

Interactive comment on “Responses to severe weather warnings and affective decision-making” by Philippe Weyrich et al.

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Introduction : Comment #1: In the introduction, I appreciate that the authors try to draw on relevant theory from psychology and decision sciences. However, how the introduction stands now, it is long and in some parts confusing. I'd recommend to focus more on the applied value of this applied study and the factors varied in them, and what it adds to the current literature. The cited work of Casteel (2016, for example) or Andrea Taylor, University of Leeds, UK (2019) are excellent examples for how this could be done. If it is necessary to draw on several theories from cognitive and decision sciences, please clarify what are the precise predictions regarding the main independent variables explored here, based on these theories. This should be either based on theories, previous findings, or ideally integrate both. Regarding theory, I'd suggest narrow

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the theoretical focus and delineate precise predictions; and then pick these up again in the discussion and discuss whether and how they have been met, and why. If however, the aim of the authors is to review several theories rather than focusing on only one, please clarify why and how those were selected and relevant for IBW (for example construal level theory would then currently be missing).

Response: We are somewhat confused by this comment, as indeed we attempt to do in the introduction exactly what the reviewer suggest. In the first paragraph we focus on the question of whether more information leads to improved decisions, with a particular focus on the additional information contained in impact-based warnings (IBW) over standard warnings. Our question is thus whether it is worthwhile to include the added information that IBW contain. We then describe two different theoretical predictions, based on different cognitive pathways leading to human behaviour. When the analytic pathway is engaged, the rational actor model suggest that IBW will necessarily lead to improved decision-making. Where the affective pathway is engaged, it is unclear whether IBW will trigger improved decision-making. Additionally, we suggest that the method of investigating the response to IBW is important, because some methods (e.g. hypothetical surveys) make it more likely that the analytic pathway will be engaged, and these methods would then have an inherent bias in favour of showing IBWs to be superior. To capture the likely behaviour in real-world situations, we argue, it is necessary to replicate the conditions that could lead the affective pathway to be engaged. In this case, it is unclear whether IBWs will outperform SWs, and indeed the results may be highly context specific. Because we have tried to capture these points in the introduction, we are unclear how to revise it to meet the reviewer's request.

Comment #2: On a side note, I'd like to put forward that a dual systems-approach is widely accepted is not correct – the 'ecological rationality' approach (see reference to Gigerenzer & Gaissmaier, 2011 below) is an entire interdisciplinary research paradigm in psychology and decision sciences (in social sciences more broadly, a much wider range of theories exist). It contrasts the dual system-approach. In my view, the inherent

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problem of the latter is that it doesn't clarify, generally and also in potentially threatening situations where people may rely on weather warnings, what a 'rational' and therefore 'correct' decision actually is. Extreme weather leads to situations characterised by high uncertainty, where not all information is known and a 'rational' decision, based on all available information, is impossible to make. Also, quick and intuitive decisions could potentially be very helpful and adaptive in such uncertain emergency situations, if performed in the right decision context. While the introduction of the current paper is not the place to reflect this entire debate, the point I wish to illustrate is that the most important existing theories and predictions based on these need to be selected and described more carefully.

Response: We thank the reviewer for this side note, and agree that it does not require a change to the manuscript.

Comment #3: Having a more clear outline in the introduction would also clarify from an applied viewpoint what type of behavioural response is actually adequate in the authors' view (and 'rational', though I'd recommend to skip this term overall) – this may be based on general recommendations by weather warning services, or insurances, or Table 2? If there is an adequate response, please describe it in the introduction. If not, that also needs to be clear, because then there isn't such a thing as a rational decision in this context. A more applied focus as mentioned above would help in clarifying this.

Response: As with comment 1, we are challenged as to how to modify the manuscript to address this concern, as it is one that we have tried to address in our original submission. It is clear that in most situations, there is not one "correct" or "rational" response. In our study, our dependent variable is whether people attempt to change their behaviour in response to a piece of information. While we make no claim that a change of behaviour is a "correct" response for all people, it is also the case that a change in behaviour suggests that people both took note of the information and chose the change their behaviour, whereas not changing behaviour indicates either (a) that they failed to take note of the information, or (b) that they took note of it and decided actively not to

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change their behaviour. Observing a greater rate of behavioural change would suggest that at least some people who would have otherwise fallen into group (a) did take note of the information.

Comment #4: This would also help re-structuring the abstract according to short background, re- search question, design, method and sample, main findings and a short comment on results. Currently, it doesn't precisely reflect what was done here – and lacks for ex- ample the impact-based weather warning manipulation, or introduces the term 'stress' which doesn't re-appear prominently anywhere else (as far as I can see). Response: We thank the reviewer for this comment, and agree that the abstract could be restructured to better reflect the overall content of the paper. The new abstract is as follows: "When public agencies provide information provision to help people make better decisions, they often face the choice between parsimony and completeness. For weather services warning people of high-impact weather events, this choice is between offering standard warning (SWs) only of the weather event itself , such as wind-speed, or also describing the likely impacts (so called impact-based warnings, IBWs). Previous studies have shown IBWs to lead to a greater behavioural response. These studies, however, have relied on surveys describing hypothetical weather events; given that participants did not feel threatened, they may have been more likely to process the warning slowly and analytically, which could bias the results towards finding a greater response to the IBWs. In this study, we conducted a field experiment involving actual and potentially threatening weather events, where there was variance with respect to the time interval between the warning and the forecasted event, and where we randomly assigned participants to receive SWs or IBWs. We observe that shorter time intervals led to a greater behavioural response, suggesting that fear of an imminent threat to be an important factor motivating behaviour. We observe that IBWs did not lead to greater rates of behavioural change than SWs, suggesting that where fear is a driving factor, the additional information in IBWs may be of little importance. We note that our findings are highly contextualized, but we call into question the prevailing belief that IBWs and necessarily more helpful than SWs."

Methods: Comment #5: #5 P.5 l.10 says “we asked participants whether the weather described in the warning would pose a risk to them and whether it would affect them in carrying out their usual activities (e.g., commuting, working, shopping etc.). If they answered yes, they continued with the survey” – wouldn’t this exclusion of people who don’t change, be an explanation of why no effects were observed – if for example more people didn’t react to a SW, compared to a IBW?

Response: These questions that the Reviewer is referring to, helped to filter out those people who were not affected by the warning at all, because they were for instance the whole day at work and thus the wind warning from 2 to 5 pm would not be relevant for them at all. In the following we asked questions whether people changed behaviour as we also explain in the manuscript. So, there was a slight misunderstanding: we did not exclude people who did not react to the warning, but only those for which the warning was not relevant in the first place!

Comment #6: Page 5 l.34 reports that the regression is conducted on behaviour, but the regression table header indicates that it predicts behaviour ‘change’ – please clarify whether it predicted absolute behaviors or a difference score reflecting change (I don’t think it did the latter).

Response: The Reviewer is right it is about this comment on behaviour and thus we adapted the language in the Table heading “. . .with behaviour as dependent variable”.

Comment #7: Table 2: Please report a measure of dispersion for age, and a range.

Response: We included a measure of dispersion (St. dev.) and a range.

Comment #8: Figure 2: I think this is not about likelihood (as the header implies) but a continuous score measuring actual behavior. I think the figure header should accordingly be something like “Mean self-reported behaviour in response to two warning types, . . .”. Please avoid the use of acronyms or explain them again in the caption to allow the figure to stand on its’ own. I’d also prefer to see the full scale that was presented to participants on the y-axis; to get an idea about effect magnitude. Please do

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also apply these points to all other figures where appropriate.

Response: The Reviewer is absolutely right with respect to the measuring scale. We also avoid acronyms. Thus, we changed the header into “Mean self-reported behaviour in response to two warning types (standard and impact-based) and the three lead times (no, short and long), respectively the two severity levels (moderate and severe).” However, we decided to not present the full scale on the y-axis as this would deteriorate the readability of the figure. Also, the scale is explained in the caption.

Comment #9: Table 3 and overall results section: The use of standard significance levels and *** has been widely criticized in psychology and decision sciences, and other fields (see for example <https://www.nature.com/articles/d41586-019-00857-9> or <https://journals.sagepub.com/doi/abs/10.1177/0959354302126005> for some context). Albeit still widely in use unfortunately, I'd thus refrain from marking results with *** and in bold, dependent on these levels; and follow reporting standards outlined in this literature; including for example confidence intervals where appropriate for all study results.

Response: We thank the Reviewer very much for her comment. As the reviewer highlights, the use of standard significance levels is widely used in research, we believe that this approach is absolutely fine even though there exists some criticism. We also included error bars in the figures and indicate standardized coefficients in the regression analysis.

Discussion: Comment #10: As the authors note in line 27 on page 7, the study is based on self-reports and a self-selected sample. Please acknowledge the dearth of literature on samples and self-reports in psychology and social sciences more generally where this has been criticized, and provide a recommendation for future studies.

Response: We are happy to include this note. We have revised the relevant text as follows, following the reviewer's suggestion: “Moreover, participants were self-selected as they had downloaded the weather app and decided whether or not to participate

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in the survey. This may indicate higher levels of weather awareness and knowledge, which could also be another explanation for the lack of effect of warning type. There is a dearth of literature on the effects of such self-selection in social science research, though ideally researchers would design field experiments where self-selection is not present.”

Technical corrections #11 - #17 Will be addressed.

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