

Review of Baudrot and Charles, Recommendations to address uncertainties....

Reviewer Qualifier:

I am not a modeler. I am a “seasoned” ecotoxicologist and will be reviewing the paper for issues pertaining to assumptions of the models, model predictions and ERA statements, and the general ability to communicate the story to the reader. Model equations and assertions in Sections 2 and 3 MUST be reviewed by someone qualified to do so.

General:

The authors address an extremely important issue of uncertainties in environmental risk assessments (ERAs) using toxico-models. They state they are recommending an “innovative approach” using a Bayesian framework – which is an excellent approach, but perhaps not innovative. They focus on lethality vs. time – which is an appropriate first stage approach. Nevertheless, the key issue in ERAs and the controversial issue in decision-making at contaminated sites is chronic toxicity – not acute toxicity. The authors should acknowledge this and suggest further research in this area. The abstract leaves one “hanging” and should state explicitly what they are recommending and how they are moving science forward. The reader should not have to read the paper to find those answers! At the same time – the Conclusion (4.5) is completely void of specific conclusions from the paper. These must be stated explicitly – what did the authors develop in this Bayesian approach? How is it better? What are the uncertainties (do not simply refer to subsection 4.4.3)? and how will it improve ERAs? The meat of the paper is in subsections 4.3 and 4.4 (Discussion section), yet these need clarification and examples provided as they are confusing and not substantiated.

Specific:

1. The first paragraph of the Introduction is too long. Break it into separate thoughts.
2. Many of the acronyms being used, such as PNECs – are not being spelled out the first time they are used. In addition, they should be explained the first time used – as many will not understand what they are. In fact – there is an excess of acronyms, making reading challenging. Get rid of some of these.
3. Lines 347-38. The authors are incorrect in stating that ERAs rely on “fitting classical dose-response models to quantitative toxicity test data.” This is but one SMALL portion of ERAs, and those conducting ERAs use a multitude of approaches, and consider chronic toxicity and ambient biological responses to populations and communities – when those data are available. The authors must tone-down their assertions. Their proposed models, if used, will be a small part of the ERA process.
4. While much of the paper is well written, there are occasional basic English mistakes which must be corrected. I will not list all of them.
5. Please read and incorporate the findings from the following paper, which is a summary of a U.S. EPA Science Advisory Board report dealing with the needs for improving ERAs: Dale VH, Biddinger GR, Newman MC, Oris JT, Suter GW, Thompson T, Armitage TM, Meyer JL, Allen-King RM, Benfield EF, Burton GA, Chapman PM, Conquest LL, Fernandez JJ, Landis WG, Master LL, Mitsch WJ, Mueller TC, Rabeni CF, Rodewald AD, Sanders JG, van Heerden IL. 2008. *Enhancing the ecological risk assessment process. Integr Environ*

Assess & Mgmt 4:306-313. As you will find, the issues of temporal and spatial variance/characterization were recognized as a major uncertainty in ERAs and in need of improving. The authors study nicely addresses this need, but their recommendations should be placed in proper context with the many other issues associated with ERAs.

6. In the introduction the authors spend some time noting how the Bayesian approach is superior. Many others have done this already in terms of ERAs and assessment of contaminated sediments.
7. Line 366 - "... may hide a very large uncertainty due to its limitation to 100% of covering." This makes no sense.
8. Lines 392-394: Yes – but how are you addressing these uncertainties? You cannot – as they are often unknown.
9. Lines 396-400: This is hugely important and great that the authors note it – however the last sentence does not make sense. If there is a multiplication factor to address these uncertainties – what is it and how can it be selected? It is doubtful that it has a scientific basis. I find this all quite concerning...
10. Subsections 4.3 and 4.4: These subsections are VERY confusing and non-intelligible for the typical reader. It likely makes sense to the author, but not others. These should be moved to the end of the Results section and examples provided to support the statements being made.
11. The authors should feel free to share these comments with Theo Brock, as he understands the important issues and provided input on this paper.