Nat. Hazards Earth Syst. Sci. Discuss., 2, C773–C778, 2014
www.nat-hazards-earth-syst-sci-discuss.net/2/C773/2014/

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "A multi-scale risk assessment for tephra fallout and airborne concentration from multiple Icelandic volcanoes – Part 1: Hazard assessment" *by* S. Biass et al.

Anonymous Referee #2

Received and published: 21 May 2014

General

This paper presents results from an extensive probabilistic analysis for several volcanoes located in Iceland. The approach is generally sound and accepting the comments below does a good job of conveying the multiple levels of uncertainty and variability present in perfuming such a study.

The paper is however overly long and primarily due to the somewhat repetitive nature of describing serval volcanoes and also statistical model configuration it is hard to read through. The paper should be shortened. If the author wishes this could be through

making more use of references and tabulated information or by placing more in the supplementary material.

In terms of statistical robustness I feel in disagreement with the authors for the long range dispersion simulations. They argue that ∼100 simulations is able to capture the statistical range of the problem. The dimensions of the problem (multiple meteorological parameters (3D wind, rain, cloud) varying on different time and length scales; the ESP parameters (PSD, mass, height, etc)) is rather large. I think that ∼100 simulations over 10 years of weather and with the impacts of changing the various parameters will result in very poor sampling of certain combinations and states. This may be the reason that some of the air concentration results seem to be rather 'noisy' when comparing locations. I feel this needs to be explored more in order to establish just how robust this is. I hasten to add that even if not robust, which I feel is likely the case, I would still support publication with the obvious changes in text, caveats etc as I appreciate that running more simulations is not necessarily achievable and also the paper still acts as a good method paper for what is a complex undertaking.

Specific Comments

pdf with imbedded comments attached. All are also included here:

— Page 2 —

Strikeout (red), 18 May 2014 23:57: In the assessment and comparison of

— Page 5 —

Highlight (yellow), 18 May 2014 23:57: Why inferred?

Highlight (yellow), 18 May 2014 23:57: This can only be arrival time for first ash. If it is not the leading edge of plume then how can you get travel time?

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Strikeout (red), 18 May 2014 23:57: Is intended to act

— Page 8 —

Strikeout (red), 20 May 2014 21:53: a few

Strikeout (red), 20 May 2014 21:53: of

— Page 13 —

Strikeout (red), 20 May 2014 21:53: by counting

Highlight (yellow), 20 May 2014 21:53: So this is time to first disruption. Arrival time is used in dispersion modelling to link specific release time to specific detection times. Your definition some what different. I would encourage the use of a different naming convention as I consider the current use confusing.

Highlight (yellow), 20 May 2014 21:53: Do you define the discretisation you use? At this point you have not.

Highlight (yellow), 20 May 2014 21:53: Strictly speaking not a point given that the model is Eulerian.

Highlight (yellow), 20 May 2014 21:53: Not sure I would call them time slabs. Time periods or intervals might be better. You have not defined the duration of time slab.

Highlight (yellow), 20 May 2014 21:53: The control of airspace differs around the world. In Europe it is strictly not a case of closing or opening. The exact way flights are controlled is complex and relates to how air traffic control services are provided and the safety risk assessment framework.

The use of concentrations is part of a risk assessment framework and aircraft can fly in air where certain concentrations are forecast.

I suggest that you need to read and reference the European and North Atlantic rules as published by ICAO.

Highlight (yellow), 20 May 2014 21:53: The next

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Highlight (yellow), 20 May 2014 21:53: This model only uses a single profile does it not? Therefore the resolution is irrelevant. This sentence makes a link between resolution on your model run what I think you want is just to point out the configuration of the NCEP run.

Highlight (yellow), 20 May 2014 21:53: I would like to see a few sentences describing the model. You are requiring the reader to be familiar with a range of models and subjects and little more info would help.

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Highlight (yellow), 20 May 2014 21:53: I find it odd that you have used two different re-analysis products for the two types of impact. These data sets will differ and so you analysis can not be considered consistent across the scales. I think you need to acknowledge this. You may say they are close enough but you need to provide some references for such a statement.

Highlight (yellow), 20 May 2014 21:53: Explain briefly please.

Highlight (yellow), 20 May 2014 21:53: What was the time resolution you extracted at? This is important, especially for your short duration eruptions where you will be smoothing out time based transitions.

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Highlight (yellow), 20 May 2014 21:53: You mean maximum total mass?

Highlight (yellow), 20 May 2014 21:53: You do this in both models? Would seem a odd thing to do for a true 4D NWP driven model and could well introduce inconsistencies.

Highlight (yellow), 20 May 2014 21:53: What is the sampling strategy? Just random with equal probability? Please explain.

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Highlight (yellow), 20 May 2014 21:53: Do you every explain the settings?

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Highlight (yellow), 20 May 2014 22:45: This paper is of interest across disciplines. Plus you have already used mm. I therefore suggest that you also quote sizes in metres or appropriate sub unit.

Highlight (yellow), 20 May 2014 22:45: So you did $\sim$100 simulations over 10 years. Given the dimensions of the problem (multiple meteorological parameters varying on different time and length scales; the ESP parameters) I find myself disagreeing that this number can adequately capture the statistical space.

I feel that you need to demonstrate that this is robust beyond the reference given or acknowledge that it is not.

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Highlight (yellow), 20 May 2014 22:45: Century

— Page 25 —

Strikeout (red), 20 May 2014 22:45: impacts are

Strikeout (red), 20 May 2014 22:45: are

— Page 26 —

Strikeout (red), 20 May 2014 22:45: an

Strikeout (red), 20 May 2014 22:45: has a

Strikeout (red), 20 May 2014 22:45: chance of

— Page 27 —

Highlight (yellow), 20 May 2014 22:45: This should be stated at the point the scheme is described earlier in the paper.

Highlight (yellow), 20 May 2014 22:45: I find it odd that at no point do you reference Leadbetter 2011. They conducted a simple impact assessment for a short Hekla eruption which while based on the old London VAAC approach can be shown to correspond to a concentration approach of 0.2 mg/m3 (Webster 2012). It is therefore possible to compare. They used 6 years of NWP and sampled at very high temporal frequency by repeating the eruption every few hours for the entire period.

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Highlight (yellow), 20 May 2014 22:45: Not algorithms. These are logic diagrams.

— Page 63 —

Highlight (yellow), 20 May 2014 22:45: The numbers on the figure legend of this and a number of other figures seem to be cut off at the bottom slightly meaning that the numbers are not as clear as they could be.

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Highlight (yellow), 20 May 2014 22:45: Either this or the later reference to 2 mg is.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/2/C773/2014/nhessd-2-C773-2014-supplement.pdf

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 2463, 2014.