Author's response to reviews

Title: Perceptual influences on self-protective behavior for West Nile virus, A survey in Colorado, USA.

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Author's response to reviews: see over
Reviewer: Albert

1. How do aims 1 and 2 differ? (1. Compare the effect that cognitive, affective, ecological, and proximity risk perception models have on self-protective behavior for WNv. 2. Evaluate how health beliefs affect self-protective behavior for WNv.)

Another reviewer also comments that Aims 1 and 2 are strongly similar. They have been combined into an exploratory aim. The purpose is to examine new adjunct concepts that have been presented in the literature with respect to self-protective behavior for WNv and consider a hybridized treatment of the HBM.

2. "It was therefore feasible to proportionally sample within census tracts and have all study participants located in near-by areas of equivalent historic and current WNv." Not clear if sampling was meant to proportionately represent residents from areas according to the spatial distribution of cases.

Clarified p. 5 line 14: Sampling was proportional for ethnicity, and as the sample areas were in proximity the overall degree of mosquito activity was similar. That might not be the case, for example, if some areas were within an urban zone while others outside in a agricultural area.

3. "Additive indices were created for each of the five risk perception measures." Reliability was low for risk cognition and risk proximity. Could this affect findings? (For example, "The relative lack of associations between the HBM variables and cognitive risk perception is interesting...")

Added to limitations.

4. The authors mention proximity as "how near and how frequently cases are occurring, as well as the degree to which people similar in age and identity are being affected," but only appear to have asked about spatial proximity, not similarity. Reason?

Description of the measure has been revised for better accuracy: the degree of similarity in age, etc. was not assessed in the original study we replicated but were described inadvertently.

5. The full regression model 4 shows that traditional HBM indicators seem most important: susceptibility, benefit, cues (though not severity). Affective measures seem less important. This is in contrast to results from the stepwise block model, which gives more importance to affective elements. Which is the better model?

The treatment of the regression models has been slightly modified to better focus on the potential effects of the additional risk perception variables, with the two "newer" concepts being entered in separate blocks for separate assessment before being included with the HBM items. Important change is that ethnicity has been dropped from that model. The results of the first model are then re-expressed in the second regression that used stepwise entry for the predictor variables and then forced entry of ethnicity in a second block. This allows the assessment of the hybrid model separately, then the assessment of ethnicity superficially. In terms of supporting one model over the other we now state that the second, trimmed model is superior based on its relative simplicity and its inclusion of ethnicity. This is added to the discussion.
Reviewer: Hamer

Overall it would help to add a statistical analysis section to the end of the methods section rather than having it embedded in the results/discussion. This stats section can be more descriptive with what analyses were performed. My specific comment below outline one example of confusion. Also, it would be nice to be clear with regard to what the dependent and independent variables were in the regression analyses.

_A brief section discussing the statistical analysis has been added._

The last paragraph of the manuscript outlines some issues with the study. I’m not an expert at human dimensions nor analyzing results from surveys but are the authors using standard techniques considering that less than half of the surveys were returned? To me, this appears to be a low response rate and while judging these types of studies, I am always concerned about potential biases introduced by this. Was there something unique with the half that responded versus the half that didn’t? Are there standard quantitative techniques to account for these issues in the social science field? Having the statistical analysis section in the methods would give the authors a chance to build the case justifying their statistical approach.

_A 50% response rate for a self-administered mail survey is actually considered good, at least acceptable. Nonetheless it needs to be addressed, as it is a limitation. We added comparisons between key demographic values in the sample and from Census figures to illuminate the differences, and have added some discussion of this to the limitations._

I’ll note that the results, discussion, and conclusions are heavy with social science jargon and it is hard for me to follow the logic and conclusions. I’m not sure how to rectify this other than being a little more careful with the use of these terms that are “vague”. For example, when reading the first paragraph of the conclusions, I get almost nothing out of it.

_An effort was made to clarify the discussion points and remove as much jargon as possible, although some terms that are tied to the theories are difficult to revise The methods, results, and discussion sections have been reorganized under subheads to provide better structure and clarity._

I have one personal observation that might present an issue with interpreting the results of the current study. I have noticed that public concern for a health hazard like WNV is often driven by local media coverage. I’m amazed at how the media in some communities creates hysteria and heated debate over mosquito control during a WNV epidemic while in other communities, with similar levels of WNV transmission, the media doesn’t pay attention to it and the public is mostly unaware that it is occurring. Are the authors able to discuss this given that their survey included “cues”? I see in the 4th model the cues had a significant positive association with self-protective behavior so is this an example of media influencing public behavior and their practices to reduce exposure to WNV?

_This is an interesting question, but one which we can only partially answer. Additional analysis was added in looking at the cues (page 7 line 20). By breaking the cues into mediated and personal measures it is now shown that respondents reported significantly more exposure to mediated cues than personal. However, we_
did not monitor the media environment prior to or during the study period and do not have specific media exposure measures that would allow us to examine a direct effect. It’s also well established that media don’t affect attitudes or behaviors in a direct fashion, but rather interact with and flow through interpersonal sources. Our study was not designed to examine this phenomenon. A note on this has been added to the conclusions.

Specific comments

Pg 2 Ln 5-9. The “background” of the abstract needs to mention that this is in regards to “human” behavior. Birds have evolved several behavioral mechanisms to reduce mosquito biting and exposure to mosquito-borne pathogens. This is especially needed since the manuscript title does not specify this.

Specified.

Pg. 2 Ln 20-21. I would think these results are also relevant to vector-borne diseases in general (not just mosquito-borne).

Good observation, this was revised as vectored diseases generally.

Pg. 3 Ln 29. Doesn’t this Zielinski-Gutierrez paper (two papers?) need a citation number to link it to the full reference? If they are 7, and 11, they need to be cited earlier for clarity.

Citations moved up and repositioned.

Pg. 5 Ln 24-27. The methods don’t describe these stats so it is tricky to interpret them. Given that only 49% of respondents returned the survey, was there any stats done to see how these various demographic factors influenced the response? Perhaps that is what this initial paragraph reports?

We added comparisons between key demographic values in the sample and from Census figures to illuminate the differences, and have added some discussion of this to the limitations.

Pg. 9 Ln. 10-17. This final conclusion paragraph pointing out the weaknesses of the study looks out of place. Is there any way to discuss these shortcomings earlier?

The discussion section was reorganized using subheads, including limitations. It’s common for articles in this journal to conclude the discussion with limitations, then present conclusions.

The table legends are very brief. Is this a journal requirement or could the authors fill in more information in the legend so it is easier to interpret them without needing to refer to the text? For example, the binomial coefficients (e.g. “sex”) in the regression models (Table 4) do not clarify if they refer to male or female so it is difficult to interpret the direction of the effect.

Tables have been reformatted into journal style.
Reviewer: Henry

What is not clear is the rationale and justification for including the individual risk perception perspectives (cognitive, affective, ecology, proximity) with the HBM in a single model that also includes ethnicity. The HBM itself contains components that reflect affective, cognitive, ecology and proximity risk perception and including them in the same model with these perceptions as independent variables may not be valid. There should be an explanation and justification of why these were combined.

Another reviewer also comments that Aims 1 and 2 are strongly similar. They have been combined into an exploratory aim. The purpose is to examine new adjunct concepts that have been presented in the literature with respect to self-protective behavior for WNv and consider a hybridized treatment of the HBM.

We must partially disagree with the reviewer concerning the adjunct concepts already being part of the HBM. While appropriately related within the broad domain of risk perception the formulation of proximity-based risk perception, ecological-based risk perception, and cognitive risk perception are reasonably distinct from individual assessments of susceptibility and disease severity. The point of conceptual overlap that may be of concern is with the severity item "scares me" and the negative affect item "fear." Indeed, the correlation between affective risk and severity is .51. We nonetheless feel that it is worthwhile to include these in a combined model as each approach is strongly embedded in what have been fairly distinct literatures (the HBM in the health literature and cognitive/affective in the risk literature). So, we have added language to specify this approach and do also note that in the hybrid final model it is the measure of affective risk that remains, providing some support for its superiority over the severity approach in the HBM.

Concerning the inclusion of ethnicity, we have revised the first blocked model to not include ethnicity, thus focusing that analysis on optimization of a hybrid perceptual model. Ethnicity is then included in the final trimmed model. The rationale is also further emphasized as useful for consideration of alternate forms of prevention health messages.

I have a number of concerns with the questionnaire for example it asks about use of citronella candles which are known to not be effective mosquito repellants and can be dangerous and some of the questions use a 5 point scale while others a 3 or 4 point scale and it is unclear the rationale for that. The questionnaire would be better informed if it had been piloted for validation or the authors had used previously validated tools.

Citronella candles are not especially effective but they are marketed as mosquito repellant and widely available, so whether effective or not this is a self-protective behavioral response. It would be an interesting study to examine how effectively individuals practice the various forms of protection! The differences in response scales are due to us replicating measures from the previous studies. While not formally validated, this still allows the possibility of comparison.

The power of this study to detect a difference given the sample size and numbers of questions is unclear. A power calculation should be included.

Post-hoc power analyses were included in the revised statistics section

The results and discussion section is fragmented and difficult to follow. The authors need to take more time to describe what the correlations do and do not represent and then include their interpretation.
The methods, results, and discussion sections have been reorganized under subheads to provide better structure and clarity.

The tables are not labeled appropriately and the description of the variables should be written out in a glossary for the table.

*Tables have been reformatted into journal style.*

It is not clear that the conclusions are supported by the data given the very real potential for confounding and the lack of an understanding of the power of the study.

*Power analysis included. Confounding is always a problem in non-experimental studies, this is mentioned in the limitations.*

Specific comments:

Page 4 Lines 19-26 should be included in the intro not methods

*moved up*

Page 5 lines 7-12 is the same as what is in the background; does not need to be repeated but details of where the questions came from should be included

*Trimmed, and placed under a subhead.*

Page 5 line 19 ‘best practice follow up and cash incentives’ should be described.

*Included*

Page 5 line 20 numbers of questionnaires and % return rate are results

*moved*

Page 5 line 26 ‘others’ should have been described in the methods; it states ‘Anglos’ in the methods but these are not defined

*Anglos restated as Others with information provided on composition (primarily white, non-Hispanic)*

Page 6 lines 15-21 these describe methods and should be in that section

*Moved to new section on data analysis*

Page 7 lines 3-5 also belong in the methods

*Moved to new section on data analysis*