "plasticity" means in this context.

We have removed this from the abstract- as we do not actually discuss plasticity in the manuscript (given it is not important for the arguments advanced in the paper).

- Page 3: The sentence "However, restructuring hinges on prior information and beliefs, which are not always well-founded" seems to be incomplete

We have changed this (page 3) to: *"However, restructuring hinges on prior information and beliefs, which can be misleading or ill-founded themselves."*

- Page 3: The statements of the last section seem to be already mentioned in the first section of the same page. I think the paper would be easier to read without this repetition.

Thank you for the suggestion. If it's okay with you, we have kept it as is. We feel it's important to highlight the fact that misleading contextual factors and insights can result in erroneous beliefs formed before and after the psychedelic experience. As such, we have kept a modified statement at this part of the manuscript outlining that insights are a foundational motivation for the paper.

- Page 7: The authors state that "positive feelings produced by insight problem-solving can solicit feelings of plausibility regarding content presented alongside insights". It seems clear, that psychedelics can also induce insights that are associated with negative feelings (maybe these insights might be particularly maladaptive). How does this fit into the model?

We have now included a footnote (page 7), noting that negatively valenced emotions are also associated with dopaminergic release in several regions. As such, it is plausible that both positive and negatively valenced insights can be imbued with an unfounded sense of plausibility.

"We note that negative feelings can also increase dopamine (Yin, 2019). Moreover, anhedonia (i.e., lack of positive or negative emotion), is inversely related to dopaminergic release (Belujon & Grace, 2017; Serreri, 2023). As such, dopaminergic release from insight moments could feasibility arise when insights are strongly negatively valenced, as well as strongly positively valenced."

- Page 11: "We also describe another pathway to psychedelic-facilitated belief changes that does not rely on the assumptions of the REBUS model": I think it would be useful to state more clearly to which particular model the authors are referring. Related to this: on

pages 13-15, it was not clear to me if this section refers to this particular model or the REBUS model. "In contrast, this memory systems account predicts..." It was unclear to me to which section this sentence is referring. Overall, it would be beneficial to make a clearer distinction between the models discussed.

Apologies this was not clear. On page 13 we have amended this to make clear we are talking about a path to belief revision which does rely on REBUS. We then broadcast the fact we will talk about a pathway to revision which does not rely on the assumptions of REBUS.

"Below we start with outlining an influential theory of how the brain may be more amenable to belief change under psychedelics known as Relaxed Beliefs Under pSychedelics or REBUS (Carhart-Harris & Friston, 2019). We discuss how REBUS effects from psychedelics can result in belief changes. Following the description of REBUS and how it can drive belief change, we then describe an alternative pathway to psychedelic-facilitated belief change that does not rely on the assumptions of REBUS."

On page 15:

"Outside of the active inference or computational frameworks, psychedelics may impact beliefs via effects on fluency and relative weighting of hippocampal and cortically dependent memories. This pathway to belief revision, which we will term a 'memory systems account' does not preclude REBUS effects, but we highlight this account since there may be differing predictions on the specific brain-based substrates to belief changes. We offer ways to test this account against REBUS effects in the final section of this piece."

- Page 12: I don't think that the statement "Recent work suggests psychedelics change metaphysical beliefs" is an argument for the REBUS model because there are no specific connections between this observation and the REBUS model (i.e., the same statement could be used in the context of other models). This is also true for the statement "studies from clinical trials suggest that revision of self-related beliefs..."

Thank you for pointing this out, we have now made clear that the fact that psychedelics can facilitate these revisions to beliefs, this does not mean that such belief changes evidence only the REBUS hypothesis (page 15).

"After the acute psychedelic experience, there are documented changes to metaphysical beliefs, particularly away from a physicalist worldview (we note this does not provide evidence exclusively for REBUS- but just that psychedelics can seemingly change beliefs) (Nayak et al., 2023; Nayak & Griffiths, 2022; Timmerman et al., 2021). Finally, qualitative studies from clinical trials suggest that revision of self-related beliefs (arising from REBUS processes) may underpin positive psychological changes (Belser et al., 2017; Noorani et al., 2018; Watts et al., 2017)."

- Page 19: What exactly are the clear implications for future studies? Is this referring to testability? If yes, how could the model be tested?

Thank you for pointing this out. We have now added a section outlining testable hypotheses arising from our model (page 30-31).

- Page 22: It is unclear to me how exactly sociocultural factors might fit into the model. Could you explain?

This simply refers to factors of set and setting. We have removed the word 'sociocultural'.

- Page 26: I do not understand how the framework of the mentioned "gentle touch" fits into the model. Could you explain?

Apologies this was not clear. We have now added a passage at the end of page 30 outlining the correspondence between the gentle touch framework and our model (particularly Figure 2 which discusses the epistemic facets of psychedelically derived insights):

"A recently developed framework aimed at addressing issues of psychedelic-induced false insights proposed the fostering of a 'gentle touch' for revelations occurring during psychedelic sessions. In this framework (deemed 'psychedelic apprenticeship'), the relational processes (e.g., therapeutic interventions performed by a therapist) occurring before, during, and after the psychedelic session could serve as a scaffold for users' to hold novel insights lightly (Timmerman et al., 2022). These relational processes could be seen as a form of 'thinking through other minds' or 'cultural affordances' (Veissière et al., 2019), whereby an experienced facilitator or therapist can aid in the modulation of the users' precision weighting of newly acquired insights or beliefs during psychedelic therapy. With respect to our model, the gentle touch framework, alongside a rubric for making sense of the adaptiveness, veracity, and falsifiability of psychedelically derived insights (Figure 2), could offer clinicians a framework for integrating psychedelic insights in a clinically useful way. Below, we divide discussion of our model into research and clinical implications, offering novel hypotheses and considerations for clinicians involved in integrating psychedelic insights."

19th Apr 24

Dear Hugh,

Your Perspective titled "The power of insight: Psychedelics and the emergence of false beliefs" has now been seen by our referees, whose comments appear below.

I am pleased to say that I hope to be able to accept your paper in principle for publication, on the condition that remaining editorial and referee concerns are addressed in the next revision.

The referees' comments are appended below and I include edits and explanations in the attached manuscript file. Based on my re-reading of your work and in light of Reviewer #2's comments, I ask you to provide a better introduction and visual explanation of the neuropharmacology/neuroanatomy of hallucinogenic action on the brain (more details are enclosed in the manuscript). This means that your revision will likely be subject to a further round of peer review, to ensure accuracy in that description, though without seeking additional feedback on more general points.

As you will see, there are two further domains in which we require revisions; first, similarly to Reviewer #1, I would like to see greater clarity in the references to active inference and a clearer introduction to the Bayesian terminology (if this is maintained). Second, I ask that you provide a more explicit overview of the differences between the REBUS model and your account; to achieve this, we strongly recommend that you make use of display items, as detailed in the manuscript.

EDITORIAL REQUESTS:

* Please review the changes in the attached copy of your manuscript, which has been edited for style, and address the comments and queries I have added. If using Word, please use the 'track changes' feature to make the process of editorial reevaluation more efficient.

* Please check whether your manuscript contains third-party images, such as figures from the literature, stock photos, or clip art. If any of the display items in your manuscript (figures, tables, boxes or movies) include images that are the same as, or are adaptations of, previously published images, please fill in the Third Party Rights Table, and return to us when you submit your revised manuscript. This information will enable us to obtain the necessary rights to re-use such material. If we are unable to obtain the necessary rights to use or adapt any of the material that you wish to use, we will contact you to discuss alternative options.

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*If you have not done so already, please alert me to any related manuscripts from your group that are under consideration or in press at other journals, or are being written up for submission to other journals (see www.nature.com/authors/editorial_policies/duplicate.html for details).

FORMATTING GUIDELINES:

You will find a complete list of formatting requirements following this link:

<https://www.nature.com/documents/commsj-style-formatting-checklist-review-perspective.pdf>

Please use the checklist to prepare your manuscript for final submission. In the following, I also highlight some issues of particular importance.

*Footnotes

Footnotes are not used.

* References

References appear as superscript Arabic numerals, in order of mention. The reference list mentions references in the numerical order in which they are mentioned in the main text. If a reference is cited more than once, the same number is used throughout the text and the reference receives a single entry in the reference list.

We ask that you select the most significant 5–10% of references in your list for highlighting, and add a single sentence in bold after each of these references to describe the main result and its significance.

Only papers that have been published or accepted by a named publication should be in the reference list (preprints and citations of datasets are also permitted). Unpublished/Submitted research should not be included in the reference list; it should only be mentioned briefly and

parenthetically in the main text. Note that no major arguments should rely on unpublished research.

Published conference abstracts and URLs for web sites should be cited parenthetically in the text, not in the reference list.

I hope to hear from you within four to eight weeks; please let us know if the process may take longer.

Sincerely

Marike

Marike Schiffer, PhD

Chief Editor

Communications Psychology

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I am grateful to the authors for their responses to my previous comments. Generally, these have resolved my concerns.

I now understand the authors' statements about prediction errors during Bayesian model reduction. This is probably ok at a heuristic level. However, the more general comment I think the authors are trying to get across is that model reduction and insight happen when there is a definitive model comparison that causes a large change from prior to posterior in the distribution over models.

Of note, the categorical distribution over models is not the same as the Gaussian distributions more associated with many accounts of predictive coding and prediction errors, but the large

update in the categorical beliefs can be seen heuristically as being driven by a prediction error. It might be more accurate to frame this as Bayesian surprise in the Itti and Baldi (2009) sense.

Also of note is that loss of accuracy Karl has communicated to you may not be communicated to higher hierarchical levels at all. I imagine if pressed on this he would agree that the point is that a loss of accuracy is tolerated in model comparison if the associated reduction in complexity is at least as great. This does not mean that the loss of accuracy and increase in prediction errors at lower levels are propagated to higher levels, as this loss is mitigated by the complexity reduction. Only the combined accuracy minus complexity of each model would be propagated upwards and by definition this must be greater than it was prior to model reduction.

Although I would not object to the paper being published in its current form, I also feel there is potential to improve on some of the new additions to make them more precise and meaningful.

1. "necessarily solicits a prediction error at a higher-order

conscious level of abstraction" - this is not a very scientific statement. Perhaps it needs to be followed by "under the hypothesis that precisely held beliefs enter consciousness". It is not obvious otherwise why these would be conscious.

2. "higher-order" is also unnecessarily vague. It is clear from the authors' response that it is beliefs about plausibilities of a set of models that are updated (i.e., one reduced model goes from being similarly to much more plausible than the others).

3. Framed in the above way, the authors' difficulty in being specific about what precision is assigned to is resolved if they simply talk about a precise posterior distribution over models.

Reviewer #2 (Remarks to the Author):

Review of 467_1 "The power of insight: Psychedelics and the emergence of false beliefs" by H. T. McGovern et al., re-submitted to Nature Communications (Psychology)

General impression

I reviewed this paper before and was somewhat sceptical about its contribution to the literature and its intelligibility for a wider audience. Having looked at the changes in the revised version, I don't think a lot has changed in this respect, so I think most of the points I made in my first review still hold. True, there are some more examples now, but I was hoping for more of an introduction of key neuropsychological terms where needed (for non-neuropsychologists). I also acknowledge the nod to dual process models and even to cognitive dissonance theory (even though cognitive dissonance theory is not a phenomenon like belief or insight; that should be reworded), as well as the explicit outlining of predictions from the framework.

Still, I am mostly not sure about the overall contribution (for a journal in the Nature Communications family); my feelings is that there is not much theoretical development over and above the existing REBUS framework, and not enough systematic coverage of previous research for a review paper. Please see my initial review for further detail.

Having said this, I reiterate that I'm not an expert in this area of neuropsychology, and others may see a contribution where I don't – and perhaps it is just a matter of presentation to make this contribution clearer.

Hartmut Blank

(I sign my reviews)

Reviewer #3 (Remarks to the Author):

Thank you for the revision. I recommend publication.

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I am grateful to the authors for their responses to my previous comments. Generally, these have resolved my concerns.

I now understand the authors' statements about prediction errors during Bayesian model reduction. This is probably ok at a heuristic level. However, the more general comment I think the authors are trying to get across is that model reduction and insight happen when there is a definitive model comparison that causes a large change from prior to posterior in the distribution over models.

Of note, the categorical distribution over models is not the same as the Gaussian distributions more associated with many accounts of predictive coding and prediction errors, but the large update in the categorical beliefs can be seen heuristically as being driven by a prediction error. It might be more accurate to frame this as Bayesian surprise in the Itti and Baldi (2009) sense.

Also of note is that loss of accuracy Karl has communicated to you may not be communicated to higher hierarchical levels at all. I imagine if pressed on this he would agree that the point is that a loss of accuracy is tolerated in model comparison if the associated reduction in complexity is at least as great. This does not mean that the loss of accuracy and increase in prediction errors at lower levels are propagated to higher levels, as this loss is mitigated by the complexity reduction. Only the combined accuracy minus complexity of each model would be propagated upwards and by definition this must be greater than it was prior to model reduction.

Thank you for pointing this out. We have now added a slight caveat to page 11 to make this distinction clear, and just to point out that it also could be possible that lower level prediction errors are not necessarily propagated to higher levels if the loss in accuracy is accompanied by complexity reduction.

Although I would not object to the paper being published in its current form, I also feel there is potential to improve on some of the new additions to make them more precise and meaningful.

1. "necessarily solicits a prediction error at a higher-order conscious level of abstraction" - this is not a very scientific statement. Perhaps it needs to be followed by "under the hypothesis that precisely held beliefs enter consciousness". It is not obvious otherwise why these would be conscious.

Thank you for pointing this out, we have now added this caveat on page 20.

2. "higher-order" is also unnecessarily vague. It is clear from the authors' response that it is beliefs about plausibilities of a set of models that are updated (i.e., one reduced model goes from being similarly to much more plausible than the others).

We have added this to page 14 as follows: *"By relaxing the DMN's constraints on the rest of the brain, the brain's hierarchy of information processing is thought to be "flattened" or less*

controlled and constrained by higher-order abstraction and 'freer' to change according to new input (note that by higher order, and in a more technical sense, we are referring to beliefs about plausibility's of a set of models that are updated)".

3. Framed in the above way, the authors' difficulty in being specific about what precision is assigned to is resolved if they simply talk about a precise posterior distribution over models.

Thanks, we have amended the text throughout to note we are referring to the fact that we are talking about precise posterior distributions over models. We note in particular on page 8: Here, we are referring to the fact that this process allows the agent to arrive a precise posterior distribution over models, allowing the agent to optimally navigate its sensory landscape and thus their environment.

Thank you again to the reviewer for the opportunity to clarify.

Reviewer #2 (Remarks to the Author):

Review of 467_1 "The power of insight: Psychedelics and the emergence of false beliefs" by H. T. McGovern et al., re-submitted to Nature Communications (Psychology)

General impression

I reviewed this paper before and was somewhat sceptical about its contribution to the literature and its intelligibility for a wider audience. Having looked at the changes in the revised version, I don't think a lot has changed in this respect, so I think most of the points I made in my first review still hold. True, there are some more examples now, but I was hoping for more of an introduction of key neuropsychological terms where needed (for non-neuropsychologists). I also acknowledge the nod to dual process models and even to cognitive dissonance theory (even though cognitive dissonance theory is not a phenomenon like belief or insight; that should be reworded), as well as the explicit outlining of predictions from the framework. Still, I am mostly not sure about the overall contribution (for a journal in the Nature Communications family); my feelings is that there is not much theoretical development over and above the existing REBUS framework, and not enough systematic coverage of previous research for a review paper. Please see my initial review for further detail.

Having said this, I reiterate that I'm not an expert in this area of neuropsychology, and others may see a contribution where I don't – and perhaps it is just a matter of presentation to make this contribution clearer.

Hartmut Blank
(I sign my reviews)

Thanks you for the feedback. As stated above in our response to the editor, we agree that our manuscript indeed required editing to make the presentation of our model more streamlined by providing relevant background information in an order appropriate to the

level of complexity as the paper progresses. We have also added a table to outline how our model departs from REBUS as well as the relevant components of the other theories discussed. Namely, whilst REBUS is a model of psychedelic brain action, ours is on belief change arising from psychedelic brain action. Such a process arises in part from REBUS effects, and by altered memory processes occurring post-acutely. We believe these changes have significantly improved the flow and readability of our paper, making it more accessible to a wider audience without removing any crucial information or departing from our thesis.

Reviewer #3 (Remarks to the Author):

Thank you for the revision. I recommend publication.

Thank you again to the reviewer for their helpful feedback.

16th Jul 24

Dear Hugh,

Following my editorial evaluation of your revision of the submission titled "The Power of Insight: An Integrated Theory of False Insights and Beliefs Under Psychedelics" I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Psychology under the open access CC BY license (Creative Commons Attribution v4.0 International License).

You will need to undertake the following edits before acceptance. First, please amend the title to align with our guidelines. We do not permit the title:subtitle format and we don't allow punctuation. I suggest: "An Integrated Theory of False Insights and Beliefs Under Psychedelics" or similar.

Second, please review your use of the model labels REBUS and FIBUS. The introduction of FIBUS makes sense; in the final paragraphs, you revert to the use of REBUS, and it is not entirely clear whether you are referring to the original models, to both, or whether this is a misnomer.

In the attached editorial request table, you will find a list of formatting requests and reminders, and an editorial summary. Please respond to each request and also provide feedback on whether you agree with the editorial summary.

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** Figures

Please remove all figures from the main text and upload them individually, one figure per file. To ensure the swift processing of your paper please provide the highest quality, vector format, versions of your images (.ai, .eps, .psd) where available. Text and labelling should be in a separate layer to enable editing during the production process. If vector files are not available then please supply the figures in whichever format they were compiled in and not saved as flat .jpeg or .TIFF files. If your artwork contains any photographic images, please ensure these are at least 300 dpi.

* Figures should be simple and informative — multi-part figures are best avoided. Boxes should occupy no more than half a page in the PDF (less than 500 words) and may include a figure.

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* Production-quality versions of all figures, supplied as separate files. Photographic images should be 300 dpi in RGB format (.jpg, TIFF or native Photoshop format) and any labels/scale bars included in a separate layer from the image. Line art, graphs and schemes should be vector format (.ai, .eps, .pdf); Adobe Illustrator files are preferred and will minimize production time. Any chemical structures or schemes contained within figures should additionally be supplied as separate Chemdraw (.cdx) files.

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We hope to hear from you within two weeks; please let us know if the process may take longer.

Best wishes,

Marika