Natural Hazards and Earth System Sciences
Discussions

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Comment on nhess-2021-264

Keith Beven (Referee)

Referee comment on "Long-term hazard assessment of explosive eruptions at Jan Mayen (Norway) and implications for air traffic in the North Atlantic" by Manuel Titos et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-264-RC1, 2021

This paper presents a forward uncertainty analysis of the dispersion of ash from eruptions on Jan Mayen island. While it is the first to do so for this particular location, it is not the first uncertainty analysis of this type, and some previous studies are not cited (e.g. Woodhouse et al., 2015; Prato et al, 2019; see also Harvey et al., 2018).

Since it is a forward uncertainty analysis the results depend completely on the prior assumptions made by the authors, given very limited information about prior eruptions, in terms of type, magnitude and impacts. While the authors give some background to their choice of prior distributions the subjectivity inherent in such choices is rather glossed over. Indeed, it is in some cases presented in a rather pseudo-scientific way to appear more persuasive – for example the mention of the Akaike information criterion to choose between distributional forms in representing the distribution of total erupted volumes when the primary influence has to be subjective choice of probabilities for the different types of eruption and their range of magnitudes.

It is therefore somewhat ironic that the authors suggest that their analysis is unbiased (L353). This is clearly not the case, it is biased by all their prior assumptions. The results are the logical consequence of those assumptions (given enough samples) so it is not clear what they mean by unbiased (probably best to avoid it).

The other major conceptual issue with the paper is the value of such a forward uncertainty analysis to inferences about impacts on air traffic. The results presented in the paper are integrated over the whole distribution of events simulated. They therefore clearly do not apply to any single event, and will not apply to the next event that might actually cause disruption. So certainly the areas indicated as at risk in the maps presented could be affected, and the maps could be used to guide an initial response of areas to avoid lacking further information. But application of such an approach to the next event would require Bayesian updating given information about that event (see e.g. Harvey and Dacre, 2016). Initially this might perhaps be to identify a sub-set of “similar” simulations but later perhaps using more explicit data assimilation – and about the specific meteorological conditions that might be quite different to those associated with a "similar" event run with reanalysis data, particularly in respect of wind fields relative to the integral of reanalysis sampling. I think this should be discussed further in the paper.

There is one technical point that I did not follow. On L377 it is stated that *The probability of each combination is weighted in accord with the associated magnitude.* It is not clear why this weighting should be applied. You have already sampled from the distribution of magnitudes which gives the (assumed) probability of such an event occurring, so why weight by magnitude?

Some minor points

Section 3.4 Location differences – is this a matter of resolution and limitations of the reanalysis rather than lack of real differences (what does such a wind profile really mean at reanalysis scales??).

L85. Delete "occurred"

L228. *Predictions made without uncertainty quantification (UQ) are usually not trustworthy and inaccurate.* I think this could be better worded. Normally for a forward uncertainty analysis the "best guess" prediction without uncertainty estimation would be within the modal range of the uncertainty analysis. Thus if it is not trustworthy and inaccurate so too are the equivalent ensemble members close to it … which is not really what you meant.

L276. *This is due to the fact that the height of the eruptive column for medium eruptive class eruptions does not exceed 11 km (see section 3.1)*

This a clear example of how inference depends on prior assumptions in a forward uncertainty analysis

L389. *This result, that we quantify at each point of the target domain, allows integrating hazard in quantitative risk analysis, through fragility curves. In this view, it represent the most complete way to quantify hazard.*

This could be discussed later but is not really relevant here since you do not apply any such fragility curves (or mention evaluating their uncertainties at additional computational expense….)

L330. *Finally we want to highlight the robustness of our PVHA in terms of uncertainty quantification, that should be routinely considered in all this kind of studies.*

Errrr …. What do you mean by robust exactly? Why should your subjective prior assumptions be considered robust?

Keith Beven

References

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