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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 #1

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This paper presents new maps showing probabilities of exceedance of specific thresholds in tephra deposition within Iceland and in ash-cloud concentration over Europe as a result of eruptions at Hekla, Katla, Askja, and Eyjafjallajökull volcanoes. The probabilistic maps are based on thousands of model simulations using Tephra-2 (for deposit runs) or Fall3d (for ash-cloud runs), using a Monte-Carlo approach to assigning input parameters. Results are in the form of maps that appear to show the probability of exceedance given a particular eruption scenario.

From a scientific standpoint the methodology seems appropriate and reasonably well explained, with a few exceptions: 1) Some details of model setup for the Fall3d model,

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such as nodal spacing in x, y, and z; and the value of the Suzuki constant, do not appear to be described (at least I couldn't find them). 2) Some results, such as that in fig 13 b, e, and h "probability maps of exceeding an arrival time of 24 h for a concentration of 2 mg/m3", were confusing and not well enough explained that I could understand them. 3) Results in table 5 show large discrepancies in the arrival time at European airports from different eruptions, all originating in Iceland. It was not clear to me why such large discrepancies would exist. 4) It should be emphasized that probabilities in Figures 8-13 are conditional probabilities given that an eruption has occurred. I was expecting them to be annual probabilities that multiplied the conditional probabilities by the annual probability of an eruption.

By far my greatest criticism of this paper is that it is too long and exhausting to read. Section 2, giving the geologic setting, could be abbreviated given that this information is available in other publications. Section 5, the Discussion Section, should be cut almost entirely and rewritten to bring out and explore the important findings. The current material in this section is mainly a justification of specific methods, or a re-discussion of the results in Figure 8-13. This section is seven pages long and contains almost nothing new.

Below are specific comments, some of which duplicate or expand on those above.

Abstract, Line 1: I suggest leaving out the first clause of this sentence and starting with "We developed a new approach . . ." –Lines 16 and 18: are the probabilities that you cite for Askja tephra accumulation and Katla cloud concentration annual probabilities? How is time incorporated into these probabilities?

p. 2465, line 4; I would change "sequences" to "deposits" for clarity. –line 5; consider changing "development of comprehensive eruptive scenarios" to something like "better constraining past eruptive histories". –line 16, delete "a wide range of aspects such as" –line 23: change "far-range" to "far-ranging"

p. 2466, line 18: change "integrant" to "integral"

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p. 2467, line 5: change "greater or equal to" to "greater than or equal to"

p. 2468, line 7, change "Iceland" to something like "the island of Island" (to distinguish the geologic feature from the country). –line 17: change "On the contrary" to "In contrast" –line 20: change "volcano" to "volcanoes"

page 2469, line 16: change "regime" to "pattern" –line 25: why are the historical Hekla eruptions referred to as "mixed"? What are they a mixture of? Is "mixed" a term coined by the two Thordarson studies you cite?

page 2471, line 1: change "observe two cycles" to "infer two cycles" (I assume Oladottir did not actually observe two cycles during the Holocene). –line 4: it's not clear to me what you mean by "volumes of eruption frequency". Volume output? –line 8: change "conservation" to "preservation". Also, it's not clear to me why preferential preservation of deposits to the east would be related to the presence of an ice cap. Is the ice cap primarily on the western side of the volcano? And what does the current period of low activity have to do with preservation? Please clarify. –line 18 and elsewhere. I would prefer that tephra volumes be given in DRE rather than "freshly fallen", especially given that you haven't define quantitatively what it means, other than it's 40% greater than that of old tephras. Better yet would be an estimate of erupted mass. –line 23: add a semicolon after "Katla", and another on the following line, after "2011".

page 2472, line 20-21. Saying that 80% of the Eyja tephra was airborne and the remaining 20% was transported by ice and water seems artificial. Is it certain that the ice- and water-transported tephra was not also in the air at some point? –line 26-27: change "Askja volcanic system is composed by . . ." to "The Askja volcanic system comprises . . ."

page 2473, line 5: delete "phase" from after "activity" (or change to "active phase") –line 7: change "eruption" to "eruptions" –line 8: add "one" before "on the south of it". –line 11: is the stated tephra volume for Askja DRE? Bulk? New fallen?

page 2474, line 25: what does the backwards "E" mean?

page 2475, line 8 (and elsewhere): change "time slabs" to "time intervals". Also, it's not clear to me how you calculate mean persistence from the description on this line. –line 20, change "map comprehensive" to "comprehensive map"

page 2476, line 8-9: I'm not sure I understand what you mean by a "fall-time threshold". A threshold time to fall from the particle's initial elevation to the ground surface?

page 2477, line 4: what value of the Suzuki constant do you use for the models. – Section 3.1: Perhaps I missed it, but I don't see any information on the horizontal or vertical grid resolution used in the Fall3d model simulations. Vertical resolution can affect the cloud concentration illustrated in Fig. 13. –line 8: what is meant by "the stratified sampling technique"? Can you describe it in a sentence? You cite Scaini et al. (2012), which constructs a "typical meteorological year" and then generates synthetic wind fields based on that using the WRF-ARW model. Is that what you do here? –line 9: add "us" after "allowing" –lines 17-18: what do you mean by "a single occurrence of the model"? A single simulation? A simulation based on a wind field at a single point in time?

page 2478, lines 19-20: why is tropopause height fixed at 10 km? Can't it be determined from the meteorology?

page 2481, lines 5-6: Could you explain in a sentence or two what the sampling technique of Scaini et al. (2012) is?

page 2484, line 25: what do you mean by "area of conservation"?

page 2487, line 19: cite Fig. 1 when referring to Gulfoss and Vik I Myrdal.

page 2488, line 10: change "a" to "an"

page 2491, line 19: change "airports" to "airport" –line 29: here you use an airborne concentration threshold of 0.2 mg/m3, which is 10x less than the threshold of 2 mg/m3

used elsewhere (e.g. fig. 13). Was this intentional? Perhaps just delete the word "threshold" here to avoid confusion?

page 2493, line 2: "the arid climate of Iceland is prone to fast erosion of . . . deposits". Rainfall maps of Iceland that I've seen show mean annual precipitation of hundreds to a few thousand mm per year, which is not arid. And why would an arid climate promote rapid erosion of the deposits? Perhaps the cold, windy climate and lack of vegetation promote rapid erosion?

Section 5: Discussion section. This section is overly long, seemingly directionless, and does not do the job of a Discussion section, which is to bring out and explore the main findings of the paper. Section 5.1, for example, is mainly a justification of the choice of limiting the time period of examination to historical events. This should be covered in the Methods section. Section 5.1.2 is similarly a justification of your method of choosing whether deterministic or stochastic sampling methods are used for the different simulations. Figure 5.1.4 simply covers in more detail your use of multiple phases in long-lived eruptions but does not add any new insights that I can see. Section 5.2 re-describes Figures 8-13, noting the general directionality of deposits (more towards the east) but offering no other significant new insights. I would completely delete this section and write a much shorter one.

Table 1 caption: the use of the term "freshly fallen" is confusing to me. Can you simply state the deposit density used to derive the tephra volumes in column 2? Adding another column for estimated erupted mass would also be useful.

Table 2: indicate "C.E." or something similar after "Eruption year" in Column 1.

Table 4: I think that 2 mg/m3 was the boundary of the "no-fly" zone in Europe rather than the threshold for precautionary maintenance. I thought the threshold for precautionary maintenance was 0.2 mg/m3.

Table 5: I'm surprised at the large difference in mean arrival times at a given airport

from different eruptions. At the Amsterdam airport for example, mean arrival times for the Hekla and Askja eruptions is 15, 15, and 22 hours, but for Katla eruptions it's 48 hours. The wind pattern from Katla to Amsterdam can't be that different. In fact, Katla arrival times at all airports are more than twice that from other eruptions. Why?

Figure 1 caption, line 5: change "The small scale map shows" to "Fig. 1b shows"

Figure 2 caption. It would be useful to define the terms "get", "set", and "sample" here. I notice also that aggregation is not mentioned in the LLOES scenario. Is this intentional?

Figure 3 caption, last 2 lines: change "probability of the wind to blow" to "probability that the wind will blow"

Figure 13: I'm a little confused about the meeting of the maps portrayed in b, e, and h. "Probability of exceeding an arrival time of 24h for a concentration of 2 mg/m3"? I would expect the arrival time to increase with increasing distance from the volcano; hence the probability that the arrival time >24hr should also increase with distance. But in these figures it appears to increase towards the volcano.

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